Odd letters and loose correspondence, amalgamated into one sequence and sorted chronologically

Publication/Creation

1957-1979

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1962

UNIVERSITY OF LONDON
BRITISH POSTGRADUATE MEDICAL FEDERATION

INSTITUTE OF PSYCHIATRY

THE MAUDSLEY HOSPITAL,
DENMARK HILL,
LONDON, S.E.5.

RODNEY 6333

AJL/HM

25th April 1962

Dear Bowlby,

As one Adolf Meyer Lecturer to another, many thanks for the reprint. I expect you found it as difficult a paper to prepare as I did: addressing an unpredictable collection of members of the American Psychiatric Association puts a strain on one's "audience-adjustment". You seem to have coped with the problem effectively.

With kind regards,

Yours sincerely,

Aubrey Lewis.

John Bowlby, Esq., M.D.,
The Tavistock Clinic,
2, Beaumont Street,
London, W. 1.

5 Mch 75 9.35 am.

PARENTS AND CHILDREN: "SUBSTITUTE MOTHERS"

Sonia Jackson said in the broadcast:

"I think the effects of this kind of separation* have been tremendously exaggerated, an awful lot of what has been written about separation, children and their mothers, is based on studies of children in institutions, who were not only separated from their mothers but totally deprived of the right sort of care and I think Dr. Bowlby, who was responsible for spreading the results of this research, has now recognised this and thinks that his original position was a bit too extreme."

* The programme only concerned itself with day time separation.

Producer: Mary Ratcliffe
Compère: Lily Morgan

JB/DES 6th March 1975

The Producer
Parents and Children Programme (Radio 4)
British Broadcasting Corporation
Portland Place
LONDON W1A 1 AA

Dear Madam:

On the Parents and Children Programme broadcast at 9.35 a.m. on 5th March, a leading speaker stated that I had changed my views on the dangers of separating young children from their mothers and now believed them to have been too extreme.

The speaker is misinformed since I have not changed my views although I have elaborated and amplified them. I refer you to my most recent book on the subject SEPARATION: ANXIETY AND ANGER (1973) Hogarth Press which at no point modifies views expressed earlier (see especially passages on page 22 and pages 72-73.

Since the speaker unwittingly misrepresented my views on an important matter of public health upon which my opinion is influential, I am writing to request that you take an early opportunity to broadcast a correction.

I am sending a copy of this letter to the British Medical Association.

Yours faithfully

John Bowlby
MD FRCP FRCPsych
Honorary Consultant Psychiatrist

Anna FREUD

as from:

20, MARESFIELD GARDENS,
LONDON, N.W.3.
HAMPSTEAD 2002.

20. 9. 61.

Dear Dr. Bowlby,

The marginal markings looks to me very much as if they were made by my father. Also it seems to me that they were made at the time when he wrote the psychological essay attached to the Fliess letters.

Yours sincerely

Anna Freud

45 Kirkhill Road
Edinburgh EH16 5DE
031-667 9284

9th December 1979

Dear John,

It was lovely to receive your collected lectures and your letter. Thank you so much.

My admiration to you for finishing Vol 3. What a magnificent task & so valuable to all of us working in the field of family psychiatry. It is strange & comforting to remember the period in my life at the Tavistock and I find that it helps me so much in the work I am still doing for the S.I.H.R.

I have now on average six patients – all of whom are working in the 'helping professions' and we have started a monthly seminar of those working in this field – now about eight of us. With these people I find my experience over the 23 years with you are invaluable in relating their own childhood experiences with what is happening in their present life.

I am afraid my visits to London are very rare – far between because although Alison has made a good recovery, she still is unable to use her right hand & cannot do any jobs like cooking. Also my very nice neighbours have moved & have

been replaced by three nice but very young students & so I cannot get a baby - (or old lady) sitter.

I continue very well & active so I am very lucky.

It is good to hear you & your family are all well. Alison & I had a lovely August with visits from all her daughters & four lovely little grand daughters. It did us both a world of good. I don't need to tell you families are very important.

I do hope we meet some time in 1980. My love to you both.

As ever
Elizabeth

HINDE.

MRC
Medical Research Council

MRC Unit on the Development and Integration of Behaviour
University Sub-Department of Animal Behaviour
Madingley, Cambridge CB3 8AA

telephone Madingley (0954) 210301

reference

Rep. 14 Nov.

Dr. J. Bowlby,
Wyldes Close Corner,
Hampstead Way,
London, NW11.

6 November 1979

Dear John,

I meant to write immediately to say how much I enjoyed the meeting last week. I think it could turn not only into a very important book, but also into the start of a very important series of meetings. It was important to me because, while I have been of course well aware of the impact of 'attachment' on studies of child development, it brought together that work and the long term issues of the later consequences of early attachment, which had previously been in a separate pigeon hole in my mind. I hope that you felt as pleased as I think you should have done about the outcome of the meeting, and about its implications for what you have done for psychology and psychiatry.

Now another thing has come up. At the beginning of the month I spent a week in Hungary. It was very interesting to see the consequences of their quite different social system on early child care. The mothers get three years off work after the birth of each child if they want it, but then pretty well have to go back to work. The day care facilities and nursery schools which this necessitates are oriented towards parental involvement, with stable social groups of children who have the same teacher over successive years, and a special provision for the nurse or teacher to tell the parents everything of importance that had happened to the children during the day. You will feel, I am sure, that there are pros and cons about all that. But the question of hospital visiting seemed still to be very primitive and in one provincial town that I visited I was told that it was bad for the children to be visited by their parents, because it upset them! (I think I mentioned that to you).

Now I have had a letter from a friend I made there asking for references 'on the daily visits of the parents to their hospitalised children'. It appears that such a system has recently been introduced into two pediatric departments in Budapest and a pediatrician in one of them is anxious to be able to read up previous literature. He says that he is faced with the same type of problems that I had mentioned to him - resistance by the nurses etc. Can you help me with any references that I could pass on to him?

~~Yours sincerely,~~ Best wishes

yours ever

ROBERT A. HINDE.

Vol. III is tremendous. I am amazed by its scope. Please let us know if you need this city back.

Oct 2.79

46 Laurier Road,
London, NW5 1SJ
01-485 4984.

Dear John,

It was a great pleasure and surprise to receive 'The Making and Breaking of Affectional Bonds.' I had heard neither of its conception nor its arrival. Thankyou very much indeed.

I intended to put your new book on one side as I am under pressure at the moment (to write a paper for a conference of behaviour modifiers), but once I had started

looking at it I could not put it down and I have now re-read all your papers with great pleasure. As always I am filled with admiration for your excellent prose and your lucid presentation of ideas which I hope will be increasingly widely appreciated.

Winnie's birthday comes soon. Keith and I will be driving from home, and back the same afternoon. If any of your party would like a ride with us do let us know. I look forward to seeing you at Newark. With love from Hubert

TWA IN FLIGHT

HINDE

July 23 '79

23 July 79
Rep. 7 Aug.

Dear John, I have been wanting to write to you especially since, a few weeks ago, I had an ecstatic letter from a Radcliffe friend about a lecture she had heard you give in London. We have been spending the last few months in California — I had a visiting Professorship at Berkeley. We have been having a very nice time — a lovely home to live in, & a very stimulating group of colleagues in the lab. Joan & I shared a room in Psychology, & I also had contacts in Zoology & Anthropology. It was a special pleasure to have a room next to Mary Main — she is such a nice person. We were able to talk a little about 'attachment', & just what it is that the concept implies. It's an issue I have been thinking about because I have just finished an attempt to pull together

USA EUROPE AFRICA ASIA

some of the literature on interpersonal relationships — I had intended to send you the manuscript for criticism last Winter, but heard you were very busy with Vol 3 & thought you would not thank me. I'm not sure what you will think of it — much of the literature is, I think, rather poor. But I think I'm sure you'd agree that the subject impinges on so many disciplines that it needs a focus in its own right. I'm hoping to get the proofs & cover from Acad. Press next week.

We have just had a fortnight's holiday in Oregon & N California — a lovely time. The months here have been good for the children, & I think they have both grown up a lot. Now for a visit to members of Joan's family on the E coast, & home in mid-August. Don't bother to reply — this is only written to express the view that I wish I could talk with you more often. I hope all has been going well — & look forward to seeing you — Oct (24th?)

Yours ever — Robert.

SARABHAI

B. M. Institute of Mental Health
Ashram Road, Navrangpura, Ahmedabad-9. India. Gram : Manopchar Phone : 78256/7/8

BM/125-25/3120/78-78

21st October, 1978

My dear John:

Your letter introducing Dr. Atul Shah is greatly appreciated, particularly at this time when we have vacancy for well trained psychiatrists. I am writing to Dr. Shah for his bio-data.

Each of our new clinical services (for infants and babies at risk, opportunity class for children with learning disabilities who need remedial work and a small hospital school programme for autistic, schizophrenic children) has or should have a psychiatrist attached to it.

I am not sure whether I mentioned to you last summer that Dr. Ramanujam has left us after 13 years of work at BM. He is planning to settle in America. Some of the younger psychiatrists who were with us have also left their full time appointment with us to settle in private practice.

Perhaps I have already shared with you that I was discussing with Bob Gosling and Peter Hildabrand the possibility of sending a group of people from BM to the Tavi for training in psychotherapy. I am enclosing an outline that we have formulated for this development project. We are trying to work towards their initial visit to the BM next February.

Gautam and I have to go to N.Y. for the follow-up of my treatment early in December. We hope to stop in London on our way back. We shall be delighted if we can meet you then.

Thelma McConnel sends her warm greetings.

With warm regards to you all.

Kamalini

encl : as above

B.M. INSTITUTE OF MENTAL HEALTH
Ashram Road, Near Nehru Bridge, Ahmedabad 380 009, India

DEVELOPING THE PRACTICE AND TRAINING OF DYNAMIC PSYCHO-THERAPY IN INDIA

A PILOT STUDY

Generally, in India, when people seek help for their emotional problems, they are often treated with drugs; if they are more disturbed, they are given shock therapy. This is a common psychiatric practice. In Bangalore, as in some of the other centres, the Behaviour therapy approach is also practised, which introduces a different treatment modality. The term "Psycho-therapy" is often used to signify some inclusion of "talking sessions" along with drugs and other physical methods.

The B.M. Institute has been interested in and is adequately equipped to develop a dynamically based psycho-therapeutic approach to the understanding and treatment of various types of clients-children, adolescents, adults and families.

Although the B.M. Institute has instituted a course in psycho-therapy training which is recognised by Gujarat University, it has failed to develop a systematic programme. This is partly because this approach is still not commonly used in psychiatric training and adequately integrated in the training courses offered in India; partly because of the large turnover of staff within the Institute; and partly because, in the diversified services and training programmes which the Institute has undertaken, it has been unable to provide the concentrated effort that such a serious training attempt might require.

There is, therefore, an urgent need to establish an integrated programme for the practice and teaching of psycho-therapy based on a psycho-dynamic approach in India.

A collaborative relationship has existed between the B.M. Institute and the Tavistock Centre in London for more than 20 years. Some staff members of BM have been trained at Tavistock and some senior members of Tavistock have come to BM as consultants.

In May 1978, a series of meetings were held in London to explore the possibility of setting up a Pilot Study at BM in India, jointly by BM and Tavistock. This would involve not only providing training to a selected number of staff members of BM who are familiar with the Indian scene at the Tavistock Centre, but also a continuing exchange between the two institutions over a period of say the next 5 years, to ensure that there is a meaningful adaptation of the new learning to the Indian situation. Repeated visits by members of the Tavistock Centre to BM would enable them observe at first hand and to appreciate the differences between the cultures, and thus avoid the danger of the not uncommon blind borrowing of concepts

and techniques, whether or not they seem relevant to the Indian scene. Staff members of both institutions will thus be involved in modifying what is learnt in England, to how it is to be applied in India.

BM Institute's Role

BM would provide the necessary professional staff who are willing to commit themselves to undertake training in England and on their return, to continue to develop training and service at BM for an adequate period of time to make this a viable project. There could be 10 to 12 medical and para-medical BM fellows selected for this programme and the disciplines could include psychiatrists, clinical and educational psychologists, psychiatric social workers, teachers, psychiatric nurses and psychiatric occupational therapists. They might go for training to Tavistock in 2 or 3 batches, one following the other.

The focus and concentrated effort would be on this "critical mass". Their training would be intensive. The programme for each individual would be "tailor-made" and in addition, they would all participate in a 'core' programme that would enable them to develop a common language and influence a directional change on their return to India. There would be a choice to work in one or more areas; and with children, adolescents, adults and/or families. There would be 'learning' as well as 'doing' under supervision. And they would also have personal analysis. The duration of this exposure is important: some time to get 'acclimatised' and then sufficient time to make the new experiences their own, and yet, not too long to uproot them. A period of 12 to 13 months seems reasonable. The objectives of the training programmes will therefore be limited to what can be realistically achieved within this period of time. A second year a t Tavistock later on, after the experiences of this initial programme have been internalised and put into practice back in the Indian situation, would on the whole, seem more profitable than a more ambitious one-stage plan that entails an absence of 3 or 4 years away from home ground.

A senior staff member of BM accompanying each batch, would act as Co-ordinator and 'hold together' the team in their personal and training situations.

On their return to India, the "BM fellows" will not only get involved in using the psycho-dynamic approach in their clinical work, but also spread this orientation by participating in the training programmes of BM for its other staff members, who could not be included in the batch for training abroad, which has of necessity to be limited to a few members. The BM Co-ordinators will continue to hold the group together and can play the role of a catalyst to activate the process.

Tavistock Centre's Role

Two members from Tavistock would be invited to come to India for a month in February 1979 as Consultants to set up this Pilot Study. During this period they would see the k ind and level of work at BM, finalise the scheme and select the candidates proposed by BM for training abroad.

Tavistock would also identify a person or a team to co-ordinate this programme at their end and to assign the BM Fellows to appropriate training areas, courses, supervisors and personal treatment. They may also assist in personal matters concerning the BM Fellows such as their living accomodation and other facilities. In other words, the BM Group would have to be 'held together' by the Tavistock Team as well as by their own BM Co-ordinators.

Members of the Tavistock Team would also make periodic visits to BM during the next 5 years. This will provide continuing support to the returning BM Fellows to use their new insights in their clinical work as well as in the on-going training programme of BM for those who have not been included in the batches for training abroad.

HAMBURG

INSTITUTE OF MEDICINE
NATIONAL ACADEMY OF SCIENCES
2101 CONSTITUTION AVENUE WASHINGTON, D.C. 20418

DAVID A. HAMBURG, M.D.
PRESIDENT

September 25, 1978

John M. Bowlby, M.D.
The Tavistock Institue of Human Relations
School of Family Psychiatry and Community
Mental Health
Tavistock Centre, Belsise Lane
London NW3 5BA
England

Dear John:

Many thanks for your very nice letter of 25 August. We were delighted to hear from you. Betty says you probably have the only copy of the President's Commission on Mental Health Reports that now exists in the United Kingdom. It may be a handy object for occasional reference. Some of the portions are very good indeed. Needless to say the ones for which Betty was primarily responsible.

I am sorry that you cannot make the Pugwash Conference but I certainly do understand. You no doubt detected my fine hand in the invitation. I think it may be an interesting workshop, but it certainly must take a backseat to volume three. I look forward with greatest pleasure to volume three. In regard to my own trip to Geneva I very much appreicate your invitation and certainly hope to take it up. I can hardly emphasize strongly enough how eager I am to visit with you and Ursula.

Peter Bourne's resignation was not only a personal tragedy but a serious loss to the nation. In the foreseeable future, I doubt whether anyone else can stimulate the President's interest in health to the same extent that Peter did. I have seen a fair bit of him since his resignation and am doing everything in my power to help him land on his feet. I think he is doing well. By the same token, those of us who have some chance to be influencial in respect to health policy are intensifying our efforts to partially make up for the gap by Peter's departure. I really think he did an outstanding job up to the time of the unfortunate error.

I am glad to hear that your Canadian trip went well and that you are all flourishing. We are too. Eric is in his third and final year as a student of law at the University of San Francisco. He spent two summers working here in the United States Court of Appeals, one notch below the Supreme Court, in a very stimulating experience indeed, working with Judge David Bazelon, one of America's most distinguished jurists. Peggy graduated from Harvard with high honors, took organic chemistry at Harvard this summer, and is now preparing her medical school applications. During the coming year she will be doing research at NIH and is looking forward to it. She has a natural inclination for research and by now has had considerable experience. Betty is just now completing her work on the HEW task force to implement the President's Commission on mental health, and will shortly return to the NIH intramural program where she will be in the laboratory of developmental psychology with Marian Yarrow. Betty will be resuming her research on early adolescence, an area in which they have not previously had investigations at NIH. So all is well with us.

Betty joins me in sending our very best to you and Ursula.

