Racial Discrimination and Lost Innovation

Evidence from US Inventors, 1895–1925*

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Abstract

How can racial discrimination harm innovation? We study this question using data on US inventors linked to population censuses in 1895-1925. Our novel identification strategy leverages plausibly exogenous variation in the timing of lynchings and the name of the victims. We find an immediate and persistent decrease in patents granted to inventors who share their names with the victims of lynchings, but only when victims are Black. We hypothesize that lynchings accentuate the racial content of the victim's name to patent examiners, who do not observe inventor race from patent applications. We interpret these findings as evidence of discrimination by patent examiners and provide evidence against alternative mechanisms.

Keywords: Discrimination, Innovation, Lynchings.

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1 Introduction

Large and persistent racial gaps are documented almost everywhere in the economy, from education to the labor market. A vast literature documents that race-based discrimination is one of the leading forces generating and perpetuating these gaps. Yet, it is typically hard to determine exactly at which stage discrimination generates such disparities. In this paper, we focus on the innovation process, where racial gaps are large and have been linked to racial discrimination (Cook, 2014; Sarada *et al.*, 2019; Andrews and Rothwell, 2020). Discrimination in various settings, such as in access to education, will result in gaps in the *pipeline* of inventors, i.e., before individuals submit patent applications. On the other hand, racial discrimination at the Patent Office may contribute to the observed racial gaps in patenting at the *last-mile* of the process, i.e., after applying for a patent. Understanding whether racial gaps are driven by pipeline or last-mile problems is a crucial precondition for implementing policies to alleviate them. Yet, causal evidence of discrimination in the patenting process – as in many other contexts – is scarce.

This paper provides evidence that discrimination in institutional settings – such as the patent office – may contribute to racial gaps. When an inventor files an application at the patent office, their name and surname are disclosed to patent examiners, but their race is not. We hypothesize that, in reviewing applications, patent examiners form beliefs about the race of inventors based on their names. We draw on the universe of US patents linked to population censuses between 1900 and 1920 from Berkes (2018) and Bazzi *et al.* (2022) and document that individuals with Black-sounding names are systematically less likely to obtain patents. The census data allow us to further account for various characteristics of individuals. We show that the negative correlation between Black-sounding names and inventor status is not explained by gender, age, occupation, education, or location. Importantly, it is not explained by race. In fact, we find a similar negative association if we restrict the sample to include white individuals only. This result is in line with extensive literature exploiting the fact that names convey racial signals (among others, see Bertrand and Mullainathan, 2004). However, names are also associated with other characteristics, such as socio-economic status, education, and cultural values, which may influence innovation activity (*e.g.*, Fryer and Levitt, 2004) and which we may still not be able to control for appropriately. Most importantly, discrimination at an earlier stage, – creating a leaky pipeline – is likely also correlated with names.

The historical setting enables a novel identification strategy that relies on plausibly exogenous variation in the names and timing of victims of lynching in the United States. Between 1882 and 1936, 2735 lynching episodes have been recorded in the US (Hines and Steelwater, 2006; Seguin and Rigby, 2019). Black individuals were the vast majority of victims of these episodes. Despite their geographic concentration in the South, lynchings received substantial media coverage throughout the country. Newspapers typically reported both the name of the victim and their race. We hypothesize that, following a lynching, the racial content of the victim's name becomes more salient to patent examiners. Under this assumption, a biased examiner would be more likely to reject patents whose inventor shares the name with a recently lynched individual. We exploit variation in the names of victims of lynchings over time in a difference-in-differences setting, coding a treatment variable that indicates when someone with a given first – or last – name is lynched. This setting allows us to provide causal estimates of the effect of racial discrimination on innovation.

Our main finding is that, after a lynching, fewer patents are issued to individuals who share the name with the victim. We first show this aggregating patent data at the name-year level. In a staggered difference-in-differences design, we document an immediate and persistent drop in patents whose inventor shares the name with the victim of a lynching in the years following the episode. This effect is sizable and stable in magnitude: 0.03 patents per person with a lynched name are lost in each of the ten years following a lynching episode, amounting to a reduction of by half their prior yearly patent production. Name – or surname – fixed effects account for name-level – or surname – time-invariant characteristics which may correlate with patenting activity, including parental investment, religiosity, individualism, and socioeconomic background. The underlying identifying assumption can be stated in terms of classical pre-trends and requires that, absent the lynching, there would be no time-varying difference in the number of patents granted to individuals who share their name with a victim of a lynching compared to those who do not. While the empirical analysis supports this assumption, there may be concerns that individual unobserved characteristics correlate with the timing of name-specific lynchings and with innovation activity.

To ease residual endogeneity concerns, we repeat the analysis at the individual-inventor level.[1] We confirm that the number of patents granted to inventors with the same name as the victim of a lynching suddenly and persistently drops in the years following the episode. We confirm that the effect is immediate, large in magnitude, and remains stable over a five-year horizon after the lynching. The crucial advantage of this exercise is that it allows us to include inventor-level fixed effects. These control for time-invariant unobservable characteristics which may correlate with innovation activity. Moreover, in robustness specifications, we include surname-by-year fixed effects to partial out possibly time-varying information conveyed by the surname of inventors.[2] In this setting, we compare inventors with the same surname but different names, thereby isolating the effect of the name-level lynching shock on innovation activity. These tests confirm our baseline results.

[1] We use novel data linked to non-anonymized US population censuses. This linked sample, which has been constructed by Bazzi *et al.* (2022), allows us to observe inventor-level demographic and socio-economic characteristics and explicitly include those as controls in our analysis.

[2] Where the surname identifies the treatment status, we include symmetric name-by-year fixed effects to partial out time-varying information contained in the name of inventors. Results remain qualitatively unchanged in these alternative specifications.

We interpret our findings as evidence of racial discrimination by patent examiners. Two alternative interpretations are conceivable. First, lynching episodes may either signal or exacerbate discrimination, preventing access to the innovation process (Cook *et al.*, 2018). That the effects we document are immediate, i.e., present and significant in the year of the lynching itself already, is strong *prima facie* evidence that other channels affecting the pipeline of innovation, such as increased local discrimination, cannot account for the entirety of our results. We perform three exercises to further rule out that discrimination outside of the patent office is responsible for our result. First, we exploit the fact that there is a slight time delay – 1.5 years, on average – between the application and the issue year of patents. If increased discrimination outside of the patent office discourages innovation activity, then we would expect to see our results using the application year instead of the issue year. However, we do not find any effect under using the former. Second, increased local discrimination based on names might drive our results. We investigate individual-level heterogeneous treatment effects depending on the distance between the lynching and the inventor. If local discrimination increased in response to the lynching, we would expect stronger effects closer to the lynching. Instead, our estimated treatment effect remains remarkably stable irrespective of the distance between the inventor and the lynching. Lastly, we only consider applications that were filed before the year of the lynching. While this necessarily yields a reduced and highly unbalanced sample, the results from this exercise further confirm that discrimination taking place after the lynching itself and outside of the patent office does not account for the immediate drop in patenting we observe for those inventors sharing their names with the victims. Note that we cannot rule out that discrimination outside of the patent office partially drives the reduced patenting rate by individuals sharing a name with lynching victims in the decade after the lynching.

The second alternative explanation is that lynching shocked the perceived violence associated with names. We provide two additional analyses against such an interpretation and instead highlight the shock's importance to the racial content of names. First, we exploit the fact that some victims were white. For those instances, we find far smaller coefficients that are statistically indistinguishable from zero. Second, we document heterogeneity along the blackness of the names of lynching victims. Our baseline results are driven by unambiguously and ambiguously black names. On the contrary, we find no effect of lynchings on patenting when the names of the victims were unambiguously white. Taken together, these exercises support our argument that discrimination in the patent–granting process, rather than local violence and discrimination, or the association of lynchings with violence per se, is the mechanism underlying our results.

This paper informs several streams of research. First, we inform the vast literature studying the economic effects of discrimination (among others, Bertrand and Mullainathan, 2004; Oreopoulos, 2011; Edelman *et al.*, 2017). More specifically, we connect to the recent set of papers studying racial discrimination by public officers, namely, judges (Arnold *et al.*, 2018), police officers (Goncalves and Mello, 2021), local public service workers (Einstein and Glick, 2017; Giulietti *et al.*, 2019), and elected politicians (Butler and Broockman, 2011). We provide evidence consistent with discrimination by US patent examiners, a class of public officials with a fundamental role as "gatekeepers of quality [of knowledge]" (Bryan and Williams, 2021, p. 17).

Second, we inform the literature studying discrimination against Black Americans in the pre-Civil rights movement period. In her seminal study, Cook (2014) shows that Black patenting declines in years after major discriminatory episodes, such as lynchings, race riots, and segregation laws take place, both nationwide and in particular states. We build on this work by showing that discriminatory episodes triggered discrimination in the Patent Office. Previous studies document the pervasive impact of discrimination against Black Americans in Antebellum Unites States. Among others, previous research documents impacts on employment segregation (Aneja and Xu, 2022), wealth inequality (Akbar *et al.*, 2019; Derenoncourt *et al.*, 2022), housing segregation (Logan and Parman, 2017a; Shertzer and Walsh, 2019; Derenoncourt, 2022), and violence and mortality (Cook, 2014; Black *et al.*, 2015; Cook *et al.*, 2018; Albright *et al.*, 2021). Andrews and Rothwell (2020) examine the contributions of Black Americans during the Golden Age of Innovation. This paper documents one additional effect of discrimination, namely, that it adversely affected innovation among the Black and white populations.

Third, recent studies have found that talent misallocation and barriers to innovation entail sizable social costs (Aghion *et al.*, 2019; Bell *et al.*, 2019; Hsieh *et al.*, 2019; Bloom *et al.*, 2020). We contribute to this literature by providing causal evidence that racial discrimination at the patent office is one of these barriers.

Lastly, we connect to a set of studies that exploit the informational content of names and surnames to identify individual race (Fryer and Levitt, 2004; Abramitzky *et al.*, 2020b), parental investment, and individualism (Bazzi *et al.*, 2020; Knudsen, 2021), religious beliefs (Andersen and Bentzen, 2022; Berkes *et al.*, 2022), socioeconomic background (Biavaschi *et al.*, 2017; Olivetti *et al.*, 2020), regional origins (*e.g.* Ochsner and Roesel, 2020), nationalism (*e.g.* Jurajda and Kovač, 2021), and immigrant assimilation (*e.g.*, Fouka, 2020). A common approach of these papers is to use names to measure individual-level characteristics. To the best of our knowledge, this paper is the first to exploit name-specific shocks as a source of identifying exogenous variation.

The paper proceeds as follows. Section 2 outlines the institutional and historical background. Section 3 describes the data sources. We document the negative association between Black names and patenting in section 4. In 5 we present the results of the difference-in-differences analysis, while section 6 provides further evidence supporting our interpretation of the results as racial discrimination by patent examiners. Section 7 concludes.

2 Institutional and Historical Background

In this section, we describe the patenting process and provide details on the internal functioning of the United States Patent and Trademark Office (USPTO). Then, we provide some background on the history of lynchings in the United States and the media coverage they received.

2.1 The USPTO and the Patenting Process

The first article of the United States Constitution recognizes the importance of scientific progress and establishes that inventors be granted exclusive rights over their discoveries for a limited amount of time. The USPTO – then named United States Patent Office – was established in 1790 pursuant to this constitutional dictate. The 1836 Patent Act formally instituted the US Patent Office and established the first modern patent system in the world (Khan and Sokoloff, 2004; Khan, 2020). The new American patent system was characterized by two distinguishing features compared to existing European models. First, patent applications were to undergo an examination of novelty to ascertain their originality. After the 1836 Patent Act, these examinations were carried out by qualified public officers – patent examiners – who were forbidden from obtaining patents. Second, application fees were affordable, especially when compared to the English system. Throughout the century, the USPTO maintained low fees which ensured that access to intellectual property protection was widespread (Sokoloff and Khan, 1990).

Until 2012, the USPTO was solely based in Alexandria, Virginia. In the period we analyze, the application and patenting process would roughly unfold in three phases. First, an applicant would file their patent application at the USPTO by mail. At the patent office, applications were assigned to one or more technological classes depending on their content. Unlike today, a single examiner would cover multiple technological classes with no overlap with other examiners.[3] Besides their name, surname, and geographic location, applicants did not disclose any information to the USPTO, except for the content of their patent application. Importantly for our empirical strategy, the race of inventors was not reported on the application or any other record available to them. Finally, examiners would either grant or patent protection or not, with an average delay between filing and issue date of 1.5 years. In the former case, the data on the issued patents were stored, and are ultimately available to us. In the latter, all information would be destroyed, and no surviving record exists (Andrews, 2021). For this reason, our sample only includes *issued* patents.

Our empirical strategy builds on the assumption that examiners did not know the race of applicants.This is plausible as the only information applicants had to disclose were their city of residence and their first and last name. Since the city of residence is too coarse to meaningfully convey information

[3] In 1910, there were 41 examiners at the patent office. Each examiner covered, on average, 3.3 technological classes without overlap across classes.

about the race of applicants, we argue that the name and the surname are the only information available to examiners which could signal the race of inventors.[4]

2.2 Lynchings in the United States and their Newspaper Coverage

A lynching is a form of extrajudicial public execution, perpetrated by a mob. It is operated to intimidate, punish, or kill, an alleged or convicted transgressor. In the United States, lynchings are typically associated with violent uprisings of the white population against the Black minority, which were common during the Reconstruction era and the early 20$^{\text{th}}$ century (Hines and Steelwater, 2006; Wells-Barnett, 2019). An extensive literature identifies economic and social factors as major determinants of this dramatic episode of modern American history (Tolnay and Beck, 1995; Olzak, 1990; Cook *et al.*, 2018). Lynchings had immediate political and economic effects (*e.g.*, Cook, 2014; Jones *et al.*, 2017), but their legacy reverberates until today (Williams, 2017).

A typical lynching episode involved a criminal accusation followed by arrest. A mob would typically assemble and physically assault the alleged transgressor. These violent episodes often resulted in the murder of the victim. Alleged crimes ranged from murder, arson, and robbery, to sexual violence, which features prominently in surviving records (Seguin and Rigby, 2019). Using data from Hines and Steelwater (2006), in Figure IIa we report the number of lynchings that took place between 1882 and 1935, by county. Lynchings were concentrated in the South, although a non-negligible number of episodes occurred in other areas of the country. Black Americans were the single most targeted group, as shown in Figure IIb.[5] By 1900, lynchings against the non-Black population had essentially stopped. However, more than fifty lynchings against Black Americans were perpetrated every year until the early 1920s, *i.e.* the time span this paper analyzes. The sharp increase in violence against Black people in the early 1890s documented in Figure IIb coincides with a period of economic downturn and the rise of the Populist party in the South.

Mass media, chiefly newspapers, covered lynching episodes extensively (Perloff, 2000; Weaver, 2019). This is crucial for our empirical strategy because it ensures that despite their distance from these episodes, patent examiners were likely well-informed about them. In Online Appendix A.1 we display two typical reports of lynchings from newspapers based in DC. They both report the name and surname of the victim, along with their race.[6] Lynching episodes, we argue, were covered throughout the country as well. Appendix Figure A.2 displays four articles from diverse locations reporting the same lynching that oc-

[4] While previous research assumed today's distinctively Black names to be a legacy of the Civil Rights movement (Fryer and Levitt, 2004), recent evidence has found that distinctively Black names date back to the end of the nineteenth century (Cook *et al.*, 2014).

[5] Figure A.3 breaks down lynchings by the race of the victim. Lynchings against Black Americans were concentrated in the South, whereas in other regions they targeted non-Black groups, most notably, European immigrants.

[6] We manually verify that newspapers covered lynchings recorded in the data from Hines and Steelwater (2006). More specifi-

curred in Lafayette county, Missouri. Newspaper readership was high throughout our analysis period.[7]

While there is ample literature documenting that lynchings represented a major shock to local communities as well as nationwide, to the best of our knowledge we lack any evidence that they impacted the racial perception of names. To validate this argument, in Appendix E we document that, after a person with a given name is lynched, the probability that newborn babies share their name decreases. Finally, once concern to our interpretation is that the names of lynched individuals may be associated with increased violence, rather than revealing information on the race. While we cannot unequivocally disentangle the two, in our analysis we do not find any impact of lynchings of individuals with unambiguously Black names. This suggests that lynching episodes acted mainly as information shock to the *racial* content of names, rather than to their association with violence.

3 Data

This section presents our data sources and describes the construction of the main variables used in the empirical analysis.

3.1 The Racial Content of Names

Building on previous empirical studies, we construct an indicator of the racial content of first and last names (Fryer and Levitt, 2004; Abramitzky *et al.*, 2020b; Fouka, 2020). From full-count confidential US population census data we retrieve information on names and surnames of the universe of the US population between 1900 and 1930 (Ruggles *et al.*, 2021). The Black Name Index (BNI) we define builds on the assumption that names that are relatively more common within the Black population signal that an individual is Black.[8] The associated indicator takes the following form:

$$\mathrm{BNI}_n = \mathrm{BNI}\,(\mathrm{Name} = n) \equiv \frac{\Pr\,(\mathrm{Name} = n \mid \mathrm{Black})}{\Pr\,(\mathrm{Name} = n \mid \mathrm{Black}) + \Pr\,(\mathrm{Name} = n \mid \mathrm{White})} \tag{1}$$

cally, we find that newspapers digitized at `newspapers.com` cover 96% of a randomly selected sub-sample of lynchings in Hines and Steelwater (2006). Newspapers based in Washington, DC, by comparison, covered 65% of them. We provide additional details in Online Appendix section D. These high reporting rates are not surprising, since the primary source of lynching data is newspapers. However, they nonetheless attest that newspapers covered by current providers offer an arguably complete picture of historical events (Beach and Hanlon, 2022).

[7] Newspaper circulation per capita in Washington, DC was high over the period, doubling from 0.37 in 1896 to 0.78 in 1928 (Own calculation based on data from Gentzkow *et al.* (2004)).

[8] Throughout the paper, we refer to the Black and white populations with a slight abuse of language. By "white" we mean all those who do not report their race as Black in the US census. This comprises individuals identified as white, Asian, and other races. However, since Black and white individuals make up 99.57% of the overall population, for the sake of brevity we refer to them only, under this caveat.

where $\Pr(\text{Name} = n \mid \text{Black})$ is the share of Black individuals with name n, and analogously we define $\Pr(\text{Name} = n \mid \text{White})$ as the share of white individuals called n. The index ranges from 0 to 1. It returns value 0 if all individuals with name n are white, 1 if they are all Black. If a name has equal relative frequency in the Black and white populations, its BNI is .5. If, for instance, Black people are three times more likely to choose name n relative to the white population, then $\text{BNI}_n = .3/(.3 + .1) = .75$. More in general, any BNI above (resp. below) .5 indicates that the name is relatively more (resp. less) common among Black than among white individuals. Following Fouka (2020) and Abramitzky *et al.* (2020b), we drop all names that appear less than five hundred times in our sample. Moreover, we only include names of U.S.-born individuals to ensure that the BNI does not purely reflect heterogeneous naming patterns across groups of immigrants. We define a Black Surname Index (BSI) to exploit the informational content of surnames and report all surname-level results in the Online Appendix. In Online Appendix Table A.1 we report a subset of names and surnames, and their associated BNI and BSI. Because of bunching on the lower tail of the BNI and BSI distributions, names and surnames with the lowest BNI and BSI are picked at random. Bunching is most likely due to the fact that names that were common among immigrant groups are never picked by the Black population.[9]

In Online Appendix Figure A.5 we report the distribution of the BNI and the BSI. Following the literature, in our main analysis we report results using the BNI and report the surname-based evidence in the Online Appendix. In Online Appendix Figure A.7 we report the accuracy and the power of the BNI as a classification variable for individual races.

3.2 Lynching Data

Data on lynching episodes are from Hines and Steelwater (2006) and Seguin and Rigby (2019), who in turn rely on historical newspaper reports as their primary source. Cook (2012) provides a detailed discussion of lynching data. These contain the date of the lynching, the name, surname, and race of the victim, and the alleged crime. We construct a yearly variable that, for every name and surname, returns value one the first time someone with the given name is lynched, zero otherwise.[10]

Lynchings were extensively covered by the media (Weaver, 2019). However, what is key in our analysis is that newspapers in Washington, DC covered these episodes. To validate this, in Online Appendix D we manually check that historical newspapers located in the DC area, as well as any newspaper in the

[9] Note that this does not necessarily imply that either low BNI or BSI are attached to wealthy descendants of early immigrants. In fact, strongly non-Black-sounding names are of Irish, Italian, and Polish descent. These groups tended to feature below-average education levels and were commonly employed as low-skilled workers (*e.g.* Abramitzky *et al.*, 2020a). This is relevant in our analysis because it implies that names with associated low BNI or BSI do not artificially sample positively-selected groups.

[10] For instance, if someone called "Herbert" is lynched in 1898, 1911, and 1925, the treatment variable $\text{Lynched}_{\text{Herbert}}$ returns value one in 1898 only. In a robustness exercise, we code an alternative indicator which, in the previous example, would return value one in 1898, 1911, and 1925.

nation, report information on the lynching episodes recorded in our data. We find that 65% of the universe of lynchings in our sample are reported in local DC-based newspapers, while virtually the universe of them is reported in national media outlets.

3.3 Patent and Inventor Data

Our measure for innovation activity is the universe of patents granted by the USPTO between 1895 and 1935. The data was digitized by Berkes (2018) from historical documents and linked to population censuses by Bazzi *et al.* (2022). Berkes (2018) provides detailed information on this dataset. The linked sample is constructed as follows: inventors listed on a patent filed between $t-5$ and $t+4$, where t is a given census year, are matched to the full-count census in year t. To perform the matching, Bazzi *et al.* (2022) uses state-of-the-art linking algorithms (*e.g.*, Bailey *et al.*, 2020; Abramitzky *et al.*, 2021), conditioning the set of matches to be no further than a given distance threshold from the geo-coded location of the patent. Inventors can thus be paired with none, one, or multiple matches in the census. While the number of failed matches is very low, multiple matches are possibly problematic. In our baseline analysis, we keep inventors with no more than five matches in the population census. Moreover, to avoid multiple counting, we weigh each inventor by the inverse of the number of matches they are paired with.

From this data, we construct two main outcome variables. First, we aggregate patents by the name of their inventors to construct a yearly name-level series of patenting activity. We use this as the main outcome variable in section 5.2. Second, we assemble an inventor-level series. Given the structure of the dataset linking inventors to the population census, we observe the patenting activity of an inventor over a five-year window around each census between 1900 and 1920. We then stack inventors over the three censuses. We use this dataset in section 5.3.

The major advantage of linking inventors to the census is that we observe a large set of individual-level variables, most importantly the applicants' race, which are not reported on patents. In Table I we produce summary statistics for the linked inventor sample. Inventors in this period are largely white – 95% of the total pool. The majority of them live in urban centers and, unsurprisingly, are literate. They are generally employed in skilled occupations and live in the Midwest and Northeast.

