My work in reference to the Cesarean operation : a word of protest in reply to Dr. Henry J. Garrigues / by M. Saenger.

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Publication/Creation

New York : William Wood, 1887.

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MY WORK

IN REFERENCE TO THE

CESAREAN OPERATION.

A WORD OF PROTEST IN REPLY TO
DR. HENRY J. GARRIGUES.

BY

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[Reprinted from the AMERICAN JOURNAL OF OBSTETRICS AND DISEASES OF WOMEN AND CHILDREN, Vol. XX., June, 1887.]

NEW YORK:
WILLIAM WOOD & CO., PUBLISHERS,
56 & 58 LAFAYETTE PLACE.
1887.

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A WORD OF PROTEST IN REPLY TO DR. HENRY J. GARRIGUES.

During the last few years, Dr. Garrigues has published, aside from his papers on gastro-elytrotomy, two articles on the Cesarean section. In the former of these, printed in this Journal, April–June, 1883, and entitled "The Improved Cesarean Section," he entered most fully on the discussion of my book which had appeared early in 1882 in Leipsic, published by Wilhelm Engelmann, under the title "Der Kaiserschnitt bei Uterusfibromen, nebst vergleichender Methodik der Sectio Caesarea und der Porro-Operation. Kritiken, Studien und Vorschlaege zur Verbesserung des Kaiserschnittes," etc. Of course, how largely Garrigues draws from this monograph can be appreciated only by those who will compare it directly with the latter's paper. But even without doing so, it will be at once seen that Garrigues refers uncommonly often to my monograph, my name appearing no less than twenty-two times in the foot-notes. I did not take the trouble to ascertain how often he would have had to do it, if he had given the source of every particular which he first learned from my book. For nearly all the historical notes in Garrigues' paper, especially those bearing on the treatment of the uterine wound, are taken from my book and its supplement.[1]

The object of this first paper was to study the behavior of the uterine wound in the Cesarean operation; this formed the basis for the correct principles governing its surgical treatment by a suture adapted to the physiological peculiarities of the uterus. I

[1] "Zur Rehabilitirung des classischen Kaiserschnittes. Nebst einem Anhange: Nachtraege zur Geschichte der Uterusnaht beim Kaiserschnitt." Archiv f. Gyn., Bd. xix., p. 370.

had begun this work after my attention had been directed to the subject by a case which terminated favorably under great difficulties, namely, a Cesarean section with suture of the uterus, performed on account of a retro-cervical myoma, with dead fetus, and the existence of a renal-pelveo-abdominal fistula. The main portion of my book, the history of the uterine suture, was preceded by the following motto by Cazin: "Une étude expérimentale et clinique bien faite sur toutes les sutures de la matrice, après l'incision de cet organe serait, dans l'état actuel de la science, un grand service rendu." (*Arch. de Tocol.*, I., p. 717, note.) Whether I had satisfactorily accomplished this fundamental task may be learned from the remarks of but two authors who were among the first to utilize and criticise my investigations. Mangiagalli,[1] one of Porro's pupils, said: "Nessuno più di Saenger corrispose meglio a mio aviso a tale desiderio (such as had been formulated by Cazin) ed al suo lavoro ed all' opera di Alfonso Corradi chiamata dallo Saenger stesso fenomenale, coattinsi largamente onde fare questo schizzo storico sulla isterorafia." Wilh. Fischel,[2] then Assistant Professor of Obstetrics at Prague, and at that time a pronounced follower of Porro, remarked: (I translate.) "Saenger very fully discusses the history of the uterine suture. This portion, the result of the most thorough historical studies, disposes of a number of erroneous statements heretofore often brought forward, and is of the greatest actual interest on account of his mode of treating the historical material. It is of fundamental importance for the old method of the Cesarean operation."

Soon after this, came the first Cesarean operation performed, in accordance with my propositions, by Leopold with my assistance; it was successful. A larger number of other cases presently followed, with results so "wonderful," as Harris, Lusk, and Tarnier were pleased to call them, that the Cesarean section, at one time almost abandoned in despair, became again the legitimate operation for the majority of surgeons and was placed above the Porro operation which was reserved for certain cases only. That my monograph was the instigation to these results, and that the latter were attained largely through my later writings, through my own operations, through my long-continued personal efforts in behalf of the, in my opinion, correct principles governing the improvement of the Cesarean section I may justly claim without

[1] "Le più recenti modificazioni del taglio cesareo, studio storico-critico," etc. Milano, 1884.

[2] Prager medicinische Wochenschrift, April 16th, 1882, No. 17.

stultification because the facts bear me out. I have impartially accorded full recognition to the labors and merits of co-workers in this field, but have determinedly rejected unqualified claims. Among those against whom I had to proceed in this manner, Garrigues was one. This fact explains the difference between his first article on the Cesarean section (April–June, 1883) and the second one, published by him in this JOURNAL for October, 1886. The latter was written for the sole purpose of attacking me personally and to depreciate my labors on the subject in question. The reason why Garrigues turned against me is, that he felt offended by some remarks contained in the last but one of my papers[1] on the Cesarean section, but in which I was perfectly justified, as I may say right here. A defence, a vindication I would have considered as quite natural; but I feel that I must energetically oppose those impassioned, rankling attacks, devoid of every foundation, such as Garrigues has hurled at me.

Let us see from what an amiable point of view G. now looks upon my book. He says (p. 1,009): "Saenger has not even proposed anything new that in the hands of others has proved valuable. . . . (His book) is a meritorious work in so far as the author protested against the indiscriminate use of Porro's operation, etc., but as to originality, its two hundred pages contain only one new idea, and that one has been found in practice to be an unnecessary complication of the operation, and has therefore again been given up. . . . Thus nothing has been left of Saenger's only original idea"—the sub-peritoneal resection of the muscularis of the uterus and infolding of the peritoneum—"and still the operation shall bear his name?"