As always,

David /eb

Dictated by Dr. Hamburg and signed in his absence.

BRONSON

April 27 '78

Dear John:

Let me send you a copy of my most recent work -- one piece published, the other in press. Both argue (more forcefully, in the now published piece) for two discrete mechanisms underlying avoidant behaviors: a transient "wariness" phenomenon in response to the unfamiliar, and a more enduring "fear" pattern rooted in previous disturbing experiences. Both make sense from an adaptive perspective -- but what entitles me is evidence that these constitute independent sources of individual differences. Being prone toward wariness is not predictive of a disposition to acquire learned fears. (In my own thinking -- not published -- I associate the former with reticular system effects, and the latter with limbic system processes).

I trust that Vol. III is moving toward completion -- Wanda and I both anticipate its publication. Wanda is starting the final

year of her grant, and soon we should see a series of rather definitive articles describing patterns of development in peer relationships. I am awaiting news on a major grant proposal of my own, directed toward the microanalysis of visual learning (2-6 months) using recently developed scan-path technologies.

Perhaps the summer after this we may again get to England; it would be very good to see you both.

Regards,

Harlan.

Wyldes Close,
Wyldes Close
London NW11 7JB

Sir,

Last century we went to
war to force the Chinese to
open their ports to our opium
trade. Now we seem in the
same bellicose mood because
the Americans will not open
their airports to Concorde.
Backward peoples seem still
not to know what's good
for them

Yours faithfully

22/76 Contd

psychiatry should be encouraged to do more to remedy this situation. In this way perhaps more attention could be paid to improving the overseas doctors' performance in the MRCPsych and to the inclusion of an optional paper based on overseas mental health services.

It was agreed that the British Council and WHO should be asked to consider sending fellowship holders to provincial teaching centres rather than exclusively to centres like the Maudsley in order to give them broader experience.

The provision of mandatory courses was not thought to be practicable

23/76 APPROVAL VISITS

Noted: There was a growing feeling among some overseas trainees that the Approval Teams often received a misleading impression of the prevailing working conditions and training programmes in hospitals and the overseas doctors were not given adequate opportunity to see the Panel to discuss their special problems.

Agreed: to ask the Education Committee to bring this matter to the atention of the Court of Electors.

24/76 REGULATIONS FOR MRCPsych EXAMINATION

Noted:

(i) After 1 July, 1977 candidates will be permitted to attempt the Preliminary and Membership Examinations on five occasions only. Previous attempts before July 1977 will also be counted.

(ii) Candidates failing the Preliminary Test on three or more occasions may not re-sit within one year subject ot a satisfactory report from their Clinical Tutor or Consultant Chief who is required to verify that they are pursuing their studies.

Agreed:

(i) The Sub-Committee will ask the advice of the Clinical Tutors' Sub-Committee on this matter but feel the retrospective limitation on the number of re-sits is unfair to trainees and especially to overseas trainees, as they seemed to be the largest group who might otherwise have allowed a longer interval between re-sits if they had fully understood; and

(ii) the permission to re-sit after three failures should be based on previous performance in the examination as well as a personal report of the tutor or consultant.

25/76 DATE OF NEXT MEETING

The date of the next Meeting will be notified as soon as possible.

HARRE

JB/DES

5th January 1977

Dr. R. Harré
Sub Faculty of Philosophy
12 Merton Street
OXFORD

Dear

I was very glad to receive copies of PERSONALITY and to read the other contributions which I found very interesting. The directions in which social psychology is now moving is very congenial to me - especially the abandonment of traits and the adoption of the various concepts you and de Waele describe in the final chapter. Let us hope the book will exert an influence.

I am still inclined to think that you give too little weight to the biologically determined biasses and constraints that I believe play such a large part in shaping human social interaction. One of their principal effects, I suspect, is their influence on which social situations we select and seek after and which we either ignore or avoid. This is a variable I believe to be of immense importance. I am glad to see that Argyle is beginning to give it attention.

The other area in which I suspect biological biasses to be playing a big part is in social interactions that are accompanied by strong feeling. Did you ever get my letter of 27th May 1975 (written in response to yours of 1st May) or did either my letter or your reply perhaps go astray? Your comments on it would help me understand how you view the kinds of problem in human social behaviour which are of special interest to me.

With good wishes for 1977

Yours sincerely

John Bowlby, M.D.

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JB Copy of script sent to HARRÉ, Oxford Feb 1976

The self-reliant personality: Self-reliance and some conditions that promote it

JOHN BOWLBY

THE CONCEPT OF SECURE BASE

Evidence is accumulating that human beings of all ages are happiest and able to deploy their talents to best advantage when they are confident that, standing behind them, there are one or more trusted persons who will come to their aid should difficulties arise. The person trusted, also known as an attachment figure (Bowlby, 1969), can be considered as providing his (or her) companion with a secure base from which to operate.

The requirement of an attachment figure, a secure personal base, is by no means confined to children though, because of its urgency during the early years, it is during those years that it is most evident and has been most studied. There are good reasons for believing, however, that the requirement applies also to adolescents and to mature adults as well. In the latter, admittedly, the requirement is commonly less evident, and it probably differs both between the sexes and at different phases of life. For those reasons and also for reasons stemming from the values of western culture, the requirement of adults for a secure base tends often to be overlooked, or even denigrated.

In the picture of personality functioning that emerges there are two main sets of ~~variables~~ influences. The first set concerns the presence or absence, partial or total, of a trustworthy figure willing and able to provide the kind of secure base required at each phase of the life-cycle. These constitute the external, or environmental, ~~variables~~ influences. The second set concerns the relative ability

HARRÉ

or inability of an individual, first, to recognize when another person is both trustworthy and willing to provide a base and, secondly, when recognized, to collaborate with that person in such a way that a mutually rewarding relationship is initiated and maintained. These constitute the internal, or organismic, variables.

Throughout life the two sets of variables interact in complex and circular ways. In one direction the kinds of experience a person has, especially during childhood, greatly influence both whether he expects later to find a secure personal base, or not, and also the degree of competence he has to initiate and maintain a mutually rewarding relationship when opportunity offers. In the reverse direction the nature of the expectations a person has, and the degree of competence he brings, play a large part in determining both the kinds of person with whom he associates and how they then treat him. Because of these interactions, whatever pattern is first established tends to persist. This is a main reason why the pattern of family relationships a person experiences during childhood is of such crucial importance for the development of his personality.

Looked at in this light healthy personality functioning at every age reflects, first, an individual's ability to recognize suitable figures willing and able to provide him with a secure base and, secondly, his ability to collaborate with such figures in mutually rewarding relationships. By contrast, many forms of disturbed personality functioning reflect an individual's impaired ability to recognize suitable and willing figures and/or an impaired ability to collaborate in rewarding relationships with any such figure when found. Such impairment can be of every degree and take many forms: they include anxious clinging, demands excessive or over-intense for age and situation, aloof non-committal, and defiant independence.

Paradoxically, the healthy personality when viewed in this light proves by no means as independent as cultural stereotypes suppose. Essential ingredients are a capacity to rely trustingly on others when occasion demands and to know on

whom it is appropriate to rely. A healthily functioning person is thus capable of exchanging roles when the situation changes. At one time he is providing a secure base from which his companion or companions can operate; at another he is glad to rely on one or another of his companions to provide him with just such a base in return.

A capacity to adopt either role as circumstances change is well-illustrated by many women during the successive phases of their lives running from pregnancy through childbirth and on into motherhood. A woman capable of coping successfully with these shifts is found by Wenner (1966) well able, during her pregnancy and puerperium, both to express her desire for support and help and also to do so in a direct and effective fashion to an appropriate figure. Her relationship with her husband is close and she is eager and content to rely on his support. In her turn she is able to give spontaneously to others, including her baby. By contrast, Wenner reports, a woman who experiences major emotional difficulties during pregnancy and puerperium is found to have great difficulty in relying on others. She is either unable to express her desire for support or else she does so in a demanding aggressive way, in either case reflecting her lack of confidence that it will be forthcoming. Commonly she is both dissatisfied with what she may be given and is herself unable to give spontaneously to others.

In order to provide the continuity of potential support that is the essence of a secure base, the relationships between the individuals concerned must persist over a period of time, measured usually in terms of years. Although, for clarity of exposition, theory is often best formulated in non-feeling terms, it must be borne constantly in mind that many of the most intense human emotions arise during the formation, the maintenance, the disruption, and the renewal of those relationships in which one partner is providing a secure base for the other, or in which they alternate roles. Whereas the unchallenged maintenance of such relationships is experienced as a source

of security, threat of loss gives rise to anxiety and often to anger, and actual loss to the turmoil of feeling that is grief.

The theoretical position proposed includes a number of concepts familiar in psychoanalytic object relations theory; for example, Fairbairn's concept of mature dependency and Winnicott's concept of the facilitating environment (Fairbairn, 1952; Winnicott, 1965). It differs, however, from traditional clinical theory at a number of points. One is the avoidance of the terms 'dependence' and 'dependency needs', which, it is held, are in part responsible for very serious confusion in existing theory. A second is to attribute importance for development to experiences during all the years of childhood and adolescence instead of almost exclusively to the earliest months or years. Others are that the schema proposed is cast in terms of control theory and that it draws not only on clinical data but also on the findings of a broad range of descriptive and experimental studies both of humans and of non-human primates.*

The aims of this paper are to indicate some of the evidence that supports the viewpoint sketched, to consider briefly what is known of the conditions that favour or impede the development of healthy personality as here conceived, and, if possible, to clarify theoretical issues that have proved troublesome.

STUDIES OF SELF-RELIANT MEN AND YOUTHS

During the past decade or two a number of clinicians have turned their attention to the study of individuals who, it is reasonable to believe, possess well-functioning and healthy personalities. Not only do these people show none of the usual signs of personality disturbance, either in the present or, as far as can be told, in their past, but they are plainly self-reliant and successful both in their human relationships and their

* Both the theory itself and the evidence on which it rests are presented at greater length in the first and second volumes of *Attachment and Loss* (Bowlby, 1969, and 1973).

work. Though each of the studies so far published is inadequate in a number of ways, findings are suggestive. First, these well-adapted personalities show a smoothly working balance of, on the one hand, initiative and self-reliance, and, on the other, a capacity both to seek help and to make use of help when occasion demands. Secondly, an examination of their development shows that they have grown up in closely knit families with parents who, it seems, have never failed to provide them with support and encouragement. Thirdly, though here evidence is less substantial, the family itself has been and still is part of a stable social network within which a growing child is welcome and can mix both with other adults and with peers, many of whom are familiar to him from his earliest years.

So far as it goes, each study gives the same picture, the picture of a stable family base from which first the child, then the adolescent and finally the young adult moves out in a series of ever-lengthening excursions. While autonomy is evidently encouraged in such families, it is not forced. Each step follows the previous one in a series of easy stages. Though home ties may be attentuated, they are never broken.

Astronauts rank high as self-reliant men capable of living and working effectively in conditions of great potential danger and stress. Their performance, their personalities and their histories have been studied by Korchin and Ruff. In two articles (Korchin and Ruff, 1964, and Ruff and Korchin, 1967) they publish preliminary findings on a small sample of seven men.

Despite a high degree of self-reliance and a clear preference for independent action, all the men are reported to be 'comfortable when dependence on others is required' and to have a 'capacity to maintain *trust*, in what might seem conditions of *distrust*'. The performance of the crew of Apollo 13, which met with a mishap *en route* to the moon, is testimony to their capacity in this respect. Not only did they maintain their own efficiency in conditions of great danger but they continued to

cooperate trustingly and effectively with their companions at the base on earth.

Turning to their life-histories we find that these men 'grew up in relatively small well-organized communities, with considerable family solidarity and strong identification with the father . . . [They showed] a relatively smooth growth pattern in which they could meet available challenges, increase levels of aspiration, succeed and gain further confidence, and in this way grow in competence'.

Another study, this time of young men at college who appeared to their teachers to be of good general mental health and to promise well as youth leaders and community workers, is reported by Grinker (1962).

Among the sixty-five students interviewed Grinker regarded only a handful as showing neurotic character structure. The large majority seemed straightforward youths, honest and accurate in their self-evaluations, with a 'capacity for close and deep human relationships . . . to members of their families, peers, teachers and to the interviewer'. Their reports of experiencing anxiety or sadness suggested that such feelings arose in appropriate situations and were neither severe nor prolonged.

As regards their experience of home life, the overall picture reported by the students is remarkably similar to that reported by the astronauts. In almost every case both parents were still alive. The typical picture presented was of a happy peaceful home in which both parents shared responsibilities and interests, and were regarded by the children as loving and giving. During childhood, they said, they had felt with mother above everything else secure. At the same time they had identified strongly with father. Grinker reports much further evidence in support of these conclusions.

The findings both of a developmental study, from age ten to seventeen years, of thirty-four adolescents of very different characters (Peck and Havighurst, 1960) and also of a small study of successful students during their transition from high

school to first year in college (Murphey *et al.*, 1963) are very similar to Grinker's. Evidence presented suggests that both self-reliance and the capacity to rely on others are alike products of a family that provides strong support for personal aspirations, sense of responsibility and ability to deal with the world. So far from sapping a child's self-reliance, it seems clear, strong family support can encourage it. Similar findings are reported from a more recent study of seventy-three teenage boys (Offer, 1969).

This same pattern of self-reliance resting on a secure attachment to a trusted figure, and developing from it, can be seen as early as a child's first birthday. Whether these early patterns are true forerunners of later ones, or not, must await further research. To those with experience of family psychiatry, however, it seems probable that they are.

DEVELOPMENT DURING INFANCY

Since Freud's earliest work a main tenet of psychoanalysis has been that the foundations of personality are laid during the early years of childhood. Opinion has differed, however, as to which years are the most important, what psychological processes are involved and what experiences are influential in determining outcome. So long as relevant empirical data were lacking it was inevitable that debate should reach deadlock. Now, however, thanks to the work of psychoanalysts, clinically oriented psychologists and ethologists, the position is changing. Though data are still woefully insufficient, enough are available to permit an attempt at a systematic articulation of data and theory. Thanks to developments in theoretical biology, moreover, theory can itself be reformulated in ways better suited to the data. Thus prospects for advance are now good.

Among those in the van of this movement is Mary Salter Ainsworth, who worked in the Child Development Research Unit at the Tavistock between 1950 and 1954 and who, since leaving, has continued to work on problems of attachment and

separation. Resulting from this she has published a naturalistic study of mother-infant interaction in Uganda (Ainsworth, 1967) and is now presenting ~~some early~~ results of a planned study of mother-infant interaction in white middle-class homes in Baltimore, Maryland.

During her study of infancy in Uganda Ainsworth noticed how infants, once mobile, commonly use mother as a base from which to explore. When conditions are favourable they move away from mother on exploratory excursions and return to her again from time to time. By eight months of age almost every infant observed who had had a stable mother-figure to whom to become attached showed this behaviour; but should mother be absent such organized excursions became much less evident or ceased. Subsequently Anderson (1972) has made similar observations of exploration from a base of children aged between fifteen months and two and a half years playing in a secluded part of a London park whilst mother sits quietly on a seat.

In her carefully planned project in Baltimore, Ainsworth is not only able to study this kind of behaviour more closely but has described many individual variations of it to be seen in a sample of twenty-three infants* at twelve months of age. Observations have been made of the infants' exploratory and attachment behaviour, and the balance between them, both when the infants are at home with mother and also when they are placed in a slightly strange test situation. In addition, having obtained data on the type of mothering each infant had been receiving throughout his first year of life (by means of prolonged observation sessions at three-weekly intervals in the child's home), Ainsworth is in a position to propose hypotheses linking certain types of behavioural organization at twelve months with certain types of preceding mothering experience. The project is described and preliminary findings reported in Ainsworth and Bell (1970); individual differences and their

* Although the total sample studied in the strange situation comprises fifty-six infants, only twenty-three of them have been observed also with mother at home.

antecedents are discussed in Ainsworth, Bell and Stayton (1971~~)~~, 1974).

The findings of the study show that, with only few exceptions, the way a particular infant of twelve months behaves with and without his mother at home and the way he behaves with and without her in a slightly strange test situation have much in common. Drawing on observations of behaviour in both types of situation it is then possible to classify the infants into five main groups, according to two criteria: (a) how much or how little they explore when in different situations and (b) how they treat mother – when she is present, when she departs and when she returns.*

The five groups, with the number of infants classifiable into each, are as follows:

GROUP P: The exploratory behaviour of an infant in this group varies with the situation and is most evident in mother's presence. He uses mother as a base, keeps note of her whereabouts, and exchanges glances with her. From time to time he returns to her and enjoys contact with her. When she returns after a brief absence he greets her warmly. No ambivalance towards her is evident. N = 8.

