

Institutions, Wages and Inequality: The Case of Europe and its Periphery (1500-1899)*

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This version: January 2005

Abstract

This paper explores the long-run relationship between institutions and wage outcomes in Europe and its periphery. I find that cities that exercised stronger institutional protection of private property experienced: (i) higher levels of both skilled and unskilled real wages, as well as (ii) lower levels of inequality as measured by the skilled-unskilled wage ratio. While the first result corroborates existing work on the positive growth effects of better institutions, the second finding is more novel to the literature. Some explanations are proposed for how stronger institutions can cause an increase in the relative supply of skilled workers, thus lowering wage inequality.

Keywords: Institutions; Wage inequality; European cities.

JEL Classification: J31, N13, N33, O10, O15

*I thank Daron Acemoglu, Philippe Aghion, Robert Allen, Filipe Campante, Francesco Caselli, Ruimin He, Michael Kremer, James Robinson, Jeffrey Williamson, as well as the editor, Robert Margo, and two anonymous referees for their helpful comments. Thanks are also due to seminar participants at Harvard, the 2004 Conference on Economic Growth and Distribution (Lucca, Italy), and the Singapore Management University (SMU). I am grateful to SMU for hosting me from June-August 2003, during which a substantial draft of this paper was written. All errors are my own.

1 Introduction

In recent years, the research agenda on the sources of economic growth has seen a renewed focus on the role of institutions. Following North (1981) and North and Weingast (1989), much of this literature has dwelt on one particular form of institutions, namely the provision of a secure system of property rights protection. According to this view, a strong set of institutions is needed to prevent the executive or other elite groups from laying claim to private wealth, in order to foster an environment conducive to the accumulation of capital, and hence growth. A slew of empirical studies have lent weight to this hypothesis. Knack and Keefer (1995) and Barro (1997) among others identified a positive cross-country correlation between aggregate growth and measures of institutions such as the rule of law. More recently, Acemoglu, Johnson and Robinson (2001) used settler mortality rates as an instrumental variable for the risk of capital expropriation, in order to isolate the causal effect of institutions on income levels.

Within the context of Europe, institutions have received much attention as a leading explanation for why some states grew faster than others over the long run.¹ Characterizing absolutist rulers as those who “saw the legal system as an instrument of control rather than a constraint on their actions” (p. 673), DeLong and Shleifer (1993) showed that European states run by such “princes” experienced slower growth in city populations than those governed as free republics. Acemoglu, Johnson and Robinson (2002) pursued this research program further by constructing indices on the strength of capitalist institutions stretching back to 1000A.D. Using these measures, the authors argued that access to Atlantic trade was crucial for growth, and that it was precisely those cities in Atlantic states with strong institutions where commerce expanded in full force. Notably, England (after the “Glorious Revolution” of 1688A.D.) and the Netherlands (after shaking off Spanish rule) established political systems where legislative power resided in a parliament, effectively checking any arbitrary powers of the sovereign, and thereby facilitating the rise of an urban merchant class.

This paper contributes to the growing empirical literature on institutions, by exam-

¹The large variance in the timing and pace of growth across Europe is well-documented. DeLong and Shleifer (1993) identified a northward shift in the growth of cities away from the Mediterranean Basin between 1000-1500A.D. Separately, Van Zanden (1999) and Allen (2001) observed that a divergence in wage levels had set in between Western and Eastern Europe by the mid-1700s, before the onset of the Industrial Revolution.

ining how they affected wage outcomes in a broad sample of 17 cities in Europe and its periphery (including Istanbul). The use of wage data confers two distinct advantages over existing studies. First, real wages are a more direct measure of the welfare of workers than aggregate incomes, since wages in principle capture the actual returns earned by labor.² This paper thus tests for the effects of institutions on income levels directly, without having to use a proxy for the dependent variable such as urbanization or city population, as has been the practice to date. Second, the data from Allen (2001) and Özmucur and Pamuk (2002) includes wages for both skilled craftsmen and unskilled laborers, therefore allowing us to investigate the relationship between institutions and wage inequality. Historians have documented how the urbanization of Europe was accompanied by a widening rich-poor divide, which on occasion boiled over into outbreaks of violence such as food riots (Hohenberg and Lees 1985, p. 131). Inequality was thus a very real problem, and it is relevant to ask how improvements in institutions might have affected the relative economic position of different groups of workers.³

Using the institutional indices coded by Acemoglu, Johnson and Robinson (2002), I show in Section 2 that cities with more stringent protection of private property saw higher subsequent real wages for both skilled craftsmen and unskilled laborers. This finding is consistent with the broader literature reviewed above on the positive growth effects of better property rights protection. More interestingly, I also find that stronger initial institutions were associated with lower levels of urban wage inequality, as measured by the skilled-unskilled wage ratio.

