

Andrej Svorenčik  
Harro Maas *Editors*



# The Making of Experimental Economics

Witness Seminar on  
the Emergence of a Field

 Springer

# The Making of Experimental Economics



Andrej Svorenčák • Harro Maas  
Editors

# The Making of Experimental Economics

Witness Seminar on the Emergence of a Field

 Springer

*Editors*

Andrej Svorenčák  
Department of Economics  
University of Mannheim  
Mannheim, Germany

Harro Maas  
Centre Walras-Pareto d'études  
interdisciplinaires de la pensée  
économique et politique - IEPI  
Université de Lausanne  
Lausanne, Switzerland

ISBN 978-3-319-20951-7

ISBN 978-3-319-20952-4 (eBook)

DOI 10.1007/978-3-319-20952-4

Library of Congress Control Number: 2015947288

Springer Cham Heidelberg New York Dordrecht London

© Springer International Publishing Switzerland 2016

This work is subject to copyright. All rights are reserved by the Publisher, whether the whole or part of the material is concerned, specifically the rights of translation, reprinting, reuse of illustrations, recitation, broadcasting, reproduction on microfilms or in any other physical way, and transmission or information storage and retrieval, electronic adaptation, computer software, or by similar or dissimilar methodology now known or hereafter developed.

The use of general descriptive names, registered names, trademarks, service marks, etc. in this publication does not imply, even in the absence of a specific statement, that such names are exempt from the relevant protective laws and regulations and therefore free for general use.

The publisher, the authors and the editors are safe to assume that the advice and information in this book are believed to be true and accurate at the date of publication. Neither the publisher nor the authors or the editors give a warranty, express or implied, with respect to the material contained herein or for any errors or omissions that may have been made.

Printed on acid-free paper

Springer International Publishing AG Switzerland is part of Springer Science+Business Media (www.springer.com)

# Contents

<b>1 A Witness Seminar on the Emergence of Experimental Economics</b> . . . . .	1
Harro Maas and Andrej Svorenčik	
Introduction . . . . .	1
The Method of the Witness Seminar . . . . .	2
Background to the Witness Seminar on the Experiment in Economics . . . . .	5
Preparation of the Seminar . . . . .	7
Whom to Invite and Why? . . . . .	7
What Topics to Cover and Why? . . . . .	9
How to Prepare for and Organize the Seminar . . . . .	10
The Seminar and Its Results . . . . .	12
The Transcript . . . . .	15
How to Read the Transcript of the Witness Seminar . . . . .	16
References . . . . .	17
<b>2 The Very Beginnings</b> . . . . .	19
Choosing a Dissertation Topic . . . . .	19
Crossing Disciplinary Boundaries . . . . .	22
Early Meetings and Seminars . . . . .	27
Chance Encounters and Conversions . . . . .	28
The JEL-Code and Closeted Experimentalists on the Job Market . . . . .	37
Institutional Settings . . . . .	41
<b>3 The Growth of a Community</b> . . . . .	47
The German Experimental Community . . . . .	47
Caltech, Public Choice, and the “Experimental Bug” . . . . .	50
The Tucson Meetings, the NSF, and Dan Newlon . . . . .	52
Bringing Experiments to a Larger Audience . . . . .	55
Struggle for Acceptance or Standard Battles with Referees? . . . . .	58
Educating Editors . . . . .	60

Amsterdam: Bridging Experimental Cultures . . . . .	63
Internationalization and the Need for a Journal . . . . .	65
A Separate Journal: A Ghetto or a Premium Site to Publish? . . . . .	68
Lowering the Barrier to Entry: Editors and Handbooks . . . . .	71
Dissents on Method . . . . .	73
What Constitutes an Observation? Part I . . . . .	76
Payment and Deception: Spoiling the Subject Pool or Spilling the Beans? . . . . .	77
What Constitutes an Observation? Part II . . . . .	80
Signs of Success: And the Resources It Takes . . . . .	82
<b>4 Funding . . . . .</b>	<b>87</b>
Payment: Tightening Up the Structure of the Model . . . . .	87
Paying the Subjects: Tax Money Spent on Frivolous Things . . . . .	91
How to Fund an Economics Lab? . . . . .	97
Contract Money: The Best Kind of Money? . . . . .	100
Contract Research: Putting Careers at Risk . . . . .	104
Continuities Between the Lab and the World . . . . .	106
The (Missing) Boilerplate in Contracts . . . . .	108
The Experiment as Interface for Arguments . . . . .	110
<b>5 Knowledge and Skills . . . . .</b>	<b>113</b>
Learning from Failures . . . . .	114
Confused Subjects and the Logistics of an Experiment . . . . .	118
Institutional Resistance to Experimental Results . . . . .	121
Isolating Confounding Factors and Learning from Them . . . . .	123
Costly Consequences of Actions: Who Will Pay for the Soufflés? . . . . .	126
Anything You Change Can Make a Difference . . . . .	129
How to Ask an Experimental Question? . . . . .	134
Designing an Experiment Is a Joint Effort . . . . .	136
Learning from Teaching . . . . .	137
Thinking as an Experimenter . . . . .	144
Transferable Skills from Other Fields . . . . .	147
The Questions Depend on the Relevant Audience . . . . .	150
Learning the Theory from Experiments . . . . .	150
Discovering the Sub-game Perfect Equilibrium . . . . .	154
<b>6 Laboratories . . . . .</b>	<b>157</b>
The PLATO System . . . . .	157
Technology Changes the Message Space . . . . .	160
Austin Hoggatt’s Visionary Laboratory . . . . .	162
A Laboratory Is More Than Its Physical Infrastructure . . . . .	164
Space Fights: NASA Pays for Labspace . . . . .	166
Portable Laboratories . . . . .	168
The Management of Laboratories, Software, and Subject Pool . . . . .	170
Lab Funding from an Administrator’s Perspective . . . . .	173
Loosing Control . . . . .	174

**7 History and Future** . . . . . 177

    How to Do Science and How to Name a Society? . . . . . 177

    SEEing Is Believing: Armchairs on Fire . . . . . 178

    An Anomaly Is Just Another Regularity . . . . . 180

    Relics of the Past: And of the Future . . . . . 181

**8 Biographies of Participants** . . . . . 185

**9 Episodes from the Early History of Experimentation  
in Economics** . . . . . 195

    Andreas Ortmann

    Introduction . . . . . 195

    Episode One: The Wallis–Friedman (1942) Critique of the Thurstone  
    (1931) Experiment . . . . . 196

    Episode Two: Morgenstern (1954) on Experiment and Large-Scale  
    Computation in Economics . . . . . 198

    Episode Three: Thomas Juster (1970) on the Possibilities of  
    Experimentation and the Quality of Data Input in the  
    Social Sciences . . . . . 203

    Episode Four: Token Economy and Animal Models  
    for the Experimental Analysis of Economic Behavior  
    (Kagel & Battalio, 1980) . . . . . 206

