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Crisis discussions in psychology—New historical and philosophical perspectives

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ABSTRACT

In this introductory article, we provide a historical and philosophical framework for studying crisis discussions in psychology. We first trace the various meanings of crisis talk outside and inside of the sciences. We then turn to Kuhn's concept of crisis, which is mainly an analyst's category referring to severe clashes between theory and data. His view has also dominated many discussions on the status of psychology: Can it be considered a "mature" science, or are we dealing here with a pre- or multi-paradigmatic discipline? Against these Kuhnian perspectives, we point out that especially, but not only in psychology distinctive crisis declarations and debates have taken place since at least the late 19th century. In these, quite different usages of crisis talk have emerged, which can be determined by looking at (a) the content and (b) the dimensions of the declarations, as well as (c) the functions these declarations had for their authors. Thus, in psychology at least, 'crisis' has been a vigorous actor's category, occasionally having actual effects on the future course of research. While such crisis declarations need not be taken at face value, they nevertheless help to break the spell of Kuhnian analyses of psychology's history. They should inform ways in which the history and philosophy of psychology is studied further.

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1. The many meanings of 'crisis'

This special issue is devoted to the analysis of discussions of the crisis in psychology that took place from the 1890s through to the mid-1970s. Before we explain why this is of interest for the history and philosophy of science and how the essays collected here address it, and before we present the main findings, some reflection on the concept of crisis in general is in order. As will become clear, in different contexts the term has had quite different meanings. This will help us to understand various core aspects of discussions of crisis in the sciences, particularly those in psychology.

The term 'crisis' (and its cognates in other languages, like German *Krise* or *Krisis*, French *crise*, and Latin and Spanish *crisis*) has been, and remains, an important catchword. The history of the term is too complex to be given here in full (see Koselleck, 1982/2006; Shank, 2008), but we can sketch several important points. The ancient Greek word κρίσις (derived from the verb κρίνειν, meaning 'to separate', 'to distinguish', 'to judge', or also 'to decide') meant 'evaluation' or 'decision', or even 'culmination'. Originally, 'crisis' was predominantly used as a medical term, found in texts of the ancient Greek physicians Hippocrates and Galen (Cortés Gabaudan, 2011), and as a term from military theory. In both cases it meant a turning point or crucial stage, either within a disease or battle, namely, the precise moment at which the recovery, or death, of a patient, or the outcome of a battle becomes clear. This encouraged use of the term to also indicate 'culmination' or 'decision', implying the presence of stark alternatives in choice. In the Enlightenment, the concept was also carried over into other areas, such as politics, economics, sociology, and psychology. The term also became emotion-laden and associated especially with discomfort and danger. Moreover, in contrast to the original meaning, a crisis could now also be something extended through time, or even as something permanent. Finally, like the term 'revolution', the concept of crisis also found its way into theories of cyclical processes. The frequency of economic crises hardened the belief that crisis is always a transitory state, and that, for instance, the sole cause of fiscal depression is material prosperity. Crises came to be seen as natural phases of business cycles, running their

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course according to laws of economics (Koselleck, 1982; Masur, 1968).

Despite the often threatening character of crises, they have also been credited with sometimes having positive effects, or with having created new opportunities. Marx noted that crises periodically destroy not only existing products, but former productive forces as well, and so are important for the development of human history. Since John F. Kennedy, politicians have pointed out that the Chinese word for 'crisis' is composed of two characters, one meaning danger, the other opportunities. This seems paradoxical, but expresses the hope that an unwelcome crisis may have "cleansing" or healing effects. Crises, therefore would only appear to be a threat to inefficient, or otherwise "ill" agents.

During the nineteenth and twentieth centuries, the word 'crisis' also came to be applied to specific epochs of history. The *Krisenjahre*, for example, a time in Germany between 1919 and 1923 characterized by hyperinflation and attempted coups, were explicitly considered years of crisis, as was the great depression that began in 1929. Particularly during the Weimar Republic, talk of crisis was widespread, touching every aspect of human life (Peukert, 1987). Accordingly, Föllmer and Graf (2008) call the society of the Weimar Republic a *crisis culture*. They and several other historians have argued that political and social developments and events nurtured a profound sense of insecurity and crisis (cf. Ash, 2000; Ringer, 1969; Müller-Seidel, 1998; Weisbrod, 1996; Paul, 2008). World War I and dramatic changes in political order, social conflict, and the economic pressures of inflation and unemployment all threatened fundamental values of society, encouraging a certain awareness of crisis among contemporaries (see e.g. Rotenstreich, 1947; Urban, 1941). Other periods also became seen as infected by crisis, and the duration of crises could vary from decades to centuries. Egon Friedell's (1927–1931) monumental 3-volume cultural history of Europe since the Renaissance was subtitled *Die Krisis der europäischen Seele von der schwarzen Pest bis zum Ersten Weltkrieg* (*The crisis of the European soul from the plague to World War I*). In 1935, French historian of ideas Paul Hazard published his then influential *La Crise de la conscience européenne*, referring to the years between 1680 and 1715. He identified a crisis of "classicism", following the religious conflicts of the 17th century, which paved the way for the Enlightenment's *esprit de critique* with its emphasis on free thinking, reason, and science. Alexandre Koyré, again, took up the phrase of a "crisis of European consciousness" in his own account of the scientific developments of the 16th and 17th centuries (Koyré, 1957, p. vii).[2]

These are the major historical meanings and references of the term 'crisis'. One conceptual point, however, is worth highlighting in order to dispel naiveté from our regard for crisis declarations in psychology (and in science in general): All these various concepts of crisis assume that some particular system (or science) can be described in functional terms, i.e., in terms of whether it is functioning well, or not. If it is not functioning well, certain strategies are needed to overcome the crisis. In other words, there is a *normative* dimension to crisis talk. Also, the criteria for such talk will surely be disputed by various authors. Thus the meaning of crisis talk may easily change with the passing of time.

