Academic entrepreneurship and institutional change in historical perspective

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ABSTRACT
This article provides a historical perspective on academic entrepreneurship and its role in institutional change, and serves as an introduction to a special issue devoted to the subject. Unlike approaches that define academic entrepreneurship narrowly as the commercialization of academic research, we argue that historical research and reasoning justify a broader conceptualization focused on the pursuit of future forms of value in academic knowledge production, application, and transmission. Understood in this way, academic entrepreneurship has long been a significant driver of institutional change, not only within the academic world but also in shaping the organization of markets and states. The article develops this argument in three major sections. First, it draws out themes implicit within the historiography of science and technology that highlight the role of entrepreneurship in reshaping academia and its relationship to society. Second, it establishes conceptual foundations for more explicitly examining the processes by which academic entrepreneurship acted as a driver of institutional change. Finally, it synthesizes the findings of the articles in the special issue pertaining to these entrepreneurial processes. The article concludes by arguing for the role of history in rethinking academic entrepreneurship in our own time, and by outlining directions for further research.

Entrepreneurship, we are often reminded, is an intrinsically elusive concept. It is all the more elusive when attempts are made to apply it to seemingly non-commercial fields of life. Over the past 20 years, a growing body of literature has been produced on one such field, namely academia (Rothaermel, Agung, and Jiang 2007; Siegel and Wright 2015a, 2015b). The main focus of this literature on academic entrepreneurship is on the commercialization of research and teaching activities carried out within higher education institutions. It often assumes that academic entrepreneurship is a relatively new development.

Institutions of higher education and research, however, have never been ‘ivory towers’ (Martin 2012; Shapin 2012). Especially from the nineteenth century, academic teachers and researchers have sought out opportunities related to the practical application of academic knowledge for the development of technologies, businesses, and novel social and governmental practices. In pursuing opportunities for the development of teaching and research
facilities, the launching of new disciplinary sectors, and the application of knowledge to other fields of social life, academic entrepreneurs often shaped or reshaped institutions, by defining strategies and mobilizing various resources. In some cases, these initiatives were directed toward important but relatively circumscribed aspects of the social and economic life that defined patterns of interaction between the academic world and the rest of society. In other cases, the efforts of academics as agents of institutional change were from the start of a more far reaching and strategic character. Irrespective of its scale, the consequences of academic entrepreneurship extended well beyond the immediate commercial or practical purposes that are sometimes associated with the goal or outcome of such efforts. In no small way, academic entrepreneurship played a role in shaping the trajectories of knowledge development, the structure of establishments where knowledge was produced and transmitted, and the relation of such structures to modern markets, society, and the state.

In this special issue, we examine the process by which academic entrepreneurship created institutional change in historical perspective, with an emphasis on the scientific and technological areas of academia. In doing so, we draw attention not only to the involvement of individual higher education scientists in entrepreneurial activities, but also to the collective and cumulative dimensions of academic entrepreneurship. Our focus is on the social processes and mechanisms through which academic entrepreneurship reshaped institutions and institutional orders. Historical research and reasoning provides an especially valuable way to examine the relationship between academic entrepreneurship and institutional change because it offers the critical distance and retrospective vantage point from which to consider the long-term consequences of entrepreneurial action, including the sometimes difficult to study processes of long-term institutional change (Wadhwani and Jones 2014).

This introductory article outlines the historiographical background for why the effort to analyze the relationship between academic entrepreneurship and institutional change is important, defines the key terms and conceptual underpinning for the issue, and introduces the main findings from the papers. We first clear the ground for a historical analysis of academic entrepreneurship by contrasting the focus on commercialization characteristic of much recent research with a number of findings emerging from the historiography of science, technology, and education that are also relevant to the theme of academic entrepreneurship. Next, we establish some key conceptual and theoretical foundations that allow us to define the framework in which the special issue has been cast. The subsequent section introduces the individual essays and highlights their common intellectual contributions. We end by discussing the implications of the special issue, how it seeks to make a contribution to the current debate on this theme, and possible paths forward.

**Academic entrepreneurship and history: not just commercialization**

Our approach for studying academic entrepreneurship departs in a very important way from the large body of literature on the rise of entrepreneurship within academia that economists, sociologists, management scholars, and science and innovation policy researchers have produced in the last few decades. Scholars from this diverse group of disciplines have sought to examine and explain the changes that have been taking place in the organization of higher education and research systems since the WWII, notably the development of ‘market’ or ‘entrepreneurial’ universities (Berman 2011; Etzkowitz 2003). They have focused their attention on what they regard as the distinctive feature of that process, namely the direct
involvement of academic organizations and actors in the commercialization of knowledge originating from within those establishments. According to a comprehensive and oft-cited review of this kind of literature, for example, the majority of English language articles on academic entrepreneurship examined either (1) the creation of new firms or (2) the relationships between research universities’ organizational design and their effectiveness in commercializing inventions (Rothaermel, Agung, and Jiang 2007). Over the past decade, academic entrepreneurship has continued to be discussed primarily in relation to university–industry technology transfer (see the literature survey of Siegel and Wright [2015a]). Moreover, even scholars who argue that a ‘rethink’ of academic entrepreneurship is necessary do not fundamentally question the emphasis on commercialization. Rather, they suggest looking beyond formal academic technology transfer offices and science parks and their involvement in patenting, licensing, and the creation of spin-off firms, and paying more attention to other forms of commercialization and the broader range of actors involved (Siegel and Wright 2015b).

A second feature of this set of works on academic entrepreneurship that stands in contrast to the approach of this special issue concerns the periods of time under consideration. Most of these studies do not go further back than the 1970s–1980s. There are, to be sure, some notable exceptions (Martin 2012). With regard to the European system of higher education, for instance, Geuna and Muscio (2009, 94) have pointed to organic chemistry as a field where academic scientists and business firms closely collaborated in the nineteenth century. Other scholars mention a few well-documented examples of scientists (for instance, the chemist Justus Liebig in Germany and the physicist William Thomson [Lord Kelvin] in Britain) who were involved in the creation of new business enterprises in the course of their academic employment (e.g. Etzkowitz 1983; Shane 2004). As for the other side of the Atlantic, there seems to be a growing awareness that entrepreneurial responses to market opportunities and pressures have long been a feature of the American higher education system (Etzkowitz 2003; Greenberg 2007, 83; Rosenberg and Nelson 1994; Washburn 2008, chapter 2). In particular, nineteenth-century Morrill Act land-grant colleges and universities, which initiated programs in agricultural science and other practically oriented fields, are frequently brought up as precedents to present-day universities with a mission to contribute to regional economic development. It is also acknowledged that U.S. universities’ involvement in patent management activities predated the Bayh–Dole Act of 1980 by several decades (Mowery and Sampat 2001; Mowery et al. 2004). Still, consistent with Etzkowitz’s (1983) claim that ‘the traditional ethos of science did not permit [erosion of …] the boundary between science and private, profit-seeking-business’, the literature on academic entrepreneurship produced by these branches of scholarship continues, on the whole, to characterize such earlier cases as exceptions. The widespread assumption is that prior to the 1970s, academic scientists pursued work that ‘interested them intellectually regardless of whether they had clear economic potential’ (Berman 2011, 35). More often than not, earlier historical developments and events seem to be brought up to highlight what are supposed to be fundamental differences with the contemporary transformation of academia. The use of history in these studies, moreover, can be quite Whiggish and sometimes inspires a simplistic vision of ‘entrepreneurial activity as a step in the natural evolution of a university system that emphasizes economic development in addition to the more traditional mandates of education and research’ (Rothaermel, Agung, and Jiang 2007, 705).
To challenge this narrow focus on commercialization, as well as this use of history, this special issue draws on works of historians of science, technology, and education that throw light on the relationships between academic entrepreneurship and institutional change since the nineteenth century. It should be pointed out, however, that, with a few notable exceptions (e.g. Carlson 1988; Keith 1984), these historians have used the term ‘entrepreneurship’ – as well as the notion of ‘institutions’ – in a loose and sometimes metaphorical way rather than as clearly defined analytical concepts. In most cases, they have also not taken into account the ongoing debate on the meaning and significance of these concepts. Notwithstanding these important limitations, there are six themes that emerge from the historical literature that are especially relevant to this special issue and that, therefore, will be addressed in more detail in the following paragraphs. They concern (1) the integration of research into academia, including the creation of science laboratories and institutes; (2) entrepreneurial processes associated with the development and promotion of the scientific and technological disciplines; (3) attempts to reform teaching and educational curricula that involved processes of boundary crossing between academia and other spheres of social life; (4) academics’ involvement in institutional innovation outside the higher education systems; (5) the notion that highly centralized higher education and research systems inhibited entrepreneurialism; and (6) the role of key concepts, and language more generally, in initiating and legitimizing major institutional changes. The remainder of this section will consider each of these themes in some detail.

**Integrating research into academia**

First of all, historians of science, technology, and education have brought to light aspects of entrepreneurial behavior in the activity of academics engaged in efforts to change the work settings in which they were operating. Those aspects emerge especially in studies of the transformations of individual academic establishments, as well as of higher education systems, from the mid-nineteenth century onward. And here there is already a significant departure from the interpretive perspective outlined above. Rather than identifying the integration of scientific and technological research into higher education establishments as an antecedent to the emergence of entrepreneurial universities, the works of these historians suggest that this development was in itself shaped by entrepreneurial processes of opportunity identification, resource allocation, and legitimization.

Key features of the research university, such as the research seminar and the research-based doctoral dissertation, first emerged in the German kingdoms of Prussia and Hannover in the decades around 1800 (Clark 2008). Support for research in higher education institutions was initially targeted to areas of study such as pedagogy and classical philology. The natural sciences were by no means at the forefront of this innovation. However, the seminars, in a way that was not foreseen by the promoters of that innovation, helped pave the way for the establishment of laboratories as the pedagogical and research equivalent for the experimental sciences. In the new discipline of physics, for example, early academic seminars were forerunners of the fully fledged laboratory-based institutes created in the second half of the nineteenth century (Cahan 1985; Olesko 1991). The institutionalization of chemistry laboratories had already occurred at a significantly earlier date: by 1830 they were commonly used to train students in chemical analysis (Homburg 1999). However, the transformation of these early teaching laboratories into larger laboratories dedicated to not only teaching
but also research purposes was by no means a natural or inevitable process. Instead, historians of science have shown how chemists such as Justus Liebig helped initiate this change by exploiting scientific, political, and economic opportunities (Holmes 1989; Morrell 1972).

Studies of the development of research and teaching laboratories in the German universities and institutes of technology show the extent to which academic scientists individually and collectively became involved in the creation of new scientific infrastructure (Cahan 1985; Jackson 2011). The rise of the civic universities in nineteenth-century Britain, with their strong commitment to the development and transmission of scientific and technological knowledge, has been analyzed in similar terms (Kargon 1977; Sanderson 1972). In France, the early modern tradition of concentrating research in non-university-based structures prevailed well into the nineteenth century. Later the university system was by-passed in favor of ad hoc institutions for the promotion of research such as the École pratique des hautes études and the Institut Pasteur (Fantini and Morange 1991; Fox 2012; Paul 1985). And in the United States, the relatively small number of research universities that had emerged by WWI had to further legitimize their commitment to creating new knowledge in the interwar period, while struggling to place it on a firm financial foundation (Geiger 1986). The process of integrating laboratories and related institutions of scientific and technological research into higher education establishments is often described in terms that imply entrepreneurial qualities and processes at work. These include references to an alertness to internal and external opportunities, organizational skills, strategic decision-making, leadership, and the promotion of links and collaborations within academia as well as with actors in political and socioeconomic fields.

The integration of research into academia also depended on the ability to raise funds to cover the costs of the buildings, equipment, and manpower necessary for conducting experimental scientific investigations. These expenses seemed to increase constantly from the second half of the nineteenth century onward. Their rise mirrored not only a dependence on ever-more costly equipment but also the expansion of the scientific enterprise in scale, scope, and complexity, and precisely for those reasons required considerable organizational skills (Capshew and Rader 1992; Fox and Guagnini 1999).

In order to mobilize such resources, academic scientists and science administrators often exploited, as well as promoted, the growing demand for scientific and technological knowledge and expertise from industrial firms and modernizing nation-states. For instance, they established patronage relationships with knowledge-intensive firms who depended on a regular supply of science graduates and/or the services of scientific consultants (Boudia 2001; König 1996; Marsch 2000). They also endeavored to legitimize requests for expensive research facilities by skillfully drawing attention to and exploiting potential medical and military uses of exceedingly expensive laboratory facilities (Heilbron and Seidel 1989). In other cases, they did so by highlighting the economic and national security value of their fields of study (Bonneuil and Pestre 2015; Kohlrausch and Trischler 2014).

**Building research schools and disciplines**

The historical literature on the integration of research into academia tends to focus on academic scientists’ institution-building efforts, particularly on the way in which economic, commercial, and social links were harnessed to the creation of new institutional settings. Historians of science and technology have, however, not only referred to entrepreneurial
qualities and behaviors in analyses of the organization of the infrastructure of research and education establishments. Similar characteristics are also apparent in publications that examine the process by which new academic knowledge was developed and managed. These latter studies are a reminder that institutional changes affect the epistemic and cognitive aspects of academic activity as much as the organizational ones. We refer here to the process by which academic scientists sought to launch new research programs and schools, forge highly reputed science centers, and establish new disciplinary and interdisciplinary areas in an ever-more specialized and competitive academic landscape.

As historians of science and technology have documented, the creation of research programs and centers entailed the identification of new directions of inquiry, including problems that could only be solved by departing from established routines and experimental methods (Geison and Holmes 1993; Morrell 1972, 1993; Pinault 2000; Servos 1993). In launching such scientific enterprises, the academics involved faced a good deal of risk as both their own reputations and the career prospects of their collaborators were at stake. The success of these initiatives depended on the presence and exploitation of favorable circumstances in particular institutional environments. It involved the capacity of selecting and coordinating collaborators with different scientific and technological skills and competences; the ability to motivate and sustain the combined effort of the members of research teams; and the creation of not only physical but also disciplinary spaces within or outside of existing institutional settings. In addition, it required the capacity to secure recognition for new research agendas, and to establish prominent (and whenever possible hegemonic) positions in a competitive environment. This meant creating effective channels for the diffusion of research results, and career opportunities for students and collaborators. Through the combination of such actions and qualities academic fields were being transformed. The physics department of the University of Bristol, for example, rose from relative obscurity to become an internationally acclaimed research center in the interwar period thanks to what Keith (1984) explicitly characterizes as the entrepreneurial drive of the departmental head.

Similar skills and abilities are highlighted with respect to the closely related, although not necessarily overlapping, attempt to launch and promote new disciplinary areas (or subareas). In this case as well the work of historians of science and technology suggests that academic scientists had to engage in entrepreneurial activities (Geison 1978; Lenoir 1988). These include the securing of academic recognition and legitimization within particular institutions and the scientific community more generally; the controlling of tensions and conflicts that arose within established academic structures due to the emergence of new disciplines; and the creation and management of institutional frameworks meant to enable and sustain the growth of such disciplines, as well as of publication outlets and other channels through which public visibility and scientific recognition could be obtained.

Finally, entrepreneurial qualities are also described in analyses of the creation of research programs and centers that strategically transcended disciplinary boundaries (Mody and Choi 2013). In the interwar United States, as an example, the scientists George E. Hale, Robert A. Millikan, and Arthur A. Noyes conceived and organized a characteristically interdisciplinary enterprise that was pacesetting with regard to both its contents and its institutional embedment in the California Institute of Technology and its surrounding area (Kargon 1982).
Reforms of teaching and educational curricula

Although historical research invoking entrepreneurial qualities is most commonly found in publications focused on changes in the institutional framework for research, such qualities are also evident in analyses of historical processes of conceiving and implementing new educational programs. The creation of the ‘red brick universities’ in the late nineteenth-century United Kingdom provides a notable example. The educational patterns of these new institutes, which entirely depended on private patronage in a cultural environment that could be quite hostile to academic learning, were shaped by the interests and expectations of their local communities. This however did not mean that academic scientists’ initiative was limited to the adaptation to existing needs; on the contrary, they proactively tried to shape their environment through a process that entailed risky decisions and strategies. This situation is exemplified by Kargon (1977) in his analysis of the University of Manchester in the third quarter of the nineteenth century. Here, academics endeavored to convince possible potential local patrons of the benefits of an advanced education with high academic standards and a strong emphasis on the sciences. More specifically, they claimed that, in economic terms as well as in terms of social status, such a form of instruction would prove more valuable than a strictly functional technical training.

In the United States, reforms of teaching curricula likewise involved academics’ engagement in the identification of new opportunities for educational programs, the securing of physical spaces, and the legitimization of these programs within and outside the schools in question. Historians of science and technology have shown that this occurred at a broad range of establishments and had started well before the creation of America’s first research universities in the late nineteenth and early twentieth centuries. Importantly, these reforms were not just initiated at land-grant colleges that had, in the decades following the Morrill Act of 1862, succeeded in obtaining government funds to offer training in applied scientific and agricultural subject areas (Geiger 1998; Rosenberg and Steinmueller 2013). New educational arrangements were also launched in industrializing cities where institutes of higher learning were rapidly developing, and where decisions about how to organize educational arrangements took into account the new demands and expectations created by the socioeconomic developments affecting local communities. In fact, according to Kargon and Knowles (2002), institutions such as the Case School of Applied Science in Cleveland and the Armour Institute of Technology in Chicago innovatively adjusted their curricula in the last two decades of the nineteenth century to meet the needs of local knowledge-intensive industries.

When it came to the creation of new institutional settings for engineering education at universities such as Stanford and the Massachusetts Institute of Technology (MIT), similar initiatives have also been examined in this light (Lécuyer 2006; Leslie and Kargon 1996; Servos 1980). One study on MIT explicitly uses the notion of entrepreneurship to analyze how the introduction of a cooperative engineering course depended on a matching of supply and demand and to highlight the risk-taking involved in this type of institutional innovation (Carlson 1988). In general, though, the entrepreneurship concept has not been central to the arguments of historians examining the reform of teaching and educational curricula.

Institutional innovation outside the higher education systems

A notable feature of the history of the sciences since the late nineteenth century concerns the multiplication of the sites of knowledge production (Bonneuil and Pestre 2015;
In the decades around 1900, for example, laboratory research not only became more common in higher education institutions, as discussed in the previous paragraphs; research laboratories were also introduced in industry and in government agencies. Moreover, in the same period academic scientists got involved in the creation of research infrastructure outside of the university system, often in collaboration with partners in industry and government. This included the founding of national research councils, hybrid science establishments jointly funded by industry and the state, and institutes devoted to research problems considered of high national importance. In many cases, these initiatives were launched with a strong practical orientation. Historians of science have examined the process by which they were conceived and developed and have identified the complex range of interests involved, including those of the academic scientists, the local and central governments, industry, and the military.

These studies highlight the role of academics as active participants in the creation and development of these new institutions. They show that members of the scientific community often reacted to what they perceived as limitations resulting from the rigidity of the higher education establishments where they were employed, including when these were relatively autonomous private schools. In fact, as early as the end of the nineteenth century, a frequent complaint was that a rapid growth in student enrollments negatively affected scientists’ research performance (Fox and Guagnini 1999). In this literature, too, academics’ institution-building activities are rarely analyzed in light of an explicitly defined notion of entrepreneurship, although the qualities and characteristics described in some of those studies would have lent themselves to such an interpretation. For instance, we find entrepreneurial processes implicitly at work in studies of the 1887 origin and development of the Imperial Institute of Physics and Technology in Germany (Cahan 1989), and of the Research Institute for Experimental Therapy and the Royal Prussian Institute for Experimental Therapy, set up, respectively, in 1896 and in 1899 (Lenoir 1988, 1997). With respect to the United States, the creation of a National Research Council during the WWI owed much to the vision, as well as opportunism, of the astronomer George E. Hale, who famously regarded the war as ‘the greatest chance we ever had to advance research’ (Kevles 1968). In the following decades, other scientific institution-builders followed in the footsteps of Hale by developing initiatives to establish a national research fund (Kargon and Hodes 1985). To this purpose they initially turned to industry and the philanthropic foundations, thus continuing the prevailing approach within the U.S. scientific establishment of the time. From the 1930s, and especially after the outbreak of the WWII, however, the federal government was increasingly considered an attractive patron.

**Entrepreneurship in centralized higher education and research systems**

Another important theme in the historical literature that is also relevant to contemporary discussions about academic entrepreneurship concerns the relationship between entrepreneurialism and levels of state control. It is often argued that national systems of higher education and research characterized by a strongly centralized control of resources were relatively slow to innovate in the scientific and technological fields and to respond to the changing needs of their environments. Based on such considerations, for example, Carlsson et al. (2009) and Rosenberg (2003) have described the American system as more dynamic and conducive to academic entrepreneurship than its Continental European counterparts.
It is certainly true that some new disciplines (for example, chemical engineering, genetics, and molecular biology) in general were more rapidly introduced in U.S. higher education establishments than in European schools belonging to more centralized academic systems (Harwood 1987; Strasser 2002). It would, however, be wrong to infer from this that academic entrepreneurship could not produce significant results in European countries where centralized systems of higher education and research were in place. This is not to deny that high levels of centralization imposed constraints on the possibility of bringing about institutional change. However, historical analyses have shown how the cohesiveness of those systems was fraught by internal tensions and conflicts, especially in particular historical periods; and how even in highly centralized systems local interests played a role in opening windows of opportunity for change. In their analyses of the complexity of these systems, historians have drawn attention to the way in which members of the academic communities, individually and/or collectively, endeavored to promote institutional changes by identifying those windows of opportunity, especially at the local level. They did this by establishing alliances with potential partners outside the academic sector, and in some cases by adopting strategies that sought to exploit the peculiarities of these systems. What is worth considering, in an historical perspective, is not only to what extent academic entrepreneurship was effective in promoting institutional change depending on the nature of the systems, but also the forms in which this process was carried out.

The French system of higher education, which is commonly regarded as the epitome of centralization, can serve to illustrate the point. The creation in Paris of the École supérieure de physique et de chimie in 1882 (Shinn 1981) and of the École supérieure d’électricité in 1894 (Ramunni 1995) are good examples of institutional changes outside the sphere of the national system of higher education. Away from the capital, the instituts were launched in the 1890s as semi-independent units of several provincial science faculties (Grelon 1989). As historical inquiries indicate, all these initiatives were the result of a close collaboration and partnership between academic scientists, industrialists and local authorities. In fact, the ground for the creation of the instituts was paved by the relative autonomy that was given to the science faculties/universities in the last decade of the nineteenth century (Nye 1986). The French system was far from monolithic, and this new context allowed these academic institutes to diversify their educational offerings and their research capabilities, as in Grenoble, for instance (Caron 2000; Guthleben 2016; Pestre 1990). Local institutional initiatives then became the basis for the twentieth-century development of scientific clusters (Chapoulie, Fridenson, and Prost 2010; Grossetti 1995; Grossetti 2016). If anything, this literature suggests that the flurry of academic entrepreneurship around the turn of the twentieth century was to some extent supported from within the centralized system, rather than created in opposition to it. In this way even members of the professoriate of public institutes, who had the status of civil servants, could overcome the rigidities of the system without challenging long-established and deeply rooted traditional settings. The result was a process of growth by layering and conglomeration, where the centralized system in fact ended up integrating and absorbing new establishments originating outside the boundaries of its structure.

Language and institutional change

Finally, the writings of historians of science and technology point to the importance of language in initiating and legitimizing institutional innovations of the kinds described above.
Drawing on works in conceptual history, literary studies, and/or the sociology of science, scholars like Bud (2014), Godin (2006), Kline (1995), and Schauz (2014) have examined the emergence and significance of keywords such as ‘scientist,’ ‘pure/applied science,’ ‘basic research,’ and ‘innovation.’ Their studies indicate that these types of concepts helped bring about institutional changes within academia by defining or contesting the relationships between different disciplines, pedagogical approaches, and areas of investigation. In addition, they show that these keywords were used to negotiate and reconfigure the boundaries between academia and other societal spheres, including industry and the state.

In fact, the scientists who strategically deployed these vocabularies often targeted both academic and broader audiences. In mid-nineteenth-century Britain, for example, the notion of ‘applied science’ helped unite a diverse coalition of educational reformers in defining and justifying the curriculum of a new generation of colleges (Bud 2014). Likewise, in the United States, physicist Henry Rowland’s (1883) passionate plea for ‘pure science’ was meant to attract support for laboratory-based graduate training among not only academic teachers and students but also philanthropists (e.g. Dennis 1987). According to Lucier (2009), moreover, Rowland’s speech was part of a wider attempt to distinguish truth-seeking ‘scientists,’ a term that was still relatively new at the time, from ‘professionals’ selling their services on a for-profit basis.

Arguably, the role of language became even more consequential in the twentieth century, due to the challenges and opportunities presented by far-reaching war efforts and the availability of unprecedentedly high levels of public and private funding. In this context, the nineteenth-century emphasis on ‘purity’ of motive – that is, the pursuit of knowledge for its own sake – was either reinterpreted or replaced by new keywords that seemed better suited for highlighting the military, economic, and social relevance of the sciences (e.g. Clarke 2010; Kline 1995; Schauz 2014). At the same time, the desire to limit the amount of government interference in academia contributed to seminal formulations of values and norms that were claimed to be characteristic of the scientific enterprise (e.g. Hollinger 1996).

Taken together, these studies thus point to scientists’ innovative uses of language to protect and advance their interests, address societal problems, and make sense of their work. This literature also demonstrates that they did this by pursuing intra- and extra-academic opportunities, attracting resources, and challenging or legitimizing institutional configurations. As we argue in the next sections of this introduction, these are all processes that are fundamental for understanding academic entrepreneurship. Still, while there are a few theoretically grounded reflections on the significance of institutions (e.g. Björck 2016), the notion of entrepreneurship is almost completely absent from these writings.

Conceptual foundations

As we pointed out in the previous section, entrepreneurial themes have played an implicit role in historical research on the behavior of academics – individually and collectively – as agents of institutional change. However, historians of science and technology have discussed these themes and processes without focusing on the development of concepts or connecting frameworks that could assist in providing a more analytical approach to studying the relationship between academic entrepreneurship and institutional change. As a result, the research conducted by these historians has not focused on central intellectual questions or problems concerning academic entrepreneurship and has remained marginal to the scholarship on the topic.
To overcome these limitations, the authors and editors for this special issue sought to ensure that the papers were not only thematically related but also drew on common key constructs and explored a set of relationships between these. The aim was to create a connecting interpretive framework pertaining to the relationship between academic entrepreneurship and institutional change, and in this way to allow comparison between studies, enhance the analytical depth of particular papers, and cultivate conversation among scholars, particularly as it relates to entrepreneurial processes. Establishing key terms and concepts is especially important in the case of this special issue because the study of academic entrepreneurship has been inherently cross-disciplinary, engaging science and technology scholars, economists, sociologists and management scholars, as well as historians. Here, we clarify what we mean by three central concepts – entrepreneurship, institutions, and academic entrepreneurship – and we discuss possible relationships between them.

**Entrepreneurship and entrepreneurial history**

While studies of entrepreneurship have become common across the social sciences, the focus of this research has varied widely. Entrepreneurship researchers have defined their field based variously on the characteristics of entrepreneurial individuals (Chell 2008), the creation of new organizations (Gartner 1990), and the pursuit of new opportunities (Shane and Venkataraman 2000; Stevenson and Jarillo 1990). This special issue builds on the understanding that entrepreneurship involves the pursuit of opportunities to create future forms of value.

In commercial entrepreneurship, value is typically defined by the creation of new products, services or transactions that customers are willing to purchase and that render a profit for the entrepreneur. But value need not be defined in commercial terms; indeed, historical thought has often emphasized that actors may be motivated by many different kinds of ends (Schumpeter 1954; Wadhwani and Lubinski, forthcoming). Hence, future value may be understood variously in terms of civic good, technical efficiency, scientific advancement, or any other end that an entrepreneur may deem worthy.

Entrepreneurial history (Casson and Casson, 2013; Wadhwani 2010) focuses specifically on the nexus between such entrepreneurial actions and processes of historical change (Wadhwani and Lubinski, forthcoming). The basic premise of entrepreneurial history is that in pursuing future opportunities, enterprising agents implicitly or explicitly are involved in changing the social, political, and economic order of their present, and are hence inherently engines of historical change. The premise underlies the Schumpeterian claim (Schumpeter 1942) that capitalism is not a stable but rather a constantly evolving system of economic practices and relationships. This assumption distinguishes entrepreneurial history from both classical and neoclassical economic approaches to entrepreneurship, which presume relatively stable, equilibrating markets, as well as materialist historical accounts (such as Marxism), which focus on materialist conflicts of interest as the foundations for the dialectics of change (Wadhwani 2010).

This special issue pays particular attention to three entrepreneurial processes and their relationship to historical change (Wadhwani and Lubinski, forthcoming). The first is the process by which actors, individually and collectively, imagine and articulate opportunities related to future forms of value. The second is the process by which entrepreneurial actors assemble resources to pursue these opportunities. Because entrepreneurship involves action
that seeks to render the imagined future a reality, entrepreneurs inherently engage others to allocate resources to pursue the opportunity and to organize resources in a coherent way. The third is the process by which entrepreneurs legitimize their project vis-à-vis the present social, cultural, and political order. Because entrepreneurial history is particularly focused on processes of change, it pays particular attention to when and how entrepreneurs change economic, social, and cultural institutions to pursue their projects.

Institutions and institutional change

Institutions can be defined as the ‘rules, norms and ideologies’ that govern behavior. Scott (1995, 33) characterizes institutions as ‘social structures that have achieved a high degree of resilience’ and categories them based on their ‘cultural-cognitive, normative and regulative elements’. Institutions are value laden in that they indicate who should engage in a particular activity, how the activity should be performed, and why behavior should be ordered in that particular way. Over time, such rules, norms, and ideologies can come to be ‘taken for granted’ by the people involved, and constitute what they see as social reality (Meyer and Rowan 1977).

Institutions can be thought to govern behavior at different levels of social order. For the purposes of the special issue, it is heuristically useful to think of institutions governing behavior within academic fields as well as between the academic world and industry or government. Institutions within academic fields might include the rules and principles along which a research initiative is organized or the boundary separating one discipline from another. Institutions governing the relationship with other fields might include the form of non-governmental organizations engaged in supporting academic research or the rules governing the relationship between industries and universities. Institutions are important because they shape the flow of resources and define legitimate and illegitimate behavior, hence governing orderly behavior between people.

While institutional theory has been employed throughout the social sciences, it is worth noting disciplinary differences in the focus of research. Economic new institutionalists tend to focus on whether institutions create optimal economic outcomes by facilitating efficient transactions (North 1990; Williamson 1981). Sociological new institutionalists consider how institutions create social pressures to conform (DiMaggio and Powell 1983). Lastly, historical institutionalism considers the relationship between multiple institutions (often in institutional configurations) and how these evolve over time (Steinmo 2008; Thelen and Steinmo 1992). Given our interest in institutional change, we adopt a historical institutionalist approach.

Given that institutions are presumed to explain social order, one of the persistent questions that arises is how to explain change. Indeed, as the previous section highlighted, the historiography indicates that the academic world has often undergone significant changes. The emergence of new domains of research and activities required reordering the boundaries and relationships between disciplines. The organization of research councils and research centers required the creation of new institutions governing cooperation between fields. And the relationship between universities and states has been constantly evolving. How then to explain institutional change?

While there are many different scholarly approaches to the subject, our focus is on the relationship between academic entrepreneurship and institutional change. Entrepreneurship has long been recognized as a force driving institutional change (Schumpeter 1947).
Entrepreneurial history helps us examine the process of institutional change not only by bringing specific actors into the narrative, but by considering how new institutions were imaged, resourced, and legitimated (Wadhwani and Lubinski, forthcoming).

**Academic entrepreneurship and institutional change**

How does academic entrepreneurship relate to institutional change? To consider the central intellectual question at the heart of the special issue, we extend the concepts above a little further by considering the specific nature of academic entrepreneurship and how it might reshape the institutional contexts in which it occurs.

Academic entrepreneurship can be understood as a particular form of entrepreneurship that is specific to academic fields. For the purposes of this special issue, it is most useful to regard an academic world or field as a set of organizations and actors that recognize one another as devoted to valuing scientific and humanistic knowledge production and dissemination as central to their *raison d’être*. In other words, actors and organizations in academic fields share a common understanding that they value the production and dissemination of knowledge as crucial to their identities. Organizations such as universities and research institutions are characterized by the production and dissemination of scholarly knowledge as a primary end or purpose of organized activity. Such a purpose does not preclude those organizations from pursuing other ends, such as profit or social impact, but academic actors, institutions, and organizations are only legitimately considered academic if they pursue knowledge creation, application, transmission, or dissemination as a central purpose of their activity. In this sense, the notion of ‘the academic field’ can be understood to itself be comprised of a set of nested subfields of the sciences, social sciences, and humanities. It can also be understood in relationship to proximate fields to which academic knowledge is applied, such as public/state fields and private/industrial fields (Fligstein and McAdam 2012).

Academic fields cannot and should not be understood in static or functional terms. Indeed, the academic world has undergone dramatic changes over the past few centuries, as the historiography above highlights. Accordingly, it is best to think of academic settings as contextually specific and constantly evolving. The expectation that academic actors should contribute new and original scholarly knowledge, for example, gained a strong foothold in academia as a result of the ‘research revolution’ of the nineteenth and early twentieth centuries. And the boundaries of what constituted ‘academic knowledge’ has always been contested and in flux. Indeed, it is the inherently historical character and boundaries of academic fields – the dynamics shaping and reshaping them over time – that are the focus of the papers in this issue.

Following on this, we define academic entrepreneurship as the pursuit of future forms of value pertaining to academic knowledge production, application, and transmission. In practice, this may take different forms, aimed either at the internal development and transmission of disciplinary knowledge within academic fields and subfields or at the application of academic knowledge to non-academic fields, such as industry, commerce or the state. One can thus consider academic entrepreneurship opportunities as those pertaining to efforts related to the development of a discipline or area of knowledge, or the application to other fields, such as industry through commercialization or the state through policy. In each of these cases, academic entrepreneurship involves resource acquisition and legitimization processes, and often involves efforts that cross the boundaries of different fields and that seek different kinds of future value. Our definition of academic entrepreneurship is hence different from
the one based more narrowly on the commercialization of academic knowledge; we instead treat commercialization as only one form of academic entrepreneurship.

The description above should make it clear that while academic entrepreneurship involves knowledge creation, application or transmission it is not synonymous with intellectual activity alone. Rather, academic entrepreneurship involves the resource acquisition and legitimacy seeking activities necessary to render such opportunities as realities in practice. For instance, as we have already pointed out, the development of an emerging discipline might involve the identification of new and original lines of research. This could include departing from routine programs, the creation and management of complex and often numerous research teams, winning the resources necessary to develop research in the discipline and building the legitimacy necessary for it to be taken seriously in the academic world. For the development and application of academic knowledge for industry/commercial or policy/public purposes, it requires not only the activity of imaging such applications but also the processes of gathering resources for it and establishing its legitimacy in both the academic and non-academic fields.

It is for this reason that we can consider the process of engaging in academic entrepreneurship as an essential aspect of institutional change. It involves not only processes of change within the academic world, as new disciplines and fields strive for resources and the establishment of academic legitimacy. It also involves the establishment and institutionalization of new ways of organizing the interface between the academic world and other social fields in order to apply academic knowledge beyond the academic world. Academic entrepreneurship is hence inherently involved in institutional change because it was fundamental the establishment of new institutions through which ideas, resources, and legitimacy flowed in a routinized and orderly way. It is the process by which new institutions are established by academic entrepreneurs that is the focus of this special issue.

**Research process and findings**

This special issue is the result of an effort to bring together scholars from different sub-disciplines – history of science and technology, economic and business history, and management and organization studies – to examine academic entrepreneurship historically. The dialog was established in the course of sessions organized at two conferences (Society of the History of Technology annual meeting, Dearborn 2014; European Business History Association meeting, Utrecht 2014), and especially during a conference on ‘Academic entrepreneurship in history’ held in 2015 in Ghent and a follow-up workshop in Lille in the next year. What we offer here is a selection of five of the papers that were presented and discussed at those meetings, and in particular those dealing more specifically with the relationship between academic entrepreneurship and institutional change. (For another set of articles, concerned with the commercialization of academic science, see Mercelis, Galvez Behar, and Guagnini 2017). The contributions of those papers are summarized in Table 1.

