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The Genetic Epistemology of Jean Piaget

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Summary

Jean Piaget (1896–1980) is known for his contributions to developmental psychology and educational theory. His name is associated especially with Stage Theory. That we believe him to have focused solely on cognitive development, however, is not because he did. This is instead the result of the popularization of his writings in the United States during the Cold War. (A period of crisis and subsequent education reform.) The overpowering influence of those interests blinded us to his larger framework, which he called “genetic epistemology,” and of which his stages were just a part. To address the resulting and continuing misunderstandings, this essay presents original historical scholarship—distilling over a thousand pages of archival documents (correspondence, diary entries, budgets, and reports)—to provide an insider’s look at Piaget’s research program from the perspective of the Rockefeller Foundation: genetic epistemology’s primary funding agency in the United States from the mid-1950s through the early-1960s. The result is an examination of how a group of interested Americans came to understand Piaget’s writings in French in the period just prior to their wider popularization in English, as well as of how Piaget presented himself and his ideas during the reconstruction of Europe after World War II. My goal, however, is not to summarize the whole of this misunderstood program. Instead, I aim to provide a source of archivally-grounded perspective that will allow for new insights about the Genevan School that are unrelated to American Cold War interests. In the process, we also derive new means to see how Piaget’s experimental examinations of the development of individual knowledge served to inform his team’s investigations of the evolution of science (and vice versa).

Keywords: Jean Piaget, Rockefeller Foundation, stage theory of cognitive development, experimental philosophy, developmental epistemology, naturalized epistemology before Quine, analyticity debate, Cold War history of human science

Introduction

Genetic epistemology was a scientific research program dedicated to examining the genesis of knowledge. It focused especially on explaining the evolution of science and its supporting concepts as they develop in the individuals whose understanding then contributes collectively to whatever they in turn judge to be scientific advancement (while also satisfying certain formal criteria). This took advantage of the psychological methods for which Jean Piaget (1896–1980) is now known, which had informed his earlier Stage Theory of cognitive development. And so it was, in short, a scientific study of science: an investigation of how we

come to know what we know in a way that can be trusted not to be illusory. But the program itself remains mostly unknown, in its details, except as an interest associated with Piaget's name.

Genetic Epistemology?

The endeavor spanned several decades of active research by a large international team of collaborating theoreticians and experimentalists based at the University of Geneva. It was supported by what would now be millions of dollars, which enabled an enormous output of published research. However, most of this was never translated into English. As a result, many of its most significant works had only a partial impact outside of French-speaking Switzerland.

In addition to Piaget's own solo- and jointly authored experimental and theoretical texts, the bulk of genetic epistemology's published results is collectively constituted by 37 edited volumes of an in-house series called the *Études d'Épistémologie Génétique* (Studies of Genetic Epistemology, but usually referred to acronymically as EEG). Several related collections were also published separately (e.g., Piaget, 1967b), as were important conference proceedings (e.g., Piaget, 1973), and even some books without his name included in the byline (most recently by Henriques, Dionnet, & Ducret, 2004; for an annotated translation of his introduction, see Piaget, 2004/2006). In addition, at least two other unpublished books—handwritten, typed, corrected, organized into narrative arcs, and then reorganized, but never sent to a publisher—have since been found among the piles of papers in his home office (see Ratcliff & Burman, 2015). The Genevan School's journal, *Archives de Psychologie* (Archives of Psychology), also collected many more individual studies by the team's collaborators. (For names and photos, see Ratcliff, 2010, pp. 77–87.)

There is insufficient space here to review all of this in detail. Just the last years of the team's efforts have been the subject of entire books (e.g., Ducret, 2000; Vuyk, 1981). The three volumes that inaugurated the program, collectively entitled *Introduction à l'Épistémologie Génétique* (Introduction to Genetic Epistemology), also remain unavailable in English but for a section included in the *Sociological Studies* collection (viz. Piaget, 1950/1995). However, it has indeed been summarized (see Kitchener, 1981). The institute that was launched on its back, the *Centre international d'Épistémologie Génétique* (International Centre for Genetic Epistemology or CIEG), has also received some critical attention (e.g., Bronckart, 1980; Burman, 2012; Dionnet, 1998; Ratcliff, 2010, 2019; Ratcliff & Tau, 2018; see also Bringuier, Gruber, Carreras, & Cellérier, 1977/1980).

What isn't clear that ought to be, though, is how what they did there was different from what we understand of it today. Related to this, and perhaps more important, is *why* outsiders have *continued* to fail to grasp what seemed obvious to insiders (see e.g., Inhelder, 1962/1970, 1977, 1978; Inhelder & de Caprona, 1990). This then also has several associated concerns, including why textbooks continue to refer to Piaget as a “genetic epistemologist” without providing a clear explanation of what this meant, beyond that being the higher-order counterpart to the development of intelligence that was popularized as “the mind's staircase” (Case, 1991; Kesselring, 2009; see also Flavell, 1996; Morra, Gobbo, Marini, & Sheese, 2008).¹

Very briefly, then, by way of introduction: Piaget saw himself traveling the same paths as Thomas Kuhn (1922–1996) and Alexandre Koyré (1892–1964).² His goal was the study of justified knowledge as it changes over time, and as it is constituted by premises which change. But he also went a step further, combining their shared interest in what is now called the History and Philosophy of Science with empirical studies of knowledge development *in situ* (in children). This then meant engaging with questions regarding the construction of fundamental experiential categories—such as those implicated in causality, space, time, and speed—that had been discussed since Aristotle (384–322 BC) and assumed *a priori* since Immanuel Kant (1724–1804). And that in turn required blurring the boundary that had grown up over the centuries between philosophy and psychology. As Piaget explained directly:

If one takes a dynamic rather than a static point of view, it is impossible to maintain the traditional barriers between epistemology and the psychogenesis [development] of cognitive functions. If epistemology is defined as the study of the formation of valid knowledge, it presupposes questions of validity, which are dependent on logic and on particular sciences, but also questions of fact, for the problem is not only formal but equally real: How, in *reality*, is science possible? In fact, all epistemology is therefore obliged to invoke psychological presuppositions, and this is true of logical positivism (perception and language) as well as of Plato (reminiscence) or of Husserl (intuition, intentions, significations, etc.). The only question is to know if it is better to content oneself with a speculative psychology or whether it is more useful to have recourse to a verifiable psychology!

(Piaget, 1968/1970, p. 731)

In other words, philosophers have been making psychological assumptions about knowledge since they started writing about *what it means to know*. And Piaget argued that we ought to replace naïve, folk-psychological “wisdom” with a more sophisticated understanding informed by actual study (see also Piaget, 1965/1971).

The founding of CIEG in 1955 reflected these interests, and thus so too did the grant applications written to support it (some of which are examined here in detail). He explained this directly a handful of other times in texts that haven’t been translated (e.g., Piaget, 1957a, 1957b, 1961). But he also made it clear in later reflections that were translated. For example:

. . . we have founded an International Center for Genetic Epistemology, so that psychologists, logicians, cyberneticists, epistemologists, linguists, mathematicians, physicists, etc., may collaborate there, depending on the problems being considered. This center . . . has therefore had as its goal, from the beginning, to study a certain number of epistemological problems seeking to analyze experimentally the psychological data necessary for the other aspects of the problem.

(Piaget, 1968/1970, p. 731)

In short: Piaget had a mission driven by skepticism, decades of experimental experience to draw upon in testing presuppositions, and an entire institute dedicated to addressing the issues arising from the combination of his team’s empirical investigations and the novel epistemological insights generated as those results were theorized. (For a particularly good high-level English-language examination of the program’s philosophical aims, goals, and

assumptions, see especially the writings of Richard Kitchener, 1986, 1992, 2004, 2006; for a book-length introduction to Piaget more generally, see Chapman, 1988; for a curated selection of readings from primary sources, in English, see Gruber & Vonèche, 1977/1993.)

What's the Problem?

The present problem is not that genetic epistemology was never explained. It was, many times; including in translation (e.g., Piaget, 1952/1953, 1965/1971, 1968/1970, 1970/1971, 1970/1972; also Bringuier, 1977/1980; Evans, 1973). The problem is rather that these explanations were not well-received (pun intended). In other words, this neglected aspect of Piaget's project constitutes what has elsewhere been called a "foreign invisible" (Burman, 2015). The explanations existed, therefore, but we never saw them clearly as a result of our own conflicting interests.

In this way, the reception of Piaget's epistemology can be understood to have been similar to that of Wundt's *Völkerpsychologie* (Burman, 2016a, pp. 4, 106). That is, the historical Piaget is not identical with the "Piaget" we know today: his reflection in American psychology is seen through a "mistaken mirror" (as it was put for Wundt by Leahey, 1981). So there are substantial pieces missing, in our understanding, as a result of how the importation was made (see of Piaget, e.g., Beilin, 1992a; Brown, 1988; Dean & Youniss, 1991; Rowland, 1968; Vidal, 1994; Voyat, 1977/1977). Furthermore, unlike with Wundt, the underlying cause of our neglect of Piaget continues to be active: readers influenced by his popularization through Cold War-era American psychology misunderstand his broader and newer epistemology as a result of how we now see his narrower and older psychology. Indeed, as Piaget (1963) himself once noted in this connection, he felt that he had "been understood . . . more from without than from within" (p. viii).

Recognizing this affords the source of the contribution to be made here. Adopting this view *from within*—which is to say *historicism* (according to how it was seen then), rather than *presentism* (according to how we see it now)—is consistent with the methods required of contemporary research in the History of Psychology (see e.g., Capshew, 2014). Thus, I propose that we cannot understand the most foreign-seeming aspects of this material except by examining it historically (Burman, 2012, 2013, 2015, 2016a, 2016b, 2019, 2020a, 2020b; Müller, Burman, & Hutchison, 2013; Ratcliff & Burman, 2015, 2017).

To do this, and thereby begin to address the resulting divergences between the Swiss source and its American popularization, I decided to take advantage of the meticulous records kept by the Rockefeller Foundation (RF). These then provide a microhistorical frame through which we can contextualize Piaget's research program and its reception before its psychological aspects were reinterpreted in the way we now understand.

The main advantage of adopting this approach is that the RF's documentation is detailed and very clear. This is because they operated during this period as a nongovernmental intelligence-gathering unit, and as an alternate source of funding for international development, in the projection of American soft power during the reconstruction of Europe after World War II (see e.g., Krige, 2006; Mueller, 2013; Parmar, 2012; Solovey, 2013). Its collections are therefore an extremely valuable resource for historians.³ Indeed, the insights

here result from the examination and distillation of nearly a thousand pages of previously unpublished primary-source material. To share this “new” wealth, I have quoted from these sources at length.

Contextualizing the Popularization, to See Behind It

Piaget now has the status of one of the Great Men of psychology. Archival records show, however, that—before the success of former American Psychological Association president Joseph McVicker Hunt’s (1906–1991) book, *Intelligence and Experience* (1961)—American publishers were unwilling to take a risk on a book dedicated solely to explicating Piaget’s work. (See the account of Flavell’s frustrations by Müller et al., 2013.) The RF’s grant-making process that led to the founding of Piaget’s institute in 1955 also began before the unexpected Soviet success with Sputnik in 1957, another oft-cited justification for his later uptake (e.g., Stendler, 1965; also Bliss, 1995; Murray, 1992). Thus it was before the passing of the U.S. National Defense Education Act of 1958, and therefore also before its resources had been made available for anything and everything related to the faster production of scientists, technologists, engineers, and mathematicians (viz., the STEM disciplines) at a rate that would surpass their production in the Soviet Union (e.g., Urban, 2010). The RF’s support even originated before the *Brown v. Board of Education* decision of 1954 and its impact on the modern discourse regarding the meaning of “intelligence” as something that could be enhanced in schools (Porter, 2017).

