

Margaret Schabas, University of British Columbia

1. Introduction

To an outsider, contemporary economics might seem like one big thought experiment. The perennial joke about three scientists stranded on a desert island, with only a can of food, and the economist naively offering the supposition of a can opener, captures well the proclivity of economists to enter into an imaginary world. Current economic discourse consists mostly of models, which in turn invoke stylized abstractions, idealizations and *ceteris paribus* conditions. Moreover, economists privilege the inner workings of the human mind. The reason markets purportedly work is because we can extrapolate to other minds and impute a similar instrumental rationality. For these reasons, one might expect enthusiastic appeals to the laboratory of the mind. I will argue here, however, that thought experiments, as distinct from models, are uncommon in economics, past or present. Paul Krugman (2011) has reached a similar verdict, although his definition of a thought experiment is broader than mine.ⁱ

There are some philosophers, Julian Reiss most prominently, who take a different view on the matter. After noting that mainstream economics is “dominated by mathematical model building”, he goes on to say that “I am going to look at a practice that is both older and, arguably, more widely used in economics than mathematical modelling: thought experimentation” (Reiss 2013, p. 177). Mary Morgan, Uskali Mäki, and Robert Sugden have each, independently, characterized the mental manipulation of models as thought experiments and thus implied their extensive appeal (Morgan 2002; Mäki 2002; Sugden 2005). Most recently, Harro Maas (2014) has argued that both Thomas Robert Malthus and Paul Samuelson made use of thought experiments. I differ with these scholars. We manipulate ideas in our heads all the time, but that is not what counts as a thought experiment. In fact, among contemporary economists, thought experiments are used infrequently and even when the label is applied, as is the case for example in models of climate change, it is not done so properly (see e.g. Stern 2008; e.g. Mankiw 2013).

I argue here for the non-proliferation of thought experiments. I suggest we distinguish the mental manipulation of models, informal conjectures, exhibits, and long-term forecasting, from thought experiments. One missing ingredient, quite simply, is something akin to an experiment. As David Gooding (1992) argued, a thought experiment must resemble an experiment. Otherwise, it is something else, a demonstration, a train of thought, or a set of suppositions. A mathematical proof engages mental manipulation but it is not experimental. And I mean nothing more by the adjective “experimental” than the way it is commonly used in the natural sciences to distinguish those pursuits from the “theoretical”. If all mental manipulations of models by economists were to count as thought experiments, then it would be difficult to see what is excluded, especially in the science of economics where modelling is so prevalent. I thus do not share the move by some philosophers of science, such as Nancy Nersessian, who deliberately conflate mental models with thought experiments.ⁱⁱ

It is important to keep in mind that thought experiments are rare in the natural sciences. If this were not the case, if thought experiments but not laboratory experiments

were the dominant mode of inquiry, our sciences would have turned out very differently. As Kuhn (1977) points out, thought experiments tend to congregate at the time of revolutions, precisely because they dredge up deeply held convictions and position them in a new light.ⁱⁱⁱ In my efforts to delimit the identification of thought experiments in economics, I will conform to its more entrenched usage in the physical sciences, such as the paradigmatic experiments undertaken by Galileo and Einstein. For better or for worse, economists have long sought to emulate the epistemic standards set by physics; taking these as the exemplars is not distortionary.

There are some genuine cases of *gedankenexperiment* in economics, for example by David Hume and Ludwig von Mises, although neither used the term. But current appeals to thought experiments, as I will argue here, are often misnomers because there is nothing experimental to the line of reasoning. As Roy Sorensen has emphasized, a thought experiment “is an experiment that purports to achieve its aim without the benefit of execution” (Sorensen 1992, 205). The experiment is carried out in the laboratory of the mind, but mimics an experiment that might have been carried out in physical space-time, but at the time is infeasible. For that reason, Newton’s famous bucket experiment does not qualify as a thought experiment. Indeed, a careful reading of the *Principia* indicates that Newton is describing an actual experiment, albeit one from which he extracts a theoretical result (the existence of absolute motion).^{iv}