In the first article, Ellan Spero examines the design of a new institution – the industrial fellowship program – in early twentieth-century America. Using micro-historical research methods, she analyzes a set of correspondence between chemistry professor Robert Kennedy Duncan and industrialist E. Ray Speare in 1906 as they discover their common interest in the application of the rapidly expanding field of chemistry to industrial processes. Spero tracks
in detail the creative, and at times playful, processes by which the two figures consider the possibility for the application of chemistry research to industrial laundries, discuss how a graduate fellowship program funded by industry and focused on applied knowledge could serve the purpose, and imagine how the program would work. Speare, and especially Duncan, it becomes clear, are aware of the broader institution building process in which they are engaged, as they conceive the fellowship program as a model to be replicated.

In the second article, Thomas Brandt shifts our focus from the micro-level processes at work between individuals engaged in academic entrepreneurship to the macro-level institution building processes unfolding over half a century at the level of the nation state. Brandt examines a series of efforts to establish national institutions to support and coordinate scientific endeavors in Norway between the late nineteenth century and the 1950s. Specifically, he shows how early twentieth-century efforts to establish a privately funded science academy were linked to the subsequent interwar push to create a central research institute, and the post-WWII creation of a national research council. Brandt demonstrates that these experiments in institution building were profoundly shaped by the international flow of ideas and models for the organization of science and motivated by a rationale of nation building. He also shows how experiences with one institution building effort shaped subsequent movements, and considers the role of memory and history in the process of long-term institution development.

In the third paper, Gabriel Galvez-Behar’s treatment of efforts to shape the scientific institutions in early twentieth-century France in many ways echoes the article by Brandt. Galvez-Behar focuses on the formation of national level institutions over multiple decades and demonstrates the cumulative processes at work as successive generations of academic entrepreneurial groups attempted to establish the central institution through which scientific efforts received resources and were coordinated. But Galvez-Behar also focuses on the

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contestation between competing groups of academic entrepreneurs to shape the institutionalization of academic science. He highlights both the skill and position of these groups vis-à-vis industry and the state as they tried to influence the design of the national institution, and incorporates the impact of events (e.g. the WWI) and contexts in shaping which ideas and entrepreneurial groups managed to exercise influence.

In the fourth paper, Giovanni Favero shifts attention back from national-level institutions shaping science in general to the dynamics related to the emergence and legitimization of a specific new field of academic knowledge at the boundary between scientific and social disciplines: statistics. Focusing on the figure of Corrado Gini as an academic entrepreneur, Favero uses a biographical lens to examine the processes by which Gini sought to legitimize and institutionalize statistics in the context of the rise and fall of the fascist regime in Italy. Favero focuses in particular on how Gini's efforts to legitimize the new field involved tying it to the applied policies and knowledge production practices of the state. He demonstrates the process of reciprocal legitimization at work between the emerging academic field and the political regime's efforts to establish its authority.

Finally, in the fifth paper, Cyrus Mody pushes us into the late twentieth-century United States to examine the formation of institutions to facilitate collaborative research in micro-electronics. Specifically, Mody examines the introduction of both industrial consortia and university centers as institutional responses by U.S. micro-electronics firms and academic organizations to sharing the costs of basic research needed to remain competitive against increasingly aggressive Japanese firms in the 1980s and 1990s. Mody demonstrates the evolution of these institutional forms through a series of cases, and concludes by showing how university centers, as an institutional model, proved more adaptable in the long run as the needs of industries evolved.

Each of the five papers makes distinct and original contributions to historical perspectives on academic entrepreneurship and their relationship to historical change. But together, they also point to six major or common contributions that challenge preconceived conceptions of the character of academic entrepreneurship and its relationship to processes of change in markets, societies, and states. In the remainder of this section, we draw out these major thematic findings and how the individual papers contribute to them.

The evolving character of entrepreneurial opportunities

The papers highlight why a focus on commercialization alone (Shane 2004) provides at best a very limited perspective on academic entrepreneurship and its role in economic change. As the historiographical section showed, the boundaries between academic and other fields of social life have long served as a particularly generative space for entrepreneurial imagination and ambition. The articles in the special issue explore the evolving character of entrepreneurial opportunities that arose at the intersection of science and society from the late nineteenth century until the late twentieth century, and the processes by which entrepreneurial actors envisioned and pursued them. The changing character of academic entrepreneurship opportunities over the twentieth century was shaped by, among other factors, the emergence of new academic subfields (Favero), the changing needs of industries and states (Spero, Galvez-Behar, Brandt, Mody, Favero), and the sheer scale and cross-sectoral complexity of these ambitions (Mody, Galvez-Behar, Brandt).
Imaginative processes in academic entrepreneurship

The special issue also highlights the role of imaginative processes in academic entrepreneurship. The identification and articulation of entrepreneurial opportunities at the intersection of science and society involved creative processes of bridging the constraints and arrangement of the present with newly imagined futures (Beckert 2016). Opportunities were not simply discovered through the synthesis of available information, they were produced by the imaginative process in which entrepreneurial actors engaged. As Schumpeter (1947) had posited, historical research allows authors to examine these creative processes at work, both at the micro-level through the character of interactions and dialog between entrepreneurial individuals (Spero) and at a more macro-level as collective visions of science in the service of industry and the nation shaped institution building efforts over time (Brandt, Galvez-Behar).

The contested character of new institution building

Much of the literature that has applied institutional theory to systems of entrepreneurship and innovation in academia has posited that institutions create stable patterns in the interactions between universities, industries, and governments, allowing for the categorization of ‘typologies’ or ‘national systems’ of innovation (Nelson 1993). But the historical cases examined here suggest a very different picture: contestation and contingent compromise rather than stability, system, and order were the characteristics that defined institution development historically. Rather than marked by the emergence of clearly ordered and stable rules of interaction around innovation, the building of new institutions was marked by a multiplicity of competing, and occasionally cooperating, efforts (Galvez-Behar, Brandt, Mody). Overall, the picture that emerges challenges the contention that orderly innovation systems are inherent to particular countries or groups of countries, and instead suggests the way in which ongoing competition and compromise between rival institutions was in fact the heart of the institutionalization of the relationships between academia, the government, and industry.

The importance of social skill in institution building processes

In part because of the contested nature of institution building, strategic positioning, social skills, and collaborative processes were crucial to academic entrepreneurship. The exercise of what Fligstein (1997) has called social skill was hence crucial to understanding why and how one particular institutional arrangement emerged and another did not. Given the cross-sectoral character of academic entrepreneurship, several different kinds of skill proved important. Academic entrepreneurs who had recognized status across multiple fields were better positioned to advance their institution building projects (Galvez-Behar, Brandt). Moreover, the abilities to engage in cross-sectoral dialog and reciprocity were crucial to the emergence of successful commonly imagined futures and important to the allocation of resources and legitimacy to those future-oriented endeavors (Spero). And cross-sectoral team formation in some cases proved crucial to the historical success of particular institution building ventures (Galvez-Behar).
The cumulative and evolutionary process of institutional building

The papers also highlight that the relationship between entrepreneurial processes and historical change was complex, unfolding not through single events or moments but through complex and cumulative sequences of developments over time. A number of historians have emphasized the complex and cumulative processes through which entrepreneurship leads to historical change (Cole 1959; Wadhwani and Jones 2014). Most recently, Galambos and Amatori (2016) have proposed the construct of the ‘entrepreneurial multiplier’ as the sequential processes through which entrepreneurial endeavors build on one another. The papers support this claim and extend it to the domain of entrepreneurial institution building; new institutions were often pieced together over time, sometimes across multiple generations, as academic entrepreneurs built on or reconfigured the efforts of their predecessors (Brandt). But the papers also add two additional complex dynamics to this process. First, they show how major events played a role in this process by reshaping the paths and logics on which new institutions were built and extended over time (Favero). The WWI, for instance, lent greater weight to both the need to engage in scientific institution building in service of the nation and the justification for coordinating such efforts and engaging in them at a large scale (Galvez-Behar). Second, the papers highlight that some institutions were better able to adapt to such processes of change over time, because they were able to be used toward purposes that had not been initially envisioned (Mody).

Shaping modern markets and states

Finally, the articles together show that academic entrepreneurial processes played an important role in shaping the emergence of modern academia, the legitimacy and boundaries of the state, and the rules of modern markets. In other words, academic entrepreneurship as a process was an integral aspect of modern state, market, and academy formation. Whereas the literature that draws on neo-institutional theory to explain academic entrepreneurship typically takes for granted the coercive, normative, and cognitive pressures of states, societies, and market contexts in shaping entrepreneurial action (Etzkowitz and Leydesdorff 2000), the historical studies examined here suggest a mutually constitutive processes at work. The boundaries and capabilities of states (Favero, Brandt, Galvez-Behar) and markets (Mody) not only shaped academic entrepreneurship but were also shaped by academic entrepreneurial processes.

Looking ahead

In recent years, several scholars have suggested that the scope of the research on academic entrepreneurship (Franzoni and Lissoni, 2009; Siegel and Wright 2015a) and related transformations of the university system (Martin 2012) needs rethinking. The wave of research that has emerged since the 1990s focused narrowly on university technology transfer and university-based startups and understood academic entrepreneurship as a relatively new phenomenon. The recent efforts to rethink this scholarship have sought to broaden the scope of activities studied as academic entrepreneurship and contextualize it within the longer relationship between institutions of higher education and society. This article, and the special issue more broadly, has highlighted that historical research and reasoning should be an integral part of scholarly efforts to reconsider the scope of academic entrepreneurship.
Historical perspective, we have showed, does not only provide a longitudinal perspective onto current phenomena, it allows us to confront the limitations of the concepts and theories we take for granted in the present. The articles produced here suggested to the editors and authors a need to think beyond commercialization as a foundation for academic entrepreneurship. The broader definition not only allowed us to take into account non-commercial endeavors that worked to transform academia internally, but also to consider the complex and evolving relationship between academia, the market, and the state. Moreover, rather than treating academic entrepreneurship as a product of institutional context, it allowed us to tap the promise of entrepreneurial history in examining academic entrepreneurship as a driver of historical change in these relationships.

Of course institutional change in academia has been the object of research in other fields of scholarship; we are well aware of the fact that important contributions could be brought to bear by extending the dialog to those fields, such as science and technology studies and the scholarship on education. The creation of a broader connecting framework for research on academic entrepreneurship remains a task for further cross-disciplinary initiatives. In fact we hope that this special issue will encourage further collaboration and interplay with students of the dynamics of institutional change in academia from other scholarly backgrounds and perspectives.

We are also convinced that much remains to be done in extending historical inquiry on entrepreneurial processes to other areas of academia. This special issue focuses on the natural and engineering sciences because they are areas upon which much of the attention of students of academic entrepreneurship has converged. However, we believe that there is much scope for related studies on academic entrepreneurship in the humanities and social sciences.

Finally, the analysis of historical case studies offers to all of us who are working in institutions of higher education and research an opportunity for reflecting on academic entrepreneurship ‘in vivo’, as we see it developing and evolving at present around us. It is a good reminder that we are not only observers of this big game. We are all involved in it, more or less actively engaged but nevertheless participants.

**Disclosure statement**

No potential conflict of interest was reported by the authors.

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“Commercializing Science: Nineteenth- and Twentieth-Century Academic Scientists as Consultants, Patentees, and Entrepreneurs’ with Anna Guagnini and Joris Mercelis.

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An entrepreneurial opportunity in process: creating an industrial fellowship in early twentieth century America

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ABSTRACT

This paper offers insight into the processes by which a pair of entrepreneurial actors envisioned and articulated their model for an industrial fellowship scheme designed to address their mutual needs, interests, and strengths. I employ the framework of 'the new entrepreneurial history,' which focuses on 'the processes through which actors, individually and collectively, pursue uncertain future forms of value,' to analyze the formation of an industrial fellowship research system as it developed in 1906 through a body of correspondence between a chemist, Robert Kennedy Duncan, and a businessperson, E. Ray Speare. The close-reading of these letters provide a clear view into the complex dance of ideation between these entrepreneurial actors as they imagined new opportunities for their fields, assembled resources, negotiated terms for information sharing, and crafted narratives and structures to foster legitimacy.

Introduction and methodology

Entrepreneurship and technological development are both products of and processes for change; transforming what might have been only plausible into the pervasive or even prosaic by bringing together previously unlikely combinations of people, practices, and systems, and in turn creating future institutions, goods, and concepts of value. In this paper, I trace a particular entrepreneurial process, the formation of an industrial fellowship research agreement, as it developed through a body of correspondence. I employ the framework of 'the new entrepreneurial history,' specifically 'the study of the processes through which actors, individually and collectively, pursue uncertain future forms of value, thereby transforming capitalism from within,' as developed by business historians R. Daniel Wadhwni and Christina Lubinski (2016). This approach treats the process itself, rather than organizational form or institutional constraint, as the primary object of study. Their definition incorporates four key premises that highlight what makes entrepreneurial history unique: (a) the temporal foundations of agency; (b) multiplicity in the forms of value; (c) the collective and cumulative character of entrepreneurial processes; and (d) entrepreneurship as a driver of historical change. The critical role for historical methods for understanding the processes of
entrepreneurship and the ‘new combinations’ that its actors devise in their pursuit of future goods, practices, markets and organizational forms is also emphasized by Jones and Wadhwani (2006) in their thoughtful interpretation of the contributions of Joseph Schumpeter.

The close-reading of correspondence is well-suited to a focus on understanding processes of entrepreneurship in action and the embedded sense-making practices in which actors work to understand new issues or actions that may deviate from established practices or expectations (Maitlis and Christianson 2013). Even within the relatively brief exchange of 13 letters sent between 8 October 1906 and 18 March 1907, it is possible to clearly observe the complex dance of ideation between a chemist, Robert Kennedy Duncan, and a businessperson, E. Ray Speare, as they iteratively craft their fellowship agreement. Correspondence can provide a unique window into the entrepreneurial process as documents that occupy a liminal space between the more public world of actions and the private sphere of ideas. The correspondence, however, should also not be interpreted as an artifact of an entirely private or intimate domain. The nature of their communication, though not explicitly public is most certainly rooted in the professional arena and was consciously crafted to establish a business relationship as well as to persuade the other party. In their research methods article about business communication Locker et al. (1996) encourage scholars to be aware of the historical context and writing conventions of the time period outside of the arena of business practice (Wolff 1979; Richardson 1980; Carbone 1994; Lund 1998). While the particular style of communication is not the focus of my study, I found this set of letters to be generally consistent with Duncan’s published works as well as his other surviving correspondence in the Carnegie Mellon University Archive’s Mellon Institute collection. The exchange between Duncan and Speare, though cordial, is free from references to conversation, or other aspects of everyday life, which is further evidence that the entire scope of their interaction is contained within these documents.

By following their correspondence, it is possible to observe entrepreneurial processes as they developed in their actual context, rather than as retrospective finished forms such as reports or resulting organizational structures. Business historians Andrew Popp and Robin Holt (2013) encourage the use of correspondence to ‘make present’ the long and iterative origin of entrepreneurial opportunities. Through a close reading of a body of letters between John Shaw and Henry Crane, who established a new merchant house in Calcutta in the early nineteenth century, Popp and Holt place emphasis on their navigation of uncertainty, opportunity, and place within historical context. Through the documented iteration of ideas between entrepreneurial actors, these types of sources can help us to ‘move away from the idea of “origin” as a unitary moment of enlightened commencement by a person of specific traits, or the mere outcome of structural forces, and instead conceive of it as a constant interplay.’

Wadhwnani and Lubinski (2016) offer three main processes by which actors (1) imagine and design novel goods, services, markets, practices, and organizations; (2) assemble and recombine resources in order to pursue these opportunities; and (3) legitimize the innovations they introduce. These categories are especially helpful when analyzing the interplay between the correspondents as their collaboration progressed, since they allow us to focus more on the iterative shaping of ideas themselves rather than taking a primarily chronological approach. The sections of this paper also similarly follow this organization, highlighting the development of the fellowship agreement through the lenses of imagining, assembling,
and legitimizing. They identify the fellowship as something of future value, which does not yet exist in the present, and consider how to allocate and combine resources to shape that particular opportunity. These three categories may appear to be linear steps that take an idea from concept to the practical concerns of implementation, and finally to justification of the new product in its finished form. However, these themes often interplay as a relationship takes form, resources are lost or found, and audiences change. The categories of imagining, assembling, and legitimizing are also not mutually exclusive. For example, legitimacy-making was present in some form throughout the whole process, whether manifested in Duncan’s thinly veiled criticism of other universities and praise for his own students in his letters, or in the more publicly oriented discourse about the value of industrial science to American industry.

The letters, with their open back-and-forth quality, illustrate the inner workings of these arrangements in the making. While Duncan first formulated the vision for his particular model of academic–industrial research based in applied chemistry, it was Speare who grounded Duncan’s theory through his critique drawn from his own industrial experience. They discussed the need for chemical research within industrial settings; the practical concerns associated with setting up the fellowship; and the necessary rules placed on usage of knowledge generated through the fellowship. Unsurprisingly, the topic of intellectual property was the most contentious of the concerns that Duncan and Speare addressed in their letters.

**Entrepreneurial actors and their historical context**

The United States in the early twentieth century was a place of both vast industrial power and systemic uncertainty. In the lingering glow of the economic prosperity associated with the late nineteenth century boom in industrial production, development of urban markets, growth of transportation systems, Duncan, Speare, and their colleagues would have wondered—how would the nation maintain this pace of development? Unsurprisingly, along with the newfound wealth, productivity gains, and increasing opportunity came also significant growth in organizations that might be able to contribute to longer-term stability (Boyle 2001). Research, an enterprise that both relied upon and supported the growth of personnel with specialized knowledge, emerged as one of these possible remedies. However, there was a great diversity in places and practices for the work of applied or industrial science broadly configured. Laboratories were maintained at universities, independent institutes (often with academic affiliation and industrial projects), consulting firms, and corporate laboratories (Bartlett 1941; Geiger 1986; Kahn 1986; Hounshell and Smith 1988; Cornell 2004; Kline and Lassman 2005; Bertrams 2007). Educational institutions and specialized degree programs in engineering and applied sciences grew along with new professional identities. At both public and private institutions, programs often developed in coordination with regional industries (Aker and Seely 2015). Noble (1977) characterizes this period in engineering higher education as ‘the wedding of science to the useful arts’, a practice-driven strategically by the needs of corporate capitalists, who stood only to gain by the creation of a sustainable skilled workforce in collaboration with institutions of higher education. However, this straightforward characterization of corporate agency neglects the role of academic-entrepreneurs, who often worked at the intersection of academy and industry. ‘Institution builders’ such as Dugald Jackson, helped to shape engineering education
according to the priorities of his own academic institution through collaboration with industrial partners despite the initial reluctance of corporate leaders. As chair of the Department of Electrical Engineering at the Massachusetts Institute of Technology (MIT), he championed a cooperative course with General Electric from 1907 to 1932 (Carlson 1988).

Robert Kennedy Duncan participated in a similar professional world, aiming to link practical industrial research opportunities for students with their university studies. In 1906, he was a chemistry professor at the University of Kansas, recently arrived from Washington and Jefferson College in Pennsylvania where he taught between 1901 and 1906. He had previously taught at the post-secondary level and was known for his ability to explain chemical principles to a non-specialist audience (Hamor 1929). Duncan was known for his role as a ‘chemical crusader,’ promoting applied chemistry through popular science writing (Rhees 1987). Duncan was also very close with his brother Norman, a writer at literature professor who no doubt influenced this expression (Rhees 1987; Spero 2014). His belief in applied science as both a necessary and indeed urgent tool, for a technologically mediated idea of progress echoed throughout his public writing. It also highlighted emerging concerns in the industrial sector. Following his first book, _The New Knowledge_ (1905), Duncan wrote ‘The Chemistry of Commerce’, which was originally published as an 11-part series in Harper’s Monthly Magazine, between 1905 and 1907. These articles drew on his observations in Europe and included a range of topics from the chemistry underlying rare earth metals to the chemistry of medicine. They were aimed at a general readership and were placed in this popular variety magazine interspersed among other serial features on topics such as romance, drama, law and history, poetry, travel, and cartoons and humor. Harper & Brothers later published the series as a book in 1907, as well as Duncan’s third and final book Some Chemical Problems of Today in 1911. Duncan is perhaps best known for his later role in establishing the Mellon Institute for Industrial Research, with the University of Pittsburgh in 1913, an institution which was built around the same industrial fellowship model that arose from the initial program created between Duncan and Speare discussed in this paper (Weidlein and Hamor 1936; Bartlett 1941; Rhees 1987; Servos 1994; Spero 2014).

The kinds of industrial issues that intersected around chemical issues, like those Duncan described in his popular writing would have been familiar to businesspeople like E. Ray Speare, the manager of Alden Speare’s Sons Company, which specialized in the import, export, and manufacture of oils, starches, emery, laundry and mill supplies. Established by his father Alden Speare in 1851, the company had plants in East Cambridge, and a general business office in Boston, and employed between 400 and 500 operatives, in addition to a national network of sales representatives. The area of East Cambridge where Alden Speare’s Sons Company operated its plants included many similarly small- and medium-sized single building operations linked to industries such as textile manufacture that had mills and processing plants in towns farther away from the city center (Stone 1930). The largely mechanical world of textile manufacture and processing that Speare operated within was well positioned for increased chemical intervention. Historian of technology and industrialization, Arwen Mohun (1997) describes many of the challenges that Speare and his colleagues faced as they tried to shape a technologically centered niche for cleaning textiles and win customer support. The ‘steam laundry’ and the ‘laundrymen’ who were developing this new business through the application of technologies adapted from textile finishing to take cleaning outside the domestic and feminized spheres encountered both technical and marketing challenges. In order to lure customers away from the familiar domestic methods for laundry, they needed to consistently perform without damaging goods.
A company like Alden Speare’s Sons might have some chemical knowledge on staff, but hardly the resources for a research and development project. Although some PhD scientists were employed in the railroad and oil industries, and many had also consulted for industrial firms before 1900, the formalized industrial research laboratory as an institution was new to both science and the corporation in the early twentieth century. Even in large successful firms such as General Electric, Du Pont, Eastman Kodak, American Telephone and Telegraph, which became known for their research divisions, the industrial laboratory emerged from smaller informal efforts, not unlike the exploratory projects undertaken through industrial fellowships (Hounshell and Smith 1988). Hounshell and Smith (1988) argue that even in these large firms ‘science has been a dynamic element, changing and being changed by other elements of corporate performance,’ without a singular model of strategic development. In their history of Alcoa, Graham and Pruitt (1990) describe the industrial laboratory as ‘an institution suspended between two worlds – that of industry and the marketplace, on the one hand, and that of the scientific professions, on the other.’ This image of the industrial laboratory, within a corporation or research institute, as one delicately balancing the domains of business and science, calls to mind both the power and fragility of this position.

Although the connections between science and industry were growing during this period, the exact relationship between these domains (and perhaps also how to define them) is still debated by scholars from business and science studies perspectives (Grandin, Wormbs, and Widmal 2014). Shapin (2004, 2012) problematizes the assumption of an inherent conflict between scientific values and the professional role of the industrial scientist in the twentieth century, along with the idea of the ‘ivory tower’ as an academic mode of practice beyond its use as a rhetorical device. Likewise, Edgerton (2004) not only warns against the reification of a linear model of innovation, which presumes that fundamental scientific research in the academy drove industry, but goes further to assert that it actually never existed and that the concepts of ‘science’ and ‘research’ need to be understood within institutional and historical contexts. In this paper, I am less concerned with supporting a particular model of interaction than I am in tracing the process by which two entrepreneurial actors crafted their relationship and used the relevant categories of their own professional domains to describe and legitimize their actions. Rather than reifying categories of particular academic or industrial domains as entirely separate, fundamentally entrenched, or connected in a particular arrangement, perhaps it is more useful to recognize the multiplicity of forms that the work that people within these fields sought to create, along with value, and professional identities.

In the sections that follow, I analyze the correspondence between Duncan and Speare in an effort to ‘make present’ their entrepreneurial process. For these two individuals a clear articulation of their own professional interests, and more broadly the values of cooperation between academy and industry that resonated with especially creative power in their particular moment in time (Popp and Holt 2013). This process helps us to see the world as they saw it, fresh with possibilities and rich with uncertainty.

**Imagining**

**Old problems and new connections**

This venture originated neither around a meeting table nor in a club lounge. Rather, it emerged most intriguingly in a body of correspondence between two strangers. Although
the fellowship concept would later provide a model for scientists looking beyond the academy for either funding and future employment (or both) from industrial partners, in this formative case, it was a letter sent in the other direction, from an industrialist to an academic that initiated the collaboration.

In September of 1906, E. Ray Speare, read Robert Kennedy Duncan’s article ‘The Wonders of Cellulose’ in Harper’s Monthly Magazine. For Speare, the chemical challenges of industrial-scale laundry that Duncan described in his article were all too familiar. Duncan estimated the large scale and cost of the laundry business at £2,000,000 a week in England alone. He criticized the way that ‘at present the laundry practises its trade with a joyous ignorance of the properties of cellulose and of the chemical agents it employs, and it is admirable only as it increases the consumption of textiles.’ He noted the potential to link the well-developed chemical processes of bleaching and finishing to systems of washing, which were primarily mechanical.

It is high time this work was organized along sensible chemical lines … It is to be hoped that some man will write a chemical ‘Song of the Shirt’ that will establish in the minds of laundrymen the conditions that make for its longevity. (Duncan 1906, 578)

Confronted with what Speare characterized as a ‘resentful reference to the process in use for the cleansing of linen and cotton textiles’ in Duncan’s article on cellulose chemistry, he reached out to Duncan with the hope of gaining access to ‘a little bit of the brain that lies back (sic) of such an article.’ While the Boston location of this industrial partner for the inaugural fellowship at the University of Kansas may be surprising because of the abundance of universities in closer proximity, the nature of the laundry business, especially at an industrial scale, was quite well-suited to implementation of research in applied chemistry. When describing his industry to Duncan in his first letter, Speare noted the marked growth in the business in the United States and England due to mechanical improvements in laundry equipment. However, he also expressed the need for chemical, rather than mechanical solutions to problems in industrial laundry at this stage of development. He lamented that the chemical aspects of the laundry business ‘has had but little attention.’ Speare noted that ‘the only marked advance’ in his industry was the creation of ‘higher grade product’ for use in standardized processes of washing, bleaching, and blueing, with ‘little or no careful chemical attention to the actual improvement of the process itself.’ Although Speare had encountered some chemists in his business, he found them far from the creative type described by Duncan. He emphasized that they were

fully occupied with dealing with the new products we work from in our manufacture, and, like many of the chemists I have come into contact with, their long work on specialized lines has made them men of few ideas, highly developed.

For Speare, Duncan’s article and advocacy for greater employment of applied chemistry to industrial problems spoke to an issue that had been long weighing on his mind. He wrote, ‘I have felt for sometime the need of advanced ideas and actual chemical knowledge to improve the process of laundering.’ Speare envisioned that chemical improvements would both increase cleanliness and reduce mechanical stress on laundered goods. He described the practice of laundering both at home and in an industrial setting as a ‘partially chemical but largely physical’ process. The ‘physical wear on the goods so treated is unnecessarily excessive.’ He lamented that even ‘goods washed in the most modern and up-to-date steam laundry … go to pieces in short order.’
He described leaders in the laundry industry such as himself as weary of complaints about damaged goods, and he assured his reader that they ‘would more than welcome advanced ideas.’ From his perspective, process improvements should result in ‘reduced wear and tear on goods washed’ as well as economic benefit for the company by a reduction in material and labor costs. Speare also saw the opportunity for increasing the impact of these potential improvements through the already established scale of his own business network that employed a force of salesmen that visit every steam laundry in the United States during the year. He acknowledged that process changes might be difficult for his colleagues in the textile trades to universally accept as improvements. ‘I could bring about the adoption of sane methods that would certainly save the man who pays countless dollars, even though the “textile industries” might suffer from loss of trade thereby.’ This apparent tradeoff between the prolonged lifespan of goods versus the creation of new products and their markets also highlights the difficulty of characterizing an industry and its interests as a whole.

In response to this first letter from Speare, Duncan acknowledged that he had received a great many letters from laundrymen in the U.S. and England, but chose to respond to Speare particularly because his letter, unlike those from many of his peers, ‘showed real knowledge and because it was so broadminded and so sane and hopeful.’ Throughout the exchange of letters between the two men, this tone of mutual respect for each other’s specialized knowledge, balanced with an appreciation for ‘broad mindedness,’ sets a foundation for their nascent business relationship.

Although Duncan himself couldn’t spare the time to undertake such a project, he offered another solution to Speare: graduate student researchers. He enthused, ‘they know their business, they seem to have limitless energy, are trustworthy, and altogether a fine type of men, speaking generally, as I say.’ He suggested a fellowship program, relying on graduate researchers, aimed at addressing Speare’s kind of practical industrial problems. The idea came from his own recent observations abroad, where he had been conducting research for his Harper’s Monthly article series. In Europe, Duncan ‘found conditions of extreme significance,’ especially in Germany where he noted with enthusiasm that, ‘universities, factories, banks and carrying companies, are coordinated into a most efficiently working mechanism.’ He retold the ‘not uncommon’ story of Professor Adolf Frank of Berlin who upon creating a new type of fertilizer approached a large manufacturing firm, Siemens and Halske. Together the professor and the company representative then went to the Deutsche Bank, which ‘employed its experts to decide upon the process.’ They then formed a new company that included ‘the Deutsche Bank with its money, Professor Frank with his invention, and Siemens and Halske with their immense experimental facilities.’ The arrangement that Duncan proposed did not directly mirror the German case, yet his plan shared its idealized view of an efficiently coordinated working relationship with university research at the center. Duncan envisioned both the inventions and experimental facilities would be housed at his university, supported by financing from the sponsoring company. At this time German chemical companies were world leaders especially in the creation of synthetic dyestuffs, and also known for coordinated systems of knowledge transfer and business development. (Beer 1959; Schröter 1993; Murmann 2000, 2003) By telling this German story to Speare, Duncan was able to ground his own proposal on an established example of productive coordination between apparently disparate entities to foster successful business development. This story also served to bolster Duncan’s credibility as someone who had not only traveled abroad in a professional capacity but could also use these experiences to strengthen American business.
Duncan imagined the fellowship program he proposed not only as a boon for the creation of what he called ‘useful knowledge’ and practically oriented pedagogy, but also as a way to establish a stable employment path for his students. Indeed, Duncan’s concept for the fellowship system quite consciously not only created roles for young scientists to gain experience through the individual project, but also offered them opportunity for longer term employment at the sponsoring company. ‘Only because I have been impressed by the intelligence and broad mindedness of your letter,’ Duncan offered the proposition of a temporary two-year fellowship (for $500/year) to Speare himself. Furthermore, Duncan proposed that he would personally advise and supervise the research of a fellow who was devoted entirely to the chemistry of laundering. He described the plan as he saw it,

the young man chosen should first of all thoroughly investigate the literature on the chemistry of laundering, and digest it all. Second, that he should go into a laundry or laundries, two or three in succession, and learn practically all of the chemical details of the business. Then knowing practically all there is about the subject, he should enter my laboratories, and work for his life.16 He laid out an ambitious plan for this young industrial fellow who would ideally also produce a book at the end of the two-year term, ‘treating exhaustively and critically the chemistry of the laundry business’. Duncan would help him find a publisher for this book and ‘by that time I should expect him to have made some steps of practical importance, working as I say under my guidance and direction’.17 Warren Fred Faragher, a 23 year old chemistry instructor at the University of Kansas was selected as the inaugural fellow for Speare’s laundry research. Described by Duncan as having ‘a splendid training and, as well, mental attainments of a high order’, this young researcher was unanimously deemed the ‘best young man’ in the Chemistry Department. He gave up a higher paying two-year post with Sir William Ramsay in London for the fellowship position.18 This young scientist, Dr. Fred Faragher went on to serve on the staff of the Mellon Institute for Industrial Research in Pittsburgh and later continued his career in the field of petroleum. This laundering fellowship led to the establishment of the American Institute of Laundering, whose president, Dr. George H. Johnson, was also a former industrial fellow (Weidlein 1963).

Assembling

Role-playing and compromise
Not only did Duncan and Speare devise a new combination of resources and rules for their own fellowship, they also quite self-consciously took on the archetypical stances from their own professional domains (the cold blooded businessman and the knowledge-driven academic) in an effort to setup their fellowship as a model that would extend beyond their immediate needs. Duncan and Speare both agreed that information should be shared in the form of periodic reports. However, the issue of knowledge sharing more broadly outside of the particular partnership remained a point of contention. Although Duncan did not ask for any royalties on behalf of the university, he did suggest that, ‘one tenth of the net profits of any discovery made during the course of this investigation should belong to the holder of the fellowship.’19 He considered this ‘a wise thing’ which ‘would afford every possible incentive to the man’s utmost efforts’.20

Speare then offered some changes that from his point of view seemed necessary regarding the mechanics of the fellowship agreement. Not surprisingly, all of these alterations related to proprietary information and the maintenance of competitive advantage through
the proposed research collaboration. Speare made both his role and point of view in the agreement quite plain:

Of course, this comes down, as you can readily understand, to a cold blooded business proposition with us, and, for this reason, we should discountenance the publication of a book making public property of the results of these investigations attendant on this work. 21

He reiterated that, ‘our basis in considering a scheme of this kind is, of course, to make money.’ 22

While the financial agreement was proposed between the university and the industrial sponsor, the intellectual property was shared between the research fellow and the sponsor. Speare made an effort to ensure a long-term relationship with the fellow:

we would also want an understanding established with the young man whom you might pick for the work, to the effect that the expiration of this course, or prior to that time if the results obtained justified it, his services should be ours for a certain term of years – the terms to be mutually satisfactory. I think you will agree with me that this would be a wise proviso, as it would be only natural to suppose a case working out somewhat as follows: the man in question might make some valuable discoveries during this work, and, at the completion of the course, take the matter to our competitors and let them reap the benefits which should rightly be ours for fathering the idea from its inception. 23

He agreed without reservation to the 10% net profit benefit for the fellow. He emphasized,

we would want the holder of this fellowship to have every interest in working out this proposition and will be only too glad to make it worth his while – it being of course understood that any and all discoveries he might make during his course should be our property subject to the payment of the royalty you mention on the sale of such products. 24

Only the fellow would be entitled to royalties of any kind through this agreement and the university would get the right to publish findings from the research only after an agreed-upon period of time, but would not have any role in licensing technologies or filing patents.

Just as Speare had adopted the role of the ‘cold blooded businessman,’ Duncan argued on behalf of knowledge sharing, invoking in particular the point of view of the board of regents. From this perspective, he questioned the public benefit of the fellowships arrangements, ‘What has the university got to do with it? … Is it simply for helping a factory and one young man?’ 25 Recognizing that a potential corporate partner would of course ‘want the worth of your money and your risk,’ Duncan offered a compromise that could both safeguard and disseminate research findings.

the work of the proposed fellowship shall, at the conclusion of the two years, be written out for you and the university: that the university shall place this sealed report in its archives until the expiration of three years, when it shall be at liberty to publish the report. In this way the university confers knowledge on men, and you get the knowledge you want and three years’ advantage of it. 26

To further dispel Speare’s potential concerns, he placed emphasis on the fact that ‘such a report, no more than a patent specification would contain all that is required for the application of whatever discovery might be made.’ 27 Indeed, opportunity for regular informal contact with the research fellow would also give the company access to information that might not be conveyed in a formalized final report. Duncan also highlighted the differences between the academy and industry in terms of knowledge value over time. He noted that after the three-year period of secrecy had expired, the industrial partner ‘would care nothing,
with such a start, as to what was known three years before' whereas this information would still have academic value. In response, Speare agreed to the three-year protection plan provided that ‘it is understood that any patentable original ideas developed during this time should become the property of this Company, subject to a royalty payment to the inventor.

