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CENTRE FOR DECISION RESEARCH & EXPERIMENTAL ECONOMICS

Discussion Paper No. 2020-16

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Is Economics An Experimental Science? A Textbook Perspective

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Abstract

Traditionally, students of economics have often been told that it is a non-experimental science. Using a quantitative and qualitative analysis of introductory economics textbooks, we track the historical evolution of this rhetoric from 1970 to the present day. We find that anti-experimental rhetoric was dominant and largely unchanged prior to the turn of the 21st century. Since then, there has been a growing trend towards textbooks making positive statements about the role of experimentation in economics. Remarks that experiments are impossible in economics have been (almost) eliminated only this decade, evidencing a sluggish change in rhetoric. We outline the evolution of statements over revised editions of influential textbooks and show that, while most have become considerably more supportive of experimental economics, there is substantial variation over when this happened. Interviews with key textbook authors confirm the historical trend of increased enthusiasm towards experiments, and suggest they are now accepted within the economic mainstream. Our results hold important implications for the research methodology of our science and how it is understood by current and former students.

Keywords: Economic Methodology; Experimental Science; Economics Principles Textbooks; History of Economic Thought

JEL Codes: **A20** – Economic Education and Teaching of Economics – General; **B29** – History of Economic Thought since 1925 – Other; **B41** – Economic Methodology; **C90** – Design of Experiments – General;

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The debate as to whether economics is an experimental science has a long, rich history with both pessimism and enthusiasm. Historically the anti-experimental rhetoric held sway (Mill, 1836; Friedman, 1953), with even the most ardent non-conforming economist believing experiments in economics to be ‘almost impossible’ (Boulding, 1941). However, having long operated on the margins of the discipline (Plott, 1991; Smith, 1991), experimental economics seems in recent decades to have received greater acceptance. This is anecdotally evidenced through the increased number of experimental papers published in mainstream journals, establishment of *Journal of Economic Literature* codes to classify experimental work, development of specialised courses in experimental economics, and – perhaps the greatest recognition of all - through several winners of the *Nobel Memorial Prize in Economic Sciences* for their pioneering use of experiments in economic research (Starmer, 1999b; Noussair, 2011; Colman and Pulford, 2015; Jacquemet and L’Haridon, 2018). Yet, pessimism and anti-experimental rhetoric can still be found within this more recent period (Siakantaris, 2000; Sims, 2010).

Whether economics is – or should be – an experimental science has been addressed via discourse, with philosophical arguments, sometimes supported by specific examples and simple pieces of evidence, presented in support of or opposition to the application of experimental methods to economic problems (e.g. Friedman, 1953; Plott, 1991; Starmer, 1999a, 1999b; Siakantaris, 2000; Guala, 2002; Bergstrom, 2003; Levitt and List, 2007; Croson and Gächter, 2010). It is interesting to note that several such discourses (Starmer, 1999b; List, 2011; Serra, 2012; Jacquemet and L’Haridon, 2018) feature quotations from classic textbooks – Paul Samuelson’s *Economics* or Richard Lipsey’s *An Introduction to Positive Economics* (both later co-authored) – as means to evidence the historical scepticism towards experiments. The quoted passages highlight the impossibility or extreme difficulty of doing economic experiments, unlike

in the natural sciences, and are presented as representative of widely held contemporaneous opinion within the economic discipline.

This paper approaches the debate in a way that is unique within this literature, yet inspired by this tradition of capturing the prevailing views of the profession through the writings of its introductory textbooks. Our contribution is to add quantitative – as well as qualitative – analysis to the question of how economists perceive the role experimentation, and how these perceptions have changed over the last 50 years. This we have done through construction of a very extensive sample of 278 introductory economics textbooks published during this period, and systematic examination of the statements made in these books about experiments. Through this approach, we can answer: how have introductory economics texts viewed economics with respect to experimentation? What are the varieties of rhetoric or statements about experiments that can be documented in introductory texts? Were the textbooks of previous decades, in general, as dismissive of experiments as the oft-cited examples of Samuelson and Lipsey? Has this rhetoric changed over time and, if so, when? Do the types of statements made vary by author characteristics such as country of origin, sole/co-authorship, or research activeness?

Under the mild assumption that textbooks *are* shaped by prevailing opinion, our novel approach offers insight as to whether economics really has shifted, in the eyes of its practitioners, from a non-experimental to an experimental science, thus bringing hard evidence to bear on a question previously covered mainly by methodological discourse. How experimentation is (or has been) viewed by economists is important, in part because of the implications for how vast streams of economic research are (or have been) perceived. The clearest example is behavioural economics, whose rapid rise has also helped usher in related sub-fields such as behavioural game theory, behavioural finance and neuro-economics. Most of the theoretical advances in these areas

have come from experiments, such that behavioural economics is intrinsically linked to experimental economics (Dhami, 2016). For example, psychologically-based theories of risky decision-making such as Prospect Theory and models of other-regarding preferences (to explain pro-social behaviour) have both been informed to a very large extent by experimental evidence. The extent to which one takes the ideas of behavioural economics seriously is inescapably associated with one's attitude towards the validity and usefulness of economic experiments.

We are aware that our titular question might surprise some readers. A very large number of current economists have made careers solely out of experimental research; several well-ranked economic journals publish little else. It is not just behavioural economics that has leant heavily on experiments, but also fields such as development and poverty alleviation (Banerjee & Duflo, 2011) and public economics (see e.g. Luca and Bazerman, 2020). And, indeed, most of the aforementioned discourse literature in recent years has been written from a pro-experimental position. Is it not then self-evident that economics is now an experimental science? We argue not. Much has been written about the increased fragmentation of the economic 'mainstream' (e.g. Colander et al., 2004; Hargreaves Heap, 2013; Cedrini and Fontana, 2018); thus it appears quite plausible that streams of research could receive publication, including in the most highly-reputed economic journals, even if based on methods whose validity would be rejected by the majority of economists.¹ Most papers in support of experimental methods are written by experimental economists themselves. Anecdotally, our experience of interacting with fellow economists suggests a low-level scepticism towards experiments might remain. And, while such scepticism

¹ As noted by Hargreaves Heap (2013), increased specialisation within economics 'might explain how 'discomforting' results in one part of the *American Economic Review* (AER), so to speak, don't trouble those who publish in other parts of the AER.'

may be voiced more in departmental coffee lounges than in the economic literature, as recently as 2010 the *Journal of Economic Perspectives* published an article entitled ‘But Economics Is Not an Experimental Science’ (Sims, 2010).²

Our analysis, moreover, has implications that go beyond the domain of economic research, and reach also into teaching and learning. Textbooks do not just reflect economic thinking, but also influence it. How economic science is portrayed in the textbooks used for Econ 101 is likely to mould how it is perceived by future business and political leaders, and future economists within industry and academia (as well as some of the current instructors of Econ 101). It has also been argued that there can be ‘enormous’ time lags between the development of ideas in economic research and their introduction into principles textbooks, and that these lags may be even longer for fundamental issues such as the methods of empirical discovery (Colander et al., 2004). Therefore, even if one argues that economics is widely perceived today as an experimental science within its research community, very different perceptions might be held by new or old graduates, many of whom might define themselves in some way as ‘economists’, or by those who teach economics but are not involved in producing research. Our findings can shed light on these questions.

This paper’s quantitative analysis, outlined in detail below, shows that anti-experimental rhetoric was indeed the textbook norm in the 1970s, the earliest decade our dataset includes. Little changed until approximately the turn of the 21st century, but since then the language towards experimentation has become strikingly more positive. Given that the uptick in experimental

² It is interesting to note a historical parallel between the experimental debate and earlier methodological exchanges focusing on the analysis of secondary data through econometrics, in which similar expressions of conflicting pessimism and enthusiasm were voiced, most notably in the well documented Keynes-Tinbergen debate (see Morgan, 1990). Recent years have seen a different kind of methodological debate, as exemplified by the *Journal of Economic Perspectives* Spring 2010 Symposium on ‘Con Out of Economics’.

research began rather earlier, the data suggests there was indeed a long lag before these methodological developments came to be reflected in the textbooks. As evidence of this sluggish change in rhetoric, we find that in the 2000s it was still quite common for textbooks to define economic experiments as impossible, but by the 2010s such statements had almost completely disappeared.

To supplement our quantitative exploration, we qualitatively analyse several particularly influential textbooks which were repeatedly revised over the period covered. We identify specific changes to the authors' statements about experiments, and the dates they occurred. We also conducted interviews with several of these prominent authors to establish their current and former opinions about experimentation, and situate whether and when their views have changed. Taken together, these investigations largely support the quantitative evidence in terms of nature and timing of changes in perspective (with some exceptions: Paul Samuelson's text was ahead of the curve). The universally positive attitudes of all interviewed authors cement the impression that experimental economics is, today, accepted within the mainstream.

Before proceeding, we note two additional strands of literature that our paper connects to. One comprises research analysing the contents of economics principles textbooks (e.g. Stiglitz, 1988; Feiner, 1993; Skousen, 1997; Robson, 2001; Giedeman and Lowen, 2008; Lopus and Paringer, 2011; Green, 2012; Madsen, 2013; Zuidhof, 2014; Kovzik and Johnson, 2016). The other (somewhat overlapping) literature discusses what students *should* be taught in introductory economics textbooks and courses, and also how they should be taught it (e.g. Bartlett, 1996; Krueger, 2001; Balaban et al., 2016; Røpke, 2020; Mankiw, 2020b). It has been argued that introductory economics should include more focus on psychologically-grounded models of behaviour which depart from the traditional rational agent (Bowles and Carlin, 2020). Moreover,

it has been argued that the best pedagogical method of conveying behavioural insights to students is through classroom experiments that allow students to experience decision-making (Holt, 2019). A world in which the Econ 101 textbooks tell students there are no experiments in economics would not appear conducive for either of these changes to occur. This paper traces the distance that the textbook literature has now moved away from such a position.

Textbooks as Evidence

Constructing the database

To study the place of experiments in economic science we analyse introductory economics textbooks from the years 1970-2019. To this end, we first identified a large sample of applicable textbooks, and then systematically analysed any coverage of experimentation in the introductory section of each text. Focusing specifically on the introductory sections provided the dual benefits of allowing us to accurately categorise every textbook and to do so within a large sample; it would not have been practicable to conduct close reading on the full text of hundreds of books, but we could do this specifically for their introductory sections. We argue that these opening pages comprise the most natural and fruitful section in which to inspect the authors' positions towards experiments. There is, on the whole, remarkable consistency in the structure of an economic principles textbook's introduction. Books tend to start by defining what economics is, leading on to an account of its methods. Here, authors will often talk about the type of empirical methods used – or not used – by economists, sometimes with comparison to other sciences. Through analysis of the introductory sections, we were able to classify the arguments, if any, made by authors about economic experiments, using categorisation criteria that were consistent across the full sample.

We constructed our database through a careful search protocol. First, to allow breadth and depth to our dataset, the national library of the United Kingdom, namely the *British Library*, was searched. As a legal deposit library, it is supposed to automatically receive a copy of every book produced in the UK and Ireland, and also possesses a substantial volume of international titles. We identified textbooks by searching for (1) “economics” and (2) “Principles of economics”, restricting the publication date to between 1970 and 2019. For (1), we selected all books with this name, except for those which upon inspection turned out not to be textbooks (e.g. books consisting solely of exercises). For (2), we selected all textbooks with this exact name, as well as others turned up by the search whose names clearly indicated they were introductory economics textbooks (e.g. “Survey of economic principles”, “An introduction to modern economics”). We did not select any books which indicated they were focused on a subfield of economics, or labelled themselves as “advanced” or “intermediate”.³

Secondly, we considered it important to comprehensively include certain textbook series which we believe to have been highly influential. Specifically, we selected all editions, excluding those published pre-1970, of the following series: *Economics* by Paul Samuelson and (in later editions) William Nordhaus; *Economics* by Campbell McConnell and (later) co-authors; *Principles of Economics* by Gregory Mankiw; *An Introduction to Positive Economics* (subsequently renamed as *Principles of Economics* and then *Economics*) by Richard Lipsey and (later) Alec Chrystal; and *Economics* by John Sloman and (later) co-authors.⁴ Samuelson’s text is particularly famous and its longstanding popularity and influence is often cited (e.g. Skousen, 1997; Lopus and Paringer, 2011; Bowles and Carlin, 2020; Giraud, 2020; Mankiw, 2020a), while

³ We ended our “Principles of economics” search after the 510th result, by which point the search had long since stopped returning any titles of introductory economics books.