4 Individuals with Black-Sounding Names Are Less Likely to Be Inventors

This section documents two novel stylized facts for historical patents. First, individuals with more Black-sounding names are less likely to become inventors. Second, this negative association holds also among *white* individuals.

In Figure I we show that individuals with more Black-sounding names, as captured by the Black Name Index (BNI), are less likely to be inventors. The unit of observation is an individual, observed in one population census between 1900 and 1930. We assign to every observation an indicator equal to one if the person was granted at least one patent in the ten-year window around the census year, and zero otherwise. In the graph, we control for race, literacy, and census decade fixed effects, but the negative correlation remains robust to the inclusion of additional controls and fixed effects, as reported in Table II. Columns (1)–(3) display the unconditional correlation for, respectively, the whole population, white individuals, and Black individuals. In column (4) we include county fixed effects to partial out time-invariant unobserved heterogeneity at the county level. In column (5) we include cohort fixed effects. In column (6) we control for race to ensure that patent examiners cannot perfectly distinguish between white and Black applicants. In column (7) we include occupation fixed effects as a proxy for socioeconomic status. Finally, in column (8) we explicitly include surname-fixed effects to leverage name-level variation across individuals with the same surname. Figure B.2 and Table B.1 repeat this exercise using the BSI as the main independent variable, and show patterns consistent with the BNI. Finally, in Appendix Table B.3 we estimate the simple correlation between the BNI and inventor status, excluding individuals located in one US region at a time. The negative correlation between BNI and inventor status holds throughout the various sample definitions.

In Figure B.1a we find that the share of inventors among whites is decreasing in the Black Name Index. Relative to the previous section, we now restrict the sample of analysis to whites only. The negative correlation between inventor status and BNI that holds for the entire population remains on the white sub-sample. These differences are sizable. The share of inventors within the white population with names that are four times more likely among Black than among white people is approximately 70% lower than among white individuals with names that are similarly common among white and Black people.

We interpret these findings as suggestive of racial discrimination against Black Americans. This may operate on two layers. First, widespread discrimination against the Black population has been documented in disparate areas, including schooling (Donohue III *et al.*, 2002), housing and employment segregation (Logan and Parman, 2017b; Aneja and Xu, 2022), and wealth (Derenoncourt *et al.*, 2022). All these factors likely contribute to lower patenting rates among Black people (Cook, 2014). Second, this finding suggests the possibility that patent examiners discriminated, either unconsciously or deliberately, against Black Americans. If examiners had racial biases against the Black population, then the race signaled by applicants' names may drive this discriminatory behavior. In the rest of the paper, we focus on this second channel.

5 Racial Discrimination and Innovation: Evidence from Lynchings

This section presents the main empirical results of the paper. We first discuss the research design and the identification strategy, which leverages lynchings as a shock to the racial perception of names. We then present two sets of empirical results which provide causal evidence that discrimination dampens innovation. We conclude by summarising the robustness analysis.

5.1 Identification and Research Design

Our identification strategy hinges on finding name-specific shocks to the racial content conveyed by names (or surnames). We argue that lynchings represent one such shock. After a lynching, newspapers all over the country reported the episode, along with the name and the race of the victim, and most often the crime they allegedly committed.[11] Our main assumption is that patent examiners – and, more generally, readers of newspapers – were influenced by such reports. In particular, we claim that articles covering lynchings increased the perceived racial content of the name of the victim. Since examiners did not know the true race of patent applicants, newspaper coverage of lynching episodes represents a shock to the racial information of applicants' names. To validate the salience of lynchings, in Online Appendix E we show that these episodes impacted other aspects of behavior. In particular, we find a sizeable decrease in the probability that newborn children are named after the name of a victim of lynchings.

Our approach relies on two testable assumptions. First, the names of victims of lynchings must not be unequivocally Black. More generally, they should not *ex ante* perfectly identify any racially denoted group of the population. In Online Appendix Figure A.6 we report the distribution of the BNI and BSI in the entire population, in colored bars, and that of lynched names, in black-contoured bars.[12] Lynched names and surnames are more Black-sounding than the average. However, their distribution spans the entire support of the distribution in the overall population, which provides sufficient variation for our empirical analysis. The second assumption can be stated in terms of standard parallel trends and requires that absent the lynching, there would have been no time-varying differences in the racial perception of lynched names, *vis-à-vis* non-lynched ones. Throughout the paper, we rely on event study designs which empirically support this assumption.

We present our empirical results under two specifications, both in the form of difference-in-differences (DD) models. Our first approach is to aggregate patents by the name of the inventor, at a yearly frequency. We define a treatment indicator, call it Lynching_{nt}, which returns value one the first year t that someone

[11] In Online Appendix D we manually verify that newspapers in Washington, DC covered lynching episodes in our data.

[12] For the sake of brevity, if there is at least one victim of a lynching which carries a particular name, we refer to that name as a "lynched name".

with name n is lynched, and is zero otherwise. We then estimate the following flexible DD specification:

$$\text{Patents}_{nt} = \alpha_n + \alpha_t + \sum_{k=-a}^{b} \beta_k \times \mathbf{1}\left[\text{Lynching}_{nt} = k\right] + \varepsilon_{nt} \tag{2}$$

where n and t denote respectively name and year with associated fixed effects α_n and α_t, $\mathbf{1}(\cdot)$ is an indicator variable, and $\left(\text{Lynching}_{nt} \equiv t - \text{Lynching}_n\right)$ denotes the number of years since name n was first lynched. The dependent variable is the number of patents per capita (Patents_t), which we define as the number of patents granted to inventors with a given name, normalized by the number of people carrying that name in the 1880-census.[13] Standard errors are clustered at the name level. The set of coefficients $\{\beta_k\}$ with $k \geq 0$ yields dynamic treatment effects of the lynching on innovation activity, and under the assumption of no pre-trends those with $k < 0$ are expected to be statistically indistinguishable from zero. The two-way fixed effects control for aggregate time-varying factors and name-specific time-invariant ones. They jointly imply that the identifying variation in this setting compares lynched and non-lynched names, around the year when they are first lynched.

The second specification we employ is entirely similar to (2), except that it operates at the individual-inventor level. Including inventor fixed effects allows us to control for unobserved variation at the individual level which may correlate with innovation activity. Our sample is the universe of inventors with patents issued between 1895 and 1925. These are linked to the decennial population censuses of 1900, 1910, and 1920. As discussed in section 3.3, we observe every inventor for ten years around each census. In particular, to be included in the estimation sample, an individual must have been issued a patent before and after the first time, their name or surname is lynched. We impose this restriction because we would otherwise not observe some inventors in the pre-treatment period or, on the contrary, only observe them after the treatment.[14] We estimate the following baseline specification:

$$\text{Patents}_{inct} = \alpha_i + \alpha_{c \times t} + \sum_{k=-a}^{b} \beta_k \times \mathbf{1}\left[\text{Lynching}_{nt} = k\right] + \varepsilon_{inct} \tag{3}$$

where the notation follows from specification (2), except that i now denotes an inventor, and c his county of residence. The baseline dependent variable is the number of patents granted to inventor i in year t. However, we also report results for a categorical dependent variable taking value one if inventor i obtains at least one patent in year t, and zero otherwise. Model (3) includes inventor-level fixed effects which control for individual unobserved heterogeneity, and county-by-year fixed effects to compare inventors within the same county and year. As before, standard errors are clustered at the name level, which is the dimension at which the treatment varies.

[13] Results holds virtually unchanged if we take the $\ln(1 + \cdot)$ or the inverse hyperbolic sine of the dependent variable.

[14] This sample cut is necessary to estimate the difference-in-differences specification. The major cut it imposes is that we drop all inventors with one single registered patent

Because the setting is staggered, *i.e.* the treatment period varies across units, either them be names or inventors, a recent literature argues that the common two-way fixed effects (TWFE) OLS estimator may fail to yield the correct estimate of the average treatment effect (Sun and Abraham, 2021; Callaway and Sant'Anna, 2021). To ensure that our baseline estimates are not affected by this, we repeat the exercises using the estimator proposed by De Chaisemartin and D'Haultfœuille (2022). First, this allows us to adjust for staggered treatment roll-out. This is a concern in our setting, because different names may be lynched in different time periods. Second, it enables us to explicitly allow for repeated treatments.[15] Repeated treatment periods in our context arise because the same name may be lynched multiple times. If that is the case, the TWFE estimator in models (2)–(3) may yield upwardly-biased estimates because it conflates multiple treatment instances in the unique post-treatment period. All our results are qualitatively unchanged when using the De Chaisemartin and D'Haultfœuille (2022) estimator. We prefer not exploiting variation arising from multiple treatment periods in our baseline analysis, since there are very few "switchers", *i.e.* names that are treated multiple times.

5.2 Name-Level Results

Figure III reports the estimates of model (2), and in Table III we show the associated static specification. Both document a significant and persistent decline in the number of patents issued to inventors who share their name with the victim of a lynching after the lynching episode. The decline is sharp and immediately follows the lynching. The immediacy of the drop speaks directly to our interpretation as information treatment for patent examiners. Due to the delay from application to issuance, such a treatment may take a few years to materialize. The continued and increasing negative effect could be explained by several other mechanisms as well, for instance, affecting the pipeline of inventors via occupational choice, their access to financing, access to job positions allowing for patenting, or even name changes.

In terms of magnitude, lynched names are issued on average 0.03 fewer patents per everyone with that name every year in the decade following the lynching, compared to non-treated names. This is a sizable drop, as it accounts for 21% of the average number of patents per name year and 48% of the average number of patents for names that are ultimately lynched before the lynching. Figure III presents evidence supporting the parallel trends assumption. We find that lynched and non-lynched names are comparable before the lynching period, as no $\beta_{k<0}$ coefficient is statistically different from zero. Table III provides some additional insights. In column (2) we show that the effect holds if we weight each patent by a text-based measure of "breakthrough" innovation derived by Kelly *et al.* (2021). In columns (3) and (4) we distinguish between lynching episodes against white and Black individuals. We find no effect of

[15] These two features make the estimator by De Chaisemartin and D'Haultfœuille (2022) more suited for our setting compared to other estimators, such as the imputation method discussed in Borusyak *et al.* (2021).

lynchings against white people on patenting rates. We return to this in section 6, when we discuss the possible underlying mechanisms. Lynchings exert a negative effect on innovation by white and Black inventors alike, as shown in columns (5)–(6). Patents vary widely in terms of quality and subsequent economic value. If lynchings impacted low-quality innovation only, the effect of discrimination on innovation would bear little economic relevance. In column (7) of Table III we show that the number of “breakthrough" patents decreases after a lynching shock.[16] Finally, in column (8) we use the share of “breakthrough" patents relative to the total number of patents granted as the dependent variable. Under taste-based discrimination, we would expect the relative share of “breakthrough" patents to increase following a lynching shock.[17] Our findings confirm this empirical prediction: after a lynching, the share of “breakthrough" patents by inventors with the same name as the victim of the lynching increases by 0.8 percentage points, amounting to a reduction of 34% from the sample mean, and 50% from the pre-lynching mean of lynched names.

These results provide evidence that lynchings had a negative impact on innovation. At a first glance, however, they do not unambiguously favor our interpretation of racial discrimination by patent examiners. There are at least two concerns with this interpretation. First, names of victims of lynchings may convey an impression of violence, rather than a racial signal. Second, local discrimination following lynching episodes may deter inventors with Black-sounding names from attempting to obtain a patent. In section 6 we return to these potential interpretation challenges.

An third potential issue is *passing*. Dahis *et al.* (2019) document that a substantial share (16%) of Black individuals “passed as white”, i.e. they changed the race they report across Censuses from Black to white. This does not affect the majority of the results we present in this paper, for they hold equivalently on the whole sample of inventors, as well as restricting it to the white population only. Passing may induce downward bias in our estimates if Black people sharing their first names with lynching victims are systematically more likely to report themselves as white across Censuses *and* are less likely to invent than the average white person with that name after the lynching. Since our estimates on the white subsample are consistent with those on the entire sample, we conclude that this is not a major threat to the validity of our analysis.

We perform several checks to ensure the robustness of our results. We summarize our findings here but provide full details in Online Appendix C.1. First, results are qualitatively robust to defining

[16] Empirical analyses typically measure patent quality with citations. This is unfeasible in our historical setting for citations were not mandatory and, in fact, relatively rare (Berkes, 2018). We follow Kelly *et al.* (2021) and label a patent as “breakthrough” if it is in the top 5% of the overall distribution of text-based measure of quality.

[17] The argument follows from Becker (2010). Discrimination imposes a “sunk cost” which implies that the marginal patent from an inventor belonging to the group which is discriminated against will be of higher quality compared to one from a non-discriminated individual.

the treatment in terms of surnames, rather than names, as shown in Figure C.1 and Table C.4. Second, in Table C.1 we show that the significance of our results is virtually unaffected when using alternative standard error estimators. Third, in Figure C.2a we employ the estimator proposed by De Chaisemartin and D'Haultfœuille (2022) which accounts for the possibility that a single name may be lynched multiple times over the analysis period. In Figure C.4 we show that the results are virtually unaffected if we either (i) estimate model (2) on a subset of US counties, or (ii) we exclude selected counties from the estimation sample. In particular, results obtained by either dropping Southern states or estimating the model only on Southern states are statistically indistinguishable from those using the full sample. Finally, we present evidence that the significance of the baseline results is unaffected under alternative standard error estimators.

5.3 Inventor-Level Results

We now present results at the individual level. These are motivated by the fact that the name-level analysis cannot account for unobserved heterogeneity at the inventor level. This would bias our previous results if unobserved factors were correlated with innovation activity *and* with the timing of name-specific lynching shocks. The structure of our data allows for performing a more granular analysis, where we can include fixed effects that explicitly account for this concern.

Figure IV presents the estimates of model (3), and Table IV reports the associated static specification. The model includes inventor-fixed effects along with county-by-year fixed effects. Our analysis compares inventors within the same county and year, net of unobserved time-invariant individual heterogeneity. Similar to previous results, we find an immediate and persistent decline in the number of patents after a lynching. The effect is large, as 0.098 patents per inventor are lost every year in the five years after the lynching, compared to an average of 0.19 yearly patents per inventor (50%).[18] Moreover, this likely provides a lower bound to the cost of discrimination on innovation, because we include in the estimation sample only those inventors who are issued at least one patent before *and* after the treatment period. The Figure provides direct evidence of the absence of time-varying differences in patenting activity across treated and non-treated inventors, before the lynching. Since anticipation of the treatment is unlikely in this setting, we do not worry that this may invalidate the causal validity of our design.[19] Importantly, the inventor-level specification allows to explicitly control for surname-level fixed effects (columns (2) and (6). These ensure that we partial out the informational content conveyed by inventors' surnames,

[18] Given the positively selected sample of treated inventors, i.e., those that successfully apply for patents before and after the lynching, their average number of patents (before the lynching) amounts to 0.54 patents per year. Hence the effect for those is a yearly reduction of 18%.

[19] To anticipate the treatment, examiners would need to know that someone with a given name would be lynched one year ahead. This appears to be implausible, all the more so given that examiners were located hundreds of kilometers far from the epicenter of lynchings in the South.

as we compare inventors with the same surname, but different names. Third, we show that our results are quantitatively stable regardless of the geographic fixed effects we include. This provides one further robustness test for the name-level analysis, where we do not control for such level of variation.

In Online Appendix section C.2 we discuss in detail the robustness analysis of the inventor-level results. First, in Figure C.5 and Table C.6 we repeat the entire analysis using surnames instead of names as the treatment indicator and find qualitatively similar results. Second, employ alternative standard error estimators and find analogously significant estimates of the treatment effects, in Table C.5. Finally, as in the previous section, in Figure C.6 we repeat the estimation following the methodology by De Chaisemartin and D'Haultfœuille (2022) and find quantitatively similar results.

6 Mechanism: Discrimination at the Patent Office

The results presented so far provide evidence of the causal impact of lynchings on innovation. We argue that one underlying mechanism links lynchings to increased racial discrimination by patent examiners. This is driven by wide newspaper coverage of lynching episodes increasing the salience of the racial content conveyed to examiners by applicants' names. There are two possible counter-arguments to our reasoning. First, the names of victims of lynchings may convey an impression of violence, rather than a racial signal. Under this interpretation, lynchings would not shock the racial content of names and consequently, the mechanism underlying our finding would not be discrimination, but rather a generic aversion to violence. Second, lynchings may negatively affect the pipeline of inventors sharing a name with the victim due to discrimination taking place outside the patent office and after the lynching. This may, for instance, discourage inventors from seeking intellectual property protection due to the increased risk of racial threats. In this case, the underlying mechanism would be one of discrimination elsewhere than the patent office. Both arguments build on Cook (2014), who connects violence measured as lynchings to missing innovation and increased local discrimination.[20] In this section, we provide evidence against both hypotheses.

First, suppose that lynching episodes shock the perceived violence associated with names. If that was the case, we would expect similar effects of lynchings of Black *and* white individuals. In Table III, columns (3)–(4), however, we find no effect of lynchings against white people on name-level patenting race. This difference suggests that the race of the victims is crucial and, consequently, provides evidence against the violence mechanism. Figure Va documents additional evidence that the racial content of lynched names is decisive for our findings. If lynchings merely increased the perceived violence of names, then

[20] It is worth noting that local discrimination is unlikely to challenge our interpretation because, in section 5, we do not distinguish between Black and white inventors. While it is entirely possible that local discrimination affected the Black population, it is unlikely that it affected white people for the mere reason that they shared the name with the victim of a lynching. Because of passing, however, we provide robust evidence against this alternative interpretation (Dahis *et al.*, 2019).

we would expect homogeneous treatment effects across the BNI distribution. In other words, unambiguously white names, labeled as those featuring a BNI below .2, should display a similar response to the lynching shock as other names. If, by contrast, lynchings increased the racial content of names, then we would expect to find no such effect on unambiguously white names. This is because names that are clearly indicating white individuals are unlikely to signal Black people, even in case of lynching. This simple observation allows us to test the two hypotheses. In Figure Va we show that we find no effect of lynchings on unambiguously white names. This contrasts the results presented in section 5.2 and supports our preferred narrative. In Appendix Figure C.3 we undertake a more rigorous exercise and estimate the baseline model (2) by 2-quantiles of the Black Name Index distribution. We find that names at the top and bottom of the BNI distribution display little response to the lynching, whereas significant estimated effects are concentrated in the middle of the distribution. This corroborates our interpretation of lynchings as shocks to the racial perception of names.

Next, we show that discrimination elsewhere and after the lynching alone is unlikely to explain the entirety our results. Note ahead that our results support the conclusion that such discrimination accounts for some of the effect we document, particularly in the later years after the lynching. For instance, discrimination in access to financing and on the job market surely contributes to a leaky pipeline for potential inventors with such a name. What we focus on here is documenting that some of the effect, especially in the immediate years after the lynching, results from racial discrimination at the patent office and not entirely by discrimination in other walks of life. We employ three strategies to support this assessment. First, we exploit an empirical regularity of the patenting process whereby there is a slight delay – 1.5 years on average – between the filing and the eventual issuance of a patent.[21] If discrimination outside of the patent office explained lower invention rates of individuals sharing their name with a victim of a lynching, then we would expect (i) a delayed response of innovation using the issue date as the time indicator, and (ii) an immediate effect on the filing year of the patent. In Figures III and IV, where the time indicator is the issue year of patents, we already noted that the drop in innovation activity is immediate. This contrasts hypothesis (i). In Figure Vb we estimate model (2), but substitute the issue year with the filing year. The difference is clear: we find no effect of lynchings on innovation activity after filing since the effect is present only in the post-issuance period. In Appendix Table C.2 we present regression-based evidence considering alternative delay thresholds. We find no statistically significant drop in patenting rates after the filing date and negative and statistically significant decreases after the issue year. Taken together, these results provide one first piece of evidence suggesting that local

[21] In Appendix Table A.2 we report sample statistics on the delay between filing and issue year in our sample. Appendix Figure A.8 displays the average delay in the empirical distribution function of the delay time, and the average 2-year delay we set in the baseline specifications.

discrimination alone is inconsistent with our results.[22]

As an additional check, we leverage the geographic distance between inventors and lynching episodes. If local discrimination was a major driver of our results, then we should expect that the treatment effect decreases in the distance between an inventor and the lynching.[23] We test this intuition in Table V. In columns (1), (2), and (3) we report the estimated impact of lynchings that are closer than, respectively, 300, 500, and 700 km to the inventor; columns (4) includes the continuous distance between inventors and lynching episodes, interacted with the baseline treatment, as one further control. Finally, in column (5) we include the interaction between the baseline treatment and quintile indicators of the distance between inventors and lynchings. The estimated treatment effects remain remarkably stable across all specifications. In fact, we cannot reject the null hypothesis that they are all statistically equal. Table V thus provides evidence against local discrimination as the main mechanism underlying our findings. In Appendix Figure C.7 we estimate the difference-in-differences model (3) varying the distance threshold between the inventor and the lynching that activates the treatment. The estimated treatment effects do not statistically differ with distance. This confirms that local discrimination alone is unlikely to be the main driver underlying our results. However, the Figure provides suggestive evidence that close lynchings further penalize invention rates. This squares well with the estimated negative and significant effect of the interaction between the treatment and the first quintile of the distance distribution in Table V, and with the evidence presented by Cook (2014).

Finally, we estimate the baseline name-level regression on the sub-sample of patents that are filed before someone with the same name as their inventor is lynched, *i.e.*, we only look at patents filed before the treatment is activated. Because (i), on average, the filing-issue delay time is two years, and (ii) we exclude never-treated names, the caveat of this exercise is that we lose a substantial number of observations (around 90% of the baseline sample). Moreover, the resulting panel is unbalanced for names observed over spells of different duration. Bearing these limitations in mind, Table C.3 presents the results of this exercise. These confirm all the baseline results, despite a generalized loss of statistical significance. Note that since we only include patents that were filed before each name-lynching, confounding factors arising from violations of the exclusion restriction would not invalidate this evidence.

[22] Further auxiliary evidence supports this interpretation. In columns 5 and 6 of Table III we document that the size or the effect is highly comparable for Black and white inventors. If local discrimination were driving our results, we would expect this to be particularly consequential for Black inventors instead.

[23] Note that it is unlikely that inventors close to lynching episodes alone drive this result. In Appendix Figure A.9 we report the distribution of the distance between inventors and lynchings. The average distance is slightly above 1,400 Km ($\approx$ 870 mi), hence it seems implausible that the core of the effect was driven by those very close to actual lynching episodes.

