

Debate article

Reply to Knutsen and Sjögren

Increased International Integration is a Prerequisite for More and Better Research in Economic History

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In the article “Is Swedish Research in Economic History Internationally Integrated?” (*SEHR* 2005:2), I present new facts on the past international publications and conference participation activity of Swedish economic historians. In contrast to claims made in a recent large public investigation, my data show that Swedish economic historians have not published extensively in international journals, or books, in recent years. This can in part be explained by the custom to write predominantly monographs, to write mostly in Swedish, and to use hardly any quantitative methods or theory-based economic analysis. Naturally, I am well aware that there may also be some other factors at work, and that empirical investigations of this kind are always open to objections. Problems regarding sample selection, variable definitions and so forth cannot be avoided, and to focus mainly on journal article publications in a field where books and anthologies play an important role raises some concern.¹ However, my article does not advocate any methodological dogmatism and acknowledges that economic history research can be conducted and presented in many different ways, using several different methodologies. The important thing is to recognise that there is great potential in combining such an open-minded methodological attitude with an active interest and participation in the research that appears in the many international peer-reviewed journals. This would not be to import some foreign (American) methods or views of the field so much as trying to revive the true Swedish economic history in the spirit of Eli F. Heckscher. In my view, this is the most consistent strategy to ensure both more and better future Swedish research in economic history.

Source criticism is fundamental, but what does it really mean?

In a comment on my article, Sverre Knutsen forwards some interesting criticisms.² First, he is concerned that my calls for more of statistics and economics signal that I do not appreciate sufficiently the merits of the source-critical method (p. 82). However, I regard methodological awareness a *fundamental* requisite for all kinds of empirical research, historical or contemporary, qualitative or quantitative. Source criticism is a crucial part of this awareness, recognised as the main tool for dealing with problems of, e.g., dependency and reliability of primarily qualitative sources.³ However, it is not in itself a sufficient tool in the economic historian’s toolbox. In many situations, approaching quantitative sources requires a critical scrutiny of the numbers used, especially how they were collected and interpreted.⁴ Moreover, the *statistical*

¹ See, e.g., the discussion in my article on these issues relating to the works by Diana Hicks and others.

² To save space, I leave out Knutsen’s more general discussions about the current state of economic history (Knutsen, Sverre, What Status Should Clio have in Economic History?, *Scandinavian Economic History Review*, vol. LIII, 2005:2, 81–86)

³ For a more detailed characterisation of source criticism, see, e.g., Jarrick, Arne and Söderberg, Johan, *Praktisk historieteori*, Almqvist & Wiksell, Stockholm, 1993.

⁴ For a fine example of “quantitative source criticism”, recall the Nye-Irwin debate about the nineteenth-century trade policies of Great Britain and France (Nye, John V., The Myth of Free Trade Britain and

techniques used when analysing these numbers must also be treated with scepticism. As is well known, statistical estimates are quite sensitive to empirical applications, and it is crucial that they correspond with the structure and contents of data.⁵ To be able to assess critically such research, the reader must have gone through substantial training in quantitative methods (read: higher level statistics and econometrics courses) and, when dealing with economically relevant issues, often also economic theory. Of course, this does not diminish the importance of also being well trained in qualitative research methods. The point is simply that today's economic historians active internationally must be familiar with many empirical methodologies in order to be critical readers and competent producers of any research, and this is something that the current Swedish undergraduate and graduate programmes in economic history cannot provide them with adequately.

Economic theory needs economic history – and vice versa

In his second point, Knutsen opposes my argumentation that more of modern economic analysis would be beneficial for Swedish economic history. Knutsen states that neo-classical economics “pays no attention to institutional, cultural and sociological factors in shaping economic behaviour” (p. 82) and is therefore not “capable of giving an overall explanation of historical processes” (p. 85). Beyond doubt, neoclassical economics is not without flaws, although one should also recognise that in recent years it has developed to the better by opening up and allowing dynamic and institutional factors to play a role.⁶ To explain better what I meant, let me point to three recent examples of how economic historians have used modern economics, and sometimes econometrics, without losing contact with contextual or source-critical practices.

