



# From Synthetic Marx to Synthetic Kafka: A Rejoinder to Magness and Makovi

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[LINK TO ABSTRACT](#)

Phillip W. Magness and Michael Makovi (2024) have objected to my critique of their “The Mainstreaming of Marx” article in the *Journal of Political Economy* (JPE). They state that I “pivot between mistaken or misrepresented quantitative claims” (2024, 405). My criticisms of their use of the synthetic control method (SCM) are particularly misguided, they claim, and are the product of a dubious motivation. “Such complaints,” they write, “amount to little more than specious handwaving by an author who has decided, a priori, that SCM analysis of Marx’s citation patterns is not possible because he personally finds the results to be objectionable.” They also claim that my use of the JSTOR data suffers from “significant distortion” due to “built-in sampling bias” (ibid., 405, 406, 409).

This rejoinder has three parts. First, I elaborate further on the problems with Magness and Makovi’s use of the SCM. Second, I present a simpler analysis of the Google Ngram Viewer data that is more in line with traditional accounts of Marx’s rise to fame. Third, I point out some errors in Magness and Makovi’s analysis of the JSTOR data. To reiterate, my argument is that counting names is unlikely to tell us much about intellectual history, whether it deals with Marx or anyone else. Quantitative methods should only be used with care.<sup>2</sup>

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1. University of Birmingham, Birmingham B15 2TT, United Kingdom. This rejoinder is indebted to discussions with John Ruf, as well as with Drs. Magness and Makovi. Two reviewers kindly provided comments.

2. For examples of the successful use of quantitative methods in intellectual history, see Brahim and Leperlier (2024).

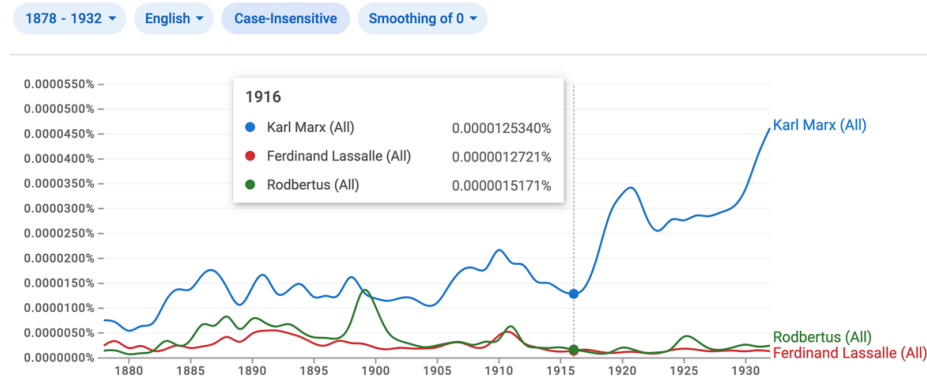
## The synthetic control method revisited

According to Magness and Makovi (2024, 398), my critique of their use of the SCM suffers from a “confusion about citation levels vs. rate of change.” In making this semantic distinction, Magness and Makovi’s point seems to be that they are only interested in the effect of the Russian Revolution on the trend in Marx’s n-gram share. From this perspective, my concerns about how the algorithm constructed the Synthetic Marxes to match the actual Marx’s n-gram levels appear irrelevant. Yet levels are central to the novel and provocative aspect of Magness and Makovi’s argument. Specifically, they argue that, before the Russian Revolution, Marx was just “a second- or third-tier scholar, mainly known within socialist circles and from highly specialized criticisms within the economics discipline.” Consequently, he would have remained “merely one among many socialists of niche interest” had it not been for the Bolsheviks (2024, 387, 388). In this way, Magness and Makovi obviously refer to levels, despite their assertion that they are only interested in rates of change. Furthermore, the SCM is also concerned with levels. Hence, Magness and Makovi task the *synth* package in Stata with matching the levels of Marx’s n-gram share from 1878 to 1916 in the construction of their Synthetic Marxes. Both the argument and the method are therefore about levels.

As I explained in my initial critique (Francis 2024, 370–375), the SCM’s results reflect the need to match the relatively high levels of Marx’s n-gram share before the Russian Revolution. Figure 1 provides a simple illustration of the problem using a screenshot taken directly from the Google Ngram Viewer. In 1916, the ngram share of “Karl Marx” was already eight times the level of “Rodbertus” and ten times the level of “Ferdinand Lassalle”—the two names from Magness and Makovi’s donor pool that the algorithm uses to make the Synthetic Marx meet the criterion of being a German Socialist. The problem it then faces is to boost the Synthetic Marx’s n-gram share to that of the actual Marx. In this way, the SCM itself demonstrates that Marx was relatively well-known. To match his n-gram share, it must combine minor socialists such as Lassalle and Karl Rodbertus with a world-famous figure like Abraham Lincoln to make a Synthetic Marx. This combination is necessary because Marx was already a big fish on the eve of the Russian Revolution, as the raw n-gram data in Figure 1 illustrates. The SCM therefore combines a (non-German, non-socialist) whale like Lincoln with minnows like Lassalle and Rodbertus to match Marx’s n-gram share. I demonstrated this in my initial critique, and Magness and Makovi do not adequately address it in their response. Instead, they run a version of the SCM with Lincoln excluded, which then leads the algorithm to instead use the name “Newton” to boost the Synthetic Marx’s n-gram level. It simply swaps one whale for another,

further demonstrating that Lassalle and Rodbertus were not at Marx’s level before 1917.

