The Wild West *is* Wild: The Homicide Resource Curse

Mathieu Couttenier
Pauline Grosjean
Marc Sangnier

The Wild West *is* Wild: The Homicide Resource Curse*

Mathieu Couttenier† Pauline Grosjean‡ Marc Sangnier§

March 2014

Abstract

We uncover interpersonal violence as a dimension and a mechanism of the resource curse. We rely on a historical natural experiment in the United States, in which mineral discoveries occurred at various stages of governmental territorial expansion. "Early" mineral discoveries, before full-fledge rule of law is in place in a county, are associated with higher levels of interpersonal violence, both historically and today. The persistence of this homicide resource curse is partly explained by the low quality of (subsequent) judicial institutions. The specificity of our results to violent crime also suggests that a private order of property rights did emerge on the frontier, but that it was enforced through high levels of interpersonal violence. The results are robust to state-specific effects, to comparing only neighboring counties, and to comparing only discoveries within short time intervals of one another.

KEYWORDS: Homicide, Institutions, Resource curse, United States
JEL CODES: K42, N51, Z13

*We are grateful to Sam Bowles, Ann Carlos, Karen Clay, Paul Dower, Richard Holden, Suresh Naidu, Estefania Santacreu-Vasut, William Schworm, Gavin Wright, as well as participants to the Barcelona GSE Summer Forum 2013, ISNIE 2013, AEA meetings 2014, the 2014 Santa Fe Institute meeting on the co-evolution of behaviors and institutions and to seminars at ESSEC/THEMA, Goethe University in Frankfurt, Queensland University of Technology, Tilburg University, University of New South Wales, University of Otago, and University of Victoria for helpful comments and suggestions. We thank Dan Berkowitz and Karen Clay for sharing data with us. Mathieu Couttenier acknowledges financial support from the ERC Starting Grant GRIEVANCES-313327.

†University of Lausanne. Mail: mathieu.couttenier@unil.ch

‡University of New South Wales. Mail: p.grosjean@unsw.edu.au

§Aix-Marseille Univ.(Aix-Marseille School of Economics), CNRS & EHESS. Mail: marc.sangnier@univ-amu.fr

"It is manifest that during the time men live without a common power to keep them all in awe, they are in a condition which is called war; and such a war, as is of every man, against every man. [...] The nature of war consists not in actual fighting but in the known disposition thereto, during all the time there is no assurance to the contrary" (Hobbes 1651, page 262).

1 Introduction

Why is the homicide rate in the United States more than four times higher than in Western Europe or other neo-Europes, such as Australia?[1] Elias (1994)'s civilizing process links the decline in homicide to the development of a Weberian state, which enforces contracts and monopolizes violence, making violence both superfluous and ineffective.[2] In sharp contrast with this view, others argue that self-interest suffices to sustain a peaceful order, even in anarchy. This is demonstrated, according to Anderson and Hill (2004) in "*Not So* Wild Wild West", by the private enforcement of property rights in the West of the United States in the 19$^{\text{th}}$ century. A closer look at the evidence in this study reveals that while a private order of property rights order did exist, it was not peaceful.

The circumstances of mineral discoveries in the United States provide the ideal natural experiment to examine the relationship between state development and interpersonal violence. Mineral discoveries generally occurred after the state was established, but they preceded the state in more than a third of cases. Incorporation of large swathes of territory in the West took place at the same time as an intense mineral rush, but for independent, geopolitical reasons according to Davis (1972). Motivated by the fact that in the United States, contrary to most countries, individuals own mineral rights, private prospectors flocked to the West, irrespective of whether the state was already in place or not.[3]

[1] This comparison is based on UNODC data for homicide rates per 100,000 people in 2010: United States: 4.7, United Kingdom: 1.6, France: 1.1, Germany: 0.8, Australia: 1.

[2] Pinker (2007) observes that homicide has been falling steadily in Europe since the emergence of modern states in the Middle Ages.

[3] Lang (1983) (first published in 1882) witnessed how: "*Within the short space of half a year from the discovery of gold in California, extensive prospecting had been done, and the gold-bearing*

In this study, we take advantage of the respective timings of state development and mineral discovery in order to test the civilizing process hypothesis. We track more than $4,500$ mineral discoveries across counties and time, and we match discoveries with historical county formation from the Atlas of Historical County Boundaries. Our measure of state development consists of the territorial status of the place of discovery: colony or state, in which case we consider that discovery postdates the establishment of the state; territory or unorganized land, in which case we consider that the discovery predates the state. We regress measures of crime today on the presence of minerals and on our measure of whether formal state institutions were in place at the time of the discovery. Our analysis is at the county level and controls for state fixed effects throughout. We also study the historical relationship between violence and mineral discovery.

Our results give unambiguous support to the civilizing process. Counties that experienced mineral discoveries *before* statehood exhibited higher levels of homicide and assaults, even to this day. Mineral discoveries are associated with about 200 additional assaults and murders per $100,000$ people in 2000 in these early mining counties – a 40% increase relative to the mean. In contrast, no effect is observed if discoveries occurred *after* the state was established.

Our first difference approach rules out the possibility that our results are due to differences between mining and non-mining areas. However, early and late mining counties may differ along characteristics that also affect crime, thereby jeopardizing our identification strategy. A particular concern is that late mining counties simply have older institutions, and older institutions are associated with less crime. To address this concern, we control throughout for the initial date of county creation as well as historical population density, the main driver of county incorporation. We control for numerous other historical and contemporary county characteristics that may affect crime, such as income, education, ethnic fragmentation, population density, and the presence of women. Early and late mining counties do not differ from one another along any of these characteristics.In addition, the inclusion of state fixed effects remove the influence of any unobserved heterogeneity across states related to mineral discovery and to state incorporation. In robustness tests, we avoid comparing older and more recent discoveries and restrict our attention

territory had been examined for hundreds of miles along the Sierras" (Lang 1983, page 294).

to discoveries that occur within 5 or 10 years of one another. We also restrict our analysis to counties within arbitrarily defined geographic areas, as well as to neighboring counties only. Another potential concern is that territorial status may reflect not only political but also economic and social development of a county. The robustness of the results to the inclusion of a battery of historical economic and social development controls described above alleviates this concern.

Another concern is that miners were negatively selected. Clay and Jones (2008) argues, to the contrary, that miners were more educated and more likely to be white collar workers compared with the rest of the population. Moreover, to threaten our identification, miners should have been able to self select on the basis of the precise timing of the first discovery relative to state incorporation, within a given state. This seems farfetched in the context of the mineral rush that was taking place.

We next turn to the mechanisms that underlie the persistence of violence, more than a hundred years after the state was established in most places. Initial violence may have had deleterious effects on the quality of subsequent institutions, through several direct and indirect channels. Tabellini (2008) and Belloc and Bowles (2013) suggest that social norms and institutions are complements, so that inefficient institutions may follow from the tendency to resolve conflict not through formal but informal, violent means. The use of force may also enable violent agents to accumulate substantial power, which they can use to corrupt nascent formal institutions.[4] We find direct evidence that the persistence of our homicide resource curse is partly explained by a political resource curse, namely by enduring lower quality of judicial institutions. We use the state-level panel dataset of historical judicial quality indicators constructed by Epstein et al. (2001) and we find that in late-mining states, discoveries are associated, yearly, with a reduction by between 2% and 5% in various indicators of the independence of state judges.

To the best of our knowledge, our findings document for the first time the existence of a homicide resource curse, which, like other resource curses, is conditional on the initial quality of institutions. To our motivating question, we offer an

[4]Engerman and Sokoloff (1997, 2002), Acemoglu and Robinson (2006), Rajan and Zingales (2006), among others, discuss the implications of economic inequality for the design of political institutions

answer that consists of a combination of the tardiness of the civilizing process in the United States compared to Europe, and of the shock due to natural resource discovery, which, as we show, further delayed the civilizing process.[5] However, only violent crime – not property crime – is affected, both historically and today. The interpretation is that a private order of property rights did emerge even in the absence of the state, as documented by Anderson and Hill (1979, 2004) and in Clay and Wright (2005). However, in the absence of third-party enforcement, the security of property rights was enforced by high levels of interpersonal violence.

The first contribution of the paper is to the literature on the resource curse and on how initial endowments shape institutions in the long run (Engerman and Sokoloff 1997, 2002, and Rajan and Zingales 2006). The effect of natural resources on development outcomes depends on the quality of institutions (Mehlum et al. 2006), and resources have further deleterious effects on institutional quality (Brollo et al. 2013).[6,7] A sizeable literature also finds a positive relationship between natural resources, in particular oil, and civil conflict (see Ross 2006 for a partial review). We differ from this literature by focusing on privately rather than publicly owned resources and on interpersonal rather than political violence. The most closely related paper is Buonanno et al. (2012), which shows that a resource windfall in the context of weak formal property rights gave rise to the Sicilian mafia. Our work differs from theirs in two ways. First, we abstract from the organizational form that may have substituted for formal, state-based property rights enforcement. Second, and more importantly, we also study the counterfactual situation in which the resource windfall occurs in the presence of strong formal institutions.

Our contribution to the literature on interpersonal violence is two-fold. We

[5] Australia, another mineral rich neo-Europe, experienced a less violent fate, not only because mineral exploitation occurred when state institutions were more developed relatively to the West of the US, but also because mineral rights in Australia are property of the Crown, making state presence a prerequisite to mineral exploitation McLean 2013.

[6] Torvik (2009) provides a review of this literature.