Conversely, Galileo’s thought experiment (1632) to demonstrate the uniform acceleration of falling bodies could not be carried out because there were no good clocks, nor a vacuum pump to remove the air resistance. Had he in fact attempted an experiment (say in Pisa), it would have refuted his law. By the 1660s, however, the first steps toward providing the apparatus necessary to measure “g” (9.8 meters/second-squared) were taken, by Huygens, who built one of the first mechanical clocks with a minute hand, and by von Guericke, who constructed an air pump to produce a particle vacuum. It remains an open question if all thought experiments are simply experiments-in-waiting, but the main point is not to lose sight of the fact that a thought experiment is still an experiment.

Economics has become more and more an experimental science. Of the 1665 mentions of “experiment” in articles in the *American Economic Review*, founded in 1911, the majority (1100 mentions) have appeared since 2006. Moreover, it can boast some crucial experiments, for example with public goods or rational choice theory. It may well be that, as economics increases its experimental reach by devising new tools, the need for thought experiments will diminish all the more.

Another important distinction between models and thought experiments draws upon the extent to which we can characterize thought experiments as “expeditions to possible worlds” (Sorensen 1992, p. 135). A thought experiment, unlike a model, is launched by a jarring, often bizarre counterfactual, but then restores some mental equanimity by introducing familiar objects to assist the mind of the experimenter as she reaches her destination. The initial counterfactual reorients the mind and dredges up some more deeply rooted beliefs that may prove contradictory to others. Were the thought experiment to pile one jarring counterfactual after another, it is unlikely that pre-existing intuitions could be availed upon to do the deliberative work at hand. Precisely because a given thought experiment arrives at a new understanding of the world without appealing to new empirical findings means it functions by realigning existing and familiar beliefs.

A model, by contrast, builds on what Robert Sugden (2005) has aptly called a “credible world”. A model aims to establish a structure and internal coherence to a world that one can inhabit, explore, and manipulate further. By living in that world for a period of time, the model-builder and her converts acquire new insights over time, ones that have potential analogues to our actual world. Mary Morgan has brought this out admirably in her recent book (2012). The model-builder is a colonizer, while the thought experimenter is a tourist.

2. Models versus Thought Experiments

In the nineteenth century, economists defined their science as a set of laws that govern the production and distribution of commodities. In the twentieth century, the list of laws grew considerably and, at its high-water mark circa 1970, economics had over one hundred laws or regularities (theorems, principles, identities, etc.) on the books. But it is now passé to search for laws. In fact, as Stephano Zamagni (1989) has observed, the number of laws has been shrinking quite steadily. Some laws now deceased were long-lived, Say’s Law or Walras’s Law, while others were meteoric, the Laffer curve or Phillip’s curve. The diminution of laws has been replaced by the construction of models. In the 1930s, the pioneering econometricians, Ragnar Frisch and Jan Tinbergen, transferred the idea of model building from physics to economics (Boumans 2005). Within a decade, models became the most common medium of analysis in economics. ^v

As several leading philosophers have emphasized, every proposition in a model is, strictly speaking, false (see Gibbard and Varian 1978; Hausman 1992). Models necessarily simplify and abstract and assume away extraneous noise. But the whole thrust of the model is to enlighten us about the world we inhabit and for this reason, the model-builder normally points to some empirical verifications for some of the derivations. Models, as Mary Morgan (2012) has argued, create a self-contained world. They enlighten us insofar as the structures they construct correspond to structures we believe hold true in our own world. There is, however, no formal proof to warrant these beliefs. Sugden (2000) has pointed out that the inductive leap between the model and our world is always left implicit, partly because it would undercut the rigor of the model to impose additional bridge conditions (that is, weaken the abstractions and generalizations). What makes for a good model is that each proposition is posited at the precise level of abstraction that the overall structure of the model can withstand and yet still offer new insights.

