THE

CABINET CYCLOPÆDIA.

London:
Printed by A. Spottiswoode,
New-Street-Square.

HISTORY
of
NATURAL PHILOSOPHY,

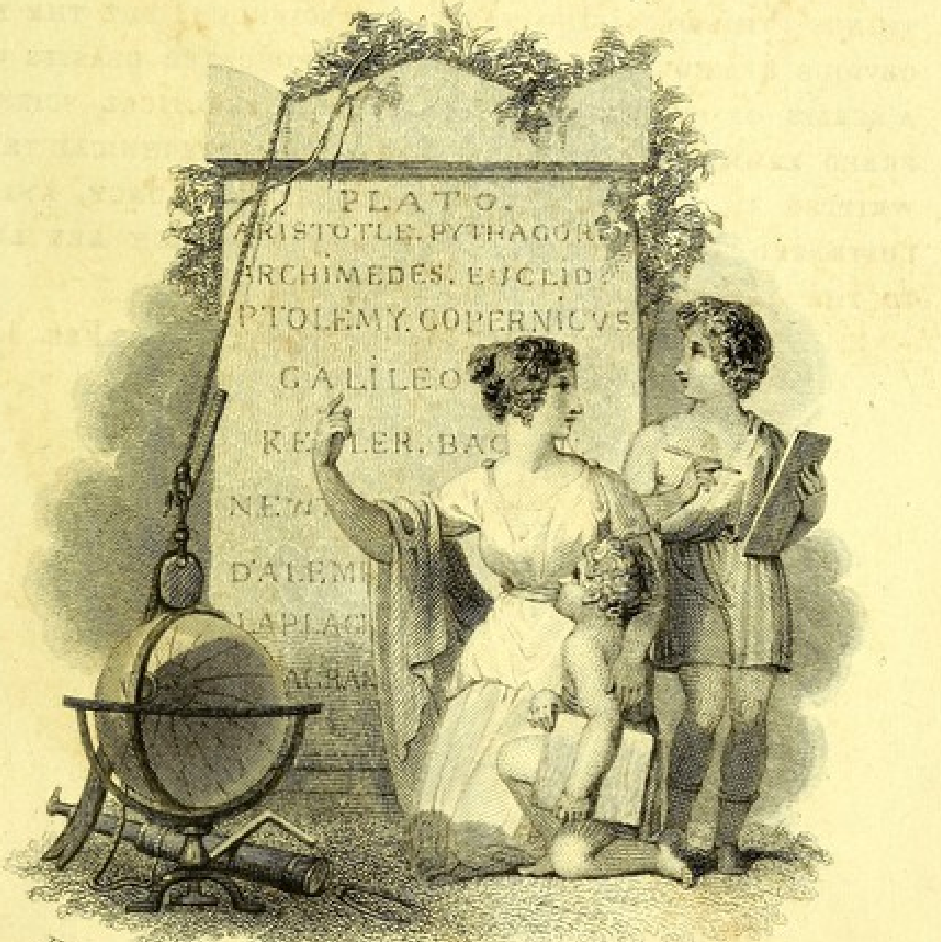
FROM THE EARLIEST PERIODS

TO THE PRESENT TIME,

by

BADEN POWELL, M.A.

Savillian Professor of Mathematics in the University of Oxford.



H. Corbould del. *E. Finden sc.*

London:
PRINTED FOR LONGMAN, REES, ORME, BROWN, GREEN & LONGMAN, PATERNOSTER ROW.
AND JOHN TAYLOR, UPPER GOWER STREET;
1834.

ADVERTISEMENT.

In presenting this second volume, the Author conceives it almost unnecessary to remark, that a work like the present can hardly be expected either to possess much originality of observation, or to exhibit much depth of research. To select and arrange the materials already furnished to his hands, so as best to suit the design of a popular compendium, has been his principal object; and to render the account of scientific inventions as clear and intelligible as their nature would admit, and to invest them with a character, if possible, inviting to the general reader, has been his main endeavour. He is anxious to acknowledge the sources from whence he has most largely derived assistance:— The Dissertation on the Progress of Mathematical and Physical Science, by Professor Playfair; the "Histoire de l'Astronomie," by M. Bailly; The History of Astronomy in "The Library of Useful Knowledge;" "The Lives of Galileo and Kepler," by Mr. Drinkwater; and that of Newton, translated from M. Biot, in the same collection; and the life of the same philosopher by Sir D. Brewster.

THE

CABINET CYCLOPÆDIA.

CONDUCTED BY THE

REV. DIONYSIUS LARDNER, LL.D. F.R.S. L. & E.
M.R.I.A. F.R.A.S. F.L.S. F.Z.S. Hon. F.C.P.S. &c. &c.

ASSISTED BY
EMINENT LITERARY AND SCIENTIFIC MEN.

Natural Philosophy.

AN
HISTORICAL VIEW OF THE PROGRESS
OF
THE PHYSICAL AND MATHEMATICAL SCIENCES,

FROM THE EARLIEST AGES TO THE PRESENT TIMES.

BY
THE REV. BADEN POWELL, M.A. F.R.S.
SAVILIAN PROFESSOR OF GEOMETRY IN THE UNIVERSITY OF OXFORD.

LONDON:
PRINTED FOR
LONGMAN, REES, ORME, BROWN, GREEN, & LONGMAN,
PATERNOSTER-ROW;
AND JOHN TAYLOR,
UPPER GOWER STREET.
1834.

IT IS NOT EASY TO DEVISE A CURE FOR SUCH A STATE OF THINGS (THE DECLINING TASTE FOR SCIENCE) ; BUT THE MOST OBVIOUS REMEDY IS TO PROVIDE THE EDUCATED CLASSES WITH A SERIES OF WORKS ON POPULAR AND PRACTICAL SCIENCES, FREED FROM MATHEMATICAL SYMBOLS AND TECHNICAL TERMS, WRITTEN IN SIMPLE AND PERSPICUOUS LANGUAGE, AND ILLUSTRATED BY FACTS AND EXPERIMENTS WHICH ARE LEVEL TO THE CAPACITY OF ORDINARY MINDS.

QUARTERLY REVIEW FOR FEB. 1831.

ADVERTISEMENT.

In presenting the ensuing volume, the Author conceives it almost unnecessary to remark, that a work like the present can hardly be expected either to possess much originality of observation, or to exhibit much depth of research. To select and arrange the materials already furnished to his hands, so as best to suit the design of a popular compendium, has been his principal object; and to render the account of scientific inventions as simple and intelligible as their nature would admit, and to invest them with a character, if possible, inviting to the general reader, has been his main endeavour. He is anxious to acknowledge the sources from whence he has most largely derived assistance: — "The Dissertation on the Progress of Mathematical and Physical Science," by Professor Playfair; the "Histoire de Physique," by M. Libes; "The History of Astronomy," in "The Library of Useful Knowledge;" "The Lives of Kepler and Galileo," by Mr. Drinkwater; and that of Newton, translated from M. Biot, in the same collection; and the life of the same philosopher, by Sir D. Brewster.

The Author also conceives it necessary to add (what indeed he has expressed at the beginning of his last section), his confession that the work is not completed as he had from the first intended it should be. He found, too late, that he had transgressed the necessary limits, when it was impracticable to modify the earlier portion of the history: the last, and most important, period of it is, therefore, unfortunately curtailed to a very meagre sketch.

CONTENTS.

SECTION III.

THE STATE OF SCIENCE DURING THE AGE OF THE ROMAN EMPIRE TO THE PERIOD OF ITS DISSOLUTION.

PART II.

THE PROGRESS OF MATHEMATICAL AND PHYSICAL SCIENCE FROM THE MIDDLE AGES TO THE TIME OF NEWTON.

SECTION I.

THE SCIENCE OF THE MIDDLE AGES, AND ITS FIRST RENOVATION, TO THE END OF THE FIFTEENTH CENTURY.

SECTION II.

SCIENCE IN THE SIXTEENTH CENTURY — THE FIRST GREAT MODERN IMPROVEMENTS — THE DISCOVERIES OF COPERNICUS AND TYCHO BRAHE.

SECTION III.

THE DISCOVERIES OF KEPLER AND GALILEO.

SECTION IV.

THE CONTEMPORARIES AND SUCCESSORS OF GALILEO — THE BACONIAN PHILOSOPHY, AND THE PRECURSORS OF NEWTON.

PART III.

THE PROGRESS OF PHYSICAL AND MATHEMATICAL SCIENCE FROM THE TIME OF NEWTON TO THE PRESENT DAY.

SECTION I.

THE DISCOVERIES OF NEWTON.

SECTION II.

THE DISCOVERIES OF NEWTON'S SUCCESSORS.

CHRONOLOGY

OF

PHYSICAL AND MATHEMATICAL SCIENCE.

B. C.

2000. Alleged Cultivation of Astronomy and other Sciences among the Indians, Chinese, Chaldeans, and Egyptians.

1500. Moses.

1100. Chinese Observation of Solstices recorded.

1020. Solomon: Natural History.

1000. The Surya Siddhanta.

720. Earliest Chaldean Observations recorded by Ptolemy.

600. Thales: Astronomy.

590. Pythagoras born.
Pythagoras: Astronomy; Solar System; Geometry.

500. Anaximander. Anaximenes. Anaxagoras.

450. Philolaus. Nicetas. Archytas. Empedocles.

432. *Cycle of Meton* began.

400. Eudoxus. Democritus.

350. Aristotle: Physics; Mechanics.
Plato: Conic Sections; Geometrical Analysis.

300. Epicurus. Menechme; Aristæus: Geometry.
Foundation of the School of Alexandria by Ptolemy Philadelphus.
Euclid: Geometry; Optics. *Method of Limits and Exhaustion.*

287. Archimedes born. Aristarchus: Astronomy.

250. Apollonius: Geometry.
Archimedes: Exhaustions; Mechanics; Hydrostatics.

212. Archimedes died. Eratosthenes: Arc of Meridian.

200. Nicomedes: Geometry.

150. Hipparchus: Astronomy. *Catalogue of Stars; Precession of Equinox.*
Ctesibius; Hero: Mechanics and Hydrostatics.
Lucretius; Posidonius: Tides.

100.

50. Theodosius; Menelaus: Geometry.
J. Cæsar; Sosigenes: Astronomy; Calendar.

A. D.

50. Strabo; Seneca; Pliny: Various Points of general Physics.

100. Plutarch.

130. Aulus Gellius: Optics.

140. Second School of Alexandria.

150. Ptolemy: Astronomy; System of the World; Epicycles.

200. Diophantus: Algebra.

300.

350. Theon; Pappus: Geometry.

400. Proclus: Geometry.

450. Diocles: Geometry.

500.

600.

700.

713. Moorish Empire in Spain.

800. Astronomy among the Arabs. Al Mamoun, &c.

827. Translation of Ptolemy's Almagest into Arabic.

870. Alfred.

900. El Batani: Astronomy.

959. Ben Musa: Algebra.
Gerbert introduced *decimal Arithmetic* from the East into Europe.

1000. Ibn Junis: Astronomy. Aboul Wefa: Trigonometrical Tables.

Adhelard: Translation of Euclid from Arabic.
1100. Alhazen: Optics. Foundation of Universities.
Campanus: Translation of Euclid.
1180. Magnetic Needle: Guyot.
1200.
1202 Leonarda introduces Algebra into Europe.
Frederic II.: Latin Translation of Ptolemy.
1214. Roger Bacon born.
1220. Nasseer ad deen: [Astronomy.
1250. Alphonso X.: Astronomical Tables.
1260. Mariner's Compass: Gioja.
1266. *R. Bacon's Opus Majus.*
1270. Vitello: Optics.
1292. R. Bacon died.
1300. Dante: Physics.
1313. Lenses in Use.
1350.
1400.
1436. Regiomontanus born.
1437. Copernicus born.
1440. Ulugh Beg: Astronomy. Tartar Sovereigns encourage Astronomy.
Regiomontanus: Trigonometry; Translation of Ptolemy.
1450. Purbach: Astronomy.
1452. Capture of Constantinople. Greek introduced into Europe.
1476. Regiomontanus died. Walther: Astronomy.
Commadine: Commentary on Euclid.
Lucas de Burgo: Algebra.
1500.
Peter Ramus opposes Aristotelian Philosophy.
1540. Fracastor: Dynamics.
1543. *Copernicus's System of the World* published. His Death.
1544. Stiphelius: Algebra. Recorde: Algebra.
1545. Cardan: Algebra; Rule for Cubic Equations.
1546 Tycho Brahe born.
1558. Werner: Geometrical Analysis. Pelitarius: Algebraical Equations.
1560. Bombelli: Algebra.
William Landgrave of Hesse: Astronomy. Rothmann. J. Byrgius.
B. Porta: Magia Naturalis; Optics.
1561. Francis Bacon born.
1564. Galileo born.
1571. Kepler born.
1572. Tycho Brahe patronised by Frederic, King of Denmark: Observatory of Uranibourg.
1575. Maurolycus: Optics; Geometry.
1576. Dip of Needle; Norman.
1577. Guido Ubaldi: Mechanics.
1580. *Harriot: Algebraical Equations* (not published till 1631).
1582. Reformation of Calendar by Pope Gregory XIII.
1585. Benedetto: Mathematics.
1589. Galileo, Professor at Pisa: Falling Bodies; Opposition to Aristotle.
1590. Vieta: Algebraical Equations; Angular Sections.
Albert Girard: Negative Quantities (published 1669).
Gilbert: Magnetism; Electricity.
1592. Galileo at Padua: Treatise on Mechanics.
1596. Kepler's Mysterium Cosmographicum. Des Cartes born.
1598. Wright: Trigonometrical Curves.
1600. Stevin: Inclined Plane; *Foundation of. Statics.* Bruno burnt.
1601. Tycho Brahe died.
1602.
1603.
1604.
1605.
1606.
1607.
1608.
1609. *Kepler:* Commentary on Mars; *Two First Laws.*
Galileo's Telescope.
1610. Satellites of Jupiter discovered by Galileo.
1611. Spots in the Sun: Saturn, &c.
Kepler: Optics; the Eye.
De Dominis; Rainbow.
1612.
1613. Galileo's Thermoscope; Laws of Hydrostatics.
1614. Napier: *Logarithms*; Trigonometrical Formulæ.
1615. Kepler: Stereometry. Foscarinus. Galileo: Longitude.
1616. Galileo before the Inquisition.
1617. Arc of Meridian measured by Snell.
1618. *Kepler's Third Law.*
1619. *Snell: Law of Refraction.*
1620. BACON'S NOVUM ORGANON.
1621.
1622.
1623.
1624. Briggs: Arithmetica Logarithmica; Binomial Coefficients.

1625. Variation in the Declination of the Needle. Gellibrand.
1626. Bacon died.
1627. Rudolphine Tables: Kepler.
1628.
1629.
1630. Kepler died.
1631. Transit of Mercury: Gassendi. *Vernier.*
1632. Galileo: *Dialogues on the System;* before the Inquisition.
1633. Abjuration; Confinement.
Des Cartes: System of the World.
1634.
1635. Arc of Meridian. Norwood; Fernel:
Cavalieri: *Indivisibles.*
1636. Galileo: Dialogues on Motion.
1637. Des Cartes: GEOMETRY (published); Dioptrics.
1638.
1639. Transit of Venus: Horrox.
1640.
1641.
1642. Galileo died. Newton born.
1643.
1644. *Barometer: Toricelli.*
1645. Philosophical Meetings commenced in London.
1646. Leibnitz born.
1647.
1648. Philosophical Meetings commenced at Oxford.
1649. Wallis, Wren, Wilkins, Boyle, &c.
1650. Astronomical Telescope: Scheiner. Des Cartes died.
1651.
1652. Academia Naturæ Curiosorum founded.
1653.
1654. *Air Pump:* Otto Guericke.
1655.
1656.
1657. Academia del Cimento founded.
1658. Huyghens: Theory of *Pendulums;* Application to Clocks.
1659. Huyghens: Saturn's Ring and one Satellite.
1660. Wendelein: Motions of Jupiter's Satellites.
1661.
1662. Pascal died (Works published afterwards). Royal Society incorporated.
1663. J. Gregory: Optica Promota; Reflecting Telescope.
1664. Newton's prismatic Experiments commenced.
1665. *Inflexion of Light:* Grimaldi. Telescopes to Quadrants; Picard.
Newton: Series for Quadrature of Curves; Binomial Theorem; PRINCIPLE OF FLUXIONS.
1666. Newton's first Idea of Gravitation. Royal Academy of Sciences at Paris founded.
Borelli on Jupiter's Satellites; Hint of Gravitation.
1667. Quadratures; Mercator, Wallis. Royal Observatory at Paris founded.
1668. Laws of Collision: Wren, Wallis, Huyghens.
1669. Newton: ANALYSIS OF LIGHT; Reflecting Telescope.
Lucasian Professorship; Optical Lectures.
Bartholinus: double Refraction.
1670. Huyghens: Horologium Oscillatum published.
Mercurial Thermometer; Lana.
1671. Cassini: second Satellite of Saturn.
Newton's Analysis of Light communicated to the Royal Society.
Retardation of Pendulum at the Equator: Richer, Varin, Des Hayes.
1672. Newton's Reflecting Microscope. Improvements in the Telescope.
Attacks on Optical Discoveries.
Inflexion of Light: Hooke.
1673. Hevelius: Libration of Moon in Longitude.
1674. Hooke approaches to the Theory of Gravitation.
Leibnitz communicated Series to Oldenburgh.
Colours of thin Plates: Boyle and Hooke.
1675. Newton's Experiments on thin Plates, &c. communicated to the Royal Society.
Observatory at Greenwich founded.
1676. Newton's Letter to Leibnitz, concealing Fluxions.
Velocity of Light: Roëmer.
Halley at St. Helena.
1677. Leibnitz's Letter to Newton, containing *Differential Calculus.*
Cassini: Rotation of Jupiter.
1678. Huyghens: Law of Double Refraction; *Theory of Undulations.*
1679. Falling Bodies and Elliptic Motion: Hooke.
Arc of Meridian, by Picard.
1680.
1681.

1682. Newton resumes the Subject of Gravitation.
1683.
1684. Cassini discovers three more Satellites of Saturn.
Discussion on Elliptic Motion: Hooke, Wren, Halley.
Halley learns Newton's Results, and informs the Royal Society.
THEORY OF UNIVERSAL GRAVITATION.
1685.
1686. Marriotte on Motion of Fluids.
Newton's Theorems communicated to the Royal Society. Dispute with Hooke.
1687. *Publication of the Principia.*
1688. Halley: Evaporation, Origin of Springs.
1689.
1690. Huyghens: Traité de la Lumière published. Roëmer: Transit Instrument.
1691. Newtonian System taught by J. and D. Gregory in Scotland.
1692. Newton's Illness. Loss of MSS. by Fire.
1693. ——— Letters to Bentley.
1694. ——— Correspondence with Flamstead on Lunar Theory.
1695. Newton made Warden of the Mint. Nieuwentyt attacks Fluxions.
1696.
1697. ——— Solution of Problem of swiftest Descent. Clarke's Notes on Rohault. Newtonian System studied at Cambridge.
1698.
1699. Commencement of Controversy on the Invention of Fluxions: De Duillier, Keill, Leibnitz, &c.
Newton, Master of the Mint, and Member of the Academy of Sciences. Thermometrical Scale.
1700.
1701. La Hire and Cassini resume Arc of Meridian.
1702. Attacks on Differential Calculus, by Rolle and Gallois: answered by Leibnitz, Bernoulli, &c.
1703. Newton resigns Professorship; elected President of the Royal Society.
1704. Publication of the Optics and Mathematical Tracts. Vanishing Fractions: Bernoulli.
Keill's Lectures at Oxford.
1705. Newton knighted. Review of his Tracts in Leipsic Journal.
1706.
1707. Arithmetica Universalis published by Whiston.
1708. Keill replies to Leipsic Journal.
1709.
1710.
1711. Newton's Analysis per Æquationes, &c. published. Leibnitz appeals to the Royal Society.
1712. Report and Commercium Epistolicum published.
1713. Desagulier's Lectures. Second Edition of Principia, by Cotes.
1714. Newton's Report on Longitude. Establishment of Board of Longitude.
1715. *Taylor's Method of Increments.*
Leibnitz attacks Newton's Doctrines: Reply by Newton and Clarke. Division of Saturn's Ring: Cassini.
1716. Problem of Trajectories.
1717. Stirling's Commentary on Newton's Lines of 3d Order.
1718. Arc in North of France. Deduction of prolate Spheroid.
1719.
1720. Electricity: Gray. Halley on Theory of Moon.
1721. Cotes's Harmonia Mensurarum.
1722. Daily Variation of Needle: Graham.
1723.
1724. Bernoulli on Communication of Motion. Discussion on Vis viva.
1725. Third Edition of Principia, by Pemberton.
1726.
1727. Newton died. *Aberration* discovered by Bradley.
1728.
1729.
1730.
1731. Hadley's Quadrant.
1732. Du Fay: Electricity.
1733. Integrations: Clairaut.
1734. Bishop Berkeley attacks Fluxions in the Analyst.
1735. Arc by La Condamine in Peru and Maupertuis in Lapland.
Oblate Spheroid deduced. Attraction of Mountains.
1736. Publication of Newton's Fluxions by Colson.
1737. Dynamics: Clairaut.
1738.
1739. Rectification of Error of Prolate Spheroid.
1740. Maclaurin on Figure of the Earth from Theory.

1741.
1742.
1743. Clairaut: Figure of Earth. D'Alembert: *Dynamique.*
1744. Euler: Isoperimetrical Problems.
1745. *Nutation:* Bradley. Clairaut: Perturbations.
1746. Leyden Phial: Muschenbroeck.
1747. Franklin: Identity of Electricity and Lightning. Vibrations of Musical Strings: D'Alembert.
1748. Euler on *Perturbations* of the Planets.
1749. Chronometers: Harrison. Laplace born.
1750. Arc in Italy by Boscovich. Mural Quadrant and Transit employed by Bradley at Greenwich.
1751.
1752. La Caille: Observations at Cape of Good Hope. Euler's Second Memoir on Perturbations.
1753.
1754. Expansion by Heat: Smeaton.
1755. Riccati: Analysis.
1756.
1757. Landen: Residual Analysis.
1758. Comet predicted by Halley. *Achromatic Telescope;* Dollond.
1759. Mathematical Theory of Electricity: Æpinus. Boscovich's Theory of Matter.
1760.
1761. Integrations: Euler.
1762. *Latent Heat;* Black.
1763.
1764. Harrison's Chronometers rewarded. Arc in America. Mason.
1765.
1766.
1767. Resolution des Equations: Lagrange.
1768.
1769.
1770.
1771. Electricity: Cavendish.
1772. Developement of Functions: Lagrange.
1773.
1774. Attraction of Mountains: Maskelyne. Density of Earth.
1775. *Tides:* Laplace.
1776. Differential Equations: Lagrange.
1777. Developement of Functions: Laplace.
1778.
1779. Electricity: Lord Stanhope.
1780.
1781. Uranus discovered by Herschel. Wilcke: Specific Heat. Friction: Coulomb.
1782.
1783. Nebulæ, &c.: Herschel.
1784. Calculus of *Variations:* Lagrange.
1785. Electricity: Coulomb.
1786. Integrations: Legendre. Hydraulics: Du Buat.
1787. Trigonometrical Connection of Observations of Greenwich and Paris. Commencement of Trigonometrical Survey. Lunar Theory: Laplace.
1788. *Mécanique Analytique: Lagrange.*
1789. Herschel's large Telescope. Two more Satellites of Saturn.
1790.
1791. *Galvani and Volta.*
1792.
1793. Englefield on Comets.
1794.
1795. Theory of Probabilities: Laplace.
1796. *Théorie des Fonctions; Lagrange.*
1797. Catalogue of Stars: Bode.
1798. Attraction: Cavendish.
1799. MECANIQUE CELESTE: LAPLACE. Supplements published successively.
1800. Solar Light and Heat: Herschel. Arbogast: Calc. des Derivations.
1801. Deoxidizing Rays: Ritter. Ceres discovered by Piazzi. Colours of Striæ, &c.: Young. Spence: Analysis.
1802. Pallas discovered by Olbers. Lines in Spectrum: Wollaston.
1803. Binary Systems of double Stars: Herschel. INTERFERENCES: YOUNG.
1804. Juno discovered by Harding. Leslie on Heat, Screens, &c. Hydraulics: Prony.
1805. Capillary Attraction: Laplace.
1806. Completion of French Arc from Dunkirk to Barcelona: Mechain and Delambre.
1807. Vesta discovered by Olbers. Satellites of Uranus: Herschel.
1808. Planetary Theory: Lagrange.
1809. Gauss: Theoria Motûs. Prevost: radiant Heat. Attractions of Spheroids: Ivory, Gauss. Acoustics: Chladni.
1810. *Polarisation of Light:* Malus.

1811. *Colours in crystallised Plates* by polarised Light: Arago and Brewster independently.
1812. Colours in Mica by polarised Light: Biot. In Quartz; Theory, Biot (continued 1818).
1813. Lunar Theory: Laplace. *Polarised Rings* (uniaxial): *Brewster.*
1814. Theory of Dew: Wells.
1815. Parallax: Pond and Brinkley (continued till 1825).
Theory of Waves: Poisson and Cauchy.
1816. Theory of Flame; Safety-lamp: Davy.
Stability of System: Poisson. *Interferences of polarised Light: Fresnel.* Colours in unannealed Glass, &c.: Brewster. Calculus of Functions: Babbage. Integrations: Bromhead.
1817. Berard: Radiant Heat. Biaxial Rings: Brewster.
Integrations: Herschel.
1818. Dulong and Petit: Law of Cooling, and Specific Heat.
Kater on the Pendulum. Seebeck on Prismatic Heat.
1819. *Electro-Magnetism: Œrsted.* polarised Tints: Herschel.
Periodical Comet: Encke. Absorption of Light: Brewster.
Biaxial Crystals: Biot. Laws of Magnetism: Barlow. Series: Babbage. Equations: Horner.
1820. Foundation of Astronomical Society. Struve: Double Stars.
1821. Magnetism: Scoresby, Sabine, &c. Electro-Magnetism: Ampère; Arago, Davy, &c. Extension of Arc to Shetland and Minorca; Biot and Arago. Attractions of Spheroids; Ivory. Analysis: Cauchy.
1822. *Double Stars:* Herschel and South. Thermo-Electricity and Thermo-Magnetism: Seebeck, Cumming, Moll, &c. Savart: Acoustics.
1823. Velocity of Sound: Moll and Van Beek.
Effect of Heat on Crystals: Mitscherlich. *Condensation of Gases:* Faraday.
Frauenhofer: Lines in Spectrum.
1824. Perturbations: Bessel. Double Stars: Herschel and South (continued 1826).
Systems of Rays: Hamilton.
1825. Pendulum: Sabine. Figure of Earth. Biela: Comet.
Figure of Equilibrium: Ivory (continued 1831).
Magnetism by Rotation: Barlow, Christie, Babbage, &c. (continued 1827).
Radiant Heat: Powell and Ritchie (continued 1826-7).
Angular Sections: Poinsot.
1826. Atmosphere: Dalton. Light and Magnetism: Christie, Mrs. Somerville. Parallax: Herschel.
Compression of Water: Perkins. Magnetism: Parry, Foster, Christie, &c. (continued 1827).
1827. Solar Theory: Airy (continued 1828).
Laplace died. Pouillet: Meteorology.
1828. Astronomical Observations at Paramatta: Dunlop and Brisbane.
Endosmose and Exosmose: Dutrochet.
1829. Fluid Lens: Barlow (continued 1831, 1833). Colours of grooved Surfaces: Brewster.
1830. Compression and double Refraction. Analysis of Light: Brewster. Metallic polarisation, ditto.
Pendulum: Sabine. Physical Astronomy: Lubbock (continued 1831, 1833).
Airy: Undulatory Theory: Modification of Newton's Rings. Elliptic Polarisation, &c.
Cauchy: Dispersion of Light on Undulatory Theory.
1831. *British Association for Promotion of Science,* commenced its Meetings at York.
Electric Spark from Magnet: Faraday, Nobili, Forbes.
Electro-Magnetism: Barlow, Fox, &c. Elliptic Transcendents: Ivory.
Tides: Lubbock (continued 1832, and Whewell, 1833). Radiant Heat: Nobili and Melloni.
1832. Inequality of Venus: Airy. Perturbations: Ivory.
Electricity: Faraday. Atmosphere of Mars: South. Meeting of British Association at Oxford.
1833. At Cambridge.

AN

HISTORICAL VIEW

OF

THE PROGRESS

OF

THE PHYSICAL AND MATHEMATICAL SCIENCES,

FROM THE EARLIEST AGES TO THE PRESENT TIMES.

INTRODUCTORY REMARKS.

Among a systematic collection of treatises on the different branches of human knowledge, and more particularly in that branch which relates to mathematical and physical truth, the object of those which are professedly scientific is to convey, in the order best suited to the purposes of elementary instruction, a connected view of the principles and results of each department of science. There is, however, another class of treatises of a more mixed character; in which the exactness of scientific discussion may be blended with the lighter character of the narrative style, and whose object is not to exhibit or explain the actual facts, reasonings, or conclusions, but rather to recite the history of that train of events and circumstances by which they were originally brought to light; and to record the labours and achievements of those distinguished individuals by whose abilities and exertions the discovery of new truths, and the new applications of those before known, were effected. To this class the ensuing volume is intended to belong.

The province of history has been usually restricted to the record of events connected with the revolutions of states and empires. It has hence commonly presented little else than a varied representation of intrigue and violence; the artifices of ambition and the calamities of war; the crimes and miseries whether of despotism or of anarchy. In these stirring scenes of the pageant the interest of the many is usually engrossed. The contemplation of the more quiet progress of civilisation, of the arts of life and of literature as connected with them, is comparatively devoid of excitement, and therefore far less generally attractive, even if these topics should sometimes find a place in the narrative of the judicious historian.

But in a more especial degree is this the case with respect to the progress of *abstract science.* Intimately as it is in reality connected with the advance of the arts, and, above all, with the intellectual improvement of mankind, its effects are remote and not easily traced; and the subjects of its enquiries bear an appearance of abstruseness which causes them to be but little generally studied or understood. Hence the history of science is hardly ever a matter of popular interest or attention. The common impression has even been unfavourable to physical science. Those who cultivate it have been regarded as a set of men isolated as it were from the rest of the world, and immersed in occupations with which the body of mankind feel no sympathy. Their speculations are imagined to be little applicable to any useful purposes, and often of doubtful or even dangerous tendency. Hence, the pursuits of science have not uncommonly been regarded with suspicion, dislike, or ridicule. And, upon the whole, it will not be matter of surprise, that to trace their progress in different ages should have been so little recognised as a legitimate portion of the historian's province.

An attempt, therefore, to supply this deficiency is certainly needed. And though much detailed information of the highest value on these points may be found in various works of a professedly scientific character, yet such a sketch as we here propose may not be without its use, if it should happily succeed in removing any of the prejudices or misconceptions just referred to, and in putting the department of scientific history in such a form as may be sufficiently attractive to engage the attention of the general reader. Nor need the want of profound scientific knowledge on the part of the reader be any insuperable obstacle to his deriving some pleasure from a perusal of the records of physical discovery. For such a history is by no means a mere barren record of dates, names, and inventions. In matters of philosophical research, so close is the dependence of one truth upon another, that the history of discovery very generally presents to us the history and order of the deduction of truth. The later inventions, generally speaking, cannot be rendered intelligible till we are acquainted with the earlier: thus, in recording the history we are often actually delivering the principles of science.

In the ensuing cursory outline of such a history, it will, however, be our anxious endeavour to avoid as much as possible all dry and abstruse investigation, and to introduce no more of the technicalities of science than may be absolutely necessary for rendering our statements intelligible. In doing this we shall generally find, that the view we have to take of the progress of invention will itself supply the means of explaining those technicalities, which we shall always endeavour to illustrate in the most familiar manner which the nature of the subject will admit.

In following out an extensive enquiry, it is always a matter of great convenience to find in our subject any grand points of division naturally presenting themselves. These constitute so many landmarks, as it were, on which the eye may rest, and which assist us materially in estimating our position and the progress we are making. In attempting to sketch the history of the physical and mathematical sciences from the earliest ages to the present, we shall find three principal divisions naturally suggested by well marked differences of character in the science of different periods; and these intimately connected with causes which influenced the whole condition of society in those periods. These chief divisions in our subject will be, —

I. The Progress of Science among the Ancients.
II. Its Condition from the Middle Ages till the Time of Newton.
III. Its Advance from the Discoveries of Newton to the present Day.

Under each of these great divisions we shall pursue as perspicuous a description of the physical knowledge of those times as our limits will allow, and the nature of entirely popular illustration will permit. We shall endeavour to direct the attention of our readers mainly to those leading researches and discoveries which gave a character to the science of the age, or were remarkable as opening the way to the yet more valuable inventions of after times; and as producing the most beneficial results on the improvement of the human species in knowledge and happiness. We are, at the same time, fully aware that there will occur many subjects of considerable importance which we shall be necessitated to pass over without such notice as they deserve; but which could not be adequately discussed without entering into details absolutely inconsistent with our plan.

PART I.

THE PROGRESS OF PHYSICAL AND MATHEMATICAL SCIENCE AMONG THE ANCIENTS.

The first period of scientific history which we here propose to examine is one of the highest interest in every point of view. It is in a great part associated with the records of those distinguished nations whose language, history, and institutions occupy so large a share of our earliest studies: it leads us to the contemplation of those primary rudiments of scientific truth which have afforded the basis on which the whole modern superstructure has been reared; whilst the manner in which those speculations were carried on, and the errors and extravagancies with which they were often mixed up, will supply us with many useful cautions as to the only safe way of pursuing philosophical truth.

We shall find this period naturally and conveniently subdivide itself into three portions: the first reaching from the earliest records to the foundation of the school of Alexandria, about B. C. 300; the second continuing thence to the decline of that school; the third embracing the state of science in the age of the Roman empire down to the period of its dissolution.

SECTION I.

THE PROGRESS OF SCIENCE FROM THE EARLIEST RECORDS TO THE FOUNDATION OF THE SCHOOL OF ALEXANDRIA, B. C. 300.

Origin of Science.

In attempting to trace the vestiges of early science, we find ourselves involved in no small degree of confusion and uncertainty, among conflicting claims and fictitious pretensions. The obscure terms again in which many of the statements of opinions, as well as facts, have been handed down to us, often leave us very much in doubt as to what were the real views or discoveries of some of the most eminent philosophers.

In the elucidation of these points the labours of the most able critics have been called forth; and though, on some such questions, considerable difference of opinion has prevailed, they have now been so fully discussed, that most of them may be considered as completely set at rest; and we can avail ourselves of the decisions of some late writers with the fullest confidence in the soundness of the reasons on which they are built. We shall therefore generally content ourselves with adopting that view of such questions, which, upon examining the authorities adduced, appears to us to possess the highest probability; always, however, stating both opinions where we feel them entitled to any thing like equal consideration.

It will assist the apprehension of the particular class of discoveries and inventions of which we here propose to treat, in the first instance, to distinguish clearly what properly fall under the designation of advances in philosophical truth. It is not every invention, however ingenious, which is properly referable to this title, nor even every process of art which may be traced to a philosophical principle; unless the knowledge of the principle really led to the practical application. The progress of the *arts* then will only be noticed so far as it was connected with that of science; so far as practical experience may have led to the suggestion of abstract principles, or mechanical contrivance may have resulted from philosophical reasoning.

In the earlier stages of society art must have preceded science. The immediate necessities of life first called forth the resources of invention, and it could not be till a much later period that the human mind had leisure to derive, from the objects of its daily notice, the elements of philosophical thought, or could be led to speculate on causes and effects. Accordingly, we are prepared to expect even considerable advances in practical arts long before we have any traces of speculative science. The Mosaic history refers to the very earliest epoch, the working of metals, the construction of musical instruments, and some advance in various arts of life. But we find no distinct allusions to any cultivation of science properly so called.

Early Astronomy.

Of all sciences astronomy is probably that which may lay claim to the earliest origin; but in speaking of the astronomy of remote ages we must understand the term in a very confined and limited sense. We must neither imagine speculations of that extent to which we are now accustomed to apply the name, nor observations conducted with the accuracy and regularity which we now consider essential to confer the character of scientific results. The astronomy of the earlier ages was probably confined to noting the most obvious phenomena of the motions and eclipses of the sun and moon; the rising and setting of the principal fixed stars; and the apparent positions of the planets among them: and the means of observing were no other than such as were supplied by the use of the naked eye, and some of the simplest and rudest instruments. The progress of the sun in his apparent orbit was followed by remarking the different well known stars, as at different parts of the year they were successively lost in the twilight, and also by the variation in the length of the shadow of a suitable object, or "gnomon," on a level horizontal plane, observed at the time of day when it was shortest. In order to recognise the fixed stars, the heavens were divided into constellations, or groups, in which, by the aid of a fertile imagination, some resemblance was traced out in the configuration to the shapes of the various objects whose names were assigned to them. They are alluded to by those distinctive names in the most ancient records we possess, the books of the Old Testament, and particularly in the book of Job; to which some critics have assigned a date even earlier than the time of Moses.*

The portion, or zone, of the heavens within whose limits the paths of the sun and planets were found to be confined, received the name of the Zodiac, and included twelve remarkable constellations, called signs, by a reference to which the positions of the movable bodies were determined. It is on all hands agreed that the origin of astronomical observation is to be traced to the East; but the particular people among whom it commenced, as well as the date, are lost in obscurity. It is most probable that its rise was simultaneous among various early nations, and its progress must have been so gradual that it would be impossible to fix any precise period for its commencement, even were our knowledge of remote antiquity far more complete than it is. Meanwhile, it is not an uninteresting topic of reflection to notice the probable circumstances which attended the early cultivation of these seiences. The fine climate and clear atmosphere of the oriental regions render the heavens an object of far more striking and attractive splendour than they appear to us. The habits of pastoral life were such as to lead the people of those regions to the nightly contemplation of the glorious spectacle thus placed before their view. The dullest apprehension could searcely fail to be impressed with admiration and curiosity, and to recognise some of the more obvious changes which would soon be found going on among the brilliant objects before them. When these changes were found closely connected with those of the seasons, other reasons of practical utility would mix themselves up with the spirit of contemplation; and the importance of celestial phenomena, in connection with the institutions of civil society, and with the labours of agriculture, would by degrees lead to more extended and precise observations. Other motives would not be wanting from the natural influence of those feelings of religious awe with which

* See Hales's Chronology, ii. 57.

those most glorious portions of the visible universe would be contemplated; and while their real nature was unknown, and no approach as yet made to an apprehension of the laws and causes of their motions, it was far from unnatural that that adoration which can properly originate only out of a perception of causation and design, and rise to the great origin of that design, should have been degraded to the character of a grovelling superstition, and have been transferred to the heavenly bodies themselves.

In immediate connection, too, with these last motives, was the application of a knowledge of the stars to the purposes of astrology. The desire to penetrate the future, one of the most deeply seated in ignorant minds, was really gratified as soon as astronomical observation had reached far enough to notice those periods called cycles, in which the celestial motions and configurations recurred. The prediction of the future appearances of the sky, and, above all, of eclipses, led the ignorant to expect, and the learned too frequently to keep up the expectation, that other events also might be predicted. Hence the combination of motives which tended to foster the cultivation of astronomy, and to impart to it a sacred character in the early periods of human society. The appearances of eclipses and of comets were regarded as prognostics of misfortune and public calamities. The conjunctions and configurations of the planets were considered as influencing the fortunes of states and individuals; and thus their motions came to be studied, though often with worthier motives, yet very generally with the view of being made subservient to the prediction of future events, and not unfrequently as giving to the initiated a command over the multitude, and as furnishing an engine of priestcraft. It is also far from improbable that in many cases pretences accordant with popular superstition were held out merely as an excuse and protection to the real enquirer after knowledge; whose pursuits, if openly avowed, might have met but with reproach and ridicule, or even with opposition and persecution, at the hands either of people or rulers, equally incapable of appreciating their value. If, however, some motive were assigned more level to the apprehensions of the vulgar, and especially one connected with their superstitions, this would at once secure their respect and enhance their reverence for the individual engaged with such lofty objects.

Several ancient nations have laid claim to considerable advances in astronomy at periods of antiquity far beyond reasonable probability. Thus the Chinese and the Indians each refer to a date of about 3000 years before the Christian era, as that to which their astronomical records reach, and the Chaldeans to more than 2000 years. But though these extravagant claims are unsupported by evidence, and at variance with all probability, we have still good grounds for considering the science to have made some progress among these nations upwards of 1000 years before our era.

The Chaldeans.

The antiquity of the Chaldean astronomy rests upon the authority of Porphyry, who says that Callisthenes transmitted to Aristotle a series of observations made at Babylon, during a period of 1903 years preceding the capture of that city by Alexander, or reaching back to about 2234 B. C. Ptolemy, however, who afterwards made great use of them, quotes none prior to 720 B. C. This certainly is not conclusive against the existence of any of earlier date, though it doubtless furnishes a strong presumption.

These early observations, though probably but of a rude description, yet possess a peculiar interest as having furnished the first materials for those comparisons of the state of the heavens at distant periods, by which the great progressive changes in our system have been brought to light.

The Chaldeans appear to have found, with considerable accuracy, the length of the remarkable cycle of $6585\frac{1}{3}$ days or about 18 years, in which the series of the moon's revolutions under the same conditions as compared with those of the sun come round again, so that very nearly the same series of eclipses will recur. By these means they were able to predict eclipses with tolerable accuracy. They observed, with great care, the motions of the planets *; and according to some testimonies (though contradicted by others), they would appear to have formed just notions of the nature of comets. Herodotus † ascribes to them the duodecimal division of the day, and the use of the gnomon.

The Chinese.

Our knowledge of the Chinese astronomy is entirely derived from the laborious researches of the Jesuit missionaries stationed in that country, and in particular from the works of father Gaubil. ‡ The Chinese records refer to the date of 2461 B. C., at which they assert a remarkable conjunction of five planets took place. This point has been much discussed; and it seems, on the whole, most probable, that it was a purely fictitious epoch obtained by reckoning backwards. They also mention a solar eclipse in the reign of the emperor Tchong Kang, for neglecting to predict which the two chief astronomers were put to death. To this, dates are assigned, varying between B. C. 2159 and 2128. By calculating backwards, on modern data, Gaubil and others find an eclipse in the last-mentioned year; but the data are so completely uncertain that little weight can be attached to the statement. No eclipses are mentioned for many centuries after this.

More importance may fairly belong to the recorded observation of the length of the shadow of a gnomon, compared at the summer and winter solstices, about B.C. 1100. The obliquity of the ecliptic deduced is found to agree very closely with what would result from the modern calculation of the diminution of the obliquity founded on the theory of gravitation: this

* Diod. Sic. ii. 8. † ii. 109.

‡ See History of Astronomy, Lib. Useful Knowledge, p. 2. *et seq.*

agreement is the more remarkable, as the diminution was unknown to Gaubil. Observations of about the same period, on the position of the winter solstice, compared with others of about the date B.C. 600, have been found, in like manner, to agree with deductions from the law of the precession of equinoxes. During the period B.C. 720—481 observations of many eclipses are recorded, several of which have been verified by calculating backwards; appearances of many comets also are described. The regulation of the calendar and the prediction of eclipses, were regarded in China as matters of national importance, and a mathematical tribunal was established for the superintendence of astronomy; but the obstinate attachment of the Chinese to whatever had become established extended itself to their astronomy, and repressed all improvement in science.

Such is the view of the Chinese astronomy, derived from authorities of considerable weight. A different opinion, however, has been maintained by Mr. Davis*, who has contended, from an elaborate examination, that nothing really original in astronomical science can be attributed to the Chinese, who, he conceives, were entirely ignorant of its objects and principles before its introduction among them by the Arabians in the middle ages.

On this one subject, he says, "That singular nation has deviated from its established prejudices and maxims against introducing what is foreign, — they have even adopted the errors of European astronomy;" for he discovered, in a Chinese book, the exact representation of the Ptolemaic system. He adds, "It is indeed impossible not to smile at the idea of attributing any science to a people whose learned books are filled with such trumpery as the diagrams of Fo-hi, and a hundred other puerilities of the same kind."

Mr. Davis offers several other proofs of the propensity of the Chinese to appropriate to themselves the

* Phil. Trans. 1823.

discoveries of other people. He also shows that they had no solar year, but merely employed a period of twelve lunar months of twenty-nine and thirty days alternately, with a triennial intercolation of a thirteenth month to make it correspond more nearly with the sun's course.

In other branches of science, the national vanity of the Chinese has led them to claim an equally early acquaintance with some of the most important facts. They have pretended to a knowledge of the mariner's compass, at a period beyond 1200 B. C., and have also laid claim to originating many of the fundamental theorems of geometry and even of trigonometry. From what has been discovered of their extreme want of honesty, and fondness for appropriating to themselves the inventions of others, but little credit is now generally attached to these claims.

The Indians.

Astronomical tables pretending to an enormous antiquity have been brought from India. The first were imported from Siam by M. de la Loubère in 1687; to which several other collections have since been added, particularly those from Tirvalore, Chrisnabouram, &c. These tables have afforded matter of great discussion. They refer to an epoch B. C. 3102; and their claim to this, as a real date, has been supported by M. Bailly* with great learning and ingenuity, whilst it has been held to be entirely fictitious by MM. Laplace†, Bentley, and Davis.‡ An ancient treatise on astronomy, called the Surya-Siddhanta, has been also the subject of close scrutiny; and it seems now admitted by those best qualified to judge, that it was customary with the Hindoos to take for an epoch a fictitious general conjunction, obtained by calculating backwards, with the respective mean motions attributed to the several planets. Laplace has assigned very probable reasons for believing that the

* Astronomie Indienne. † Syst. du Monde, lib. v.
‡ Asiatic Researches, ii.

tables are of comparatively modern date, not older, probably, than the age of Ptolemy (A. D. 150); though there is no doubt that the cultivation of astronomy is of far higher antiquity. The age of the Surya-Siddhanta has been placed by Mr. Bentley about B. C. 1000.

The Siamese tables assign certain cycles of the motions of the sun and moon, which give the lengths of the sidereal and tropical year as well as the lunar revolution, agreeing very closely with modern determinations. One of the early Indian astronomers, Aryabatta (whose date is uncertain), advocated the doctrine of the earth's rotation, as did also Bramah-Gupta at a much later period. They were certainly acquainted with some of the chief inequalities in the motions of the sun and moon. It has been a subject of controversy whether they borrowed from the astronomy of the Greeks. Sir W. Jones contended for the improbability of this idea from the known aversion of the Brahmins to foreigners in general, and the Greeks in particular. On the other hand, Mr. Colebrooke has cited from one of their writers an acknowledgment of the superiority of the Greeks in astronomy.

In the mathematical sciences there seems great probability that the Indians had attained considerable proficiency at a remote age. They appear to have been early in possession of the fundamental theorem of geometry, which we have as the 47th of Euclid's 1st book, though at what date is uncertain, possibly before the age of Pythagoras, who may have borrowed it from them. However this may be, they show great ingenuity in their methods of proving it, which they do upon principles rather analytical than geometrical. Indeed it appears, from the testimony of all who have enquired into Indian literature, that they were early conspicuous for their acquaintance with algebra; they have evinced particular skill in the solution of problems of the class called indeterminate; their astronomical tables prove that they were acquainted with the principal theorems of spherical trigonometry; and their tables of sines appear to be calculated by means of second differences.

All this certainly indicates a greater advance than was made by the western nations even at a much later period.*

The Egyptians.

The Egyptians seem to have obtained considerable celebrity for astronomical knowledge at a remote period. But little, if any thing, of their astronomy has been preserved; for Hipparchus and Ptolemy, though collecting ancient observations, give none from Egyptian astronomers, but have recourse to those of the Chaldeans. Diodorus Siculus informs us that the Egyptians were able to calculate eclipses; and other writers speak of the records of observed eclipses. They must have collected observations to assign the recurring period by which their predictions were framed. One of their early kings, Osymandyas (whose date is uncertain), is said to have constructed a large circle marked with the signs of the zodiac. Their religious solemnities were regulated by the lunar revolutions; while their civil year consisted of 365 days. They soon found that neither of these accorded with the true solar year: but without caring to obtain an agreement between the religious or civil periods of the year with the physical seasons, they contented themselves with remarking the recurrence of a period of 1461 such years at which the succession of months and festivals returned to the same seasons. This was called the Sothaic period; and the occurrence of one such epoch is mentioned in A. D. 139. The preceding was therefore in the year B. C. 1322. Some writers have even contended that there was one before this.

It appears from good authority that the Egyptians conceived the two inferior planets, Mercury and Venus, to revolve round the sun, accompanying him in his annual revolution round the earth.† The invention of

* See Prof. Playfair's Memoir on the Astronomy of the Brahmins, Edinb. Trans. 1792. ii.

† Macrobius, Comm. in Somn. i. 9.

the signs of the zodiac has been ascribed to them, though on very insufficient grounds. The existence at this day of representations of the zodiac in some of their temples is a fact from which we derive little certain information as to their astronomy. The representations are not easily explicable; and various conflicting theories have been suggested as to their meaning. According to Dio Cassius*, the Egyptians were the inventors of the period of seven days, distinguished by the names of the planets. This has been doubted; but it is, at all events, certain, that this period with corresponding names was in use among all the oriental nations, and even in India, at a very early period. It is remarkable that they all commenced their reckoning with the day dedicated to Saturn; whilst the Hebrews alone considered the following day as the first.

Of the physical knowledge of the Egyptians we have little information which can be relied on. Here, as in other instances, we find extravagant claims put forth, and as strenuously supported by one party as decried by another. One thing appears certain, that whatever knowledge existed in that country was confined to the priests, who employed a sacred language of symbols, utterly defying all attempts to decipher it except by the initiated; and which might undoubtedly be employed to preserve valuable truths and philosophical speculations; or equally well might have served to invest with all the veneration so freely accorded to whatever is hidden and mysterious the most absurd superstitions and puerile conceits.

It has been alleged that the origin of the ancient geometry is to be traced to Egypt; and that its theorems arose from the necessity of recurring to some principles of mensuration for fixing the boundaries of lands where all landmarks were obliterated by the periodical overflowing of the Nile. This account appears by no means probable in its circumstances; since it would seem very easy to invent landmarks which should obviate

* xxxvii. 18.

the difficulty; and, again, it is not readily apparent how the theorems of abstract geometry could have been rendered applicable to such a purpose.

The cultivation of geometry as a science unquestionably took its rise in the abstracted conceptions of philosophers, and not from any of its mechanical applications; these being not of a nature to occur to any one but as a result of principles which must have been first understood.

The Hebrews.

The Hebrews, from their early connection with Egypt, probably derived whatever science they possessed from that country; but this would appear never to have amounted to much: as a nation, they do not seem to have acquired any taste for the cultivation of physical knowledge. There may have been individual exceptions: Moses, we are told, was initiated into all the learning of Egypt (B.C. 1500); and at a later period their historians ascribe to king Solomon (B.C. 1020) a great proficiency in the study of natural history. [1 Kings, iv. 33.] Several of the writers in the Old Testament (as, indeed, we have before remarked) allude to the contemplation of the heavenly bodies, as well as to other phenomena of nature; but their records present no traces of philosophical speculation. From certain expressions occurring in them, we may, indeed, collect that the prevalent belief was in accordance with the theory of the quiescence of the earth and the motion of the sun; and the magnificent description of the origin of the world, with which the first book of Moses opens, has been regarded by some as the delivery of a system of cosmogony and geology; but (without going into extraneous questions) it seems to us extremely improbable that the representation was designed with any such object; indeed, we can never infer much in reference to matters of philosophy from passages occurring (as those in question do) in writings devoted to subjects of an entirely different nature, and whose object is almost exclusively religious.

The Greek Schools.

The origin of astronomy among the Greeks, not less than among other nations, is involved in obscurity and fable. The first construction of the sphere was ascribed to Chiron, B. C. 1300; and the constellations are mentioned by Homer and Hesiod, B. C. 950; but no really authentic records of the science carry us beyond the age of Thales and the philosophers of the Ionic school, about B. C. 600. It has been a point of dispute whether Thales did not derive his knowledge from the East, or from Egypt. We are told by Herodotus*, that he predicted a solar eclipse memorable in history from the effect it produced in separating the contending armies of the Lydians and Medes. The date of this eclipse has been controverted: Mr. Baily assigns B. C. 610. Though it seems unquestionable that Thales understood the cause of eclipses, yet it is equally clear that he must have had access to long series of previous observations to enable him to make such predictions, and consequently must have been conversant with the astronomy either of Egypt or Chaldæa. It appears probable that he committed none of his opinions to writing; this was done by his disciples Anaximander and Anaximenes. Thales made attempts to measure the apparent magnitude of the sun and moon; and appears to have introduced the use of the zodiac into Greece. He taught the sphericity of the earth, and the obliquity of the ecliptic. The doctrine of the earth's motion, and the true system of the planetary orbits, is supposed by some writers to have been held by him, or, at least, by Anaximander. It is generally admitted, however, to have been maintained somewhat later by Pythagoras.

This distinguished philosopher was born B. C. 590, at Samos. He travelled into Egypt and the East, pene-

* I. 74.

trating even into India. From those regions he borrowed his metaphysical doctrines, and probably much of his astronomical and mathematical knowledge. The Ionic school, subsequently to the age of some of its original ornaments, became devoted chiefly to the cultivation of moral philosophy. The Italian school, on the contrary, founded by Pythagoras, appears to have been more inclined to the study of nature and its laws. Indeed, none of the departments of human knowledge were excluded from the pursuits of either of these principal divisions of the Grecian sages, until the taste for metaphysical subtleties began to infect the Ionic school. This so completely engrossed the attention, by the substitution of its imaginary theories for the realities of nature, and the mistaking words for things, as to exclude all disposition to reason coolly and clearly on natural causes and effects. To Pythagoras, "philosophy" is indebted for the name it bears. His predecessors had been in the habit of calling themselves "wise;" he pretended only to the denomination of a "lover of wisdom."* He had studied under Pherecydas, a disciple of Pittachus; but to neither of these does it appear that he was indebted for any knowledge of mathematical or physical subjects. On his return from his travels in Egypt and the East, in the time of the last Tarquin, finding his native country, Samos, under the dominion of the tyrant Polycrates, he went as a voluntary exile to seek a tranquil retreat in Italy. At Croto (as we learn from Ovid) he studied and taught those sublimer views of the material universe into which an insight is only to be acquired by the rejection of artificial systems, and a free communing with nature in her own domains. Pythagoras † seems to have clearly understood what we now distinctively call the *solar* system of the world, and to have recognised the diurnal rotation as well as the annual revolution of the earth, the central position of the sun, and the revolutions

* σοφος, wise: φιλο-σοφος, lover of wisdom. From the former word came the term sophist, afterwards applied to the logical disputants of the Peripatetic school, and now usually employed in the sense of a *false* reasoner.

† Diog. Laert. lib. viii.

of the planets; to which he added a just idea of the nature of comets. All this, however, was communicated only to his disciples in private. It has also been supposed that he, or his immediate followers, taught the probable existence of other systems, of which the fixed stars were the suns. To these opinions, in which we recognise the real order of nature, the Pythagoreans certainly added many fanciful notions and extravagant speculations upon numerical combinations, to which they ascribed mystical virtues; these were supposed to create certain harmonies in the celestial orbits, by which the motions of the universe were regulated. It is fair, however, to observe, that the passages in the ancient writers in which these tenets are mentioned, are very obscure, and some opinions have been ascribed to them with which they were not fairly chargeable.

When, however, we observe that no substantial basis of facts or reasoning appears to have been alleged in support of their system of the world, it must be allowed that the Pythagoreans, though guided by some instinctive principle to the truth, yet had no real means of distinguishing it from error; and, that they conjectured rightly in one instance, afforded no security against the conjecturing erroneously in another. The music of the spheres was to them supported by exactly as good arguments as the motion of the earth.

On the same ground we need not wonder at the subsequent neglect and oblivion into which the Pythagorean system fell. The false theories of the Peripatetics were supported on equally good reasoning; and these having gained the ascendency, continued to maintain it, from the influence of a variety of causes.

The Pythagorean school, however, continued long to flourish, and was adorned by a succession of men eminent for the same sort of speculations both in astronomy and other branches of science. Philolaus, about B.C. 450, and Nicetas, bore a principal share in disseminating a knowledge of the solar system, which they were the first to promulgate openly to the world.

Among the speculations of these philosophers, there are one or two notions recorded which seem like visions of the system of gravitation. Pythagoras is said to have observed, that a musical string gives the same sound with another of twice the length, if the latter be straitened by four times the weight that straitens the former: so the gravity of a planet is four times that of another which is at twice the distance. This has been much commented upon by Gregory and Maclaurin.

We are informed by Diogenes Laertius*, that Anaxagoras maintained that the heavens are kept in their place by the rapidity of their revolution, and would fall down if that rapidity were to cease. [*Cœlum omne vehementi circuitu constare, alias remissione lapsurum.*]

Democritus held that the atoms would all, from their gravity, have long since united in the centre of the universe, if the universe were not infinite, so as to have no centre. We may infer that Lucretius † derives this notion from him.

Both Anaxagoras and Empedocles considered the moon to shine by reflected light; arguing justly from her phases; and regarding this as the reason why her light is faint, and unaccompanied by sensible heat. Some of the early philosophers attributed absolute coldness to the moon's rays; and chimerical as the notion may at first sight appear, yet modern discoveries have shown a real connection between clearness of the atmosphere, (which, of course, is accompanied with a greater brightness of the moon,) and the cold produced by the radiation of heat from the earth's surface at night, which is impeded by the presence of clouds.

Democritus supposed the dark spots on the moon to be occasioned by the shadows of inequalities in the surface; a singular anticipation of what is now revealed to us by the telescope. He also broached another bold and sublime speculation, not less fully confirmed by telescopic examination,—that the milky way is formed by clusters of minute stars.

* M. Anax. ii. 12.

† Lib. i. v. 983.

It is quite unknown how early the ancient astronomers had recognised five primary planets. But it is a proof of the assiduity of their observations, that they should have been able to distinguish a planet so little conspicuous as Saturn: and by what means they could ever have detected the existence of Mercury, is at this moment a matter of extreme difficulty to imagine.

The regulation of the Calendar was an object of much solicitude to the Greek astronomers. The difficulties with which they had to contend arose chiefly from their persevering attempts to produce some agreement in reckonings derived from the motions of the sun and of the moon: the month being determined by a lunar, and the year by a solar revolution, they soon found that the former was not contained any integral number of times in the latter; their object then was to find a number or period of years, at the end of which a compensation would be effected, and the beginning of the month and of the year again coincide. Their knowledge did not enable them to perceive all the difficulties of the subject. Such an exact compensation can never occur, owing to irregularities of which they were not aware; but it was very possible to find some tolerably exact method sufficient for their purposes: to this point the attention of several of their most eminent philosophers was accordingly directed.

An imperfect cycle of eight years, proposed by Cleostratus, was soon replaced by the very accurate one of Meton, consisting of 19 solar years, which contain 235 lunar revolutions almost exactly, the difference being only about two hours. This possessed so much practical simplicity and convenience, that it was publicly adopted by the Grecian states with great applause at the Olympic games. The first cycle began B.C. 432; and it is the same as that still retained in our Calendar under the name of the Golden Number. Calippus subsequently endeavoured to render the accordance more exact, but in so doing introduced an error of an opposite kind.

Eudoxus appears to have first broached a theory of the planetary motions, at variance with the Pythagorean, which he derived from Egypt: this was nearly the same as that subsequently adopted by the Peripatetics. He conceived the sun and each planet to be surrounded by solid spheres, whose different motions modified one another so as to produce the actual motion of the body: every new inequality required the introduction of a new sphere: this soon rendered the system extremely complicated.

Physical Sciences.

We have before hinted at some of the causes which might have contributed to the early and peculiar cultivation of astronomy. It would seem to be a principle in human nature that the attention is most powerfully attracted towards the remote, the splendid, and the mysterious, whilst things of ordinary character immediately before us, and in which we might seem more directly concerned, are passed by unheeded. Thus, while the early philosophers were watching the stars and speculating on the motions of the heavens, they comparatively neglected to examine the phenomena presented on the surface of the earth; and in the records of early science but a small space is occupied by the history of mechanical and physical discovery. Of this part of the subject, however, we must now proceed to a brief sketch.

Thales appears to have been acquainted at least with the attracting power of magnetism; he also noticed the excitation of electricity in amber by friction: he attributed to both a certain degree of animation, which he considered as the only original source of motion: he supposed water to be the first principle out of which all things were formed, and into which they are resolved. He appears to have been the first who appreciated the value of a serious examination of the phenomena of the natural world; though both he and his followers deviated widely from the path of sober enquiry in many of their speculations. In the Ionic school originated the notion of four elements *; and as Thales resolved every thing into water, so Anaximenes assumed air, and Heraclitus fire, as the first principle of all things, and the basis of their respective systems.

Among the disciples of Thales, we find that Anaximander paid some attention to meteorology, and had just notions of the cause of the winds as derived from the local rarefaction of the atmosphere by heat; but many of his other views were extremely fanciful.

Anaxagoras struck out some remarkable conjectures, in which we may perceive an anticipation of modern discoveries. He rejected the notion of four elements, and supposed an indefinite number of them. He conceived matter resolvable into ultimate atoms: an opinion carried further by Democritus and Leucippus; whose systems, connected with the Pythagorean idea of the mystical virtue of numbers, seem nearly to resemble the modern atomic theory. Indeed, the mysterious importance attached to numbers by Pythagoras has been interpreted by some as really designed to convey to the initiated a doctrine closely accordant with that of the combinations of all material elements in definite proportions. †

To Democritus the credit is certainly due of strongly insisting upon the appeal to sense and observation, as the only real way to the knowledge of nature; and he appears to have set the example himself, in making a more frequent use of experimental enquiry as the basis of all his reasonings than most of his contemporaries. None of his works, however, remain. He flourished about B. C. 400. Some have attributed to him the invention of the arch, but this has been much called in question by antiquarians. Democritus, as well as Archytas and Eudoxus, were followers of the Pythagorean school; and the two last-named philosophers were among the first who have had the merit of endeavouring to disse-

* Diog. Laert. viii 31.
† See Daubeny on the Atomic Theory.

minate knowledge, and to bring down philosophical truths to the level of popular apprehension. We have little evidence, indeed, to what extent they succeeded; but this more humble department of scientific labour is one which must always claim, among the well-wishers of the human race, scarcely less praise than that given to original discovery.

Plato, in the midst of his mystical speculations, occasionally exhibits some definite ideas on physical subjects. He points out the distinction of rarer from denser matter by its inertia; and though he seems to have had some notion of gravity, it does not appear how far he connected it with inertia.

The physical researches of Aristotle (about 350 B.C.) present an extraordinary mixture of sound and chimerical opinions. His vast and industrious collection of facts in natural history evinces the sober and patient enquirer; his mechanics contain something of the real application of mathematical reasoning; whilst his physical speculations display all the extravagance of gratuitous theorising and verbal dogmatism. He attributed absolute levity to fire, and gravity to earth; considering air and water as of an intermediate nature. He considered gravity to be a tendency to the centre of the earth, which he also regarded as the centre of the universe. He also introduced the celebrated principle of Nature's abhorrence of a vacuum.

Epicurus (B.C. 300) appears to have reasoned as justly respecting many particular subjects of natural philosophy, as he did absurdly respecting the origin of the world and of animated nature. He adopted in a great measure the principles of Democritus respecting atoms, but ascribed to them an innate power of affecting each other's motions, in such a manner as to constitute, by the diversity of their spontaneous arrangements, all the varieties of natural bodies. He considered both heat and cold as material: the heat emitted by the sun he thought not absolutely identical with light; and conjectured that some of the sun's rays might possess the power of heating bodies, and yet not affect the sense of vision. He explained magnetism, by supposing a current of atoms passing in certain directions through the magnet and the iron, which produced all the effects by interference with each other. Earthquakes and volcanos he derived from the violent explosions of imprisoned air.

The mechanical powers must have been known in their practical applications long before it had been considered the business of philosophy to investigate their theory. The lever and wedge must have occurred to the mind of the most untutored workman. The pulley and the screw are expressly ascribed to Archytas (B.C. 450). Amidst several erroneous deductions, the mechanics of Aristotle display some correct notions of the doctrine of forces. He has certainly laid down, at least in a general way, the principle of the composition of motion. Supposing two forces, in a given finite ratio to each other, to act upon a body in directions at right angles, he shows, that assuming straight lines in those directions to represent the intensities of the two forces by their lengths, on completing the rectangle, its diagonal will express the direction and quantity of the resulting motion: but the soundness of the reasoning by which he supports this may, perhaps, be open to question. It is also a remarkable circumstance, that he proceeds to consider the case where the two forces have, as he expressly calls it, "no ratio" to each other, and the motion takes place in "no time;" in which case, he concludes that *curvilinear* motion must result. We shall afterwards perceive how singular an approach he here seems to have made to one of the most abstruse and important principles of modern discovery. He held, that motion is caused by something in contact with the body moved; and was hence led to the opinion, that falling bodies are accelerated by the air through which they pass. He distinguished motions into natural and unnatural. Falling was a natural motion, as were the motions of the celestial bodies: hence, the latter continue undiminished, and the former are even accelerated. But the motion which we impress upon a body by pushing or throwing it is contrary to nature, and therefore speedily diminishes, and is destroyed. All matter seems perpetually ready to get rid of these unnatural motions, and to resume a state of congenial repose. His followers regarded these unnatural motions as acquired qualities, like heat or cold, and in the same way liable to be lost. Among his disciples, we ought not to omit the name of that sedulous enquirer into natural history, Theophrastus, who flourished about B. C. 320.

Of the nature and properties of light very little appears to have been known in the earlier times of ancient science. There were, indeed, some phenomena so obvious, that they must have been noticed in the rudest ages. The reflection of light from polished surfaces must have been one of the first natural appearances to which the attention was daily called. Every river and fountain afforded a mirror; and the effect was doubtless very early imitated by polished metallic surfaces.

The rectilinear course of the propagation of light could not but be soon an object of observation; and this circumstance seemed to bring its laws at once under the cognisance of the geometer. The equality of the angles of incidence and reflection was very early known. The phenomena of refraction were not investigated till a later period. That a straight rod partially immersed in water appeared bent, and that the refracted ray in a denser medium is bent towards the perpendicular to the surface, was nearly all that was known in this part of the subject for many ages.

Empedocles (about B.C. 450) was the first who treated systematically of optics. He held, that light consisted of particles projected from luminous bodies, yet that vision was not performed without the assistance of a certain influence, or emanation, transmitted *from* the eye to the object.

Epicurus held, that objects are seen by means of certain spectra, or simulacra, thrown off from the surfaces of bodies, and received into the eye.

Aristotle speculated on the subject in a metaphysical point of view, and called in question this doctrine of Empedocles; contending that light is not a material substance. He supported this by arguments drawn from its velocity, which he supposed infinite. He seems to have regarded it as something like an impulse propagated through a medium. But his whole doctrine was so mixed up with verbal mysticism, that it is difficult to determine whether this idea was really a sort of conjectural anticipation of the theory of undulations.

Mathematical Sciences.

In the review already made of the earlier astronomical and physical discoveries of the ancients, we have found scarcely any instances of sound investigation, without an accompanying admixture of extravagant and gratuitous hypothesis. In turning, however, to their inventions in pure mathematics, we find a pleasing contrast. In this department we recognise the sure, though very gradual, developement of the most important elementary truths, established with a perfection of logical accuracy in the reasoning which is, to this day, the theme of general approbation and the model of universal imitation. And, unless, indeed, the obscure mystical properties ascribed to numbers by Pythagoras be deemed an exception, we do not perceive these investigations ever degraded by a mixture with frivolous conceits or visionary speculations.

Those elementary truths, which necessarily form the foundation of the whole science of quantity in all its species and dimensions, we have already seen, have been claimed as the inventions of several early nations. It is highly probable that the discovery of them took place among different people without any communication. They have been handed down as the speculations of the earliest age; and such primary and simple relations are among those first subjects which would naturally exercise the skill of a contemplative mind devoting itself to the consideration of the different combinations to which geometrical figures and magnitudes can be subjected. It was, doubtless, long before the scattered truths were collected and arranged in any systematic form.

Pythagoras, no doubt, directed his attention to these subjects; and the story related of his sacrificing a hecatomb for joy on the discovery of the fundamental theorem of geometry, would hardly have been invented if he had not at least enjoyed the reputation of originality in the discovery.

The early Greek philosophers employed themselves in speculating upon all the relations they could discover in the simple geometrical figures, and in those constructions which involved only the use of the straight line and the circle: by degrees they extended their enquiries to the properties of planes and solids, especially the regular solids contained by plane sides, and those generated by the revolution of a circle, a triangle, or a rectangle, the sphere, the cone, and cylinder.

It was in the Platonic school, and, as some contend, by Plato himself, that some of the most valuable accessions to geometry were made. The science had as yet taken cognisance of no other curves than the circle. Plato perceived, that if a cone be cut by a plane in certain positions, the intersection of the surface of the cone with the plane will neither be a portion of a circle, nor rectilinear, but will take the form of certain peculiar curves. It was soon found that only three distinct species of these curves could be formed; they were termed accordingly the conic sections, and named (according to another analogy arising out of one of their properties) the parabola, the ellipse, and hyperbola. The manner in which the formation of the curves was first conceived, affords a striking instance of the slow progress of discovery among a class of truths as yet new to the apprehension. A plane was conceived touching a cone along one of its sides: another plane, perpendicular to this, of course cut the cone, and gave rise to the curve. If the cone had a right angle at its summit, the curve was a parabola: if the vertical angle was less than a right angle, an ellipse: if greater, an hyperbola. A different species of cone was thus supposed necessary to give each different curve. A century elapsed before it was seen that they might all be obtained from one and the same cone of any species, by merely altering the inclination of the cutting plane. Of the high importance of these curves in the researches of modern physical science, this is not the place to speak. We must here confine ourselves to noticing that the ancients happily attached due value to them, and continued with unremitting diligence to investigate their various properties. In these researches, none was more eminent than Menechme, the friend and disciple of Plato.

But the Platonic school was scarcely less distinguished for originating other important branches of mathematical speculation. Among these, the most remarkable, perhaps, was the geometrical analysis. This invention is expressly ascribed to Plato himself, by Proclus. Any geometrical question, whether problem or theorem, being submitted to analysis, is assumed as solved, or as true. From this assumption, a chain of consequences is drawn, which, by the ingenuity of the geometer, is continued, until he arrives at some proposition known to be true or false, possible or impossible. The final consequence points out whether the question be true or possible; and by retracing the steps, a synthetic proof or solution may be found.

Another class of speculations commenced and pursued in this school, was the geometric loci. These, in their simplest form, arose out of problems where it was required to find a point determined, for instance, by the intersection of two lines under certain given conditions, and where it was found that there were an infinite number of points fulfilling the requisition, but each restricted to a certain position: so that if they were all assigned, they would all lie in a certain line or *locus*, straight or curved. For example, the *locus* of the vertices of triangles of equal area on the same base, is a straight line parallel to the base; and the *locus* of the vertices of right-angled triangles on the same hypothenuse, is a semicircle. These loci were chiefly employed as affording the means of solving other problems of the determinate kind.

Of these, one which largely occupied the attention of the ancient geometers, and received its solution in the Platonic school, was a response of the oracle of Delos, requiring, that of the altar in that temple, which was an exact cube, the exact double in solid content should be made also in the form of a cube. This was done at first mechanically; but Menechme applied to the solution the resources of the method of loci, and produced a geometrical construction. The trisection of a circular arc was another problem of some celebrity, which, in like manner, was made to yield to the growing powers of geometry, though it had resisted all attempts by means of the elementary methods.

To the principles developed in these several discoveries, we shall, at a future period, have occasion to recur. We will merely remark, in this place, that they unquestionably contain the germs, as it were, of the most valuable inventions of modern times.

Upon the whole, the geometry of the ancient schools is that portion of their speculations to which we look back with by far the greatest interest and satisfaction; and to which we are really indebted for the removal of the difficulties which we should otherwise have to contend with, *in limine*, in all our enquiries. What were to them matters of high and original discovery, now form the necessary elements of all well-conducted education: and even though modern science had given us more direct and easy methods of arriving at those results which we want for their actual applications, yet, as a subject of abstract study, for the refined elegance of their train of deduction, for the exact taste of their style, and the fastidious precision of their reasoning, the ancient geometers will retain their pre-eminence as models to succeeding ages.

The invention of mechanical modes of construction, by which certain curves could be traced out and some problems solved, and which began to prevail with Eudoxus, Archytas, and their followers, was much censured by Plato, who considered such methods derogatory to the abstract philosophic dignity of geometry, and destructive to its purely intellectual character. This sentiment, to a certain degree perfectly just, was extensively adopted by the philosophers of the Platonic school, and produced the effect of alienating mechanical invention from mathematical speculation: a result highly injurious to the former science, and repressive of much of the spirit of invention and improvement in the latter. Indeed, it was, probably, to the wide distinction maintained in the ideas of many of the ancient philosophers, between the respective characters of geometrical and of physical investigation, that we may attribute much of the neglect of the latter, and the slowness of its progress, from the want not only of the powerful aid it might have derived from the former, but even of recognition as a legitimate branch of philosophy. Nothing, indeed, can be more striking, than the contrast afforded by the manner in which these two classes of enquiry were respectively carried on: and we can hardly help feeling astonished, that the same philosophical genius which was so rigorously precise in its demands for the perfect demonstration of the most primary notions on which mathematical truth was to rest, should have been ready to satisfy itself with the most flimsy conjectures or unsubstantial analogies, in matters of physical speculation.

SECTION II.

THE PROGRESS OF SCIENCE FROM THE ESTABLISHMENT OF THE SCHOOL OF ALEXANDRIA TO ITS DECLINE.

The School of Alexandria.

WE have now arrived at a memorable epoch in the history of science. When, at the death of Alexander, the division of his empire among his officers took place, Egypt fell to the share of Ptolemy Lagus, — a prince, whose love of learning and disposition to encourage it soon attracted to Alexandria, his capital, a number of learned men from Greece and other countries. His son, Ptolemy Philadelphus, was the inheritor of the taste and ability, as well as the throne, of his father; and soon evinced his zeal in carrying on what his predecessor had commenced. The same spirit, indeed, animated several of his successors on the throne. But the second Ptolemy gave the most convincing proof of his ardour in the cause of improvement, by the magnificent foundation of the schools of science, the observatory, and the library, which adorned his capital, and gave his sovereignty a pre-eminence in its best attribute,—that of promoting the good of the human species. The patronage so munificently bestowed was amply recompensed in the results it produced. A succession of philosophers of the most distinguished ability continued long to adorn this royal school; and the stimulus and encouragement thus given to abstract enquiry produced some of the most valuable accessions to philosophical knowledge, and laid deeply and permanently the foundations of astronomical, mechanical, and geometrical science. Above all, a spirit of enquiry was called forth; and the human mind taught and encouraged to exert its energies in the search after truth.

