

# Reflections on the 2017 Nobel Memorial Prize Awarded to Richard Thaler

TILL GRÜNE-YANOFF

*Royal Institute of Technology (KTH)*

Richard Thaler received this year's *Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel*. In my view, this is an outstanding decision that puts the spotlight on an interesting and controversial field in economics, with many fascinating methodological and foundational questions yet to be answered. I therefore gladly accepted the editors' invitation to reflect on the methodological and foundational aspects of Thaler's research.

The Economic Sciences Prize Committee (consisting of five economists and a philosopher, Peter Gärdenfors) explicitly mentioned many methodological aspects of Thaler's work in its motivation.<sup>1</sup> In particular, it refers to interdisciplinary integration, realistic assumptions, explanation vs. description, normative implications of cognitive limitations, and policy design. This offers me a good opportunity to structure my discussion here.

## 1. INTEGRATING ECONOMICS WITH PSYCHOLOGY

The committee wrote that Richard Thaler's research has been "instrumental in creating [...] behavioral economics". Historically, this claim seems uncontroversial. A number of historical accounts already have identified Thaler, amongst others such as Daniel Kahneman, George Loewenstein and Eric Wanner from the Sloan Foundation, as key figures in the development of behavioral economics (see, for instance, Angner and Loewenstein 2012; Heukelom 2014). However, the committee went on to describe the development of behavioral

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<sup>1</sup> I am referring here to their press release (Royal Swedish Academy of Sciences 2017a). All references in these reflections are to the press release, except if otherwise indicated. You can also read the Economic Sciences Prize Committee's essay-long summary of Thaler's research (Royal Swedish Academy of Sciences 2017b), and watch an interview in which Gärdenfors explains the decision (Rose and Gärdenfors 2017).

economics as an interdisciplinary achievement. Thaler, the committee stated, incorporated “psychologically realistic assumptions into analyses of economic decision-making”, built “a bridge between the economic and psychological analyses”, thus “*integrating* economics with psychology” (my emphasis).

Philosophers often characterize *integration* as the increase of unity between two scientific disciplines or sub-disciplines. This might consist of theory reduction, unification of explanations, integration of methods, or integration of data (Brigandt 2013; O’Malley 2013). But behavioral economics is not the result of integration of economics and psychology in this sense, for the following three reasons.

The two disciplines were firstly very selective in their appropriation and importation from each other. Disciplinary differences that were upheld despite these exchanges included the continuous focus on axiomatization in economics, which never became prominent in psychology, and the differences in both experimental methodology and explanatory strategies between economics and psychology. I have described these differences in a study of the field of intertemporal choice (Grüne-Yanoff 2015), to which Thaler has been one of the key contributors, both through his empirical (Thaler 1981) and his theoretical work (Shefrin and Thaler 1981; Thaler 1985; Shefrin and Thaler 1988).

Second, behavioral scientists had to admit relatively early on in their research that their new models had substantial difficulties in quantitatively capturing stable deviations from standard theory. This held (and still holds) both for the domain of intertemporal choices, where the slope and even the shape of the discounting function is a matter of debate (see, for example, Frederick et al. 2002), as well as for the domain of risky choices, where the form of risk preferences remains a controversial issue (see, for example, Harrison and Ross 2017). Behavioral economists and psychologists have reacted rather differently to this problem. Behavioral economists have typically fallen back on more abstract models that can be axiomatised and whose effects can be shown through theoretical proof. Psychologists, in contrast, have typically narrowed the domains of their hypotheses, trying to respond to the divergence of measurements by a more piecemeal approach (Grüne-Yanoff 2015). This difference puts further doubt on the integration claim.

What is often forgotten by those arguing for the integration claim is, thirdly, that psychology is a much more heterogeneous science than economics. Multiple theoretical and methodological approaches currently coexist, and none can claim dominance over the others. For example, dual process theories, while certainly prominent in psychology, in the last years have come under increased scrutiny with the publication of a number of replication failures (for a recent overview, see Lurquin and Miyake 2017). That economists currently champion such dual process accounts—perhaps through the influence of Kahneman’s *Thinking Fast and Slow* (2011) and Thaler’s planner-doer model—does not constitute an integration of psychology and economics, even in the sub-domain of self-control.