⁴ Most editions of these textbooks already appeared through our *British Library* search, but where they did not we sourced copies from elsewhere.

more recently McConnell and, later, Mankiw have dominated the American market (Lopus and Paringer, 2011; Mankiw, 2020a). Lipsey's textbook was widely used at universities in the UK and Commonwealth countries in previous decades (Giraud, 2020), while Sloman's is the current market leader in the UK, according to the Open Syllabus Project⁵.

Finally, our sample includes items we identified in a pilot study of introductory economics textbooks in the library of our campus. Most of the textbooks analysed in the pilot would have found their way into our eventual dataset via the aforementioned search protocol, but the pilot added a further 15 textbooks which did not appear in our *British Library* search results or on our list of influential texts.

Having accessed the books, we carefully read the introductory section and identified any passages which discussed experimentation. Based on the statements, we then classified the book's position on economic experiments into one of five categories:

1. The book's introduction states that doing experiments in economics is impossible, or that economic experiments are never done (hereafter, *Impossible/Not Done*).
2. The book's introduction states that doing experiments in economics is difficult, or that economic experiments are rare (hereafter, *Difficult/Rare*).
3. The book's introduction states that experiments are done in economics, or refers to economic experiments without mentioning their being rare or any difficulties involved in conducting them (hereafter, *Done*).
4. The book's introduction does not mention experimentation in economics (hereafter, *No Mention*).
5. The book does not contain an introductory section (hereafter, *No Intro*).

⁵ This project counts the appearance of texts, by discipline, on publically available syllabi. We accessed the UK economics rankings in July 2020 from the following link: https://opensyllabus.org/result/country-field?id_country=GB&id_field=Economics. On the same date, the website listed Mankiw's *Principles of Economics* as the most popular introductory text on US syllabi.

We are mostly interested in the first three categories. The first is the most strongly anti-experimental and technically was inaccurate even in 1970, as experiments had already been done in economics by that time (see Smith, 1991). The second category conveys a more nuanced but still primarily sceptical view of experimentation, signalling to students that they are not going to need to worry much about this method in their economics classes. The third category represents a position that is more positive towards experiments, transmitting the message that they are a normal part of what economists do and that students should be aware of them. The main difference between the second and third categories is arguably one of rhetoric. Neglecting to mention any difficulties in running experiments does not necessarily imply that such difficulties do not exist, but it changes the tone markedly. Our main focus is, therefore, on the frequencies of each of these three categories and how they vary over time. It is, however, also of potential interest if we observe changes over time in the proportion of textbooks whose introductions mention experiments at all.

For each textbook, in addition to coding its position towards experiments, we also collected the following information: publication date; edition; number of authors; the home country of each author; and whether each author was an active economic researcher. From this information, we constructed explanatory variables. Home country was defined according to the country of the author's institution at the time the book was published, as identified either by information in the book itself or internet searches.⁶ We defined an author as an active researcher if they have written at least one research article, as revealed by searches on *Google Scholar* and *Repec*.

Our full dataset contains 278 textbooks, across 114 series; Table 1 contains summary statistics about the sample. The following sub-section reports a quantitative analysis on the frequencies of the textbook categorisations. This will explore the way the introductory textbook literature at large

⁶ If we could find no information about the author, we instead defined home country on the basis of the book's publisher.

has characterised economic experiments, and how this has changed over the years. In Appendix A, we report additional analyses conducted to address certain potential concerns about our empirical strategy, such as our inclusion of pilot data. There we also provide more information on our inclusion criteria, and outline our coding protocol for the few textbooks whose statements could not unambiguously be classified into one of our categories; the appendix also presents analyses exploring the sensitivity of our results to alternative coding approaches. With rare exceptions, the results reported in the main text do not change meaningfully in any of the additional analyses.

Table 1: Summary Statistics (N=278)

Publication year	1970-1979	1980-1989	1990-1999	2000-2009	2010-2019
	15.1%	20.9%	16.6%	21.2%	26.3%
Country of author(s)	US Only	US + Others	UK Only	UK + Others	Other
	56.5%	10.1%	19.8%	7.2%	10.4%
Edition	First		Later		
	24.8%		75.2%		
Number of authors	Sole authored		Co-authored		
	43.2%		56.8%		
Authorship included active researchers	Yes		No		
	87.1%		12.9%		

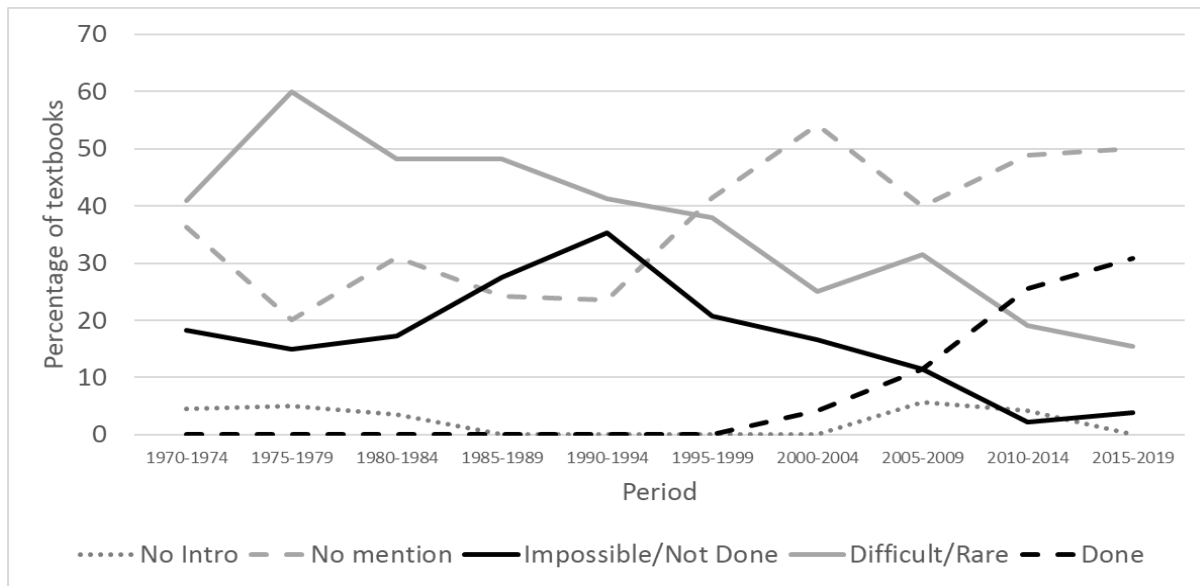
Note: Country of authors(s) sums to more than 100% because of overlap between US + Others and UK + Others categories.

Evidencing the rhetoric

We first present a visual overview of the textbooks' categorisation, and how this has shifted over time. We divide the dataset into ten five-year periods, and within each period calculate the percentage of textbooks falling into each category. This is displayed in Figure 1.

As expected, anti-experimental rhetoric, represented by the *Impossible/Not Done* and *Difficult/Rare* categories, dominated in earlier time periods. Interestingly, however, the data shows that although statements about experiments being impossible or not done in economics were common in the earlier decades, they never represented the most common category. The percentage of textbooks in our sample declaring this view peaked at 35% in 1990-1994. More often, the books instead characterised economic experiments as difficult or rare. This position was most popular in the 1970s – reaching 60% of our sample in the second half of the decade – but has been in fairly steady decline since. This provides a nuanced understanding of the narrative, somewhat distinct from the extant discourse which has at times portrayed earlier views as having been harsher towards economic experiments, branding them as outright impossible (see Jacquemet and L'Haridon, 2018).

Figure 1: Categorisation of textbooks over time



Still, the pessimistic view about experimentation in earlier periods is inescapable. This, likely, was a view inherited from much longer ago. As far back as 1836, John Stuart Mill remarked that the moral (social) sciences are distinct from the physical (natural) sciences in that it is seldom possible to conduct experiments (Mill, 1836). Even the non-mainstream and inter-disciplinary economist, Kenneth Boulding, opined in his 1941 textbook *Economic Analysis* that it seems ‘clear at the outset that the experimental method, so fruitful in the natural sciences, is of strictly limited application in the social sciences’ (p.11), and that because the economist, unlike the chemist, cannot enact preconceived events in a highly simplified environment, experiment in economics is almost impossible (Boulding, 1941).⁷ In his *Essays in Positive Economics*, Milton Friedman also concurred that we cannot test predictions in economics using controlled experiments (Friedman, 1953).

⁷ Boulding’s 1941 textbook was published before the first (1948) edition of *Economics* by Samuelson and featured a whole section discussing ‘methods of economic analysis’. What makes this anti-experimental view interesting and perhaps surprising for some is that Boulding was well-known for his unconventional work in economics and criticism of the mainstream, and some of his contributions have drawn parallels to behavioural economics – for instance, Boulding’s concept of ‘The Image’ has been linked to the modern idea of ‘framing’ (Kesting, 2017). We therefore find pessimism about economic experimentation from someone who viewed economics as a behavioural science *avant la lettre*, despite the later contribution of experiments in the development of behavioural economics.

However, this scepticism seems to have gradually declined over time. Indeed, in 2015-2019, only 15% of the books came under the *Difficult/Rare* categorisation, while the *Impossible/Not Done* characterisation also lost ground from the late 1990s and, despite maintaining a certain prominence until quite recently, has now almost disappeared. In the most recent decade, only 2 of the 73 textbooks we analysed maintained that experiments in economics are impossible or never occur.

In contrast, textbooks have increasingly been communicating that economic experiments are done, without mentioning their rarity or difficulty. No textbook in our dataset could be categorised as conveying such a message until 2002, when it appeared in O’Sullivan and Sheffrin’s *Survey of Economics: Principles and Tools*. The *Done* classification has become more prominent in each five-year period since, comprising 31% of the sample in 2015-2019. It overtook both *Impossible/Not Done* and *Difficult/Rare* (and the combination of the two) in the years 2010-2014. It is interesting to note that the emergence of this pro-experimental rhetoric seems to have lagged some way behind its appearance in the discourse literature. For example, in an aptly titled article ‘Will Economics Become an Experimental Science?’ in 1991, Charles Plott answered this question in the affirmative. Three years earlier still, future Nobel Laureate Al Roth had endorsed experimentation in economics as a ‘means for investigating many kinds of economic phenomena’ (Roth, 1988, p. 974).

To address the significance in the category changes over time, we ran a set of logistic regressions. Tables 2 and 3 report the results, with separate models corresponding to different dependent variables, which in each case are discrete identifiers of the defined categories (we do not run a model with *No Intro* as the dependent variable, and exclude books without introductory sections from the data for the other models). The independent variable *Year* represents a textbook’s

publication date, and therefore tests for a significant change over time in the likelihood of a textbook falling within the given category.

Table 2: Logit regressions: 1970-2019

	Dependent variables			
	(Impossible/ Not Done)	(Difficult/ Rare)	(Done)	(No Mention)
Year	-0.027 (0.018)	-0.049** (0.020)	0.201*** (0.057)	0.029* (0.017)
US Only	0.235 (0.631)	-0.667 (0.502)	-1.117 (0.822)	0.777* (0.437)
First Edition	-0.070 (0.652)	-0.014 (0.419)	-1.163 (1.107)	0.182 (0.351)
Sole Author	0.643 (0.415)	-0.568 (0.564)	-0.284 (0.887)	0.180 (0.447)
Researcher	-1.129 (0.707)	1.564** (0.639)	0.729 (1.016)	-0.664 (0.595)
Constant	52.312 (35.470)	96.139** (40.575)	-406.195*** (114.687)	-58.678* (33.398)
Pseudo R ²	0.079	0.100	0.333	0.047
Observations	271	271	271	271

Note: *** p<0.01, ** p<0.05, * p<0.1; Coefficients are presented; in parentheses are robust standard errors, clustered by textbook series (110 clusters). Data from the whole time period is included.

The models include a set of further covariates. First, we included a dummy variable *US Only*, which equals 1 if all authors of the textbook were based in the United States. For various reasons, including the centrality of the United States to developments in the economics profession, American-based writers might plausibly offer different perspectives on research methods from those elsewhere.⁸ Secondly, we included a dummy variable for *First Edition* textbooks. We conjectured that first editions might reflect more up-to-date accounts about the nature of economics, as authors of repeat editions may have a tendency towards leaving existing text unchanged. We also included dummy variables for textbooks written by a single author (*Sole*

⁸ An alternative approach would be to separate our sample according to whether or not they were written (solely) by British authors. In our sample, British-written books might differ from international volumes, partly because they face a lower bar for entry into the *British Library*: any British-produced book should be found in the library, whereas only selected international volumes are present. However, including in our regressions a dummy variable *UK Only*, instead of *US Only*, reveals no significant impact of UK-based authorship on the textbooks' categorisations. Output of these regressions is available from the authors upon request.