In their book on French Early Modern credit market institutions, Phil Hoffman et al. integrate two hundred years of archival evidence of lending contracts with modern contract theory and a considerable portion of institutional detail to challenge the conventional notion that well-functioning capital markets did not exist outside the Netherlands and Great Britain before the industrial revolution.⁷ Peter Temin and Hans-Joachim Voth study the emergence of stock price bubbles in London in 1720, i.e., at the time of the South Sea Bubble.⁸ Using a unique dataset collected from the archives of a

Fortress France: Tariffs and Trade in the Nineteenth Century, *Journal of Economic History*, vol. LI, 1991:1, 23–46; Irwin, Douglas A., Free Trade and Protection in Nineteenth-Century Britain and France Revisited: A Comment on Nye, *Journal of Economic History*, vol. LIII, 1993:1, 146–152, and Nye, John V., Reply to Irwin on Free Trade, *Journal of Economic History*, vol. LIII, 1993:1, 153–58).

⁵ A great example of this kind of quantitative and statistical source criticism is a current debate about the quality of instruments and regression models used by Acemoglu and co-authors to control for the endogeneity of institutions in growth regressions (Acemoglu, Daron, Johnson, Simon and Robinson, James A., “The Colonial Origins of Comparative Development: An Empirical Investigation, *American Economic Review*, vol. XCI, 2001:5, 1369–1401 and by the same authors: A Response to Albouy’s “A Reexamination Based on Improved Settler Mortality Data, mimeo, MIT, and, finally, Albouy, David, The Colonial Origins of Comparative Development: A Reexamination Based on Improved Settler Mortality Data, mimeo, University of California at Berkeley).

⁶ See, e.g., Greif, Avner, Cliometrics after Forty Years, *American Economic Review Paper and Proceedings*, vol. LXXXVII, 1997:2, 400–403 and Myerson, Roger, Nash Equilibrium and the History of Economic Theory, *Journal of Economic Literature*, vol. XXXVII, 1999:3, 1067–1082.

⁷ Hoffman, Philip T., Postel-Vinay, Gilles and Rosenthal, Jean-Laurent, *Priceless Markets. The Political Economy of Credit in Paris, 1660–1870*, University of Chicago Press, Chicago, 2000. The book serves as a good example, I think, of how the writing of books can be combined with the publishing of journal articles. Apart from the book itself, the authors have also published five joint articles in peer-reviewed journals and some additional book chapters on the same issues.

⁸ Temin, Peter and Voth, Hans-Joachim, Riding the South Sea Bubble, *American Economic Review*, vol. XCIV, 2004:5, 1654–1668.

goldsmith banker, the authors can reconstruct the daily trading patterns of the actors and thereby evaluate modern market microstructure theories, as well as make instructive comparisons with more recent bubbles. Finally, in his seminal work on institutions in history, Avner Greif uses a (non-technical) game theory model to explain how cultural, religious and historical institutions enabled medieval long-distance trading in the Mediterranean both to emerge and to last, despite the absence of formal legal institutions for penalising overseas defectors.⁹

Hence, when used in these pragmatic and contextual ways, economic models can both add considerable value to the historical analysis and be enriched by it. Of course, these models should never be used out of context, which could lead to research “embodying its conclusions in its assumptions”, as Charles P. Kindleberger warns against.¹⁰ Again, however, my argument is not that economic historians should convert to macroeconomics or game theory. The emphasis is rather on the importance that *economic* historians are able to understand contemporary economics and avoid getting stuck with atheoretical approaches or, sometimes, outdated theories.

Clarifying some misunderstandings

In this final section, I discuss all “ten examples of misunderstanding and false information” that Hans Sjögren specifies in his reply.¹¹

Sjögren’s first point is a claim that my article lacks any “reservations for the fact that the top-ranking system of journals within economics hardly fits economic historians” referring to the EEA ranking of three top journals. This complaint is unfounded, since I in fact make several explicit reservations when discussing my grouping of journals, especially in the case of the top-ranked journals. I state that my selection of “top-4” journals “relies on an economics-based ranking and using a more history-related ranking (if one exists) could well include other journals (but also most of the ones in the ‘top-4’)”.¹² Moreover, I amend the *JEEA* ranking by including a fourth journal and discuss the sensitivity of journal rankings in the context of using the Social Science Citation Index (SSCI) databases.¹³ Finally, the analysis includes 14 other international economic history journals, spanning several sub-fields, so that the assessment is broadened. Next Sjögren believes me to have omitted counting “Edited books” by Swedish professors (Table 3, p. 59). This is, however, not the case, since I count the authorship of the introductory chapter in an edited volume as a book chapter publication, almost invariably written by the editors themselves. Hence, “Edited books” appear in Table 3 under *Chapters in books*, in classes 2a, 2b or 2c.

