

## **ABSTRACT**

A substantial part of this paper is not written by the author, but consists of a correspondence between Ronald Lippitt and John Collier about action research. Lippitt is recognized as a close associate of the supposed inventor of action research, Kurt Lewin. Collier was Commissioner for the US Bureau of Indian Affairs 1933 – 1945, and architect of the “Indian New Deal”; and he too played a role in the invention of action research. Indeed his early action research projects predated similar work by Lewin, with whom Collier collaborated, although this is rarely acknowledged. Lewin himself famously wrote very little about action research. This correspondence followed shortly after Lewin’s death, so has slightly later than contemporary significance. Its significance, as of 2002 can be seen four areas. First, there is a discussion of the role of science and the scientist in action research. Second there is a debate around the role of the scientist action researcher as social activist. Third there is the question of whether the purpose of action research is to achieve content or process goals. Fourth there is a consideration of the strategic and tactical consequences for the action researcher/ action research institute of adopting/ not adopting the scientist identity. These are all current concerns for action research; this article shows how they have been from its start. In the correspondence Collier argues against the action researcher as value free process only technocratic expert; however those who sympathise with this position will find a sting in the tail.

## INTRODUCTION

The most important, and substantial part of this paper is not written by the named author, and is included here as an annex. This annex consists of excerpts from a correspondence between Ronald Lippitt and John Collier. This starts in 1945, but that in the annex dates from 1948. Ronald Lippitt's correspondence is reproduced with the kind permission of Dr Larry Lippitt. John Collier's correspondence is reproduced with the kind permission of Mrs Grace Collier. It is to be found on reel 38/452 of the *Collier Microfilm Archive of the John Collier Papers 1922-1968*.

The correspondence relates to action research, and it is, it is argued here of historical and contemporary significance. In assessing that significance it is important first to address the question of historiographical epistemology. Jenkins (1991) sees the past as epistemologically fragile. The past consists of an infinity of events. History, our knowing of the past, is constructed by selecting some of those events as significant and some not, and giving meaning, ie interpreting those which are selected. This of course leads to a possible problem of post-modern relativism, and the danger of a process of reasoning which leads to a conclusion that there are no absolute truths in history. Without getting too far into historiographical philosophy, that is not the view held here. At the same time it is accepted that our understandings of the past are constructions, albeit with certain events at their core which are, or can, or must be taken as objective givens.

Moreover, Jenkin's (1991:17) claim that "history is never for itself, it is always for someone" is given some credence here. If this is true for orthodox histories, though, then it is also the case for the revised fragment that is presented here. The significance claimed in this paper therefore should at the very least be read with an understanding of its author's own sympathies. These are a generally favourable disposal towards Critical Management approaches which critique presentations of management as ideologically neutral; and a general scepticism toward the uses of

participatory methodologies like action research and OD (eg Cooke and Kothari 2001 is endorsed). This certainly influenced the decision to research Collier (who, Cooke 1999 also points out, was on the political left), the author's recognition of the correspondence as important, and choices about which parts thereof were included and excluded here. However, readers following this paper through to its end are not obliged to agree with me, or with one or either of the positions set out by Collier and Lippitt. Particularly important to note here is that those who initially sympathized more with Collier, and/or with my more general position will find a discomfoting sting in the tail of this paper.

## **THE PLAYERS AND THEIR IDEAS**

### **Kurt Lewin and Action Research**

Kurt Lewin is identified by the OD/Change management orthodoxy as its founding father (eg according to Schein 1980: 283 "there is little question that the intellectual father of contemporary theories of applied behavioral science, action research and planned change is Kurt Lewin"). Lewin is renowned for his development of force field analysis, the three stage unfreeze/move/refreeze model of planned change, and more generally his determination to integrate theory and practice. This was embodied in the title of Marrow's (1969) biography of Lewin, "The Practical Theorist". Lewin was also one of the founders of the journal *Human Relations*, although its publication coincided with his untimely death in 1947. Amongst his other achievements are the first, the invention of "group dynamics" (both the idea and the term), which is described as developing from the first group process workshop in New Britain, Connecticut, in 1946 (see Cooke 1999). Particularly important for this paper is Lewin's development of action research. Although Lewin only wrote two articles on action research, one of which was also about the New Britain workshop (*Action Research and Minority Problems*), he is nonetheless widely, with an exception discussed below, seen as the inventor (eg Raelin 1999).

Action research is important as a methodology in its own right, with many internal debates and differences over the detail which are not addressed here. Although first written about in the mid-1940s, new and substantial work on the field continues to emerge, not least Reason and Bradbury's (2001b) 45 chapter *Action Research Handbook*. *Human Relations* (Elden and Chisholm 1993) and *Management Learning* (Raelin 1999) have had special issues on Action Research, both emerging from Academy of Management symposia.

Action research also contributes four key ideas to OD, and to change management more generally. First, the understanding evident in its name, and Lewin's aphorism "no action without research, no research without action", is that all organizational interventions (ie action) should be informed by research. Conversely, research should not be for its own sake, but to lead to organizational change. Second, there is the idea of collaboration between researcher and researched, which translates in OD terms to consultant and client. This is supposed to lead to a greater ownership of the organizational problem being addressed, in that the client and consultant reach a shared understanding of the root cause and subsequently what is to be done. It is also assumed that the client has a level of understanding and expertise which they can bring to the intervention, that the action researcher/consultant will not be able to match. Finally, action research suggests a series of steps - building an agreement between consultant and client, data collection, data analysis, action planning, action, evaluation, and so on, which underpin OD/change management process in their own right. These steps can also be seen to underpin separate models of the consultancy process, and of planned change.

### **Ronald Lippitt - Lewin's Inheritor and Promoter?**

In the history of management ideas, Ronald Lippitt is probably the better known of the two correspondents. Lippitt was co-author of two management classics. *The Dynamics of Planned Change* (1958), which made current the term "change agent" and presented a step by step quasi action research approach to planned change which was to underpin the OD and Change Management orthodoxy (eg see French and Bell

1998). *The Consulting Process in Action* (1978), co-authored with his brother Gordon was one of the first guides to consultancy to set out the ideas both of the consultancy process, and of the distinction between “task” and “process” oriented consultancy. This was not all. In the 1930s, with Kurt Lewin and Ralph White, Lippitt conducted the famous research into leadership styles, which made the distinction between authoritarian, laissez-faire, and democratic leadership (Lippitt et al 1939). Lippitt was also a participant in the 1946 New Britain workshop. Although it is true than Lewin himself produced little about it, and about action research more generally, Lippitt’s own extensive account of New Britain, *Training in Community Relations*, published in 1949 is often overlooked. Indeed, it is case that Lippitt is in Lewin’s shadow, and his own achievements are underplayed as a consequence. Lippitt himself did much to promote Lewin’s reputation post-mortem. Hence *The Dynamics of Planned Change* was dedicated to Lewin, and the planned change process it outlined was explicitly based on Lewin’s three stage unfreeze/change/refreeze process.