2. Crisis in the history of science: Two Kuhnian claims

In today's history and philosophy of science, the concept of crisis is firmly associated with Thomas Kuhn's theory of scientific development.[3] This is so despite the fact that his concept of crisis has not been examined all too closely. Over the past fifty years, many objections have been brought forward against *The Structure of Scientific Revolutions* (Kuhn, 1962/1970), for example, that paradigm shifts are not as sharp as Kuhn claimed them to be, at least not in every case; or that an improved understanding of the historical record and of the cognitive procedures used by researchers reveals that scientific change is often more continuous and less revolutionary than Kuhn thought; or that his concepts of incommensurability, paradigm, and revolution are vague and ambiguous, and that assumptions he made based on them are questionable (e.g. Anderson, Barker & Chen, 2006; Bird, 2000; Hoyningen-Huene, 1993; Kitcher, 1995; Krüger, 1974; Lakatos and Musgrave, 1970). Not that both Kuhn's followers and adversaries ignore the concept of crisis, but they neglect examining it closer. *A forteriori*, they fail to relate it to both earlier and later thought on science.

This is odd, given the crucial role crises play in Kuhn's account of the rise of scientific revolution. For Kuhn, most revolutions are anticipated by a crisis of a dominant scientific paradigm; and, conversely, crises are often resolved by those revolutions.[4] Furthermore, he makes two assumptions regarding the concept of crisis that are worth reviewing.

(1) Kuhn maintains that scientists hardly, if ever, acknowledge states of crisis in their field. Although scientific crises occur regularly in the sciences, "explicit recognitions of breakdown are extremely rare" (ibid., p. 84). The constraints of normal science usually prohibit any such diagnosis. In other words, Kuhn's notion of crisis is not that of an actor's category, but a historian's or analyst's category. Furthermore, while Kuhn emphasizes shifts in scientific standards, he does not apply such historicism to his own notions of revolution and crisis. But, as we pointed out above and as the contributions to this issue reveal in more detail, there are different notions of crisis and the criteria for them can easily change.

(2) For Kuhn, crises always follow the accumulation of significant anomalies. Science devises theories for the purpose of explaining and predicting phenomena. The "core of the crisis" then, lies in the fact that large amounts of observed discrepancies exist between theory and phenomena, or data, which lead to a "technical breakdown" in normal scientific puzzle-solving (Kuhn, 1962/1970, p. 69). What within a normal scientific tradition is seen merely as a "puzzle", to be solved by standard procedures of the ruling paradigm, then becomes a serious threat to dominant theory (ibid, p. 79).

The authors of this collection of essays show to the contrary that it is not so very rare for scientists themselves to diagnose their field as being in a crisis. Moreover, when, in the past, they proclaimed such a crisis, that by far not always involved significant clashes between theory and data. This means that we need to reflect and better understand the history of the concept of crisis. The work in this issue shows that Kuhn overlooked a longstanding tradition of reflection on crisis in science, an effort pursued by members of various scientific communities and by several philosophers as well. One field in particular, namely psychology, seems

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[2] Recently, Jonathan Israel (2002) has argued that Hazard's decision to let the crisis start around 1680 is too late, since it ignores the impact the "new philosophy" had between 1650 and 1680.

[3] It is somewhat interesting to note that in 1941 Kuhn (then a young student at Harvard) spoke of a "major crisis" in his own life, induced by the question of whether the USA should engage in the war in Europe, since such an intervention would imply overthrowing the radical pacifism that he had acquired in his youth (Kuhn, 1941; see Andresen, 1999). But we do not suggest that his notion of crisis in science was prefigured in this personal crisis.

[4] In the first edition of *The Structure of Scientific Revolutions*, Kuhn still held that *all* revolutions are precipitated by crisis (e.g. 1962/1970, p. 67f., 77), and that revolution is the *only* way to overcome a crisis. Later he revised both claims, admitting that some revolutions may come about without previous crises—i.e. some radically new theories might emerge without the previous accumulation of significant anomalies (see 1962/1970, p. 181)—and that revolution is but one of three ways in which a crisis can be resolved (ibid., p. 84). See Hoyningen-Huene, 1993, pp. 232f., 235.

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to have been often exposed to pronouncements of crisis, and lies therefore at the center of our attention.