It is easy to conflate models with thought experiments; both feed on counterfactuals and abstractions, including the appeal to perfect or ideal conditions (although I would submit that thought experiments on average invoke fewer idealizations than models). Both rely heavily on a narrative that weaves a cogent argument. And both are devised with the aim of capturing one or more general truths about our actual world. Moreover, the degree of human intervention to move from a demonstration or observation to the status of an experiment is not always clear and is likely to vary from one science to the next. What counts as an experiment in botany may differ from what counts as an experiment in chemistry, for example. One need only think of the near-contemporaries Gregor Mendel and Dmitri Mendeleev to get a sense of the range.^{vi} For these reasons, I accept that thought experiments differ from models more by degree than by kind, and fall on a continuum. If thought experiments must be experimental to warrant the label, there will be borderline

cases. There are likely some models that are near cousins to thought experiments, and vice versa. One of these, Ricardo's analysis of mechanization, will be discussed below.

If we could obtain a metric for the journey the mind takes to that counterfactual world, then in both cases (models or thought experiments) we might have a better means of settling the distinction. Jon Elster (1978) struggled with this in his quest for the "optimal counterfactual" in doing social science. But the received view, from David Lewis (1973), is that we cannot find that metric. It must be left to judgment, and for that reason as well, it is preferable to construe models and thought experiments as falling on a continuum.

To carry out the return journey, thought experiments rely heavily on a narrative, arguably more than models, especially mathematical models where so much is left implicit. As Sorensen has noted, there are often particular details in the narrative of a given thought experiment that are of no obvious value to the final results. This is not true in models, for the most part, since the aim is to ascend to the highest possible level of abstraction and to create a self-sustaining structure that will prompt further applications. Models are not built with the intention of being used only once, although no doubt that is what happens in most cases. Thought experiments, by contrast, once they are devised to make their point, have no future shelf life; as Ian Hacking (1992) has pointed out, they are analytically stillborn.

To summarize thus far, to count as a thought experiment in economics there ought to be something experimental, something that manipulates and demonstrates. Unlike a model, thought experiments tend to be launched by a jarring counterfactual that transports the mind to a distant alternative world, then offers a narrative peppered with familiar details that facilitate a return journey on the part of the mind. Hume did this well when he supposed that every Briton awoke with an extra five pounds in his pocket, as a way to demonstrate that the interest rate was immune to inflation.

Friedman also carries out a thought experiment, much like Hume's. He supposes that helicopters drop \$1000 bills evenly distributed across the U.S.A. such that each person collects (all in an instant) just enough to double their money holdings, from 5.2 to 10.4 weeks of income. Friedman also needs to establish that this is a singular event, insofar as expectations of repetitions would undercut the result. He also rules out that the bizarre event upsets longstanding economic patterns of behaviour, such as the factors that undergird the demand for money holdings. Everything else stays the same, but the doubling of money holdings causes a series of subsequent readjustments to production and prices until the optimal quantity of money is restored.

The helicopter drop is a thought experiment because it meets the two criteria: it is launched with a bizarre counterfactual and it is an experiment. In fact, like the case of Galileo on uniform acceleration of falling bodies, it is an experiment-in-waiting. In 2008, a single nation-wide tax rebate was described as a "helicopter drop". American tax-filers received, by check or direct deposit, a cash injection ranging from \$300 to \$600. This Economic Stimulus Act did not meet many of the conditions stipulated by Friedman's thought experiment. It was not instantaneous, universal, or equitable; nor was it a singular event. There was an earlier rebate in 2001, for example. Nor did it use helicopters or \$1000 bills. But like many other classic thought experiments in physics, say the measurement of muon decay in particle accelerators to demonstrate Einstein's analysis of time dilation, there is a clear progression toward actual experiments.

One reason thought experiments might not be common in economics is that, as Kuhn recognized, thought experiments tend to come in the wake of a crisis. Economics has had its upheavals; two even fit well the model offered by a Kuhnian revolution, namely the Marginal Revolution of the 1870s, and the Keynesian Revolution of the 1930s. But there has been nothing comparable since then; nor did I find thought experiments to coincide with those upheavals. Many key terms were redefined or given a different priority, utility, capital, or the demand for money. None of those terms were novel, however.