Sjögren’s second claim is that “Waldenström disqualifies articles in special issues, by arguing that they have been written by invitation” (p. 78). Again, this is an unwarranted claim. My paper *distinguishes* between the two types of articles, which is straightforward, considering that they are actually not identical types of publications. It discloses all numbers for both invited and non-invited articles in the main table (Table 1, p. 56) and also discusses both cases throughout the analysis. In the cases when data did not allow me to separate the two types from each other (Table 2, p. 57, and Table 3,

⁹ See, e.g., Greif, Avner, Reputation and Coalitions in Medieval Trade: Evidence on the Maghribi Traders, *Journal of Economic History*, vol. XLIX, 1989:4, 857–882.

¹⁰ Kindleberger, Charles P., *Historical Economics: Art or Science?*, University of California Press, Berkeley, 1990, p. 25.

¹¹ Sjögren, Hans, Do Not Write Thick Books in Swedish, Please! *Scandinavian Economic History Review*, vol. LIII, 2005:2, 78–80.

¹² Also note that I always put quotations around the “top-4” group, underlining its tentative nature.

¹³ See Waldenström, Is Swedish Research, p. 54, fn 15.

p. 59), I even chose to present the numbers *including* invited articles. Furthermore, as one of the footnotes states, “Invited articles in special issues can at times be preceded by more restrictive and critical selection procedures than non-invited articles” (footnote 5, p. 52). This is hardly to “disqualify” invited articles. In fact, similar examinations of research output, made for other disciplines, have excluded invited articles in special issues altogether, which actually indicates that my approach leans towards the generous side.¹⁴

Thirdly, Sjögren calls my coverage of international conferences “another conscious mistreatment” (p. 78). He first argues that the different length of time windows across conference types indicates a biased sample selection.¹⁵ Since my data were collected “to span both general meetings and most sub-fields, both in the Americas and Europe” (p. 59) and to get as much data as possible from reasonably available (internet) sources, however, the variation in time frames was not deliberate, but a function of data availability. Besides, in his specific example I have a hard time conceiving how the time frames can bias the results in the way Sjögren suggests. The Cliometric Society meetings are triennial, contributing three observations in total, whereas those of the EBHA are annual, also totalling three observations.

Sjögren also states that cliometrics conferences and the EHA and EBHS meetings are overrepresented and that I deliberately exclude SHOT (Society for the History of Technology) and business history conferences. It is difficult to speak about overrepresentation, unless one is knowledgeable about the whole population of international conferences. My sample consists of 23 conferences and 1811 presented papers (Table 4, p. 60), and Table A presents some additional descriptive statistics on their coverage. Starting with subject coverage, “General economic history” represents 44 percent of all meetings and 30 percent of all papers, whereas “Business History” represents 42 percent and 43 percent, and “Historical economics” 14 percent and 26 percent, respectively. Hence, these summary statistics do not suggest that my sample contains too little business history. Finally, Sjögren claims that I exclude Swedish and Nordic international conferences. This is also wrong, since I list the 2002 EBHA congress in Helsinki, Finland.

But Sjögren has a point in that I did not include the SHOT conferences in my sample. To correct for this lapse, I collected data for two of the annual SHOT conferences, 2004 and 2005, shown in Table A. They exhibit an even smaller representation of Swedish economic historians than my original sample. In 2004, there was only one paper by a Swedish economic historian out of 156 papers altogether ($1/156 = 0.6$ percent) and in 2005 there were none (zero percent). Hence, not even when I extend my sample coverage to include the conferences Sjögren himself points out, does the picture change.