GROUP Q: The behaviour of these infants is much like that of infants in Group P. Where it differs is, first, that infants in this group tend to explore more actively in the strange situation, and, secondly, they tend to be somewhat ambivalent towards mother. On the one hand, if ignored by her, an infant may become intensely demanding; on the other, he

* The classification presented here, based on behaviour in *both* types of situation, is a slightly modified version of one presented by Ainsworth *et al.* (1971) in which a child's behaviour in his own home is the *sole* source of data. Infants classified here into Groups P, Q, and R are identical with the infants classified into Ainsworth's Groups I, II, and III. Those classified here into Group T are the same as those classified into Ainsworth's Group V, less one infant who, although passive at home, proved markedly independent in the strange situation test and is therefore transferred to Group S. The infants in Group S are the same as in Ainsworth's Group IV plus the one infant transferred. The classification presented here has Professor Salter Ainsworth's approval.

may ignore or avoid her in return. Yet at other times the pair are capable of happy exchanges together. N = 4.

GROUP R: An infant in this group explores very actively whether mother is present or absent, and whether the situation is familiar or strange. He tends, moreover, to have little to do with his mother, and is often not interested in being picked up by her. At other times, especially after his mother has left him alone in the strange situation, he behaves in a very contrary way, alternately seeking proximity to her and then avoiding it, or seeking contact and then wriggling away. N = 3.

GROUP S: The behaviour of infants in this group is inconsistent. Sometimes they appear very independent, though usually for brief periods only; at others they seem markedly anxious regarding mother's whereabouts. They are distinctly ambivalent about contact with her, seeking it frequently yet not seeming to enjoy it when given, or even strongly resisting it. Oddly enough, in the strange situation they tend to ignore mother's presence and to avoid both proximity and contact with her. N = 5.

GROUP T: These infants tend to be passive both at home and in the strange situation. They show relatively little exploratory behaviour but much auto-erotic behaviour. They are conspicuously anxious about mother's whereabouts and cry much in her absence; yet they can be markedly ambivalent to her when she returns. N = 3.

When an attempt is made to evaluate these different patterns of behaviour as forerunners of future personality development the eight children in Groups S and T seem the least likely to develop a stable self-reliance combined with trust in others. Some are passive in both situations; others explore but only briefly. Most of them seem anxious about mother's whereabouts, and relations with her tend to be extremely ambivalent.

The three children in Group R are most active in exploration

and appear strongly independent. Yet their relations with mother are cautious, even slightly detached. To a clinician they give the impression of being unable to trust others, and to have developed a premature independence.

The four children in Group Q are more difficult to assess. They seem to lie half way between those in Group R and those in Group P.

If the perspective adopted in this paper proves correct, it would be the eight children in Group P who would be most likely in due course to develop a stable self-reliance combined with trust in others; for they move freely and confidently between a busy interest in exploring their environment, and the people and things in it, and keeping in intimate touch with mother. It is true that they often show less self-reliance than those in Groups Q and R, and that in the strange situation they are more affected than the latter by mother's brief absences. Yet their relations with mother seem always to be cheerful and confident, whether expressed in affectionate embraces or in the exchanging of glances and vocalizations at a distance, and this seems to promise well for their future.

When the type of mothering received by each of these infants is examined, using data obtained during the long visits the observers paid to the home every three weeks during the infant's first year of life, interesting differences appear between infants in each of the five groups.

In assessing a mother's behaviour towards her child Ainsworth uses four distinct nine-point rating scales. Ratings on these scales inter-correlate so highly, however, that in this paper the results of only one scale are drawn upon. This is a scale that measures the degree of sensitivity or insensitivity that a mother shows to her baby's signals and communications. Whereas a sensitive mother seems constantly to be 'tuned in' to receive her baby's signals, is likely to interpret them correctly and to respond to them both promptly and appropriately, an insensitive mother will often not notice her baby's signals, will

misinterpret them when she does notice them, and will then respond tardily, inappropriately or not at all.

When the ratings on this scale for the mothers of infants in each of the five groups are examined, it is found that the mothers of the eight infants in Group P rate uniformly high (range 5·5 to 9·0), those of the eleven infants in Groups R, S, and T rate uniformly low (range 1·0 to 3·5), and those of the four in Group Q are in the middle (range 4·5 to 5·5). These differences are statistically significant (using the Mann-Whitney U test).

Differences between groups, in the same direction and of roughly the same order of magnitude, are found when mothers are rated on the other three scales. Thus mothers of infants in Group P are rated highly on an acceptance-rejection scale, a cooperation-interference scale and on an accessibility-ignoring scale. Conversely, mothers of infants in Groups R, S, and T are rated medium to low on each of these three scales. Mothers of infants in Group Q show ratings that lie roughly midway between the ratings for mothers of infants in Group P and those of infants in Groups R, S, and T respectively.

Plainly a very great deal of further work will be required before it is possible to draw conclusions with any high degree of confidence. Nevertheless the overall patterns of personality development and of mother-child interaction visible at twelve months are sufficiently similar to what is seen of personality development and of parent-child interaction in later years for it to be plausible to believe that the one is the forerunner of the other. At the least, Ainsworth's findings show that an infant whose mother is sensitive, accessible and responsive to him, who accepts his behaviour and is cooperative in dealing with him, is far from being the demanding and unhappy child that some theories might suggest. Instead, mothering of this sort is evidently compatible with a child who is developing a limited measure of self-reliance by the time of his first birthday combined with a high degree of trust in his mother and enjoyment of her company.

Further strong evidence pointing in this direction is presented by Baumrind (1967) who made a very detailed study of thirty-two nursery school children, aged three and four years, and their mothers.

Thus, so far as the present all too meagre evidence goes, the hypothesis that a well-found self-reliance develops in parallel with reliance on a parent, who provides the child with a secure base from which to explore, is sustained.

POINTS OF DIFFERENCE TO CURRENT THEORETICAL FORMULATIONS

Although the theoretical schema presented here is not very different from that adopted implicitly by many practising clinicians, it differs at a number of points from much currently taught theory. Among these differences are the following:

(a) An emphasis in the present schema on the environmental parameter familiar-strange, which finds no place in traditional theory;
(b) Emphasis in the present schema on the many components of mother-infant interaction other than feeding, an overemphasis on which, it is held, has greatly hindered our understanding of personality development and the conditions that influence it;
(c) The replacement of the concepts 'dependence' and 'independence' by the concepts of attachment, trust, reliance and self-reliance;
(d) The replacement of the orally derived theory of internal objects by a theory of working models of world and self that are conceived as being constructed by each individual as a result of his experience, that determine his expectations and on the basis of which he plans.

Let us consider in turn each of these differences, which are closely inter-related.

The immense importance in the lives of animals and men

of the parameter familiar-strange has been fully recognized only during the past two decades, long after the various versions of clinical theory still taught had been formulated. In very many species, it is now known, whatever situation has become familiar to an individual is treated as though it provided safety, whereas any other situation is treated with reserve. Strangeness is responded to ambivalently; on the one hand it elicits fear and withdrawal, on the other it elicits curiosity and investigation. Which of these antithetic responses become dominant depends on many variables, the degree of strangeness of the situation, the presence or absence of a companion, and whether the individual responding is mature or immature, fit or fatigued, healthy or sick.

Why the properties of familiarity and strangeness should have come to have such powerful effects on behaviour is discussed in the final section of the paper, with special reference to their role in protection.

So long as the influence on man's behaviour of familiarity and strangeness was not appreciated, the conditions leading a child to become attached to his mother were little understood. The most plausible view, subscribed to by Freud and most other analysts and also by learning theorists, was that being fed by mother was the major variable. This theory, a theory of secondary drive, although never supported by systematic evidence or argument, soon became widely accepted and led naturally to two other views both of which have attracted strong support. One is that what happens during the early months of life must be of very special importance for later development. The second is that, once a child has learned to feed himself, there is no further reason for him to be demanding of his mother's presence: he should therefore grow out of such 'dependency', which is thenceforward stigmatized as infantile or babyish.

The view taken here, and supported by much evidence (Bowlby, 1969), is that food plays only a marginal role in a child's attachment to his mother, that attachment behaviour is shown most strongly during the second and third years of life

and persists at less intensity indefinitely, and that the function of attachment behaviour is protection. Corollaries of this view are that involuntary separation and loss are potentially traumatic over many years of infancy, childhood, and adolescence, and that, at appropriate degrees of intensity, the propensity to show attachment behaviour is a healthy characteristic and in no sense infantile.

From out of the same traditional assumption, that a child becomes attached to his mother because of his dependence upon her as the source of his physiological gratifications, come the concepts and terminology of 'dependence' and 'independence'. Once a child can provide for himself, say advocates of the theory of secondary drive, he should become independent. Henceforward, therefore, signs of dependency are to be regarded as regressive. Thus, once again, any strong desire for the presence of an attachment figure comes to be regarded as an expression of an 'infantile need', part of a 'baby' self that should have been left behind.

As terms and concepts in which to express the theory advanced here 'dependence' and 'independence' have a number of grave objections; they are therefore replaced by terms and concepts such as 'trust in', 'attached to', 'reliance on' and 'self-reliance'. First, dependence and independence are inevitably conceived as being mutually exclusive; whereas, as already emphasized, reliance on others and self-reliance are not only compatible but complementary to one another. Secondly, to describe someone as 'dependent' inevitably carries with it a pejorative flavour, whereas to describe someone as 'relying on another' does not. Thirdly, whereas the concept of attachment implies always attachment to one (or more) specially loved person(s), the concept of dependency entails no such relationship but instead tends to be anonymous.

Also much influenced by the special role given to feeding and orality in psychoanalytic theorizing is the concept of 'internal object', a concept that is in many ways ambiguous (Strachey, 1941). In its place can be put the concept, derived

from cognitive psychology and control theory, of an individual developing within himself one or more working models representing principle features of the world about him and of himself as an agent in it. Such working models determine his expectations and forecasts and provide him with tools for constructing plans of action.

What in traditional theory is termed a 'good object' can be reformulated within this framework as a working model of an attachment figure who is conceived as accessible, trustworthy, and ready to help when called upon. Similarly, what in traditional theory is termed a 'bad object' can be reformulated as a working model of an attachment figure to whom are attributed such characteristics as uncertain accessibility, unwillingness to respond helpfully, or perhaps the likelihood of responding hostilely. In an analogous way an individual is thought to construct a working model of himself towards whom others will respond in certain predictable ways. The concept of a working model of the self comprehends data at present conceived in terms of self-image, self-esteem, etc.

The extent to which such working models are valid products of a child's actual experience over the years or are distorted versions of such experience is a matter of the greatest importance. Work in family psychiatry during the past twenty-five years has presented much data suggesting that the form models take is in fact far more strongly determined by a child's actual experiences throughout childhood than was formerly supposed. This is a field of vital interest and calls urgently for skilled investigation. A particular clinical and research problem is that disturbed individuals seem often to maintain within themselves more than one working model both of the world and of the self in it. Such multiple models, moreover, are frequently incompatible with each other and can be more or less unconscious.

Enough perhaps has been said to show that the concept of working models is central to the schema proposed. The concept can be elaborated to enable many aspects of personality

structure and internal world to be described in ways that permit precision and rigorous research.

Thus the theory advanced here is not only couched in different language but contains a number of concepts distinct from those of traditional theory. Amongst many other things, these concepts enable a fresh approach to be made to the age-long problem of separation anxiety which, when excessive, is inimical to the development of self-reliance.

THE PROBLEM OF SEPARATION ANXIETY

The many observations of the behaviour of young children when removed from their parents and placed in strange surroundings with strange people, described by James Robertson and others during the past twenty years, have still not been fully articulated into clinical theory. There is still no agreement on why the experience should be so distressing to a child at the time, nor why afterwards he should be so intensely apprehensive lest it happen again.

During recent years a number of experiments have been conducted on young monkeys in which they are separated from mother, usually for about one week. Whatever differences there may prove to be between the responses of monkeys and humans in such a situation, what is immediately striking is the similarity of response. In most species of monkey studied, protest at separation and depression during it are very pronounced, and, after reunion, clinging to mother is much increased. During the subsequent months, though individuals vary, the separated infants tend on average to explore less and to cling more; and they remain detectably more timid than young monkeys that have not had a separation. (For a review of these findings see Hinde and Spencer-Booth, 1971.)

These monkey studies are of great value in that:

(a) They provide clear evidence from planned experiments that hold stable many variables that in real life observations of humans make firm conclusions difficult to draw;

(b) They demonstrate that, even when all other variables are held constant, a period of separation from mother elicits protest and depression during the separation and much increased separation anxiety after it;
(c) They make it clear that the kinds of response to separation that are seen in humans can in other species be mediated at a primitive, and presumably infra-symbolic, level.

This last finding calls in question the various clinically derived theories that seek to explain separation anxiety, since most of them take it virtually for granted that involuntary separation from a mother-figure cannot of itself elicit anxiety or fear and that there must therefore be some other danger that is foreseen and feared. Many and diverse suggestions have been advanced of what this other danger might be. For example, Freud (1926), who from the first regarded separation anxiety as a key problem, suggested that for humans the ultimate 'danger-situation is a recognized, remembered, expected situation of helplessness'. Melanie Klein has advanced theories invoking a death instinct and fear of annihilation, and also theories deriving from her views about depressive and persecutory anxiety. Birth trauma is yet another suggestion. On reading the literature it is abundantly clear that many of the most strenuously debated issues in psychopathology and psychotherapy have turned, and still turn, on how we conceptualize the origin and nature of separation anxiety (Bowlby, 1960, 1961, 1973). Since the debate has been going on for so long and with so little progress the question is raised whether the wrong questions are being asked and/or the wrong initial assumptions made. Let us examine, therefore, what the initial assumptions have been.

Almost all theory about what arouses fear and anxiety in humans has started from the assumption that fear is aroused appropriately only in situations that are perceived as intrinsically painful or dangerous. Such perception is thought to derive either from previous experience of pain or else from

some innate awareness of dangerous forces within. One or other of these assumptions is to be found in learning theory, in traditional psychiatry, as exemplified for example in a paper by Lewis (1967), and in all the various versions of psychoanalysis and its derivatives.

Anyone who adopts an assumption of this sort is, of course, very quickly faced with the fact that human beings frequently show fear in a number of common situations that do not seem inherently painful or dangerous. How many of us, it may be asked, would relish entering a completely strange house in the dark on our own? What a relief it would be were we to have a companion with us, or had a good light, or preferably both companion and light. Although it is during childhood that situations of this sort elicit fear most readily and intensely, it is idle to pretend that adults are above such things. To refer to fears of these kinds as 'infantile', as is often done, begs a lot of questions.

It is striking how few empirical studies there have been of the situations that commonly arouse fear in humans since Jersild's systematic work of the early thirties. The publications in which they are reported (e.g. Jersild and Holmes, 1935, Jersild, 1943) are mines of useful information.

In children between the second and fifth years of life, Jersild reports, there are a number of well-defined situations that commonly elicit fear. For example, records of 136 children over a three-week period show that not less than 40 per cent of them showed fear, on one occasion at least, when confronted with *each* of the following: (a) noise and events associated with noise, (b) height, (c) strange people, or familiar people in strange guise, (d) strange objects and situations, (e) animals, (f) pain or persons associated with pain.

There was also abundant evidence that children showed less fear when accompanied by an adult than when alone. To anyone familiar with children these findings are hardly revolutionary.

Yet it is not easy to square them with the assumptions from

which most theorizing starts. Freud was keenly alive to the problem and confessed himself perplexed. Amongst the solutions he tried was his well-known attempt to distinguish between a real danger and an unknown danger. The argument he advances in *Inhibitions, Symptoms and Anxiety* (1926) can be put in a nutshell, using his own words. 'A real danger is a danger which threatens a person from an external object.' Whenever anxiety is 'about a known danger', therefore, it can be regarded as 'realistic anxiety'; whereas whenever it is 'about an unknown danger' it is to be regarded as 'neurotic anxiety'. Since fears of being alone in the dark or with strangers are, in Freud's view, about unknown dangers they are to be judged neurotic (*Standard Edition*, Vol. 20, pp. 165–7). Because all children experience such fears, moreover, all children are held to suffer from neurosis (pp. 147–8). There must be many who find themselves dissatisfied with this solution.

The difficulties Freud struggled with disappear when a comparative approach to human fear is adopted. For it becomes evident that man is by no means the only species to show fear in situations that are not intrinsically painful or dangerous (Hinde, 1970). Animals of very many species show fear behaviour in response to noise and other sudden changes of stimulation, to darkness, and also to strangers and strange events. The visual cliff and a stimulus that rapidly expands also regularly elicit fear in animals of a number of species.

When we ask how it has come about that situations of these sorts should so readily elicit fear in animals of so many species, it is not difficult to see that, whilst none of them is *intrinsically* dangerous, each is in some degree *potentially* dangerous. Put in another way, while none may carry a *high* risk of danger, each carries a slightly *increased* risk of danger, even if the risk is increased, say, only from 1 per cent to 5 per cent.

