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Has psychology “found its true path”? Methods, objectivity, and cries of “crisis” in early twentieth-century French psychology

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ABSTRACT

This article explores how French psychologists understood the state of their field during the first quarter of the twentieth century, and whether they thought it was in crisis. The article begins with the Russian-born psychologist Nicolas Kostyleff and his announcement in 1911 that experimental psychology was facing a crisis. After briefly situating Kostyleff, the article examines his analysis of the troubles facing experimental psychology and his proposed solution, as well as the rather muted response his diagnosis received from the French psychological community. The optimism about the field evident in many of the accounts surveying French psychology during the early twentieth century notwithstanding, a few others did join Kostyleff in declaring that all was not well with experimental psychology. Together their pronouncements suggest that under the surface, important unresolved issues faced the French psychological community. Two are singled out: What was the proper methodology for psychology as a positive science? And what kinds of practices could claim to be objective, and in what sense? The article concludes by examining what these anxieties reveal about the type of science that French psychologists hoped to pursue.

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1. Introduction

In 1911, Nicolas Kostyleff—a now little known young Russian psychologist living and working in Paris—announced to the world that experimental psychology was in a state of “crisis” (Kostyleff, 1911b). According to *La crise de la psychologie expérimentale*, experimental psychology was fractured into rival schools and characterized by diverse methods that produced reams of data but no real insight into fundamental psychological processes. Without an immediate and drastic re-orientation, Kostyleff proclaimed, psychology was in danger of losing all hope of unity and degenerating into an unscientific chaos of applied procedures. Kostyleff’s solution was to turn to the so-called “objective psychology” of Russian psychologist Vladimir Bechterev and to seek to unite mental and physiological phenomena through the concept of a “cerebral reflex.” Neither Kostyleff’s diagnosis nor cure attracted anything like the support he presumably hoped they would, one reason, perhaps, that both Kostyleff and his

“crisis” quickly faded from view, even within the French psychological community.

Nonetheless, Kostyleff was by no means alone in France in worrying about the state of experimental psychology—nor even in proclaiming that there was a crisis—and the issues he focused on in his polemic—objectivity, method, and the future of experimental psychology—were raised in one form or another by many other practitioners and commentators assessing the state of French psychology. Thus, whatever its actual influence, *Crise* and other diagnoses of crisis from the period may be useful to help cast light on important undercurrents present in French psychology during the first decades of the twentieth century.

In this article I use Kostyleff’s crisis proclamation to explore how early twentieth-century French psychologists understood the state of their field, and in what ways they might or might not have portrayed it as being in crisis. The article begins by situating Kostyleff himself, and then examines his analysis of the troubles facing experimental psychology, his proposed solution, and

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the rather muted response his diagnosis received from the French psychological community. French psychologists' much more sanguine assessment of their endeavor notwithstanding, however, the article goes on to show that Kostyleff was not alone in arguing that experimental psychology was in turmoil. [Jacob] Chazottes, Gaston Rageot, and Alfred Binet each also suggested that psychology might be facing a period of crisis, with Binet, like Kostyleff, wondering if even a revolution were in the offing.¹ Though the diagnoses were not exactly the same, together they are taken to suggest that under the surface optimism, important unresolved issues faced the French psychological community. Among these, two inter-related ones arose repeatedly, particularly in discussions surrounding introspection and Russian reflex psychology: What was the proper methodology for psychology as a positive science? And what kinds of practices could claim to be objective, and in what sense? The article concludes by examining what these anxieties might reveal about the type of science that French psychologists hoped to pursue.

2. A Russian in Paris

First, Nicolas Kostyleff (or Nikolaï Nikolaevich Kostylev). At the time of writing *Crise*, Kostyleff was a 35-year-old Russian émigré and maître de conférences at the Ecole Pratique des Hautes Etudes in Paris. He was almost certainly associated with the circle of psychologists around Théodule Ribot and the *Revue philosophique*; there are no indications of much direct contact with Binet and the Sorbonne psychological laboratory. Born in 1876, Kostyleff arrived in France around the turn of the century, and soon began to publish prolifically: his first book was in 1903 (Kostyleff, 1903), followed in 1906 by his thèse de doctorat at the University of Paris, *Les substituts de l'âme dans la psychologie moderne* (Kostyleff, 1906). Over the next nine years, in addition to *Crise*, he wrote *Le mécanisme cérébral de la pensée* (1914b) plus many articles, as well as translating a number of works by the Russian anatomist/psycho-reflexologist, Vladimir Bechterew (1909, 1913). In the process, Kostyleff gained something of a reputation in French psychological circles, both for his championing of Russian reflexology (Kostyleff, 1910, 1914a) and for his engagement with Freudian psychoanalysis (Kostyleff, 1911a, 1912), which he believed could be reconceptualized in objectivist terms and integrated into reflex psychology (Ohayon, 1999, pp. 87–90). And then, with the outbreak of the war, Kostyleff returned to Russia, reporting on war conditions from Petrograd. After the war there was silence, at least in the French-speaking world, until the late 1940s and 1950s, when he published another book (Kostyleff, 1947) and some articles on reflexology, as well as a second translation of Bechterew (1957). He seems to have died in 1956.² Thus, his active career within French philosophy/psychology was brief. Moreover, like many even of the most important figures in early twentieth-century French psychology, Kostyleff remained institutionally, if not socially, marginal, consigned to the academic periphery in places such as the Ecole Pratique des Hautes Etudes, which, for all its lofty name, was mostly a loose collection of poorly funded research institutions separate from, and distinctly inferior to, the places of real influence in French academia, such as the University or the Collège de France.³

Kostyleff's status, or rather lack thereof, was not, in all probability, unrelated to his conviction that there was a serious problem with psychology. Kostyleff's double marginality, both as a foreigner and as an underemployed researcher advocating an unorthodox

(for France) methodology, put him in an excellent position to survey the many trends within experimental psychology from a vantage point somewhat removed from them all. And this he did with relish in *Crise*. After noting that Wundt had founded the first experimental psychology laboratory scarcely thirty years earlier, Kostyleff went straight to the heart of the matter: since then, experiments had accumulated, topics had been taken up and then dropped, and psychology had become a field lacking both a well defined subject matter and any sort of unity. "But the innumerable experiments that have piled up," he declared,

do not allow us to affirm that psychology has found its true path. On the contrary, the more it advances, the more the way becomes uncertain. That derives, on the one hand, from the experiments remaining fragmentary, . . . because of the imprecision of the object, and, on the other hand, from the lack of continuity, with certain kinds brutally abandoned or pushed aside for entirely different research. This last characteristic is becoming today particularly salient, and leads one to conclude that there is a true crisis in the development of experimental psychology (Kostyleff, 1911b, p. 1).