For the moment, let us reflect on what to think overall of crisis discussions in the history of psychology, and how one might revise Kuhn's two claims, given what we now know about the actor's views and various pronouncements of crisis in psychology. One might eye this suspiciously, since Kuhn saw his theory as applicable to mature natural science only. He doubted that his theory applied to the social sciences (Kuhn, 1962/1970, p. 15), and it is not clear what he thought of the development of psychology.[5] However, psychologists and historians of the field frequently debate whether their discipline squares with his model, and after the 1970s some psychologists even found it attractive because if we have identifiable crises and revolutions in psychology, does that not prove its status as a mature scientific discipline? Others felt that psychology lingers in a pre-paradigmatic or multi-paradigmatic state, or that Kuhn's framework needs revising.[6] If only to break this spell in the debate over the development of psychology, it helps to consider problems arising with both of Kuhn's claims.

After raising doubts about both claims based on crisis pronouncements from different sciences (section 3), we will provide an overview of the crises discussions in psychology treated in this book (sections 4–5) before summing up major findings and indicating what needs further research (section 6).

3. Crisis talk in science and philosophy before Kuhn

Let us begin with counterexamples to Kuhn's first assumption. In many cases that antedate Kuhn's work, scientists themselves explicitly made public the awareness of a crisis in their field. The counterexamples involve very different disciplines, in both of what Kuhn called "mature" and non-mature sciences.

One case that Kuhn (1962/1970, p. 83f.) acknowledged is well known. After the ground-breaking developments of relativity theory and quantum mechanics in the early twentieth century, many physicists said that their field had fallen into crisis (Einstein, 1922; Kremer, 1921; Stark, 1922; see Seth, 2007).[7] But during the same period, because of the well-known antinomy of set theory (Russell, 1903, chap. 10) and ensuing fundamental debates between intuitionism and formalism (e.g. Weyl, 1921; Hölder, 1926; Hahn, 1933), mathematicians were equally confronted with such assessments made by members of their own trade. In those contexts, even some ancient and early modern mathematical problems came to be described as crises as well (Hasse and Scholz, 1928; Waerden, 1940; for criticisms of these descriptions, see Becker, 1959; Thiel, 1972, 1995).

Another case in point stems from medicine, a socially and scientifically respected discipline. In the late 1920's, under the heading of "crisis in medicine", a series of heterogeneous debates took place in Weimar Germany, involving eminent physicians like Ferdinand Sauerbruch. There was a lack of trust in academic medicine, and disappointment with it; medicine's scientific ambitions threatened to outweigh its focus on therapeutic healing and the medical class fell into disrepute (see Knipper, 2009). Likewise, logicians (Menger, 1933), economists (Pohle, 1911; Stolzmann, 1925; Jansen, 2009), historians (Troeltsch, 1922; Strzygowski, 1923; Paul, 2008), and, perhaps inevitably, philosophers (Flechtner, 1940) began to claim that their disciplines were in crisis. Naturally, their grounds for declaring crises differed, sometimes strikingly so.

Now, in clear response to these inner-disciplinary debates, philosophers and philosopher-scientists from disparate camps began extending crisis talk to more or less all sciences. Martin Heidegger and Karl Popper were among them. Husserl was too, but we pass over him here because Uljana Feest examines his case closely (more examples can be found in Mark et al., 1933; Urban, 1941).

Heidegger (1927, §3) claimed that "the level which a science has reached is determined by how far it is *capable* of a crisis in its fundamental concepts. In such immanent crises of the sciences the very relationship between positively investigative inquiry to the things studied begins to waver." We have a dual use of crisis talk here: First, the crisis of a science's basic concepts determines the degree of reflection about conceptual presuppositions of that science. Second, Heidegger suggests—in his somewhat opaque way—that we need such reflection because of "immanent crises" in the sciences, when researchers can no longer study their subject matter using common empirical methods. Because he assumes that "positive investigation" alone is the way that scientists normally proceed with research, in other words, without pondering conceptual presuppositions, Heidegger also says that the "proper 'movement' of the sciences takes place in the more or less radical and in itself intransparent revision of basic concepts."[8] So although a crisis involving critical deliberation is needed, and although the revision of concepts may be radical, scientists themselves are normally not clearly aware of it.

Popper expressed a quite different view. In 1932 he wrote to Egon Friedell, alluding to the latter's already mentioned history of the "crisis of the European soul" and characterizing his own upcoming *Die zwei Grundfragen der Erkenntnistheorie* (forerunner to *Logic of Scientific Discovery*): "My book is an epistemology, more precisely a methodology. It is a child of its own time, a child of crisis—even if predominantly a crisis of *physics*. It asserts the permanence of the crisis; if it is right, then crisis is the normal state of a highly developed rational science."[9] In contrast to Heidegger,

[5] It would be nice to clarify this. Unfortunately, Kuhn never stated clearly whether or not to include psychology among the mature sciences. This is a bit puzzling, given his own use of Gestalt psychology, perceptual research, and so on. He later (1991) argued that we must constantly reinterpret the social and political world and therefore can never achieve the level of "normal science" when studying them. But because he referred to the political and social sciences, his claim might not straightforwardly refer to psychology, situated as it is at the intersection between the natural and the social sciences.

