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FORCED COEXISTENCE AND ECONOMIC DEVELOPMENT: EVIDENCE FROM NATIVE AMERICAN RESERVATIONS

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Studying Native American reservations, and their historical formation, I find that their forced integration of autonomous polities into a system of shared governance had large negative long-run consequences, even though the affected people were ethnically and linguistically homogenous. Reservations that combined multiple sub-tribal bands when they were formed are 30% poorer today, even when conditioning on pre-reservation political traditions. The results hold with tribe fixed effects, identifying only off within-tribe variation across reservations. I also provide estimates from an instrumental variable strategy based on historical mining rushes that led to exogenously more centralized reservations. Data on the timing of economic divergence and on contemporary political conflict suggest that the primary mechanism runs from persistent social divisions through the quality of local governance to the local economic environment.

KEYWORDS: Economic development, population heterogeneity, political centralization, institutions, factional politics.

1. INTRODUCTION

MANY COUNTRIES AND OTHER, SMALLER JURISDICTIONS today are "artificial" in the sense that their political boundaries do not coincide with those desired by the people inside them, often because of historical circumstances like colonization (Alesina, Easterly, and Matuszeski (2011)). Their generally poor economic performance has most commonly been attributed to ethnic or linguistic cleavages (Easterly and Levine (1997), Alesina, Devleeschauwer, Easterly, Kurlat, and Wacziarg (2003)). However, recent research has highlighted that artificial jurisdictions often integrated people who not only differed in language and ethnicity, but who also—perhaps more importantly—had no history of shared and centralized governance (Gennaioli and Rainer (2007), Michalopoulos and Papaioannou (2013)). These separate forces are often difficult to untangle in the data, especially in cross-country analyses. This paper contributes to that debate by showing that imposing a common jurisdiction onto people with no history of shared governance can have significant negative economic consequences, even when the affected population is ethnically and linguistically homogenous. This is important because it suggests that historical social divisions can persist and have long-run effects, even when they are not tied to easily observable characteristics like ethnicity.

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Reservations lend themselves to studying this question because of the unusual mapping of ethnic groups (tribes) into present-day jurisdictions. The U.S. government did not combine different tribes when forming reservations, in order to avoid open conflict (Fahey (1986)). Reservations are therefore ethnically and linguistically homogenous in the sense that they are uniquely associated with a single tribe. However, while tribes had a shared cultural identity, this did not imply shared governance. Tribes were connected by language and intermarriage, but often *not* by any coordinated political decision making (Driver (1975), Diamond (1997, Chapter 14)). Instead, tribes were collections of subtribal bands, and decisions were made at the band level. In most cases, a reservation's formation was the first time its constituent bands were thrust into a system of shared governance (Stern (1965, p. 75)). I operationalize this notion with a binary measure for *forced coexistence*, indicating whether a reservation integrated multiple bands or was instead formed out of a single band. Because *some* tribes did have a historically more integrated political structure in that some decisions were made collectively, I study the effect of forced coexistence *conditional* on a reservation's constituent tribe's *historical centralization*, taken from the Murdock (1967) *Ethnographic Atlas* (EA).[2]

In ordinary least squares (OLS) regressions, I find that forced coexistence decreases per capita incomes on reservations today by about 30%, controlling for historical centralization. This effect is highly significant and robust to a range of controls, including per capita income and unemployment in the surrounding counties, distance to the nearest major city, reservation lands' ruggedness and size, population size, demographic structure, and casino operations. I also include the four measures of historical tribal organization that are consistently available in the EA.[3] Finally, I flexibly control for the remaining unobservable differences in reservations' economic and regulatory environment with state fixed effects. However, the coefficient on forced coexistence is remarkably robust to all these controls, showing little evidence of selection on observable characteristics.

There is, however, a potential concern about selection on unobservable characteristics. Comparing the Cherokee, who had indigenous Harvard-trained lawyers (Ehle (1997)), with the North-Pacific tribes, many of which had to rely on missionaries as translators during treaty negotiations (Colson (1977)), suggests that unobservable tribe characteristics, which could have had a direct impact on economic development, may have also played an important role in the

[2]Historical centralization is a binary measure of whether a tribe's local bands were historically politically autonomous, or centrally governed. This is called the *level of jurisdictional hierarchy beyond the local community* in the EA, but I follow the labeling in Gennaioli and Rainer (2007) and Michalopoulos and Papaioannou (2013).

[3]These four measures are subsistence patterns, sedentariness, wealth distinctions, and the social complexity of local communities. Other EA measures such as historical population density and slavery, used in Fenske (2014), are mostly unavailable for North American ethnic groups.

formation of reservations. I address this concern in two ways. First, I run all regression specifications with tribe fixed effects, identifying only off within-tribe variation in forced coexistence across reservations.[4]

Second, I pursue an instrumental variable (IV) strategy based on historical mining as a source of exogenous variation in the value of the land that each reservation's constituent bands occupied. Importantly, a reservation's *ancestral homelands* are spatially separate from both the reservation itself and from its present-day economic environment. The logic of the IV strategy is that more valuable Native lands led the government to form fewer, more concentrated reservations in order to free up more land and to better monitor tribes, in order to prevent them from spilling back onto their ancestral homelands (Fahey (1986)). I overlay maps of ancestral homelands with mining maps from historical Census volumes to calculate the value of precious-metal mining activity on each reservation's constituent bands' ancestral homelands from 1860 to 1890, and I use this as an instrument for forced coexistence.[5]

I discuss and investigate the instruments' validity. To address potential direct effects of the instruments on reservations' economic environment, I separate historical mining in a reservation's present-day economic environment from the instrument, which is mining on a reservation's ancestral homelands. To address selection into mining, I control for land characteristics of ancestral homelands. I also address the possibility that reservation formation was more violent where mining was taking place. First, I include distance between a reservation and its constituent bands' ancestral homelands to control for a possible shock associated with being removed farther. Second, I use anthropometric data on 12,000 Native Americans collected in the 1890s to directly investigate whether the process of reservation formation was more violent in the presence of a mining rush. I find that growing up during one's reservation's formation had a negative impact on the affected cohorts' long-run height, but this effect was not more severe when interacted with the instruments.

The three estimation frameworks make different identifying assumptions. OLS identifies the causal effect of forced coexistence under the assumption of selection only on observables. The specification with tribe fixed effects allows for selection on unobservable characteristics at the tribe level. The IV strategy allows for selection on unobservable characteristics at the sub-tribe band level but assumes that these unobservables were orthogonal to the instruments, at least conditional on observable characteristics. Despite the different identifying assumptions, the overall results are qualitatively identical in significance

[4]There are 182 reservations and 78 tribes in the data. Of the 78 tribes, 46 have only one reservation, so that the tribe-fixed-effects specification effectively identifies off variation in outcomes of 136 (182 − 46) reservations across 32 (78 − 46) tribes.

[5]The spatial variation in historical mining is richer across than within tribes' territories so that I am underpowered when I include tribe fixed effects in the IV. Instead, I condition on a rich set of observable tribe and reservation characteristics as well as state fixed effects.

and magnitude across all three frameworks. The evidence therefore supports the assumption of quasi-random assignment of forced coexistence and suggests that the estimates identify a causal effect.

Next, I turn to the mechanisms underlying the reduced form estimates. I present three pieces of evidence that support a political-economy explanation centered on the quality of local governance.[6] First, I turn to panel data, taking advantage of the fact that it was only in the late 1980s that federal regulation ceded responsibility for reservation governance from the Bureau of Indian Affairs (BIA) to the local reservation governments. Using a reservation-fixed-effects estimation strategy, I find that the effect of forced coexistence was stable and relatively small from 1970 to 1990, but that two-thirds of the cross-sectional 30% effect in 2000 was due to divergence from 1990 to 2000, when local governance began to really matter on reservations.

Second, to glean further evidence on the local governance channel, I investigate the relationship between forced coexistence and political conflict directly, both in the cross-section and in panel data. In the absence of measures of actual conflict, I use the *Proquest* newspaper database to generate counts of reported episodes of internal political conflict, conditioning on overall news coverage. I collect separate counts for conflict involving and explicitly not involving local governance institutions. I also collect counts of episodes of reported corruption in the same way. Consistent with the local governance mechanism, I find that forced coexistence significantly increases reported episodes of both internal conflict and corruption. The effect is stronger for conflict that involves local governance institutions. I also find that, mirroring the income results, the effects are significantly stronger after 1990. In combination, these two sets of results strongly suggest a local governance explanation of the reduced form estimates.

Third, I study the economic mechanisms linking local governance to reservation incomes. Qualitative accounts suggest the mechanism involves factional reservation politics, which lead to uncertainty in the local contracting environment and under-investment because politicians control the tribal courts adjudicating on-reservation legal conflicts (Jorgensen and Taylor (2000), Cornell (2006), Cornell and Kalt (2008, Chapter 7)). While I cannot test this directly, I investigate a range of outcomes that favor this explanation over the two main alternatives, namely federal transfers and the degree of reservations' integration into the surrounding off-reservation economy. I find that forced coexistence significantly reduces aggregate wage income, employment, and salaries. On the other hand, forced coexistence has no impact on transfer income or on

[6]This explanation is suggested by qualitative accounts that link income disparities across reservations to differences in local reservation politics (Cornell and Kalt (1995, 2008), Nichols (2003)). These qualitative accounts have also linked political conflict to social divisions rooted in historical tribal structures. This idea lies at the heart of this paper.

measures of reservations' residents' integration into the off-reservation economy. The evidence therefore supports the view that the sources of the observed income differences across reservations are mainly internal to the reservations.

