Pecuniary Motivations to Create: Some Evidence from a Demand Shock to Famous Authors[1]

Abstract

This paper studies how creativity responds to monetary incentives exploiting an exogenous positive demand shock that affected a quasi-random group of famous authors whose books were included in the New York Times list of bestseller books. Using data on authors' productivity from Goodreads and Wikipedia the results do not suggest that on average monetary rewards increase famous authors' creativity. The results in this paper are important because the intellectual copyright system assumes that monetary rewards induce creativity.

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1 Introduction

Creativity is the main driver of economic growth. This paper focuses on the book industry to study whether or not creativity responds to monetary incentives. Examining this question is important for evaluating the need of granting intellectual property rights to creators as a mechanism to incentivize their creativity. From an economic theory perspective, a system granting intellectual property rights is inefficient if the supply of creativity is not responsive to monetary rewards because it would not induce additional long run creations to offset the short run inefficiencies of allowing creators to charge prices above the cost of reproduction.

Although it is difficult to imagine a more fundamental question in the academic and policy debate surrounding intellectual property rights, there is limited empirical research examining how monetary incentives affect creativity. Moreover, the available empirical evidence to a large extent concludes that intellectual property does not increase creativity, contradicting the theoretical foundation for the existence of intellectual property rights.

A stream of literature studies how intellectual property affects creativity by exploiting historical changes in the intellectual property right system. Focusing on the patent system, Lerner (2009) studies changes in patent policy across a sample of 60 countries finding that more patent protection decreases the number of applications. Boldrin and Levine (2004, 2008) present evidence from economic history showing that the introduction of copyrights and patents led to less innovation, such as the case of James Watt whose patent allegedly hampered the development of the steam engine consequently delaying the industrial revolution, although this claim has been challenged by Selgin and Turner (2006, 2009, 2011). Scherer (2004, 2008) also presents evidence from economic history studying the introduction of musical copyright in Europe in the eighteenth and nineteenth century finding mixed evidence regarding whether the introduction of copyright laws increased musical composition. Giorcelli and Moser (2019) exploit exogenous variation in the adoption of copyright laws across eight states in Italy during the years 1770 and 1900. Using data on 2,598 operas from 705 composers that premiered across these eight Italian states in over a century, they find that the introduction of copyright increased the number and quality of new operas.

Instead of analyzing the effect of a change in the intellectual property rights system, Liebowitz and Zentner (2019) directly measure the effect of monetary reward on future creativity. This is based on viewing the question of how creativity responds to monetary incentives as a special case of the questions studied in the literature on the estimation of the labor supply (e.g., Goolsbee, 1999). Liebowitz and Zentner (2019) use data on fiction and non-fiction book authors, finding that non-superstar authors publish more new books when they earned higher payments for previously written books and that the supply of creativity bends backward for superstar authors. However, while the labor literature has extensively used exogenous tax rate changes to estimate the elasticity of the supply of labor (e.g., Goolsbee 1999), Liebowitz and Zentner (2019) do not use such an exogenous shock.[2]

This paper follows the idea of directly examining the effect of monetary reward on future creativity used in Liebowitz and Zentner (2019). But to assess whether or not creators respond to future monetary incentives, it uses an exogenous positive demand shock that famous authors experience when their books are included in the New York Times list of bestseller books.

2 Identification Strategy

Sorensen (2006) uses data on fiction books that were released in 2001 and 2002 and shows that books included in the New York Times list of bestseller books experience an increase in sales. To measure the causal effect of inclusion of a book in this list on sales, he exploits mistakes made when constructing the New York Times bestselling list:

"Although the list is by and large quite accurate when compared with the 'true' sales numbers available from BookScan, it is not uncommon for a bestselling book to be missed -- i.e., a book may not appear on the list even though its sales exceeded the sales of listed books. In principle, these mistakes provide a means of identifying the effect of appearing on the list, by serving as an appropriate control group."

Specifically, Sorensen (2006) compares two sets of books:

"Those that were listed at rank 13, 14, or 15 when they first appeared on the New York Times list (n= 44), and those that should have appeared at 13, 14, or 15 when they were mistakenly omitted (n=75)."

Using this methodology, he finds that:

"For books that were published for the first time on the New York Times list, sales declined by an average of 7.6 per cent relative to the previous week (recall that the dominant pattern in sales over time is a steady decay: even for books appearing on the bestseller list, it is rare for sales to increase from one week to the next). For mistakenly omitted books, sales declined by an average of 22.7 per cent. Taken at face value, the difference implies that in the first week, being listed leads to 19 per cent more sales than would have otherwise occurred."

Sorensen (2006) also argues that the effects can be larger than estimated since: a) appearing on the bestseller list increases the popularity of future books by the same author, and b) his

[2] This paper is also related to a broader literature on how monetary incentives affect innovation (e.g., Acemoglu and Linn (2004), Filkenstein (2004)).

analysis focuses on hardcover books, but appearing on the bestseller list also affects paperback book sales.

