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File ID	130814
Filename	1: The disunified sciences of economics and psychology
Version	Final published version (publisher's pdf)

SOURCE (OR PART OF THE FOLLOWING SOURCE):

Type	Dissertation
Title	Kahneman and Tversky and the making of behavioral economics
Author	F. Heukelom
Faculty	Faculty of Economics and Business
Year	2009
Pages	V, 177
ISBN	978 90 361 0125 7

FULL BIBLIOGRAPHIC DETAILS:
http://dare.uva.nl/record/301297

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1. The disunified sciences of economics and psychology

1. Understanding the relation between economics and psychology

The two-fold aim of this thesis is to understand Daniel Kahneman and Amos Tversky's research, and to understand how this research has altered economics in fundamental ways. Thus, this thesis is an exercise in postwar history of the relation between economics and psychology. The difficulty with this exercise, however, is that it is both unavoidable and deeply problematic. To regard the history of Kahneman and Tversky's work as an interaction between the scientific fields of economics and psychology is unavoidable because that is the language in which economists have described and understood it.[1] More generally, the division between the different disciplines is so deeply ingrained in twentieth-century Western social science – institutionally, conceptually, rhetorically, financially and so on – that it is virtually impossible to bypass. A historical analysis pertaining to the relation between psychology and economics in the twentieth century has in one way or another to use or deal with this division.

Yet, the distinction is problematic. A first problem is that the labels 'psychology,' 'economics,' 'psychologist,' and 'economist,' are not stable entities in postwar science. For instance, judged by received training, non-economists who have won the Nobel memorial prize in economics include Kahneman, Herbert Simon, and a whole range of physicists and engineers, including in-between cases such as Vernon Smith, who received a BA in electrical engineering and a MA and PhD in economics. Or consider Colin Camerer, currently one of the leading behavioral economists who holds a PhD in behavioral decision research. Moreover, the same is true for psychology. Foremost postwar mathematical psychologists such as R. Duncan Luce, Patrick Suppes and David Krantz, for instance, received degrees in engineering or mathematics before migrating to psychology.

In addition, these postwar scientists were labeled economist or psychologist flexibly and depending on the occasion. Depending on the situation, Simon called himself a political scientist, economist, psychologist and mathematician. Mathematician Leonard Savage has been claimed to be an important economist by economists and an important psychologist by psychologists. Even on the level of

[1] I refer to 'Kahneman and Tversky' throughout this dissertation. The order of their names bears no significance.

individual publications the standard divisions are problematic. Von Neumann and Morgenstern's *Theory of Games and Economic Behavior* (1944) has been described as a major contribution to their field by economists, psychologists, biologists, and mathematicians. Mathematical psychologists Krantz, Luce, Tversky, and Suppes conceived their three-volume *Foundations of Measurement* (1971, 1989, 1991) to extend the work of economist Gérard Debreu. However, at the same time they described it as a contribution to the empirical sciences in general, that is physics, economics, psychology and others, and thus as a contribution to the "methodology" of science. Hence, although it has been fundamentally ingrained in postwar science, the distinction between the different disciplines that scientists have employed has been anything but stable or clearly defined.

There is a second reason for the problematic nature of the division between psychology and economics. If there is one constant in postwar Western economics and psychology it has been the attempt to cross the alleged boundary between the two disciplines and to make this boundary disappear. Letters and minutes in Luce's archive in Harvard University show that ever since the Ogburn Report was initiated by President Herbert Hoover in 1929, the attempt to unify the behavioral and social sciences has been a constant theme in the National Science Foundation's (NSF) recurring reports from committees on Basic Research in the Behavioral and Social Sciences.[2] In the late 1950s Ward Edwards created behavioral decision research (BDR), a new field in psychology that applied economic theories to psychological problems. Three decades later Kahneman and Tversky introduced an adjusted Edwards program back into economics. Simon for the better part of his career tried to use the insights he gained originally in political science to alter economic theorizing, which led him to produce a new theory in psychology. Vernon Smith developed his new experimental methods for economists in the 1950s in collaboration with psychologist Sidney Siegel, effectively applying a psychological method to economic questions. Fifty years later he defended his program as good economics against increasing criticism from behavioral economists by claiming heritage to the work of