In short: when Piaget first began to discuss his plans with RF officers, the great and pressing interest in education reform that textbook authors now take for granted simply did not exist, including Hunt’s involvement in launching Head Start more than a decade later (see Pickren, 2012). Piaget was therefore not yet the “developmental psychologist of intelligence” in the way that we know him. Instead, his proposal advancing genetic epistemology fit into Warren Weaver’s (the RF vice president for Natural and Medical Sciences) long-standing goal of promoting interdisciplinary research as a practical solution to hyperspecialization (see Cohen-Cole, 2014, pp. 86, 102).

Interdisciplinarity is now usually described as the cause of the misunderstandings that continue to this day (see e.g., Inhelder, 1977; also Chapman, 1988). But at the time—for Weaver and the RF—it was a feature, not a bug. This was Piaget’s view too. As he explained:

. . . specialists determined to remain within the boundaries of their own discipline remain irremediably short-sighted, because it is only through contact with other branches of scientific knowledge that real progress can be achieved. Without recourse to biology, logic and mathematics, developmental psychology can again only be descriptive. . . : the explanatory level involves underlying structures which inevitably cut across the traditional boundaries between the different disciplines.

(Piaget & Inhelder, 1969, p. 148)

Interdisciplinarity resulting in greater understanding today, rather than education reform to make greater understanding possible in the future, was therefore the overriding virtue of genetic epistemology (see also Ratcliff & Burman, 2017).

Of course, Piaget himself was not unknown to Americans after World War II (see Hsueh, 2009). For decades, he had been the director of the International Bureau of Education (see Hofstetter, 2015; Hofstetter & Schneuwly, 2013). Translations of his books had also resumed after a long delay, starting with *Psychology of Intelligence* (Piaget, 1947/1950b) and the first book of the well-known infancy trilogy: *The Origins of Intelligence in Children* (Piaget, 1936/1952c). An English-language autobiography was also published in the *A History of Psychology in Autobiography* series (Piaget, 1952a). But many of his most influential works—such as the other two books of the infancy trilogy, *The Construction of Reality in the Child and Play, Dreams, and Imitation in Childhood* (Piaget, 1937/1954, 1945/1962)—were not yet available to American readers. Nor had his honorary doctorates from American institutions yet resumed: after Harvard in 1936, the first of these was from Chicago in 1953. And that, too, was for his psychology of intelligence: the topic of the first translations after the war (Office of Press Relations, 1953).

Moreover, the social network of American Piagetians did not yet exist in the way we now expect. Most of the supporters and popularizers whose names we recognize as defining everything that came after weren't yet active. Although Jerome Bruner (1915–2016) was already established at Harvard, having returned there in 1945 following his wartime service at the Office of War Information in Paris, the rest of that postwar generation was still at school. As Kessen (1996) noted:

The '50s were . . . the decade of professional origin for the group of young people who would oversee the American rebirth of Piaget (the place and year of each person's Ph.D. degree are shown in parentheses): Brown (Michigan, 1952), Chomsky (Pennsylvania, 1955), John Flavell (Clark, 1955), Jerome Kagan (Yale, 1954), Lawrence Kohlberg (Chicago, 1958), and Ulric Neisser (Harvard, 1956). (p. 198)

To this list we can add Daniel Berlyne (Yale, 1953), who collaborated to produce the translation of the first intelligence book after the war, as well as Adrien Pinard (Montreal, 1954), David Elkind (UCLA, 1955), Hans Furth (Portland State, 1960), Thérèse Gouin Décarie (Montreal, 1960), Monique Laurendeau (Montreal, 1960), and many others whose contributions are now known primarily in relation to what most psychologists still call the Cognitive Revolution (debated by Hobbs & Burman, 2009). But these additions serve only to reinforce Kessen's point: the landscape of developmental or child (or cognitive) psychology changed enormously at exactly the moment when Piaget was being imported into it. This shift—from a focus on postwar interdisciplinarity to Cold War education reform—then had effects that continue to this day: what we see now is a reflection through that mistaken mirror, and we cannot help but be misled while continuing to look through it. (For discussions of the broader history of Cold War human science, see e.g., Cohen-Cole, 2014; Erickson et al., 2013; Isaac, 2007; for social science, see Engerman, 2010; Solovey & Cravens, 2012; for education reform, see Evans, 2011; Rudolph, 2002.)

An Example of a Valuable Archival Insight

Before we dive deeply into the RF's archival files, let's consider one document out of context. This provides a quick overview of what it is we are trying to see. And it begins with a variation of what is now the standard textbook description:

Professor Jean Piaget has long been one of the most distinguished psychologists in Europe. In his work at the Institute of Educational Sciences of the University of Geneva (formerly known as the Jean Jacques Rousseau Institute), he has studied carefully the development of the mentality of children in particular relation to their ability to comprehend and use abstract concepts. He has demonstrated the progressive stages by which children expand their knowledge of the world around them and their gradual development toward full intellectual maturity. His research and extensive publications have been essentially in the field of child development and genetic psychology.

(Fahs, 1953)

Note, here, the older use of “genetic” meaning *genesis* or *generation*. (Piaget’s use of the word does not refer to genes in the Mendelian nativist or mutationist sense, but to the older sense of construction de novo of functional novelties [see Burman, 2013, 2016b, 2019; Burman, Bazar, & Weizmann, 2020].)⁴ The archival document then continues, providing additional details not typically mentioned even today:

Actually, Professor Piaget began his scholarly career as a philosopher and turned his attention to psychology and child development as a means of obtaining insight into some important philosophical problems. He has now reached the point where he wishes to return to philosophy and to use the results of his knowledge and research in psychology for an application to epistemology or the science of knowledge.

(Fahs, 1953)

This is a bit surprising, in no small part because Piaget is now usually presented—including in his autobiography (published the year before)—as having trained as a biologist. Note, however, that it wasn’t the RF’s unconsidered opinion. Rather, it was their conclusion following a detailed investigation leading up to their decision to fund his research.

In addition to placing the origins of genetic epistemology in the period directly after World War II, this archival document suggests that the CIEG’s founding represented a return by Piaget to his intent with his unfinished and little-known second doctorate in philosophy (see Liengme Bessire & Béguelin, 1996/2008). That discovery alone is of major significance for our understanding not only of the research program with reference to which he later defined himself, but also of what came before: Piaget engaged in several different ways with the same basic set of interests that his well-known psychological research program had sought to pursue (Burman, 2020b; Vidal, 1994). This means that the ancestor of contemporary developmental or child (or cognitive) psychology, and its subsequent applications in educational theory, has at least one younger sibling that has yet to make its major debut with English-speaking audiences.

In other words, a clear consideration of genetic epistemology—unencumbered by past interests and misunderstandings—may well be as productive as Stage Theory has been (see e.g., Cunningham, 2006; Hall, 2000; Lovell & Shayer, 1978; Murray, 1979). If only we can come to see it well enough to assess its continued significance: to *understand* before we *judge*. For this, though, we can follow the RF. Because that’s also what they were trying to do.

In short: by delving into these archives, we see how Piaget's goals were described and understood in a context that was very different from today's context. Because our contemporary perspective was then derived directly from the popularizations that followed, the documentation produced during the period of the RF's support—between their first grant for genetic epistemology in 1953 and the end of their funding in 1963—is also valuable for clarifying our understanding of what came both before and after. Indeed, it is especially useful for highlighting what got lost in translation. The goal here is therefore to use this evidence in order to see something that we couldn't otherwise have seen. We are then able to transcend the particulars to understand the larger program, taken as a whole, while identifying some reasons for our continued misunderstanding (which have broader consequences for all those who are interested in the development of knowledge and evolution of science). What follows is therefore intended both as a deep dive and as a microhistory: "*use something small to say something big*" (Burman, 2017, p. 119; emphasis in the original).

From the Development of Intelligence to the Genesis of Knowledge

Piaget's well-known *The Psychology of Intelligence* (1947/1950) is an edited collection of his wartime lectures at the Collège de France in Occupied Paris (see Meyer, 1951; Ratcliff & Jaccard, in press). His formal articulation of genetic epistemology was then developed afterward: "Since I had enough experimental data on the psychological processes underlying logico-mathematical and physical operations, it seemed the right time to write the synthesis I had been dreaming about from the beginning of my studies" (Piaget, 1952a, p. 255; cf. his 1925 inaugural address at the University of Neuchâtel).

The founding of the International Centre for Genetic Epistemology (CIEG) thus began in the "*4e trimestre*" (fourth trimester) of 1949, with the publication of the first volume of *Introduction à l'épistémologie génétique* (Piaget, 1950a). For the RF, though, this book was invisible. Piaget was known to them as he is known to us: for his contributions to psychology.

There are several documents in the RF archives that show us what they could see at the time. For example: Piaget's educational institute (the Rousseau Institute) had been supported between 1925 and 1939 by the Laura Spelman Rockefeller Memorial, and other RF funds, to the tune of \$109,200 (noted in the later approvals; EC, 1955, 1958). This is the equivalent of several million dollars today. After the war, RF officer Robert R. Struthers then visited Piaget in Geneva to—as he put it—"pay my respects" (RRS, 1947). He visited again in 1949, and they discussed the then-ongoing perception project (RRS, 1949; see Ratcliff & Hauert, 2006). But it seems there was no discussion of genetic epistemology worth mentioning.

Back in the United States, another officer—cited as RSM and probably Robert S. Morison—spoke at length in June 1950 with David Rapaport (1911–1960) about his efforts to make postwar European psychology more accessible to American readers (see Rapaport, 1951). During that conversation, Rapaport mentioned that Piaget's recent works were unknown because no new translations had yet been made.⁵ RSM's diary then concludes: "What little we know of Piaget . . . suggests that his work ought to be better known here than it is" (RSM, 1950).

A confidential internal memo by Else Frenkel-Brunswik (1908–1958), dated summer 1950, was similarly laudatory:

The outstanding experimental psychologist in Europe is Piaget, at Geneva. His experimental work on children's intellectual and moral approach [sic] is so well known that it need not be described here. Piaget's work on the stages of empirical and logical reasoning has implications far beyond the field of developmental psychology.

(Frenkel-Brunswik, 1950; emphasis in the original)

Someone at the University of Michigan, then unnamed, had also inquired with the RF as to whether they had a complete bibliography of Piaget's writings (they did not), and also if they could induce him to visit the United States.⁶ Such observations and inquiries led to the resumption of active contact, in response to which "Piaget was cordial but not loquacious" (EFD, 1950).

By December of 1952, in the process of following up about other things not specified, Piaget asked John Marshall—then associate director for the RF Division of the Humanities (DH) and a champion of transatlantic cooperation (see Buxton, 2003)—for his support with something a bit audacious:

Piaget said that if the DH officers would look into his publications, they would discover that his original interest was not psychology but in epistemology. As a younger man, he had turned from this original interest to psychological studies of childhood because of a deepseated [sic] belief that a knowledge of this subject was essential for the reformulation in epistemology which he feels necessary. Actually, his psychological studies had occupied him for many more years than had at the time foreseen. Now, in what is clearly a ripe maturity, he wishes to return to his original interest to formulate an epistemology based not on logic but on empirical evidence of psychology and to some extent sociology. He realizes that to carry out this aim he will need the assistance of other younger minds and therefore wishes to establish a team with which he can closely work during the coming years.

