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This paper sketches a historical account of interdisciplinarity. A central claim advanced is that the modern array of scientific and humanistic disciplines and interdisciplinarity emerged together; both are moving targets, which must therefore be studied historically in relation to one another as institutionalized practices. A second claim is that of a steadily increasing complexity; new fields emerged on the boundaries of existing disciplines beginning in the late nineteenth century, followed by multi- and transdisciplinary initiatives in the twentieth, and finally transdisciplinary programmatic research in the late twentieth and early twenty-first centuries. The latter two phases in this development have been driven primarily by funding agencies seeking to move the sciences in particular directions deemed socially or politically desirable (in dictatorships as well as democracies), while the existing disciplines remained in place and new ones came into being. Such policy initiatives have transformed both disciplinarity and interdisciplinarity in unanticipated ways. The question whether multi- or transdisciplinary arrangements produce epistemically better science or scholarship appears not to have been raised, let alone examined, by the policy actors driving their creation.

1. Introduction

The following remarks are presented from the perspective of a historian of science. This is justified, first of all, because a historical perspective can shed light on what the constraints on interdisciplinary interactions have been over time. Second, historical examples add to our awareness of the actual variety and changing character of such interactions, thus posing a

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challenge to efforts by philosophers and others to establish general definitions of (or norms for) interdisciplinary practices. At the outset, let me state briefly just what a historical perspective entails in this case. On first impression the history of science appears to be interdisciplinary per se, because historical methods are employed to investigate other disciplines. Though this impression is surely not false, given the current state of the field it seems to me more appropriate to call the history of science an interdiscipline, in which historical methods are employed to analyze changes in other knowledge areas over time (For discussion of "interdisciplines," see Barry, Born, and Wezkalnys 2008, as well as Graff 2015). The field is no longer limited either to the natural and medical sciences or to the history of disciplines, but also addresses the historical development of what might be called knowledge areas in the broadest sense, with the result that the history of science and the general history of knowledge are no longer entirely distinct activities (see Alder 2013; Daston 2017). With respect to subject matter, my own approach to history of science is transdisciplinary, in the sense that I limit my studies neither to the history of any single discipline nor to the history of scientific ideas or world views, but rather try to describe and analyze transformations both in knowledge and in the institutions in which it has been produced and distributed over time (for a programmatic statement see Ash 1999). Methodologically speaking, however, my approach is monodisciplinary, in keeping with Lorraine Daston's assessment of the current state of the field: "in large part because of the mandate to embed science in context, historians of science have become self-consciously disciplined, and the discipline to which they have submitted themselves is history" (Daston 2009, p. 808).

What benefit can any of the historical perspectives just named provide in general considerations of interdisciplinarity? Before attempting to address this question, some basic remarks on conceptual issues are needed, for reasons that will become clear below. As is well known, interdisciplinarity (abbreviated in the following as ID) can and should be distinguished from two other forms of research going beyond disciplinary boundaries: multidisciplinarity (abbreviated in the following as MD) and transdisciplinarity (abbreviated in the following as TD) (for a more detailed classification based on this tripartite distinction, see Klein 2010a). Multidisciplinary enterprises bring together work from several disciplines in order to address a single topic or research issue, with little or no transgression of disciplinary boundaries in the individual contributions. An example might be a volume bringing together sociological, historical, and political science approaches to nationalism. Collected volumes of this kind, though they are often labeled as ID, actually turn out to be MD upon closer examination, with ID taking place, if at all, only in the editor's introduction. Transdisciplinarity generally

involves addressing an issue that—as the name implies—transcends disciplinary boundaries, because the topic cannot be dealt with adequately by assigning it to any single discipline or combination of disciplines. An obvious recent example of subject-matter TD is climate science. However, TD may also be methodological, when it entails modes of addressing such issues that are not limited to single disciplines or additive combinations of disciplines. Seen in this light, ID is neither TD nor MD, but such a definition *ex negativo* seems insufficient.

Positive criteria and definitions of ID in the literature are legion, due to the obvious, yet perhaps not entirely banal fact that "ID has many manifestations" (Lamont 2009, p. 204). Some efforts have been made to bring order to this variety by developing "taxonomies" of ID (Klein 2010a). Whether such efforts are of help to philosophers or science studies experts, I dare not say, but to this historian at least three points seem clear *prima vista*. First, analyses of ID, historical or not, must also address and somehow define disciplinarity, explicitly or implicitly (Graff 2015 also makes this point). Secondly, both disciplines themselves and the relations among them, whatever forms they might take, are what social scientists like to call "moving targets." This claim implies that many, indeed most disciplines have not always existed, but have specifiable locations in historical time, and also that even those disciplines that have existed for long periods exhibit change over time (for the last point see Collini 2001). This suggests that classifications or "taxonomies" of ID are little more than snapshots of the situation at a given time. Lumping together past and current forms and styles of ID practice, as often happens in such taxonomical efforts, is not an aid to precise analysis. Thirdly, there is in principle no limit to the varieties of ID, which would appear to render hopeless in advance any effort to derive binding criteria or norms for this concept.[2] Ideally, in genuine ID each of the disciplines involved would adopt vocabulary, concepts, or methods from the cooperating partner disciplines.[3] Such a norm may appear to be attractive on its face, but ID projects do not always or

2. Graff makes a similar point by writing that "There is no single path to interdisciplinarity, no single model, no single standard of successful development" (Graff 2015, p. 5). Yet the book includes multiple examples of what ID is not, suggesting a residual desire to be able to define ID after all.

