ECONSTOR
Make Your Publications Visible.

A Service of
ZBW
Leibniz-Informationszentrum Wirtschaft
Leibniz Information Centre for Economics

Hamermesh, Daniel S.

Working Paper

Replication in Labor Economics: Evidence from Data, and What It Suggests

IZA Discussion Papers, No. 10403

Provided in Cooperation with:
IZA – Institute of Labor Economics

Suggested Citation: Hamermesh, Daniel S. (2016) : Replication in Labor Economics: Evidence from Data, and What It Suggests, IZA Discussion Papers, No. 10403, Institute of Labor Economics (IZA), Bonn

This Version is available at:
https://hdl.handle.net/10419/161026

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

WWW.ECONSTOR.EU

Mitglied der Leibniz-Gemeinschaft

I Z A Institute
of Labor Economics
Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 10403

Replication in Labor Economics: Evidence from Data, and What It Suggests

Daniel S. Hamermesh

DECEMBER 2016

I Z A Institute
of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 10403

Replication in Labor Economics: Evidence from Data, and What It Suggests

Daniel S. Hamermesh
Royal Holloway University of London
and IZA

DECEMBER 2016

Any opinions expressed in this paper are those of the author(s) and not those of IZA. Research published in this series may include views on policy, but IZA takes no institutional policy positions. The IZA research network is committed to the IZA Guiding Principles of Research Integrity.

The IZA Institute of Labor Economics is an independent economic research institute that conducts research in labor economics and offers evidence-based policy advice on labor market issues. Supported by the Deutsche Post Foundation, IZA runs the world's largest network of economists, whose research aims to provide answers to the global labor market challenges of our time. Our key objective is to build bridges between academic research, policymakers and society.

IZA Discussion Papers often represent preliminary work and are circulated to encourage discussion. Citation of such a paper should account for its provisional character. A revised version may be available directly from the author.

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Replication in Labor Economics: Evidence from Data, and What It Suggests*

Examining the most heavily-cited publications in labor economics from the early 1990s, I show that few of over 3000 articles citing them directly replicates them. They are replicated more frequently using data from other time periods and economies, so that the validity of their central ideas has typically been verified. This pattern of scholarship suggests, beyond the currently required depositing of data and code upon publication, that there is little need for formal mechanisms for replication. The market for scholarship already produces replications of non-laboratory applied research.

JEL Classification: B21, J01, B41

Keywords: reliability of research, scientific method, citation analysis

Nontechnical summary:

How valid are research results in labor economics? Are the data and/or methods wrong? Examining the 10 most heavily-cited publications in labor economics from the early 1990s, I find that 7 of them were replicated at least 5 times and each at least once. This suggests that an important empirical point, once established, is examined by other researchers and either supported, as these are, or thrown into the dustbin of past science.

Corresponding author:
Daniel S. Hamermesh
Royal Holloway University of London
Department of Economics
214 Horton Building
Egham, Surrey TW20 0EX
United Kingdom

E-mail: Daniel.Hamermesh@rhul.ac.uk

* I thank Frances Hamermesh, Andrew Leigh, Leigh Linden, David Sibley, Stephen Trejo and Alistair Wilson for helpful comments.

There is a paucity of pure "hard-science" style replication in applied economics (Hamermesh, 2007), although a few role models have used data that others had made available to re-examine conclusions that had appeared solid enough to pass muster with editor(s) and referees. My questions here are whether the profession can or should produce more pure replications, whether the market for economic research has created good substitutes for formal replication, and what the incentives are for this kind of work. To provide an empirical basis for the discussion I first examine the citation histories of leading articles in empirical labor economics published between 1990 and 1996, thus with at least twenty-year citation histories. This sample allows examination of ways in which earlier scholarship affects or stimulates subsequent research and consideration of how these impacts change as time passes since the original article appeared.