### **John Collier, and Lewin and Lippitt and Action Research**

The New Britain workshop was about improving race relations; and in Action Research and Minority problems, Lewin speaks out against Jim Crowism, and the potentially harmful domestic effects of US imperialism. He calls for the policies proposed by one “John Collier” leading to gradual independence to be followed. In their lifetimes, and possibly subsequently, Collier was/is probably generally better known than Lewin. A public figure, Collier was the longest serving ever Commissioner of the Bureau of Indian Affairs (BIA), from 1933 to 1945 (ie during Roosevelt’s Presidency), was responsible for the Indian Reorganization Act (IRA), and the so called “New Deal for the Indians”. Subsequently in 1945, he established the Institute for Ethnic Affairs in Washington DC, of which more below. Collier is the subject of two biographies (Kelly 1983, Philp 1977), published widely himself, including an autobiography (Collier 1963), and there are a number accounts of the impacts of the New Deal (which are the subject of controversy) on Native American groups (eg Parman 1976).

Mention of Collier's involvement in the invention of action research is limited in OD texts, but always tracks back to French and Bell (1998) who in turn cite personal correspondence from Lippitt. According to French and Bell, Collier invented action research at the same time as Lewin, but independently of him. This is not how Collier himself represents his relationship with Lewin. His biography contains a photo of Lewin and the claim that before his death he had become "...one of my own intimate friends..." (1963:233). Certainly, there is evidence within the Collier archive that he and Lewin were close, not least a letter on Lewin's death from Lippitt to Collier stating:

I think I can truly understand your feeling at the news as expressed in your note [not found in the archive] and in your poem, which I have not as yet shown to Mrs Lewin, although I think one of these days she will like very much to read it.

I think you will be glad to know that after the first shock the result of our loss has been a high state of morale on the part of the students and the staff in reacting to what is perceived as a crucial challenge to somehow fill in by group ability the loss of great individual ability.

Collier was to apparently disagree with this latter hope, writing in 1963 (233) that Lewin's "... human insights and principles faltered at the hands of somewhat lesser men...." This may be an intended slighting of Lippitt, given the existence and nature of the correspondence discussed below, and Collier's reputation for frankness. If so, it is not fair to Lippitt, in terms of his own work, or his promotion of Lewin's reputation. If Lippitt was aware of this opinion then his own apparent pointing out of Collier's role at the birth of action research to French and Bell is particularly generous. Of course it may not have been intended as slight; and another reading sees ironic self criticism, as Collier himself had previously claimed (see below) to be acting on Lewin's heritage. Either way, it does not do justice to the correspondence which follows.

Another part of the correspondence fascinating in its own right is Lippitt to an IEA administrator on May 14 1946:

I am sorry to say that it will be impossible for me to get away on May 29<sup>th</sup> for our annual dinner meeting. I would certainly like to do it, but we'll be right in the middle of a cooperative project in the training of community leaders in about a dozen Connecticut communities, working with the Governor's Committee on Intergroup Relations. It is organized as a genuine action research project and is giving us an opportunity to test out a number of hypotheses in a way which I think is rather exciting.

This is of course what was to become known as the New Britain Workshop. Lippitt's excited anticipation somewhat belies the popular understanding that the outcomes of the event were an unexpected surprise (see Cooke 1999).

### **Beginnings of a Debate on Action Research**

Turning to action research per se, earlier, in August 1945 while Lippitt was working at the Federal Security Agency Training Section, he wrote to Collier about his IEA prospectus and its reference to action research. This is worth quoting at some length:

.... the process of action-research as we have meant it and developed it in usage denotes quite a new thing. It is not research-to-be-followed-by-action, or research-on-action, but research-as-action. The getting of citizens involved in planning, executing, and facing in analysis of a fact-finding process about themselves. Such projects can be counted on your fingers and over half of them failed. The others were dramatically successful. Many others will fail until there is recognition that this is not a simple process. Basically it is social therapy with skilled, non-directive leadership in the fact-facing and insight-having process. As Kurt Lewin says in the conclusion of a paper which will soon be out in the *Journal of Social Issues*, "... this principle of in-grouping

makes understandable why complete acceptance of previously rejected facts can be achieved best through the discovery of these facts by the group members themselves. Then, and frequently only then, do the facts become really *their* facts (as against other people's fact). An individual will believe facts he himself has discovered in the same way that he believes in himself or his group. The importance of this fact-finding process 'for the group by the group itself' has been recently emphasized with reference to re-education in several fields. It can be surmised that the extent to which social research is translated into social action depends on the degree to which those who carry out this action are made a part of the fact-finding on which action is to be based." Many of the projects mentioned in the prospectus are of course not action-research....

This is indicative of an early recognition of the complexities of action research; but also (on the part of both Lewin and Lippitt) of the positive "ownership of the problem" that action research can generate. It also provides a reminder that action research projects had been under way for some time before action research itself began to be written about. This was as much the case for Collier as it was Lippitt and Lewin. Indeed, as has been noted elsewhere (Cooke 1999) Collier was writing as early as 1917 of the New York People's Institute with which he was then connected "The Institute's is role action not talk. Experimental sociology is action..." (in Kelly 1983:73). In May 1945, Collier published a review of his time at the BIA, in which he described his approach there in terms of a series of principles. Principle seven (also cited by French and Bell):

"... I would call the first and the last; that research and then more research is essential to the program, that in the ethnic field research can be made a tool of action essential to all the other tools, indeed that it ought to be the master tool.... We had in mind research impelled from central areas of needed action....since the finding of the research must be carried into effect by the



administrator and the layman, and must be criticized by them through their experience, the administrator and the layman must participate creatively in the research, impelled as it is from their own area of need." (1945:275).

### **The Institute for Ethic Affairs**

Despite the evidently warm relations between Collier and Lippitt, that continued for a while anyway, Lippitt's August 1945 letter does point to what was to become the issue at dispute between them; namely the true nature of action research. The Collier archive contains several drafts of the prospectus for the IEA, and in some places it is hard to make out where one draft stops and another takes over. We cannot therefore be sure which version of the document to which Lippitt is referring. What the archive does contain, however, is a copy of the prospectus which was finally printed by the IEA. We can be fairly sure that this was *not* the version that Lippitt commented on, in that the prospectus page and section numbers mentioned in other parts of Lewin's letter not reproduced here do not match up. It is however a final, not a draft document, and thus goes as far as is possible towards being a definitive statement on Collier's part. The preamble (1945: no page number) defined the IEA as: "An action research agency created to find and to achieve solutions to problems within and between white and colored [sic] races, cultural minority groups, and dependent peoples at home and abroad."

It continues (1945:1) that the IEA

“has been established to deal with the profoundly disturbing problems of group tension and conflict, problems commonly referred to as “racial” in origin..... [IEA founders] believe that solutions are possible only if peoples develop to the utmost their native capacities, utilize them in harmony with other peoples, and participate fully in the determination of their own destinies...they believe these solutions must summons a deliberate and integrated use of the sciences.”