**Legitimizing**

**Future goods and value**

There were of course wider implications embedded in this carefully negotiated initial arrangement. Process improvements to the chemistry of laundry systems were just a single example in an open field of unmet industrial needs. Through these fellowships, Duncan’s goals combined both knowledge production and labor to establish the role of science and scientists in industry. Although the arrangement between Duncan and Speare began as an individual agreement, Duncan was keenly aware of the precedent for collaboration that they were setting, as well as the opportunity to create a model that would scale far beyond the interests of a single company or even industrial sector. The scalability of the model was also a key factor in building its future value as well.

With this potential for creating a model to promote the coordination of future industrial research, Duncan offered both his services and those of his university to generate publicity to promote Speare’s ‘wise and generous action in establishing such a fellowship.’ Duncan lauded Speare for his actions, which he considered to be ‘an innovation and one which it would do industry an incalculable amount of good to follow.’ Speare initially had no interest in publicity, fearing that publication ‘while working as a simple advertisement, [would] at the same time cause more or less annoyance by solicitations for the same purpose from other sources.’ However later, when the fellowship carried the name of his father, the late Alden Speare who was active in both education and industry, the prospect of publicizing seemed more appealing.

Emphasizing the impact that he and Speare could have on the future, Duncan forecasts,

> the establishment of a fellowship of this kind would be an innovation and would have, I believe, ultimately, an importance in the relation of modern chemistry to industry and in the progress of American industry that could hardly be exaggerated.

With this in mind, Duncan urged Speare to project beyond their immediate concerns.

> I am sure that you will agree with me, then, that we ought to arrange matters between ourselves in such a way that the arrangement will stand more or less as a model for all others between all universities and all industries.

Duncan’s fellowship plan addressed the need for skilled workers, as well as efficiency and organization in American industrial centers. In his mind, it was only through coordination of these too often disparate actors that progress could emerge. He phrased this desire in terms of a system of networked benefits.

> Now, what interests me is some method by which I can bring into coordination the factories, the universities, and that not for the good of the factories alone, but for the good of the people and factories, and our young men, and for the increase of knowledge .... At present the old established industrial processes are working at great waste and their efforts towards betterment are most haphazard.
Duncan viewed this coordination of university and industry also as a solution to the difficulties faced by companies in identifying employees with chemical skills. He called for ‘some kind of sympathetic cooperation’ between the university and business partners and emphasized the need to establish the criteria for mutual benefit. ‘This then is our problem. This cooperation in the form of a fellowship has got to be good for you and it has got to fall into line with our work or else it is not practical.’

Duncan reiterated the importance of the precedent that they were setting through this agreement and urged Speare to ‘draw up your agreement statement in as liberal a spirit as possible … written in a dignified way.’ He predicted, ‘If this fellowship is brought to a successful issue it is going to initiate a great change in American industry’. In the postscript to that same letter, Duncan reiterated the importance of their agreement document as a model which will stand for all coming ones of this kind. This research partnership, developed with Speare on the specific topic of laundry chemistry, quickly and quite deliberately became a model for academic-industrial collaboration more broadly. Over the next three years, another 16 fellowships were added to the program at the University of Kansas. They included (in order of creation) work on the following areas; alfalfa, salt-rising bread, casein, oil, enamel, glass, cement, varnish, borax, adrenaline, vegetable ivory, gilsonite, fats, leather and copper.

The fellowship structure and knowledge generated were only part of the ‘future goods’ created. Indeed, the fellows themselves and their specialized research were also crucial drivers and products of the system. Duncan intended the industrial fellowship program as a way for young scientists to begin to build successful careers. For this cohort of young scientists, acquiring not only this particular skillset, but also along with it legitimacy was important to establish value when placed into their projects and for future employment. He said,

> It seems clear that these problems can be best answered by combining the practical knowledge and large facilities of the factory with the new and special knowledge of the universities, and by making this combination through young men who will find therein success and opportunity. (Duncan 1907, 255)

Through the industrial fellowship program, Duncan intended young scientists to transform, as he put it in The Chemistry of Commerce, the ‘vast body of knowledge called Science and to make it subserve the practical needs of the human race (Duncan 1907, 255).’ He also made it quite clear that this type of scientist should always be employable and highly valued. For Duncan (1907, 247), the kind of expertise created by academic–industrial cooperation was timely and necessary because problems that could be handled in the factory through rule of thumb methods were now a thing of the past. He asserted that the ‘problems having obvious and apparent answers have all been solved.’ Despite the dearth of unsolved problems with what he called ‘obvious solutions,’ there was still much work to be done for the academic laboratory and profit to be had for the industrialist who invested in chemical research. Chemical processes played a critical role in maintaining and creating new opportunities for competitive advantage by streamlining existing processes by synthesizing or substituting for naturally occurring components, or finding use for waste materials. In addition, chemistry could offer several practical advantages over other scientific fields for a high potential return on investment in both corporate and academic settings. First of all, experimental practice most often included laboratory bench scale chemical work. Unlike building or testing physical machinery (especially related to large scale factory production), a chemical lab required comparatively little investment in specialized equipment or large workspaces. Moreover,
single process could have several applications across different industrial sectors, making chemical work adaptable and flexible both physically and intellectually. For Speare, and other likeminded businesspeople, the fellowship program was a mechanism for not only securing a competitive edge through the development of technological solutions to pressing industrial problems, but also a means for gaining access to skilled labor at relatively low cost.

Conclusion

In the preceding sections, I have made a conscious effort to offer the interaction between Duncan and Speare in their own present, as they con-construct their industrial fellowship agreement iteratively imagining, assembling, and legitimizing the entrepreneurial opportunity before them. This industrial laundry fellowship, although an end in itself, is also part of another beginning. Duncan publicized the fellowship program at the end of his second book, The Chemistry of Commerce, which gained the attention of Chancellor Samuel Black McCormick of the University of Pittsburgh, and financial backing of bankers, industrialists, and philanthropists Andrew and Richard Mellon. (Bartlett 1941; Rhees 1987; Cannadine 2008; Spero 2014) The narrative of the founding of the Mellon Institute that follows seems to unfold with the linearity of legend, in part because it was shaped and written by the actors themselves and often retold. The history of the Mellon Institute for Industrial Research, as an academically affiliated, yet business-oriented research center is in itself a rich example of institutional academic entrepreneurship. This mainly post-graduate research center, a so-called ‘armory of applied science,’ founded in Pittsburgh in 1913, played a foundational role in shaping what was then an emerging American research and development movement. During its first 25 years of operation alone, the Mellon Institute served 3600 companies from a wide variety of industrial sectors either as firms or members of trade associations. Out of these collaborations came 500 novel processes and products, and from them ten new industries were created. (Weidlein and Hamor 1936) Over the quarter century that followed, the Mellon Institute continued to shape the landscape of American R&D as collaborative fellowship projects in some cases grew into in-house corporate laboratories. Some large corporations such as General Electric and DuPont had their own research divisions from early on, yet smaller companies could begin their research activities through collaborations with institutes and universities setup along the lines of the Mellon Institute fellowship model. Following a period of decline, which Servos (1994, 222) has characterized as ‘more than a decade of erosion and soul searching’ it merged with Carnegie Tech to form Carnegie Mellon University in 1967. Despite earlier success, Servos’ critical interpretation of this later period underscores the need for adaptation both within an organization as well as in relation to the broader professional world. Servos attributes the decline of the Mellon Institute after the World War II in part to an outdated model of science fundamentally linked to industry both technically and financially failing to adapt to the emerging importance of federally funded basic research. By the time of the merger, indeed the landscape of American research and development had shifted. Laboratories in large corporations had become more the norm than the exception. In 2013, the American Chemical Society designated the Mellon Institute of Industrial Research, now part of the campus of Carnegie Mellon University, as a National Historic Chemical Landmark.

Without the later success of the Mellon Institute, the 1906 correspondence between Duncan and Speare might not have been saved, let alone collected in its own folder for
future reference and archiving by Lois Whittle, Duncan’s first employee, who would spend a 45-year career managing the operations of the Mellon Institute. I offer this case not only as an exercise in ‘making present’ a window into the processes of imagining, assembling, and legitimizing an entrepreneurial opportunity, but also as a broader call to revisit organizational histories that may appear already ossified into familiar legend with the lens of experiment and uncertainty. The focus on process and sense-making rather than a perhaps more tempting method of working backward to find an origin story for a successful endeavor, highlights the role of narrative and identity-shaping that persists throughout the interaction, as well as in Duncan and his colleagues’ public writing (Duncan 1905, 1907, 1911; Weidlein and Hamor 1931, 1936). The role-playing as an ‘academic scientist,’ interested in knowledge for its own sake, and the profit-oriented ‘cold-blooded businessman’ that Duncan and Speare take on quite consciously during their correspondence serves to both reify and problematize these categories. Only with the knowledge that these categories were fluid, would Duncan and Speare be able to use them as rhetorical devices to achieve compromise. Likewise, they were mutually interested in creating and promoting a type of skilled worker who would be able to more easily traverse these professional spheres. Finally, the broader role of narrative, especially in legitimacy-making was present throughout the creation of the fellowship. In these shared stories, they could not only describe familiar problems, but also link them to imagined solutions, and also work to legitimate the future goods of both discovery and personnel in a changing world of research and business. Indeed, much of Duncan’s success as an academic entrepreneur could be linked to his public writing in which he described a plausible, yet markedly different vision of the changing world at large – one that could be made possible through coordinated efforts in applied chemistry. After all, it was Duncan’s article in Harper’s that sparked the correspondence with Speare, and consequently his book The Chemistry of Commerce attracted the investment of the Mellon brothers. Sidney Kirkpatrick (1937), the editor of Chemical and Metallurgical Engineering, began his commendation of the Mellon Institute for Industrial Research and the legacy of its founder Duncan, not with praise for contributions to the field of chemistry, but rather the catalyzing power of narrative.

In this story the moral must come first: if you have a big idea, bigger perhaps than you can master alone, write a book about it. The chances are that somebody will read your book and, if sufficiently impressed, may do something about it.

Notes

1. Records pertaining to the Alden Speare’s Sons Co. especially the properties owned and managed in Cambridge are located at the Cambridge Historical Trust.
3. ‘E. Ray Speare to R.K. Duncan October 8, 1906.’
4. Ibid. ‘the actual process of washing, the actual chemistry, of the laundry business has had but little attention. It has been taken for granted that the old process of washing, bleaching and bluing the good was right and the only marked advance has been in the turning out of higher grade product to perform these functions with little or no careful chemical attention to the actual improvement of the process itself.’
5. Ibid.
6. Ibid.
7. Ibid.
8. Ibid.
9. Ibid.
10. Ibid.
13. Adolf Frank (1834–1916) studied chemistry at the University of Berlin and earned his PhD at University of Gottingen. He is most well known for inventing a process to fix atmospheric nitrogen along with Nicodem Caro. Based on this work, he founded Cyanid Gesellschaft in 1899 with the support of Deutsche Bank, Siemens & Halske, and Deutsche Gold und Silber Scheideanstalt. The formation of this company is what Duncan is relaying to Speare. While Duncan does not get the details about Frank’s career quite correct, the main point that he is trying to convey to Speare that in this German case there is a close and productive connection between banks, corporations, and individual scientists. Cyanid Gesellschaft, was not yet profitable at the time when Duncan was telling this anecdote. It was only after World War I that the company became commercially successful. Frank’s personal papers are located at Leo Baeck Institute Center for Jewish History in New York – the finding aid is accessible online at: https://findingaids.cjh.org.
17. Ibid.
20. Ibid.
21. ‘E. Ray Speare to R.K Duncan October 31, 1906.’
22. Ibid.
23. Ibid.
24. Ibid.
27. Ibid.
28. Ibid. ‘The only possible hitch to a practical arrangement of this very important matter for American industry is this question of secrecy, and I am hoping that the compromise of the matter that I have suggested will be acceptable to you.’
30. ‘R.K. Duncan to E Ray Speare October 13, 1906.’
31. ‘R.K. Duncan to E Ray Speare October 13, 1906.’
32. ‘E. Ray Speare to R.K Duncan October 31, 1906.’

Alden Speare was one of the original founders of Boston University, and former president of its board of trustees. He was also a former president of the Boston chamber of commerce and associated board of trade. E Ray Speare noted, ‘in regard to your proposed article for the North American Review … any expression you might deem wise to make in reference to this fellowship and our connection with it, would of course, be very much appreciated by us and we should be much pleased to have you mention our name in connection with this in any way you see fit.’
35. ‘E. Ray Speare to R.K Duncan November 10, 1906.’
36. Ibid.
37. Ibid. Duncan described his views of the problems facing industrial leaders, ‘when they want “good men,” and they always do, they do not know where to apply for them, and when they are confronted with chemical problems, and what industry isn’t, they don’t know what to do with these problems.’
38. Ibid.
39. ‘R.K. Duncan to E. Ray Speare December 5, 1906’ in Carnegie Mellon University Archives, Mellon Institute Documents Box 209, ff7664. Duncan also reminded Speare ‘the University is entering upon this work solely for the purpose of increasing useful knowledge and that in order to do this it is extending help to you.’
40. ‘R.K. Duncan to E. Ray Speare December 5, 1906’.
41. A phosphoprotein commonly found in milk.
42. A hard white endosperm of seeds from palm trees that resembles ivory.
43. A naturally occurring hydrocarbon resin found in northeastern Utah.

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References


Envisioning a national infrastructure for science – academic entrepreneurship in 1890s–1950s Norway

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ABSTRACT
This paper investigates the importance of entrepreneurship in the establishment of institutions for scientific research in Norway from the late nineteenth century until the mid-twentieth century. By rethinking the historical development of three forms of institutionalization of scientific research – the science academy with its private funds; the central research institute; and the research council – this paper provides insights into how the entrepreneurial process worked to identify future opportunities, gather resources and legitimize ventures for the organization of science in Norway. The paper pays special attention to the role of a select group of scientists, and their inter-generational transfer of knowledge. At the same time, the paper argues for academic entrepreneurship as an inherently collective endeavor, involving also state representatives and industrialists. The methodology of the long-term historical narrative allows for highlighting the specificity of the Norwegian development, while acknowledging the importance of the shifting contexts of entrepreneurial action in a period marked by two world wars and economic crises.

KEYWORDS
Nationalism and Scientific Research; motivations, modus operandi and perspectives at different organizational levels; transfer and appropriation/adaptation of organizational models; academic entrepreneurs and their national/local environment; collective learning in processes of institutional entrepreneurship; scientific research organization; entrepreneurship and historical change; Norwegian science policy 19th–20th century

Introduction
The purpose of this paper is to examine the importance of entrepreneurship in the establishment of the institutional arrangements for scientific research in Norway from the late nineteenth century until the early 1950s. In this period institutions for research outside academia were founded in most industrialized nations, and Norway thus followed a general pattern of growing public awareness of science as both a means for and a measure of modernity (Fox and Guagnini 1999). Norway was a young nation with relatively limited resources for science and a small scientific community. Still, like many small Western nation states in this period, Norway to a large extent mirrored, or at least echoed, international research policy trends (for a similar observation on neighboring Sweden, see Kaiserfeld 2013). Thus, focusing on Norway enables the discerning on a small scale of a general international pattern of organization of scientific institutions spurred by efforts we may term entrepreneurial.

Although my main concern is the organization of science in relation to industry – what we today would think of in terms such as research-based innovation or applied science – my paper relates to historical developments predating the establishment of clear-cut distinctions...
in the Norwegian language between the equivalents of ‘pure/fundamental’ and ‘applied’ science or terms such as Research & Development. The situation was different in other languages. As shown by Clarke (2010), the use of distinctions such as ‘pure science’ and ‘fundamental research’ had implications for British science policy after 1916. The absence of such concepts and distinctions in late nineteenth and early twentieth century does not entail that there was no relation between the realms of science and industry (see Fox and Guagnini 1999, 1–4). The historical actors in interwar Norway would often use science and research interchangeably and specify by adding ‘industrial’ or ‘technical’ when describing scientific research intended to be useful. Such distinctions are of little consequence in this paper, apart from in a specific situation in the immediate post-World War II period, to which I will return.

The organization of science, especially related to industrial purposes, has been a recurring topic in Norwegian historiography (Andersen, Yttri, and Wiedswang 1997; Collett 1983; Gulbrandsen and Nerdrum 2009; Kvaal 1997; Sejersted 1993; Wicken 2009). My contribution will be to highlight the importance of entrepreneurship in shaping research institutions in Norway, a perspective that so far has received little attention. An entrepreneur may be broadly defined as someone who is ‘discovering, creating and exploiting opportunities to generate future goods and services, new economic activity, new organizations, etc.’ (Chiles, Bluedorn, and Gupta 2007). This definition may be criticized for being too wide, but at least it opens up for considering the entrepreneurial qualities of ventures beyond the narrowly commercial. Academic entrepreneurs are, according to Mody (2011, 9), often playing complex roles within wider research communities that are only partially oriented to commerce. Academic entrepreneurship as an activity is thus best understood, in a very general sense, as a set of processes where actors identify future opportunities, assemble resources to pursue those opportunities and seek to legitimize their ventures (See also the introduction to this special issue).

Through a re-reading and re-assessment of primary sources and the secondary literature on Norwegian scientific research organization, this paper will methodologically contribute to our understanding of academic entrepreneurship by studying a historical development in 1890s–1950s Norway. How did the actors involved in scientific research envision and identify future opportunities for organizing science on a national scale? What resources could be gathered for scientific research and how were they allocated in a period marked by economic recession, and how did the various ventures find legitimization? The long time span also allows the question of to what extent inter-generational learning occurred.

Norwegian scientists, industrialists, and state representatives considered several models for organizing research in ways that would maintain the interests of science, industry, and the public. To limit the scope of the investigation I will concentrate on three distinct forms of organization: the science academy with its funds; the central institute; and the research council. These are all instances of what Harwood (1994) calls ‘third-sector institutions,’ meaning institutions devoted to research that were neither exclusively industrial nor academic in character, with funding coming from both public and private sources and located – geographically as well as institutionally – separate from both firms and universities (for a different approach, see Kaiserfeld 2013). The three institutional forms also mark a periodization as well as a development, from the academy as an Enlightenment construction setup to ensure scientific autonomy, the central institute was an early twentieth century effort to create more co-operation between academia and industry, while the research council became a corporate
governance vehicle for the nation in the postwar pursuit of the ‘endless frontier’ of science (Bush 1945).

The paper proceeds as follows: First I will provide a brief introduction to the international development of institutions for industrial research, in order to contextualize the Norwegian development. Then follows the discussion of the first of the organizational modes, the Nansen fund established by Waldemar Christopher Brøgger, after which I clarify the background for the increase in public interest in science in Norway in the wake of World War I. Next, I proceed to the presentation and discussion of the second organizational mode, the visions of a central institute, and the ensuing economic crises that hindered the realization of many plans for science before I go on to analyze the impact of the World War II and the establishment of a Norwegian research council in 1946, also entailing the realization of the central institute.

The development of institutions for scientific research

In *The New Atlantis* (1627) Francis Bacon proposed what may be regarded as a first attempt at envisioning a science policy based on public support for a research institution. To obtain his goal of useful knowledge, Bacon outlined an integrated scheme with experimental facilities, equipment and trained personnel that he called ‘Solomon’s House’ (Etzkowitz 1993). Although Bacon’s idea indicates that public science policies are as old as modern science itself, it was only in the late nineteenth century that governments began to extend their involvement in science and technical matters beyond the modest contributions to universities, academies and science societies, and military research and development (Freeman and Soete 2004, 375).

Leading industrialized nations started setting up institutions that would link industrial development and public service to scientific research. These institutions were often a result of entrepreneurial initiatives by influential individuals or actor-groups.

In Germany, the Imperial Institute of Physics and Technology (*Physikalisch-Technische Reichsanstalt*), founded in 1887, became the first of several new institutions devoted to scientific research. The driving force behind this new form of institution was the industrialist and scientist Werner von Siemens, who saw the need for a research institute devoted to pure science, while also catering for the industrial demand for technological research and development (Walker [1997] 2003). This form of institutional setup, independent of both universities and industries, inspired similar arrangements in Germany and elsewhere. The National Physical Laboratory in Great Britain was established in 1900, after a campaign by British physicists fueled by public fear of the German competition (Moseley 1978). A year later, the U.S. Congress established the National Bureau of Standards, encouraged by leading American physicists referring to the German Reichsanstalt as a success (Cahan [1989] 2004). Most notable of these pre-World War I institutions was the Kaiser Wilhelm Society (*Kaiser-Wilhelm-Gesellschaft*, established in 1911), which grew into a larger system of institutes devoted to fundamental research (Walker [1997] 2003).

The World War I would later spawn several new research institutions due to the importance of science-based industry for warfare and the general rise in status of scientific research. The British Department of Scientific and Industrial Research (DSIR) was founded in 1916, the same year as the National Academy of Sciences established the National Research Council in the U.S., primarily based on private funding and with only loose connections to the federal
government (Hull 1999; Kaiserfeld 2013; Kevles 1968). In the U.S., private foundations played an important role in science funding, with the Rockefeller and Carnegie foundations welding huge power through their research programs. In Germany, however, money was so scarce that a Public Emergency Foundation for German Science (*Notgemeinschaft der deutschen Wissenschaft*, 1920) was setup, with peer review as a new instrument for discerning between competing schemes for science (Walker, [1997] 2003).

Smaller nations would follow similar paths of development, as science policy trends tend to transfer easily across national borders, although appropriated to meet specific needs (Elzinga 2012; Ruivo 1994). While a latecomer both in terms of industrialization and academic institutionalization, Norway also experienced the development of a national system for industrial research marked by the translation of transnational trends into a national setting. Because of the severity of the economic crises in the 1920s and 1930s, however, Norway had to wait until after World War II before a ‘national innovation system’ could evolve (Gulbrandsen and Nerdrum 2009; Wicken 2009). Already in the late nineteenth century, however, there were pioneering efforts seeking to bring Norway up to standards in what was always already a competition in scientific progress between nation states.

In the 1890s, Norway was an emerging nation experiencing economic growth through the increased industrialization of traditional sectors such as wood and metal processing and new technology led chemical industry. Also, substantial political change was underway, especially concerning the extension and strengthening of democratic institutions, but also in terms of a growing nationalistic sentiment concerned with extending Norwegian international interests and breaking the union with Sweden. The two patriotic impulses of independence and international engagement overlapped in the much-publicized polar expeditions led by Norwegian explorers at the time.

**Brøgger and the Nansen fund**

On the night of 20 August 1896 a telegram created commotion throughout Norway. ‘All well,’ it shortly stated. It was from Fridtjof Nansen and his crew on board the ship *Fram*; they had survived their expedition to the Arctic. The news of Nansen’s safe return to the mainland was met with a resounding, patriotic roar in all Norwegian cities. Nansen and his brave men embodied a national spirit of adventure that could easily be fueled into the political agenda of seeking independence from Sweden.

Professor Waldemar Christopher Brøgger was among the first to congratulate Nansen on his safe return. Brøgger had known Nansen since they met in Stockholm in 1887 during Brøgger’s spell as professor of Geology and mineralogy at the newly established *Stockholms högskola*. Brøgger was an academic entrepreneur, always envisioning ways to improve conditions for science in the service of the nation. How could Nansen’s reputation become a resource?

Brøgger had already during his stay in Stockholm demonstrated his entrepreneurial abilities. There he had managed to build up a well-working research institute, secure the support from leading industrialists and state representatives, while advancing his scientific career internationally. When Brøgger returned to the university in Kristiania (today’s Oslo) in 1891 at the age of 40, he was already a leading Norwegian scientist and a prominent member of the international geological science community (Hestmark 1999, 303–335). As Brøgger was a self-conscious national strategist his peers called him ‘the Bismarck of Norwegian science’
(Hestmark 1999, 15). He was for a long period the nation’s strongest proponent of science, with numerous public addresses and newspaper articles to his name. Few, if any, other Norwegian academics shared Brøgger’s interest and ability in forging a public science policy. As a ‘public scientist’ (Hull 1999; Turner 1980) he was prepared to serve the nation, provided he would retain full autonomy as a scientist. He stood almost alone in his public struggle for science in Norway until the end of the World War I (Collett 1983, 50). Then others would take up his lead.

Brøgger’s immediate reaction to the news of Nansen’s return in 1896 was to propose a fund for science named after the famous explorer. Brøgger was well connected to wealthy and influential people within shipping, industry, and academia, and eventually managed to secure enough support to establish the Fridtjof Nansen Fund for the Advancement of Science already before Christmas 1896 (Brøgger 1916, 15). The Nansen fund became one of the most important sources for science in Norway in the ensuing years. Not that there was much competition: The state of public as well as private support for science remained modest in Norway throughout the first four decades of the twentieth century.

Brøgger had many motives for wanting to establish a fund for science in 1896. Norwegian science was in a state of crisis in the years after the parliamentary revolution of 1884 in Norway. Scientists were seen as part of the ruling elite, or even as the educators of the civil servants ruling Norway. The legitimacy of science had to be reestablished. By strengthening scientific research through a fund Brøgger hoped to prove that scientists could make important contributions to progress and prosperity. In a newspaper article in 1904 he ensured that any nation unable to keep up on the field of science would ‘inevitably be outdistanced in a material sense.’ (Brøgger 1904).

For Brøgger it was first of all paramount to prevent Norway from falling behind in the race for scientific progress, and that a promising generation of able men of science found ways to pursue their ideas. It was also important for Brøgger to make Norwegian scientists less directly dependent on the state and the impoverished university (Collett 1983, 34). Science in direct service of industry, let alone the more contemporary notion of applied research, was alien to Brøgger. His overarching ambition was to turn the Science Society in Kristiania into a full-fledged national academy that could ensure Norway’s most prominent scientists time and resources for fundamental research. For this he needed funds.

Brøgger did not come up with his ideas ex nihilo; he was deeply influenced by his period in Sweden, where science had far better conditions than in Norway (a fact, as well as a rhetorical point frequently used by proponents of science in Norway). Swedish capitalists had been relatively eager to support science, and fund-raising campaigns there had resulted in important contributions to science. Besides, Sweden had an influential science academy with tenured positions that oversaw the distribution of funds. This combination of private funds and an academy was something Brøgger thought would be a solution to the crisis in Norwegian science. With the help of Nansen’s name Brøgger hoped more money could be collected for science. Nansen had a strong identity also as a scientist, and was of course eager to support science, and was willing to use his name in Brøgger’s service.

At the time there was a veritable race to capitalize on Nansen’s name. Brøgger had to compete against business ventures like ‘Dr. Nansen cigars’ and ‘Nansen’s polar beer’ (Hestmark 2000, 18). The professor also had to use a wide variety of entrepreneurial skills to promote his fund. He setup a fund raising committee balancing political interests from left to right, religious convictions and scientific schools of thought. He used his access to the national
newspaper *Verdens Gang* to publish eulogies to Nansen and his crew, and he benefitted from his network of rich men interested in science, attracting a substantial amount even from a Swedish benefactor, Alfred Nobel. In 1918, Brøgger and his allies also finally managed to convince the Norwegian Parliament to grant substantial support for the fund through a share of the revenue from the National Lottery, and was thus an embryonic case of public-private collaboration in science funding in Norway (Collett 1983, 258). Yet, Brøgger’s plan held no openings for others than scientists when it came to deciding over how these funds should be spent; industrialists, politicians, and civil servants were only interesting as generous patrons for science.

Although Brøgger obviously enjoyed basking in the public glory of his own achievements, it was hardly his own research or business enterprises he sought strengthening. Brøgger was a nation builder. He was convinced that Norway could become an ‘Athens of the North’ (Hestmark 1999, 15), provided that the young nation got around to building strong institutions and to encourage talented youth to pursue a career in science. It was thus a hallmark for Brøgger when in 1924 the Science Society was turned into a national academy for science (a name change that was of symbolic rather than substantial importance at the time). He strongly believed in science, and insisted nothing would benefit humankind more than the pursuit of scientific research. Yet, in particular he advocated science as a fundament for progress, growth and industry in a national perspective.

The Nansen fund for the advancement of science was established in 1897, and Brøgger remained chairman of the board until 1937, when he retired due to his age. He died in 1940. The Nansen fund was, as Brøgger’s biographer Geir Hestmark (2000, 25) has pointed out, ‘an opportunistic masterpiece.’ It was also an entrepreneurial accomplishment. Brøgger had seized the moment, captured the spirit of heroic nationalism tied to the polar expeditions, and managed to raise support for his cause while keeping at bay the volatile forces of politics. The Nansen fund remained an important source of money and academic legitimacy in Norwegian science even long after Brøgger had passed (and is still in operation). The fund also provided the Norwegian Academy of Science with enough resources so that it in 1933 rightfully could claim to be a ‘central organ in Norwegian science’ (Amundsen 1960, 286, 287). Still, neither the Nansen fund nor the Norwegian academy became as significant as Brøgger had hoped for when he started gleaning ideas from abroad.

**Scientific opportunities after World War I**

The World War I experience created a new emergency in the discussions of science policy, also in Norway. Brøgger now became preoccupied with how the belligerent nations organized themselves to take advantage of science. In the Kristiania Science Society, he frequently referred to new initiatives and organizations he saw abroad that provided him with fresh arguments and models for building research capacity. Organizations under development in France, Germany, and Britain at the time provided entrepreneurs such as Brøgger with a range of organizational options they could consider. Yet, it was the initiatives taken by George Ellery Hale and Robert A. Millikan for establishing a National Research Council in the U.S. that caught Brøgger’s attention (Collett 1983, 48, 49; Kevles 1968). This form of institution was ideal, Brøgger felt, because it was controlled by the National Academy of Sciences that would ensure the support of fundamental, long-term research in every field of inquiry, with the financial and moral backing from federal government, as well as private funds. He
admired the donations made by Rockefeller and Carnegie, and preferred private endowments over the public funding model he saw in Germany (Collett 1983, 45).

In 1916, shortly after Hale had presented his ideas for the National Academy of Sciences, Brøgger, and his growing number of allies within the Science Society in Norway’s capital, proposed a similar design for the Norwegian government (Collett 1983, 59). The response from the authorities was rather welcoming, but the discussions about a new science institution drowned in the precarious war situation, with dramatic shortages of supplies. This heralded a myriad of initiatives to establish research collaborations between the state, industry and science in Norway (Gulbrandsen and Nerdrum 2009). It became paramount to solve the raw material shortage crisis, and the industrialists and the politicians primarily wanted scientists to contribute with their expertise in finding short-term solutions to these problems. Brøgger passed the assignment on to his younger colleague Victor Moritz Goldschmidt, a professor in mineralogy with ties to industrial firms, who was able to come up with plans for organizing industrial research pertaining to Norway’s raw material resources (Collett 1983, 66). One of the important issues that came out of these initiatives was the question of building industrial research institutes and laboratories. Most Norwegian companies lacked the resources for setting up their own R&D departments, and many industrialists were skeptical to the use of expenditure on science and technological innovation, unless it was specifically targeted toward solving industry-related problems (Gulbrandsen and Nerdrum 2009). Put shortly, many Norwegian industrialists and politicians were eager to establish a set of institutes with laboratories for industrial, applied research targeted at some specific areas of Norwegian industry (Collett 1983, 91–94).

Within academia these plans were met with resistance. Like Brøgger, many influential Norwegian scientists believed science best could benefit society and industry if it was autonomous and oriented toward basic research (Gulbrandsen and Nerdrum 2009). The establishment of ‘third-sector institutions’ residing outside of academia would challenge the necessary autonomy of science. It is, however, notoriously difficult to draw the boundaries between the inside and outside the realm of science. Also, science may have many different ‘insides.’ In Norway, this became apparent in 1910 with the inauguration of a higher engineering education in Trondheim, the Norwegian Institute of Technology – Norges Tekniske Høiskole (NTH) – that changed the national academic landscape (Brandt 2014).

For decades Norwegian industrialists, engineers, politicians, and scientists had longed for, and bickered over, a higher engineering education institution. The opening of the NTH in the quiet town of Trondheim had meant that Norway now had a new academic community, with professors in architecture, physics, chemistry, and various branches of engineering. Although most of them had been engaged in industrial and other business activities, they had not managed to setup a joint strategy for industrial research. While historically a fertile ground for academic entrepreneurship, engineering education institutions were also marked by tensions between diverging values and cultures in business and science (Carlson 1988). With the emerging discussions of industrial research institutes from 1919 on, the leading professors at the NTH felt their position was under threat. It became part of a lasting rivalry between Trondheim and Norway’s capital (Kristiania, from 1925 on named Oslo) over the leadership within industrial research. In the interwar years, the Central Committee for Scientific Cooperation for the Advancement of Industry, established in 1919, became a battleground for these struggles. The Central Committee was an initiative in the spirit of Brøgger,
with representatives from ‘autonomous public institutions’ pertaining to science, which excluded both private corporations and the government (Collett 1983, 103–108).

**The Mellon Institute as model**

One of the leading figures at the NTH was Sem Sæland, a physics professor with limited formal scientific qualifications. Of the 12, first appointed professors only six held a doctoral degree, and Sæland was not one of them (Wittje 2003, 48). Yet, Sæland had many other qualities that made him stand out as a natural and popular leader, as a young “founding father” for a young engineering school. He served as NTH’s first Rector from 1910 to 1914, a period marked by growth and prosperity both for the engineering institution and for Norway’s economy. Sæland was also member of the Norwegian parliament from 1916 to 1918, where he gained valuable experience about the political game. As an experimental physicist trained in Oslo and Heidelberg he was more of a scientist than an engineer, and his contributions to industrial development in Norway came more as a result of his efforts at institution building than from his direct engagement with industry (Brandt and Nordal 2010).

After his period as Rector at NTH Sæland’s main concern was to strengthen scientific research in Trondheim through building new laboratories and institutes. Sæland was strengthened in his vision for investing in scientific infrastructure after a study trip to England and the U.S. in 1918. He came home with ideas for an independent industrial research institute after the model of the Mellon Institute in Pittsburgh. Founded in 1913, the Mellon institute was, according to Servos (1994), the result of the ‘conjunction of a chemist’s dreams, financiers’ money, and an educator’s ambition.’ The chemist was Robert Kennedy Duncan, who, after a study trip to Europe, had been inspired by how European industrialists benefitted from collaboration with scientists (Weidlein and Hamor 1936, 21, 22. For a further discussion of the Mellon Institute, see the contribution by Ellan Spero in this issue).

Duncan’s idea for an academic entrepreneurial venture was to setup an institution around the concept of industrial fellowships. Through the fellowship scheme American companies seeking solutions to specific problems could finance the tenure of a research fellow. In return, the company would retain exclusive rights to the scientific results, while the university’s chemistry department providing the fellow would get some extra hours’ worth of teaching (Duncan 1909). In this way, promising researchers could be lured out of their laboratories to collaborate with industrial firms that in turn had money and specific problems to be solved. Duncan, after having made a trial of his fellowship arrangements at the University of Kansas, eventually found the necessary resources for his venture in Pittsburgh, with the Mellon brothers as investors, and the University of Pittsburgh as provider of qualified researchers.

Sæland was enthusiastic about the Mellon model, and in 1920 he helped bring it into the Norwegian debate on the organization of industrial research in the wake of World War I. Sæland was not the first to point out the Mellon institute as a potential model for Norway. Already in 1918 his colleague at the NTH, professor in mining engineering and respected industrialist Alfred Getz, had described the Mellon institute as one of several international organizations that could serve as an example for Norway (Getz 1918). Sæland, however, provided a more elaborated discussion of how an industrial fellowship system based on Duncan’s ideas could be institutionalized in a Norwegian setting.
Sæland was arguing against the proposals of establishing industry-specific laboratories or institutes that were prevailing in parts of Norwegian industry at the time. Rather, Sæland argued, scientific research for industrial purposes had to be conducted under the supervision of leading scientists. The autonomy of science was thus as important to Sæland as it was for Brøgger in legitimizing academic entrepreneurship ventures. Further, in order to provide a truly scientific environment for industrial research, the institution would benefit from being closely linked to the NTH in Trondheim, which in turn would be able to grow by having access to industrial partners, financial support, research facilities, and scientific talent (Sæland 1920). Sæland thus realized that the resources necessary for this venture were not only a question of money, but also of providing talented scientists and engineers with state-of-the-art research laboratories to solve industrial problems.

