Can Communities Self-Govern the Commons? Evidence from Imposed Reforms in the Italian Alps by the Habsburgs and Napoleon*

Trevor O’Grady† Claudio Tagliapietra‡

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Abstract

Widespread state intervention in local common pool resource management has set off a long and contentious debate about the appropriate locus of natural resource governance. However, the causal impacts of these interventions have proven difficult to measure. This paper investigates a natural experiment in which hundreds autonomous self-governing alpine communities in Northern Italy come under the direct control of centralized states (Austria and France) at different times in the late 18th century. Our main empirical design exploits the timing and location these interventions to estimate the impact of state centralization on communal economies in a difference-in-differences type design. We find a large, significant, and persistent relative increase in infant mortality rates and a more modest decrease in birth rates as a result of state centralization. We provide evidence that these demographic changes reflect a critical loss of natural resource income caused by the disruption of communal institutions. Impacts are most severe in communities that have no prior experience with formal, centralized institutions.

Keywords: Common Property, Political Centralization, Infant Mortality

JEL Codes: D23, K11, L14, N53, P48

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†Department of Economics, Harvard University. Email: trevorogrady@fas.harvard.edu

‡Email: tagliapietra.claudio@gmail.com
1 Introduction

Common-pool resources such as forests, pastures, and fisheries have a long history of communal ownership throughout the world.\(^1\) It is well-known that common-pool resources are vulnerable to overexploitation (Gordon, 1954; Hardin, 1968). The conditions under which communal self-governance can effectively address this issue has been at the center of a long and contentious debate in both policy and academic circles (Ostrom, 1990, 2005; National Research Council, 2002). On many occasions, the mere potential for overexploitation has induced state authorities to directly intervene in local resource management.\(^2\) The economic effects of these interventions, however, have been difficult to evaluate and measure objectively. One challenge has been in constructing sufficiently large data sets capable of isolating variation in institutional characteristics. Perhaps a more fundamental challenge has been in identifying the appropriate counterfactuals to interventions (Agrawal, 2001). Indeed much of the empirical work in this area has struggled to separate the impacts of interventions from the impacts of the factors causing them (Casey, Glennerster, and Miguel, 2012). In this paper, we exploit a historical experiment using community level data to show distinctly negative consequences of state centralization of resource management on the livelihood of resource users.

Our analysis investigates 168 resource-dependent communities in the Trentino region of the Italian Alps. Until the late 18th century, Trentino communities maintained local authority over natural resource management and devised both formal institutions (Carte di Regola, rural charters) and informal institutions to regulate resource use and enforce property rights within the community (Casari, 2007). However in the late 18th century, communal governance was upended during separate but similar interventions by Austria and France, after which all rules, including those pertaining to resource use, were directly issued by external state authorities and enforced by state bureaucrats. Critical to our estimation strategy, the earlier intervention by Austria only applied to a subset of Trentino communities known as the Welsch Confines, which came under Austria’s direct control by historical accident. While the Welsch Confines lost their autonomy during the Austrian reforms, the rest of the Trentino (which we refer to as Trent) maintained their communal institutions.\(^3\) In contrast, the centralization of governance following the French intervention under Napoleon applied to all Trentino communities, but by this time the Welsch Confines had no autonomy to lose.

\(^1\) Though many of these systems have been gradually replaced by private property and state management, communal resource governance is still prevalent in substantial parts of the developing world today.

\(^2\) See Baland and Platteau (1996, pg. 235-283) for a broad array of examples.

\(^3\) As we explain in section 2, despite differences in local governance autonomy, both Trent and Welsch Confine communities did face similar tax burdens.
Our main empirical design measures differences in outcomes at five-year intervals from 1765-1845 between Trent and Welsch Confine communities relative to their differences in 1795, when Trent communities are autonomous and Welsch Confine communities are under direct state control. Therefore in the periods before Napoleon’s invasion, the difference in differences estimate captures the average impact of state centralization on Welsch Confine communities relative to a Trent control group. Similarly in the periods after the invasion, the difference in differences estimate captures the average impact of state centralization on Trent communities relative to a Welsch Confines control group. Pooling the estimates into one design allows us to compare the similarity of the point estimates and their evolution over time.

We infer the economic impact of state centralization by observing changes in key demographic variables at the village level. Our main outcome of interest is the infant mortality rate, which we argue has a close, negative relationship with income per capita. We also construct a number of secondary outcome variables at the community/year level including births per population, deaths per population, and the natural growth rate. Our demographic data are constructed from original archival sources accessed from church parish registers of baptisms and deaths. Importantly, this data is collected by ecclesiastical (rather than political) authorities whose jurisdictions do not change during our study period.

Our main regression estimates indicate that state centralization leads to large, persistent, and statistically significant increases in the infant mortality rate for affected communities. Difference-in-differences estimates stabilize around 0.10 in both tails of our study period, indicating a ten percent increase in the infant mortality rate (or an additional 100 infant deaths per 1,000 births) due to centralization. The magnitude of this impact is greater than 60 percent of the average infant mortality rate across the entire study period (0.16). Consistent with our identification strategy, the trends in the estimates follow symmetric patterns pre and post-invasion and reach a minimum immediately prior to Napoleon’s invasion. The estimates and their general trajectory over time are robust to various sets of geographic controls. We also find a marginally significant temporary drop in births per population following centralization, but this difference gradually disappears over time. However, once we account for the higher number of infant deaths following centralization, the rate of children surviving past the first year per population is significantly lower after centralization and this

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4In contrast to income data, data on infant mortality rates can be consistently collected across communities throughout our study period. Baird, Friedman, and Schady (2010) find a significant negative relationship between income shocks and infant mortality rates in a developing country context. We provide similar cross-sectional evidence of this relationship in section 4. Others have linked high infant mortality rates directly to natural resource degradation and food shortages (Monteleone, 1964; Roseboom, van der Meulen, Ravelli, Osmond, Barker, and Bleker, 2001).
negative effect persists.

We also find that the negative impacts from state centralization are significantly smaller, though still large, in communities where formal governance is in place (i.e. a rural charter has been adopted) prior to intervention. Furthermore, the mitigation of the effect is largest when the formal institution in place has governance functions spanning multiple communities. These results appear to be consistent with other recent empirical findings that historical experience with formal rules, and centralized governance in particular, translates into better economic performance under externally imposed state regimes (Gennaioli and Rainer, 2007; Michalopoulos and Papaioannou, 2013; Dippel, 2013). These results may also relate to the tradeoff between formal laws and informal norms (Posner, 1997; Ellickson, 1998; Benabou and Tirole, 2011), where the establishment of formal rules has the potential to crowd out informal mechanisms used to foster cooperation. Viewed in this light, our results potentially reflect additional costs of formalization born by non-charter communities during centralization, rather than a greater capacity of charter communities to assimilate to the new regime. In either case, the general implication seems to be that a greater mismatch between the imposed institution and the institution it replaces entails higher costs.