**Figure 1.** N-gram shares of three German socialists, 1878–1932



Magness and Makovi themselves implicitly suggest that their results should be rejected for this reason. In the Statistical Appendices accompanying their JPE article, they create a Synthetic Marx using the n-grams for his surname alone. They find, however, that the largest weight is given to the surname “Nietzsche.” This is a problem because “there is no clear reason why Nietzsche should be such an important counterfactual for Marx.” They therefore decide to reject this robustness test “because we wish our synthetic Marx to resemble the real Marx not [only] in terms of pretreatment outcomes, but also in terms of causes of those outcomes” (2023, Statistical Appendices A29). By the same logic, therefore, Synthetic Marxes that rely so heavily on an American president or the surname of a seventeenth-century British philosopher should also be rejected. There is no clear reason why Lincoln or Newton should be such important counterfactuals for Marx. Magness and Makovi have thus ignored Alberto Abadie’s (2021, 401) injunction that “each of the units in the donor pool have to provide a reasonable control for the treated unit. Including in the donor pool units that are regarded by the analyst to be unsuitable controls,” Abadie continues, “is a recipe for bias.”

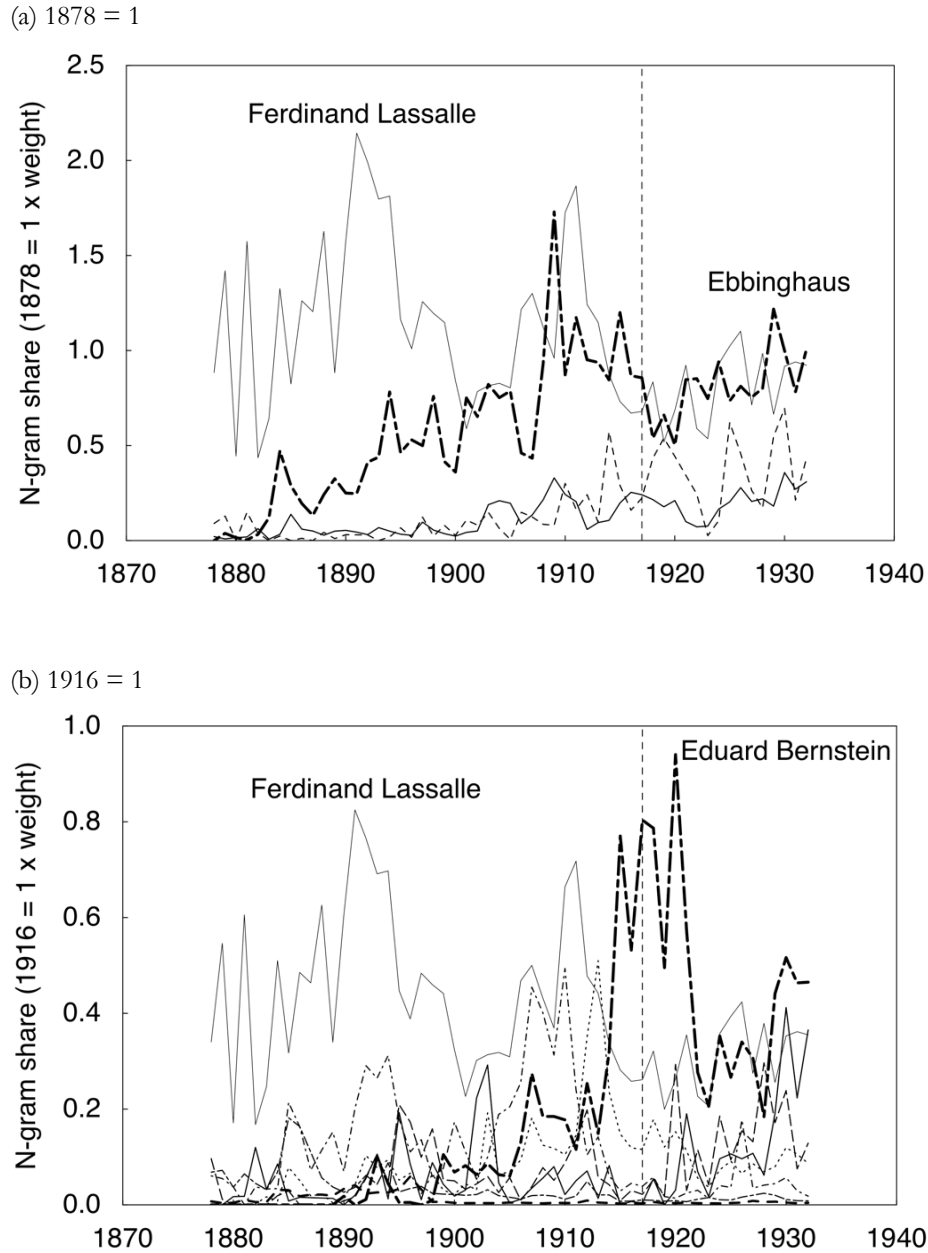
The other robustness checks suggest that the problem of inappropriate donors is a fundamental flaw in Magness and Makovi’s research design. In their response, for example, Magness and Makovi (2024, 401) include a Synthetic Marx in which they restricted the donor pool “to only those authors whose average citations in the pretreatment period (1878–1916) are between half and double Marx’s average pretreatment citations.” This was intended to ease my concerns about how the algorithm was using inappropriate donors to match Marx’s pretreatment levels. Nonetheless, looking at their results shows that the algorithm