This is preparatory ground for the introduction of action research, the definition of which starts out along terms which would then and subsequently be generally accepted, say within OD. However, it concludes by taking quite a different turning:

The Institute’s approach to these problems will stress the importance of “action-research”. It is intended that which has been used successfully on other occasions, and to push through with all energy from research finding to knowledge in action. In brief action-research combines these essential elements: (a) assembling data, published or unpublished, experimentally proven or subjectively experienced in the lives of people (b) sharing the task of research with the very people whose hazards and whose needs are under scrutiny – indeed inviting and encouraging the leaders of people to assume a prime responsibility in working out the task: and (c) calling to assistance all the agencies of government, of private and public finance, of public opinion,

and of conscience, in programs of action which arise out of the needs of people and move toward a better ordered world.

Point (c) is important in relation to understanding the correspondence that follows, in that it sites the action that is to be taken following the research outside those being researched. The action-research orthodoxy, including that to be found in OD, has it that the researchers and the research have responsibility themselves for action. This, in essence, is the focus of the correspondence in the annexe, and is discussed further in the commentary thereon which follows. Before moving on, though, it should also be noted that prospectus continues:

Action-research by preference will be the method used, because in human affairs, research is immeasurably more effective when evoked by the needs of action and made to flow into action and to be tested through action. And action without prior and continuing research is wasteful, when not dangerous (1945:2).

A resonance here, then with Lewin's "No action without research, no research without action". Also clear, and relevant to the following correspondence, is the claim made for scientific authority; thus while affirming the belief in action research,

"...the Institute emphasizes that its approach is scientific. No implicit assumption is made that all ethnic or minority groups can avoid responsibility

for the disadvantages of their own situation. The institute will seek to operate not with the one-sided zeal of the reformer, but with the scientists passion for discovering the truth, conditioned by the peoples' dream of an energetic democratic society" (1945:3).

So those being researched do have some responsibility for problem solving. The ground is set, then to consider the correspondence. Before we do so, though, we should note first, that Lewin and Lippitt apparently signed up to the contents of the prospectus, in that it lists Lewin as one of three Vice Presidents, and both Lewin and Lippitt as members of the board of directors, along with other prominent social scientists like Clyde Kluckhohn and Laura Thompson. Second, the handwritten aide memoire that accompanies the correspondence in the archive suggests that Collier thought it was important, and to be preserved for posterity, even though circumstances at the time [which are not specified] meant that it could not enter the public domain.

### **The Relevance of the Correspondence to Understandings of Action Research**

The correspondence to our understanding of action research has several layers of relevance before even its intrinsic content is addressed. First, its very existence, hitherto unacknowledged, provides evidence that Collier has a far greater claim to be recognised Lewin's equal, at least in action research than has hitherto been the case. This in turn points to the need to continue to rigorously research Collier's contribution, not least to explore the detail of the action research projects that he conducted. That this is the case, second points to a historical ignorance, even denial

on the part of the action research literature. These words are chosen carefully. Accounts of action research have usually tracked back to Lewin, the founder, and claim to be acting on his heritage. Yet at the same time it is recognised that Lewin himself wrote very little about action research; and some concerns he did manage to raise are continuously ignored. At the same time other quite astonishing historical connections are made. It is claimed, for example that action research belongs in the Marxist/Gramscian tradition (because it is about changing the world (Reason and Bradbury 2001a). Yet the contribution of Collier, no shadowy figure, and to a lesser extent Lippitt, goes unrecognized.

The sin is compounded because of the very claims made for research rigor, for reflexivity, and for a commitment to learning on the part of action research practitioners on the part of practitioners is clearly not followed through in terms of understanding action research's own development (again see many of the chapters in Reason and Bradbury 2001b). This particular dimension of self awareness, should be, it may be thought, a foundation level competence. Yet it does not exist, despite the huge amount of material on action research which suggests that it is not under-researched per se.

Turning to the content of the correspondence itself, it speaks for itself, and it is not intended to provide a comprehensive summary here, nor to perform the presentist act of detailed reinterpretation according to current debates within the field. The correspondence does speak for itself, and for this author, with telling insights not just

into the history of action research, but also into current theory and practice. From that current standpoint of the end of 2001, though, four overlapping themes stand out, and other readers may see more. There is the role of science and the scientist in action research; there is the role of the scientist action researcher as social activist; there is the question of whether the purpose of action research is to achieve content or process goals; and there is the question of the strategic and tactical consequences for the action researcher/action research institute of adopting/not adopting the scientist identity.

Thus Lippitt argues that the action researcher role as a scientist requires a focus on process, and a requirement that an alignment with particular causes or content goals undermines claims to science, and also can be strategically and tactically problematic. Colliers response is to argue first, that the action researcher cannot help but make content goal choices when, for example intervening in one arena and not another, and that the very presence of the scientist changes, and helps construct the field being researched. Therefore the action researcher has to make explicit choices about the nature of the society sought, and the overall policy goals which action research is seeking to achieve.

Insofar as there is a parallel with more recent work on action research, it is with that conducted in Human Relations in 1993, rather than the more recent Management Learning. The former was more critically distinctive, not least because the initial orthodoxy was challenged by others who went back to first principles, not least

questions about the extent to which researchers change and create the reality in which they intervene, and use fundamentally shaky claims to science to protect their professional position. (Bartunek 1993, Mangham 1993, Ledford and Mohrman 1993) Again, space permitting, these parallels could be drawn out in detail; but the key point to make, again, is the very absence of any mention of Collier or his contribution therein in this debate.

What this indicates is that Lippitt's philosophy prevailed. It is certainly the case that action research, in the First World at least, did not develop along the lines Collier proposed, in that the action emanating from action researched continued to be primarily the responsibility of the research, and not societal institutions (Collier's point (c) in the prospectus.) More, action research became famous, if not infamous (again, see Mangham 1993, Cooke 1999) for its emphasis on process rather than content (hence the generic critique of change management that it focuses on the process of change to the exclusion of its context). There is some (ignored) evidence that Lewin himself was sensitive to this danger. In *Action Research and Minority Problems* he wrote:

“...let us examine the way... intergroup relations are handled. I cannot help feeling that the person returning from a successful goodwill meeting is like the captain of a boat who somehow feels that his ship steers too much to the right and therefore has turned the steering wheel sharply to the left. Certain signals assure him that the rudder has followed the mover of the steering wheel.

Happily he goes to dinner. In the meantime of course, the boat moves in circles. In the field of intergroup relations all too frequently action is based on observations made “within the boat” and all too seldom based on objective criteria regarding to the relations of the movement of the boat to the objective to be reached (1945: 38).

But by the mid 1950s this was not the view which had prevailed. Lippitt’s arguments can be seen to have prepared the ground for action research’s shift away from the early focus on social change to intra-organizational and workplace from the early 1950s onwards identified by Elden and Chisholm (1993). Process came to be all, and in *The Dynamics of Planned Change* Lippitt et al’s position on the change agents value judgements, and how they inform practice was merely that the “Judeo Christian democratic ethic provides general prescriptions which can guide the activities of an agent” (1954:98).

## CONCLUSION

The conclusion does not lead where the preceding analysis might suggest, however. In this all too brief consideration, it should be recognised that Lippitt was unequivocally correct in one thing. There were strategic and tactical advantages in adopting scientific neutrality rather than seeking social change. Interweaved in the correspondence are mentioned the IEAs financial precariousness, and Collier’s hope that this would be resolved by the Internal Revenue awarding tax exempt status.