I focused here on intertemporal choice. Others have argued for similar claims for choice under risk (such as Sent 2005; Davis 2013; Heukelom 2014), namely that behavioral economics is a very selective and limited inclusion of psychology into economics, and that it more often amounts to an inspiration for a change in economic modeling than a genuine integration of psychological concepts, theories, models or methods.

## 2. REALISTIC ASSUMPTIONS

The committee stressed that Thaler “has incorporated psychologically realistic assumptions into analyses of economic decision-making” and “has shown how these human traits systematically affect individual decisions as well as market outcomes”.

“Realistic assumptions” of course is one of economic methodologists’ favored *bêtes noires*, and for good reason. First, the concept is ambiguous. To say that an assumption is realistic might mean either (i) that it describes its target correctly in every aspect (‘the-whole-truth’ view), or (ii) that it describes at least some aspect correctly (‘nothing-but-the-truth’ view), or (iii) that it approximates all or some aspects of its target (Mäki 2011). This, secondly, drives a wedge between realisticness and truth. A theory with unrealistic assumptions may be true, for example when it correctly describes some aspect of the target, even though some of its assumptions are considered unrealistic because they violates the-whole-truth view. For this reason, thirdly, realism (as a theory of theories) is perfectly comfortable with unrealistic assumptions, as has long been established by Cartwright (1989), and Mäki (2011). But then realisticness of assumptions by itself is not a good

criterion for assessing models, nor is unrealisticness by itself a reason to reject a model. Instead, what matters is whether these assumptions are considered *relevant* for the modeling purposes. Here economists continue to disagree.

Robert Shiller, in his *Guardian* article congratulating Thaler with the Nobel, names two late economists (Merton Miller and Stephen Ross) who considered “stories of such [psychological] mistakes almost completely irrelevant to finance” (2017). They did not deny that biases and psychological mistakes described by Thaler exist (that is, they agreed that these were realistic assumptions), but argued that when modeling the *dominant* factors determining prices, demand and production, such biases—despite being realistic—could be legitimately neglected. In fact, models that neglect them and thus are unrealistic would sometimes be better models, precisely because they avoided the distraction by all these realistic but quite irrelevant assumptions. Now, Miller and Ross are dead—and perhaps Shiller suggests that their methodological views should be on their way out as well. But I suspect that plenty of economists alive today harbor similar sentiments, and I certainly think that their insistence on relevance and their skepticism against realisticness is legitimate.

In any case, endowing models with more realistic assumptions was not Thaler’s original motivation, as a closer look at his early career shows. Instead, he was grappling with a measurement problem that arose from his attempts to systematically measure subjective evaluations of fatality risk, crime control, and other non-monetary features (for instance in Thaler and Rosen 1975; Thaler 1978). For example, Thaler and Rosen developed a willingness-to-pay (WTP) concept for a marginal change in mortality risk and estimated it from salary data for different professions with differential fatality risks.<sup>2</sup>

Yet, soon it turned out that willingness-to-accept (WTA), another measure which according to standard theory should yield identical results to WTP, yielded systematically higher results. This is a classic case of a failed *convergent validity*, and, as such, poses a problem for the measurement construct. Thaler’s subsequent work on identifying the culprit of the WTP-WTA divergence—in particular, his (1980) proposal of *loss aversion* as the explanation—thus can be understood as a search for systematic measurement errors. Systematic errors, however,

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<sup>2</sup> This was explicitly based on a neoclassical framework, “follow[ing] up Adam Smith’s ancient suggestion” (Thaler and Rosen 1975, 266)

are properties of the measurement instrument, and do not concern the assumptions of the model whose parameters are being measured.

By identifying systematic measurement errors, one can explain why an empirical test of a theoretical hypothesis yields a negative result, without being forced to reject the main hypothesis. Instead, the blame can be put on the auxiliary assumptions regarding the measurement: it was the non-validity of the measurement instrument that brought about the negative test result. In this sense, Thaler's early work, instead of attacking mainstream economic theory for being unrealistic, rather could be said to have protected it. Furthermore, identifying systematic biases (and here the term bias has a precise meaning, namely as a deviation of measurements from the true value) does not commit one to considering these biases relevant for the theoretical core of economics. Skeptical economists can thus accept Thaler's empirical findings and still argue that for their predictive and explanatory projects, these factors are 'almost completely irrelevant'.

