In brains we trust: How neuroeconomists stylize trust, the brain, and the social world

Klaassen, P.

Link to publication

Citation for published version (APA):
Klaassen, P. (2014). In brains we trust: How neuroeconomists stylize trust, the brain, and the social world.

General rights
It is not permitted to download or to forward/distribute the text or part of it without the consent of the author(s) and/or copyright holder(s), other than for strictly personal, individual use, unless the work is under an open content license (like Creative Commons).

Disclaimer/Complaints regulations
If you believe that digital publication of certain material infringes any of your rights or (privacy) interests, please let the Library know, stating your reasons. In case of a legitimate complaint, the Library will make the material inaccessible and/or remove it from the website. Please Ask the Library: https://uba.uva.nl/en/contact, or a letter to: Library of the University of Amsterdam, Secretariat, Singel 425, 1012 WP Amsterdam, The Netherlands. You will be contacted as soon as possible.
Chapter 4

Neuroeconomics ascending, a collective assembled

Every epistemological theory is trivial that does not take this sociological dependence of all cognition into account in a fundamental and detailed manner. But those who consider social dependence a necessary evil and unfortunate human inadequacy which ought to be overcome fail to realize that without social conditioning no cognition is even possible. Indeed, the very word ‘cognition’ acquires meaning only in connection with a thought collective.

Fleck (1979, p.43)

4.1 Introduction

As shown in the first and introductory chapter, in the previous decade neuroeconomists have started to investigate colloquial and ubiquitous social phenomena such as trust. Put briefly, we can say that this was done by putting together behavioral economics and neuroscience. From the alliance between these disciplines and their respective theoretical, technological and methodological repertoires has emerged a novel experimental system in which trust has become amenable to natural-scientific investigation. Arguably, this experimental system has distinctive stylistic features, and in accordance with these things which might be conceived of as demanding investigation by social scientific means today, can no longer be considered social in any straightforward sense. Not, at least, if social is supposed to form an opposition with biological and if social science is supposed to differ significantly from biology or natural science more generally.
Arguably, then, what has happened to trust is indicative of a significant change of meaning which the more wide-ranging category of the social is currently in the process of undergoing. For when we look at the neuroeconomics of trust, we see the withdrawal or even elimination of the borders between what counts as social and what as biological, paired with a contestation of the authority of so-called traditional social science over social phenomena. Of course this does not follow solely from the fact that neuroeconomists have started to investigate trust. But if we judge it on the basis of number of publications, special issues, conferences and the like, neuroeconomics must be considered a very lively discipline in which many more phenomena involved in or related to (strategic) social interaction and (economic) decision making are scrutinized. Moreover, neuroeconomics is but one member of the family of social neurosciences that has emerged over the past years (see e.g. Decety & Cacioppo 2011).

However, there is something puzzling to all of this. For is not a discipline something people are disciplined in, something people receive a training in? So far only a handful of university programs are dedicated exclusively to neuroeconomics, and the field’s “fathers” certainly were not trained in it! Like so many of its present-day academic kin, neuroeconomics is a very hybrid endeavor, building on expertise from a very wide range of disciplinary backgrounds, behavioral economics and neuroscience being only two obvious shorthands for the motley collection. This raises two related questions: First, how did the neuroeconomic field of research come into being? And second, how did and how do people enter such an inter- or multidisciplinary field as neuroeconomics? In other words: How did the esoteric circle of neuroeconomics emerge in the first place and, now that it is established, how can it be reached by outsiders? These are the main questions dealt with in this chapter. That style is a more suitable unit of cognitive organization for understanding neuroeconomics than is discipline forms part of the answer to these questions. Hence, the analysis of the style of neuroeconomics runs from this chapter through chapter 7 as a leitmotif.

On the road to answering these questions, I take my cue from Fleck’s considerations concerning different routes along which esoteric circles can be reached. Fleck opens the third chapter of his *Genesis and Development* as follows:

> For a long time I wondered how I could describe the Wassermann reaction to a layman. No description can take the place of the idea one acquires after many years of practical experience with the reaction. It is a complex, extremely rich field related to many branches of chemistry, physical chemistry, pathology, and physiology. (1979, p. 52)

Of course something similar holds for almost any scientific discovery made in almost any specialized field of study—including such discoveries in neuroeconomics as will figure especially prominently in chapter 7. Moreover, also with respect to neuroeconomics, the
practical wisdom of practitioners is hard to replace by description and here too the field is tremendously complex, being related to such different fields of knowledge as psychology, evolutionary biology, economics, game theory, sociology, neuroendocrinology and psychiatry.

Regarding the difficulty of description, the problem is not merely that this would require making explicit a great deal of tacit knowledge implied in the successful execution of neuroeconomic experiments—no doubt an impossible task in itself already (cf. Collins 1985)—but additionally yet relatedly, as Fleck puts it, that “words as such do not have fixed meanings. They acquire their most proper sense only in some context or field of thought” (Fleck 1979, 53). In order to correctly perceive the exact meaning of words central to scientific endeavors, Fleck suggests, one requires a proper introduction. Such introduction, furthermore, can be historical or didactic, as I will both elaborate and complicate in section 4.2.

Given that there have been few academic training programs devoted exclusively to neuroeconomics, the didactic route is not likely the only, or even the most prominent, candidate for those who have not yet entered the core of neuroeconomics. And, since immersing themselves in the history of ideas is hardly the core business of neuroeconomists, it appears that Fleck’s division is not exhaustive and that there might be one or more additional routes along which access to the neuroeconomic core can be gained. However, a textbook in neuroeconomics has been published (Glimcher et al. 2009a), and this provides something of an official introduction to the field that is simultaneously didactic and historical. I will analyze this disciplinary history in section 4.3, and my analysis will facilitate the outlining of a roundabout history of neuroeconomics. It is roundabout, firstly, because its main end is not (simply) to correctly depict the emergence of neuroeconomics but instead to draw epistemological lessons concerning the role of disciplinary history in its establishment and therewith in the formation of objects of research, and secondly because it builds almost entirely on a history of neuroeconomics formulated by others. In addition to this, one of this chapter’s main arguments is that, borrowing a concept from Michel Callon (1986), “entrance through interessement” constitutes an alternative way through which entry to the core of neuroeconomics can be achieved. I analyze this alternative entranceway into the esoteric circle of neuroeconomics in section 4.4.

4.2 Initiation: history and interessement

Fleck claims that scientific experience cannot be properly understood without considering the history and development of ideas and scientific practices to be something of a repository that is always a part of the scientist’s background. Furthermore, he underlines the
significance of the fact that scientists are always trained in some scientific discipline: the way in which people are introduced to scientific practice has its effects on how they come to experience the world (that is, in accordance to a particular style). This can be understood by analogy with tribal practice. To both tribal and scientific practices, Fleck reminds us, it is central that newcomers go through initiation rites.

Focusing on neuroeconomics, both the role of history and that of training are treated in this chapter. Before turning to those treatments in sections 4.3 and 4.4, however, I will clarify how, exactly, I will deal with that history and initiation.

Disciplinary history

One prominent reason why we are concerned with the historical emergence of neuroeconomics relates to the objective of elucidating the conditions of possibility for trust to have become an object of neuroeconomic investigation (i.e., an object of study in natural-scientific terms). Such conditions can be drawn up in a number of ways, and thus some words on the route chosen are in order. The focus in this historical tale is not on trust. However, an investigation into how the project of neuroeconomics became conceivable by the end of the twentieth and beginning of the twenty-first centuries forms an essential part of the story on the stylization of trust in neuroeconomics.