[6] For a positive application of Kuhn to the history of psychology see e.g. Brauns (1997), Caparrós (1985), Palermo (1971), Weimer (1974), and Weimer & Palermo (1973). Others, like Staats (1983, 1999) and Warren (1971) took up Kuhnian views as well, but argued that psychology lacks a unified paradigm and so was (or is) not a mature science (see also the Special Section on Crisis in Psychology in the *Journal of Social Distress and the Homeless*, 5 (1996); Kelly, 1998). Various basic objections to applying Kuhn to psychology can be found in e.g. Briskman (1972), Leahey (1992a, 1992b), and Westmeyer (1994). These lists could be continued, since references to Kuhn are almost endless. Coleman & Salaman (1988) show in numbers how popular Kuhn has been for psychologists, and argue that many of them understood or used him merely in limited and even superficial ways. As to why Kuhn might have become so attractive to psychologists, see Driver-Lynn (2003); but see also Green (2004).

[7] Kuhn also refers to the preface of Copernicus' *De revolutionibus* as "one of the classic descriptions of a crisis in science" (Kuhn 1962/1970, p. 69). The terminology of crisis does not show up in this text, however. This is an instance of 'crisis' as analyst's category. One might of course argue that it was a crisis nevertheless, at least on Kuhn's understanding (he also speaks of the state of astronomy prior to Copernicus as a "scandal"; ibid., p. 67). This cannot be discussed here; but it is a matter of serious dispute (see e.g. Gingerich, 1975; Griffiths, 1988).

[8] Engl. transl. by the authors. The full German original of the two passages cited here is as follows: "Die eigentliche 'Bewegung' der Wissenschaften spielt sich ab in der mehr oder minder radikalen und ihr selbst nicht durchsichtigen Revision der Grundbegriffe. Das Niveau einer Wissenschaft bestimmt sich daraus, wie weit sie einer Krisis ihrer Grundbegriffe *fähig* ist. In solchen immanenten Krisen der Wissenschaften kommt das Verhältnis des positiv untersuchenden Fragens zu den befragten Sachen selbst ins Wanken." Heidegger explained himself more closely in a university course in 1928-29, see Heidegger, 2001, pp. 26-45.

[9] Engl. transl. by TS. German original: "Es ist ein Kind der Zeit, ein Kind der Krise - wenn auch vor allem der Krise der Physik. Es behauptet die Permanenz der Krise; wenn es Recht hat, so ist die Krise der Normalzustand einer hochentwickelten rationellen Wissenschaft." Popper to Friedell, 30 June, 1932. *Hoover Institute Archives. Karl Popper Papers*, box 297, file 22.

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Popper does not see crisis as something that occurs only occasionally in science, but as a permanent, normal, and even desirable feature of at least "rational science". Obviously, Popper here uses crisis talk to hint at his (then emerging) view that criticism and a critical attitude is essential to science.

The fact that scientists themselves have been aware of crises puts serious strain on Kuhn's first claim. And that philosophers as different as Heidegger, Popper, and Husserl entered into such discussions further underscores how widespread that awareness was. Of course, one might object that we are dealing here with two disparate views of the historical development of science: Kuhn's theory on the one side, and on the other reflections on crisis made by several scientists themselves, and taken up by contemporary philosophers. From historical evidence for the latter kinds of discussion one cannot immediately refute Kuhn's paradigm-shift approach. After all, it might be argued that ultimately crises and revolutions are only properly recognized from a historical distance, independent of how scientists perceive the state of their art at the time they are working. But this view takes Kuhn's conception of crisis as being independent of criteria and as an absolute standard; and this must be questioned.

We now turn to the crisis discussions in psychology studied in this publication. One effect this will have is to undermine Kuhn's, ahistorical claim that crises in science are always about problems in the relationship between theory and data. To historicize the concept of crisis, it is necessary to reflect on how and when crisis declarations emerged, and on the possible function they have or were intend to have for the development of a given science.

4. Crisis declarations in psychology: The questions

Psychology has seen numerous such declarations of crisis (see e.g., Goertzen, 2008). Despite their frequency, however, many researchers fail to take such pronouncements seriously (e.g. Gruber and Gruber, 1996; Westmeyer, 1994). While the large number of claims of crisis might be seen as undermining Kuhn's view that crisis is not a category used by the scientist himself, the denial of the importance or credibility of such claims by other researchers seems to speak for his view. We do not claim, of course, that all or even the majority of psychologists are or have been claiming that their science is in a crisis. If the measure is the whole community of psychologists, those who do perceive crises are most likely in a minority, even in more disputed sub-fields.[10] When compared with other sciences, however, the number of explicit crisis declarations in psychology is quite large and provides ample material for research in the history and philosophy of psychology.