Looked at in this light each of these fear-arousing situations is seen to be a natural clue to an increased risk of danger. To respond with fear to all such situations is therefore to reduce risks. Because such behaviour has survival value, the argument

runs, the genetic equipment of a species becomes such that each member of it at birth is biased to develop so that it usually comes to behave in these typical ways. Man is no exception.

A distinction that is invoked here that is a commonplace for ethologists but a source of much confusion and perplexity amongst psychologists, both experimental and clinical, is the distinction between causation and biological function – the distinction between, on the one hand, what conditions elicit behaviour and, on the other, what contribution to species survival such behaviour may make. In this theory strangeness and the other natural clues is each regarded as playing a causal role in eliciting fear behaviour; whilst the function of such behaviour is protection.

Perhaps the distinction between the cause and the function of a piece of behaviour can be clarified by reference to sexual behaviour in which the distinction is so patently obvious that it is usually taken for granted and virtually forgotten. Spelled out the distinction runs as follows: hormonal states of the organism and certain characteristics of the partner, together, lead to sexual interest and play causal roles in eliciting sexual behaviour. The biological function of that behaviour however is another matter; it is reproduction. Because causation and function are distinct, it is possible, by means of contraception, to intervene between the behaviour and the function it serves.

In animals of all non-human species behaviour is engaged in without the animal (presumably) having any insight whatever into function. The same is true also of most humans most of the time. Seen in this light there is nothing to surprise us that humans should habitually respond with fear behaviour in certain situations despite the fact that an external observer might know that in such situations risk to life is only marginally increased, or even not increased at all. What a person responds to initially is simply the situation – sudden change of noise or light level, a strange face or strange happening, sudden

movement – not to any estimate of risk. Sober calculation of risk may or may not follow.

Unwilling separation of young from parent, or for that matter of adult from trusted companion, can be regarded simply as another situation of the same sort, although rather a special example of it. Even in civilized communities there are many circumstances in which the risk of danger is somewhat greater when one is alone than when one is with a companion. This is especially so during childhood. For example, risk of accidents in the home are obviously rather greater when a child is left alone than when his mother or father is around. The same is true of accidents in the streets. During 1968 in the London Borough of Southwark, 46 per cent of all traffic accidents happened to children under fifteen, with the highest incidence in the age-groups three to nine. More than 60 per cent of these children were entirely alone and two-thirds of the remainder in the company only of another child. For the elderly or sick, living alone is a notorious hazard. Even for healthy adult males to go hill-walking or climbing alone is materially to increase risk to life. In the environment in which man evolved the risks attendant on being alone are likely to have been much greater. Reflection shows therefore that, because being alone increases risk, there is good reason why man should have evolved behavioural systems that lead him to avoid it. For humans to respond with fear to loss of a trusted companion is thus no more puzzling than that he should respond with fear to any of the other natural clues to potential danger – strangeness, sudden movement, sudden change in noise or light level. In every case to respond so has survival value.

A very special feature of fear behaviour both in humans and other animals is the degree to which it is increased in situations characterized by the presence of two or more of the natural clues; for example, the stranger who suddenly approaches, the strange dog that barks, the unexpected noise heard in the dark. Commenting on the twenty-one-day observations made by

parents of fear-arousing situations, Jersild and Holmes (1935) note that combinations of two or more of the following features were frequently reported to be present together: noise, strange people and situations, the dark, sudden and unexpected movement, and being alone. Whereas a situation characterized by a single one of these features might only alert, fear, more or less intense, may well be aroused when several are present together.

Because the response to a combination of factors is often so dramatically greater than, or different from, what it is to any one singly, it is convenient to refer to such situations as 'compound', a term chosen to echo the chemical analogue (Bowlby, 1973).

In keeping with other findings on the effects of compound situations, experiments both with human children and with rhesus monkeys (Rowell and Hinde, 1963) show what a tremendous difference to the intensity of fear responses is made by the presence or absence of a trusted companion. For example, Jersild and Holmes (1935) found that, when children in their third and fourth years were asked to go alone to find a ball that had gone down a dark passage, half refused to do so despite encouragement from the experimenter. When the experimenter accompanied them, however, almost all were ready to do it. Differences of a similar kind were seen in a number of other slightly frightening situations, for example, when a child was asked to approach and pat a large dog brought in on a lead.

These findings are so much in keeping with common experience that it may seem absurd to labour them. Yet, it is evident that, when psychologists and psychiatrists come to theorize about fear and anxiety, the significance of these phenomena is gravely underestimated. For example, when due attention is paid to these findings it ceases to be a mystery that, in all but very familiar situations, fear and anxiety are greatly reduced by the mere presence of a trusted companion. These findings enable us to understand also why the accessibility of parents and their willingness to respond provides an

infant, a child, an adolescent and a young adult with conditions in which he feels secure and with a base from which he feels confident to explore. They cast light too on the way that, from adolescence onwards, other trusted figures can come to provide similar services.

This brings the argument full circle and helps to explain how it comes about that strong and consistent support from parents, combined with encouragement and respect for a child's autonomy, so far from sapping self-reliance, provide the conditions in which it can best grow. It helps to explain, too, why, conversely, an experience of separation or loss, or threats of separation or loss, especially when used by parents as sanctions for good behaviour, can undermine a child's trust both in others and in himself, and so lead to one or another deviation from optimum development – to lack of self-confidence, to chronic anxiety or depression, to aloof non-committal, or to defiant independence that has a hollow ring.

A well-based self-reliance, we may conclude, is usually the product of slow and unchecked growth from infancy into maturity during which, through interaction with trustworthy and encouraging others, a person learns how to combine trust in others with trust in himself.

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To promote understanding of the emotional needs of infants and young children

Robertson Centre

51 Corringham Road London NW11 7BS
01-455 3914

30th December 1976

Dr John Bowlby,
Wyldes Close Corner,
Hampstead Way, NW11.

Dear John,

Thank you for your letter of 28th December and for the paper by Julian Katz which you kindly enclosed.

I am returning the paper because I already have a copy which was published in one of the nursing journals which are amongs the publications now flooding in to help us orientate to the Australian scene.

My shingles lingered for a depressing months, then wholy vanished in mid-November during a sunny week in Majorca of swimming and climbing. Then, unfortunately, on the very last day Joyce did something to her back and has been on a board ever since - nothing so mild, I am afraid, as the 'flu. Fortunately, being 'retired' I have been able to look after her and keep her out of hospital. There has been some easement in the last couple of days from this most painful and demoralising disc condition, but because it has gone on so long she will probably go into hospital in the next few days for a full check-up.

With Australia only 16 to 18 weeks away there is some anxiety lest she is unable to cope with the long flight and the stress of the four day Conference which is to be based on our work - plus seminars for judges, et al arising from our current work in family law and their uncertainties about recent Australian legislation requiring more understanding of what is in the best interests of young children when court decisions have to be made.

But we are assured that the transition once began from immobility to activity can be quiet dramatic. We have to hope so.

Yours sincerely,

James Robertson

PS. Our Conference, incidentally, is not primarily about children in hospital. We now operate on the view that a common language hes to be developed for all people dealing with young children, and that to compartment them is mistaken. Therefore the Conference will deal with 'The Emotional Needs of Infants and Young Children: Implications for Policy and Practice' and will cover family life, day and residential nursery care, child minding, one-parent-families - attempting to provide a unifying approach.

We are also firmly holding the agem group down to birth to three years, having already in Britain had some success in getting it understood that the umbrella term 'under 5s' is toomwide sine the under-3s are very different from the over-3s. Some Australian pediatricians seem to have pulled out because of this focussing down. Most people find it easier and less disturbing to talke about the IO year olds than about the more vulnerable under-3s; there is also much anxiety about what they do not know.

An event which began modestly has now attracetd Government support, and participation by key administrators who are said to have much money to dispense but no concepts. So it is hoped the Conference will produce some in concencus. The string op official contributors is quite imposing, but Joyce and I are firmly in control of the first two days with no other speakers, and in as consultants for the last two. We have just heard that with some months to go applications are twice as great as places.

To promote understanding of the emotional needs of infants and young children

Robertson Centre
51 Corringham Road London NW11 7BS
01-455 3914

27th October 1976

Dear John,

It is kind of you and Ursula to enquire after my health.

Yesterday~~m~~, just three weeks after onset, I began to think I would survive and today I can contemplate that life may be worth living again.

I had no knowledge of shingles except as a name and a condition that afflicted possibly odd people. But now I know it for a real, painful, debilitating and depressive illness.

I was guinea-pig for a new wonder drug HERPID which put an end to the pain in a couple of days, which was a great relief. But of course the illness had still to run its course, yet with relief from pain one feels one must be all right - and only defeat through fatigue ultimately convinces otherwise. In that state I had the heaviest week of term, facing for a day the new rumbustious intake of social worker students on the Friday then 500 conference members in Hampshire on the Saturday. After that, near collapse.

But as I have said, life begins to call again.

I trust that you and Ursula are well. Apart from my shingles we are in good form. Our only complaint is that have not yet found the leisure we imagined to be included in 'retirement'.

Yours,

James Robertson

G. BRONSON

Feb 6'76.

Dear John:

To keep you up to date — my recently finished ms. on wariness & fear. It is long, but the summary at the end will give the essence.

It seems long since I knew what you were up to, but I imagine that I can guess. I hope it goes well, and is comfortably near to completion. Have you by the way, seen E. O. Wilson, Sociobiology? A truly extraordinary work reviewing the evolutionary approach to man; in its scope, scholarship, and style I would place it next to Darwin's "Origins" — I shock myself a bit when I say this, but would defend it.

Wanda and I might possibly be going through London some time this summer, and a visit with you and Ursula would be a delight. We will let you know when (& if) our plans work out.

Regards to you both,

Brandon.

HARRÉ

JB/DES 27th May 1975

Dr. R. Harré
Sub Faculty of Philosophy
12 Merton Street
OXFORD

Dear

Thank you for your letter which reached me on a working holiday in Skye.

The difference between us lies, I think, in your idea that in ordinary times the determinants of behaviour in which I am interested are "contained in and over ridden by rule and convention", whereas I hold that in health there is constant co-operation and mutual give and take between the two sorts of determinant and that pathology is due mainly to a derangement of the biological. But, as you suggest, even in the forms taken by pathological behaviour rules and conventions often play a major part.

The rules and conventions formed in human societies are far from random and are greatly constrained, I believe, by our biological heritage. When they deviate too far from what suits, the rules and conventions are changed back towards a norm. A good example is the history of family living in many Israeli Kibbutzim. Some were deliberately set up with a social structure intended to do without family life or reduce it to a shadow. As years have gone by shifts have taken place all of them towards a reconstitution of family living. Amongst the leaders of this change, I am told, have been individuals who were themselves reared in the minimum family days. As social anthropologists have pointed out, we have only to look at the social structures characteristic of other species to see the common structures that underlie the diversities present amongst man's ways of living.

Another set of phenomena that are relevant to my position are mourning customs and the emotional responses of bereaved people (on which I am now working). Customs certainly vary greatly between cultures but, again, are far from random. The emotional responses and behaviour of bereaved people seem to vary very little, and are of tremendous social importance by greatly influencing a bereaved person's social behaviour, often for many years. Such changes are so widespread that it would be artificial to regard them as pathological, even though a sizable minority, perhaps up to 15% become so (and thus make a major contribution to the sum total of mental illhealth).

In my view the gravity of responses to loss shown by ordinary grown-up people can be understood only by reference to our biological heritage. It is true that such episodes are not the "very ordinary humdrum passages of life" to which you refer. Yet they are of great moment in the lives of nearly everyone, sooner or later, and a viable social psychology will have to find concepts for dealing with their consequences.

I suspect the differences between us stem largely from our being interested in different aspects of social behaviour, which are certainly not distinguishable in terms of health and pathology as you are inclined to think, but which lead me to emphasise the biological determinants and you the cultural ones.

I shall look forward to hearing what you think. When I get back to London (in early June) I shall read your article in the Mischel book.

Yours sincerely

John Bowlby, M.D.

HARRE

SUB-FACULTY OF PHILOSOPHY,
12, MERTON STREET,
OXFORD.

1 May 1975

Dear Dr Bowlby,

I was delighted to get your letter. There may be a difference between us, which would be worth debating. Let me go a stage further than in the Richards article.

I would be the last to deny that there is a biological heritage, and that this is modulated by the experiences you so beautifully describe. My point is that, via the transition to rule and convention-following and the acquisition of skill in the presentation of conventional forms of the self, we come to a situation in adulthood in which what it is to be 'parent' or 'child' or 'lover' or 'friend' or 'rival' etc. is a matter of the local ethnography. And that many of these social forms are sufficiently a matter of rule-following as to operate in despite of powerful surges of feeling. I tried to give this a bit of substance in a discussion of possible social universals in a piece in Mischel (ed.) On Understanding Persons, Blackwell,

SUB-FACULTY OF PHILOSOPHY,
12, MERTON STREET,
OXFORD.

1974, I don't know whether you've seen the book.

It would follow from my argument and the phenomena I draw upon, e.g. the conventionalization of family life, that when the emotional or sentiment basis, coming from our biological X 'Bowlby' experiences of childhood, cannot be contained in and overridden by the system of rules, then we have pathological behaviour. This would fit well with your notion that attachment persists into adult life, for me as a feeling or system of feelings, urges, sentiments etc..

I wonder whether the difference I felt there was between us (gathered from your first volume) is more a matter of our paradigms of action. I am looking at the very ordinary, humdrum passages of life, you at the psychopathological moments (not exclusively, but perhaps primarily). Maybe between us we cover the spectrum. But again I have a sneaking feeling that psychopathological behaviour may, too, have a strong element of convention. Do let me know what you think.

Yours, Rom Harre

ROY SCHAFER

Int. J. PsA 56, 41-55

JB/DRS

6th September 1975

Dr. Roy Schafer
Yale University Health Services
17 Hillhouse Avenue
New Haven
CONNECTICUT 06520

Dear Dr. Schafer:

I have been reading your article PSYCHOANALYSIS WITHOUT PSYCHODYNAMICS in the International Journal with much interest and appreciation. So far as I can see, our positions are highly compatible within the limitations you set yourself. Throughout my work on ATTACHMENT AND LOSS I have been doing my best to adhere to the kind of action language you advocate and for reasons similar to yours. Like you, I have explicitly discarded dynamic and economic concepts (ATTACHMENT pp 13-16).

The only substantial differences between us seem to lie in how we approach causation and the use we make of evolutionary biology. I notice that on page 52 you explicitly exclude causation from your area of concern. Although I respect the legitimacy of your doing so, I think it impoverishes our discipline and is hardly in keeping with Freud's aspirations. In my own work I am attempting to replace Freud's outdated formulations by proposals deriving from control theory, Hinde's version of ethology and evolution theory. In drawing on such ideas in an attempt to understand human behaviour and experience I am clearly taking a very different course to you. For example, whilst recognizing man as biologically distinctive, I also emphasize the continuities with other species. Moreover, so far from sharing your belief that a Darwinian viewpoint reduces man to brute, I hold that it helps explain very many puzzling features of human life including much psychopathology. Your outright rejection of everything biological makes me think that you have been exposed to a barrage of what I would regard as naive and misguided attempts to apply biological concepts to man, as in learning theory. How successful my own attempts will prove remains to be seen. Meanwhile, I regard the big task which Freud set us is to find a way to integrate conceptually our distinctively human characteristics with those that derive from our animal inheritance.

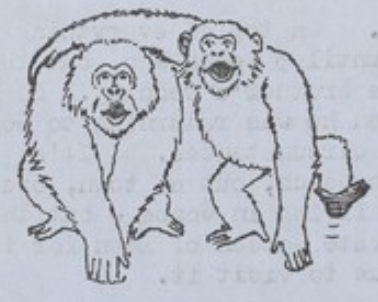
(over)

With good wishes for your further contributions.

Yours sincerely

John Bowlby, M.D.

GOODALL 1975



GOMBE STREAM RESEARCH CENTRE
P. O. Box 185 KIGOMA,
TANZANIA.

H.Q. P.O. Box 727,
Dar es Salaam.

20 August, 1975

Dear John,

This is to thank you very much indeed for your letter. It meant a lot to me - you would be surprised how few people actually wrote to me during that awful, ghastly time of waiting, and I was very touched that you did.

You wrote that I would be far too busy to answer. That was the awful thing. I really wasn't busy - there really was very little to do so far as helping the students was concerned. There was a vast amount I could have been doing in other ways - but I found it virtually impossible to set to work and do constructive things or think useful thoughts. I just felt like doing nothing. Perhaps worst of all was a secret dread that part of the dead feeling would grow when the students were all safely back - the knowledge of the blow to Gombe and to me personally. While there were students to worry about then I could push the other fear away to the back of my mind, telling myself it was an unworthy thought. Then Steve finally came back, and it was like the lifting of a great black cloud.