Nonetheless, I have not yielded to the temptation to attempt a full-fledged account of the coming about of neuroeconomics. Rather, again partly inspired by remarks by Fleck, but this time on the diversity of functions that different genres of scientific writing have, I have made this an incomplete and “parasitic” case study in historical epistemology. It is parasitic because it runs via an analysis of the disciplinary history of neuroeconomics, and it is incomplete because the disciplinary history leaves out historical material. I have already discussed some of the substance that would have figured in a more complete rendering of this history in chapter 3, but most of the gaps in the disciplinary history discussed below will be only occasionally expanded upon or perhaps spotlighted.

Presenting the disciplinary history of neuroeconomics in this fashion allows it to play two roles simultaneously. First, it provides insight into the style of neuroeconomics, because it reveals how the mobilization of historical appearances legitimates one highly particular (and not uncontroversial) present. Paraphrasing Nikolas Rose, this chapter investigates how the past is rewritten as the present is being reconfigured (2007, p. 189). Second, this take on the disciplinary history of neuroeconomics allows me to investigate epistemologically the very genre of disciplinary history. This helps create a historiographically more satisfactory, if incomplete, account of the conditions of possibility for the emergence of neuroeconomics than is provided by the disciplinary history in itself.

Perhaps the most obvious starting point when writing about disciplinary history would
be Thomas Kuhn’s discussion of the genre and its role in textbook science in his *Structure of Scientific Revolutions* (1970), as well as in his original lecture from 1959 “The Essential Tension.” To be sure, these are probably the most famous discussions of the topic around. However, what stands out immediately in these works is that Kuhn only discussed the genre of disciplinary history in regard to its role in science teaching. Kuhn thus understated the role disciplinary history plays in the production of knowledge, facts, truths, disciplines and objects proper.

What will become clear in section 4.3 however, is that, in the present context, how relevant forces in the neuroeconomic field today represent the history of neuroeconomics is as pertinent as is the form and substance this history actually had. With this distinction between representations of history, on the one hand, and, on the other hand, history as it happened, I do not wish to suggest that history could actually be represented as it really was—indeed, for instance, from the means of investigating, the particular questions one has in mind or the purpose with which one scrutinizes sources. Neither would I want to suggest the opposite—that representations of history do not link up with that history in any relevant sense of the word. However, the distinction is useful insofar as it helps make clear that any disciplinary account of its own origins or of the origins or background of a certain topic of investigation will always be stylized in some particular way. Those features fitting the present self-image will be sought after in historical publications and events, omitting what is not (perceived to be) relevant for achieving the present state of affairs. Looking at disciplinary histories, therefore, can be very telling if one is after an understanding of today’s science—as I am here. At the same time, however, we need to be aware of the fact that such histories might deflect certain events in non arbitrary ways—that they might be guilty of rewriting history, as the expression goes.

In sum, what we will see by looking at the history of neuroeconomics as displayed by the actors involved are, of course, not necessarily all the sources that, together, add to the substance of neuroeconomics today. We will find only and exactly that part of history that can be fruitfully employed as a resource in telling the story of the field’s emergence from the point of view of the very protagonists of this story. However, investigating this precise part of the history is useful for our present purposes, because by showing how neuroeconomists stylize their own history, the disciplinary history of neuroeconomics can guide us in our investigation of the conditions of possibility for trust to have become an object of neuroeconomic research.

**Interessement**

In order to be recognized as a scientist by one’s mentors and peers, one has to go through several phases toward one’s final initiation into an esoteric circle, each phase convention-
ally designated as a stage in one’s academic training. This takes tests of all sorts, such as those that examine the command of theoretical knowledge and of various types of less or more tacit knowledge—making the right (perceptual) judgments, the minutiae of laboratory work and the command of literary conventions.4

Concerning those literary conventions, let me be clear that the respective epistemological significance of different literary genres is an explicit topic of this chapter: first, in my discussion of disciplinary history in section 4.3 and then with a very specific type of “introduction” in section 4.4. I have taken my cue for this from an in-depth analysis of the epistemological significance of systematic pedagogical introductions that Fleck gave of Julius Citron’s textbook entry from 1910 on “The Methods of Immunodiagnositics and Immunotherapy” (Fleck 1979, pp.55-64)—which Fleck calls “the rite of initiation into the field of the Wassermann reaction according to the German ritual” (Fleck 1979, p.54). Citron was a student of Wassermann and Fleck explains how his text shows all signs of the dogma appropriate to introductions. Through such introductions, an “intellect is prepared for a given field” (Fleck 1979, p.54) as one is taught to see some things as obvious while others may escape one’s notice altogether. As a manifestation of this genre, Citron’s texts shows all such traits, and, so Fleck argues, it serves well to identify the active linkages at work in the immunodiagnostics the Wassermann test is such prominent example of. In the present chapter, I will show how an article written for the journal Nature by Antonio Damasio (2005) exhibits much the same traits and fulfills a related, yet in some respects different, role.

Despite obvious differences between disciplines, many patterns of scientific training are structurally similar in a range of fields. Therefore neurophysiologists will recognize an astrophysical article as a scientific publication, irrespective of the fact that they will not fully understand it. The lack of exhaustive comprehension will for instance show itself in the incapability on the part of neurophysiologists to reproduce the astrophysical results. But shared statistical skills, for example, might to some extent allow them to assess the significance of the results reported on.5

Harry Collins and Robert Evans, in their widely discussed paper on the “third wave of science studies,” have developed a taxonomy of different levels of expertise, which is clearly in keeping with this (Collins & Evans 2002). The levels of expertise they distinguished range from “no expertise” through “interactional expertise” to “contributory expertise.”6 Notwithstanding the fact that Collins and Evans nowhere explicitly refer to Fleck’s work, the terminology they employ has a clear Fleckian ring to it, speaking as they do of esoteric science, core-sets and core-scientists, for instance.

In their work on expertise Collins and Evans aim to achieve a better understanding of the different types of expertise people who relate differently to particular sciences have. In
Fleckian terms such relation could be conveyed by placing someone in any of the circles that all thought collectives are made up of. Contributory experts, then, belong to the esoteric circle, since only they are capable of doing the research and writing the journal articles that add substantially to the stock of recognized knowledge on some well-defined issue, whereas other academics would belong to either the class of interactive experts (if their discipline is stylized in a way sufficiently similar to the one at stake) or to the class of laymen with no relevant expertise whatsoever. In Fleckian terminology, again, the exoteric circle of science has at least all those with some academic education as its constituents, and in some sense, even most Westerners in general.

This is important, because it means that people who are scientifically educated but not initiated in an esoteric circle should nonetheless be able to be drawn toward a particular circle relatively easily. As I will substantiate in section 4.4, one shape this can take falls under the rubric of interessement.

Callon defined the concept of interessement as “the group of actions by which an entity [...] attempts to impose and stabilize the identity of the other actors it defines through its problematization” (Callon 1986, p.207-208), and he immediately went on to state that such actions can take all kinds of form and may implicate any number of devices. A very inclusive definition it is, then. So inclusive even as to not exclude education and history as pathways to a core, the two alternatives the notion of interessement was meant to complement here. Therefore let me point out right now that, even though the concept interessement can be of much use in the global meaning attached to it by Callon, here I use it to distinguish one rather specific path connecting the esoteric circle of neuroeconomics to the (comparably stylized) cores of other scientific specialties. With this description of interessement as a pathway between two esoteric circles, the connection with Callon’s original explication of the concept is kept intact, since Callon justified his choice of the term by saying that “[t]o be interested is to be in between (inter-esse), to be interposed” (Callon 1986, p.208).