Geometry : — Euclid.

In no department are we under greater obligations to the school of Alexandria than in pure mathematics. One of the first of those eminent men who did so much honour to the wise liberality of its founder, was Euclid; pre-eminently known in every quarter of the globe, and in every age, from his own to the present, by his system of elementary geometry. We have few particulars of his life, and can only assign the period at which he flourished as that immediately succeeding the foundation of the school of Alexandria, or between B. C. 300 and 250. Our principal source of information respecting him, and his works, is from the commentary of Proclus on his Elements. There is no doubt that elements of geometry had been previously compiled by Hippocrates, Eudoxus, and others. Euclid's principal design would seem to have been that of correcting their errors, supplying their deficiencies, and uniting the series of elementary truths into a perfectly systematic chain of deduction. The extreme logical precision which is evinced in the style of demonstration, is equalled only by the admirable perspicuity and simplicity which, with very few exceptions, characterise every part of the work. But few treatises, probably, ever suffered more than this in the course of time, at the hands not only of mere transcribers, but of commentators, and pretended improvers. Hence it has been the constant object of modern editors to restore the text to its probable original character. Among such attempts, none has been more conspicuous, or more generally admitted as successful, than that of Dr. R. Simson. There still seems, however, to be something wanting to its complete perfection. A certain tinge of mysticism infects some of the definitions and axioms; and the primary defect in the theory of parallel lines still remains. And though various remedies have been proposed, none has as yet fulfilled the conditions of simplicity and conformity to the elementary style, even if admitted to be conclusive.

That notwithstanding these defects, and many objections which have been brought against it, and attempts which have been made to improve upon it, Euclid's treatise should still sustain its high reputation, is certainly no small praise. Its adoption as the text-book for elementary instruction is a different question. In England, it is still generally used; but on the Continent, other works more specifically designed for the student's initiation have superseded it. This, however, is independent altogether of its merits as a philosophical system. And in this point of view we are inclined to believe that its excellencies are hardly yet fully appreciated. The entire work has been usually edited as extending to fifteen books; but the two last are admitted not to be Euclid's: the Arabic translator expressly ascribes them to Hypsicles. It appears probable that the work we have is a combination of two or, perhaps, three treatises, originally distinct. Besides geometry, these books include the theory of arithmetic, and its application to geometry. The investigation of the philosophical principles of reasoning adopted by Euclid in the different parts of his writings is a subject of high interest, and one which deserves more attention than it has usually received, both with reference to a right apprehension of his excellencies, and to the questions which have arisen in modern times between the rival claims of geometry and algebra.

But we cannot here pursue questions of this kind. It will be more to our purpose to remark, that the boundaries of what have been distinctively considered *elementary* methods were reached by Euclid in his demonstration of the proportionality of the areas of circles to the squares, and of the contents of spheres to the cubes, of their diameters. The problem of squaring the circle, or assigning by geometrical methods the side of a square whose area is equal to that of a given circle, was one much celebrated among the ancients, and which has indirectly proved of incalculable advantage to science; since, in the unceasing and fruitless attempt to solve it, methods and principles were suggested, which have been of the most important use.

In order, however, to proceed satisfactorily in our account of these and several further investigations, we must premise some more general considerations.

The proper object of contemplation in geometry is *extension*, in its several species of length, area, and solidity. The course which the geometer has to pursue, is to consider, in the first instance, the different combinations of the simplest conceptions which produce the different species of extended figures : these will be lines, straight or curved, and the spaces enclosed between such lines. Extended surface, again, may be such as to lie all in one plane, or it may be variously curved or rounded : under such surfaces, again, will be contained variously shaped solids. The geometer, then, distinguishes all such cases of these as he can reduce to any sort of regularity in their structure or mode of formation. Whenever any such principle of formation has been assigned, he can proceed to the consequences which will result from the supposed method of construction, and thus establish the properties belonging to such figures. All this, it must be borne in mind, applies to figures, lines, and magnitudes, such as are the pure creatures of intellectual conception, and entirely abstracted from all gross physical ideas, derived from our notions of material substances. The line of the geometer is the abstract conception of the *length* of a thing, entirely discarding all reference to its breadth, thickness, or any other property whatsoever; and so, in like manner, the ideas of surface, of solidity, &c. And in a corresponding way, the deductions which he makes, and the method he pursues, for tracing their dimensions, and establishing their properties, are not like those of actual mechanical measurement or numerical computation, but the abstract and rigorous logical conclusions from the first definitions and assumptions. It is in this way that the high superiority of geometry as a school of close reasoning is displayed; and, if regarded solely as a refined and beautiful field of philosophical speculation, it may fairly justify all the encomiums which have been bestowed upon it.

One of the main points of enquiry in elementary geometry, is that which refers to the comparison of the areas of plane figures: so long as the figures are contained by rectilinear sides, this presents no material difficulty. Triangles being the simplest of all rectilinear figures, the comparison of their areas was the first step; and the conception of equality of area is ultimately reducible to that of two figures, which, superposed, shall coincide in every part. From this elementary principle, by various trains of deduction, Euclid has led the way to the comparison of the areas of rectilinear figures (which may all be conceived as made up of triangles), and established the conditions under which such figures, differently constructed, can be shown to be equal. He has introduced into his train of deduction, as a point of fundamental importance, and established by a singularly beautiful specimen of geometrical proof, the theorem before referred to as the invention of Pythagoras; that the squares upon the sides of every right-angled triangle are together equal in area to the square upon its hypothenuse, and the same truth is extended to any similar figures. The principle of superposition may be traced through the whole of this refined deduction: and the student may amuse himself by the actual mechanical dissection and recombination of the parts of these areas, which, by a very easy process, will be found actually to cover the same space. Speculations of this kind were much pursued at a later period by the Persian mathematicians; and while upon the subject, we may also notice the numerical application of the same truth. If the sides of a right-angled triangle be respectively 3, 4, and 5 inches, feet, &c., it is immediately obvious that the above truth holds good with respect to their numerical squares. The same will of course be true of any given multiples of those numbers; but other numbers may be found to which the same property applies; — for example 9, 12, and 15, or 6, 8, and 10. Plato is recorded to have perceived a general rule by which such a relation in whole numbers may be expressed. If N be any number, the three sides will be respectively twice N, the square of N diminished by unity, and the square of N increased by unity. If we draw a triangle whose sides are measured by these numbers, we know at once that it is right-angled: this principle is practically applied by common workmen in laying out any lines which they wish to make at right angles; as, for example, the foundation of a building.

This, however, is a digression. The comparison of rectilinear areas is an easy matter: not so the case where we wish to compare a rectilinear with a curvilinear space, or to express the latter by means of the former; which is the only way in which we can express it in definite terms so as to make it the subject of computation.

In one very simple case this was effected by Hippocrates. By means of an extension of the fundamental theorem before spoken of, he showed that the semicircles described on the sides are together equal in area to the semicircle on the hypothenuse of a right-angled triangle. But this last semicircle will (by another property) always pass through the point of the right angle: hence it cuts off from the other two, certain crescent-shaped portions called *lunulæ*. Deducting, therefore, the spaces common to both, it follows that these *two lunulæ* are together equal to the area of the *triangle*. This singularly elegant investigation is, however, no further applicable, nor does it lead in any degree to a principle of similar investigations for other cases.

What are termed strictly the methods of *elementary* geometry may perhaps be characterised by the application in some deduction (however remote) of the principle of exact *superposition*. This principle, then, in any of its forms, is inadequate to the task of assigning generally the areas of curvilinear figures; and the science requires here to be reinforced with some higher resources.

Such were found in what was termed the method of *exhaustion*, and the general principle of *limits*. These methods are, indeed, of far more extensive application, as we shall see at a future stage of our enquiries; for the present it will suffice to observe, that by the term exhaustion is meant that sort of process in demonstration by which it is shown that we have taken *every* conceivable case which the particular subject in hand admits; and having demonstrated that the point to be established is *not* included in any of them except one, we infer that it must be found in that one. This of course presumes that it must be found in some of them, or that we have previously established the truth of the alternative. For example, if it be shown that one thing can neither be greater nor less than another, we infer that it is equal to it. This would be a simple instance of the method of exhaustion. It is, doubtless, an indirect mode of proof, analogous to the *reductio ad absurdum*, so frequently used in geometry; but which is only admissible where direct proof cannot be attained. The main point usually lies in establishing, as we have said, the *alternative*; in assuring ourselves that the relation sought must really exist among some of the possible cases. Now, this part of the process is precisely that of establishing what is called a *limit*. No term in science, perhaps, has been more mystified by technical refinements than this; and yet, perhaps, there is hardly any conception more simple when it is but simply stated. We cannot illustrate it better than by proceeding to the very case considered by the ancient mathematicians.

When a regular polygon (a hexagon, for example) is inscribed in a circle, every one sees that it occupies a less area, and is contained by a less perimeter, than the circle. If we now convert it into a figure of 12 sides, this will evidently approach nearer to the circle in both respects; still more, if we again double the number of sides: and in this way we may go on till we suppose a polygon of a number of sides as great as the imagination can conceive; it will still be less than the circle; it never can become equal to the circle without becoming actually a circle, and ceasing to be a rectilinear polygon. The circle, then, is the *limit* to which the polygon cannot reach, however we continue to increase its number of sides and diminish their individual lengths. Just in the same way, if we take the case of *circumscribed* polygons: the circle is likewise their *limit*. Its area is a limit to their areas, and its circumference the limit of their perimeters.

Euclid, having shown by elementary methods that the areas of all similar rectilinear figures are as the squares of their diagonals, extended this truth to the case of the inscribed and circumscribed polygons, of whatever number of sides ; and therefore, by the above principle, to the curvilinear areas of the circles which are their *limits*, their diagonals being the diameters of the circles. The process of verifying the limit, or demonstrating in each case that the curvilinear area was really the boundary, was that which, when followed out in all its detail, caused the principal inconvenience in this method, from the excessive length and tediousness which it most frequently involved.

Geometry: — Archimedes.

These methods, of which we have here endeavoured to illustrate the nature by referring to a simple instance of their use, were soon applied much more extensively. In the earliest period of the Alexandrian school, we find one of the most powerful intellects the world ever produced, shedding a new and brilliant illumination over nearly the whole field of physical and mathematical truth. Archimedes was born at Syracuse about B.C. 287. In the school of Euclid he laid the foundations of his geometrical attainments, and soon obtained a distinguished reputation in that science. Returning to his native place, that city became the asylum of his studies, and afterwards the theatre of his practical mechanical triumphs. Among the various subjects to which he applied his gigantic powers of investigation, the questions of geometry were among the first; especially those for the solution of which the invention of the principle of limits had prepared the way. His attention was readily engaged with the beauty and fertility of the method of exhaustion, and he proceeded to apply it to the investigation of a variety of new subjects, and the solution of problems as yet unattempted. Some of the most considerable of these we will endeavour briefly to explain; and in doing so we shall find the principle still further elucidated. Dry and abstracted as the speculations to which it is here applied may appear to be, yet it is by the extension of the same principle that, in modern times, the empire of the human mind over matter has been established, our extended insight into the system of the universe attained, and, by consequence, the high elevation of man in the scale of intellectual beings mainly effected.

Directing his attention to the many singular properties and analogies of those remarkable curves already alluded to, which are formed by the sections of a cone, Archimedes perceived a beautiful application of the method of exhaustions to assigning the area of any portion of a parabola. This, however, involved a new kind of limit, viz. the same notion of a boundary, as it were, applied to numerical quantity. Such a limit may be assigned to the continual addition of a series of numbers in geometrical progression, when the common multiplier is a fraction. It is very easy to see that if we take, for instance, a geometrical series, 1, $\frac{1}{4}$, $\frac{1}{16}$, &c., where the common multiplier is $\frac{1}{4}$, the series will go on indefinitely: but if we take the sum of any number of terms, we shall find that every new term added will add less and less to its amount; and there is a *limit* beyond which no continuation of the process can carry us, but to which we may approach as near as we please, and this limit will be the number $\frac{4}{3}$.

Now, in the parabola, if a triangle be inscribed; and in each of the curvilinear segments which it leaves, other small triangles in a similar way; and in the remaining segments, others again in like manner, and so on continually; it may be demonstrated by the known properties of the curve, that the areas of the original triangle, and of each of the sets of small triangles so formed at each successive operation, will constitute a series in geometrical progression, 1, $\frac{1}{4}$, $\frac{1}{16}$, &c. The limit, therefore, of the sum, that is, of the whole rectilinear area, is $\frac{4}{3}$ of the original triangle. But the limit of the sum of all the inscribed triangles is also shown to be the curvilinear area of the parabola. Hence the curvilinear area is exactly and rigidly equal to $\frac{4}{3}$ of the inscribed triangle, or, as easily follows, $\frac{2}{3}$ of the circumscribed rectangle.

By processes similar to this, the same distinguished geometer proceeded to investigate a vast number of other properties and relations of geometrical figures. One of the most celebrated, perhaps, is his demonstration of the singular relation which subsists between the surfaces and the solid contents of the sphere inscribed in an equilateral cone, and the cylinder described about the sphere. Both the surfaces and the contents form geometrical progressions, and with the same common ratio, $\frac{2}{3}$. He was so struck with this discovery, as to order it to be inscribed on his tomb.

By the further application of these methods, he advanced so far in the question of the circular area, as to show that if the circumference of a circle be conceived extended into a straight line, the area will be equal to a right-angled triangle, whose base is the radius and altitude the circumference: it would consequently be expressed by taking half the rectangle, or product of the radius, into the circumference. The quadrature of the circle, therefore, is reduced to finding the length of its circumference in rectilinear measure. By an extensive comparison of polygons, he proved that the ratio of the circumference to the diameter must be less than $3\frac{10}{70}$ to 1 and greater than $3\frac{10}{71}$ to 1.

Conon, the friend of Archimedes, suggested the idea, which the latter developed, of the spiral, which still goes by his name. A point being supposed to move uniformly towards the centre of a circle along its radius, while the radius itself revolves uniformly, describes in its course by these combined motions the spiral in question. But it would be in vain to go on enumerating the different mathematical topics which engaged the attention and displayed the pre-eminent talents of this distinguished man. What we have said may suffice to illustrate the general nature and character of his researches, and will prepare us to apprehend more readily, at a future stage of our review, how far he paved the way for the more extended discoveries of his modern successors.

Mechanical Science.

We have already remarked, that of the science of motion very little was known to the Greek philosophers: those speculations upon it in which they did indulge, were of an entirely abstract nature, and so mixed up with metaphysical notions, that very few real deductions applicable to any thing in nature were, or could be by such methods, brought out. There was, however, another branch of the subject in which their endeavours were attended with better success, and which began to be cultivated by Archimedes. This was the science of mechanics as referring to the action of forces in equilibrio, and producing, not motion, but rest; the doctrine of Statics, as it has been called in modern times, as contradistinguished from Dynamics, which treats of bodies in motion. This subject might be, and, as we shall see, was clearly understood, though the laws of motion were as yet unknown. The doctrine of equilibrium was treated by Archimedes in a way eminently evincing those masterly abilities which have placed him by common consent at the head of all the mathematical philosophers of antiquity. He establishes it on principles so clear and satisfactory, that to this day scarcely any thing has been added to the evidence and simplicity of the reasonings he has advanced. His fundamental point is the property of the lever: the demonstration of this is made to rest ultimately on the truth, that equal bodies at the ends of the equal arms of a rod, supported on its middle point, will balance each other; or, what is nearly the same thing, that a regular cylinder of uniform matter will balance on the middle point of its length. These principles, in whatever light we view them, may be considered as not admitting any ulterior proof. Proceeding, then, upon these, as indisputable, if not self-evident, he pursues his proof with singular ingenuity to the conclusion, that bodies will be in equilibrio when their distances from the fulcrum or point of support are inversely as their weight. He advances from this to the kindred subject of the centre of gravity; establishes several propositions relating to it; and finds its position in many differently shaped bodies. It would be incompatible with our limits to enter into further details of his investigation; and it will be superfluous to do more than allude to the practical applications of mechanical skill which he displayed in the renowned engines constructed for the defence of Syracuse. The mirrors with which he is said to have burned the Roman fleet, have excited much discussion. The possibility of making such powerful specula, especially by a combination of small plane mirrors, forming a surface of many faces, approaching to a curved concave form, has been shown by Buffon. The extent to which the effect could be applied under the circumstances of the case, is the part of the subject most open to question; the distance at which the vessels were, and their remaining sufficiently stationary, are doubtful points. The philosopher may have produced an effect in one instance, under a favourable combination of circumstances, which sufficiently terrified the enemy to prevent their approaching again.

Though we have no record of any labours of Archimedes in the actual field of astronomical observations, yet he certainly directed his attention to the subject. He is said to have constructed a species of planetarium; and adopting the views of Aristarchus respecting the system of the world, he took the distance of the sun, as estimated by that astronomer, for the basis of a calculation of the number of grains of sand which would be contained in the whole sphere of the earth's orbit. The treatise in which this investigation is made, is, from this circumstance, entitled "De Arenario;" and was designed as an illustration of the powers of arithmetic, and of the utility of a decimal system of notation.

To the comprehensive genius of Archimedes we owe the original discovery of the principal laws of hydrostatics; though, possibly, a few detached facts had been noticed by Aristotle and his predecessors, yet, certainly, no general or accurate examination of the subject had been made previously to the time of the philosopher of Syracuse. He unquestionably established the general law, that a solid body, when immersed in a liquid, loses a portion of its weight equal to that of the liquid it displaces. It has been remarked, in the "Introductory Essay on the Study of Nat. Philosophy" (p. 232.), how singular an instance of the force of prejudice is here afforded, in the circumstance, that it should not have been seen that the weight lost is only counteracted by the upward pressure of the liquid; and in the adherence to the dogma that "liquids do not gravitate in their natural place." Archimedes, however, if impeded in the extent to which he might have gone by such notions, did not mix up these speculations with his researches. He was exceedingly struck with the discovery of the principle referred to, which is said to have occurred to him from observing the water rise in a bath on the immersion of his body: in the ecstasy of the moment he ran out without stopping to clothe himself, exclaiming, Ευρηκα (I have discovered it). The story of Hiero's crown, connected with this, has been so variously repeated, that it is almost impossible to decide what the real circumstances were, or by what means the determination was actually made. Thus much is quite clear; two masses of equal weight, but of different density or specific gravity, being successively immersed in water, the less dense being the larger mass, will displace a larger body of water. If, then, the adulterated crown contained some metal much less dense than gold, it would displace a greater mass of water than one of the same weight of pure gold. But if the difference of density was small, it would require an extremely accurate measurement of the water displaced, to determine the question.

His treatise on floating bodies rests upon mathematical principles which are still the foundation of the science: he assumes as a postulate, that in water, the parts which are less pressed, are always ready to yield in any direction to those that are more pressed; and from this, by the application of geometrical reasoning, he derives the whole theory of floating bodies, of different geometrical forms. He was unquestionably the first who made any considerable application of mathematics as an instrument of physical research; and no individual, perhaps, ever laid the foundation of more discoveries. The ingenious and simple pump, which consists of a pipe twisted in the form of a corkscrew; and, being held in an inclined position, with one end immersed in the water, is made to revolve about its axis, by which means the water falls into the successive turns of the screw, and ultimately runs out at the top; was the invention of this eminent philosopher, and is at this day known by the name of Archimedes's screw. Upon the whole, we must entirely agree in the praise bestowed upon him by Dr. Wallis:—"Vir stupendæ sagacitatis, qui prima fundamenta posuit inventionum fere omnium in quibus promovendis ætas nostra gloriatur." (A man of wonderful sagacity, who laid the foundations of nearly all those inventions, the further prosecution of which is the boast of our age.)

The death of the philosopher took place, as is well known, by the hands of a soldier who did not know him, at the taking of Syracuse, though the Roman general, Marcellus, had given orders, and even offered a reward, for saving his life, B. C. 212.

Apollonius Pergæus.

Among the eminent mathematicians who adorned the school of Alexandria, none was more celebrated than Apollonius of Perga; a disciple of Euclid, and nearly contemporary with Archimedes. He was distinguished among the writers of the subsequent age by the title of *the great geometer*. He certainly ranks next to Archimedes; but, according to Pappus, his high intellectual endowments were considerably tarnished by arrogance and envy, and a disposition to depreciate the merits and decry the originality of others.

His most celebrated work, perhaps, is that on the conic sections. Taking up the subject where his predecessors of the school of Plato had left it, he would appear to have been the first to perceive what we have before referred to, the formation of all these curves by differently inclined sections of one and the same cone. To us, indeed, this appears sufficiently obvious; but when any new discovery is presented by its inventor under a peculiar aspect, it may be long before it comes to be contemplated in any more general point of view. Apollonius, however, proceeded to the discussion of a vast variety of properties and analogies of these curves. Having deduced them, in the first instance, from the cone, he proceeded to establish those properties by which we are enabled to conceive the curves as described in plano. It is worthy of notice, that in doing this he does not consider, as of fundamental importance in his system, those remarkable points situated within these curves, called their foci; he alludes to them, indeed, but not by that express name. These, however, are, to our apprehension, mixed up with the most primary notion of these curves as described in plano. We fasten a string by its two ends to two pegs fixed in a plane surface, and keeping the string stretched, according to the greater or less length allowed between the pegs, we trace out, by the point at which it is kept extended, an *ellipse* of greater or less rotundity. If the two pegs be brought very close (the length of string remaining the same), the ellipse approaches very nearly to a circle; and will become one, if the pegs coincide, or form *one* centre: in the other extreme it will become very long and narrow, and at last differ little from a straight line. The two pegs are situated at the foci. If one of the pegs be removed to a distance extremely great (the string being lengthened accordingly), the ellipse will approach very nearly to a parabola; and will become one, if the distance be infinite, or if the longer branch of the string be made to move parallel to itself. As the ellipse is thus constructed by two revolving radii, whose sum is a constant quantity at all points in the curve, so the hyperbola requires a construction a little more complex, in which we have a constant difference. But all these constructions may be rendered evident at once by the following very simple illustration:—Provide three sheets of transparent paper: cover one of them with parallel straight lines ruled at equal distances, say $\frac{1}{4}$ of an inch apart; on each of the other two sheets describe a series of concentric circles at exactly the same distances apart as the parallel lines. This being prepared, it is only necessary to lay one of the sets of circles upon the other, in any way: if the circles coincide, we of course see nothing but one set of circles; if not, the two sets of circles will intersect one another in an unlimited number of points; and placing another transparent paper upon both, we may easily trace out upon it any one set of the points of intersection. These being joined, will give various curves. Those lying towards the part between the two centres will be ellipses, of which those centres are the two foci; the curves being the intersections of two radii whose *sum* is always a constant quantity. The curves lying outwards from the foci will be hyperbolas, generated by the intersections of two radii which always *differ* by a constant quantity. If, now, for one of the sets of circles, we substitute the parallel lines, the curves will be parabolas ; the straight lines being, as it were, portions of circles whose radii are infinitely great. Or here the intersection is that of two radii, one of which is infinite, and always in a position parallel to itself ; either the sum, or the difference here, may be taken as a constant quantity ; and the parabola is, in every sense, the limit between the ellipse and hyperbola.

The section formed by a plane passing through the vertex of the cone, is obviously two straight lines meeting at that point. Any section parallel to this is an hyperbola, and hyperbolas will be formed by all such planes up to the position of coincidence with the vertical section : this rectilinear section is, therefore, the limit of all the hyperbolas. Again, there is no difficulty in conceiving any of these hyperbolas referred to, or projected upon, the plane of the vertical section : and in this case it is easy to show, that the rectilinear section and the hyperbola (which falls within it) being both supposed, along with the cone, to be prolonged indefinitely, the hyperbola is constantly approaching nearer to the rectilinear section ; and at no distance, however great, does it meet it : this straight line is hence the *limit*, again, to the direction of the branch of the hyperbola, and is called its *asymptote.* It will be easily apparent that nothing of this kind can happen with the parabola, and still less with the ellipse. This curious subject was investigated by Apollonius.

There is no limit, however, to the various speculations of this kind into which the fertile analogies and relations of these curves lead us. We will merely add one more ; that as in the circle the rectangle of the abscissæ is *equal* to the square of the ordinate, so in these other sections of the cone it is equal to it when again multiplied in a certain constant ratio ; and as in the parabola one abscissa is infinite, the variable part is here reduced to a single term, multiplied by a constant quantity. This gives rise to the equations of the curves. From that of the ellipse is deducible, by no very long process, the construction of the curve by means of the trammel, which is a straight bar, carrying two pegs, which move in two grooves at right angles: any point in the bar traces out a quadrant of an ellipse. If the two pegs are placed close together, the ellipse approaches the form of a circle. All the curves may be seen in the forms assumed by the shadow of any circular object against a plane surface variously inclined.

The conic sections of Apollonius consist of eight books: the first four are known in the original Greek, the eighth has been lost, and the other three we have through an Arabic version. Halley attempted a restoration of the lost book. The four last books contain the more original portions of the work. Among the author's inventions are the first notion of osculating circles and evolutes, and several investigations relating to maxima and minima. Of the former a notion is easily formed, if we conceive a portion of any curve to which lines are drawn, so that they are perpendicular, at every successive point, to the tangent at that point, or (what is the same thing) to the curve at the point of contact. These perpendiculars, or "normals," as they are called, will, of course, when produced, intersect with one another. If all the points of their intersections be marked and joined, there will result a certain peculiar curve which may be traced out; dependent evidently upon the nature of the original curve. This is called the *evolute* of the original curve; and there will be little difficulty in perceiving, that if this evolute be supposed formed, and a string wound upon it, the end of the string, as it is unwound, will trace out the original curve, which relatively to the other is called its *involute*.

The point where two consecutive normals, very near together, intersect, being taken as a centre, a circle described at the distance of the original curve is called its circle of curvature for that point, or osculating circle.

Of maxima and minima the general conception is still more simple. If from any point a number of lines be drawn to a given straight line, one of these will be shorter than all the rest, or the *minimum:* this will be the perpendicular. Of all lines cutting a circle the diameter is the *maximum.* These are obvious cases; but others of greater complexity soon came to be investigated.

The subject of the geometric loci, as we have already observed, had been cultivated in the Platonic school. Pappus records, with high commendation, a work on this subject, by Aristæus, the pupil of Plato and the teacher of Euclid. Neither this work, nor a treatise by the same author on conic sections, has come down to us: but Apollonius took up the former subject as well as the latter; and his work, "De Locis Planis," is a production of extensive learning and ingenuity, comprising a remarkable collection of curious properties of the circle and straight line. This work, especially as restored in its imperfect parts by Dr. R. Simson, is an admirable specimen of the style of the ancient geometry. Another production of Apollonius, entitled "Sectio Rationis," is an elementary treatise on the geometrical analysis; in which, pursuing the methods commenced in the data of Euclid, he enters at large into the subject, and delivers many of those refined and beautiful methods by which these favourite speculations of the Greek geometers were carried on with such distinguished elegance and success.

Nicomedes.

Nicomedes (about 200 B.C.) is well known as one of those geometers who engaged in the solution of the celebrated problems of the duplication of the cube and the trisection of an angle: for these solutions he employed the method of loci, and invented a peculiar geometric curve, by means of which constructions are easily made for solving these problems. The curve was named the *Conchoid*, from a resemblance, in its form, to the outline of a shell. The mechanical description of it which he devised, is, perhaps, from the several combined motions exhibited, one of the most remarkable of this class of mechanical constructions. Upon a ruler in the form of a T, another straight ruler is moved, so that a fixed point in it moves along the horizontal line of the T, whilst the ruler always passes through a fixed point in the stem of the T. If, then, any point in this movable ruler be taken as the tracing point, it will be found to describe a species of curve differing according to the position assumed for the tracing point, and exhibiting all the varieties of conchoids, which are very remarkable. No other monument of the genius of Nicomedes has come down to us. This construction is certainly no more than merely an elegant speculation; yet it may afford materials of contemplation to those who feel an interest in tracing the singular results which often arise from what are apparently the most simple combinations of geometrical conditions. It is a sort of paradox, that every point in a straight ruler is thus describing, at the same time, a distinct curvilinear path of widely different form. If it answer no other purpose, it may be useful in exciting curiosity and stimulating enquiry.

Astronomy:—Aristarchus, Eratosthenes.

In the school of Alexandria astronomy seemed to acquire new life. In this magnificent observatory, for the first time, a regular and systematic course of observation seems to have been kept up, with the view of accurately determining those primary facts which are the only real basis of the science. This important and laborious work was begun by Aristillus and Timocharis, and was principally confined to those fundamental elements, the places of the fixed stars: to these succeeded Aristarchus, B. C. 281.

To the diligence of an observer, Aristarchus added the spirit of a philosophical enquirer into some of the actual relations of the system of the world. He advocated the Pythagorean or solar system. One of the most serious objections brought against it was, that if the earth were in motion, a fixed star seen from one point in the earth's orbit would be referred by us to a point in the heavens different from that to which it would be referred when we are at the opposite point, but that, in fact, no such difference is observed. The reply of Aristarchus evinced a correct conception of the magnitude of the celestial spaces: he alleged that the whole orbit of the earth is a mere point in comparison with the distance of the fixed stars. This would, of course, render such difference in apparent position (called parallax) so small as to be quite insensible to the nicest observations.

He also suggested an ingenious mode of obtaining the *relative* distances of the sun and moon from the earth. When the moon is exactly half way between new and full, it requires but a moment's consideration to perceive, that the three bodies form a triangle which has a *right* angle at the moon. In this case, therefore, if we measure the *angle* subtended between the moon and the sun, the ratio of their distances from the earth is simply (in modern language) that of the cosine of that angle to radius. His determination, though but roughly obtained, served to give much more accurate notions of these distances than had as yet been entertained. He also endeavoured to estimate the magnitudes of the two luminaries.

The name of Eratosthenes, another Alexandrian astronomer, has been rendered ever memorable from his attempt, the first ever made, to estimate the actual magnitude of the globe on which we live. That the figure of the earth was of a spherical kind had been long before held in the schools of Greece; and there were so many obvious arguments in favour of the belief, that it must have become evident as soon as men began to reason at all on the subject. We have no ground to suppose that any actual attempt was made to measure the earth's magnitude before the time of Eratosthenes. Aristotle, indeed, mentions that mathematicians had assigned the circumference of the earth as 40,000 stadia. But no grounds for this are assigned; and it seems that various conjectural ideas on the subject were prevalent. The principle on which Eratosthenes proceeded was the very same which has been adopted by the modern astronomers, and in theory is perfectly exact and satisfactory. Observations of the meridian altitude of the heavenly bodies made at two stations under the same meridian will give the difference of latitude of those stations. If, then, the distance between them be actually measured, we shall obviously have the length of a degree of latitude in terms of the measure employed; and thence the length of the whole circumference of the globe. Hence, again, we can calculate (to any degree of approximation) its diameter, and again its mass or solid content.

When, however, Eratosthenes proceeded to reduce this idea to practice, so loose were the determinations of the data (contenting himself with a mere guess at the distance of the two stations), that the result would be of no value even were it not lost to us from our ignorance of the length of the stadium by which he reckons. His observations were made by means of the shadow of a gnomon; and by this method he has also recorded observations of the solstices, which agree remarkably well with what the values should have been at that date upon the modern theory of the diminution of obliquity according to the principles of gravitation. He died B. C. 194.

Hipparchus.

Hipparchus, perhaps the most distinguished ornament of the Alexandrian observatory, flourished about B. C. 150, and has been called the father of astronomy. Unfortunately all his works, except one of trifling importance, have been lost. We learn, however, the particulars of his researches from Ptolemy. From his existing work it has been elicited that he was in possession of the principles of spherical trigonometry, of which we find no traces in any previous Greek author. Delambre considers him as the inventor of that science; and if so, this alone would entitle him to the highest degree of praise; both in an abstract point of view, and since without this auxiliary science astronomy could not advance a step.

Hipparchus not only gave a more accurate determination of the length of the solar year than had been previously done, but investigated with particular care the inequality in the sun's motion, which had, in a general way, been long before noticed. The most ordinary observations of the solstices and equinoxes sufficed to show that the sun took a longer time in passing through the northern than through the southern half of the ecliptic. Hipparchus determined the former to be 187 days, and the latter $178\frac{1}{4}$. To account for the increased velocity of the sun's motion during this latter half of his course, Hipparchus imagined the theory of the sun's apparent orbit still circular, but the earth not in its centre. This would give a plausible explanation of the apparent difference of motion. He pursued also similar observations with regard to the moon, and constructed a similar theory of her orbit; observing, also, its slight inclination to the plane of the ecliptic.

With regard to Hipparchus's theory of the solar orbit above mentioned, and to the views of the planetary system generally adopted in the school of Alexandria, considerable difference of opinion is found between different historians. By some it is contended that the primary principles at least of the theory afterwards adopted by Ptolemy were introduced in the age of which we are now treating. It may not, therefore, be improper here to give a cursory sketch of its nature.

As soon as the actual apparent motions of the planets were tolerably well known by observation, it of course became an object of interest and importance to form some scheme by which their real nature might be best represented. The simplest and most natural, that of an uniform motion round the earth, was soon disproved, when it was noticed that the motion at some periods became slower, the planet at length stationary, and then for a time retrograde; again stationary; and after that progressive: all this recurring at certain periods, which were known from observation.

The original suggestion of a mode of solving the difficulty, and representing these apparently complex motions on a simple hypothesis, has been ascribed to Apollonius. He conceived, that in the circumference of a circle, having the earth for its centre, there moved the centre of another circle, in the circumference of which the planet revolved. The first was called the *deferent,* the second the *epicycle,* and the motion in each was supposed uniform. The motion of the centre of the epicycle in the circumference of the deferent was towards the east, that of the planet in the epicycle towards the west. In this way the observed changes from direct to retrograde motion, with intermediate stationary points, were readily explained, and the ratios of the radii necessary to account for the observed extent of these changes were also calculated.

Thus an object which was then considered of great importance to astronomy was accomplished, viz. the production of a variable motion, or one which was continually changing both its rate and its direction, from two uniform circular motions, each of which preserved always the same quantity and the same direction.

The theory framed by Hipparchus to represent the inequality of the sun's motion (of which we have already spoken) has been represented by some authors as involving this principle of epicycles; and as consisting in an epicycle of small radius, in which the sun revolved with the same angular velocity, but in an opposite direction to that with which the centre of the epicycle moved in the deferent. We feel, however, little interest in entering upon such a question: it was necessary to refer to it, and to the general subject of the theory of the epicycles, on account of the celebrity they afterwards acquired, as we shall see in the sequel.

He also made the first attempt to estimate the distances of the sun and moon. His calculation was founded on measures of their apparent diameters, and of the diameter of the earth's shadow at the moon's orbit; which was derived from the time occupied by the moon in passing through it in an eclipse.*

The most important, perhaps, of all the services rendered to astronomy by Hipparchus was the formation of a catalogue of the fixed stars; an enumeration, that is, of all the principal stars referred to their actual positions in latitude and longitude. Indeed nothing but such an examination can ratify their claim to the title of *fixed* bodies. This was obviously a work requiring immense assiduity as well as precision. But its chief value is in being an exact representation of the state of the heavens *at a particular epoch.* It is the comparison of an ancient catalogue with one made from observations of the present day, which gives the value to both sets of observations. It is by this comparison that we learn whether, in the course of ages, the actual configuration of the stars has undergone any change; and by this means alone that we can decide whether the fixed stars are what they are assumed to be, points actually fixed as standards of measurement, to which we can refer the places of the obviously varying bodies of our system; and by measuring from which, as fixed points, we can estimate their motions, and deduce with accuracy the laws which govern them. Hence the values of catalogues of different ages. If these ancient astronomers had enjoyed the instrumental means of making their catalogues as perfect as ours, there are numerous questions of the highest interest in astronomy which might now have received their solution, but which, under existing circumstances, must wait for ages to come, perhaps, before they can be decided. There are many of the smaller variations which require the accumulation of

* Libes Hist. de Phys. i. 68

centuries to render them sensible; yet so powerfully have the great modern instrumental improvements aided the researches of the astronomer, that even in the comparatively short period since the invention of the telescope the greater accuracy of observations has compensated, as it were, for the shorter intervals of time at which the comparisons are made; and many of these most interesting points of enquiry have been cleared up, though many more probably yet remain in obscurity.

It was in this way that Hipparchus, comparing his catalogue with the observations of Aristillus and Timocharis of 150 years before, perceived that all the fixed stars, while they retained their latitudes sensibly unaltered, had advanced about two degrees in longitude; or, what amounts to the same thing, the equinoctial points appeared to have retrograded along the ecliptic by the same quantity. In other words, he made the first discovery of the *precession of equinoxes*; as a bare fact which the lapse of years had brought to light, but which received no explanation till the time of Newton.

To Hipparchus, also, we are indebted for the first attempt at fixing geography upon exact principles, by referring the position of places on the earth's surface to their latitudes and longitudes: he proposed to determine the latter by means of eclipses of the moon. He also directed his attention to the more accurate correction of the calendar. He proposed to quadruple the period of Calippus, and then to subtract a day. Upon the whole, the praise we must ascribe to Hipparchus is unquestionably very great, even though we should consider the encomium of Pliny somewhat extravagant:— "*Hipparchus nunquam satis laudatus, ut quo nemo magis comprobaverit cognationem cum homine syderum, animasque nostras partem esse cœli . . . . . ausus rem etiam Deo improbam, annumerare posteris stellas.*" (Nat. Hist. ii. 26.)—"Hipparchus, never sufficiently to be praised; than whom no one more fully proved the kindred between the stars and man, and that our souls are a part of heaven . . . . . who ventured also to do a thing wrong in the sight of the Deity, to enumerate the stars to posterity."

With this indefatigable astronomer the zeal for prosecuting observations appears to have died away; and from various causes, among which perhaps the wars of the later sovereigns of Egypt may have had their share; and, notwithstanding the endeavours made by Ptolemy Physcon to stimulate the declining sciences (B. C. 137), astronomy was gradually reduced to a very low ebb. And though (about B. C. 50) Theodosius and Menelaus wrote on the sphere, and on spherical trigonometry, no advances of importance were made in these sciences for several centuries.

Physical Science.

From the school of Alexandria emanated the first optical treatise. The regularity with which the rays of light take rectilinear courses seemed naturally to make them a subject of study to the geometer. Euclid, perceiving this affinity, began to apply the science he had already cultivated with so much success to explain the laws by which the directions taken by the rays of light are regulated. This was probably some years previous to the labours of Archimedes in reducing other parts of physical science to the dominion of mathematical laws. Two treatises are extant, one on optics (in the more limited sense of the theory of vision), the other on catoptrics, both ascribed to Euclid, but, as is now generally allowed, erroneously. In the optics, the principles are investigated by which we judge of the magnitudes of objects; but the reasoning proceeds too exclusively on the mere geometrical consideration of the angle subtended, without taking into account other causes. In other respects also the investigations are faulty. In the catoptrics, the general principles of the place of images by reflection are also laid down, but not by any means correctly; and the proofs are obscure and defective.

These and other faults, so unworthy of the distinguished author to whom these books have been ascribed, have led critics to deny the genuineness of both treatises. Some have considered them (but especially the catoptrics) as the compilations of some ignorant pretender to science; whilst others have supposed them to have been vitiated versions of an original treatise, which it is on all hands believed Euclid actually did compose, and of which these works are careless extracts or unskilful abridgments.

The mechanical enquiries begun by Archimedes were extended by Ctesibius and Hero in the school of Alexandria a century later, or about B.C. 150. They were the first who, by an analysis of all mechanical engines into their primary elements, reduced all their actions to some combinations of five simple principles, to which they gave the name of δυναμεις, or mechanical powers; the same system which is retained at the present day.

In hydrostatics, after the developement (as we have seen) of some of the most essential principles by Archimedes, it does not appear that any very material advances were made; though several of his successors enriched the science by practical improvements.

It is uncertain when the common pump was first invented; but with the knowledge possessed long before this period it must have been an easy application. That since the pressure of the air acts on the surface of the water in the reservoir, but is removed from that in the tube, the latter ought to be forced up, was a consideration much too simple to accord with the prevalent notions of a philosophical theory; and nature's abhorrence of a vacuum had already been established as a more logical explanation of this class of phenomena. It is certain, however, that the principle must have been practically known before the more complex application of it in the *forcing-pump*, which was the invention of Ctesibius, perhaps the greatest mechanic of antiquity after Archimedes. It was produced almost exactly in the form of the modern fire-engine. To the same philosopher are also ascribed the clepsydra, or water-clock, and the air-gun.

His contemporary, Hero, was distinguished for inventions rather curious than useful; such as ingenious combinations of syphons, &c. forming various kinds of fountains and water-works. Such contrivances serve to illustrate the progress which was made in the knowledge of principles, however fallacious the theories to which those principles were referred.

Hero adopted the idea of nature's abhorrence of a vacuum, at least as extending through any considerable or sensible portion of space. He made this limitation because he conceived that in the insensibly small interstices of bodies a vacuum does exist; and by this means accounted for the compressibility of matter. He contrived a method for the exhaustion of air out of a vessel, very nearly resembling the air-pump, and practically applied it to the purpose of cupping instruments. But to explain the effect, he devised the long celebrated theory of suction.

According to this theory, the upper portion of the liquid or substance is drawn up in a tube or vessel, being attracted and supported, as it were, by some mysterious and occult power, applied in the act of suction, whilst the next portions in succession are sustained by those above them; and the extent to which such action can be carried is limited by the weight of the column suspended.

Posidonius, somewhat later than Hero, followed in the cultivation of the same sciences. He enquired into the nature of the tides*, and discussed the atmospheric refraction. In this he was followed by Cleomedes, who also compared the magnitudes of the earth and of the sun. †

Our knowledge of all these inventions is principally derived from the writings of Vitruvius. Some of the works of Hero, and others of this age, appear in the valuable collection published under the title of "Mathematici Veteres." Others exist in manuscript in the various public libraries throughout Europe.

* Cic. de Nat. Deor. ii. † Meteor. c. ii.

SECTION III.

THE STATE OF SCIENCE DURING THE AGE OF THE ROMAN EMPIRE TO THE PERIOD OF ITS DISSOLUTION.

Physical Science among the Romans.

A VERY slight acquaintance with the literature of the Romans suffices to show that in that nation so eminent in war, in polite literature, and civil policy, there prevailed at all times a remarkable indisposition to the pursuit of mathematical and physical science. When the treasures of Greek literature were opened to the incipient curiosity of Rome, reposing from her earlier triumphs, the works of the poets, orators, and moral philosophers of that country, were sought with avidity and studied with ardour; whilst those of the geometers and astronomers were totally neglected: and these sciences, so highly estimated in the country which gave them birth, were not merely disregarded in Italy, but even considered beneath the attention of a man of good birth and liberal education: they were imagined to partake of a mechanical, and therefore servile, character. The practical results were seen to be made use of by the mechanical artist, and the abstract principles were therefore supposed to be, as it were, contaminated by his touch. This unfortunate peculiarity in the taste of his countrymen is remarked by Cicero. And it may not be irrelevant to enquire, whether similar prejudices do not prevail to some extent even among ourselves; and whether the exclusive attachment to classical studies, and the cultivation of the Roman literature, as the sole basis of the education of the higher classes, may not be the source from which such prejudices are too commonly imbibed.

Nevertheless it must in fairness be admitted, that there are instances in some of the Roman authors of better views of the claims of science: thus the well known exclamation of Virgil,—

> "Felix qui potuit rerum cognoscere causas!"
> (Happy he who knows the causes of things!)—

would seem to indicate a more just estimation of the value of physical truth.

Nor, again, must we omit to mention the philosophical poets, Lucretius, Manilius, and Ovid. Of their well known productions it would be superfluous here to say more, than that the mere circumstance of their selecting the subject of natural philosophy affords a presumption that a certain degree of interest in it must have been generally felt among their readers. And though they support, with all the eloquence of poetry, those theories of the schools, which in truth are much better adapted to such illustration than to sober discussion, yet it must be allowed that they have placed some parts of the system of nature in a striking point of view, and sought to render them attractive by investing them with all the charms of poetical imagery.

During the whole existence of the republic, we hear but of one Roman who attained any reputation for eminence in astronomy. C. Sulpitius Gallus is mentioned by Cicero as an indefatigable calculator of eclipses; and it appears from Livy, that he predicted an eclipse of the moon on the night preceding a battle between the Romans and Macedonians, which would have dismayed the superstitious Romans, and perhaps have occasioned their defeat, had it not been foretold to them, and adroitly converted into an omen of success. We know that a similar cause produced a defeat of the Athenian army at the siege of Syracuse.

Besides Sulpitius Gallus we have the names of one or two Romans who are said to have written on physical subjects; but their works are lost, and we have no means of judging of their scientific acquirements. One of these is Varro. Cicero himself translated the poem of Aratus, which, in fact, contains little worth translating;

and in its best part is a mere exposition of some of the most obvious elements of astronomy; whilst a large portion of it is occupied with astrological precepts.

The neglect of astronomy among the Romans was palpably conspicuous in the confusion in which their calendar became involved. In the time of J. Cæsar the difference between the beginning of the civil and solar year had accumulated so as to amount to three months. The universal genius and penetration of that eminent man led him, among other subjects, to a cultivation of astronomy. We learn from Pliny that he both made observations and composed some works on the subject; which, however, are entirely lost. In the present instance he applied his knowledge, with the assistance of Sosigenes, a Greek astronomer, to the correction of the calendar. The principle proposed was to take the civil year as 365 days, and every fourth year to add an intercalary day. This is very nearly exact, but not quite, since the length of the year is accurately rather *less* than 365¼ days. However, it was then considered sufficiently precise, and the day added was placed after that called "Sexto Calendas Martii," and hence was named "*Bis* sexto," &c., whence the name bissextile. This will be recognised as the mode of computation adopted at the present day, with a slight modification subsequently introduced.

A disposition to physical enquiry, where it did exist among the Romans, scarcely ever went further than to produce an acquaintance with the previous researches of others, a just admiration of them, and a diligence in collecting and recording them. It never rose to the character of originality; and the whole remains of Roman literature do not present us with a single work of original scientific invention or physical discovery. We, nevertheless, possess the writings of several assiduous collectors of the treasures of amassed knowledge, to whom we are under deep obligations; and some of whom have certainly shown themselves possessed of talents far beyond those of mere compilers.

The elder Pliny has left us the largest record we possess of the knowledge of nature, in all its departments, which had been attained in his day. It does not always appear how far he is to be understood as proposing his own views, or how far merely recording what had been observed by others: but we may here properly notice a statement which he gives, remarkably illustrative of the knowledge then attained respecting that highly curious subject, the tides. After having mentioned that Pytheas, a contemporary of Aristotle, had maintained, though in a vague way, some connection between the tides and the moon, he proceeds*: — "The ebb and flow of the tide is very wonderful: it happens in a variety of ways; but the cause is in the sun and moon." He then very accurately describes the course of the tide during a revolution of the moon; and adds,—"The flow takes place every day at a different hour; being waited on by the luminary, which rises every day in a different place from that of the day before, and with greedy draught drags the seas with it." . . . . "When the moon is in the north, and further removed from the earth, the tides are more gentle than when, digressing to the south, she exerts her force with a closer effort." The meaning of this last sentence is not very obvious; but the whole representation is certainly remarkable.

Strabo, also, mentions that Posidonius maintained the existence of three periods in the tide—daily, monthly, and annual — "in sympathy with the moon." Strabo, as a philosophical geographer, may himself be considered as one of the ornaments of Roman science. He died A. D. 25.

Plutarch, at a somewhat later period (about A. D. 110), claims a place among the philosophers, as well as among the literary writers of Rome. He not only travelled to collect scientific information, but, patronised by the emperor Trajan, opened a school in Rome, where he lectured with great reputation. His philosophical speculations do not appear, in general, to possess

* Lib. ii. c. 97.

any very high character; but there is one idea broached in them deserving of notice: he suggests that the moon's motion is preserved by a cause similar to that which retains a stone in a sling when whirled rapidly round. This must be allowed to form a fair illustration of the way in which physical enquiry proceeded among the ancients. The facts were observed as well as their means of observing allowed; then some analogy was perceived, or imagined, or some casual illustration struck out; but there the matter was left. It was not perceived that such analogies and illustrations, fairly followed out, are the infallible road to philosophical truth. It was from never pursuing these conjectural theories into their consequences, and from never attempting to subject them to the test of numerical values,—in a word, from *mistaking the use* of hypothesis, rather than from an undue fondness for it,—that the ancients failed in interpreting nature aright, and in arriving at a sound system of physical knowledge. In a similar way, Lucretius mentions the fact, that *in vacuo* all bodies fall in the same time; but there he leaves the matter, without further notice, or pursuing it into any of its consequences.*

In practical optics, we know that the ancients had advanced to the construction of metallic specula, both plane and spherical. Plutarch also informs us, that the fire of Vesta was only allowed to be rekindled by the rays of the sun concentrated by a *conical* speculum of copper.

Glasses in the form of spheres were in use as lenses for burning, by bringing to a focus the rays of the sun, probably as early as the time of Aristophanes, B. C. 434. Pliny says that a globe of rock crystal was used for the same purpose, and employed in surgical operations. He also mentions the power of a glass globe filled with water to produce a strong heat, by collecting the rays of the sun; and expresses his surprise that the water itself should all the while remain quite cold.†

The fact that, in certain instances, vision through glasses produced a magnifying effect, is noticed by

* Lib. ii. 238. † Lib. xxxvi. 67.

Seneca; but the dependence of this on the lenticular form of the glass seems to have been wholly unknown; and this case was confounded together with others, in a way evincing a total absence of all conception of the cause. Seneca mentions the magnifying power of a round glass vessel of water, associated with a statement purely imaginary, that the stars are magnified by being seen through a cloud; and ascribes the cause to some power of *humidity*. Speculations of the like nature are also given on the rainbow. Perhaps one of the most curious and interesting subjects adverted to by this writer is the phenomenon of the prismatic colours. He distinctly describes an angular glass rod, evidently of a prismatic form, which, he says, produces colours like those of the rainbow. This he considers to be occasioned from the circumstance that the image is irregularly formed; (quia enormiter facta est;) and adds, that such a glass, properly made, will give as many images of the sun as it has angles: which is nothing more than the multiplication of an object, by a glass cut with a number of faces.* He appears to have remarked, also, the atmospherical refraction, and the elasticity of the air. †

Aulus Gellius (about A. D. 130), among the variety of subjects he has recorded, has preserved some curious particulars relative to practical optics, as known in that age: in particular, when speaking of the properties of reflectors, he has mentioned one relating to concave mirrors, which, for a long time, completely baffled the skill of the most scientific commentators to interpret, until it received, very recently, a complete elucidation from Mr. Horner.‡ This property, so obscurely expressed (as, indeed, are several others of those here discussed), is extensively available in optical deceptions, and, doubtless, afforded the secret of many a miraculous apparition in an ignorant age. The particular property

* Quæst. Nat. i. 6. † Lib. v. 8. and vi. 16.
‡ A forgotten Fact in Optics. Bath, 1832.

referred to depends on an extremely simple consideration of the course of the reflected rays.

However little the Romans were qualified to make any additions to the truths of physical philosophy, they readily adopted its practical applications; and the growth of luxury put in requisition many of those resources of science, to study the principles of which was far beyond their stage of intellectual refinement. Athenæus mentions, that the cooling power of evaporation was known and extensively employed in the economy of the table. But in many cases their mechanical contrivances evince a remarkable ignorance of some physical principles which would appear of a very obvious nature. Thus, their aqueducts, built with vast labour and expense, afford a proof that they were unacquainted with the law that water will rise, through any communication by pipes, to its original level.

A diligent collector might, doubtless, find much information scattered in the writings of these ages, which would afford a number of curious illustrations of the state of scientific knowledge: but, without further details, what we have here stated will probably suffice to convey a general idea of the subject.

Upon the whole, we may remark, in conclusion, that if the philosophical writers of Rome advanced little in the actual career of discovery, yet some of the most profoundly learned among them have been foremost in perceiving the emptiness of the artificial systems of their day, and have expressed the most truly philosophical views as to the present deficiencies and future prospects of science as it then stood. Thus, Seneca, when alluding to the subject of comets, and remarking our ignorance of their nature and motions, adds, — "Veniet tempus, quo ista quæ nunc latent in lucem dies extrahat, et longioris ævi diligentia: ad inquisitionum tantorum ætas una non sufficit. Veniet tempus, quo posteri nostri tam aperta nos nescisse mirentur." —*Nat. Quæst.* vii. 25. (The time will come when a fnture day, and the diligence of a distant age, shall bring to light those things which now lie hid: one age will not suffice for such great discoveries. The time will come when our posterity will wonder that we should have been ignorant of things so obvious.)

And Cicero, as if in anticipation of the triumph of the inductive philosophy, emphatically exclaims,—"Opinionum commenta delet dies: naturæ judicia confirmat." (The lapse of time obliterates the glosses of human opinion, but establishes what is conformable to the testimony of nature.)

The Second School of Alexandria.

After Egypt had become a province of the Roman empire, the city of Alexandria still continued to be a favourite abode of the sciences. Though, for a long period, the high fame of its school had declined, yet there were not wanting those who continued to cultivate various branches of literature there. And at a later age, under the dominion of the Antonines, commencing A. D. 140, and especially under the sway of the philosophical Marcus Aurelius Antoninus, a considerable revival of science took place in the scene of its former triumphs; a new constellation of genius began to arise; and though, neither in numbers nor in eminence to be put in comparison with their predecessors in a former age, yet the *Second School of Alexandria* produced several philosophers of considerable merit, and some works which ably sustain their reputation.

Ptolemy, and his System.

Claudius Ptolemæus, a native of Ptolemais in Egypt, commonly known by the name of Ptolemy, flourished at Alexandria about A. D. 140. He revived, in that school, the cultivation of astronomy, which had been almost entirely dormant during the three centuries since Hipparchus. Animated with the ambition of restoring the science to its just estimation, and of perfecting what his predecessors had left unfinished, he felt the necessity of collecting the materials existing in the works of Hipparchus and others into a regular system, and combining these with his own investigations, so as to form a complete body of astronomy. This gave rise to his great work entitled Μεγαλη Συνταξις (Great System), the publication of which forms an epoch in the history of science. This work, becoming known throughout the East, was preserved there, and survived the barbarism of the middle ages. It formed the basis of the astronomy of the Arabians, and, for a considerable time, of modern Europe also: from the former it obtained the title of "Almagest," by which it was afterwards known in Europe.

Comprehensive as was the design, and elaborate the execution, of this work, it labours under one cardinal defect,—the adoption of that view of the system of the world, which makes the earth the centre, and supposes the sun and planets to revolve round it. We have already remarked that there was little in point of real argument from facts, to decide the judgment of the ancient astronomers on this point; yet it is difficult to conceive that the arguments from probability, analogy, and simplicity, should not have preponderated with all who were imbued with any thing like a philosophic spirit.

Ptolemy, however, argued against the solar system. He reiterated the already refuted objection of parallax; and reasoned upon the false and gratuitous theories of the peripatetics about motion; contending that, if the earth were really in motion, it ought to leave behind it all the loose bodies upon its surface, which (according to their theory), as being lighter, must move more slowly. This, and many equally absurd arguments, he adduced; though he admitted that the Pythagorean system possesses much greater simplicity.

We have before mentioned the theory of the epicycles, to account for the planetary motions: these were adopted by Ptolemy in their full extent. It has been contended by some writers, that, in adopting them, he merely intended to frame a mathematical construction which should conveniently represent the facts; but this would appear hardly reconcilable with the earnestness he displays in arguing for that physical system of the planets, and the Aristotelian doctrine of motion, which rendered the epicycles necessary. His own expressions, on which this notion has been defended, are certainly far from conclusive either way. He merely speaks of the hypothesis as one which will "*save* the phenomena," as we may, without violating the English idiom, *literally* translate the Greek. It is certain that his later followers considered these to be the *real* motions of the planets; as they also believed in the real substantial existence of the crystalline spheres, which Eudoxus had devised as the causes of the celestial motions. Mæstlin, in particular, has a strong and explicit passage to this effect.

However this may be, the various inequalities which were successively observed in the planetary motions gave rise to new applications of the same principle. An inequality had been discovered (as we have seen) in the moon's motion by Hipparchus. Ptolemy discovered another, which varied according to the angular distance of the moon from the sun, and was called the *evection*. The first of these was explained by the moon's motion in an epicycle whose deferent was the *mean* orbit. But, for the second, it was necessary further to suppose that the centre of this deferent again moved in another circle. Of the enormous and increasing complication of such a system, arising from the extension of this principle to all the planetary orbits, as well as to the real inequalities of the sun and moon, it is hardly necessary to say much. It certainly owed its general adoption to the convenience it gave in calculation. Owing to the preservation of the principle of circular motion throughout, the geometrical constructions, and the arithmetical computations, involved, were never of great difficulty; and the accuracy of the results was sufficient for the purposes of astronomy as it then stood. The system was, therefore, adopted by astronomers as a convenient one for practice, even if they had been inclined to question its physical truth. Nevertheless, the conception of the planetary world carrying on its motions by the aid of such mechanism, must have been a considerable effort even to the most powerful imagination. Some illustration of the supererogatory difficulty thus occasioned may be afforded, if we observe, in passing through a wood, in how extremely complicated a manner the relative positions of the trees appear at each step to be continually changing; and by considering the difficulty with which the laws of their apparent motions could be traced, if we were to attempt to refer these changes to a real motion of the trees, instead of to that of the traveller. In order, however, to complete the illustration, we must yet further imagine what would be the increased complexity, if each of the trees had a real motion of its own, besides their apparent motions resulting from that of the observer; which is the case in regard to the planets and the earth.

Such, however, was the Ptolemaic system, which obtained an ascendancy for a longer series of ages, and over a larger portion of the world, than any other. A monstrous complication of purely gratuitous hypotheses, which, to account for every new inequality, had to be entangled in new perplexities, and, to solve every fresh difficulty, had to be involved in more hopeless confusion; and its votaries went on, in the language of the poet,—

> "To *save* appearances; and gird the sphere
> With centric and excentric scribbled o'er,
> Cycle and epicycle, orb in orb."

All this complexity being incurred solely to avoid impugning the grand dogma, that "uniform motions belong by their nature to celestial bodies;" and the sublime principle, that "circular motion is perfect and incorruptible." Yet such was the system, not only adopted in that age by the contemporaries and followers of Ptolemy, but which actually held its ground in modern Europe until a comparatively late period, and was obstinately retained as long as the increasing light of inductive truth still left a dark nook where its partisans could find refuge in congenial obscurity.

Of the instruments commonly employed by the ancient astronomers, the gnomon was at once the simplest, and probably the most accurate for its purpose. Besides this, we find them, especially in these later ages, using astrolabes, and armillary spheres of various constructions, which being fixed with their circles in the actual positions of the real sphere, the equinoxes were observed by the coincidence of the plane of the circle representing the equator with its own shadow. Altitudes were measured by a circle in the plane of the meridian, and the shadow of a small projecting piece at the extremity of a revolving diameter, made to fall exactly on a corresponding projection at its other extremity. Ptolemy himself contrived a quadrant with a similar apparatus. He also used an instrument of a similar principle, but of which all the parts were rectilinear: he thus measured the *chord* of the angle observed; and found the angle by a table of chords. The great defect in all these instruments was that want of certainty in fixing upon the precise position of any celestial object, which could not be remedied without a knowledge of the telescope. Their graduation also, probably, was but of a very inferior degree of accuracy. Another fundamental deficiency in their observatories was in the accurate means of measuring time. The clepsydra was open to manifest objection. Upon the whole, it is rather matter of astonishment that, in the department of actual observation, the ancients should have done so much, considering the limited means at their command, than that they should not have effected more.

Optics.

The labours of Ptolemy were not confined to astronomy: he produced a treatise on Optics; the first in which the subject of refraction is accurately enquired into. This treatise, though known in the middle ages, and quoted by Roger Bacon, had disappeared, and was supposed to be entirely lost, till, in very recent times, a copy (professedly a translation from the Arabic) was found in the king's library at Paris: another copy also exists in the Savilian library at Oxford.

Ptolemy, actuated, doubtless, by the necessity he felt, as an astronomer, for a more accurate knowledge of atmospheric refraction, as affecting the places of the heavenly bodies, examined with great care and precision the angles of refraction corresponding to all angles of incidence from 0 to 80 degrees, when a ray enters a a medium of water, or of glass, out of air. The theory which he hence gave of astronomical refractions was even more correct than that adopted by some of the moderns. In an optical point of view, his measurements accord very exactly with the modern law of the sines: though he did not advance to any such generalisation from them.

Some modern writers (probably from incorrect versions of Ptolemy's work), ascribe to him the explanation of the fact, that the sun and moon on the horizon appear larger than in the zenith, as depending on the circumstance that, in the former case, our judgment is influenced by comparison with terrestrial objects. It has been ascertained by De Lambre, that in the original he does not give any thing like this explanation (which is, doubtless, the correct one), but only a very vague theory. This explanation is, therefore, probably due to his Arabic translator; and most likely derived from Alhazen.

Ptolemy distinguishes what has since been called the *virtual* focus, in the reflection from convex specula, or the point where the reflected rays, if produced, would meet. And, among other interesting glimpses of truths not fully discovered till many ages afterwards, he notices the fact, that colours are confounded together by the rapidity of motion; and gives the instance of a wheel painted with different colours, and turned quickly round.

Progress of Mathematics.

Diophantus flourished at Alexandria in an early age of the Christian era; but his precise date is matter of question. He has left a treatise, in which the principles of what is in fact algebra, though not given in the form in which that science is now used, are laid down with great ingenuity and ability, in thirteen books, entitled "Arithmetical Questions." Some of the problems are of considerable intricacy; and much address is displayed in stating them, so as to bring out equations in a form involving only one power of the unknown quantity. The whole processes are expressed in common language, assisted by a very few symbols, which are mere abbreviations. Considering how little power and convenience the instrument had yet acquired with which he worked, it is remarkable how much he was able to effect. He directed his attention particularly to the class of problems called indeterminate, or which admit of a number of solutions: they have been hence known by the name of Diophantine problems. Though algebra was not at all yet reduced into a symbolical form, it is remarkable that Diophantus distinctly expresses the rule for the signs in multiplication, by saying, that λειψις into λειψις gives ὑπαρξις, &c. (minus into minus gives plus).

Pappus. — Decline of ancient Science.

The declining science of the fourth century was ably upheld by Pappus, one of the last distinguished ornaments of the Alexandrian school, who flourished between A. D. 350 and 400: he cultivated, with success, almost all branches of geometry; and, in most of them, has left some investigations of considerable value.

The subject of the loci was one to which his attention was especially directed: in particular, he considered that case which was called the "locus ad quatuor rectos;" a problem which baffled the powers of the ancient geometry, but which has become celebrated as affording one of the highest triumphs to the modern analysis, and has led to the most extensive applications, of which we shall give an account in the sequel.

The principal work of Pappus is his "Collectiones Mathematicæ:" in this, among a variety of other curious discussions, we have a considerable space occupied in the establishment of the doctrine of maxima and minima, on geometrical grounds; and a remarkable application of it to the form of the cells of bees. This, in fact, involved the consideration of a class of problems which, in a more extended sense, have since been called *isoperimetrical.** The nature of these will be rendered intelligible by a very simple illustration.—Let a circle be cut out in card, and its length of circumference measured by, and marked upon, a piece of tape wound round it; let then several other regular figures, as a triangle, square, hexagon, &c. be also cut out; by a little care in cutting, they may be gradually brought to such sizes, that the same tape will measure round their perimeters, so that they shall be all of exactly the same length as that of the circle; the perfect regularity of the respective figures being all the while carefully preserved. This being effected, it will be very easy to see, by merely placing them successively upon one another, that although their lengths of perimeter are exactly the same, yet their areas will be seen to be of palpably different extent.

To investigate such points by mathemetical reasoning is a matter of some difficulty; but the subject was treated by Pappus with great success in a variety of cases. It is found (and may be easily shown by the above method of illustration) that, of all figures having the same perimeter, that will have the greatest area which has the greatest number of sides: hence the circle (being regarded as a polygon of an *infinite* number of sides) will include the greatest area of all *iso-*

* ἴσος, equal; περίμετρος, circumference.

perimetrical polygons. Similar considerations apply to solids and their surfaces.

The form in which the cells of the honeycomb are made appears to accord with the deductions of geometry, on principles of which this is the foundation, but to which several other considerations must be added. The only figures which, placed together, leave no interstices, are equilateral triangles, squares, and hexagons: of these, the last, of course, have the greatest content with the same circumference; in forming contiguous cells, therefore, there is a saving of materials in using the hexagonal form. The mode of terminating the cells is the most curious part of the whole. Geometrically, an extension of the same advantages would be gained if they ended in three-sided pyramids, formed by planes cutting off each alternate solid angle of the hexagonal cell, and inclined at a certain angle, which is found by calculation. Precisely such conditions have been found to be observed by taking the average of a great number of measurements of the actual cells of honeycombs.

This part of Pappus's work has come down to us in an imperfect state, but has been restored by Maclaurin. Pappus forcibly expresses his admiration at this very singular fact in the economy of the bee, exclaiming,—"Κατα τινα γεωμετρικην μηχανωνται προνοιαν." (They work by a sort of geometrical forethought.)

In his fourth book, Pappus discusses several points connected with the quadrature of the circle, and describes some of the inventions made by preceding geometers in their researches having that object in view. One of the most remarkable, perhaps, is the construction of a curve called the quadratrix, or squaring line, invented by Dinostratus and Nicomedes, of which he gives a demonstration; and by which the length of a circular arc is assigned: but the application of it involves the principle of limits, and does not, therefore, materially help the question. It affords, nevertheless, some very beautiful geometrical speculations.

The labours of this great geometer, however, extended to many other subjects, on which we cannot here enter. In his mechanical speculations he was less fortunate: he applies geometry with considerable effect to many questions in this science; but, in the investigation of the inclined plane, betrays an erroneous notion of the resolution of forces, which renders defective his estimate of the force necessary to sustain a body on the plane: this defect in the knowledge of so essential a principle marks a point beyond which the ancient mechanics did not extend, and which must have precluded a further advance.

Theon and Proclus, about the same period, or rather later, were known chiefly as commentators on Euclid. To the former we are little indebted for his attempts to improve upon his author; the latter wrote also a treatise on Motion.

Diocles, who lived before A. D. 500, is known as the inventor of an elegant curve, called the cissoid, to which he was led by a previous construction of Pappus for finding two mean proportionals between given extremes. This curve is the locus traced out by the intersection of a chord from one end of the diameter of a semicircle drawn to the summit of an ordinate, with another ordinate equidistant from the centre. A perpendicular at the other end of the diameter becomes an asymptote to the curve; which was named the cissoid*, from a fancied resemblance to a sprig of ivy mounting up a wall, as the curve does up its asymptote.

On alluding to these abstract topics, these elegant fictions, as it were, of a geometrical imagination in which the ancient mathematicians loved to indulge, the question of utility will doubtless be raised; a question which, of course, can only be answered when the objector states to what class of objects he will extend the character of utility. For any practical results these methods are no longer of use, because we now possess easier and shorter processes. But that matters of abstract contemplation, as such, are not useful, is an assertion which surely betrays the most confined

* κισσος, ivy.

notions on the part of him who urges it, and will hardly be maintained by any one who has acquired just views of the relative importance of the different branches of philosophical speculation, and the intimate connection and dependence which subsists between them. And that, of such abstract speculations, those relating to the singular properties of curve lines arising as logical consequences out of the simplest constructions, are among the most beautiful, is a point of taste, for the justness of which we can only appeal to the convictions of every one who will go through the necessary steps to enable him to judge.

The fifth century of the Christian era witnessed the almost total extinction of the sciences in Alexandria. Some students, indeed, were to be found, who continued to keep alive the memory of the achievements of a former age. But the circumstances of the times were unfavourable to the extension of physical research, and at length the invasion of the Saracens, and the wanton destruction of that great repository in which the treasures of literature had been accumulating at Alexandria for nine centuries (A. D. 640), struck the final blow; and from that period we may date the extinction of Greek philosophy.

In Rome, we have already seen that the little taste ever evinced for the physical sciences had long before declined. The writings of the philosophers were composed exclusively in the Greek language. The great division of the empire under the sons of Theodosius placed so broad a line of separation between the eastern and western portions of it, that Greek literature was now no longer cultivated in Rome. Thus the records of science ceased to be accessible to the nations of the West; while the troubles in which this division of the empire was now involved, the continual wars, and irruptions of the Gothic tribes in its later ages, wholly prevented any attempts to raise an indigenous growth of science, even had the germs of it existed in the genius of the people.

General Remarks on the Progress and Character of ancient Science.

Though, in the course of the preceding pages, we have from time to time made incidental remarks on the causes which have affected the progress of science among the ancients; yet it may not be superfluous, in concluding this division of our subject, briefly to recapitulate, and add a few general observations on such causes, on the spirit and genius of the ancient methods, and its influence on the true interpretation of nature.

If we consider man as a being gifted with the powers of observation and reason, and especially as receiving knowledge through the medium of the external senses, it might seem that the examination of nature, and the investigation of the laws by which its phenomena are regulated, would naturally and necessarily form both an early object of attention, and one to which a peculiar importance would be generally attached. Nevertheless we have observed that the case has been much otherwise: and, in following the course of the gradual advance of moral, as well as physical civilisation, we have been compelled to acknowledge that the progress in this great department of intellectual improvement has taken place so tardily, and to such a very limited extent, even under the most apparently favourable circumstances, that it may not be unimportant briefly to notice some of the causes which have probably tended to produce such a result.

In the earlier stages of society, and with a large portion of mankind in all its stages, there are other and more pressing wants to be satisfied before those of the mind. But, supposing that we are arrived at a point beyond this state of things, and that the requisite freedom from the immediate pressure of necessity is enjoyed, still it is long before the particular contemplation of nature, with a view to tracing its order and harmony, takes possession of the mind. A few, perhaps, are irresistibly led on by some loftier aspirations to devote their attention to what the rest disregard. But in general, even in the most favourable instances, there are many causes operating to render the advance towards the real principles of science extremely slow. The first and foremost obstacle is the want of perception of the value and importance of making exact enquiries into nature; or even of contemplating the material world at all, in any other light than as at best a topic of passing admiration, or in general as the source of new physical enjoyments. This want of a due recognition of the claims of pure philosophic enquiry, apart from the examination of the natural world with a view to the purposes of art or of gain, has been, and still is, the first great impediment in the way of the more general diffusion of a taste for such pursuits, and the more extended cultivation of them. In fact, so slowly, and so rarely, is the full recognition of the intrinsic excellence of abstract enquiry brought about, that in tracing the growth of human society in its emergence out of the depraved condition of savage life towards its just and fair proportions, whenever we can find anything like a due estimation only avowed of the value of the search after truth for its own sake, we cannot hesitate to associote it with the attainment of an uncommonly high point of real moral civilisation. Even when a gifted individual may have opened the way, yet it has usually required the lapse of centuries, and the occurrence of many such brilliant examples, before any similar taste could be at all diffused among the generality of mankind.

But again: with the best disposition, the extent and variety of the objects on all sides soliciting attention will rather distract the contemplation, than favour that concentration of the powers on the several parts, by which the gradual knowledge of the whole can alone be promoted.

"Man," (observes Professor Playfair,) "could not at first perceive from what point he must begin his enquiries, in what direction he must carry them on, or by what rules he must be guided. He was like a traveller going forth to explore a vast and unknown wilderness, in which a multitude of great and interesting objects presented themselves on every side, while there was no path for him to follow, no rule to direct his survey." *

But, amid all this variety of objects, the selection of those to which the attention should be directed, will hardly be a matter of deliberate choice. The mind will always be more powerfully excited by those phenomena which are rare, sudden, and imposing; and the curiosity will be dead to those which are daily surrounding us. Hence, in the earlier stages of knowledge, awe and wonder are first excited by the blaze of a meteor, the explosion of thunder, or the eruption of a volcano; but no admiration is called forth by the falling of a stone to the ground, or our vision of the face of nature. Surprise at length gives way to a spirit of curiosity: this in like manner is always at first directed to enquire into the causes of the most marvellous, rather than of the most common events; and it is little considered that it is from these last that we can alone reason to the causes of the first. When, however, in any case curiosity is once excited, there is always some hope that rational enquiry may in time follow; or, as an Italian poet expresses it, —

" La maraviglia
Dell' ignoranza e' la figlia,
E del sapere
La madre."

But as awe and wonder are the first powerful emotions called forth by the great phenomena of the natural world, it too commonly happens that from this source a very different spirit from that of rational curiosity takes its rise — the feeling of superstition. And, perhaps, of all causes tending to check the spirit of enquiry into the laws of the material world, and the causes of physical phenomena, none is more powerful or extensive in its operation. The origin of this impression would seem to

* Dissertation on the Progress of Science, p. 56.

be most expressively intimated in the incidental phrase of Pliny, occuring in a passage before quoted: — "ausus rem etiam Deo improbam;" — the notion of a sort of irreligious presumption in attempting to penetrate the mysteries of nature. As if those very mysteries were not the actual indications of Divine power; and the employment of our reasoning faculties in investigating them, the dedication of those powers to the highest purpose for which they are bestowed upon us.

And while superstition thus checks enquiry, it at the same time conjures up a train of miserable and pernicious delusions, which that very enquiry would be the most efficacious means of dispelling. The impressions so powerfully influencing the untaught mind are always (as we have observed) those made by the more uncommon and striking phenomena. But it is from patiently tracing the resemblances between the common events of nature, and those apparently more marvellous occurrences which are sometimes presented, that the advance is made towards a knowledge of their common causes; and towards a perception of that unfailing *unity* of design, and *uniformity* of operation, which constitute the firm basis of a rational notion of the *one* great First Cause, and, by consequence, of all enlightened religion. But the ignorant mind, giving way to the momentary impression, usually one of terror rather than admiration, created by the sudden manifestation of some rare phenomenon, in this way soon conjures up for itself all the phantoms of a blind superstition. Untaught how to connect real causes and effects, the votary of this delusion soon begins to invest with such supposed connection circumstances the most entirely separate. Men, incapable of understanding the effect of the moon on the tides, were firm in the persuasion of the influence of the stars on their fortunes. They would listen with overwhelming awe to the response of an oracle, and bow with adoration before the healing virtue of a charm or a relic, while they could not hear the great truths which the universal voice of nature proclaims to them, nor trace the divine hand in the organisation of created beings. Nay, these very superstitions forbade them to enquire; and repressed them in the desire, if they entertained it, of rising to higher and more worthy contemplations.