Now I can think and wrk again. Of course Gombe has suffered a terrible blow - right at its peak, too. I had a group of excellent students, and we were about to find out things that I have wanted to know about for years - relationship between communities, for example. Now it will be years and years before I can work up towards this goal again. On the other hand, there is a lot left. The Tanzanian field assistants are very well trained. We had an excellent young man who had just learned how to organize the field assistants and the records - he came to translate and finds himself on-the-spot director! But he's doing very, very well. I can get to Gombe for about a week every 6 weeks or so. Long reports come in the mail each week. And we have daily contact by radio so that I can advise straight away on any difficult problems. I wish I was not going off to the States quite so soon, but I could hardly let Stanford down at this late time.

The chimps are getting more human - in that we are finding out some rather horrible aspects. In a year we think that adult males of our home community "murdered" 3 adult males, on 3 separate occasions, of a neighbouring community. One of these was my very old friend Goliath - an old, old male. Why they should sneak up to him and attack him so brutally that he could not sit up afterwards (and then we never found him again) I simply can't imagine. But even worse happend last week. The oldish female Passion, who has 10 year old daughter Pom and 3 year old infant Prof chased young female Gilka out of camp, attacked her, seized her 3 week old daughter, bit her on the head and then, for the rest of the day, proceeded to eat the baby, sharing the meat with Pom. I am still aghast at this, and do not know how to interpret it. I now begin to wonder about the fate of other tiny babies which have mysteriously disappeared with no trace.

I must close and get on with everything. On top of everything else I am Grub's teacher now. He had a tutor until 5 weeks ago, but the young man left and went back to England for his brother's wedding. He had planned to retu~~r~~n, initially - but that was when he was returning to Gombe. I don't blame him for deciding not too, in the circumstances. It's okay here in Dar - at least the house is on the beach, out of town, breezy - but its not Gombe! Nothing will ever be like living in Gombe - but then, one should not expect to live in one's own private Garden of Eden for too long in this world, and at least we can continue to visit it.

Thank you again for writing to me, and please give my warmest regards to Ursula.

Yours ever,

Jane

KUNJO LA KWANZA HAPA

Sender's name and address :

Gombe Stream Research Centre
P. O. Box 185, KIGOMA,
Tanzania.

AN AIR LETTER SHOULD NOT CONTAIN ANY ENCLOSURE; IF IT DOES IT WILL BE SURCHARGED OR SENT BY ORDINARY MAIL.

Issued by the East African Posts and Telecommunications Corporation

SECOND FOLD HERE

KUNJO LA PILI HAPA

PAR AVION
AEROGRAMME

Dr. John Bowlby,
Tavistock Clinic,
Belsize Lane,
London, NW3 5BA
England.

SLATER

UNIVERSITY OF LONDON
BRITISH POSTGRADUATE MEDICAL FEDERATION
THE BETHLEM ROYAL HOSPITAL
AND
THE MAUDSLEY HOSPITAL

INSTITUTE OF PSYCHIATRY
DE CRESPIGNY PARK
DENMARK HILL
LONDON, SE5 8AF
01-703 5411

6 May 1975

Dear John,

Many thanks for yours of the 9th April. (I may have answered it before, but do not know, my memory being what it is — there it is on my desk with unanswered correspondence.)

I only wish you'd put your points in a letter to the B.J. Psych., but I expect you feel with me that much public debate is a bearniess

of the spirit. Of course you and I are not
far apart. I hope very much that
your and your co-workers' efforts (Emile) thrive
and the new science you envisage
takes root and grows

Best wishes

Erich

SLATER

JB/DES

Dr Eliot Slater.

9th April 1975

Dear

I have enjoyed reading your recent article in the Journal (which has only just reached me) and would like to make a few comments. Thank you for your references to my work.

I think you underestimate the scientific potential that psychoanalysis originally had and also the possibilities that remain. There seems little doubt that Freud began his work believing he could create a scientifically based body of knowledge but that as time went on he succumbed increasingly to the temptation to create an all embracing system. In my view his original ideas can properly be regarded as primitive scientific hypotheses. In Harre's terms, which I believe are nearer to working science than those of the logical positivists, Freud's early ideas were scientific models plausible and legitimate in the climate of the times but which proved in the event ill-designed to lead on to the discovery of true causal mechanisms. (Vaihinger's term 'fiction' does them much less than justice). The trouble came, I think, when Freud recognised their inadequacies and, instead of attempting to replace the early models with something more hopeful, moved away from scientific method - though he never abandoned his original hopes.

During the sixty years or so since then there have always been analysts who have aspired to carry out Freud's original intention, or at least yearned for it to be done, even though many others have been perfectly content to have an all-embracing System with no scientific pretensions at all.

The interesting question is whether Freud's original plans can still be carried through, and if so how. There are no precedents in the history of science for the procedure you recommend in your last paragraph - that of pruning the elaborate theory etc. etc. By contrast, what has happened repeatedly is that some part of the phenomena which a previous all embracing philosophy has attempted to 'explain' is carved off and a new model advanced which both tries to account more adequately for those phenomena and also proposes new and possible causal mechanism. When successful this is the birth of a new discipline or sub-discipline, more limited than the original system but compensating for that by its capability for growth.

(over)

This of course is my own strategy. All the human phenomena embraced by such concepts as attachment and affectional bonds, separation and loss, can be dissected from the remainder of the phenomena that psychoanalysis tries to explain and a new attempt made to deal with them scientifically. The attachment and loss phenomena have always been a major theme in psychoanalysis as I show in the appendices to my work and there is nowadays widespread agreement among people in dynamic psychiatry that they are of great relevance for understanding a sizable proportion of psychiatric casualties. As you already know, my hope is that a model derived from ethology, control theory and cognitive psychology (all mutually compatible) can serve. My friend, David Hamburg, believes that such a psychological model is also compatible with work going forward in neuroendocrinology and brain physiology and so on to genetics. (see Hamburg & al in PARAMETERS OF EMOTION ed. L. Levi, 1974.

From all this you will see that I am quite hopeful that your psychiatrist in search of a science will only have to wait another decade or so before there will be one available to him. So we must both live to be ninety!

Yours

John Bowlby, M.D.

Dr. Eliot Slater
Institute of Psychiatry
The Maudsley Hospital
Denmark Hill
LONDON S.E.5.

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Yours

John Bowlby, M.D.

Dr. Eliot Slater
Institute of Psychiatry
The Maudsley Hospital
Denmark Hill
LONDON S.E.5.

Nov 1. 74

Juliet HOPKINS

Dear John,

I wonder if you can help me. I am currently enrolled on a course in Family Therapy run by Kitty La Perriere from the Nathan Ackerman Inst., and I shall shortly be working with two families while my supervisor hides behind the one-way mirror. What I would like is some guidance about what, if anything, on the subject of family therapy is worth reading. The American literature that I have so far browsed through is not at all impressive. There is no good library on human relations here where I can search for myself, so it would be most helpful to have a couple or so of good references to acquire through inter-library loan. I am not sure where you stand on family therapy but suppose you would consider it a good thing! However, some of the methods used in this country I'm sure would not be to your taste.

We are enjoying Princeton very much, especially our long-extended summer (today is 75°) and the magnificent foliage. Keith is writing his well, the children like their school and are thrilled with the freedom to cycle at will around the neighbourhood. I find it a great pleasure to be in a small town after the hustle of London

Juliet Hopkins 1974

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Dr John Bowlby
Wyldes Close Corner
Hampstead Way
London N.W.11.
U.K.

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(2) Second fold

From Hopkins
66 Einstein Drive
Princeton
NJ 08540

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← (1) Fold first at notches →

Additional message area

and although I miss my work I certainly enjoy the freedom. We have seen Lis Dunbar several times and visited her lovely home on Shelter Island. Did you know that she plans to take sabbatical in 76 in London, and maybe write about Evan, his friends and the Labour movement at that time.

I hope Vol III is taking good shape.

My love to you, Ursula from Juliet

PADDINGTON GROUP HOSPITAL MANAGEMENT COMMITTEE

The Tavistock Clinic
ADULT DEPARTMENT

TAVISTOCK CENTRE, BELSIZE LANE, LONDON, N.W.3

Telephone: 01-435 7111 Ext.:

20 June 1973

Dear Doctor Bowlby,

Next Wednesday I am obliged to go to Birmingham and hence cannot attend the last meeting of your seminar. I have enjoyed the seminar very much and I think that it will have a lasting effect on the way I think about personality development and psychopathology. The fact that the seminar could work successfully even in your absence seems to me to be evidence of the warm regard that the participants developed for each other and for you. Thank you very much for leading this very meaningful series of discussions.

Yours sincerely,

Alan Morgenstern

3 March 73

Dear John and Ursula.

It was an unforeseen pleasure for us yesterday to receive Ursula's two letters of 23 and 28 Jan — they were addressed, very naturally, to the Dept of Economics at Berkeley, but my own desk was in the Institute of Industrial Relations, in another part of the forest, so the letters must have lain in a pigeonhole until a few days ago. Yes, we did meet the Bronsons — three times indeed: we found in a way we had more in common with them than with my own opposite numbers. You spoke of him as a dark horse: I found him very shrewd & perceptive (I re-wrote a section of my lectures, on the distribution of IQs, after discussion with him) but he's still very much the engineer, and when we last met him he'd been spending the day, yes, trying out a Wankel engine in his launch. Wanda has plenty to say, but isn't boring, and she's a superb cook! You'll be meeting them soon — please tell them how much we enjoyed their company and hospitality. — Altogether we enjoyed our visit much more than I'd foreseen, and I've come back with a very different picture of the U.S. from the unhappy one I formed in 1930-31 — "culture shock" for a very Oxfordy young man was part of the trouble then, and being demoted from a fellowman to a nonentity. But this time we found a relaxed society, and were unaware of the mood of self-disparagement and failure of confidence said to have afflicted it. The signing of a peace treaty did not seem to have made much difference in itself — already by the end of 1970 Mr. Nixon had brought the bulk of the troops back, and the draft had been ended. The only manifestation on the campus was the Jesus freaks, singing hymns with joyous smiles, & guitar accompaniment, and shouting Hallelujah. We enjoyed wholehearted hospitality, and it was a very active enjoyment, for the standard of cuisine was very high, the welcome unaffected, and the talk lively. The

Dr. and Mrs. John Beasley
Guest #107
Royal Alexandra Hospital for Children
Camperdown
New South Wales
2050
Australia

BY AIR MAIL
AIR LETTER
PAR AVION AEROGRAMME

SECOND FOLD HERE

AN AIR LETTER SHOULD
NOT CONTAIN ANY ENCLOSURE;
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SENDER'S NAME AND ADDRESS (PLEASE SHOW YOUR POSTCODE)

Philip Brown
16 Bradmore Rd.
OXFORD OX2 6QP
U.K.

TO OPEN SLIT HERE

only snag was almost continuous rain – twice, or was it three times the average? It's been a pleasure to meet ~~[struck out]~~ a no less unusually dry and sunny March on our return. Now we're just off to Scotland: a weekend playgroup conference in Edinburgh (to be addressed by Jock Sutherland), then a week to fill in, and we plan to spend it in Skye, probably at Elgol, before we spend the next weekend with Tom & Anne in their new house. (At a recent meet of the South of Scotland Ski Club, Anne won the Ladies' Championship & Tom the Novices'). – I wonder on reflection how much of this letter repeats an earlier one? – Wherever this finds you, may you be continuing to enjoy your travels in every way, but we look forward to your return.

Henry.

MONTEFIORE HOSPITAL AND MEDICAL CENTER

111 EAST 210TH STREET, BRONX NEW YORK 10467

Telephone:
Area Code 212
920-4441

November 10, 1972

John Bowlby, M.D.
Wyldes Close Corner
Wildwood Rd.
London NW 11
England

Dear Dr. Bowlby:

I want to thank you again for your most helpful consultation. You will be happy to know, I'm sure, that the information and suggestions you offered have proved greatly beneficial to Elizabeth and me in our work together. She also told me about the letter you sent her, which she found quite constructive. As it has turned out, our visit could not have been better timed. Perhaps I am overly optimistic, but all the signs point now to an early resolution of her difficulties in this area.

Again, many thanks. It was indeed a pleasure to meet you.

Sincerely,

Edward J. Sachar

Edward J. Sachar, M.D.
Professor of Psychiatry
Albert Einstein College
of Medicine
at Montefiore Hospital

EJS:mab

MONTEFIORE HOSPITAL AND MEDICAL CENTER

111 EAST 210TH STREET, BRONX NEW YORK 10467

Telephone:
Area Code 212
920-4441

October 24, 1972

Dr. John Bowlby
Wyldes Close Corner
Wildwood Rd.
London NW 11
England

Dear Dr. Bowlby:

Your friend, Elizabeth Durbin, is now beginning her fifth year of analysis with me. I believe she has made excellent progress. A period of her life about which we have made tentative reconstructions involves the five months of her parents' absence when she was a small child. This experience, only vaguely remembered, appears to have significantly influenced her subsequent attitudes toward herself, her relationships, and separations. She has worked through a great deal in this area, but we both agree that it could be helpful to learn something from you about your observations of her during this time.

I realize how busy you are, but as I will be in London from October 30-November 4, I wondered whether we might arrange to talk for a bit, about this question, and anything else you feel could be helpful to Miss Durbin and myself. Such a meeting is, of course, somewhat unorthodox, but at this stage of her analysis, I doubt very much that it could be anything but useful.

Miss Durbin has, I believe, written you separately.

I will be staying at Ciba House, giving some lectures sponsored by the Ciba Foundation. I will call you at the number Miss Durbin gave me on my arrival. Many thanks.

Sincerely yours,

Edward J Sachar

Edward J. Sachar, M.D.
Professor of Psychiatry
Albert Einstein College of Medicine
at Montefiore Hospital

EJS:mab

Apt 3D
215 West 88th St
NYC 10024

October 23, 1972

Dearest John,

It doesn't really seem like it's almost two years since we walked across the Heath together, and yet so much has happened. I finally finished my doctorate, and have an article coming out in the Journal of Human Resources; and I am kept very busy at NYU teaching economics and running an undergraduate program of Metropolitan Studies. Jos and Roger and the kids really seem to be enjoying St. Louis very much, I look forward to spending Thanksgiving with them. Roger is doing well in his research, but having some problems coping with the politics of American medicine! The children flourish.

We all really enjoyed having Mummy over this summer and wished she'd stayed longer. I am just in the process of buying a large old summer house on Shelter Island (off the east coast of Long Island) which has lots of rooms. I am hoping Mummy, the G.M. Dobbins and the Secker Walkers will come next summer, if the Secker Walkers and/or

I do not make it back to England. I shall be using the Trust Fund money as down payment, so I feel very glad and grateful that I am able to afford a place where we can all enjoy being together despite being scattered over the globe. I hope maybe you can come and visit sometime also - fine sailing and fishing, it's more built up but a lot warmer than Scotland!

My main reason for writing is to tell you that my analyst, Dr. Edward Sachar, is going to be in London from Monday October 30 - Saturday November 4. He was in London last winter sometime, and as you may recall tried to get in touch with you. However, it was so much at the last minute that I didn't have time to write to you about it. This time, we have discussed the matter at some length and if you have the time, I would really appreciate it if you could talk with him.

There are two matters in particular which have come up in the analysis that I felt you might be able to provide some helpful insights. First, any observations you can recall of the period when Mummy and Daddy came to the States and I was

left with Margaret and "Bogey" would be useful. I remember nothing, but in trying to reconstruct the situation it has seemed that many of the problems I have trusting people in general and, in particular, waiting endlessly for men to show they really care for me, stem from this experience. Especially since you have worked so extensively in the area of childhood separation, Dr. Lachan and I thought you might remember details which would help to understand what I really experienced. Mummy has often joked that I was your first case observation of separation, I have I never known if this is true!

The second issue which I am grappling with at present is an understanding of what kind of person Daddy was, and, of course, in particular, what our relationship was all about. I remember the dichotomy he drew between Jos and me, that I was the intellectual one and Jos the emotional one, and many of the experiences I had with him confirmed this view. One of the things that bothers me is why he did this.

So far as I understand it, at heart is the question of what he felt about me and why, and at the same time what I felt about him. Particularly troublesome is the specific question of why Daddy treated me so differently, and the doubts this created about whether he really loved me. However, being in the midst of making on these questions I may not see the issues as clearly as Dr. Sachar or what help you might be able to provide, and probably he can explain the problem better.

I have taken the liberty of ~~to~~ giving him your home phone number ~~as~~ and address, in case you are not in the Tarry. He has said that he will try to contact you and see if there is a mutually convenient time for you to talk

I was somewhat hesitant to ask all this of you in case it might be painful ~~to~~ for you to discuss Daddy with a stranger, and in any case you have already done so much for us all in providing emotional support. So if you prefer not to discuss those issues or are too busy, I shall perfectly understand. I also felt you would understand the request and that you had a unique

perspective, particularly on the early period, that could be very helpful.