[2] In 1929, President Hoover commissioned a committee of social scientists to report on trends in the social and behavioral sciences, an effort to augment the knowledge base for his social policy. *Recent Social Trends in the United States* was published in 1933 and regularly updated during the following decades [Smelser (1986), p.21]. In the 1984 edition, the members of the committee, senior historians, sociologists, economists, and psychologists, discussed how the integration of the different social and behavioral sciences could be institutionalized, given that in practice they were already closely related and largely overlapped [Based on minutes, letters and reports in Luce's archive at Harvard University].

political scientist and psychologist Simon. The 1952 Santa Monica conference organized by mathematician Robert Thrall and psychologist Clyde Coombs is often cited as a major event in the history of game theory in economics, in the history of mathematical psychology, and in the history of experimental economics. As much as the division into different disciplines has been part of postwar science, so has the crossing and dissolving of the boundary been a constant.

A third and more subtle problem is that in the postwar period economists and psychologists have understood themselves, each other, and the boundary separating them in different ways. By and large, economists have understood economics to be a positive science refraining from normative claims, leaving those to the policy makers. With respect to psychology, postwar economists when pressed have made a distinction between psychological assumptions and the scientific field of psychology. The received view was that economists made psychological assumptions to build their theories and models on. However, that did not mean that economics and psychology were part of the same scientific discipline. Psychology, in this view, was a science of human behavior independent of economics, although it could always be used as source to improve the psychological assumptions made by economists. For economists, the economics-psychological border lay somewhere in between the scientific field of psychology and the psychological assumptions made by economists.

Psychologists for their part understood psychology as a science of human behavior in which the label psychology was used as a broad concept covering a range of sometimes very different ways of understanding different parts of human behavior. Psychology was understood by psychologists as being basically a descriptive and explanatory science, but the normative or prescriptive aspect of it was never far away. In fields such as psychotherapy, organizational psychology, and behavioral decision research the descriptive and the normative parts were kept separate, but there was always a clear and direct link between the two. The psychologists who were concerned with economics understood economics as a sub-discipline of psychology. Psychology in their understanding referred to the total of all different forms of investigation into individual human behavior, whereas economics referred to the sub-field that investigates a single part of human behavior, namely economic behavior. Moreover, economics was understood by psychologists as a scientific field which focused on the normative aspects of economic behavior. Because psychologists considered both the descriptive and the normative to be legitimate parts of science,

this exclusive focus of economists on the normative theory was in itself not problematic. But what it did imply was that from the psychologists' perspective economists focused only on one aspect of their scientific project. Hence, for psychologists the border between economics and psychology lay somewhere between the normative theories of economic behavior and the remainder of the psychological investigation of human behavior.

A further difference between economics and psychology in their perception of themselves, each other and the border between them was the status of the question how the two were related. For economists living in the twentieth century the relation between economics and psychology was something that was always in the back of their minds. It was always an issue that had to be dealt with, one that could never be dealt with satisfactorily and hence a question that would always reappear. Sometimes an economist would take a particularly clear and authoritative stance in this regard which made the issue disappear for a time. But it was always there and it always returned. For psychologists, on the other hand, the relative understanding of both disciplines was self-evident and rarely discussed. One of the surprises for the historian of economics who dives into the history of psychology is the absence of the question regarding how psychology is related to economics. It is not that the question simply is ignored, for psychologists do ask themselves from time to time how their discipline relates to economics, sociology, political science and other sciences. But in contrast to the relation with sociology, the relation between economics and psychology has always been clear to psychologists.[3] At the end of the twentieth and beginning of the twenty-first century, and after thirty years of active use of psychology by economists, psychologists could complain that although "economists are now becoming more psychologically receptive, it is unfortunately less apparent that psychology is becoming more economically receptive" [Murnighan and Ross (1999), p.7], and that "[u]ntil recently, economists were more active in using and referring to social psychology than social psychologists were in using economics" [De Cremer, Zeelenberg, and Murnighan (2006), p.7]. The relation between the fields of economics and psychology, in other words, has been very much a concern of the economists.