7 Conclusions

This paper documents racial discrimination at the patent office in the early 20th century, drawing on patent data linked to the full count census and a novel identification strategy based on the shocks to the racial content of names. We start by showing that individuals with Black-sounding names are substantially less likely to be granted a patent compared to those whose name is not informative of their race. This correlation is not explained by individual characteristics, including occupation, income, education, and *race*. In fact, it also holds in the white sub-sample of the population. This is sizable: white individuals with names that are four times more common among Blacks are 60% less likely to invent compared to white people with names that are equally common among the white and Black populations. We interpret this fact as suggestive evidence that names may convey information on the race of applicants to patent examiners (Fryer and Levitt, 2004; Cook *et al.*, 2014).

To address residual endogeneity in naming patterns, we develop a novel identification strategy that exploits variation in the names of the victims of lynching episodes. We argue that since lynchings received widespread news coverage, they increased the perceived racial content of the names of the victims. Under this interpretation, racially-biased examiners would reject more often patents whose applicants share their name with one recently lynched individual. We test this claim in a difference-in-differences setting leveraging name-specific shocks to the racial content of names. We show that, after a name is lynched, the yearly number of patents granted to inventors with that name drops by 15% over the subsequent ten years. We confirm this result in a more demanding specification run at the individual-inventor level. We interpret these results as evidence of racial discrimination by patent examiners, and rule out two alternative mechanisms. First, we exclude that lynchings convey a general impression of violence as we find no effect of lynchings of *white* victims on patenting by inventors with that name. Second, we test whether our results are explained by discrimination outside of the patent office, which discourages inventors from seeking intellectual property protection after a lynching (Cook, 2014). We run a series of check that suggest that, in the years immediately following the lynching, discrimination *inside* the patent office is likely to be the main driver of our results. More specifically, we find (i) no effect on the application year of patents, (ii) no heterogeneous treatment effects depending on the distance between the inventor and the lynching, and (iii) that our results are comparable when we limit the entire analysis to patents that were filed at the patent office before the lynching. Taken together, our results show that racial bias of patent examiners can have a negative impact on the production of innovation.

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Tables

TABLE I: INVENTOR SUMMARY STATISTICS

	Observations	Count	Mean	Std. Dev.	Min.	Max.
	(1)	(2)	(3)	(4)	(5)	(6)
Categorical variable = 1 if:						
Panel A: Demographics						
is White	471284	448955	0.953	0.212	0	1
is Black	471284	21978	0.047	0.211	0	1
lives in urban center	471284	307073	0.652	0.476	0	1
can read and write	471284	459835	0.976	0.154	0	1
Panel B: Origin						
is born in U.S.	471284	330715	0.702	0.457	0	1
is born in Germany	471284	53370	0.113	0.317	0	1
is born in Ireland	471284	29235	0.062	0.241	0	1
is born in Great Britain	471284	25510	0.054	0.226	0	1
Panel C: Residence						
lives in Midwest	471284	184831	0.392	0.488	0	1
lives in Northeast	471284	183615	0.390	0.488	0	1
lives in South	471284	84502	0.179	0.384	0	1
lives in West	471284	14679	0.031	0.174	0	1
Panel D: Income & Professions						
is in income quintile $\in [0, 5]$	388794	1453983	3.740	1.387	1	5
is employed in skilled manufacture	471284	88342	0.187	0.390	0	1
is employed in agriculture	471284	68417	0.145	0.352	0	1
is employed as manager	471284	57326	0.122	0.327	0	1
is employed as clerk	471284	49588	0.105	0.307	0	1
is employed as professional	471284	36843	0.078	0.268	0	1

Notes. This Table provides summary statistics for our sample of inventors. Variables are from the full-count population censuses 1900–1920. Each variable is a dummy equal to one if the inventor belongs to a given category, and zero otherwise, except for the income quintile which is a categorical variable taking values between one and five. Panel A reports the two most common races. Panel B reports the four most common origin countries. Panel C reports residence of origin by Census Bureau divisions. Panel D reports the five most common coarse occupation classes.

TABLE II: CORRELATION BETWEEN BLACK NAME INDEX AND INVENTOR STATUS

	Unconditional Correlation – Sample:			Including Fixed Effects:				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	Whites	Blacks	County	+ Cohort	+ Race	+ Controls	+ Surname
Black Name Index	-2.107***	-1.428***	-1.897***	-0.293***	-2.030***	-1.952***	-1.142***	-1.125***
	(0.080)	(0.063)	(0.153)	(0.051)	(0.068)	(0.066)	(0.040)	(0.038)
County FE	No	No	No	Yes	Yes	Yes	Yes	Yes
Cohort FE	No	No	No	No	Yes	Yes	Yes	Yes
Race FE	No	No	No	No	No	Yes	Yes	Yes
Controls	No	No	No	No	No	No	Yes	Yes
Surname FE	No	No	No	No	No	No	No	Yes
Sample	All	Whites	Blacks	All	All	All	All	All
Observations	220708585	193656026	26593737	220708585	220708581	220708581	220708579	219275079
R^2	0.000	0.000	0.000	0.001	0.002	0.002	0.007	0.012
Mean Dep. Var.	2.112	2.293	0.817	2.112	2.112	2.112	2.112	2.123
Std. Beta Coef.	-0.008	-0.005	-0.011	-0.001	-0.007	-0.007	-0.004	-0.004

Notes. This Table reports the cross-sectional correlation between the Black Name Index (BNI) and inventor status. The unit of observation is an individual, observed in a full–count population census between 1900 and 1920. The sample excludes those aged less than 18 in each census year, and women. The dependent variable is an indicator returning a value of one if the individual has obtained at least one patent over a ten-year window centered in the given census decade, and zero otherwise. For concreteness, an individual in the 1910 census is flagged as an inventor if he has obtained at least one patent between 1905 and 1909. In columns (1)–(3) we report unconditional correlations for the whole sample (column 1), the white (column 2), and the Black (column 3) populations. In columns (4)–(8), we include individual-level controls and fixed effects incrementally. Columns (4) includes county fixed effects; column (5) adds cohort-level fixed effects to control for time-varying aggregate unobserved heterogeneity; in column (6) we add race fixed effects since the sample includes both white and Black populations; in column (7) we include literacy and occupation fixed effects as additional individual-level controls; in column (8) we finally add surname fixed effects to exploit variation in the BNI holding constant the informational content of surnames. Standard errors are clustered at the name level and are displayed in parentheses.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

TABLE III: NAME-LEVEL EFFECT OF LYNCHING ON INNOVATION

	Baseline		Race of Lynching Victim:		Race of Inventor:		High-Quality	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			White	Black	White	Black	Volume	Share
Lynching × Post	-0.030***	-0.041***			-0.024**	-0.028	-0.002***	0.810**
	(0.009)	(0.010)			(0.010)	(0.021)	(0.001)	(0.354)
White Lynching × Post			-0.013					
			(0.020)					
Black Lynching × Post				-0.034***				
				(0.009)				
Name FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All	All	All	All	White	Black	All	All
Weight	–	Quality	–	–	–	–	–	–
Number of Names	1235	1235	1265	1250	1028	171	1235	1235
Observations	38285	37696	39215	38750	31868	5301	38285	22821
R^2	0.220	0.241	0.220	0.220	0.204	0.244	0.087	0.106
Mean Dep. Var.	0.143	0.153	0.141	0.142	0.125	0.062	0.004	2.390
Std. Beta Coef.	-0.024	-0.030	-0.005	-0.026	-0.020	-0.032	-0.017	0.030

Notes. This Table reports the name-level effect of lynching on innovation. The unit of observation is a first name, which we observe at a yearly frequency between 1895 and 1925. The time variable is the issue year of each patent. The dependent variable is the number of patents granted to inventors with a given name, normalized by the number of individuals with that name in the 1880 census. For a given name, the treatment variable returns a value of one after someone with that name appears as the victim of a lynching episode, and zero otherwise. The timing when the treatment is activated is name-dependent. Column (1) reports the baseline specification. In column (2) we weight each patent by its quality score computed following Kelly *et al.* (2021). In columns (3) and (4) we restrict the sample of lynchings by the race of the victim. In column (3), the treatment is equal to one after someone with a given name appears as the victim of a lynching episode only if the victim is white; the treatment in column (4) is defined analogously for lynching episodes against Black people. In column (5) we restrict the sample to white inventors, which make up approximately 95% of the entire population; in column (6) we restrict the sample to include Black individuals only. In column (7) the outcome variable is defined as the (logarithm of the) share of high-quality patents relative to the number of people by name; in column (8) the dependent variable is the share of high-quality patents relative to the total number of patents per name-year. High-quality patents are defined as those in the top 5% of the distribution of the quality indicator described in Kelly *et al.* (2021). All regressions include name- and year-fixed effects. Standard errors are clustered at the name level and are displayed in parentheses.
*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

TABLE IV: INVENTOR-LEVEL EFFECT OF LYNCHING ON INNOVATION

	Patents				1 (Patents > 0)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lynching × Post	-0.098***	-0.096***	-0.097***	-0.123	-0.110***	-0.105***	-0.109***	-0.111**
	(0.035)	(0.029)	(0.034)	(0.093)	(0.024)	(0.021)	(0.024)	(0.043)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Surname-Year FE	No	Yes	No	No	No	Yes	No	No
Sample	All	All	Whites	Blacks	All	All	Whites	Blacks
Number of Inventors	125034	97358	119931	3810	125034	97358	119931	3810
Observations	1276387	993086	1224701	38473	1276387	993086	1224701	38473
R^2	0.325	0.480	0.326	0.388	0.148	0.342	0.148	0.282
Mean Dep. Var.	0.193	0.193	0.195	0.164	0.147	0.147	0.147	0.131
Std. Beta Coef.	-0.031	-0.030	-0.031	-0.030	-0.064	-0.061	-0.064	-0.056

Notes. This Table reports the inventor-level effect of lynching on innovation. The unit of observation is an inventor, who we observe at a yearly frequency. We observe each inventor over a ten-year window around one census year. For concreteness, if an inventor has patents filed between 1895 and 1904, he shall be linked to the 1900 census and we shall observe his innovation activity between 1895 and 1904. The timing variable is thus the issue year of each patent. In columns (1)–(4) the dependent variable is the number of patents that the inventor files in a given year. In columns (5)–(8) the dependent variable is an indicator returning a value of one if the inventor has at least one patent in a given year, and zero otherwise. For a given inventor, the treatment variable returns a value of one after someone with the same name as the inventor appears as the victim of a lynching episode, and zero otherwise. The timing when the treatment is activated is name-dependent. The sample comprises all those inventors who file one patent both before and after their name are treated, and those whose name is never lynched. We thus drop inventors that are "always treated" within the observation sample and those who obtain only one patent. In columns (1), (3), (4), (5), (7), and (8) the model includes individual and county-by-year fixed effects. In columns (2) and (6) we further add surname-by-year fixed effects to leverage variation among inventors with the same surname but different first names. In columns (3) and (7) we restrict the sample to include only white inventors, whereas in columns (4) and (8) we only include Black inventors. Standard errors are clustered by name and are reported in parentheses.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

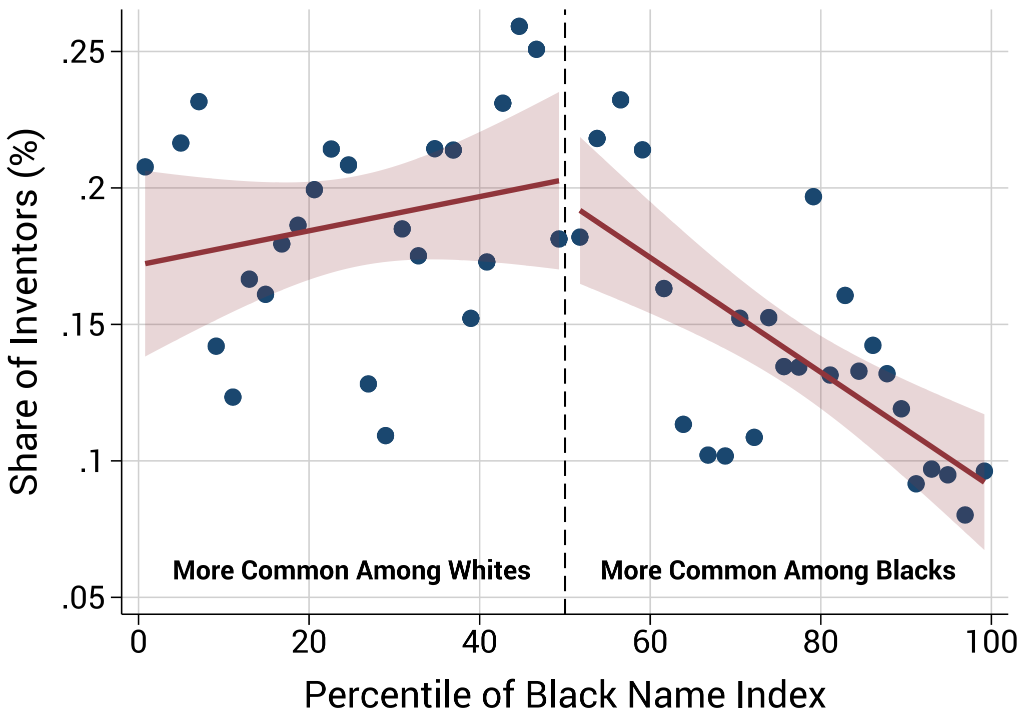
Table V: Effect of Distance from Lynching on Innovation

	Patents					1 (Patents > 0)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	< 300 km	< 500 km	< 700 km	Distance (cont.)	Distance (discrete)	< 300 km	< 500 km	< 700 km	Distance (cont.)	Distance (discrete)
Lynching × Post				-0.089^{***}	-0.054^{***}				-0.124^{***}	-0.091^{***}
				(0.022)	(0.020)				(0.026)	(0.024)
$\text{Lynching}_{<300\text{km}}$ × Post	-0.021^{*}					-0.030^{**}				
	(0.011)					(0.013)				
$\text{Lynching}_{<500\text{km}}$ × Post		-0.041^{***}					-0.059^{***}			
		(0.010)					(0.012)			
$\text{Lynching}_{<700\text{km}}$ × Post			-0.052^{***}					-0.075^{***}		
			(0.014)					(0.016)		
Lynching × Post × Distance				0.000					0.000	
				(0.000)					(0.000)	
Lynching × Post × k-Quintile of Distance (w.r.t $k = 3$)										
$k = 1$					-0.060^{***}					-0.057^{**}
					(0.021)					(0.024)
$k = 2$					-0.017					-0.012
					(0.018)					(0.020)
$k = 4$					-0.004					0.009
					(0.016)					(0.016)
$k = 5$					-0.031^{**}					-0.027^{*}
					(0.014)					(0.016)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Inventors	468954	426115	370151	138211	138211	468954	426115	370151	138211	138211
Observations	4815525	4370275	3791256	1411776	1411776	4815525	4370275	3791256	1411776	1411776
R^2	0.141	0.147	0.154	0.179	0.179	0.078	0.081	0.085	0.099	0.099
Mean Dep. Var.	0.104	0.106	0.108	0.115	0.115	0.135	0.136	0.138	0.145	0.145

Notes. This Table reports the inventor-level effect of lynching on innovation, varying the distance between the inventor and the lynching. The unit of observation is an inventor, who we observe at a yearly frequency. The definition of the sample is analogous to that in Table IV. In columns (1)–(5) the dependent variable is the number of patents that the inventor files in a given year. In columns (6)–(10) the dependent variable is an indicator returning a value of one if the inventor has at least one patent in a given year, and zero otherwise. In columns (1)–(3) and (6)–(8) we consider lynching episodes that are closer than, respectively, 300, 500, and 700 kilometers from the inventor. In columns (4) and (9) we add an interaction between the baseline treatment and the distance from the closest lynching. In columns (5) and (10) we interact the baseline treatment with the quintiles of the distribution of the distance between the inventor and the closest lynching, setting the third quintile as the baseline category. All regressions include name- and year-fixed effects. Standard errors are reported in parentheses and are clustered at the name level. *: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

Figures

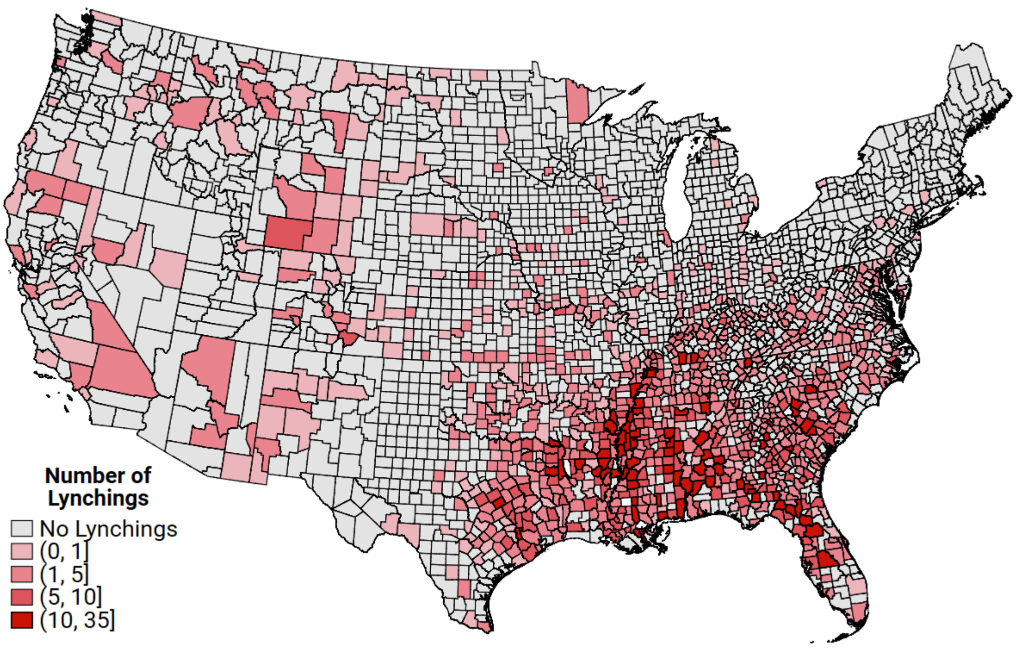
FIGURE I: CORRELATION BETWEEN INVENTOR STATUS AND BLACK NAME INDEX



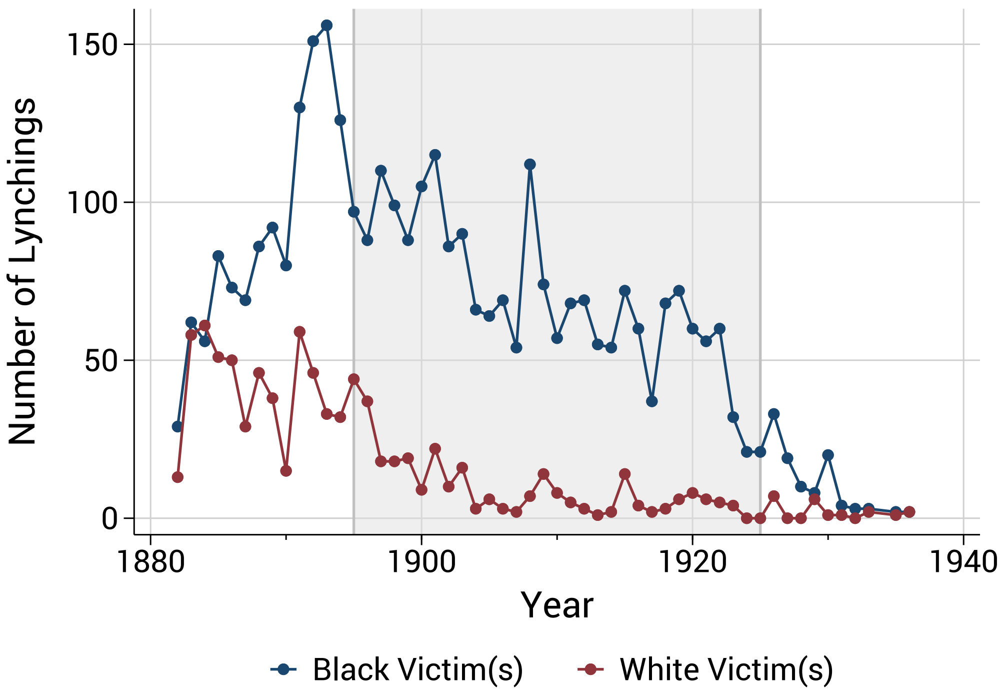
Notes. This Figure reports the correlation between the Black Name Index (BNI) and the share of inventors in the population, in percentage points. To construct the sample, we stack full-count population census data between 1900 and 1920. An individual in census t is flagged as an inventor if he has at least one patent filed between $t-5$ and $t+4$. In each census, we drop women and all men younger than 18 years. The unit of observation is a county, for which we observe the share of inventors among the population by birth year and percentile of the BNI. The Figure reports the associated binned scatter plot, partialling out county-by-cohort fixed effects. The dashed black line separates the bottom 50% of names that are more common among white people, from those that are more common among Black people. The red line superimposes the fitted values of a linear regression, and the associated 95% confidence bands, where standard errors are clustered at the county level.

