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ABSTRACT

The long-forgotten debate of 1929-31 between T. L. Kelley and W. H. Kilpatrick is reviewed with the aim of reviving the crucial, but dormant, issue of the competence of quantitative empirical research to answer educational questions. They debated the need to supplement scientific method with a philosophical approach sensitive to needs, impulses, and interests. First, the causes and objectives of the debate are placed in historical and intellectual perspective by describing the wave of technological optimism that occurred about 1890-1920. Then, the principal points made by the debaters are reviewed and examined. It is concluded that Kilpatrick won the debate by making a strong case for the complementarity of scientific and philosophical methods in education. But, it can be granted to Kelley that educational analysis and theorizing has often seemed undisciplined and concerned more with displaying cleverness or doctrinal purity than with getting things right. Today, the situation of the late 1920s still exists: a narrow positivist orthodoxy among influential methodologists coupled with dissatisfaction on the part of others about triviality, irrelevance, and inconclusiveness of research using the scientific method. It is suggested that it is possible for research to be both rigorous and relevant; thus, the issue should be reopened. (Author/ND)

SCIENCE VS. PHILOSOPHY IN EDUCATION: The Kelley-Kilpatrick Debate of 1929-31 as a Moment in the Development of Educational Analysis

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SCIENCE VS. PHILOSOPHY IN EDUCATION: The Kelley-Kilpatrick Debate of 1929-31 as a Moment in the Development of Educational Analysis[1]

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> If scientism is right, philosophy itself is an ideological relic that no longer has any raison d'etre and from which we must be liberated.
>
> But if, on the other hand, philosophy really tells us something about the nature of man, then every attempt to destroy it necessarily obstructs the understanding of human reality. In this case, the human sciences will have to be philosophical in order to be scientific.
>
> --Lucien Goldmann[2]

Since I sympathize, in a way, with the old lady who refused to enroll in a history course because, as she said, she had decided to let bygones be bygones, I should make clear at the outset that I am here unconcerned with the past qua past. It is rather the living past of the present and future that I intend to scout as a way of approaching a perennial problem of fundamental importance to the educational theorist: that of the relation of empirical-knowledge of the kind resulting from behavioral and quantitative research--such as scores on intelligence tests--to the practices of teachers and the policies of educational institutions.

This paper began as an incidental fragment of an ambitious project aimed at writing a preface to a post-positivistic philosophy of educational science--one having no dogmatic prior commitments to "scientism" (to be characterized below), reductionism, determinism, empiricism, positivism, nor the sovereignty of ordinary language; nor to their negations or opposites. To avoid prejudging issues, let us define educational science as all the systematic knowledge we have

about educational matters, and--as process--every inquiry undertaken with the aim of obtaining such knowledge.

I

My central objective is to lay out some issues about the respective roles of science (or "scientific method") and philosophy in educational inquiry that emerged in a running debate of 1929-31 between William Heard Kilpatrick and Truman Lee Kelley. This long-forgotten debate is exhumed because I believe the fundamental issue between them is still very much alive, and still with us in ways I shall try to suggest. One could claim, with much justice, that their positions were confused and that the whole debate was futile and unnecessary; but this would be an unhistorical refusal to take seriously their version of a methodological conflict that, in new guises, still divides educational theorists, and one that I believe will become even more salient in the years ahead, during the gathering crisis of what I call the Galton-Pearson-Fisher Paradigm in educational research.

In my opinion, Kilpatrick was a third-rate thinker, and his large influence on a generation of American educators was a misfortune for the intellectual reputation of educational theory. Despite this generally low estimate, I shall hold that his position in this debate is much the more defensible one, notable both for its intrinsic merits and the remarkable ways in which it anticipates certain themes and theses set forth by anti-positivist historians and philosophers of science in the past fifteen or so years. His writings are replete with the "elastic generalities and resounding platitudes" Max Black once excused us for being impatient with; but the three papers on which I focus are much the best things by Kilpatrick I've read, and are, I believe, still worthy of close attention by educational researchers. I shall suggest that this is so because they incorporate, to a striking degree, but without acknowledgement, certain distinctions

prominent in the non-reductionistic and non-positivistic philosophies of science of Aristotle and Kant.

The Kelley-Kilpatrick debate over method in educational research derives further importance from the fact that it was in a way a debate-by-proxy between Thorndike and Dewey, as represented by two of their most prominent students and disciples. I assume that Dewey and Thorndike were without question the two most important influences on American educational thought in the first half of the twentieth century. (I would not care to take a stand on how much effect either had on practice; I suspect Thorndike had far more than Dewey.) So, some of the larger significance of the debate is that Dewey and Thorndike were, so to speak, standing in the wings behind Kilpatrick and Kelley respectively, and can to some extent be identified with the general tendencies, if not all the details, of their respective positions.

There are two subordinate objectives. One is to show indirectly that the position recently taken by R.H. Ennis on the centrality of causal investigations in educational research is an excessively narrow and truncated view, in that it concedes too much to the Thorndike-Kelley view of educational inquiry as a kind of applied or engineering science. Ennis wrote that " . . . the central thrust of educational research taken as a whole must be toward the establishing of causal statements"; and again, ". . . the general thrust of educational research must be toward the production of causal statements."[3] I do not attack this too-narrow view directly, but hope to undercut it indirectly by arguing that Kilpatrick was largely right on the basic issue and wins the debate on points in arguing that the central thrust of educational research must be elsewhere, in the direction of practical inquiry.