I. The Pathways of Scholarly Influence

I selected ten of the most heavily-cited publications in empirical labor economics from this period, with the selection criteria requiring each to have been published in a so-called "Top 5" journal and to have accumulated at least 1000 Google Scholar (GS) citations as of Summer 2016.[1] The studies, along with the number of GS and Web of Science (WoS) citations of each and its rank among all articles published in that volume of the journal, are listed in Table 1. As the Table shows, each was sufficiently well-cited to be in the top 10 percent of all articles published in the particular volumes of the journals, themselves the most heavily cited in the economics profession (Hamermesh, 2017). For each of these articles I examined every publication that the WoS, which is more easily usable than GS for this purpose, had recorded in September 2016 as having cited

[1]Several other articles met the criteria but were excluded as being as much methodological as making a purely empirical point.

the work, in each case reading first the abstract and then, if necessary, skimming through the citing paper itself.

I classified each citing article by year post-publication of the principal article and by whether it was: 1) Related to; 2) Inspired by; 3) Very similar to but using different data; or 4) A direct replication at least partly using the same data.[2] These classifications are obviously arbitrary, but since one person (this author) did the classifying, at least they should be consistent across the ten articles.

The final column of Table 1 shows the percentages of citations to each paper that were merely related to the new article (Category 1 above). The overwhelming majority of citations to these highly-cited papers were based on their important roles in the relevant literatures. Few of the citing papers were inspired by the original paper, and fewer still involved a replication. The distribution of the over 3000 citing papers in the four categories was: Related, 92.9 percent; inspired, 5.0 percent; similar, 1.5 percent; replicated, 0.6 percent. (The ranges in the last three categories are 2.3 to 7.0 percent; 0 to 5.2 percent; and 0 to 2.3 percent.) Replication, even defined somewhat loosely, is fairly rare even of these most highly visible studies.

The life cycle of replication is also interesting. Figure 1 shows a scatter and linear fit of the annual fractions of citing articles that are similar to the original article or are direct replications (Categories 3 and 4) by post-publication year of the original article, leaving out years 21+ so that citations to all ten original articles appear in the annual averages. The probability that a citing article is either similar to or explicitly replicates the original paper diminishes over the years after

[2]To be classified as "inspired" the citing paper had to refer repeatedly to the original paper and/or had to make clear that it was inspired by the methodology of the original work. To be noted as "similar" the citing paper had to use the exact same methodology but on a different data set, while a study classified as a "replication" went further to include at least some of the data in the original study. Thus even a "replication" in many cases involved more than simply re-estimating models in the original article using the same data.

the original publication. Moreover, adding a quadratic term to the fit adds nothing—the relationship is strictly linearly. Estimating the same equations (using either least squares or probit analysis) on the underlying micro data yields almost identical coefficients to the linear model estimated over averages.

Figure 2 presents a similar scatter for the fraction of citing articles that were classified as inspired by the original piece. A quadratic model in post-publication year fits this scatter better than a linear model: This relationship is significantly convex. As with the citing articles that were similar or replications, the results are almost identical if we estimate these models over the underlying micro data.[3]

One might be concerned that the relative paucity of replications of these important papers results from selectivity—less important papers were replicated, found wanting and henceforth essentially ignored. To examine this possibility I collected WoS citations to the five least-cited empirical articles in labor economics published in these four journals between 1990 and 1996, classifying each citation to them in one of the same four categories used for major articles. One of these five articles received 155 GS citations, but references to the other four ranged from 37 to 89 GS citations. 92.8 percent of the 111 WoS articles citing one of these five papers were merely related to the original article (Category 1), almost identical to the percentage of articles in this Category that cited the ten major papers. None of the WoS citations to these five articles could be classified either as similar to or a direct replication of the original study. Sparsely-cited articles in major journals are not killed by replications that cast doubt on their results; rather, they "die" from neglect.

[3]The results using the micro data underlying the scatters in Figures 1 and 2 are only barely altered with fixed effects for each original article included.