Thaler, I suspect, would disagree. His writing from the mid-1980s onwards suggests that he took aim at economic theory itself, not just at auxiliary measurement assumptions. His famous "Anomalies" series in the *Journal of Economic Literature* from 1987 to 1991, and then irregularly from 1995 to 2006, is perhaps the clearest expression of this. The first two papers of the series quoted Thomas Kuhn's *Structure* (1970).<sup>3</sup> From the third onwards, Thaler gave his own definition of an anomaly: "An empirical result qualifies as an anomaly if it is difficult to "rationalize", or if implausible assumptions are necessary to explain it within the paradigm" (Thaler 1988, 191). Here failure of expectation or prediction had been replaced by the failure of rationalization or explanation.<sup>4</sup> The aim is clear: it is the "paradigm", which fails to explain adequately. No doubt Thaler was aiming at core theory here.

Whatever the precise goal, these anomalies were conceived as a collection of *exhibits*: empirically observed phenomena that economists should be able to explain, but couldn't. The main focus thus was on

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<sup>3</sup> The quotation from Kuhn is: "Discovery commences with the awareness of anomaly, that is with the recognition that nature has somehow violated the paradigm-induced expectations that govern normal science" (1970, 52-53). In the same paragraph, Kuhn continued: "And it closes only when the paradigm theory has been adjusted so that the anomalous has become the expected." It is perhaps telling that Thaler chose not to make these revisionary intentions explicit at that point.

<sup>4</sup> Given the popularity of Friedman's methodological argument at that time, I suspect that economists' main goal in the 1980s was still prediction. So, Thaler's pronouncement here constitutes a noteworthy methodological shift that I believe would merit further investigation.

phenomenal description, with theoretical development to come later. Not by coincidence have some fellow methodologists described the behavioral economics project in its entirety as such a collection of exhibits or “bottled phenomena” (Guala 2005; Sugden 2005). Even if economists could have agreed that these phenomena should be properly accounted for by their theories, how core theory was to be changed, and *what kind* of assumptions in particular should be reformulated in the models, remained a contentious issue.

Thaler, it seems, had clear views about this from early on: namely to make model assumptions more “psychologically realistic”, whatever that exactly means. That, rather than correcting measurement errors or providing bottled phenomena, he got the Nobel Prize for. In fact, the committee mentions his extensive experimental work only in *passim* (and then only in its role as a measurement tool for social preferences) and does not mention anomalies at all. Clearly, behavioral economics today is seen as more ambitious than merely improving measurements and collecting anomalies. Rather, it strives to introduce “more realistic” assumptions into economic models, and for these new assumptions to provide (i) explanations of economic decision-making, (ii) normative justification of intervention in the decision-making process, and (iii) a basis for designing interventions in the decision-making process. To these three aspirations I turn now.

### 3. EXPLAINING COGNITION AND BEHAVIOR

The committee lauded Thaler for developing theories to “explain why people value the same item more highly when they own it than when they don’t” and for “explaining how people simplify financial decision-making”. But are these actual explanations?

Most philosophers today agree that scientific explanation involves identifying some relevant contributing cause (see, for example, Woodward 2003). At the very least, to answer whether Thaler’s theories provide explanations requires determining whether they identify relevant contributing causes of the behavioral or social phenomena to be explained.

Clearly, behavioral economists often use causal language. For example, Thaler (with co-authors Tversky, Kahneman and Schwartz in the title of a 1997 *QJE* paper) investigates “the *effect* of myopia and loss aversion on risk taking” (my emphasis). Loss aversion here is declared a

cause of behavior, and thus loss aversion becomes a potential *explanans* of this behavior.

By loss aversion, Thaler meant people's tendency to prefer avoiding losses to acquiring equivalent gains. To make this more precise, he employed Kahneman and Tversky's *Prospect Theory* (1979) to represent people's reference-point dependent subjective evaluations of certain outcomes: a continuous value function is concave to the right of the reference point and convex to the left of it (Thaler 1980).