Apart from the thought experiments of Hume and Friedman, there are not many more clear-cut cases to be found. As Julian Reiss has pointed out, Ludwig von Mises offers a thought experiment with his account of the “evenly rotating economy” (von Mises 2007, pp. 244-250). He constructs this as a contrast to the economy in a “state of rest”; the latter is one in which prices settle dynamically after a series of exchanges. In the rotating economy, both time and market fluctuations are eliminated. It is an “imaginary construction” that cannot ever happen in “reality” (von Mises 2007, p. 247). Because there is no uncertainty, there is also no choice. As a result, he announces “such a rigid system is not peopled with living men making choices and liable to error; it is a world of soulless unthinking automatons; it is not a human society, it is an ant hill” (von Mises 2007, p. 248). He then submits that there is no place for money in this imaginary economy, since money is necessarily dynamic. At best, money serves the purpose of a *numeraire*, that is, a unit of account. Von Mises has created a thought experiment of sorts, but not one, it seems, that had much influence on subsequent discourse. Most of the Austrian economists favoured armchair economics, and were thus engaged in long ruminations that might seem at times to resemble thought experiments. Reiss also describes Carl Menger’s account of the origins of money as a thought experiment, but in my view it is just a narrative, and lacks the critical ingredient of an experiment. Narratives about the origin of money are found throughout the history of economics and are essentially conjectural history used to motivate current theoretical claims regarding the form and function of money.

The same holds, I believe, for the work of cliometricians. Reiss has characterized Robert Fogel’s famous book, *Railroads and American Economic Growth* (1964) as offering an example of a genealogical thought experiment. Fogel considered in great numerical detail the difference in economic growth that would have ensued up to 1890 if America had not adopted the railroad, and reaches the conclusion that it would not have differed by more than five per cent. Hence, it was wrong of historians to deem the advent of the railroad a critical factor in American economic expansion. Cliometrics thrives on counterfactuals, but so do many historians. Indeed, I slipped one in near the start of this chapter, conjecturing an alternative history of the natural sciences dominated by thought experiments rather than actual experiments. Although history hovers in the penumbra of science, it seems yet to become an experimental science. The delay or absence altogether of the railway is also not an outlandish counterfactual. The genesis of human inventions is replete with contingencies, and there is nothing to strain credulity that removes a particular invention from our historical landscape. Cliometrics is better characterized as a project to verify mainstream economics using extensive data-gathering and econometric methods (see Schabas 1995).

3. Ricardo on Machinery: An Intermediate Case

Let me turn now to an example of what strikes me as an intermediate case between the thought experiment and the model. In his third and last edition of the *Principles of Political Economy and Taxation* (1821), Ricardo famously inserted Chapter Thirty-One entitled "On Machinery." It challenged the widespread belief that Ricardo confessed to have previously held, that new machinery caused only a temporary decrease in employment (frictional unemployment). Because the new machinery would reduce the cost of production for that particular commodity, it would thereby free up capital that would shift to another sector and promptly restore employment to the same prior level. Moreover, everyone would benefit because the good produced by the new machinery was now cheaper.

To overturn this received view, Ricardo devises a case whereby the return on capital (profit-rate) is the same across the entire economy (ten per cent) but the ratio of fixed to variable capital due to mechanization suddenly increases and thus renders a group of workers permanently redundant. He believes that it is entirely possible that the capitalist would be content with the same aggregate return and have no reason to reinvest the hitherto sum of variable capital (wages fund) in a new sector. Ricardo then claims that his analysis is borne out by recent labour disruptions, specifically the Peterloo Massacre of 1819, and that he is "convinced that the substitution of machinery for human labour, is often very injurious to the interests of the class of labourers" (Ricardo 1951, p. 388).

