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To my parents

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Abstract

This thesis consists of four essays. In the first essay, I examine how the historical planter elite of the Southern US affected economic development at the county level between 1840 and 1960. I find that counties with a relatively wealthier planter elite before the Civil War performed significantly worse in the post-war decades and even after World War II. In the second essay we investigate the link between religious membership and rainfall risk across US counties in the second half of the nineteenth century. Our results indicate that church membership and seating capacity were significantly larger in counties likely to have been subject to greater rainfall risk. In the third essay, we examine the effect of removing restriction to bank entry on bank failures exploiting the introduction of free banking laws in US states during the 1837-1863 period. Our main finding is that counties in free banking states experienced significantly more bank failures. In the fourth essay we examine the effects that within-county changes in the cultural composition of the US population had on output growth during the age of mass migration. Our main finding is that increases in cultural fractionalization significantly increased output, while increases in cultural polarization significantly decreased output.

Resum

Aquesta tesi consisteix en quatre articles. En el primer assaig, s'examina com l'èlit històrica del sud dels EUA va afectar el desenvolupament econòmic a nivell de comtat entre 1840 i 1960. He trobat que els comtats amb una èlit relativament més rica abans de la Guerra Civil empitjoraven significativament en les dècades de la postguerra i fins després de la Segona Guerra Mundial. En el segon assaig s'investiga la relació entre l'afiliació religiosa i el risc de pluja a través dels comtats dels Estats Units en la segona meitat del segle XIX. Els nostres resultats indiquen que la comunitat de l'església i el nombre de seients van ser significativament majors en els comtats amb probabilitats d'haver estat subjectes a un major risc de pluja. En el tercer assaig, s'analitza l'efecte de l'eliminació de restriccions a l'entrada de bancs en la fallida de bancs que exploten la introducció de les lleis del "free banking" als estats dels EUA durant el període 1837-1863. La nostra principal conclusió és que els comtats en els estats amb "free banking" experimentaven significativament més fracassos bancaris. En el quart assaig s'examinen els efectes que els canvis dins del comtat en la composició cultural de la població dels EUA, van tenir en el creixement de la producció durant l'era de la migració massiva. La nostra principal conclusió és que l'augment de fragmentació cultural, van augmentar significativament la producció, mentre que l'augment de la polarització cultural, disminuia significativament la producció.

Forword

This thesis consists of four chapters. The first chapter examines how the historical planter elite of the Southern US affected economic development at the county level between 1840 and 1960. To capture the planter elite's potential to exercise de facto power, I construct a new dataset on the personal wealth of the richest Southern planters before the American Civil War. I find that counties with a relatively wealthier planter elite before the Civil War performed significantly worse in the post-war decades and even after World War II. I argue that this is the likely consequence of the planter elite's lack of support for mass schooling. My results suggest that when during Reconstruction the US government abolished slavery and enfranchised the freedmen, the planter elite used their de facto power to maintain their influence over the political system and preserve a plantation economy based on low-skilled labor. In fact, I find that the planter elite was better able to sustain land prices and the production of plantation crops during Reconstruction in counties where they had more de facto power.

The second chapter examines the link between rainfall risk and religious membership in the late nineteenth-century US. Historically, religious organizations have often been at the core of local and cross-regional mutual assistance and social aid networks. Joining such networks can be more attractive in communities facing greater aggregate uncertainty as the value of partially insuring idiosyncratic shocks may increase with aggregate background risk. We show this in a theoretical model where aggregate background risk is driven by rainfall risk common to all members of the community. We then examine the link between religious membership – proxied by church membership or seatings – and rainfall risk across US counties in the second half of the nineteenth century. Consistent with our theoretical model the results indicate that religious membership was significantly larger in counties likely to have been subject to greater rainfall risk. This link is present among more agricultural counties and among counties with low population densities, but not among less agricultural or more densely populated counties. Among agricultural counties, a one-standard-deviation increase in rainfall risk is associated with an increase in church seating capacity of around 65 percent in 1860 and 32 percent in 1890.

The third chapter examines the impact of removing barriers to bank entry on bank failures exploiting the introduction of free banking laws in US states during the 1837-1863 period. Focusing on this historical event allows us to: (1) rule out the confounding effects of state implicit guarantees; (2) identify the causal relation using contiguous counties on the border of states with different regulation. Our main finding is that counties in free banking states experienced significantly more bank failures. We also provide evidence that the individual probability of failure of both incumbent and entering banks was significantly higher in free banking states. We argue that the destabilizing effect of free banking is consistent with the view that bank competition leads to more risk taking. Our results suggest that the introduction of free banking led to more bank entry and a significant drop in the market share of incumbent banks.

In the fourth chapter we exploit the large inflow of immigrants to the US during the 1870-1920 period to examine the effects that within-county changes in the cultural composition of the US population had on output growth. We construct measures of fractionalization and polarization to distinguish between the different effects of cultural diversity. Our main finding is that increases in cultural fractionalization significantly increased output, while increases in cultural polarization significantly decreased output. We address the issue of identifying the causal effects of cultural diversity by using the supply-push component of immigrant inflows as an instrumental variable.

Contents

Chapter 1

THE PERSISTENCE OF DE FACTO POWER: ELITES AND ECONOMIC DEVELOPMENT IN THE US SOUTH, 1840-1960

1.1 Introduction

Wealth inequality may slow down economic growth (e.g. Galor and Zeira, 1993; Alesina and Rodrik, 1994; Deininger and Squire, 1998; Aghion et al., 1999). The historical plantation economies in the New World often serve as an extreme example. Although relatively rich in the past, these economies have fallen behind since. One explanation is that the great concentration of wealth in the hands of a small elite promoted the establishment of oppressive institutions which were harmful for modern economic growth (Engerman and Sokoloff, 1997, 2002; Acemoglu et al., 2005; Acemoglu, 2008). Recent research has started to analyze whether historical wealth inequality might have been affecting economic development within the United States (Nunn, 2008; Galor et al., 2009; Ramcharan, 2010). I contribute to this literature by using county-level variation within the US South to examine how the relative wealth of the historical planter elite affected local economic development after the American Civil War and during the 20th century.[1]

Before the American Civil War, a large fraction of Southern wealth belonged to a small number of large plantation owners (Wright, 1970, 1978; Soltow, 1971, 1975). Historians have documented that their great wealth helped the planter elite to retain de facto power over economic institutions and politics after the Civil War, despite legal and political challenges like the abolition of slavery and black enfranchisement for example

[1]The US South had a more unequal wealth distribution in the nineteenth century compared to the northern US states or Cundinamarca a state in Columbia (Acemoglu et al., 2008).

(Wiener, 1976, 1978; Wright, 1986; Alston and Ferrie, 1999; Ransom and Sutch, 2001). I construct a new dataset on the personal wealth of the richest Southern planters before the Civil War (in 1860) to evaluate the long-run effects of the planter elite's de facto power on local economic development. A key feature of my analysis is a measure of the planter elite's relative wealth at the county level – which I regard as a proxy of their de facto power – based on these personal wealth data.[2]

My empirical analysis points to a significant negative association between the pre-Civil War wealth of the planter elite and levels of labor productivity across Southern counties in the post-war decades and even after World War II. Since my focus is on evaluating the long-run effects of the planter elite's pre-Civil War wealth on local economic development rather than the economic consequences of slavery per se, my empirical specifications always control for the extent to which local economies relied on slave labor before the Civil War.[3] The negative association between the relative wealth of the pre-Civil War planter elite and long-run labor productivity proves to be robust to a wide range of controls for geography and specialization in (certain types of) agriculture. My estimates imply that a two-standard-deviation increase in the relative wealth of the planter elite translates into productivity levels that are about 7 percent lower at the turn of the 19th century and 23 percent lower in 1950.

It is well understood that geography may have long-term effects on economic development (e.g. Diamond, 1997; Gallup et al., 1998; Rappaport and Sachs, 2003; Nunn and Puga, 2012). For example, climate and the types of available soils determine the agricultural production possibilities of an economy (e.g. Engerman and Sokoloff, 1997, 2002). I therefore examine whether the negative association between the relative wealth of the planter elite before the Civil War and long-run productivity levels in the US South is robust to a detailed set of controls for the geography, climate, and soil types of counties. I find that controlling for geography does not affect my results. The economic development of counties in the US South may also have been determined by their historical specialization in agriculture, especially in producing large-scale plantation crops like cotton, tobacco, rice, and sugar. For example, high agricultural productivity may have led to high productivity in the past but low productivity in the 20th century as agriculture crowded out manufacturing production and the learning externalities that might come with it (e.g. Matsuyama, 1992). I therefore reexamine the effect of the pre-Civil War planter elite's relative wealth on economic development after controlling for the direct effect of specialization in (large-plantation) agriculture as well as a range of plantation crops. I continue to find a significant negative association between the relative wealth of the planter elite before the Civil War and long-run labor productivity, with a

[2]To my best knowledge, this is the first comprehensive dataset on the personal wealth of the Southern planter elite. Below I argue that the planter elite's relative personal wealth reflects the elite's de facto power better than existing measures of wealth inequality based on the farm size distribution.

[3]For evidence on the long-run effects of slavery within the US see, for example, Mitchener and McLean (2003), Lagerlöf (2005), Nunn (2008), and Bertocchi and Dimico (2012).

quantitative effect that is similar to my baseline specifications.

The second contribution of this paper is to provide empirical evidence on specific mechanisms through which the planter elite's use of de facto power may have affected Southern economic development after the Civil War. The empirical literature on the determinants of long-run economic growth has documented that underinvestment in human capital is detrimental for economic development (e.g. Barro, 1991; Hanushek and Kimko, 2000; Castelló and Doménech, 2002; Ciccone and Papaioannou, 2009; Becker et al., 2011). Also the theoretical literature on inequality and growth has argued that wealth inequality may delay economic development because of the elite's reluctance to establish human capital promoting institutions (e.g. Galor and Moav, 2006; Galor et al., 2009). I therefore examine whether counties with a relatively wealthier planter elite before the Civil War accumulated less human capital following the Civil War and in the 20th century, controlling for the pre-Civil War illiteracy rate and the extent to which local economies relied on slave labor. My results indicate that illiteracy rates after the Civil War fell more slowly in counties with a relatively wealthier pre-Civil War planter elite. Moreover, I find that in 1940 and 1950 there was a significantly smaller fraction of high-school as well as college educated adults in counties with a wealthier planter elite before the Civil War. I also show that counties with a richer pre-Civil War planter elite were less likely to build so-called Rosenwald schools for black children.[4] Taken together, these results suggest that counties with a richer planter elite before the Civil War remained relative less productive well into the 20th century because of their low levels of human capital investment.

For the planter elite to be able to block reforms against their interests (such as mass education) they needed to maintain their political influence after the Civil War. While legal reforms like the abolition of slavery and black enfranchisement threatened the planter elite's capacity to control Southern institutions, historians have documented that rich planters used their wealth to maintain economic and political influence (Shugg, 1937; Wiener, 1976; Ransom and Sutch, 2001). That is, planters were able to use the de facto power that came with their wealth to substitute for a loss of de jure power (Acemoglu and Robinson, 2006, 2008a,b).[5] One way in which the planter elite could have maintained their political influence after the Civil War was to support violent actions against black political representation. For example, Foner (1996, p. xxviii) reports that more than 10 percent of the black officeholders were victims of violence during the Reconstruction period (1865-1877). To investigate whether black officeholders were

[4]The Rosenwald Rural Schools Initiative (1914-1931) supported the construction of schools for black children in rural counties in the US South (Aaronson and Mazumder, 2011).

[5]Acemoglu and Robinson argue that the underlying distribution of political power in captured economies might persist even if there are frequent changes in political institutions. Legal reforms as in the US South after the Civil War often failed to dismantle the dominant role of the elites, since these elites invested in de facto political power (e.g. by using bribes or violence) to offset their de jure political losses brought by such reforms.

more likely to be victims of violence in counties where the planter elite was relatively wealthy before the Civil War, I combine my measure of the relative wealth of the planter elite with data from Foner's directory of black officeholders during Reconstruction. My results indicate a positive and statistically significant association between the relative wealth of the pre-Civil War planter elite and violence against black officeholders following the Civil War. This suggests that the planter elite may have used their de facto power to support violent actions against black officeholders.

Moreover, I show that the political influence of the planter elite persisted in the postwar period despite the legal and political reforms accompanying Northern intervention during Reconstruction.[6] I find that 48 percent of the counties in Alabama and Mississippi – two representative states of the so-called Deep South – had county delegates in their constitutional conventions at the beginning and towards the end or following the Reconstruction period with direct family connections to the pre-Civil War planter elite.[7] I also show that family connections between the planter elite and county delegates in the constitutional conventions were more likely when the planter elite was wealthier. This suggests that – in line with Acemoglu and Robinson (2006, 2008a,b) – the planter elite used their de facto power to maintain their influence over the political system and preserve a planter-friendly regime.

One way to examine whether the greater de facto power of wealthier planters allowed them to better defend their interests when legal and political reforms during Reconstruction brought losses to the elite's de jure power is by studying the evolution of land prices during and following the Reconstruction period. Since land prices can be taken to capitalize agricultural profits (e.g. Plantinga et al., 2002; Deschênes and Greenstone, 2007), the planter elite's capacity to defend their (agricultural) interests should show in land prices. I use a difference-in-difference approach to examine the cross-county association between the planter elite's pre-Civil War wealth and land prices during Reconstruction and following the adoption of the new constitutions, when planters managed to partly restore some of their de jure power. I find that during the Reconstruction period, land prices were relatively higher in counties with a wealthier planter elite. This suggests that the planter elite's de facto power allowed them to capture local institutions for their own interest until the new constitutions restored some of their de

[6]So far there is little comprehensive data on the connections of the pre-Civil War planter elite to local politicians (delegates) after the Civil War. For anecdotal evidence on the political connections of planters after the Civil War see, for example, Moore (1978), Wynee (1986), Billings (1979), Foner and Mahoney (1995), and Cobb (1988).

[7]Both states had their first constitutional convention after the Civil War in 1865. With these constitutions came the so-called "Black Codes" – mainly vagrancy and anti-enticement laws – which intended to restrict black mobility and civil rights of Afro-American citizens. These laws were suspended during Reconstruction by the Reconstructions Acts in 1867. For more details see e.g. Wilson (1965), Cohen (1976), and Foner and Mahoney (1995). The first constitutional conventions after Reconstruction were in Alabama in 1875 and in Mississippi in 1890.

jure power.[8]

My findings on higher land prices are consistent with the so-called paternalistic view of the planters' behavior after the Civil War discussed in Alston and Ferrie (1993, 1999). According to this view, plantation owners offered blacks a set of amenities – such as protection from violence, improved housing, or medical care – in exchange of contractual arrangements that were favorable for plantation production. Alston and Ferrie argue that these paternalistic arrangements were easier to establish by wealthier planters because they required political influence. In line with the paternalistic view, my difference-in-difference analysis also yields that during Reconstruction, counties with a relatively wealthier pre-Civil War planter elite saw an increase in the production of plantation crops relative to all other main field crops grown in the US South (corn, wheat, barley, rye, oats, and sweet potatoes). I also show that there were significantly less lynchings and a higher share of black tenants in counties with a wealthier planter elite.

My work relates to the recent literature on economic inequality and development in the US. Galor et al. (2009) find a negative association between inequality in the farm size distribution and public spending on education at the county level at the beginning of the 20th century. Ramcharan (2010) documents that a more unequal farm size distribution at the county level leads to less redistribution between 1890 and 1930. Looking at the early 20th century, Rajan and Ramcharan (2011) show that counties with a more unequal farm size distribution had fewer banks per capita. On the other hand, Nunn (2008) does not find that a more unequal farm size distribution was detrimental for long-run economic development at the county level. One main difference between these contributions to the literature on the effects of wealth inequality on economic development and my work is that my measure of wealth inequality is based on personal wealth data rather than on data on farm size distributions. The two measures of wealth inequality can differ for two main reasons. First the data on farm sizes do not refer to ownership but to the farm as a unit of production. This is important as farms might have been operated by different tenants but owned by the same person. Farm tenancy was a feature of the US South even before the Civil War (Reid Jr., 1976; Winters, 1987; Bolton, 1994). For example, Bode and Ginter (2008) estimate tenancy rates from 3 to 40 percent for several counties in Georgia before the Civil War.[9] Another reason why my measure of wealth inequality differs from measures based on farm sizes is that my wealth measure also reflects the

[8]Once the planter elite largely regained their de jure political power, there were less needs to use de facto power to achieve their main objective: keeping the plantation system going (Wiener, 1976; Ransom and Sutch, 2001; Acemoglu and Robinson, 2008a). The restoration of de jure power should have benefited especially less wealthy planters who did not have the de facto power to sustain a planter-friendly system during the Reconstruction period.

[9]Since wealthy planters – the group of interest in this paper – often owned more than a single plantation (see e.g. Oakes, 1982; Rowland et al., 1996; Scarborough, 2006) an inequality measure based on farm sizes would underestimate their landholdings.

value of land. This is important if the planter elite tended to own the most valuable land. For my purposes it is therefore preferable to measure wealth inequality using personal wealth data.

Another difference between my work and the existing literature on the effects of wealth inequality in the US South is that my measure of wealth inequality is meant to proxy for the pre-Civil War planter elite's capacity to defend their interests vis-à-vis the rest of the society.[10] Since I am particularly interested in the ability of the planter elite to use their de facto power in order to repress the rest of the population, it seems sensible to measure wealth inequality by wealth of the planter elite relative to the total wealth in the county. Measures of wealth inequality based on the farm size distribution as used by Nunn (2008), Ramcharan (2010), and Rajan and Ramcharan (2011), seem better suited as a proxy of the distribution of de facto power among landowners.[11] Using the relative wealth of the planter elite as a measure of wealth inequality turns out to be key for my empirical findings. When I rerun the specification after replacing my measure of inequality with the Gini coefficients implied by the farm size distributions in each county, I do not find any statistically significant association between inequality and levels of economic development after the Civil War.

There is also a literature on the long-run effects of slavery on economic development in the US. Using variation across US states for the years 1880 to 1980, Mitchener and McLean (2003) find that the legacy of slavery adversely affects productivity. Nunn (2008) documents a negative link between slavery and current income per capita by examining US state and county level data. Within the US South, Lagerlöf (2005) finds that counties with a larger population share of slaves in 1850 are overall poorer today. However, more recently, Bertocchi and Dimico (2012) do not find any robust link between slavery and current income per capita at the county level (but document an effect on current income inequality).[12] Since my focus is on evaluating the long-run effects of the planter elite's pre-Civil War wealth on local economic development rather than the economic consequences of slavery per se, my empirical specifications always control for the extent to which local economies relied on slave labor.

The remainder of the paper is structured as follows. Section 2 provides a brief overview and discussion of the planter elite in the US South. Section 3 describes the data used in my empirical analysis. Section 4 analyzes the planter elite's impact on the

[10]See Engerman and Sokoloff (1997, 2002), Acemoglu et al. (2005), and Acemoglu (2008) for work emphasizing the conflicts of interests between the elite and the masses and the elite's capacity to repress others when it is in their interest.

[11]This becomes clear by considering an extreme example where all the land is distributed equally among a few land owners. Looking at the distribution of landholdings would yield to a complete equal distribution. On the other hand, the relative wealth of the farmer elite would depend on the (landless) population in the county, and could indicate great wealth inequality.

[12]On the economics of slavery in the US South see, for example, Fogel and Engerman (1974), Genovese (1988), Ransom and Sutch (2001), Wright (2006), or Wahl (2008) and the references therein.

post-war Southern economy. The last section concludes.

1.2 The Planter Elite in the US South

Historians have documented that a large fraction of wealth was in the hands of large plantation owners in the pre-Civil War South which resulted in a high degree of inequality in the distribution of wealth (Wright, 1970, 2006; Soltow, 1971, 1975; Niemi Jr., 1977).[13] The unequal distribution of wealth was a particularly salient feature of the Southern agricultural sector before the Civil War.[14] The reported average wealth of farmers owning slaves was $33,906 in 1860, about fourteen times larger than the wealth reported by farmers without slaves (see Ransom, 1989, Table 3.1, p. 63). Around 60 percent of the agricultural wealth was in the hand of the 10 percent richest Southern farmers and, even more strikingly, 24 percent of all wealth belonged to the 2 percent richest farmers (Ransom, 1989, p. 63). The great disparity of agricultural wealth points to the economic power of the richest farmers (planters) before the Civil War. Ownership of slaves accounts for a large part of this large disparity.[15] Slave farms had an average personal estate of $19,828 compared to the $1,188 reported by free farms, and slave farms also had the better land (see Ransom, 1989, Table 3.1, p. 63). For example, in the Cotton South,[16] the value of improved land of slave farms was $46.74 per acre in 1860 and about 3.5 times higher than the per acre value of improved farmland of farms without slaves (see Ransom, 1989, Table 3.2, p. 66).[17] Since wealthy planters tended to own also the better land, it is important that my measure of wealth inequality accounts for the value and not the size of the planter's real estate.

Before the Civil War it was the planter elite – who owned the large Southern plan-

[13]Soltow (1971, 1975) provides further information on the distribution of wealth in the United States during the 19th century. On race related economic inequality in the postbellum South see, for example, DeCanio (1979).

[14]For example, Acemoglu et al. (2008, Table 5.3) document that land inequality in the US South was considerably higher than in the northern US states in 1860.

[15]Slaves were a valuable asset during the antebellum period. The price for a prime field hand in historical dollars increased from approximately $600 around 1800 to $1,500 at the eve of the secession (Engerman et al., 2006). The value of a slave just before the Civil War was about $130,000 in 2009 dollars (see Williamson and Cain, 2011). Slave ownership was very concentrated and owning slaves in the South was the exception (Soltow, 1975, Table 5.3). According to the 1860 Census, there were 393,967 slaveholders out of 8.25 million free citizens that owned 3.95 million slaves in the South (Wahl, 2008, Tables 2 and 4) and the largest slaveowners held a disproportionate fraction of slaves within the slaveholder class. For example, in the Cotton South of 1860, large slaveowners with 50 or more slaves occupied one third of the total slave workforce (Wright, 1978, p. 31).

[16]Ransom (1989) refers to the states of Alabama, Georgia, Louisiana, Mississippi, South Carolina and Texas as Cotton South

[17]See also Wright (1970) on the agricultural wealth concentration in the Cotton South for the years 1850 and 1860.

tations and most of the slaves – that controlled Southern politics and institutions (Ransom, 1989).[18] With the adoption of the thirteenth amendment (in 1865), slavery and involuntary servitude were outlawed. The ratification of the fourteenth amendment (in 1868) and the fifteenth amendment (in 1870) granted blacks citizenship and the right to vote.[19] However, despite such major institutional changes, the existing planter elite was able to sustain a plantation-based agricultural system after the Civil War. Economic historians have argued that the reason why the planter elite could maintain economic and political influence after the Civil War was their control over landholdings (Wiener, 1976; Ransom, 1989; Ransom and Sutch, 2001). It was one of the early purposes of the Freedmen's Bureau to distribute land confiscated from former slaveowners to freedmen and finance the construction of black schools and emergency relief by selling the other confiscated property from former slaveowners (Ransom and Sutch, 2001, p. 82). A main setback for the Congress' redistribution plans was the Amnesty Proclamation of May 1865, which restored all rights to property except as to slaves, and returned confiscated land to their original owners (Ransom, 1989, p. 234).[20] A bill proposing to grant 40 acres and $50 to every former slave who was head of a household was defeated by the Congress in 1867 (Ransom and Sutch, 2001, p. 82). As Wright (1986, p. 84) noted "*[...] the key to the survival of the plantation was the ability of the former slave owners to hold onto their land in the midst of intense legal and political struggles after 1865. In national politics, the planters successfully blocked proposals for land confiscation and redistribution to the freedmen.*"

In addition to the political resistance against the redistribution of land, many Southern whites were also reluctant to sell land to blacks (Ransom and Sutch, 2001, p. 87). Often the threat of violence against white sellers and prospective black purchasers increased the cost and risk of land sales, preventing black landownership (Ransom and

[18]The definition of the Southern planter elite varies in the economic history literature. Fogel and Engerman (1974) or Campbell (1982) define large planters by ownership of slaves for example. Fogel and Engerman define a large planter as slaveholder with at least 50 slaves. Campbell uses a less narrow classification defining large planters as owners of 20 or more slaves. Wiener (1976) defines the planter elite by landownership. According to Wiener, a planter needs to own at least $10,000 in real estate in 1850, $32,000 in 1860 and $10,000 in 1870 to be considered in the planter class.

[19]In general, the freedmen were now able to accumulate wealth and savings, acquire higher skills, start their own businesses (e.g. farming), and engage in politics (see e.g. Ransom and Sutch, 2001). The US Congress founded the Freedmen's Bureau in 1865 to assist the freedmen in daily life. With the help of the Union Army and the Freedmen's Bureau established the Republican administration some new institutions for blacks like a financial saving institution (the Freedman's Saving and Trust Company) and school facilities. Moreover, black candidates were allowed to be elected as delegates for national and state governments and many served as public officeholders in local governments which brought political representation to the Afro-American community (Foner, 1988; Du Bois, 1999).

[20]The only exemption was made on the Sea Islands (a small stripe along the costs of Georgia and South Carolina), where blacks were allowed to keep the confiscated land (Ransom and Sutch, 2001, p. 82).

Sutch, 2001, p. 87).[21] That landownership remained extremely concentrated following the Civil War is quite well documented. For example, see the evidence in Shugg (1937) based on Louisiana tax records, and in Wiener (1976) as well as in Ransom and Sutch (2001) based on the real estate holdings reported by the Census enumerators for counties in Alabama.[22] Moreover, there was not only a high degree of persistency in the concentration of landownership, but also persistence in the planter elite's identity. For five black belt counties in Alabama, Wiener (1978, p. 9) finds that 18 of the 25 planters with the largest landholdings in 1870 were in the group of the largest landholders in 1860 and 16 were in the group of the largest landholders in 1850.

Although the planter elite continued to own most of the Southern land, they lost direct control over the black workforce after the Civil War and it became a main challenge for the planter elite to secure black labor (Alston and Kauffman, 2001; Ransom and Sutch, 2001; Naidu, 2010). Planters did not succeed in reintroducing the gang labor system on their plantations (Fogel and Engerman, 1974),[23] they had turned to other labor arrangements such as tenancy and sharecropping (Reid Jr., 1973; Shlomowitz, 1984; Ransom and Sutch, 2001).[24] Southern states responded directly after the Civil War to planters' needs and introduced the so-called Black Codes – mainly vagrancy and anti-enticement laws – which intended to keep black labor immobile (Wilson, 1965; Cohen, 1976).[25] The planter elite also used violent de facto power to keep black labor working on their fields. For example Wiener (1978, p. 62) writes that *"Planters used Klan terror to keep blacks from leaving the plantation regions, to get them to work, and keep them at work, in the cotton field"*, see also Trelease (1971) and Wiener (1979). Facing the potential threat of violence, freedmen often agreed to keep working on plantations in exchange for protection from Klan terror and other threats (Alston and Ferrie, 1985, 1993, 1999). Alston and Ferrie argue that planters with political influence protected the freedmen and also provided amenities such housing or medical care with the aim of

[21]Mississippi even enacted a law to prohibit black landownership after the Civil War. This law was however quickly overturned by the Freedmen's Bureau (Ransom and Sutch, 2001, p. 87).

[22]Further studies with similar findings on the persistence of landownership are for example Huffman (1974) and Billings (1979).

[23]Engerman and Fogel argue that large-scale plantations employed slave labor in producing stable crops (rice, tobacco, sugarcane and cotton) during the antebellum period more efficiently by using a gang work system. The gang work system allowed slaveholder to allocate slaves efficiently among jobs. According to Fogel and Engerman (1977) gang work started to yield efficiency gains on plantations with 16 slaves or more. On the profitability of the Southern slavery economy see also David and Temin (1979), Wright (1979), and more recently Acemoglu and Wolitzky (2011).

[24]The efficiency of new labor arrangements such as sharecropping is discussed in Reid Jr. (1973), DeCanio (1974), Higgs (1977), and Ransom and Sutch (2001).

[25]Despite the Black Codes were repealed by the 14th amendment during Reconstruction, they were largely reenacted by the Southern Democratic party (Redeemers) after Reconstruction. For example Naidu (2010) shows that the enforcement of anti-enticement laws effectively mitigated recruitment difficulties of planters between 1875 and 1930 by depressing labor mobility and wages.

reducing monitoring costs and labor turnover.[26] Since establishing such so-called paternalistic arrangements was cheaper at larger scale and required political influence they were mostly used by wealthier planters.[27]

On the political side, the planter class primarily found its representation in the Southern Democratic Party, which had the objective to restore the "old Southern system" (Key, 1949; Foner, 1988; Stampp, 1965). When the Democratic party – the so-called Redeemers – gradually regained control over Southern politics in the late 1870s, they started to cut taxes and introduced labor and tenancy laws in favor of the landowners (Woodman, 1995; Foner, 1988). Most of the Southern states also reintroduced some of the former Black codes and imposed voting restrictions such as literacy tests and poll taxes, which restricted the political participation of blacks (Key, 1949; Kousser, 1974; Woodward, 1951).[28]

Acemoglu and Robinson (2006, 2008a,b) argue that the Southern elite's exercise of de facto power after the Civil War explains why economic or policy outcomes in the US South were invariant to changes in de jure institutions.[29] It required a series of adverse economic shocks – for example, the boll weevil infestation starting around 1890 (Lange et al., 2009), the Great Mississippi Flood in 1927 (Hornbeck and Naidu, 2012), the extension of the railroad system to the Deep South (Wright, 1986) and the demand for labor during wartime (Henri, 1975; Grossman, 1991) – combined with the introduc-

[26]With the rise of tenancy and sharecropping were many (black) farmers also often tied to their landlords and local merchants by the way they had to finance their business (Ransom and Sutch, 1972, 2001). Local merchants – frequently with strong social ties to the planter class and in many cases the same person as the landlord – supplied credit to small farmers which were in general secured by crop liens. The credit conditions imposed by the merchants forced many of the tenants and sharecroppers into a from of debt peonage, see, for example, Ransom and Sutch (1972, 1975, 2001) and Wiener (1975).

[27]According to Alston and Ferrie (1985, 1993, 1999) emerged paternalistic arrangements as a response to the planter's problem to secure a stable labor supply after the Civil War. It was in the interest of the planters to use their political influence to make paternalistic arrangements more valuable. Planters used their local political power (e.g. by influencing county courts and police) to ensure security for their black workforce. Outside the plantation created the planter elite a hostile legal environment (e.g. black disenfranchisement, low public spending for education, or anti-enticement laws) to impose external threats to black workers with the aim to impede their mobility.

[28]Feldman (2004) documents a drop of registered voters between 1900 and 1903 from 79,311 to just 1,081 in fourteen Black Belt counties of Alabama. More recent studies are Chay and Munshi (2012), who analyze the link between political participation and black mobilization around the Reconstruction Era and Naidu (2012), who examines the political and economic effects of black disenfranchisement in the US South during the 19th century.

[29]More evidence on planters' activities after the Civil War can be found, for example, in Alston and Ferrie (1985), Billings (1979), Shifflett (1982), Wiener (1976, 1978), and Wayne (1983). Moreover, a huge literature studies the economic consequences of emancipation and the subsequent development of the Southern economy after the Civil War (e.g. Engerman, 1971; Higgs, 1971; Goldin, 1973; Ransom and Sutch, 1975, 1979; Irwin, 1994). At the beginning of the 20th century the South was poor and representative of this consensus view like Ransom and Sutch (2001, pp. 174-176) describe the Southern economy as underdeveloped.

tion of immigration restrictions at the end of World War I to trigger black migration to the northern US states at a large scale[30] and a gradual decline of the planter elite's economic and political power (Alston and Ferrie, 1985, 1993, 1999).[31] Still it took until the 1940s for the Southern states to escape the post-emancipation equilibrium and starting to converge towards the productivity levels of other US regions (Wright, 1986, 1999).

1.3 Data

My measure of the planter elite's ability to exercise de facto power in a county is their relative personal wealth. To calculate the pre-Civil War planter elite's relative personal wealth across counties, I use the US Census to compile an individual-level database on the personal wealth of members of the planter elite – defined as planters who owned at least 100 slaves – just before the American Civil War (1860).[32] The US Census of 1860 reports personal data such as name, address, place of birth, value of real and personal estate and profession of each free person and, in a separate slave schedule, slaveholders are listed together with the slaves they own. This allows me to identify members of the planter elite and their personal wealth. According to the aggregated county statistics of the 1860 Census there are approximately 2,350 slaveholder in the planter elite as I define it. My database contains individual-level information on about 85 percent of these slaveholders.[33]

To compile the individual-level dataset on large planters from the US Census files, I work with the genealogical website *Ancestry.com*. This website provides digitized images from all Census records before 1940 (including the slave schedules), and offers a search engine to locate the slaveholders by first, middle and last name, birthplace and year as well their place of residence. To identify the slaveholders with more than 100 slaves I counted the number of slaves owned by each slaveholder listed in the 1860 slave schedules. I then matched the names of the slaveholders in the slave schedule to the corresponding names reported in the schedule of free inhabitants. For some cases

[30] Before World War I, the Kansas Exodus of 1879 is the only known larger scale migration response of Afro-Americans to violence, bad labor conditions, and the loss of civil rights and political representation brought by the Redeemers in the US South (Painter, 1976). Estimates range between 15,000 to 60,000 black migrants (Van Deusen, 1936).

[31] For example Alston and Ferrie argue that the mechanization of the cotton harvest led to a decline of paternalistic arrangements in the US South.

[32] My definition of the planter elite intends to capture the most powerful and wealthiest pre-Civil War planters in the US South and is more narrow than the definitions used in the existing literature, see for example, Fogel and Engerman (1974), Wiener (1976), or Campbell (1982).

[33] I use a less restrictive definition when I could not identify a slaveholder in the Census of 1860. This is the case if there are mistakes by the enumerators like miscounting the number of slaves or if the surname of a slaveholder is impossible to decipher. In this case I include the next largest slaveholder listed in the Census who owns close to 100 slaves (the slaveowner with the smallest holding in my sample lists 81 slaves).

the search engine does not provide correct matches, because of the difficulty to decipher the handwriting of the enumerators. I then tried to match the slaveholders manually. Finally, I collected and entered the value of real and personal estate of each identified slaveholder in my database.

Table 1 reports the descriptive statistics of the members of the planter elite identified in my dataset. In the 1860 Census, the average member of the planter elite was 50 years old, male, worked in the agricultural sector (about 90 percent listed as occupation planter or farmer) and reported on average $101,384 in real estate and $148,598 in personal estate. The average slaveholding was 154 slaves. With $248,320 of total wealth, the average member of the planter elite was 359 times wealthier than an average free person in the US South in 1860 (the mean wealth is $692; the median wealth is zero).[34] My descriptive statistics of the planter elite highlight that a small number of large plantation owners held a disproportionate fraction of wealth in the US South before the Civil War and resonate with the earlier findings of Wright (1970), Wright (1978), Soltow (1971), and Soltow (1975). The planter elite in my sample – 2006 individuals who made up only 0.02 percent of the population of the US South – owned about 6 percent of the Southern wealth in 1860.

To obtain a measure of relative wealth of the planter elite at the county level, I aggregate the personal wealth of the planter elite in each county and divide it by total county wealth as reported in the aggregated county statistics of the US Census in 1860 (Figure 1 shows the spatial distribution of the relative wealth of the planter elite at the county level). Planters are assigned to their counties of residence in 1860. Hence, my measure of relative wealth of the planter elite can be expressed as

$$WealthPE_{c,1860} = \left(\frac{\sum_{p=1}^{P} WealthPE_{pc,1860}}{Wealth_{c,1860}} \right). \tag{1.1}$$

Summary statistics are reported in Appendix Table. The Data Appendix provides a detailed description of all other variables and data sources used in my empirical analysis.

1.4 The Planter Elite and the Southern Economy

I use the following baseline estimating equation to empirically investigate the link between the relative wealth of the pre-Civil War planter elite and local economic development across Southern US counties,

$$ln(y_{cs}) = \alpha + \lambda_s + \beta WealthPE_{cs,1860} + \Gamma X_{cs,1860} + u_{cs}. \tag{1.2}$$

[34] I retrieved the one percent random sample of the free population for the 1860 Census from the IPUMS (http://www.ipums.org/) to calculate the mean and median wealth of the free Southern population.

The dependent variable, $ln(y_{cs})$, stands for the ln total value added per worker which is my measure of labor productivity at the county level. I include state fixed effects, λ_s, to capture unobservable time-invariant state characteristics. The main right-hand side variable of interest is the fraction of 1860 wealth owned by the planter elite in county c, $WealthPE_{cs,1860}$, defined in (1). I also include a set of pre-Civil War county characteristics, $X_{cs,1860}$, such as ln slaves, ln population, and ln area, to control for the extent to which local economies relied on slave labor and the county size.

1.4.1 Direct Effect

Table 2 presents my estimates of the link between the relative wealth of the pre-Civil War planter elite and levels of labor productivity looking at ten-year intervals between 1840 and 1960. The estimating equation is (2) and the method of estimation least squares. Columns (1)-(3) contain the estimates for the years before the Civil War. The estimates show a positive and statistically significant association between the relative wealth of the planter elite and total value added per worker between 1840 and 1860. In columns (4)-(12), I present the results for 1870 to 1960. The link between the relative wealth of the historical planter elite and total value added per worker remains positive in the immediate post-war decades 1870 and 1880, but is statistically insignificant. In 1890 there is a flip in the sign of the estimated coefficient on the relative wealth of the planter elite. Starting in 1900 and until 1950, I obtain a negative and statistically significant link between the relative wealth of the historical planter elite and total value added per worker. The point estimate on the relative wealth of the planter elite in 1900 is statistically significant with a p-value of 0.068.[35] And for the period 1920 to 1950 the link between the relative wealth of the planter elite and total value added per worker is at least statistically significant at the 5 percent level. My estimates imply that a two-standard-deviation increase in the relative wealth of the planter elite translates into productivity levels that are about 4 percent lower at the turn of the 19th century and 14 percent lower in 1950. In 1960 the link between the relative wealth of the planter elite and total value added per worker is no longer statistically significant.

Researchers have pointed out that geographic factors affect long-run economic development (e.g. Diamond, 1997; Gallup et al., 1998; Rappaport and Sachs, 2003; Nunn and Puga, 2012). And Engerman and Sokoloff (1997, 2002) argue that climate and soils suitable for the production of plantation crops may have also fostered economic inequality.[36] To address this issue, I add county-specific controls for geography to the baseline estimating equation (2). The set of geographical controls includes mean elevation, standard deviation in elevation, average yearly temperature, average yearly rainfall,

[35] I have no results for the year 1910, since there are no manufacturing data available from the 1910 Census at the county level.

[36] For the relation between geography and economic inequality see, for example, Easterly (2007), Galor et al. (2009), Ramcharan (2010), and Vollrath (2010).

53 different soil types, growing degree days, cotton suitability as well the county's latitude and longitude.[37] Table 3 reports the results on the association between the relative wealth of the planter elite and levels of labor productivity after controlling for geography. Columns (1)-(3) present the results for the decades before the Civil War. The estimated coefficient on the relative wealth of the planter elite between 1840 and 1860 remains positive and statistically significant. In columns (4)-(12) I show the results on the link between the relative wealth of the planter elite and total value added per worker for the 1870-1960 period. The results are similar to Table 2, but quantitatively somewhat stronger. As in the baseline specification the link between the relative wealth of the planter elite and total value added per worker is positive but statistically insignificant in 1870 and 1880. The relationship between the relative wealth of the planter elite and total value added turns negative and statistically significant in 1890, and remains negative and statistically significant for the whole period until 1960. Between 1890 and 1950 the negative association between the relative wealth of the planter and total value added is statistically significant at the 1 percent level. In 1960 the negative association is statistically significant at the 5 percent level. The point estimates imply that a two-standard-deviation increase in the relative wealth of the planter elite translates into productivity levels that are about 7 percent lower at the turn of the 19th century and 27 percent lower in 1950.

To ensure that my results are not driven by the historical specialization in agriculture, and especially in producing large-scale plantation crops, I add a range of controls that are meant to capture differences in the extent of (large-plantation) agriculture across counties in the US South. These controls are the number of slaves working on large plantations, the fraction of land cultivated by large farms and the shares of Southern plantation crops (i.e. the shares of sugar, cotton, rice and tobacco production).[38] Table 4 contains the results on the link between the relative wealth of the planter elite and levels of labor productivity after controlling for geography as well as the historical specialization in large-plantation agriculture. Although the reported coefficient on the relative wealth of the planter elite for the pre-Civil War years remains positive, see columns (1)-(3), the effect is now only statistically significant in 1840. Columns (4)-(12) show the results on the link between the relative wealth of the planter elite and total value added per worker between 1870 and 1960. As in Table 3, there is a positive, but statistically insignificant, association between the relative wealth of the planter elite and total value added per worker in the immediate post-war decades 1870 and 1880. The relationship between the relative wealth of the planter elite and total value added turns negative and statistically significant in 1890. Between 1890 and 1950 the estimated coefficient on the relative wealth of the planter remains negative and at least statistically significant at the 5 percent level. In 1960 the link between the relative wealth of the planter elite and total

[37] I provide a detailed description of each geographic variable and its source in the Data Appendix.

[38] I provide a detailed description of these controls in the Data Appendix.

value added per worker becomes somewhat weaker and is only statistically significant with a p-value of 0.09. My estimates imply that after controlling for geography and historical specialization in large-plantation agriculture, a two-standard-deviation increase in the relative wealth of the planter elite translates into productivity levels that are about 7 percent lower at the turn of the 19th century and 23 percent lower in 1950.[39]

1.4.2 The Planter Elite's Lack of Support for Mass Education

The literature on inequality and growth argues that an unequal distribution of wealth may be a hurdle for economic development because of the elite's reluctance to establish human capital promoting institutions (e.g. Deininger and Squire, 1998; Galor and Moav, 2006; Easterly, 2007; Galor et al., 2009). Hence, I am interested in examining whether counties with a relatively wealthier planter elite before the Civil War had higher illiteracy rates (conditional on pre-Civil War illiteracy rates) following the Civil War and at the beginning of the 20th century. Table 5 presents my estimates of the link between the relative wealth of the pre-Civil War planter elite and illiteracy rates after the Civil War between 1870 and 1930.[40] The estimates are based on estimating equation (2). I use the same set of control variables as in Table 4 and also account for the pre-Civil War illiteracy rate in 1860. Between 1870 and 1930 there is a positive and statistically significant association between the relative wealth of the planter elite and illiteracy. Taken together my results indicate that illiteracy rates after the Civil War fell more slowly in counties with a relatively wealthier planter elite and may suggest that planters delayed the convergence of illiteracy rates in counties where they had more de facto power (wealth) before the Civil War.

Table 6 contains the link between the relative wealth of the planter elite and the fraction of high-school as well as college educated adults in 1940 and 1950. The estimates are based on estimating equation (2) using the same set of control variables as in Table 5. Columns (1)-(2) of Table 6 show that there is a negative and statistically significant association between the relative wealth of the planter elite and the fraction of adults with high-school as well as college education in 1940. Columns (3)-(4) report the estimates for 1950. The link between the relative wealth of the planter elite and the fraction of high-school as well as college educated adults remains negative and statistically significant.

Table 7 presents additional evidence indicating that the planter elite may have used

[39]As further robustness check I include the agricultural employment share and ln acres of farmland in 1860 as additional controls to account for the general historical specialization in agriculture of US Southern counties. The estimates are qualitatively similar to Table 4, but the link between the relative wealth of the planter elite and levels of labor productivity remains positive and statistically significant throughout all the pre-Civil War decades. The results are available upon request.

[40]The US Census reported information on literacy until 1930. Furthermore, there are no literacy data available from the 1890 Census at the county level.

their de facto power to block educational improvements after the Civil War. The Rosenwald Rural Schools Initiative supported the construction of schools for black children in rural areas between 1914 and 1931 to improve their educational attainment. The principle of the Rosenwald Fund was to provide help for communities where they received local support by local blacks, state, and county governments (Aaronson and Mazumder, 2011). One might therefore expect to have fewer Rosenwald schools built in counties were the planter elite had more de facto power to coordinate their resistance against black education. Columns (1)-(2) contain the estimates of the link between the relative wealth of the planter elite and the total number of Rosenwald schools built in the county between 1914 and 1931. The estimates are based again on estimating equation (2) using the same set of control variables as in Table 5. The method of estimation is least squares. Column (1) shows that there is a negative and statistically significant association between the relative wealth of the planter elite and the total number of Rosenwald schools built. Since the Rosenwald Rural Schools Initiative intended to improve black education in rural areas, I also control for the pre-Civil War urban share in column (2). The estimated coefficient on the relative wealth of the planter elite remains negative and statistically significant at the 5 percent level. Hence, counties with a relatively wealthier planter elite before the Civil War were less likely to establish Rosenwald schools for black children. Taken together the results in Table 5-7 suggest that counties with a wealthier planter elite before the Civil War saw less human capital investment after the Civil War and during the first part of the 20th century. This could be a main reason why these counties remained relatively less productive well into the 20th century.

Moreover, the aggregated county statistics of the US Census provide information on the illiteracy of black adult men of voting age (age 21 and over) for 1900 to 1920, around the time when Southern states had introduced voting restrictions based on literacy tests and poll taxes (Key, 1949; Kousser, 1974; Naidu, 2012). In Table 8, I find that there is a positive and statistically significant association between the relative wealth of the planter elite and the fraction of illiterate black men of voting age between 1900 and 1920 using the same set of control variables as in Table 5. This again suggests that the planter elite may have used their de facto power to impede mass education. As an important by-product the planter elite's lack of support of mass education may have also facilitated the exclusion of blacks from political participation. Since many of the planters' political opponents were illiterate they could not interfere with the political goals of the planter elite once voting restrictions based on literacy tests were implemented.

1.4.3 The Planter Elite and the Use of de Facto Power

Violence against Black Officeholders

Intimidation to prevent blacks from participating in the political life after the Civil War was one of the tools used by the planter elite to maintain control over Southern politics and the economy. Foner (1996, p. xxviii) writes regarding black officeholders: *"Numerous Mississippi officials were threatened or driven from their homes during the 1875 campaign [...] Abram Colby, a member of Georgia's legislature was beaten "in the most cruel manner" by Klansmen in 1869. [...] Richard Burke, a minister and teacher in Sumter County, Alabama, who served in the state House of Representatives, was murdered in 1870. [...] In Edgefield County, South Carolina, violence was pervasive throughout Reconstruction."* Overall more than ten percent of the black officeholders were victims of violence during Reconstruction (Foner, 1996, p. xxviii).

Inspired by the anecdotal evidence I use *Foner's directory of Black Officeholders during Reconstruction* to examine whether the use of violence against black officeholder was higher in counties with a wealthier planter elite before the Civil War. This directory recorded over 1,500 black officeholders who served either at the national, state or local level. Foner (1996) also lists the names, county of residence and office positions of black officeholders who were victims of violence during their political career. I use this information to construct two measures of violence against black officeholders. The first measure is a binary variable which is unity if at least one black officeholder was a victim of violence in a county during Reconstruction. My second measure is the total number of black officeholders in a county who were victims of violence during Reconstruction.[41] Column (1) of Table 9 shows the link between the relative wealth of the planter elite and the probability that a black officeholder was a victim of violence using estimating equation (2). The estimated coefficient on the relative wealth of the pre-Civil War planter elite is positive and statistically significant at the 5 percent level. Column (2) reports estimates for the total number of black officeholders in a county who were victims of violence during Reconstruction. The estimated coefficient on the relative wealth of the pre-Civil War planter elite is again positive and statistically significant at the 5 percent level. These results suggest that the planter elite may have used their de facto power to support violent actions against black officeholders.