II. Implications for Replication

Does this evidence show that the replication glass is 2 percent full, or 98 percent empty? Replications are not published for most studies, even those published in Top 5 journals, nor should they be: The majority of articles in those journals are, as I showed (Hamermesh, 2017), essentially ignored, so that the failure to replicate them is unimportant. Even as a fraction of citations to major papers, replication is quite rare; but 7 of the 10 articles examined above were replicated at least 5 times, with the remaining 3 replicated 1, 2 and 4 times. Published replications of these most heavily-cited papers are performed, so that one might view the glass as 100 percent full. Moreover, an unknown number of additional replications may have been made but never published, perhaps because they corroborated the original results.

Replication in the case of labor economics (and presumably of other areas of applied microeconomics) does not take the form of repetition of a particular protocol on laboratory rats (or their equivalent in economic research, undergraduate students). Instead, in most cases it proceeds by taking the economic idea that motivated the original empirical study in a literature and examining its predictions and implications using a set of data describing a different time and/or economy. Applied microeconomics is not a laboratory science—at its best it consists of the generation of new ideas describing economic behavior, independent of time or space. The empirical validity of these ideas, after their relevance is first demonstrated for a particular time and place, can only be usefully replicated at other times and places, since if they are general descriptions of behavior they should hold up beyond their original testing ground. Simple laboratory-style replication is important in catching errors in influential work, as in the recent case of the Herndon *et al* (2014) examination of Reinhart and Rogoff (2010); but the more important

replication goes beyond this and is, as I have shown, usually undertaken for the most important work in labor economics.

People will differ about the optimal amount of replication. The evidence suggests, however, that the system is not broken and does not need fixing—appropriate replications are being conducted. Despite this evidence and these arguments, what if one believes that more replication, using mostly the same data as in the original study, is necessary? First, a bit of history: During the 1960s the *American Economic Review* was replete with replication-like papers, in the form of sequences of Comments (often in the form of replications on the same or other data), Replies and even Rejoinders.[4] For example, in the four regular issues of the 1966 volume 16 percent of the space went to contributions of this form. In the first four regular issues of the 2013 volume only 4 percent did, reflecting a change that began by the 1980s. The editors have shifted away from cluttering the *Review*'s pages with Comments, etc. This change may have reflected their desire to maximize its impact on the profession in light of their conscious or subconscious realization that pages devoted to this type of exercise do not generate the same attention from other authors' scholarly work as does journal space devoted to original articles (Whaples, 2006). We have had replications or approximations thereof in the past, but the market for scholarship—as indicated by their impact—has exhibited little interest in them.

Perhaps this market-based evidence showing a decline in publishing of replication-like work is irrelevant, either because it is out of date or simply not on point? If one believes this, a possible method for encouraging replication would be a statement by the AEA urging its editors, and those of other journals, explicitly to encourage replications among their submissions—to announce and follow through with replication-friendly editorial policies that lead to publishing replications in

[4]The *Journal of Political Economy* published "Confirmations and Contradictions" for a while also.

major journals. Why editors in a competitive publishing market would be willing to do this, even editors who are supposed to be creatures of the organization suggesting this approach, is unclear; but perhaps moral suasion would effect changes in editorial policies.

Another approach would be to create a *Journal of Economic Extensions and Replications (JEER)*, publishing articles whose sole purposes would be to replicate and extend other articles (presumably those previously published in major journals). Given today's lags in publishing, by the time an article appears in a major journal it has been circulating in something near its final form for at least two years, and often four or more. Even with a fairly quick turn-around, articles in the *JEER* would be attempting to replicate research that had been known to most interested scholars for three years or more. Who would wish to publish in such a journal? Who would bother subscribing to or reading it? The current nature of economics publishing—the ridiculously long publishing lags—would seem to doom such a journal to obscurity and early death.

Since the market for major articles in applied microeconomics demonstrates that they are usually replicated, and appropriately so, in other "laboratories" (other times or economies) than the original one, perhaps there is a place for direct replication aimed mainly at catching errors. Such a scheme would go well beyond current practice at AEA-funded journals requiring authors to deposit data and code (in those studies that do not use proprietary data), which are then checked to verify that the code runs.