Arguably, this account of interessement as one path pertinent to entry to esoteric circles helps us understand the topicality of such general scientific journals as Science, Nature and Proceedings of the National Academy of Sciences of the United States of America (PNAS).

These journals are today’s exceptions in the landscape of academic journals because they do not focus on one highly specialized and esoteric sub-sub-disciplinary field of investigation—such as enzyme inhibition or molecular and cellular processes in the nervous system. Instead, they cover a wide range of sciences and yet still publish original research material. Sometimes, such original research is accompanied by introductions aimed (also) at relatively laic people, and it is in this way that the publications on the neuroeconomics of trust by Michael Kosfeld and colleagues (2005) and Damasio (2005) can be understood.
All in all, the central idea here is that, even though one productive way of getting acquainted with a field of scientific study is to follow the conventional initiation rites associated with academic education, this does not constitute the only route along which esoteric circles can be reached. That is to say, entrance into esoteric circles does not have to follow the path of disciplinary university curricula and the accompanying examinations. It can also potentially be provided through the annals of journals such as *Nature*. Specifically, then, in section 4.4 I will make a case for the idea that interessement can provide an alternative route to the esoteric circle of neuroeconomics. This will be done by analyzing one of the pair of papers mentioned above, that were published in *Nature* in 2005: Damasio’s “Brain Trust.”

### 4.3 Disciplining history, delineating objects

How did neuroeconomics emerge as something akin to an independent scientific discipline? And how do neuroeconomists mobilize history in the delineation of their discipline (i.e., in the establishment of neuroeconomics as a discipline)? Those are the questions this section aims to answer, and to reach this objective I will take my lead from the “official” history of neuroeconomics as it is presented in the introductory first chapter to the flagship of neuroeconomics—*Neuroeconomics: Decision Making and the Brain* by Paul Glimcher and colleagues (2009a), which is, to date, the only textbook in neuroeconomics available.

As its title states this introductory chapter comprises “a brief history of neuroeconomics” (Glimcher et al. 2009a). It is coauthored by all four editors of the book, which makes for a fair representation of both the founding fathers of neuroeconomics and its mother disciplines. The authors and the disciplines they represent are Paul Glimcher, a neurobiologist by training, Colin Camerer and Ernst Fehr, both trained in economics, and Russell Poldrack, who was a cognitive psychologist originally. Emulating their method, here too I will outline the history of neuroeconomics by providing overviews of the respective histories of economics and neuroscience and the diverse ways in which these have interacted so as to constitute neuroeconomics.

**Step one: the neuroeconomists’ history of economics**

Following custom in the history of economics, the first thing we learn about this history from Glimcher et al.’s textbook history concerns Adam Smith’s *The Wealth of Nations* from 1776—the text with which economics as a discipline, allegedly, first began. In their view, Smith’s legacy contains valuable insights with regard to choice and the aggregation of individual choice into market activity. According to Glimcher and colleagues, what Smith delivered “were, in essence, psychological insights” (Glimcher et al. 2009b, p.1).
Traveling fast, we read that after classical economics had begun with Adam Smith formulating psychological insights regarding individual choices and their aggregation into market activity, an era of heterogeneity followed. Various economists, including Francis Edgeworth, Frank Ramsey and Irving Fisher, are mentioned to have been dreaming about physically measuring utility. However, these “early neuroeconomists,” as Glimcher et al. call them, did not have the requisite tools to make their dreams come true. Following the same logic, John Maynard Keynes is presented as an author whose theory of how fiscal policy could be used to manage economic fluctuations was ultimately based in, again, a psychological theory—this time of “animal spirits” and the “propensity to consume” (2009b, p.2).

Having arrived in the 1930s—the time at which it is generally accepted that the so-called “neoclassical revolution” unfolded—the authors slow down a bit. We are briefly introduced to the mathematization of market behavior and consumer choice which was furthered by Paul Samuelson, Kenneth Arrow and Gérard Debreu, and we are reminded of the axiomatic and normative nature of their venture. The authors emphasize that idealized choices and the efficient allocation of resources received far more attention in this era than did describing how people actually make choices and how markets in fact work. In this context, Samuelson’s work on the weak axiom of revealed preference is discussed (Samuelson 1938), as is the expected utility theory which John von Neumann and Oskar Morgenstern developed some years later in their groundbreaking *Theory of Games and Economic Behavior* (1944).

Thus, whereas economics began, in the eighteenth century, as a science of choice and market activity that was driven by psychological insights, by the middle of the twentieth century it had turned into a highly mathematical and normative endeavor that showed little interest in real-life phenomena. Even though the disinterest in factual behavior and its underlying (psychological, individual) phenomena is obviously at odds with the neuroeconomic approach to economics, Glimcher et al. explicate that this disinterest helps explain the success economics had at the time, both intellectually and politically. To quote them extensively:

> These theories of consumer choice [developed by neoclassical economists] would later form the basis for the demand part of the Arrow-Debreu theory of competitive ‘general’ equilibrium, a system in which prices and quantities of all goods were determined simultaneously by matching supply and demand. This is an important tool because it enables the modeler to anticipate all consequences of a policy change—for example, imposing a luxury tax on yachts might increase crime in a shipbuilding town because of a rise in unemployment there. This sort of analysis is unique to economics, and partly
explains the broad influence of economics in regulation and policy-making. (Glimcher et al. 2009b, pp.2-3)

Glimcher et al. point out that neoclassical economics stuck to the advice Pareto gave around the turn of the century to rely “as little as possible on the domain of psychology” (2009b, p.3). Indeed, it cannot be emphasized enough how much the revealed-preference view suppressed interest in the psychological nature of preference, because clever axiomatic systems could be used to infer properties of unobservable preference from observable choice. (Glimcher et al. 2009b, p.3)

Furthermore, while apparently there was a de facto justification in place for mid-century neoclassical economics, Milton Friedman provided what became a canonical de jure justification in his influential book Essays in Positive Economics (1953). Economists, so Friedman argued, are justified in ignoring deviations from perfectly rational behavior on the part of economic subjects, since predictions about market behavior may be accurate even if the assumptions about the mechanisms underlying their behavior (i.e., assumptions about the perfect rationality of economic agents) are wrong.

In the same year Friedman published his book, the French economist, physicist and later Nobel laureate in economics Maurice Allais developed what would later come to be known as the Allais paradox and which, according to Glimcher et al., is “critical for understanding where neuroeconomics arose” (2009b, p.3). The Allais paradox arises when comparisons are made between the choices people make in two experiments, both of which consist of choosing between two different gambles. In expected utility theory it is assumed that people will display a consistency in their choices, which in fact they do not. More precisely, Allais revealed a reliable pattern of preferences violating the “independence axiom” of expected utility theory. Famously, Allais presented these results at a conference where “many participants, including Savage, made choices which violated their own theories” (2009b, p.3).

