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Afterword

My first reaction to Eric Trist's proposal for a three-volume anthology of the Tavistock work was quite cold. I was not interested in another voluminous institutional history gathering dust in neglected corners of university libraries. I was much more interested, personally, in continuing in the Tavistock tradition of engaging with the workface. As far as the earlier Tavistock publications were concerned I felt that I had them under my belt and that they belonged to the past.

Before Volume I went to press, I was of a very different mind. As Trist's health faltered during the preparation of Volume III, I was more than happy to offer my assistance with the editorial task. This change of mind, which was brought about by renewed acquaintance with those earlier documents and seeing them anew with the benefit of hindsight, prompted two questions that remain as challenging as ever as the third, and last, volume goes to press:

- Why do so many of these papers, many dating back to the 1940s, 1950s and 1960s, remain of "continuing interest" (Vol. I:xi)?
- How did the Tavistock Institute maintain such "group creativeness" (Vol. I:28) for more than two decades?

The puzzle leading to the first question is that in a field that is accumulating knowledge the early contributions are usually absorbed by later advances and, apart from a few classics, simply forgotten. The working scientist is concerned with keeping abreast of the field by reading current publications and reports. The papers brought together in these volumes have retained their interest value because, unfortunately, the social sciences have proven little able to accumulate knowledge. The social sciences have been dominated by academic research and the requirements for academic recognition of both the disciplines and the practitioners. That research has been unrelentingly in-grown. To gain academic respectability for the disciplines, it has been necessary to mold them in the guises of the physical sciences. At the same time, the social sciences have had to be careful in their theorizing not to trespass on, nor challenge, the traditional wisdoms embodied in the humanities.

To gain promotion and professional standing social science researchers have had to get publication in professionally controlled, peer-reviewed journals. This has meant addressing matters that are currently viewed as important by the profession and addressing them in ways that will impress their peers by their scholarship and sophistication. History is ignored, not because it has been

absorbed but because it is no guide to present facts and, worse, it would suggest a lack of proper appreciation of some current fad. In academia, it has nearly always been quite unimportant as to whether the matters have social significance or the methods have practical relevance. Trist's account of the emergence of the Tavistock-related Institute of Operational Research is precisely about people trying to avoid the consequences of being academicized (Vol. I: 17).

By contrast, the Tavistock papers emerged from a context of multidisciplinary project teams committed to the social engagement of the social sciences. There was a mutual commitment, enforced by peer review, to the aim of doing socially relevant science and to the transparency of methods for the benefit of non social scientist clients. Accordingly, the papers, for the most part, dealt with real issues in their own terms. (We were not perfect. We brought to our multidisciplinary projects many of the concepts and methods that we had each absorbed in our own disciplinary educations.) As realistic studies in a welter of unreal and turgid studies, they have remained as welcome as a breath of fresh air in a deep cave.

The negative views expressed above are not peculiar to myself. In 1959 there appeared a massive, multivolume series entitled *Psychology: Study of a Science*. This was supposed to be a monument to the achievements of the science of psychology. Sigmund Koch was the editor of the series. Two years later he published his own deep and depressed concerns: "Psychology has been far more concerned with being a science than with courageous and self-determining confrontation of its historical subject matter" (1961:624) and "has constructed a language that renders (this) virtually impossible" (p. 631). It was almost 20 years further on that Paul Kline, professor of psychometics, reached even more pessimistic conclusions but diagnosed the same basic fault: "academic psychology . . . is not concerned with what appear to non-psychologists to be the most important or interesting aspects of being human" (1988:11).

The Tavistock Institute was founded to forge a different, more practical, path for social scientists without deviating from the aim of advancing the competence of the social sciences. There was never a lack of friends and allies from within academia but there was persistent institutional hostility. It is due to the default of the academic social sciences that the Tavistock contributions have retained their relevance.