The remainder of this paper develops as follows. In section 2 we briefly describe the history of self-governance in Trentino communities and the events surrounding the Austrian and French interventions. In section 3 we develop a model of common pool resource management to help guide the empirical analysis. Section 4 describes the data and links infant mortality rates to income per capita, section 5 lays out the empirical specification and discusses the estimation results, and section 6 concludes.

2 Historical Background

2.1 Trentino Region and Community Self-Governance

The Trentino region is located in the Alps of northeastern Italy between the German-speaking region of South Tyrol to the north and the Republic of Venice to the south. By the 19th century, approximately 300 small villages occupied the region with a median population of 450. Due to low agricultural suitability, most traditional villages in the region are highly

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5For example, in a laboratory public goods game, (Kube and Traxler, 2011) finds that voluntary sanctioning of free riders significantly decreases after centralized formal sanctions are introduced.

6This historical overview draws on Casari (2007); Nequirito (1996, 2002, 2004); Monteleone (1964); Andreatta and Pace (1981); Curzel (1999); Giacomoni (1991); Tovazzi (1986); Castagnetti and Varanini (2004); Tarle (1950).

7The region also has two relatively large towns, Trento and Rovereto, that we exclude from our analysis to focus on resource-dependent rural communities.
dependent on forest and pasture land for income and survival. These resources tended to be owned by the community in common. In the limited areas where crop production was feasible, the most common uses were for vineyards, fruit orchards, and wheat. Though these areas were more likely to be private property, it was often difficult to enforce.

The Trentino region roughly corresponds to the original boundaries of the Prince-Bishopric of Trent, a temporal and ecclesiastical division of the Holy Roman Empire founded in 1027 and effectively dissolved in 1796 after Napoleon’s invasion of Northern Italy. During the Prince-Bishopric period, local communities were given significant autonomy in governing community life including the right to hold community assemblies (Regole), appoint officials, establish and enforce resource appropriation rules, and protect property against encroachment (Giacomoni, 1991; Casari, 2007). Governance rules were often codified into formal community charters (Carte di Regola), which were then approved by the Prince-Bishop given that they were compatible with the laws of the territory. The earliest charter we have record of dates back to 1202. Over time, charters became increasingly more common and by the time of Napoleon’s invasion, nearly all communities in the region produced a charter or were linked to one that spanned multiple villages. The rules of charters across communities are very diverse, but common characteristics include: access fees for outsiders; resource quotas for insiders; monitoring mechanisms and paid guards; and inheritance and marriage rules that determine how access rights are transferred.

Even in the absence of a charter, forests and pastures were not open access resources, and communal property boundaries had legal standing. Given the mountainous terrain, migration to villages outside of one’s own valley was difficult and generally helped prevent outside encroachment. Even if temporary trespass into a village common could be achieved relatively easy, permanent entry into a community would not be. One typical way of joining a community was by paying a fee, which even when offered, still might require the approval of a village assembly. The other two ways of becoming a member are through birth and marriage. These latter two raise a distinctly Malthusian issue that even with exclusion of outsiders, the growth of the insider population can deplete income per capita.

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8Forests were a critical source of firewood, food for villagers (chestnuts, wild game), and building materials. Pasture lands provide feeding grounds for livestock mainly used for the production of meat and dairy products.

9Though natural resource governance is the focus of most charters, it does not appear that charters were limited to this function. Casari and Lisciandra (2014) give examples of charters with rules related to criminal law.

10Casari and Lisciandra (2014) document that rules regarding inheritance and marriage tend to get stricter as population pressure increases, effectively slowing the growth of the user group. In a similar spirit, charter rules in which quotas are allocated per household rather per individual are increasingly frequent over time.
2.2 Transition to State Governance

2.2.1 The Welsch Confines and the Habsburgs

In the medieval period, the Principality of Trent increasingly came under the influence of its northern neighbor Tyrol. In a series of agreements reached with the Prince-Bishops of Trent throughout the 14th-16th century, the Counts of Tyrol expanded their political authority over parts of the Trentino region. The areas ceded to Tyrol tended to be near access points to travel routes through Trentino to other parts of Northern Italy and became known collectively as the Welsch Confines, which translates to the “Italian Borders” (Bellabarba and Olmi, 2000). By the 18th century, Tyrol had been taken over by the powerful House of Habsburg. Though large, the Habsburg Empire was very fragmented in the early 18th century. Empress Maria Theresa who began her reign in 1748, gradually introduced reforms to centralize the administration of Habsburg lands. By 1754, she established a provincial government in Innsbruck which would oversee Tyrol including the Welsch Confines. Despite these reforms, there was still little change at the local level and therefore Welsch Confine communities retained their autonomy during this period.

Maria Theresa’s eldest son Joseph II was named co-ruler of Habsburg lands in 1765 and would later become sole ruler in 1780 after her death. In his earliest years, Joseph had little impact on Habsburg policy. However, he became increasingly influential over time and issued a number of reforms that would both increase the scope of the state and disrupt local authority. Between 1775-1780 The Austrian government issued reforms that imposed taxes on the local nobility, liberalized community inheritance rules (Stolz, 1949, pg. 445-450), and centralized all local appellate and nobel courts (Bellabarba and Olmi, 2000, 463). After 1780 Joseph’s reforms intensified further\textsuperscript{11} and culminated in the establishment of a system of centrally appointed civic magistrates to deal with all matters judicial, political, and economic across Habsburg territories in 1789, thus marking the definitive end to communal autonomy (Levy, 1988, 38-39).

2.2.2 Taxation Pre-Napoleon

A unique feature of our empirical setting, is that although Trent communities were not directly affected by Joseph’s reforms, they were subjected to Habsburg taxes and military conscription during his reign. The events leading up to this begin in 1363, when the Prince-Bishop of Trent and the Count of Tyrol agreed to an “eternal confederation”, which stated that all land within the two territories would become unified in perpetuity for purposes of

\textsuperscript{11}Table 6 in Dickson (1995) shows the number of government decrees issued by the House of Austria increased more than five-fold during 1780, Joseph II’s first full year as sole-ruler.
taxation and defense (Levy, 1988; Bonazza, 2004). The tax system was further aligned in 1511, when Maximilian I negotiated a treaty in which the Prince-Bishoprics of Trent and Brixen would no longer pay taxes to the Holy Roman Empire, but directly to Tyrol.\textsuperscript{12} Still there were differential tariffs imposed on Trent and Tyrol during this time period, though these eventually equalized over time. The last notable conflict between tariffs was resolved in 1777, in which Maria Theresa and Prince-Bishop Peter Vigil Thun signed a treaty to remove the small remaining differences between tariffs in the two regions (Bonazza, 2004, pg. 62-75). Therefore in the decades leading up to Napoleon’s invasion, we can be reasonably confident that the tax burden is shared equally across Trent and Welsch Confine communities.\textsuperscript{13}

### 2.2.3 Napoleonic Invasions and Aftermath

In the fall of 1796, Napoleon and the French Revolutionary Army invaded the Trentino Region, and brought with it a beginning to the end of the old institutional regime. In fact, Prince Bishop Peter Vigil Thun had already fled the region in anticipation of Napoleon’s arrival. However, he assigned a temporary regency to govern the Bishopric in his place.\textsuperscript{14} Napoleon conquered the region (including the Welsch Confines) swiftly and proceeded to dismantle the interim government. He named a representative from the aristocracy as Prime Minister, and had all members of the new government swear an oath of obedience to the French Republic. Like other territories conquered by the French, the Trentino became a department of the state-centered French regime ruled by a prefect. The prefect was stationed in Trento, and implemented orders of the central authority (Nequirito, 1996).