Like many of his professor colleagues at the NTH, Sæland was concerned about the future development of the new engineering education institution in Trondheim, where important infrastructure in terms of laboratories and buildings were missing due to the public parsimony (e.g. Heje 1920). A central institute in Trondheim along the lines of the Mellon system could help concentrate the nation’s sparse resources in terms of laboratories, manpower and funding, and thus buttress Trondheim as the hub of Norwegian industrial research and development (Sæland 1920).

Campaigning for Trondheim was, however, not the only motive legitimizing Sæland’s vision of a central institute. The physics professor was convinced that industrial research, like any other kind of research, had to be directed, supervised and conducted by scientists with freedom to pursue unexpected problems and explore new avenues. Such an approach to industrial research and development departed quite drastically from the plans for industry-specific laboratories located in the vicinity of the manufacturing plants, as the Norwegian canning industry in the Stavanger area had envisaged. Sæland referred to the director of the Mellon institute, who he claimed had warned against the dangers of introducing ‘the factory spirit’ into the institute’s research. Rather, Sæland insisted, the ‘research drive’ or the ‘scientific spirit’ ought to be guiding the work at such an institution (Sæland 1920).

What Sæland had not noticed, or failed to report, were the growing difficulties facing the Mellon institute. Lauritz Jenssen Dorenfeldt, a Norwegian engineer visiting the Mellon institute in the same period, had found that the contact between the institute and the university was far from as close as Sæland claimed, and that the professor clearly was ‘on the wrong track’ when he suggested a scheme like this for the NTH in Trondheim. In Dorenfeldt’s experience, the Mellon institute hardly contributed to the university, and he advised against spending public money on an organization that only catered for the private interests of individual industrialists unwilling to share the results of their investments in science (Dorenfeldt 1921). Dorenfeldt was no neutral observer; since 1919 he had been chairing a state department committee setup to investigate the potential for a paper industry research institute, which alongside the canning industry institute, were the only industrial research cooperatives established in interwar Norway (Gulbrandsen and Nerdrum 2009; Kaldal 2009). As noted by Servos (1994), the Mellon Institute model had indeed run into difficulties due to disagreements between university faculty and the institute. The university, especially the Department of Chemistry, was hostile to the institute because it provided next to nothing in return for the benefits of having access to the best students. They paid no rent for being placed on campus, and shared none of their revenues.
The objections voiced by Dorenfeldt did not prevent Sæland from using the idea of an institute co-located with the academic institution in Trondheim. Still, there was a conspicuous discrepancy between the rosy image of academic freedom painted by Sæland and the sobering account of the Mellon Institute presented by Dorenfeldt for the Norwegian parliament.

The discussions about industrial research took place in a period marked by political and economic turmoil in Norway. The immediate postwar years of 1918 and 1919 were prosperous and the surplus of money also found their way into several new funds, private, and public, for scientific research (Collett and Skoie 1981, 97). Then the Norwegian economy went through three major setbacks in the period between the two world wars. The crisis following the World War I was due to the international postwar depression, but it hit harder in Norway due to a monetary policy bent at restoring the Norwegian currency to the pre-war gold standard. From 1920 to 1921 the GDP fell by 9.6% (Central Bureau of Statistics of Norway 1969, 350). The sustained deflationary policy resulted in a new recession in the mid-1920s, and in the early 1930s the international Great Depression also hit Norway. Although there were glimpses of upturns, the decades between the world wars were marked by high unemployment rates, low rates of investment, a large amount of bankruptcies, and stagnation in public expenditure (Grytten 2002).

For a newly founded academic institution like the NTH the economic situation in the 1920s was stifling. The graduates had a hard time finding jobs, and the number of enrolled students plunged. Investments in new buildings and laboratory facilities dried up almost completely, and the institution struggled to fill vacant positions. Also, the decline in industrial development had severe repercussions, as there were fewer commissions and R&D projects. In this situation, more than becoming a lever for modernization, industrial, and economic development, the NTH by and large became an insulated academic institution (Hanisch and Lange 1985, 89). The professors felt that the pressure from the industry to establish independent research institutes outside of Trondheim was threatening to undermine the institution.

Although the politicians generally welcomed the proposals put forward by Sæland and the NTH, they ended up with supporting the alternative ideas of industrial laboratories outside of Trondheim. Sæland, it turned out, was unable to muster the necessary resources for his entrepreneurial vision of a central laboratory in Trondheim. Yet, although the central institute plans were temporarily postponed, mainly due to the precarious economic situation after 1921, Sæland’s ability to seek out important international developments and appropriate them into a Norwegian context demonstrates his position as one of the foremost academic entrepreneurs in early twentieth century Norway (Brandt and Nordal 2010, 155).

While Sæland, with the support of his colleagues, managed to secure the establishment of a physics laboratory amidst Norway’s enduring economic crisis before he moved on to execute the grand scheme for a new campus at the University of Oslo (Fure 2011, 109), his ideas for a central industrial research institute did not come to fruition during his lifetime. He died in 1940, the same year as Brøgger. Sæland’s vision of a research institute modeled on the Mellon Institute, located on the campus of a higher education institution, was however sustained. It became one of the main points of contention in the immediate postwar.
Fredrik Vogt and the research council

The idea of a research council had surfaced several times during the interwar years in Norway, after Brøgger had brought it up in 1916, with reference to the National Research Council in the U.S. (Kvaal 1997, 75). A first attempt had been a follow-up of the growing governmental interest in organizing science after World War I, but this effort dwindled away during the 1920s. In 1935, the Norwegian state nourished a hope that scientific industrial research could ensure job creation in a time of severe unemployment. Again, Sem Sæland had been instrumental, he had worked behind the scenes to make sure this new initiative would be on the terms of science, not the industry (Kvaal 1997, 180). His position was here as adamant as Brøgger’s. The result was The Council for Technical Industrial Research, established as part of the state administration, and with a mandate to make assessments and provide policy recommendations for public funding of science. It ended up as many of the other Norwegian science policy initiatives from the 1920s and 30s: with the best of intentions, but without necessary resources and support. After the World War II this research council was sidetracked, and new impulses dominated, with new people to support them.

On October 12, 1945 a group of 10 men met in Oslo at the Ministry of Trade. They were the members of the newly appointed Committee for the organization of technical research. They were scientists, industrialists, and state representatives. Fredrik Vogt, Rector of the NTH, had already been selected as the chair of the research committee (Kvaal 1997, 319). Vogt had just returned to Norway after two years in British exile during the war and was now in charge of rebuilding the engineering education at the NTH in Trondheim. During his years in the U.K. he had been chairing another committee, the so-called Norwegian industry committee responsible for gathering as much information as possible on British and American military-industrial build up (Brandt and Nordal 2010, 228).

The lessons learned by the industry committee during the war were profound for Norwegian decision-makers: Industrial capacity could be built fast and strong enough to win the war if only science, state and industry collaborated. Further, science-based industrial development was profoundly a team effort, and could therefore not be left to individual inventors or scientists. Teams of engineers, scientists, and industrialists working together – often supervised by military interests – were behind many of the efforts observed by the industrial committee. This committee was therefore also a joint effort, made up of members from industry, the state and science. The reports from the wartime industrial committee gave the Norwegian politicians in exile clear arguments for the necessity of a large-scale, systematic approach to science-based technological and industrial development. From another important group, the Norwegian Defense Supreme Command’s technical committee (FOTU), came similar recommendations. Norway needed to establish a research facility for developing military defense technology as a national security measure. These recommendations resulted in the expedient creation of the Norwegian Defense Research Establishment (FFI) immediately after the war in 1945 (Forland 1988).

The committee for the organization of technical research, chaired by Vogt from October 1945 on, had to deal with the complex issue of creating an institutional framework for industrial and scientific research in Norway. The two terms ‘industrial research’ and ‘technical research’ were used interchangeably in relation to the committee’s work, which hinted at a small unresolved difference between the involved actors regarding the focus and scope of the entrepreneurial venture: To what extent should the organization of research be directly
targeted toward industrial needs? While the negotiations in the committee were setup as a balancing act between the interests of the state, industry and science, the mandate given by the government was to ensure that the benefits for industry from scientific research should be the main priority. While the interwar public interest in finding ways to organize research had been motivated by pressing, short-term needs for new raw materials and job creation, the postwar discourse had a broader scope: ‘In order for our nation to be able to participate in the strong industrial competition one currently is facing,’ the government mandate stated, ‘it is of the utmost importance to ensure an increased and more targeted work in the field of technical research.’ Given the small size of the Norwegian scientific community – with less than 70 professorial chairs within science and engineering at three separate academic institutions and a public expenditure on technical research of 13.5 million NOK – the committee was facing a daunting task in fulfilling this mandate. 4

The committee was considering several models for organizing research for industry. This time, the entrepreneurial efforts were not based on a specific model from abroad, but rather on an assessment of a broad range of international options. Vogt was especially focusing on the institutions for science in Scandinavia, and on the British Department of Scientific and Industrial Research that he knew well from his time in London during the war. The representatives from Norwegian industry, most notably Alf Ihlen, were more preoccupied with ensuring that the public effort this time would be of a sufficient scale. Already in the first committee meeting Ihlen suggested the amount of 30 million NOK as a suitable level of funding for the new organization, a number way beyond anything previously invested in Norwegian science funding. 5 Ihlen could not find immediate support for his proposal, but it served as a demonstration of a sense of urgency among leading Norwegian industrialists for establishing a large-scale national science operation.

While the question of funding remained unresolved, the issue of finding a suitable organizational model for scientific research dominated the discussion among the committee members. Three alternatives were put on the table; an academy for science; a research council within the department of trade; or an independent organization governed by a research council with board members from science, the state and industry. The academy model was similar to the Swedish Engineering Science Academy. Also, Brøgger, as shown above, had been a strong proponent of the academy model, and his ideals of scientific autonomy in the governance of research funding had several proponents. After the war, however, the academy model was considered to ‘lack the necessary public status for a national coordinating organ of science.’ (Vogt 1946).

The other alternative discussed was a research council organized as part of the Department of Trade. The deputy chair of the Vogt committee, Einar Slåtto, representing the department, backed this proposal. Slåtto had been instrumental in forging the mandate for the committee and was well connected with leading figures in the dominating Labor Party, and thus for him it was logical to propose an organizational solution as part of the state apparatus. The leading industrialists in the committee were however opposed to the idea of placing a research council within a state department. Instead, the committee began discussing another option, a research council of a ‘freer kind,’ placed outside the departments of the state, and funded jointly by public and private money. 6 This was an innovative model for scientific research in Norway, and it was proposed by Alf Ihlen. He was wary of any arrangement that smacked of governmental control, as he assumed that the industrialists would be reluctant to participate. Instead, he wanted to organize the research council more like a private
corporation, with an executive director supervised by a board, and with the authority to forge national strategies for R&D. Committee chairman Vogt, who had thought the state department model proposed by Slåtto to be more in line with similar arrangements he admired, like the British DSIR, was initially reluctant to back the idea of an independent research council. What eventually made Vogt budge is not clear, but may have been related to the funding of the new council: If the industry wanted an independent research council they would have to pay for it. Ihlen conceded, and promised he could rally financial support from the Norwegian industry.

The Vogt committee's final report then ended up with recommending the establishment of an independent research council for science and technology. It started out with painting a picture of how the recent war had demonstrated the decisive effect of coordinated research and technological development. It would be just as decisive in the ensuing peaceful competition between nations. ‘Those not up to standard will inevitably fall into economic dependency,’ warned the committee, echoing the voice of Brøgger from 42 years earlier (Vogt 1946, 5). Every nation of ‘culture’ was energetically pursuing scientific research. It was as if technological modernization through scientific research was the ultimate ‘measure of men,’ to paraphrase Michael Adas (1989).

The Vogt committee spent only a few months preparing their report, and in July 1946 the Norwegian Parliament unanimously voted for the establishment of a Royal Norwegian Council for Scientific and Industrial Research – the official translation into English, suggesting a closer tie to industry than the Norwegian name did (Norges Teknisk Naturvitenskapelige Forskningsråd, NTNF). In October 1946 the 24 members of the council, representing industry, the state, and the science institutions, were appointed. Alf Ihlen, a Norwegian engineer educated in the U.S. and owner of a mechanical workshop, became the first chairman, ensuring continuity from the Vogt committee along with several of the other members. The balance between representatives from science, industry, and the state were also maintained. Although this solution was meant to bring about the autonomy of the research council, and ensure democratic representation in a corporative fashion typical of Norwegian governance at the time, the research council was very much in the hands of a small, tightly knit group of men from the elite.

While Norwegian scholars previously have pointed to the importance of various actor groups in the establishment and development of the research council system in Norway, they hardly recognize the entrepreneurial efforts that paved the way for the creation of this powerful scientific research organization (Collett and Skoie 1981, 104; Kvaal 1997, 713, 714; Nygaard 2014, 115, 116; Slagstad 1998, 350). There is a clear line from Brogger, who almost single-handedly raised the issue of organizing science in Norway and through his efforts was making scientific research a matter of public importance, via the like-minded Sæland who set a course for research institute building, to the work of Vogt, Slåtto and Ihlen. Although Ihlen eventually sidetracked the representatives from science and the state in his quest for an industry-minded research council, they all collaborated on envisioning a possible future for Norwegian scientific and technological research. The Vogt committee report is a hallmark of collective academic entrepreneurship in that it created an innovative model for public-private collaboration on industrial research. While unique due to the distinct nature of any national setting, similar acts of entrepreneurship aimed at building institutions for science took place in other Scandinavian countries, resulting in the establishment of research councils, although with important differences due to the more prominent role of academies,
private funds and the greater number of large firms investing in R&D (for Denmark, see Knudsen [2005], for Sweden Pettersson [2012]).

The success of the research council model was by no means a given, even in the postwar drive for modernization through science. The German Research Council, established in 1949, became a short-lived failure. According to Carson and Gubser (2002), it was based on ‘generic, ill-informed comparisons with British and American models’ with inadequate concern for the specific challenges facing science in the new Federal Republic. The German Research Council was working under the conviction that research policy should be decided by scientists, but in the end it ‘failed to persuade industrialists or economics officials of this.’ (Carson and Gubser 2002).

In Norway, the issue of creating a central research institute was eventually solved as a result of the new research council, albeit not without conflict. Again, the main point of contention were the consequences for NTH in Trondheim. The visions of a central institute catering for the R&D needs of small and mid-sized Norwegian firms had lived on throughout the 1930s and 40s. In 1947 the question was brought up again within the NTNF council, at the initiative of the industry. The research council drew up a plan for funding and organizing an institute, with explicit reference to a ‘Mellon-department’ (Kvaal 1999, 6). Early on it became clear that Sæland’s vision of locating the institute in the vicinity of NTH would not be fulfilled.

The NTH was in a poor state after the German occupation, overcrowded with students returning to finish their studies, with several vacant professor chairs and a dilapidated campus containing laboratories no longer up to date. Vogt, although no longer at the NTH, fought vehemently against the plans for an institute outside of Trondheim. It was of little use, as the industrialists, among them Ihlen, had lost their patience with NTH. Therefore, the Research Council instead decided to establish a research institute on the campus of the University in Oslo, with the idea of taking up research problems common for many industrial companies in Norway. The professors at the NTH strongly opposed this idea. They had so far not been in the habit of working collectively toward a common goal, but the situation called for entrepreneurship: They rallied support and organized a local fund-raising for an alternative research institute in Trondheim. The result was the industrial research foundation SINTEF, established in 1950. The institutional symbiosis of SINTEF and NTH became a highly successful institutional innovation, an act of entrepreneurship born out of the pressure of competition. SINTEF soon became Norwegian industry’s favorite partner for applied research, while also engaging in long-term fundamental science. Sæland’s vision was finally brought to fruition (Brandt and Nordal 2010, 249–251).

Conclusion

The actors involved in the creation of institutions for scientific research in Norway during the first half of the twentieth century used a variety of strategies and practices that are recognizable as entrepreneurial. For analytical purposes they may be categorized as the ability to identify future opportunities, gather the necessary resources to pursue those opportunities and find ways to give legitimacy to the entrepreneurial venture.

In order to identify the future societal opportunities attainable through the organization of science, the Norwegian academic entrepreneurs to a large extent relied on transferring concepts, models and ideas from abroad. The circulation of ideas, people, and scientific
artifacts such as books, journals, specimens, and instruments are a well-known feature of modern science, dating back to at least the seventeenth and eighteenth centuries and the ‘Republic of letters’ with its cosmopolitan ideals and universalist ambitions for scholarship. Albeit vitiated by narrow-minded views of science that frowned upon experiments and fieldwork, the Republic of letters ensured networks of knowledge production that allowed for rapid and far-flung spread of information (Mayhew 2005). While the nationalist fervor of the nineteenth and early twentieth century bridled cosmopolitan ideals, the established transnational, border-crossing practices of science meant that drawing ideas, designs and models for organizing scientific research was ingrained in the academic entrepreneurial mindset. In late nineteenth and early twentieth century Norway, the transfer of models across borders was key to the academic entrepreneurial processes of envisioning as well as creating a future organization of research.

Brøgger was initially looking to other Scandinavian countries for inspiration for his vision of a large national science fund. Eventually he was however more intrigued by the initiatives taken in the U.S. to create a national institution to support scientific research based on a combination of private endowments and federal governmental support, yet under the auspices of the National Academy of Sciences. In terms of financial resources for the Nansen fund, Brøgger was initially relying on private benefactors to step up, but when this proved insufficient public money through the National Lottery saved the venture (Collett and Skoie 1981, 258). Money was, however, not the only resource Brøgger made use of; unlike most of his peers in academia, Brøgger was able to create publicity for his schemes through media; he also had access to the nation’s political and financial elites; as a prominent member of a first generation of modern scientists he could engage his younger colleagues in his struggle for achieving a broad scientific program in Norway.

Sæland would follow Brøgger’s lead in envisioning a model for Norwegian scientific research with scientists at the helm. Sæland also looked to the U.S. for inspiration, with the Mellon Institute as a model he deemed suitable to cater for a national effort for science-based industry, while at the same time strengthening the newly founded Norwegian Institute of Technology in Trondheim. Sæland had access to some of the same resources as Brøgger had, but the economic recessions between 1921 and 1936 curtailed his plans. In the new situation after 1945, Vogt and his fellow members of the committee for the organization of technical research made a broader assessment of international models, but with more explicit intention of using these as raw material in the assembly of an institutional arrangement suitable for the Norwegian postwar situation, which entailed a substantial scaling up of the public financial engagement in science.

While the resulting institutions for research were innovative, even unique, they should not be understood in terms of exceptionalism. Instead of flagging the historical development in Norway as a Sonderweg (Sejersted 1993), it would be more worthwhile to consider the entrepreneurial processes of creating institutions for research as ‘junctures where the nation-state’s permeability might be brought into view’ (Rodgers 1998, 2). Especially for small countries like Norway in the allegedly ‘periphery of science’ (for a discussion, see Gavroglu et al. 2008) the transnational transfer of knowledge, experience and schemes concerning how to organize industrial research was a concomitant of the entrepreneurial practice.

One of the most striking effects of adopting a long time span is the demonstration of entrepreneurial collaboration across generations. The paper demonstrates that visions, ideas and specific organizational designs were transmitted through time periods as a form of
cultural memory residing within the scientific community. While beyond the scope of this paper, it is at least worth pointing out that there seems to be an untapped potential for combining historical studies of organizational change with the theoretical frameworks of memory studies (Booth and Rowlinson 2006). The ideas and processes put forward by Brøgger and Sæland in the early interwar years transmuted into a full-fledged system of research institutions in the postwar period. What was the ‘learning outcome’ from this inter-generational transfer of visions, ideas, and solutions for organizing resources for science? Most prominent was the realization of the importance of involving all stakeholders in industrial research in the process of creating a viable organizational framework that would balance the integrity of science with its relevance for society. The interwar visions of scientists in control of substantial resources were, especially in the field of industrial research, replaced by a postwar corporate model. In other words, bringing people together was an indispensable resource for the entrepreneurial venture.

Bringing stakeholders together to form communities around the endeavors of scientific research was thus crucial, and it involved overcoming differences of interest that were ingrained in science. One of the key issues here are related to the changing power-relations between the stakeholders of science. This has to do with a recurring issue in the history of science policy; in what Elzinga (1990) has described as the ‘triangular drama’ of science policy, involving science, industry, and the state, the question of ‘epistemic drift’ looms large. That is, to what extent should forces outside of science be allowed to influence the definition of scientific problems, the solutions of these problems and the value of research results (Elzinga 1984; Kaiserfeld 2013). The organization of resources for science on a national level was a result of the negotiations within this triangular field of interests. Through the establishment of the Royal Norwegian Council for Scientific and Industrial Research in 1946 as an independent entity, a form of ‘trading zone’ (Galison 1997, 783, 784) was created where various actors from industry, science, and public administration could interact. The ‘triangular drama’ was by no means put to an end by this, but it was given a well-defined arena held up by institutional ‘rules of the game.’

When it comes to understanding the legitimization of the various entrepreneurial ventures, a salient feature of the creation of institutions that becomes apparent from these Norwegian examples is the importance of historical and geographical context. Nations, nationalism, and international competition were important determinants of the entrepreneurial visions in various ways. Baumol (1990), with a keen eye for the importance of historical perspectives in the understanding of entrepreneurship, reminds us that entrepreneurial activities sometimes are ‘unproductive,’ even ‘destructive.’ While Brøgger’s staging of the competition with Sweden may have had productive effects, enabling the capitalization of the scientist-explorer Nansen as a resource, the national perspective also proved unproductive, especially in the interwar years, simply because it impeded the flow of talent, material resources, and ideas that is so important for science.

Like in most other countries, the prominent scientists in Norway were nation-builders, and this paper demonstrates that their efforts at organizing resources for science also were state-building contributions. Yet, it is worth noting that these efforts were not always of a unifying nature, consolidating a national research community. At times their initiatives were just as much aimed at local interests, as part of strategies for strengthening their own scientific disciplines or as measures in the inter- or even intra-university competition. In other words, academic institutional entrepreneurs have crossing identities that enable them to
be pivotal in both concerting and conflicting schemes for organizing resources for research. In the specific case of industrial research in Norway in the first half of the twentieth century, the cagey relationship between Oslo and Trondheim was especially challenging. This may serve as a reminder of the importance of being sensitive to ‘the historical geography of scientific knowledge,’ as recommended by Finnegan (2008).

Finally, one of the overarching frameworks for legitimizing the entrepreneurial ventures of both Brøgger and Sæland had been the importance of maintaining an idea of scientific autonomy. This idea had been productive insofar as to establish the authority of science as a vehicle for modernization in Norway. As in most industrialized nations, Norwegian industrialists and politicians held great hopes for how scientists and their research would result in progress also within the sphere of production. Many university professors did contribute in highly profitable ways to Norwegian industry in the years before World War II. Yet, the scientists were often reluctant of letting commercial or political interests into deciding over the direction of science. There were exceptions to this, but for academic entrepreneurs like Brøgger and Sæland the autonomy of science was a central prerequisite in legitimizing the venture of erecting institutions for research. Only scientists could decide over matters pertaining to scientific research. For Brøgger, with his idea of a science academy with a substantial fund, this attitude served him well, while Sæland never got to test his central institute plans due to the overall lack of funds in interwar Norway.

What happened to this idea of a clear-cut division between science and society during and after World War II in Norway? Obviously the distinction was blurred, the autonomy of science was no longer a primary reason for legitimizing institutional ventures within scientific research related to industry. The observations made by Norwegians in exile in the allied nations during the war had made clear that great industrial productivity could be achieved through the collaboration between scientists, engineers, industrialists, civil, and military authorities. After the war, a general spirit of common purpose in the reconstruction of the country after years of German occupation, combined with a democratic corporative ideology were among the factors explaining how industrialists, state officials and scientists realized they should sit around the same table to make decisions about the future direction of Norwegian research. The establishment of a national council for scientific and industrial research as an independent institution allowed for a rethinking of the limits of scientific autonomy. As pointed out by Guston (2000, 9), a recurring issue concerning public policies for science since the late 1880s has been how to maintain the integrity of science while also ensuring its accountability and productivity. Here, the research council of 1946 turned out to be a viable solution that also became a very fertile ground for academic entrepreneurship with a number of ensuing institutional establishments.

Notes

1. Already by 14 August, Nansen had sent a telegram stating he was ‘Home safe after fortunate expedition.’ The news was published worldwide, e.g. in the article ‘Dr. Nansen is Still Alive and Making Good Progress Toward His Comfortable Home,’ in Los Angeles Herald, Volume 25, Number 317, 14 August 1896.
2. I am grateful to Berris Charnley for pointing this out to me.
3. For this section I rely on Stig Kvaal’s collection of archival material from the establishment of the Royal Norwegian Council for Scientific and Industrial Research.

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References


Institutional enterprise as a compromise: the national organization of science in France

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ABSTRACT

Institutional entrepreneurship implies the capacity of actors to mobilize resources to create institutions in which collective action is embedded. Its historical analysis may consider it as a long-term process involving the role of different generations of actors and highlighting such phenomena as transmission and competition. Inspired by institutional analysis, this paper examines these temporal dynamics of institutional change by focusing more particularly on the nature of competition between different institutional enterprises in the early twentieth-century French scientific field. It highlights also the role of the State, which plays a particular role by ensuring stability. Part 1 examines the efforts of several institutional enterprises to shape the Caisse des recherches scientifiques, one of the first national organizational reforms of French science. Part 2 focuses on World War I, which provided opportunities to lay the foundations for a new organization of science. Part 3 examines the subsequent reorganization of science during the interwar period. It presents the creation of the Centre national de la recherche scientifique in 1939 as a new compromise resulting from a long cumulative process. The last section sums up the different theoretical issues and insists on the specificity of institutional change within the scientific field.

Introduction

For decades, social studies of science brought to the attention of historians the fact that science is a social phenomenon (Ben-David and Sullivan 1975). For this reason, these historians pay particular attention to the role of institutions within the scientific field, drawing a distinction between three kinds. A first set of scientific institutions are of the epistemic type. Scientific statements – or knowledge – are constructed in a very specific institutional framework such as peer-reviewed journals, conferences and/or laboratories. Scientific activity is also based on another kind of institution, which provides symbolic and material credit within the scientific field. These credit institutions divide the resources which the scientific field has at its disposal so that scientists can implement their tasks. Some well-known instances of these institutions are prizes or grants, which still play an important role in the scientific field (Merton 1968; Crawford 1980; Crosland and Gálvez 1989; Khan 2015). The third set of
institutions attract material resources and symbolic recognition from non-scientific fields and convert them into assets available to scientists. Governmental agencies or private foundations are good examples of such converting institutions: they attract resources from non-academic fields and distribute them in scientific fields.

In each case, these institutions are subject to evolution; one challenge for historians is to provide an adequate explanation and analysis of change when it occurs. For such a purpose, the institutionalist approaches may be of help to them. In the very large institutionalist toolbox, the notion of institutional entrepreneurship is particularly useful for the historian of science who studies the period before the emergence of so-called Big Science. Indeed, the nineteenth and early twentieth centuries were at that time influenced by a romantic perspective on science which accorded importance to heroes of science such as Humboldt, Darwin, Pasteur and Curie. This heroic vision, however, has long been abandoned by scholars (MacLeod 2007). Institutional entrepreneurship analysis helps not only to remain detached from this perspective, but also to inquire into whether these important figures were not also institutional entrepreneurs. Such a label has important heuristic consequences. Institutional entrepreneurship theories, in fact, insist on the notion that institutional entrepreneurs depend on their social position within a specific field. It also depends on their ability to mobilize resources from different fields in order to introduce divergent change from the pre-existing institutional framework (Battilana, Leca, and Bowenbaum 2009). Considering big names of science as institutional entrepreneurs leads to the recognition of their individual agency in a more institutionalized context without being limited to their epistemic work.

Because science is a social phenomenon, the history of science has to deal with institutional change, and because institutional change depends on institutional entrepreneurs who belong to various fields, the history of science has to pay attention to them by taking advantage of institutional analysis. The latter gives to the State a particular status both for institutional change and for the evolution of various fields (Fligstein and McAdam 2012). The State guarantees legal order and the relationship between the different fields thanks to its specific authority. This role has strong significance for the history of science in the nineteenth and early twentieth century since the institutionalization of science – i.e. the development of formal institutions enabling the political recognition necessary to find material resources – is embedded in the development of state functions (Pestre 1997). The question is the extent to which the institutional entrepreneurs of science were able to base their institutional strategies on state actors and institutions.

French science offers a particularly fertile ground for investigation into this question. French science in the late nineteenth and early twentieth centuries was still characterized by very specific traits: a weak university system compared to the German system, an influential bureaucratic technocracy, a much more accentuated level of individualism than in other countries because of a lack of collective scientific work, an enduring lack of means (and more particularly a relatively weak interest of philanthropy in science), and, finally, a strong engagement in a process of emulation at the international level (Paul 1985; Marais 1999; Fox 2012). Moreover, French science at that time also had particular characteristics on the political level. The first is the existence of a tradition of political commitment by scientists such as the chemists Jean-Antoine Chaptal, Jean-Baptiste Dumas, and Marcellin Berthelot and, later, the mathematician Paul Painlevé and the physicist Jean Perrin, all of whom became ministers in different fields. Furthermore, in the 1900s, the intellectuels, who emerged during
the Dreyfus affair, gained considerable influence on the political system, giving rise to the feeling that the Third Republic was a republic of professors and scholars.

One might think that this intense link between scientists, politics and the State would have provided a favorable context for institutional entrepreneurs in science. Paradoxically, however, the process of the institutionalization of science was not particularly rapid in France. The French scientific community complained of backwardness compared to Germany and, later, to the U.S.A., despite the recurring attempts to reorganize French science, from the university reforms of 1896 to the creation of the Centre national de la recherche scientifique (National Centre for Scientific Research, CNRS) in 1939. The fact that the process was slow and difficult, whereas French scientists enjoyed apparently a comfortable institutional position, is a source of questioning.

A first answer concerns the fact that French scientists were socially dominated by other members of the elite (Charle 1987, 1994). In this paper, however, I will provide a complementary explanation based on institutional analysis. It must be borne in mind that institutional entrepreneurship is considered to be the introduction of a divergent change in a pre-existing institutional framework. No institutional enterprise, however, is isolated from other attempts to introduce change. Far from sharing the same understanding of the change, institutional entrepreneurs may be in competition with each other. This situation explains why institutional change may take time and sometimes necessitates a cumulative process before compromises allowing the emergence of new institutions are reached. If the process takes time, it is precisely because actors use time as a resource (Bucheli and Wadhwani 2014). Consequently, the historical analysis has to take this issue into account by avoiding the constraints of a linear narrative, by describing the different possible attempts to introduce change in the institutional framework, and by paying attention to the way actors can use time to construct cumulative change (Thelen 2003).

These theoretical requirements have obvious consequences on the historiography of French science, which has been strongly influenced by Joseph Ben David’s conclusions about its decline (Ben David 1970). Among a considerable body of revisionist literature addressing this issue are works dealing more particularly with the institutionalization of French science; these tend to evaluate its performance through a general analysis of the system (Shinn 1979, 1989; Fox and Weisz 1980; Paul 1985). Other aspects of historiography insist on the evolution of professional groups, related to either a particular institution such as universities or grandes écoles (Shinn 1980; Weisz 1983) or certain disciplines (Weart 1979; Pestre 1992; Weisz 1995; Jas 2000; Gispert 2015). French academics of this period have also been studied by social historians, who tended to examine the elite during the Third Republic and, more precisely, the emergence of the intellectuels (Charle 1994).

This historiographical approach did not really consider this institutional change to be a result of a social process based on institutional entrepreneurship. Some general narratives about French science (Picard 1990; Guthleben 2013) insist generally on some grandes figures or stress the key role of the group that emerged around Marie Curie, Jean Perrin, and Paul Langevin in the organization of French science in the early twentieth century (Weart 1979; Picard 1990; Pestre 1992, 1997). This group, the so-called ‘Arcoest group’ named after the Breton village where several of its members spent their holidays, often appears as a common thread linking several major reforms and can be regarded as an institutional entrepreneur. That being said, it was not the sole group involved in such a transformation. Despite some exceptions (Eidelman 1986), there has never been a general account of the competition
between different institutional entrepreneurs and the impact of this competition on the temporality of the institutional change of French science. Blancpain’s pioneering study (Blancpain 1974) took into account certain dimensions of this process without dealing with entrepreneurship. In a way, institutional change within the scientific field remained largely unexplored.

Inspired by institutional analysis, this paper addresses this question by focusing more particularly on the nature of competition in the scientific field between different institutional enterprises, which explains the time needed for institutional change. It analyzes these competitive dynamics and shows how scientific entrepreneurs construct temporality, whether it deals with transmission of their enterprise over time and how this process gives rise to ratchet effects. In this process, the State, which also depends on actor’s mobilization, plays a particular role by gathering multiple fields and ensuring stability. Part 1 examines the creation of the Caisse des recherches scientifiques (Fund for Scientific Research, CRS) in 1901, one of the first reforms of French science, and summarizes the role of several institutional entrepreneurs. Part 2 focuses on World War I, which may be considered a contingent event providing new opportunities to lay the foundations for a new organization of science. Part 3 returns to the main stages of the reorganization of science during the interwar period. It presents the creation of the CNRS in 1939 as a new compromise resulting from a long cumulative process. The last section sums up the different theoretical issues and insists on the specificity of institutional change within the scientific field.

1. Reforming science at the Belle époque: the competition of institutional enterprises

The process underlying the organization of French science in the first half of twentieth century was far from linear. It was the result of a long competition between different entrepreneurial initiatives that had its roots in the late nineteenth century. In that period, French science experienced a series of reforms that did not entirely satisfy the scientific community. The scientific institutional framework resulted from a long process of institutional layering (Thelen 2003), where the place of the State was quite ambiguous. In 1868, the creation of the École pratique des hautes études (Practical School for Advanced Studies, EPHE), thanks to the initiative of the historian and minister Victor Duruy, had already been an indication of the State’s commitment to promoting scientific research (Weisz 1983). Later the foundation of the Institute Pasteur in 1886–1888 was based not only on public funds but also on private donations; it reflected the search for a real autonomy by academics who did not want to be totally dependent on the state authority. The 1896 university reform was the outcome of changes carried out under the Third Republic when some large provincial towns encouraged the creation of new scientific institutions (Nye 1986). Yet this effort fell well short of the needs of a community engaged in a form of international competition. This situation was all the more difficult because, in the early twentieth century, philanthropy alone was unable to meet the financial demands of science, and a system of prizes, which were granted by learned societies or academies, was unable to provide financing for increasingly costly research (Shinn 1979; Crawford 1980; Crosland and Gálvez 1989). The dynamic and unstable equilibrium between state, local and private means of support, on the one hand, but also between basic and applied research, on the other hand, had to change. In this context, institutional arrangements were needed.
The *Caisse des recherches scientifiques*: new resources, old model

One institutional innovation to meet this need was the *Caisse des recherches scientifiques* (CRS, Fund for Scientific Research) created by the law of 14 July 1901 (Pinault 2006). It was the result of five years’ work by a republican deputy, Jean-Honoré Audiffred (1840–1917). A lawyer by training, Audiffred developed expertise in the domain of agronomy thanks to the development in his own estate. This interest in agronomy and his election as deputy in 1879 allowed him to support measures of agricultural modernization using a scientific approach. At that time, agronomy allowed the conquest of French agriculture and the modernization of the countryside (Jas 2000). Moreover, for Audiffred, policy-making was a science (Pinault 2006, 22) and this conviction led him to encourage scientific research although he was not an academic scientist himself.

Audiffred was re-elected in 1898 and submitted a bill in 1900 for the creation of a Fund for Scientific Research for pure science not only in medicine, agriculture, and veterinary science, but also in other domains like physics and chemistry. This enlargement of Audiffred’s project to other disciplines very likely contributed to its rapid adoption in July 1901. By dealing with different disciplines and not only with medicine, it contributed to building the representation of science as a whole.