Thus, then, to furnish the first philosopher there was needed a singularly happy combination of dispositions and capacities; a mind capable of seeing the value of enquiry, as such; of taking the best point of view from which, as it were, to contemplate the vast complexity of objects presented; of assigning to each class of them, whether more or less imposing at first sight, its due importance; and lastly of seizing, at least, some of the leading analogies and relations between them. This, of course, in its full extent is more than can reasonably be expected in any one individual even in more advanced stages of science; much less in its infancy. In fact, the utmost we could look for would be that some luminary might arise on the intellectual horizon, sufficiently powerful to cast a general, though not brilliant lustre over the region to be explored. It must be reserved to the accession of more numerous and shining lights, to throw out the different parts and objects of the scene in full relief, and to invest them with their proper colours.

Precisely such harbingers of science were Thales, Pythagoras, and others to whom we have referred. In their schools, of course, no system of minute observation, no exact experimental results, were to be expected; nor, we may add, at this stage of science were they required. What was wanted was the power of a master mind to impart some great comprehensive principles, and give the right turn and direction to philosophical contemplation; to indicate what were its proper objects, and what the reasonable extent to which it might hope to push its enquiries; and thus to give the right tone and spirit to its character. And we may well suppose the world must have existed, and society have gone on, for many ages before such men appeared.

The great merit of these distinguished men was, that without the advantages of experiment, and availing themselves but in a limited degree of the rude observ-ations of their predecessors, they should, by a sort of gifted intuition, have struck out those grand conceptions of many of the leading analogies of the natural world, to which all the exact enquiries of after ages have administered increasing demonstration. Such happy combinations of intellectual character are, however, of the most extreme rarity in the course of ages. And nothing can be less a matter of surprise, however it may be of regret, than that these lofty imaginings were not followed out by the succeeding philosophers.

There was soon evinced a disposition towards the sublime but unsubstantial mysticism which sought to connect every thing in nature with some high and imaginary archetype existing in another intellectual world, whence emanated the essence of unity, order, and harmony. So long as this sort of speculation was confined to its own elevated region, and the philosopher who might indulge in it, yet did not suffer it to intrude on the province of physical observation, or the exactness of mathematical demonstration, it did not impede the progress of physical truth: perhaps even exercised a salutary influence in tending to keep it unmixed with objects and pursuits of a merely mechanical kind. Such, we apprehend, was the genius of the Platonic school. The mischievous effect produced by such a principle was rather that of withdrawing men from the more laborious study of facts by the fascinations of its eloquent mysticism, than of perverting the course of physical enquiry, or corrupting its methods with technical fallacies. It was altogether too much involved in the regions of mystery to mix itself up with researches connected only with the world of matter.

The classification of the innumerable objects presented to our view is an indispensable step towards enabling the mind to grapple with their examination at all. Thus, species, genera, and orders, become the fixed and permanent objects of knowledge, though individuals are subject to perpetual change. By this effort of mental abstraction, man produces, as it were, within himself a new intellectual creation; and this work, great and abstruse as it may perhaps appear, is yet actually more or less involved in the commonest comparison of things, and in the use of the very terms of which civilised language is formed. There are departments of enquiry of a perfectly abstract and intellectual origin; such as pure mathematics; others, which essentially require only some one or two simple principles or data derived from our experience, on which, without extraneous aid, it is their professed design to construct a system; such as logical and ethical science: in these the process of classification begins from the first principles adopted, and descends to individual cases and particular facts. In other branches, on the contrary, which originate in observation and collection of facts, the classification begins from the comparison of facts, and must be guided wholly by the nature of their different varieties of which it takes cognisance: from these it ascends to the more general facts and comprehensive laws.

The leading principle of the Aristotelian philosophy was precisely that of the former of these two classes. Hence, in its proper and legitimate province, it attained, in the hands of its great founder, to the highest perfection: and the systematic forms which it gave to dialectical logic, or the theory of conclusiveness; to ethics, or the theory of morals as deduced from some high first principles of the nature of moral good; and to other subjects admitting of the same mode of investigation, have to the present day commanded the admiration of all capable of appreciating them; and have retained their pre-eminence amid all the boasted improvements of subsequent ages.

The same principles, however, were wholly inapplicable to physical enquiry; the very circumstance which gave them their peculiar value in moral science obviously rendered them so, and the main fault in the Aristotelian physics was precisely not bearing in mind this distinction. The mind of that great philosopher was cast in so purely abstractive a mould, that he could proceed upon no other model to fashion his system of nature. It was not, as is often incorrectly alleged, that he neglected to collect facts, for he was most industrious in doing so; but he did not consider the collection of facts to have any reference to that scientific classification of ideas, which he made the groundwork of all his theories. Hence he presents us with a mass of statements of facts, more or less correctly determined, and of natural phenomena observed with more or less accuracy; but these have no correspondence with the abstract principles which he lays down as the groundwork of his reasoning.

For illustration take his definitions (if such they can be called) of motion, and of light. "Motion is the act of a being in power, so far as it is in power," and "light is the act or energy of a transparent body, inasmuch as it is transparent." That is, he first assumed the notion of body or being, then that of act or energy; things were to be considered either as in act or energy, or in capacity or power: motion then would be the peculiar characteristic of body considered as in energy; light again, (which, as we observed before, he regarded as not material, but as the result of some kind of impulse,) was that sort of energy which is characteristic of transparency. By this sort of systematising from metaphysical distinctions and the primary adoption of some vague generalities, such as the repugnance of nature to all motions but those which are natural, the abstract perfection of circular motion, &c., was the whole theory of the peripatetic system constructed; and to attempt to square nature to these imaginary principles was alone considered to be true philosophy.

The seeming irregularity and confusion which at first sight appears to pervade the multifarious phenomena of nature, and to render hopeless all attempts to analyse them into any sort of order, must, doubtless, have been one of the most discouraging circumstances attending the first endeavours to enquire into the laws of the material world. The Aristotelian philosophy tended to increase the difficulty, by representing all things as full of irregularity and confusion, and the principles of heat and cold, moisture and dryness, as in a state of perpetual warfare; thus the motion of bodies on the earth's surface was considered to be of a nature essentially different from that of the heavenly bodies; all attempts, therefore, to refer them to common laws were supposed useless. Perhaps, indeed, it may be a fair question, whether this part of the system is not really consistent with the true principles of inductive philosophy; for certainly we are not warranted in inferring the existence of order and connection, until we have at least traced some indications of it; nor could the celestial and terrestrial motions be entitled to be classed together, until we had some experimental grounds for doing so: they were properly set down as each a thing "sui generis," until they could be shown to be referable to one common "genus."

The ancients, then, were wanting in the power of tracing the relations between one class of facts and another, and this could only be attained by the extended use of *observation*, on the one hand, in all cases which presented themselves beyond the reach of our powers to control; and on the other, by carefully making repetitions and new combinations, in a word, *experiments*, on all cases within our command.

Now, in many departments of observation, we have seen, they were not only not deficient, but eminently diligent and successful; those classes of facts, therefore, they were able to trace with accuracy, but they failed in perceiving their relations to others. This was doubtless occasioned, in part at least, by the imperfection of much of their process of observation, and the almost total absence of experiment. The contrast is extremely remarkable between their astronomy and their dynamics; between the elaborate care with which they determined the motions of the celestial bodies, and their absolute neglect of examination in reference to those on the surface of our globe. In the former case, they made very correct observations; in the latter, they satisfied themselves with noticing a small part of the facts and imagining the rest, and knew just enough of the circumstances to be misled by them; they were assiduously awake to the motions of the heavens, but dreamed over those of bodies on the earth.

The former case was a matter of *observation*, hence it was successfully elucidated, as far at least as their powers and means of observation extended: again, there were not wanting some other classes of facts, even on the earth's surface, to which observation was applicable, and which they did accordingly observe.

In general, they noticed the facts which offered themselves, and even brought some of them under the dominion of geometrical demonstration, but they did not attempt to make any new combinations, and thus to trace the cause through new modifications of the phenomena; in a word, they made *observations* but not *experiments;* they received the information which nature spontaneously furnished, but they did not question her for more. This prevented them from tracing the relations between one class of facts and another; and particularly was this the case with regard to the science of motion.

The motion of bodies on the earth's surface could not be investigated without special experiments instituted on purpose. No regular cases of its occurrence presented themselves to be watched and recorded by the observer; its laws, therefore, remained completely unknown; and, without that essential preliminary, no step could be taken towards assimilating terrestrial with celestial motion. But, in referring to the deficiency of experimental enquiry in the ancient physics, it ought, in the first instance, to be borne in mind what small means they possessed for conducting it; and we should recollect, that to experimental research an advanced state of the *arts* is absolutely requisite. This consideration alone would, in a great degree, account for the little progress made by the ancients in such enquiries.

But, besides the mere power of making experiments, there must exist a correct appreciation of their use, and a capacity to reason correctly from them. Now, in this point of view, instances are certainly not wanting among the ancients, of philosophers who have evinced the most correct apprehension of the nature and value of inductive reasoning; and, where they did confine themselves to arguing from the facts, their conclusions were usually just. They did not, perhaps, advance to any high generalisations, but their inductions, as far as they went, were sound and legitimate. Unfortunately, however, this was by no means the common case. If the limited extent of experimental enquiry had been the only cause affecting the progress of the ancient physics, and their philosophers had all made full use even of those means which they did possess, we should have had a very different picture presented to us of the state of natural science. We shall find the most powerfully efficacious cause of the deficiency, in the prevailing *misconception of the entire nature and scope of physical enquiry.*

The almost universal error seems to have been, that of recognising hardly any connection between observed results and philosophical theory. The boundaries between truth and fiction, between fact and hypothesis, seem to have been habitually transgressed without censure or hesitation; or, rather, such distinctions seem never to have been clearly marked or understood. The facts were not studied with a view to the formation of the theories; nor were the hypotheses constructed with the object of representing the facts. The idea of philosophical theory was imagined to involve something quite remote from all connection with matter; it was rather some law emanating from an elevated intellectual principle, to which it was imagined all the affections of gross matter ought to conform themselves. By the disputants of the schools, the *object and aim* of natural philosophy was mistaken; they looked to the maintenance of certain metaphysical principles, of far too abstruse a nature for any physical conclusions, however generalised, ever to reach. They attempted to support theories far too remote from all relation to the regions of sense and the order of nature, constituted as it actually is, for any deductions from those sources ever to establish: their speculations, therefore, remain a monument, on the one hand, of chimerical but lofty conceptions, never realised; and of unfinished attempts, on the other, which, if carried on, might have formed part of a substantial edifice of a far more humble, but yet more useful description.

The systems of the ancient schools were wholly *conjectural*, though they were in some cases right; hence science did not advance with the lapse of time. The state of opinion was often not only stationary, but retrograde. The earlier views were often the best: the reveries of Democritus and Anaxagoras were more just, and more like waking sense, than those of Aristotle and Ptolemy; but they were all dreams alike: there was, therefore, no sequence in them, and the later age could not profit by the experience of the earlier. The veneration which is felt for antiquity cannot apply in reference to these subjects; and even where we do perceive the attempt to found knowledge on experience, still, in that respect, the observation of Bacon holds good: — "Antiquitas sæculi juventus mundi;" or, "We are more ancient than those who went before us." The later age has had, at least, more opportunity of profiting by the accumulated results of the experience and observations of their predecessors. This applies, be it remembered, only to those branches of knowledge which rest upon observation. Those, on the other hand, which, like pure mathematics, are properly matters of original invention and abstract speculation, have no such necessary connection in the order of time: and here, accordingly, the highest praise of the ancient philosophers will ever be considered to rest.*

* On this whole subject we refer our readers to the CAB. CYC. "Introductory Essay on the Study of Natural Philosophy," p. 104. et seq.

PART II.

THE PROGRESS OF PHYSICAL AND MATHEMATICAL SCIENCE FROM THE MIDDLE AGES TO THE TIME OF NEWTON.

The decline and dissolution of the Roman empire forms an epoch not more remarkable in the civil and social, than in the intellectual history of mankind. We have observed how entire and universal was the neglect into which all philosophical pursuits and physical enquiries had fallen, during the later portion of those ages which are assigned to ancient history. Here, then, one grand and well defined period marks a corresponding division in the narrative we are attempting to trace; and, as far as Europe is concerned, a long and unhappy interval of general darkness and ignorance succeeded, into which the light of improvement only began to shoot a few tremulous and uncertain rays about the tenth century. Meanwhile, in the East, the great events of this period were accompanied by at least a partial cultivation of science: this, perhaps, might be classed as following in the train of the ancient philosophy; but, nevertheless, we shall prefer considering it under this division, since it had a much closer connection with the modern revival of science.

We shall find, in this period, the following convenient divisions: — The first will comprise the state of science during the middle ages, and its first revival extending to the end of the fifteenth century. The second will include its progress during the next age, as far as to comprehend some of the first grand advances beyond the science of the ancients made by original modern research — the age of Copernicus and Tycho. The third will embrace the great discoveries of Kepler and Galileo. The fourth will be occupied by the delivery of the Baconian philosophy, and the researches of the increasing phalanx of philosophers, who, as it were, prepared the way for Newton during the earlier and middle part of the seventeenth century.

SECTION I.

THE SCIENCE OF THE MIDDLE AGES, AND ITS FIRST RENOVATION, TO THE END OF THE FIFTEENTH CENTURY.

Science in the East during the Middle Ages.

During the period of the extinction of science in the Roman empire, the various branches of natural and mathematical knowledge which had long before been cultivated in the oriental regions, though they were not altogether lost or neglected, yet made no progress, and were pursued chiefly by persons of low attainments, and in connection with the arts of astrology and alchemy. Just, however, at the time of its lowest ebb in Europe, the tide of science made a considerable advance in the East. The Arabians had at all times evinced some disposition to astronomical and mathematical studies; and when their warlike tribes had pushed their conquests over a large part of these regions, and had finally established the Arab empire in the East, the throne was occupied by a succession of princes who patronised and encouraged learning. Their conquests put them in possession of the works of the Greek philosophers, which they held in high estimation, and caused translations of them to be made into Arabic. With these aids, as well as the resources of science which they possessed among themselves, the Arabian philosophers devoted increased attention to the cultivation of the mathematical sciences, and particularly astronomy. The prevalence of this taste dates itself from the time of the Caliphs El-Mansour and

Haroun-el-Reschid, a little before A.D. 800; and was at its height under El-Mamoun, soon after that period. This last sovereign ordered a new measurement of the length of a degree, which we are told was performed in the plains of Mesopotamia; but the result is lost to us, owing to an uncertainty about the kind of measure employed. The great work of Ptolemy was also translated into Arabic by his command, under the title of Almagest, A. D. 827.

The two centuries following this æra were fruitful in astronomers, who particularly devoted themselves to accurate observation: the records of their labours are of great extent. Among these Albategnius (or El Batani) discovered the motion of the point of the sun's apogee. He also corrected Ptolemy's determination of the precession of equinoxes.

Ibn Junis made his observations at Cairo about A. D. 1000. It appears that about this time the Arabian astronomers were aware of the properties of the pendulum, and employed it in their observations as a measure of time.

In trigonometry, the Arabian mathematicians certainly introduced some improvements. The most valuable, perhaps, was the substitution of the sine in their calculations instead of the chord used by the Greeks. Aboul Wefa made tables of tangents and cotangents about A. D. 1000. They do not seem to have used the cosine till about a century later, when Geber, a Mahometan Spaniard, gives a formula into which it enters.

It may indeed excite our surprise that such apparently obvious improvements should not have been introduced earlier; and that the speculative genius of the Greek geometry, even without any reference to the practical use of trigonometrical formulæ for astronomical purposes, should not have pursued the innumerable curious relations they present, as matters of abstract contemplation. That the mathematicians of Greece did not do this is, perhaps, to be traced partly to the fastidious dread they always appear to have entertained of mixing up their geometry with any thing having the appearance of a relation to *numerical* computation; and it was with an object of this kind in view that trigonometry first presented itself to them. Partly, again, the sort of investigation was in itself of a new class, and of a kind which, perhaps, would not exactly harmonise with the style and manner adopted in their demonstrations.

Yet the whole science is nothing but a continued and highly elegant application of one simple idea, that of *giving names* to the several ratios subsisting between the sides and angles of a right-angled triangle. The ratio of one of the rectangular sides to the hypothenuse is called the *sine* of the angle opposite to it, while that of the other side to the hypothenuse is the *cosine* of the same angle. Then, again, that of the sine to the cosine, is named the tangent; its inverse, the cotangent. These simple *names* imposed upon ratios often recurring, added to the idea of applying the triangle in different positions, so that its hypothenuse is always the radius, and its sides coincident with the diameters of a circle, whose arc measures the angle, are the elements out of which the whole superstructure of the science is directly reared; and from such simple principles a machine is constructed, which, in the hands of the skilful analyst, has exceeded in power perhaps every other invention of geometry, and has mainly achieved the triumphs of the mathematician in every department to which his science has been applied.

The etymology of the terms above defined has been a subject of question, especially as bearing upon the origin of the science. Some writers have contended for the derivation of the term "sine" from the Arabic. And though others have dwelt upon the Greek and Latin etymologies of the arc, or bow, its chord, and the sagitta, or arrow (a name sometimes applied to the portion of the diameter intercepted), as giving a sort of fanciful description of the diagram, yet, certainly, no meaning of the word "sinus" will very aptly apply to the case.

The Arabs seem to have evinced a considerable taste for the cultivation of algebra. The origin of this science has been traced to India; and it seems very probable that the Arabs derived their knowledge of it from nations further to the East. Their writers mention that Mahomet Ben Musa of Chorasan, (about A.D. 959) distinguished among his countrymen for his mathematical knowledge, travelled into India for the purpose of acquiring further instruction in the science of algebra. The information we have of late years received respecting the algebra of the Hindoos, tends to give it a distinct character of originality.

Though we are here speaking of science as flourishing in the East, we may yet include in the description the Moorish empire in Spain; under which we find, among the most distinguished ornaments of Arabian science, Alhazen, who flourished in the eleventh century. His principal production is a treatise on optics. Before the rediscovery of Ptolemy's work on the same subject, it was supposed that Alhazen's was in a great degree copied from it. The originality of Alhazen, and, in many respects, his superiority to Ptolemy, are now fully admitted; his applications of geometry are of a very refined and skilful description. He suggested the explanation of the apparent magnification of the heavenly bodies near the horizon, before referred to. He treated of refraction; and gave an explanation of the cause of twilight. His investigation of the atmospheric refraction as applied to astronomy is the only point, perhaps, in which he is inferior to Ptolemy. He appears to have thrown out some good conjectures on the optical structure of the eye, and remarks, that when corresponding parts of the retina of each eye are affected, we see but one image. His writings are prolix, and without method.

The Tartar conquerors who succeeded the Arabs in the East, were also zealous promoters of science, especially of astronomy. The grandson of Gengis-khan founded an observatory in Persia, and furnished it with the best books and apparatus. Under his protection the astronomer Nasseer-ad-deen flourished, known by his tables and other works (about A. D. 1220). Among the successors of Timour, though at a considerably later period (A. D. 1440), Ulugh Beg, sovereign of Samarcand, established one of the largest observatories recorded in history, and fitted it up with various instruments, especially quadrants of very great radius. With the help of many scientific assistants he published elaborate collections of tables, and a catalogue of the stars. Their accuracy has been admitted to be considerably superior to that of the Greek tables.

The same patronage was extended to science by the successors of Gengis-khan on the throne of China. The thirteenth century may be considered, owing to this cause, and the importation of the astronomy of the western nations, as the most brilliant epoch of Chinese science.

The subject of the quadrature of the circle engaged the attention of the Arabian mathematicians, who carried somewhat further the approximation by which Archimedes and Apollonius had expressed the ratio of the diameter to the circumference. They gave the ratio as that of 1250 to 3927, which is a very near approach. Another value in still simpler terms, and nearly as accurate, was afterwards given by Metius, which is easily remembered, from its containing only the first three odd numbers, each repeated twice in order; viz. 113 to 355.

This subject is, in fact, one which has continued to exercise the speculations of men in a most singular degree in all ages. It seems to stand forth as an exception to the general truth of the indifference manifested towards the abstract researches of geometry. Here we have a problem which, as far as it is connected with any practical application, has ages ago received such a solution as to suffice amply for all purposes of computation in which it can be introduced; and which later mathematicians have found means of expressing, by a decimal continued to any number of places of figures which the accuracy of our calculation may demand: a problem undistinguished in this respect by any peculiarity, but which stands only as one among many others of the same class, and in the same predicament. And yet, without any conceivable practical object in view, it has in all ages, since its first introduction, obtained an unexampled celebrity even among the most ignorant classes; and has called up a host of investigators who have devoted themselves to its solution with an ardour absolutely bordering on insanity; and which in many instances, if it did not commence, actually terminated in mental aberration. Entirely ignorant of the real object to which such an enquiry ought to be directed, they have pursued their pretended solutions by almost any path except that to which the very conditions of the problem restrict it, viz. strictly geometrical demonstration by elementary methods. That to attain this is absolutely impossible, we have every degree of assurance short of actual demonstration. It certainly has not been *demonstrated* that the circumference and diameter of a circle are incommensurable; but every argument from analogy and probability leads us to believe it. By other methods than those of strict geometry the problem is solved, and the pretended methods are therefore superfluous.

We shall see at a future stage of our enquiry how much good has been done indirectly by those discussions; in that, from the agitation of the question, real mathematicians have been led to examine the various forms and laws of the series in which the approximations to these and other values of the same kind may be carried on. But the fame which the problem has acquired among ignorant empirics has probably arisen from some connection it was supposed to have with we know not what mystical secrets; hence the persevering diligence with which they have been always going on proposing, one after another, every variety of childish conceits, dignified with the name of demonstrations: some measuring the length of the line gone over during one revolution of a wheel, others emptying the contents of a cylindrical into a cubical vessel; one cutting out the areas and weighing them, another turning the four quadrants of a circle inside out; some finding the quadrature in the stars, others in the mystical number of the beast in the Apocalypse.

This mighty secret, and that of the philosopher's stone, the mysteries of alchemy, and, above all, of judicial astrology, appear to have obtained considerable influence in the ages of which we are treating, especially among the Arabians. Indeed the astronomy of this people, zealously and ingeniously as it was pursued and cultivated, would appear to have been generally associated with astrology, and made subservient to it. The whole theory of its predictions was built upon the configurations of the planets with respect to the sign of the zodiac which was rising at the moment of an individual's birth (and which was called his horoscope). This, of course, depended on a tolerably exact knowledge of the planetary motions; and, to construct the horoscope for any person, required tables of those motions, and a facility of calculating the places of the heavenly bodies at any given epoch. There can be little doubt that the belief in these absurdities produced at least one powerful motive for the cultivation of astronomy, both in the age of which we are speaking, and even long afterwards among the Europeans.

The occult arts had found their way into Europe at an earlier period, and various circumstances conspire to show that they originated in the East. Astrology was certainly unknown in Greece before the expeditions of Alexander; and it appears to have first made its appearance in Rome in the time of Augustus, when an immense influx of Orientals into the capital took place, as we well know from the satirical remarks of Juvenal.

From that time downwards, even to the seventeenth century, the whole of the civilised world in the West, as well as the East, was enslaved by this extraordinary and superstitious delusion; and, even at this day, we have hardly got rid of the traces of it from some of our almanacs.

Science in Europe in the Middle Ages.

During the middle ages in Europe, the cultivation of science had sunk to its lowest ebb. Not only was there no original energy displayed in enlarging the stores of knowledge, but there did not exist even the desire to retain what had been already known. There were no writers able to add to the stock of information, and few readers even capable of informing themselves of what their predecessors had done. The records of ancient learning and philosophy were a sealed book, from the total ignorance which prevailed of the Greek language; and though there existed a few Latin versions, or rather compendiums, of one or two of the treatises of Aristotle, and perhaps of some other works of science, either derived from the oriental translations, or possibly from the remains of the literature of the Roman empire, yet these were little read, and still less appreciated or understood. The little knowledge which existed was confined exclusively to the clergy and the monks; and if, in any instance, this extended to more than what barely sufficed for the discharge of their ecclesiastical functions, the cases were rare exceptions. The only philosophy professed was an adherence to the dogmas of Aristotle; and, probably, this profession was made by numbers who knew no more of his opinions than a few technical phrases, and little of his works except their titles.

The monastic establishments were, however, the sanctuaries which afforded an asylum to learning, such as it was; and, unquestionably, whatever remains of ancient literature have been preserved to us, we owe to them.

In ages like those of which we are speaking, when nothing but universal barbarism, tumult, and bloodshed prevailed around, the monasteries were generally held sacred by all parties; and amidst the ignorance which extended without, the more powerfully did superstition guard these sanctuaries from spoliation. In the libraries attached to these institutions, the manuscripts of the ancient writers were at least preserved from the violence of military plunder, and the wanton devastation of hostile incursions. The works of the philosophers, and the precious records of the mathematical and astronomical discoveries of past ages, might here at least have reposed in undisturbed security from external injury. But, unfortunately, they were not safe from destruction of another kind; and there can be no doubt that numbers of these invaluable remains of antiquity perished in supplying materials to the monkish transcribers, who ruthlessly erased the pages of ancient science and literature to insert the legends of the saints. In process of time, probably, they would all have met with this ignoble fate; but before that period could arrive, the dawn of a brighter age had begun to appear: and a few individuals arose among the monastic orders superior to the general character of their brethren, who were at least sufficiently aware of the value of some of the treasures contained in these repositories, to rescue them from such unworthy and barbarous destruction.

The mere act of preservation of these precious remains of antiquity, is one which must entitle those who performed it to the gratitude of posterity, and would alone suffice to give them a claim to the title of enlightened and discriminating men, superior to the general intelligence of their contemporaries. But between a bare apprehension that these volumes possessed some value, and the taste and ability requisite to appreciate justly their contents, there was, doubtless, a considerable interval. And, indeed, where such taste existed, there were still many difficulties in the way of its cultivation; for the work of completely laying open the treasures of ancient science, much time and much subsidiary knowledge were necessary. The repositories of these valuable stores were to be made accessible; the knowledge of the language in which they were written was to be acquired; the manuscripts were to be decyphered; and the critic and the grammarian had to apply their skill, before the curiosity of the geometrical or astronomical student could be gratified. This must, under any circumstances, have been a long and laborious task; and peculiarly so in those ages, when so little assistance or encouragement could be looked for in any quarter; or rather, when every thing seemed to wear an aspect hostile to the prosecution of such labours.

The dominion of the Moors in Spain (which commenced in 713) appears to have been one principal channel through which the science of the East found its way into modern Europe. After the Greek language had ceased to be known in the West, the translations of the ancient mathematicians into Arabic, which were introduced into the Moorish universities of Spain, became known to many who were totally unacquainted with the Greek originals. The crusades, also, probably had their share in conveying some portions of the science of the Saracens into the West. These oriental works, in most instances, were translated again into Latin; and through this channel alone many of them were known for ages, when modern research discovered the Greek originals. Of some, also, the originals have never yet been found.

Some improvement in the state of knowledge and taste was, however, gradually evinced; and, as the difficulties were diminished which stood in the way of acquiring a knowledge of the ancient authors, the number of those who had their curiosity awakened to the subject increased. To study these writings with profit, and to enter fully into the spirit of the ancient geometers, required minds already somewhat cultivated, and taste considerably superior to what had been generally prevalent in those ages. But the perusal of these works had a tendency to create such a taste, and to confer such skill; and thus, by a gradual reaction, the study of these records produced men of more enlightened notions, and more awakened intelligence, who were soon able to correct the faults of the manuscripts they copied, and to explain the difficulties of the authors they translated.

Thus was preserved to us all the knowledge we now possess of those highly interesting records of ancient philosophy, of which, in former sections, we have endeavoured to give some account; and thus, again, was the first impulse given to the further cultivation of science, and the wider extension of knowledge in modern times. Those to whom we are indebted for the renovation of the sciences in the middle ages, commenced their labours with the careful study of antiquity: this at once informed them of what had been effected before, and pointed out to them what remained to be done; stimulated their curiosity, and inspirited their efforts to pursue the enquiry; and supplied them with implements sufficiently powerful to enable them to conduct at least the commencement of the work with success. Of the individuals who first took a share in the great work of the restoration of science we know little: few even of their names have been recorded. One of the most eminent was Gerbert, a monk of the Low Countries, who lived in the tenth century; he appears to have been gifted with talents very much in advance of the age in which he lived. Impressed with an ardent desire to study the sciences, he found that so low was the state of barbarism and ignorance in which the monkish schools of Europe were then immersed, that he could acquire the information he sought nowhere but in the Moorish institutions of Spain; he here obtained a knowledge of ancient science through the Arabic translations; and, returning into France, composed several treatises which show an acquaintance with geometry and astronomy considerably above the usual standard of that age, and evincing a familiarity with the works of Euclid, and the inventions of Archimedes.

One of the most important services rendered by Gerbert to the cause of useful knowledge, was the introduction of the Arabic numerals, or decimal notation. Perhaps no single invention ever produced is at once more simple in its application, more refined in its principle, or of more universal value and utility than this.

Its actual origin, it is difficult to discover; and it has been traced by some to Arabia, by others to India. Gerbert certainly obtained a knowledge of it from the Moors. It is hardly possible for us to conceive the state in which every part of science dependent on numbers must have been without it; or to imagine by what means the ancient astronomers got through their calculations. The single circumstance of the want of this system of computation, would almost alone account for that absence of the application of mathematics to physical subjects, which so unhappily characterises the science of the ancients. Gerbert afterwards became pope, under the title of Silvester II.

In the eleventh century the example of Gerbert was imitated by Adhelard, an English monk, who, for the sake of acquiring scientific information, travelled into Spain and Egypt, and made himself master of the Arabic language. He turned this knowledge to account, by translating into Latin from the Arabic, the "Elements of Euclid." This appears to have been the first translation of Euclid known in Europe; it, however, seems to have excited little attention, and is said to remain in manuscript at the present day. Another translation, likewise from the Arabic, made by Campanus about a century later, was the first which acquired any celebrity: all the early printed editions are taken from his text.

Establishment of Universities.

The institution of universities is a most important feature in the history of the renovation of science. It may be traced with considerable certainty to the eleventh century, though probably before that time some imperfect establishments of the kind were in existence in several parts of Europe. The only schools of learning hitherto accessible were the monasteries. To some of these, and to the cathedral churches, schools were annexed, in which the monkish literature and science of the times, such as it was, might be acquired.

In England, the reign of Alfred (commencing in 870) had been marked by many advances in civilisation. That eminent sovereign was not less conspicuous for his own attainments than for his munificent and enlightened endeavours to promote and encourage the acquirement of learning among his subjects. To what precise extent he actually completed any establishment of the nature of an university at Oxford, is a matter of question among antiquarians; but it seems to be admitted that he certainly made some attempts towards such an object.

The tumults and wars of the next 300 years effectually interrupted the course of improvements, and nearly extinguished the little learning which had begun to appear. Some of the Norman sovereigns, and especially Henry II., were distinguished for their patronage of letters; and it seems to have been under their auspices that the universities of Oxford and Cambridge first effectually assumed the form of privileged seats of learning and science. It is, perhaps, impossible at the present day to decide which of these two has the priority in date.

It appears to be generally admitted that in this age, or soon after, they had both, from local circumstances, become favourite places of resort for study, and had acquired an extensive celebrity from the great eminence which even a very moderate share of learning conferred in those days on the scholastic recluse. The numbers of those who followed this mode of life seems to have been almost incredibly great, if we can trust the records of those times; and the names of several individuals have been handed down who enjoyed a high reputation among their contemporaries as teachers in the different departments of letters and philosophy.

To the celebrity of such teachers, and the incitements to study held out by the habits of life adopted in these places, and the stimulus of companionship and rivalry in the pursuit of knowledge, we may most probably trace the increasing resort of students to these seats of learning; and from similar causes originated the subsequent institutions which gradually took their rise, and at length compacted the whole into organised bodies. As the students began to increase in numbers, something like a regular system soon sprang up as necessary for the good order of the communities which were formed. The students lived together in certain houses which acquired the distinctive appellation of *halls*, each of which had its own internal regulations, while they attended in common the public lectures which were given by any who could command an audience. Some kind of system was soon found necessary; some distinction between *scholars* and *masters* was obvious; some regulations as to the course of instruction became indispensable; some exercises to show the proficiency of the students were naturally exacted. Thus originated degrees, professors, and disputations.

Colleges were afterwards added: they were founded on the model of the religious houses, and were endowed usually for the sole purpose of the maintenance of a certain number of persons termed fellows and scholars, under the government of a head, who were to devote themselves to the cultivation of literature, and conform to the ordinances of their founder, very much in the same way as the monks of the different orders were under similar obligations to the religious observances of their societies. The colleges were, in several instances, established upon the basis of the original halls, and thus, in many cases, the names became synonymous. Some of the colleges afterwards began to admit within their walls independent students, besides the members of their foundation.

The public lectures, exercises, and disputations of the universities were carried on in appropriate public buildings called schools. To these public libraries were subsequently added; and, in process of time, a certain regular system or course of studies became fixed, and was distinguished into several *faculties*, in which the distinction of a degree was granted. The faculty of arts was understood to comprehend the whole of the liberal sciences as then known ; after passing through this the student might advance into the superior faculties of law, theology, and medicine.

Remarks nearly similar will apply to the probable origin of the other universities of Europe. It was not till from various causes they had become places of resort for learning, that they were regularly constituted as universities, and had privileges conferred upon them by the sovereigns of the respective countries in which they were situated. Thus, Paris had probably been long a seat of science before it was partially organised as an university in 1101, and subsequently more fully recognised, and a charter granted to it, by Philip Augustus about 1200. It long maintained a more distinguished reputation than any other European school. The universities of Bologna, Padua, Salamanca, and a few more, trace their origin to about the same date. Various others, especially those of Germany, were erected about a century later. The original plan and constitution of all these universities bore a general resemblance, as to the faculties recognised, and the degrees conferred. The annexation of colleges appears to have been almost peculiar to the English universities ; and has since, probably, exercised a considerable influence over their condition, and the progress of science in this country.

The studies pursued in the universities of Europe, from these ages down to a comparatively recent period, comprised hardly any thing but servile comments upon Aristotle ; and the school exercises consisted of frivolous verbal disputes raised with endless ingenuity out of every subject which could supply matter capable of being so perverted ; each branch of knowledge being valued precisely in proportion to the degree of perplexity in which it could thus be involved. The most favourite subjects were, of course, metaphysics and theology.

Students absorbed in such puerile discussions were not likely to advance far in the acquisition of sound knowledge, nor to do much towards promoting the discovery of new truths. We have, however, in these times one brilliant example of a student and a monk who rose superior to the prejudices of his age, and became the harbinger of a more enlightened epoch.

Roger Bacon.

Roger Bacon was born near Ilchester in 1214. At Oxford he made distinguished progress in the usual course of literature and the school logic. He subsequently went to complete his studies in the university of Paris, then in such high repute as to attract students from all parts of Europe. He here obtained the degree of doctor, and laid the foundation of his extended celebrity. Returning to England he took the habit of the Franciscan order, and pursued his enquiries at Oxford into a vast variety of subjects, far beyond what then constituted the usual range of study. His ardour in the pursuit of knowledge led him to attempt new investigations in almost every department which he studied; and with the assistance of various liberal patrons, whose favour his high reputation had now secured, he is said to have expended large sums in collecting books, and procuring and constructing apparatus, which he had devised for the prosecution of experimental enquiries.

He appears to have been well versed in mathematics, and in the theory of mechanics as then known; though it does not seem that he made any advances in these departments. In astronomy he has left indications of attainment far superior to those of his contemporaries; and pointed out the necessity for a further reformation of the calendar beyond the Julian correction, the same as that which has been since applied. In practical mechanics and in chemistry we have on record many of his actual inventions, and still more unfinished projects and speculations, many of which have been since realised. The principle of the composition of gunpowder is distinctly pointed out in his writings, though he never brought it into practice, actuated, as it is said, by motives of humanity. He describes clearly the diving bell, and various self-moving machines. He attained considerable skill in medicine; and wrote on various parts of moral, philosophical, and metaphysical learning.

His discoveries in optics have been the subject of much discussion; he certainly appears to have understood the theory of lenses. Alhazen had remarked that small objects, such as letters, viewed through a segment of a glass sphere, were seen magnified, and that the larger the segment of the sphere, the greater will be the degree of magnification. The spherical segment was supposed to be laid with its flat side on the letters or object. Bacon discusses the comparative advantages of segments, including a greater or less portion of the spherical surface; but though his arguments have been considered by some to involve certain errors, yet, upon the whole, what he has said is admitted to give a more general view of the subject than had hitherto been taken, and to prove sufficiently that he knew how to trace the progress of the rays of light through a spherical transparent body, and understood how to determine the place of the image. Dr. Smith, in his Optics, has endeavoured to show that these conclusions were purely theoretical, and that Bacon had never made any actual experiments on the subject. This has been controverted by Mr. Molyneux, who contends that Bacon was not only acquainted with the properties of lenses theoretically, but that he also applied them practically.

His knowledge, however, was clearly such, that if he did not himself reduce it to practice in the actual formation and use of lenses, he made known principles which could hardly have remained long without such application. The use of lenses, especially as spectacles, was certainly a very early invention, but in an ignorant age we cannot expect to find the origin of discoveries very carefully recorded, and to trace their beginning is often like attempting to pursue a river to its source, where we are soon totally baffled among the multiplicity of small streams which unite to form it.

Dr. Smith, after fully discussing all the evidence, is of opinion that lenses were in use for assisting vision about 1313, but that nothing further can be collected with certainty. Some passages in Bacon's writings, which were once interpreted as referring to the principle of the telescope, seem to have been completely misunderstood, and to contain in reality nothing of the kind. He seems unquestionably to have believed in the possibility of transmuting metals, and probably also in astrology; but was superior to his age in absolutely rejecting the pretensions of magic.

Roger Bacon's great merit is to be found, not so much in the mere details of his various inventions and projects, as in the bold appeal which he made to experiment and the observation of nature; he stood forth as the champion of unfettered enquiry, and vindicated the rejection of all the trammels of authority in matters of science. In an age like that in which he lived, indeed, there were few capable of profiting by his example and instructions; but there were not wanting those who were able to appreciate their value, nor again those who had good reason to dread the influence of such principles, and who accordingly took measures to impede their progress, and, if possible, to suppress their promulgation. A pretext was supplied in the allegation that Bacon's pursuits were allied to magic, though he had actually written a work expressly against that art; he was accordingly prohibited from reading lectures in the university, and was subjected to confinement in his convent. He had his eyes open to the corruptions and superstitions of the Romish church, and avowed the most enlightened views in recommending the cultivation of natural science, with the express object of leading men to more just conceptions of the true foundations of religion: this, of course.

drew down upon him the whole weight of ecclesiastical vengeance. The blow was indeed averted for a time, during the liberal administration of pope Clement IV.; who not only secured Bacon from molestation, but encouraged him to draw up a collection of his treatises; which was afterwards published, under the title of "Opus Majus," in 1266. On the death of that pontiff, however, the storm of persecution rose more fiercely against him: under various pretences he was repeatedly imprisoned, his books prohibited, and his doctrines condemned. At length, through the interposition of some powerful friends, he obtained a release; and returning to the pursuit of his studies, though now at an advanced age, he continued the composition of several of his works, the last of which was a compendium of theology; and died peaceably in his convent at Oxford, in 1292.

It is perhaps difficult for us fully to appreciate the merits of such a man as Roger Bacon, from the want of an adequate idea of the extreme ignorance with which he was surrounded; but the respect with which his memory has been popularly cherished, is a proof of the commanding elevation of his master mind: he also claims our sympathy as, perhaps, the first of the long list of the victims of ecclesiastical persecution, and as associated with that illustrious band of patriots to the republic of letters and science, who maintained the cause of intellectual and moral liberty against the odious encroachment of spiritual despotism.

Revival of Algebra, Physics, Astronomy, and Geometry.

The oriental origin of algebra seems unquestionable, but there has existed some difference of opinion as to the period when it first became known in Europe. The most probable account assigns a date at the very commencement of the thirteenth century. Leonarda, a merchant of Pisa, having made frequent visits to the East in the course of commercial adventures, returned to

Italy, not only enriched by the traffic, but also instructed in the science of those countries; he brought with him a knowledge of algebra, and two works of his, on that science, bear the dates of 1202 and 1228. The subject is pursued only as far as quadratic equations; the technical language is very imperfect, and as yet unformed, the quantities and operations being expressed in words at length, with the help of only a few abbreviations. The rule for solving quadratics by completing the square is demonstrated by a geometrical construction.

The polarity of the magnetic needle is described in some lines attributed to Guyot, a French poet, who lived about 1180; but some authors are of opinion that this description was actually written by Hugo Bertius, in the middle of the succeeding century: and it is generally believed that the compass was first employed in navigation by Flavio Gioja of Amalfi, about 1260. He is said to have marked the north with a fleur-de-lis, in compliment to the branch of the royal family of France then reigning in Naples.

The poet Dante, who flourished at the close of the thirteenth century, distinguished himself also in philosophical pursuits: and we find among his numerous works an essay on the nature of the elements.

Vitello, a Polish philosopher, commented on Alhazen; and improved on his writings in a treatise published in 1270. He examined largely the refraction in glass and water, and gave a table of the deviations at different incidences, but without any perception of the law which connects them. He conceived refraction to arise from a sort of impediment or retardation which the light undergoes in entering the denser medium; and reasoned upon an obscure idea of the resolution of force. He treated imperfectly of the rainbow, and of the eye, and showed, as indeed Alhazen had done, that vision is not performed, as the peripatetic philosophy taught, by emission from the eye to the object. He also gave a sort of vague theory of parhelia and halos.

In the thickest darkness of the middle ages, the study of astronomy had never, perhaps, been entirely abandoned; but it is almost certain that the encouragements to the pursuit of it were mainly derived from the popularity of astrology, and the assistance which that art derived from astronomical determinations. In those ages, the state of knowledge was generally characterised by the mixture of a few fragments of real philosophy and sound science in a vast mass of ignorant conceits and superstitious delusions. Astrology then shared alike the reverence of the multitude and the patronage of the great and powerful. During the thirteenth and fourteenth centuries, it was even publicly taught in the universities of Italy: and in particular at Padua and Bologna professors were specifically appointed to initiate the most apt and hopeful disciples into the mysteries of this sacred science. The effects of this infatuation unhappily survived for ages after the actual profession of it had been discarded; and it long continued to cast a sort of mysticism over the pursuits of legitimate science, by no means favourable to its due developement or extended progress: and even when the astrological art had been completely exploded, and had become in its turn the subject of reproach and ridicule, so close had the connection been, that astronomy sometimes shared in the contumelies cast upon the occult arts, and, in the apprehensions of many, above the lowest class, the claims of the one were by no means accurately distinguished from the pretensions of the other.

The thirteenth century may be regarded as the period at which we perceive the most decided symptoms of a reviving attention to literature and science. The knowledge which had hitherto been confined to monasteries, now began to spread into cities and courts. The barbarous monarchs of the dark ages were succeeded by princes who cultivated and protected letters. It is hardly to be doubted that the zeal displayed by some of them for the encouragement of astronomy originated in their reverence for the astrological art: yet we must view with considerable indulgence a weakness which produced substantial benefits to science.

The emperor Frederic II. was a prince distinguished as well by a very cultivated mind, as by a liberal patronage of learned men. To his encouragement we owe the first translation of Ptolemy's great work into Latin from the Arabic (Greek being as yet unknown in Europe); but the Arabic translation was far from being faithful to the original, and the work (executed by Gerard of Cremona) was consequently very faulty; yet it was of importance for promoting the knowledge of astronomy in that age.

To Alphonso X., king of Castile, science owes still more than to Frederic. His situation in a country bordering on that of the Arabs, who then occupied the south of Spain, was favourable for collecting about him able astronomers, who were then only to be found in the Moorish universities. He employed many of these learned persons in the elaborate construction of a set of astronomical tables, which were to be enriched with the observations of the centuries since Ptolemy. (A. D. 1250.) Notwithstanding the zeal and munificence of the king, however, these tables do not appear to have realised his design. One of their worst faults was the introduction of a peculiar inequality in the motion of the fixed stars in longitude, which had at that time obtained credit, and was called the *trepidation*. It was made out as a theoretical consequence from a peculiar motion in the eighth sphere of Ptolemy's system. It was for some reasons strenuously maintained as of high importance by the Arabian astronomers, though not recognised by Ptolemy himself; and was connected by the Jewish astronomers or astrologers with certain cabalistical mysteries. It had not, however, the smallest foundation in any thing actually observed.

Of Alphonso's own attainments in science we have no precise indications; but his often quoted remark on the complexity of the Ptolemaic system certainly indicates a mind superior to the prejudices of his age.

A comparison of the Alphonsine tables with those of Ptolemy really evinces but a very slight advance in the science during the eleven centuries intervening. Science, in fact, had not yet begun effectually to awake from the torpor in which it had so long lain. And it was not till nearly 200 years later that any very decided results of such renovation were perceptible.

The first dawn of improvement may be recognised in the labours and designs of George Purbach, professor of astronomy at Vienna, about the year 1450. He undertook to amend the astronomical tables then in use, and felt the necessity for an accurate translation of Ptolemy. Since the very faulty one of Gerard, another had been made from the Greek, by George of Trebizond, which appears to have been little better. Purbach, however, does not seem to have realised this design. He did effect some improvements in the tables of the planets; but the amendments thus introduced were, perhaps, more than counterbalanced by his adherence to the ridiculous notion of trepidation. The most essential service which he rendered to science was the formation of a table of sines upon a *decimal* division of the radius, now for the first time employed.

But these and other similar projects for the improvement both of pure mathematics and astronomy, were carried on with still greater zeal and success by Muller of Konigsberg (born in 1436), more commonly known by the latter appellative, latinized, after the fashion of the day, into the sonorous title of Regiomontanus. To him we owe many translations and commentaries, together with several original and valuable works. Trigonometry, which (as we have remarked) had never been known to the Greeks as a separate science, and which took that form in Arabia, advanced in the hands of Regiomontanus to a great degree of perfection, and approached very nearly to the condition which it has attained at the present day. He also introduced that simple but most valuable modification of the decimal notation, which consists in fixing the unit's place at any figure, and not necessarily at the right hand, by placing there a comma, all arithmetical operations going on just the same; in a word, the use of decimal fractions. This, indeed, gives the scale its full extent, and confers on numerical computation the utmost degree of simplicity and enlargement which it seems capable of attaining. The Latin translation of Ptolemy's Almagest, designed by Purbach, and perhaps commenced by him, was completely executed by Regiomontanus: who added also a commentary and a number of problems and illustrations. He greatly improved the tables of sines, and added one of tangents. He appears to have been persuaded of the reality of the earth's motion, though he did not advance to a recognition of the entire solar system. He constructed a planetarium, which is described as exciting great wonder. He also calculated ephemerides of the places of the planets for many years in advance. His skill in practical mechanics was such as to give him in that ignorant age a reputation for miraculous powers; and marvellous things are recorded of the automatons which he constructed. He certainly contributed much to the improvement of clocks in the imperfect form in which they were then made before the application of the pendulum. His high reputation induced pope Sixtus IV., to request his attendance at Rome to assist in a projected reform of the calendar. Arriving at that city, he had not been long engaged in the preliminaries of the work, when he was carried off by an epidemic disorder in the prime of life [1476], and was honoured with a public funeral in the Pantheon. He died amidst innumerable projects for the advancement of the sciences, which he had enriched by productions evincing a genius of the first order, coupled with the most earnest zeal in the cause, and an indefatigable perseverance in pursuing its most laborious details.

The labours of Purbach and Regiomontanus form the link between the astronomy of the middle ages and that of modern times. The end of the fifteenth century was not distinguished by any great discoveries; yet it is easy to see, in the rise of a spirit of enquiry and investigation, the dawn of that light which was about to illuminate Europe with so much brilliancy. The progress of such improvements was, no doubt, at first slow and uncertain; but all its subsequent advance may fairly be traced back to the original impetus given by the philosophers of the preceding age. These, indeed, were few in number, and ill provided in their resources, yet they were carried forwards by the irresistible impulse of genius; they began to feel the necessity of establishing the truths of science upon a firmer basis than the authority of Aristotle, and aspired to a higher character than that of commentators on Ptolemy.

Among those who contributed most assiduously to rearing the edifice of astronomy on the basis of observation, was Bernard Walther, a rich citizen of Nuremburg, one of the earliest and most zealous of modern astronomers. His observations, interesting on many accounts, are particularly remarkable for having been made with clocks regulated by fly-wheels, which seem to have answered tolerably well; at any rate the importance of such aid to the labours of the observatory was now generally beginning to be felt.

Walther was one of the first among the moderns who recognised the atmospherical refraction; though he had a very incomplete notion of it, and supposed it sensible only near the horizon: his observations on it appear to have been made independently of those of Vitello and Alhazen.

The capture of Constantinople by the Turks in 1452 drove many Greek cultivators of science to seek a retreat in Italy: here an asylum was afforded them, and the libraries of that country became the depositories of what remained of the treasures of Greek literature and philosophy. It was, probably, hence that the first stimulus was given to the study of the Greek language in Europe. Translators of the Greek authors, and commentators upon them, began to multiply; and the rapid progress of the art of printing gave an additional impulse, by the facilities it created for the dissemination of learning. Amongst these numerous writers we may particularly notice Venatorius, distinguished by his translation of Archimedes, and Commadine by that of Euclid, and various other ancient mathematicians, enriched with commentaries.

The Greek mathematicians, in particular, now began to attract the attention of students, and the value of their geometrical labours to be rightly appreciated. The elementary treatises had been already adopted as the basis of mathematical study; and the more difficult and abstruse parts of the ancient systems were now beginning to be studied with all the zeal and ardour which is commonly inspired by the acquisition of a new treasure, and the discovery of a fresh source of intellectual gratification.

We have before adverted to that beautiful portion of the ancient mathematics known by the name of the geometrical analysis: this may, perhaps, be fairly called the most valuable part of the ancient geometry, inasmuch as a method of discovering truth is more valuable than the particular truths it has already discovered. Unfortunately, but few works of the ancients on the subject have come down to us; and those few have suffered more from the injuries of time than almost any other remnants of their writings. Among the first of the moderns who turned their attention to the subject of these methods, and to unfolding the contents of some of the ancient treatises, was Werner, who lived about the end of the fifteenth century: his works are now very rare. He first studied the data of Euclid; and, from the elementary notions of the subject there imparted, he proceeded, without further help, except that of his own powerful and original genius, to pursue the principles of the system, and to apply it to the solution of several recondite problems. The work of Apollonius, entitled "Sectio Rationis," was known to him only in the Arabic; but perceiving its value, as coming next to Euclid's data in the order of elementary exposition of the subject, he entertained the project of translating it into Latin: it does not, however, appear that he executed it.

Though later in time, the connection of our subject leads us here to mention, that his steps were followed by Benedetto, an Italian mathematician, who continued the investigation of the subject, and published a treatise on the geometrical analysis at Turin, in 1585. Maurolycus of Messina, also, who flourished in the middle of the sixteenth century, and may be regarded as the great geometer of that age, furnished several translations from the ancients, and commentaries upon them: besides which, he wrote a treatise on the Conic Sections, of considerable value. He made an attempt also to restore the fifth book of Apollonius's Conic Sections, in which the subject of maxima and minima is treated. His writings indicate a man of clear conceptions, and of a strong understanding; though, in common with almost all persons of that age, he was a believer in astrology, and even an adept in the art.

One of the most eminent of the early cultivators of algebra, was Lucas de Burgo, a Franciscan, who, towards the end of the fifteenth century, travelled like Leonarda into the East, and was there instructed in the principles of this science. On his return he published a treatise on the subject, in which he advances a little in the art of notation; employing more abbreviations, and, in particular, using the letters p. and m. to stand for plus and minus, or the symbols of addition and subtraction: he also gives the rule for the signs in multiplication. Though, as we have already seen, Diophantus had advanced to this point of symbolical algebra, yet Lucas de Burgo arrived at it independently; since the writings of Diophantus were not discovered till the middle of the sixteenth century, when a Latin translation of the arithmetical questions was published by Xylander. Thus the origin of the algebraic notation was simply that of a short-hand writing, or abbreviation of ordinary language, to save trouble and repetition. This was the simple principle which, being more and more improved upon by each successive writer, was at length framed into that refined and highly artificial system which we have at present in use; and which has in its turn proved the means of indicating new relations of quantity; and, from a machine to assist the labours of our hands, has become a vehicle conveying us into new regions of truth. Thus, upon the whole, we may remark that up to the end of the fifteenth century, the advances made to emerge from the darkness of the preceding age were, with very few brilliant exceptions, confined to restorations of ancient science, and some immediate improvements which directly arose out of those restorations. A beginning, however, was effected, and a foundation laid; and, in the next century, we shall find abundant proofs of its solidity in the substantial superstructure which then began to be raised upon it, and shall trace the rapidity and success with which the work proceeded.

SECTION II.

SCIENCE IN THE SIXTEENTH CENTURY. — THE FIRST GREAT MODERN IMPROVEMENTS. — THE DISCOVERIES OF COPERNICUS AND TYCHO BRAHE.

Progress of Algebra.

THE knowledge of algebra had hitherto been almost wholly confined to what was acquired from the introduction of the Oriental treatises, upon which, as we have seen, some few improvements, rather in form than in substance, were engrafted. The next age, however, exhibited a more busy scene of research, and in this department original enquiry now actively extended itself, and new investigations were continually made into the essential principles on which the analytical relations of quantity are founded.

The first subject which arrests our attention on surveying the state of science in this period, is the theory and solution of equations. Those of the first and second degree, or simple and quadratic equations, were obviously and completely capable of solution in all possible cases. But it had long been felt that the moment an attempt was made to solve any of higher dimensions, as, for example, those of third degree, or cubic equations, (as by an adherence to the old geometrical analogy they are still called) there, except in a few obvious cases, no such general principle had been assigned as could lead to a solution. To this subject the early algebraists directed their researches.

Among those who were earliest in the career of discovery, and whose fame will endure latest, we may rank Cardan, who was born at Milan in 1501. His character was a strange compound of good and ill, of strength and weakness. With great abilities and industry, he was capricious, false, and vainglorious. And such was his infatuation in astrology, that he starved himself to accomplish the prediction of the day of his death.

Before his time very little advance had been made in attempts at the solution of any equations higher than the second degree: except that, about 1508, Scipio Ferreo, professor of mathematics at Bologna, had found out a rule for resolving one of the cases of cubic equations; which was made much more general by Tartalea of Brescia: but he kept his method a profound secret. The curiosity of Cardan was stimulated; and, after many entreaties, and binding himself by oath not to divulge it, he succeeded in extorting from Tartalea his rule, though without its demonstration. Cardan soon found out the proof, and extended the rule to a large class of cubic equations. The greatest share of the merit was thus his own; but this will hardly excuse him in proceeding to publish the whole (in 1545), utterly disregarding his solemn compact with Tartalea.

Science, however, was the gainer; and this theorem, known by the name of "Cardan's rule," at this day marks a point beyond which no efforts of subsequent algebraists have been able to carry them in the solution of cubic equations; and, indeed, but little success has attended their researches in the general exact solution of equations of higher degrees. Cardan pointed out the approximation to the roots of those cubic equations which his rule did not solve. He also clearly understood some of the leading principles of the roots, and the formation of the co-efficients, when we examine the forms of equations in general synthetically. This must have been more difficult than we can readily conceive, owing to the imperfection of the algebraic language. The usual practice of the day was to put the rules into verse. Cardan delivered his in a poetical dress. The unavoidable obscurity of such a mode far more than counterbalances any advantage it may give to the memory. Indeed, even in this respect, a symbolical formula must far surpass any versification.

Algebra was at the same time cultivated in Germany. Stiphelius, in a work on that science published at Nuremburg in 1544, employed integer numeral exponents of powers, both positive and negative; and introduced the same characters for plus and minus which we now use. In equations he did not go beyond the second degree.

Robert Recorde, an Englishman, of about the same date, published the earliest treatise on algebra in the English language, in which the sign of equality first appears.

The properties of algebraic equations were discovered very slowly. Pelitarius, a French mathematician, in a treatise bearing the date of 1558, first observed that the root of an equation is a divisor of the last term. He also remarked the curious property of numbers, that the sum of the cubes of the natural numbers is the square of the sum of the numbers themselves.

Bombelli, in Italy, wrote on algebra; and pointed out that the problems involving the irreducible case of Cardan's rule, admit of geometrical construction by the trisection of a circular arc. He also mentions a manuscript of Diophantus in the Vatican, in which the invention of algebra is ascribed to the Indians. Nothing of this, however, appears in the works of Diophantus as subsequently printed. This point remains without explanation.

Among the mathematicians of the 16th century, none, perhaps, has secured a more lasting reputation than Vieta, a native of Fontenoy, who is entitled by Riccioli the great ornament of French science. He was equally remarkable for industry and originality. He advanced the algebraic notation by the introduction of letters to stand for the known as well as the unknown quantities, so that it was in his hands that the language of algebra first became capable of expressing general truths. He delivered a rule for solving some cases of biquadratic equations. And in the case of an equation of any degree, complete with all its terms (that is, containing in separate terms all the powers of the unknown quantity up to the highest, by which the degree of the equation is designated); and when the roots are all positive, he discovered the relation between the roots and the coefficients of the terms. Thus was another step slowly taken towards the complete theory.

Vieta was not less celebrated for the improvements he introduced into trigonometry: and in his treatise on angular sections, made a most important application of algebra to the theorems and problems of geometry. He restored some of the books of Apollonius in a manner highly creditable to his own ingenuity, though not precisely accordant with the taste and style of his author. He also directed his attention to astronomical subjects, and submitted to Pope Clement VIII. a plan for reforming the Calendar. He is said also to have composed an important astronomical work, called "Harmonicon Cæleste," which being lent to a friend, was, by some unprincipled rival, surreptitiously taken from him, and destroyed or suppressed. His mathematical treatises were first published about 1590, and afterwards collected by Schooten in one volume, in 1646.

About the same period algebra became greatly indebted to Albert Girard, a Flemish mathematician; though his principal work, "Invention Nouvelle en Algebre," was not printed till 1669. This ingenious author perceived to a greater extent than Vieta had done, but still not in all its generality, the principle of the successive formation of the co-efficients of an equation from the sum of the roots; the sum of their products taken two and two, the same taken three and three, &c., whether the roots be positive or negative. He also conceived the notion of imaginary roots; and showed that the number of the roots of an equation cannot exceed its dimensions. To these speculations he added some others connected with the use of negative roots in geometrical constructions, &c. He also first introduced the phrase of "a quantity less than nothing," which has been so severely censured by some mathematicians, who cavil at terms without considering that they only become fair subjects of question when the wrong use of the *term* is likely to involve a mistake in the *thing*. This phrase, of course, never implies any thing more than the very conception by which negative quantities take their origin; the supposed increase of the quantity subtracted till it becomes equal to, and then greater than, that from which it is to be taken. The variable value of the whole is thus said to *pass through zero*, and then become *negative*; or, in this *figurative* sense, *less than zero*. That is, it becomes a quantity at variance with the first hypothesis; but which may still enter into our equation, if we change the signs of all the terms; or, which comes to the same thing, if the signs remain, and we allow the fiction of a quantity essentially negative. This is, in fact, one of those simple but refined artifices arising out of the symbolical language of algebra, which has been so mystified by persons of obscure conceptions, and ignorantly copied from one treatise to another, as to have become involved in real obscurity, and to have given ground for serious accusation against the science.

The greatest advance in this part of the science of quantity was effected by Thomas Harriot, who was born at Oxford in 1560. Having graduated in the university in 1579, he was employed afterwards in the second expedition sent out by Sir W. Raleigh to Virginia; and on his return published an account of that country. He afterwards devoted himself entirely to the study of mathematics and astronomy. His principal work is entitled "Artis Analyticæ Praxis," which was not published till after his death, in 1631. In this work we find, for the first time, the complete developement of the apparently very simple truth, to which Vieta, Girard, and others we have named, had been so long making their successive approximations; viz. the formation of general equations of all degrees, by the multiplication together of as many simple equations as amount to their dimension, or the highest power of the unknown quantity involved. This, of course, includes the case where one or more of such factors is an equation of a higher degree than the first, but where, in general, the number of factors is such that the sum of their dimensions is the dimension of the resulting equation. The slow progress made in the developement of this principle, which, when understood, does not appear to be of any very abstruse or difficult kind, is a remarkable fact in the history of discovery. It would seem as if these labourers in science had been working with, and gradually improving upon, an instrument whose full powers they did not yet understand. It is to the further extension of those powers, and the full confidence with which the analyst may trust himself to be passively borne along by the apparatus he has thus set in motion, that the great modern discoveries are owing.

Harriot introduced a minor improvement in the facility of the notation, by using the small Italic letters instead of Roman capitals, which had hitherto been employed; thus bringing the notation almost exactly into its present form. His labours were encouraged, and even a maintenance afforded him by that liberal patron of science and letters, Henry Percy, earl of Northumberland.*

Progress of Optics, Mechanics, &c.

Maurolycus, already mentioned as a mathematician, was also distinguished in optics. He conceived the use of the crystalline lens in the eye, though not the office of the retina; and the application of lenses for remedying both long and short sight. In his work, "Theoremata de Lumine et Umbrâ" (1575), he gives an explanation of the fact noticed by Aristotle, that the light of the sun passing through a small hole of whatever shape, always gives a *circular* illuminated space on a screen at a little distance. The rays from the different parts of the sun's disk cross at the aperture (which we will suppose to be, for example, triangular), and each ray gives a small triangular bright spot on the screen; these being partially superposed, but arranged in the form of the sun's disk, will give an image sensibly circular; and the more accurately so as the hole is smaller, or the screen more distant.

This principle is precisely that of the camera obscura in its simplest form, which was invented about this time (1560) by Baptista Porta, who described it in a work entitled "Magia Naturalis." The subsequent addition of a lens at the aperture, only substitutes the artificial crossing of the rays at the centre of the lens, and at the same time increases the quantity of light. He perceived, in general, the resemblance between this construction and that of the eye; but extraordinary as it may seem, failed in tracing the place of the image,

* Some highly interesting particulars of his analytical labours will be found in Prof. Rigaud's "Supplement to the Works of Bradley." Oxf. 1833, pp. 43. 52, &c. To his astronomical pursuits we shall refer in another place.

and imagined the crystalline lens to be the part in which it is formed. In one passage he has been interpreted as anticipating the discovery of the telescope, but an attentive examination clearly shows that his words will not fairly bear any such construction. He mentions the reflection of cold at the focus of a concave speculum, which is estimated by using the *eye* in the place of a thermometer. This very singular suggestion would seem to imply a greater advance in speculations on the nature of heat than we can suppose to belong to this age. It does not clearly appear whether this method was ever actually tried, or is a mere suggestion ; nor at this day are we aware of any thing being known which can clear up the point.

Though much of the "Magia Naturalis" is devoted to experiments and contrivances of a frivolous nature, yet it exhibits a close acquaintance with scientific principles. The work became very popular, and was translated into several languages. The general disposition of the age was now more favourable to the dissemination of knowledge, and the facilities afforded by the rapid multiplication of books, were beginning to be productive of the best effects in enlightening the public mind. Porta also was personally popular, and his house became the resort of the curious and learned ; but all this awakened the jealousy of the church ; and not only the works of the Neapolitan philosopher, but even his private parties became objects of suspicion to the ecclesiastics. We do not, however, find that they proceeded to any direct acts of persecution ; perhaps their victim had the caution to give them no occasion for doing so.

Dr. Gilbert, of Colchester, published a work on magnetism in 1590, which contains a copious collection of valuable facts and ingenious reasonings. He may be said to have laid the foundation of our knowledge in this department of science, which has since opened into so many new and important analogies.

He extended his enquiries, also, into the kindred subject of electricity, and made many new experiments on its developement by excitation in different substances. The declination, or deviation of the needle from the true north, was observed by Sebastian Chabot, and the dip by William Norman before this period.

Though not, perhaps, directly concerned in physical science, yet, as a vigorous opponent of the Aristotelian system, which placed such formidable obstacles in the way of the advance of experimental enquiry, we may here notice the labours of the celebrated Peter Ramus, who was born in 1502, and subsequently became a professor of philosophy in the university of Paris. In this situation he strenuously contended against the scholastic dogmas which placed such pernicious restraints on the energies of the human mind, and so grievously impeded the progress of truth. Such conduct, of course, drew down upon him the hostility of the heads of the university; and before a tribunal, which they obtained to be appointed under royal authority, he was tried and condemned. His enemies appear to have exulted in their triumph; but his friends were sufficiently influential to obtain for him another professorship; and his powerful eloquence seems to have been again employed with great effect in opposing the Aristotelian tenets. The unsettled state of the country, however, greatly interfered with all philosophical pursuits; and Ramus, after many sufferings, perished in the massacre of St. Bartholomew, 1571.

Jerome Fracastor, about 1540, published a treatise "De Stellis," in which he distinctly refers to the principle of the resolution of motion; observing, that bodies, having a tendency to fall straight to the centre of the earth, when thrown transversely to that direction take an intermediate course.

We have seen, in a former section, that the lever was the only one of the mechanical powers whose theory was perfectly established by the ancients. The equilibrium of bodies, consequently, could only be successfully investigated in cases which were reducible to this. We have also observed, that the doctrine of the inclined plane was unsuccessfully attempted by Pappus. No further investigation appears to have been tried till the sixteenth century, when Cardan considered the problem in a more distinct light; though what he advanced on the subject was no more than a plausible conjecture.

Guido Ubaldi, an Italian mathematician, also attempted the problem of the oblique action of forces with imperfect success in 1577. He considers chiefly the wedge; and comparing the direction in which it tends to produce motion with the direction in which the body thus acted on really moves, he observes that there is between these directions "a certain repugnance," which is greater as the angle of the wedge is more obtuse. He hence infers that the wedge will produce its effect the more easily the more acute it is, but without obtaining the exact proportion of the force. He also observes rightly, that the screw may be considered as a wedge wrapped round a cylinder.

It has been stated that the problem of the inclined plane had been solved by Jordanus in the thirteenth century, and that the solution is given in Tartalea's edition of his works in 1565. The discussion, however, is so vague and obscure, and so mixed up with the peripatetic views, that it cannot be admitted as having a valid claim to be a real demonstration.

The individual who really first solved the problem of oblique forces on solid principles, was Simon Stevin of Bruges, whose works were published soon after 1600. He not only deduced correctly the ratio of the power to the weight on the inclined plane, but, on the same principles, resolved forces so as to obtain their effect in different directions, and solved a number of important problems relating to them. The principle of his reasoning was highly original and simple. He supposed a perfectly flexible, uniform chain, joined at its ends, and hanging upon the inclined plane down its perpendicular side, and in a festoon below. However free the motion upon the plane, he conceived this chain will always sustain itself at rest; the part below cannot affect its motion. Hence the parts on the plane and in the perpendicular support each other, and their weights are proportional to those lengths respectively.

These investigations entitle Stevin to be regarded as the father of modern statics. After the inclined plane had thus been investigated, various other authors completed the different parts of the subject by following up the conclusions resulting from the principle of resolution of forces.

To Stevin we likewise owe the first suggestion of the fundamental principle of hydrostatics, that the pressure of fluids is in proportion to their depth.

The rainbow had from the earliest times been an object of admiration to every spectator, but it was long before any observer knew the full extent to which that admiration ought to be carried, or even cared to understand it. If it be unpardonable to shut our eyes to the most glorious spectacles in nature, it is doubly so to close our mental vision against that more perfect and more intimate perception of them, which the knowledge of their causes affords.

Among those who felt any interest in such enquiries, the rainbow was generally understood to arise in some way from the light reflected by the drops of rain falling opposite to the sun.

Maurolycus suggested that the light in passing through the drop, so as to be reflected from its back, somehow acquires colour from the refraction. But he proceeded no farther with this idea. Others made suggestions which only tended to perplex the matter.

Antonio de Dominis, archbishop of Spalatro, approached very nearly to the complete explanation. Having placed a globular bottle of water opposite to the sun, and above his eye, he saw coloured rays issue from the under side of the globe; the colours were different, according as it was more or less elevated; and in the order of the rainbow. He correctly traced the course of the rays refracted at entering and quitting the water, and reflected at the back of it. The same would, therefore, hold good with a globular drop of water in a shower; and, from the same angle being invariably required for each colour in a plane passing through the eye, the drop, and the sun, the circular form of the bow was accounted for. Still, the actual origin, or law of the connection between refraction and colour was totally unknown. The explanation, too, extended only to the primary or interior bow: in attempting that of the secondary, the author failed. This investigation of De Dominis is the more remarkable, since he is not known for any other scientific discovery; he published an account of it in a work, "De Radiis Visus et Lucis," in 1611. Yet the treatise is in some points so faulty, that Boscovich calls him "homo opticarum rerum supra id quod patiatur ea ætas imperitissimus," (a man ignorant of optics to a degree even beyond what that age would endure). This seems unduly severe upon a man who had been the first to propose an explanation so perfectly just and philosophical, as far as it went, of a very complex phenomenon. And if deficient in some points of detail, yet he certainly possessed a philosophical love of truth; which was evinced in a freedom and independence of opinions on theological subjects, extraordinary to be avowed by a dignitary of the Romish church: and which, as he had not the hypocrisy to disguise it, was, of course, heresy, and exposed him to a furious persecution. From this he found an asylum at the court of James I. of England, in 1616. But, returning to Italy, the persecution was, after some time, revived, and he died, as is supposed by poison, in prison; his body, and all his writings, being condemned to the flames by the Inquisition.

Copernicus.

Nicolas Copernicus was born at Thorn in Prussia, in 1473. In the university of Cracow, he attained considerable proficiency in the mathematical sciences; and, incited by a laudable emulation of the fame then obtained by Regiomontanus, he devoted himself to the further prosecution and improvement of those sciences, and especially of astronomy. Having studied for some time in the university of Bologna, and subsequently at Rome, his reputation became such, that he was appointed professor of mathematics in that city; a situation which he retained several years with high credit. Returning to his native country, and having entered the church, he was promoted to a canonry in the cathedral of Warmia. Here, exempted from the necessities and cares of life, he turned the advantages he enjoyed to their most legitimate and highest purpose, in devoting himself, not merely to the routine of his ecclesiastical duties, but to those contemplative studies, and laborious researches, which demand leisure and seclusion, and the fruits of which were to benefit not only that age, but the latest posterity. Though he lived in studious retirement, he declined no opportunity of giving his country those services which his talents enabled him to bestow, and was eminently useful in the regulation of the coinage, and of the calendar. His life was marked by no vicissitudes; and he died, universally honoured and esteemed, in 1543; when the publication of his great work, entitled "De Revolutionibus Orbium Cælestium*," had just been completed. Of its contents, and of the grand and comprehensive speculations into which his soaring genius had carried him, we must now proceed to a brief notice.

Continued meditation on the system of the universe had impressed Copernicus with a strong persuasion of the superior simplicity of the solar system, as originally proposed by the Pythagoreans, contrasted with the complication of the Ptolemaic, and the weakness of the arguments by which it was supported. In his great work he proceeded to develope his reasonings on the subject.

The spherical figure of the earth had been generally

* Editions of this work were published after the author's death, under the title of "Astronomia Instaurata."

admitted: the question then arose, was it suspended motionless in the universe, as the centre of the heavenly motions; or did it of necessity, as some argued, from the supposed impossibility of its remaining unsupported, revolve round another body? The conclusions of Copernicus were marked with caution and deliberation. Whether he was really not superior to the puerilities of the Aristotelian arguments, or whether he adopted that style of reasoning in order, by conforming himself to the prejudices of those he addressed, to have the greater chance of convincing them, may be matter of question. At any rate, though many of his arguments are just and sensible, yet, in several instances, he certainly runs into a maze of childish subtleties worthy of the most devoted disciple of the schools.

He contends, with great perspicuity and soundness, that if we suppose the distance of the earth from the fixed stars to be infinitely great, compared with its distance from the centre of the universe; but on the contrary, this distance to be very considerable, when compared with the orbits of the planets; then all the phenomena may be just as well explained, by supposing the earth to revolve on its axis from west to east in twenty-four hours, and to have, besides this, a motion of translation in its orbit, as by supposing the earth immovable, while the sun, planets, and fixed stars revolve round it.

That the earth itself is a mere point compared with the distance of the fixed stars, was admitted, but Copernicus well remarked, that it by no means followed that the earth is at rest in the centre of the universe; on the contrary, he says, it seems the more extraordinary, that such a vast circumference should revolve in twenty-four hours, rather than this mere point within it, the earth. To the arguments of the Aristotelians, and in particular those of Ptolemy, before referred to, Copernicus replied in the same style, by alleging that the earth's motion was of the natural and not of the violent kind, and that natural motions have not the same effects as violent ones; the latter tending to dissolution, the former, to conservation. This was at least talking to them in their own language: but he adds, much more reasonably, that if Ptolemy's argument be worth any thing, it ought to apply with much greater force against the motion of the celestial spheres, which must be so infinitely more rapid. He puts in a very forcible manner the illustration of the apparent motion of bodies, when the real motion is unperceived in ourselves, as in a ship; and objects to the Aristotelian notion of the earth's centre being the centre of gravity of the universe, representing gravity as nothing else than the tendency of parts to draw together and coalesce in the form of a globe; and observes, that it is probable that such a tendency exists in the sun and moon, and other heavenly bodies; but that this does not hinder them from describing their respective orbits. He then explains the solar system at length, pointing out the way in which it simply and satisfactorily accounts for all the apparent motions of the planets, especially their stationary points and retrogradations, as seen from the earth, itself in motion.

The belief in the perfectly circular form of the celestial orbits was an essential point in the peripatetic system. Copernicus in this particular does not appear to have been superior to his predecessors, but held it as a necessary truth; he therefore was obliged to adopt the epicycles so far as to account for the obvious deviations of the orbits from perfect circles: this, however, occasioned a very slight complexity, compared with that arising from their introduction to explain all the retrogradations and apparent irregularities.

His system again was unnecessarily complicated, by a fanciful hypothesis he made to explain the cause of the precession of equinoxes: nor did he discard the trepidation.

With all these defects, however, the Copernican system will always be esteemed, as giving, at least in its general outline, a correct and elevated conception of the great system of the planetary orbits. It was based upon the only kind of argument which could in that age have been adduced; viz. that it completely explained the phenomena which observation had revealed, and that it did so with infinitely greater simplicity than any hypothesis hitherto proposed. The objections which had been brought against it on the grounds of the Aristotelian physics were completely answered, and it was even defended on principles which its opponents of that school would be the last to call in question.

Copernicus had himself been assiduous in making observations, and we learn from his commentator, Rhæticus, that what first led him to entertain the Pythagorean doctrine, was, that his observations on the planet Mars showed him distinctly the very considerable difference in the apparent magnitudes of that planet at different times, whence he inferred the relative position of the orbits of Mars and of the earth; he made also this remarkable observation, — that if the *sense of sight could ever be rendered sufficiently powerful,* we should see phases in Mercury and Venus.