Figure II: Spatial and Time Distribution of Lynching Episodes

(a) Spatial Distribution of Lynching Episodes

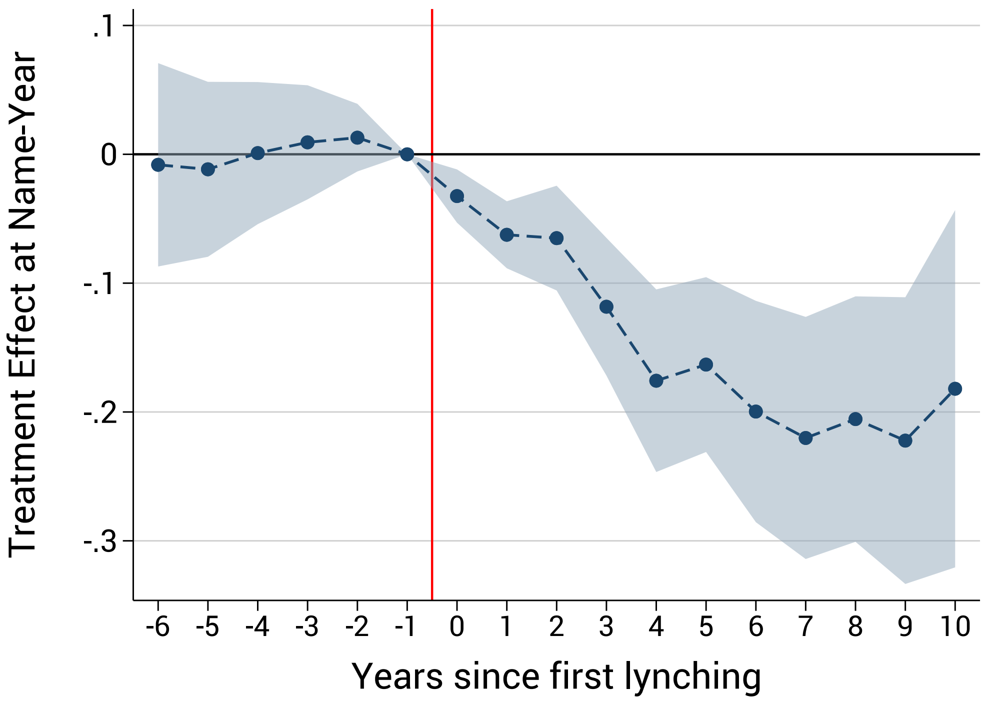


(b) Time Series of Lynching Episodes



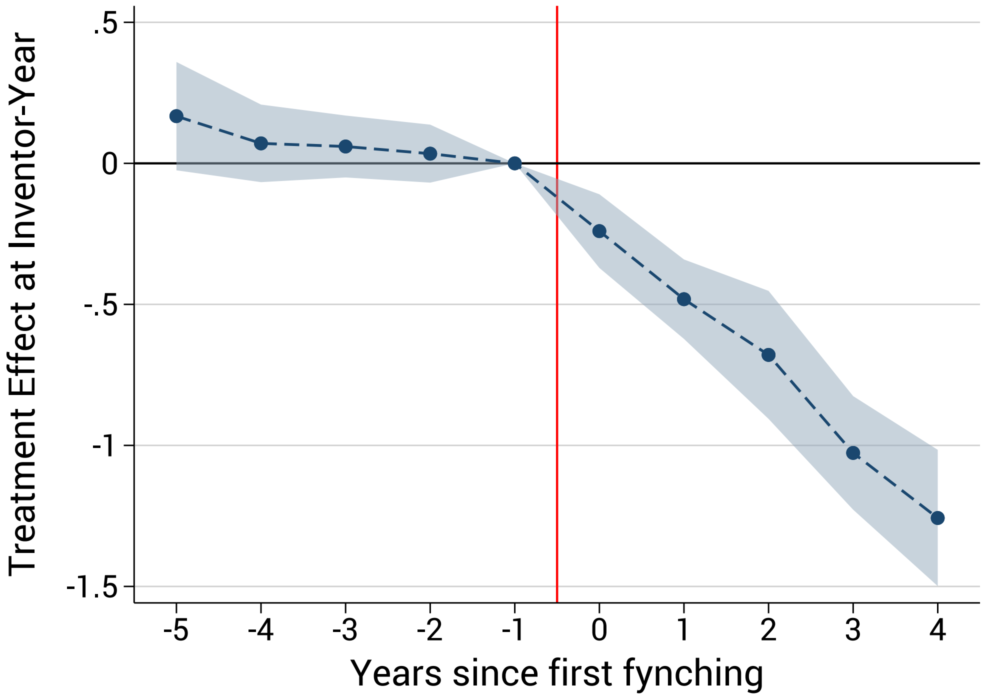
Notes. Panel IIa displays the spatial distribution of lynching episodes in the United States, over the period 1882-1935, by county. Panel IIb reports the time series of lynching episodes over the same period, broken down by the reported race of the victim. The shaded area is the sample of our analysis. Data are from Hines and Steelwater (2006) and Seguin and Rigby (2019).

FIGURE III: NAME-LEVEL EFFECT OF LYNCHING ON INNOVATION



Notes. The Figure reports the name-level effect of lynching on innovation. The unit of observation is a first name, which we observe at a yearly frequency between 1895 and 1925. The time variable is the issue year of each patent. The dependent variable is the number of patents granted to inventors with a given name, normalized by the number of individuals with that name in the 1880 census. For a given name, the treatment variable returns a value of one after someone with that name appears as the victim of a lynching episode, and zero otherwise. The timing when the treatment is activated is name-dependent. Each dot reports the dynamic treatment effects associated with model (2), and the related 95% confidence bands, where standard errors are clustered at the name level. Under parallel trends, we expect treatment effects before the first treatment period not to be statistically different from zero.

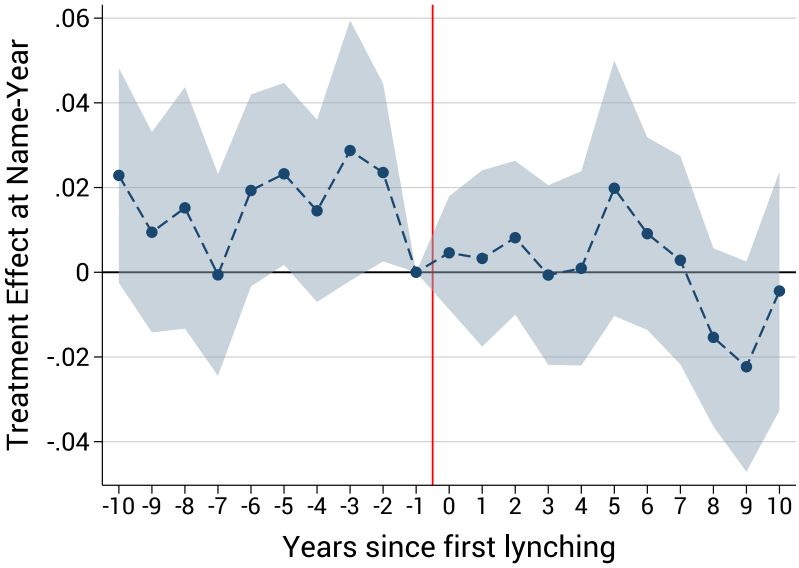
FIGURE IV: INVENTOR-LEVEL EFFECT OF LYNCHING ON INNOVATION



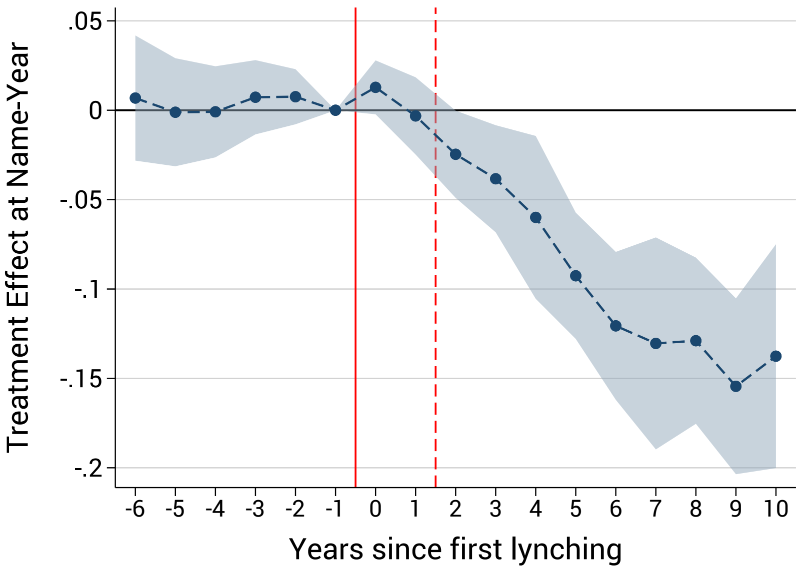
Notes. The Figure reports the inventor-level effect of lynching on innovation. The unit of observation is an inventor, who we observe at a yearly frequency. We observe each inventor over a ten-year window around one census year. For concreteness, if an inventor has patents filed between 1895 and 1904, he shall be linked to the 1900 census and we shall observe his innovation activity between 1895 and 1904. The timing variable is thus the issue year of each patent. The dependent variable is the number of patents that the inventor files in a given year. For a given inventor, the treatment variable returns a value of one after someone with the same name as the inventor appears as the victim of a lynching episode, and zero otherwise. The timing when the treatment is activated is name-dependent, and the treatment is interacted with time dummies. The sample comprises all those inventors who file one patent both before and after their name is treated, and those whose name is never lynched. We thus drop inventors that are "always treated" within the observation sample, and those who obtain only one patent. The Figure reports the estimated dynamic treatment effects associated with regression (3). The model includes individual, county-by-year, and surname-by-year fixed effects. Dots report the point estimates, and the blue bands report their 95% confidence intervals, where standard errors are clustered at the name level.

Figure V: Evidence on the Mechanisms – Violence and Discrimination

(a) Effect on Unambiguously White Names



(b) Effect on Filing Year



Notes. Figures report the name-level effect of lynching on innovation. The unit of observation is a first name, which we observe at a yearly frequency between 1895 and 1925. The dependent variable is the number of patents granted to inventors with a given name, normalized by the number of individuals with that name in the 1880 census. For a given name, the treatment variable returns a value of one after someone with that name appears as the victim of a lynching episode, and zero otherwise. The timing when the treatment is activated is name-dependent. In Panel Va the time variable is the filing year, and we restrict the sample to names with a BNI below .35, which we label as unambiguously white. In Panel Vb the time variable is the *filing* year. The solid red line denotes the timing of the first lynching, and the dashed red line indicates the average waiting time between the filing of an application and the issuance of a patent. Dots report the point estimates, and the blue bands report their 95% confidence intervals, where standard errors are clustered at the name level.

Online Appendix

Racial Discrimination and Innovation

Evidence from US Inventors, 1895–1925

Davide M. Coluccia, Gaia Dossi, and Sebastian Ottinger

March, 2023

A Data Sources and Description

A.1 Data Sources

We leverage information from three main sources. Enrico Berkes graciously granted us access to his Comprehensive Universe of US Patents (Berkes, 2018). Lynching data are compiled from two sources, Hines and Steelwater (2006) and Seguin and Rigby (2019). Finally, de-anonymized individual-level census data are from the IPUMS project (Ruggles *et al.*, 2021). In this section, we briefly discuss the first two data sources, although we encourage the interested reader to refer to the cited references for a better and more detailed description of the data.[24]

A.1.1 Patent Data

Patent data are from Berkes (2018). He digitizes the universe of patents granted by the United States Patent Office (USPTO) over the period 1836-2010 from original documents. When comparing alternative datasets of historical US patents, Andrews concludes that "CUSP is currently the gold standard both in terms of completeness and scope of the types of patent information it contains" (Andrews, 2021, p. 391). In our analysis, we focus on the period 1895–1925, and we exploit the linking between patents and historical US population censuses provided by Berkes. This is key in our analysis because it allows us to observe individual demographic and socioeconomic characteristics of inventors. These variables, which are typically not observed in modern settings, enable a deeper analysis of the stylized fact that we document. For instance, Fryer and Levitt (2004) argues that once one controls for proxies of income status and occupation as well as geographic locations, the negative correlation between the BNI and later-life outcome variables among Black individuals disappears. The linked inventor sample allows us to largely dismiss Fryer and Levitt's critique.

To construct the linked inventors' sample, Bazzi *et al.* (2022) qualitatively proceed as follows:[25]

1. Consider patent p, filed in year y_p, with inventors $i_j^p \in \mathcal{I}^p = \{i_1^p, \dots, i_N^p\}$. In most cases, in our setting $|\mathcal{I}| = 1$. For simplicity, assume solo-authored patents with inventor labeled i^p;
2. Set a threshold of τ kilometers around the geo-coded coordinates x_i of i^p. Consider the closest census to y_p. Let N_i^τ be the set of individuals in that census that live inside the circle centered in x_i and of radius τ;
3. Compute, for every $n_i^\tau \in N_i^\tau$, a similarity distance $\sigma_{n_i^\tau, i^p}$ between the name and surname of n_i^τ, and i^p. Set a threshold α: if $\sigma_{n_i^\tau, i^p} > \alpha$, keep the match. Otherwise, discard it.

[24] We do not describe census micro-data here because (i) many researchers are probably familiar with the IPUMS project, and (ii) several resources are readily available on the IPUMS website.

[25] Please note that this is a *qualitative* description of the matching algorithm. We refer the interested reader to Berkes (2018) and Bazzi *et al.* (2022) for a technical description of the procedure.

4. If the set of matches with $\sigma_{n_i^\tau, i^p} > \alpha$ is not empty, keep the sample of matches as is. Otherwise, increase the threshold τ and repeat steps 2–3.

This procedure yields, for each inventor, a set of possible matches to the census. In our application, we only keep inventors with less than 5 matches and weight each match by the inverse of the total number of matches. The interested reader is encouraged to refer to Berkes (2018) for a more detailed discussion of the dataset, and to Bazzi *et al.* (2022) for details on the linking algorithm, which we only sketch here, and related statistics.

A.1.2 Lynching Data

We assemble lynching data from Hines and Steelwater (2006) and Seguin and Rigby (2019). Both datasets rely on historical newspapers as their primary source. These data report, among other variables, the county where the lynching was perpetrated, the day, month, and year of each episode, the race of the victim, the alleged crime he or she committed (although this variable is missing for many instances), and the first and last name of the victim. We augment these data by geo-coding each lynching to the centroid of the county where it was perpetrated to compute – approximate – geodesic distances between inventors and lynching episodes.

Cook (2012) discusses the quality and coverage of available lynching data. In particular, Hines and Steelwater (2006) focuses on lynching episodes that occurred in Southern states. Seguin and Rigby (2019) includes the rest of the country, where many more non-black lynchings were committed.

A.1.3 Consistent County Borders

All data come at historical county borders. By the 1890s, most county geographies had become consolidated. However, Western counties still underwent some major boundary redrawing. This is unlikely to drive our results, for the vast majority of inventors resided in Midwest and Eastern States. However, to ensure consistency we consolidate the geography at 1900-county borders following the simple method proposed by Eckert *et al.* (2020).

A.2 Additional Tables

TABLE A.1: SAMPLE OF ESTIMATED BLACK NAME AND SURNAME INDICES

First Name (1)	BNI (2)	Surname (3)	BSI (4)
Panel A. Unambiguously Black-Sounding			
Dilsy	.995	Coaxum	.9986
Dilsey	.9933	Ginyard	.9976
Stepney	.9922	Mayweather	.9971
Primus	.9906	President	.9967
Panel B. Suggestively Black-Sounding			
Hampton	.7999	Moseley	.8000
Eliah	.7999	Lathan	.8000
Florida	.7998	Mcleon	.7998
Vandora	.7997	Niblet	.7998
Panel C. Suggestively White-Sounding			
Helma	.1999	Wardin	.1999
Orissa	.1997	Teas	.1999
Alvi	.1997	Grapp	.1999
Thursey	.1996	De leon	.1999
Panel D. Unambiguously White-Sounding			
Stanislawa	.0000	Schoover	.0000
Boleslaw	.0000	Ehlers	.0000
Stefania	.0000	Lundberg	.0000
Wladyslaw	.0000	Schlosser	.0000

Notes. This Table reports a set of estimated Black Name and Surname Index. In panel A, we report a set of unambiguously black names featuring BNI and BSI above .99. In panel B, we report names and surnames with a BNI and BSI around .8, which qualitatively means that they are four times more common among black individuals than among white ones. Panel C repeats this exercise, but on the opposite shows a set of names and surnames which are approximately four times more common among white individuals. Finally, in panel D we report names and surnames which are unambiguously white because in the entire population no black individual bears them.

TABLE A.2: SAMPLE STATISTICS ON THE DELAY BETWEEN APPLICATION AND ISSUE YEAR OF PATENTS, 1895–1925

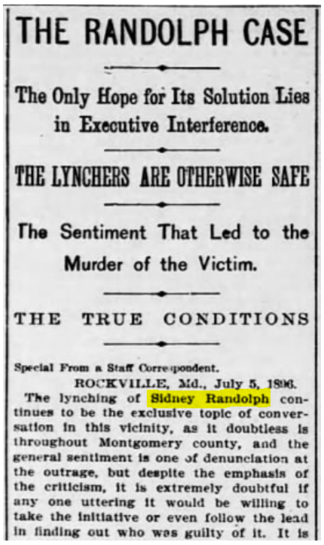
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sample	Mean	Std. Dev.	Min.	Max.	Observations	50^{th} Pct.	75^{th} Pct.
Panel A. Full Sample							
Baseline Sample	1.700	1.634	0	30	1110241	1	2
Drop top 1% Delay Time	1.620	1.431	0	7	1098616	1	2
Drop top 5% Delay Time	1.389	1.078	0	4	1038552	1	2
Panel B. Patents Adjusted for Quality							
Quality-Weighed	1.746	1.687	0	30	1109411	1	2
Top 20% Quality	2.347	2.201	0	30	189452	2	3
Top 5% Quality	2.856	2.571	0	30	37389	2	4

Notes. This Table reports a set of sample statistics on the delay between the issue and the filing year of patents filed between 1895 and 1925. Note that the filing year is missing for approximately 5% of the total sample. The delay is defined as the simple difference between the issue and the filing year. Column (1) reports the average delay; column (2) reports the standard deviation of the delay; in columns (3) and (4) we display the minimum and maximum values; column (5) reports the total numerosity of the sample; columns (6) and (7) report, respectively, the 50^{th} and 75^{th} percentiles. Panel A refers to the full sample of patents: in the second and third rows, we drop, respectively, patents in the top 1% and 5% distribution of the delay time. Panel B adjusts each patent by its quality measure, as computed in Kelly *et al.* (2021). In particular, in the second and third rows, we keep only patents in the top 20% and 5% of the quality distribution.

A.3 Additional Figures

FIGURE A.1: EXAMPLE OF LYNCHING NEWSPAPER COVERAGE IN DC

(A) EVENING STAR (DC),
JULY 1896



THE RANDOLPH CASE

The Only Hope for Its Solution Lies in Executive Interference.

THE LYNCHERS ARE OTHERWISE SAFE

The Sentiment That Led to the Murder of the Victim.

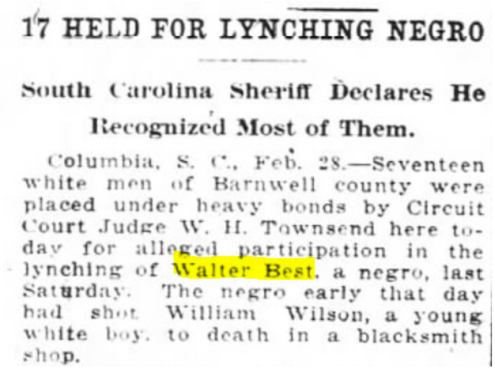
THE TRUE CONDITIONS

Special From a Staff Correspondent.

ROCKVILLE, Md., July 5, 1896.

The lynching of Sidney Randolph continues to be the exclusive topic of conversation in this vicinity, as it doubtless is throughout Montgomery county, and the general sentiment is one of denunciation at the outrage, but despite the emphasis of the criticism, it is extremely doubtful if any one uttering it would be willing to take the initiative or even follow the lead in finding out who was guilty of it. It is

(B) WASHINGTON POST (DC),
MARCH 1918



17 HELD FOR LYNCHING NEGRO

South Carolina Sheriff Declares He Recognized Most of Them.

Columbia, S. C., Feb. 28.—Seventeen white men of Barnwell county were placed under heavy bonds by Circuit Court Judge W. H. Townsend here today for alleged participation in the lynching of Walter Best, a negro, last Saturday. The negro early that day had shot William Wilson, a young white boy, to death in a blacksmith shop.

Notes. Examples of newspapers in Washington D.C. reporting the name or race of the victim. Both newspapers were based in Washington, DC, at the time of the lynching.

SOURCE: Author's query on the newspapers.com archive.

FIGURE A.2: EXAMPLE OF LYNCHING COVERAGE ACROSS STATES

(A) THE CHICAGO DAILY TRIBUNE, CHICAGO (IL), SEPTEMBER 1935

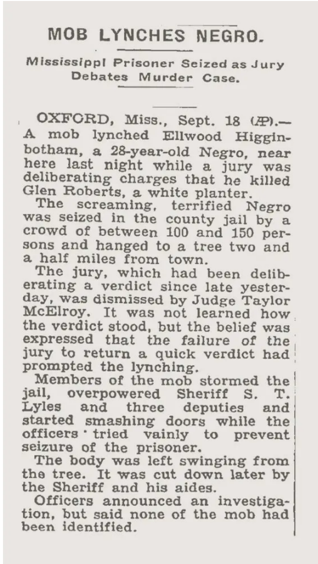
DEATH SUSPECT IS LYNCHED AS JURY DEBATES

Colored Defendant, 28, Dragged from Cell.

Oxford, Miss., Sept. 18.—[Special.]—Judge Taylor McElroy today called before him and dismissed the jury that had been deliberating the case of Ellwood Higginbotham, 28, colored, who was charged with slaying a white planter. The services of the jury were no longer needed, for the defendant was lynched last night by a mob.

Higginbotham had been on trial for killing Glen Roberts on May 21. Witnesses said the planter was killed in "cold blood" when he went to the

(B) THE NEW YORK TIMES, NEW YORK CITY (NY), SEPTEMBER 1935



MOB LYNCHES NEGRO.

Mississippi Prisoner Seized as Jury Debates Murder Case.

OXFORD, Miss., Sept. 18 (AP).—A mob lynched Ellwood Higginbotham, a 28-year-old Negro, near here last night while a jury was deliberating charges that he killed Glen Roberts, a white planter.

The screaming, terrified Negro was seized in the county jail by a crowd of between 100 and 150 persons and hanged to a tree two and a half miles from town.

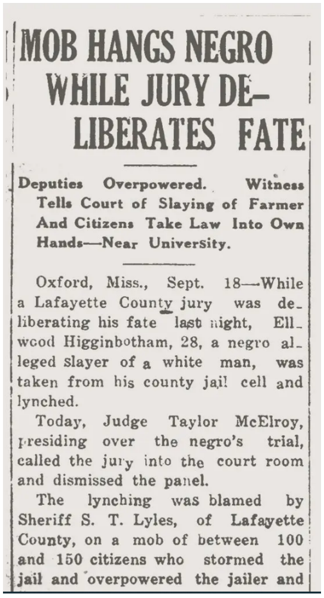
The jury, which had been deliberating a verdict since late yesterday, was dismissed by Judge Taylor McElroy. It was not learned how the verdict stood, but the belief was expressed that the failure of the jury to return a quick verdict had prompted the lynching.

Members of the mob stormed the jail, overpowered Sheriff S. T. Lyles and three deputies and started smashing doors while the officers tried vainly to prevent seizure of the prisoner.

The body was left swinging from the tree. It was cut down later by the Sheriff and his aides.

Officers announced an investigation, but said none of the mob had been identified.

(C) THE ATLANTA DAILY WORLD, ATLANTA (GA), SEPTEMBER 1935



MOB HANGS NEGRO WHILE JURY DELIBERATES FATE

Deputies Overpowered. Witness Tells Court of Slaying of Farmer And Citizens Take Law Into Own Hands—Near University.

Oxford, Miss., Sept. 18—While a Lafayette County jury was deliberating his fate last night, Ellwood Higginbotham, 28, a negro alleged slayer of a white man, was taken from his county jail cell and lynched.

Today, Judge Taylor McElroy, presiding over the negro's trial, called the jury into the court room and dismissed the panel.