Importantly, these findings are robust to the inclusion of several key variables that have been proposed as competing hypotheses to the importance of institutions. First, Engerman and Sokoloff (2002) and Lal (1998) have argued that factor endowments were a more fundamental determinant of growth, as the initial cards dealt by nature often shaped the types of political and economic institutions set up to adapt to these external conditions. Nevertheless, I find that the institutional variables were significant even when controlling for variables related to the supply of land and labor, which suggests that

²This argument in favor of factor returns data over macro aggregates as a measure of welfare is well-articulated in the economic history literature. See, for example, Williamson (1995).

³In a modern context, De Soto (2000) has proposed that the establishment of legal institutions to recognize the ownership of property occupied by the poor could help to improve their economic condition significantly, as this would allow them to use their property as collateral to gain access to credit.

institutions were not completely endogenous to factor endowments in the case of Europe and its periphery. Similarly, the effect of institutions was unchanged when a dummy for Atlantic trader states was included, which would appear to mitigate the overriding role assigned to Atlantic commerce as a pre-condition for growth emphasized by Acemoglu, Johnson and Robinson (2002). Finally, Glaeser et. al. (2004) have argued for the primacy of human capital over institutions as a source of long-run growth by examining post-World War II panel data. For the historical sample in this paper though, the use of adult literacy rates as a proxy for human capital did not overturn the significance of institutions as a determinant of real wage levels (although the results are slightly less robust for the regressions involving wage inequality).

The key finding that stronger initial institutions were linked to lower skill premiums clearly warrants more discussion, as there is little theoretical work (to the best of my knowledge) that posits a causal link from institutions to inequality. Section 3 offers some candidate explanations that describe how an improvement in the security of private property can increase the incentives for skill accumulation. This would raise the supply of skilled workers in urban centers, and subsequently reduce the wage gap between craftsmen and unskilled workers. Section 4 of the paper presents some concluding remarks.

2 Empirical evidence from Europe and its periphery

The empirical work in this paper is based on a data set of 17 cities in Europe and its periphery, put together from several independent sources. This sample contains a good geographic spread, including cities at the heart of the Atlantic economy, such as London and Amsterdam, as well as several cities in Eastern Europe and the periphery, such as Gdansk, Warsaw and Istanbul.⁴

The data on wages are from Allen (2001) for European cities and from Özmucur and Pamuk (2002) for Istanbul.⁵ The wages (in grams of silver per day) of building craftsmen and construction laborers are used as measures of skilled and unskilled wages respectively. To obtain real wages, the silver wages are deflated by price indices constructed by the

⁴A full list of cities and the countries that they were mapped to can be found in the Data Appendix under the item labelled “Total Population”.

⁵Özmucur and Pamuk (2002) gathered and constructed their data in a manner similar to Allen (2001), which facilitates direct comparisons between the two sources.

respective authors, based on consumption baskets comprising mainly foodstuffs, cloth and fuel items. I use 50-year averages of this data (for 1500-1549, 1550-1599, . . . 1850-1899), in order to net out short-term fluctuations. This provides up to 8 data points for each city, although the panel is slightly unbalanced due to a small number of missing entries.

The empirical work focuses on the role of institutions as a key determinant of these wage outcomes. Property rights institutions, or the lack thereof, clearly left an imprint on the economic well-being of the residents of pre-modern Europe. Its rulers were constantly in need of resources to finance wars or sovereign expenditures, and obtained these funds either by coercion or by turning to capitalist interests such as urban federations or bankers (Tilly 1990). Blockmans (1994) writes that: “Initially, population growth and the commercialization of the economy simply produced more collective wealth, of which the kings and other princes took their share in the form of rising and new income from tolls, mintage, justice, and the granting of privileges. As a further step, the princes discovered they could raise more money through new forms of taxation and by loans from foreign merchants” (p. 225). There was clearly a lot of variation across Europe in the extent to which private property or capital could be regarded as secure. At one extreme were the absolutist regimes, described by DeLong and Shleifer (1993) as states in which all property “was always potentially insecure” (p. 679), citing France under Louis XIV in the 17th century and Spain under Philip II in the 16th century as two of the most egregious examples.⁶ On the other hand, Europe also witnessed the emergence of state systems in which the powers of the ruler were circumscribed: Great Britain became a constitutional monarchy after 1688, while merchant interests came to dominate the affairs of state in the Dutch Republic.