Since I myself, from the very outset, have designated this "one new idea" as a non-essential point, nothing, absolutely nothing remains as the outcome of all my labors, according to Garrigues. And yet he knows very well, as do others who again and again pretend that the peculiarity of my method consists in nothing but this sub-peritoneal resection, that I only recommended it as the means for the better execution of the symperitoneal suture. Thus Garrigues says regarding it (of course, in his first, amicable paper): "Saenger admits himself that the excision of muscular tissue is superfluous if the uterus is flaccid, or if in a contracted uterus the cut surfaces are parallel, and the two portions of the serous membrane can be drawn together and applied one against the other without dissection. But under the opposite conditions,

[1] "Neue Beitraege zur Kaiserschnittfrage," Archiv f. Gyn., Bd. XXVI., p. 168.

it is doubtless a valuable suggestion, and the recommendation of the operators who have tried it in practice speaks in favor of it" (p. 521).

This may serve as an example how Garrigues judged of the same thing before and after his sensibility had been excited.

As in all similar cases, the remarks made by me against Garrigues which so greatly provoked his wrath, when taken out of their proper connection, sound quite different from the way they were intended. I hope therefore that both the editor and the reader will understand why I insist that for my justification the passage be printed *in extenso*, so as to enable them to form their own opinion. It reads:

"In North America, gastro-elytrotomy, which made its appearance as 'Thomas' operation' about the same time as the Porro operation, absorbed almost equal interest with the latter; but mainly owing to the excellent critico-statistical labors of R. P. Harris, the conservative Cesarean section was never dropped from the scientific order of business. Some of the most important innovations in the technique of the uterine suture, as I have shown, emanated from that country—the employment of silver wire for the sutures, and the insertion of more numerous sutures. *Another author, whose pen had hitherto been devoted more to the establishment of gastro-elytrotomy, Garrigues, now enters into the current of these good traditions. His arguments against the general employment of the Porro operation fully harmonize with my own, and he likewise demands a more frequent use of the improved Cesarean section:* 'Since the Cesarean section is conservative in principle, and may be improved in many ways, as proposed by different writers and operators, it is certainly not only justifiable, but wise to try how it will work in its new shape.' In one case, where Garrigues was afraid to perform gastro-elytrotomy because the parturient was too sick and weak to stand the wound suppuration connected with that operation, he did the Cesarean section and employed a mode of suturing *which bears a close resemblance to the one devised by me* (*without resection*). The uterine wound was closed with twenty-four silk sutures, one-half of which passed through the entire thickness of the uterine wall, while the other half united only the peritoneum. The woman died after fifty hours. The real cause of death was sepsis. At the autopsy, the uterine tissue was found normal. 'The sutures were still as they had been inserted, only closer together. The peritoneum and the external two-thirds of the muscularis were united by first intention, the inner third ad-

joining the decidua had not united. The peritoneum along the line of incision was to a great extent covered with a fine layer of new-formed tissue' (?). Garrigues' assertion, that he had arrived at his method of suturing independent of the later publications on the Cesarean section, I am inclined to doubt. In his last paper on gastro-elytrotomy, published in January, 1883, not a word is said about the considerations which, as he claims, led him to that procedure. In that place he deals almost exclusively with gastro-elytrotomy and the Porro operation. I must also point out that my book was concluded in December, 1881, and appeared early in 1882, while Garrigues did not perform his operation until September 9th, 1882. *He also has drawn pretty freely but loyally from the former*, but did not need to take the trouble of making historical researches for himself, since he found even the full and valuable American literature on the Cesarean section digested by me. In his synoptical description of the modus operandi he has included all the main points of my programme, even the subperitoneal resection, but without citing any authorities, so that nothing but my protest would prevent our American colleagues from designating the method as that of Garrigues. *But at all events, we can calculate with the greatest probability on American support in the rehabilitation of the Cesarean section, because even heretofore the operation did not bear such a bad reputation there, owing to better results, and the right course will probably now be taken,* and for this reason favorable results will assuredly not fail."

In his polemical article, Garrigues always speaks only of the insult he has received, but he is silent about the praise bestowed upon him and his American colleagues. Every unbiassed reader will see and admit that I have upbraided Garrigues for only one thing, namely, that in the conclusion of his first article, in the summary of the description of the modus operandi, he had incorporated all the main points of my programme, even the subperitoneal resection, without giving the source, represented more particularly by my book on the Cesarean section, up to that time the only modern comprehensive paper on that subject. I was, and am still, fully justified in protesting against this. But what I said with reference to Garrigues' case of Cesarean section is not a personal remark; for I did *not* say "independent of *my* publications," but "independent of the *later* publications on the Cesarean section," among which I included, aside from the cases of Spencer Wells, cited of course also by myself, the several American authors, especially Byford, Lungren, and others, which

could hardly have remained unknown to Garrigues, since he must have seen at least the collections by Harris. *Of myself I did not speak at all in this connection.*

It is particularly because I myself, in all my scientific papers, have always striven for the greatest accuracy and conscientiousness in the citation of the sources, the labors and publications of other authors, that I demand a like action on the part of others towards myself. I have merely reminded Garrigues of this duty by writing in protest as I did.

I therefore beg again to call attention to the salient point of Garrigues' polemic against me. He makes it appear as if I had asserted he had in his *operation* copied the method of suturing the uterus devised by me. I emphatically protest against this substitution. My personal recrimination, my blame of Garrigues, has only reference to the fact that, at the conclusion of his first article on the Cesarean section (1883), he had reproduced the essential particulars of the method of performing the operation laid down in my book and subsequently, without specifying it.

Hence there is no cause for him to play the part of one sorely offended, and to say that I had charged him with being "a man who by lying tries to steal some great discovery made by another." I am quite willing to believe that he arrived at his method of operating independently, especially as he was aware of Spencer Wells' experience. But when he says: "It was so natural a thing to put in the sutures just as I did, that it never occurred to me that I had done anything remarkable, and do not think so yet," he nullifies his merit again by this observation which is perhaps merely intended to show his modesty, in that he manifests thereby that he completely misunderstood the importance of the matter. Very well, Garrigues has operated as he has described, independent of my book, which then had long appeared, independent of the first operation by Leopold which preceded it by some months. I do not doubt it and have not done so. But thereby he had done nothing else but performed a Cesarean operation as others have done; only that he, as I appreciatively pointed out, acted in a rational manner in the suture of the uterus on the principles developed by me and based on scientific grounds.