    Episode Five: Siegel’s Work on Guessing Sequences . . . . . 209

    Concluding Remarks . . . . . 213

    References . . . . . 214

**Erratum to: A Witness Seminar on the Emergence  
of Experimental Economics** . . . . . E1

**Endnotes** . . . . . 219

**References** . . . . . 235



# Chapter 1

## A Witness Seminar on the Emergence of Experimental Economics

Harro Maas and Andrej Svorenčik

*Economists cannot make use of controlled experiments to settle their differences; they have to appeal to historical evidence, and evidence can always be read both ways.*  
“What Are the Questions?” (Joan Robinson, 1977)

### Introduction

On May 28 and 29, 2010, eleven experimental economists gathered at the premises of the Royal Netherlands Academy of Arts and Sciences (KNAW) to participate in a so-called witness seminar on the history of the experiment in economics. The seminar was organized by Harro Maas and Andrej Svorenčik, principal investigator and Ph.D. student on a grant project that was funded by the Netherlands Organisation for Scientific Research (NWO) on the history of observational practices in economics.<sup>1</sup>

Some of the participants, like Vernon Smith and Charles Plott, will not have been surprised by our invitation. Others, like Jim Friedman or John Ledyard, perhaps more so as they did not and do not consider their main contribution to economics to be in experimental economics and may well have thought of someone else as a better candidate. Yet all except one agreed to participate in this event, moderated by the British experimental and behavioral economist Chris Starmer,

---

An erratum to this chapter can be found at DOI 10.1007/978-3-319-20952-4\_10

<sup>1</sup> The witness seminar was made possible by generous funding of the Netherlands Organisation for Scientific Research (NWO), VIDI-research grant 276-53-004. Briefly, the grant project consisted of three subprojects that took sites of observing as its starting point: the observatory, the laboratory, and the armchair. On the observatory and the armchair as sites of observing, see D’Onofrio 2013 and forthcoming; on the armchair see Maas 2011. See also Maas and Morgan 2012.

H. Maas

Centre Walras-Pareto d’Etudes Interdisciplinaires de la pensée économique et politique, IEPI,  
Université de Lausanne, Bâtiment Géopolis, 1015 Lausanne, Switzerland  
e-mail: [Harro.Maas@unil.ch](mailto:Harro.Maas@unil.ch)

A. Svorenčik (✉)

Department of Economics, University of Mannheim, L7 3-5, 68163 Mannheim, Germany  
e-mail: [svorencik@uni-mannheim.de](mailto:svorencik@uni-mannheim.de)

that took place over 2 intensive days in a stately room of the premises of the KNAW.

The seminar was audio and video recorded. These tapes, with all concomitant documentation, have now been deposited in the David M. Rubenstein Rare Book & Manuscript Library at Duke University as part of its ongoing *Economists' Papers Project* and are accessible for scholarly investigation. This book contains the fully edited and annotated transcript, short biographies of the participants, and a slightly revised background paper that Andreas Ortmann wrote in preparation of the seminar. Our introduction presents a discussion of the witness seminar method, an account of our preparations, what we think can be learned from it, and a consideration of its limitations.

## The Method of the Witness Seminar

A witness seminar is a moderated group conversation on a specific topic that was introduced as a method of historical inquiry almost simultaneously at the Wellcome History of Twentieth Century Medicine Group and the Institute for Contemporary British History in the early 1990s.<sup>2</sup> It has been tried at several other places afterward as well, for example, at the Royal Institute of Technology in Stockholm as a tool to uncover the history of IT in Sweden (Lundin, 2009).<sup>3</sup> One of the primary motivations for a witness seminar is to record memories that otherwise will be irrevocably lost.

There is no standardized way to conduct a witness seminar, at least not to date. Witness seminars can be devoted to strictly circumscribed events in place and time, such as the “Winter of Discontent in 1978-1979” (1987) or “Let Us Face The Future: the 1945 Labour General Election Victory” (5 July 1995), both run by the Institute for Contemporary British History, but also seminars on events spanning several decades, such as “The Bretton-Woods Exchange Rate System 1944-72” (1994).<sup>4</sup> In the history of science, technology, and medicine, they have been used as

---

<sup>2</sup> See Tansey's work for useful accounts of the method of the witness seminar. E. M. Tansey initiated and organized (and still does so) the witness seminars at the Wellcome History of Twentieth Century Medicine Group. **Tansey, E. M.** 2008. “The Witness Seminar Technique in Modern Medical History,” H. Cook, A. Hardy and S. Bhattacharya, *History of the Social Determinants of Health*. Orient BlackSwan, 279–95, **Tansey, E. M.** 2006. “Witnessing the Witnesses: Pitfalls and Potentials of the Witness Seminar in Twentieth Century Medicine,” R. E. Doel and T. Söderqvist, *The Historiography of Contemporary Science, Technology, and Medicine: Writing Recent Science*. London; New York: Routledge.

<sup>3</sup> To document the history of IT in Sweden, the science museum in Stockholm had organized two witness seminars that they combined with extensive face-to-face interviews of people involved in that history.

<sup>4</sup> For a complete list of witness seminars organized by the Institute for Contemporary British History at King's College, see <https://www.kcl.ac.uk/sspp/departments/icbh/witness/WitSemscomplete.aspx> [Accessed March 31, 2015]. The Winter of Discontent was a very cold

an entry for a better understanding of scientific discoveries and technological or institutional innovations that similarly may span several years if not decades or as a tool to uncover untapped archival resources. Examples of seminars run by the Wellcome History of the Twentieth Century Medicine Group are “Ashes to Ashes: The History of Smoking and Health” (1995), “Clinical Research in Britain, 1950-1980” (1998), or “Beyond the Asylum: Anti-psychiatry and Care in the Community” (2003).<sup>5</sup>

A witness seminar aims to bring together key participants of an important historical event to obtain a mix of different perspectives that may agree or disagree, but preferably lead to an exchange of memories that feed upon one another in interesting and unexpected ways. Participants not necessarily have to be high-profile individuals—Nobel laureates, politicians, and administrators high up the ladder—though in some cases it is difficult to conceive a seminar without them. The first witness seminar organized by the Wellcome History of Twentieth Century Medicine Group was about the discovery of monoclonal antibodies for which César Milstein, an Argentinean-born scientist, and Georges Köhler, his German postdoctoral fellow, received the Nobel Prize in Physiology and Medicine for 1984 (together with Niels Jerne). The seminar was especially organized to uncover the context around their conscious and controversial decision not to patent their invention (Tansey, 2006).

Clearly, the presence of these two key participants was crucial to the seminar and turned out to be even more so as both died within a few years after the seminar took place, thus preserving information that would have been permanently lost otherwise. But high-profile individuals tend to have been overexposed to the media and so maintain an almost scripted version of past events that make their contributions hardly go beyond what is already known from other (published) sources. The experience from both the Wellcome and Contemporary History seminars is that the communal setting may help to lift such individuals out of their standard grooves, though this of course not always happens (Tansey, 2006).