Political Connections

The journals of the constitutional conventions of several Southern states list the names of all participating delegates together with the counties (districts) they represented. With this information it is possible to evaluate whether the political influence of the planter elite at the county level persisted over time. For Alabama and Mississippi – two Deep South states with cotton-based economies – this information on the delegates can be found in the *Journals of the Proceedings and Debates of the Constitutional Convention* of the states of Alabama (1865, 1875) and Mississippi (1865, 1890).[42] This allows me

[41]Foner's directory has no information on black officeholders for the states of Maryland, Missouri, Delaware and Kentucky.

[42]The journals of the proceedings and debates of the constitutional convention of the state of Mississippi (1865, 1890) report in addition the delegates' age, postoffice, nativity, occupation and political

to investigate the delegates' family connections to the pre-Civil War planter elite. I do this for the delegates that participated in the first constitutional convention after the Civil War as well as for delegates of the first constitutional convention after Reconstruction.

Both states held their first constitutional conventions after the Civil War in 1865. In these conventions the participating delegates introduced the Black Codes and planned the reestablishment of the "old" Southern system. The Black Codes together with the constitutional conventions were suspended by the Reconstruction Acts in 1867 which placed ten former Confederate states under military control and required them to draft a new state constitution.[43] Towards the end or following the Reconstruction period, Alabama held a constitutional convention in 1875 and Mississippi in 1890. These conventions were marked by the Democratic Party's re-establishment of their political control.[44] I use three different selection criteria for the delegates' connections to the pre-Civil War planter elite. First, if the delegate or a direct family member of the same household is listed in the slave schedule as slaveholder (Alternative 1). Second, if the total wealth of the delegate or a direct family member exceeds \$10,000 in 1860 (Alternative 2). The third criteria is a combination of the first two alternatives and requires delegates or a direct family member to have at least \$10,000 of wealth and being listed as slaveholder in 1860 (Alternative 3).[45] I provide a detailed description of the data and how I linked the delegates to the pre-Civil War planter elite in the Data Appendix (pp. 31-36).

Table 10 contains the descriptive statistics for the constitutional conventions for Alabama and Mississippi. In the constitutional convention of 1865, I find that 78 percent of Alabama's delegates (or direct family members) and 69 percent of the delegates in Mississippi were listed as slaveowners in the slave schedules of the Census in 1860. The later constitutional conventions reveal a similar pattern. In Alabama, 73 percent of the delegates of the constitutional convention of 1875 had direct connections to slaveholders in 1860; in Mississippi, 60 percent of the delegates of the constitutional convention in 1890 had direct connections to slaveholders in 1860. Looking directly at whether the reported wealth of a delegate exceeds \$10,000 in the 1860 Census yields similar results. If I use the selection criteria that requires delegates or a direct family member to have at least \$10,000 of wealth and being listed as slaveholder in 1860, I obtain that 63 percent of the county delegates of the constitutional convention of Alabama in 1865 had a family connection to the pre-Civil War planter elite; for Mississippi, the corresponding

preference.

[43] Alabama and Mississippi introduced new constitutions in 1868. I do not consider the constitutional conventions in 1868, because delegates in both states were selected under military supervision.

[44] Foner (1996, Table 1) dates the end of Reconstruction in Alabama in 1874 and Mississippi in 1875.

[45] If it was not possible to identify the delegate or a direct family member in the 1860 Census, but in the 1870 Census instead, I use the wealth reported by the enumerators of the Census for the delegate in 1870. Note, that using the reported wealth in 1870 might result in a under selection of delegates since many of them lost a significant fraction of their personal estate due to the abolition of slavery.

number is 59 percent. In the 1875 constitutional convention in Alabama, 66 percent of the delegates had a family connection to the pre-Civil War planter elite; in the 1890 constitutional convention in Mississippi, 52 percent of the delegates had a family connection to the historical planter elite.

To examine the delegates' connection to the pre-Civil War planter elite at the county level in Alabama and Mississippi, I construct a binary variable, PC_{cs}, for each county in Alabama and Mississippi that takes the value of unity if at least one delegate in both constitutional conventions had a family connection to the planter elite using the most stringent selection criteria (Alternative 3). This indicator variable should reflect the political influence of the pre-Civl War planter elite in the constitutional conventions. I then investigate whether rich delegates with family connections to the planter elite were more likely in counties with a relatively wealthier planter elite using the following estimation equation

$$PC_{cs} = \alpha + \lambda_s + \beta WealthPE_{cs,1860} + \gamma Delegate_{cs} + \Gamma X_{cs,1860} + u_{cs}. \tag{1.3}$$

The parameters λ_s are state fixed effects, and the variable of interest, $WealthPE_{cs,1860}$, is defined in (1). As controls I include the average number of county delegates, $Delegate_{cs}$, as well as the ln population and ln area, denoted by $X_{cs,1860}$, to control for the county size. The method of estimation is probit.

In column (1) of Table 11, I show that there is a positive and statistically significant association between the probability that a county is politically captured by the planter elite and the relative wealth of the planter elite.[46] The estimated coefficient on the relative wealth of the planter elite is statistically significant at the 1 percent level. In addition, I re-estimate equation (4) using a county panel specification

$$PC_{cs,t} = \alpha + \lambda_{s,t} + \beta WealthPE_{cs,1860} + \gamma Delegate_{cs,t} + \Gamma X_{cs,1860} + u_{cs,t}. \tag{1.4}$$

The dependent variable, $PC_{cs,t}$, is again a binary variable that is equal to unity in year t if at least one delegate in the county was listed as a slaveholder and reported more than \$10,000 of wealth in the 1860 Census (Alternative 3). I replace the state fixed effects, λ_s, by time varying state fixed effects, $\lambda_{s,t}$, which capture observable and unobservable time varying characteristics at the state level.[47]

Column (2) of Table 11 reports the link between the relative wealth of the planter elite and the probability of having at least one county delegate with family connections

[46] I obtain qualitatively similar results when using Alternative 1 and Alternative 2 for the construction of the indicator variable, instead. These results are available upon request.

[47] The constitutional conventions after the Civil War in 1865 are coded as $t = 1$ and the constitutional conventions in 1875 (AL) and in 1890 (MS) are coded as $t = 2$.

to the planter elite using estimating equation (5). The estimated coefficient on the relative wealth of the planter elite is positive and statistically significant at the 5 percent level. Since some counties were allowed to send more than one delegate to the constitutional conventions, I also examine the link between the number of rich delegates with family connections to the pre-Civil War planter elite and the relative wealth of the planter elite before the Civil War. Column (3) reports the least squares results using the same right-hand-side controls as in column (2). The estimated coefficient on the planter elite's relative wealth is positive and statistically significant at the 1 percent level. Hence, there is a positive and statistically significant association between the relative wealth of the pre-Civil War planter elite and the number of delegates sent to the constitutional conventions that had family connections to planter elite. In line with Acemoglu and Robinson (2006, 2008a,b), my findings suggest that the planter elite used their de facto power to capture local politics in order to preserve a planter-friendly political system in the post-Civil War South.

Land Prices

The planter elite's ability to exercise de facto power should have allowed them to better defend their interests during the Reconstruction period when they had less de jure power. Since historians have documented that planters maintained land ownership after the Civil War (e.g. Shugg, 1937; Wiener, 1976; Ransom and Sutch, 2001), a main objective of the planter elite should have been to preserve their rents from land. As land prices can be taken to capitalize agricultural profits (e.g. Plantinga et al., 2002; Deschênes and Greenstone, 2007), the planter elite's capacity to defend their (agricultural) interests should show in land prices. To explore whether wealthier planters were better able to defend their interests in times with less de jure power, I therefore compare the evolution of land prices at the county level during Reconstruction, when legal reforms like the abolition of slavery or the enfranchisement of freemen for example brought losses in the elite's de jure power, with the period when Southern states overrode the Reconstruction conventions and planters recouped de jure power. My estimating equation is

$$ln(LP_{cs,t}) = \lambda_c + \lambda_{s,t} + \beta TE_{s,t} \times WealthPE_{cs,1860} + u_{cs,t}. \tag{1.5}$$

The dependent variable, $ln(LP_{cs,t})$, stands for the ln value of farmland per acre in county c of state s in year t. I also include county fixed effects, λ_c, and time varying state fixed effects, $\lambda_{s,t}$, to capture time-varying state characteristics. $TE_{s,t}$ is a binary variable that takes the value one for all years after the Civil War *and before* the state overrode its Reconstruction convention (the direct effect of the treatment effect, $TE_{s,t}$, is captured by the time-varying state fixed effects).[48] The main variable of interest,

[48]For estimating equation (6), I consider a state in the Reconstruction period until the state overrode its

$TE_{s,t} \times WealthPE_{cs,1860}$, denotes the interaction of the treatment effect and the relative wealth of the planter elite. If the planter elite was better able to sustain land prices during Reconstruction in counties where they had more de facto power, the estimated coefficient on the interaction term should be positive.

Panel A of Table 12 contains my results on the link between land prices and the planter elite's wealth during Reconstruction and following the adoption of the new constitutions for the decades 1870 to 1930. The method of estimation is least squares. Column (1) shows that during Reconstruction land prices were relatively higher in counties where the planter elite had more de facto power. The estimated coefficient on the interaction term is statistically significant at the 1 percent level. Columns (2)-(3) report the results when I also interact the treatment effect with other pre-Civil War county characteristics, like the reliance on slave labor and county size in column (2) and variables capturing the historical specialization in plantation agriculture in column (3). The estimated coefficient on the relative wealth of the planter elite remains statistically significant at the 1 percent level in both cases.

I also estimate a version of equation (6) that focuses on the sample of contiguous counties that lie on the opposite sides of state borders. The advantage of comparing only contiguous border counties is their similarity, which mitigates the concerns related to the heterogeneity between treatment and control group. To implement this so-called border county approach I need to modify estimating equation (6) by including additional time varying border segment fixed effects. These border segment controls account for common observable and unobservable factors that vary across state border segments over time. The new estimation equation is

$$ln(LP_{bcs,t}) = \lambda_c + \lambda_{b,t} + \lambda_{s,t} + \beta TE_{s,t} \times WealthPE_{cs,1860} + u_{bcs,t} \tag{1.6}$$

where the main difference to estimation equation (6) is the inclusion of time varying border segment fixed effects $\lambda_{b,t}$ and restricting the sample to border counties.[49] Figure 2 highlights the border counties used in my empirical analysis.[50]

Panel B of Table 12 shows the estimates for the border county approach for 1870 to 1930. The method of estimation is least squares.[51] The results reported in column

Reconstruction convention. A list of the timing of the first constitutional conventions after Reconstruction of each Southern State is available from the author upon request.

[49]The border county approach follows closely the regression discontinuity design of Black (1999), Dube et al. (2010), Fack and Grenet (2010), and Naidu (2012) for example.

[50]Note that a border county can be in multiple border segments.

[51]To account for within-state over time and within border segment over time correlations I use a two-dimensional clustering at the state and border segment level, see Cameron et al. (2011) for more information on multiway clustering. Hence, my estimates are robust to arbitrary correlation across counties in each US state and across counties in each border segment. The two-dimensional clustering accounts also for the mechanical correlation induced by the presence of a single county in multiple border segments (see e.g. Dube et al., 2010 and Naidu, 2012).

(1)-(3) are qualitatively similar to Panel A. The estimated coefficient on the interaction term is at least statistically significant at the 5 percent level. The estimates continue to indicate that during Reconstruction, land prices were relatively higher in counties with a wealthier planter elite. This suggests that the planter elite's de facto power allowed them to capture local institutions for their own interest until the new constitutions restored some of their de jure power.

Paternalism

My findings on land prices are consistent with the so-called paternalistic view of the behavior of the planter elite after the Civil War discussed in Alston and Ferrie (1985, 1993, 1999). According to this view, plantation owners offered blacks a set of amenities – such as protection from violence, improved housing or medical care – in exchange of contractual arrangements that were favorable for plantation production. Alston and Ferrie argue that these paternalistic arrangements were easier to establish by wealthier planters because they required political influence. In Panel A of Table 13, I examine whether counties with a wealthier planter elite saw an increase in the production of plantation crops relative to all other main field crops grown in the US South (corn, wheat, barley, rye, oats and sweet potatoes) during Reconstruction using the difference-difference approach of subsection 4.3.3. The estimating equation is (6) and the method of estimation is least squares. Column (1) shows that counties where the planter elite had more de facto power before the Civil War experienced a relative increase in the production of plantation crops compared to other main field crops during Reconstruction. The estimated coefficient on the interaction term is statistically significant at the 1 percent level. This positive association remains statistically significant in columns (2)-(3) where I control for interactions between the treatment effect and pre-Civil War county characteristics like the reliance on slave labor, size and historical specialization in plantation agriculture. Panel B of Table 13 contains the qualitatively similar results using the border county approach based on estimating equation (7).

An important amenity included among the planters' paternalistic arrangements was security from violence and lynching. I calculate the total number of lynchings between 1882 and 1930 for each county in the US South using the dataset of the Historical American Lynching Data Collection Project (HAL) to examine whether there was more security from violence and lynching in counties with a wealthier planter elite. The HAL contains historical data on individual lynchings in ten Southern states between 1882 and 1930.[52] Table 14 contains the results for the link between the planter elite's relative wealth and the number of lynchings at the county level. The estimation equation is (2) and the method of estimation is least squares. I include the same set of control variables as previously in Table 4. Column (1) shows that the estimated coefficient on the relative

[52]The data are available at http://people.uncw.edu/hinese/HAL/HAL%20Web%20Page.htm.

wealth of the pre-Civil War planter elite is negative and statistically significant at the 1 percent level. In column (2), I restrict the sample to the 1882-1900 period before most of the Southern states introduced barriers to voting. As before, there is a strong negative and statistically significant association between the relative wealth of the pre-Civil War planter elite and the number of lynchings. This suggest that – consistent with the so-called paternalistic view – a relatively wealthier planter elite may have used their de facto power to offer black workers protection from violence.

Planters with more de facto power may have preferred to establish contractual arrangements with black tenants, because the additional paternalistic goods they could offer like protection from violence or housing were especially attractive to them (Alston and Ferrie, 1985, 1993, 1999).[53] In Table 15, I examine whether there was a greater share of black tenants in counties with a relatively wealthier planter elite. My estimates are based on estimation equation (2) and the method of estimation is weighted least squares with weights equal to the farmland of counties. I include the same set of control variables as previously in Table 4. Column (1) contains the results on the link between the relative wealth of the planter elite and the share of black tenants in 1900. The estimated coefficient on the relative wealth of the planter elite is positive and statistically significant at the 1 percent level. Column (2) and (3) divide the tenants into cash or share tenants, respectively. I find that there is a positive and statistically significant association between the relative wealth of the planter elite and the share of black cash tenants in column (2). There is also a positive but somewhat weak link between planters' wealth and the fraction of black share tenants in column (3). The p-value of the point estimate is 0.08. My empirical evidence suggests that there existed relatively more contractual arrangements with black tenants in counties with a relatively wealthier planter elite before the Civil War.

1.4.4 Further Issues: Measuring Inequality

One important difference between my work and the recent contributions of Nunn (2008), Galor et al. (2009), Ramcharan (2010), and Rajan and Ramcharan (2011) to the literature on the effects of wealth inequality on economic development in the US is that my measure of wealth inequality is based on personal wealth data, whereas these studies use measures of wealth inequality based on data on farm sizes. There are several reasons why for my purposes it is preferable to use data on the personal wealth of planters. First, the data on farm sizes does not refer to farm ownership, but to the farm as a unit of production. Historians have documented that farm tenancy was not an unusual form

[53] Alston and Ferrie (1993, footnote 17) argue that paternalistic arrangements may have been cheaper for planters than using cash. Hence, these arrangements can be regarded as a possibility for planters with more de facto power to funnel rents away from black tenants. For example, Alston and Ferrie (1999, p.30) interpret the higher cash rents per acres paid by black compared to white tenants as evidence of a "paternalism premium" for receiving protection.

of contractual arrangement in Southern agriculture even before the Civil War (Reid Jr., 1976; Winters, 1987; Bolton, 1994). Tenancy rates for several counties in Georgia before the Civil War varied between 3 to 40 percent for example (Bode and Ginter, 2008). Hence, farms might have been operated by different tenants but owned by the same person. Since wealthy planters often owned more than a single plantation (see e.g. Oakes, 1982; Rowland et al., 1996; Scarborough, 2006), an inequality measure based on farm sizes would tend to underestimate their relative wealth. Second, compared to wealth inequality measures based on farm sizes, my wealth inequality measure also reflects the value of land. This is important if the planter elite also tended to own the most valuable land. Third, the data on farm sizes would not allow me to identify the landholdings of the planter elite – the group of interest in my paper.

Another important difference between my work and the existing work on the effects of wealth inequality in the US South is that my measure of wealth inequality is meant to proxy for the capacity of the elite to repress the rest of the society if it is in their interest, as emphasized by Engerman and Sokoloff (1997, 2002), Acemoglu et al. (2005), and Acemoglu (2008). Whereas the measures of the unequal distribution of farm sizes used by Nunn (2008), Ramcharan (2010), and Rajan and Ramcharan (2011) seem better suited as a proxy of the distribution of de facto power among landowners. Still, in Panel A of Table 16, I replace my measure of the planter elite's relative wealth in estimating equation (2) with the Gini coefficient implied by the farm size distributions in each county.[54] This specification does not yield a statistically significant association between inequality and levels of labor productivity after the Civil War. This continues to be the case when I control for geography and historical specialization in plantation agriculture, see Panel B of Table 16. Finally, I simultaneously include my measure of the relative wealth of the pre-Civil War planter elite and the Gini coefficient implied by the farm size distributions in Panel C of Table 16, controlling also for geography and historical specialization in plantation agriculture. The estimates on the Gini coefficient remains insignificant, whereas there is a negative and significant association between the relative wealth of the planter elite and levels of labor productivity after the Civil War. Overall my results suggest that using the relative wealth of the planter elite as a measure of wealth inequality is key for my empirical findings.[55]

[54]The Gini coefficient implied by the farm size distributions for 1860 is retrieved from Nathan Nunn's webpage: http://www.economics.harvard.edu/faculty/nunn/data_nunn.

[55]My main result using the measure of the relative wealth of the planter elite as proxy for their de facto power is also robust to including other measures of wealth inequality based on data on farm sizes such as the share of big farms, the fraction of land cultivated by large farms (already included as control variable in my analysis) and the number of large farms constituting 20 percent of all land for example. The results are available upon request.

1.5 Conclusion

I document that the great concentration of wealth in the hands of the planter elite before the American Civil War appears to have been detrimental for subsequent local economic development within the US South. To capture the planter's elite potential to exercise de facto power, I construct a new dataset on the personal wealth of the richest Southern planters before the Civil War. My estimates imply that the planter elite's ability to exercise de facto power at the local level – proxied by their relative wealth in the county – adversely affected levels of labor productivity in the post-war decades and even after World War II. After controlling for geography and historical specialization in large-plantation agriculture, I find that a two-standard-deviation increase in the relative wealth of the planter elite translates into productivity levels that are about 7 percent lower at the turn of the 19th century and 23 percent lower in 1950.

I argue that the negative association between the relative wealth of the historical planter elite and the long-run economic development of counties in the US South is the likely consequence of the planter elite's lack of support for mass schooling. My results indicate that illiteracy rates after the Civil War fell more slowly in counties with a relatively wealthier planter elite, and that these counties also saw a smaller share of the population attending high school or college in the beginning of the 20th century. I also show that counties with a richer planter elite before the Civil War were less likely to establish so-called Rosenwald schools for black children. My results suggest that more economically powerful planters may have undermined blacks and poor whites capacity to accumulate human capital by delaying the establishment of human-capital promoting institutions.

My results indicate – in line with Acemoglu and Robinson (2006, 2008a,b) – that the planter elite's de facto power allowed them to maintain their economic and political status after the Civil War. In response to legal changes like the abolition of slavery and black enfranchisement during the Reconstruction period, the planter elite used their de facto power to repress black politicians. I also find that the Reconstruction policies failed to curb the political connections of the planter elite. The US South remained a captured economy well into the 20th century with the most adversely affected places being counties where the planter elite was relatively powerful before the Civil War.

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1.7 Data Appendix

Dependent Variables

Dependent Variables (1)

VARIABLE	YEARS	DESCRIPTION
Total Value Added per Worker	1860-1960	Total value added per worker in logarithmic units is constructed as the sum of manufacturing value added (i.e. output minus the cost of materials) and agricultural output per worker (in the agricultural and manufacturing sector) at the county level. The data for manufacturing value added and agricultural output come from the US Census and are retrieved from the ICPSR file 2896 (Haines, 2010). For 1910, there are no manufacturing Census data available at the county level. The data for agricultural workers come from Craig and Weiss (1998) for the years 1860 to 1900. For the years 1910 to 1920 data are taken from the IPUMS-US database by aggregating information from variable *ind1950* on agricultural workers (industry classification 105) at the county level (Ruggles et al., 2010). The Census data on rural farm population is used as proxy for agricultural workers for the years 1930 to 1960 (ICPSR 2896). Data on manufacturing workers are retrieved form the ICPSR 2896 file.
Illiteracy Rate	1870-1930	Fraction of the population who cannot read and write. For the years 1870 and 1880, I construct the illiteracy rate using the Census data provided by the IPUMS-US. In 1890, there is no information on literacy at the county level. For the years 1900 to 1930 the illiteracy rate is retrieved from the ICPSR 2896 file. The US Census 1900-1920 provides also information on illiterate males of voting age by race at the county level (ICPSR 2896).
Number of Rosenwald Schools	1914-1931	Data on Rosenwald Schools are retrieved from the data archive of Aaronson and Mazumder (2011) provided by the Journal of Political Economy. The dataset contains all Rosenwald Schools which opened during the period 1914-1931 at the county level. For more information see Section 4.2 and Aaronson and Mazumder (2011).

Dependent Variables (2)		
Share of Adults with Higher Education	1940-1950	Fraction of adults over age 25 with high school or college degree at the county level (ICPSR 2896).
Violence against Black Officeholder	1865-1877	I use Foner's (1996) directory of black officeholder during Reconstruction to compile data on black officeholders who became victims of violence during Reconstruction (see also Section 4.3.1).
Members of the Constitutional Convention	1865-1890	The Journals of the Proceedings and Debates of the Constitutional Convention for the states of Alabama (1865, 1875) and Mississippi (1865, 1890) listed for each constitutional convention the name of each participant and the corresponding county (district) the delegate represented. I connect the listed delegates to the nearest compiled US Census before or after a constitutional convention took place to obtain additional personal information about the delegates such as their age, birthplace, occupation and the birthplace of their parents (for the delegates of the constitutional convention in 1865 I use either the Census of 1860 or 1870, for the convention in Alabama in 1875 the Census of 1870 or 1880, and for the convention in Mississippi in 1890 I use the Census of the same year). Afterwards, I link the delegates to the Census of 1860 in order to verify whether the delegate himself or a direct family member – i.e. the delegate's wife, his parents or any other relative listed in the *same* household – is connected to the planter class (I do not consider whether the delegate had a connection to the planter class via siblings or other relatives such as uncles or the father in law unless they are listed in the same household as the delegate in 1860. My genealogical research is therefore a conservative estimate of the delegates' family connections to the planter class.) If it is impossible to identify the delegate or its direct family members in the 1860 Census but in the 1870 Census instead, I use the wealth reported by the enumerators of the Census for the delegate in 1870 (note, that using the reported wealth in 1870 might result in a under selection of delegates since many of them lost a significant fraction of their personal estate due to the abolition of slavery). See Section 3 for a detailed description of the 1860 Census.

Dependent Variables (3)		
Values of Farmland per Acres in $	1860-1930	Values of farmland and buildings per acre (ICPSR 2896, part 106).
Plantation Crops Ratio	1870-1900	The ratio of plantation crops (cotton, sugarcane, rice and tobacco) to non-plantation crops (wheat, rye, corn, oats, sweat potato and barley) is calculated as the sum over each individual planation crop measured by quantity and multiplied by its price divided by the sum over each non-plantation crop measured by quantity and multiplied by its price (I use the individual crop prices of 1860), see ICPSR file 2896.
Number of Lynchings	1880-1930	I construct the number of lynchings at the county level using the information provided by the Historical American Lynching Data Collection Project (HAL). For more information see also Section 4.3.4 and the HAL website http://people.uncw.edu/hinese/.
Share of Black Tenants	1900	The share of black tenants in 1900 (ICPSR 2896).

Control Variables

Control Variables (1)		
VARIABLE	YEARS	DESCRIPTION
Population	1860	Total county population in 1860 (ICPSR 2896).
Slaves	1860	Total number of slaves in a county in 1860 (ICPSR 2896).
Area	1880	County area in square miles (ICPSR 2896).
Slaves on Large Plantations	1860	Number of slaves living in holdings with 50 and more slaves. I assume that these slaves work on large plantations. I obtain the number of slaves living in large holdings by taking the median bin size of slaves in every slaveholder category above 50 slaves multiplied by the number of slaveholders in each category. The slaveholder categories with more than 50 slaves listed in the Census are the number of slaveholders with 50-69, 70-99, 100-199, 200-299, 300-499, 500-999, and with 1000 and more slaves (see ICPSR 2896). The variable is taken in logarithmic units as ln (Slaves Large Plantations + 1).
Share of Farmland Large Farms	1860	The fraction of farmland cultivated by large farms is obtained by using the median bin size of farmland multiplied by the number of farms of the following categories: farms with 500 to 999 acres and farms with 1000 acres and more (see ICPSR 2896). For the latter category, I assume that each farm is exactly 1000 acres.

Control Variables (2)

VARIABLE	YEARS	DESCRIPTION
Share Cotton	1860	The share of cotton production in 1860 is calculated as the value of cotton output in bales over the total sum of the following crops produced in a county: tobacco, rice, cotton, sugarcane, wheat, rye, corn, oats, sweat potato and barley. To obtain the crop output in dollars, I multiply each crop with its price in 1860 (see ICPSR 2896).
Share Sugar	1860	The share of sugarcane production in 1860 is calculated as the value of sugarcane output in hogsheads over the total sum of the following crops produced in a county: tobacco, rice, cotton, sugarcane, wheat, rye, corn, oats, sweat potato and barley. To obtain the crop output in dollars, I multiply each crop with its price in 1860 (see ICPSR 2896).
Share Rice	1860	The share of rice production in 1860 is calculated as the value of rice output in pounds over the total sum of the following crops produced in a county: tobacco, rice, cotton, sugarcane, wheat, rye, corn, oats, sweat potato and barley. To obtain the crop output in dollars, I multiply each crop with its price in 1860 (see ICPSR 2896).
Share Tobacco	1860	The share of tobacco production in 1860 is calculated as the value of tobacco output in pounds over the total sum of the following crops produced in a county: tobacco, rice, cotton, sugarcane, wheat, rye, corn, oats, sweat potato and barley. To obtain the crop output in dollars, I multiply each crop with its price in 1860 (see ICPSR 2896).

Control Variables (Continued)

Climate Data: The precipitation and temperature data come from the PRISM climate group.[56] The PRISM provides for each month since 1895 precipitation and temperature data for the Conterminous United States at a 4x4 km grid size. For each month since 1895 the grid data is mapped into counties which yields to monthly precipitation and temperature data at the county level using historical county boundaries. For this paper, I use the average yearly temperature and average yearly precipitation over the period 1895-2000 to proxy for average climate characteristics at the county level. Since cotton production was one of the most salient agricultural features in the US South during the 19th century, I control also for the growing degree days and a measure of cotton suitability at the county level. Growing degree days are an important information for crop choice and are calculated as the time span between last frost in spring and first frost in fall measured in days using the Julian calendar. Cotton suitability measures the suitability of rainfall in a county for cotton production. For each county, I include also the geographical coordinates (latitude and longitude) of the county centroids. The measures of the growing degree days, cotton suitability and the geographical coordinates are downloaded from Dietrich Vollrath's website.[57] A detailed description of the datasource used and the construction of the cotton suitability measure and the growing degree days can be found in Vollrath (2010, pp. 32-34).

Soil and Elevation Data: The soil data comes from the United States Department of Agriculture SSURGO database and contains the soil taxonomy at different resolutions for the Conterminous United States.[58] For this paper I use the soil types at the suborder level. The suborder level classifies the soil types into 53 different categories. The soil data is mapped into counties using historical county boundaries. With this information at hand I construct the corresponding share of land of a county which falls into different soil categories. The Environmental System Research Institute (www.esri.com) is the source of the elevation data. In the paper I control for average elevation and the standard deviation of elevation at the county level.[59]

[56] www.prism.oregonstate.edu.

[57] For more details see https://sites.google.com/site/dietrichvollrath/Home/geogwealth.

[58] For more details see http://soils.usda.gov/surveys/geography/ssurgo/.

[59] I thank Antonio Ciccone for sharing the climate, soil and elevation data.

1.8 Figures and Tables

TABLE 1
DESCRIPTIVE STATISTICS: MEMBERS OF THE PLANTER ELITE

Members of the Planter Elite	***Obs***	***Mean***
Age	1874	50.77
Real Estate in 1860 Dollars	1970	$101,384.30
Personal Estate in 1860 Dollars	1963	$148,597.90
Slaves Owned	2006	154.21
Gender	***Freq.***	***Percent***
Female	135	7.11
Male	1765	92.89
Birthplace	***Freq.***	***Percent***
South Carolina	494	27.23
Virginia	249	13.73
Georgia	233	12.84
North Carolina	221	12.18
Mississippi	153	8.43
Louisiana	145	7.99
Tennessee	63	3.47
Alabama	57	3.14
Kentucky	40	2.21
Maryland	39	2.15
Other	120	6.62
Occupation	***Freq.***	***Percent***
Farmer	897	47.51
Planter	806	42.69
Estate	42	2.22
Merchant	24	1.27
Lawyer	23	1.22
Physician	21	1.11
Other	75	3.97
Wealth of Planter Elite vs. Average Free Person		
Wealth Average Member of the Planter Elite		$248,319.80
Wealth Average Free Person		$692.00
Ratio		358.84
Comparison to US South Population		**Percent**
Fraction of Wealth Owned		6
Fraction of Total Population		0.02

Figure 1.1: Relative Wealth of the Planter Elite in 1860

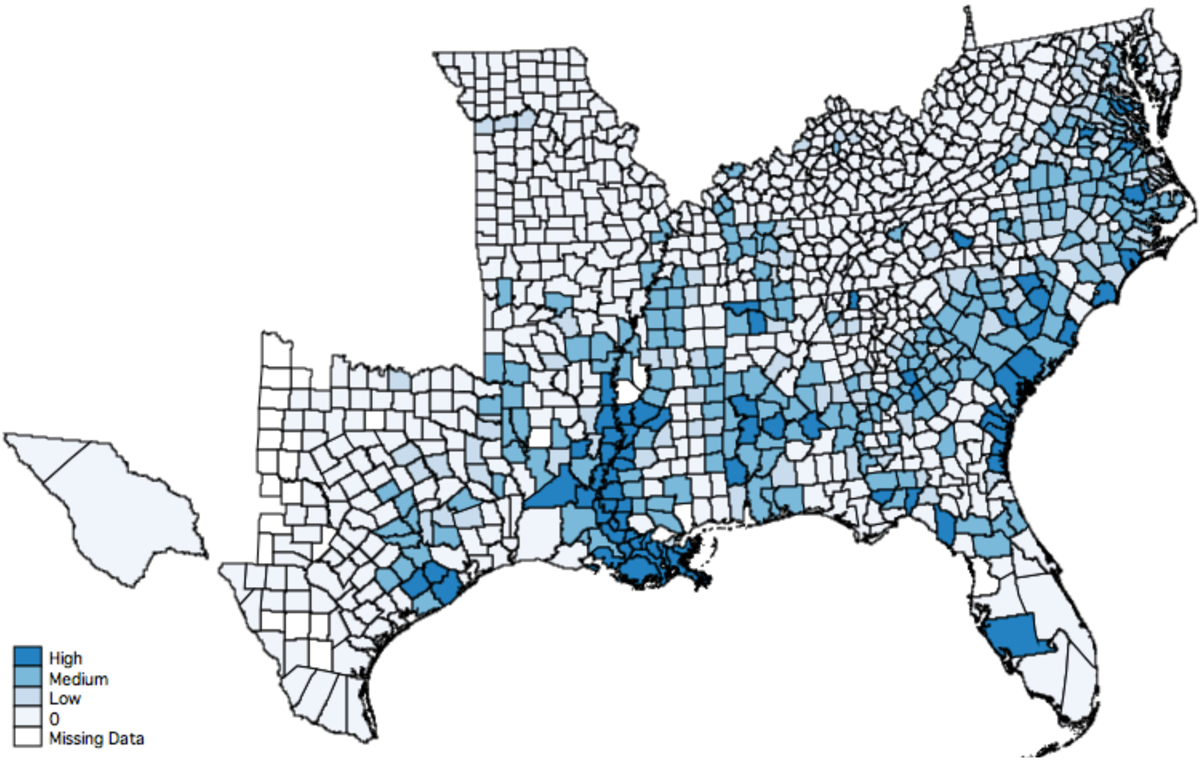

TABLE 2

THE PLANTER ELITE AND SOUTHERN PRODUCTIVITY LEVELS, 1840-1960

	LN(Total Value Added per Worker)											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	1840	1850	1860	1870	1880	1890	1900	1920	1930	1940	1950	1960
$WealthPE$	0.475*	0.548***	0.298*	0.0913	0.267	-0.160	-0.260*	-0.873***	-0.943**	-0.801**	-0.859**	-0.181
	(0.267)	(0.210)	(0.174)	(0.172)	(0.181)	(0.158)	(0.143)	(0.223)	(0.393)	(0.343)	(0.391)	(0.402)
$Ln(Slaves)$	-0.0307	-0.0312	0.0393**	0.106***	0.0867***	0.0677***	0.0423***	0.0581***	-0.00516	0.0642**	0.106***	0.0768**
	(0.0308)	(0.0219)	(0.0185)	(0.0373)	(0.0227)	(0.0197)	(0.0149)	(0.0191)	(0.0367)	(0.0318)	(0.0356)	(0.0344)
$Ln(Population)$	0.218***	0.257***	0.192***	0.0928	0.190***	0.157***	0.106***	-0.0310	0.395***	0.188**	0.203***	0.162**
	(0.0631)	(0.0566)	(0.0460)	(0.0905)	(0.0413)	(0.0398)	(0.0379)	(0.0463)	(0.0798)	(0.0771)	(0.0775)	(0.0705)
$Ln(Area)$	-0.213**	-0.0309	-0.117***	-0.199***	-0.203***	-0.139***	-0.0855***	-0.00138	-0.0124	0.0265	-0.0162	0.0508
	(0.105)	(0.0603)	(0.0304)	(0.0608)	(0.0353)	(0.0362)	(0.0279)	(0.0384)	(0.0839)	(0.0764)	(0.0780)	(0.0748)
Observations	743	943	1072	1072	1072	1072	1072	1072	1072	1072	1072	1072
R^2	0.474	0.747	0.347	0.375	0.490	0.367	0.481	0.174	0.160	0.182	0.235	0.197
State FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Notes: In columns (1)-(12), the left-hand-side variable is the ln total value added per worker for the years 1840 to 1960. The fraction of county wealth in the hands of the planter elite in 1860 is denoted by $WealthPE$ (for more details see Section 3). The estimating equation employed is (2). See Section 4 for more details on the specification. Other right-hand-side variables used are the ln slaves (1860), ln population (1860) and the ln area of the county. See the Data Appendix for more details on the left-hand side variable and the other right-hand-side controls. The method of estimation is least squares. Standard errors account for arbitrary heteroskedasticity and are clustered at the county level. ***, **, and * denote significance at the 1%, 5%, and 10% level respectively.

TABLE 3

THE PLANTER ELITE, GEOGRAPHY AND SOUTHERN PRODUCTIVITY LEVELS, 1840-1960

	LN(Total Value Added per Worker)											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	1840	1850	1860	1870	1880	1890	1900	1920	1930	1940	1950	1960
$WealthPE$	0.532**	0.445**	0.334*	0.119	0.149	-0.424***	-0.525***	-1.045***	-1.643***	-1.688***	-1.667***	-0.752**
	(0.256)	(0.206)	(0.183)	(0.178)	(0.186)	(0.162)	(0.159)	(0.224)	(0.366)	(0.347)	(0.381)	(0.355)
$Ln(Slaves)$	0.0402	0.0157	0.0270	0.0569	0.0409*	0.0364	0.0374**	0.0508**	-0.0551	0.0548	0.0592	0.0679
	(0.0407)	(0.0301)	(0.0254)	(0.0445)	(0.0241)	(0.0264)	(0.0188)	(0.0199)	(0.0425)	(0.0371)	(0.0370)	(0.0443)
$Ln(Population)$	0.147*	0.253***	0.161***	0.129	0.207***	0.203***	0.112***	-0.0309	0.473***	0.257***	0.253***	0.187**
	(0.0777)	(0.0619)	(0.0580)	(0.101)	(0.0411)	(0.0443)	(0.0394)	(0.0408)	(0.0817)	(0.0736)	(0.0682)	(0.0745)
$Ln(Area)$	-0.294**	-0.109*	-0.110***	-0.139***	-0.162***	-0.168***	-0.106***	0.0562	-0.0919	-0.0355	-0.0585	0.0560
	(0.127)	(0.0586)	(0.0339)	(0.0456)	(0.0339)	(0.0346)	(0.0274)	(0.0386)	(0.0783)	(0.0728)	(0.0714)	(0.0747)
Observations	733	933	1072	1072	1072	1072	1072	1072	1072	1072	1072	1072
R^2	0.543	0.781	0.437	0.464	0.582	0.487	0.619	0.363	0.272	0.343	0.395	0.290
State FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Controls for Climate	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Soil Shares	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Controls for Elevation	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Notes: In columns (1)-(12), the left-hand-side variable is the ln total value added per worker for the years 1840 to 1960. The fraction of county wealth in the hands of the planter elite in 1860 is denoted by $WealthPE$ (for more details see Section 3). The estimating equation employed is (2). See Section 4 for more details on the specification. Other right-hand-side variables are a set of geographic controls (see Section 4), ln slaves (1860), ln population (1860) and the ln area of the county. See the Data Appendix for more details on the left-hand side variable and the other right-hand-side controls. The method of estimation is least squares. Standard errors account for arbitrary heteroskedasticity and are clustered at the county level. ***, **, and * denote significance at the 1%, 5%, and 10% level respectively.

TABLE 4
THE PLANTER ELITE, AGRICULTURAL SPECIALIZATION AND SOUTHERN PRODUCTIVITY LEVELS, 1840-1960

	LN(Total Value Added per Worker)											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	1840	1850	1860	1870	1880	1890	1900	1920	1930	1940	1950	1960
$WealthPE$	0.571*	0.306	0.231	0.0253	0.232	-0.429**	-0.444**	-0.926***	-1.514***	-1.379***	-1.460***	-0.619*
	(0.300)	(0.204)	(0.190)	(0.192)	(0.205)	(0.188)	(0.173)	(0.235)	(0.425)	(0.360)	(0.406)	(0.368)
$Ln(Slaves)$	0.0729*	0.0138	-0.0109	0.0664	0.0369	0.0453	0.0501**	0.0839***	0.00177	0.134***	0.119***	0.134**
	(0.0376)	(0.0349)	(0.0235)	(0.0450)	(0.0246)	(0.0287)	(0.0205)	(0.0234)	(0.0508)	(0.0422)	(0.0450)	(0.0535)
$Ln(Population)$	0.137*	0.249***	0.173***	0.121	0.207***	0.192***	0.110***	-0.0363	0.440***	0.228***	0.230***	0.164**
	(0.0713)	(0.0624)	(0.0467)	(0.103)	(0.0410)	(0.0436)	(0.0398)	(0.0424)	(0.0827)	(0.0765)	(0.0732)	(0.0772)
$Ln(Area)$	-0.286**	-0.107*	-0.0916***	-0.125***	-0.148***	-0.153***	-0.0951***	0.0491	-0.0980	-0.0317	-0.0558	0.0569
	(0.129)	(0.0578)	(0.0315)	(0.0468)	(0.0337)	(0.0346)	(0.0277)	(0.0397)	(0.0800)	(0.0716)	(0.0726)	(0.0738)
$Ln(Slaves\ Large\ Plantations)$	-0.0412***	-0.0127	-0.0166**	-0.0178**	-0.0102	-0.00509	-0.0121**	-0.0122	-0.00137	-0.0183	-0.00235	-0.0212
	(0.0152)	(0.00865)	(0.00715)	(0.00875)	(0.00641)	(0.00724)	(0.00489)	(0.00752)	(0.0148)	(0.0122)	(0.0129)	(0.0136)
$Share\ Farmland\ Large\ Farms$	0.399	0.289	0.177	0.322	-0.0478	0.0508	0.235	0.0746	-0.476	-0.0890	-0.0821	0.117
	(0.341)	(0.208)	(0.199)	(0.241)	(0.189)	(0.206)	(0.150)	(0.267)	(0.473)	(0.387)	(0.390)	(0.440)
$Share\ Cotton$	0.483***	0.319**	1.008***	0.266	0.434***	0.149	0.0234	-0.363***	-0.503***	-0.671***	-0.741***	-0.532***
	(0.137)	(0.131)	(0.156)	(0.218)	(0.107)	(0.105)	(0.0786)	(0.0957)	(0.167)	(0.159)	(0.158)	(0.161)
$Share\ Sugar$	0.325***	0.493***	0.700***	0.213	0.290***	0.0339	0.00886	-0.107	-0.208	-0.322**	-0.313**	-0.101
	(0.112)	(0.168)	(0.136)	(0.151)	(0.105)	(0.0984)	(0.0689)	(0.0834)	(0.163)	(0.134)	(0.138)	(0.140)
$Share\ Rice$	0.374	0.259	0.721***	0.739**	-0.0654	0.286	-0.424	-0.363	0.414	-0.731	-1.038	-0.136
	(0.419)	(0.383)	(0.240)	(0.325)	(0.265)	(0.356)	(0.280)	(0.358)	(0.722)	(0.840)	(0.879)	(0.652)
$Share\ Tobacco$	0.0946	-0.0247	0.312***	-0.283**	-0.132	-0.480***	-0.242***	-0.0660	-0.728***	-0.928***	-0.895***	-0.656***
	(0.159)	(0.112)	(0.0972)	(0.122)	(0.0894)	(0.111)	(0.0726)	(0.119)	(0.241)	(0.181)	(0.215)	(0.228)
Observations	733	933	1072	1072	1072	1072	1072	1072	1072	1072	1072	1072
R^2	0.556	0.789	0.534	0.478	0.608	0.503	0.629	0.382	0.285	0.372	0.416	0.308
State FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Controls for Climate	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Soil Shares	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Controls for Elevation	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Notes: In columns (1)-(12), the left-hand-side variable is the ln total value added per worker for the years 1840 to 1960. The fraction of county wealth in the hands of the planter elite in 1860 is denoted by $WealthPE$ (for more details see Section 3). The estimating equation employed is (2). See Section 4 for more details on the specification. Other right-hand-side variables are a set of geographic controls (see Section 4), ln slaves (1860), ln population (1860), ln area of the county and controls for the historical specialization in plantation agriculture (see Section 4) in 1860. See the Data Appendix for more details on the left-hand side variable and the right-hand-side controls. The method of estimation is least squares. Standard errors account for arbitrary heteroskedasticity and are clustered at the county level. ***, **, and * denote significance at the 1%, 5%, and 10% level respectively.

TABLE 5

THE PLANTER ELITE AND ILLITERACY

	$IlliteracyRate$					
	(1) 1870	(2) 1880	(3) 1900	(4) 1910	(5) 1920	(6) 1930
$WealthPE_{1860}$	0.293***	0.140**	0.110***	0.118***	0.105***	0.0633**
	(0.0655)	(0.0672)	(0.0322)	(0.0396)	(0.0388)	(0.0297)
$IlliteracyRate_{1860}$	0.257***	0.310***	0.144***	0.161***	0.124***	0.0881***
	(0.0290)	(0.0229)	(0.0110)	(0.0131)	(0.0119)	(0.00977)
Observations	1068	1072	1072	1072	1072	1072
R^2	0.469	0.752	0.818	0.771	0.724	0.646
State FE	yes	yes	yes	yes	yes	yes
State FE	yes	yes	yes	yes	yes	yes
Controls for Climate	yes	yes	yes	yes	yes	yes
Soil Shares	yes	yes	yes	yes	yes	yes
Controls for Elevation	yes	yes	yes	yes	yes	yes
Controls for Agricultural Specialization	yes	yes	yes	yes	yes	yes

Notes: In columns (1)-(6), the left-hand-side variable is the illiteracy rate for the years 1870 to 1930. The fraction of county wealth in the hands of the planter elite in 1860 is denoted by $WealthPE$ (for more details see Section 3). Other right-hand-side variables are a set of geographic controls (see Section 4), the ln slaves (1860), ln population (1860), ln area of the county, the illiteracy rate (1860) and controls for the historical specialization in plantation agriculture (see Section 4) in 1860. See the Data Appendix for more details on the left-hand side variable and the right-hand-side controls. The method of estimation is least squares. Standard errors account for arbitrary heteroskedasticity and are clustered at the county level. ***, **, and * denote significance at the 1%, 5%, and 10% level respectively.

TABLE 6

THE PLANTER ELITE AND THE SHARE OF ADULTS WITH HIGHER EDUCATION

	Share of Adults over 25 with High School/College Education			
	(1)	(2)	(3)	(4)
	High School (1940)	College (1940)	High School (1950)	College (1950)
$WealthPE$	-0.0752***	-0.0435***	-0.0537*	-0.0458**
	(0.0270)	(0.0134)	(0.0299)	(0.0182)
$Illiteracy\,Rate(1860)$	-0.0813***	-0.0274***	-0.0967***	-0.0313***
	(0.0121)	(0.00596)	(0.0130)	(0.00719)
$Ln(Slaves)$	0.0126***	0.00572***	0.0128***	0.00536***
	(0.00295)	(0.00126)	(0.00308)	(0.00152)
$Ln(Totpop)$	-0.00366	0.00261	0.00102	0.00699**
	(0.00540)	(0.00268)	(0.00586)	(0.00307)
$Ln(Area)$	0.00128	-0.000569	-0.00141	5.99e-06
	(0.00494)	(0.00415)	(0.00535)	(0.00537)
Observations	1073	1073	1073	1073
R^2	0.537	0.349	0.491	0.297
State FE	yes	yes	yes	yes
Controls for Climate	yes	yes	yes	yes
Soil Shares	yes	yes	yes	yes
Controls for Elevation	yes	yes	yes	yes
Controls for Agricultural Specialization	yes	yes	yes	yes

Notes: In column (1) and (3), the left-hand-side variable is the share of adults over 25 with high school education in 1940 and 1950. In column (2) and (4), the left-hand-side variable is the share of adults over 25 with college education in 1940 and 1950. The fraction of county wealth in the hands of the planter elite in 1860 is denoted by $WealthPE$ (for more details see Section 3). The estimating equation employed is (2). See Section 4 for more details on the specification. Other right-hand-side variables are a set of geographic controls (see Section 4), the ln slaves (1860), ln population (1860), ln area of the county, the illiteracy rate (1860) and controls for the historical specialization in plantation agriculture (see Section 4) in 1860. See the Data Appendix for more details on the left-hand side variable and the right-hand-side controls. The method of estimation is least squares. Standard errors account for arbitrary heteroskedasticity and are clustered at the county level. ***, **, and * denote significance at the 1%, 5%, and 10% level respectively.