But *at the time of his operation* he did not draw any general conclusions from the technique he had employed for a typical method, any more than Lungren did, whose two cases I cited in my book with all their details and in full appreciation of their

importance. Garrigues enriched the record of the rational uterine suture by an interesting case, but he did not create a method. I, however, founded one which clearly laid down all the particulars of the operative technique, *and this method existed, had undergone its practical test, even before Garrigues had operated.* This is an absolute fact. As Garrigues himself confesses, he hardly knew any other author than Spencer Wells, the importance of whose statement respecting the necessity for the symperitoneal suture of uterine wounds, though it was not made with any reference to Cesarean section, was probably emphasized by no one so much as by myself. Garrigues did not know the extensive history of the uterine suture, nor the cause of its failures, nor the conditions governing the healing of the uterine wound, etc. That he learned all these things subsequently is proved by page after page of his first article, May to June, 1883. That he did not enter on the study of the questions respecting the improvement of the Cesarean section until after his operation Garrigues admits himself. In 1883, he did not disdain to cull plentifully from my book, the preparation of which required a most laborious scientific study, theretofore undertaken by no one in the same manner, in order to find later that it contained nothing original in two hundred pages. *Sapienti sat!*

It has always appeared very remarkable to me that, in his paper published in January, 1883, on gastro-elytrotomy, Garrigues was absolutely silent regarding improvements of the Cesarean section. We read there on page 44: "Thus, if working in the country, and being called to a case in which Cesarean section or one of its substitutes were required, keeping in mind the many cases of successful Cesarean operations in country practice, I would not hesitate to perform this operation, as has been done without assistance, and even without a bistoury, simply using a common razor. In a hospital, I would, everything otherwise equal, prefer gastro-elytrotomy, etc. . . . If it were necessary to operate before the dilatation of the cervix had begun, I would perform Mueller's operation, which has given better results than Porro's." And page 50, paragraph 9: "In country practice, the old-fashioned Cesarean operation will in most cases be preferable to all its substitutes."

Thus Garrigues here declares unconditionally for gastro-elytrotomy or the Porro-Mueller operation in hospital practice, and for the "old-fashioned Cesarean section" in country practice, "by simply using a common razor." This was Garrigues' standpoint before he became aware that the "old-fashioned Cesarean

operation" had become rejuvenated. And when, the reader will inquire, was this article printed? *Exactly three months previous to the appearance of Garrigues' second article on Cesarean section* which all at once heralded the improved operation. Not a word, not a single word about improvement and possibility of improvement of the "old-fashioned Cesarean operation." Not a syllable, not an intimation by Garrigues that he had reflected on its defects and devised some kind of remedy. No notice, no indication that he himself had performed an antiseptic Cesarean section with a rational uterine suture. And only three months later, what a transformation! The connecting link between these two successive, so radically different articles by Garrigues can be recognized psychologically only in the fact that in the mean time he became aware of the strong movement in favor of improving the classical Cesarean section and against the Porro operation, against gastro-elytrotomy, during which he also became familiar with my labors in this field and utilized them. In this way I have come back again to the point from which I started.

Garrigues sets himself up, moreover, as the defender of others, of Porak, Leopold, and Kehrer, and of course takes sides against me without any objective investigation of the state of affairs. There is no need for me here to enter again on my discussion with Porak, in which I was not the party who had the worst of the argument—a fact which every one may ascertain who will take the trouble to consult my reply to Porak in the *Arch. f. Gyn.*, XXVII., p. 170. The same paper will also show that my complaint of the reviewer in the *Amer. Journ. of the Med. Sci.* was directed less against the latter than against Porak, whose "review" was so worded as to be mistaken for an original article by the reviewer. I have again occasion to call attention to this mode of reviewing in which the real author is entirely ignored: In *The International Journal of the Med. Sciences*, July, 1886, reference is made to Schauta's first Cesarean operation which even to its minutest details was performed according to the method described by me, without my name being mentioned by the reviewer who might have learned that much from Schauta's paper.

As to Leopold, he was the first—and for this I have always shown him grateful recognition—who employed my procedure on the living subject, but it was done after reading my book, after repeated conversation with me, during which the mode of operation was accurately determined, and with my personal assistance. Regarding Leopold's "modification" at that time, I have shown

long ago (*Arch. f. Gyn.*, XX., p. 304, note) that it was merely an unimportant variation of the procedure. Garrigues, of course, takes no notice of this. Since he was not able to enumerate essential alterations by Leopold, he clings to the incidentals, such as resting the uterus on gutta-percha tissue, sprinkling iodoform into the body of the uterus—measures which, to say the least, are common to us, having been included beforehand in the plan of operation discussed between us. Besides, Leopold himself has formally acknowledged that it was my method which he followed in his operations (*Arch. f. Gyn.*, XXVIII., p. 97). So Garrigues wants to be even more Catholic than the pope! Is it consistent when he who disputes the originality of my method puts such great weight upon variations of it, made by others? But whatever details of the procedure have been modified hitherto by Leopold and others—and I dispute no one's right to this and even find it advantageous to do so—no single alteration has touched the fundamental principle of the matter, namely, how a primary union of the uterine wound by an appropriate exact suture can be certainly attained in any other manner than in the way I have done.