In the coming decade these territories would pass back and forth between Austrian and French rule several times.\textsuperscript{15} Efforts were taken by the French regime to abolish all decentralized governance in the region, including the community Regole. On January 5th 1805, a circular letter of the Emperor informally announced the prohibition of the Regole, and two years later, an official decree of Maximilian Joseph officially determined their abolition. The Regole were considered “anomalous institutions, incompatible with the new organization of the judicial districts and with any other regular administration of justice and police.”\textsuperscript{16} In 1814, Napoleon is defeated for the final time and with the signing of the Treaty of Paris, the Trentino and Tyrol regions are again back under Austrian control where they remain until 1919.

\textsuperscript{12}At the time, Maximilian I was both Holy Roman Emperor and the Count of Tyrol.  
\textsuperscript{13}We thank Marcello Bonazza for his guidance on this point.  
\textsuperscript{14}ACapT, Acta et Producta de Annis 1796-1797, n. 51  
\textsuperscript{15}The region is governed by the Kingdom of Bavaria from 1805-1810 and the Kingdom of Italy from 1810-1814, which are essentially extensions of the French Regime.  
\textsuperscript{16}Authors’ translation from Andreatta and Pace (1981, 19), see also Nequirito (2004); Perini (1852)
2.2.4 Rural Economy under the State

The Napoleonic and Hasburg reforms share many of the same characteristics, particularly when comparing the reforms of Joseph II to the reforms of Napoleon. Besides both possessing immense military power, their reforms aimed to dissolve feudal privileges, improve land productivity, promote private property rights, deregulate trade, and most of all, centralize rules and administrative power under one ruler (Grab, 1995). In both cases, the communities of the Trentino region were a rather insignificant part of a much larger empire, and the institutional changes they faced were largely caused by the ruling power’s desire to assert its political authority over the new territories. Therefore, the centralization of communal resource governance was the byproduct of larger goals, rather than a targeted intervention based on the state of the resource. The state regimes did however, have a direct interest increasing their tax base, and therefore an incentive to maximize the productivity of the land.

In both cases, the era of reform was closely followed by periods of food shortage, economic crisis, and deforestation in the countryside of the affected regions (Monteleone, 1964; Levy, 1988; Tarle, 1950; Nequirito, 2004; Serafini, 1807). While we do not claim this relationship is causal, the correlation between the reforms, high rates of resource extraction, and subsequent crises suggests that resource governance may have played an important role in affecting this change. Prior to 1795, there are several indications that the Austrian governments were directly concerned with the stock of natural resources and sought to regulate its use directly (Tedeschi, 2011). However, the government often had difficulty enforcing such regulations. In the years following Napoleon’s invasion, there is generally less action to regulate resource use. In 1806, the Bavarian Chief Consul issued his concern to the central authority about a need for effective forest laws to control rampant deforestation that occurred after communal governments had become dysfunctional (Nequirito, 2004).

3 Model

To motivate the empirical analysis, this section develops a model of common-pool resource extraction, and how governance rules and their enforcement affect the behavior and welfare of the user group.

\footnote{Levy (1988) discusses the lack of compliance with a ban on converting open land into vineyards during times of food shortage.}
3.0.5 Production

We model a community of $i = 1, 2, \ldots, N$ members who generate income from a common-pool resource. Total resource output for the community is determined by the function $Y = aE - bE^2$ so that the marginal product is decreasing in $E = \sum_i^N e_i$, the total effort exerted on the resource.

For sake of simplicity, we assume that community members are homogenous, risk-neutral profit-maximizers, and that each has a constant marginal cost of effort $c$. Their individual profit function is

$$\pi = \frac{e_i}{E(e_i)}(aE(e_i) - bE(e_i)^2) - ce_i$$

(1)

such that each member’s share of the total output is proportional to his share of total effort.

The noncooperative Nash equilibrium effort level is $e^0 = (a - c)/(N + 1)b$, whereas the profit-maximizing effort level is $e^* = (a - c)/(2Nb)$. Therefore for any $N > 1$ the noncooperative solution exhibits the tragedy of the commons where the resource is overexploited.

3.0.6 Preferences and Consumption

Community members derive utility from consumption of the resource $x$ and the number of their surviving offspring $m$. Surviving children are determined by $m = (1 - \gamma)k$, where $k$ is births and $\gamma \in (0, 1)$ is the child mortality rate. We assume preferences are Cobb-Douglas and correspond to the following utility function:

$$u(x, k, \gamma) = x^\beta m(k, \gamma)^{1-\beta}$$

(2)

where $\beta$ is between 0 and 1.

A member’s income is equal to per capita rent $\pi$ and is allocated between consumption of a numeraire good and expenditure $v$ on surviving children. Members maximize equation 2 by choosing $x = \beta y$ and $v(k, \gamma) = (1 - \beta)y$, so that both consumption and child expenditure are a constant fraction of income. Parents can devote resources to increasing $k$ at cost $p$ per child or decreasing $\gamma$.

3.0.7 Governance

Trentino communities used diverse methods to reduce pressure on the resource stock relative to unregulated levels. It is difficult to capture all of these aspects in a single model. Instead we propose a simple model in which communities choose to impose a per unit tax $(\tau)$

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18 Our simple formulation assumes that the cost of child mortality comes from the sunk investment in children that did not survive. Adding additional costs of child mortality, such as emotional costs, do not change the analysis qualitatively.
on effort. In addition, we assume effort is observed imperfectly with probability $\alpha$ and that the tax revenue generated from observed effort is divided equally among the $N$ community members. Community members set the level of $\tau > 0$ only if they adopt a governance system which costs $\Gamma$ to set up. While simple, we believe this basic structure captures the relevant features and tradeoffs of communal governance in the Trentino. While quotas were more common than taxes in practice, they should have similar effects on behavior. With imperfect enforcement, the tax is analytically convenient as it regulates the level of effort and the incentive to cheat in one step.$^{19}$

For a given $\tau$, each member maximizes

$$\pi = \frac{e_i}{E(e_i)} (aE(e_i) - bE(e_i)^2) - (c + \alpha \tau)e_i + \frac{1}{N} (\alpha \tau E(e_i)).$$  \hspace{1cm} (3)

The corresponding Nash equilibrium effort level in this game is

$$e(\tau) = \frac{a - c - \alpha \tau (1 - 1/N)}{(N + 1)b}$$ \hspace{1cm} (4)

If a governance system is adopted, the community chooses $\tau$ to maximize welfare. We therefore set $e(\tau) = e^*$ and solve for the optimal tax

$$\tau^* = \frac{a - c}{2\alpha}$$ \hspace{1cm} (5)

Equation 5 shows that as the probability of detection ($\alpha$) decreases, the optimal tax increases.$^{20}$ Therefore when $\tau$ is unconstrained, the community can always achieve the first-best effort-level $e^*$. In practice however, there are limits to the size of the tax (or fine) that can be credibly imposed.$^{21}$ We label this maximum feasible tax $\tau^{max}$. If externalities are sufficiently large or enforcement is sufficiently poor, then $\pi'(\tau^* = \tau^{max}) > 0$, implying that communities could increase rent if a higher tax could be imposed, all else equal.