This paper speaks to a large literature documenting the negative consequences of ethno-linguistic fractionalization and artificial jurisdictional boundaries (Easterly and Levine (1997), Alesina, Easterly, and Matuszeski (2011)). This literature suggests that ethnic or linguistic divisions can lead to the underprovision of public goods (Alesina, Baqir, and Easterly (1999)), lower community participation (Alesina and La Ferrara (2000)), more free-riding (Vigdor (2004), Miguel and Gugerty (2005)), less trust (Nunn and Wantchekon (2011)), and lower-quality politicians (Banerjee and Pande (2009)). However, recent research also emphasizes that ethno-linguistic fractionalization correlates with whether ethnic groups had a history of shared governance, which could matter independently (Gennaioli and Rainer (2007)). In relation to this literature, I find that forcing people with no history of shared governance into shared jurisdictions can have large negative long-run consequences, even when the affected people are ethnically and linguistically the same.[7] This is an important finding, because it suggests that social divisions can persist and have long-run economic legacies, even when they are not anchored in observable characteristics like ethnicity and language.

While it is not the focus of this paper, I do find a significant positive legacy of ethnic groups' (tribes') historical political centralization across all specifications. Since this partial correlation may not be a causal effect, I view this evidence simply as lending empirical support to existing findings on the positive legacy of historical state capacity and centralization (Bockstette, Chanda, and Putterman (2002), Gennaioli and Rainer (2007), Acemoglu and Robinson (2012), Michalopoulos and Papaioannou (2013), Acemoglu, García-Jimeno, and Robinson (2014)).[8] In addition, the finding that forced coexistence determined 150 years ago mattered much more in 2000 than it did in 1980 is relevant to an ongoing debate about the discontinuous "start-stop" nature of economic growth (Jones and Olken (2008)). Specifically, this finding is consistent with the view that historically determined differences in social organization can persist relatively unnoticed for long periods, before they have large and relatively sudden effects on economic outcomes when they interact with big shocks (Acemoglu, Johnson, and Robinson (2002), Nunn (2008)). In this instance, the big external shock is that regulatory changes in the late 1980s gave decision-making powers to the reservation governments (Cornell and Kalt (2008)).

The rest of this paper proceeds as follows: Section 2 provides background on the history of reservations and on the measurement of forced coexistence. Sec-

[7]I cannot directly relate the magnitude of my estimated effects to the existing estimates on the effect of ethno-linguistic fractionalization, since I have a binary rather than an index variable.

[8]Given the joint distribution of forced coexistence and historical centralization, I cannot distinguish whether historical centralization primarily works through mitigating the negative effect of forced coexistence, or through other more direct channels. This is discussed in Section 2.3.

tion 3 presents the estimation framework for the effect of forced coexistence. Section 4 provides evidence on the mechanisms. Section 5 concludes.

2. RESERVATION FORMATION AND MEASURING FORCED COEXISTENCE

2.1. *The Process of Reservation Formation*

The first Native American reservations were formed in the 1850s, after the annexation of Texas (1845), Oregon (1846), and California (1848) "changed the situation in the West almost overnight" and made the policy of Indian westward removal untenable (Nichols (2003, p. 128)). The overwhelming majority of present-day reservations had been formed by the 1890s. The government's objective in forming reservations was to maximize the amount of land that could be freed up for settlement and to be able to easily monitor Native Americans once they were on the reservation.[9] The U.S. government avoided integrating different tribes onto shared reservations because this was likely to lead to open violence and conflict (Fahey (1986)). This means that each reservation is mapped to a unique tribe and, in that sense, ethnically and linguistically homogenous. Because many larger tribes obtained several reservations, the mapping from reservations to tribes in the data is many-to-one.

2.2. *Reservations Today*

The 2000 U.S. Census reports reservation-level averages for 210 reservations.[10] From this population, I drop 12 reservations that are not federally recognized. I also drop nine reservations that were founded after the 1934 Indian Reorganization Act, since obtaining a reservation after this date involved a lengthy application process. Lastly, I drop seven reservations that combined more than one tribe, since these cannot be clearly mapped to the EA data.[11] This leaves a sample of 182 reservations, covering over 90% of the on-reservation Native American population. Figure 1, which displays the 182 reservation centroids, gives a general sense for their spatial dispersion.[12] Fig-

[9]The most important reason was to prevent them from leaving the reservation. The majority of late-19th-century armed conflicts between Native Americans and the U.S. Army were fought when tribes attempted to leave their reservations (Heard (1987), Michno (2003)).

[10]There are 304 reservations today, but the Census reports data only for reservations above a population threshold of 100. Reservations below this threshold primarily have their origin in the 1887 General Allotment Act (or Dawes Act), which broke up communal ownership of reservation lands by allotting each Indian family an area of 160 acres and putting the (substantial) remainder up for sale. An unintended side effect was the formation of clusters of 160-acre plots of non-reservation Native Americans. Many of these clusters were later turned into these small reservations.

[11]Five of these are in Oregon, which appears like a local idiosyncracy in reservation formation.

[12]This map is based on 2000 U.S. Census shapefiles. One feature of the map that is worth explaining is that Oklahoma has no reservations despite having the highest density of Native

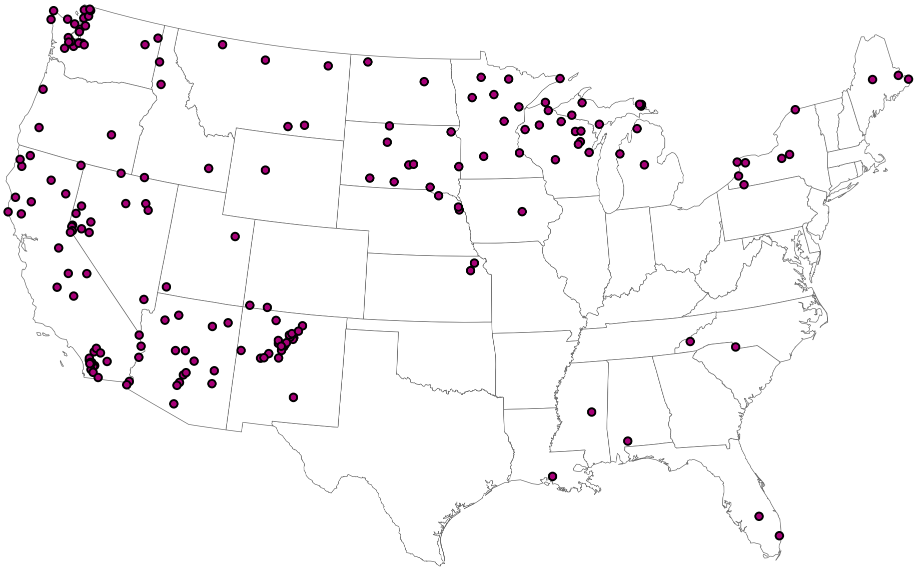

FIGURE 1.—U.S. reservation centroids.

ure 1 clearly shows that spatial variation in the economic environment or in state regulation could be potentially important, and needs to be controlled for.

2.3. *Measuring Forced Coexistence and Historical Centralization*

The key variation in the data is whether a reservation was formed at the level of the local community, that is, the band. If a reservation was formed above the level of the local community, that is, from a collection of bands, I say it suffers from *forced coexistence*. For illustration, consider Figure 2, which shows the eight bands that constituted the Apache tribe. The Chiricahua, Mescalero, and Lipan bands were combined onto the Mescalero Reservation. The Tonto, Cibecue, San Carlos, and White Mountain bands were combined onto the San Carlos Reservation. Only the Jicarilla Apache obtained their own reservation. In my coding, the Mescalero and San Carlos Reservations suffer from forced coexistence, and the Jicarilla Reservation does not.

I coded forced coexistence based on several sources of information about the formation of reservations. The most readily available sources are the en-

Americans today. During the pre-1850 *Indian Removal* policy, most Eastern tribes were removed to *Indian Territory*, which is roughly Oklahoma. When Oklahoma declared statehood in 1907, all tribes in Oklahoma lost their reservations (Prucha (1970, Chapter 9), Satz (2002), Unrau (2007)).

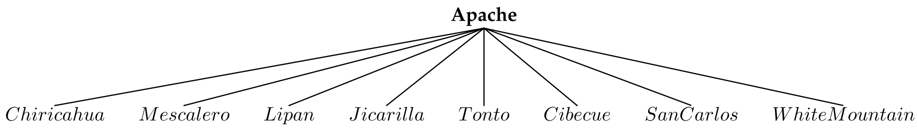

FIGURE 2.—The bands, that is, local communities, of the Apache tribe.

cyclopedias of reservations in Tiller (1996) and the multi-volume *Smithsonian Handbook of Native Americans* (Sturtevant (1981)). In addition, the University of Oklahoma maintains an online database that contains all treaties with Native Americans, many of which underpinned the formation of reservations and included a list of signatory bands. Most reservations also have websites that explicitly describe their historical origins, and many historical and contemporaneous studies of individual reservations further verify the obtained coding. All sources are listed in the Supplemental Material (Dippel (2014)).

Clearly, tribes exhibited various degrees of historical centralization, and centralized tribes were probably more likely to end up on reservations formed above the level of the local community, either voluntarily because they negotiated treaties as a tribe or involuntarily because the government viewed more concentrated reservations as more appropriate for them (Opler (1983, p. 369), Ehle (1997)). This potential correlation matters because the existing literature suggests that forced coexistence and historical centralization are likely to have opposing effects on long-run development, the former being a measure of heterogeneity and the latter proxying for historical development. Table I displays a clear positive correlation between forced coexistence and historical centralization. Of the 38 reservations associated with historically centralized tribes, 35 were formed above the level of the local community, that is, through forced coexistence.[13,14] The 78 reservations that suffer from forced coexistence and whose tribes were not historically centralized have clearly lower per capita incomes. By contrast, the 35 reservations characterized by both forced coexistence and historical centralization do not fare significantly worse than reservations without forced coexistence.[15] It is clear from Table I that controlling for historical centralization is essential.