(JM, 1952)

Furthermore, he indicated that this was sufficiently important to him that he was open even to leaving Switzerland for France in order to pursue it, "since he is now dividing his time between the university of Geneva and the Sorbonne" (JM, 1952).

It's now shocking, for those who consider Piaget's name to be synonymous with Geneva, to read that anything could have induced him to leave. And so in considering his goals in advancing genetic epistemology, we are left to wonder about his motives in choosing to accept the invitation from the Sorbonne to replace Merleau-Ponty—just two months before his conversation with Marshall (viz. October of 1952)—and thus also to spend two days a week in Paris (see Meljac & Diener, 2000; Ratcliff & Burman, 2017). Although that position obviously wasn't made permanent, and indeed he left it soon after the end of the RF's support a decade later, these documents show clearly that this was a critical period.

In addition to understanding the importance of genetic epistemology to Piaget, in looking at these documents, we also get a sense of the RF's grant-making process at the time. This then affords the interpersonal context for what follows, although the records themselves often use a shorthand that must in turn be interpreted:

JM explained that work in this general field was primarily the responsibility of EFD and CG. He could not be sure but it seemed to JM that what Piaget had in mind was likely to be of interest to the DH officers. To explore this possibility, would Piaget be willing to write a brief memorandum setting forth his ideas. . . . Piaget made it clear that he was still in the first stages of formulating his plans and that perhaps the next step in any case might be further discussion with EFD when EFD was in Europe in the Spring of 1953.

(JM, 1952)

Recognizing that these were all written in the third person helps. JM is therefore John Marshall. The other two are his deputies in the DH (Division of the Humanities). Their names are then made clear in subsequent interactions: EFD is Edward F. D'Arms and CG is Chadbourne Gilpatric (who, like Director Charles Fahs, was also formerly of the Office of Strategic Services and then afterward served as a founding member of the Central Intelligence Agency). Indeed, these four—Marshall, D'Arms, Gilpatric, and Fahs, along with Vice President Weaver—were Piaget's key intermediaries in securing American financial support for genetic epistemology. They were, in short, the program's original "obligatory passage point" in navigating the changing interests of the postwar American audience, and in mobilizing these interests into grants that could be cashed out as collaborations and published results: human resources, and social relations, that needed to be husbanded even as the research they enabled was carried out (cf. Callon, 1984).

First Proposal, 1952

Piaget submitted the requested memo within the month, giving his proposed institute a name that is a bit different from what we now know: "*Projet d'un centre international d'épistémologie génétique et opérationnelle*" (Project for an International Center of Genetic and Operational Epistemology; Piaget, 1952b). This memo reinforced the subject of his call with Marshall and added some additional details: he referred to the three volumes of his *Introduction à l'épistémologie génétique*, as well as his recent lectures in Manchester that were soon after—under the supervision of Professor Polanyi, he noted—published as *Logic and Psychology* (Piaget, 1953b). He also mentioned his age, 56, and indicated that he sought to continue working until his retirement at 70 (i.e., 1966).⁷

Marshall circulated Piaget's plan, as promised. In the archives we then find a handwritten note back to Marshall from D'Arms:

I have read this over only once and am not quite sure I understand just what Piaget has in mind, but it strikes me it might be very good and very important. There would certainly be difficulty over long term—i.e. 12 year—support. CG's comments would be very valuable.

(EFD, 1952)

I didn't find Gilpatric's comments in the paper files, but his diaries are available online. These show that both he and D'Arms were busy at the time with the American Philosophical Association meeting. So perhaps they discussed the proposal in person. In any case, Marshall summarized their discussion in his letter back to Piaget:

Your very interesting "essay" on your interest in work in epistemology during the next years reached us just ten days ago. Since then all of us have read it and have spent a good deal of time discussing it. This is work which seems to us of very considerable significance. . . . At this point, however, I should at once make it clear that we are not convinced that we understand at all adequately what it is that you have in mind. So far as Gilpatric was aware, the book which you published on this general subject, . . . in 1949 and 1950, has not come to any general attention here. Gilpatric, who certainly attempts to keep up on things of this kind, readily acknowledged that he did not even know of its publication. He grasped, as I think the rest of us do, the general trend of your research, but until we have had some further exposition from you, we are only able to come to grips with this proposal in the most general terms.

(Marshall, 1953a)

Despite the cautionary tone, Marshall then proceeded to gather additional information. (What were these French books that Piaget had mentioned? Were they any good? And what about genetic epistemology itself? Is that worthwhile?)

One of Marshall's interdisciplinary interlocutors on the Continent, the physicist Pierre Auger (then director of UNESCO's Department of Mathematical and Natural Sciences), replied immediately to his request:

I have, indeed, read Piaget's book and think very highly of it. The author combines a vast and precise knowledge of mathematics and modern physics with a very typically Swiss gift for the observation of children's psychology. His work goes a long way toward the junction between the "human sciences" and the "exact sciences," and his effort is perfectly integrated, to my mind, in the general program of our time in this direction. May I add that Piaget has a very good reputation as a "scientific educationalist" or a psychologist of the development of the intellectual capacities of children.

(Auger, 1953)

Paul Schrecker, a philosopher at the University of Pennsylvania who had previously edited collections of Leibniz's and Malebranche's papers, was also enthusiastic:

The purpose of the new discipline proposed by Piaget [viz., genetic epistemology] is clearly and distinctly conceived: to emancipate epistemology from its systematic ties to general philosophy, and to establish it as a relatively independent discipline, following the example set by psychology, logic, and in fact all the particular [sic] sciences. This, of course, requires a theory of knowledge to devise its own special method, separate from that of philosophy proper—which after this secession actually would keep under its jurisdiction hardly more than a few highly controversial branches of knowledge such as meta-physics, ethics, and aesthetics; the latter two having been compelled some time ago to yield important parts of their domains to psychology, semantics, and the social sciences.

(Schrecker, 1953)

He continued:

The new, genetic, theory of knowledge proposes to study knowledge in terms of its actual, psychological construction, that is, as a development in time. It avails itself of two related methods: 1. The historico-critical method, which investigates the processes of knowledge in their collective genesis, and may be compared to phylogeny's role in the biological sciences [evolution], and 2. The psychogenetic method, a sort of embryology of knowledge, which studies the ontogeny of reason in the individual mind [development], a study to which Piaget's earlier work has made the most enlightening contributions.

(Schrecker, 1953; underlining in the original)

Still, the response was not universally positive. For example: Marshall and D'Arms also interviewed Robert J. Havighurst, the Chicago professor of education, who was then at work on his own stage theory of lifespan development. As he noted:

American psychologists in general are not very sympathetic to Piaget at the present time, and RJH feels that a grant to him would probably be criticized by American psychologists, although eventually approved of as an opportunity to carry Piaget's methods as far as possible. . . . RJH made the further comment that Piaget is certainly a ponderable figure and that his sociological judgments appear to him sounder than some of his psychological judgments. Because of the philosophical and basically moral orientation of Piaget's proposal, RJH feels that it could probably be considered by DH [the Division of Humanities], although undoubtedly consultation with psychologists and DMPH [Division of Medicine and Public Health] would be useful.

(JM & EFD, 1953)

In private, however, Havighurst also endorsed Piaget's honorary doctorate at Chicago—jointly with members of the Committee on Human Development, Department of Education, Department of Psychology, and Counselling Centre (including Bruno Bettelheim, Allison Davis, James G. Miller, David Riesman, Carl Rogers, Maurice Seay, and Lloyd Warner [Henry, 1953]). And so we see that the Americans were willing, in the end, to take a bit of a risk.

Marshall (1953b) thus concluded that interest was sufficient to merit a “relatively modest grant.” With the recommendation of the Division of the Humanities, the RF provided funds for Wolfe Mays of the University of Manchester to spend part of 1953 working in Geneva to make connections between Piaget’s genetic epistemology and the Anglophone world. The amount was \$1,700. That’s the approximate equivalent of \$16,000 today; sufficient only to cover Mays’ salary between April 15 and July 15, with some related travel for both Mays and Piaget (D’Arms, 1953).

Mays’ Report, 1953

The full text of Mays’ (1953) original report back to the RF, afterward, has been preserved. This overlaps substantially with a report published in the *Proceedings of the Aristotelian Society* (Mays, 1954b). But there are also several additions in the archival copy. The most interesting of these are from the conclusion. The whole is worth reviewing, though, as a further introduction to Piaget’s epistemological project that doesn’t suffer from the problems we have in seeing it today.

In explaining what he understood of the program to the RF, Mays provided what a reader new to Piaget would need to know in order to navigate the more foreign-feeling aspects of his work. Thus, for example, children’s classifications of their experiences are driven by their “overt activities in building up the conceptual machinery of thought” (p. 50). As a result, the Gestaltists’ perceptual wholes are to be reconsidered as being the result of these constructions: the *operational* summing of parts (p. 51). Mays then explained that Piaget’s use of the terms “operation” and “operational” referred to specific acts of thinking—having if not formal properties, at least formal descriptions—rather than representing a veiled reference to Bridgman’s operationalism (pp. 55–58). This in turn set up a complexification of Ryle’s distinction between “‘knowing how’ and ‘knowing that’” (pp. 50, 58–60), such that that difference between them is the result of an observable developmental sequence. (In a supplement, he said much the same thing of Piaget’s approach to Kant’s categories [Mays, 1954c, p. 95].)

The resulting set of transformations are describable *both* as a grammar of science *and* as a grammar of pedagogy: “logical conditions and their epistemological correlates,” which Mays (1954b) explained had become blurred in our understanding by “recent comparisons drawn between human minds and computing machines” (p. 60). This distinction could be extended further to the difference, due to Reichenbach and developed by Carnap, between non-psychological logic and instrumental pragmatics. Piaget’s version, however, was developmental: the former must ultimately be derivable from the latter. New formalisms must arise from action and be internalized non-computationally (cf. Piaget, 1949a).

In addition, Mays (1954b) explained, Piaget’s approach was informed by empirical studies of how psychological functions actually work: “he would argue,” for example, that “perceptual data are not as simple, straightforward and objective” (p. 61) as the Logical Positivists believed.

Even the notion of an object, one of the simplest forms of perceptual invariants, which is essential for the statement of the most elementary propositions, assumes a definite learning process. The young child . . . looks at things, follows them with his eyes, listens, tries to relate sounds to visual data, explores them tactually, rubs, lifts, shakes, etc. It is these activities when combined together which give rise to his spatial concept of the object. (p. 61)

In other words, objects are *constructed from* sensations and the movements that produce them, abstracted from many interactions, and combined in a new understanding that's separate from the sensations themselves. (Soon afterward, this became "abstracted and reflected at a higher level from that of the sensations" [paraphrasing; see Burman, 2016b].) This is then also how meanings are constructed, such as in the statement, "this tree is green." As he explained:

. . .the word "tree" here refers to a whole range of characteristics, having a trunk, branches, leaves, etc. It implies, in other words, a specific set of classificatory and relational activities by means of which we discover and differentiate such qualities. (p. 62)

Furthermore, these relations can be formalized using logic, but not—Piaget insisted—using a logic separate from the operations represented. It must instead be a logic that is used "as an instrument for co-ordinating and explaining the intellectual processes" (p. 63; cf. Piaget, 1949b).