This is not to suggest, of course, that a witness seminar is intended to establish “the truth” of an event in any sense. Well known are the objections to oral history writing about biased and distorted memories, and such objections hold for the setting of the witness seminar as well and perhaps even more so. Because of the

---

winter in which Britain was haunted by a sustained period of strikes that eventually brought Margaret Thatcher to election victory. The name refers to Shakespeare’s Richard III: “Now is the winter of our discontent/Made glorious summer by this son of York. . . .” See also **Hay, Colin**. 2010. “Chronicles of a Death Foretold: The Winter of Discontent and Construction of the Crisis of British Keynesianism.” *Parliamentary Affairs*, 63(3), 446–70.

<sup>5</sup>The first two were published as **Lock, Stephen; L. A. Reynolds and E. M. Tansey**. 1998. *Ashes to Ashes: The History of Smoking and Health*. Amsterdam; Atlanta, GA: Rodopi, **Reynolds, L. A. and E. M. Tansey**. 2000. “Clinical Research in Britain 1950–1980.” *Wellcome Trust*, 7(7). The third is archived at the Wellcome Library: [http://search.wellcomelibrary.org/iii/encore/record/C\\_\\_Rb1946919\\_\\_SGale,%20Robert%20Peter.\\_\\_P0,3\\_\\_Orightresult\\_\\_X1;jsessionid=CFA74E92884D5DE50E02B3172212F614?lang=eng&suite=cobalt](http://search.wellcomelibrary.org/iii/encore/record/C__Rb1946919__SGale,%20Robert%20Peter.__P0,3__Orightresult__X1;jsessionid=CFA74E92884D5DE50E02B3172212F614?lang=eng&suite=cobalt) [Accessed on March 31, 2015].

communal setting, individuals may feel inhibited rather than encouraged to speak up on specific issues, in which cases recorded pauses and silences may become more informative than what is actually said. Alternatively, the very fact that participants engage in a conversation in which memories start feeding upon one another may lead to a communal group vision that is at tension with published or unpublished (archival) materials. As well argued by Hoddeson, rather than counting against oral histories or against this specific form of oral history, such tensions between oral and archival sources can provide fruitful nods that may lead to a reconsideration of interpretations and meanings that have been given to past events or that may lead to further investigations (Hoddeson, 2006). These may include substantial interpretative revisions of existing archival materials or the search for new archival sources.

This does not mean that commonly pronounced objections against oral history are invalid. But they not only hold against oral history, including the collective setting of a witness seminar. Archives are sometimes demonstrably incomplete or provide an image of events that hinges on the ordering of the archive and so run into the same kinds of difficulties that surround oral history sources. The reordering of the Alfred Marshall archives in Cambridge was also a reordering of the man and his work. The editions of the complete works and correspondence of David Ricardo, John Stuart Mill, or John Maynard Keynes created the persons as much as they conserved them. If an archive of a prominent individual enters special collections fully cataloged, that should be source of worry rather than joy for the historian. There is no reason to hold archival sources for the gold standard of historical evidence, and certainly for contemporary history, writing historians would do wise to cast their net a bit wider than the resources they traditionally have worked with.

This certainly holds for the history of economics where innovations in the documenting and writing of history so far have met with considerable skepticism. Ivan Moscati even went to so far to limit the historian of economics' field of vision to published sources only, thus straitjacketing the topics that can be studied and limiting the audiences that the history of economics might address (Moscati, 2008). This is especially important when it comes to the writing of contemporary history, for which important actors are still with us. Arjo Klamer's *Conversations with Economists: New Classical Economists and Opponents Speak Out on the Current Controversy in Macroeconomics* still stands out as a stellar example of an oral history endeavor that not only conserved but deliberately intervened in current debates (Klamer, 1984).

Within the history of economics especially Roy Weintraub has persistently advocated the use of oral resources to gain access to aspects of the development of the economics discipline that may remain hidden otherwise (Weintraub et al., 2007; Weintraub et al., 1998). Ross Emmett explains that one of the reasons he started his extensive oral history project on the Chicago Economics Department was to modify existing, sometimes self-gratifying stories of the success of the so-called Chicago school of economics (Emmett, 2007). Emmett interviewed individuals on a one-to-one basis. In addition to the great names, he interviewed students who did not finish their Ph.D. or minor economists, administratives, and

other actors who might shed light on the role of funding agencies, institutionalized structures as the so-called Chicago workshop system, or on the interaction between the faculty and students or faculty and (local) government agencies and business.

While George Stigler may have held that the “details of a man’s personal life” only serve to “distort . . . the understanding of scientific work” (Emmett, 2007, p. 172), Emmett’s oral history project confirms contemporary work in history and sociology of science that has convincingly shown that it is exactly such details, broadly taken, that help to understand the development of a discipline, including the individual’s scientific work. The idea that the sources of scientific work are irrelevant to understanding is a figment of the mind, a relic of worn versions of the “view from nowhere” (Nagel, 1986). Historical or methodological studies that merely focus on the context of presentation—the published texts—rather than on the context of research practices simply fall short of contemporary standards in understanding the business of science, and economics is no exception. This pertains also to scientific developments of which the actors are still with us.<sup>6</sup>

## Background to the Witness Seminar on the Experiment in Economics

Our decision to organize a witness seminar on the experiment in economics was certainly motivated by our firm conviction that the history of a discipline does not map to its published sources, but there were other substantial reasons to organize the seminar as well.

The introduction of the experiment in economics produced, by any standards, a major change in the economist’s research practice that goes beyond methods and methodology used. Joan Robinson was not alone in denying the experimental

---

<sup>6</sup> See especially **Doel, Ronald Edmund, and Thomas Söderqvist.** 2006. *The Historiography of Contemporary Science, Technology, and Medicine: Writing Recent Science*. London; New York: Routledge; **Söderqvist, Thomas.** 1997. *The Historiography of Contemporary Science and Technology*. Amsterdam: Harwood Academic.