3. In a programmatic document, the National Research Council advocated such a norm in 2009: "The members of interdisciplinary teams learn from each other to produce new approaches to a problem that would not be possible through any of the single disciplines. Typically, this process begins with team members first learning the language of each other's disciplines, as well as the assumptions, limits and valid uses of those disciplines' theoretical and experimental approaches" (Cited in Graff 2015, p. 4. Graff falsely attributes this statement to the National Institutes of Health). The science policy context of such statements will be discussed below.

even frequently conform to this high standard. Fortunately, the historian's task is primarily empirical rather than normative. Nonetheless, conceptual discussions of ID can be of use for the analysis of specific examples, and also for historical remarks on ID, within the limits I have just tried to suggest.

2. Toward Historicizing ID

Definitions of ID or lists of ID practices often say little about the institutional, social, economic, or policy contexts within which such practices come into existence. When historians use the term ID, however, they speak not only or even primarily about abstract ideas or practices taken for themselves—whatever that might mean—but about institutionalized practices. If we agree with the claim advanced above that disciplines as historical entities are not fixed entities but moving targets, it should be clear that institutionalized practices of ID cannot be taken as given, but also need to be historicized. This means that they need to be queried as to the circumstances in which they came into being, are or are not stabilized, and pass away or develop in new directions. For me, this also means that implicit assumptions or explicit claims about the value or lack of value of ID need to be historicized as well. Of course, I will not be able to present such a complex historicization of both disciplinarity and ID in the space available, but I do hope to give some hints of what such a project might look like.

If we want to take the plain meaning of the term ID—with emphasis on the Latin root inter—seriously, then the relations named by this term must be relations between or among entities called disciplines. This is where the first point made above—the claim that it is impossible in principle to historicize ID without first trying to say something about the emergence and subsequent history of disciplines—comes into play. This brings me to my first thesis:

> Thesis 1: The possible range of interdisciplinarities is dependent upon the range of existing disciplines, however these may be defined. Correlative to this is a related claim: an increase in the number, variety, and kinds of disciplines inevitably leads to an even greater increase in the potential number and variety of ID activities.

The word "discipline" refers to a single, relatively well-defined field of knowledge, often though not necessarily paired with a subject of academic or professional training. Disciplines are thus concerned with the advancement, but also, indeed primarily, with the certification of knowledge. In the latter respect disciplines are conservative by definition, in spite of the claims often made about innovation being the goal of science. Disciplines have existed in some form since there have been schools. That means that disciplines have

always been units of knowing and also units of administration, at least in European and North American culture, and perhaps outside Europe and North America as well, though I cannot go into that here.

In the early Middle Ages, training preliminary to the professions then regarded as truly academic (law, medicine, and theology) was organized according to some version of the canonical seven "liberal arts" (*artes liberales*): the *trivium*: (grammar (= Latin), dialectics (= Logic), and rhetoric (= the arts of speaking and (letter) writing), and the *quadrivium*: (arithmetic (number as such), geometry (fixed numbers, or forms) astronomy (including astrology), and music theory (including the theory of cosmic harmony) (Weingart 2010). The idea was not only to acquire basic education, but also, indeed mainly, to learn what it means to know or prove something at all. Practical knowledge was not the point, though it was assumed that people with well-ordered minds who knew what it meant to know or prove something would be better prepared to learn the "higher" professions just named, or at least to work effectively in administrative positions. Children of the nobility, who were the people with real power in society, did not go to university then, but they did acquire the rudiments of ordered knowledge either from private tutors or later at schools called academies, which despite their names had little to do with the academies of sciences that were founded from the seventeenth century onward. The canon of *trivium* and *quadrivium* ceased to be a universal standard even before the Renaissance, but the idea of acquiring basic knowledge and the means of engaging in critical reflection in the "philosophical propaedeutic" before proceeding to more advanced study in law, medicine, or theology, remained in force. Galileo Galilei originally taught mathematics in such "philosophical" faculties in Pisa and Padua. The emergence of the modern university in the eighteenth and nineteenth centuries, especially the version of it driven by the so-called research imperative, brought a previously unknown dynamism into this mix.

3. Historicizing ID and Disciplinarity—A Three-Phase Model

So how can or should we historicize all of this? We are now ready for more substantive considerations. For the purpose of discussion, let me propose three historical phases to describe what has taken place since about 1800, without making any claim to completeness or precision.[4]

4. Graff (2015) provides a detailed historical account of ID which is in certain respects consistent with what follows; examples from this book will be cited below. Unfortunately, the study is limited entirely to the United States and the period since the late nineteenth century. Moreover, although ID is often and correctly attributed to external factors, the role of funding agencies in enabling or mandating ID is not emphasized or analyzed to the extent it is here.