[13]The other three are bands that obtained their own reservations despite belonging to more centralized tribes. The Tonawanda, for example, are a band of the Seneca, and obtained their own reservation.

[14]Breaking the data down by tribe instead of by reservation reveals that 21 out of 78 tribes had a level of historical centralization of 1.

[15]One possible interpretation is that having a reservation combine different local communities was only detrimental when these local communities had no history of shared governance. Because Table I is underpopulated in the bottom-left, it is not possible to empirically distinguish this interpretation from one where combining different local communities was always bad and historical centralization was good for other reasons. The qualitative evidence is also not clear on whether historical centralization made the joint governance of multiple bands any easier in the

TABLE I

JOINT DISTRIBUTION OF FORCED COEXISTENCE AND HISTORICAL CENTRALIZATION[a]

	Forced Coexistence = 0		Forced Coexistence = 1		
	Number	log(p.c. income)	Number	log(p.c. income)	Total
Historical centralization = 0	66	9.34 (0.41)	78	8.98 (0.27)	144
Historical centralization = 1	3	9.58 (0.39)	35	9.26 (0.32)	38
Total	67		115		182

[a]The total number of reservations is 182. For each of the four cells, the table reports the number of reservations as well as the mean and standard deviation (in parentheses) of log(per capita income). If the data are broken down by tribe instead of by reservation, 57 tribes have a level of historical centralization of 0 and 21 of 1. (There are 78 tribes in the data.)

In Table I historical centralization is binary, but the EA actually codes historical centralization in levels that a tribe's governance structure is removed from its local communities: 0 means "no political authority beyond the local community," 1 means "petty chiefdom," 2 means "larger chiefdom," 3 means "state," and 4 means "large state." The Aztec and Inca are examples of large states.[16] All North American tribes in the EA are coded as 0, 1, or 2, with more than two-thirds coded as 0.[17] Because my data contain more 0's than 1's and 2's combined, I follow Gennaioli and Rainer (2007) in treating historical cen-

context of the modern-day governance arrangement on reservations (Cornell and Kalt (1993)). This is because historically, even in centralized tribes, agreements were typically reached only once complete unanimity was achieved (Crawford (1994)), an arrangement that is very different from the majority-will practiced on reservations today.

[16]The Apache in Figure 2 are coded as having had "no political authority beyond the local community," but their flat tribal structure also meant that they could not constitute anything more centralized than a "petty chiefdom" in the EA, because political authority could be at most one level removed from the local bands. For tribes to exhibit higher levels of historical centralization, they needed to exhibit more complex sub-tribal structures. To illustrate, for the Iroquois, which are coded as 2, that is, a "larger chiefdom," the equivalent of Figure 2 exhibits an intermediate level below that of the tribe. At that intermediate level are the sub-tribal units Mohawk, Oneida, Onondaga, Cayuga, Seneca, and Tuscarora, each of which combined several local communities.

[17]Of the 78 tribes in the data, 57 are coded as 0, 16 as 1, and 5 as 2. Tribes coded as 2 include the Cherokee, Creek, and Iroquois. Tribes coded as 1 include the Arapahoe, Cheyenne, and Crow. The EA's coding accords very well with individual anthropological studies. For example, Spier (1923, p. 298) argued that for the Diegueno, coded as having a historical centralization of 0, the "real unit is the local group." For the Apache, also coded as 0, Opler (1983, p. 369) argued that "the notion of tribe in Apachean cultures is very weakly developed. Essentially it was only a recognition that one owed a modicum of hospitality to those of the same speech, dress, and customs."

tralization as a binary variable, which takes a value of 1 if the level of historical centralization is 1 or above.[18]

3. THE EFFECT OF FORCED COEXISTENCE

3.1. *Estimation Strategy*

The baseline estimating equation is

$$\mathbf{y}_{ie} = \alpha + \beta\mathbf{FC}_{ie} + \gamma_1\text{res-controls}_{ie} + \gamma_2\text{tribe-controls}_{e} + \varepsilon_{ie}. \tag{1}$$

The outcome variable $\mathbf{y}_{ie}$ is the log of per capita income on reservation i of tribe (ethnicity) e and the main regressor of interest is reservation i's forced coexistence $\mathbf{FC}_{ie}$, conditional on vectors of reservation-controls and tribe-controls. The error term ε_{ie} is likely to have a component that is common to all reservations of a tribe, that is, $\varepsilon_{ie} = \eta_e + \upsilon_{ie}$. Standard errors are therefore clustered at the level of the tribe. Specification (1) identifies the causal effect of $\mathbf{FC}_i$ if selection into $\mathbf{FC}_i$ was essentially random, conditional on observables.[19] However, the historical record does suggest substantial *unobservable* differences across Native American tribes, for instance in their ability to negotiate favorable treaty terms (Colson (1977), Ehle (1997)). Such unobservables may well correlate with both $\mathbf{FC}_{ie}$ and $\mathbf{y}_{ie}$. Under the assumption that only tribe-level unobservables affected selection into $\mathbf{FC}_{ie}$, a specification with tribe fixed effects α_e estimates the causal effect of $\mathbf{FC}_i$, identifying only off within-tribe variation across reservations:

$$\mathbf{y}_{ie} = \alpha_e + \beta\mathbf{FC}_{ie} + \gamma_3\text{res-controls}_{ie} + \varepsilon_{ie}. \tag{2}$$

Lastly, it is possible that there was selection into $\mathbf{FC}_i$ based on unobservable band characteristics, even conditional on observable tribe characteristics. For instance, Morgan (1962) suggested that the Tonawanda band of the Seneca were the only Iroquois band that obtained a separate reservation precisely because they were particularly keen on preserving their culture and governing themselves separately. I address this concern with an IV strategy based on historical mining rushes, which exogenously changed the government's incentives to form more centralized reservations. The IV estimates identify a causal

[18]Because the African data have more ethnic groups that are centralized, Gennaioli and Rainer (2007) defined historical centralization as a binary variable that takes a value of 1 if the level of jurisdictional hierarchy is 2 or above.

[19]Under this assumption, forced coexistence would have been determined by local variation in settler pressure and attitudes of government representatives and soldiers (Nichols (2003, p. 128), Stammel and Josef (1989)).

effect of $\mathbf{FC}_i$ if historical mining on tribes' *ancestral homelands* was orthogonal to their unobservable characteristics and had no independent effects on a reservation's economic development.[20] I discuss the IV logic and validity in Section 3.5.

3.2. *Controls*

Reservations today can be highly integrated into their economic environment, which is potentially a market for both their output and their labor (Cornell and Kalt (2008)). I therefore include three controls for both market access and employment opportunities: surrounding counties' average per capita income, surrounding counties' unemployment rate, and reservations' distance to the nearest major city. The type and amount of land available to a reservation was determined at the same time as forced coexistence, so I control for both of these. In addition, I can control for some observable reservation-internal characteristics: population size, demographic structure, and a dummy for casino-operations. The Supplemental Material describes in more detail how these variables were constructed as well as their sources. Lastly, I control for the four consistently available historical tribe characteristics from the EA: the share of calories from agriculture, sedentariness, the social complexity of local communities, and whether a tribe had wealth or nobility distinctions.[21]

Table II investigates selection on observables by regressing each control on forced coexistence. Since the cultural anthropology literature suggests a correlation between historical centralization and other tribe characteristics (Driver (1975)), I regress controls on forced coexistence first with no other controls and then with historical centralization included as a control. In Panel A, I investigate the characteristics of reservations and proxies of their economic environment. They appear fairly balanced. Only per capita incomes in the surrounding counties appear somewhat lower with forced coexistence, although not at conventional significance levels, with a t-statistic of -1.417. Reservations with forced coexistence do not appear to be on worse land, and there are no apparent size differences. Conditioning on historical centralization reduces the already insignificant differences further. In Panel B, I focus on tribe characteristics from the EA. There is a strong correlation between forced coexistence

[20]The variation in the instruments within tribes across reservations is rich enough to allow controlling for all observable tribe characteristics but not for tribe fixed effects.

[21]These are variables 5, 30, 32, and 66 in the EA. Historical subsistence patterns and sedentariness could matter because of potentially long-lasting adjustment costs from forcing the sedentary and agricultural reservation life onto tribes (Ballas (1987, pp. 18–21)). As well, more social complexity could proxy for more social capital, while historical class stratification could be an alternative source of persistent tribe-internal conflict (Gennaioli and Rainer (2007)).