The other subordinate objective is to make a beginning of discrediting and correcting positivist and progressivist accounts of the history of the social and behavioral sciences

These accounts are characterized by the ever-upward-and-onward view that ever greater investments of money, muscle and computer time are the necessary and sufficient conditions of progress in the social sciences, and the associated view that all that is worthwhile in the history of these sciences is what anticipates the theories that are currently accepted as correct. This progressivist or "Whig history of science" has come under heavy attack since the pioneering work of Koyré, Butterfield and Kuhn in the history of the physical sciences.[4] So I am, in part, calling for an analogous new-style history of the policy sciences.

II

Turning now to broader perspectives on the "science vs. philosophy" debate, we can attempt to situate it better historically and intellectually. The immediate background was of course the America of Coolidge and Hoover. The advanced thinkers of the period were heralding the advent of a new "scientific" civilization. Science was thought to be triumphant, in large part because it was equated in the minds of most with technology; while technology in turn was largely equated with machine industry. Atomistic, reductionistic, and mechanistic modes of thought seemed to have all the force and prestige of the triumphs of modern natural science behind them.[5] There was a corresponding attempt to stigmatize as "unscientific," or even "anti-scientific," those who did not acquiesce in, or embrace with sufficient enthusiasm, billiard-ball materialism and Laplacean determinism. The public was encouraged--even by some who should have known better--to think of science as a wonderful new cornucopia of goodies and gadgets, of comforts and conveniences, of marvels of communication and transportation that were transforming human life and civilization. "The machine is the authentically embodied Logos of modern life," wrote Dewey, "and the import of this fact is not diminished by any amount of dislike to [sic] it."[6]

This wave of technological optimism, of enthusiasm for "the machine," carried before it many of the era's most influential educational thinkers. The first self-consciously "scientific" researchers in American education were the cohort born in the 1870's and 1880's. If we assume that they came of age intellectually between their 20th and 30th years, this places their formative period in about 1890-1920: they were of that optimistic and energetic generation before World War I that exhibited such enthusiasm for the "new psychology" and other developments inspired by Darwinism.

They were impatient with the inconclusive debates among historians and philosophers and the "literary" sort of material then being taught to prospective teachers. Symptomatically, when Judd arrived to head Education at the University of Chicago in 1909 he promptly dropped the requirements in the history and principles of education in favor of courses in the psychology and sociology of education.[7] Quantitative facts about occupations and social institutions were to provide the ends of education and shape the curriculum; while descriptive laws summarizing experimental data revealed by the new laboratory science of psychology would dictate the best means and methods of instruction in attaining those ends.

This burst of enthusiasm for the "scientific" study of education in the teens of this century took several forms: (1) the "scientific management" and closely related "social efficiency" movements, which some say are getting a rerun in the 1970's; (2) the interest in "experimental pedagogy," or attempts to establish the superiority of certain instructional methods and materials by direct comparisons; and (3) the mental testing movement, which, after small and hesitant prewar beginnings, became the educational rage of the postwar years. Cronbach has remarked that "the 1920's were a period of almost hysterical enthusiasm for tests in certain circles."[8]

The upshot of all this "science" and testing was the much-bruited "fact" that the average American had the intelligence of a thirteen-year-old: a "scientific" result that

gladdened elitists and social Darwinists and seemed to discredit democracy.[9] Further, if the aims of science were "prediction and control," as positivists had held since Francis Bacon, the next step might be the prediction and control of the behavior of the unintelligent masses by "the best and the brightest": the new class of engineers, technocrats and social scientists.[10]

Against the background of this brave new world of machine industry and mass production one can begin to understand the disillusioned, "existential" reaction of many literary intellectuals of the 1920's--the Theodore Roszaks of 45 years ago--who concluded that there must be something wrong with science if its deliverance was that man was merely a naked ape-shaped mechanism which arose by chance in an unimportant niche of a cold, indifferent, and--for all we knew--otherwise quite desolate universe. "We are the hollow men," wrote T.S. Eliot in 1925, "We are the stuffed men / Leaning together / Headpiece filled with straw. Alas!" In a chapter on "The Disillusion with the Laboratory," J.W. Krutch wrote in his somber book titled The Modern Temper (1929):

> We went to science in search of light, not merely upon the nature of matter, but upon the nature of man as well, and thought that which we have received may be light of a sort, it is not adapted to our eyes and is not anything by which we can see.

Clarence Ayres, in his Science: The False Messiah (1927), pointed out that science was valued as the source of gadgets and conveniences but did not provide a humanly satisfying outlook on life; it was dissolving old folklore and traditional ways of life, he said, without putting anything in their place. Walter Lippmann made similar points in his elegaic and influential Preface to Morals (1929).