This did not happen. This was according to Collier's biography, because the IEA criticism of US Navy policy to overseas holdings had offended then Navy Secretary Forrestal who determined to shut the IEA down. This he did, according to Collier, by persuading the Internal Revenue to withhold tax exemption. On appeal the finding was that it was "oriented to *action-research* [Collier's emphasis]. Research which involved action was by definition (Internal Revenue's definition) political and the tax privilege must be denied" (Collier 1963:33). It is ironic, then, that the Revenue agreed more with Collier than did Lippitt, and on Colliers own terms was correct to do so (which is not to deny Colliers claims of victimization). Lippitt's claims for science, and his (at risk of caricature) removal of planned change/change agency from the political arena did arguably, in the era of McCarthyism and the Cold War provide space for action research to flourish conceptually and in practice.

What this paper has provided is an understanding of the history of one particular, but important social and behavioral science idea. Revealing the history of ideas per se is an important academic endeavor, moving forward our knowledge of the development of knowledge, so to speak. This case is made by Hill (1993), who also points out the benefits of archival research as bringing more rigor to historical understandings of the development of disciplines, which often occur through the codification of oral histories and anecdote, with little attempt at verification. It is hoped that this paper, along with any intrinsic merits it may have, supports this case. Hill also points out that that archives themselves produce only partial understandings, and that too should be noted here. Not least, for example, it may be

there are substantial parts of the correspondence which did not find their way into the archive. There is certainly one piece missing, Collier's note to Lippitt and accompanying poem on Lewin's death.

Finally, the sting in the tail for those, like this author, initially sympathizing more with Collier's than Lippitt's position. It is one thing to say the action-researcher must take sides; it is another to get that side right. Collier's social and political aims were actually problematic. Not least, his approach to decolonization, and perhaps to the development of action research, was heavily influenced by the British Colonial administrative policy of indirect rule, which gave only a limited amount of autonomy (Cooke 2000, Hauptmann 1986). This suggests more problems with action research per se, and of course further makes the case for a full exposition of Collier's involvement with action research.

## REFERENCES

- Bartunek, J.M. (1993). Scholarly dialogues and participatory action research. *Human Relations*, 46 (10), 1221-1334.
- Collier, J. 1945. United States Indian Administration as a Laboratory of Ethnic Relations. *Social Research*, 12, May: 265 -303.
- Collier, J. 1963. *From Every Zenith*. Denver: Sage.
- Cooke, B. 1999. Writing the Left Out of Management Theory: The Historiography of the Management of Change. *Organization*, 6 (1): 81-105.
- Cooke, B. and Kothari, U. 2001. *Participation, the New Tyranny?*, London: Zed Books.
- Cooke B. 2001. *From Colonial Administration to Development Management*, presented at US Academy of Management, Washington DC, August
- Elden M. and Chisholm R.F. (1993) Emerging Varieties of Action Research: Introduction to the Special Issue. *Human Relations*, 46(20), 121 - 141
- French, W.L. and Bell, C.H. 1998. *Organization Development – Behavioral Science Interventions for Organizational Improvement*. Englewood Cliffs: Prentice Hall.
- Hauptmann, L.M. 1986. Africa View: John Collier, The British Colonial Service and American Indian Policy, 1933-45. *The Historian*, 48 (3): 359-374.
- Hill, M.R. (1993) *Archival Strategies and Techniques*, Newbury Park: Sage
- Institute of Ethnic Affairs (1945) *The Institute of Ethnic Affairs, Inc*, Washington D.C. : Insitute of Ethnic Affairs.
- Jenkins K. (1991) *Rethinking History*. London: Routledge.
- Kelly L.C. (1993) *The Assault on Assimilation*. Albuquerque: University of New Mexico Press
- Ledford G.E. and Mohrman S.A. (1993) Looking backward and forward at action research. *Human Relations*, 46 (11) 134 - 145
- Lewin, K. 1946. Action research & minority problems. *Journal of Social Issues*, 2, 4: 34 - 46.
- Lippitt, R. 1949 *Training in Community Relations*. New York: Harper.

- Lippitt, R. and Lippitt, G. 1978. *The Consulting Process in Action*. La Jolla: University Associates.
- Lippitt, R. Watson, J. and Westley, B. 1958. *The Dynamics of Planned Change*. New York: Harcourt Brace.
- Mangham, I.L. (1993) Conspiracies of silence? Some critical comments on the action research special issue, *Human Relations*, 46 (10)
- Marrow A. (1969) *The Practical Theorist*. New York: Basic Books.
- Parman D.L. (1976) *The Navajos and the New Deal*. New Haven Ct: Yale
- Philp, K.R. 1977. *John Collier's Crusade for Indian Reform, 1920 – 1954*. Tucson: Universtiy of Arizona Press.
- Raelin, J. (1999) Preface to the special edition, *Management Learning*, 30 (2) 115- 125
- Reason P. and Bradbury H. 2001a. Preface, in P. Reason and H. Bradbury (Eds) *The Action Research Handbook*. London: Sage
- Reason, P. and Bradbury H. 2001b. (Eds) *The Action Research Handbook*. London: Sage.
- Schein, E.H. 1980. *Organizational Psychology*. Englewood Cliffs: Prentice Hall.

## **THE JOHN COLLIER – RONALD LIPPITT CORRESPONDENCE ON ACTION RESEARCH 1948- 1950**

**Edited by Bill Cooke**

### **Aide Memoire in Colliers file, dated: December 19, 1948**

The letter which follows is in reference to one from a “charter member” of the IEA. His letter, we are not free to reproduce. It questioned whether a social scientist could rightly continue as a member of an organization which professed, and acted upon, “content goals”, i.e. which was moved by, and which moved towards, public policy objections. The [illegible] of the [illegible] valued member’s letter can be inferred from the letter sent in reply. The correspondence relates to one of the central occupations and latent controversies and conflicts of present social science. The minor part, it considers the record and the purposes of IEA.

### **Lippitt to Collier, December 8, 1948 (a)**

When I re-read the enclosed letter to you I decided to tear it up as sounding too critical, rather than expressing the state of puzzlement which is a more accurate description. But as long as it is clear that I do not feel personally critical in any way, I think the note does express my questions as to what direction the Institute is taking, and my own feeling of non-participation in that direction....

### **Lippitt to Collier, December 8, 1948 (b)**

I was very sorry to hear, from your letter of December 1, about the financial difficulties of the Institute. Certainly you have been carrying on an active program of publication and communication toward influential sources of action. I am sorry to say that I have been unable to find much in these activities which resembles what I had understood and hoped the Institute would become. Our own limited program of research and consultation during the past two years, and currently, makes it clearer than ever to me that there is a very widespread readiness for collaboration in

action research formulated and executed with high scientific standards. We are forced to turn down 90% of such requests because of our problems of securing personnel to do a high level job. Financing does not seem to be a problem. At the moment, we are most enthusiastic about the request from the United Nations to explore the possibilities of collaborating on research analysis of committee procedures, with members of secretariat staffs on the research team. The staff, led by Stuart Cook at the Commission on Community Interrelations, has made a great many methodological advances in action research techniques while they have been making major action contributions at the same time. Our training program for action research teams in the summer laboratory of the National Training Laboratory in Group Development is meeting with a most enthusiastic response from all types of agencies and organizations. We are hoping for more social scientists from abroad this summer than last summer.