It is much more difficult to address the second question about group creativity. Trist noted that the drift into a typical managerial hierarchy stifled creativity and led to the revolt and split in the Tavistock Institute in 1961 (as it had done in the Tavistock Clinic in the early 1930s [Vol. I: 2]). Few social scientists would disagree with his observation that a democratic framework is required by a research institute for sustained creativity. I think, however, that there are a couple of matters that ought to be noted in these closing remarks. The most innovative move of the reconstituted Institute (the Human Resources Centre)

was probably the establishment of the "meeting in college" as the authority to determine the directions of Institute activity. This replaced the Management Committee. The college was composed of all of the social scientists, regardless of grade. It met at least once every nine months under search conference conditions—in a "social island" away from the offices for two days and nights. Only these meetings in college had the authority to lay down policy and appoint the officers and administrative committee. The latter were empowered to call for an earlier meeting in college if they found that pressing circumstances demanded new policy determinations. These meetings had the important additional function of reaffirming the collective identity of the Centre and its standards. As a result of our conscious decisions our members were increasingly engaged in long-term projects scattered around Europe (North America only became significant in the later 1960s after jet travel emerged). Only the college meetings provided a regular opportunity for all to review all of the projects and systematically relate new developments in the segments of social life touched on by those projects. Incidentally, these reviews permitted us to check the adequacy of our arrangements for administrative support.

The other innovation (for us) was the separation of the formal statuses (and salaries and promotions) from operating roles. The first had to be geared to the expectations of families, friends and the labor market. Our research projects had to be based on what were, in effect, semiautonomous, self-managing groups. A person's role in a project team was based on potential contribution, not on formal status.

These measures were not expected to create individual creativity but we thought they would release the creativity of our members. Creativity was welcomed, from no matter whom, because of its contribution to shared project goals and enhanced our institutional competence. Creativity was not frowned on, as is so often the case in academia, as a threat to existing pecking orders or as a source of interpersonal rivalry and jealousy. As it happened, this proved to be an attractive milieu for other social scientists who felt that their creativity was frustrated by their current jobs. This was important to us because we could neither offer the security or tenure of a university nor the money available in consultancy.

There is a third general matter that any independent social science institute has to confront and that is "appropriate means of securing financial stability" (Vol. I: 29). The second, post 1960, phase of the Institute provides some leads. If the social scientific mission is to be sustained then indulgence in activities solely for the money-earning potential can only end in disaster. The top end of this market, consulting to chief executives, is fickle and distorts the power structure inside the Institute. The bottom end is "tinkers' work and draws tinkers' pay" (a common saying in the Tavistock during the internal dispute). We achieved financial stability in that period by focusing on important practical concerns that held out a social scientific challenge. We were lucky in that there

were powerful organizations in Europe that realized that they faced major challenges and thought that social science might be able to help. By being flexible in our organization and being able to think on the run, we were able to convince those people that social science could, indeed, be of some help. University-based social scientists could not and would not expose themselves to these sorts of challenges. In many cases they were approached first as corporations etc. wanted the extra reassurance that a university connection appears to give. Consulting firms were not trusted to come up with new solutions. The organizations we dealt with usually had plenty of experience with consulting firms and regularly used them to extend the range of proven options. They also well knew that that was the way consulting firms made money and that they did not make money from innovating. Scientific innovation, on the other hand, was our bread and butter.

What does this tell us? To be independent, a social science institute has to be proactive in sensing where new and important practical problems are emerging and seek to engage with them. The more that the institute is engaged at society's frontiers, the better equipped it is to sense emergent problems. Financial security follows from being seen as competent to engage with these problems. Such competence certainly does not follow from the provision of financial security. The major threat that the Tavistock Institute was facing when I left, for family reasons, at the end of the 1960s was that of being accepted as part of the British establishment. That was a fair guarantee of financial security, nine-to-five jobs in London and an end to creativity.

Creativity is a property of individuals not groups. The group can affirm the creativity of the individual and amplify it through exploiting the new visions it brings. On the other hand, the group can take alarm and stamp out both the visions and the visionary. The question is why the Tavistock was such a place that it chose to amplify the creativity of Eric Trist, his colleagues and those who followed.

But then we have to raise a deeper question. Why do creative centers of social science last not much more than 10 years in our so-called modern industrial societies? It is possible that we are still at the stage where creativity in the social sciences raises more questions than it solves.

References

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