The lynching was blamed by Sheriff S. T. Lyles, of Lafayette County, on a mob of between 100 and 150 citizens who stormed the jail and overpowered the jailer and

(D) THE NORTH MISSISSIPPI HERALD, WATER VALLEY (MS), SEPTEMBER 1935

Mob Lynches Man At End Of Trial

Certain Sentence Is No Deterrent For Unmasked Gang

JURY IS OUT

OXFORD, Miss.—(ANP)—Another Negro has been lynched in Mississippi and again whatever investigation is made will mean naught.

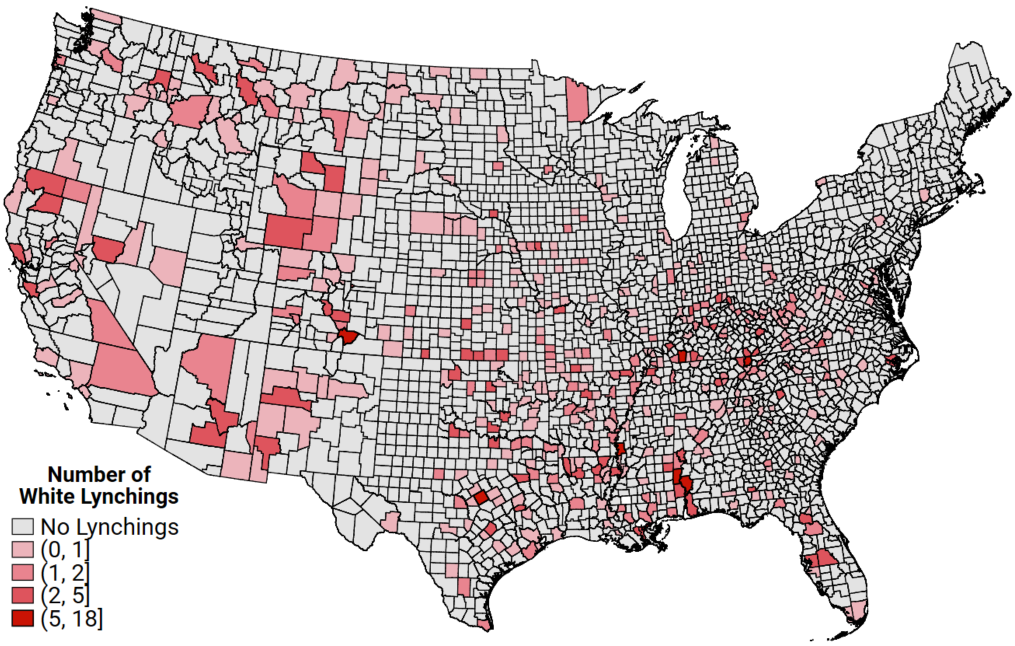
Elwood Higginbotham was the victim and the lynching occurred Tuesday night as the jury deliberated his fate on a charge of murdering a white man. The procedure was as usual: a group of forty or fifty unmasked white men "overpowered" the sheriff and his THREE deputies removed the prisoner from the jail

Notes. Examples of coverage of lynching episodes across states. All articles are related to one single lynching, which resulted in the death of the victim, Elwood Higginbotham, in September 1935. The lynching took place in Lafayette county, Missouri. These attest to the wide coverage that lynchings gathered across the entire United States, possibly even far away from the place where they were perpetrated.

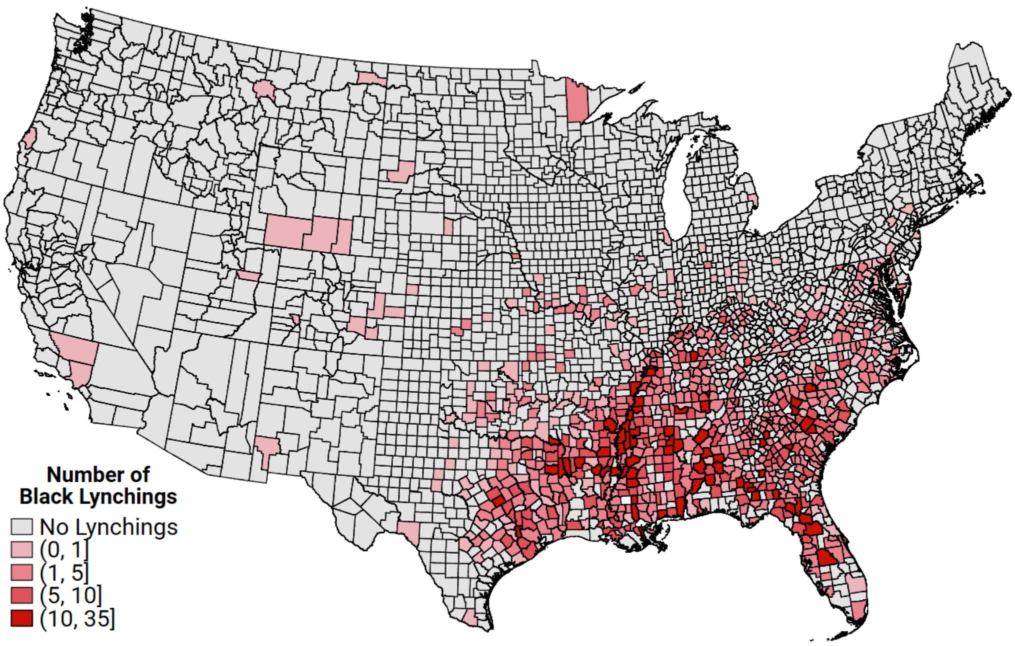
SOURCE: The New York Times, April 2018.

FIGURE A.3: SPATIAL DISTRIBUTION OF WHITE AND BLACK LYNCHING EPISODES

(A) DISTRIBUTION OF WHITE LYNCHING EPISODES



(B) DISTRIBUTION OF BLACK LYNCHING EPISODES

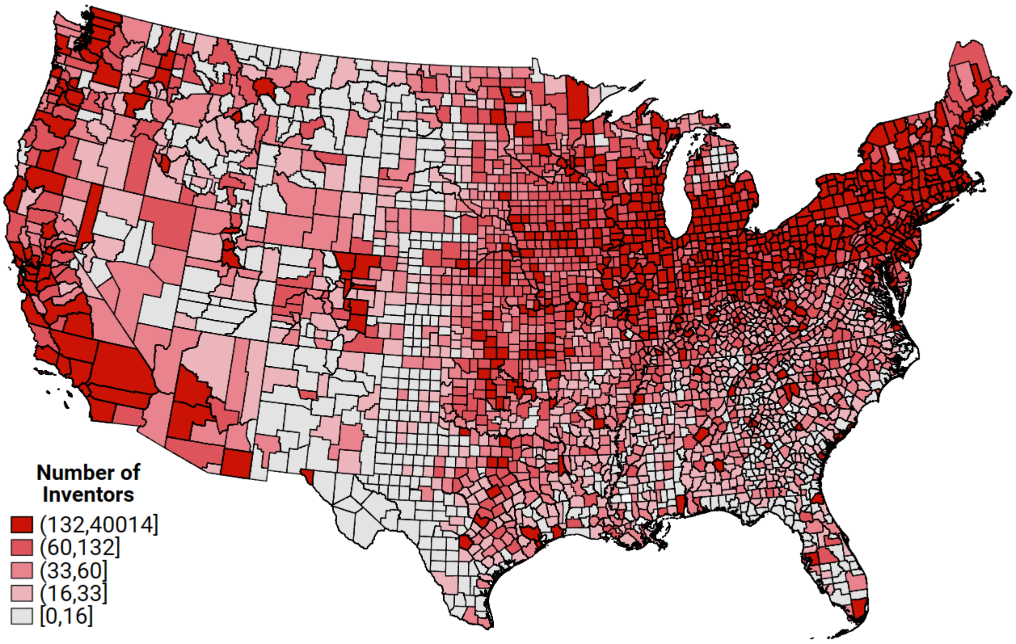


Notes. Panel A.3a (resp. A.3b) displays the spatial distribution of lynching episodes against reportedly white (resp. Black) individuals in the United States, over the period 1882-1935, by county.

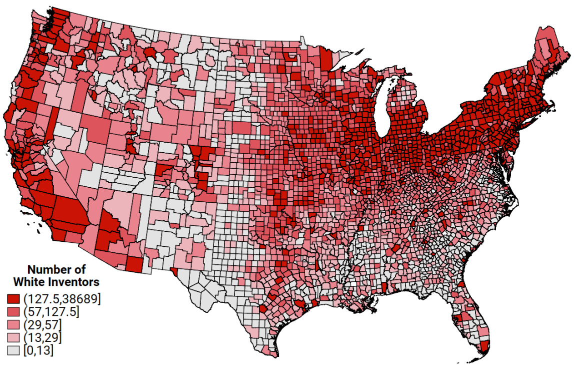
SOURCE: authors' calculations from data from Hines and Steelwater (2006) and Seguin and Rigby (2019).

Figure A.4: Spatial Distribution of Inventors, also by Race

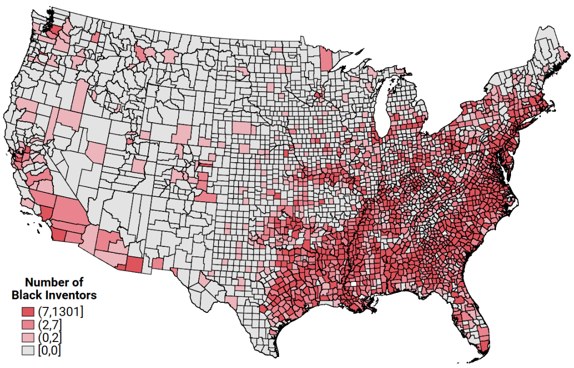
(a) Distribution of All Inventors



(b) Distribution of White Inventors



(c) Distribution of Black Inventors

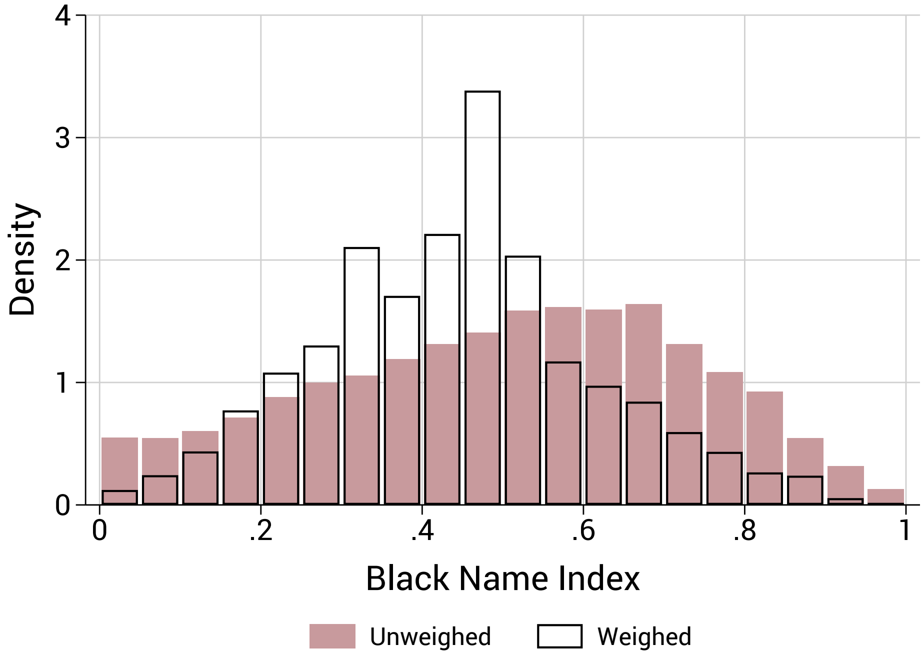


Notes. The Figure reports the spatial distribution of inventors (Panel A.4a), white inventors (A.4b) and Black inventors (A.4c). Using patent data from the CUSP (Berkes, 2018) linked to individual census records, we obtain the race of each inventor, whose coordinates are retrieved from the patent documents and overlaid on a 1920-map of counties.

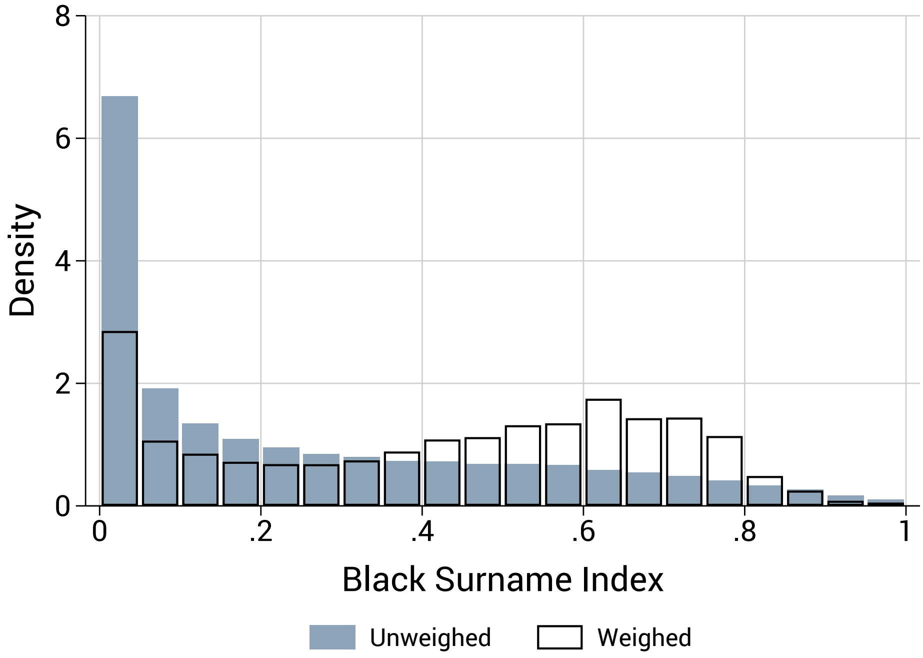
Source: authors' elaboration on data from Berkes (2018) and Ruggles *et al.* (2021).

FIGURE A.5: DISTRIBUTION OF BLACK NAME AND SURNAME INDEX

(A) DISTRIBUTION OF BLACK NAME INDEX



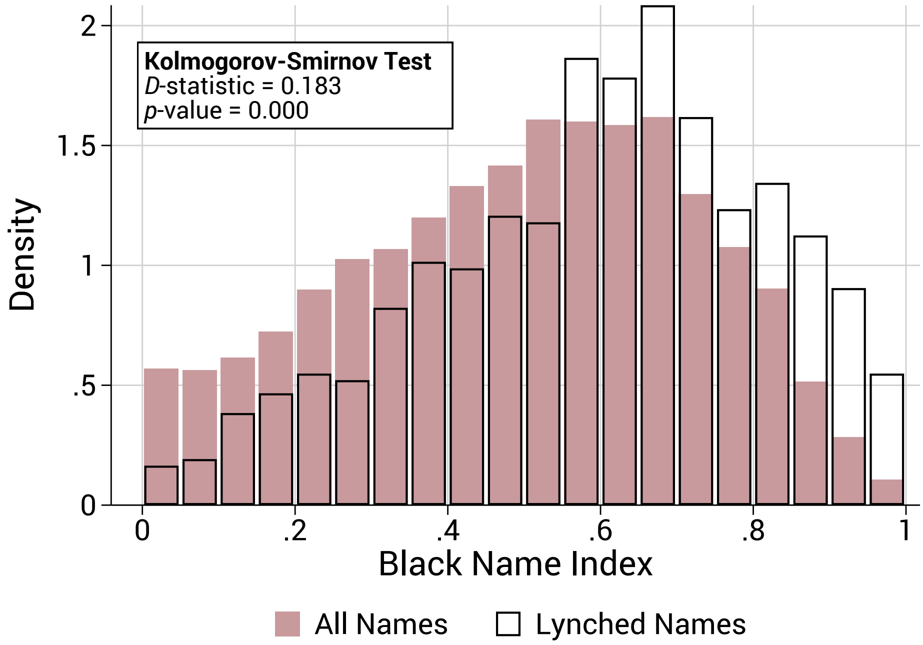
(B) DISTRIBUTION OF BLACK SURNAME INDEX



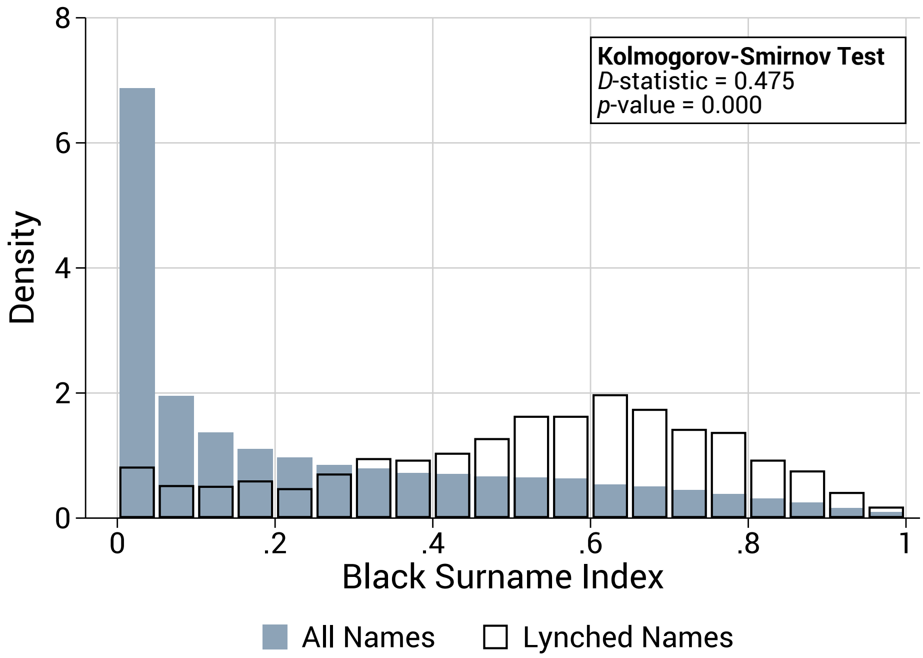
Notes. The Figure reports the distribution of the Black Name Index (Panel A.5a) and of the Black Surname Index (Panel A.5b). The colored bars report the raw distribution of the two indices. The black-contoured bars report the distribution weighting each name by the number of people carrying it, as recorded in the 1880 full-count census. Thus, the colored bars display the estimated BNI and BSI, whereas the contoured bars show the distribution of those scores within the population.

Figure A.6: Distribution of the Black Name and Surname Index of Victims of Lynching Episodes

(a) Distribution of Black Name Index

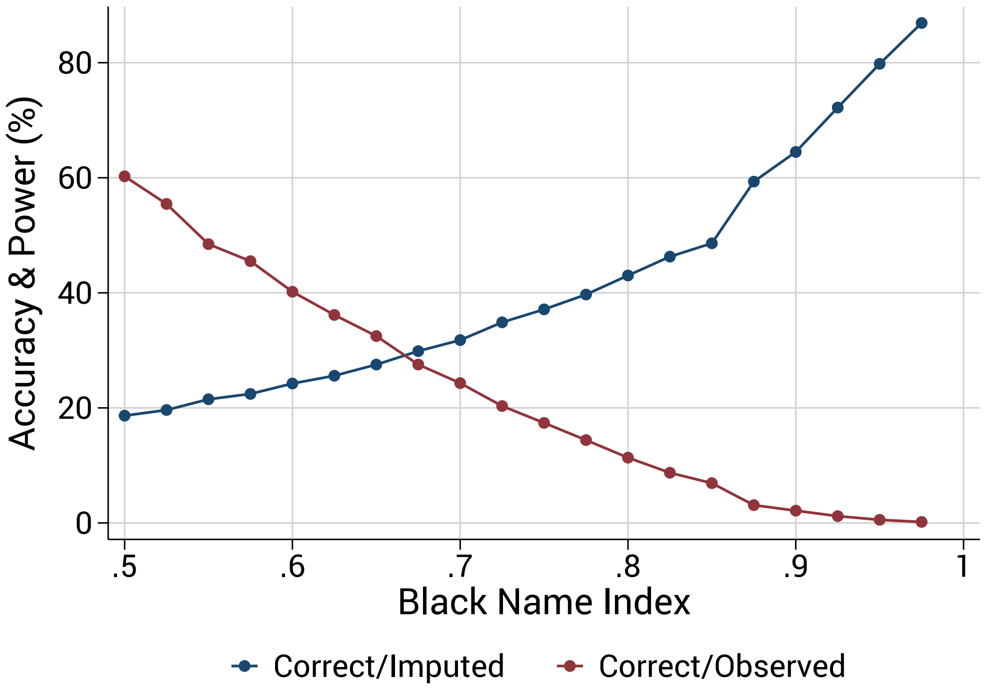


(b) Distribution of Black Surname Index



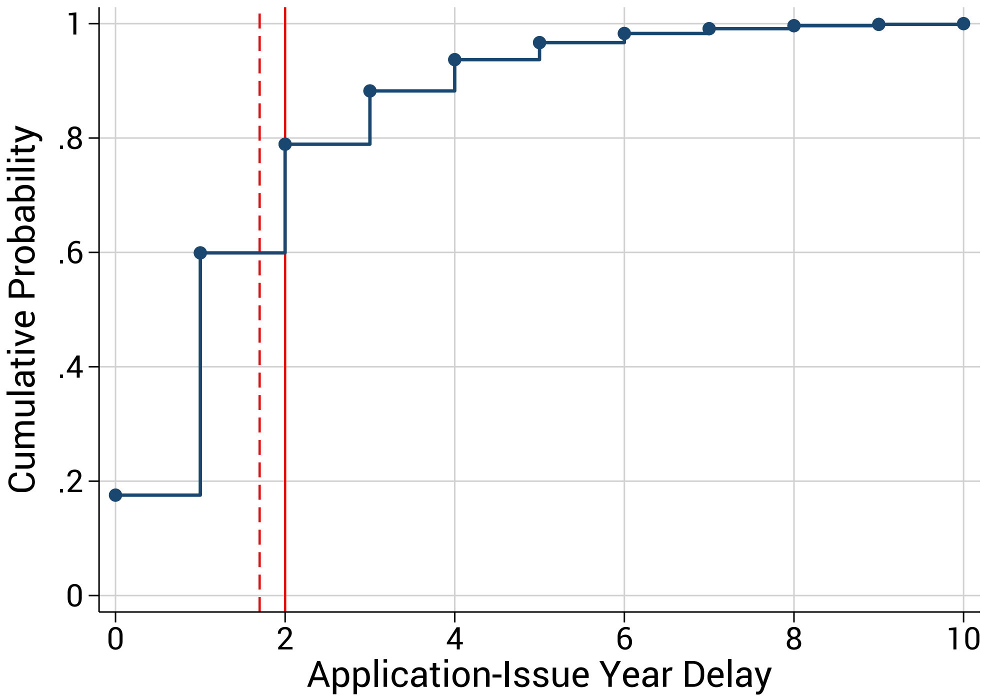
Notes. The Figure compares the distribution of the Black Name Index (Panel A.6a) and of the Black Surname Index (Panel A.6b) of victims of lynching episodes *vis-à-vis* the entire population. The colored bars report the BNI and BSI of all names; the black-contoured bars report the BNI and the BSI of victims of lynching episodes. Each panel further reports the *D*-statistics and associated *p*-value of a Kolmogorov-Smirnov test of equality of the two distributions. A low *p*-value indicates that we cannot reject the null that the two distributions are different.