As measures of the strength of institutions, I use the indices recently coded by Acemoglu, Johnson and Robinson (2002). These indices take values on an integer scale from 1 to 7 (higher numbers denoting stronger institutions), which helps to capture the large variance in property rights regimes observed in practice. Two such measures are available: The “constraint” index measures the extent to which codified laws or regulations formally curbed the ability of the state executive to extract private wealth. On the other hand, the

⁶On the latter example, Albeladejo (1994) provides a comprehensive account of how the Habsburgs sent the Castilian cities into a systematic decline with their penchant for raising finances through tax increases and unsustainable debt issues.

“protection” index captures the degree to which property rights protection was formally extended to urban merchants, particularly in the event of a dispute with the monarch or other nobles. The scoring of these indices followed stringent guidelines: The “constraint” variable was coded using the same criteria as the Marshall and Jaggers (2000) Polity IV “constraint on the executive” index, while the “protection” variable adapted this framework to the context of ruler-merchant relations. (For more details on the precise coding criteria used, please see the Data Appendix.)

I treat these indices as measures of initial institutions, and use them as explanatory variables for the average wages observed over each subsequent 50-year period. In theory, one would expect a lack of secure protection for merchant capital to mute the level of economic activity and commerce, hence depressing the wages of workers employed in these urban centers.

2.1 Econometric specification

To explore the relationships between institutions and wage outcomes, I run regressions of the following form:

$$\log w_{jt} = \alpha \times INST_{it} + \beta' \times X_{ijt} + D_j + D_j \times t + D_t + \epsilon_{ijt} \quad (1)$$

where the dependent variable, w_{jt} , is either the level of real wages or the skilled-unskilled wage ratio. Here, i indexes states/countries, j indexes cities, and t denotes time (1500, 1550, ..., 1850). The key coefficient of interest is α , which captures the effect of institutions (coded at the country-level) on wage outcomes in individual cities.

The vector X_{ijt} controls for other potential determinants of wages. This includes first a measure of the city population from Bairoch, Batou and Chèvre (1988) for European cities and Chandler (1987) for Istanbul. Such variables relating to city size have been used in the economic history literature to proxy for the overall level of prosperity and growth in an urban center, and hence should be positively correlated to the level of real wages. Next, the supply of labor would have mattered too for wages. I thus control for the ratio of the city population over the total country population, where the variable in the denominator is drawn from McEvedy and Jones (1978). I view this ratio as an estimate for the concentration of labor in the city relative to its surrounding rural areas. Higher values would thus imply more downward pressure on the level of urban wages.

Last but not least, I include an estimate of the land-labor ratio, namely the agricultural land area divided by the total country population. States endowed with relatively more arable land would in principle have been able to generate more agricultural output per head to raise the populace above a subsistence level. A higher land-labor ratio would thus imply potentially higher levels of worker productivity and hence have a positive impact on real wages. Such domestic constraints on the land would have been especially binding pre-1850 when the volume of cross-border trade in grain was limited, both because of the high cost of transportation and trade barriers such as Britain’s Corn Laws. Estimates of the agricultural land area are taken from the Food and Agricultural Organization’s (FAO) *Production Yearbook*; given the lack of historical data on land utilization, this measure is a constant value over time, reflecting the total potential area that could be exploited for agricultural purposes in each country.⁷ (A more detailed description of the data sources is provided in the Data Appendix.)

The estimated equation in (1) includes city and year fixed effects, D_j and D_t respectively, as well as a city-specific linear time trend, $D_j \times t$ (city dummies interacted with the year of observation), to mitigate any potential omitted variables bias. In particular, it is possible that the observed correlation between institutions and wages may in fact be independently driven by an unobserved third variable that itself exhibits time persistence, such as successive technological improvements or an underlying industrialization process. The use of city-specific time trends thus helps to control for such omitted variables.⁸ To further address possible simultaneity issues, all the explanatory variables used are measures of initial conditions at the beginning of each 50-year period, so that the results reflect how a given configuration of institutions and factor supplies impacted subsequent wage outcomes.

⁷The regression results are very similar when using the data on total land area from the same FAO source in place of the agricultural land variable. The results were similarly robust to replacing the total country population throughout by estimates of the size of the rural population. For more information on how this latter variable was constructed, please see the Data Appendix.