I hope all continues well with the magna opera and that the family is thriving. Please give my love to Ursula -

As always, very fondly,

Lissy.

P.S. I haven't told Mummy any of this and would prefer you didn't mention it. She has always been very "uptight", as they say over here, about my being in analysis.

SUNA,
BRAYE ROAD,
ALDERNEY,
CHANNEL ISLANDS

Alderney 2424

21st July, 1972.

Dear John

Thank you for your note - you probably have more than enough introductions for your Australian/New Zealand trip, but should you care to have some non medical ones do let me know. It would give us great pleasure to link you up with people we have known and loved for many, many years.

Alex and I will drink to you from here on Thursday - he keeps me well furnished with the nicest "medicines"!

Please tell your wife that in February/March this year the temperature swung from 100°/. → 60°/.. Clothes needed for all contingencies. Long "tenace frocks" very much in vogue for evening parties.

Ch always

Jeanne

ZIMAN

UNIVERSITY OF BRISTOL

H. H. WILLS PHYSICS LABORATORY
ROYAL FORT
TYNDALL AVENUE
BRISTOL
BS8 1TL

Professor J. M. Ziman, F.R.S.
Telephone Bristol 24161 Ext.441

13th October 1970

Dr. J Bowlby,
The Tavistock Institute of Human Relations,
Tavistock Centre,
Belsize Lane,
London N. W. 3.

Dear Dr. Bowlby,

How very kind of you to write about PUBLIC KNOWLEDGE. I feared that I had been a little too unkind to psychoanalysis there to ever have friendly conversation with one of your profession again; or am I providing yet another weapon in the battles between the Schools!

The point you make about practical application is most valuable and proper, and perhaps I did not sufficiently emphasize it in the work. My own field being rather academic, I tend to find the flow of persuasive arguments the other way - really sound academic theory begins to have an effect amongst the myths of the practitioners who only know empirically what actually works. Have you read Medvedev on Lysenko? It is quite clear that even complete practical failure was not enough to convince the political authorities that he was a charlatan. In other words, I would regard the successful practical application of a piece of scientific theory as just another case of the general rhetoric of "prediction" and "verification".

May I say, to return your kind compliments, that as the father of four adopted children, I have found your general ideas about their upbringing - now part of our whole climate of thoughts on such matters - extremely valuable and sympathetic.

Yours sincerely,

J M Ziman

JB/DES 7th October 1970

Professor John Ziman, F.R.S.,
Department of Physics,
The University,
BRISTOL.

Dear Professor Ziman:

I have enjoyed reading your PUBLIC KNOWLEDGE so much that I am recommending it to all my friends and felt I must write to thank you for it. Your article in the Listener I had read and often quoted, but unfortunately had mislaid the reference.

I wonder if you would agree that in discussing the safeguards against the exponents of a science becoming a self-perpetuating and narrow clique (e.g. p.146) you may be underestimating the role of practical application. Although of course a great deal of scientific work is done without any application in view and much of the best may wait long before anyone sees how to apply it, nonetheless the fact that when applied, it achieves or fails to achieve what the scientists predict seems to me of importance. For example, the prestige and authority of a Pasteur or a Kelvin were based, I imagine, at least partly on the effective application of their ideas, though that would obviously have been insufficient without the distinction of their pure science. Lysenko, by contrast, came to grief in part through the failure of his ideas in practice. Although applicability may play little part in the short-run in determining the status of a group of scientists, as a long-term criterion it seems likely to be of some relevance.

As a developmental psychologist and psychoanalyst I find myself agreeing with your remarks about our field. We suffer from the influence of two main orthodoxies, both of them prematurely jelled, psychoanalysis and learning theory, each with its own sub-schools. Freud, I'm afraid, was dreadfully impatient, a double personality made up of sober scientist and Old Testament prophet. But I believe that, with the advent of ethology, things may be changing.

(over)

I shall look forward to your longer treatise and meanwhile thank you again for its forerunner.

Yours sincerely,

John Bowlby, M.D.

Donald F. KLEIN

for
psychiatric treatment,
training
and
research

HILLSIDE HOSPITAL / 75-59 263rd Street, Glen Oaks, New York / Fieldstone 3-7800

September 9, 1970

John Bowlby, M.D.
School of Family Psychiatry
Tavistock Centre
Belsize Lane
London, NW3, England

Dear Dr. Bowlby:

Thank you for your very kind letter of August 12th. We are continuing with our studies of school phobia and our analyses of the data. We will forward these to you as soon as they are in readable shape since we would be very pleased if you could use them in the preparation of your new book.

I certainly agree that there are probably several different sorts of activating processes.

Sincerely,

Donald F Klein

Donald F. Klein, M.D.
Director of Research

DFK/th

JB/DES

12th August 1970

Dr. Donald F. Klein,
Hillside Hospital,
75, 263rd Street,
Glen Oaks,
NEW YORK.

Dear Dr. Klein:

Thank you very much for your letter of 28th July and the enclosed reprints. I am naturally very glad that you are finding my ideas useful.

I shall read your papers on anxiety states and school phobia with much interest since I am planning to deal with these topics in the final chapters of my second volume SEPARATION, which I hope to have complete by next summer.

Yes, I agree with you that there are likely to be conditions that bring to the ready a number of different behavioural systems. But I see no reason to suppose that their existence would make redundant the theory of disinhibition to account for at least some displacement activities. Evidence from animal studies suggests that several different sorts of activating processes may be at work and that evidence for the existence of one set is not necessarily evidence against the existence of other sets. In all these matters I find Hinde an invaluable guide (incidentally the second edition of his book is just out).

With best wishes for your further work.

Yours sincerely,

John Bowlby, M.D.

Donald KLEIN
1970

for
psychiatric treatment,
training
and
research

HILLSIDE HOSPITAL / 75-59 263rd Street, Glen Oaks, New York / Fieldstone 3-7800

July 28, 1970

Dr. John Bowlby
Tavistock Institute of Human Relations
London, England

Dear Dr. Bowlby:

Your remarkable book struck a very responsive chord in me since it deals with two areas of interest in my own work, namely, separation anxiety and control systems theory. I am taking the liberty of sending you several papers of mine, in these areas, that I hope will be of some interest to you.

The first paper, "Psychiatric Reaction Patterns to Imipramine," includes my initial observations on the usefulness of antidepressants in blocking anxiety attacks in phobic patients with manifest separation anxiety. The second paper, "Delineation of Two Drug Responsive Anxiety Syndromes" contains an early description of a double-blind test of the effects of drugs on what I now recognized as a specific syndrome. I also at that time recognized that separation anxiety was not a secondary learned phenomena and related my formulations to your work.

In "Importance of Psychiatric Diagnosis in Prediction of Clinical Drug Effects" and "Psychiatric Diagnosis and a Typology of Clinical Drug Effects" I continued describing the effects of anti-depressants on adult separation anxious phobics.

The fact that approximately half of such adult patients showed evidences of childhood separation anxiety led me to study the effects of anti-depressants in school phobics. The paper, "Imipramine Treatment of School Phobia" describes our early positive results in this area and the paper by Gittelman-Klein and Klein describes our latest work, demonstrating in double-blind placebo controlled fashion that imipramine is a powerful agent in the treatment of school phobia.

I have also tried to conceptualize the effects of the major psychopharmacological agents in terms of effects upon self-regulating cybernetic mechanisms, as opposed to the usual conceptualization of drug effects as being simple rheostat effects. For your interest I enclose three papers in this area, "Behavioral Effects of Imipramine and Phenothiazines: Implications for a Psychiatric Pathogenetic Theory and Theory of Drug Action", and "Diagnosis and Pattern Reaction to

Drug Treatment", and "Psychotropic Drugs and the Regulation of Behavioral Activation in Psychiatric Illness".

I have one small quibble with your otherwise unexceptionable book. You utilize the term activation in almost a completely specific sense, that is, specific behavioral systems are activated by specific mechanisms. The idea that there may be general activating systems that activate, in the sense of "making it more likely to operate", entire groups of behavioral systems is not dealt with by you - except on page 233 where you give an example of testosterone as eliciting both attachment and sexual behavior. However, even here the multiple effects are parallel rather than centrally organized.

I believe that there is considerable evidence for general activating systems and that the value of such considerations to your theory would be quite useful. In particular, in the section that deals with the economic principle in psychic functioning you, quite properly I think, discard the notion of psychic energy and the associated economic principle. However, you then have to deal with the phenomenon of displacement activity, previously explained by the displacement of "psychic energy". In your discussion on page 100 to 102, you advance an alternative hypothesis ("disinhibition"), that is displacement preening may occur when two prepotent behaviors, i.e. flight and incubation are mutually inhibiting, thus leading to a disinhibition of preening. However, this implies that all behavioral systems have a constant tendency towards expression and that in the absence of the prepotent behavioral systems, the previously inhibited behavioral systems will express themselves. This seems somewhat artificial. The gull who is resting and is neither in flight or incubating, is not necessarily preening. The displacement activity seems to me to be the result of a generalized activation of the gull which forces him to be doing something. If indeed prepotent responses are inhibited, then less potent responses (preening) are activated. However, there is no necessity to assume that all less potent behavioral systems are continually striving for expression, rather under conditions of lack of generalized activation the gulls can truly be at rest, without the postulated inhibitory processes being predominant throughout.

One reason that I raise this point is that the idea of generalized activation processes are important in my understanding of drug effects. Such processes do not demand an economic model or a psychic energy model. However, they do allow for a parsimonious explanation of displacement behavior.

I hope that I have not burdened you unduly with this discussion and wish to repeat my thanks for your extremely valuable work.

Sincerely,

Donald F. Klein

DFK:sw
enc.

Donald F. Klein, M.D.
Director of Research

Lindfield
10 Shalford Rd
Guildford
Surrey

4 Jan 1967

Dear Dr Bowlby

I was thrilled to get your very kind letter which is an honour in itself. Thank you very much indeed & for all you have done so steadily to help the Dept over all my years in it. Do you remember an evening many years ago when the war was still on, & kindly coming to help me as I wanted advice on what research we could most usefully do? I consulted Susan Isaacs & she said she believed you would come & you did & she also came herself & Gwen Chesters did - I forget now who else, but I

I think there was another - I was never
able to do the actual work we
hoped for as I found the Ministry
of Ed (though it had urged us to
re-open) was not prepared to
~~[illegible]~~ release any students to do
the course & , no doubt feeling
a bit sheepish, took a long
time to admit this - kept
refusing everyone on individual
grounds - too old - too young
not intending to take up Training
College work - or Nursery School
work etc etc till I got an applicant
aged 35 who said her motives
were simply to help them
implement the Butler Act in
any capacity in which they
wished for her services! That
compelled them to "come clean"
& tell me they wouldn't be
able to release any one & I had

just to run evening lecture courses
'till the war was over - that is
for the English students. The only
full-time people were refugees
from Poland etc who hoped to
do welfare work with children
but were not "research-
minded" enough to be used on
our war-time projects. I felt
I'd wasted your time, but was
very conscious of the comfort
of your support when entering,
for me, the unknown world of
of the University Teacher & very
conscious of how easily I
might let down a Dept. which
Sydney had started so well.
Your support however has
continued ever since - you
have helped us again & again
& I want to thank you for your

share in the recognition of the Dept which this means. It is also of course a personal pleasure to me & not least that it was ~~already~~ clearly given enormously pleasure to so many of my ex-students.

Yes, it is thrilling about Anna Freud. The P.A. Soc. will rejoice, I know. Again my memory was awakened. I was with Susan on New Years Day 1948 when her C.B.E was announced. I used to see her on Thursdays when she was ill as it was her housekeeper's day off. Melanie Klein rang up & I told her Susan was asleep & she wouldn't have her wakened but asked if I could tell her whether the Honour had come through Susan's work for psycho-analysis.

I had to break it gently to her that I did happen to know that the initiative had come from the nursery school world & she said "Oh, dear! Well at least the fact that she is a psycho analyst hasn't prevented her from getting it. We shall have to make do with that!" Things have moved a long way since then haven't they?

With very best wishes for a very happy new year

Yours very sincerely

Dorothy Gardner

LORD'S WOOD,
MARLOW,
BUCKS.
TEL. MARLOW 4169.

11th June '67

Dear Dr. Bowlby,

Many thanks for your very kind letter. Yes, James's death has been horribly crushing. He had a preliminary attack, you know, but they said he would get over it; & then he had another, & died. But it was instantaneous I, they promise me, without pain.

My brother, Professor Sargant

Florence, has come back from America, where he was lecturing, & 'commutes' between here & Birmingham, where he belongs, so that I feel pretty well supported. — It is a great comfort to me, too, that James's got the work done which was so near to his heart.

I personally have had fifty years of happiness with him & that is a good deal more than most people can expect.

Yours sincerely,
Alix Strachey

21 ALBION MEWS
LONDON. W.2
TEL. AMBASSADOR 3647

5.12.66.

Dear John,

Following the farewell meeting at the Tavi last week I feel drawn to let you know more personally what, in the emotion of the moment, I probably failed to convey at the time. This is that I feel my whole potentiality for being able to do what I am now undertaking is due in very large part to all the various experiences over the last twelve years which only you have made possible for me. For this I cannot really express my gratitude to you adequately, but I want you to know that I feel it very strongly indeed.

In particular your example as a research thinker, writer and administrator has been of unique value for me. Furthermore I feel the background of benevolent support, constructive criticism, freedom for initiative and tolerance of differences which you have so consistently provided has given me an environment for growth that few researchers

could have enjoyed as I have in such full measure.

I shall always be indebted to you and if I can try to repay you even partially in any way I shall be only too glad to do so.

With kindest regards.

Yours ever,

Tony

12 Pembroke Road
London W.8.

20th May, 1966.

My dear John.

Thank you for your letter which has moved me very much.

As you know the appointment means a great deal to me, but not only because of the security it gives. I am now able to go on working on the subject I most want to pursue with a colleague who is also a friend. and this means a great deal to me. The clarity and cogency of your thought has helped me

many times to bring order into a confusion of ideas. You are kind to mention my gifts and swift ascent, but I know how much of this is due to you

Again thank you,

Dorothy

Radnitzky

108, Grantchester Meadows
Cambridge
Tel. 53376

Oct 31st 65

Dear John,

Thanks for sending me the notice about the Tavistock seminars. I want to take this opportunity of telling you how much I feel that I owe to these seminars — a large part of the work I do now stems directly from the meetings in that room in 10+ years ago. My work & life owes a the other building,

lot of its inspiration to you.

I have been feeling that things have been very much on top of me lately, & this Autumn, with several new students starting, my assistant away for the year, & my book proofs to correct, I feel I must cut down my commitments to a minimum. I wonder if you often feel that you can't quite keep your nose above the surface? So I feel

108, Grantchester Meadows
Cambridge
Tel. 53376

I ought to say - please don't bother
about sending me pre-meeting
abstracts. But if there are
to be any meetings which would
be specially in my line of
country, I hope you will let
me know & I will do my
best to come. I always enjoy them.
With very best wishes
yours ever
Robert

UNIVERSITY OF BRISTOL

DEPARTMENT OF SOCIAL WORK AND ADMINISTRATION
40 BERKELEY SQUARE,
BRISTOL 8.
TEL. 24161

1st October 1965

Dear Dr. Bowlby,

Now that I'm back, and beginning to take stock of last year, I realise I should like to make sure you know how much I appreciated the seminars and other times of meeting you during last year. It was a very good year anyway; but looking back I can see that much of the basis of 'goodness' must stem from your own contribution in various overt and covert ways.

Yours sincerely,

Barbara Butler

Miss B.M.I. Butler
Lecturer in Social Work and
Administration

Dr. J. Bowlby,
Tavistock Clinic,
2 Beaumont Street,
London W1.

PADDINGTON GROUP HOSPITAL MANAGEMENT COMMITTEE

THE TAVISTOCK CLINIC
2, BEAUMONT STREET,
LONDON, W.1.

TELEPHONE:
WELBECK 5415

18th August 1965.

Dear John,

I have at last heard from York to the effect that they would like to appoint Kathleen Jones as Professor of Social Admin. & Training, & me as Reader in Social Work. (at £2,750 p.a.) I think on the whole this would suit me better than the Chair, since I was a bit daunted by the thought of all that admin. & all the University ropes I didn't know.