FIGURE A.7: Accuracy and Efficiency of the Black Name Index



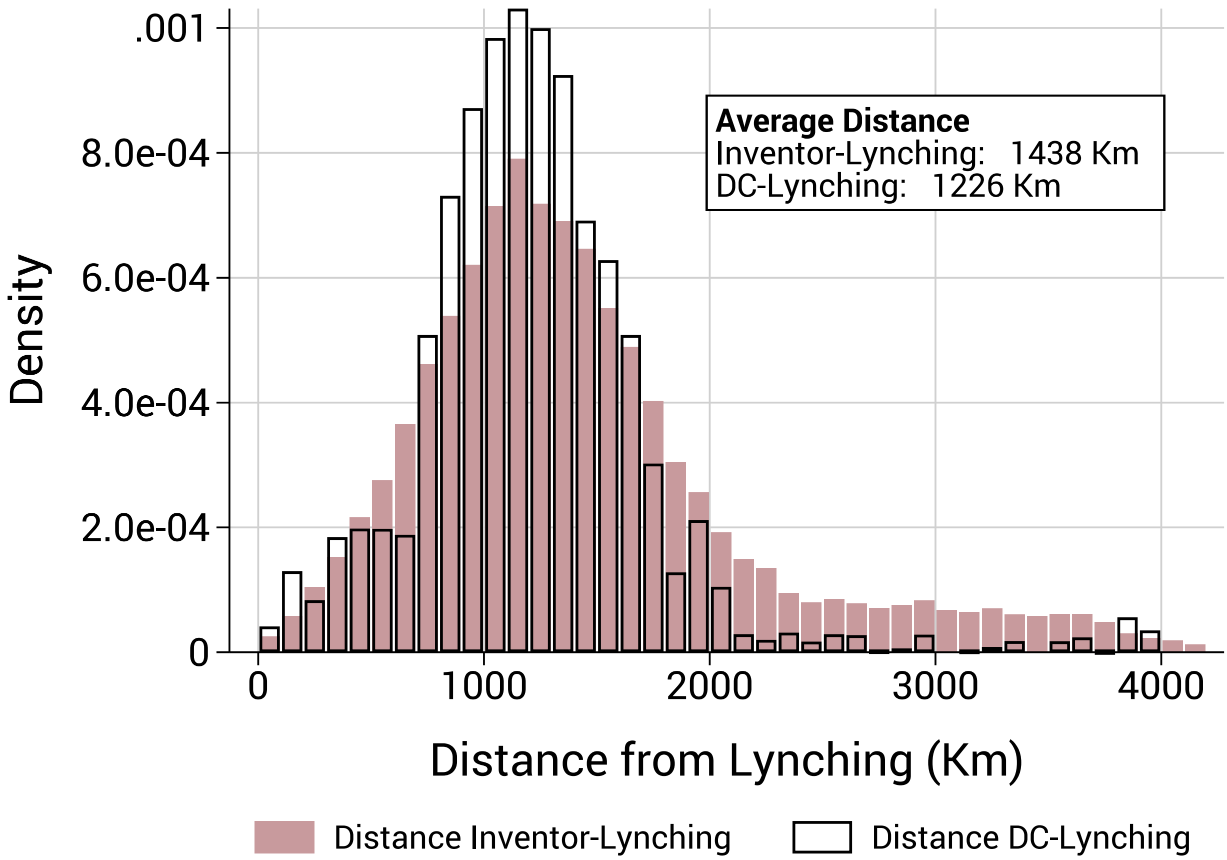
Notes. This Figure reports, in blue, the accuracy of the Black Name Index, measured as the ratio between the number of Black individuals and the number of individuals with a BNI above a given value. For instance, approximately 40% of individuals with a BNI above .8 are Black. In red, the Figure reports the power of the classification, measured as the ratio between actual Black individuals and the overall number of Black individuals, for a given threshold.

FIGURE A.8: EMPIRICAL DISTRIBUTION OF THE DELAY BETWEEN FILING AND ISSUE YEAR



Notes. The Figure reports the empirical cumulative distribution function of the patent-level delay between the filing and issue year. The sample is the universe of patents granted over the period 1895-1925. For the sake of readability, we drop all those patents with a delay time above ten years. The dashed red line indicates the sample average delay time. The solid red line indicates the 2-year lag that we adopt – the closest integer to the sample mean – in the empirical analysis.

FIGURE A.9: DISTANCE FROM LYNCHING EPISODES, INVENTORS, AND WASHINGTON, DC



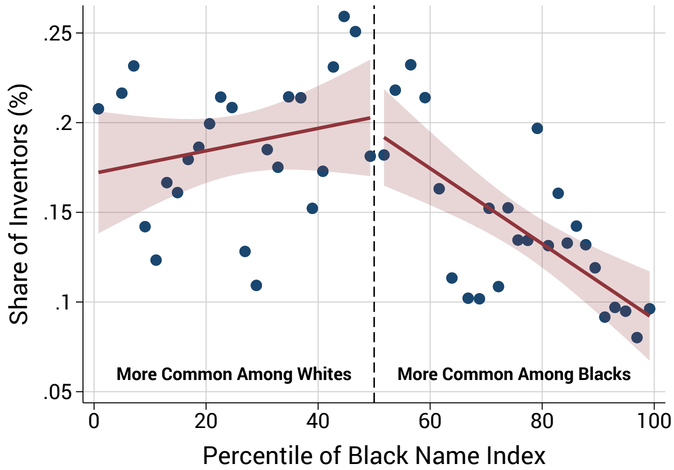
Notes. The Figure reports the distribution of the distance between inventors (in red) and Washington, DC (black-contoured) and lynching episodes over the period 1895–1925, in kilometers. To construct the Figure, we use the baseline panel of inventors from section 5.3, where each inventor is observed over ten years around a census decade. To each inventor-year pair, we attach the closest lynching whose victim shares the name with the inventor. Inventors are geo-coded to the place recorded in their patent, while we locate lynchings to the coordinates of the centroid of the county where they were perpetrated. The red bars display the distribution of the distance between each inventor-year pair and the associated lynching, whereas the black-contoured bars report the distribution of the distance between the same lynching and Washington, DC, where the USPTO was located. The Figure also reports the first moment of each distribution. As a comparison, the width of the United States is approximately 4,000 Km.

B Robustness of Stylized Facts

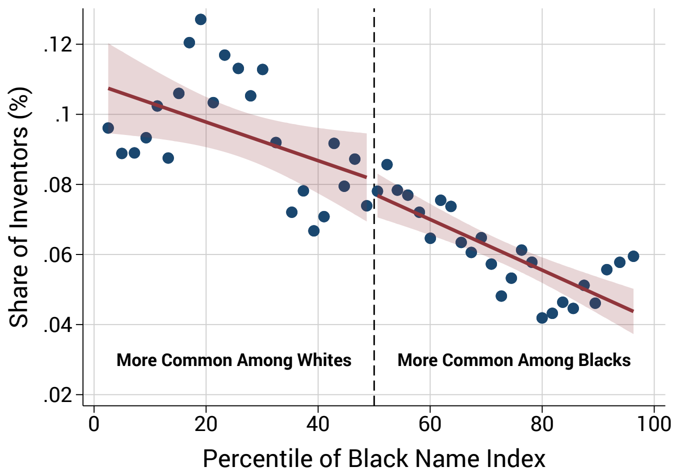
B.1 Heterogeneity By Race

Figure B.1: Correlation between Black Name Index and Inventor Status, by Race

(a) White Sub-sample



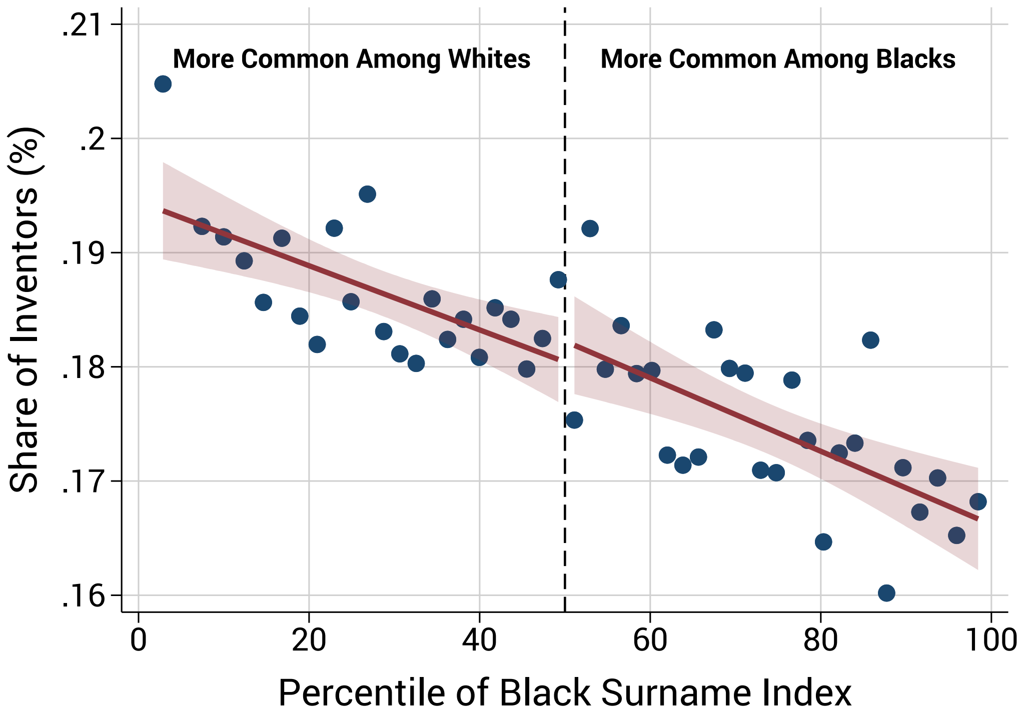
(b) Black Sub-sample



Notes. Figures report the correlation between the Black Name Index (BNI) and the share of inventors in the population, in percentage points, for white (Panel B.1a) and Black individuals (Panel B.1b). To construct the sample, we stack full-count population census data between 1900 and 1920. An individual in census t is flagged as an inventor if he has at least one patent filed between $t-5$ and $t+4$. In each census, we drop those below 18 years old and women. The unit of observation is a county, for which we observe the share of inventors among the population by birth year and percentile of the BNI. The Figure reports the associated binned scatter plot, partialling out county-by-cohort fixed effects. The dashed black line separates the bottom 50% of names that are more common among white individuals, from those that are more common among Black people. The red line superimposes the fitted values of a linear regression, and the associated 95% confidence bands, where standard errors are clustered at the county level.

B.2 Surname-Level Results

FIGURE B.2: CORRELATION BETWEEN BSI AND INVENTOR STATUS



Notes. This Figure reports the correlation between the Black Surname Index (BSI) and the share of inventors in the population, in percentage points. To construct the sample, we stack full-count population census data between 1900 and 1920. An individual in census t is flagged as an inventor if he has at least one patent filed between $t-5$ and $t+4$. In each census, we drop those below 18 years old and women. The unit of observation is a county, for which we observe the share of inventors among the population by birth year and percentile of the BSI. The Figure reports the associated binned scatter plot, partialling out county-by-cohort fixed effects. The dashed black line separates the bottom 50% of surnames that are more common among white people, from those that are more common among Black people. The red line superimposes the fitted values of a linear regression, and the associated 95% confidence bands, where standard errors are clustered at the county level.

Table B.1: Correlation Between Inventor Status and Black Surname Index

	Unconditional Correlation – Sample:			Including Fixed Effects:		Unconditional Correlation – Sample:			Including Fixed Effects:	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	All	Whites	Blacks	County	+ Cohort	All	Whites	Blacks	County	+ Cohort
Black Surname Index	-1.834***	-1.005***	-0.794***	-0.139**	-0.185***					
	(0.077)	(0.053)	(0.074)	(0.066)	(0.061)					
BNI × BSI						-4.080***	-2.985***	-2.293***	-0.635***	-2.101***
						(0.191)	(0.142)	(0.183)	(0.086)	(0.086)
County FE	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Cohort FE	No	No	No	No	Yes	No	No	No	No	Yes
Sample	All	Whites	Blacks	All	All	All	Whites	Blacks	All	All
Observations	145119928	118834440	25995332	145119928	145119925	135727127	111616888	23857759	135727127	135727124
R^2	0.000	0.000	0.000	0.001	0.002	0.000	0.000	0.000	0.001	0.002
Mean Dep. Var.	2.060	2.338	0.802	2.060	2.060	2.177	2.460	0.864	2.177	2.177
Std. Beta Coef.	-0.007	-0.003	-0.005	-0.001	-0.001	-0.015	-0.010	-0.013	-0.002	-0.008

Notes. *Notes*. This Table reports the cross-sectional correlation between the Black Surname Index (BSI) – in columns (1)–(5) – and an interaction between the Black Name Index (BNI) and the BSI – in columns (6) and (10) – and inventor status. The unit of observation is an individual, observed in a full–count population census between 1900 and 1920. The sample excludes those aged less than 18 in each census year, and women. The dependent variable is an indicator returning a value of one if the individual has obtained at least one patent over a ten-year window centered in the given census decade, and zero otherwise. For concreteness, an individual in the 1910 census is flagged as an inventor if he has obtained at least one patent between 1905 and 1914. In columns (1)–(3) and (6)–(8) we report unconditional correlations for the whole sample (columns 1 and 6), white (columns 2 and 7), and Black individuals (columns 3 and 8). In columns (4)–(5) and (9)–(10), we include individual-level fixed effects incrementally. Column (4) includes county fixed effects; column (5) adds cohort-level fixed effects to control for time-varying aggregate unobserved heterogeneity. Standard errors are clustered at the name level in columns (1)–(5), and are two-way clustered by name and surname in columns (6)–(8), and are displayed in parentheses.
*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

B.3 Additional Heterogeneity Analysis

TABLE B.2: BLACK NAME AND SURNAME INDEX AND INDIVIDUAL CHARACTERISTICS

	Black Name Index			Black Surname Index		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	White	Black	All	White	Black
Dummy variable = 1 if:						
Panel A. Literacy and lives in urban area Status						
can read and write	-.021	-.036	-.017	.009	-.001	-.013
	(.003)	(.003)	(.005)	(.002)	(.002)	(.005)
lives in urban area	-.057	-.056	-.04	-.002	-.004	-.006
	(.003)	(.003)	(.005)	(.002)	(.003)	(.004)
Panel B. Nativity & Residence						
was born in US	.012	.01	.007	.204	.205	.013
	(.005)	(.006)	(.002)	(.009)	(.008)	(.003)
lives in Northeast	-.243	-.257	-.162	-.235	-.248	-.041
	(.026)	(.026)	(.037)	(.029)	(.03)	(.007)
lives in Midwest	-.275	-.297	-.138	-.195	-.205	-.043
	(.027)	(.03)	(.02)	(.029)	(.031)	(.006)
lives in South	.584	.628	.314	.442	.466	.093
	(.014)	(.02)	(.038)	(.013)	(.015)	(.008)
lives in West	-.066	-.074	-.014	-.013	-.013	-.008
	(.011)	(.013)	(.003)	(.006)	(.007)	(.002)
Panel C. Income & Profession						
is in income quintile $\in [0, 5]$	-.363	-.371	-.23	-.018	.028	-.061
	(.012)	(.015)	(.021)	(.01)	(.009)	(.018)
is employed in agriculture	-.034	-.037	-.005	-.016	-.013	.008
	(.003)	(.003)	(.005)	(.001)	(.001)	(.005)
is employed in skilled manufacture	-.024	-.028	-.007	-.003	-.001	-.004
	(.001)	(.001)	(.001)	(.001)	(.001)	(.002)
is employed as manager	-.014	-.017	-.001	0	0	-.001
	(.001)	(.001)	(.001)	(0)	(0)	(.001)
is employed as professional	-.015	-.017	-.006	.005	.005	-.001
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
is employed as clerk	-.027	-.031	-.004	0	.002	-.002
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)

Notes. This table reports the correlation between the Black Name Index (in columns (1)–(3)) and the Black Surname Index (in columns (4)–(6)) and a set of individual-level observable characteristics, tabulated from the population census. The sample includes the entirety of the US population, stacked across the 1900, 1910, and 1920 censuses. Each dependent variable is coded as a dummy, except the income quintile which has five distinct levels. Each regression in panels A and C includes county, cohort, and race fixed effects. Regressions in panel B do not include county fixed effects. The sample includes only white individuals in columns (2) and (5), and only Black people in columns (3) and (6). Standard errors are clustered by county and are reported in parentheses. Regressions are estimated on a 1% random sample of the population.

TABLE B.3: INVENTOR STATUS AND BLACK NAME INDEX, LEAVE-OUT GEOGRAPHIC REGIONS

	Baseline	Drop States in:			
	(1)	(2)	(3)	(4)	(5)
		North-East	Midwest	South	West
Black Name Index	-1.952***	-1.760***	-1.711***	-2.560***	-1.951***
	(0.066)	(0.078)	(0.067)	(0.080)	(0.069)
County FE	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes
Race FE	Yes	Yes	Yes	Yes	Yes
Leave out:	All	Northeast	Midwest	South	West
Observations	220708581	164628066	147818310	143703264	205976102
R^2	0.002	0.002	0.002	0.002	0.002
Mean Dep. Var.	2.112	1.814	1.926	2.741	2.045
Std. Beta Coef.	-0.007	-0.007	-0.007	-0.008	-0.007

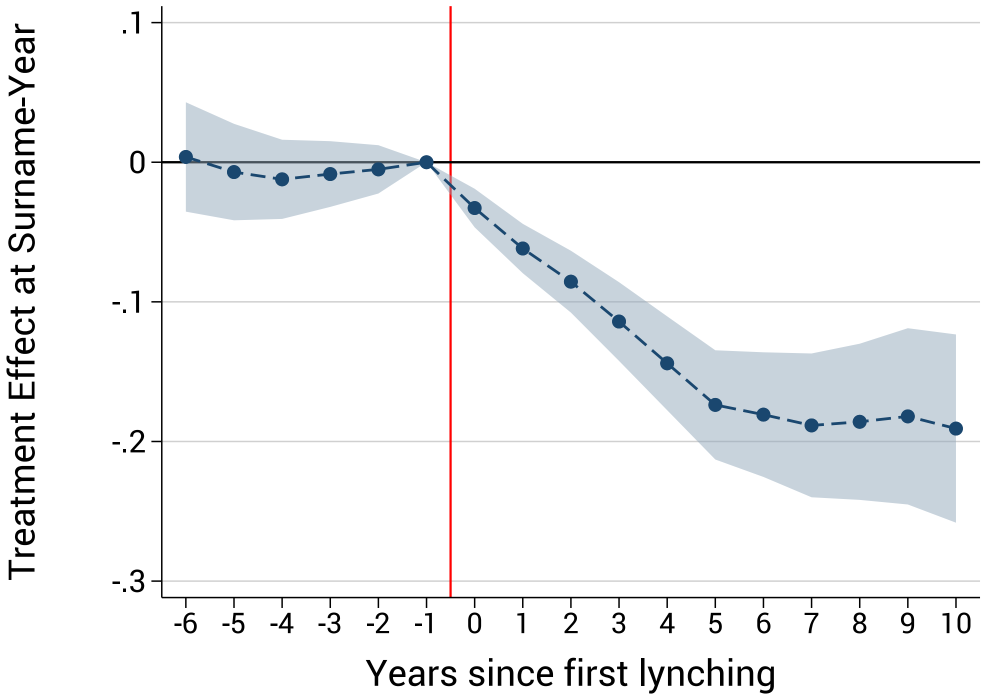
Notes. This Table reports the baseline correlation between inventor status and the BNI, dropping selected regions in the US. The unit of observation is an individual, observed in a full–count population census between 1900 and 1920. The sample excludes those aged less than 18 in each census year, and women. The dependent variable is an indicator returning a value of one if the individual has obtained at least one patent over a ten-year window centered in the given census decade, and zero otherwise. For concreteness, an individual in the 1910 census is flagged as an inventor if he has obtained at least one patent between 1905 and 1914. In column (1) we report the baseline specification. In columns (2)–(5) we drop individuals living in all the states in the specified US Census Bureau region. All regressions include county, cohort, and race fixed effects. Standard errors are clustered at the county level and are reported in parentheses.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

C Robustness of the Difference-in-Differences Results

C.1 Name- and Surname-Level Results

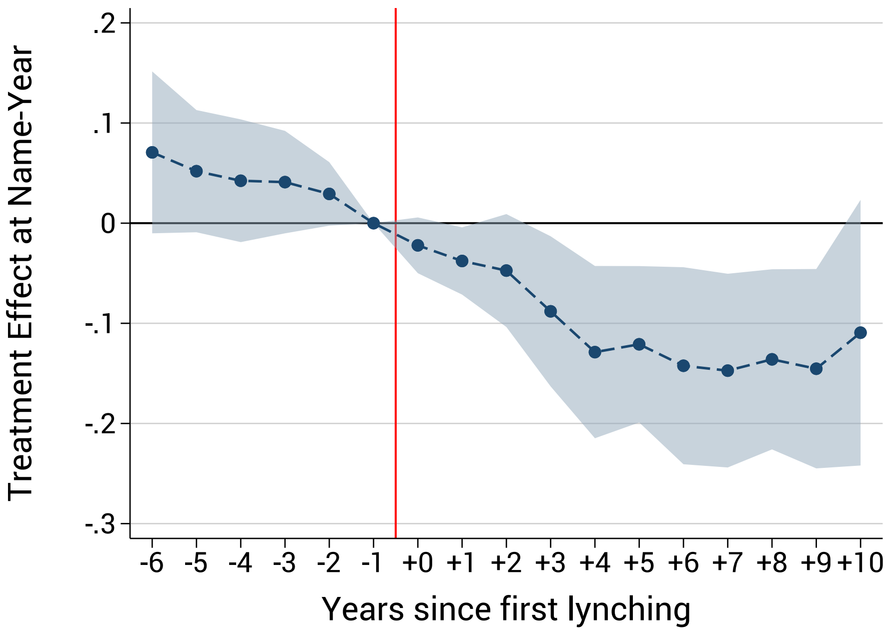
FIGURE C.1: SURNAME-LEVEL EFFECT OF LYNCHING ON INNOVATION



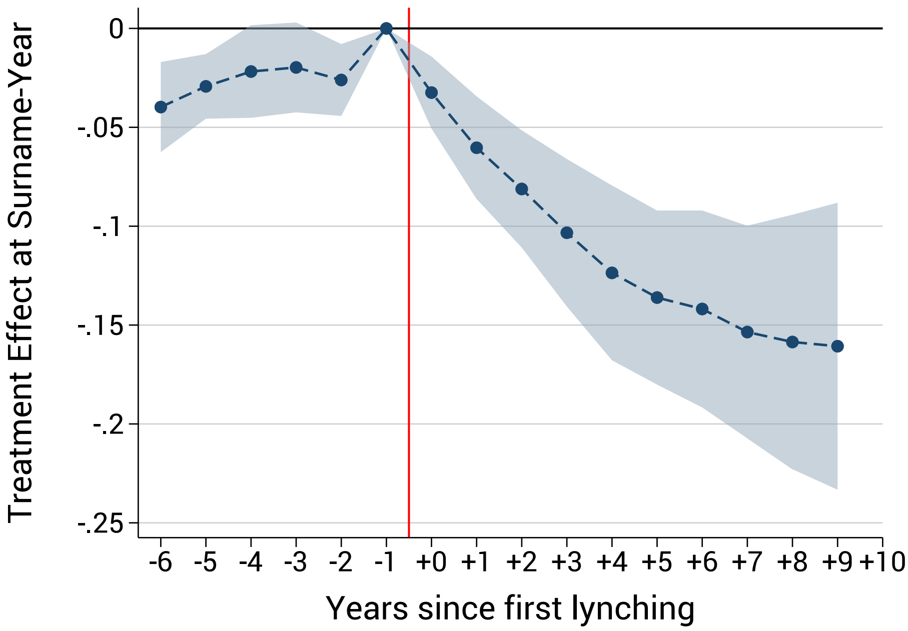
Notes. The Figure reports the surname-level effect of lynching on innovation. The unit of observation is the last name, which we observe at a yearly frequency between 1895 and 1925. The time variable is the issue year of each patent. The dependent variable is the number of patents granted to inventors with a given surname, normalized by the number of individuals with that surname in the 1880 census. For a given surname, the treatment variable returns a value of one after someone with that surname appears as the victim of a lynching episode, and zero otherwise. The timing when the treatment is activated is surname-dependent. Each dot reports the dynamic treatment effects associated with model (2), and the related 95% confidence bands, where standard errors are clustered at the surname level. Under parallel trends, we expect treatment effects before the first treatment period not to be statistically different from zero.