[3] It is true that many psychologists from the late nineteenth century onwards challenged psychology's hedonistic/utilitarian basis [Lewin (1996), pp.1299-1300], which served as a departure point for economics also. But this discontent was directed at philosophy, not economics.

For the historian writing about the postwar history of economics and psychology, the last point is particularly problematic because it renders the existing literature on the relation between economics and psychology in the two disciplines problematic as a basis on which to conduct further research. The accounts of both practicing scientists and historians in each of the disciplines of the relation between the two disciplines are largely incompatible. References to and accounts of Louis Leon Thurstone's work are illustrative in this regard. In postwar economics Thurstone (1931), "The Indifference Function" has been a constant reference for economists and historians of economics discussing the relation between economics and psychology. In these accounts Thurstone was the first to experimentally test economic demand theory. His experiments were initially set aside as irrelevant by most economists and followed by only a few [Moscati (2007)]. With the emergence of psychological experimentation in economics in the final quarter of the twentieth century, however, Thurstone was rediscovered and became regarded as an important precursor to contemporary research. Thus, Thurstone (1931) plays a significant role in economists' thinking about the relation between psychology and economics: in the early postwar years it was dismissed as of possible use in economics, and in the last few decades it was an important precursor for ways in which psychology could inform and improve economics' psychological assumptions.

Although Thurstone (1931) plays a role in economics and in histories of economics, Thurstone is not considered a major figure by economists. In psychology, however, Thurstone is seen as a key figure. Thurstone greatly improved measurement methods for social psychological research, thus ensuring the scientific status of social psychology. Applying his own measurement theory, Thurstone furthermore initiated a program of attitude measurement that became a cornerstone of modern psychology. Moreover, in the process he played a key role in uniting the fiercely opposed experimental and correlational (or Pearsonian) psychology. But in all this recognized importance of Thurstone's work and the accompanying discussion of his major works, Thurstone (1931) is completely absent. Thurstone's one-shot attempt to use the social psychological method to test economists' demand theory does not play any role in psychology, either in the past or in the present. Thus, whereas Thurstone seems to economists, both practitioners and historians, as an illustration of the relation between twentieth century economics and psychology in fact this only illuminates how economists and their historians have conceived of the relation between economics and

psychology. On the other hand, psychologists and their historians who consider economics to be a subfield of psychology view Thurstone as a major figure, but not with regard to the question of how economics and psychology are related to one another.

The unavoidable but problematic division of parts of postwar science into psychology and economics and especially the very different understanding of themselves, the other, and the border between them presents a dilemma for the historian who wants to do justice and to draw on both sides to provide a coherent understanding of a particular episode in the relation between the two. To understand why a particular branch of psychology could come to influence economics from the early 1980s onwards, and especially to understand in what way the experimental results and theories of this psychology were adopted and adapted to the economic framework, we need to understand the history of economics and psychology independently of one another. That is, we need to dive into the history of economics independently of how psychology thought and thinks of economics, and in particular, we need to draw out some of the threads in the history of psychology independently of how economists conceived and conceive of this history. We need to understand what was important in economics according to economists, and what was important in psychology according to psychologists. But at the same time we need to set out a story that will show us how the two scientific disciplines understood each other, and how they came to interact. We do not want two independent histories of psychology and economics, but two historical accounts that can be related. This boils down to the historiographical question of how to understand different, but related disciplines.