Phase 1: Disciplinarity and Specialization in the Establishment of the Research University

This phase runs from the late eighteenth through the early twentieth century, which is the period of the conversion of universities, initially in the German states and then in other countries, into combined teaching and research institutions. I emphasize teaching and research together because the establishment of the "research imperative" (as historians of universities have called it since R. Steven Turner (1971) introduced the term) was inseparable from a concomitant transformation in the certification function of universities (Ringer [1969] 1990; Clark 2006). Central here, at least in the German-speaking world where the process began, was the radical transformation of the Philosophical Faculty from a supplier of "general education" in the sense described above to a provider of science-based and scholarly research training for higher-level secondary school (Gymnasium) teachers. The range of the disciplinary canon in this faculty was thus closely connected with, though not strictly limited to, that of higher secondary school subjects. In contrast to often-presented accounts that have emphasized the natural sciences, technology, and medicine (e.g., Stichweh 1984; Tuchman 1993; Lenoir 1997), the following points should be emphasized: no natural science nor the research laboratory, but rather classical philology and the research seminar initially formed the paradigm of *Wissenschaft* in this complex; market demand in the narrow sense surely drove the process in chemistry and some other natural sciences, but not everywhere; and though industrialization and the technologization of agriculture were important stimulants, they were not the only drivers of the institutionalization of the research university.

In any case, the process I am trying to describe was not limited to the so-called "Philosophical" Faculty (roughly equivalent to the Faculty of Arts and Sciences at American universities), but extended to the Medical and Law Faculties as well. Multiple markers can be used to indicate the emergence of disciplines as institutions, including the founding of academic journals and scientific or scholarly societies; relevant in this context is the establishment of designated professorships and their associated research institutes or seminars. Sociologists have long used the term "differentiation" to describe the process involved (Stichweh 1994). This process has two faces: differentiation of science from non- or pseudoscience (e.g., versus religion or psychical research), with corresponding "boundary work" in the public sphere (e.g. Gieryn 1999), and differentiation within the field of science and scholarship, often (misleadingly) termed "specialization." I am discussing the latter process here.

Space limitations do not permit me to go into detail about how this process worked; I can only name some names as pointers in the direction

of a proper analysis. Examples include the separation of "modern" history from "general" history, which then became "ancient history," or the "modern" languages and literatures from "philology" in general, which originally meant the study of Latin and Greek literatures. Examples in the natural sciences include the separation of organic and inorganic chemistry, as well as botany and zoology, as divisions within the part of "natural history" devoted to living things (the integrative term "biology" was unknown until the twentieth century) (Nyhart 1995), and the earth sciences (geology, mineralogy, and petrology), which also split off from "natural history" (Schübel 2010). Toward the end of the nineteenth century, as is well known, theoretical physics became a specialty within physics with its own professorships, while at the same time subdisciplines of experimental physics such as optics and acoustics came to the fore (Jungnickel and McCormmach 1990, esp. vol. 2). In medicine, physiology differentiated from anatomy, and in clinical medicine the specialties such as ophthalmology, otolaryngology, and orthopedics, as well as neurology and psychiatry, had all emerged by 1900 (Lesky 1977. On the separation of laboratory physiology from anatomy, see Lenoir 1997).

None of these differentiations within the scientific and scholarly fields happened automatically, nor were they enacted everywhere at the same time. Yet one point seems salient: the claim that there was societal or market demand for new specialties was often the main reason why governments agreed to fund them; scientific or scholarly arguments were never sufficient by themselves, even when these were put forward by respected professors. In the humanities, but also in the sciences, the need for teachers trained in scholarly research to help deal with the shift to "modern" subjects in the schools (which included both the natural sciences and modern languages) was a common, but not the only argument. The practices needed to persuade funding agents—in Continental Europe during this period mainly state officials—to establish professorships as well as seminars or laboratories for new disciplines, but also journals and scientific societies, should therefore be added to the list of activities I just cited. Such activities were not somehow "external" to disciplinarity, but rather the conditions of its being possible at all. Note that I refer here not only to "scientific," but rather to scientific and scholarly disciplines. Contrary to widespread essentialistic mystifications of the "two cultures" kind, the activities involved in the establishment of the humanities and social sciences, seen as institutionalized practices, differed surprisingly little from the ones involved in the establishment of natural, medical, or technical sciences. It was during this period, by the way, that philosophy became a discipline like any other and began to lose its special status as a meta-level "science of science" or a

privileged location for establishment of norms for science and scholarship as such (Schnädelbach 2009).[5]