Communities decide whether to adopt a governance system based on whether the gain in rents relative to unregulated use is larger than the fixed setup cost $\Gamma$. The community solves

$$\max_{\tau} \sum_{i=1}^{N} (\pi\left(\tau^*(\alpha, \tau^{max})\right) - \pi^0) - \Gamma 1[\tau > 0] \quad \text{s.t. } 0 \leq \tau \leq \tau^{max} \hspace{1cm} (6)$$

where $1[\tau > 0]$ is an indicator function for whether a governance system is adopted.

$^{19}$In this setting, the tax can be interpreted as a fine for violating a no-harvest policy (i.e. $e^{max} = 0$) that increases with more severe violations.

$^{20}$In some charters there are explicit mentions of an increased fine when a rule is violated at night when detection is more difficult.

$^{21}$See Baland and Platteau (1996, 151) for a discussion of this point.
We expect state-level regimes to have a similar objective function, in that they want to minimize rent dissipation due to excessive effort. The locus of governance impacts the parameters $\alpha, \tau^{\max}$, and $\Gamma$. Following the general guidance of Ostrom (1990) and Baland and Platteau (1996) we expect a greater capacity to impose punishments (higher $\tau^{\max}$) and lower cost of setup (lower $\Gamma$) for state regimes, but a lower cost of catching rule violators (lower $\alpha$) in a communal regime.

4 Data and Descriptive Statistics

4.1 Historical Boundaries

Figure 1 shows the study region. In order to define community boundaries as well as the boundaries between Trent and the Welsch Confines we georeferenced and traced historical boundaries using Dioezesankarte Tirol (Pfaundler (1792) and Miller (1805)), a detailed map of the County of Tyrol and surrounding regions depicting both ecclesiastical and political boundaries as well as the locations of villages and parishes. The original map was created by Johann Anton Pfaundler in 1792 and later improved by Philip Miller in 1805. The basis of the map comes from the highly respected geodetic survey Atlas Tyrolensis (Anich and Hueber 1774). After defining the regions, our results were cross-checked with the map La diocesi di Trento alla meta del 1700 da Donati, C. Ecclesiastici e laici nel Trentino del Settecento (1748-1763) Roma, 1975.

4.2 Demographic Outcomes (1765-1845)

Data on births, deaths, and infant mortality come from registers recorded by parish priests. Following the Council of Trent in the 16th century, church parishes within the Archdiocese were given a mandate to maintain annual registers of baptisms, marriages, and deaths. This practice continued until 1913, and records are now preserved on microfilm at the Archdiocese of Trento.\footnote{We gratefully acknowledge the assistant director of the Archive, Ms. Katia Pizzini, and the staff of the Archive for allowing access to the registers and their generous assistance throughout the data collection process.} Births reflect the number of baptisms, which typically occurred a short time after birth. Some registers record children that die prior to baptism while others do not. Since such cases are not consistently recorded across registers, we restrict births to only children surviving until baptism. Infant deaths are counted as deaths occurring at less than one year of age.
Not all recorded registers survived to the present and some were illegible because of water damage. To balance cross-sectional coverage with sufficient time periods prior to Napoleon’s intervention, we begin our data collection at 1765 and focus on the 227 village parishes with consistent coverage across each year in the study period. For each parish, we collect annual counts at five-year intervals from 1765-1850 (18 periods) of births, deaths, and infant deaths.

An important feature of the register data is that both the Principality of Trent and the Welsch Confines belonged to the same Archdiocese in charge of collecting and maintaining the data. Register data collection by parish priests continued uninterrupted even after the secularization of the Bishopric in 1802. Therefore it is unlikely that differences in record keeping across the two regions are significantly influencing our empirical results.

In the same archive we collect parish population data from the registers of the clergy for the years 1826, 1832, and at five year intervals from 1835-1850. In cases where communities have multiple parishes, we sum the data across parishes to get 177 independent community-level observations. We also use independently collected community-level data on population and assessed land rent for 1847 published by a local statistician (Perini, 1852).²³

Our main outcome variable is the infant mortality rate calculated as the number of infant deaths per live births, where infant deaths are those that occur within the first year after birth. We also calculate the birth rate (births/population), surviving children per population ([births-infant deaths]/population), and the natural growth rate ([births-deaths]/population). Because we do not have population data in every year, throughout the paper we report rates using 1826 population as a constant denominator. In contrast, the infant mortality rate has a contemporaneous denominator.

4.3 Demographic Indicators and Income Relationship

As we argue in section 3, we expect growth rates and surviving children per population in particular to increase and infant mortality rates to decrease with income. In figure 3, we plot the partial cross-sectional correlations between our demographic indicators (1845, 1850) over logged per capita rent (1847). The y axis represents the residuals from regressing the demographic outcome on elevation, terrain slope, latitude, longitude, and district fixed effects. Each graph shows the raw data scaled in proportion to the denominator of the outcome variable (e.g. points in panel a are scaled by births) and a fitted regression line also weighted by the outcome denominator.

Panel a shows a significantly negative relationship between infant mortality and logged rent per capita (slope=-0.035, p-value=.038). The slope estimate indicates that a 10 percent

²³The correlation between the Perini population data and average parish population for 1845 and 1850 is \( \rho = 0.96 \).
increase income would be associated with 3 less infant deaths per 1,000 births. Though the other outcomes in panels b-d have the expected positive slope, their cross-sectional relationship with income is not nearly as strong.\textsuperscript{24}

5 Empirical Analysis

5.1 Identification Strategy

This section outlines the basic identification argument required to relate our difference-in-differences estimates to the impact of state centralization. We start with the assumption that an outcome \( Y \) in community \( i \) at time \( t \) is affected by resource governance and additive components that relate to the community and time period. The outcome in steady-state has the following structure:

\[
Y_{it} = X_{it}^G + \alpha_i + \alpha_t + \nu_{it}. \tag{7}
\]

where \( \alpha_t \) and \( \alpha_i \) are time and community fixed effects, and \( G \) represents the governance regime. The variable \( X \) represents the part of \( Y \) affected by governance and \( X_{it}^G = X_i^G \) represents the steady-state value of \( X_i \) under governance regime \( G \).\textsuperscript{25} \( G \) can take on the values \( A \), \( C_1 \), or \( C_2 \). \( A \) indicates autonomous communal governance, while \( C1 \) and \( C2 \) indicate centralized governance before and after Napoleon’s invasion respectively.