⁸The structure of this time trend is kept to a simple linear process, since the inclusion of a quadratic term would increase the number of parameters to a minimum of $17 \times 3 = 51$ (for the city dummies and their interactions with year and year squared). This would have been an unreasonably large number of parameters to estimate with only a total of 118 data points.

2.2 Institutions and real wages

Applying this econometric framework, I find strong evidence that cities with better initial institutions did indeed witness higher real wages for both skilled and unskilled labor.

Looking first at the results for skilled wages, a basic regression of the log real wage for craftsmen provides hints of a positive correlation with the institutional variables:

$$\log \text{Real skilled wages} = \left\{ \begin{array}{l} 0.018 \quad \times \text{“Constraint”} \\ (0.023) \\ 0.039 \quad \times \text{“Protection”} \\ (0.025) \end{array} \right\} + D_j + D_j \times t + D_t + \epsilon_{ijt}$$

The standard errors reported are clustered within each country-year cell, to allow for correlated shocks in the ϵ_{ijt} term for cities in the same country (such as Florence, Milan and Naples in Italy) in any given year.⁹ Although the coefficients estimated above are not statistically significant, the addition of the vector of controls, X_{ijt} , delivers much more compelling results.

Table 1 shows that property rights institutions had a positive and significant impact on real skilled wages, after suitably controlling for various variables related to the supply of land and labor. Specifically, Column 1 includes the city population, the ratio of city to total population, and the ratio of agricultural land to total population; the regressions are now also run with weights proportional to the city population. Both the “constraint” and “protection” indices are significant in their respective regressions at the 5% level. The results are very similar when using either institutional variable, which is not surprising given the high correlation between these two indices (correlation coefficient: 0.88).¹⁰

Moreover, the coefficients of the three control variables all show up with the predicted sign. Real skilled wages are positively (and significantly) correlated with city population levels, so cities at a more advanced stage of urban development did sustain higher incomes. On the other hand, the ratio of the city to total population has a negative (though not statistically significant) effect, suggesting that a higher relative supply of urban labor tends to lower wages in the city. Finally, the coefficient for the land-labor ratio is positive

⁹I also report results later in the paper that cluster the standard errors by country only, to alleviate concerns about how serial correlation in both the wage and institutions variables might lead to a downward bias in the standard errors.

¹⁰In contrast, Allen (2003) does not find institutions to be a significant determinant of his Allen (2001) real wage data. This is likely due to the fact that the variable used in his study was the cruder binary indicator variable for absolutist “princes” from DeLong and Shleifer (1993).

and significant, consistent with the interpretation that less population pressure on the land leads to higher levels of worker productivity and hence higher wages. Overall, given that the institutional indices are significant after controlling for the supply of land and labor, it is hard to make the case that factor endowments were a more fundamental determinant of income levels than institutions as Engerman and Sokoloff (2002) have posited, albeit for a different historical and geographical context (namely, the colonial settlement of the Americas).

[Insert Table 1]

Based on these estimates, the predicted impact of an improvement in institutions is non-trivial. *Ceteris paribus*, a change in property rights regime from the worst possible to the best (from 1 to 7) would have implied an increase of $0.046 \times 6 = 0.276$ log units in the real skilled wage based on the “constraint” coefficient in Column 1. The mean log real skilled wage for the 59 data points in the sample with a “constraint” value of 1 was 1.77, so this wholesale improvement in institutions would have amounted to a respectable 16% increase on average in the log real wage for such cities.

The rest of Table 1 performs a series of comprehensive robustness checks. Column 2 examines some further forces that might have been driving income levels in Europe during this period. In particular, Acemoglu, Johnson and Robinson (2002) have drawn attention to the importance of access to Atlantic trade for a city’s prospects for growth. I thus include a dummy variable for Atlantic traders (equal to 1 for cities in England, the Netherlands, France and Spain). Column 2 also controls for the latitude of the city (as a proxy for geography or climate), as well as the fraction of years of war. This last variable was constructed at the country level from Kohn’s (1999) *Dictionary of Wars* for 40-year windows (namely 1480-1519 for the 1500 data point etc). However, none of these three variables appears to be a particularly robust determinant of movements in the skilled wage. More importantly, these additional controls did not detract from the significance of “constraint” or “protection”.¹¹

¹¹These conclusions do not change if the Atlantic dummy, latitude or the war variable are added individually to the regression specification. The results are also not altered substantively if a shorter 20-year window (1490-1509 for the 1500 data point etc) is used for the coding of the war variable instead. This statement on robustness also applies to the findings below for unskilled wages (Table 2) and wage inequality (Table 3).

