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# Synthetic Karl Marx and His Clumsy Critic

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[LINK TO ABSTRACT](#)

In the prologue to his classic book *Capitalism, Socialism, and Democracy* (1947, 3) Joseph Schumpeter presented a puzzle about one of the primary objects of his historical commentary, Karl Marx. “The last twenty years have witnessed a most interesting Marxian revival” in two locales, he noted, Russia and the United States. The Russian example had an obvious connection to the establishment of a revolutionary Marxist state in 1917, but the American “revival is less easy to explain.” As Schumpeter continued:

This phenomenon is so interesting because until the twenties there was no Marxian strain of importance in either the American labor movement or in the thought of the American intellectual. What Marxism there was always had been superficial, insignificant and without standing. (Schumpeter 1947, 3)

Schumpeter was not alone in this assessment. Multiple testimonials reiterate a common theme: Marx attracted very little following outside of the peripheral socialist movements of the far-left during his lifetime, or for many years after his death in 1883. The distinguished British historian C. Northcote Parkinson (1967, 12) summarized the state of Marx’s renown at his death thusly: “[Marx’s] reputation as a theoretical revolutionary never spread in his lifetime beyond a narrow revolutionary circle. His articles in German had only the smallest circulation. His American articles were unsigned. In England he was virtually unknown, of little interest even to the police.” In the more recent bibliometric literature, empirical

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data have hinted at this pattern as well. For example, Steven Skiena and Charles Ward (2014, 78) classify Marx on a list of figures whose citation patterns strongly evince “posthumous fame,” defined as having an extremely high ratio between Google Ngram mentions dating at least 20 years after their death and mentions that occurred in their adult lifetimes. In this regard, he is comparable to Vincent van Gogh, Edgar Allan Poe, and Franz Kafka—all persons who were comparatively obscure in their lifetimes and only gained notice many years after their passing.

Academic testimonials generally concur, with several pointing to Marx’s peripheral influence in the American university system at the turn of the 20th century. During a visiting stint on the faculty at Harvard, Swiss economist William Rappard (1912, 122) remarked that “in the last century the influence of the followers of Karl Marx was negligible in America.” W. E. B. Du Bois, who by the 1930s had become a devout Marxist, concurred. He complained of the lack of Marxist instruction during his own time in the American university system in the 1890s, and indicated this situation did not change until 1917. Prior to the Russian Revolution, Du Bois (1933) observed, Marx “was treated condescendingly in the universities, and regarded even by the intelligent public as a radical agitator whose curious and inconvenient theories it was easy to refute.”

Marx’s early reputation was not much better in other countries. “[T]he inescapable fact is that the Marxist alternative was rejected by the overwhelming majority of late nineteenth-century Englishmen—whether economists, labour leaders, workers, politicians or intellectuals,” observed historian Kirk Willis (1977, 419). Willis’s assessment finds support in the testimonial of the British statesman and avid collector of philosophical treatises Arthur Balfour, who observed a century prior that “Marx is but little read in this country,” whereas “[Henry] George has been read a great deal” (Dilke 1885, 344; Mackay 1985, 33). Economist Ludwig von Mises (2010/1944, 154) concurred when writing about Marx’s dissemination at the turn of the twentieth century: “In the Anglo-Saxon countries Marxism in those days was practically unknown.” The philosopher Bertrand Russell (1918, 71) agreed, writing shortly after the Russian Revolution that “In England Marx has never had many followers.”

Germany “of all countries had the strongest Marxian tradition” according to Schumpeter (1947, 3), yet even there Marx’s reputation was sustained by “a small orthodox sect...kept alive during the post-war socialist boom as it had during the previous depression.” The leadership of its German political home, the Sozialdemokratische Partei Deutschlands (SPD), “while worshiping the deity [of Marx], took good care to keep it at a distance and to reason in economic matters exactly like other economists.” Ludwig von Mises (2010, 154) described these same intellectuals as a “very small groups of zealous Marxians—probably never more than a few hundred persons in the whole Reich” who “were completely segregated

from the [SPD] membership.” Subsequent investigations by Hans-Josef Steinberg (1976; 1979) and Eric Hobsbawm (1998, 8–9) have confirmed that Marx’s works attained only limited mainstream circulation in pre-World War I Germany. The SPD, “with its hundreds of thousands of members and millions of voters, published new editions of the Manifesto in print runs of not more than 2,000–3,000 copies” a year, per Hobsbawm’s estimates. In the decade from 1895 to 1905, this yielded only 16,000 printings. In sum, he concludes, Marx’s readers “were almost certainly not a representative sample of [the SPD’s] membership.”

The list of scholars who have remarked on Marx’s relative obscurity in the immediate decades following his death is long and distinguished. Among economists, Schumpeter, Rappard, and Mises are joined by Thomas Nixon Carver (1922) and Thomas Sowell (1985), among others. Philosophers Frederick Copleston (2003), Loren Lomasky (1989), and Alan Ryan (2014) have all posited that Marx would have remained a little-studied niche subject had the Bolsheviks not championed him at a later date. Non-Marxian socialists including G. D. H. Cole (1924) and H. G. Wells (1933) credited the Soviet Union with resuscitating a defeated thinker’s doctrines in Marx, as did distinguished Lenin biographer Louis Fischer (1964).

Many otherwise sympathetic scholars on the far left acknowledge that Marx enjoyed only peripheral influence in the late 19th century. In addition to the above-noted examples from Du Bois and Hobsbawm, similar assessments of Marx’s early reputation appear in the works of anarcho-communist political theorist Murray Bookchin (1996) and Marxist philosopher Alain Badiou (2019). Henry Hyndman (1911, 272), a British Marxist who became a personal acquaintance of Marx near the end of his life, remarked that “Marx was practically unknown to the English public,” save for a passing mention in association with other far-left radical movements. Even devout Marxists such as Fidel Castro (1961) readily conceded that Marx was “little known in his time” and his “work was only known in small circles,” even ranking behind other socialist contemporaries.

We call attention to this lengthy literature not only to show that it anticipates our own empirical analyses of the same questions about Marx’s reputation (Magness and Makovi 2023; 2024a; 2024b), but to also illustrate just how far outside of mainstream intellectual history Joseph Francis falls when he attacks our thesis. In his second response to our work, Francis is unequivocal in describing our conclusions as a “revisionist take” and a “novel and provocative” claim (2025, 102, 92). “In both of Magness and Makovi’s pieces,” he writes, “the novel aspect of their argument is that Karl Marx was not well-known before the Russian Revolution” (*ibid.*, abs.). “Revisionist,” “novel,” and “provocative” are strange descriptors to affix to an observation that Schumpeter made over 80 years ago, and that was put forth by dozens of noteworthy thinkers from across the political spectrum long

before we began our empirical investigation.

In our previous reply to Francis, we asked him to account for this substantial body of qualitative literature and its distance from his own allegedly “conventional” assessment, which portrays Marx as a well-known and influential figure in the late 19th century. Curiously, his most recent response offers no answer. It is an inauspicious beginning for someone who styles himself as a stalwart defender of qualitative approaches to intellectual history. It is also symptomatic of the flustered nature of Francis’s latest attacks on our findings.

## Answering a scattershot barrage

In this comment, we examine Francis’s (2025) second rejoinder to our article “The Mainstreaming of Marx” (Magness and Makovi 2023). To briefly recap, our article provided empirical evidence of the longstanding hypothesis that the Russian Revolution of 1917 rescued the reputation of Karl Marx from a state of rejection by most economists in the decades following his death, and elevated him into the intellectual mainstream as a preeminent thinker of the modern academic canon—albeit almost entirely outside of the economics profession. We used Google’s Ngram database of scanned books and parallel measures constructed from scanned-newspaper search engines to test the rate of Marx’s citations for treatment after the Russian Revolution, employing the Synthetic Control Method (SCM). Our findings establish clear evidence of a statistically significant treatment for Karl Marx’s name in 1917 against his synthetic counterfactual in English texts (Google Ngram and the Newspapers.com historical newspaper database), which we have successfully replicated in German-language sources (Google Ngram and the Deutsche Digitale Bibliothek newspaper database) (Magness and Makovi 2024b).<sup>3</sup> Combined with the existing qualitative literature, these data indicate that Marx migrated from a state of relative obscurity in his lifetime and for several decades thereafter to becoming one of the most heavily cited figures of all time in the aftermath of 1917.

Francis’s (2025) most recent critique makes it clear that he finds these

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3. Evidence for a treatment in 1917 has remained robust after more than a dozen subsequent SCM tests, using different specifications to account for alternative hypotheses of Marx’s post-1917 rise (Magness and Makovi 2023; 2024b; Gonzales et al. 2024). We interpret these results as empirical confirmation of the existing qualitative literature, including the long-standing observation that Marx remained a rejected and relatively obscure figure during the three decades following his death. As we acknowledged in our original paper, the main exceptions to this pattern came from (1) mainstream economists, who gave Marx’s economic arguments thorough consideration and found them lacking, and (2) radical activists on the far-left periphery of the political spectrum who adopted Marx as their intellectual prophet.

conclusions abhorrent, no matter the evidence we muster for them. It is admittedly difficult for us to extract a coherent thesis out of his latest scattershot array of objections, which now consist of multiple concurrent threads that pivot between blanket denunciations of empirical approaches to intellectual history, touting his own proposed alternative empirics to measure Marx's influence, selectively invoking the authority of a small number of historians who align with his interpretation of Marx, and attacking our paper over claims we did not make and on margins we did not purport to test. We have nonetheless attempted to add structure to this debate by focusing upon three broad categories of Francis's criticism of our empirical design.

First, we respond to Francis's ongoing attacks on our use of the SCM, including a novel addition to his criticism where he attempts to discredit the p-values of our main result. We show that Francis's objection rests on an elementary misunderstanding of statistical significance testing, and is further compounded by his failure to notice that we updated our source data to a newer and more accurate Google Ngram corpus between the draft and published versions of the paper. When combined, these oversights cause him to misinterpret our published p-values in ways that violate the most basic assumptions of statistical significance testing.

Second, we turn our attention to Francis's attempts to construct his own SCM test with the aim of invalidating our results. Francis selects another author, German novelist Thomas Mann, and claims to find evidence of a post-1917 treatment that vaguely matches Karl Marx. He then implies that our results for Marx must be spurious. We show that Francis has badly misinterpreted Mann's citation patterns by failing to notice a separate treatment event affecting this author, but not Marx, approximately seven years after the Russian Revolution.