2.1 Mechanism: How does the Studied Demand Shock Affect Creativity?

The inclusion of a book in the New York Times list causes a shock to demand and increases current revenues for the authors who wrote them. Whether the shock affects authors' expected revenues from future books depends on how authors form expectation. If authors use the success of their last book when forming expectation, then the shock to demand will affect authors' expectation about financial rewards from future books.

This paper assumes that authors use current revenues when forming expectations about financial rewards from future books. Laying out a model formalizing how authors form expectations appears unnecessary since the assumption that they use current revenues when forming expectations is simple and intuitively tenable. It would also not possible to empirically test any such formalizations.

Although what really matters is whether authors *expected* revenues from future books are affected by the revenue shock, the shock is likely to affect *observed* revenues from future books for authors via learning and product discovery (e.g., Sorensen and Hendricks 2009, Moretti 2011, and Sorensen 2017). These mechanisms suggest that the shock to demand will have a persistent impact on revenues. However, authors will revise their expectations if their future revenue expectations are not validated. For this reason, the effect of the revenue shock on creativity might be short lived and, when measuring this effect, we will have more confidence on the results in the short than in the long run.

2.2 The Size of the Demand Shock

The labor literature has extensively used tax rate changes to estimate the elasticity of the supply of labor (e.g., Goolsbee, 1999). How does the size of the demand shock used in this paper compare to the size of the demand shocks from tax changes used in the labor literature? This is a difficult question to answer because tax changes affect the amount of work via a more certain effect on future revenue streams than a shock to demand that affects the amount of work via affecting expectations about future compensation.

In terms of size of the shock, the tax cut included in the Tax Reform Act of 1986 (TRA86) decreased the marginal tax rate for individuals from 50% to 38.5% for the top income bracket, in addition to making other tax changes that create complications when using tax changes to measure the elasticity of the labor supply. The tax increase in the Omnibus Budget Reconciliation Act of 1993 (OBRA93) increased marginal tax rates from 31% to 39.6% for income greater than $250,000 and from 31% to 36% for income between $140,000 and $250,000 (Goolsbee, 2000).

Although it is difficult to compare the shock used in this paper to the changes in tax rates used in the labor literature, the shock to revenues used in this paper led to a 19% difference in sales between the treatment and control groups, although these differences are during the week immediately following the shock. The assumption in this paper is that this increase in current revenues affected authors' revenue expectations from future books.

3 Empirical Model

To estimate the effect of monetary reward on creativity, this paper uses the following difference in differences model which includes author-level fixed effects:

$$Productivity_{it} = \beta_0 + \beta_1\gamma_t + \beta_2\delta_i + \beta_3 Post_t \times Mistake_i + \varepsilon_{it} \qquad (1)$$

where i represents an author, t represents a year, $Productivity_{it}$ represents either the number of books or the number of pages, δ_i represents an author specific fixed effect, γ_t represents a year fixed effect, $Post_t$ represents a dummy variable that takes the value 0 for years predating and including 2002 and the value 1 for years after 2002, and $Mistake_i$ represents a dummy variable that takes the value 1 if author's i book was mistakenly excluded from the list of bestselling books and the value 0 if author's i book was included in the list of bestselling books.

The coefficient β_3 measures how the shock to revenues affected the number of books, other works, or number of pages written. The coefficient β_3 is expected to be negative if monetary reward increases future creativity since authors of books mistakenly omitted from the list of bestselling books did not experience a positive shock to their future revenues and are thus expected to write fewer books or fewer pages than authors of books who experienced a positive shock to their future revenues.

4 Data

This paper uses data from three sources. The original list of books in the treatment and control groups come from Sorensen (2007) and the information for each of the authors on this list was collected from Goodreads (a company that belongs to Amazon) and Wikipedia.

Sorensen (2006) uses weekly data and some authors are in the treatment group in some weeks and in the control group in other weeks. These authors are excluded from the analysis since the interest in this paper is on authors' productivity levels before and after the shock. Moreover, some of the books in the list have more than one author and for these books we follow the publication history for all coauthors. Accounting for these issues there are 96 authors in the sample, 32 in the treatment group (authors who experienced a revenue shock because their books was included in the list of bestselling books) and 64 in the control group (mistakenly excluded from the list of bestselling books).[3]

However, some authors who were mistakenly excluded from the NY Times list of bestseller books during the period of time examined in Sorensen (2007) were included in this list before or after the time period he examined. We investigate below how excluding these authors from the analysis affects the results. Finally, the sample of authors used in the empirical analysis will be different when using data on productivity from Goodreads and Wikipedia because Wikipedia contains productivity information for most but not all authors.