In addition to the lack of a clear goal and experiments done almost at random, psychology suffered, according to Kostyleff, from a second fundamental problem: the split between the physiological and the mental. Classical psycho-physics allowed for exquisitely precise measurements of reaction times or various sensory characteristics, but appeared incapable of being connected to basic psychological processes, because of the complexity of the phenomena and the enormous individual differences in reactions (Kostyleff, 1911b, p. 19). Explorations starting with mental phenomena, such as psychometrics, on the other hand, generated reams of data, but proved difficult to link directly with any corresponding physiological phenomenon. Indeed, for Kostyleff, all that precise measurement-based experimental psychology could accomplish was to explore individual differences in isolated physiological or psychological performances. This left experimentation not only fragmentary, but with an object that was not mind as a general phenomenon, but only individual minds bound in time and space (Kostyleff, 1911b, pp. 46–47).

To establish his diagnosis, Kostyleff surveyed what he took to be the representative contributions to contemporary experimental psychology: psycho-physics and its offspring; the physiological psychology of the Italians and French; the psychometrics of the French and Germans; the attempts at systematization by Edouard Toulouse, Nicolas Vaschide, and Henri Piéron for the French and Edward B. Titchener for the Americans; and the so-called (by Wundt) "Ausfrageexperimente" of the Würzburg school. All suffered from severe problems, according to Kostyleff, and all were leading psychology down the wrong path (Kostyleff, 1911b, pp. 47–48). Like any good prophet calling the faithful back to the one true way, Kostyleff was not content solely with condemning the sinners; having established that there really was a crisis, he also wanted to provide psychology with a new direction toward integration and proper scientific method. Kostyleff turned as precursors to the insights provided by Ernst Mach and Richard Wahle about how to link mental images to cerebral reflexes, and by Alfred Binet (1903d) in his study of his daughters' intellects in *L'étude expérimentale de l'intelligence* (Kostyleff, 1911b, pp. 55–65, 113–135). In particular, he praised Binet's work, finding Binet's use of both quantitative and qualitative methods, including

¹ In the relevant article, Chazottes only provides the initial for his first name. However, it seems probable that it was Jacob Chazottes, born in 1864 and a boursier d'agrégation at the University of Paris in 1889.

² There is no one source of biographical information on Kostyleff; the information in this paragraph has been pieced together primarily from his publications and library catalogue entries.

³ See Havet, Meillet, and Haussoullier (1922), Ringer (1992), Smith (1982), and Weisz (1983).

introspection, to construct elaborate and holistic portraits of the mental characters of his two daughters as promising a psychological approach able to unify, and not just fragment, the psychological subject (Kostyleff, 1911b, p. 56).

But the true heroes of Kostyleff's account were the Russians, Pavlov and especially Bechterew, whose work on reflexes as both physical and psychical objects promised, Kostyleff argued, finally to allow for the creation of an objective science (Kostyleff, 1911b, p. 128). Extending Bechterew's method, however, Kostyleff proposed that the notion of reflexes could be linked with the findings of introspection via the concept of cerebral reflexes, so that mind and body, psychological and physiological, could be fully integrated (Kostyleff, 1911b, p. 138). Moreover, Kostyleff promised that the use of the notion of cerebral reflexes would allow much of the old physiological and psychological work to continue, so long as it was interpreted as illuminating the process by which new reflexes were constituted. "The old laboratories could specialize as previously," Kostyleff concluded, "... But they would no longer work blindly: they would find in the study of the cerebral reflexes the common base that they had lacked and which could alone give to psychology the character of a positive, homogeneous, and precise science" (Kostyleff, 1911b, p. 173–174).

From one perspective, it is clear enough what the language of crisis was doing for Kostyleff. Not only did it let him remind his readers that all was not well with experimental psychology as currently practiced, but it suggested that Kostyleff himself had the cure. If believed, he would of necessity become a central figure in French psychology, both for his own ideas and his critical role in bringing reflexology to the French community. Even if not completely accepted, of course, it was possible that his jeremiad would generate controversy, especially in a community intensely aware of its own marginality, and thus that he might become a significant voice, albeit as a somewhat notorious figure.

It would certainly be unwise to discount completely the elements of self promotion suffusing Kostyleff's crisis talk (nor the crisis talk of anyone else, for that matter), but such an inclination does not seem to have been the whole story. Kostyleff's commitment to reflexology proved deep and long lasting, and there was certainly as much likelihood that an approach so radically at odds with much of mainstream French psychology—especially one tainted with the whiff of materialism—would prove more destructive to his career than beneficial. Moreover, as many reviewers remarked, whatever they thought of his cure, he had certainly identified the disease: experimental psychology was marked more by fragmentation than unity. As no less a figure than the renowned American Wundtian psychologist Edward Titchener remarked, Kostyleff's "criticism of experimental psychology contains, no doubt, a measure of the truth" (Titchener, 1912, p. 478). The rival schools in Germany, and particularly Wundt's harsh attack on the Würzburg psychologists, were well known, as were the growing divisions between the structuralists and functionalists in the United States.⁴ French psychology itself also came in many flavors, ranging from Ribot's pathological-clinical methodology to the classic brass-instrument work of Toulouse, Vaschide, and even Binet, to the very *en vogue* intuitionism of Henri Bergson.⁵ And some French psychologists, including Piéron, were even exploring "objective" or "behaviorist" approaches. Different psychologists employed different apparatuses, examined different physiological or psychological phenomena, used or abused quantitative methods, praised or pilloried introspection, and generally seemed to agree on very little.

Nothing seemed able to unify that diversity into a comprehensive and comprehensible whole. Everyone pretty much knew that; all they had to do was to peruse the journals themselves. The question, though, was whether Kostyleff was right that this lack of unity meant that the emperor had no clothes and psychology was really in crisis? That is where the story gets interesting.

Published in 1911, Kostyleff's tome generated a number of reviews and mentions in that and the subsequent year, but after those mostly silence. No sustained reverberations to alert the community that a major crisis was in the offing. The most thorough review of *Crise* was from experimental psychologist Nicolas Braunshausen of Luxembourg in the *Archiv für die gesamte Psychologie*. His was distinctly hostile, dismissing Kostyleff's central claim that there was anything like a real crisis in experimental psychology (Braunshausen, 1911). A few reviewers were enthusiastic, such as Jean Dagnan-Bouveret, a student working with the psychologists Georges Dumas and Joseph Babinski, whose analysis appeared in the *Revue philosophique* (Dagnan-Bouveret, 1911); or Gaston Danville, author of *La psychologie de l'amour* (1894), who wrote about *Crise* in the *Mercure de France* (Danville, 1911); or Lionel Dauriac, a spiritualist philosopher and disciple of Charles Renouvier, who praised Kostyleff's rejection of pragmatism and positivism in *L'Année philosophique* (Dauriac, 1910); or the anonymous reviewer for the Belgian journal *La Revue psychologique* (Anonymous, 1912).