To motivate his argument, however, there is one critical step, namely that the "improved machinery is *suddenly* discovered and extensively used" (Ricardo 1951, p. 395). Ricardo suggests this may happen in the production of cotton (the power loom that the Luddites attempted to suppress), but he then weakens his stand and suggests that in fact there may never have been such a case. "The truth is, that these discoveries [of new machinery] are gradual, and rather operate in determining the employment of the capital which is saved and accumulated than in diverting capital from its actual employment" (ibid.).

Is this a thought experiment? Well, Ricardo admits that his sudden intervention of a new machine that is extensively used and funded not by credit but by an existing wages fund, strains credulity. Early in his text he acknowledged a proposition established by Adam Smith and others that most capital accumulation was funded by credit and not by accumulated savings. Ricardo must also limit the commodity the machine produces to a wage good, that is, a good universally consumed by workers. In short, Ricardo spins a tall tale out of whole cloth. His account meets the two criteria of an experiment (a sudden intervention) and a jarring counterfactual. But he also wants to extract real world insights and suggest that recent strife between workers and factory owners are based on sound principles. Machines can permanently displace workers and do not simply produce frictional unemployment. The argument has many flaws and a sign, perhaps, that Ricardo had become too clever for his own good, but it does appear that it fits the example of a thought experiment more than a model.

But there are other aspects of the analysis that resemble a model. The entire argument depends on many stylized assumptions. There are three temporal periods during which everything adjusts instantaneously. The workers who are not employed in building the new machines in period two are able to continue working in the same traditional

manner, but it is not clear that this is possible (do the factories close or is the labour input cut in half uniformly?). And if the good produced by the new machine is and was a central commodity in the worker's consumption bundle, what provides for the shortfall during the transition? Everything else about the situation is stylized. There is perfect competition, full employment at a uniform subsistence wage, and a uniform profit rate. Capital moves all of a sudden from variable to fixed, and labour input is perfectly measured by money values. Ricardo's chapter on machinery, I believe, falls somewhere on the continuum between models and thought experiments, but closer to the model. It consists of a string of propositions that invoke idealizations or stylized facts rather than an expedition to an incredible world.

4. Misnomers

Harro Maas has recently classified Malthus's argument that agrarian output will fall drastically behind population growth as a thought experiment. In my view, however, there is nothing experimental about the argument. There is no manipulation. In fact, Malthus wants to emphasize that population growth is driven mostly by the sub-rational passions in the marriage bed and not by anything that is subject to our control (his main recommendation to reduce population is to postpone the age of marriage). Similarly, agrarian output is limited by the unrelenting scarcity of arable land and the principle of diminishing returns. Insofar as Malthus is projecting forward in time, his argument is clearly more inductive than many at the time, certainly compared to the strong deductive structure found in Ricardian economics. But he also makes a number of stylized assumptions at the start (the arithmetic progression of agrarian output for each period and the geometric progression of population growth). He also posits a quarter-century period and thus neatly projects ahead one century. But I do not see his demonstration of the increasing gap between population and food supply as thus qualifying as a thought experiment. There are countless renditions of the Malthusian argument in the form of a model (terse propositions and equations).^{vii}

Environmental economists now offer "thought experiments" when they propose models that project a century into the future, often in the context of global warming. Nathaniel O. Keohane (2009) offers what he calls "a simple thought experiment" in his efforts to compare the cap and trade policy with carbon taxes projected up to the year 2100. Martin L. Weitzman (1992) also uses thought experiments to motivate intuitions regarding biodiversity as well as address the problems of climate change. The point is that once one bites off any more than about five years, extant models are not easily applicable, if only because most of the capital has been consumed and the production possibility frontier would have shifted. The confidence intervals diverge and the standard models become unstable. Hence, environmental economists use the term thought experiments because they cannot construct formal models. But are they using the term correctly? I think not. There is nothing experimental about their train of thought; the term is used as a placeholder for informal modelling. Again, let us look to the natural sciences. Astronomy, geology and evolutionary biology use significantly greater temporal projections, to the past and the future, and yet do not employ the term thought experiment. The problem lies rather in the short time horizon adopted in mainstream economics.