For very perceptive essays on the problems and issues surrounding the writing of “the history of now,” see **Hughes, Jeff.** 1997. “Whigs, Prigs and Politics: Problems in the Historiography of Contemporary Science.” *The Historiography of Science and Technology*, 19–37; **Lewenstein, Bruce V.** 2006. “The History of Now: Reflections on Being a “Contemporary Archivist””; R. E. Doel and T. Söderqvist, *The Historiography of Contemporary Science, Technology, and Medicine: Writing Recent Science*. London and New York: Routledge; **Weintraub, E. Roy.** 2005. “Autobiographical Memory and the Historiography of Economics.” *Journal of the History of Economic Thought*, 27(1), 1–11, \_\_\_\_\_, 2010; “Breit and Hirsch, Eds., Lives of the Laureates: Twenty-Three Nobel Economists.” *History of political economy.*, 42(4), 779–82, \_\_\_\_\_, 2007. “Economists Talking with Economists, an Historian’s Perspective,” P. A. Samuelson and W. A. Barnett, *Inside the Economist’s Mind: Conversations with Eminent Economists*. Malden, MA: Blackwell Pub., 1–11; **Weintraub, E. Roy and Evelyn L. Forget.** 2007. *Economists’ Lives: Biography and Autobiography in the History of Economics*. Durham; London: Duke University Press.

method to economics. Way up into the 1980s, one could find in Paul Samuelson's and other textbooks statements in which it was emphasized that the specific nature of economic phenomena and the concomitant lack of a dedicated space of experimentation precluded the controlled experiment from the toolbox of the economist. In Samuelson and Nordhaus, we read:

One possible way of figuring out economic laws ... is by controlled experiments. ... Economists [unfortunately] ... cannot perform the controlled experiments of chemists or biologists because they cannot easily control other important factors. Like astronomers or meteorologists, they generally must be content largely to observe. (Samuelson and Nordhaus, 1985, p. 8)

Apart from the fact that Samuelson and Nordhaus subscribe to a distinction between experimentation and observation that is at least problematic (Daston and Lunbeck, 2011; Maas and Morgan, 2012), they made their pronouncement at a time when experimental economists had been practicing their trade, with increasing intensity and in a rapidly growing community of experimentalists, for some 20 years or so.

Around the same time, that is halfway the 1980s, some of the more high-profile members of this growing community started reflecting on the novelty of their endeavors in papers, essays, and handbooks. They started considering the establishment of a separate community of like-minded economists, and they started discussions about the pros and cons of a separate journal for publishing experimental research. At the beginning of this new century, philosophers of (social) science turned toward a systematic investigation of the philosophical and methodological questions experimental economics posed.

Especially Francesco Guala's pathbreaking book on the methodology of experimental economics in 2005 opened up a new and still growing independent field of philosophical and methodological reflection on economic experimentation (Guala, 2005).<sup>7</sup> But Guala was well aware of the limitations of his book. Even though some chapters built on Guala's earlier published work in which he engaged with rich historical case material, he noted that at the time his book was published, no history of the emergence of the experiment in economics existed.

This was still the situation in early 2009, when we decided to organize our witness seminar. By then some good historical work had been published, for example, by Edward Nik-Khah and Kyu Sang Lee, but their work, understandably, zoomed in on particular case studies, such as the FCC auction that had also been studied, from a different angle, by Guala, or on particular individuals, such as the 2002 winner of the Sveriges Riksbank Prize in Memory of Alfred Nobel, Vernon Smith (Lee, 2004; Lee and Mirowski, 2008; Nik-Khah, 2006, 2008).

---

<sup>7</sup> See, for example, Bardsley, Nick; Robin Cubitt; Graham Loomes; Peter Moffatt; Chris Starmer and Robert Sugden. 2010. *Experimental Economics: Rethinking the Rules*. Princeton: Princeton University Press, Santos, Ana Cordeiro dos. 2009. *The Social Epistemology of Experimental Economics*. London: Routledge. Also issues of *Economics and Philosophy* and *Journal of Economic Methodology* are peppered with contributions on the methodological questions posed by the experiment in economics.

Lee was able to study Vernon Smith's work in some detail because Smith had donated his personal archives to the Economists' Papers Project at Duke University. Put somewhat uncharitably, Lee followed the historians' backfall option: go to the archives available, see what is there, and write a good story—and this is not to denounce such work; on the contrary, historians all do it and to a certain extent necessarily so. But for a better understanding of the emergence of the experiment, and the establishment of the laboratory as a dedicated site of inquiry, clearly more was needed than the scholarly work published until then. We had to create our archives rather than to rely on the scarce existing ones.

As we were particularly interested in the changes that the emergence of the experiment and the laboratory as a dedicated site of research had produced in the economists' research practices, a lack of sufficient materials was potentially damaging to this part of our grant project, and thus the potential that a witness seminar might offer in uncovering new or untapped resources was an important motive to push the idea forward. It was of course not clear if such materials, if uncovered, would prove merely helpful in writing a thick description of the emergence of the experiment in economics or if such materials would help pose different kinds of historical and methodological questions that had been asked so far. We hoped to be able to use the witness seminar as a lever for this purpose, and we think it served its purpose well, not only for what we will focus on here, its transcript, but also because it served as an important building block for Andrej Svorenčik's Ph.D. thesis *The Experimental Turn: A History of Experimental Economics* (Svorenčik, 2015).

## Preparation of the Seminar

Several decisions had to be made to move forward. Whom to invite and why? What topics to cover and why? How to practically prepare for and organize the seminar (and when)? And what to expect as a result? We will discuss each of these issues in turn, although, unsurprisingly, they cannot be wholly separated from another. We will devote a separate section to the seminar's results.

## Whom to Invite and Why?

Thesis work of our colleague Floris Heukelom, recently published with Cambridge University Press as *Behavioral Economics: A History*, convincingly showed that the historical trajectories of what are nowadays referred to as experimental and behavioral economics had been very different (Heukelom, 2014). Following Heukelom's work, we decided to importantly restrict the scope of the seminar and so to exclude (the history of) behavioral economics from our considerations. There was an additional reason for this important restriction. We were interested to uncover the history of a new method of research in economics, but we were not interested in battles from



the trenches on theoretical stances, especially not in diverging theoretical stances on rational choice theory. We were afraid (rightly or wrongly) that the inclusion of behavioral economics could lead to discussions on the virtues and vices of rational choice theory, discussions that, as shown by Heukelom, could be separated from discussions on the historical trajectory of the experimental method. Limiting the time horizon roughly to the middle of the 1990s gave an additional rationale to our decision. Thus, to ensure the focus of the witness seminar would be on the method—the experiment in economics—rather than on theoretical considerations about rational choice, we limited our field of vision to experimental economics.

For any conversation on the emergence of the experiment in economics, we considered it inconceivable if the pioneers Vernon Smith, Reinhard Selten, and Charles Plott would not be present. Selten was also important because we wanted to include an international perspective, as even a cursory acquaintance with the history of the experiment in economics shows that there had been a flourishing experimental community of economists in Germany long before there was one in the United States. This community was instigated by Heinz Sauermann in the late 1950s, and Selten was an early and its most prominent member.

With these names fixed, we subsequently included interconnected individuals with different characteristics who could shed light on the different topics we considered that needed to be addressed (on which more below). We also had to address nontrivial considerations such as whether individuals could get on sufficiently well with one another to engage in an open conversation. We thus narrowed down our initial list of 25 names to the 12 individuals we decided to invite to the seminar.