Fourthly, Sjögren argues that Table 2 shows that Swedish economic historians “outperform larger countries such as Germany and Canada” (p. 79) in terms of publications in business history. As a matter of fact, the table shows that Canadians wrote 20 articles in business history journals (10 in *Business History Review (BHR)* + 10 in *Business History (BH)*), and the Germans wrote 14 (2 + 12) while Swedes wrote

¹⁴ See, e.g. Shane, Scott A., Who is Publishing the Entrepreneurship Research?, *Journal of Management*, vol. XXIII, 1997:1, 83–95: “To ensure that the study focused on peer reviewed research, only articles and research notes that were non-invited were included. Therefore, excluded from this analysis are all editor’s notes, cases, special issues in which papers were invited, and any other articles for which there was an indication that the article was not refereed.” (p. 86).

¹⁵ The first Cliometrics meeting in my sample took place in 1997, whereas the first European Business History Association meeting was in 2002.

11 (0 + 11).¹⁶ However, Swedes did outperform scholars from other large countries, such as Spain (1 + 6), France (2 + 1) and Italy (1 + 2).

As for Sjögren's fifth claim that "American journals in particular favour articles based on data and topics relevant for the American audience" (p. 79), I redirect the question to the editors of these "American journals".

Sixthly, Sjögren criticises my comparison of the productivity of U.S. and Swedish economic historians because it mentions only U.S. economics departments and thus disregards all economic historians at history departments or business schools. Here Sjögren has a good point, and I should have addressed this issue in my article, along with some related problems.¹⁷ However, the important question is whether such an addition would change my basic finding that Americans are much more active than Swedes in publishing in international economic history journals. In the absence of relevant data it is tough to assess exactly the input of U.S. and Swedish economic historians. As for the United States, anecdotal evidence suggests that about half of the active economic historians work outside the economics departments.¹⁸ Hence, my count of the economic historians active at the 50 largest U.S. economics departments, resulting in 86 scholars, should be multiplied by a factor of at least 2, giving $86 \cdot 2 = 172$. An even better estimate is perhaps the membership of the Economic History Association, said to be between 900 and 1000 (according to Alex Field, Santa Clara University in November 2004), despite the fact that several of these are non-U.S. scholars or non-researchers. As for the Swedish population of economic historians, there were 123 individuals with a Ph.D. at the Swedish departments (in late 2004). Adding all graduate students (117 in 2003) and, to achieve a comparable number with the EHA membership, everyone outside Swedish economic history departments, we get a Swedish number of roughly 300–400. This gives a size, or input, relation of 3:1, which could be approximately correct.

Turning to the relation of output, defined here as the number of articles in the six large journals listed in my Table 2, the number is 90:1 for the "top-4" economic history journals, 18:1 for the two business history journals (with all the Swedish articles being invited articles, though), and a weighted average of 50:1.¹⁹ Hence, U.S. economic historians, including everyone outside economics departments, have been between 6 and 30 times more productive than their Swedish colleagues in producing articles for the six major international economic and business history journals during 1986–2004.

On Sjögren's seventh remark regarding my attitude towards source criticism, see my previous discussion of Knutsen's similar remark.

Sjögren's eighth point is a question about why I neglect the role of Eli F. Heckscher when describing Swedish economic history as founded by "people from the history discipline" (p. 64). Well, since I am no expert on this matter I merely referred to

¹⁶ Please note the unfortunate mix-up of the titles *BHR* and *BH* in Table 3, which should be transposed.

¹⁷ What Sjögren does not mention, but which is another problem with my comparison, is that linking the number of current researchers to output numbers from past years may be misleading if there have been large differences in recruitment patterns over the last years. As for now, however, I lack data for both the United States and Sweden to be able to control for this possibility.

¹⁸ Looking in the Social Science Citation Index at the 506 U.S. articles published during 1986–2004 in the *Journal of Economic History*, 306 (60 percent) had at least one author from a U.S. economics department, 64 (13 percent) had at least one author from a U.S. history department and 136 (27 percent) had authors affiliated elsewhere. When also including *Explorations in Economic History* and *Economic History Review* and the two business history journals *BHR* and *BH*, the share of U.S. economics departments decreases to 47 percent whereas the share for U.S. history departments is unchanged (12 percent).