4.1 Measuring Productivity using Data from Goodreads

We scraped data from Goodreads including information from all the books written by the 96 authors in the sample. For each book, these data include the number of pages. We used text analysis to filter duplicates, missing data, compilations of previous works, omnibus publications, previews, etc.

4.2 Measuring Productivity using Data from Wikipedia

Wikipedia contains information on authors' lifetime list of works for most authors in our sample (92 authors out of 96).[4] This is unsurprising since the authors on the list are "famous." For each of the authors, we collected the history of works (Wikipedia presents unstructured data and several research assistants helped retrieving these data).[5]

There are some caveats when measuring an author's productivity from Wikipedia. Take the example of Mario Puzo. Wikipedia lists 40 pieces of work under his name within five categories: "Novels," "Non-Fiction," "Short Stories," "Screenplays and Film Adaptations," and "Video Game Adaptations." Some of these works are not "books" and it is unclear whether all of these works should be counted when measuring productivity. When counting books using the data from Wikipedia, we sought of counting works that we thought would have an ISBN. In addition, some of these works are shorter than others and there might be quality differences across an author's different pieces of work, although these factors might be captured by the author-level fixed effects in the empirical model as long as they are time invariant. Other caveats include: some books are listed as published in more than one year (in which case we sought to record the date of first publication), some authors published books using more than one pen name (Wikipedia typically includes all pen names), some books were published after the author's death, some works are compilations of previous work, some works are anthologies or part of omnibus publications, and some works have more than one author. In our empirical analysis,

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[3] Note that the variable *Mistake* in Model (1) takes the value 1 when an author is in the control group and not in the treatment group.
[4] Adele Lang, Carol Gino, Lalita Tademy, and Deborah Smith are the exceptions.
[5] These data were retrieved during the months of September, October, and November of 2019.

we will examine whether the results change when measuring productivity including and excluding works that are not “books.”

Measuring productivity using information from Wikipedia has advantages and disadvantages compared to measuring productivity using information from Goodreads. The data from Wikipedia does not contain information on the number of pages written, but allow measuring the number of books and the total number of “works” which includes books and other types of productivity as explained before. An advantage of using Wikipedia is that it typically includes information written by an author under more than one pen name whereas Goodreads does not accommodate multiple pen names. A disadvantage of using Wikipedia is that it does not contain information from 4 of the authors in our sample. Comparing older and more recent data from Wikipedia and Goodreads it is noticeable that Wikipedia records more older productivity information than Goodreads (e.g., the first recorded productivity from the authors in our sample is from year 1923 on Wikipedia and from year 1951 on Goodreads).

4.3 Summary Statistics

Table 1 presents summary statistics. Using data from Goodreads, Table 1 presents the yearly number of books and pages written before and including year 2002 (Pre) and after year 2002 (Post). Using data from Wikipedia, Table 1 presents yearly number of books and total number of works before and after the shock.

Table 1 shows that yearly productivity is substantially higher when measuring it using data from Goodreads than when using data from Wikipedia. The number of books for the authors in the samples from Goodreads and Wikipedia are different due to several reasons such as omissions in both Wikipedia and Goodreads, double counting, errors when categorizing works as books using the data from Wikipedia, and the inclusion of different authors in the samples as explained before. The data from Goodreads suggest that authors wrote more books and more pages per year after than before the shock but the data from Wikipedia suggest that authors wrote slightly fewer books and produced slightly fewer works per year after than before the shock. The empirical analysis below will show how the statistics in Table 1 evolved for the treatment and control groups.

Table 1
Summary Statistics

	Number of Books		Number of Pages		Total Number of Works	
	Pre	Post	Pre	Post	Pre	Post
Goodreads	1.40	1.75	423	587	na	na
	(1.99)	(2.26)	(623)	(840)	na	na
Wikipedia	1.16	1.10	na	na	1.21	1.15
	(1.66)	(1.89)	na	na	(1.68)	(1.90)

Standard deviations in parentheses

5 Results using Data from Goodreads

Table 2 presents estimates of Model (1) using the number of books and number of pages as dependent variables. The standard errors are clustered at the author level to avoid incorrect inference. The DiD coefficient estimates are not statistically significant. The sizes of the estimated coefficients are small when compared to the number of books and pages per year in Table 1. The positive signs of the coefficient estimates in Columns (1) and (2) suggest that the supply of creativity has negative slope.