In this polarized climate of opinion one finds John Dewey and some like-minded educational thinkers of the late 1920's (Bode, Counts, Kilpatrick, Raup, Rugg) having it both ways: while rhetorically devoted to "science," "scientific method," and the virtue of modern, up-to-date thinking, they

yet demur from the empiricist, positivist and reductionist tendencies in Thorndike, Kelley, and--at one remove--Karl Pearson's Grammar of Science, apparently the main proximate source of the Thorndike-Kelley philosophy of educational science.[11] What was being done in the name of science was distorted and inadequate, they repeatedly suggest; indeed, after more than a quarter-century of aggressively empirical and quantitative research in education, Dewey continued to write as if a proper science of education were still entirely in the future.[12]

Suppes has noted this strangely ambiguous and ambivalent attitude toward theoretical empirical science among the Deweyians:[13]

> Dewey himself . . . continually stood on shifting ground in advocating empirical and innovative attitudes toward teaching. If fact, one does not find in Dewey the emphasis on tough-minded empirical research that one would like, but rather a kind of hortatory expression of conviction in the value of methods of inquiry brought directly to the classroom, and indeed more directly to the classroom than to the scientific study of what was going on in the classroom.
>
> . . .
>
> Historically . . . it is important to recognize that under the influence of Dewey educational leadership moved away from development and testing of theory, and Dewey himself did not properly recognize the importance of deep-running systematic theories.

Several reasons have now been sketched why, in the late 1920's, the question began to loom: what was science? Was "scientific method" adequate to solve the problems of education? Dewey set forth a conception of science so broad and vague as seemingly to equate it with all disciplined inquiry and coherent knowledge; characteristically, he held that the educational and political problems of the age could be solved only when the public was willing to apply to these areas of "social engineering" the same scientific method so strikingly successful in controlling and manipulating the physical world.[14]

Deweyians such as Bode, Raup and Kilpatrick began to say, however, that the difficulty was rather too much science (of the wrong sort): scientific method, as expounded and sometimes practiced by the influential self-styled "scientists" among

the new class of psychometricians (Thorndike, Terman, Kelley), was intrinsically deficient for the purposes of the educator just because, in its quantitative approach and emphasis on "objective" and "accurate" facts, it systematically excluded from the purview of the educational researcher many of the most important considerations in educational theory: the values, norms, convictions, aspirations and ideals of everyone touched by the educational system--including the researcher and theorist themselves.[15] They concluded that scientific method, on the dominant non-Deweyian definition, was limited in its usefulness to the educator, oversold, probably dangerous to equalitarian and democratic ideals, and in need of supplementation by what they tended to call "philosophy." We might choose to call it "ideology." In any case, we shall see that Kilpatrick, in his running debate of 1929-31 with Kelley, looks back beyond Dewey to an older rationalistic viewpoint exemplified in the philosophies of Aristotle and Kant; and he redraws essentially their map of the intellectual disciplines without acknowledging their influence.

III

My method is to gesture toward, and perhaps even give a glimpse of, what can be done for the history of educational analysis by looking at an example of what happens in the intersection of three promising approaches to the historiography of the policy sciences.

A. From the standpoint of the "history of ideas," as practiced by A.O. Lovejoy and G. Boas, we are looking at the "unit-idea" of <u>scientific method</u> in its interrelations with and bearings upon the ideas of <u>science</u>, <u>philosophy</u>, and <u>education</u>.[16] What is "scientific" in educational investigation and what is not? Does what is sound and defensible in the idea of educational science need protection from the overweening "scientism" of some of its friends?

B. I see much value in the new-style, post-positivistic history of science alluded to above (p. 4), which attempts <u>not</u> to ignore or suppress those aspects of the history of science that now appear "unscientific," "subjective," wrong,

embarrassing, or unedifying: e.g., the mystical and theological preoccupations of Kepler and Newton.[17]

C. I am attempting to illustrate the need for some "collective biography" of the elite of the pioneering cohort of educational thinkers and researchers born between 1870 and 1890: their social antecedents, intellectual debts, patterns of personal influence and career mobility, political inclinations, and the like, so that we may gain a deeper understanding of where they came from, what they aspired to do, what they did, where they went wrong, and what they left for us to do.[18] In this exploratory study, I suggest that Kilpatrick, in his "dualistic" attack on educational positivism, anticipated some important points made recently by the post-positivist generation of historians and philosophers of science born in the 1920's.[19]