Author) and those whose authorship includes at least one active researcher (*Researcher*). An author who produces research might keep abreast of methodological developments to a greater extent than one focused solely on teaching, which might result in differences in perspectives.

Table 2, whose regressions were conducted on data from the full 1970-2019 timespan, shows the coefficient on *Year* is significantly positive (at the 1% level) in the model with the dependent variable *Done*, indicating that, *ceteris paribus*, more recent textbooks are more likely to make statements under this category. *Year* also has a positive coefficient when the dependent variable is *No Mention*, but it is significant only at the 10% level. This is consistent with the evidence of Figure 1, which shows a moderate increase over time in the proportion of textbooks not mentioning experimentation at all in their introductory sections (indeed, since 1995, *No Mention* has been the most populated of our five categories). The coefficient on *Year* is negative and significant (at the 5% level) when the dependent variable is *Difficult/Rare*, confirming that statements in this category have become less likely over time. However, there is no significant effect of *Year* on the likelihood of stating that experiments are impossible or not done.

The other covariates are mostly insignificant. *US Only* is significant only in one of the models, and then only at the 10% level, offering little evidence of different perspectives between authors inside and outside of the United States. We find also a lack of evidence that the likelihood of observing any particular type of statement differs between textbooks which are first or later editions, or differs between sole-authored and co-authored textbooks.

The variable *Researcher* is, however, significant at the 5% level in the model with the dependent variable *Difficult/Rare*, indicating an increased likelihood of textbooks falling into this category if their authors are research-active. Although *Researcher* is insignificant in the other models, the increased use by research-active authors of the *Difficult/Rare* characterisation appears

likely to be partly at the expense of *Impossible/Not Done*, for which the coefficient on *Researcher* is negative. This suggests a preference of active researchers – possibly because of their proximity to the research culture – towards making more nuanced remarks about experiments, as opposed to dismissing them entirely.

Table 3: Logit regressions: 1970-1994 and 1995-2019

	<i>Dependent Variables</i>						
	1970-1994			1995-2019			
	Impossible/ Not Done	Difficult/ Rare	No Mention	Impossible/ Not Done	Difficult/ Rare	Done	No Mention
Year	0.063*	-0.052	-0.002	-0.120**	-0.052	0.191***	0.005
	(0.037)	(0.035)	(0.040)	(0.050)	(0.036)	(0.066)	(0.030)
US Only	0.333	-0.880	0.872	0.626	-0.570	-1.104	0.785
	(0.729)	(0.626)	(0.685)	(0.898)	(0.715)	(0.824)	(0.664)
First Edition	-0.075	-0.099	0.162	-0.266	0.364	-1.149	0.278
	(0.739)	(0.575)	(0.516)	(1.148)	(0.623)	(1.121)	(0.582)
Sole Author	1.079**	-1.330***	0.616	0.233	0.172	-0.284	-0.065
	(0.530)	(0.479)	(0.586)	(0.791)	(0.802)	(0.886)	(0.655)
Researcher	-0.551	1.954**	-1.414*	-2.455**	1.095	0.732	0.295
	(0.751)	(0.871)	(0.780)	(1.067)	(1.026)	(1.010)	(0.734)
Constant	-125.824*	102.754	3.727	240.091**	103.149	-386.381***	-11.864
	(73.233)	(68.738)	(78.778)	(101.303)	(71.551)	(132.586)	(60.424)
<i>Pseudo R</i> ²	0.061	0.132	0.067	0.173	0.043	0.194	0.032
<i>Observations</i>	114	114	114	157	157	157	157

Note: *** p<0.01, ** p<0.05, * p<0.1; Coefficients are presented; in parentheses are robust standard errors, clustered by textbook series (68 clusters for 1970-1994 and 53 clusters for 1995-2019). No model is estimated for Dependent Variable (Done) for the 1970-94 period, because it has no positive values during this sample.

To account for the possibility that the variables may well have exerted different effects at different points in time, we separated our dataset into the two quarter-centuries, 1970-1994 and 1995-2019, and reran our regressions on the sub-samples covering each of these time ranges (Table 3). Here, we find the effects of *Year* to be generally insignificant in the earlier period, but much stronger in the later period. The coefficients show that since 1995 textbooks have become significantly more likely (at the 1% level) to state without criticism that experiments are practiced in economics, and significantly less likely to state that they are impossible or not done. Thus, the regression results support the apparent observation from Figure 1 of a substantial shift in perspectives during the most recent quarter-century, but no clear shift during the preceding one.

We note the effects of the variable *Researcher* differ across the two time periods: in the earlier period (like across the whole timespan) it has a significantly positive effect on *Difficult/Rare* and an insignificant effect on *Impossible/Not Done*, whereas in the later period it has an insignificant effect on *Difficult/Rare* but a significantly negative effect on *Impossible/Not Done*. This suggests that, in more recent years, the argument that economics is a completely non-experimental science has tended to be made by non-researchers more than by those involved in doing economic research. We also find that, in the period 1970-1994, sole-authored textbooks were more likely to describe experiments as impossible or not done, and less likely to describe them as difficult or rarely done, than co-authored textbooks. This raises the intriguing possibility that when two or more authors collaborated on a text during these earlier years they tended – perhaps because one of the authors had more exposure than the other(s) to experimental research – towards language that was more nuanced and factually accurate (given that experiments at that time did exist in economics).

The general observation that the opinions presented did not change much in the earlier years but have changed strikingly in more recent years is supported by chi-squared tests, which investigate whether the overall distribution of category frequencies within each decade differs significantly from that of the preceding or following decade. The tests results reveal no significant differences between the distributions of the 1970s and 1980s ($p=0.82$), or between those of the 1980s and 1990s ($p=0.59$). However, there is a weakly significant difference between the distributions of the 1990s and the 2000s ($p=0.06$) and a strongly significant difference between those of the 2000s and 2010s ($p<0.01$). This suggests that the strongest changes in how introductory textbooks have portrayed experiments have only occurred in the last decade.⁹

⁹ See Appendix A for further analyses using additional chi-squared tests to explore the changes across time.

Evolving Statements in Influential Texts

So far, our quantitative approach has explored evolving patterns of rhetoric across the introductory textbook literature at large. We acknowledge, however, that our categorisations are by necessity approximations and may miss some of the subtleties contained within the passages they code. Therefore, as a supplementary approach, we conduct an in-depth qualitative examination of the narrative. The repetition in our dataset of unbroken sequences of editions from several textbook series allows us to carry out within-series analyses, in which we trace the exact language used by particular textbooks and any modifications the authors have made to it from one edition to the next. Specifically, the textbooks we focus on are all those that appear more than five times in our dataset *and* whose introductory sections include a discussion of the role of experimentation in economics. This comprises nine textbook series and includes all the influential texts that we specified earlier as compulsory inclusions to our dataset, plus a further four series, written by: David Begg and co-authors; Karl Case, Ray Fair and Sharon Oster; Michael Parkin and co-authors; and David Colander. We can safely assume, given the success of all these textbooks in achieving multiple editions stretching over many years, that they have also been influential.

In most cases, our sample includes every post-1970 edition of the series covered below, so we can detect the years in which changes were made to the text. We present the series chronologically, starting with those first appearing further back in our dataset.

Samuelson and Nordhaus' *Economics* (1970 – 2010)

The most famous economics textbook of all changed its tune on experimental economics earlier than most others. However, back in its eighth edition when our dataset begins in 1970, the language of Paul Samuelson's *Economics* certainly reflected the prevailing sceptical view of the economics discipline towards experimentation. He wrote (p.6): 'Because of the complexity of human and

social behavior, we cannot hope to attain the precision of the physical sciences. We cannot perform the controlled experiments of the chemist or biologist. Like the astronomer, we must be content largely to "observe". There were, however, minor inconsistencies in the text, with Samuelson noting on page 7 that 'it is usually not possible to make economic observations under the controlled experimental conditions characteristic of scientific laboratories.' The tantalizing inclusion of 'usually' suggests that Samuelson was probably aware of – though not enthusiastic about – experimental economics. On page 13, Samuelson reverted to his unqualified statement that economics 'cannot employ controlled experiments of the physicist.'

This discussion of experiments remained substantively unchanged until the 12th edition in 1985, the first to be co-written with William Nordhaus. Here, the authors refined their argument as to why experiments were impossible in economics. They stated (p.8): '(Economists) cannot perform the controlled experiments of chemists or biologists because they cannot easily control other important factors.' The next edition in 1989, however, saw wholesale changes to tone and content. 'In an exciting new development, economists are turning to laboratory and other controlled experiments to study economic phenomena,' the authors wrote (p.6). They still cautioned, however, that 'experiments in economics are more difficult than in most sciences', explaining that this was because of difficulties in measuring variables accurately and replicating the economy realistically in the lab, and because 'people often behave peculiarly in experimental situations'. 'Nevertheless,' they remarked, 'in one group of controlled experiments over the last 15 years, economists and others have measured people's reactions to different kinds of government programs to raise the incomes of the poor...In the last decade, economists have turned increasingly to laboratory experiments. In these, groups of people are set up as firms and consumers to determine how prices would behave in different markets' (p.6-7). This indicates Samuelson and/or

Nordhaus were particularly familiar with the brand of market experiments promoted by the likes of Vernon Smith.

This section was cut down slightly for the 14th edition (1992) and, notably, the comment about people behaving peculiarly in experiments was removed. More sweeping changes were made to the introductory section for the 15th edition (1995), and all references made to experimentation were cut out. All subsequent editions continued to exclude such discussion, and indeed contained only brief introductory comment about empirical methods in general. The last edition in our dataset was published in 2010, the year after Samuelson's death.

Lipsey and Chrystal's *An Introduction to Positive Economics* (1971 – 2015)

Richard Lipsey's celebrated *An Introduction to Positive Economics* (the book subsequently underwent two name changes) first enters our dataset in its third edition, published in 1971. In this edition, he – in similar terms to Samuelson – firmly argued against economics being an experimental science: (p.7) 'Experimental sciences, such as chemistry and some branches of psychology, have an advantage because it is possible for them to produce relevant evidence through controlled laboratory experiments. Other sciences, such as astronomy and economics, cannot do this. They must wait for time to throw up observations that may be used as evidence in testing their theories.' In a later section, he furthermore stated (p.40): 'Economics is a nonlaboratory science. It is rarely if ever possible to conduct controlled experiments with the economy.' Much like Samuelson's 'usually', Lipsey's 'rarely' hints at a possible understanding that experiments had been done in economics, but that the author was not an enthusiastic supporter of such projects.

These two sections remained substantively unchanged through to the sixth edition, published in 1983. The seventh, in 1989, saw a mild softening of Lipsey's position, however. In the earlier section, the statement that economics 'cannot' produce relevant evidence through controlled laboratory experiments was removed, though Lipsey still wrote (p.19) that 'Other sciences, such as astronomy and economics, must wait for evidence to be produced in the natural course of events.' In the later section, he now contended (p.35) that 'economics is mainly a non-laboratory science'. He explained (p.34-35): 'In economics, controlled laboratory experiments are, as a general rule, impossible. (In recent years, however, some economists have conducted controlled experiments to observe people's behaviour with respect to many of the choices that are studied in economic theory).' Thus, Lipsey's tone had changed, somewhat, at the same time as that of Samuelson and Nordhaus, but not by as much.

In the following edition (1995) – in which Alec Chrystal joined as a co-author – the later section was eliminated. There was also a subtle but significant change to the earlier section, which now read (p.29), 'Other sciences, such as astronomy and most of economics, must wait for evidence to be produced in the natural course of events,' therein intimating that there were *some* areas of economics in which evidence could be collected through controlled experimentation. The introductory section was then revised for the ninth edition (1999), and experimentation no longer received any mention. This has remained the case in subsequent editions up to 2015.

McConnell, Brue and Flynn's *Economics* (1972 – 2018)

Campbell McConnell's best-selling American textbook *Economics* was already in its fifth edition by 1972. In this text, the author pursued similar arguments to those, at this time, of Samuelson and Lipsey. McConnell wrote (p.6): 'the economist cannot conduct controlled experiments - he does

not have a laboratory in which he can create simplified and controlled conditions. The laboratory of the social sciences is the real world...' The eighth edition in 1981 saw a revision to this text, but not to the essence of the message (p.7): 'economics is not a laboratory science. The economist's process of empirical verification is based upon "real world" data generated by the actual operation of the economy.' Unlike Samuelson/Nordhaus or Lipsey, there is no evidence of McConnell's opinions evolving through the 1980s and 1990s. The book continued to make this same case of economics not being a laboratory science, with only superficial changes to the text, as far forward as the 16th edition in 2005 (Stanley Brue had joined as a co-author in 1990). However, in the seventeenth edition in 2008, it disappears and we see no mention of experiments in the introduction. This has remained the case through to the 21st edition in 2018 (the series has been co-authored by Sean Flynn since the 18th edition in 2008; McConnell passed away in 2019).