Copernicus was sufficiently cautious in the promulgation of his system, being fully aware of the prejudices he had to encounter. In the first instance his views were only made known through the channel of private correspondence and friendly conversation. Thus his contemporaries found nothing to startle their prepossessions, or to provoke hostility. Opinions emanating from one of their own body excited no jealousy among the ecclesiastics: and several prelates were even urgent with Copernicus to publish his researches. At length his disciple Rhæticus printed, in 1540, "An Account of a Manuscript of Copernicus." This being received with approbation, the author himself was prevailed upon to proceed with the publication of his own work, which he dedicated to the pope, in order, as he says, that the authority of the head of the church might silence the calumnies of individuals who attacked his views by arguments drawn from religion.

Of those attacks we have no satisfactory account; and it is not quite evident whether he refers to objections actually made, or only apprehended. Certainly no such objections emanated from the heads of the church, who were his friends and supporters; and he alludes to them only in a brief and somewhat contemptuous manner. It was not till some time afterwards that we have on record any systematic opposition to these views of the celestial motions, on the ground of alleged repugnance to the truths of revelation.

The system advocated by Copernicus found at first few supporters; but the number, though small, included several of the most eminent philosophers of the day. The testimony given in its favour was that of few individuals, but of those best qualified to judge; and the adoption of the Copernican doctrine by these men was equivalent to its extended promulgation.

Such were Rhæticus, already mentioned, the author of a commentary on the "Revolutions," and of extensive trigonometrical tables; Erasmus Rheinhold, who improved upon the astronomical tables of Copernicus; and also taught that the orbit of Mercury is elliptical, and that the *deferent* of the moon is likewise elliptical; Rothmann, astronomer to William, landgrave of Hesse; and Mæstlin, the instructor of Kepler.

The landgrave of Hesse was a remarkable instance of a sovereign prince devoting himself to the actual labours of astronomical observation, and composing a catalogue of the fixed stars (1560). He was assisted by Rothmann, and also by Justus Byrgius, who directed considerable mathematical talents to the improved construction of the astronomical instruments, used by the landgrave, in the superb observatory which he had erected at Cassel. Mæstlin was known by several astronomical researches. He availed himself of the principle of the camera obscura to measure the apparent diameter of the sun by throwing his image into a dark room. He suggested the explanation now generally received of the faint light visible on the dark part of the moon, as being due to the light reflected from the earth. Rothmann was the first to point out the error of Copernicus respecting the precession of the equinoxes, and the trepidation before spoken of: this was in a letter to Kepler, 1590.

Tycho Brahe.

The middle of the sixteenth century, we have seen, was rendered memorable by the publication of the great work of Copernicus: the close of it was adorned by the discoveries of Tycho Brahe. He was born in 1546, of a noble Danish family: and while at the university of Copenhagen, the occurrence of a great solar eclipse, in 1560, is said to have excited in his mind an irresistable desire for astronomical pursuits. For these he quitted the legal profession; and going to Augsburg, he inspired with a similar love of science Peter Hainzell, an opulent citizen of that place, who, at his own expense, erected an observatory, in which Tycho continued his labours for several years. In 1570, he returned to Copenhagen, and in a retreat near that city had the good fortune to observe, through all its variations, the brilliant new star which appeared in 1572, in Cassiopeia, and disappeared in the following year. His high reputation now attracted the notice, and secured for him the liberal patronage, of Frederic, king of Denmark, who conferred on him a munificent pension, gave him the island of Huene in the Baltic, and erected upon it a splendid observatory, which was named Uranibourg. Here he continued for twenty years, amassing an invaluable collection of observations, made with instruments constructed upon a far larger and more accurate scale than had been hitherto attempted. The arcs were divided up to parts containing ten seconds: the best hitherto made did not extend to divisions of as many minutes.

After the death of Frederic, the enemies of Tycho induced the minister of his successor to withdraw from him the donation of the late king. Thus he was driven to seek refuge in other countries: and, under the patronage of the emperor Rodolph II. (who appears to have valued him chiefly from the assistance his labours would give to the astrological art), he renewed his observations at Prague. But his health and spirits were broken; and he at length sunk under a lingering disease, in 1601.

In reviewing his scientific labours, we find that in the commencement of them, Tycho felt most sensibly as the capital deficiency in the observatory, the want of accurate measures of time: he successively tried a number of contrivances, and at length the imperfect clocks then constructed, but without any material success. Before the perfect construction of clocks, astronomers were necessitated to adopt various methods of finding the right ascensions, and thence the longitudes of the stars, by observations on their distances from the sun, which was effected by an intermediate comparison with the moon; or, according to Tycho's suggestion, with Venus. He endeavoured to improve upon the accuracy of preceding catalogues, by numerous determinations of this kind. He also invented the simple method of finding the latitude of a place by observing the meridian altitudes of a circumpolar star when above and below the pole. He investigated very fully the correction due to atmospheric refraction; though he ascribed it rather to vapours than to the atmosphere itself, and entirely exploded the trepidation which had so long disgraced the astronomical tables.

In comparing the positions given by Hipparchus with those in his own catalogue of the fixed stars, Tycho was led to the important discovery of the slow diminution of the obliquity of the earth's axis, which is so gradual, that it cannot be detected but by comparison of observations at very distant intervals: its existence was, therefore, long disputed; but modern observations have established it beyond question, and it has been shown to be a consequence of gravitation.

In the theory of the moon, some important discoveries were also made by this indefatigable and skilful observer. Two inequalities we have noticed as known to Ptolemy. Tycho discovered a third, called the "Variation." He also showed that there is a small periodical change in the inclination of the moon's orbit: and again, that a periodical change takes place in the motion of its nodes.

Tycho argued ably, from observations on the comet of 1577, against the received Aristotelian doctrine, that comets are only meteors formed in our atmosphere. He determined the horizontal parallax of the comet in question, which was such as to prove that it moved beyond the orbit of the moon. It hence followed that the celestial spaces could not be occupied by solid crystalline spheres. He was violently attacked on this point by the adherents of the school doctrine; and his adversaries completely exposed the weakness of their cause by having recourse to invective, and even personal calumny. The real nature of the cometary motions, however, remained undiscovered; though both Tycho and Mæstlin proposed theories involving epicycles.

The objections which Tycho unhappily entertained against the Copernican system were built partly on physical and partly on theological grounds. He, however, speaks of Copernicus in the highest terms of admiration; and even joins in the refutation of the objections urged against the diurnal motion of the earth by Ptolemy. The argument by which he himself was satisfied of the fallacy of the diurnal rotation was this; that if a stone be suffered to fall from the top of a high tower, it ought to reach the ground far behind the foot of the tower, which it does not. To this Rothmann replied, that a stone is a part of the whole mass, and therefore partakes in the earth's motion.

Against the earth's annual motion, Tycho urged the old and thrice refuted objection of parallax; for the complete answer to which he might have been referred back to Aristarchus. Another argument was derived from the erroneous impression conveyed by the eye, that the stars have a sensible disk or magnitude; and to have such an apparent magnitude, if as distant as Copernicus would make them, Tycho calculated that their real size must be larger than the whole orbit of the earth. The answer is, we cannot say that this is not the case. But again, the use of the telescope has since shown that the assumption is incorrect, the stars having no real disks.

The theological objections which appeared to Tycho so formidable against admitting the motion of the earth, were deduced from certain passages in scripture, in which expressions occur attributing motion to the sun and rest to the earth. Considering the state of knowledge in his time, it is not a matter of surprise that such objections should have possessed considerable weight. On the one hand, it must be remembered, that the question of the celestial motions had not yet obtained any *demonstrative* solution: and, on the other, though the light of the Reformation had, before this period, been diffused throughout Europe, yet the general illumination of the age was not such as to allow of accurate discrimination in the use or application of the sacred writings. The philosophical theory could not command absolute conviction; and the use thus made of the authority of scripture was not perceived to be a misapplication. Hence it cannot surprise us, that the two appearing to be at variance, the former was made to give way. In truth, it can hardly be said, that even at the present day such questions are placed upon their right basis. The advance of illumination during several centuries, has hardly yet (among the generality of mankind) exhibited in their just light such contradictions between the letter of scripture and the results of science. But of this we shall see more instructive examples in the sequel. To return now to Tycho. Fully impressed, in his own mind, with the weight of these objections, he directed his thoughts to framing another system, by which they should be avoided. He supposed the five planets to revolve round the sun, whilst the sun, carrying this system with him, revolved round the earth, at rest in the centre, as did also the moon, at a less distance. This system, no doubt, answered tolerably well the purpose of explaining the actual apparent motions, as far as the accuracy of observation had then been carried. It is, in fact, a modification of the principle of the epicycles: and had it been proposed prior to the system of Copernicus, it might justly have been regarded as a step in the progress of simplification and improvement. But coming, as it did, after the suggestion of that theory, which bears upon it the impress of the grandeur and simplicity of nature, it can only be regarded as a decidedly retrograde movement. It is a melancholy proof not only of the weakness, but of the perverseness of the human mind; not only of a backwardness in the pursuit of truth, but of an unhappy rejection of it when presented to our grasp.

The character of Tycho exhibits a remarkable mixture of acuteness and weakness. He was a firm supporter of astrology, and wrote in defence of it; he believed in omens, and even regulated his conduct in accordance with such warnings. At the same time, his conduct exhibits many instances of liberality and generosity; and, upon the whole, we cannot but regard him as fully entitled to the respect in which he has been generally held.

Reformation of the Calendar.

The period of which we have now been treating was distinguished by a transaction of some general interest, the reformation of the calendar by Pope Gregory XIII., in 1582. We have already noticed the devices adopted to keep the same nominal days and months to the same physical seasons, and the necessity for doing so, arising out of the circumstance, that the solar year is not measured by an exact number of days. The Julian correction, which intercalated a day every fourth year, made the calendar year about eleven minutes longer than the true solar year. This difference accumulating every year, had amounted to nearly eleven days in the year 1582. Thus the equinox which, in Julius Cæsar's time, fell on the 21st of March, now took place on the 10th. The Gregorian reformation aimed at two objects: the first was that of preventing the accumulation of the difference in future, by ordaining that the concluding year of the first three out of each four centuries (which would have been bissextile) should be a common year, the fourth remaining bissextile. This diminishes the difference to an amount almost insensible: to make it quite so, Laplace has since suggested leaving the 4000th year a common year. The second object was to restore to the equinox the name of the 21st of March, with a view to the regulation of the festivals of the church, especially Easter. Hence the adoption of what was called the new style, or dating eleven days forward; what was the 10th of March, being now called the 21st. This gave rise to great confusion, as the states opposed to the Pope's authority long refused to adopt it, and two styles prevailed. It was not adopted in England till 1751; nor is it in Russia to this day.

SECTION III.

THE DISCOVERIES OF KEPLER AND GALILEO.

In the period last treated of, we have had occasion to notice the advances which began to be made in the sixteenth century, in the various parts of the abstract science of quantity, and of its application to some of the phenomena of the natural world. We have also seen the commencement of a revolution in opinions on philosophical subjects: an opposition beginning to manifest itself against the Aristotelian dogmas, and an increasing disposition to recur to experiment and observation as th only legitimate ground of theory. The great problem of the system of the world we have found engaging the attention of two philosophers of pre-eminent celebrity: but one of these, though he laboured in the observatory to obtain accurate measures of the facts, failed in the comprehensiveness of the theory he propounded: the other made a happy adoption of a satisfactory theory, but it was supported only by arguments from analogy and probability, and the demonstrated insufficiency of the old hypotheses. We have now to turn our attention to a train of discoveries in which we shall find the true theory supported on a firm basis of facts, and accordance with observation.

Kepler.

John Kepler was born at Weil, in the duchy of Wirtemberg, December the 21st, 1571. In infirm health from his birth, his life was more than once despaired of, yet he continued to make considerable progress in his studies, both at the school of Maulbronn and at the college of Tubingen, under the tuition of Mæstlin. His time was here occupied, as he has himself minutely related*, in the ardent pursuit of certain astronomical speculations, which evince at once the fertility of his imagination, and the indefatigable perseverance with which he pursued and worked out the most laborious details of calculation required by these theories, which, after all, turned out entirely unsubstantial, and which he discarded without hesitation on finding them so.

Meanwhile he had acquired a reputation which at once recommended him to be appointed professor of astronomy at Gratz. Here, in the intervals of his public duties, he pursued these researches with unabated ardour. "There were three things," he says, "in

* His own published correspondence supplies us with the most interesting details of his pursuits. Copious references to the original authorities will be found in Drinkwater's Life of Kepler.

particular, of which I pertinaciously sought the causes why they are not other than they are: the number, the size, and the motion of the orbits." He was delighted with the simplicity of the Copernican system, and was thus anxious to trace numerical relations among its elements, from a firm persuasion that some abstruse principle of numerical harmony pervaded all nature, and regulated the proportions of all its parts. One after another, he proposed, tried, and rejected every sort of numerical relation he could devise. Notwithstanding perpetual failures, he adds, — "I was comforted in some degree, and my hopes of success were supported, as well by other reasons which will follow presently, as by observing that the motions in every case seemed to be connected with the distances, and that when there was a great gap between the orbits, there was the same between the motions; and I reasoned that, if God had adapted motions to the orbits in some relation to the distances, it was probable that he had also arranged the distances themselves in relation to something else."

It is far from our intention to follow Kepler through the vast number of speculations which he pursued on these points; we merely refer to them as admirable indications of the spirit and character of his method of philosophising, and as illustrative of the genius of his investigations. The theory which pleased him best, and which, though it deviated considerably from the observed results, yet he regarded as differing only by the errors of observation, was a geometrical construction which determines the proportions of the orbits, by inscribing successively in them the five regular solids, each sphere being drawn to touch internally the sides of the solid inscribed in the preceding. This, with some other supplementary theories, forms the substance of his "Mysterium Cosmographicum," 1596.

In 1597 he withdrew from Gratz into Hungary, and subsequently visited Tycho at Prague. Owing to some dissensions, it seems that he shortly after gave up his appointment at Gratz; and his affairs being in a state of great embarrassment, he was actually supported by the generosity of Tycho, and appointed his assistant in the office of imperial mathematician to the emperor Rodolph. He was engaged with Tycho in the formation of new astronomical tables, which, it would appear, the emperor patronised almost entirely with a view to the purposes of astrology; and which were to be brought out on a magnificent scale, under the title of the "Rudolphine Tables." Tycho's former assistant, Lomberg (or Longomontanus), had retired into Denmark on an appointment to an astronomical professorship: on Kepler, therefore, devolved the task of discussing Tycho's observations, and more especially those on the planet Mars. This work was, however, interrupted by the death of Tycho; on which Kepler succeeded to the principal situation which he had filled. He now prosecuted his observations; and in particular had the opportunity of examining the new star which appeared in 1604 in Serpentarius. At this time, also, he entered largely into discussions on the pretensions of astrology, in which he appears to have believed in a modified sense; but it is rather difficult to make out precisely what his opinions were, from the extraordinary style in which the statement of them is couched.

His appointment in the emperor's service appears to have been attended with the constant vexation of a difficulty in getting his pension paid. This interrupted the publication of the "Rudolphine Tables;" but meanwhile he produced a treatise on Comets, and his "Paralipomena in Vitellionem," in 1604. This work contains many original views, though mixed up with much visionary speculation. It bears unquestionable marks of genius; but, like many of his other writings, much of it is excessively tedious, from the detail in which he gives the account of the whole train of his investigations. It contains two principal researches of interest: one relating to the structure of the eye, the other to the law of refraction.

In 1609, Kepler published that great and extraordinary work, his "Treatise on the Motions of the Planet Mars." He had devoted himself to this subject at intervals from the commencement of his engagement with Tycho; and the determined perseverance with which he laboured at the most overwhelming arithmetical calculations, is, perhaps, scarcely less surprising than the powerful genius which shines conspicuous in the successful treatment of so arduous an enquiry, or the wonderfully prolific imagination, which was never at a loss in starting a new theory to work upon when the last had been found insufficient. This volume contains, in effect, the developement of two of those great laws of the planetary orbits by which the name of Kepler has been immortalised, and which form the basis of the whole system of Newton. The author seems thoroughly impressed with the importance of the work on which he is entering; and having an eye to the prejudices which had been evinced with respect to the promulgation of the Copernican system, and the views entertained by Tycho, he introduces his subject in the following emphatic terms: —

"If any one be too dull to comprehend the science of astronomy, or too feeble-minded to believe in Copernicus without prejudice to his piety, my advice to such an one is, that he should quit the astronomical schools; and condemning, if he has a mind, any or all the theories of philosophers, let him look to his own affairs, and leaving this worldly travail, let him go home and plough his fields; and as often as he lifts up to this goodly heaven those eyes with which alone he is able to see, let him pour out his heart in praises and thanksgiving to God the Creator; and let him not fear but he is offering a worship not less acceptable than his to whom God has granted to see yet more clearly with the eyes of his mind, and who both can and will praise his God for what he has so discovered."

In the introductory part of the work, the author discusses at length the received opinions respecting gravitation; and after exposing many parts of the

Aristotelian system, he expounds his own views of it in remarkably strong and perspicuous terms. Among other illustrations of its nature and laws, we find the following: —

"If the moon and the earth were not retained in their orbits by their animal force, or some other equivalent, the earth would mount to the moon by a fifty-fourth part of their distance, and the moon fall towards the earth by the other fifty-three parts, and they would thus meet; assuming, however, that the substance of both is of the same density. If the earth should cease to attract its waters to itself, all the waters of the sea would be raised, and would flow to the body of the moon."

He hence proceeds to a general view of the tides, and other phenomena dependent upon gravitation. Thus was a theory started, as yet only an hypothesis, and which future discoveries were to verify; a theory given in the same work which contained the exposition of the laws of the planetary orbits, but delivered without the smallest apprehension of any connection between them: yet these two subjects, broached, indeed, in the same work, but not as yet seeming to have the most distant relation to each other, were, before long, to stand arrayed in the most intimate union, and to minister proof to each other and to the whole system of the celestial motions, as well as of those on the surface of the earth.

In the main body of the work, Kepler proceeds to discuss the orbit of Mars. The observations of Tycho had determined the motions of that planet to a great degree of accuracy; and Kepler soon found that its inequalities of motion differed widely from any thing which could be represented by the system of epicycles, as ordinarily constructed. He accordingly, with almost incredible diligence, calculated and recalculated a new system of the same kind, in which, for a while, he believed he had succeeded; but a few more comparisons showed that it only involved him in errors of another kind. No sooner, however, did he find this hypothesis fail, than he plunged into fresh computations in pursuit of a new construction. He was, in this instance, led by a singular train and combination of ideas to one of those real laws which have since become the foundation of physical astronomy.

His object was to investigate the real form of the orbit of Mars, from the motions of that planet as observed from the earth, itself in motion. Hence it was necessary to distinguish that apparent part of the inequality of the planet's motion, which is caused by the inequality of the earth's motion in its orbit.

Considering, therefore, the earth's orbit as circular, after many laborious trials, he at length deduced a tolerably exact representation of the law of the earth's motion in its orbit, by supposing the orbit to be described about the sun, which is *not* placed in the centre, but in a point from which, if lines be drawn to successive points in the circumference, the areas of the sectors formed by these lines, and the intercepted portions of the circumference, will always be proportional to the times in which those portions are described; which, in other words, is called the law of the *equable description of areas.*

In considering the orbit circular, he assumed what is very nearly true for that of the earth. In proceeding to carry on his investigations with respect to Mars, he could now divest the subject of those difficulties which arose from the inequalities of the earth's motion; and he soon found that no circular orbit would represent that of Mars; nor would the equable description of areas about a point within a circle represent its motion: it then occurred to him that the form might be elliptical or oval. But a number of subsidiary hypotheses were yet to be framed and rejected in turn before the simple doctrine of an elliptic orbit could really be established. The picture which Kepler presents to us of the workings of his mind while pursuing this research is full of the most intense interest. It would be impossible, without entering into mathematical details, to explain the process by which the ultimate suggestion was brought under his consideration; and it would be equally so to convey an idea of the immense mass of calculation through which he toiled, in putting each of his successive theories to the test of agreement with the observations. Finally, after working his way in alternate exultation at anticipated triumphs, and bitter disappointment when, one after another, they vanished in air, driving him, as he says, "almost to insanity," he at length had the intense gratification of finding, that *an elliptic orbit described about the sun in one of the foci*, agreed accurately with the observed motions of the planet Mars. This is called Kepler's first law. His exultation knew no bounds — his very diagram was drawn with a figure of victory in the corner. He also found immediately that his former inference of the equable areas in the circular orbit was only an approximate case of the law, which is accurately true of motion in an elliptic orbit about the focus; and his second law is announced by saying that the *radius vector of the elliptic orbit passes over equal areas in equal times.* A third of equal importance was discovered afterwards.

In connection with the second law, it became a point of importance to find some method of determining the arcs of the ellipse which correspond to equal sectorial areas. Kepler, in proposing this point to the attention of geometers, stated his belief that no exact solution of it could be attained. It has engaged the attention of the most eminent mathematicians since his time; and has received solutions by means of series carried to any degree of approximation. It is distinctively known by the name of Kepler's problem.

This difficulty of calculating the place of a planet, and which Kepler confessed he had not the means of overcoming, was urged by his opponents as a serious objection to his system, as if he were answerable for the proceedings of nature.

Lastly, all the planes of the planetary orbits were supposed by Ptolemy to pass through the earth. Copernicus did not go so far as to reject this notion. Kepler showed that they all passed through the sun, and that the lines of their nodes all intersect in the centre of that luminary.

In 1611 Kepler published his Dioptrics, in which he made further efforts to investigate the law of refraction; but with only a limited degree of success. He was led to see the importance of it chiefly in connection with the theory of lenses, then an object of attention with reference to the construction of the telescope. His investigations, however, are quite independent of their application, which must be spoken of in its proper place. He attempted to deduce the connection between the angles of incidence and refraction. In the case of glass, he found by experiment that these angles, when of any small value, *always* preserve very nearly the ratio of three to two. This sufficed for the immédiate purpose of finding the focal length of a lens, when the angles of incidence are practically comprised within very small limits. Mathematical views were hardly enough advanced to enable him to hazard the conjectural hypothesis, which would have instantly occurred to any modern mathematician in such a case, to pass from the arc to the sine, and try whether that relation held good in other cases.

Under the same limitation of the angle of incidence being small, he made several other useful applications of the same approximate law.

Of the astronomical, or atmospheric refraction, he had a much more correct notion than his predecessors; he understood that it extended from the horizon to the zenith, and even gave an approximate law to express the rate of its decrease.

He incidentally suggests the idea, afterwards so fully verified, that the air is a substance possessing weight; and that the refractive power varies with the temperature. He also remarks, that during a lunar eclipse.

the moon's surface is still illuminated by rays refracted in their passage through the earth's atmosphere.*

The constitution of the eye, if there be anything in mathematical science which comes home with a direct personal concern to every one, is that which is of the most intense interest. We have observed the steps which had been made towards the investigation of the mode in which vision takes place, by Maurolycus and Baptista Porta: the former nearly discovered the secret; and it was but a thin, though to him impenetrable veil, which still concealed one essential part of the truth from the Neapolitan philosopher. This veil was drawn aside, and the complete discovery of the truth effected by Kepler, who, to the glory of finding out the laws of the planetary system, added that of first analysing the whole scheme of nature in the structure of the eye. He perceived the exact resemblance of this organ to the camera obscura; of the rays entering the pupil, the separate small pencils from all points in the object, cross at the centre of the crystalline lens, and are each brought to their respective foci in the vitreous humour, whose refractive power, compared with that of the lens, is precisely adjusted so that the focal distance shall coincide with the utmost exactness with the position of the retina, when the succession of those luminous points paint an inverted picture of external objects. But here again, by another law, in whatever direction a ray falls on the retina, we, by instinctive judgment, refer it to an object in that direction produced; hence the ray coming downwards, or falling on the lower part of the retina, is referred by us upwards, or to the upper part of the object; or, in other words, we see objects erect. When we have traced the process to the picture on the retina, we have arrived at the limit to which optics can carry us. Physiology may yet show whether any particular action of the nerve is connected with vision; but the boundary between the physical impression and the mental perception will probably never be penetrated.

Kepler's affairs in the meanwhile continued in the same unhappy condition as heretofore; though, on the death of the emperor Rodolph, he was nominated to continue in the office of imperial mathematician under his successor Matthias; and, at the same time, was appointed to a professorship in the university of Linz, in 1612. But, in addition to his pecuniary embarrassments, he was at this time labouring under domestic affliction, in the loss, first, of a son, and then of his wife: he seems, however, with his usual elasticity of spirits, to have soon recovered from the blow, and we find him giving a minute, and highly characteristic and amusing account, of his choice of a second wife.

His new residence at Linz was not long undisturbed; he became involved in some disputes with the Roman catholic priests, apparently connected with the tenet of transubstantiation; but it does not exactly appear how far the question had any reference to his philosophical speculations: it ended, however, in his being excommunicated. He says, in one of his letters, "The priest and school inspector have combined to brand me with the public stigma of heresy, because in every question I take that side which seems to me to be consonant with the word of God." Whatever the particular point in dispute may have been, the real and unpardonable offence probably was, that he had dared to think for himself.

It was at Linz that his thoughts were particularly directed to geometrical investigations; and, from the accidental circumstance of noticing the blunders of an ignorant gauger in measuring his wine casks, he was led to investigate the subject of the mensuration of solids: this gave rise to some speculations, which afterwards assumed a more peculiar and prominent interest.

Kepler published, in 1615, a tract on Stereometry, in which the measurement of the contents of many solids was proposed, which had not as yet fallen under the notice of mathematicians: such, for example, were the solids generated by the revolution of a curve, not about its axis, but about any line whatever. This included solids formed by the revolution of an arc of a curve about its *chord*, which are precisely of the form of casks and other vessels, so that the investigation had an immediate practical object as supplying rules for gauging. Kepler proposed a vast number of varieties of such solids for the consideration of geometers; he was himself unequal to the task of solving more than a very small number of the simplest of such cases: but, in doing this, though he had recourse to what was in reality the method of limits, yet he devised means of avoiding much of the tediousness of the ancient processes by the introduction of the phraseology of *infinitely* small quantities. He conceived a circle to be made up of an infinite number of triangles, having their common vertex at the centre; and their infinitely small bases, portions of the circumference: this was, in fact, the same thing as the method before mentioned of Archimedes, and assigned in the same way the area.

In 1619, Kepler reprinted his "Mysterium Cosmographicum;" and, nearly at the same time, published his "Harmonics." We might, indeed, have been surprised at his thus deliberately re-editing his juvenile reveries, did not the last-named work of his mature years fully equal them in extravagance. Of its very miscellaneous contents, we shall not attempt even an enumeration, but shall only take occasion to notice how strangely some bright flashes of real genius and philosophical truth break out here and there in the midst of the chaos. In the course of a speculation on the reason why the heptagon is not employed in the actual construction of the universe, the author drops the hint, that "the root of an equation which cannot be accurately found, may yet be found within any degree of approximation by an expert calculator." He expounds his notions of astrology; and, while he strongly condemns the absurdities of the vulgar belief, attempts to substitute a system of celestial influences, in which he seriously represents the earth as an enormous living animal, the tides being its act of respiration, and its vital sympathies being excited by the configurations of the planets. In "the very exquisite harmonies," as he calls them, "of the celestial motions," he represents Saturn and Jupiter as taking the bass, Mars the tenor, the Earth and Venus the counter-tenor, and Mercury the treble. With some inconsistency he rejects the theory, which had at least poetical beauty to recommend it, adopted by several of the astrologers, that each planet had its guardian angel, who conducted its motions: he here drily refers to the elliptic motion, which, he observes, "rather smacks of the nature of the lever and material necessity." It is in the midst of all this extreme nonsense, and in a discussion as to whether the planets are naturally light or heavy, that the author announces his discovery of the third great law of the planetary orbits: he speaks of it in terms of the most unbounded rapture, as the grand consummation of his desires and labours; to the pursuit of which he had devoted his life, and which excels all the harmonies of the celestial spheres. It was, indeed, as connected with this idea, that he had been led into the enquiry.

"Great," he says, "as is the absolute nature of harmonics, with all its details, as set forth in my third book, it is all found among the celestial motions; not, indeed, in the manner which I imagined (that is not the least part of my delight), but in another very different, and yet most perfect and excellent. It is now eighteen months since I got the first glimpse of light, three months since the dawn, very few days since the unveiled sun, most admirable to gaze on, burst out upon me. Nothing holds me: I will indulge in my sacred fury; I will triumph over mankind by the honest confession that I have stolen the golden vases of the Egyptians to build up a tabernacle for my God, far away from the confines of Egypt. If you forgive me, I rejoice; if you are angry, I can bear it: the die is cast; the book is written; to be read either now or by posterity, I care not which: it may well wait a century for a reader, as God has waited 6000 years for an observer." . . . . "If you would know the precise moment, the idea first came across me on the 8th of March of this year, 1618; but, chancing to make a mistake in the calculation, I rejected it as false. I returned again to it with new force on the 15th of May; and it has dissipated the darkness of my mind, by such an agreement between this idea and my seventeen years' labour on "Brahe's Observations," that at first I thought I must be dreaming, and had taken my result for granted in my first assumptions. But the fact is perfect; the fact is certain, that the proportion existing between the periodic times of any two planets is exactly the sesquiplicate proportion of the mean distances of their orbits." That is, as we may express it with more perspicuity, *the squares of the times are as the cubes of the mean distances.*

Thus was the third of these important principles established, as laws simply derived from induction. In fact, the essential desideratum, at this stage of the science of the system of the world, was precisely this determination of its numerical laws, deduced without reference to any physical hypothesis. Had such relations been deduced in connection with any theory, they would have been received with suspicion; and, very probably, in the then state of philosophy, would have been thrown aside along with the theory, if it had been in its turn exploded to make way for a newer system. The great advantage possessed by these laws of Kepler, was their *purely inductive* character; and we cannot help recognising, in the extremely peculiar turn of Kepler's mind, which sought after these arithmetical relations purely for their own sake, the precise intellectual character requisite for taking this particular and essential step towards the establishment of the system of the world, raised up at the precise period when he was wanted, and fulfilling this one great purpose in conducing to the promotion of truth, amid all the innumerable avenues to error which were opened in his other speculations.

Kepler had no sooner published his epitome of the

Copernican astronomy in 1618, than it was honoured by being placed, along with the works of Copernicus, on the list of books prohibited by the church: he was considerably alarmed at this circumstance, anticipating difficulties in the publication of other contemplated works; but it would seem without reason. In the same year he became acquainted with the invention of logarithms, and even calculated tables of them. He has put on record a curious notice of the spirit in which this invention was received; many mathematicians appearing to think it derogatory to their dignity to make use of tables of results already calculated to their hands.

In 1620 he was visited by the English ambassador sir Henry Wootton, who endeavoured to persuade him to settle in England, but he declined, from a desire to prosecute the publication of the Rudolphine tables, which had so long been delayed from want of the necessary funds, promised indeed, repeatedly, from the imperial treasury, but not more readily forthcoming than his pension. A variety of other causes contributed also to delay their publication, especially the disturbances which accompained the progress of the Reformation in Germany. Kepler's situation as imperial mathematician alone saved him from the violence of Romish party: but his library was taken possession of by the Jesuits.

In 1627, however, these tables appeared, and contained great improvements upon all which had preceded them; the volume is remarkable in containing the first suggestion of the method of determining the longitude from occultations of fixed stars by the moon.

Kepler was now patronised by Albert Wallenstein, duke of Friedland, who valued him only as an astrologer, but was at least a better paymaster than the emperor: in his service Kepler died, November 1630. He left a mass of unpublished writings, which were examined by Euler and others, but nothing has been printed; and we may presume nothing appeared worth publication.

Galileo.

A few years earlier than Kepler (February 15. 1564) was born his illustrious friend and contemporary in public fame, Galileo de' Galilei, the son of Vincenzo de' Galilei of Pisa. After the fashion of that age and country, he appears to have been more commonly designated by the name of Galileo, by which he has been since known.

Vincenzo was a man of considerable learning and talent, who early appreciated and properly cultivated the expanding genius of his son: at the university of Pisa he was very soon distinguished, not merely for general proficiency in his studies, but for the singular boldness with which he maintained original opinions often at variance with the received scholastic dogmas.

In the cathedral of Pisa, one of the chandeliers hanging from the lofty roof had accidentally been set swinging; this instantly struck the observant mind of the young philosopher, who noticed the fact that its vibrations were performed in exactly equal times, by comparing them with the beatings of his pulse. This subject afterwards occupied much of his attention; but for the present he went no further than to apply the principle to the purposes of the medical profession, to which his views were directed, by contriving a pendulum with a variable length of string, by which its beats might be made to accord with those of the pulse, and thus give a measure of its rate.

From that profession, however, his thoughts were soon withdrawn by the attractions of mathematical studies, upon which he was now entering; he made himself master of the ancient geometers, particularly Archimedes, and immediately proceeded to enlarge and improve upon the last-mentioned philosopher, by composing a work on the "hydrostatical balance." This essay introduced him to the notice of Guido Ubaldi, then at the head of the Italian mathematicians, who directed his attention to various points in hydrostatics requiring more full illustration, and was ultimately the means of recommending him to the patronage of Ferdinand de' Medici, grand duke of Tuscany, through whose influence he was appointed to the lectureship in mathematics at Pisa, in 1589.

Settled in his new office, he directed his attention with increased energy to a rigid examination of the favourite dogmas of the Aristotelian physics by the test of experiment. In such enquiries he was not unsupported by the concurrence of several of his enlightened countrymen. Benedetto had controverted some of the positions of Aristotle's mechanics; and that universal genius Leonardo da Vinci employed much of his time in similar discussions. The views of Copernicus also were now making a silent, but sure progress in the estimation of learned men. But we have a more striking instance in Giordano Bruno, who attacked the scholastic doctrines with unsparing boldness, and exposed their absurdities to the most deserved ridicule. He was, of course, soon brought under the power of the Inquisition, condemned as a heretic, and ultimately burnt at Rome, in 1600.

Galileo was not backward in contributing to the same great work of intellectual emancipation; and as fast as he succeeded in refuting the scholastic tenets by his appeal to experiment, he denounced them from his professorial chair with an energy and success which irritated more and more against him the other members of the academic body.

One grand tenet of the schools was, that heavy bodies fall to the earth more rapidly than lighter in proportion to their weight. Galileo, in the presence of the university, ascended the leaning tower of Pisa, and dropped from its summit bodies of different weights: with an inconsiderable difference, due to the resistance of the air, they reached the ground nearly at the same moment. The learned quoted Aristotle in preference to their senses, and contended that a mass of ten pounds must, and does, fall in one tenth of the time occupied by the falling of one pound: and, as Galileo had made them fall in the same time, the only result was an inveterate hostility against him. About the same time don Giovanni de' Medici had projected a scheme for cleansing the port of Leghorn, on which Galileo, being consulted, gave an unfavourable opinion, which was afterwards fully justified by the failure of the plan. This, however, excited violent hatred and machinations against him on the part of the projector, which, in conjunction with the odium already called forth among the scholastics, determined Galileo in quitting Pisa, and taking a similar situation, just then offered him, at Padua (1592), under the patronage of the state of Venice.

In this appointment his writings and discoveries multiplied fast; and, with the prodigality of genius, he allowed many of his researches to get circulated among his friends, and even to be published surreptitiously, and claimed by others as their own, without notice.

In the same year, 1592, Galileo published a treatise "Della Scienza Mecanica," in which, after examining the theory of the mechanical powers, he lays down the general proposition, that all the advantage given by them is simply this: they make a small force equivalent to a great one, by causing the former to move over a proportionally greater space in the same time. And, in following out this principle, he further shows, that if the effect of a force be estimated by the weight it can raise to a given height, in a given time, this effect can never be increased by any mechanical contrivance whatsoever. He proceeded also to the theory of the oblique lever and the inclined plane, though upon principles more purely mathematical than those of Stevin.

In the theory of motion he had, as we have seen, investigated some of the laws of falling bodies; he extended this enquiry by first assuming that they receive equal increments of velocity in equal times; and thence deduced mathematically, that the *spaces described* must be *as the squares of the times*, and that the space fallen through in one portion of time is exactly half that which would be described in the same time with the velocity last acquired, continued uniformly. He soon saw that a body descending on an inclined plane must be, in like manner, accelerated: he therefore adopted this as a simple mode of putting his theory of falling bodies to the test of experiment.

From his knowledge of the inclined plane, it easily followed that the times of falling down all the *chords* of a circle, terminating at its lowest point, must be the same. He, however, fell into an error in maintaining the same of the circular *arcs*, and in applying this to the vibrations of the pendulum.

Knowing the law of falling bodies to give spaces proportional to the squares of the times, he deduced the motion of projectiles, and showed their path to be a parabola. This reasoning involved a principle which Galileo does not expressly refer to by name, but which is, in fact, one of the cases of the general principle of composition of forces.

The question as to the motion of projectiles had been put by Aristotle, but without any solution. Some of the early writers on gunnery noticed that for certain distances the gun must be pointed upwards: and Thomas Digges, in his "New Artillerie" (1591), had remarked that the ball has, from the beginning, a downward motion, which, though insensible at first, draws it from its direct course. Tartalea supposed the ball's path to be made up of an ascending and a descending straight line, connected in the middle by a circular arc. To these imperfect notions succeeded the sound and philosophical theory of Galileo.

The mechanical and dynamical researches of this great philosopher are unquestionably those in which we recognise the first real union of experimental and mathematical reasoning in investigating the laws of force and motion.

It was at Padua that Galileo first avowed himself a convert to the Copernican system. The earliest notice of this appears in a letter to Kepler, returning thanks for the present of his "Mysterium Cosmographicum," in 1597. He seems, however, to have adopted the Ptolemaic hypothesis in his public lectures for some time after he had embraced the new doctrine; but it must be borne in mind how little of decisive proof had yet been offered in support of it. A work on the sphere, however, which is decidedly Ptolemaic, long ascribed to Galileo, appears to be of very doubtful authenticity. Both on this occasion, and afterwards, he professed great esteem for Kepler, though he did not conceal his dissent from his strange hallucinations. "Kepler," said he, "possesses a bold and free genius, perhaps too much so; but his mode of philosophising is widely different from mine."

Galileo's reputation was now beginning rapidly to increase. His lectures were attended by many persons of the highest rank; and when, according to the constitution of the university, the period arrived for a new election, he was not only re-appointed, but his salary considerably augmented.

The appearance of a new star in 1604, before referred to, of course attracted the diligent attention of Galileo. Crowds flocked to his lecture-room, to hear his comments upon it; and he appropriately took occasion to contrast their eagerness at the novelty and splendour of this spectacle, with their negligence in studying the ordinary appearances of nature, not at all less wonderful, and far more instructive. He showed distinctly, from this star having no sensible parallax, that it must be situated far beyond the region of our atmosphere, to which the Aristotelians referred all comets and transitory meteors, and to whose apprehension a new and variable body engendered in the perfect and unchangeable celestial spheres was utterly contradictory.

Notwithstanding the hostility which these avowals of free enquiry produced, Galileo was a second time reelected. His lectures were so thronged, that he was often obliged to adjourn to the open air, and his reputation extended far and near. About this period he seems to have first invented the thermometer, or rather a thermoscope*; for the former name could not be properly applied until the adaptation of a fixed scale at a later period. The instrument is mentioned in a letter from Sagredo in 1613, but how long before the invention had taken place is uncertain. Even this date, however, is prior to that of Drebel (1620), who may yet have devised it independently, and was certainly the first who introduced it in Holland.

Galileo's thermoscope was a tube ending in a bulb, inverted in a vessel of water, the bulb being occupied by air. Leopold de' Medici, some years afterwards, filled it with spirits of wine, and sealed the open end while the spirits were boiling. Mercury was finally substituted by Lana in 1670.

Galileo was soon after engaged in a dispute with Balthasar Capra, who appears to have pirated his invention of a scale of proportional parts. He also turned his thoughts to the subject of magnetism, bestowing high commendations on Gilbert, with whom the experimental investigation of this science had originated.

In 1609, proposals were made to him to return to Pisa. Cosmo de' Medici, who had now succeeded his father as grand duke of Tuscany, regretted that so masterly a genius had been allowed to leave the university, which he should naturally have adorned. Galileo's answers to these overtures are extant, and give a highly interesting and characteristic picture of his views, designs, and disposition: he, however, did not accept the offer.

The year 1609, the same in which Kepler's commentary on Mars appeared, is also for ever memorable, from Galileo's invention of the telescope. This, indeed, is, in the minds of many, the sole important discovery associated with his name; whilst, again, some

* Θερμος, hot; σκοπεω, to observe; μετρεω, to measure.

writers have contended that it adds but little to his reputation. Without disparaging his other exalted merits, we, however, regard this as constituting one of his fairest claims to that immortality of fame with which he has been so justly invested.

Lenses, as we have already observed, were known long before this time, and used to assist vision; and hints, more or less obscure, may certainly be traced in the records of earlier times of effects produced or expected from combinations of lenses. *After* the announcement of the construction of the telescope by Galileo, several writers were not backward in ascribing the principle of it to others. These claims rest entirely on passages the meaning of which is *ambiguous*. Fracastoro and Baptista Porta have been named among the claimants, as also Digges and De Dominis. Again, Jansen and Lipperhay, two Dutch spectacle-makers, contest the priority of having combined two lenses, so that distant objects were seen through them magnified and *inverted*.

Galileo himself states that he had heard a rumour of such a contrivance. He immediately applied himself to consider, upon optical principles, in what way such an effect could be produced; and at length constructed a telescope, which showed distant objects magnified and *erect*. He did not stop to investigate the subject in all its bearings, but, satisfied for the moment with the success he had obtained, he eagerly proceeded to make further improvements upon the construction, and to enlarge the powers of the instrument, so as to render it available for the purposes of astronomy.*

The principle of the telescope and the microscope are, to a mathematical optician, one and the same. The telescope is merely made to collect parallel rays from distant objects; the microscope, diverging rays from near objects. The latter invention, therefore, could hardly fail to follow immediately upon the former. Galileo constructed microscopes in 1612; but he did

* The whole evidence is admirably discussed in Drinkwater's Life of Galileo, p. 24. See also a paper by prof. Moll., Roy. Inst. Journ. i. 319.

not dwell upon the invention, his thoughts being now wholly absorbed on the perfection of the telescope, and the glorious field of astronomical discovery which was laid open to him.

Being at Venice, his house was thronged with visiters, who came to satisfy themselves of the truth of the wonderful stories they had heard of his invention. The doge suggested that a telescope would be an acceptable present to the state. Galileo took the hint, and was in return confirmed for life in his professorship at Padua, and his stipend doubled. The public curiosity on the subject was excited to the highest pitch. Sirturi, who had made one of these instruments, attempting to try its powers from the top of the tower of St. Mark's, in Venice, was soon observed by the crowd, who detained him for hours to satisfy their curiosity in looking through his telescope. Instruments of an inferior sort were now made every where, and spread rapidly over Europe; but the manufacture of the superior kind was confined almost solely to Galileo, and those whom he instructed.

"The invention of the telescope," professor Playfair observes, "is the work in which (by following unconsciously the plan of nature in the formation of the eye) man has come the nearest to the construction of a new organ of sense. . . . . A series of scientific improvements, continued for more than 200 years, has continually added to the perfection of this noble instrument, and has almost entitled science to consider the telescope as its own production. . . . . After the invention of the telescope, that of the microscope was easy; and it is to Galileo that we are indebted for this instrument, which discovers an immensity on the one side of man scarcely less wonderful than that which the telescope discovers on the other. The extension and divisibility of matter are thus rendered to the natural philosopher almost as unlimited as the extension and the divisibility of space are to the geometer." *

* Dissertation on the Progress of Science, pp. 248—250. WORKS, vol. ii.

Now that the telescopic appearance of the heavens is so familiarly known, it is hardly possible for us to conceive the intense interest with which the first glimpse of it must have been obtained. The multiplicity of the brilliant objects calling for examination, the undefined expectation of what might be revealed in them by the powers of an instrument yet untried, and the probability of numerous additions to the list of those bodies which had as yet come under the cognisance of man,— these, and the host of kindred emotions which must have been excited on such an occasion, are more readily imagined than described; and they must have united to give an overwhelming impulse to the progress of observation.

Galileo, having sufficiently improved upon his instrument, now began assiduously to direct it to the heavens. The moon naturally formed the first object of his attention; and we cannot fail to recognise the original of our great poet's picture, since we know he had the opportunity of painting from the life: —

> . . . . . the moon, whose orb
> Through optic glass the Tuscan artist views
> At evening, from the top of Fesolè,
> Or in Valdarno, to descry new lands,
> Rivers, or mountains, in her spotty globe.
>
> *Par. Lost*, i. 558.

Jupiter formed the next object of examination; and no sooner was the telescope pointed to that planet, than the existence of the satellites was detected, and their nature soon ascertained (February, 1610). These and other observations were described by Galileo in a tract, entitled "Nuncius Sidereus," which excited an extraordinary sensation the moment it appeared. Many positively denied the possibility of such discoveries; others hesitated; all were struck with astonishment. Kepler describes, in a letter to Galileo, the impression made on him by the announcement. He considered it totally incredible; nevertheless, his respect for the authority of Galileo was so great, that it set his brain afloat on an ocean of conjectures to discover how such a result could be rendered compatible with the order of the celestial orbits, as determined by the five regular solids. Sizzi argued seriously with Galileo, that the appearance must be fallacious, since it would invalidate the perfection of the number 7, which applies to the planets, as well as throughout all things natural and divine. Moreover, these satellites are invisible to the naked eye; therefore they can exercise no influence on the earth; therefore they are useless; therefore they do not exist.

Others took a more decided, but not less rational, mode of meeting the difficulty. The principal professor of philosophy at Padua pertinaciously refused to look through the telescope. Another pointedly observed, that we are not to suppose that Jupiter has four satellites given him for the purpose of immortalising the Medici (Galileo having called them the Medicean stars). A German, named Horky, suggested that the telescope, though accurate for terrestrial objects, was not true for the sky. He published a treatise discussing the four new planets (as they were called), what they are? why they are? and what they are like? concluding with attributing their alleged existence to Galileo's thirst of gold. From misunderstanding a passage in Kepler's writings, he imagined Kepler to be an opponent of Galileo, and went to the former with his book, anticipating a triumphant reception. He was received in a way which effectually set him right. Kepler relates the interview in a highly characteristic letter to Galileo. Horky begged hard to be forgiven: Kepler made him promise to *see* the satellites when he showed them.

Galileo's fame, and that of his telescope, meanwhile were universally increasing. The grand duke begged to have the original telescope deposited in the museum at Florence; to which Galileo willingly consented. An old instrument was shown there not many years ago, said to be the same; but some sceptics have called in question its genuineness. The grand duke made Galileo his own philosopher and principal mathematician, with a salary of 1000 florins. The truth of his discovery was now acknowledged. The tide turned; and observers began to discover more satellites: some went as far as twelve. But a short time sufficed to show their fallacy; and the four satellites of Galileo retained their places.

The news of the telescopic discoveries reached England in 1610. Harriot (already mentioned as a mathematician) sedulously cultivated astronomy, and had thrown out, from analogical considerations, the idea that there might exist secondary planets invisible from their size and distance. He was the first to verify the results of Galileo, and the observations were eagerly repeated by his friend, sir W. Lower. They jointly carried on their telescopic researches into the various other phenomena which now crowded upon their notice; and some highly interesting records of their labours are extant, which afford a lively picture of the intense interest which these discoveries had excited.* It appears that telescopes were made in London in 1610; but, so little was the art founded on any good principles, that the observer furnished himself with a great number, out of which he had the chance of finding some which were good.

Among the earliest objects of attention, both to Galileo and Harriot, were the spots on the sun. There is considerable uncertainty as to the precise date of the first observation of them, but little doubt exists as to Galileo's claim to priority; though he did not publish any account till 1613. The description of his observations is contained in a letter written in May, 1612, in which he speaks of having commenced these observations eighteen months before; but we cannot here enter into a discussion of the question. From the spots, Galileo deduced the rotation of the sun on his axis. A spot observed by Kepler had by him been supposed to be Mercury passing the sun's disk: he renounced this opinion immediately on learning Galileo's discovery.

* See prof. Rigaud's Supplement to Bradley's Memoirs. Oxford, 1833.
† Id. p. 36.

Galileo's observations on the moon gave additional support to the solar system, from the entire confirmation they afforded to the effects of reflected light seen in the lights and shadows on the moon's surface. The Aristotelians had determined that the moon was a perfect body. Hence they loathed the doctrines of Galileo, who took delight, they said, in distorting and ruining the fairest works of nature. Lodovico delle Colombe endeavoured to reconcile the contending parties, by suggesting that the apparently hollow parts were filled up with a pure transparent crystalline medium.

The appearance of a slight illumination in the dark part of the new moon (commonly called the old moon in the new moon's lap), was explained by Galileo, as it had been by Leonardo da Vinci and Mæstlin, to arise from "*earth-shine.*" This was another source of offence to the Aristotelians, who could not condescend to admit that the earth could shine like a planet. Examining the fixed stars, he was at first disappointed at finding no increase in their sensible magnitudes; but was equally astonished at the vast multiplication of their numbers. Directing his telescope to the milky way, he discovered its real nature, as consisting of a multitude of small stars: and extended the same conclusion to other nebulæ.

The planet Saturn next engaged his attention: and he describes in detail the perplexities in which he was involved from the apparently anomalous figure of the planet; sometimes single, sometimes *with a body on each side, or triple.* He recorded this observation in an anagram, as also he did another telescopic discovery, *the phases of Venus.* This was one of the most decided proofs of the Copernican system, and a fulfilment of Copernicus's prediction.* The supposed *absence* of such phases had been urged by a distinguished Aristotelian as a proof of the old system. Galileo also noticed the less conspicuous changes of phase in Mars.

On receiving the appointment from the grand duke Cosmo, he resigned his chair at Padua; which occa-

* See p. 136.

sioned not only regret, but even offence, in the state of Venice, who had patronised him there. In all other places he was received with marks of the highest estimation. Going to Rome in 1611, all ranks vied in paying him attention; and evinced the greatest anxiety to witness the facts he had observed. In 1612, on his return to Florence, he published his discourse on floating bodies, in which he explains the principles of that part of hydrostatics with great accuracy; noticing, among other points, the remarkable experiment called the hydrostatic paradox.

In December, 1612, Galileo was much perplexed by the disappearance of the *lateral appendages* he had before noticed in the planet Saturn: but no adequate theory seems to have suggested itself of their true nature.

In 1615, we find him entering warmly into various schemes, then under the consideration of the Spanish government, for the discovery of the longitude; and, in particular, suggesting the use of the eclipses of Jupiter's satellites for the purpose.

The year 1618 was remarkable for the appearance of three comets: these gave occasion to much discussion, in which Galileo, at least, displays his caution in observing that the distance of these bodies cannot be accurately determined from their parallax. He, probably, still inclined to the notion of their being only a species of meteors.

Galileo's telescope was composed of a convex object glass and a concave eye glass,—precisely the same construction as that now adopted for the common opera glass. Kepler, in his "Dioptrics," had investigated the more general theory of the combinations of lenses, adjusted with the principal foci in coincidence, and so (in theory) forming telescopes; and among them had treated of the case when both lenses are convex, and are placed at a distance equal to the sum of their focal lengths, giving an inverted image. This is the construction called the astronomical telescope. But none of this kind were made till Scheiner took up the invention and constructed such; of which he gave an account in a treatise called "Rosa Ursina," in 1650.

This able observer, indeed, has contested with Galileo the first observation of the spots in the sun; but the evidence appears decidedly in favour of the latter. Scheiner was a monk; and, on communicating to the superior of his order the account of the spots, received in reply from that learned father a solemn admonition against such heretical notions:—"I have searched through Aristotle," he said, "and can find nothing of the kind mentioned: be assured, therefore, that it is a deception of your senses, or of your glasses."

Notwithstanding such prejudices in some quarters, the progress of the new opinions was rapid among all orders. One instance is somewhat remarkable.

In 1615, Paul Antony Foscarinus, a Carmelite monk, composed, under the patronage, and by the desire of, Vincenzio Caraffa, a Neapolitan nobleman, an essay in explanation and defence of the Copernican system. He conducts his argument with considerable address and ingenuity: and meets the prejudices of his readers in such a way as to avoid offending them. In particular, he enters largely into specious interpretations, with the view of reconciling to the doctrine of the earth's motion and the sun's immobility, the several passages in Scripture which express the contrary. The whole is conducted with high and, probably, sincere professions of piety, and reverence for the authority of the church; and at the same time the author expresses his hope that his attempt will be found acceptable to philosophers, especially to Kepler and Galileo. The book is dedicated to the general of the order of the Carmelites; and was published at Florence, with the sanction of the ecclesiastical authorities, in 1630. The case of Foscarinus is interesting in several points of view, and affords a singular contrast, as well to that of Bruno, before mentioned, as to that of Galileo himself, as we shall presently see. It does not appear, at first sight, easy to reconcile the indulgence with which the former was received, with the harsh treatment experienced in the other cases, where precisely the same system was inculcated. We shall, however, take occasion to recur to this subject at a future stage of our history; and will now return to Galileo.

The boldness with which his speculations were carried on, and the uncompromising freedom with which he advocated his views, and attacked those of the Aristotelians, soon attracted the notice of the Inquisition; and, in 1615, he was summoned to appear at Rome to meet the charges which might be brought against him. Meanwhile his powerful friends, and especially the grand duke, seem to have exerted their influence in his favour as much as they dared; and succeeded so far as to obtain a mitigation of the severity of the proceedings. The decision of the tribunal was, that cardinal Bellarmine should, on their behalf, reprimand Galileo, and enjoin him to abandon and cease to teach his false, impious, and heretical opinions. This was accordingly done, Feb. 26. 1616, under the threat of imprisonment and severer measures if he refused. Galileo could, of course, do no otherwise, in common prudence, than submit; and accordingly, on making this forcibly extorted promise, he was discharged.

Returning to Florence, he continued his researches and studies with unabated ardour. This, indeed, was in no way at variance with the wrongfully extorted agreement into which he had been forced. And he employed himself in drawing up what was in fact a covert but complete defence of the Copernican system; though in no degree transgressing even the letter of his promise; since it was composed under the form of a dialogue, in which the case is fairly argued out on both sides, and the reader forms his own conclusion. In 1630, he had completed this great work, which was entitled "Dialogues on the Ptolemaic and Copernican Systems;" but considerable delay occurred in obtaining the necessary permission for its publication. It, however, appeared in 1632, with a dedication to the grand duke. These dialogues consist chiefly of illustrations of the author's various previous discoveries, and a developement of the arguments deducible from them; together with a full discussion of the several objections usually brought against the Copernican system, and the reasons by which it is supported. The dispute is carried on with infinite spirit between a Copernican, an Aristotelian, and a man of wit and acuteness, who is half a convert, and whose province it is to draw out the arguments of the other two. The tendency in favour of the Copernican system is obvious, though all the arguments on the other side are stated in the most forcible manner of which they are capable.

The difficulties as to the solar system derived from the Aristotelian notion of the perfection of the celestial bodies are cleverly combated. The analogy of the system of Jupiter and his satellites is forcibly urged. The phases of those planets which are so situated as to exhibit them, and the transits of the inferior planets over the sun, appearing as dark spots, are alleged as striking proofs that they shine by reflected light only. In particular, the author comments largely on the variable stars of 1604 and 1572, and on the spots on the sun, as entirely at variance with the unchangeable nature of the celestial bodies.

The physical objections which had been urged by the Ptolemaists against the earth's motion were, as we have already seen, chiefly grounded on certain alleged difficulties which it was asserted must follow from it: that a stone, dropped from a height, would fall far to the westward, from the rapidity with which the earth would move away from under it; birds in flight, in like manner, would be left behind; and even the atmosphere, and indeed all detached bodies, would be carried away. To these objections, the answers given before the time of Galileo were all grounded, more or less, on the same kind of metaphysical distinctions, by which it was attempted to be shown that these bodies are all parts of the earth, and therefore partake in its motion. The analogous case of a ball dropped from the mast-head of a ship in motion, and of a body thrown across the deck, were referred to; and it was contended, they, in like manner, would fall behind. This was admitted by the Copernicans, because, as they said, these bodies were not part of the ship.

The true way of answering was not attempted; namely, by trying the experiment. The real reply would have been, that the ball does fall precisely at the foot of the mast; and the body thrown strikes exactly the opposite point of the deck. In the case of a body dropped from a tower, there would, indeed, be an inappreciable deviation, owing to the velocity of the top of the tower being slightly greater than that of the surface of the earth; in the ratio of the height of the tower, to the radius of the globe. But, abstracting from this, the body falls at the foot of the tower. Galileo also showed that bodies thus in motion partake in the motion of the mass to which they belong, not by virtue of any such metaphysical principles as those referred to by the Aristotelians, but from mechanical causes dependent on the communication of motion.

The physical difficulties being disposed of, it remained only to compare the respective systems in an astronomical point of view. And here the advantage of the Copernican is palpable and immense. Its simplicity and uniformity is admirably and convincingly contrasted with the complexity of the Ptolemaic hypothesis, and the inconceivably difficult suppositions which the latter involves as to the velocity with which the heavenly bodies must move. Indeed, the Ptolemaists themselves were constrained to admit the superiority of Copernicus in this respect: they held out, not on these grounds, but on the physical objections drawn from the Aristotelian doctrine, and the decrees of the church.

On the subject of the tides Galileo is less happy. He suggests, indeed, a theory, but of a very unsatisfactory nature. The college of Jesuits at Coimbra appear to have first given a hint of the real cause in their commentary on Aristotle's book on Meteors: where they observe, it is "more probable that the moon raises the waters by some inherent power of impulsion, in the same manner as a magnet moves iron, according to its different aspects," &c. The same idea was maintained by De Dominis. Indeed, though not followed up, yet in some parts of the Dialogues we find curious allusions and hints thrown out as to the existence of a principle of universal gravitation.

The style in which the whole work is composed, is such as justly rendered it extremely popular; and professor Playfair observes, that "the Dialogues are written with such singular felicity, that one reads them at the present day, when the truths contained in them are known and admitted, with all the delight of novelty; and feels oneself carried back to the period when the telescope was first directed to the heavens, and when the earth's motion, with all its train of consequences, was proved for the first time." *

Though, as we have already remarked, the very form of the Dialogues fairly exonerated Galileo from the charge of violating the letter of his promise to the Inquisition, yet, with its characteristic jealousy, that tribunal soon discovered that as the Copernican side of the argument had the advantage, the author had violated the spirit of his engagement; or, even if he had not, yet had laid himself open to a vehement suspicion of a desire to do so, which was the same thing. And such a renewed attack upon the orthodox system, however covertly conducted, was not to be overlooked or forgiven.

The dialogues came out at a juncture which might have appeared to offer a peculiarly fair prospect of safety and impunity to the author: some of his most powerful enemies were now dead, and his old friend Urban was pope. But the inquisitors were bent upon his condemnation; and, unfortunately, there was a trifling circumstance which greatly aggravated the offence: Urban most unreasonably chose to imagine himself personally

* Diss. p. 190.

ridiculed in some parts of the dialogue. And so entire was the subjection under which all men were held by their spiritual tyrants, that, powerful as were the friends of Galileo, they were overawed, and could attempt no more than to obtain some slight indulgences in the manner of conducting the proceedings against him. The pope had the courtesy to send a private intimation of the painful necessity he was under of subjecting the Dialogues to the Inquisition: while the friendly ecclesiastics, and even the grand duke, could find no better way of employing their influence than in prevailing upon Galileo, as the safest course, to make all possible submission. Meanwhile, particular care was taken to exclude from the tribunal all who were in the least degree acquainted with the subject of the book, or the opinions of the culprit on which the charge of heresy was raised.

Galileo, *invited* to Rome, was received with every attention in the palace of Nicolini, the grand duke's ambassador, but was *recommended* to keep within doors. And when taken to the most holy office for the purpose of examination, he was not consigned to a dungeon, but lodged in the apartments of one of its officers. The secrets of the examination have never transpired; though hints have been thrown out of, at least, the threat of torture having been used. The result, however, was achieved of extracting from the prisoner the admission of being the author of the Dialogues, and a *free* and *unbiassed* declaration of willingness to submit and recant.

After a time he was brought up to receive his sentence, and make his abjuration (June 21. 1633). The tribunal solemnly delivered its condemnation of his works and opinions: extending to him, however, its merciful pardon, upon his abjuring his impious and heretical errors, and submitting himself, generally, to the authority of the church, and, in particular, to the salutary penance of imprisonment and certain penitential exercises.

Thus wholly in the power of the Inquisition, he of course uttered the compulsory abjuration, extorted from him with the same degree of free choice as the purse of the traveller by the pistol of the highwayman. He swore that the motion of the earth is heretical, and that he abjured all heresy. Moreover, as he rose from his knees he is said to have whispered in the ear of a friend, "E pur si muove."

It has been asked by some writers, in discussing the narrative of this disgraceful and revolting transaction, what result the church could expect from an extorted oath, and a compulsory abjuration, which might not be felt binding; and from a confession under fear of torture, which could be worth nothing. This, however, is to mistake the nature and object of the tribunal and the offence. The Inquisition was not a court of justice to try heresy as a crime; but rather a sort of spiritual board of health, whose office was to apply a salutary remedy, possibly a painful one, to stop the contagion of error, and, if possible, to restore the heretic to the pale of salvation. The object was not conviction, but submission; not truth, but profession: this being once obtained, by whatever means, the sole end was accomplished.

The inquisitors took unusual pains to publish Galileo's recantation all over Europe, thinking, no doubt, they were administering a complete antidote to the Copernican heresy. And a friar denounced his opinions from the pulpit, with a miserable pun upon the text, "Viri *Galilæi*, quid statis in cœlum suspicientes."

Actual imprisonment was continued only four days; when the victim was remanded to Nicolini's residence: ultimately he was ordered to be confined at Sienna; and this was considered to be fulfilled by a residence in the palace of the archbishop Piccolomini, which, after a few months, was allowed to be exchanged for his own villa at Arcetri. It is true, his advanced age and increasing infirmities alone would almost have confined him to his house; so that the sentence was little more than nominal: this, however, can in no degree diminish our detestation of the principle of its infliction.

Whilst restricted to his residence at Arcetri, Galileo could hardly venture to direct his attention much to astronomical studies, which, under the circumstances of his case, was both unsafe and could not but carry with it the most unpleasant associations. His mind recurred, therefore, with increased satisfaction to the favourite subject of his earliest enquiries, the doctrine of motion. The fruit of his speculations are presented to us in the "Dialogues on Motion," which were published in 1636; but as all his works, "edita et edenda," were placed by the Inquisition on the prohibited list, there was considerable difficulty in getting it printed. It appeared, however, at Amsterdam.

These dialogues are a continuation of those on the system, and are carried on between the same speakers. They contain the developement of all Galileo's researches on the theory of motion; and form, in fact, the most complete statement of the first principles of dynamics, as far as he had investigated them.

Among the principal topics of discussion, is that of the rectilinear descent of bodies. The author's experiments are related in full detail; but, owing to the imperfection of his methods, he deduced a result differing considerably from the truth. He enters largely into the refutation of the Aristotelian doctrine of the acceleration of a falling body by the air; and investigates the motion of projectiles, showing by distinct and satisfactory reasoning that their path in a vacuum will always be a parabola. In this, as well as some other parts of his researches, he distinctly introduces the great principle of the composition of forces, which he establishes on grounds at least sufficiently general for the purposes of these investigations.

It is somewhat remarkable, that a philosopher of alileo's penetration should not have entirely rejected the notion of nature's abhorrence of a vacuum. Yet he did not consider a vacuum impossible; for he describes an experiment in the Dialogues devised for the purpose of forming one. With regard to the pump, he certainly would seem to have still held the theory of suction; which is not only a very extraordinary circumstance in itself, but the more so because he describes an experiment by which he attempted to measure the weight of air as compared with water.

In another part of the Dialogues we have some curious anticipations of the time in which light is transmitted; an account of the vibrations of musical strings, and the coincidence of their pulsations as occasioning harmony; with a description of the original experiment, since so much enlarged upon, of the regular figures assumed by sand strewed on a plate made to vibrate. These subjects, together with that of the strength of beams, comprise the principal materials of these Dialogues.

After the completion of the "Dialogues on Motion," Galileo turned his thoughts again to the method of determining the longitude by the eclipses of Jupiter's satellites, and became involved in a discussion of that and other schemes. Morin, a French philosopher, had proposed, about 1636, a plan, which is, in fact, the same as the method of lunar distances. Galileo raised against it what were, indeed, insuperable objections at that period; the practical impossibility of executing it with the necessary exactness, both from want of tables and instrumental means. Galileo naturally preferred his own method, the practical difficulties of which do not seem to have struck him.

The application of the pendulum to clocks has been a subject of question, as to the priority of invention, between Galileo and Huyghens. We shall here only observe, that Galileo never claimed it; and, upon the whole, there does not appear to us any ground for depriving Huyghens of the credit of it.*

Galileo was now suffering from the increasing infirmities of age, as well as several severe attacks of illness: yet, such was the inveterate rancour of his persecutors,

* The evidence is discussed in Drinkwater's Life of Galileo, p. 98.

that he was for a long time refused permission to go to Florence, with a view to benefit his health; until, in 1638, leave was reluctantly granted, under close restrictions. In a few months, he returned to Arcetri. His sight now began to fail, and he shortly became totally blind. He had, however, occasionally continued to make astronomical observations; and had just noticed the phenomenon of the moon's libration, the last of the long list of his discoveries.

The restrictions of the inquisitors were now so far relaxed that his friends had free access to him; and persons of the highest rank and distinction crowded round him to express their admiration and sympathy, when they found it no longer unsafe. Many eminent men of other countries visited him, among whom we find Milton. Some, indeed, came to Italy for the sole purpose of seeing him, and enjoying his conversation, which retained all its charms.

In addition to his other infirmities, he at length became deaf; yet his intellectual powers remained unimpaired, and he used to complain that he found his head too busy for his body. He was, in fact, entering largely into mechanical speculations, with a view to continuing the "Dialogues on Motion;" but, in the midst of these employments, he was seized with a renewed attack of his complaints, and died January 8th, 1642.

The ecclesiastical powers disputed his will, as being that of a heretic; and at first refused him burial: this was at length permitted, though in a very obscure manner; and no monument was allowed to be erected over him. Medals, however, were struck in commemoration of him by his disciple Viviani. About a century after, a monument was erected in the church of Santa Croce, at Florence.

Lastly, to complete the triumph of bigotry, a collection of his unpublished MSS. in the possession of his family, was subjected to the expurgation of the priests; and even what they suffered to remain in the possession of his grandson, Cosimo, was deliberately committed to the flames by his own hand, as a pious sacrifice before devoting himself to the life of a missionary.

We cannot better conclude our account of Galileo and his discoveries, than by quoting the able summary of his character and labours given by professor Playfair:—

"One forms, however, a very imperfect idea of this philosopher from considering the discoveries and inventions, numerous and splendid as they are, of which he was the undisputed author. It is by following his reasonings, and by pursuing the train of his thoughts, in his own elegant, though somewhat diffuse, exposition of them, that we become acquainted with the fertility of his genius—with the sagacity, penetration, and comprehensiveness of his mind. The service which he rendered to real knowledge is to be estimated, not only from the truths which he discovered, but from the errors which he detected; not merely from the sound principles which he established, but from the pernicious idols which he overthrew. His acuteness was strongly displayed in the address with which he exposed the errors of his adversaries, and refuted their opinions, by comparing one part of them with another, and proving their extreme inconsistency. Of all the writers who have lived in an age which was yet only emerging from ignorance and barbarism, Galileo has most entirely the tone of true philosophy, and is most free from any contamination of the times in taste, sentiment, and opinion." *

By the writings of Copernicus, of Kepler, and Galileo, the solar system, and the subordinate series of truths referring to the theory of motion, were so completely established and unanswerably demonstrated, that nothing was wanting but time to allow the opinions of men to come gradually round to the truth. The determined adherents of the old systems gradually disappeared from the scene, and the younger generation were open to profit by the light now afforded; and, in a few years, Tycho and Ptolemy had no followers.

* Diss, p. 143.

Reception of the new Discoveries by the Church.

The reception which the new philosophy met with among the authorities of the church, is too remarkable a point in this period of its history to be passed over without a brief remark or two.

It might, at first sight, appear, that a religion professing to stand on its own basis, could find little to affect it one way or another in the advance of experimental science; and, above all, a church like that of Rome, reposing on the claim to infallibility, it might be supposed would, of all others, be the last to feel any alarm at researches utterly unconnected with the authority of that claim: but it must be borne in mind, that the pretensions of the Romish church extended to the most unlimited authority over every thing. In particular, the Aristotelian philosophy had long since been incorporated, as it were, into its system; and its speculations were so closely mixed up with the scholastic theology, even to its physical details *, that it constituted almost an integrant part of the creed of the church: any attempt to impugn it was, therefore, heresy.

If we look at the actual treatment experienced by some of the principal advocates of the new discoveries, there is a difference observable which, though at first sight somewhat unaccountable, yet, upon a little consideration, may serve to give us a more instructive insight into the real nature of the case. Copernicus and Foscarinus taught the very same doctrines as Bruno, Kepler, and Galileo; but the former were, as we have seen, received into the highest favour, while the latter were persecuted with unrelenting severity. Yet were they not all equally guilty of heresy, and equally dangerous enemies to religion? Their doctrines were, indeed, the same; but we may have observed a considerable dif-

* For a full explanation the reader is referred to Dr. Hampden's Bampton Lectures, pp. 191. 334. Oxf. 1833.

ference in the manner and circumstances of their promulgation.

Copernicus cautiously kept his opinions, at first, within the circle of his personal acquaintance, and afterwards allowed them silently to make their way among astronomers, without any public discussion or appeal to popular judgment; and his writings were too profound, too dry and abstruse, to attract the notice of general readers.

Foscarinus manifested the most devout submission to the authority of the church; and insinuated the Copernican theory, under the shelter of an attempt to reconcile it with the dogmas of the theologian.

Galileo, on the other hand, as we have seen, could not refrain from plainly and publicly expounding the truths he had discovered, and that, too, in the most popular form of illustration. He not only attacked the tenets of Aristotle in abstruse disputations, but turned him into ridicule before crowded assemblies. His writings were of a nature to be read by every one: composed in a lively and pleasing style, they display by turns all the depths of science, the charms of eloquence, and the keenness of satirical humour, and cannot fail to animate every reader with a portion of the author's spirit.

Bruno, without any attempt at concealment or conciliation, boldly upheld the truths of inductive science, and vehemently denounced the errors of the schools.

Kepler even went out of his way to combat the positions of the priests, on points unconnected with his discoveries; though there can be no doubt that there were passages enough in his writings to ensure his condemnation with them.

Copernicus was hardly known for any other investigations than those connected with the solar system. Galileo acquired a widely-extended and popular fame from the more tangible invention of the telescope, and the train of brilliant discoveries to which it gave rise: all this conferred a celebrity on his more abstruse speculations, which alone they might have failed to acquire, and occasioned the more rapid promulgation of some knowledge at least of his discoveries among all classes.

Hence we may, even upon so slight a comparison, clearly perceive one ground of distinction, in this, that in the one instance the heretical poison was likely to be but little diffused, and that in a comparatively neutralised form, and only among those who were most secure from its bad effects: in the other, it threatened to infect the whole body of the people; and its noxious influence was enabled to spread through all the ramifications of society. But it was the *spirit* and *manner* in which the subject was taken up, the keen and powerful sarcasms, the uncompromising appeal to the unbiassed judgment, the rejection of the trammels of authority, and unrestrained boldness, whether in attack or defence, which mainly distinguished the obnoxious party.

In truth, the maintenance of Aristotle's doctrines by the church simply as abstract tenets, was, after all, but a point of secondary consideration. They did, indeed, afford support to the scholastic system of the theologians, but they had another more important practical use; their great value to the Romish hierarchy was, as auxiliaries to the spiritual tyranny they sought to exercise. The main beauty of Aristotle's philosophy, in their eyes, was, that it exhausted the whole subject of which it treated, and reduced it to a perfect system; so that no new discoveries could possibly add any thing to it.

To accustom their disciples, then, to this species of philosophy, and to instil carefully into their minds the utter falsity of all other kinds, had, of course, an obvious and invaluable application in bringing them into intellectual bondage; destroying even the inclination to attempt new enquiries in any subject; and habituating them to submit, without question, to a fixed and unalterable system. Hence the extreme importance of repressing all infractions of this discipline, as extinguishing every disposition to freedom of thought.

Thus it was a comparatively light offence to propose any philosophical hypothesis in the way of abstract discussion, especially if done cautiously, and with unlimited professions of deference. But the unpardonable crime was, to display an open disregard of authority, and to advance opinions with a boldness and confidence which both evinced and called forth the exercise of unfettered judgment and free enquiry. It was not the Aristotelian dogmas which they cared so much to uphold, as the principle of unenquiring submission to authority. It was not the theory of the earth's motion which they sought to suppress, so much as the spirit of free discussion. In a word, it signified but little what a man's real views might be: if he only bowed down to authority, he was orthodox; if he dared to think for himself and avow it, he was a heretic.

On these grounds we may most satisfactorily explain the favourable treatment experienced by those who yielded to the ruling hierarchy, in their submissive and conciliatory manner of introducing the obnoxious tenets, as contrasted with the bitter unrelenting cruelty with which such men as Galileo were persecuted. In the eyes of the ministers of an intellectual and moral despotism, they were necessarily objects of perpetual suspicion and hatred. In an establishment grounding its ascendency, not on reasonable convictions, but on arbitrary authority, and the prostration of reason, the doctrines of the school of Galileo must ever be regarded with aversion and dread.

But persecution, instead of supporting the cause of the papal tyranny, no doubt tended to exasperate the hostility already excited against it, and to hasten its downfall. It in no degree tended to check the progress of knowledge, but rather promoted its extension: and the unshackled discussion of physical truth has since progressed with accelerated rapidity. Men's eyes have been more and more opened to the real laws and order of nature; and in proportion they have been brought to recognise fresh proofs of design in every part, whilst the profession of Christianity has been in no degree impaired.

Thus, however, was the church arrayed in mortal hostility against science, and thus ineffectual was that hostility; but, in other instances, we have noticed a more pacific spirit, and attempts at an accommodation.

Foscarinus, we have seen, sought to reconcile the church to the solar system, by that species of ingenious glosses, and casuistical commentaries on the opposing texts, which the Romish theologians knew so well how to employ in other cases, and by which any required sense might be put upon any passage. He at least succeeded in his attempt so far as to screen himself from persecution.