The Allais paradox was only the first in a number of developments in economics that are seen by Glimcher et al. as having contributed to the refutation of some of the axioms of (subjective) expected utility theory. Glimcher et al. also successively discuss “Ellsberg’s paradox,” which suggests that in choosing people tend to be averse to ambiguity and which is conventionally interpreted as contradicting Leonard Savage’s theory of subjective expected utility; Herbert Simon’s work on the computational boundedness of rationality, calling for the empirical investigation of choice algorithms; and Daniel Kahneman and Amos Tversky’s work, which showed, for example, “framing effects” and thus criticized what is known as the axiom of “description invariance,” that is, “the idea that choices among objects should not depend on how they are described” (Glimcher et al. 2009b, p.4).
These and other empirically based criticisms of the neoclassical axiomatic approaches, so Glimcher et al. (p.4) point out, led many to believe that more psychologically plausible and simultaneously more general axiomatic systems could be developed. Those attempting this called themselves “behavioral economists.” Three lines of work are mentioned to have developed under this broad umbrella: Kahneman and Tversky’s prospect theory, the theory of heuristics and theories of social preference. According to prospect theory, in risky choices we see something that we are all familiar with in the context of the sensation of heat; that is, it is dependent upon reference. With regard to heat this means that how hot or cold something feels depends on previously sensed temperature, and with regard to revealed preference it means that preference is dependent upon context. According to the theory of heuristics, heuristics are supposed to be a potential basis for a theory of choice. Such heuristics can be investigated empirically in different circumstances, and include, for instance, tendencies to rely on the first piece of information offered on a certain problematic. Under the label of social preference, research is conducted concerning people’s valuation of choices when those choices influence values others hold. What all these developments share, is that they put psychological experiments—and sometimes field data—at the center of their endeavor.

Around the same time that behavioral economics developed, people including Vernon Smith and Charles Plott argued that economic principles no less than physical principles should apply universally and, on that assumption, started doing controlled experiments with economic systems. Underlying their work was the consideration that, if economic theories would fail in the simple conditions of laboratory experiments, this would seriously raise doubt about their application to the more complex milieu outside the lab. This line of work is commonly called experimental economics and together with behavioral economics it constitutes a second empirically minded approach in economics, which began to develop in the 1950s. Close as the affinity between these approaches may be, behavioral and experimental economics can be roughly distinguished as follows. Whereas behavioral economists incorporate psychological principles in economic analysis, experimental economists incorporate psychological methods (i.e., rigorously controlled experiments) in tests of (standard) economic theory. Certainly though, it must be understood that, in order to get to the psychological principles on which behavioral economics seeks to base economic analysis, behavioral economists, too, tend to use controlled experiments.

Having come this far, Glimcher and colleagues conclude that

neuroeconomics emerged from within behavioral and experimental economics because behavioral economists often proposed theories that could be thought of as algorithms regarding how information was processed, and the choices that resulted from that information-processing. A natural step in test-
ing these theories was simultaneously to gather information on the details of both information processing and associated choices. If information processing could be hypothesized in terms of neural activity, then neural measures could be used (along with coarser measures like eye tracking of information that choosers attend to) to test theories as simultaneous restrictions on what information is processed, how that processing works in the brain, and the choices that result. [...] Forcing theories to commit to predictions about underlying neural activity [...] provides a powerful way to adjudicate among theories. (Glimcher et al. 2009b, p.4)

In sum, the picture of the economic pile beneath neuroeconomics, sketched by Glimcher et al., can be summarized as follows: with Smith’s psychological theory of market behavior, economics started out correctly, and during a time of heterodoxy economics was rightly impressed by the idea of physical experimentation and investigation (Edgeworth and others). However, technological hindrances (i.e., lack of noninvasive neuroscientific research technologies), as well as theoretical aberrations (from Samuelson to Friedman), had long prevented economics from entering the secure road of science. Behavioral and experimental economists started to mend those weaknesses in the 1950s, but with the advent of neuroeconomics, economists have at long last truly started to walk the scientific high road—be it by providing structure or theory to what without that remains something of a “blind” science (i.e., neuroeconomics in the sense of economics of the brain), or by incorporating psychological and neuroscientific methods in the investigation of economic decision making (i.e., neuroeconomics as the neuroscience of economic decision making). The story of the second meaning of neuroeconomics is translated into a somewhat simplified diagram representing how sound neuroeconomists think economics has been throughout its history in figure 4.1.

Step two: the neuroeconomists’ history of neuroscience

The neuroscientific part of the story by Glimcher et al. also runs along two lines, one neurological and one physiological. In conformity with most accounts in the history of (cognitive) neuroscience, the 1960s are conceived of as being of paramount importance: as Glimcher et al. stated, since the 1960s neuroscientific approaches have started, first, to move closer to each other and then, eventually, became more or less fused during what they call the “cognitive neuroscientific revolution.”

Neurology is that part of medicine concerned with diseases and malfunctioning of the brain and nervous system, and its main line of work originally consisted of investigating human patients with brain lesions or experimental animals in various behavioral tasks. Deficits in the behavior of patients were correlated with their brain lesions and neurolog-
Figure 4.1: A diagram depicting the history of economics according to neuroeconomists.

Figure 4.1: A diagram depicting the history of economics according to neuroeconomists. As Adam Smith was presented as a visionary from the economic camp, the British neurologist David Ferrier (1843–1928) is presented as the primary localizationist pioneer of neuroscience’s classical period. Most of his work focused on damage to the sensory and movement control systems, for the obvious reason that these provide for relatively easily observable data as compared with something as elusive and allegedly private as mental state. During the classical period, neurology lacked a theory relating neurological damage to mental state. The physiological approach was even more severely limited in the classical period because of the invasiveness and often destructiveness of physiological measurement necessarily involved in the correlation between “direct measurements of biological state, such as the firing of action potentials in neurons, changes in blood flow, and changes in neurotransmitters, with events in the outside world” (Glimcher et al. 2009b, p.5).

The revolution that brought the neurological and physiological lines of work together took place in the period from the 1960s to the 1980s. Crucial steps in this revolution were, first, the use within neurology of models from psychology in order to understand brain-behavior relationships and, second, technical advances which made it possible to physiologically examine awake, active animals and their mental states.

However, the newly developing science of the brain found obstacles on its way too. Glimcher et al. mention an excess of different models of mental processes and their correlations...
to physiology and lesion-induced deficits, which could all account for the same phenomena; a scarcity of data, due to the “agonizingly slow rate” and notorious difficulty of physiological experiments, and the imprecision of neurological experiments in humans, resulting from the fact that the placement of lesions in neurological patients cannot be controlled for. Overcoming these problems, so the authors observe, “was at the heart of the cognitive neuroscientific revolution” (Glimcher et al. 2009b, p.5).

For the remainder of their coverage of the neuroscientific preliminaries to the development of neuroeconomics, Glimcher et al. concentrate on studies of decision making. For while these have been at best a marginal topic in the neurosciences for a long time, they are most pertinent to the emergence of neuroeconomics—or, as Glimcher et al. put it, the neuroscience of decision making “forms the central piece for understanding the source of neuroeconomics in the neuroscientific community” (Glimcher et al. 2009b, p.5).

As a brief historiographical aside, let me point out that the phrasing is telling here. That is to say, the use of the word “source” suggests that neuroeconomics automatically or effortlessly flows from the neuroscience of decision making, as if it did not take all kinds of active investments and positive and negative choices to contrive neuroeconomics. Arguably, it is more suitable to describe all the intellectual, technical, methodological and conceptual ingredients that figure in neuroeconomics as resources that have been—and have had to be—actively appropriated toward the establishment of neuroeconomics than as sources that all by themselves do all the requisite work toward building a discipline and delineating its objects of research.

Returning to the topic of decision making, then, Glimcher et al. again approach this from both a neurological and a physiological angle. The physiological history is relatively short. It starts in the 1960s, when the theory of signal detection which had been developed in the 1950s was first applied in a psychological context. It took until the 1980s before William Newsome and J. Anthony Movshon would deliver what was called a “psychometric-neurometric match”—a clear-cut correlation between stochastic choice on the one hand and neuronal activity on the other (see e.g. Newsome et al. 1989). However, because it proved hard to deliver similar results correlating single neuron activity with, for instance, movement generation, it remained controversial whether signal detection theory would be capable of providing a comprehensive framework for studying decision making.