So the bird is in the hand, and although I am asking for a few more details before accepting, I think my decision is really taken. I have found it difficult to talk to you about this (owing to guilt, no doubt) but I do want

to tell you how much I have enjoyed working at Tavi, with you, all those years, & how much I have gained from it. I confess I have several times hankered after a more academic job, with more time for teaching, reading & writing, & I think I have grown a bit stale after 14 years. Oddly enough, I find it easier to leave now the tide is flowing in the directions I have favoured - more flexibility & variety of therapeutic method, more adequate work with the family etc. But I wouldn't have left for anything less tempting than this thing at York. I feel I now want to work ~~out~~ on some things for which a clinical setting is not really suitable, such as the influence of sub-cultural patterns on child-rearing & personality development, the variety of social norms & the implications of Bernstein's theories of language use for communication in casework with different social groups. The Sociology Dept. at York

seems to be very well-disposed to this approach.

I do very much hope that it will be possible to raise funds for a Director of Social Work Training here, & to find a good person for the job. Would you kindly favour of repeating the whole Scottish application, with additional material about the projected course in Supervision, or would you favour diverting the D.S.W.T. application from ~~E.D.B. Training~~, grants to students, etc? Or do I just ask Eileen Younghusband (when I can get hold of her)?

Please believe I feel quite heavy-hearted about leaving, but at the same time I feel it is the right thing for me.

Yours sincerely,

Betty

24 Lansdowne Rd,
W.11.

16-6-64.

Dear John,

Your letter was quite one of the nicest I have had. Thank you so very much for it, and for writing so generously.

I feel happy to be the first one to speak, or write the Powers that Be have bestowed an award for Social Work. This is a step forward in the recognition

that it is beginning to win.

In fact you yourself have contributed more to it than any other single person, with the exception of Trend.

I am so very sorry not to be able to be at the Tavistock tomorrow as I have been for a long while committed to going to LSE to take part in a presentation to two retiring ex-colleagues.

Yours ever,
Eileen

MAX-PLANCK-INSTITUT FÜR
VERHALTENSPHYSIOLOGIE

SEEWIESEN UND ERLING-ANDECHS

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Prof. Dr. J. Bowlby,
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Dear John,

thank you very much for your congratulations to my election. What you say in your letter is quite exactly what I tried to express in my letter of thanks to the Foreign Secretary of the Royal Society. What fills me with the greatest joy is, that this election implies not only the recognition of myself as a scientist, who may not quite deserve that high honour, but more particularly of the young science which I represent and which certainly does. As you say it can be a very real help to ethology if Bill Thorpe, Niko and I, who after all are some of its oldest representatives, are members of the Royal Society.

Gretl and I wish we could have been present at Mary's wedding, we are sorry we weren't. But some day we must see your house in Skye. The only trouble is that I am really overburdened with work at present.

Love from Gretl and me to both of you and your children,

ever Yours

Konrad

PRUGH & HARLOW
Summer '59.

Dr. Bowlby 12th February, 1960.

"Masked Deprivation in Infants and Young Children"
by Prugh and Howard

I regret that this paper has only just reached the top of my pile and had some attention.

I agree with you that it is a very good paper and valuable not only for the scholarly and somewhat detached way in which it reviews the subject, but also for restoring the balance somewhat by dwelling on the possibilities of deprivation occurring within the family.

I have one or two minor points to make which stem perhaps from my longstanding feeling that there is a vein of scepticism in Prugh regarding the separation hypothesis, and that this leads him perhaps to devise formulations which while largely correct tend to have a bias in the other direction.

Page 4, last two sentences I think there is here an ambiguous formulation of the kind I have in mind. After conveying reassurance by saying that "in most instances these reactions to brief separation in a medical situation appear to be short-lived", he goes on to say in the final sentence that "children under 4 .... may suffer more lasting interference with their emotional development". Since Prugh's [redacted] published figures relating to brief separations show 60% of the under fours still "significantly disturbed" three months after separation , and 15% of them six months later, it cannot be said that these effects were of short-duration. I think we might have formulated these ideas the other way round to give due emphasis to the age group in which separation is a major problem - perhaps thus "children under 4 may suffer lasting interference in their emotional development, but with older children reactions appear to be short-lived". In other words, I think he does not give proper perspective to the matter by making it his main generalization that "in most instances these reactions.... appear to be short-lived".

Page 8, para.1. Here Prugh joins the procession of people who misunderstand the significance of the S.F.U. publication, by regarding it as a modification of our views on the severity of the outcome of prolonged separation. The fault, as we know, probably lies more with the publication than with Prugh.

I think our real attitude (mine anyway) is that the

S.F.U. findings were got on the basis of a superficial study and that a more adequate (current) type of study might well show more disturbance - even if not of the delinquent or affectionless type - than did the S.F.U. Prugh's presentation does not convey this reservation.

Page 8, Para.2. I believe we also doubt whether all the "immune"children were as immune as reported in the S.F.U. paper. The touchstone for this hunch is provided by the fact that none of my seven C.S.I. children have ascaped unscathed.

Page 11. I have the impression that Prugh tends to overstate the possibilities of disturbance occurring within the family. In such a scholarly and grammatical paper it is surprising to find in line 6 the obscure phrase "may occur frequently" in relation to the instamce of deprivation in intact families. The qualifying word "may" seems to make nonsense of whatever is being said. As far as I know, there is no way of comparing the incidence of depression within and without the family or the degrees of severity to be attached to these two kinds of deprivation. Here again I think Prugh's formulation is loaded by scepticism about the separation hypothesis.(I should have said above that in his best-known paper I seem to recall that he actually says that separation per se is of less significance than the ~~ways~~ phase - i.e. anal, oral, etc. - in which it occurs.)

Page 30,line 8. In the sentence beginning "Significant trauma", Prugh seems to make a typical confusion between "trauma" and "deprivation".

Page 30,para.3. There appears to be some confusion and inadequacy in this paragraph. I doubt, for instance, whether we would be confident that Maenschen had the evidence to support her statement, and might suspect that she is more devoted to the assumptions of Anna Freud than to available facts.

Furthermore, without having read the Maenschen paper, it seems clear that she is referring to very young children, whereas Prugh, who uses her statement to support his thesis in advocating "proper institutional placement", speaks of "adequate preparation of the child", and is therefore by implication referring to much older children. Ahen Maenchen.

The paper has a pin-prick for me, namely that in his lengthy review of publications which have drawn attention to the phenomena of separation, and in a definitive list of references, he omits "A Two-Year-Old Goes to Hospital". Probably I cannot blame Prugh for this since a number of

other authors do not yet seem to have realised that there are publications other than those written which should properly be given as references.

James Robertson

27 HAMILTON TERRACE
ST. JOHN'S WOOD
N.W.8.
Cunningham 4326

Dear John,

With this letter I am enclosing some letters of Yvonne Blakes and a copy of a letter I have just sent to Hattie Harris and Dina. These letters are private and personal and for your reading only. When you have read them I would like them back at your convenience.

Just before Christmas Dina told me that she believed infant of Mrs Yvonne Harris to be Hers. and that she did not believe Mr Harris was honest. I asked if anyone had got in touch with Yvonne or asked for an explanation before arriving at this conclusion and was told that Hattie was going to but had not yet done so.

I at once got in touch with Yvonne and received the enclosed letter card in reply. Yvonne also wrote to Hattie at this time either giving

or appearing to give the address of the parents of infant G. Hattie seemed to be satisfied about the photographs of Meg and to believe Yvonne in this but was not satisfied that infant G was not Meg. She came round to see me and pointed out among other a number of things that by the dating of her work Yvonne could not have done it.

Up to this point I had not been so much impressed by Hatties arguments as by her vehemence, but on quickly re-reading Yvonnes thesis I did not see how dating the work from her sons birth she could have done the thesis as systematically as she claimed. I was also misled into believing that the details of her subsequent analysis of her records must have been present in an equally detailed form in her mind when she made the observations.

I wrote to Yvonne to this effect. Hattie, to whom I read my letter before sending it, agreed not to send the letter she was then writing to Yvonne, and this is why the reply has come to me. I received the other letter which I enclose.

I then wrote back to Yvonne asking her permission to show these letters to you at least, not only for her sake but also for yours since it was not just that you should be allowed to feel that you had been let down as badly as you must have felt you had been.

Yvonne replied that she did not wish to justify herself further to Hattie or Dina, but that there was nothing secret in what she had to say other than the preservation of the identities of the people concerned. She did not want them involved in anyway and did not herself wish to become involved in further trouble since she had had great trouble with them in the first place. She did however say that I might show you these letters privately.

From my letter to Hattie you will see that I feel sure that Yvonne has in fact presented an honest thesis. I am quite sure Dina and Hattie were right to express their doubts to you and to say what they believed but I do not think it right that Yvonne should be condemned without a hearing. But even with a hearing and even though Yvonne be innocent I myself feel that it is tragic that these accusations must be damaging.

Yours sincerely

John A. Brennan

from J. Brenda April 59

Dear Mattie,

I heard from Yvonne a little while ago.

In her reply she asked me not to show her letter to other people because it could lead to the disclosure of the identities of the children and parents concerned and because she had had considerable trouble with the parents in getting permission to publish her material.

Both these facts are true and can be proved. She was allowed to use her material only provided it was disguised. In order that identities could not be traced table 3 setting out the parents background is not a true statement and, because she was using her own sons and therefore people knowing her and her friends would be able to place children in relation to them, table 4 is not true either, and any apparent evidence which might lead people to try to find out where or when the work was done or who the people are is deliberately misleading. In fact the major portion of the work was done in South Africa. The hospital also is disguised and is in South Africa.

After I had received her letter I wrote back seeking permission to show her letter and to check on other points which you had raised. I have now heard again.

She says that during the last two years when she was feeling very cut off and in despair neither you nor Dina answered any letter of hers and that she only received letters of encouragement from me. She does not particularly care what you or Dina may think or have any wish to justify herself to you. Knowing Yvonne as I do it does not seem strange to me that under attack she is angry, particularly if in fact you have not

answered her letter. However she continues that she does care what I think and would explain herself to me, and that she would give me permission to show her letter to Bowlby privately.

The extent to which you have kept up with Yvonne since she left I don't know, but it is true that up till the time I married I wrote about once a week and since then about once a month, and gave her all the encouragement I could to struggle with her thesis, particularly during the last years which were very grim. She had an enormous amount of trouble in getting her mothers to agree to her use of the material and her relations with the friends on whom she reports have been strained ever since. Before ever this trouble arose her letters about this were full of despair, and both Diana and I know how depressed she was by it all. That she should find herself involved with further trouble over here she seems to find about the last straw and she just doens't care what anyone thinks anymore.

I am very sorry that I can't show you her letter and even more sorry that Yvonne was not over here herself when you would have met and talked as a friend. Had she been I think this situation would have been different. However what I will now do is to set out what I may of what Yvonne has said in her letter and her thesis, and take this into consideration with what you have said and what I know, as well as I can.

In a case of this sort it is very difficult to prove things either way by the very nature of the material and the conditions under which it must be assembled and published. For this reason I think that whatever the apparent evidence one should be extremely cautious in forming a judgement. Innocent or guilty anyone so accused would find it very difficult to defend themselves. It would be more difficult, indeed almost impossible when a distance of continents lies in the way and when one side prejudges that any evidence coming from the other will be dishonest.

The evidence against Yvonne comes under three headings:
Circumstantial evidence
Inherent discrepancies in the work
Opinion.
Under the first heading we have the various coincidences which lead you to believe that Infant G is Meg.

When you came to see me you will remember that I asked for evidence, that I was not convinced that these coincidences were not in fact capable of explanation.

In this day and age it does not seem unreasonable that Yvonne should have numbered among her friends more than one where the mother was working and where the background was much the same as yours. Nor does it seem completely unlikely that the children should be girls as I believe the chances are about 50/50. The point that you should have kept daily notes on your child and then given up does not strike me as strange nor that Yvonne should have asked her friends to keep notes and that they too gave up, incidentally not only the parents of infant G.

We know that Meg's behaviour was not unique in that Anna Freud had already noted the type of behaviour where G could only be comforted by being held looking away from the person who was not her mother. Since Oswald adopted this as a conscious technique with Edwin, it must have been significant behaviour to Yvonne.

Now the types of behaviour open to children who are small are limited, and of the available types of behaviour, that which will be noticed and felt to be significant by any single observer is likely to be even more limited. Granted therefore that Meg's behaviour was not unique and that it was significant behaviour for Yvonne it is very likely indeed that she would notice similar behaviour in another child.

If there are similarities in the background of Infant G and Meg it is indeed likely that there should be some similarity of response. This would increase the over all picture of likeness and Yvonne's method of disguise has certainly added even to that.

It is certainly not possible to prove that Meg is G, and taking all this into consideration I would think there is cause for some doubt as to Meg being G.

However the evidence does not rest there alone. In my second letter to Yvonne I asked whether there were any possibility that any notes Oswald might have taken could have got muddled with hers. Had she used Meg such an admission could have been a credible excuse. She says there is no possibility whatever. She tells me who the parents were and I believe that I did meet them while they were over here. I believe she has given you their name and address and know that she offered to. The fact that they are South African may be inconvenient but it would hardly be unlikely. This is evidence which under no circumstances can just be dismissed. The possibility that these witnesses could be suborned is extremely unlikely, moreover there are people at the Tavistock who know people in South Africa and it is not impossible to check certain facts such as whether these people have children of the right sort and age. To get people to lie would not I think be easy, to get people with the right background would be incredibly difficult.

When you came to see me I think it would be fair to say you were convinced that Yvonne had been dishonestly using your daughter and that Bowlby had been misled. You had acted upon that conviction. Now conviction is a summing up and a closure to the weighing of evidence and the capacity to entertain doubt. As an attitude it is necessary to action but not necessarily helpful in unravelling truth. Once convinced it is very much harder to admit the possibility of any other view. This is a fact which I think should also be considered.

Yvonne has always been pretty independent and it is in character that she should have wished to be in control of her own work. She admired Mrs Bick and she still does admire and respect her, but she often disagreed with her and felt furious. It can hardly be suggested that Yvonne and Mrs Bick were particularly compatible personalities. It would therefore have surprised me very much if she had discussed it with Mrs Bick or with anyone who might bring it into discussion where she might become involved.

There was an occasion where I know she was in a flap because Mrs Bick had suggested some joint paper on baby watching and when I know she was very worried because she felt this would mean her using some of the material she had collected, and this would have made it invalid for her thesis since a thesis must be original work. She told me this at the time and of her relief when neither you nor Dina were very keen. She felt a bit mean about keeping her material to herself at this time and still does and mentions this in one of her letters to me.

It does not particularly surprise me that she did not go into it with you or Dina, for in addition to the reasons she already had for not doing so you were her sisters on the course and you yourself her sister in analysis, and it can hardly be suggested that there was no rivalry between you, which I think would have increased her reluctance to talk about her work till she had something to show.

In considering the question of discussing her material, however, I would think it impossible, and I think you would also consider it incredible, if her work had never come up in her analysis, expecially when one of the themes stressed in the thesis is Mrs Sandford's view of the importance of the mother together with the child.

Now I know that Yvonne sent a copy of her thesis to Mrs Sandford. It seems unbelievable that she should do so, or that anyone should do so, if the thesis were dishonest, particularly as Mrs Sandford had also had you in analysis.

Summing up the circumstantial evidence I think it can therefore be shown that there is not only a reasonable doubt as to Meg being G but that Yvonne has offered proof which can be examined that she isn't.

The inherent discrepancies together with the fact that I had not read the thesis properly were the factors which led me to believe that the work was not honest. If Yvonne claimed by dating her observations from her sons birth that most of her work was done over here I just did not believe her. In fact she deliberately altered the dates of her observations so that anyone knowing her, her sons, and her friends and their children, could not say with certainty which was which.

It was, I expect, the combination of coincidence and discrepancy which led Dina first to doubt this work. It led me to believe that if she were claiming to have done the major part of the work over here, it was as likely she was being dishonest over Meg as well.

At this point we may consider the work that she did do over here. I myself saw, for instance, a pile of notes taking up about as much room as would be occupied by a bundle of about 20 copies of the Journal of Psycho Analysis -- (I have just made a pile to see). I glanced at them and they were to do with observations on babies and Yvonne told me she intended to use these for her thesis. She had never told me a lot about it but she had even in the early days before she started the course mentioned her thesis to me, and she had mentioned from time to time that she was collecting material for it.

There seems to me to be two very good reasons why she did not go into it more deeply with me. To start with a good deal was confidential, and anyway I didn't follow the subject up, and in the second place if you are doing something of the sort you may not want to go into it with your friends until you have got a pretty long way with it. "You are not to look until I have finished" is not only a remark and attitude of childhood. Moreover she was only collecting material, she had not decided how she would use it at the time she was over here, so there was not a lot to discuss.

When you came to see me I did not quite follow the point you were making when you said that Yvonne never had much material for baby seminars. Perhaps you meant that Yvonne could not have been doing much observing of babies because she was so short of material. The implication would be that if she had had the material she were collecting for her thesis she would have used it. That does not follow, and nor does the obverse that if she didn't use the material she didn't have it. In fact we know she had a very considerable amount of material before she left this country. This is fact not conjecture.

There now comes the question of whether this could have been collected systematically.