TABLE II
BALANCING TESTS[a]

	Regressors					
	Forced Coexistence Only			Forced Coexistence, Conditional on Historical Centralization		
Dependent (Below)	Coeff.	*t*-Stat.	R^2	Coeff.	*t*-Stat.	R^2
Panel A: Reservation Characteristics						
Surround. p.c. income	−0.051	(−1.417)	0.024	−0.042	(−0.986)	0.030
Surround. p.c. unempl.-rate	−0.028	(−0.646)	0.003	−0.003	(−0.046)	0.025
Distance to major city	0.083	(0.321)	0.002	0.072	(0.260)	0.002
log(Ruggedness, Reserv.)	−0.140	(−0.646)	0.003	0.086	(0.427)	0.066
log(Re-Area in sqkm)	0.182	(0.291)	0.001	0.218	(0.292)	0.001
Panel B: Tribe Characteristics						
Historical centralization	0.266***	(2.973)	0.101	—	—	—
Percent calories from agriculture	−1.021	(−0.737)	0.037	−1.456	(−1.059)	0.096
Sedentariness	−0.290	(−0.323)	0.005	−0.616	(−0.699)	0.062
Complexity of local community	0.075	(0.657)	0.006	0.013	(0.095)	0.041
D(Wealth distinctions)	0.211	(1.469)	0.025	0.318	(1.601)	0.080
Panel C: Endogenous Reservation Controls						
log(Population)	0.501*	(1.896)	0.038	0.450	(1.417)	0.041
Pop-Share Adult (0–100)	−3.495***	(−2.929)	0.086	−3.781***	(−2.944)	0.092
D(Casino)	0.029	(0.319)	0.001	0.008	(0.085)	0.005

[a]Each row reports on a separate regression of one control variable on forced coexistence. Each row reports the same regression in two different specifications: first, with forced coexistence as the only regressor, then additionally controlling for historical centralization. In both specifications, I report only the coefficient on forced coexistence. The number of observations is 182 in each regression. The t-statistics are for standard errors that are two-way clustered at the tribe and the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

and historical centralization, as suggested in Table I. Among other tribe characteristics, only the indicator for wealth distinctions appears higher with forced coexistence, although again not at conventional significance levels.[22]

Panel C shows that the potentially endogenous reservation-internal controls appear, not surprisingly, the least balanced. A positive correlation between population size and forced coexistence is intuitive since several bands should be larger than one. While this becomes insignificant once historical centralization is controlled for, the difference is still large in magnitude. I therefore include both population and the square of population in the regressions to control for any scale effects. The share of the population below working age is significantly higher with forced coexistence in both specifications, perhaps because residents of less well-off reservations have more children, in part because welfare payments contribute to their income (Cornell and Kalt (2008)). There

[22]This correlation is driven by the Pacific fishing tribes, which were the only tribes with a hereditary nobility and which typically ended up with multi-band reservations.

TABLE III

OLS AND TRIBE FIXED-EFFECTS RESULTS[a]

	log(per capita income)				
Dependent	(1)	(2)	(3)	(4)	(5)
	Panel A: OLS				
Forced coexistence	-0.358^{***}	-0.334^{***}	-0.364^{***}	-0.302^{***}	-0.291^{***}
	(-3.662)	(-4.090)	(-7.192)	(-4.913)	(-5.072)
Historical centralization	0.278^{***}	0.304^{***}	0.351^{***}	0.313^{***}	0.282^{***}
	(3.887)	(4.812)	(5.274)	(4.875)	(3.803)
R^2	0.212	0.360	0.393	0.457	0.600
	Panel B: Tribe Fixed Effects				
Forced coexistence	-0.401^{***}	-0.318^{***}	—	-0.274^{***}	-0.276^{**}
	(-3.095)	(-3.965)	—	(-3.559)	(-2.275)
R^2	0.596	0.652	—	0.682	0.757
Reservation controls		Y	Y	Y	Y
Tribe controls			Y	Y	Y
Additional reservation-controls				Y	Y
State fixed effects					Y

[a] $N = 182$ observations in all regressions. Reservation-controls are surrounding-county. p.c. income and unempl.-rate, distance to the nearest major city, log(Ruggedness) and log(Res-area). Tribe-characteristics are subsistence patterns, sedentariness, wealth distinctions, and social complexity of local communities. Additional reservation-controls in column 4 are log(Population), log(Population-squared), adult population-share and D(Casino). t-statistics reported for two-way clustered standard errors, at tribe and state level. Column 3 of Panel B is the same as column 2 because EA characteristics are not identified with tribe fixed effects. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

is no correlation between casinos and forced coexistence, perhaps because the social costs of running gambling operations deter some successful reservations from opening casinos (Evans and Topoleski (2002)).

3.3. *Ordinary Least Squares Estimates*

Table III, Panel A shows the results of estimating specification (1), incrementally including the control variables. Without any other controls, column 1 shows that forced coexistence reduces per capita income by about 35% while historical centralization raises income by about 30%. To conserve space, I defer reporting the coefficients on controls to the Supplemental Material. The exception is historical centralization, which is of independent interest in the literature (Bockstette, Chanda, and Putterman (2002), Gennaioli and Rainer (2007), Acemoglu and Robinson (2012), Michalopoulos and Papaioannou (2013)). Overall, the coefficients on controls have the expected signs: Surrounding counties' per capita income raises on-reservation income and distance to the nearest major city lowers it. Geographic characteristics and size do not appear important. The four tribe characteristics from the EA (introduced in column 3) are not significant. Of the endogenous reservation controls (introduced in column 4), only the adult-share of the population has a significant (and positive) impact. The inclusion of state fixed effects (in column 5) raises the R^2 considerably, indicating that they soak up considerable residual variation in reservations' economic and regulatory environment. Overall, the estimated coefficient on forced coexistence appears remarkably robust to all these controls, decreasing only slightly (in absolute terms) from about -35% to -30%. Furthermore, this robustness is meaningful in the sense that the R^2 increases markedly as more controls are added across columns.

3.4. *Tribe Fixed Effects*

Despite this robustness, unobservables may nonetheless introduce a serious bias. In Panel B, I therefore estimate specification (2) with tribe fixed effects. The inclusion of tribe fixed effects provides a causal estimate of forced coexistence if selection occurred on unobservables only at the tribe level. The R^2 in Panel B clearly shows that tribe fixed effects are very important predictors of per capita reservation incomes.[23] However, the coefficient on forced coexistence changes only marginally from Panel A to Panel B. This implies that if selection on unobservable characteristics is important, it does not appear to occur at the tribe level. In the next section, the IV therefore allows for the possibility of selection on unobservables at the band level, conditional on observable tribe characteristics.

3.5. *Instrumental Variable Strategy*

3.5.1. *Logic of Historical Mining as an Instrument*

Where the U.S. government deemed Native American lands more valuable it formed fewer, more concentrated reservations, in order to free up more land and to better monitor tribes to prevent them from migrating back onto their ancestral homelands (Fahey (1986)).[24] The value of land that tribes occupied was therefore a key determinant of forced coexistence. Many determinants of land value, such as suitability for grazing and agriculture, cannot plausibly be thought of as exogenous because they were important for Native Americans, too. By contrast, the location of historical mining activity was essentially

[23] Panel B includes all previous reservation controls, but leaves all EA variables unidentified because there is no within-tribe variation in the EA variables. Forty-six of the reservations are mapped to only one tribe, and there are 78 tribes altogether. This implies Panel B identifies off outcome variation of 136 (182 – 46) reservations across 32 (78 – 46) tribes. Figure 1 in the Supplemental Material illustrates within-tribe variation by highlighting the Chippewa reservations inside a residual regression plot.

[24] Warren (2009), for example, described how pressure from lumbermen and farm settlers induced the government to move the bands of the Mississippi and Pillager Chippewa to the integrated Leech Lake, Mille Lacs, and White Earth Reservations, while the bands of the Lake Superior Chippewa, on the colder and less fertile lands farther north, retained separate band-based reservations.

random because Native North Americans, unlike, say, the Inca, did not have mining. I do, however, control for ruggedness on each reservation's ancestral homeland—because ruggedness correlated with mining and may have correlated with unobservable tribe characteristics—in addition to including all observable tribe characteristics in all IV regressions.[25] In the absence of data on historical mineral deposits, the mining instruments are valid if the location of minerals *and* their extraction were orthogonal to tribes' characteristics. The historical record clearly suggests this was the case. Most minerals were alluvial, so discoveries were made with little systematic prospecting, and tribe characteristics like military strength posed little deterrent to either the discovery or extraction of minerals. The 1870s Black Hills Gold Rush, for example, occurred on the Dakota Sioux territory, militarily the strongest tribe in the Great Plains. Furthermore, mining rushes were historically an important factor in the formation of reservations: Fahey (1986, p. 14) and Nichols (2003, p. 50, p. 132) described how the Pend d'Oreille gold rush of 1854 influenced the governor of Washington to pursue a policy of fewer, more centralized reservations. McGovern (1995, pp. 66–76) described how government agents in 1850s California negotiated reservation treaties primarily with a view toward removing Indians from mining districts. Knack (2001, Chapter 6) described how miners in Arizona and Nevada lobbied for military intervention to place Southern Paiute bands on reservations during the regional 1870 silver rush.

3.5.2. *Construction of the Instrument*

I first digitized the *Map of Early Indian Tribes* in the *National Atlas of the United States* (Gerlach (1970)) to construct a baseline map of ancestral homelands, and I supplemented this in places with more detailed local maps from the *Smithsonian Handbook of Native Americans* (Sturtevant (1981)). I then digitized gold and silver mining maps from the 1880 Census. These maps identify clusters of mining activity, which I digitized as raster-points. I then individually associated these raster-points with mining-values by dividing state-level statistics on mining activity by the number of raster-points in a state.[26] Figure 3 shows an overlay of these maps.[27] I then mapped each reservation to its *ancestral homeland*, that is, the land that its constituent bands had historically occupied. Finally, I calculated a reservation's exposure to historical mining by summing mining values over the raster-points that fell inside its constituent bands' ancestral homelands and dividing by the size of that area.

[25] In Table 6 in the Supplemental Material, I show correlations between the instruments and all other controls.

[26] State-level statistics were separately reported for gold and silver in the 1870, 1880, and 1890 Censuses. I aggregate these over time by mining type to get the total value of mining extraction from 1860 to 1889, which covers practically the entire period of reservation formation.

[27] Figures 2–4 in the Supplemental Material show the *National Atlas* Map, the Southwestern map from the *Smithsonian Handbook*, and the gold mining map from the 1880 Census.

