To Publish or Not to Publish?

Marie Jahoda

Sussex, England

I am deeply grateful for the honour you have bestowed on me—and very embarrassed. An award inevitably leads to self-examination, never a totally agreeable experience, and in my case leading me near to the brink of becoming a Skinnerian in the realisation that historical events, family influences, good luck and good friends shaped my professional life more than I shaped it. In spite of my great respect for Professor Skinner I had to withdraw from this particular brink, unless I wanted to deny every psychological thought I had ever had before. This was facilitated by the assumption that your award committee, in its wisdom, was not so much concerned with establishing the causes of a contribution to psychology as with demonstrating that all sorts of contributions, orthodox and unorthodox, find room in its many-splendoured mansion. Perhaps the award was meant as a positive reinforcement (Skinnerian thoughts have a way of persisting) not for me but for those who may be in danger of following a party-line in their research rather than remaining in touch with the continuously changing world around them. In any case, the meaning of an award for the donor and the recipient is more complex than meets the eye; anybody in search of a thesis theme could do worse than investigate it.

When the award is designated to celebrate the memory of Kurt Lewin, the phrase that springs to mind as a mitigation of the embarrassment is that we stand on the shoulders of a giant. Robert Merton (1965) has traced throughout the centuries the history of this phrase which puts things into a proper perspective; but whatever comfort a recipient receives from it is rudely destroyed by Merton’s final words, quoting Freud: but when you are a louse on the head of a giant, your vision is hardly improved.

So I searched my past for evidence of non-Skinnerian shaping

Correspondence regarding this article may be addressed to Marie Jahoda, 17 The Crescent, Kaymer, Hassocks, Sussex, England BN6 8RB.
from the inside out, for having done things which were not in themselves immediately as rewarding as the large proportion of my professional life was. I began to wonder whether the fact that there exist a number of studies I conducted but deliberately never published could qualify as a genuine achievement. I was encouraged in this belief by evidence of a frightening overproduction of psychological literature from which we all suffer. A hard-pressed editor of psychological journals, researcher and academic administrator (Tighe, 1979) was driven by that situation to propose a radically new policy: the encouragement, perhaps even requirement, of publishing professional writings only posthumously. But since even he allows for talking about one's work during one's lifetime, let me try to discover whether I can derive some claim to glory for not publishing everything I did.

Here is a brief description of some of these hidden items that leave, on a rough estimate, at least 8 years of my research life undocumented. Two of these studies stem from the age of innocence, almost half a century ago in Vienna, when doing something interesting was an end in itself and preparing a publication too time-consuming because there were so many other things to be done. Only the memory remains of an investigation of the standard of living of Viennese beggars, in the process of which we discovered that they had a highly regulated professional organisation. Then there was a survey of the adult population during the Abessynian war, with the help of an empty map of Africa where people were asked to draw in Abessinia. We discovered that, in the minds of the population, this then topical country occupied three-quarters of the whole continent.

History intervened and innocence was lost. A third study had the ambitious aim of analysing the deep split in Austrian culture by comparing two types of jokes then current: jokes, in which der kleine Moritz was central, symbolising urban, Jewish, intellectual sophistication; and jokes about Count Bobby, symbolising the aristocratic, naive and impotent tradition. The examination of this joke collection during one of the nightly interrogations by the Austrian State police while I was in prison provided one of the most uncannily hilarious episodes in my life. The manuscript was never returned to me.

All the other studies reached the stage of a finished or nearly finished manuscript. The scene changed to England. In 1938 I conducted a study in the coalfields of South Wales, then a depressed area (Jahoda, Note 1). The very high unemployment among miners induced a group of Quakers to establish an
experimental scheme for subsistence production. The unemployed men were to produce through their own labour most necessities of life, which they could buy from the scheme for the cost of raw materials. The organisers provided capital equipment. The purchasing power of the unemployment allowance was thus to be significantly increased. The idea was to alleviate not only economic suffering, but also the psychological misery of unemployment, by a communal effort through production for use, not for profit. The study method was participant observation.

I used the same technique when I worked for 6 months in a paper factory as an unskilled factory hand to study the transition from school to the world of work (Jahoda, Note 2).

Another change of scene. You may recall that in 1947, when Eisenhower was President of Columbia University, there was a grass roots movement to draft him for the other presidency. He received many hundreds of letters from ordinary citizens urging him to seek nomination. Merton obtained Eisenhower's permission to conduct a content analysis of these letters which was performed under his guidance by Leila Sussman and myself (Merton, Sussman & Jahoda, Note 3).