In Column 3, I remove a possible outlier, London, given its status as the industrial leader of the pack, while also being the only city to obtain a score of 7 on either institutional index. This actually increases the institutions coefficients, both of which are now significant at the 1% level, a result which should allay some of the concern that the institutions variables may simply be picking up the effects of an underlying industrialization process. Interestingly, the Atlantic dummy now assumes a large positive and significant role in this specification. The results are similarly robust to the removal of Amsterdam (regression not shown), another major commercial leader within our sample of cities. The removal of Istanbul also does not alter the results much (regression not shown), so the findings are not being skewed by the merging of wage data from separate sources. However, the results are slightly weaker when the regressions are run unweighted in Column 4. The significance levels of “constraint” and “protection” drop off, although the point estimates do remain positive.¹²

Column 5 considers a different way of computing the standard errors by clustering them by country, instead of within each country-year group. Bertrand, Duflo and Mullainathan (2004) recommend this procedure as a way of correcting for the downward bias in standard errors that emerges when both dependent and explanatory variables are serially correlated in a differences-in-differences estimation.¹³ Reassuringly, this correction only reduces the statistical significance of the institutions variables slightly.

The final specification in Table 1 includes estimates of adult literacy rates from Allen (2003), in order to test the contention in Glaeser et. al. (2004) that human capital is a more fundamental determinant of long-run growth than institutions. In this context, adult literacy was gauged by the ability to sign one’s name, and is clearly a simple proxy at best for human capital, while being subject to the usual caveats about the reliability of such historical data. (Estimates were available for all states except the Ottoman Empire.)

¹²The statistical significance of the institutions coefficients is similarly diminished when the total population of each country is used as regression weights instead. However, the results involving unskilled wages (Table 2) and the skilled-unskilled wage ratio (Table 3) are not as sensitive to this alternative choice of regression weights, with “constraint” and “protection” both remaining significant in these cases.

¹³This critique is most salient in applications with one policy intervention, but where many pre- and post-intervention data points have been used to estimate a treatment effect. As a further possible fix for this problem, Bertrand et. al. (2004) also recommend averaging the pre- and post-intervention data when estimating the treatment effect. In this regard, the fact that the wage variables in this paper are 50-year averages instead of the annual time series underlying Allen (2001) should further lessen any potential bias in the standard errors.

Although the regression results in Column 6 suggest a positive (though insignificant) effect of higher literacy rates on real skilled wages, this does not detract from the significance of either “constraint” or “protection”. In short, there is insufficient evidence to suggest that initial human capital mattered more than institutions in determining skilled wages.¹⁴

At this juncture, despite the robust correlation uncovered between the security of private property and real skilled wages, it is useful to caution against interpreting this as foolproof evidence of a direct causal relationship from institutions to wage outcomes. The inclusion of city and year fixed effects and a city-specific linear time trend should help to control for other variables that might be driving both the institutions and wage data simultaneously, although this might not be fully adequate if the omitted variables are underlying processes that follow a more complicated time trend. Also, the use of initial values for the right-hand-side variables helps to reduce the possibility of reverse causality, but this is a simple solution at best given the absence of a suitable instrumental variable, particularly for the measures of institutions.¹⁵

Turning now to the data on real unskilled wages, one reaches very similar conclusions on the positive effect of institutions. In fact, the estimates indicate that property rights institutions had an even larger impact on unskilled workers than they did for skilled craftsmen. This is already clear from the larger coefficients on “constraint” and “protection” in the basic (unweighted) specification without the controls, X_{ijt} :

$$\log \text{Real unskilled wages} = \left\{ \begin{array}{l} 0.063^{**} \quad \times \text{“Constraint”} \\ (0.025) \\ 0.084^{***} \quad \times \text{“Protection”} \\ (0.027) \end{array} \right\} + D_j + D_j \times t + D_t + \epsilon_{ijt}$$

Here, ** and *** denote significance at the 5% and 1% levels respectively, where the standard errors have been clustered within each country-year group.

Expanding the analysis to include more control variables in Table 2, one finds that the institutions coefficients tend to increase further; for example, in Column 1, the “constraint” coefficient is now 0.085 and that for “protection” is 0.088, slightly less than double

¹⁴The institutions measures remain significant when the adult literacy variable is added to the specification in Column 1, which excludes the Atlantic dummy variable.