[7] In the context of the United States, while Wright (1990) argues that natural resources contributed to the growth of industrial exports, recent literature finds empirical evidence for the presence of a resource curse (Goldberg et al. 2008). Clay (2010) points out that the differences in the time periods considered by the two studies (1879–1929 for the first one and 1929–2002 for the second) may explain these divergent findings. Our investigation covers the whole time period, as we shed light on crime both at the time of mineral discoveries and today, and we consider a state panel of institutional quality from the mid 19^{th} century to the present.

consider a rather encompassing definition of formal enforcement – whether there is state at all –, and confirm prior evidence that enforcement deters crime, particularly violent crime (Levitt 1997, Kessler and Levitt 1999, and Draca et al. 2011). Our interpretation is that the security of property rights is ensured even in the absence of the state, but by high levels of interpersonal violence. The second contribution is to highlight the importance of reputation as a driver of violence. We review the theoretical background on this in Section 2. A related paper is Grosjean (2014), which links homicide in the US South today to the cultural background of 18th century Scots-Irish immigrants. The underlying assumption is that this group was prone to violence because it came from lawless areas and relied on easily stolen resources (Nisbett and Cohen 1996). This paper demonstrates how the combination of weak formal institutions and easily appropriable resources spurs violence in the first place.

We also contribute to the economic history literature on violence during the Gold Rush. In contrast to the popular myth of a lawless and murderous Wild West, the conventional view in economics is that property rights were secure and violence rather limited in mining districts (Umbeck 1975, 1981, Zerbe and Anderson 2001, and Clay and Wright 2005, 2011). While our empirical results support the first part of this claim, they do not support the second. In that, we agree with historians of crime in the US, in particular Roth (2009).

The remainder of the paper is organized as follows. The conceptual background of rule of law and violence is discussed in Section 2. Section 3 introduces mineral discoveries data and provides some historical background. Sections 4 and 5 provide historical and contemporary evidence. Section 6 discusses channels of persistence and institutional quality. Section 7 concludes.

2 Conceptual background

Until recently, economists have essentially focused on expropriation as a motive for violence. As in Skaperdas (1992) and Gonzalez (2010), several models describe the incentives to engage in predatory behavior when the rule of law is absent or weak, as it was in some areas in the West of the US during the Gold Rush (Umbeck 1975). The threat of violence by individuals creates a "*semblance of property*

rights," to use the words of Skaperdas (1992). It is a substitute for formal law enforcement, albeit an inefficient one, as it involves wasteful individual investments in force. At equilibrium, property rights are sustained by respective investments in power.[8] A common prediction of these complete information models is that, in the absence of bargaining inefficiency, force is not used at equilibrium.[9] Better third-party enforcement does not affect (observed) violence but remove incentives for individual predation. This increases efficiency and affects the division of the surplus, with the powerful party losing much from the withdrawal of the threat of violence (Skaperdas 1992).

Other papers have recently introduced other motives for violence. Chassang and Padro-i-Miquel (2010) show how the presence of strategic uncertainty introduces a new motive for violence: preemption. Players, while second-guessing each other's moves, may decide to attack in order to avoid suffering a debilitating surprise attack from an opponent who is expected to be aggressive. Silverman (2004) formalizes a reputation-based theory of crime in a dynamic game of incomplete information. When the population contains a fraction of violence-committed types, normal types may engage in violence in order to build up their reputation and deter attacks. This "culture of violence" equilibrium prevails if the cost of participating in crime is low enough, for example, if the state is absent. Perfect enforcement unravels the culture of violence.[10] In Ghosh et al. (2013), repeated acts of violence build a reputation for being committed to violence, which leads the other party to concede. The intuition easily applies to a dispute over mineral claims. Signaling one's strength and determination with a violent attack will convince other agents to concede their claims and enable the violent agent to grab a larger share of the surplus.

This overview of the relevant literature suggests a few testable predictions. No violence occurs in the presence of a Weberian state, which enforces agreed settlements and monopolizes violence. In its absence, but with perfect bargaining,

[8]Investments in force are either symmetric as in Umbeck (1975) and Gonzalez (2010) or not, with one party specializing in predation as in Skaperdas (1992)

[9]Actual violence only occurs in the case of limited commitment, as in Gonzalez (2010) or incomplete information, as in Donohue and Levitt (1998) or Aney (2012).

[10]However, intermediate policy crackdown can actually increase violence if they strengthen the signaling value of a violent encounter.

a "semblance of property rights" prevail and violence is not observed in the process of expropriation. The only type of violence that is observed in equilibrium aims at intimidation.

Although very different states of the word prevail in absence or presence of the state, it is important to note that the result by no means rests upon the composition of the underlying population to be different, or in applied econometricians terms, upon selection. In expropriation models, more people commit crime if the costs of crime are lower, irrespective of their types. In reputation-based models of violence, normal types may engage in violence when enforcement is absent, irrespective of the actual proportion of committed types in the population.

Our empirical analysis is primarily focused on present-day outcomes, when the state has been established everywhere. What could underlie persistent differences?

The first channel of transmission consists of the quality of institutions. A common prediction of all crime models is that the use of force enables the violent party to secure a larger share of the surplus. According to Engerman and Sokoloff (1997, 2002) and Rajan and Zingales (2006), this party is then able to influence the design of formal institutions to preserve her rents, with negative long-run consequences on the quality of institutions. The persistence of formal institutions – documented by Acemoglu et al. (2001) and Dell (2010) among others –, implies that high levels of interpersonal violence also persist over time.

A second channel of transmission is culture. In models of social interactions *à la* Glaeser et al. (1996), violence feeds on itself, so that initial violence can persist over time. Another possibility is that differences in the quality of institutions give rise to different cultural norms, which can persist over time, regardless of changes in the quality of institutions. Tabellini (2008) and Guiso et al. (2008) describe how parental transmission of preferences depends on the spatial pattern of external enforcement and becomes biased towards excessively conservative priors. If formal enforcement is weak, parents transmit low trust in order to protect their children from situations in which they would be taken advantage of if they cooperate. This results in low levels of social trust, which become self-fulfilling, despite potential changes in formal institutions. The same argument can easily be extended to aggressiveness. If enforcement is weak at one period of time, the returns to interpersonal violence are high and parents would want their children to internalize

the benefits of using violence. They transmit norms that legitimize the use of force. Violent norms also become self-fulfilling, and the strong strategic complementarity between violent strategies implies that a culture of violence should be highly persistent. Elias (1994) already had the intuition that state development affects social norms, as agents internalize the social control of violence. Grosjean (2014) shows empirically that violent cultural norms only persisted where formal institutions were weak.

An implication of these models is that institutional and cultural persistence are complements and not substitutes, as is suggested theoretically in Tabellini (2008) and Belloc and Bowles (2013). It is neither our goal, nor within our reach, to identify them as separate channels. Persistent weakness of formal institutions only make aggressiveness more valuable.

3 Historical background and data on mineral discoveries and the rule of law

As noted by Wright (1990), the most distinctive feature of the American mining economy was the intensity of exploration and exploitation. This was due to a particularly rewarding incentive system, as mineral rights were included in the bundle of land rights (David and Wright 1995). Knowledge of American economic geography was gained in the 18^{th} and 19^{th} centuries both from public and private surveying efforts and from unorganized private ventures (Davis 1972). Even though states and the federal government invested in geological surveys as early as the 1820s – see Clay (2010) for a full list of state geological surveys' dates –, exploration was to a large extent a private enterprise, in what has been described as an open and saturated market by Clay and Wright (2011). Clay and Jones (2008) find that miners in California in 1850 earned less on average in relative terms than day laborers in other parts of the United States, confirming that open entry gave way to a dissipation of rents. Taken together, such a private and massive search in a market with open entry - the mineral rush– guarantees that specific mineral discoveries were largely independent of territorial status at the time of discovery, at least when considering variation within a given state and controlling for a large

number of observable characteristics.

The Mineral Resources Data System (MRDS) describes mineral resources throughout the world. It contains more than $25,000$ observations for the United States. Information about discovery year is available for more than $4,500$ of them. Most observations for which the discovery year is not available are deposits that were not worthy of production. As shown in Figure 1, a handful of discoveries had been recorded before the 18^{th} century but the majority of mineral discoveries occurred between 1825 and the second World War. For each of these $4,500$ observations, the dataset contains information on localization, year of discovery and type of mineral.[11,12]

The independence of territorial status relative to the time of mineral discovery depends not only on the specificity of mineral exploration but also on the territorial expansion of the US government. Davis (1972) agues that territorial expansion of the United States was largely driven by external geo-political forces, which were orthogonal to mineral discoveries. California and the Southwest were gained after the $1846-1848$ war with Mexico, which originated in a territorial dispute over the Texas boundary. Within newly integrated states, the main driver of incorporation was population density (Davis (1972)), which we control for in all empirical specifications.