The neurological route to decision making passes a truly obligatory point of passage in the history of neuroscience: the famous case of Phineas Gage. Phineas Gage was a railroad construction foreman whose skull and brain were penetrated with a steel rod in 1848, but who miraculously survived the accident. Due to the damage to his left frontal lobe Gage suffered from a radical alteration in personality and decision making, or so the often-told (and controversial) story goes. In Glimcher et al.’s history of neuroeconomics, Gage’s
Neuroeconomics ascending, a collective assembled | 97

case provides a bridge to the systematic study of the relationship between damage to frontal cortical areas of the human brain and decision making that was undertaken in the 1990s by Antonio Damasio and colleagues. As Glimcher et al. report, the latter line of work became very influential, among other reasons, because it came at a time when “the stage was being set for combining a new kind of physiological measurement with behavioral studies in humans” (Glimcher et al. 2009b, p.6). What this refers to, of course, is the development first of positron emission tomography (PET) and later functional magnetic resonance imaging (fMRI), two technologies for measuring and imaging neuronal activation and enabling the investigation of such during behavioral tasks.

Step three: the neuroeconomists’ history of neuroeconomics

Having outlined those parts of the respective histories of neuroscience and economics pertinent to an understanding of the emergence of neuroeconomics, Glimcher et al. explain that “neuroeconomics” is the name of two different fruits resulting from the crossing of the two separate sciences of economics and neuroscience. We already saw that the parts of these disciplines that are topical to the emergence of neuroeconomics each result from two distinct directions. Recall that for economics these are the normative, rationalist and relatively non-empirical branch that commonly goes by the name of neoclassicism, on the one hand, and the behavioral and experimental economics developed from the 1950s onward, on the other. As for neuroscience, the two sides are constituted by physiological studies and neurological studies.

The two different fruits both going by the name of neuroeconomics, then, are on the one hand the project central to the present study—that field of study in which psychological and behavioral economic paradigms are translocated to neuroscientific laboratories, hence learning something about the neural “underpinnings” of economic decision making and social interaction. The other fruit called neuroeconomics is the field of study in which the normative theory of neoclassical economics is used as a tool to understand and explain neuroscientific data, as if this theory was a Swiss army knife up to any job, independent of the substrate. In this understanding of neuroeconomics, to rephrase, economic theory is used to model the competition for scarce resources between different regions in the brain, or between different neurons, types of neurotransmitters, or the like. I will leave this branch of the endeavor for what it is from now on, and focus exclusively on the “behavioral economics in the scanner” type of neuroeconomics.

After having thus arrived at the stage where neuroeconomics could perform its arts, Glimcher et al. discuss several of its first and most celebrated achievements, including two winners of the Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel11: Daniel Kahneman and Vernon Smith. Kahneman contributed to neuroeconomic research
by correlating ventral striatum activity to subjective valuations of lottery outcomes, as predicted by Kahneman and Tversky’s prospect theory (Breiter et al. 2001). Smith coauthored the first neuroeconomic publication in which behavioral game theory was used (i.e., McCabe et al. (2001)). This paper presented the so-called Trust Game which I will discuss further in chapter [3] Significantly, however, among the major advances they present is another publication concerning trust, this one by Michael Kosfeld and colleagues (2005). Central to this paper is another ingredient of the experiment I discussed in chapter [4]—the neuropeptide oxytocin. That is to say, in Kosfeld et al. (2005) the effect of oxytocin on trust is investigated, providing “the first demonstration of a neuropharmacological manipulation that alters behavior in a manner that can be interpreted with regard to normative theory”, as it suggested “a neurobiological basis for a difference between preferences for social and non-social risks” (Glimcher et al. 2009b, p.10). 

The textbook history of neuroeconomics analyzed

We can draw conclusions from Glimcher et al.’s disciplinary history of neuroeconomics on three levels. First, it provides us with a starting point for thinking about the emergence of neuroeconomics with its characteristic stylization of the social. Second, it gives us the beginning of an analysis of the conditions for the possibility of trust to have become an object of neuroeconomics. In the next chapters I will expand on several aspects of the disciplinary history just outlined, problematizing and complementing it in order to reach a historiographically more satisfactory account. Third, this disciplinary history in and of itself constitutes a particular and meaningful event in the history of neuroeconomics. And it is in this regard that I will now analyze it.

Mobilizing past and present…

The starting point for discussing the role of this disciplinary history in the establishment of neuroeconomics is the observation that the fate of today’s science is in the hands of future generations of scientists and other consumers of science. Whether a statement turns out to be a statement of fact or not depends, to a large degree, on whether it will be treated as such—of course, whether or not it is so treated again depends on many factors (intellectual, social, literary, and so on). No article reporting the results of an experiment will persuade specialist readers from the field at issue if its authors cannot convince the readers that they have mastered all the techniques and theory necessary to be capable of drawing the conclusions they have reached.

As for the fate of neuroeconomics, and hence the neuroeconomics of trust, it seems that as yet it is still too early to judge what this will be. But it is not too early to analyze the role of the disciplinary history of economics on its uncertain path toward the future.
To start with, then, it is helpful to distinguish between what we might call rebellious and loyal disciplinary histories, or disciplined and transgressive histories, for that matter. A good example of the former is provided in molecular biology, “where accounts of the discovery of the double helix were written while the dramatic development of molecular biology was still going on.” (Lepenies & Weingart 1983, p.xvii) Loyal disciplinary histories would be those that refrain from taking such risks and stick to the display of the established outcome of earlier controversies in science. Clearly, the disciplinary history by Glimcher et al. should be classified among the rebellious disciplinary histories: it concerns a discipline that this very textbook assists in building up. As such, their history simultaneously serves to augment the authors’ own achievements in the field and to add to the future success of the field. Indeed, the disciplinary history of neuroeconomics Glimcher et al. provide simultaneously constitutes an active intervention in history and an attempt at its description.

Glimcher et al. present neuroeconomics as if it is the inevitable outcome of a history in which a line of research with too little data complements one with too much data. Neuroeconomics finally fulfills a promise that was made long ago, but which, due to many obstacles, could not be delivered upon in earlier times. Adam Smith had already put the psychology of decision making at the core of the economic discipline (which did not exist as such at the time of Smith’s writing, but that is beside the point just now) and Irving Fischer and Francis Edgeworth had already dreamed of and worked toward physically measuring utility. The generalization concerning disciplinary histories that Wolf Lepenies and Peter Weingart formulate is exactly to the point here, as Glimcher et al.’s neuroeconomic history—written by neuroeconomists—appears to be written in order to extend the present (or what is to become the future) as far as possible into the past, thereby constructing an image of continuity, consistency and determinacy. (Lepenies & Weingart 1983, p.xvii)

Approaching this history from the point of view of today’s economics, we see that some of the proverbial giants recognized as such by neuroeconomists today, were also the giants of their close colleagues, who as yet had not been convinced of the necessity—or even of the possibility—of a neuroscientifically informed and practiced economics. But even though some of these giants have been identified as such by both neoclassicists and neuroeconomists, it has been for differing reasons. That is, the relative impacts of historical events are reordered. For neuroeconomists, Smith is not first and foremost the author of a text promoting laissez-faire capitalism—as he conventionally is among the neoclassicists—but one directing our attention to the psychological drives behind individual decisions. Neoclassical economics, for all the use that was made of it in policy circles, is not hailed as a hero, but criticized for its empirical vacuity and flaws in regard to the model of agents
that is at its core. In other words, the old has been unmasked as flawed or rewritten using the favored categories of neuroeconomists. In the meantime, the long-celebrated *Homo economicus* has been quickly, yet forcefully, invited to step down in favor of the *Homo neuroeconomicus*.