More often than not, declarations of crisis have been viewed as being symptomatic of a *real* crisis (e.g. in the Marxist or "critical" tradition in psychology: Jaroschewski, 1974; Fritsche, 1981; Maiers, 1988; Teo, 2005, but also in other approaches: e.g. Hildebrandt, 1990, 1991). Since such works take for granted that there was or is a real crisis in psychology, they try to advise on how to overcome it (e.g. Bakan, 1996; Buss, 1978; Goertzen, 2008; Gummersbach, 1985; Jüttemann, 2005; Wellek, 1959a, 1959b; Yela, 1987, Zittoun, Gillespie & Cornish, 2009). Because criteria can shift (see section 1), however, it is overly naïve to take crisis talk at face value. The essays collected here do not themselves purport to solve any crisis in psychology. Instead, they raise questions like these:

1. In what contexts do diagnoses of crisis occur? How did contemporary psychologists perceive the historical development and state of the discipline at a given time?
2. What terms were used to diagnose a crisis? Was the crisis said to be due to methodological or ontological presuppositions of psychology, to theoretical or empirical problems, or perhaps to demands for the practical importance of psychology for society that the discipline fails to meet?
3. How adequate are the crisis diagnoses, given the state of the art at a given moment and location?
4. Are diagnoses combined with constructive suggestions for remedy, and if so, which ones?
5. Did constructive proposals have any real effects? Did crisis declarations influence, say, the course of psychology (or related disciplines) and the way histories of psychology were written?
6. How can a historical and philosophical analysis of the development of psychology profit from a study of crisis declarations, their causes, contexts, and consequences?

Naturally, the contributions to this issue do not each answer all of these questions. But they deal with their subjects—the authors and debates under discussion—in ways sensitive to historical contexts and consequences, and spell out the philosophical problems of psychology with which the historical actors were faced. To properly understand the differences between the various diagnoses, it is also essential to closely analyze the *arguments* brought forth by the authors of those diagnoses. This often requires taking account of voices from several sides, opinions of those that defend a diagnosis of crisis as well as those that oppose it, and connecting these with psychologists' general reflections on psychology at the time in question.

While we cannot cover all crises discussions in psychology, the work presented in this special issue aims for balance by investigating crisis declarations from the *fin de siècle* to at least the 1970's, and from contexts as different as Germany, France, the Soviet Union, and the United States.[11]

5. Crisis declarations in psychology: The contributions

The earliest claim that psychology finds itself in a crisis was voiced in the late nineteenth century. It stems from a nowadays mostly unknown Swiss philosopher-psychologist named (Rudolf Willy, 1897, 1899) who undertook a thorough critique of the dominant psychology of his time. Annette Mülberger shows that his considerations emerged at a time when a discussion of fundamental problems of psychology was already underway between Wilhelm Wundt, founder of the Leipzig psychological laboratory in 1879 and teacher of many influential psychologists, and the empiriocriticist philosopher Richard Avenarius, of whom Willy was a student. Willy's diagnosis started from the observation of a growing fragmentation in psychological research. Psychology attracted researchers and practitioners from many other sciences and professions, linked to approaches that could cause the fragmentation of the field. In this sense, Mülberger argues, one may see fragmentation and awareness of a crisis as both being due to psychology's early *success* in science and society.

Willy's crisis declaration and critique was followed some ten years later by another now little-known author, the French-Russian

[10] As one current philosopher of psychology remarks: see Trout (1998, p. S260).

[11] It may seem that crisis discussions, at least prior to the debates on the crisis in social psychology starting in the 1960s (see Faye, this volume), are a European phenomenon. That is, at best, a rule of thumb. In 1912, Kostyleff's (1911) book was reviewed in the USA (Titchener, 1912; Weyer, 1912). A little later, John Dewey noted that "the future of philosophical teaching... seems to be intimately bound up with the crisis psychology is going through." (Dewey, 1914, p. 511) Mary Whiton Calkins (1926), who had studied with William James, reviewed Driesch's work (1925), which had grown out of a lectures series given in the USA, Japan, and China (see Allesch, this volume), and first appeared with a publisher in the USA. Over and above these explicit references to crisis discussions, there were also many instances of profound dissatisfaction with the discipline that did not use the terminology of 'crisis'. Watson's famous manifest for behaviorism (Watson, 1913) is a clear specimen of it.

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psychologist Nicolas Kostyleff. His declaration was a response to high expectations placed on nascent psychological laboratories, institutes, journals, societies, and research practices established since the 1870s in Germany and in other countries (see e.g. Danziger, 1990). Many researchers hoped that new funding and improved frameworks would put psychology on the secure path of an experimental and quantitative science. But these expectations were soon frustrated by doubt and criticism. Were the results of the new experimental studies to be trusted and did they produce objective knowledge? Would different experimental approaches converge to create a uniform framework for theory and methodology? John Carson's analysis deals with these queries for the state of French psychology during the first quarter of the twentieth century. Beginning with Kostyleff's 1911 crisis declaration, Carson examines whether French psychologists during the early twentieth century thought their field was in crisis. He argues that although Kostyleff's crisis declarations (and those of others) did not get much attention, they nonetheless indicated some unresolved issues faced by the French psychological community. Whether the crisis was or was not considered real depended largely on how individual psychologists assessed the implications borne by these "unresolved issues".

The second half of the 1920s may be called "the Golden Age" of crisis declarations in psychology. The diagnosis became so widespread that in 1926 the then influential Viennese psychologist and philosopher Karl Bühler wrote that "one can now even read in the newspapers that there is a crisis in psychology" (Bühler, 1926, p. 455). A great number of such declarations appeared in articles and even book-length studies coming from quite different schools and traditions in psychology (see contributions by Allesch, Hatfield, Hyman, and Sturm in this volume) and from several countries throughout Europe and from North America, having repercussions, too, for philosophy (Feest, this volume).