gave a 0.451 weight to the German surname “Hamman,” which Magness and Makovi included in their donor pool as a reference to an eighteenth-century theologian. The next largest donor is Oscar Wilde, who receives a weight of 0.172. Magness and Makovi would need to explain why this is a meaningful counterfactual for Marx if a counterfactual based on Nietzsche is not.

The 39 normalized Synthetic Marxes in their original JPE article suffer from similar problems, as I detailed in my original critique (Francis 2024, 372–374). As a *mea culpa*, I should acknowledge that Magness and Makovi are correct to observe that my claim that “Lassalle’s n-gram share trended downward after he died in 1864, [...] whereas Marx’s trended upwards” was an error (Francis 2024, 373). My sentence should have said that Lassalle’s n-gram share rose after he died in 1864, peaked in 1891, then trended downward, whereas Marx’s trended upwards throughout this period. Making this correction does not, however, alter my original critique. To make those normalized Synthetic Marxes, the algorithm had to find a way to counteract Lassalle’s falling n-gram share from 1891 onwards, which it did by turning to younger donors whose n-gram shares were rising simply as part of their lifecycles. The normalized Marxes for 1878 and 1916 are shown in Figure 2 as two examples of the results. In the former, Lassalle’s declining n-gram share is mainly counteracted by the surname of Hermann Ebbinghaus, a German psychologist who was born in 1850 and died in 1909, after which his n-gram share fell. For the 1916 normalized Marx, meanwhile, the algorithm uses the German socialist Eduard Bernstein, who was also born in 1850 and died in 1932. In both cases, the algorithm is stitching together donors from different periods to match Marx’s pattern of a persistently rising n-gram share after his death. The onus is again on Magness and Makovi to explain why these Frankenstein’s monsters are supposed to provide a meaningful counterfactual for Marx.

The contrast with the classic articles in the SCM genre is stark. Abadie and Javier Gardeazabal (2003) combined Catalonia and Madrid to make a Synthetic Basque Country, while Abadie, Alexis Diamond, and Jens Hainmueller (2010) summed Colorado, Connecticut, Montana, Nevada, and Utah to make a Synthetic California. In each case, they were summing things that were similar—Spanish regional entities and American states, respectively—and contemporaneous. The donors chosen by Magness and Makovi, on the other hand, range from an Ancient Greek poet to an American president to an Anglo-Irish playwright. The logic of their research design is therefore hard to fathom. Why exactly do they consider Aesop to be “a reasonable control” for Marx, as Abadie (2021, 401) indicates the donors should be? Why did they include Nietzsche if they subsequently decided he wasn’t a reasonable control when the algorithm included him in a Synthetic Marx? These are the kind of questions that motivated me to write my critique.

**Figure 2.** Components of two normalized Synthetic Marxes, 1878–1932



Furthermore, in writing this rejoinder, I have found other issues with Magness and Makovi’s JPE article. Out of curiosity, I revisited their original working paper and realized that there had been a substantial loss of statistical

significance in the published version. For the working paper’s headline Synthetic Marx, the treatment effect of the Russian Revolution had a p-value of 0, while the p-values of their normalized Synthetic Marxes—their key robustness test—were below the conventional 5 percent level of statistical significance in 34 out of 39 cases (Magness and Makovi 2020, 49, 54, Tables 6 and 11).<sup>3</sup> In the JPE article, by contrast, the headline p-value has gone up to 0.047. Meanwhile, only 19 out of the 39 normalized versions are below the 5 percent level, which somewhat contradicts the claim that their robustness test found “a statistically significant treatment effect similar in magnitude to our main result” (Magness and Makovi 2023, 1530 Table 4, 1538, Online Appendices A33 Table A.8).

To find out why the JPE article’s headline Synthetic Marx verges on statistical insignificance, I replicated it using simpler code in R.<sup>4</sup> By avoiding the more compute-intensive elements of Magness and Makovi’s script, I can see how the changes in its p-value are affected by the composition of the donor pool, allowing me to roughly determine why there was such a reduction in statistical significance in the JPE article. As seen in Table 1, the problem seems to have arisen when numerous German-language donors were added to the pool, perhaps on the suggestion of a peer reviewer. The inclusion of these donors drastically reduces the statistical significance of the results of my replication, raising the p-value from 0.01 to 0.06. Only the addition of further donors makes the p-value come back down below the crucial 5 percent level. The SCM thus struggles to even verify the uncontroversial claim that the Russian Revolution boosted Marx’s n-gram share.