... building the future

By mobilizing history and present accomplishments in the host of sciences they regard as essential, neuroeconomists are building a better world for themselves, their science and, accordingly, all inhabitants of the neuroeconomic universe. It is this latter point which I now must to discuss in more detail. For the claim I wish to defend is that a disciplinary history such as Glimcher et al.’s does not fulfill a didactic role only. It surely fulfills a role in reorganizing or reshuffling academic work, as it may add to the construction of new (under) graduate programs and help establish new organizational ties. But more philosophically relevant is the role such disciplinary history plays epistemologically and ontologically.

Fleck’s work in historical epistemology provides a useful model here. In this model, the further out from the esoteric circle of science we go, the fewer controversies we get to see and the “harder” facts become. Facts are harder outside of the core for the simple reason that what is controversial can only be recognized as such by those members of the epistemic community active in the core. Only if one is a member of the community of specialists will one be in a position to trace back from an alleged statement of fact to where it “originates,” trace back along a trail of transformations and translations from a simple statement of fact to the highly complex assemblage of (theoretical) assumptions, knowledge, (laboratory) skills and the like that together make up the statement’s point of departure. Textbook science is not journal science. When we have reached the textbook, we have moved away from the core, away from the place where *anything* can still be doubted, where any statement’s history of production can be found out. By the time a statement has made it into a textbook, it has become much harder to trace its conditions of production. It has become almost “devoid of any trace of ownership, construction, time and place,” to use Latour’s words (cf. Latour 1987, p.23).

Facts become harder and knowledge more certain. But facts about what, and knowledge of what? Indeed, of those objects granted a place in the neuroeconomic chain of being. This is object formation in action, so to speak. What our world is made up of is continuously renegotiated by way of all such transformations and translocations that we can see happening when we follow the path from scientists in their basement laboratories, to the statements that flow from there into journal science, to textbook science, to popular science, to the news media and the blogosphere, to commercial instantiations, technological
or otherwise, and then follow it all the way back. Perhaps no item of knowledge will ever be a stable, secure and unchanging object or phenomenon in this world. But if something is ever to approach such status, it will have to move through textbooks. And as I just showed, neuroeconomic trust has done just that.

4.4 Nature, trust, and the literary genre for interessement

Today there is an abundance of ways to disseminate scientific ideas. Next to the *locus classicus* for the publication of novel experimental results or theoretical insights (i.e., the academic journal article), there are popular scientific magazines, science sections in newspapers, televised documentaries and blogs; and even Facebook and Twitter are casually used by scientists who want to share their insights with the rest of the world. However, these media or genres can be divided roughly into two categories: those aimed at reaching colleagues in one’s own inner circle, and those aiming, less discriminately, at the larger nonspecialized public. Interestingly, however, there is also a third intermediate category of scientific publications. It is this which I turn to now.

Rather than going over this genre of scientific writing in the abstract, I will take a close look at a journal article that is illustrative of the class and that deals with the neuroeconomics of trust. Thus, my focus here is on an article by renowned neuroscientist Antonio Damasio, published in the highly respected scientific periodical *Nature*. However, even though this article was published in *Nature*, it does not itself present original research. On the contrary, it only announces and praises others’ research, in this case research by Kosfeld et al. (2005) that was published in the same issue of the same journal. This is topical to my present purposes, since Kosfeld et al.’s article is considered to have established as fact that the neuropeptide oxytocin increases trust in humans.

The interconnection between the paper by Damasio and that by Kosfeld et al. is so strong, that Damasio’s article cannot really be understood independently from Kosfeld et al.’s article at all. Both papers even have basically the same structure, with the major difference being that Damasio’s paper leaves out the details regarding methodology and interpretation of data, which Kosfeld et al. spend most of their time discussing. On the whole, the paper by Damasio is best understood as an attempt to familiarize the general scientifically educated and interested audience of *Nature* with neuroeconomics and, to potentially arouse their interest such that they will also read the paper by Kosfeld et al., which subsequently elaborates in more detail what role oxytocin plays in the promotion of trust in humans.

If we take a better look, not just at this pair of papers but at the journal *Nature* in general, we can discern a pattern. *Nature* customarily publishes pieces such as Damasio’s in their
section “News and Views” to accompany papers in two sections reserved for original research, “Articles” and “Letters.” Furthermore, the authors of the shorter pieces published in “News and Views” tend to be big names in the respective field. That we mainly find well-known scientists in this section should come as no surprise: as Nature’s author information pages state, the “News and Views” section “is a commission-only section.” Furthermore, articles published in this section explicitly aim to “inform non-specialist readers about new scientific advances, as reported in recently published papers”.

This first-rank science journal, then, explicitly reserves space for communicating new scientific developments to domains beyond the esoteric circles of the various sciences for which this is a platform, toward relative outsiders that is. Given, however, that articles in “News and Views” tend to accompany original research papers, a type of entranceway into scientific fields appears to open, and this can surely be likened to more typical initiations, such as those found in textbooks.

Especially in the case of hybrid sciences such as neuroeconomics it can be argued that papers like Damasio’s can play a crucial role in enrolling people into a new esoteric circle. They can do so by way of interessement, as they make experts from adjacent fields come to see their own work as fitting in with or relating to the work reported on, and inviting them to explicitly align with this. That is to say, when it comes to hybrids like neuroeconomics, there will likely be many people for whom some part of the puzzle at issue belongs to his or her own esoteric circle, while other parts do not. These people, then, may become interested in adding to the new field, hence becoming members of novel collectives, changing their identity and adding to the stability of the field. For example, neurophysiologists might be drawn to neuroeconomics because of what they find in a pairing of papers that show how neural pathways may be correlated with types of behavior of which they have no knowledge whatsoever, while economists or psychologists reading that same pair of papers might become interested for just the opposite reason: Familiar as they are with the quirkiness of human behavior and social interaction, they might be drawn to neuroeconomics to find out more about the physiology allegedly underlying such behavior and potentially explanatory of it. Or take (neuro-)psychiatrists, who might find the behavioral game theory used in neuroeconomics of interest in their own cause of understanding deviant development, behavior or personalities and their (neurophysiological) causes.

Entering and leaving the laboratory with oxytocin

Fleck points out that what we have to look for in initiations such as Citron’s “pedagogical” one, which he examines, are what he calls “active linkages,” i.e., “factors which are not subject to logical legitimization” but that are “essential both to the further development of knowledge and to the justification of a branch of knowledge that constitutes a
science in itself.” (Fleck 1979, p.54) He describes such active linkages as collectively shared preconditions for cognition:

The preconditions correspond to active linkages and constitute that portion of cognition belonging to the collective. The constrained results correspond to passive linkages and constitute that which is experienced as objective reality. (Fleck 1979, p.40)

It is important to understand here that what outsiders experience as logically unjustified steps are, by those who carry the style at issue, experienced as necessary and logical. This, then, is where we recognize a style at work, and this, hence, is what we should be looking for here too. Thus, the remainder of this chapter, as well as chapters 5–6, is largely dedicated to identifying and analyzing such active linkages at work in the neuroeconomics of trust.