As Christian Allesch shows, the first statement at that time came from the vitalist biologist and philosopher Hans Driesch, whose *Crisis in Psychology* appeared first in English in 1925, then in German in 1926. Driesch wanted a decision about "which road psychology is to follow in the future". He stressed five "critical" requirements, including that psychologists should convert the theory of psychic elements into a theory of meaning, abandon associationism, acknowledge the significant role of the unconscious, reject any epiphenomenal theory of the mind-body relation, and take account of para-psychological "facts". Driesch saw close parallels in the developments of psychology and biology, namely, a theoretical shift from "sum-concepts" like association and mechanics, to "totality-concepts" like soul and entelechy.

Gary Hatfield's article on Gestalt theorists' thought on crisis in psychology shows that these thinkers shared Driesch's idea that in circa 1900 a crisis in psychology did exist and that it was due to sensory atomism, which failed to deal with meaning. Gestalt theorists also tried to counter the skepticism from so-called "speculative" psychologists (philosophers and historians using hermeneutical methods), that natural scientific psychology cannot handle meaning. Hatfield argues, first, that around the turn of the century there was indeed an explicitly acknowledged crisis in new experimental psychology pertaining to sensory atomism. Second, the Gestaltists responded with new experimental findings and theoretical concepts (*Gestalten*) that challenged that atomism. They did so in both their initial German-language presentations and later on in their works written for an American audience. Gestaltists like Koffka raised problems of meaning only after first presenting their scientific, experimental research. However, while they did introduce phenomenological observations on meaning and perceptual organization, they did not succeed in developing a theory of meaning that would solve philosophical worries.

Thomas Sturm clarifies in his paper the background and philosophical implications of Bühler's *Krise der Psychologie* (1927), and of work by Karl Popper, at the time Bühler's student, who, in his dissertation, tried to extend his teacher's therapeutic suggestions. Bühler described the crisis not as being the failure of one approach to deal with certain phenomena (as had Driesch and the Gestaltists), but as an instance of overflow among competing schools and approaches in psychology—basically, the competition between *Erlebnispsychologie*, behaviorism, and *geisteswissenschaftliche* psychologies. According to Sturm, what is distinctive about the constructive proposals made by Bühler and Popper for dealing with the crisis is that they are both Kantian: Both inquired into the conditions for the possibility of a specific branch of psychology, answering the question by reflecting on the presuppositions made by psychology in its existing forms. For Bühler, this led to an integrative pluralism of method in psychology, exemplified by his new, pragmatic theory of language. Popper pursued, but less successfully, a similar path for the domain of the psychology of thinking. And yet, these Kantian approaches showed that one could take crisis talk seriously without slipping into the more revolutionary tone of other crisis declarations.

In 1926–27, the Soviet psychologist Vygotsky wrote *The Historical Significance of the Crisis in Psychology* (see Hyman in this volume). This book-length text did not become available in Russian until 1982 and was published in English in 1997. Like Bühler, Vygotsky saw the cause of the crisis in a splitting of different schools, but he described the schools in more Marxist terms: objectivism versus subjectivism, materialism versus idealism. He studied the various approaches with a rather definite goal in mind—to build a psychology that would promote his own philosophical agenda. According to Vygotsky, Soviet society had launched an new social experiment: The socialist revolution had prepared the way for establishing social conditions that might help create "the new man" of the future, "the first and only species in biology that would create itself." Vygotsky envisioned psychology as a science that would serve this humanist teleology. To achieve this redirection of psychology, and to identify the causes of the present crisis, he posed a problem: How can we systematically account for the development of knowledge in psychology? As Ludmila Hyman argues, historical irony is at work here. While Vygotsky has been widely applauded for his cultural-historical approach to psychology, and while this approach can be seen as a consequence of the stance he takes in *The Historical Significance*, his philosophical arguments for how to understand and overcome the crisis are rather unsatisfying. Sometimes, therefore, higher-order reflection (or, to use another Kuhnian term here, extraordinary science) cannot help in overcoming a crisis.

Uljana Feest's article turns to a work that has already been widely discussed, namely Husserl's mature work, *The Crisis of the European Sciences* from 1936. Feest reinterprets that work by examining how it relates to crisis discussions in psychology. She shows that Husserl saw psychology as having a central role in philosophy, namely, that of providing a scientific analysis of subjectivity. He also maintained that naturalistic psychologies failed to pursue that task adequately. Among other things, Feest traces central themes of Husserl's *Crisis* back to his relationship with Franz Brentano, and she shows that Husserl was aware of Bühler's book from 1927. This brings out some aspects of the complicated relationship between Husserl and members of the Würzburg School of the psychology of thought, including Bühler: The latter had drawn from Husserl's writings, but Husserl found his central thesis misunderstood. Feest concludes by placing Husserl's work in the broader context of scientific, cultural, and political crisis-discourse of the time.