To illustrate the empirical design, we consider three points in time \( \{t^A, 0, t^C\} \) where \( t^A < 0 < t^C \) and distinguish between groups \( T \) (Trent) and \( W \) (Welsch Confines). We assume centralization accomplishes two things: 1) the elimination of traditional communal governance institutions and 2) the imposition of a state-centered regime. The timing of institutional change occurs as follows: Before \( t^A \), groups \( T \) and \( W \) are both communally governed. The Habsburg intervention begins at \( t = t^A \) and applies to group \( W \) only. Napoleon’s intervention begins at \( t = 0 \) and applies to the entire Trentino region.

Because the difference in governance between the two groups is greatest at \( t = 0 \), we use this period as our main point of comparison. The standard difference-in-differences estimator is:

\[
\hat{\beta}_i = (\hat{Y}_{Tt} - \hat{Y}_{T0}) - (\hat{Y}_{Wt} - \hat{Y}_{W0}) \tag{8}
\]

where \( \hat{Y} \) represents a group/period average of the outcome.

In order to relate this measure to the causal impact of centralization, the critical identifi-
cation assumption is that \( \nu_{it} - \nu_{i0} \) is uncorrelated with group assignment. In other words we assume that between periods \( t \) and 0, the only differential change in the outcome between groups is due to centralization. To derive the full impact of centralization, we make two additional simplifying assumptions: 1) outcomes are fully adjusted to steady-state values at \( t = 0 \), and 2) the impact of state-centered governance is homogenous across all communities (i.e. \( X_{ij}^{C1} = X_{ij}^{Cj} \) for \( j = 1, 2 \)). Conditional on these assumptions, the estimator at \( t^A \) and \( t^C \) yields:

\[
\hat{\beta}_{t^A} = X^{C1} - \hat{X}_{W}^{A} = V_{W} \quad (9)
\]

\[
\hat{\beta}_{t^C} = X^{C1} - \hat{X}_{T}^{A} = V_{T}. \quad (10)
\]

Equations 9 and 10 show that an effect of centralization is identified at both \( t^A \) and \( t^C \). For \( t < 0 \) the estimate represents the average effect for Welsch Confine communities, and for \( t > 0 \) the estimate represents the average effect for Trent communities. Note that in both cases, communal autonomy is compared to pre-Napoleon state governance. Therefore, Napoleon’s intervention is only relevant to the estimate in that it causes Trent communities to lose their autonomy. The estimate is unaffected by the performance of state governance post-invasion, because both groups are under the same regime during this time. In the remainder of this section, we consider the special case in which the average effect of communal autonomy on the outcome is identical across \( W \) and \( T \) groups, such that \( V_{W} = V_{T} = V \).

It is unlikely that the full impacts of the reforms arise immediately after they are initiated, and it is worth considering the value of the estimate across all time periods in addition to the steady-state values. To derive transition period estimates, we assume \( X_{t} \) follows a linear path between steady state values that takes \( \lambda > 0 \) periods to complete.\(^{26}\)

The estimator for a generic period \( t \) is then:

\[
\hat{\beta}_{t} = \begin{cases} 
V & \text{for } t \leq t^A \\
(1 - (t - t^A)/\lambda)V & \text{for } t^A < t \leq t^A + \lambda \\
0 & \text{for } t^A + \lambda < t \leq 0 \\
(t/\lambda)V & \text{for } 0 < t \leq \lambda \\
V & \text{for } \lambda < t 
\end{cases} \quad (11)
\]

The basic pattern shown in equation 11 is for \( \hat{\beta}_{t} \) to trend toward 0 following the Habsburg

\(^{26}\)Under this setup a transition from \( X' \) to \( X'' \) starting at \( t = \tilde{t} \) takes on the value \( X_{t} = (1 - z)X' + zX'' \) for \( z \leq 1 \) where \( z = \frac{t - \tilde{t}}{\lambda} \). The linear path is assumed for simplicity, but the qualitative conclusions below can be generalized to any monotonic specification of the transition path.
intervention and then trend away from 0 following Napoleon’s intervention. By design the estimate at the time of Napoleon’s intervention is $\hat{\beta}_0 = 0$. Our framework suggests that if the estimate is identified, then $\hat{\beta}_t$ represents a minimum (if $X^{C1} > X^A$) or a maximum (if $X^{C1} < X^A$) value at $t = 0$. However, this value need not be unique. If group $W$ reaches a steady-state under centralization prior to $t = 0$ (i.e. if $-t_A > \lambda$), then the estimate is flat at 0 from $t = t_A + \lambda$ until $t = 0$.\footnote{Of course if there is no effect of centralization, the estimate equals 0 for all $t$.}

The general nonlinearity of the estimator’s time path provides a useful tool for evaluating the basic identifying assumption. For example, if differential trends between $T$ and $W$, rather than the effects of the interventions, drive the changes in $\hat{\beta}_t$ over time, it seems unlikely that the estimate would correspond to the full path described in equation 11, particularly the abrupt change in trend that occurs at $t = 0$.

In practice, estimates of the full causal effect are more limited in the pre-Napoleon period. While we believe the most important Habsburg reforms start with the sole rule of Joseph II in 1780, as we discussed in section 2 it is possible that earlier Habsburg reforms partially eroded traditional communal institutions in the Welsch Confine villages even prior to 1765. In the language of the above framework, this means we do not observe period $t^A$. To the extent that this is true, we expect the difference-in-differences estimates in 1765 to underestimate $V$ in absolute value relative to steady-state periods post-invasion in which we are relatively confident we observe $t > t^C$.

A related but distinct concern may be that group $W$ has not completed a full transition to a new steady-state under the centralized regime by $t = 0$ (i.e. $\lambda > -t_A$). Note that in this case, both pre and post-invasion estimates of $V$ will be biased toward 0.\footnote{An incomplete transition implies $X_{W0} = (1 - z)X^A + zX^{C1}$ where $z < 0$, which further implies $\hat{\beta}_{t_A} = \hat{\beta}_{t_C} = zV < V$.} Lastly, our exposition has generally ignored the influence of heterogenous effects on the estimate. We leave the exploration of these effects to the empirical analysis.

### 5.2 Estimation

The main estimating equation of our empirical analysis is as follows:

$$Y_{it} - Y_{i1795} = \alpha_{dt} + \beta_t Trent_i + \gamma_t X_i + \epsilon_{it}$$

where the left-hand side variable is the difference between the outcome in year $t$ and the outcome in 1795 for community $i$, $\alpha_{dt}$ is a district-by-year fixed effect, $Trent_i$ is an indicator for whether the community is within the Principality of Trent before Napoleon’s invasion,
and $X_i$ represents time-invariant geographic controls.

An estimate of $\beta_t$ is analogous to $\hat{\beta}_t$ from the section above in that it measures the average difference in the outcomes between groups $T$ and $W$ in period $t$ relative to their difference in 1795, though here the estimate is conditional on the controls. To allow flexibility in the influence of the controls, we estimate their coefficients in each period $t$. In our baseline specification we only control for district-by-year fixed effects and subsequent specifications add controls to assess the robustness of the results.