A few years later I conducted a large scale study—questionnaires, interviews and participant observation—of the students of Vassar College (Jahoda, Note 4). Soon after, at the Research Centre of Human Relations at New York University, I was in charge of a team effort that aimed at elucidating the relation between mental health and the environment by studying two new communities, Fairless Hills and Levittown, both erected virtually overnight to house the families of staff and workers of a new steel works (Jahoda & Walkley, Note 5).

Back in England, I was running a seminar for graduate students when 30,000 Ugandan Asians were expelled by Amin and had to settle in Britain, whose race relations had already considerably deteriorated. With these students a study was designed aiming to estimate the positive and negative factors which would confront these new immigrants. The study was conducted at the reception centres by collecting life histories of heads of families and eliciting their attitudes, plans, expectations, hopes and fears. As best we could we also described the culture from which they came, with special emphasis on their religious beliefs and practices (Jahoda, Davis, Brewer, Rush & others, Note 6).

In every instance the original intention had been to publish a book, but none was ever submitted to a publisher. Why? Before I explain the various reasons for this self-restraint, I have to
ask you to give me the benefit of the doubt about the quality of these studies. In my own judgement they are on the level of, if anything perhaps better than, some others I did publish.

What these studies had in common is that they all dealt with aspects of the 'real' world, by which I mean that the study situations were not contrived for the benefit of research but the aim was to explore the actual life space of individuals in a social context. Because they tapped reality in the raw, as it were, it is not surprising that the outcome of research mattered not only to the researcher and, with luck, to the advancement of knowledge, but—with one exception—even more to the people involved, either as subjects or as sponsors of a study.

Let me first deal with the exception. The study of the two communities mattered mostly to the research team. The basic question we wished to address by a comparison of a working-class and a middle-class new town was this: what aspects of a community become psychologically relevant in facilitating or obstructing what we defined as mentally healthy behaviour? The definition of mental health implied certain approaches to problem-solving combined with a perception of social reality relatively free from need-distortion. We interviewed several hundred women about the individual problems each of them experienced, first soon after they moved in; then, eight months later, we asked about their then current problems and their perceptions of their community. The result was two interesting community studies, involving a reasonably sophisticated conceptualisation of problem-solving with the help of a flow chart over extended time, a nice application of Kurt Lewin's (1951a) idea of circular causality, a tremendous amount of hard work and, in the end, the realisation that the basic question could not be approached, let alone answered, with the tools we brought to it. The reason for this failure was a too simple-minded conceptualisation of the social environment (social class, participation in voluntary organisations, etc.). This oversimplification, it seems to me, still plagues much social-psychological research, even though I realised a couple of years later when I came across Roger Barker's concept of behaviour setting (Barker & Wright, 1955; Wicker, 1979) what we should have done. I still experience a pang of disappointment when I look at this study, but I believe the decision not to publish was right. In the experimental field the often heard call for publication of negative results is justified lest we accept theories on the strength of one statistically significant divergence from the null hypothesis. For an ambitious theoretical exploration in
the field, that did not pay off on the central issue, it is not justified, however interesting the by-products.

To confuse the issue, I’d better admit that I did publish much later a study (Jahoda, 1963) in the course of which I also realised that I had bitten off more than I could chew; in that much less ambitious enterprise I decided to switch horses in mid-stream and emerged with a book that has less innovative thought than the community study, hardly any explicit theory, but was nevertheless of some demonstrable practical use. Theory is, after all, not the only contribution that makes research useful. Perhaps my standards had slipped, you may think. In my defence I would propose that the decision to publish or not to publish requires the maintenance of several standards, depending on the various aims of projects. In the community study I aimed at a theoretical exploration but achieved only a description; in the second case the aim was to describe a relatively new educational institution, as reflected in the minds of students, for those who might wish to emulate the innovation. Even though not complete, the description served a purpose.

The other studies remained unpublished for one or the other of two related reasons: either because a gentleman’s agreement had been arranged to publish only if those affected agreed; or because I came to the conclusion that publication might conceivably damage somebody.

The first reason applied to the factory, the Eisenhower, and the Vassar studies. The management of the factory felt that the documentation of the shock effect of the transition from school to work would reflect adversely not only on their firm (whose identity might after all have been disguised) but on industry in general. They were right, for the study showed a rather frightening discontinuity in basic values of human decency acquired in those days in schools. The 14-year old girls quickly learned that making an effort, being interested in learning or showing respect to older people brought ridicule and isolation; flirting, personal gossip and excessive make-up brought popularity. The honouring of this particular gentlemen’s agreement cost me some moral qualms. My dilemma was resolved by the intensification of the war—the time was 1940—when the question to publish or not to publish was not one’s major worry.