What I am after, then, is determining out what investments had to be made in order to arrive at the aforementioned principal lesson of both Kosfeld et al.’s and Damasio’s articles: namely, that the neuropeptide oxytocin increases trust. By making explicit things that, for example, are to the authors at issue too obvious to merit discussion, I will identify and clarify the style at work in neuroeconomics. This analysis, moreover, further supports the idea I have been developing in this chapter—that Damasio’s article enrolls people into the esoteric circle of neuroeconomics by way of interessement.

Naturally, for a text to do the work of interessement, it is in no way necessary that the alignment of interests is an explicit goal of it author. Intentions on behalf of any party involved need not be denied, though they may be considered to be of, at best, secondary importance. Structural features of the field, of types of literature and of their specificities in terms of content, form and audience are much more important, as must be clear from the above. Moreover, this also becomes clear when we turn to the first thing that attracts attention in Damasio’s article. This surely is the picture that goes with it (reproduced here as figure 4.2), but that was most likely not picked out by Damasio himself. We see two immaculately clothed people shaking hands. The people pictured are without a doubt men, probably businessmen but almost certainly members of society’s well-to-do. It is a firm yet friendly grip with which they hold each other, and the handshake is illumined in such way as to give an impression of warmth—or more to the point, an impression of trust and trustworthiness. Moreover, the way the handshake—which is surely very transient—is captured, gives it the impression of something enduring. This suggestion of permanence adds to the appearance of trustworthiness and trust, as does the quality of the suits the men are wearing. Similarly, the impression that the men shaking hands are involved in serious business adds to the weight of the issue addressed in the article it accompanies: this is important.
In Damasio’s paper there is no caption with this photograph. Probably it is thought that a picture—this picture—says more than a thousand words, and that it would be redundant to explain what it says or illustrates. We can conclude that the picture is simply there to illustrate the bottom line of the article it accompanies. And the headline to Damasio’s article, which is immediately below the picture, clearly states this bottom line. In bold letters it reads:

As is the case with other social interactions, financial transactions depend on trust. That fact is behind ingenious experiments that explore the neurobiological underpinnings of human behaviour. (Damasio 2005, p.571)

There we have our entranceway into the neuroeconomics of trust then: trust is said to be of utmost importance in finance, and there is a biological basis to it, the investigation of which has recently started to take shape. Attention is directed at an important issue, something with great societal significance which is open to scientific investigation.

If, thereupon, we start reading the main body of the article, we are swiftly immersed in a whole new world of trust. After a very minimal and abstract introduction to the Trust Game—leaving out not only its name but also most details of the game, really only noting that it is a very “serious game” for the reason that it involves “real monetary exchanges” (Damasio 2005, p.571)—we almost immediately take a dive in the (deep) waters of neuroscience. Let the cobbler stick to his last, Damasio must have thought, as the article has hardly begun before we are already following the neuropeptide oxytocin going into the noses of the experimental subjects and through their blood-brain barriers, so as finally to increase the subjects’ trust.

After we have thus been introduced to the subject matter of the research reported on and have been acquainted with its primary conclusion, we are reminded of the ubiquity and
importance of trust in “love, friendship, trade and leadership” (Ibid.). This importance is casually connected to the polarity of reward and punishment that permeates biology. That is to say, Damasio writes:

> Given the polarities of reward and punishment that pervade biology at various levels, trust is essential for the normal operation of human societies. (Damasio 2005, p.571)

That trust is critical to society may be the case, as it may be the case that punishment and reward can be found at many levels of biological organization. However, how the latter implies the former, is not as immediately and obviously clear as Damasio assumes. That is to say, Damasio actively links society and biology, but without explaining in any detail what this link is supposed to be made of or how it is supposed to work. Arguably the underlying assumption responsible for this rather offhand association is that sociological properties of whatever kind simply have to reduce to biological ones—many a philosophical naturalist’s intuition, undoubtedly. But precisely how and at what level of analysis such reduction (or correlation) would have to take place is less obvious. This statement, which is almost in passing, functions simultaneously as a reminder of the importance of the topic at issue and as a bridge to a more detailed discussion of some allegedly basic facts about oxytocin.

We are told of oxytocin that it is a small peptide, consisting of nine amino acids, that is produced mostly in the hypothalamus, the brain’s master controller of biological regulation, including emotion. Oxytocin acts both on certain targets of the body (it is best known for inducing labor and lactation) and on brain regions whose function is associated with emotional and social behaviours (the amygdala and nucleus accumbens, for example)—that is, it works both as a hormone and as a neuromodulator, a kind of neurotransmitter. In animals, oxytocin contributes to social attachments, including male and female bonding after mating, mother and infant bonding after childbirth, and assorted sexual behaviours. (Damasio 2005, 571)

Focusing here on allegedly firm facts about oxytocin’s promotion of approach behavior in sexual relations and in bonding between mother and infant, concluded from research in neuroendocrinology that conventionally concerns voles (i.e., small mouselike rodents), prepares us for the next step. This next step constitutes a big investment—the hypothesis that actively links trust in humans to oxytocin. How exactly we get there, Damasio again is not all too explicit about, but at this point we are nonetheless clearly beyond any doubt about the fact that there is a connection between oxytocin and trust which deserves experimental investigation. After some colloquial observations establishing the connection between the animal behaviors oxytocin is involved in and human behaviors that we tend
in ordinary language, to classify as resulting from or giving expression to trust, the subsequent discussion concerns the various possible mechanisms that oxytocin may be involved in and that may be “behind [Kosfeld and colleagues’] findings” (Damasio 2005, p.571).

As Damasio explains, both the possibility that (1) the researchers’ behavioral findings are due to a nonspecific positive effect of oxytocin on social behavior, and (2) that these findings are due to oxytocin reducing sensitivity to risk, are rejected. The pair of factors Damasio describes Kosfeld et al. as settling on, then, is that

oxytocin overcomes the aversion to betrayal (which applies only to the investors), and that this is combined with the effects of reward that result from enhanced approach behaviour. (Damasio 2005, p.571)

In other words, trust is redefined as betrayal aversion and/or approach behavior, and trust is increased both by oxytocin and, additionally, by the rewards that oxytocin-induced enhanced trusting behavior (i.e. approach behavior) reaps.39 As Damasio states with the certainty typical of the genre of his writing: “[a]fter all, trust and approach behaviour are indelibly linked” (2005, p.571). And the connection between trust and approach behavior is subsequently justified by Damasio when he states that we “commonly describe the child who approaches others with ease as ‘trusting’” (Ibid.). Thus, Damasio substantiates Kosfeld et al.’s claim through an implicit turn to a kind of casual use of ordinary language philosophy, suddenly (and atypically) making our quotidian use of language the arbiter of reality. What goes unnoticed is that Damasio’s example provides but one of the many diverse ways in which we use the notion of trust. And likewise, it goes unnoticed that this aspect of trust might not be generalizable to other instances of our use of the word—for instance when we speak of trust in politicians or banks, or trust in one’s child’s capacity to ride a bike without training wheels, to name but some alternative colloquial uses of the term.

That the findings by Kosfeld et al. are not the result of nonspecific positive effects of oxytocin on social behavior, firstly, shows from the fact that oxytocin affects behavior by investors and trustees differently; exactly how is not further spelled out by Damasio, but I will discuss this in chapters3 and 5 What is clear, though, is that after all the requisite conceptual, experimental, technological and theoretical work has been done, it follows passively that approach or trust dominate investor behavior, whereas reciprocity is central to the behavior by trustees. The latter is not impacted on by oxytocin, the former is.