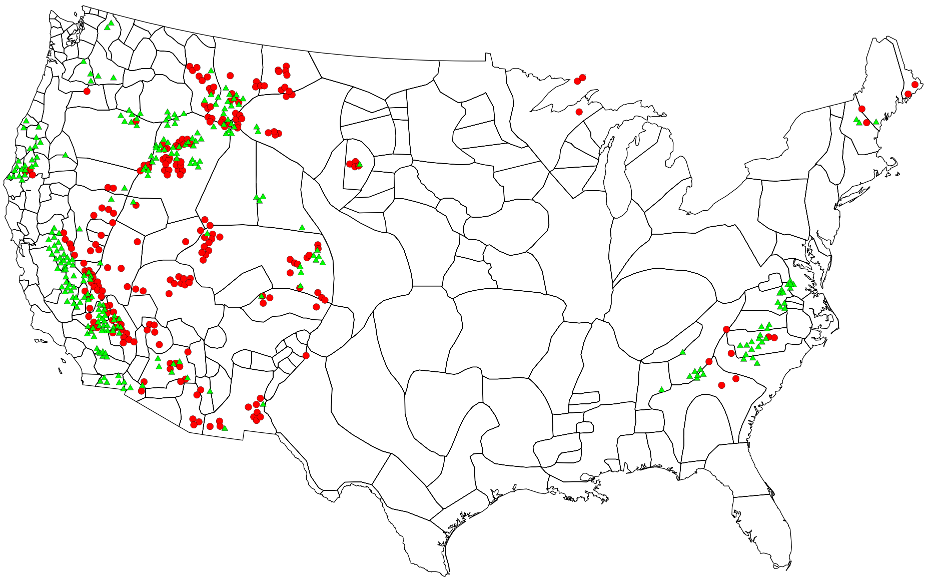

FIGURE 3.—Geo-database of gold (triangle) and silver (circle) mining clusters.

3.5.3. *Concerns About Instrument Validity*

The IV strategy requires the exclusion restriction that mining on ancestral territories affected long-run outcomes on reservations *only* through forced coexistence. Two concerns arise with this assumption: First, historical mining may have had direct effects on per capita incomes today through the reservation's economic environment. Second, the process of reservation formation itself may have been different, possibly more violent, in the presence of a mining rush. I address these two concerns in turn.[28] Regressing today's per capita incomes in a *county* on its historical gold and silver mining, constructed in the same way as the instruments, does show a weak positive correlation.[29] While Native Americans did not benefit from historical mining directly, this weak positive correlation raises the possibility that they benefited indirectly from positive effects on their economic environment. While I do control for the present-day economic environment as before, this may not be sufficient. To address this, I also control directly for historical mining inside a reservation's economic environment. Doing this implies that the IV uses only historical mining *inside* a reservation's ancestral territory that is far enough away to

[28]In matrix notation, if $y = X\beta + u$, then $\hat{\beta}_{\mathrm{IV}} = (Z'X)^{-1}Z'y = \beta + (Z'X)^{-1}Z'u$, where X and Z are $N \times K$ matrices, and y and u are $N \times 1$ vectors (Wooldridge (2002, Chapter 8)). The first concern would imply that $(Z'X)^{-1}Z'u > 0$; the second concern would imply that $(Z'X)^{-1}Z'u > 0$.

[29]I do not report further results on county-level regressions; the t-statistics on the two types of mining are 1.921 and 1.557, respectively.

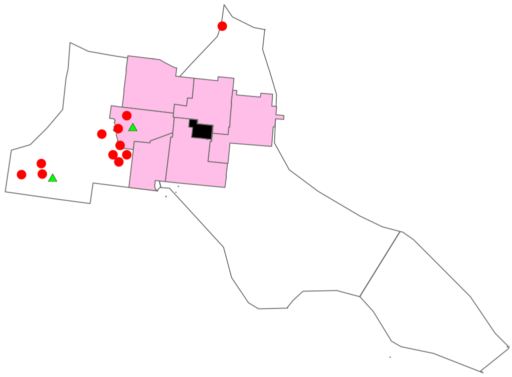

FIGURE 4.—Mescalero Reservation (in black) with surrounding counties (shaded), ancestral homelands (in white), and gold (triangle) and silver (circle) mining clusters.

fall *outside* its present-day economic environment. Figure 4 illustrates this. In the center (in black) is the Mescalero reservation in New Mexico. Its economic environment (in shaded area, clockwise from Northwest) consists of Socorro, Lincoln, Chavez, Otero, Dona Ana, and Sierra counties. Lastly, Mescalero's ancestral homeland is made up of the three band-territories of the Chiricahua, Mescalero, and Lipan. Because historical mining inside the economic environment is a separate control, the IV identifies only off the mining activity in the territory of the westernmost Chiricahua Apache and one mining cluster in the far north of the Mescalero band's territory.

A second concern is that mining may have made the process of reservation formation itself have meant more violence, further removal, or more disease exposure (Ehle (1997), Cole (1988, pp. 89–92, p. 118), Frazier (2001, p. 139)). To address differentially further removal, I include the distance between a reservation and its ancestral homelands as an additional control. To address the possibility of differential violence or disease exposure, I test for a differential negative health effect associated with the formation of a reservation in the presence of mining, using anthropometric data on the height of 12,000 Native Americans that was collected by Franz Boas in the 1890s, and—after being lost for many decades—published in Jantz (1995). I test whether individuals growing up in the years around a reservation's formation were shorter in the long run *and* whether this effect was more severe in the presence of mining. Specifically, I construct a share $\mathbf{s}_i$ of individual i's growth years from age 0 to 19 that overlapped with the years leading up to the formation of i's reservation r. I then regress i's height on a reservation fixed effect, gender-specific age fixed effects, $\mathbf{s}_i$ and $\mathbf{s}_i$ interacted with the instruments. For a 5-year window, the effect of $\mathbf{s}_i$ is a height reduction of 29 millimeters, significant at the 5% level.[30] This implies individual i was $-29 \cdot 5/19 = -7.66$ millimeters shorter if i's growth years fully overlapped with the five years before his reservation's formation. The interactions with the gold and silver instruments have t-statistics of -1.432 and 0.354, respectively. Since -1.432 is not far off conventional significance levels, I explore this further. The interaction with gold has a point estimate of -2.532, which implies that growing up during the formation of one's reservation, a one-standard-deviation increase in gold mining (3.6$) stunted the affected cohort by $-2.532 \cdot 5/19 \cdot 3.6 = -2.39$ millimeters on average, although this effect was not statistically different from 0. The 95% confidence-bound of this estimate is -6.07, so that one can be 95% confident this stunting was no worse than $-6.07 \cdot 5/19 \cdot 3.6 = -5.75$ millimeters. While it is difficult to assess at what severity a health shock in the 19th century could have long-run effects today, this health shock, aside from being insignificant, is small. Even the upper bound scenario is only 20% to 30% of the very precisely estimated two to three centimeters stunting of cohorts affected by the Great Chinese Famine of 1959 (Meng and Qian (2009)).

3.5.4. *Instrumental Variable Results*

Table IV, Panel A shows the results for the first-stage regression of forced coexistence on the instruments. Columns 1 to 5 introduce the controls in the same way as in Table III. In column 6, I include the additional controls discussed in Section 3.5.3, reservations' ancestral homeland's ruggedness, distance to their ancestral homeland, and historical mining in its economic environment. The effect of both instruments on forced coexistence is positive, significant, and comparable in magnitude across all specifications. A "mining rush," taken to mean a one-standard-deviation increase in the value of mining, increases the probability of forced coexistence by 6% to 11%, depending on whether the calculation is based on gold (3.6$ $*$ 0.018) or silver (2.3$ $*$ 0.048). In Panel B, I also report the reduced form relationship between reservations' per capita incomes and the instruments. Both instruments reduce incomes today. The effect of silver mining, while imprecisely estimated, is stable and of very similar magnitude to gold mining across specifications.

[30]I estimate

$$\text{height}_{ir} = \alpha \mathbf{s}_i + \mathbf{u}_r + \beta \mathbf{s}_i \cdot \mathbf{Z}_r + \Psi_i' \delta + \varepsilon_i, \tag{3}$$

where $\mathbf{u}_r$ are reservation fixed effects and Ψ_i are gender-specific age fixed effects. The direct effect of the instrument is absorbed by $\mathbf{u}_r$. I drop children under the age of 4 and include yearly age fixed effects for ages 5 to 23, but follow Steckel and Prince (2001) in including one common age fixed effect for ages 23–48. For time windows T longer than 5, the range of $\mathbf{s}_i$ is between 0 and $T/19$. Coefficient-estimates mechanically decline, but significance levels are unchanged.

TABLE IV

FIRST STAGE AND REDUCED FORM RELATIONSHIP WITH MINING INSTRUMENTS[a]

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: First Stage, Dependent: Forced Coexistence					
Historical gold-mining	0.018**	0.017**	0.015*	0.018**	0.023***	0.030***
	(2.510)	(2.346)	(1.941)	(2.068)	(2.959)	(3.567)
Historical silver-mining	0.048***	0.048***	0.054***	0.053***	0.059***	0.062***
	(3.201)	(3.226)	(4.314)	(3.767)	(4.526)	(4.181)
R^2	0.177	0.194	0.291	0.379	0.549	0.555
	Panel B: Reduced Form, Dependent: log(per capita income)					
Historical gold-mining	−0.012***	−0.010***	−0.008**	−0.008***	−0.010***	−0.017***
	(−3.546)	(−3.862)	(−2.442)	(−3.051)	(−2.786)	(−4.326)
Historical silver-mining	−0.010	−0.010	−0.018	−0.015	−0.014	−0.019
	(−1.028)	(−1.035)	(−1.699)	(−1.203)	(−1.102)	(−1.668)
R^2	0.040	0.212	0.239	0.365	0.538	0.553
Historical centralization	Y	Y	Y	Y	Y	Y
Reservation-controls		Y	Y	Y	Y	Y
Tribe-controls			Y	Y	Y	Y
Additional reservation-controls				Y	Y	Y
State fixed effects					Y	Y
Additional IV controls						Y

[a]There are 182 observations in all regressions. The instruments are defined in tens of dollars of minerals mined per square kilometer of ancestral homeland in the period 1860–1890. Columns 1–5 introduce controls in the same way as before. Column 6 adds the ruggedness of ancestral homelands, distance between ancestral homelands and the reservation, and the value of historical mining (defined like the instruments) in a reservation's surrounding counties. t-statistics are for standard errors that are clustered two-way at tribe and state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Gold and silver mining are almost completely uncorrelated, which is already suggested by their spatial dispersion in Figure 3.[31] This low correlation suggests the use of one aggregated precious-metal mining instrument, with the trade-off being that two separate instruments allow for over-identification tests but one combined instrument is more powerful. Because of this trade-off, Table V reports the IV results with the gold and silver instruments included separately, in Panel A, and aggregated into one precious-metal mining instrument, in Panel B. Columns 1 to 6 introduce the controls in the same way as in Table IV. The over-identification test in Panel A is passed at the 10% level in all specifications except column 2. As expected, the F-statistic for weak instruments is considerably stronger in Panel B for the single aggregate instrument and the IV results are correspondingly more significant. I therefore view Panel B with the single precious-metal mining instrument as the core set of results, while Panel A is supplementary in that it allows over-identification tests. In both panels, the reported endogeneity test suggests that forced coexistence is exogenous, that is, quasi-randomly assigned.