Third, we scrutinize alternative citation measurement efforts by Francis, where he purports to provide additional evidence of Marx's pre-1917 importance. Francis's alternative approaches generally eschew econometric analysis, preferring instead to make comparisons between individual authors through crude visual observations from Google's Ngram viewer and simple tabulations from the JSTOR database. We show that Francis's continuation down this path has failed to answer our previous criticisms of his JSTOR tabulations. Furthermore, we discover that Francis neglected to disable the automatic smoothing feature of the online Ngram viewer tool when constructing one of his primary alternative tabulations, thereby causing him to misinterpret the resulting visual depiction. Viewed in sum, Francis's objections to our empirical design have fallen into a pattern of endorsing or rejecting individual authors on an ad hoc basis, while resisting all attempts to get him to specify general criteria for their inclusion or exclusion. This renders his objections unfalsifiable, and contingent upon

improvised rationales.

We conclude our assessment by returning to the qualitative literature on Marx's dissemination. Although Francis claims to present a "conventional" account of Marx's intellectual adoption in contrast with our "revisionist" thesis, we show that he has cherry-picked his narrative from just two secondary sources, Isaiah Berlin and Leszek Kolakowski, without accounting for numerous other scholars who lend direct support to our argument or, in Berlin's case, passages that contradict Francis's reading of an author he has chosen to endorse. We suggest that this shortcoming is symptomatic of a deeper problem with Francis's criticism insofar as he has settled into a haphazard approach to attacking our thesis on any margin where he thinks he can land a hit. His latest attacks not only fail under scrutiny, but they reveal Francis's own unfamiliarity with both the empirical and qualitative literature on the subjects he attempts to critique.

## The trouble with p-values

In his most recent response, Francis attempts a novel line of criticism of our SCM results by claiming that we experienced "a substantial loss of statistical significance in the published version," compared to earlier drafts of our paper.<sup>4</sup> The basis for his charge is that we obtained a p-value of 0.047 for the main result in our published paper, whereas our earlier working paper draft (Magness and Makovi 2020) obtained preliminary results of exactly 0 (zero), using a much smaller donor pool and dataset.<sup>5</sup> Francis then declares that our published "JPE article's headline Synthetic Marx verges on statistical insignificance" (2025, 96). Reasoning that since 0.047 is close to the conventional marker of  $p = 0.05$  for 95% confidence, Francis asserts that our "results therefore seem quite fragile" despite meeting this widely accepted threshold. At another point Francis even slips into describing our finding as "statistically insignificant" (*ibid.*, 97) based on alleged comparison to other p-values.

Francis's contentions here are concerning, as they appear to betray his own

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4. In a footnote, Francis objects (2025, 96 n.4) that "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." But in our original paper (Magness and Makovi 2023, 1524), we explicitly acknowledged this limitation of the AEP, citing literally the exact same references that Francis does. We explained that therefore we were also using the more conservative Simes method of p-value meta-analysis.

5. SCM can obtain a p-value of exactly zero because it estimates p-values by a non-parametric method. The SCM procedure is repeated for every untreated donor unit, and the p-value is the number of untreated donors who experience a treatment effect at least as large as the treated unit's, divided by the number of trials. If the treated unit experiences the largest treatment effect, its p-value will be exactly zero.

unfamiliarity with statistical significance testing at an elementary level. The adoption of the 0.05 threshold dates to Sir Ronald Fisher's classic text, *Statistical Methods for Research Workers*. Fisher (1925, 45) analyzed the normal distribution and observed that "[t]he value for which  $p = .05$ , or 1 in 20, is 1.96 or nearly 2" standard deviations from the central point of the distribution. Noting this near-convergence, Fisher reasoned that "it is convenient to take this point as a limit in judging whether a deviation is to be considered significant or not." The 95% confidence threshold was thus born, with Fisher observing that "[d]eviations exceeding twice the standard deviation are thus formally regarded as significant." This threshold is therefore a reflection of probabilistic testing to rule out the likelihood of a false indicator falling on the tail of the normal distribution (as is also the case of other commonly accepted thresholds for social science including the higher p-value of 0.01, conveying 99% confidence, and more relaxed p-value of 0.1, or 90% confidence).

Note, by implication, that a result does not become "**in**-significant" by falling beyond the 0.05 threshold. Crossing this level simply reflects a slightly higher probability of a tail result, albeit one that still meets a more relaxed threshold for significance (e.g. 90% confidence, and so forth). And yet our main result does not even fall outside of the 0.05 threshold, Francis's creative wordsmithing notwithstanding. By describing it as "verg[ing] on statistical insignificance," Francis not only finds himself dismissing a result that clearly meets the acceptable norms of econometric research. He also commits an elementary misinterpretation of the meaning of p-values, reminiscent of the type of error one commonly finds when grading exams in an introductory undergraduate statistics class.

Francis next contends that the change in p-values between our earlier working paper and the published results is evidence of weakness in the latter. He writes that the "extent to which the level of statistical significance can be changed through adjustments to the donor pool is worrying" (2025, 97). This criticism is laughable on its face, as p-values are virtually certain to exhibit small changes following modifications to the composition of a dataset, including the addition of new data. But Francis also compounds his interpretive error by failing to account for another change between the 2020 draft and 2023 published version of our paper.

Francis attributes the differences in our main p-value to a large expansion of names in our SCM donor pool between the two versions. It is true that we expanded our donor pool from an initial 97 authors to 225 (in addition to Karl Marx). We did so for a number of reasons: (1) to take advantage of increased computing power, (2) to allow for more extensive robustness testing using subsets of the donor pool, (3) to improve the overall comprehensiveness of our analysis, and (4) to satisfy the requests of referees for additional donors.<sup>6</sup> In doing so, we

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6. Francis (2025, 96) says, "the problem seems to have arisen when numerous German-language donors



confirmed our initial finding of a treatment in 1917 while also remaining within the commonly accepted threshold for significance testing. It is simply not the case, however, that different p-values reveal an unacceptable sensitivity to “adjustments to the donor pool,” as Francis contends. A different explanation may be found in changes to the Ngram corpus. Our 2020 draft paper used Ngram data taken from the 2012 Google Books corpus, which was the most recent available series at the time when we ran our initial SCM test. In mid-2020, Google released an updated corpus through 2019.<sup>7</sup> This release featured a larger body of scanned historical books as well as accuracy improvements to Optical Character Recognition (OCR) technology. We accordingly updated our dataset to the 2019 corpus as our paper was undergoing review.

The text of our published paper and its appendices described the 2019 update of the Google Books corpus in detail (Magness and Makovi 2023, 1525), indicating that its SCM test involved an expanded and improved underlying dataset. Just the same, our earlier draft clearly noted that it used the 2012 corpus (Magness and Makovi 2020, 14–15, 33). Francis now claims to show, by comparison of the two papers, that “changes in [our] p-value are affected by the composition of the donor pool” (2025, 96). In reality, he is attempting to parse differences in p-values by segmenting the additions to our donor pool without realizing that the underlying data corpus has also changed.

In Table 1 we present p-values for our successive SCM tests of a 1917 treatment for Karl Marx, including robustness tests using subsets of the full donor pool.<sup>8</sup> Magness and Makovi (2023) is our original published paper, using the 2019 Google Ngram corpus. Magness and Makovi (2020) is the SSRN draft of that same paper, using the 2012 Google Ngram corpus and a smaller donor pool. Magness and Makovi (2024a) is our previous reply to Francis (2024) in *Econ Journal Watch*. Magness and Makovi (2024b) is a recently published sequel to that paper, primarily testing the effect of the 1891 Erfurt Program on Marx’s citations, but secondarily using German newspapers to test the effect of 1917.

Several new SCM results are also presented in Table 1, all calculated from the

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were added to the pool, perhaps on the suggestion of a peer reviewer.” Francis complains that “The logic of their research design is therefore hard to fathom” because our donors are so wide-ranging. But at least part of the reason lies with our obligation to satisfy our referees. Nevertheless, as we note later, our original draft (Magness and Makovi 2020) and published paper (Magness and Makovi 2023) both include a robustness test which restricts the donor pool to socialists only. This single test obviates all of Francis’s complaints about the idiosyncrasy of our donor pool, and how Marx is being compared to dissimilar authors.

7. See Google’s announcement ([link](#)) and additional information ([link](#)).

8. We also omit Bernstein and Bebel from the donor pool of this newest test, following Magness and Makovi (2024b), who realized that because Bernstein and Bebel were instrumental in the 1891 Erfurt Program of the SPD, they should be considered to potentially share Marx’s treatment—meaning they should be omitted for much the same reason as Engels and Lenin are.



new 2024 Google Ngram corpus. These results update our tests using the earlier Ngram releases and are included for comparison. First, we test the effect of 1917 in both the English and German languages. Next, we test the “socialists only” and “non-socialists only” subsamples. And third, for the first time, we test a subsample of authors who are exclusively “political.” All of these new results, original to the present paper, are labeled in the table as “New result.”

In Table 1 we group common specifications together in adjacent rows, so that a given specification can be compared across different data sources. For example, the first three rows list the p-values of the “main result” using the Google Ngram corpora for 2019, 2012, and 2024. Reading across the columns, the first column describes the test and the second column lists the sample size (number of donors or placebo units).<sup>9</sup> The third column lists the data source (e.g., 2019 English Google Ngrams), the fourth column lists the citation (e.g., Magness and Makovi 2023, 1530 Table 4), and the last several columns list the different p-values.

Scanning down Table 1, one can see how remarkably stable and consistent the p-values are. Across different data sources and donor pools, the p-values are nearly always between 0.01 to 0.07. In fact, the new 2024 Google Ngram corpus has caused p-values to become smaller in several of our tests. Under “main result,” the joint post p-value has fallen from 0.047 in Magness and Makovi (2023) to 0.016 using the 2024 corpus. Under “German Google Ngrams (through 1932, all authors),” the joint post standardized p-value has fallen from 0.069 to 0.014.