Magness and Makovi’s (2023) results therefore seem quite fragile. To illustrate further, I have added a simple robustness test in which the Synthetic Marx is constructed from a donor pool of the 55 German-language donors who Magness and Makovi (2023) include in the pool of their own headline Synthetic Marx. As shown in Panel (a) of Figure 3, the result looks convincing. Yet this Germans-only Synthetic Marx has a p-value of 0.109, above even the more generous 10-percent level of statistical significance. The reason why can be seen in Panel (b), which

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3. The focus here is on the standard SCM joint post p-value, which is the only one reported in their working paper. It is calculated by first taking the ratio between the Root Mean Square Prediction Error (RMSPE) of the actual and synthetic Marxes in the post- and pre-treatment periods; then, it calculates the same for the actual and synthetic versions of all the donors; and, finally, the p-value is the percentage of donors that have a post/pre RMSPE ratio that is greater Marx’s. When no donors do, the p-value is zero.

4. González et al. (2024) only checked the p-value of the headline Synthetic Marx in their study for the Institute for Replication because they found Magness and Makovi’s code too compute-intensive. Nonetheless, I have found it possible to replicate Magness and Makovi’s findings with simpler code. I have not, however, replicated the alternative p-values that Magness and Makovi (2023) report because they are not used in the SCM literature. Their use of the asymptotically exact p-value (AEP) is particularly problematic because it is likely to be “anticonservative” when observations are autocorrelated, as is often the case in time series (Wilson 2019, 23384; see also Goeman, Rosenblatt, and Nichols 2019).

shows the SCM’s result for Thomas Mann. His synthetic alter ego is made up of a predictably inappropriate collection of donors, with the largest weight assigned to Hermann Cohen, a Jewish German philosopher who died in 1918. There is a major divergence between the actual and synthetic Mann after the First World War, which the SCM interprets as being a result of the Russian Revolution. Mann and various other German-language authors thereby make the post-1917 divergence between the actual and synthetic Marxes appear statistically insignificant because the SCM cannot distinguish between the Russian Revolution’s effect on Marx’s n-gram share and the way in which other German-language authors were mentioned more frequently in English-language texts after the First World War.<sup>5</sup>

TABLE 1. Donors to Magness and Makovi’s headline Synthetic Marx

	Donors	P-value
Working paper:		
48 from brainstorming	48	
+19 from political philosophy compilations	67	
+30 from Volumes 1 to 20 of <i>Harvard Universal Classics</i>	97	0.010
JPE article:		
45 from German-language anthologies	142	0.056
+3 from brainstorming	145	0.055
+1 from political philosophy compilations	146	0.055
+49 from Volumes 21 to 26 and 29 to 39 of <i>Harvard Universal Classics</i>	195	0.046
<i>Notes:</i> The p-values are for the treatment effect of the Russian Revolution from my replication of Magness and Makovi’s (2023) headline Synthetic Marx. They are calculated using all the donors in the pool after each tranche is added. Before the 30 donors from Volumes 1 to 20 of <i>Harvard Universal Classics</i> are added, the <i>synth</i> package is unable to calculate synthetic Marxes due to insufficient variability in the pool.		

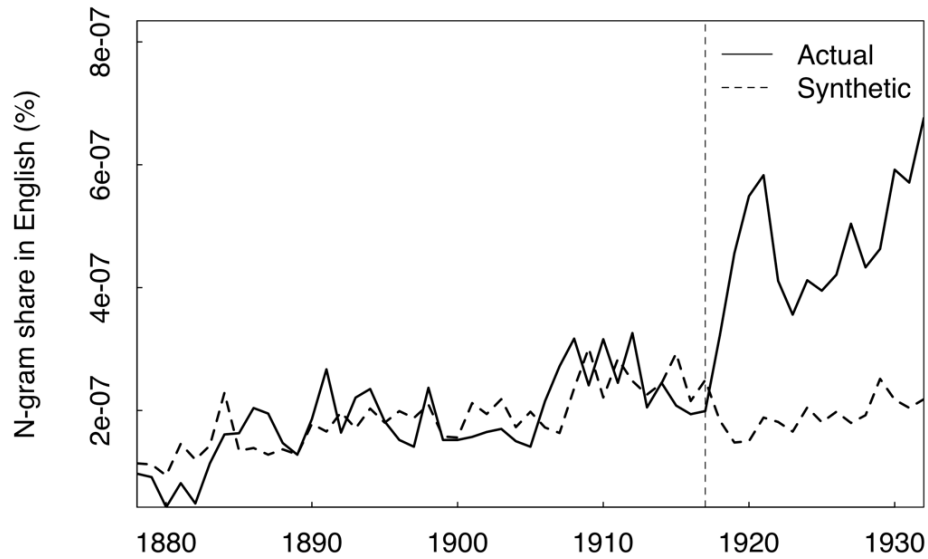
The extent to which the level of statistical significance can be changed through adjustments to the donor pool is worrying. That a substantial portion of Magness and Makovi’s donors were picked by “brainstorming” exacerbates the issue. They are also vague on how the German-language authors were selected. Magness and Makovi (2023, 1519) state that they used unspecified “German-language encyclopedic anthologies” to choose “prominent German-language writers whose lives preceded or overlapped Marx’s.” Yet they omit Sigmund Freud,

5. Magness and Makovi (2023, 1532–1536) consider German-language n-gram shares and Germans-only German-language n-gram shares, which return p-values of 0.069 and 0.062, respectively, in their JPE article. In my replication, these p-values are considerably lower (0.018 and 0.047). The difference seems to be due to Magness and Makovi’s use of the “nested allopt” function in the *synth* package in Stata, which is so compute-intensive that it forces them to limit the number of iterations to 20, whereas the default is 1,000. This may explain why their script leads to more failures to converge than in my simpler implementation in R.