2. The disunity of science

Galison and Stump (1996) and Galison (1999) use the notion of the disunity of science to capture the idea that sciences and scientific practices may be separate and different, but at the same time be communicating and mutually influence each other. Galison applies disunity to the case of twentieth century physics in which he distinguishes three subcultures: theorizing, experimenting and instrument making: "[d]ifferent finite traditions of theorizing, experimenting, instrument making, and engineering, meet – sometimes even transform one another – but for all that they do not lose their separate identities and practices" [Galison (1999), p.137]. He makes two central points. First, contrary to the logical positivist tradition in science, there is not

one unified basis upon which different scientific subcultures are conducted. "Experimentalists – and one could make a similar statement about theorists and instrumentalists – do not march in lockstep with theory. For example, the practice of experimental physics in the quantum mechanical revolution of 1926-27 was not violently dislocated *despite* the startling realignment of theory" [Galison (1999), p.143]. Second, contrary to the anti-positivist tradition in the history and philosophy of science, neither are different sciences and scientific practices entirely unrelated [Galison (1999), p.143].

Galison makes a comparison with the interactions of geographically scattered cultures that are studied by the anthropologist and introduces the concept of the trading zone. Different (sub-)cultures influence each other, for instance in their language and cultural habits. They trade with one another and goods travel from one culture to the next. But despite all their trading and mutual influencing, they remain different cultures nevertheless. Moreover, it is not necessarily true that because of the frequent cultural exchanges and trade, the different cultures are brought closer to one another over time. A cultural habit that travels from one culture to the next may acquire an entirely different meaning. Traded goods may be used for an entirely different purpose in the culture that they end up in compared to the culture in which they originated.

Consider the following example (this example is derived from Taussig (1980), and is used by Galison (1999)). In the Cauco Valley in Columbia, two groups of people live among each other each with their own culture. One culture consists of black peasants, descendants from slaves, running shops or working on the vast sugarcane farms. The peasants maintain a culture with magical cycles, sorcery and curing. The other culture is that of the rich, white landowners. The cultures exist alongside each other and frequently interact, for instance when a member of the landowner culture exchanges money for some eggs with a member of the peasant culture. The two cultures are, in other words, perfectly able to communicate with one another in specific contexts and can even be said to depend upon each other for their survival. However, the understanding of the exchange of money for eggs may be entirely different for the members of the two cultures. For a member of the landowner class, money is a neutral means of exchange that can accumulate into capital. For a member of the peasant culture the bank note possesses animistic and moral properties. In the most telling instantiation of this aspect of peasant culture, the godparent-to-be

hides a peso in his hand when a newborn is baptized by the Catholic priest. By doing so, also the peso bill is baptized and obtains the child's name, and the godparent-to-be also becomes the godparent of the peso bill. When this peso bill is put into circulation it will always come back to its owner when it is silently called upon three times. As a result, the members of the two cultures may be comfortable exchanging eggs for money with one another, but in a broader sense they interpret this transaction in a completely different way. For the landowner it is a neutral exchange of money for eggs, whereas for the peasant it signifies the return of the baptized peso bill to its owner.

Galison urges us to think of exchanges between different scientific (sub-) cultures in a similar fashion. At some level the different cultures may devise a context or set of rules within which they can exchange ideas, experimental results and instruments. Each is fluent in the exchange process in the sense that a member of the other culture will behave exactly as anticipated. But in a broader cultural context, the members of the different cultures will interpret the exchange in a completely different manner. "[T]he trading partners can hammer out a *local* coordination despite vast *global* differences" [Galison (1999), p.138]. The two cultures may entirely disagree about the implications of the information exchanged or its epistemic status. But at the same time "there is a context within which there is a great deal of consensus" [Galison (1999), p.146]. Depending on the topic to which we apply this anthropological analogy, we could also think of this context as a pidgin language. The different cultures devise a language that allows for a smooth exchange between the two cultures, but through back-and-forth trial and error and compromising devise a language that will never be able to satisfactorily capture each culture individually.