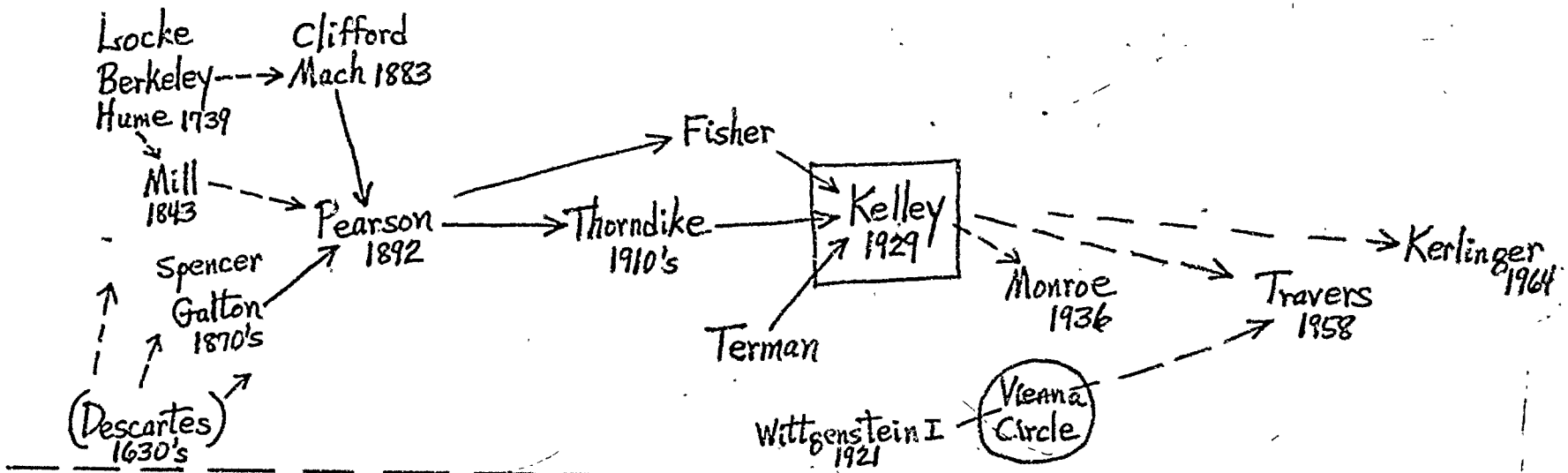
IV

William Heard Kilpatrick (1871-1965) was born at White Plains, Georgia, the son of a Baptist minister and former slaveholder. He graduated from Mercer University, a Baptist college in Macon, Georgia, in 1891. He studied briefly at Johns Hopkins, Chicago, and Cornell, taught mathematics in Georgia, and arrived at Columbia University in 1907. There he wrote a dissertation in the history of education, became a disciple of Dewey, and was appointed professor of the philosophy of education at Teachers College in 1918. He seems to have been by far the most popular and influential teacher of educational philosophy in America throughout the 1920's and 30's. Surprisingly, R.S. Peters has called his magnum opus, Philosophy of Education (1951), "a classic, with a similar type of coverage but with a less analytical approach" than Peters' own Ethics and Education (1966).[20]

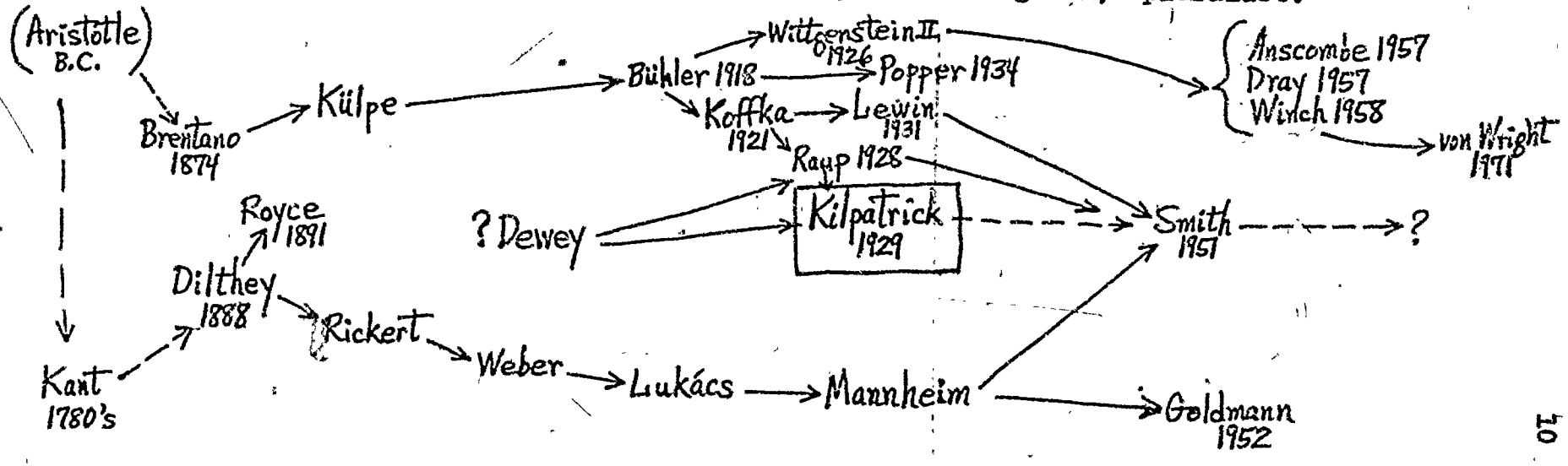
Truman Lee Kelley (1884-1961) was born at Whitehall (Muskegon County), Michigan. He received the A.B. in mathematics at Illinois in 1909 and the A.M. in psychology, also at Illinois, in 1911. He was awarded the Ph.D. at Columbia in 1914, studying there primarily under Thorndike and

Figure 1. Sketch of two methodological traditions in the policy sciences as they relate to education (very selective and simplified).