The specialization within and the fragmentation of disciplines that resulted from all this was widely lamented from the beginning, yet it was also understood to be an inevitable result of the growth of knowledge. As the physiologist and physicist Hermann Helmholtz put it already in 1862, "One obvious consequence of this vast extension of the limits of science is that every student is forced to choose a narrower and narrower field for his [*sic!*] own research, and can only keep up an imperfect acquaintance even with allied fields" (Helmholtz [1862] 1995, p. 78). Seen in this context, the emergence of the first inter-disciplines toward the end of the nineteenth century might be understood as a complex subset of the differentiation process. Such moves could be made in various ways. One way to go was hybridization: the classic and well-studied case here is that of physical chemistry (Nye 1993). Two kinds of ID practices appear to have been paramount in this case: Empirical studies of the relations of physical and chemical characteristics of substances came first, but the term "physical chemistry" was first used for the production and use of quantifiable analogies between physical and chemical processes (e.g., osmotic pressure or Boyle's law), and especially for the application of mathematical models to both. Early in the twentieth century, new hybrids, such as biochemistry and biophysics, emerged along similar lines. Of course, none of this happened without opposition: as late as 1936 the British chemist Henry Armstrong, referring to Svante Arrhenius' advocacy of physical chemistry, complained of "the intrusion of the Arrhenic faith" into chemistry, which resulted in "the addition of a new class of worker into our profession—people without knowledge of the laboratory arts and with sufficient mathematics, to be led astray by curvilinear agreement… The fact is, the physical chemists never use their eyes and are most lamentably lacking in chemical culture" (cited in Brock 1992, p. 388.). Noteworthy is Armstrong's use of the word "culture" to describe a shared style of working.

Another inter-discipline that emerged in this period is experimental psychology. Here the move was to address issues in philosophy—specifically the portion of philosophy already then called epistemology (*Erkenntnistheorie*) with the aid of natural scientific concepts and practices (Ash 2003, esp. pp. 252–60; Smith 1997). The example of Wilhelm Wundt, who actually migrated from physiology to philosophy, has long been employed in support of the claim that here, too, hybridization was at work (Ben-David and Collins 1966). But his case was exceptional. Far more frequent was the

5. Seen in this light, the Vienna Circle's effort to re-create a "unified science" was the last hurrah of a conception of philosophy that was already decades out of date at the time.

import of concepts and research methods from physics, for example in Gustav Theodor Fechner's psychophysics, or techniques from laboratory physiology, including the use of research instruments like the chronoscope, by younger philosophers (Schmidgen 2005, 2014). Their efforts were regarded for decades as a new approach within philosophy. They were initially viewed with some interest, but then hotly criticized and repulsed from both "sides." Though some physiologists like Ewald Hering proposed to take what subjects actually see as the explanandum of sensory physiology (Turner 1994), which would have required collaboration with empirical psychologists, most worked on the functioning of the sense organs without help from psychology. On the other hand, Neo-Kantian and phenomenological philosophers vehemently rejected what they called "psychologism" in the theory of knowledge, denying that empirical knowledge of conscious mental processes had any implications for arguments about the nature of consciousness as such (Kusch 1995). The resulting legitimation crisis of the new specialty, which some already called a new science, had a practical side; the time required to learn and carry out experimental psychological research practices separated the "new" psychologists from philosophy and philosophers. Nonetheless, they insisted on addressing issues in epistemology and therefore on the philosophical relevance of their results. The obvious alternative—the creation of a new discipline separated from both philosophy and physiology—was first enacted in the USA, where the transfer of experimental methods from Germany coincided with the rise of the research university based on departments rather than German-style Faculties; this institutional coincidence enabled the creation of new professorships and departments of psychology. Not accidentally, the establishment of psychology as an independent discipline in the United States coincided with a shift in the subject matter of the discipline away from conscious experience and toward behavior, without giving up laboratory experiment as the royal road to scientific status. In contrast, psychology professorships in Germany continued to be assigned to philosophy until the 1940s. As these and other examples show, the emergence of the modern system of disciplines and the subsequent emergence of inter-disciplines were historically contingent processes that never happened without resistance. The acts of persuasion that advanced their establishment in spite of that opposition should be part of any history of interdisciplinarity, even when—or especially because—they were not always addressed to scientists or enacted in scientific terms.

Phase 2—Multidisciplinarity (MD) and the Rise of National Funding Agencies

Phase two runs from the early to the last third of the twentieth century. Central here from the institutional side is the transformative impact of the

emergence of large scale privately or publicly supported research funding agencies acting mainly, though not entirely, on a national level. Markers for the beginning of this process are the establishment of the Carnegie Foundation in the United States in 1903 and the Kaiser Wilhelm Society in Germany in 1911. The phase could be seen as more or less completed with the creation of the CNRS in France and the NSF in the USA just after the end of WWII. From this time onward, at the very latest, ID practices were no longer limited to research per se; practices of research evaluation, funding acquisition and management became relevant as well. This brings me to:

> Thesis 2: The fundamental shift from mono-disciplinary modes of research practice, with imports and transfers from neighboring fields and genuine ID as innovation at the margins (or, as would now be claimed, at the cutting edge) to issue-related TD and policy-focused MD began during this period, not at the end of the twentieth century.