Prospect theory (in particular the 1992 version of *Cumulative Prospect Theory*) is itself a modification of von Neumann-Morgenstern (vNM) utility theory. It shares with vNM the view that choice options are lotteries—that is, sets of uncertain outcomes, whose value is identified through a value function, and whose uncertainty is quantified through an (objective) probability distribution. Prospect theory adds to this that the value function is reference-dependent as described above, and that the choosing agent imposes a subjective weighting on the probabilities, with higher probabilities being overweighted relative to smaller probabilities. The value of choice options then is calculated as the weighted average of the thus-valued outcomes, and individual choices are predicted as the optimization of these choice option values.

What matters for explanation here is that some economists argue that vNM theory (as well as its Bayesian relatives) must not be interpreted causally. Utility functions can be fitted to human behavior, but it would be a fallacy to claim that the behavior is *caused* by subjects—consciously or unconsciously—maximizing the fitted utility function inside their heads (Binmore 2008, 7). This argument also applies to prospect theory, as it is just an expansion of vNM theory, with a few more free parameters that allow more flexible fitting. Consequently, if Binmore's argument is correct, then Thaler's claim that prospect theory identifies a contributing cause of behavior, or that indeed it is explanatory, seems dubious.<sup>5</sup>

In a similar vein, it has been argued that behavioral economists typically engage in developing *as-if* models: namely, models that fit the behavioral phenomena, but that makes no (legitimate) claim to the underlying psychological mechanisms that brought about this behavior. Such strategies have been explicitly used by theorists such as Milton Friedman and Jimmy Savage to defend standard utility theory (Starmer

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<sup>5</sup> I should mention that this argument received considerable criticism from philosophers (see, for instance, Hausman 2011).

2005). Berg and Gigerenzer (2010), for example, argue that behavioral economists have largely inherited the strategy from neoclassical economics. Behavioral economists' claim to improved empirical realism, so they argue, is based only on adding new parameters to fit behavioral data, rather than specifying psychological processes that genuinely *explain* these data.

The alternative that Gigerenzer and colleagues have proposed is to abandon the vNM framework altogether and instead to investigate the actual psychological processes that produce behavior. The result of their substantial work so far indicates that such an endeavor will not yield any unified and universal framework of rational decision-making, but rather a collection of heterogeneous processes whose application is highly context-dependent.

Whatever the merits of this and other alternatives, my point here is only to document some of the substantial criticism that claims about the explanatoriness of constructs like loss aversion, reference dependence, altruistic preference, or hyperbolic discounting face. To critics, such constructs refer to experimental phenomena, rather than causal mechanisms. Consequently, in their view, these constructs could not be considered either causes or *explanantia*. Behavioral economists have, in my view, done little to counter these arguments, and thus it remains an open question whether their contributions so far have provided any explanations.

#### 4. NORMATIVE JUSTIFICATION OF POLICY INTERVENTIONS

In its motivation, the committee also noted that Thaler studied “how cognitive limitations influence financial markets”, and that the result of these investigations “may help people” to overcome these limitations. Concepts like bias, cognitive limitations, bounded rationality that Thaler and his colleagues have coined all have a normative connotation. People in their day-to-day deliberation and cognition systematically fail to achieve a normative ideal, and therefore are biased, limited or bounded.<sup>6</sup> Yet what is this normative ideal, and is it applicable to the cases that behavioral economists ultimately are interested in?

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<sup>6</sup> The term “bounded rationality” of course is older than behavioral economics in its current form. Herbert Simon (1982) arguably employed the term in the sense of “bounded in cognitive capacities, yet rational”, while behavioral economists tend to use it as “limited in rationality”.