As some of the participants are of high age, there was of course the risk of cancellations. This fortunately did not happen between the moment we sent out the invitations, in May 2009, and the seminar itself, in May 2010. Only Reinhard Tietz had to cancel because of brief ill-health a few days before the seminar. Unfortunately, that reduced the number of European participants to two, turning the seminar into a largely U.S. affair.

Another important decision concerned the choice of the moderator of the seminar. As neither of the organizers is a native speaker of English, we obviously had to look for a native speaker who ideally would have sufficient knowledge of experimental economics, without being him- or herself part of the historical trajectory under discussion. Also, it was a *conditio sine qua non* that the moderator would have sufficient historical sensibilities to focus the contributions of the participants on their historical relevance, without drifting off to an exchange of arguments that might better suit a philosophy or economics seminar. We found ourselves extremely happy that Chris Starmer agreed to play this role, possibly also because Robert Sugden had spoken favorably with Chris about our project, having participated in its opening workshop.<sup>8</sup>

---

<sup>8</sup>“Observation in Natural and Social Sciences, Historically Considered,” opening workshop of NWO project Observation in economics, historically considered, March 12–13, 2009, Hortus Botanicus, Amsterdam.



Chris Starmer had taught economic history with one of the leading British historians of economics, the late Bob Coats, at Nottingham. Because our choice of timeline excluded the separate trajectory of the emergence of the experiment in Britain, Chris Starmer seemed the ideal person to take on this role. Also in this case, nontrivial considerations about his loud and clear voice and his way of addressing fellow participants in a seminar (with which one of us had very positive experiences) enhanced our decision to invite him. Chris agreed on the condition that a sufficient number on our selected list would agree to participate, somewhat assuming, as he told us afterward, that this would never happen. We started our invitations with Vernon Smith, Charles Plott, and Reinhard Selten, and once having their commitment, we proceeded with the rest. Within a few days, all but one invitee had accepted to participate.

## What Topics to Cover and Why?

Nowadays there is a large literature in the history and sociology of science on the experiment and the experimental method.<sup>9</sup> We used some of the more pertinent texts in this literature to identify the areas to be covered at the witness seminar: community building, funding, skills and techniques, and the laboratory as a research site. These topics and the timeline served as a loose grid to choose our cast of characters: participants should be spread over these areas, and there should be sensible relations between participants in terms of pioneers, first-generation Ph.D. students, early presidents of the *Economic Science Association*, first editor of the journal *Experimental Economics*, economists on NSF-committees, etc. We also structured the succession of sessions at the witness seminar along these topics. Below we list the economists we invited ordered by the year of birth (in brackets). Short biographical information can be found in Chap. 8 that indicates our various reasons for inviting them: pioneers, first generation of Ph.D. students, a spread in backgrounds in public choice, game theory, engineering, editors of *Experimental Economics*, members of NSF panels, or other administrative

---

<sup>9</sup>The literature is too vast to cover here. Classic references are **Collins, Harry M.** 1985. *Changing Order: Replication and Induction in Scientific Practice*. London; Beverly Hills: Sage Publications, **Galison, Peter.** 1987. *How Experiments End*. Chicago: University of Chicago Press, \_\_\_\_\_. 1997. *Image & Logic: A Material Culture of Microphysics*. Chicago: The University of Chicago Press, **Knorr Cetina, K.** 1999. *Epistemic Cultures: How the Sciences Make Knowledge*. Cambridge, Mass.: Harvard University Press, **Latour, Bruno and Steve Woolgar.** 1986. *Laboratory Life: The Construction of Scientific Facts*. Princeton, N.J.: Princeton University Press, **Shapin, Steven and Simon Schaffer.** 1985. *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life*. Princeton, N.J.: Princeton University Press.

For a recent reflection on histories of the laboratory, see **Kohler, Robert E.** 2008. "Lab History: Reflections." *Isis*, 99(4), 761–68, and subsequent articles in that focus section of *Isis* for further references. Histories of the experiment and the laboratory have predominantly investigated the experiment/laboratory in the natural sciences, in medicine, and in psychology.

positions. Thus, this list, and our reason for inviting them, also reflects our own rudimentary understanding of the history of experimental economics at the time.

1. Vernon Smith (\*1927)
2. Reinhard Tietz (\*1928)
3. Reinhard Selten (\*1930)
4. Jim Friedman (\*1936)
5. Charlie Plott (\*1938)
6. John Ledyard (\*1940)
7. John Kagel (\*1942)
8. Elizabeth Hoffman (\*1946)
9. Frans van Winden (\*1946)
10. Charlie Holt (\*1948)
11. Stephen J. Rassenti (\*1949)
12. Alvin Roth (\*1951)
13. Chris Starmer (\*1961) (moderator)

## How to Prepare for and Organize the Seminar

To prepare for the seminar itself, we followed several tracks. From our reading in the witness seminars held by the Wellcome History of Twentieth Century Medicine Group and by the Institute for Contemporary British History, we took the idea that it could be useful to increase the historical awareness of the participants by having them read one or two position papers. In the end, and after many fierce discussions among ourselves, we asked Andreas Ortmann to write a short paper in which he would discuss a few “museum pieces” to trigger the participants’ memories and used a comparison between a physics and biology laboratory from the sociologist of science Karin Knorr Cetina’s widely acclaimed *Epistemic Cultures: How the Sciences Make Knowledge* (Knorr Cetina, 1999). Ortmann’s selection covers a wide range of historical routes of experimentation on economic issues and on purpose includes some not performed by economists. A slightly rewritten version of his paper is included as Chap. 9.

Highly consequential for the seminar itself proved the in-depth interviews that Andrej held with all participants separately over the course of the year following the invitation. Apart from participants of the seminar, Andrej interviewed other experimental economists who were close to our invitees in relevant ways (as coauthors, supervisors, students, etc.). By the time of the witness seminar, all participants had been interviewed separately at least once, with an average duration of the interview of around two hours. At present the number of interviews with experimental economists stands at 63, with over 100 hours of recordings in total.

These interviews enabled us to create “thick CVs” that showed the particular strengths of the individual participants, points of contact with the other participants, and unexpected moments and events that we could use to give instructions to Chris Starmer on issues to be covered and whom to give the first word on specific issues.

We sent Chris these CVs a fortnight in advance of the actual seminar for his personal preparation. This does not mean Chris simply followed a list of scripted questions, but it did mean there was an informal list of fallback options to which he could take recourse once a topic was, or seemed, prematurely exhausted. During the breaks at the witness seminar, we used this information to discuss with Chris Starmer the points of entry for the upcoming sessions.

The interviews also helped to stimulate the memories of the participants, and it helped to make them aware of the importance of the archival materials they had stored during their lifetime. Some of the participants granted Andrej access to their personal archives in advance of the seminar; others did so afterward. And as a consequence of the seminar, some participants agreed to hand over their archives (or parts of it) to the *Economists' Papers Project* at Duke University.