For instance, if B. Schultze did not make a sero-serous button suture, but a rosette suture, is it not for all that a symperitoneal fold suture? If Krukenberg, instead of the two-rowed button suture, employed one resembling the Gussenbauer-Woelfler intestinal suture, the same effect can be obtained with it, and I have also considered and figured it in my book. If Leopold has in his last cases inserted a running sero-serous suture of chrome catgut, it still remains the same sero-serous fold suture. In fine, any desired mode of suturing can be employed, as well as the most variable material—silver, silk, silk-wormgut, catgut—provided the uterine wound is closed quite accurately, and the peritoneal wound margins are flatly brought together. For in my book it will be seen that I have subjected eight different modes of suturing to a test, and additional ones subsequently, in order to find by practical experience that the interrupted suture, with the deep stitches of silver, the superficial ones of silk, is preferable. Some latitude is left to personal choice, provided the purpose inherent in the method is left unaltered. Since Leopold has had the greatest number of personal cases, his alterations and changes of certain details of the mode of operation are of great importance for comparison, and have contributed largely towards determining and simplifying the technique; but the main point, *i. e.*, the exact closure by suture of the uterine wound according to the principles laid down by me, remains unaltered; this is proved by

the fact that the course was equally favorable. Another great merit of Leopold's which I am pleased to acknowledge is, that by the unsurpassed results of his numerous operations he has helped to spread my method and has gained for it increasing confidence.

I am not surprised that Garrigues, in order to prove that there is nothing original in my method, has also pointed to Kehrer. But since the fact cannot be altered that my book appeared three months before Kehrer's paper, from which I naturally first learned of his two operations, and this disposes of the question of priority in my favor, Kehrer himself could not help submitting to the inevitable, and only allows it to appear in a foot-note of his last article (*Arch. f. Gyn.*, XXVII., p. 257) that I had perhaps got the idea of a double suture of the uterus from one of his students—an insinuation which requires no answer from me. To Boudon,[1] who goes so far as to assert that Kehrer's paper had induced me to occupy myself with the improvement of the Cesarean operation, I am willing to pardon this error. Would not Garrigues have been delighted to have known this author?

It is especially Kehrer's proposals, though theoretically similar, yet materially differing from my own, which prove that in the Cesarean section much depends on the most accurate adaptation to the existing conditions, even to the minor details. Kehrer's mode of suturing is calculated for the opening of the uterus by a deep transverse incision. But since this mode of incision has no prospect of being generally accepted, and Kehrer's method of suturing is not adapted to the anterior median incision which I have demonstrated to be the most suitable, the difference between us is thus at once briefly indicated. Kehrer's method, therefore, has not been imitated nor extended.

Why must I defend myself at all? Why must I write such an article? By degrees only I have become irritated and embittered when I saw that some brought forward, as new, things which had long appeared in my writings, and others praised immaterial modifications as great innovations; that some used my articles without naming the source, and others made believe that the acquisitions gained had been common property, as if that which has now become clear had always been self-evident. The egg of Columbus! Now every one can stand it on end.

To be sure, Dr. Garrigues, I identify myself with the improvements of the conservative Cesarean section because I have the right of saying of myself that not only have I initiated the move-

[1] "Etude critique sur l'opération césarienne et l'opération de Porro." Paris, Delahaye et Lecrosnier, 1885, p. 24.

ment for its rehabilitation, but have more than others advanced it by appropriate propositions, by experiments, by personal operations during five years of indefatigable labors with a definite object. Indeed, this is vigorous self-praise, but it is based on truth and facts. If I must defend myself in person against the attacks of Garrigues, I could not avoid praising myself.

Although to-day most obstetricians in all countries, Italy not excepted, again look upon the conservative Cesarean section as the legitimate operation, as late as 1881 the Porro operation was the only one from which any good was hoped for. This revulsion in favor of the natural method of delivery with preservation of the uterus—incised merely for the removal of the child and again closed by suture—could be brought about neither by denunciation of the mutilating Porro operation nor by high-sounding expressions of the hope that in time it would be possible to re-establish the conservative Cesarean section, but only by the active improvement of the technique of the latter and by putting in force the correct principles as the basis on which the main portion of the operation rested, beside observation of antisepsis; that is, a rational treatment of the uterine wound. That I have accomplished this task is proved by the successful results attained since then. My book appeared just at a time when the possibility of improving the conservative Casarean section was despaired of. The indissoluble uterine suture, on which the successful issue of the operation was mainly dependent, was thought to be impossible. This fact induced some authors (Frank, Cohnstein) to make some strange proposals with a view to avoid the suture. The greater number accepted the Porro operation unconditionally, and when the possibility of healing the uterine wound by first intention by means of a rational suture was shown both theoretically and practically, they would not believe it at first. Even to-day there are some doubters to whom the proofs thus far are insufficient. Therefore, we must still labor for the good cause in which Garrigues aids me unwittingly by his attack. Another question which has again come into the foreground since the rehabilitation of the Cesarean section and has been followed by me as far as its consequences is entirely passed over by Garrigues, viz., *the displacement of craniotomy of the living child and in part also of artificial premature labor by the Cesarean section.* The discussion of this question is beyond the scope of the present article, but I was greatly surprised that Garrigues prefers craniotomy of the living child to the Cesarean section, for *socialistic* reasons. He thus advocates a view which has hitherto been foreign to practical obstetrics as an indication. I may be pardoned for this digression.

The fact can hardly be emphasized sufficiently that isolated operations do not found a method unless the considerations which guided the surgeon in the several steps of the operation have a scientific motivation, unless inductive conclusions are drawn from the single case for general application. *This is the difference between clinical cases and method,* and it explains how I could evolve a method from the clinical cases before having operated myself, and how others who operated in a similar manner so as to approach the principles of this method still failed to establish one.

"When the time is ripe for a certain idea," says Garrigues, "it is quite natural that it should occur about simultaneously to different men." He is certainly right in that. But to whom does it apply in the present instance? At most to Kehrer, who, like myself, devised a method extending beyond the reported cases. The men who, among the number for planless and ill-advised Cesarean operations, stood forth prominent as surgeons who approached the true method of operating, from d'Avanzo to Spencer Wells and Lungren, have had their importance justly appreciated by no one more fully than by myself. But not one of them remedied the defect of a scientific foundation for the operative technique of the Cesarean section, not one of them produced a firmly established method and secured for it universal recognition. They merely furnished valuable cases to be utilized as the foundation for the superstructure.