I apply Galison's approach to the case of economics and psychology. Economics and psychology are disunified cultures. They exist alongside each other and exchange results, instruments and ideas. They find it relatively easy to talk to one another, especially those representatives of the two cultures who operate in each other's vicinity. Indeed, one might say that for those scientists that operate close to the boundary on each side, the boundary seems more of a gradual continuum than a sharp line drawn in the sand. Both are also affected by the same challenges of the larger world and may at times come up with responses and adjustments that are very much alike. Yet despite all this exchanging and sharing of results, instruments and ideas, i.e. despite all their local coordination, they remain two clearly distinguished and

distinguishable sciences. Economists and psychologists have a lot in common, and yet are very different. They encounter each other frequently, they draw partly on the same authoritative sources, they inspire and influence each other, and psychologists have won the Nobel memorial prize in economics twice. At the same time, however, it is clear to both psychologists and economists that the two have been different in the past, that they are different at present, and that they will be distinguishable academic disciplines in the foreseeable future.

The disunity approach neatly captures how economists and psychologists talk about their relationship. Murnighan and Ross (1999), for instance, argue that although closely related, there will always be an "(invisible) dividing line between microeconomics and social psychology" [Murnighan and Ross (1999), p.2] because "the two fields promote different kinds of thinking and different philosophies, and these differences make it difficult for people in the two disciplines to collaborate, much less appreciate each other's work" [Murnighan and Ross (1999), p.6]. The difference is that "[t]he objective of much of social psychology is to better understand how individuals make decisions in social situations. [..] Economics, on the other hand, is ultimately about explaining aggregates like market prices and quantities, incomes, employment and market efficiency" [Murnighan and Ross (1999), p.3]. Another good example is how one of these psychologists, Keith Murnighan, and an economist, Alvin Roth, recall the beginning of their rare thirty-years psychology-economics collaboration:

> In essence, I did not speak economics and Al [Roth] did not speak psychology. (It is still not clear whether either of us has really picked up the other language, but at least we now think that we understand each other.) We had to work our way through a lengthy process to determine how we could express what we wanted to say about our joint work without offending each other or insulting each other's fields in the process. [Murnigham and Roth (2006), pp. 322-323]

Murnigham and Roth, in other words, had to invent a pidgin language in order to be able to communicate. But that does not mean that they now, after thirty years of collaboration, understand the other's native language.

The anthropological analogy also captures the idea that the different cultures may not be equally happy with the existing form of their interaction. In the interaction between economists and psychologists in the late twentieth and early twenty-first century it was the psychologists who sometimes felt abused in the exchange relation. A good example can be found in Lunt (1996). Lunt first of all notes that although economics and psychology may be investigating the same phenomena and relying on the same methods, "the way that social psychology approaches the study of the agent is very different from that of economics" [Lunt (1996), p.280]. But Lunt takes it a step further. It is not only that the two approach the agent in a different way, the exchange between economists and psychologists on this topic is also more favorable to economics, and in fact not at all to psychologists' advantage. "[E]conomists work with simplified and anachronistic applications of psychological theory. We [psychologists] have to understand that psychology has become a resource for the economist, and [that] the motivation for integration is all on the side of psychology." Furthermore, Lunt emphasizes that

> [..] this is made particularly problematic by the kind of psychology utilized within economic theory. In my view, economists are not ready, prepared or even vaguely interested in changing their core assumptions as a response to psychological work. Indeed, we should realize that if an economist sounds interested in our work they are only trying us out to see what kind of resource we have to offer. The agenda for their interest will be some debate in economics that we won't have even heard of. [Lunt (1996), p.283]

The different parties to the cultural exchange understand the exchange and the object exchanged in a different way. But on top of this, they may not be equally happy with the exchange, so that some members of one of the cultures may start to argue that they are being exploited.

