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## Economics and other social sciences: A historical perspective

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ROGER BACKHOUSE AND PHILIPPE FONTAINE

ECONOMICS AND OTHER SOCIAL SCIENCES:  
A HISTORICAL PERSPECTIVE

ESTRATTO

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*Annals of the Fondazione Luigi Einaudi* is a new international series of *Annali della Fondazione Luigi Einaudi* founded in 1967. The *Annals* aim at facilitating communication and encouraging cross-fertilization of ideas among both established international scholars and younger scholars working in the field of history, economics and other social sciences, with a view to fostering interdisciplinarity.

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SYMPOSIUM:  
ON THE RELATIONSHIP  
BETWEEN ECONOMICS AND OTHER  
SOCIAL SCIENCE DISCIPLINES

ECONOMICS AND OTHER SOCIAL SCIENCES:  
A HISTORICAL PERSPECTIVE

ROGER E. BACKHOUSE <sup>\*</sup>  
PHILIPPE FONTAINE <sup>\*\*</sup>

ABSTRACT

Since the Second World War, economists have often claimed that their discipline developed largely independently of other social sciences until it got closer to them again from the 1980s onward. Taking the story back to the end of the First World War, we show that there exists a rich history of interactions in which economists have learned from other social sciences. In the interwar period, attempts to promote interdisciplinarity were made to offset the shortcomings of too much disciplinary specialization but they concerned individual economists, notably institutionalists, more than economics as a whole. From the Second World War and in the two decades following it, the social sciences entered a cross-disciplinary age. Foundations, university administrators and scholars regarded multidisciplinary and interdisciplinary research as the key to solving social problems. Economists worked alongside mathematicians and natural scientists but they also participated in cross-disciplinary research ventures with other social scientists, an experience that often led them to depart from *homo economicus* and more generally from methodologies commonly taken to characterise economics. From the late 1960s, with the shift towards greater specialization, the interactions with other social scientists enjoyed less support and opportunity; they became much rarer and more individually driven, but remained significant as illustrated by the emergence and consolidation of behavioural economics.

**Keywords:** Economics, Social Science, Interdisciplinary Interactions, Social Problems, Funding.  
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## INTRODUCTION

As he concluded his Nobel Prize Lecture in Economic Sciences, in December 2017, the University of Chicago behavioural economist Richard Thaler remarked: “It is possible to do economics without *homo economicus*. If we learn from other social scientists, we can improve economics, we can increase its explanatory power and it can give us all kinds of new tools that we can use to improve people’s outcomes”. Two days later, in his Nobel Banquet Speech, he insisted: “To be sure, we still need traditional economic theories. But to make accurate predictions we need to enrich those theories by adding insights from other social sciences. Incorporating human behaviour into economic models improves the accuracy of economics”.<sup>1</sup>

By urging economists to turn to other social scientists, Thaler distinguished himself from other economists who emphasize instead the benefits associated with applying microeconomic tools to studying noneconomic topics. Thaler was not the first economist to take this position. The pioneers of behavioural economics in the 1960s – Herbert Simon, Richard Cyert, James March, George Katona and a few others – had already pointed out the importance of other social sciences for economics and by the mid-1980s George Akerlof (1984: 6) liked “to think that psychosocio-anthropo-economics is at the beginning of a period when many people will be working in this area”. Akerlof proved right: the 1990s witnessed the consolidation of behavioural economics and, ever since, suggestions like Thaler’s have multiplied within the economics profession (Sent 2004).<sup>2</sup>

Thaler’s conviction that economics should be more open to other social sciences needs to be placed in a longer historical context. The common wisdom is that economics was relatively close to other social sciences in the interwar period, moved away from them following WWII until it got closer to them again from the 1980s onward. Unlike what is often suggested, however, the movements of economics away from and towards the other social sciences did not occur in sequence but concomitantly. As postwar neoclassicism was endorsing methods from the natural sciences following the Second World War, a number of economists began to challenge economics’ behavioural assumptions in a way that suggested that greater attention to other social sciences was necessary for economists to

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<sup>1</sup> “Richard H. Thaler – Prize Lecture: From Cashews to Nudges: The Evolution of Behavioral Economics”. *Nobelprize.org*. Nobel Media AB 2014. Web. 8 May 2018. [http://www.nobelprize.org/nobel\\_prizes/economic-sciences/laureates/2017/thaler-lecture.html](http://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2017/thaler-lecture.html); and “Richard H. Thaler – Banquet Speech”. *Nobelprize.org*. Nobel Media AB 2014. Web. 8 May 2018. [http://www.nobelprize.org/nobel\\_prizes/economic-sciences/laureates/2017/thaler-speech.html](http://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2017/thaler-speech.html).

<sup>2</sup> For a more comprehensive history of behavioural economics, see HEUKELOM (2014).

offer improved understanding of economic phenomena. In other words, at the same time as some economists endorsed *homo economicus*, others in the profession doubted that this schematized model of an unlimitedly rational agent could provide a suitable foundation for a theory of economic behaviour. In the mid-1950s, for instance, the polymath Herbert Simon (1955: 99) wrote: “the task is to replace the global rationality of economic man with a kind of rational behavior that is compatible with the access to information and the computational capacities that are actually possessed by organisms, including man, in the kinds of environments in which such organisms exist. One is tempted to turn to the literature of psychology for the answer”.

Simon’s critique of economics’ traditional behavioural assumptions and emphasis on the need to look outside economics for inspiration was not new. Throughout the twentieth century and the beginning of the twenty-first, the debates concerning the relations of economics to other social sciences have crystallised around the issue of its behavioural assumptions and their limitations. Our research hypothesis is that the description of economics as estranging itself from other social sciences from the Second World War helped economists establish an image of “practitioners of a rigorous, dispassionate, and apolitical discipline” (Bernstein 2001: 152), but that it also obscured its actual transformation over the past hundred years. Though the gradual shift away from interwar pluralism to postwar neoclassicism is well-established (Morgan and Rutherford, 1998), that shift does not mean that economists stopped using insights from other social sciences over the period. Just as economists drew on other social sciences when their discipline was pluralistic, they continued to do so even as it became less pluralistic. Though the environment of economics (and other social sciences) changed dramatically after the war, the use of findings and approaches from other social sciences remained inextricably linked with the critique and amendment of *homo economicus*.

In what follows, we look at the current position of economics in relation to other social sciences in the United States by giving an account of their changing interactions over the past hundred years. It is difficult to examine that question historically without taking heed of the prevalent belief among economists that their discipline is special among the social sciences, having increasingly moved away from them after the Second World War (Solow 1997).<sup>3</sup> That belief played no minor role in minimizing the extent

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<sup>3</sup> Charles Camic (1995: 1006) uses the phrase *disciplinary separatism* for a different time period and describes it as “the assumption that during the 1890-1940 period, the various social sciences each developed in relative isolation one from another and from intellectual and organizational changes in other academic disciplines”.

of interactions between economics and other social sciences in the interwar era; it has likewise obscured the extent of such interactions in what may be called the cross-disciplinary age (1940s-late 1960s); finally, it has encouraged the perception that behavioural economists were the only ones to consider insights from other social sciences when the reality is that other economists have also looked outside their own discipline.<sup>4</sup>

## 1. ECONOMICS WITHIN SOCIAL SCIENCE: THE INTERWAR ERA

With the gradual professionalization of the U.S. social sciences from the late nineteenth century, at a time when “society became increasingly interdependent” [Haskell 2000 (1977): 15], came the view that the objective conditions were satisfied for the social sciences to part company with one another. By the first decade of the twentieth century, the departmental structure of the U.S. University was in place (Abbott 2001: 122); likewise, the main social science disciplines had their own professional societies: economics since 1885, political science since 1903 and sociology since 1905. There was a sense of consolidating disciplinary identity, occasionally prompting the fear that disciplinary isolation might gain ground.

By the late 1920s this process had gone sufficiently far that social scientists were occasionally lamenting the tendency to disciplinary isolation, conceding the beneficial achievements attending specialization but warning as well that too much departmentalizing and compartmentalizing could bring about evils (Ogburn and Goldenweiser 1927: 6; Kuhlman 1928: 583). It was believed that with accelerated social change, the interrelatedness of various segments of society had increased, with the result that social scientists could hardly hope to understand and solve problems by confining themselves to their own discipline. The degree of specialization, not specialization itself, posed problems. Pushed too far, specialization encouraged intellectual particularisms, making synthesis even more difficult, but in principle it did not hinder multidisciplinary approaches. Of the various social problems considered by social scientists in the 1920s, rare were those that were not recognized as falling in more than one disciplinary domain.

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<sup>4</sup> Julie Thomson Klein (1990: 56) emphasizes the distinction between multidisciplinary, interdisciplinary and transdisciplinary activities, arguing that they correspond to various degrees of integration, from the juxtaposition of disciplines, which is itself susceptible to various definitions, to the attempt to exchange concepts, tools and methods, and finally the construction of a relatively shared conceptual framework. As commentators have defined these adjectives in various ways, it should be noted that we use “cross-disciplinary” when no further detail is needed as to the nature of the connections between two or more disciplines, and “multidisciplinary” and “interdisciplinary” in the same way as Klein does.