Contrary to Francis’s claims of fragility, these tests evince the robustness of our findings. No matter how we change our donor pool or data source, the results for a 1917 treatment remain statistically significant at conventional levels.<sup>10</sup> If anything, it appears we would probably have to engage in p-hacking to obtain a result that fell well outside of conventional significance levels.<sup>11</sup>

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9. The sample sizes vary for three reasons. First, the draft (Magness and Makovi 2020) had slightly less than 100 donors, while all subsequent papers have slightly more than 200 donors. Second, some donors’ placebo tests fail to numerically converge, so every test has a slightly different number of usable donors. Third, some tests deliberately restrict the sample, such as by using only socialists or only non-socialists.

10. González et al. (2024) replicate the magnitude of the treatment effect (of Magness and Makovi 2023) with several new robustness tests (e.g., adding control variables for authors’ birth years), but they do not attempt to estimate p-values.

11. The lone exception to the robustness of our p-values is near the bottom of the table, under “German newspapers,” reporting results from Magness and Makovi (2024b). There, the p-values are 0.180, 0.147, and 0.208. But as we note in Magness and Makovi (2024b, 19 Table 7), these p-values should be meta-analyzed with the original p-values reported in Magness and Makovi (2023). To quote Magness and Makovi (2024b, 17),

The effect[] of 1917...[has] been tested using two independent datasets—viz. German-language Google Ngrams and German newspapers. Meta-analysis will be used to

TABLE 1. A comparison of p-values for tests of Synthetic Karl Marx

Description	No. of donors	Data source	Magness and Makovi citation	P-value (standardized)		
				Joint post	AEP	Simes
Main result	193	2019 Eng Ngrams	2023, 1530 Table 4	0.047	0.032	0.083
Main result	96	2012 Eng Ngrams	2020, 49 Table 6	0		
Main result	189	2024 Eng Ngrams	New result	0.016	0.017	0.028
English-lang. newspapers	17	Newspapers.com	2023, 1532 Table 5	0	0.109	0.078
German Ngrams thru 1932, all authors	203	2019 Ger Ngrams	2023, 1535 Table 7	0.069	0.031	0.063
German Ngrams thru 1932, all authors	94	2012 Ger Ngrams	2020, 57 Table 14	0.043		
German Ngrams thru 1932, all authors	210	2024 Ger Ngrams	New result	0.014	0.009	0.013
German Ngrams thru 1932, German authors only	81	2019 Ger Ngrams	2023, 1535 Table 7	0.062	0.032	0.04
Socialists only	18	2019 Eng Ngrams	2023, Appendix A19 Table A.3	0	0.084	0.059
Socialists only	12	2012 Eng Ngrams	2020, 50 Table 7	0		
Socialists only	16	2024 Eng Ngrams	New result	0	0.097	0.067
Non-socialists only	175	2019 Eng Ngrams	2023, Appendix A23 Table A.4	0.046	0.025	0.055
Non-socialists only	82	2012 Eng Ngrams	2020, 51 Table 8	0		
Non-socialists only	172	2024 Eng Ngrams	New result	0.012	0.013	0.023
Political authors only	75	2024 Eng Ngrams	New result	0	0.016	0.014
Cross-validation (dividing pre-treatment period into training and validation periods)	186	2019 Eng Ngrams	2023, Appendix A27 Table A.5	0.048	0.037	0.086
Cross-validation	94	2012 Eng Ngrams	2020, 55 Table 12	0.064		
Outcomes for “Marx”	193	2019 Eng Ngrams	2023, Appendix A31 Table A.6	0.047	0.033	0.083
Outcomes for “Marx”	96	2012 Eng Ngrams	2020, 52 Table 9	0.042		

determine their joint or overall statistical significance. Two large and nonsignificant p-values from two independent studies may imply a smaller and significant p-value when combined. This is one reason why failure to publish statistically non-significant results can lead to publication bias and biased meta-analysis.

There, we found that the p-value of 0.069 from Magness and Makovi (2023) combined with the p-value of 0.180 from Magness and Makovi (2024b) yielded meta p-values of 0.067 (Fisher method) and 0.031 (Edgington method). As we observed in Magness and Makovi (2024b, 18), “[t]his indicates a successful replication of the previous German-language Ngram results using German newspapers. Although the newspaper result is not statistically significant when taken in isolation, it has nevertheless increased the overall level of significance when considered in combination with the prior result.”

Description	No. of donors	Data source	Magness and Makovi citation	P-value (standardized)		
				Joint post	AEP	Simes
Normalization (normalizing all author outcomes to exactly 1 in each of the pre-treatment years, running SCM once each time, and aggregating all the resulting p-values)	193–195	2019 Eng Ngrams	2023, Appendix A34 Table A.8		0.060, 0.026, 0.048	0.083, 0.032, 0.052
Normalization	96–97	2012 Eng Ngrams	2020, 54 Table 11		0.023	0.037
Restrict sample to authors whose citations are between half and double Karl Marx's	32	2019 Eng Ngrams	2024a, 401f.	0	0.052	0.042
Restrict sample to authors whose citations are between half and double Karl Marx's	35	2024 Eng Ngrams	New result	0.029	0.058	0.065
German newspapers	200	Deutsche Digitale Bibliothek	2024b, 17 Table 5	0.18	0.147	0.208
Meta-analysis of German Google Ngrams and German newspapers			2024b, 19 Table 7		0.067 (Fisher) 0.031 (Edgington)	Meta-analyzed from p-values of 0.069 and 0.180.

Francis complains that our p-values have risen between our 2020 working paper and the 2023 published version. But the nature of probabilistic statistics is that effect sizes and standard errors will never be the same when the specification changes. Does Francis assert that a study will never be valid whenever p-values change the slightest amount from one robustness test to the next? Or does he assert that p-values should only ever decrease and never increase? If a published paper has a p-value of 0.01 and a replication by another author—with a slightly different specification—finds a p-value of 0.02, would Francis claim that this invalidates the original paper? That would be absurd, and yet that seems to be Francis's standard.

Francis is unfazed by the consistency of our successful replications, each designed to exert strain on our main result. Indeed, he sidesteps them while constructing a confused and tendentiously argued case against an econometric norm. We are only left to wonder if such a hapless line of attack could survive the scrutiny of a competent peer reviewer.

## **Of whales, minnows, and falsifiable scientific criteria**

Repeating his last critique of our paper, Francis continues to assert that our SCM results for Marx are a byproduct of the presence of what he calls “whales” in

our donor pool. A “whale,” according to Francis, is a figure who had high levels of raw citations in the late 19th and early 20th centuries. He continues to give the example of Abraham Lincoln, and claims that Lincoln’s high level of Ngram citations is effectively driving our main result vis-a-vis other “minnows,” or donors with smaller levels.

Part of Francis’s argument here rests upon his confusion over the purpose and claims of our SCM test. Francis observes (2025, 92) that “levels are central to the novel and provocative aspect of Magness and Makovi’s argument” because we claim that Marx was just “a second- or third-tier scholar, mainly known within socialist circles and from highly specialized criticisms within the economics discipline.” Francis continues that “In this way, Magness and Makovi obviously refer to levels, despite their assertion that they are only interested in rates of change.”

But as we documented in our previous response, our statistical tests attempted to measure relative rates of change rather than absolute citation levels due to the limitations of the Ngram data source. In our original paper (Magness and Makovi 2023, 1526), we described the measurement issues with Google Ngram—especially the fact that some authors are most easily identified by “firstname lastname” while others are identifiable only as “lastname.” Therefore, we said, “In the face of multiple sources of measurement bias, we assume that Google Ngrams can identify relative rates of change over time. ... If a weighted average of several authors’ names reliably predicts outcomes for the name ‘Karl Marx’ before 1917 but not after 1917, we consider this evidence of a treatment effect, which changed the relative citation rate even though we cannot identify absolute levels.” In other words, while Francis is correct that absolute citation levels are also implied by our basic research question, our empirical strategy uses rates of change to test for treatment given that absolute levels are not consistently identifiable for all combinations of names in the available data.<sup>12</sup>

At the same time, we have reason to believe that Marx’s absolute citations grew as well, owing to the extensive qualitative literature that aligns with our results. As we’ve documented, numerous distinguished scholars have observed that Marx was unknown or lesser-known until the Russian Revolution propelled him to international fame and renown. It was this qualitative claim—made by others—

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12. Anticipating that Francis will likely pivot to using this observation as a pretext for dismissing Ngram entirely, we note that the same difficulty in consistently measuring absolute levels is a challenge for all OCR text measurement databases, including Francis’s own preferred alternative of JSTOR. The bibliometric literature around OCR is already well aware of these and similar challenges. For example, Skiena and Ward (2014, 74–76) note the difficulty of distinguishing Martin Luther from Martin Luther King, Jr. and astronomer Oliver Wendell from Oliver Wendell Holmes. For a further discussion see Magness and Makovi 2023, Appendix A.6.

that we resolved to investigate with a new statistical measure. Due to data limitations, we could not directly test absolute levels themselves, so we instead tested relative rates of change. We interpreted a treatment effect that increased Marx's relative rate of change as indirect but strong evidence for these qualitative observations.

This discrepancy between a research interest and its statistical formalization or test is not unusual. It is quite commonplace for researchers to be unable to measure or identify the target outcome, forcing them to instead measure some proxy. Francis has nonetheless fixated upon absolute levels as the sole margin on which he is willing to interpret our results. In his latest response he dismisses the difference between rates of change and absolute levels as a “semantic distinction” and proceeds as if his “whales” vs. “minnows” analogy remains intact (2025, 92, 102). Francis's grievances with our study accordingly amount to berating us for testing on a different empirical margin than the one he wishes we tested, and for using a different dataset (Ngram) than the one he prefers (JSTOR), even though his choice exhibits the same limitations from OCR.

At the same time, Francis does not seem to acknowledge the strongest evidence against his objection to our SCM test of Ngram, viz., our “socialists-only” test found in the Appendix of our original paper (Magness and Makovi 2023, A.4.1). As we observe there,

It is important in SCM to restrict the donor pool to authors who are *prima facie* similar to the treated unit. SCM avoids extrapolation bias by restricting donor weights to be non-negative and sum to one, but SCM cannot avoid interpolation bias if donors whose indicator values are much smaller than the treated unit's are averaged with donors whose indicator values are much larger, resulting in a synthetic control whose indicator values match the donor's, but whose donors individually are very different than the treated unit (Abadie 2021, 409). ... Therefore, we repeat our procedure using a sample of only socialists.