Secondly, the possibility that the increased willingness of investor (Players One) in the oxytocin group to send money to the trustees (Players Two) is due to a reduction of sensitivity to risk is ruled out by a control experiment the researchers conducted. For in an experiment in which subjects knew that they were playing against a computer rather than against a human being—the so-called Risk Game30—investors in the oxytocin groups did not take significantly more risks than those in the placebo groups. The Risk Game is very
similar to the Trust Game, but does not involve a trustee. Here the investor’s risk depends not on a decision by a trustee, but on a random decision. Put simply, this is a Trust Game in which a computer takes on the role of Player Two, or trustee. Due to this the game no longer features social interaction—which supposedly is the relevant factor here.\textsuperscript{13}

And with these interpretations of Kosfeld and colleagues’ findings, we finally leave the laboratory. After the article opened by emphasizing the societal significance of the research at issue (in word and image), Damasio once more highlights that “[t]he significance of the study lies in what it can tell us about non-experimental circumstances, when the equivalent of an investor is not sniffing oxytocin” (2005, p.571).

However, that we are retiring from the laboratory does not of course mean we are leaving behind the brain. Damasio opens his discussion of the relevance of Kosfeld et al.’s research with a statement on the probable natural release of oxytocin in certain areas of the human brain, as caused by the cognitive appraisal of certain social configurations (2005, p.571-572). This is fairly hypothetical, as it is hard, if not impossible, to monitor neurochemical events in vivo. After said hypothetical release of oxytocin, the peptide’s effect on cognitive neural networks would be to increase the trusting behavior such networks can give rise to.

In discussing this, Damasio does not fail to emphasize that Kosfeld et al.’s findings point “to the crucial involvement of emotional phenomena in the processes leading from cognition to behaviour” (2005, p.572; my italics). Emotional phenomena are seen as playing this pivotal role because “neural events arising in brain areas associated with social and emotional responses” are part and parcel of the above explanation. Earlier in the article, Damasio more poetically brings in this issue as well, when he refers to his own research (i.e., Damasio 1994) in order to mention that there he “likened oxytocin to a love potion, the magic elixir that makes Tristan fall for Isolde” (Damasio 2005, p.571). Kosfeld and colleagues are here assumed to have added a crucial ingredient to the mixture, as “there is no love without trust” (Ibid.).

That Damasio stresses the emotional character of the trusting behavior and the neural events associated with it, when Kosfeld et al. do not frame their findings in this way at all, would probably come as no surprise to those who know that Damasio more or less built his career on his ventures in the neuroscience of emotion. In this field of research he has gained fame through his so-called somatic marker hypothesis, which involves connecting emotion, cognition and decision making in one intricate whole, reminiscent of William James’ psychology (see e.g. Damasio 1996). The affinity between Damasio’s work on emotion and Kosfeld and colleagues’ work on trust, however, is of course not all too odd. Both seem to give rise—at least at first sight—to a view of human cognition and conduct in which more than “cold” rationality alone is given explanatory value, thus constituting an antidote to much of neoclassical economics and (American) social science more generally. In other
words, it is clear why Damasio is interested in the neuroeconomic work by Kosfeld he introduces here.

As we approach the end of Damasio’s “Brain Trust,” we take a turn towards the clinic. We learn that several psychiatric or neurological disorders in some way or other involve either diminished or augmented trust. Mentioned by name are autism and Williams syndrome, a rare genetic disorder. The findings of Kosfeld and colleagues open up new investigative routes in this regard. Too much or too little oxytocin release might be involved in the augmentation or decrease of trust, Damasio speculates (2005, p.572).

And then, just before Damasio concludes his article by stating that “Kosfeld et al. have made a valuable contribution to our understanding of the role of neuromodulators in human behaviour that involves choice” (2005, p.672), he takes an abrupt turn. That is, Damasio considers the possibility that people may be concerned about the abuse that can be made of the finding that oxytocin increases trust. Might this not invite “political operators [to] generously spray the crowd with oxytocin at rallies of their candidates” (Damasio 2005, p.672)? Immediately thereafter this fear is disarmed or, if not disarmed, relativized. For, so Damasio suggests, it might well be the case that marketing techniques that have been in use for a long time already work their charm by promoting the natural release of oxytocin. Moreover, Kosfeld and colleagues cannot be blamed for having raised, implicitly and unintentionally, said possibility. Thus, in an implicit defense of the idea of value-free facts, Damasio proclaims that, independent of either the beneficial biomedical use made of the findings by Kosfeld et al. or the political or commercial abuse thereof, these findings are valuable in and of themselves.

4.5 Conclusion

In this chapter I have built on Fleck’s observation that it is a precondition to practicing science that one is enrolled in some or other esoteric circle and hence one’s work is “true to style.” To do this I reviewed the disciplinary history of neuroeconomics and analyzed an example of a relatively nontechnical introduction to the neuroeconomic account of (an aspect of) trust. The former gave us insight into the history of neuroeconomics and its interest in trust, and simultaneously into the epistemological significance of the literary genre of disciplinary history. The latter provided us with an example of a way of entering esoteric circles that differs from those identified by Fleck. Therefore, we saw how general experts—scientists who share at most only part of the knowledge and skills engaged in neuroeconomics—are introduced to both the field of expertise and the topic of trust as it is stylized in this field.
As concerns the disciplinary history of neuroeconomics, it must be emphasized that this simplified and stylized disciplinary historical narrative on neuroeconomics’ emergence not only makes up an attempt at describing the history of neuroeconomics, but at the same time also constitutes an intervention in its development. If anything, history is enrolled to help build a future for neuroeconomics, complete with its own stylized ontology and epistemology. That trust thusly stylized figures as a neurobiological reality and that this reality is best scrutinized using the proven methods of neuroscience and behavioral economics is clear.

We see that, with his article in *Nature*, Damasio clearly did not intend to reach those in the esoteric circle of neuroeconomists. Rather, his text functions as a kind of signboard, a marketing device more than anything else, potentially contributing to the enrollment of future neuroeconomists. The (relatively) nontechnical nature of the article and the introductory function it plays undoubtedly add to its lucidity. The style at work shows itself for instance in what proves to be too obvious to merit discussion, what is considered a possible investigative or explanatory lead and what methods are considered appropriate. In this way, a variety of active linkages crucial to the field of neuroeconomics become visible.

At times Damasio is quite explicit about the provisional nature of a particular linkage. This is the case, for example, when it comes to the modality of the statement that oxytocin is released in social configurations—a statement he characterizes as probable (Damasio 2005, p.371). At other times, however, linkages are conceived to be so obvious as to not merit discussion. This we see when Damasio moves from research on the neuromodular effects of oxytocin in voles’ love relations and mother and child relations, to the (alleged) effects of oxytocin in humans. That evolution provides a bridge from facts about one species to facts about another is beyond doubt; that in concrete cases the basis for extrapolation or generalization can be a very complex matter is kept out of sight entirely.

In the next chapters I will further elaborate on the style of neuroeconomics. In order not to get lost while doing so, I will use Damasio’s article as a sort of map to navigate through the maze. For in all its introductory clarity, this article supplies us with the perfect mindset with which to enter the esoteric circle of neuroeconomics—that is, it opens our eyes to many of the relevant details and directs our attention to what matters most as it reveals a number of significant active linkages at work. This preliminary identification of active linkages, then, allows and invites us to analyze them in more detail—to trace the connections that have so painstakingly been built by various collectives over a long period of time—connections, also, which will sometimes turn out to be not that strong, or which will link things that are not as proximate to each other as Damasio’s text suggests.