I. Galilean-Cartesian: empiricist, positivist, behavioralist, "monist."



II. Aristotelian-Kantian: rationalist, critical, phenomenological, "pluralist."



Woodworth. He assisted Thorndike in developing the classification tests used by the Army in World War I. In 1920 he went to Stanford, working there with Terman on refining the Stanford Achievement Tests and the Stanford-Binet IQ test. He went to Harvard in 1931, retiring in 1950. Kelley is credited with important contributions to applied statistics, psychometrics, and factor analysis.[21] Boring identifies him as "Thorndike's pupil and for some years America's leading psychologist-statistician," adding that it was Kelly, "once Thorndike's student, who successfully broke away in 1928 from Spearman's conceptions" in factor analysis; the allusion is to Kelley's book, Crossroads in the Mind of Man.[22]

We turn now to a closer look at the central papers in the debate: three by Kilpatrick and two by Kelley.[23] Kilpatrick's initial paper, titled "The relations of philosophy and science in the study of education," was delivered as an invited address at the AERA meeting in Cleveland in 1929. Later, in December of that year, Kelley replied in his address as retiring vice-president of section Q, Education, of the AAAS, published as "The scientific versus the philosophic approach to the novel problem" in March 1930. Almost a year later, on 6 February 1931, the adversaries engaged in formal debate before the New York Society for the Experimental Study of Education on the issue. "Resolved, that for some of the vital problems of education philosophy not science is and must remain a guide to the solution." Kilpatrick argued the affirmative, Kelley the negative. Their papers were published in the Harvard Teachers Record of 1931 as "A defense of philosophy in education" and "A defense of science in education." Kilpatrick restated and reargued his position in another presentation later in that same month of February 1931 before the AERA meeting in Detroit, published as "The relation of philosophy to scientific research." Kelley did not reply, so far as I can determine, but simply included his two contributions to the debate as chapters 5 and 6 in the expanded second edition of his book on Scientific Method: Its Function in Research and in Education (1932).

Kilpatrick's case consists principally in setting forth, without mentioning Aristotle, the well-worn Aristotelian distinctions between types of intellectual disciplines. These are of three kinds, Kilpatrick says, depending on the kind of question they are fundamentally intended to answer (see Figure 2, column 3). Kilpatrick then describes some examples illustrating types of cases in which scientific method as widely understood--i.e., as involving experimental controls, "objectivity," precise measurement and the like--would seem to be helpless: should the old farmer spend his nest egg on a new water works or on sending his daughter to college? It is just such questions of "conduct and policy" that are the central ... shaping concern of education, he says.

Kilpatrick contrasts science and philosophy in several respects.[24] Science is concerned with facts, philosophy with a situation of necessary action. Science isolates variables and excludes everything except causal relations; philosophy is concerned with the meanings that things have for people. Science eliminates bias, wish and interest in the name of "objectivity"; philosophy centrally deals with and uses these as its "primary subject-matter"; it seeks a course of action that best harmonizes and saves all interests. Science deals with the part/whole relationship by analysis and separation of variables; philosophy deals with whole situations, the parts as they are actually concretely related. Science deals with what actually exists, philosophy with what is not yet, but could and perhaps should be brought about.

He mentions four types of problems that science cannot solve: cases of doubt or dispute regarding "the good life"; cases where the school must make a choice among persons or in the relation of person to person; cases where a principle, or even fact, abstracted from selected data is to be generalized; and cases where assumptions are questioned, especially the assumptions upon which the facts and data of empirical science depend.[25] Kilpatrick grants, of course, that science can shed some light on why some solutions to these problems would be more acceptable than others.

In a remarkable passage on the cult of the "fact" that deserves quotation, Kilpatrick wrote:[26]

> Certain seeming opinions to the contrary notwithstanding, facts are not just bare things standing alone, true independently of all else, waiting in such independence to tell us what to think. . . . They are noted or seen as such only in relations, actual or possible. One sees things as facts only because there are already present certain outlooks of theory and interest to which these things as seeming facts have pertinence. . . . The process of "establishing" a fact necessarily establishes it in and for that relationship and not, necessarily, in or for any other. That is to say, what is proved in one limited relationship need not remain true when other factors are introduced and the relationships are thus changed.

The last part of this passage does not give due weight, ironically, to the fact that the methodological canons and safeguards of experimental inference are designed just to prevent overgeneralization from insufficient data. But the larger import of the passage is a fascinating anticipation of recent claims about the "theory-ladenness" and paradigm-dependence of facts. Facts do not have, Kilpatrick adds, the "almighty sovereignty" sometimes attributed to them by careless thinkers. "We have to know the conditions under which the alleged facts were established."[27]

Kilpatrick states his thesis of methodological dualism or complementarity as follows:[28]

> (D) There are problems, and regions of problems, with which the processes of "exact" science are inadequate to cope; the complementary processes of philosophizing and of "exact" science are needed to deal with them successfully.

These are, most prominently, the problems of "conduct and policy" to which he draws attention; philosophizing is "forever essential" in educational research, he says, because it supplies what empirical science, with its intrinsic limitations of content and method, must lack when it attempts to deal with practical problems. Thus there are two basically different and equally essential methods of research: S-type research, which is "scientific procedure" applied with originality; and P-type research, which is "a careful and extended study--analytical and critical, and possibly con-

structive--into records or experience as such but done from the point of view of a P-type inquiry or interest."[29]

P-type research is not undisciplined, not "mere speculation"; it draws upon accumulated cultural resources of three types: terms and distinctions for the description and analysis of our interests and values in life; "strategic conceptions" for picking out basic values and clusters of values, such as virtues and ideals; and unified points-of-view on life, such as systems of ethics and conceptions of the highest good in life. To ignore the helps provided by the best of this cultural heritage would be very unintelligent, in Kilpatrick's view.