3.5.5. *Heterogenous Treatment Effect*

The IV estimates in Panel B are somewhat larger than the OLS estimates. A possible explanation is suggested by the literature: there were historical ties between some sub-tribal bands and not others. For instance, both the Mescalero and the San Carlos Apache reservations are coded with forced coexistence, but Cornell and Gil-Swedberg (1995) suggested that Mescalero is doing better than San Carlos today because Mescalero's constituent bands were historical allies while San Carlos's were not. If bands that had these unobservable ties were more likely to agree to shared reservations and if such ties also made it easier to cooperate on the reservation, then IV estimates the local average treatment effect (LATE) on only the bands that would not have agreed to shared reservations without the added pressure from mining. Since this LATE is identified off bands with less friendly ties, it should be more negative than the OLS if the OLS consistently estimated the *treatment effect on the treated*. I explore this more formally in the Supplemental Material.

4. MECHANISMS

4.1. *Background*

Political Conflict

Qualitative accounts have long linked income differences across reservations to local political conflict (Cornell and Kalt (1995, 2008)). Political con-

[31]Their correlation-coefficient is 0.117 and not significant at the 10% level. The point estimates in Table IV are therefore similar when including only one instrument at a time, and I do not separately report these.

TABLE V

IV RESULTS[a]

Dependent	log(per capita income)					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Two Instruments						
Forced coexistence	−0.329*	−0.304*	−0.360***	−0.316**	−0.302**	−0.403***
	(−1.738)	(−1.886)	(−3.079)	(−2.490)	(−2.300)	(−3.080)
F-statistic (instruments)	5.144	5.212	9.585	7.111	10.29	9.958
p-val. (over-identification test)	0.210	0.088	0.502	0.478	0.319	0.188
p-val. (endogeneity test)	0.991	0.981	0.869	0.586	0.482	0.481
Panel B: One Instrument						
Forced coexistence	−0.406*	−0.371**	−0.397***	−0.350***	−0.339***	−0.443***
	(−1.988)	(−2.409)	(−3.817)	(−3.575)	(−3.259)	(−3.974)
F-statistic (instruments)	10.20	9.769	23.30	20.05	48.60	29.79
p-val. (endogeneity test)	0.733	0.678	0.743	0.615	0.555	0.265
Historical centralization	Y	Y	Y	Y	Y	Y
Res-controls		Y	Y	Y	Y	Y
Add. tribe-controls			Y	Y	Y	Y
Endog. res-controls				Y	Y	Y
State fixed effects					Y	Y
Add. exclusion controls						Y

[a]There are 182 observations in all regressions. In Panel A, I instrument for forced coexistence with two instruments. In Panel B, I combine gold and silver into one precious metal mining instrument. Columns 1–6 introduce controls in the same way as before. t-statistics are for standard errors that are clustered two-way at tribe and state level. I report the Kleibergen–Papp F-test and the Hanson J over-identification test. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

flict seems likely to be salient on reservations because the 1934 Indian Reorganization Act (IRA) instituted a governance system in which local politicians, once elected, were unconstrained by the usual checks and balances of a separate judiciary and legislature.[32] As a result of the IRA, reservation politicians combine executive powers with legislative control over reservation by-laws and judicial control over reservation courts through their ability to hire and fire judges. There is ubiquitous anecdotal evidence that this system is prone to patronage politics: Lawrence (2003), for example, complained that "reservation councils possess near dictatorial control over reservation members, control the court, police and flow of money and which members get homes, jobs and healthcare services," and McGovern (1995, p. 47) quoted a reservation resident as saying that "the reservation council is employer, landlord and local government. Reservation members who oppose it are threatened with job loss, lose their housing arrangements, phones are disconnected and lives are threatened." While these anecdotes do not uncover exact mechanisms, the evidence resonates with models of clientilistic politics such as Besley (2006, Chapter 3.4.5), where politicians choose suboptimal policies that benefit only their own group.

The Historical Roots of Political Conflict

Accepting for the moment that political conflict is at the heart of present-day income differences on reservations, is it plausible that political divisions could be rooted in historical band divisions? This argument has been made by several scholars of Native American reservations. Cornell and Kalt (1993, 1995) argued that there is a general pattern across reservations that today's social divisions are rooted in historical tribal structures. The most publicized case of reservation-internal political conflict was the 1973 occupation of Wounded Knee, in which members of the Pine Ridge reservation demanded that the federal government displace their chairman, who was seen to represent only a small faction on the reservation (Nichols (2003, p. 201)). Holm (1985) stressed that the factionalism on Pine Ridge originated in the traditionally band-based political organization of the Lakota Sioux. Similarly, Fahey (1986, p. 135) described how the decentralized traditions of the Kalispel led to factionalism and rivalries when the tribe was integrated into a shared reservation, and Stern (1965, p. 75) described how the Klamath Reservation combined diverse subtribal bands and how band cleavages have persisted and dominated reservation politics ever since the formation of the reservation in 1864. While these qualitative descriptions do not uncover exact mechanisms, they do resonate with models of the persistence of social divisions through the intergenerational transmis-

[32]Before 1934, reservations were governed by "Indian Agents" of the BIA. Even earlier, the 1908 Curtis Act had abolished tribal jurisdiction of Indian lands, which had originally prevailed on the reservations.

sion of preferences that sustain trust and cooperation only within groups and not across (Bisin and Verdier (2001), Tabellini (2008)).[33]

4.2. *Evidence From Timing*

The first piece of evidence for the mechanisms comes from panel data.[34] A big structural break in the importance of local governance on reservations makes this panel variation informative about mechanisms. Until the late 1980s, reservations continued de facto to be run by the BIA despite the wide-ranging de jure internal powers of tribal councils, which had originated in the 1934 IRA. Starting in the late 1980s, federal regulation reduced reservations' dependency on the BIA and granted substantial political autonomy to reservation governments. Federal funding was significantly tightened, which forced reservations to look for non-transfer income and non-government jobs. At the same time, federal grants became disbursed as *block grants*, which gave reservations substantial autonomy in the projects they funded with federal money (Castile and Bee (1992), Nichols (2003)). The importance of this break in federal policy for reservations is clear from qualitative accounts such as those in Cornell and Kalt (2008), who argued that, "from the IRA onwards, most reservations came to have the feel of branch offices of the federal government, [...] with tribal governments totally dependent on BIA programs and funds." They further stated that "before about the 1980s, most reservations looked similar to each other, dependent on some combination of federal grants, leasing of tribal lands and occasional off-reservation employment," and further noted that there has been a dramatic divergence in fortunes since the late 1980s.

The qualitative evidence strongly suggests that the economic divergence between reservations was possible only *after* local governments became autonomous of the BIA. This gives rise to the prediction that forced coexistence should have become greatly more important after this policy break in the late 1980s. I could test this prediction in a difference-in-differences specification. However, the panel is highly unbalanced, because the coverage of the Census reports increased over time: the sample size increases from 87 in 1970 to 159 in

[33]In these models, parents may pass on social divisions even if they are detrimental to their own children's income, precisely because these divisions are preference-based. In Section 4.2, I present evidence suggesting that the negative consequences of forced coexistence may have, in fact, only appeared until relatively recently. Based on this evidence, the persistence of social divisions can be rationalized more easily, since it would for a long time have had no negative consequences.

[34]The Census published per capita income figures for reservations going back as far as the 1970 Census, allowing me to construct a 40-year panel of per capita incomes from 1970 to 2000. I thank Nick Parker for bringing the pre-1990 data to my attention.

1980 to 175 reservations in 1990. Because of this unbalancedness, a fixed-effect specification of the form

$$y_{it} = \alpha_i + \lambda_t + \delta_t \mathbf{FC}_i + \beta_t \mathbf{X}_{it} + \varepsilon_{it} \tag{4}$$

is preferable. The vector λ_t is decade fixed effects $\{\lambda_{1980}, \lambda_{1990}, \lambda_{2000}\}$ and δ_t is a coefficient-vector $\{\delta_{1980}, \delta_{1990}, \delta_{2000}\}$.[35] I also allow a time-variant coefficient-vector β_t on the controls to avoid mechanically loading the divergence between 1990 and 2000 onto the *only* time-variant coefficient δ_t. If the negative effect of forced coexistence was stable from 1970 to 1990, then $\delta_{1980} = \delta_{1990} = 0$. The baseline negative effect of forced coexistence is absorbed by α_i in the fixed-effect specification (4). It is estimated as about a 10% difference from 1970 through 1990 in a difference-in-differences specification, which is not separately reported.[36] The core prediction is that the effect of forced coexistence increased significantly after 1990, that is, that $\delta_{2000} > 0$.