Table 2
Estimates of Model (1) including fixed effects by author and year

	(1)	(2)
	Total number of books	Number of pages
Interaction - Post x Mistake	0.0744	60.86
	(0.3010)	(96.32)
N	3,153	3,073
Clustered standard errors by author in parentheses		
* p<0.05		

To check for parallel trends, we estimate the following model:

$$Productivity_{it} = \alpha_0 + \alpha_1\gamma_t + \alpha_2\delta_i + \alpha_3\gamma_t \times Mistake_i + \nu_{it} \qquad (2)$$

Figures 1 and 2 plot estimates of α_3 in model (2) over time (including 95% confidence intervals) for the number of books and number of pages respectively. Figure 1 does not appear to show much of an effect (except perhaps in years 2003 and 2005) and Figure 2 appears to show weak evidence of an increase in productivity for the control group during the years immediately after the shock, which would suggest that the supply of creativity has negative slope. We explained before that we have more confidence on the results in the short run than in the long run, since the shock might be short lived if authors revise their expectations when their revenue expectations formed using the demand shock are not validated. Since the shock happened in years 2001 and 2002, year 2002 is a year after the shock for some authors and year 2003 is a year after the shock for other authors.

Figure 1
Number of Books - Test of Parallel Trends – Baseline Year 2000

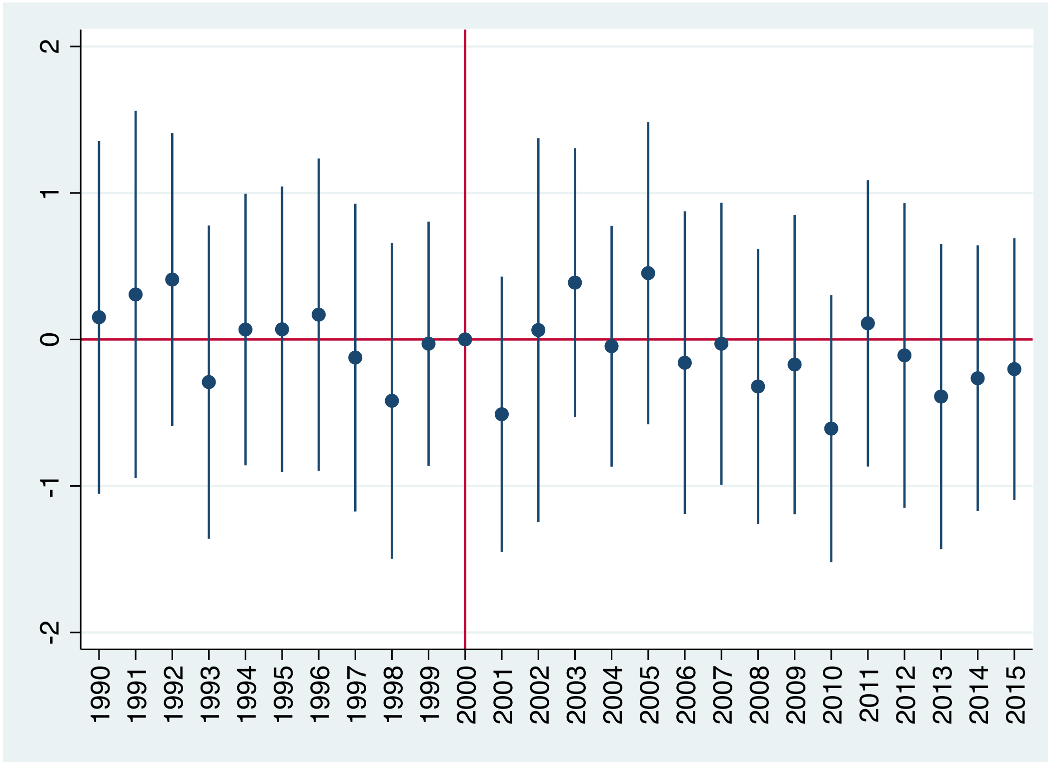
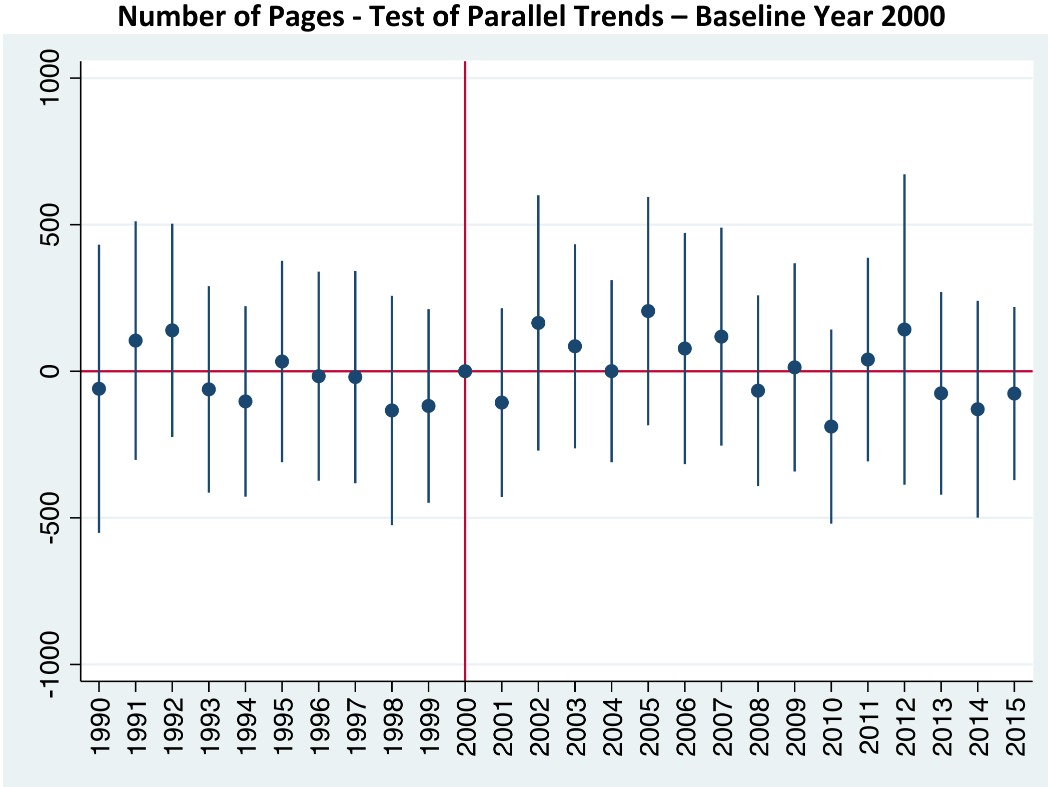