The Ellsberg paradox is normally described as a thought experiment, both in the original offering and in the substantial journal literature that it spawned. It established the interesting result, now ascribed to us humans, of adversity to ambiguity. Ellsberg describes a series of hypothetical drawings of different coloured balls from different urns and some unexpected outcomes when the parameters are varied. But it seems preferable to deem Ellsberg's original account as simply a description of an experiment that he did not conduct, much like many examples in the theory of probability. Now, given sufficient research funding, Ellsberg's experiments and many variants on that theme have been instantiated in the laboratory (see Helevy 2007). They resemble the set of experiments conducted by Tversky and Kahneman (1981) that establish systematic deviations, under contrived conditions, from the central axioms of rational choice theory.

Robert Sugden (2005, p. 252) has defined these experiments as "exhibits". At their preliminary stages, he refers to them as thought experiments, particularly in the sense that they focus upon intuitions about such deviations. But insofar as they are carried out more often than not, it seems preferable to view them as accounts of experimental designs, not thought experiments. Furthermore, there is nothing strange or bizarre about these experiments at all. They use everyday objects, balls, bets, cash, or theatre tickets. They are, in short, about credible and not incredible worlds. Nor would I classify them as models, since they do not create an independent or robust structure based on a set of mildly counterfactual propositions. Rather, they demonstrate that prior theoretical assumptions about rationality may need to be revised. They *exhibit* a specific pattern or regularity of human agency.

5. Conclusion

I cannot possibly canvass the entire field of economics, but in my study of the leading journal, the *American Economic Review*, I found that the term "thought experiment" was mentioned 76 times, all in the past 20 years. Since some articles had several mentions, this is trimmed to at best 50 articles out of a total of approximately 2000 articles (for the past 20 years). This comes out as a negligible incidence of 0.025. Thought experiments exist in the mainstream literature, but are not prevalent.

I found one discernable pattern, however. About a dozen economists in the upper echelon of the profession invoke a thought experiment in their invited address to the American Economic Association (the Richard T. Ely annual lecture or the Presidential address). The list is an impressive one: Edmund Phelps (1995), Lawrence A. Summers (2000), Robert E. Lucas (2003), and Larry Epstein (2014), to name just a few. These articles are meant for a larger readership, and not subject to the normal peer review process. Each economist would introduce a hypothetical scenario, one that the author had not troubled to instantiate or put to test in a laboratory.

For example, Akerlof (in his 2007 Presidential address) writes: "In a thought experiment, consider a woman living on \$50,000 a year who learns that her uncle will die in one year leaving her \$2,000,000. Even if she has considerable savings in the bank, it would be unseemly for her to run down her savings in anticipation of the bequest. She is not *entitled* to do so. She should stick to spending from her current income. This gives another example in which norms regarding entitlements to spend are related to current income, in violation of the life-cycle hypothesis" (Akerlof 2007, p. 17). This is an account of

a hypothetical scenario, at best an exhibit like Ellsberg. There is nothing experimental about it, nor is there a jarring counterfactual. Such cases probably happen daily in the U.S.A., where there are more than ten million millionaires. Did Akerlof seek out sociological studies of recently-informed heirs? No, he used this so-called thought experiment to take a swipe at the life-cycle theory of consumption behaviour. He sought to show that the Keynesian view that consumption is sensitive to current and not just permanent income still holds a grain of truth.

No doubt there are other examples of thought experiments that have been subject to more rigorous refereeing than the sample I read in the *American Economic Review*. But most are similar to those offered by Akerlof. In another leading journal, Gregory Mankiw (2013) offers a thought experiment about a society with perfect economic equality. He then introduces Steve Jobs, J. K. Rowling, and Steven Spielberg as a way to justify those in the top one-percent income bracket. Again, there is no experiment present. We have a strong counterfactual but nothing that introduces variables via human agency.