A number responded as had Titchener for the *American Journal of Psychology* or Edward Weyer (1912) for the *Philosophical Review* in praising the diagnosis if not the cure. Perhaps the most extended response in this vein was from Emile Steinilber in his 1912 book, *Essais critiques sur les idées philosophiques contemporaines* (Steinilber, 1912, esp. pp. 296–313). An enthusiastic Bergsonian, Steinilber seized on Kostyleff's condemnation of the sterility of experimental psychology, describing *Crise* as "remarkable" and "scrupulous," and basically summarized much of Kostyleff's critique in his own account. But when Kostyleff concluded that Russian objective psychology provided the cure, Steinilber demurred, arguing that the real solution to the static and fragmentary nature of experimental psychology lay in a dose of the dynamism and metaphysics provided by Bergson. And then there was Louis Barat (1911), another student of Dumas's, whose review in the *Journal de psychologie* managed to praise Kostyleff for providing an excellent overview of the principal methods employed in experimental psychology, while missing completely Kostyleff's point that the field was facing a crisis and that Kostyleff was criticizing the entire enterprise.

These reviews notwithstanding, in various retrospectives written just before and after World War I, including those by the Jesuit psychology professor Jules de La Vaissière (1912), the philosopher Dominique Parodi (1919), and the Belgian experimental psychologist Georges Dwelshauvers (1920), not to mention Georges Dumas's comprehensive mid-1920s compilation, *Traité de psychologie* (1923–24), Kostyleff is scarcely mentioned. Moreover, in his annual reports on the state of French philosophy for the *Philosophical Review*, philosopher André Lalande examined at some length the new Russian reflex psychology of Pavlov and Bechterew, even noting that Kostyleff had translated one of Bechterew's important works, without ever discussing the so-called "crisis" that Kostyleff had so sharply and impassionedly delineated.⁶ It is true that Georges Palante (1916), an agrégé in philosophy and frequent commentator on French philosophy for the *Mercure de France*, not only made reference to but largely concurred with Kostyleff's

⁴ See Danziger (1990), chs. 3, 9; Danziger (1980), Kusch (1999); and O'Donnell (1985).

⁵ For a general overview of psychology during this period, see Ben-David and Collins (1966); and Hatfield (2003). On French psychology, see Brooks (1993), Carroy, Ohayon, and Plas (2006), Carroy and Plas (1996), Carroy et al. (2006), Carroy and Schmidgen (2002), Carson (2007), esp. chs. 4, 6; Nicolas (2002), Plas (2000), Reuchlin (1965, 1980); and Rose (2011).

⁶ See Lalande (1915, 1920). On French philosophy, see Brooks (1998), Gutting (2001); and Vogt (1982).

diagnosis of crisis in the context of a 1916 piece discussing Kostyleff's *Mécanisme cérébral*, but after that brief mention, Palante never returned to the topic again. Thus, was Kostyleff simply wrong about the supposed woes of experimental psychology, or was he saying something so familiar that the French psychological community had little reason to respond? A bit of both, as we shall see.

3. Crisis? What crisis?

1911, the year *Crise* was published, was also the year of Binet's untimely death at age 54. The loss of one of the most productive and important figures in French experimental psychology, as well as one of the few heroes of Kostyleff's tome, might have been thought to have underscored Kostyleff's point that experimental psychology had reached a difficult juncture, at least in France. And yet, not only was the response to Kostyleff's own work muted in the French scholarly community, but the various overviews and articles summarizing the state of the field that appeared in the years 1900–1925 rarely gave much indication that other practitioners were troubled about the intellectual aspects of their endeavor. The lack of secure positions, unquestionably, worried many, and the decimation of the community by the war led at least one commentator, Lalande, to announce that the field faced a crisis (Lalande, 1920). But the more typical tendency, particularly in the postwar years, was to celebrate the fecundity and diversity of French psychology, seeing the various methodologies employed as signs of the discipline's health and the complexity of the object—the human mind—that they were all trying to understand.

The most imposing example of what might be called this confident side of French psychology was undoubtedly Sorbonne professor George Dumas's much praised two-volume work of 1923–1924, *Traité de psychologie* (Dumas, 1923–24). Over 2100 pages long, with thirty-nine chapters written by twenty-five authors, Dumas produced less a treatise than a compilation surveying the most important trends in French psychology. All the major figures in the community are represented, and one comes away from reading it with a sense of the variety, and perhaps even vibrancy, of the research being carried out within the ambit of French psychology. What one gets very little sense of is that all this diversity was deemed a problem. There was little hand wringing over the state of French psychology and seemingly few worries about the lack of a unified whole. Rather, as Dumas himself declared in the final sentence of the *Traité*:

We do not regret that different conceptions of psychology are presented, since divergences in opinion are, in all fields, the precondition for intellectual activity; ours testify to the activity of French psychology which has never been more varied nor lively; moreover, by them, our collective work is representative of our time and our nation (Dumas, 1924).

Lalande (1919), professor of logic and scientific method at the Sorbonne, opened the *Traité* with a survey of the major trends and methods in contemporary psychology. While by no means as sanguine about the state of experimental psychology as was Dumas, Lalande evinced little pessimism about the present or future, even while noting that it was difficult to define precisely what psychology was and that few of its methods could be employed unproblematically. Nonetheless, with suitable cautions Lalande argued, the method of introspection, including the new experimental introspection associated with the Würzburg school in Germany and with Binet in France, and the pathological method that French

psychologists had pioneered, would continue to produce important advances and contribute to progress in the field (Lalande, 1919, pp. 190–195, 198–203).