Figure 1 illustrates our basic identification strategy: some discoveries occurred before county incorporation, some after. County creation date is too crude a measure since one needs to distinguish whether counties were created ex-nihilo or carved out of an existing politically organized entity. We consider a finer measure of the territorial status of an area at the time of mineral discovery. We proceed

[11]Notice that the MRDS does not convey information on the quantities extracted, but this would be a more endogenous measure, which would be influenced by factors such as security of property rights. Oil, gas, and coal are not minerals and are not included in the MRDS. We nevertheless control for the presence of such resources in the empirical analysis

[12]An alternative source of information on mining activity is provided by occupational information in the Historical Census, which gives the proportion of people employed in mining activity and industry (Ruggles et al. 2010). This is, however, a less satisfactory proxy for mineral discoveries than physical discoveries for two main reasons. First, people may declare mining as their activity but may be either unemployed or migrant workers, so that self-declared miners may not adequately reflect the extent of mining in a given area. Second, the number of miners in a given region may be endogenous to other regional characteristics, in particular the quality of institutions. Lastly, Census data, does not make it possible to date mining activity, especially if it occurred before statehood and the organization of the Census.

in two steps. First, we map each mineral discovery on yearly maps of the United States constructed from the Atlas of Historical County Boundaries. Each discovery ends up inside an area that is either on unorganized land or enclosed in some administrative boundaries. The latter includes colonies, states, and organized territories. An organized territory refers to the territorial status between the transfer of sovereignty to the Union and the actual establishment of formal institutions. Even though territories are technically under the jurisdiction of the United States, in practice law enforcement was minimal to non-existent. County jails, sheriffs, constables, and courts only came with the incorporation of local areas into counties and state Lane (1992). We consider that discoveries occurring in territories or on unorganized land – as opposed to states or colonies –, pre-date the rule of law. We refer to these as *early* discoveries. Second, we project back each discovery on a stabilized map of American administrative boundaries. This enables us to observe whether each county or state ever had mineral discoveries within its boundary and whether any of those occurred before organization of the land.

We retain two dummy variables that capture an area's mining and state development history. The first takes value one if any discovery ever occurred inside a county's boundary. Minerals were discovered in 355 counties in 29 states, 11% of all US counties. The second takes value one if any discovery occurred inside what is today a county but was unorganized land or territory at the time of discovery. This is the case in 126 counties in 15 states, about 4% of all US counties and 35% of mining counties. Figure 2 maps the counties that had mineral discoveries, and the category each falls into.

4 Historical evidence

Our work is motivated by explaining contemporary violence, which we explore in the next section. First, however, we document the historical association between mineral discoveries and crime.

4.1 Data and descriptive statistics

The historical crime data used in this study was collected by historians and comes from the Historical Violence Database of the Criminal Justice Research Center at Ohio State University. The limitation of this data consists of its limited geographic coverage and its lack of consistency across different locations. However, it contains a wealth of details on the circumstances of homicide, such as the occupation of offender and victim, the place where the homicide took place, and the underlying motivations.

McKanna (2002) gathered data on the population of homicide, the occupation of offenders and victims, and the circumstances of homicide in seven California counties between 1849 and 1899 based on coroner's reports and newspaper accounts.[13] Gold was first discovered in California in 1848 and statehood was achieved in 1850. in addition, new state institutions were very slow to be established (Umbeck 1975). As David and Wright (1995) note: "*the great California Gold boom occurred under a virtually complete absence of government authority*" (page 218). We match McKanna's data to the 1% US Censuses from 1850 to 1940 in order to obtain homicide rates per 100,000 inhabitants. The average homicide rate between 1850 and 1899 was higher in the two mining counties of Calaveras and Tuolumne than in the other, non-mining counties (42.13 per $100,000$ against 32.68, t-stat of 2.33). The correlation between homicide rates and the proportion of people employed in the mining sector between 1850 and 1880 is 0.82. Miners were also over-represented as victims and perpetrators of homicide. In Calaveras and Tuolumne, they represented nearly 73% of perpetrators of homicide, against 50% of the population (t-stat of 4.13).

Another striking feature is that, contrary to the popular view of the Wild West, according to which expropriation was the main goal of homicide, but consistent with our stress on intimidation as the main purpose of violence, the majority of homicide did not occur in the context of expropriation, nor in the mines, but rather in the context of quarrels. 78% of homicides in mining areas occurred in the context of a quarrel, against 68% in non-mining areas (t-stat of 1.83), and

[13]Counties for which McKanna (2002) provides information are: Calaveras, Sacramento, San Diego, San Joaquin, San Luis Obispo, Santa Barbara, and Tuolumne.

only 10% in the context of a robbery, a proportion no different than in non-mining areas (t-stat of 0.04). Only 12% of homicide took place in the gold fields, against 35% in saloons or streets.[14]

4.2 Regression results

We now turn to more systematic evidence on the short-term historical relationship between mineral discoveries and violence at the county level. We match McKanna's yearly crime data in Californian counties with information about yearly mining discoveries extracted from the Mineral Resources Data System. Because we have a yearly panel of homicide and mineral discoveries for these counties, it is possible to control for common shocks as well as county-invariant characteristics. As such, we estimate the relationship between yearly homicide and the stock of mines, controlling for time and county-specific effects:

$$\begin{aligned} \text{Homicide}_{it} = \; & \beta_0 + \beta_1 \text{Stock of mines}_{it} + \beta_2 \text{Early mining}_i \\ & + \beta_3 \text{Stock of mines}_{it} \times \text{Early mining}_i + \delta_i + \delta_t + \epsilon_{it}, \end{aligned} \tag{1}$$

where $Homicide_{it}$ denotes the number of homicides in county i in year t, $Stock\ of\ mines_{it}$ is the accumulated sum of mineral discoveries from 1849 to t and $Early\ mining_i$ denotes the mining history of the county.[15]

Regression results are displayed in Table 1. In the first column, the coefficient associated with the stock of mines is positive and statistically significant.

[14] McKanna (1997) also gathered data in three counties of Arizona, Colorado, and Nebraska between 1880 and 1910. In the "early" mining county of Gila (Arizona), the average homicide rate was 70 per 100,000, with miners, again, over-represented as victims and offenders (31% against 18% of the population at large). The homicide rate in Gila was 40% higher than in Las Animas (Colorado), also a mining county but a "late" one. In Douglas (Nebraska), where no discovery occurred, the homicide rate was "only" 14 by comparison. As in California, quarrels were twice as frequently the cause of homicide in the early mining county of Gila (at nearly 70%), compared to other areas. Robberies, however, were less frequent (3.2% of homicide against 5.6% in Douglas). In Illinois, where all mineral discoveries occurred after state incorporation, homicide rates between 1850 and 1890 were not only much lower overall but also displayed a reversed pattern as the one described so far. Homicide rates were higher in non-mining areas (16.65) than in mining counties (7.82) (Roth 2009) and miners were under-represented as homicide offenders.

[15] There is an average of 3.5 homicide per year per county (s.d. = 3.7), and a total of 1.5 discoveries per county over the period (s.d. = 2.8) with a maximum of 11 discoveries.

Although common time-varying shocks are already taken into account, counties may have experienced different population growth over the period, which could also affect homicide patterns. To tackle this issue, we introduce yearly population as a control variable in the second column. As expected, the estimated coefficient of this variable is positive and statistically significant. However, its introduction does not change the magnitude and statistical significance of the stock of mineral discoveries. The coefficient is roughly equal to one-fourth, meaning that four more discoveries in a county are associated with one more homicide during subsequent years. In column 3, we interact the *Stock of mines*$_{it}$ with the mining history of the county (*Early mining*$_i$). The effect of the stock of mines on homicides is higher in counties where mineral resources was discovered before land organization.

Before turning to the question of whether such violence has persisted until today and why, we discuss two other sources of data. Vital statistics provide information on deaths due to violent accidents at the state level since 1900. This data is of limited use to us for two reasons. First, it is only available at the state level. Second, violent accidents include both homicide and other forms of violent deaths, including job related accidents. Yet, descriptive evidence backs our emerging story of a positive relationship between mineral discoveries and crime, which is particularly strong if discoveries occurred before formal institutions were in place. The rate of violent death, defined as the proportion of violent deaths over total mortality, is highest in 1923 Wyoming (20.32%), a state which experienced several "early" discoveries, and lowest in 1918 New Hampshire (3.27%), a state which never experienced any discovery. Previous literature on historical crime, such as Eriksson (2013) has used censuses of prisons. This data is of less use to us, not only because the reasons for imprisonment are often unspecified but also because its availability and quality are endogenous to the quality of state institutions.

5 Contemporary evidence

In this section, we present evidence that today's violence is related to counties' mining history.

5.1 Data

Contemporary data is from the Uniform Crime Reporting (UCR) Program Data by the United States Department of Justice and Federal Bureau of Investigation. UCR data provides information on 43 types of offenses and the monthly count of arrests for each offense in more than $17,000$ reporting agencies. Because the number of reporting agencies has been increasing over the years, we use more recent, more reliable data. Our independent variable consists of the average crime rate per $100,000$ people in 2000, which we compute by merging UCR data with the 2000 US census.[16] To proxy for violence aimed at intimidation, we use the rate of murder and aggravated assaults.

On average, there were 617 murders and aggravated assaults per $100,000$ people across US counties in 2000. The rate of murders and assaults is much higher in counties with mineral resources, at 881 per $100,000$ people against 511 in mineral poor counties. Within mining counties, the rate of murders and assaults is more than twice as high in early mining counties, with $1,416$ murders and aggravated assaults per $100,000$ in 2000 compared to 603 in counties that experienced mineral resources discoveries only after land organization. We turn to regression analysis to test the robustness of this discrepancy.

5.2 Empirical strategy

We estimate the following specification:

$$\text{Violence}_i = \beta_0 + \beta_1 \text{Mining}_i + \beta_2 \text{Early Mining}_i + \beta_3 X_i + \delta_{s(i)} + \epsilon_i, \qquad (2)$$

where Violence_i captures today's crime in county i. One of the main insights of the theoretical background discussed in Section 2 is that a distinction should be made between violence for the purpose of intimidation and violence for the purpose of expropriation or the defense of property rights. We first consider as dependent variable only violent crimes, i.e. murders and assaults, and then look at property crime, i.e. robbery, burglary, and larceny.