The case of Tycho Brahe affords a remarkable and not uninstructive contrast. He in like manner pursued the path of reconciliation between opposing systems. But Tycho was a sincere and zealous Protestant; and, in the extreme Protestantism of his day, he worshipped the very letter of every part of Scripture, as devotedly as the Romanist did the decrees of his church. Hence he was no less scandalised at the idea of the earth's motion; and we have seen the way in which he got rid of the difficulty.

Thus the monk attempted to bend Scripture to fact, and the Protestant to bend fact to Scripture; but both attempts were equally futile. The orthodox expositions of Foscarinus are unknown, and the pious theory of Tycho is exploded and forgotten; the Copernican heresy has triumphed; yet the essential truths of Revelation stand unimpeached and unimpeachable on the rock of their proper moral evidence; whilst natural theology has found, in this very system, the most powerful of all its arguments.

But still, are there not actual contradictions? and how are we to get over them? The words of Galileo, in his admirable letter to the Grand Duchess of Tuscany, give a reply to which, even at the present day, we can hardly add any thing more forcible: — " I am," he says, "inclined to believe that the intention of the sacred Scriptures is to give to mankind the information necessary for their salvation; and which, surpassing all human knowledge, can by no other means be accredited than by the mouth of the Holy Spirit. But I do not hold it necessary to believe that the same God who has endowed us with senses, with speech, and intellect, intended that we should neglect the use of these, and seek by other means for knowledge which they are sufficient to procure us. . . . . . . In the discussion of natural problems, we ought not to begin at the authority of texts of Scripture, but at sensible experiments and necessary demonstrations; for, from the divine word, the sacred Scripture and nature did both alike proceed; and I conceive that, concerning natural effects, that which either sensible experience sets before our eyes, or necessary demonstrations do prove unto us, ought not upon any account to be called into question, much less condemned, upon the testimony of scriptural texts, which may under their words couch senses seemingly contrary thereto."

In a word, the *object* of Revelation is of a kind entirely distinct from the inculcation of science; and the *incidental* parts of any book must, in all common reason and fairness, be regarded in a totally different light from its *essential* points.

But it will be said, no one now doubts the truth of the solar system; nor is any one led to reject Revelation on the ground of its being at variance with it.

Yet the fact is, the very same difficulties and objections are still alleged by many at the present day; not indeed with regard to the solar system, which they (very inconsistently) admit, but in reference to the discoveries in other parts of science, and especially in geology. We have, at the present day, zealots animated by as bitter a spirit of persecution, though happily without the power of exercising it, as those of the Roman tribunal. We have also, "mutato nomine," our Tycho and Foscarinus; but we shall profit little by the experience of history if we do not learn to avoid the errors of that period: and we shall assuredly find the very same principles, so eloquently advocated by Galileo, to be those which alone can effectually secure either religion or science from abuse and perversion.

SECTION IV.

THE CONTEMPORARIES AND SUCCESSORS OF GALILEO. — THE BACONIAN PHILOSOPHY, AND THE PRECURSORS OF NEWTON.

In proceeding to trace the advance of science as promoted by the labours of the contemporaries and immediate followers of those great men, whose pre-eminent discoveries have demanded our almost undivided attention in the preceding section, we find various departments of physical research, in their hands assuming a widely extended and highly improved character.

We shall, in the first place, mention some important improvements in mathematics, which belong to the early part of the seventeenth century, and shall then proceed to review the philosophy of Bacon, the promulgation of which constitutes so leading a feature in the literature of the age. We shall afterwards survey the varied labours of that illustrious train of philosophers, who were the disciples of Galileo and Bacon, and the precursors of Newton; including one who added involuntary evidence to the truth of their principles, by the attempt to set up an imposing but delusive theory of a different kind, which shot across the philosophical horizon like a brilliant meteor, containing the elements of its own explosion.

Improvements in Mathematics.

In former periods of scientific history (as we have already had occasion to observe), the progress of pure mathematics had very little connection with that of physical knowledge. In the age of which we are now treating, when great changes had begun to operate on the characters of the different branches, and on the relation between them, we find the progress of each much more closely dependent on that of the other. In proportion as the combination of mathematics with physical research was more generally recognised, the advances of the latter created a necessity for some material improvements in the former: and this necessity, once felt, was the most effectual means of calling forth the resources of mathematical genius, to supply those methods which the exigencies of physical investigation demanded.

The discovery of Kepler's laws had exhibited a striking instance of the connection of phenomena by numerical relations; but the discovery had involved an amount of labour, in the mere work of calculation, almost beyond belief. If we look only at the *successful* theories of the author, the mass of figures through which he worked to establish them was incredible; and these theories only presented some general laws, to which the increasing discoveries of astronomy soon added the necessity of numerous supplementary investigations, all alike requiring toilsome and extensive computations, not only to verify them in the first instance, but afterwards to turn them to practical account in the various applications which were to be founded upon them.

With such quantities, for instance, as the sines and tangents of the tables, taken only to five or six places of decimals, so simple a calculation as merely finding a fourth proportional is extremely troublesome if it occur often; and, for any long series of calculations, occasions a most intolerable sacrifice of labour and, what is more serious, of time; while the extraction of roots and more complex operations, often repeated, would soon become appalling to the most resolute calculator. In fact, with the methods in use about the beginning of the seventeenth century, the single circumstance of the enormously increasing labour of calculation must almost necessarily have seriously impeded, if not altogether stopped, the progress of astronomical and physical research, which, in other respects, was now beginning to assume so promising an aspect. Thus, some methods of abridging the overwhelming toil of the computer were daily becoming of more imperious necessity; and we now find a remarkable example of the powers of original inventive genius arising to remove these difficulties, and to supply the so much desired instrument of calculation, precisely at the period when it was most needed.

Napier. — Logarithms.

Whether we choose to estimate the value of inventions by their practical utility, or by their refinement of principle, we must in every sense esteem, as one of the very first importance, the invention of Logarithms, by Napier of Merchiston. He was born of a noble family, in 1550; and enjoyed all the advantages which the best education attainable in those times could bestow. He appears early to have turned his mind to arithmetical and astronomical studies. He soon felt the increasing difficulties we have just mentioned, and was led to try various mechanical devices for abridging these processes. One of these is known by the name of Napier's rods or bones (from the substance of which they were made), which afford an ingenious mechanical help in multiplication. He explained this method in a work called "Rabdologia," in 1616; but, viewing the subject in all its bearings, he soon perceived a far higher mathematical principle, which afforded a method as simple as powerful, and which he not only discussed in theory, but reduced into a practical form.

It has been said, that a hint at least of the principle may be found in the writings of Archimedes: this, however, is only so far true as that a remark of that philosopher exactly serves to suggest the main difficulty which it was Napier's particular merit to overcome.

The first suggestion of the principle may be thus explained: — If we suppose a series of numbers in *geometrical progression,* any one tolerably conversant with arithmetic will see that, if any two terms in such a progression are multiplied together, the product will also be a term in the same series, and will be found by inspection, if the series has in the first instance been carried far enough. And we can always tell *at what term* it will be found: it will be that term whose *number,* reckoning from the commencement, is the *sum* of those of the two terms which are to be multiplied. Thus, to find the product of the third and seventh terms, we have only to take that number which forms the tenth in the series. It is also evident, that these *numbers* of the terms are also the *indices* of the powers of the common multiplier, which enter into each term respectively. This is the leading idea which may be supposed suggested by a passage in Archimedes.

Thus, if our computations always involved no other numbers than such as are terms in a geometrical progression, we should only have to *add the indices,* and thus be led directly to the product; or, conversely, to subtract them, and find the term which is the quotient. Again, by doubling, tripling, &c. the indices, we should find their products indicating a term which would be respectively the square, cube, &c. of the original term; and, conversely, we should have the square, cube, &c. roots. Thus far all was sufficiently clear and simple.

But here arose the main difficulty: this would apply only to a very few limited systems of numbers, and could not be of any general practical utility. The grand discovery of Napier, therefore, amounted to this: —that a geometrical progression may be found in which ALL *the natural numbers* are terms.

Of the methods by which he arrived at this conclusion, or of the general principle on which such a series can be assigned, we cannot here say much; but we may sufficiently illustrate it by an example. If we suppose a geometrical series whose first term is 10 and whose common multiplier is likewise 10, it is evident that the second term will be 100, or 10 in the power whose index is 2; the third will be 1000, or 10 in the power of 3, &c. But since in algebra the notion of powers includes those whose indices are any numbers, whole or fractional, there may be terms intermediate to these, which shall be powers of 10 whose indices are fractions, or mixed numbers intermediate to the whole indices. Upon this principle we should have, for example, 40, as a term in the series between 10 and 100, which is a power of 10 whose index is the mixed number 1·602. Again, we may carry the series backward to numbers below 10, and we find 5, a power of 10 whose index is the fraction 0·6989; and 1 is, upon a well known algebraical principle, the power of 10 whose index is 0.

In a word, all the natural numbers may find their places, by interpolation somewhere among the terms of such a series, and the corresponding *indices* are called their LOGARITHMS.* Different systems of logarithms will be formed, according as different geometrical series are assumed. To find the means of assigning such series, and to calculate the indices belonging to the natural numbers, or, conversely, the numbers from the indices, has been the subject of the profound labours of modern analysts, who have devised various methods for the purpose: but the undivided honour of having first accomplished such a work belongs exclusively to Napier.

It has been said, that a hint of some method of the same kind, ascribed to Longomontanus, had been given to Napier; but, from the total absence of all traces of such a principle in the writings of the former, and even of any claim or pretence to it, we can attach no importance to the story. Napier associated in his labours his friend Mr. Briggs, who conversed freely with him on the improvements of which his system was susceptible. Napier, however, published his "Canon

* Λογος, ratio, αριθμος, number; the numbers or measures of ratios.

mirificus Logarithmorum" in the form he had originally planned in 1614; and left to Briggs the task, which he subsequently brought to perfection, of calculating a new set of tables, upon another system or series, with a different base, which presented many practical advantages.

The great principle of Napier's invention is of such a nature as must always claim admiration for its singular combination of simplicity and power; but we cannot appreciate his full merit without recollecting the state of algebra in his time. The general theory of indices was hardly at all understood: nor were the forms of series investigated so as to afford him any facilities for his calculations. Hence, to conceive the fundamental idea that all numbers might be regarded as some powers of one given number, and to devise the actual means of finding the indices of those powers, must be allowed to have been indications of genius of the highest order. The existing state of knowledge had by no means prepared the way for any such conceptions; and all the means of perfecting them were supplied from the original resources of the inventor's powerful mind. In another respect, too, this invention has been remarkable: most other discoveries have been produced at first in a comparatively crude state from the hands of their inventors, and have subsequently received their perfection from the successive labours of those who trod in the footsteps of the original discoverers; but the system of logarithms came out of the hands of its author so perfect, that it never received but one material improvement; and that it derived, as has been just remarked, from the ingenious suggestions of the illustrious inventor's friend, in conjunction with his own. Subsequent advances in science, instead of superseding this invention by better methods, have only enlarged the circle of its applications, and given more extensive confirmation of its utility.

The originality of the conception by which Napier illustrated his idea, and on which he founded his theorems, renders it worthy of notice; especially as principles not dissimilar were afterwards broached in reference to a different subject, and have given rise to great discussion. To find the relative places in the interpolation which must be occupied by the number and by the index or logarithm, he adopted the supposition of two points describing two different lines, the one with a constant velocity, and the other with a velocity always increasing in the ratio of the space which the point had already gone over. The first of these would generate magnitudes in arithmetical, and the second, magnitudes in geometrical, progression. On this principle he demonstrated his rule for finding the logarithms of numbers.

This, however, is not the sole monument of Napier's profound mathematical talent. The two theorems, or rules, which go by his name, absolutely involve all the most important cases of the solution of spherical triangles. They were published at the same time with his "System of Logarithms;" though before communicated in manuscript to Cavalieri, who mentions them with high commendation.

The intellectual character of Napier exhibits an instance of one of those singular inequalities which not unfrequently characterise high genius. Exact and comprehensive as were his views of mathematical truth, he could not discriminate other kinds; and engaged with all the sober assurance of certainty in a puerile commentary, in which he imagined he had decyphered all the mysteries of the Apocalypse. He died in 1622.

Among those who contributed greatly to the advancement of mathematical learning in this period, we must not omit to mention sir Henry Savile. He became distinguished in the university of Oxford for his knowledge of mathematics; and, about the year 1575, voluntarily read public lectures on that science. He afterwards attained several preferments, and became deservedly esteemed as a man of universal attainments. In 1619, he founded at Oxford two professorships, for astronomy and geometry, in the hope, as he expressly says, of giving some stimulus and encouragement to these studies, "at present so generally neglected in the university." His own lectures were published in 1620, and comprise an elaborate commentary on Euclid. He nominated, as the first occupants of the chairs he had instituted, Bainbridge and Briggs. He died in 1621.

Briggs has been already mentioned as the friend of Napier; and perfected his invention by calculating logarithms on another system, better adapted to practice; the same, in fact, as that since generally adopted. He also published the first part of a work called "Trigonometria Britannica." He was one of the early professors in Gresham college, founded about this period by the eminent merchant of that name in London; and which, for nearly a century, continued to be adorned by some of the first philosophers of the day; but has subsequently dwindled away into the formal repetition of certain lectures, for which an audience is hardly ever collected. Briggs's "Arithmetica Logarithmica," published in 1624, is remarkable for containing the first announcement of the law by which the co-efficients are formed in the involution of a binomial quantity to any integer power, which has since been so much extended, and has acquired so much importance. The relation of these quantities, and a mode of forming them, had, indeed, been partially shown by Stiphelius and Cardan.

The Philosophy of Bacon.

The zeal and ability with which Galileo had carried on his experimental enquiries; the boldness with which he exposed the deficiencies of the philosophy of the schools; the confidence with which he had appealed to the evidence of experiment and observation; and, above all, the brilliant success which had crowned his efforts, and the universal reputation which he had so deservedly secured ; — all concurred in giving such a powerful stimulus to the progress of experimental enquiry at that period, as it had never before, and, perhaps, has seldom since, experienced.

But forcible as was the *impulse* thus given, there was little to ensure the guidance of investigation in a right *direction*. Galileo had laid down very little in the way of systematic rules of philosophising. He had attacked and well nigh destroyed the artificial and delusive methods of the schools; but he had not (formally at least) erected any other system in their place. The Aristotelian logic had professed to supply infallible rules, and had afforded paths ready cut out, in which the excursions of philosophy might safely proceed; but there was as yet nothing of the kind substituted in their place, when all these were demolished. To supply such a substitute, and to exhibit a perfect system, distinctly opposed to that of Aristotle, was the design conceived and executed by the father of the inductive philosophy.

Francis Bacon was born in 1561; he was entered at Trinity College, Cambridge, in 1573; and, having been early recommended by his splendid abilities to the notice of his sovereign, he at length attained the eminent post of chancellor in 1618, with the title of baron Verulam. In the midst of his official duties he continued to cultivate philosophy; which, on his reverse of fortune in 1621, afforded him the highest consolation; while retirement gave him leisure for bringing to perfection those immortal works on which his fame has been established. He died in 1626, of a cold taken in the prosecution of a philosophical experiment.

But though he turned his attention to the actual labours of the experimentalist, and though there can be little doubt that numbers of those experimental illustrations with which his writings abound, and which, at the present day, the reader is tempted to pass over as sufficiently obvious, were really, at the time, highly original specimens of the mode of investigation he was recommending; yet, these instances were not of such a kind as to lead to any striking physical discoveries. The commanding elevation in which he stands as a philosopher, and even as the founder of inductive philosophy, is due rather to the comprehensiveness of his genius, which embraced the whole range and compass of natural science, seized the highest principles of almost every branch of physical enquiry, and traced out the method to be followed in bringing them to perfection: he foresaw the results which were afterwards to be obtained, and pointed out the process by which they would be arrived at. He surveyed, as from an eminence, the rich and varied region which was to become the domain of philosophy, and furnished a complete guide to the passes and roads by which the incursions of science were to be effected, and the subjugation of the territory to be ensured.

Though most of the writings of this eminent man are full of valuable illustrations and suggestions powerfully tending to the advancement of science, yet the work on which his fame chiefly rests, and by which his name is imperishably associated with the endurance of the inductive philosophy, is the "Novum Organon." The title was adopted in imitation of that of Aristotle's great logical system; and as that treatise contained a systematic classification of the various heads to which syllogistic reasoning was reducible, so the new "Organon" was in like manner designed to exhibit a similar technical system, under which the various species of experimental evidence might be arranged. This philosophical classification of the several heads of inductive argument is conceived with the most profound skill; and, when we consider the actual state of physical knowledge, our admiration cannot fail to be increased at the sagacity and penetration which could foresee and discriminate the particular classes of experimental facts which would always be the most available for eliciting their points of general agreement, and pursuing the process of generalisation. To put the subject in such a form, was also eminently useful in that age, as meeting the prepossession in favour of systematic method, and affording a substitute for the old system, presented under a similar form.

Some, perhaps, may think the whole discussion dry and tedious; may regard the names given to the different classes of arguments as fantastic, and consider the style formal and conceited: yet the profound observations which enrich every page, the luminous and masterly illustrations of the various positions, and the prophetic anticipations of after discoveries, can but call forth one sentiment of admiration at the transcendent ability of the author, and one general acknowledgment of the benefit which science has reaped from the system advocated in his work. Of that system we must now take a rapid survey.

After some introductory remarks on the existing state of science, the author proceeds to observe, that as things are at present conducted, a sudden transition is made from sensible objects and particular facts to general propositions, which are accounted principles, and round which, as round so many fixed poles, disputation and argument continually revolve. From the propositions thus hastily assumed, all things are derived, by a process compendious and precipitate, ill suited to discovery, but wonderfully accommodated to debate. The way that promises success is the reverse of this: it requires that we should generalise slowly, going from particular things to those that are but one step more general; from those to others of still greater extent, and so on to such as are universal. By such means we may hope to arrive at principles, not vague and obscure, but luminous and well defined, such as nature herself will not refuse to acknowledge.

Before laying down the rules to be observed in this inductive process, Bacon proceeds to enumerate the causes of error, the *idols*, as he terms them in his figurative language, or false divinities, to which the mind had been so long accustomed to bow. He considered this enumeration as the more necessary, that the same idols were likely to return, even after the reformation of science, and to avail themselves of the real discoveries that might have been made, for giving a colour to their deceptions. These idols he divides into four classes, to which he gives names, fantastical no doubt, but abundantly significant. The first are the "idola tribûs" (idols of the tribe); the causes of error founded in human nature in general, or on principles common to all mankind. "The mind," he observes, "is not like a plane mirror, which reflects the images of things exactly as they are; it is like a mirror of an uneven surface, which combines its own figure with the figures of the objects it represents." Among the idols of this class, we may reckon the propensity which there is in all men to find in nature a greater degree of order, simplicity, and regularity, than is actually indicated by observation. Thus, as soon as men perceived the orbits of the planets to be round, they immediately supposed them to be circles, and the motion in these circles uniform; and to these hypotheses, so rashly and gratuitously assumed, the astronomers of all antiquity laboured incessantly to reconcile their observations. The propensity which Bacon has here characterised so well, is the same that has been since his time known by the name of the *spirit of system;* and the history of modern science fully justifies his anticipation, that this cause of error would continue to infect the renovated philosophy: and it is likely always to be so, because, unfortunately, the illusion is founded on the same principle of association and combination from which our love of knowledge take its rise.

2d. The "idola specûs" (of the den) are those which spring from the peculiar character of the individual. He conceives each individual having his own dark cavern or den, into which the light is imperfectly admitted, and in the obscurity of which a tutelary idol lurks, at whose shrine truth is often sacrificed. He here remarks, that one great distinction in the capacities of men is derived from this, that some minds are best adapted to mark the differences, others to catch the resemblances of things: each of these tendencies easily runs into excess; and each individual has his peculiar liability to be misled by impressions of one or the other kind. The particular studies, also, to which a man is most addicted have a great effect in influencing his opinion, and warping his judgment.

3d. The "idola fori" (of the forum or market) are those that arise out of the commerce or intercourse of society, and especially from *language*, which may become the guide, and assume the government of our thoughts, instead of being only the conventional symbols to express them. This is nearly allied to the excellent remark of Hobbes, that words are the money of fools, but only the counters of wise men.

4th. The "idola theatri" (of the theatre) are the deceptions which have taken their rise from the systems or dogmas of the different schools of philosophy. Bacon's idea was, that each of those systems brought a representation of an imaginary world upon the stage: hence the name. They do not enter the mind naturally, like the former three; a man must labour to acquire them, and they are often the result of great learning and study. "Philosophy," he observes, "as hitherto pursued, has taken much from a few things, or a little from a great many; and, in both cases, has too narrow a basis to be of much duration or utility." The former kind he calls empirical philosophy; it takes all its principles from a few facts: such, in his time, was the philosophy of the alchemists. The latter he calls sophistical; and of this kind were all the physical systems of the ancients, the main part being superadded by the invention of the philosopher.

Bacon then proceeds to sketch the history of ancient philosophy, and to point out the circumstances which had hitherto favoured these perverse methods of philosophising. He shows the influence of false ambition on the one hand, and visionary hopes on the other; the pernicious effect of the reverence for antiquity, and the authority of great names; of the propensity to enquire into those things only which are rare and unaccountable, and the neglect of things of every-day occurrence. After these introductory, yet highly important parts of his work, the great restorer of philosophy proceeds in the second book to describe and exemplify the nature of that process of *induction* which he seeks to establish as the only true way of investigating physical truth.

The first object is to prepare a *history* of the phenomena to be explained, in all their modifications and varieties: he justly enlarges on the care, diligence, and fidelity, with which this part of the work must be executed. It is in this comprehensive sense that he uses the term natural history, both here and in other parts of his writings.

The next step is by a comparison of the different facts thus described and arranged, to find out what Bacon calls its "form." This is nearly synonymous with what we should call the *cause* of the phenomenon: something which is present where the particular quality exists; and conversely, wherever the quality is present, the form must be likewise. Thus, if transparency be the quality, there is some particular constitution of matter (which is the object of enquiry) which is the form or cause of that quality.

In endeavouring to obtain the knowledge of "forms," there are two subordinate points of enquiry of general importance, which, in the language of the author, are the "latens processus," and the "latens schematismus." The former is the secret and invisible process by which sensible changes are brought about, and seems to involve the same principle as that since designated *the law of continuity;* according to which no change, however small, can take place except in *time*. To know the relation between the time, and the change effected in it, would be to have a perfect knowledge of the latent process. In the firing of a cannon, for example, the succession of events, in the short interval between the application of the match and the expulsion of the ball, constitutes a latent process of a very complex kind. The latent schematism is that invisible structure of bodies on which so many of their properties depend; as the structure of crystals, &c.; or that arrangement of particles by which the peculiar constitution of matter in regard to elasticity, magnetism, &c. is determined.

In pursuing the enquiry after the "forms" of phenomena, the first step is to see what forms must, from the nature of the case, be excluded. This limits the field of hypothesis, and brings the enquiry within a narrower compass. Thus, if we were enquiring into that quality which is the cause or form of transparency, we must at once exclude rarity or porosity, because in the diamond we have an instance of a very dense body which is also transparent. There is also great importance in attending to negative instances, as glass when pounded is not transparent. After a great number of exclusions have left but a few principles common to every case, one of these may be assumed as the cause, and the validity of the assumption tried, by reasoning from it hypothetically, to see whether it will account for all the phenomena. "To man," the author observes, "it is only given to proceed at first by negatives, and in the last place to end in an affirmative after the exclusion of every thing else." He gives an admirable exemplification of his method by taking the subject of heat, and going through the process recommended, as far as the then state of knowledge would permit.

In the process of inductive enquiry thus pursued, it could not but occur that some facts would be found of much more importance than others to the discovery of the truth. Some of them show the thing sought in its highest, others in its lowest degree: some exhibit it simple and uncombined; in others it appears confused with a variety of circumstances. Some facts are easily interpreted; others very obscure, and are understood only in consequence of the light thrown upon them by the former. These differences led Bacon to distinguish what he calls the "prerogativa instantiarum," or the comparative value of facts, as means of discovery of causes. He enumerates not less than twenty-seven such points of distinction, entering at length into the peculiarities of each. We will exemplify their nature by noticing a few of the most remarkable.

The "instantiæ solitariæ" are examples either of the same quality existing in two bodies which have nothing else in common, or of a quality in which two bodies differ, when in all others they are alike. In either case the hypotheses as to the form or cause are limited: in the first case they can involve none of those things in which the bodies differ; in the second, none of those in which they agree.

Bacon gives an exemplification of the first of these cases, which is somewhat remarkable. Of the cause, or form of *colour*, he says, *instantiæ solitariæ* occur in crystals, prisms of glass, and drops of dew, which occasionally exhibit colour, and yet have nothing in common with the stones, flowers, and metals, which possess colour permanently, except the colour itself. Hence he concludes that colour is nothing else than a modification of the rays of light, produced, in the first case, by the different degrees of incidence; and in the second, by the texture or constitution of the surfaces of bodies: a remarkable anticipation of what Newton was soon to establish experimentally.

The *instantiæ radii* are cases measured by lines and angles; the *instantiæ curriculi* by time.

Under the former head Bacon makes some remarks, which are singular for the extent of view they discover, even in the infancy of physical science. He mentions the forces by which bodies act on one another at a distance, and throws out some hints on the attraction which the heavenly bodies exert on one another. "It is to be enquired," says he, "whether there be any magnetic force which acts mutually between the globe and heavy bodies, or between the moon and the sea, or between the starry heaven and the planets, by which they are called and raised to their apogee. These are all cases of action at a distance." *

Under the second head, after remarking that every change and motion requires time, he introduces the following remarkable anticipation of subsequent discoveries: —

"The consideration of those things produced in me a doubt altogether astonishing; viz. whether the face of the serene and starry heavens be seen at the instant it really exists, or not till some time later; and whether there be not, with respect to the heavenly bodies, a true time and an apparent time, no less than a true place and an apparent place, as astronomers say, on account of parallax. For it seems incredible that the *species*, or rays of the celestial bodies, can pass through the immense interval between them and us in an instant, or that they do not even require some considerable portion of time." † The actual measurement of the velocity of light since effected, and the train of curious consequences deducible from it, are the best commentaries on this passage, and the highest eulogy on its author.

"Instantiæ ostensivæ," which he also calls "elucescentiæ" and "predominantes," are cases in which some particular quality is shown in its highest state of power and energy. In these cases such quality is freed from the impediments which ordinarily impede or counteract it, or predominates over others by which it is commonly confined or disguised. Bacon instances the thermometer, or vitrum calendare, as it was termed (then newly discovered), as exhibiting the expansive power of heat in a magnified degree. We might exemplify it, perhaps, more perfectly in the Torricellian experiment, by which the actual pressure of the atmosphere is rendered manifest, though commonly disguised by its pressure in all directions.

"Instantiæ clandestinæ," also called "instantiæ

* Novum Organon, ii. aph. 45. † Id., ii. aph. 46.

crepusculi," are the reverse of the last. They exhibit some power in the faintest stage of its existence, as tracing capillary attraction to the extreme limit when the vessel ceases to be capillary.

"Instantiæ manipulares," or those which we call collective instances, or general facts, are the most important, perhaps, of any; such collective instances often constituting the utmost extent to which our generalisations can be carried. Of this we have an instance in one of the most important steps ever taken in any part of human knowledge, the laws of Kepler. From a comparison of a number of observations, the form and magnitude of the orbit of a planet is collected; in like manner, its periodic time in that orbit. This for each planet is a collective fact. Comparing the same results for all the planets, we have a more comprehensive collective fact; and the law of Kepler, which connects their periodic times and mean distances, is thus a collective fact of a still higher order.

Analogous, or parallel instances, are particularly noticed by Bacon as often of the greatest use in guiding us to the investigation of truth. Again, *monodic*, or singular facts, are important to be noted as differing, in some remarkable particular, from the class to which they belong; as the sun among stars, Saturn among the planets, meteoric stones, &c. *Instantiæ comitatûs* are cases where one property is invariably accompanied by another, as flame and heat, heat and expansion, solidity and weight.

But, perhaps, the most essential, as ministering to the support of all the others, are what Bacon calls the *instantiæ crucis*. When two or more causes suggest themselves, each of which may, as far as yet appears, account equally well for the phenomenon, some new circumstance is found in the case, which can be explained by the one and not by the other cause: this determines the question at once, and performs the office of a *guide post* at the separation of two roads; whence the name. This, indeed, is, perhaps, the most familiarly known of all his enumeration of philosophic rules; and we recognise its use in almost all the great discoveries of science. We shall not attempt to enumerate more of these classes, to which the comprehensive genius of Bacon sought to refer every case which might occur in the philosophic analysis of phenomena. What we have mentioned will amply suffice to convey an idea of the general nature of such classification; and, without going through the whole, it is impossible to appreciate it as a system.*

The "Novum Organon" is a work which must be thoroughly studied by any one who would competently judge of its real merits. In the extremely imperfect sketch to which our limits necessarily confine us, we have, however, we trust, said enough to direct the attention of the enquiring reader to the fountain-head. We will add a few general remarks.

A question arises, not unnaturally, in looking over the long list and classification of arguments,—can all this complicated system be necessary to a right apprehension of the principles by which experimental research is to be guided? Is not the inductive process really and practically a very simple thing, easily understood without all this metaphysical apparatus?

To this we would reply, that, practically speaking, there can be little doubt that in the single, restricted pursuit of any one limited object of experiment, many a most useful labourer in the cause of science has successfully worked out his result, and many more may do so, guided only by their intuitive common sense, and without any reference whatever to rules or principles like those we have enumerated; and so, in like manner, numberless enquirers on other subjects reason accurately and even profoundly, who, perhaps, are totally unacquainted with the systems of logic and metaphysics. Many speak and write correctly, powerfully, and elo-

* On another ground, too, it is the less necessary for us to enter largely into the systematic applications of the inductive method. It has been already done in the most complete manner, in sir J. Herschel's Introductory Essay on the Study of Nat. Phil. (Cab. Cyc.)

quently, who have never studied systems of rhetoric; but all this does not in the least disparage the abstract philosophic excellence of such systems respectively. These systems present a view of the theory of those principles into which the real results may be analysed, and from which they might synthetically be traced. The value of such systems is of a higher and more philosophical kind; and though it may be true that many have succeeded in arriving at their conclusions without such aid, yet it by no means follows that all can dispense with it, nor that even these persons might not have succeeded more completely and more easily if they had been in possession of some settled principles on which to proceed.

Again, it must be borne in mind that all the systematic enumeration of Bacon was made purely from theory and by anticipation. He was bound to consider and examine every possible avenue to philosophic truth, to scrutinise every conceivable channel through which investigation might be conducted; and it might well happen that some of these should afterwards be found practically of far more importance, and far more frequent use than others: that, in point of fact, inductive science should mainly proceed by means of some few of the principles here developed, whilst the occurrence of others should hardly be recognised.

We have a remarkable instance of this in the "instantiæ radii." Bacon appears to attach no more importance to this class of facts than to any others; but yet in modern research it has come to pass that nearly every subject of enquiry, which can claim to belong to the exact sciences, has been brought under the dominion of *measurement*, and subjected to the test of *numerical* agreement with mathematical laws. Something to the same purpose we have already remarked of the "instantiæ crucis." This is, in fact, in real practice, the sort of experiment most frequently appealed to, and that from which the most satisfactory conviction is derived. Again, it not unfrequently happens, and more especially when any branch of science has been long cultivated, and research is limited to a narrow track, that, in fact, a large portion of the work is already done to our hands, and the trouble of referring to a number of different considerations to guide our experimental labours, superfluous.

These, and other considerations of a similar kind, may tend to show the real value and efficacy of such a system as that which Bacon has developed; and to convince any one, that, although the study of it may not be of absolute necessity to every prosecutor of experiments, yet it will in all cases be accompanied with inestimable advantages: and we cannot doubt, if it were more generally and attentively dwelt upon, we should see the records of science much less frequently blotted with unphilosophical arguments, and should much less frequently witness the labours of ingenious experimenters thrown away in the pursuit of unskilfully devised theories.

It is undoubtedly true, that we find the principles of inductive philosophy not only acted upon, but distinctly professed, by writers prior to Bacon. We have already seen this exemplified in the productions of Kepler and Galileo: and Tycho Brahe, in a letter to Kepler, gives him this advice: — "first to lay a solid foundation for his views by actual observation: and then, ascending from these, to strive to reach the causes of things." Gilbert, in his treatise "De Magnete," has laid down very explicitly the inductive principles by which he was guided in his experiments. We have also in some passages of the writings of Leonardo da Vinci equally strong expressions to the same purport.

"In treating any particular subject," he observes, "I would first of all make some experiments, because my design is first to refer to experiment, and then to demonstrate why bodies are constrained to act in such a manner. This is the method we ought to follow in investigating the phenomena of nature. It is very true that nature begins by reasoning, and ends with experiment; but it matters not; we must take the opposite course; as I have said, we must begin by experiment, and endeavour by its means to discover general principles."

Again,—"Theory is the general: experiments are the soldiers. The interpreter of the works of nature is experiment: that is never wrong. It is our judgment which is sometimes deceived, because we are expecting results which experiment refuses to give. We must consult experiment, and vary the circumstances, till we have deduced general rules, for it alone can furnish us with them. But you will ask, What is the use of these general rules? I answer, that they direct us in our enquiries into nature and the operations of art. They keep us from deceiving ourselves and others by promising ourselves results which we can never obtain." *

We have been led to refer to these views more particularly, because some modern writers of considerable eminence have been rather disposed to undervalue the character of Bacon's writings, and to deny their extensive or beneficial influence on the researches of subsequent philosophers. It has been alleged that Galileo, Copernicus, and even Kepler, had exhibited perfect examples of the inductive method of philosophising, and that Leonardo da Vinci, in the passages just quoted, as well as Galileo and some other authors of that period, have, in fact, in such expressions delivered a brief but complete summary of all the essentials of the inductive method. Even the alchemists of that day, absurd and visionary as were the objects of their pursuit, yet, it is urged, in the indefatigable toils of their laboratories, at least showed that they considered the whole value of their science to rest upon experiments. Kepler, in the wildest of his reveries, submitted them most scrupulously to the test of accordance with observation, and unhesitatingly sacrificed the labours of years if the results did did not stand the test. Thus, it is said, those philosophers had all thrown off the yoke of the schools, and

* Venturi, Essai sur les Ouvrages de L. da Vinci, p. 32.

not only fully understood the value of the inductive method, but resolutely acted in accordance with it. Thus, Bacon cannot be said to have invented or constructed the method to which modern science owes its existence. And later philosophers have not disclosed in their methods any closer adherence to Bacon's rules than his predecessors did. Newton, Boyle, Huygens, and their followers, have never alluded to the guidance of the "prerogativæ instantiarum," nor mentioned to which class their arguments were to be referred.

The remarks we before made will almost supply an answer to these animadversions. We may, however, more completely reply by putting a parallel case. We possessed many eloquent writers in the English language before an English Grammar was composed; and since that, we boast many more who have neither written better English, nor, in every sentence, quoted the grammatical rule by virtue of which that sentence was constructed. Will it, therefore, be argued that grammar is useless, or the man who first formed one entitled to no praise?

The "Novum Organon" is the *grammar* of inductive philosophy. Its principal merit lies, not in supplying practical rules, without a technical knowledge of which no man could conduct a philosophical enquiry; nor in teaching men the value or utility of the unfettered appeal to experiment: but its main excellence consists in bringing into the form of a philosophical system those principles which, though already practically recognised, had not yet been viewed in their mutual connection and dependence; and in reducing to a scientific arrangement those scattered truths which were already approved by the practice of the most cautious and judicious enquirers into nature.

If, then, a philosopher of a subsequent age, conducting his discoveries on inductive principles, should make no especial mention of Bacon, and should state his experiments and describe his results, without explicitly telling us whether they belong to the class of "instantiæ curriculi," or "experimenta crucis;" should refute errors, without formally classifying them as "idola specus," or "idola fori;" would this be any argument either that those systematic distinctions were unknown to him, or that the philosophical arrangement of them, according to their characteristic differences, was an idle, useless, or chimerical speculation?

We conceive, upon the whole, we hardly need urge more to vindicate the exalted rank to which Bacon is entitled among the philosophres, not only of his own, but of any age. He was, doubtless, deficient in mathematical knowledge, and he did not, himself, push his principles to the discovery of any actual physical laws. He did not, perhaps, shine with so much lustre among his contemporaries, as he does to posterity. We walk familiarly on the surface of the earth without perceiving the light it reflects, but to the distant lunarians it is a brilliant luminary. The immediate value of the discoveries of Galileo was more striking. Bacon wrote for ages to come. And it may be safely affirmed, that, though we could name several philosophers who, placed in the same circumstances as Galileo, might have made the same discoveries, yet we cannot say this of Bacon; we might find substitutes for the one, but not for the other. Galileo was the immediate minister of science, whose services enlightened his own times: Bacon was its prophet, whose credit was not established till his predictions were verified. Galileo entered and took possession of the vast regions which science was henceforth to call her own. Bacon, from his lofty elevation, took a complete survey of the rich territory of the promised land; but expired, like Moses on Mount Nebo, without himself entering it. In a word, we must entirely agree with D'Alembert, that, "when one considers the sound and enlarged views of this great man, the multitude of the objects to which his mind was turned, and the boldness of his style, which unites the most sublime images with the most rigorous precision, one is disposed to regard him as the greatest, the most universal, and the most eloquent of philosophers." *

Algebraic Geometry. — Des Cartes.

The succession of discoveries to which we have referred, during the latter part of the sixteenth and beginning of the seventeenth centuries, brought algebraic analysis to a state of very considerable perfection. The way was thus prepared for the brilliant discoveries in reference to the application of this analysis to geometry, which constitute the peculiar characteristic of the "modern geometry," and confer upon it those extended powers and capacities to which the ancient system could not attain. Such applications had been made in a few instances (as we have noticed) by Vieta and others. But the grand advance, to which we now refer, was that effected by Des Cartes, and which forms one of the most important epochs in the history of mathematical science.

This philosopher, celebrated in so many departments of science, was born at La Haye, in Touraine, in 1596, and at an early age displayed great proficiency in various branches both of literature and science. After going through the usual course in the Jesuits' college at La Flêche, he entered upon the military profession, and, soon after, visited various parts of Europe. The versatility of his genius showed itself in the variety of different studies he pursued, embracing almost every department of science, metaphysical as well as mathematical. He died in 1650. It was in Holland, in 1637, that he first published his Geometry, in which the great inventions above referred to are contained. This work is a tract of no more than 106 quarto pages: but there is, probably, no production of the same size which ever conferred so much and so just celebrity on its author.

In the first book, he treats of such geometrical problems as can be resolved by the help of circles and

* Disc. Préliminaire de l'Encyclop.

straight lines, showing the application of algebra to such questions. He then proceeds to the more immediate subject of his researches, in which the powers of the modern analysis are so conspicuously displayed. We will endeavour briefly to explain it.

We have, in a former section, mentioned the problem called "locus ad quatuor rectos," proposed and partially discussed by Apollonius and Pappus. To this, Des Cartes, in the first instance, turned his attention, and soon effected a complete algebraic solution of it. The nature of the problem may be thus stated in a general way:— Four lines are given in position; and it is required to determine a point such, that if from it perpendiculars be drawn to the four lines, a certain complex combination of the rectangles or products of the perpendiculars shall be of a constant magnitude. The given lines may, in fact, be more than four, and the other lines may be inclined at any given angles. It is, of course, impossible to give a more exact idea of the case without entering into algebraic details; it must suffice to say that Des Cartes readily showed that the problem is indeterminate; that an infinite number of points fulfil the condition; and these, being laid down and connected together, form a *locus*, which he found to be always one of the curves of the conic sections when the prescribed combinations of the quantities are products of only two factors, or, in the language of algebra, rise only to *two dimensions*. When these are of higher dimensions, corresponding curves of different kinds result: when of only *one* dimension, the locus is a straight line. The problem, as originally proposed, is involved in much greater complexity than is at all necessary for the purpose of this theory. And one of the main improvements consisted in taking, as the fundamental lines to which the whole construction is referred, only *two* lines, inclined at any given angle. These are called the *axes;* and upon these, or upon lines respectively parallel to them, are measured the corresponding values of the variables, in the indeterminate equation, when solved for one in terms of the other; each two of these values determine a point in the curve; and, by consequence, we successively trace out the whole locus, as successive corresponding values are taken.

Thus, in general, any indeterminate equation with constant co-efficients would, when solved for one of the variables, always give a peculiar locus, under certain invariable conditions both as to extent and position, determined by the constant quantities. An excellent illustration of the idea is afforded by the familiar consideration of a map, in which the latitudes and longitudes, always measured from a given origin, being assigned, for successive points, along a coast, or a river; for example, we can lay down on the map the form and outline of that shore, or the course of that river. And if the latitude and longitude of each point had some fixed and invariable relation which connected them together, and, however the actual values might alter, yet that relation should remain the same for all points in the course of the line traced out, the announcement of such relation would constitute the *equation* of the locus. We have before observed, that the business of the geometer is first by his definition to frame some construction of a geometrical figure, and then by demonstrative deduction from the conditions of such construction to establish its various properties. The regular curved lines of which geometry takes cognisance, are such as result from such particular mode of construction, instances of which we have already noticed. From these constructions their properties are deduced. The ancient geometers investigated many such curves; but they were all defined upon perfectly independent constructions, and had no principle of any common relation. Thus, the discovery of each particular curve, and the investigation of its properties, cost the geometer a distinct effort of invention, and demanded a separate expenditure of intellectual energy; and even when successful, he was as often indebted to chance as to his own sagacity.

Now, by the method of Des Cartes, the subject was generalised: — " Perceiving the importance and power of the principle which he used in the solution" (of the problem of the locus), " he immediately conceived the notion of founding upon it the whole geometry of curve lines. By this felicitous application of equations of two unknown quantities, the science of geometry was utterly revolutionised. Every curve described by a given law, being expressed by an equation between two variables deducible from that law, was thus brought under the dominion of algebra. This equation including the essence of the curve, its various properties flowed from it. . . . . . The immediate consequence of this memorable discovery was, that geometry at once oversprang the narrow limits which had circumscribed it for ages, and took a range, the extent of which is literally infinite. Instead of a few simple and particular curves, which had hitherto constituted the only objects of the science, the geometer discussed the properties of whole classes of curves, distinguished and arranged according to the degrees of the equations which represent them. The variety of curves thus became as infinite as that of equations."*

The construction of a curve corresponding to any given equation is thus easy to be conceived. Its general form would, of course, thus be traced out. But it does not immediately appear how its various properties are deducible from its equation. The investigations of Des Cartes, however, extended in some measure to this part of the subject also. The properties of curves may be said to depend mainly upon the positions assumed by those straight lines which are the *tangents* to them at any points. The determination of these positions, or the solution of the problem, " to draw a tangent to a curve," forms the basis of all such speculations. In particular individual cases, this had been done by the ancient geometers.

But the ancient geometers had felt certain difficulties

* Lardner's Alg. Geom. Pref. xxii.

attending the problem. Even the very simplest case of it,—to draw a tangent to a circle,—discussed by Euclid, required that excellent geometer to use his utmost caution and skill: in fact, in the transition from a curve to a straight line, which has a single point in common with it, and yet lies wholly without it, it seems almost unavoidable, that we must have recourse to a method in some measure indirect. There appears to be a combination of two ideas, which have not, strictly speaking, any common point of comparison. And, perhaps, the principle of a *limit* is, in reality, at the basis of all investigations of the subject. It certainly is so, if we proceed to consider it in any extended point of view. In this way Des Cartes did proceed to view it; and based his general method of tangents on the principle, that if we conceive a *secant*, or straight line, cutting a curve in tw o points, to move parallel to itself, it will come into positions where the two points of section successively approach nearer together, and at length merge in one: this position is the *limit;* and the *secant* then becomes a *tangent.* In following out this idea, so as to found on it an analytical method of assigning the position of the straight line, so that it shall have this one point in common with the curve, or that one value of the ordinate shall be the same in the equations both of the curve and the straight line, considerable complexity was involved; and though Des Cartes certainly completed his system in this particular, yet his method of tangents is tedious and laborious, compared with others since discovered. The exposition of this method, and the discussion of certain particular curves, called the ovals of Des Cartes, remarkable for their optical applications, occupy his second book. The third treats of the construction of equations by means of curve lines, and contains a method of resolving certain biquadratic equations.

The main excellence, then, of these methods is their absolute generality. "The equation to a curve is, as it were, a short formula, in which all its properties are embodied, and from which the analyst is always able to deduce them by fixed and general rules, which are not peculiar to the equation of any particular curve, but indifferently applicable to those of all curves." The investigations of the ancients were solely applied to isolated individual cases. "Their method of drawing a tangent to one curve furnished no clue which could lead to the solution of the same problem with another curve; and, therefore, the geometer was beset with the same difficulties at every new curve which he approached. The application of algebra at once removed these defects. It determined uniform and general rules for the investigation of the properties of any curve whatever. Nay, it did not alone assist the operation of the reasoning faculty, but actually supplied the place of invention, by furnishing means of discovering curves in infinite variety. No equation between two unknown quantities can be proposed, but a corresponding curve is immediately discoverable, whose nature and properties afford matter for geometrical speculation."*

The principle, however, was by no means confined to the case of a curve traced out upon one plane. The two co-ordinates, of course, give only its figure in one plane, or refer to the two dimensions of *length and breadth*. But the geometer contemplates, besides these, the third dimension of *depth*, giving rise to the idea of solid figures. Thus, by an easy extension of Des Cartes's original idea, equations involving *three* variables in like manner expressed a locus not lying in one plane, but each successive point in which had a different position from the last, both in length, breadth, and depth; or, in other words, a *curved surface*. Thus, both plane and solid geometry were brought under the dominion of the same analytical principles.

The same principle is, moreover, susceptible of other modifications. Equations may be used to express curves which do not lie in one plane, but have a twisted form; as, for instance, like that of a corkscrew. Here there

* Lardner's Alg. Geom. Pref. xxiii.

are three co-ordinates concerned, and they are connected by two equations, each involving some two of the variables.

We have hitherto supposed the equations employed to be what are distinctively called *algebraic;* that is, involving only simple, finite, algebraical quantities as the variables. But other classes of equations, which do not fall under these limitations, may yet have a similar geometrical application. For instance, if the variables depend upon one another in some relation assigned by trigonometry, or by logarithms, they may still have corresponding loci, provided those relations can be actually exhibited. Such are called transcendental curves. Simple cases of these are found in what are called the trigonometrical curves, which were invented by E. Wright, about 1600. The curve of sines is constructed by taking equal distances or *abscissæ* along one axis corresponding to *arcs,* whilst the *ordinates* are the *sines* of those arcs. Their summits trace out the locus, otherwise called the sinusoid. In like manner we have the curve of tangents, of cosines, &c. &c. Again, the *logarithmic curve* will, from its name, be readily understood as constructed upon an analogous principle. It was invented by James Gregory.

Most of these and other curves formed subjects of considerable discussion at the period of their invention, the interest of which has in a great degree gone by. Nevertheless, they were of essential value to the progress of discovery, as furnishing the tests by which those more general principles of analysis were tried, which were now beginning to be discovered.

Infinitesimals.

We have before noticed the advances made by Kepler in that part of the science of quantity which depends upon the extension of the ancient method of limits. Galileo, in a very curious passage in the "Dialogues on Motion," introduces a discussion in which similar principles are involved; and some of his expressions have been interpreted as a sort of anticipation of the idea of prime and ultimate ratios and fluxions; but he did not pursue the idea.

Cavalieri (born at Milan in 1598) was the friend and disciple of Galileo, but much more profound in mathematics. In his hands the subject took a more regular and systematic form; and he developed a method by which problems involving exhaustions might be solved with greater brevity and facility, in a work on "Indivisibles," published in 1635.

The principle on which he sought to avoid the embarrassments of the ancient methods, was by adopting the phraseology of areas made up of an infinite number of parallel lines; solids, of an infinite number of planes; and even lines themselves, of an infinite number of points. Thus, when these series of planes or lines respectively were arranged according to a determinate law, and such that their sums could be assigned, the contents of the solids and areas of the curves were then found. The *phraseology* was, of course, absolutely at variance with the firstdefinitions of geometry; but, as we observed before in a corresponding case, the question should rather be, Is there involved any misapprehension or contradiction in the *things signified?* It was, in fact, a way of speaking, undoubtedly incorrect, but introduced as a convenient mode of avoiding circumlocution and prolixity. The lines and planes are the *limits* of the parallelograms and parallelopipeds, which, when of finite magnitude, put together in a diminishing series, form figures whose outline ascends as it were in *steps.* The *limits* of such figures are those whose boundaries are continuous lines, straight or curved according to circumstances. Cavalieri conducted his reasonings in a compendious manner by the adoption of this incorrect mode of speaking: but it was in substance that of limits; and might have all along been reduced to, and verified by, the method of exhaustions. Indeed, there is no doubt that he himself took the exact and satisfactory view of the subject; for, in the beginning of his seventh book, he expressly says (in anticipation of objections which might be brought against the apparent looseness of his reasoning),—"There is no necessity to suppose the continuous quantities made up of these indivisible parts, but only that they will observe the same ratios as those parts do." Nevertheless, the method of indivisibles exposed him to many attacks: but the essential principle was ingeniously explained and defended by Pascal.

Cavalieri pursued his method into many valuable solutions and results, especially as regards the areas of spherical triangles, and deduced some important theorems; though so great was the impulse now beginning to be given to the mathematical sciences, that many geometers in other countries were pursuing the same track. These, in several instances, laid claim to the same inventions; which, probably, they had really arrived at independently and simultaneously.

Galileo had, before this time, invented the cycloid, or curve traced out by a point in the circumference of a wheel as it rolls along a straight track. In 1639, Galileo mentions to his friend Toricelli that he had thought of it forty years before that time. He made various efforts to determine its area, but wanted that geometrical skill which was necessary for the discovery. It also baffled the attempts of Cavalieri; but shortly afterwards, solutions were given both by Toricelli, and by Roberval, a French mathematician of great originality and invention; each claiming it as his own discovery. The area is three times that of the generating circle, or rolling wheel. A controversy arose on the question of priority; but it is most probable that each arrived at the result independently.

Roberval improved upon the method of quadrature proposed by Cavalieri; a research in which he was followed by Dr. Wallis. Fermat applied analogous methods for finding the maxima and minima of the ordinates of a curve; as also their tangents: and investigations of the same kind were pursued, with his usual ability, by Dr. Barrow.

Roberval devised a method of drawing tangents to curves, grounded on geometrical principles, but applicable to those cases only in which the curve is constructed by the intersection of two lines which go on increasing in a given ratio to each other. The essential principle is, that, in such cases, the increments of the two lines being completed into a parallelogram, its diagonal will be the tangent. But this can only be established by an application of the principle of limits.

Barrow originated the idea of what has been called *the incremental triangle*, and is, perhaps, the best adapted to the purposes of general illustration. The notion that a portion of a curve may be taken, so small that it may without error be considered as a straight line, is one which the mind readily admits: it is, nevertheless, utterly untrue, and contradictory to the first principles of geometry. It is one of those instances of which we have already noticed some, where language absolutely contradictory in terms may yet be conventionally employed to avoid circumlocution. The idea really at the basis of such expressions is that a straight line is the *limit* to which a portion of a curve continually diminished approaches. But, adopting the incorrect but more convenient phraseology, the small increment of the curve, and the corresponding increments of the abscissa and ordinate, form a small triangle. If, from the relation of the two latter, we express that of their *infinitely small* increments, we have, upon the principles of plane trigonometry, the position of the hypothenuse, or the direction of the tangent to the curve. Nearly the same view of the subject was taken by Fermat.

John Wallis was one of the most eminent mathematicians of the period of which we are treating, and made important advances towards the solution of those great problems to which the attention of geometers was then so powerfully drawn. But a limit seemed yet to be placed to the success of their efforts, which it was resrveed for the genius of Newton to pass.

From the university of Cambridge, Wallis was transplanted to Oxford, by an appointment to the Savilian professorship of geometry, in 1649. In one of his letters referring to that period, we are presented with an interesting illustration of the state of mathematical knowledge in the former university which has since so amply redeemed its credit in this respect.—"Mathematics were not at that time (with us) looked upon as academical learning . . . . . . and of more than two hundred students at that time in our college, I do not know of any two who had more mathematics than myself, which was but very little, having never made it my serious study (otherwise than as a pleasant diversion) till some little time before I was designed for a professor of it."

Thus unfurnished at the outset, the powers of Wallis's genius will appear the more conspicuous when we consider the rapid progress he made in mathematical pursuits, whilst at the same time he was scarcely less distinguished for his attainments in various branches of literature, philosophy, and theology. Besides a number of controversial works, the "Commercium Epistolicum" contains the best evidence of the zeal and ability with which he entered upon the important and then difficult enquiries relative to the rectification and quadrature of curves, and other topics connected with these. His edition of "Archimedes de Arenario," &c. bespeaks his critical acuteness; while his "Essay on the Tides," and his "Mechanica," exhibit those dynamical enquiries in the highest state of developement which they had as yet attained. The "Mathesis Universalis" comprises his elaborate treatises on algebra and the arithmetic of infinites; and the early volumes of the Philosophical Transactions are enriched by many communications from him. He died in 1703.

It is in his "Arithmetic of Infinites" that Wallis has displayed to the greatest advantage his powers of original invention. He here avails himself of the principle, already admitted to a partial extent by Kepler and others, of introducing the phraseology of infinite quantity, and estimating the values which algebraical expressions assume in different cases, when we suppose a term involved in them to become infinitely great or infinitely small. In his investigations we may trace the germs of those methods which, very soon after, in the hands of Newton, received an expansion fitting them for the purposes of analysing the most complex laws of physical phenomena. But the principles which Wallis had adopted, were not yet so far developed as to be understood in the full extent, or to exhibit the power which they really possessed. He was, however, enabled to pursue the subject of quadrature upon much more general grounds than any of his predecessors. Hitherto, as we have seen, geometers had succeeded in assigning in finite terms the value of the area of the spaces bounded by curve lines, only in a few limited and very simple cases. Des Cartes had generalised the mode of conceiving the construction of all curves; and it hence followed, that by certain algebraical processes, those applications of the method of limits which had been devised by Kepler and Cavalieri for expressing the areas, might be extended in a manner equally general, by virtue of the relation subsisting between the abscissa and ordinate,—the two variables, which enter Des Cartes equation.

Wallis found, that in all cases where the value of one of these could be expressed in terms of the other, without involving fractional or negative indices, he could exhibit the value of the area, or quadrature of the curve, in finite terms.

A partial extension was given to this method by Nicholas Kauffman, (more commonly known by his Latinised name, Mercator,) who devised a method of reducing some of the expressions into a continued series of terms. In this way he obtained the quadrature of the hyperbola, in 1667.

Wallis, however, was anxious to extend his solutions beyond this limited portion of the subject. He saw a wide field open before him, and in this new region of research he perceived that many geometrical truths of great value and utility were to be discovered; the quadrature of the circle, and of many other curves, together with an incalculable variety of applications of such results bearing upon a number of points of physical enquiry, were all inviting research, promising a rich harvest of further discoveries, and stimulating the attempts of the enquirer with the prospect of an immortality of fame.

It occurred to him, that if the equations of the curves which he had squared were ranged in a regular series, from the simpler to the more complex, their areas would constitute another corresponding series, the terms of which were all known. He further remarked, that in the first of these series, the equation to the circle might be introduced, and would occupy the middle place between the first and second terms of the series, or between the equation of a straight line, and that of the parabola. He concluded, therefore, that if in the second series he could interpolate a term in the middle, between its first and second terms, this term must necessarily be no other than the area of the circle. But when he proceeded to pursue this very refined and philosophical idea, he was not so fortunate; and his attempt towards the requisite interpolation, though it did not entirely fail, and made known a curious property of the area of the circle, did not lead to an indefinite quadrature of that curve.

In these researches Wallis was closely associated with sir C. Wren, who in his early youth had evinced high mathemetical talents; he gave a rectification of the cycloid and several other mathemetical investigations, which Wallis published in his treatise on the cycloid. He devoted himself much to astronomy, and became professor of that science at Oxford in 1670, as well as in Gresham college; he also entered largely into the dynamical questions then discussed by the English philosophers and Huyghens. But ultimately his magnificent architectural labours withdrew him entirely from the pursuits of abstract science.

The Cartesian System.

Des Cartes, having brought geometry under the dominion of a comprehensive principle, seems to have been misled into the splendid but visionary notion, that the system of the world and the philosophy of mechanics might, in like manner, be established upon a theory arising out of a few first assumed axioms. These, according to his view, were to be found in certain metaphysical ideas of the Deity and his attributes; from these he affected to reason downwards, and to deduce the laws of nature: to show why things are constituted as they are, and to explain the causes of material phenomena. In this way he pretended that, by a long train of consequences, he could always determine, at last, what ought to be the laws and modifications to which material agents would be subjected; and reason from the first cause to secondary causes, and from secondary causes to their visible effects. At the same time (though, it would seem, with some inconsistency) he did not wholly reject experiment and induction; and he seems to be tacitly admitting the fundamental deficiency of his whole system when he says, that the number of different shapes, which effects might assume, is so great, that he could not determine without experiment which of them nature had preferred to the rest. "We employ experiment, not as a reason by which any thing is proved; for we wish to deduce effects from their causes, and not, conversely, causes from their effects. We appeal to experience only, that out of innumerable effects, which may be produced from the same cause, we may direct our attention to one rather than another."

In the use, however, which he did make of induction, Des Cartes appears to have acknowledged the truth of Bacon's principles. He was certainly but little disposed to recognise the claims of any preceding philosophers;

and it has been said by some writers that he did not treat Bacon with more respect than the rest. But it appears from his correspondence with Mersenne (published in 1642), that in several letters he distinctly refers to the works of "Verulam" with a respect which he yielded to no other author, and in a way which shows that he had both studied them and approved the method they deliver. This sufficiently accounts for some remarkable coincidences observed by Mr. D. Stewart in the writings of Des Cartes with the ideas, and even the very words, of Bacon; although he was led to assert that, if Des Cartes ever read the works of Bacon, he has nowhere alluded to them; and in this opinion Professor Playfair also coincides. Indeed, before this time, Bacon's writings seem to have been well known on the Continent, and justly esteemed. A letter is extant from him to Baranzon, who lectured on natural philosophy at Annecy, in Savoy, in 1621 (and, it appears, had consulted him on the introduction of the inductive method), containing a very perspicuous summary of his views, and showing the authority and influence his writings had obtained at that period: his suggestions to Baranzon are almost identical with some of those referred to by Des Cartes in the letters above mentioned.*

It must be confessed, however, that we find very little of the influence of Bacon's principles in the system of Des Cartes, however he might own their general truth, and follow their guidance, when he did condescend to any experimental enquiry: this, as we have seen, he only resorted to as a subordinate means of assisting his theories; and it was very seldom that he thought it necessary to have recourse to it.

The preliminary positions on which his system rested, led him extensively into mechanical speculations. He laid down as an original view the estimate of forces by the momenta, which is no other than that before proposed by Galileo. He also introduced into the

* See Rev. W. V. Harcourt's Address, British Assoc. Reports, note, p. 24.

theory of motion the inertia of matter, regarded as a real active power, and not merely a passive indifference to motion or rest. With respect to curvilinear motion, he pointed out distinctly the necessity of supposing a *deflecting* force, which being removed, the motion would be rectilinear and in the direction of the tangent. He laid it down as a general principle, that there is always the same quantity of motion in the universe, which, it would seem, is the clue to his notions of inertia, &c. just mentioned. He appears to have regarded motion as a sort of quality superinduced upon matter, but which might, in some cases, be, as it were, *latent*. All this arose out of the theoretical principle of the permanence of such qualities; and this he deduced from the immutability of the divine attributes. The inherent fallacy of such reasoning must be sufficiently apparent; and we shall not be surprised to find that though, in several cases, he has brought out true results, yet, in others, as in the whole theory of the collision of bodies, he has run into palpable errors.

This last subject, indeed, is one which long remained without a completely satisfactory investigation. Such investigations were first given by Dr. Wallis and Sir C. Wren, nearly at the same time, in 1668; and, also independently of these, by Huyghens in 1669. They each founded their reasonings on the principle, then first fully developed, that action and reaction are equal and in opposite directions.

In 1633, Des Cartes had completed his "System of the World," having previously broached the metaphysical doctrines on which he founded his entire method of philosophical reasoning.

With regard to this system, he must be allowed the credit of having been the first who attempted to suggest any one physical principle to explain and connect all the planetary motions. For we can hardly class under the designation of philosophical theories the crystalline spheres of Ptolemy, or the vitality assigned to the earth by Kepler. Des Cartes proceeded upon principles which, though involving gratuitous assumptions, were at least of a more philosophical character than these. He attempted to reason *à priori* from no other assumptions than the ideas of matter and of motion, and the attributes of matter, extension, impenetrability, and inertia.

Matter, thus constituted, he conceived to fill all space. All its parts were endued with motion in an infinite variety of directions: from the combinations of these motions it was impossible that rectilinear motion could result; continual deflection from rectilinear directions must take place: hence circular motion and centrifugal force originated; and thus, at length, matter came to be formed into a multitude of vortices, or whirlpools, the more subtile parts constituting the real vortex in which the denser bodies float. Thus the universe consists of a multitude of vortices, which limit and circumscribe each other. The earth and planets are bodies carried round in the great vortex of the solar system; the subtile matter of which acts upon them with a pressure towards the centre, while the centrifugal force preserves them from falling into it. Each planet is, in like manner, the centre of a lesser vortex, which produces a similar tendency to its centre in bodies within the sphere of its influence.

Such is the outline of this "baseless fabric of a vision;" which, coming as it did *after* the discovery of the laws of Kepler, is at once condemned in exhibiting no conformity to them, nor supplying the smallest explanation of any thing but *circular* orbits, which had been now shown not to exist.

Yet this hypothesis, framed on the most extravagant reasoning from purely imaginary assumptions, and, after all, not affording any explanation of the facts, was soon actually adopted, and seriously defended, in the universities of Europe. It was not, however, received without some opposition at first, and was even prohibited in one or two continental universities, especially where the Jesuits had any influence; but the metaphysical tone of the author's speculations probably tended to its favourable reception in other schools, as it at least furnished interminable subjects for scholastic disputation.

In the English universities it obtained almost undisputed sway in proportion as the Aristotelian physics were rejected, and it was necessary to have *some determinate system* to substitute in their place. The "Physics" of Rohault, conceived entirely on the Cartesian principles, continued, till a much later period, the favourite and established text-book in Cambridge. That and other systems of the same kind were also read in Oxford and the Scottish universities. We shall not be surprised at the popularity which the Cartesian system acquired throughout Europe, if we remark that it appealed strongly to the imagination, and very little to the reason, of mankind. In explaining all the movements of the heavenly bodies by a system of vortices in a fluid medium diffused through the universe, Des Cartes had seized upon an analogy of the most alluring kind. Those who had seen heavy bodies revolving in the eddies of a whirlpool, or in the gyrations of a vessel of water to which a rotatory motion had been given, had no difficulty in conceiving how the planets might revolve round the sun by analogous movements. The mind instantly grasped at an explanation of so palpable a character, and which required for its developement neither the exercise of patient thought nor the aid of mathematical skill. Above all, the immediate chain of connection by which the author affected to deduce it from the attributes of the Deity stamped it with that religious sanction which, once obtained, gives currency to any absurdities, however glaring; and even stigmatises with impiety the most rational and demonstrable truths opposed to the notions it has once countenanced.

Thus much, however, we may safely say in praise of the Cartesian theory, that its popularity certainly helped materially to explode the more gross errors of the Ptolemaic system; and, from seeing one system give way to another, men's minds were so far opened as to begin to lose their excessive devotion to authority, and to acknowledge that every system should be fairly open to examination.

Optics. — Law of Refraction.

We have before noticed the successive attempts made by Alhazen, Vitello, and Kepler, to investigate the law of refraction. The enquiry appears to have been first prosecuted with complete success by Willebrord Snell, a Dutch mathematician, who flourished about 1600.

His mode of conceiving the problem by a geometrical construction is certainly more complex than necessary; but this construction, when put into the more convenient and perspicuous language of trigonometry, unquestionably amounts to the simple announcement of the law which had been so long sought, and which connects together the deviation of the refracted ray towards the perpendicular, and the angle of incidence, for all values of that angle. The relation is that of a *constant ratio* between the *sines* of the angles which the incident and refracted rays form with the perpendicular. This remains invariable for all incidences, while the media are the same, but varies for different media. If we suppose the ray to pass out of vacuum into the given medium, the ratio for the particular medium in this case is called the refractive index of that medium.

Snell, however, certainly did not announce his discovery in this precise language. Des Cartes, in his Dioptrics, published in 1637, gives it in this form of enunciation, without making the smallest mention of Snell; and the discovery, in consequence, went forth as that of Des Cartes. It has been said that Snell's works containing this discovery were not published at the time Des Cartes wrote; other accounts, however, give as the date of their publication 1619, or even earlier. At any rate, Snell had communicated his researches to his friends, and they had been made public by his countryman, Professor Hortensius, in his lectures. It may also be true that Des Cartes made an independent discovery of the same truth. His character, however, is well known to have been marked with an envious desire to disparage the merits of those who might be his rivals; and this circumstance throws considerable doubt on his claim to originality.

Des Cartes, in this as well as other parts of his speculations, affected to reason from those abstract principles, which were, in fact, nothing more than arbitrary assumptions. He deduced the law of refraction, not from a comparison of observations, but from the hypothesis that light proceeds more rapidly in denser media.

The hypothetical character of this reasoning was exposed and censured by Fermat, who, nevertheless, himself attempted to deduce the law upon a principle which, in the existing state of the science, was scarcely less hypothetical, though it has been fully confirmed by later researches. This was termed "the principle of least action;" that is, he assumed that light must always move so as to pass from one given point to another in the least possible time, and that the course it takes under the influence of the different density of the media will be determined in accordance with this principle: and contrary to the theory of Des Cartes, assuming that light is *retarded* in proportion to the density of the medium, he deduced, on this principle, the same result, a refraction regulated by the law of the sines.

Des Cartes also directed his attention to the forms of lenses, and the means of collecting incident rays accurately to one point as the focus. No spherical lens does this accurately, even if limited to a very small arc of a sphere; and if at all of considerable curvature, the aberration is very great. Des Cartes, therefore, investigated generally the nature of the curve which should give this accurate convergence; and by a very complete and elegant analysis showed that a certain class of curves of the fourth degree will fulfil the conditions: these, in certain cases, become of the second degree. But the mechanical difficulties in working any glasses except those of a spherical form, are so great as to forbid all hopes of improving optical instruments from the introduction of other curves.

The explanation of the rainbow was advanced an additional step by the researches of Des Cartes. He explained the secondary bow; and accurately traced the paths of the rays, and the angles under which the effect is produced; but the extension of the same principles to the distinct colours was yet wanting. He is, as usual, entirely silent as to the claims of previous writers, and never mentions De Dominis.

The Disciples of Galileo. — Physical Science.

Galileo had opened the way to a vast field of research, and also succeeded in inspiring with the desire to explore it, a number of zealous and able disciples, who soon proceeded to the task with equal diligence and success: among these, none were more pre-eminently distinguished than Torricelli, who flourished about 1640.

Torricelli made some additions to the mechanical discoveries of Galileo, in his treatise "De Motu Gravium naturaliter Descendentium et Projectorum." He investigated general theorems relative to the centre of gravity and the equilibrium of bodies.

In hydraulics he seems also to have taken the first step, by showing that the water issues from a hole in the side or bottom of a vessel with the same velocity as that which a body would acquire by falling from the level of the surface to that of the orifice. It is needless to remark the importance of this principle to nearly the whole science of the motion of fluids.

But this is not the greatest discovery we owe to this distinguished friend and disciple of Galileo: he prosecuted with success another enquiry, in which even his illustrious teacher had failed. Galileo had observed the fact, that water will not rise in an exhausted tube (as in a pump) to a height greater than about thirty-three feet, but he was unable to give an explanation of the principle: Torricelli, however, perceived it, and proceeded to verify it. The column of water was held in equilibrium with a column of air: in the tube all pressure was removed from it; on the other side, or in the reservoir, it was pressed by the whole weight of the column of air reaching from the surface of the water to the top of the atmosphere. The height at which the water stood, or the quantity of water supported, depended on nothing but its weight compared with the weight of air: the same thing ought then to hold good with all other fluids. Mercury is a fluid about thirteen times more dense than water; a quantity of it, therefore, about one thirteenth of the quantity of the water, would be in like manner supported by the same column of air: that is, the column of mercury would stand at about the height of thirty inches.

This, therefore, Torricelli proceeded to try. A tube, closed at one end, being filled with mercury, carefully stopping the open end with his finger, he inverted it with the open end in a basin of mercury: the mercury in the tube immediately fell, and remained stationary at a height of about thirty inches. Thus it was evident that the principle assumed was correct: the weight and pressure of the atmosphere were established; and the theory of suction, nature's horror of a vacuum, and a host of kindred absurdities, at once and for ever exploded. The whole of this class of phenomena were now reduced to one simple law; and the action of the air referred to the same causes as those which influence the grosser forms of matter, however little its subtile nature might at first sight appear amenable to them. A new and wide extension was thus given to our perception of the simplicity and unity of design which pervades all nature; a fresh train of reflection opened to those who were capable of profiting by it; and an important step gained in the emancipation of the mind from the shackles of prejudice and the trammels of long-established authority.

Our admiration at the genius which thus enabled Torricelli to make so great an advance into the regions of truth, in despite of all the obstacles presented by long-cherished error, must be enhanced by his disinterestedness, in lamenting that these discoveries had not fallen to the lot of Galileo. "The generosity of Torricelli," observes professor Playfair, "was, perhaps, rarer than his genius: there are more who might have discovered the suspension of mercury in the barometer, than who would have been willing to part with the honour of the discovery to a master or a friend."

The principle once established, a multitude of consequences were soon seen to flow from it. Whenever, from any cause, the density of the air varied, a corresponding change must take place in the height of the column of water, mercury, or other liquid, in the exhausted tube. It only remained, then, to measure these changes by a fixed scale, and the construction of the *barometer* was complete. Mercury, being the densest fluid known, was of course universally adopted for the purpose, as requiring the smallest length of tube.

It is not to be supposed that a truth of such magnitude and value could be introduced without opposition and controversy on the part of those who, being incapable of understanding it, exposed their own ignorance by urging their difficulties as real objections.

Such objections, however, were the occasion of calling forth Pascal, who suggested a complete *experimentum crucis*, by observing that the height of the column of mercury, supported at the bottom and at the top of a considerable elevation, ought to be different, as the column of air above it is different. The experiment was tried on the mountain called the Puy de Dôme, in Auvergne. The diminished column of air at the summit, of course, sustained only a diminished column of mercury. When the law by which the density of the air diminishes in relation to the height to which we ascend in the atmosphere is known, this principle affords us the most refined and philosophical means of calculating our elevation; and, with the introduction of various modifications and corrections in detail, now constitutes the method of the barometrical measurement of heights.

Closely connected with these researches, and nearly contemporary with them, was the invention of the air-pump by Otto Guericke of Magdeburg, about 1654. He filled a barrel with water, and began to draw out the liquid by a common pump affixed to its lower part: he had proceeded but a little way, when the air burst into the barrel with a loud noise; and the failure of the experiment was thus as instructive as its success would have been. He soon found means of succeeding by the use of a glass globe instead of the barrel.

This, however, was an inconvenient process; and, so long as it was necessary to have the receiver, in the first instance, filled with water, hardly any experiments could be performed illustrative of the effects due to the want of air. Mr. Boyle made the first improvement, and reduced the air pump to nearly its present construction.

The machine being thus provided, its application soon brought to light a great variety of important properties of air: its elasticity, its necessity to combustion and the support of animal life, and the absorption of a certain portion of it during those processes: its action as the medium of conveying sound, with other modifications of its effects, were now made matter of obvious demonstration; and, by the striking and popular character of the experiments, a knowledge of those great principles of nature dependent on the pressure of the atmosphere became generally diffused.

In all these experiments Wren took a leading part among the scientific men in England: he also proposed many schemes for conducting meteorological observations on a systematic plan.

Otto Guericke has the credit of first inventing the electrical machine, in the form of a glass globe, which was made to rotate and communicate with a conductor, in the same manner as those now constructed with a cylinder, or plane circle of glass. He pursued a variety of researches, connected both with this subject and pneumatics. He was born in 1602, and died in 1686. His principal work is entitled "Experimenta Nova," published at Magdeburg.

To Pascal we owe the experimental establishment of the great hydrostatical law, that liquids press in proportion to their perpendicular depth. In his "Traité de l'Equilibre des Liqueurs" (ch. i.), we find the description of that fundamental experiment in which, in vessels of all shapes, sizes, or inclinations, communicating at the bottom, water will stand in all alike at the same horizontal level. He perceived the fertility of this principle, and followed it out into its various applications. There is, however, some doubt as to the precise share in the establishment of these truths which we are to assign to him, since Stevin undoubtedly laid down a principle which we may understand as comprehending that of perpendicular pressure; but how far he may be considered as having completely established it, or traced its consequences, may be matter of question. Pascal, however, appears to have been the first to exhibit, experimentally, the fact deducible from this great principle, that the pressure of a very narrow column of water communicating with one of any diameter, however great, of the same height, will maintain it in equilibrium; the equilibrium will, therefore, equally be maintained, if, instead of the large column of water, a piston be fitted into the cylinder loaded with an equal weight: in other words, the small column of water will thus balance any weight; and, by consequence, the mere addition of a little water will force that weight up. The existing state of the arts did not at the time permit the practical application of this beautiful result: it is now in common use in the hydrostatic press.

The same highly gifted philosopher also ascertained that two liquids of unequal density, communicating in a bent tube, will stand in equilibrium at heights inversely proportional to their densities: he investigated further the equilibrium of solids immersed in fluids. In a word, he may be said to have furnished us with nearly all the material advances made upon those fundamental principles originally demonstrated by Archimedes, and subsequently by Galileo and Stevin.

Pascal was, in many points of view, a very extraordinary character. He was born at Clermont, in Auvergne, in 1623: his father, an able mathematician, fostered his rising genius; and, at the age of sixteen, he produced a treatise on conic sections, which excited great admiration. His scientific career was short, though brilliant; and his retirement from these studies, to devote himself to theology, and even ascetic seclusion, has been variously construed, according to the religious views of the writers. His "Lettres Provinciales" are generally esteemed as models of powerful composition. His hydrostatical works were not published till after his death, which took place, at the early age of thirty-nine, in 1662.