Others within the French psychological tradition as well, from philosopher Frédéric Paulhan at the turn of the century, to Binet shortly before his death, to Dwelshauvers just after the war, to Montpellier psychophysicist Marcel Foucault in 1924 seemed, if anything, to celebrate the many advances being produced within French experimental psychology and to admire the breadth of the endeavor, even if worried about this feature of the field or that. Thus Paulhan (1900, p. 67), a close associate of Ribot's, declared that “the work is proving fruitful,” while in 1905 Lalande (1905, p. 432) spoke of an “active revival” in philosophy, which he associated with the turn to scientific approaches, and Binet commented in his review of the field for 1909 that the abundance of treatises on experimental psychology “demonstrates that psychology is approaching a period of maturity” (Binet, 1910, p. 1).⁷ In the same year, Abel Rey, a philosopher and experimental psychologist at Dijon, while reporting on the Sixth International Congress of Psychology in Geneva, explained that what had impressed him most from attending the “long and laborious sessions of the Congress” was the position of “psychology as an experimental science, and the continual progress and vitality that it [psychology] owed to this attitude, in spite of the enormous difficulties that it had encountered in its object, difficulties that no other science has been exposed to up to this point” (Rey, 1909, p. 349). Binet's successor as editor of the *Année psychologique*, Henri Piéron, was even more optimistic in his first article after assuming the editorship in 1912: “the progress of our psychological knowledge,” he declared, “thanks to experimental work, has increased considerably in the last thirty years” (Piéron, 1913, p. 7). Surveying French psychology in 1920, Dwelshauvers (1920, p. x) also praised the “richness and complexity” of French psychology, even while lamenting that such diversity made it difficult to give a simple overview of the field.

Perhaps most ebullient was Raymond Meunier, chef de travaux for psychologist Edouard Toulouse at the Laboratory for Pathological Psychology of the Ecole Pratique des Hautes-Études at Villejuif, who proclaimed in 1912 that “psychology is now one of the great forces, and perhaps the most effective, that aids humanity in its incessant struggle against pain, in its eternal aspiration toward ‘that which is’ most elevated. That is its work” (Meunier, 1912, p. 67). While Foucault would seek to rein in such exuberance in his 1924 article on the forms of psychology by noting that “it would be an exaggeration to claim that its [scientific psychology's] discoveries have changed the face of the world,” and that psychology is still developing its methods, he did second Piéron that “in the last half century . . . [psychological] research has been fruitful and new and solid truths have been progressively established” (Foucault, 1924, p. 353).

Worries French psychologists did have, but the crises that seemed more on their minds revolved around the field's tenuous institutionalization within the academy and the effects of external events.⁸ With no mandatory teaching of psychology in the lycées, and with minimal official sanction, at best, for the three or four main psychology laboratories, training of the next generation of French psychologists was erratic and unsystematized, bespeaking the difficulties for practicing psychologists actually to find permanent positions. In 1903 Nicolas Vaschide, at that point chef des travaux at the Laboratory for Physiological Psychology at Villejuif, described this situation at length, pointing out that there was only one chair of experimental psychology at the Sorbonne and one at the Collège

⁷ See also Marillier (1900).

⁸ For a discussion of the negative effects of the institutional structure of French academia on the development of psychology in France, see Ben-David and Collins (1966), esp. pp. 463–65.

de France (Vaschide, 1903).⁹ By 1920, the situation had barely improved. Binet's difficulties with obtaining a permanent position are well known: in his attempt to gain the chair of experimental psychology at the Collège in 1901, he was edged out by Pierre Janet in a close vote; the following year, Binet was again denied when he sought the position at the Sorbonne being vacated by Janet (it went to Dumas).¹⁰ Piéron did not get a professorship until 1923, when he received the chair of the physiology of sensations at the Collège; Théodore Simon, Toulouse, and Vaschide spent most of their careers working in asylums; Benjamin Bourdon (Rennes) and Foucault (Montpellier) remained in provincial universities; and others, like Kostyleff, survived on small grants from institutions such as the Ecole Pratique.¹¹ Although psychology was taught to a limited extent in the lycées, that teaching was included in the philosophy instruction and thus those trained in psychology wanting lycée positions had to compete with the many philosophy graduates also seeking such opportunities.

Compounding these difficulties, for much of the decade following the publication of Kostyleff's book, the French psychology community, like every other segment of French society, had to grapple with the effects of World War I. As Lalande noted ruefully in 1919:

At the present moment French philosophy is passing through a crisis. Not only were many young scholars killed in the war who were expected to succeed the teachers of to-day, but social and financial difficulties have increased with peace, and created conditions very unfavorable to disinterested work and intellectual productivity. (Lalande, 1920, pp. 413–414).

Piéron reacted more defiantly in 1920, proclaiming that the war had not killed off French psychology, whatever the Americans might think, and that “it can regain its fruitful vigor” (Piéron, 1920, p. 261). Nonetheless, recovery took some time, and it was not until the early 1920s that the field was back to its pre-war status.¹²

Thus, from the perspective of many in the French experimental psychology community, Kostyleff's book must have seemed odd indeed. Although they would have recognized most of the features of experimental psychology that he depicted—particularly its diversity in both objectives and methods—there is little evidence that they were perturbed by this state of affairs. Rather, many both before and after the war seemed proud of French psychology's accomplishments and optimistic about its future. Moreover, the area in which many were feeling anxiety—over securing permanent positions—did not really figure in Kostyleff's account. It is true that French psychology journals throughout the early twentieth century were full of the language of crisis; however, the word “crise” appeared most often in medically related articles, signifying the onset of a period of intense agitation and distress, such as occurred in an epileptic or hysterical fit. Social, political, intellectual, economic, and even moral crises also were discussed, to be sure, and certainly one gets a sense that the possibility of seeing the sky falling or of proclaiming the need for radical change in almost

any area was always present.¹³ Nonetheless, when it came to psychology itself, cries of crisis were largely absent—largely, but not entirely.

4. Cries of crisis and questions of method

Although Kostyleff's cry of crisis attracted the most attention, it was by no means the only such diagnosis of French psychology appearing during the first decades of the twentieth century. Nine years earlier, an obscure professeur in Guéret, J[acob] Chazottes (1902, p. 249), declared in “The Current Conflict Between Science and Philosophy in Psychology” that psychology was “going through a period of crisis,” because it was “no longer a philosophical science and not yet a positive science.” According to Chazottes, psychology's troubles derived from being split into rival camps—the philosophers and the natural scientists—neither of which was developing a psychology untainted by the influence of the other. The solution, in his view, was obvious: psychology must cast off its metaphysical interests, particularly the search for causes and desire to understand the nature of thought, and instead become a fully positive science, focused solely on well defined psychic facts and devoted to determining the laws of their appearance and succession (Chazottes, 1902, p. 259).