The creation of the Social Science Research Council (SSRC) in 1923 helped address these concerns by providing flexible institutional structures supporting multidisciplinary research work serving the public interest (Worcester 2001: 14).<sup>5</sup> The SSRC started as a political science project under the leadership of Charles E. Merriam. Its primary purpose was to advance the research methods of political science and related social sciences by encouraging greater cooperation between the American Economic Association, the American Sociological Society, the American Political Science Association and the American Statistical Association (Crane *et al.* 1924). With economics, political science and sociology moving towards increasing use of quantitative methods, it is no accident that the original institutional basis for the activities of the SSRC was formed by the three core social sciences and statistics.<sup>6</sup> From the outset, likewise, the conviction existed among its officers that the SSRC dealt primarily with “cases where problems overlap the boundaries of one or more of the special fields concerned” (Merriam 1926: 185) so the question may appropriately be raised to what extent that conviction reflected the degree of interaction between economics on the one hand and other social sciences on the other, especially as the former held a leading position in the hierarchy of the social sciences in the United States (Camic 1987: 428).

Though economics has long taken a close interest in political questions, so much so that the term *political economy* has maintained a significant presence in the literature up to the present, by the beginning of the twentieth century, the only remaining manifestation of that connection in the United States was the occasional inclusion of political science courses in economics curricula. As far as research was concerned, the parallels between economics and political science as suggested by Pigou’s (1906) analogical reasoning found little echo in the United States.<sup>7</sup> Economists doubted that their discipline could benefit from drawing on political science especially at a time when the latter’s inclinations encouraged it to follow economics in its endorsement of quantitative methods. Interactions between the two disciplines were more likely to occur at the initiative of political scientists like Merriam who felt that their own discipline could learn from other social sciences.

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<sup>5</sup> The creation of SSRC was anticipated by a meeting of the American Political Science Association in December 1921 during which a Committee on Political Research, chaired by Merriam, was appointed to examine the scope and method of political research (KUHLMAN 1928).

<sup>6</sup> By 1925, however, the American Psychological Association, the American Anthropological Association and the American Historical Association had joined the SSRC. MERRIAM (1921: 180) noted that it was not desirable for the three main social sciences to be “based wholly and exclusively” upon statistical methods.

<sup>7</sup> Starting from the premise that both types of action originated in “mental facts”, Pigou analysed economic and political actions in terms of supply and demand.

Like many other political scientists in the 1920s, Merriam (1921) believed that the time had come for a “new science of politics”, but when it came to translate that inspiration into practice the interrelations between political science and economics seemed secondary in comparison with its interrelations with psychology and statistics (Somit and Tanenhaus 1967: 110).<sup>8</sup> Among the proponents of a more scientific political science in the 1920s, only George Catlin, the Cornell professor, was willing to achieve this through probing the analogy between politics and economics (Somit and Tanenhaus 1967: 116). This kind of exploration provoked little enthusiasm and Catlin himself saw its limitations (Wormuth 1961: 808). When the sociologist William Ogburn, who had just joined Chicago from Columbia University, together with the anthropologist Alexander Goldenweiser, edited the 500-page *The Social Sciences and Their Interrelations* (1927), these relations were devoted less than 4 pages whereas economics’ relations with other disciplines, especially anthropology, ethics, law, statistics, psychology, history and sociology received more substantial treatment.<sup>9</sup>

Of course, there were interactions between political scientists and economists, but they constituted exceptions. Within the SSRC, for instance, Merriam hoped to bring together a variety of viewpoints from social science disciplines to bear on the “problem of social behavior” with a view to helping solve social problems (Worcester 2001: 16). Even if Merriam received the support of the Columbia economist Wesley Mitchell and collaborated with him on a number of other occasions, including the Recent Social Trends survey (Smith 1994: 109), their interdisciplinary ambitions did not translate into substantial interactions between their parent disciplines.<sup>10</sup> Within research universities, the attempts to build cross-disciplinary research paradoxically reflected the high degree of departmentalism –

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<sup>8</sup> In his 1921 article, MERRIAM (1921: 180) referred to economics negatively when he argued: “We do not look forward, it is true, to a science of politics or of economics or of sociology based wholly and exclusively upon statistical methods and conclusions”. Yet, he did consider what sociology, psychology and statistics could bring to political science.

<sup>9</sup> The volume offered a useful, though extremely unequal, treatment of the interactions between social science disciplines. The eight chapters dealing with economics in relation to other disciplines testify to the importance of issues pertaining to interdisciplinary interactions within economics at the time and contrast with its subsequent image as estranging itself from other social sciences. Ogburn was the director of President Herbert Hoover’s Research Committee on Social Trends, running from 1930 to 1933, with Mitchell as chairman and Merriam as vice-chairman.

<sup>10</sup> It is usually recognized that along with Merriam, Mitchell and Beardsley Rumel played a central role in the early success of the organization (WORCESTER 2001: 18). Like Merriam, Mitchell was one of these “service intellectuals” who “never took a position on what should be done but regarded all social and political issues as simple problems of administration” (SMITH 1994: 27). On Rumel’s vision for the social sciences, see LEMOV (2005: Ch. 3). On Mitchell’s view of the integration of the social sciences, see Mitchell in WIRTH (1940: 113-122).

itself associated with the figure of the head professor – characteristic of the U.S research university in its early history. At Chicago, for instance, Merriam's efforts gelled with those of other social scientists who were of the same mind. The vision of collaborative research owed much to Merriam, the sociologist Albion Small and the economist Leon Marshall who made every effort to provide an institutional basis for interdisciplinary interactions. Realizations such as the Local Community Research Committee in 1923 left an indelible imprint on Chicago social science, encouraging a cooperative spirit between disciplines which had grown apart since the early twentieth century. Such was the imprint of that spirit that leading scholars thought it indispensable for social science research to have a single centre for itself and that one of the main reasons put forward for the erection of the Social Science Research Building was precisely to "foster cross-disciplinary contacts" (Bulmer 1980: 95). Yet, once again, the "reality of interdisciplinary cooperation was very different from its rhetoric" (102). At Chicago, the attempts to please the Laura Spelman Rockefeller Memorial's interdisciplinary ambitions for social science (Bulmer and Bulmer 1981) were real, but vested academic interests were strong as well. Both economists and political scientists played an important role in the Chicago experiment in cross-disciplinary social science research, but it would be an exaggeration to suggest that their interactions flourished because of that.

For the celebration of the 10th anniversary of the Social Science Research Building, in December 1939, the economist Mitchell listed a number of things he would have done better while studying business cycles had he been more familiar with the other social sciences (in Wirth 1940: 114-115). Though he conceded that a closer integration of the social sciences was desirable, he also pointed to the possible danger of disintegration within the several social sciences because of the multiplication of subfields. In other words, it was a disciplinary, not an interdisciplinary, issue, that held his attention. He believed that economists needed to keep an eye on the integration of their own discipline before they could think of playing a role in the integration of the social sciences. Nonetheless, from the example of the integration of economics, he drew a method for interdisciplinary integration: "stating some definite problem we wish to solve and then address our minds specifically to the ways in which that problem can be attacked" (152). The convergence of methods suggested parallelisms more than interrelations between the social sciences.

The relationships between economics and sociology have always been complex, partly because of their competing ambitions to dominate social science and partly because of what has often been considered their mutually exclusive behavioural assumptions, methodological orientations and ideological implications. As Daniel Geary (2010: 310) shows, in the post-



Second World War era, “there was a definite shift from complementary conceptions in the 1945-1968 period to competitive understandings ... in the period thereafter”. Even if Geary is more concerned with the way economists and sociologists defined their disciplines in relation one to the other after the Second World War, one needs to keep his conclusion in mind when considering their relationships in the interwar era, for the move towards lesser collaboration between the two disciplines has accompanied the turn away from interwar pluralism in economics.

It is remarkable that Talcott Parsons, the dominant figure of sociology between 1945 and 1968, moved away from economics (Brick 2000) in the interwar period and placed institutions at the core of sociology at a time when institutional economics was on the wane.<sup>11</sup> With the gradual affirmation of a neoclassical mainstream in economics, it became more difficult for a number of economists to assert their differences, among which was an emphasis on institutions as they determine the nature of the economic system. That transformation marked a significant shift in the relations of economics to sociology as they had existed before Parsons criticized institutionalism for being too sociological in a *Quarterly Journal of Economics* article in 1935 (Young 2009: 109) and drew an important distinction between the two disciplines in *The Structure of Social Action* in 1937 (see Geary 2010: 298).

Judging from the work of Parsons in the second half of the 1930s, the history of the relationships between economics and sociology sounds paradoxical. As the field of sociology placed institutions at its core and strove to achieve greater scientific legitimacy by drawing from the method of neoclassical economics, institutional economists included sociological elements in their theories (Camic 1987). In this respect, it may be argued that Parsons marked a profound change in the relations between economics and sociology if only because, as he challenged the ambitions of institutional economists for a comprehensive social science, he argued that it was for sociologists, not economists, to study economic institutions.