There were a great many practical issues that we won't discuss in detail here, even though none of them is trivial. One can think about the choice of hotel, choice of venue for the event itself, choice of recording technology, and seating arrangement—all of them are important also because of budgetary considerations. We choose a great location for the seminar's dinner—so great that we returned far too late back in the hotel. A half a year in advance of the seminar, we invited Chris Starmer to go through these matters on site and to discuss the best way to physically organize the seminar. We divided the seminar itself into six sessions to allow a smooth transition from one topic to another and to incorporate sufficient flexibility to expand on one of them if that seemed appropriate from the enfolding discussion.

We would like to single out one of these practical issues for discussion, however, and that is the choice to have or not to have an audience present at the seminar itself. Both the Wellcome Trust and the Modern History Research Centre seem to be more lenient in this than we considered appropriate. We wanted to ensure all participants would feel as little inhibited as possible to express whatever they wanted to express. We estimated an audience would be a hindrance, especially if this audience would consist of experimental colleagues of the experimental economics center in Amsterdam, CREED. Spouses who were present were neither allowed inside of the seminar room, but attended all other activities.<sup>10</sup> At the same time, we compromised by being present ourselves and also inviting Andreas Ortmann and our historian and philosopher of economics colleague Marcel Boumans to be present—Ortmann because he wrote the position paper and Boumans because we wanted to have feedback on the event from a colleague of the history of economics community. We did not allow either of them to take notes to ensure that the participants would feel their privacy was secured. Clearly, this is an issue on which one can take a different stance, and whichever is taken will be of influence on the unfolding of the seminar itself.

---

<sup>10</sup>The spouses included Marcia Friedman, Martha Ann Talman (wife of Charlie Holt), Harriette Kagel, Marianna Plott, and Candace Smith.

## The Seminar and Its Results

Although we sometimes talked about the witness seminar as an “experiment,” even in our communication with the participants, whatever one considers an experiment, a witness seminar intentionally lacks the controlled intervention of an experiment. There were episodes that came out of Andrej’s one-to-one interviews, of which confirmation during the seminar would have been useful, but it would have been almost a violation of our own intention to only create the conditions in which participants would feel free to express their memories, rather than repeat what we considered their memories should be. The choice of topics, and the advance preparation with Chris Starmer, limited, but did not control, what participants could and would contribute.

This doesn’t mean that we, as organizers, did not intervene in the unfolding of the witness seminar itself. Chris Starmer remembers our emphasis on the use of *surprise* in asking questions, such as asking about examples of *failed* experiments and telling something about them (in the session about “skills and techniques”). Surprise also was an important consideration in how we envisioned the start of the seminar. We considered it imperative that the first question would not be posed to Vernon Smith. And this was not just for the obvious reason that he might give a scripted answer, but that it could possibly confirm other participants in expectations about the seminar. We instructed Chris to ask the first question to Jim Friedman, which would most likely come as a surprise to the participants and thus, we hoped, would unsettle their expectations. We informed Jim Friedman about this briefly before the first session, and we also informed him about the substance of the question, which dealt with his decision to use experiments in his thesis. Chris would then move with a similar question to Reinhard Selten and only then to Vernon Smith. Afterward the floor was open to all participants. A few further remarks on these results are in order.

First, and as already indicated, the seminar helped to tap into a wealth of private resources, some of which are now in the (semi-)public domain, in the *Economists’ Papers Project* at Duke University in particular. Among the papers deposited there due to the witness seminar (and Andrej’s persistence) are the papers of Alvin Roth and Jim Friedman. Archives of several other experimentalists are still being negotiated. There are personal collections of papers Andrej got access to that will be deposited elsewhere, because they had been promised already as in the case of Elizabeth Hoffman’s papers. Second, not in terms of archival materials, the witness seminar pointed us, for example, to unexpected actors such as Austin C. Hoggatt whose laboratory at Berkeley turned out to be an important reference point for Reinhard Selten and Vernon Smith and important for Jim Friedman’s experimental work. Third, the witness seminar made the community of experimental economists

aware of its own history, and this has led to some activity at international conferences.<sup>11</sup>

More important is whether the witness seminar changed and improved our understanding of the history and meaning experiments in economics. In the context of our grant project, we were particularly interested in issues that pertained to what counts as an observation in an economics laboratory setting. We do not intend to fob off the reader with a bromide, if we say the seminar directed us to material practices of control not, or not in sufficient detail, addressed in earlier scholarship. These can be almost trivial, such as making sure experimental subjects should “watch the screen” or that they should be enforced to read their payoffs in the intended direction, as reported by Plott. The seminar also made us realize how intricately interwoven epistemic and social cultures of experimentation are and how important it is to pay attention to the dates of events and episodes.

Smith, Ledyard, Kagel, and Plott are all connected via Purdue, but when Kagel came there at the end of the 1960s, Smith had already left and there was no institutional memory of the experiments Smith had performed in the early 1960s. Even during John Ledyard’s studies at Purdue in the mid-1960s, he did not encounter experiments at all. Thus, Kagel and Smith’s pathways into experimental economics were very different. Friedman, Smith, and Selten are all three connected to Hoggatt, but whereas Friedman’s experiences with Hoggatt made him, more or less, give up on experiments, Smith and Selten were inspired by Hoggatt’s laboratory and adjusted his example to their own needs once they had the opportunity to build a lab for their own. In addition, their experiences highlight that a state-of-the-art technological infrastructure is not enough, and not even a precondition, for the emergence of an experimental culture. Game theory was important for some, but not for all participants to become infected with the “experimental bug,” as Plott put it.

Both Purdue and Caltech were important institutions for the emergence of the experiment in economics, which can also be seen as part of our selection bias of participants. However, the fact that Purdue and Caltech were not like traditional economics departments divided in separate groups (macro, micro, labor, etc.) appeared an important enabling condition for experimenting with experimentation. Carnegie Mellon, commonly less associated with experimental economics, offered Friedman a job,<sup>12</sup> Holt studied there, and Ledyard went there after graduating from Purdue. Just as at Purdue and Caltech, there was an openness to new ways of doing research not found at established economics departments, thus confirming studies in history of science which show that scientific innovation in great many cases comes from the (relative) fringes of a discipline.

---

<sup>11</sup> There has been a joint session at the ASSA between the History of Economics Society (HES) and the Economic Science Association (ESA) on the twenty-fifth anniversary of ESA (2012), a similar session organized by the ESA itself (2012), and another session on experimental economics’ history at ASSA (2013) and the relationship of economics and psychology at ASSA (2015) that were also attended by experimental economists.

<sup>12</sup> Friedman’s Yale classmate F. Trenery Dolbear who also wrote an experimental thesis was offered a job at Carnegie as well, but unlike Friedman he took it.