TABLE 7

THE PLANTER ELITE AND ROSENWALD SCHOOLS

	Number of Rosenwald Schools	
	(1)	(2)
$WealthPE$	-6.536**	-5.884**
	(2.666)	(2.685)
$Ln(Slaves)$	0.0666	0.207
	(0.192)	(0.165)
$Ln(Totpop)$	0.889**	0.339
	(0.384)	(0.332)
$Ln(Area)$	1.730***	2.002***
	(0.338)	(0.385)
Observations	1071	1071
R^2	0.427	0.433
State FE	yes	yes
Climate Variables	yes	yes
Soil Shares	yes	yes
Elevation Variables	yes	yes
Controls for Agricultural Specialization	yes	yes

Notes: In columns (1)-(2), the left-hand-side variable is the number of Rosenwald schools built in each county between 1914-1931. The fraction of county wealth in the hands of the planter elite in 1860 is denoted by $WealthPE$ (for more details see Section 3). The estimating equation employed is (2). See Section 4 for more details on the specification. Other right-hand-side variables are a set of geographic controls (see Section 4), the ln slaves (1860), ln population (1860), ln area of the county, the illiteracy rate (1860) and controls for the historical specialization in plantation agriculture (see Section 4) in 1860. I add the urban share in 1860 as additional control in column (2) . See the Data Appendix for more details on the left-hand side variable and the right-hand-side controls. The method of estimation is least squares. Standard errors account for arbitrary heteroskedasticity and are clustered at the county level. ***, **, and * denote significance at the 1%, 5%, and 10% level respectively.

TABLE 8

THE PLANTER ELITE AND THE ILLITERACY RATE OF BLACK MALES

	Illiteracy Rate Black Males of Voting Age		
	(1)	(2)	(3)
	1900	1910	1920
$WealthPE$	0.119**	0.112***	0.102***
	(0.0509)	(0.0420)	(0.0386)
$Ln(Slaves)$	0.0159***	0.0100***	0.00604***
	(0.00271)	(0.00227)	(0.00190)
$Ln(Totpop)$	-0.0105**	-0.00862**	-0.00530
	(0.00470)	(0.00422)	(0.00365)
$Ln(Area)$	-0.0145***	-0.00697	-0.00753**
	(0.00533)	(0.00443)	(0.00376)
Observations	1072	1072	1073
R^2	0.860	0.834	0.819
State FE	yes	yes	yes
Climate Variables	yes	yes	yes
Soil Shares	yes	yes	yes
Elevation Variables	yes	yes	yes
Controls for Agricultural Specialization	yes	yes	yes

Notes: In columns (1)-(3), the left-hand-side variable is the share of black males of voting age (age 21 and over) who cannot read and write for 1900 to 1920. The fraction of county wealth in the hands of the planter elite in 1860 is denoted by $WealthPE$ (for more details see Section 3). The estimating equation employed is (2). See Section 4 for more details on the specification. Other right-hand-side variables are a set of geographic controls (see Section 4), the ln slaves (1860), ln population (1860), ln area of the county, the illiteracy rate (1860) and controls for the historical specialization in plantation agriculture (see Section 4) in 1860. See the Data Appendix for more details on the left-hand side variable and the right-hand-side controls. The method of estimation is least squares. Standard errors account for arbitrary heteroskedasticity and are clustered at the county level. ***, **, and * denote significance at the 1%, 5%, and 10% level respectively.

TABLE 9

THE PLANTER ELITE AND POLITICAL REPRESSION

Violence against Black Officeholders during Reconstruction		
	(1)	(2)
$WealthPE$	0.427**	0.982**
	(0.178)	(0.441)
$Ln(Slaves)$	0.0151*	0.00189
	(0.00789)	(0.0169)
$Ln(Population)$	0.0761***	0.181***
	(0.0189)	(0.0504)
$Ln(Area)$	-0.0412*	-0.0352
	(0.0223)	(0.0412)
Observations	841	841
R^2	0.235	0.231
State FE	yes	yes

Notes: In column (1), the left-hand-side variable is a binary variable which is unity if at least one black officeholder was a victim of violence in a county during Reconstruction. The left-hand side variable in column (2) is the total number of black officeholders which were victim of violence during Reconstruction. The fraction of county wealth in the hands of the planter elite in 1860 is denoted by $WealthPE$ (for more details see Section 3). The estimating equation employed is (2). See Section 4 for more details on the specification. Other right-hand-side variables are the ln slaves (1860), ln population (1860) and the ln area of the county. See the Data Appendix for more details on the left-hand side variable and the right-hand-side controls. The method of estimation is least squares. Standard errors account for arbitrary heteroskedasticity and are clustered at the county level. ***, **, and * denote significance at the 1%, 5%, and 10% level respectively.

Table 10: The Political Connection of the Planter Elite after the Civil War

States	Constitutional Conventions (Years)	# Delegates Identified in 1860	Alternative (1)	Alternative (2)	Alternative (3)
Alabama	1865	99/99	77.78	67.68	62.63
	1875	70/92	72.86	72.86	65.71
Mississippi	1865	99/99	68.69	68.69	58.59
	1890	83/130	60.24	68.67	51.81

Source: Journals of the Proceedings and Debates of the Constitutional Conventions of Alabama (1865/75) and Mississippi (1865/90)
Alternative (1): % of Delegates listed as Slaveholders in 1860
Alternative (2): % of Delegates listed with more than $10,000 Wealth in 1860
Alternative (3): Both Criteria (1) & (2)

TABLE 11

THE PLANTER ELITE'S POLITICAL CONNECTION

	(1)	(2)	(3)
	PC_{cs}	$PC_{cs,t}$	$\#PC_{cs,t}$
$WealthPE$	4.375***	2.730**	1.201***
	(1.443)	(1.090)	(0.336)
Observations	109	217	217
R^2	0.206	0.187	0.507
State FE	yes	no	no
State Year FE	no	yes	yes
Other Controls	yes	yes	yes

Notes: In column (1), the left-hand-side variable, denoted by PC_{cs}, is an indicator variable whether a county was politically captured by the planter elite using Alternative (3) (for more details see Subsection 4.3). The fraction of county wealth in the hands of the planter elite in 1860 is denoted by $WealthPE$ (for more details see Section 3). The estimating equation employed is (4); see Subsection 4.3 for more details on the specification. Other right-hand-side controls are the average number of delegates, ln population (1860) and ln area of the county and state fixed effects . In column (2), the left-hand-side variable, denoted by $PC_{cs,t}$, is an indicator variable whether at least one delegate of county c in a given year t was connected to the planter elite in 1860 using Alternative (3). The main variable of interest is the relative wealth of the planter elite, $WealthPE$. The estimating equation employed is (5); see Subsection 4.3 for more details on the specification. Other right-hand-side controls are the number of delegates, ln population (1860) and ln area of the county and state time fixed effects. The method of estimation in column (1) and (2) is probit. The Pseudo-R^2 is reported for columns (1)-(2). In column (3) the left-hand side variable denotes the number of delegates with political connections to the planter elite in a given year, $\#PC_{cs,t}$, using Alternative (3). The right-hand side variables are the same as in column (2). The method of estimation is least squares. Standard errors account for arbitrary heteroskedasticity and are clustered at the county level. ***, **, and * denote significance at the 1%, 5%, and 10% level respectively.

Figure 1.2: Border Counties in the US South 1860

TABLE 12

THE PLANTER ELITE AND LAND PRICES

LN(Average Value of Farmland per Acre) 1870-1930			
PANEL A	(1)	(2)	(3)
$TE \times WealthPE$	1.192***	0.996***	0.667***
	(0.224)	(0.182)	(0.219)
Observations	7553	7553	7553
R^2	0.889	0.893	0.894
County FE	yes	yes	yes
State Time FE	yes	yes	yes
Other Controls	no	yes	yes
PANEL B	(1)	(2)	(3)
$TE \times WealthPE$	1.526***	1.131***	0.568**
	(0.187)	(0.221)	(0.287)
Observations	4769	4769	4769
R^2	0.954	0.955	0.956
County FE	yes	yes	yes
State Time FE	yes	yes	yes
Border Segment Time FE	yes	yes	yes
Other Controls	no	yes	yes

Notes: For Panel A and B, the left-hand-side variable is the ln value of farmland per acre (in $) for the years 1870 to 1930. The variable $TE \times WealthPE$ denotes the interaction between the treatment effect, TE, and $WealthPE$ (for more details see Section 4.3). Note the treatment effect, TE, (that is a binary variable which takes the value one for all years after the Civil War *and before* the state overrode its Reconstruction convention) is captured by the state time fixed effects and the fraction of county wealth in the hands of the planter elite in 1860, $WealthPE$, is captured by the county fixed effects. For Panel A, the estimating equation employed is (6) and for Panel B the estimating equation is (7) (see Section 4.3). Column (2) includes for Panel A and B the interactions of the treatment effect with the following control variables: ln slaves (1860), ln population (1860) and ln area of the county. In Column (3) I further add the interactions of the treatment effect with controls for the historical specialization in plantation agriculture in 1860. See the Data Appendix for more details on the left-hand side variable and the right-hand-side controls. The method of estimation is least squares. In Panel A, standard errors account for arbitrary heteroskedasticity and are clustered at the county level. In Panel B, standard errors account for arbitrary heteroskedasticity and are two-way clustered at the state level and border segment. ***, **, and * denote significance at the 1%, 5%, and 10% level respectively.

TABLE 13

THE PLANTER ELITE AND PLANTATION CROPS

Plantation Crops Ratio 1870-1900			
PANEL A	(1)	(2)	(3)
$TE \times WealthPE$	1.738***	1.165*	2.098***
	(0.609)	(0.652)	(0.796)
Observations	4202	4202	4202
R^2	0.178	0.181	0.196
County FE	yes	yes	yes
State Time FE	yes	yes	yes
Other Controls	no	yes	yes
PANEL B	(1)	(2)	(3)
$TE \times WealthPE$	2.577***	2.691***	1.950*
	(0.825)	(0.827)	(1.118)
Observations	2639	2639	2639
R^2	0.940	0.941	0.941
County FE	yes	yes	yes
State Time FE	yes	yes	yes
Border Segment Time FE	yes	yes	yes
Other Controls	no	yes	yes

Notes: For Panel A and B the left-hand-side variable is the plantation crops ratio for the years 1870 to 1900. The variable $TE \times WealthPE$ denotes the interaction between the treatment effect, TE, and $WealthPE$ (for more details see Section 4.3). Note the treatment effect, TE, (that is a binary variable which takes the value one for all years after the Civil War *and before* the state overrode its Reconstruction convention) is captured by the state time fixed effects and the fraction of county wealth in the hands of the planter elite in 1860, $WealthPE$, is captured by the county fixed effects. For Panel A, the estimating equation employed is (6) and for Panel B the estimating equation is (7) (see Section 4.3). Column (2) includes for Panel A and B the interactions of the treatment effect with the following control variables: ln slaves (1860), ln population (1860) and ln area of the county. In Column (3) I further add the interactions of the treatment effect with controls for the historical specialization in plantation agriculture in 1860. See the Data Appendix for more details on the left-hand side variable and the right-hand-side controls. The method of estimation is least squares. In Panel A, standard errors account for arbitrary heteroskedasticity and are clustered at the county level. In Panel B, standard errors account for arbitrary heteroskedasticity and are two-way clustered at the state level and border segment. ***, **, and * denote significance at the 1%, 5%, and 10% level respectively.

TABLE 14

THE PLANTER ELITE AND THE PATERNALISTIC VIEW: LYNCHINGS

	Number of Lynchings	
	(1)	(2)
	1882-1930	1882-1900
$WealthPE$	-7.636***	-5.232***
	(2.365)	(1.925)
$Ln(Slaves)$	0.355	0.349
	(0.258)	(0.215)
$Ln(Population)$	-0.0402	0.106
	(0.511)	(0.426)
$Ln(Area)$	1.192***	0.753***
	(0.332)	(0.283)
Observations	682	682
R^2	0.364	0.321
State FE	yes	yes
Climate Variables	yes	yes
Soil Shares	yes	yes
Elevation Variables	yes	yes
Controls for Agricultural Specialization	yes	yes

Notes: In columns (1)-(2), the left-hand-side variable is the number of lynchings in ten Southern states (Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Kentucky and Tennessee). The fraction of county wealth in the hands of the planter elite in 1860 is denoted by $WealthPE$ (for more details see Section 3). The estimating equation employed is (2). See Section 4 for more details on the specification. Other right-hand-side variables are a set of geographic controls (see Section 4), the ln slaves (1860), ln population (1860), ln area of the county and controls for the historical specialization in plantation agriculture (see Section 4) in 1860. See the Data Appendix for more details on the left-hand side variable and the right-hand-side controls. The method of estimation is least squares. Standard errors account for arbitrary heteroskedasticity and are clustered at the county level. ***, **, and * denote significance at the 1%, 5%, and 10% level respectively.

TABLE 15

THE PLANTER ELITE AND THE PATERNALISTIC VIEW: BLACK TENANCY

	Share of Black Tenants in 1900		
	(1)	(2)	(3)
	Cash & Share	Cash Tenants	Share Tenants
$WealthPE$	0.302***	0.417***	0.176*
	(0.0930)	(0.102)	(0.102)
$Ln(Slaves)$	0.0272***	0.0230***	0.0263***
	(0.00624)	(0.00643)	(0.00649)
$Ln(Population)$	-0.00485	-0.0121	0.00463
	(0.0117)	(0.0126)	(0.0121)
$Ln(Area)$	-0.0157	-0.00458	-0.0253*
	(0.0130)	(0.0140)	(0.0134)
Observations	1072	1071	1072
R^2	0.863	0.832	0.854
State FE	yes	yes	yes
Climate Variables	yes	yes	yes
Soil Shares	yes	yes	yes
Elevation Variables	yes	yes	yes
Controls for Agricultural Specialization	yes	yes	yes

Notes: In column (1), the left-hand-side variable is the share of black tenants in 1900. In column (2), the left-hand-side variable is the share of black cash tenants in 1900. Column (3) reports the share of black share tenants in 1900. The fraction of county wealth in the hands of the planter elite in 1860 is denoted by $WealthPE$ (for more details see Section 3). The estimating equation employed is (2). See Section 4 for more details on the specification. Other right-hand-side variables are a set of geographic controls (see Section 4), the ln slaves (1860), ln population (1860), ln area of the county and controls for the historical specialization in plantation agriculture (see Section 4) in 1860. See the Data Appendix for more details on the left-hand side variable and the right-hand-side controls. The method of estimation is weighted least squares with weights equal to the farmland of counties. Standard errors account for arbitrary heteroskedasticity and are clustered at the county level. ***, **, and * denote significance at the 1%, 5%, and 10% level respectively.

TABLE 16

THE RELATIVE WEALTH OF THE PLANTER ELITE VS THE GINI COEFFICIENT IMPLIED BY THE FARM SIZE DISTRIBUTIONS

	LN(Total Value Added per Worker)											
	PANEL A: REPLICATION OF TABLE 2 USING THE GINI COEFFICIENT											
	1840	1850	1860	1870	1880	1890	1900	1920	1930	1940	1950	1960
Gini Coefficient	0.552	1.060***	0.351	-0.529*	-0.0972	-0.182	-0.0358	0.195	0.240	0.0987	0.267	0.203
	(0.375)	(0.378)	(0.337)	(0.280)	(0.225)	(0.222)	(0.172)	(0.256)	(0.473)	(0.407)	(0.414)	(0.361)
Observations	736	935	1070	1070	1070	1070	1070	1070	1070	1070	1070	1070
R^2	0.483	0.758	0.364	0.386	0.481	0.369	0.489	0.162	0.155	0.183	0.236	0.200
State FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
	PANEL B: REPLICATION OF TABLE 4 USING THE GINI COEFFICIENT											
Gini Coefficient	1.204***	1.254***	0.326	-0.415	-0.0925	-0.291	-0.0729	0.267	-0.284	-0.423	0.0617	-0.0216
	(0.407)	(0.387)	(0.351)	(0.312)	(0.259)	(0.246)	(0.179)	(0.255)	(0.471)	(0.378)	(0.398)	(0.387)
Observations	732	931	1070	1070	1070	1070	1070	1070	1070	1070	1070	1070
R^2	0.561	0.796	0.545	0.511	0.604	0.502	0.631	0.369	0.277	0.368	0.412	0.310
State FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
	PANEL C: HORSERACE											
$WealthPE$	0.371	0.226	0.195	0.0875	0.246	-0.409**	-0.448***	-0.959***	-1.505***	-1.363***	-1.487***	-0.631*
	(0.290)	(0.216)	(0.191)	(0.193)	(0.207)	(0.193)	(0.172)	(0.234)	(0.424)	(0.360)	(0.408)	(0.372)
Gini Coefficient	1.143***	1.238***	0.310	-0.422	-0.112	-0.259	-0.0372	0.343	-0.164	-0.315	0.180	0.0288
	(0.406)	(0.389)	(0.353)	(0.315)	(0.259)	(0.249)	(0.176)	(0.249)	(0.465)	(0.372)	(0.394)	(0.390)
Observations	732	931	1070	1070	1070	1070	1070	1070	1070	1070	1070	1070
R^2	0.561	0.796	0.545	0.511	0.604	0.504	0.634	0.381	0.285	0.378	0.421	0.312
State FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Notes: In columns (1)-(12), the left-hand-side variable is the ln total value added per worker for the years 1840 to 1960. The Gini coefficient implied by the farm size distributions is retrieved from Nathan Nunn's webpage: http://www.economics.harvard.edu/faculty/nunn/data_nunn. The calculation of the Gini coefficient is described in Nunn (2008). For Panel C, the fraction of county wealth in the hands of the planter elite in 1860 is denoted by $WealthPE$ (for more details see Section 3). For more information on the control variables used in Panel A see Table 2, and for Panel B and C see Table 4. The estimating equation employed is (2). See Section 4 for more details on the specification. The method of estimation is least squares. Standard errors account for arbitrary heteroskedasticity and are clustered at the county level. ***, **, and * denote significance at the 1%, 5%, and 10% level respectively.

APPENDIX TABLE: SUMMARY STATISTICS

Summary Statistics (Section 4)

Variable	*Obs*	*Mean*	*Std. Dev.*
Relative Wealth of the Planter Elite (1860)	1072	0.04	0.08
Ln Slaves (1860)	1072	7.30	1.64
Ln Population (1860)	1072	9.03	0.78
Ln Area	1072	6.28	0.54

Variable	*Obs*	*Mean*	*Std. Dev.*
Ln Total Value Added Per Worker (1840)	733	5.1	0.74
Ln Total Value Added Per Worker (1860)	1072	5.34	0.47
Ln Total Value Added Per Worker (1880)	1072	5.15	0.44
Ln Total Value Added Per Worker (1900)	1072	5.56	0.38
Ln Total Value Added Per Worker (1920)	1072	6.92	0.46
Ln Total Value Added Per Worker (1940)	1072	5.29	0.73
Ln Total Value Added Per Worker (1960)	1072	7.58	0.78

Variable	*Obs*	*Mean*	*Std. Dev.*
Illiteracy Rate (1860)	1072	0.41	0.21
Illiteracy Rate (1880)	1072	0.39	0.16
Illiteracy Rate (1900)	1072	0.17	0.09
Illiteracy Rate (1920)	1072	0.12	0.08
Illiteracy Rate (1930)	1072	0.09	0.06

Variable	*Obs*	*Mean*	*Std. Dev.*
Share of Adults with High School Education (1940)	1072	0.20	0.07
Share of Adults with High School Education (1950)	1072	0.25	0.07
Share of Adults with College Education (1940)	1072	0.07	0.03
Share of Adults with College Education (1950)	1072	0.08	0.04

Summary Statistics (Section 4 continued)

Variable	*Obs*	*Mean*	*Std. Dev.*
Number of Rosenwald Schools (1914-1931)	1071	3.52	5.85
Indicator Variable Violence against Black Officeholder	842	0.10	0.31
Indicator Variable of Political Persistence	109	0.48	0.50

Variable	*Obs*	*Mean*	*Std. Dev.*
Ln Value of Farmland Per Acres in $ (1870)	1055	1.82	0.94
Ln Value of Farmland Per Acres in $ (1880)	1055	1.92	0.76
Ln Value of Farmland Per Acres in $ (1890)	1055	2.24	0.74
Ln Value of Farmland Per Acres in $ (1900)	1055	2.34	0.71
Ln Value of Farmland Per Acres in $ (1910)	1055	3.06	0.64
Ln Value of Farmland Per Acres in $ (1920)	1055	3.80	0.63
Ln Value of Farmland Per Acres in $ (1930)	1055	3.60	0.57

Variable	*Obs*	*Mean*	*Std. Dev.*
Number of Lynchings (1882-1932)	682	3.3	4.17
Share of Black Tenants (1900)	1072	0.29	0.29
Gini Coeffcient (1860)	1070	0.45	0.06

Chapter 2

RAINFALL RISK AND RELIGIOUS MEMBERSHIP IN THE LATE NINETEENTH CENTURY US (JOINT WITH A. CICCONE)

2.1 Introduction

One aspect of today's major religions is that they spiritually reward mutual aid and charity (McCleary and Barro, 2006). This could be a reason why religious organizations have often been at the center of community assistance and mutual aid networks (e.g. Bremner, 1994; Parker, 1998; Pullan, 1998, 2005; Belcher and Tyles, 2011). The access to such networks through religious membership may help explain the historical spread of religion in agricultural societies subject to much uncertainty, as well as recent stagnation in countries where the government provides substantial social insurance (Gill and Lundsgaarde, 2004; McCleary and Barro, 2006; Franck and Iannaccone, 2009).

The US in the second half of the nineteenth century provides a good opportunity for examining the link between the economic uncertainty faced by agricultural communities and membership in religious organizations. The US started the second half of the nineteenth century as a mostly agricultural economy with little social insurance supplied by the government. US agricultural income exceeded manufacturing income until around 1880, and agriculture was the dominant sector in four of five counties in 1890. Government social spending was low, around a third of a percentage point of GDP in 1880 (Lindert, 1984). As much of US agriculture was rainfed, output was subject to rainfall risk (USDA, 1923, 1925). There is data to quantify the temporal variability of rainfall at the county level since 1895, and it is therefore possible to measure the rainfall risk local communities were likely to be subject to in the nineteenth century. Moreover, the US

Census collected data on the seating capacity of churches at the county level from 1850 to 1890 and their membership in 1890. Hence, we can examine whether membership in religious organizations in the second half of the nineteenth-century US was greater in local communities likely to have been facing more rainfall risk.

Historically, religious organizations have often been at the core of local and cross-regional mutual assistance and social aid networks, see Trattner (1974), Bodnar (1985), and Gjerde (1985) for US evidence. Joining such networks can be more attractive in communities facing greater aggregate uncertainty as the value of partially insuring idiosyncratic shocks may increase with aggregate background risk. We show this in a theoretical model where – as in our empirical setting – aggregate background risk is driven by rainfall risk common to all members of the community. The condition for partial insurance against idiosyncratic risk to become more valuable as common risk increases turns out to be a degree of relative risk aversion that is plausible empirically.[1] The assistance networks around churches could also prove valuable because these networks often extended across local communities and could therefore provide some inter-regional insurance, see Overacker (1998) and Szasz (2004) for US evidence.

Our empirical analysis of the link between rainfall risk and membership in religious organizations in the late nineteenth-century US is cross county. For 1890, where we have data on both church seating capacity and church membership, the necessary data are available for around 2650 counties. Our analysis yields a statistically significant link between rainfall risk and church seating capacity as well as membership, controlling for a range of county characteristics likely to affect agricultural productivity as well as for county size. A one-standard-deviation increase in rainfall risk is associated with an increase in church membership and church seating capacity of around 12 percent. The link between rainfall risk and membership in religious organizations is similar when we use the 1860 and 1870 data on church seating capacity (the 1880 data were never published).

If rainfall risk affects membership in religious organizations through economic risk – as in our theoretical model – we would expect the link between rainfall risk and religious membership to be stronger among nineteenth-century agricultural counties as their economies were more dependent on rainfall. We therefore examine the link between rainfall risk and religious membership separately among counties with population densities below and above the median, and also split counties into those with value added in agriculture relative to manufacturing above and below the median. We find a statistically significant link between rainfall risk and religious membership among counties with population densities below the median and also among the more agricultural counties. On the other hand, the link between rainfall risk and membership in religious organizations among more densely populated counties and among less agri-

[1] Another implication of this framework is that (religious) communities with better mutual insurance would find places with greater economic risk relatively more attractive.

cultural counties turns out to be statistically insignificant. The link between rainfall risk and religious membership in 1890 is strongest among more agricultural counties. There, a one-standard-deviation increase in rainfall risk is associated with an increase in church membership of around 23 percent and an increase in church seating capacity of around 32 percent.

There continues to be a strong link between rainfall risk and membership in religious organizations among local communities likely to depend more on rainfall in 1860 and 1870. Rainfall risk is a statistically significant determinant of 1860 and 1870 church seating capacity among counties with population densities below the median and among more agricultural counties, but not among more densely populated counties or among less agricultural counties. The link between rainfall risk and church seating capacity remains strongest among more agricultural counties. In 1870, a one-standard-deviation increase in rainfall risk is associated with an increase in church seating capacity of around 50 percent among these agricultural counties. In 1860, the effect is close to 65 percent. In 1850, we do not find a statistically significant link between rainfall risk and church seating capacity. We argue that this is due to the smaller number of counties with the necessary data. In 1850, there is data for around 1450 counties compared to more than 1800 counties in 1860 and around 2650 counties in 1890. Moreover, most of the counties lost in 1850 compared to 1860 or 1890 are agricultural counties and counties with low population densities – the type of counties where the link between rainfall risk and membership in religious organizations was statistically significant.

In 1910, 1920, and 1930, the US Census collected county-level data on the value of crops produced. This allows us to examine the effect of rainfall on agricultural productivity using a within-county approach for a period close to the late nineteenth century. The data on the value of crops can also be used to assess the relative importance for agricultural productivity of rainfall during the winter months compared to the months of spring, summer, and fall. Our results indicate that rainfall during the winter months has a weaker within-county effect on the value of crops than spring-fall rainfall, which is consistent with data on the agricultural growing and non-growing seasons at the beginning of the twentieth century (Covert, 1912). We use our theoretical model to map the relative importance of winter versus spring-fall rainfall for agricultural productivity into the relative importance of winter relative to spring-fall rainfall risk for religious membership. When we relate membership in religious organizations to winter rainfall risk, spring-fall rainfall risk, and a cross-season covariance term, we find that the statistically significant link is mostly with spring-fall rainfall risk.

Our work relates to a small literature on risk, religion, insurance, and the welfare state as well as a large literature on insurance in historical agricultural societies and developing countries today. Bentzen (2013) finds that people in regions that are more frequently razed by natural disasters are more religious. Gill and Lundsgaarde (2004) and Franck and Iannaccone (2009) find a negative effect of country-level welfare spending on church attendance in cross-sectional and panel data respectively. Scheve and Stasvange (2006) show that religiosity has a negative effect on preferences for social insurance in individual data and on social spending outcomes in cross-country data. Dehejia, DeLeire, and Luttmer (2007) show that involvement with religious organizations results in better insurance against idiosyncratic income shocks using 1982-1998 US micro data. Hungerman (2005) finds that the 1996 US welfare reform decreasing welfare to non-citizens crowded in member donations and community spending of Presbyterian congregations; Gruber and Hungerman (2007) show that the New Deal crowded out charitable spending of Christian denominations and Hungerman (2009) finds that a Supreme Court-mandated expansion of social security insurance in 1991 crowded out charitable spending of United Methodist churches.

The literature on insurance in historical agricultural societies and developing countries today – often characterized by high risk and little insurance supplied by governments or markets – has found evidence of insurance mechanisms ranging from the scattering of agricultural plots to reciprocal gift exchange, see the surveys of Alderman and Paxson (1994), Townsend (1995), and Dercon (2004) for example. This literature also contains interesting results on how the need for insurance in environments with high rainfall risk effects social outcomes. For example, Rosenzweig (1988a,b) and Durante (2010) – who also exploits the heterogeneous effect of rainfall on agricultural productivity over the growing year – show that rainfall risk ends up affecting family structure and trust respectively.

The rest of the paper is structured as follows. Section 2 presents a model of the value of partial insurance against idiosyncratic risk and membership in religious organizations in the presence of common background risk. Section 3 discusses our estimation framework and Section 4 presents our data and empirical results. Section 5 concludes.

2.2 A Model of Community Rainfall Risk, Idiosyncratic Risk, and Religious Membership

Religious membership may depend on the aggregate risk faced by a community because the value of partially insuring idiosyncratic risk through membership in religious organizations varies with aggregate risk. We show this in a model where farmers in a county are subject to both county-level economic risk – driven by rainfall risk – and idiosyncratic economic risk. The model yields that membership in religious organizations is more attractive in counties subject to greater rainfall risk as long as farmers' relative risk aversion is above unity.

Consider a county c inhabited by a continuum of ex-ante identical farmers of measure one. Agricultural output Y_{fc} produced by farmer f in the county depends on fixed county characteristics Z_c, an idiosyncratic shock s_f, and county-level rainfall R_c,

$$\ln Y_{fc} = \ln Z_c + \ln s_f + \beta \ln R_c \tag{2.1}$$

where R_c is a weighted average of monthly rainfall levels,

$$R_c = \prod_{m=1}^{12} R_{mc}^{\alpha_m} \tag{2.2}$$

with $\sum_{m=1}^{12} \alpha_m = 1$. Hence, β captures the percentage increase in agricultural output in response to a one-percent increase in monthly rainfall and α_m that rainfall may matter more in some months than others.

Monthly rainfall levels in the county, R_{mc}, are taken to be jointly log-normally distributed. The distribution of the idiosyncratic shocks farmers face is independent of rainfall and log-normal with $Var(\ln s) = \sigma^2$.

The utility function of each farmer over consumption C is of the constant relative risk aversion type, $U = (C^{1-\rho} - 1)/(1-\rho)$ where $\rho \geq 0$ is the coefficient of relative risk aversion. This implies that the expected utility of a farmer consuming output Y_{fc} in (1) is

$$EU_c = \frac{1}{1-\rho}\left((EY_c)^{(1-\rho)} e^{-\rho(1-\rho)(1/2)(\sigma^2+\beta^2 RVar_c)} - 1\right) \tag{2.3}$$

where $RVar_c$ measures rainfall risk in the county

$$RVar_c = Var(\ln R_c) = Var\left(\sum_{m=1}^{12} \alpha_m \ln R_{mc}\right) \tag{2.4}$$

and EY_c is expected agricultural output in the county

$$EY_c = E\left(sZ_c R_c^{\beta}\right) = \delta Z_c E\left(\prod_{m=1}^{12} R_{mc}^{\beta\alpha_m}\right) \tag{2.5}$$

with $\delta = Es$.

If farmers were able to marginally reduce idiosyncratic consumption risk through mutual insurance, their utility gain would be $MIB_c = -\left.\partial EU_c/\partial\sigma^2\right|_{EY_c}$. From (3) it follows that this marginal utility gain depends on rainfall risk and expected agricultural output in the county,

$$\ln MIB_c = \mu - (\rho - 1)\ln EY_c + \frac{\rho(\rho-1)\beta^2}{2} RVar_c \tag{2.6}$$

with μ an unimportant function of preference and technology parameters. Hence, the marginal utility gain of mutually insuring idiosyncratic risk is increasing in rainfall risk $RVar_c$ in the county if the coefficient of relative risk aversion is strictly greater than unity, $\rho > 1$. In this case, greater rainfall risk increases the benefits of mutually insuring idiosyncratic risk. Intuitively, this is because $\rho > 1$ implies that the marginal utility gain from reducing risk is higher the more risk there is. Most estimates of the coefficient of relative risk aversion in the literature are above unity, see for example Attanasio and Weber (1989), Vissing-Jorgensen and Attanasio (2003), and Chiappori and Paiella (2011). While these estimates all rely on post-World War II data, it seems reasonable to presume that risk aversion in the late nineteenth-century US – when incomes were closer to subsistence levels and less government insurance was available – was at least as high.

We close the model by assuming that church membership provides partial insurance against idiosyncratic risk and that farmer f's cost MIC_{fc} of becoming a church member is $MIC_{fc} = p_f (EY_c)^{\theta}$. The cost could be decreasing in expected output EY_c ($\theta < 0$) as there may be more churches in richer counties or farmers in these counties may be able to afford better means of transport for example. But the cost of church membership may also be increasing in expected output ($\theta > 0$) because of a higher opportunity cost of church membership in richer counties or because more alternative social activities may be available in these counties. (Our empirical model also accounts for other county characteristics that may affect the cost of church membership, population density for example, but we do not include them here for simplicity.) With p_f we capture individual heterogeneity in the cost of church membership. Combining the cost and the benefit of mutual insurance through church membership yields that farmers will become church members if and only if $\ln p_f \leq \mu - (\theta + \rho - 1) \ln EY_c + \rho(\rho - 1)\beta^2 RVar_c/2$. Hence, church membership M_c in county c will be

$$M_c = G\left(\mu - (\theta + \rho - 1)\ln EY_c + \frac{\rho(\rho-1)\beta^2}{2} RVar_c\right) \tag{2.7}$$

where $G(x) = H(\exp(x))$ with $H(x)$ the cumulative distribution function of p_f. As $G'(x) \geq 0$, church membership is greater the more rainfall risk there is in the county as long as $\rho > 1$.

The agricultural production function in (1)-(2) allows for heterogenous effects of monthly rainfall. It is commonly assumed in the literature on the effect of weather on crop yields that rainfall matters more during the agricultural growing season than the non-growing season (e.g. Schlenker and Roberts, 2009). The US non-growing season varies by crops and states, see USDA (2007) for modern and Covert (1912) for historical data, but generally includes the three winter months.[2] We therefore examine the implications of a smaller effect of winter rainfall on agricultural output for rainfall risk and membership in religious organizations in our theoretical model. To do so, it

[2]According to Covert (1912), the growing season for corn, cotton, and wheat (including winter wheat) went from March to November. The non-growing season would therefore have been the winter months December, January, and February.

is useful to collect the winter months in the set $W = \{\text{December, January, February}\}$ and the months in spring, summer, and fall in the set S. The agricultural production function in (1)-(2) implies that the effect of a one-percent increase in monthly rainfall during winter on agricultural output is $\beta_w = \beta\widetilde{\alpha}_w$ while the effect of a one-percent increase in monthly rainfall during spring-fall is $\beta_s = \beta\widetilde{\alpha}_s$ where

$$\widetilde{\alpha}_w = \sum_{m \in W} \alpha_m \text{ and } \widetilde{\alpha}_s = \sum_{m \in S} \alpha_m. \tag{2.8}$$

Hence, $\widetilde{\alpha}_w/\widetilde{\alpha}_s$ is a measure of the importance for agricultural productivity of rainfall during winter relative to spring-fall.

Using the notation in (8), the rainfall risk measure in (4) can be written in terms of rainfall risk during spring-fall, rainfall risk during winter, and a covariance term,

$$RVar_c = \widetilde{\alpha}_s^2 RVar_c^s + \widetilde{\alpha}_w^2 RVar_c^w + \widetilde{\alpha}_s\widetilde{\alpha}_w RCov_c \tag{2.9}$$

where $RVar_c^s$ and $RVar_c^w$ are analogues of (4) for spring-fall and winter respectively

$$RVar_c^s = Var\left(\sum_{m \in S} \frac{\alpha_m}{\widetilde{\alpha}_s} \ln R_{mc}\right) \tag{2.10}$$

$$RVar_c^w = Var\left(\sum_{m \in W} \frac{\alpha_m}{\widetilde{\alpha}_w} \ln R_{mc}\right), \tag{2.11}$$

and $RCov_c$ is twice the covariance of spring-fall and winter rainfall

$$RCov_c = 2Cov\left(\sum_{m \in S} \frac{\alpha_m}{\widetilde{\alpha}_s} \ln R_{mc}, \sum_{m \in W} \frac{\alpha_m}{\widetilde{\alpha}_w} \ln R_{mc}\right). \tag{2.12}$$

It follows from (7) and the expression for rainfall risk in (9) that the importance of winter rainfall risk for membership in religious organizations relative to the importance of spring-fall rainfall risk is $(\widetilde{\alpha}_w/\widetilde{\alpha}_s)^2$. Hence, if the effect of winter rainfall on agricultural output relative to the effect of spring-fall rainfall, $\widetilde{\alpha}_w/\widetilde{\alpha}_s$, is small, the effect of winter rainfall risk on membership in religious organizations relative to the effect of spring-fall rainfall risk should be even smaller.

2.3 Estimating the Effect of Rainfall Risk on Religious Membership

Our empirical investigation of the link between rainfall risk and membership in religious organizations across US counties in the second half of the nineteenth century begins with a log-linearized version of (7),

$$\ln \textit{\textbf{RelMember}}_c = \varphi + \lambda RVar_c + \gamma \ln EY_c \tag{2.13}$$

where $\textit{\textbf{RelMember}}_c$ is the number of church members or seats, and rainfall risk $RVar_c$ and expected agricultural output EY_c are defined in (4) and (5) respectively. The parameter of interest is λ – the link between rainfall risk and membership in religious organizations – which according to (7) should be positive as long as relative risk aversion is above unity.

To estimate the link between rainfall risk and membership in religious organizations using (13) we need measures for rainfall risk $RVar_c$ in (4) and expected agricultural output EY_c in (5) for all counties in our sample. This requires county-level data on rainfall over a sufficiently long period of time, as well as values for β and α_m. Our main analysis is for the case where monthly rainfall enters the agricultural production function in (1)-(2) symmetrically, $\alpha_m = \alpha$. But we also examine the case where rainfall during the winter months has a different effect on agricultural output.

Symmetric effects of monthly rainfall When monthly rainfall enters the agricultural production function in (1)-(2) symmetrically, the rainfall risk measure in (4) becomes

$$RainfallRisk_c = Var\left(\frac{1}{12}\sum_{m=1}^{12} \ln R_{mc}\right) \tag{2.14}$$

and expected output in (5) can be written as

$$\ln EY_c = \ln \delta Z_c + \ln ER_c = \ln \delta Z_c + \ln E\left(\prod_{m=1}^{12} R_{mc}^{\frac{\beta}{12}}\right). \tag{2.15}$$

To get a sense for β – the average effect of rainfall on agricultural productivity – during the nineteenth century, we estimate the effect of rainfall on the county-level value of crops reported by the US Census in 1910, 1920, and 1930. The availability of multiple observations for each county allows us to take a within-county approach. Our estimating equation is based on (1),

$$\ln Y_{ct} = \text{county FE \& time effects} + \beta\left(\frac{1}{12}\sum_{m=1}^{12} \ln R_{mct}\right), \tag{2.16}$$

where Y_{ct} is the value of crops per unit of farmland. The county fixed effects (FE) capture all fixed county characteristics. The time effects capture changes over time and are allowed to vary by state. We also control for ln farmland and estimate specifications with lagged rainfall and temperature controls.

Substituting (14) and (15) into (13) yields our estimating equation for the link between rainfall risk and membership in religious organizations

$$\ln \textbf{\textit{RelMem}}ber_c = \lambda RainfallRisk_c + \gamma \ln ER_c + \phi X_c \tag{2.17}$$

where $RainfallRisk_c$ is defined in (14); $\ln ER_c$ captures the effect of rainfall on average output and is defined in (15) with β estimated using (16); and X_c stands for other county characteristics that may affect agricultural output or the cost of church membership, like soil quality, elevation, population, and area for example. $RainfallRisk_c$ and ER_c are calculated as the corresponding moments over the 1895-2000 period (the county rainfall data is only available since 1895).

When rainfall matters less during the winter months To get a sense for the link between membership in religious organizations on the one hand and rainfall risk during the winter months and during the spring-fall months on the other, we reestimate (17) after replacing the term for rainfall risk by

$$\lambda_s RVar_c^s + \lambda_w RVar_c^w + \delta RCov_c. \tag{2.18}$$

The variances and the covariance are defined in (10)-(12) and calculated as the corresponding moments over the 1895-2000 period. We take the effect of monthly rainfall during winter as well as during summer-fall to be symmetric, $\alpha_m/\widetilde{\alpha}_w = 1/3$ for $m \in W$ and $\alpha_m/\widetilde{\alpha}_s = 1/9$ for $m \in S$.

Our theoretical model implies that the importance of winter rainfall risk relative to spring-fall rainfall risk for religious membership in (17)-(18) should be linked to the importance of winter relative to spring-fall rainfall for agricultural productivity, $\widetilde{\alpha}_w/\widetilde{\alpha}_s$, see (7)-(9). To get a sense for $\widetilde{\alpha}_w/\widetilde{\alpha}_s$ we return to the agricultural production function in (16) but break up the rainfall effect into a spring-fall term and a winter term

$$\text{Rainfall effect} = \beta_s \left(\frac{1}{9} \sum_{m \in S} \ln R_{mct} \right) + \beta_w \left(\frac{1}{3} \sum_{m \in W} \ln R_{mct} \right) \tag{2.19}$$

where $\beta_s = \beta\widetilde{\alpha}_s$ and $\beta_w = \beta\widetilde{\alpha}_w$ according to (1)-(2). Reestimating the agricultural production function in (16) after substituting (19) for the rainfall effect allows us to obtain $\widetilde{\alpha}_w/\widetilde{\alpha}_s$ as the effect of winter rainfall, β_w, relative to the effect of spring-fall rainfall, β_s.

2.4 Data and Empirical Results

2.4.1 Data

Religious Membership 1850-1890 The decennial US Census in 1850-1890 collected information on churches at the county level. There are two measures of membership in religious organizations, the seating capacity of churches in 1850, 1860, 1870, and 1890 (the 1880 data were never published) and the number of church members in 1890. Our data refers to all denominations listed in the Census. These data are retrieved from ICPSR file 2896 (Haines, 2006). For summary statistics see the Appendix Table.

Climate Data Our rainfall data come from PRISM, which provides monthly rainfall data since 1895 on a 4x4 km grid.[3] PRISM was developed for the National Oceanic and Atmospheric Administration and the PRISM model is used by the US Department of Agriculture, NASA, and several professional weather channels.[4] We map the PRISM grid into counties to obtain monthly rainfall at the county level. PRISM also provides data on monthly average temperature which we process analogously to the rainfall data.

Soil and Elevation Data We control for 53 soil types using the US Department of Agriculture's SSURGO database.[5] We use these data to calculate the fraction of each county's land area falling into the different soil categories. The source of our elevation data is the Environmental System Research Institute.[6] We calculate the fraction of each county's land area falling into the following 11 bins: below 200 meters, 200 to 400 meters; 400 to 600 meters and so on up to 2000 meters; and above 2000 meters.

Other Data The data on land area, population, and value added in agriculture and manufacturing come from the US Census. Value added in manufacturing 1860-1890 is calculated as manufacturing output minus the cost of materials. Value added in agriculture is obtained as output minus the cost of fertilizers in 1890 and as output in agriculture in 1860-1870 as there is no information on fertilizer purchases. The data on the value of all crops produced and total farmland by county 1910-1930 also come from the US Census. The data are retrieved from ICPSR file 2896 (Haines, 2006).

2.4.2 Empirical Results

Table 1 contains our results on the effect of rainfall on the value of crops per unit of farmland from the US Census in 1910, 1920, and 1930 using the within-county estimation approach in (16). Our method of estimation is weighted least squares. We weight counties by their average farmland over the period as within-county changes in the value of crops per unit of farmland should be more closely related to county-level

[3] See www.prism.oregonstate.edu.
[4] See Deschenes and Greenstone (2007) who also use the PRISM data.
[5] See http://soils.usda.gov/surveys/geography/ssurgo/.
[6] See www.esri.com.

average rainfall when more land is under cultivation.[7] The value of crops reported in the US Census corresponds to the year preceding the census year so that t in (16) refers to 1909, 1919, and 1929. The rainfall "year t" data we use in column (1) goes from December $t-1$ to November t. That is, the rainfall year t encompasses the four seasons ending in year t, which facilitates comparisons when we allow for separate effects of rainfall during winter months and during spring-fall months.[8] Column (2) adds a control for the rainfall year $t-1$ which is defined analogously to rainfall year t and therefore goes from December $t-2$ to November $t-1$. The results in columns (1)-(2) indicate a statistically significant effect of rainfall in year t while the effect of rainfall in year $t-1$ is statistically insignificant. The effect of rainfall in year t implies that a one-percent increase in monthly rainfall in year t raises the value of crops produced by around 0.5 percent. The average temperature controls added in column (3) are statistically insignificant. This probably reflects in part that capturing the effect of temperature on agricultural productivity requires data on daily temperatures or even the distribution of temperature within a day, see Deschenes and Greenstone (2007) and Schlenker and Roberts (2009) for example.[9] Such data are not available for our period of analysis.

Tables 2-5 contain our results on the link between rainfall risk and membership in religious organizations. The estimating equation is (17) and the method of estimation least squares. The left-hand-side variable is either $\ln$ church membership (1890) or $\ln$ church seating capacity (1890, 1870, and 1860). The right-hand-side control $\ln ER$ in

[7]One reason is that idiosyncratic shocks to the output of different units of farmland are more likely to average out. Another reason is that our measure of average rainfall refers to the average in a county as a whole, not the average on cultivated land. The discrepancy between these two averages should tend to be smaller in counties with more farmland, holding the share of land under cultivation constant. Moreover, the discrepancy should also tend to be smaller in counties with a larger share of land under cultivation and counties with more farmland tend to have a larger share of land under cultivation in our data. To see these last two points in a concrete example, let F be the acres of farmland in a county and $\phi \in (0,1)$the share of land under cultivation. Take rainfall on acre $i$to be $R_i = R + \varepsilon_i$ with ε_ii.i.d. with mean zero and variance σ^2. Then the variance of the difference between rainfall per acre in the county and rainfall per acre on cultivated land is $\sigma^2(1-\phi)/F$. Hence, average rainfall in the county is a better proxy of average rainfall on cultivated land in counties with more farmland and counties with a greater share of land under cultivation. In any case, the unweighted results are similar to those in Table 1 in that all effects other than rainfall at t are statistically insignificant; the effect of rainfall at t is statistically significant at the 1% level but smaller than in Table 1, 0.27 as compared to 0.52 in the specification in column (3). Using the value of 0.27 to obtain $\ln ER$ in (17) does not affect any of our findings on the link between rainfall risk and religious membership (the point estimates change by very little).

[8]The 12 months of rainfall on the right-hand side of (16) would have also spanned the four seasons if we had used rainfall during the winter that starts in year t instead of the winter that ends in year t. But it seems more reasonable to presume that the value of crops in year t depends on rainfall during the winter that ends in year t than the winter that starts in year t.