From the early twentieth century to the late 1930s, the relations between economics and sociology had been one of collaboration as argued by Young (2009). Young points out the large numbers of joint professional meetings and joint presidential addresses between the American Economic

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<sup>11</sup> Charles Camic (1987) makes clear that the dispute over methods between neoclassical and institutional economics was central to the formation of Parsons’s methodological views. In concentrating on the methodological implications of Parsons’s move into sociology from economics, Camic reveals purposes different from ours. However, he sheds light on the question of the relationships between the two disciplines because Parsons’s estrangement from economics dovetailed with his attempt to rescue institutions from economics at a time when their place in its subject matter was threatened.

Association (AEA) and the American Sociological Society (ASS) from 1906 to 1935 and concludes that economists (and statisticians) had a significant interest in sociology. It may be wondered whether *collaboration* is not too strong a term to characterize what remains, for the most part, organizational arrangements. Young himself is aware that theoretical considerations are much more useful when it comes to explaining the estrangement of economics from sociology. His idea is that a number of people, like Charles Cooley and Thorstein Veblen, whose work could be defined as sociological today, played an important role in establishing the identity of institutional economics both because they offered directions for an evolutionary and institutional explanation of economic behaviour and because they believed in the significance of the social dimension of economic phenomena. Young insists that in trying to construct an alternative to neoclassical economics, “institutional economists drew heavily upon sociology” (2009: 109), but he does not go into detail.

In considering the relation of economics to sociology in the interwar period, there is always the risk that our present conception of the two disciplines makes us see their earlier manifestations as equally mature and autonomous. That was not the case. Economics’ disciplinary boundaries were less permeable, which often prompted economists to take sociology for a stand-in for all social science disciplines other than their own (Geary 2010: 292). For instance, as he reviewed Edward Ross’s *Principles of Sociology*, the Harvard economist Thomas Carver (1920: 139) noted:

Almost the only general statement that one can make regarding sociology as actually taught in schools and expounded in treatises is that it is a refuge for all those who revolt against the cold, unemotional reasoning of the dominant school of economists, and who turn to sociology as a means of finding quasi-scientific labels for their sentimental whims.

Whatever conception economists generally entertained about sociology’s membership, Carver considered Ross’s sociology primarily a psychological science (141), endorsing “the views of such social psychologists as MacDougal [*sic*], Thorndyke [*sic*] and Veblen” (142). Despite his great experience of sociology, which he acquired while teaching the discipline at Harvard, Carver did not find much in Ross’s sociology that could help economists. Considering the relations of economics to sociology, he would not have necessarily disagreed with the suggestion of his Harvard colleague that the path to follow

consists not so much in emphasizing sociological or other elements within economics, as in working out a sociological supplement to an avowedly abstract economics. In certain respects this has formed one of the most important trends of

recent sociological thought which ... has been determined by this means of defining its relation to economics (Parsons 1935: 421).<sup>12</sup>

With sociologists taking responsibility for the relations between economics and sociology and defining the latter's task as supplementing the former, the insistence of institutional economists on incorporating sociological elements into economics became even less convincing.

It can be argued that the relation of economics to psychology underwent a similar transformation in the interwar period, the main difference being that economists were much more vocal about that relation. The idea that psychology is not necessarily relevant to economics has a long history in the discipline. Economists often assume that a better knowledge of psychology cannot substantially change their postulates, and so it has not been unusual for economists to endorse common-sense psychological notions without putting them to the test of psychology.

Following the introduction of marginal utility theory in the United States, a number of writers were not long in questioning the validity of its hedonistic psychological assumptions and suggesting new directions on the basis of a "new psychology".<sup>13</sup> Taking the occasion of the publication of *An Introduction to Social Psychology* (1909) by the English psychologist William McDougall, Mitchell (1910a, 1910b) began to reconsider the question of the relation of economics to psychology, suggesting prospects other than divorce.<sup>14</sup> Though it is not necessary to go into details with regard to his thesis, echoes of which can be found in Veblen, that the instincts are the prime movers to action, it should be emphasized that McDougall pointed to the indispensability of sound psychological knowledge for social

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<sup>12</sup> In his rather favourable review of Adolf Löwe's *Economics and Sociology*, Parsons recognized that economic man, competition and modern industrial technology – that is the "sociological middle principles" put forward by the German sociologist – offered the theoretical supplement needed by economic theory, but wondered whether it might not be possible to address that problem "on a higher plane of generality". He found that "[p]ure' economic theory ... is not *the* theory of a class of concrete phenomena but is part of a broader system of analytical theory on the same level of generality—the 'theory of action'" (PARSONS 1937: 480).

<sup>13</sup> The critique of hedonistic psychological assumptions was part of a more general critical movement which John M. Clark, in his presidential address at the annual meeting of the American Economic Association in 1935, characterized in the following terms: "for some years before the outbreak of the World War there was an increasing spirit of scepticism mixed with active iconoclasm, which before long came to seem the characteristic mood" (1936: 4). Some responded by seeking a new psychology whilst others did so by trying to eliminate psychology altogether.

<sup>14</sup> Even if he found hard to identify the differentiating characteristics of an "institutional economics", Paul Homan (1932: 13) nonetheless listed the "insistence upon the relevancy of psychological theory" among its influences.

scientists in general and accordingly made a special effort to make his ideas accessible outside psychology.<sup>15</sup>

Even though Mitchell (1910a: 113) found McDougall's recommendations especially appropriate for institutional economics, he confessed that "for the present our most conspicuous economists, in America at least, cultivate the types of theory which admit nothing beyond a formal contact with psychology". In assessing the interest of McDougall's psychology for economists, Mitchell (1910b: 213) pointed to an "environment which unremittingly drills everyone in the recognition and acceptance of pecuniary motives and pecuniary calculations as norms of conduct". He invited economists not to take economic rationality for granted, but to consider instead the conditions of its formation, which suggested in turn the possibility that it did not equally apply to all economic agents and sectors of the economy.<sup>16</sup> Following the publication of *Business Cycles* and his appointment at Columbia, Mitchell (1914) wrote a survey of the literature on human behaviour and economics. There he meant to counter what he saw as the nascent conviction in economics that non-intercourse with psychology should be a matter of policy.<sup>17</sup> Accordingly, he suggested that hedonistic preconceptions could be given up without much consequence for economics and contended that writers as diverse as Maurice Parmelee, Edward Thorndike, Graham Wallas, Veblen, Werner Sombart, Walter Lippman and William Walling had begun to build a theory of human action that went beyond hedonistic principles and could enrich the psychological toolbox of economists. From the list of names above, it should be clear that Mitchell's actual engagement with psychological knowledge was rather fragmentary and instrumental. It mostly concerned a variety of studies of human nature whose psychological principles could help transform economics into a science of human behaviour.

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<sup>15</sup> After the First World War, psychologists seemed even more persuaded of the usefulness of psychological knowledge to other social sciences though they tended to consider its contribution through the participation of psychology in the world of practical affairs. Accordingly, they emphasized solutions to business problems – most notably the employer-employee relation and the seller-buyer relation – more than economic problems (see KINGSBURY, 1923 and more generally the special issue of the *Annals of the American Academy of Political and Social Science* devoted to applications of psychology published in November 1923).

<sup>16</sup> About Mitchell, Dorothy Ross wrote: "Working on Veblen's evolutionary model, with its instinct psychology, he concluded that economists needed more psychological training" (1991: 379).

<sup>17</sup> A good illustration of that conviction is found in Whitaker's (1916) analysis of Frank Fetter's (1915) effort in *Principles of Economics* to reconstruct the theory of value on the basis of the new psychology after having cast out hedonism. WHITAKER (1916: 440) writes: "The utility theorist could say that goods are necessary for the *realization of instinctive impulses*, and then eliminate the word 'satisfaction' and explains his points all over again. But to what end.... What advantage would be gained by saying that value ... depends on the *marginal enabling power of goods to further the realization of instinctive impulses?*"

By the late 1910s, another institutional economist – John M. Clark (1918a, 1918b) – provided a more systematic survey of the relations of economics to psychology. The context was different: the world, including the United States since April 1917, was at war, experiencing significant societal changes that raised the question of possible readjustments in theory. The hope for amended behavioural assumptions in economic theory found greater resonance.<sup>18</sup> As Clark (1918a: 2-3) noted, “the kind of economic theory suited to the twentieth century and its place in the growing body of differentiated studies and activities are yet to be decided, and they will be different from what they have been in the past”. It is important to note that Clark’s effort started with a desire to go beyond the static point of view and static assumptions, which resulted in an “attempt to square economic theory with modern psychology” (3).<sup>19</sup> Yet, Clark’s critique of the view of human nature in marginal utility theory and his subsequent outlining of a theory of the guidance, formation and determination of economic choices (Clark 1918b) does not show that he was drawing extensively on ideas from modern psychology; instead, it shows that he recognised that, since “man is molded by his environment” (Clark 1918a: 6), a new kind of “economic man” needed to be constructed, whose inspiration some attributed to the “behavioristic school of economists” (Carver 1918: 195).<sup>20</sup>

Though the membership of that school is not as precise as one might expect, economists like Lawrence Frank (1924) specifically argued for the use of a behaviouristic method in economics. The author of a study commissioned by the Laura Spelman Rockefeller Memorial on the status of social science in the United States, Frank had an overview of social science (Bulmer and Bulmer 1981: 371–377). Moreover, as an institutional economist and a key associate of Ruml at the Memorial from June 1923, he could not but be aware of the debates concerning the role of psychology within

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<sup>18</sup> By the mid-1920s, Frank Knight (1925a: 406) noted: “The World War and its aftermath have greatly accentuated this shift in emphasis and change in standards of judgment. Critical attention has been focused ... on the practical consequences which follow from the Machiavelian-Mandevillian standards which make intelligent selfishness equivalent to virtue, and power and cunning the main components of our human ideal. The values which animate our economic activity are being made explicit and subjected to critical scrutiny as has not been done before since the industrial era began”. In the same vein, Craufurd Goodwin (2016: 117) notes the psychologists’ charge after the Second World War “that the selfish competitive behavior glorified by economists had led perhaps to intolerant and mutually destructive attitudes when carried over to relations among nations”.