Figure 2
Number of Pages - Test of Parallel Trends – Baseline Year 2000



As explained above, some authors who did not experience a demand shock because their books were mistakenly excluded from NY Times list of bestseller books during 2001 and 2002 (the period of time examined in Sorensen 2007) did experience a demand shock in other time periods (before 2001 or after 2002), when their books were included in the NY Times list of bestseller books. It is not straightforward whether these authors should be included or excluded from the analysis because their earnings were smaller than they would have been had their books not been mistakenly excluded from the bestseller list during the period examined in Sorensen (2007). To investigate how including or excluding authors whose books were included in the list of bestseller books during other time periods affect the results, however, Table 3 replicates Table 2 excluding these authors from the analysis. The number of authors is smaller than in Table 2: there are 32 authors in the treatment group (authors who experienced a revenue shock because their books was included in the list of bestselling books during the period of time examine in Sorensen (2007)) and 11 authors in the control group (authors mistakenly excluded from the list of bestselling books during the time period examined in Sorensen (2007) and who never during their lifetimes wrote books that were included in the NY Times list of bestseller books).

Similar to Table 2, the coefficient estimates in Table 3 are small in size and are not statistically significant. Contrary to the result in Column 1 of Table 2, the sign of the coefficient estimate in Columns (1) of Table 3 suggests that the supply of creativity has positive slope. The sign of the coefficient estimate in Column (1) of Table 3 suggests that the supply of creativity has negative slope, similar to the sign of the coefficient estimate in Column (2) of Table 2.

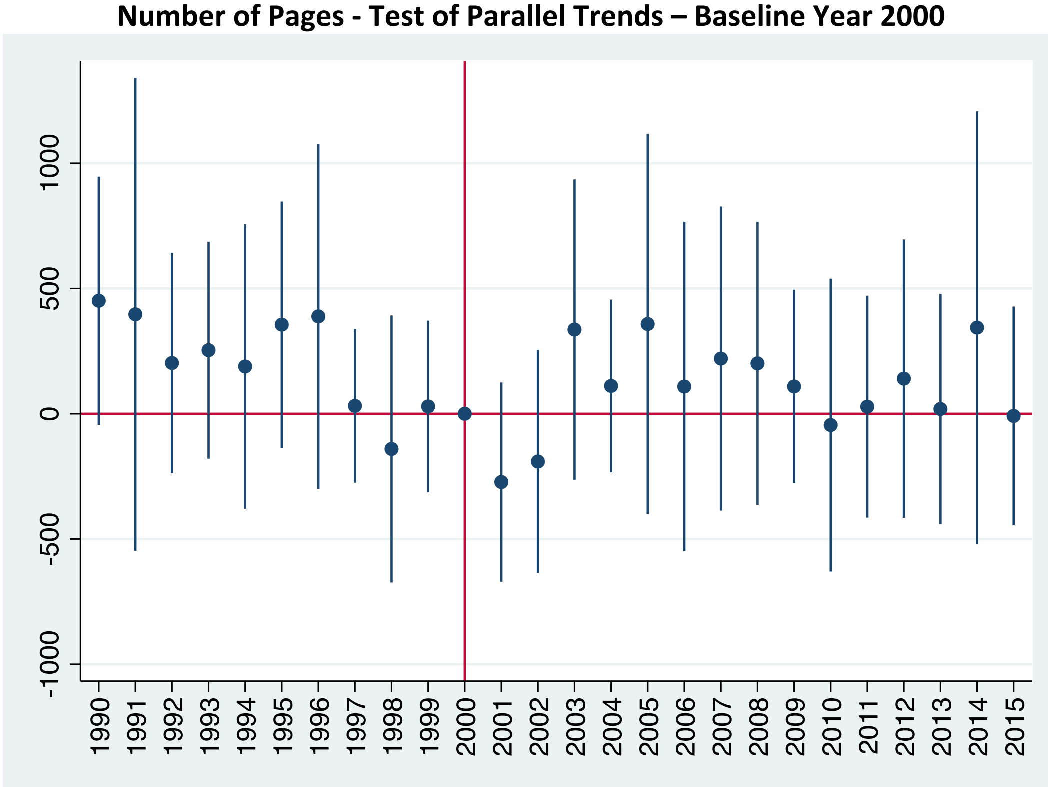
Table 3
Estimates of Model (1) including fixed effects by author and year