This should not lead one to conclude the economic mainstream was explicitly hostile to the experiment. On the contrary, the early experiences of Jim Friedman or Alvin Roth in getting their first experimental paper published in journals such as *Econometrica* and *Management Science*, or in receiving research funding (as in Friedman's case), gave them the illusion that their academic lives would be easy. This seemingly contrasts with the shared sense among the first generation of Ph.Ds. trained in experimental economics not to advertise themselves as such in the job market. Seemingly, because at the time there was no sustained separate teaching in experimental economics, and job interviews followed JEL code classifications in which experiments until the mid-1980s did not figure at all. At conferences, experimental papers were placed in educational sessions, which did not contribute to their standing. Publication seemed to become more difficult once the "experimental bug" had spread more widely in the economics community and referee reports began to become much more sophisticated than the "this is an experiment, that's interesting, let's publish it" experience of Roth. Such an experience almost cries for further research. From Svorenčík's subsequent work, acceptance and rejection policies in mainstream journals turn out to be a far more complex story, but it is exactly such further research that, we hope, will be prompted by this witness seminar (Svorenčík, 2015, Chap. 5 in particular). Again, institutional constraints as trivial as the JEL code may inhibit new techniques and methods to catch on. Institutional support, from NSF administrators such as Dan Newlon, market-sensitive editors of major presses, or contract research for companies who searched solutions for concrete problems, provided the necessary funding for subject payments as well as personal and technological infrastructure for the method to survive, stabilize, and grow.

A remark is in order on the relation between theory and data, and the role of the experiment in this relation, which led to some of the most heated discussions at the seminar. If we look at the characteristics of our participants, we see a strong predilection for theory. Most of the participants were theorists by training, more specifically mathematical economists, which was not typical in the 1950s and 1960s when many of them graduated. This may be seen to confirm received ideas that early experiments in economics were predominantly theory driven; intended to test theory. Attitudes to theory testing and how such a test should look like have differed however widely among participants. In addition, the relation between theory and experiment is much less straightforward or one directional than commonly assumed. Selten would not have gotten to the idea of sub-game perfect equilibrium, if it had not been for the experiments he had performed. Friedman left experimentation and turned into a theorist because of a lack of connection between his experiments and the theory he intended to test.

There were other themes explored during the seminar as well, for example, the evolution of the laboratory from the classroom to the virtual space of some of Plott's recent experiments or to the portable laboratories that can transform any laptop to a site for experimentation. Participants easily traced back the economists' emphasis on real incentives, not only to the well-known Friedman-Wallis critique but also to the psychologist Sidney Siegel, which may come as a surprise to most economists. Such themes indicate that the witness seminar not only opened up

research venues that are predominantly of historical interest but also venues that bear on the philosophical and methodological reflections on the meaning and role of the experiment in economics. Thus, the witness seminar invites us to rethink the epistemic and sociological status of the experiment in economics.

## The Transcript

The transcript is preceded with a brief manual on how to read the transcript, the footnotes, and the endnotes. It is important to stress the difference between a spoken and a published text. A literal transcript would be, literally, unreadable. A rough first transcript was made by a professional company after we sampled several agencies to make an example of a transcript, which differed substantially the one from the other. Once we had a rough transcript, Andrej relistened the tapes and watched the videos and revised the text. Then he further edited the transcript by adding extensive footnotes and questions for participants where he deemed necessary, in loose consultation with Harro. This version of the transcript was then sent to the participants for approval and comments. Approval and comments do not mean control. In advance of the seminar, we asked all participants to sign an agreement that the original audio- and videotape, and a transcript of the event, would be made available for scholarly purposes. Approval thus only meant the participants agreed with the transcription, even though they would have phrased things differently on second thought. During the revision process, participants wanted to expand on their points or react to what others said. Such remarks and clarifying additions that were made after the seminar were on our discretion relegated to the endnotes, as explained in the reading guide of the transcript. The participants subsequently granted us permission for the present publication.

In literary studies, the question “what is a text” was and still is an important question, and it is pertinent to the present text as well. We deleted time markers and added subject headings, increasingly moving away from the experience of listening to the original tapes. The text that is published here thus also serves as an invitation to rethink the way we make our historiographical choices. In his introductory words, Chris Starmer mentioned the four partly overlapping themes that were to be discussed during the witness seminar, but only loosely, as he said, because “in many ways, it is for us to create the story for those themes as we go along” (this volume, p. 20).

Starmer’s remark is very true. The text presented will not be the last say on the issues that were on the table, community building, techniques and skills, funding, and the laboratory as a site of research, but rather serves as a start. The text is a collectively created document in which first-person participants jointly reflected on a major event in the economics discipline, the introduction of the experimental method. But it tells only one story, and as Joan Robinson reminds us, history can be read in different ways. Thus, the transcript may serve as an entry to a fuller understanding of this episode in the history of economics and to the understanding

of the experiment as a method of social inquiry. Hopefully, this volume will be an incentive to create other stories that need to be told.

## How to Read the Transcript of the Witness Seminar

The recording of the two days of the witness seminar lasts seven hours and fifteen minutes. This includes small parts made during breaks, which were not transcribed. The recording was originally transcribed in a verbatim fashion with some grammar editing. Then the recording was listened to again and the transcript was pruned of hundreds of filler words such as—ehm, uhm, well, I think, and, you know, kind of, I guess, I mean, of course, okay, etc. Whenever such words were used as qualifiers reflecting a state of uncertainty, confirmation of what others said, or groping in one's memory, they were retained. Further grammar errors were remedied and some stylistic changes to transform spoken word to written were introduced (e.g., breaking up long sentences, word order, nested ideas rearranged, etc.) while maintaining individual speaker's tone and authenticity of the transcript. Some additions to help the reader were made—missing subjects, unfinished thoughts—all inserted in square brackets [].

**Sounds**—most sounds were omitted in the transcript (coughs, tearing of paper, etc.)—all very infrequent. Hitting the desk, snapping fingers, and particularly laughs were retained—and recorded in several forms according to intensity—*laughter*, *some laughter*, and *group laughter*. There were only a handful of pauses made during the seminar. All are noted and inserted in square brackets [].

**Speakers**—are introduced by their full names. Two Johns and Charlies were present, and to avoid confusion, their surnames are inserted in square brackets whenever they are mentioned by name.

**Paragraphs**—long monologues are broken into paragraphs, which only infrequently reflect pauses made by the speakers.

**Cross talk**—In cases when someone made a short insertion, this is indicated in the text of the other person—in square brackets [].

**Names**—many people were mentioned during the seminar. Most have a short biographical entry on the following site: <http://www.springer.com/de/book/9783319209517>. Names that appear in the biographical appendix are in italics.

**Footnotes**—are used for three purposes. (1) They contain bibliographic references for papers referred to in the seminar. (2) They furnish details on alluded historical events



and substantial issues. (3) They are answers to editor's follow-up questions. Continuous Arabic numbering is used.