The variable $Mining_i$ indicates the presence of any mine at any point in time

[16] All the results to come are unaffected if we consider the average for 2000-2007 instead.

on the physical area of county i. *Early Mining*$_i$ is a dummy variable equal to unity if minerals had been discovered in county i before the area was administratively organized, that is to say before incorporation of the state in which county i is situated, or before incorporation of county i to a state. X_i is a vector of historical and contemporary controls. $\delta_{s(i)}$ is a vector of state dummies.

Identification of expression (2) requires that discoveries are independent both of contemporary violence and of the interactive effect of discoveries and territorial status at the time of discovery. The presence of minerals is exogenous and the specificity of mineral exploration that we have already discussed implies quasi-exogeneity of territorial status at the precise time of a particular discovery. However, the independence of contemporary violence, mineral discoveries and their precise timing would be jeopardized if some variable not included in expression (2) influenced discoveries, their timing, and historical violence, and if violence persisted over time. To alleviate this concern, we include in X_i a wide range of historical and contemporary characteristics. In particular, to avoid confounding the effects of mineral discovery relative to state development from that of the the age of institutions, we control for the initial date at which the county was organized. We also control for population density, which was the main driver of county incorporation. A potential criticism to our approach is that territorial status may capture more than political development. To address this concern, we use a large number of historical covariates that aim to capture economic and social dimensions of development. In addition to the share of women in the population in 1880, we add manufactured, agricultural and other farming products outputs, and population density measured in 1880, as well as manufacturing wage, white and black literacy rates, and county's population in 1900. The choice of these dates is driven by a trade-off between measuring variables as close as possible to the date of mineral discoveries and the loss of observations inherent to going back further in time. These choices result in a sample of $2,015$ counties. Contemporary controls include the shares of blacks and women in the population, ethnic fractionalization, Gini coefficient, population density, urbanization rate, aggregate income, and per capita income. We also control for the presence of coal mines and oil/gas mines.[17]

[17] Availability of data does not allow us to distinguish whether the discovery of oil, gas or coal has been achieved before incorporation or not. We only have the information on whether a mine

Unless otherwise specified, all regressions are estimated on this sample and with White heteroskedastic standard errors adjusted for clustering at the state level. Descriptive statistics of variables are presented in Table A3 in the appendix.

A particular concern is that mining on unincorporated lands may have attracted particularly violent individuals, who then transmitted their violent traits to their offspring, explaining a higher prevalence of violence in these counties today. While we cannot completely rule out this possibility, a number of factors temper this concern. First, there is no evidence of adverse selection of miners in the West compared to the rest of the population. In fact, it is quite to the contrary. Clay and Jones (2008) use data collected from the 1850 and 1852 census of Population for California together with the 1% sample of the 1850 census to compare the characteristics of those who went to California to the rest of the population. Contrary to popular conception, they conclude that positive selection occurred, with those migrating to California being more educated and more likely to be from a middle class background. Second, state fixed effects are included in the analysis. To jeopardize our identification, miners should have systematically migrated to different areas within a given state in anticipation of the precise date of incorporation of these counties and as a function of expected violence.

5.3 Results

We first test whether interpersonal violence today is higher in mining areas. The first three columns of Table 2 present the estimated coefficients of equation (2) from which we remove the *early mining* variable and vary the set of covariates. The theoretical prediction is that the discovery of mineral resources fosters violence, but only in the absence of effective third-party enforcement. A test of this prediction is implemented in columns 4 to 6 by adding the *early mining* dummy to the specification. Regression results fully corroborate our prediction. The effect of *mining* completely vanishes as we introduce the *early mining* variable. In contrast, the coefficient associated with *early mining* is positive and statistically significant at the one percent confidence level. In other words, mining discoveries are positively and robustly associated with higher interpersonal violence, but only

was operated in a given county since 1945.

in counties in which some minerals were discovered before the rule of law was in place. The effect is robust to controlling for the wide range of historical controls discussed above.

Another theoretical prediction is that the effect of resource abundance conditional on the absence of rule of law is different for different types of violence. More specifically, we predict that it affects violence for intimidation (as illustrated by the previous estimation results using violent crime) but not violence aimed at expropriation. We test this prediction in the first column of Table 3, which displays the estimated coefficients of a specification identical to the one in the fourth column of Table 2, but with property crime (robbery, burglary, and larceny) as the dependent variable. There is no evidence of any relationship between *early mining* history and today's rate of property crime. As illustrated by the results presented in subsequent columns of Table 3, we do not find any evidence of any effect of mining history on other crimes such as sex offense, drunk driving, forgery, or vagrancy. This further advocates a specific relationship between mining history and the use of interpersonal violence for intimidation.

5.4 Robustness checks

Our identification strategy relies on a comparison across space within the same state. This may raise three particular concerns. First, a county may have been affected by the mining history of its neighbors. Second, counties located near state boundaries may share characteristics not captured by state fixed effects. Finally, mineral resources endowment is not evenly distributed across space. In Table 4, we pursue a number of strategies to tackle these concerns. We first use the estimation method developed by Hsiang (2010) to take into account spatial correlation. In column 1, standard errors are adjusted for spatial correlation adjustment with a 300-kilometers radius. Standard errors appear to be very similar to those previously estimated.

In column 2, we restrict the sample to counties that are either mining counties or that are immediate neighbors of mining counties. While the point estimate of the variable of interest is lower, it still has the same order of magnitude. We apply a similar but different strategy in column 3 where we restrict the sample to states

where the three types of counties – i.e. mining, early mining and non-mining – coexist. Given the use of state fixed effects, these states, located in the West, are those who allow us to identify the relations we are interested in – the other states contributing only to the estimation of covariates. This approach leaves our point estimates and their statistical significance fairly unchanged.

In columns 4 to 6, we address the issue that the borders of states may not reflect the geological characteristics that are relevant for mineral discoveries. We cut the United States using arbitrary 2−, 5− and 7−degree grids. We use these groups to replace state fixed effects and we adjust standard errors at this level. Estimated coefficients of *early mining* are lower the wider the squares of the grid, but still the effect on this variable on today's violence persists.

Finally, we show in columns 7 and 8 of Table 4 that results are not sensitive to the use of propensity score matching as an alternative estimation method. Given that all early mining counties are by construction also mining countries, it is not possible to estimate both effects simultaneously. We thus start in column 7 by matching mining and non-mining counties on the basis of observable characteristics. We obtain a positive treatment effect that is not statistically significant. Then, we match early mining and mining counties on the basis of observable characteristics in column 8. We obtain a positive and significant treatment effect that is of the same order of magnitude as former estimates.

Another possible concern regarding our identification strategy is that we compare mineral discoveries that occurred before organization of the land to mineral discoveries that occurred anytime between land organization and today. One may thus be inclined to worry that we do not capture an effect due to land organization at the moment of discoveries, but rather simply an effect due to ancient mining activity. We show here that there is some credence to our interpretation and less to this alternative one. To do so, we zoom in on each county's history and observed discoveries that occurred within a small time window around land organization. More precisely, we constructed a dummy variable equal to unity if mineral resources were discovered up to five years prior ro land organization, and a dummy variable if mineral resources were discovered up to five years after. We then used these variables to replace the *early mining* dummy in (2). Estimated coefficients are presented in column 9 of Table 4. While discoveries up to five years

prior to land organization have a positive effect that lies just above the 10% statistical significance threshold, discoveries up to five years after land organization have an unambiguously non-significant effect. We undertake the same exercise in column 10 using a 10-year window and reach a similar conclusion.

The results presented here support the existence of a robust positive correlation between discoveries in the absence of the rule of law and interpersonal violence today.

6 Persistence and the political resource curse

In this section, we explore how and why discoveries more than a hundred years ago are still associated with homicide today. We suggest the homicide resource curse as a mechanism for a political resource curse. Initial violence might have deleterious effects on the quality of subsequent institutions, through several direct and indirect channels. Tabellini (2008) and Belloc and Bowles (2013) suggest that social norms and institutions are complements, so that inefficient institutions may arise simply due to the presence of violence and the tendency to resolve conflict not through formal means but though informal, violent ones. An indirect channel possibility is that the use of force enables violent agents to accumulate substantial power. When the state arrives, these individuals have the incentives and the financial resources to corrupt and shape nascent formal institutions in the aim of preserving their rents. We first describe the data and document the existence of a political resource curse in early mining areas.

6.1 Data and empirical methodology

The legal prerogatives of counties and municipalities generally involve some dimension of law enforcement, but judicial and legislative powers lie at the state or federal level. Data on local law enforcement at the county level, particularly historical data, is very limited, and only consist in the number of police or police budgets, which are difficult to interpret because it is endogenous to crime.

Because of these conceptual and data availability considerations, we turn to a well accepted measure of the quality of judicial institutions at the state level.

We rely on Epstein et al. (2001)'s measures of judicial quality in a state panel available yearly from 1866 (or the date of statehood) to 2000. Appointment and retention methods, as well as length of the terms of state Supreme Court Judges are taken as measures of independence of the judiciary. The first variable consists in the nomination procedures for state judges. It is equal to 0 if judges are appointed, to 1 if they are elected through partisan elections, and to 2 if they are elected through non-partisan elections. Direct appointment of state judges by the legislature is considered to be the most restrictive of judicial independence, and non-partisan elections to lead to the highest independence of judges Epstein et al. (2001). The second measure considers nomination and retention procedures. The third variable consists in the length of state judges's terms, which vary from 1 to 25 years throughout the period. The last variable is a dummy indicating the presence of intermediary appellate courts. Longer terms and the presence of intermediary appellate courts indicate more judicial independence.