Table VI shows the results of estimating specification (4). In column 1, I include only the reservation fixed effects α_i and the time fixed effects λ_t. The fixed effects alone account for 92% of the variation in the data. In column 2, I estimate the main vector of coefficients of interest, δ_t. Forced coexistence did not appear to have a differential effect in 1980 or 1990, that is, neither δ_{1980} nor δ_{1990} is significantly different from 0. Forced coexistence really began to show up in 2000 per capita incomes, with an estimated $\delta_{2000} = -0.222$. In column 3, I include the reservation controls with decade-specific coefficients β_t. To conserve on space, I report only p-values of the joint significance of all controls for each decade. The p-values indicate that the reservation controls also have a differential effect only in 2000, significant at the 5% level. In column 4, I add the tribe-controls, also with decade-specific coefficients. The p-values show that *both* sets of controls become significantly more important in 2000. Lastly, I address the concern that casinos also became a major determinant of Native American incomes after 1990.[37] In column 5, I therefore report results with a separate casino growth trend after 1990. Reservations with casino operations did in fact grow about 12% faster from 1990 to 2000. But this specification also does not reduce the coefficient on forced coexistence much.

Overall, all specifications show that the real divergence between reservations with and without forced coexistence occurred only after 1990, despite the fact that forced coexistence was determined more than 100 years prior to that. This striking finding is consistent with the qualitative evidence suggesting that

[35]To avoid notational clutter with the added time-subscript t, I drop the group-subscript e from specification (1), although I do continue to control for tribe characteristics.

[36]In Table 7 in the Supplemental Material, I report cross-sectional results by decade, which also show about a 10% difference from 1970 through 1990, although this difference is less precisely estimated in separate cross-sections.

[37]The Indian Gaming Regulatory Act (IGRA), which started the opening of reservation casinos, was passed in 1989, that is, just before the 1990 Census data.

TABLE VI

DIVERGENCE IN PANEL DATA[a]

Dependent	log(per capita income)$_{it}$					
	(1)	(2)	(3)	(4)	(5)	(6)
$D(1980)$	1.012***	0.984***	0.906	0.982***	0.944	0.828
	(21.341)	(11.118)	(0.249)	(11.057)	(0.259)	(0.227)
$D(1990)$	1.863***	1.870***	0.634	1.868***	0.793	0.642
	(43.997)	(25.548)	(0.203)	(25.503)	(0.252)	(0.203)
$D(2000)$	2.135***	2.271***	3.170	2.196***	2.193	3.308
	(47.222)	(28.581)	(1.009)	(26.840)	(0.654)	(0.733)
$D(1980) *$ Forced coexistence		0.051	0.026	0.049	0.026	0.021
		(0.495)	(0.219)	(0.474)	(0.220)	(0.173)
$D(1990) *$ Forced coexistence		−0.011	0.050	−0.012	0.049	0.042
		(−0.118)	(0.584)	(−0.139)	(0.565)	(0.490)
$D(2000) *$ Forced coexistence		−0.222**	−0.242**	−0.227**	−0.237**	−0.253***
		(−2.328)	(−2.458)	(−2.354)	(−2.415)	(−2.629)
$D(2000) *$ Casino				0.115**	0.077	−1.858
				(2.026)	(1.172)	(−0.365)
$D(2000) *$ Casino $*$ log(Distance major city)						−0.161**
						(−2.562)
$D(2000) *$ Casino $*$ log(Surr. p.c. income)						0.133
						(0.417)
$D(2000) *$ Casino $*$ log(Surr. p.c. unempl)						0.238
						(0.491)
R^2	0.921	0.925	0.932	0.926	0.933	0.934

(*Continues*)

TABLE VI—*Continued*

	log(per capita income)$_{it}$					
Dependent	(1)	(2)	(3)	(4)	(5)	(6)
p-val. (reservation-controls 1980)			0.4607		0.4935	0.5799
p-val. (reservation-controls 1990)			0.1415		0.1558	0.1979
p-val. (reservation-controls 2000)			0.0565		0.0829	0.0343
p-val. (tribe-controls 1980)			0.9361		0.9361	0.5771
p-val. (tribe-controls 1990)			0.5587		0.5631	0.9369
p-val. (tribe-controls 2000)			0.0669		0.0856	0.0713

[a]The panel runs from 1970 to 2000; the number of observations is 603. The key pattern is that two-thirds of the −30% cross-sectional effect of forced coexistence in 2000 is driven by divergence between 1990 and 2000, that is, after reservations obtained more independence. (A 10% cross-sectional difference is precisely estimated in 1970, 1980, and 1990 for an unreported difference-in-difference specification.) All regressions reported here include reservation fixed-effects and decade fixed-effects. Column 3 includes the five reservation-controls (surrounding-county incomes and unemployment vary by decade; the other three are time-invariant.), and the five (time-invariant) tribe-controls used in Table III. All controls have decade-specific coefficients, to avoid loading significance artificially onto forced coexistence as the only variable with decade-specific coefficients. (Specifications with decade-specific controls (columns 3, 5, and 6) render the decade fixed effects insignificant.) p-values, reported at the bottom for the joint-significance of controls by decade, show that *all* characteristics matter more in 2000. To control for the importance of gambling after 1989, column 4 includes a separate casino-specific growth-trend, without the other controls. Column 5 shows that the casino-effect is not, however, robust to including the other controls. Column 6 allows for a heterogenous casino-effect, by interacting with controls for the economic environment (in 2000). The only significant interaction is with distance to markets. Together, columns 4–6 show that casinos do not explain the differential effect of forced coexistence, even when the casino-effect is allowed to be heterogenous, that is, dependent on interactions. t-statistics are for standard errors clustered at the reservation-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

forced coexistence *could* only matter after the legal changes in the late 1980s. The tests of joint significance of the control variables further support this view. Both reservation and tribe characteristics started to matter differentially for per capita incomes after 1990, suggesting that before then, *nothing* mattered very much because the BIA ran all reservations in more or less the same way.

The patterns in Table VI have antecedents in the literature on institutions and growth. For example, Acemoglu, Johnson, and Robinson (2002) found that while initial conditions in colonies determined the institutions that European settlers implemented from 1600, these only began to translate into per capita income differences in the 1800s, when industrialization made growth-enhancing institutions salient. Perhaps the closest connection in the previous literature is with Nunn (2008), whose results also provide a nice benchmark for conceptualizing the magnitude of the divergence reported here. Nunn (2008) studied the long-run effects of the slave trade on African countries, and found that low-slave-export countries were about 200% richer in 2000 than high-slave-export ones, but only about 80% richer in 1975.[38] In other words, most of the divergence he found occurred two centuries after the peak of the slave trade. This is because colonial rule muted the chief negative consequences of the slave trade, that is, worse institutions and lowered trust, though they were surely already present before 1975 (Nunn and Wantchekon (2011)). Interestingly, the annualized growth rate differential of about 2% between reservations with and without forced coexistence, implied by Table VI, is almost exactly the growth-rate differential between low-slave-export countries and high-slave-export countries from 1975 to 2000 in Nunn (2008).[39] Based on this comparison, the estimated magnitudes in Table VI seem quite reasonable; even more so because of the small size and physical separation of reservations, as well as their integration into the rich U.S. economy.

4.3. *Evidence for* Political *Conflict*

The second piece of evidence related to the mechanisms comes from looking for a link from forced coexistence to politics in *reported* episodes of political conflict and corruption in the *ProQuest* newspapers database. For each reservation, I ran search queries for key words that signify reservation-internal political conflict and counted the number of matching articles. I also generated the same count for key words that signify reservation-internal corruption. To check whether either conflict or corruption was associated with the local governance institutions, I ran separate searches for episodes involving the local government and for those that explicitly did not.[40] I also generated a count of

[38]These numbers are based on Figure VIII in Nunn (2008): The 200% difference in 2000 is calculated as $\frac{2600-850}{850}$, and the 80% difference in 1975 as $\frac{2000-1100}{1100}$.

[39]Calculated as the difference between $(\frac{2600}{2000})^{1/25} - 1 = +0.0106$ and $(\frac{850}{1100})^{1/25} - 1 = -0.0103$.

[40]For conflict, I searched with the query "(conflict* OR faction* OR interfer* OR internal dispute)" plus the name of the reservation. For corruption, the query was "(embezzle* OR corrupt*

all articles mentioning a reservation, to control for a reservation's overall news coverage. I collected data from 1981 to 2000. To glean evidence from timing in the same way as in Section 4.2, I aggregated the counts by decade, that is, separate counts for 1981–1990 and for 1991–2000. An observation is thus the count of articles reporting on a reservation and on a specific set of key words, over the course of a decade.

In contrast to the income data in Section 4.2, there are only two time periods, but the panel is balanced. I therefore test for an effect of forced coexistence on counts of reported conflict in a difference-in-differences specification of the form

$$\text{Conflict-Count}_{it} = \alpha + \lambda_t + \delta_t \mathbf{FC}_i + \gamma \text{All-Count}_{it} + \beta_t \mathbf{X}_{it} + \varepsilon_{it}, \tag{5}$$

where Conflict-Count$_{it}$ is the number of newspaper articles pertaining to internal conflict or corruption on reservation i in period t and All-Count$_{it}$ is the overall count of articles that mention reservation i in decade t. Because the newspaper article counts are discrete data with a preponderance of zeros and small values, I use Poisson regressions. As a robustness check, I also run OLS regressions on the continuous *share* of total newspaper coverage that pertained to conflict, that is, dividing Conflict-Count$_{it}$ by All-Count$_{it}$ for each type of conflict or corruption. In addition to accounting for overall news coverage with All-Count$_{it}$, the controls $\mathbf{X}_{it}$ include the five time-invariant tribe characteristics, as well as the time-varying measures of the economic environment that were also included in the baseline specifications in Table III. I control for size effects by including the log of population and its square. I also control for a casino-effect for the decade 1991–2000 because of the salience of casinos in the press in that decade and because of casinos' noted association with conflict and corruption (Evans and Topoleski (2002)). Lastly, I include a decade fixed effect λ_t.