<sup>19</sup> José Edwards (2016) offers an instructive account of the impact of behaviourism on American institutionalism.

<sup>20</sup> Note however that for Carver “‘behaviorism’ fits into the classical scheme of economics” (1918: 199). For a more nuanced view of the necessary psychologizing of economic theory, see DICKINSON (1919).

economics and its possible reform. He thought that economic problems and their solutions were but “variations on a single theme: the reconciliation of the antithetical concepts of a system of economic forces and of human volition or autonomy” (1924: 17). His suggestion was to regard “human behavior as a learned response to a stimulus” (25), which placed economics (political science and sociology) in a relation of dependence to psychology to the extent that it studied the learning process. Psychologists occasionally supported that orientation in leading economics journals. Thus, a Northwestern University psychologist and the author of one of the first books on the psychology of personal selling, A.J. Snow (1924) thought that though it was not for psychologists to provide psychological notions to fit existing economic theories, they could nonetheless produce case studies relevant to economics.

From the beginning of the twentieth century, the repeated efforts of a number of economists to convince their peers to make effective use of psychology created the appropriate conditions for increased communication. Nevertheless, the long practiced non-intercourse with psychology continued to dominate the profession. Few economists shared the enthusiasm of Mitchell, Clark and others. By the mid-1920s, even Mitchell had altered his position regarding the relation of economics to psychology. As one of the “sciences of behavior”, economics could not turn away from psychology, but it was “equally naïve to talk as if the economist borrowed or could borrow all of his psychological notions from the psychologists” (cited in Young 1925: 177-178; see also Ross 1991: 384-385). It made little sense for economists to borrow ready-made notions from psychology, but greater attention to what psychologists were saying was particularly useful if they wanted to avoid making false psychological assumptions (Dickinson 1924).

More important than Mitchell’s clarifications and the occasional pronouncements for a more reserved appreciation of psychology, however, was Frank Knight’s (1925a, 1925b) critique of the behaviouristic temptations of economics. In two notable articles, the then University of Iowa economist considered the economists’ “urge to be ‘scientific’ in the manner of the laboratory sciences” (1925a: 372) and defined *behaviourism* as “the application of scientific method in the strict sense, as developed in the natural sciences, to the study of human phenomena” (1925b: 247). Knight concluded that the behaviouristic interpretation of human conduct did not work in economics (and for that matter in social science) because knowledge of what one observes presupposes “intercommunication between minds as conscious centers” (1925a: 398) in the sense that such intercommunication is a condition of observation itself. Behaviourism, which dismissed what cannot be observed and treats human beings as if they were unconscious organisms reacting to the environment, neglected the fact that the communication

with other minds is necessary to acquire knowledge about human phenomena. In Knight's words "man is *more* than an observed object" (1925b: 248).<sup>21</sup>

By the late 1920s, the criticisms against marginal economics had given way to more positive types of study, including some that would flourish after the Second World War. The shift away from the critical mood, which became more marked during the 1920s, encouraged the relegation of the question of the relation of economics to psychology to the periphery, even if institutional economists strove to keep the conversation alive. Typically, J.M. Clark (1936), in his presidential address to the annual meeting of the AEA, mentioned the attacks on hedonistic psychology in passing as part of the past, and concentrated on the role of economists in policy making. A few months before Maynard Keynes published *The General Theory of Employment, Interest and Money*, Clark was more interested in the role of economists in shaping policy. He found that only a few economists had played a strategic role and lamented that even "the consensus of economists has no absolute authority, and no right to claim it" (1936: 8). The Great Depression had created a sense of urgency among economists, making the use of their knowledge a priority over their interactions with other social sciences.

In the U.S., following the First World War, "the internal problems of the society of the metropole became the intellectual center of sociology" (Connell 1997: 1535) while indigenous North American cultures had already been the province of anthropology for a couple of decades.<sup>22</sup> As anthropology carved out a place for itself within social science, the question of its relation to economics could be considered independently of that of sociology even if the latter also studied the social context of economic life. It was appropriate therefore for the sociologist Ogburn and the anthropologist Goldenweiser to include in their 1927 volume a chapter on the relationships between anthropology and economics in the U.S. context. Written by N.S.B. Gras (1927), this chapter distinguished "anthropological economics", taken as "the study of the ideas that primitive peoples held about economic matters" and "economic anthropology", taken as "the study of the

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<sup>21</sup> Morris Copeland (1925: 147), an institutional economist who had written his doctoral thesis under J.M. Clark, rejected Knight's implication that behaviourist psychologists and institutional economists were "seeking to apply the methods of mechanics to man" and argued instead that they "believe their subjects to be biological sciences, and they employ concepts and (statistical) methods appropriate to classes of which the individual members differ from one another, to species that originate and evolve".

<sup>22</sup> At the turn of the century, in his attempt to provide a definition for "economic man", the American anthropologist Albert E. Jenks (1902: 201) already emphasized the gap between the beliefs and practices of the "primitive American" (in that case an Ojibwa) and the "modern American". He found that gap "nowhere more marked than it is in the realm of material possessions". Jenks takes the modern American as an illustration of economic man which he defined as "one who produces or traffics for future gain" (203-204).

way in which primitive peoples obtained a living” (10). Interestingly, Gras set aside the former study on the pretext that the ideas of primitive peoples about economic matters would be vague or confused with other matters and concentrated instead on economic anthropology. A subfield within anthropology, the study of the economic activity of primitive peoples had crystallised disputes around economic man from the twentieth century.

Philip Mirowski (1994a: 314) has provided a reminder of George Stocking’s advice to look at German Historicism if one wants to understand the early history of anthropology and, by extension, the history of economic anthropology. Talking about German Historicists, he noted:

By shifting the premises of the Natural to contemporary ‘savages,’ a novel method of attack upon British precepts could be mounted. One could raise various counterexamples to *homo economicus* from the ethnographic reports and then assert that the Nature of the British did not resemble the Nature found in the bush. This is indeed the standard trope of all the early writings in economic anthropology, up to and including that of Malinowski and Mauss (316).

Bronislaw Malinowski’s (1921) “The Primitive Economics of the Trobriand Islanders” indirectly shed light on the historical antecedents of economic anthropology. A cursory reading of that article could convince its reader that Malinowski’s primary intention was to fill the gap in the existing ethnological knowledge about the economic life of traditional societies so that interested economists could determine whether their theories were applicable to societies other than theirs (see also Firth 1927: 312). That reading is not necessarily mistaken, but it masks a more important point, namely that in these societies economic activities are organized on a social basis, with “one constant flow of gift and counter-gift” (Malinowski 1921: 8). Malinowski’s observation that economic considerations pervade the social life of traditional societies was meant to provoke interest among economists.<sup>23</sup> However, rather than expressing the dominance of the economy, it conveyed its subjection to the social obligations of gift and counter-gift and therefore implied a study of primitive economies that, instead of denying economic organization altogether, embedded it with the broader social structures.<sup>24</sup>

In the anthropological imagination, the above article was supplanted by Malinowski’s *Argonauts of the Western Pacific* (1922), notably its contribu-

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<sup>23</sup> We do not mean to suggest that economists were not interested in anthropology before Malinowski’s effort in their direction. Veblen, for instance, had couched the description of “the modern economy in the language of anthropology and ethology” (PEARSON 2000: 961). As co-editor of the *Economic Journal*, Maynard Keynes himself encouraged the dialogue between the two communities (GREGORY 2000: 999).

<sup>24</sup> Heath Pearson (2000) contrasts Malinowski’s representation of “primitive man” with earlier representations which stressed his lack of economic sense.



tion as an analysis of the Kula and its “astringent implications for Western conceptions of ‘primitive economics’ and its relationship to Western economics” (Mirowski 1994a: 319). There is no need to go into detail here. Suffice it to say that the Kula represented a form of exchange that fit uneasily within the utilitarian framework. At best, its analysis opened the door for alternative explanations of exchange; at worst, in putting the gift into the picture, it weakened the role of self-interest as a foundation of social harmony. As noted by Mirowski, the other major theorist of gift and exchange before the Second World War was Marcel Mauss (1923-1924) with his *Essai sur le don*. Whatever reading we reserve to the *Essai* [Mirowski (1994a: 325) argues that Mauss “can be read as very good example of German Histori-cist economics”], we should bear in mind that for Mauss giving, receiving and reciprocating are obligations. As a result, the “reason for exchange” could not but differ from that implied by “economic man” and the self-interest motive, especially as Mauss’s “fertile idea was to present the gift cycle as a theoretical counterpart to the invisible hand” (Douglas 1990: xiv).<sup>25</sup> How much of Mauss’s analysis entered the reflection of American anthropologists in the interwar period is something we have yet to determine but there is little doubt that some of his ideas circulated even before the translation of the *Essai* appeared in 1954.