Our aim is to study how mineral discoveries affect institutional quality. We regress yearly judicial quality indicator indices on the number of mineral discoveries that occurred during the preceding period. The analysis controls for state fixed effects, which remove the influence of potential heterogeneity across states in the development of institutions, as well as for year fixed effects, which capture general trends in the development of institutions. We estimate the following equation:

$$\text{IQ}_{st} = \beta_0 + \beta_1 \text{Mining}_{st} + \delta_s + \delta_t + \epsilon_{st}, \tag{3}$$

where IQ_{st} denotes each of our different measures of judicial institutions quality in state s at time t. Each measure is scaled such that a higher value indicates higher judicial independence. $Mining_{st}$ denotes the total number of mineral discoveries in state s at time t. δ_s and δ_t are state and year fixed effects, and ϵ_{st} is the error term.

In the second step of the analysis, we distinguish between states in which some mineral discoveries had occurred before complete incorporation and states that were completely established before any mineral was discovered. This distinction is introduced in order to test our model, which predicts that the acquisition of economic power is only possible in areas in which violence is not illegal. Thus, the

deterioration in institutional quality should be observed in those instances only. We estimate the following specification:

$$\text{IQ}_{st} = \beta_0 + \beta_1 \text{Mining}_{st} + \beta_2 \text{Mining}_{st} \times \text{Early Mining}_s + \delta_s + \delta_t + \epsilon_{st}, \qquad (4)$$

where *Early Mining*$_s$ is a dummy variable taking the value of unity if a mineral discovery occurred in any county of state s before the state was incorporated into the Union, or before the county was incorporated into the state.[18] All other variables are identical to those defined in expression (3). Because state fixed effects are included in the specification, the interactive term is correctly estimated.

6.2 Results

Figure 3 illustrates the main argument. It displays the evolution of (i) the nomination procedure for state judges; (ii) a combination index of nomination and retention methods of state judges; (iii) the length of terms of state judges; and (iv) the presence of an intermediary appellate court, for states without mineral discoveries, for states with mineral discoveries after the organization of the land and for "early mining" states.

In the period following the 19$^{\text{th}}$ century and up until the second World War, there is a large difference in nomination procedures for state judges between early mining states and others. In early mining states, judges are predominantly appointed, whereas the prevalence of elections is much larger in late mining or non mining states. This gap closes quickly around the time of the second World War, when the importance of mineral exploitation in the American economy sharply declined (David and Wright 1995). In other words, judicial independence was lower in states with early mineral discoveries and the gap was widened with each additional discovery, but the gap began closing as the rents associated with mineral discoveries started to decline. The independence of both selection and retention of state judges exhibits the same pattern, but the difference persists for longer. The length of judges' tenure is much shorter in mining states, which is indicative of

[18] States in which some early discovery occurred are: Arizona, California, Colorado, Idaho, Louisiana, Montana, New Jersey, New Mexico, Oklahoma, South Dakota, Texas, Utah, Washington, and Wyoming.

less independent judges, but there is no statistically significant difference between early and late mining states.

Estimation results of specifications (3) and (4) presented in Table 5 confirm these broad patterns. For each dependent variable, the first column considers the relationship between judicial institutional quality and mineral discoveries. Mineral discoveries are negatively associated with judicial independence, illustrating the presence of a political resource curse. The effect is statistically significant for nomination and retention procedures. However, as indicated in the second column for each variable, the effect is entirely driven by early mining states. Selection and retention procedures, terms of judges, and presence of intermediary appellate courts in a given year and in a given state are not affected by mineral discoveries in states that were incorporated to the Union well before minerals were first discovered. However, all measures of judicial independence are negatively affected by mineral discoveries in states in which at least some of the discoveries occurred before full state incorporation to the Union.

The magnitude of the effect is substantial. In a state where some discoveries occurred before incorporation, mineral discoveries are associated with a 2.5% reduction in the measure of independence of selection procedure for state judges, a 5% reduction in their terms lengths, and a 1.7% reduction in the likelihood that an intermediary appellate court is present.

These results mirror those obtained for violence. We put forward the homicide resource curse as a mechanism for the political resource curse through the direct and indirect channels discussed in the preamble to this section.

6.3 Robustness checks

The results are robust to using logit, or ordered logit specifications in the relevant cases. The results are also robust to clustering standard errors both at the year and state level and to correcting for serial and spatial correlation (Hsiang 2010).

We include lags of discoveries in order to investigate dynamic effects. Results are displayed in Table A1 in the appendix. The effect is quite persistent over time, except for the length of terms of state judges, for which the effect is no longer significant after two periods. This may be due to the fact that term length is more

easily adjustable than nomination and retention procedures.

A potential concern here is that these results are not due to the causal effect of mineral discoveries but to unobservable state level characteristics that are correlated with poor institutional quality. Since we control in all regressions for state and year fixed effects, such state characteristics should be varying over time in order to jeopardize causal identification. This substantially restricts the set of possible contestants. One possibility is that mineral discoveries generated short term immigration of a poorly educated and disenfranchised population, explaining the observed deterioration in institutional quality. Two elements argue against this possibility. First, as we have noted before, Clay and Jones (2008) find that migrants were positively selected to mining areas, in terms of education and income. Second, we perform a falsification test and find that measures of competitiveness of national politics (Besley et al. 2010) are affected by mineral discoveries, but without distinction between early and late mining states (see Table A2 in the appendix). Moreover, the effect on the competitiveness in general elections is positive, which suggests that if anything, the relationship between mining and institutional quality is upwardly biased.

As argued in Section 2, both initial violence and quality of subsequent institutions can affect social norms. We attempted to test this hypothesis with data on attitudes towards the use of interpersonal violence from the General Social Survey. However, data is only available in 300 out of about 3,000 US counties. This subsample of counties is geographically clustered within each state. As a consequence, there is virtually no variation in mineral resources endowment between counties of the same state surveyed in the General Social Survey. Although we are unable to document the cultural legacy, the review of the literature in 2 suggests that it would go in the same direction as the institutional legacy: one should expect more violent norms where institutions are of lower quality.

7 Conclusion

This work documents the negative short- and long-run consequences of mineral discoveries in the context of weak polities. It uncovers the existence of a homicide resource curse, which is conditional on the quality of formal institutions. In areas

of the US in which mineral discoveries occurred before the formal rule of law was established, violence spiked up and higher levels of interpersonal violence have persisted until today.

We also illustrate the existence of a political resource curse in the US and suggests interpersonal violence as an underlying mechanism. Mineral discoveries are negatively associated with the quality of judicial institutions, but only in states that had experienced mineral discoveries before full incorporation. The prevalence of violence concomitant to discoveries in these places may explain the deterioration of formal institutional quality through direct and indirect channels, namely if violence endowed agents with substantial resources available corrupt nascent political institutions. In turn, the persistence of institutions has paved the way to the persistence of the homicide resource curse.

We also provide a new interpretation for the economic role of interpersonal violence. Violence that aims at intimidating potential partners plays a crucial role in determining surplus allocation. Because this type of violence is efficient, it is observable by the econometrician, in contrast with violence aimed at expropriation. An implication is that as the rule of law progresses, only an effect on violence for intimidation rather than violence for expropriation will be observed. We thereby reconcile the robust yet puzzling result of the crime literature that violent crime is much more responsive to policing than property crime.

Our findings on interpersonal violence also mirror three robust findings of the literature on political violence. First, violence is the hallmark of weak polities. Second, the presence of natural resources is an important driver of violence in the context of weak institutions, where it can lead to the further debilitation of institutions. Third, because institutions are persistent and because violence often begets more violence, countries can be trapped in an unfortunate equilibrium of poor quality institutions and high violence.

One criticism to our approach is that our proxy for state development may also capture other dimensions of political, economic, and social development. We address this issue by controlling for a large number of historical proxies for economic and social development, but some dimensions may be still remain unobserved. Even so, were territorial status to encompass more than the rule of law, the policy recommendation remains identical. Early mining, before strong and impartial polities are developed, has deleterious and lasting effects on interpersonal violence and on institutional quality.

References

Acemoglu, Daron, Simon Johnson, and James A. Robinson. "The Colonial Origins of Comparative Development: An Empirical Investigation." *American Economic Review* 91, 5: (2001) 1369–1401.

Acemoglu, Daron, and James A. Robinson. "De Facto Political Power and Institutional Persistence." *American Economic Review* 96, 2: (2006) 325–330.

Anderson, Terry L., and Peter J. Hill. "The American Experiment in Anarcho-Capitalism: The Not So Wild, Wild West." *Journal of Libertarian Studies* 3, 1: (1979) 9–29.

———. *The Not So Wild, Wild West: Property Rights on the Frontier*. Stanford University Press, 2004.

Aney, Madhav S. "Conflict with Quitting Rights: A Mechanism Design Approach." Working Papers 18-2012, Singapore Management University, School of Economics, 2012.

Belloc, Marianna, and Samuel Bowles. "The Persistence of Inferior Cultural-Institutional Conventions." *American Economic Review* 103, 3: (2013) 93–98. `http://www.aeaweb.org/articles.php?doi=10.1257/aer.103.3.93`.

Besley, Timothy, Torsten Persson, and Daniel M. Sturm. "Political Competition, Policy and Growth: Theory and Evidence from the US." *Review of Economic Studies* 77, 4: (2010) 1329–1352.