No disguise would, of course, have been possible with the Eisenhower letters. What happened was this: between the time he permitted the study to begin and the time it was finished he had after all decided to become a candidate for the other
presidency. His political advisors thought that publication might cost him some votes, for the study revealed, of course, less about him than about a portion of the electorate, and in a not always flattering manner. Furthermore, they understandably wrote to him as a war-time leader, while he was about to establish his image as a peace-time leader.

The results of the Vassar study shocked its faculty, whom I had come to respect and admire. This was around 1950, when the faculty consisted of a group of academically outstanding teachers, mostly women, whose devotion to the highest standards was beyond doubt. But the study of the entire student population revealed the dominance of other values. Perhaps it should not have come as a surprise that family background, wealth, number of boyfriends, or weekends at Yale and other unliberated female aspirations, preoccupied the majority of the girls more than academic questions; but it apparently did. The lengthy report was debated in a series of rather heated discussions with the faculty, and these fulfilled the purpose of at least considering the possible truth of some unwelcome statements. Publication would not have served an equally useful aim.

Now to the entirely self-imposed censorship: during the conduct of the study of the unemployed miners in South Wales, Hitler marched into my native Vienna. Many of you may no longer fully appreciate what this meant for me, my Jewish family in Vienna, and my Jewish and non-Jewish friends there. I will not try to describe it here. Lord Forrester, one of the Quaker organisers of the subsistence production scheme, understood. He offered to go to Vienna immediately, visited my family, arranged a code in which to correspond, and brought them hope and support at a moment when fear and despair ruled their lives. A few months later he read my report, which showed that the great idealism with which the scheme had been started could not be transformed into reality. “You are destroying my life’s work,” he said. I decided not to publish.

A similar decision not to publish the study of the Ugandan Asians prevented the participating graduate students from establishing early in their careers a well-deserved reputation for good work, a fact that bothered me more than them. The reason was this: What we discovered about some of the Asians’ attitudes to the Africans among whom they had made their living for many years showed that they were prejudiced against them and had treated them with contempt. I feared that if this became known at a time when good will had to be mobilised throughout
the country to facilitate their settlement in England, it would create a further obstacle in a situation that was already difficult. A check with an experienced social worker, who knew the Ugandan situation first-hand and was then engaged in the resettlement procedures, confirmed both the conclusions of the report and my fears about making them public.

It seems to me now that these various reasons for deciding not to publish were by and large good reasons. At the time, however, matters were less clear. In most cases there were, after all, also good reasons of a personal and of a more general nature for going into print. The conflict in myself and the negotiations with those concerned before a decision was reached required an additional investment of time and energy. I cannot deny that doing what I now think was right, then felt sometimes like being unfairly defeated. But the decisions were, after all, right or inevitable. What is more, I think that they were not just the result of an accidental accumulation of external circumstances, but that in one form or another they stem from risks that are unavoidable if you take social psychology outside the laboratory. If this is correct, two questions arise: is the risk worth taking? and what, if anything, has it got to do with publication overload?

Consideration of the first question involves, inevitably, some critical comments on mainstream social psychology, which eschews this particular risk by concentrating on laboratory experiments. I am deeply convinced that social psychology as a discipline has suffered from an unwillingness among many to take the risks of field research. I know I am on dangerous grounds with that statement, particularly at an occasion designed to celebrate the memory of Kurt Lewin, for his field-theoretical approach was, after all, most convincingly realised in a series of ingenious experimental simulations of 'real' life. He wrote: "If the views of the field-theoretical approach are correct, there is a good prospect of approaching experimentally a great number of problems which previously seemed out of reach . . . the experimenter . . . does not need to be afraid of creating 'artificial', 'unlifelike' situations. Experiments become artificial if merely one or another factor is realised, but not the essential pattern" (Lewin, 1951b). The nub of the matter seems to me to lie in his lack of specifying how one comes to identify 'the essential pattern'. If he could rely on his intuitive insight into what makes the world go round, I am not sure that we could or should. The South Wales miners who were socialists, who in conversation asserted their belief in production for use and not profit, who affirmed in their inspired
evening conversations that everyone should contribute according to his ability and receive according to his need, might have professed adherence to these great ideals in an experimental simulation; in the subsistence production scheme they could not live up to it.