Among Husserl's broader thoughts on crisis was that of the cultural crisis of European civilization between both World Wars. Horst Gundlach asks how crises discussions could develop in Nazi Germany. He studies a now little known article by psychologist

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Peter R. Hofstätter, another of Bühler's students. The central elements of Hofstätter's diagnosis and therapy recommendations were couched in National Socialist jargon, but—with some care and without apology—one might also suspect something other beneath it. Hofstätter saw the crisis in broader perspective than his teacher, identifying the lack of practical usefulness as the cause of the discipline's "fatal crisis" (*Absterbenskrise*). As Gundlach shows, Hofstätter wanted a crucial feature of psychological practice to be the secular, non-therapeutic guidance of individuals. His work was perhaps linked to his ambition for the Berlin university chair of Wolfgang Köhler, and other factors of context, and his Nazi terminology might have been meant ironically. In any case, after World War II Hofstätter became influential in the reconstruction of psychology in West Germany.

After the end of World War II claims that psychology was in crisis became less frequent, but they returned time and again, sometimes for special areas such as social psychology or psychoanalysis, sometimes for the whole field (see e.g. Wellek, 1959a, 1959b; Fromm, 1970; Flammer, 1978; Mertens & Fuchs, 1978; Westland, 1978; Staats, 1983, 1999). As Cathy Faye shows, throughout the 1960s and 1970s many social psychologists diagnosed their field as suffering from a state of disciplinary crisis. Directly after World War II, social psychology had gotten increased funding and began influencing social policies. Its influence was turned down, and the question was why. Psychologists were concerned about conceptual, methodological, and theoretical weaknesses in their research, and especially about its practical impact (or increasing lack thereof). As some of them said, a "crisis of confidence" had shaken the foundation of their discipline. Faye's analysis suggests that the crisis reflected a larger crisis in American society and also drew on the language of crisis prevalent at the time. Employing this language may have offered the field a way of making sense of, reframing, and redirecting internal and external critiques of the discipline.

6. Conclusions

Let us sum up three major results, differentiating the talk of crisis in terms of (1) contents, (2) dimensions and (3) the function the idea of crisis had for those engaged in crisis discussions, and close by asking how such discussions may be used for studying the development of psychology through history.

(1) In the course of the last century, crisis talk in psychology had quite different *contents*.[12] If psychology were in a permanent crisis, or tended to crises more often than other sciences, that would not only or even primarily have to do with problems in the relationship of theory to data, as Kuhn's views require. Nor did all crises in psychology involve a lack of unity in the field (Goertzen, 2008; Yela, 1987). Psychology has no one single persistent problem, perhaps not even a clearly definable set of such problems. The problems that Willy said cause the crises of psychology were different from those that Bühler, Vygotsky, Driesch, Husserl, or current psychologists pointed out. For instance, while Bühler saw the crisis in terms of a fragmentation of *methodologies*, Driesch saw a major cause (also) in deep *ontological* dilemmas of the mind-body relation. Again, social psychologists in the 1970s struggled with entirely different methodological problems—for instance, in many social psychological experiments subjects exercise self-impression management, or submit willingly to the authority of the experimenter, even in questionable tasks. Then there were also societal and political demands that psychological research be practically useful (see Parker, 1989).

To add another example not included in the present volume but that deserves close analysis, (Kurt Lewin, 1927, p. 376; cf. Lewin, 1931, 1977; Métraux, 1992) thought that psychology's crisis was due to having an inadequate conceptual framework. He claimed, for instance, that psychology was undergoing a transition from an "Aristotelian" framework (a framework of, among other things, value-laden concepts that classify things along historical and geographical lines, directed at formations and phenotypes, and leads to mere regularities) to a "Galilean" framework (one that, among other things, uses [allegedly] value-free concepts that classify things along causal-genetic lines, is directed at processes and genotypes, and leads to strict scientific laws).

(2) The concept of crisis in science covers not only—as just shown—a multitude of different contents but, moreover, has different *dimensions*: permanent or temporary, constructive (*Aufbaukrise)* or fatal (*Absterbenskrise*). Four basic options emerge from this. While Bühler viewed the crisis as a temporary and constructive one (along with Willy, Kostyleff, Driesch and others), Hofstätter saw it as temporary and destructive. Again, Popper, in his 1932 letter to Friedell, characterized crises in the "rational" sciences as permanent and constructive. In debates over social psychology, Parker has maintained that the field is in permanent crisis or, alternatively, "racked by a number of intersecting crises" (Parker, 1989, p. 9)—a paradigm crisis, a political, and a conceptual crisis. Unlike Popper, he doesn't see that as a sign of good health but still as something that can be overcome constructively, although that takes more than philosophical and scientific work. Finally, one may observe just who viewed the crisis in psychology as permanent and fatal or destructive. Perhaps one might here cite those who, being aware of the repeated crisis declarations in psychology, conclude that the discipline never was and never will be a mature science—as Kuhn felt about the social sciences.