¹⁵Although settler mortality rates have become a popular instrument for institutions since Acemoglu, Johnson and Robinson (2001), this variable cannot be used for our purposes. The available data lists mortality rates of European settlers in the former colonies and excludes coverage of most of Europe itself, nor does it extend sufficiently far back in time to the 1500s where the sample in this paper starts.

the size of the corresponding estimates for real skilled wages in Table 1. In particular, an increase from 1 to 7 on the “constraint” index would raise real unskilled wages by $0.085 \times 6 = 0.510$ log points. Since the mean log real unskilled wage for data points with a “constraint” value of 1 was 1.27, this improvement in the property rights environment would have boosted log real unskilled wages dramatically by 40% on average for such cities. Comparing this with the previous estimate for skilled wages, one can already see the key implication for wage inequality, namely that unskilled laborers gained more relative to skilled craftsmen from better institutions.

[Insert Table 2]

The results in Table 2 also show that the variables related to population size and land area generally have the predicted effects on real unskilled wages. Once again, cities with larger populations experienced higher wage levels; in Columns 1-5, the city population variable has a positive coefficient when significant. Also, a higher relative supply of urban labor appears to lower unskilled wages, though the coefficient estimates here are not statistically significant. Lastly, a high agricultural land-population ratio tends to raise the incomes of the unskilled.

As was the case in Table 1, the role of institutions is robust to various modifications. Both “constraint” and “protection” beat out the Atlantic dummy, latitude and the fraction of years of war in the regressions in Column 2. The findings are moreover not driven by London (Column 3), Amsterdam or Istanbul (regressions not shown). The institutions coefficients remain significant when the regressions are run unweighted (Column 4), unlike the corresponding regressions for skilled wages in Table 1. Clustering the standard errors by country also makes little difference (Column 5). Finally, controlling for adult literacy in Column 6 does not erase the importance of the institutions variables.

In summary, using either measure of the security of private property, I find that states with stronger institutions experienced higher real wages for both skilled and unskilled workers. This conclusion is especially strong since it holds after controlling for other potential determinants of wage levels such as factor supplies, access to Atlantic trade and even adult literacy.

2.3 Institutions and wage inequality

I turn now to examine the relationship between institutions and inequality, using the skilled-unskilled wage ratio as a measure of inequality in labor market returns. A basic (unweighted) regression of the log relative wage on just the institutional variables, city and year fixed effects, and city time trends reveals a negative partial correlation:

$$\log \text{Relative wage} = \left\{ \begin{array}{l} -0.045^{**} \times \text{“Constraint”} \\ (0.018) \\ -0.045^{**} \times \text{“Protection”} \\ (0.018) \end{array} \right\} + D_j + D_j \times t + D_t + \epsilon_{ijt}$$

Note that the effect of property rights institutions is significant at the 5% level, where the standard errors are once again clustered by country-year groups.

This conclusion is reinforced by Table 3 where other control variables are added. In Column 1, “constraint” and “protection” both retain a significant effect (at the 1% level) on the log relative wage. I also obtain a negative (though not statistically significant) coefficient on the city to total population ratio, consistent with what theory would predict: Low values of this variable indicate a relative abundance of rural workers, which would imply a large pool of surplus unskilled labor that could potentially migrate to the urban centers. This would tend to depress the wages of the unskilled relative to the skilled, hence increasing urban inequality. Of some interest, the city population coefficient shows up with a negative sign when significant, suggesting that larger cities were less unequal.

[Insert Table 3]

This strong negative relationship between institutions and inequality survives the full battery of robustness checks. Controlling for access to Atlantic trade, latitude, and the fraction of years of war does not alter the effects of “constraint” or “protection” (Column 2). The results are also similar if London (Column 3), Amsterdam or Istanbul (regressions not shown) are removed. Both institutions coefficients remain significant at the 5% level when the regressions are run unweighted (Column 4), or when the standard errors are clustered by country (Column 5). The inclusion of adult literacy as a proxy for human capital does reduce the significance levels, but does not change the negative sign of the institutions coefficients (Column 6). The Atlantic dummy takes on a large positive and significant value in this specification, suggesting that one of the cities with access to

Atlantic trade might be driving this result. Indeed, when this regression is re-run without Amsterdam, our results are partially restored: Both “constraint” (coefficient = -0.044) and “protection” (coefficient = -0.048) are now significant at the 10% level.

Figure 1 offers an illustration of this distinct relationship between institutions and wage inequality. The vertical axis plots the residuals from the regression in Column 2 of Table 3 but excluding the “constraint” variable, while the horizontal axis plots the residuals from regressing “constraint” against the same set of covariates (weighted by the city population). This partial scatter plot reveals the negative correlation between the relative wage and institutions after filtering out the effects of population size, land endowments, access to Atlantic trade and other controls. The regression line in the figure has a slope of -0.038 (after weighting by city population), significant at the 1% level. The “STR” observation for 1650 which has the largest positive wage residual at the top of the figure may appear like a potential outlier, but the slope of the regression line still remains negative (value = -0.037) and significant if this single point is removed.