A more plausible candidate is the one where Larry Summers considers a scenario of a bank such that “everyone would be paid off [in a bank run] as long as no more than one-third of the investors chose to withdraw” (Summers 2000, p. 7). Although he grants that there are several possible outcomes, he deems the likelihood of a bankruptcy improbable. He then remarks: “I think that this thought experiment captures something real. On the one hand, bank runs or their international analogues do happen. On the other hand, they are not driven by sunspots: their likelihood is driven and determined by the extent of fundamental weaknesses” (ibid).

Is this a thought experiment? Perhaps. Banks are predicated on the supposition that one-third of depositors will never withdraw on a given day. Summers also offers a more formal analysis of the situation to generate the various equilibria and, in that sense, he is devising a model where one of the parameters is highly unusual (a third of depositors withdraw, but credit is forthcoming such that the bank does not collapse). But these scenarios have indeed transpired. One of the reasons the Canadian banks survived so well after the 1929 crash is that they were in close proximity to one another. On a given day of panic, the Bank of Montreal manager would prevail upon the good graces of his neighbouring Toronto Dominion manager to provide the cash necessary for the sudden bank run, thus upholding faith in the banking system. The next day, somewhat like a potlatch ceremony, the TD manager could count on the BofM manager to assist him if he was subject to a run. As Summers concludes, “avoiding situations where the bankrun psychology takes hold, . . . will depend heavily on strengthening core institutions and other fundamentals” (ibid). I think he uses the term “thought experiment” to glide quickly over a point that might have been fleshed out with historical support.

Thought experiments that meet the criteria I have proposed here are uncommon in economics. The term is used very infrequently in mainstream economics and, in my view, such references are often misnomers. This may be partly due to the fact that economics has only recently embraced experimentation on a significant scale, or because models are such a dominant method. It seems advisable to keep models distinct from thought experiments while granting that there will be intermediate cases. But, like Hume, I do not wish for anything given here to commit the naturalistic fallacy; if economists have not used thought experiments very often that does not preclude their using them more frequently in the

future. Krugman (2011) thinks this would be a beneficial turn of events. And maybe tourism is a better outcome than colonization.

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ⁱ There is very little literature on the subject. A recent editorial by Paul Krugman (2011) is predicated on the view that they are not commonplace. He defines the thought experiment as a model that uses an informal economic argument rather than one with mathematical equations or econometric analyses. Krugman views Hume's monetary thought experiments as the standard, and adds to these a few more examples, a study of a recent babysitting co-op (Sweeney and Sweeney) and the more famous case of the use of cigarettes in POW camps in World War II. They fit his definition but not mine. Another example that Krugman cites, by Karl Smith, uses imaginary gnomes switching people about overnight to challenge standard market assumptions. This example counts; there is a strong counterfactual to launch the experiment, and a follow up using familiar this-world details, but the example is only on the web; it is not a refereed publication.

ⁱⁱ This not a new claim; Nersessian (1992) also characterizes thought experiments as mental modeling.

ⁱⁱⁱ T.S. Kuhn pointed out that thought experiments undertake conceptual reform and thus sort out a misfit between familiar concepts and the world (Kuhn 1977, p. 264).

^{iv} According to Newton (1687, p. 10), the water "will ascend to the sides of the vessel, forming itself into a concave figure (as I have experienced)".

^v There are rare instances of models in economic discourse before the postwar explosion, although no one used that label. Good examples are found in the work of Jean-Francois Melon (1735), Francois Quesnay (1757), and Johann von Thünen (1826) (see Morgan 2014). But just as there were instances of decision theory and game theory before the twentieth century (e.g. Blaise Pascal, Daniel Bernoulli, Condorcet, and A.A. Cournot), it is only in the postwar period that models, game theory, and decision theory became widespread.

^{vi} Mendel's breeding of pea plants is commonly referred to as an experiment, but such practices were rare in botany at the time in contrast to chemistry. Most breeders were not conducting scientific experiments. Lorraine Daston (2011) offers an excellent account of the gradual disambiguation of observation from experiment in early modern science.

^{vii} Several are offered in the Norton anthology on Malthus (2004).