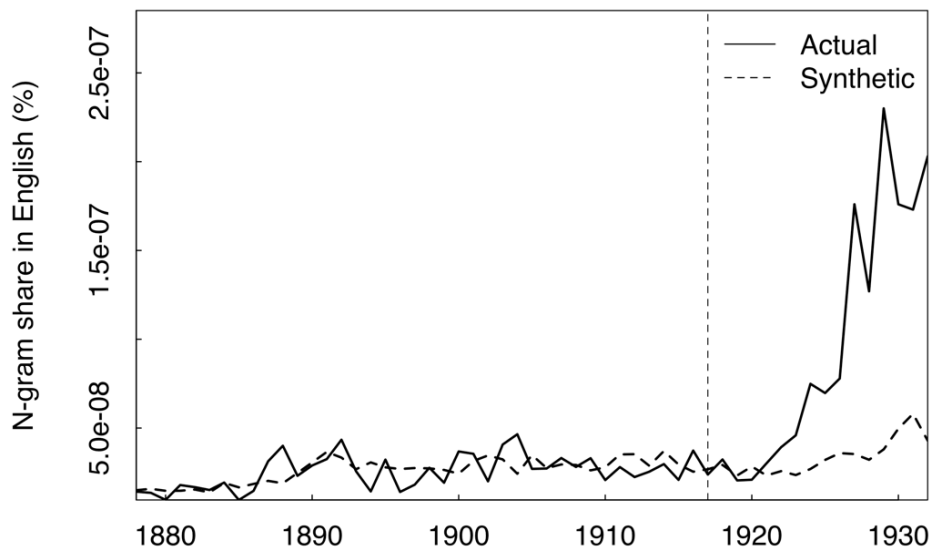


**Figure 3.** Germans-only Synthetic Karl Marx and Synthetic Thomas Mann, 1878–1932

(a) Karl Marx



(b) Thomas Mann



born in 1856, and Max Weber, born in 1864, despite Marx having died in 1883. What guided this decision is opaque, although when Freud and Weber are added to the donor pool in my replication, the p-value for the Germans-only Synthetic



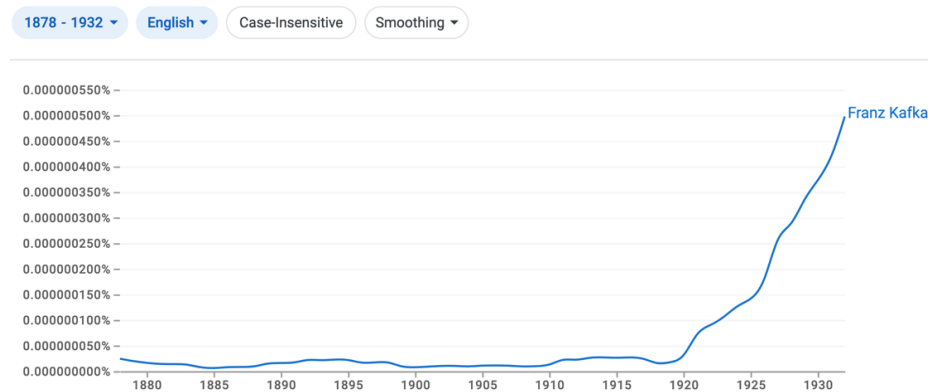
Marx actually falls to a more respectable 0.088. In this way, they further illustrate how greater statistical significance can be achieved through arbitrary choices by the researcher.

**Figure 4.** “Kafka” and “Franz Kafka” from Google Ngram Viewer, 1878–1932

(a) Kafka



(b) Franz Kafka



The inclusion of Franz Kafka as a donor is symptomatic of this arbitrariness. In my replication, he is given the largest weight in the Synthetic Weber, but it is unclear why Magness and Makovi (2023) included him in the donor pool at all. Kafka was born on July 3, 1883, 111 days after Marx died on March 14, making him a strange choice as a potential donor. On top of that, Magness and Makovi use the surname Kafka, even though its n-gram share fits poorly with the basic facts of his rise to fame. As seen in screenshots from the Google Ngram Viewer in Panel (a)

of Figure 4, it first spikes in the late 1880s, when the future author was still a child; it then collapses as he reached adolescence, before steadily rising until his death in 1924, when it begins to fall—just as his friend Max Brod began to publish the books that would make him posthumously famous. On the other hand, Kafka’s full name, shown in Panel (b), provides an n-gram share that fits this pattern, making it unclear why it was not used once the decision to include Kafka was taken. Such are the arbitrary decisions that abound in Magness and Makovi’s use of the SCM.