Perhaps my difficulty is that my definition of action-research is somewhat different from your own conception. Or perhaps I was quite incorrect in my original understanding of the statement of purpose of the organization. I do wish I could get a more adequate understanding of the situation.

...I would like to tender my resignation from the Board of Directors. It is certainly not fair for me to continue on such a non-participant basis....

**Collier to Lippitt, December 19, 1948**

[...] I don't know that our conception of research and action research differs from yours. In my own case, in a social research seminar which I am leading, I use Kurt Lewin and Group Dynamics material and ideas more than any source.

It is rather, I think, that a major purpose of the Institute, as stated from the beginning, was to help in offshore dependency problems, particularly those of the American dependencies but also a worldwide intent. And it still remains a fact - regrettably - that no organization except the Institute is handling the subject of the USA

dependencies, and especially of the Pacific Islands beyond Hawaii. In this last area, while we have done research currently on the ground (the Joseph-Murray Saipan project, not yet published), we have had to rely on numerous other research results (US Commercial Co. and now, Pacific Science Board). The Saipan research, under naval government limitations, could not be of the action type.

Our action-research project for the Near East, formulated with Kurt Lewin, remains adjourned until peace comes over there. Our research into organic acts and administrative technics and goals, conceived thoroughly as action research involving the Islanders and local administrators, awaits both the coming of civil rule and the finding of money: so far, we have only the Navy's offer of transportation and hospitality to and on the Islands, and a pending fellowship grant (Viking) for one worker. Meantime, the Foundation for Research in Ethnic Affairs is incorporated but has not yet been brought alive; its projects of research will all be of the integrative and action research sort.

Unless you disapprove of what we are doing, as distinct from wishing we were doing other and more, I hope you will remain on the Board . . . . The real difficulty, I believe, is absence of face to face contact, and this applies to too many Board members....

**Lippitt to Collier, January 5, 1949**

Thank you very much for your good letter of December 19<sup>th</sup>. I am afraid I wasn't very coherent in indicating to you the reasons why I felt it was necessary for me to resign as one of the Directors of the Institute of Ethnic Affairs in spite of my belief that you are doing a very important piece of work which should be continued with all the financing that can be located.

I tried to indicate to you that the reason for this decision is the clearer and clearer differentiation which I have been forced to make between citizen activities toward political goals and social scientist's activities toward social science values as I

perceive them. This problem has become more and more apparent to me during the period that it has also become clearer to me that the Institute of Ethnic Affairs is not primarily a social research organization but a group having specific "content goals" concerning American policy in regard to the American dependencies. I am completely in sympathy with these goals, but I have come to question my right to identify with them. I indicated above the logical distinction between my role as a citizen and my role as a social scientist... As a result of some stimulating conversations with A.T.M. Wilson of Tavistock and other colleagues here in this country, I have come to question the possibility of maintaining this as a psychological and social differentiation, logical though it may be. I have pretty much decided that in order to carry on most effectively my political and scientific activities as a social scientist, I must try to build this role of a scientist as the "socially visible role" as well as increasing its psychological weight. As a consequence I must inhibit the attainment of visibility in a citizen role dedicated to the attainment of a wide variety of "content goals".

What do I mean then by political activities as a social scientist? As I see it, my major public or political goal as a social scientist must be to "lobby" in every way possible for the application of scientific methodology in the solution of the problems of human affairs and human relations. Universal sensitivity to and participation in scientific activity and the "experimental outlook" seems to be the surest approach to the distribution of a genuine democratic power.

If I identify with the content goals of the United Auto Workers with whom we are now working, then I tend to lose the possibility of furthering this goal of scientific skills and outlook with the American Telegraph and Telephone Company or the Ford Motor Company, and visa versa. But to the extent that I can clearly define my role as that of the methodological collaborator or consultant, oriented to the job of helping them find out the consequences about their own goals and their own ways of locomoting toward these goals then I find I can work effectively with these and many other groups which are even more incompatible in their "content goals".



Because I am most vitally interested in the application of scientific methodology to the area of conflict and collaboration between groups, I of course have an interest in stimulating various strategic groups to apply scientific methodology to the examination of their relationships with other groups as well as the analysis to their own functioning. The role of the "methodological middle-man" seems to be absolutely essential in the stimulation of adequately objective approaches to such inter-group examination of relationships and behavior. And such a middle-man must be perceived as neutral as concerns a range of specific group goals of each of the groups concerned.

Perhaps at this point you are inclined to question my assumption about the impossibility of maintaining a clear and visible distinction between my social scientist role and my citizen role. I have collected quite a number of anecdotal observations which are indications to me of the loss of effectiveness of various social scientist colleagues of mine because of what I have now come to regard as their impulsive readiness to allow themselves to achieve visibility as citizens. This loss of effectiveness is primarily of course in the area of working as a scientist with citizen groups. I do not have any observations about loss of effectiveness as academic teachers or as supervisors of graduate student theses. I am also inclined to believe that this is primarily a problem of the social scientist working with groups on problems of social environment as contrasted to the physical scientist working on problems of physical environment.

If you think this general outlook is screwy, I would like very much to review your thinking about it before you take my name off the list of Directors. As you can see, I am inclined to press my letter of resignation.

I am of course very interested in the whole idea of the Foundation for Research in Ethnic Affairs. I shall be glad to contribute whatever help I can through a distance to that activity when it becomes vitalized. I certainly hope it may be possible for us to

collaborate with you in the training of an action research team through our summer laboratory. If you have any thoughts about a possible team this summer, please let us know as soon as possible because the program of the past two summers seems to have resulted in quite a flood of applications for next summer.

I quite agree with you about the big problem of a policy board trying to operate when geographically spread out. Even active correspondence on policy decisions (such as we have in SPSSI) is not enough. I belong to one group which has done a fairly adequate job by diminishing the restraining forces to the use of the telephone. It would be an interesting experiment to see what an executive secretary could do with an adequate organizational budget to hold telephone conferences with his board when he needed them.

**Collier to Lippitt, February 4, 1949**

I am tardy in replying to your January 5 letter: not only because of trying to work in 8 or 9 successive places of residence in 4 weeks, but because the subject of your letter is profoundly interesting – important, I believe – and cannot be discussed (usefully) in few words. I have, in fact, wanted to re-think the question. I have not finished trying to re-think it, although my own position is implicitly clear.

What follows goes beyond the issues directly posed by your own trend of thinking and choice as formulated in your letter. It goes to the question which hovers about and stirs within all social science now, of whether “value” and purposiveness do or do not functionally belong within social science. Science, the knowing process, John Stuart Mill’s “Logic” concludes, is in the indicative mood; art, the doing process, is in the imperative mood. It has been held to follow, that the scientist, including the social scientist, is obligated to be, and entitled to be, an indifferentist. Clearly, Mill was within the Cartesian century. The historical necessity of that century is clear. The necessity was both sociological and intellectual. Its monition remains permanently needful, wherever testing or verification is pursued. But the Cartesian century is a good many decades past its end, now, intellectually speaking, and can be

as much an inhibitor of new discovery as Thomism came to be; and sociologically, its holdover in the momentum of institutions, and of philosophical pre-suppositions which are evermore re-verified by the actions of the market economy, are hag-riding a large part of the world, and are extremely handicapping the effort to keep and to replace in the changed world those very values of freedom and disinterestedness which Locke and Descartes helped to establish. (Karl Polanyi is one of those who have most lucidly stated the above proposition).