What was at stake here was the meaning that was attributed to the term ‘science.’ One of the characterizing aspects of science, as it emerges in the early twentieth century, is the ever-increasing costs of technical equipment. A way to mobilize resources was to highlight the benefits of science: by doing so, the discussion referred more or less explicitly to the issue of the antagonism between ‘pure’ and ‘applied,’ ‘disinterestedness’ and ‘profit.’ This legitimizing strategy (Favero 2017), however, needed to find a subtle balance in order to avoid any opposition from supporters of either ‘pure’ or ‘applied’ science. At the senate, senator Édouard Prillieux, who was also a botanist and a former professor at the National Institute for Agronomics, presented a report to defend the bill. He insisted on the fact that ‘the conquests of pure science quickly provide multiple applications whose effective result is to improve the human condition’ (quoted Pinault 2006, 49). This justification echoed Pasteur’s conception about the unity of science: ‘There is no such thing as a special category of science called applied science; there is science and there are its applications’ (quoted in Debré 2000, 84). By rhetorically preserving the unity of science, Audiffred and his supporters brought about the acceptance of the CRS on the part of the scientific community; by insisting on the applications of science, they obtained political support.

The role of the CRS was to raise funds. Its resources were rather modest, but not negligible. Between 1903 and 1913, the CRS allocated approximately 750 grants for a total amount of 2,371,500 francs. If we set aside the funding for research on wastewater, which had its own budget, the average amount of each grant was quite similar to the amount of a single small ‘prize’ delivered by the Academy of Sciences. In fact, the functioning of the CRS was based on a rather narrow notion of what to support (Pinault 2006): it did not fund research programs, but only dealt with individual projects. Although some scientists benefited from above average support, the resources were quite scattered for individual projects, and although the CRS was not limited to specific disciplines, 63% of the budget was devoted to life sciences. The openness of the CRS to other sciences had been useful in order to make its creation possible; in reality, however, it mainly supported the disciplines which had been targeted in Audiffred’s 1896 project.
This imbalance was particularly obvious with the exceptional support the CRS gave to Albert Calmette (1863–1933). After studying medicine, Calmette had entered the navy’s health service, and then spent several periods in the French colonies before joining the Pasteur Institute in 1890. As a member of this school, Calmette was aware of the industrial and economic importance of basic research, even in the area of human health (Cassier 2008). Shortly thereafter, he was sent to Saigon to set up a branch of the Institute, and in 1895 he was given the task of creating a site of the Pasteur Institute in Lille. As director of this new institute in 1899, he pursued his research not only on tuberculosis but also on water purification and set up a sewage treatment plant which was an essential part of the development of the sewage networks in Northern France. Calmette’s research had a powerful industrial dimension and received strong local and political support. The Lille lawyer Edmond Ory, who was at the same time president of the Northern Consortium of sewage, succeeded in obtaining from Parliament a special credit for the CRS dedicated to this project. Thanks to this special subvention, the CRS awarded Calmette 220,000 francs from 1903 to 1907. These subsidies were equivalent to one-third of resources distributed in that period. Thanks to strong political support and to Audiffred’s involvement, the CRS was able to provide new material resources to the life and medical sciences. Calmette’s project excepted, however, the CRS subsidies were delivered on the basis of individual submissions, and the model for dividing resources remained a traditional one, based on division between individual projects.

Reforming society, organizing science

Other actors tried to introduce more significant change by using another kind of strategy, as shown by Henry Le Chatelier’s initiative. Le Chatelier was a professor at the prestigious École Polytechnique, and a big name in metallurgy in early twentieth-century France. He was the creator of the Revue de métallurgie and also a consultant for private companies. Nevertheless, his campaign for industrial science was far from easy. In 1894, he firmly criticized the scientific training offered by the École polytechnique, which he considered too theoretical for engineers. The criticism was so fierce that Le Chatelier had to abandon his position. In that circumstance, he needed the support of Marcellin Berthelot, a famous chemist but also a former minister, who helped him to be elected professor at the Collège de France in 1897. In 1907, after two unsuccessful attempts, he succeeded in being appointed professor at the Sorbonne. This event was perceived as the victory of supporters of applied science (Henry 2000).

However, Le Chatelier’s purpose was not only to defend applied science but also to promote improvements in the organization of science. As the propagator of Frederick Taylor’s work in France (Fridenson 1987; Letté 2004; Henry 2013), he considered that industry and science as well could be made more efficient by rejecting individualistic behavior. Scientists first had to abandon the postulate according to which ‘science with a capital S is not to be regimented’ (Le Chatelier 1914, 88). According to him, scientific research needed to act on new rules and in line with research programs set up by a committee of scientists and industrialists. According to this plan, of course, subordinate researchers were expected to devote their work to the problems identified by others. Le Chatelier’s ideas actually offered a particular conception of the management of science, which rejected the romantic ideal of
autonomous scientists and was based on a hierarchical structure of the scientific community.

Le Chatelier’s conception of science was not only a theoretical discourse on science but also a concrete project he intended to implement as an institutional entrepreneur. In the early 1900s, Le Chatelier was one of the managers of the Société d’encouragement pour l’industrie nationale (Society for the Promotion of National Industry, SEIN) and became its president in 1904. Inspired by the British Royal Society of Arts, the SEIN was established in the early nineteenth century to improve the French industry through the diffusion of technical knowledge and the granting of prizes (Butrica 1988; Benoit, Emptoz, and Woronoff 2006). Le Chatelier tried to change the prize system so that it could fund coherent research programs and not only individual inventors. This initiative offers a good example of institutional conversion (Thelen 2003). Le Chatelier attempted to use the material resources of a pre-existing institution, in which he had succeeded in reaching a strong position, in order to implement his own conception of the organization of science on two levels: changing the credit institutions (the SEIN prizes system) in order to modify the epistemic ones (the research programs). Pierre and Marie Curie were supported through this funding scheme, for instance. His attempt was not wholly successful, given internal resistance within the SEIN. However, Le Chatelier continued to develop its vision in another institution, the Musée social, which had been set up in 1894 as a place to reflect on and promote social reform using a scientific approach (Chambelland 1998; Topalov 1999; Horne 2002). In 1911, a section on economic studies was created within the Musée social, and it addressed the role of science in industrial production just before the war. The debate developed around a report submitted by Le Chatelier in favor of a new organization of science (Letté 2004). In his report, Le Chatelier considered that ‘collaboration in comprehensive research, systematically organized and directed by proven scientists, was the best preparation for original production, as discipline in the army is indispensable to the training of leaders’ (Le Chatelier 1914, 88). This hierarchical dimension in the management of science, very likely inspired by Taylor’s ideas, also had to be implemented for funding. Le Chatelier then suggested a complete scheme as a result of such principle:

The initiative of any grant proposal must be made by a small group of scientists or industrialists, not more than half a dozen, who are equally competent on a given subject and of an uncontested reputation. These groups may report the sums to be subsidized to the committee in charge of distributing the funds: they draw up a research program, designate the scientists or engineers most capable of carrying out the proposed research, the execution of the work and they indicate the requested sums, specifying those intended to pay the experimental expenses or to remunerate researchers for their work. (Le Chatelier 1914, 88)

For Le Chatelier, the institutionalization of science had to be based on a very strict form of management similar to that of Taylor.

The debates within the Musée social thus highlighted the different conceptions concerning its organization. On the eve of World War I, French science was considered a political issue from two points of view: its social and economic role and its own organization. It is worth noting that the institutions in which Audiffred was involved took initiatives and in turn became a locus of conflict between competing enterprises. Le Chatelier’s project was not compatible with Audiffred’s (Eidelman 1986). The CRS was based on an individualist vision of science, whereas Le Chatelier’s goal was to regiment it. For that reason, we cannot understand institutional enterprises without first examining the competition among them.
This is even more necessary since another group of entrepreneurs intervened in this reorganization of French science on the eve of World War I.

**A Dreyfusard science?**

Scientific entrepreneurship refers to a field of activity with idiosyncratic traits. French science in the early twentieth century cannot been understood without taking into account the Dreyfus affair, which divided society in general and the scientific community in particular. This political crisis took place between 1894 and 1906 and centered around the unfair prosecution of army captain Alfred Dreyfus, who was accused of treason because of antisemitic sentiment. A great number of intellectuals denounced the falsified documents on which the Army based the accusation. In a sense, the Dreyfus affair was also a scientific controversy. The highly influential mathematician Paul Appell (1855–1930), who played a key role in the organization of scientific projects, was convinced of Dreyfus’ innocence as early as 1897 (Appell 1923, 208). Paul Painlevé (1863–1933), also a mathematician, took longer to be convinced, but was nevertheless in the front line of the campaign to defend Captain Dreyfus. Both epitomize the Dreyfusard scientists from the milieu of the École normale supérieure. They formed the core of a solid group and were joined by a third mathematician, Émile Borel (1871–1956), who founded the Revue du mois, a scientific and literary journal in which Painlevé collaborated along with the physicists Jean Perrin and Paul Langevin. This Revue du mois group, who spent holidays in Brittany and took the name of Arcoest group, played a major role in the organization of French science.

These political and personal affinities were reinforced by shared scientific convictions. Painlevé and Borel were both interested in applied mathematics, and their scientific investigations led them to focus on the subject of aeronautics. Drawing on his research on mechanics and the subject of friction, Painlevé gave the first course in fluid mechanics at the newly created École supérieure d’aéronautique et de construction mécanique (Higher School for Aeronautics and Mechanical Construction, ESACM) inaugurated in 1909. That same year, he started a campaign to promote aeronautics, and the following year he published a work on aviation with Borel (Borel and Painlevé 1910). Painlevé, finally, was also involved in the creation of the Institut aéronautique (Aerotechnical Institute) in Saint-Cyr, which then hosted research on aerodynamics (Anizan 2012).

The creation of such a research institute was particularly important insofar as it aimed to give the necessary equipment for new experiments to scientists interested in aeronautics (Fontanon 1998). Until the inauguration of the Institut aéronautique in 1911, only Gustave Eiffel’s laboratory responded to these experimental needs. Nevertheless, despite the very generous donation – Henry Deutsch de la Meurthe gave some 500,000 francs –, budgetary constraints prevented the new Institut aéronautique from working effectively. Painlevé’s election as a republican-socialist deputy in 1910, in which Appell and Borel took an active part, allowed him to step up his action. As a member of the Committee of Air Navigation at the Ministry of Public Works, he developed an increasing interest in national defense issues and succeeded in having subsidies voted for the new institute. In 1912, Painlevé held the positions of member of the Military Aviation Committee, member of the so-called Commission des poudres in the Ministry of War and was budget rapporteur for the navy in the National Assembly. In a sense, Painlevé was an institutional entrepreneur taking charge of converting external resources into the scientific field in order to develop new epistemic institutions such as the Institut aéronautique.
On the eve of World War I, French science was the focus not of one but of several institutional enterprises. Three in particular stand out: that of Audiffred, that of Le Chatelier, and that of the Painlevé group, although these overshadow others, which only a more detailed historical analysis would reveal. These enterprises diverged and at times competed with each other. Understanding them requires taking these games of alliance or opposition into account. Each of them had a collective dimension: it was generally associated with an individual entrepreneur – Audiffred, Le Chatelier or Painlevé – but the latter appears to be more the symbol of a larger group than the head of an individual enterprise.

From then on the configurations of each of these enterprises can be compared by stressing their most salient features (see Table 1). It is important to note that the most striking creation, in spite of its failures, originated outside the academic field, while those originating solely in academia were only partially implemented. The ‘École’ that Le Chatelier managed to create after his appointment to the Sorbonne in 1907 was more an invisible college than an institution in the strict sense (Letté 2004). As regards Painlevé, his plans for a national laboratory of aviation were only partly realized with the inauguration of the Institut aéro-nautique in Saint-Cyr in 1911. A scientific enterprise could only succeed if it managed to find the means necessary through philanthropy or public funding. Donations to science were sometimes very high in France, but, as it has been pointed out above, both were rare in the early twentieth century (Marais 1999, chap. 6). As a result, political powers had the upper hand in determining the budgets of universities and large institutions in the national reorganization of science.

Table 1. Three institutional enterprises in the domain of French science in the early twentieth century.

<table>
<thead>
<tr>
<th>Leader</th>
<th>Audiffred</th>
<th>Le Chatelier</th>
<th>Painlevé</th>
</tr>
</thead>
<tbody>
<tr>
<td>Entrepreneur’s identity</td>
<td>National Republican Association (political movement) Musée social</td>
<td>Revue de métallurgie (importance of engineers)</td>
<td>Associated with the milieu of the École normale supérieure, and the Revue du mois</td>
</tr>
<tr>
<td>Hierarchical organization</td>
<td>Horizontal</td>
<td>Vertical with Le Chatelier in a dominant position</td>
<td>Horizontal with Painlevé in a strong position</td>
</tr>
<tr>
<td>Involvement in different fields</td>
<td>Scientific involvement</td>
<td>Indirect via links with the milieus of agronomics, health-related issues and chemistry</td>
<td>Strong links in chemistry and engineering</td>
</tr>
<tr>
<td></td>
<td>Political involvement</td>
<td>Direct via Audiffred’s election to the National Assembly</td>
<td>Indirect via networks such as the section of economic studies at the Musée social</td>
</tr>
<tr>
<td></td>
<td>Economic involvement</td>
<td>Strong via links with the milieu of employers (Musée social) and the chemical and food industries</td>
<td>Strong relations via his work as a consultant with large enterprises</td>
</tr>
<tr>
<td>Achievements</td>
<td>Epistemic institutions</td>
<td>Personally funded research by the CRS; Calmette’s water purification plant</td>
<td>Revue de métallurgie, the ‘École’ of industrial science set up under the chair for chemistry at the Sorbonne</td>
</tr>
<tr>
<td></td>
<td>Credit institutions</td>
<td>CRS</td>
<td>SEIN prizes system</td>
</tr>
<tr>
<td></td>
<td>Converting institutions</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
This had two main outcomes. The first concerned the political dimension of such enterprises. Resorting to the State was a way to circumvent a relative lack of resources, but one that required strong political support. The success of a nationwide scientific institutional enterprise only occurred thanks to the very particular relation of scientific entrepreneurs with politics. On the other hand, having political support was not necessarily decisive within the scientific field to convince a majority of scientists to change their institutions. The second implication concerned the discourse on the social role of science. At least for political reasons, the strong financial involvement of the state had to be justified by a type of discourse insisting on the virtue of a useful science which could improve the situation of society. For instance, the support offered by the Parliament to the Institut aéronautique had been justified for military purposes. Such discourse aroused the never-ending controversy between ‘pure’ and ‘applied’ science. The institutional scientific enterprises based on the technological opportunities provoked strong negative reactions as exemplified by that of mathematician Émile Picard, who feared to have the faculty of science transformed into a ‘Arts and Craft School’ (Fontanon 1998, 77).

In this context, institutional change within the French scientific field could only be incremental and slow. Because different visions and different projects were in competition, no scientific entrepreneurs were able to impose their own vision of the organization of science. The commitment of the State depended on personal contacts with politicians and even on individual involvement in politics. Within the scientific field, the discourse about ‘pure science’ had always been highly esteemed, although it was essentially a rhetorical cliché.

2. World War I, a contingent opportunity for institutional initiative

World War I upset these subtle relations by prioritizing the plans of those scientists who not only mobilized for the war effort, but also maneuvered to obtain key political positions in the government. From a theoretical perspective, the war may appear as a contingent event ‘which set into motion institutional patterns or event chains that have deterministic properties’ (Mahoney 2000, 507). Indeed, it offered the opportunity to move forward with the organization of science by creating institutions which the conflict required. This time the ‘Painlevé group’ clearly had the upper hand and succeeded in creating a new institution which was the first to be truly embedded in the governmental system in order to coordinate scientific work.

An under-secretary of State for Inventions: an institutional innovation

World War I produced unexpected and contingent effects within the scientific field. First, the classic controversy between basic and applied research was no longer relevant. In this particular context, rapid technical and military outcomes were demanded. The coordination of scientific work became more and more urgent insofar as the first initiatives were not convincing. From the start of the conflict, the Academy of Sciences undertook to coordinate research on national defense (Belot 2000). If the engagement of the Academy of Sciences in the conflict largely reflected the drive for reform that was already evident before the war, this commitment was still based on old patterns (Crosland 1976; Roussel 1989; Bret 2002). Members of the Academy were essentially invited to review proposals made by inventors and did not have sufficient material resources to conduct their own research. The traditional
way of mobilizing science was inefficient in a conflict whose duration was unexpected. This limit provided an opportunity for an institutional innovation.

Such innovation was based on pre-World War I institutional enterprises and reached a turning point when Painlevé was appointed Minister of Education, Arts and Inventions relating to national defense in November 1915. In this new ministry, a Department for Inventions for National Defence was set up (Roussel 1989; Galvez-Behar 2005; Anizan 2012). It consisted of 50 members, including five academics, nine university professors, and 10 civil engineers, but Painlevé's group stood out with Émile Borel as head of the technical office, assisted by Jean Perrin. The Department for Inventions encouraged scientific research, financed it, evaluated inventions, and sometimes developed them. Painlevé and his group were not only at the heart of the scientific mobilization but made a vital contribution to its organization (Anizan 2012, 155, 156). With this institutional innovation, science clearly became a governmental mission. The Department for Inventions and Painlevé's ministry were not only funding institutions devoted to supporting other epistemic ones but also represented a political recognition for the contribution of science to the common good in the war context.

This process had to be reinforced, however. The formation of a new government in December 1916 left Painlevé out of power but saw the creation of an Undersecretary of State for Inventions to which the Department was attached. The deputy Jules-Louis Breton was then appointed Undersecretary of State. The new figure responsible for inventions was already known in the political and scientific world (Moissinac and Roussel 2010). Breton (1872–1940) was a deputy who had studied chemistry and held a place of assistant at the Collège de France in the early 1890s. In the 1910s, Breton then joined the Republican-Socialist Party close to Painlevé. Elected as a member of the board of directors of the CRS in 1912, he introduced a budget amendment to boost its financial resources. As a member of the Commission supérieure des inventions (High Commission for Inventions) in 1914, he had carried out research on a device to cut through barbed wire. Breton's footing in the scientific and political milieus, his involvement in the CRS, and his political and personal proximity to Painlevé explain his appointment as new Undersecretary of State for Inventions in 1916.

With the Undersecretary of State for Inventions, the men of science appointed by Painlevé acquired greater influence and held the upper hand over frequently hostile technical-military bodies. In 1917, the Undersecretary of State for Inventions evolved with the political rise of Painlevé. When the latter became Minister of War in 1917, the Undersecretary of State was renamed and its functions were much reinforced. When Painlevé became Prime Minister in September 1917, the Under-secretaryship of State for Inventions returned to his authority under the Ministry of War. The arrival of Painlevé as head of government thus allowed a concentration of technical-military research services under the authority of a civil entity and with the active participation of experts and inventors.

In spite of this, the Department of Inventions, then the Under-secretaryship of State, did not have a straightforward development and was heavily contested by antagonistic forces among civil society and the military and by supporters of competing or even clashing enterprises. After Painlevé's resignation in November 1917, the institution became more vulnerable and Jules-Louis Breton faced increasingly strong criticism. The Undersecretary of State reverted to being a simple department under the Ministry of Armaments, which exercised a strict, and even wary control over its activities. Because the social and political demand remained strong, Breton nonetheless had to maintain this institution of scientific mobilization.
The initiatives of Le Chatelier in the Academy of Sciences

World War I certainly did not erase the competition of institutional enterprises that had emerged before the conflict. Henry Le Chatelier tried to promote his plans to reorganize science to the Academy of Sciences, which had been one of the first scientific institutions to be involved in the war. In November 1915, during a secret committee of the Academy, Le Chatelier insisted on the need to support industrial science and to devote a sum of money. Some months later, he called on the Academy to ‘more actively involved in the direction of research’ in subsidizing laboratories on the basis of a scientific program. In 1916, he proposed that the Academy take the lead in reorganizing existing scientific bodies and suggested setting up a commission to draft a report on the ways and means to develop scientific methods in industry. The idea was met with general approval and the proposal was accepted, whereupon a new body, the Commission d'action extérieure (committee for external action), was set up to prepare a report. With Le Chatelier, the commission brought together half a dozen members of the Academy, several of whom – including the chemist Albin Haller – had already declared themselves in favor of reconciliation between science and industry before the war.

In line with Le Chatelier’s plan, the work of the new commission dealt with three types of issues: the organization of large laboratories for research in industrial science; changes in teaching methods; and the organization of work in factories, thanks to industrial laboratories and the impact of Taylorism. Yet in the end the commission focused on two basic issues: the setting up of a national laboratory of physics and mechanics and the creation of a new section within the Academy. The Commission’s plans vied with those of Painlevé as the Minister of Education. Starting in autumn 1916 Painlevé planned the creation of a national office of applied sciences, whose primary role would be to provide information and to coordinate and reinforce the links between science and industry (Anizan 2012, 170–175). The project failed due to Painlevé’s departure from the Minister of Education in December 1916, but his successor, René Viviani, reported back to the Academy of Sciences. He judged that the two projects were not incompatible per se, but stressed the difficulty of providing the means necessary for both of them if implemented at the same time. Le Chatelier’s plan and Painlevé’s plan clearly competed with each another.

The other question addressed by the Academy was the creation of a section dealing with the application of science to industry. André Blondel, who had studied at the École Polytechnique and with whom Le Chatelier was linked through his activities as a consultant, proposed that the Academy create five places for renowned figures in the domain of industry or the application of science to industry. The idea was taken up by the Commission d'action extérieure (committee for external action) and sparked intense debate. It was not until January 1918 that a section on ‘Applications of Science to Industry’ within the Academy of Sciences was created. The election of members of the new section was the chance to witness the influence of Henry Le Chatelier at work with his three candidates – Auguste Rateau, Maurice Leblanc, and Georges Charpy – while Georges Claude, even if defended by Paul Appell and Paul Painlevé, was not elected.

The opposition between these two institutional enterprises took place against the backdrop of an attempt to reorganize French capitalism initiated on the eve of the war and which gathered speed during the war thanks to Étienne Clémentel, who was appointed Minister of Trade and Industry in 1915 (Druelle-Korn 2012). Yves Roussel has demonstrated Clémentel’s interest in projects developed in the Academy of Sciences (Roussel 1985, 38–40). Providing
France with high-quality laboratories became one of the goals of Clémentel’s team. Being member of that team, Le Chatelier benefited political support to promote his institutional enterprise.

Facing World War I as a contingent event, the institutional enterprises of the Belle époque had produced contrasting results. Painlevé’s enterprise was clearly a success. It profited from the needs and opportunities generated by the war, and exploited the political position he had acquired before the conflict. He managed to set up an organization to promote science at war. This enterprise was clearly motivated by patriotism and had to operate within strict military constraints. The fact remains that its implementation rested, albeit not entirely, on processes which already existed in the early twentieth century. The war allowed the pursuit of a project by other means and, obviously, in another context. In particular, it provided Painlevé’s enterprise with support for further developments. Success in the case of Le Chatelier was far less evident. But here too, the project planned from 1914, albeit reformulated, was not abandoned, and the troubled backdrop of war facilitated the emergence of new alliances and new positions. The uncertainty generated by the conflict also created opportunities; the fact that Le Chatelier seized these opportunities emphasizes the entrepreneurial dimension of his approach. War brought actors from different fields together: on ‘the other front’ (Fridenson 1977), the military, politicians, industrialists, and experts collaborated more closely than before.

For the French scientific field, the war constituted an exogenous shock which might well have had no consequence. Contingent events or turbulence do not automatically imply institutional change unless institutional actors have not prepared it with a ‘capacity for social construction and strategic agency’ (Fligstein and McAdam 2012, 20). A new scientific institutional framework emerged in France during World War I because of the particular connection between the scientific and political fields, but also thanks to brokers such as Painlevé and his group, who were able to take advantage of this opportunity. Their capacity to do so was based not only on their powerful position but also on a vision of science, which had been asserted before the war. Even if the main explicit objective was to win and not necessarily to change the scientific institutions, it was possible to kill two birds with one stone. Painlevé was not the only entrepreneur who attempted to introduce change. Contingent events or exogenous shock do more than provide the conditions for an episode of contention between the challenger and the incumbents (Fligstein 2012, 20). They can also be the moment when different challengers compete among themselves, and this situation makes the change more ambiguous. It was particularly the case with the French scientific field, whose epistemic institutions were then bypassed to a certain extent. During the conflict, the priority was to produce useful knowledge; secrecy was imposed on sensitive research; and a number of young scientists were mobilized for military tasks. But with the war over, the normal process of scientific life had to be reasserted; it was a question of reinforcing the changes which had just been implemented. An institutional reconversion was indispensable.

3. From one enterprise to another: the reorganization of French science in the interwar period

The post-World War I period constituted a sensitive phase for science. In the U.S.A., the scientific field experienced a ‘ratchet effect’, which enabled the institutions of scientific
mobilization to be maintained (Agar 2013, 110). In fact, it was far from obvious that such a process would take place. It required actors’ involvement so that its irreversible dimension could be implemented within artifacts and institutions. This construction of the ratchet effect was particularly necessary in France because scientific institutions which had been set up during the conflict were challenged as soon as fighting ceased. Institutional entrepreneurs thus had to persevere by converting their initiatives without founding them on a logic based upon war.

**An impossible compromise: the Office national des recherches scientifiques et industrielles et des inventions**

This institutional reconversion was particularly difficult because divergent visions of science had to be gathered. Just after the war, on 2 December 1918, Paul Painlevé, as chair of the Academy of Sciences, delivered an important speech illustrating these contradictory conceptions. After a vibrant tribute to the dead soldiers, Painlevé drew a sharp distinction between the ‘recherche désintéressée’ (‘disinterested research’), which young French scientists were used to dealing with, and German science, which had become ‘a gigantic enterprise [to build] the most terrifying killing machine’ (Painlevé 1918, 800). German science was not the only target of Painlevé’s denunciation, which was made in more general terms. For Painlevé,

> scientific culture, when it is harshly pursued for immediate use, sordid lucre or oppressive domination, degrades the soul instead of elevating it above itself. It leads to a sort of savage barbarism, organized cruelty which takes for its adepts the aspect of a wild religion, of which all the crimes are sacred and before which the infidels must bend their knees. (Id.)

In fact, this statement might be understood as relevant for all scientists, French ones included. The claim for a back-to-normalcy policy threatened these new scientific institutions, which had been built during the war. On the one hand, the perspective of having them disbanded was a way to impose their change by putting pressure on Painlevé’s group. On the other hand, Painlevé’s competitors had no interest in having them totally removed, as this did necessarily mean that their own project would be implemented. The real ratchet effect was not the Direction of Inventions, which remained weak, but the emergence of a new set of possibilities, which made a compromise profitable for each entrepreneur. Thus, in December 1918, an interministerial commission was set up to find a compromise. This 40-member group was presided over by Clémentel himself, with Painlevé as one of the three vice-presidents, and consisted of three sub-commissions.13 The first dealt with the construction of new national testing laboratories and attempted to bring the experimental CNAM laboratory and the Central Laboratory of Electricity closer together by setting them up in the same place in a suburb of Paris. The second sub-commission focused on the creation of a laboratory of the Academy of Sciences. Le Chatelier wrote the report calling for such an institution to pursue ‘general research, without an immediate industrial objective’ attached to the Office national des recherches scientifiques, industrielles, agricoles et des Inventions (National Office of Scientific, Industrial and Agricultural Research and Inventions, ONRSII). The future National Office was supposed to develop and coordinate applied scientific research, while guaranteeing studies required by the government and assisting inventors. Each report was accompanied by a bill. When it completed its work early in 1919, the interministerial commission drafted a general plan to accommodate competing projects.
Yet the government only submitted one bill in 27 May 1919. The bill was based on a report by the deputy Henri Pottevin (1865–1928), who had been a key actor in the interministerial commission. Pottevin was a physician, chemist, and biologist, a former member of the Louis Pasteur laboratory and then the Pasteur Institute. He had been also a Dreyfusard, had specialized in questions of hygiene and, in 1909, had become secretary general of the International Office of Public Hygiene. In 1914, he was elected deputy for Tarn-et-Garonne: he chaired the Nitrogen Commission in 1917 and worked with the Department of Inventions. To some extent, the figure of Pottevin is comparable to that of Painlevé: that of a politician-cum-scientist. However, it is important to note that the bill he submitted in May 1919 was the only one of the three projects drafted four months earlier to be actually submitted to the Parliament for approval. This bill differed nevertheless from the original project in some respects. The new Office was to be a financially independent public body to promote, coordinate, and encourage not only applied research, but all branches of scientific research. The Department of Inventions and the CRS were connected, although the latter preserved its autonomy. In fact, the bill tried to make a synthesis of the three earlier projects.

The bill was adopted by the National Assembly on 10 July 1919, but it took over three years for the Senate to approve it; it was finally enacted on 29 December 1922. Meanwhile, the Department of Inventions was under attack and only managed to resist attempts to close it down thanks to the action of its head, Jules-Louis Breton. More than ever, Breton turned out to be the pillar of the new institution. Like Painlevé or Pottevin, he was a bridge between the scientific and political milieus, and particularly between different stages of the process of the institutionalization of science in France. Although the compromise in the shape of the Office ran the risk of not satisfying anyone, it was nonetheless a significant step forward. This is clear from the fact that it acquired the status of public institution with legal standing contrary to the original plan in 1919, and had financial and legal autonomy. It could rely on a national council made up of representatives of expert bodies, learned or technical societies, higher education institutions and industrial groups. All told, the Office was a converting institution which gave resources and political recognition to science (Blancpain 1974; Chatriot 2006, 2013).

Despite these influences, the ONRSII clearly corresponded more to the needs of Painlevé than to those of Le Chatelier. Its mission was to coordinate research; it was not based on a precise scientific program. The idea was instead to respond to the needs of all parties in what was a very open-ended perspective. The other feature of the Office was its tendency to promote research with a strong industrial orientation; this led, as we have seen, to the expectations expressed around the figure of Clémentel. As a compromise, the Office had to satisfy different logics and also the material needs of scientists. The project related to the laboratory of the Academy of sciences was not abandoned. Building institutional enterprises did not consist only in converting institutions but also in epistemic ones. In the 1900s, the physicists Aimé Cotton (1869–1951) and Pierre Weiss (1865–1940) and Jules-Louis Breton put the ONRSII at the service of the Academy of Sciences in order to realize a large electromagnetic installation (Shinn 1993; Guthleben 2013). The ONRSII played a large part in financing the installation of this extensive plant, which housed the work of its promoter, the physicist Aimé Cotton, from 1928 (Shinn 1993). Thus, the Office also tried to respond to the needs expressed in basic research (Paul 1985).
From the Perrin plan to the creation of the Centre national de la recherche scientifique

The reorganization of French science between the two wars is often presented as a success story for the institutional entrepreneurs led by Jean Perrin. Perrin achieved his goals thanks to his commitment to ‘pure science’ and his political support (Weart 1979; Morize-Charpentier 1997). In fact, this individualistic or heroic vision of institutional entrepreneurship is already undermined by the preceding analysis. No institutional enterprise can be understood without referring to competitors and also to supporters. Also, the description of an institution needs to include the various contradictory forces which are embedded in it. As in other cases, no scientific institution can be conceived as a demiurgic creation of individual scientific entrepreneurs. The conventional narrative surrounding the creation of the CNRS in 1939 thus has to be revised, especially given that the CNRS itself appears to be an ambiguous compromise.

At the same time as one sequence put in motion by World War I came to a close, the creation of ONR SII opened yet another. As an institutional compromise generated by different logics, the Office only partially responded to the needs of a scientific community damaged by the war and worried about the financial situation. Faced with rampant postwar inflation, the state was unable to guarantee universities funding comparable to those of prewar period: in 1921, the budget for universities in real terms was equivalent to under 80% of the 1913 budget (Pestre 1992, 308). Faced with this disastrous economic situation, a new kind scientific mobilization was necessary and other institutional enterprises emerged around the figure of the physicist Jean Perrin (Morize-Charpentier 1997). Perrin had graduated from the École normale supérieure, was a professor of physics, a Dreyfusard and member of the Painlevé group. In the early twentieth century, he was known for his work on the atom. At the Sorbonne, where he was appointed in 1910, he was opposed by Le Chatelier, who found his work too theoretical. During World War I, he was actively involved with the Undersecretary of State for Inventions through his work on acoustic tracking. With the war over, Perrin became a leading figure in the reorganization and financing of science.

Putting aside the actual details of this campaign, it is important to recall the milestones and the role played by Perrin and his group (Picard 1990, chap. 2). The creation of a foundation by Baron Edmond de Rothschild in 1921 and the launching of an appeal for private donations with a day in honor of Pasteur in 1923 (la journée Pasteur) were the first two steps of this philanthropy-based mobilization. The Nobel Prize of Physics awarded to Perrin in 1926 was likely instrumental in helping him to reach his goals. In 1927, the Rothschild Foundation financed the creation of the Institut de biologie physico-chimique (Institute of Physico-Chemical Biology) with Perrin at his head. Although it played a vital role, generosity—private or public—was not enough. In 1924 Émile Borel was elected deputy for the French Radical Party, and the following year pushed through a bill to reform the apprenticeship tax to help subsidize laboratories. The political context clearly counted since the Prime Minister at the time was Édouard Herriot, an alumnus of the École normale supérieure and a personal friend of Jean Perrin. A further step was taken in 1930 when Perrin presented a plan for a ‘National Scientific Research Service’ to finance grants, allowing researchers to devote themselves entirely to their scientific work free of teaching responsibilities (Picard 1990, 34–42).

At this point, we should recall the time it took to develop this project until the final establishment of the CNRS in 1939. The Caisse nationale des sciences (National Fund for Sciences)
was created in 1930. Its original goal had been to finance the pensions of scientists, but later it was made responsible for awarding research grants (Frédéric Joliot-Curie, Langevin’s pupil, was one of its first beneficiaries). Three years later, in April 1933, the *Conseil supérieur de la recherche scientifique* (High Council for Scientific Research) was set up to ‘facilitate disinterested research’ and to advise on how to organize such research.16 In 1935, Pierre Laval’s government merged the functions of the CRS and the *Caisse nationale des sciences* to create a *Caisse nationale de la recherche scientifique* (National Fund for Scientific Research). The enterprise led by Perrin increased momentum when he was nominated Undersecretary of State for Scientific Research from September 1936 to June 1937. Under the Popular Front government, and thanks to the support of the Minister of Education, Jean Zay, Perrin created a *Service central de la recherche scientifique* (Central Service for Scientific Research) and obtained a significant increase in the research budget and the retirement of Jules-Louis Breton, thus hastening the closing down of the ONRSII. The fall of the first Popular Front government in June 1937 did not stop the progress of the enterprise which had developed around Perrin, but it did make its course more difficult. The High Council for Scientific Research became the locus of collective reflection on the organization of science. In 1938, it voted to reinforce the links between pure and applied science that the sluggishness of the French economy and international tensions made ever more necessary. A legislative decree of 24 May 1938 thus created the *Centre national de la recherche scientifique* (National Centre for Applied Scientific Research, CNRSA) replacing the ONRSII and instituted a High Committee responsible for coordinating research in pure and applied science. Unlike the Office, the CNRSA’s task was to plan research in a context of impending hostilities. It thus echoed the model proposed 25 years earlier by Le Chatelier. In April 1939, a decree allowed the *Caisse nationale de la recherche scientifique* to set up laboratories. Finally, on 19 October 1939, a decree led to the creation of the CNRS by merging the CRS, the CNRSA and the *Service central de la recherche scientifique* to make the action of different institutions more efficient in the interests of ‘good management’.17

**Enterprises, competition, compromise**

The process that led to the creation of the CNRS in the 1930s merits some comment. The role of Jean Perrin had been crucial, but we should resist the temptation to portray him as the champion of French science. It is true that he had a key role thanks to his scientific reputation, his political support, and his membership of the so-called Arcoest group. His relations with Édouard Herriot and then with Jean Zay were also key resources when it came to turning his plans into legislation. We can also assume that Perrin’s influence grew following the death of Paul Painlevé in 1933. This generational change, exemplified by the gradual exclusion of Jules-Louis Breton, largely due to Perrin himself, brought about a change in the core of a group, which had been remarkably active in 1914.