FIGURE C.2: NAME- AND SURNAME-LEVEL EFFECT OF LYNCHING ON INNOVATION, ALTERNATIVE ESTIMATOR

(A) EFFECT BY FIRST NAME

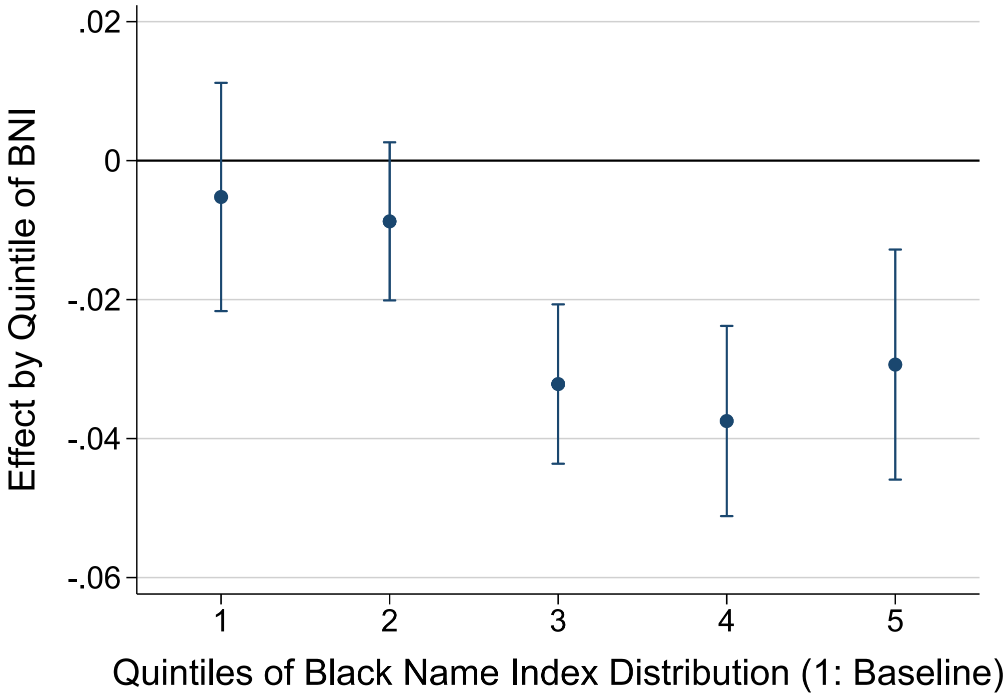


(B) EFFECT BY LAST NAME



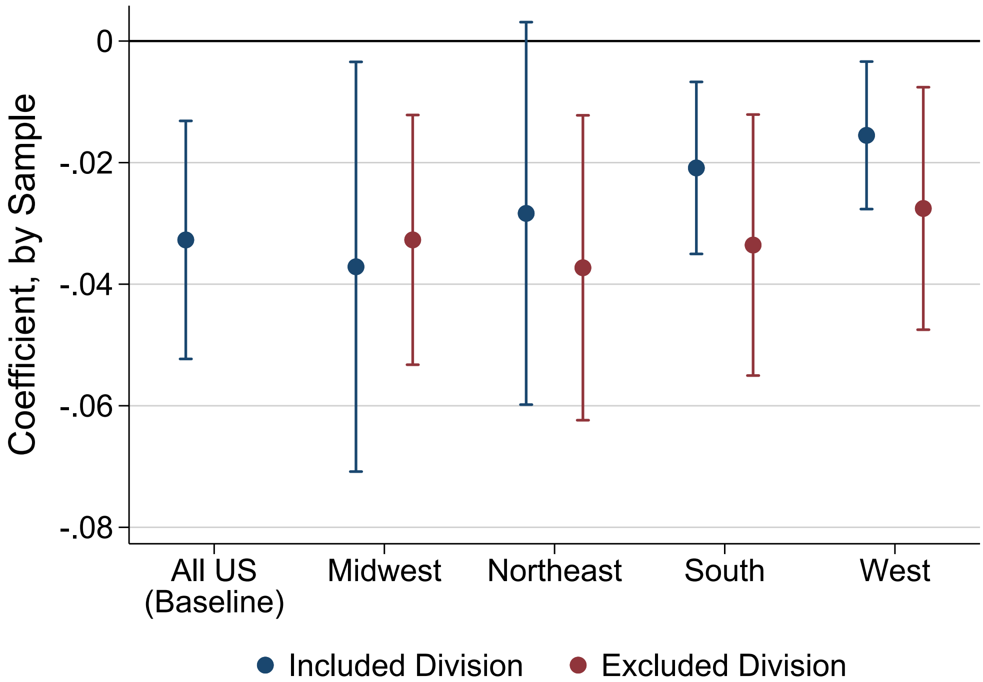
Notes. Panel C.2a (resp. Panel C.2b) reports dynamic treatment effects of lynching episodes on name- (resp. surname-) level innovation. Standard errors are clustered by name (resp. surname), and bands display 95% confidence interval. Sample definition and construction are analogous to the models presented in Figures III and C.1. We estimate both models through the estimator proposed by De Chaisemartin and D'Haultfœuille (2022), which allows for repeated treatments and correct for staggered adoption.

FIGURE C.3: EFFECT OF LYNCHING ON NAME-LEVEL INNOVATION, BY QUINTILES OF THE BNI



Notes. This Figure reports the effect of lynching on name-level innovation, over the distribution of the Black Name Index of Inventors. The unit of observation is a first name, which we observe at a yearly frequency between 1895 and 1925. The time variable is the issue year of each patent. The dependent variable is the number of patents granted to inventors with a given name, normalized by the number of individuals with that name in the 1880 census. For a given name, the treatment variable returns a value of one after someone with that name appears as the victim of a lynching episode, and zero otherwise. The timing when the treatment is activated is name-dependent. The baseline treatment is then interacted with quintile dummies which code the quintile of the BNI distribution of each name. If, for instance, the effect that we find was driven by more black-sounding names, we would expect coefficients on the right tail of the BNI distribution to be larger, in absolute value. The first quintile serves as baseline. Each dot reports the marginal effect by quintile. Bands report 95% confidence interval. Standard errors are clustered at the name level.

FIGURE C.4: EFFECT OF LYNCHING ON NAME-LEVEL INNOVATION, BY CENSUS DIVISION



Notes. This Figure reports the effect of lynching on name-level innovation, by census division. The unit of observation is a first name, which we observe at a yearly frequency between 1895 and 1925. The time variable is the issue year of each patent. The dependent variable is the number of patents granted to inventors with a given name, normalized by the number of individuals with that name in the 1880 census. For a given name, the treatment variable returns a value of one after someone with that name appears as the victim of a lynching episode, and zero otherwise. The timing when the treatment is activated is name-dependent. Blue dots report regressions run on one single census division, except for the first coefficient which refers to the full US sample. Red dots report regressions excluding one division at a time from the estimation sample. Bands report 95% confidence intervals. Standard errors are clustered at the name level.

TABLE C.1: NAME-LEVEL EFFECT OF LYNCHING ON INNOVATION, ALTERNATIVE CLUSTERING

	Cluster		AC Correction		HAC Correction			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Name	Name + Year	$k = 2$	$k = 5$	$k = 2$	$k = 5$	$k = 2$, Name	$k = 5$, Name + Year
Lynching × Post	-0.032***	-0.032***	-0.032**	-0.032**	-0.032***	-0.032***	-0.032**	-0.032**
	(0.011)	(0.011)	(0.013)	(0.014)	(0.005)	(0.006)	(0.014)	(0.014)
Name FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster Var.	Name	Name, Year	–	–	–	–	Name	Name, Year
Order of AC	–	–	2	5	2	5	5	5
Number of Names	1600	1600	1600	1600	1600	1600	1600	1600
Observations	49600	49600	49600	49600	49600	49600	49600	49600
Within-R^2	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Mean Dep. Var.	0.131	0.131	0.131	0.131	0.131	0.131	0.131	0.131
Std. Beta Coef.	-0.023	-0.023	-0.023	-0.023	-0.023	-0.023	-0.023	-0.023

Notes. This Table reports the name-level effect of lynching on innovation. The unit of observation is a first name, which we observe at a yearly frequency between 1895 and 1925. The time variable is the issue year of each patent. The dependent variable is the number of patents granted to inventors with a given name, normalized by the number of individuals with that name in the 1880 census. For a given name, the treatment variable returns a value of one after someone with that name appears as the victim of a lynching episode, and zero otherwise. The timing when the treatment is activated is name-dependent. Column (1) reports the baseline specification where standard errors are clustered by the first name; in column (2) we apply a two-way clustering procedure by name and issue year; columns (3) and (4) adjust standard errors to account for autocorrelation in the error term, by a 2 and 5 order, respectively; in columns (5) and (6) we adopt a HAC correction which implies that standard errors are robust to heteroskedasticity and autocorrelation of order, respectively, 2 and 5; in columns (7) and (8) we apply a HAC correction of order 5, and we additionally cluster by, respectively, first name and 2-way name and issue year. All regressions include name and year fixed effects.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

Table C.2: Name-Level Effect of Lynching on Innovation, Filing Year

	Filing	Issue – Delay = 1 year		Issue – Delay = 2 years	
	(1)	(2)	(3)	(4)	(5)
Lynching × Post Filing	-0.191**		-0.020		-0.033
	(0.078)		(0.038)		(0.038)
Lynching × Post Issuance (1 yr.)		-0.203***	-0.187***		
		(0.077)	(0.053)		
Lynching × Post Issuance (2 yrs.)				-0.213***	-0.189***
				(0.077)	(0.056)
Name FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Delay (Years)	–	1	1	2	2
Number of Names	1823	1823	1823	1823	1823
Observations	56513	56513	56513	56513	56513
R^2	0.249	0.249	0.249	0.249	0.249
Mean Dep. Var.	0.463	0.463	0.463	0.463	0.463
Std. Beta Coef. (Filing)	-0.010	–	-0.001	–	-0.002
Std. Beta Coef. (Issue)	–	-0.011	-0.001	-0.011	-0.002

Notes. This Table reports the name-level effect of lynching on innovation. The unit of observation is a first name, which we observe at a yearly frequency between 1895 and 1925. The time variable is the *filing* year of each patent. The dependent variable is the number of patents granted to inventors with a given name, normalized by the number of individuals with that name in the 1880 census. For a given name, we code two treatments. The first returns value one for all those years after the patent is *filed* that a person with the inventor's name is first lynched. The second is analogous, but it is activated after k years of delay. In columns (2)–(3), we set $k = 1$, and in columns (4)–(5) we let $k = 2$. We report sample statistics on the average delay between the filing and the issue year in Appendix Table A.2 and in Appendix Figure A.8. The timing when the treatments are activated is name-dependent. In column (1) we report the effect of the first treatment. In columns (2) and (4) we include the second. In columns (3) and (5) we include both. All regressions include name and year fixed effects. Standard errors are clustered at the first name level and are reported in parentheses.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

TABLE C.3: NAME-LEVEL EFFECT OF INNOVATION: PATENTS FILED BEFORE LYNCHINGS

	Baseline		Race of Lynching Victim:		Race of Inventor:		High-Quality	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			White	Black	White	Black	Number	Share
Lynching × Post	-0.103*	-0.183			-0.036***	-0.097	-0.025	3.456***
	(0.058)	(0.115)			(0.013)	(0.070)	(0.023)	(1.235)
White Lynching × Post			-0.071*					
			(0.036)					
Black Lynching × Post				-0.102*				
				(0.058)				
Name FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All	All	All	All	White	Black	All	All
Weight	–	Quality	–	–	–	–	–	–
Number of Names	91	91	92	93	82	16	91	91
Observations	1760	1692	1763	1766	1570	447	1760	1427
R^2	0.208	0.345	0.190	0.207	0.278	0.340	0.136	0.178
Mean Dep. Var.	0.083	0.097	0.083	0.083	0.069	0.060	0.005	3.019
Std. Beta Coef.	-0.191	-0.218	-0.044	-0.184	-0.168	-0.275	-0.106	0.127

Notes. This Table reports the name-level effect of lynching on innovation. The unit of observation is a first name, which we observe at a yearly frequency between 1895 and 1925. The time variable is the issue year of each patent. For each name, we only keep patents filed before someone with that name is lynched. This implies that the panel is unbalanced, and discards never-lynched names. The dependent variable is the number of patents granted to inventors with a given name, normalized by the number of individuals with that name in the 1880 census. Column (1) reports the baseline specification. In column (2) we weight each patent by its quality score computed following Kelly *et al.* (2021). In columns (3) and (4) we restrict the sample of lynchings by the race of the victim. In column (3), the treatment is equal to one after someone with a given name appears as the victim of a lynching episode only if the victim is white; the treatment in column (4) is defined analogously for lynching episodes against Black people. In column (5) we restrict the sample to white inventors, which make up approximately 95% of the entire population; in column (6) we restrict the sample to include Black individuals only. In column (7) the outcome variable is defined as the (logarithm of the) share of high-quality patents relative to the number of people by name; in column (8) the dependent variable is the share of high-quality patents relative to the total number of patents per name-year. High-quality patents are defined as those in the top 5% of the distribution of the quality indicator described in Kelly *et al.* (2021). All regressions include name- and year-fixed effects. Standard errors are clustered at the name level and are displayed in parentheses.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

TABLE C.4: SURNAME-LEVEL EFFECT OF LYNCHING ON INNOVATION

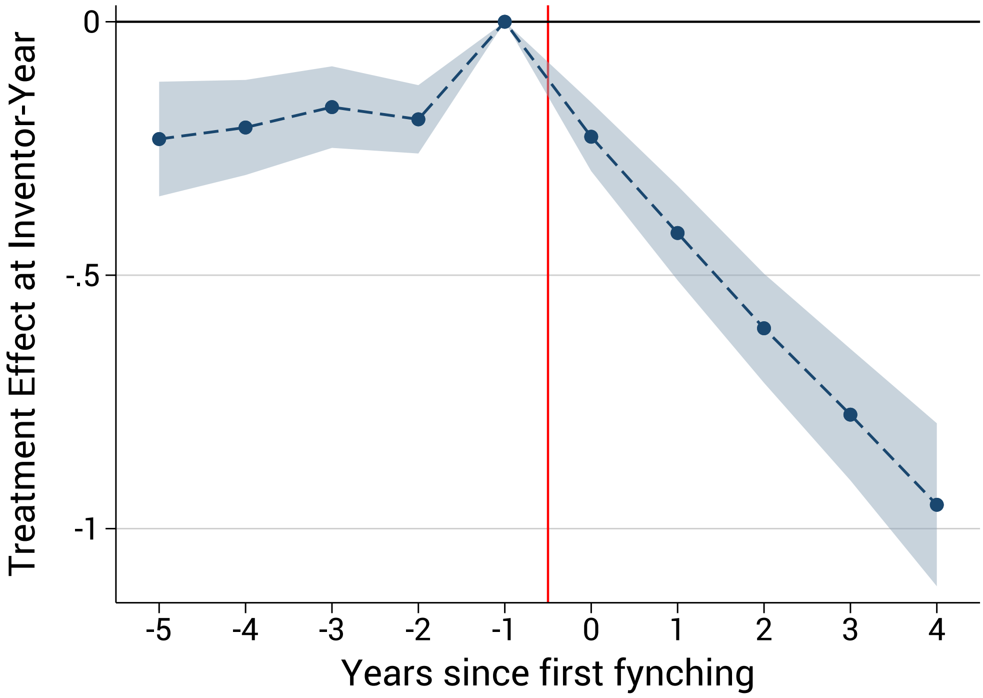
	Baseline		Race of Lynching Victim:		Race of Inventor:		High-Quality	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			White	Black	White	Black	Volume	Share
Lynching × Post	-0.207***	-0.235***			-0.211***	-0.207*	-0.011***	-0.026
	(0.043)	(0.053)			(0.046)	(0.107)	(0.004)	(0.235)
White Lynching × Post			-0.224***					
			(0.043)					
Black Lynching × Post				-0.202***				
				(0.044)				
Surname FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All	All	All	All	White	Black	All	All
Weight	–	Quality	–	–	–	–	–	–
Number of Surnames	10623	10623	10676	10641	8364	405	10623	10620
Observations	329313	328449	330956	329871	259284	12555	329313	155189
R^2	0.167	0.211	0.167	0.167	0.154	0.190	0.097	0.121
Mean Dep. Var.	0.614	0.701	0.611	0.613	0.451	0.146	0.020	2.329
Std. Beta Coef.	-0.005	-0.005	-0.002	-0.004	-0.006	-0.035	-0.002	-0.001

Notes. This Table reports the surname-level effect of lynching on innovation. The unit of observation is a surname, which we observe at a yearly frequency between 1895 and 1925. The time variable is the issue year of each patent. The dependent variable is the number of patents granted to inventors with a given surname, normalized by the number of individuals with that surname in the 1880 census. For a given surname, the treatment variable returns a value of one after someone with that surname appears as the victim of a lynching episode, and zero otherwise. The timing when the treatment is activated is surname-dependent. Column (1) reports the baseline specification. In column (2) we weight each patent by its quality score computed following Kelly *et al.* (2021). In columns (3) and (4) we restrict the sample of lynchings by the race of the victim. In column (3), the treatment is equal to one after someone with a given surname appears as the victim of a lynching episode only if the victim is white; the treatment in column (4) is defined analogously for lynching episodes against Black victims. In column (5) we restrict the sample to white inventors, which make up approximately 95% of the entire population; in column (6) we restrict the sample to include Black individuals only. In column (7) the outcome variable is defined as the (logarithm of the) ratio between the number of high-quality patents relative to the number of people by surname; in column (7) the dependent variable is the share of high-quality patents relative to the total number of patents. High-quality patents are defined as those in the top 5% of the distribution of the quality indicator described in Kelly *et al.* (2021). All regressions include surname and year fixed effects. Standard errors are clustered at the surname level and are displayed in parentheses.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

C.2 Inventor-Level Results

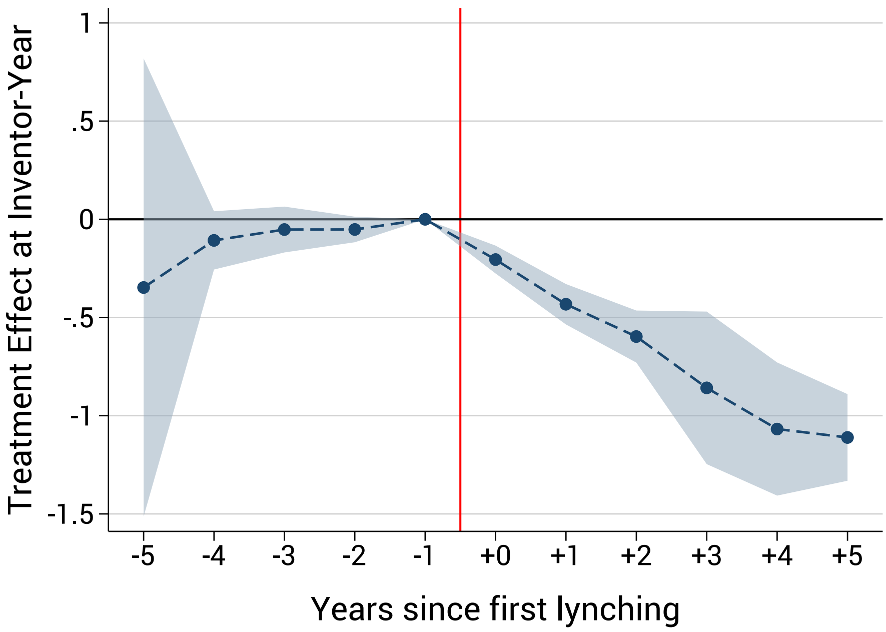
FIGURE C.5: INDIVIDUAL-LEVEL EFFECT OF LYNCHING ON INNOVATION, SURNAME-LEVEL TREATMENT



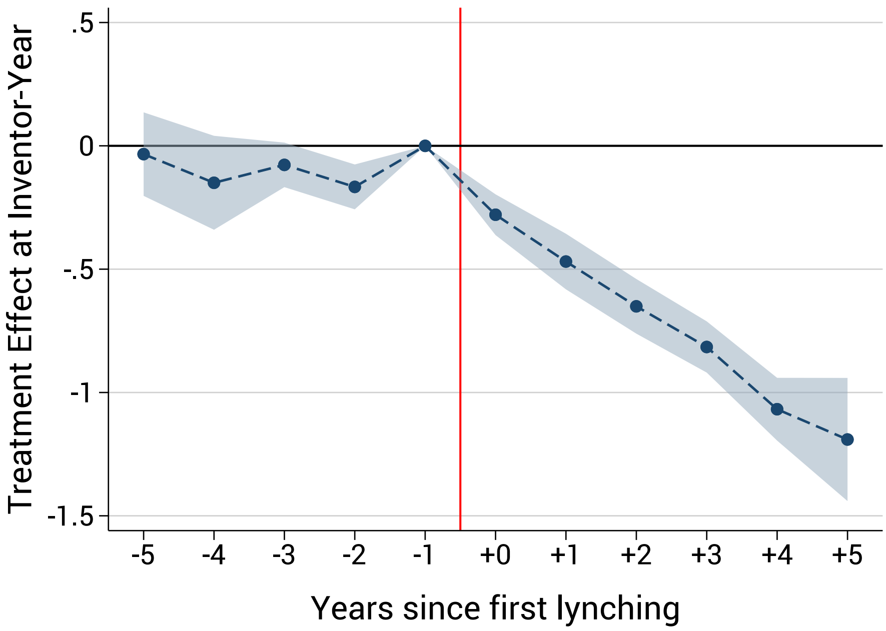
Notes. The Figure reports the inventor-level effect of lynching on innovation. The unit of observation is an inventor, who we observe at a yearly frequency. We observe each inventor over a ten-year window around one census year. For concreteness, if an inventor has patents filed between 1895 and 1904, he shall be linked to the 1900 census and we shall observe his innovation activity between 1895 and 1904. The timing variable is thus the issue year of each patent. The dependent variable is the number of patents that the inventor files in a given year. For a given inventor, the treatment variable returns a value of one after someone with the same surname as the inventor appears as the victim of a lynching episode, and zero otherwise. The timing when the treatment is activated is surname-dependent, and the treatment is interacted with time dummies. The sample comprises all those inventors who file one patent both before and after their surname is treated, and those whose surname is never lynched. We thus drop inventors that are "always treated" within the observation sample, and those who obtain only one patent. The Figure reports the estimated dynamic treatment effects associated with regression (3). The model includes individual, county-by-year, and name-by-year fixed effects. Dots report the point estimates, and the blue bands report their 95% confidence intervals, where standard errors are clustered at the surname level.