Surely, we can all now see the thing that some deep thinkers in all ages have seen, and that field and subliminal psychology and, now biology, make plain: that knowing process is acting process, that perceiver and field partly make each other, that discovery is a creative, not only a passive operation, that the whole “apperceptive endowment” is needfully involved in the identification of problems and the feeling-out and meditative effort (unconscious as well as conscious) which leads to hypotheses, and even, and perhaps hardly less, in the invention of tecnic of verification. (Who more greatly than Kurt Lewin shows these factors?). We are coming, in social science, to an additional realization, making clear again that the *whole man* is the productive social researcher: to wit, that the feeling-out, the tracing, and the persuasively and courageous statement of the implications of research findings is the way that the findings are brought into world meaning, the way that values generically emerge from scientific findings, and one of the ways that social science delivers its weight to the world.

Some additional realizations are emerging fast, among them these: That social science has potentialities (of good and ill) as great for human life as biology or physics; that there are interests and power groups which are not going to be deterred from their limited, their sometimes explicitly anti-social objectives; that toward their purposes they will use physics, biology, social science, if the scientists are willing to help them. This consideration by itself, apart from the more generically compelling facts stated in the preceding paragraph, confront all scientists, but uniquely the social scientist, with the question: are you going to take civic, citizen, moral, human

responsibility for choosing whether, in the face of conditions not hypothetical, you shall contribute, perhaps decisively, to the ruin of the world? The question then shifts to the positive side: Are you going to choose to devote your limited resources to the making (and, through action research, to the implanting) of social discovery, for and within those groups, those enterprises – those “causes”, if one will – which are committed and through new social discovery, may be made more irrevocably and potently committed to the saving and making of the world? The choice may be one decided purely from within the social-scientific data if the implications of findings are boldly and creatively spelled out (see last sentence of above paragraph, starting line 2, page 2). But the choice may have to be made in the absence of such spelled-out implications: Soviet scientists have had to make it thus, and we here may have to make it simply on the basis of our inherited ethics and values and ideals and sense of the real.

One more, partly disparate, consideration. Time is running out. The programming of social research – first things first – is imperative. It is not to be taken for granted – rather, the opposite is well known – that governments, diplomats, military officials, businesses, political parties, church organizations, pressure groups, functional associations, philanthropic foundations, etc. etc., are going to attempt to do this imperative programming in terms of world requirement and possible wealth of most universally usable research-yield. In your letter you speak of the social scientist’s duty to propagandise for social science. Yes, but a duty more uniquely his own, is to propagandise for, to sacrifice for, and to direct his discovering activity toward this urgent world-programming of social science. This includes planned, cooperative endeavour by social scientists to establish the discovering process in areas of the human and man-nature problem which are critical areas of maximum potential social and world influence. Here, once again, valuing, citizenship, unbeatable social purpose, must be conjoined within the scientific functioning.

(An item in the neighbourhood of some of the above, but especially of paras. 2 and 3 of this letter. I chanced today to come upon Chapter 3 of Havelock Ellis’ “The Dance

of Life". It is the chapter on "The Art of Thinking". It is not only graceful and rich but surprisingly *au courant*.)

Not all of the above applies to the suggestions contained in your letter: yet perhaps it does apply. To elect to have no "socially visible role" other than that of an indifferentist social scientist plus a visible advocacy of the use of social science, would seem - if one really does such a thing - to involve abandoning most of the affairs and the sources of significant apperception which are of this world. Socially invisible roles become roles non-existent or only sterilely alive inwardly, for we live through communicating, we can act only with others, thought immured from action lies down and dies. But perhaps most relevant: thus much of socially invisible - rather, thus little of socially visible, therefore of actual - role very often (in our present society, usually) will forbid the tracing through and the bold statement of the implications of research findings. It will truncate the research process and ultimately deliver research into the limited-purpose confines of special-interest groups. Among other lines, the veritably immense and profound potentialities of "group dynamics research" cannot be realized within so self-limiting a role, I believe.

About Ethnic Institute. It has specific "content goals", as you state, although goals much broader than the welfare of the American Dependencies. (Its equal concern is with the world dependencies, with all of Chapters 11 and 12 of the UN Charter, and it finances and operates the USA branch of the Interamerican Indian Institute. Much else beside, but see the News Letter etc.)

But what your letter omits, is that these "content goals" derive from the results of research deep and broad, while also explicitly moving toward further research. I refer to research by those who formed the Institute research, whose generalized findings caused them to organize the Institute. (Among other research-sources of the "content program" I mention only a few: The twenty-years' continued research, involving numerous disciplines, into Indian life, and Indian administration, experimentally conducted as a special case of cross-cultural and dependency

administration. The studies of sub-professional health work in Oceania, and of native medicine and its present and future used among the Navajos, and the sub-professional health enterprise of Nicaragua. The analytic and experimental studies of bi-lingual education, mass literacy etc. The social analysis experience in WRA. Kurt Lewin's work, and yours. The Spanish colonial record, and the successes and shortcomings of the post-revolutionary ejidal enterprise of Mexico. Exhaustive research into the Guam record, including the record and present of the Naval government there. These examples, preceding, but supplying the research foundation for the "content program", could be multiplied.) Subsequently, in treating of all the American dependencies, the Institute has used all source material, the interview method, psychiatric and psychological research "on the ground" in Micronesia; its representative soon will depart for a re-survey of all the Pacific Islands, the Navy being our host. The research purpose, and concepts of research methods etc., would be more conspicuously evident had the Institute more money. We hope that with tax-exemption of the Ethnic Research Foundation, we *shall* have more money.

This turns out to be a very long letter; as remarked at its beginning, your letter caused me to try to re-think the whole subject, and I haven't finished trying yet. You realize, I know, how high a value I place on your own work. Consider this letter as not really addressed to you, but to the numerous social scientists whose thinking upon the subject-matter of our letters seems to have moved in the same direction as yours . . . . In Washington the coming Monday, back in N.Y. for good by the 10<sup>th</sup>.

Every wish, as ever,

**Lippitt to Collier, February 23, 1949**

Dear John

Many thanks for your helpful contribution of February 4 to the problem we are both trying to solve – the proper role of social science and the social scientist in the solution of the "action problems" of human affairs. I certainly cherish your

collaboration in helping me to clarify my rusty efforts at thinking about the problem. Some of what follows is new territory into which you have pushed me, and some is an effort to communicate more clearly certain notions that I evidently did not say very well in my last letter.