Many of Kurt Lewin's students have acted on the assumption that they too could identify 'the essential pattern'. As a result, mainstream social psychology is often no longer social, treats people like objects rather than persons and, where it does not, limits its concern to the cognitively rational and consistent; it has not tackled the circular causality between individual and group. It has amassed a great amount of data through sophisticated methods based on a variety of mini-theories that come and go as fashions used to do. But it has not yet transcended, as it surely should, the inevitable technicalities and jargons of our research world to provide a point of view to the world outside the laboratory that could serve as an orientation to the perplexing social psychological problems of our times.

These criticisms are not new, of course. Kenneth Gergen (1978) has recently traced some of them back through decades; in Europe an interesting though inconclusive debate has been published (Moscovici, 1979; Semin & Manstead, 1979; Tajfel, 1979; Taylor & Brown, 1979) on the question whether social psychology needs to become more social. Dorwin Cartwright, in his Kurt Lewin memorial address, raised the same misgivings when he said: "Social psychology, I regret to say, has in recent years become increasingly less social . . ." (Cartwright, 1978). Inherent in such self-criticism is invariably the doubt that we have got 'the essential pattern' right. It is positively embarrassing to remember how many good social psychologists spent untold hours to demonstrate that group decisions are more risky than individual decisions, in view of the many groups organised to advocate caution on, for example, nuclear reactors or genetic manipulation. The 'essential pattern' in attribution theory is based on a model of rational man, even though a cursory glance at any daily paper any day makes it clear that "the world, in oldest fashion, by hunger moves and passion." Fortunately the number of studies is growing in which the 'essential pattern' is checked against the actual behaviour of people in uncontrived situations. Ellen Langer (1978) has recently criticised the assumption of rationality in attribution theory, introduced the concept of 'mindlessness' and demonstrated in ingenious empirical work that in our daily transactions we are all inclined to be guided by established
patterns of behaviour and gut feelings, rather than by our minds. And Tversky and Kahneman (1971) have convincingly demonstrated that even the minds best trained in formal logical reasoning use intuitively, a different, inductive 'real-world-reasoning', when put on the spot in actual decision-making (Boden, 1980). There are, of course, other examples. What distinguishes them from mainstream social psychology is their emphasis on external validity, that form of validity most neglected in our methods courses.

From the point of view of publication possibilities field studies are riskier than experiments, as I know all too well; but they are safe from the accusation of substituting preconceived notions for the real thing. Lest I oversell the need for more field studies notwithstanding their risk of remaining in a filing cabinet, let me add that they have, of course, also their intellectual hazards. How valid are their findings from one, inevitably limited, setting to another, or even within the same setting transhistorically? (Gergen, 1973). Suppose I were to try—which I will not—to publish my unpublished work now that the danger of bad consequences has passed and the heat of debate subsided. Would they have anything but historical interest? Or, to put it in more general form, is 'the essential pattern' inevitably changing over time, so that every new generation of social psychologists would be well advised not to take what we can teach them too literally, but to look anew in an everchanging world for changing essential patterns? For the present this is good advice, because we have not yet given enough attention to what changes rapidly and what remains relatively constant and because too many devote their work to a search for high-level abstractions. I hope that the current generation of young social psychologists will change all this.

Now to the second question of publication overload: On the assumption that I have made a case for more field studies, will the inherent danger of their remaining unpublished reduce the literature mountain significantly? I am afraid not. Eight working years of my life feel like a long period on the personal level. On the collective level of social-psychological publications they form a very small drop in an ocean that is threatening to engulf us. So not only is there not a particular personal merit in my not having published, risky field studies are also no solution to the publication overload. We must search for additional ways of stemming the deluge. Why this is necessary will become clearer if I first review the very good reasons for going into print.

Foremost among them is that science is a public affair; it cannot flourish without entering into the market of ideas. Related
and equally important is the idea of freedom of information. I now belong to a research group that, as a basic principle, does not accept any research contract that excludes publication possibilities, however great the temptation at a time when money is hard to come by. While I stick by this principle, the right to publish does not imply a duty to do so. As it happens, these weighty reasons are strengthened in the minds of us all by their coincidence with our self-interests. I cannot see anything wrong with self-interest in this respect, particularly when it coincides with some interests of the discipline and when the ‘publish or perish’ idea is as solidly institutionalised as it is in psychology. But even for the lucky ones among us who have established a reputation, a publication is quite rightly a deeply satisfying event in one’s professional life, testimony and preliminary end-point to a lot of hard labour, and confirmation of one’s public standing.