Consider the following change in the publication process at major journals: Once an empirical article is accepted for publication it, along with all the data and code that underlay it, would be sent to one of a cadre of Replicators that the journal has established. Members of this cadre would have agreed to take no more than three months to examine the study, including using the data to replicate and even expand upon the original article (perhaps using additional specifications). Their

completed replication—written up in three to five manuscript pages—would then be guaranteed publication in the journal just behind the original article.

The benefit from this scheme is clear—it would discover errors of calculation and transcription that referees rarely have the time, inclination or material to catch, and it would allow some extension of the original research. Given current publication lags, an extra three-month lag is minor—although those lags and, more important, the requirements for depositing data and code that disadvantaged empirical relative to theoretical work, make the proposal marginally less attractive.

The suggestion has, however, more serious problems. Who would wish to become a Replicator? Only younger scholars or extremely senior scholars are likely to be interested; and younger economists at more influential institutions would probably not see their careers advanced by publishing several pages of replication once per year, even in a top journal, and would be unlikely to agree to participate. The program might only attract scholars outside the most highly-regarded institutions. They would catch some errors, and some interesting new findings might develop from the articles that these Replicators produce. But would their work be credible to the profession—who would guard the guardians?

Many replications of applied work do take place—in graduate econometrics and applied classes. The AEA could modify its website so that, in addition to a space for Comments on posted articles, there could be space for graduate students to post their code and replication results. While these would not have the visibility of published replications, they would provide a way of catching errors at extremely low marginal cost to researchers and to the Association.

III. Whither Replication in Empirical Microeconomics?

This examination of the reality of replication in empirical work suggests that research which the community of scholars implicitly deems important is replicated, including both on other data and to a lesser extent on the data that are now typically required to be deposited with the journal. The more important type of replication is not like that of "hard-scientific" research, but rather in the only sensible way for a **social** science—by testing the fundamental idea or construct in a different **social** context. Important mistakes do get caught, and important ideas initially tested on only one set of data must survive tests on other data. The market appears to work well, and for various reasons the complex alternatives that I have suggested do not seem practicable or necessary. Taking these considerations together, *Mad's* attitude, "What—Me Worry?" is apropos the existing role of replication in empirical economics.

REFERENCES

Ashenfelter, Orley C., and Alan B. Krueger. 1994. "Estimates of the Economic Return to Schooling from a New Sample of Twins." *American Economic Review* 94 (5): 1157-73.

Borjas, George J. 1995. "Ethnicity, Neighborhoods and Human-Capital Externalities." *American Economic Review* 85 (3): 365-90.

Card, David E., and Alan B. Krueger. 1994. "Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania." *American Economic Review* 84 (4): 772-93.

Currie, Janet M., and Duncan Thomas. 1995. "Does Head Start Make a Difference?" *American Economic Review* 85 (3): 341-64.

Davis, Steven J., and John C. Haltiwanger. 1992. "Gross Job Creation, Gross Job Destruction, and Employment Reallocation." *Quarterly Journal of Economics* 107 (3): 819-63.

Hamermesh, Daniel S. 2007. "Viewpoint: Replication in Economics." *Canadian Journal of Economics* 40 (3): 715-33.

Hamermesh, Daniel S. 2017. "Citations in Economics: Measurement, Impacts and Uses." *Journal of Economic Literature*, 55, forthcoming.

Hamermesh, Daniel S., and Jeff E. Biddle. 1994. "Beauty and the Labor Market." *American Economic Review* 94 (5): 1174-94.

Herndon, Thomas, Michael Ash, and Robert Pollin. 2014. "Does High Public Debt Consistently Stifle Economic Growth? A Critique of Reinhart and Rogoff." *Cambridge Journal of Economics* 38 (2): 257-79.

Jacobson, Louis S., Robert J. LaLonde, and Daniel G. Sullivan. 1993. "Earnings Losses of Displaced Workers." *American Economic Review* 83 (4): 685-709.