But if, as Garrigues maintains, my method of performing the Cesarean operation was really hovering in the air, why, I may ask, has no one grasped it, especially in the fatherland of Lister and Spencer Wells? For in no country have matters remained unchanged, even to the most recent times, as in England. On the one side we see the classical Cesarean section performed in the old primitive way with corresponding unfortunate results; on the other, we see the unconditional advocacy of Porro. No wonder that Lawson Tait, having in view the English conditions and ignorant of others in other countries, particularly Germany, purposely exaggerates the mortality to 99⅔ per cent. Only now a change is preparing which finds its best expression probably in a single sentence from a card recently directed to me by Clement Godson, the well-known advocate of Porro: "I am getting to be converted, as you prognosticated I would—I have had an unsuccessful Porro." How diametrically opposed is this confession to another expression by T. M. Dolan a little more than a year ago! The latter compares (*Brit. Med. Journal*, Dec. 26th, 1885) 134 Porro operations with 59 recoveries to 136 Cesarean

sections (of course, "old-fashioned") with only 25 recoveries, and adds: "In the face of these striking figures, we think that the obstetrician who does not discard the old classic operation for the modern method inaugurated by the great Italian professor, takes upon himself a great responsibility!" What would Dolan say now were he to learn the results obtained with my method?

Now is the time to demolish completely Garrigues' fiction that Spencer Wells had been his great prototype. For in his book, "Diagnosis and Surgical Treatment of Abdominal Tumors," Spencer Wells does not declare himself an adherent to the conservative Cesarean section, but an advocate of Porro. At the same time he believed the Cesarean section to be so unpromising that he says the anti-vivisection bill must first become inoperative before it would be possible to lay the foundation for improving the Cesarean section by experiments on animals (!): "Whether the necessary experimental trials of both methods are, in this country, to be made on women only, or upon females of some of the lower animals, must depend upon the degree in which British physiologists (!) are hampered by the vivisection act." So spoke Spencer Wells, the only prop to which Garrigues despairingly clings, at a time when my method was already completely published and could point to the best results. Until the most recent times, the movement in favor of the improved Cesarean section, inaugurated by my own, Kehrer's, Leopold's, and other writings, was next to unknown in England. The *Edinburgh Med. Jour.* alone repeatedly called attention to it (Simpson, Angus Macdonald). The reader is referred to the paper by Robert Barnes, "The Alternatives to Craniotomy" (introduction to a discussion in the Section of Obstetric Medicine at the annual meeting of the *Brit. Med. Jour.*, October 2d, 1886). Barnes did not say one word about improvements in the technique of the Cesarean section which were then established and tried for years. Indeed, by Cesarean section he understands hardly anything else than the Porro operation which he weighs as an alternative as opposed to craniotomy. However, aside from the point of view which is likewise in favor of the conservative Cesarean section, it is no longer possible to consider that craniotomy of the living child can be replaced by the Porro operation. This, as I believe I have conclusively proved (*Arch. f. Gyn.*, XXVI., p. 222), can only be done by the conservative Cesarean section. Kinkead, in the paper following that of R. Barnes, and entitled "Craniotomy and Cesarean Section," betrays at least some knowledge of the reforms in the field

of the Cesarean operation. He was acquainted with Leopold's first operation, but quotes it and my method quite incorrectly.

In the discussion to that famous paper by Lawson Tait, in which he gives the mortality of the Cesarean section as exactly 99.971 per cent, and proposes to reduce the mortality of the Porro operation to five per cent, Routh expressed himself as follows: "There were three conditions which, if carried out, would make this operation (the Cesarean section) successful; first, antiseptic measures rigidly carried out; secondly, no preliminary tampering with the patient by forceps or other measures, all of which produced delay and exhaustion; thirdly, the uterus should be opened high up, with as small an opening as practicable, care being taken not to cut through any part of the cervix. If this was done, ligatures, as in Dr. Edmunds' case, would be found unnecessary."

Dr. Routh, therefore, believes even ligatures to be superfluous, and says not a word of suturing the uterus! And this in spite of the fact that "the time was ripe."

Dr. Lusk, of New York, first called the attention of British surgeons—and for this I am very grateful to him—to my method and the results obtained with it: "Cesarean section always held out promise when performed under favorable circumstances. The weak side was always the gaping of the uterine wound. Porro's great method consisted in doing away with this danger by the removal of the organs. But a still greater work was the re-awakening of the professional interest in the Cesarean operation. The result was the wonderful success of Leopold and other Germans with the uterine suture of Saenger. This method, according to the latest reports, showed in twenty-six cases nineteen recoveries and seven deaths. In all the latter, the operation was performed under circumstances well-nigh hopeless. Cesarean section, modified by Saenger, Porro's operation, and that of Dr. Thomas, were not rival, but supplementary operations, etc." The further discussion betrayed very plainly that Lusk's statements were as good as unknown.

But it gives me great pleasure and satisfaction to state that a short time after this, at a meeting of the British Gynecological Society, October 13th, 1886, in a paper entitled "Ought Craniotomy to be Abolished?" Meadows occupies the same standpoint as I do, mainly on the strength of the results of the operations performed according to my method, as communicated to him by R. P. Harris. Altogether, I can hardly be grateful enough to Dr. Harris for the lively and warm interest manifested by him in

my labors for the Cesarean section which were largely stimulated and supported by his own ; and no less grateful for the zeal and disinterestedness displayed by him in spreading in English-speaking medical circles the results obtained with the improved Cesarean section.