Using a sample of only socialists, we obtained a p-value of exactly zero. This outcome from a socialist-only donor pool should not be possible if non-socialist “whales” are sustaining our synthetic counterfactual for Karl Marx, as Francis alleges.

Virtually all of Francis's criticisms of mixing “whales” and “minnows,” and of including disparate and dissimilar authors such as Lincoln, Aesop, and Nietzsche, were already addressed in the Appendix of our original publication. Francis objects (2025, 94) that “The contrast with the classic articles in the SCM genre is stark” because in canonical studies, “they were summing things that were similar. ... 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.” And yet we *did* restrict our donors to “things that were similar.” Through his negligent reading of our paper, Francis accuses us of failing to do the very thing that we did in fact do, as per the preceding SCM literature.

Francis (2025, 93) nonetheless excoriates us for his own oversight, saying, “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.’” We are dumbfounded by the accusation because our paper explicitly acknowledged and addressed Abadie’s injunction.

We wonder: if our original paper’s headline, main result had been our “socialist-only” test, and if all our other tests had been relegated to the robustness sections or the Appendix, would Francis have ever written any response for *Econ Journal Watch* at all? Did Francis simply not read our Appendix thoroughly? In fact, in our original draft (Makovi and Magness 2020, 21)—which Francis cites—the socialist-only test occurs in the main body (section 3.2.1), not in any appendix. It was regrettably relegated to the published Appendix only to meet the publisher’s word/page-count restrictions, although we referenced it in a footnote of the main paper (Magness and Makovi 2023, 1530 n.36). Francis’s lack of diligence in basic reading cannot be faulted to us.

In our previous response to Francis, we nonetheless entertained his suggestion that “whales” were driving our main result. As noted, he previously alleged that Abraham Lincoln was distorting our synthetic counterfactual for Marx by pulling up the other donors, predominantly consisting of less-cited socialist contemporaries in the donor pool. In our previous reply (Magness and Makovi 2024a) we provided evidence of a clear treatment of Marx in 1917 even after removing Lincoln, the alleged “whale.” Francis is still not satisfied, and now objects that our test excluding Lincoln selects another “whale” in its donor pool, Isaac Newton. He continues to miss the most relevant criterion, namely that neither Lincoln nor Newton exhibited treatment effects that altered their respective rates of citation in 1917.<sup>13</sup>

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13. In his latest rejoinder, Francis (2025, 93) further claims that we “implicitly suggest” our main results should be rejected, pointing to a robustness test in our original paper that selected Nietzsche in its donor pool. Francis is misrepresenting this robustness test, which aimed to determine whether the common German surname “Marx” could function as a reliable proxy for “Karl Marx” in the Ngram dataset. (Groucho Marx and the Weimar German chancellor Wilhelm Marx are just two examples of why the name “Marx” may not identify citations of Karl Marx.) While we found evidence of a treatment in 1917, we decided against using this broader term because the selected donor pool, consisting of Nietzsche and other non-socialist writers with only one exception, “fails to achieve adequate indicator balance for the socialist indicator variable” (Magness and Makovi 2023, Online Appendix A29). We concluded,



In response to Francis’s previous comment, we also conducted an interpolation bias test that intentionally constrained the SCM donor pool to a fixed proximity around Karl Marx’s pre-treatment citation levels, thereby eliminating the possibility of “whales” entirely. The lower boundary was set at 50 percent of Marx’s pre-treatment level, whereas the upper boundary was twice Marx’s level. By design, this test excluded alleged “whales” like Lincoln and Newton, as well as smaller “minnows” that fell below the lower citation level threshold. Even with these parameters, we were still able to obtain clear evidence of a treatment for Marx’s citation rate in 1917. By implication, our main finding is robust to the “whales” and “minnows” critique.

Francis’s only answer to this second test is to shift tack to a different set of ad hoc objections against individual donors. In place of Lincoln and Newton, Francis now complains that our constrained donor pool contained 18th-century German philosopher Johann Georg Hamann, although he offers no elaboration for this *sui generis* dismissal.<sup>14</sup> Next, he objects that our constrained donor pool test selected Oscar Wilde, and accuses us of failing to make the case for why Wilde is “a meaningful counterfactual for Marx.” This case is not difficult. In addition to his literary work, Wilde was a non-Marxian socialist and author of the 1891 pamphlet “The Soul of Man Under Socialism.” We specifically called attention to this fact in our original paper, which also selected Wilde in the donor pool of its main result (Magness and Makovi 2023, 1528). In his search for yet another ad hoc reason to dismiss a disliked donor, Francis has instead revealed his own inattention to detail when reading our paper.

Francis continues his case against our SCM results by making different ad hoc objections to Ferdinand Lassalle. Lassalle’s direct relationship with Marx and

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Our failure to achieve indicator balance when using citations of “Marx” does not impugn our primary results, but it does call into question the validity of using outcomes for “Marx” to test the robustness of our primary result. In other words, these results for “Marx” do not contradict our primary result, but their value as supporting evidence is questionable. Therefore, we rely more on the normalization test reported in the next section. (Magness and Makovi 2023, Online Appendix A29)

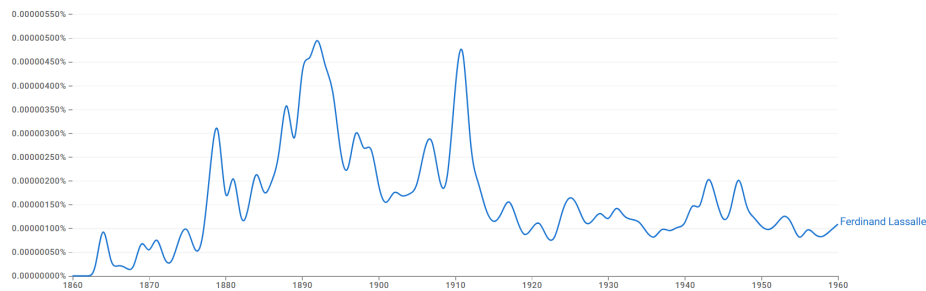
In other words, we did not—as Francis claims (2025, 93)—“reject this robustness test.” Instead, we declined to rely on it. In fact, we said we “rely more” on the subsequent test. We said this robustness test was “questionable”—meaning we did not reject it, but someone could perhaps reasonably reject it. Therefore, we relied less on this test and more on a subsequent robustness test.

14. We note that Francis refers to “Hamman” (*sic*) as an “an eighteenth-century theologian” and implies that this is sufficient to dismiss him as a donor. Hamann (1730–1788) is best known, however, as a philosopher of the German Counter-Enlightenment, and for being the primary subject of a critical treatise by G. F. W. Hegel, published in 1828. Hamann was also the academic mentor of Johann Gottfried von Herder, whose works Marx studied during his academic training. Both characteristics place him within Marx’s own intellectual genealogy and lend credence to his inclusion as a plausible counterfactual.

Engels and his status as the leader of a rival socialist faction make him difficult to disqualify over lack of ideological kinship or other contemporary similarities. Instead, Francis pivots to attacking Lassalle as a “minor socialist” (2025, 92, 104). Francis struggled with Lassalle in his previous response. He claimed that the socialist organizer’s influence went into steady decline after his death in 1864, when in fact Lassalle’s Ngram citations rose for several decades thereafter. To his credit Francis now acknowledges his error, only to follow it with the brazen assertion that “this correction does not...alter my original critique” (ibid., 94). Instead, he contends that Lassalle exhibits a “falling n-gram share from 1891 onwards” and claims that our SCM algorithm must “counteract” this pattern with “younger donors” such as Hermann Ebbinghaus (1850–1909), yielding what he then dismisses as a “Frankenstein’s monster” for the synthetic counterfactual (94).

A closer look at Lassalle reveals that, yet again, Francis has misstated his citation patterns. As Figure 1 below shows, Lassalle hit an initial peak in 1891, before dropping back to a level that nonetheless exceeded his 1880s citation patterns. This is almost certainly a reflection of the leadership decisions at the SPD’s 1891 Erfurt Congress, where the Marxist faction of the pattern triumphed over the Lassallean faction. But Lassalle nearly matches his 1891 citation level a second time in 1910—a fact that goes unmentioned by Francis. Although Lassalle returned to the lower plateau by 1913, he remained at this stable level for several decades thereafter and, notably, did not exhibit any signs of treatment in 1917.

**Figure 1.** English Ngram for Ferdinand Lassalle

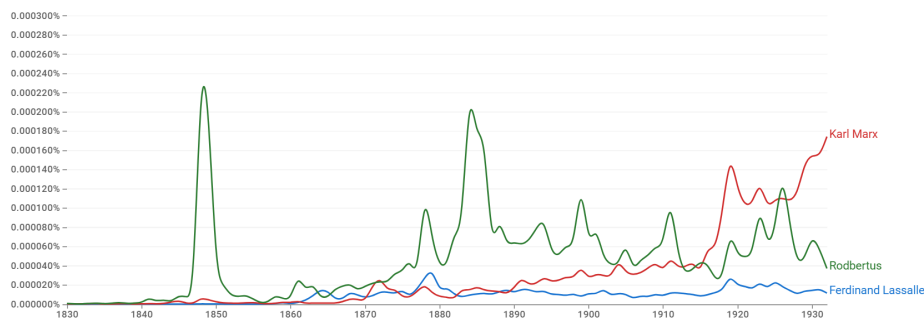


*Source:* Google Ngram Viewer, 2024 corpus.

Tellingly, Francis’s (2025, 95 Figure 2) own normalized comparison of Lassalle and Ebbinghaus does not depict the story he describes. Ebbinghaus hit his own peak in 1909, a year before Lassalle’s second peak in 1910. Thereafter, both authors settle into a stable—and nearly identical—plateau in the normalized patterns. Again, we find that Francis is simply not a reliable interpreter of his own data depictions.

Suppose, for a moment, that we were to adopt Francis’s preferred approach of only examining citation levels using visual identification from raw Ngram totals. Instead of ad hoc rationalizations around specific donors, a properly constructed comparison of this type should draw upon the qualitative historical literature. Although Francis dismisses Lassalle and Johann Karl Rodbertus as “minor socialists,” we previously established (Magness and Makovi 2024a, 404) that these two contemporary German competitors of Marx were viewed with comparable esteem to Marx among late 19th-century mainstream scholars of socialism.<sup>15</sup> Since all three originally published their works in German and since the same 19th-century scholarly commentaries compared their statuses in the context of competing German socialist schools, we would also employ the German language portion of the Ngram corpus to make this comparison. The resulting comparison of raw Ngram levels appears in Figure 2. It too is at odds with Francis’s story.