Begg, Fischer, Dornbusch and Vernasca's *Economics* (1984 – 2014)

Our dataset contains all eleven editions of *Economics* written by David Begg and co-authors, between 1984 and 2014. This series was initially a British adaptation of Stanley Fischer and Rüdiger Dornbusch's American textbook, first published in 1983 (Dornbusch died in 2002; Gianluigi Vernasca joined the team for the 2011 tenth edition). In that edition Fischer and Dornbusch had acknowledged some presence of experimental research in economics, without conveying too much enthusiasm for it: (p.37) 'Economics data come mostly from the real world, not from experiments...'; (p.45) 'economics...does not have much scope for laboratory experiments. There has been some experimentation with animals...and some large-scale experiments about how people react to welfare payments. But by and large we have to take our data where we find them.' The statements in the 1984 first British edition, co-authored by Begg,

were shorter on details, though again stopped short of entirely ruling out the practice of economic experiments: '(p.30) economics is primarily a non-experimental science... (p.39) Economics can only rarely be an experimental science.' Begg does not seem to have changed his mind over the following 30 years: these statements have remained in the textbook, substantively unchanged, up to the 11th edition in 2014.

Case, Fair and Oster's *Principles of Economics* (1989 – 2017)

In the first edition of Karl Case and Ray Fair's *Principles of Economics*, published in 1989, the authors discussed the ease with which ceteris paribus conditions could be imposed in different scientific disciplines. They noted that physical scientists held a big advantage over social scientists in that, 'They can, for example, observe and measure the effect of one chemical on another while literally holding all else constant in an environment that they totally control. Social scientists very rarely have this luxury, our subjects being people rather than chemicals.' (p.16)

This anti-experimental language remained substantively unchanged in the 1992 second edition, but was softened two years later in the third edition. The 'very rarely' of Case and Fair's sentence above was downgraded to 'rarely' (p.15), and the authors further added: 'While controlled experiments are difficult in economics and other social sciences, they are not impossible. Researchers can isolate and measure the effect of one variable on another.' The example that followed is illuminating about the still-nascent state of experimental economics in the early 1990s. Case and Fair proposed that one could estimate the effect of a tax rate change on saving behaviour by studying households whose income remained constant over the period of time during which the rate changed. Today, while this type of study might be considered a natural experiment, it is doubtful that many economists would regard it as a truly controlled experiment.

In subsequent editions, while keeping the rest of the text as above, they changed the example to one of studying demographic changes on crime rates, and explicitly acknowledged they were referring to natural experiments, adding: ‘Even though economists cannot generally do controlled experiments, fluctuations in economic conditions and factors like birthrate patterns in a way set up natural experiments’ (8th ed., p.13).¹⁰ Thus it is not clear that the authors’ revisions were influenced by the development of lab and field experiments.

This section remained unchanged up to and including the ninth edition in 2009, in which Sharon Oster joined the authorship team. However, the next edition, in 2013, saw a major change. All remarks about the difficulties of experimenting in economics were eliminated. Instead, the authors simply noted (p.45): ‘In recent years, economics has seen an increase in the use of experiments, both in the field and in the lab, as a tool to test its theories.’ This has remained unchanged up to the twelfth edition in 2017, published the year after Case passed away.

Parkin, Powell and Matthews’ *Economics* (1990 – 2017)

Our dataset contains 14 editions of Michael Parkin’s *Economics*, three of them being international variants co-authored with Melanie Powell and Kent Matthews. The first two editions, from 1990 and 1993, did not discuss experimentation in their introductory sections. This changed in the co-authored third European edition (1997), which added a rather nuanced account of the position of experiments in economics (p.14): ‘In the non-experimental sciences such as economics (and astronomy), we usually observe the outcomes of the simultaneous operation of several, perhaps many, factors...To cope with this problem, economists take three complementary approaches...Third, economists are beginning to design experiments which are undertaken in

¹⁰ Our dataset is missing the sixth and seventh editions, but as the relevant statements in the fifth and eighth editions do not substantively differ, we presume that we have not overlooked any important changes from the missing editions.

economic laboratories. This relatively new and exciting approach puts real subjects (usually students) in a decision-making situation and varies their incentives in some way to discover how they respond to one factor at a time.' Thus, while experiments were included in the mooted economist's toolbox, and considered an exciting part of it, the overall discipline could still be labelled 'non-experimental'.

The following year's sole-authored fourth edition tempered the enthusiasm somewhat. The word 'exciting' was removed, and the qualification 'when they can' was added to the statement about economists conducting experiments (p.21). Furthermore, an additional sentence remarked (p.20) 'We cannot easily do economic experiments'. Another change was that Parkin now stated simply that economists performed experiments, without specifying that they did so in economic laboratories.

The fifth edition (2000) left the discussion substantively unchanged, but a subtle revision for the co-authored sixth European edition (2005) deleted the words '(usually students)'. This perhaps reflected the reality that by the early years of this century experimental economics was more regularly spreading its wings beyond university laboratories, and can arguably be considered an increase in the positivity of the book's language towards experimentation, given that overuse of student samples is a common criticism levelled at experimental economists. However, the older version, including '(usually students)', remained in Parkin's sole-authored editions in 2005 and 2008.

A significantly more pro-experimental message emerged, in 2010, in the sole-authored ninth edition. Economics was no longer characterized as a non-experimental science, and the comment about economic experiments being difficult to perform was eliminated. Experiments were still listed as one of the ways of overcoming the problem of the simultaneous operation of multiple

factors, but – perhaps representing the maturity of experimental economics – were no longer described as representing a ‘relatively new’ approach. Parkin also revised his description of what an economic experiment does, declaring (p.11) that it ‘puts people in a decision-making situation and varies the influence of one factor at a time to discover how they respond’. We would agree that this comprises a more accurate representation of economic experiments than the one Parkin employed in earlier editions, as such experiments can indeed vary other factors besides incentives.

This section of text was cut down slightly for the tenth edition (2012), without substantively altering the meaning. Subsequently editions have not seen further changes.

Sloman, Wride, Garratt and Guest’s *Economics* (1991 – 2018)

John Sloman’s position on experiments was unequivocal in the first edition of his popular British series *Economics*, published in 1991: ‘It is impossible to conduct controlled experiments in economics’ (p.28). This statement remained in place four editions later, in 2003. It was, however, removed for the sixth edition in 2006, in which the introductory section made no mention of experimentation. Further changes appeared in the 2009 seventh edition, the first to be co-authored with Alison Wride. The authors here added (p.26): ‘One branch of economics that has seen considerable growth in recent years is ‘behavioural economics’. This sets up experiments and simulations to see how people respond to different sets of circumstances.’ Thus, within six years, the textbook evolved from being negative to neutral to positive about the role of experiments in economics. The eighth and ninth editions, in 2012 and 2015 (in both of which Dean Garratt was added to the authorship team), did not see substantive changes to this section. However, the tenth edition in 2018 (for which Wride was replaced by Jon Guest) expanded the discussion to cover different types of experiment. It commented (p.29): ‘Much of the early evidence in support of

behavioural economics came from laboratory experiments where people made decisions in simulated environments - normally a computer room. More recent evidence has come from field experiments, where people make decisions in a more natural environment and do not know their behaviour is being observed.' The tone of the final sentence arguably implies a favourable opinion towards the way the sub-discipline of experimental economics has progressed.

Colander's *Economics* (1998 – 2017)

David Colander's *Economics* is another series in which the discussion of experiments has been dramatically transformed over a relatively recent timeframe. The earliest edition to appear in our dataset is the third, in 1998, in which there was no mention of experimentation in the introductory section. This remained the case until the fifth edition in 2004, in which Colander introduced the following statement (p.12): 'Because economics is an observational, not a laboratory, science, economists cannot test their models with controlled experiments.' This pessimism about experiments was, however, completely reversed in the eighth edition in 2010. 'Modern empirical work takes a variety of forms,' Colander wrote, 'In certain instances, economists study questions by running controlled laboratory experiments. That branch of economics is called **experimental economics**' (p.6). Two editions later, in 2017, Colander further expanded the prominence of experiments in the introductory discussion on empirical methods, noting – like Sloman and co-authors – that economic research encompasses a variety of types of experiment (p.13-14): 'These include laboratory experiments - experiments in which individuals are brought into a computer laboratory and their reactions to various treatments are measured and analyzed; field experiments - experiments in which treatments in the real world are measured and analyzed; computer experiments - experiments in which simulated economies are created within the computer and

results of various policies are explored; and natural experiments - naturally occurring events that approximate to a controlled experiment.'

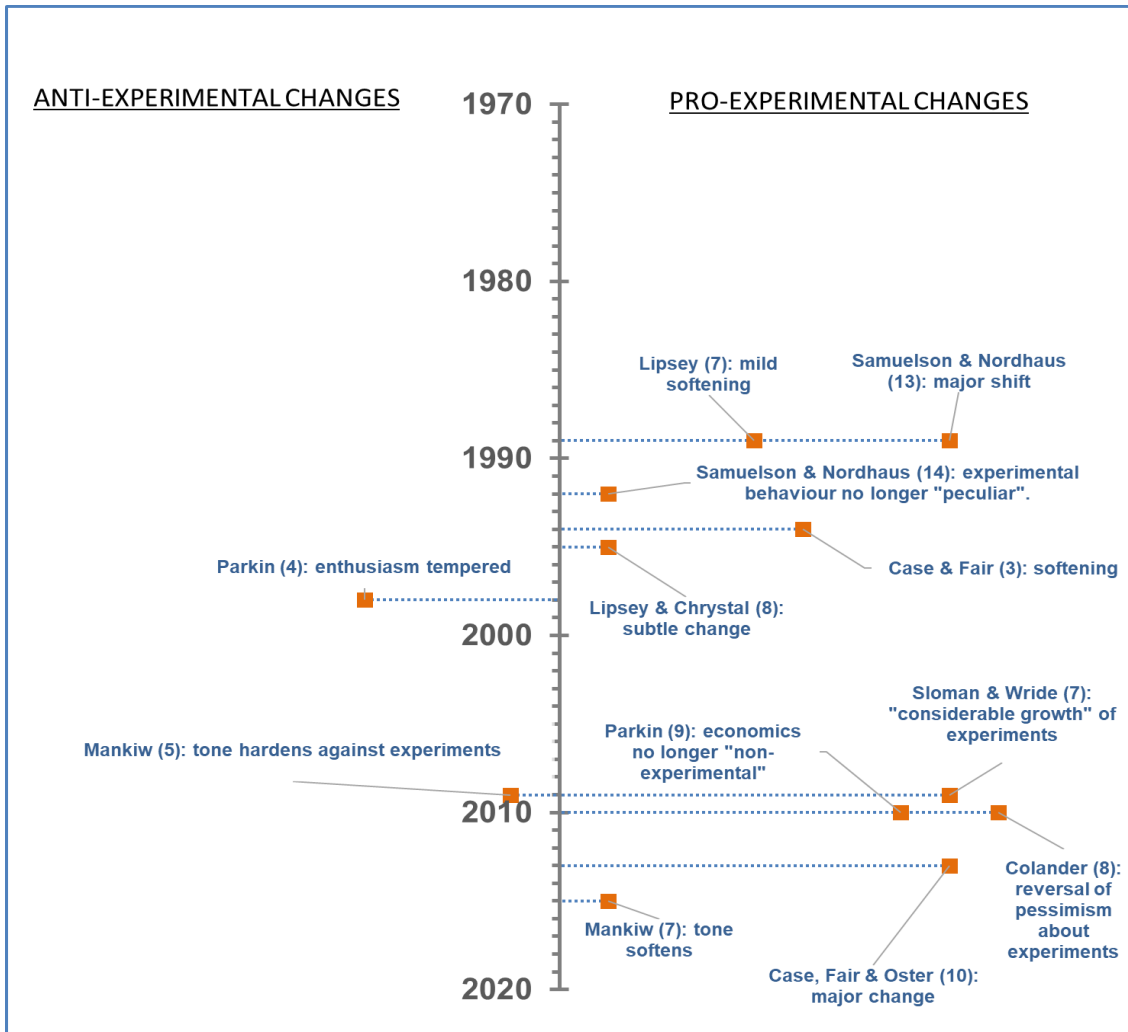
Mankiw's *Principles of Economics* (1998 – 2018)

Gregory Mankiw's best-selling *Principles of Economics* is one series in which a message of scepticism towards experiments has been transmitted throughout, with only very minor changes across editions. In the first edition, published in 1998, Mankiw wrote (p.19): 'Although economists use theory and observation like other scientists, they do face an obstacle that makes their task especially challenging: Experiments are often difficult in economics.' This text remained unchanged until the fifth edition, in 2009, when the final part of the sentence was revised in such a way as to, in fact, harden the tone against experiments: (p.22) 'In economics, conducting experiments is often difficult and sometimes impossible.' This language was, however, softened in the seventh edition in 2015 (p.20), to become: 'In economics, conducting experiments is often impractical'. This was left unchanged for the most recent eighth edition, published in 2018.