The first of these misunderstandings is your identification of my self-defined role as that of the “indifferentist” – the scientist who splits *knowing* from *acting*. Certainly there are many thoughtful scientists today who maintain that wholehearted devotion to “increasing our stockpile of knowledge” is the only appropriate role for the scientist – *as a scientist*. This type of scientist of course is free to take a variety of active citizen roles, as is the banker or the shoemaker. I agree with you that in our field (1) the “knower” cuts himself off from much of the field of data that must be known if he tries to split knowing and action as independent realms; (2) and also the need for everyone to contribute to social problem solving is too urgent for us to reject this responsibility.

What we are faced with is the problem of making the most effective contribution we can. At this point, I think the two of us begin to analyze the situation somewhat differently.

You stress that the urgent need is for the productive social researcher to “- - - - persuasively and courageously state the implications of the research findings (so) that the findings are brought into world meaning - - - -”.

I stress the notion that the social scientist is in most cases quite ill-prepared to make direct interpretations of his findings to *new* problem situations, partly because of the state of development of the basic science, but more because every important decision situation in human affairs demands its own unique problem solving effort, its own separate diagnosis of the relevant variables and hypotheses about probable consequences. Therefore I arrive at the conclusion that the greatest social need is for the widespread application of sound problem solving methodology, which we

scientists are want to call scientific method. It seems compellingly clear to me that whatever relevant nuggets of knowledge social science has turned up will only be utilized appropriately within the framework of intelligent problem solving processes being carried out in regard to each specific decision and action of each unit of society. Until the scientist can get over his godlike role of “shouting interpretations and implications through a distance” he is doomed to be rejected, and should be rejected as a appropriate resource person in the problem solving processes of a democratic culture.

If (this is an assumption I’m making) this problem solving process is the application of the principles of scientific methodology to the continuous selection and assessment of goals and means, values and tools, then this is the crucial social situation that calls for the collaboration of those social researchers who want to extend their role most appropriately beyond the other very important role of scientific stock-piling of knowledge which we assume will acquire greater or lesser relevance for the solution of certain action problems at some time – distant or soon.

As you point out so cogently there are a number of difficult problems which I run into in attempting to test this extension of the scientist role as my “scientist-citizen role”.

1. What about my value system concerning certain goals for action in this society which I have come to think are good or bad? Is this problem solving methodology amoral? Will I accept the opportunity to give, or sell, my services to any group?
2. What about my notions about democratic process as well as goals? How do these relate scientific methodology as process?
3. Are the skills I have acquired as a research scientist really the appropriate skills for this job or training others in a research approach to



problem solving, or are there other functionaries who would and can do a better job?

As I try to start the examination of these questions I find I must first check myself on where I stand on the question of social change. In the position I am taking is there any basis for saying that there should be change? Or what kind of change it should be?

It seems clear to me that social change is required by the nature of events. The field of relations or forces to which adjustment must be made is constantly changing – so the formulating of new goals and means is a continuous requirement if our behaviour is to be oriented to the realities of the changing environment (which to quite an extent we are creating). Change is certainly neither good nor bad – it is just a fact.

But the process by which we go about changing – that is something I have a value system about in the position I'm trying to explore and define for myself. Explicitly my value judgment or assumption is that the application of the principles of scientific methodology define the best process of changing our human relations toward more appropriate patterns. (I want to acknowledge my indebtedness here and at many other points to Kenneth Benne, Professor of Social Philosophy at the University of Illinois for clarifying this general point for me in a manuscript which he has just completed).

But does this give me any satisfying and stable end values for my behavior? I think so. The end value is that there be a maximum utilization of scientific problem solving procedures in dealing with all problems of human endeavor – goal setting and unsetting, means setting and changing, etc. This implies that I have arrived at the notion for myself that what we customarily call the democratic way of life is an attempt to formulate the group conditions in which scientific problem solving can emerge and develop.

Let me test this notion with an example. Obviously a lot more work must be done to push to a real test.

It seems to me that one criterion of scientific method is that data about a state of affairs being analysed be derived from objective measurement of that situation rather than from projections of the wishes and hopes of the scientist who is attempting to arrive at knowledge and interpretations about that state of affairs.

If we ask what implications this methodological value or norm has for the actual the actual problem solving operation of a person or group which accepts this value, I come up with such derivations as the following:

1. The group must establish and maintain the type of inter-member communication that will provide maximum flow of data for decision and action at any time.
2. The group must resist stratification along influence dimensions based on emotional and prestige factors which are not task-oriented (status weight in opinion influencing should be proportional to contribution-of-data weight, also manipulation of data skill at a later stage, etc.).
3. The group must so constitute itself and organize its effort that it can and does seek and receive intelligence from its environment relevant to the decisions it is making and action it is taking.

etc. (we could go on)

Let's look at some implications of this criterion at the level of individual functioning rather than group functioning:

1. The individual must recognize continuously the psychological fact of emotional involvement in activities and ideas as both an asset and a liability – as a source of motivation and a source of unintelligent resistance – and must practice skill in inhibiting his tendencies to defend and promote ideas which need objective evaluation and reformulation.
2. The person must achieve sensitivity in assessing the sources of influence on himself, to differentiate between depending on status figures and dependence on fact-oriented influences.

These then would seem to be a few of the skills I would promote in the interests of this particular methodological value, as a scientific methodologist working with this group.

Each element of scientific method which one selects for this type of analysis – regarding it as the value to be achieved (to be fought for) seems to lead directly to statements about personal functioning and group functioning which seem to me to be the operational definition of democracy.

Now I come to the question of whether I'm ready to work with any group in our society in the role of scientific methodologist? My answer at the moment is Yes – and No. Yes – as far as my personal value reaction to their goals of the moment is concerned. But No –

1. If my efforts to create a need for my participation with them as a methodologist is a failure; if there is a lack of sensitivity to the problem of need for help on methodology of problem solving.
2. If the spread potential of my effort (within the group or outward from the group) is very low compared to other situations and there is opportunity for choice.

3. If, after a tryout, I find unyielding resistance to the application of these methodological principles in the examination of the values or goals of the group in its relation with its environment – limiting its efforts only to some minor aspects of internal functioning. (This I think is a crucial point in regards to your remarks about the misuse of science).

4. If my working relationship with this group would be inevitably perceived (in spite of the best I could do) as identifying me with the goals of this group in the eyes of a large proportion of the groups I would hope to have access to.

This last point brings up a rather important strategic issue I think, one which you point to at some length in your letter. At the present moment I think there are a number of reasons why it is psychologically difficult or strategically unwise to identify oneself with the objectives of a particular group or organization in which one is functioning as a scientific methodologist.

1. First of all, many of the groups which are most in need of help do have, or perceive themselves as having, incompatible interests – so there is intergroup tension. The methodological consultant is in a position to work with both groups – in fact very often to serve as communication middleman in problem solving of intergroup tensions – if he restrains carefully from any public identification with the goals of any of the groups.

2. In his work with a particular group the scientific methodologist (as also the therapist) begins to run into a number of difficulties if he begins to be perceived as “one of us” rather than as actively and sympathetically interested in “how we do things we are trying to do”.