Thaler early on (1980) proposed to distinguish descriptive models of consumer choice from normative ones. The former predict what consumers actually do, while the latter describe what rational consumer should do. Thaler's explicit aim in 1980 was to improve the descriptive models by revising them in the light of the recent experimental evidence: "exclusive reliance on the normative theory leads economists to make systematic, predictable errors in describing or forecasting consumer choice" (39). Importantly, he left the normative model untouched, in effect asserting that the standard economic models of choice were normatively valid. The thus-opened chasm between descriptive and normative models led behavioral scientists to think about ways to lead people back from how they actually behave to how they should behave, and hence provided both motivation and justification for behavioral interventions. This was a new role for decision theory—as long as its models were considered both descriptively and normatively adequate at the same time, this question simply did not arise.

With the normative-descriptive distinction also came renewed worries about the justification of normative models. This is somewhat curious, as neither Thaler nor his colleagues changed anything in the normative models. Yet the new role that came with the distinction made the question of normative validity much more pressing, and so it has remained until today.

As mentioned above, in standard economics, choice under uncertainty is modelled according to the vNM expected utility model. This model is highly restrictive: it requires both an exhaustive set of mutually exclusive states, each to be part of an objective probability distribution, as well as a set of outcomes, each evaluated by a utility function. Economists had, long before vNM, distinguished between situations where states, outcomes, probabilities and utilities are known (called situations of risk) and where some or all of this information was missing (called situations of ignorance). vNM models are only applicable to situations of risk—in particular, because they require objective probabilities. For situations under ignorance, they do not provide a normative benchmark.

Economists, when faced with this restriction, will typically reply that *Bayesian decision theory* fills this gap. Bayesian theory does not require objective probabilities but instead assumes that probabilities are subjective epistemic attitudes. The normative significance of these

theories lies in their consistency mandates, cashed out as choices that can be represented by consistent utility and probability assignments. When behavior best fits a descriptive model deviating from these consistency requirements, Thaler and his colleagues speak of systematically irrational behavior.

There is a substantial and often technical literature on the normative validity of the standard model. Here are just three thoughts that worry me about it. First, models like prospect theory indicate inconsistency only if objective probabilities are available. As I mentioned above, prospect theory introduces a subjective weighting on the probabilities of outcomes, thus allowing that higher probabilities are overweighted relative to smaller probabilities. If objective probabilities are available (for instance, as frequencies of numbers coming up on a roulette wheel), the subjective weights can be interpreted as cognitive mistakes: the deliberating agent distorts the available information in a systematic way. However, if objective probabilities are not available, this distinction disappears. It is entirely unclear how to distinguish subjective weights on probabilities from *subjective* probabilities. Yet without this distinction, one cannot tell apart normatively correct and incorrect deliberation with the help of *Cumulative Prospect Theory*.

Second, although Bayesian theory can deal with a lack of objective probabilities, it has much greater problems dealing with lack of information about states and outcomes. In such cases of deep uncertainty, the standard model is not applicable as a normative benchmark. Yet arguably, most questions of interest in macroeconomics of finance are riddled with such deep uncertainty—so that exactly here, economists lack a normative standard.

It should, finally, be noted that the founder of modern Bayesianism, Jimmy Savage, was much more careful about the applicability of his theory than many of his followers. He argued that his theory required “artificially confining attention to so small a world that the [expected utility] principle can be applied here”, but also admitted that he was “unable to formulate criteria for selecting these small worlds and indeed believe[d] that their selection may be a matter of judgment and experience about which it is impossible to enunciate complete and sharply defined general principles” (Savage 1954, 16). Yet clearly, which small world is selected to represent a grand-world problem will affect consistency judgments about that grand world—so that without reasons

for selecting it, no small world model in itself can claim to be a rational benchmark for any real, grand-world problem.

As long as one stays in the model world (and to some extent in the behavioral laboratory), these issues don't really arise. Models and many lab settings are the closest we have to Savage's small worlds. The question is whether a given model or experiment is an appropriate small-world representation of a grand-world problem from macroeconomics, finance, or many other walks of life. Yet if these standards of appropriateness cannot be completely and sharply enunciated, then most of the normative claims professed by behavioral economists lose their teeth. Unless behavioral economists were willing to restrict themselves to judging abstract model situations or designing laboratory scenarios (which I am sure they are not), they would have to admit that the normative standards they are using are much less powerful than often claimed.