Three years later in 1905 Gaston Rageot, an agrégé in philosophy and homme de lettres who would later publish *Les savants et la philosophie* (Rageot, 1908), echoed Chazottes. Reporting on the Fifth International Congress of Psychology in Rome, Rageot informed readers of the *Revue philosophique* that “experimental psychology is going through a crisis,” one provoked by the “offensive return of philosophy” (Rageot, 1905, p. 86). The enemy, in this case, was introspection, which Rageot feared was being revived in response to the discovery that precision instruments alone were insufficient to produce the kind of careful laboratory results that experimental psychology required, and thus that theories were being promulgated without an adequate empirical basis (Rageot, 1905, p. 87). Rageot, like Chazottes, called for renewed dedication to careful experimental work, including the embrace of the specialization, narrowness, and discipline that it demanded.¹⁴

Finally, in 1911 no less a figure than Alfred Binet joined the chorus, though with a tune much different than Rageot's. Binet (1911b) opened his long lead article in *L'Année psychologique*, “What is an Emotion? What is an Intellectual Act?,” with an introductory section entitled “The Crisis of Psychology.” In all likelihood unaware that Kostyleff was about to publish a book on the subject, Binet, too, believed that a crisis was in the offing, perhaps even a revolution, but a constructive one, one that would only benefit psychology, and one where he saw himself in the vanguard, along with Oswald Külpe, Karl Marbe, and the so-called Würzburg school. Their new experimental methodology, Binet enthused, was employing introspection with much greater rigor than when it had been used by old-school psychologists such as Wundt, and was thereby revolutionizing the discipline by demonstrating that

⁹ See also Lalande (1905), pp. 430–31; and La Vaissière and de (1912), pp. 32–33.

¹⁰ See Wolf (1973), pp. 22–28.

¹¹ For information on the 1939 jumps in French psychology, see Nicolas (2002). Piéron (1992) provides a detailed account of his efforts to secure permanent employment.

¹² On the drop in submissions to philosophy journals during the decade of the war, see Vogt (1982). For psychology during the early 1920s, see Ohayon (1999), ch. 1.

¹³ My claims about the use of the word “crise” in French psychology journals are based on an analysis of the appearance of the term in the *Année psychologique* from 1898 to 1940 and in the *Revue philosophique* from 1876 to 1934. In the *Année psychologique*, “crise” appears approximately twenty-four times in each of the five-year periods from 1900 to 1914, and then from 1925 to 1939 jumps substantially, to about seventy-two appearances per five-year period. In the *Revue philosophique*, there are over eighty-seven instances of “crise” in the five-year periods from 1896 to 1915, with a spike in 1906–1910, just before Kostyleff's book appears. After the war, the instances decline a little, to over seventy-six per five-year period. “Crise” also appears in approximately 650 titles in the Bibliothèque Nationale de France catalog for publications during the period 1900–1930, generally referring to social and especially economic crises. Not counting the war years, the average for the five-year periods 1900–1929 was about 118 titles per period, with a bit of a spike in 1905–1909, when there were 138 works published with “crise” in the title. For general discussions of the notion of crisis, see Shank (2008); and Starn (1971).

¹⁴ In his later work, Rageot (1908) expressed many of the worries shared by others about experimental psychology, particularly that the rigorous laboratory methods associated with Wundt's physiological psychology were not elucidating the most important aspects of the mind. Nonetheless, though he cautiously praised the French tradition of pathological psychology, especially as practiced by Janet, in the end he upheld the ideal for psychology of a physiological approach, though one that must get down to the level of nerves.

data derived from precision instruments alone were not adequate to capture psychic processes (Binet, 1911b, pp. 1–2, 6–7).¹⁵

Thus whether praising or bewailing the newest trends in experimental psychology, a number of French psychologists preceded Kostyleff in claiming that some sort of crisis was brewing. As attempts to provoke the French psychological community to action they were even less successful than Kostyleff's; no one seems to have reacted at all. And yet, considering these four diagnoses of crisis together, something of a shared set of preoccupations emerges. Each author on the one hand sought to uphold experimental psychology's commitment to "positive methods," to approaches that would maintain psychology's status as an objective, empirical science. And yet, on the other hand, each also suggested that psychology's current practices were not yet providing an adequate understanding of the nature of mind. For Chazottes and Rageot, that understanding would eventually come, as long as psychology kept philosophy, and particularly introspection, at arm's length. For Kostyleff and Binet, however, current methods in and of themselves were inadequate; only some sort of revolutionary transformation, be it via Russian reflexology or the new introspectionism or even a combination of both, could produce a psychology at once scientific and able to truly account for mental phenomena in their entirety and complexity.

Most of the rest of the French psychological community did not see the situation in such grave terms. Nonetheless aspects of this tension can be detected in almost all the works surveying the field during the early twentieth century, particularly when they considered the merits of introspection as a method for psychological research or the challenges posed by reflexology's claim to be the one "objective" form of psychology. In both instances, writers tended to ask whether these methods were able to produce objective positive knowledge, and whether they were the ones best suited to doing so.

4.1. *Introspection old and new*

Few psychologists in France or elsewhere would have disputed the claim that modern psychology was rife with different methodologies, whatever they thought about that state of affairs. Even those most sanguine about the field, such as Ribot (1909), routinely acknowledged that French experimental psychology was composed of various strands. Indeed, as we have seen, from Paulhan to Binet to Lalande to Piéron to Dumas, many argued that such diversity of method was a real strength of the field, allowing the complicated object of their investigations, the mind, to be approached from a number of vantage points.

But even those practitioners championing methodological pluralism routinely distinguished among the methods, suggesting which had the most to offer experimental psychology and which should be considered secondary at best. Thus in 1909 Rey was happy to celebrate the "positive methods" of experimental psychology and the many advances that had been achieved because of the turn to the laboratory and precise instrument-based measurement, while condemning Bergsonian psychology as "metaphysics."¹⁶ In the same year, Ribot published a piece on psychological methods in which he laid out all of the major approaches then being employed in France, from introspection to questionnaires to laboratory measurements to the pathological studies he championed, and suggested some of the strengths and weaknesses of each. Pathological

methods, not surprisingly, came off best, with his most sustained skepticism directed toward questionnaires and other methods that valued quantity over quality (Ribot, 1909). Lalande used his introduction to Dumas's *Traité* not only to provide an overview of the main methods being employed in contemporary psychology, but also to comment on their strengths and weaknesses. He cautiously praised introspection, including the new variety associated with Binet and Würzburg; described pathological psychology as "the principal agent of progress in psychology;" but, while acknowledging some role for psychophysics, declared that it had not yet accomplished much (Lalande, 1919, pp. 190–195, 199, 213–218). Parodi saw things very similarly in his 1919 review of French contemporary philosophy (including psychology), arguing for the centrality of introspection to psychology and the critical importance in French psychology of the pathological approach. Like Lalande, he also disparaged psychophysics ("in the German style") as not having much to offer (Parodi, 1919, pp. 78–103).