3. Scope and limits of this thesis

The first two chapters of this thesis in particular are focused on the context in which scientific developments have taken place, yet this thesis is primarily a history of ideas. In addition, although the greater part of the history described in this thesis took place on American soil or was otherwise strongly connected to the United States, it is not

exclusively an American history. First, because the scientific developments described are a continuation of earlier science that has its origins outside the United States, principally in Europe. Second, because the two main protagonists of the present history, Kahneman and Tversky, although substantially Americanized, conducted the research for which they became famous at Israeli universities. Finally, the focus in this thesis will be on the developments leading to behavioral economics and on the development of behavioral economics itself. But I will use experimental economics as well to portray the differences between them.

The remainder of this thesis has been organized as follows. The upcoming second chapter discusses the work of the mathematical psychologists and behavioral decision researchers. I place this work in the unique context of the social and psychological research at the University of Michigan in the 1950s and 1960s. Although much attention has been paid to the Institute of Social Research (ISR), mathematical psychology and behavioral decision research at the University of Michigan have not yet been thoroughly investigated. However, these research programs and their related institutes would foster a development in psychology that was not only influential in the psychological discipline, but that would also transform economics from the 1980s onward. These research programs contributed to the development of both experimental economics, which developed from the 1960s onwards, and to Kahneman and Tversky-inspired behavioral economics that developed from the early 1980s onwards.

In the 1950s and 1960s the University of Michigan was arguably the center of American psychology, hosting the Institute for Social Research (ISR), Coombs' Michigan Mathematical Psychology Program, and Edwards' Engineering Psychology Laboratory and its related field of behavioral decision research. The second chapter argues that the key to understanding mathematical psychology and behavioral decision research is to see that, although largely separated and focused on different questions, both presumed the same two-sided understanding of psychology. In order to measure, one needed a sound theory of the measurement instrument, which was the human decision maker. Psychology at the same time measured human decision behavior and investigated the human being as a scientific measurement instrument.

This double understanding of psychology as using a measurement instrument to investigate that same measurement instrument became problematic when it turned out that the measurement instrument did not behave as it should. That was the

problem Tversky struggled with. Tversky had to choose between declaring the experimental results invalid and saying that the received theory of the measurement instrument was incorrect. Kahneman came to the rescue by suggesting that the human decision maker systematically and predictably deviates from how it should behave. Thus, the experimental results could be accepted, while at the same time the axioms of the measurement theory could be maintained. It did, however, give psychology the new task of investigating how and when human decision makers deviate from how they should behave. That new task was the basis of Kahneman and Tversky's collaborative research of the 1970s.

The third chapter, then, discusses the work Kahneman and Tversky did before their collaborative research. Tversky was educated at and received his PhD in the early 1960s from the University of Michigan under the supervision of Coombs and Edwards. Tversky's research embodied the synthesis of mathematical psychology and behavioral decision research. Towards the late 1960s, however, Tversky increasingly struggled with the tension between Leonard Savage's *a priori* axioms of decision theory and the behavioral deviations he observed in his experiments. Kahneman, for his part, came from a very different background. Strongly influenced by his experience as psychologist in the Israeli army, Kahneman's different research interests focused on human's cognitive mistakes. Kahneman showed that despite the fact that we think we do cognitively quite well in the course of our daily lives, in fact we constantly make systematic cognitive mistakes.

In 1969 Kahneman and Tversky started their long and fruitful collaboration. The most productive period was during the 1970s, which laid the foundation for their subsequent fame. The fourth chapter discusses Kahneman and Tversky's research of the 1970s and shows how Kahneman's psychology of cognitive mistakes provided a solution to Tversky's struggle with the *a priori* axioms of Savage's decision theory and experimental deviations from them. Kahneman and Tversky's solution was to rigorously separate the normative from the descriptive. This allowed them to maintain Savage's *a priori* axioms as the normative rules of decision making, while at the same time acknowledging the experimental results as proof that actual human decision making deviates systematically from these norms. In 1979 Kahneman and Tversky's research culminated in prospect theory, a theory which describes actual human decision behavior as a systematic deviation from the normative rules. Using prospect theory, Kahneman and Tversky deliberately broadened their scope to economics.