From Entrenched Scepticism to Enthusiastic Endorsement

We highlight several points from our qualitative analysis. First, we detect no changes until the late-1980s. This is consistent with the quantitative analysis of our complete dataset, which also did not find a clear tendency for a pro-experimental shift in the earlier decades. In the current analysis, this may also partly reflect that our qualitative review only covers three textbook series from the 1970s. However, while all three maintained the same language throughout the 1970s and early 1980s, both Lipsey and Samuelson/Nordhaus changed their message from the same year, 1989. Other textbooks followed, though some of the most striking changes did not occur until around the end of the first decade of the 21st century. In the century's second decade, several authors have

Figure 2: Timeline of key changes in series analysed



Note: points further right generally indicate more strongly pro-experimental changes, but precise location is arbitrary. We only include changes which unambiguously move the author to a more anti- or pro-experimental position (e.g. we do not include changes where in either the earlier or later edition the author does not mention experiments). Edition number in parentheses after name(s) of author(s).

communicated in particular the increasing prominence of field experiments. Overall, the progression of an increasingly positive tone towards experimental economics within the last decade or so is consistent with the quantitative evidence. That we also find some key texts changed their statements as early as the late 1980s, while our quantitative analysis revealed little widespread change before around 2000, may reflect that the most influential textbooks revised their language first and were only followed by other authors rather later. Figure 2 provides a visual summary of the key changes we have noted in our coverage of the series above.

However, we also note that changes have not been universally towards portraying experiments more positively. There are occasional cases of language becoming more anti-experimental (e.g. Mankiw's fifth edition) or a pro-experimental passage being deleted and the introductory section not mentioning experiments at all in subsequent editions (e.g. Samuelson and Nordhaus's 15th edition). There is also quite some heterogeneity in the extent to which authors' positions have evolved. While some have comprehensively revised their discussion of experimentation (e.g. Sloman et al., Colander), others have always maintained a sceptical message (e.g. Begg et al., Mankiw).

What do Influential Authors say?

To gain further insight about the changes to the texts we evaluated above, we contacted the living authors of these series. Our requests for written interviews were granted by Nordhaus, Lipsey and Chrystal, Fair and Oster, Parkin, Sloman, Colander and Mankiw. We asked the authors to report how favourably or unfavourably they viewed the use of experiments in economics, both at the time of writing their first editions and in the present day. In the case that their opinions had evolved over time, we asked them when and why this change occurred.¹¹

Some of the responses took us by surprise. There is remarkable consistency in the current opinions expressed about experiments. All nine authors report being broadly or wholly enthusiastic about economic experiments today. Nordhaus, for instance, describes experiments as 'a critical element to economic research and... particularly useful for filling in gaps where econometric analysis is hard to apply'. Lipsey states: 'I currently hold an entirely favourable view experiments in economics, being particular impressed with those that challenge basic assumptions such as that agents let bygones be bygones, or that they make rational decisions between present and future

¹¹ Full transcripts of the interviews can be found in Appendix B.

payoffs, or that utility is solely a function of one's own consumption – and of course there are many more.' Colander broadly concurs: 'I see (experiments in economics) as necessary and good. One needs a combination of field, computer, and natural experiments', though he also adds: 'I sometimes think experimenters draw too strong conclusions from experiments, and don't adequately take into account their limitations.' Parkin and Sloman both mention the usefulness of experiments as pedagogical tools in teaching economics. Overall, that we received such a pro-experimental set of statements, from a group of economists whose own research interests diverge, strikes us as strong evidence that experiments have firmly entered the economic mainstream.

More surprisingly, perhaps, many of the authors record having *always* held favourable views towards economic experiments throughout their textbook-writing careers. This is particularly the case for those who started writing textbooks in the 1990s. Sloman recalls: 'I (in 1991) viewed the use of experiments highly favourably'. Similarly, Colander recollects that 'from the beginning I have been a strong supporter of experiments in economics. In the 90s we pushed for behavioral economics, which used experiments, but they were used earlier.' Mankiw asserts: 'I have always had a "big tent" view of economic methodology, and (in 1998) I thought experiments were a useful addition to the economist's toolbox.' Going further back, Nordhaus writes that, as a new textbook author in the 1980s, he 'had always been extremely impressed by the use of experiments. The most impressive were the negative income tax experiments in the 1960s. My concern was that they tended to be very expensive and often poorly designed (this does not apply to the NIT). Moreover, they were few and far between at that time.'¹²

Others describe having been neutral rather than hostile towards economic experiments in earlier years. 'I wrote the 1st edition of my textbook between 1960 and 1963 and I don't think I

¹² Of Paul Samuelson, he adds 'My recollection is that we had a shared view here.'

knew much about experimental economics at the time. In so far as I had any view, it would have been favourable,' tells Lipsey. Parkin states: 'As best as I can recall, I didn't (in 1990) have a view about the use of experiments in economics'. These comments likely reflect the limited extent to which knowledge of experimental economics was disseminated throughout the discipline in earlier decades.

Both Colander and Mankiw argue their opinions about experiments have not changed at all since they began writing textbooks. Lipsey contends that his has not changed much – 'it just got firmer as I read more and more of the evidence from various experiments.' Sloman recalls: 'My opinion has not really changed about the use of experiments. Rather, my enthusiasm has grown as I have experienced both the pedagogical advantages in terms of making learning more active and engaging and the insights it provides into decision making.' Parkin acknowledges a more transformed perspective, describing the change as 'a gradual process'. Nordhaus explains why his opinion has become more positive: 'I had not realized earlier that (economic experiments) could be done so inexpensively. Part of that, of course, was changes in technology and communications.'

The interviews go some way to explaining the evolution of their textbooks' statements. The shifts away from anti-experimental language in the Samuelson/Nordhaus and Lipsey/Chrystal texts reflect in turn Nordhaus's increasing realization of the feasibility of experimental economics and Lipsey's growing awareness of the field. That Mankiw's commentary has changed little across revisions is consistent with his assertion that he has not changed his mind, while the increasing positivity in tone of Sloman's and Parkin's texts chimes with their recollections of rising enthusiasm for experiments over the years.

On the other hand, there are discrepancies between the favourable views Sloman and Colander recall holding towards experimentation in earlier years and the statements in their textbooks about

experiments being impossible. It is clear that both were well aware of experimental economics. The interview with Colander, however, brings further insight: ‘My views on experiments don’t necessarily reflect what is in the intro text... In terms of introductory teaching...I think that too much focus on experiments is problematic — and that teaching constrained optimization modelling remains useful.’ This offers a plausible interpretation behind not only Colander’s statements but those of many of the authors in our dataset who described economic experiments as impossible: the authors themselves may have been fully aware of and perhaps even sympathetic towards the tool, but regarded it as sufficiently irrelevant for the purposes of introductory students that it could be simplified to the level of non-existence.

Concluding Remarks

Our paper’s focus was to provide an answer to the question of whether economics is – and, in earlier years, *was* – an experimental science, as documented by economic principles textbooks. Our analysis has shown that the rhetoric has changed from the pessimism of ‘experiments are rare or impossible’ to the enthusiastic endorsement of ‘experiments are done’. Evidence drawn from the textbooks and author interviews suggest that economics no longer considers itself a non-experimental science. The implications of this are positive for the credibility of fields strongly associated with experimentation, in particular behavioural economics. The changes we document have, however, varied across texts and have generally come slowly. Across the introductory textbook literature as a whole, striking changes in statements have occurred only since the turn of the century.

Why did authors, in relatively large numbers, continue to describe economic experiments as impossible well into the 2000s? One interpretation is that the authors were simply not aware of experimental economics. This strikes us as unlikely. The authors we interviewed reported having

already held knowledge of experiments several decades ago; both Nordhaus and Fair specifically recalled following the negative income tax experiments of Kershaw (1972). A more plausible explanation may be that authors knew about economic experimentation, but were engaging in rhetorical simplification for the purposes of introductory students. Such an approach would perhaps reflect a wider scepticism across the profession – a view that although some economists were running experiments, economics *should* be a non-experimental science – or perhaps simply the fact that experiments made up a relatively small proportion of economic research. The *Impossible/Not Done* characterisation may have (almost) disappeared in the last decade as a result of both the wider scepticism diminishing and the proportion of experimental research mounting.

An alternative interpretation is that the anti-experimental rhetoric of many textbooks is best understood as the passing-on of received wisdom, rather than any conscious attempt to contribute to debate on economic methodology. This possibility was raised by Starmer (1999b), who described discussions of experimentation in introductory textbooks as consisting of ‘passing references’ made by authors whose primary interests were not methodological. We presume there are limits to the attention that can be paid to every sentence when one’s manuscript runs to several hundred pages, and many authors may have thought little of describing economics as a non-experimental science, when such statements were a staple of earlier texts. To this effect, the remarks in earlier editions of famous textbooks, especially Samuelson’s, may have been pivotal. Samuelson was certainly not the first to prominently introduce economics to students as a non-experimental science – we find similar arguments, for instance, in Boulding (1941) – but his book was in a highly influential position when the market for principles textbooks proliferated towards the end of the 20th century. Stiglitz (1988) characterised many of the textbooks that arose during this market expansion as ‘clones’ of Samuelson’s (or perhaps clones of books which had

themselves cloned Samuelson's), while describing Samuelson himself as an 'innovator'. This may be borne out by the fact that, while Samuelson's book took a markedly more positive tone towards experiments in 1989, it took about another decade for the market to follow.

Taken together, these explanations show why the wide dissemination and manifestation of ideas may be necessary before they receive attention in principles textbooks. It is interesting to note that we did not identify *any* textbooks with an unambiguously positive characterisation of experimentation until 2002, the same year in which experimental economics received a highly significant endorsement in the form of the Nobel Prize being awarded to Vernon Smith. This slow process for the diffusion of ideas into principles textbooks has been noted in the context of other important topics in economics, including game theory and information asymmetry (Bowles and Carlin, 2020). It has been examined in detail by Colander et al. (2004), who wrote: 'The process from conception of an idea to its appearance in graduate textbooks can take up to ten years. Intermediate and upper level undergraduate textbooks usually take another five to ten years to include these ideas... Principles books take another five to ten years to actually incorporate the idea as a central element.' This mooted timescale for change appears quite accurate in our case, with many textbooks eliminating anti-experimental rhetoric in the 2000s and 2010s.

A likely consequence of such sluggish change is on the perceptions of economics graduates. The class of 2020 may not conceive economics to be a wholly non-experimental science, but the views of the class of 2000 could be quite different. It is reasonable to believe that what students learn in economics courses does influence them in later life, and what they read in their textbooks forms an important part of what they learn.¹³ The varying perception of and exposure to

¹³ Samuelson is reported to have claimed, 'I don't care who writes a nation's laws — or crafts its advanced treaties — if I can write its economics textbooks,' and to have spoken hopefully in 1967 about the possibility of his textbook influencing the outcome of the 1984 US Election (Skousen, 1997).

experimentation, across generations of graduates, has important implications over the extent to which former economics students who work in non-academic fields are likely to be receptive to the use of experiments in, for instance, informing policy design and business decisions. Field experiments are increasingly used for policy purposes (see e.g. Duflo, 2017; Luca and Bazerman, 2020) and the business world has been penetrated to some extent by ideas from behavioural and experimental economics (Charness and Chen, 2002; Chen and Krakovsky, 2010; Gino, 2017). We would conjecture that policy and business leaders who graduated more recently would be more open to such approaches, which would make them likely to become more popular still in the future.

Our paper also has pedagogical implications in terms of what – and how – economics students are taught. The increasingly pro-experimental tone of introductory textbooks is likely to reflect a greater openness among today’s instructors towards teaching experimental findings – and, indeed, such positive views expressed in the textbooks may also *cause* some instructors’ perspectives to become more pro-experimental. As behavioural economic insights make their entrance into syllabi, experimentation may also find a way into methods courses. In the spirit of our paper, future research could examine how experimentation is – and has been – discussed in econometrics textbooks. Regarding *how* we teach, as experiments permeate the mind-set of economists, they are likely to be more widely used as pedagogical tools – something reflected in the responses of our interviewees. This may be beneficial for students, who, for example, might find abstract market concepts difficult to grapple with, but might engage more vividly if the instructor conducts a demonstrational pit market experiment (Holt, 2019). More generally, greater exposure of students to experimentation and laboratories may ultimately help dispel popular perceptions that economics is not really very scientific.