Within this generally pluralistic outlook, the method that generated the most controversy, and in the eyes of many lay at the heart of whatever crisis experimental psychology might be experiencing, was undoubtedly introspection, whether in its old guise as part of Wundt's psycho-physiological experimental approach, or its new version, which claimed that even the most established techniques within the "brass instrument" tradition of laboratory psychology must be revisited and revised.¹⁷ Binet was probably unsurpassed in the French psychological community in his enthusiasm for the new form of introspection, and indeed claimed priority over the Würzburgians for invention of the "*méthode de Paris*," as he called it, on the basis of his investigations into the mentalities of his two daughters, published in 1903.¹⁸

According to Binet, the revolutionary nature of this form of introspection, which in one guise involved asking subjects to report on their experiences as they underwent psychological experiments, was that it demonstrated that even the seemingly clear-cut results produced using classic Wundtian procedures to investigate basic sensations might largely be artifacts. Experiments in which respondents were asked to perform a simple introspection, such as determining when the pressure from two weights felt the same, for example, became more complicated once subjects were also asked to perform a second kind of introspection and describe precisely what they felt when reporting that two sensations were "identical." As it turned out, Binet argued, different participants described very different subjective experiences, and did not necessarily mean the same things when reporting that two weights felt the same (Binet, 1903a, 1903b, 1903c).¹⁹ Binet combined such insights with his concurrently developing ideas about the possibilities of imageless thought to argue for an experimental psychology that sought not to eliminate subjective experience but to embrace it, that emphasized the dynamic over the static, that sought to interconnect thought and emotions, and that provided room for the unconscious as well as the conscious (Binet, 1903b, 1903d).

Few other French psychologists went as far as Binet in their engagement with the new introspectionism. But many did argue for the importance of preserving some form of introspective method at the heart of psychological practice as perhaps the only way to access the phenomena of consciousness. In his 1911 article on introspection, for example, Ludovic Dugas (1911, p. 625), a co-editor of *l'Année philosophique*, declared that "introspection is thus the fundamental, original, and characteristic method for psychology," a

¹⁵ See also Binet (2008), pp. 123–24.

¹⁶ Rey (1909), pp. 349–50. On reactions to Bergson, see Grogin (1988), esp. ch. 5.

¹⁷ On introspection, see Hatfield (2005).

¹⁸ For Binet's claim to priority in having invented the new experimental introspection, see Binet (1909), pp. VIII–X; Binet (1911a), pp. VIII–IX; and Binet (2008). For the basis of his claims, see Binet (1903d).

¹⁹ On this issue, see Carroy and Plas (1996), pp. 80–82; and Danziger (1990), pp. 44, 139.

position echoed by La Vaissière (1912, p. 23), Parodi (1919, pp. 84–85), and Dumas (1924, p. 1126) in his concluding chapter to the *Traité*, where he observed that “psychology . . . is a science where introspection plays an essential . . . role.”²⁰ Even Piéron (1913, p. 8), for all his commitment to the study of behavior (*comportement*) as the key to doing proper experimental psychology, nonetheless argued that “there is no opposition between introspection and objective psychology,” and that the method of questioning (experimental introspection) that the “German psychologists” [the Würzburgians] took from Binet furnishes documents and observations important for the behavioral approach.

4.2. Introspection, objectivity, and scientific method

In wrestling with the question of whether introspection belonged in their repertoire of tools, the question that came most immediately to the surface for most French psychologists concerned its status as a scientific method, often framed around the issue of objectivity. In one sense, there was little to debate. For most French psychologists, at its core “objective” was understood as a purely descriptive term, signifying externally observable, and was contrasted with “subjective” in the sense of observable only internally, typically by means of introspection. This was the distinction that Louis Gérard-Varet, author of *L'ignorance et l'irréflexion. Essai de psychologie objective* (1899) and professor of Philosophy at the University of Dijon, relied on in his “La psychologie objective.” Introspection, he explained, was a feature of “subjective psychology . . . which proceeds by interior observation,” and not of objective psychology, which “applies to the observation of other people” (Gérard-Varet, 1900, p. 492). Vaschide (1902) used “objective” in much the same way in his report on the 1902 Congress of Psychology at Turin, as did Dugas (1911) in his impassioned defense of introspection, La Vaissière (1912, p. 23), and Lalande (1919, p. 184). In this guise, “objective” signified a particular vantage point from which experiments or observations would be conducted, and while some, such as Dugas, wondered whether it was really so easy to divide categorically external observations from internal ones, in the main this use generated little controversy.

There was, however, a second aspect to “objective” that was not so easily resolved, the normative claim that it referred to the proper way to conduct scientific inquiries.²¹ Much of the debate over the validity of introspection as a methodology hung on whether, while clearly “subjective” in one meaning of the term (because reliant on internal observation), it could still be considered a form of observation and experimentation that was positive and scientific. Some, such as the Barcelonan psychologist and bacteriologist Ramón Turró, an enthusiast for the work of Bechterew as well as an associate of both Ribot’s and Janet’s, were adamant in their opposition. In his 1916 article “La méthode objective,” for example, Turró argued that the goal of a science was to provide causal explanations of the phenomena it investigated. Introspection alone, he insisted, could not meet this test, because internal observations yielded only “a science of pure phenomena that float within time like shadows, that come, go, and vanish, without it being possible to guess how it is that they did so” (Turró, 1916, p. 302). Such an “indefinite series” of floating phenomena, Turró declared, made it impossible to determine causal connections and thus would not allow psychic phenomena to be explained in the same way that “the physicist explains the phenomena of physics, the chemist those of chemistry, the physiologist those of biology” (Turró, 1916, p. 303). Turró did not insist that introspection be wholly abandoned, as he found it invaluable as a descriptive en-

deavor, generating a wealth of depictions of psychological states. But inspired by, among others, Bechterew, Turró argued that these introspective accounts only took on real value when linked causally to the physiological states that he believed generated them. Thus, for Turró, the “objective method” did not just signify a particular way of observing psychological phenomena—externally—but an orientation toward the kind of accounts that were the hallmark of modern science—causal explanations.

Dugas, not surprisingly, saw things rather differently. Although he was more than willing to distinguish between an objective psychology founded on external observations and a subjective one based on introspection, Dugas saw no problem with establishing a rigorous empirical psychology—including one with causal explanations—on the basis of introspective observations. Indeed, Dugas went so far as to argue that introspections, performed by a trained observer, were subject to fewer intrinsic errors than external ones. Both forms of observation required extensive experience and intense discipline to be performed well; both required, in addition, near obsessive attention to controlling error, something he suspected might be more readily achieved with internal than external observations (Dugas, 1911, pp. 615–618). Introspection, Dugas concluded, “is not only the premier source of psychological information, it is also the most pure and complete, because it is the most direct, the least loaded with interpretations and commentaries” (Dugas, 1911, p. 624).