From this perspective, for example, Piaget could be distinguished from Hull in his orientation to theorizing. Rather than applying logic to formalizing existing theory, Mays (1954b) explained, Piaget applied it to the *operations* needing to be theorized. The result, however, is not a logic of propositions. It is instead a logic of *groupements* (groupings), having four defining characteristics: composition, reversibility, associativity, and identity (p. 64; subsequently referred to using Piaget's acronym in French: "INRC" [see e.g., Easley, 1964]). Mays then made the connection from groupings to the truth tables used by computers, game theory, and early neural networks: "all the possible products of p , q , etc., set out in the form of a $g \times g$ multiplication table completely specify the group" (pp. 64–65, citing Jevons rather than Wittgenstein's *Tractatus*). This, however, could also be misleading. So Mays (1954b) expanded further:

To avoid confusion, it should be emphasized once again that Piaget is in no way identifying the logical structures constructed by him with actual thought processes. He is merely pointing out that some correspondence may be established between their respective structures. We cannot, by its very nature, substitute an abstract system for a first-hand experimental investigation of the psychological facts, since it only gives us a simplified model of structural detail, and neglects the complexities and subtleties of actual thought processes. (p. 65).

What this means is that Piagetian stage theory, at least in the immediate postwar period, was not expected to be *identical with* the observations of individual children. One is a scientific model of the other. (Hence the distinction that's made between the "psychological subject"

and the “epistemic subject” [e.g., Kitchener, 1993].)⁸ And thus, so too was genetic epistemology not expected to be identical with the history of science. However, although the map is not the territory—as Kuhn soon afterward riffed on Koyré (see Schuster, 2018)—the process of making a map must *interact with* the territory (see also Winther, 2020). And the qualities of this interaction, in turn, affect the mapmaker’s developing knowledge of that territory. (How?)

Mays (1954b) explained that *groupements*, for Piaget, organize the different stages of child development. This proceeds from simple to complex: called “sensori-motor,” “pre-operational,” “concrete,” and “propositional or formal” (p. 67). However, he also added two provisos that got lost in subsequent popularizations:

It should be noted that no universality is claimed for the above stages of intellectual development, they are to be interpreted as applying only on *the average*. . . . He would also agree that social environmental influences may accelerate or retard the child’s development from one stage to another. (p. 68)

From explaining Piaget’s conception of the development of children’s thinking, Mays then moved on to explain the construction of knowledge in general. This was accomplished by means of the child’s construction of the notion of “number.”

This way of explaining things has always made sense to me: the reversibility of operations is easiest to see in relation to number because that’s part of what we all use numeracy to do. Without reversibility, one can’t derive subtraction as the symmetrical relation of addition (or division of multiplication). One also can’t decompose one’s compositions. But, of course, math isn’t just a formalized system of operations. It’s also a body of knowledge that details the operations that are possible, along with their necessary consequences and the means of demonstrating that necessity. So, again, there’s a relation between the system that evolves and a user that develops: the math, and the mathematician.⁹

In an example that Piaget often quoted later, albeit without attribution, Mays (1954b) explained how the *number operation* is different from the *counting action*:

In order that objects may be combined, separated or put into correspondence, our activities have first themselves to be combined, separated, etc. As an example, consider a child who counts ten pebbles and finds they always make ten. In this case he is really not experimenting with pebbles, but with his own activities of arranging and enumerating; varying their order at will, and observing that no matter what combination of activities he employs, whether he numbers from left to right or from right to left, he always arrives at the same results. (p. 70)

This kind of observation had informed an entire book, which was among the first to be translated into English after the war (Piaget, 1941/1952b). And that in turn also has relevance to the philosophical problem of what numbers actually are in an epistemological sense (viz., class relations, correspondences, etc.).

Mays' (1953) original report to the RF includes a longer conclusion than did the published version. This foreshadowed some of the work to come—by unnamed cyberneticians, by Arne Næss (whom Mays and Piaget met in Oslo as part of this preliminary grant), and by Bärbel Inhelder (Piaget's deputy and chief collaborator on the experimental side)—as well as some justification of the program's importance. It also clarifies that the concrete example of number development was just a stand-in for a bigger issue:

In the *Épistémologie Génétique* we have a field of possible investigation in which logicians, psychologists, neurologists, linguists and historians of science can join together to make a multi-dimensional study of concept formation. Indeed, we might say that one of the reasons why the traditional epistemologist did not get very far in his investigations, was that the concepts studied by him referred to a large range of topics, with which he often only had slight acquaintance. For this reason, he tended to restrict himself to non-empirical studies, such as logic and mathematics, about which it was presumed he knew something. (pp. 63-64)

Then he concluded his report with the following:

The day when the epistemologist could produce learned treatises merely by collating data gleaned from various fields of learning, is now a thing of the past. With advances in scientific thought, brought about by continued specialisation, it is difficult to find any one man who is able to be master equally in all these fields. By wise collaboration, however, one can arrive at some synthesis of our ideas. A planned objective study of concept formation carried out by a group of specialists, may yet have important results for both science and knowledge in general. (p. 64)

In other words: the intended contribution of genetic epistemology—from Mays' point of view—was to make a science of the armchair philosophy of knowledge, but as a function *jointly* of its empirical and formal orientations *and also* of its potential for yielding to collaborative interdisciplinarity.

Given that basically nothing was known at the time of Piaget's larger project, in English, I thought Mays' report provided a fantastic introduction. At the RF, however, Gilpatric was underwhelmed; he was, as he put it, "slightly disappointed" (CG, 1953). This is because Mays hadn't covered any truly recent American philosophical advances, especially from Quine, Goodman, or Carnap. And that objection now seems prescient (if such a thing can be said of someone who holds the purse strings), given the issues that were subsequently popularized in relation to Carnap's (1947/1956) "intension vs. extension," Goodman's (1955) "grue," and Quine's (1960) "*gavagai*." But we can see allusions in Mays' report to the discourses in which those other authors participated. So it's clear, in retrospect, that Piaget's genetic epistemology can be understood as a parallel development related to, but separate from, those better-known discussions of the meaning of meaning.

It's not clear, however, if Mays ever received Gilpatric's comments. Still, we can see that his own further contributions did indeed cover some of the areas that needed development, especially related to Quine and Carnap (see in French Mays, 1957; 1962 in English; for their

correspondence on related matters, see Creath, 1990). And those efforts in turn became the basis for the most important early book of Piaget's in-house series—EEG Volume IV (Apostel, Mays, Morf, & Piaget, 1957)—about which much was said afterward.

Mays augmented his report with several commentaries, including in *Nature* (Mays, 1954a). There, he explained what was at stake: “it might be said, without undue exaggeration, that Piaget's psychological studies are the genetic counterpart of Russell and Whitehead's attempt in ‘Principia Mathematica’ to put mathematics on to a logical basis” (p. 626). Elsewhere, but equally as important:

The essence of Piaget's position might be said to lie in his claim that psychologists have in symbolic logic an instrument as useful as statistics. Though symbolic logic has already been applied to diverse fields—to language, to the design of logical and mathematical computers, to nerve physiology—Piaget has shown how it may be fruitfully applied to the analysis of intellectual activities.

(Mays, 1954c, p. 99).

These efforts at interpretation were also supported with new translations: not only of the lectures on *Logic and Psychology* that Piaget mentioned had been given in Manchester the year before (Piaget, 1953b), but also of an important later book with Evert Beth (1908–1964) entitled *Mathematical Epistemology and Psychology* (Beth & Piaget, 1961/1966), the autobiographical *Insights and Illusions of Philosophy* (Piaget, 1965/1971), and the short *Principles of Genetic Epistemology* (Piaget, 1970/1972). In addition, Mays authored several later essays about Piaget's long misunderstood social theory (Mays, 1974, 1985, 1986, 2000; Mays & Smith, 2001) and the relations between genetic epistemology and phenomenology (Mays, 1977, 1981, 1998b, 1998a, 2002). It therefore seems useful to consider these collectively as offshoots of the original Rockefeller-funded project to make genetic epistemology more accessible to Anglo American audiences (i.e. the RF's interest, not Piaget's).

Second Proposal, 1954

That summer, D'Arms met with Piaget while he was on vacation near Berne. Piaget then gave him a copy of Mays' report, while also indicating his hopes for an immediate follow-up:

P described in enthusiastic fashion what he regards as the significance of his psychological work with children on the comprehension of abstraction and the formation of concepts. The results he has obtained have led him to believe that the ability to deal with abstractions develops progressively and has a relationship to the psychological development and maturity of the child, especially in the pre-adolescent and adolescent periods. The pattern which P believes he has observed has suggested to him an isomorphism with certain logical patterns. Hence, what he would like to do is to test this hypothetical isomorphism by experimental work in conjunction with a logician.

(EFD, 1953)

Piaget's later summary of this discussion, marked *confidentiel* in French, was entitled "Bref rapport sur les centres européens d'Epistémologie susceptibles de collaborer avec le futur centre d'Epistémologie génétique de Genève" (Piaget, 1953a). This brief report on the European centers that might be amenable to future collaborations covers the same basic territory as D'Arms' contact sheet, but in greater detail. Especially noteworthy is an underlined passage in the conclusion: "à quoi correspondent les opérations logiques dans les mécanismes concrets de la pensée?" (p. 6). To what do logical operations correspond in concrete (operational) mechanisms of thought?

Answering this question would make it clearer what the role of logic was to be in genetic epistemology. But that had to wait, because Piaget felt inclined to accept an invitation to spend part of 1954 at the Institute for Advanced Study in Princeton. (This is also marked *confidentiel* in D'Arms' [1953] contact sheet.) Indeed, it was from there—not from Geneva—that Piaget later sent an English translation of an updated proposal (see EFD, 1954b).

The reason for the delay, beyond accommodating the needs of his translators, isn't clear. In any case, Piaget (1954b) explained his goals clearly and directly in English:

It is frequently asserted in various circles of philosophy and methodology of science that the study of the psychological genesis of concepts and operations is a purely psychological problem, without philosophic interest. In these circles, logic is considered the foundation of thought, and a psychological process is believed to acquire a logical meaning only when connected through language and collective rules with a logico-linguistic system of norms. (p. 1)

He continued:

But the logic which is thus so often invoked has become an abstraction, a "logic without subject." The central problem of genetic epistemology, on the contrary, is to connect knowledge to activity. It explains why there cannot exist a "subject without logic", and how the subject necessarily is led to construct a body of logic just as he constructs a system of elementary mathematics and physics. (pp. 1–2)

This was therefore the contribution to be made by the proposed research program: science can purport to know things justifiably (in the sense formalized by logic) because it is the product of interactions with the world (in the sense of empiricism), and it's these interactions—the actions of individuals—that should be studied empirically and represented formally in order to correct past errors and make new advances.

To put it another way: the original instruments of science are human perception, the minds that interpret the resulting sensory data, and the knowledge that guides them. In addition, these all change with development and social context. So their formal description, in logical terms, must support those changes and their contexts. In short, Piaget had showed—empirically, through his studies of children's reasoning—that Logical Empiricism was incomplete.