The effect of the resistance of the air in the falling of bodies was explained by Mariotte by means of the air-pump, in the manner of what is called the guinea and feather experiment. Nothing, however, is easier than to make a coin and a piece of paper of the same size fall to the ground in precisely the same time without any exhaustion of air: we have only to lay the paper upon the coin, and to take care to drop it so that it shall preserve a horizontal position.

Mariotte was principally distinguished for his researches on the pressure of the atmosphere. He established the important law, that the density is precisely proportional to the compressing force, as also is the elasticity. He ascertained the existence of air in a state of mechanical mixture with liquids, and showed that it exists between their particles in a state of condensation. He also made experiments on a variety of other subjects, especially on the collision of bodies.

Astronomy.

Among the most eminent of the disciples of Galileo, Gassendi followed in the train of the great advocates of the Copernican system, and verified its principles by accurate observations. He distinctly pointed out the analogy between the laws of motion, as deduced by mechanical writers, and the motion of the earth: and made experiments to show that a body carried along by another acquires a motion, which it retains after it has ceased to be so carried. This referred to the old arguments of the Ptolemaists against the earth's motion, which had been lately revived by Morin in a treatise, called "Alæ Terræ Fractæ."

He was the first who observed the transit of a planet over the sun, that of Mercury in 1631. Kepler had predicted it, though he did not live to enjoy the triumph of witnessing a phenomenon furnishing at once so satisfactory a proof of the truth of the system of elliptic orbits on which the calculated prediction was founded.

The first transit of Venus ever observed took place a few years later, in 1639, and was seen only by Horrox and Crabtree, in England; the former having detected an error in the calculation from the received tables, which had led to the belief that no transit would occur. Horrox was one of the first to appreciate rightly the merit of Kepler's discoveries. His premature death, in 1640, was an irreparable loss to science. Some unfinished methods of computing tables of the moon, which he left behind him, were highly valued and made use of by Newton.

The elliptic orbits were first introduced into a systematic treatise on astronomy by Bullialdus, or Bouilaud, in his "Astronomia Philolaica," 1645. There was still, however, an attempt to refer the motion in these ellipses to some centre, about which it should be uniform. Indeed, for convenience of calculation, it was often desirable to find such a point. Bouillaud devised several methods for finding such a centre, and made calculations, founded on that hypothesis, which were, perhaps, accurate enough for his purpose. Dr. Seth Ward, then professor of astronomy at Oxford, and afterwards bishop of Salisbury, improved greatly upon these methods, by assuming the focus in which the sun is *not* situated as the centre about which the motion of the planet is uniform. In orbits of small excentricity, this fiction affords a rule very nearly approximating to the truth. Dr. Ward, however, appears to have regarded it rather as a real theory of the nature of the planetary motions. Indeed, very few, if any, of the astronomers of that day seem to have studied or completely understood the discoveries of Kepler, from which they ought to have seen, at once, that, however convenient the fiction of a centre of uniform motion may be for the purposes of computation, yet the law of nature is no other than that which Kepler discovered, of a motion, about the focus in which the sun is situated; not uniform in linear velocity, but uniform in the areas of the sectors which the radius passes over. Riccioli, in enumerating various hypotheses, does not so much as mention Kepler's discovery. His work, entitled the "New Almagest," is an elaborate account of the state of astronomical knowledge in the middle of the seventeenth century. He was, however, an enemy to the Copernican theory; and, though he states the reasonings for and against it, yet he estimates the evidence, not by the weight, but by the number, of the arguments. He was exceedingly anxious to prop the falling system of epicycles, and betrays, in the midst of much accuracy and ingenuity, a mind either totally incapable of comprehending discoveries like those which were now beginning to enlighten the world, or, what is perhaps not improbable, a determination to shut his eyes against them, and a design to blind others. He was a priest and a Jesuit.

Huyghens has the merit of first adapting the micrometer to the telescope: a contrivance on which all the nice determinations of minute distances in modern astronomy depend.

But perhaps the most essential circumstance to the perfection of astronomical instruments, is the adaptation of the telescope to quadrants instead of the plain sights, or small apertures, by which the unassisted eye determined the position of the object observed. Without entering into a variety of other causes of superiority (and these were subjects of great discussion at the time), it will amply suffice to mention one. The eye cannot appreciate an angular space in the sky less than about thirty seconds. In the best quadrant, with a plain sight, the altitude must be uncertain by that quantity: which is a large error compared with the exact requisitions of modern science. If, then, we substitute a telescope magnifying only thirty times, it will, consequently, enable us to fix the position to one second.

This plan appears to have been first practised by the French astronomer Picard, about 1665; but the idea was originally started by Gascoigne, before 1644. It has, indeed, been contended, from a letter of Gascoigne's, that he had actually brought this method into use.

Various contrivances were suggested for increasing the accuracy to which the *reading off*, as it is termed, of the indications of the index on the divided arc of any astronomical instrument might be carried. That called, from its inventor, the *vernier*, which has superseded all others, was suggested in 1631. It is an invention hardly to be surpassed for refinement of principle and simplicity of application.

Nearly forty years elapsed, after the first observation of an anomalous appearance in Saturn by Galileo, until Huyghens, with much more powerful telescopes of his own construction, discovered that all the appearances were no other than those presented by a broad flat ring surrounding the planet, and viewed under different degrees of obliquity from the earth. The gradual manner in which this explanation was unfolded is very instructive, and is given in full detail by the discoverer in his "Systema Saturninum," in 1659. He also discovered one of the satellites, but, after the fashion of the age, convinced himself that there could be no more, *because* the number of primary and secondary planets now amounted to twelve, the double of six, which is the first perfect number. However, in 1671, Cassini discovered another satellite, and this kind of speculation soon went out of vogue.

We have already remarked the difficulties in which astronomers had long been involved, in finding the right ascensions of the heavenly bodies: all the determinations of their positions may be ultimately reduced and analysed into methods of referring them to two planes at right angles; one of these is the plane of the meridian; and this being readily found, it was a comparatively easy task for the astronomer, even in the ruder stage of the science, to measure the position of the stars in this plane; that is, to observe their altitude, or their distance from the pole when they were on the meridian; thus, one essential element, their declination, and thence their latitude, was found without difficulty. Modern refinement, indeed, has given the method increased accuracy, but the principle could not be more simple or exact. The measurement of position in a direction *transverse* to this is the other essential requisite: in this, the older astronomers could devise nothing better than very circuitous and uncertain methods: there was here no fixed plane to which the stars could be referred; but there was one circumstance which had indeed been noticed, and some attempts made by astronomers to avail themselves of it, as yet without success. This is, the perfectly uniform rotation of the earth: if a star is accurately on the meridian, another star to the eastward of it will come on to the meridian in a certain time after it; and if we compare these intervals of time for a number of different stars, they are *exactly* proportional to the distances of those stars in a direction at right angles to the plane of the meridian: the whole difficulty, then, is in getting an *exact* measure of *time*. If this be provided, the two simple observations of the height of the stars upon the meridian, and the differences of their times of passing the meridian, give us at once the two essential elements on which all other determinations in astronomy depend. Now, till the time of Huyghens, there had been, as we have before observed, no precisely, or even tolerably accurate means of measuring time. Clocks had been constructed, but they wanted that essential principle, an exact regulating power: this was the grand desideratum which Huyghens supplied by adapting the pendulum to them; they were thus rendered available to the astronomer; and the clock, increased in its accuracy by the labour and ingenuity of a long succession of eminent artists, is now an essential part of the furniture of the observatory, along with an instrument always kept in the plane of the meridian, and that which gives the measure of altitude in that plane. Godfrey Wendelein, a Dutch astronomer of considerable merit about 1660, confirmed by a comparison of his own observations with those of the ancients the diminution in the obliquity of the ecliptic, originally pointed out by Tycho Brahe: he did not, however, determine it to any exactness: he also found that Kepler's laws extended to the revolutions of Jupiter's satellites.

John Hevelk or Hevelius, a citizen of Dantzick, who flourished about the same time, was a most assiduous observer, and enriched astronomy by his accurate delineations of the surface of the moon: he also discovered, in addition to the moon's libration in latitude observed by Galileo, a libration in longitude also: the former consisting in the circumstance, that we see more of the upper and lower edges of the moon alternately, from her not moving exactly in the plane of the ecliptic; the latter, in that she always presents the same face to the centre of her orbit, but not to the earth, owing to the elliptic form of the orbit, in the focus of which the earth is placed. He also threw out some hints of the parabolic form of the orbits of comets.

Another contemporary astronomer, Gabriel Mouton, of Lyons, first practised that important process, the interpolation, for determining the place of a planet, at some instant of time intermediate to two others, for which its place is given in the tables: he also used the pendulum to measure differences in right ascension.

The first idea of the transit instrument, a telescope moveable only in the exact plane of the meridian, to watch the *transits* of stars across it, though in a very imperfect form, seems to have been suggested by Roemer about 1690.

The elder Cassini, was invited from Italy into France by Louis XIV., in 1669, and settled in the observatory at Paris, where he continued a long series of accurate and valuable labours, particularly directing his attention to perfecting the theory of the interesting system of Jupiter, which so beautifully represents, on a small scale, the greater planetary system. He also discovered ultimately four satellites of Saturn, in addition to the one observed by Huyghens.

Both he and Maraldi, and probably other astronomers of the period, observed a highly remarkable circumstance connected with the eclipses of the satellites of Jupiter. Their theory had now been sufficiently determined to afford the data for calculating these eclipses; a remarkable difference, however, sometimes appeared between the time of their occurrence, as computed and observed. It was soon found that the emersions took place behind the calculated time nearly fourteen minutes when the earth was at that part of its orbit furthest from Jupiter; and that the difference diminished up to the position where it was nearest. It seemed to be connected with this circumstance alone, and independent of all others. This was, however, only as yet ascertained with certainty with regard to the first satellite.

A simple explanation suggested itself at the same time to Cassini, and to Olaus Roëmer, a Danish astronomer. The former, however, withheld his theory, being uncertain whether it would be confirmed by its holding good also in regard to the other satellites, whose elements were as yet too little known to warrant any conclusion. The latter, less scrupulous, broached at once the idea that the difference is owing to the *velocity*, inconceivable indeed, but yet finite, *with which light travels:* and that it is precisely such as to occupy about fourteen minutes in traversing the diameter of the earth's orbit. He communicated an account of the whole investigation to the Academy of Sciences, in 1676.

Maraldi felt a certain degree of difficulty in admitting this explanation, observing, that a similar effect ought to follow, in a less degree, according to the position of Jupiter with regard to his aphelion; but observation was not then accurate enough to detect such an effect.

More modern observations have shown that it is actually the case; and have also extended the result to all the satellites. The caution, however, both of Cassini and of Maraldi is perfectly justified on the soundest principles of inductive science; and while we cannot fail to regard with more immediate satisfaction the bold announcement of Roëmer, yet we ought, perhaps, to esteem still more highly the sound philosophy of Cassini, which at once enabled him to grasp so happy an explanation, founded on a great principle of nature, now for the first time evinced by observation, and yet to resist the temptation of announcing it as a discovery, because it still wanted a full and legitimate induction to its establishment.

Cassini was certainly one of the greatest astronomers of his age; he was born at Nice, in 1625, and died in 1712. He determined the rotations of several of the planets on their axes by means of their spots. He gave accurate measurements of the ring of Saturn, and of the flattened form of Jupiter's disk. He greatly improved the tables of refraction, and, from a more correct notion of its amount, was enabled to make corresponding corrections in the estimate of parallax. He discovered that singular phenomenon, the zodiacal light, or luminous train, sometimes seen extending upwards from the sun, when on the horizon, and which, to this day, has remained without explanation. He completed the theory of the moon's libration, by showing that her axis of rotation is not perpendicular to the ecliptic, but slightly inclined, and that the nodes of the lunar equator always coincide with those of the orbit. This explained satisfactorily what had been before observed, that the period of the inequalities of the libration coincided with the revolution of the nodes of the orbit.

Optics. — Telescopes.

Optics, as indeed all branches of the natural sciences, are under great obligations to Huyghens. This science, however, seems to have been that which most occupied his mind. His Dioptrics is a work, the greater part of which was composed in his youth, but which was not published till after his death. It is written with great perspicuity and exactness, and is said to have been a favourite book with Newton. Though commencing with the first elements, it contains a full developement of all the details of the construction of telescopes: a department in which the author was also practically eminent. He polished lenses and constructed telescopes with his own hands, and has appended to his Dioptrics the results of his experience in a tract, "De formandis Vitris." He gave enormous lengths to his telescopes. Some of his object glasses are now in the possession of the Royal Society, of 130 and 150 feet focal length. Such instruments were very unmanageable; to render them less so, Huyghens adopted the plan of dispensing altogether with a tube, mounting the object glass on the top of a lofty pole or building, and turning it in the requisite direction by machinery, whilst an eye-piece was applied below. There was, however, a twofold advantage obtained by these great lengths.

The magnifying power of a telescope depends upon the *relative* focal lengths of its object and eye glass: the greater the *ratio* of that of the former to that of the latter, the greater the magnifying power. But this does not (in theory at least) involve the consequence, that the *absolute* focal length of the object glass should be very great. There is, however, practically, an advantage in this respect; but the main object was dependent on another principle. The fact had already been well understood, that, in oblique refraction, light is separated into colours; and any small portion of a convex lens is, in fact, a prism, so that the rays proceed to the focus, separated into the prismatic colours, which occasions the image formed at the focus to be edged with a fringe of colours, and also rendered indistinct, owing to the want of convergence of those coloured rays at the same point: but it was soon found that the degree in which this takes place is independent of the focal length of the lens, and so long as its diameter (or aperture, as it is termed) remains the same, the degree of colour will be the same. Hence, by increasing the focal length to a great extent, the edge of colour remains the same, while the image (with a given eye glass) is greatly magnified in proportion: or the coloured edge may be made to bear an insensibly small ratio to it, and thus the inconvenience be almost got rid of. We shall afterwards see why these constructions are now no longer resorted to.

The subject of colours in the refraction of light had attracted the attention of Dr. Barrow, then Lucasian professor of mathematics at Cambridge; but the theory he gave was very unsatisfactory and unphilosophical. It is, however, highly probable that its promulgation may have been the immediate occasion of directing the attention of Newton to the subject. Barrow treated of the mathematical parts of optics with all his powerful ability, and discussed some of the most difficult problems relating to the subject, which then engaged the attention of geometers, in his lectures delivered in 1668, and published in the following year.

The invention of the *reflecting* telescope exhibits a modification of the same theoretical principle as that on which the refracting telescope is constructed; in an abstract point of view, perhaps, even simpler and more obvious than the latter; at any rate such as must have immediately suggested itself, as far as the principle is concerned: but to reduce it to practice required some further contrivance.

Rays of light from a distant object falling upon a concave reflector, would give an image in its focus which might (in theory) be magnified by an eye-glass; but how could the eye be placed there to see it without intercepting the light? The use of the object glass, or, in this case, the reflector, is simply to collect a great *quantity* of light: hence the cutting away a small *hole* in the middle of it will not materially interfere with its office. A second small reflector, placed facing the large one, will also intercept only a small quantity of the light. The eye being placed with an eye-glass at the back of the great reflector opposite the hole, may then receive the image thrown back by the small reflector, which has received from the large one the rays going to its focus to form that image: thus the difficulty is surmounted.

This invention was made by James Gregory, professor of mathematics at St. Andrew's, and afterwards for a short time at Edinburgh. He gave a description of it in his Optica Promota in 1663. It has been since known by the name of the Gregorian construction. Cassegrain afterwards modified it by making the small reflector convex instead of concave.

The Optica Promota contains also many important investigations relating to other parts of optics, especially the formation of images by lenses. The author likewise directed his attention to the advantages which would theoretically result from having a reflector, whose section should not be the arc of a circle, but of a parabola; in which case, by a property of that curve, all rays falling parallel to the axis would be *accurately* brought to convergence at the focus. He devised methods for forming such surfaces; but the practical difficulties encountered were so great that no advantage could be derived from the idea. Subsequent artists have experienced the same difficulties; and all attempts to prosecute such a plan have long since been abandoned; and after all, perhaps, the advantages have been overrated, since a very slight deviation from exact parallelism to the axis, would occasion considerable aberration from the true focus.

Gregory was a man of highly acute and original mind; and in a remote situation supplied the want of intercourse with the scientific world from the powerful resources of his own genius. Having no communication with the Continent, he had never seen the works of Snell or Des Cartes; but he deduced for himself the law of refraction, by an independent investigation.

Double Refraction.

Erasmus Bartholinus, in 1669, first observed the singular fact, that a small object seen through a transparent crystal of the Iceland spar appeared double. He pursued the subject, however, but little further than to give some very general description of it. It was evident that the ray was divided into two within the crystal, following different laws of refraction.

Huyghens soon after directed his attention to the phenomena of the doubly refracting crystal; by a series of accurate measurements, he determined the directions assumed by the two rays under all varieties of direction of the incident ray. One of the rays was found to follow the ordinary law of the sines in the plane of incidence; the other followed a variable law of deviation, and this, too, in a variable plane. Huyghens, however, succeeded in tracing the law of these changes, which is somewhat complex. He illustrated it by a geometrical construction, which represents the position of the ray in all cases; but of which it would be impossible to give any general description. It was, however, intimately connected with a theory of which we must now proceed to give some account.

Theory of Undulations.

Perhaps the most curious portion of Huyghens's investigation consists in his very remarkable theory of light, which, framed in the first instance upon the simplest conceptions, and admirably applying to the representation of the ordinary phenomena, was soon found to be no less beautifully applicable to the more complex case of double refraction. This theory was first communicated to the Academy of Sciences at Paris in 1678; but afterwards published in a separate form in 1690, under the title of "Traité de la Lumière." Propounded, in the first instance, to explain the limited range of optical phenomena then known, this theory, with a few modifications, has been found in the hands of subsequent philosophers to afford by far the most complete and satisfactory representation of nearly all the varied and complicated results which optical experiments have disclosed. The original idea of Huyghens was simply this: that an inconceivably subtile and elastic medium, or æther, pervades all space and all bodies, existing within denser media in a state of greater condensation. Waves, pulsations, or undulations excited in this medium are propagated in different directions, according to the impulse originally communicated by some peculiar action of those bodies which we call luminous; and these pulsations reaching our eyes, affect us with the sensation of vision. Under ordinary circumstances these undulations are propagated from the original centre of excitation in a regular circular or spherical form, somewhat like the circles produced on dropping a stone into the water.

By an application of these views by no means difficult, he gave a complete explanation of the ordinary phenomenon of reflection and refraction. In reflection the waves rebound in a way easily imagined; in the case of refraction, owing to the increased density, the undulations are propagated more slowly within the transparent medium than in the air. Hence, in order to pass in the same time, the waves must take a shorter course; that is, (impinging obliquely) must proceed in a direction nearer to the perpendicular, and this in proportion to the increase of density. The ratio is that of the refracting power of the medium; and it easily follows, that it is the same as that of the sines of the angles which the incident and refracted rays (or direction of the radius of the front of the waves) make with the perpendicular to the surface. This agreed exactly with Fermat's reasoning, before referred to.

The undulatory theory thus admirably applying to the ordinary refraction of a single ray, Huyghens proceeded to enquire into its applicability to the phenomena of double refraction. The ordinary ray was admitted to follow the ordinary law of the sines, and to be represented by spherical undulations. The extraordinary refraction could be expressed by no simple law (as before observed), but it might be represented by a complicated construction, in which its position is assigned by means of a plane always touching a *spheroid*. This geometrical theory corresponded exactly with the physical theory of a set of undulations propagated no longer in a *spherical*, but in a *spheroidal* form. By assuming, then, undulations of this kind in certain crystallised media, by which one portion of the light proceeded, whilst the other was propagated in common spherical undulations, a faithful presentation was given of the phenomena of double refraction. The theory thus far, then, was assigned purely as an hypothesis which explained the phenomena. It was further to be tried, to be received or rejected, as it might apply or not to such new phenomena as might afterwards be discovered. Though, as we shall see, several facts in optics were brought to light soon after, yet it does not appear that any attempt was made to apply this theory to their explanation.

Inflection of Light.

Grimaldi, a learned Jesuit, published at Bologna in 1665 an account of some remarkable phenomena in optics, which have subsequently acquired a high degree of interest and importance. Indeed, considering the very singular and even paradoxical nature of one of the results, it is astonishing that they did not attract more attention at the time.

The main fact, which he examined with great care, was this: — On placing a narrow opaque body, such as a wire or hair, in a beam of the sun's light, admitted through a pin-hole into a dark room, he found the shadow received on a screen at different distances considerably broader than, upon a geometrical construction of rectilinear rays, it ought to be at those distances. The width of the shadow was defined accurately by certain bright lines, which appeared running parallel to the edges of the hair: all within these being considered as shadow, though the darkness gradually diminishes from the central part towards the boundaries. He also noticed, that when the breadth of the opaque body does not exceed a certain amount, the middle part of the shadow, instead of being uniformly dark, is streaked with several parallel bright and dark bands, in the direction of its length, the centre band being always bright, the number varying with the breadth and distance.

He considered all these phenomena due to a certain bending or *inflection*, as he called it, which he supposed the rays to undergo in passing near the edges of the opaque body; and that these stripes within the shadow were due to the joint action of the two portions of light coming from each side. The result he broadly announced by saying, that in this case the joint action of two portions of light produced darkness.

Dr. Hooke appears to have tried similar experiments, without any knowledge of what Grimaldi had done. In 1672 and 1674, he communicated two papers to the Royal Society on the subject. From some of his expressions, it would seem that he adopted a theory of light resembling that of Huyghens, and gave a sort of general explanation of the facts by supposing a principle analogous to that since termed *interference*, and which has been most extensively applied in optics.

Mechanics.

The mechanical researches of Huyghens are of great value. In addition to those on collision, before mentioned, he was the first to demonstrate the relation between the length of a pendulum and the time of its vibrations, as also between this and the time of rectilinear descent down the length of the pendulum.

His practical application of these principles is that which has introduced the great improvement in clocks by the use of a pendulum as the regulating power. This grand invention is explained in his "Horologium Oscillatorium," published in 1670, though the date of the actual invention is 1656.

The common pendulum vibrates only in circular arcs; but so long as these are not extended beyond very small limits, the times of all the vibrations are precisely equal. If the arcs be greater, this equality is no longer preserved. It was one of Huyghens's investigations to find a curve, in which, if a body moved as a pendulum, the vibrations in all arcs should be equal; and it was a mathematical result, that that curve must be the cycloid. By the ordinary mode of suspending a pendulum, it necessarily performs its vibrations in circular arcs; but Huyghens devised a method, founded on a geometrical property of the cycloid, for making a pendulum oscillate in that curve; in which case, its motions in all arcs, great or small, would be strictly *isochronous*.* The property referred to was, that the *involute* of a cycloid is a cycloid: hence the thread sustaining the weight being unwound from the arc or surface of a cycloid, the body which it carries will at the same time move in the arc of another cycloid. The weight was, therefore, suspended between two pieces of wood or cheeks, cut in the shape of cycloids. When it hung motionless, the string was a tangent to both the cheeks at the point of suspension, where they also touched each other. In any other position the string was partially wound upon one of the cheeks, while the remainder formed a tangent to the curve.

This method is, however, inapplicable in practice, and can only be regarded as an elegant theoretical speculation. For practical purposes, a pendulum simply suspended and vibrating in small circular arcs, possesses every requisite, even for purposes of the utmost exactness.

In these researches, several eminent mathematicians communicated with Huyghens, among whom we find Wren and Wallis taking a conspicuous part.

In the theory of motion, another important principle seems to have been first brought to light by the researches of Huyghens; the discovery of the *centre of oscillation:* an enquiry of a singularly refined and beautiful character, and which has become connected with the most extended speculations in analytical mechanics. The nature of this enquiry will be readily understood, when we merely consider for a moment, that in a pendulum consisting of a large mass of matter, every particle, if suspended separately, would vibrate in a different time, according to its distance from the point of suspension: when, therefore, these particles are all connected together, they must affect each other's motions; but upon the whole there will be some one of them whose times of vibration, when independent, are the

* ἴσος, equal; χρόνος, time.

same as those of the connected mass. The distance of such a point from the point of suspension, was what Huyghens investigated, and its position in the mass is called the centre of oscillation of that mass ; it will obviously vary considerably, according to the figure given to the mass. Huyghens devised means of obtaining a general solution of the problem, though subsequent improvements in analysis have shown that his method was more complicated than necessary, and have considerably generalised its applications.

English Physical School. — Boyle, Hooke, &c.

Whilst these researches were being carried on on the Continent, in England an experimental school was now forming, faithful to the principles of the inductive philosophy. Foremost among its supporters was the hon. Robert Boyle, whom Boerhaave styles " the ornament of his age and country, who succeeded to the genius and enquiries of the great chancellor Verulam." And he has left no doubt as to the master by whom he had been taught; for, in recording his experiments, he has retained not only the method, but the peculiar idiom and technical phrases of Bacon. He has been already mentioned for his principal invention, the air-pump. But his attention was directed to a vast range of physical subjects, which he pursued in conjunction with several other devoted cultivators of science who were closely associated together about the year 1650. Boyle had studied at Leyden, and after travelling on the Continent, resided for some time at Oxford, where at that time the kindred genius of Wallis, Wren and others, had drawn them together under the auspices of Dr. Wilkins, warden of Wadham college, in whose lodgings private meetings of these friends of science regularly took place; and Wadham acquired the name of " the philosophic college." Besides pneumatics, Boyle's researches were more especially devoted to chemistry, to enquiries respecting light and colours, and to the properties of solar as compared with terrestrial heat. These and a variety of other topics were freely discussed at their meetings, and experiments publicly tried.

Such meetings had indeed already taken place in London: in 1645, Dr. Willis had formed a sort of club, in the first instance consisting chiefly of the professors of Gresham college, then comprising some of the most distinguished men of science of the day. Their numbers were soon increased; and, in 1648, many of the members retired to Oxford, the metropolis being then in a state of great disturbance, and joined in the meetings before mentioned. "Their first purpose," says bishop Sprat (History of the Roy. Soc.), "was no more than only the satisfaction of breathing a fresher air, and of conversing in quiet one with another without being engaged in the passions and madness of that dismal age. And from the institution of that assembly it had been enough if no other advantage had come but this, that by this means there was a race of young men provided against the next age, whose minds, receiving from them their first impressions of sober and generous knowledge, were invincibly armed against all the enchantments of enthusiasm. But, what is more, I may venture to affirm, that it was in good measure by the influence which these gentlemen had over the rest that the university itself, or at least any part of its discipline and order, was saved from ruin. Nor were the good effects of this conversation only confined to Oxford, but they have made themselves known by their printed works, in our own and in the learned languages, which have much conduced to the fame of our nation abroad, and to the spreading profitable light at home."

Dr. Wallis, who took a leading part in these meetings, has thus described their objects: "Our business was (precluding matters of theology and state affairs) to discourse and consider of philosophical enquiries, and such as related thereto: . . . . with the state of these studies, as then cultivated abroad and at home. We then discoursed of the circulation of the blood, the valves in the veins, the lymphatic vessels; the nature of comets and new stars, the satellites of Jupiter, the oval shape (as it then appeared) of Saturn, the spots in the sun, and its turning on its own axis; the inequalities and selenography, the several phases of Venus and Mercury; the improvement of telescopes, and grinding of glasses for that purpose; the weight of air, the possibility or impossibility of vacuities, and nature's abhorrence thereof; the Toricellian experiment on quicksilver, the descent of heavy bodies, and the degrees of acceleration therein, and divers other things of the like kind; many of which were new discoveries at that time, and others not so generally known and embraced as now they are; with other things appertaining to what has been called the New Philosophy, which, from the time of Galileo at Florence, and sir Francis Bacon, lord Verulam, in England, hath been cultivated in Italy, France, Germany, and other parts abroad as well as with us in England."

This interesting passage gives us a characteristic sketch of the actual points of philosophic enquiry, which at that time attracted the most peculiar attention; and presents us with a delightful picture of the small band of associated philosophers, evincing the efficacy of one of the best uses of true philosophy, that of elevating the mind above the turmoils of the world, and supplying, in its pure intellectual pleasures, a source of consolation amid external troubles. The original journal books of their transactions are preserved in the Ashmolean Museum at Oxford.

Dr Robert Hooke, one of the most eminent of the associated English philosophers of this period, was a man of singular ability and most indefatigable industry. He was distinguished by acute inventive powers, and an unusual versatility of talent. But a certain narrowness of mind threw an unfavourable shade over his character. He was envious and jealous of the fame of others, and arrogant in pushing his own claims. Exact and ingenious, he directed his attention to almost every branch of science, and in most departments prosecuted considerable improvements. But he was too ready to lay claim to priority in any discovery which was announced, to the disparagement of others, accusing them of appropriating his ideas.

When Huyghens's application of the pendulum to clocks, and his idea of the cycloidal cheeks, were published, Hooke immediately raised pretensions to both as his own inventions. But the latter idea could not have occurred to any one except upon a knowledge of the mathematical principle of involutes; and in geometry, Hooke was very deficient. The priority of Huyghens, however, on other grounds, is quite unquestionable. Hooke also claimed the invention of the air-pump: there is no doubt that he improved upon it, and extended its applications. He contributed much to the perfection of the diving-bell; and has certainly the merit of a material improvement on watches; that of adding the spiral spring to regulate the balance. He probably aimed at giving greater perfection to these machines, from perceiving their importance in the great problem of the longitude. He devoted himself much to astronomy; effected some improvements in astronomical instruments; and entered warmly into controversy with Hevelius, in support of the principle, then first introdued, of fitting up quadrants with telescopes instead of plain sights: this was about 1665.

In practical mechanics, we owe to Hooke many ingenious contrivances, into the details of which it is impossible here to enter. The fertility of his invention was inexhaustible, and we find a vast number of theoretical suggestions of the highest ingenuity recorded in the diary he kept of all his pursuits, besides the numerous improvements which he actually reduced to practice.

Hooke associated with Wren in barometrical observations, with the special view of examining the hypothesis of Des Cartes, that the tides are occasioned by the pressure of the moon on the atmosphere at her passage over the meridian; it is hardly necessary to say that they completely disproved the existence of such an effect.

The phenomena of capillary attraction engaged the attention of many of the associated philosophers about this period, and Hooke's dissertation on the subject seems to have been considered the most satisfactory then offered : the subject, however, was yet but imperfectly understood.

So multifarious were his researches in almost every part of physical science, that we shall not attempt to enumerate them. In all of them he displayed those decided marks of ability and genius, which, had it been more concentrated on a few of the more important subjects, would doubtless have placed him in a far higher rank among philosophers, and have conferred more substantial benefits on science.

The gradual variation in the declination of the magnetic needle was first completely established by Gellibrand, professor in Gresham college, in 1625, from an elaborate comparison of the recorded observations of Gunter and Mair, in 1612, with those of Burrows, in 1580. The subject afterwards attracted the attention of Hooke and Halley.

A remarkable optical phenomenon observed about this time has since become of the highest interest, in connection with the theory of light. Mr. Boyle appears to have been the first who has recorded any observations on the colours exhibited by *extremely thin films* of transparent substances. The common exhibition of them in blowing soap bubbles had probably been known for ages. Dr. Hooke first found that similar colours might be obtained by splitting mica into extremely thin films. He found that the tint depended on the thickness, but could devise no means of measuring such extremely small thicknesses, with the view of comparing the change of colour with tnat of thickness. He observed, however, another form of the experiment, which consists in forming a thin film of *air*, by pressing two glasses hard together, which produces the same colours. They vary as the glasses are more closely pressed. If one or both the glasses be lenses of very small curvature, the colours are arranged in the form of rings round the point of contact. It was soon seen that none of these are pure prismatic colours ; no precise analysis, however, of the phenomenon was yet made. The same colours are also formed by a minute drop of oil which spreads itself into a state of extreme tenuity on the surface of water. They are in like manner often seen in the films which collect on the surface of stagnant water. No satisfactory explanation seems to have occurred to these philosophers, and the subject was left to occupy, at a future period, the enquiries of Newton, and to afford the first hint of a certain peculiarity in the intimate nature of light, which was long afterwards to supply a clue to a vast range of phenomena.

Establishment of Scientific Societies.

The latter part of the seventeenth century witnessed, as we shall afterwards more fully perceive, the most complete revolution in physical, and especially astronomical science. In tracing the steps which led to this important change, we must not omit to notice, as one of the most efficient, the establishment of philosophical societies, which took its rise in the early part of this century, and some of the most celebrated of such institutions were fully organised before its close. The utility of such societies is readily evident: it is needless to enlarge upon the facilities they afford, or the stimulus they create, for the advance of philosophical research. But their importance was still greater at the period of which we are speaking than at the present day. Perhaps we can hardly estimate fully the condition of society in those times, destitute, as it was, of those means of constant intercourse and uninterrupted communication which, at the present day, almost annihilate distance, and connect the most remote recluse with the busy world. Scientific communication in particular, which now circulates weekly and monthly information of every advance in knowledge, and every improvement in experimental research, was then unknown. The philosopher of those times lived in a state of isolation, and carried on his labours single-handed; circumstances which must have greatly retarded the progress of discovery. To remedy these evils, the first and natural expedient was, for those engaged in a common pursuit, where proximity of situation permitted, to associate together, and contribute all the advantages which co-operation and interchange of ideas could afford, to the promotion of the work in which they were engaged.

Several small associations of this kind were successively formed in Italy about the period of Galileo's discoveries: the most considerable of these was termed the Lyncean Society, under the patronage of the marchese Frederico Cesi. It took its rise about 1611, and Galileo was himself an active member of it. It afterwards declined; but was succeeeded, in 1657, by the Academia del Cimento, at Florence, which owed its origin, and its high, though brief reputation, to the disciples of Galileo, Viviani, and Toricelli.

In England, we have already observed that during the civil wars a small party, attached to science, withdrew from the miserable dissensions of that unhappy period into the retirement of philosophic discussion. Their meetings appear to have commenced about 1645, in London; but, in 1648, the most active of their number having removed to Oxford, their meetings were there continued; though it appears that those who remained in London likewise kept up theirs. Subsequently, the two branches of the original society reunited, and were incorporated by a charter from Charles II. in 1662. The first idea of such an institution seems to have been suggested by the writings of lord Bacon, who, in his "Nova Atlantis," had given a highly interesting sketch of the plan of a society devoted to the purpose of advancing the natural sciences.

"The foundation of the Royal Society," it has been well observed, "was an attempt to reduce to practice the splendid fiction of the New Atlantis. The same comprehensive mind which first developed the true method of interpreting nature, sketched also the first draught of a national association for undertaking, by a system of distributed and combined exertion, the labour of that work. This philosophical romance was not composed by its great author to amuse the fancy, but to dispose the minds of the legislature towards the foundation of a public establishment for the advancement of science. . . . . . It appears that the basis of the great institution which Bacon meditated, was a public provision for the maintenance and promotion of science. It was one of the defects noted by him in his masterly survey of the state of learning, that science had never possessed a *whole man;* and he exerted all the influence of his high station and commanding talents, to promote the supply of that defect. . . . . The Royal Society did not attempt to execute this part of Bacon's plan; but, in other respects, it copied as closely as possible the model of the 'six days' college.' It was not, then, an association of individuals throwing their contributions casually into a common stock, but a body politic of philosophers acting in a corporate capacity, and with systematic views, allotting to its members their respective tasks, and conjunctively debating and consulting for the advancement of knowledge." *

To this systematic division of labour in the early stages of the Royal Society's existence, we owe some of the most important researches of that day. It may be sufficient to name the "Sylva" of Evelyn, and the "Laws of Collision," by Huyghens, Wren, and Wallis. But this system did not long continue. In Germany, the Academia Naturæ Curiosorum dates its commencement from 1652; and the historian of this institution expressly ascribes its origin to the suggestions given in the writings of Bacon. The Royal Academy of Sciences

* Rev. W. V. Harcourt's Address. Reports of British Association, p. 25.

at Paris was founded in the reign of Louis XIV., under the administration of Colbert, in 1666. The Institute of Bologna, and several others in various parts of Europe, date their origin at about the same period.

The Academy of Sciences at Paris has approached more nearly, in one respect, to the model proposed by Bacon, than any other institution of the same nature. It recognised, besides independent members, a class of *pensionnaires*, twenty in number, who received salaries from the government, and were bound in their turns to furnish the meetings with scientific memoirs; and each of them also, at the beginning of every year, was expected to give an account of the work in which he was to be employed. The benefits derivable from some sort of endowment thus bestowed, are incalculable. To detach a number of men of ability from every thing but scientific pursuits; to deliver them alike from the embarrassments of poverty, or the temptations of wealth; to give them a place and a station in society the most respectable and independent, is to remove every impediment, and to add every stimulus to exertion. "To this institution," observes professor Playfair, "operating upon a people of great genius and indefatigable activity of mind, we are to ascribe that superiority in the mathematical sciences which, for the last seventy years, has been so conspicuous." We may add, that, fully concurring in the truth of this representation as far as relates to the state of science and scientific men in France, we do not feel by any means sure that a similar system would succeed equally well in countries where the general constitution of society may be different, and the national genius of a character in many respects dissimilar.

The Royal Society of London is an association perfectly independent; and whose members, so far from being pensioned, contribute to defray the expenses of their meetings and Transactions, which, consisting in a great measure of elaborate memoirs, many of them of an abstruse nature, and none of a popular character, could not be published by any other means. In this respect alone, the society confers an important benefit on the science of the country: and the high character and extended celebrity which the volumes of its Transactions have acquired, again reacts upon the cultivation of science, and affords a powerful stimulus to those engaged in such pursuits, to produce papers which may obtain the honour of insertion in those select depositaries. The interchange of ideas, and the personal acquaintances formed or kept up at the meetings of such bodies, are advantages also of no small value; while the concentration of scattered information, and the directing to one centre the energies of many minds, each powerful in its own way, affords the means of mutual assistance in their researches, and thus increases the light thrown upon particular points of enquiry in an almost incalculable ratio.

Bacon, in proposing his new scheme of a philosophical institution, is of necessity led to justify the proposal by a reference to the services of existing institutions, and by showing how little they had done, or by their very nature and constitution were likely to do, towards the real advancement of science. He censures with severity, indeed, but without bitterness, in strong terms, but with a masterly exposition of the facts, which evinces the perfect justice of his condemnation, the system of the colleges and universities of his day.* He observes, that the lectures and exercises were all of such a nature, that no deviation from the established routine was likely to be thought of;—that if a solitary attempt were made, the whole burden of it would rest on the individual, who would, moreover, find such attempts a serious impediment to his own advancement. The studies of these places were confined to a certain set of authors, as it were imprisoned within those limits: any one who should venture to deviate from this course, would be immediately condemned as a turbulent innovator. "But," he adds, "in the arts and sciences, as in mines, the

* Nov. Organon, i. Aph. 90.

whole region ought to resound with new works and further advances." Such was the state of the case in his day; nor shall we find it much better in later times; for the present it must suffice to remark, that (in regard to Oxford) when the scholastic forms were in a great measure broken up under the reign of Cromwell, the favourable opportunity which was afforded for establishing a better system in their place, was, as we have seen, not lost by a few ardent friends of true science: owing, however, to a variety of causes, their partial attempts failed, as far as the university was concerned; and, with the return of the Stuarts, the old system was re-established in all its authority.

Approaches to the Theory of Gravitation.

Boulliaud, as we have observed, introduced the elliptic orbits in his astronomical system. It is not a little remarkable, that in the same work he should also have broached an idea respecting gravitation, the connection of which with the form of the orbit was of course hidden from him. He remarks, that "if attraction exist, it will decrease as the square of the distance." The scattered elements of the truth were thus being brought together from all quarters; but there still was wanting the master genius to perceive their connection, and to combine them into a whole.

The influence of gravity was, perhaps, yet more distinctly recognised by Borelli, in his work on "Jupiter's Satellites," in 1666. He here maintains expressly, that all the planets perform their motions round the sun according to a general law: that "the satellites of Jupiter and of Saturn move round their primary planets in the same manner as the moon does round the earth, and that they all revolve round the sun, which is the only source of any virtue, and that this virtue attaches them, and unites them so that they cannot recede from their centre of action."

In the same year, Dr. Hooke read to the Royal Society an account of a series of experiments for determining whether bodies experience any variation in their weight at different distances from the centre of the earth. His first experiments (as he himself confesses) were very unsatifactory; but they led him to the ingenious idea of measuring the force of gravity by observing the rate of a pendulum at different altitudes. How far he put this in practice does not appear. He also gave a sort of mechanical illustration of motion in an orbit, by a weight freely suspended by a string, and thus made to oscillate either in an elliptic or circular course.

In 1674, Hooke resumed the subject in a dissertation entitled "An Attempt to prove the Motion of the Earth from Observation," in which the following remarkable passage occurs:——

"I shall hereafter explain a system of the world differing in many particulars from any yet known, answering in all things to the common rules of mechanical motions. This depends upon three suppositions. 1st, That all celestial bodies whatsoever have an attraction or gravitating power towards their own centres, whereby they attract not only their own parts, and keep them from flying from them, as we may observe the earth to do, but that they also do attract all the other celestial bodies that are within the sphere of their activity; and consequently, that not only the sun and moon have an influence upon the body and motion of the earth, and the earth upon them, but that Mercury, Venus, Mars, Jupiter and Saturn also by their attractive powers have a considerable influence upon its motion, as in the same manner, the corresponding attractive power of the earth hath a considerable influence upon every one of their motions also. The 2d supposition is this; That all bodies whatsoever, that are put into a direct and simple motion, will so continue to move forward in a straight line, till they are by some other effectual powers deflected and sent into a motion describing a circle, ellipsis, or some other compounded curve line. The 3d supposition is, That those attracting powers are so much the more powerful in operating, by how much the nearer the body wrought upon is to their own centres. *Now what these several degrees are, I have not yet experimentally verified:* but it is a notion which, if fully prosecuted, as it ought to be, will mightily assist the astronomers to reduce all the celestial motions to a certain rule, which I doubt will never be done without it. He that understands the nature of the circular pendulum and circular motion will easily understand the whole of this principle, and will know where to find directions in nature for the true stating thereof. This I only hint at present to such as have ability and opportunity of prosecuting this enquiry, and are not wanting of industry for observing and calculating, wishing heartily such may be found, having myself many other things in hand, which I would first complete, and therefore cannot so well attend to it. But this I do not promise the undertaker, that he will find all the great motions of the world to be influenced by this principle, and that the true understanding thereof will be the true perfection of astronomy."

This passage (though much misunderstood by Delambre,) is extremely interesting, both as showing how nearly Hooke approached to a correct conception of the truth so soon afterwards developed by Newton, and also as giving his own confession as to the points in which he had failed in gaining possession of the whole truth.

Dr. Wallis, in 1666, also threw out some ideas of the same kind, and remarked, " to the objection that it appears not how two bodies that have no tie can have one common centre of gravity, I shall only answer, that it is harder to show *how* they have it than *that* they have it."

In 1683, sir C. Wren stated that, several years before, he had endeavoured to explain the planetary motions " by the composition of a descent towards the sun, and an impressed motion, but he at length gave it over, not finding the means of doing it."

Establishment of Observatories.

To the period under review belongs the establishment of some of the most celebrated national observatories. If the formation of philosophic societies be a matter of national importance, much more must the institution of observatories be so. To be effective to the great purposes for which they are destined, these establishments must be constructed and fitted up upon a scale far beyond the means of any individual to attain. And the character of the results deduced from the observations made in them, so essential to the purposes of navigation (and in this respect alone, therefore, of immediate national utility), ought to be such as can hardly be secured, unless that responsibility attach to the observer which belongs to a public functionary. But, in a higher point of view, the science which has the heavenly bodies for its objects of contemplation, demands, as far as possible, to be exempted from the vicissitudes of sublunary things. As it is a science which gains strength but slowly, and requires ages to complete its discoveries, the plan of observation must not be limited by the life of the individual who pursues it, but must be followed out upon the same system for a long succession of years. We have before noticed the munificence displayed by several sovereigns of former ages in this respect. The breaking up of Tycho Brahe's observatory, on which such vast sums had been spent, and which might have continued to be more gloriously employed than it was even under his superintendance, became a sad memorial of the instability of whatever depends on individual greatness, and the necessity of erecting such institutions on the more substantial basis of public endowments. Both the English and French governments had the wisdom to see this, and the national observatory of Greenwich was established in 1675, that of Paris in 1667. In the former, Flamstead and Halley, in the latter, La Hire and Cassini, are at the head of long lists of illustrious names who have fully evinced the wisdom of national liberality in the maintenance of these important institutions. If there be in the British empire any establishment, in the conduct and success of which the nation has reason to boast, it is the Royal Observatory. In spite of a climate which so continually tries the patience, and so often disappoints the hopes of the astronomer, this single observatory has furnished a longer continued series of observations, upon a systematic plan, and those more completely to be relied on, than all the rest of Europe together. Its determinations have afforded the data for the extensive researches, and the nicest tests for the theory, of physical astronomy; and have furnished the basis for the computation of those elaborate tables, by which the mathematicians of Europe have expressed with such surprising accuracy the past, the present, and the future condition of the heavens, and which, in their highest degree of refined accuracy, are essentially necessary to the commonest operations of navigation.

Figure and Magnitude of the Earth.

We have before noticed the rude attempts made by the ancients towards determining the magnitude of the earth; attempts characteristic of the genius of ancient science. The mathematical principle on which they were founded was perfect and exact, and is, in fact, the same as that now adopted: but how to apply the principle practically, was a point of very secondary consideration with them. Habituated to the abstractions of pure geometry, the problem was considered solved if it could be shown that we had the power of solving it; but the actual and practical exhibition of the result was of inferior moment. The slowness with which the art of observation was matured, and the great distance at which it kept behind theory, is a remarkable fact in the history of physical science. It has been well remarked, that mathematicians had found out the area of the circle, and calculated its circumference to a hundred places of decimals, before artists had divided an arc into minutes; and that many excellent treatises had been written on the properties of curves before a straight line had been drawn of any considerable length, or measured with any tolerable exactness, on the surface of the globe.*

The first attempt to renew such measures in modern times, was that of Snell, professor of mathematics at Leyden (already mentioned as the discoverer of the great law of refraction), described in a work called "Eratosthenes Batavus," 1617. The operations were carried on by a series of triangles, and the result, after the correction of some errors, has been found to differ but little from subsequent measures.

Another attempt was made by Norwood in 1635. He had observed the differences of latitude of London and York, and then measured the distance, though in a singularly rough manner, by measuring along the high road: "Sometimes," he says, "I measured, sometimes I paced; and I believe I am within a scantling of the truth." This affords a characteristic record of the infancy of observation.

Fernel, a French physician, soon after measured, with a similar object, the distance from Paris to Amiens, by means of the revolutions of a wheel; they lie nearly under the same meridian, and his resulting length of a degree was not far short of the truth.

But investigations like these, it must be evident, could not satisfy the increasing demands of science. The French Academy of Sciences became interested in the question, and an elaborate measurement was undertaken under its auspices, by the abbé Picard, which afforded the first result on which any reliance could be placed.

All these measurements had been employed as furnishing the data for determining the magnitude of the

* Edin. Review, v. 391.

globe. From the length of one degree in miles, the length of the whole circumference was deducible, and thence, likewise, that of the earth's diameter; the earth, of course, being supposed to be a *perfect sphere:* this supposition had been generally adopted, and seemed almost identified in the minds of men with the truth that the world was *round.* There were, certainly, some phenomena, such as the form of the earth's shadow in an eclipse, from which this opinion might have obtained more powerful support. Still, however, there was no proof of its being an *exact sphere;* and an incident now occurred which gave ground for the first suspicion that this was not the case.

In 1671, M. Richer had been sent out to make certain astronomical observations in the equatorial regions. At the island of Cayenne, on the coast of South America, it was found that an excellent clock, whose pendulum vibrated seconds at Paris, lost two minutes and a half daily; or, in other words, the pendulum required to be shortened to make it keep true time. The same thing was observed by Varin and Deshayes, who, some years afterwards, visited different places on the coast of Africa and America near the equator; yet the clocks, on being brought back to northern latitudes, regained their original rates.

We have said that this circumstance occasioned the first suspicion that the figure of the earth differed from a perfect sphere; but this inference was not made at the time: it remained as a very singular and perplexing fact, until Huyghens, in 1690, gave an explanation, very imperfect, indeed, but dependent upon the influence of the same class of physical causes as that suggested by the discoveries of Newton, which had been published in England before that date, but not yet received on the Continent. It is to our great philosopher that we owe the establishment of the connection between the real figure of the earth, and the length of the seconds' pendulum in different latitudes. In his researches, also, the importance of an accurate estimate of the magnitude of the earth is first clearly shown.

Astronomy in England.

John Flamstead had already distinguished himself by two small but excellent treatises on the "Equation of Time," and on the "Lunar Theory." The subject of the first of these was little understood by astronomers, till he thus illustrated it. As soon as he was appointed to the Greenwich observatory, he devoted himself, with the utmost zeal and ardour, to his duties. The first of his long-continued labours is seen in his great work, "Historia Cælestis Britannica," containing a vast mass of observations, and an extensive and accurate catalogue of the fixed stars. He also constructed a celestial atlas on a large scale, improving greatly upon that of Bayer. He was born in 1646, and died in 1719.

His successor in the observatory, Halley, commenced his brilliant scientific career at an early age. His first work was that of observation of the stars in the southern hemisphere, for which purpose he undertook a voyage to St. Helena in 1676. He resided there a year; but the climate was singularly ill adapted to astronomical observations: nevertheless, so great was his ardour, and so indefatigable his industry, that he succeeded in making a survey, though necessarily very imperfect. He also observed a transit of Mercury over the sun's disk: this rare and interesting phenomenon was connected with one of Halley's most happy speculations, and which has gained him most deserved credit, —his method of determining, by observations of these phenomena, those important elements of the solar system, the parallaxes of the planets. We must briefly explain it. Parallax, in general, signifies the change in apparent position of an object, owing to a real change of position in the observer. To two observers, one on the surface of the earth, the other at its centre (supposing it of course transparent), the sun would be referred to two different points in the sky. The arc, or angle which measures this difference, is called the sun's parallax: it is the same thing, in other words, as the angle which the earth's radius would subtend if seen from the sun. This evidently depends upon two things, the actual length of the earth's radius, and its distance from the sun. In like manner, the parallax of a planet is the angle subtended by the earth's radius at that planet. The radius of the earth being known by the actual measurement of the length of a degree on its circumference, of course the knowledge of the parallax of any heavenly body gives us its distance: hence one important use of the determinations of these parallaxes.

A method, remarkable for its simplicity and elegance, occurred to Halley, upon considering the phenomenon of the transit of Mercury over the sun's disc, which he observed while at St. Helena. Practical difficulties, indeed, hinder its application in the case of Mercury, but, in the transits of Venus, it becomes a most invaluable resource to the astronomer. The method will be easily understood after what we have said above. Venus, when between us and the sun, is nearer to us, and of course has a greater parallax. An observer, at one part of the earth, sees the dark image of the planet move across the sun's disc, cutting off, perhaps, a very small segment of it, and the time of passing is accurately noted. Another observer at the same moment, at an opposite or distant part of the world, refers the planet to a different part of the sun's disc, owing to its parallax and the parallax of the sun jointly; so that he sees it describe another track parallel to the former, but cutting off, perhaps, a very large segment, and notes the time of passing. From these different times of passage, it is easy to calculate the *difference* of the parallaxes of the sun and Venus.

The periodic times of Venus and of the sun (*i. e.* properly the earth) are known accurately from observation; by Kepler's law, the *ratios* of their mean distances are thence deduced; and, by consequence, the *ratios* of their parallaxes. This of course assumes the truth of Kepler's law; but (as in almost all astronomical reasoning) the approximate values are assumed as a basis on which to obtain the more accurate. Now, when we have given the *difference* of two quantities and their *ratio*, it requires only the solution of a simple equation to find the *actual quantities* themselves. In this way, then, the absolute parallaxes of the sun and Venus may be found, and, consequently, their actual distances: hence those of the other planets follow by means of Kepler's law.

Such was the idea started by Halley. From this one phenomenon he proposed to conclude the dimensions of all the planetary orbits; and the observation in question is of a kind admitting of the highest degree of precision. The phenomenon, indeed, is one of very rare occurrence. Halley, however, earnestly recommended the careful observation of them. The next which could happen was calculated for 1761; and, as he could not expect to live to witness it, he addressed to succeeding astronomers an eloquent and even affecting admonition, not to suffer so precious an occasion to pass unheeded, but to unite all their efforts to make, and procure to be made, observations at remote stations on the earth. We shall afterwards see that his exhortation was not made in vain.*

Both before and after his succession to the observatory, (in the year 1720,) the theory of the moon attracted, in a peculiar degree, the attention of Halley. He introduced several improvements in the details, and made some suggestions for perfecting the lunar tables; into these we cannot here enter, though they were of great importance as affording the *practical* means for finding the longitude by the lunar motions. We will only proceed to describe his important discovery of a fact, in regard to the moon's motion, which no previous astronomer had suspected.

* Phil. Trans. 1691 and 1716.

It had hitherto been the received doctrine that all the planets were subject to such inequalities only as are renewed within a certain space of time, and which, on that account, are called periodic inequalities. The mean motion is determined by a comparison of the planets' places at very distant times, embracing a great number of the periods within which the inequalities are renewed, so that the result obtained is quite independent of these inequalities. No astronomer had hitherto ventured to doubt the uniformity of these mean motions: and, in fact, this has been found correct for the primary planets. But this is not the case with the moon. The mean motion of this satellite is continually, though very slowly, accelerated; and this, from not being subject to periodical changes, is called the *secular acceleration*: though, strictly speaking, it has been shown to have a *period* of great length. It was sufficiently established by Halley, and has been confirmed by subsequent observations. Of his speculations on comets we must speak in a future section.

PART III.

THE PROGRESS OF PHYSICAL AND MATHEMATICAL SCIENCE FROM THE TIME OF NEWTON TO THE PRESENT DAY.

THE period of scientific history, which we have surveyed in the preceding section, has presented us with a varied and busy scene, which, in every department, has exhibited great advances. In the first instance, the master spirits, both of Galileo opening the path of experimental research, and of Bacon indicating the route to be followed; — in the next, the increasing phalanx of their disciples, prosecuting, with renewed ardour, the varied objects to which research had been directed: and their labours, whether individual or combined, all, in their several ways, co-operating to the discovery of fresh sources of information, and the opening of new avenues to truth. In the results of their enquiries, we have noticed certain infallible indications, not merely in one, but in several branches of science at the same time, of the approaches which were constantly being made to certain boundaries, which seemed to oppose themselves as barriers to the further prosecution of research, as yet insurmountable, even to those energetic minds and active powers of genius, which characterised so many distinguished philosophers towards the middle of the seventeenth century. Every thing seemed to indicate, either that the human mind had brought its inventions to that point, where, from the finite extent of its powers, they must find an impassable limit, and where a boundary must be placed beyond which the most exalted genius may in vain strive to penetrate, or that science was on the very threshold of those mysteries of nature, into which there only wanted some highly fav ured and privileged guide to give her admission.

We have now to contemplate the grateful and cheering spectacle of the fulfilment of the latter anticipation.

Our first section will be occupied with the discoveries of Newton, and some general account of his system. We shall proceed in the second to review the labours of his successors, who, by gradual improvements upon his discoveries and extensions of his principles, have brought the physical sciences to their existing state of advancement.

SECTION I.

THE DISCOVERIES OF NEWTON.

His early Progress.

In the same year as the death of Galileo, the birth of Isaac Newton took place, on December 25th, 1642, in the small manor-house, of which his family were the hereditary owners, though in humble circumstances, at Woolsthorpe, in Lincolnshire. The place is religiously preserved to this day. Every thing relating to the life of so great a man acquires an extraordinary interest, but as our object here is distinct from that of personal biography, we must pass over many points of this nature, which we could willingly enlarge upon, and confine ourselves solely to those which have an immediate reference to his philosophical progress, and to his history as identified with that of science.

In delicate health from his birth, he excelled little in the ordinary sports, or even studies of boyhood, but the bent of his genius showed itself in the pursuits of practical mechanics. While his companions were flying kites, he was occupied in investigating the best forms which could be given them, and the most advantageous point for attaching the string; and his ingenuity was displayed in a variety of contrivances, such as models of machinery, sun-dials, and a water-clock, constructed at an early age. At a later period, his attention was more absorbed by books, and he is described as "a sober, silent, thinking lad:" but he does not seem to have given his mind to mathematical studies till he had commenced his residence at Cambridge, where he was admitted at Trinity College, in 1660, under the tuition of Dr. Barrow.

Commencing his studies with the "Elements of Euclid," he is said to have taken in the whole, as it were, by intuition, and thence proceeded immediately to "Des Cartes' Geometry." This, together with "Wallis's Arithmetic of Infinites," and "Kepler's Optics," formed his earliest mathematical course of study: and no doubt the same powerful mind which could connect, under a single point of view, the theorems of elementary geometry, would soon digest into some uniformity of method the valuable materials supplied by the last-mentioned works; and the germs of his future mathematical discoveries were very probably sown in the perusal of these and other writings of that age, in which so many near approaches had been made, as it were, on every side to the essential principle, of which it was reserved for him to gain possession.

Analysis of Light.

In 1664, Newton has put it on record that he purchased a prism "to try the celebrated phenomenon of colours." But intimately as he was acquainted with Dr. Barrow, and consulted, as we know he was, by that distinguished man, upon the publication of his "Theory of Colours" (to which we have already alluded) in his lectures in 1668, Newton could not, at that date, have arrived at any decisive results himself, or he certainly would not have allowed his friend to publish so faulty a theory without remark. But it appears that in the very next year he had performed his principal experiments. He alludes to his result, as already obtained, without distinctly stating what it was, in a letter, dated February, 1669.

The steps by which his investigation proceeded are clearly traced. At the same time that he was trying the phenomenon of colours, he was also attempting to grind lenses of those particular forms to which we have alluded, as recommended by Des Cartes, for converging light *accurately* to a single focal point. Besides the difficulties attending this process, Newton was soon led to perceive that, even supposing the form of the glass ever so accurate, there is an inherent defect arising from the production of colour. This, indeed, in spherical lenses, had already been noticed: but its laws had never been investigated; and it seems probable that, in the case of the Cartesian lenses, the mathematical condition of accurate convergence which they ought to fulfil, was what led Newton to the probable connection between refraction and the production of colours, and suggested the hint of differently coloured primary rays, each undergoing a different degree of refraction.

The phencmenon then appeared in lenses; but in the prism it would be found in a state of greater developement: this, then, was the "instantia ostensiva" of Bacon; and this was the case which Newton accordingly selected to reason upon. His whole series of experiments is characterised by the most admirable exemplifiation of philosophical caution. A narrow beam of the sun's light admitted into a darkened room, and passed through a prism, exhibited on a screen opposite, not a circular image of the sun, as it ought to do, if the rays all obeyed the same law of refraction, but an image elongated to nearly five times its breadth, and coloured from one end to the other by a succession of vivid tints passing indistinguishably one into another, but of which Newton marked seven principal divisions: in this particular point he was guided by some distinctions, which probably appeared well marked to his eye, but which certainly would not occur to an indifferent observer, unless prepossessed with that idea. Most individuals, probably, seeing the spectrum for the first time, would rather distinguish it into three principal tints, red, yellow, and blue, with intermediate shades. To return, however, to Newton: the first question as to the cause which suggested itself was, whether the difference of colour might not be owing to the difference of thickness of the glass through which the different rays had to pass. This was accordingly put to the test, by causing two rays to pass, one through the thick part of the prism, another through a part near the edge; but each ray produced a complete spectrum of its own: this, therefore, could not be the real cause. The next suggestion was, that the effect might be due to irregularities in the glass; but this must take place equally in all positions of the prism, and, in fact, be doubled, if the light passed through two prisms; accordingly, two prisms of exactly the same angle were placed together, one being inverted, so that they together formed a solid with parallel surfaces, the light passing through both, came out unaltered, uncoloured, and giving a perfect undistorted image of the sun: this supposition was, therefore, rejected. But the rays from the different parts of the sun's disk form a small angle with each other, and their incidence on the prism is, therefore, slightly different; might not this be magnified in the course of two refractions, and so account for the effect? This was simply matter of calculation and measurement, and was shown to be utterly insufficient.

Newton next enquired whether the rays might not be bent into curve lines, after passing through the prism, and so, in proportion to the degree of inflection, might fall on different parts of the screen, with different degrees of obliquity. This was refuted by measuring the length of the spectrum at different distances from the prism: it was always in exact proportion to the distance, and the rays consequently strictly rectilinear.

Having disposed of all these suppositions, the question was reduced to narrower limits; a perfect exemplification of the process recommended by Bacon. Newton now thought of trying the actual properties possessed by each ray separately. Through a hole in the screen, any one ray could be transmitted, while the rest were stopped. The transmitted ray was subjected to further experiments by being again refracted through a second prism. The rays were found to be refrangible by this second prism, in different degrees; the violet most, the red least; precisely, that is, in the same order as by the first prism in forming the elongated spectrum. This was the "experimentum crucis;" and Newton came to the conclusion that the sun's light is not homogeneous, but is a compound of a number of primary rays, which (distinguished according to the order of spaces, reckoning from one end of the spectrum to the other) have each a different degree of refrangibility corresponding to the difference of colour, and that the same power of refrangibility is inherent in the same ray, or part of the spectrum, whatever subseqent modifications it may be made to undergo.

Reflecting Telescope.

Having thus investigated the main fact, Newton immediately recurred to the practical applications, with reference to which he had originally undertaken the enquiry. An accurate refraction in lenses appeared now not only practically difficult, but demonstrably impossible; and having completely satified himself, that in all the primary rays, the simple law of *reflection* holds good with perfect accuracy, he devoted his attention to the improvement of *reflecting* telescopes, which he saw were thus theoretically unlimited in the perfection which might be given them, if only perfect figures could be practically given to the specula. In considering Gregory's construction, it occurred to him, that instead of reflecting the image back again through a hole in the large mirror, it would be more convenient to place a small plane reflector diagonally within the tube, and so throw the rays out through a hole in the side, where the eye glass might be fixed. He accordingly constructed such an instrument with his own hands: and though it was only six inches long, it bore a magnifying power of about 40 times; and this, he remarked, was more than any refracting telescope, as then made, of six feet could do: he represented this as "an epitome of what might be done." And it is in the same letter (before referred to) in which he describes this telescope to his friend, Mr. Ent, that he alludes to his discovery of the composition of light, observing that a refracting telescope, if made according to the most perfect theory of those curves for lenses which Des Cartes had devised, would still scarcely perform better than a common telescope. This, he adds, may seem a paradoxical assertion, "yet it is the necessary consequence of some experiments which I have made concerning the nature of light." Still the telescope appeared to be the subject predominant in his mind. And it seems probable, that although Gregory's construction was proposed earlier, Newton's was the first actually made. He soon completed another of somewhat larger dimensions, which being, by request of the Royal Society, sent up to them for examination, was presented to them by the maker, in 1671, and soon after exhibited to the king. A particular account of it was transmitted to Huyghens, and it has been carefully preserved by the Society to this day. At the same time they elected Newton a fellow.

In 1672, Newton was occupied in the construction of a reflecting microscope, upon a principle analogous to that of the reflecting telescope. About the same time, he also proposed a plan for improving his telescope, by diminishing the loss of light at the second reflection. This was done by employing, instead of the small speculum, the total internal reflection from the hypothenusal side of a right-angled prism: the light entering perpendicularly on one of the rectangular sides, being reflected at half right angles, and emerging at the other, without refraction, was thrown out to the eye glass in the side of the tube, as in the former construction.

Publication of Optical Experiments.

Having been, in 1669, appointed Lucasian professor of mathematics, on the resignation of Dr. Barrow, in that and the two following years, Newton read some lectures at Cambridge, containing an account of his researches on the unequal refrangibility of light; but they do not seem to have become much known till some time after. In a letter to Oldenburgh, secretary to the Royal Society, in 1671, he mentions his intention of communicating to that body "an account of a philosophical discovery, which induced me to the making of the telescope: and I doubt not but will prove much more grateful than the communication of that instrument; being, in my judgment, the oddest, if not the most considerable detection which hath hitherto been made in the operations of nature." The communication followed soon after, giving an account of the principal experiments already described. It was received with all those marks of approbation and honour to which such a production was justly entitled: and he soon after followed it up by a second, in which some further researches on the same subject were detailed.

These researches were, in the first instance, directed to the reverse experiment of recompounding the prismatic rays into white light. This was done either by a second prism inverted, or by uniting the rays at the focus of a lens, or by a mixture of coloured powders, in the proper proportion, which, when illuminated by the sun's rays, and compared with a white paper, under the same circumstances, appeared of precisely the same whiteness. He showed also that the colours of all bodies are dependent wholly on the light reflected from their surfaces; since whatever be the original colour of a body, if no other light fall on it except one of the prismatic rays, it will appear wholly of the colour of that ray. These papers soon acquired for their author an extended reputation: his discoveries became known on the Continent, and, though in general due justice was done them, yet they soon had to withstand attacks from ignorant or prejudiced antagonists, both abroad and at home.

Attacks on the Optical Experiments.

Pardies, professor of mathematics at Clermont, displayed his ignorance by starting the very difficulty which Newton had so cautiously examined, before coming to his conclusion, arising from the angular magnitude of the sun; and when this error was pointed out, persisted in running into others still more frivolous.

Linus, a physician at Liege, affirmed that the phenomenon was due to some reflection of the sun's light from a cloud, and denounced the law of unequal refrangibility as impossible; adding a number of absurd cavils at Newton's mode of conducting his experiments.

Newton, for some time, declined answering any of these objections; but at length, at the earnest request of Oldenburgh, he was prevailed upon to draw up a reply. Marriotte and others made objections, because they could not succeed in repeating the experiments. Desaguliers showed evidently that this was merely owing to the want of proper precautions.

Gascoigne, a friend of Linus, next took up the subject, but mistook for the real spectrum that formed by reflection within the prism, which is coloured, if the prism be not equilateral. Lucas, a friend of the same parties, at Liege, brought forward the only real and substantial difficulty, by observing that the spectrum formed by *his prism,* though in all other respects similar to Newton's, was *not nearly so much elongated* as Newton had found it.

The discussion of this objection was remarkable, from the positiveness with which each party maintained the accuracy of their respective results. And it is still more singular, that a philosopher of Newton's extreme caution in drawing conclusions, should, throughout, have taken for granted, and even positively affirmed, what certainly was not proved, viz. that all kinds of glass were equal in their power of separating the rays ; or that a medium, which has a greater power of refracting a white or compound ray, would necessarily have a *greater difference* in the degree in which it refracted the different primary rays. Yet such was the case, and this difference, which exists to a great amount among transparent bodies, remained undetected as a general philosophical truth till a much later period. Lucas's prism was of a different sort of glass from Newton's, having a lower separating or "*dispersive*" power.

Newton had no sooner ended his dispute with these experimentalists, than he had to encounter an attack from philosophers of more formidable powers. The first of these was Hooke: a man who, from what we have already seen of his attainments and disposition, would be likely to be an antagonist as unpleasant as powerful. In the present instance it was chiefly in the former light that he exhibited himself. His inordinate love of fame had, in fact, led him into a number of speculations on light, in which he imagined he should be able to make far greater advances than had yet been attempted. Accordingly, when Newton produced his actually complete labours, — the reflecting telescope and the analysis of light, — Hooke, mortified at being out-done, took occasion, in the first instance, to criticise the telescope with unbecoming severity, and at the same time announced that he possessed an infallible method of perfecting all kinds of optical instruments, so that "whatever, almost, hath been in notion and imagination, or desired in optics, may be performed with great facility and truth." What this great secret was, has never appeared. But in the next place, he proceeded to examine Newton's prismatic experiments, and was constrained to confess their accuracy. It was, however, the conclusion from them which he combatted, as at variance with the theory of undulations which he had adopted. The unequal refrangibility has, indeed, been held as an objection to that theory, down to much later times; but it is not certain that the theory of Hooke was the same as that of Huyghens. He attempted to make out a theory of the colours which should agree with his hypothesis; but the explanation which he gave, was not more wide of all experimental conclusions, than of any hypothesis connected with undulations which would be now considered worth listening to. His arguments were chiefly directed against the doctrine which makes light consist in an emission of material particles, endued with inconceivably great velocity, and to which Newton had inclined. And he contended, in accordance with his own views, that there were only two primary colours — yellow and blue.

Newton gave a dignified refutation to arguments so little deserving it; and replied to all such speculations, that they in no way affected his experimental conclusions, which ought to be regarded as wholly independent of any theory of the nature of light. He exposed the entire fallacy of supposing only two primary colours. Huyghens had been one of the first to recognise the value of Newton's optical discoveries; and in a letter he speaks of them as truths, "in comparison with which, all that have yet been made known are altogether empty and puerile." Yet, fearing probably for his theory of undulations, he soon after communicated to Oldenburgh some objections which had occurred to him, and endeavoured to make out a theory of the origin of the colours, more accordant with the principles he had adopted. It appears, however, to have been a speculation altogether unworthy of his high ability; and Newton replied to it in a temperate, but thoroughly convincing manner. It does not seem that this correspondence occasioned any breach of friendship between these two illustrious men. But yet, such was the morbid dread which Newton entertained for any sort of controversy, that he felt annoyed by these disputes, in a peculiarly acute manner. In his letter to Oldenburgh, containing his first reply to

Huyghens, he expresses strongly his dislike at these interruptions of his tranquillity, and even adds, "I intend to be no further solicitous about matters of philosophy." In a subsequent letter, in 1675, he speaks of some further researches he had intended to communicate to the Society, but adds, "I find it yet against the grain, to put pen to paper any more on that subject." And to his illustrious contemporary, Leibnitz, he expressly says, "I was so persecuted with discussions arising from the publication of my theory of light, that I blamed my own imprudence for parting with so substantial a blessing as my quiet, to run after a shadow."

Periodical Colours, &c.

In December, 1675, Newton sent to the Royal Society "A Discourse on Light and Colours," which, besides containing complete details of the various experiments on the decomposition and recomposition of light, fortifying the whole doctrine against all conceivable objections, concluded with an account of his experiments on the *colours of thin plates*, and their connection with the colours of natural bodies.

We have already described the general nature of this phenomenon. The principal obstacle in the way of an explanation, was the difficulty of obtaining any estimate of such extremely small thicknesses. But Newton overcame this difficulty with his usual sagacity and accuracy. He formed a thin plate of air, by pressing together two lenses, of very large but slightly different radii of curvature. These radii being known, it was a matter of calculation to estimate the intervals by which the two surfaces were separated at different distances from the central point, where they were in contact. The colours (as before noticed) were formed in the shape of rings round the central spot. The diameters of these rings could be easily measured; and Newton, having performed such measurement with most scrupulous accuracy, deduced, by an easy process, *the thickness of the plate of air corresponding to any given tint*.

The colours were neither the same, nor in the same order as those of the spectrum: they went through various successions of tints, commencing from the centre, which was black; and this order of tints has been since known by the distinctive name of Newton's scale. The whole, at first sight, appears complex; but when analysed by Newton's penetrating skill, it instantly assumed the utmost simplicity of arrangement. If the light were perfectly homogeneous, —red rays, for example,— the appearance was that of red rings, with dark intervals; if blue rays were employed, the rings were blue, with dark intervals, — *but of contracted dimensions*: — intermediate rays gave rings of intermediate size. The result then was obvious: sets of such rings *superposed*, would give a series of compound tints in no two parts exactly alike, and at no point exhibiting any simple prismatic colour.

By an analysis of this kind Newton obtained very precise values for the thicknesses at which any simple ray (red, for example) gave a bright or a dark space. This appeared to depend on nothing but the thickness. At the centre, where absolute contact might be supposed to take place, darkness was produced: at a thickness a little greater, a bright space: a little greater thickness gave a dark space again: a little greater, bright; and so on alternately, till after a certain thickness the alternations became too close and too faint to be visible; but there seemed no assignable limit.

What then was the nature of the phenomenon? it seemed the natural conclusion to say that, where the dark parts appeared no light was reflected, whilst at the bright parts reflection took place, and this was somehow dependent on the thickness.

But there was another part of the appearance yet to be examined: when the light was *transmitted* through two lenses thus combined, rings were also seen; but they were exactly complementary to those seen by reflection ; the centre spot here was bright, and the first ring about it dark. In other words, *at those thicknesses where the light was not reflected, it appeared to be transmitted :* every thing then depended upon the differences of these thicknesses. The red ray, for example, would be reflected or transmitted, according as it fell upon a part of the plate of air, or upon another part near it whose thickness differed by a certain extremely minute, but assignable quantity. The ray then penetrating the thin plate of air, and arriving at its lower surface, we will suppose was transmitted : another ray penetrating a thickness, greater by that extremely minute quantity, was not transmitted, but reflected back to the eye. There was then something in the nature of the ray which allowed it to be transmitted at the one point, and something different in its nature at a point in its length distant from the last by the extremely small interval mentioned, which prevented it being transmitted, and obliged it to be reflected : and this difference recurred perpetually at the same intervals.

Such was Newton's train of reasoning : he inferred that it was a new, peculiar, inherent property of light, altogether *sui generis ;* and to express these alternations of state or nature at those successive intervals, he named them "*fits of easy transmission or reflection ;*" their length was matter of calculation ; they were greatest for the red rays, least for the violet, and of intermediate values for the others.

The whole investigation, perhaps, more than any other, from the extreme apparent complexity of the phenomenon, displays to the greatest advantage the surprising powers of investigation with which Newton was gifted : whether we regard the simple analysis of the colours, the precision of the measurements, or the sagacity which deduced the mode in which the effect may be supposed to take place, we are equally lost in admiration.

The subject has since been largely discussed. Newton's measurements have retained their credit for superior accuracy among all the more refined improvements of modern times. But there is one part of the question to which we may here properly allude, which is important in reference to the philosophical character of the reasoning: viz. how far the existence of these *fits* can be regarded as simply a mode of stating the *facts*, or how far it involves any *hypothesis*. This question had been long keenly agitated before the disputants perceived the real point in the discussion to which their attention ought to have been directed for resolving it: that is, whether, of all the light falling on the plate of air, a *part only*, or the *whole*, is transmitted or reflected; and again, whether the *whole* effect is produced at the *second* surface of the plate, or whether the first is not in any way concerned.

Of these questions, it might be said, that no means appeared of obtaining any experimental answer to them: yet, as to both, Newton had certainly made *assumptions* without positive proof. Those assumptions then gave the conclusion the character of being somewhat more than a bare experimental result or statement of facts; but this was not perceived till very lately.

These considerations, however, in no degree affect the character of the experimental results, nor invalidate the precision of Newton's determinations of the lengths of those *intervals*, by whatever name they may be called, which occur, marked by some sort of peculiar alternation of character, along the length of a ray of light. The extreme minuteness of these intervals, as well as the very peculiar nature of such a property of light, increases our admiration at the genius which could succeed in thus detecting and measuring it.