These powerful reasons for publishing have unintended consequences of frightening proportions. The Psychological Abstracts contain over 13,000 items produced in 1978; since 1960 about 200,000 were there cited. Abstracts for special fields are relatively rare, but the Index of Psychoanalytic Writings, for example, has long passed the 100,000 mark. The Mental Measurement Yearbook (Buros, 1978) first published in 1938 and always applying rigorous standards has, to date, listed 77,367 tests. No psychologist can any longer keep up with the literature, not even in special fields. Publications, though essential for science, become a self-defeating enterprise if they can no longer be read. As we struggle to read and yet get more and more behind, surfeit and disappointment set in. When we complain, as many of us do, about the trivialities that nevertheless find their way into print, we are partially justified in rational terms; but partly this is self-defence against the growing flood, and an excuse for giving up the hopeless struggle. The legitimate interest of each of us in wishing to publish conflicts with the interest of all of us.

As a result of this, psychology seems to be splitting at the seams. There is an increasing parochialism with regard to geographical and historical coverage, and a tendency to ever narrowing specialisations and less interaction with neighbouring fields. It is cold comfort to realise that most sciences face similar problems.

What shall we do? The first thing is to recognise that there are self-correcting mechanisms at work that will affect the situation in the long run, whether or not we engage in deliberate action. Exponential curves have a way of bending under their own weight in economic growth, in energy consumption, in population increase
and in the production of the psychological research literature. Producers and consumers of that literature find obstacles to their goal-directed actions that induce them to take longer routes or even leave the field.

Kenneth Clark and George Miller (1970) reported that in a period when the population of the USA had grown by 50%, the number of psychologists had risen by almost 600%. The extrapolation of this trend yields the daunting prospect of more psychologists than people early in the 21st century. If that study were repeated now, it might already offer a less gruesome spectacle for the future. My guess is that the rate of increase of research students has dropped more sharply than that of the population, for a variety of reasons, mostly economic, but perhaps also because of the fragmented current state of psychology. If I am right, this will ultimately reduce the research literature.

Lest we entirely rely on such self-regulating mechanisms it is well to remember not only that they may take a long time to work their way through the system, but also that they are blind to quality. What if they turned off the first-rate minds and left the field to the mediocre? What if they were biased against new ideas and innovators, against the young and for the establishment? Deliberate action is called for, in an attempt to push self-correcting mechanisms into desirable directions. Some such efforts are already under way. Attempts at quality control result in the fact that some journals reject over 80% of submitted articles; but there are also some complaints of rejections for the wrong reasons. Are there some anonymous referees who share the possible biases of the self-correcting mechanisms?

The life of the consumers of research literature is facilitated by the policy of producing more and more summary articles, annuals and 'Advances in . . . '; the retrospective review articles in Contemporary Psychology are designed to counteract historical blindness and some—a very few—social psychologists (e.g. Apfelbaum, 1980; Lubeck 1980) have started systematic efforts in that direction. This is all to the good. Whether it is enough is another matter. The pressure to produce is mounting as competition for tenured positions increases. Appeals to self-restraint in rushing into print are futile. The debate in the American Psychologist about uses and abuses of the citation index has shown that self-restraint amounts to self-punishment and to the possible down-grading of the institution to which such a masochist belongs.

In some branches of psychology the resulting frustration of the younger generation is mitigated because there are alternative
or additional ways of manifesting achievement and excellence: they have practising professionals. The satisfaction of a successful clinical, educational or industrial psychologist, who has helped individuals, groups or institutions to face better the inevitable conflicts of life should be at least as great as that derived from publication. In social psychology we have as yet no professional praxis, though the need for it is great. Should we begin to think how such a profession could be created? Or should we advise more of our students to enter the already available professions?

Of course, some publications are required from professional fields too, but they will be fewer in number and take much more time to produce. Most of them will be the result of prolonged field studies, and since the pressure to produce will be not as unrelenting as it is for the young university teacher, they may even benefit in quality by permitting more time for thought.

For the present, however, the pressure will continue. As many of you might find it both more necessary and more difficult to get into print, there is the hope that if you persist you will one day be standing where I stand now to tell a captive audience in one swoop what they have missed, perhaps more effectively than if it had gone into print and remained unread.

REFERENCE NOTES


REFERENCES