As he considered the “functional study of economic institutions”, Raymond Firth (1927: 333) mentioned the work of Malinowski and Mauss and a few others but found that American anthropologists such as R.H. Lowie, Clark Wissler and Goldenweiser, despite their sympathy for this approach, were only incidentally interested in economic problems. In this respect, the exchange between the American anthropologist Melville Herskovits and Knight in the *Journal of Political Economy* in the early 1940 is something of an oddity. “Anthropology and Economics”, Knight’s review essay of Herskovits’s *The Economic Life of Primitive Peoples*, may serve as a pretext for concluding this section as it testifies to the way a number of economists saw the relationship between economics and other social sciences as the U.S. was about to enter war.<sup>26</sup> Knight (1941: 253) wrote:

The first very ‘crying need’ of social science in general, at the present juncture in history, is clarification on the old, old question of the relations between induction and deduction. The point of this observation just here is that to no small extent this means in practice the relation between other social sciences and economic theory. For the latter is the one social science which effectively uses inference

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<sup>25</sup> It should be remembered that the subtitle of *Essai sur le don* reads: “Forme et raison de l’échange dans les sociétés archaïques”.

<sup>26</sup> On Herskovits and the economists see PEARSON (2010: 171-77).

from clear and storable abstract principles, and especially intuitive knowledge, as a method. In contrast with it, all other social sciences are empirical, including those which use the word 'economics' (or 'economic') in their designation.<sup>27</sup>

Seen in this light, the field of primitive economics stood in stark contrast with economic theory: its main orientation had long been to challenge the abstract principles of the latter by using traditional societies as illustrations of their inapplicability. For Knight, economic principles could not even be "approximately verified – as those of mathematics can be, by counting and measuring" (254) so the efforts of anthropologists and other social scientists in that direction were ill-conceived. Knight (1940) also targeted institutional economics and he objected strongly to Terence Hutchison's arguments that economic theories should be testable. If, as he believed, the reason for the difficult relation between economics and other social sciences was method (and incidentally the hostility against the abstract principles embodied in "economic man"), the ascendancy of hypothetico-deductive modelling in economics and its move away from the descriptive exposition of facts in the 1940s did not presage increased cooperation with other social sciences. In his rejoinder to Knight, Herskovits (1941: 269) showed himself perfectly aware of the difficulty for interdisciplinary studies such as his to reach critical mass and confessed that it could be even greater "if the deductive point of view stressed by Professor Knight comes to be the accepted approach".

## 2. ECONOMICS IN A CROSS-DISCIPLINARY AGE, 1940s-LATE 1960s

Herskovits's worries were not altogether unfounded, for the Second World War was a turning point in the social sciences, one that confirmed economics' gradual shift towards hypothetico-deductive modelling. That development, however, occurred in a context where interdisciplinary studies were all the vogue. As Jamie Cohen-Cole (2014: 77) reminds us, "[b]y the end of 1939, the SSRC and social scientists it supported were absolutely convinced that the best way to proceed in social science was to frame a problem and then attack it from multiple directions using the techniques of several disciplines". The SSRC and its beneficiaries, foundations and university administrators: all venerated interdisciplinarity, there was still a gap between intentions and realisations, but the war helped narrow it. From

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<sup>27</sup> It is worth noting Knight's idiosyncratic reference to "economic theory" as a social science, and his implication that it is a social science distinct from empirical economics.

the early 1940s, indeed, social scientists were recruited into the war effort on a large scale, often to work in problem-oriented, as opposed to disciplinary-oriented, environments. The experience continued with “nearly 250 new interdisciplinary social science research institutes created in the first twenty years after the war” (Crowther-Heyck 2006: 421) so that the period spanning the early 1940s through the late 1960s can aptly be described as a cross-disciplinary age (Fontaine 2015).

The enthusiasm for interdisciplinary social science came together with a conception of what it meant to be scientific based on method rather than object of study. In the late 1940s, Donald Marquis (1948), the University of Michigan psychologist and fervent supporter of interdisciplinarity, explained in detail why the social sciences, including economics, were lacking in comparison to the natural sciences. They did not follow the sequence of six steps believed to characterize the scientific process: problem formulation, review of knowledge, preliminary observation, theory construction, verification, and theory application. Marquis believed that interdisciplinary cooperation could help the various social sciences acquire all steps in the scientific process.

Given that economists had already begun to build their scientific legitimacy on the idea of methodological rigor associated with the use of mathematics, they had little incentive to bow to Marquis’s exhortation to undertake interdisciplinary research with other social sciences.<sup>28</sup> Whatever the nature of their wartime experience, a perception had emerged by the late 1940s that economics could be like engineering – a technical discipline that possessed mathematical tools that could be used to solve logistic and other allocation problems.<sup>29</sup> The prime example was linear programming, developed by Tjalling Koopmans at the Combined Shipping Adjustment Board and then sponsored by the US Air Force: linear programming was technical, seemingly non-ideological and brought economists close to applied mathematicians. But this was one of many instances of economists working alongside mathematicians and natural scientists.

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<sup>28</sup> We are aware that mathematically based rigor, as suggested by Roy Weintraub (1998), is far from being univocal.

<sup>29</sup> Needless to say, that perception was largely encouraged by the history of science’s focus on the history of natural sciences even if in the past 25 years or so the balance has tilted in the direction of the social sciences (see ISAAC 2007, 2011). It is telling that while he pointed out valuable developments in the history of the social sciences in the mid-1990s, I. Bernard Cohen (1994: xii) mentioned the efforts of scholars who had “begun to study the history of the social sciences, taking cognizance of the interactions with natural sciences”. By paying special attention to the relationships between economics on the one hand and the physical and biological sciences on the other, economists also encouraged that perception (for instance, MIROWSKI 1989, 1994b).

At the Statistical Research Group, at Columbia, Milton Friedman, George Stigler, Allen Wallis and Abraham Wald were heavily involved in improving the effectiveness of anti-aircraft operations, optimizing the number and type of pellets in a shell, assessing the vulnerability of aircraft to attack, and how far from its target a proximity fuse should be detonated (Guglielmo 2008: 141; Friedman and Friedman 1998: 131-144; Wallis 1980). Friedman even claimed that his analysis of an experiment on the performance of different alloys at the high temperatures found in aircraft engines had a “major effect on [his] approach to empirical work for the rest of [his] professional life” (Friedman and Friedman 1998: 143). Paul Samuelson, was less enthusiastic about his work on designing radar-guided artillery in MIT’s Radiation Laboratory but he was impressed with the mathematicians and physicists alongside whom he was working (Backhouse 2017: 342-439).

In the Office of Strategic Services (OSS), economists Richard Ruggles, Sidney Alexander and William Parker used serial numbers from captured German equipment and documents to estimate production levels (Guglielmo 2008: 123). For example, they established that most tyre production was done by five firms, and they estimated the ability of Germany to substitute synthetic for natural rubber in the event of a shortage of the latter. They also produced estimates of Soviet strength that proved more accurate than those produced by more traditional intelligence methods. Economists, including John Kenneth Galbraith, were involved in the US Strategic Bombing Survey, which tested some of these estimates (Galbraith 1981: 196-197, 213-215; Parker 2005: 174-184).

In many of these examples, economists were essentially working as general, technical problem solvers – working as statisticians as much as economists. In other agencies, such as the Office of Price Administration, the Bureau of Labor Statistics, the Department of Commerce, the Federal Reserve, the National Resources Planning Board, and the War Production Board, they were solving strictly economic problems (see, for example, Backhouse 2017: 382-413, 479-488; Galbraith 1981: 124-175; Parker 2005: 142-152). National income accounting and the application of Keynesian techniques had played a significant role in the mobilization of resources for the war. Robert Nathan and Simon Kuznets established a system for planning military procurement that was considered a great success (Perlman 1996: 217; Lacey 2011). Most of these instances of economists acting *qua* economists are located in settings that were customarily in their domain, but specifically economic techniques also proved useful in military problem-solving. Linear programming is one example. Another was the use of Wassily Leontief’s input-output analysis to estimate German vulnerability to strategic bombing (Guglielmo 2008: 133-134). Such developments, which distanced economists from other social scientists, were reinforced by the belief that economics had been very

successful in the war. Samuelson no doubt had many of these examples in mind when he claimed that it had been an “economists’ war”. Such attitudes were a symptom of great self-confidence, encouraging the belief that there was something distinctive about economics that placed it in a different sphere from the other social sciences.