The salient educational question, Kilpatrick insists, is characteristically a P-type question about what I (we) really want and what, all relevant things considered, I (we) ought to do. The attempt to answer it usually gives rise to what he calls a technological or T-type question: how shall I do it--i.e., bring about what I want? A T-type inquiry into means toward an end in turn normally generates an S-type question about what is really true: if I do this, what will happen?[30] These three questions, so reminiscent of Aristotle and Kant, are summarized in the middle column of Figure 2, where I also display some pleasing parallels with other teasing trichotomies. The parallels are of course inexact; but perhaps it can now be granted that Kilpatrick has persuasively restated, in his own way, a perennial argument for a kind of methodological dualism between the natural and policy (moral, political) sciences.

Kelley's rebuttal, to which I now turn, consists primarily in refusing to grant the cogency of Kilpatrick's distinctions. He insists throughout that science and philosophy are in their different ways trying to answer the same questions, insofar as they both are trying to get at the truth, and so are competitors for the same "turf." He puts forth, in opposition to Kilpatrick's dualistic thesis (D), the thesis I shall call methodological monism:[31]

(M) There is only one method tending to establish truth in the world of phenomena.

Fig. 2. Some pleasing parallels and teasing trichotomies.

Aristotle ^B.C. Sciences are:	Kant 1780's Divisions of philosophy (sciences):	Kilpatrick 1929 Fund. problems of life & education are:	Peters 1953 Questions of; answered by:	The next step? 1970's Types of systematization & justification (i.e. sciences).
Theoretical or speculative. Involve knowing, noesis.	Logic (forms of thought in general). Laws of thought.	Scientific: "If I do this, what will happen?"	Theory; descriptions, based on "going to look."	Deductive: "chain theory." Pure, exact, formal sciences of logical order.
Productive. Involve making, poesis.	Physics (natural & applied sciences). Maxims, instrumental rules of phenomenal (sensible) world. Laws of nature.	Technological: "How shall I do it?"	Technology; procedures for bringing about states of affairs that satisfy our requirements.	Inductive: "pebble theory." Experimental & correlational sciences of causal order.
Practical. Involve conduct, praxis.	Ethics (moral or human sciences). Imperatives of noumenal (intelligible) world. Laws of freedom.	Philosophical: "What shall I do?" (all relevant things considered)	Policy; appraisals, prescriptions concerning what ought to be the case.	? Human sciences.

Met. 1025 b25. Found. Met. Mor. 387 (Preface). NEA 1931, p. 417. Brett's History, ch. 1.

(Nothing hangs upon Kelley's reference to the "world of phenomena"; it is not a hint that there is another world, such as a Kantian noumenal world.) This method, he tells us, is that of careful, patient observation and experiment. Until it provides an answer, the scientist refuses to try to answer questions that the too-hasty philosopher attempts to answer on the basis of "mere speculation."

Still, Kelley grants that the exigencies of decision and action require that scientific conclusions must be supplemented by what he calls "judgment": something that experienced and sensible decision-makers have, he says, but of which he offers no account. He seems to think that this too is only a poor substitute for scientific knowledge, to be replaced as soon as possible by the results of quantification. And, strangely, on his view the decision-maker becomes a kind of "existential hero," since Kelley holds that in exercising "judgment" the executive should "be the responsible party in the matter and personally held to account for any mistakes."[32] This suggests the scientistic view that everything we get right is due to "science," while everything we get wrong is due to the blunders of people.

The <u>scientific</u> answer to a practical question, Kelley says, is "If in doubt delay decision and investigate." "The scientist," he says, "procrastinates decisions, is otherworldly, is of little aid in times of stress," while the philosopher is all too eager to come forward with immediate answers. But the difficult problems of education cannot and need not be solved in a moment, and it is only by the slow and sure processes of empirical science that we can be sure of getting the right solutions. Science continues and carries through the Deweyian complete act of thought, he says; philosophy does not.[33] "To the philosopher any outcome of his cerebration suffices," while the beauty of scientific hypotheses is that they can be proved right or wrong by experimental trial. Philosophy should attempt to "ape mentally the steps of science."

Two peculiarities of Kelley's philosophy of science may be noted here. Throughout he seems to accept as science only what ultimately turns out to be scientifically true; to him, science, as true, is an ideal product and not a process. Yet if any meaning attaches to the notion of scientific method, it is just the process of adhering to the canons of scientific procedure, however things may turn out in the end. Further, as science progresses and changes, everything we think of as science today may conceivably turn out to be wrong, hence nonscience. But surely it is a conceptual truth that whatever we now take to be paradigm cases of science just is science, irrespective of what may happen in the future, what the subsequent course of science may turn out to be, or how mistaken present-day science may one day be shown to be.[34]