[9]When we allow for separate effects of winter and spring-fall weather in Table 6, the $t-1$ spring-fall temperature enters positively and statistically significantly. The magnitude of the spring-fall temperature effect is such that our theoretical model implies a minor role of temperature risk relative to rainfall risk for religious membership, see the discussion of Table 7 on page 14.

(17) is calculated using a value for β of 0.52 based on the results in Table 1. Other controls used are $\ln$ population and ln land area; the share of land of a given soil type using a 53-category soil classification system; the share of land at a given elevation using 11 elevation bins; average elevation; average temperature over the period 1895-2000; and state fixed effects.

Table 2, column (1) shows that the link between rainfall risk and church membership in 1890 is statistically significant at the 1-percent level. The point estimate implies that a one-standard-deviation increase in rainfall risk is associated with an increase in church membership of around 12 percent (the cross-county standard deviation of rainfall risk is 0.054). Columns (2)-(3) split the full 1890 sample into counties with population densities below and above the median. Counties with relatively low population densities are more likely to depend mostly on agriculture.[10] Hence, if rainfall risk affects church membership through economic risk in the agricultural sector as in our theoretical model, we would expect a link between rainfall risk and church membership among these counties. Column (2) confirms this prediction as the link between rainfall risk and church membership in below-median population density counties is statistically significant at the 1-percent level. The point estimate implies that a one-standard-deviation increase in rainfall risk is associated with an increase in church membership of around 17 percent. On the other hand, the link between rainfall risk and church membership among counties with relatively high population densities – which are often urban counties that rely mainly on manufacturing and related services – in column (3) is estimated imprecisely and statistically insignificant.[11]

Table 2, columns (4)-(5) show the results when the full 1890 sample is split into counties with value added in agriculture relative to manufacturing above and below the median. The median value-added share of agriculture over agriculture plus manufacturing in 1890 is 0.87 and the average share in counties with above-median agriculture/manufacturing is 0.95. Hence, counties with above-median agriculture are essentially agricultural. As the economic repercussions of rainfall variability are stronger in agriculture, we expect a link between rainfall risk and church membership among these agricultural counties. The result in column (4) shows that the link between rainfall risk and church membership among these counties is in fact statistically significant at the 1-percent level. The point estimate implies that a one-standard-deviation increase in rainfall risk is associated with an increase in church membership of around 23 percent. On the other hand, among the less agricultural counties in column (5), there is no statistically significant link between rainfall risk and church membership (the average share

[10] Agricultural value added relative to agriculture plus manufacturing in below-median population density counties is larger than in the full sample, 0.82 as compared to 0.76, but the standard deviation is quite large (0.23).

[11] Our findings on the link between rainfall risk and religious membership in Tables 2-5 are not affected when we also control for the variance of the annual average temperature over the 1895-2000 period. The temperature variance always enters statistically insignificantly.

of agriculture over agriculture plus manufacturing in these counties is 0.43).

Table 3 reestimates the specifications in Table 2 using church seating capacity in 1890 as a measure of membership in religious organizations. The pattern of results is similar to the results for church membership. The link between rainfall risk and church seating capacity in the full sample in column (1) is statistically significant at the 1-percent level. The point estimate implies that a one-standard-deviation increase in rainfall risk is associated with an increase in church seating capacity of around 10 percent. When we split the sample according to population density in columns (2)-(3), we find that the link between rainfall risk and church seating capacity among counties with population densities below the median in column (2) is statistically significant with a p-value of 0.051. The point estimate implies that a one-standard-deviation increase in rainfall risk is associated with an increase in church seating capacity of around 13 percent. Among counties with relatively high population densities, the link between rainfall risk and church seating capacity is imprecisely estimated and statistically insignificant. Columns (4)-(5) split the full sample into those with agriculture/manufacturing value added above the median and those with agriculture/manufacturing below the median. Among the most agricultural counties in column (4), the link between rainfall risk and church seating capacity is statistically significant a the 1-percent level. The point estimate implies that a one-standard-deviation increase in rainfall risk is associated with an increase in church seating capacity of around 32 percent. Among the less agricultural counties in column (5), there is no statistically significant link between rainfall risk and church seating capacity.

Table 4 summarizes our results on the link between rainfall risk and church seating capacity in 1870. This sample is around 20 percent smaller than the 1890 sample. The pattern of results is similar to that for 1890 church seating capacity. The link between rainfall risk and church seating capacity in the full sample in column (1) is statistically significant at the 5-percent level. The point estimate implies that a one-standard-deviation increase in rainfall risk is associated with an increase in church seating capacity of around 13 percent, which is somewhat larger than the effect we estimated for 1890 church seating capacity. In columns (2)-(3) we consider the sample split according to population densities below and above the median. The link between rainfall risk and 1870 church seating capacity is statistically significant at the 1-percent level in counties with population densities below the median in column (2). The point estimate implies that a one-standard-deviation increase in rainfall risk is associated with an increase in church seating capacity of around 23 percent among these counties. On the other hand, the link between rainfall risk and church seating capacity among counties with relatively high population densities in column (3) is imprecisely estimated and statistically insignificant. Columns (4)-(5) consider the sample split according to value added in agriculture relative to manufacturing above and below the median. The median share of agriculture over agriculture plus manufacturing in 1870 is 0.89. The link between rainfall risk and church seating capacity among the agricultural counties in column (4) is statistically significant at the 5-percent level. The point estimate implies that a one-standard-deviation increase in rainfall risk is associated with an increase in church seating capacity of around 65 percent among these counties. Rainfall risk shows a weaker, but still statistically significant, link with 1870 church seating capacity among counties with agriculture/manufacturing value added below the median in column (5).

Table 5 contains our results on the link between rainfall risk and church seating capacity in 1860. The sample is around 30 percent smaller than the 1890 sample and 10 percent smaller than the 1870 sample. Results are similar to those for 1870 and 1890 church seating capacity. The link between rainfall risk and church seating capacity in the full sample in column (1) is statistically significant with a p-value of 0.054. The point estimate implies that a one-standard-deviation increase in rainfall risk is associated with an increase in church seating capacity of around 12 percent, which is similar to the finding for 1870 church seating capacity and somewhat larger than the estimate for 1890 church seating capacity. The sample split according to population densities below and above the median is in columns (2)-(3). The link between rainfall risk and 1860 church seating capacity is statistically significant at the 5-percent level in counties with population densities below the median in column (2). The point estimate implies that a one-standard-deviation increase in rainfall risk is associated with an increase in church seating capacity of around 13 percent, which is similar to the finding for 1890 church seating capacity and somewhat smaller than the estimate for 1870 church seating capacity. On the other hand, the link between rainfall risk and church seating capacity among counties with relatively high population densities in column (3) is imprecisely estimated and statistically insignificant. Columns (4)-(5) report the results of the sample split according to value added in agriculture relative to manufacturing above and below the median. In 1860, the median share of agriculture over agriculture plus manufacturing is 0.91. The link between rainfall risk and church membership is statistically significant at the 10-percent level among the agricultural counties in column (4) with a p-value of 0.082. The point estimate implies that a one-standard-deviation increase in rainfall risk is associated with an increase in church seating capacity of almost 65 percent. On the other hand, rainfall risk does not show a statistically significant link with church membership among counties with agriculture/manufacturing value added below the median in column (5).

In 1850, we do not find a statistically significant link between rainfall risk and church seating capacity. We attribute this to the smaller number of counties. The necessary data are available for approximately 1450 counties in 1850 compared to around 1820 counties in 1860; around 2070 counties in 1870; and around 2650 counties in 1890. Moreover, most of the counties lost in 1850 compared to 1860, 1870, or 1890 are counties with low population density and high agriculture/manufacturing value added – the type of counties where the link between rainfall risk and religious membership was statistically significant. The consequence of the drop in sample size between 1860 and 1850 can be illustrated by reestimating the link between rainfall risk and church seating capacity in the 1860 subsample of counties for which there is data in 1850. This always yields statistically insignificant estimates.

Table 6 examines how the effect of rainfall on the value of crops per unit of farmland in Table 1 changes when we distinguish between rainfall during the winter months on the one hand and during the months of spring, summer, and fall on the other. Column (1) reproduces column (2) of Table 1 where we control for rainfall in year t as well as year $t-1$. In column (2) we split rainfall during year t as well as during year $t-1$ into winter rainfall on the one hand and spring-fall rainfall on the other. The specification of the rainfall effects follows (19) but we also allow for lagged effects. The estimates can be interpreted as the effects on agricultural productivity of a one-percent increase in monthly rainfall during spring-fall and during winter in years t and $t-1$ respectively. We find that both spring-fall rainfall in year t and rainfall in the winter ending in year t have a statistically significant positive effect. A one-percent increase in monthly rainfall during spring-fall raises agricultural productivity by 0.33 percent and a one-percent increase in winter rainfall raises productivity by 0.15 percent. For year $t-1$, only spring-fall rainfall is statistically significant and enters positively. A one-percent increase in monthly rainfall during spring-fall of year $t-1$ increases agricultural productivity in year t by 0.28 percent. The results in columns (3)-(4) show that the effects of rainfall on agricultural productivity change little when we control for temperature.[12]

Table 7 summarizes our results on the link between winter and spring-fall rainfall risk on the one hand and membership in religious organizations on the other. The estimating equation is (17) with the rainfall risk term replaced by (18). The control variables are the same as in Tables 2-5. As rainfall during the spring-fall months is a significant determinant of agricultural productivity according to our results in Table 6, we should expect a link between spring-fall rainfall risk and religious membership. Winter rainfall matters less for agricultural productivity than spring-fall rainfall in Table 6 and we should therefore expect winter rainfall risk to matter less for membership in religious organizations according to our theoretical model. To get an idea of how much less winter rainfall risk should matter, note that (7), (9), and (19) imply that the importance of winter relative to spring-fall rainfall risk for membership in religious organizations can be calculated as $(\beta_w/\beta_s)^2$ with β_w and β_s the effects on agricultural productivity of winter and spring-fall rainfall respectively. However, in contrast to our estimating equation, our theoretical model did not feature lagged rainfall effects on agricultural productivity. When such lagged effects are incorporated into the theoretical model, the relative im-

[12]Reestimating the specifications in Table 6 without weighting by farmland also yields that rainfall during the winter months matters less than rainfall during the months of spring-fall. The main differences with Table 6 are that rainfall during spring-fall is only statistically significant in year $t-1$; and that rainfall during the winter ending in year t enters significantly positively while rainfall during the winter ending in year $t-1$ enters significantly negatively.

portance of winter compared to spring-fall rainfall risk for religious membership also depends on the correlation of rainfall in different years. In our data, the correlation of rainfall in different years is close to zero. The relative importance of winter relative to spring-fall rainfall risk implied by the theoretical model is therefore approximately $\left(\beta^2_{w,t} + \beta^2_{w,t-1}\right) / \left(\beta^2_{s,t} + \beta^2_{s,t-1}\right)$ with subscripts t and $t-1$ denoting year t and year $t-1$ rainfall effects respectively. This ratio is 0.11 according to the statistically significant rainfall estimates in column (4) of Table 6.[13] Hence, winter rainfall risk should matter approximately an order of magnitude less for membership in religious organizations than spring-fall rainfall risk according to our theoretical model.[14]

Table 7, column (1) presents our results on the link between spring-fall and winter rainfall risk on the one hand and 1890 church membership on the other. The link between spring-fall rainfall risk and church membership is statistically significant at the 1-percent level. The link between winter rainfall risk and church membership is statistically significant at the 5-percent level with a point estimate that is around 28 percent of the estimate on spring-fall rainfall risk. When we examine the link between spring-fall and winter rainfall risk on the one hand and 1890 church seating capacity on the other, we find a link between spring-fall rainfall risk and church seating capacity that is statistically significant at the 5-percent level. The link between winter rainfall risk and church seating capacity is statistically insignificant. The results for 1870 and 1860 church seating capacity are similar to those for 1890. The link between spring-fall rainfall risk and church seating capacity is statistically significant, while the link between winter rainfall risk and church seating capacity is statistically insignificant.[15] The covariance term is statistically insignificant in all specifications except for 1870 church seating capacity.

2.5 Conclusion

One way to look at religious organizations is as mutual assistance networks that are supported by a spiritual framework rewarding aid and charity. Such networks would

[13]The same approach can be used to calibrate the importance of spring-fall temperature risk – the variance over time of average spring-fall temperature – for religious membership relative to the importance of spring-fall rainfall risk. In this case the formula is $\omega^2_{s,t-1}/(\beta^2_{s,t} + \beta^2_{s,t-1})$ with $\omega_{s,t-1}$ the effect of $t-1$ spring-fall temperature on agricultural output. Using the statistically significant estimates in column (4) of Table 6 yields 0.056 which indicates that temperature risk should be considerably less important for religious membership than rainfall risk. When we add a control for the spring-fall temperature variance over the 1895-2000 period in our religious membership regressions in Table 7, it always enters statistically insignificantly.

[14]Reestimating the specifications in Table 6 without weighting by farmland implies a greater role of rainfall during winter for the value of crops and therefore a greater role of winter rainfall risk for religious membership. In this case the theoretical model implies that the effect of winter rainfall risk on religious membership should be about half the effect of spring-fall rainfall risk.

[15]Our findings on the link between rainfall risk and religious membership are unaffected when we also control for the variance of spring-fall average temperature over the 1895-2000 period.

have been especially valuable in communities facing substantial risk but lacking formal insurance structures, such as historical agricultural societies for example (McCleary and Barro, 2006).

Insurance of idiosyncratic risk provided by religious organizations should be more valuable in communities facing greater aggregate risk, which would make membership in religious organizations more attractive in high-risk environments. We investigate the link between membership in religious organizations and community risk across US counties in the second half of the nineteenth century. The US started into the second half of the nineteenth century as a mostly agricultural economy with little social insurance supplied by the government. As agriculture was mainly rainfed, local economies were subject to rainfall risk. It is possible to quantify the temporal variability of rainfall at the county level since 1895, and there is county-level data on the seating capacity of churches from 1850 to 1890 and their membership in 1890. This allows us to investigate whether membership in religious organizations in the second part of the nineteenth century was greater in local communities likely to have been facing more rainfall risk.

We find that church seating capacity in 1860-1890 and church membership in 1890 were significantly larger in US counties likely to be subject to more rainfall risk. This effect is present among the most agricultural counties and among counties with low population densities, but not among less agricultural or more densely populated counties. Among the most agricultural counties, a one-standard-deviation increase in rainfall risk is associated with an increase in church seating capacity of around 32 percent in 1890 and 65 percent in 1860.

2.6 References and Tables

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Tables

Table 1: Rainfall and the Value of Crops Produced in 1909, 1919, and 1929

	(1)	(2)	(3)
Rainfall t	0.515***	0.511***	0.516***
	(0.183)	(0.178)	(0.181)
Rainfall t-1		0.177	0.178
		(0.144)	(0.144)
Temperature t			0.0246
			(0.0377)
Temperature t-1			0.0212
			(0.0438)
County FE	yes	yes	yes
Time effects	yes	yes	yes
Farmland	yes	yes	yes
R2	0.633	0.634	0.634
Number of counties	8787	8787	8787

Notes: The left-hand-side variable is the ln value of crops per acre produced at the county level in 1909, 1919, and 1929. The estimating equation employed in column (1) is (16), see Section 3 for more details. Columns (2)-(4) add controls for lagged rainfall and temperature. See Section 4.1 for the data sources and Section 4.2 pages 10-11 for more details on the specification of the rainfall and temperature controls. The method of estimation is weighted least squares with weights equal to the farmland of counties. All specifications control for ln farmland; time effects; and county fixed effects. The time effects are allowed to vary by state. Standard errors account for arbitrary heteroskedasticity and are clustered at the county level. ***, **, and * denote significance at the 1%, 5%, and 10% level respectively.

Table 2: Rainfall Risk and Church Membership in 1890

		Sample split: population density		Sample split: agriculture/manufacturing value added	
	Baseline	Below median	Above median	Above median	Below median
	(1)	(2)	(3)	(4)	(5)
Rainfall risk	2.122***	2.865***	0.771	3.606***	-1.426
	(0.631)	(0.933)	(2.385)	(1.160)	(1.045)
lnER	0.175	0.109	0.569*	0.328	-0.207
	(0.185)	(0.167)	(0.331)	(0.324)	(0.184)
Soil shares	yes	yes	yes	yes	yes
Elevation shares	yes	yes	yes	yes	yes
Average elevation	yes	yes	yes	yes	yes
Average temperature	yes	yes	yes	yes	yes
Size	yes	yes	yes	yes	yes
State FE	yes	yes	yes	yes	yes
R2	0.914	0.876	0.882	0.903	0.921
Number of counties	2693	1346	1347	1341	1341

Notes: The left-hand-side variable is ln church membership at the county level in 1890. The estimating equation employed is (17). The right-hand-side measure of rainfall risk is based on 1895-2000 Rainfall data. LnER is based on the same rainfall data and a value of β of 0.52. See Section 3 for more details on the specification and Section 4.1 for data sources. Other right-hand-side controls used are ln population and ln land area of the county; the share of the land of a given soil type using a 53-category soil classification system; the share of the land at a given elevation using 11 elevation bins; average elevation; average temperature over the period 1895-2000; and state fixed effects. The method of estimation is least squares. Standard errors account for arbitrary heteroskedasticity and are clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% level respectively.

Table 3: Rainfall Risk and Church Seating Capacity in 1890

		Sample split: population density		Sample split: agriculture/manufacturing value added	
	Baseline	Below median	Above median	Above median	Below median
	(1)	(2)	(3)	(4)	(5)
Rainfall risk	1.742***	2.253*	2.776	5.587***	-1.633
	(0.633)	(1.119)	(2.067)	(1.885)	(1.280)
lnER	0.896**	0.709*	0.574	1.546***	0.355*
	(0.343)	(0.358)	(0.357)	(0.541)	(0.195)
Soil shares	yes	yes	yes	yes	yes
Elevation shares	yes	yes	yes	yes	yes
Average elevation	yes	yes	yes	yes	yes
Average temperature	yes	yes	yes	yes	yes
Size	yes	yes	yes	yes	yes
State FE	yes	yes	yes	yes	yes
R2	0.902	0.870	0.832	0.895	0.916
Number of counties	2651	1325	1326	1322	1323

Notes: The left-hand-side variable is ln church seating at the county level in 1890. The estimating equation employed is (17). The right-hand-side measure of rainfall risk is based on 1895-2000 Rainfall data. LnER is based on the same rainfall data and a value of β of 0.52. See Section 3 for more details on the specification and Section 4.1 for data sources. Other right-hand-side controls used are ln population and ln land area of the county; the share of the land of a given soil type using a 53-category soil classification system; the share of the land at a given elevation using 11 elevation bins; average elevation; average temperature over the period 1895-2000; and state fixed effects. The method of estimation is least squares. Standard errors account for arbitrary heteroskedasticity and are clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% level respectively.

Table 4: Rainfall Risk and Church Seating Capacity in 1870

		Sample split: population density		Sample split: agriculture/manufacturing value added	
	Baseline	Below median	Above median	Above median	Below median
	(1)	(2)	(3)	(4)	(5)
Rainfall risk	2.268**	3.531***	0.897	7.220**	1.733*
	(1.074)	(0.957)	(4.379)	(3.388)	(0.916)
lnER	0.449*	0.392*	0.724	1.426*	0.294
	(0.246)	(0.218)	(0.495)	(0.558)	(0.318)
Soil shares	yes	yes	yes	yes	yes
Elevation shares	yes	yes	yes	yes	yes
Average elevation	yes	yes	yes	yes	yes
Average temperature	yes	yes	yes	yes	yes
Size	yes	yes	yes	yes	yes
State FE	yes	yes	yes	yes	yes
R2	0.825	0.678	0.799	0.721	0.898
Number of counties	2068	1034	1034	1033	1034

Notes: The left-hand-side variable is ln church seating at the county level in 1870. The estimating equation employed is (17). The right-hand-side measure of rainfall risk is based on 1895-2000 Rainfall data. LnER is based on the same rainfall data and a value of β of 0.52. See Section 3 for more details on the specification and Section 4.1 for data sources. Other right-hand-side controls used are ln population and ln land area of the county; the share of the land of a given soil type using a 53-category soil classification system; the share of the land at a given elevation using 11 elevation bins; average elevation; average temperature over the period 1895-2000; and state fixed effects. The method of estimation is least squares. Standard errors account for arbitrary heteroskedasticity and are clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% level respectively.

Table 5: Rainfall Risk and Church Seating Capacity in 1860

		Sample split: population density		Sample split: agriculture/manufacturing value added	
	Baseline	Below median	Above median	Above median	Below median
	(1)	(2)	(3)	(4)	(5)
Rainfall risk	2.079*	2.282**	4.417	8.999*	-0.444
	(1.047)	(0.989)	(3.033)	(5.006)	(0.989)
lnER	0.0640	-0.292	1.100*	1.543*	-0.275
	(0.456)	(0.494)	(0.571)	(0.784)	(0.255)
Soil shares	yes	yes	yes	yes	yes
Average elevation	yes	yes	yes	yes	yes
Average temperature	yes	yes	yes	yes	yes
Elevation shares	yes	yes	yes	yes	yes
Size	yes	yes	yes	yes	yes
State FE	yes	yes	yes	yes	yes
R2	0.805	0.665	0.807	0.726	0.873
Number of counties	1822	911	911	909	909

Notes: The left-hand-side variable is ln church seating at the county level in 1860. The estimating equation employed is (17). The right-hand-side measure of rainfall risk is based on 1895-2000 Rainfall data. LnER is based on the same rainfall data and a value of β of 0.52. See Section 3 for more details on the specification and Section 4.1 for data sources. Other right-hand-side controls used are ln population and ln land area of the county; the share of the land of a given soil type using a 53-category soil classification system; the share of the land at a given elevation using 11 elevation bins; average elevation; average temperature over the period 1895-2000; and state fixed effects. The method of estimation is least squares. Standard errors account for arbitrary heteroskedasticity and are clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% level respectively.

Table 6: Rainfall and the Value of Crops Produced in 1909, 1919, and 1929

	(1)	(2)	(3)	(4)
Rainfall t	0.511***		0.516***	
	(0.178)		(0.181)	
-- Rainfall t, Spring-Fall		0.326*		0.325*
		(0.186)		(0.194)
-- Rainfall t, Winter		0.148***		0.147***
		(0.0363)		(0.0382)
Rainfall t-1	0.177		0.178	
	(0.144)		(0.144)	
-- Rainfall t-1, Spring-Fall		0.279***		0.314***
		(0.0837)		(0.0837)
-- Rainfall t-1, Winter		-0.0482		-0.0497
		(0.0666)		(0.0644)
Temperature t			0.0246	
			(0.0377)	
-- Temperature t, Spring-Fall				-0.0203
				(0.0459)
-- Temperature t, Winter				-0.00891
				(0.0214)
Temperature t-1			0.0212	
			(0.0438)	
-- Temperature t-1, Spring-Fall				0.107**
				(0.0453)
-- Temperature t-1, Winter				-0.0208
				(0.017)
County FE	yes	yes	yes	yes
Time effects	yes	yes	yes	yes
Farmland	yes	yes	yes	yes
R2	0.634	0.638	0.634	0.639
Number of counties	8787	8787	8787	8787

Notes: The left-hand-side variable is the ln value of crops per acre at the county level in 1909, 1919, and 1929 . The estimating equation is (16) with the rainfall term split into rainfall over the winter months and the spring-fall months as in (19), see Section 3 and Section 4.2 pages 10-11&14 for more details on the specification of the rainfall and temperature controls. The data sources are in Section 4.1. Column (1) and (3) are reproduced from Table 1. The method of estimation is weighted least squares with weights equal to the farmland of counties. All specifications control for ln farmland; time effects; and county fixed effects. The time effects are allowed to vary by state. Standard errors account for arbitrary heteroskedasticity and are clustered at the county level. ***, **, and * denote significance at the 1%, 5%, and 10% level.

Table 7: Rainfall Risk in Spring-Fall and Winter

	Church membership	Church seating capacity		
	1890	1890	1870	1860
	(1)	(2)	(3)	(4)
Rainfall risk: Spring-Fall	0.949***	1.281**	1.351***	1.631***
	(0.291)	(0.515)	(0.465)	(0.577)
Rainfall risk: Winter	0.268**	0.108	-0.175	-0.524
	(0.122)	(0.153)	(0.349)	(0.454)
RCov(Spring-Fall, Winter)	0.327	-0.784	2.462*	0.753
	(0.407)	(0.563)	(1.394)	(1.861)
lnER	yes	yes	yes	yes
Soil shares	yes	yes	yes	yes
Elevation shares	yes	yes	yes	yes
Average elevation	yes	yes	yes	yes
Average temperature	yes	yes	yes	yes
Size	yes	yes	yes	yes
State FE	yes	yes	yes	yes
R2	0.914	0.903	0.825	0.805
Number of counties	2693	2651	2068	1822

Notes: The left-hand-side variable is ln church membership or ln chuch seating at the county level from the US Census in 1890, 1870, or 1860. The estimating equation employed is (17) with the rainfall risk term replaced by (18). The right-hand-side measures of rainfall risk are based on 1895-2000 Rainfall data. LnER is based on the same rainfall data and a value of β of 0.52. See Section 4.1 data sources and Sections 3 and 4.2 for more details on the specification. Other right-hand-side controls used are ln population and ln land area of the county; the share of the land of a given soil type using a 53-category soil classification system; the share of the land at a given elevation using 11 elevation bins; average elevation; average temperature over the period 1895-2000; and state fixed effects. The method of estimation is least squares. Standard errors account for arbitrary heteroskedasticity and are clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% level respectively.

Appendix Table: Summary Statistics

Panel A: Full sample

	1890			1870			1860			1850		
Variable	Obs	Mean	StdDev	Obs	Mean	StdDev	Obs	Mean	StdDev	Obs	Mean	StdDev
Ln Church Membership	2693	8.14	1.37	-	-	-	-	-	-	-	-	-
Ln Church Seating	2651	9.07	1.32	2068	8.53	1.30	1822	8.59	1.25	1448	8.48	1.31
Rainfall Risk	2693	0.06	0.05	2068	0.05	0.04	1822	0.04	0.04	1448	0.04	0.03
Rainfall Risk (Spring-Fall)	2693	0.07	0.07	2068	0.06	0.07	1822	0.06	0.06	1448	0.05	0.05
Rainfall Risk (Winter)	2693	0.22	0.24	2068	0.15	0.12	1822	0.14	0.10	1448	0.12	0.06
Cov (Spring-Fall, Winter)	2693	0.01	0.02	2068	0.01	0.02	1822	0.01	0.01	1448	0.01	0.01
Average Temperature	2693	12.29	4.47	2068	12.78	4.10	1822	13.01	3.94	1448	13.13	3.71
Ln Population	2693	9.47	1.06	2068	9.32	0.97	1822	9.28	0.94	1448	9.23	0.90
Ln Area	2693	6.49	0.76	2068	6.37	0.71	1822	6.31	0.65	1448	6.26	0.58
Population per Square Mile	2693	73.1	669.65	2068	74.5	1128	1822	67.2	1010	1448	58.45	729.4
Agricultural Value Added relative to Agriculture plus Manufacturing	2682	0.76	0.26	2067	0.81	0.21	1818	0.84	0.21	1446	0.78	0.23

Panel B.1: Counties with population density above the median

	1890			1870			1860		
Variable	Obs	Mean	StdDev	Obs	Mean	StdDev	Obs	Mean	StdDev
Ln Church Membership	1347	8.94	0.81	-	-	-	-	-	-
Ln Church Seating	1326	9.89	0.69	1034	9.38	0.82	911	9.37	0.83
Rainfall Risk	1347	0.04	0.03	1034	0.03	0.02	911	0.03	0.01
Average Temperature	1347	12.27	3.38	1034	11.90	3.19	911	12.06	3.23
Ln Population	1347	10.10	0.73	1034	9.94	0.71	911	9.86	0.68
Ln Area	1347	6.12	0.53	1034	6.09	0.54	911	6.07	0.56
Population per Square Mile	1347	133.7	943.10	1034	138	1593	911	122.9	1426
Agricultural Value Added relative to Agriculture plus Manufacturing	1347	0.70	0.27	1034	0.78	0.22	911	0.81	0.21

Panel B.2: Counties with population density below the median

	1890			1870			1860		
Variable	Obs	Mean	StdDev	Obs	Mean	StdDev	Obs	Mean	StdDev
Ln Church Membership	1346	7.34	1.35	-	-	-	-	-	-
Ln Church Seating	1325	8.26	1.30	1034	7.67	1.11	911	7.82	1.12
Rainfall Risk	1346	0.08	0.06	1034	0.06	0.06	911	0.06	0.05
Average Temperature	1346	12.31	5.35	1034	13.65	4.69	911	13.97	4.34
Ln Population	1346	8.84	0.97	1034	8.69	0.77	911	8.70	0.78
Ln Area	1346	6.85	0.79	1034	6.65	0.75	911	6.56	0.63
Population per Square Mile	1346	12.46	9.09	1034	11.24	7.12	911	11.63	6.81
Agricultural Value Added relative to Agriculture plus Manufacturing	1335	0.82	0.23	1033	0.85	0.20	907	0.87	0.20

Panel C.1: Counties with agricultural share above the median

Variable	1890 Obs	1890 Mean	1890 StdDev	1870 Obs	1870 Mean	1870 StdDev	1860 Obs	1860 Mean	1860 StdDev
Ln Church Membership	1341	7.71	1.29	-	-	-	-	-	-
Ln Church Seating	1322	8.68	1.31	1033	8.23	1.10	909	8.30	1.10
Rainfall Risk	1341	0.07	0.05	1033	0.05	0.03	909	0.04	0.03
Average Temperature	1341	13.12	4.52	1033	14.34	3.63	909	14.54	3.56
Ln Population	1341	9.10	0.95	1033	9.05	0.75	909	9.03	0.75
Ln Area	1341	6.52	0.76	1033	6.33	0.58	909	6.30	0.54
Population per Square Mile	1341	22.34	15.47	1033	20.90	14.12	909	20.62	13.49
Agricultural Value Added relative to Agriculture plus Manufacturing	1341	0.95	0.04	1033	0.95	0.03	909	0.96	0.03

Panel C.2: Counties with agricultural share below the median

Variable	1890 Obs	1890 Mean	1890 StdDev	1870 Obs	1870 Mean	1870 StdDev	1860 Obs	1860 Mean	1860 StdDev
Ln Church Membership	1341	8.60	1.27	-	-	-	-	-	-
Ln Church Seating	1323	9.48	1.18	1034	8.83	1.40	909	8.90	1.32
Rainfall Risk	1341	0.05	0.05	1034	0.05	0.05	909	0.04	0.05
Average Temperature	1341	11.45	4.24	1034	11.21	3.95	909	11.48	3.68
Ln Population	1341	9.87	0.99	1034	9.59	1.07	909	9.54	1.03
Ln Area	1341	6.45	0.77	1034	6.41	0.82	909	6.33	0.74
Population per Square Mile	1341	124.4	946.25	1034	128	1594	909	114.1	1428
Agricultural Value Added relative to Agriculture plus Manufacturing	1341	0.43	0.25	1034	0.67	0.22	909	0.71	0.23

Chapter 3

FINANCIAL LIBERALIZATION AND BANK FAILURES: THE UNITED STATES FREE BANKING EXPERIENCE (JOINT WITH F. SPARGOLI)

3.1 Introduction

In the last three decades many financial crises occurred after countries liberalized their financial system. This is not a new phenomenon. Between 1837 and 1863 several U.S. states experienced a period of bank entry and failure after introducing a free banking system. With the introduction of free banking, governments gave up their control powers over bank entry and made the banking business more responsive to market rather than political forces.[1] Under the free banking law any individual could establish a bank provided that certain capital and circulation requirements defined by the law were satisfied.[2] We exploit the state-year variation in the introduction of free banking laws to test whether the relaxation of entry restrictions leads to more bank failures.

The existing literature about financial crises has found a systematic negative relation between financial liberalization and financial stability (see e.g. Demirguc-Kunt and Detragiache (1998) and Kaminsky and Reinhart (1999)). Using cross-country panels,

[1] During the free banking era, that is the period between the closing of the Second Bank of the United States in 1836 and the passage of the National Banking Act in 1863, the regulation of banks was in the responsibility of individual states free from federal intervention.

[2] Typical requirements were a minimum level of capital and a bond-secured banknote circulation (see e.g. Rockoff (1972) and Hasan (1987)). See Section 2 for a more detailed description of banking regulation in antebellum U.S.

these papers cannot control for unobservable or difficult-to-measure omitted variables that might confound the relation between liberalization and financial stability. For example, financial crises might be caused by severe economic downturns, or excessive risk taking fueled by state implicit guarantees, rather than liberalization.

Our contribution to the financial stability literature is twofold. First, we focus on an institutional setup where banks could not count on any state implicit guarantee, as opposed to studies based on contemporary data. Between 1837 and 1863, the US did not have any central bank that could act as a lender of last resort. Deposits and banknotes were uninsured, and governments never bailed any bank out. Focusing on the 1837-1863 US allows us to rule out the hypothesis that implicit state guarantees, rather than liberalization, drive our results. Second, the federal structure of the U.S. allows us to use a regression discontinuity setup where we can exploit within-country across states, rather than across country variation in financial liberalization. Our identification strategy compares contiguous counties lying on the border of states that passed a free banking law at different points in time[3]. The main advantage of our approach is to use geographically close counties that are more likely to have similar growth paths and face similar shocks. In contrast to a traditional cross-county panel approach, we would not base our inference on the comparison of heterogenous treatment and control groups like comparing counties of Wisconsin and Alabama. This makes the threat that unobservable variables drive the results less credible.

On the historical front, the existing literature links the frequent bank failures in the US between 1837 and 1863 to the characteristics of banks chartered under the free banking law[4]. We take a more systemic approach and investigate how the introduction of free banking affected the stability of the county banking system as a whole. We consider also the traditionally chartered banks in free banking states as a potential source of financial instability since they could coexist with banks chartered under the free banking law in most of the states. However, we do not claim that the introduction of free banking laws caused the many financial panics that occurred in the 1837-1863 US.

We use Warren Weber (2011a)'s antebellum balance sheet and bank census data. Weber's census of banks lists every entry and exit of chartered banks during the antebellum period. For most of these banks, Weber provides yearly balance sheets that we aggregate up at the county level. Our main finding is that the fraction of failed banks increased roughly by 2 percent in counties where the state government switched to a free banking system. To link our result more closely to the existing literature on the instabil-

[3]Our study is not the first one in the finance literature which exploits policy discontinuities at the state border to investigate how regulatory changes affect bank performance. Huang (2008) uses contiguous county-pairs separated by state borders to investigate the local economic effects of relaxing bank-branching restrictions in the US between 1975 and 1990. Further studies using policy discontinuities at state borders are among others Holmes (1998) and Dube et al. (2010).

[4]See, for example, Rockoff (1972); Rolnick and Weber (1984, 1985); Economopoulos (1990) and Hasan and Dwyer (1994).

ity of the free banking era, we complement our analysis by examining the instability of free and state banks at the individual bank level. Consistent with the existing literature we find that the probability of failure of free banks is significantly higher than for banks in states that did not introduce a free banking law.[5] We also find that the probability of failure of traditionally chartered banks in states which switched to free banking is relatively higher than their counterparts in states without free banking laws. We consider our results at the individual level as suggestive evidence that the financial instability of the free banking era cannot be entirely explained by the fragility of free banks.

The theoretical banking literature considers competition as an important channel through which financial liberalization affects financial stability. Banking theory has, however, conflicting views on how competition affects financial stability. The competition-fragility view suggests that liberalization leads to fiercer competition, which erodes banks' charter value and provides incentives to take excessive risk (Marcus (1984), Chan et al. (1986) and Keeley (1990)). In a more competitive environment banks reduce their effort to screen borrowers properly, because they earn fewer informational rents (Allen and Gale (2000a)). Models supporting the competition-fragility view predict that liberalization measures facilitating entry and increasing bank competition lead to more financial instability. The competition-stability view developed by Boyd and DeNicolo (2005) suggests that banks with greater market power charge higher interest rates which in turn induces borrower to choose riskier projects. Recently, both views were tested empirically, with ambiguous results.[6] We consider the introduction of the free banking law as a measure increasing bank competition in two ways. First, free banking gave any individual the possibility to open up a bank in profitable markets where the incumbent enjoyed monopoly rents. Second, free banking could make incumbent - traditionally chartered – banks act in a more competitive manner in order to prevent bank entry.

From a historical perspective, our paper contributes also to a strand of literature that investigates the effect of free banking on entry. Historians generally consider the free banking as a system facilitating entry, but the empirical literature provides conflicting interpretations. Ng (1988) finds that bank assets in states enacting free banking did not grow relative to regional or national trends. This means that free banking did not lead to more entry. After controlling for a number of factors likely to influence bank entry, Bodenhorn (1993) finds that free banking had little influence on entry into six antebel-

[5] Our individual level estimates for free banks are in line with the recent study of Jaremski (2010), which uses Warren Weber's dataset to test the two main hypotheses for free banks failure: falling asset prices (Rolnick and Weber (1984, 1985)) and under-diversification of bank portfolios (Economopoulos (1990)). Jaremski results are in favor of the falling asset prices hypothesis, where free banks failed, because they were exposed to systemic risk. Jaremski also shows that free banks were significantly more likely to fail than traditionally chartered banks, but in contrast to our approach he does not distinguish between state banks in free banking vs. non-deregulated states.

[6] Examples of recent empirical studies are Berger et al. (2009) and Anginer et al. (2012). See also the surveys of Beck (2008) and Vives (2010) and the references therein for further information on relation between competition and financial stability.

lum urban markets. Economopoulos and O'Neill (1995), however, find in their study of ten free banking states that growth in bank capital and net entry was more responsive to underlying economic influences in free banking states than in states that retained legislative chartering. Bodenhorn (2008) shows that free banking led to a consistent increase in bank entry using county level data of New York, Massachusetts and Pennsylvania. Compared to these studies we have the advantage of having a comprehensive dataset about the free banking era at hand. Our contribution is to provide a rigorous empirical investigation of the competition channel, both in terms of identification and sample coverage. We find that the entry rate increased approximately by 6 percent in counties where the state government switched to a free banking system. When looking at incumbent banks, our results support the view that competition affected also traditionally chartered banks. We find that incumbent state banks significantly reduced their market share after free banking was introduced. This evidence, together with the evidence about bank failures, is consistent with the hypothesis that the introduction of free banking led to fiercer bank competition and, in turn, to more risk taking by both traditionally chartered and free banks.[7]

The reminder of our paper is organized as follows. Section 2 provides a brief overview of the free banking era. Section 3 describes our data and how we construct our samples. Section 4 explains the estimation strategy. Section 5 presents our main results. Section 6 discusses further robustness checks. The last section concludes.

3.2 The Free Banking Era (1837-1863)

The antebellum US was an emerging economy with sustained output growth and rapid capital accumulation, but also prone to financial crises.[8] While historians usually viewed banks as the root of instability, Bodenhorn (2000) emphasizes their role in the economic development of the antebellum US. Bodenhorn points out the importance of banks in the provision of means of payments, in the accumulation of savings and in their effi-

[7] A paper in a similar spirt, but investigating the effect branch banking had on competition and financial stability is the work of Carlson and Mitchener (2006). The authors show that the increased competition by branch banking in the 1920s and 1930s drives weak banks out of the banking market. This consolidation, Carlson and Mitchener argue, increases financial stability. The effect of lifting branch banking restrictions in the U.S. starting in the 1970s was the center of Jayaratne and Strahan (1996, 1998) work. Using U.S. state-level panel data, Jayaratne and Strahan find that the relaxation of bank branch restrictions increased bank efficiency and spurred economic growth after the branching reforms. Huang (2008), by using a more sophisticated identification strategy, finds in contrast to Jayaratne and Strahan (1996) only minor effects for the local economy after statewide branching restrictions are lifted.

[8] Many regional and four nationwide panics (in 1833, 1837, 1839 and 1857) happened during the antebellum period. Jalil (2012) identifies regional and nationwide panics in the US and provides explanations of their causes. Studies by Calomiris and Schweikart (1991), O'Grada and White (2003), Kelly and O'Grada (2000) and Temin (1969) focus on specific panic episodes.

cient allocation. This is important for the purpose of our study as it highlights that the function of banks at that time was similar to modern banks. Still, they differed in some important aspects. First, banknotes were not issued by a central bank but by individual banks. Banknotes were the most common means of payment.[9] They entitled the holder to demand redemption in specie at any time. Banknotes were liabilities for antebellum banks as deposits are for banks nowadays. Second, banks lent mainly short term (typically at three months) to finance trading transactions by firms. As Bodenhorn (2000) claims, this practice did not reflect individual banks' preferences, but rather the financing needs of firms.

All US states had their own system of bank regulation in the antebellum period.[10] Before 1837, states exerted their control over banks mainly through bank chartering.[11] In order to open up a bank, the aspiring banker had to apply for a bank charter. The state government decided whether to grant the charter and, in case it did, it set the requirements the bank had to satisfy. Requirements differed from bank to bank, but generally consisted of an initial capital level and constraints on the allocation of funds.[12] It was usually difficult to obtain a bank charter, because states wanted to limit the number of banks in order to protect the interests of incumbents.[13] This policy constrained supply in an economy that needed bank credit to finance its development. Starting from Michigan in 1837, New York and Georgia in 1838, US states responded to economy's needs and introduced free banking laws (see Table 1). Free banking laws allowed any individual to open up a bank subject to the requirements defined by the law. Banks must have a minimum amount of capital and the banknote circulation must have been fully backed by government bonds or mortgages (see e.g. Rockoff (1972), Hasan (1987) and Jaremski (2010)). In some states free banking laws also defined shareholders' liability and constrainted circulation to specie. Nowhere they imposed constraints on the allocation of credit.

The regulatory system had implications for the activities of banks. Table 2 compares the balance sheet of an average bank in states that introduced a free banking law with states sticking to the traditional charter policy. We choose to report the aggregate rather than the individual balance sheet of an average free and state bank, because of two reasons. First, the focus of our analysis is on aggregate outcomes. Second, using

[9] Bodenhorn (2000) argues that banknotes widely circulated within the US, and that there was an active market for banknote discounts (see also Gorton (1996) and Jaremski (2011)).

[10] See Bodenhorn (2002), Hammond (1957) and Schweikart (1987) for a description of banking in the antebellum US.

[11] Few states had general banking laws. Banking laws were usually defining managers and shareholders liability and tying banknote circulation to bank capital or specie. In no state the law allowed individuals to open a bank without a charter. See Dewey (1910), Knox (1903), and Hendrickson (2011) for banking regulation in the 19th century US.

[12] Some charters required banks to lend to companies involved in the construction of railroads or canals, or to invest in state bonds (see e.g. Knox (1903).

[13] See Bodenhorn (2006, 2008).

state averages rather than bank averages allows us to capture the effect that competition could have on the liability structure of banks.[14] On the asset side, public bond holdings were larger in states that passed a free banking law by almost a factor of 10. This is not surprising since free banks were required to back their banknotes with public bonds. The higher fraction of public bonds was compensated mainly by a lower fraction of loans. On the liability side, the capital of banks in states that introduced a free banking law was 10 percent lower, whereas deposits were 5 percent larger. The evidence about capital is consistent with the findings in Hanson et al. (2010).[15] The higher ratio of deposits to assets suggests that free banks had to issue deposits in order to lever up, because their banknote circulation had to be fully backed by bonds.

3.3 Data and Sample Construction

3.3.1 Data

Our analysis builds on the individual balance sheets and the census of banks in the antebellum U.S. collected by Weber (2011a).[16] Starting from 1789, the census of state banks contains the location, name, the beginning and ending dates for all banks that existed in the United States prior to 1861. Weber provides also information on the charter type of the bank, i.e. whether a bank was traditionally chartered or established under the free banking law, and whether the bank failed, closed or still existed in 1861. The bank balance sheet dataset contains detailed information about banks' assets and liabilities that U.S. antebellum banks had to report to the state banking authorities. We merge the census of state banks with the individual bank balance sheet data.

According to Bodenhorn (2008) banking in the free banking era was generally a local affair both in legal and economic terms. Bodenhorn uses county-level data to study bank entry in nineteenth century New York, Massachusetts and Pennsylvania. In general, when studying local banking markets it has been a convention in the banking literature to use a county as the unit of analysis (see e.g. Berger et al. (2009); Black and Strahan (2002) and Huang (2008)). Many researchers use county level data to study the impact of bank activities on economic output (Ashcraft (2005); Calomiris and Mason (2003) and Gilber and Kochin (1998)). Following Bodenhorn's argumentation, we take the county as the appropriate unit of analysis to study the effect of free banking on financial stability. We match the location of each bank to its respective county and aggregate our dataset at the county level.

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[14] Hanson et al. (2010) argues that competition leads banks to decrease their capital holdings in order to save on funding costs.

[15] See footnote 15.

[16] Both datasets are publicly available at Warren Weber's data archive: http://www.minneapolisfed.org/research/economists/wewproj.html.

The focus of our empirical analysis is on the period from 1833 to 1860.[17] We choose 1833 as starting year, i.e. four years before the first state – Michigan – introduced a free banking system, in order to have a sufficiently large pre-treatment window to implement a difference in difference (DID) estimation. With 1833 as starting point we avoid also measurement error due to data availability problems of earlier years.[18] Our empirical analysis ends in 1860 right before the outbreak of the U.S. Civil War. The 1861-1865 Civil War was an atypically large, negative shock to the US economy. This event could have affected the banking sector in an unusual way.

3.3.2 Sample Construction

We analyze the effect of introducing a system of free banking on county banking markets using the DID method on a sample of contiguous counties sharing the same state border. We compare outcomes in counties where the state introduced a free banking law, i.e. the treatment group, versus outcomes in counties where the state retained the traditional chartering policy, i.e. the control group. We restrict our analysis to a pre and post treatment period. In the pre-treatment period both states did not introduce a system of free banking. We define the pre-treatment period as the 5-year interval before one of the two bordering states adopted the free banking law. The post-treatment period is the time interval in which one of the bordering states has a free banking law while the other still sticks to a traditional charter policy. Following Huang (2008), we select a county as control group only if it belongs to a state that introduced free banking at least three years after its bordering state. Once both states have adopted a free banking law, we drop the corresponding counties from our analysis.

We use the 1860 census boundary file map downloaded from the National Historical Geographic Information System (NHGIS) to identify all U.S. counties that straddle a state border. ArcGIS is used to find the set of antebellum counties lying on state boundaries. We assign a unique border segment identifier to any contiguous border county.[19] Our sample consists only of border segments for which we have at least five years of observations for any county on each side of the state border. We believe that using contiguous border counties is a well suited method to estimate the impact of deregulation effects if we have enough border segments with a different regulatory status and there is substantial variation in financial instability between treatment and control group over the period of interest.

[17] We exclude Washington D.C. from our sample, since it was a federal district.

[18] During the free banking era banks sent annual reports to the state authorities, and the problem of missing data is less problematic. Despite the comprehensive data availability, there are a few cases where balance sheet information is missing for certain years. In these cases we imputed the missing values of these banks. We describe our imputation method in detail in a supplementary appendix available upon request.

[19] Note that a border county can be in multiple border segments.