The importance of Perrin’s role should not, however, hide the fact that the creation of the CNRS was the result of a plurality of logics (Blancpain 1974). Perrin did not create his project alone; furthermore, it was only one of several such projects initiated in the 1920s. If we know little about their outcome, it is precisely because the historiography focuses on Perrin. In any case, his project gave rise to counterproposals that echo the situation of competition before World War I. Obviously, the terms of competition between different projects were not the same, and it would be a mistake to consider that Perrin’s plans could be implemented
without any change. Perrin’s project was also redefined during its implementation to the point that one might question whether the attributions of paternity on the subject should not be questioned. It would be interesting to rewrite a narrative of the Le Chatelier project up to the creation of the CNRS showing that an a priori programming of science never disappeared during the period. The question of the relation between pure and applied science is also quite revealing. While Perrin is generally held to have based his project on the promotion of pure science, the rivalry among different projects and the demands of the time (the economic crisis, preparation for war) meant that great importance was given to applied science. The CNRS thus appears as a compromise between differing logics and between different projects; it is not surprising that its creation prompted a debate on its significance. For some scholars, the merging of the different institutions, which had been promoted by Perrin, was his own personal victory (Guthleben 2013); for others, this attempt to reduce costs and to improve productivity might well be considered retrospective success for Le Chatelier (Henry 2000).

Summary and conclusions

Conceptually speaking, the history of French science at the beginning of the twentieth century reveals a series of sequences alternating between competition and compromise and involving different institutional enterprises. These enterprises were not only based on great figures of science leading the initiatives but also on groups that gave them their collective support. In addition, these enterprises played on several institutional levels: they aimed, in varying degrees, for the emergence of epistemic institutions and also for the development of those institutions which were necessary to integrate more substantial resources into the scientific field because of its own expansion. This article highlights three types of compromise – the CRS, the ONRSl, and the CNRS – which correspond, respectively, to a different contingent event: the emergence of a new political majority in 1899 in the wake of the Dreyfus affair, the World War I and the economic crisis of the 1930s.

Of course, this parallelism does not claim to identify any law of institutional change in the scientific field. It suggests, however, that some institutional reforms depend on the specific relation of the scientific field to other fields in very particular contexts due to contingent events. The latter are contingent indeed compared to the logic of the referred field; this contingent dimension, furthermore, is always relative: the entry into the war did not depend on the functioning of the scientific field. Moreover, these events were only catalysts accelerating the institutional enterprise and not necessarily conditions of possibility. However, they had the particularity of aligning the fields in such a way as to create opportunities which actors could take advantage of, especially if they were situated between these different fields which at that period held a dominant position for a time. The case of Painlevé is iconic. As a scientist and a politician in connection with the army, he found himself at the forefront to take initiatives once hostilities were triggered.

It should be noted, however, that such events, however significant they may be, do not by themselves create institutional irreversibility and do not trace a path spontaneously. The ratchet-effect or the path-dependency rhetoric must not encourage an approach that would consider that a given event can possess the power to change a situation by itself. Even though social processes are more or less performative, it is the actors’ involvement in that process, combined with the structural constraints of the fields, that creates the institutional
irreversibility and then traces the path. This interplay is all the more necessary given that each contingent event may give a singular meaning to the institutional enterprises that it makes possible. But it is precisely the perpetuation of the institution that needs to go beyond this singularity. In our case, the so-called ratchet-effect required abandoning the war logic of the Direction of Inventions and to base it on a new logic. Converting the meaning of the institution made it necessary to insist on new imperatives, such as the economic recovery. The original logic was not totally abandoned, however, since the ONRSII was in charge of preparing the scientific mobilization for the next war. If the discourse about ‘pure’ and ‘applied’ science appears to be a permanent feature of the institutionalization of science (Schauz 2014), it has also its own temporality.

Additionally, the institutional innovations emerging after these events were stabilized because they constituted compromises between different competing enterprises. The competitive nature of institutional enterprises is essential for the analysis. Focusing on a single enterprise carries the risk of concealing competition from rivals disputing access to resources, both symbolic and material. This competition is vital when it comes to understanding how projects adapt to prevailing conditions. Institutional competition also explains how necessary the compromises are. The CRS was a compromise between Audiffred’s project and other ones. The ONRSII was a compromise between the Painlevé-Breton project and that of Le Chatelier’s. The CNRS itself was a synthesis. Of course, these trade-offs are shaky and unsatisfactory since they do not fully answer to the different expectations and different needs. These compromises are in fact unstable, and are meant to be circumvented by those who stick to their original goals.

This sequence of competition-contingency-compromise indicates that the institutional change is constructed in the long term. Time is already necessary to engage in the mechanism of competition, to find compromises and to redirect them toward the entrepreneurs’ original goals. Because of their rickety nature, the institutional compromises are not definitive solutions; they become new stakes for institutional entrepreneurs. They can be bypassed, controlled, or deleted. Perrin’s attitude toward the ONRSII is noteworthy because it fits with many of these strategies. Wanting to switch from one compromise to another means first admitting the failure of the previous one, and this process takes time.

Institutional entrepreneurs have to develop an enterprise that is not only collective but also cumulative in time. The question of transmitting the institutional enterprise, then, is essential. But, at the same time, the enterprise is necessarily called to change because of the inevitable revision of initial objectives, perceptions, and needs. The institutional entrepreneur must think about those who will be able to take his achievements over, admitting that his successors will give his inheritance another meaning and direction.

This cumulative dimension of the institutional enterprise brings one to consider the relationship to the State of French scientific entrepreneurs differently. This involvement in politics on the part of some scholars was certainly a convenient means to attract resources in the scientific field in a country where private resources were more scarce than elsewhere. However, the interest in soliciting the State was not limited to this material consideration alone. Creating institutions under the governmental aegis also helps to ensure the sustainability of the scientific activity since the State is an institution lasting in time. The continuity of the state guarantees the continuity of science. The creation of a governmental National Research Service enabled the recognition of science as an activity which was almost sovereign, and therefore durable. For scientific entrepreneurs, relying on the State meant not only
taking advantage of means but also to engraving their initiatives in time. In that perspective, the centralization of both power and science in France was certainly a driving force.

Nevertheless, considering politics the *ultima ratio* of the institutionalization of science would be a mistake. On the one hand, other institutions may fulfill the role of durability elsewhere, such as via private foundations, for instance. On the other hand, the conception according to which the rise of science is based on that of the State is meaningless if the mediations provided by the institutional entrepreneurs are not specified. A priori, the material and temporal resources that State can invest in the scientific field say nothing about the possible evolutions of the field. It is precisely the role of institutional entrepreneurs to give this investment meaning and direction.

The State cannot be the decisive factor in this institutional cumulative process, which also depends, as a last resort, on the field in which it is rooted. In other words, scientific entrepreneurs first have to ensure points of support in the scientific field before promoting their institutional initiatives in others. Painlevé, Le Chatelier, Perrin and, later, Joliot-Curie showed that institutional scientific entrepreneurs are all the more efficient actors as they have a dominant position in the scientific field thanks to the symbolic credit they accumulated during their career. The other lever of accumulation in the scientific field is made up of epistemic institutions. Reforming the institutional framework of science has no meaning if it does not allow for the emergence of epistemic institutions. Schematically, the creation of the CRS goes hand-in-hand with Calmette’s sewage plant, the ONRSII with Cotton’s electro-magnet, the *Caisse nationale* with Joliot’s laboratory of atomic synthesis. These institutions enable progress in the scientific field, by providing the material necessary for scientific work and for the symbolic credit which its results provide. In the first part of the twentieth century, French science did not lack scientific institutional entrepreneurs. There is little doubt that their number and their diverging initiatives explain the slow pace of compromise, which was accelerated by exogenous events such as the Great War or the economic crisis as well as by their more or less close relationship with the State. These institutional enterprises certainly aimed to ensure a better recognition of science, but these entrepreneurs never forget to respond to practical needs. At that time, new institutions without epistemic places would have made little sense.

**Notes**

1. French National Archives (AN), F 17 17432, technical committee, first section, meeting of 16 January 1904.
2. *Journal officiel de la République française*, 14 November 1915, report to the President of the Republic and Decree of 13 November 1915, 8200.
3. *Journal officiel de la République française*, 21 April 1917, Decree of 14 April 1917, 3163.
4. *Journal officiel de la République française*, 16 September 1917, Decree of 14 September 1917, 7732.
5. French Academy of Sciences Archives, AS, Secret Committee, record n. 7, meeting of 22 November 1915. In 1911 the industrialist Auguste Loutreuil died and bequeathed over 7 million francs to various scientific institutes three of which under the *aegis* of the Academy of Sciences.
7. AS, Secret Committee, record n. 7, meeting of 22 May 1916.
9. AS, DG 46, letter of 19 February 1917 from the Minister for Education to the President of the Academy of Sciences.
10. AS, Secret Committee, record n. 7, meeting of 22 May 1916.
11. AS, Secret Committee, record n. 7, meeting of 14 January 1918.
12. AS, Secret Committee, record n. 7, meeting of 6 May 1918.
17. *Journal officiel de la République française*, 24 October 1939, 12,594.

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A reciprocal legitimation: Corrado Gini and statistics in fascist Italy

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ABSTRACT
This article deals with the relationship between science and politics and in particular with the reciprocal legitimation process involving research schools and political regimes. It focuses on the case of Italian statistics during the early twentieth century. Its emergence as both an independent scientific field and a national research school, in fact, went together with the rise of nationalism and the establishment of the fascist regime. The paper uses the biography of Corrado Gini to analyze the process of mutual legitimization between science and politics under fascism. Gini’s academic and professional careers show in fact how actors and ideas could compete through their ability to alter the status of the discipline, the technical functions it was assigned, and to attract funds in a changing political context. Gini, as an institutional entrepreneur, was able to make his research school hegemonic in Italy by leveraging the need for scientific legitimation of new state policies during World War I and under fascism. The reinterpretation he provided of his career after the end of World War II is crucial both to deconstructing this process and to shedding light on the postwar de-legitimation of Italian statistics.

1. Introduction
The literature on the cultural practices of legitimation (Lounsbury and Glynn 2001) has extensively discussed the cognitive and sociopolitical issues at stake in these processes. However, aside from the metaphorical use of political mobilization as a model, much of the literature on legitimacy in entrepreneurship ignores or takes for granted the role of politics and the state (Suchman 1995; Suddaby, BITEKTINE and Haack 2017). Historical studies on the creation and consolidation of research schools and disciplinary fields highlight instead the crucial role the political context may exert on these processes (for an example see Weingart 1999). What is more, as Bucheli and Kim (2014) highlight, the specific historical characteristics of state legitimacy highly affect, in turn, the legitimizing action that the state itself may exert toward entrepreneurial efforts of any kind. In particular, the role of the scientific legitimation of political power suggests the opportunity to explore the mutual relationship between emerging academic fields or research schools and political regimes.

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The case of the rise of Italian statistics as an independent scientific field in the early twentieth century, and the role that Corrado Gini’s research school had in it, here discussed, shows that a mutually reinforcing legitimation process took place between the emergence of Italian statistics and the consolidation of the fascist regime (Prévost 2009a). The claims of statistics and demography over appropriate scientific knowledge in Italy meshed with the ‘corporatist’ vision of the state and of the economy that was developed under fascism (Favero 2010) and with its pro-natalist population policy (Ipsen 1996; Treves 2001). Statistical classifications, defining what and who counted and how they should be counted, exerted a constitutive effect, reinforcing a particular political vision, and were in their turn legitimated by their official use in the fascist state. Statistics was also legitimated as a technical function of public administration, providing the state with the instruments to manage the Italian population, society and economy in a totalitarian perspective. In a centralized educational system, controlled by ministerial agencies, this allowed statistics to expand its academic scope by taking advantage of its political and administrative ‘usefulness’. Such a liaison obviously induced de-legitimation claims from home and abroad, and resulted in the reversal of the academic fortune of statistics in Italy after the end of the fascist regime (Favero 2011).

The above-cited historical literature on Italian statistics has duly highlighted the connection between statistics and politics in fascist Italy, yet the actual working of legitimation mechanisms requires further investigations to be fully understood. Did statistics have inherent features favoring its connection with interventionist and totalitarian regimes? Was it a matter of the peculiar evolution of the discipline in Italy? Or was it the cultural background of Italian politicians and civil servants that made of statistics a favorite technique to manage the problems of an emerging mass society?

The existing studies on elite culture and the development of social sciences in Italy show the presence of a growing nationalistic bias in the academic and scientific milieu since the years preceding World War I (Lanaro 1979; Patriarca 1996; Favero 2001). The active role of individual scholars and their political preferences in framing this peculiar feature of the new social sciences in Italy is also evident (Treves 2001; Prévost 2009b). This article focuses on the establishment of Corrado Gini’s research school, and its successful identification with Italian statistics. Gini’s biography offers the occasion to analyze in detail the process of legitimation of statistics as an independent discipline in Italy and the function it came to perform in the context of the fascist regime, providing some answers to the questions above.

The next two sections (2) define the concept of ‘reciprocal legitimation’ as a tool to provide historical cognizance to theories of legitimacy, and (3) identify the ‘research school’ as the unit of analysis, to be inquired through a biographical lens. The subsequent sections distinguish four different moments in Gini’s entrepreneurial endeavors, starting (4) with Gini’s role as a scholar in the definition of methodological statistics as an independent field. Italian statistics (5) was eventually identified with his research school and connected to the establishment of the fascist regime. Subsequently (6), following a gradual shift in fascist science policy and the emergence of alternative schools in Italian statistics, Gini started new methodological controversies at national and international level, trying to impose a realignment on his positions of Italian statisticians. The radical change in the political context following World War II (7) implied delegitimation of statistics as a politicized science, forcing Gini to reinterpret his scientific career retrospectively in a new light. Finally, the conclusions (8) summarize the historical findings and draw out the implications of this study for a theoretical
interpretation of mutual legitimation processes between scientific schools and state politics.

2. Historicizing legitimacy: the reciprocal legitimation of science and politics

The managerial literature has broadly defined legitimacy as the ‘generalized perception or assumption that the actions of an entity are desirable, proper or appropriate within some socially constructed system of norms, values, beliefs, and definitions’ (Suchman 1995, 574). Such a general definition has been developed in the last two decades from different perspectives, which identified legitimacy as a property, as a process or rather as a perception. Interestingly, the conceptualization of legitimacy as a property is criticized because of its a-historical essentialism, i.e. the assumption that legitimacy itself, the organization that is legitimized, and the social environment that legitimizes it ‘are stable, universal, and enduring properties’ (Suddaby, Bitektine and Haack 2017, 458). Theorizing legitimacy as a process offers instead the possibility to investigate the mechanisms of its social construction. However, despite a focus on contextual factors, this approach did not produce a generalized theorization of their role, leading to ‘a tendency to conceive processes of legitimation as heroic acts of institutional change’, somehow artificially dividing ‘actors’ from their ‘audience’ (Suddaby, Bitektine and Haack 2017, 462). A way out from this stalemate has been identified in a definition of legitimacy as a matter of perception of evaluators, whose individual judgments may be suppressed when perceived as not aligned with collective validity judgments, creating an illusion of consensus (Bitektine and Haack 2015). It would then be wrong to infer isomorphism at individual level from collective uniformity (Slater), as ‘hidden transcripts’ resist hegemonic discourse (Scott 1990).

This paper argues that, aside from this dynamic relationship between individual (micro) and collective (macro) judgments of legitimacy, further feedback mechanisms can be identified in legitimation processes, in particular focusing on the reciprocal relationship between different fields, organizations, and actors. These mechanisms are crucial in explaining not only the collapse of legitimation following the risk of a negative contagion (Suchman 1995, 597), but also the success of institutional entrepreneurship, as they can provide a stable legitimation to new initiatives.

Reciprocal legitimation has not been the subject of explicit consideration in the legitimacy literature. It is possible to define it as an exchange of mutual support that can emerge when an actor or entity is (part of) the audience of (part of) its own audience. The fact that ‘legitimation is frequently mutualistic’ was in fact highlighted since the early debate on institutional entrepreneurship, yet it was framed as the need to ‘act in concert’, to resort to ‘collective evangelism’ (Suchman 1995, 591–597), or to recruit ‘the help of subsidiary actors’ (Di Maggio 1988, 15) in order to succeed. The limits of such a strategic interpretation of reciprocal legitimation can explain why, in the legitimacy literature, institutional feedback loops and consequent path dependencies were for a long time poorly understood. However, even most recent theorizations introducing a multi-level perspective do not see the possibility of a reciprocal legitimization between different sources of collective validity, which are narrowly circumscribed to the media, the government, and the judicial system (Bitektine and Haack 2015, 51–52). Such conditions limit the scope of application of the existing theories on legitimacy and legitimation processes to present-day Western liberal-democratic societies.
The neglect of a wide range of variation in the political and institutional context denotes a lack of ‘historical cognizance’ (Kipping and Üsdiken 2014a) in the legitimacy literature, as Bucheli and Kim (2014) have shown. This in turn may explain the inattention to reciprocal legitimation processes, which develop historically, are contingent and related to specific periods, and display different features in different contexts. Perhaps only ‘historical approaches can provide institutional reflexivity to researchers who, like the phenomenon they study, may suffer from the paradox of embeddedness’, as the same Suddaby, Bitektine and Haack (2017, 470) argue. Historical rigor and the related attention to context, change over time, causality, complexity, and contingency may then result in a much-needed guard against shortcuts to theoretical development that lose sight of long-term processes and variables, sometimes undergoing unexpected surprises (Perchard, MacKenzie, Decker and Favero 2017).

Taking this perspective at heart, this paper makes the case of a reciprocal legitimation process involving a specific political and institutional regime (fascism and the fascist state) and an emerging academic research field (the ‘Italian school of statistics’) in interwar Italy. The role that quantification and quantitative methods played historically in the management of social conflicts and in the construction of legitimate forms of modern state has been broadly highlighted in the social studies of science and in the history of social sciences (Porter and Ross 2003; Rottenburg, Merry, Park and Mugler 2015; on statistics in particular see Woolf 1989; Hacking 1990; Porter 1996; Desrosières 2002). However, it has not been examined in the managerial research on legitimacy, which has instead highlighted the legitimating role of language and discourse through persuasion, translation, and narration (Suddaby, Bitektine and Haack 2017, 460). The above-cited literature in the history and sociology of statistics shows that it is clearly possible to argue that quantification itself has been, at least in the last two centuries, a particularly effective strategy of discursive legitimation, different from the other typologies identified by Vaara, Tienari and Laurila (2006). The same literature highlights indirectly the legitimating action that state power exerted in its turn on the scientific development of quantitative methods through the establishment of official technical bodies and the legal recognition of expertise, not to mention the role of the state in regulating the university system.

In this perspective, in interwar Italy we may identify the conditions for a pragmatic exchange between the rational credibility that statistical expertise could provide to the fascist state, and the support of the authoritarian state power to the institutionalization of an emerging national ‘research school’ in statistics. Such an exchange evolved into a procedural legitimation of statistically supported state decisions, and into a structural legitimation of the role of statistics in the education of state officials. The connection between Gini’s school of statistics and the fascist state thus ended being taken for granted, so much to persist despite the crisis of Gini’s personal relationship with Mussolini.

3. A biographical approach to the study of a research school

The concept of ‘research school’, as elaborated in the history of science (Morrell 1972; Geison 1978; Geison 1993), seems particularly fit to define the scientific entity in search of legitimation in the context under analysis here. Identifying the research school as the unit of analysis shifts the focus from theoretical debates to the institutional innovations in the organization of research work, and on the social context that makes the emergence of new scientific ideas
possible. However, its application to modern statistics and its political legitimation in a dictatorial context is not immediately obvious. As Secord (1986, 261) reminds, ‘a research school, like a discipline, is essentially a descriptive category for charting patterns of changing and contingent social relations. Although it cannot explain those patterns in a causal sense, it does suggest fruitful ways of exploring them’.

Most of the existing studies on research schools deal with the development of natural sciences during the nineteenth century. The peculiar political environment of liberal Europe and the complex connection of natural sciences with state policies may explain why such studies somewhat overlooked the role of politics in the legitimation of new fields of scientific inquiry.

Perhaps more importantly, the ‘research school’ model was developed on laboratory-based disciplines such as chemistry and physiology and, as Kushner (1993) and Geison (1993, 232) effectively observe, ‘locally constructed experimental systems’ seem to be constitutive of research schools.

In such a perspective, Gini’s entrepreneurial activism in establishing laboratories of statistics in Italian universities can be interpreted as part of his effort to build a research school in statistics (Prévost 2009a, 60–75). The same is true for his engagement in scientific controversies as instrumental to identify Italian statistics as a distinct research school (Prévost 2009a, 34–57; Geison 1993, 237; Turner 1993).

The focus on Gini as the leader of the main research school in Italian statistics under fascism seems then justified by his own strategic attitude and awareness. He was explicitly using the term ‘school’ to define Italian statistics as distinguished from other national traditions (Gini 1926a, 1939, 1965; Cassata 2006, 142–148). A last characterizing feature of a research school being in fact the role of its leader, it is also possible to argue that such an attitude was reinforced by the emphasis put by fascist rhetoric on leadership and charisma. So, even the bias toward the importance of the leader’s personal characteristics in the ‘research school’ approach seems to fit with the mutually reinforcing legitimation between fascist politics and Gini’s statistical school.

There is however in this approach an evident risk of interpreting the political legitimation of Italian statistics as the result of Gini’s sole strategic action, following the interpretation of the events provided in his own writings. The triangulation with independent sources, from other scholars’ letters and papers to the official records of national and international organizations, is crucial to put his role back into perspective. Finally, also the factum Gini presented during the purge trial in 1945, reinterpreting retrospectively his own career under the fascist regime, can be used to disentangle the emergent and strategic features of the reciprocal legitimation process between Italian statistics and the fascist regime.

Avoiding getting stuck into the shoes of her subject, the historian can thus make of a biography a moving point of observation on research politics in a specific context. The life of an academic entrepreneur may be used as the narrative thread highlighting the complex interactions that make possible entrepreneurial processes of change.

As Latour (1993) has shown in his book on Louis Pasteur, a leading scholar can be interpreted as an effect, rather than a ‘prime mover’ (an actor), of the strategies, arrangements, and mobilizations of different entities into a network. However, actor–network theory is far from suggesting that personal agency is not relevant (Latour 2005). Clearly it is relevant, and yet the ability to connect, and to recognize the connection, takes here the place of doing everything – leading, managing, and creating. In this perspective, collective or distributed
agency becomes the actual source of change, in a process that becomes visible only at a microanalytical level. A similar idea of ‘interstitial’ entrepreneurship is suggested by the microhistorical reflection on the uses of biography. As Levi (1989, 1334) has remarked, ‘there is a permanent and reciprocal relationship between biography and context; change is precisely the endless sum of these interactions.’ In its turn, the unpredictable variety of individual choices is the result of the inconsistencies and contradictions between different contexts, which authorize the multiplication and diversification of practices. As Seo and Creed (2002) suggested from an institutional perspective, the friction between the different logics in which an actor is embedded opens the way to a diverse range of possible choices, which have in their turn the potential to change these environments by putting them in relationship.

Gini’s entrepreneurial role emerges in fact at the intersection of different fields, as science and academia, ideology and the state, business and the economy, the national and international context, where the function of knowledge translator, information intermediary, and power broker are mixed together. The comparison between sources originating from these different contexts is crucial to put into perspective Gini’s own narrative and to use his biography to highlight the interactions between science and politics at different levels.

4. Defining a discipline: Italian methodological statistics

The first Italian scholar to define statistics as ‘a branch of logic, namely a method’ was Rodolfo Benini (1901, 10). In his words, methodological statistics was a ‘form of observation and induction appropriate to the quantitative study of phenomena that appear as a plurality or mass of cases’ (Benini 1906, 1). Today readers are acquainted with such a definition as obvious. Yet in the late nineteenth century, in Italy as in large part of other countries, the dominant opinion among scholars was that statistics should be rather classified as a social science, focusing on the identification of laws or regularities in the development of human societies. Such a definition of the discipline fitted with its position inside of the university programs in law, where public officials were usually trained.

The new methodological approach determined a different articulation of the teaching of statistics: new textbooks started neglecting more and more the traditional historical and institutional approach to deal with technical problems of data observation, collection, elaboration and analysis, and especially with the correlation and regression methods that were being developed by British mathematical statistics. The knowledge and use of mathematics emerged rapidly as a discriminating element between the ‘old’ statistics (which resisted inside of university degrees in law) and the ‘new’ approach (which found better reception in the schools of commerce). Very few of the Italian statisticians educated before of the ‘methodological turn’ could understand mathematics. Benini was himself an exception because of his experience inside of the central statistical office.

Only with World War I a ‘new generation’ of statisticians emerged. They were trained in the new methodological and mathematical methods, but also in a changing cultural and political environment. The presence of distinctive generational features was crucial in framing the identity of Italian statisticians as a community and in legitimating their ambitions. They largely adhered to the positions of the nationalist movement, and were able to take on roles of technical responsibility within the administration during the war. This allowed them to demonstrate the effectiveness of their specific quantitative skills not only in the management
of the war economy but also in developing useful arguments for postwar diplomatic battles (Prévost 2016; Beaud and Prévost 2012, 133–152). The technical success of their engagement during the war was a first important step toward the legitimation of statistical expertise. However, the war also reinforced a specific pattern of relationship between Italian statisticians and the state, defining the usefulness of their discipline in terms of the contribution of its applications to the national interest. The same happened in other fields, as for instance in applied mathematics. Mauro Picone, later a colleague of Gini at the university of Rome and the founder of the National Institute for Computing Applications, stated in his autobiography that the war experience as an engineer in charge of elaborating new ballistic tables convinced him that ‘mathematics is not only beautiful, it can be useful as well’ (Picone 1972, 8).

In this way, during and immediately after the war, new scholars emerged who dominated the field of statistics until World War II: among them, Costantino Bresciani Turroni, Giorgio Mortara, Riccardo Bachi, Livio Livi, Marcello Boldrini, Felice Vinci, and especially Corrado Gini, together with his many students. In 1926, Gini was put at the head of Italian official statistics, which the fascist government renovated with the establishment of the Central Statistical Institute (Istituto Centrale di Statistica, from here on Istat). This appointment marked the official recognition of his leading role in Italian statistics, both at scientific and academic level. In fact, Gini more than anyone else had contributed in the previous two decades to legitimize the specificity and autonomy of statistics as a discipline in Italy.

After Benini’s foundational definition of ‘methodological statistics’ as based on mathematics, Gini’s ‘methodological’ contributions on probability (Gini [1911] 2001a) and on the concentration indices (Gini [1911] 1922) were indeed the most scientifically relevant in the field. As Prévost (2009a, 34–57) has shown, the strategic goal Gini was pursuing with these articles was the delimitation of a specific disciplinary field for statistics, in spite of its use of mathematical tools and of their application to the traditional subjects of other social sciences, first of all of economics.

The marginalist revolution in Italian economics had been at first associated with the development of methodological statistics. New statistical methods were discussed in the same journals and collections where marginalist economists published their articles. This link was made evident in the third series of the Giornale degli economisti, started in 1911 under the direction of Giorgio Mortara and Gustavo Del Vecchio, which added the subtitle Rivista di statistica. An emphasis on quantitative methods also distinguished the Italian economists that in the following decades grouped in Milan around the Bocconi University and the Banca Commerciale Italiana.

In such a context, the efforts Gini undertook since 1908 to create a specialized journal of statistics (finally accomplished in 1920 with Metron: see Cassata 2006, 89–96) can be interpreted as part of an effort aimed precisely to emancipate statistics from its role of a sophisticated instrument of economic theory. The creation of specific editorial instruments was an important step in the process of legitimation of the field. At the same time, the control of the new specialized publication outlets was crucial to establish a research school under Gini’s leadership.

Gini explained in a letter the strategic meaning of his scientific project. In 1910, comparing his own work on the concentration (or inequality) index with Pareto’s distribution law, he stated that his own aim
was perhaps a little wider [than Pareto’s], as I did not limit my inquiry to economic phenomena, but much more modest: I intended only to search for formulas that were useful in practice to study the concentration and the mutual dependence of phenomena, without any pretense that they could describe the variations with great precision in all cases. (letter of Gini to Vladimiro Furlan, September 1910: Pareto 1975, 2, 704–705)

In that same article on concentration indices, Gini precisely identified the limitations that affected the most advanced methods of mathematical statistics and hindered their application to the study of economic and social phenomena. In his opinion, the use of Bravais’ correlation and Galton’s regression coefficients required ‘a so detailed knowledge of the studied phenomena that is not always available’. Hence, the need for ‘indices of the distribution of quantitative phenomena and of their mutual relationships that are sensitive enough but may be applied to conventional statistical data avoiding arduous calculations and assumptions that are far from the real world’ (Gini [1911] 1922, 4–5; Giorgi 1992).

This way, Gini started taking distance not only from marginalist economics, but also from its preference for the probabilistic and inferential methods of ‘Anglo-Saxon statistics’. He identified an alternative solution in the (neo-)descriptive approach that qualified the Italian contribution to statistics, as he would argue in a lecture held at the London School of Economics (Gini 1926a). In that occasion, he proposed a consistent story of the recent evolution of statistical studies in Italy that legitimized the dominance of his research school, at the same time suggesting a national, if not nationalistic, interpretation of scientific positions and debates. The theorization proposed by Gini exerted a crucial role in legitimizing the specific features of statistical research as developed in Italy against existing alternative models.

Gini’s (1926a, 707) idea of ‘statistics with the least mathematical means possible’ was in fact clearly opposed to Fisher’s (1925, 1) definition of the discipline as ‘a branch of applied mathematics’ (Cassata 2006, 143–144). He was thus able to mark the boundary between the ‘empirical’ use made of probability by statisticians versus the ‘abstract’ and deductive nature of mathematical probability. At the same time, however, as a ‘method’ based on the use of specific mathematical tools, statistics could boast a higher scientific rigor against applied disciplines, in particular against social sciences with a weaker definition, as sociology. The occupation by statisticians of these areas of study in Italy, and an extension of statistical applications to a wider range of disciplines followed.

5. Legitimating Gini’s research school

The new relationship between methodological statistics and its empirical applications implied the need for a greater division of scientific work and for a more structured organization. With this aim, Gini himself promoted since prewar years the establishment of university laboratories and institutes of statistics in Cagliari, where he took tenure in 1910, then in Padua from 1913 and in Rome since 1926. Inside a laboratory, professors designed the methodology of research, assistants interpreted the results, and students collected and elaborated data. The model for such an organization of research work was derived from the Laboratory of Political Economy established by the economist Salvatore Cognetti De Martiis in 1893 in the University of Turin, in the premises of Cesare Lombroso’s Laboratory of Forensic Pathology (Prévost 2009a, 64–65). The ‘mathematical turn’ in economics and then in statistics fostered the imitation of the laboratory research methods and practices of natural sciences. The
creation of the two dominant ‘research schools’ of marginalist economics and methodological
statistics was in part an effect of these institutional dynamics. The emphasis that fascism put
on the practical applications of science was also favoring the adoption of the university
laboratory as a model for the organization of statistical research. The laboratory made pos-
sible to connect a focus on methodology with visible results attracting external supporters
and stakeholders. Examples of external collaborations multiply in 1926, when Gini was
appointed as the president of the Higher Statistical Council (Consiglio Superiore di Statistica,
from here on CSS) supervising the Istat, and moved from the University of Padua to the
University La Sapienza in Rome. In the same year, the national industrialists’ association
(Confederazione nazionale degli industriali, Confindustria) commissioned the publication
of the Indici del movimento economico italiano to Gini’s Institute of Economic Policy at La
Sapienza. Confindustria paid for the publication and the elaboration of the data it provided,
following the scheme of the collaboration previously reestablished with the Laboratory of
Statistics at the University of Padua. The first volume of the Indici was in fact completed in
Padua, as Mario Saibante wrote to Gini in a letter reporting on the situation in the Laboratory
that Gini had left quite abruptly after his appointment in Rome (ACS, Fondo Gini,
Corrispondenza, b. 7, Mario Saibante, 25 February 1926).

The contract with Confindustria made available to the laboratories of statistics in Padua
and Rome a flow of financial resources that was decisive to develop their activities through
the acquisition of books, maps and mechanical and electrical calculators (on Padua, see
Pietra 1943). But commercial gains were not the main benefit of this relationship: student
placement and political support were certainly more relevant. Some of the students working
in Padua and Rome became part of the statistical staff of Confindustria and its sister associ-
ation of joint-stock companies (Assonime): Saibante became himself the director of the
Assonime’s statistical office in 1927, and of Confindustria’s survey and study office in 1936
(Barberi 1958).

The collaboration with Confindustria is mostly interesting as it shows how the reciprocal
legitimation between Italian school of methodological statistics and the fascist state may
be crucial to attract the consensus of other actors in a process that the legitimacy literature
broadly labels as ‘constituency building’ (Bitektine and Haack 2015, 59). For the fascist regime
in the mid 1920s, the support of industrialists was decisive, and Gini found himself to be at
the same time the ‘statistician of confidence’ of both the industrialists and the Duce.

He was in contact with Confindustria since before World War I, as his brother, the engineer
Aldo Gini, had a role in the establishment of the industrialists’ association. One of his pupils
in Cagliari, Giovanni Dettori, was also collaborating with the association. In the early postwar
years, Gini (1923) published an article in the association’s journal arguing against the esti-
mation of real industrial wages resulting from the statistics of the National Fund for Industrial
Accidents (Inail). Mortara (1922), as a member of the Inquiring Commission on Industry, had
used these data to argue that the average monthly wage for industrial workers did not grow
after the war as much as the industrialists were claiming on the basis of the nominal increase
in hourly wages.

In the same years Confindustria was financing the elaboration by Gini’s statistical labo-
rary in the University of Padua of the data on workers, worked hours and hourly wages
collected from the accounting books of its associated businesses. These data, and Gini’s
(1923) detailed methodological discussion of the faults of Inail statistics, became politically
strategic in 1926, when Mussolini decided the stabilization of the Italian lira at ‘quota 90’, i.e.
90 liras per pound sterling, re-evaluating it much further than what Confidustria advocated. In order to obtain the association’s support to this deflationary move, the fascist government needed to assure the industrialists that it would apply wage cuts reducing nominal labor costs in due proportion. The assessment of real wages should thus be based on data provided by Confidustria itself in order to fit with the ‘superior needs of production,’ as Gini himself (1926b) argued in an international publication. Their official level should then be calculated comparing hourly wages in industrial businesses with a measure of the cost of living taken from factory outlets. Following these considerations, in 1928 Confidustria started publishing a monthly series of the hourly earnings of industrial workers in its Bollettino di notizie economiche. Gini’s Istat would then use these data to calculate an official index of wages that was published from 1930 in the Bollettino dei prezzi and in the Bollettino mensile di statistica (Favero 2010, 321–330).

The statistical measurement of prices and wages was a critical stake in the political governance of the Italian economy: since 1926 fascist unions, put under the government control, approved a series of wage cuts based on technical considerations deriving from official data. During the 1920s, statistics played then a fundamental political role in settling the power relationships between the fascist regime and Italian industrialists. Gini’s scientific authority, and the work performed by colleagues, assistants, and students in the university laboratories in Padua and Rome, was instrumental to such a settlement. His double allegiance contributed to make of him the ideal candidate to lead the reorganization of Italian official statistics when the Istat was established in 1926. The reorganization of official statistics was part of the political and administrative authoritarian transformation of the state organization devised by the newly established fascist regime. In the trade-off between legality and efficiency (Melis 1988), fascism neatly privileged the second in order to gradually reinforce the power of central state authority and its control of population and economic dynamics. This entailed the multiplication of new autonomous institutes directly supervised by the government and by scientific advisory councils, following a model that had been already experimented in the prewar years for some technical bodies (Cassese 1981; see also the essays in Varni and Melis 1999).

At first, then, the Istat was put under the political authority of the head of government and under the scientific authority of the CSS, which brought together university professors of statistics, ministry officials, and representatives of trade associations. Gini was in fact appointed as its chair. In 1929, however, in a further step toward centralization, he was put at the presidency of the Istat under Mussolini’s direct authority, while the CSS was maintained as an advisory body. From such a position, Gini’s authority reverberated on academic dynamics, fostering the expansion of statistical teaching and the dominance of his research school inside of the discipline.