Garrigues likes to cite authors who devised improvements in the technique of the Cesarean section before I did. Among these he includes Guéniot. His proposal to draw the intact uterus into the abdominal wound by a loop of silver wire, to incise it "hors du ventre" and after being emptied to take it entirely out of the abdominal cavity, has been discussed and criticised in my book (p. 130). But eventration of the empty uterus has been done already by Ritgen. And Guéniot himself? He did not in the least harbor the illusion that his proposal had materially advanced the technique of the Cesarean section. In order to prove this, I may be permitted to cite some remarks made by Guéniot in a report [1] to the Académie de Médecine of Paris on three successful Cesarian sections performed by Dr. Closmadeuc (of Vannes) in the old-fashioned way. This report appeared in 1885, that is, fifteen years after he made his proposal, before Porak and Potocki had called the attention of French physicians to the new era of the improved Cesarean section. Guéniot expresses his surprise that the great advances, especially in abdominal surgery—anesthesia, antisepsis, hemostasis—had been productive of so few improvements in the Cesarean section. Assuming the main causes of failure of hysterotomy to be hemorrhage and peritonitis, he adds: "Supprimer ces deux complications, ou plus exactement, les reduire à une extrême rareté equivoudrait donc à un immense progrès, c'est-à-dire à la transformation, des resultats présents et passés de l'opération césarienne. Un tel perfectionnement est-il chose possible ?"

This possibility appeared so improbable to Guéniot that he must have been surprised to learn shortly afterwards that it was really in existence, and that the endeavors to paralyze these dangers of the Cesarean section had been in the main successful.

In this modest fashion spoke Guéniot himself, who was set up by Garrigues as one of my most important forerunners in order to bolster up an inferior detail in the *ensemble* of the Cesarean operation. Such a procedure is quite characteristic of Garrigues' pole-

[1] " Rapport sur un travail de M. le Docteur de Closmadeuc de Vannes intitulé opérations césariennes, au nom d'une commission composée de M. Tarnier et Guéniot, rapporteur." Bull. de l'Acad. de Méd., etc., 2ème série, tome xiv., 16, vi., 1885.

mic against me. Those who are not familiar with the literature of the Cesarean section may be misled by him, but not I, who have studied it most thoroughly before I even thought of occupying myself with it, knowing, as I do, whence he has taken his captious wisdom.

Nil novi sub sole! If any inventor produces anything new nowadays, it will hardly have arisen by "spontaneous generation," but will always show some elements of the activity of others. And then it does not take long before everything is dug out that has previously existed and which often has but little to do with the invention. The "dii minorum gentium" also demand their part, and the envious, who carelessly passed by the find secured by somebody else, do not rest until they have discovered that this came from this and that from that, even if it finally has become something altogether different. Thus it is also with advances in medicine which, of course, have all the previous science behind them, but still where the slightest alteration may be productive of the greatest revolutions. It is not necessary for me to make excursions into the history of medicine in order to prove this.

If Garrigues disputes my claim of having devised a new improved method for the Cesarean section, he should, in order to be consistent, do the same with Porro and Thomas, because they were by no means the first to perform the operations called by their names. It is generally known that Horatio Storer amputated the uterus evacuated by the Cesarean section before Porro had done so, not to speak of the authors who had considered this operation theoretically, before Porro operated on Giulia Cavallini and published his famous treatise. As regards gastro-elytrotomy, it may be read in every text-book on obstetrics that it was first conceived by Joerg and executed by Ritgen—a fact which does not by any means lessen Thomas' merit for having reawakened it and brought its development up to the times. Still less can Porro's fame be clouded by Storer's actual priority, for the reason among others that he did not learn of it until afterwards. Porro[1] him-

[1] As it may be interesting to many to see the passage in question, I copy it in the original language: "Il non aver pensato e preveduto l'amputazione uterina, fece si que il dott. Stover (instead of Storer) dovesse procedere poi con mezzi e con metodi, disadatti all' operazione, consigliata gli dalla gravezza ed urgenza del caso. Qui abbiamo due vittime che certo non avrebbero incorrazziato alcuno a ripetere it tentativo dell dott. St. e ci troviamo di contro a un caso particolarissimo che non poteva di certo far nascere it concetto della metodica amputazione dell' utero nel taglio cesareo" (pp. 76, 77).

self points out incontrovertibly the great difference existing between the unpremeditated operation by Storer, performed under the stress of circumstances and with insufficient means, and his own projected operation, previously determined in all its details, and carried out on the living subject in accordance with the programme.

A new operation, if it is not performed according to a predetermined method based on a sound foundation, but is rather the result of an accidental chain of circumstances, remains an isolated fact which does not interest a wider circle unless this single case becomes the basis of a new method. This shows that in the operative field two ways are equally feasible and equally justified: first the operation and from it the method; or, first the method and from it the operation. It can be shown precisely on the example of Ephraim McDowell chosen by Garrigues how erroneous it is to conclude anything further from the historical fact that this surgeon performed the first ovariotomy than that circumstance alone; to Spencer Wells only are we indebted for the method of ovariotomy and its successful propaganda. None but a method complete in itself has a lasting value and a permanent influence on matters relating to operations.

In the light of these general explanations, what is Saenger's method of performing the Cesarean section? Briefly defined: "Laparo-hysterotomy, performed with all the auxiliaries of modern obstetrics and surgery against the former chief dangers of hemorrhage and infection, with closure of the uterine incision by a mode of suturing adapted to the physiological peculiarities of the organ and firmly established on scientific principles, which completely compensates the injury to the uterus and guarantees healing by first intention; all the methodical and positive improvements in the technique, proposed for the purpose of again restoring to the first place the almost abandoned conservative Cesarean section and to limit it from the Porro operation by the establishment of definite indications."

Before me, no one has formulated such a programme and, what might be the main thing, brought it to its realization in every direction. It is easy for me to furnish the most positive proof of this. It rests on the direct recognition of the profession who did me the honor to call the new method of the Cesarean operation—which alone among others, devised at the same time or subsequently, put a stop to the universally victorious Porro operation—after my name, so that to-day every one versed in modern obstetrics knows what is meant by it. When one man

like R. P. Harris, so eminent an authority in all matters relating to the Cesarean section, has recognized my method as original, it should really be indifferent to me that Garrigues does not do so, since his denial is based not on objective, but on personal reasons —a grave but just charge as regards the objectivity of an author! The case record of Cesarean sections performed according to my method—now forty-five—is at the same time a list of those who think like Harris; but even among those who have had no opportunity as yet to operate I number adherents, such as Lusk, Tarnier, Meadows, and others, not to speak of German authors; and I think no one will allow Garrigues to lead him astray, but rather will wonder that I deem him worthy of such a thorough refutation, since he has taken his weapons from my writings. But "qui tacet consentire videtur," and for this reason I do not keep still.