## 5. DESIGNING NUDGES

The committee also noted Thaler's key role in defining the *nudge* concept and proposing a host of behavioral interventions to improve people's welfare. This contribution is a direct consequence of the conceptual distinction between descriptive and normative models, and draws both motivation and justification from it. Already in the 1980s, Thaler addressed public policy questions regarding consumer choice, retirement savings, and stock market investments. In collaboration with legal scholar Cass Sunstein this cumulated in Thaler's probably most popular book *Nudge: Improving Decisions about Health, Wealth, and Happiness* (2008), which proposes to use knowledge of psychological biases in order to influence people's decisions for their benefit.

Critics of the nudge approach often point out that it contains really nothing new—those kinds of strategies had been known for a long time and found countless applications in advertisement and marketing departments of this world. Yet this overlooks that Thaler and Sunstein's central contribution is conceptual, not in the development of any particular new strategies. What is indeed novel about their approach is (i) that it proposes to use these strategies for the benefit of those influenced, (ii) that it co-opts the identified biases in order to exert this benevolent influence, and (iii) that it claims to be compatible with liberal principles, first because it aims to improve people's behavior according

to their own evaluation, and second because people can always opt out of these interventions.

Philosophers and many social scientists have eagerly seized on the various ethical questions that such a proposal raises, including what it means to improve people's behavior according to their own evaluation, whether it is manipulative, whether it is non-transparent and whether people like being nudged or not. I have surveyed this discussion elsewhere (Barton and Grüne-Yanoff 2015).

Instead, I want to briefly mention a few worries about how to evaluate the effectiveness of nudges. One problem for many nudge interventions is that the only evidence in their favor is the effect size from experiments in a specific laboratory or field setting. This evidence says little about whether these interventions can be transferred to some other setting. For this, evidence for the mechanisms through which the interventions operate is required (Grüne-Yanoff 2016). Yet Thaler and other behavioral economists have largely eschewed investigating such mechanisms.

Mechanistic evidence is important because mechanisms help understand which conditions must be in place for a policy intervention to be effective. For example, a nudge like Thaler's *Save More Tomorrow* (Thaler and Benartzi 2004) will be effective if most people's discounting function is hyperbolic in shape, and doesn't change form under the intervention. If however people have widely differing discount functions, or their discount functions are not stable under intervention, then such a nudge is unlikely to be effective. Consequently, one should pay more attention to the mechanisms through which nudges operate, and use this knowledge to select those situations in which nudges are most likely to be effective (Grüne-Yanoff et al. in press). Furthermore, once one pays more attention to mechanisms, it will also become clearer that nudges are not the only kind of behavioral interventions, but just one amongst many (Hertwig and Grüne-Yanoff 2017). Thus, while the nudge proposal has opened new conceptual avenues for policy-science interaction, the underlying behavioral evidence is still far from providing solid answers to questions like what kind of causal pathways these interventions take, how effective they will be in different environments, and how to systematically design new interventions.

## 6. CONCLUSION

Robert Shiller has called Richard Thaler “a controversial Nobel prize winner—but a deserving one”. I agree with that appraisal. In my commentary here, I hope to have demonstrated that the controversy is also for the philosophers and methodologists of economics to engage in: plenty of the achievements that Thaler has been praised for—including interdisciplinary integration, realistic assumptions, explanation vs. description, normative implications of cognitive limitations, and policy design—raise yet unresolved methodological and foundational questions. I sometimes wondered while writing this commentary “perhaps the award motivation even contains a subliminal message?”—although I doubt that the Economic Sciences Prize Committee did this on purpose. Whatever their intention, I hope that philosophers and methodologists of economics make good use of this renewed focus, pick up the message and engage with these fascinating questions.

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**Till Grüne-Yanoff** is professor of philosophy at the Royal Institute of Technology (KTH) in Stockholm. He is associated with the Academy of Finland Centre for the Philosophy of the Social Sciences (TINT) at the University of Helsinki, and the Max Planck Institute of Human Development in Berlin. His research focuses on philosophy of science, decision theory, and the relation of science and policy-making.

Contact e-mail: <[gryne@kth.se](mailto:gryne@kth.se)>

Website: <<https://people.kth.se/~gryne/>>