Ultimately, Magness and Makovi’s research design is itself somewhat Kafkaesque. Their starting point is an obvious truth: that the Russian Revolution made Marx more famous. Yet they then use a needlessly convoluted econometric methodology that ultimately struggles to demonstrate that obvious truth to any great degree of statistical significance, at the same time as they imply that it supports a dubious claim about Marx being largely unknown outside of economics before 1917. Their research design is, moreover, characterized by arbitrariness. Indeed, Panel (b) of Figure 4 suggests that it could easily be used to construct a Synthetic Franz Kafka to demonstrate how that author’s fame was in fact a result of the Russian Revolution, after which his n-gram share began to take off. It is another sign that the SCM was a bad methodological choice to begin with. The question is why such complicated research design was needed to confirm something so obvious. Simpler techniques are available.

## Isaiah Berlin and the Google Ngram Viewer

In their historiographical discussion, Magness and Makovi (2024, 389 n.7) also attempt to enlist Isaiah Berlin to buttress their argument. They include him in a list of “scholars who have noted Marx’s relative obscurity during his lifetime, and especially so among the general public,” leading to the “question of how and when Marx’s work attained a high level of posthumous influence.” They then quote Berlin’s statements that “the fate of the world would have been very different” if there had been no Russian Revolution and that Marx was “little known to the general public” before his death, which leads them to conclude that Berlin was “closer to our own thesis about the Soviet Union than Francis acknowledges” (Magness and Makovi 2024, 411 n.29).

Nonetheless, Berlin’s account of Marx’s rise to fame is clearly at odds with Magness and Makovi’s narrative. Furthermore, it fits well with what the n-gram data actually show if they are taken as an indicator of fame rather than intellectual influence, as I argued they should be in my original critique (Francis 2024, 375). Hence, from 1917 onward, Marx would have been mentioned in many books about world affairs that do not necessarily indicate that his ideas were becoming

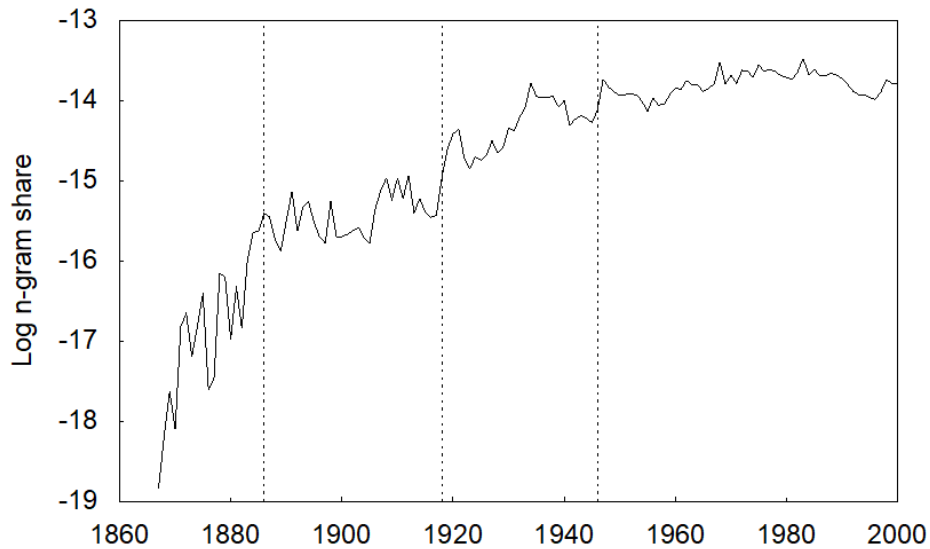
more influential in academia. Understood in this way, the n-gram data fit well with Berlin's qualitative account. Hence, before *Capital* was published in 1867, Marx's n-gram share is often zero, which matches Berlin's (2013, 203) statement that "[i]n 1860 Marx's fame and influence were confined to a narrow circle." Only in the late 1860s and 1870s did Marx begin to become well-known. As the results of a Bai-Perron test for "structural breaks" (that is, significant changes in trends and levels) illustrates in Figure 5, that period of rapid growth in Marx's n-gram share lasted up to 1886. For transparency, I quote Berlin's description of this period at length:

Fifteen years later [that is, in 1875] all this had altered. Still comparatively unknown in England, he had grown abroad into a figure of vast fame and notoriety, regarded by some as the instigator of every revolutionary movement in Europe, the fanatical dictator of a world movement pledged to subvert the moral order, the peace, happiness and prosperity of mankind. By these he was represented as the evil genius of the working class, plotting to sap and destroy the peace and morality of civilised society, systematically exploiting the worst passions of the mob, creating grievances where none existed, pouring vinegar in the malcontents' wounds, exacerbating their relations with their employers in order to create the universal chaos in which everyone would lose, and so finally all would be made level at last, the rich and the poor, the bad and the good, the industrious and the idle, the just and the unjust. (Berlin 2013, 203)