I myself regret most deeply that my book on Cesarean section is so little known, especially among English-speaking physicians. But I may boldly assert that since its appearance in the literature of the Cesarean section no special theme can be broached that has not been thoroughly discussed in it on a historical and modern basis. But to-day I can no longer permit that, when my method is spoken of, this book alone be made the basis, as Garrigues does. I must demand that all my writings on the Cesarean section published by me in the last five years, all of which aim at a progressively greater security and simplicity of the procedure, and ending with my paper read at the first congress of German gynecologists in Munich, be included. When I wrote my book, I myself had performed only one conservative Cesarean section which, not having been made according to a definite method, was not even incorporated in my lists—probably the best proof of the strictness with which I acted, since I might have counted this successful case as the first of all the following, it having been performed antiseptically and with an accurate uterine suture. According to my method as subsequently formulated, I myself have performed to the present time, at Credé's clinic, four operations and have assisted at one, while the whole number of cases operated on thus far at the Leipzig institution is seven, in which all the mothers recovered and seven living children were secured. Garrigues takes pleasure in referring to me as a theorist—here is the practice! To be sure, I have altered the method originally proposed in many ways, partly in consequence of the experience of others, especially that of Leopold. But is there a single operation in surgery the performance of which is immovably fixed

forever? It is precisely in the alteration of the details that progress lies. If but the fundamental principles of the operation remain, that is the main thing.

Garrigues attempts to prove that not a single step of the Cesarean operation called by my name was originated by myself and peculiar to me alone. Herein he shows a sophistry which is hard to match; for he makes it appear as if I had unconcernedly appropriated all the labors of those who had contributed something towards the improvement of the Cesarean section; while the fact is, that all these authors, with whom Garrigues was made acquainted only through my book, could not have obtained more suitable recognition than they received by me. And not one of those praised by Garrigues as my predecessors has arisen to complain of me. How could they? They all have merely contributed details, some of them very valuable; yet no one but myself has united all these into one total of a new method, with the addition of the most important innovations. "Ubi sunt qui ante nos in mundo fuere?" If Garrigues' example were to be followed, then no operator could henceforth boast of having devised a new method. He demands that everything belonging to the operation should be a personal invention, even the chloroform and the antisepsis. Do not musicians always work with the same chords, authors with the same stock of words which are common property?

Garrigues divides the Cesarean operation as now performed into eight groups of manipulations, and attempts to prove that I had no share in originating any of them. These eight steps are:

1. Antisepsis (Lister).
2. Early operation (R. P. Harris).
3. Eventration of the uterus (Guéniot, P. Mueller).
4. Temporary closure of the abdominal wound, etc., behind the eventrated uterus (Frank).
5. Resting the uterus on gutta-percha tissue (Leopold).
6. Temporary compression or rubber ligature of the lower uterine segment (Rein, P. Mueller, and others).
7. Keeping the uterus warm by cloths (Leopold).
8. Uterine suture (numerous operators).

Strange to say, no mention is made of the point which really governs the whole and formerly actually constituted the operation itself, viz., *the uterine incision*. Did Garrigues forget this point? I am immodest enough to assert that I have decided this question as to the most appropriate incision in favor of the an-

terior median incision. Perhaps this is now taken as self-evident, though formerly this was by no means the case, as may be seen in my book (pp. 183–186).

The importance attaching to the proper direction of the uterine incision I have explained above in connection with Kehrer's method, which is based on the deep transverse incision. The plasticity of the uterus is extraordinary, its tendency to heal much greater than had been formerly believed; I am sure the uterine wound could be made in any direction and still heal up if rightly sutured. But it is equally certain that the anterior median incision is the best and most appropriate for delivery, if but on account of being parallel to the majority of the fibres and away from the large vessels of the uterus.

In considering the eight steps of Garrigues, I can at once cross off numbers five and seven, neither of which was devised by Leopold, but by me.

Self-evident though it is that I as well as surgery in general have extensively utilized the blessings of *antisepsis* (No. 1), still I must say—thus inclining somewhat towards Lawson Tait—that it is not absolutely essential, at least as chemical antisepsis, to the success of a Cesarean section, or could be secured by the simplest measures of cleanliness in the absence of septic germs. Otherwise not a single woman could have recovered in former times. For even the uterine suture is not absolutely necessary; still it is certainly more important than antisepsis. *The highest degree of technical perfection, the greatest security to life in the Cesarean section, however, is only guaranteed by the co-operation of antisepsis with a rational uterine suture*, as I have strongly emphasized in my book and in all my writings. If in addition thereto the principle be followed of performing the operation as early as possible and abstaining from unpromising attempts at delivery—a fact always recognized as important, but the full weight of which has only been pointed out by R. P. Harris (comp. my monograph, p. 157)—we have secured the three most important principles of the improved Cesarean section, in comparison with which points 3, 4, and 5 are quite inferior, but which in my labors have been left sufficient latitude, in the manifold extension of the technique of their authors, none of whom has devised a true method for the Cesarean section.