Table VII reports the results by query type.[41] Columns 1–4 report on counts of internal conflict, while columns 5–8 report on counts of corruption. Columns 1–2 and 5–6 explicitly involve the reservation government, while columns 3–4 and 7–8 explicitly do not. Panel A reports on Poisson regressions for counts, and Panel B reports on OLS regressions for count-shares. Consistent with the qualitative evidence presented in this paper, columns 1–4 show a positive relationship between forced coexistence and reservation-internal reservation conflict. This relationship is stronger for conflict that explicitly involves the reservation governance institutions (columns 1–2). The expected count of newspa-

OR improper* spen* OR improper* disburs*)". To both "(tribal council OR tribal chairman)" was added, with either "AND" or "NOT" in front of it.

[41]The number of observations is 352, less than 364 (= 182 ∗ 2), because I drop all reservations that do not have the term "reservation" in their official name. For instance, Little River reservation is commonly referred to as "Little River." Thus, when searching for Little River, the count increases by an order of magnitude, and most hits are unrelated to the reservation.

TABLE VII

COUNTS OF NEWSPAPER REPORTS OF CONFLICT AND CORRUPTION[a]

	Count: Conflict AND Government		Count: Conflict NOT Government		Count: Corruption AND Government		Count: Corruption NOT Government	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Poisson on Counts								
Forced coexistence	1.101**		0.451		0.117		−0.033	
	(2.247)		(1.435)		(0.553)		(−0.250)	
$D(1990) * $ Forced coexistence		0.860*		0.512		0.043		−0.065
		(1.810)		(1.261)		(0.160)		(−0.363)
D(2000) $*$ Forced coexistence		1.139**		0.471		0.185		−0.007
		(2.262)		(1.445)		(0.905)		(−0.045)
Pseudo R^2	0.654	0.668	0.743	0.747	0.748	0.752	0.841	0.843
Panel B: OLS on Count-Shares								
Forced coexistence	0.013**		0.001		−0.000		0.009	
	(2.336)		(0.339)		(−0.030)		(0.674)	
$D(1990) * $ Forced coexistence		0.002		0.001		−0.005		0.009
		(0.679)		(0.402)		(−0.179)		(0.447)
$D(2000) * $ Forced coexistence		0.023**		0.000		0.004		0.009
		(2.256)		(0.061)		(0.296)		(0.579)
R^2	0.090	0.121	0.081	0.091	0.061	0.067	0.173	0.188

[a]This table reports on counts of newspaper articles for key-words, in combination with reservation names. Counts were aggregated by decade (1981–1990 and 1991–2000). I report on Poisson regressions for counts (Panel A), and OLS regressions on count-shares (of total articles covering a reservation). Counts are defined in four ways: on conflict (columns 1–4) versus corruption (columns 5–8), and involving the reservation government (columns 1–2 and 5–6) versus explictly not. All specifications control for the five tribe level characteristics and the measures of the economic environment from previous tables, as well as a decade fixed effect, and the overall count of all articles pertaining to a reservation. In addition, all specifications control for the log of population and its square, as well as for a casino-dummy in the last decade, as in Table VI. The full set of results on all coefficients is reported in the Supplemental Material. For each type of count, the first specification pools the data and estimates a single repeat cross-sectional effect of forced coexistence, while the second specification breaks the effect down by decade. Z-statistics in Panel A and t-statistics in Panel B are for s.e. clustered at the reservation level. The number of observations is 352 in all specifications in Panel A. (See footnote 41 in text.) In Panel B, the number of observations varies between 348 and 352, because the share is undefined if the total count is 0. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

per reports of internal conflict is three times as high in 1991–2000 for a reservation with forced coexistence ($\frac{\mathbb{E}[\text{Conflict-Count}_{it}|\mathbf{FC}_i=1]}{\mathbb{E}[\text{Conflict-Count}_{it}|\mathbf{FC}_i=0]} = e^{1.101} = 3$). By contrast, there is no clear relationship between forced coexistence and corruption, at least with the exhaustive set of controls included here.[42] Regressions for the shares of newspaper articles confirm the basic results, in Panel B. In addition, column 2 shows that the conflict results appear a lot stronger in 1991–2000 than in the decade before. This divergence pattern suggests that, like the economic divergence in Section 4.2, differences in internal political conflict increased when local governance began to matter more.

4.4. *Economic Mechanism*

Section 4.2 provides evidence that forced coexistence decreased incomes significantly more after more autonomy was granted to reservations. Section 4.3 provides evidence that forced coexistence increases internal conflict on reservations, that it did so more after increased autonomy made local governance more important, and that this conflict is primarily political, that is, associated with governance institutions. While both pieces of evidence are strongly suggestive of a political-economy explanation of the reduced form causal estimates in Section 3, this does not yet pin down the economic mechanism linking social divisions to per capita incomes on reservations. The economics literature suggests several possible explanations, centered on the logic of collective action: public goods may go under-provided (Alesina, Baqir, and Easterly (1999)), community participation may be lower (Alesina and La Ferrara (2000)), and free-riding may be higher (Vigdor (2004)), especially if citizens care about social sanctions only from their own group (Miguel and Gugerty (2005)). While these channels are certainly plausible, qualitative evidence points in a different direction, because of two specific institutional features of reservations. One is that public goods are provided largely by the BIA. The second is that reservations have wide-ranging control over their contracting environment, because commercial legal disputes are typically resolved in reservation courts.[43] The first feature means that public good provision is unlikely to drive the results. The second feature means that the local business environment is likely to be very affected by factional politics because politicians' control over local judges creates contracting uncertainty and under-investment (Anderson (1992)). Haddock and Miller (2004) labeled this the "sovereign's paradox," highlighting the downside of reservations' judicial autonomy.

Since no publicly available data exist on Native American businesses or investment on reservations, I focus on ruling out the two main alternatives to the

[42]The coefficients on corruption in columns 5–8 are positive and significant only when fewer controls are included, whereas the coefficients on conflict in columns 1–2 are robustly significant regardless of the controls.

[43]There is some state-level variation in this (Anderson and Parker (2008)), but this gets absorbed by state fixed effects in the regressions.

TABLE VIII

MECHANISMS[a]

Dependent (Below)	Coeff.	t-Stat.	R^2	Partial R^2	N
log(p.c. wage income)	−0.134*	(−1.860)	0.485	0.027	169
log(avg. salary)	−0.207**	(−2.413)	0.394	0.083	169
Share employed	−0.018**	(−2.125)	0.481	0.032	169
log(p.c. transfer income)	0.031	(0.316)	0.520	0.001	169
log(p.c. receipts from BIA)	0.214	(1.600)	0.468	0.021	169
Share who work in county of residence	0.008	(0.229)	0.435	0.001	169
Average travel-time to work	−0.296	(−0.173)	0.395	0.000	169
Share whose primary language is English	−0.015	(−0.885)	0.705	0.008	173
Share with college degree	−0.006	(−1.315)	0.450	0.009	173

[a]Each row reports the coefficient on forced coexistence, from a regression of one additional outcome on the full set of reservation and tribe-controls and state fixed effects as in Table III, column 5. Data on BIA receipts are from the 2000 BIA "Greenbook." The last two variables are from the core data set, and were reported for 173 of 182 reservations. All other data come from the 2000 U.S. Census Summary File 3, which included 169 of the 182 reservations. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The partial R-squared measures by how much the total R-squared falls when forced coexistence is omitted from the regression.

explanation that local governance impacts the business environment. Namely, I rule out that income differences are driven by either federal transfers or varying degrees of integration into the off-reservation economy. To do this, I re-run the most restricted baseline specification (from Table III, Panel A, column 5) with all controls and state fixed effects for a number of outcomes, with an emphasis on income breakdowns and proxies for integration into the off-reservation economy. Table VIII reports the results, with each row reporting on a separate regression with a distinct outcome. The first five rows focus on income breakdowns. The last four rows focus on available measures of integration into the off-reservation economy. The results in the first five rows clearly show that forced coexistence has significant effects on earned income, salaries, and employment rates. The fourth row shows that income from individual government transfers does not correlated with forced coexistence. The fifth row shows a weak *positive* relationship with program spending by the BIA, implying that the negative effect of forced coexistence on incomes cannot possibly be driven by lower access to federal funding. The last four rows show no evidence of differential integration into the off-reservation economy. Residents on reservations with forced coexistence are not less likely to work outside their county of residence and do not travel significantly shorter distances. Further, they do not appear to be less culturally integrated as measured by the share of residents whose primary language is English. There is some indication that they have fewer college graduates, although this relationship is weak. Overall, the evidence therefore suggests that income differences are indeed primarily due to *earned* incomes *on* the reservations.

5. CONCLUSION

Ethnically and linguistically fragmented jurisdictions often also integrated ethnic groups with no history of centralized or shared governance. This paper's contribution is to isolate this latter effect by using a natural experiment that occurred during the formation of Native Americans reservations in the United States. Using variation in the degree to which reservations integrated sub-tribal bands into shared reservations and conditioning on tribes' pre-reservation historical levels of political centralization, I find that forcing independent polities into shared reservations in the 19th century reduces per capita incomes on those reservations today by a sizeable 30%. A specification with tribe fixed effects and an IV strategy suggest that this estimate is the causal effect of forced coexistence. Additional evidence on political outcomes and on the timing of economic divergence suggests that the primary mechanism is persistent social divisions leading to factional politics that create an uncertain business climate. This paper is novel in isolating the effect of integrating polities with no history of shared governance into centralized jurisdictions. It is important because it suggests that forcing autonomous polities into a system of shared governance can give rise to persistent social divisions, even when these polities are ethnically and linguistically homogenous.

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