Since our outcomes of interest are rates for a community/year and the size of communities in our sample varies considerably, one concern is that rates based on small samples will tend to lead to imprecise estimates. With this in mind, we weight regressions by the denominator of the outcome variable (e.g. infant mortality rate regressions are weighted by births) to improve estimation efficiency.\(^{29}\) Finally, all standard error estimates are clustered at the community level to account for serial correlation and are robust to heteroskedasticity.

### 5.3 Main Results

Baseline estimates of $\beta_t$ from equation 12 are shown in Figure 3. Each graph plots estimates of $\beta_t$ in each year with 95 percent confidence intervals and has a vertical line at 1795, the year before Napoleon’s invasion.

Panel (a) shows estimates for the infant mortality rate. In each period, the estimates of $\beta_t$ are positive and both sides of the graph trend toward zero as they get closer to 1795. The estimates farthest from 1795, are generally statistically significant (especially when testing for joint significance) and stabilize around 0.10. This indicates that on average, state centralization leads to an increase of 100 infant deaths per 1,000 births. To give a sense of just how large this effect is, the average rate across the entire sample is roughly 160 infant deaths per 1,000 births. The paths of the graph line up also appear to with the timing of reforms. The estimates on the left side of the graph show a rather pronounced decline after 1780, as Josephinian reforms begin to affect the Welsch Confine communities. On the right side of the graph, the equality in resource governance between the two groups is restored, albeit through centralization.

We also find a marginally significant temporary drop in births per population following centralization, but this difference gradually disappears over time. However, once we account for the higher number of infant deaths following centralization, the rate of children surviving

\(^{29}\)OLS point estimates of our empirical results are qualitatively similar, but generally less precise than weighted least squares estimates. The average gains in precision are largest in the infant mortality rate regressions. Alternative specifications that weight observations by a fixed population are qualitatively similar to the main estimates.
past the first year per population is significantly lower after centralization and this negative effect does appear to persist. Lastly, we find no effect of centralization on the natural growth rate of the population, which among other things may reflect the persistence of inheritance institutions beyond the reforms Cole and Wolf (1999).

In table 2 we report results for a number of specifications for infant mortality and surviving children per population. The estimates and their trajectory over time, particularly for the infant mortality rate, are robust to the inclusion of various geographic controls. In what follows, we continue to focus on the infant mortality rate as our key indicator of interest.

5.4 Heterogenous Impacts of Centralization

In this section we explore how the impact of centralization varies across observable characteristics. Our basic strategy investigates heterogenous effects across simple binary categories indicated by a dummy variable $d$. To keep the analysis from getting unwieldily, we condense the set of results by pooling data from 1765-1780, the periods before Joseph’s major reforms, and 1830-1845, the latest years in our sample. As before, the outcomes are differenced relative to their value in 1795. The general estimating equation is:

$$Y_{it} - Y_{i1795} = \alpha_{it} + \beta_t Trent_i + \theta_t(Trent_i \times d_i) + \delta_t d_i + \gamma_t X_{ik} + \epsilon_{ikt}$$  \hspace{1cm} (13)

Compared to equation 12, equation 13 includes an additional interaction term between the $Trent$ variable and the category of interest with a time-varying coefficient and an additional category-by-year fixed effect. In this equation, estimates of $\beta_t$ represent the average relative change in Trent communities when $d = 0$ and estimates of $\beta_t + \theta_t$ represent the average relative change when $d = 1$. The average change across the full sample depends on the proportion of observations where $d = 1$. The importance of $d$ on the treatment effect can be assessed by observing whether $\theta_t$ is statistically different than zero.

A natural question to ask is whether the impact of the imposed state regime differs by the characteristics of the communal institution it replaces. In section 3, we argue that communities adopt formal institutions when the net gains are sufficiently large. If costs of adoption are homogenous across communities, we would expect average losses from centralization to be larger in the communities that chose to adopt formal institutions. On the other hand, this relationship would be less clear if the reason some communities did not adopt charters was because of a greater ability to regulate resource use through informal governance, such as in the application of norms and social sanctioning mechanisms. Furthermore, if formal rules inhibit the efficacy of these informal mechanisms, as recent research suggests may be the case (Kube and Traxler, 2011; Benabou and Tirole, 2011), we would expect the costs of
centralization to be higher in the communities where formal institutions are absent at the
time of centralization.

We investigate this relationship by estimating equation 13 where Formal is the category
of interest and equals one when rules governing community insiders are codified in an official
charter. These results are reported in column 1 of table 3. The results show that the impact
of centralization both before and after 1795 is significantly smaller in communities where
formal governance is present at the time of the relevant intervention. In fact, the negative
impact shrinks by more than half. In the period before 1795, the reduction is enough to
make the estimate statistically indistinguishable from zero.

An additional consideration we explore is the heterogeneous ability of communities to
assimilate to state-centered governance. Recent empirical findings suggest that historical
experience with centralized governance structures translates into better performance under
externally imposed state regimes (Gennaioli and Rainer, 2007; Michalopoulos and Papaioannou,
2013; Dippel, 2013). In the Trentino region, it was not uncommon for communities to
form federated governance structures that were coordinated across communities, and thus
more “centralized” in nature than single village governance.\(^{30}\) If experience with central-
ized governance matters, we would expect to see relatively better outcomes in (previously)
federated communities following state centralization.

We categorize a community as Federated if it is part of a multi-community organiza-
tion.\(^{31}\) The results are reported in column 2 of table 3. Similar to the case with formal
institutions, federated communities appear to do better than non-federated communities
following centralization, however the difference is federally smaller and not statistically sig-
nificant.

The indicator variables Formal and Federated are not nested, nor are they mutually
exclusive. Communities with formal charters often did not coordinate rules across community
boundaries, and villages within mutli-community organizations often did not codify their
rules internally. We estimate both interactions in one regression. Results are reported in
column 3 of table 3. The estimates in column 3 show that the independent effects of the
two interactions are similar to those found in columns 1 and 2 though the reduction in the
impact for both terms is generally larger in column 3.\(^{32}\) In communities with both formal
and federated institutions, the impact of centralization appears to be completely nullified.

\(^{30}\)Federated institutions typically did not specify the full range of rules as those covered in individual
community charters, but instead tended to specifically address transboundary issues.

\(^{31}\)In practice, we identify a federated community by whether we observe some formal agreement between
communities or evidence of a multi-community assembly.

\(^{32}\)However, we do not find significantly different results in specifications that include an interaction of the
two interaction terms.
Column 4 shows that the inclusion of geographic controls do not influence these relationships qualitatively.

In general, the fact that differential changes in the outcome vary by the characteristics of the local resource institutions in place prior to centralization, seems to support our basic story that the changes we observe are the result of changes in resource governance. It is also worth noting the remarkable consistency in the estimates across both periods. Therefore, our conclusions do not appear to depend on which community group is providing the variation used for identification.