If from the whole operation one step should be selected to be placed at the head of the modern improvements, it is unquestionably the *uterine suture*. I may presuppose that the principle on which the uterine suture devised by me is based is well

known. Briefly defined, it consists in the exact closure of the entire uterine wound by a two-rowed suture, the lower tier of which is to comprise the whole uterine wall without passing free through the cavity of the organ ; while the superficial tier, placed between the deeper stitches, merely unites the surfaces of the serous edges. Both tiers of sutures to be so close and so numerous as to insure an absolutely perfect reunion of the uterine wound and its covering with peritoneum. The suture material to consist of a substance the knots of which do not loosen and which will not dissolve (silver, silk, or chrome catgut). Since the uterus does not present a movable subserous layer of tissue, the undermining of the peritoneum and resection of the muscularis are intended for cases in which the parallel union of the wound and the symperitoneal plane suture could be effected only with difficulty. (This measure proved to be not essential, but was of great value, as it was best suited to bringing the symperitoneal plane suture into recognition). The ultimate object of the uterine suture was stated to be : *the uterus after the suture must be like an uninjured organ, the uterine wound must heal by first intention without adhering to the abdominal wall.* It is self-evident that this my procedure did not arise as Minerva did from the head of Jupiter, but has predecessors, all of whom I enumerated conscientiously ; it is natural, too, that it admits of numerous combinations ; but there is hardly any combination which I have not mentioned and tried—some of them, such as the continuous suture and Gussenbauer's suture have meantime been preferred by several operators without having to be considered as a material deviation from the fundamental plan, if only the closure of the uterine wound is complete in the sense above given, and the peritoneal surfaces are brought into close contact. The effrontery of declaring my improvements and amplifications of the uterine suture to have no existence could be ventured by Garrigues only with a reading public who were unacquainted with my book and my numerous succeeding writings in the original language. This excludes, of course, those who know and have read my articles, who will admit that I treated the difficult subject of the uterine suture, which now appears so simple, not in vague generalities, not in a few lines and aphorisms dashed off, but in the most comprehensive manner possible —historically, experimentally, comparative-anatomically, histologically, clinically, etc.—which nobody had done before me, until I finally arrived at that perfect method which to-day proves its correctness in the most brilliant results.

With diplomatic cleverness Garrigues seeks to prove that my method of suturing is nothing but that of Lungren, Baker, and Spencer Wells. The reader please refer to p. 138 of my book. He will find there, to his greatest surprise, that the three authors named are cited there in the same order and almost in the same words, with extracts from their writings. Thus it may be seen that he takes his proofs against me from my book. But how does he utilize it? Let us first examine Baker's mode of suturing. Though he avoided the decidua and took care to unite the peritoneum, *his entire suture consisted of four stitches of carbolized silk!* That is all. Healing took place with the formation of a utero-abdominal fistula. Surely, there can be no stronger proof of my conscientiousness in enumerating everything ever written about the symperitoneal suture than that I pointed with praise to Baker's statement that he inserted his sutures near the edge of the incision, "so that, when they were tied, they brought the peritoneal coats together first," though nothing further was contained in his remarks. Lungren was cited by me with praise because he laid particular stress on the symperitoneal union; I cited his statements, "great care being taken to unite the peritoneal edges," and "the peritoneal surfaces are retained in contact until union takes place and all danger of escape of fluids averted." Let us see now *how* Lungren stitched the uterus. In his first operation (1875), he inserted altogether only five silver sutures; in his second operation, he inserted twelve horse-hair sutures through half the thickness (!) of the uterus; of these, the hemorrhage not ceasing (!), the last three were replaced by silver sutures, and still the hemorrhage from the incision was not arrested.

Every unprejudiced person will see at once how greatly Lungren's mode of suturing differs from mine; he caried the suture only through half the thickness of the uterine wall, the number of stitches was scarcely half that in my method, the suture material chosen by him proved so unsuitable that he had to replace it in part by another; but, particularly, his suture was only single-rowed, and though he called attention to the importance of uniting the peritoneal edges, *he did not employ a separate symperitoneal plane suture.* Besides, how little Lungren was able to raise himself above his single case to the level of a more general conception is shown by the facts that he fails absolutely to deduce a peculiar method from it; that Porro was apparently unknown to him; that he sterilized his patient by ligation of the tubes without any pressing reason therefor. And Spencer Wells?

I have literally given his remarks on the symperitoneal suture of uterine wounds, and drawn from them the deductions applicable to the Cesarean section. That Spencer Wells himself did not do so is beyond any doubt. This is evident even from the title of his lectures which Garrigues, of course, suppresses and which is: "Observations on Recent Improvements in the Mode of Removing Uterine Tumors." That Spencer Wells did not dream of applying his proposals to the Cesarean section, I have incontestably proven on p. 15 of this paper. Garrigues attributes to him a rôle which he himself did not claim.

But to say that Spencer Wells has devised a two-rowed suture, as Garrigues asserts, is simply an untruth. *Not one word of it is to be found in Spencer Wells' writings.* He says merely: "I would insist that the peritoneal edges of the divided uterine wall should also be carefully brought together — like the parietal peritoneum of the abdominal wall—by many sutures, or by uninterrupted suture along the whole extent of the gap." This uninterrupted suture has been used by Spencer Wells in a case mentioned on p. 99 of my book. Though the patient recovered, no one would think to-day of imitating it, for the reasons explained on p. 166. It is only by a continuous tier-suture with chrome or juniper catgut, similar to the colporrhaphy and perineal suture as recommended by Schroeder, that the uterine wound could be closed as by an interrupted two-rowed suture, but without offering the security of the latter. This perfected uninterrupted suture was entirely unknown to Spencer Wells. As far as I was concerned, he was merely one of the authors who recommended the symperitoneal closure of uterine wounds in general; as regards the technique of the uterine suture in the Cesarean section he furnished no example.

Finally, with reference to Kehrer, who is again set up against me by Garrigues, I simply point to what I have said above. Whoever is desirous of learning the material differences existing between his and my method of suturing will find them in my paper in the *Arch. f. Gyn.*, XXVI., "Neue Beitraege zur Kaiserschnittfrage," and loco citato.

I am tired of hunting for other tricks in Garrigues' polemic which I have passed over. My defence has any way become longer than the frivolous, purely personal, and not objective attack deserved. "*Audiatur et altera pars.*" Now that I have answered Garrigues quite thoroughly, I can calmly await the verdict of all impartial persons. I have the satisfaction of knowing that it will not be unfavorable to me, for right and

truth are on my side. Since the time of François Rousset and Jeremias Trautmann efforts were made to render the Cesarean operation life-saving. It was left to our day to reach this goal. I can expect that the history of our science will, in justice, accord recognition to the part I have played in the attainment of this aim by my labors and operations.