Here, then, Berlin is describing the exponential growth in Marx's fame that can be seen in the first period in Figure 5. The Bai-Perron test shows that period ending in 1886, when there is a structural break, three years after Marx's death. According to Berlin (2013, 251), Marx was, at this point, "without question the supreme moral and intellectual authority of international socialism," even if he was still not widely known outside of socialist circles in the English-speaking world. "His death passed largely unnoticed among the general public; *The Times* did, indeed, print a brief and inaccurate obituary notice, but this, although he died in London, appeared as a message from its Paris correspondent, who reported what he had read in the French socialist press." But, Berlin (2013, 263) continues, Marx's "fame increased steadily after his death [in 1883] as the revolutionary effects of his teaching became more and more apparent." Marx's n-gram share confirms this: there is steady growth until the Russian Revolution, which brings another structural break in 1918, when the rate of change increases, leading to Marx's new status as a world-historical figure. In Berlin's (2013, 231) words, Marx's book *Capital* achieved "a symbolic significance beyond anything written since the age of faith," such that it "has been blindly worshipped, and blindly hated, by millions who have not read a line of it, or have read without understanding its, at times, obscure and tortuous prose." Berlin's actual narrative is thus quite different from Magness and Makovi's because

he clearly describes how Marx was well-known before the Bolsheviks made him world-famous. This narrative is also supported by the simple analysis of Marx's n-gram share in Figure 5.

**Figure 5.** Structural breaks in Karl Marx's n-gram share, 1867–2000



*Notes:* The dashed vertical lines indicate the structural breaks in 1886, 1918, and 1946 detected using a Bai-Perron test. Calculated from data underlying Magness and Makovi (2023).

A more traditional account of Marx's rise to fame is therefore more consistent with the n-gram data than Magness and Makovi's revisionist take. In the case of Berlin, he can only be made to support their narrative by taking his words out of context. Both Berlin and the Google Ngram Viewer data actually suggest that Marx rapidly became better known following the publication of *Capital* until around the time of his death. His fame and notoriety then grew steadily until the Russian Revolution, when they accelerated again, until he became world-famous. This suggests that, in 1916, Marx was not a minnow like Lassalle and Rodbertus, even if he was not yet a whale.

## **JSTOR and Leszek Kołakowski**

Before restating my argument, some comments are also warranted on Magness and Makovi's critique of my use of the JSTOR data. When making their

case, they have chosen some loaded terms: “crude tabulations” (twice), “suspect,” “distortion” (six times), “distortive,” “exaggerates,” “corrupt,” “bias” (twice), and “without merit” are the words used (Magness and Makovi 2024, 406–409). It will therefore be necessary to address their critique at some length.

They take particular issue with my use of JSTOR’s disciplinary categories, which have some cross-listing among disciplines, as I stated in a note under the table presenting the data for the first time in my initial critique (Francis 2024, 361 Table 1). According to Magness and Makovi, using these categories leads me to overstate Marx’s influence before 1917. They focus especially on the *Annals of the American Academy of Political and Social Science*, which JSTOR includes in economics, in political science, and in sociology. Table 2 demonstrates how excluding *Annals* reduces Marx’s total mentions from 1907 to 1916 by 16 percent in economics and by 37 percent in sociology—a finding that Magness and Makovi use to justify their choice of words.

TABLE 2. Mentions in economics and sociology, 1907–1916

	Economics			Sociology		
	w <i>Annals</i>	w/o <i>Annals</i>	% dif.	w <i>Annals</i>	w/o <i>Annals</i>	% dif.
Karl Marx	68	57	–16	30	19	–37
Adam Smith	216	197	–9	45	26	–42
John Stuart Mill	69	61	–12	20	12	–40
Herbert Spencer	32	24	–25	35	27	–23
Henry George	56	45	–20	16	5	–69
Thomas Malthus	1	1	0	1	1	0
Jean-Baptiste Say	0	0	0	0	0	0
David Ricardo	15	15	0	1	1	0
W. Stanley Jevons	6	4	–33	2	0	–100
Alfred Marshall	15	15	0	0	0	0
Carl Menger	0	0	0	0	0	0
Philip Wicksteed	4	4	0	0	0	0
Eugen von Böhm-Bawerk	3	3	0	0	0	0
Johann Karl Rodbertus	0	0	0	0	0	0
Ferdinand Lassalle	7	7	0	2	2	0

*Notes:* The mentions were found using full-name searches of articles and reviews in JSTOR by disciplinary category. In the *w* columns, they include the mentions in the *Annals of the American Academy of Political and Social Science*; in the *w/o* columns, those mentions have been subtracted.

The problem is that they adjust only Marx’s mentions and not those of the other thinkers who I compared him to. As such, they do not demonstrate how significant my alleged error actually is. When these adjustments are made, it becomes clear that all five of the big names—Marx, Smith, Mill, Spencer, and

George—fall by similar percentages when the *Annals* is excluded, as seen in Table 2. Consequently, excluding *Annals* makes no difference to my observation that the JSTOR data indicate that Marx was relatively well-known before the Russian Revolution. Without the *Annals* included in economics and sociology, his mentions dwarf both the marginalist economists and the minor socialists, Lassalle and Rodbertus. Magness and Makovi’s criticism of my use of JSTOR’s disciplinary categories is therefore misguided.<sup>6</sup>

Next, Magness and Makovi (2024, 408) present a “corrected” version of part of a time series in which I estimate the percentage of articles in the sociology category that mentioned Marx. When the mentions in the *Annals* are excluded, they show a minor reduction in the percentage mentioning Marx from 1895 to 1928, although his mentions fall to zero before 1895. This latter finding is particularly important, Magness and Makovi claim, because my “tabulations show high levels of sociology citations” in these earlier years. “By contrast,” they write, their “corrected figure shows no sociology citations before 1895, since all of these hits come from cross-listed journals in other disciplines” (Magness and Makovi 2024, 408).