¹⁹ If one would exclude the *EREH*, the numbers would instead be 260:1, 18:1 and 70:1, respectively.

other Swedish scholars who are. For example, Professor Ulf Olsson writes: “[W]hen the subject of economic history was becoming established at Swedish universities around 1950, the Heckscher tradition with its clear links with economics or political economy was broken to a large extent. The new subject emerged instead as an offshoot from the subject of history”.²⁰ Björn Hettne writes in his book on economic history in Sweden that the Swedish economic historians who succeeded Heckscher disliked his focus on economic theory and relaxed attitude towards source criticism and marginalised him.²¹ In fact, Heckscher is today generally associated with historical economics and cliometrics, not with the historical source-critical tradition that has dominated Swedish economic history for so long.²²

Ninthly, Sjögren thinks that I “discredit the academic community” by stating that Swedish economic historians over the years have enjoyed a relatively favourable situation regarding research funding, and that this has relieved it of the necessity of showing evidence of being internationally competitive. But I rely only on information coming from the public evaluation of Swedish economic history, Ulf Olsson’s historical remarks and figures from the departments of economic history and economics.²³ These three separate sources describe the field as having enjoyed “relatively much financing” and “developed considerable expertise in applying for external funds”. Naturally, a favourable financing record also reflects other things, including high quality of research.

Finally, Sjögren’s last point underlines that writing monographs in Swedish does not imply that one is incapable of writing journal articles in English. I definitely agree. Some research simply does not fit into a 25-page format, and writing in Swedish is sometimes important and instructive. My argument is only that it would be beneficial to the field as a whole if Swedish scholars would spend more (though not all) of their available time writing research in the form of short papers in other languages, preferably English, and then submitting them to international journals and conferences. This would benefit both themselves, through better feedback and international recognition, and the field as a whole.

²⁰ Olsson, Ulf, *Fluctuat nec mergitur: economic history in Sweden at the turn of the century 2000*, *Scandinavian Economic History Review*, vol. 50, 2002:3, 68–82, p. 69.

²¹ Hettne, Björn, *Ekonomisk historia i Sverige. En översikt av vetenskaplig produktion och forskningsinriktning*, Lund: Tryckbanken, 1980, pp. 60, 68.

²² Henriksson, Rolf G., Eli F. Heckscher: the economic historian as economist, *The History of Swedish Economic Thought*, Ed. B. Sandelin, Routledge, London and New York, 1991, and Hettne, *Ekonomisk historia i Sverige*, pp. 63–64.

²³ See *Utvärdering av ämnet ekonomisk historia vid svenska universitet*, Stockholm: Swedish Agency for Higher Education (Högskoleverket), 2003, pp. 47–48, 81, 95, and Olsson, *Fluctuat nec mergitur*, pp. 69, 72. The main problems with getting data from departments are that they are not compiled and that they come from both internal university funds (*fakultetsanslag*) and external research funds (*externa anslag*).

Table A: Conference coverage in Waldenström (SEHR 2005:2), and author distributions at recent SHOT conferences.

	General economic history	Historical economics	Business History	Total			
Counting conferences							
<i>In Europe</i>	2 (20%)	5 (50%)	3 (30%)	10 (100%)			
<i>In the Americas</i>	5 (38%)	1 (8%)	7 (54%)	13 (100%)			
<i>Total</i>	7 (30%)	6 (26%)	10 (43%)	23 (100%)			
Counting participants							
<i>In Europe</i>	186 (25%)	209 (28%)	343 (46%)	738 (100%)			
<i>In the Americas</i>	607 (57%)	44 (4%)	422 (39%)	1073 (100%)			
<i>Total</i>	793 (44%)	253 (14%)	765 (42%)	1811 (100%)			
			Other Swedish*	Other European	Others*	All	
SHOT conference, Amsterdam, Oct. 2004			1	6	50	95	156
SHOT conference, Minneapolis, MN, Nov. 2005			0	3	30	118	152

Notes: I classify the conferences (in my Table 4, p. 60) as “General economic history” (EHA and EHS meetings), “Historical economics” (Cliometrics society, EHES meetings), and “Business History” (BHC, EBHA and EBHS meetings). * denotes that “Swedish” refers to Swedish economic historians only, while “Others” includes the few Swedish presenters affiliated elsewhere (mostly at engineering schools).