[Insert Figure 1]

The potential quantitative impact of a strengthening of institutions on wage inequality turns out to be large. Using the Column 1 estimates in Table 3, an increase in “constraint” from a score of 1 to 7 would result in an expected decrease of $0.038 \times 6 = 0.228$ log points in the skilled-unskilled wage ratio. Given that the average log relative wage for cities with a “constraint” value of 1 was 0.51, this would imply an average decline in wage inequality of 45%. This represents a large compression in the wage distribution, even if it were gradually spread out over the course of several centuries.

I conclude that stronger property rights institutions were associated with a reduction in wage inequality, with unskilled workers receiving a larger boost in wage returns than skilled workers. The robustness of this finding also provides a partial response to a common criticism of institutional codings like the Acemoglu, Johnson and Robinson (2002) variables, namely that they rely too much on *ex post* assessments after income levels have been observed, and are thus inevitably correlated with whether countries are seen to be growing well. While this might explain the observed correlation with real wages, it is unclear why these institutional variables would also necessarily exhibit such a systematic negative correlation with **relative** wages. Instead, it would appear that the concept of

“institutions” does capture some substantive economic forces that have predictable effects on other aspects of the economy, including the distribution of incomes.

3 Some explanations for the link between institutions and inequality

The observed relationship between stronger security of property rights and lower levels of inequality clearly merits more discussion. Indeed, existing theoretical models have been more concerned with the reverse direction of causation, namely how an initial configuration of the income distribution can affect a society’s choice of policies regarding state taxation (Alesina and Rodrik 1994) and the extent of property rights protection (Tokman 1999). These two models are however not particularly relevant to the context of pre-modern Europe, given the general absence of voting democracies then. Separately, Glaeser, Scheinkman and Shleifer (2003) propose a model in which legal institutions are more liable to be subverted by the rich or politically influential in countries with higher initial levels of inequality. Their model however takes a stylized investment game between individual players as a starting point, from which it is hard to relate the quality of institutions to labor market outcomes.

Therefore, I focus instead on explanations of how the institutional environment (taken as exogenous) can affect subsequent wage inequality, consistent with the evidence presented in this paper. A decline in the skilled-unskilled wage ratio might be due to two proximate causes, either a decline in the relative demand for or a rise in the relative supply of skilled labor. In the context of pre-modern Europe, it seems unlikely that the former of these possibilities was at play: Improvements in property rights institutions would raise income levels and hence the demand for construction, but that likely would have raised the wages of skilled craftsmen and unskilled laborers across the board, rather than being so largely biased in favor of unskilled workers.

The explanations I propose thus center on how having better institutions can actually improve the incentives for workers to acquire skills, thereby increasing the relative supply of skilled workers. The working paper version of this paper (Chor 2004) presented a general equilibrium model in the spirit of Harris and Todaro (1970) that incorporated such labor supply shifts within the context of the rural-urban migration that drove the expan-

sion of Europe’s cities:¹⁶ When the protection of property rights improves and wages rise for both skilled and unskilled workers, it becomes relatively less costly for rural laborers to migrate into the city and attempt to acquire a skilled craft. This induces a movement of workers into urban centers, raising the supply of potential apprentices, and hence of skilled craftsmen over the course of time. If this labor supply response is sufficiently large, skilled wages could rise less relative to unskilled wages in the long-run equilibrium after labor supplies have adjusted, thus resulting in lower urban wage inequality.¹⁷ In support of this model, I presented evidence in this working paper confirming that cities with stronger institutions had larger populations relative to their surrounding rural population, suggesting that rural-urban migration did take place in larger volumes into such cities.¹⁸

As an alternative explanation, one can also posit a link between institutions and prevailing interest rates that could affect the incentives to accumulate human capital more directly.¹⁹ It has been argued that one of the key benefits of the strengthening of property rights institutions was that it lowered the costs of borrowing by increasing the likelihood of repayment by debtors, even when the debtor was the state or sovereign. North and Weingast (1989), for example, have shown that interest rates paid on long-term government borrowing declined in England after the “Glorious Revolution”. Insofar as the market interest rate is positively correlated with the opportunity cost of not working now and investing in skills instead, a drop in this interest rate would increase a given urban resident’s likelihood of embarking on an apprenticeship to try to acquire a skilled craft.