It is true enough that these funding agencies worked initially on a disciplinary basis. Funding recommendations at both the Carnegie Foundation and the Emergency Society for German Science (later called the German Research Foundation or DFG), founded in 1920, came from committees made up of leading members of specific fields; indeed, the Emergency Society borrowed its reviewing procedures from those of the Carnegie Foundation (On the history of the Emergency Society/DFG, see: Walker, Orth, Herbert and vom Bruch 2013.) But the founding of the Kaiser Wilhelm Society signaled something different. Its institutes were deliberately not established as foundations for alternative disciplines, even when their names (such as chemistry, physical chemistry and electrochemistry, and later biology) sometimes may have sounded as though this were the case. Rather, the Kaiser Wilhelm Institutes were created as refuges for high level research outside the universities; though they also had a certification function, this was limited to the post-doctoral phase. Such research often had economic relevance, for example in the Kaiser-Wilhelm-Institutes for Metals, Fiber, and Coal Research (Rasch 1987), or policy implications, for example in the Kaiser-Wilhelm-Institute for Anthropology, Human Heredity, and Eugenics (Schmuhl 2005). This differed only to some extent from the situation in the United States, where activities of the Carnegie and Rockefeller foundations and the National Research Council, founded as a private initiative in 1916, strengthened so-called "basic" research at universities, also at the post doc level, while at the same time advancing thematically oriented ID.

In the 1920s, topically defined project-oriented research institutes integrating inputs from multiple disciplines began to emerge alongside funding for mono-disciplinary and ID projects. I cite only three examples here.

The first example encompasses the institutes for child study financed during the 1920s by the Laura Spellman Rockefeller Memorial fund at Cornell University, the University of California at Berkeley, the University of Minnesota at Minneapolis, and the University of Iowa in Iowa City (Cravens 1993); funding from the same program also went to Jean Piaget at the University of Geneva and to Karl and Charlotte Bühler at the University of Vienna (Vidal 1994; Benetka 1995). In the United States, these child study centers were not attached to single departments, but established in separate spaces, which had the effect of marking their ID or MD character. Child or developmental psychology was generally, though not always the lead discipline, but pediatrics, nutrition, and pedagogy (then called "educational science") were also involved. The overall aim was explicitly stated: to provide foundations for science-based educational policy and thus help create a rational society (for the context see Kohler 1991). A second case is the "Atlas of the German Races and Peoples," funded by the Emergency Society for German Science with substantial assistance from the Rockefeller Foundation from 1929 through 1934 (Schmuhl 2005, pp. 118ff.). Results from this transnational project were published, *inter alia*, in a monograph series entitled "German Racial Studies" (*Deutsche Rassenkunde*), which appeared in 17 volumes from 1930 to 1937. The lead discipline here was what was then called anthropology (of the skull-measuring kind, now called physical anthropology), but eugenically oriented genealogical research and genetics were included from the start. Grandiloquent programmatic statements called for nothing less than a "complete survey of the whole person" (*Totalerfassung des ganzen Menschen*), including quantitative measurement of the frequency and variation of allegedly "racial" characters, as well as the impact of the social environment. In practice the work was more MD than ID. The overall goal was alleged to be to help establish a scientific basis for "racial hygiene," though this work was not explicitly placed in the service of selectionist eugenics.

Despite obvious differences, two points are common to both of these examples: the problem-centered approach, and the announced goal of providing scientific basis for policy-making. In view of all of this, it is surely not a coincidence that the first recorded use of the term ID occurs during this period, in a 1930 report of the American Economics Association to the Social Science Research Council, and not earlier. The report states that,

> the Council's thinking thus far has been largely in terms of social problems which cannot be adequately analyzed through the contributions of any single discipline. It is probable that the Council's interest will continue to run strongly in the direction of

> these inter-discipline [*sic!*] inquiries. (Barnett, Secrist, and Stewart 1931, cited in Calhoun and Rhoten 2010, p. 106. See also Sills 1986)

It is difficult to exaggerate the impact of the two world wars on this fundamental shift. Goal-oriented basic research and multidisciplinary collaborations took place during WWI on a vast scale, which was a radically new experience for everyone involved. The cases from physical chemistry, from the development of chemical fertilizers in order to increase agricultural output to gas warfare, are well known (Johnson 1990; Hoffmann 2014), but the actual range of such projects was far broader. Relevant for our discussion are cases in which basic research was embedded in weapons development projects. To name one example: the first measurement of the speed of sound under water took place in connection with efforts to develop torpedo detectors in the context of anti-submarine warfare (Millikan 1920). In a less well-known example, the collaboration of experimental psychologists, acoustics experts, and theoretical physicists led to the development of sound-ranging devices for artillery, which were quite literally human-machine syntheses, because they incorporated human listeners into their operation (Hoffmann 1994). As Robert Millikan's paper on wartime physics published after the war shows, for some of those involved at least it was clear that MD organizational issues were at least as important as research practices in the narrower sense (Millikan 1920, pp. 34 ff.).