If perceptions towards experimentation have become dramatically more favourable in recent decades, should we expect this process to continue, with even fewer textbooks in future highlighting the difficulties in running economic experiments? Not necessarily, we argue. There is some evidence that the march of experimental economics has now become a retreat, at least in terms of declining coverage in the top journals (Nikiforakis and Slonim, 2019). If this retreat is genuine and sustained, it may come to be reflected – perhaps after a time lag – by a return of anti-experimental rhetoric to introductory economic textbooks.

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Online Appendix A

Empirical approach: additional details and analyses

Inclusion in dataset of related textbooks

In general, if our search returned multiple versions of the same textbook, we include all versions in our dataset. There are some exceptions, however. Our search turned up some textbooks which have been published under multiple versions in different parts of the world. If two regional variants of the same textbook were published in the same year and we found that the relevant statements upon which we coded each were identical, we treated the two as providing just one combined observation. If any two regional variants were published in different years, or were published in the same year but differed in the relevant statements they made, we included both.

We excluded abridged versions of longer textbooks already included in our dataset (for instance, we exclude Krugman et al.'s *Essentials of Economics*, since we include the longer textbook *Economics*, on which it is based).

All regressions correct for heteroscedasticity with robust standard errors, clustered by textbook series.

Categorisation of textbook statements

As noted in the main text, those textbooks which have an introductory section in which experimentation is mentioned are categorised either as *Impossible/Not Done*, *Difficult/Rare* or *Done*. We argue that these three categories are defined such that they meaningfully place authors at distinct points on a pro/anti-experimental spectrum. We do note that there is scope for statements to overlap between categories. One might contend that experiments are in principle difficult rather than impossible, but also that in practice they never occur. In fact, however, we never encountered this combination of arguments.

Instead, we sometimes found statements at different points of the same text that were inconsistent: specifically, an author arguing in one place that economic experiments are impossible, and in another that they are merely difficult. In such cases we took the view that the less extreme statement took precedence and coded the book into the *Difficult/Rare* category, under the interpretation that the characterisation of impossibility was a rhetorical simplification of the book's more nuanced statement made elsewhere. There were also cases of statements which themselves were ambiguous and it could be argued either way whether they were characterising experiments as, for instance, rare or not. We used our own judgment to code such cases.

There were a total of 17 books which could be interpreted under more than one category; we provide details of all in Table A1. As robustness checks, we also re-ran our main analyses first having re-coded all books making ambiguous or self-contradictory statements under the most anti-experimental category they were interpretable within, and again having placed them into the most pro-experimental category they were interpretable within. We report the output of the regression analyses within these robustness checks in Tables A2-A5 below. The regressions of Table 2, covering the full period 1970-2019, are replicated in Table A2 (anti-experimental interpretation) and Table A3 (pro-experimental interpretation). The sub-sample regressions in Table 3, covering 1970-1994 and 1995-2019, are replicated in Table A4 (anti-experimental interpretation) and Table A5 (pro-experimental interpretation). With rare exceptions, the robustness checks yield results which are very similar to those based on our preferred classifications. We detail, below, the only noteworthy ways in which these results differ.

One difference relates to the estimated time effects on the frequency of occurrence of the anti-experimental categorisations. In our regressions for the whole time period reported in Table 2 of the main text, we found the *Year* variable had a significantly negative effect on the dependent

variable *Difficult/Rare*, and an insignificant effect on the dependent variable *Impossible/Not Done*. When we code textbooks under the most anti-experimental interpretation, we find instead a significantly negative effect of *Year* on *Impossible/Not Done* but an insignificant effect on *Difficult/Rare* (Table A2). This change likely reflects that in earlier years there were more statements that were on the cusp of branding experiments as impossible, relative to later years when there were few that could even possibly be interpreted as making such an argument. In spite of the difference in results here, the key finding of a movement over time away from more anti-experimental statements and towards more pro-experimental ones remains.

Under this anti-experimental coding approach, we also find that the positive effect of the variable *Researcher* on the *Difficult/Rare* categorisation during the period 1970-1994 loses significance (comparing Table 3 and Table A4). However, the positive effect of *Researcher* on *Difficult/Rare* across the full timespan remains significant (Table 2 and Table A2).

Coding according to the most pro-experimental interpretation produces slight differences to our chi-squared test results, which explore changes between decades in the overall distribution of category frequencies. In the main text, we reported a weakly significant difference between the distributions of the 1990s and 2000s, and a strongly significant difference between the 2000s and 2010s. Re-running these tests under the pro-experimental coding approach, we find the difference between the 1990s and 2000s becomes insignificant ($p=0.16$), while between the 2000s and 2010s it remains strongly significant. Under this interpretation, therefore, the evidence that time changes occur primarily in later years becomes even stronger than that reported in the main text.

Table A1: List of textbooks with multiple possible categorisations

Textbook	Statement(s)	Our interpretation	Alternative interpretation
Checkley (1984)	<i>There are differences in the way in which the economist experiments because unlike the physical scientist he cannot use controlled laboratory conditions. (p.6)</i>	Impossible/Not Done	Difficult/Rare
Fischer et al. (1988)	<i>NONEXPERIMENTAL DATA...Some experiments have been used in economics. For example, in the 1970s the U.S. Department of Health, Education and Welfare funded large-scale experimental studies in Denver and Seattle concerning the effects of different types of income maintenance (welfare) payments. A less expensive and increasingly popular alternative is to experiment with students, setting up classroom experiments designed to approximate real market situations. But most data used by economists are not derived from controlled experiments. (p.37)</i>	Difficult/Rare	Done
Hardwick et al. (1999)	<i>A major problem with (the approach of induction) is that economic statistics are so complex that it is often difficult to disentangle them and, of course, economists cannot perform laboratory experiments in the same way as the physical scientists can. (p.14)</i>	Difficult/Rare	Impossible/Not Done
Lipsey (1971)	<i>Experimental sciences, such as chemistry and some branches of psychology, have an advantage because it is possible for them to produce relevant evidence through controlled laboratory experiments. Other sciences, such as astronomy and economics, cannot do this. They must wait for time to throw up observations that may be used as evidence in testing their theories. (p.7)</i> <i>Economics is a nonlaboratory science. It is rarely if ever possible to conduct controlled experiments with the economy. (p.40)</i>	Difficult/Rare	Impossible/Not Done
Lipsey (1975)	<i>Experimental sciences, such as chemistry and some branches of psychology, have an advantage because it is possible for them to produce relevant evidence through controlled laboratory experiments. Other sciences, such as astronomy and economics, cannot do this. They must wait for time to throw up observations that may be used to test hypotheses. (p.7)</i> <i>Economics is a non-laboratory science. It is rarely if ever possible to conduct controlled experiments with the economy. (p.41)</i>	Difficult/Rare	Impossible/Not Done

Lipsey (1979)	<p><i>Experimental sciences, such as chemistry and some branches of psychology, have an advantage because it is possible to produce relevant evidence through controlled laboratory experiments. Other sciences, such as astronomy and economics, cannot do this. They must wait for time to throw up observations that may be used to test hypotheses. (p.8)</i></p> <p><i>It is rarely, if ever, possible to conduct controlled experiments with the economy. Thus economics must be a non-laboratory science. (p.39)</i></p>	Difficult/Rare	Impossible/Not Done
Lipsey (1983)	Identical to Lipsey (1979)	Difficult/Rare	Impossible/Not Done
Livesey (1972)	<i>The economist cannot undertake controlled experiments in the same way as the physical scientist, since the raw materials of Economics are people acting either individually or in groups. (p.4)</i>	Difficult/Rare	Impossible/Not Done
Mings & Marlin (2000)	<p><i>Economists usually are not able to conduct controlled experiments...They must depend on the observation of real-world events. (p.11)</i></p> <p><i>Economics makes use of the scientific method, but it is a social science, not a laboratory science, and cannot do controlled experiments. (p.28)</i></p>	Difficult/Rare	Impossible/Not Done
Parkin et al. (1997)	<p><i>In the non-experimental sciences such as economics (and astronomy), we usually observe the outcomes of the simultaneous operation of several, perhaps many, factors...To cope with this problem, economists take three complementary approaches...Third, economists are beginning to design experiments which are undertaken in economic laboratories. This relatively new and exciting approach puts real subjects (usually students) in a decision-making situation and varies their incentives in some way to discover how they respond to one factor at a time. (p.14)</i></p> <p><i>Economists...develop statistical and experimental methods for isolating the factors of interest. (p.22)</i></p>	Difficult/Rare	Done
Powicke (1971)	<i>The economist is essentially concerned with analysing problems, building up explanations and then testing his results...this last process is difficult, one cannot try theories out on a living community just to see if they work! One has to accept instead the judgment or assessment of</i>	Difficult/Rare	Impossible/Not Done

	<i>those who have made a careful study of the matter. (P.2)</i>		
Samuelson (1970)	<p><i>Because of the complexity of human and social behavior, we cannot hope to attain the precision of the physical sciences. We cannot perform the controlled experiments of the chemist or biologist. Like the astronomer, we must be content largely to "observe." (p.6)</i></p> <p><i>As we noted, it is usually not possible to make economic observations under the controlled experimental conditions characteristic of scientific laboratories. A physiologist who wishes to determine the effects of penicillin on pneumonia may be able to "hold other things equal" by using two test groups that differ only in the fact that they do and do not get penicillin injections. The economist is less fortunately situated. (p.7)</i></p> <p><i>(Economics) cannot employ controlled experiments of the physicist. (p.13)</i></p>	Difficult/Rare	Impossible/Not Done
Samuelson (1973)	Identical to Samuelson (1970).	Difficult/Rare	Impossible/Not Done
Samuelson (1976)	<p><i>Because of the complexity of human and social behavior, we cannot hope to attain the precision of the physical sciences. We cannot perform the controlled experiments of the chemist or biologist. Like the astronomer, we must be content largely to "observe." (p.7)</i></p> <p><i>As we noted, it is usually not possible to make economic observations under the controlled experimental conditions characteristic of scientific laboratories. A physiologist who wishes to determine the effects of penicillin on pneumonia may be able to "hold other things equal" by using two test groups that differ only in the fact that they do and do not get penicillin injections. The economist is less fortunately situated. (p.9)</i></p> <p><i>(Economics) cannot employ the controlled experiments of the physicist (p.15)</i></p>	Difficult/Rare	Impossible/Not Done
Samuelson (1980)	<i>Because of the complexity of human and social behavior, we cannot hope to attain the precision of the physical sciences. We cannot perform the controlled experiments of the chemist or biologist. Like the astronomer or meteorologist, we must be content largely to "observe." (p.5)</i>	Difficult/Rare	Impossible/Not Done

	<i>As we noted, it is usually not possible to make economic observations under the controlled experimental conditions characteristic of scientific laboratories. A physiologist who wishes to determine the effects of penicillin on pneumonia may be able to "hold other things equal" by using two test groups that differ only in the fact that they do and do not get penicillin injections. The economist cannot. (p.7)</i>		
Samuelson et al. (1975)	<i>It is usually impossible to make economic observations under conditions of controlled experiment similar to those achieved in scientific laboratories.' (p.7)</i> <i>The economist's inability to use laboratory methods is an undoubted handicap. (p.8)</i> <i>Economists cannot use the techniques of controlled experiment which are available in some of the natural sciences. (p.14)</i>	Difficult/Rare	Impossible/Not Done
Solmon (1980)	<i>Although economists are committed to the use of the scientific method, it is not possible to apply this method as comprehensively in the study of human behavior as it is in laboratory studies of inanimate chemicals or caged animals (author gives examples of government being unable to set up experiment where people are unemployed)...Ideally, the scientific method also requires constant repetition of experiment and result...But in economics, as in all social sciences, it is impossible to duplicate the exact conditions of any situation under study. (p.6)</i> <i>Because they are unable to conduct controlled experiments, economists are unable to define a single theoretical model which would yield universal agreement on outcomes of policy changes. (p.9)</i> <i>Unlike many natural and physical scientists and some social scientists, economists generally cannot run controlled experiments in a laboratory setting. (p.15)</i>	Difficult/Rare	Impossible/Not Done

Table A2: Logit regressions: 1970-2019 (anti-experimental interpretation)

	Dependent variables			
	(Impossible/ Not Done)	(Difficult/ Rare)	(Done)	(No Mention)
Year	-0.057*** (0.015)	-0.026 (0.017)	0.201*** (0.057)	0.029* (0.017)
US Only	-0.118 (0.528)	-0.521 (0.511)	-1.117 (0.822)	0.777* (0.437)
First Edition	-0.629 (0.534)	0.397 (0.375)	-1.163 (1.107)	0.182 (0.351)
Sole Author	0.946** (0.470)	-0.893 (0.635)	-0.284 (0.887)	0.180 (0.447)
Researcher	-0.550 (0.684)	1.235** (0.624)	0.729 (1.016)	-0.664 (0.595)
Constant	112.645*** (30.584)	50.136 (34.833)	-406.195*** (114.687)	-58.678* (33.398)
Pseudo R ²	0.149	0.069	0.333	0.047
Observations	271	271	271	271

Note: *** p<0.01, ** p<0.05, * p<0.1; Coefficients are presented; in parentheses are robust standard errors, clustered by textbook series (110 clusters). Data from the whole time period is included.