6 Conclusion

Today much of the developing world still depends on traditional communal institutions for the management of vital resources. While these communities are inherently susceptible to the tragedy of the commons, they are many examples where this tragedy is seemingly avoided. It is less clear, however, to what degree these communal institutions promote economically efficient resource use or whether alternative arrangements can generate better long-run outcomes. This paper provides novel quantitative evidence on the economic value of communal institutions, and the potentially devastating impacts of externally imposed institutional change. Furthermore, we find that even in the cases where traditional institutions are formally abolished, their characteristics may be an important determinant of how they fair under imposed governance regimes. More generally, our results suggest that costs of external intervention are lower when the imposed institutions more closely match traditional institutions, a point that is emphasized by Ostrom (2005).

Given the long time horizon of both institutional and environmental change, it is often difficult to identify the long-run implications of institutional change from short-run disturbances. History is helpful in this case, and this paper explores a relatively long, yet sufficiently precise, panel of data on rural communities to identify the consequences of changes to resource management. Future work investigating large time series and long run historical data sets of communal resource outcomes more generally are an important step toward developing better informed and wiser policy approaches to resource management in the developing world.
References


7 Data Appendix

Historical Boundaries
In order to define community boundaries as well as the boundaries between Trent and the Welsch Confines we georeferenced and traced historical boundaries using Dioezesankarte Tirol (Pfaundler (1792) and Miller (1805)), a detailed map of the County of Tyrol and surrounding regions depicting both ecclesiastical and political boundaries as well as the locations of villages and parishes. The original map was created by Johann Anton Pfaundler in 1792 and later improved by Philip Miller in 1805. The basis of the map comes from the highly respected geodetic survey Atlas Tyrolensis (Anich and Hueber 1774). After defining the regions, our results were cross-checked with the map La diocesi di Trento alla meta del 1700 da Donati, C. Ecclesiastici e laici nel Trentino del Settecento (1748-1763) Roma, 1975.

Travel Time to Trento
Travel times are calculated between parish centers and Buonconsiglio Castle in Trento measured in terms of minimum hiking time between the two points. Travel times are calculated in ArcGIS using the path distance function which incorporates elevation data to measures distance over a three dimensional surface. Travel time is a function of the path distance divided by travel speed, which is decreasing in the steepness of terrain traversed. Travel speed is calculated using a hiking velocity function proposed by Tobler (1993):

\[ V = 6e^{-3.5|S + 0.05|} \]

where \( V \) is velocity in km/hr and \( S = \Delta \text{elevation}/\Delta \text{distance} = \tan(\theta) \) where \( \theta \) is the slope angle (in degrees) of the topographic surface. \(^{33}\) Lastly, we again follow Tobler (1993) and assume \(|\theta| > 80 \) is impassible and corresponding path segments are assigned a velocity of zero.

Topography
Topography data is derived from the Shuttle Radar Topography Mission (SRTM). Average elevation represents the average pixel value within the community boundaries. Average slope is calculated by first converting the elevation DEM into a slope raster corresponding to the average change in elevation across neighboring cells divided by the average distance

\(^{33}\)The function \( V(S) \) is symmetric and shifted slightly left of zero so that travel time generally increases when traversing steeper slopes, but gives a slightly higher penalty for uphill as opposed to downhill travel. Speed on flat land is approximately 5km/hr and reaches a maximum of 6km/hr on a slight decline (\( \theta \approx -3.5 \)). Hiking speed drops quickly with slope and is less than 1km/hr for \(|\theta| > 30 \).
between cell centers. The slope raster pixels are then averaged within community boundaries.

**Land Area**

Land area is calculated as the total area of a community polygon minus area classified as water or as unproductive in the Corine Land Cover 2006 raster data.

**Annual Temperature**

We estimate the average air temperature in each village using elevation from the sea level data obtained from the Google Elevation API and yearly air temperature estimates from a high-altitude stalagmite record in the Central Alps of Austria. Point estimates are calculated from the chemical composition of a stalagmites retrieved in a cave in Spannagel, in the Central Alps of Austria and studied by Mangini, Spotl, and Verdes (2005). We use a common method proposed by Kappenberger and Kerkmann (1997) which uses elevation differences between the community and the Spannagel cave calculate annual temperature at the community level:

\[
\text{temperature}_{it} = \text{temperature}_{i}^{S} + 0.0065(\text{elevation}^{S} - \text{elevation}_{i})
\]

where \(i\) indexes a community, \(t\) indexes a year, and \(S\) indicates data from the Spannagel cave (\(\text{elevation}^{S}=2347\) meters above the sea level).
Notes: The map indicates community boundaries and governance jurisdictions prior to Napoleon. Communities within the Bishopric of Trent are locally autonomous and are in light green. Communities within the Welsch Confines are under direct Habsburg control and are in dark green. Pink dots indicate parishes where register data is available. Thick black lines indicate district boundaries.
Figure 2: Demographic Indicators and Income per Capita (1847)

Notes: Each panel shows the partial correlations between a demographic indicator and per capita rent after controlling for observed geographic characteristics of the community: average elevation, average terrain slope, latitude, longitude, travel time to Trento, and district fixed effects. The demographic indicator in panel a is infant mortality rate (deaths(<1yr)/births), panel b is birth rate (births/population), panel c is (births-deaths(<1yr))/population), and panel d is the natural growth rate ([births-deaths]/population) divided by population). The fitted regression line is in red. Regressions are weighted by their denominator (e.g. infant mortality rate regressions are weighted by population). The relative size of the raw data points are proportional to the weights used. Rent per capita is calculated for the year 1847 using data from Perini (1852). All demographic indicators are calculated from the parish register data for 1845.
Figure 3: Difference in Difference Estimates Over Time

Notes: Parameter estimates for Trent variable in each period relative to 1795 after controlling for district fixed effects. The outcome in panel a is infant mortality rate (deaths(<1yr)/births), panel b is birth rate (births/population), panel c is (births-deaths(<1yr))/population), and panel d is the natural growth rate ((births-deaths)/population) divided by population). The outcome variable in panel a is the infant mortality rate, in panel b is the infant mortality rate. Dashed lines represent 95% confidence intervals. Standard errors are clustered at the community level.
Table 1: Descriptive Statistics

<table>
<thead>
<tr>
<th></th>
<th>Trent</th>
<th>Welsch Confines</th>
<th>Trent Share</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Totals</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>122</td>
<td>46</td>
<td>73%</td>
</tr>
<tr>
<td>1852 Population (thousands)</td>
<td>133</td>
<td>72</td>
<td>65%</td>
</tr>
<tr>
<td>Land Area (km$^2$)</td>
<td>323</td>
<td>178</td>
<td>64%</td>
</tr>
<tr>
<td><strong>Means</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Average Elevation (m)</td>
<td>1,176</td>
<td>1,018</td>
<td>158</td>
</tr>
<tr>
<td>Average Terrain Slope (%)</td>
<td>40.55</td>
<td>40.92</td>
<td>-0.38</td>
</tr>
<tr>
<td>Travel Time to Trento (hours)</td>
<td>9.50</td>
<td>9.04</td>
<td>0.46</td>
</tr>
<tr>
<td>Year of First Documented Evidence</td>
<td>1359</td>
<td>1352</td>
<td>6.7</td>
</tr>
<tr>
<td>Charter Adoption by 1700 (share)</td>
<td>0.46</td>
<td>0.52</td>
<td>-0.06</td>
</tr>
<tr>
<td>Charter Adoption by 1800 (share)</td>
<td>0.55</td>
<td>0.61</td>
<td>-0.06</td>
</tr>
<tr>
<td>1897 Land Use</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Crop/Orchard/Vineyard (share)</td>
<td>0.15</td>
<td>0.16</td>
<td>-0.01</td>
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<tr>
<td>Pasture Share (share)</td>
<td>0.26</td>
<td>0.21</td>
<td>0.06</td>
</tr>
<tr>
<td>Forest Share (share)</td>
<td>0.54</td>
<td>0.58</td>
<td>-0.04</td>
</tr>
</tbody>
</table>