**Figure 2.** German Ngram comparison of Marx, Lassalle, and Rodbertus



*Source:* Google Ngram Viewer, 2024 corpus.

Several observations may be made. First, for most of the 19th century, Rodbertus is indeed the more recognizable socialist name in German-language sources and easily exceeds Marx until the 1910s.<sup>16</sup> He attained fame before the other two authors in 1849, a reflection of his service as the opposition leader and then, briefly, a cabinet minister in the Prussian National Assembly. Rodbertus retired from politics thereafter, but his economic writings on the theory of “surplus value” attracted scholarly attention from the 1870s until the early 20th century.

15. In fact, multiple authors in the late 19th century identified Rodbertus as a stronger expositor of German socialism than Marx (see Osgood 1886; Böhm-Bawerk 1890; Andrews 1892).

16. We use Rodbertus’s last name alone because of its recognized uniqueness, and because it was the most common iteration of his name in German sources. The resulting Ngram pattern suggests that we may reliably isolate Rodbertus through this term, as seen in the late-1840s citation spike that coincided with his time in political office.

This pattern matches several late 19th-century attestations that ranked Rodbertus's exposition of German socialism above Marx (Osgood 1886; Böhm-Bawerk 1890; Andrews 1892). It also accords with the attention that Engels gave to rebutting the Rodbertians after Marx's death, including his introduction to the second volume of Marx's *Das Kapital* (1885). Far from being a "minor socialist," Rodbertus was acknowledged as a primary socialist competitor of Marx in the eyes of Marx's own intellectual heir and curator.

Second, Lassalle's German citation pattern is closely comparable in level to Marx during their respective lifetimes and for another decade thereafter. Lassalle had slightly higher levels of citations than Marx in almost all years prior to the aforementioned 1891 Erfurt Congress. After Erfurt, Marx pulled ahead of Lassalle, who later plateaued. Marx's growth in this period was gradual though, until undergoing a rapid acceleration after 1917.

Third, Marx lagged behind both of his German socialist competitors for most years in his lifetime. The only point where Marx surpassed both Rodbertus and Lassalle was 1871, presumably due to a temporary bump in attention on the socialist far left that followed from Marx's writings about the Paris Commune. Otherwise, Marx was consistently in third place behind his two most viable German-language competitors until gradually overtaking Lassalle after 1891. Would this qualify Marx as a "minor socialist" in his lifetime? We leave that to the reader.

We offer this German-language example as an exercise to illustrate the haphazard approach that Francis has taken to selecting authors for comparison, even when restricting our scope to visual observations of citation levels.<sup>17</sup> Of course, our empirical design's focus remains on the rate of change in Ngram citations, not absolute levels.

In our previous response, we noted that "Francis offers no consistent set of objective criteria" for the selection of an author to compare with Marx (Magness and Makovi 2024a, 405). Despite our invitation to propose his donor pool criteria, Francis still has no answer and instead makes only ad hoc objections to individual donors. We see little value in following Francis down the path of semantic tedium, and simply note that his posited "whales vs. minnows" distinction remains unfalsifiable in the terms he has presented it. Potential donors are designated "whales," "minnows," or irrelevant comparisons on the whims of Francis's own subjective tastes for them, with only Marx retaining an equally subjective status as a figure of late 19th-century importance. Such an approach may be dismissed as

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17. One virtue of the SCM is that it selects its donors without the biases of ad hoc comparison on subjective criteria. We therefore only use the German Ngram comparison of Marx, Lassalle, and Rodbertus to illustrate how ad hoc comparison can nonetheless lead to conclusions in line with our SCM test.

unscientific editorializing under Karl Popper’s (2002, 316) criteria: “In so far as a scientific statement speaks about reality, it must be falsifiable: and in so far as it is not falsifiable, it does not speak about reality.”

## Testing for a misidentified treatment

We turn our attention next to Francis’s addition of a parallel SCM test, intended to mirror our main result as a point of comparison. He proposes this alternative “robustness test” by borrowing a subset of 55 German-language authors from our English Ngram donor pool. He then runs SCM tests in this subset for Karl Marx and, separately, for German novelist Thomas Mann, using 1917 as a treatment date for both.<sup>18</sup> Referring to his test of Mann, Francis then claims to have found “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” (2025, 97). By implication, Francis then claims to have invalidated our main result for Karl Marx by showing that “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” spiked in the same period.

Francis bases this conclusion upon an alleged visual similarity between his two SCM tests, as both Marx and Mann diverge from the patterns of their synthetic counterfactuals in the 1920s. A closer look establishes that Francis has once again misinterpreted the underlying data due to what appears to be simple inattention to detail.

Figure 3 shows the comparative evolution of Marx and Mann’s Ngram citations. While Marx and Mann both rise in the 1920s, Marx’s citations spike begins immediately following the treatment year of 1917. Mann’s citations remained at approximately their pre-1917 level for another seven years and only started to rise around 1924.

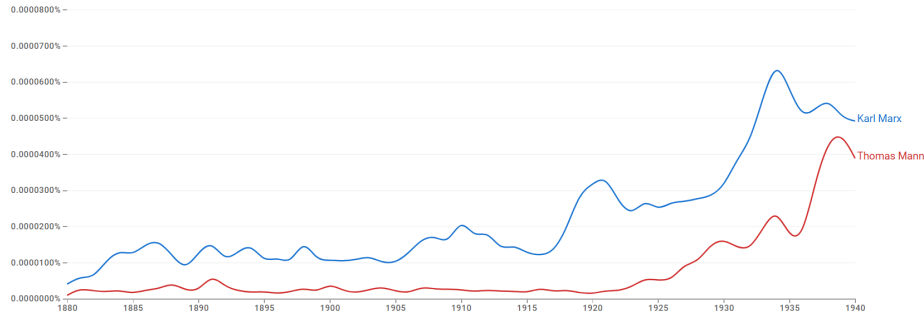
The reason for Mann’s later surge relative to Marx is not difficult to identify. The year 1924 marks the publication of *Der Zauberberg*, or “The Magic Mountain.”

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18. We note that in constructing his own test of the German-language portion of our donor pool, Francis ignores the fact that we have analyzed and expanded upon this same subset of donors through additional robustness checks using SCM (Magness and Makovi 2024b). It is not clear why Francis’s separate and less-comprehensive test of German-language authors yields a p-value outside of the 90% threshold, although he does not attempt to reconcile this disparate finding with our subsequent work on German-language sources. Oddly, Francis also uses his own German-language SCM tests to argue that changes in his own generated p-values somehow invalidate our separate findings. For reasons that should be abundantly clear, we lack sufficient confidence in Francis’s implementation of SCM to assess the meaning of his results for our own findings, and only claim credit for the specific SCM tests that we have performed in Magness and Makovi (2023; 2024a; 2024b).

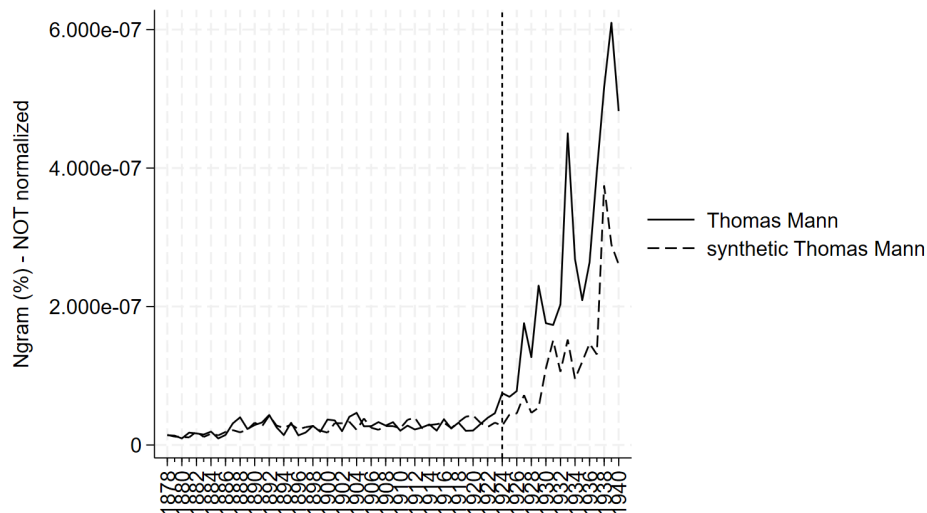
Rather than detecting a spurious treatment effect from the 1917 Russian Revolution on Mann’s citations, Francis has unwittingly picked up a separate and well-known treatment of Mann after 1924, when his most famous novel’s publication earned him international literary acclaim.

**Figure 3.** Karl Marx vs. Thomas Mann in English Ngram



Source: Google Ngram Viewer, 2024 corpus.

**Figure 4.** Synthetic Thomas Mann with treatment in 1924



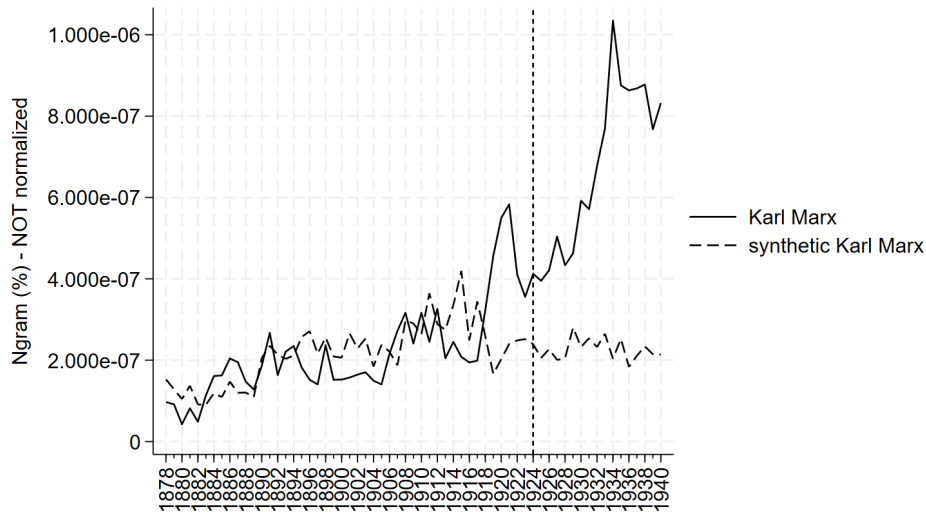
In Figure 4 we show this distinct treatment by replicating Francis’s SCM test of Thomas Mann with the year set to 1924 instead of 1917. We use the original dataset from Magness and Makovi (2023), testing 2019 English Ngram results for the subset of authors who wrote in German. Because the treatment date is a few years later—1924 rather than 1917—we also extend the end of the post-treatment period to 1940. And like Francis, we refrain from normalizing outcomes to the



interval  $[0,1]$ —although in theory, this should make no difference.<sup>19</sup> We find strong evidence of Mann’s treatment in 1924. P-values (standardized) are 0.024 (joint post), 0.039 (AEP), and 0.040 (Simes). The sample size (donors used) is 42.