Table A3: Logit regressions: 1970-2019 (pro-experimental interpretation)

	Dependent variables			
	(Impossible/ Not Done)	(Difficult/ Rare)	(Done)	(No Mention)
Year	-0.027 (0.018)	-0.046** (0.020)	0.143*** (0.038)	0.029* (0.017)
US Only	0.247 (0.634)	-0.658 (0.517)	-0.979 (0.812)	0.777* (0.437)
First Edition	-0.160 (0.657)	0.097 (0.408)	-1.330 (1.025)	0.182 (0.351)
Sole Author	0.615 (0.416)	-0.474 (0.543)	-0.438 (0.771)	0.180 (0.447)
Researcher	-1.006 (0.740)	1.333** (0.614)	0.802 (0.913)	-0.664 (0.595)
Constant	52.856 (35.909)	91.283** (39.844)	-288.223*** (76.239)	-58.678* (33.398)
Pseudo R ²	0.069	0.091	0.271	0.047
Observations	271	271	271	271

Note: *** p<0.01, ** p<0.05, * p<0.1; Coefficients are presented; in parentheses are robust standard errors, clustered by textbook series (110 clusters). Data from the whole time period is included.

Table A4: Logit regressions: 1970-1994 and 1995-2019 (anti-experimental interpretation)

	<i>Dependent Variables</i>						
	1970-1994			1995-2019			
	Impossible/ Not Done	Difficult/ Rare	No Mention	Impossible/ Not Done	Difficult/ Rare	Done	No Mention
Year	-0.023 (0.039)	0.023 (0.036)	-0.002 (0.040)	-0.135*** (0.048)	-0.041 (0.036)	0.191*** (0.066)	0.005 (0.030)
US Only	-0.400 (0.564)	-0.450 (0.526)	0.872 (0.685)	0.485 (0.829)	-0.565 (0.735)	-1.104 (0.824)	0.785 (0.664)
First Edition	-0.792 (0.609)	0.680 (0.517)	0.162 (0.516)	-0.516 (1.157)	0.474 (0.620)	-1.149 (1.121)	0.278 (0.582)
Sole Author	1.666*** (0.524)	-2.144*** (0.483)	0.616 (0.586)	-0.061 (0.738)	0.294 (0.802)	-0.284 (0.886)	-0.065 (0.655)
Researcher	0.292 (0.646)	1.241 (0.826)	-1.414* (0.780)	-2.332** (1.024)	1.057 (1.025)	0.732 (1.010)	0.295 (0.734)
Constant	43.354 (76.427)	-46.933 (71.967)	3.727 (78.778)	240.091** (101.303)	103.149 (71.551)	-386.381*** (132.586)	-11.864 (60.424)
<i>Pseudo R</i> ²	0.120	0.215	0.067	0.173	0.043	0.194	0.032
<i>Observations</i>	114	114	114	157	157	157	157

Note: *** p<0.01, ** p<0.05, * p<0.1; Coefficients are presented; in parentheses are robust standard errors, clustered by textbook series (68 clusters for 1970-1994 and 53 clusters for 1995-2019). No model is estimated for Dependent Variable (Done) for the 1970-94 period, because it has no positive values during this sample.

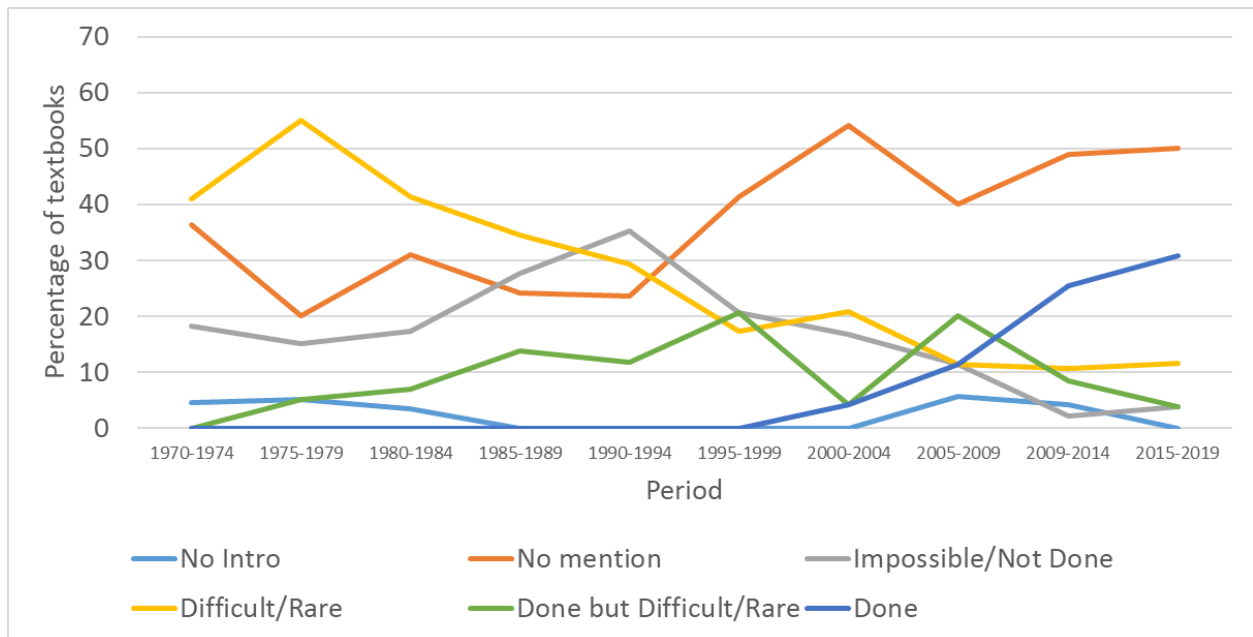
Table A5: Logit regressions: 1970-1994 and 1995-2019 (pro-experimental interpretation)

	<i>Dependent Variables</i>						
	1970-1994			1995-2019			
	Impossible/ Not Done	Difficult/ Rare	No Mention	Impossible/ Not Done	Difficult/ Rare	Done	No Mention
Year	0.060 (0.038)	-0.049 (0.034)	-0.002 (0.040)	-0.120** (0.050)	-0.045 (0.033)	0.161*** (0.047)	0.005 (0.030)
US Only	0.327 (0.733)	-0.945 (0.644)	0.872 (0.685)	0.626 (0.898)	-0.494 (0.706)	-1.158 (0.799)	0.785 (0.664)
First Edition	-0.175 (0.740)	0.046 (0.563)	0.162 (0.516)	-0.266 (1.148)	0.416 (0.610)	-1.206 (1.087)	0.278 (0.582)
Sole Author	1.049* (0.536)	-1.187** (0.465)	0.616 (0.586)	0.233 (0.791)	0.230 (0.802)	-0.365 (0.818)	-0.065 (0.655)
Researcher	-0.291 (0.778)	1.627** (0.822)	-1.414* (0.780)	-2.455** (1.067)	1.037 (1.012)	0.813 (0.965)	0.295 (0.734)
Constant	-120.566 (75.415)	96.356 (67.073)	3.727 (78.778)	240.091** (101.303)	88.741 (65.698)	-325.880*** (94.736)	-11.864 (60.424)
<i>Pseudo R</i> ²	0.051	0.108	0.067	0.173	0.035	0.171	0.032
<i>Observations</i>	114	114	114	157	157	157	157

Note: *** p<0.01, ** p<0.05, * p<0.1; Coefficients are presented; in parentheses are robust standard errors, clustered by textbook series (68 clusters for 1970-1994 and 53 clusters for 1995-2019). No model is estimated for Dependent Variable (Done) for the 1970-94 period, because it has no positive values during this sample.

One might question whether our distinction between *Difficult/Rare* and *Done* is too stark. For this reason, we conducted further analysis when we split the *Difficult/Rare* category in two, between those textbooks which merely state that experiments are difficult or rare, and those which explicitly mention that experiments are done in economics but also add that there are difficulties in doing experiments or that they are only rarely done. There is a subtle difference in rhetoric between the two categories, with the latter conveying a more positive message about experiments. We do not find, however, that creating this separation provides striking insight. There are generally few observations in the more pro-experimental category (28 compared to 69 in the more anti-experimental category), and the frequency of this category does not clearly change over time. See Figure A1, which replicates Figure 1 while allowing for this split in the *Difficult/Rare* category.

Figure A1: Categorisation of textbooks over time (*Difficult/Rare* category split)



Note: Here, Difficult/Rare refers to textbooks which merely state that experiments are difficult or rare. Done but Difficult/Rare refers to textbooks which explicitly mention that experiments are done in economics but also add that there are difficulties in doing experiments or that they are only rarely done.

Sensitivity of results to inclusion of pilot data

We have noted in the main text that our dataset includes 15 textbooks identified solely through a pilot study of textbooks held in the library at our campus. Unlike the other textbooks included in our pilot – which also showed up in our *British Library* search results, or featured on our list of essential influential texts, and therefore would have ended up in our dataset anyway – these 15 books are only included in our analysis as a result of the pilot. For completeness, we considered it preferable to include all our pilot data in the final analysis, rather than dropping any observations. However, we note that our identification of relevant textbooks in the pilot was less scientific than the search protocol we adopted in the rest of our study – we simply analysed all the introductory textbooks we found on the shelves of our library’s economics section. We also acknowledge that the characteristics of our pilot sample may be skewed by the peculiarities of our institution’s library staff. Therefore, we have re-done our main analyses excluding these 15 titles. The output of re-done regressions is reported in Tables A8-A9. Table A8 replicates the regressions of Table 2; Table A9 replicates the sub-sample regressions of Table 3. Overall, the results look very similar. The only change to the significance of a variable in any model is that *US Only* falls from significant at the 10% level to insignificant in the regression covering the period 1970-2019 with the dependent variable *No Mention* (Table 2 and Table A8).

After excluding the pilot-only data, our chi-squared test results look slightly different. The p-value on the test comparing the 1990s and 2000s decreases from 0.06 to 0.03, while for the test comparing the 2000s and 2010s it increases from below 0.01 to 0.02. Excluding the pilot-only data, therefore, would lead to slightly stronger evidence that change occurred before the most recent decade.

Table A8: Logit regressions: 1970-2019 (pilot-only data excluded)

	Dependent variables			
	(Impossible/ Not Done)	(Difficult/ Rare)	(Done)	(No Mention)
Year	-0.025 (0.017)	-0.049** (0.021)	0.207*** (0.061)	0.028* (0.017)
US Only	0.190 (0.629)	-0.496 (0.481)	-1.293 (0.854)	0.659 (0.415)
First Edition	-0.096 (0.643)	0.039 (0.416)	-1.215 (1.096)	0.159 (0.342)
Sole Author	0.622 (0.407)	-0.513 (0.559)	-0.554 (0.909)	0.173 (0.434)
Researcher	-1.074 (0.698)	1.445** (0.632)	0.764 (0.989)	-0.582 (0.581)
Constant	47.901 (34.944)	96.290** (42.308)	-417.070*** (121.632)	-56.979* (34.179)
Pseudo R ²	0.070	0.096	0.358	0.041
Observations	257	257	257	257

Note: *** p<0.01, ** p<0.05, * p<0.1; Coefficients are presented; in parentheses are robust standard errors, clustered by textbook series (109 clusters). Data from the whole time period is included. Textbooks featuring only in the pilot study are excluded.