As Dugas understood it, the most serious argument against the scientific status of introspection was the claim by some that only externally observed psychic facts could be shared and generalized into psychological laws. To this Dugas responded that all individual observations, however generated, were particular, and thus it must be no harder to generalize internal ones than external ones. Moreover he claimed that piling up individual facts was not the way to reach general truth or a law, anyway, because the problem of induction was present, however the facts were attained (Dugas, 1911, pp. 619–620). “Introspection,” Dugas then concluded, “does not merit any of the accusations raised against it. It is not a psychological impossibility, a contradiction in and of itself: it need not be challenged like suspect testimony”; rather “it is or could be, like all observation, the perception of the law in the fact or of the universal in the particular” (Dugas, 1911, p. 624). Whether or not it could be deemed objective, introspection could be considered a proper scientific method, according to Dugas, because in science the possibility of uncovering universal laws was achieved, not by generating mountains of empirical data, but by producing even a few rigorous observations by a trained and disciplined observer.

French psychology certainly had its proponents of experimental approaches that were oriented more toward producing reams of data, and with as little human interference as possible, what Lorraine Daston and Peter Galison (2007, chs. 2–4) have termed “mechanical objectivity.” Binet, Bourdon, Foucault, Piéron, Toulouse, and Vaschide all were important practitioners of instrument-based methods associated with the laboratory. But most, like Binet (1903d, p. 299), also lamented the arbitrary limitations and artificialities of many instrument-based investigations, and thus few insisted that such approaches were the only ones consonant with practicing psychology as a positive science. Instead, French psychologists tended to subscribe to Dugas’s general point, that there was nothing about introspective observations that intrinsically precluded using them as the empirical foundations for scientific theorizing. This may have been one reason that French experimentalists were less consumed by the controversy roiling the German psychological community between the “old”

²⁰ See also Lalande (1919), pp. 190–95, on the value of introspective methods for all of their problems.

²¹ For a parallel case in the United States, see the cogent discussion of the multiple meanings of objectivity for Titchener by Green (2010).

brass-instrument approach associated with Wundt and the “new” experimental introspective one championed by the Würzburgians.²² French psychologists, by and large, did not accept that only one empirical method could be considered properly scientific.²³ Committed to methodological pluralism, they could approach introspection, in whatever its guise, as just another method at their disposal, to be used as circumstances and predilections dictated. For some this meant expanding the meaning of “objective” to include, at least potentially, internally generated observations; for others it meant rejecting the normative presumption that objective methods were the only ones consonant with positive, empirical science. This latter move was revealed most vividly in the community’s response to the claims by one group of practitioners, the reflexologists, that theirs was the only objective form of psychology.

4.3. *The Russians are coming! “objective psychology” and its critics*

There was certainly no question about the objectivity of the other method that troubled French psychologists during the first decades of the twentieth century. No one doubted that Russian reflexology, whatever its other virtues, was fully consistent with the precepts of positive science. But many French psychologists were dumbfounded, as well as more than a little put off, but what they saw as the audacious move by the Russian reflexologists, including Kostyleff, to appropriate the very word “objective” as a name for their psychology and to claim that it was the sole form of psychology that could properly be termed objective. As the French learned primarily through Kostyleff’s translations of Bechterew, according to the Russians, “objective psychology” referred to the work on conditioned reflexes associated most prominently with the investigations of Pavlov and Bechterew. Bechterew (1909, p. 481) made this clear in an article adapted and translated by Kostyleff for the *Journal de psychologie* in 1909: “objective psychology” drew a sharp line dividing the study of “objective manifestations of neuro-psychic activity” from “the subjective character of such phenomena.”

While Bechterew did not rule out the value of the subjective to psychology, he did declare that “objective psychology” must scrupulously avoid “all subjective terms and any subjective interpretation of neuro-psychic phenomena” (Bechterew, 1909, p. 482). In particular, he singled out introspection as the characteristic method of traditional subjective psychology, and insisted that his new objective approach would not rely on interior observations of mental states or processes, but instead focus solely on externally visible motor responses, especially those conditioned by the organism’s previous experiences. By so doing, Bechterew explained, “the relations of external reactions to the excitations that provoked them can offer an objective criterion for neuro-psychic activity without it being necessary either to go into the unknown ‘moi,’ or to make recourse to subjective interpretations” (Bechterew, 1909, p. 489). As Kostyleff (1911b, p. 139) elaborated, while Bechterew sought “to eliminate introspection,” he did not want “to reject the phenomena that it revealed”; rather, he tried “to catch the objective aspects” of those phenomena, through focus on the conditioned reflex and stimulus-reaction pair. “He saw in the conjunction of the nervous current with traces of previous reactions,” Kostyleff explained, “the objective schema of consciousness.”²⁴

Not surprisingly, those outside of the reflexology camp for the most part proved resistant to the appropriation of the term “objective” by the Russians. Thus Piéron (1913, p. 8), though expressing real admiration for Pavlov’s salivation experiments

with dogs and certain that his own behaviorist approach was fully objective, nonetheless did not argue that objective psychology must reject introspection outright. Indeed, in his view, one of the human behaviors for which psychology had to account was the tendency to analyze the mechanisms of thought, even as they were in operation. Rather, the key criterion for a science to be considered objective, he argued, was that it be predictive (Piéron, 1913, p. 10).

Lalande (1915) was much harsher toward the Russians. Provoked by the appearance of Kostyleff’s translation of Bechterew’s *La psychologie objective* in 1913, Lalande declared that

there is a great gulf separating the reactions of the salivary glands from artistic productions and moral appreciations. And that is why we must deny to psychological physiology the right to monopolize for its own profit the name of “objective psychology.” The objective is not essentially that which is material and experimental; it is that which has the power of putting minds in harmony, whatever else may be the process employed to arrive at that result. There is something of the objective in the psychology of consciousness and sympathy; there is something of it in the reflective and critical psychology (Lalande, 1915, pp. 267–268).

In a later essay he returned to this theme, again taking the Russian psychology to task for its appropriation of the term “objective”: “Nothing proves that of all the forms of psychology, the psychology of reaction [reflexology] is the only one that presents this character [of objectivity]; and an indispensable methodological rule is not to prejudge things by the names attributed to them” (Lalande, 1919, p. 184).