3. A third reason has to do with the strategic contribution of energy of the scientist in working with groups in this role. If the nature of the objectives of the group is used as the basis for his selection, it is highly probable that much effort will be wasted, because the potentialities for change must be painstakingly diagnosed in terms of readiness or resistance to the exploration of scientific problem solving procedures – as well as in terms of strategic position in society in relation to the overall dynamics of change.

As far as I can see so far this self-imposed restriction for strategic purposes on becoming socially visible as actively identified with the objectives of various action groups does not inhibit the responsibility for intensive personal analysis of all these objectives and inevitably arriving at private value judgements. I think we must know how we feel in order to do a disciplined job of controlling our role as a methodological therapist. [illegible] certain this line of analysis opens up whole new areas for vigorous socially visible political activity and decrying unintelligent problem solving procedures in establishing the goals and means of all types of groups in our society.

Let me return now to the final question I posed – is this a job for a social researcher? My answer is yes – this is basically a job of training in social research methods, usually by close collaboration and consultation rather than in any formal “teacher role”. Although the specific requirements of this scientist consultant role call for broader skills in a number of ways that those we need in order to personally produce good research – I think the categories of skill are the same – the creating of readiness, the training of other in the objective observer role, the design for hypothesis testing, methodology of sampling, etc. All of these are major skill requirements of the scientist-citizen as problem solving methodologist.

I guess I have shot my bolt for this time. I don't feel anywhere near as certain as I may have sounded. I feel you've pushed me to considerable progress in formulating

some of the aspects of this particular position so that we can examine it a bit more effectively. I'm eager for your next share of this conversation.

**Collier to Lippitt, April 27, 1949**

This is a belated follow-up of our earlier correspondence on role, value, purpose in social science. The remarks will be fragmentary . . . a rounded-out discussion would be lengthy indeed.

1. The scope of social research and social science. It includes inter-personal relations and intra- and inter-group dynamics but includes much beside. Economics, for example, the man-nature relationship; population problems; organization of industry, of government etc.; administration. Etc. True, that most social action is mediated or implemented through inter-personal relations and group dynamics, but so, equally, it is mediated through muscle action (at one extreme), semantics (at another). As soon as the breadth of scope of social science is held in mind (for example, in terms of resources exhaustion, of human hygiene, of housing, of the assembly-line, of fascism and democracy), it seems to become plain that the social scientist must be possessed of value and purpose, whether or not he chooses a socially visible or a socially less visible role. I mean, not just as a man but as a scientist; for otherwise there will be no assurance that he will choose problems of critical importance to work on or that he *will not* wind up by becoming simply a technician for power groups, ideological groups, etc.

Within group dynamics, I would agree that there is some little of room for usefulness and for discovery, in the absence or suppression of any purpose beyond that of increasing the experience of reasonableness among men. Yet even there, I recollect Kurt Lewin's remark at the end of his discussion of your democratic-authoritarian experiment. Reasonableness is a situational response, he suggests. Achieve the democratic situation and you achieve reasonableness. Within the authoritarian situation you can get all sorts of

other results, but not reasonableness. Thus, the social scientist, though he may seek only to increase reasonableness, has to have the value and purpose of aiming at comprehensive and deep democracy and aiming against (say) fascism overt and covert. He must make choices in terms of both worldwide and domestic and intimate. And those choices often will have to eventuate in overt socially purposive roles. No escaping it, that I can see.

2. The matter (see my Feb 4 letter, p.2, and your Feb 23 letter p.1) of: "To wit, that feeling out, tracing, and persuasively and courageously stating the implications of research findings, is the way that findings are brought into world meaning, the way that values generically emerge from research findings" etc. This does not mean, as your letter puts it, that the scientist in a "godlike role" would be "shouting interpretations and implications through a distance", although it *might*, sometimes, mean that the scientist would be "doomed to be rejected".

The proposition can be stated in two ways or at two levels. First, *methodological*. It is the scientist's job not merely to make particular discoveries but to generalize them into hypotheses covering wider fields of fact, which hypotheses he or someone else proceeds to test. This proposition is contained in all the textbooks and illustrated by the whole history of science. Second, if one will, *political or ethical*. The nuclear scientists were entirely capable, in advance of making the atom bomb, of spelling out to themselves and to the world the consequences [rest of sentence illegible]. However, it is multiplied in such instances as the insecticides, the consequences of engineering dependence on big downstream dams, the consequences of public health work in (say) Puerto Rico, the consequences of the guided missile. All these are negative examples. The way that Wm. Ja [?] backed Thomas Beers in his mental hygiene crusading is an example on the positive side; the way that Hugh Bennet spelled out into economic and social terms the implications of his findings into soil erosion and methods of conservation, is a positive

example. In this second, or ethical aspect, I'm not suggesting that it is the obligation of every scientist to go beyond his intellectual competence or to stand himself up to be shot at in tracing the indirect consequences of his invention or discovery and drawing out its implications for the social whole: I only am suggesting that much of such tracing-out is in fact within the scientist's intellectual competence, and that often he can enrich human thought and sometimes he is categorically obligated to such tracing out. And a research becomes more richly of the inter-disciplinary and integrative type, *the research team* will possess great intellectual competence for the tracing out.

3. A generalized quote from Kurt Lewin, in conclusion. From *Action Research and Minority Problems*, Nov 1946. "Unfortunately there is nothing in social laws or social research which will force the practitioner toward the good. Science gives more freedom and power to both the doctor and the murderer, to democracy and fascism. The social scientist should recognize his responsibility also in respect to this.

**Lippitt to Betty Cooper, IEA, cc John Collier December 8, 1949**

Thank you for your letter of November 28 calling my attention to the expiration of my membership in the Institute of Ethnic Affairs this month. About a year ago, I wrote to John Collier asking that my resignation from the Board of Directors of the Institute be accepted. I indicated at that time the reasons why I felt it was necessary for me to resign from the Board of Directors, although I was in active sympathy with the objectives and activities of the Institute and had planned to continue my membership. As far as I can see from the letterhead, no action has been taken on my resignation, so the only recourse I seem to have is to allow my membership in the Institute to expire in order to make my resignation effective.

I certainly think you are doing a fine piece of work and I wish you every success in the gruelling work of the Institute.



**Cooper to Lippitt, February 17, 1950**

... I think Mr Collier hoped that you would change your mind about resigning from the Institute Board and consequently never formally accepted your resignation. Then at the annual meeting in October, 1949, the membership indicated its desire to include you among the Institute directorate. That is why your name continued to appear on the letterhead.

Now, before initiating a large membership drive in March, in which names of Board members will be printed on promotion pamphlets and letterheads, I thought it best to ascertain your current wishes in the matter of Board membership. Your consent to serve would be most welcome, but at the same time I do not want to overlook entirely the desires you have previously expressed.

P.S. I would be interested in seeing any current publications of the Research Center.

**Lippitt to Cooper, March 13 1950**

Thank you for your letter of February 17. I would appreciate very much your removing my name from the membership of the Board. I am afraid I am rather compulsive about having my name related to activities where I cannot be active in the affairs of the group.

I am glad to enclose a copy of the current Research Center bibliography as you request. Please give my very best greetings to John Collier when you see him next. I would certainly relish an opportunity for some good conversations with him to hear how the affairs of the Institute are developing.

Enclosure