Table A9: Logit regressions: 1970-1994 and 1995-2019 (pilot-only data excluded)

	Dependent Variables						
	1970-1994			1995-2019			
	Impossible/ Not Done	Difficult/ Rare	No Mention	Impossible/ Not Done	Difficult/ Rare	Done	No Mention
Year	0.063* (0.037)	-0.052 (0.035)	-0.002 (0.040)	-0.118** (0.050)	-0.040 (0.035)	0.196*** (0.071)	-0.003 (0.032)
US Only	0.333 (0.729)	-0.880 (0.626)	0.872 (0.685)	0.534 (0.889)	-0.225 (0.673)	-1.279 (0.856)	0.598 (0.646)
First Edition	-0.075 (0.739)	-0.099 (0.575)	0.162 (0.516)	-0.362 (1.133)	0.549 (0.576)	-1.206 (1.104)	0.220 (0.564)
Sole Author	1.079** (0.530)	-1.330*** (0.479)	0.616 (0.586)	0.169 (0.792)	0.373 (0.800)	-0.550 (0.906)	-0.096 (0.641)
Researcher	-0.551 (0.751)	1.954** (0.871)	-1.414* (0.780)	-2.333** (1.047)	0.866 (1.006)	0.771 (0.982)	0.420 (0.720)
Constant	-125.824* (73.233)	102.754 (68.738)	3.727 (78.778)	236.115** (100.475)	79.219 (70.141)	-395.195*** (141.543)	5.484 (64.360)
Pseudo R ²	0.051	0.108	0.067	0.167	0.028	0.215	0.022
Observations	114	114	114	143	143	143	143

Note: *** p<0.01, ** p<0.05, * p<0.1; Coefficients are presented; in parentheses are robust standard errors, clustered by textbook series (68 clusters for 1970-1994 and 51 clusters for 1995-2019). No model is estimated for Dependent Variable (Done) for the 1970-94 period, because it has no positive values during this sample.

Additional chi-squared tests

In addition to running our chi-squared tests to investigate differences between full decades, we considered a couple of supplementary approaches. One was to shift the cut-off points between decades, and compare the periods 1975-1984 vs 1985-1994, 1985-1994 vs 1995-2004, and 1995-2004 vs 2005-2014. Doing this produced essentially similar results to those in the main text, indicating the rate of change speeding up: $p=0.31$ for 1975-1984 vs 1985-1994; $p=0.07$ for 1985-1994 vs 1995-2004; $p<0.01$ for 1995-2004 vs 2005-2014.

We also ran chi-squared tests to investigate differences between each successive five-year block (as defined in Figure 1), but with the resulting small sample sizes, we are not able to detect any significant changes from one period to the next.

Online Appendix B: Transcript of written interviews with authors (Conducted June-July 2020)

Interview with William Nordhaus

Q: You first co-authored the textbook *Economics* for the 1985 edition. At that point, what was your view about the use of experiments in economics? For instance, did you view this method favourably or unfavourably?

WN: I had always been extremely impressed by the use of experiments. The most impressive were the negative income tax experiments in the 1960s. My concern was that they tended to be very expensive and often poorly designed (this does not apply to the NIT). Moreover, they were few and far between at that time.

Q: What is your view now about the use of experiments in economics?

WN: They are now a critical element to economic research and are particularly useful for filling in gaps where econometric analysis is hard to apply. The audits are a fine example of where we have benefited there.

Q: If your answer to question 2 is different from your answer to question 1, when did you change your opinion and what led you to do so? (If not, please ignore this question.)

WN: I had not realized earlier that they could be done so inexpensively. Part of that, of course, was changes in technology and communications.

Q: To the best of your knowledge, what was Professor Samuelson's opinion about the use of experiments in economics? Did his opinion change over time?

WN: My recollection is that we had a shared view here.

Interview with Richard Lipsey and Alec Chrystal

Q: When you first wrote economics textbooks, in the 1960s and 1970s, what views did each of you hold about the use of experiments in economics? For instance, did you view this method favourably or unfavourably?

RL: I wrote the 1st edition of my textbook between 1960 and 1963 and I don't think I knew much about experimental economics at the time. In so far as I had any view, it would have been favourable. I don't remember any articles that I read on the subject during that time but I do recall mentioning to a colleague that almost as soon as I heard my boss, Lionel Robbins, saying that emotional states were unmeasurable, I

read a piece on how supermarkets used hidden cameras to count eye blinks as an index of the emotional effects of their displays.

AC: The key thing I liked about the Lipsey text as a reader was the emphasis on evidence rather than dogma being the way to go in validating theories. What I helped to add to the book was a set of case studies that illustrate and apply the analytical framework that economics provides, using real data where possible. From the 10th edition onwards (2004) we have included a case study on experimental economics and fairness in the chapter where we discuss utility maximisation etc.....thus raising questions about whether economic man is truly selfish. Experimental evidence seemed important on this. Later we added material on behavioural economics where some of the evidence was also experimentally derived (I think).

Q: What views do each of you hold now about the use of experiments in economics?

RL: I currently hold an entirely favourable view experiments in economics, being particular impressed with those that challenge basic assumptions such as that agents let bygones be bygones, or that they make rational decisions between present and future payoffs, or that utility is solely a function the one's own consumption – and of course there are many more.

AC: I think that experimental methods are important, especially on issues of micro behaviour and personal choice. A key message (I think) is that some well-established assumptions about personal motivation and reactions may be wide of the mark. However, I should add that I am more of a macro economist and experiments are more difficult (and more dangerous!) in this area, though simulation of stylised macro models is close to experimentation, and has an important role. Having spent some time in both HM Treasury and the Bank of England, I should add that the main role of models seems to be to enforce various adding up conditions. Policy makers cannot ask “what if” questions without seeing the full implication of knock-on effects in unexpected parts of the economy. This does not stop politicians from ignoring some undesirable effects or even from taking quick decisions before the downsides can be pointed out. This is not strictly about experiments, but it is about how to evaluate likely outcomes of specific action: You are planning to do X to achieve Y, but actually X may lead to Z. Any tool that helps us to do this better has to be good.

Q: If your answers to question 2 are different from your answers to question 1, when did you change your opinions and what led you to do so? (If not, please ignore this question)

RL: I do not think my opinion changed much, it just got firmer as I read more and more of the evidence from various experiments.

Interview with Ray Fair and Sharon Oster

Q: Professor Fair, you co-authored the first edition of the textbook Principles of Economics in 1989. What was your view at that time about the use of experiments in economics – for instance, did you view this method favourably or unfavourably? Has your view changed since then, and if so when did it change, and why?

RF: I was at Princeton in the early 1970's at the time of the negative income tax experiment, which was run by Mathematica based in Princeton. I followed this fairly closely. I was also aware of Vernon Smith's work.

Q: Professor Oster, you first co-authored the textbook for the 2009 edition. What was your view at that time about the use of experiments in economics? Has your view changed since then, and if so when did it change, and why?

SO: From my earliest time in economics I was aware of the experimental work of Charlie Plott and others at Cal Tech. It seems to me that the range of experimental work has broadened a good deal since then. In our text we discuss experimental work by scholars like John List and Muriel Niederle for example. I have spent much of my career at a business school where experiments are commonly done especially in the area of Marketing, much of it relevant to economics. Our text book certainly has more coverage of experiments than it had thirty years ago! All of the field experiments in the development area for example are now featured in most of the textbooks including our own as are the natural experiments. Lab experiments are also covered in our text but less so.

Q: To the best of your knowledge, what was Professor Case's opinion about the use of experiments in economics? Did his opinion change over time?

RF: Chip Case had a wide range of interests in empirical economics, including experimental.

Interview with Michael Parkin

Q: You published the first edition of your textbook Economics in 1990. At that point, what was your view about the use of experiments in economics? For instance, did you view this method favourably or unfavourably?

MP: As best as I can recall, I didn't have a view about the use of experiments in economics. The term isn't in the index of my first edition. I don't have a copy of the instructor's manual but I would be surprised if it contains ideas on how to use experiments.

Q: What is your view now about the use of experiments in economics?

MP: My view now is that economics experiments done well enable students to experience the application of key principles in an engaging and memorable way. I have used experiments in a limited way. I have sometimes been frustrated by slow Internet connections and had to abandon an experiment and resort to describing what would have happened bandwidth permitting. This experience is from the earlier 2000s. Maybe not a problem today. My favorite experiment is generating a large class demand schedule for pop/bottled water/anything. Use it later to find class price elasticity of demand.

Q: If your answer to question 2 is different from your answer to question 1, when did you change your opinion and what led you to do so? (If not, please ignore this question.)

MP: I don't have a record of my thoughts on experiments so can't give you a date. It was a gradual process. I became a fan of online market experiments around the time Paul Romer created Aplia and encouraged my publisher (then Addison-Wesley) to create experiments for MyEconLab (now renamed MyLab Economics).

Interview with John Sloman

Q: You published the first edition of your textbook *Economics* in 1991. At that point, what was your view about the use of experiments in economics? For instance, did you view this method favourably or unfavourably?

*JS: I viewed the use of experiments highly favourably and introduced some experiments in my teaching. In 1990 I attended a three-day workshop on the use of classroom experiments at the Centre for Experimental Economics at the University of York run by Graham Loomes. In the earlier editions of my books, experiments were not specifically covered, although right from the first edition of *Economics for Business*, we looked at experimental methods in estimating and forecasting (John Sloman and Mark Sutcliffe, *Economics for Business*, Prentice Hall, 1998, Chapter 6).*

Q: What is your view now about the use of experiments in economics?

JS: My enthusiasm has grown for experimental economics and especially for the use of classroom experiments as a means of both engaging students and deepening their understanding.

My work in leading the Economics Network involved running workshops on classroom experiments, having sessions at our biennial conference, having a [specific session on experiments](#) at our annual two-day workshop for new and early careers lecturers, posting [blogs](#) on lecturers' experience of experimental economics and classroom experiments, games and simulations and providing a [range of open-access resources](#) that lecturers can use. Since I retired from the role of Director, the Network continues to promote the use of experiments. I continue to participate with the Network as a Visiting Fellow.

I and my co-authors have increasingly covered behavioural economics in all four books in the context of both demand and business decision making, and within that context we have looked at experiments as a way of establishing and explaining behaviour.

We have introduced experiments in the accompanying resources for the textbooks. For example, there are six single-player experiments on the website accompanying the books (password protected). These are: Market Experiment, Price Floors Experiment, Price Ceilings Experiment, Taxes Experiment, Public Goods Experiment, Lemons Market Experiment. Many of the 200+ case studies available to students have activities which are experimental in nature.

Q: If your answer to question 2 is different from your answer to question 1, when did you change your opinion and what led you to do so? (If not, please ignore this question.)

JS: My opinion has not really changed about the use of experiments. Rather, my enthusiasm has grown as I have experienced both the pedagogical advantages in terms of making learning more active and engaging and the insights it provides into decision making.

Interview with David Colander

Q: You published the first edition of your textbook Economics in 1993. At that point, what was your view about the use of experiments in economics? For instance, did you view this method favourably or unfavourably?

DC: I can't remember specifically the date, but from the beginning I have been a strong supporter of experiments in economics. In the 90s we pushed for behavioral economics, which used experiments, but they were used earlier. In my view experiments don't test for rationality—they test from the utility function of individuals. But my views on experiments don't necessarily reflect what is in the intro text which is designed to teach students certain models and a way of thinking that blends science (formal experiments) with normative judgements that come from philosophical reasoning.

Q: What is your view now about the use of experiments in economics?

DC: I see them as necessary and good. One needs a combination of field, computer, and natural experiments. I sometimes think experimenters draw too strong conclusions from experiments, and don't adequately take into account their limitations. I also favor the use of case studies, which is now discouraged. In terms of introductory teaching however, I think that too much focus on experiments is problematic — and that teaching constrained optimization modelling remains useful.

Q: If your answer to question 2 is different from your answer to question 1, when did you change your opinion and what led you to do so?

DC: They are not different. I have maintained the same view.

Interview with Gregory Mankiw

Q: You published the first edition of your textbook *Principles of Economics* in 1998. At that point, what was your view about the use of experiments in economics? For instance, did you view this method favourably or unfavourably?

GM: I have always had a “big tent” view of economic methodology, and I thought experiments were a useful addition to the economist’s toolbox. I do not cover experiments in particular as a methodology, but they show up in a few places (depending on how exactly one defines experiment). In the Oligopoly chapter, there is discussion of a “Prisoners’ Dilemma Tournament.” In the chapter on Earnings and Discrimination, there is a case study on a field experiment that sent out fake resumes with typically white and black names. In the chapter on Frontiers of Microeconomics, there is a discussion of experiments with the Ultimatum Game. If I recall correctly, that chapter was added in the second or third edition.

Q: What is your view now about the use of experiments in economics?

GM: I do not think my views have changed on this topic.