Brollo, Fernanda, Tommaso Nannicini, Roberto Perotti, and Guido Tabellini. "The Political Resource Curse." *American Economic Review* 103, 5: (2013) 1759–96.

Buonanno, Paolo, Ruben Durante, Giovanni Prarolo, and Paolo Vanin. "Poor Institutions, Rich Mines: Resource Curse and the Origins of the Sicilian Mafia." Carlo Alberto Notebooks 261, Collegio Carlo Alberto, 2012.

Chassang, Sylvain, and Gerard Padro-i-Miquel. "Conflict and Deterrence under Strategic Risk." *The Quarterly Journal of Economics* 125, 4: (2010) 1821–1858.

Clay, Karen. "Natural Resources and Economic Outcomes." In *Economic Evolution and Revolutions in Historical Time*, edited by Paul Rhode, Stanford University Press, 2010.

Clay, Karen, and Randall Jones. "Migrating to Riches? Evidence from the California Gold Rush." *The Journal of Economic History* 68, 04: (2008) 997–1027.

Clay, Karen, and Gavin Wright. "Gold Rush Legacy: American Minerals and the Knowledge Economy." In *The Evolution of Property Rights Related to Land and Natural Resources*, edited by Daniel Cole, and Elinor Ostrom, Cambridge: Lincoln Land Institute Press, 2011.

Clay, Karen B., and Gavin Wright. "Order without law? Property rights during the California gold rush." *Explorations in Economic History* 42, 2: (2005) 155–183.

David, Paul A., and Gavin Wright. "The origins of American resource abundance." Technical report, 1995.

Davis, Lance E. *American Economic Growth: An Economist's History of the United States*. Harper & Row, 1972.

Dell, Melissa. "The Persistent Effects of Peru's Mining Mita." *Econometrica* 78, 6: (2010) 1863–1903.

Donohue, John J., and Steven D. Levitt. "Guns, Violence, and the Efficiency of Illegal Markets." *American Economic Review* 88, 2: (1998) 463–67.

Draca, Mirko, Stephen Machin, and Robert Witt. "Panic on the Streets of London: Police, Crime, and the July 2005 Terror Attacks." *American Economic Review* 101, 5: (2011) 2157–81.

Elias, Norbert. *The Civilizing Process*. Blackwell Publishers, Oxford UK & Cambridge USA, 1994.

Engerman, Stanley L., and Kenneth L. Sokoloff. "Factor endowments, institutions, and differential paths of growth among New World economies: A view from

economic historians of the United States." In *How Latin America Fell Behind*, edited by S. Harber, Stanford University Press, 1997, 260–304.

———. "Factor Endowments, Inequality, and Paths of Development Among New World Economics." Working Paper 9259, National Bureau of Economic Research, 2002.

Epstein, Knight, and Shvetsova. "Unknown." Unknown, 2001.

Eriksson, Katherine. "Does elementary schooling reduce crime later in life?: Evidence from the early twentieth century US South." *Mimeo, California Polytechnic State University* .

Ghosh, Sambuddha, Gabriele Gratton, and Caixa Shen. "Terrorism: A Model of Intimidation." *Mimeo, University of New South Wales* .

Glaeser, Edward L, Bruce Sacerdote, and Jose A Scheinkman. "Crime and Social Interactions." *The Quarterly Journal of Economics* 111, 2: (1996) 507–48.

Goldberg, Ellis, Erik Wibbels, and Eric Mvukiyehe. "Lessons from Strange Cases: Democracy, Development, and the Resource Curse in the U.S. States." *Comparative Political Studies* 41: (2008) 477–514.

Gonzalez, Francisco M. "The Use of Coercion in Society: Insecure Property Rights, Conflict and Economic Backwardness." In *Oxford Handbook of the Economics of Peace and Conflict*, edited by Michelle R. Garfinkel, and Stergios Skaperdas, Oxford University Press, 2010.

Grosjean, Pauline. "A History of Violence: The Culture of Honor and Homicide in the US South.", 2014. Forthcoming in *Journal of the European Economic Association*.

Guiso, Luigi, Paola Sapienza, and Luigi Zingales. "Social Capital as Good Culture." CEPR Discussion Papers 6657, C.E.P.R. Discussion Papers, 2008.

Hobbes, Thomas. *Leviathan*. Pinted for Andrew Crooke, at the Green Dragon in St Paul's Churchyard, 1651.

Hsiang, Solomon M. "Temperatures and cyclones strongly associated with economic production in the Caribbean and Central America." *Proceedings of the National Academy of Sciences* 107, 35: (2010) 15,367–15,372.

Kessler, Daniel P, and Steven D Levitt. "Using Sentence Enhancements to Distinguish between Deterrence and Incapacitation." *Journal of Law and Economics* 42, 1: (1999) 343–63.

Lane, Roger. "Urban Police and Crime in Nineteenth-Century America." *Crime and Justice* 15: (1992) 1–50.

Lang, Herbert O. *A History of Tuolumne County, California*. Tuolumne County Historical Society, 1983.

Levitt, Steven D. "Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime." *American Economic Review* 87, 3: (1997) 270–90.

McKanna, Clare V. *Homicide, Race, and Justice in the American West, 1880-1920*. Tucson: University of Arizona Press, 1997.

———. *Race and Homicide in Nineteenth-Century California. Reno and Las Vegas*. University of Nevada Press, 2002.

McLean, Ian. *Why Australia Prospered: The Shifting Sources of Economic Growth*. Princeton University Press, 2013.

Mehlum, Halvor, Karl Ove Moene, and Ragnar Torvik. "Institutions and the Resource Curse." *Economic Journal* 116, 508: (2006) 1–20.

Nisbett, Richard, and Dov Cohen. *Culture of Honor: The Psychology of Violence in the South*. Westview Press, 1996.

Pinker, Steven. *The Better Angels of Our Nature*. New York: Viking, 2007.

Rajan, Raghuram G., and Luigi Zingales. "The Persistence of Underdevelopment: Institutions, Human Capital or Constituencies." CEPR Discussion Papers 5867, C.E.P.R. Discussion Papers, 2006.

Ross, Michael. "A Closer Look at Oil, Diamonds, and Civil War." *Annual Review of Political Science* 9, 1: (2006) 265–300.

Roth, Randolph. *American Homicide*. Harvard University Press: The Belknap Press, 2009.

Ruggles, Steven, J. Trent Alexander, Katie Genadek, Ronald Goeken, Matthew B. Schroeder, and Matthew Sobek. "Integrated Public Use Microdata Series: Version 5.0." Minneapolis: University of Minnesota, 2010.

Silverman, Dan. "Street Crime And Street Culture." *International Economic Review* 45, 3: (2004) 761–786.

Skaperdas, Stergios. "Cooperation, Conflict, and Power in the Absence of Property Rights." *American Economic Review* 82, 4: (1992) 720–39.

Tabellini, Guido. "The Scope of Cooperation: Values and Incentives." *The Quarterly Journal of Economics* 123, 3: (2008) 905–950.

Torvik, Ragnar. "Why do some resource-abundant countries succeed while others do not?" *Oxford Review of Economic Policy* 25, 2: (2009) 241–256.

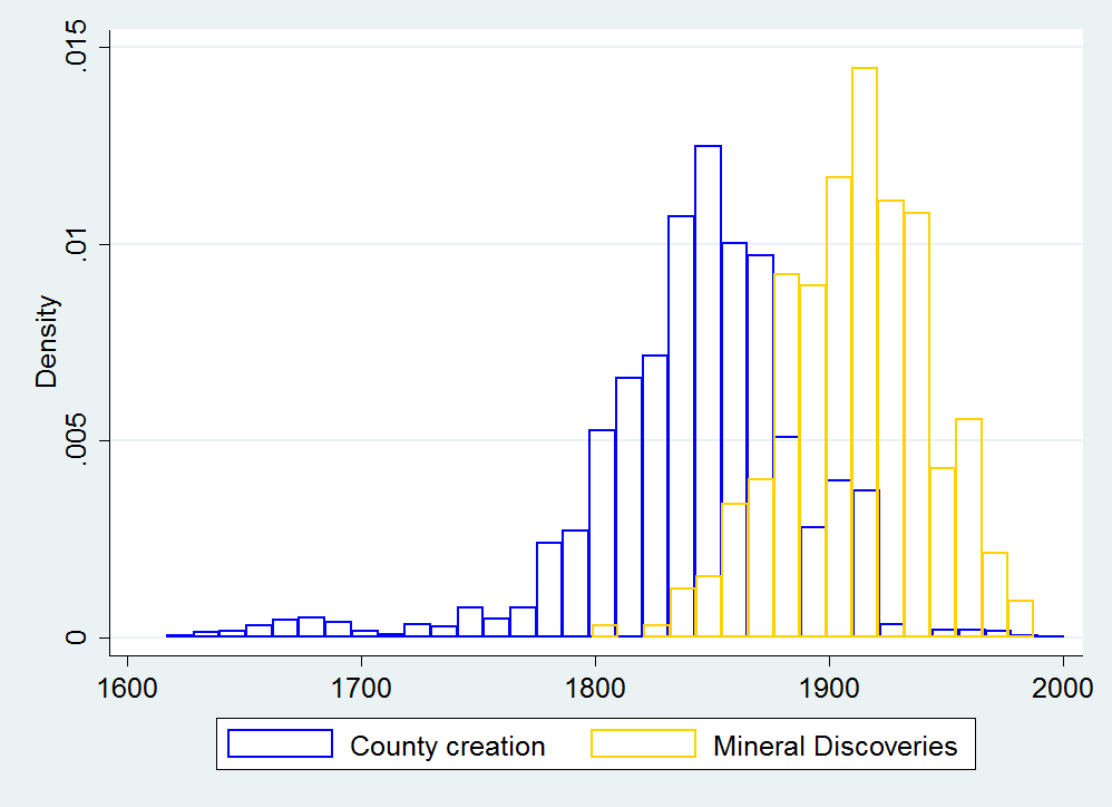
Umbeck, John. "The California Gold Rush: A Study of Emerging Property Rights." *Explorations in Economic History* 14: (1975) 197–226.