(3) As these points show, in psychology (as in other disciplines), 'crisis' has often been an actor's category—contrary to Kuhn, who thought of it more as being an analyst's category. (Not without irony did his views became influential for self-characterization by psychologists during the "cognitive revolution" of the 1960s and 1970s; see e.g. Palermo, 1971; Weimer and Palermo, 1973). For the agents who produced them, crisis declarations could, even simultaneously, serve different *functions*. On the one hand, some used 'crisis' as a catchword for directing the reader's attention to problematic aspects of the field. In doing so, they typically tried to promote their own research agenda or even personal careers (as perhaps was the case with Kostyleff and Hofstätter, and perhaps for some of the statements in the debate on social psychology from the 1960s and 1970s). On the other hand, those same authors frequently expressed concern about the current state and future of the field. Most crisis declarations called for change, referring to both the problems and potentials of the situation. In this sense they do have similarity with Kuhn's view that crises often (though not always) lead to new and potentially progressive developments in science. But, naturally, not all crisis declarations were successful. Some philosophers tried to help psychologists—but failed, because their view was dismissed as non-expert or their proposals too remote from actual psychological research and the knowledge of the time (as for Driesch, but also for Husserl). Others also invested philosophical arguments, but ones reasonably close to the psychological state of the art or to fundamental problems that actually troubled many researchers. Clearly crisis declarations reached greater acceptance when pronounced by recognized researchers, who based their claim on a thorough critique of existing approaches and the related epistemological and ontological

[12] Some earlier efforts have been made in offering typologies of crises (e.g. Wellek, 1959a, 1959b; Westland, 1978; Gummersbach, 1985; Caparrós, 1991; Hildebrandt, 1991, p. 11f.). Many details would have to be discussed more carefully than is possible here.

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assumptions. Some of these declarations even included reflections on which methods to use at this meta-level of thinking. Two authors that fulfill this condition are Bühler and Vygotsky. And, as one can see from their cases, thinking about crisis could even lead to actual changes and innovations in psychological research.

These points have significant implications for the way the history of psychology should be written. Crisis declarations usually crop up when scientists interrupt or stop their normal research and reflect on the development and state of their discipline. In doing so, they invite their colleagues to reflect on the state of their art and to evaluate the historical development and outcome of their field. Roughly, we can distinguish two types of such reflection: one positive or even triumphant, to be found mostly in textbooks or Whiggish histories, and the other negative or critical, to be found in crisis declarations and similar texts. While in the past history of psychology the first kind of reflection, found, for example, in Edwin G. Boring's well-known *A History of Experimental Psychology* (1929 and 1950) has been scrutinized (see e.g. Danziger, 1990; Hatfield, 1997), it has still to happen for the second line. Textbooks, as Kuhn has noted, codify and unify the scientific knowledge of a time and try to impose consensus. In contrast, crisis declarations and similar statements reflect, so to speak, the open wounds of a discipline. The historian and philosopher of science should deal with both, asking also about the dynamics between them.

In other words, it is our hope that the present volume paves the way for taking crisis declarations more into account when studying the development of the field.[13] Models of major shifts as well as divisions in the history of psychology ought not be merely developed from the outside, as has been done by psychologists and historians of psychology who use Kuhn's theory or closely related alternatives. Kuhn's model should at least be regularly contrasted with the views expressed by insiders and contemporaries, or with their perceptions of crisis (and revolution) in psychology. As mentioned above, we do not take the actors' crisis declarations at face value; they must be viewed with a critical eye at least. In the same vein, we do not contend that the historian and philosopher can speak of crises in psychology only when actors themselves acknowledge them.[14] But given that not few of those who declared such a crisis developed their views based on thorough philosophical reflections, sometimes with a close understanding of the state of the art, and that some statements even had an effect on the course of psychology's history, it would disclose a certain blindness to ignore them further. (We cannot say here whether similar claims hold for other disciplines.)

Having presented the main findings of the articles in this volume, and their broader significance for the history of psychology, let us close with a point that we stumbled across at various times in our research, but that we could not pursue further. It brings us back to Kuhn. Might there exist a connection between the history of crisis discussions in psychology and the history of philosophy of science leading up to his work?[15] Some of the psychologists speaking of crisis in their field influenced philosophers and historians of science that indirectly or directly influenced Kuhn. Bühler's 1927 *Die Krise der Psychologie* was then and is now counted as perhaps the most sophisticated attempt to understand and cope with a crisis of the field. As noted before, it influenced not only Popper but also Husserl (see the contributions by Sturm and Feest). Husserl, in turn, was a teacher of Alexandre Koyré, whose influence upon Kuhn need not be emphasized here. Koyré—as noted above—not only took up Hazard's (1935) terminology of a "crisis of the European consciousness" when thinking about major scientific changes. Also, he seems to have been acquainted with Husserl's *Crisis* book, and perhaps they exchanged their views on the impact of Galileo for modern science (see Gandt, 2005, pp. 97–103). But Kuhn never mentioned Bühler or Husserl, and it is not clear that the topic of crisis was passed on to him via Koyré. Still, the history of philosophy of science may have its subterranean pathways.

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[13] This resembles the approach taken by Friedman (1993), who calls for a connection between the history of philosophy and the history of science in order to rationally account for long-term developments in science.

[14] This is directed against the "rule" formulated by Seth (2007, p. 29): "crisis, as a descriptive category for the historian, can only be utilized when it is an actors' category". Seth also says that if "actors do not speak of crisis *or something provably similar* . . . we cannot invoke crisis as either an explanation of behaviour or as a description of interaction" (emphasis added). This formulation opens up many possibilities, weakening his own rule. It is better not to state such a strict rule in the first place.

[15] We owe this question to Lorraine Daston.

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