Lazear, Edward P. 1990. "Job Security Provisions and Employment." *Quarterly Journal of Economics* 105 (3): 699-726.

Meyer, Bruce D. 1990. "Unemployment Insurance and Unemployment Spells." *Econometrica* 58 (4): 757-82.

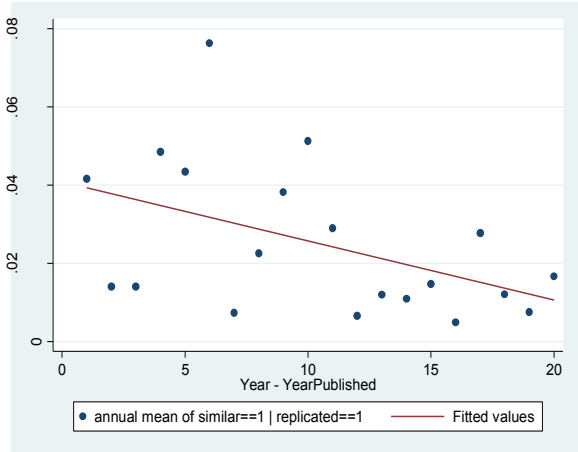
Neal, Derek A., and William R. Johnson. 1996. "The Role of Premarket Factors in Black-White Wage Differences." *Journal of Political Economy* 104 (5): 869-95.

Reinhart, Carmen M., and Kenneth S. Rogoff. 2010. "Growth in a Time of Debt." *American Economic Review* 100 (2): 573-8.

Whaples, Robert M. 2006. "The Costs of Critical Commentary in Economics Journals." *Econ Journal Watch* 3 (2): 275-82.

TABLE 1. CITATIONS AND RANK OF STUDIES

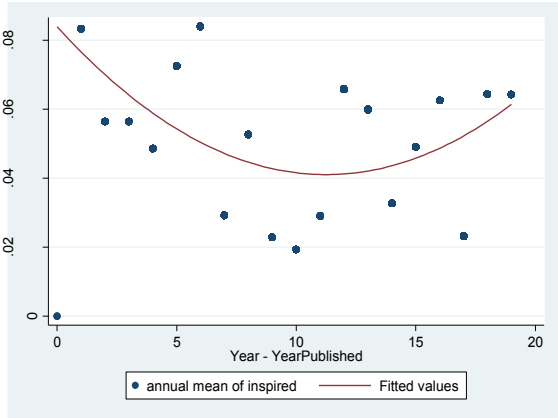
Study, and Rank in Volume	GS Citations	WoS Citations	Percent Only Related
Ashenfelter and Krueger (1994) 12/182	1284	303	91.7
Borjas (1995) 10/175	1099	269	94.1
Card and Krueger (1994) 6/182	2053	460	91.7
Currie and Thomas (1995) 9/175	1026	254	94.5
Davis and Haltiwanger (1992) 5/56	1736	399	87.7
Hamermesh and Biddle (1994) 10/182	1289	322	91.6
Jacobson *et al* (1993) 3/176	1785	457	95.0
Lazear (1990) 3/54	1423	305	94.4
Meyer (1990) 5/68	2068	549	92.9
Neal and Johnson (1996) 2/48	1365	397	95.5



pr(More than inspired) = 0.0409 - 0.00151PostPubYear;
(0.0081) (0.00068)

Adj. R^2 = 0.174 (N = 20)

FIGURE 1. PROBABILITY OF CITING ARTICLE BEING SIMILAR OR REPLICATION



$$\text{pr(Inspired)} = \underset{(0.0134)}{0.0808} - \underset{(0.00294)}{0.00679}\text{PostPubYear} + \underset{(0.00014)}{0.00029}\text{PostPubYear}^2;$$

$$\text{Adj. } R^2 = 0.154 \text{ (N = 20)}$$

FIGURE 2. PROBABILITY OF CITING ARTICLE BEING INSPIRED