However, he also went a step further. In the process, he backed down from his earlier suggestion that he would leave Geneva for Paris:

The reason I believe a center of genetic epistemology is necessary in Geneva, besides the existing Institute of Psychology which it is my privilege to direct, and which has for 30 years been studying the genesis of mental concepts and operations in the child and adolescent, is the following: Research in genetic epistemology by nature must be interdisciplinary. It must associate in the closest possible teamwork specialists of different sciences. Its aim is to give results not solely of a psychological nature but having also an epistemologic [sic] or philosophic significance. To this end the association of psychologists with specialists of other sciences in a permanent center would be necessary, permitting a common effort of analysis of the development of concepts and operations in man. (pp. 2-3)

This was followed by the outline of a general plan of study, including research methods and focused topics. But he also clarified:

We do not think of course of a return to gross psychologism. We believe in the ideal of logical formalization and the autonomy of logic are definitely established. But it must be recognized that formal logic has become entirely abstract, and the problem remains to know to what logical structures correspond in the actual thought processes of man. (p. 5)

Genetic epistemology would therefore return the knower to the study of knowledge.

Furthermore, anticipating a study that would soon be Americanized—famously without support for developmental change—by Chomsky and later Fodor, he added:

A considerable amount of research work has already been devoted to the relations existing between the structures of logic and those of language. Evidently, further research must continue from here. The logico-linguistic correspondence is but a first approximation, for it is probable that structures of language are rooted in deeper processes. This is the core of the problem. (p. 7)

The proposal concluded with a two-year budget in the amount of 176,000 Swiss francs. This includes line items for specialists in the relations between logic and language; between logic, physiology, and cybernetics; between logical formalization and scientific or common norms of thought; and a research psychologist (p. 9). In addition, sufficient funds were earmarked for an annual symposium and the associated publications arising therefrom (should commercial publishers not be interested).

Reaction, 1954

Piaget's proposal was met with some trepidation. It was a big ask for a short period (after having previously hinted at a much longer duration [WW, 1954a]). Yet, after an interdivisional luncheon at which several of the RF's leaders met with him in person—to have “a complicated bi-lingual conversation on a complicated subject” (RSM, 1954)—the resistance among those

present melted away. Marshall was then prepared to “go ahead and recommend aid for Piaget’s work if other divisions were prepared to give DH some moral support” (quoted in GRP, 1954).

Still, the due diligence continued. This time, though, Piaget was nearby and interacting closely with the RF’s local social network. D’Arms then interviewed the director of the interdisciplinary Institute for Advanced Study where Piaget was temporarily resident: J. Robert Oppenheimer (1904–1967), who is, of course, better known today for his management of the Los Alamos Laboratory (of the Manhattan Project) a decade earlier.

EFD asked O if he had any opinion as to the importance or validity of Piaget’s project at Geneva during the next two years. O replied that in general terms he is always unhappy when an experimentalist forgoes the experimental technique, but actually he believes that Piaget is so thoroughly committed to it that he will not abandon it completely, whereas, on the other hand, his methods during the past years have not been experimental in the ordinary sense of the term. . . . O recommended Gerald Jerome S. [struck out and handwritten] Bruner and Morton White at Harvard as good persons to consult about Piaget’s future plans.

(EFD, 1954a)

It was Marshall (1954a) who then wrote to Bruner. His reply is interesting for its apparent focus on Piaget’s (1942, 1949b) earlier attempts to formalize the development of cognitive processes:

I think that Piaget’s objectives are quiteth closely akin to those of the English mathematician Bool [sic], who in the 19th century thought that he could set forth a symbolic logic that somehow mirrored the natural course of thought. Bool’s book, which he actually entitled *The Laws of Thought*, was of course a landmark insofar as it represented the beginning of symbolic logic. . . . Piaget is much more sophisticated. . . . What he is trying to do is to set forth what amounts to the necessary sequences in the development of thought that are required for the handling of conventional problems. . . . His effort, then, is not so much to state the natural laws of thought but to state some of the inevitable sequences in the development of thinking operations.

(Bruner, 1954; citing Boole, 1854)

Piaget had recently made this comparison himself, writing in French, albeit as part of a broader discussion of cybernetics (Piaget, 1953c, pp. 385–386). Mays also wrote similarly in the published version of his report to the RF: “Piaget applied logical systems to child-behaviour much as Shannon applied Boolean algebra to electrical switching circuits” (Mays, 1954b, p. 65).

However, the advancement of some sort of neo-Boolean developmental cybernetics wasn’t Piaget’s intent with genetic epistemology. (Cybernetic theory was involved, but not as the program’s aim.) Such an endeavor also shouldn’t be, in Bruner’s view, the main purpose of the grant. Instead, its goal should be to reduce Piaget’s social isolation in both Switzerland and Piagetian groupthink:

I think that it has been one of the failings of Piaget that he has isolated himself too much from some of the deep currents within psychology and philosophy, surrounding himself instead with a group of disciples in Geneva. From this point of view, I think there is a great deal to be said in favor of getting some people over there with him.

(Bruner, 1954)

This was Marshall's view too:

. . .the proposal he has placed before us seems to aim at remedying the isolation in which he has worked: as I understand it, he now hopes to bring to Geneva people who would represent other related currents, not only in psychology but in mathematics.

(Marshall, 1954b)

Indeed, in retrospect, sustained social interaction does seem to have been critically important for developing mutual understandings of these complex issues.

In another study, I found this to be the case in addressing the early criticisms of Piaget's logic (Burman, 2016b). However, a further short vignette helps to demonstrate its relevance to the question of this grant in particular: Piaget was clearer in person, and more convincing, than he was in his writing.

Briefly: the first luncheon seems to have been crucial for addressing the RF officers' concerns. But Warren Weaver had not been able to attend. And he had unresolved concerns regarding Piaget's use of logic, having previously illustrated the conflict by comparing his own views with Russell and Piaget's with Brouwer (WW, 1953; responding to Piaget, 1952/1953).¹⁰ As a result, he continued to worry: "this scares me a little" (WW, 1954a). A second luncheon was therefore convened on December 15: the day before Piaget's departure from Princeton, despite—it was mentioned—the inconvenience that this late meeting no doubt caused.

The most detailed documentation of the resulting meeting is not in the RF records, surprisingly, but in Piaget's. As he put it in one of the books that Mays translated:

I remember the practical questions: How will you find people sufficiently intelligent so as to achieve a real collaboration and at the same time sufficiently stupid to abandon for a year their studies in mathematics or logic, etc., and to embark on a dialogue with “child psychologists”? But I particularly remember the theoretical questions, due among others to Weaver, the mathematician interested in information theory. . . : How will you find interesting epistemological ideas, for example, the theory of relativity, in studying children who know nothing and who in any case are brought up in the intellectual tradition dating from Newton? What do children think of set-theory and of the one-to-one correspondence used by Cantor, et al.? I had the luck to be able to remark on the first point that Einstein himself had advised me in 1928 to study the formation of the intuitions of velocity in order to see if they depended on those of duration, and that further, when I had the good fortune to see Einstein again at Princeton. . . , he was quite delighted by the reactions of nonconservation of children of four to six years. . . , and was greatly astonished that the elementary concepts of conservation were only constructed toward seven or eight years. As for the second question, I was able to answer Weaver that children readily handle one-to-one correspondence, and that the study of this problem shows that the transition from logical class to number in the child is much more complex than the formal account of this relationship given by Whitehead and Russell in *Principia Mathematica*. In short, I tried to cope

(Piaget, 1965/1971, p. 30)

It seems, however, that Piaget did better than simply cope. Weaver’s diary reports afterward that he was “pleasantly surprised,” “simply fascinated,” and “most enthusiastic about joining in on a recommendation” (WW, 1954b).

By the 21st of January, 1955, a comment by Fahs appended to a memo from D’Arms indicates that Weaver had secured a copy of Mays’ translation of the *Logic and Psychology* lectures (CBF, 1955). Within the week, D’Arms then wrote to Piaget to indicate that the RF was prepared to offer 270,000 Swiss francs for *three* years, building on the original two-year proposal (D’Arms, 1955). All that was needed was for a request to be made officially by Geneva’s rector.

This request was received in short order. And the executive committee of the RF approved it, in the higher amount of 288,000 Swiss francs, on February 25, 1955. (This is roughly \$650,000 today.) Originating from the Division of the Humanities, however, the grant was explicitly for “a program of cooperative research in philosophy, logic, psychology, and related fields” (Rhind, 1955). Piaget then objected, albeit with a wink: *if it must be called philosophy, rather than genetic epistemology, then at least say philosophy of science!* (paraphrasing Piaget, 1955).

Initial Operations, 1955–1957

A draft press release—in English, but on Genevan letterhead (an unusual combination)—arrived at the RF nearly a full year later (on February 10, 1956). There, we again see Piaget’s self-presentation for an American audience not accustomed to his French:

The centre, which was opened on Oct. 1st. 1955, is an interdisciplinary one, and is closely associated with the Department of Psychology, University of Geneva. It consists of a team of research workers (from the logico-mathematical, natural and psychological sciences) interested in the experimental and theoretical study of developmental behaviour. . . . The general topic of study for the present year is the relation between logical structures and the behaviour and thought of the subject in his development.

(Centre International d'Épistémologie Génétique, 1956)

The press release also indicated that the proposed logicians' roles had been filled by Mays, Leo Apostel, and Benoît Mandelbrot (now known for his later invention of fractal geometry), and that the psychologist's role was filled jointly by Inhelder, Albert Morf, and Jacques Rutschmann. D'Arms' diary then gives us the outsider's view of how this team's interests aligned with the center's goals:

Mandelbrot of the University of Paris, a native Pole but now a French citizen, is a mathematician who has also done considerable work with language. He is completing a study on information theory and the development of language. Apostel, a logician from the University of Brussels, is working on two subjects: "la logique et l'apprentissage", and (with Mandelbrot) "la logique et la precorrection des erreurs (décodage)", which involves "operations sémantiques." (EFD uses the French terms which Piaget used, since even after P's explanation, EFD was far from certain just what was involved. . . .) Mays, whose field at Manchester is logic and cybernetics, is working on a critique of Carnap from the point of view of genetic epistemology. Morf and Rutschmann, both psychologists from Zurich, have been working with Piaget on his introduction and his chapter on "equilibrium."

(EFD, 1956a; underlining in the original)

The resulting papers were presented at the first annual symposium, which was also attended by Evert Beth, François Bresson, Jerome Bruner, Ferdinand Gonseth, Aimable Robert Jonckheere, Paul Lorenzen, Arne Næss, and Pierre Oléron (Piaget, 1957a, pp. 5-6).

Following a request from D'Arms, afterward, Mays (1956) once again shared his thoughts. We then see an Anglo perspective of the CIEG's first year:

As far as the end of the year symposium was concerned it could certainly have been described as a success, especially on its social side. At the symposium most of the years [sic] work was reviewed and discussed. Some of the guests remarked that there seemed to have been too much emphasis on the theoretical aspects of the questions discussed and not enough on their experimental testing. Nevertheless, they showed themselves favourably disposed towards the future of the centre and parted with offers of help and collaboration. (p. 5)

In response, D'Arms agreed that "experimental work to test the various hypotheses proposed in these papers would seem highly desirable" (EFD, 1956b).

That first year also produced four books in the EEG series, and the first three are indeed highly theoretical.¹¹ (With the exception of a chapter by Morf, there were no contributions from the team's experimentalists.) However, Volume IV was conceived explicitly as a collaboration between the logicians and the psychologists (see the note by Piaget in Apostel et al., 1957, p. 3). This is also the volume in which Piaget expressed the most pride (Piaget, 1957d). Moreover, it's the one that the RF came to lean on most heavily in assessing the value of their investment. Looking with the benefit of hindsight, however, it's also clear that many directly related experiments were published separately—either in the Genevan journal (*Archives de psychologie*) or in parallel books with commercial publishers interested in Piaget's psychology (see esp. Inhelder & Piaget, 1955/1958, 1959/1964).