We also repeat Francis’s proposed test for Karl Marx, using the same donor pool of authors who wrote in German and the same 2019 English-language Google Ngrams, and only changing the treatment date to 1924 to match Mann. When Marx’s treatment is set to 1924, the effect is *not* statistically significant—with standardized P-values of 0.238 (joint post), 0.264 (AEP), and 0.202 (Simes). Figure 5 makes it apparent why Marx loses statistical significance, as his divergence from the synthetic counterfactual began seven years earlier with the Russian Revolution. Unlike Mann, 1924 is a spurious year for Marx.

**Figure 5.** Synthetic Karl Marx with treatment in 1924



Because Marx’s true treatment effect began circa 1917, the pre-treatment period is now estimated with a worse fit, creating a larger pre-treatment RMSPE. And because all p-values are standardized by the RMPSE—in order to penalize or discount post-treatment effects according to the largeness of any spurious pre-treatment effects—this causes the p-values to become larger. We may therefore rule out Francis’s contention of a spurious treatment affecting both authors in

19. See Magness and Makovi (2023, 1522 n.25, Appendix A3), where we explain that normalizing to the interval  $[0,1]$ —by dividing all outcomes by the sample maximum outcome—facilitates numerical convergence and minimizes CPU and RAM requirements. Because every author’s outcome is divided by the same scalar, it is similar to converting feet to miles or inches to centimeters. *Numerically*, this normalization *does* make a difference because the computer has an easier time finding an optimum (convergence). But *theoretically*, it should make no difference.

1917.<sup>20</sup>

Furthermore, Francis's appeal to Mann's treatment effect as a refutation of Marx's betrays an implicit misunderstanding of p-values. For any p-value greater than zero, it must be true that some placebo (untreated) units will appear to be spuriously treated. Whenever a p-value of less than (say) 0.01 is accepted as evidence of a genuine treatment effect, the researcher is accepting that up to 1 out of 100 untreated units may appear to be treated. When Francis objects that Mann appears to be treated, he is implicitly requiring us to obtain a p-value of exactly zero. Only if the p-value is exactly zero will it be impossible to find any untreated units who appear to be treated in the vicinity of 1917. Now, if Francis wishes to establish zero as the critical p-value, that is his prerogative, but we suspect that that is not his intention, because it would invalidate all parametric statistics.<sup>21</sup> Instead, we suspect that Francis simply did not think through all the implications of his claim. The same could be said of his other assessments of our empirical design.<sup>22</sup>

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20. We call attention to the fact that Marx consistently experiences treatment in 1917, but not other spurious years. In Magness and Makovi (2023, 1531), we test an in-time placebo for "Karl Marx," using English-language Google Ngrams (2019 corpus), assigning treatment to the year 1889. P-values (standardized) are 0.280 (joint point), 0.463 (AEP), and 0.443 (Simes). In the draft version (Magness and Makovi 2020, 56), using 2012 Ngrams, the joint post p-value is 0.293. And in Magness and Makovi (2024-b, 16), we test "Karl Marx" using German-language newspapers, assigning treatment to the year 1891. The sample there is restricted to non-socialists only. P-values (standardized) are 0.371 (joint post), 0.351 (AEP), and 0.314 (Simes). For this present paper, we repeated the specification of the 1889 in-time placebo from Magness and Makovi (2023, 1531), but using the newer 2024 Google Ngram data. We also omit Bernstein and Bebel from the donor pool, following Magness and Makovi (2024b). P-values (standardized) are 0.283 (joint point), 0.561 (AEP), and 0.492 (Simes). Notwithstanding the significant results for Thomas Mann, nevertheless, in general, synthetic control is finding statistically significant results for Marx in the hypothesized year (1917) but not in spurious years.

21. Synthetic control, as a non-parametric statistical method, is able to obtain p-values equal to exactly zero. This is because the p-value is calculated as the number of placebos who experience normalized treatment effects at least as large as the treated unit does. But conventional parametric statistical estimators will estimate the p-value by integrating a probability distribution, forcing the p-value to always be more than zero.

22. In addition to the foregoing issues, Francis's assessment of our SCM test evinces a broader misunderstanding of our empirical design. In a footnote, Francis (2024, 97 n.5), Francis observes that we "limit the number of iterations to 20, whereas the default is 1,000" in order to reduce the computational intensity. We wish to clarify. As we note in Magness and Makovi (2023, Appendix A3f), our problem in this specific instance was not computational intensity, but rather, the fact that a minority of placebo trials (to compute p-values) appeared to be stuck in endless loops which did not terminate even after days of running. Ordinarily the Stata *synth\_runner* module identifies placebo units which fail to converge and silently eliminates them from the donor pool. But for some reason, this mechanism began to fail as our sample size grew. By setting the maximum iterations, we eliminated the endless loops. We found that setting maximum iterations to 10 generally produced a good fit (RMSPE), so out of caution, we set maximum iterations to 20. As the maximum iterations increases, the maximum estimation time grows more than proportionally, so it was important to restrict the maximum iterations to a reasonably small number. For example, with 15 maximum iterations, estimation of a single SCM regression with *synth\_runner* takes up to 1.5 hours, but with 20 iterations, it takes up to 4 hours. Since most of our SCM regressions are completed in less than one

## The complexities of simple visual analysis

After presenting and misinterpreting his own parallel SCM tests for Marx and Mann, Francis (2025, 91) shifts his efforts back to a “simpler analysis of the Google Ngram Viewer data.” This approach amounts to little more than eyeballing perceived trend lines from searches of specific authors on an ad hoc basis, and offers little room for formal testing.<sup>23</sup> To complicate matters further, Francis has poorly implemented his alternative approach. An illustrative example appears in one of his primary attempts at using simple Ngram Viewer results to challenge our econometric findings.

To this end, Francis (2025, 99) conducts an ad hoc Ngram Viewer search for Czech novelist Franz Kafka, a name he describes as “symptomatic of this arbitrariness” in our donor pool. In his telling, Kafka’s name “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.” Francis then purports to show as much with a screenshot from Ngram Viewer, where Kafka’s name rises to prominence from 1887–1893, declines, then rises again in the 1910s. Throughout, Francis contends that this pattern makes Kafka unsuitable as a donor for Marx, even though our SCM results did not select or use Kafka as a meaningful contributor to our synthetic counterfactual.

An even simpler problem confounds Francis’s attempt to conduct “simple analysis” of Kafka from Ngram Viewer. When downloading his search results,

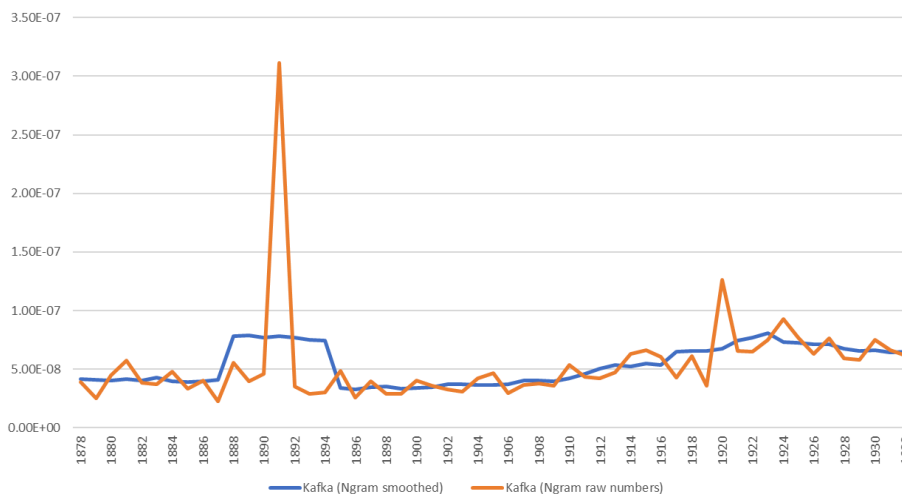
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hour even with 20 maximum iterations, we believe that generally, 20 maximum iterations is a non-binding constraint. Setting a maximum iteration count is only a binding constraint in those rare cases where a small number of placebo trials become stuck in endless loops. It appears that the default maximum iterations of 1,000 is just an arbitrary placeholder which should never be approached in the real world. Also, as we observed (Magness and Makovi 2023, Appendix A4), if the maximum iterations is too low, it will cause pre-treatment fit (RMSPE) to be worse, causing all standardized p-values to become larger. So we argued that even if a small minority of placebo units are being estimated with a poor fit by setting the maximum iterations too low—or because they should have been dropped entirely—nevertheless, this should not introduce very much bias. To give a sense of how setting maximum iterations affects the fit, here are some RSMPE values for different iteration constraints, estimated using our primary model (Karl Marx treated in 1917, 2019 English-language Google Ngrams): 8 iterations → RMSPE 0.00194; 10 → 0.00187; 12 → 0.00177; 15, 17, and 20 → 0.00178. After 12 iterations, increasing the number of iterations actually causes the fit to become slightly worse, perhaps suggesting that the numerical optimizer has hit a wide plateau with multiple possible optima, causing it to vacillate indecisively.

23. Curiously, Francis (2025, 99) accuses us of adopting an approach that, allegedly, attains statistical significance “through arbitrary choices by the researcher.” This is in apparent reference to our attempts to construct a large donor pool, although again he fails to elaborate upon or even offer any objective criteria for donor selection. We note that Francis’s preferred alternative of conducting ad hoc searches on Ngram Viewer would, at minimum, exhibit a greater susceptibility to biases introduced through arbitrary choices by the researcher than the construction of a large donor database using reasonable criteria.