	(1)	(2)
	Total number of books	Number of pages
Interaction - Post x Mistake	-0.122	28.20
	(0.586)	(209.4)
N	1,331	1,312
Clustered standard errors by author in parentheses		
* p<0.05		

Figures 3 and 4 replicate Figures 1 and 2 using the authors used in Table 3. Figures 3 and 4 do not appear to show a clear slope of the supply of creativity, although the results would appear to show a negative slope of the supply of creativity if the analysis was restricted to years 1997 through 2009.

Figure 3
Number of Books - Test of Parallel Trends – Baseline Year 2000

Figure 4
Number of Pages - Test of Parallel Trends – Baseline Year 2000



6 Results using Data from Wikipedia

Table 4 presents estimates of Model (1) for the number of books and the total number of works (including works that are not books). The standard errors are clustered at the author level. The DiD coefficient estimates are small in size and are not statistically significant. The positive signs of the coefficient estimates suggest that the supply of creativity has negative slope.

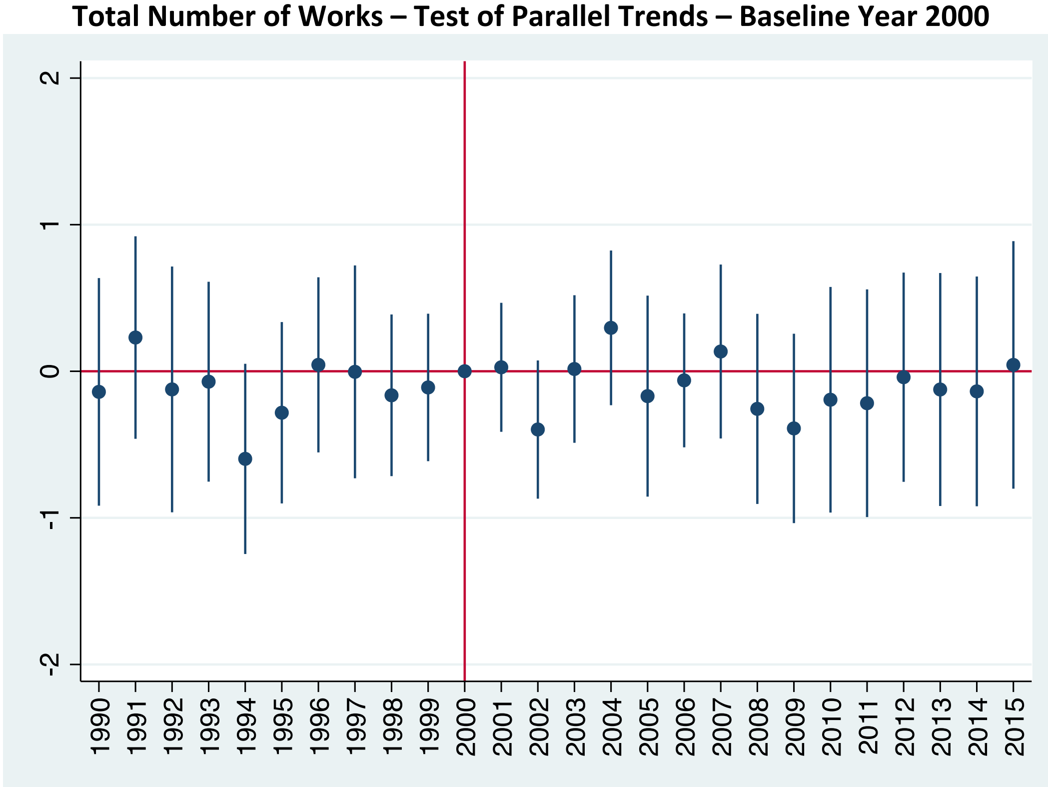
Table 4
Estimates of Model (1) including fixed effects by author and year

	Number of books	Total number of works
Interaction – Post x Mistake	0.1005	0.1077
	(0.2769)	(0.2950)
N	3,533	3,602
Clustered standard errors by author in parentheses		
* p<0.05		

Figures 5 and 6 show estimates of Model (2) to check if the parallel trends assumption holds when using data on books and works from Wikipedia, respectively. The productivity levels after the shock in Figure 5 appear to show weak evidence of an increase in productivity for the control group after the shock which would suggest that the supply of creativity has negative slope. This is particularly salient when comparing the years immediately before and after the shock. Figure 6 shows that the productivity levels are more erratic when using the total number of works.