They considered prospect theory applicable to both economists' and psychologists' use of expected utility theory. The paper was published in *Econometrica* and argued that cognitive psychology and economic were unified in one field of behavioral science.

Chapter five investigates how economists responded to Kahneman and Tversky's understanding of experimental violations of expected utility theory and their descriptive alternative, prospect theory. It argues that there were two main responses, each with their own history. Experimental economists such as Vernon Smith corroborated and accepted the experimental results, but rejected all expected utility theories as a solution, including prospect theory. In addition, experimental economists inferred that the experimental deviations further emphasized the importance of the market as the mechanism that over time drives the economy to a rational equilibrium. Financial economists, such as Richard Thaler, also accepted the experimental results, but instead they took it as proof of the observed irrationalities in financial markets. In addition, financial economists hailed Kahneman and Tversky and prospect theory as being the most important, if not the only claimant to a solution to the problem. The use of prospect theory in financial economics led to the new field of behavioral finance. The reason for prospect theory's swift success was that it offered financial economists an elegant way out of the problems. The normative – descriptive distinction ensured that traditional, neoclassical models could be maintained as the normative theory, while at the same time it offered a descriptive alternative that was only slightly different from previously-used theories and hence easy to learn by economists.

In the late 1980s and early 1990s Thaler also started applying the behavioral finance approach to problems outside the field of financial economics. The new field grew quickly and in 1994 it was officially called behavioral economics. Once the traditional economic theories were saved in the normative realm and new theories could be developed under the rubric of descriptive theory, a surge of explorations ensued. The sixth chapter describes the history of behavioral economics in the 1990s and 2000s. Using the examples of intertemporal choice and emerging preferences it shows that behavioral economists explored many opportunities for constructing descriptive theories of economic behavior, but at the same time they always remained faithful to Kahneman and Tversky's normative – descriptive distinction. Gradually the labels of normative and descriptive were replaced by full rationality and bounded

rationality, which in turn allowed the behavioral economists to develop their own view of economic policy advice under the label of paternalism.

These developments contributed to the gradual emergence of behavioral economics as a stable and clearly defined mainstream economic program. As a result, it also brought to the fore how behavioral economists saw their program as being different from other economic programs and disciplines. Behavioral economists began to distinguish their program, in particular with regard to psychology and experimental economics. This might seem slightly schizophrenic. Behavioral economists defined behavioral economics primarily by its incorporation of psychology into economics, but at the same time they sought to distinguish behavioral economics specifically as economics, and therefore as being different from psychology. Behavioral economists relied heavily on the use of experiments and claimed the intellectual heritage of Simon, but simultaneously they explicitly distinguished behavioral economics from that other economic program that used experiments and claimed Simon: experimental economics.

How then should we understand this apparent schizophrenia in contemporary behavioral economics? In the seventh chapter I argue that the history discussed in this thesis shows how economists have actively used psychology to redefine economics. The flow of theories, methods and experimental results from psychology to economics was not a neutral process that left these theories, methods and experimental results unaffected. Instead, they lost some of their psychological connotations and gained new economic connotations. What is particularly illustrative in this regard are the two cases of experimental and behavioral economics, which both added different new economic connotations to the theories, methods and experimental results drawn from psychology. Experimental and behavioral economists used the theories, methods and results from psychology to redefine economics in their own ways. Thus, as I argue in this final chapter, this thesis not only shows that the theories, methods and experimental results that travelled from psychology to economic have not been stable entities, but it also shows that the definition of economics has not been constant. Therefore, the history of economics and psychology can only be understood by recognizing economics and psychology as disunified cultures.