The second peculiarity is his apparent belief that definitive disproofs of scientific hypotheses can be carried out: the old empiricist faith in the "crucial experiment." This belief has found an echo in the more recent Popperian view that science grows by conjectures and refutations. But few philosophers of science are convinced that the thesis of the "crucial experiment" can be sustained in the face of the critique put forth by Pierre Duhem in 1906. Duhem showed how it was in principle possible for an ingenious scientist to "save" any hypothesis by arguing that it was something in the conditions of the test and not in the hypothesis itself that produced negative results. Indeed, a wide variety of seemingly incompatible psychological theories have proven amazingly immune to definitive experimental disconfirmation.[35]

We have now passed in review the principal points made by the debaters in the five papers under examination: Kilpatrick upholding the "dualist" thesis that science and philosophy were both essential, each supplying something the other lacked; Kelley countering with the "monist" thesis that scientific method alone could establish truth in education. Though the issue between them has fitfully stirred from time to time since, especially in the writings of Harold Rugg, R. Bruce Raup, B. Othanel Smith, and their students and

collaborators, the potentials of the Kelley-Kilpatrick debate as a turning-point in the discussion of educational research methodology seemingly never were realized.[36] The exchange was broken off in 1931, perhaps because these two protagonists had nothing to add. But the "external" reason the debate rested there was, no doubt, the economic collapse of the early 1930's, which turned the attention of educators, and everyone else, to the unprecedented socio-economic crisis torturing the nation, then to the challenging question of what the schools should do--if they could do or dared to do--to build a new, reconstructed social order on the ruins of the discredited one.

V

Without wanting to defend everything he said, I conclude that Kilpatrick won the debate on points, making a strong prima facie case for the complementarity of scientific and philosophical methods in education. Further, his case has enough continuing validity to claim our attention still. The time may be ripe to reopen the controversy. Today we can see somewhat the same combination of circumstances obtaining as in the late 1920's: a narrow positivist orthodoxy among our most influential methodologists (e.g., Travers, Kerlinger) coupled with much dissatisfaction on the part of almost eveyone else about the triviality, irrelevance and inconclusiveness of research done according to their recipes for "scientific method."[37] Again the statistical researchers are bringing dubious--or anyway unwelcome--news to believers in education, equality and democracy, as in the work of Coleman, Jencks, Jenson, and Herrnstein.

It can be granted to Kelley, and like-minded scientistic positivists, that educational analysis and theorizing, as practiced by some philosophers, has often seemed, and sometimes undoubtedly been, undisciplined, ignorant, willful, arbitrary, tendentious, and concerned more to display cleverness or doctrinal purity than to get things right. Granted that some of it has been self-indulgent and undisciplined, it does

not follow that all of it must be; here, as everywhere, one must discriminate. It will not do to equate philosophy with bad philosophy and science with good science. A good philosopher will be no more careless of "facts," where truly relevant and reliable facts are available, than is the scientist.

Kilpatrick, in restating and defending essentially the Aristotelian and Kantian divisions of the intellectual disciplines (i.e., sciences), has shown the way toward formulating a non-reductionistic metatheoretical framework that exhibits in their proper relations the exact-scientific inquiries of formal model-building, the quantitative empirical studies of technological inquiry, and systematic-synoptic or philosophical inquiry focussing on what, in view of all relevant considerations, ought to be decided and done.[38] If he is right, the inadequacy of the Ennis view (p. 3), largely equating educational research with the second sort of inquiry, should be patent.

And if I am right about Kilpatrick, his unfashionable views, contrary to progressivist or "Whig history" of the policy sciences, cannot be ignored because they diverge from the currently approved doctrines about "scientific method" found in our research textbooks and handbooks. They may yet be the basis of a "research program" and model of intelligibility, or "paradigm," that has more to offer than our current "normal science," the Galton-Pearson-Fisher paradigm, with its wearying results of "no significant differences." (If the samples are large enough the differences are statistically significant but unimportant.)

VI

The larger import of the inquiry is thus the suggestion that there is an intellectually respectable alternative to the positivistic orthodoxy our researchers have been taught. Contrary to the impression to be gained from the research manuals, it may yet be possible for our research to be both "rigorous" and "relevant." Kelley's argument reveals one weakness of orthodoxy: its characteristic inability to give a coherent

account of the role of common sense and nonscientific knowledge in educational theory. The typical positivist view is that all knowledge is scientific knowledge; where scientific knowledge is unavailable or incompetent, Kelley must fall back on his unanalyzed notion of "judgment." This might suggest that what the educator needs is not so much scientific method as a method of practical judgment.[39]

There is no indication Kelley knew anything of the "logical positivism" or "logical empiricism" of the Vienna Circle. He probably cannot fully be identified with its virulent scientism, a metascientific doctrine I would summarize in four propositions. Scientism says that science, always "properly understood," is:

1. Unitary. There is only one kind of empirical science; it is monolithic in its method (or perhaps language, or laws, or something).

2. Autonomous or self-sufficient. Science need not be supplemented, guided, or corrected by anything outside itself.

3. Unlimited. It can study anything and everything, without exception. It can recognize no boundaries set by theology or ideology, perhaps even morality.

4. Omnicompetent. Science can answer all legitimate questions. Those that it cannot in principle answer are illegitimate pseudo-questions.

In addition, scholastic scientism holds that all of science can in principle be made perfectly explicit and precise.