———. "Might Makes Rights: A Theory of the Formation and Initial Distribution of Property Rights." *Economic Inquiry* 19, 1: (1981) 38–59.

Wright, Gavin. "The Origins of American Industrial Success, 1879-1940." *American Economic Review* 80, 4: (1990) 651–68.

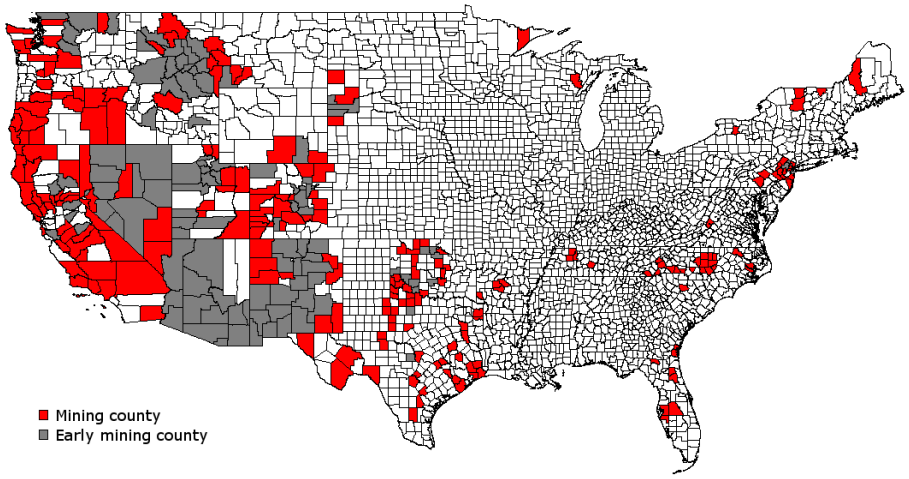
Zerbe, Richard O., and C. Leigh Anderson. "Culture And Fairness In The Development Of Institutions In The California Gold Fields." *The Journal of Economic History* 61, 01: (2001) 114–143.

Figure 1: Distributions of counties' incorporation dates and mineral discoveries.



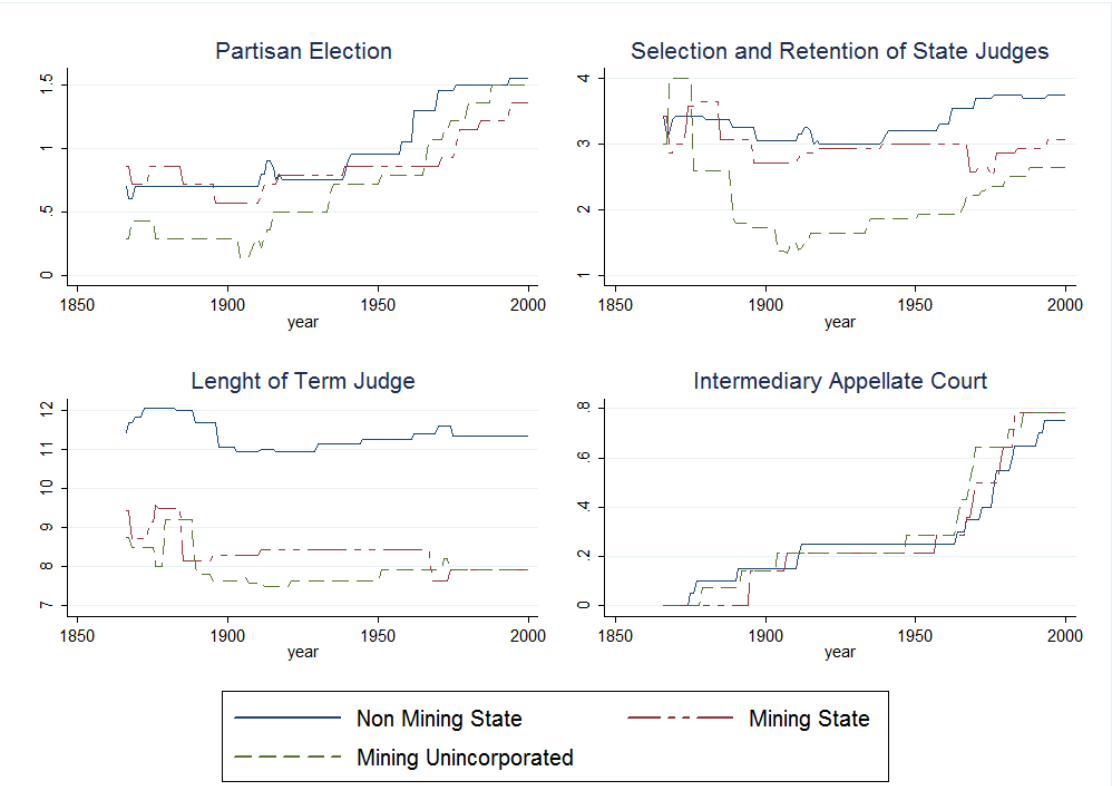
Histograms of the distributions of dates of county first creation (in blue) and dates of mineral discoveries (in yellow). Sources: National Association of Counties, and Mineral Resources Data System.

Figure 2: Map of US counties, distinguishing between mining and early mining counties.



Sources: Atlas of Historical County Boundaries and Mineral Resources Data System.

Figure 3: Institutional quality and mineral discoveries.



Sources: Epstein et al. (2001) and Mineral Resources Data System.

Table 1: Historical relationship between mining and homicides in a handful of counties.

Dependent variable: number of homicides in county i at time t			
	(1)	(2)	(3)
Stock of mines$_{it}$	0.187**	0.237***	0.260***
	(0.090)	(0.087)	(0.088)
Population (thousands)$_{it}$		0.278***	0.351***
		(0.043)	(0.050)
Stock of Mines$_{it}$ × Early mining$_i$			3.700***
			(0.922)
Observations	357	329	329
Adjusted R-squared	0.155	0.349	0.389

*** p<0.01, ** p<0.05, * p<0.1. White heteroskedastic standard errors in parentheses. OLS regressions. Each column presents the estimates from a separate regression. The dependent variable is the number of homicides in county i at time t. *Stock of mines* is the cumulative sum of mineral discoveries over the 1849–1895 period. *Early mining*$_i$ is a dummy variable equal to unity if the county i experienced mineral resources discoveries before land organization. All regressions include state fixed effects, year fixed effects, and a constant term.

Table 2: Relationship between mining history and today's violent crimes.

Dependent variable: rate of violent crimes per 100,000 inhabitants in 2000

	(1)	(2)	(3)	(4)	(5)	(6)
Mining county	6.29	40.09*	38.75*	-23.45	7.17	5.74
	(32.16)	(23.76)	(21.53)	(33.08)	(20.99)	(18.80)
Early mining				181.30**	202.53***	208.65***
				(70.04)	(58.25)	(60.11)
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Contemporary controls		Yes	Yes		Yes	Yes
Historical controls			Yes			Yes
Observations	2,015	2,015	2,015	2,015	2,015	2,015
Adjusted R-squared	0.28	0.39	0.40	0.28	0.40	0.41

*** p<0.01, ** p<0.05, * p<0.1. White heteroskedastic standard errors adjusted for clustering at the state level in parentheses. OLS regressions. Each column presents the estimates from a separate regression. 2,015 observations in each regression. *Violent crimes* include murders and aggravated assaults. *Mining county* is a dummy variable equal to unity if the county had any mining activity. *Early mining* is a dummy variable equal to unity if the county experienced mineral resources discoveries before land organization. All regressions include state fixed effects and a constant term. All regressions include the date of creation of the county and the following *Contemporary controls* measured in 2000: (log of) county's aggregate income, (log of) income per capita, shares of blacks and women in the population, fractionalization, Gini coefficient, population density, and urbanization rate. *Historical controls* include the following covariates: manufacturing wage, white and black literacy rates, and county's population in 1900, manufactured, agricultural and other farming products outputs, population density, and share of women in 1880, and dummies for the presence of any coal or oil/gas mine.

Table 3: Relationship between mining history and today's property crimes and other crimes.

Dependent variables: rate of different crimes per 100,000 inhabitants in 2000

	Property crimes (1)	Sex offense (2)	Drunk driving (3)	Forgery (4)	Vagrancy (5)
Mining county	-24.40	-0.89	10.23	0.13	-0.01
	(20.18)	(2.96)	(13.91)	(3.74)	(1.00)
Early mining	45.50	12.19	20.62	2.44	-0.67
	(48.15)	(7.97)	(27.89)	(5.84)	(2.55)
Observations	2,015	2,015	2,015	2,015	2,015
Adjusted R-squared	0.34	0.26	0.61	0.24	0.11

*** p<0.01, ** p<0.05, * p<0.1. White heteroskedastic standard errors adjusted for clustering at the state level in parentheses. OLS regressions. Each column presents the estimates from a separate regression. *Property crimes* include robbery, burglary, and larceny. *Mining county* is a dummy variable equal to unity if the county had any mining activity. *Early mining* is a dummy variable equal to unity if the county experienced mineral resources discoveries before land organization. All regressions include state fixed effects and a constant term. All regressions include all contemporary and historical covariates used in Table 2.