The advantage of comparing only contiguous border counties is their similarity in observable and unobservable characteristics such as underlying growth trends. Traditional cross-state panel studies like Jayaratne and Strahan (1996, 1998) implicitly assume that a randomly chosen U.S. county is a good control independently of the state in which the treatment occurs. We compare in section 5 our proposed method with the traditional approach where we consider all U.S. counties to highlight potential differences in the estimated coefficients. Table 3 provides the summary statistics for both samples.

3.4 Estimation Strategy

For the traditional approach we estimate the effect of introducing free banking on financial stability as follows:

$$y_{csr,t} = \lambda_c + \lambda_{r,t} + \beta TE_{s,t} + \gamma FBEXP_{s,t} + \Gamma' X_{csr,t} + \epsilon_{csr,t}. \tag{3.1}$$

Our variable of interest, $y_{csr,t}$, is the failure rate of banks. The failure rate of banks is constructed as the number of banks that failed in county c of state s and region r at time t, normalized by the total number of banks in c at time t. The county fixed effects, λ_c, capture time-invariant factors such as geography and any other determinants of the county steady state. We include regional time period fixed effects, $\lambda_{r,t}$, that control for the variation between U.S. census regions.[20] Hence, our estimates are based only on the variation within each macro-region. The treatment effect, $TE_{s,t}$ is a binary variable which takes the value one for all years t since a state decided to switch to a free banking system. We use a state specific linear trend starting from the liberalization year, $FBEXP_{s,t}$, to take into account the experience of states with the free banking system. The matrix, $X_{csr,t}$, includes time varying county-specific control variables. We cluster the error term, $\epsilon_{csr,t}$, at the state level to ensure that the computed standard errors of our estimates are robust to arbitrary correlation across counties in each US state.

Our identification strategy, which we call border-county approach, follows closely the regression discontinuity design of Black (1999) and Fack and Grenet (2010). Our preferred estimation equation is:

$$y_{cbs,t} = \alpha + \lambda_{b,t} + \beta TE_{s,t} + \gamma FBEXP_{s,t} + \Gamma' X_{cbs,t} + \epsilon_{cbs,t}. \tag{3.2}$$

The important difference to equation (1) is the inclusion of border segment time period fixed effects, $\lambda_{b,t}$. The border segment time period fixed effects control for any common observable and unobservable factors varying across state borders which would otherwise bias our findings. Equation (2) pools the estimates by exploiting the within-border segment variation across all border segments. Our identifying assumption is that any

[20] The U.S. census regions in our sample are: New England, Mid-Atlantic, Midwest and the South.

within-border segment difference in the treatment effect is uncorrelated to the within-border segment difference in the error term, that is $E(TE_{s,t}, \epsilon_{cbs,t}) = 0$. Following Huang (2008), we do not consider any time-invariant factor in our preferred estimation equation (2) since our objective is to choose our set of variables as parsimoniously as possible. If there are any time-invariant observable or unobservable factors that affect financial instability, they should not bias the point estimate of the difference-in-difference treatment effect. If a certain time-invariant county-specific factor affects financial stability, it should affect the treatment and control group in the pre and post treatment period in the same way and hence not confound our results.

We use two-dimensional clustering to account for within-state over time and within border segment over time correlations. Hence, our estimates are robust to arbitrary correlation across counties in each US state and across counties in each border segment. The two-dimensional clustering accounts also for the mechanical correlation induced by the presence of a single county in multiple border segments.

To complement our analysis we examine the instability of free and traditionally chartered banks at the individual bank level. We compare the probability of failure of free banks and traditionally chartered banks in liberalized states relative to traditionally chartered banks in non-liberalized states by using the following linear probability model:

$$y_{isr,t} = \lambda_s + \lambda_{r,t} + \lambda_i t + \beta FB_i + \gamma TE_{s,t} \times TB_i + \Gamma' X_{isr,t-1} + \epsilon_{isr,t}. \tag{3.3}$$

The probability of failure, $y_{isr,t}$, is a binary variable which takes the value one in case a bank fails at time t. The parameters λ_s, $\lambda_{r,t}$ and $\lambda_i t$ denote state fixed effects, regional time period fixed effects and a trend for bank i, respectively. We do not control for individual fixed effects, because we are interested in the effect of both types of charters, which is usually time-invariant.[21] The variable FB_i is a dummy variable taking the value one if a bank is a free bank.[22] The interaction term, $TE_{s,t} \times TB_i$, captures the effects of being a traditionally chartered bank (TB) after a state switched to free banking. The omitted category is the group of traditionally chartered banks in non-liberalized states. The matrix, $X_{isr,t-1}$, includes lagged time varying individual balance-sheet controls.

Since we are also interested in how incumbent banks respond to removals of entry barriers, we employ the following DID estimation:

$$y_{isr,t} = \lambda_i + \lambda_{r,t} + \beta TE_{s,t} + \Gamma' X_{isr,t-1} + \epsilon_{isr,t}. \tag{3.4}$$

[21] In three states the free banking law was repealed (Michigan, 1838; Connecticut, 1855 and Tennessee, 1858). The banks in these states who were initially chartered under the free banking law continued their business under the traditional system, given they did not fail before. In some rare cases, state banks continued their business under a free banking charter after their original charter expired (New York). We exclude the very few banks that switched their charter from our analysis.

[22] Note, that we are not able to use a DID estimation here, since free banks only existed after a state introduced free banking.

We use the market share of incumbents defined as the fraction of own assets over total assets in a county as dependent variable. Individual, time-invariant, bank characteristics are absorbed by the inclusion of individual fixed effects, λ_i.[23] Our variable of interest is the treatment effect, $TE_{s,t}$, which takes the value one after a state introduced a free banking system.

3.5 Results

3.5.1 Financial Instability

Table 4 reports the treatment effect for the failure rate of banks. Column (1) shows the estimates of the traditional approach using equation (1). In the first specification we only control for county fixed effects, regional time period fixed effects and states experience with free banking. In column (2), we add to the regression controls for county-specific differences in the banking sector, such as, the ratios of loans to assets, specie to assets, capital to assets, public bonds to assets and the average asset size. The failure rate of banks in counties which were exposed to treatment is approximately 1.2 percent higher and significant at the 5 percent significance level.[24] We are aware that the results of the traditional approach might suffer from omitted variables bias and that the inclusion of the county-specific bank controls further exacerbates endogeneity problems. The potential bias introduced by the inclusion of the county-specific bank controls in column (2) seems, however, not very large and might by therefore only a minor concern.

With the border-county approach, we try to tackle the endogeneity issues inherent in the traditional approach. Columns (3)-(6) present our main results using equation (2). Column (3) reports the estimates when controlling for border segment time period fixed effects and states experience with free banking. In column(4), we restrict the relevant variation even further and also include regional time period fixed effects in the regression to wipe out any heterogeneous trends at the regional-level which could still confound our results. In column (5) we include the same county-specific bank controls as in column (2). One advantage of our border-county approach is to obtain coefficients for control variables using out-of sample information, that is, we can also exploit information about counties of border segments where none of the states adopted a free banking system.[25] The out-of sample method solves the problem of having potentially biased coefficients of the bank controls due to the introduction of free banking. That

[23]In contrast to equation (3), we are able to apply a DID estimation, since we are only looking at incumbent banks, i.e. banks which are in the sample during the whole period, 1833-1860.

[24]Note, that there is a drop of observations, because we do not have information on the balance sheet controls for all counties at hand. This drop in observations does not change our result, qualitatively.

[25]We provide a detailed explanation how we used the out-of sample method to obtain point estimates for the bank controls which are not contaminated by (in-)sample-selection problems in the supplementary appendix available upon request.

is, if we include bank controls in-sample, one does not consider how bank controls normally affect financial stability, since the coefficient on the bank controls could be contaminated by the deregulation itself.[26] In the final column, we adopt the out-of sample method proposed by Huang (2008) and use the out-of sample coefficients for the county-specific bank controls to obtain an unbiased estimate of our treatment effect. Our coefficient of interest in column (6) shows the treatment effect after we subtracted the bank controls – using the estimated out-of sample coefficient – from the dependent variable. The failure rate estimates using the border-county approach all range between 1.5 and 2.6 percent and all estimates are at least significant at 10 percent significance level. In general the treatment effect in the border-county approach is larger, which implies that the traditional approach yields to downward biased estimated coefficients.

Table 5 shows the bank-level results for the probability of failure of free and state banks.[27] Column (1) shows the estimates controlling for state fixed effects and regional time period fixed effects. In column (2), we add a bank specific trend and the lagged asset size of a bank as control to avoid problems of reverse causality. In the last column we add further lagged bank-specific control variables, such as, the ratios of loans to assets, specie to assets, capital to assets, public bonds to assets and deposits to assets. We find that free banks had a probability of failure approximately 3.5 percent higher than traditionally charted banks in non-liberalized states. This result is significant at the 1 percent significance level. Our findings are consistent with the evidence in the free banking literature that (e.g. Rolnick and Weber (1984, 1985), Economopoulos (1990), and Jaremski (2010)) free banks had a significantly higher probability of failure than traditionally chartered banks. The individual dataset also allows us to examine whether the traditionally chartered banks in liberalized states were more likely to fail than their counterpart in non-liberalized states. We find that traditionally chartered banks in liberalized states had a significantly higher likelihood of failure. Our results show that not only free banks, but also the traditionally chartered banks were a source of instability in the liberalized states.

3.5.2 Competition

Table 6 reports the treatment effect for the entry rate of banks. Similar to the failure rate, the entry rate of banks is constructed as the number of banks that entered in a county in a given year normalized by the total number of banks in the county. We use the same specifications as in Table 4, except that we control only for the asset size of banks in columns (2), (5) and (6) since it is unlikely that the balance sheet ratios affect the entry decision of banks. We find that liberalization leads to significantly higher entry rates. The entry rate estimates all range between 6 and 11.2 percent and are significant at the 1

[26] We refer to Huang (2008) for more details about the out-of sample method.

[27] The specification is equation (3).

percent significance level using the traditional approach and at least significant at the 10 percent significance level using the border-county approach. Our estimates confirm the results of Economopoulos and O'Neill (1995) and Bodenhorn (2008) that free banking led to more bank entry.

The Herfindahl-Hirschman-Index (HHI) is a standard measure used in the banking literature to measure the degree of concentration in the banking sector. Table 7 shows the results using the HHI on deposits as dependent variable. Columns (1)-(2) report the results for the traditional approach.[28] In both specifications we find a negative and statistically significant association between the introduction of free banking and the HHI on deposits. Our estimates turn insignificant once we use the border-county approach.[29] We use the turnover rate as further evidence that free banking increased competition. We believe that the turnover rate is a good measure to capture the dynamics of the banking sector. We expect a competitive banking system to have higher bank entry and failures than a regulated banking system. We define the turnover rate as the sum of new and failed banks over the total number of banks in a county in a given year. In Table 8, using the same specifications as in Table 6, we show that the turnover ratio was significantly higher in free banking states.

Since we are also interested in how incumbent banks reacted to increased banking competition in free banking states, we analyze the adjustments of incumbent's market share. The incumbent's market share is defined as the fraction of the incumbent's asset relative to the total assets of a county. We use equation (4) to estimate the treatment effect. In the first specification we only control for individual fixed effects and regional time period fixed effects. In column (2), we add to the regression a bank-specific linear trend and the lagged asset size. In Column (3) we add additional lagged variables to control for bank-specific differences, such as, the ratios of loans to assets, specie to assets, capital to assets ratio, public bonds to assets and deposits to assets. The results of columns (1)-(3) in Table 9 are in line with our macro level findings. Traditionally chartered banks experienced a significant drop of about 3% in their asset share after a state switched to a free banking system.[30] We consider our findings as substantive evidence that free banking led to more banking competition and that competition also affected incumbent banks. Overall, our results on competition and financial stability are consistent with the hypothesis that the introduction of free banking led to fiercer bank competition and, in turn, to more risk taking by both traditionally chartered and free banks.

[28]In columns (2), (5) and (6) we add the asset size and the average ratio of deposits to assets in a county as further controls.

[29]Results are qualitatively similar for the HHI loans and are available upon request.

[30]Using estimation equation (3), we also find that free and traditionally chartered banks in free banking states had significantly lower market shares than banks in non-liberalized states. These results are provided in the supplementary appendix and available upon request.

3.6 Robustness

A major concern when using a DID approach is that anticipation effects drive the results. If banks anticipate the deregulation event before the actual introduction they might adjust their behavior in advance. We include leads of the treatment effect up to three years in equation (2) and test the significance of their cumulative sum in order to examine whether anticipation effects contaminate our findings. In the supplementary appendix we report the results for the failure and entry rate. Neither the failure rate nor the entry rate displays any significant anticipation effect. For both cases the cumulative effect is not significant until the introduction of the free banking system in time t. The insignificance of the cumulate effect indicates that our results are not driven by any anticipation effects.

Spurious effects at the state border constitute a further threat to the internal validity of our border-county approach. We construct a placebo sample to address this concern. We match the border counties of the deregulated states with adjacent – hinterland – counties of the same state and assume that the hinterland counties are counterfactually not affected by the free banking law.[31] We re-run equation (2) for our main variables of interest (failure and entry rate) using the constructed placebo sample. If spurious effects are not a concern, the treatment effect in these regressions should be insignificant. We report the results of the placebo sample in the supplementary appendix. None of the specifications show a statistically significant treatment effect, indicating that spurious effects at the state border are not contaminating our border-county estimates.

A potential threat to our border-county design consist in financial instability spillovers, since treatment and control counties are only separated by state borders. Theory suggests various channels through which shocks to few banks propagate to the whole banking system. In an incomplete information setup, a bank run in a region might signal problems at banks in another region. The arrival of bad news might cause self-fulfilling expectations of a bank run in the other region.[32] Contagion might also occur through interbank claims, as the default of a bank in the network might cause the default of its creditor banks[33]. In our case, the presence of contagion attenuates the coefficient of the treatment effect towards zero, so that we can consider our estimates for the failure rate of banks as a lower bound.

Other potential confounders for our identification strategy are state-year varying legislation correlated with the introduction of the free banking system. During the 19th century, U.S. states used usury laws to regulate the maximum legal interest rate a bank can charge on a loan. More financially liberal states might not only switch to a free banking system, but also lift restrictions on the maximum legal interest rate. A laxer

[31] We refer to counties contiguous to border counties in the same state as hinterland counties, if they do not share any border with another state.

[32] As in Diamond and Dybvig (1983).

[33] See for example Allen and Gale (2000b) and Freixas et al. (2000).

usury law increases the potential pool of borrowers thereby making bank entry more attractive, but it also increases risk taking, because it allows banks to lend to riskier borrowers.[34] We collected data on usury laws from Holmes (1892) to test whether the presence of state-year varying usury laws might contaminate our findings. Columns (1) and (2) of Table 10 report the results for the failure rate (Panel A) and the entry rate (Panel B) when we control for state usury laws. Including state usury laws does not alter our main results, in both cases the coefficient remains positive and statistically significant.[35]

Similar to the traditional banking charter policy, state governments authorized the formation of non-financial incorporations by special charters. The evolution of the charter policy for non-financial corporations resembles the charter policy of the banking sector. During the 19th century, U.S. states gradually lifted barriers to entry for non-financial corporations by introducing general incorporation laws. When states liberalized their charter policy, new firms established under general incorporation laws could potentially spur the demand for external finance thereby making entry in the banking sector more profitable. Granting the privilege of limited liability, incorporation laws could encourage risk-taking by the new firms thereby increasing the probability of failure of banks. Hence, the introduction of general incorporation laws and not free banking could drive our results. To address these concerns, we exploit the state-year variation in the adoption of general incorporation laws reported in Evans (1948). Columns (3) and (4) of Table 10 provide the results for the failure and entry rate. In both cases the sign of treatment effect coefficient is positive and remains statistically significant.

State-specific liability insurance systems, clearing arrangements and branch-banking laws impose a further threat to the identification strategy, since they affect the probability of bank failure. Our evidence might not be driven by the introduction of free banking laws, but whether states adopted those arrangements. In New England, most of the banks joined the Suffolk Banking System (1827-1858), a privately organized banknotes clearing system. By clearing notes for New England banks, the Suffolk Banking System objective was to prevent bank failures and to act as lenders of last resort.[36] Since the Suffolk Banking System was a regional clearing system operating only in New England, we can control for it by including region time varying fixed effects. Six states, New York, Vermont, Indiana, Michigan, Ohio and Iowa established state-specific liability insurance systems during the antebellum period with the objective to reimburse creditors of insolvent banks. We collect information about the period of time a liabil-

[34]Rockoff (2003) provides a detailed examination of the economic history of usury laws in the United States. For a study of the political economy of U.S. state usury laws we refer to Benmelech and Moskowitz (2010).

[35]Columns (1) and (2) display the results for the traditional and the border-county approach, respectively.

[36]See Hammond (1957) for an early interpretation of the Suffolk Banking System. More recent studies are Mullineaux (1987) and Calomiris and Kahn (1996).

ity insurance system existed in these states from Weber (2011b) and Klebaner (2005) to test whether state-year variation in liability insurance systems contaminate our results.[37] The coefficient of the treatment effect reported in columns (5) and (6) of Table 10 remains qualitatively unaffected when controlling for liability insurance systems. Calomiris and Schweikart (1991) argue that branch-banking states in the South were better able to cope with the financial panic in 1857 and experienced low bank failure rates, because of cooperative planning. In Columns (7) and (8) of Table 10 we add a binary variable which equals one if branch banking existed in a state at time t to the list of controls.[38] Including a dummy for branching does not change our results.

The decision to suspend the convertibility of banknotes during a crisis period could also confound our results lowering the probability of bank failure. If states that did not adopt a free banking system suspended convertibility in crisis periods, our evidence might not be driven by the introduction of free banking laws. We identify statewide bank suspensions by using the information on national-wide and local panics from Jalil (2012). The last two columns of table 10 report the results. We do not find any evidence that statewide bank suspensions affect our results.[39]

3.7 Conclusion

Eighteen US states introduced free banking laws between 1837 and 1860. Free banking laws subtracted the costitution of banks from the discretion of governments and allowed any individual to establish a bank subject to the requirements defined by the law. We exploit these historical events to investigate the relation between liberalization and financial instability. The fact that the liberalization measure varies over time and across states allows us to use an identification strategy relying on the variation across contiguous counties separated by state borders. The similarity of shocks and trends among bordering counties mitigates the threat that unobservable variables correlated to liberalization confound the result.

In line with the existing evidence, our results support the hypothesis that liberalization leads to financial instability. We find that the introduction of free banking laws caused the failure of a significantly larger fraction of banks. In line with the antebellum history literature, we provide evidence that free banks were more likely to fail than

[37] We construct a binary variable which equals one if a state had a liability insurance system in time t. Note that in some states only certain types of banks (e.g. in Ohio and Indiana state banks and their branches) were members of the insurance system, other states (e.g. Vermont) required new chartered banks to join the system, but decided later on to base membership on a voluntarily basis. We refer to Weber (2011b) for more details about the antebellum liability insurance systems.

[38] We use the information on branch banking from Weber (2011a)'s database.

[39] Jalil (2012) lists among other things the emergence of bank suspensions as requirement to identify financial crises. A financial crisis in his series is only defined if there were bank suspensions in a given month (year).

other banks. More interestingly, we show that free banking also increased the individual probability of failure of incumbent banks. As suggested by banking theory, we consider increased bank competition as a possible explanation for our results. We find that free banking led to more bank entry and eroded the market share of incumbent banks. These results suggest that bank competition causes more risk taking.

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3.9 Figures and Tables

Table I: The Eighteen U.S. Free Banking States

STATES	***YEAR***	
MICHIGAN	1837;1857	(a)
NEW YORK	1838	
GEORGIA	1838	
ALABAMA	1849	
NEW JERSEY	1850	
MASSACHUSETTS	1851	
VERMONT	1851	
OHIO	1851	
ILLINOIS	1851	
CONNECTICUT	1852	(b)
INDIANA	1852	
WISCONSIN	1852	
TENNESSEE	1852	(c)
FLORIDA	1853	
LOUISIANA	1853	
MINNESOTA	1858	
IOWA	1858	
PENNSYLVANIA	1860	

(a) Michigan suspended the free banking law in 1838 and reenacted it in 1857. Source: Rockoff (1972).
(b) Connecticut repealed the free banking law in 1855. Source: Rockoff (1972).
(c) Tennessee repealed free banking in 1858. Source: Schweikart (1987).

Table II: Comparison of Balance Sheet Items

Asset Side

Items	*Free Banking States*	*Traditional Banking States*
Loans	0.70	0.75
Cash	0.06	0.07
Due from banks	0.09	0.08
Notes other banks	0.03	0.03
Real Estate	0.03	0.02
Public Bonds	0.06	0.01
Other Assets	0.04	0.04

Liability Side

Items	*Free Banking States*	*Traditional Banking States*
Capital	0.41	0.50
Circulation	0.31	0.29
Deposits	0.18	0.13
Due to banks	0.03	0.03
Profits	0.03	0.02
Other Liabilities	0.04	0.03

Table III: Summary Statistics

PART A	***TRADITIONAL APPROACH***				
VARIABLES	*Obs*	*Mean*	*Sd*	*Min*	*Max*
Entry Rate	9089	0.07	0.22	0.00	1.00
Failure Rate	9089	0.01	0.10	0.00	1.00
Turnover Rate	9089	0.11	0.27	0.00	2.00
HHI (Deposits)	7199	0.65	0.33	0.03	1.00
Treatment Effect (TE)	9089	0.34	0.47	0.00	1.00
Loans to Assets	7397	0.67	0.20	0.00	1.00
Deposits to Assets	7397	0.13	0.10	0.00	0.84
Public Bonds to Assets	7397	0.05	0.13	0.00	1.00
Capital to Assets	7397	0.41	0.14	0.00	1.00
Cash to Assets	7397	0.08	0.07	0.00	0.69
Log (Assets)	7397	8.84	1.32	4.38	14.36
Experience Free Banking	9089	2.91	5.38	0.00	24.00

PART B	***BORDER-COUNTY APPROACH***				
VARIABLES	*Obs*	Mean	Sd	Min	Max
Entry Rate	2683	0.07	0.22	0.00	1.00
Failure Rate	2683	0.01	0.09	0.00	1.00
Turnover Rate	2683	0.10	0.26	0.00	2.00
HHI (Deposits)	2182	0.65	0.34	0.03	1.00
Treatment Effect (TE)	2683	0.29	0.45	0.00	1.00
Loans to Assets	2245	0.68	0.19	0.00	1.00
Deposits to Assets	2245	0.13	0.09	0.00	0.84
Public Bonds to Assets	2245	0.04	0.11	0.00	0.84
Capital to Assets	2245	0.40	0.13	0.00	0.99
Cash to Assets	2245	0.07	0.06	0.00	0.56
Log (Assets)	2245	8.71	1.10	5.20	12.32
Experience Free Banking	2683	2.10	4.21	0.00	22.00

TABLE IV

FREE BANKING AND FINANCIAL STABILITY
– COUNTY-LEVEL ANALYSIS –

	FAILURE RATE					
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Effect (TE)	0.0128**	0.0121**	0.0148***	0.0260***	0.0144*	0.0211***
	(0.00579)	(0.00440)	(0.00521)	(0.00351)	(0.00817)	(0.00723)
Observations	9089	7397	2601	2601	2186	2163
R^2	0.098	0.079	0.006	0.008	0.031	0.012
Experience Free Banking	yes	yes	yes	yes	yes	yes
County FE	yes	yes	no	no	no	no
Region Time FE	yes	yes	no	yes	yes	yes
Border Segment Time FE	no	no	yes	yes	yes	yes
Balance Sheet Controls	no	yes	no	no	yes	yes*

Columns (1)-(2) report the results for the traditional approach. Robust standard errors clustered at the county level in parentheses. We report in columns (3)-(6) the results for the border-county approach. Standard errors clustered at the state and border segment in parentheses: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. In columns, (2), (5) and (6), we include the following balance sheet controls: the ratio of loans to assets, specie to assets, capital to assets, public bonds to assets and the average asset size (estimates not reported in the table). We provide a detailed description of the control variables included in the supplementary online appendix. *In column (6), we control for the balance-sheet variables out-of sample. See Section (5) for further details.

TABLE V

FREE BANKING AND FINANCIAL STABILITY
– BANK-LEVEL ANALYSIS –

FAILURE RATE			
	(1)	(2)	(3)
Free Banks	0.0351***	0.0286***	0.0302***
	(0.00709)	(0.00773)	(0.00797)
$TE \times TraditionalBanks$	0.00924**	0.00719*	0.00698*
	(0.00399)	(0.00383)	(0.00379)
Observations	25807	21422	21376
R^2	0.118	0.095	0.098
State FE	yes	yes	yes
Region Time FE	yes	yes	yes
Bank Specific Trend	no	yes	yes
Balance Sheet Controls	no	yes	yes

Robust standard errors clustered at the county level in parentheses: *** p<0.01, ** p<0.05, * p<0.1. In column (2) we include the following balance sheet control: the lagged asset size. In column (3) we add to the regression: the ratio of loans to assets, specie to assets, capital to assets, public bonds to assets and deposits to assets (estimates not reported in the table). Traditional banks are the omitted category. See Section (5) for further details.

TABLE VI

FREE BANKING AND COMPETITION
– COUNTY-LEVEL ANALYSIS –

ENTRY RATE

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Effect (TE)	0.0601***	0.0611***	0.0666*	0.112***	0.0682**	0.0603**
	(0.0189)	(0.0177)	(0.0342)	(0.0416)	(0.0280)	(0.0257)
Observations	9089	7397	2601	2601	2186	2163
R^2	0.103	0.127	0.005	0.010	0.012	0.004
Experience Free Banking	yes	yes	yes	yes	yes	yes
County FE	yes	yes	no	no	no	no
Region Time FE	yes	yes	no	yes	yes	yes
Border Segment Time FE	no	no	yes	yes	yes	yes
Balance Sheet Controls	no	yes	no	no	yes	yes*

Columns (1)-(2) report the results for the traditional approach. Robust standard errors clustered at the county level in parentheses. We report in columns (3)-(6) the results for the border-county approach. Standard errors clustered at the state and border segment in parentheses: *** p<0.01, ** p<0.05, * p<0.1. In columns, (2), (5) and (6), we include the following balance sheet control: the average asset size (estimate not reported in the table). We provide a detailed description of the control variables included in the supplementary online appendix. *In column (6), we control for the balance-sheet variables out-of sample. See Section (5) for further details.

TABLE VII

FREE BANKING AND COMPETITION
– COUNTY-LEVEL ANALYSIS –

HERFINDAHL-HIRSCHMAN-INDEX (DEPOSITS)

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Effect (TE)	-0.0605**	-0.0612**	0.0823	-0.0540	0.00911	0.0179
	(0.0290)	(0.0246)	(0.0835)	(0.0619)	(0.0496)	(0.0623)
Observations	7084	7084	2114	2114	2114	2114
R^2	0.248	0.444	0.024	0.065	0.576	0.031
Experience Free Banking	yes	yes	yes	yes	yes	yes
County FE	yes	yes	no	no	no	no
Region Time FE	yes	yes	no	yes	yes	yes
Border Segment Time FE	no	no	yes	yes	yes	yes
Balance Sheet Controls	no	yes	no	no	yes	yes*

Columns (1)-(2) report the results for the traditional approach. Robust standard errors clustered at the county level in parentheses. We report in columns (3)-(6) the results for the border-county approach. Standard errors clustered at the state and border segment in parentheses: *** p<0.01, ** p<0.05, * p<0.1. In columns, (2), (5) and (6), we include the following balance sheet controls: the average asset size and the ratio of deposit to assets (estimates not reported in the table). We provide a detailed description of the control variables included in the supplementary online appendix. *In column (6), we control for the balance-sheet variables out-of sample. See Section (5) for further details.

TABLE VIII

FREE BANKING AND COMPETITION
– COUNTY-LEVEL ANALYSIS –

TURNOVER RATE

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Effect (TE)	0.0753***	0.0747***	0.0983**	0.156***	0.0995***	0.0889***
	(0.0260)	(0.0238)	(0.0442)	(0.0571)	(0.0351)	(0.0300)
Observations	9089	7397	2601	2601	2186	2163
R^2	0.078	0.089	0.007	0.013	0.014	0.006
Experience Free Banking	yes	yes	yes	yes	yes	yes
County FE	yes	yes	no	no	no	no
Region Time FE	yes	yes	no	yes	yes	yes
Border Segment Time FE	no	no	yes	yes	yes	yes
Balance Sheet Controls	no	yes	no	no	yes	yes*

Columns (1)-(2) report the results for the traditional approach. Robust standard errors clustered at the county level in parentheses. We report in columns (3)-(6) the results for the border-county approach. Standard errors clustered at the state and border segment in parentheses: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. In columns, (2), (5) and (6), we include the following balance sheet control: the average asset size (estimate not reported in the table). We provide a detailed description of the control variables included in the supplementary online appendix. *In column (6), we control for the balance-sheet variables out-of sample. See Section (5) for further details.

TABLE IX

FREE BANKING AND COMPETITION
– BANK-LEVEL ANALYSIS –

INCUMBENTS MARKET SHARE

	(1)	(2)	(3)
Treatment Effect (TE)	-0.0339*	-0.0314*	-0.0339**
	(0.0179)	(0.0174)	(0.0165)
Observations	9546	9181	9181
R^2	0.152	0.163	0.183
Individual Bank FE	yes	yes	yes
Region Time FE	yes	yes	yes
Bank Specific Trend	no	yes	yes
Balance Sheet Controls	no	yes	yes

Robust standard errors clustered at the county level in parentheses: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. In column (2) we include the following balance sheet control: the lagged asset size. In column (3) we add to the regression: the ratio of loans to assets, specie to assets, capital to assets, public bonds to assets and deposits to assets (estimates not reported in the table). Traditional banks are the omitted category. See Section (5) for further details.

TABLE X

ROBUSTNESS

	Usury Laws		Incorporation Laws		Insurance Systems		Branches		Suspensions	
PANEL A: FAILURE RATE	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment Effect (TE)	0.0120**	0.0246***	0.0109**	0.0192**	0.0129**	0.0269***	0.0108**	0.0255***	0.0124***	0.0252***
	(0.00871)	(0.00439)	(0.00280)	(0.00432)	(0.00502)	(0.00484)	(0.00449)	(0.00431)	(0.00448)	(0.00471)
PANEL B: ENTRY RATE	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment Effect (TE)	0.0611***	0.0577**	0.0608***	0.0627**	0.0583***	0.0608**	0.0597***	0.0535**	0.0621***	0.0585**
	(0.0179)	(0.0264)	(0.0196)	(0.0273)	(0.0159)	(0.0298)	(0.0189)	(0.0240)	(0.0178)	(0.0278)
Observations	7397	2163	7397	2163	7397	2163	7397	2163	7397	2163
R^2 (PANEL A)	0.079	0.018	0.079	0.012	0.080	0.014	0.079	0.013	0.079	0.013
R^2 (PANEL B)	0.127	0.004	0.127	0.004	0.129	0.004	0.127	0.005	0.127	0.004
Sample	Traditional	Border	Traditional	Border	Traditional	Border	Traditional	Border	Traditional	Border

Columns (1), (3), (5), (7) and (9) report the results for the traditional approach. Robust standard errors clustered at the county level in parentheses. We report in columns (2), (4), (6), (8) and (10) the results for the border-county approach. Standard errors clustered at the state and border segment in parentheses: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. For the traditional approach we use for panel A specification (2) of table IV (see page 24) and specification (2) of table VI (see page 26) for panel B. For the border-county approach we use for panel A specification (6) of table IV (see page 24) and specification (6) of table VI (see page 26) for panel B.

Chapter 4

CULTURAL DIVERSITY AND ECONOMIC GROWTH: EVIDENCE FROM THE US DURING THE AGE OF MASS MIGRATION (JOINT WITH M. BRÜCKNER)

4.1 Introduction

During the 1850-1920 period the US experienced a mass inflow of immigrants: more than 30 million people migrated from Europe to the US, with the average annual immigration inflow rate measuring about 0.75 percent of the total US population (Hatton and Williamson, 1998). We exploit this historically unique inflow of immigrants to the US to study the effects that changes in the cultural composition of the US population had on output growth. Because immigrants came from different European countries, the mass immigration wave not only affected the overall share of foreign-born in the US. It also affected significantly the diversity of the working and voting-age population.[1]

A by now well-established literature has investigated the effects of cultural diversity on economic growth in the cross-section of countries.[2] Our aim is to contribute to this literature in two main dimensions. First, our empirical analysis is based on comparing how within-county changes in cultural diversity affect within-county changes in output. An often made critique of the cross-country growth literature is that in the

[1]See for example, Cohn (2010), Hatton and Williamson (1998), or Kim (2007) and the references therein for further information on the history of immigration to the US.

[2]See for example, Mauro (1995), Easterly and Levine (1997), Alesina et al. (2003), or Montalvo and Reynal-Querol (2003, 2005a).

cross-section there are many difficult-to-measure omitted variables, such as history and geography, that affect both economic growth and the cultural diversity of the population. Our empirical analysis circumvents this critique by using exclusively the within-county variation of the data. Fixed factors, such as geography or history are therefore differenced out. Second, as Alesina and La Ferrara (2005) point out, the cultural diversity of regions may itself respond over time to changes in the economic environment. We address this important issue by using the supply-push component of immigrant inflows as an instrumental variable. The supply-push component of immigrant inflows is a widely used instrumental variable in the labor economics literature (see e.g. Card and DiNardo, 2000; Card, 2001, 2009; Saiz, 2003; Ottaviano and Peri, 2005, 2006). This instrumental variables estimation strategy has, however, not been used before to examine the causal effects that cultural diversity had on output growth at the US county level during the age of mass migration.

On the theoretical front, the cultural economics literature has suggested several potentially countervailing channels through which cultural diversity affects output growth. Cultural diversity can have a positive effect on output growth if a more diverse working-age population is associated with a greater variety of skills that in turn enable the production of a greater variety of goods and services (e.g. Alesina and La Ferrera, 2005). A more diverse and, in particular, a more polarized population can however also have detrimental effects on output growth if it is associated with increased social conflict and a reduction in the quality and quantity of public good provision. We examine these countervailing effects by constructing measures of cultural fractionalization and cultural polarization. While our measure of cultural fractionalization is strictly increasing in the number of groups, our measure of cultural polarization is maximized when two groups are of equal size. Our polarization measure is, therefore, closely related to Horowitz's (1985) statement that conflicts are more likely in societies where a large ethnic minority faces an ethnic majority. Conflict models such as those in Esteban and Ray (1999, 2011) formalize the non-monotonic relationship between diversity and conflict emphasized by Horowitz (1985). These models predict that social tensions are greatest when there are two equally powerful groups that contest for resources.

Our first main finding is that increases in cultural fractionalization led to significant increases in output per capita during the 1870-1920 period. Focusing on within-county changes in cultural fractionalization to eliminate fixed county-specific characteristics, we find that a 1 percentage point increase in counties' cultural fractionalization increased output per capita by up to 2 percent. To link this result more closely to the cultural economics literature where a key argument for a positive effect of cultural fractionalization on output per capita is an increase in skill variety, we show that within-county increases in cultural fractionalization were associated with significant within-county increases in the occupational diversity of workers.

Our second main finding is that increases in cultural polarization had a significant negative effect on output per capita. Our fixed effects instrumental variables estimates yield that a 1 percentage point increase in cultural polarization decreased output per capita by up to 3 percent. The political economy literature has linked polarization to voracious redistribution (Tornell and Lane, 1999; Lane and Tornell, 1996), a large government sector and distortionary taxation (Azzimonti, 2011), as well as violent conflict (Esteban and Ray, 1999, 2011; Montalvo and Reynal-Querol, 2005a,b). In line with this literature, we document that during the 1870-1920 period increases in polarization led to a significant increase in the tax quote and the number of public sector employees. We also provide some anecdotal evidence for conflict tensions among European immigrants.

The results from our empirical analysis also illustrate the importance of including polarization and fractionalization jointly in the regression model. This point was already made in the context of cross-country regressions by Montalvo and Reynal-Querol (2005a,b) who show that the effects of ethnic polarization on a variety of determinants of economic growth, such as, investment and civil war, differ from the effects of ethnic fractionalization. Our within-county regressions show that, when the effect of fractionalization on economic growth is estimated without controlling for the (negative) effect of polarization on economic growth the obtained coefficient on fractionalization is negative and statistically significant. Hence, estimating the effects of fractionalization on economic growth without controlling for the effects of polarization would lead to the conclusion that an increase in fractionalization is bad for economic development. However, this would be a mistaken conclusion. Once the negative effect of polarization is controlled for in the regression model, the coefficient on fractionalization becomes positive and significant thus in line with the predictions from the theoretical literature that we discussed above.

A further finding of our within-county estimation approach is a significant mean reversion in the cultural fractionalization and polarization index. This finding is important for the empirical literature on the effects of cultural diversity: it implies that using initial cultural diversity indices to circumvent endogeneity problems will lead to coefficients that reflect the opposite of the true causal effect that within-county changes in cultural fractionalization and polarization have on output growth. Our finding of mean reversion in the fractionalization and polarization index is particularly important for studies that focus on periods of significant change in the cultural composition of the population.[3] Statistically, the mean reversion follows from the bounded nature of these indices on the unit interval. By definition, mean reversion implies that counties with initially higher levels of cultural fractionalization and polarization experienced subsequently smaller changes in fractionalization and polarization. Using initial values to

[3]Changes in the cultural composition of the population may occur because of significant immigration inflows, as it was the case during the age of mass migration. Changes in the cultural composition of the population may however also occur during episodes of major intra-state conflict such as, for example, during civil war.

examine how a change in the cultural diversity of the population induces a change in output per capita requires to deal, therefore, correctly with the mean-reverting time-series nature of these indices. We show that this mean reversion of the cultural diversity indices can be accounted for by using an IV approach where the change in fractionalization and polarization is instrumented by the initial level. Via the first-stage, the IV approach takes into account the mean reversion in the cultural fractionalization and polarization index. Importantly, we find that our main results continue to hold when using this alternative instrumental variables approach and controlling for initial conditions.

Methodologically, our instrumental variables estimation strategy that uses the supply-push component of immigrant inflows as an instrument is related to the empirical work of Ottaviano and Peri (2006). Ottaviano and Peri tackle endogeneity problems by using, as we do, the identification strategy from Card (2001) and Saiz (2003). Consistent with our results, Ottaviano and Peri find that increases in the cultural fractionalization of cities has a positive effect on productivity.[4] However, our empirical analysis differs from the work of Ottaviano and Peri in two main aspects. First, Ottaviano and Peri focus on cultural fractionalization only. They do not examine the effects of cultural polarization. Our estimates show that increases in cultural polarization have a significant negative output effect while increases in cultural fractionalization have a significant positive output effect. The empirical findings in our paper therefore highlight the importance of distinguishing between cultural fractionalization and cultural polarization, as suggested by the theoretical literature.

A second key difference between Ottaviano and Peri and our empirical analysis is the time period covered. Ottaviano and Peri focus on the 1970-1990 period while we focus on the period from 1870-1920. There are several reasons why focusing on the 1870-1920 period has advantages. First, in contrast to the 1970-1990 period, the 1870-1920 period was a period of free immigration. Illegal immigration was therefore not an issue due to the very liberal US immigration policy before 1920. Second, immigration during the 1870-1920 period changed significantly the demographic structure of the US population. Between 1870-1920, the annual inflow rate of immigrants peaked in some years around 1.5 percent, a value which was never reached again in the 20th century (see Figure 1). Finally, from a historical perspective, the period of mass migration constituted the first large inflow of immigrants to the US.

The remainder of our paper is organized as follows. Section 2 provides a brief overview and discussion of the age of mass migration. Section 3 describes the construction of our cultural fractionalization and polarization index. Section 4 explains the

[4]In complementary work, Ottaviano and Peri (2005) show that employment density and wages of US-born workers are significantly higher in US cities with a higher linguistic fractionalization. More recently, Sparber (2009, 2010) shows with decennial panel data that racial fractionalization has a significant positive effect on wages in most of the US industries and cities for the 1980-2000 period. Peri (2012) shows for the 1970-2006 period that immigration had a significant positive effect on total factor productivity in US states.

estimation strategy. Section 5 presents our main results. Section 6 discusses further robustness checks. A supplementary appendix contains the estimation results for the robustness checks that we discuss in Section 6 as well as further graphics, simulations and descriptive statistics.[5] The last section concludes.

4.2 The Age of Mass Migration in the US: A Natural Source of Cultural Variation

During the age of mass migration, which is commonly referred to as the period between 1850 to World War I, about 55 million Europeans emigrated to North and South America and Australasia. The US received about three-fifths of the 55 million European emigrants, and thus the bulk of the mass migration stream (Hatton and Williamson, 1998). Emigrants from non-European countries (like China, Japan and Mexico) constituted during this period only a minor part of the overall immigration wave to the US (see e.g. Cohn, 2010).

One important and key aspect of the age of mass migration is that it was a period of free immigration. Despite the Chinese exclusion act in 1882, immigration policy remained liberal for the overwhelming European part of immigrants until the introduction of a literacy test in 1917 (Immigration Act of 1917) and the establishment of immigration quotas in 1921 (Emergency Quota Act of 1921).[6] The 1850-1920 immigration inflow was therefore a natural source of variation in cultural diversity. It stands in contrast to the post-1920 US immigration inflows, which were strongly affected by US immigration policy.

The focus of our empirical analysis is on the period from 1870 to 1920. There are three main reasons for choosing this period. First, starting in 1870 ensures that our analysis excludes the 1861-1865 American Civil War. The 1861-1865 American Civil War was an atypically large, negative shock to the US economy. This event could have affected both the cultural diversity and economic development of US counties in an atypical way. Second, the US Census collected systematic county level data on foreign-borns' country of origin from 1870 onwards. Hence, a second key reason for starting in 1870 is the availability of detailed data on foreign-borns' country of origin. Third, the reason why our empirical analysis ends in 1920 is that in the early 1920s there was a significant change in US immigration policy. As Goldin (1994, p. 223) notes: "With the passage of the Emergency Quota Act in May 1921 the era of open immigration to the United States came to an abrupt end."

[5] The supplementary appendix is also available for download at https://sites.google.com/site/markusbrucknerresearch/research-papers.

[6] See Goldin (1994) for an analysis of the political economy of immigration restrictions in the United States, 1890 to 1921.

Figure 1 shows that during the 1870-1920 period the annual inflow rate of immigrants was about 0.75 percent of the total US population. This is a fairly large inflow rate; in particular, when compared to the post-1920 period where among other factors immigration restrictions significantly reduced the inflow rate of immigrants to about 0.20 percent.

Table 1 presents descriptive statistics of the share of foreign-born in the total US population during the 1870-1920 period. On average, over 9 percent of the US population were foreign-born. The West had the largest share of foreign-born (19 percent), followed by the Midwest (15 percent), the Northeast (15 percent), and the South (2 percent). The descriptive statistics show also substantial variation in the foreign-born share across US counties (the across-county standard deviation of the foreign-born share is 0.11) and a smaller but still significant variation in the foreign-born share within counties (the within-county standard deviation of the foreign-born share is 0.04).

It is also interesting to note that during the age of mass migration the geographic origin of immigrants varied substantially over time. Around 1870, most of the immigrants were from Ireland, Germany, Great Britain, and Scandinavia. These countries remained until the end of the 19th century very important emigration nations. At the end of the 19th century, however, the bulk of immigrants came from Eastern and Southern Europe including countries like Italy, the Austro-Hungarian Empire, Poland, Russia, and Greece (see e.g. Barde et al. 2006, Cohn, 2010, or Hatton and Williamson, 1998). This geographical shift of immigration from Western and Northern Europe to Southern and Eastern Europe resulted in a remarkable variation in the cultural composition of the US county population between 1870 and 1920. We explain in the next section how we calculated indices of cultural fractionalization and cultural polarization to capture these substantial changes in the cultural diversity of US counties.

4.3 Two Different Concepts of Cultural Diversity: Fractionalization and Polarization

We use the country of origin defined by the US Census to measure different cultural groups and to construct our indices of cultural diversity. In contrast to ethno-linguistic or religious diversity indices that distinguish among different races, linguistic, or religious characteristics of individuals, our indices are based on the country of origin of individuals. This coding of cultural groups has the key advantage that our indices are based on well-defined units.

We construct an index of cultural fractionalization as

$$FRAC_{cs,t} = 1 - \sum_{i=1}^{N} \pi_{i,cs,t}^2,$$

where $\pi_{i,cs,t}$ is the county population share of group i living in county c of state s in year t. The N groups are: foreign-born from the Austro-Hungarian Empire (Austria, Bohemia and Hungary), the Benelux (Holland, Luxembourg, Belgium), Canada, Central and South America, Scandinavia (Denmark, Norway, Sweden and Finland), Eastern Europe, France, Germany, Greece, Ireland, Italy, Poland, Portugal, Spain, Switzerland, the United Kingdom (England, Scotland and Wales), Asia, the Pacific and we distinguish among US-born whites and US African-Americans. Conceptually, the fractionalization index captures the probability that two randomly selected individuals are from different cultural groups. This index increases monotonically in the number of groups.

We construct an index of cultural polarization as

$$POL_{cs,t} \quad = \quad 1 - \sum_{i=1}^{N} \left(\frac{1/2 \; - \; \pi_{i,cs,t}}{1/2} \right)^2 \pi_{i,cs,t}.$$

Our polarization index follows Reynal-Querol (2002) and Montalvo and Reynal-Querol (2005b). This polarization index captures how far the distribution of the cultural groups is from the bipolar distribution.[7] The index is maximized when there are two groups which are of equal size. Note that the polarization index that we use is based on the binary criteria of "belonging" or "not belonging" to a particular cultural group. Using such a discrete metric (belonging/not belonging) implies that the distance across ethnic groups is the same. Hence, what matters for our index of polarization is only the size of each group and not the distance between groups. In Section 5.5 we also discuss results for an alternative index that attempts to capture distances between groups.[8]

Figures 2A and 2B show scatter plots of the polarization and fractionalization index; Figure 2A for the 1870 period and Figure 2B for the 1920 period. Both scatter plots show an inverted U-shaped relationship. For low values of fractionalization the relationship is positive, for intermediate values the relationship is zero, and for high values of fractionalization the relationship is negative. Hence, the correlation between the polarization and fractionalization index is low when the cultural heterogeneity is high. The U-shaped relationship between polarization and fractionalization that emerges in the cross-section of US counties during the 1870-1920 period is in line with Montalvo and Reynal-Querol (2005b) who document a U-shaped relationship between the level of polarization and fractionalization in the cross-section of countries.

In Figure 3 we plot the 1870-1920 changes in polarization of US counties against the 1870-1920 changes in fractionalization. The top left-hand-side panel of Figure 3 shows that for the majority of observations within-county increases in fractionalization

[7]For further details on the theoretical properties of a discrete polarization measure see Montalvo and Reynal-Querol (2008).

[8]See the Appendix for a detailed description of the data used to construct the fractionalization and polarization index.

are associated with within-county increases in polarization. The top right-hand-side panel of Figure 3 also shows that within-county decreases in fractionalization tend to be associated with within-county decreases in polarization during the 1870-1920 period. The correlation coefficient in the former case is 0.89 and 0.83 in the later. The fact that changes in fractionalization and polarization are highly positively correlated has important implications, to be discussed in detail in Section 4, for estimating causal effects of changes in fractionalization and polarization on economic growth.