During the 1920s, the academic position of statistics as a discipline was also affected by the university reform introduced on September 1923 (Royal decree 2102) by the Minister of Public Education, Giovanni Gentile. This deregulated the organization of university studies, allowing an expansion of the applications of methodological statistics to a wider range of fields, but making the course of statistics optional in the Faculty of Law, where it was traditionally placed as compulsory (Gini 1926a, 704). After an ephemeral multiplication of statistical courses within scientific programs, statisticians were able to consolidate their presence in the newly established Faculties of Political Science (Alvazzi Del Frate 2000), in this way confirming their traditional attention for the education of civil servants.
The establishment in 1927 of undergraduate biennial schools of statistics in Padua and in Rome can also be interpreted in this same perspective. In 1930, the diploma they issued became a legal requirement to be hired in the statistical staff of Italian public administrations (Gini 1926a, 704; Favero and Trivellato 2011, 43–44). Gini as the president of the Istat actively promoted such a measure as a tool to ensure a standardization of procedures and methods in the production of statistical data through the selection of an educated and specialized staff.

At the same time, the Istat was unsuccessfully trying to impose such standards through an active coordination of the statistical work performed inside of different administrations, or through their advocated centralization (Beaud and Prévost 1997, 441; see as an example D'Autilia 1999). Institutional conflicts followed, leading Gini to ask for the support of Mussolini and, this failing, to finally resign in 1932 (Leti 1996, 150–151). A detailed reconstruction of the growing difficult relationship between Gini and the Fascist government administration is provided by Cassata (2006, 92–101).

Interestingly, Gini would declare in 1945, in the defense report against his purge from an academic body, that he ‘resigned in 1928, considering as accomplished the mission he undertook to rebuild the organization of Italian statistics, yet his resignation was accepted only in 1932’3. Gini’s authoritarian management of the Istat had in fact led many distinguished scholars as Livio Livi, Ugo Giusti and Guglielmo Tagliacarne to leave the institute in 1928, questioning his scientific legitimacy as the main representative of Italian statistics. Conflicts between Italian statisticians in the late 1920s corresponded to the emergence of different scientific positions. These found expression in the creation of different local ‘research schools’ based in university laboratories and publishing their own statistical journals. Livi established in 1929 in Florence the Barometro economico italiano, which adopted the Harvard approach to economic forecasts, against which Gini (1926c) had explicitly taken position. In the same year, Felice Vinci established in Bologna the Rivista italiana di statistica, which soon added corporatist economics and finance to its title, and Luigi Amoroso and Alberto De Stefani as editors, finally becoming the Rivista italiana di scienze economiche (Prévost 2009a, 72–75).

As Gini’s political influence declined after his resignation from the Istat in 1932, divisions emerged also at the academic level. Gini and his pupils privileged a ‘methodological’ approach, in the framework of an ‘organicist’ view of social sciences (Favero 2004). Other statisticians were more interested in the autonomous development of quantitative methods suitable for different applied disciplines, from quantitative economics to demography. A debate launched in 1935 on the Barometro economico italiano on whether to establish an association of Italian statisticians made these different visions of the discipline explicit (Tagliacarne 1935). They implied also a different way of conceiving the relationship between academic statisticians and the practitioners who worked in private and public institutions, from insurance companies to the research centers of banks up to the Istat. Indeed, the problem of defining the relationship between official statistics and the future association emerged as the main obstacle to the constitution of the latter in these years (Leti 1990, 38–67; Cocchi and Favero 2009, 216).

6. Fascist policy and Italian statistics

The relationship between basic science and the practical application of scientific knowledge in Italy became during the early and mid 1930s the matter of conflict and disillusion in many
disciplinary fields. The political attention for technical improvements and efficiency, together with the substantial public investments in applied research during the 1920s, had lured many scientists into a collaboration with the fascist state (Maiocchi 2003). The support was however reciprocal, or mutual, as argued here and already suggested in other terms (Saraiva and Wise 2010).

Most of the political campaigns (or ‘battles’, as they were defined at the time) that fascism started, from autarky to pronatalism, implied in fact an involvement of scientific expertise. This triggered processes of more or less intense reciprocal legitimation between individual ‘research schools’ and the regime. One example is the mobilization of Nazareno Strampelli’s plant genetics in the context of the ‘battle for grain’ and national self-sufficiency (Saraiva 2011). The antimalarial campaign even offered political support to discredited scientific positions with tragic outcomes, as in the experiment conducted by Giacomo Peroni and Onofrio Cirillo with toxic mercury therapy on hundreds of peasants during the late 1920s (Snowden 2009, 143–146). Less horrific but perhaps having longer term consequences was the clear preference of the regime for the practical applications of science rather than for the development of fundamental research. Following Mussolini’s impatience with the long procedures of scientific inquiry, Alberto Missiroli, the leading Italian malariologist, declared that ‘the need today is to extend the practical application of the knowledge that we already have rather than to broaden what we know’ (Missiroli 1929, 118). As the disappointment of the representatives in Italy of the Rockefeller Foundation confirms (Donelli and Serinaldi 2003), such a position discouraged the investments in medical research on malaria, dispersing the competences accumulated in Italy in the previous decades (Snowden 2009, 177–180).

The problem of a scarce and episodic attention of the fascist state for basic research applies to almost any scientific field, from mathematics to chemistry. The case of the development of applied mathematics in the interwar years (Nastasi 2006), when no university appointment was made in algebra, is an example of this attitude (Tazzioli 2011, 412). The attempt to establish a stronger relationship between chemical science and industry for the production of new materials in the context of autarky generally failed because of the lack of investments in fundamental research (Cerrutti 2001; Ciardi 2011, 453–454), yet much depended from the actors involved. Whereas Nicola Parravano (1936), the main representative of Italian chemistry at institutional level, was a promoter of the priority of practical applications, the physicist, senator and former minister Orso Mario Corbino was able to obtain funds for basic research too. He not only established the first chairs in theoretical physics, to which Enrico Fermi was appointed (Bordoni 2011, 431), but also launched a research program that was soon to produce important results at international level (Battimelli, Paoloni and De Maria 2001).

Some of the scientists involved in the making of science policy in fascist Italy were then able to convey state funding to theoretical research through the promotion of large-scale applied projects that attracted the interest of the regime. In this way, they made up for the lack of interest of the fascist government for the long-term perspectives in the development of scientific knowledge. At the same time, they enjoyed of an unprecedented authority and power in their scientific field, becoming in fact the brokers of the relationship of reciprocal legitimation that was established between fascism and some scientific research schools. The case of Gini is an excellent example of this process. As seen above, his ability to connect in an organic system the role of methodological statistics, its applications to demography,
economics and sociology, and the creation of new institutions was crucial in explaining the success of his research school.

Yet this success was questioned in the early 1930s, following a change in the regime attitude toward science. The vicissitudes of the National council of research (Consiglio Nazionale delle Ricerche, CNR) shed light on the national and international implications of this change, and can explain the shifting position of leading scientists as Gini (Paoloni and Simili 2001).

From the inter-allied scientific collaboration during World War I originated the establishment of the International research council (IRC) in 1919. This was created as a double federation of general national scientific councils and of disciplinary international scientific unions, aggregated by field in international councils. Political instability and the opposition of part of the university academia prevented the establishment of the CNR in Italy until 1923, when Vito Volterra was elected as its president. In the place of Volterra, following the fascist reformation of the CNR in 1927, Mussolini appointed Guglielmo Marconi, the famous inventor of radio-telegraphy and entrepreneur. This choice was the expression of the anti-academic turn of the regime, and made explicit a focus on applied science and technology. From the standpoint of the fascist state, the new CNR was instrumental in modifying the trend in university research toward applications of national interest. However, its scientific legitimation still derived from the participation of the main Italian scientists into its disciplinary national committees, which were being established and becoming members of international scientific unions (Paoloni 2011, 191).

It was at this time that Gini attended the first world congress on population in Geneva in 1927, where the International Union for the Scientific Investigation of Population Problems (IUSIPP) was created as part of the Social Science Research Council (SSRC), one of the disciplinary international scientific unions. Gini established in 1928 the Italian committee for population studies (Comitato Italiano per gli Studi di Popolazione, CISP), and became one of the three vice-presidents of the IUSIPP, under the presidency of the American biologist Raymond Pearl. In a 1928 meeting of the IUSIPP in Paris, Gini was able to fix its first congress in Rome in 1931. Yet doubts on the possible politicization of the meeting and the evident conflict between the then dominant neo-malthusianism and the natalist positions of Italian, French, and German members of the IUSIPP convinced the president of the SSRC, the American statistician Edwin B. Wilson, not to finance the congress in Rome. Pearl then moved it to London, the CISP exited the IUSIPP, and Gini organized in 1931 an alternative congress in Rome with the participation of scholars from Germany, France, and the United States (Cassata 2006, 26–33).

Again, Gini’s move paralleled the exit in 1931 of the Italian CNR from the International Council of Scientific Unions (ICSU, as the IRC had been renominated). The CNR underwent a new reformation that extended its powers and made of it the highest technical council of the state, and the pivot for the intended creation of a military–industrial complex of research (Paoloni 2011, 194, 201). The decision to discontinue the membership in international scientific institutions was justified as the result of the failure of the scientific internationalism of the 1920s. Following these events, Gini pushed forward his idea of an ‘Italian school of statistics’ as an example of ‘fascist science,’ in which science and politics found a synthesis. As scientific theories followed national interests, a properly national science should be elaborated to support population and economic policies (Gini 1931).
The CISP maintained the financial support of the state until Gini’s resignation from the Istat in 1932. However, budget cuts were soon after made following Mussolini’s disappointment for the results of the population policy inspired by Gini. Later in the 1930s, the racist turn in fascist demographic policy, which accompanied and followed the Ethiopian War, enhanced the divisions among Italian statisticians and interacted with their international relationships.

In 1934 Gini’s committee was finally able to launch *Genus*, a journal of demography and eugenics sponsored by the CNR, where Gini found the support of the vice-president Amedeo Giannini, a senior official of the Foreign ministry. In 1935, however, Livio Livi attended the congress of the IUSIPP in Berlin, was nominated as one of its vice-presidents, and was officially authorized to accept the position in 1936 by the Italian Government. As a consequence of this, in 1937 a new Advisory committee for population studies (Comitato Consultivo per gli Studi di Popolazione, CCSP) was established as the new national reference for the IUSIPP, in which German scientists had a prominent role (Bertaux 2002, 237–250). Under the directions of the next congress of the IUSIPP, held in 1937 in Paris, the Italian Government put Livi’s CCSP in charge of studying the Italian population to assess the viability of a new population policy on imitation of the German model, which included economic incentives and racial selection (Treves 2001, 331–334). In the same year, after Marconi’s unexpected death, Mussolini appointed the military chief of staff Pietro Badoglio at the presidency of the CNR, clearly marking a further turn toward the engagement of scientific research with autarky and possibly a war on the side of nazi Germany (Maiocchi 2003, 261–264). In the following years, measures providing economic support to families and marriage were introduced in Italy together with both ‘preventive’ and ‘repressive’ eugenic measures that culminated with the laws on the racial discrimination of Jews in 1938 (Treves 2001, 260–274).

In 1938, the CCSP became the Italian society of demography and statistics (Società Italiana di Demografia e Statistica, SIDS), which gathered many scholars active in applied statistics around Livio Livi, Felice Vinci and Franco Savorgnan, Gini’s successor at the head of the Istat. In response to this initiative, in January 1939 a group of pupils of Gini established the Italian society of statistics (Società Italiana di Statistica, SIS) (Leti 1990, 67–68). The conflict between two ‘research schools’ and their competition for political support seemed the main motive for the formal establishment of the two associations. Competition overcame the obstacles that made it impossible in 1935 to establish a single association.

The founding group of the SIS gathered around the *Supplemento statistico* to the journal of corporative studies *Nuovi problemi di politica, storia ed economia* (*NPPSE*). The latter had been established in Ferrara in 1930 under the direction of Nello Quilici and with the support of Italo Balbo, at the time Minister of Aviation and later governor of the Libyan colony. The *Supplemento* started its publications only in 1935, and collected contributions from scholars of the universities of Ferrara and Padua. Gaetano Pietra, Gini’s long-time assistant and successor in Padua, dean of the Faculty of Political sciences from 1939 to 1941, was the director of the *Supplemento* and became the first president of the SIS, which established its seat in Padua (Pietra 1939; Cocchi and Favero 2009, 226–233).

The association could enjoy the support of Gini’s connections with the CNR, the Assonime, Confindustria, the National insurance institute (INA), and the Bank of Italy, yet was mainly financed by the University of Padua and the local Council of corporations (as fascism redefined the former Chamber of commerce). The entry of Italy into the war in June 1940, the death of Nello Quilici together with Balbo in Libya, and the subsequent closure of the
NPPSE offered Gini the opportunity to take direct control of the SIS and move its seat to Rome.

Gini found then in the association the ideal forum to revive the debate on statistical methods in contrast with the most recent developments of ‘Anglo-Saxon’ mathematical statistics (Gini [1939] 2001b, [1943] 2001c). He started a scientific polemic against the inferential use of significance tests and confidence intervals proposed by Ronald Fisher and Jerzy Neyman (together with Karl Pearson) in their articles, which were the basis of sampling procedures. Gini used a Bayesian argument to dismantle the reliability of statistical inference, making reference to the fallacy of ‘drawing from the inconsistency of an observation with the null hypothesis an argument in favour of the hypothesis one champions’ (Prévost 2016, 160–161). As Prévost has shown, this ‘controversy’ (as Gini saw it) was the final outcome of the ‘strategic’ use that Gini made of methodological debate, which during the war took on a heavier nationalistic flavor. Gini in fact recruited into his new ‘project’ different scientific positions, including the (neo-)descriptive approach he had defined as specific of Italian statistics (Gini 1926a; 1939), the skepticism about sampling methods that was widespread among official statisticians (Beaud and Prévost 1998), and the subjective, neo-Bayesian definition of probability formulated by Bruno de Finetti, of which he did however not fully appreciate the innovativity (Piccinato 2011; Giorgi and Gubbiotti 2016, 5). The idea of a conflict between national ‘schools’ undoubtedly contributed to make Italian statisticians suspicious toward statistical inference. Such an attitude would last well beyond the end of the fascist regime (Cassata 2006, 147), and hindered the adoption of sampling methods until the 1950s.

The ‘autarkic’ turn in Italian statistics described above went together with the closure in the late 1930s of most of the university courses in statistics that had been opened in the 1920s. The centralist reorganization of university programs, introduced in 1935 by the minister Cesare De Vecchi, discriminated between mandatory and optional courses, at the same time reducing the total number of courses. Statistics was erased in most of the programs, except for the ones in Political science and in Law, where however it was optional. It remained mandatory in the new programs in Economics resulting from the transformation into university faculties of the former institutes or schools of commerce.

In 1936 Gini replied by establishing the new Faculty of Statistics at the University of Rome, resulting from the merger of the mathematical School of Statistics and Actuarial Science and his Institute of Statistics and Economic Policy. The new faculty resulted from an academic alliance with financial mathematics and probability theory, involving in particular Francesco Paolo Cantelli and Guido Castelnuovo. The establishment of an autonomous faculty assured the reproduction of academic staff in statistics. Yet such a move strengthened the hierarchical relationship between the statistical method and its applications, thus confirming a privileged connection between statistics and the social sciences that in theory the methodological approach had made obsolete.

Even after his resignation from Istat in 1932, Gini went on supporting any possible synergy between the university training of statisticians, the recruitment of technical staff in the civil service, and the organization of official statistics. Such an effort was part of a strategic project aiming at making the applications of methodological statistics essential for the construction of a corporatist and totalitarian system for the control of the population and of the economy, which the fascist regime was trying to build in the late 1930s. Gini’s pupils and associates made such intentions even more explicit with the project of a ‘corporatist statistics’ (Pietra 1934; Fortunati 1936). The increasing politicization of the discipline was made evident when
new courses of ‘applied’ statistics were made mandatory in all the universities of Italy following the racial laws. A new generation of young statisticians thus entered then the university teaching courses of ‘Comparative demography of the races’ or ‘Colonial sociology’ (Cocchi and Favero 2009, 222–223).

7. Coping with delegitimation, in retrospective

This evolution somehow established the conditions for the decline of Italian statistics in postwar years. The involvement of Italian statisticians in the ideological and political choices of the fascist regime stirred a reaction of postwar governments and of the same university system. Abruptly, what had been a major factor of legitimacy became an element of delegitimation, starting a process in which the ‘successes of the past become impediments to the future’ (Suchman 1995, 597). A paradoxical result of the postwar ostracism against statistics was not the expulsion of most statisticians from Italian universities. Given the outcome of the purge process in Italy (Woller 1997), they maintained their place. Yet no more statisticians were appointed for some years, in this way reinforcing the academic dominance of the elder generation at least until its retirement.

At the same time, in official statistics the introduction of sample surveys and the reorganization of data collection to fill in the framework of national accounting were imposed to Italy as a condition to participate in the European Recovery Program. In the postwar decade, statistical innovation in Italy was then led by official statistics, while the new methods continued to stir suspicion and widespread resistance among university statisticians, following the lasting influence of Gini’s positions and the intransigence of some of his pupils (Favero and Trivellato 2000, 269–280).

Gini himself insisted, even in the late 1950s, on a reactionary view of modern progress as an actual decadence (Cassata 2006, 183–188). The connection he established between sampling, decolonization, and decadence is evident for instance in his claim that

Giving up the effort towards a complete and quantitative knowledge of phenomena, which was once the aim of statistical surveys, we have accepted the expedient of sampling, this way conforming to the backward conditions of the underdeveloped countries to which such surveys were extended. (Gini 1959, 1141)

In this perspective, Gini’s rearward battle in the 1950s for the revitalization of the International institute of sociology against the International Sociological Association established by the United Nations Educational, Scientific, and Cultural Organization (Unesco) can be interpreted as part of his last project for the construction of a reactionary social science, which Francesco Cassata (2006, 194–213) has reconstructed in detail.

Yet interpreting such a project as the result of a coherence of Gini’s political and scientific positions would be misleading. When Rome was liberated by the American army in June 1944, Gini started in fact elaborating a new role for himself and his competences in the frame of the new context. In a new book on ‘post-war problems’ (Gini 1944), he resumed from some previous articles the idea of the United States as a ‘laborist society’, built with the force of European immigrants (Gini 1940). In the new context, his proposal was a geopolitical and socio-biological reunification of the United States and Europe as an alternative to Asian and African immigration. This idea found political realization in the charter of the Unionist Italian Movement that he wrote (Cassata 2005). However, in the first elections after the war on June 1946, the movement did not succeed to elect any representative in the Constituent Assembly.
What is more, the Italian political situation remained uncertain, as the communist and socialist parties remained part of government majority.

This situation, and the suspension from teaching that followed the start of a purge trial against him in November 1946, pushed him to explore the possibility to emigrate. During 1945 he wrote to Henry H. Sonnabend at the University of Witwatersrord in South Africa, to F.W. Nichol at IBM and to Alfred J. Lotka at the Metropolitan Life Insurance Company in the United States, to Gunnar Dahlberg in Sweden and to Benoy Sarkar at the Bengali Institute of Economics, asking information and support to find lecturing or research jobs (Cassata 2006, 149–151).

At the same time, he was mobilizing the members of his widespread research school to collect signatures in support of his defense from university colleagues in Rome, Naples, Milan, Padua, and Bologna. Gini’s defense strategy followed two main lines, as highlighted by Cassata (2004, 89–90). The first, trying to identify as apolitical his scientific contributions, was evidently falsified by many of the statements that were included in such contributions. The second line was to argue that he was collaborating with the regime as a technician but remained loyal to his scientific principles. This position somehow reflects his genuine belief and his peculiar concept of science. In the end, he was able to provide a ‘normalizing account’ (Suchman 1995, 597–599) that allowed a partial re-legitimation of his research school in postwar Italy, and the restoration of large part of his academic power. The writings of his pupils in a collective volume published in his honor demonstrate the allegiance to this account of a widespread network of ‘Ginian’ scholars both inside the university and official statistics (Castellano 1960). Even the most recent celebrations in the 50th anniversary of his death repeat the arguments of Gini’s defense (Giorgi and Gubbiotti 2016, 7–9), despite many historical studies that have put them into critical perspective (Cassata 2004, 2006; Prévost 2009a).

What is interesting here is the way Gini was able to re-frame his whole career as a scientist, making of this idea the thread connecting disparate episodes. Gini reinterpreted the strategic relationship of reciprocal legitimation between science and politics that was highlighted above into an instrumental collaboration. In this way, he not only bent the events to his purposes, but also highlighted some aspects of his attitude that were crucial to his own identity as a scientist. It is worth then to cite some examples of the considerations he made from a document he presented against the purge from an academic body, as he repeated the same statements in most of the papers he produced in his defense.

On his relationship with the fascist party, Gini alleged that his party membership was assigned him honoris causa without consulting him, this way declining any responsibility. He also mentions his successful efforts to retain at the Istat several employees who were not members of the party or even were renown as antifascists, opposing the tentative politicization of the institute. The reconstruction of the events of 1930 and 1931 proposed by Cassata (2006, 92–101) confirms his assertions. A defense of the technical and scientific autonomy of the Istat from political interference was in fact coherent with the strategy of reciprocal legitimation he was pursuing between the Italian school of statistics and fascist politics. This may in fact work only as long as the two poles remained independent. In the same perspective, it is possible to interpret Gini’s (1942) article criticizing the appointment of university professors by the Ministers of Education without competition.

Gini had also an easy game in demonstrating that his opinions on population policy were different from the measures adopted by the fascist regime during the 1930s, that he did
not stick to the anti-Semitic policy and dissolved the board of the journal *Metron* rather than expelling its Jewish members,\(^9\) that he was against the entrance of Italy into World War II,\(^10\) and that he offered protection to colleagues and employees during the Nazi occupation.\(^11\)

However, the tentative representation of his conflicts with the regime as a proper opposition to fascism becomes unrealistic when he describes his minority report in the Commission for the revision of the Constitution as a defense of parliamentary democracy,\(^12\) denying his radical aversion to the latter (Prévost 2001). In the same way, his opposition to the war did not prevent Gini (1941) from attacking the weakness of democratic countries. He also betrayed his positions on racism (which would resurface in the 1950s) when he declared that, as the president of the Italian society of genetics and eugenics, he fought against racial discrimination ‘among civilized populations’.\(^13\)

The convergence between Gini’s ‘Italian school of statistics’ and fascism was not simply the result of an instrumental collaboration: it was rather emphasizing a nationalistic view of science and providing technocratic strength to an authoritarian regime.

### 8. Conclusion

The last paragraph shows one of the most evident implications of this paper, which focuses on both the proper political dimension of the legitimation of new research schools and on the scientific legitimation of political regimes and their policies. The case study of Italian statistics in the early twentieth century and of Corrado Gini’s scientific career offers an unusual perspective on the problem. Retrospectively, Gini’s reaction to the political changes connected to the end of World War II sheds light on the limits of a possible reinterpretation of an academic enterprise. Somehow, the effort to disentangle the scientific development of his career from his involvement with a defeated political regime provides a measure of the level of reciprocal legitimation, and the constraints resulting from this process.

In order to draw from this case some theoretical considerations, however, it is necessary to assess the idiosyncrasy of the mechanism of reciprocal legitimation between science and politics here identified in the case of interwar Italian statistics. Resuming the general historical questions posed in the introduction may be useful in this perspective.

Was such a relationship with politics a peculiarity of statistics? In part, it was. The function statistics has performed as a tool of government, but also as a matter of political conflict, has been widely emphasized in the historical literature (Porter 1994; Desrosières 2002; Stapleford 2009). Many studies highlight also the contradictions arising in totalitarian contexts between statistical rigor and governmental manipulations of data (Ipsen 1996; Blum and Gousseff 1997; Tooze 2001). Even in the case of Gini, despite his personal agreement with totalitarian politics, scientific rigor sometimes brought him to resist political interference. This attitude was crucial to maintain the scientific legitimation of Gini’s research school, but also to allow it to perform a legitimating role on the fascist state.

The involvement of research schools and scholars in other disciplines in a special relationship with fascism has been discussed above. Different degrees of instrumentality and political interference distinguish some cases from others, yet a common trait is the selection of scientific applications directly contributing to the national interest. Is it possible to identify such a feature as a peculiarity of Italian statistics too? A strong attention for the issues concerning the nation, its government and administration characterizes in fact the history of statistics in Italy since the nineteenth century (Sofia 1988; Patriarca 1996; Favero 2001; Prévost
2009a), culminating in the above-mentioned nationalism of the generation of Italian statisticians that emerged in the early twentieth century.

On the other hand, it is also possible to trace back to the late nineteenth century the spread among the high ranks of Italian state bureaucracy of a culture that valued quantification as a privileged tool for government. From the 1870s to the 1890s, the central statistical office in Rome was in fact employing as ‘statistical officials’ a number of brilliant university graduates. Most of them made their later career through the ranks of administration, in politics and in business, becoming an influential group inside of the Italian ruling class in the early twentieth century (Marucco 1996, 44–49). This may explain how an emphasis on the role of statistics may find support in different milieus during the interwar period. The confidence that both state officials and trade representatives put in a quantitative assessment of conflictual matters allowed Gini to find a favorable ground when offering the technical legitimation of statistics to the corporatist political fixing of wage levels, which made possible for industrialists to accept Mussolini’s deflationary policy in the late 1920s.

The peculiarity of the case results then historically grounded in the specific configuration of the fascist regime, whose totalitarian grip of Italian society depended from the consent of relevant interest groups (Gentile 2008), and in the singular concept of science that was proper of Gini’s demographic and sociological theory, whom a reviewer accused to ‘put a pseudo-scientific foundation under a nationalistic complex’ (Reuter 1931, 648).

Such a peculiarity, however, does not prevent the possibility to draw some general considerations, insofar as it is common with any historical case. A first finding is that the political context in which the emergence of a new research school or field happens matters a lot in the configuration the latter takes. An important implication of this is that the general models explaining the historical development of science are contingent and may find a limiting scope condition in the political regime to which they make reference. Academic entrepreneurship has a relationship with politics, not only because it is largely a matter of ‘political’ negotiation and conflict, but also because state politics itself affects the mechanisms of legitimation of disciplines and schools. In turn, the scientific legitimation of political regimes takes a different color following the kind of disciplines and schools involved.

The co-evolution of political regimes and related research schools finds its origin in the gradual shift of the mechanism of reciprocal legitimation from pragmatic exchange to taken-for-grantedness. What is interesting in this process is its final invisibility, resulting from the cognitive lock-in of scientific procedures and state regulations, establishing their relationship as obvious. Their reciprocal legitimation remains in fact implicit and unquestionable if set against the presentist frame of most of the legitimacy theoretical literature (Hartog 2015). This literature usually takes for granted the ‘social paradigm’ (Handa 1986) of liberal democracy, together with the scientist ‘research tradition’ (Laudan 1977) that became dominant in the social sciences since the 1960s. Quantification was a crucial element in the process of legitimation of this tradition and in de-legitimating historical and institutional approaches, as for management studies Kipping and Üsdiken (2014b, 37–38) have shown.

In this context, the contribution of an historical approach to the debate on legitimacy and legitimation acquires a critical function, questioning the presumed universality of theoretical models, putting into perspective obvious assumptions, and showing the contingent and contextual quality of elements that are taken for granted as a result of historical dynamics. If addressed in historical perspective, successful processes of reciprocal legitimation
emerge in fact retrospectively as one of the mechanisms at the origin of cognitive embeddedness and institutional stability.

Notes

1. Here and below, where not stated otherwise, the translation into English of quoted texts in other languages is made by the author of this article.

2. Payments to the Laboratory of Statistics of the University of Padua are documented in the Central State Archive in Rome (from here on ACS), Fondo Gini, Corrispondenza, b. 1, Confederazione Generale dell’Industria. The first volume of the Indici was in fact completed in Padua, as Mario Saibante wrote to Gini in a letter reporting on the situation in the Laboratory that Gini had left quite abruptly after his appointment in Rome (ACS, Fondo Gini, Corrispondenza, b. 7, Mario Saibante, 25 February 1926).


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References


Academic centers and/as industrial consortia in American microelectronics research

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ABSTRACT
In the U.S., in the late 1970s and early 1980s, academic research centers that were tightly linked to the semiconductor industry began to proliferate – at exactly the same time as the first academic start-up companies in biotech, and slightly before the first U.S. industrial semiconductor research consortia. I show that some of the same factors stimulated institutional entrepreneurs to found both industrial consortia and academic centers. But industrial consortia and academic centers were not just co-emergent. They were also commingled organizational forms – consortia took advantage of ties to academic centers and vice versa. Thus, any understanding of the one must account for the other as well. However, academic microelectronics research centers possessed greater flexibility to forge alliances with other industries than did industrial consortia – a flexibility they increasingly took advantage of in the 1990s, as their importance to their original patrons in the semiconductor industry receded.

Introduction: co-emergent institutional innovation in microelectronics research

The editors of this special issue on academic entrepreneurship and institutional change have defined entrepreneurship as ‘the process by which actors identify opportunities for the future, allocate resources to them, and legitimize their actions’. Academic institutional entrepreneurship, then, is the process by which actors identify opportunities for universities and allied organizations to forge new institutions, i.e. novel procedures, norms, rules, cognitive frames, and bureaucratic lines-of-sight. Of course, this type of academic entrepreneurship does not take place in a vacuum, but rather in an environment that includes a variety of non-academic actors and organizations. Academic institutional entrepreneurs identify opportunities in large part by observing events in domains to which universities are connected (or would like to be connected) such as industry and government.

At the same time, institutional entrepreneurs outside the university are capable of making the same observations and therefore of identifying the same opportunities. Any analysis of academic entrepreneurship, therefore, must account for actors outside the university who:
(A) form their own innovative institutions that mirror or compete with institutional innovations that simultaneously develop within universities; and/or (B) stimulate and aid academic actors to forge new institutions that will benefit both the university and its extra-academic partners; and/or (C) develop their own innovative institutions that link up with (or sometimes fail to link up with) those emerging in academia.

In this article, I explore all three of these processes (A, B, and C) in fields related to microelectronics and semiconductor research from the late 1970s to the mid-1990s. This period was marked by the novel and frequent founding of university centers focusing on semiconductor research. But this was also the time when another new organizational form emerged with a very similar mission and for some of the same reasons: the industrial microelectronics research consortium – an example of (A). Actors in industry and especially in government stimulated or aided the formation of both university centers and industrial consortia in microelectronics – an example of (B). Because of these similarities in the environments that incubated them, and similarities in the specific guidance provided by their stakeholders, university centers and industrial consortia were convergent institutions – they mirrored each other in many ways. My stronger claim, however, is that they were also commingled institutions: consortia took advantage of resources offered by academic centers, and vice versa.

In the short term, many of the institutional innovations inspired by the U.S. semiconductor industry’s late 1970s woes were found wanting: some of the earliest academic research centers and industrial consortia disappeared relatively quickly. Over the longer term, consortia became a permanent feature of the landscape in part by claiming a special expertise in managing large, multi-university networks of academic researchers (Khan, Hounshell, Fuchs 2015). Academic microelectronics research centers (MRCs), meanwhile, secured their legitimacy in part through a difficult process of adjusting their practices to conform with the expectations of stakeholders in industry and government. That was not enough, however. The importance of the semiconductor industry to academic semiconductor research centers has decreased steadily since the early 1990s. The final piece of my argument, therefore, is that these centers have had to find new patrons and make new kinds of connections afforded by their position within the university. Academic entrepreneurs look to an external environment that simultaneously looks to them – and since that environment continuously changes the entrepreneur must continuously innovate to catch up or stay ahead.

(Changing) environmental factors

Understanding institutional innovation in semiconductor research is important in its own right. The microelectronics industry, after all, contributes more to U.S. GDP than any other manufacturing industry and takes similar pride of place in the economies of many other nations. However, the semiconductor case is doubly useful because it illuminates aspects of co-emergent academic and industrial institutional entrepreneurship (aided from both ends by the state) that have been obscured in the dominant literature on post-1970 academic entrepreneurs in biotechnology. Academic centers rarely appear in influential studies of early biotech pioneers such as Cetus, Genentech, Hybritech (Smith 2011; Rasmussen 2014; Kenney 1988; McKelvey 1996; Casper 2014). Similarly, consortia appear not to have been as important in biotech as in semiconductors; while research partnerships are common in biotech, they tend to involve two or perhaps three firms rather than a dozen or more. Biotech partnerships are also usually focused on some specific end-goal, and therefore tend to run
their course and then dissolve, often unhappily and accompanied by lawsuits (de Rond 2003). In the semiconductor industry, however, research consortia that are large, enduring, and retain an ever-changing portfolio of projects with a variety of endpoints have been the norm since at least the early 1980s (Grindley, Mowery, and Silverman 1994).

My own view is that examples of co-emergent, convergent institutional entrepreneurship by academic and non-academic actors could probably be found in biotech. Indeed, I will end with some examples where greater attention to the role of academic centers would add to our understanding of biotech. However, the phenomena I will discuss here are more visible in microelectronics than in biotechnology because of several relatively durable features of the industry. The first is that semiconductor research depends on equipment and clean rooms that have become astronomically expensive since the early 1970s. These tools represent huge sunk costs that few single firms or universities can bear alone.

Second, most semiconductor product and process innovations are covered by patents that are held by multiple firms which cross-license their intellectual property. Thus, while firms compete vigorously to be the first to achieve a particular product or process innovation, they are rarely able to retain their monopoly for long. Even if a firm could keep exclusive control of an innovation, it wouldn’t matter for long since, thanks to Moore’s Law, semiconductor products are often obsolete by the time they reach the market. Semiconductor firms therefore use their in-house R&D to focus on pressing short-term needs – to gain first-mover advantage or to limit the window in which a competitor has sole mastery of an innovation.

Thus, since the late 1960s the most successful semiconductor firms have relied on external sources for longer range research (Bassett 2002). Firms with large in-house basic research capacities, such as RCA, AT&T, and IBM, have either gone bankrupt or shed much of that capacity. But because semiconductor innovations often come from unexpected directions, feed into a few product cycles, and then disappear, semiconductor firms need sources of long-range R&D that gather together a varied and changing portfolio of discoveries. Lacking their own, in-house long-range research capacity, semiconductor manufacturers instead rely on organizations which pool funding from multiple firms to hedge their individual risks, and which generate a varied portfolio of discoveries on which multiple sponsors can draw.

Such considerations might have been enough to stimulate the formation of industrial semiconductor research consortia and industry-oriented academic MRCs no matter what. Indeed, a few such centers formed in the 1960s and small, rudimentary consortia were founded in the early 1970s. However, in the U.S. the environment dramatically shifted in favor of institutional innovation in semiconductor research in 1975, thanks to the Japanese Government’s announcement of a major initiative to bring that country’s firms up to the state of the art in manufacturing very large-scale integrated (VLSI) circuits. American firms had already been losing market share to the Japanese semiconductor industry since the early 1970s. With the announcement of the VLSI program, panicked, nationalist calls to ‘save’ the American semiconductor industry erupted (Langlois and Steinmuller 1999). All of the institutional innovations I examine in this article came after 1975, and most explicitly referenced competition with Japan as a justification for innovation.

The first wave of centers and consortia

One response to the declining competitiveness and basic research capacity of U.S. semiconductor firms was the industrial research consortium, where member firms would contribute
rotating personnel plus money for equipment and permanent personnel working on a coordinated menu of ‘pre-competitive’ research. In 1978 U.S. semiconductor firms spent 11% of their R&D budget in the form of technological collaborations or industrial consortia. By 1990, that figure had almost tripled (Zorpette 1990). Industrial research consortia seemed a logical institutional innovation to some leading industry executives and allies in government in the late 1970s because they believed that other countries – especially Japan – were far ahead of the U.S. in experimenting with these institutions. As one U.S. consortium, SEMATECH, put it in 1991,

The formation of consortia to address semiconductors and supporting fabrication and material technologies started in 1971 in Japan. Before that, the French and other governments had been active in reorganizing their electronics companies into various industrial groups… Between 1971 and 1980 in Japan alone, five consortia were formed that had semiconductor development activities (including the VLSI Cooperative Society in 1976). In the late 1970s, Korea, the Republic of China, and the United Kingdom experimented with various kinds of joint efforts. (Sematech 1991)

Yet despite these international models, cooperative industrial research organizations faced an uphill battle in the U.S. because of antitrust laws. The Justice Department’s interpretation of antitrust law also made consortia seem unnecessary prior to the late 1970s. That is, quasi-monopolistic firms such as IBM and AT&T were expected (or required by consent decree) to conduct and publicly disseminate basic research in order to avoid being broken up (Choi 2007). Smaller firms could therefore benefit from basic research performed by the giants without having to contribute anything themselves.