The colours produced in thin plates of air were also found to be produced in other media, as in liquids, by allowing a drop to insinuate itself between the glasses. But Newton observed, that the liquid being of greater refractive power, the rings contracted: or, in other words, the same tint was produced at a less thickness; and this precisely in proportion to the refractive power:

he hence inferred that the length of the *fits* or *intervals* was diminished when the light entered a more refracting medium.

Another phenomenon, somewhat analagous, was likewise observed by Newton, and referred to a similar principle; this he called the colours of *thick plates.* It may be stated thus: light being transmitted through a small hole in a screen so as to be incident on a spherical concave reflector of glass with concentric surfaces, the back being silvered, and the aperture situated at the centre of the spherical surfaces, on the screen surrounding it were seen *coloured rings;* or in homogeneous light alternate bright and dark circles. They became faint and disappeared if the distance of the screen were increased or diminished beyond a small difference from the original position. They diminished in diameter as a *thicker* glass was used. The reflection from the *back* of the mirror was essential, as they were rendered faint if the silvering was removed, and disappeared altogether if a substance nearly equal to glass in refractive power was applied: they were also not produced in metallic specula.

We shall not follow this great philosopher into his various speculations connected with this subject;—his theory of the colours of natural bodies, which was dependent on those of thin plates; or his conjectures respecting the relations of the optical and other properties of bodies. We will merely mention his singularly prophetic suggestion with regard to the composition of the diamond. He had been trying experiments by which he determined with great accuracy the refractive powers of a number of transparent substances, and concluded in general that those powers followed nearly the order of their densities. He found, however, a remarkable exception in what he called "unctuous and sulphureous," that is, highly inflammable bodies, which possess a much higher refractive power than accords with any relation to their densities. He also found that the diamond (apparently of so totally different a nature) had a similar peculiarity in its very high refractive power; hence he hazarded the conjecture (so completely verified by modern discoveries), that the diamond "is an unctuous substance coagulated."

It appears highly probable that, about the period of the researches to which we have alluded, Newton was engaged in repeating and varying the experiments of Grimaldi on the inflexion of light before mentioned, and most probably before Hooke had communicated to the Royal Society his experiments, which was in 1674. The account of these researches was not published till it appeared in the "Optics" in 1704; and he then speaks of them as investigations carried on long before, and put together out of scattered papers. His attention was directed entirely to the fringes formed externally along the edges of the shadows of bodies in light diverging from a single point; and having subjected these to very precise measurements, he framed a theory to explain the manner in which the rays might be inflected in passing by the edge of an opaque body. He also showed some curious forms which the fringes assume when opaque edges are made to approach each other, so as to form an aperture through which the light passes: in this case the effect is considerably complicated, especially when the sides of the aperture are inclined to each other, which was one case of which he has given a very particular account. He has also mentioned some other results, which are of a nature hardly intelligible, but which were left, as he tells us, in an unfinished state. "I designed," he says, "to repeat most of them with more care and exactness. ... But I was then interrupted and cannot now think of taking these things into consideration." We have occasion on all grounds to deplore the loss of his further examination of the subject, but on none more than the probable reason suggested by Sir J. Herschel;—"doubtless owing to the chagrin and opposition his optical discoveries produced to him; an unmeet reward, it must be allowed, for so noble a work, but one of which unhappily, the history of science affords but too many parallels." (*On Light.* Art. 742.)

In the "Optics," we have the complete theory of the rainbow, in which the author applies his discovery of the unequal but constant indices of refraction belonging to the different rays to complete what was wanting in Des Cartes' explanation of the phenomenon. To the same work also is appended that rich collection of philosophic hints and suggestions, under the title of queries, in which so many singular anticipations of after discoveries are found: nor is it any disparagement to the author, if in some of these he was less fortunate than in others. His observations on the double refraction of the Iceland crystal, in whichhe was misled by some inaccurate measures into conclusions at variance with the law so accurately established by Huyghens, must be classed among these. On the other hand, he distinctly threw out the suggestion, that under certain circumstances a ray of light has different properties on its different *sides*, which he referred to something analogous to *polarity* in its particles: an idea which, under a somewhat different aspect, has been recognised throughout an immense class of phenomena elicited by modern research.

We have here followed the connection of subject, rather than the order of time, in order to give, in one point of view, a general idea of Newton's optical discoveries: and it must on all hands be admitted, that scarcely a parallel case can be produced in the annals of science, where a single individual has at once originated the idea of reducing to simple experimental examination a class of phenomena, long known only as a subject of admiration or frivolous speculation, and carried that design into execution in the most perfect manner: whilst, again, the same scrutinising penetration has shown itself in detecting and measuring, with the nicest accuracy, certain minute phenomena from which a physical affection of light was deduced, of a kind not only antecedently unsupported by any sort of analogy or probability, but even such as the mind can hardly attempt to grasp by any satisfactory mode of conception, but which yet (as far as the most essential point of the result is concerned), has been confirmed as an inherent peculiarity in the nature of light, by the whole extent of modern research. The "Optics" is prefaced by a declaration, that its design is to explain the properties of light by means of experiment alone, without any admixture of hypothesis. In this respect alone it was quite a novelty in the age when it appeared: and when we compare the true philosophic spirit thus exhibited, with the extent, importance, and precision of the actual results, we shall be constrained to assign it a place in the first rank of experimental investigations, and to say that in that rank it stands alone.

Newton's Mathematical Discoveries.

In tracing the progress of those inventions which were directed to the object of overcoming the difficulties which elementary methods were incompetent to surmount, we have noticed the improvements successively made by some of the greatest mathematicians, which were chiefly called forth in the attempts to solve the problem of the quadrature of curvilinear areas, or the determination of an area equal to that of the curve expressed by some algebraical value in simple terms, derived from its equation. We have noticed the several principles which were suggested, but which received very limited applications; and the ingenious idea of Wallis, of interpolating in the series of known areas those which ought to hold an intermediate place.

Invention of the Methods of Series and Fluxions.

Newton, on taking up the subject, which, we have reason to believe, he did as early as 1665, soon extended the method of Wallis by the invention of general serieses for this purpose; and, at the same time, connected the process of such serieses with a highly refined method, which was, in like manner, an improvement upon the hints supplied in the inventions of his predecessors.

The grieses which he deduced, as applying generally to the quadrature of curves, were, as he tells us, founded on Wallis's principle; and, from comparing the forms of those already obtained, he discovered a law which was common to them all. In such of these forms as involved fractional exponents, he found the series infinite; though, wherever he could render it convergent, it would of course give a value to any requisite degree of approximation. This, however, was closely connected with an object of more extensive importance and utility, a theorem for expressing *any root of a quantity composed of two terms* in a series involving certain combinations of those terms, according to a given law, and which would be convergent. To such a theorem he was almost immediately led; and, as he instantly perceived that roots were comprised under the same algebraic formulas as powers, by the mere adoption of the idea of fractional indices, he saw that this theorem was no other than the most general form of that for raising a binomial quantity to any given power, in a series of terms involving the index and the whole powers of the two terms in a certain regular progression. It hence acquired the name of the *binomial theorem*.

This theorem, besides its other numberless applications, was directly employed in the quadrature of curves, as supplying a more direct method than that originally adopted, and out of which it had in fact originated. The whole course of the discovery is related in detail, in a letter to Oldenburgh; one of a valuable series to which we shall often have occasion to refer, collected under the name of "Commercium Epistolicum." The application, then, consisted mainly in this; that he reduced the value of the ordinate of a curve into an infinite series of the integer powers of the abscissa by the binomial theorem, or by any simpler algebraical process, where the case admitted it. After this, other considerations, like those employed in the method of indivisibles, would assign the small area corresponding to each term, and the sum of these would give the whole area required.

But such methods as those of indivisibles were operose, tedious, and unconnected by any very general principle. The fertility of Newton's genius soon supplied this part of the process also, and devised a method which, by the application of certain easy but highly general rules, gave the ready solution of all problems of this nature. A letter of Dr. Barrow, in the collection above referred to, in 1669, mentions, that some years before Newton had shown him a MS. treatise, but would not publish it, entitled "Analysis per æquationes numero terminorum infinitas," in which not only were such serieses employed, but the general connecting principle and method clearly pointed out, though certainly not given in precise formal rules, with a peculiar notation.

Newton first gave to the public an account, though a very brief one, of his method, in the second lemma of the second book of his Principia, in 1687; and the precise rules and notations were made known by Dr. Wallis, in the second volume of his works, in 1693, where he extracts an account of them from two letters of Newton, written in 1692. This method, then, which its inventor had called *fluxions*, was known for years only to his friends; but those friends soon included, by means of their correspondence, some of the first mathematicians of Europe. In the earlier stage of the investigation, however, the different serieses employed for quadratures were the principal objects of attention, and these were soon communicated to some of the most eminent geometers.

Leibnitz visited England in 1673; and, having formed an acquaintance with Oldenburgh, secretary to the Royal Society, continued a correspondence with him on his return to the Continent. At the time of his visit, he was but slightly conversant with mathematics; but his powerful mind grasped every subject: and, after his return, he was soon able to enter into those topics which were beginning to excite so much interest, and even in a condition to take a part in the discussion; and, in 1674, communicated to Oldenburgh some serieses of his own. In 1676, Newton, at the request of Oldenburgh, wrote an account of his method of quadratures, containing also his method of fluxions, concealed under an anagram. This was sent to Leibnitz; who, in 1677, replied to Oldenburgh by sending a short account of an equally general method which he had invented, which he called the *differential calculus,* and of which he gave the principal rules and notation. An account of it was first published in the "Acta Eruditorum," in 1684.

Thus, while Newton's method remained known only among his friends and correspondents, that of Leibnitz was publicly announced, and spreading rapidly on the Continent. Two most able coadjutors, the brothers John and James Bernoulli, joined their talents to those of the original inventor, and illustrated the new methods by the solution of a great variety of difficult and interesting problems. Such was the reserve of Newton, and so little were his methods known or followed up among his countrymen, that the first book which appeared in England on the new geometry (as it was called), was a treatise by Craig, professedly derived from the writings of Leibnitz and his disciples.

Thus far, then, the history of these important inventions is clear. Two valuable and general methods of analysis, applicable to the solution of the same classes of problems, though differing in their notation, and in the mode in which the first principle was conceived, were discovered, separately and independently, by Newton and Leibnitz, within a very short time of each other. Newton's invention was first in order of time, but not published to the world till long after. Leibnitz could not have derived his idea from that of Newton, that idea being concealed in cypher: he discovered it independently, though rather later in time, but was the first to publish it. Newton, in his Principia, gave testimony to the independence of Leibnitz's discovery, and expressed a favourable opinion on its merits. Leibnitz, moreover, seemed equally willing to admit the claim of Newton. Leibnitz, indeed, enjoyed the advantage of seeing his calculus rapidly improving, and extending its applications in the hands of his friends and disciples, whilst that of Newton remained in comparative obscurity. But still nothing like rivalry or hostility appeared between the parties. A few years later, we shall find this tranquillity strangely disturbed: for the present we must briefly recur to some general view of the nature of the discoveries in question, and then proceed to other subjects of discussion, of which so many, and all of such high interest, crowd upon us at this most eventful period of scientific history.

General Idea of the Fluxional Calculus.

What we have before said of the methods of infinitesimals, and the attempts at the problems of tangents and quadratures, will have sufficed to show how near an approach had been made to general principles. The method of serieses is in itself, perhaps, at once the most satisfactory and the most easily applicable, in cases where an exact solution is impossible: the essential idea is one which is practically familiar to the mind of every one who has even gone so far as to calculate by decimal fractions. The notion of a value carried on to any number of places of decimals, but which, strictly speaking, never terminates, though each successive figure we obtain brings us, in a tenfold degree, nearer the truth, and though we may approach nearer to the exact value than to be able to assign or even conceive the difference,—this notion, we repeat, is practically familiar to the mind of every computor. There is, then, nothing more than this in the principle of those applications of serieses to which we have referred. The object and nature of the series is simply to afford the means of expressing a succession of terms, each formed according to the same rule or law, but each (like the successive decimal figures), having a value rapidly decreasing; so that, by taking the sum of a certain number of them, we obtain the same kind of approximate value as that given by taking a certain number of places of decimals.

The labours of Wallis and Mercator, and the first efforts of Newton and Leibnitz, had been directed to the discovery of a variety of such serieses, for the different purposes of expressing those values which they could not obtain in simple and finite terms, but which gave the areas and other results of which they were in search. The grand point, however, was to connect all such values, whether exact or approximate, and the method of arriving at them, by a common principle and a common rule.

Now we have seen several methods successively started by Kepler, Cavalieri, and others, which were devised with the view of passing from elementary problems referring to finite quantities, to those more recondite properties which essentially involved the principle of limits; and, at the same time, contrived to obviate the excessive tediousness of the ancient methods: some of these methods approached very nearly to certain cases of fluxions, or differentials. But what distinguishes the invention of *the calculus* (to give it at once its generic designation), is the *generalisation* of such methods, and reducing the elementary idea to such a form, as to supply rules applying to all the objects before referred to, and connect together, by a common principle, the processes by which these objects were attained; and thus, not merely establishing those particular truths, but putting us in possession of a method of investigating all questions of the same kind, and opening the way to the solution of a vast number of others as yet unthought of.

This grand step was taken by the two great inventors of the calculus in very nearly the same manner, though in the mode of describing it a different language was employed.

The idea common to both, is that of considering all quantities expressed by some combination of algebraic terms which receive variable values, dependent solely on the value successively given to some one or more of the simple quantities involved, and which are supposed to vary according to a simple and uniform manner; the complex quantity, thus dependent on the simple quantity or quantities, is called a *function* of that *variable*, or those *variables*, if more than one. We shall then in future speak of a function of one or more variables, without risk of being misunderstood. According to the particular nature of the algebraic combination which constitutes the function, it will be easily seen that it will vary at very different rates, as compared with the variable, which we suppose to increase uniformly. To take a simple illustration: the square of a quantity is a function of it: as the quantity is supposed to increase uniformly, its square increases in a much more rapid ratio. Its logarithm is another function, but this increases at a totally different rate. Was it then possible to devise any general method of comparing the *rate of variation* of a function with the *uniform* variation of its variable? The rate of the function's increase was changing at every moment: it could never, therefore, be expressed by any constant finite ratio; but might it not be of such a nature, as to be susceptible of *a limit?* This was the essential point, and both the great inventors showed, in their several ways, that *the ratio was in all cases bounded, as it were, by a* LIMITING RATIO, *which they showed how to express in terms of the variable.* This, in whatever language or notation it might be couched, or in whatever way demonstrated or illustrated, was the great, essential principle, for which the world is indebted to the original unaided independent inventive powers of Newton and Leibnitz.

Neither of them, however, expressed the principle by an explicit reference to the simple notion of a *limit:* if they had, not only would all the mysticism and obscurity of the subject have been removed, but much serious misapprehension and bitter controversy would have been avoided. But the inventor of a great principle, conscious of the value and power of the idea which he has conceived, is not disposed to stop and enquire very closely into the exact methods by which it is best supported and proved, or even rendered intelligible: this was pre-eminently the case with Newton. The view he gave of it was just that which a master genius takes of a comprehensive subject: without entering into any niceties, he expressed the whole at once by a *bold metaphor*. The limit of the rate of the function's increase he called *the fluxion* of the *flowing quantity* or fluent, the actual value of the quantity, space, or other result generated at a given stage of the variation. He considered the *velocity* with which it flows, the space traversed in a given time, or which would be if the velocity were uniform: all these ideas, derived from mechanical considerations, could not, strictly speaking, have any place in a purely analytical investigation. Nevertheless, they afforded by no means an unapt illustration, and were even so precisely applicable by analogy, that that analogy was preserved even to the details of the proof: moreover, such a view of the matter applied directly to those particular subjects to which it was in the first instance mainly designed to apply: viz. the generation of curvilinear spaces, and, subsequently, the doctrines of mechanical force and motion.

Leibnitz had recourse to the consideration of the *infinitely small elements* simultaneously generated, of the function and the variable, of the abscissa and ordinate, &c.; between quantities infinitely small, and even between orders of these, each successively infinitely smaller than the preceding, he conceived finite ratios to subsist; — a method involving various metaphysical difficulties which have given rise to lengthened and complex discussion, but which is after all not essential to the subject, and belongs rather to the terms in which it has been couched than to the ideas signified by them. Leibnitz called these ratios of the infinitely small elements the *differential coefficients.*

There was, however, in any point of view, less difficulty in the first conception of the idea than in showing its generality, and in proving that it applied to all sorts of functions. Both its inventors gave a complete system of such rules. This constituted the *direct,* the *fluxional,* or the *differential* calculus: the process necessary for expressing the limiting ratio would be more or less long and difficult, according to the nature of the particular function ; but in all cases it could be applied without any inherent obstacle. There was, however, the *reverse process* to be considered and provided for ; that is, what is practically the most important, when expressions are given involving fluxions or differentials mixed up with other terms which are combinations of the variable, to find the quantity from which such expression would be derivable, as its fluxion or differential, according to the rules before laid down. This reverse process was in most cases, except a few which were obvious inversions of the preceding process, very difficult, and in a large number no method appeared of obtaining the result at all in any exact or finite terms. These, however, were the results actually wanted for all the most important applications ; here then the value of serieses was apparent; here those forms before investigated found their proper place in the system of analytical mathematics : wherever the expressions in question could be reduced into a series of simple terms, each of those terms separately could be easily made to give the quantity which by *differentiation* would have produced it, and the sum of all the terms, if a finite series, would give the exact value of the whole quantity generated, or an approximation to it if the series were infinite but yet convergent. This process was called the *inverse* calculus, or that of *fluents* or *integrals:* according to Leibnitz's view it gave the *sum* of all the small elementary parts ; hence the term *integration.*

The calculus then took its origin in the attempts to solve certain geometrical questions respecting curves, their tangents and normals, their rectification and quadrature. The incremental triangle conceived by Barrow was nothing else than a sort of geometrical picture of the limiting ratio of the increments, or the fluxions of the curve or tangent, its ordinate and abscissa. The general method, therefore, supplied at once the means of drawing tangents and normals to curves, the ratio which determined the tangent being simply the differential coefficient of the equation to the curve. This idea was soon generalised, and the contact of curves with other than rectilinear tangents was found to be expressed by an extension of the methods of the same calculus. The notion of circles which *osculated* with curves, or had the *same curvature* with them at different points in their periphery, was already familiar to the mind of the geometer: the new system afforded a more general mode of viewing the subject, and enabled the analyst to compare in this way successive orders of contact. Similar conclusions were deduced respecting curved surfaces and solid problems. But though the calculus was thus in its first design limited to such subjects, and borrowed much of its tone and character from those geometrical purposes to which it was subservient, yet it soon took a wider range, and was seen to apply to functions of the most comprehensive class. Des Cartes had provided the means of bringing geometry within the dominion of the calculus, by reducing it to algebraic equations. But to bring the relations of quantity in general under its power, and to furnish the means of considering functions of all kinds in such a way as to investigate their forms and modifications by means of the calculus, the further labours of analysts were yet demanded, and more general views required to be taken.

Some of the simpler and more important of these relations were, however, perceived and investigated at the earliest stage of the progress of the calculus. Such was the consideration of maxima and minima, to which, on a former occasion, we briefly referred. The idea of Newton was a singularly happy instance of the universal and exact applicability of his metaphor. When a function increases or decreases up to a certain point, and thence again decreases or increases, at that point it is a maximum or minimum. It is obvious that its *velocity* of increase or decrease varies up to that point where it is *absolutely nothing;* and then begins to increase again. Thus, by finding the value of the quantities involved which will make the fluxion become nothing, we find the maximum or minimum.

Another kind of maximum or minimum, abounding in problems of the highest interest, but far more difficult than the last mentioned, is that in which the function itself is required to be found which will be the greatest or least under certain conditions. One class of such problems we have before referred to, as known by the name of iso-perimetrical problems. But the investigation, in its more extended form, exercised the talents of the two Bernoullis, and has received from them, and from subsequent analysts, the most complete investigation.

In a sketch like the present, it is of course impossible to enter into any adequate account of the different branches into which the direct or inverse parts of the calculus extend themselves. It must suffice to say, that the latter, as it is the most difficult and important, so it is by far the most extensive. One very large and important class of problems which it has to consider, are those which involve two or more variables mixed up with their differentials in any manner of combination in an equation. The difficulty here is that of separating them, if possible, so that each variable shall stand combined with only its own differential; but the process is only capable of being effected in certain cases; and though extensive classes of such *differential equations,* as they are termed, have been solved, especially by the early labours of the two Bernoullis, and have subsequently occupied the researches of the most eminent mathematicians, yet even at the present day the subject is invested with difficulties which have not been overcome.

Progress of the Fluxional Methods.

Newton and the English geometers did very little towards perfecting this branch of analysis. The continental mathematicians, on the other hand, were zealously engaged in improving its methods and pushing forward its applications. Leibnitz published in the "Acta Eruditorum," and other journals, a number of papers full of original views and important hints, thrown out very briefly, and requiring the elucidations which his illustrious friends the Bernoullis and others were always so willing and able to supply. Their tracts, like his, were scattered in the different periodical works of that time; and several years elapsed before any complete treatise explained the general methods, and illustrated them by examples. The first book in which this was done, so far, at least, as concerned the differential or direct calculus, was the "Analyse des infiniment Petits" of the marquis de l'Hôpital, published in 1696, a work of great merit, and which did much to diffuse a knowledge of the calculus. The author was a man of considerable genius and indefatigable industry, and enjoyed the advantages of instructive intercourse with John Bernoulli. In the collection of the works of the latter (not published till 1724) is inserted a tract of some length on the integral calculus, written in 1691, as is expressly mentioned, for the use of M. de l'Hôpital, to whose work it would seem intended as a sequel.

Newton, besides his letters, inserted in the "Commercium Epistolicum," had written at an early period, as we before observed, the "Analysis per æquationes," &c. But this, together with another tract, was not published till 1711. The treatise on the quadrature of curves, though written in 1666, did not appear till 1704, when, together with the "Enumeratio linearum tertii ordinis," it was appended to the "Optics." The treatise on fluxions, translated by Colson, was not published till after the author's death (in 1736); and another analytical tract remained unpublished, till bishop Horsley collected and edited Newton's works, in 1779. The "Arithmetica Universalis" was published by Whiston, in 1707.

Such were the principal productions of Newton on subjects of pure mathematics. And it is certainly a curious fact, and one of much interest, as illustrative of the genius of Newton's character, that not one of them was voluntarily published by himself. When his youthful composition on the "Quadrature of Curves" had been extolled by Barrow, and shown to, and eagerly copied by Collins, and when he had been urged by the former to publish it, he could not be induced to do so. And afterwards, referring to this circumstance, and the partial success in such researches which Mercator had attained, he says, "I suspected that Mercator must have known the extraction of roots as well as the reduction of fractions into series by division; or at least that others, having learnt to employ division for this purpose, would discover the rest before I myself should be *old enough to appear before the public*; and, therefore, I began henceforward to look upon such researches with less interest." The tracts at the end of the first edition of the "Optics," he tells us, he was compelled to print on account of the plagiarisms from the MSS. of them lent to his friends.

The "Arithmetica Universalis," which contains the substance of his lectures as Lucasian professor, was obtained by Whiston, without the consent of the author; perhaps taken down at the lecture, and published surreptitiously, and, as has been alleged, in a way involving an unjustifiable breach of confidence. All this singular reluctance to make known the valuable truths he had discovered, has been the subject of much observation and conjecture among his biographers; and a variety of suppositions have been made, which to us appear quite uncalled for, since the whole singularity of the case seems easily resolvable into the natural consequences of the peculiar character of Newton's temperament.

From the first we may trace, in the very constitution of this great man, a morbid sensitiveness of mind, and an excessive shyness of disposition. This led him to retire from the public gaze, and to reserve his most valuable discoveries for the mere information of his friends. The same peculiarity we have already seen manifested in regard to his optical works; and he would appear actually to have viewed with regret the publication of those discoveries, when his repose had been disturbed by the controversies which resulted. He thought his celebrity dearly purchased at the price of his tranquillity; and, perhaps, had personal reputation been the only consideration, he reasoned wisely; but when mankind were to be benefited by the promulgation of his discoveries, the question surely assumed a different aspect. But it was probably more to the general reserve of his disposition than to any positive apprehensions of critical controversy, that we are to ascribe his backwardness to produce his mathematical inventions. One thing is certain; that controversy was not, in fact, at all prevented by such precaution, if it be regarded in that light; and indeed it must have been obvious, that could Newton have foreseen the questions which were soon to be agitated, he might, by the mere immediate publication of an explicit account of the method of fluxions, have entirely prevented the whole of those discussions which afterwards occasioned so much vexation to himself, and such unhappy and even disgraceful hostility among the mathematicians of the age. It may, however, be fairly said, that no circumstances at the time could have led any one to foresee such a dispute. From what has been already stated, nothing would appear more unlikely than that the two great rivals would be drawn into hostility; each candidly admitted the just claims of the other, and there seemed nothing to excite discord or jealousy.

One circumstance which tended to limit the applications, and impede the progress of the fluxional system, was the fondness which Newton evinced for the synthetical method in delivering his propositions. This is conspicuous throughout the quadrature of curves, and is upheld and defended in the treatise on fluxions. By synthetical methods we do not here mean the peculiar style of geometrical demonstrations, but the delivery of the results announced as propositions, each proved upon independent considerations. This method may be well adapted for the mere communication of elementary truths; but it has this capital defect, that it does not put the student in possession of those general principles by which the truths in question were discovered, or by which he can be led to the discovery of others. Newton seems, in fact, to have been guided in his choice rather by considerations of taste than of utility; for, in the "Fluxions," he thus expresses himself on the subject:—"After the area of a curve has been found and constructed, we should consider about the demonstration of the construction, that, laying aside all algebraical calculation as much as may be, the theorem may be adorned and made elegant, so as to become fit for public view." (§ 107.) He exemplified these principles very advantageously in that work; but whatever may be thought of this preference abstractedly, it certainly cannot be commended in point of utility: and unquestionably the spirit thus instilled into the English school of mathematicians, had, for a long time after, the worst effect in cramping their energies, and impeding the applications of the calculus, and the progress of mathematical discovery.

We must here, though very briefly, refer to that modification of the same principle of limiting ratios, which Newton adopted with a special reference to certain geometrical cases, under the name of "prime and ultimate ratios." This doctrine he has fully and elegantly expounded in the lemmas which form the introduction to the first book of the Principia; where, with a view to those geometrical investigations which were to follow, he establishes the different truths with regard to the limits of polygonal areas, the limiting ratio between the arc and its sine and tangent, &c.; in all which the substantial truth is, that these quantities, though perpetually varying in their ratio, have yet a finite ratio, which is the constant limit. And to avoid the tediousness of the ancient method of exhaustions, the author makes an application of his highly illustrative idea of motion, conceiving the arc to increase or diminish; and speaking of the ratio subsisting between the quantities as either in its "nascent" or its "evanescent" state. He skilfully obviates some objections which might be raised, and is particularly cautious to insist upon the real meaning being restricted to the consideration of the actual finite *limit* in the strict and simple sense of the word in which it was used by the ancient geometers.

Controversies respecting Fluxions.

It would be obviously impossible, in the sort of discussion to which we are necessarily limited in an historical sketch like the present, to give any thing like an adequate account of such subjects as these analytical researches. All we have attempted is, to offer some general considerations, such as might relieve us from the charge of being altogether unintelligible in the statements which the necessary course of our subject obliges us to give of the most material improvements successively effected in this department of science, so supremely important as affording the instrument, as it were, by the aid of which the great discoveries in almost all other branches were effected, independently of its own intrinsic claims to attention. It only remains, in connection with this division of our subject, to give a very brief account of two controversies which arose relating to it; each of which obtained considerable notoriety from the celebrity of the parties engaged, and each originating out of causes altogether trivial, yet led to consequences of no small importance to science.

The first of these was a dispute which took its rise on the question of priority between Newton and Leibnitz, as to the invention of the calculus; a question for which, as we have already observed, it would be difficult to conceive any possible pretext: nevertheless there was soon a sort of jealousy felt between the English and continental mathematicians, which, though destitute of any single rational foundation, only wanted a plausible opportunity to break out into open hostility. So rapidly had the differential calculus spread on the Continent, and so little was Newton's method known, that it is not surprising that throughout Europe Leibnitz should acquire the sole fame of being its original inventor. The friends of Newton felt that such an impression was unjust towards their illustrious teacher.

In 1699, Fatio de Duillier, a Swiss mathematician resident in England, in a paper read before the Royal Society, thought proper to introduce (though little connected with his subject) not only a remark on Newton's priority, but an unwarrantable insinuation that Leibnitz had derived his idea from Newton. To this Leibnitz temperately replied, by a simple statement of his own originality, admitting Newton's priority.

In 1705, a passage in the "Leipsic Journal," in a notice of Newton's quadrature of curves, though certainly not necessarily bearing that construction, was interpreted as retorting the charge, and insinuating that Newton was the plagiarist. Dr. Keill hastily came forward to defend Newton; but instead of the simple reference to facts, which would have settled the question, his zeal impelled him, in an angry tone, to reiterate the charge upon Leibnitz, and to attempt to show that Newton's communication might really convey to him the principles of the method. Leibnitz appealed to the Royal Society. That body accordingly appointed a committee to examine the question. They drew up a report, in the main accurate and fair, but omitting one circumstance; viz. the impossibility of Leibnitz extracting Newton's discovery from his letter, in which it was purposely concealed in cypher. Thus, as they had not shown the impossibility of the charge being true, it was immediately construed into an admission of its possibility. The report, together with the whole correspondence, was published in 1712, under the title of "Commercium Epistolicum." Leibnitz, under the impression just referred to, complained of the report as unfair; and Bernoulli anonymously wrote a severe and acrimonious comment on it; on which several other no less bitter than absurd publications on both sides came out. But the conflict soon began to take a more rational, though hardly less hostile turn.

The custom had already prevailed of proposing in the public journals various problems, as trials of skill to the mathematicians of different countries. In the "Leipsic Journal" such a problem had been proposed, as to the nature of the curve into which a chain hung by its two ends will form itself, by Bernoulli, in 1690. Solutions had been given by Huyghens, Leibnitz, and others, demonstrating the nature of the curve under the name of the catenary.

Another such problem, proposed in 1697, referred to the nature of the line along which a body must descend in order to go from one point to another (not perpendicularly under it) in the shortest possible time. The question belongs to a class of peculiar difficulty; but it was solved by Newton, Leibnitz, the two Bernoullis, and M. de l'Hôpital, and the curve shown to be the cycloid. Leibnitz solved it the same day he received it. Newton's solution was published without his name. Bernoulli at once recognised its author, exclaiming, "ex ungue leonem!"

The problem of the orthogonal trajectories is another, dependent on the higher parts of analysis; being, to find the curve which shall cut at right angles a whole system of other curves, constructed under given conditions. The solution was known to Bernoulli;

but Leibnitz proposed it in 1716, to try the skill of the English mathematicians. Newton received it in the afternoon, and, though fatigued with the business of the day, solved it before night.

This sort of intellectual skirmishing was, of course, soon mixed up with the controversy about the origin of fluxions. Newton's solution of the trajectories was cavilled at by Bernoulli. Brook Taylor defended it; but most unjustifiably and impertinently concluded by charging Bernoulli with *ignorance* in his critique. Bernoulli revenged himself on Taylor with considerable warmth. The latter then came forward with a defiance to all the Continent, in the shape of a problem of some difficulty in the integral calculus. Unfortunately, however, a solution of the whole class of problems to which it belonged had long before been given by Bernoulli. This was a blow to the English mathematicians, which, however, did not long hinder them from entering the field afresh; and Keill, with equal zeal and want of discretion, proposed the formidable problem of the resistance of fluids, levelling his challenge personally at Bernoulli; who in a short time replied, that he was ready with a solution, which he offered to deposit with an umpire, provided Keill would in like manner send his. Keill never made any reply to so fair a proposal. His antagonist, indeed, had more than suspected him of forgetting, that when a man challenges another with a problem, he ought to be able to answer it himself; and thus instantly assigned the true reason for his backwardness, at the same time exulting over him most cruelly, and losing sight of all common decency in the abuse and invective with which he overwhelmed him.

The controversy was kept up by many minor attacks and replies, which do not appear worth notice. We have seen enough to exhibit a lamentable view of the weaknesses from which the highest intellectual endowments do not exempt their possessors, and we are glad to turn away from such a scene. The controversy was not wholly without some beneficial consequences in giving rise to the discussion of the problems mentioned; but its after effects were those of alienating the British from the continental mathematicians, to a degree highly injurious to the advance of the sciences in England. This, however, is a subject belonging to a future stage of our history. For the present we must recur to the date of Newton's discoveries, and have now to notice the rise of another controversy, in which the fluxional method was yet destined to be involved, though of a more rational character than the last, being directed to the question of the validity of its principles.

So great a revolution in science as that which the new analysis effected it was not to be expected would be accomplished without opposition. There were numerous mathematicians throughout Europe wedded to the old methods; many who, from prejudice or even incapacity, were likely to prove formidable enemies to the introduction of so great an innovation. But it is satisfactory to find that, among the older mathematicians, some of the most eminent were foremost in perceiving the value and forwarding the introduction of the new methods. Among these none deserves to be mentioned with more respect than Huyghens; who (though now advanced in life) was among the first to make himself acquainted with its principles, to master its difficulties, and to teach and recommend its processes.

Opponents, however, were not wanting. Nieuwentyt, a Dutch naturalist and writer of some ability, but very little versed in mathematics, aimed the first blow at the differential calculus, objecting to the notion of infinitely small quantities, in two tracts published in 1695. It was well remarked of him that he was unwilling to believe in the reality of objects less than those shown by his microscope. Leibnitz, Herman, and Bernoulli answered him.

Rolle was a man of that pleasant disposition, whose principal gratification consists in detecting faults. He employed his mathematical acuteness, which was considerable, not in attacking any of the general principles of the calculus, but in searching out with incredible diligence a number of particular examples, in which he contended the rules of the calculus led to incorrect results. But Varignon, Saurin, and others clearly pointed out that the errors were entirely his own, and arose from his misapplication of the rules. This discussion was brought before the Academy of Sciences in 1701.

The Abbé Gallois joined with Rolle in these attacks, and kept the field after the latter had retired. His objections chiefly arose from too prejudiced a partiality for the ancient geometry. Here the controversy seems to have dropped for the time, though we shall find it afterwards renewed.

Newton's Dynamical Discoveries.

The improvements which Newton effected in the sciences of mechanics and dynamics abstractedly, may be viewed as quite separate from the application he made of them to the great problem of the forces actually concerned in the motions of the planetary system.

We have before noticed the advances in statics made by Stevin, Galileo, and subsequently by Wallis. The two former, in fact, generalised the property of the lever, and showed that an equilibrium takes place whenever the sums of the opposite *momenta* are equal: meaning by momentum the product of the force or weight into the velocity of the point at which it is applied. This was extended much further by Wallis, who, in the researches we have referred to, collected in his "Mechanica," published in 1669, founded an entire system of statics on the same principle.

To the mention of these we must add that of Varignon, who, in 1687, published his "Projet d'une Nouvelle Mécanique," in which he had the merit of deriving the whole theory of the equilibrium of the mechanical powers from the single principle of the composition of forces.

The "Principia" of Newton, published in the same year (considered at present merely in reference to its abstract mechanical and dynamical parts), must unquestionably be regarded in that light alone as one of the most extraordinary productions of human genius: it effected an entire revolution in the mechanical sciences. In its introductory part the same general principles of equilibrium, of the centre of gravity, and of the mechanical powers, as those of Varignon and Wallis, are adopted; but they are merely noticed as introductory to the dynamical enquiries which follow.

In this important department, in which the first advances had been made by Galileo in examining the motion of projectiles and of falling bodies, Newton reduces to settled principles the more comprehensive laws on which the whole system depends. These, which he designates as the "axioms" of the science (not necessarily implying the notion of self-evident truths), are the three "laws of motion." They had been already employed by preceding writers, though, perhaps, never yet explicitly laid down, and distinctly proved as the foundation of all the subsequent reasoning. Of late years, some discussion has arisen as to the precise character of these elementary truths, and the philosophical nature of the evidence on which they are supported: and a considerable difference has existed in the manner of viewing the subject by the English and the French writers. We shall not, of course, in a work like the present, attempt to enter upon such a subject. We shall merely observe that the questions which have been raised, and the critical improvements which have been suggested in the mode of exhibiting these first principles, do not in the least affect Newton's credit, as he does not state them with any claim to originality, nor will the important reasonings which he founds upon them be affected by the somewhat different point of view in which they have been since regarded.

As a corollary to these laws of motion, he gives the demonstration of the composition of forces, which is, in fact, the essential principle on which every system of dynamical reasoning must proceed. We must briefly refer to this fundamental doctrine, in order to preserve the train of reasoning by which his ulterior conclusions were established.

According to Newton's view of the matter, which (like the view taken of the laws of motion) has been canvassed by the French mathematicians, a body is supposed to have an impulsive force acting on it, by virtue of which alone it would continue moving uniformly in a straight line, while at the same time another force acts upon it, having, at every moment of its course, an equal tendency to draw it in a direction inclined to the former at a given angle. It consequently obeys neither impulse entirely, but takes an intermediate course, called the "resultant" of the two former, and which is determined by drawing the diagonal of the parallelogram the two sides of which represent the two former forces. The truth of this is made to depend on the laws of motion simply, by virtue of which the body retains each of its first motions unaltered as to its quantity, and at the end of a given time is found at precisely the same distance, measured on a *parallel*, to which it would have reached in the original line, by virtue of either of the first forces alone. Of the truth of the result there can be no doubt: but the French writers adopt a different mode of proving it.

Central Forces.

It is on the above simple principle that Newton proceeds to ground the whole theory of central forces. He arrives at this doctrine by a very striking application of the ancient idea of limits. He supposes a body to be projected in a given straight line, as in the last instance, but to take a resultant course from the action of a new force. At the end of a given time, it has described the diagonal of the parallelogram, constructed as before, with sides proportional to those two forces. It would now continue in the diagonal. At this point let it be again deflected by a new force, making it describe a new diagonal: at the end of another interval, let, again, a new deflection take place in the same way, and so on successively: then, if all these deflections be towards the same side, it is obvious that the successive diagonals will form the successive sides of a polygon, regular or irregular; and the forces which at each point caused the deflection, will have directions, which, if produced, will cut one another at some points within the polygon. They may cut in the same point: if so, it becomes easy to show, that, supposing the portions of time equal, the triangles formed by these lines so meeting, and the bases or sides, will be all *equal in area*. This equality of areas, then, in equal times, is produced when a body describes in equal times the successive sides of a polygon, by virtue of an original impulsive force which alone could carry it on for ever in a given straight line, and the action of another force which acts at equal intervals of times, and at each time changes both its amount and direction, all its directions at successive intervals converging to one point within the polygon.

This, abstractedly, would have been a curious theorem, but Newton investigated it with a view to a higher use; and, by following up the same idea to its extreme case, by a beautiful application of the principle of a limit (though represented by him under a different form of expression), he succeeds in making this theorem the basis of the whole system of central forces.

The *limit* of such a *polygon* as we have described would be a *curvilinear periphery*; and the *limit* of motion uniformly continued along each of the sides, but varying from one side to another, would be a perpetually varying motion in a curvilinear path. Few ideas are really more simple than this, when divested of technical obscurity. In the case of the finite quantities, and finite uniform action, we can subject the forces to mathematical estimation. The relations which we establish *remain unal-*

tered, when we pass from the elementary quantities to their limit. This is the only part of the reasoning which involves any sort of difficulty, and this it is which Newton had abundantly provided for, in previously establishing those various cases of limits which he denominated "prime and ultimate ratios." It was thus that he was led to the demonstration of motion in a curvilinear orbit, within which a certain point was so situated that portions of the orbit described in equal times being assigned, though those portions themselves might be very unequal, yet lines or radii from them to that point would intercept *equal sectors or areas ;* or, in other words, a *radius,* supposed to revolve about this point, with the variable velocity belonging to the body in the orbit, would *sweep over equal areas in equal times.* The point with respect to which such a property held good was denominated the *centre of force.* As the curve, then, formed the *limit* of the uniformly described rectilinear increments, so the action of a force directed to this centre, but varying in intensity at each successive point of the orbit, was the *limit* of the successive unequal forces acting throughout each of the rectilinear portions.

In any such polygonal orbit at the end of any one side, the other *side* of the parallelogram was the measure of intensity of that part of the force by which the body tended to the centre. It easily followed from Newton's lemmas, that the limit of this, in a curvilinear orbit, measures the amount by which the arc, at its further end, has deviated from the direction of the tangent, or the *deflexion of the body ˏrom a rectilinear course ;* and this will evidently be, at any point, the *measure of the intensity* of the central force. The limit of the other side of the parallelogram (which measures the *impulsive* force) is the same as that of the *tangent* to the curve. Thus the *limiting ratio* between these two forces, is that which determines the curvilinear path which the body will pursue. To some idea of this kind, it would seem that we must refer the extraordinary expression of

Aristotle, formerly quoted as to the cause of circular motion.

What remained was, to investigate the mathematical conditions which would give this determination in different cases. It was here that, in a pre-eminent degree, the wonderful geometrical resources of Newton's mind were displayed. He proceeded to examine, first, some simple cases of curvilinear motion, and then the more general principles which might apply to any species of orbit. All this, it is to be borne in mind, was an entirely abstract speculation, utterly independent of anything actually existing in nature, and belonging only to a system of mathematical dynamics. The whole was Newton's original creation. He originated the very idea by which *motion about a centre* was accurately conceived; and referred to the principle of equable description of areas, as the sole intelligible test of a central force, without involving any physical hypothesis, or occult qualities, to give an idea of the action of such a force.

A body, then, projected by some original impulsive force, in the direction of a given straight line, but at the same time exposed to the action of a central force, will neither proceed in the direction of the tangent, nor fall direct into the centre; but will take an intermediate curvilinear course. The nature of this curve depends entirely on the limiting ratio, resulting from a comparison of the impulsive or tangential with the intensity of the central force. Here Newton displayed the richness and fertility of his powers of geometrical invention, in tracing various imaginable cases, and the forms of orbits which would result, when the force was supposed to vary according to different powers of the distance from the centre. Here he brought to bear upon his dynamical investigations all those abundant stores of geometrical truth, which the labours of mathematicians had for ages been accumulating. In particular, the properties of the conic sections, which had so largely, and, as might have been imagined, so uselessly, occupied the attention of the whole race of ancient geometers, were now brought into use, and, marked as those singular curves are by so many curious characteristics and interesting analogies, the applications now given to them were of a nature never before imagined, even in the loftiest flights of the Platonic visions, and associated them with a vast range of the most sublime physical truths. By the happiest combination of the anciently established properties of those curves, (and mainly those of the "circles of curvature,") with his own original methods of ultimate ratios, Newton succeeded in establishing, with the most incontestable evidence, the beautiful and highly important theorem, that *a body projected in a straight line, and subjected to the action of a central force, will revolve in some one of the conic sections, if the force vary inversely as the square of the distance from the focus.*

Which of the conic sections it will be, depends upon the actual value of the limiting ratio of the tangential or projectile to the centripetal force. Such is the most important dynamical theorem, established as the foundation of Newton's system. But the scope of the Principia is not limited to this. The illustrious author follows out his principle into a vast variety of consequences. He traces the laws which, by mathematical consequence, must regulate the velocities in elliptic orbits; the periodic times in which those orbits are completed; and establishes the remarkable relation, that the squares of the periodic times are, as the cubes of the major axes, the halves of which are the mean distances, as a dynamical consequence from a law of force inversely as the square of the distance. The exuberant riches of the author's genius, lavished in profusion upon every page of the Principia, perhaps, more than any other circumstance, will impress upon the reader's mind the vastness of that intellectual might which enabled him, not only to grapple successfully with so difficult a subject, but even, as it were, to play with it, and take delight in going out of the more direct path which might have sufficed to conduct him to the particular conclusions he especially wanted, to follow out innumerable collateral topics which suggested themselves by the way. But, valuable as all these speculations are, it is more than probable that the introduction of them has tended materially to abridge the number of readers of the Principia; the impression is certainly that of overwhelming and confusing the mind of a learner first approaching the study of a subject, sufficiently difficult in itself, if only followed up in the direct line of its most important applications.

But besides the abstract consideration of central forces, the author examines the curious and important subject of the *attractions* which portions of matter may be conceived to exercise upon one another; and shows that if small particles, attracting according to the law of the inverse square of the distance, be aggregated into spherical masses, those spheres would themselves be subject to the same law as the particles which compose them: that the attraction would be directed to the centres of the spheres, and be proportional to the quantity of matter contained in them, divided by the square of the distance from the centres.

Passing over a vast number of other researches, which it would be impossible even to enumerate in a sketch like the present (including those on the motions of bodies in resisting mediums, and various points of hydrostatics), we will barely allude to one investigation, dependent on the attractions of bodies, and relating to the effects which would take place upon extremely minute molecules, projected with an inconceivably great velocity, and entering a medium in which they were subjected to new attractions,—an investigation which, in fact, supplies the principle of that hypothetical explanation of the *phenomena of light*, which results from supposing it to consist in such an emission of material particles from luminous bodies. We have already referred, in a passing way, to this hypothesis; and we will here take the opportunity of noticing, that it no where appears that Newton regarded it as any more than a mere hypothesis. And though it has often gone abroad as the *Newtonian* theory of light, yet it is more than probable that Newton himself was disposed to lean to the hypothesis of undulations.* In the Optics, he most cautiously urges the reader to attach no physical conception to the theory of *fits*. He certainly employed the molecular theory for the purpose of calculating the results mathematically, especially of establishing the law of refraction. But, as Professor Airey has pointed out, there was a sufficient reason for this, in the circumstance that here he had the *mathematical methods* ready established: in the theory of the undulations, the first principle of such investigations was as yet unknown.†

Newton's System of the World. — History of the Discovery.

In 1666, the plague had broken out in Cambridge, which drove Newton into the country. Retiring to Woolsthorpe, he devoted himself to philosophical contemplations. As it is the characteristic of vulgar minds to attach the greatest importance to whatever is uncommon, astonishing, and marvellous, though often quite undeserving of any particular attention, so it is the mark of a powerful mind to perceive, in things of common occurrence, how much is to be learnt; and to deduce important reflections from circumstances which the generality of men pass by unheeded. Mankind had been for ages accustomed to witness the fact that all bodies fall directly to the ground the moment support is withdrawn. But no one ever saw in it any thing wonderful. To Newton, however, apples falling, as it were, spontaneously from the trees in the orchard, at Woolsthorpe, afforded a topic of reflection, and led him into a train of profound thought, from which, ultimately, resulted nothing less than a complete discovery of the system of the world.

This power or force, he observed, extends to some distance from the surface of the earth: it exists at the

* See Dr. Young's paper, Phil. Trans. 1802.
† Journal of Science, June, 1833.

tops of the highest mountains; it extends to the highest region of the atmosphere: may it not reach as far as the moon? Again, within the short distances to which observation extends near the surface of the earth, this force does not diminish sensibly as we recede from the earth; but may it not diminish at greater distances? If so, according to what law? Supposing it, as in some cases of the dynamical speculations in which he had been engaged, to decrease as the square of the distance from the centre of the earth, it might be calculated in what degree its intensity would be diminished at the moon, the moon's distance being known. Indeed, this calculation might be carried a little farther, and the result subjected to the test of observation. For the actual intensity of gravity at the earth's surface is estimated by the space through which a body falls in one second. By reducing this, in the ratio of the squares of the distances from the earth's centre, we should have the space through which the moon ought to fall towards the earth in one second, if not impeded by any other cause. But how could this be ascertained, since such an experiment cannot be tried upon the moon? Newton saw that such an experiment is, in fact, constantly exhibited to us. The moon performs a revolution in an orbit whose dimensions had been ascertained by astronomers. Consequently, the *velocity* with which she moves was known. But this velocity impressed upon such a body must, if nothing else interfered, carry it off in a straight line through space. The actual motion of the moon is in an orbit round the earth: and in any given portion, or arc, of that orbit, the quantity by which, at the end, for example, of one second, the moon has deflected from the direction of the straight line, which is a tangent to the orbit at the commencement of that second, is known. This, then, is the space through which, in one second, the moon is actually falling towards the earth.

Newton, then, in his calculation, had only to take the distance of the moon from the centre of the earth, and the distance of the surface from the centre, that is, the radius of the earth; and, squaring these numbers, the inverse proportion would be that of the spaces fallen through, in one second, by the moon, and by a body at the surface of the earth. If, then, this calculated result agreed with the result actually observed, his conjecture would be verified; and the very same force of gravity which causes bodies to fall near the earth, would be that which causes the moon to fall, or, in other words, to be deflected from a rectilinear course, and to describe her orbit about the earth. In this calculation, Newton took for the radius of the earth that value which resulted from the measurement of the length of a degree, according to the determination of Norwood and others, before mentioned, the best known at that time. The result of the calculation *did not fulfil* Newton's anticipation. Hence, with real, philosophic love of truth, he modified his suppositions to accord with the result: and concluded that, though gravitation is in part the cause of the moon's motion, yet, some other, as yet unknown, cause conspires with it to produce the effect.

On his return to Cambridge, his attention, as we have already seen, was fully occupied with other subjects; and, not having brought his speculation on gravity to any satisfactory termination, he carefully concealed his ideas; and probably dismissed the subject from his thoughts for some time.

Meanwhile we have already noticed the approaches which had been made by several philosophers, but especially by Hooke, towards the theory of gravitation. And it was by some speculations of this eminent man (now secretary to the Royal Society) that Newton's attention was recalled to the subject. In 1679, Newton addressed a letter to him, in reply to some queries proposed by the society, in which he suggested, that, if the earth be really in motion, it would be actually evince by the circumstance, that a body let fall from a great height (since, by virtue of its elevation, it participates in a velocity greater than that of bodies on the surface) will not fall exactly in a perpendicular line, but will deviate towards the east. Hooke entered upon the subject both experimentally and theoretically: he verified the fact, and improved upon the theory, by showing that the deviation ought to be a little to the south-east, the direction of gravity being (except at the equator) oblique to the earth's axis of rotation. This topic being discussed, led to the further question, in what *line* the body would descend. Newton, it appears, had inferred that it would be a species of spiral, owing to the resistance of the air. Hooke alleged that in vacuo it ought to be a portion of an ellipse, *but he assigned no reason or proof of this.* Nor could he do so afterwards, when earnestly pressed, both by Halley and Wren, to give a demonstration. It was about this time that his other speculations, to which we have before referred, on gravity, &c., were communicated to the Royal Society. It is probable that, at this time, Newton had succeeded in investigating some of those dynamical problems respecting elliptic motion, to which we have before adverted. Still, these were mere abstract speculations, the lofty exercises of a mathematical genius of the highest order, but which, as it yet seemed, were not to be recognised in nature; so far, that is, as the accuracy of observation had then extended.

But upon what did the insufficiency of Newton's calculated result depend? Might not the numerical values assumed be open to question? The orbit and distance of the moon were so well ascertained, by the long-continued labours of astronomers, that with respect to them there could be little doubt. Not so the magnitude of the earth. We have already seen how slow was the advance to accuracy, in obtaining this result from the measurement of arcs of the meridian. The more accurate determination of Picard had, however, now been effected; and his recent result became the subject of discussion at a meeting of the Royal Society, in June, 1682. Newton, being present, felt, of course, a degree of interest in the discussion, totally unsuspected by the bystanders.

Noting down Picard's value of the earth's radius, he hurried home, and, having substituted this number in his former proportion, and proceeded a little way in the calculation, he was utterly unable to carry it on, from the overpowering excitement of its anticipated termination. He requested a friend to finish it for him ; and the result was, a perfect accordance of the force which acts upon the moon, with the force of gravity at the earth's surface, diminished in the exact ratio of the squares of the distances.

General View of the System.

This one grand result sufficed as a clue to the whole mechanism of the planetary system. Newton now devoted himself to follow up the ideas which this conclusion suggested, in connection with the deductions he had already made with respect to the dynamical laws of central forces. The great inductive laws of Kepler had shown that those relations obtain, at least, to a great degree of exactness, between the distances and periods of all the planets, the forms of their orbits, and the equable description of areas which, on abstract dynamical principles, ought to belong to bodies freely revolving about a common centre of force, from which the diminution of that force takes place, in proportion to the squares of the distances. This accordance was exhibited in the system of the primary planets round the sun, and even yet more palpably in the small systems of satellites round Jupiter and Saturn; and, lastly, but not least, in the motion of the moon round the earth. This last case, in fact, was the key to the others. Equable description of areas, as we have observed, was the test or index of a centre of force. But that centre might be an empty point of space. In the case of the earth and moon, the centre of force was at the centre of the earth ; and that particular physical property which we find belonging to particles of matter on the earth's surface, and which we call gravity, or weight, was proved to be the very same which affects the moon. Its identity was defined by the actual observed diminution of its intensity as we ascend in the atmosphere, and its precise agreement with the diminished force with which it was proved to act on the moon. Does it, then, asked Newton, act between every two particles of matter throughout the universe? Is there (without any physical hypothesis as to the *modus operandi*) a real tendency to approach each other, with an intensity inversely as the square of the distance actually existing and operating throughout the planetary system? To this question the accumulating testimony of the observed phenomena, as examined by Newton and his successors, to the present day, has been giving the answer. The force of the argument depends upon the *collective* proofs which all the facts, in their several ways, minister to the truth of the great principle of gravitation.

In a system of bodies, connected by this sort of attractive influence, and revolving about their common centre of gravity, if one body were considerably greater than the other, the common centre of gravity might fall within the mass of the greater body, and even be situated sensibly in its centre. Thus, the centre of the earth was seen to be, at once situated in the centre of force of the moon's orbit, and, the actual source of the physical force of gravitation. In the same way it followed that Jupiter and Saturn were, in like manner, actual sources of gravitating influence to their satellites, as well as situated in the centres of force of their orbits. In like manner, therefore, the centre of the sun, by virtue of the universal observance of the same law, of the inverse squares of the distances, was the source of gravitating influence to all the planets, as well as the point in space to which the equable description of areas, in all their orbits, pointed as the centre of force. Among the various remarkable applications which Newton made of this truth, none are, perhaps, at first sight, more striking, or even seem more incredible, to a person unacquainted with the subject, than the determination of the actual densities, or specific gravities, of the matter of which the several planets are composed. Yet the principle on which they are found, is no other than that by which the intensity of the earth's gravitation on the moon is ascertained. Gravity is nothing else than weight, proportional to the quantity of matter; and density is measured by the quantity of matter and the magnitude.

But this same principle of the mutual attraction between all particles of matter throughout the universe, and the law of gravitation, inversely as the square of the distance, and directly proportional to the mass or quantity of matter, was yet found to be applicable to a different class of phenomena, and susceptible of proof from other facts in the system of the world, of which, they afforded an explanation. Such were the tides; the general facts of which were investigated by Newton, to a sufficient extent to evince, at least, the general applicability of his principles. He showed that they were due to the joint action of the sun and moon, and even estimated the amount to which the tide ought to rise from theory, supposing its course uninterrupted by continents, and the depth uniform. No theory, it is evident, can accurately agree with facts modified by circumstances so widely different as the actual cases are from these suppositions; but still there is a very near accordance between Newton's result and the average height of the tide in the main ocean.

The waters of the sea having a free motion, which the solid parts of the globe have not, are able to obey the impulse of attraction to a certain extent, limited by their terrestrial gravitation. According to the relative positions of the two luminaries, the resulting effect of their two attractions, conspiring or opposing, produces a greater or less rise in the waters, as each portion comes successively under its influence; and a corresponding rise at the opposite point, since the water on that side, being farther from the sun or moon, is less attracted than the earth, which is thus, as it were, drawn away from it. The action of the sun is very small compared with that of the moon; hence, the high tide occurs almost exactly when the moon comes upon the meridian. This is highest when the sun, earth, and moon are in the same line; and this, whether at opposition or conjunction, is called the spring tide. The action is weakest when the directions of the sun and moon are at right angles; this is the neap tide: in this, or at quadratures, the solar and lunar tides are opposed, or the high water of the solar takes place at the same time as the low water of the lunar, and conversely; so that the actual tide is the difference of the two: at opposition and conjunction it is the sum.

Another important subject in which the principle of gravitation was found to be concerned, is the figure of the earth. The force of mutual attraction would aggregate the parts of matter together into a perfectly spherical form. But if a rotatory motion were given to such a sphere, it is an obvious consequence that — if its parts have in any degree a sufficiently yielding consistency to allow of it — the equatorial region will bulge out, the polar parts will be flattened, and the whole assume the figure of an oblate spheroid. Newton not only established this inference, but endeavoured to estimate what its amount ought to be under given circumstances. This he did by means of the very ingenious illustration of conceiving a pipe or canal reaching from the pole to the centre of the earth, and another at right angles to it, opening from it and reaching from the centre to the equator, the polar and equatorial waters freely communicating through it. The water in such a canal must be in equilibrio, or the weight of fluid in the shorter equal to that in the larger branch. Taking into consideration then the effect of centrifugal force, and the other more complicated conditions which were introduced by the spheroidal form, Newton calculated what the respective lengths of the canals ought to be; or, in other words, the proportion of the polar to the equatorial radius of the earth. In this calculation, the earth is supposed to be homogeneous; and though Newton was necessitated to supply some data of the calculation upon assumptions which have been since found not altogether exact, yet, upon the whole, the modern investigations give a result differing very little from his, which makes the proportion that of 230 to 229.

As yet, however, there were no *measurements* from which the least hint of any such actual deviation from a supposed perfect sphericity in the earth's figure had been suggested. But there were certain facts lately disclosed, which, though not appearing to the generality of astronomers to have any relation to such a subject, yet, in the mind of Newton, were instantly associated with it in the closest connection.

From the spheroidal figure it followed, that the intensity of gravity at different parts of the surface would be different; and would decrease, in going from the pole to the equator, in proportion to the square of the sine of the latitude. The vibrations of a pendulum depend entirely on the force of gravity: hence, the same pendulum, near the equator, ought to vibrate more slowly than in higher latitudes. We have before seen that this was precisely the fact which had been observed by Richer, Varin, and Des Hayes, and had excited so much astonishment.

Another investigation, closely connected with this fact, perhaps exhibits in a still stronger light the surprising powers of the genius with which Newton was gifted for seizing remote analogies, and bringing to bear upon the same point, facts and arguments apparently of the most widely dissimilar character.

We have already seen that the successive observations of astronomers had disclosed the fact, that the equinoctial point, or intersection of the plane of the earth's equator with that of its orbit, is constantly but slowly changing its position, and retrograding in the order of the zodiac. This was termed the precession of equinoxes, and the amount of it had been assigned as about fifty seconds annually.

But how could this be connected with gravitation? To this Newton replied, by not merely showing the way in which it was produced, but calculating its amount from the known laws of gravity, its agreement with which thus furnished a fresh proof of their truth. If, argued Newton, the earth were a sphere, it might be any how inclined, and no action of gravitation would alter its inclination. If, however, it were spheroidal, and placed *at rest* with its equator inclined to the plane of the orbit, the attraction of the sun upon the protuberant equatorial points would bring it down into the plane of the orbit. But if it were in diurnal rotation, the effect of this attraction would be insensibly small in actually diminishing the obliquity, but would cause the point of the intersection to shift slowly in a direction opposite to that of the rotation. The mechanical reasoning is somewhat of a refined character, and perhaps not easily rendered intelligible without going into greater detail than we have done. The attraction of the moon had to be taken into account, as well as that of the sun.

We have here, for the sake of illustration, spoken of simple elliptic orbits, and the observance of Kepler's laws as exactly true. But from the same principle of attraction by which, in a general way, they would hold good, it follows, that, as all the different bodies of the system will exercise more or less of attraction one upon another, the *exact* fulfilment of these conditions must be in a certain degree disturbed. As, however, observation indicates that the concidence is very nearly exact, it follows that the amount of these disturbances is very small. Newton recognised the existence of such disturbance; but it appears that, in regard to the planets, he conceived it so small, that the consideration of it might be in general safely neglected, except, perhaps, in regard to Jupiter and Saturn, when near their conjunction.* With regard to the moon, he saw that the amount would be much greater. The observance, then, of Kepler's laws would give a certain general proof of the truth of the

* Principia, iii. 5.

principle of gravitation. But if this were *perfectly* exact, the principle could not be true. The existence of certain deviations was absolutely necessary to make out the case. If, then, these could be calculated by the theory, and the results found to agree with observation, the most perfect and satisfactory proof would be furnished. But this, as it may well be imagined, was a field of research of the most extensive and bewildering kind: the different paths of investigation to be pursued were perplexed in the utmost intricacy, and the computations of the most difficult nature; Newton, however, found the means of overcoming those difficulties, to a sufficient extent to carry him satisfactorily through the investigation of the most important cases. These were principally those inequalities of the moon's motion, some of which were of sufficient amount to have been noticed (as we have before seen) by the ancient astronomers, and which were now ascertained with considerable accuracy. The greater phenomena, then, of the planetary motions, constituted one class of proofs of Newton's theory. But these would be incomplete and unsatisfactory, without those of the second kind, derived from the theory of "perturbations," as they are termed. Of these, the lunar inequalities formed the most conspicuous portion, and which occupied largely the attention of Newton. It is true that the investigation of the planetary system is thus involved in much difficulty and intricacy, which would not belong to it were it restricted to exact elliptic motion; and there are probably many to whom the simplicity of the absolute elliptic orbits would afford the most powerful evidence, but who might turn away with impatience from the laborious details of the theory of perturbations. Few of these, again, are of such magnitude as to appear to an ordinary apprehension worth the labour bestowed upon them; nevertheless, it must be borne in mind, that on their successful exposition, the truth of the whole system essentially depends.

Besides the force of the earth, or rather of the mutual gravitation of the moon and earth, the moon must be acted on by the sun; and the same force which was sufficient to bend the earth's course into an ellipse, could not but have a sensible effect on the orbit of the moon. Newton then proceeded to estimate the difference of the forces of the earth and sun, by which the moon's motion is affected. This investigation, and those connected with it (in a collective point of view, termed "the lunar theory"); the deviations from the regular law of velocity which belongs to an elliptic orbit; the retrogradation of the nodes of the moon's orbit (or intersection of its plane with that of the earth's); the decrease of the angular motion, and the moon's gravity; were the points which Newton successfully investigated. With respect to some others, many of the data were by no means perfectly well determined, on the one hand, and the methods of analysis had not yet the requisite extension to fit them for such delicate enquiries, on the other. Newton's labours, therefore, on these points, were unavoidably such as to leave much for his successors to do in bringing them into a more complete and exact form. Nevertheless, even with these necessary imperfections, his results must be regarded as astonishing proofs of that intuitive skill which, notwithstanding these adverse circumstances, enabled him to seize upon and preserve the clue which, through all difficulties, brought him to some conclusion never very far from the truth.

After all that related to the motions of the earth, the moon, and the other planets had been satisfactorily investigated, there still remained one important and very striking class of phenomena, the appearance and motions of *comets*. We have before observed that these apparently singular and anomalous bodies had been already elevated from the rank of meteors, engendered in our atmosphere, in which the ancients had placed them, to that of celestial bodies moving through the immensity of space, by the observations of Galileo and Cassini. Several conjectures had been broached as to the nature of their paths. Hevelius had established the fact that their line of motion is more curved at some parts of their course than at others; and had even suggested that it resembled a *parabola*, having its vertex at the point where the comet came nearest to the sun.

Newton, seizing this hint, perceived in it a simple case of the law of gravitation. The nature of that force might make a body move in a parabola, provided the intensities of the central and original projectile force were properly adjusted. Or, again, they might be so adapted as to cause it to move in an ellipse of very great excentricity; such, for example, that the part of it which came within our system might hardly sensibly differ from a parabola. The reader will have no difficulty in conceiving this, by merely attending to what we observed, in an early part of our history, respecting the mode of describing the conic sections. Would then observation accurately confirm the idea of such orbits? Newton showed, by the example of precise observations made on the brilliant comet of 1680, that this agreement was very close. The observations on the position of the comet accorded with the form of a parabolic orbit, and its rate of motion with the law of the equable description of areas. Comets range into our system from all quarters of the heavens, and from distances inconceivably great in the depths of space. Hence gravitation extended in all directions, and to unknown and inconceivably great distances from the centre of our world.

We have thus taken a rapid survey* of the main points of Newton's investigations, establishing the great system of the world, in a collective point of view, as they are presented to us in his immortal "Principia." In terminating our sketch, therefore, we must not omit to mention, that, after pursuing the elaborate

* The reader will, of course, look for the further explanation of all the points here referred to, in their proper places in the TREATISE ON ASTRONOMY, Cab. Cyclo.

exposition of the mechanism of the heavens, which constitutes the third book; after that beautiful developement of the entire system of forces which guide and modify the revolutions of the planets primary and secondary, Newton concludes the whole with a sublime application, worthy of the preceding parts of the work, n which he deduces, in the direct line of argument from the laws of material phenomena, the proofs of the existence of the Great First Cause, and the evidences of the divine perfections.

In the view we have before taken of the progress of enquiry in the several departments on which the discoveries of Newton shed so new and brilliant a light, we have clearly traced at what point the labours of preceding philosophers had failed; and, on the other hand, precisely how much they had effected, and to what amount, each in their several ways, they had contributed towards the great work which it was reserved for Newton to complete.

The mathematical methods of investigation had been, in some measure, prepared by the invention of Newton's immediate precursors, and the geometrical truths which, from the immovable foundation of the whole, had been established by the ancients. But the discoveries of Plato, Euclid, and Apollonius, and the investigations of Kepler, Wallis, and Barrow, wanted their connecting principle, till it was supplied in the powerful methods created by the master mind of Newton.

The dynamical and mechanical truths contributed by Archimedes and Galileo; the laws of motion and force collected by Wren and Huyghens; the notions of attraction expounded by Hooke, had been brought together, but not as yet reduced into systematic connection.

The phenomena of the heavens, recorded from the infancy of the world, had been referred to certain laws, and order educed out of apparent confusion by the successive labours of astronomers; the magnitude of the earth had been measured, and computation had been rendered easy by logarithms.

The telescope, in the hands of Galileo, had assimilated the planets to the earth, and placed the earth in the rank of a planet. The small systems about Jupiter and Saturn had presented the most striking analogies; and, above all, the wonderful harmony pervading all the motions of our system, displayed in the constant observance of those singular proportions elicited by Kepler, and extended by subsequent observers, formed a mass of not less valuable and important than mysterious truths.

But these great facts and relations of the planetary world were unconnected with the dynamical laws; nor were the mathematical truths yet applied to combine them in their proper places and relations.

Thus from all quarters the materials were provided; but there wanted an arrangement and a connection. The substantial foundations were supplied from one quarter, the richly finished workmanship from another; but there wanted the genius of the architect to arrange and cement the whole into a well-designed edifice. The stones had been hewn out of the mountains; the cedar had been contributed by Hiram, and David had provided brass and gold; but there wanted the Solomon to plan and rear the edifice. He erected it, and consecrated it a temple to the Lord.

Style and Method of the "Principia."

With regard to the style and manner in which these invaluable researches are presented to us by their author, it appears unquestionable that, though he has throughout adopted the language and method of the ancient geometry, deviating from it only where the nature of the case absolutely compelled him to do so, yet the investigations were originally pursued by a very different method from that in which they are actually delivered in the "Principia." There can be no doubt that the results were first obtained by the use of the fluxional calculus, and then synthetical proofs of them invented with that happy facility which so powerfully characterised Newton's genius.

This, indeed, appears certainly to have been the case from the unpublished correspondence of Newton with Cotes, preserved at Cambridge, relating to the preparation of the second edition of the "Principia," which Cotes superintended. In these letters the analytical method is almost always used in their mutual discussion of such points as appeared to want further explanation. It has also been supposed that Newton must have been in possession of some analytical processes of a higher class, such as "the calculus of variations," for solving some of the problems which now appear in the "Principia" in a totally different form. But, perhaps, it is more probable that, without any such general systematic method, he worked out his way by some mode of investigation peculiar to himself, hardly reducible, perhaps, to fixed rules, and evincing in the highest degree the resources of his genius. "L'inspiration," says Bailly*, "de cette faculté divine lui a fait apercevoir les déterminations qui n'étoient pas encore accessibles; soit qu'il eût des preuves qu'il a supprimées, soit qu'il eût dans l'esprit un sorte d'estime, une espèce de balance pour approuver certaines vérités, en pesant les vérités prochaines, et jugeant les unes par les autres."

When drawing up the first portion of his researches to send to the Royal Society, he says (in one of his letters), "I then *composed* a few theorems;" evidently using that word in the sense of the old geometers, and meaning, that, having already discovered the results by analysis, he now put them into a *synthetical* form; without which he was unwilling that they should go forth to the world.

In some parts of the "Principia," when he could not keep entirely to the ancient model, we find him apologising to his readers, and, as it were, excusing himself for so transgressing, on the plea of desiring to be concise. "Componi possent harum assertionum

* Hist. de l'Astron. tom. ii. liv. xii. 28.

demonstrationes more magis geometrico, sed brevitati consulo." Often, probably, he satisfied his own mind as to the truth of the conclusions at which he had arrived, without thinking it necessary to give them a formal demonstration at all; or worked them out by various indirect methods of calculation, which, with singular sagacity and address, he could always apply to cases apparently the least susceptible of such solution: but it was utterly offensive to his taste to allow any investigations in such a state to appear before the public. We have already had occasion to quote his recorded sentiments on this point, with regard to mathematical theorems. In this case there was also another and more powerful motive; he was well aware that he had on all hands violent prejudices to encounter. He feared the announcement of the fluxional principle, because he foresaw it would have been misunderstood, and have laid him open to every species of cavil and objection; and he had now to announce discoveries, which would not only stir up every remnant of the Ptolemaic prejudices, to which there were still some lingering adherents, but would raise up against him the powerful phalanx of the disciples of the Cartesian philosophy, which now reigned triumphant in the universities. It would then have been highly imprudent to risk the announcement of his discoveries in a form in which they would be exposed to double misrepresentation, equally on their own account, and on that of the method by which they were delivered. In the case of almost any philosopher under similar circumstances, this would surely afford a perfectly sufficient explanation for a backwardness in bringing such investigations before the public; but when to this we add the consideration of the known peculiarities of Newton's character, it seems to us every appearance of difficulty will vanish in accounting for his reluctance to publish his system, and for his preference of the ancient form of demonstration, which have afforded so much matter of surprise and speculation. Both, in fact, have been strangely and most unworthily represented by some French writers. Bossut, after justly observing that the subject has been involved in unnecessary difficulty by the method adopted, and referring both to the synthetical form and the extreme brevity of some of the statements, is led to suppose, "ou que Newton, doué d'un sagacité extraordinaire, avoit un peu trop présumé de la pénétration de ses lecteurs ; ou que par une faiblesse, dont les plus grandes hommes ne sont pas toujours exempts, il avoit cherché à surprendre un admiration que le vulgaire accorde facilement aux choses qui passent ou fatiguent son intelligence." And again, D'Alembert, speaking of Newton's fondness for the ancient geometry, says, "Il s'en servoit pour cacher sa route, en employant l'analyse pour se conduire lui-même."

It will be quite unnecessary to observe, that the whole tenour of Newton's conduct and character at once refutes the supposition of his being influenced by such motives as those here alluded to, were the case not fully explained by what we have just said.

History of the Discoveries — resumed.

We have thus far been cursorily examining the subjects of Newton's researches. We must now recur to the history of their production and publication.

From the period of his renewed and successful computation in 1682, Newton appears to have been completely absorbed in the vast variety of investigations necessary for following up the innumerable consequences of the great principle of universal gravitation which now crowded upon his mind. "Henceforward," says M. Biot, "he devoted himself entirely to the enjoyment of these delightful contemplations ;" and, during the two years that he spent in preparing and developing the materials of his great work, "he lived only to calculate and to think." Lost in the contemplation of these grand objects, he often acted unconsciously : his thoughts preserving no connection with the ordinary concerns of life. It is said that frequently, on rising in the morning, he would sit down on his bed-side, arrested by some new conception, and would remain for hours together engaged in tracing it out, without dressing himself. It is also related, that he sometimes forgot his meals, unless reminded by those about him. To those concentrated powers of thought, totally abstracted from all external objects, he owed his intellectual conquests. At a later period, when asked by what means he had arrived at his great discoveries, he is said to have replied, "By always thinking unto them." And on another occasion, "I keep the subject," he observed, "constantly before me, and wait till the first dawnings open slowly by little and little into a full and clear light." And again, in a letter to Dr. Bentley, he says, "If I have done the public any service this way, it is due to nothing but industry and patient thought."

We have already observed, that the immediate occasion which directed Newton's enquiries more particularly to the subject of elliptic motion, was the suggestion of Hooke relative to falling bodies. We have noticed the approaches already made to investigating the subject. We have observed some of the most eminent philosophers acknowledging their inability to discover the law in question; and even Hooke, though hazarding an assertion, yet unable to give a proof.

In January, 1684, Dr. Halley, on the hypothesis *of circular orbits,* by means of Huyghens's theorems respecting centrifugal force, had determined the tendency in the different planets to recede from the sun; and from the analogies of Kepler he had recognised these tendencies to be reciprocally as the squares of the distances; so that the force of the sun to retain them in their orbits, which must be exactly equal to this, must follow the same law. Halley, however, clearly saw that this, though true in *circular,* was not proved for elliptic, orbits. In a discussion on the subject with sir C.

Wren and Dr. Hooke, the former candidly owned the failure of his attempts to investigate the general problem, and offered to make a present to any philosopher who should solve it within two months. Hooke affirmed that he had some time since discovered the whole, and was in possession of a complete demonstration of it; but when pressed to disclose it, he stated his intention to keep it secret for some time, "that others trying and failing might know how to value it, when he should make it public." It appears, also, that Hooke had frequently, before this, stated his ideas, and had even given some kind of arguments in support of them, to sir C. Wren; but that able mathematician found them totally inconclusive.

Halley, knowing the powers of Newton's genius, and possibly having some intimation that his attention had been turned to this subject, went to Cambridge in August, 1684, for the express purpose of consulting him on the question. Newton showed him the solution of the whole; and, upon his earnest request, promised him a copy of it. He was, however, very slow in fulfilling this promise; and though he allowed Halley to mention to the Royal Society that he had such an investigation complete in its essential points, yet he wished to reserve it for some time, as not considering it yet in a fit state to meet the public eye. Halley privately, and the society officially, urged him at least to enter his discovery on their register, in order to secure his just claim to priority. To this he consented; and promised all expedition in preparing his manuscript. It was not, however, till April, 1686, that it was actually presented to the Royal Society. It comprised very nearly the contents of what now constitutes the first book of the "Principia," that is, the chief of the abstract dynamical part, but not including its actual application to the planetary system. The reading of Newton's communication was followed by some just encomiums upon it, and especially one from sir J. Hoskins, who was in the chair on that occasion, a particular friend of Hooke.