Yvonne says she did not observe any babies from the time she was expecting Edwin until the time she left this country, other than Edwin. Therefore my feeling concerning the impossibility of her observing babies while pregnant, while at Hornsey and while doing the course is irrelevant.

In fact the infants at the nursery were observed before she started her course. One other infant was observed at this time, and I think at this time would have been discussed with Mrs Bick but for the fact that the mother said Yvonne might observe but that she was not going to be discussed by anyone. Only two more infants were observed over here, and they were observed before Edwin's conception.

Now Yvonne knew she had to do a thesis before she came over here, for she knew she had to get an M.A. before she could practice in South Africa. She was interested in babies and it seems natural that she should have considered at a very early stage using the material she was getting at the day nursery. Since she wanted it for a thesis it is, I think, reasonable to suppose she would have collected it as thoroughly as she could. That she should have also made up a questionnaire to supplement her material is certainly quite a likely thing for her to have done, as a psychologist.

The point was raised that it was unbelievable that these mothers would have kept the notes or answered it. I think a more careful reading of Yvonne's thesis will show that this point can not be brought ~~up~~ as evidence against her, because in her thesis she says or implies that only one answered it. There is no reason to suppose that the mothers would not have given the verbal reports she requested or that the staff would not have cooperated similarly.

I think one of the difficulties in accepting the possibility that Yvonne did this work is that one becomes hypnotised by the thesis itself, by the detailed analysis and by the intricacy and complexity of its graphs and tables, and that this leads one to imagine that she could not have done it unless she had all this in her mind while she was over here, and therefore to assume she is claiming that she had.

Yvonne's claims are not so spectacular. They are mainly that she kept a chronological account of the infants' and mothers' behaviour. This she took back to South Africa and discussed with her supervisor what use she should make of it and how she should proceed., The graphs, which were not her idea but her supervisors, were worked out from the chronological material taken in her periods of observation. While she was in this country she was not working out her thesis, she was collecting some of the material for it. I think it can truly be said that it is possible that she could have done what she claimed she did.

When we come to examine the thesis itself I feel it is evidence of a very considerable amount of its own truth. The observations are brilliant and I do not think it in any ~~way~~ way credible that Yvonne could have made it up. It is possible to hold that this is a valuable piece of work without agreeing with all the conclusions, and of course the use to which Yvonne's work is put is no proof of lack of integrity in the work itself.

Yvonne's letters are also evidence. She has answered promptly and without any involved tale or excuse. They are in no way whatsoever frightened or defensive and I must say that the letters I have received seem to be entirely truthful.

So much then for the internal evidence. We come now to the final heading Opinion.

Now opinion can not prove fact. In such a matter as this opinion may support one in ones belief that one is right, if one needs such support. I would agree with you that it is bound to influence ones view, but it cannot be proof, and both opinion and hearsay, which is not considered to be evidence in our code of justice, can be most unfairly damaging without proving anything.

Well Mattie there we are. Circumstantial evidence is never entirely satisfactory and when it is possible to put more than one interpretation on it it must increase ones doubts. We have too to take into account that Yvonne has offered to put you in touch with the parents of infant G. As far as the inherent evidence goes, and with it some of the circumstantial evidence, Yvonne's explanation is one of perfectly common practice with regard to professional papers. As to whether she did have a large quantity of material when she left this country I can witness that she had. With regard to whether she could have collected it systematically, we must take into consideration what she actually does claim and this is not so very great. As far as having time to do so I think we must agree it would have been possible. Finally hearsay and opinion can do enormous damage and get one nowhere.

This has been a very difficult letter to write. It is horrible to have to defend one friend to another friend. After all that you have said I cannot believe that you would read this letter without doubts. It would be quite fair for you to take into consideration the fact that I have always been devoted to Yvonne and that we have always been the closest of friends. It is this which gives me the right to write this letter. But I have tried to see the truth and to set irrelevant

feelings on one side, and to weigh up all that you have said, and to check all that Yvonne says both with her thesis and with her letters and my memory of the past. Having done so I must tell you what I believe to be true and that is that you and Dina are mistaken.

304 Rockridge Heights
Berea, Jhb.
5th Jan 1959.
~~[stamped address struck through]~~
~~[stamped address struck through]~~
JOHANNESBURG
SOUTH AFRICA

My dear John,
Thanks so much for your letter which I received today when I returned from my holiday. I'm so glad things are going well with Jock, — he certainly seems to be growing like a toadstool! I'm so glad Diana has been able to breastfeed him. I was very shocked about the story Dina told you. I've not heard from Mattie, & as your letter is dated 23rd Dec. I wonder whether they've sorted the muddle. Meg was certainly not observed by me & I'm sure Mattie of all people should be able to see that. I do not know any facts about Meg:- breastfeeding, birthweight etc but surely to God nothing can be the same as any of the babys I observed.
None of the babies have any connection with the Tary. But I now have a horrible suspicion that I made a dreadful mistake — the 'photos. I did not send Bowlby any snaps but as I think I told you — the 4 copies I had to hand in to the University I included snaps just to illustrate what I meant by my categories of analysis (such as watching etc). (The University sent Bowlby one of these copies; I have sent him 5 other copies subsequently & I did not know he still had this copy.) These snaps were not taken specifically for my thesis in fact most of them were taken by you of Edwin, but I had snaps of several other babies but on the whole no negatives. As I needed 4 copies I could not use them, but of 3 I had "polyphotos" — my supervisor & I both thought one of what I was sure was Inf. G. was good for illustrating "watching". I now remember we did also have a "polyphoto" of Meg — I may have made the mistake & included it here. I've not seen this child (or Mag) for some time & for that matter & may have muddled it. If I did I'm most frightfully upset — especially as Mattie also worked, but that I'm sure is the only similarity. This other woman had no connection with psychology. They're S. Africans & back in this country. I'll write to Mattie & will if she wants it send her this woman's name & address (if I get the woman's permission — which I'm sure I will). But I can't understand it at all —

surely Hattie knows her own child's history & it just cannot be the same in any detail to those other little girls. This child is I think younger than Meg — well in fact they're so different in most ways (though now I can see similarities I'd not thought of.) that it had never occurred to me. In fact in reading your letter I did not know for a moment whether you were referring to Inf. E or G (the two girls of friends) because E too was lively & up in arms at times — but then I realized about the snap & about the working mother.

Anyhow, until I am told these things directly I can't do much about it, though I will write to Hattie.

My boys were thrilled with their Xmas presents that you sent. Thankyou so very much.

PAR AVION — AIR MAIL — LUGPOS

AÉROGRAMME AIR LETTER LUGBRIEF

SOUTH AFRICA SUID-AFRIKA 6d

J. A. Bremner, Esq.,
27 Hamilton Terrace,
London N.W.8
England

SECOND FOLD— TWEEDE VOU

IF ANYTHING IS ENCLOSED, THIS LETTER WILL BE SENT BY ORDINARY MAIL
AS ENIGIETS INGESLUIT WORD, SAL HIERDIE BRIEF PER GEWONE POS GESTUUR WORD

SENDER'S NAME AND ADDRESS
NAAM EN ADRES VAN AFSENDER

Yvonne Blake
304 Rockridge Heights
Berea, Jhb.

FIRST FOLD—EERSTE VOU

We'll all be looking forward to receiving snaps of Jock. I can't get over his ability to wriggle forward. He must be an exceptionally strong child — and to put on over a pound a week! I am sure you must find it just too wonderful to have him. Have you stopped your analysis? Or are you intending to stop? With my work things are going quite well, though after a three week break I have to get into the swing of things again. Fondest regards & lots of love to you all

Yvonne

304 Rockridge Heights
Berea
26th Jan 1958

My dear John,

Thankyou very much for your letter & airletter & above all for the snaps. We think young Jock is a lovely & remarkably forward baby. He really looks a much older child especially in the bath — those are wonderful photo's. Even at a younger age he seems so well integrated — so much of a person. Both Oswald & I really congratulate you on having a very fine son — someone well worth waiting for. I am sure both you & Diana must be feeling so proud & happy.

Edwin started school on the 14th Jan. & he is very pleased with himself. Oswald feels I've put him in a "snobbish" school — they wear the conventional English schoolboy uniform: caps, blazers, long-sleeved white shirts, ties, grey trousers, grey socks & black shoes. The blazers & caps are bottle green, the school colours green & gold. I must agree this uniform is rather ridiculous in our weather & many schools have discarded it, but at the same time both he & I feel so proud of his appearance that he hasn't once complained of the heat. John Robert, poor little soul, is feeling rather envious & unhappy, but is slowly working through this.

The idea of this letter is really to tell you about my thesis. The whole wretched business irritates & tires me, partly because I have no wish to justify myself, & partly because I've had enough trouble with the whole damned thing & had hoped I'd never have to talk about it again. I'd prefer you not to show this letter to the others because I gave my word to the matron of the hospital & to the mother of H, who nearly drove me silly, that I'd not in anyway reveal their identities & put things — such a way that their identities could be traced.

First of all I want to say that as far as I'm concerned I believe my conclusions are valid, & the work was done to the best of my ability. It all started a long time ago — even before going to England I'd wanted to do

an M.A. When I arrived in Sept 1949 Mrs Bick suggested that I go to Hocroft Day Nursery & observe babies. I started there in Oct 1949 & for 4 months spent every day of my life observing babies. Jessica (then Jessica Riggs) helped for a few days. I discussed these observations occasionally with Mrs Bick. Then I hit on the idea of perhaps using them. I did them as well as possible, but as described in my-Thesis these records were merely a chronological account of each child's behaviour. Also when asking me to go to the Day Nursery Mrs Bick suggested I try & observe babies (or a baby) at home. A South African friend was pregnant at the time & agreed to let me observe her child born in Nov. 1949. But then my trouble started — she refused to allow me to discuss my observations. She said she had no objection if I felt I wanted to know more about babies but she was not prepared to be discussed & criticised by outsiders. I respected her wishes but continued to see the child (called E).

During 1950 (before I started the course but while doing the Educ. Psyc.) I continued going to the Day Nursery once a week & I still saw E weekly. During the first year of the course I continued at the Day Nursery weekly ("my babies" were now over a year) & observed another baby there (not included in the thesis). I was not observing any new babies at home. Then through a doctor friend (Leo Bender) whom you met & for whom I sometimes "baby-sat" in 1951 I met the mother of F. The husband was connected with Guys (?) Hospital. She was a relaxed person & the least touchy (or 'paranoid') of all my friends observed. The mother of G was an art student — really a friend of Oswald's. She too I think you met. She is not interested in psychology (or children, I think) & could not care less whether I observed her or not, neither has she ever asked about my thesis. Then I expected & had Edwin. I observed nobody

else during that time, & actually did tell you that we were keeping notes on him. I did not talk about a thesis because frankly I did not believe anything would come of it & I had not at all planned how I'd use or analyse my material.

It was only when we returned (Sept 1953) that my hopes soared: My sister Vida, my sister-in-law Sybil, & my very intimate friend Babs, were all pregnant: also a friend (really an acquaintance) who ran a nursing home became interested & arranged with eight pregnant woman-patients to allow observations. I was naive enough to believe these mothers-to-be would not mind this. Well, Vida & Babs backed out even before their babies were born, Sybil soon afterwards, one mother also before the baby came, & another I had to drop a month later (as mentioned in my thesis) I had 6 now left of a possible 11. Then I became pregnant - John was born some 15 months after we arrived back here. Two months later a doctor friend (mother of H) gave birth to a son & allowed observations. She is a tense conscientious woman, who~~m~~, I think, really hoped for support from my observations. She religiously kept notes of her baby too. I had aimed at 20 babies but found I had neither the time, the courage, nor the money to do more work. John was demanding & difficult, & just observing H was as much as I could do. I had already registered with the University (originally to do an M.A.) & discussion of observations with my supervisor was under way. The analysis of the records was her idea & literally took months, working every blessed night & every spare moment during the day. Then - 1957 when the whole thing was written up, typed & I was checking the stencils the mother of H wanted to see it. She was upset, hostile, & refused permission for me

to use the material on H.[4] I was quite desperate. I could not contemplate rewriting it all & a hell of a lot of work had gone into it & into the observations on H. At last I decided to camouflage the facts. I changed H to fit into the same year as Edwin, thus making him - effectively two years older, & I called her a 'psychologist' instead of a 'medical doctor'. Also I expressly said - two places that P (John) was observed last (pages 39 & 49 of my thesis). In this way anybody knowing us both would be misled & she unidentifiable. She was mollified, & I felt that I had not invalidated any findings by these changes. But she has expressly forbad me ever to publish any material on H.

Table IV should thus read:

Year 1 (1949-1950) - Inf ABCD & E (as stated in the thesis)
(1950-51) - no observations
Year 2 (1951-52) - F & G (as stated in the thesis)
Year 3 (1952-53) - O only
Year 4 (1953-54) - I, J, K, L, M & N (as in thesis)
Year 5 (55-56) - P & H.

Not really so very different from the table given in the thesis is it? I don't know what makes you think Infants I to N were observed in England — I've nowhere given that impression — they are given as observed after O (Edwin) & younger than him, & he was under a year when we came back.

Then the matron of the nurses' home who was most co-operative, felt that her patients might object if the nurses' home was identifiable so I changed it to "a large hospital" but did not describe it's procedure as state inaccurate facts about it.

I know that "dressed up" these observations probably look better & bigger than they in fact were but I have nowhere intentionally been dishonest,

anyhow not to the extent of making my conclusions invalid. I've lost all contact with the Day Nursery mothers, I don't even know whether the matron there would be the same woman. I did not specifically ask their permission to use the observations - a thesis (I merely said I wanted to know more about infantile development, as stated - my thesis) because at the time I was not clear about how I'd use the material, & later on I did not think it was necessary (or perhaps it was just too much trouble to bother about it). All the other mothers know that I used the data - a thesis, but only the mothers of E & H have seen the thesis. And I certainly did not encourage the others, especially after my experience with mother H. As you see I cannot really 'prove' anything without stirring up a hell of a lot of nonsense, & I don't think it's worth while "or as you put it "wise to try to prove it." The University & my supervisor who saw my work & discussed it all at length with me found it thorough enough to be acceptable, & I feel I did my best. For me that is enough & I'm sorry if "all my colleagues" - London are disappointed. Sorry, but I'm just feeling a little catish.

I did not know how strongly mothers felt about their relationships to their babies, & how very foolish it was of me to try & study this problem (probably that's why it wasn't done before: fools rush in where angels fear to tread!). A more touchy & sensitive subject I could not have chosen, & it has tended to leave me exhausted & disillusioned.

This explanation - um - justification is for your sake only. I've not spoken about it openly because it isn't something I feel I should discuss. The mother of H (or any others for that matter) does not even know that I distributed copies of my thesis to friends & the Tavistock. My friendship with her was more or less wrecked, & even E has been cool & most unenthusiastic as a result of the thesis (I

suppose I do - my conclusions to some extent imply criticism of them) & I hope no more relationships are to be strained for something so foolish & really unimportant.

With fondest regards to Diana & much love to you & Jack,

Yvonne

May 7, 1958

Dr. John Bowlby, M.D.

Dear John:

It is a source of very great regret to me that a laisser-faire tennis match with Milton Friedman prevents me being present this evening. I would wish everyone to know, however, how much I have enjoyed our talks together on American economic infancy.

As you say, there is no doubt that the sad difficulties American parents have got into through giving their children too much freedom provide a valuable clue to the analysis of our socio-economic maladjustments.

Humbly yours,

George Stigler, Ph.D.

TELEPHONE, HARTING 261.
TELEGRAMS, ELSTED, HARTING.

THE PLAT,
ELSTED,
NR MIDHURST, SX.

December 24,57

Dear Dr. Bowlby,

Your very delightful and interesting letter gave me great pleasure. Oddly enough it came on the day when I was mentioning your father's name in my Autobiography, and I find it sad to reflect that there are analysts who would regard this as a supreme proof of telepathy. All the same the distance to San Fra does seem to be over-doing it.

My retinal haemorrhage in July cleared up partly, but increasing cataract blurs the vision in an annoying fashion. That, however, has been the least of my troubles. I have had some months of very serious illness all originating ultimately in my cardial degeneration and so have come to the end of my active life. Not being allowed to mount any steps I should have been marooned for good in my bedroom were it not for some kind American friends who subscribed to install a lift a fortnight ago, so now I descend safely and even try to do some work.

These jaunts to the Golden West never came my way in my youth and in the years I spent in America I never once got to the 'Coat'. Your ?Coast vivid account of your activities there interested me greatly, though I must confess to a certain misgiving on hearing of anyone leaving his analytic practice for long which is of course our only sure ~~basis~~ basis.

Please give my regards to Anna Maenchen. Colby I know well. He is a great fellow so long as you don't play chess with him at which he is a demon.

Wishing you all the best for the New Year

Yours Sincerely