Figure C.6: Name- and Surname Individual-Level Effect of Lynching on Innovation, Alternative Estimator

(a) Effect by Inventor First Name

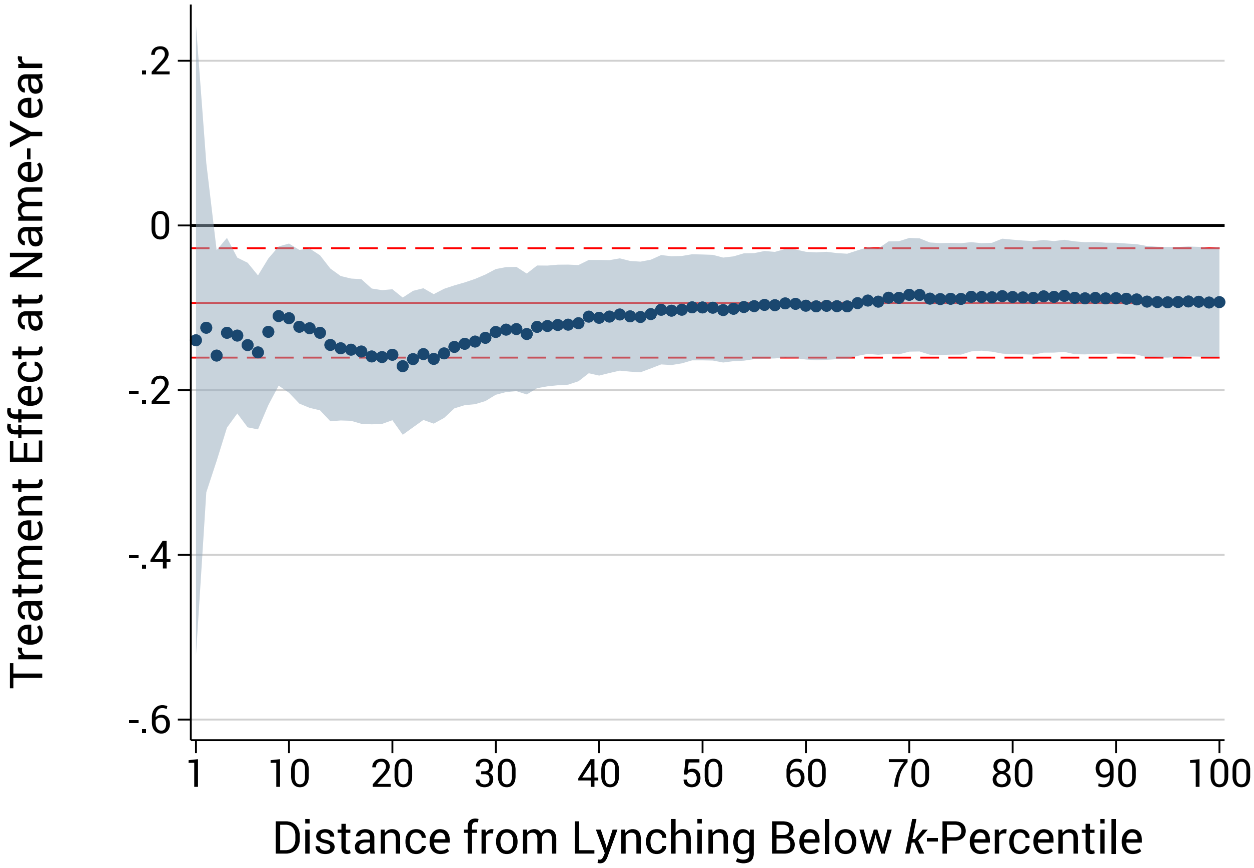


(b) Effect by Inventor Last Name



Notes. Panel C.6a (resp. Panel C.6b) reports individual-level dynamic treatment effects of lynching episodes on name- (resp. surname-) level innovation. Standard errors are clustered by name (resp. surname), and bands display 95% confidence interval. Sample definition and construction are analogous to the models presented in Figures IV and C.5. We estimate both models through the estimator proposed by De Chaisemartin and D'Haultfœuille (2022), which allows for repeated treatments and corrects for staggered adoption.

FIGURE C.7: INDIVIDUAL LEVEL EFFECT OF LYNCHING ON INNOVATION, BY QUINTILES OF DISTANCE BETWEEN THE INVENTOR AND THE LYNCHING



Notes. The Figure reports the inventor-level effect of lynching on innovation. The unit of observation is an inventor, who we observe at a yearly frequency. We observe each inventor over a ten-year window around one census year. For concreteness, if an inventor has patents filed between 1895 and 1904, he shall be linked to the 1900 census and we shall observe his innovation activity between 1895 and 1904. The timing variable is thus the issue year of each patent. The dependent variable is the number of patents that the inventor files in a given year. For a given inventor, the treatment variable returns a value of one after someone with the same surname as the inventor appears as the victim of a lynching episode, and zero otherwise. The timing when the treatment is activated is surname-dependent, and the treatment is interacted with time dummies. The sample comprises all those inventors who file one patent both before and after their surname is treated, and those whose surname is never lynched. We thus drop inventors that are "always treated" within the observation sample, and those who obtain only one patent. We estimate the model by considering only lynchings whose distance with the inventor is below the k-quantile of the distribution of distance, and we vary k. Clearly, at $k = 100$ we estimate the baseline model with the full sample of inventors and the same treatment definition. If the effect was driven by inventors that were closer to the lynching, we would expect a below-average treatment effect at the left tail of the distribution of distance. The solid red line reports the average treatment effect, with associated 95% confidence interval reported by the dashed red lines.

TABLE C.5: INVENTOR-LEVEL EFFECT OF LYNCHING ON INNOVATION, ALTERNATIVE CLUSTERING

	Cluster		AC Correction		HAC Correction			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Name	Name + Year	k = 2	k = 5	k = 2	k = 5	k = 2, Name	k = 5, Name + Year
Lynching × Post	-0.098***	-0.098***	-0.098***	-0.098***	-0.098***	-0.098***	-0.098***	-0.098***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.008)	(0.009)	(0.004)	(0.004)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster Var.	Name	Name, Year	–	–	–	–	Name	Name, Year
Order of AC	–	–	2	5	2	5	5	5
Number of Names	125034	125034	125034	125034	125034	125034	125034	125034
Observations	1276387	1276387	1276387	1276387	1276387	1276387	1276387	1276387
Within-R^2	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Mean Dep. Var.	0.193	0.193	0.193	0.193	0.193	0.193	0.193	0.193
Std. Beta Coef.	-0.031	-0.031	-0.031	-0.031	-0.031	-0.031	-0.031	-0.031

Notes. This Table reports the inventor-level effect of lynching on innovation. The unit of observation is an inventor, who we observe at a yearly frequency. We observe each inventor over a ten-year window around one census year. For concreteness, if an inventor has patents filed between 1895 and 1904, he shall be linked to the 1900 census and we shall observe his innovation activity between 1895 and 1904. The timing variable is thus the issue year of each patent. In columns (1)–(4) the dependent variable is the number of patents that the inventor files in a given year. In columns (5)–(8) the dependent variable is an indicator returning a value of one if the inventor has at least one patent in a given year, and zero otherwise. For a given inventor, the treatment variable returns a value of one after someone with the same surname as the inventor appears as the victim of a lynching episode, and zero otherwise. The timing when the treatment is activated is surname-dependent. The sample comprises all those inventors who file one patent both before and after their surname is treated, and those whose surname is never lynched. We thus drop inventors that are "always treated" within the observation sample, and those who obtain only one patent. Column (1) reports the baseline specification where standard errors are clustered by the first name; in column (2) we apply a two-way clustering procedure by name and issue year; columns (3) and (4) adjust standard errors to account for autocorrelation in the error term, by a 2 and 5 order, respectively; in columns (5) and (6) we adopt a HAC correction which implies that standard errors are robust to heteroskedasticity and autocorrelation of order, respectively, 2 and 5; in columns (7) and (8) we apply a HAC correction of order 5, and we additionally cluster by, respectively, first name and 2-way name and issue year. All regressions include individual and county-by-year fixed effects.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

TABLE C.6: INVENTOR-LEVEL EFFECT OF LYNCHING ON INNOVATION, SURNAME-LEVEL TREATMENT

	Patents				1 (Patents > 0)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lynching × Post	-0.064***	-0.065***	-0.060**	-0.151**	-0.064***	-0.063***	-0.062***	-0.092***
	(0.023)	(0.023)	(0.023)	(0.062)	(0.014)	(0.013)	(0.013)	(0.028)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Name-Year FE	No	Yes	No	No	No	Yes	No	No
Sample	All	All	Whites	Blacks	All	All	Whites	Blacks
Number of Inventors	270650	264443	259621	9460	270650	264443	259621	9460
Observations	2778719	2715076	2666118	96480	2778719	2715076	2666118	96480
R^2	0.270	0.289	0.271	0.326	0.111	0.133	0.112	0.219
Mean Dep. Var.	0.174	0.174	0.175	0.156	0.137	0.137	0.137	0.128
Std. Beta Coef.	-0.012	-0.012	-0.011	-0.032	-0.019	-0.019	-0.019	-0.034

Notes. This Table reports the inventor-level effect of lynching on innovation. The unit of observation is an inventor, who we observe at a yearly frequency. We observe each inventor over a ten-year window around one census year. For concreteness, if an inventor has patents filed between 1895 and 1904, he shall be linked to the 1900 census and we shall observe his innovation activity between 1895 and 1904. The timing variable is thus the issue year of each patent. In columns (1)–(4) the dependent variable is the number of patents that the inventor files in a given year. In columns (5)–(8) the dependent variable is an indicator returning a value of one if the inventor has at least one patent in a given year, and zero otherwise. For a given inventor, the treatment variable returns a value of one after someone with the same surname as the inventor appears as the victim of a lynching episode, and zero otherwise. The timing when the treatment is activated is surname-dependent. The sample comprises all those inventors who file one patent both before and after their surname is treated, and those whose surname is never lynched. We thus drop inventors that are "always treated" within the observation sample, and those who obtain only one patent. In columns (1), (3), (4), (5), (7), and (8) the model includes individual and county-by-year fixed effects. In columns (2) and (6) we further add surname-by-year fixed effects to leverage variation among inventors with the same surname but different first surname. In columns (3) and (7) we restrict the sample to include only white inventors, whereas in columns (4) and (8) we only include Black inventors. Standard errors are clustered by surname and are reported in parentheses.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

D Validation: Newspaper Coverage of Lynching

In this Appendix, we present a simple exercise we performed to verify the coverage of lynching episodes by news outlets. In particular, we find that the vast majority of lynchings were reported in newspapers. Articles most often featured the name and the race of the victim. Moreover, we find that a substantial share of the episodes was reported in newspapers based in Washington, DC, as well.

We draw a random sample of 128 instances of lynching episodes against Black men recorded in Seguin and Rigby (2019) and Hines and Steelwater (2006). From all states, we draw at most 5 instances for each state.[26]

For each of those instances, we first manually verify whether at least a newspaper recorded in newspapers.com mentions the full name of the victim in the year of the lynching. In almost 90% of instances (114), the victim's name is reported. This is not surprising, since the source of the lynching data is historical newspapers, but speaks to the broad coverage achieved by the digitized sample of newspapers.

Second, we verify that newspapers in Washington, D.C., where the patent office was located, similarly covered the lynchings and the victims' names. For 71 instances of lynchings, newspapers located in Washington, D.C. mentioned the name of the lynching victim in the year of the lynching. This amounts to more than half of the sample (55.5% of all 128 instances) and supports our assessment that lynchings all across the nation were covered by newspapers in Washington, D.C.

Lastly, we consider the accuracy of this approach. To this end, we read individual articles for each of the lynchings covered by newspapers in Washington, D.C., and assess whether at least one of those in fact indicates that a person by this name was black and lynched. For 44 (61%) of these 71 instances, that is the case.

[26] It is worth emphasizing that existing scholarship provides a more thorough validation of our hypothesis (Perloff, 2000; Weaver, 2019). This notwithstanding, because newspaper coverage of lynching episodes is a central assumption underlying our empirical methodology, we undertake this manual check.

E Validation: Lynching Affected Behavior

In this section, we provide additional suggestive evidence that lynching episodes impacted the racial perception of the names (and surnames) of the victims. Our argument is that, as someone with a given name was lynched, newspaper coverage across the country implied that the racial content of the name of the victim increased. More specifically, if an African American was lynched, individuals across the nation would be more likely to view the name of the victim as African American. This observation is naturally hard to test. In this Appendix, we show that after someone with a given name is lynched, the number of newborn children carrying the same name as the victim steadily and persistently decreases. Consistently with the evidence provided in Table III, we find a large and negative effect for lynching episodes that targeted Black people, while we cannot detect any significant effects for lynchings against white people.

We estimate models along the following specification:

$$\ln(1 + \text{Children})_{nt} = \alpha_n + \alpha_t + \sum_{k=-a}^{b} \beta_k \times \mathbf{1}\left[\text{Lynching}_{nt}\right] + \varepsilon_{nt} \tag{E.1}$$

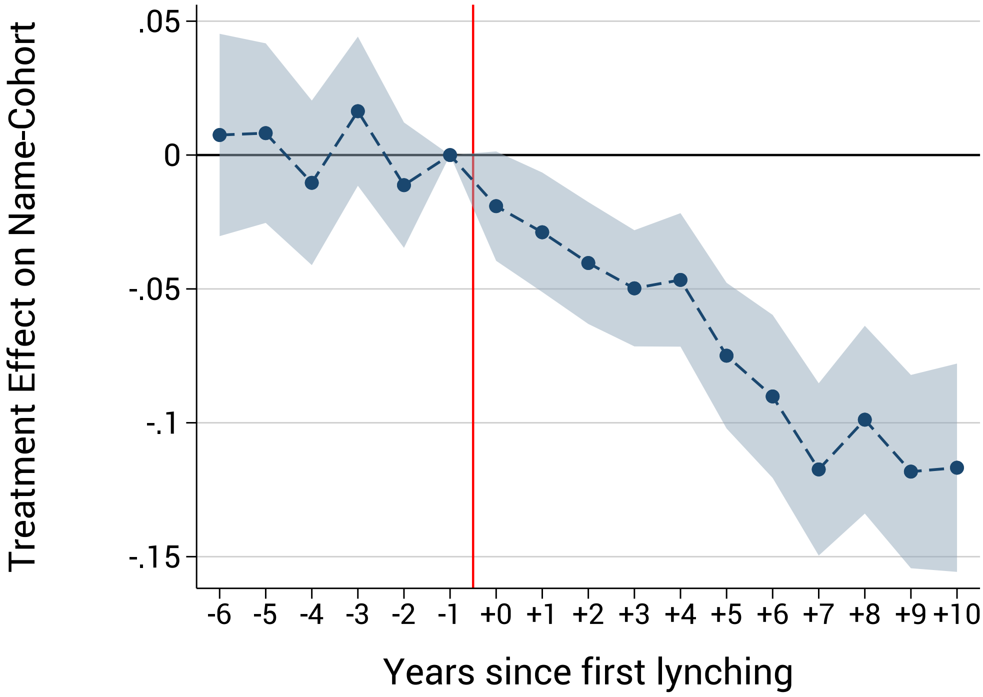
where n and t denote respectively name and year with associated fixed effects α_n and α_t, $\mathbf{1}(\cdot)$ is an indicator variable, and $\left(\text{Lynching}_{nt} \equiv t - \text{Lynching}_n\right)$ denotes the number of years since name n was first lynched. The dependent variable is the (log) number of children born in year t carrying name n. We exclude from the sample names given to less than 100 people in the entire 1920 sample. We also estimate a static variant of model (E.1) where a single post-treatment indicator conflates all periods after the first name-lynching.

We report the results of the flexible model in Figure E.1. In Panel E.1a we focus on the entire population of newborn, while Panels E.1b and E.1c narrow down the sample to, respectively, white and Black newborn kids. After a given first name is lynched, the number of children carrying the same name as the victim decreases immediately, and substantially. With no evidence of pre-treatment significantly different from zero trends, the effect of lynchings is persistent and remains significant for at least a decade after the episode. This holds irrespective of the race of the newborn kid. In Table E.1 we investigate in some more detail this finding. The effect is larger for females (column 3) than for males (column 2). Moreover, while the effect of lynchings against the Black community is negative and significant (column 4), that of lynching episodes targeting white individuals is not (column 5).

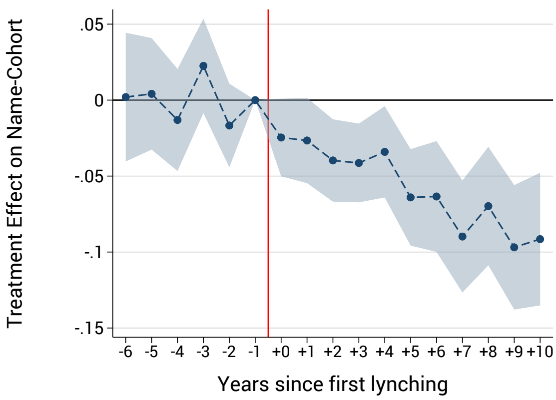
Taken together, we interpret this as further evidence that (i) lynching episodes retained a substantial impact on individual behavior, and (ii) because the effect is prevalent for lynchings against Black individuals, our findings point to racial discrimination as the prevailing underlying mechanism.

FIGURE E.1: NAME-LEVEL EFFECT OF LYNCHING ON NAMING BEHAVIOR

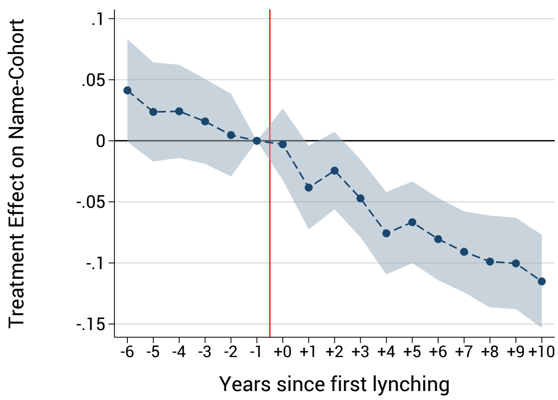
(A) ALL NEWBORN CHILDREN



(B) WHITE NEWBORN CHILDREN



(C) BLACK NEWBORN CHILDREN



Notes. The Figure reports the name-level effect of lynching on naming behavior. The unit of observation is a first name, which we observe at a yearly frequency between 1895 and 1925. The time variable is the cohort. The dependent variable is the number (log) number of children born with a given name. For a given name, the treatment variable returns a value of one after someone with that name appears as the victim of a lynching episode, and zero otherwise. The timing when the treatment is activated is name-dependent. Each dot reports the dynamic treatment effects associated with model (E.1), and the related 95% confidence bands, where standard errors are clustered at the name level. Under parallel trends, we expect treatment effects before the first treatment period not to be statistically different from zero.

Table E.1: Name-Level Effect of Lynching on Naming Behavior

	All Races Newborn					Black Newborn	White Newborn
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Baseline	Males	Females	Black Lynching	White Lynching		
Lynching × Post	-0.125***	-0.069***	-0.364***			-0.099***	-0.140***
	(0.011)	(0.012)	(0.037)			(0.012)	(0.010)
Black Lynching × Post				-0.145***			
				(0.012)			
White Lynching × Post					0.026		
					(0.029)		
Name FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All	Male	Female	All	All	All	All
Race of Victim	All	All	All	Black	White	All	All
Number of Names	15817	6365	6699	15817	15817	15817	15817
Observations	490327	197315	207669	490327	490327	490327	490327
R^2	0.878	0.900	0.877	0.878	0.878	0.875	0.861
Mean Dep. Var.	2.608	2.679	2.673	2.608	2.608	2.415	1.053
Std. Beta Coef.	-0.013	-0.010	-0.016	-0.014	0.001	-0.010	-0.017

Notes. This Table reports the name-level effect of lynching on naming behavior. The unit of observation is a first name, which we observe at a yearly frequency between 1895 and 1925. The time variable is the cohort. The dependent variable is the number (log) number of children born with a given name. For a given name, the treatment variable returns a value of one after someone with that name appears as the victim of a lynching episode, and zero otherwise. The timing when the treatment is activated is name-dependent. Column (1) reports the baseline specification. In columns (2) and (3) we split the sample into seemingly male and seemingly female names. A name is regarded as seemingly female if more than 80% of individuals with that name are female, in the 1880 full-count census. In columns (4) and (5) we restrict the attention to lynching whose victim was, respectively, Black and white. In columns (6) and (7) we restrict the sample of newborn children in the dependent variable to include Black and White only, respectively. All regressions include name and year fixed effects. Standard errors are clustered at the name level and are displayed in parentheses.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$