Extension, 1957

The following year, the team continued to advance their collaborative interdisciplinary project. However, greater emphasis was placed on finding a “fusion” between the experiments and the theory, as Piaget (1957c) noted in a letter to D'Arms.

With Apostel promoted to professor in Brussels, the theorists for the second year were Mandelbrot, Jonckheere, and Bresson. Morf continued as psychologist, and Rutschmann was replaced by Prince Vinh Bang of Vietnam (Piaget, 1956). Inhelder, too, continued as the chief collaborator on the experimental side: “*naturellement*,” wrote Piaget (1957c, p. 2). Næss also joined the Center for the full year. The annual symposium then brought together philosophers Apostel, Gonseth, and Richard Braithwaite with psychologists Ivo Kohler, Howard Gruber, Paul Fraise, and Frederick Langdon (Piaget, 1957e).

Their efforts produced another two books, delivering on the promise to do more experiments, including Geneva's contribution to the “New Look” that had emerged at Harvard (noted by Piaget, using the English term, in his foreword to Bruner, Bresson, Morf, & Piaget, 1959). Yet these subsequent books did not receive the same attention as had the first four. (Piaget later described Volumes V–X as empirical investigations of the assumptions of Verificationism, which I suppose should have been equally as interesting [see his 1965/1971, p. 127].) Nor did the dozens that followed. Like the first book that inaugurated the program, *Introduction à l'épistémologie génétique*, most of the subsequent contributions to the series remained mostly invisible to Americans.

After that summer, the RF began contemplating the renewal of Piaget's grant. D'Arms also departed for a position with the Ford Foundation, so Piaget lost his closest supporter. But Marshall remained enthusiastic:

I still have the impression that this is one of the most important projects that has come to the Foundation from Europe in recent years (I am sure WW [Warren Weaver] would concur) and feel, therefore, that we should be looking forward to the consideration of a new request, possibly as early as the autumn.

(JM, 1957a)

Gilpatric, however, was more skeptical. Or perhaps, as a former professional spy, he was less amenable to Piaget's oft-remarked upon personal charm. (He would also certainly have preferred to support Wittgenstein, who died of cancer just months after their meeting in 1951; see Leach, 2020.) As he had written to Marshall the day before:

I would be glad to undertake a close analysis of Piaget's theories and program and then begin a rather extensive process of consultation, largely through correspondence, with individuals mainly in the U.S. who have been associated with his work. It is quite possible that we have not taken enough outside opinion on Piaget's program for research in genetic epistemology. Now that publications and symposia have resulted, they surely should be carefully appraised. . . .

(CG, 1957a)

Marshall then replied with a handwritten note: "I very much hope you can get opinions so that you can brief me thoroughly before Nov 20th !" (written directly on the archival copy of CG, 1957a).

Gilpatric thus began his letter-writing campaign. In reviewing the resulting documents, we discover that his American interlocutors were either insufficiently familiar with the recent French editions to comment or they misunderstood the program as a continuation of Piaget's psychology. This seems odd, given that Marshall had quickly found commentators with useful insights to share. But perhaps the difference is a result of broader changes then still ongoing: Sputnik was orbiting, and beeping, even as Gilpatric wrote. It's clear that the pressure on him would have been quite different, and much greater, than it had been for D'Arms and Marshall. (We see in these files that he was aware in advance that, within ten days of Sputnik's launch—i.e. on October 14, 1957—the Ford Foundation would announce a \$65,000 expansion of an earlier \$30,000 grant for Piaget to support Inhelder's psychological experiments, totalling roughly \$895,000 today [CG, 1957b].) Despite this, though, there is no mention of Sputnik in connection to genetic epistemology.

Indeed, the reply from Bruner remained surprisingly consistent across what we now consider to have been an important rupture:

It is very difficult to write clearly about Piaget's work and his contribution. I have great difficulty following in detail much of the work he does on genetic epistemology. I think, however, that I can express its objective pretty simply. It is to state the underlying grammar for logic which must be operative at different stages of development in order for a child to perform cognitively as he is observed to perform. Much additional effort is put into the task of attempting to describe the formalism of the underlying logic either in mathematical or symbolic terms. In this last respect, with respect to formalism, Piaget is in the role of an empirically oriented George Boole.

(Bruner, 1957)

Yet he also continued in a more critical vein, reflecting what now looks like a move away from Piaget's interdisciplinarity in favor of ever-greater focus on what it was that Inhelder was doing:

I confess that I found the Symposium in Geneva the summer before last very frustrating indeed. The closer we got to his observational work and its prediction [sic], the happier and more excited I became. But when we got off onto very high-brow logic and mathematics, I had great difficulty seeing how it was related to the other work. Perhaps I am massively wrong and that the formalistic part of his work is truly revolutionary. This is possible. Of one thing, however, I am quite convinced: the empirical research of his, going into the immediate interpretation of this work, is of the highest order of importance and fruitfulness. Perhaps this earns him the right to explore what formalism appeals to him!

(Bruner, 1957)

It seems this skepticism is what Gilpatric then brought to his meeting with Marshall. As he put it in a follow-up letter: “Piaget should be asked to formulate in layman’s terms what he is driving at with some concrete problems and illustrations” (Gilpatric, 1957).

Marshall thus wrote to Piaget to ask for clarity: “in considering the needs of your work it is necessary, as you will understand, for us to be able to explain it in terms that our colleagues can grasp” (Marshall, 1957a). They soon afterward met in Geneva to discuss the problem in person, and how the RF could help Piaget to communicate his intent more clearly. More specifically, Marshall asked him to respond to three key questions: “(1) to what extent is the study of learning in children relevant to adult thinking? (2) What philosophical contributions does P now expect from this work? (3) How is this outcome likely to affect thinking in logic, mathematics, science, etc.?” (JM, 1957b).

Following their meeting, Piaget therefore wrote an “*aide-memoire*” (memory aid) addressing the issues arising from their discussion (Piaget, 1957f). Marshall in turn translated this and distributed it to his team. That translation then became the RF’s new organizing principle in considering their support of genetic epistemology:

The aim of our Center is to draw epistemological conclusions from psycho-genetic experiments. . . . In studying to what, in the logic of the subject, correspond certain of the structures or concepts of the logician’s [adult] logic, we wondered, for example, if there exists, at different levels of development [in children’s thinking], the same radical distinction between “analytical” and “synthetic” relations which Carnap establishes at the level of scientific thought or if intermediates are to be found among them, as W.V. Quine supposes. Experiments undertaken have yielded results that are very clear and relatively new. . . . We are awaiting with great interest the reactions of specialists in this problem to the appearance of our Fourth Study: Professor W.V. Quine has just sent us on this subject a very encouraging letter which shows that our method has been well understood by this leading logician.

(Piaget, 1957g, p. 5)

Indeed, it seems clear here that genetic epistemology—especially as expressed in Volume IV of the series (although Piaget noted that focusing solely on this would be unfair)—was an investigation of philosophical assumptions by experimental means.

At this time, specifically, the goal was the experimental engagement with the first of what Quine (1951) had recently called “the two dogmas of empiricism.” (Namely, they were decomposing and complexifying the Kantian distinction between truths grounded in meanings, or the analytical a priori, and truths grounded in facts, the synthetic a posteriori [see also Piaget, 1965/1971, pp. 31–32].) In a follow-up letter, Piaget then shared a copy of Quine’s comments about Volume IV. This is marked *confidentielle* at the top, in Piaget’s distinctive handwriting. But we can see why Piaget thought it was encouraging. As Quine said:

I have read the book, and am much pleased with it. I completely agree that experimental psychology is the proper domain for the analycity [sic] debate. I admire the precision and justice with which the controversy is surveyed in the opening chapter and the ingenuity with which, in ensuing chapters, the experiments were managed. I found good entertainment in the experimental details, especially the children’s answers regarding the buttons. Finally I thought the last chapter a masterly summing up of findings and residual disagreements. Above all I am gratified to learn what experiment shows about analycity [sic] and I am glad that so able a crew saw fit to take the problem on. (Quine, 1957a; note that throughout the “Two dogmas” paper, he refers to this as “analyticity,” which is how we refer to the concept today.)

Marshall then wrote to Fahs from Paris:

I hope you and Gil will agree that we ought to give high priority to the consideration of further support for Piaget’s project during the next weeks, in order that, if it is at all possible, we may bring it up at the February [executive committee] meeting and thus avoid any discontinuity in the work it is to support. . . . I should perhaps add that I am virtually convinced from comment here (see my diary) that this is work of importance and, consequently, that our support should not lapse.

(Marshall, 1957b; underlining in the original)

He enclosed a dozen pages from his diary, which includes detailed observations and commentaries about his interactions with Piaget’s team. (Officer diaries are now available online; see Marshall, 1957d, pp. 200–205, in PDF format [pp. 52–57, his pagination].)

Meanwhile, back in the United States, Gilpatric continued to follow-up with commentators. When Quine replied, he first excerpted his letter to Piaget in which he had endorsed Volume IV of the EEG series. Then he concluded:

I am too busy to undertake any further examination of the series, but I hope the favourable impression reported above may be of some help to you when adjoined to other evidence. My own hunch is that a renewal would be all right.

(Quine, 1957b)

This seems lukewarm, but Quine has a reputation for having been curmudgeonly.

Apostel, who had co-authored Volume IV and seen the program from the inside, was much more enthusiastic. We see this clearly in his corrections to Marshall's (1957c) translation of his own letter: "The Center is one of those rare places where logicians, epistemologists, and psychologists can collaborate" (p. 1) and "the Center, in my opinion, is one of those few efforts where the spirit of the exact sciences confronts the spirit of the human sciences" (p. 1). He then explained why this mattered:

I should like to say at the outset something ~~of~~ about the usefulness that the Center has for formal logic. Formal logic is in a state of crisis. Its central concepts: class, ~~consequence~~ *entailment*, and (~~illegible~~) *constructivity* are susceptible of multiple interpretations. Logicians are divided and are trying to reach agreement ~~so as to put~~ *by putting* their discipline into relation with others. (p. 1; strikeouts and italics indicate handwritten corrections by Apostel)

He continued:

I am of the opinion that the entire intuitionist trend of modern logic (Brouwer, etc.) is based on psychological concepts that the Center for Genetic Epistemology alone is subjecting to direct study. (p. 2; underline in original)

And finally:

I would add a) that the teaching of logic will benefit from the results of a study of these psychological correspondences, and b) that the study of ~~this mechanism~~ *a* retroactive, complex, half-statistical, half-determinist, half-~~analytical~~ *analogical*, half-digital *mechanism*, as is man himself, will develop relations between *the study of* logic and *of* calculating machines. (p. 2; strikeouts and italics indicate handwritten corrections by Apostel)

These endorsements seem to have been more than sufficient, despite Gilpatric's objections in handwritten notes (made directly on Piaget, 1957g).¹²

Fahs presented the grant extension at the executive committee meeting on February 27, 1958. A new 5-year plan was then approved: 600,000 Swiss francs, or \$141,000, now approximately \$1.35 million (EC, 1958). This resulted in yet more books, including on the construction of number—reporting research spanning 1958–1960—that indeed seem consistent with Brouwer's intuitionism (see esp. Beth & Piaget, 1961/1966; Beth, Grize, Martin, Matalon, Naess, & Piaget, 1962; Gréco, Grize, Papert, & Piaget, 1960; Gréco, Inhelder, Matalon, & Piaget, 1963; Gréco & Morf, 1962; cf. Van Dalen, 1981). But the archival records also show that what Piaget's team was actually doing was considered less important, inside the RF, than how their efforts were perceived piecemeal, indirectly, and from an increasingly distant perspective.