Importantly, it is not a necessity that within-county changes in fractionalization move in the same direction as within-county changes in polarization. This is documented in the bottom two panels of Figure 3. Both panels show that there are observations in the sample where changes in polarization and fractionalization move in opposite directions. These observations comprise about five percent of the sample; thus are fairly rare. Nevertheless, these observations do make it clear that a within-county decrease in polarization that is coupled with a within-county increase in fractionalization (and vice versa) is not only a theoretical possibility but also a scenario that is reflected in the data.

4.4 Baseline Estimation Strategy

The commonly used econometric model in the empirical growth literature relates the change in the log of output per capita, $lnY - lnY_0$, between the current and the initial period to the level of a set of explanatory variables, X, as well as initial output per capita (see e.g. Durlauf et al., 2005): i.e $lnY - lnY_0 = \phi lnY_0 + \Gamma X + e$, where $-\phi$ captures the convergence rate; equivalently, $lnY = (1 + \phi)lnY_0 + \Gamma X + e$. In this model changes in the explanatory variables have a long-run level effect and a short-run growth effect. Our baseline econometric model follows this model specification with the key difference that we estimate it in first differences. Specifically, our first-difference estimating equation relates the 1870-1920 change in the log of output per capita ΔlnY_{cs} of US county c in state s to the 1870-1920 change in fractionalization $\Delta FRAC_{cs}$ and polarization ΔPOL_{cs}:

$$\Delta lnY_{cs} = \alpha_s + \beta\Delta FRAC_{cs} + \gamma\Delta POL_{cs} + \Delta u_{cs}. \tag{4.1}$$

As a benchmark we estimate equation (1) by least squares. The state fixed effects α_s capture factors that vary over time at the state-level such as, for example, changes in economic and social policies implemented by the local governments of US states. These are, therefore, factors that affect both changes in output per capita and changes in cultural diversity across counties in a US state in a similar way. Because we estimate equation (1) in first differences, time-invariant variables such as geographic characteristics of counties that affect both the level of output per capita and cultural diversity are differenced out.[9] Stated in another way, equation (1) has an analogous level-form representation that includes county fixed effects. Because these county fixed effects are time-invariant they drop out in equation (1) (since that equation uses the change in variables between 1870 and 1920). We cluster the error term, Δu_{cs}, at the state level to ensure that the computed standard errors of our estimates are robust to arbitrary spatial correlation across counties in each US state.

It is important to note that we include the fractionalization and polarization index jointly in the regression model. This has the advantage that we identify independent effects of changes in polarization and fractionalization on economic growth. Greene (2003, p. 9), for example, writes that: "one of the most useful aspects of the multiple regression model is its ability to identify the independent effects of a set of variables on the dependent variable". As discussed in Section 3, within-county changes in fractionalization and polarization are strongly positively correlated. Including fractionalization or polarization individually would thus imply that the estimates do not capture independent effects and suffer from an omitted variables bias.[10]

On theoretical grounds, we cannot rule out that changes in US counties' cultural diversity are endogenous to changes in output per capita. For example, US counties that experienced a large increase in output per capita may appear particularly attractive to immigrants, causing an inflow of immigrants. It is also possible that due to demand effects counties which grew a lot demanded a more diverse workforce. In this case, least-squares estimation of equation (1) produces biased estimates.

To address the concern that the least-squares estimates are biased due to a causal effect of economic growth on changes in US counties' cultural diversity we use instrumental variables techniques. Our first instrumental variables approach follows the labor economics literature and uses the so-called supply-push inflow of immigrants as an instrumental variable. For the 1870-1920 period, we compute the supply-push inflow of immigrants from European country i to US county c as

$$SP_{i,cs} = \theta_i^{7020} \pi_{i,cs}^{1870},$$

where $\pi_{i,cs}^{1870}$ is the share of foreign-born from European country i living in year 1870 in US county c in state s; $\theta_i^{7020} = \left(\frac{\pi_i^{1920} - \pi_i^{1870}}{\pi_i^{1870}}\right)$ denotes the US-region growth rate (North, Midwest, South and West) of the foreign-born share from European country i

[9]For a paper that demonstrates the endogeneity of ethnolinguistic diversity to cross-country differences in geography, see Michalopoulos (2011).

[10]To see this formally, it is useful to consider the simplest possible model $Y = \beta Frac + \gamma Pol + e$, where we have dropped the Δ for simplicity. If one estimates $Y = \beta Frac + e'$ then the least squares estimate is $\beta^{LS} = cov(Y, Frac)/Var(Frac) = cov(\beta Frac + \gamma Pol + e, Frac)/Var(Frac) = \beta + \gamma\rho Var(Pol)/Var(Frac)$, where ρ is the correlation coefficient between fractionalization and polarization (we also assume in the calculation that $cov(e, Frac) = 0$). Clearly the bias is increasing in ρ; and its sign and size also depends on γ, the effect that polarization has on economic growth.

between 1870-1920. The identification strategy therefore exploits that newly arriving immigrants tend to settle where previous immigrants of the same nationality live (see e.g. Bartel, 1989, Munshi, 2003 or Boustan, 2010). Because we use the initial, that is, the 1870 distribution of immigrants across US counties to predict the inflow of immigrants during the 1870-1920 period the supply-push variable is exogenous to demand and supply shocks that occur during this period in US counties.[11]

The predicted foreign-born share, $\widehat{\pi}_{i,cs}^{1920}$, is calculated as

$$\widehat{\pi}_{i,cs}^{1920} = (1 + \theta_i^{7020})\pi_{i,cs}^{1870}.$$

With these shares in hand, we construct an imputed fractionalization and polarization index – using exactly the same formulas as described in Section 3. We then use these predicted fractionalization and polarization indices as instrumental variables for the actual indices of fractionalization and polarization.

We note that even though the supply-push based instruments are well suited to address reverse causality bias, there is still the issue of omitted variables bias. If, for example, particular population groups are more inclined to select themselves into regions that are more prosperous, and this prosperity persists, then omitting initial (1870) cross-county income differences will lead to biased estimates. We address this issue by also showing estimation results from an econometric model that includes a rich set of county-specific initial control variables. The set of county-specific initial control variables comprises the initial (1870) level of output per capita, the urbanization rate, the Gini coefficient of land concentration, the manufacturing share, the population size, the labor participation rate, an indicator for counties' rail access, the share of US-born white and the share of African-Americans**.** By controlling for counties' initial level of output per capita, we clean the error term of serial correlation. The control for other initial county characteristics serves the purpose to further reduce the variance of the error term.

Following the note by Vigdor (2002) on the importance of including the population shares of the different groups when examining the effects of fragmentation, we also control for the population shares that we use to construct the polarization and fractionalization index. Controlling for these population shares should make it clear that our main question of interest is not about the effects that specific immigrant groups had on economic growth during the age of mass migration. Rather the focus of our paper is on the effects that cultural diversity as measured by indices of fractionalization and polarization had on output growth. For a paper that focuses on the question of the returns to migration during the age of mass migration see, for example, Abramitzky et al. (2012).

[11] We use the same method to calculate the predicted population shares of all remaining (non-European) nationalities.

4.5 Main Results

4.5.1 Economic Growth

Table 2, Panel A, presents our baseline estimates of the average effect that within-county changes in cultural fractionalization and polarization have on output growth. Column (1) reports the state-fixed effects estimates for regressing the 1870-1920 change in the log of output per capita on the 1870-1920 change in the polarization and fractionalization index. The result is a negative coefficient on the polarization index that is statistically significant at the 5 percent significance level; the coefficient on fractionalization is positive but insignificant.

In column (2) of Table 2 we add to the regression additional controls for cross-county differences in initial conditions, such as, initial output per capita, the urbanization rate, the Gini coefficient of farm size distribution, the manufacturing share, the population size, the labor participation rate, an indicator for rail access, the share of US-born white and the share of African-Americans. Controlling for these variables improves the explanatory power of the econometric model: the adjusted R-squared increases from 0.26 in column (1) to 0.55 in column (2). The significance of the estimated effects of fractionalization and polarization on output also increases. Now, the negative coefficient on polarization is significant at the 1 percent significance level, while the positive coefficient on fractionalization is significant at the 5 percent significance level.

In column (3) of Table 2, following Vigdor (2002), we add to the regression the population shares of the different groups.[12] We also add the population growth rate during the 1870-1920 period to capture additional variation in output per capita growth. Adding these population variables to the right-hand side of the regression decreases somewhat the absolute magnitude of the estimated coefficient on the fractionalization index. Nevertheless, the negative coefficient on the polarization index is still significantly different from zero at the 1 percent level; the positive coefficient on the fractionalization index is statistically significant at the 5 percent level.[13]

We illustrate graphically the relationship identified by the regression analysis in Figure 4. In Figure 4A we show a scatter plot of the relationship between the 1870-1920 (residual) changes in the log of output per capita and polarization. The residuals are obtained from regressing the 1870-1920 changes in the log of output per capita and polarization on the 1870-1920 change in fractionalization as well as the same set of control variables as in column (3) of Table 2. In Figure 4B we show a scatter plot

[12] The state fixed effects and the coefficients on the population shares are jointly significant at the 1 percent level. We provide a list of the means of the population shares in Appendix Table 20 of the supplementary appendix. We also provide in Appendix Tables 1 to 3 of the supplementary appendix the full set of estimates that correspond to the regression results reported in Table 2.

[13] Our main findings also remain unaffected when we control for the share of American Indians. We report the estimates in Appendix Table 12 in the supplementary appendix.

of the relationship between the 1870-1920 (residual) changes in the log of output per capita and fractionalization. The residuals are obtained from regressing the 1870-1920 changes in the log of output per capita and fractionalization on the 1870-1920 change in polarization as well as the same set of control variables as listed in column (3) of Table 2. As Figure 4 clearly shows there is a positive relationship between changes in fractionalization and output, but a negative relationship between changes in output and polarization.[14]

To correct for a possible endogeneity bias of our least-squares estimates, we report in column (4) of Table 2 two-stage least squares estimates that use the supply-push component of immigrant inflows as an instrumental variable. The main result is that there continues to be a negative output effect of cultural polarization and a positive effect of cultural fractionalization when using the supply-push instrumental variables strategy. Although standard errors roughly double in column (4), the coefficients on polarization and fractionalization continue to be significant at the conventional confidence levels. Quantitatively, the IV estimates are – in absolute value – larger than the least squares estimates. This is particularly true for the polarization index, where the IV estimate is more than twice the size of the least squares estimate. Taken at face value, the IV estimates in column (4) imply that a 1 percentage point increase in cultural polarization significantly reduced output per capita by around 2.7 percent on average; a 1 percentage point increase in cultural fractionalization significantly increased output per capita by around 2.0 percent on average.

For comparison, we report in Panels B and C of Table 2 regression results for including the polarization and fractionalization index individually on the right-hand side of the estimating equation. The main finding is that, in this case, the size of the coefficients on the polarization and fractionalization index is – in absolute value – smaller than in Panel A. Moreover, not including polarization on the right-hand side would (falsely) suggest that fractionalization has a significant negative output effect. The reason for these results is that in Panel C of Table 2, which reports estimates for the fractionalization index, the polarization index is part of the error term. Analogously, in Panel B of Table 2, which reports estimates for the polarization index, the fractionalization index is part of the error term. Because the fractionalization and polarization index are positively correlated, there is a positive omitted variables bias in Panel B, and a negative omitted variables bias in Panel C. An important message of Table 2 is, therefore, that it is important to include both the fractionalization and the polarization measure jointly on the right-hand side of the estimating equation.

In terms of instrument quality, the supply-push instrumental variables estimation strategy yields a reasonable first-stage fit. In column (4) of Panel A, the joint Kleibergen-

[14]We note that the slope coefficients on the residual changes in polarization and fractionalization, plotted in Figure 4, are exactly the same as in column (3) of Table 2 (this follows from the Frisch-Waugh-Lovell Theorem).

Paap F-statistic is 27.97. According to the tabulations in Stock and Yogo (2005), for an exactly identified IV model with two endogenous regressors, this F-statistic implies that we can reject the hypothesis that the IV size distortion is larger than 5 percent at the 5 percent significance level.[15] We can also reject the hypothesis that the relative IV bias is larger than 5 percent at the 5 percent significance level.

Columns (1) and (2) of Table 3 document that, not only does this IV strategy produce a highly significant first-stage fit; the first-stage coefficients on the relationship between the excluded instruments and endogenous regressors are also economically sensible. In particular, column (1) shows that the supply-push based polarization index has a positive effect on the actual polarization index. The coefficient is above 0.4 and statistically significant at the 1 percent significance level. On the other hand, the coefficient on the imputed supply-push based fractionalization index is insignificant and quantitatively small. Column (2) shows that the opposite holds regarding the supply-push based fractionalization index. Hence, the two supply-push based instruments have opposite effects on the two endogenous regressors.

Column (3) of Table 3 shows the reduced-form effects. The imputed supply-push based polarization index has a negative effect on output while the fractionalization index has a positive output effect. Both of these reduced-form effects are significant at the 1 percent significance level. Taking into account the first-stage estimates in columns (1) and (2), the reduced-form estimates resonate the second-stage estimates reported in column (4) of Panel A in Table 2.

The assumption in the instrumental variables estimation is that our instruments are uncorrelated with the error term in the second stage. That is, the supply-push based fractionalization and polarization index should be exogenous to output growth. Furthermore, these indices should only affect output growth through actual fractionalization or polarization. Because the supply-push indices use information that is based on the county-specific, initial (1870) population shares, they are unlikely to be affected by demand-pull factors (such as, for example, county-specific productivity shocks) that over the 1870-1920 period attracted certain groups of immigrants.[16] We would also like to restate here that we are controlling in our regressions for initial cross-county differences in output per capita as well as other county characteristics that may have made it particularly attractive for immigrant groups to select into certain counties, for reasons other than network effects, at the beginning of the sample period.

A standard way to empirically test instrument validity is to search for an additional instrument, such that, the IV regression is overidentified. This allows to compute the Hansen J test on the joint hypothesis that the instruments are uncorrelated with the second-stage error term. In columns (1) and (2) of Table 4 we therefore report second-

[15]The Stock and Yogo tabulations for weak instruments are based on iid errors. No tabulations exist for non-spherical errors. Nevertheless, the Stock and Yogo tabulations are commonly used in the applied IV literature to check for weak instrument bias, even when standard errors are Huber robust and clustered.

[16]See also Card (2001) for further discussion on this point.

stage estimates that use, in addition to the supply-push based instruments, two additional instruments: the initial fractionalization index and the initial polarization index. After controlling for a rich set of initial control variables, such as, initial output per capita and nationality shares – which might affect contemporaneous fractionalization and polarization – the initial values of the polarization and fractionalization index should be uncorrelated with the second-stage error term.

The joint first-stage F-statistic of the four excluded instruments in column (1) of Table 4 is around 17. This F-statistic implies that according to the tabulations in Stock and Yogo (2005) we can reject the hypothesis that the IV size distortion is larger than 5 percent at the 5 percent significance level. We can also reject the hypothesis that the relative IV bias is larger than 5 percent at the 5 percent significance level. The Hansen J test on the overidentifying restriction produces a p-value of 0.32. Hence, the Hansen J test does not reject the joint hypothesis that the instrumental variables are uncorrelated with the second-stage error term.

The estimated second-stage coefficients of this overidentified two-stage least squares estimation are -2.2 and 1.2, for polarization and fractionalization respectively. The estimated coefficient on the polarization index is significant at the 1 percent significance level. The estimated coefficient on the fractionalization index is not significant at the conventional significance levels but this is due to a standard error that is twice the size of the standard error on the polarization index.

In column (2) of Table 4 we show estimates that are obtained when using instead of the 2SLS estimator the GMM IV estimator. In overidentified IV estimation, the GMM estimator is more efficient (Greene, 2003). When using the GMM estimator we also obtain a positive coefficient on the fractionalization index. But, with the GMM estimator this coefficient is significant at the 5 percent significance level.

For comparison, we report in column (3) of Table 4 second-stage estimates that are based on an exactly identified IV estimation. In this IV estimation the excluded instruments are the initial fractionalization and the initial polarization index. As in all our other regressions the set of control variables includes state fixed effects as well as initial output per capita and other initial control variables. This IV estimation yields similar to columns (1) and (2) a significant negative coefficient on polarization. For fractionalization the estimated coefficient is positive but insignificant.

Table 5, columns (1) and (2), report the corresponding first-stage estimates for the second-stage estimates that we reported in column (3) of Table 4. Beyond showing that initial fractionalization and polarization are strongly correlated with the endogenous regressors, these first-stage estimates demonstrate the significant mean-reversion of the fractionalization and polarization index. The estimated coefficient on the initial polarization index is negative and statistically significant at the 1 percent significance level. Hence, counties that had relatively high levels of polarization in 1870 experienced smaller changes over the 1870-1920 period in polarization than counties that started out with relatively low levels of polarization. Similar results hold regarding the fractionalization index. We note that, statistically, this mean-reverting nature of the polarization and fractionalization index follows from the boundedness of these indices on the unit interval.

An important implication of the significant mean reversion in the polarization and fractionalization index is that the reduced-form effects of initial polarization and initial fractionalization on economic growth have the opposite sign than the two-stage least squares estimates where the changes in polarization and fractionalization are instrumented by their initial values. We show this in column (3) of Panel Table 5. There, the growth rate of output per capita is regressed on the initial values of polarization and fractionalization. The main finding is that the reduced-form coefficients have exactly the opposite sign than what the two-stage least squares estimates revealed in column (3) of Table 4. The reason for this discrepancy is that the two-stage least squares estimates, via the first stage, fully take into account the mean reversion of the polarization and fractionalization index. The reduced-form regression, on the other hand, misses this information. For empirical research, we thus note that using initial measures of fractionalization and polarization in a growth regression can lead to erroneous results.

4.5.2 Intermediate Channels

Skill Variety

During the 1870-1920 period, the occupational distribution reveals a striking pattern of occupational clustering by nationalities (Hutchinson, 1956). As Kim (2007, pp. 17-18) points out: "The Germans specialized in many food related industries as brewers, distillers, butchers and confectioners ... The Irish were highly specialized in gas works and other heavy industries ... The English and Welsh were concentrated in textiles, iron and steel ... The Scandinavians were highly specialized in a few occupations: sail and awning makers and those related to lumber industry."

Inspired by this anecdotal evidence we examine in Table 6, using the estimation strategy discussed in Section 4, how cultural fractionalization affected the diversity of skills of the working-age population in US counties. To measure the diversity of skills of the working-age population we constructed a fractionalization index of workers' occupations as $OCC_{cs} = 1 - \sum_{o=1}^{O} O_{ocs}^2$, where O_{ocs} is the fraction of workers living in US county c in state s that work in occupation o.[17] This fractionalization index of workers' occupations captures the probability that two randomly selected workers have a different occupation. Columns (1)-(4) of Table 6 show that, once instrumental variables techniques are used there is a positive effect of cultural fractionalization on the fractionalization of workers' occupations. And, for two out of three IV specifications,

[17]Table 21 in the supplementary appendix lists the means of the shares of the different occupations.

the estimated coefficient on the fractionalization index is at least significant at the 5 percent significance level.

To reinforce the above result we constructed an industry diversity index. The industry diversity index captures the probability that two randomly selected individuals work in a different industry. Analog to the fractionalization index of workers' occupations we compute the industry diversity index as $IND_{cs} = 1 - \sum_{m=1}^{M} I_{mcs}^2$, where I_{mcs} is the fraction of workers living in US county c in state s that work in industry m.[18] The instrumental variables estimates in columns (6)-(8) show that changes in cultural fractionalization are positively associated with changes in the industry diversity index. These findings are consistent with the argument in the cultural economics literature (e.g. Alesina and LaFerrera, 2005) that cultural fractionalization has a positive effect on output per capita because it increases the variety of skills.

Conflict Tensions, Government Size, and Taxation

The conflict literature has argued that increases in cultural polarization are associated with increases in conflict potential. Anecdotal evidence suggests that, indeed, conflict tensions among European immigrants were not rare in the US during the 19th century. Antagonism between Europeans due to the rise of nationalism in 19th century Europe was, at least in part, imported to the US by the large inflow of European immigrants. The following examples illustrate some of the cultural conflicts among European immigrants in the US during the 1870-1920 period: "In 1868 the *New York Times* reported a riot between German and Irish immigrants that ended up with thirty men wounded and sixty arrested by the police at Ward's Island, New York. The dispute between Irish and Germans started, when"the contestants used vile epithets toward each other's nationality".[19] Another riot took place in Scranton, Pennsylvania, 1871 among Welsh, Irish and German coal miners. Violence occurred among the strike members when German and Irish miners were attacked by Welsh strikers.[20] In Pittsburgh, Pennsylvania, 1886 a conflict between Irish and Italian laborers – residing in the same neighborhood – arose, "in which two of the participants received fatal injuries".[21] A severe fight among Swedish, Polish and Hungarian immigrants in Denver, Colorado, 1887 resulted in one man being shot and several others seriously wounded.[22] In 1915, a clash between Italians and Austrians occurred at the Federal Pressed Steel Company in Milwaukee, Wisconsin. Seven men were hurt after the Austrian steel workers left because the workers discovered that they were hired by the company to keep up with orders for shrapnel which were sold to

[18] Table 22 in the supplementary appendix lists the means of the shares of the different industries.
[19] *The New York Times*, published: March 6, 1868 as *"Riot on Ward's Island"*.
[20] *The New York Times*, published: May 11, 1871 as *"The Coal Riot"*.
[21] *The New York Times*, published: September 20, 1886 as *"Fatal War Among Races"*.
[22] *The New York Times*, published: April 12, 1887 as *"A Race Riot in Denver"*.

Russia.[23]

The political economy literature also suggests that polarization may lead to excessively large government size and distortionary taxation. For example, Azzimonti (2011) builds a model where more polarization induces a large government sector and a higher level of distortionary taxation. Voracity models such as those in Tornell and Lane (1999) and Lane and Tornell (1996) also predict that fiscal redistribution is maximized when there are two groups contesting for redistribution from the government budget.

Consistent with these political economy models, we document in Table 7 that increases in polarization led to a significant increase in the tax ratio and the share of public sector officials. Columns (1)-(4) of Table 7 show that increases in polarization led to a significant increase in the tax ratio: a one percentage point increase in polarization increased the tax ratio by about 2 percent. Columns (5)-(8) show that there is also evidence for increases in polarization leading to a significant increase in the share of public sector officials. On the other hand, fractionalization is associated with a significantly smaller tax quote and share of public sector officials.

4.5.3 Urbanization and Population Growth

In the economic history literature the urbanization rate is sometimes used as a proxy for economic progress and development. In our sample the unconditional correlation between output per capita growth and the within-county change in the urbanization rate is 0.18. Table 8 shows that, in line with this rather low correlation, there are no significant effects of within-county changes in polarization on within-county changes in the urbanization rate. Similarly there are also no significant effects of within-county changes in fractionalization on within-county changes in the urbanization rate for the majority of specifications.

We obtain stronger results if, instead of the change in counties' urbanization rates, we focus on counties' population growth. Both, least squares and instrumental variables estimation yield a negative coefficient on polarization and a positive coefficient on fractionalization. The estimated coefficient on polarization is significant in the least squares regression and in two out of three instrumental variables regressions. The estimated coefficient on fractionalization is only significant in the least squares regression. Quantitatively the estimates in columns (5)-(8) of Table 9 imply that, roughly, a one percentage point increase in counties' polarization led to a one percent decrease in their population size. One possible interpretation of this result is that counties, which experienced over the 1870-1920 period low output per capita growth due to increases in polarization were less attractive to live in and thus experienced declines in their population size.[24]

[23] *The New York Times*, published: August 16, 1915 as *"Seven Hurt in Race Riot"*.

[24] See, for example, Glaeser, Scheinkman, and Shleifer (1995) who also argue that in the US a high population growth of regions and cities reflects that these are areas which are more attractive to live in.

4.5.4 Group Aggregation

All empirical studies of the effects of cultural diversity have to address the issue at which level to aggregate cultural groups (see e.g. Desmet et al., 2009). In contrast to cross-country studies that use measures of ethnic diversity, our analysis of the effects of cultural diversity is based on well-defined groups: immigrants' country of origin. By using immigrants' country of origin to construct the cultural diversity measures, our empirical analysis codes cultural groups at a fairly disaggregated level.

To check on the importance of disaggregation of the immigrant groups, we report in columns (1)-(4) of Table 9 estimates for a polarization and fractionalization index that does not distinguish immigrants by country of origin – i.e. all immigrants are pooled into one group. The main result is that the size of the estimated coefficients is in absolute value much smaller than the size of the coefficients that we obtained in the other tables where we distinguished immigrants by country of origin. Also, statistically, the estimates are insignificant. This should not be surprising if an expectedly large share of the variation in skill variety and conflict tensions comes from differences between immigrants as, indeed, the anecdotal evidence suggests. When we pool immigrants into one group these differences are washed out. Hence, in that case, the effects of fractionalization and polarization are quantitatively and statistically weaker.

An alternative strategy is to distinguish immigrants by geographic region of origin. Climatic differences between Mediterranean Southern Europe and Central Europe suggest that immigrants from these different regions differed in their trades and skills due to differences in comparative advantage which led to specialization. Differences in climatic conditions possibly also implied differences in habits and norms between the different geographic regions. Of course, geographic regions are less well-defined units than countries. This is precisely the reason why in our baseline analysis we group immigrants by country of origin. Still, it is interesting to note that when we calculate indices of fractionalization and polarization that use the following regional grouping – Northern, Eastern, Central, Mediterranean Southern Europe, Asia, Central and South America, and the Pacific – we obtain negative output effects of polarization and positive output effects of fractionalization. This is documented in columns (5)-(8) of Table 9. Both, least squares and instrumental variables estimation produce a negative coefficient on polarization that is significant at the 1 percent significance level. For fractionalization the estimates are significant at the 5 percent significance level at least. Quantitatively, the coefficients on the polarization and fractionalization index are similar in size to our baseline estimates.

4.5.5 Group Distances

In this section we address the issue that our polarization and fractionalization indices are based on the binary criteria of "belonging" or "not belonging" to a particular cultural group. Using such a discrete metric (belonging/not belonging) implies that the distance across cultural groups is the same. Hence, it is the size of each group that matters for the constructed fractionalization and polarization indices.

Concerning the polarization index, our discrete index of cultural polarization is related to Esteban and Ray's (1994) class of polarization measures for variables that have a continuous dimension. For empirical purposes the measurability of inter-group distances on the real line is not trivial and this complicates the empirical application of Esteban and Ray's (1994) polarization measure. Still, in theory, distances between ethnic groups are a very important factor for the incidence of conflict. This is also made clear in Caselli and Coleman's (2013) paper on the theory of ethnic conflict.

Fearon (2003), and more recently Desmet et al. (2009) and Esteban et al. (2012), try to tackle the empirical measurement of cultural distances by proxying inter-group distances with linguistic distances. In order to see what we would obtain if we were to follow their approach, we construct indices of fractionalization and polarization that are based on linguistic distances. In particular, we construct an inter-group distance based fractionalization index as

$$FRAC_{LD,cs,t} = \sum_{i=1}^{N} \sum_{j=1}^{N} \pi_{i,cs,t} \pi_{j,cs,t} d_{ij},$$

and an inter-group distance based polarization index as

$$POL_{LD,cs,t} = \sum_{i=1}^{N} \sum_{j=1}^{N} \pi_{i,cs,t}^2 \pi_{j,cs,t} d_{ij}.$$

Note that these indices include a measure of inter-group distances, d_{ij}.

To proxy for these inter-group distances, d_{ij}, we follow Fearon (2003), Desmet et al. (2009) and Esteban et al. (2012) and use information on linguistic groups compiled by the Ethnologue project to construct a measure of linguistic distances between any two groups as $d_{ij} = 1 - b_{ij}^{\delta}$.[25] The parameter b_{ij} is the ratio of the number of shared branches between i and j to the maximum number of branches between any two languages; $\delta \in (0, 1]$ represents a sensitivity parameter determining how fast the distance declines as the number of shared branches increases (see Desmet et al., 2009).[26] The abbreviation LD denotes the use of language distances for the metric d_{ij}. We use the representative language of each country of origin to construct the linguistic distances. Thus, if two

[25]See http://www.ethnologue.com, Desmet et al. (2009) and Esteban et al. (2012) for further information on the Ethnologue project.

[26]We used the language trees reported by Ethnologue to construct the parameter b_{ij}. See also Desmet et al. (2012) for a detailed discussion on the construction of such a language tree.

groups speak the same representative language we set $b_{ij} = 1$. Following Fearon (2003) we compute the linguistic distances d_{ij} by setting $\delta = 0.5$.[27]

Columns (1)-(4) of Table 10 show that, with the distance-based diversity indices, polarization has a significant negative effect on output and fractionalization has a significant positive effect. Hence, using distance-based diversity indices does not change our main finding that cultural polarization has a negative output effect while cultural fractionalization has a positive effect. We also note that quantitatively the estimated coefficients are quite a bit larger for the distance-based diversity indices than the coefficients which we obtained for our baseline analysis that used a binary criteria for constructing the cultural diversity indices.

4.5.6 Alternative Instruments

In this section we report instrumental variables estimates based on two alternative instruments: the change in a war-years supply-push polarization index and the change in the number of immigration groups. During the 1870-1920 period European countries were engaged in several wars. Due to differences in the number of wars and their duration there are substantial differences in the number of war-years between European countries during the 1870-1920 period. We had argued that the polarization index is most suitable for capturing conflict tensions of cultural diversity. Hence, in order to generate additional variation in the polarization index that are of plausibly exogenous nature it might be useful to exploit differences in the number of war-years between European countries during the 1870-1920 period.[28]

Specifically, we construct the war-years supply-push polarization instrument as

$$POL_{WAR} = \sum_{i=1}^{N} \sum_{j=1}^{N} \hat{\pi}_i^2 \hat{\pi}_j w_{ij}$$

where $w_{ij} = (b_{ij}/T)^{\delta}$. The parameter b_{ij} stands for the years of war between European country i and j; and $\delta \in (0, 1]$ is a sensitivity parameter which we set to 0.5.[29] The parameter $\hat{\pi}_{i(j)}$ stands for the predicted foreign-born shares as discussed in Section 4. For the construction of the war-years supply-push polarization index in 1870, we use

[27]As a robustness check we have constructed the fractionalization and polarization index using a linguistic distance parameter $\delta = 0.05$, as in Desmet et al. (2009). This yielded qualitatively similar results, which we have reported in Appendix Table 19 of the supplementary appendix.

[28]We are grateful to an anonymous referee for suggesting wars in European countries as a natural experiment.

[29]Our main data source for years of war is the Correlates of War Project. This database provides information on interstate wars from 1816 until 2012. See http://www.correlatesofwar.org or the ICPSR 24386 datafile for more information. We obtain data on interstate wars before 1816 from Wikipedia, see http://en.wikipedia.org/wiki/List_of_wars.

for b_{ij} the number of war-years that occurred during the past 100 years (1770-1870) and set $T = 100$. For the construction of the index in 1920, we use for b_{ij} the number of war years during the past 150 years (1770-1920) and set $T = 150$. Hence, the important point to note is that w_{ij} varies over time.

The idea behind the war-years supply-push polarization index is that cultural polarization leads to particularly severe conflict tensions in US counties if immigrant groups are from countries that historically were often engaged in war. Since the number of war-years between European countries is unlikely to be a function of events in US counties, using wars between European countries in the construction of the supply-push instrument generates additional variation that is plausibly exogenous.

Column (1) of Table 11 shows that when we use the war-years supply-push polarization instrument that the coefficient on the change in the polarization index is negative and significant at the 1 percent level. In column (2) we add the change in the fractionalization index to the right-hand side of the regression. The main finding is that the coefficient on the polarization index continues to be negative and significant at the 1 percent level; and the coefficient on the fractionalization index is positive and significantly different from zero at the conventional significance levels. Next, in column (3), we show that these results also hold when we instrument the change in the fractionalization index with the supply-push fractionalization index that we used in our previous instrumental variables analysis.

An alternative instrumental variable for changes in US counties' cultural fractionalization is the change in the number of immigrant groups. The benefit of this instrument is that it eliminates biases which arise from the potential endogeneity of changes in the population shares of existing immigrant groups in US counties. We note that variations in this instrumental variable only arise if there are a greater or smaller number of immigrant groups relative to the initial period. In column (4) of Table 11 we show that with this alternative instrumental variable we obtain similar results: polarization has a significant negative effect while fractionalization has a significant positive effect. Importantly, the Kleibergen Paap first-stage F-statistic is in excess of 20 in all of these alternative instrumental variables regressions. According to the critical values provided in Stock and Yogo (2005) we can therefore reject the hypothesis that the relative IV bias is larger than 5 percent at the 5 percent significance level. Furthermore, the p-value of the Hansen J test is above 0.1. Hence, the Hansen J test does not reject the joint hypothesis that the instrumental variables are uncorrelated with the second-stage error term. And this continues to be the case in column (5) where we add to the instrumental variables set the change in the supply-push fractionalization index.

4.6 Further Robustness Checks

We have carried out a number of further robustness checks. For economy of space purposes, these results are reported in the supplementary appendix.

We show in Appendix Table 4 that our findings are robust to using the changes in the logs of fractionalization and polarization. The estimated coefficient on the change in the log of polarization is significantly negative while the coefficient on the change in the log of fractionalization is significantly positive. We note that the reason why in our baseline regressions we do not log the indices is that both indices are on the unit interval. Hence, changes in these variables already capture percentage changes.[30]

We show in Appendix Tables 5-11 that our results are robust to controlling for past output growth. In Appendix Table 5 we report cross-section estimates where the dependent variable is the change in the log of output per capita over the 1870-1920 period as in our baseline regressions. For the pre-1870 period there are data available for output per capita only in 1850 and 1860. Hence, columns (1) and (2) of Appendix Table 5 show estimation results from regressions that control for past output per capita growth during the 1860-1870 and the 1850-1870 period, respectively. When we control for past output per capita growth during the 1860-1870 period the coefficient on polarization is negative and significant at the 1 percent level; the coefficient on fractionalization is positive and significant at the 5 percent level. When we control for past output per capita growth during the 1850-1870 period the number of observations drop by about 30 percent relative to our baseline regression, so it is not surprising that statistical significance drops as well. The coefficient on polarization is negative and significant at the 1 percent level while the coefficient on fractionalization is positive but insignificant in this smaller sample. In columns (3)-(5) we show that similar results are obtained if we control for past urbanization growth for which we have data available for the entire 1820-1870 period, although only for a much smaller sample. In Appendix Table 6 we repeat the regressions in columns (1) and (2) of Appendix Table 5 including also an indicator variable for counties' railway access during the 1850-1870 period. Again results are very similar.

Our baseline regressions are for the 1870-1920 period; as discussed in Section 2, the reason for this is that the aim of our paper is to examine the long-run effects that changes in the cultural diversity of US counties had on output growth during the age of mass migration. In Appendix Tables 7-10 we show that our main findings continue to hold when focusing on somewhat shorter time periods, such as 1880-1920 (Appendix Table 7), 1890-1920 (Appendix Table 8), 1900-1920 (Appendix Table 9), and 1910-

[30]We furthermore note that in the literature (e.g. Mauro, 1995, Fearon, 2003, Montalvo and Reynal-Querol, 2005, Esteban et al., 2012) the common practice is not to log the fractionalization and polarization index.

1920 (Appendix Table 10).[31] The main result is that for these alternative time periods cultural fractionalization has a positive effect on output growth and cultural polarization has a negative effect. Statistically, the estimates are significant at the conventional significance levels for the majority of the alternative time periods. Column (4) of Appendix Tables 7-10 also shows that the results are robust to controlling for counties' past output growth.

We have further examined the robustness of our baseline results to using a 10-year non-overlapping panel dataset that spans the period 1870-1920. The estimates are reported in Appendix Table 11. In columns (1)-(3) we report least squares estimates. In column (4) we report IV estimates that also control for lagged output per capita growth. Following Bond et al. (2010), we address biases arising in the first-difference specification of the dynamic panel by instrumenting the change in output per capita by the lagged level of output per capita (we also do that for Appendix Tables 5-10 once we control for past output per capita growth). The changes in the fractionalization and polarization index are instrumented by the respective supply-push instruments. The main finding in the panel estimation is that changes in polarization have a significant negative effect and changes in fractionalization have a significant positive effect on output per capita growth.

Another issue is whether our estimates are driven by the Southern US states. The Southern US states are characterized by a high degree of polarization. This high degree of polarization is due to a low share of immigrants but a high share of African-Americans and white US-borns. It is also interesting to note that output per capita in the Southern US states was much lower than in the other US states (see Table 1). To check whether our estimates are driven by the Southern US states, we report in Appendix Table 13 estimates that exclude all counties in the US South. We find that excluding the Southern US states leaves our main finding regarding a significant negative output effect of polarization unaffected. The effect of fractionalization on output per capita is however insignificant in this sub-sample that excludes the US South.

Ashraf and Galor (2013) argue that there is a potential trade-off between the beneficial and detrimental effects of fractionalization. In line with their argument, the authors provide empirical evidence for a significant positive linear effect of fractionalization on development and a significant negative quadratic effect. Our focus is on the average marginal effects that fractionalization and polarization have on output per capita. Thus, in our baseline specification we did not control for a quadratic fractionalization term. In columns (1)-(4) of Appendix Table 14 we document that our baseline estimates are robust to controlling for a quadratic fractionalization term. In particular, controlling for a quadratic fractionalization term does not significantly change the estimated coefficient on the polarization index while the effect on the fractionalization index turns insignif-

[31] We had to impute the manufacturing data for the year 1910 because no manufacturing data were reported in the 1910 Census at the county level.

icant. On the other hand, columns (5)-(8) of Appendix Table 14 show that, without including polarization in the regression, there is a significant positive linear effect of fractionalization and a significant negative quadratic effect.

All of our regressions control for initial county characteristics such as initial output per capita and population size. There are several reasons why we controlled for these initial conditions. The first, and most obvious reason, is that controlling for these initial conditions increases the R-squared; it also reduces the variance of the error term and thus standard errors on the parameter estimates. A second, and more subtle reason for controlling for these initial conditions, is that these initial conditions could be determinants of initial fractionalization and polarization. By controlling for these initial conditions, we ensure that our estimates are not driven by cross-county differences in output per capita and other factors which could affect both, the change in fractionalization and polarization as well as the change in output per capita. For completeness, it may also be of interest to see results that include in the regression model the 1870-1920 changes of these control variables. Appendix Table 15 presents the results. This appendix table shows that using the 1870-1920 period changes in the control variables does not lead to significantly different effects of polarization and fractionalization on output from those estimates that we had presented in Section 5.1.

Appendix Table 16 shows that the instrumental variables regressions produce similar results when splitting the sample into above and below median output per capita. Both, in the above and below median output per capita sample does the instrumental variables estimation yield a negative and significant effect of polarization. For fractionalization, the size of the coefficient is positive and of similar magnitude as in the baseline estimation. However, standard errors are larger when doing the sample split. We also note that in the instrumental variables regressions we cannot reject the hypothesis that the coefficients on polarization are the same in the two sub-samples. And this is also true for the estimated coefficients on fractionalization.

As a closing remark, we note that multicollinearity is a small-sample problem (e.g. Greene, 2003, Ch. 4) that is unlikely to be a major issue in our regressions. The number of observations in our regressions often exceeds two thousand and statistical significance on the polarization and fractionalization index is maintained in the majority of specifications when including both variables on the right-hand side of the regression. Thus, the common symptom of a multicollinearity problem – insignificant estimates – is not present in our regressions. We document in Appendix Table 17 that our main results continue to hold when we exclude the top and bottom 1st percentile of the 1870-1920 changes in counties' fractionalization, polarization, and the log of output per capita; in Appendix Table 18 we show that our results are also robust to excluding the top and bottom 1st percentiles of the levels of these variables. Hence, it is not the case that small changes in the data produce starkly different estimation results.

4.7 Conclusion

In this paper we investigated the effects that changes in cultural diversity had on economic growth by exploiting the wave of mass immigration to the US during the 1870-1920 period as a plausibly exogenous source of variation in cultural diversity. As immigrants flocked from European countries to the US the cultural composition of the US population changed significantly. We showed that increases in the cultural fractionalization of US counties significantly increased output per capita, while increases in the cultural polarization had a significant negative output per capita effect. Our finding that increases in cultural fractionalization significantly increased output per capita is consistent with the argument in the cultural economics literature that greater diversity of the population increases output per capita due to greater variety of workers' skills and a greater variety of goods and services. On the other hand, our finding that increases in cultural polarization significantly decreased output per capita is consistent with the conflict literature that has emphasized the negative, socially destabilizing effect of cultural polarization. An important implication of the findings in our paper is that it is crucial to distinguish between fractionalization and polarization when examining empirically the effects that cultural diversity has on economic growth.

Appendix: Data Source and Variable Description

Our main data source is the Inter-University Consortium for Political and Social Research (ICPSR) 2896 data file. The ICPSR 2896 data file contains detailed decennial US county and state level data on demographic, economic, and social variables which were collected by the US Bureau of Census for the period 1790-2000.[32] One key advantage of the ICPSR data set is that it enables us to exploit the underlying cultural heterogeneity in the United States at the county level. In particular, the ICPSR data set comprises – from 1870 onwards – detailed information about the country of origin of foreign-born, white US-born citizens and African-Americans which is necessary to calculate the cultural diversity indices described in Section 3.

As a further database, we use the Integrated Public Use Microdata Series (IPUMS-USA) which consists of more than fifty high-precision samples of the American population drawn from fifteen federal censuses and from the American Community Surveys of 2000-2008. The IPUMS is a public project and data are freely available. For more information see: http://usa.ipums.org/usa/index.shtml. The IPUMS gives us the possibility to exploit individual level data and construct aggregate data at the county level whenever these variables are missing in the ICPSR data set, but available at the IPUMS. We used the IPUMS database to construct the fractionalization index of workers' occupations, the industry diversity index and the share of public sector employment (see Section 5). We use the IPUMS benchmark occupation classification variable *occ1950* and the benchmark industry classification variable *ind1950* to construct the fractionalization index of occupations and the industry diversity index, respectively.[33] In the supplementary appendix we provide a detailed description of the variables used in our empirical analysis (if not further specified, variables are selected from the ICPSR 2896 data file).

[32] More information about the data set (i.e. scope of study, data collection and data source) can be found at http://www.icpsr.umich.edu/icpsrweb/ICPSR/studies/02896.

[33] The occupation classification code of 1950 assigns an occupation code to the individual's reported occupation and is a reference for all Census occupation data available at the IPUMS. Census years with different occupation coding schemes (as the 1870 and 1920 Census) are converted by the IPUMS into the occupation classification of 1950 (i.e. the variable *occ1950*) to make occupations over time comparable. The same applies to the IPUMS industrial classification variable *ind1950*, which contains coded information about the industry an individual worked in. For more information see the IPUMS at http://usa.ipums.org/usa/index.shtml.

4.8 References

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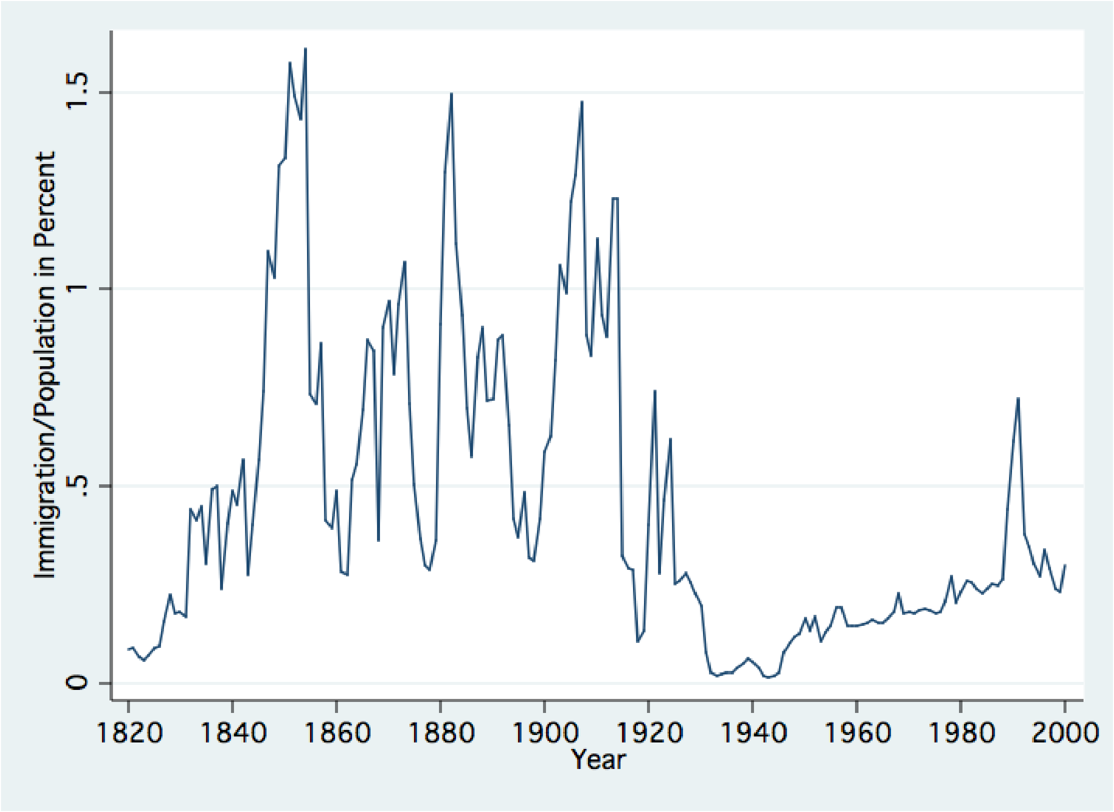
4.9 Figures and Tables

Figure 4.1: Annual Immigration as a Fraction of the US Population, 1820 - 2000



Source: Historical Statistics of the United States (Barde et al., 2006), Statistical Abstract of the US, eh.net database and Kim (Figure 3, 2007).