Francis neglected to disable Ngram Viewer's automatic smoothing function. Ngram contains a single-year false positive for Kafka in 1891, which would be of little consequence in itself using the raw data. When enabled, Ngram Viewer's smoothing function extends this single-year hit into seven years of nonexistent citations. For this reason, our paper and subsequent studies have disabled the smoothing feature as a first step before scraping the associated data from the Ngram corpus. Figure 6 depicts the raw data with the false positive in 1891 against Francis's chart with unintentional smoothing. Yet again, Francis has handled his source data in a careless manner, leading him to untenable interpretations.

**Figure 6.** English Ngram Results for Kafka, smoothed vs. unsmoothed



*Source:* Google Ngram Viewer, 2024 corpus.

It is therefore curious to see Francis doubling down on the previous mistakes from another of his proposed alternative measures. In his last critique, Francis attempted to construct a competing measure of Marx's influence by tabulating yearly citations from the JSTOR database. In our previous response (Magness and Makovi 2024a), we documented several biases that affect the depicted patterns in this proposed alternative measure.

Francis presented his tabulations of Marx's name by using JSTOR's search result counts for five selected academic disciplines (economics, sociology, political science, history, and philosophy), which he then interprets as evidence of Marx being a widely-cited pre-1917 thinker.<sup>24</sup> Unfortunately, JSTOR cross-lists several

24. Francis's own charts actually contradict his interpretation in all disciplines except for economics, where Marx encountered a robust and deeply critical assessment from the mainstream academic literature prior

journals in secondary disciplines, such that a hit for Marx in a political science journal might also appear in tabulations for economics and sociology. The effect of cross-listing is especially pronounced around the turn of the 20th century, when academic journals were far less common than today. It introduces double-counting to Francis's tabulations such that the same hit for Marx's name in a journal's main discipline also counts toward one or more other disciplines in the same year. In our previous response, we noted that a single cross-listed journal was responsible for 16 percent of Francis's pre-1917 tabulations for Marx in economics, 31 percent in political science, and 36 percent in sociology. We also showed that cross-listed journals account for the entirety of Marx's hits in sociology before 1895, essentially creating an illusion that Marx was regarded as a prominent early figure in this field.<sup>25</sup>

In his latest rejoinder, Francis is dismissive of our discovery. His response contends that other authors in his table such as Adam Smith, John Stuart Mill, and Herbert Spencer are also affected by double-counting, causing cross-listed journals to also exaggerate their citation numbers when sorted by discipline. The commensurate reduction of cross-listed hits for other authors does not salvage Francis's argument though. Instead, it illustrates why discipline-specific tabulations from the JSTOR database are unreliable measures of citation levels and patterns for Marx, as well as any other author.

The reason stems from the changing landscape of scholarly publishing in that era. In the late 19th century, most academic works appeared as books. Most of the early academic journals trace their origins to the founding of professional associations for specific disciplines in the 1880s and 1890s. Publishing schedules varied widely in these early years of the academic journal industry, and some disciplines had no dedicated journals at all.

An illustrative example is sociology, which did not acquire a dedicated jour-

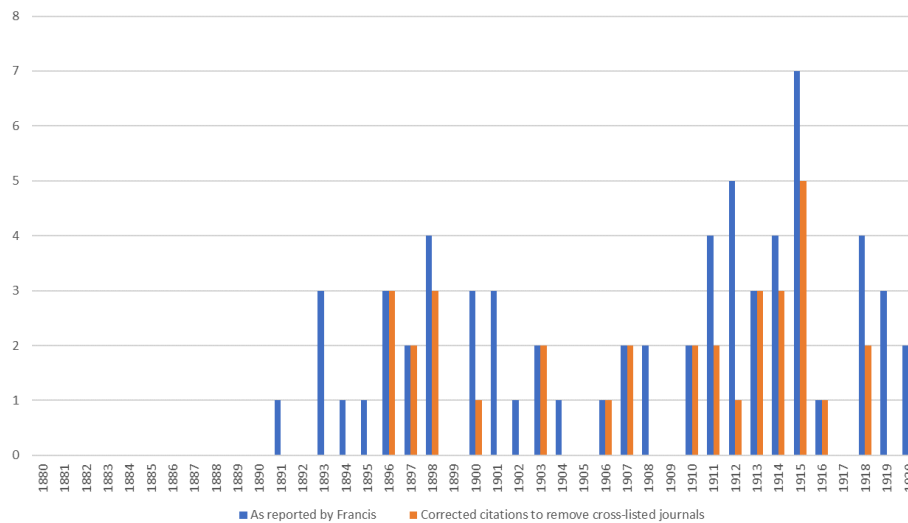
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to 1917. As we showed in Figure 5 of our previous response (Magness and Makovi 2024a, 406), Marx's JSTOR hits in philosophy, political science, history, and sociology all trend upward between 1917 and the late 20th century. Francis does not address this discrepancy between his data and his claims about Marx's JSTOR citation rates in these disciplines.

25. The illusory sociology citations for Marx may be seen in Figure 6 of Magness and Makovi (2024a). In constructing this figure, we acknowledge that we used an erroneous denominator to calculate the percentages attributable to double-counting. This situation came about after Francis previously provided us with a data file containing incorrect numbers that did not correspond to his charts and tables in Francis (2024). Upon discovery, we alerted the editors of EJW that Francis's data file did not match his charts. Francis then provided us with a corrected data file, containing a different and presumably updated denominator. We mistakenly copied this denominator into our own replication of his work, in place of the adjusted one. Unfortunately, we only noticed this error on our part after the publication of our first response and alerted the editors of EJW, whereupon we mutually decided to address it in this subsequent response. While the denominator error does alter the percentages on the vertical axis of our previous Figure 6, it does not obviate the substance of the problems arising from double-counting, particularly in 12 years where there were no actual sociology citations for Marx.

nal until the founding of the *American Journal of Sociology* in July 1895. Due to the infancy of journal publishing in this field, the JSTOR database contains no actual sociology hits prior to July 1895. A conundrum appears when Francis’s JSTOR tabulations show six pre-1895 “hits” in the numerator for Karl Marx’s name that he codes to sociology. All six come from a cross-listed political science journal that Francis also includes in his tabulations for that discipline, and for economics.

**Figure 7.** Comparison of Karl Marx citations in sociology journals, actual vs. Francis



Source: Tabulated from JSTOR and Francis (2024) replication file).

The distortive effect of cross-listing may be seen directly in Figure 1b of Francis’s (2024) first rejoinder to our paper. In this figure, Marx appears to achieve his all-time highest level of influence in sociology in 1893. Francis’s accompanying data file estimates Marx’s citation rate at 3.25 percent of all sociology articles published in 1893. For comparison, the second-highest hit rate is 2.99 percent over a century later in 1999. This pattern prompts Francis to conclude that Marx was widely discussed (albeit rejected) in sociology journals before 1917, after which he allegedly went into decline until the New Left “revival” of the 1960s and ’70s. In reality, Marx’s high hit rate in sociology journals before July 1895 is entirely an artifact of JSTOR’s cross-listing of journals from other disciplines. There were no sociology citations for Marx in 1893, because—unbeknownst to Francis at the time he compiled his JSTOR data—a sociology-specific journal did not exist. To further complicate matters, Marx’s JSTOR citations between 1895 and 1916 are also exaggerated by cross-listing at a time when only a single sociology journal published with any regularity. Figure 7 depicts Marx’s actual references in sociology



journals (orange) compared with Francis's tabulations (blue) for the same years.

In his first comment, Francis (2024) acknowledged the possibility of cross-listing, then took no further steps to mitigate this problem. In his current rejoinder, Francis (2025, 104) now claims to have “discovered that...there are simply no sociology journals in the JSTOR category before 1895” after cross-listed duplicates are removed—a point that we, in fact, brought to his attention. He further claims that, by adjusting for cross-listed duplicates, our correction to his series “in fact reinforces the impression that Marx was well-known to sociologists before the Russian Revolution.”

This is a peculiar claim, as it entails removing the first four years from Francis's previous window of analysis, including his depicted all-time high for Marx's alleged influence in sociology (1893). It also ignores the large number of years—12 in total out of the 26-year window before 1917—in which Marx had zero actual citations in sociology journals, despite Francis's chart implying otherwise. When we evaluate Francis's current argument against his original tabulations, it is difficult to avoid the conclusion that cross-listing creates a major distortion in his analysis.

Using his original period from 1891 to 1916, Francis recorded hits for Marx in eight years that actually had zero citations in sociology journals. In another six years, he presented exaggerated counts for Marx due to cross-listing from other disciplines, including severe overcounts in 1900 (three times the actual number of citations), 1911 (two times), and 1912 (five times). Double-counting accordingly affects 54 percent of the years in Francis's original tabulation. The corrected data are more consistent with sporadic and occasional notice of Marx rather than with a well-known figure receiving sustained engagement from sociologists.

Apart from the limitations caused by the paucity of journals in general in the early 20th century, Francis does not meaningfully address any of the other problems we pointed out with his JSTOR tabulations. He reiterates that he “disaggregated the data by disciplinary category and also isolated the ‘big’ journals” in his tabulations (2025, 105), but makes no effort to account for the fact that many of these same “big” journals did not begin publication until many years or even decades after the 1917 treatment event. It is obviously inappropriate to construct a weighted measure with a severe post-treatment bias in an attempt to measure an event that predates the existence of most of its composite journals.

Neither does Francis account for these same “big” journals being founded at different rates and time intervals across academic disciplines. Nor does he address the fact that his designation of them as “big” or “top” journals comes from their impact rankings in the present day, which did not necessarily apply in previous decades. Francis also makes no attempt to justify why he confines his analysis to just five disciplines (economics, history, political science, sociology, and phil-

osophy) while ignoring the growth in Marx’s academic influence in other fields such as literary criticism, education, law, and cultural studies—a point we specifically emphasized in our original discussion (Magness and Makovi 2023, 1508, 1514).