Figure 5
Number of Books – Test of Parallel Trends – Baseline Year 2000

Figure 6
Total Number of Works – Test of Parallel Trends – Baseline Year 2000



As in Table 3 and Figures 3 and 4, Table 5 and Figures 7 and 8 exclude from the analysis the authors who did not experience a demand shock during the time period examined in Sorensen (2007) but experienced a demand shock in other time periods when their books were included in the NY Times list of bestseller books. The coefficient estimates in Table 5 are small in size, are not statistically significant, and suggest that the supply of creativity has positive slope. Figures 7 and 8 that the productivity levels are erratic when limiting the analysis to the smaller sample of authors.

Table 5
Estimates of Model (1) including fixed effects by author and year

	Number of books	Total number of works
Interaction – Post x Mistake	-0.127	-0.221
	(0.165)	(0.266)
N	1,432	1,444
Clustered standard errors by author in parentheses		
* p<0.05		

Figure 7
Number of Books – Test of Parallel Trends – Baseline Year 2000

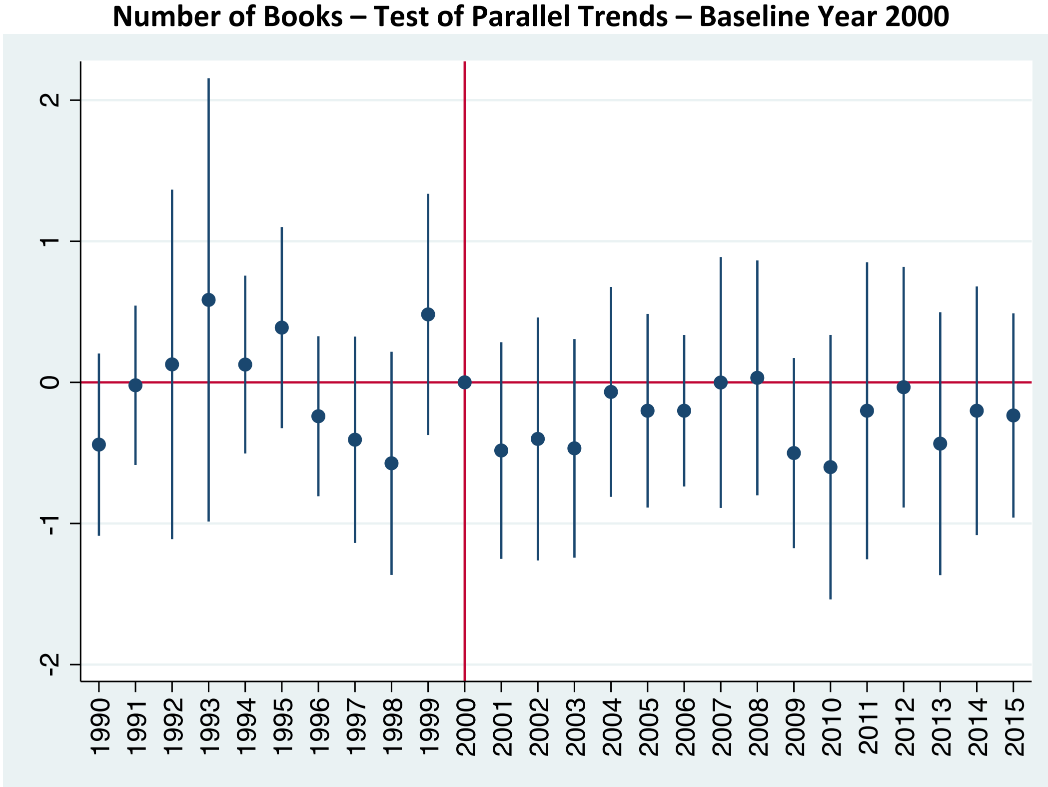
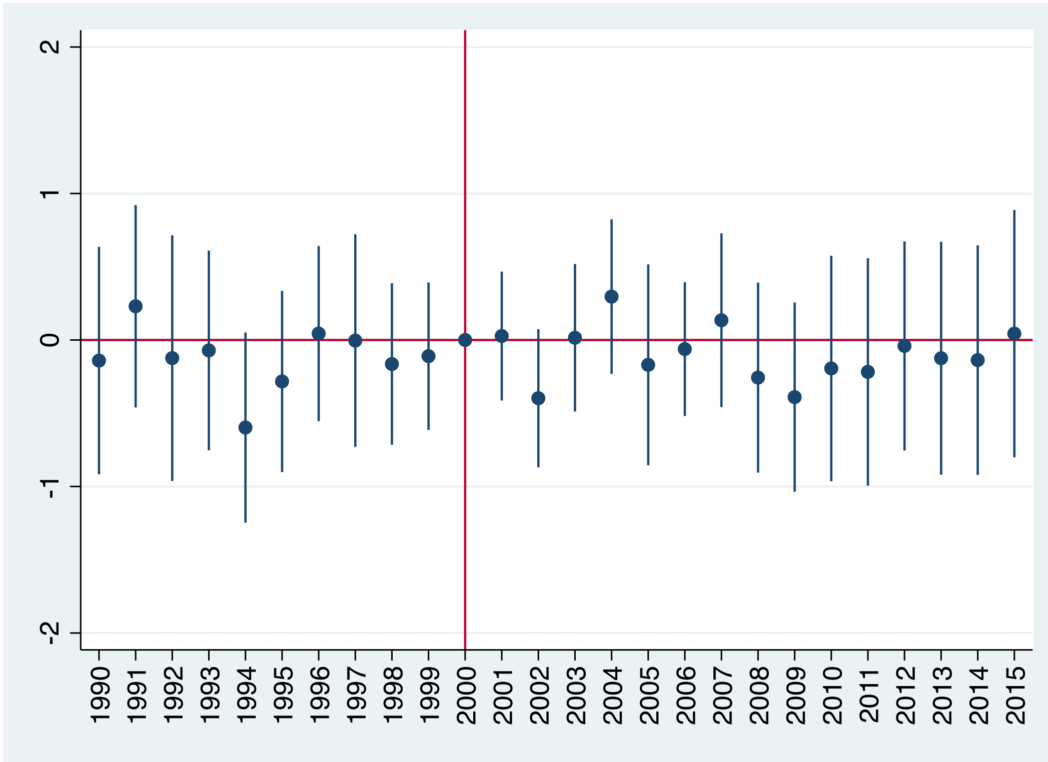