However, economists did not just work alongside natural scientists. To begin with, a number of them continued to be affiliated with bi-disciplinary social science departments. Such arrangements were often a matter of administrative convenience and reflected economics’ leading position in the hierarchy of the social sciences, but they implied at least limited awareness of other disciplines and sometimes encouraged collaboration.<sup>30</sup>

More importantly, starting with the war, the association between economists and other social scientists concerned research and not just teaching. We have already mentioned the contributions of economists to military intelligence though the idea was to emphasize their use of quantitative methods to solve practical problems. The way economists saw their interactions with other social scientists at the Research and Analysis (R&A) Branch of the OSS, for instance, seems to confirm that they were not especially sympathetic to the work of their colleagues, but the story may be more complicated than it appears at first glance. Thus, Barry Katz’s (1989) analysis of the R&A Branch points to the impatience of economists with some of their colleagues in other social sciences, but it he did not rule out the possibility of influence. To the contrary, Katz observed that “in time the attitude of the economists softened and they came to admit political, historical and even psychological categories into their scrupulously quantitative domain” (98). The story of the R&A Branch is particularly interesting as it was first organized into four autonomous functional divisions, leaving economists at some distance from sociologists, historians and geographers, but was later (in 1943) reorganized to encourage multidisciplinary, if not interdisciplinary, research. As Katz aptly observed, “for the economists to accept the humbling status of epistemological parity with the other social sciences represented a dramatic challenge to the intellectual history of the discipline”, and yet, “an intellectual *modus vivendi* came to be achieved between the economists and their humanistic brethren, the invigorating consequences of which were felt long after they were demobi-

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<sup>30</sup> For example, at MIT, the Department of Economics and Social Science, though dominated by economists, included psychologists and political scientists. Even Robert M. Solow, who speaks of the estrangement of economics from other social sciences after the Second World War, contributed to a book by a political scientist. Likewise, Paul Samuelson, trained in multi-disciplinary social science at Chicago, repeatedly contributed to projects organized by other social scientists.

lized and had returned to their academic posts” (103). As suggested by Katz in his concluding chapter, the legacy of the R&A Branch is to be found in research universities themselves and the various cross-disciplinary ventures they supported in the two decades following the Second World War.<sup>31</sup>

One such venture was the Center for International Studies (CENIS) created after the State Department approached MIT president James Killian about organizing a study on the problem of how best to communicate with populations behind the Iron Curtain (Blackmer 2002; Gilman 2003). A multi-disciplinary team was formed in the summer of 1950, including notably psychologist Alex Bavelas and economist Max Millikan from MIT, to work on the question raised by the State Department. Later, the activities of the team were referred to as “Project TROY” whose final report in early 1951 encouraged Killian to support three follow-on research initiatives, including a study of Soviet society, under the directorship of MIT economist Walt W. Rostow (formerly at the R&A Branch), a defector interview and research program led by anthropologist Clyde Kluckhohn, and the “overload and delay” program (on disrupting communications within the Soviet Union) conducted by Bavelas. It likewise suggested that a permanent research centre, CENIS, should be established under the leadership of Millikan, in early 1952, following his one-year service as assistant to the director of the CIA (Needell 1993: 416-7; Needell 1998: 22-24).

It is difficult to know exactly what CENIS economists gained from “non-stop interdisciplinary seminars and discussion groups, in which not only CENIS staff but almost all of MIT’s anthropologists, economists, political scientists and sociologists participated in varying degrees” (Higgins cited in Lodewijks 1991: 287), but it is clear that the way they approached growth, for instance, was different from that of economists who regarded their interactions with other social scientists as less of a priority.<sup>32</sup> As Nils Gilman notes, the name most commonly associated with modernization theory is that of Rostow (Gillman 2003: 190). That says something of his in-

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<sup>31</sup> GUGLIELMO (2008: 145) follows Katz’s assessment of the interactions between economists and other social scientists at the R&A Branch: “However, as they [the economists in the OSS] worked with historians, geographers, and political scientists, they came to realize that these other fields had something to offer to the study of economics. Many of them subsequently achieved distinction in their academic careers in areas of applied economics that tend to be interdisciplinary, including international trade (Alexander, Salant), economic growth and development (Mason, Despres, Rostow, Ruggles, Malenbaum, Abramovitz), economic history (Parker, Kindleberger), and the economics of defense (Hitch), education (Kaysen), and natural resources (Morse)”.

<sup>32</sup> LODEWIJKS (1991: 307) describes Rostow’s approach as “non-neoclassical as well as non-Marxist”, an approach that “involves a great many endogenous variables... all influenced by noneconomic factors and to be handled through a multidisciplinary approach, a development that disturbs economists”.

tellectual influence on the movement. Rostow and Millikan's [1998 (1954): 39] statement that the "growth of the free world economy is important to us for much more than economic reasons" suggests however that they had good reasons to hear what other social scientists had to say at CENIS.<sup>33</sup>

Were it not for lack of space, we could detail other accounts of the participation of economists in cross-disciplinary research ventures in the post-war era: the building of an interdisciplinary community by Herbert Simon at the Carnegie Tech's Graduate School of Industrial Administration (Crowther-Heyck 2006b; Khurana 2007), the interdisciplinary education of Alexander Gerschenkron's economics students at the Harvard Russian Research Center (Engerman 2010), the role of economists and home economists in the multidisciplinary network of poverty experts in the 1950s and 1960s (Huret 2010; see also Fleury 2010), the transformation of economist Kenneth Boulding into a general social scientist following his immersion in the extremely interdisciplinary environment of the University of Michigan in the 1950s and 1960s (Fontaine 2010), and George Katona's effort to build an adaptive theory of consumer behaviour on the basis of the socio-psychological principles of social learning and expectational dynamics within the Survey Research Center at Michigan (Converse 1987). Again and again, we would find similar efforts, often with roots in wartime experience, to break down disciplinary boundaries with a view to solving practical problems. These efforts, it should be emphasized, departed from "economic man" and methodologies commonly taken to characterise economics such as Friedman's advocacy of unrealistic assumptions.<sup>34</sup>

The examples above should not hide economists' reservations about interdisciplinary interactions. In this respect, given recent developments, it is ironical that economists decided to stay away from the Ford Foundation's Behavioral Sciences Program (BSP) (Pooley and Solovey 2010). One of the most ambitious cross-disciplinary research ventures of the mid-century, that programme meant to foster a scientific approach to human behaviour through the application of methods inspired from the natural sciences. Even though their fellow social scientists within the behavioural sciences movement endorsed the use of mathematics and statistical methods, economists, with a few significant exceptions such as Simon and Ka-

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<sup>33</sup> On the contributions of the histories of modernization to the history of recent economics, see FONTAINE (2016).

<sup>34</sup> It might be objected that the RAND Corporation, as one of these cross-disciplinary settings, deserved special attention. There economists and social scientists were organized in separate divisions and there do not seem to be many examples of the former drawing on the work of the latter. Instead, we find social scientists occasionally endorsing "gaming", a heuristic practiced throughout RAND, "even if it was only to the extent that they hoped to reformulate a commonly used method on their own terms" (see BESSNER 2015: 32).

tona, showed little sympathy towards BSP. As Pooley and Solovey suggest, the economists' "spirit of go-it-alone superiority" played a role. In any case, the fact that economists could stay away from the behavioural sciences movement at a time when interdisciplinarity was at its peak did not presage greater readiness to interact with other social scientists when the intellectual climate would favour specialization. With the gradual emergence of a new patronage system oriented towards disciplines, and with economists becoming more self-confident, the interactions with other social scientists enjoyed lesser support and opportunity; they became much rarer and more individually driven, but remained significant.

### 3. ECONOMICS IN A DISCIPLINARY AGE, LATE 1960S TO THE PRESENT

Hunter Crowther-Heyck (2006b) has usefully distinguished between two overlapping patronage systems for post-war social science: one spanning 1945 through the mid-1960s focused on interdisciplinary, problem-centred research and the other, starting in the late 1950s, involving more discipline-oriented patrons. By the 1970s, the second system, organized around the National Science Foundation (NSF) and the National Institutes of Health (NIH) had imposed its new vision centred on methodological sophistication: "The program officers at these agencies consciously sought to promote research that would advance the several social sciences as disciplines, especially work that would lead to methodological or instrumental advance". Whereas their counterparts in the first system had regarded advances in fundamental research as crucial for solving practical problems, the programme officers of the second system believed that there was an important distinction between basic and applied social science; they "tended to see applied social science as the application or dissemination of existing social scientific knowledge" (Crowther-Heyck 2006b: 434).

The way economists saw their own discipline also changed. Economics began to be seen as comprising a "core" of pure theory that could be applied to various problems and greater weight came to be attached to mathematical rigor, a change that affected both economic theory, where greater importance was attached to proving theorems, and empirical economics, where formal statistical tests were expected (increasingly easy to do given developments in IT). This went along with a narrowing of the way economic problems were approached in many fields. In macroeconomics, there was a move from treating markets as institutionally rich oligopolistic structures that were too complex to analyse rigorously, to seeing them as perfectly competitive structures in which aggregate behaviour could be modelled as essentially mimicking the behaviour of individual, rational



agents. In the study of economic development, there was a decline in interest in “grand theories” that saw the behaviour of people in poor countries as different from that of people in developed countries, perhaps needing input from anthropology, to seeing them as rational optimisers. The application of rational choice theory to social problems was connected to this narrowing of economics.

The above changes in patronage systems and in economics were not conducive to a rapprochement between economists and other social scientists. The methodological sophistication of much technical economics fostered self-confidence and the benefits of interactions with other social scientists during and after the war were eventually forgotten. The move away from interdisciplinarity could be interpreted as confirmation that economists’ earlier reservations about the behavioural sciences movement were justified. By the early 1970s, economists had developed a feeling of self-sufficiency that allowed them not only to ignore other social sciences when tackling economic problems but also to apply their own methods to problems that lay within the domains of other social sciences. There was little attempt to learn from other social sciences.