The errors in this analysis are again quite basic. When trying to replicate their “corrected” time series, I discovered that, once the *Annals* is excluded, there are simply no sociology journals in the JSTOR category before 1895, which explains why they found no mentions of Marx before then. Furthermore, when looking at their replication files, I discovered that they only subtracted the *Annals* from Marx’s mentions but left the estimate of the total number of sociology articles the same. In other words, they had subtracted the *Annals* data from the numerator but not the denominator. If the *Annals* data are also subtracted from the denominator, the mention rate from 1895 to 1916 actually increases to 1.5 percent, compared to the 0.9 percent I originally estimated and the 0.5 percent that Magness and Makovi erroneously claimed. Their “correction,” when done correctly, in fact reinforces the impression that Marx was well-known to sociologists before the Russian Revolution.<sup>7</sup>

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6. Magness and Makovi (2024, 407 n.27) claim that “by failing to address variations in how [...] names are spelled or presented,” my use of the JSTOR data “simultaneously inflates Karl Marx’s hits through the effects of cross-listing while also under-counting other economists on his list.” They give the specific example of Johan Karl Rodbertus, who sometimes went by the name of “Karl Rodbertus” and “Rodbertus-Jagetzow.” Yet when simply “Rodbertus” is searched for in English-language economics journals (without *Annals*) from 1907 to 1916, it produces 18 mentions, compared to 57 for “Karl Marx”; in sociology (again without *Annals*), “Rodbertus” produces 3 mentions, compared to 19 for “Karl Marx.” Hence, the point is another non-issue.

7. The calculations can be found in the supplementary files ([link](#)). For the 1920s, Magness and Makovi also subtracted the various iterations of the *Southwestern Political Science Quarterly*, which JSTOR similarly treats as belonging to both political science and sociology.

Finally, what Magness and Makovi seem to regard as their *coup de grâce* is also problematic. They present a graph showing “total JSTOR counts for Karl Marx (all disciplines)” from 1880 to 1980 (2024, 410 Figure 7). This graph suffers, however, from two issues. First, it ignores how more marginal journals are included in the JSTOR database over time, as I discussed in my initial critique (Francis 2024, 366). Second, it ignores how the disciplinary composition of the JSTOR database also changes. One example will suffice. Searching for “the” in all content in the JSTOR disciplinary category of “sociology” from 1907 to 1916 produces 4,390 results, whereas doing so in the category “botany & plant sciences” produces 12,226, giving a ratio of just 0.4 sociology articles for every botany article. From 1971 to 1980, by contrast, searching for “the” in sociology produces 64,636 results, while botany produces 45,235, giving a ratio of 1.4 sociology articles for every botany article. The composition of the JSTOR database thus changes over time, with disciplines in which Marx is more likely to be mentioned becoming a greater share of the total. For this reason, the aggregate measure used by Magness and Makovi is unreliable. Indeed, this was precisely why I disaggregated the data by disciplinary category and also isolated the “big” journals in my initial critique (Francis 2024, 363–369).

But bickering over name counts was not my goal in using the JSTOR data. In reality, my point was precisely that such name counts do not provide a reliable guide to anyone’s intellectual influence. Hence, I only used Marx’s name count for 1907 to 1916 because I had checked it against a thorough inspection of the JPE’s own back issues to confirm that he was well-known. I then used the example of the discipline of history to show how the time series calculated from the JSTOR data produce an inaccurate impression of the changes in Marx’s influence during the twentieth century. My argument was that ideas become detached from their authors, making name counts a bad way to do intellectual history. It was this methodological concern that motivated my initial critique of Magness and Makovi’s JPE article.

To conclude this rejoinder, Leszek Kołakowski (1927–2009) can be held up as an example of how intellectual history should, in my inexpert opinion, be done. Kołakowski was an exile from communist Poland who spent much of his life trying to understand Marxism. Specifically, he wanted to understand which, if any, of Marx’s ideas had inspired the crimes committed in his name. To do so, of course, Kołakowski first had to determine what Marx’s ideas were and how they differed from the ideas of some of those who would subsequently claim to be Marxists. Thanks to this effort, Kołakowski (1978, 523–528) could argue that Marx was, at least in part, responsible for Stalinism. Nonetheless, Magness and Makovi interpret my reference to Kołakowski’s concern with distinguishing Marx’s ideas from those of his followers as venturing into “endless internecine feuds” among Marxists. They then make a strange statement: “Our research question is only interested



in measuring whether and when Marx’s theories spawned subsequent intellectual engagement that explicitly credited him by name” (2024, 395). This implies that a thinker’s name is somehow more important than their ideas. As such, it points toward a research agenda that can say little about the bigger questions of intellectual history.

## Data and code

Data and code for this research are available via the journal’s website ([link](#)) and the author’s Github repository ([link](#)).

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