This is clearest at the conclusion of the 5-year extension. That feels like a big jump, but the archival details in between are sparse: mostly annual reports and accounting ledgers. There is little to no engagement with the many books published along the way, nor were there any requests from the RF regarding how Piaget would respond to either Sputnik or the National

Defense Education Act (or to the Cuban Missile Crisis, all of which—as the CIA’s founding Deputy Chief of [Clandestine] Operations, 1947–1949—must have been of great personal and professional concern to Gilpatric).

Declination, 1962–1963

By October 1962, Piaget was again looking for money. By then, however, Gilpatric had already decided to withdraw the division’s support. Perhaps unsurprisingly, this was met with objections from the other divisions that had supported the previous grants. For example, Robert S. Morison (by then the RF director of medical and natural sciences) replied: “I guess I would have to say that I was a little sorry to find that you feel that it is impossible to continue support for Piaget” (RSM, 1962). So Gilpatric went through the motions.

This renewal was different from the previous rounds. Marshall could no longer serve as Piaget’s champion: in 1959, he had taken up the founding directorship of the RF’s Bellagio Center in Italy. (This didn’t stop him from reaching out anyway [Marshall, 1963].) D’Arms had also previously left in 1957 for the Ford Foundation. Weaver had retired in 1959, then taken up the vice presidency of the Sloan Foundation. And Fahs resigned in 1961 to serve as a senior cultural attaché in the U.S. Embassy to Japan. In other words, of Piaget’s original key interlocutors, only the critic remained. Furthermore, the RF itself was reorganizing and shifting their focus away from postwar Europe. (Gilpatric himself soon afterward decamped for India.) Several gentle letters explaining the situation were written, but they remained unsent.

In seeking comments, Gilpatric cast a much narrower net than had Marshall. Yet Bruner’s comments, by telephone from Geneva, reflected the same basic interest as Marshall’s (who had pushed Piaget to clarify) in the previous round:

B thinks it important that Piaget be asked what he sees as the integration and major product of the work he has done in recent years as a general summation of many short articles and monographs put out in close to twenty published volumes. In another form, B thinks Piaget should be asked in what form and where does the payoff of this work in genetic epistemology come. This could either be looking backwards to the present or to what would result from another three-year stint.

(CG, 1963a)

It therefore seems reasonable, in this light, to understand Piaget’s subsequent expository books as delivering on this persistent request: not only *Insights and Illusions of Philosophy*—which Mays translated (Piaget, 1965/1971)—but also the first edition of the *Sociological Studies* collection (published in 1965, then translated later from the expanded third French edition [Piaget, 1977/1995]). That said, however, Bruner’s reply probably also ought to be understood against the backdrop of Lev Vygotsky’s (1896–1934) recently translated criticisms, in *Thought and Language* (1934/1962), of much earlier works by Piaget (1923/1926, 1924/1928).

As Bruner (1960, 1962) made clear, including in his introduction to Vygotsky's English edition, American psychology and educational theory were actively in need of a mechanism to explain epistemic change in individuals. A connection therefore needed to be made between learning and development, not just between individual knowledge and shared science, in which teachers could intervene. That then also seems to be what he was asking for in his conversations with Gilpatric: *How can genetic epistemology be applied to the issues of the day?* (At the time, Bruner was serving as a member of the Educational Panel of the President's Science Advisory Committee under President Kennedy, for whom he had previously campaigned [see Bruner, 1983, pp. 184, 186].)

In retrospect, Bruner's discussions with Gilpatric seem now to represent a shift away from the RF's original interest in supporting Piaget's interdisciplinarity, championed by Weaver until his retirement, toward ever more concrete experimental demonstrations of cognitive growth to inform education reform (cf. Bruner, Olver, & Greenfield, 1966). This mission creep could have been defended against if those concerns had been made explicit, including on epistemological grounds (in the EEG series, see Apostel, Jonckheere, & Matalon, 1959; Goustard, Gréco, Matalon, & Piaget, 1959; Gréco & Piaget, 1959; also Piaget, 1964; 1967a; in more detail by Inhelder, Sinclair, & Bovet, 1974/1974). However, Gilpatric—now in control of DH's recommendation regarding the RF's purse strings—seems not to have been looking for reasons to renew. Indeed, a single letter from Quine tipped the balance in favor of declining: "As anticipated, Willard Van Quine in the attached letter gives us what seems to me the conclusive appraisal needed of Piaget's work in connection with the latter's request for additional support" (CG, 1963b). Morison's handwritten reply: "this is it all right" (RSM directly on CG, 1963b).

We must, of course, read these comments differently today, if for no other reason than because we can see better how the interlocutors were positioned. Briefly: Quine took the opposite position from Piaget regarding the value of Brouwer's intuitionism, the experimental examination of which Apostel had described in 1957 as being crucial to resolving the then extant crisis in logic. (See Quine's comments on mathematical constructivism—which, decades later, he still called "unattractive," "ingeniously circuitous," and "deviant"—in his 1987 work, *Quiddities: An Intermittently Philosophical Dictionary*, pp. 33–36, 55–57.) Yet the importance of its consideration, despite its inelegance, had also been made clear in Piaget's (1957f) *aide-memoire*: the Genevan experiments suggested the necessity of a formal position *between* the two poles of the crisis, which is to say *between* the otherwise incommensurable positions of Russell and Brouwer, resulting in what would look to an outsider like an experimentally derived Frankenstein's monster composed of pieces half-foundational and half-intuitionistic.

Given this, and absent an internal champion (or even representation from the opposing side of the crisis), we can now easily understand why Quine's (1963) reaction justified the conclusion of the RF's support with no further investigation. As he put it:

Originally I had been doubtful about Piaget's project because of his irresponsible ventures in logical theory. The first volume or two of his seventeen reassured me somewhat, for I saw that the collaborators were able to proceed fairly independently of the master's logic. Thus it was that I eventually accepted his invitation to go to Geneva for a week, in 1960; but not without having declined twice previously, nor without earnest prior consultation with Bruner viva voce and with Beth by mail. (p. 1)

He continued:

Listening to the reports and discussions that week in Geneva reassured me further. Piaget's logical crotchets obtruded none. The work was strictly experimental and, to my lay ear, interesting and sometimes ingenious. I declined his subsequent invitation to a second such junket only because I saw more clearly by then that I would not be veering nearer to psychology in my own foreseeable work. (p. 1)

And then:

Meanwhile his volumes continue to drift in. Mostly I neglect them. But now I've been examining volume xvi [entitled *Implication, formalisation, et logique naturelle*], and again I'm scandalized by the logic, as I was years ago. This book is partly logical theory and partly experimental; and the former is inexcusably primitive, confused, and uninformed. It is wrong to support this irresponsibility.

(p. 1; citing Beth et al., 1962)

Finally, in conclusion:

I find it hard to condone so vast and expensive a program of ill controlled publication. Evidently Piaget feels he is justifying subventions by publishing fast and furiously. The worst of it is that his subject invites many purely logical chapters, and he is helpless in logic; helpless and opinionated too. I think we must consider at last that the good in Piaget's program is insupportable because of the mess that it is submerged in. . . . I give up. (p. 2)

Gilpatric replied to Quine on March 1, 1963. He then sent his declination letter to Piaget the following week. The Rockefeller era was over. (There were no letters to Apostel or Mays.)¹³

Afterward

Conceived of as part of a process whereby *the knowledge* of genetic epistemology was translated, even as most of *the books* that constituted it weren't (and indeed were often ignored), these archival traces show us how Piaget both succeeded and failed to engage the interests through which his institute's early finances flowed. What mattered ultimately for his hoped-for renewal by the RF was not his team's prodigious output, or its importance to insiders, but his social relations and the resulting understanding of his goals: the means by which outsiders were brought inside.

This had been the RF's original understanding of the purpose of these grants. And indeed, when Piaget collaborated in person, everything went fine; even when in collaboration with Beth he had wrestled with fixing his operational approach to logic, which Quine still—albeit from a distance, and apparently not realizing the degree of Beth's involvement (despite their discussions by mail)—called “irresponsible,” “helpless,” and “opinionated” (also “incorrigible,” quoted in Burman, 2016b, p. 760). But assessing this sort of thing both completely and remotely was nigh impossible: there was too much to read, it was too disorganized, and anyway it seems the interdisciplinarity resulted in blendings too complicated to survey from afar (see Ratcliff & Burman, 2017). Too much that was necessary for a clear understanding, it seems, was left off the page and remained tacit (cf. Polanyi, 1958/1962; 1966/2009).

Of course, doing everything in a “foreign” language could not have helped. Indeed, Piaget's insistence on speaking only French was often remarked upon (by Oppenheimer, Marshall, D'Arms, Gilpatric, and others). This no doubt also served to increase his *perceived* isolation, to the Americans, even as he traveled widely during this period—including, perhaps quite problematically for Gilpatric, to the USSR (see Piaget, 1956/1956)—in his capacity as director of the International Bureau of Education and president of the International Union of Scientific Psychology (see, respectively, Hofstetter & Schneuwly, 2013; Rosenzweig, Holtzman, Sabourin, & Bélanger, 2000). And that perception was clearly part of what needed to be managed: to ensure that the money continued to flow, the maintenance of this postwar “obligatory passage point” should not have been neglected as his internal champions retired or moved on to other projects.

New money for the Genevan enterprise was soon found elsewhere, however, as new audiences discovered their own related interests. Yet the challenge of managing interest-inflected knowledge translation continued: John Flavell's (b. 1928) influential *The Developmental Psychology of Jean Piaget* (1963) was published just four days after the date of Gilpatric's declination, on March 12, and this included Piaget's (1963) now infamous comment that its presentation was “too exclusively psychological and insufficiently epistemological” (p. viii). Its subject was then also soon afterward “rediscovered” along those lines (Ripple & Rockcastle, 1964). But as Kessen (1996) made clear, this post-Rockefeller period of Piagetian popularization represents an entirely different chapter in the history of American psychology. That, instead, is the story of the now obvious “cognitive giant” against whom a resurrected Vygotsky (1934/1962) could come to serve usefully as a prosocial foil: the rise of “Piaget” as we know him today, focusing on his contributions to psychology, with a narrow reading of the broader epistemological program taken over in the popular understanding by Thomas Kuhn (1962/2012; see Burman, 2020b; also Piaget & Garcia, 1983/1989).