The Second World War saw a vast expansion of such enterprises: Large-scale weapons projects like the Manhattan project or the German rocket program are, again, well known (Hackman 1986; Neufeld 1995; Brown 1999), and here too goal-directed basic research was central. In most cases, not hybridization or the transfer of theory or practices from one discipline to another was involved, but rather teamwork involving a pragmatic, sometimes rather rough and ready, blending of theories, models, and research practices with a common practical goal. From the point of view of institutionalized practices, we might speak here of TD without the name. Perhaps less well known is that such efforts were not limited to natural sciences and weapons technology. Additional examples include research by social psychologists and others on troop morale, MD collaborative studies of nutrition and the impacts of rationing, and collaborative work by mathematicians, economists, and others on economic planning (for examples, see Capshew 1999; on operations research in this period see Graff 2015, chap. 3). An example from Nazi Germany is the so-called "War Effort of the Humanities" (*Kriegseinsatz der Geisteswissenschaften*), a clearly MD mobilization effort to bring together humanities research from numerous disciplines in order to support Nazi cultural policy and territorial claims in

Western, Eastern, and Southeastern Europe, which produced a total of more than 540 publications (Hausmann 1999). Closer to TD were spatial planning and forced migration programs such as the "General Plan for the East" (*Generalplan Ost*), developed by agricultural scientist Konrad Meyer with the support of MD research teams and submitted to Heinrich Himmler, for the forced resettlement of farmers from German-occupied territory in Eastern Europe and their replacement by German farmers (Rössler and Schleiermacher 1993). Related to such efforts were MD "area studies" programs such as the "Southeast German Research Community" (*Südostdeutsche Forschungsgemeinschaft*), founded by Vienna cultural geographer Hugo Hassinger with collaborators in 1931 and headed during the war by historian Otto Brunner, which provided detailed expertise in support of German occupation strategy in Southeastern Europe (Svatek 2015). Such examples show that MD efforts in the service of vast political projects emerged before the Cold War.

The experience of such collaborative efforts during the Second World War helped lay the groundwork for the revolution in science funding in the United States indicated by the founding of the Atomic Energy Commission in 1946 and the National Science Foundation in 1950. These and other new agencies made available funding for multi-disciplinary research initiatives on a vast scale, alongside single-disciplinary projects. This process came into research funding in postwar West Germany as well, albeit somewhat later and on a smaller scale, with the introduction of Priority Research programs in 1953 and so-called "Special Research Areas" (*Sonderforschungsbereiche*) in 1966 alongside disciplinary single-topic projects supported by the German Research Foundation (DFG) (vom Bruch 2013, p. 50; Orth 2013). Relevant here is the new kind of ID institutional practice that emerged in this context, now called panel review, in an effort to assess potential value of multi-disciplinary programs. At first this involved little more than an assemblage of evaluators from disciplinary review committees. Transformations of the peer review process that may already have begun at this early stage are not very well researched thus far. Also of interest in this context are so-called "reform universities" founded in West Germany in the 1960s and 1970s, some of which, such as the University of Konstanz, de-emphasized departments and experimented with TD structures (Schregel 2016).

Moving briefly to the epistemic side of the process, I can only list here a series of new hybrids that came into being within this new institutional setting: molecular biology, with elements drawn from at least three disciplines; inter-disciplines in the already established dual mode such as geophysics or astrophysics; and new multi-disciplines or MD alliances, such as cognitive science, behavioral sciences, and computer science (on the "golden age" of the social and behavioral sciences during the early Cold War, see Cohen-Cole 2014; for the epistemic implications of problem-centered

research in this period, see Erickson et al. 2013). Cognitive science was a "multi-discipline" in the sense that there were inputs from cognitive psychology, linguistics, computer science (AI), neurophysiology, and other fields, all focused at least programmatically on the single issue of mind and/or mind-body or mind-machine relationships (Gardner 1985; Graff 2015, chap. 4; for a different perspective, see Boden 2006). MD initiatives in the social sciences and humanities coming out of this period include area studies, black (later African-American), women's (later gender), and of course also science and technology studies.[6] All of these entities and their relations to the new funding regimes just described are relatively well studied individually, but have yet to be seen in the long-term historical context I am trying to sketch here.

Phase 3—The Age of TD: Panel Review and New Regimes in Research Funding

Phase three of this history runs from the late twentieth century to the present and appears at first in many respects to be an extension of phase two, yet something new and transformative appears to be at work. The term "transdisciplinarity" appears to have been coined by Erich Jantsch at an OECD conference in 1970 (Jantsch 1972; See also Balsiger 2004). The meaning of the term expressed at the time differed from the definitions that came later, but a common focus was on the hope of bringing science to bear in the effort to solve broadly conceived policy problems, such as the protection of the environment, paired with the claim that existing discipline-centred academic research was unable in principle to address such challenges.[7] To this problem-centred definition later writers added the further dimension of collaboration with non-academic practitioners: "The core idea of transdisciplinarity is different academic disciplines *working jointly with practitioners* to solve a real-world problem" (Häberli, Grossenbacher-Mansuy, and Klein 2001, p. 4; for background see Hadorn et al. 2008). However, even this definition seems to me to be too limited, so let me suggest a modified one: TD is a response to problems posed from outside the existing disciplinary matrix either by science or by society, the resolution of which is not possible with the tools of any one discipline. Put in American lingo: It's not about connecting two or more boxes, but thinking outside the box.