In the 1980s, however, conditions became more favorable to formal research consortia and less favorable to the quasi-monopolies’ quasi-consortia. Tax breaks for money spent on basic research were cut back, encouraging large firms to reassign basic researchers to shorter term, proprietary tasks (Asner 2006). The Justice Department abandoned its threats to sue if large firms did not make enough of their in-house research publicly available. The Justice Department also broke up AT&T; in the ensuing settlement, a significant part of AT&T’s research quasi-consortium (Bell Labs) was split off to form a formal research consortium (Bellcore) to conduct R&D for the newly independent Baby Bells.

Thus, the basic research capacity of the informal quasi-consortia on which Silicon Valley firms had been free riders declined through the 1980s. At the same time, the federal government lifted barriers to formal consortia, and in some cases even became an enthusiastic supporter of this relatively new organizational form. Thus, semiconductor firms turned to consortia as a way to make up for their shortfall in basic research. The most visible consortia were the Semiconductor Research Corporation (SRC) and the Microelectronics and Computer Technology Corporation (or MCC), both formed in 1982, and Sematech (or Semiconductor Manufacturing Technology), founded in 1987. Over the years many smaller and/or more time- and topic-delimited consortia formed alongside the larger, more generalist ones. All of these research consortia were aided by the industry’s consortiumization of other activities. For instance, SEMI, a trade group for semiconductor process equipment manufacturers and materials suppliers, formed in 1970, and the Semiconductor Industry Association was founded in 1977 – the latter, as Saxenian (1992) puts it, to ‘shape legislation,’ including the changes in antitrust law needed to legitimize consortia.

Institutional experiments such as research consortia rarely work on the first try. Indeed, proponents of consortiumization were aware that their first attempts might fail, but would
be worth it if they led to further experimentation and eventually to answers to the challenge from Japanese manufacturers. As the White House Science Council Panel on Semiconductors put it in 1987,

The current world competitive situation demands increasing cooperation both horizontally and vertically in the industry as evidenced by the spontaneous emergence of the Sematech proposal. Sematech is not necessarily the ideal instrument, but it is a significant step, a start. Indeed, it is generally agreed even by those advocating Sematech that it will not solve all the industries’ problems. However, it will increase communication between elements of the industry, and may encourage new coalitions outside of Sematech, and may even facilitate industry restructuring. (White House Science Council 1987)

Thus, a few early consortia, most notably the MCC, faltered and eventually dissolved. In hindsight, critics blamed MCC’s demise on difficulties in balancing the competing agendas of the member companies (Gibson and Rogers 1994). And yet, MCC’s difficulties did nothing to diminish enthusiasm for consortia. If anything, MCC has come to be seen as a source of lessons which, having been learned, made consortia a permanent feature of the semiconductor industry (Browning and Shetler 2000; Corey 1997; Burger 1996).

Even before the emergence of the major semiconductor industry consortia, academic centers with strong ties to that industry started to form. Indeed, there had been a few academic centers focused on microelectronics in the late 1950s and early 1960s, but these were generally ad hoc mechanisms for a few professors to pool equipment. Often, these faculty members had experience in industry (Kenney, Mowery, and Patton 2014), but research and pedagogy – rather than industrial partnerships – were these organizations’ core purpose at the beginning. In the late 1970s, though, academic microelectronics centers started to form with the explicit aim of aiding industry.

Contemporaries observed that this trend paralleled the emergence of professorial start-ups in the biotech industry. However, competition with Japan in semiconductors generally loomed larger than biotech as a stimulant to academic microelectronics researchers’ turn toward industry. As Science put it (Norman 1982),

While attention has been focused on the expanding links between academic biologists and the corporate world, a second revolution in university-industry relationships has been taking place in a different field. Electronics companies, faced with growing competition from Japan and fearing a shortage of well-trained Ph.D.s, are pouring unprecedented amounts of cash into university electrical engineering and computer science departments.

Thus, the same conditions that led to the formation of industrial consortia were visible in the late 1970s to academic entrepreneurs who concluded that those conditions provided an opportunity to forge closer ties with semiconductor firms. Some of those ties were bilateral, but many were ‘consortium-ized’ – i.e. both risks and benefits were shared across many contributing firms – through the mechanism of an academic center.

A prominent early example which illustrates the incentives and initial obstacles to such centers was Carver Mead’s Silicon Structures Project at Caltech. This center received considerable press attention at the time; more recently, Elizabeth Popp Berman (2012) has examined it as an early instance of a ‘university-industry research center’. My analysis of the Caltech center (and several others I’ll discuss) is indebted to Berman’s, but what I contribute is a perspective on such centers from the viewpoint of an industry (semiconductors) that was simultaneously exploring other options (particularly consortia) to obtain the same benefit that Mead’s and other academic centers purportedly offered. Thinking about university centers in terms of their interactions and competition with consortia offers insight into why
centers such as Mead’s formed (and closed) when they did, why they were configured in particular ways, and why they were able to secure certain kinds of patronage.

Mead was a long-time friend and collaborator of Intel’s cofounders, Robert Noyce and Gordon Moore – for several years even flying to the Bay Area from Pasadena for weekly meetings with Moore (Brock 2006). Mead was equally famous for collaborations with other industrial researchers, most notably Lynn Conway of Xerox. Yet despite his close ties to U.S. firms, he still found it hard to gain their attention. For instance, when he made optimistic predictions for the miniaturizability of electronic components in the early 1970s, ‘the people who were listening the closest were the Japanese,’ rather than his friends in the U.S. semiconductor industry (Mead 1980).

One of the consequences of miniaturization was that integrated circuits became more complex, and therefore the design of chips became more labor-intensive. In the mid-1970s, Mead switched from studying miniaturization per se to developing automated means of designing chips. As Mead put it,

back in 1970 … there were only a few tens of people-months involved in designing a chip, and everyone said ‘well, why don’t you university folks go and mind your business and play with your toys because we don’t think we have the problem. (Mead 1980)

Thus, he could see that he needed some institutional innovation to get firms to recognize that a university could supply the long-range research that was disappearing from industry: ‘one of the functions of a university is to do a fair bit longer look ahead than it’s possible to do in a rapidly emerging and very competitive industry’ (Mead 1980).

Mead called his institutional innovation a ‘project’ rather than a ‘center’ – perhaps an indication of how fluid the terminology still was. Yet it was, in fact, a center – it ‘centralized’ the research outputs of a small coalition of faculty members and their students. It also centralized inputs, by pulling together funding from ‘IBM, Xerox, Burroughs, Hewlett-Packard, Digital Equipment Corporation, Intel, and Honeywell’ (Mead 1980). Many of the same firms would soon also invest in industrial research consortia such as MCC – precisely in order to gain the same ‘fair bit longer look ahead’ that Caltech offered. In its operations, too, the Silicon Structures Project borrowed many of the traits of an industrial consortium. It had in-house ‘staff’ (students) who worked with rotating researchers from the ‘consortium members’ (industrial sponsors):

Each of these [firms] sends a scientist on a rotating basis to work with us …. They come and work with our students …. [thereby] transferring the lore that comes in an academic research environment back into the companies …. It allows the university to do what it’s the best at, looking very far ahead, taking risks, looking at things whose outcome is very uncertain. It also uses the industrial organizations for the things they’re the best at. And in fact we get a fair bit of help with project management kinds of things and things we’d otherwise have to start stepping outside of our role as a sort of a blue sky research organization. (Mead 1980)

That is, Mead’s center was financed like a consortium, configured technology transfer in much the same way as a consortium, and even brought in corporate project management methods to help it operate less like an academic unit and more like a corporate research lab, albeit one not housed in any particular firm – just like an R&D consortium.
Enter the state(s)

Mead's center was somewhat unusual in having relatively little support from government. In most cases, though, academic entrepreneurs were spurred to form (or to expand) industry-oriented microelectronic research centers by inducements offered by federal and state agencies. Institutional entrepreneurs within government fostered the emergence of both academic MRCs and industrial semiconductor research consortia, and built thick, varied connections among them. The federal government was a member of some consortia, most notably Sematech (until 1996), while many academic centers that conducted industry-sponsored research needed extra funding from the state to support operations and purchase equipment. Government agencies also prompted formation of a few consortia with industrial, academic, and government participation – such as the Consortium for Superconducting Electronics in 1989, which united AT&T, IBM, MIT, and Lincoln Lab (a DoD laboratory operated by MIT).

One prominent example of federal intervention was a 1976 NSF-sponsored competition for a National Research and Resource Facility for Submicron Structures (Mody and Choi 2013). The NSF received about 15 proposals for the NRRFSS, several of them from small coalitions of universities and government laboratories, including: a University of Pennsylvania/Drexel University/Lehigh University team; a University of Colorado/National Bureau of Standards (Boulder) collaboration; and the second-place finisher, a joint proposal from MIT proper and MIT Lincoln Lab. In the end, Cornell won, but the competition inspired the formation of a number of similar centers and facilities in the late 1970s and early 1980s. The academic institutional entrepreneurs who had answered the NSF's call by piecing together assemblages of personnel, equipment, and money were well-positioned to continue their institutional entrepreneurship even when they lost the NRRFSS competition. For instance, the leader of the MIT Lincoln Lab proposal, Hank Smith, was invited to move from Lincoln Lab to MIT to found a Submicrometer Structures Laboratory which has competed with the Cornell facility for almost 40 years.

As the unwieldy name implies, the NRRFSS was designed both to rent out expensive semiconductor manufacturing equipment ('resource') and conduct experiments on the submicron scale ('research'). The NSF eventually imposed a third mandate, that of providing advice on how to establish and run such a facility. Academic researchers were intended to be the primary beneficiaries of all three missions, but the NRRFSS was also expected to serve industrial users. It was also intended to indirectly aid industry by making it easier for academic users to conduct industry-oriented research without unduly burdening firms. As Jay Harris, the NSF officer who put together the NRRFSS competition, recalled, when he was a professor at the University of Washington.

In the late '60s and early '70s, I used to visit various industrial laboratories to try to get some help in making small optical structures. I got my best reception at the Hughes research labs in Malibu, from a guy named Ed Wolf, who was working with electron beams, but Ed didn’t really have time to devote to supporting academics trying to work over their heads. (Harris 2003)

When Harris moved temporarily to the NSF, therefore, he began lobbying his superiors for a national user facility that would rent expensive equipment to academic researchers.

Initially, Harris’ proposal faced a significant skepticism from industry (including Robert Noyce of Intel), the National Science Board, and the military. By organizing workshops and gathering testimonials from stakeholders, however, Harris legitimized the NRRFSS as a new
way for government to bring academic researchers in closer contact with the industrial state of the art. In effect, Harris acted as an academic entrepreneur working temporarily on behalf of the state. Notably, one of the most common justifications offered for the NRRFSS was that it would help the U.S. semiconductor industry. As a report from one Harris’ workshops put it,

adding to the urgency of the need for research in the submicron domain is the effort made by our international competitors to leap-frog the US technology in this field. The most noteworthy program is the Japanese decision to spend $233 million in the next four years to develop submicron device research and fabrication capabilities with their industry-university teams. (Chang et al. 1976)

That is, institutional entrepreneurs were mobilizing Japan’s VLSI circuits program to legitimize academic microelectronics centers even before they were using it to legitimize industrial research consortia.

After the NRRFSS was founded, Harris’ former industrial colleague Ed Wolf became its director. There, Wolf cultivated a variety of university–industry interactions. Corporate researchers, especially from East Coast powerhouses like IBM, General Electric, and AT&T, developed collaborations with Cornell faculty affiliated with the facility; corporate users occasionally rented time on NRRFSS equipment; and companies looking to build their own clean rooms or buy new semiconductor process equipment looked to Cornell for advice. A report from 1986 summarized such interactions, including examples of all three involving one company, General Electric:

GE had an engineer in residence at NRRFSS for a year to learn MBE [molecular beam epitaxy], electron beam lithography and device processing for high speed GaAs [gallium arsenide] devices. He then returned to GE and established a similar processing capability …. Strong interaction continues between GE and Cornell. NRRFSS is continually called on to help/advise other companies and universities in setting up similar laboratories, such as Varian, GE, McDonnell Douglas, the Jet Propulsion Lab, Hughes, Caltech, University of Michigan and University of California San Diego. Over the last several years we have advised more than forty organizations. (Ballantyne 1986)

The NRRFSS also established an industrial affiliates program that strongly resembled a consortium in miniature: by 1986, 37 firms were each paying Cornell $8500 per year for previews of faculty research and opportunities to recruit promising graduates (Cornell University News Bureau 1981).

Like other academic centers, the NRRFSS didn’t just borrow features of industrial consortia – it also partnered directly with such consortia (in addition to its bilateral relationships with individual firms). The most important such partnership was with an SRC Center of Excellence in Microscience and Technology located on the Cornell campus. SRC established Centers of Excellence at many schools, including several affiliated with the academic centers that I’ll discuss below. Several directors of academic MRCs or facilities (including Ed Wolf) joined the SRC’s University Advisory Committee and thereby influenced the consortium’s research agenda – a notable way in which centers and consortia were commingled organizational forms.

One reason for the commingling of centers and consortia is that the latter often require an on-campus broker to mediate their interactions with universities. A center is, in many ways, the optimal form for such a broker. Interfacing with an entire university or even a department is inefficient for a consortium, since these organizational forms rarely make quick or uncontested decisions. Interfacing with a single faculty member guarantees faster response, but not breadth of knowledge – and when the consortium’s needs change, the
individual professor’s expertise may no longer be relevant. An academic center, however, can act relatively quickly and coherently, and yet can still bring the expertise of a broad and flexible array of faculty to bear on the consortium’s ever-evolving needs.

A local supply of well-funded academic centers (staffed by world-class faculty) was therefore a high priority for the semiconductor consortia that formed in the 1980s. Hence, state governments energetically fostered academic centers in order to attract consortia headquarters to their regions. The Microelectronics Center of North Carolina (MCNC), for instance, formed in 1981 with support from the state and a coalition of five universities; the MCNC was integral to North Carolina’s success in attracting the SRC’s headquarters (Casey 1981; Whittington 1985). Similarly, when the state of Texas wanted to woo the MCC to Austin in 1983–1984, part of Governor Mark White’s pitch was that the state would fund a new MRC at the University of Texas (University of Texas Office of Public Affairs, 1984).

The Texas state government’s successful bid to build Austin into a high-tech region was facilitated by – and immensely valuable to – academic institutional entrepreneurs within both the University of Texas and Texas A&M University. As Jack Kilby (co-inventor of the integrated circuit and an adjunct faculty member at the latter university) reported to his former colleagues at Texas Instruments in 1983,

I have inadvertently become involved in attempting to convince MCC that they should locate in Texas. Austin is one of the four sites left in the race. One of the primary MCC concerns is the quality of the university facilities which may be available. Since UT has very little work in the microelectronics area, A&M has been asked to help. (Kilby 1983)

Kilby was probably hoping that MCC’s presence would benefit the pioneering pedagogical ‘fab’ (integrated circuit fabrication line) at Texas A&M University which he and colleagues were trying to place at the center of the undergraduate electrical engineering curriculum.

Kilby was slightly off-target in his assessment of the University of Texas, however. In 1982, a prominent semiconductor researcher from the University of Illinois, Ben Streetman, had moved back to the University of Texas (where he had done his PhD) and had started to informally pool research equipment with other faculty members. It was Streetman who took advantage of the MCC site selection to convert those informal efforts – with state support – into the MRC. Then in 1986–1987, when the state was lobbying a second consortium, Sematech, to put its headquarters in Austin, Streetman leveraged the opportunity to convince the governor to fund a brand new, state-of-the-art facility for the MRC (University of Texas Office of Public Affairs 1987).

Texas wasn’t the only state to try using an academic center to lure Sematech. As Leslie (2001) has shown, New York’s Albany region offered the consortium a Center for Integrated Electronics at Rensselaer Polytechnic Institute but, ‘in the prevailing political climate [of the late 1980s], Texas simply had more clout than New York.’ Apparently the prevailing climate has changed, however, since Sematech’s headquarters were recently poached away from Austin to the Albany region in large part by the promise that the State University of New York system would build a Computer Chip Commercialization Center, a Chemical Mechanical Planarization Center, and other centers to serve as the consortium’s local academic partners (PRWEB 2014).
Stanford un-exceptionalism

There are many different ways for firms to organize the consortiumization of their activities both inside and outside (and at the thick boundary of) the academy. Each of the consortia I’ve described was structured differently, and there was even greater variation in the organization of the academic centers I’ve mentioned. Over time, however, many of these centers’ practices converged, through a variety of mechanisms: some (e.g. Cornell’s NRRFSS) specialized in propagating their model; there is evidence that centers kept tabs on their peers and copied successful strategies; and of course personnel (faculty members and former students) moved from one center to another, transplanting norms in the classic model of institutional isomorphism. In addition, in the late 1970s the Institute of Electrical and Electronics Engineers (IEEE) began organizing biennial ‘University/Government/Industry Microelectronics’ conferences, at which leaders of academic microelectronics centers would report on their practices. Notably, by the early 1990s (if not earlier) that conference series was sponsored by Sematech and SRC.

Despite such convergence, however, there was and is variation among academic centers aligned with the microelectronics industry, in part because that industry took different forms in the regions served by different universities. Moreover, while this article is primarily about the resemblances and linkages between industrial research consortia and academic research centers, it’s important to remember that industrial consortia and universities do many things other than research. For instance, some academic microelectronics centers positioned themselves as aids to regional economic growth not by partnering with national consortia, but with local high-tech incubators. Obviously, an incubator is different from Sematech or SRC; still, incubators consortiumize many of the things most relevant to high-tech entrepreneurs – real estate, pools of investors, administrative costs, personnel, expertise, etc. And like larger R&D consortia, incubators sometimes found it useful to interface with universities through dedicated centers.

For instance, in the 1990s the founders of the Center for Nanotechnology at the University of Washington convinced upper administrators to give them control of an existing microfabrication user facility located in a university-affiliated tech incubator, the Washington Technology Center (Spelman 1997). The WTC Microfabrication Laboratory was originally founded in anticipation of semiconductor manufacturing firms such as Intel and Taiwan Semiconductor opening sites in Washington State (Yager and Darling 1998). The founders of the Center for Nanotechnology argued, however, that firms would have easier access to the university through a facility located in the incubator but controlled by an academic center:

other Centers have experienced that well maintained user facilities act as focal points to build long-lasting relations between industry and research institutions. They create an environment in which scientists from industry can meet and collaborate with students, post-doctoral fellows, and faculty while pursuing mutual research interests. (Vogel 1996)

Indeed, sometimes centers act as more than just ‘focal points’. Because clean rooms and semiconductor process equipment are so expensive, renting time on academic equipment is sometimes much more cost-effective for cash-poor start-ups than buying tools and building a fab. In at least one instance (at the University of Texas), a local start-up actually had semi-permanent office space within the academic user facility. That may have been a slightly unusual arrangement, but other user facilities I’ve visited have been very happy to have local
start-ups use their equipment on a near-daily basis – i.e. to help start-ups cut costs by sharing the costs and benefits of their tool base with other users through an academic quasi-consortium.

Other academic microelectronics centers drew on firms’ consortiumized political activities. For instance, in 1983 Ray Warner, one of the founders of the University of Minnesota’s Microelectronics and Information Science (MEIS) Center, appealed to ‘the Minnesota High-Tech Council (MHTC), a fairly active committee of local captains of industry, and the Governor’ to force the president of the university to ‘stabilize and then rebuild’ the university’s micro-electronics research capacity (Warner 1983). Similarly, when the NSF tried to pull funding from the Cornell NRRFSS in 1985, the facility mobilized the New York Congressional delegation, its industrial affiliates and users, and the corporate executives on its advisory board to lobby the NSF to reverse its decision.

Almost all these variations on the center–consortium relationship are contained in the most hybridized example of an academic center and/as an industrial consortium: the Center for Integrated Systems (CIS) at Stanford University. There is, of course, already an abundant literature on Stanford’s relationship with Silicon Valley – probably too abundant, since as Steve Shapin (2008, 160) notes, discussions of academic entrepreneurship often mistake elite universities such as Stanford for the norm. I don’t dispute the point, but in terms of how Stanford has managed its MRCs as, and in collaboration with, industrial consortia, it is exceptional only in the degree to which it combines and amplifies traits which are also evident at less elite schools. Moreover, federal policymakers – particularly in the National Science Foundation – seem to have viewed Stanford and Cornell as models to be copied by academic MRCs elsewhere. I’ve discussed Cornell already. Let me now examine Stanford with a view to what it can tell us about centers and consortia more generally, before finally bringing Stanford and Cornell into the same frame.

As Lécuyer’s (2005) classic ‘What Do Universities Really Owe Industry?’ shows, the CIS represented the culmination of more than 30 years of Stanford’s steadily thickening ties to the local semiconductor industry. The CIS was the brainchild of John Linvill, the longtime chair of the university’s Electrical Engineering department, and his frequent collaborator, James Meindl, founder of Stanford’s Integrated Circuits Laboratory (ICL). Both men were serial technological inventors, institutional innovators, and commercial entrepreneurs, having cofounded a company, Telesensory Systems, in 1970.

Curiously, Meindl’s ICL would almost certainly have beaten Cornell in the NSF’s 1976 submicron facility competition had he chosen to compete. Yet Meindl was notably helpful to the new centers that arose in the wake of the NSF competition, even serving on the NRRFSS advisory board. As an MIT faculty member reported to his employer’s Submicrometer Structures Laboratory after a visit to Stanford in 1977,

While it may seem strange to us, Jim Meindl said that he thought MIT’s entry into the IC field would legitimize it, and give more emphasis to Stanford’s program. I cannot overemphasize that everyone I met was most cordial and friendly, and eager to cooperate. (Wolff 1977)

However, Meindl and Linvill could also see that the emergence of new academic microelectronics centers meant Stanford’s claim to unique competence was eroding, and that they therefore needed to transform the ICL into a wholly new kind of organization. Thus, in about 1978 they put together plans for a center that would unite the ICL with three other microelectronics and computing centers to form a single organization that would ‘integrate’ research from solid-state physics through computer architecture design. They began
lobbying the NSF, and at the same time approached leading industrial research managers, such as Lester Hogan of Fairchild Semiconductor, to join an exclusive (and expensive) affiliates program. By 1982, they had persuaded 18 firms to make a $750,000 up-front payment plus $100,000 in annual dues (Anonymous 1982). That rate equaled the annual dues paid by sponsors of Caltech's Silicon Structures Project (Berman 2012, 127). CIS dues eventually went up, but the number of affiliates remained around 18–20 for the next 30 years.

What did affiliates get in return? The CIS Newsletter let Hogan explain:

Dr. Hogan points out that Japan, whom he feels is America's main competition in high technology, has employed somewhat the same approach to basic research on problems crucial to Japan's economic future ….. 'To win against foreign competition,' says Dr. Hogan, 'we need programs like CIS, sponsored by industry, at a half dozen more of our best universities, coupled with renewed vigor for basic research by industry itself' (Vollmer 1982)

In other words, industry needed the CIS for the same reasons that it needed consortia: to overcome competition from Japan and to rebuild basic research capacity. And, with dues more than an order of magnitude higher than ordinary affiliates programs (such as that at Cornell’s NRRFSS), the CIS was more consortium-like than most centers in the degree of influence sponsoring firms had over research. As at Caltech's Silicon Structures Project, the CIS' initial setup allowed member firms to send researchers on sabbatical at Stanford, partly to provide a conduit for technology transfer to (and from) the firm.

The CIS differed from the Silicon Structures Project, however, in its longevity, running almost thirty years longer than the Caltech center. Berman (2012, 128) explains the Silicon Structures Project’s demise as a result of difficulty matching academic and industrial expectations for students. She also notes that the center's small size (industrial rotators far outnumbered affiliated faculty) made it unsustainable. The CIS, on the other hand, nearly matched the size of some consortia: by 1985 its affiliated faculty were using the CIS for 173 different projects. As at some other academic centers, some of those projects were supported by industrial consortia, such as an SRC Center of Excellence in semiconductor manufacturing systems. Yet Stanford’s size and proximity to Silicon Valley meant it could sometimes reverse the usual arrow of influence between centers and consortia: for instance, in 1987 the CIS bragged of ‘participating in the planning of SEMATECH’ (Losleben 1987).

Of course, as a university, Stanford was supposed to train students who would then be recruited by sponsor firms – not the usual arrangement at a more purely industrial R&D consortium. Even there, though, the CIS was more of a hybrid organization than might appear at first glance. As the San Jose Mercury noted in 1981,

Intel Corp. of Santa Clara, for one, has not rallied to Stanford’s request for money [for the CIS], even though Intel Vice Chairman Robert Noyce is one of the Stanford fundraisers. 'We were concerned about the fact we were getting relatively few graduates from Stanford… Many Stanford graduate students are on leave from other companies or are foreign students … but that's another story,' said Noyce, his voice fading. (Richards 1981; ellipsis in original)

Noyce also seems to have worried that Intel would end up funding research that others would commercialize – a problem that confronted non-academic consortia as well, and which eventually doomed the MCC. As a local paper quoted him, 'The benefit is spread much more broadly than the cost. The non-participants in CIS will benefit just as much as the participants. Unless a participating company looks at the broader benefits, the winning strategy is not to participate' (Duenwald 1982).
Academic freedom (of choice)

As the quote from Noyce indicates, not everyone in the semiconductor industry was convinced that long-range research should be consortiumized through academic centers, or even at all. Noyce expressed his own preference by taking over as Sematech’s first CEO and then steering the consortium away from basic manufacturing research and toward improving relations between semiconductor manufacturers and equipment vendors (Berlin 2005, 297, 298). Some early academic centers, such as Mead’s Silicon Structures Project, stagnated or disappeared when they failed to live up to the promise of aiding industry. Yet today the academic MRC is a thriving organizational form, with many dozens of examples across the U.S. and the world. How, then, did the entrepreneurs of this particular institution secure its legitimacy?

In part, they did so by being responsive to their sponsors’ demands. For instance, in 1982 the National Science Board judged that Cornell’s NRRFSS was inadequately aiding external users and therefore threatened to cut off its NSF grant. As a result, the facility appointed a new associate director for the User Program, and four years later the proportion of external users had risen to 60%, including 12% of users from industry (National Research and Resource Facility for Submicron Structures 1985). By that point, as well, a third of research projects conducted in the facility were funded by industry (Ballantyne 1986). Similarly, Stanford’s CIS suffered from early complaints that sponsor firms weren’t able to get specific research objectives on faculty members’ agendas. Thus, the center developed a new mode of cooperation: FMA teams (Faculty member–corporate Mentor–graduate Advisee). Through these teams, graduate students’ projects were essentially co-directed by a faculty advisor and an industrial researcher (Linvill 1990). The CIS was also quick to develop online systems for disseminating research to partner organizations, probably at the urging of federal and industrial sponsors (Losleben 1987).

By the late 1980s, however, the economic and political environment in which the NRRFSS and CIS and their peers were founded was beginning to change in ways that made it necessary for them to find new sources of support. The semiconductor industry was becoming less vertically integrated, with much manufacturing outsourced to ‘foundries’; nationalist rhetoric gradually died down, though it was not until 1998 that an Asian firm finally became a CIS sponsor; and the technological paradigm of semiconductor manufacturing settled (perhaps temporarily) on optical lithography, leaving academic experts in other techniques adrift. Academic centers therefore had to work harder to interest industrial patrons. As Jim Plummer, Meindl’s successor as director of the Integrated Circuits Lab portion of the CIS, put it in 1988,

The only available source of funds at present to support [the ICL] is the CIS sponsors annual contribution. We are actively seeking external funding, but it is not an easy matter to ‘package’ this activity in a way that is attractive to government sponsors. Everyone (CIS Sponsors included) wants to support leading edge research. To the extent that chip building is regarded as not leading edge or in direct competition with industry, it is not interesting to sponsors. (Plummer 1988)

To survive, the CIS, NRRFSS, and their peers took full advantage of the flexibility of academic institutions – a flexibility largely denied to conventional industrial R&D consortia. This point somewhat cuts against the grain of much recent work by historians, sociologists, and STS scholars on the commercialization of academic research. Some scholars in this tradition (Shapin 2008) argue that the variety and commingling of industrial and academic
organizations mean that it makes no sense to draw blanket distinctions between these domains, especially not distinctions predicated on a mythical norm of academic freedom. Others (Rabinow 1996) argue that in fact corporate research is more free than its academic counterpart. More anxious scholarship (Mirowski 2010) sees universities since 1980 as giving up on academic freedom in a rush to adopt corporate models. I don't disagree with these claims; indeed, they accord well with my argument that industrial semiconductor research consortia and academic MRCs were commingled and co-emergent forms which blurred any facile distinction between corporate and academic research. That said, academic centers’ embeddedness in a university ecology did make a difference by providing a flexible menu of options in the face of changing technological, business, and political conditions.

For one thing, the CIS and its academic peers were able to participate in university consortia just as easily as they could partner with industrial ones. As Hoddeson, Kolb, and Westfall (2008) argue, in fields such as high-energy physics the expense of research equipment makes it impossible to build state-of-the-art experiments without forming large coalitions of universities and state/provincial or national governments. The tools and clean rooms needed for microelectronics research are not in the same league as particle accelerators, but commercial chip manufacturing has almost reached the point where only coalitions of firms can afford a state-of-the-art fab. By extension, no single university can presently afford a full suite of the equipment needed for state-of-the-art microelectronics research. A few state university systems have approached that problem by consortiumizing research activities among multiple schools – I've already mentioned the MCNC and New York State’s Computer Chip Commercialization Center, both of which were attached to coalitions of universities rather than a single school.

At the national level, since 1994 NSF funding for academic microfabrication user facilities has been funneled through continent-spanning consortia of universities which, in toto, provide users with a full complement of tools: first through the 5-member National Nanofabrication Users Network and then the 13-member National Nanotechnology Infrastructure Network. Both the NNUN and NNIN were led by the descendant of Cornell’s NRRFSS and ‘co-led’ by Stanford’s CIS. The NNIN has recently been replaced by a National Nanotechnology Coordinated Infrastructure which operates in a similar manner.

Faculty who are affiliated with academic centers also have the freedom to form partnerships with a broad range of industries. It’s hard to imagine Sematech or the SRC as focused on anything other than the needs of the semiconductor industry. Academic microelectronics centers, however, can draw on faculty from almost any university department, and therefore can facilitate those faculty in bringing microelectronics and semiconductor expertise and equipment to bear on a wide range of industrial applications. For instance, one NNIN member facility that I visited in 2009 (Georgia Tech’s) proudly told me about their partnership with one of the leading firms in the paper industry! Much more common have been linkages between academic microelectronics centers and the health and biotech industries. Of the thirteen schools in the NNIN, five listed a life science area as a core field of expertise, and three were predominantly oriented to biomedicine.

That connection to biomedicine was partly mandated by the NSF, and partly a natural consequence of skyrocketing federal support for the National Institutes of Health in the 1990s. But it was also partly a reaction to notable examples of faculty entrepreneurship in biotechnology arising from the leading academic microelectronics centers. One such was the ‘gene gun’ – a technique for moving foreign DNA into the nuclei of plant cells.
co-invented by the director of the NRRFSS, Ed Wolf, and a Cornell horticulture professor, Sanford (2000). Nelson (2012) describes the gene gun as resulting in ‘the largest royalty payment to the Cornell Research Foundation up to that date and ... one of the most ‘readily recognized financial successes’ in the history of Cornell technology transfer,’ thanks to a start-up company founded by Wolf and Sanford, which licensed their Cornell patents and turned the technique into one of the most widely used tools for creating early genetically modified crops. Another project much touted in the NRRFSS’ promotional materials was a study of fungal growth on microfabricated pores – work of great relevance to the wine industry in New York state’s Finger Lakes region.

Similarly, the Stanford CIS’ proximity to the San Francisco biotech cluster led to development of DNA microarray or ‘gene chip’ (trademarked as GeneChip©) technology. As Lenoir and Giannella (2006) have shown, the DNA microarray arose through a mutual spillover of industrial research into academia and vice versa. The leading company, Affymetrix, was a spin-off of a second-generation biotech firm, Affymax, looking to develop high-throughput methods for varying and screening molecules for drug discovery. As Doogab Yi (2010) describes it, an Affymax photochemist, [Michael] Pirrung, suggested that light-controlled synthesis of polymers might be a productive and inexpensive way to create diverse sets of random chemical molecules. Pirrung drew an analogy with the production of silicon chips using photolithography. Following an innovative technology used in the semiconductor industry, VLSI (very large-scale integration), they decided to develop VLSIPS (very large-scale immobilized polymer synthesis). Since Affymax had deep Stanford roots, Pirrung soon made contact with the CIS and one of its resident experts in VLSI, Fabian Pease, to help them move from VLSI to VLSIPS. The result, according to Lenoir and Giannella, was that ‘Pease has been co-inventor ... on several key Affymetrix patents, and he has continued to maintain a consulting relationship with Affymetrix’.

Academic centers, then, can make far-flung alliances and pursue promising research trajectories toward a wider range of possible outcomes than an industrial consortium can. It’s quite possible that this dilution of influence actually benefits semiconductor firms, since enough of the research done at these centers still concerns microelectronics, but with that industry now able to share the burden of supporting the centers. Yet the dilution of semiconductor industry influence is also of benefit to the host university and society at large – especially since the possibilities that an academic center can follow are as likely to be epistemic as commercial. Basic, curiosity-driven research areas have benefited enormously from the equipment, expertise, and industrial funding associated with academic MRCs. MIT’s Submicron Structures Laboratory, for instance, has become well known for the development of diffraction gratings used in the fairly esoteric field of astrophysics.

**Conclusion**

Despite this branching out, academic MRCs are still very interested in working with both individual semiconductor firms and industrial semiconductor research consortia. Some of the commingling of these two organizational forms that took place when they first emerged still occurs – for instance, state governments’ use of academic centers to lure consortia headquarters. In other ways, though, their relationship has changed substantially. Some scholars, for instance, have argued that the main role for semiconductor consortia today is to cultivate
a large stable of diverse academic teams – and to encourage those teams not to think too much about medium-term or direct relevance to the semiconductor industry (Khan, Hounshell, Fuchs, 2015, 30, 31). Others argue, however, that state agencies – particularly DARPA – have figured out how to nudge constellations of industrial and academic researchers toward productive outcomes in ways that transcend the need for industrial consortia (O’Reagan and Fleming forthcoming). Either way, it is clear that academic researchers need, and can find, a more varied portfolio of sponsors today than they could in the 1980s, and therefore that relationships between academic microelectronics centers and industrial semiconductor consortia are more attenuated than before.

In other words, both academic centers and industrial consortia are continuing to evolve and innovate as their environments evolve, with help from institutional entrepreneurs inside and outside of both types of organizations. Those dynamics of mutual observation and co-emergence have been present since at least the late 1970s, when entrepreneurs in industry, academia, and government all perceived the U.S. semiconductor industry as losing its ability to do basic research and as existentially threatened by competition from Japan. Institutional entrepreneurs in academia (e.g. Carver Mead or John Linvill) saw that as an opportunity to found a new kind of academic unit. Some institutional entrepreneurs in industry (e.g. Robert Noyce) believed that circumstances merited a new kind of industrial organization – the consortium – and were only grudgingly supportive of academic centers. Other industry executives (e.g. Les Hogan) were more inclined to see academic centers and industrial consortia as complementary. Institutional entrepreneurs in government, meanwhile, pursued a variety of strategies which offered resources and legitimation to entrepreneurs in industry and academia – without the help of state actors like Jay Harris, many academic centers and industrial consortia would not have gotten started.

Which is to say: the historian’s task in understanding academic institutional entrepreneurship can only get more complicated. Interpreting how, why, and when academic actors introduce new institutions is difficult enough. But any conclusions we might offer are incomplete unless we also take into account actors in other organizations and even in other domains of social life who can aid or hinder the academic actor. The reward for our effort in drawing those connections, however, is that we make our studies relevant in understanding phenomena far from the university. The story of academic research centers in the U.S., for instance, sheds light on the travails of American manufacturing, on diplomatic relations between the United States and Japan, and on the interplay of state and federal governance – connections that remain invisible when the story is only told from the perspective of the academic actors.

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