Of course, changes were ongoing internally too. Piaget's response to the criticisms of his logic led eventually to a dramatic overhaul of his system along more intuitionistic and intensional lines. Indeed, in a report to the Swiss National Science Foundation reflecting on work done at the CIEG in 1978–1979, Piaget described this turn toward logics of meaning as “purging all contamination from extensional logics” (Piaget, 2004/2006, p. 5; explained in more detail by Apostel, 1982a; see also Davidson, 1988; 1993; Ducret, 1988). These changes were then sufficient that the Genevans' later efforts have since been called “Piaget's new theory” (Beilin, 1989/1992, 1992a, 1992b). And from this perspective, the objection offered by Quine—which

could be understood from these documents to have sunk the early support for genetic epistemology that has been my focus here—is then not as interesting, either scientifically or historically, as later divergences from the perspective he represented.¹⁴

To put it simply: what Apostel saw as promising, Quine rejected. The rejection of the renewal was therefore not a rejection of Piaget, or of genetic epistemology, as it may seem. Instead, it can be interpreted as boundary-work in relation to a crisis in logic to which psychologists have been blind as a function of discipline. This historical realization (that the logical basis for genetic epistemology was not refuted [see esp. Mays, 1992]) then allows us to return to the matter at hand with a new perspective.

What we can't see today of what came after is that, rather than continuing to be based upon the metaphor of stages of development, the new theory was reconceived as involving “an upwardly broadening spiral” *without* foundation (translation by Burman, 2016b, p. 762). This enabled abstractions within, between, and across levels in this spiral to scaffold new developments endogenously. And thus the mechanism sought by Bruner was provided explicitly, in terms that he himself had pioneered (viz. the “spiral curriculum” [see Burman, 2020a]). Yet the implications of this “new” effort and what came after, for developmental psychology and education and the study of knowledge in general, remain largely unexamined. Instead, discussions regarding the proper relations between psychology and epistemology were rebooted after epistemology itself was “naturalized” (by Quine, 1969).

It should be said, however, that this rebooted Americanization of Piaget's project has itself now been problematized as “Quine's cul de sac” (Hacker, 2006). So there are advantages to be had in returning to consider these related but once dismissed sources that can today be seen in a different light. The meaning of “natural” is also changing, in the hands of biologists who—recognizing the history of how their own discipline ignored development in favor of the maturation of inherited traits—now advocate for a broader “evo-devo” and “extended evolutionary synthesis” (Amundson, 2005; Laubichler & Maeinschein, 2007; Pigliucci & Müller, 2010). As a result, Piaget's later discussions of evolutionary theory that once looked “hazardous” now seem almost current (Burman, 2019). To reflect those changes, the larger body of later work that emerged after the efforts discussed here has elsewhere been called “epigenetic epistemology” (Burman, 2013, 2016a, 2016b). Indeed, referring to the same sources, Apostel (1985) called this “the future of genetic epistemology.” (The prefix *epi-* means *above* or *beyond*, and the result is explicitly *not* recapitulationist.)

In short, these archival discoveries allow us to read the original explanations with something closer to Piaget's intent while also providing a perspective that happens to be closer to several contemporary interests than the program's original relations with those fields. Thus, to sum up: the research program in genetic epistemology was an empirically and theoretically grounded effort to test philosophical ideas about scientific knowledge and then construct new scientific theories to reflect how this understanding actually develops in those who—sometimes, later—actually become scientists. But the program originated before the post-Sputnik drive to encourage the creation of scientists at virtually any cost (via e.g., disproportionate investments in STEM disciplines). As a result, it reflected different values and ideals and interests from what we now expect. And these, in turn, make those historical works harder to see and understand today: they are “invisible,” and so neglected, because they are “foreign” to our contemporary thinking (Burman, 2015). To derive new value from

this source that has already been so fruitful for psychology and education, we therefore have to make the effort to understand these historical materials—as indeed Piaget (1963) suggested—“from within.”

From this broader perspective, we see that Piagetian Stage Theory isn’t at all about the ages at which different milestones are achieved. Instead, it’s a model of how knowledge is constructed. This is *correlated with age*, when we examine how knowledge develops in children, but it’s not *caused by aging*: it’s *development* in the sense of the construction of a lineage (generation), not the timed *maturation* of inherited traits or modules (or genes). Thus, developmental psychology’s “sensorimotor stage” is reconceived (from the perspective of the infant) as a perceptual world of sensations and motor movements. This is replaced by a world of “concrete” objects (abstracted from sensations), and then of laws governing the behavior of those objects (abstracted from interactions with objects). Finally, that objective world is augmented by the construction and testing of *imaginary* objects and systems (abstracted from laws).

In each of these operationally derived phenomenal worlds, different kinds of illusions can intervene. But the sequence of addressing them and escaping their influence is the same. (We could riff on mathematics and call this “the constructive order of operations.”) The sequence is also *necessary* because strict philosophical realism—in the sense of the quiddities insisted upon by Quine—just isn’t possible: we don’t have access to the world in itself (i.e. we can’t access Kantian noumena directly), so we have to *grow our understanding* of it. As a result, we construct our knowledge from our sensations up.

First, we focus on what we feel when we orient ourselves in different ways to what there is in the world (given our sensory and motor capabilities). Then we focus on what there seems to be (given the phenomena we experience), after which we identify what must be (given how those constructed objects behave). And finally, we imagine what there could be but isn’t yet (given the laws we’ve abstracted from objects’ behavior).

These transformations provide the changing categories of our experience through which the world is made meaningful. It’s how we acquire the capacity for consistent reasoning that Kant and Quine assumed. Indeed, it’s how we organize James’ (1890/1981) “blooming, buzzing confusion” (p. 488). We assimilate our experiences to basic categories afforded by inherited reflexes and only accommodate them when we discover that they are lacking; that is, we undertake the work to change them only after previous constructions have been proved incomplete (Burman, 2016b). And we do this as adults, too, not just as children: the development is of our knowledge, not only of our selves. (We develop from simple to complex, where the meanings of “simple” and “complex” are determined both empirically and formally.)

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Notes

1. The past tense is used throughout because the program is associated primarily with Piaget. However, contemporary scholars—such as Michael Chandler (1938–2019)—have indeed continued to describe themselves in these terms (see Lalonde, 2020).

2. He gave those names in the quoted text (Piaget, 1968/1970, p. 731). But the most obvious book in connection to this interest came later: *Psychogenesis and the History of Science* (Piaget & Garcia, 1983/1989). This presents the *cadre épistémique*: an elaboration of Kuhn’s paradigm and synthesis with Foucault’s episteme.

3. The Rockefeller Archive Center maintains a Zotero database of materials informed by research in their collections, linked-to from their main website: “RAC Bibliography <<https://www.zotero.org/groups/222650/rac>>.” This resource can be productively used alongside their finding aids to help inform the development of new projects.

4. This common and continuing misunderstanding is one of the barriers that the RF also had to overcome: “EFD brought up the main objection . . . namely, that the genetic approach in psychology has been called into question largely by the results of Piaget’s own work. . . . RJH [Robert J. Havighurst] stated that this is exactly the kind of argument that he himself and many other American psychologists would raise against Piaget” (JM & EFD, 1953).
5. In several collections I’ve examined—including at the Rockefeller Archive Center (Franklin, 1957; Rapaport, 1958)—his colleague, Richard S. Ravenal, is later said to have completed a translation of at least the first volume of *Introduction à l’épistémologie génétique*, under Rapaport’s supervision, but I have yet to find an actual copy of this text.
6. This was reported in the quoted diary entry by EFD (1950) that was extracted and included in the project folder. I then found another officer diary that mentions several interactions with Michigan, and the closest fit is from an entry describing a lunch with the social psychologist Theodore M. Newcomb: “Out of discussion emerged a proposal from Newcomb that the psychologists at the University of Michigan make a critical assessment of Piaget’s work during the spring in a form to edify others as well as themselves. He agrees a total bibliography should be drawn up and a review of Piaget’s work from the beginning to the present made and comments formulated on the significance of Piaget’s work for various disciplines and problems” (Gilpatric, 1950, p. 380 of the PDF provided at the Rockefeller Archive Center website). In later correspondence, however, Marshall (1953a) noted that the publications related to genetic epistemology had been overlooked by “the Michigan project.” And I now wonder if the RF records associated with Newcomb’s name for a “small group study,” dated 1953–1956, and 1961, are related to that effort. (There is no indication in the Rockefeller Archive Center finding aid of whether that is related to this, and the finding aid for his papers at the University of Michigan also provides no additional insights.)
7. This projected end date coincides with the approximate start of what is elsewhere called “Piaget’s new theory” (Beilin, 1992b) or “epigenetic epistemology” (Burman, 2013, 2016b). That second period also lasted for the same length of time as the first—fourteen years—but it is not considered here.
8. This can also be understood as the distinction, in this period and after, between Inhelder’s focus and Piaget’s (see Karmiloff-Smith, 1993; also Gallagher & Reid, 1981). Those interested in Cold War-era “*Piagetian* psychology” are therefore directed instead to *Inhelder* (esp. Inhelder & Cellérier, 1992; Tryphon & Vonèche, 2001).
9. Soon after, Gödel (1958/1990) motivated his “*Dialectica* interpretation” by considering the same relation—between mathematical proofs, and the mathematician who constructs them—albeit with no evidence of discussion of the topic with Piaget, despite their probably having met when Piaget was a visiting fellow at the Institute for Advanced Study in 1954 (see Burman, 2016b). However, I did note with interest that Mays’ (1953) aside about Gödel was removed from the published version of his report to the RF (p. 38).
10. In an unpublished appendix to his original report, Mays (1953) also grouped Piaget’s genetic epistemology with intuitionism (Brouwer and Weyl). This is presented only schematically (p. 67). But given the intuitionists’ antagonistic relations with Russell and later Quine, which Weaver overcame, it merits further investigation. (Another member of Brouwer’s “Significa” group, Evert Beth, also overcame his objections to Piaget’s formalisms [see Burman, 2016b].) I am keen to delve more deeply into this connection. For example: what effect did Brouwer’s unpublished 1934 lectures in Geneva have on Piaget’s (1941/1952b) subsequent thinking about children’s understanding of number that was again reexamined as part of the research program in genetic epistemology? The published correspondence suggests that his impact may have been indirect—at that time via e.g., Claparède, Gonseth, or Reymond (see Van Dalen, 2011, pp. 385–386)—but, regardless, it’s clear that the previously-invisible connections between Geneva and Amsterdam merit additional research.
11. This includes the formalization of “equilibration” (Piaget, Apostel, & Mandelbrot, 1957) that was later replaced by *The Equilibration of Cognitive Structures* (Piaget, 1975/1985). Given the recognized importance of that later book—and of the widely perceived centrality of equilibration as the sustaining theme in Piagetian theory—I find it curious that this earlier attempt did not become the central focus of their discussion. Indeed, from the perspective

of the contemporary discourse, it's that book rather than Volume IV that should have received the most attention. In other words, these records show they were doing something other than what we now perceive them to have been doing.

12. For example, Gilpatric echoed what we now expect from the later Bruner: "This seems quite unsound. We learn from others, particularly in science, not from our individual infant experience" (on p. 1). Also: "learning is usually more significant than spontaneous activity in the growth of concepts" (p. 1). And referring to the analyticity debate: "I really don't think Piaget's work (vol IV in his studies) has contributed to this problem: only restated it" (p. 5).

13. Apostel's views were clearly articulated, but too late (see e.g., his 1980; 1982a; 1982b, 1986). Mays, in turn, referred to the culmination of these contributions as "Apostel's Copernican revolution" (Mays, 1986). Later, he also "refuted" the critique by Parsons to which Quine (p. 1) had referred in his objection to the renewal (Mays, 1992).

14. For example: Thomas Kuhn (1983/2000) also made the shift to an intensional approach to truth, and did so in explicit contrast to Quine. This despite having echoed Quine, on Piaget's logic, in 1960 (see Burman, 2020b).

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