6. On area studies, see, e.g., Calhoun and Rhoten 2010, pp. 106ff. For the political context, see Krige 2006 and Krige and Rausch 2012. Socialist feminism, along with dialectical materialism, general semantics, General Systems Theory, and others have been cited as examples of "deviant" ID (see Fuller 2010).

7. This critique has since become conventional wisdom: see, e.g., Wilson 2009.

An example of TD coming from within the sciences is nanoscience and nanotechnology (see Mittelstraß 2003; for a broader discussion including materials sciences, see Graff 2015, chap. 5). At the end of the 1950s, physicist Richard Feynman suggested that very small units, literally beyond the frontiers of sight or then-existing apparatus, should be a focus of basic science in the near future. After some delay, chemists and technical scientists took up this idea, and nanoscience and then nanotechnology were born; subsequent steps toward truly remarkable technological innovations in collaboration with economic actors have been widely reported. Well-known examples of TD driven by societal challenges and enacted as co-constructions by scientific and societal actors are ecology (now called environmental science), which achieved wide public attention through the work of Rachel Carson in the 1960s, and population science, which has always included much more than demography. The beginnings of climate science in its present form—linkages of geophysics, atmospheric chemistry, meteorology, and other disciplines—also go back to the Cold War (Bruno 2003; see also Oreskes and Krige 2014, chap. 5. For earlier roots, see Coen 2018). This last example is important because much of the early funding came from the military, even though it was not at all clear whether anything weapons-related would come out of this work.

The science studies community has already addressed this trend for a number of years, and I am not going to add yet another discussion of "Mode 2," "post-normal science" or the "triple helix" here, other than to say that it is unclear whether the initial authors of these terms were diagnosing a situation or acting as policy advocates for more "socially robust" versions of what they were seeing. Whatever the case may be, what came out of this discussion is important: ID and MD have now become science policy priorities. As a result, a new type of ID has emerged, which we might call top-down or policy-driven ID or MD, now called generically "program research." An early, quite explicit step in this direction came at the initiative of the NSF in 2002 (Brainard 2002; see also National Academy of Sciences 2004; and Moore 2010). Amongst the numerous more recent examples of such policy-driven ID and MD collaborations are the European Union's Horizon 2020 program and the so-called research clusters and graduate schools funded within the framework of the German Excellence Initiative. As oppositional, bottom-up counterparts to this trend one might mention research now being funded directly by NGO's or NGO-affiliated think tanks, although it is clear that the funding for such initiatives is hardly comparable with the millions on offer from established funding agencies.[8]

8. On the phenomenon of "extended expertise," of which research in NGO think-tanks is a part, see Maasen and Weingart 2005.

What is behind this development? The broader political context seems clear: after the fall of communism, which coincided in public policy perception with what historians now call the second globalization, driven this time by the financial industry (in contrast to the "first" globalization after the emergence of global trade and finance capitalism in the late Middle Ages and a world market in manufactured goods during the nineteenth century), the potential of creating a transnational science policy regime seemed manifest, at least to some of the leading actors. The opportunity seems to have been grasped not only to raise the science policy and research game to a new level, but to change the game entirely. A shift has taken place from speaking to political actors for scientists—and therefore, I would add, being forced to cope, perhaps in some desperation, with the vastly increased size and complexity of the research establishment itself—to speaking from politics (or rather: policy) to the scientists, with the openly expressed aim of shifting funding priorities toward perceived policy imperatives. Put very briefly, the desired move appears to be from rewarding research ideas on the basis of peer review alone, regardless of their perceived policy or societal relevance, to the actual government of science. Closely linked with this research funding regime is a push for new higher education management practices (Klein 2010b), leading in some cases (e.g., Arizona State University) to a transformation of institutional arrangements based on MD or even TD organizational units (Crow and Dabars 2015).

The trend initially appeared and still appears to be problem-driven, in the sense that the belief that what came to be called the "grand challenges" such as climate change, environmental protection, migration, and the like, cannot be addressed by single disciplines acting separately, as was proclaimed and continues to be proclaimed by all concerned. Astonishing in retrospect is that this belief, along with the underlying assumption that more and better networked science and scholarship necessarily yields epistemically "better," economically more profitable, AND socially more sustainable and "robust" science, appears to have gone completely unchallenged, although the evidence for it was and remains unclear, to say the least. How this claim became established orthodoxy and why the new gospel went unchallenged remain open questions. (For an example of the conservative impact of ID work in environmental science, see MacLeod and Nagatsu 2018). The results are clear for all to see: a transition from MD and implicitly TD research and funding practices to explicit TD policy talk and new MD and ID initiatives backed by such large amounts of funding that the temptation to create opportunistic "looting parties" appears irresistible.[9] This leads me, finally, to:

9. This term was used in my presence by a high-level German science manager to describe research groups trying to benefit from funding offered by the Excellence Initiative.