Exemplary of that literature were Becker’s analysis of discrimination and Anthony Downs’s work on democracy in the late 1950s, soon followed by James Buchanan and Gordon Tullock’s analysis of public choice in the early 1960s and a number of other significant instalments in the growing literature on what came to be called “economics imperialism” in the first half of the 1970s.<sup>35</sup> By 1976, with the publication of Becker’s (1976) *The Economic Approach to Human Behavior*, there was little doubt that that approach was firmly established and that it impinged on the domains of political science, sociology, anthropology, and law. Characterised by a very different conception of interdisciplinary work, involving great flexibility towards other research cultures and disciplinary traditions (Backhouse and Fontaine 2010: 207-216), psychology remained untouched by the first incursions of economists into other social sciences.

Though “economics imperialism” is outside the scope of this paper, it is important because it illustrates a significant transformation in the way a growing number of economists saw their relationships with other social sciences. As Becker emphasized in the introduction to *The Economic Approach*, he was not “trying to downgrade the contributions of other social scientists” (Becker 1976: 14). However, neither was he trying to establish the best way to integrate the political, sociological, psychological, anthro-

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<sup>35</sup> The appropriateness of the imperialism metaphor has been challenged, but this does not affect any arguments made here.

pological and economic approaches. Instead, he sought to affirm the distinctiveness of the economic approach and to propose its application to issues that had, until then, remained outside the scope of economics. It was premised on a re-definition of economics in terms of its method rather than subject-matter.<sup>36</sup> This re-definition of economics could be read as implying that economics was methodologically more sophisticated than other social sciences, implying that disciplines were competitive rather than complementary. This approach was not completely foreign to economists whose attraction to other social sciences had never been unconditional but it acquired a new momentum.

Not all economists adhered to Becker's ambitions for the discipline and some even doubted whether microeconomic tools should be applied outside economics when it was not even clear they fully applied inside it. One such economist was the Hungarian-born author of *The Joyless Economy*, a book published in the same year as Becker's *The Economic Approach*. It is impossible to do full justice to the richness of Tibor Scitovsky's book, but there is no question that its orientation was the opposite of Becker's. Unlike "economics imperialists", Scitovsky (1976 [1992]) interested himself in psychology, its theories and experimental findings, and he also believed that observing behaviour the way behavioural psychologists did could help improve the economists' theory of consumer behaviour.

Drawing on experimental psychology, especially the work of Daniel Berlyne (see Bianchi 2016), Scitovsky argued that the assumption of a rational consumer suffered limitations. Starting with the three motive forces of behaviour described by psychologists – "drives to relieve discomfort, stimulation to relieve boredom, and the pleasures that can accompany and reinforce both" (1976 [1992]: 78), he proposed a simplified version of that classification with the distinction between comfort (the economists' satisfaction) and pleasure (the hedonic value associated with changes in arousal), and made the choice between the two the centre of his argument. That distinction served to illustrate in turn cultural differences between the U.S. and European societies, with the former being described as being strong on their production skills and the latter on their consumption skills. Needless to say, Scitovsky seriously complicated the theory of consumer choice by driving a wedge between behaviour and preferences on the one hand and consumption and welfare on the other.

Unsurprisingly, Scitovsky's criticism that economics had little to say about pleasure, admittedly an essential part of human nature, did not help

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<sup>36</sup> Such approach-based definitions had been available for decades, but it was not until the 1960s that they became widely accepted (see BACKHOUSE and MEDEMA 2009).

the reception of his book among economists, some of them reaffirming their conviction that economics could do without psychology and others pointing out that its attacks against the theory of consumer choice showed inadequate awareness of its latest advances (Bianchi 2016: 296-7). It took almost twenty years for the *Joyless Economy* to be recognised with the discipline. As Robert H. Frank (1992: v) put it in his Foreword to the second edition of the book, “in 1976, most economists simply were not ready for it”. In the interval, helped by the work of psychologists Daniel Kahneman and Amos Tversky and a few others, the attraction of psychology had grown significantly within economics (Earl 1990), opening new horizons for those willing to consider its behavioural assumptions with a critical eye.

The kind of psychology that created a more hospitable climate for Scitovsky’s work differed from Berlyne’s [and for that matter from the social psychology that had inspired Katona’s (1968a, 1968b) adaptive theory of consumer behaviour]. It was cognitive and its advocates, as Sent (2004: 743) shows, “started from the rationality assumption that ha[d] characterized mainstream economics and next analyzed departures from this yardstick, as opposed to developing an alternative one”.

It would take too long to detail the research areas inaugurated by Kahneman and Tversky.<sup>37</sup> Suffice it to say that the ensuing literature – both from psychology and experimental economics (see Smith 1991) – has produced not only new evidence of departures from the predictions of rational choice theory but also suggestions for how to deal with them. By deepening the notion of rationality and increasing the realism of behavioural assumptions, that literature has facilitated the integration of psychological insights into economic theory. By the late 1990s, the real question was not so much whether psychological findings were relevant to economics but about what “psychology tells us about modifying our general assumptions about individual behaviour” (see Rabin 1998: 12). In 2002, Kahneman was awarded the Nobel Memorial Prize in Economic Sciences. That Kahneman has often been described as the first psychologist to receive that honour says something of the transformation of economics in the last quarter of the twentieth century and of way the profession sees its relations to psychology. When Simon received the same award in 1978, he was described not as a psychologist but as undertaking interdisciplinary work. Yet, the first words of his prize lecture left little doubt as to how he saw the nature of economics: “In the opening words of his *Principles*, Alfred Marshall proclaimed economics to be a psychological science” (Simon 1978).

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<sup>37</sup> SENT (2004) provides a fair characterization when she lists heuristics and biases, framing effects, and prospect theory.

Given the success of behavioural economics, it is tempting to equate the history of the relations between economics and psychology from the 1970s with that of behavioural economics. However, it is not just its advocates who have shown an interest in psychology. Other economists, whose main intention was to go beyond the self-interest model, have also shown such interest. Whether or not these attempts form a coherent whole, they do use psychological insights. A good example is the effort to develop a commitment model. Drawing on Thomas C. Schelling, for instance, Frank (1988: 11) “use[s] the term *commitment model* as shorthand for the notion that seemingly irrational behaviour is sometimes explained by emotional predispositions that help solve commitment problems”. The commitment model implies a different world for economists, one in which the emphasis shifts away from the choice of the best option to the deliberate relinquishing of options, giving up of choices and surrendering of opportunities (Schelling 2006: vii). Another example is the attempt by Oliver Williamson (1985) to combine the assumptions of bounded rationality and opportunism so as to gain a better understanding of economic organisation. Williamson’s substitution of “contractual man” for “economic man” led him to consider behaviours such as cheating and lying, which imply a different conception of human nature – one that pays more attention to psychological elements – which he aptly described as “self-interest seeking with guile”. Still another example is Harvey Leibenstein’s (1980 [1976]: 268) attempt to give microeconomics new psychological underpinnings by replacing economic man with “S.R.” man – S.R. for selective rationality. Leibenstein “argued that individuals compromise between behaving the way they would like to behave if constraints were absent and behaving the way they felt they *ought* to behave”. Accordingly, he makes economic man an extreme variant of his theory of selective rationality. Other illustrations could be given that show that the relations of economics to psychology do not stop at the boundaries of behavioural economics (see Earl 2005).

As he considered the relationships between economics and psychology from the viewpoint of experimental economics, Vernon Smith emphasized that the economic rationality of individuals cannot be fully understood unless some attention is paid to institutional contexts: “What is imperfectly understood”, he wrote, “is the precise manner in which institutions serve as social tools that reinforce, even induce, individual rationality” (Smith 1991: 881). Going further while considering the paradoxical attitude of economists toward psychology, Shira Lewin (1996) argued that to draw the lessons about the relationship between economics and psychology it is necessary for economists to pay more attention to sociological analyses, noting in passing that there are historical reasons for the difficult relationships between economics and sociology.

Relationships between economics and sociology have always been complicated and often tense. From 1945 to the late 1960s, U.S. sociology was dominated by Parsonian structural functionalism, but by the early 1970s its critics, prominent among whom was Wright Mills (1959), had found a greater audience. The convulsions of American society called for a more relevant sociology, one that could account for social change. The coming crisis of Western sociology seemed inevitable to those who believed that they “theorize[d] today within the sound of guns” (Gouldner 1970: vii). The demise of the Parsonian scientific enterprise is significant for the relations of economists to sociology. Despite Parsons’s aborted attempt to integrate economic and social theory in the mid-1950s (Parsons and Smelser 1956), only two of the three post-war core social sciences – sociology and psychology – had a place in Parsons’s interdisciplinary Department of Social Relations at Harvard. Economics was left out. Potentially, the demise of structural functionalism could reopen the question of the role of economics in sociology but the attempts from the 1950s onwards to create an economic sociology as well as its practitioners’ efforts to show that it differed from economics (Wallerstein 2007: 436) made that orientation unlikely.