Figure 8
Total Number of Works – Test of Parallel Trends – Baseline Year 2000



7 Discussion and Limitations

7.1 Fame as a Reward

Sorensen (2007) demonstrated that appearing in the New York Times list of bestseller books caused an increase in revenues, but one question that arises is whether being included in this list also affected fame. This is important because fame might be a reward by itself in which case it might change authors' incentives to write in the future, making it potentially difficult to disentangle the effects of money and fame.

However, this might not be a big problem because all authors on this list were already "famous" before being included in the list, independently of whether they are in the treatment or control group. In addition, the rationale for using a demand shock is that current revenues are likely to affect authors' expectation about financial rewards from future books. The demand shock we use might affect current fame, but it is unclear whether it affects expected fame from future books.

7.2 Focus on Famous Authors

The focus on a list of superstar authors makes it impossible to generalize our results to the entire industry, and income may have different influence on creativity for non-superstar authors than it does for the focal superstar authors. In this regard, our paper is similar to the labor economics research examining how taxes affect work and taxable income for people at the top of the income distribution (e.g., Goolsbee 2000).

Concerning generalizability, it is well known that in the creative industries there is more concentration of revenues at the top of the distribution than in most industries (Rosen, 1981). In our focal industry a few books take the majority of all revenues implying that famous authors do not take a negligible percentage of the total revenue from of all books in the market. For instance, the top 100 (1,000) books took approximately 17% (45%) of all yearly sales between 2004 and 2016 (Liebowitz and Zentner 2019).

8 Conclusion

The intellectual copyright system assumes that monetary rewards induce creativity but there exists limited empirical evidence supporting this assumption. This paper exploits an exogenous positive demand shock that affected a quasi-random group of famous authors whose books were included in the New York Times list of bestseller books. Using data on authors' productivity from Goodreads and Wikipedia we do not find evidence suggesting that monetary rewards increase famous authors' future creativity on average. Because copyrights create a static inefficiency by giving monopoly power to creators, a finding suggesting that copyrights do not induce more creations or worse yet reduce creations would imply that granting copyright to already famous authors is economically inefficient. This finding would also have consequences for optimal taxation when creators are rewarded by top incomes (Jones 2019).

It is important to note that our use of data from famous authors implies that our results are not generalizable to all authors. Liebowitz and Zentner (2019) find that the supply of creativity has a positive slope for non-superstar authors and bends backwards for superstar authors. Although the results in this paper do not show much evidence suggesting that the supply of creativity for famous authors has negative slope, our results using a smaller sample do not show that the supply of creativity for these authors has positive slope. If these results are confirmed by future research on the book industry, an intellectual property rights system limiting the amount of copyright protection for already famous book authors might be warranted. Future research would also need to study whether this result extends to other industries protected by copyrights or patents.

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