Thesis 3: The emergence and increasing predominance of MD and TD program research has altered both ID and disciplinarity alike in ways not foreseen by its advocates.

Where or when to locate, and how to characterize the impact of this radical science policy change and how to describe the new ID or even monodisciplinary practices it has generated is still a matter of intense debate. The same is true of the question of how to describe the motives and practices of scientists and scholars involved. Whatever the answers to these questions may be, the new science policy regime has clearly led to the emergence of an ID, or more often MD, practice not oriented to research, but rather to its evaluation—panel review.

As I suggested above, panel review as a mode of research assessment began much earlier than 1990, but it has become predominant only in the past generation. Michele Lamont has presented a fascinating, if rather anecdotal study of its current dynamics. Relevant here is what she calls the "emergent quality" of assessment standards developed in conversation rather than legislated or agreed in advance (Lamont 2009, p. 211). Though we need to be clear that policy priorities now drive the institution of panel review, not vice versa, in such cases a certain tension can be observed between the perceived need of research funding bureaucracies to establish and enforce uniform, seemingly "objective" standards for the evaluation of funding proposals and what review panels actually do with such guidelines. In any case, the more or less subtle negotiating—not to say, horse trading—tactics involved clearly deserve to be called ID. By the way, Lamont notes that philosophers appear to be the most resistant to cross-disciplinary interaction in review panels, while philosophers' grant proposals indicate high levels of insularity. Her explanation for this phenomenon—the predominance of analytical philosophy within the discipline—appears questionable, since other philosophical communities, such as the phenomenologists, appear to be similarly self-contained and insular. In any case, this example raises the question whether a policy shift designed to force radical change in the direction of ID might actually be reifying disciplinarity instead, along with disciplinary stereotypes in the "two cultures" mode.

4. Concluding Remarks

In conclusion, I return to the question posed at the outset: What benefit may a historical approach provide to general considerations of interdisciplinarity? The answer to this question depends at least in part on whether one is prepared to accept the definition of science and scholarship as institutionalized research practices that I proposed above. For historians and sociologists of science this is no longer an issue. Given the success of the

so-called "practical turn" within philosophy of science, and also the obvious fact that scientific and scholarly research in our time is hardly possible or even conceivable outside institutions, it would seem logical and appropriate for philosophers of science, too, to pay attention to the institutional and science policy contexts that have constrained or even at times directed the activity of scientists and scholars over time, even if they may not wish to focus their work on such issues. As I have tried to suggest here, historicizing both ID and disciplinarity as institutionalized practices provides a perspective on the changing conditions for scientific and scholarly research that cannot be yielded by an approach that ignores the historicity of knowledge claims. In particular, I have suggested that ID itself, let alone MD or TD, cannot sensibly be considered without also considering disciplinarity, and that all of these modes of organizing research have been and continue to be historically dependent on one another.

What remains of the dynamic, shifting disciplinary system that emerged in the nineteenth century? Quite a lot, as it turns out. None of the policy regime changes I have described in this paper has made disciplines obsolete.[10] But disciplinarity is clearly no longer the only game in town, and may not even be the predominant mode of knowledge production and distribution that it may once have been. One reason for the persistence of disciplines is the persistence of the certification imperative alongside the research imperative; the former and not the latter drove public funding for universities, as distinguished from public funding for research, and still does so today. Just because professors, including philosophers, along with many academic policymakers tend to be purblind about this point and to describe the university as though it were only a research institution does not mean that it makes sense to ignore it. Seen from this perspective, many disciplines might best be described today as administrative units held together by the certification imperative—including, but by no means limited to the certification of researchers—rather than as the normal units for the production of knowledge.

The informal three-phase scheme presented above may sound like a linear progression, but it is not intended as such and it is certainly not intended as a tale of scientific progress from disciplinarity to ID, and then to MD and TD. Rather, we have before us a succession of add-ons, which has led to continually increasing complexity in the research system. Peter Weingart argued some time ago that ID, in addition to being an opportunist enterprise related to policy priorities expressed by funding entities, is a necessary and inevitable counterpart to disciplinary specialization,

10. For appropriately sceptical remarks in this direction, see Weingart 2010, pp. 12–13. See also Abbott 2001; Collini 2015; Graff 2015.

which suggests that it is only apparently paradoxical that both grow and prosper together (Weingart 2000). Disciplinary boundary work continues and is often successful enough; disciplines and their alternatives continue to live alongside and in complex interactions with one another. This is likely to go on for some time, in part at least because the principle of inertia is at work in human affairs as well as in physics. This surely also applies to institutionalized research practices, which often continue to be reproduced through scientific and scholarly training long after they cease to inform cutting edge research. In any case, as I pointed out, ID presupposes the existence of disciplines. Whether their relative stability is a necessary or sufficient condition for ID is another question.

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