**Endnotes**—include comments of participants made on the transcript. Continuous Roman numbering is used.

## References

- Bardsley, Nick; Robin Cubitt; Graham Loomes; Peter Moffatt; Chris Starmer and Robert Sugden. 2010. *Experimental Economics: Rethinking the Rules*. Princeton: Princeton University Press.
- Collins, Harry M. 1985. *Changing Order: Replication and Induction in Scientific Practice*. London; Beverly Hills: Sage Publications.
- Daston, Lorraine and Elizabeth Lunbeck. 2011. *Histories of Scientific Observation*. Chicago: University of Chicago Press.
- Doel, Ronald Edmund and Thomas Söderqvist. 2006. *The Historiography of Contemporary Science, Technology, and Medicine: Writing Recent Science*. London; New York: Routledge.
- Emmett, Ross B. 2007. "Oral History and the Historical Reconstruction of Chicago Economics." *History of Political Economy*, 39, 172–94.
- Engvall Siegel, Alberta. 1964. "Sidney Siegel: A Memoir," S. Messick and A. H. Brayfield, *Decision and Choice. Contributions of Sidney Siegel*. New York McGraw-Hill Book Co, 1–23.
- Galison, Peter. 1987. *How Experiments End*. Chicago: University of Chicago Press.
- \_\_\_\_\_. 1997. *Image & Logic: A Material Culture of Microphysics*. Chicago: The University of Chicago Press.
- Guala, Francesco. 2005. *The Methodology of Experimental Economics*. Cambridge; New York: Cambridge University Press.
- Hay, Colin. 2010. "Chronicles of a Death Foretold: The Winter of Discontent and Construction of the Crisis of British Keynesianism." *Parliamentary Affairs*, 63(3), 446–70.
- Heukelom, Floris. 2014. *Behavioral Economics: A History*. Cambridge University Press.
- Hoddeson, L. 2006. "The Conflict of Memories and Documents: Dilemmas and Pragmatics of Oral History," R. E. Doel and T. Söderqvist, *The Historiography of Contemporary Science, Technology, and Medicine: Writing Recent Science*.
- Hughes, Jeff. 1997. "Whigs, Prigs and Politics: Problems in the Historiography of Contemporary Science." *The Historiography of Science and Technology*, 19–37.
- Klamer, Arjo. 1984. *The New Classical Macroeconomics: Conversations with the New Classical Economists and Their Opponents*. Brighton: Wheatsheaf.
- Knorr Cetina, K. 1999. *Epistemic Cultures: How the Sciences Make Knowledge*. Cambridge, Mass.: Harvard University Press.
- Kohler, Robert E. 2008. "Lab History: Reflections." *Isis*, 99(4), 761–68.
- Latour, Bruno and Steve Woolgar. 1986. *Laboratory Life: The Construction of Scientific Facts*. Princeton, N.J.: Princeton University Press.
- Lee, Kyu Sang. 2004. "Rationality, Minds, and Machines in the Laboratory a Thematic History of Vernon Smith's Experimental Economics," University of Notre Dame, 301.
- Lee, Kyu Sang and Philip Mirowski. 2008. "The Energy Behind Vernon Smith's Experimental Economics." *Cambridge Journal of Economics*, 32(2), 257–71.
- Lewenstein, Bruce V. 2006. "The History of Now: Reflections on Being a "Contemporary Archivist"," R. E. Doel and T. Söderqvist, *The Historiography of Contemporary Science, Technology, and Medicine: Writing Recent Science*. London; New York: Routledge.
- Lock, Stephen; L. A. Reynolds and E. M. Tansey. 1998. *Ashes to Ashes: The History of Smoking and Health*. Amsterdam; Atlanta, GA: Rodopi.

- Lundin, Per. 2009. "From Computing Machines to IT: Collecting, Documenting, and Preserving Source Material on Swedish IT-History." *History of Nordic Computing*, 2, 21–3.
- Maas, Harro and Mary S. Morgan. 2012. *Observing the Economy: Historical Perspectives*. Durham; London: Duke University Press.
- Moscatti, Ivan. 2008. "More Economics, Please: We're Historians of Economics." *Journal of the History of Economic Thought*, 30, 85–91.
- Nagel, Thomas. 1986. *The View from Nowhere*. New York: Oxford University Press.
- Nik-Khah, Edward. 2008. "A Tale of Two Auctions." *Journal of Institutional Economics*, 4(1), 73–97.
- \_\_\_\_\_. 2006. "What the FCC Auctions Can Tell Us About the Performativity Thesis." *Economic Sociology–European Electronic Newsletter*, 7(2), 15–21.
- Reynolds, L. A. and E. M. Tansey. 2000. "Clinical Research in Britain 1950–1980." *Wellcome Trust*, 7(7).
- Robinson, Joan. 1977. "What Are the Questions?" *Journal of Economic Literature*, 15(4), 1318–39.
- Samuelson, Paul A. and William D. Nordhaus. 1985. *Economics*. New York: McGraw-Hill.
- Santos, Ana Cordeiro dos. 2009. *The Social Epistemology of Experimental Economics*. London: Routledge.
- Shapin, Steven and Simon Schaffer. 1985. *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life*. Princeton, N.J.: Princeton University Press.
- Söderqvist, Thomas. 1997. *The Historiography of Contemporary Science and Technology*. Amsterdam: Harwood Academic.
- Svorenčík, Andrej 2015. *The Experimental Turn: A History of Experimental Economics*. Ph.D. dissertation. University of Utrecht.
- Tansey, E. M. 2008. "The Witness Seminar Technique in Modern Medical History," H. Cook, A. Hardy and S. Bhattacharya, *History of the Social Determinants of Health*. Orient BlackSwan, 279–95.
- Tansey, E. M. 2006. "Witnessing the Witnesses: Pitfalls and Potentials of the Witness Seminar in Twentieth Century Medicine," R. E. Doel and T. Söderqvist, *The Historiography of Contemporary Science, Technology, and Medicine: Writing Recent Science*. London; New York: Routledge.
- Weintraub, E. Roy. 2005. "Autobiographical Memory and the Historiography of Economics." *Journal of the History of Economic Thought*, 27(1), 1–11.
- \_\_\_\_\_. 2007. "Economists Talking with Economists, an Historian's Perspective," P. A. Samuelson and W. A. Barnett, *Inside the Economist's Mind: Conversations with Eminent Economists*. Malden, MA: Blackwell Pub., 1–11.
- \_\_\_\_\_. 2010. "Breit and Hirsch, Eds., Lives of the Laureates: Twenty-Three Nobel Economists." *History of Political Economy*, 42(4), 779–82.
- Weintraub, E. Roy and Evelyn L. Forget. 2007. *Economists' Lives: Biography and Autobiography in the History of Economics*. Durham; London: Duke University Press.
- Weintraub, E. Roy; Stephen J. Meardon; Ted Gayer and H. Spencer Banzhaf. 1998. "Archiving the History of Economics." *Journal of Economic Literature*, 36(3).