Francis does briefly mention our own alternative tabulations from JSTOR, which estimated Marx’s citation rate by year as a percentage of all published articles from 1880 to 1980. His response though is to dismiss it with little more than a handwave of speculative tedium. He notes the changing composition of JSTOR over time as social science journals increased their share of the database relative to physical sciences such as botany. Yet in doing so he misses the forest for a plant leaf, as the expansion of disciplines with heavier Marxist influence (and the introduction of entirely new disciplines, such as the critical theory-infused “studies” departments that emerged between the 1970s and present day) is itself an indicator of Marx’s growing intellectual influence between 1917 and the present.<sup>26</sup>

Instead, Francis leaves us with an empirical design of his own personal discretion. But these choices are not random. In each and every case, Francis attempts to confine the empirical discussion to measurements that display a superficial visual alignment with the story he wants to tell. Any attempt to place quantitative assessments of Marx on a sounder scientific footing is dismissed out of hand—by using ad hoc objections to exclude particular disliked donors; by imposing artificial constraints on the academic disciplines and types of journals we are allowed to consider; and by unsupported declamatory designations that Ngram is an inappropriate measure for turn of the century academic influence, even though books were the dominant academic medium of the day and journals were still in their infancy.

## **Kořakowski, Berlin, or Schumpeter?**

In the conclusion to his latest comment, Francis returns to the qualitative historiographical discussion around Marx’s intellectual dissemination. While he describes his interpretation as the “conventional” account, compared to our “revisionist” and “novel” argument, he offers little evidence to substantiate this

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26. We reiterate that our studies have sought to measure Marx’s influence across all disciplines and all published works, as is necessarily the case with the Ngram database. A general search of the JSTOR database is therefore the most appropriate counterpart for academic journals, and indeed the only reliable one due to aforementioned problems from cross-listings and the paucity of discipline-specific journals in the time period of analysis. In objecting to this approach, Francis is accordingly critiquing us for an argument that we did not make, and doing so with data that is ill-suited for testing his alternative approach of parsing out Marx’s citations by academic discipline.

position. As noted, Francis rests this claim entirely upon his own readings of just two authors, Leszek Kolakowski and Isaiah Berlin. He makes no effort to justify why these two authors alone constitute an authoritative final word on Marx's dissemination. We nonetheless consider them in turn.

The case for Kolakowski appears to be entirely subjective. Francis (2025, 105) writes that Kolakowski "can be held up as an example of how intellectual history should, in my inexpert opinion, be done" and appeals to this author's personal biography as an exile from communist Poland to justify his authority as an interpreter of Marx. Although Francis offers little elaboration, he appears to endorse Kolakowski's attempts to differentiate Marx from later figures who identified as Marxists, namely the Bolsheviks. Kolakowski's depiction of Lenin and his successors as a deviation from the "orthodox" Marxist theorizing of the turn of the century hearkens back to a debate that has been playing out on the far-left periphery of philosophical circles since that moment (Lenin 1918; Kautsky 1918). It is true that Kolakowski opined on this debate with a harsh assessment of the Bolshevik interpretation of Marx, but that question is only tangential to our research. Francis is dissatisfied with our previous response in which we disavowed any interest in adjudicating the endless internecine feuds among Marx's successors. And yet he makes no case as to why such an adjudication is necessary to evaluate our empirical argument, let alone why Kolakowski's interpretation of these events is the correct one.

Compared to the unelaborated endorsement of Kolakowski, Berlin occupies a more detailed place in Francis's argument. He asserts that "Berlin's account of Marx's rise to fame is clearly at odds with Magness and Makovi's narrative" and accuses us of taking Berlin's words "out of context" when we note that Berlin acknowledged Marx's relative obscurity in 19th-century London (Francis 2025, 102). To justify this position he quotes Berlin as stating that "In 1860 Marx's fame and influence were confined to a narrow circle" but "[f]ifteen years later," or 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" (Berlin 1939, 203, our emphasis).

We call attention to the italicized passage, which Francis appears not to have noticed. While Berlin does allege a growth in Marx's fame and notoriety on the European continent, he specifically excludes England from this observation. Berlin is quite clear about Marx's unremarkable stature in his adopted country of residence. Elsewhere in the same text Berlin (1939, 2) writes that "the greater part of [Marx's] working life was spent in comparative obscurity in London," that Marx "was little known to the general public" of England, and that Marx's "public appearances were neither frequent nor notably successful." Marx, he continues "remained all his life an oddly isolated figure among the revolutionaries of his time"

(*ibid.*, 4). Marx’s “death passed largely unnoticed among the general public” of England, Berlin reiterates, excepting a brief and inaccurate obituary notice in the *Times* of London. Berlin does observe in this work that Marx’s “fame increased steadily after his death” but does not elaborate on the timing or locale of this increase (*ibid.*, 263).

It is unclear how Berlin’s assessment is “clearly at odds” with our primary finding, which examines Marx’s citation patterns in the English-language Ngram corpus. Even though Berlin considered Marx to be comparatively unknown in England, Francis (2025, 101) attempts to use the English-language Ngram corpus to validate an alleged “exponential growth in Marx’s fame” on the European continent. Looking past this apparent confusion of languages and locales, Francis attempts to measure Marx’s continental European growth with a structural break test that identifies three breaks in Marx’s English citation patterns, in 1886, 1918, and 1946. These do not align with the most obvious candidate for Marx’s European notoriety as intimated by Berlin (1939, 240–242), his attempts to capitalize on the Paris Commune of 1871. Indeed, they broadly confirm our interpretation of Marx’s tepid citation growth in the late 19th century and our identification of a treatment in 1917.<sup>27</sup>

Schumpeter offered greater detail than Berlin on these pre-Soviet events. Writing in 1912, he observed that “Only in Germany was the success of Marx great and lasting” (Schumpeter 1954/1912, 122). The picture elsewhere was rather dismal. “In England,” Schumpeter continued, Marx “found only a few followers who soon dispersed.” Marx’s “proper scientific performance had little effect” in France and Italy, except to popularize “some of his catchwords” and attain modest discussion in sociological rather than economic contexts.

We do consider Marx’s spread on the European continent to be an important topic, although surely a more appropriate measure would use Ngram corpora from continental languages instead of English. Although Berlin does not elaborate on this point, it is conceivable that Marx spread widely in Europe while remaining obscure in England (as well as other English-speaking countries such as America—an observation we have directly documented in previous sections). It is also possible that Berlin erred about the extent of Marx’s dissemination in Europe, and

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27. Francis (2025, 102 Figure 5) presents the results of a structural break test, using English language Ngram to allegedly validate Berlin’s observation. Francis attributes an “exponential growth” to Marx’s citations between 1860 and 1886, the first break. It is a mistake to interpret this growth as evidence of fame though, as it merely reflects Marx picking up a small number of citations in his lifetime from a starting point of practically zero. After the first break in 1886, Marx entered a period of relatively flat citations that persisted until 1918. This is exactly the opposite of what Francis has been telling us happened in the 1890s and 1900s. The second structural break in 1918 is obviously connected to the Bolshevik Revolution, as our main finding suggests.

that Marx remained a relatively obscure figure there as well, with the exception of the far-left periphery of socialist activism. As noted in a previous section, scholars including Hobsbawm, Mises, and Steinberg have suggested that Marx only attained minor circulation in German-speaking regions prior to the Russian Revolution, the one locale in continental Europe that Schumpeter identified as Marx's greatest pre-Soviet reach.

In a separate article, we empirically tested the observations of Hobsbawm et al. using the German Ngram corpus and a parallel German newspaper database. We found only modest evidence of a pre-1917 treatment of Marx's German-language citations, particularly compared to the clear and pronounced treatment Marx received in 1917 (Magness and Makovi 2024b). The main evidence of an earlier treatment for Marx in Germany was also indistinguishable from the bump that competing socialists received in the early 1890s following the repeal of an anti-socialist censorship law. Combined with the aforementioned qualitative histories, these data suggest that casual comments about Marx's earlier rise in Germany are overstated. Observations of Marx's broader pre-Soviet spread in Europe were likely more a matter of comparative rhetorical flourish than descriptive analysis.

We also call attention to the fact that Berlin (1939) provides relatively little elaboration about the precise timing of Marx's posthumous rise, leaving Francis to simply speculate that it aligns with his own narrative. Berlin's other writings on this subject provide clear reasons to doubt Francis's interpretation. In an essay originally composed in 1956, Berlin (2000, 158) observed that "Marxist writings are not among the clearest or most readable in the literature of socialism," and recounted that even John Maynard Keynes "found himself physically unable to plod through *Das Kapital*." For Berlin, the adventitious success of the Bolsheviks in 1917 played a determinative role in elevating Marxist theory: "[I]f Lenin had not radically altered our world, I doubt whether [Marx's] his works would be as minutely studied as they necessarily are."

Schumpeter would have likely concurred. In a little-noticed passage, Schumpeter (1947, 359) characterized the 1917 Russian Revolution as a chance event: "It must be remembered that the bolshevik conquest of the rule over the most backward of all the great nations was nothing but a fluke." This designation challenges the Bolshevik self-depiction of 1917 as a fulfillment of the revolutionary inevitability of a proletarian state. Yet Marx the prophet gave way to Lenin the political actor, and in Schumpeter's assessment, Lenin's successes were entirely adventitious. As he elaborated in a footnote:

For this fluke, bolshevism was possibly indebted to the German general staff, by whose orders Lenin was transported to Russia. If this should be thought an exaggeration of his personal share in the events of 1917, there were enough

other chance factors in the situation to teach us the freakishness of this piece of history. (Schumpeter 1947, 359 n.11)

With these parallel observations, the qualitative historical interpretations of Berlin and Schumpeter arrive at a position not far removed from the questions we have examined empirically. Marx's citation patterns, as shown in our SCM testing, exhibit a pronounced treatment in 1917 that has remained statistically significant through exhaustive scrutiny and robustness testing. Far from being a "novel" or "revisionist" take, the adventitious character of that treatment and its relationship to Marx's comparatively limited rate of dissemination before 1917 were directly anticipated by several of the leading scholars of the last century.

Despite being presented twice now with extensive evidence that mainstream historiography aligns with our empirical findings, our interlocutor has avoided any meaningful engagement with this literature. Faced instead with a barrage of increasingly eclectic pedantries from Francis, we must conclude that his challenges to our thesis have run their course without anything of substance to show for his efforts.

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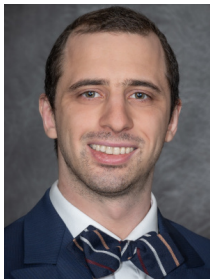


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