Parodi was equally hard on “objective psychology” in his post-war analysis of the state of French philosophy, arguing that the school of psychology concerned solely with external manifestations of psychic phenomena—one in which he lumped Piéron—was misguided not so much because it claimed to be the only form of objective psychology as because it believed there could be a proper psychology at all that could ignore consciousness. “Without doubt,” Parodi (1919, p. 84) remarked, “psychology, as an independent and positive science, must proceed by a method as objective as possible and surround itself with all the information that one could take from outside: . . . but what are called the objective givens, they are still, however, at least most often, the givens of consciousness.”

Thus, for all of French psychologists’ commitment to objectivity, their responses to the claims of the Russian reflexologists reveal clearly that there were limits as to what this commitment would entail. While few denied that psychology should be a positive science, most also believed, as Parodi argued, that descriptions of the phenomena of consciousness, derived typically from introspective observation, must be part of any psychology that truly could be said to provide an adequate account of the nature of mind. Many concluded that “objective” itself must thus be more broadly conceptualized, to include internally derived observations. The struggle, which perhaps lay at the heart of the tensions underlying French psychology during the period, was to find a way to investigate such phenomena in a manner that fit the rigors of laboratory science, and yet avoided the narrowness symbolized by the Russian approach. Kostyleff’s program to integrate reflexology, introspection, and Freudian psychoanalysis, though not very influential, may reflect his own familiarity with these issues and idiosyncratic attempt to resolve them.

²² On this controversy, see, Carroy (2004), Danziger (1990), ch. 3; Danziger (1980); and Kusch (1999).

²³ For example, when rivalries did break out, such as that between the Ribot circle and Binet and his co-workers, they were rooted in personal issues, not in differences in experimental procedure. See Ribot (2005) and Beaunis (2009), pp. 185–86.

²⁴ See also Kostyleff (1910, 1914a); and Soukhanoff (1908).

5. Conclusion

So, was French psychology in a state of crisis in the early twentieth century? Perhaps. Certainly Kostyleff was not alone in thinking that some sort of revolution needed to take place and that there were fundamental questions facing the community.²⁵ To be sure, Kostyleff's prescription for the restoration of psychology's vigor found only a few takers, in France anyway. Nonetheless, Kostyleff did identify some of the fault lines worrying other French experimental psychologists as well. Could the mass of isolated experiments on individual aspects of the mind as a reasoning and affective organ ever yield a more integrated understanding of human nature? What was the appropriate methodology to study the mind? And what about the status of their endeavors as objective science? How exactly were psychologists to merge the rigors of the laboratory with their desire to account for the full range of mental phenomena?

There was, of course, nothing entirely novel about such questions. Most had been debated by experimental psychologists since at least the 1870s, when Wundt and others sought to turn explorations of mind into a laboratory science. If there was a heightened urgency, it was pushed on the one hand by the sense that all of the precise experimentation of the past quarter century had yet to produce a major breakthrough in understanding how the mind operates, and on the other by the advocacy of alternative methodologies—such as the new introspectionism or Russian reflexology—that suggested that traditional laboratory psychology may not have been quite on the right path from the start. Most French psychologists, as we have seen, were loath to declare that there was something fundamentally wrong with their enterprise, save for the lack of suitable academic positions. But some did suggest that psychology itself was evolving, in response to the challenges presented by new methods and the limits encountered when hewing too closely to the strictures of the laboratory. Writing in 1920, for example, Dwelshauvers (1920, p. 113) argued explicitly that French psychology had embarked on a new path as of 1909:

the times had changed: . . . experimentation had not produced what psychologists had anticipated; physiological explanations were questionable and variable; interior observation, practiced with precision, had taken on a totally new value; and . . . monographs on people of genius and the study of affective states, had changed the face of psychology.

Believing that a totally mechanistic approach to phenomena was only appropriate “where interiority is at a minimum,” Dwelshauvers celebrated this shift in direction away from some of the constraints of the laboratory, and indeed emphasized the intuitive and spiritual aspects of the human mind, in the manner of Bergson (Dwelshauvers, 1920, pp. 252–253).

Not many other French experimental psychologists followed Dwelshauvers in his appreciation for Bergson. But the almost universal celebration by members of the community of the use of the pathological method in the manner pioneered by Ribot was one indication that most French psychologists felt comfortable with an eclectic approach to understanding the nature of mind. And some, such as Binet, were willing to go further still, and to suggest that the mind was not quite the rational machine imagined by some proponents of laboratory experimental psychology and by a segment of the community whose intellectual lineage could be traced back to Hippolyte Taine's *De l'intelligence* (1870). Instead, innovations such as the new introspection demonstrated, he contended, that the mind was full of competing, overlapping impulses, ideas, desires, and the like, and the rational veneer provided by

traditional experimental psychology was shown to be just that. “It does us good,” Binet (1911b, p. 47) concluded,

to see that abyss that separates the two conceptions of the mental life: the one, the traditional, so rational, which puts explanation, logic, behavior everywhere, which supposes that in the mental life all is explicable, all is coordinated, all can be justified; . . . and opposite this theory, stands the new one, a theory of action, according to which the psychic life is not a rational life, but a chaos of shadows traversed by flashes, something bizarre and above all discontinuous, which only seems continuous and rational because afterwards one recounts it in a language which puts order and clarity everywhere; but this is a false order, a verbal illusion.

In contrast to the classical picture of the mind as rational and ordered, Binet presented the modern: the mind as a chaos of fleeting thoughts and impressions whose workings occurred as much at the unconscious level as at the conscious. This was not the picture of the mind subscribed to by most other French psychologists, to be sure. But it did resonate with Bergson's and, to a degree, with Pierre Janet's; moreover it dovetailed nicely with the new psychoanalytic understanding of the mind being developed by Freud and propagated in France by none other than Kostyleff.²⁶

Binet's death in 1911 deprived French psychology of a central figure willing to advocate for this view of the mind directly. The ravages wrought by World War I, however, also did their share in convincing many that human irrationality was as powerful a motivating factor as human rationality, and that positive experimental science should not devolve into a kind of brutal mechanical objectivity that lost all sight of the individual and idiosyncratic in its pursuit of scientific truth. For much of the next two decades, French psychologists continued to embrace a pluralistic approach to understanding the mind, seeking a form of positive knowledge that need not be derived solely from attention to external phenomena, but could include interior and subjective experiences as well. If French psychology “found its true path,” it turned out to be one that included a number of different routes, along with the belief that the complexity of the mind necessitated a plurality of approaches.

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²⁵ Notice that connections were drawn between crisis and revolution well before Kuhn (1962).

²⁶ Brain (2008), Cray (1998), Hecht (2003), Micale (2004); and Monroe (2008), esp. ch. 5.

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