Figure 4.2: Relationship between Fractionalization and Polarization

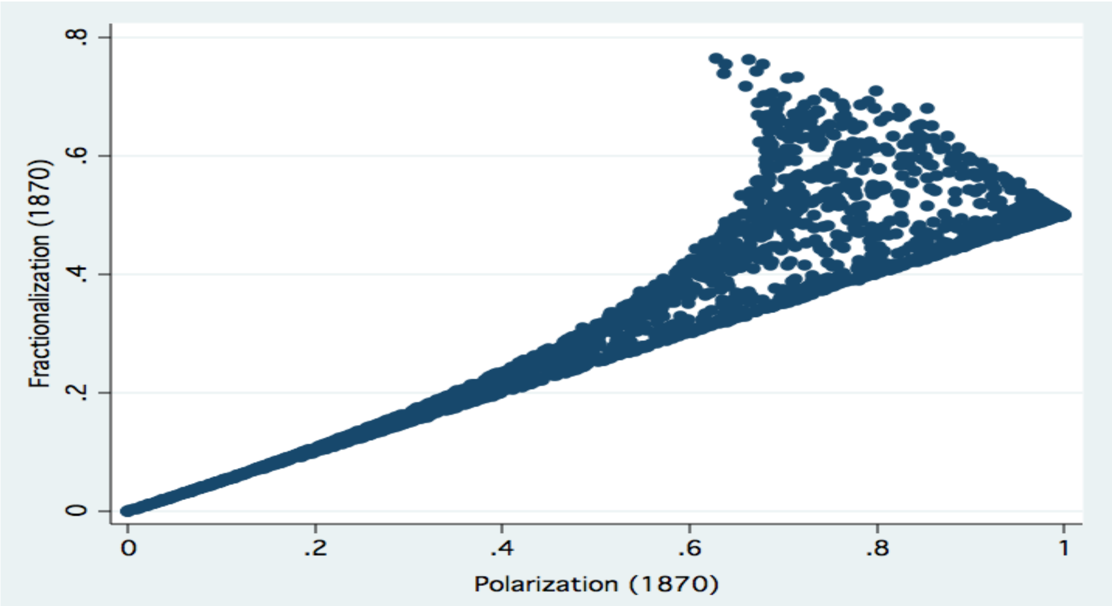


Figure 2A

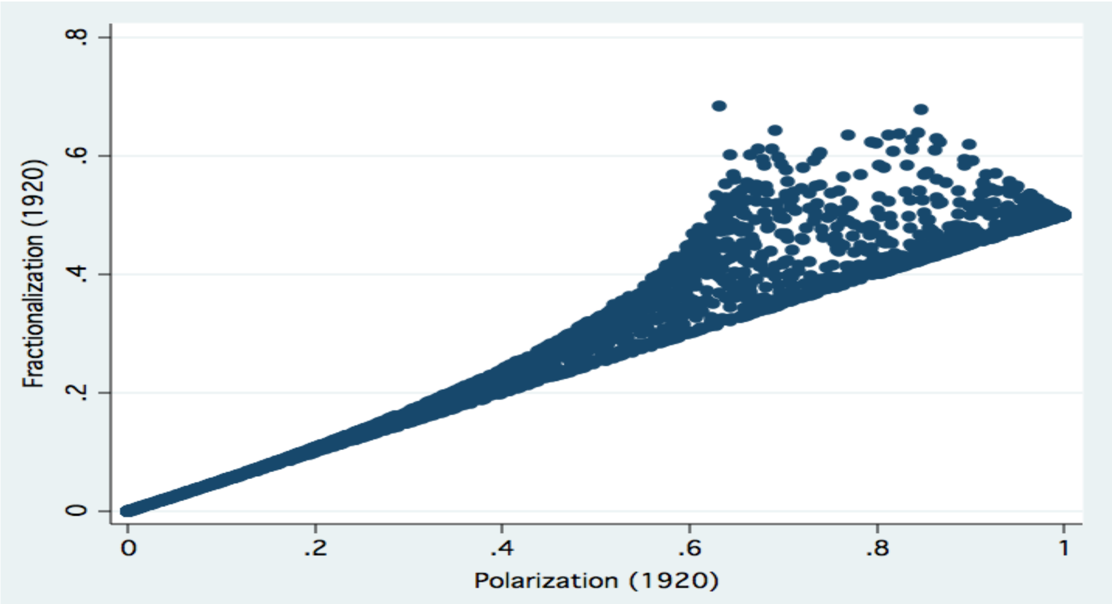


Figure 2B

Figure 4.3: Relationship between Changes in Polarization and Fractionalization

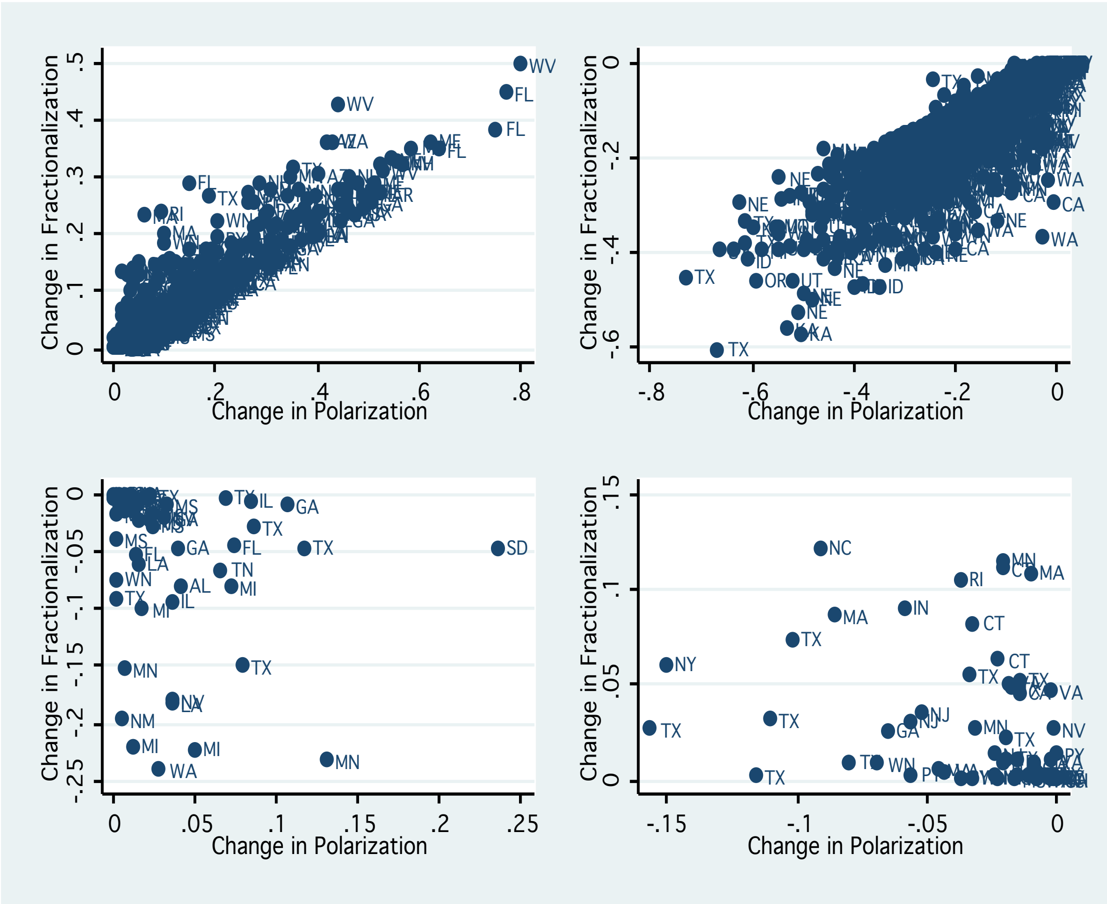


Figure 3 shows the relationship between the 1870-1920 changes of US countiesÕ fractionalization and polarization. The top left-hand panel shows this relationship for positive changes in polarization; the top right-hand side panel shows this relationship for negative changes in polarization. The bottom left-hand side panel shows observations of positive changes in polarization and negative changes in fractionalization; the bottom right-hand side panel shows observations of negative changes in polarization and positive changes in fractionalization.

Figure 4.4:

Link between Changes in Polarization (Fractionalization) and Output Growth

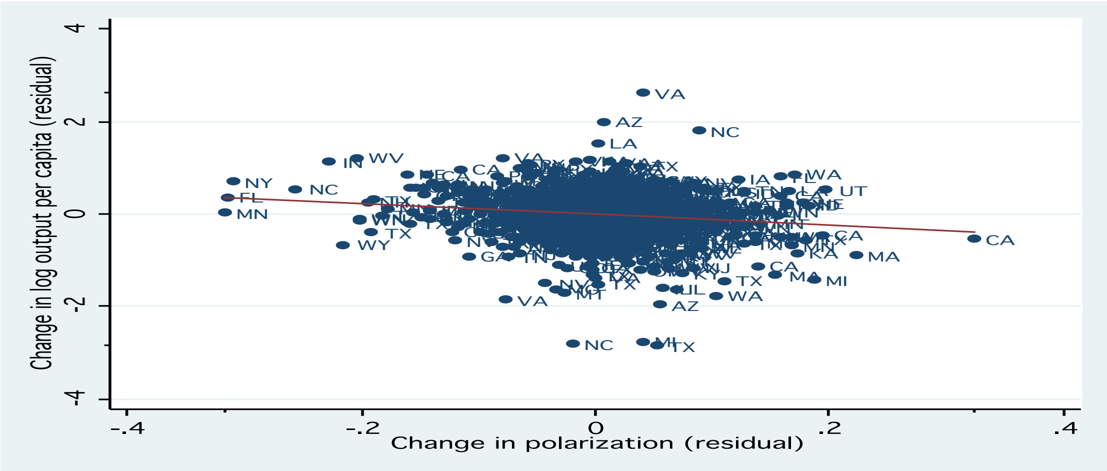


Figure 4A

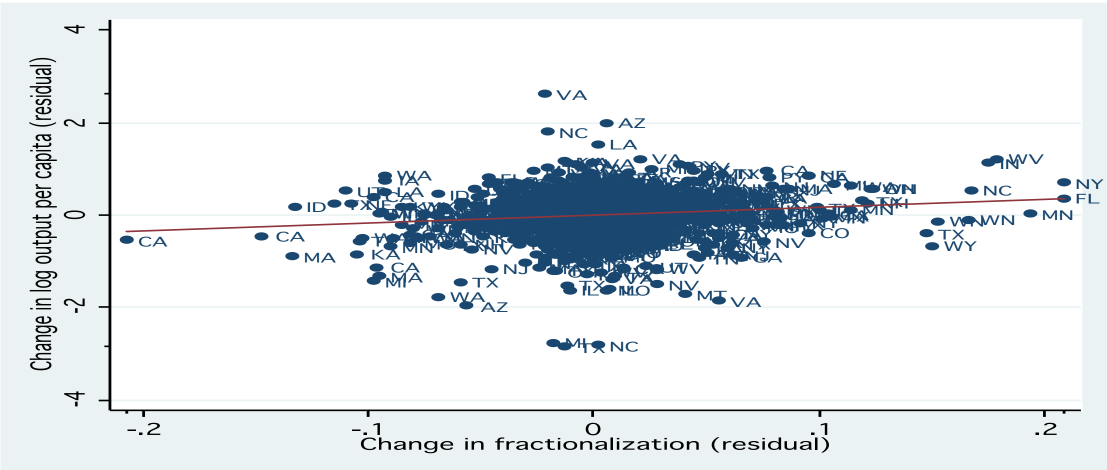


Figure 4B

Figure 4A shows the relationship between the 1870-1920 (residual) changes in the log of output per capita and polarization. The residuals are obtained from regressing the 1870-1920 changes in the log of output per capita and polarization on the 1870-1920 change in fractionalization as well as the same set of control variables as listed in column (3) of Table 2. Figure 4B shows the relationship between the 1870-1920 (residual) changes in the log of output per capita and fractionalization. The residuals are obtained from regressing the 1870-1920 changes in the log of output per capita and fractionalization on the 1870-1920 changes in polarization as well as the same set of control variables as listed in column (3) of Table 2.

Table 1: Descriptive Statistics

MEAN: US/REGIONS

REGION	(LN) OUTPUT	FRAC	POL	FOREIGN-BORN	AFRO-AMERICANS	WHITE US-BORN
US	4.56	0.30	0.52	0.09	0.13	0.76
WEST	4.70	0.37	0.52	0.19	0.01	0.77
MIDWEST	4.75	0.27	0.43	0.15	0.01	0.83
NORTHEAST	4.92	0.28	0.44	0.15	0.01	0.84
SOUTH	4.30	0.32	0.61	0.02	0.28	0.70

STANDARD DEVIATION (Overall/Between/Within): US/REGIONS

REGION	(LN) OUTPUT	FRAC	POL	FOREIGN-BORN	AFRO-AMERICANS	WHITE US-BORN
US	(0.92;0.67;0.75)	(0.17;0.16;0.06)	(0.29;0.27;0.08)	(0.12;0.11;0.04)	(0.21;0.20;0.03)	(0.20;0.19;0.05)
WEST	(1.06;0.80;0.79)	(0.16;0.15;0.08)	(0.17;0.17;0.09)	(0.11;0.09;0.06)	(0.01;0.01;0.01)	(0.13;0.13;0.07)
MIDWEST	(0.88;0.50;0.79)	(0.17;0.16;0.07)	(0.23;0.21;0.08)	(0.13;0.12;0.06)	(0.03;0.03;0.01)	(0.14;0.14;0.06)
NORTHEAST	(0.68;0.40;0.55)	(0.15;0.14;0.06)	(0.19;0.17;0.08)	(0.10;0.09;0.04)	(0.02;0.02;0.01)	(0.10;0.09;0.04)
SOUTH	(0.90;0.70;0.75)	(0.17;0.17;0.04)	(0.33;0.32;0.07)	(0.06;0.06;0.02)	(0.24;0.24;0.04)	(0.23;0.23;0.05)

Table 2
The Impact of Polarization and Fractionalization on Output Growth

	PANEL A: Polarization and Fractionalization			
	Dependent Variable: Δln(Output p.c.)			
	LS	LS	LS	IV (Supply Push (SP))
ΔPOL	-0.763**	-1.199***	-1.062***	-2.687***
	(0.335)	(0.342)	(0.320)	(0.555)
$\Delta FRAC$	0.622	1.356**	1.038**	1.984**
	(0.535)	(0.569)	(0.485)	(0.832)
Observations	2160	2160	2160	2160
R^2	0.265	0.546	0.564	-
First-stage (Kleibergen-Paap) F-Statistic	-	-	-	27.97
Anderson-Rubin Wald-Test (p-val.)	-	-	-	0.00
Endogeneity Test Statistic POL (p-val.)	-	-	-	0.00
Endogeneity Test Statistic FRAC (p-val.)	-	-	-	0.01
	PANEL B: Polarization Only			
	LS	LS	LS	IV (Supply Push (SP))
ΔPOL	-0.375***	-0.424***	-0.497***	-1.737***
	(0.0891)	(0.134)	(0.138)	(0.390)
Observations	2160	2160	2160	2160
R^2	0.263	0.541	0.562	-
First-stage (Kleibergen-Paap) F-Statistic	-	-	-	50.39
Anderson-Rubin Wald-Test (p-val.)	-	-	-	0.00
Endogeneity Test Statistic POL (p-val.)	-	-	-	0.00
	PANEL C: Fractionalization Only			
	LS	LS	LS	IV (Supply Push (SP))
$\Delta FRAC$	-0.406**	-0.423*	-0.608***	-1.422**
	(0.176)	(0.241)	(0.220)	(0.572)
Observations	2160	2160	2160	2160
R^2	0.260	0.536	0.557	-
First-stage (Kleibergen-Paap) F-Statistic	-	-	-	63.57
Anderson-Rubin Wald-Test (p-val.)	-	-	-	0.00
Endogeneity Test Statistic FRAC (p-val.)	-	-	-	0.11
State FE	yes	yes	yes	yes
Initial Controls	no	yes	yes	yes
Population Shares	no	no	yes	yes
Population Growth	no	no	yes	yes

Huber robust standard errors (shown in parentheses) are clustered at the state level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. In columns (1)-(3) the method of estimation is least squares. In column (4) the method of estimation is two-stage least squares. The instrumental variable is the supply-push component of immigrant inflows, IV (Supply Push); also see Section 4 for further details. Initial control variables (1870) are output per capita, the urbanization rate, land concentration, the manufacturing share, population size, labor participation rate, counties' rail access, the share of native-born white and the share of African-Americans (estimates not reported in the table).

Table 3
First Stage and Reduced Form for Supply Push IV Estimation

	ΔPOL	ΔFRAC	Δln(Output p.c.)
	First Stage	First Stage	Reduced Form
Supply Push ΔPOL	0.488***	0.0333	-1.246***
	(0.0889)	(0.0512)	(0.252)
Supply Push $\Delta FRAC$	-0.207	0.638***	1.820***
	(0.225)	(0.155)	(0.567)
Observations	2160	2160	2160
R^2	0.580	0.707	0.571
State FE	yes	yes	yes
Initial Controls	yes	yes	yes
Population Shares	yes	yes	yes
Population Growth	yes	yes	yes

Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1. In columns (1)-(3) the method of estimation is least squares. Initial control variables (1870) are output per capita, the urbanization rate, land concentration, the manufacturing share, population size, labor participation rate, counties' rail access, the share of native-born white and the share of African-Americans (estimates not reported in the table).

Table 4
IV Estimates Using Initial Fractionalization and Polarization as Additional Instruments

	Δln(Output p.c.)		
	IV(SP and Initial) 2SLS	IV (SP and Initial) GMM	IV (Initial) 2SLS
ΔPOL	-2.227***	-2.250***	-2.357***
	(0.428)	(0.418)	(0.458)
$\Delta FRAC$	1.232	1.707**	0.857
	(0.886)	(0.793)	(1.176)
Observations	2160	2160	2160
First-stage (Kleibergen-Paap) F-Statistic	17.31	17.31	12.91
Hansen-J statistic (p-val.)	0.32	0.32	n.a.
Anderson-Rubin Wald-Test (p-val.)	0.00	0.00	0.00
Endogeneity Test Statistic POL (p-val.)	0.01	0.01	0.01
Endogeneity Test Statistic FRAC (p-val.)	0.02	0.02	0.02
State FE	yes	yes	yes
Initial Controls	yes	yes	yes
Population Shares	yes	yes	yes
Population Growth	yes	yes	yes

Huber robust standard errors (shown in parentheses) are clustered at the state level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. In columns (1) and (3) the method of estimation is two-stage least squares. In column (2) the method of estimation is two-step GMM estimation. In columns (1)-(2), the instrumental variables are the supply-push component of immigrant inflows and the initial 1870 polarization and fractionalization index (IV (Supply Push and Initial)). In column (3), the instrumental variables are the initial 1870 polarization and fractionalization index (IV (Initial)). Initial control variables (1870) are output per capita, the urbanization rate, land concentration, the manufacturing share, population size, labor participation rate, counties' rail access, the share of native-born white and the share of African-Americans (estimates not reported in the table).

Table 5
First Stage and Reduced Form: Effect of Initial FRAC and POL

	ΔPOL	ΔFRAC	Δ ln(Output p.c.)
	First Stage	First Stage	Reduced Form
Initial POL 1870	-0.568***	0.106*	1.429***
	(0.0935)	(0.0603)	(0.303)
Initial FRAC 1870	0.585***	-0.500***	-1.807***
	(0.191)	(0.123)	(0.662)
Observations	2160	2160	2160
R^2	0.575	0.699	0.572
State FE	yes	yes	yes
Initial Controls	yes	yes	yes
Population Shares	yes	yes	yes
Population Growth	yes	yes	yes

Huber robust standard errors (shown in parentheses) are clustered at the state level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. In columns (1)-(3) the method of estimation is least squares. Initial control variables (1870) are output per capita, the urbanization rate, land concentration, the manufacturing share, population size, labor participation rate, counties' rail access, the share of native-born white and the share of African-Americans (estimates not reported in the table).

Table 6
The Impact of Polarization and Fractionalization on Occupational and Industry Diversity

	ΔOccupational Diversity				ΔIndustry Diversity			
	(1) LS	(2) IV (SP)	(3) IV (Initial)	(4) IV (SP and Initial)	(5) LS	(6) IV (SP)	(7) IV (Initial)	(8) IV (SP and Initial)
ΔPOL	0.120	0.287	0.0961	0.0482	0.283***	-0.0531	-0.0730	-0.0899
	(0.0919)	(0.179)	(0.185)	(0.150)	(0.104)	(0.152)	(0.186)	(0.184)
$\Delta FRAC$	-0.00296	0.485	1.082**	0.860***	-0.153	0.484*	0.573	0.549**
	(0.147)	(0.328)	(0.449)	(0.328)	(0.194)	(0.275)	(0.384)	(0.274)
Observations	2160	2160	2160	2160	2154	2154	2154	2154
R^2	0.335	-	-	-	0.231	-	-	-
First-stage (Kleibergen-Paap) F-Statistic	-	26.96	10.61	17.92	-	26.72	10.63	17.79
Hansen-J statistic (p-val.)	-	n.a.	n.a.	0.13	-	n.a.	n.a.	0.90
Anderson-Rubin Wald-Test (p-val.)	-	0.00	0.00	0.00	-	0.10	0.15	0.13
Endogeneity Test Statistic POL (p-val.)	-	0.00	0.00	0.01	-	0.84	0.67	0.68
Endogeneity Test Statistic FRAC (p-val.)	-	0.00	0.00	0.01	-	0.48	0.42	0.34
State FE	yes	yes	yes	yes	yes	yes	yes	yes
Initial Controls	yes	yes	yes	yes	yes	yes	yes	yes
Population Shares	yes	yes	yes	yes	yes	yes	yes	yes

Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1. In columns (1) and (5) the method of estimation is least squares. In columns (2)-(4) and (6)-(8) the method of estimation is two-stage least squares. In columns (2) and (6) the instrumental variable is the supply-push component of immigrant inflows (IV (Supply Push)); also see Section 4 for further details. In columns (3) and (7) the instrumental variables are the initial 1870 polarization and fractionalization index (IV (Initial)). In columns (4) and (8), the instrumental variables are the supply-push component of immigrant inflows and the initial 1870 polarization and fractionalization index (IV (Supply Push and Initial)). Initial control variables (1870) are output per capita, the urbanization rate, land concentration, the manufacturing share, population size, labor participation rate, counties' rail access, the share of native-born white and the share of African-Americans (estimates not reported in the table).

Table 7

The Impact of Polarization and Fractionalization on the Tax Ratio and Public Sector Employees

	ΔTax Ratio				ΔShare of Public Sector Employees			
	(1) LS	(2) IV (SP)	(3) IV (Initial)	(4) IV (SP and Initial)	(5) LS	(6) IV (SP)	(7) IV (Initial)	(8) IV (SP and Initial)
ΔPOL	1.193***	1.943***	1.983***	1.987***	0.414	2.660	3.991**	3.410**
	(0.381)	(0.564)	(0.563)	(0.547)	(1.392)	(1.756)	(1.578)	(1.394)
$\Delta FRAC$	-1.251*	-1.555*	-1.657	-1.623*	-1.961	-8.563**	-10.60**	-9.786**
	(0.641)	(0.936)	(1.068)	(0.908)	(2.301)	(4.308)	(4.129)	(3.913)
Observations	2084	2084	2084	2084	283	283	283	283
R^2	0.757	-	-	-	0.734	-	-	-
First-stage (Kleibergen-Paap) F-Statistic	-	25.53	12.26	16.62	-	15.65	9.36	9.25
Hansen-J statistic (p-val.)	-	n.a.	n.a.	0.99	-	n.a.	n.a.	0.40
Anderson-Rubin Wald-Test (p-val.)	-	0.00	0.00	0.00	-	0.14	0.02	0.01
Endogeneity Test Statistic POL (p-val.)	-	0.04	0.11	0.03	-	0.37	0.35	0.66
Endogeneity Test Statistic FRAC (p-val.)	-	0.06	0.19	0.05	-	0.22	0.16	0.31
State FE	yes	yes	yes	yes	yes	yes	yes	yes
Initial Controls	yes	yes	yes	yes	yes	yes	yes	yes
Population Shares	yes	yes	yes	yes	yes	yes	yes	yes
Population Growth	yes	yes	yes	yes	yes	yes	yes	yes

Huber robust standard errors (shown in parentheses) are clustered at the state level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. In columns (1) and (5) the method of estimation is least squares. In columns (2)-(4) and (6)-(8) the method of estimation is two-stage least squares. In columns (2) and (6) the instrumental variable is the supply-push component of immigrant inflows (IV (Supply Push)); also see Section 4 for further details. In columns (3) and (7) the instrumental variables are the initial 1870 polarization and fractionalization index (IV (Initial)). In columns (4) and (8), the instrumental variables are the supply-push component of immigrant inflows and the initial 1870 polarization and fractionalization index (IV (Supply Push and Initial)). Initial control variables (1870) are output per capita, tax quote (Columns 1-4), public administration share (Columns 5-8), the urbanization rate, land concentration, the manufacturing share, population size, labor participation rate, counties' rail access, the share of native-born white and the share of African-Americans (estimates not reported in the table).

Table 8
The Impact of Polarization and Fractionalization on Urban and Population Growth

	ΔUrban Share				Δ ln(Population)			
	(1) LS	(2) IV (SP)	(3) IV (Initial)	(4) IV (SP and Initial)	(5) LS	(6) IV (SP)	(7) IV (Initial)	(8) IV (SP and Initial)
ΔPOL	0.121	0.0543	0.0781	0.0645	-1.429***	-1.399*	-0.914	-1.110*
	(0.0818)	(0.127)	(0.106)	(0.0978)	(0.475)	(0.781)	(0.625)	(0.578)
$\Delta FRAC$	-0.0736	-0.282	-0.314	-0.293*	4.651***	1.409	0.614	1.142
	(0.139)	(0.210)	(0.193)	(0.176)	(0.826)	(1.288)	(0.952)	(0.907)
Observations	2161	2161	2161	2161	2161	2161	2161	2161
R^2	0.503	-	-	-	0.665	-	-	-
First-stage (Kleibergen-Paap) F-Statistic	-	28.03	12.92	17.40	-	26.95	10.56	18.00
Hansen-J statistic (p-val.)	-	n.a.	n.a.	0.88	-	n.a.	n.a.	0.26
Anderson-Rubin Wald-Test (p-val.)	-	0.23	0.23	0.43	-	0.12	0.31	0.17
Endogeneity Test Statistic POL (p-val.)	-	0.07	0.12	0.08	-	0.00	0.00	0.00
Endogeneity Test Statistic FRAC (p-val.)	-	0.06	0.11	0.06	-	0.00	0.00	0.00
State FE	yes	yes	yes	yes	yes	yes	yes	yes
Initial Controls	yes	yes	yes	yes	yes	yes	yes	yes
Population Shares	yes	yes	yes	yes	yes	yes	yes	yes
Population Growth	yes	yes	yes	yes	no	no	no	no

Huber robust standard errors (shown in parentheses) are clustered at the state level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. In columns (1) and (5) the method of estimation is least squares. In columns (2)-(4) and (6)-(8) the method of estimation is two-stage least squares. In columns (2) and (6) the instrumental variable is the supply-push component of immigrant inflows (IV (Supply Push)); also see Section 4 for further details. In columns (3) and (7) the instrumental variables are the initial 1870 polarization and fractionalization index (IV (Initial)). In columns (4) and (8), the instrumental variables are the supply-push component of immigrant inflows and the initial 1870 polarization and fractionalization index (IV (Supply Push and Initial)). Initial control variables (1870) are output per capita, the urbanization rate, land concentration, the manufacturing share, population size, labor participation rate, counties' rail access, the share of native-born white and the share of African-Americans (estimates not reported in the table).

Table 9

The Impact of Polarization and Fractionalization on Output Growth: Different Grouping

	Δln(Output p.c.)							
	V1: Four Groups Only				V2: Regional Grouping			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	LS	IV (SP)	IV (Initial)	IV (SP and Initial)	LS	IV (SP)	IV (Initial)	IV (SP and Initial)
Δ*POL (V1)*	-0.162	-1.296*	-1.046	-1.054				
	(0.488)	(0.742)	(0.685)	(0.686)				
Δ*FRAC (V1)*	-0.479	1.068	0.602	0.670				
	(0.829)	(1.268)	(1.191)	(1.196)				
Δ*POL (V2)*					-1.653***	-2.192***	-2.112***	-2.135***
					(0.376)	(0.517)	(0.482)	(0.490)
Δ*FRAC (V2)*					2.081***	2.202**	2.027**	2.213***
					(0.590)	(0.875)	(0.947)	(0.814)
Observations	2160	2160	2160	2160	2160	2160	2160	2160
R^2	0.545	-	-	-	0.557	-	-	-
First-stage (Kleibergen-Paap) F-Statistic	-	7.80	7.81	4.20	-	18.20	16.93	17.78
Hansen-J statistic (p-val.)	-	n.a.	n.a.	0.21	-	n.a.	n.a.	0.90
Anderson-Rubin Wald-Test (p-val.)	-	0.12	0.15	0.05	-	0.00	0.00	0.00
Endogeneity Test Statistic POL (p-val.)	-	0.47	0.47	0.33	-	0.30	0.29	0.34
Endogeneity Test Statistic FRAC (p-val.)	-	0.55	0.53	0.37	-	0.37	0.36	0.43
State FE	yes	yes	yes	yes	yes	yes	yes	yes
Initial Controls	yes	yes	yes	yes	yes	yes	yes	yes
Population Shares	yes	yes	yes	yes	yes	yes	yes	yes
Population Growth	yes	yes	yes	yes	yes	yes	yes	yes

Huber robust standard errors (shown in parentheses) are clustered at the state level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. In columns (1)-(4), the four groups used for the construction of the polarization and fractionalization index are foreign-born, US-born whites, African-Americans and Others. In columns (5)-(8), the regional groups used for the construction of the polarization and fractionalization index are Northern, Eastern, Center and Mediterranean Southern Europe, Asia, Central and South America, Pacific, Canada and US. In columns (1) and (5) the method of estimation is least squares. In columns (2) and (6), the instrumental variable is the supply-push component of immigrant inflows (IV (Supply Push)); also see Section 4 for further details. In columns (3) and (7), the instrumental variables are the initial 1870 polarization and fractionalization index (IV (Initial)). In columns (4) and (8), the instrumental variables are the supply-push component of immigrant inflows and the initial 1870 polarization and fractionalization index (IV (Supply Push and Initial)). Initial control variables (1870) are output per capita, the urbanization rate, land concentration, the manufacturing share, population size, labor participation rate, counties' rail access, the share of native-born white and the share of African-Americans (estimates not reported in the table).

Table 10
Polarization and Fractionalization Index with Linguistic Distances (LD)

	Δln(Output p.c.)			
	(1)	(2)	(3)	(4)
	LS	IV (SP)	IV (Initial)	IV (SP and Initial)
ΔPOL_{LD}	-18.54***	-26.06***	-24.92***	-26.98***
	(4.961)	(7.259)	(7.100)	(7.265)
$\Delta FRAC_{LD}$	6.365***	7.306***	6.107**	8.594***
	(2.102)	(2.433)	(2.545)	(2.597)
Observations	2160	2160	2160	2160
R^2	0.551	-	-	-
First-stage (Kleibergen-Paap) F-Statistic	-	19.33	12.06	8.28
Hansen-J statistic (p-val.)	-	n.a.	n.a.	0.10
Anderson-Rubin Wald-Test (p-val.)	-	0.00	0.00	0.00
Endogeneity Test Statistic POL (p-val.)	-	0.33	0.20	0.26
Endogeneity Test Statistic FRAC (p-val.)	-	0.38	0.22	0.27
State FE	yes	yes	yes	yes
Initial Controls	yes	yes	yes	yes
Population Shares	yes	yes	yes	yes
Population Growth	yes	yes	yes	yes

Huber robust standard errors (shown in parentheses) are clustered at the state level *** p<0.01, ** p<0.05, * p<0.1. In column (1) the method of estimation is least squares. In columns (2)-(4) the method of estimation is two-stage least squares. In column (2) the instrumental variable is the supply-push component of immigrant inflows (IV (Supply Push)); also see Section 4 for further details. In column (3) the instrumental variables are the initial 1870 polarization and fractionalization index (IV (Initial)). In column (4) the instrumental variables are the supply-push component of immigrant inflows and the initial 1870 polarization and fractionalization index (IV (Supply Push and Initial)). Initial control variables (1870) are output per capita, the urbanization rate, land concentration, the manufacturing share, population size, labor participation rate, counties' rail access, the share of native-born white and the share of African-Americans (estimates not reported in the table). We construct our measure of fractionalization with distances following Greenberg (1956) as: $FRAC_{LD} = \sum_{i=1}^{N} \sum_{j=1}^{N} \pi_i \pi_j d_{ij}$, where subindices for counties and states are left out for simplicity. The corresponding measure of polarization follows Esteban and Ray (1994, 1999) and is constructed as: $POL_{LD} = \sum_{i=1}^{N} \sum_{j=1}^{N} \pi_i^2 \pi_j d_{ij}$. Both indices include a measure of inter-group distances d_{ij}. To proxy for inter-group distances, we follow Fearon (2003), Desmet et al. (2009a) and Esteban et al. (2010) and use information on linguistic groups compiled by the Ethnologue project to construct a measure of linguistic distances between any two groups as $d_{ij} = 1 - b_{ij}^{\delta}$ (see http://www.ethnologue.com, Desmet et al. (2009a) and Esteban et al. (2010) for further information on the Ethnologue project). The parameter b_{ij} is the ratio of the number of shared branches between i and j to the maximum number of branches between any two languages and $\delta \epsilon (0, 1]$ represents a sensitivity parameter determining how fast the distance declines as the number of shared branches increases (see Desmet et al., 2009a). We used the language trees reported by Ethnologue to construct the parameter b_{ij}. See also Desmet et al. (2009b) for a detailed discussion on the construction of such a language tree. The abbreviation LD denotes the use of language distances for the metric d_{ij}. We use the representative language of each country of origin to construct the linguistic distances. Thus, if two groups speak the same representative language we set $b_{ij} = 1$. Following Fearon (2003) we compute the linguistic distances d_{ij} by setting $\delta = 0.5$. As a further robustness check we compute linguistic distance as Desmet et al. (2009a) using $\delta = 0.05$ and obtain qualitatively similar results, which we show in the supplementary Appendix Table 19.

Table 11: Alternative Instruments

	(1)	(2)	(3)	(4)	(5)
	PANEL A: Second Stage for Δln(Output p.c.)				
ΔPOL	-2.870***	-4.431***	-3.678***	-3.337***	-2.815***
	(0.708)	(1.136)	(0.956)	(0.885)	(0.491)
$\Delta FRAC$		6.258***	3.241**	2.973**	2.269***
		(1.748)	(1.370)	(1.293)	(0.781)
Observations	2160	2160	2160	2160	2160
First-stage (Kleibergen-Paap) F-Statistic	37.23	21.41	24.16	20.35	20.33
Hansen-J statistic (p-val.)	n.a.	n.a.	n.a.	0.21	0.44
Anderson-Rubin Wald-Test (p-val.)	0.00	0.00	0.00	0.00	0.02
Endogeneity Test Statistic POL (p-val.)	0.02	0.02	0.00	0.01	0.00
Endogeneity Test Statistic FRAC (p-val.)	-	-	0.00	0.01	0.01
	PANEL B: First Stage for ΔPOL				
Supply Push ΔPOL_{WAR}	6.179***	3.817***	4.933***	4.961***	3.231***
	(1.013)	(0.825)	(1.079)	(1.084)	(1.068)
Supply Push $\Delta FRAC$			0.809***	0.798***	-0.154
			(0.161)	(0.160)	(0.217)
Δ*Number of Groups*				0.00475***	0.00452***
				(0.00122)	(0.00116)
Supply Push ΔPOL					0.433***
					(0.0935)
R^2	0.538	0.932	0.562	0.570	0.591
	PANEL C: First Stage for ΔFRAC				
Supply-Push ΔPOL_{WAR}			0.456	0.469	0.370
			(0.500)	(0.511)	(0.568)
Supply Push $\Delta FRAC$			0.705***	0.699***	0.645***
			(0.0923)	(0.0917)	(0.155)
Δ*Number of Groups*				0.00220***	0.00219***
				(0.000686)	(0.000682)
Supply-Push ΔPOL					0.0247
					(0.0548)
R^2	-	-	0.707	0.710	0.710
State FE	yes	yes	yes	yes	yes
Initial Controls	yes	yes	yes	yes	yes
Population Shares	yes	yes	yes	yes	yes
Population Growth	yes	yes	yes	yes	yes

Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1. In columns (1)-(5), the instrumental variable for the polarization index is the supply-push component of immigrant inflows, weighted by the years of wars; also see Section 5.6 for further details. In columns (3)-(5) the instrumental variable for the fractionalization index is the supply-push component of immigrant inflows; also see Section 4 for further details. As further instruments we include the change in the number of immigration groups in columns (4)-(5) and the unweighted supply-push component of immigrant inflows for the polarization index in column (5). The initial control variables (1870) are output per capita, the urbanization rate, land concentration, the manufacturing share, population size, labor participation rate, counties' rail access, the share of native-born white and the share of African-Americans.

SUPPLEMENTARY APPENDIX

to the paper

Cultural Diversity and Economic Growth: Evidence from the US during the Age of Mass Migration

Regression Results

Table 1: Displaying all Controls for Table II – Panel A –

	Dependent Variable: Δln(Output p.c.)			
	LS	LS	LS	IV (SP)
ΔPOL	-0.763**	-1.199***	-1.062***	-2.687***
	(0.335)	(0.342)	(0.320)	(0.555)
$\Delta FRAC$	0.622	1.356**	1.038**	1.984**
	(0.535)	(0.569)	(0.485)	(0.832)
Urbanization Rate 1870		0.153	0.0839	0.128
		(0.139)	(0.200)	(0.195)
Labor Participation Rate 1870		0.0513*	0.0536*	0.0148
		(0.0293)	(0.0289)	(0.0325)
Population 1870		-0.00379	0.0488	0.0642**
		(0.0295)	(0.0296)	(0.0302)
Output p.c. 1870		-0.690***	-0.699***	-0.702***
		(0.0445)	(0.0477)	(0.0483)
Land Concentration 1870		-0.484***	-0.506***	-0.412**
		(0.165)	(0.159)	(0.168)
Manufacturng Share 1870		-0.0896	-0.108	-0.138
		(0.114)	(0.108)	(0.101)
Rail Access 1870		0.0880***	0.0984***	0.0865***
		(0.0255)	(0.0265)	(0.0292)
Population Growth 1870-1920			0.0676	0.126**
			(0.0424)	(0.0531)
Share Austro-Hungarian 1870			-0.0156	0.843
			(1.347)	(1.455)
Share Benelux 1870			0.389	0.261
			(0.970)	(0.941)
Share East Europeans 1870			-2.694	-16.59*
			(6.987)	(8.972)
Share Canadian 1870			-0.0300	0.349
			(0.699)	(0.810)
Share Central and South America 1870			-2.006***	-1.379*
			(0.563)	(0.802)
Share Scandinavia 1870			-0.267	-0.374
			(0.450)	(0.617)
Share French 1870			-2.472	-0.926
			(2.559)	(2.860)
Share Germans 1870			-0.900	-1.545**
			(0.540)	(0.775)
Share Irish 1870			-1.652	-1.702
			(1.013)	(1.118)
Share Italians 1870			-5.897*	-2.691
			(3.319)	(4.019)
Share Pacific 1870			-1.159	-0.917
			(0.805)	(0.888)
Share Polish 1870			-14.37	-20.15
			(36.04)	(39.76)
Share Portuguese 1870			-80.07	-157.9
			(206.3)	(202.1)
Share Spanish 1870			-80.31***	-80.06***
			(11.62)	(12.24)
Share Swiss 1870			-1.197	-1.460
			(2.451)	(2.655)
Share United Kingdom 1870			-1.436	-1.120
			(1.328)	(1.385)
Share Asian 1870			-1.700**	-1.493
			(0.782)	(0.992)
Share Afro-American 1870		-0.387	-1.370**	-0.822
		(0.350)	(0.596)	(0.850)
Share White US Born 1870		-0.172	-1.114**	-0.700
		(0.398)	(0.546)	(0.762)
Observations	2160	2160	2160	2160
R^2	0.265	0.546	0.564	-

Huber robust standard errors (shown in parentheses) are clustered at the state level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. In this table we display the control variables (except state fixed effects) that correspond to Table 2, Panel (A).

Table 2: Displaying all Controls for Table II – Panel B –

	Dependent Variable: Δln(Output p.c.)			
	LS	LS	LS	IV (SP)
ΔPOL	-0.375***	-0.424***	-0.497***	-1.737***
	(0.0891)	(0.134)	(0.138)	(0.390)
Urbanization Rate 1870		0.166	0.0713	0.108
		(0.146)	(0.202)	(0.199)
Labor Participation Rate 1870		0.0527	0.0532*	0.00947
		(0.0314)	(0.0297)	(0.0332)
Population 1870		-0.00565	0.0541*	0.0766**
		(0.0303)	(0.0297)	(0.0301)
Output p.c. 1870		-0.692***	-0.696***	-0.696***
		(0.0464)	(0.0487)	(0.0501)
Land Concentration 1870		-0.479***	-0.511***	-0.411**
		(0.169)	(0.161)	(0.171)
Manufacturng Share 1870		-0.116	-0.119	-0.164
		(0.119)	(0.112)	(0.102)
Rail Access 1870		0.0898***	0.0995***	0.0872***
		(0.0258)	(0.0264)	(0.0290)
Population Growth 1870-1920			0.0833**	0.164***
			(0.0407)	(0.0502)
Share Austro-Hungarian 1870			-0.349	0.270
			(1.311)	(1.413)
Share Benelux 1870			0.279	0.0241
			(0.960)	(0.958)
Share East Europeans 1870			-5.635	-24.14***
			(6.973)	(8.706)
Share Canadian 1870			-0.150	0.151
			(0.628)	(0.684)
Share Central and South America 1870			-1.843***	-0.978
			(0.474)	(0.612)
Share Scandinavia 1870			-0.220	-0.291
			(0.380)	(0.472)
Share French 1870			-3.306	-2.428
			(2.619)	(2.955)
Share Germans 1870			-0.882*	-1.584**
			(0.479)	(0.632)
Share Irish 1870			-1.732*	-1.868*
			(0.990)	(1.009)
Share Italians 1870			-7.019**	-4.580
			(3.232)	(3.645)
Share Pacific 1870			-0.979	-0.526
			(0.745)	(0.708)
Share Polish 1870			-11.79	-15.62
			(37.68)	(42.73)
Share Portuguese 1870			-25.26	-56.35
			(218.5)	(227.8)
Share Spanish 1870			-79.27***	-77.93***
			(11.67)	(12.37)
Share Swiss 1870			-2.145	-3.405
			(2.356)	(2.373)
Share United Kingdom 1870			-1.407	-1.025
			(1.320)	(1.370)
Share Asian 1870			-1.878**	-1.828*
			(0.796)	(1.051)
Share Afro-American 1870		-0.0555	-1.097**	-0.208
		(0.293)	(0.482)	(0.655)
Share White US Born 1870		0.172	-0.844*	-0.106
		(0.318)	(0.434)	(0.529)
Observations	2160	2160	2160	2160
R^2	0.263	0.541	0.562	-

Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1.
In this table we display the control variables (except state fixed effects) that correspond to Table 2, Panel (B).

Table 3: Displaying all Controls for Table II – Panel C –

	Dependent Variable: Δln(Output p.c.)			
	LS	LS	LS	IV (SP)
ΔFRAC	-0.406**	-0.423*	-0.608***	-1.422**
	(0.176)	(0.241)	(0.220)	(0.572)
Urbanization Rate 1870		0.153	0.0591	0.0623
		(0.149)	(0.205)	(0.201)
Labor Participation Rate 1870		0.0560*	0.0588*	0.0429
		(0.0326)	(0.0308)	(0.0307)
Population 1870		-0.00375	0.0542*	0.0664**
		(0.0313)	(0.0302)	(0.0312)
Output p.c. 1870		-0.688***	-0.695***	-0.692***
		(0.0471)	(0.0491)	(0.0495)
Land Concentration 1870		-0.501***	-0.527***	-0.495***
		(0.171)	(0.164)	(0.166)
Manufacturng Share 1870		-0.116	-0.119	-0.144
		(0.121)	(0.114)	(0.111)
Rail Access 1870		0.0926***	0.102***	0.0982***
		(0.0263)	(0.0268)	(0.0257)
Population Growth 1870-1920			0.0816*	0.123***
			(0.0416)	(0.0474)
Share Austro-Hungarian 1870			-0.628	-0.669
			(1.310)	(1.295)
Share Benelux 1870			0.249	0.0714
			(0.979)	(0.979)
Share East Europeans 1870			-4.875	-13.80
			(7.359)	(9.620)
Share Canadian 1870			-0.261	-0.248
			(0.586)	(0.561)
Share Central and South America 1870			-1.864***	-1.426***
			(0.459)	(0.529)
Share Scandinavia 1870			-0.182	-0.170
			(0.352)	(0.334)
Share French 1870			-3.912	-4.252*
			(2.545)	(2.559)
Share Germans 1870			-0.777*	-1.014**
			(0.456)	(0.493)
Share Irish 1870			-1.760*	-1.871*
			(0.997)	(0.976)
Share Italians 1870			-8.003**	-8.011***
			(3.109)	(2.970)
Share Pacific 1870			-0.934	-0.631
			(0.747)	(0.693)
Share Polish 1870			-9.764	-9.107
			(37.94)	(40.48)
Share Portuguese 1870			11.06	42.93
			(226.3)	(239.4)
Share Spanish 1870			-78.85***	-77.55***
			(11.68)	(11.45)
Share Swiss 1870			-2.532	-3.727
			(2.337)	(2.434)
Share United Kingdom 1870			-1.441	-1.282
			(1.319)	(1.321)
Share Asian 1870			-1.989**	-2.111**
			(0.789)	(0.836)
Share Afro-American 1870		-0.0653	-1.056**	-0.525
		(0.334)	(0.472)	(0.572)
Share White US Born 1870		0.181	-0.785*	-0.309
		(0.366)	(0.426)	(0.469)
Observations	2160	2160	2160	2160
R^2	0.260	0.536	0.557	-

Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1.
In this table we display the control variables (except state fixed effects) that correspond to Table 2, Panel (C).

Table 4: Log-Specification

	Δln(Output p.c.)			
	LS	LS	LS	IV (SP)
$\Delta ln(POL)$	-1.279**	-1.875***	-1.644***	-4.648***
	(0.530)	(0.581)	(0.532)	(1.130)
$\Delta ln(FRAC)$	0.991	1.893**	1.393*	2.877*
	(0.758)	(0.859)	(0.733)	(1.557)
Observations	2160	2160	2160	2160
R^2	0.266	0.546	0.565	-
First-stage (Kleibergen-Paap) F-Statistic	-	-	-	28.66
Anderson-Rubin Wald-Test (p-val.)	-	-	-	0.00
Endogeneity Test Statistic POL (p-val.)	-	-	-	0.00
Endogeneity Test Statistic FRAC (p-val.)	-	-	-	0.00
State FE	yes	yes	yes	yes
Initial Controls	no	yes	yes	yes
Population Shares	no	no	yes	yes
Population Growth	no	no	yes	yes

Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1. In columns (1)-(3) the method of estimation is least squares. In column (4) the method of estimation is two-stage least squares. The instrumental variable is the supply-push component of immigrant inflows, IV (Supply Push); also see Section 4 for further details. Initial control variables (1870) are output per capita, the urbanization rate, land concentration, the manufacturing share, population size, labor participation rate, counties' rail access, the share of native-born white and the share of African-Americans (estimates not reported in the table).