From the 1970s, despite isolated attempts to use sociological insights in economics (e.g. Piore 1973), sociologists have been more interested in economics than economists in sociology (see Calhoun 1992: 179-83). One obvious reason for such an imbalance was the success of Becker’s economic approach and its inroads into the analysis of a number of social phenomena that had long been associated with the subject matter of sociology. As Geary (2010) has noted, there was a shift from seeing the relationships between the two disciplines as complementary in the 1945-1968 period to seeing them as competitive afterwards. Though divergent understandings existed, there was convergence between economists who had made a profession of studying social phenomena and sociologists who felt that time had come to turn to economic reasoning. Whether or not they could achieve their ambitions, there were now economists and sociologists who shared common objects of analysis and methods, which implied that topical boundaries became less important than differences in approach. A sociologist doing rational choice sociology might have more in common with an economist cultivating the economic approach than with, for example, an economic sociologist. These connections mostly reflect the gradual transformation of “economic reasoning into a much more general approach to social life” (Calhoun 1992: 180), they do not imply any transformation of economics as a result of its interactions with sociology.<sup>38</sup>

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<sup>38</sup> Becker was invited to become a member of the sociology department at the University

One difficulty with assessing the effect of sociology on economics is that much of it involves work that lies at the periphery of economics, raising problems of definition. For example, the term “political economy”, used by some economists to refer to rational-choice modelling of political processes, is also used to refer to socio-political analysis of economic phenomena, and is sometimes located not in economics departments but in departments of sociology or political science. There are, however, economists closer to the mainstream who draw on sociology. Akerlof uses psychological, sociological and anthropological insights within economics, often contrasting his psycho-socio-anthropo-economics with Becker’s economic approach. Akerlof does not believe that the models developed by other social scientists can be transposed directly into economic analysis but argues, instead, that to the extent that social scientists show that people’s behaviour does not match economists’ descriptions, it makes sense to take their observations into account.<sup>39</sup>

In *An Economic Theorist’s Book of Tales* (1984) Akerlof notes that behavioural assumptions are discipline-specific so that economists’ “individualistic maximizing behaviour” can be differentiated from assumptions used in other social sciences. Differences in behavioural assumptions produce differences in models, which means that economists can occasionally draw on the models developed by other social scientists. To understand Akerlof’s use of other social sciences, however, it is necessary to note that his main goal is to explore the consequences of new behavioural assumptions within economics. These assumptions can come from other social sciences but they do not necessarily have to. He sees a continuity between his work on asymmetric information in the early 1970s, developed entirely within economics, and his work in the 1980s, in which he used concepts drawn from other social sciences, such as a code of behaviour associated with social customs, cognitive dissonance and gift exchange.<sup>40</sup> In both cases, he meant to introduce into economic models phenomena that had remained at their periphery. To use Akerlof’s (1984: 5) own description of the chapters gathered in *An Economic Theorist’s Book of Tales*: “All eight have in common ... the ‘use’ of new ingredients in economic theory”.

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of Chicago in 1983 (COLEMAN 1993: 175). Whereas COLEMAN (1993) has studied the impact of Becker’s work on sociology, little is known about the influence of Becker’s interactions with sociologists on his work. CRANE and SMALL (1992: 230) note sociology’s “increasing embeddedness in the literatures of other disciplines, including economics, a discipline with which sociology had virtually no ties in the early seventies”.

<sup>39</sup> For instance, Akerlof’s (194: 124) analysis of the economic consequences of cognitive dissonance is preceded by a translation of that theory into economic terms.

<sup>40</sup> Akerlof points to that continuity in his interview with Richard Swedberg (1991).

Akerlof draws on other social sciences to explain behaviour that can hardly be explained starting with economics' traditional behavioural assumptions. An example is the use of the concept of gift-exchange to analyse workers' behaviour (Akerlof 1982). This involves translating notions borrowed from other social sciences into economic terms before using them for economic analysis. George Homans's exchange theory of social behaviour lends itself to such translation in a way that Mauss's theory of the gift does not, so that norms, more than gift-giving itself, form the gist of the argument. Unlike Akerlof's workers, the members of the tribes studied by Mauss do not need to acquire sentiments for each other to enter the gift cycle.

Akerlof's suggestion that the conclusions of other social sciences need to be translated into economic terms before they can bear fruit in economic analysis reveals the power of disciplinary traditions in framing models of human behaviour. In this respect, it is no exaggeration to contend that the relations of economists to anthropology have been hampered by diverging theoretical ambitions to the point that Heath Pearson (2010: 166) recently remarked: "from the perspective of the present day, it would be hard to imagine two social sciences more mutually estranged than anthropology and economics" (2010: 166; see also Mirowski 2000). At this stage, if there is no need to recall the formalist-substantivist controversy of the 1960s, its conclusion is worth remembering: by the 1970s, disciplinary boundaries between economics and anthropology were well in place and it was dangerous to try to straddle them. Almost forty years after Knight's review of Herskovits's *The Economic Life of Primitive People* in the *Journal of Political Economy*, Richard Posner (1980), in the same journal, used a similar argument to conclude that economics was relevant to the study of traditional societies. Needless to say, his study of "primitive society" had little to do with the "primitive economics" of anthropologists.

If, therefore, we want to uncover the reasons for the difficult relations of economists to anthropology we need to go beyond the parallel between the gift cycle and the invisible hand and consider what Jacques Derrida [1994 (1992): 7] tried to convey when he spoke of the "relation of foreignness" between the "gift" and the "circle of exchange". Then we will realise that the very identification of the gift within a framework centred on exchange alters its nature and therefore makes the meaning of the gift altogether different. That conclusion applies to the gift but it may concern other social scientific notions used outside their disciplinary context.

It could be wondered why Akerlof's psycho- socio- anthro-po-econom-ics left out political science, one of the first disciplines to experience the incursions of economists on its territories. Part of the answer can be found in Albert Hirschman's (1970) *Exit, Voice and Loyalty*. Written at the Center

for Advanced Study in the Behavioral Sciences – the quintessentially interdisciplinary institution – at a time when the disciplinary age was taking shape, this essay was perfectly in tune with other attempts that pointed to lapses of economic agents and challenged “economic man”. In his effort to distinguish between two mechanisms – exit and voice – whereby individuals manifested their dissatisfaction with a situation, Hirschman identified a “schism” between economics and politics. In many respects, voice was the opposite of exit. Economists tended to believe that it was more efficient than voice and even that it was “in fact the only one to be taken seriously” (Hirschman 1970: 16). More generally, Hirschman pointed to the interplay of market and nonmarket forces with a view to demonstrating to “political scientists the usefulness of economic concepts *and to economists the usefulness of political concepts*”. Yet, he added:

This reciprocity has been lacking in recent interdisciplinary work as economists have claimed that concepts developed for the purpose of analyzing phenomena of scarcity and resource allocation can be successfully used for explaining political phenomena as diverse as power, democracy, and nationalism. They have thus succeeded in occupying large portions of the neighboring discipline while political scientists – whose inferiority complex vis-à-vis the tool-rich economist is equaled only by that of the economist vis-à-vis the physicist – have shown themselves quite eager to be colonized and have often actively joined the invaders (19).

Years later, Hirschman’s hope of convincing economists of the importance of political-science concepts has yet to be realised. Economists continue to believe that the impact of economics on political science is what matters (Miller 1997). Needless to say, economists’ lack of attention to political-science insights in the years following Hirschman’s essay cannot be explained just by the attitudes of political scientists. Also relevant is the fragmented nature of political science or, more negatively, the limitations of a “theory-starved discipline” (Miller 1997: 1199). More important however is the transformation of western societies since the mid-1970s and its consequences for the divide between economics and politics. If, as Hirschman argued, the problem was the economist’s overconfidence in the market mechanism, political developments from the 1980s have not been conducive to correcting this bias. Even more significantly, the transformation of ideas and culture that marked the last quarter of the twentieth century involved a shift away from society, history and power, towards individuals, contingency and choice (Rodgers 2011). That shift reinforced the confidence of economists in their own framework and encouraged political scientists to draw on ideas and metaphors consistent with it.



#### 4. CONCLUSION

In the past hundred years, economists have continually cultivated relations with other social sciences. From 1918 to the United States' entry into the Second World War, the pluralistic character of economics encouraged the multiplication of its points of contact with other social sciences, but interactions concerned individual economists, notably institutionalists, more than economics as a whole. There is, nonetheless, more continuity than is usually admitted between the interwar and the post-war eras. From the early 1940s to the late 1960s, at a time when interdisciplinarity reigned supreme in U.S. social science, some economists continued to cultivate their differences with other social sciences and accordingly endorsed natural science methods, but others pointed to the limitations of "economic man" while turning to other social sciences for inspiration. True, from the 1970s, following the move towards greater specialization in social science, interactions between economics and other social sciences took a turn. The most evident feature of this change was the use of microeconomic tools to investigate "noneconomic" topics, but, despite economists' growing sense of superiority (Fourcade et al. 2015), some economists, including behavioural economists, continued to regard the social sciences as a source of inspiration for their work. It may be that only a minority of economists shared this concern, and that those who were concerned with other social sciences treated them instrumentally rather than engaging seriously with their way of thinking, but these efforts have nevertheless been an important factor in the way economics' behavioural assumptions have evolved.

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