**Note:** Cross-sectional characteristics for communities in Trent and the Welsch Confines. White standard errors are used to calculate p-values.
Table 2: Difference in Difference Estimates

<table>
<thead>
<tr>
<th>Relative to 1795</th>
<th>Infant Mortality Rate</th>
<th>Surviving Children per Population</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>1765</td>
<td>0.076</td>
<td>0.086</td>
</tr>
<tr>
<td></td>
<td>[-0.05]</td>
<td>-0.055</td>
</tr>
<tr>
<td>1770</td>
<td>0.119***</td>
<td>0.110**</td>
</tr>
<tr>
<td></td>
<td>-0.045</td>
<td>-0.043</td>
</tr>
<tr>
<td>1775</td>
<td>0.064</td>
<td>0.062</td>
</tr>
<tr>
<td></td>
<td>-0.051</td>
<td>-0.057</td>
</tr>
<tr>
<td>1780</td>
<td>0.102**</td>
<td>0.086*</td>
</tr>
<tr>
<td></td>
<td>-0.044</td>
<td>-0.048</td>
</tr>
<tr>
<td>1785</td>
<td>0.083**</td>
<td>0.084*</td>
</tr>
<tr>
<td></td>
<td>-0.039</td>
<td>-0.044</td>
</tr>
<tr>
<td>1790</td>
<td>0.033</td>
<td>0.03</td>
</tr>
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<td></td>
<td>-0.04</td>
<td>-0.045</td>
</tr>
<tr>
<td>1795</td>
<td>0</td>
<td>0.04</td>
</tr>
<tr>
<td></td>
<td>-0.044</td>
<td>-0.052</td>
</tr>
<tr>
<td>1800</td>
<td>0.051</td>
<td>0.04</td>
</tr>
<tr>
<td></td>
<td>-0.058</td>
<td>0.116</td>
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<tr>
<td>1805</td>
<td>0.038</td>
<td>0.022</td>
</tr>
<tr>
<td></td>
<td>-0.05</td>
<td>-0.051</td>
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<tr>
<td>1810</td>
<td>0.098**</td>
<td>0.108**</td>
</tr>
<tr>
<td></td>
<td>-0.043</td>
<td>-0.046</td>
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<tr>
<td>1815</td>
<td>0.038</td>
<td>0.053</td>
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<tr>
<td></td>
<td>0.053</td>
<td>0.058</td>
</tr>
<tr>
<td>1820</td>
<td>0.154***</td>
<td>0.168***</td>
</tr>
<tr>
<td></td>
<td>0.051</td>
<td>0.054</td>
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<tr>
<td>1830</td>
<td>0.109**</td>
<td>0.113**</td>
</tr>
<tr>
<td></td>
<td>-0.049</td>
<td>-0.052</td>
</tr>
<tr>
<td>1835</td>
<td>0.127***</td>
<td>0.115***</td>
</tr>
<tr>
<td></td>
<td>-0.043</td>
<td>-0.043</td>
</tr>
<tr>
<td>1840</td>
<td>0.133***</td>
<td>0.131***</td>
</tr>
<tr>
<td></td>
<td>-0.041</td>
<td>-0.041</td>
</tr>
<tr>
<td>1845</td>
<td>0.102**</td>
<td>0.097**</td>
</tr>
<tr>
<td></td>
<td>-0.045</td>
<td>-0.04</td>
</tr>
</tbody>
</table>

District Fixed Effects: Yes, Yes, Yes, Yes, Yes, Yes
Geographic Controls: No, Yes, Yes, No, Yes, Yes
Institutional Controls: No, No, Yes, No, No, Yes
Observations: 2856, 2856, 2856, 2856, 2856, 2856
R-squared: 0.166, 0.208, 0.221, 0.229, 0.254, 0.260

Note: The table provides estimates of the Trent coefficient in equation 12 for each year. Outcomes are in first-differences relative to 1795. Column 1 estimates control for district-by-year fixed effects and correspond to the estimates in figure 3. Column 2 adds controls for average elevation, average terrain slope, latitude, longitude, and travel time to Trento. Column 3 adds controls for whether the community has a formal charter prior to centralization, and whether the community is associated with a multi-community organization. Regressions are weighted by the denominator of the outcome variable. Standard error estimates are clustered at the community level and are robust to heteroskedasticity and serial correlation. Statistical significance is indicated as *p < 0.1, **p < 0.05, ***p < 0.01.
### Table 3: Difference in Difference Estimates by Community Type

<table>
<thead>
<tr>
<th>Infant Mortality Rate</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>A) 1765-1780 relative to 1795</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Trent</td>
<td>0.187***</td>
<td>0.119***</td>
<td>0.233***</td>
<td>0.228***</td>
</tr>
<tr>
<td></td>
<td>[0.050]</td>
<td>[0.042]</td>
<td>[0.062]</td>
<td>[0.063]</td>
</tr>
<tr>
<td>Trent*Formal</td>
<td>-0.130***</td>
<td>-0.144***</td>
<td>-0.139***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>[0.048]</td>
<td>[0.055]</td>
<td>[0.056]</td>
<td></td>
</tr>
<tr>
<td>Trent*Federated</td>
<td>-0.048</td>
<td>-0.082</td>
<td>-0.094</td>
<td></td>
</tr>
<tr>
<td></td>
<td>[0.071]</td>
<td>[0.074]</td>
<td>[0.076]</td>
<td></td>
</tr>
<tr>
<td><strong>Impact by Community Type:</strong></td>
<td></td>
<td></td>
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<td><strong>B) 1830-1845 relative to 1795</strong></td>
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<tr>
<td>Trent</td>
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<td>0.149***</td>
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**Note:** The above table reports results from estimating equation 13 in the text. The left hand side variable is the infant mortality for a community relative to its rate in 1795. Panel A reports coefficients for 1765-1780 and panel B reports coefficients for 1830-1845. The impact of centralization across population subgroups is reported below the coefficient estimates in each panel. Regressions are weighted by the denominator of the outcome variable. Standard error estimates are clustered at the community level and are robust to heteroskedasticity and serial correlation. Statistical significance is indicated as *p < 0.1, **p < 0.05, ***p < 0.01.