At this the latter took violent offence, and bitterly accused the vice-president of not having done him justice, by recording his own prior discovery of the same truths. After the meeting, Hooke publicly declared to the members, not only that he had made the same discovery, but that Newton had derived the solution of it, at least in part, from him. We have already seen enough to show what ground there was for such a pretence; and the general opinion of the members of the Royal Society appears to have been unfavourable to Hooke's claim. Halley communicated an account of what had taken place to Newton, who wrote a temperate and complete reply. But before his letter was sent, receiving a different, and, in fact, exaggerated account from another friend, he hastily added a very severe postscript. No sooner, however, was he assured in reply by Halley that his remarks were stronger than the occasion really demanded, than, with his usual candour, he apologised, and suggested the addition of a scholium (which now stands in the Principia), acknowledging that in circular orbits Wren, Hooke, and Halley, had already found the law of force inversely as the squares of the distances. This happily had the effect of putting an end to the dispute.

The council of the Royal Society ordered a letter of thanks to be addressed to Newton; and undertook the printing of the whole, as a separate treatise, at their own expense. Newton, on learning this determination from Halley, was still desirous to improve further upon his work before it was printed: and though he had now completed the third book on the system of the world, yet he determined to suppress it. "Philosophy, he says, "is such an impertinently litigious lady, that a man had as good be engaged in lawsuits as have to do with her. I found it so formerly; and now I can no sooner come near her again, but she gives me warning."

In replying to this letter, Halley expresses regret that Newton's tranquillity should have been thus disturbed by envious rivals, but implores and conjures him, in the name of the society, and for the good of science, not to persist in withholding the third book; "especially," he adds, "as this will be the most interesting, and by far the most popular part of the whole, and will render it acceptable to those who will call themselves philosophers without mathematics, which are much the greater number." Newton, thus urged, could not do otherwise than comply. The second book was communicated in March, 1687; the third in the next month; and the whole published in May the same year, under the title of "Philosophiæ Naturalis Principia Mathematica." The first two books bear the more specific title "De Motu Corporum;" and the third, that of "De Mundi Systemate."

Reception of the System of Gravitation.

The reception which these brilliant discoveries met with was that which they so justly merited among those who were really capable of appreciating them; but when we allow for the real difficulty of the subject on the one hand, and the force of prejudice on the other, we shall not be surprised to find the number of Newton's followers at first but few, and the progress of his philosophy slow.

We have before observed how extensively the Cartesian system at this time prevailed, and how firmly it had entrenched itself in the strong-holds of learning. And again, independently of this prepossession, the doctrine of Newton presented a vast extent of ideas of a nature wholly new to the apprehension. The minds of men were almost totally unprepared for such notions as those of central forces, even in abstract conceptions, and much less when connected with the idea of a physical attraction. The great masses of the planets suspended in empty space, and retained in their orbits by an invisible influence residing in the sun, were conceptions far too remote from any thing to which philosophy had hitherto been accustomed, to admit of their being received without hesitation and difficulty; and so entire was the absence of all association with any thing as yet understood, that it was not to be wondered at if even the force of demonstration should fail in securing the adoption of conclusions so unexpected. If we add to this the circumstance, that the establishment of the proofs was unattainable by any previously existing methods of mathematical investigation, and that they had to be carried on by a new sort of geometry invented for the purpose, and which even, when, with all Newton's skill and admirable taste for the old synthesis, it had been accommodated to that style and language, in a way which any other geometer would have shrunk from attempting, was in itself open to considerable appearance of objection, and involved in some obscurity;—putting these considerations together, we repeat, the slow and limited progress of the Newtonian philosophy will not be matter of wonder.

To the continental philosophers, in particular, this last circumstance operated as an obstacle, perhaps, in a stronger degree than with those of England. Nor was the former consideration without its force.

Leibnitz, from his own peculiar metaphysical views, felt great objections to the Newtonian ideas of gravitation, which he considered as a revival of the occult qualities of the peripatetics. Yet, admitting all the ultimate conclusions, he sought to establish them on principles of his own.

Huyghens was unable to admit the mutual attraction of all particles of matter, though he saw that the law subsisted between the planetary masses. John Bernoulli opposed the whole system. Cassini and Maraldi were quite unable to appreciate Newton's demonstrations, and continued to calculate the orbits of comets on the most unfounded hypotheses. Fontenelle continued a decided Cartesian to the end of his life. Mairan was, perhaps, one of the first instances of a convert, after having for a considerable time continued to adhere to the vortices.

The chevalier Louville communicated a memoir to the Royal Academy of Sciences in 1720, which was the first in their collection in which the Newtonian principles are recognised, though not to their full extent. S'Gravesande introduced the Newtonian system in the Dutch universities at a somewhat earlier period. Maupertuis became a convert during a visit to England in 1728; and in 1730 published a philosophical treatise, entitled "Figure des Astres," in which he defended the property of attraction from metaphysical objections.

Objections, in fact, of this class, seem to have been among the most formidable which the notion of attraction had to encounter. It was alleged to be a metaphysical absurdity, as amounting to the assertion "that a body acts where it is not." But the disputants did not perceive that this proposition is only absurd when understood according to a particular interpretation of the terms; and that it is no more than is literally the truth, in respect to almost every species of physical action, since we have reason to believe that no bodies are ever actually in contact.

The philosophy of the age was still involved in that sort of metaphysical realism, which could not adopt a name simply to stand as the symbol for a general class of facts, without supposing some latent reference to causation in the mystified sense then attached to that term. If told that gravitation was nothing but the general fact of a tendency of bodies to approach each other in proportion to the mass directly and the square of the distance inversely, a *philosopher* of that period could not recognise this as worthy the name of philosophy, unless such an announcement was understood as also conveying some reference to a latent something, he knew not what, by virtue of which the effect was produced. Notwithstanding all Newton's care and caution, in warning the readers of the Principia that he meant nothing more than he expressed, and that, when he used such terms as attraction and gravitation, he was to be understood as referring simply to the actual laws which he had established—to the bare facts as collected in their most general forms, — yet it was evident that his readers were in general little prepared for any such simple and intelligible philosophy as this; indeed, men were hardly yet able to believe that any thing intelligible could be true philosophy. It seems highly probable that it was much more in the view of throwing out something which might accommodate itself to the taste of his readers, than of satisfying himself, that Newton proposed certain conjectures about an ether which might be supposed to fill all space, and to be somehow the medium of communication of that sort of action which constitutes gravitation. It is surely precisely in this point of view, without pretending to decide any thing as to the preference for the several hypotheses, and clearly with an equal disregard, on his own part, for them all, that he says so explicitly *, "The term gravity I here use in general to signify the tendency, of whatever nature it may be, of bodies to approach each other; whether that tendency arise from the action of the bodies mutually seeking each other, or by emitted exhalations mutually agitating each other; whether it originate from the action of an ether, or any kind of aerial medium, corporeal or incorporeal, impelling bodies floating in it continually towards each other." This, as well as several other passages in his writings, clearly show that Newton, if he adopted any such ideas, treated them as mere conjectures, the images with which his fancy sometimes amused itself; but he always carefully and resolutely kept them apart and distinct from all real philosophical conclusions.

Another question not a little agitated at that time was, whether gravity be an *inherent* property of matter. This, like most other of those verbal disputes often designated as metaphysical, was not perceived to turn wholly upon the definition of the terms. None of the properties of matter are known except by observation; none are inherent but those which, by observation, are found to be constant and universal. If observation show this to be true of gravity, then it is inherent: but the

* Schol. Prop. 69. lib. i.

phrase was, perhaps, understood differently; and it was, in some of the arguments, meant that gravitation could not be conveyed between two bodies at a distance without some interposed medium of communication. Newton's conjectures, as we have just seen, appear to refer to this view of the subject. The question then was, whether gravitation depend solely upon something in matter, or also partly on the interposed medium; a question which can never be determined until we have proved the existence and ascertained the properties of such a medium. Cotes, in the preface to the second edition of the Principia, stated gravity to be a property of matter as truly such as extension, impenetrability, or any other property, and has been censured for saying so. If he meant it in the former sense, he was undoubtedly correct; and that he did so understand it, appears to us unquestionable from the whole tenour of his observations.*

Though the progress of Newton's philosophy was thus slow on the Continent, it was not unnatural that it should be somewhat more rapid in his own country, and especially in the university to which he belonged. It appears that, before the publication of the Principia, Newton gave, as parts of the course from his professorial chair, lectures on the dynamical views which he was now developing. Whiston, his friend and successor, mentions the fact; and that he heard them through at the time, without at all understanding them. This may be readily conceived, without any disparagement either to the lecturer or auditor: the subject was wholly new, and little susceptible of illustration in a formal public lecture delivered in Latin, and would have appeared very abstruse and uninteresting before its connection with the great truths of the planetary world could have been even guessed at. The Cartesian was certainly at that time the established system in the university; and though Newton continued to explain his doctrines, which, doubtless, after the publication of the Principia, must have assumed a new degree of interest, yet it was some

* Pref. xxii. edit. Jes.

time before they were generally understood or admitted. However, in 1694, Dr. Samuel Clarke, then an undergraduate, kept the act for his bachelor's degree on a question from the Principia. This proves that the new doctrines were then recognised by the university. And, in 1697, the same distinguished individual published a new edition of the popular and standard treatise, "Rohault's Physics," to which he appended acute and elaborate notes, tending to the direct refutation of the Cartesian doctrines of the text, and thus insinuating the new philosophy under the protection of the old. After this period the Newtonian system became generally read and adopted in the university. A knowledge of Newton's discoveries, as well as a just appreciation of them, appears very early to have extended itself to Scotland. In a journal kept by his contemporary at Cambridge, Mr. Pryme, we find it mentioned, in 1692, that Newton had before that time received numerous congratulatory letters on his Principia, "especially from Scotland." In the Scottish universities, the system of gravitation found able and zealous supporters. It was taught by James Gregory at St. Andrew's, and David Gregory at Edinburgh, prior to 1691. In that year the latter was removed to the astronomical chair at Oxford, and was able so far to introduce the study of Newton in that university, that it is expressly mentioned by Whiston, "he had already caused several of his scholars to keep acts, as we call them, upon several branches of the Newtonian philosophy; while we at Cambridge, poor wretches! were ignominiously studying the fictitious hypotheses of the Cartesians." Dr. Keill soon after gave public experimental lectures in Oxford, in which, Desaguliers informs us, "he laid down very simple propositions, which he proved by experiments; and from these he deduced others more compound, which he still confirmed by experiments, till he had instructed his auditors in the laws of motion, the principles of hydrostatics and optics, and some of the chief propositions of sir I. Newton concerning light and colours. He began these courses in Oxford, about the year 1704 or 1705, and in that way introduced the love of the Newtonian philosophy." The subject was new, and the university was not at that time wholly absorbed in the single subject of classical studies. In these lectures Desaguliers succeeded him in 1710, and subsequently gave lectures in London in 1713; where, he tells us, he found "the Newtonian philosophy generally received among persons of all ranks and professions, and even among the ladies, by the help of experiments."

Among the most distinguished converts to the Newtonian doctrines we may reckon Locke; and the manner in which his conviction was formed is characteristic of the clearness of his methodical understanding. His knowledge of mathematics was but quite elementary; he accordingly applied to Huyghens for his *testimony* whether the mathematical deductions in the Principia, considered abstractedly as such, were correct: assured of their accuracy by so eminent an authority, he took their truth for granted, and examined the physical reasoning to which they are applied; this appearing to him satisfactory, he became rationally convinced of the truth of the whole system. Desaguliers mentions this fact as told him by Newton.* It would seem that afterwards he had conversed much with Newton himself on the subject; and, among his papers, was found a letter from Newton, containing a short series of propositions, by which he seems to have endeavoured to give him the direct train of deduction leading to the theorem of elliptic motion, in a form somewhat different from that adopted in the Principia.†

Newton's subsequent Pursuits.

From what we have already seen of Newton's pursuits, it is manifest how fully his time must have been occupied from the year 1665 to 1687. But the unparalleled greatness of his achievements in physico-mathematical science is yet more surprising, when we

* Exper. Phil. preface.

† Lord King's Life of Locke, p. 209.

find that his attention was also, during the same period, directed to a variety of other studies, and that we have evidence of the laborious application with which he pursued them. At a later period he produced a system of ancient chronology, constructed with great research, chiefly from the indications, in ancient history or traditions, of eclipses, the position of the equinoxes, and other astronomical data, from which he calculates the dates. This, he expressly says, was drawn up during his residence at Cambridge, he being in the habit of "refreshing himself with history and chronology when he was weary with other studies." We have, besides other testimonies, that of the journal of Mr. Pryme, who, in 1692, speaks of him as a person "mighty famous for his learning, being a most excellent mathematician, philosopher, *divine*, &c." He had, therefore, been engaged in theological studies long before this time. His critical dissertation on the genuineness of two passages in the New Testament, and his commentary on the prophecies of Daniel and the apocalypse, were not published till many years later; but they are both referred to, as works on which he had been some time engaged, in two letters to Locke, in 1691 and 1692.

Newton had always evinced a taste for chemical experiments. Imperfect as that science then was, and entirely destitute of all fixed principles and defined terms, there were yet many parts of it of practical importance, and which, at the same time, were to be investigated only by trial: such was the subject of the composition of metallic alloys, valuable to Newton, with reference to the construction of specula for telescopes, and on which he had been much occupied. He may also be considered the first who threw out hints of the existence of "elective attraction:" but, perhaps, the most remarkable of his researches of this class are those respecting heat. He ascertained, by several ingenious methods, the temperatures at which certain marked changes, especially melting, took place in different substances; and these being found absolutely invariable points for the same body, the first fixed *thermometric* scale was laid down. Former instruments, as we have remarked, were mere *thermoscopes :* they *showed* when an effect was produced, but they did not *measure* it. Newton was the first to prove that freezing, boiling, melting, and ignition, — in a word, the changes of bodies from a solid to a fluid state, took place always at invariable temperatures : hence, the scale attached to a thermometer was no longer a mere set of arbitrary divisions, but was determined by a fixed boiling and freezing point, the intervening space being divided into any convenient number of degrees. This important idea affords the foundation of our entire knowledge of the phenomena of heat. The exact date of these researches is uncertain. Prior to 1692 he had established a sort of laboratory, in which, in that year, a fire unfortunately occurred, which consumed some of his MSS., said by Pryme (who relates the story) to have contained the results of important experiments on light and colours. Newton himself nowhere makes any direct allusion to the subject ; but, upon the whole, there is much probability in Biot's conjecture, that the MSS. in question may have contained the full account of what is now given in the third book of the Optics, in a confessedly unfinished form, and merely thrown out in the way of queries and conjectures.

We cannot here omit a brief reference to an event in Newton's personal history, which has given rise to considerable discussion. It is known that in 1692–3 he suffered a severe attack of illness, and it has been alleged that it was accompanied by aberration of mind ; some effects of which on his faculties, it has been supposed, were never totally obliterated.

The evidence has been collected, and closely scrutinised.* Thus much is quite clear. Newton's letters, of the date referred to, betray an unhappy state of mental irritation and forgetfulness, almost amounting to incoherence. In a subsequent letter, he speaks particularly

* See Brewster's Life of Newton, p. 222., and the Foreign Quarterly Review, July, 1833.

of his illness, and alleges it in excuse for the expressions in the preceding: he says, that it "had prevented his sleeping for five nights together;" and again, that "he had neither ate nor slept well for a twelvemonth, nor had his former consistency of mind."

His friend, Mr. Millington, in a letter (Sept. 1693) in reply to express enquiries about his state of mind, says, that his illness had left him, "under some small degree of melancholy, but yet I think there is no reason to suspect it hath at all touched his understanding." Mr. Pryme, on mentioning the burning of his papers, says, that "every one thought he would have run mad; he was so troubled thereat, that he was not himself for a month after." Lastly, we have a version of this story related in a diary of Huyghens, as told him by one Colin, in which the case is described in terms expressive of absolute aberration of mind.

This second-hand testimony, it must be observed, is the *only one* which mentions any actual insanity; but it is obvious that the circumstances would not lose in repetition, and we may reasonably allow for some exaggeration: the plain facts are sufficiently clear from the evidence of Newton's own letters, and the positive statement of those who knew his case intimately; nor are they difficult to account for. We have seen enough of Newton's constitutional character to perceive throughout the certain indications of that morbid sensitiveness, which is so often closely interwoven with the finest texture of genius. This, acted upon by harassing illness, the distress arising from the loss of his papers, and the annoyance he appears to have felt about this time at the attempts making by some friends to procure court interest in his favour, was but too easily exacerbated into a state of hypochondriacal irritation, which, for the time, interfered with the full exercise of his intellectual powers, but soon disappeared with the return of bodily health.

As to any supposed permanent ill effects of Newton's malady, all that is alleged is mere presumption, from his not subsequently engaging in any philosophical researches; an inference obviously inconclusive, even were the assumption correct, which, we shall presently see, it is not.

But deeply interesting as such a question must be, from its mere connection with the personal history of so great a man, it has acquired an additional importance from the bearing it has been supposed to have upon the character of some of his writings, and the tone of his opinions. Laplace is said to have dwelt much upon the most unfavourable version of the circumstances, in reference to Newton's theological writings, which he thus insinuated were the production of a deranged, or at least weakened, mind; thus indirectly attacking the credit of religion, to which Newton's name was considered so powerful a support. Biot in defending Laplace, though positively denying one circumstance, yet upon the whole rather seeks to smooth down than to remove the allegations.

Now, we have already noticed the dates of some of Newton's theological speculations, which places them beyond the reach of any such insinuation. His celebrated four letters to Bentley, on the existence of the Deity, were written, the first probably before his attack, and the others within a short time after, or even about the very same period.

M. Biot has, indeed, given a singular turn to his explanation; and endeavours to show the compatibility of rational, and even eloquent, composition with mental aberration, for which he cites the case of Pascal.

But the cases are not in the least similar. Newton's abstruse argumentative discussions do not partake in the smallest degree of the character belonging to the eloquence of insanity. Nor can any one fail to admit these letters to be the production of a man in the full exercise of an acute understanding. The dates of his other productions, before referred to, render it unnecessary to extend the same remark to them; but, whatever may be the merits of their arguments, they certainly exhibit nothing like the reveries of a visionary, or the hallucinations of an enthusiast.

In fact, any one who studies Newton's character must see, that from the first his contemplative turn of mind attached him to religious meditations, and to the study of the Scriptures; and whatever may be the variety of opinions with respect to the merits of his theological speculations, there can be but one as to the steady and rational character of his piety at all periods of his life: and so far as the authority of a name may avail in such a case, Christianity may certainly boast the accession of such a champion to her cause.

But, subsequently to the period of his illness, we find Newton from time to time engaged in various philosophical researches. His scientific employments during the last forty years of his life, were doubtless thrown into the shade by the overpowering splendour of those discoveries whose collected effulgence shines forth in his earlier years; and, in fact, seems to have blinded some writers to all perception of his subsequent philosophical labours; from which they represent him as ceasing altogether during the remainder of his life.

Yet, in 1694, we find him in communication with Flamstead on the subject of the lunar theory, and pursuing the comparison of it with his observations, with a view to its more full developement: and we have before mentioned his solution of the trajectory and other problems; which alone would have sufficed to confer a high mathematical reputation on any ordinary individual.

In 1695, through his friendship with Mr. Montague, afterwards earl of Halifax, Newton was appointed warden of the Mint; and in 1699, master of the same establishment. In this situation, his chemical and mathematical attainments were directly applied to the practical service of the country. The duties of these offices, which he most conscientiously discharged, would probably have interfered materially with his carrying on any extensive philosophical work. Nor must we forget the influence which the controversy respecting fluxions had upon Newton's mind: we may well suppose it would alone have sufficed to repress in him all desire or disposition to enter upon mathematical subjects.

In 1699, the Academy of Sciences at Paris, having formed the design of admitting a limited number of foreign associates, hastened to pay the tribute of respect due to Newton, by enrolling his name among the first.

In 1703 he resigned the Lucasian chair; and in the same year was elected president of the Royal Society, and, in 1705, was knighted by queen Anne.

We have already seen that about this period several of his early mathematical productions were published. The "Arithmetica Universalis" having been at first printed without the author's consent or knowledge, from an imperfect copy, by Whiston, he himself superintended a new edition of it in 1712. At the same time he was engaged in drawing up a large treatise on fluxions; which, however, did not appear till after his death.

In 1713 was published the second edition of the "Principia," edited by Cotes. In this edition, the celebrated scholium at the end was first inserted: but there is evidence, from some letters to Dr. Gregory before 1698, that the substance of it had been then written. The correspondence on the subject of the various alterations and additions made in this edition, between the author and editor, is of great extent and curiosity; and is carefully preserved in the library of Trinity College, Cambridge.

Before this period, it seems that the same idea as that now universally known as the principle of Hadley's quadrant, had occurred to Newton: he has left a description of it, though it does not appear that he actually reduced it to practice.

The third edition of the "Principia" appeared in 1725, under the care of Dr. Pemberton. The scholium respecting the discovery of fluxions before referred to was omitted in this edition. This has been by some ascribed to the author, and by others to the editor; and has been represented as showing a pitiful hostility against Leibnitz in retracting the acknowledgment of his claims: it is forgotten that it also contained the record of his own priority. The real motive was obviously the dislike he felt of all reference to the subject, as well as the consideration that it was not essential to the work.

From all we have seen of Newton's disposition, it was, perhaps, little to be expected that he should have stood forward as one of the defenders of the University privileges against the encroachments of James II. in 1687; and that he should have subsequently represented the University in two parliaments, is more a matter of surprise, than that he should have failed in the election of 1705: he was evidently little fitted to take a part in the miserable turmoils of political life. But though not in parliament, he was on one occasion engaged in a public measure more allied to his pursuits.

In 1714, on the strong representations of a petition from various merchants and navigators, the House of Commons appointed a committee to examine the propriety of a measure for giving rewards for the discovery of the longitude at sea. Some of the principal men of science were examined before this committee, and Newton among them. On this occasion he delivered his opinions in a written paper, which, though drawn up in a brief style, is perfectly clear to any one at all conversant with the subject. In it he states the different methods proposed, and the practical objections to them. Of the method by a watch to keep time exactly, he simply says, "such a watch hath not yet been made." That by the eclipses of Jupiter's satellites he considers impracticable at sea: for that by lunar observations he conceives no existing tables exact enough; and a fourth plan, then proposed by Mr. Ditton, seems to have been merely a sort of mode of keeping an account of the ship's course rather than finding the longitude: which nevertheless might be in some cases resorted to.

Newton read this paper as his evidence to the committee. Whiston (who was also one of the persons examined) says, that nobody understood it; and after sitting down he obstinately kept silence, though pressed to explain himself more distinctly: at last, thinking the scheme was likely to be rejected, Whiston ventured to say, that Sir Isaac was afraid of compromising himself, but really approved of the plan; and Newton repeated the same declaration. The committee however recommended the measure; which was finally carried, and led to the formation of the Board of Longitude, the tribunal to which all projects on this important subject were referred; by which numberless extravagant and fallacious schemes were exposed and rejected, and those deserving attention subjected to careful examination, and if found meritorious, in proportion to their claims, were recommended as worthy of national reward and encouragement. The public utility of such premiums is too obvious to need remark; nor is the necessity for a competent and disinterested tribunal less manifest, to decide upon their merits. To recount the actual advantages which the country has directly received in the most tangible shape of practical utility from this system, belongs to a future period of our history.

But to return to Newton: his conduct on the occasion mentioned has been represented as weak and puerile, and tending to confirm the belief in the alleged aberration of his mind some years before. The plain facts, however, show nothing of this kind. That his paper should not have been understood by gentlemen of the house of commons at that period, is not at all surprising; and that, in giving evidence before a committee, he should scrupulously wish to avoid in any way committing himself, would be but natural in any one, and still more so in a person of Newton's singularly reserved disposition. It would almost seem as if M. Biot imagined him on this occasion to have been sitting as a *member* in the house, in which case his conduct might have been thought singular; but when we recollect the actual circumstances, it must surely appear in a very different light.

From the appointment which Newton held, he was necessarily introduced at the court of George I., where his brilliant and general reputation procured him, not merely the favour of his sovereign, but that species of real attention and respect, which was the due homage paid even by the highest rank to intellectual pre-eminence. This was peculiarly shown by the high esteem in which he was held by Caroline, princess of Wales (afterwards queen), whose highly cultivated taste inclined her to the study of philosophy, and who took a particular delight in conversing with Newton and corresponding with Leibnitz. It was in the course of this correspondence that Leibnitz took occasion, not merely to object to the doctrine of attraction, as physically and metaphysically untrue, but also to attack the principles of Newton's philosophy, as involving materialism, and being injurious to the cause of religion. Absurd and unfounded as such attacks must have appeared to any one who had looked into Newton's works with the smallest portion of common candour, and easily as in this instance their motive might be suspected, yet these insinuations having become the subject of conversation at court, the king expressed a wish that Newton would reply to them. Reluctantly (as we can well imagine) he obeyed, contenting himself with taking the mathematical part of the question, while he entrusted the metaphysical to his illustrious friend, Dr. S. Clarke.

In 1718, at the request of the princess, and for her perusal, he produced the abstract of the chronological enquiries he had formerly been engaged in: but, with the fatality which seemed to attend all his works, an imperfect copy of this got abroad, and was published surreptitiously at Paris, whence another controversy was excited, into which we shall not enter. It led to his subsequent enlarged work on the subject, which was published after his death, in 1728.

The last twenty years of Newton's life were spent almost wholly in London. In 1722, and the following years, he suffered from several attacks of illness, and in 1725, from gout. But in the intervals his mind reverted occasionally to philosophical topics; and we have on record a remarkable conversation held with his nephew, Mr. Conduit, after his recovery from the last-mentioned attack, in which he developed several ideas respecting the nature of comets. But his time was chiefly occupied with the study of the Scriptures. On the 28th of February, 1727, he imprudently attended a meeting of the Royal Society; the fatigue brought on a renewal of his former attacks, which were pronounced to be from calculus. After several returns of pain, with intervals of ease, he became insensible on the 18th of March, and died on the 20th, in the 85th year of his age.

His body lay in state, and a public funeral took place in Westminster Abbey; where a handsome monument was afterwards erected to his memory.

Newton's Philosophical Character.

The remarks which we have made, as occasion required, in the course of the review of Newton's discoveries, will have sufficed to point out the leading peculiarities of his character, and the distinguishing features of his method of philosophising. In the former, we have had abundant opportunity of observing those singular combinations which were unquestionably connected, in the closest manner, with the powers of that pre-eminent genius with which he was endowed. Many of these have, till of late years, been little known or remarked; and it is not surprising that they should have been misconceived, especially where a theory was concerned in the view taken of them. The extreme repugnance of Newton's mind to the publication of his researches, the weariness and disgust which he, more than once, speaks of feeling towards scientific subjects, and the strong revulsion of his mind towards those "mystical fancies," as he himself calls them, in which he delighted to lose himself; the "refreshment" he found in the driest details of ancient chronology; his excessive sensibility to the annoyances of controversy; his preference of tranquillity to every other consideration; his positive determination, on more than one occasion, to give up all scientific labours; his constant refusal, during the later years of his life, to answer enquiries put to him on mathematical subjects, which he always met by referring the enquirer to other mathematicians, especially to M. de Moivre, who, he observed, "understands those subjects better than I do;" and lastly, what we must call his unreasonable disparagement of his own discoveries, in the remarkable declaration, — "I know not what the world may think of my labours, but to myself it seems that I have been but as a child playing on the sea-shore; now finding some prettier pebble or more beautiful shell than my companions, while the unbounded ocean of truth lay undiscovered before me:"— such were the instances in which some of the marked peculiarities of his character were developed; and these circumstances put together cannot but strongly impress us with the belief, that such peculiarities were, by some of those unknown links and mysterious sympathies which connect the phenomena of man's moral and intellectual nature, intimately wound up with the operations of Newton's mighty genius, and connected with the exercise of those surprising faculties which enabled him to comprehend in a single grasp the most extensive theories, and to bring together in the closest union the most widely separated elements of truth. In the facility with which he seems to have surmounted difficulties, almost unconscious of their magnitude, we may, perhaps, perceive some explanation of the little value he was led to set upon what he had done. Celebrity attended him unsought; and, had he cared for it, his morbid reserve more than counterbalanced the desire for fame, and induced him to keep his discoveries private.

It is characteristic, perhaps, of a genius of the highest order to be insensible to its own superiority. It pursues its lofty excursions, and attains its highest conclusions, by a method which seems to itself no more than assiduous investigation, but which to others appears intuition. It follows a train of reflections obvious to its own apprehension, and, therefore, supposes it is only following a train of obvious reflections. What to genius itself seems mere ordinary thinking, appears to ordinary thinkers the inspiration of genius.

Of this class was the mind of Newton: he accomplished his great discoveries almost unconscious of their greatness, and ascribed them all to mere patient thought. But his patient thoughts were the highest flights of genius. What to him was a mere ordinary deduction, would have seemed to an inferior mind a gigantic effort; to the successful issue of which it would have attached proportionate importance. Newton "waited till light was shed upon his subject:" other men might wait without the light appearing.

"To his important inventions in pure mathematics," says professor Playfair, "Newton added the greatest discoveries in the philosophy of nature; and, in passing through his hands, mechanics, optics, and astronomy were not merely improved but renovated. No one ever left knowledge in a state so different from that in which he found it. Men were instructed not only in new truths, but in new methods of discovering truth: they were made acquainted with the great principle which connects together the most distant regions of space, as well as the most remote periods of duration; and which was to lead to future discoveries, far beyond what the wisest or most sanguine could anticipate."*

In fact, in whatever light we view him Newton appears equally remarkable. In the department of pure experimental enquiry alone, as in his optical researches, he evinced a degree of skill and accuracy, of patience and sagacity, which place him at the head of experimentalists. Considered merely in respect to scientific

* Dissertation, p. 444.

manipulation, he has had few equals even down to modern times; and in regard to extent and importance of results, his experimental discoveries alone may challenge preference over those of almost any other philosopher. In abstract mathematics, again, distinct from all the physical branches, he had confessedly no rival but Leibnitz; and he, individually, can hardly be said to have done more for the calculus than Newton did. And while the latter certainly displayed his powers equally in the geometrical and analytical styles, the former almost wholly confined himself to the analytical.

In the applied departments of mathematical science, Newton's lofty superiority above all other philosophers (except his modern rival Laplace) is universally admitted. The whole range of physico-mathematical dynamics is a science nearly of his own creation; and its application to the actual phenomena of the planetary system, which observation had classified by inductive laws, is due to the sole and unaided powers of Newton's master-mind.

Pre-eminent, then, in each department taken singly, he would have shone brightly conspicuous had he done nothing in any other. But when we contemplate him as alike unrivalled in all at once, we feel at a loss how to express adequately the wonder and admiration with which these transcendent powers impress us; and are convinced that the well-known poetical eulogies which have been bestowed on him are scarcely overstrained. Nor is this a vain and idolatrous homage paid at the mere shrine of a fellow-creature, but will rather, by the thoughtful and discriminating, be turned to the purposes of higher and more important reflections. Newton himself stands as a phenomenon in the intellectual creation; and the consideration of such phenomena may lead us into an unbounded train of contemplation on the great moral designs to which they are destined to contribute.

SECTION II.

THE DISCOVERIES OF NEWTON'S SUCCESSORS.

In laying out our plan in our introductory remarks, we professedly designed to carry down the history of the progress of discovery to our own times; but so numerous have been the topics which, from their importance, we could not omit, and, from their high interest, we could not mention without at least allowing ourselves some small latitude for remark and illustration, and this especially in reference to those grand discoveries which have occupied us in the foregoing section, that we have now the mortification of finding ourselves trenching upon the extreme limits which the nature of this treatise absolutely imposes upon us without having accomplished by any means the whole of our design: and in consequence, though with deep regret, we are compelled to conclude with a most brief and imperfect outline, a bare enumeration of the principal names and discoveries which have adorned the age since Newton, and which stand recorded mainly as forwarding and completing the investigations which he began.

Progress of Mathematics.

We have seen enough of the controversy respecting fluxions, to admit that the spirit in which it was carried on was not less disgraceful to science, than unaccountably at variance with that dispassionate tranquillity of investigation which is usually supposed to be its distinguishing characteristic. But highly to be deprecated as was the temper of this dispute at the time, it was, perhaps, yet more to be regretted in its consequences: for thus, from the era of Newton's discoveries, we have to date a period in the history of the science, distinguished by a remarkable and wide alienation between the English and foreign mathematicians, equally singular and equally to be lamented in its origin, its continuance, and its effects. Arising out of this controversy,—one of the most bitter and keenly contested, perhaps, which any disputed topics (scarcely excepting those of theology) have ever called forth,—it led to an almost total cessation of that mutual interchange of information and opinions on scientific subjects, which is always so highly beneficial to both the parties engaged. Long after the actual question of the original controversy had fairly worn itself out, the jealousy which was felt, and the line of separation which had been drawn, between the British and continental mathematicians, were maintained in their full force, and produced the most pernicious effects on science. Each party became the exclusive supporters of the system taught by the two great luminaries of their respective countries. The British mathematicians, in particular, adhered with most rigid pertinacity to the very letter of Newton's methods; and were, with few exceptions, completely ignorant both of the original investigations of the other party, and of the improvements upon them which were being rapidly introduced.

The difference in name and notation between the two methods, though in itself a trivial circumstance, was yet far from unimportant in some of the consequences which may be fairly traced to it. It tended in some measure to foster and increase the dissension between the two schools, and their ignorance of each other's researches; while the diversity itself between the two methods, though in reality little more than nominal, became also a topic of no small dispute and controversy.

But much as these differences were on all grounds to be lamented, the loss in point of scientific advantages, it must with shame be confessed, was almost entirely on the side of Britain. The English mathematicians seemed to have been so dazzled with the splendour of Newton's discoveries, that they never conceived them capable of being extended or improved upon. They regarded his inventions with a veneration almost superstitious, and adhered to them in the very form and literal method in which he had delivered them. The natural consequence was, that the method of fluxions, as adopted in England, made comparatively very little progress: it was applied to few problems beyond those which constituted its most ordinary and obvious applications, and in the inverse calculus few general principles were developed; and the whole of this branch of the science (by far the most important in its application) was left in a state differing little from a confused miscellaneous assemblage of individual propositions, referring to particular cases, which admitted of particular solutions.

A very slight acquaintance with the writings of the most eminent British mathematicians of the age succeeding that of Newton (with perhaps one or two honourable exceptions), would suffice to convince any one of the justice of the preceding remarks. The names of Gregory, Sanderson, Waring, Emerson, T. Simpson, R. Simson, and Stewart, among the most distinguished of this period, stand connected, indeed, with much which is highly valuable in geometry and algebra; but with little which bears upon the extension of the calculus, and such generalisations of its principles as increase our command over the combinations of quantity, and supply the means of investigating the more complex and abstruse problems of the theory of the universe. Let us not, however, be supposed to undervalue the labours of these distinguished men; which, abstractedly considered, were of the highest excellence. They dedicated consummate talents, and much time and labour, to the restoration and elucidation of the ancient geometry, which they effected in the most truly classical style; and to the inventions of solutions, and the composition of treatises conceived in the most rigorous spirit of its demonstrations. Others effected considerable improvements in many of the methods of algebra; and exercised their ingenuity in the invention of a vast mass of detached fluxional problems, and the suggestions of neat and elegant solutions.

Roger Cotes was a striking exception to such remarks;

and was, certainly, one of the most brilliant instances of mathematical genius among the contemporaries of Newton. He was appointed Plumiar professor of experimental philosophy at Cambridge, in 1705; and died in 1716, at the age of thirty-three. His great mathematical work, the "Harmonia Mensurarum" (published in 1722), treats of the quadrature of curves by several highly ingenious methods, derived chiefly from extensions of those employed by Newton. It contains, also, various improvements in the rules for integration, and indicates methods of finding various large classes of fluents; besides comprising some original properties of the circle, and other geometrical speculations. De Moivre, in his Miscellanea Analytica, greatly improved upon the methods of Cotes.

Dr. Brook Taylor was another original and profound writer of this period, who, in his "Method of Increments," published in 1715, added a new branch to the analysis of variable quantity. According to this method, quantities are supposed to change, not by infinitely small, but by finite increments: but the whole system, both direct and inverse, bears in other respects a close analogy to the fluxional. The author made many ingenious applications of this calculus, both to geometrical and physical questions, and especially to the summation of series: he has been censured, however, for obscurity in the manner of announcing the principles of the method. This fault was removed, and the whole theory explained with great clearness, by M. Nicol, in a series of memoirs communicated to the Academy of Sciences, between 1717 and 1727.

The discovery, however, by which Taylor is most universally known is that of a single formula, delivered in no very conspicuous place in the work on increments, and passed over by the author as of little value, but which, nevertheless, contains as it were the essence of almost the whole theory of functions. By some writers it has been made the foundation of the whole differential calculus; by all, it has been recognised as supplying the most comprehensive representation of the general form in which functions of all kinds may be developed; and though doubts have been raised as to its affording, in all cases, a finite representation of the value which the function assumes corresponding to a given increase in the variable, yet its essential excellence lies in indicating the relation which invariably subsists between the developed form of the function and the successive orders of its differential coefficients.

Nor ought we here to omit the names of Stirling, the acute commentator on Newton's "lines of the third order," and the real inventor of the analytical formula known as Maclaurin's theorem; nor the last-named mathematician, as an acute and able expositor of the doctrines of analysis; which he also successfully applied in pursuing some of the problems arising out of the theory of gravitation.

We have noticed the early attacks made on the principle of the calculus; the fluxional system had now to sustain another and more formidable attack, from bishop Berkeley, in 1734. Though eminent as a metaphysician, his mathematical qualifications were not of the highest order: yet the argument contained in "The Analyst" is carried on with great acuteness and plausibility. The author contends, that the fundamental idea of supposing a finite ratio to exist between terms absolutely evanescent,—"the ghosts of departed quantities," as he, with inimitable humour, calls them,—is completely absurd and unintelligible: and it must be confessed, that the *language* in which the fluxional principle was stated really laid it open to objections of this kind. A reply was written by Dr. Jurin, under the name of Philalethes. Several other publications also appeared on both sides. Robins and Maclaurin are considered to be those who made the most satisfactory defence of the principles of limiting ratios. But though the subject was extensively discussed, and the "Residual Analysis" of Lauden, in 1758, exhibited an analogous principle, in some measure divested of the objections, yet the point cannot be said to have been completely cleared up till D'Alembert showed the real application of the principle of limits in its simplest sense; and, finally, Lagrange, in his "Théorie des Fonctions," discarding all idea both of infinitesimals, and even of limits, reduced the whole into an elementary, though somewhat prolix, algebraical investigation, by the developement of functions in serieses: while the valuable essay of Carnot on the metaphysical principles of the infinitesimal calculus, tends more to clear the method of limits from objections than, perhaps, any thing that has appeared on the subject; and the researches of professor Woodhouse have placed the general principles of the methods of analysis on a secure basis of demonstration. To his writings we owe the first introduction of the inventions of the continental analysts into this country.

Of the vast improvements effected in every branch of analytical mathematics by the foreign mathematicians, it would here be a vain attempt to convey an idea. We have mentioned the names of the earlier of that illustrious school, who reared so large a portion of the superstructure of the science; and, in more modern times, they have not been wanting in successors truly worthy of them. The names of Legendre and Lacroix, of Abrogast and Poinsot, of Garnier and Cauchy, are a few only among a multitude of those most highly distinguished in the different branches of pure analytics; while many of those whom we have mentioned in other departments were likewise no less eminent in this. Our own country has hitherto had to boast but a very small list of names in the higher branches of mathematics; but, since we have had the wisdom to give up a few of our absurd exclusive prejudices, and to cultivate the methods in which our continental neighbours have achieved such distinguished renown, we have begun to exhibit an array of analytical talent to which we may look forward with confidence for the happiest results.

It was not, however, till at a comparatively very recent period that there appeared any marked change in the state of mathematical learning in England. It is not twenty years since we have begun to perceive that we were far behind all the rest of Europe in these sciences; not from want of abundance of first-rate talent, but from a misapplication of that talent to unworthy objects, or at least to such as were not of a nature calculated to lead to any great advance in the state of knowledge. Within the period named, the works and inventions of the great continental mathematicians have been introduced and studied; and it is needless to say, no sooner were they understood and appreciated, than they have called forth, in turn, an ardent spirit at least for the cultivation of these methods,—though, perhaps, that spirit has been shown rather in detailed improvements and amended treatises, than in any extensive original researches. Yet these have not been altogether wanting; and we need not fear to place in competition with the inventions of the Continent the analytical researches of Messrs. Woodhouse, Bromhead, Ivory, Babbage, and Herschel.

Dynamics.

In the application of mathematics to physical investigations, the English philosophers of a former age were not more happy. Here, again, with a very few exceptions, an entire devotion to the letter of the "Principia" seems to have impressed Newton's followers with the notion, that nothing further was to be effected beyond what was accomplished in that stupendous work. The "Principia," indeed, is a work which will stand as long as science shall exist, an enduring monument of the transcendent genius of its author. The truths which it establishes are unquestionably those on which the whole system of physical astronomy is securely founded; and the manner and style in which they are delivered afford the most indisputable evidence of that superiority of talent, which, by the application of such extremely simple principles, could educe such surprising conclusions. It is a work which, both for matter and style, stands forth finished and complete in itself. But it is no disparagement to its merits to admit, that when further and more complex questions arose to be determined, the work of solving them, and reducing them into a system, would not be capable of being performed by the same means, or presented in the same form.

The methods of ultimate ratios and fluxions seem to have been, as it were, created for the express purpose of conducting Newton to the analysis of the motions of bodies acted upon by gravitating and central forces, and the solution of the great problems arising out of it; but they were not so applicable when the limits of these branches of enquiry became extended: and, to investigate more complex relations in the physical system of the world, more refined, more general and comprehensive methods were required. Hence an exclusive devotion to the methods, however excellent, invented and applied by Newton, was not likely to assist his disciples in carrying farther the discoveries he had begun; and while they confined their attention and acquirements to the knowledge of his writings and methods, they were not in a condition to enlarge the boundaries of science, and were not availing themselves of those more powerful instruments which were required for the work, in proportion as it became more difficult and extensive.

Newton had stretched the powers of geometry to an unprecedented degree, and had successfully applied it in extending the dominion of science over the system of the universe; but such a mode of proceeding would by no means be applicable in all cases: the generality of those engaged in such researches would require easier methods, and more certain and systematic rules, to assist them in their investigations and computations. Geometry, as wielded by Newton, was like the sling and the stone in the hand of David; a weapon with which no ordinary combatant would choose to attack a giant.

Adhering, then, to such methods, the British philosophers in general did but little in the way of prosecuting dynamical enquiries; while we find the continental analysts busily engaged, and rapidly extending their researches in proportion to the facilities supplied to their hands by the increasing powers of the new calculus. The discussion which arose in 1724 respecting the true measure of force, and which, in the end, was clearly seen to be a mere dispute about terms, had the effect of calling forth many valuable researches; though, when the English philosophers, Maclaurin, Stirling, Clarke, and others, took a share in it, and opposed Bernoulli, Herman, S'Gravesande, and Muschenbroek, the discussion assumed too much of the angry tone of the fluxional controversy, the bitterness of which had not worked itself out till a later period. The controversy may be considered to have ended with the publication of D'Alembert's "Dynamique," in 1743.

The general character of the improvements effected in mechanical and hydrostatical science, about this period, was marked chiefly by the increasing developement of the analytical formulæ. To this has been owing the great advances in the investigation of principles and laws, which, perhaps, may be dated from Bernoulli and D'Alembert, and which have been so advantageously pursued by a long train of their illustrious successors, and perfected in the "Mécanique Analytique" of La Grange. These, however, are precisely those of which it is the least in our power, within our limits, to give any adequate notion, and of which an enumeration, without such illustration, could answer no useful purpose.

Of Lagrange, however, we must add, that the distinctive mark of his genius consists in the unity and grandeur of his views. He attached himself wholly to a simple, though just and highly elevated thought. His principal work, just mentioned, refers all the laws of equilibrium and motion to a single principle: and, what is not less admirable, it submits them to a single method of calculation, of which he himself was the inventor. All his mathematical compositions are remarkable for their singular elegance, for that symmetry of form and generality of method which constitutes the perfection of the analytical style

Physical Astronomy.

Physical astronomy was a science of purely British origin, yet, after the death of its founder, it had very few cultivators in England, and scarcely any advances of importance were made towards the investigation of the more abstruse consequences of the law of gravitation, and the more complex phenomena of the system of the world; or in providing general and comprehensive methods suited to computing the laws and consequences of such actions.

On the continent of Europe, during the same period, widely different was the condition and progress of this science. The Newtonian theory of gravitation, though at first admitted with some delay and hesitation, was at length triumphant; and no sooner were its excellencies perceived, and its conclusions assented to, than the genius of the continental mathematicians was immediately directed to the extension of its applications, and the improvement of the methods required for those applications.

To complete the theory of tides, and to investigate the lunar inequalities, the motion of comets, and the figure of the earth, were immediately among the objects which gave employment to the talents of the great ornaments of the continental school: and the success of their investigations on these subjects kept exact pace with the increased powers of the instruments of research with which they had provided themselves from the enlarged resources of the new calculus. Every year added new inventions and discoveries, and extensions of former theories, to the successes of science. The fabric of physical astronomy was rapidly extending, upon the plan which Newton had laid down; and the work advanced as might be expected, precisely in proportion to the more powerful means of carrying it on, which were put into the hands of the workmen,—in proportion to the extension and generalisation of the processes of the analytical calculus. A long and brilliant catalogue of names might be adduced of those who united the extension of abstract methods with the prosecution of physical research, from the days of the Bernoullis down to the present times.

The innumerable and complex consequences of the principle of gravitation formed an immense legacy of research bequeathed by Newton to his successors.

The first who commenced working upon them was Clairaut, in his celebrated memoirs addressed to the Academy of Sciences in 1743, 1745, and 1754. These, in fact, contain the developement of the principle of the perturbations, known by the name of "the problem of the three bodies."

Nearly contemporary with him were Euler, Mayer, Thomas Simpson, and D'Alembert, whose united talents were also devoted to the further prosecution of the same difficult but highly important and interesting subjects, the lunar and planetary theories, that is, the account of all the inequalities of their motions as affected by the disturbances of their mutual attractions.

Besides a host of other philosophers (with the exception of Maclaurin, chiefly it must be confessed continental), who were engaged in carrying on and perfecting the various elaborate details of these subjects, and the computations they involved, we now find two great luminaries of science beginning to appear on the intellectual horizon, the latter of whom has become the sole but worthy competitor with Newton for the honours attending the completion of a perfect mathematical and dynamical system of the mechanism of the heavens: Lagrange and Laplace.

The former having succeeded in developing and verifying the dynamical truths which have become the basis of the whole analytical system of forces, applied them to the system of the world; and laid down the principles on which the invariability of the mean distances of all the planets may be inferred, in the Berlin Memoirs for 1776; besides pursuing a variety of other investigations, of which it would be useless here to give the titles.

Laplace.

One of the surest tests of the truth of a philosophical theory, is that applying in the first instance to the general explanation of a whole class of phenomena, it shall afterwards be found to admit of exact application to every point of detail which may subsequently be discovered, by direct and simple modifications exactly accordant with the original principle, and legitimately deducible from it. To such a test the theory of Newton had to be subjected in the hands of his successors, and we shall observe the complete and triumphant manner in which it passed through the ordeal. Among those who conducted these enquiries, Laplace stands pre-eminently conspicuous; and to him alone is the credit due of fully perfecting the work which Newton had begun. Pursuing the subject of the planetary inequalities, he was enabled to trace the principle of gravitation to its remotest consequences, and to confirm the universality of its law.

The acceleration of the moon's motion was, perhaps, the first great problem which engaged his attention. This inequality, discovered by Halley, unlike the others in not recurring at short intervals, was called by contradistinction *secular;* but Laplace showed that it also would have a limit, in tracing it to the action of causes which are consequences of the universal principle of gravitation. The inferences, indeed, which he deduced, by remote trains of reasoning, from the principle of gravity, are often of a most surprising and unexpected nature: — such as the conclusion of the absolute invariableness of the earth's rotation, following from the theory of the moon's motion, and the deduction of the compression of the terrestrial spheroid from the same theory; the effects of attraction due to the spheroidal figure being exhibited in certain parts of the lunar inequalities. The great inequalities of long period of Jupiter and Saturn, the libration of the satellites of Jupiter, and especially the tides, display to the greatest advantage the wonderful powers of investigation with which he was gifted. The latter subject, in particular, he examined with reference to the principle of the stability of the great phenomena and laws of the system, however enveloped in immediate variability, and perpetually changing inequalities. The causes which disturb the equilibrium of the ocean are subject to boundaries which cannot be passed. The specific gravity of the sea being much less than that of the solid globe, it follows that the oscillations of the ocean are always comprehended between very narrow limits, which would not have happened, had the fluid spread over the globe been much heavier. The denser mass forming the central part, is the condition which determines the stability of the seas. We cannot, in fewer or more expressive terms, do justice to the importance of these principles, which Laplace has the merit of completely establishing, than by adopting the eloquent language of Baron Fourier, who thus describes them: — "Nature in general keeps in reserve conservative forces, which are always present, and act the instant the disturbance commences, and with a force increasing with the necessity of calling in their assistance. This preservative power is found in every part of the universe. The form of the great planetary orbits, and their inclinations, vary in the course of ages, but these changes have their limits. The principal dimensions subsist; and this immense assemblage of celestial bodies oscillates round a mean condition of the system, towards which it is always drawn back. Every thing is arranged for order, perpetuity, and harmony. . . . . Whatever may be the physical cause of the formation of the planets, it has impressed on all these bodies a projectile motion in one direction round an immense globe; and from this the solar system derives its stability. Order is here kept up by the power of the central mass: it is not, therefore, left, as Newton himself and Euler had conjectured, to an adventitious force to repair or prevent the disturbance which time may have caused. It is the law of gravitation itself which regulates all things, which is sufficient for all things, and which every where maintains variety and order. Having once emanated from supreme wisdom, it presides from the beginning of time, and renders impossible every kind of disorder. Newton and Euler were not acquainted with all the perfections of the universe: whenever any doubt has been raised respecting the accuracy of the Newtonian law, and whenever any foreign cause has been proposed to explain apparent irregularities, the original law has always been verified after the most profound examination. The more accurate astronomical observations have become, the more conformable have they been to theory." * And to this able and eloquent exposition, we will add the remark of another eminent writer, whose words hardly admit of translation: —

"La stabilité du système solaire est donc à jamais assurée: les orbites des planètes dans les ages futurs ne pourront que s'aplatir légèrement en conservant les mêmes grandes axes, et les plans de ces orbites ne feront que de petites oscillations autour d'une position moyenne: immenses pendules de l'éternité, qui battent les siècles comme les nôtres battent les secondes." †

The collected description of all the more recent researches and discoveries in physical astronomy, united into a connected system with the fundamental truths established by Newton, and all demonstrated by a uniform method of analysis, formed the idea of that vast and splendid project, conceived by Laplace, and to the execution of which his life was devoted; it was realised in the most perfect manner by the publication of the

* Fourier, Eloge de M. de Laplace, before the Academy of Sciences, June 15. 1829.

† Pontécoulant, Syst. du Monde, Int. xvi. See also ASTRONOMY, Cab. Cyclo. ch. xi.

"*Mécanique Céleste*," commenced in 1799; in its first volumes embracing a large portion of the subject, and followed, at intervals, by successive supplements, in which the various details of different important branches were followed up. The publication of this immense monument of the powers of the human mind was indeed an epoch in the history, not only of science, but of our species. Fortunately, it is rendered unnecessary for us here to attempt any detailed description of its contents, since such an account is furnished to every English reader in the profound and luminous work of Mrs. Somerville on the mechanism of the heavens.

We must, however, subjoin one or two general remarks.

It is true we cannot affirm of Laplace, that he created a science entirely new, like Galileo or Archimedes; nor that he struck out original ideas, adding an entire calculus to mathematical methods, like Descartes, Newton, or Leibnitz; nor, again, that he was the first to transport himself into the heavens, like Newton, and carry the terrestrial dynamics of Galileo into the farthest regions of the planetary world; but Laplace collected, combined, and arranged, all that had been previously known on these subjects, under the most grand and comprehensive generalisations: he traced out all the remotest consequences of the great principles already laid down, and brought under the dominion of analysis an immense range of physical truths, which did not appear at all likely to be subjected to any such system. Such, however, was the powerful command with which he wielded at pleasure the irresistible weapons of the calculus, that he at one stroke subjugated the most apparently insuperable difficulties.

We have spoken as yet only of his labours in physical astronomy; but, subordinate to these, he pursued a variety of other enquiries. We owe to him almost the entire development of that highly curious and important subject, the calculation of *probabilities*; a doctrine which applies to that vast range of the objects of our knowledge which are placed beyond the pale of absolute certainty. To supply fixed principles on which the probability of events may be estimated, and even expressed by mathematical formulæ, is of all other inventions one of the most happy and important: it tends to put us in possession of the most sound principles on which to discriminate truth from error; and embraces as well the chances of future contingencies, as the probabilities of error in the present and the past, from the fallibility of observation and testimony; and has been well designated by an able writer, "a fortunate supplement to the imperfection of our nature." The idea was first started by Pascal; it was successively improved upon by Bernoulli, Euler, and Lagrange; but owes its full development entirely to Laplace.

Laplace, in conjunction with Lavoisier, made extensive researches on the theory of heat, and especially the dilatations of bodies: his investigations on the atmospheric refraction, on capillary attraction, on the barometical measurement of heights, on electricity, on the velocity of sound, on molecular action, and on the properties of gases, most of which occupy parts of the different supplements in the Mécanique Céleste, show at once how multifarious were his enquiries, and with what unity of purpose he carried them on, at length embodying them under the laws of a comprehensive analysis, to stand as parts in the great theory of the material world which he was engaged in completing; and which, in its systematic uniformity, enables us to gain a nearer approach to the apprehension of that all-perfect *uniformity* of design which we cannot doubt is the real pervading principle of the whole fabric of nature: which, we must confess, no human powers will probably ever be able completely to analyse, but which, at every approach we can successively make, appears more and more distinctly invested with this never-failing attribute.

As a writer Laplace shines among his eloquent contemporaries with unusual brilliancy. His more popular productions on philosophical subjects, are composed with an enviable union of the most complete perspicuity, with all that elegance of style which, without being disfigured by attempts at ornament unsuited to the subject, at once illustrates it, and renders it inviting to the general reader. Like his great predecessor Newton, he evinced most sensibly the humility of true intellectual superiority; and the last sentence he uttered was, "What we know is little, and what we are ignorant of is immense."

Laplace, born in 1749, was at an early age distinguished for his mathematical abilities; and no sooner had he been named professor of mathematics in the military school at Paris, than he began to form that lofty project to which he devoted the entire energies of his genius, and the whole remainder of his life. The great work he planned and executed has been termed the Almegest of the eighteenth century; but, relatively to the age, it is a far more wonderful production than its prototype. The original discoveries of the author embodied in it, are the solutions of the highest problems which the mechanism of the universe presents; many of them had baffled the attempts of all preceding mathematicians, and some received not only their solution, but their first suggestion, from Laplace himself. He was fortunate in enjoying, through a long life, the situation and the means for the uninterrupted prosecution of abstruse research: his residence at Arcueil was the centre of attraction to all the mathematical philosophers of Europe. He died in 1827, when his varied labours had been brought nearly to their complete perfection.

The works of Laplace will be his enduring monument to the world; but to the astronomer, even of the remotest age, there will be a memorial of a loftier, and yet more enduring kind, perpetually exhibited. Those periods in all the irregularities of the planetary motions by which the stability of the system is secured, and which he has so surprisingly established, will be recognised in the observations of the most distant epochs, and the memory of Laplace will be cherished in their recurrence. In the lunar motions will be found those changes which will be the fulfilment of his predictions; and the completion of the long periods of the great inequalities of Jupiter and Saturn, will recall his investigations who was the first to examine and explain their laws.

The whole course and manner of his life was regulated with a sole view to the purposes of philosophic investigation. He devoted the energies of his existence to his labours in interpreting the mechanism of the material world; he dedicated to science a life of research; and science, in return, has conferred on him an immortality of fame.

Among the most valuable labours of his successors we may enumerate the publication of the "Theoria Motûs," &c. by Gauss, in 1809; the masterly researches on the attraction of spheroids, in the same and following year, by Mr. Ivory (almost the only British philosopher who, till very lately, attained any high distinction in these researches); the investigation of Gauss, on the same subject, in 1810; and of Bessel, on the perturbations, in 1824. Whilst the researches of Messrs. Lubbock and Ivory, on the same subject, in 1830 to 1832; and those of Professor Airy, on the figure of the earth and on the solar and planetary theories; and of Encke on the physical theory of his comet; may, perhaps, be considered as some of the most remarkable among the similar investigations of the present day.

Plane Astronomy.

In astronomical observation, the period since the time of Newton has, indeed, been productive of most valuable discoveries. The immense improvement in the construction of astronomical instruments, and in the art of observing, has been naturally followed by a highly increased accuracy of results, and numerous accessions to our knowledge of the heavenly bodies. And in all these we can, with just pride, observe, that our country has borne its full share.

We have alluded to Halley's labours. He added to them the glory of being the first to predict the return of a comet. Having noticed an agreement in the elements of several which had appeared at successive periods, he concluded them to be reappearances of the same body, obeying the law of an elliptic orbit. He foretold a return in 1758, which was completely verified.

Bradley, associated with Molyneux, commenced, in 1725, that valuable series of observations which led to the discovery of the aberration and the nutation of the earth's axis. The former a consequence of the finite velocity of light, the latter of gravitation.

The measures of the arc of the meridian were now repeated with increased accuracy in France, by La Hire and Cassini, but seemed to lead to the strange and paradoxical result of the earth's figure being a prolate or *lengthened*, instead of a flattened or oblate, spheroid. This, however, was afterwards shown to be due to an error in the fundamental measurement; and, in 1735, the comparison of arcs, measured by Maupertuis and others in Lapland, and La Condamine in Peru, established the oblate figure.

Arcs were also measured in Italy, by Boscovich, in 1750; at the Cape, by La Caille, in 1752; and in America, by Mason, in 1764.

The two transits of Venus, in 1761 and 1769, were both, and especially the last, most sedulously observed by astronomers sent to various stations in different parts of the globe, at the expense of the principal governments in Europe; and the important results of the sun's parallax completely settled.

The invention of Hadley's quadrant in 1731 furnished the instrumental means, created as it were for the express purpose of observations on board ship: and the improved lunar tables supplied the data for the easy and complete adoption of the method of lunar distances for finding the longitude at sea; which was mainly introduced by the zeal and diligence of Dr. Maskelyne, astronomer royal, in setting on foot the "Nautical Almanac," and providing tables and rules adapted to the use of seamen; which, simplified as it was necessary they should be, were yet necessarily founded upon the utmost refinements to which theory and observation had extended.

In 1749, the improvements on watches, for the same important purpose, by Harrison, obtained the Royal Society's medal; and in 1769, after the trial of a long voyage, secured to the ingenious inventor the reward offered by act of parliament. The stimulus thus supplied has been ever since steadily producing a constantly increasing perfection in these machines.

The attraction exercised by large masses of known density, compared with the attraction of the earth, enables us to infer the mean density of the globe. With this view, the attraction of mountains in causing a plumb line to deviate from the perpendicular (which had been noticed by La Condamine in Peru), was investigated by Dr. Maskelyne, in 1774, by observations on the mountain Schehallion in Scotland; and similar observations have been since made by the baron De Zach and others.

An elaborate series of experiments, having the same object in view, though conducted by totally different means, was carried on by Mr. Cavendish in 1798; who estimated the attraction of leaden balls by means of an extremely delicate apparatus. The result agreed very closely with that of the other methods.

In 1787 commenced the series of trigonometrical operations, designed in the first instance merely to connect the observatories of Greenwich and Paris, but which were subsequently extended to the formation of a survey of the whole of Great Britain, under the sanction of the British government, conducted first by general Roy, afterwards by colonels Mudge and Colby, and now extending in Ireland.

Sir W. Herschel had succeeded beyond any previous artist in forming and polishing large metallic specula for telescopes; and, with his gigantic instruments, had the satisfaction of soon adding a new planet to our system. The discovery of Uranus took place in 1781, and was immediately recognised by astronomers throughout Europe. The same eminent observer continued to devote himself to the minute examination of the heavens; and added immensely to our knowledge of the nebulæ, the appearances and probable physical nature of the sun; and, lastly, of double stars; which, by comparison of observations at considerable intervals, he found, were, in several instances, composed of two bodies, revolving round their common centre of gravity. This discovery was announced in 1803, and has been since abundantly confirmed by the joint labours of his highly distinguished son and Sir J. South; as well as of M. Struve, of Dorpat. Thus is an unlimited extension given to the dominion of the great law of gravitation; even the elliptic forms of the orbits having been, in several instances, ascertained.

Another series of brilliant discoveries were those of the four small planets between the orbits of Mars Jupiter: Ceres, discovered by Piazzi, in 1801, Pallas, by Olbers, in 1802; Juno, by Harding, in 1804; and Vesta, by Olbers, in 1807.

The establishment of the periods of two very remarkable comets has also marked the present century. One observed in 1819 and 1822 was found to resemble previous appearances. Encke calculated the orbit, and predicted its return in 1825, which was fully verified; and again in 1828 and 1832. It is thus shown to be a small nebulous body, belonging to our system, of a nature, in some sense, intermediate to a comet and a planet. Another body, of a similar nature, was in like 18nner shown to be a periodical comet by Biela, in m25.

The last-mentioned discoveries are all of continental origin; and there are numerous other important researches which our limits will not allow us even to enumerate, for which we are deeply indebted to those eminent observers, and others whose names are equally conspicuous in the annals of science, — the worthy successors of the Cassinis, of Lalande, Lacaille, and Delambre.

Astronomical research, at the present day, is doubtless chiefly directed to the increasing accuracy of details. It has been justly observed, by Professor Woodhouse, that "Astronomy has now reached a kind of maximum state of excellence, and its changes are minute, and must continue so. All great changes ended with Bradley. He swept the ground of discovery, and left little to be gathered by those that follow him. Yet, during the sixty years that have elapsed since Bradley, it cannot be said but that astronomy has greatly advanced, although not by the aid of discoveries such as those of aberration and nutation."

This immense extension of detailed research renders it, of course, the more difficult to analyse. Volumes might be occupied in giving even an outline of such results. A highly interesting discussion, as to the existence or non-existence of a sensible parallax in the fixed stars, was carried on in 1815, and several subsequent years, between Mr. Pond and Dr. Brinkley. In 1820 the establishment of the Astronomical Society in London gave a new stimulus to the prosecution of the science in this country; and its transactions have, from that time, become an invaluable depository of observations, methods, and tables, referring to all parts of plane astronomy. The institution of observatories at the Cape of Good Hope, by the British government, in 1821; at Paramatta, by Sir T. Brisbane, in 1822; and at Cambridge, in 1823, have already been productive of an abundant harvest of important results. The regular publication of observations, and of papers and discussions relating to astronomy, has been of incalculable advantage in diffusing a knowledge of the science, and in promoting communication of ideas; while the Ephemerides of Paris, Berlin, Milan, and others, have, in their turn, stimulated the English astronomers to make those extensive improvements in their Nautical Almanac, which distinguish the volume for 1834.

Optics.

The splendid discoveries of Newton in optics seem to have excited very little desire to pursue the subject, for a long time, among his successors. The fact of the unequal dispersion of different media, really observed, as we have remarked, but not understood, by Newton and Lucas, was taken up, and even practically applied to the correction of the colour at the focus of the object-glasses of telescopes, by Mr. Hall, about 1729. This invention, however, seems to have been forgotten until the idea was once more revived, or rather re-discovered, by John Dollond, in 1758, and the achromatic telescope brought to perfection by the labours of that eminent artist and his scientific successors. This principle also enabled the optician to dispense with those inconvenient lengths in telescopes, formerly adopted, as will appear from what we before remarked (p. 245.)

The enquiry into the refractive and dispersive powers of transparent bodies, and the recognition of dispersions not only unequal in amount, but dissimilar in character, was, at the same time, carried on by various eminent observers, and these important physical data ascertained with increasing precision from the determinations of Boscovich, Wollaston, and Brewster. Compound lenses, including liquid media, were proposed and tried, with considerable success, by Dr. Blair; and have still more recently been carried to the highest perfection by Mr. Barlow.

The mathematical theory of optical rays is another extensive branch of enquiry, in the first instance, indeed, of a purely speculative character, but in its results and applications embracing the whole theory of ordinary reflection and refraction. Such a theory, developed in a high degree of generality by Malus, has, at the present day, been almost wholly superseded by investigations of a yet more commanding abstraction, and prosecuted with the resources of a still more powerful calculus, by Professor Hamilton.

The phenomena of the prismatic spectrum again have been shown to possess characteristics unnoticed before, by the observations, first of Wollaston, and then of Frauenhofer, in the existence of bright and dark lines, crossing the spectrum at all points of length: of these, no theory has as yet been attempted.

In those departments of experimental optics which reveal to us the more intimate nature and affections of light, in very recent times astonishing accessions have been made to our knowledge both of the phenomena and of the theory. After slumbering more than a century, during which scarcely a single fact was added to those elicited by Newton, this beautiful and interesting branch of enquiry began to revive. In the hands of Malus, Arago, and Biot, in France, and of Wollaston, Brewster, and Herschel, in Great Britain, the phenomena of polarised light gave a completely new turn to the train of enquiry; and a succession of brilliant experimental facts and laws, served to excite the admiration, and in some degree to exercise the theoretical skill of these and other philosophers. Before this, however, though little noticed or appreciated, the revival of the theory of Huyghens, and the extension of the experiments of Grimaldi, Hooke, and Newton, enabled the masterly genius of Dr. Young to strike out the beautifully simple law of interference, which has since been extended to the most intricate and recondite phenomena, not only of the class to which it was originally applied, but to those of polarised light also, with a precision and success utterly beyond the reach of possible anticipation, in the researches of those two lamented labourers in the field of science, Frauenhofer and Fresnel, and of their living coadjutors, Professor Airy and M. Cauchy.

The curious phenomena on which, as we have before seen, Newton founded his theory of *fits*, was now brought into connection with the whole range of the new results. The *intervals,* of whatever nature they might be, whose lengths had been so accurately determined, were found to be identified with the *intervals of the waves* in the Huyghenian theory; and though either explanation might apply, nothing for a long time had been decisively urged to determine which was preferable, until, in the first instance, Fresnel reasoned on the *quantity of light* reflected (which was one of the points we before alluded to); and, again, those assumptions respecting the action of the *two* surfaces of the thin plate (which, as we remarked, were the only *hypothetical* parts of Newton's doctrine,) were examined by Professor Airy; and, by a masterly *experimentum crucis,* he determined that *both* surfaces were concerned, and, by consequence, demonstrated the undulatory explanation. To those researches have recently been added the application of Professor Hamilton's system to the theory of undulations; out of which has arisen one of the most singular facts in scientific history, the prediction of an entirely new form which a ray of light ought to take under particular circumstances; totally unlike any thing of which previous observation could supply the smallest idea, but which was completely verified by the experiments of Professor Lloyd. Nor should we omit to mention the ingenious objections which have been raised against the undulatory theory by Mr. Potter; and the suggestion of a case, not, indeed, as yet either an objection or otherwise, but to which the theory has not yet been made applicable, by Mr. Barton, which have both excited some discussion. MM. Arago and Fresnel originated the experiment in which the simple phenomenon of interference is exhibited without any extraneous circumstances. Two diverging beams of light are made (either by reflection or refraction) to cross at a very small angle; and, instead of giving rise to a uniform double illumination at the point where they mix, they exhibit a space *striped* with alternate bright and absolutely black bands. At the black part, then, we have the paradoxical result that *two conspiring rays of light produce absolute darkness:* a result totally inexplicable on any hypothesis of material particles, but a direct consequence of the theory of waves, of which a familiar illustration may be conceived in supposing two waves propagated from different origins, and reaching the same point at that exact interval which shall make the summit of one, as it were, coincide with the bottom of the other; they will then *neutralise* each other; and that point of the fluid will remain at rest. The agitation of the ethereal medium in waves produces light; when at rest there is darkness. The unequal refrangibility of light is a fact which was not to be reconciled with the particular view of the principles of undulations on which the theory had been framed. It has been long, therefore, a topic of enquiry, and several hypotheses have been suggested as to the possibility of so modifying the conception of these first principles as to make the results include this case. This appears at length to have been successfully done by M. Cauchy.

But, in addition to these, we could, if our limits would allow, increase largely our list of discoveries by reference to a vast number of other interesting facts relative to the absorption of light by different media, the constitution of the prismatic spectrum, the application of polarised light to the detection of the crystalline forms of minerals; the phenomena both of the light reflected from metals as regards its state of polarisation, the colours produced by reflection from grooved surfaces, and numerous others, for which we are almost wholly indebted to the profound experimental skill and indefatigable industry of sir D. Brewster. But into any such enumeration our limits forbid us to enter; and we can only pass, with deep regret, from this interesting portion of our survey to others of which our view must be as rapid and imperfect.

General Physics.

A vast range of science, wholly of modern creation, has arisen in tracing the relations of light, heat, magnetism, electricity, and galvanism. The great principles of electricity originally developed by Gray and Franklin, Du Fay and Æpinus; of galvanism, by Volta and Galvani; and of magnetism, first reduced to determinate laws by Mr. Barlow, were placed in the closest connection by the grand discovery of electro-magnetism, by Oersted, in 1819. This was followed up by the researches of Davy, Faraday, Ampere, Arago, Barlow, Christie, and many other philosophers, following out the numerous new analogies which the subject so abundantly offers. The new facts of thermo-electricity and thermo-magnetism elicited by Dr. Seebeck and Professor Cumming, brought the other subtle agent heat into connection with the former; and the singular influence of the simple rotation of metallic plates on the magnetic needle, investigated by Messrs. Arago, Babbage, Herschel, Barlow, and others, have all jointly contributed to form the new science of electro-dynamics.

The increasing and exact attention paid to observing the constitution of the atmosphere, the influence of moisture and temperature, the effects of wind, and the endless combination arising out of the general causes of pressure, condensation, and rarefaction, have led to the recognition of meteorology as holding a place among the exact sciences. Some, indeed, of its phenomena are intimately connected with the laws of chemical combination, another boundless region of philosophical enquiry, which our subject does not even profess to include, and to which we cannot further refer. Closely connected, however, with all these departments is the science of heat, both in its combined form, or recognised as developed *in bodies*, the subject of profound research to Black, Wilcke, Dulong, Petit, and Fourier; or in its radiant condition as propagated from hot bodies to sensible distances. In this last department (connected as it also is with the solar heat), the valuable researches of Leslie, Herschel, Prevost, and De la Roche have put us in possession of all the elementary facts and simple laws; while the theoretical inferences from some of those results have been denied, and an experimentum crucis attempted by the author of this volume, which appears to prove the existence of two distinct species or modes of transmitting heat, produced, at one and the same time, from luminous hot bodies. The attempt of Berard to prove that simple heat is subject to polarisation, like light, has failed, on being repeated by the author and by Professor Lloyd. Very recently some important results have been obtained by sir D. Brewster, and still more singular facts have been made known by MM. Nobili and Melloni, by means of a thermo-multiplier of the most extreme sensibility.

But it would be endless to dilate upon the mere titles of researches of which we can give no satisfactory history; and we are unable to do more, before concluding, than very briefly to refer to the general condition of the means we enjoy for prosecuting physical research.

The great increase of institutions and societies established for the purpose of promoting, in various ways, the prosecution of science and the diffusion of philosophical information, during the eighteenth and present centuries, has been a striking feature, equally demonstrative of the extended diffusion of a taste for such pursuits, and efficacious towards its further progress. The societies of Berlin, Turin, and Petersburgh, besides numerous others of scarcely inferior reputation on the Continent, and those of Edinburgh, Dublin, and Cambridge in our own country, have been the worthy offspring of the great parent institutions of a preceding century. The regular publication of their transactions, besides the appearance of a number of periodical journals, expressly devoted to the purposes of physical science, has exercised a most powerful sway over the scientific taste of the age in every way most beneficial to the cultivation of these branches of knowledge. Nor must we omit to mention the extraordinary impulse given to these pursuits by the annual meetings of the continental philosophers and naturalists; nor the still more effectual aid afforded to the same great objects in our own country by the British Association for the Promotion of Science, whose several past anniversaries hold out the most encouraging prospects of future utility; and which, by the judicious measure of drawing up *reports on the present state of our knowledge of each branch of science;* and recommending subjects for investigation, as well as by the concentration to one focus of all the intellectual power which can be thrown on the several subjects, and the promotion of fair and friendly discussion, instead of prolonged and irritating paper-war on any disputed points (not to dwell upon the more general and obvious benefits), may be said to have done already for the science of the country more than any other institution within the same time.

Conclusion.

In drawing to the conclusion of such a survey as we have attempted of the progress of physical knowledge, a host of reflections of the most interesting description naturally crowd upon the mind. We had hoped to have wound up a far more complete history, by entering freely upon some discussion of the advantages attending the prosecution of science, of the evils sometimes supposed to be involved in it, of its effects, though cultivated only by a few, indirectly experienced by society at large; its connection with physical and still more with moral civilisation; its existing condition and future prospects, in our own and other countries; the influence of various national institutions, whether in promoting or repressing its progress; the state of public opinion respecting its claims; the recognition of scientific instruction as a branch of education. But the consideration of these and many other kindred topics of the highest interest we are compelled to abandon, from the pressing necessity imposed on us by the limits of our volume. There are also many topics commenced in previous parts of our sketch, to which a continuation is manifestly wanting, but which we are thus precluded from supplying.

There is one consideration which alone affords us any satisfaction for the unforeseen deficiency under which this portion of our history labours, — the circumstance that *all these topics are, in fact, embodied in the different treatises of the Cabinet Cyclopædia upon the several branches of physical science.* To enumerate and explain the series of recent discoveries by which those sciences have been brought to their existing state of completeness, would be little else than to engage in systematic descriptions of the entire sciences. And when we have traced the progress by which the first principles of philosophic truth were originally fixed, and the first great advances made in the developement of the laws of the material world, the discussion of the successive improvements upon those principles would be little different from the actual exposition of the facts and theories whose aggregate constitutes the systematic scheme of the different departments. To these several treatises, then, we must be content to refer our readers to collect the recent history of scientific discovery.

INDEX.

A.

B.

C.

THE END.

LONDON:
Printed by A. SPOTTISWOODE,
New-Street-Square.