Table 5: Controlling for Past Output & Urban Growth

	Δln(Output p.c.)				
	(1)	(2)	(3)	(4)	(5)
ΔPOL	-2.745***	-2.606***	-2.801***	-2.951***	-3.758*
	(0.713)	(0.972)	(0.633)	(0.829)	(1.925)
$\Delta FRAC$	3.113**	2.080	2.141**	2.201	3.183
	(1.269)	(1.983)	(0.953)	(1.717)	(3.260)
Output Growth 1860-1870	0.276***				
	(0.0571)				
Output Growth 1850-1870		0.0879***			
		(0.0286)			
Urban Growth 1860-1870			-0.0154		
			(0.175)		
Urban Growth 1850-1870				-0.148	
				(0.194)	
Urban Growth 1820-1870					0.810**
					(0.371)
Observations	1983	1606	2001	1606	751
First-stage (Kleibergen-Paap) F-Statistic	17.04	15.46	26.75	22.18	11.14
Anderson-Rubin Wald-Test (p-val.)	0.00	0.00	0.00	0.00	0.00
Endogeneity Test Statistic POL (p-val.)	0.05	0.01	0.00	0.00	0.01
Endogeneity Test Statistic FRAC (p-val.)	0.17	0.02	0.01	0.00	0.02
State FE	yes	yes	yes	yes	yes
Initial Controls	yes	yes	yes	yes	yes
Population Shares	yes	yes	yes	yes	yes
Population Growth	yes	yes	yes	yes	yes

Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1. In columns (1)-(5) the method of estimation is two-stage least squares. The instrumental variable for the polarization and fractionalization index is the supply-push component of immigrant inflows, respectively; also see Section 4 for further details. The instrumental variable for past output growth is 1860 output per capita in column (1) and 1850 output per capita in column (2). Initial control variables (1870) are output per capita (in columns (3)-(5)), the urbanization rate, land concentration, the manufacturing share, population size, labor participation rate, counties' rail access, the share of native-born white and the share of African-Americans (estimates not reported in the table).

Table 6: Controlling for Historical Railway Access 1850-1870

	Δln(Output p.c.)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ΔPOL	-2.745***	-2.738***	-3.250***	-3.246***	-2.606***	-2.685***	-2.646***	-2.689***
	(0.713)	(0.710)	(0.882)	(0.882)	(0.972)	(0.942)	(0.938)	(0.933)
$\Delta FRAC$	3.113**	3.113**	3.823*	3.835**	2.080	2.275	2.128	2.271
	(1.269)	(1.258)	(1.983)	(1.955)	(1.983)	(1.921)	(1.919)	(1.897)
Railway Access 1870	0.0339			0.00754	0.0304			0.0132
	(0.0252)			(0.0322)	(0.0291)			(0.0313)
Railway Access 1860		0.0409		0.00892		0.0486*		0.0273
		(0.0308)		(0.0312)		(0.0283)		(0.0306)
Railway Access 1850			0.0569*	0.0512			0.0656**	0.0502
			(0.0333)	(0.0338)			(0.0322)	(0.0316)
Output Growth 1860-1870	0.276***	0.273***	0.205*	0.205*				
	(0.0571)	(0.0573)	(0.118)	(0.118)				
Output Growth 1850-1870					0.0879***	0.0885***	0.0885***	0.0891***
					(0.0286)	(0.0288)	(0.0295)	(0.0289)
Observations	1983	1983	1603	1603	1606	1605	1605	1604
First-stage (Kleibergen-Paap) F-Statistic	17.04	17.35	17.01	17.20	15.46	16.38	15.43	16.24
Anderson-Rubin Wald-Test (p-val.)	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
State FE	yes	yes	yes	yes	yes	yes	yes	yes
Initial Controls	yes	yes	yes	yes	yes	yes	yes	yes
Population Shares	yes	yes	yes	yes	yes	yes	yes	yes
Population Growth	yes	yes	yes	yes	yes	yes	yes	yes

Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1. In columns (1)-(8) the method of estimation is two-stage least squares. The instrumental variable for the polarization and fractionalization index is the supply-push component of immigrant inflows, respectively; also see Section 4 for further details. The instrumental variable for past output growth is 1860 output per capita in columns (1)-(4) and 1850 output per capita in columns (5)-(8). Initial control variables (1870) are the urbanization rate, land concentration, the manufacturing share, population size, labor participation rate, counties' rail access, the share of native-born white and the share of African-Americans (estimates not reported in the table).

Table 7: Different Time Periods 1880-1920

	Dependent Variable: Δln(Output p.c.)				
	LS	IV (SP)	IV (Initial)	IV (SP and Initial)	IV (SP and Initial)
ΔPOL	-1.098**	-2.753**	-5.208***	-3.512***	-3.491***
	(0.437)	(1.085)	(1.771)	(1.055)	(1.057)
$\Delta FRAC$	1.268*	2.636	6.728**	3.833**	3.801**
	(0.669)	(1.863)	(3.258)	(1.880)	(1.877)
Output Growth 1850-1880					0.0304
					(0.0212)
Observations	1607	1607	1607	1607	1607
R^2	0.405	-	-	-	-
First-stage (Kleibergen-Paap) F-Statistic	-	28.69	9.73	21.18	20.58
Hansen-J statistic (p-val.)	-	n.a.	n.a.	0.21	0.20
Anderson-Rubin Wald-Test (p-val.)	-	0.00	0.00	0.00	0.00
State FE	yes	yes	yes	yes	yes
Initial Controls	yes	yes	yes	yes	yes
Population Shares	yes	yes	yes	yes	yes
Population Growth	yes	yes	yes	yes	yes

Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1. In column (1) the method of estimation is least squares. In columns (2)-(5) the method of estimation is two-stage least squares. In column (2), the instrumental variable is the supply-push component of immigrant inflows, IV (Supply Push); also see Section 4 for further details. In column (3) the instrumental variables are the initial polarization and fractionalization index (IV (Initial)). In columns (4)-(5), the instrumental variables are the supply-push component of immigrant inflows and the initial polarization and fractionalization index (IV (Supply Push and Initial)). The instrumental variable for past output growth is 1850 output per capita in column (5). Initial control variables (1880) are output per capita (in columns (1)-(4)), the urbanization rate, land concentration, the manufacturing share, population size, the share of native-born white and the share of African-Americans (estimates not reported in the table).

Table 8: Different Time Periods 1890-1920

	Dependent Variable: Δln(Output p.c.)				
	LS	IV (SP)	IV (Initial)	IV (SP and Initial)	IV (SP and Initial)
ΔPOL	-1.709***	-2.575***	-3.697***	-2.942***	-2.544***
	(0.355)	(0.617)	(0.804)	(0.561)	(0.626)
$\Delta FRAC$	2.372***	2.070*	4.080**	2.708**	2.166*
	(0.683)	(1.090)	(1.757)	(1.140)	(1.180)
Output Growth 1860-1890					0.0985***
					(0.0344)
Observations	1970	1970	1970	1970	1970
R^2	0.478	-	-	-	-
First-stage (Kleibergen-Paap) F-Statistic	-	39.99	57.15	29.79	23.61
Hansen-J statistic (p-val.)	-	n.a.	n.a.	0.33	0.68
Anderson-Rubin Wald-Test (p-val.)	-	0.00	0.00	0.00	0.00
State FE	yes	yes	yes	yes	yes
Initial Controls	yes	yes	yes	yes	yes
Population Shares	yes	yes	yes	yes	yes
Population Growth	yes	yes	yes	yes	yes

Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1. In column (1) the method of estimation is least squares. In columns (2)-(5) the method of estimation is two-stage least squares. In column (2), the instrumental variable is the supply-push component of immigrant inflows, IV (Supply Push); also see Section 4 for further details. In column (3) the instrumental variables are the initial polarization and fractionalization index (IV (Initial)). In columns (4)-(5), the instrumental variables are the supply-push component of immigrant inflows and the initial polarization and fractionalization index (IV (Supply Push and Initial)). The instrumental variable for past output growth is 1860 output per capita in column (5). Initial control variables (1890) are output per capita (in columns (1)-(4)), the urbanization rate, land concentration, the manufacturing share, population size, the share of native-born white and the share of African-Americans (estimates not reported in the table).

Table 9: Different Time Periods 1900-1920

	Dependent Variable: Δln(Output p.c.)				
	LS	IV (SP)	IV (Initial)	IV (SP and Initial)	IV (SP and Initial)
ΔPOL	-1.505***	-4.998***	-4.387**	-4.719***	-4.341***
	(0.483)	(1.172)	(1.706)	(1.282)	(1.383)
$\Delta FRAC$	1.822**	6.823***	5.961	6.659**	6.295**
	(0.818)	(2.292)	(3.688)	(2.748)	(2.877)
Output Growth 1870-1900					-0.0437
					(0.0328)
Observations	2221	2221	2221	2221	2221
R^2	0.343	-	-	-	-
First-stage (Kleibergen-Paap) F-Statistic	-	30.35	34.65	35.64	29.41
Hansen-J statistic (p-val.)	-	n.a.	n.a.	0.47	0.14
Anderson-Rubin Wald-Test (p-val.)	-	0.00	0.00	0.00	0.00
State FE	yes	yes	yes	yes	yes
Initial Controls	yes	yes	yes	yes	yes
Population Shares	yes	yes	yes	yes	yes
Population Growth	yes	yes	yes	yes	yes

Huber robust standard errors (shown in parentheses) are clustered at the state level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. In column (1) the method of estimation is least squares. In columns (2)-(5) the method of estimation is two-stage least squares. In column (2), the instrumental variable is the supply-push component of immigrant inflows, IV (Supply Push); also see Section 4 for further details. In column (3) the instrumental variables are the initial polarization and fractionalization index (IV (Initial)). In columns (4)-(5), the instrumental variables are the supply-push component of immigrant inflows and the initial polarization and fractionalization index (IV (Supply Push and Initial)). The instrumental variable for past output growth is 1870 output per capita in column (5). Initial control variables (1900) are output per capita (in columns (1)-(4)), the urbanization rate, land concentration, the manufacturing share, population size, the share of native-born white and the share of African-Americans (estimates not reported in the table).

Table 10: Different Time Periods 1910-1920

	Dependent Variable: Δln(Output p.c.)				
	LS	IV (SP)	IV (Initial)	IV (SP and Initial)	IV (SP and Initial)
ΔPOL	-0.0465	-1.810***	-4.117***	-2.360***	-1.899***
	(0.851)	(0.691)	(1.551)	(0.786)	(0.699)
$\Delta FRAC$	-0.139	2.604*	7.161*	3.555*	3.045*
	(0.787)	(1.500)	(3.890)	(2.032)	(1.824)
Output Growth 1880-1910					0.0361
					(0.0272)
Observations	2401	2401	2401	2401	2401
R^2	0.460	-	-	-	-
First-stage (Kleibergen-Paap) F-Statistic	-	34.43	25.78	45.23	37.46
Hansen-J statistic (p-val.)	-	n.a.	n.a.	0.16	0.28
Anderson-Rubin Wald-Test (p-val.)	-	0.03	0.00	0.02	0.00
Initial Controls	yes	yes	yes	yes	yes
Population Shares	yes	yes	yes	yes	yes
Population Growth	yes	yes	yes	yes	yes

Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1. In column (1) the method of estimation is least squares. In columns (2)-(5) the method of estimation is two-stage least squares. In column (2), the instrumental variable is the supply-push component of immigrant inflows, IV (Supply Push); also see Section 4 for further details. In column (3) the instrumental variables are the initial polarization and fractionalization index (IV (Initial)). In columns (4)-(5), the instrumental variables are the supply-push component of immigrant inflows and the initial polarization and fractionalization index (IV (Supply Push and Initial)). The instrumental variable for past output growth is 1880 output per capita in column (5). Initial control variables (1910) are output per capita (in columns (1)-(4)), the urbanization rate, land concentration, the manufacturing share, population size, the share of native-born white and the share of African-Americans (estimates not reported in the table).

Table 11: Panel Estimation 1870-1920

	Dependent Variable: Δln(Output p.c.)			
	LS	LS	LS	IV (SP)
ΔPOL	-0.770***	-0.715***	-0.677***	-1.309**
	(0.238)	(0.240)	(0.245)	(0.539)
$\Delta FRAC$	1.216***	1.206***	1.268***	1.598**
	(0.369)	(0.377)	(0.387)	(0.777)
Lagged Output Growth				0.111***
				(0.0249)
Observations	11748	11748	11748	11748
R^2	0.672	0.683	0.687	-
First-stage (Kleibergen-Paap) F-Statistic	-	-	-	23.93
Anderson-Rubin Wald-Test (p-val.)	-	-	-	0.00
State $\times$ Year FE	yes	yes	yes	yes
Lagged Controls	no	yes	yes	yes
Lagged Population Shares	no	no	yes	yes
Population Growth	no	no	yes	yes

Huber robust standard errors (shown in parentheses) are clustered at the county level: *** p<0.01, ** p<0.05, * p<0.1. In columns (1)-(3) the method of estimation is least squares. In column (4) the method of estimation is two-stage least squares. The instrumental variable is the supply-push component of immigrant inflows, IV (Supply Push); also see Section 4 for further details. In column (4) the instrumental variable for lagged output growth is the second lag of output per capita. Lagged control variables are output per capita, the urbanization rate, land concentration, the manufacturing share, population size, the share of native-born white and the share of African-Americans (estimates not reported in the table).

Table 12: Controlling for Population Share of American Indians

	Δln(Output p.c.)			
	LS	IV (SP)	IV (Initial)	IV (SP and Initial)
ΔPOL	-1.055***	-2.672***	-2.332***	-2.206***
	(0.325)	(0.545)	(0.453)	(0.423)
$\Delta FRAC$	1.024**	1.957**	0.810	1.195
	(0.492)	(0.820)	(1.191)	(0.890)
Observations	2160	2160	2160	2160
R^2	0.564	-	-	-
First-stage (Kleibergen-Paap) F-Statistic	-	28.00	12.94	17.20
Hansen-J statistic (p-val.)	-	n.a.	n.a.	0.31
Anderson-Rubin Wald-Test (p-val.)	-	0.00	0.00	0.00
State FE	yes	yes	yes	yes
Initial Controls	yes	yes	yes	yes
Population Shares	yes	yes	yes	yes
Population Growth	yes	yes	yes	yes

Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1. In column (1) the method of estimation is least squares. In columns (2)-(4) the method of estimation is two-stage least squares. In column (2), the instrumental variable is the supply-push component of immigrant inflows, IV (Supply Push); also see Section 4 for further details. In column (3) the instrumental variables are the initial 1870 polarization and fractionalization index (IV (Initial)). In column (4), the instrumental variables are the supply-push component of immigrant inflows and the initial 1870 polarization and fractionalization index (IV (Supply Push and Initial)). Initial control variables (1870) are output per capita, the urbanization rate, land concentration, the manufacturing share, population size, labor participation rate, counties' rail access, the share of native-born white and the share of African-Americans (estimates not reported in the table).

Table 13: Excluding the South

	Δln(Output p.c.)			
	LS	IV (SP)	IV (Initial)	IV (SP and Initial)
ΔPOL	-1.532***	-2.191***	-1.944***	-2.008***
	(0.313)	(0.421)	(0.406)	(0.402)
$\Delta FRAC$	1.707***	0.673	-0.222	0.140
	(0.509)	(0.962)	(0.926)	(0.886)
Observations	1125	1125	1125	1125
R^2	0.614	-	-	-
First-stage (Kleibergen-Paap) F-Statistic	-	20.99	34.95	18.01
Hansen-J statistic (p-val.)	-	n.a.	n.a.	0.30
Anderson-Rubin Wald-Test (p-val.)	-	0.00	0.00	0.00
State FE	yes	yes	yes	yes
Initial Controls	yes	yes	yes	yes
Population Shares	yes	yes	yes	yes
Population Growth	yes	yes	yes	yes

Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1. In column (1) the method of estimation is least squares. In columns (2)-(4) the method of estimation is two-stage least squares. In column (2), the instrumental variable is the supply-push component of immigrant inflows, IV (Supply Push); also see Section 4 for further details. In column (3) the instrumental variables are the initial 1870 polarization and fractionalization index (IV (Initial)). In column (4), the instrumental variables are the supply-push component of immigrant inflows and the initial 1870 polarization and fractionalization index (IV (Supply Push and Initial)). Initial control variables (1870) are output per capita, the urbanization rate, land concentration, the manufacturing share, population size, labor participation rate, counties' rail access, the share of native-born white and the share of African-Americans (estimates not reported in the table).

Table 14: Controlling for a Quadratic Fractionalization Term

	Δln(Output p.c.)							
	LS	IV (SP)	IV (Initial)	IV (SP and Initial)	LS	IV (SP)	IV (Initial)	IV (SP and Initial)
ΔPOL	-0.440	-2.342**	-1.916**	-1.455**				
	(0.324)	(0.988)	(0.953)	(0.738)				
$\Delta FRAC$	-1.153	0.564	-0.893	-1.497	-2.146***	-5.403***	-5.895***	-5.010***
	(0.832)	(2.650)	(2.628)	(2.120)	(0.456)	(0.933)	(0.964)	(0.894)
$\Delta FRAC^2$	2.095***	1.344	1.374	2.315	2.628***	5.443***	4.005***	4.564***
	(0.695)	(2.084)	(2.304)	(1.752)	(0.626)	(1.018)	(1.443)	(1.124)
Observations	2160	2160	2160	2160	2160	2160	2160	2160
R^2	0.569	-	-	-	0.568	-	-	-
First-stage (Kleibergen-Paap) F-Statistic	-	23.50	16.05	13.00	-	34.98	17.41	19.08
Anderson-Rubin Wald-Test (p-val.)	-	0.00	0.00	0.00	-	0.00	0.00	0.00
State FE	yes	yes	yes	yes	yes	yes	yes	yes
Initial Controls	yes	yes	yes	yes	yes	yes	yes	yes
Population Shares	yes	yes	yes	yes	yes	yes	yes	yes
Population Growth	yes	yes	yes	yes	yes	yes	yes	yes

Huber robust standard errors (shown in parentheses) are clustered at the state level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. In columns (1) and (5) the method of estimation is least squares. In columns (2)-(4) and (6)-(8) the method of estimation is two-stage least squares. In columns (2) and (6) the instrumental variable is the supply-push component of immigrant inflows (IV (SP)); also see Section 4 for further details. In columns (3) and (7) the instrumental variables are the initial 1870 polarization and fractionalization index (IV (Initial)). In columns (4) and (8), the instrumental variables are the supply-push component of immigrant inflows and the initial 1870 polarization and fractionalization index (IV (SP and Initial)). Initial control variables (1870) are output per capita, the urbanization rate, land concentration, the manufacturing share, population size, labor participation rate, counties' rail access, the share of native-born white and the share of African-Americans (estimates not reported in the table).

Table 15: Using the Change in Control Variables

	Δln(Output p.c.)			
	(1) LS	(2) IV (SP)	(3) IV (Initial)	(4) IV (SP and Initial)
ΔPOL	-1.110***	-2.292***	-1.834***	-1.701***
	(0.260)	(0.561)	(0.366)	(0.352)
$\Delta FRAC$	1.158**	2.062***	0.530	0.928
	(0.491)	(0.719)	(1.063)	(0.835)
Observations	2109	2109	2109	2109
R^2	0.642	-	-	-
Kleibergen-Paap F-Statistic	-	19.49	10.51	15.78
Hansen-J statistic (p-val.)	-	n.a.	n.a.	0.20
Anderson-Rubin Wald-Test (p-val.)	-	0.00	0.00	0.00
State FE	yes	yes	yes	yes
Initial Controls	yes	yes	yes	yes
Change Controls	yes	yes	yes	yes
Population Shares	yes	yes	yes	yes
Change Population Shares	yes	yes	yes	yes

Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1. In column (1) the method of estimation is least squares. In columns (2)-(4) the method of estimation is two-stage least squares. In column (2) the instrumental variable is the supply-push component of immigrant inflows (IV (A)); also see Section 4 for further details. In column (3) the instrumental variables are the initial 1870 polarization and fractionalization index (IV (B)). In column (4), the instrumental variables are the supply-push component of immigrant inflows and the initial 1870 polarization and fractionalization index (IV (C)). Initial control variables (1870) are output per capita, the urbanization rate, land concentration, the manufacturing share, population size, labor participation rate, counties' rail access, the share of native-born white and the share of Afro-Americans (estimates not reported in the table). The change of control variables (1870-1920) included are the urbanization rate, land concentration, the manufacturing share, population size, labor participation rate, the share of native-born white and the share of African-Americans (estimates not reported in the table).

Table 16: Sample Split: Below vs. Above Median Output per Capita

	Above Median Output p.c.				Below Median Output p.c.			
	LS	IV (SP)	IV (Initial)	IV (SP and Initial)	LS	IV (SP)	IV (Initial)	IV (SP and Initial)
ΔPOL	-1.744***	-2.669***	-2.233***	-2.301***	-0.171	-2.622***	-2.565***	-2.318***
	(0.417)	(0.697)	(0.525)	(0.571)	(0.441)	(0.635)	(0.615)	(0.600)
$\Delta FRAC$	2.355***	2.346*	1.287	1.693	-0.676	2.089**	1.393	1.368
	(0.591)	(1.327)	(1.335)	(1.215)	(0.692)	(0.913)	(1.789)	(1.336)
Observations	1080	1080	1080	1080	1080	1080	1080	1080
R^2	0.537	-	-	-	0.576	-	-	-
First-stage (Kleibergen-Paap) F-Statistic	-	21.01	9.57	14.41	-	24.96	16.48	15.48
Anderson-Rubin Wald-Test (p-val.)	-	0.00	0.00	0.00	-	0.00	0.00	0.00
State FE	yes	yes	yes	yes	yes	yes	yes	yes
Initial Controls	yes	yes	yes	yes	yes	yes	yes	yes
Population Shares	yes	yes	yes	yes	yes	yes	yes	yes
Population Growth	yes	yes	yes	yes	yes	yes	yes	yes

Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1. In columns (1) and (5) the method of estimation is least squares. In columns (2)-(4) and (6)-(8) the method of estimation is two-stage least squares. In columns (2) and (6) the instrumental variable is the supply-push component of immigrant inflows (IV (SP)); also see Section 4 for further details. In columns (3) and (7) the instrumental variables are the initial 1870 polarization and fractionalization index (IV (Initial)). In columns (4) and (8), the instrumental variables are the supply-push component of immigrant inflows and the initial 1870 polarization and fractionalization index (IV (SP and Initial)). Initial control variables (1870) are output per capita, the urbanization rate, land concentration, the manufacturing share, population size, labor participation rate, counties' rail access, the share of native-born white and the share of African-Americans (estimates not reported in the table).

Table 17: Excluding Top and Bottom 1 Percentile of Changes in Fractionalization and Polarization

	LS	IV (SP)	IV (Initial)	IV (SP and Initial)
	PANEL A: Between 1 and 99 Percentile Δ FRAC Index			
	Dependent Variable: Δln(Output p.c.)			
ΔPOL	-0.967***	-2.720***	-2.382***	-2.186***
	(0.313)	(0.573)	(0.495)	(0.452)
$\Delta FRAC$	0.797	1.773**	0.491	0.986
	(0.500)	(0.838)	(1.263)	(0.904)
Observations	2126	2126	2126	2126
R^2	0.573	-	-	-
First-stage (Kleibergen-Paap) F-Statistic	-	26.09	10.88	18.21
Hansen-J statistic (p-val.)	-	n.a.	n.a.	0.17
Anderson-Rubin Wald-Test (p-val.)	-	0.00	0.00	0.00
	PANEL B: Between 1 and 99 Percentile Δ POL Index			
ΔPOL	-1.142***	-3.012***	-2.651***	-2.405***
	(0.316)	(0.615)	(0.476)	(0.437)
$\Delta FRAC$	1.119**	2.388***	1.066	1.509*
	(0.496)	(0.863)	(1.179)	(0.863)
Observations	2124	2124	2124	2124
R^2	0.563	-	-	-
First-stage (Kleibergen-Paap) F-Statistic	-	24.42	10.49	17.29
Hansen-J statistic (p-val.)	-	n.a.	n.a.	0.21
Anderson-Rubin Wald-Test (p-val.)	-	0.00	0.00	0.00
	PANEL C: Between 1 and 99 Percentile Output Growth			
ΔPOL	-0.940***	-2.244***	-1.985***	-1.797***
	(0.308)	(0.538)	(0.435)	(0.394)
$\Delta FRAC$	0.892*	1.623**	0.589	0.855
	(0.460)	(0.734)	(1.056)	(0.844)
Observations	2128	2128	2128	2128
R^2	0.531	-	-	-
First-stage (Kleibergen-Paap) F-Statistic	-	24.50	12.52	14.00
Hansen-J statistic (p-val.)	-	n.a.	n.a.	0.13
Anderson-Rubin Wald-Test (p-val.)	-	0.00	0.00	0.00

Huber robust standard errors (shown in parentheses) are clustered at the state level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. In column (1) the method of estimation is least squares. In columns (2)-(4) the method of estimation is two-stage least squares. In column (2) the instrumental variable is the supply-push component of immigrant inflows (IV (SP)); also see Section 4 for further details. In column (3) the instrumental variables are the initial 1870 polarization and fractionalization index (IV (Initial)). In column (4) the instrumental variables are the supply-push component of immigrant inflows and the initial 1870 polarization and fractionalization index (IV (SP and Initial)). Initial control variables (1870) are output per capita, the urbanization rate, land concentration, the manufacturing share, population size, labor participation rate, counties' rail access, the share of native-born white and the share of African-Americans. We further include state fixed effects, population growth and population shares as additional control variables (estimates not reported in the table).

Table 18: Excluding Top and Bottom 1 Percentile of the Level of Fractionalization and Polarization

	LS	IV (SP)	IV (Initial)	IV (SP and Initial)
	PANEL A: Between 1 and 99 Percentile FRAC Index			
	Dependent Variable: Δln(Output p.c.)			
ΔPOL	-1.022***	-2.508***	-2.224***	-2.110***
	(0.334)	(0.581)	(0.463)	(0.433)
$\Delta FRAC$	0.979*	1.855**	0.883	1.194
	(0.504)	(0.844)	(1.217)	(0.936)
Observations	2041	2041	2041	2041
R^2	0.569	-	-	-
First-stage (Kleibergen-Paap) F-Statistic	-	30.96	14.01	19.81
Hansen-J statistic (p-val.)	-	n.a.	n.a.	0.45
Anderson-Rubin Wald-Test (p-val.)	-	0.00	0.00	0.00
	PANEL B: Between 1 and 99 Percentile POL Index			
ΔPOL	-1.084***	-2.612***	-2.299***	-2.202***
	(0.320)	(0.544)	(0.446)	(0.420)
$\Delta FRAC$	1.058**	1.970**	0.934	1.271
	(0.484)	(0.820)	(1.160)	(0.881)
Observations	2115	2115	2115	2115
R^2	0.564	-	-	-
First-stage (Kleibergen-Paap) F-Statistic	-	32.65	15.29	19.27
Hansen-J statistic (p-val.)	-	n.a.	n.a.	0.38
Anderson-Rubin Wald-Test (p-val.)	-	0.00	0.00	0.00
	PANEL C: Between 1 and 99 Percentile POL and FRAC Index			
ΔPOL	-1.041***	-2.454***	-2.183***	-2.094***
	(0.331)	(0.566)	(0.451)	(0.427)
$\Delta FRAC$	0.997*	1.840**	0.938	1.217
	(0.502)	(0.838)	(1.198)	(0.932)
Observations	2010	2010	2010	2010
R^2	0.569	-	-	-
First-stage (Kleibergen-Paap) F-Statistic	-	34.44	16.08	20.66
Hansen-J statistic (p-val.)	-	n.a.	n.a.	0.49
Anderson-Rubin Wald-Test (p-val.)	-	0.00	0.00	0.00

Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1. In column (1) the method of estimation is least squares. In columns (2)-(4) the method of estimation is two-stage least squares. In column (2) the instrumental variable is the supply-push component of immigrant inflows (IV (SP)); also see Section 4 for further details. In column (3) the instrumental variables are the initial 1870 polarization and fractionalization index (IV (Initial)). In column (4) the instrumental variables are the supply-push component of immigrant inflows and the initial 1870 polarization and fractionalization index (IV (SP and Initial)). Initial control variables (1870) are output per capita, the urbanization rate, land concentration, the manufacturing share, population size, labor participation rate, counties' rail access, the share of native-born white and the share of African-Americans. We further include state fixed effects, population growth and population shares as additional control variables (estimates not reported in the table).

Table 19: Pol and Frac Index with Linguistic Distances (LD), $\delta = 0.05$

	Δln(Output p.c.)			
	LS	IV (SP)	IV (Initial)	IV (SP and Initial)
ΔPOL_{LD}	-148.0***	-212.3***	-204.1***	-219.0***
	(37.81)	(58.77)	(57.78)	(58.19)
$\Delta FRAC_{LD}$	50.33***	59.41***	50.64***	68.93***
	(15.74)	(18.85)	(19.57)	(19.97)
Observations	2161	2161	2161	2161
R^2	0.552	-	-	-
First-stage (Kleibergen-Paap) F-Statistic	-	21.47	13.17	8.70
Hansen-J statistic (p-val.)	-	n.a.	n.a.	0.12
Anderson-Rubin Wald-Test (p-val.)	-	0.00	0.00	0.00
State FE	yes	yes	yes	yes
Initial Controls	yes	yes	yes	yes
Population Shares	yes	yes	yes	yes
Population Growth	yes	yes	yes	yes

Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1. In column (1) the method of estimation is least squares. In columns (2)-(4) the method of estimation is two-stage least squares. In column (2) the instrumental variable is the supply-push component of immigrant inflows (IV (Supply Push)); also see Section 4 for further details. In column (3) the instrumental variables are the initial 1870 polarization and fractionalization index (IV (Initial)). In column (4) the instrumental variables are the supply-push component of immigrant inflows and the initial 1870 polarization and fractionalization index (IV (Supply Push and Initial)). Initial control variables (1870) are the urbanization share, output per capita, land concentration, the manufacturing share, population size, labor participation rate, counties' rail access, the share of native-born white and the share of African-Americans (estimates not reported in the table). We construct our measure of fractionalization with distances following Greenberg (1956) as: $FRAC_{LD} = \sum_{i=1}^{N}\sum_{j=1}^{N}\pi_i\pi_j d_{ij}$, where subindices for counties and states are left out for simplicity. The corresponding measure of polarization follows Esteban and Ray (1994, 1999) and is constructed as: $POL_{LD} = \sum_{i=1}^{N}\sum_{j=1}^{N}\pi_i^2\pi_j d_{ij}$. Both indices include a measure of inter-group distances d_{ij}. To proxy for inter-group distances, we follow Fearon (2003), Desmet et al. (2009a) and Esteban et al. (2010) and use information on linguistic groups compiled by the Ethnologue project to construct a measure of linguistic distances between any two groups as $d_{ij} = 1 - b_{ij}^{\delta}$ (See http://www.ethnologue.com, Desmet et al. (2009a) and Esteban et al. (2010) for further information on the Ethnologue project). The parameter b_{ij} is the ratio of the number of shared branches between i and j to the maximum number of branches between any two languages and $\delta \epsilon (0, 1]$ represents a sensitivity parameter determining how fast the distance declines as the number of shared branches increases (see Desmet et al., 2009a) (We used the language trees reported by Ethnologue to construct the parameter b_{ij}. See also Desmet et al. (2009b) for a detailed discussion on the construction of such a language tree). The abbreviation LD denotes the use of language distances for the metric d_{ij}. We use the representative language of each country of origin to construct the linguistic distances. Thus, if two groups speak the same representative language we set $b_{ij} = 1$. We compute linguistic distance as Desmet et al. (2009a) using $\delta = 0.05$.

Descriptive Statistics

Table 20: Average Population Shares: Years 1870, 1920

Year	Share 1	Share 2	Share 3	Share 4	Share 5	Share 6	Share 7
1870	.0022978	.0015995	.0013146	.014865	.0432896	.0131676	.0022796
1920	.005545	.0017838	.0062861	.0139002	.0136935	.0161658	.0008786

Year	Share 8	Share 9	Share 10	Share 11	Share 12	Share 13	Share 14
1870	.0276525	.	.0233431	.0005533	.0012711	.0001813	.0002078
1920	.0121973	.0008646	.003241	.0050011	.0020155	.0029711	.0065188

Year	Share 15	Share 16	Share 17	Share 18	Share 19	Share 20	Share 21
1870	.0005284	.001831	.0163596	.0480108	.0057655	.1503518	.7333189
1920	.001228	.0011946	.0064377	n.a.	.0096398	.1165702	.80195

Share 1: Austro-Hungarian; Share 2: Benelux; Share 3: East Europe; Share 4: Canada; Share 5: Central and South America; Share 6: Scandinavia; Share 7: France; Share 8: Germany; Share 9: Greece; Share 10: Ireland; Share 11: Italy; Share 12: Pacific; Share 13: Poland; Share 14: Portugal; Share 15: Spain; Share 16: Switzerland; Share 17: United Kingdom (England, Wales and Scotland); Share 18: Asia; Share 19: Others; Share 20: Afro-Americans and Share 21: White US Native Born.

Table 21: Average Main Occupation Shares: Years 1870, 1920

Year	Share 1	Share 2	Share 3	Share 4	Share 5	Share 6	Share 7
1870	.0269179	.3906677	.0314494	.0049546	.0120077	.0764527	.0723302
1920	.0479377	.3469686	.0539587	.0358102	.0316605	.087921	.0852661

Year	Share 8	Share 9	Share 10	Share 11
1870	.0451425	.0099688	.2524161	.0801304
1920	.0293113	.0256638	.1559264	.0998232

Share 1: Professionals; Share 2: Farmers; Share 3: Managers; Share 4: Clerical; Share 5: Sales Workers; Share 6: Craftsmen; Share 7: Operatives; Share 8: Pr. Household; Share 9: Service; Share 10: Farm Laborers; Share 11: Other Laborers.

Table 22: Average Main Industry Shares: Years 1870, 1920

Year	Share 1	Share 2	Share 3	Share 4	Share 5	Share 6	Share 7
1870	.7032434	.1542317	.0714427	.1141836	.0759488	.0861482	.019187
1920	.5209154	.1082717	.0499704	.1500325	.0839766	.0922905	.0267744

Year	Share 8	Share 9	Share 10	Share 11
1870	.0126641	.1089207	.0522843	.0606334
1920	.0150387	.084784	.0570265	.0375212

Share 1: Agriculture; Share 2: Mining; Share 3: Construction; Share 4: Manufacturing; Share 5: Transportation & Communication; Share 6: Trade; Share 7: Finance; Share 8: Business Service; Share 9: Service (other); Share 10: Professional Service; Share 11: Public Administration.

Graphs

Figure 1: Kernel Density Estimation: Change of Fractionalization, 1870 - 1920

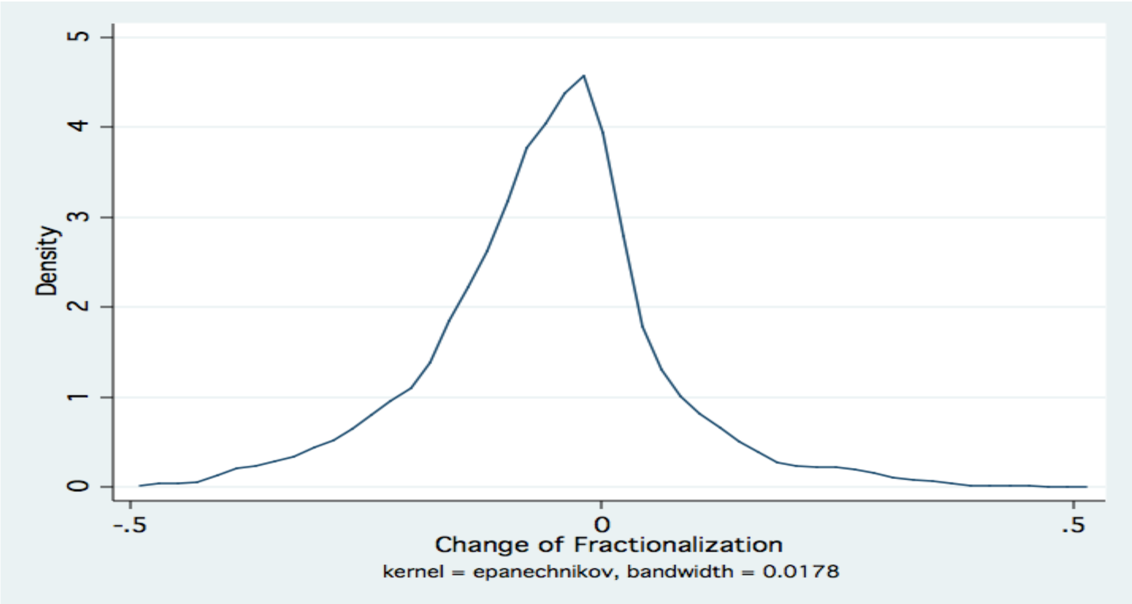
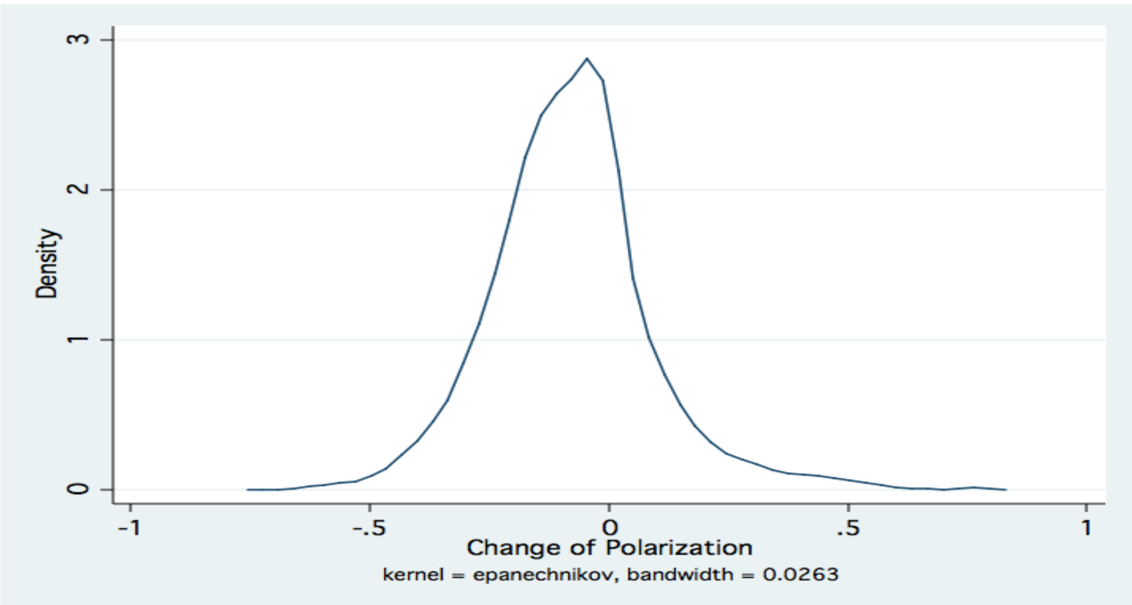


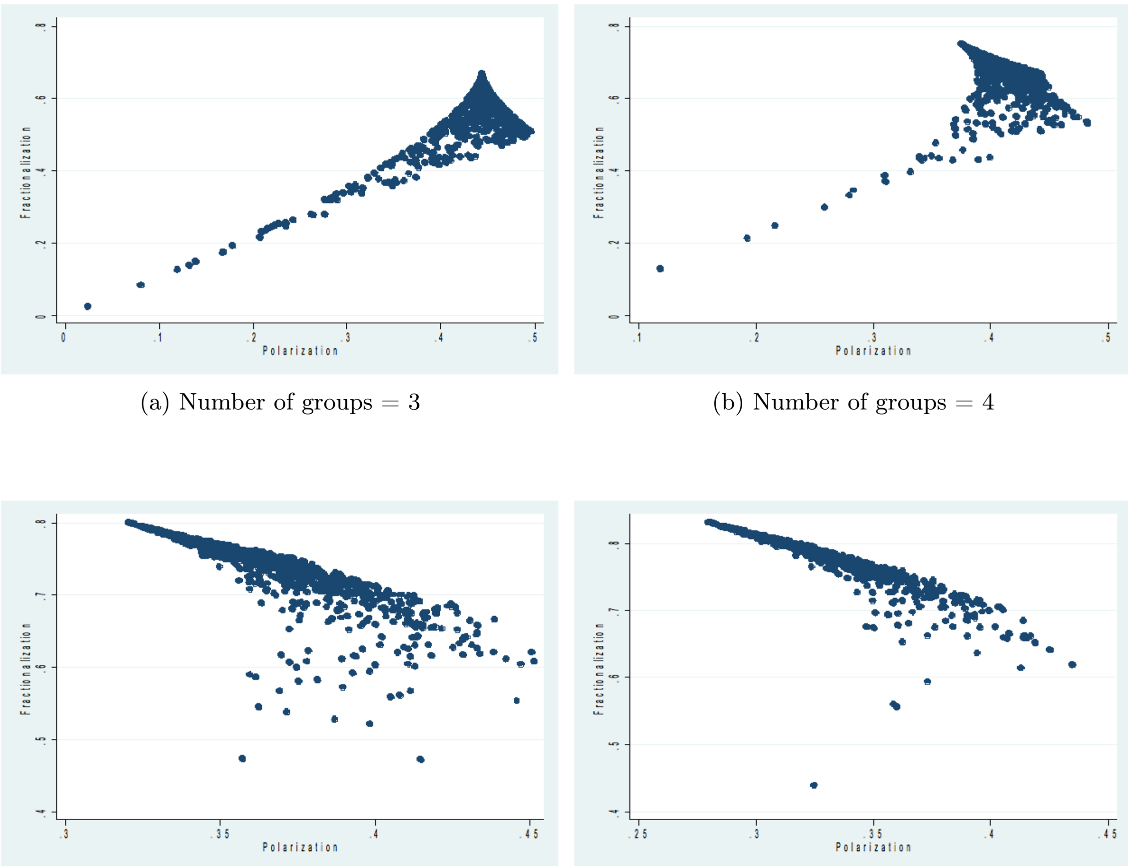
Figure 2: Kernel Density Estimation: Change of Polarization, 1870 - 1920



Simulations

To illustrate and provide some intuition for the relationship between the fractionalization and polarization index, we simulated data on the population share of n groups by drawing n times from a uniform [0,1] distribution and dividing each draw by the total size of the realizations drawn. We repeated this 1000 times, constructing at each replication the fractionalization and polarization index. Appendix Figure 3 shows the results for $n = 3, 4, 5, 6$. The key message is that, when the number of groups is small the polarization and fractionalization index are positively correlated and as the number of groups increase the correlation turns negative. We do not show the results for $n = 2$ because in this case the fractionalization and polarization index are exactly the same (see Montalvo and Reynal-Querol (2005a,b)).

Figure 3: Simulation: Fractionalization (y-axis) vs. Polarization (x-axis)



(a) Number of groups = 3

(b) Number of groups = 4

(c) Number of groups = 5

(d) Number of groups = 6

Data Sources and Variable Description

Our main data source is the Inter-University Consortium for Political and Social Research (ICPSR) 2896 data file. The ICPSR 2896 data file contains detailed decennial US county and state level data on demographic, economic, and social variables which were collected by the US Bureau of Census for the period 1790-2000. More information about the data set (i.e. scope of study, data collection and data source) can be found at http://www.icpsr.umich.edu/icpsrweb/ICPSR/studies/02896. One key advantage of the ICPSR data set is that it enables us to exploit the underlying cultural heterogeneity in the United States at the county level. In particular, the ICPSR data set comprises – from 1870 onwards – detailed information about the country of origin of foreign-born, white US-born citizens and African-Americans which is necessary to calculate the cultural diversity indices described in Section 3.

As a further database, we use the Integrated Public Use Microdata Series (IPUMS-USA). The IPUMS is a public project and data are freely available. For more information see: http://usa.ipums.org/usa/index.shtml. The IPUMS gives us the possibility to exploit individual level data and construct aggregate data at the county level whenever these variables are missing in the ICPSR data set, but available at the IPUMS. We used the IPUMS database to construct the fractionalization index of occupations, the industry diversity index and the share of public sector employment (see Section 5). We use the IPUMS benchmark occupation classification variable *occ1950* and the benchmark industry classification variable *ind1950* to construct the fractionalization index of occupations and the industry diversity index, respectively.The occupation classification code of 1950 assigns an occupation code to the individual's reported occupation and is a reference for all Census occupation data available at the IPUMS. Census years with different occupation coding schemes (as the 1870 and 1920 Census) are converted by the IPUMS into the occupation classification of 1950 (i.e. the variable *occ1950*) to make occupations over time comparable. The same applies to the IPUMS industrial classification variable *ind1950*, which contains coded information about the industry an individual worked in. For more information see IPUMS at http://usa.ipums.org/usa/index.shtml. In the supplementary online appendix we provide a detailed description of the variables used in our empirical analysis (if not further specified, variables are selected from the ICPSR 2896 data file).

Dependent Variables

VARIABLE	YEARS	DESCRIPTION
Output growth	1870 - 1920	Total output in per capita terms is formed as the sum of manufacturing value added and agricultural value added. The growth variable is calculated as the change in logarithmic units.
Manufacturing Value Added	1870 - 1920	We use manufacturing value added in per capita terms. We calculate manufacturing value added as the difference between manufacturing output and the cost of materials used in manufacturing. For 1910, there are no manufacturing Census data available at the county level.
Agricultural Value Added	1870 - 1920	Agricultural value added is in per capita terms. We calculate agricultural value added as the difference between agricultural output and the cost of inputs used in agriculture. We use the variable *farmout*, which contains the estimated value of farm products, as measure for agricultural output for the years 1870 - 1900. For 1910 - 1920 we use as agricultural output the sum of values of crops, value of dairy products, value of chickens and eggs produced, value of honey and wax produced and the value of wool produced. As a proxy of the input costs in agriculture, we use expenditure for fertilizer (available 1880 - 1920) and for feed (available 1910 - 1920). See the ICPSR 2896 codebook for more details.
Urban growth	1870 - 1920	Change in the population share living in urban counties. The Census declared a county population as urban, if at least 2500 inhabitants lived in urban places.
Population growth	1870 - 1920	Change in the county population over time. The growth variable is calculated as the change in logarithmic units.

Dependent Variables (CONTINUED)		
Change in Occupational Diversity Index	1870 - 1920	We take the occupation variable *occ1950* from the IPUMS to construct the occupational diversity index. See Section 5.2 for more details.
Change in Industry Diversity Index	1870 - 1920	We take the industry classification variable *ind1950* from the IPUMS to construct the industry diversity index. See Section 5.2 for more details.
Change in the Tax Ratio	1870 - 1920	Taxes collected by counties as a fraction of output per capita. The growth variable is calculated as the change in logarithmic units. See Rhode and Strumpf (2003) for more information.
Change in Public Sector Employment	1870 - 1920	We calculate public sector employment using the IPUMS industry classification variable (category: public administration) *ind1950*. Public sector employment is in per capita terms. The growth variable is calculated as the change in logarithmic units.

ADDITIONAL CONTROLS		
VARIABLE	YEARS	DESCRIPTON
Land Concentration	1870	Gini coefficient of farm size distribution, calculated as in Galor et al. (2009, p. 175).
Manufacturing Share	1870	Share of manufacturing output over the sum of manufacturing and agricultural output in 1870.
Labor Participation Rate	1870	Share of individuals in the labor force in 1870. The labor force classification and status is taken from the IPUMS.
Population	1870	Total population in US counties in 1870.
Urbanization	1870	Population share living in urban counties. The Census declared a county population as urban, if at least 2500 inhabitants lived in urban places in 1870.
Rail Access	1870	Indicator variable that is equal to one if a county has access to a railroad within its borders in 1870, and zero otherwise. See Atack et al. (2008) for more information on the construction of the railroad database.
Share of Native-Born White	1870	Share of native-born white out of the total population in 1870.
Share of Afro-Americans	1870	Share of Afro-Americans (declared by the historical US Census as negro population) out of the total population in 1870.