Kelley seems to have held that science was unitary and autonomous, needing no helps from philosophy. It is less clear whether he thought it was unlimited and, with Pearson and Thorndike, omnicompetent.

In conclusion: I have tried to reopen and restate the issue debated by Kelley and Kilpatrick, to show that Kilpatrick held the more defensible position, and to convince the reader that the issue is a live one: is the scientistic caricature

of science an adequate viewpoint in educational research, or is what is now needed something more like the stereoscopic vision of Kilpatrick's "dualism"? If we are to subdue the accumulating "data monster" and revitalize research, I believe the latter alternative is indicated.

Notes

1. This paper was indirectly prompted by B. Othanel Smith's suggestion, in February 1970, that I look at the issues raised in Raup's 1928 paper on the limitations of scientific method. Earlier versions were presented at the University of Illinois, Urbana-Champaign, in February 1974, and at the annual meeting of the American Educational Research Association in Washington, D.C., in April 1975. I am indebted to Eric Weir, Vynce Hines, and Donald R. Warren for their comments and criticisms.

2. Goldmann, 1952, p. 22.

3. Ennis, 1973, pp. 4, 10.

4. See Brush, 1974, for an instructive introduction to the issue of progressivist of "Whig history" of science vs. new-style history; also Agassi, 1963, for an amusingly iconoclastic attack on the old-style "inductivist" history of natural science.

5. See the essays by Bertrand Russell and John Dewey in Beard, ed., 1928; also Pupin, 1930. For general background, see Curti, 1964, ch. 27: "Prosperity, disillusionment, criticism," pp. 667-696.

6. Dewey in Beard, ed., 1928, p. 317.

7. Katz, 1966; Scates, 1967.

8. Cronbach, 1967, p. 68.

9. For samples of the anti-democratic writings of the period, see Mencken, 1926, and DeLorme & McInnis, eds., 1969.

10. See Marks, 1974, esp. pp. 346-9 and 351-5, for a penetrating review of the IQ debate of the early 1920's between Lewis M. Terman (1877-1956) and Walter Lippmann

(1889-1974) and its social implications; also Cronbach, 1975, esp. p. 9f. Dewey asserted that "The potentiality of science as the most powerful instrument of control which has ever existed puts to mankind its one outstanding present challenge" (1931, p. 324).

11. Joncich mentions that Thorndike had "read Karl Pearson's The Grammar of Science and accepted fully its proposition that science, rightly understood, is competent to solve all problems" (1968, p. 529f.). There is disappointingly little else in her book on Thorndike's deeper intellectual debts as a "sane" or primitive positivist.

12. Dewey, 1928b, p. 116; 1929b.

13. Suppes, 1974, pp. 4b, 6a.

14. Dewey, 1929b, p. 39; 1929a, p. 251; 1931.

15. See Bode, 1927, chs. 5 & 8, on the "scientific" claims of Bobbitt and Thorndike; Raup, 1928; Kilpatrick, 1929; Joncich, 1962, p. 9.

16. Lovejoy, 1936, ch. 1; Boas, 1969.

17. See note 4; also Kuhn, 1962, 1968; McGuire, 1973.

18. Stone, 1971; Shapin & Thackray, 1974.

19. Including Kuhn and Toulmin in 1922, Hanson and Feyerabend in 1924, Putnam in 1926, Scriven and Shapere in 1928.

20. See Parker, 1965, for a biographical obituary and some bibliography. Peters, ed., 1973, p. 273.

21. National Cyclopedia of American Biography 49 (1966), p. 443f.; Tiedeman, 1968; Joncich, 1968, p. 364.

22. Boring, 1950, pp. 540, 576.

23. Kilpatrick, 1929, 1931a, 1931b; Kelley, 1930, 1931.

24. Kilpatrick, 1929, pp. 41-43.

25. Pp. 44-46.

26. 1931b, p. 108f.

27. P. 109. See Hanson, 1958, chs. 1 & 2, on the theory-ladenness of "observed facts"; most of ch. 1 is reprinted in Broudy, Ennis & Krimerman, eds., 1973, pp. 164-178. Also Kuhn, 1962, esp. p. 15f., for an influential view of the determination of facts by "paradigms"--a view now increasingly under attack as exaggerated and excessively "subjectivistic."

28. 1929, p. 48.

29. 1931b, p. 106.

30. 1931a, passim.

31. Kelley, 1930, p. 295a.

32. P. 296. On "existential heroes," see Wake, 1973, pp. 18-20.

33. Pp. 298a, 299b.

34. Cf. Shapere, 1966, on the dangers of retrospective judgments that scientists were being "unscientific."

35. Kelley, 1931, p. 129a. Duhem, 1906/14, esp. pp. 180-190. Hilgard (in Hilgard & Bower, 1966, p. 9) has noted that "For the present, we must be prepared to accept the historical truth that opposing theories have great survival value, and that an appeal to the facts as a way of choosing between theories is a very complex process, not nearly as decisive in practice as we might expect it to be."

36. Rugg, 1934; Whipple, ed., 1938; Smith, 1950, 1951.

37. See Yamamoto, 1968, for a small anthology of frequently-heard complaints about triviality, statistical ritual, etc.

38. Szoke, 1974, attempts to organize these inquiries into a conception of systematic educational theory.

39. See Raup & others, 1950.

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