Table 4: Relationship between mining history and today's violent crimes: robustness checks.

Dependent variable: rate of violent crimes per 100,000 inhabitants in 2000

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Mining county	5.74	41.48**	24.33	45.17	36.86	42.61	64.01*		30.54	29.66
	(17.98)	(20.07)	(23.63)	(35.21)	(27.83)	(34.69)	(37.44)		(20.01)	(20.05)
Early mining	208.65***	168.49**	174.70**	205.41***	166.54***	109.67**		228.94***		
	(50.30)	(64.96)	(57.52)	(50.69)	(60.21)	(47.85)		(94,35)		
Discoveries 5 years after									30.60	
									(70.83)	
Discoveries 5 years before									112.34	
									(70.39)	
Discoveries 10 years after										34.68
										(74.67)
Discoveries 10 years before										111.51
										(67.96)
Correction for spatial correlation	Yes									
Only neighboring counties		Yes								
Only states with all types of counties			Yes							
Arbitrary groups (# of groups)				Yes (202)	Yes (47)	Yes (28)				
Sample for matching on observables							Full	Mining counties		
Observations	2,015	494	373	2,015	2,015	2,015	2,015	209	2,015	2,015
Adjusted R-squared	0.83	0.43	0.35	0.36	0.30	0.27			0.40	0.40

*** p<0.01, ** p<0.05, * p<0.1. White heteroskedastic standard errors adjusted for clustering at the state level in parentheses, except in column 1 where standard errors are computed using Hsiang (2010) with a 300 km spatial correlation, and in columns 7 and 8 where propensity score matching is used. OLS regressions except in columns 7 and 8 where propensity score matching is used. Each column presents the estimates from a separate estimation. *Violent crimes* include murders and aggravated assaults. *Mining county* is a dummy variable equal to unity if the county had any mining activity. *Early mining* is a dummy variable equal to unity if the county experienced mineral resources discoveries before land organization. All regressions include a constant term and state fixed effects, except in columns 3, 4, and 5 where states are replaced by 202, 47, and 28 arbitrary groups designed using 2−, 5−, and 7−degree grids. In these columns, standard errors are adjusted for clustering at the arbitrary group level. All regressions include all contemporary and historical covariates used in Table 2. Variables labeled *Discoveries x years before (after) organization* are dummy variables equal to unity if the county experienced mineral resources discoveries x years before (after) organization of the land.

Table 5: Relationship between mining and the quality of judicial institutions.

Dependent variables in columns' head

	Selection procedure		Retention	
	(1)	(2)	(3)	(4)
Mining_{st}	-1.75*	-0.06	-2.82*	-0.52
	(0.89)	(0.30)	(1.61)	(0.58)
$\text{Early mining}_s \times \text{Mining}_{st}$		-2.57***		-4.23**
		(0.96)		(1.75)
Observations	6,480	6,480	6,170	6,170
R-squared	0.66	0.66	0.85	0.85

	Length of terms		Appellate court	
	(5)	(6)	(7)	(8)
Mining_{st}	-3.00	-0.11	-0.45	0.68**
	(2.34)	(1.25)	(0.59)	(0.31)
$\text{Early mining}_s \times \text{Mining}_{st}$		-5.34**		-1.72**
		(2.58)		(0.70)
Observations	6,170	6,170	6,480	6,480
R-squared	0.90	0.90	0.67	0.67

*** p<0.01, ** p<0.05, * p<0.1. White heteroskedastic standard errors in parentheses. OLS regressions. Each column presents the estimates from a separate regression. *Selection procedure* is the selection procedure for state judges; *Retention* is an indicator of the independence of both selection and retention of state judges; *Length of terms* is the length of terms of state judges; and *Appellate court* is the presence of an intermediary appellate court. For each dependent variable, a higher value indicates higher judicial independence. $Mining_{st}$ is a dummy variable equal to unity if state s had any mining discovery at time t. $Early\ mining_s$ is a dummy variable equal to unity if state s experienced mineral resources discoveries before land organization. All regressions include state fixed effects, year fixed effects, and a constant term.

Appendix

Table A1: Relationship between mining and the quality of judicial institutions, including lagged discoveries.

Dependent variables in columns' head	Selection procedure	Retention	Length of terms	Appellate court
Mining_{st}	-0.31	-0.53	0.22	0.43***
	(0.23)	(0.36)	(0.56)	(0.15)
$\text{Mining}_{s,t-1}$	-0.04	-0.25	-0.04	0.34*
	(0.17)	(0.30)	(0.62)	(0.17)
$\text{Mining}_{s,t-2}$	0.11	-0.17	-0.38	0.24
	(0.13)	(0.27)	(0.72)	(0.14)
$\text{Mining}_{s,t-3}$	0.20	-0.07	-0.40	0.23
	(0.13)	(0.29)	(0.74)	(0.16)
$\text{Mining}_{s,t-4}$	0.41*	0.22	-0.48	0.09
	(0.21)	(0.28)	(0.66)	(0.14)
Early $\text{mining}_s \times \text{Mining}_{st}$	-1.50**	-2.30*	-3.56**	-1.09***
	(0.62)	(1.16)	(1.73)	(0.37)
Early $\text{mining}_s \times \text{Mining}_{s,t-1}$	-1.41***	-1.98**	-2.78*	-0.92***
	(0.51)	(0.94)	(1.44)	(0.34)
Early $\text{mining}_s \times \text{Mining}_{s,t-2}$	-1.34***	-1.66**	-1.85	-0.82**
	(0.47)	(0.80)	(1.20)	(0.31)
Early $\text{mining}_s \times \text{Mining}_{s,t-3}$	-1.60***	-2.51**	-2.02*	-0.90**
	(0.54)	(0.96)	(1.20)	(0.34)
Early $\text{mining}_s \times \text{Mining}_{s,t-4}$	-1.75***	-2.93***	-1.75	-0.78**
	(0.63)	(1.05)	(1.44)	(0.37)
Observations	6,476	6,166	6,166	6,476
R-squared	0.67	0.85	0.90	0.67

*** p<0.01, ** p<0.05, * p<0.1. White heteroskedastic standard errors in parentheses. OLS regressions. Each column presents the estimates from a separate regression. *Selection Procedure* is the selection procedure for state judges; *Retention* is an indicator of the independence of both selection and retention of state judges; *Length of terms* is the length of terms of state judges; and *Appellate court* is the presence of an intermediary appellate court. For each dependent variable, a higher value indicates higher judicial independence. $Mining_{st}$ is a dummy variable equal to unity if state s had any mining discovery at time t. $Early\ mining_s$ is a dummy variable equal to unity if state s experienced mineral resources discoveries before land organization. All regressions include state fixed effects, year fixed effects, and a constant term.

Table A2: Relationship between mining history and political competition at the national level.

Dependent Variable: Political competition in general elections		
	(1)	(2)
Mining_{st}	0.33***	0.44***
	(0.10)	(0.10)
Early $\text{Mining}_s \times \text{Mining}_{st}$		-0.19
		(0.11)
Observations	772	772
R-squared	0.55	0.55

*** p<0.01, ** p<0.05, * p<0.1. White heteroskedastic standard errors in parentheses. OLS regressions. Each column presents the estimates from a separate regression. $Mining_{st}$ is a dummy variable equal to unity if state s had any mining discovery at time t. $Early\ mining_s$ is a dummy variable equal to unity if the county s experienced mineral resources discoveries before land organization. All regressions include state fixed effects, year fixed effects, and a constant term.

Table A3: Summary statistics for the relationship between mining history and today's violent crimes.

	Full sample		Mining counties		Early mining counties	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
In 2000:						
Violent crimes per 100,000 inhabitants	520.82	329.05	623.34	323.42	655.79	341.88
Aggregate income (log of)	20.01	1.47	20.57	1.77	20.12	1.88
Per capita income (log of)	9.49	0.24	9.55	0.29	9.53	0.30
Share of blacks	0.10	0.15	0.05	0.09	0.01	0.01
Share of women	0.51	0.02	0.50	0.02	0.5	0.02
Fractionalization	0.25	0.18	0.33	0.18	0.29	0.16
Gini coefficient	0.43	0.04	0.43	0.03	0.43	0.03
Population density	232.3	1293.58	305.15	1120.09	124.88	291.42
Urbanization rate	0.43	0.30	0.56	0.30	0.54	0.32
Date of creation	1793.3	67.4	1817.37	63.78	1853.22	22.76
In 1900:						
Manufacturing wage	1012548	6560182	784613	2954666	252269	556000
White literacy rate	5368	12147	4427	7492	2567	2576
White literacy rate	412	816	175	412	7	15
Population	8538	24212	7061	12761	4192	4272
In 1880:						
Farming output	954936	1248379	444911	837506	192954	429809
Other farming products output	882071	917558	636151	1037276	292766	507688
Manufactured output	2330518	16154217	1540540	7536783	309740	600072
Population density	65.71	834.2	47.03	298.12	8	17.17
Share of women	0.47	0.06	0.41	0.10	0.36	0.11
Coal mine	0.05	0.23	0.04	0.19	0.05	0.22
Oil or gas mine	0.02	0.15	0.07	0.25	0.07	0.25
Observations	2,015		209		58	

See the text and notes of Table 2.