¹⁶De Vries (1984) and Hohenberg and Lees (1985) provide an in-depth study of the important role played by rural-urban migration in driving the growth of Europe’s cities.

¹⁷If one adopts the view that inequality is detrimental to growth, then improvements in institutions that reduce inequality could provide an impetus for further increases in income levels. Empirically, however, there is a lack of consensus on this link between inequality and growth, and hence this paper does not emphasize this potential feedback effect. For example, Alesina and Rodrik (1994) find that inequality appears to hurt growth, but Forbes (2000) estimates this relationship to be positive instead. Using non-parametric methods, Banerjee and Duflo (2003) argue for a non-linear trend, with both increases and decreases in inequality associated with lower subsequent growth.

¹⁸This evidence is clearly consistent with DeLong and Shleifer’s (1993) finding that cities under absolutist princes experienced slower population growth.

¹⁹I am grateful to an anonymous referee for pointing out this potential channel of impact.

4 Concluding remarks

This paper has examined the relationships between institutions, real wages and wage inequality, in the context of the long-run growth experience of cities in Europe and its periphery. I find that cities which enforced more stringent property rights protection had higher real wages for both skilled craftsmen and unskilled laborers, a result which dovetails with earlier work on the positive impact of institutions on aggregate incomes. By focusing on wages instead of macro aggregates, I also identify an additional effect: Cities with stronger institutions witnessed lower levels of wage inequality, as measured by the skilled-unskilled wage ratio.

The paper suggests two candidate explanations for the observed negative relationship between institutions and urban wage inequality, both of which work through the predicted effect of institutions on the supply of skilled relative to unskilled labor. The basic intuition behind these arguments is fairly general: When the acquisition of skills is costly, an improvement in institutions can lead to an increase in the supply of skilled labor, since workers will be more willing to invest in education or training when they are more assured of reaping the full rewards of their investment in human capital or when prevailing interest rates are lower.

There is clearly scope for more work to be done to explore the linkages between institutions, growth and inequality that have been uncovered in this study. It would be important for example to establish how the advancement of property rights institutions affected the gap between wage owners and landlords, as captured by wage-rental ratios. This is a key component of aggregate inequality in pre-modern Europe that the regressions in this paper have not been able to address, largely because of the limited historical data on land rents particularly for the European periphery.²⁰ Along these lines, it would also be interesting to investigate if similar relationships between the quality of institutions and inequality continue to hold for the more recent, post-World War II period.

²⁰In an earlier version of this paper available upon request, I present some preliminary results using the available rent series from Clark (2002) for London, and O'Rourke and Williamson (2002) for France and the Netherlands. Using regression specifications similar to equation (1), but excluding year fixed effects, there is suggestive evidence based on wage-rental ratios in this sample of three countries that institutions helped to improve the position of labor (both skilled and unskilled) relative to landowners. These results are however not robust to the inclusion of year dummies.

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Data Appendix

Wages: From Allen (2001) for all cities, except Istanbul, which is from Özmucur and Pamuk (2002). The underlying annual series for Allen (2001) can be downloaded from: <http://www.econ.ox.ac.uk/Members/robert.allen/WagesPrices.htm>

Total population: From McEvedy and Jones (1978), with geometric interpolation where necessary. In units of millions of people. Cities are matched to states as follows: Antwerp (ANT) to Belgium; Amsterdam (AMS) to The Netherlands; London (LON) to England; Florence (FLO), Milan (MIL) & Naples (NAP) to Italy; Valencia (VAL) & Madrid (MAD) to Spain; Paris (PAR) & Strasbourg (STR) to France; Augsburg (AUG) & Leipzig (LEI) to Germany; Vienna (VIE) to Austria; Gdansk (GDA), Krakow (KRA) & Warsaw (WAR) to Poland; Istanbul (IST) to the Ottoman Empire.

City population: From Bairoch, Batou and Chèvre (1988) for all cities, except Istanbul, which is from Chandler (1987). In units of millions of people.

Rural population: Computed as $(1 - \text{Urbanization Rate}) \times (\text{Total Population})$, with geometric interpolation where necessary. Data on urbanization rates are from Acemoglu, Johnson and Robinson (2002).

Agricultural land area: Sum of arable and pasture land areas in each country, as listed in the Food and Agricultural Organization's (FAO) *Production Yearbook*, in tens of millions of hectares. This figure does not change much across different years of the *Production Yearbook*, and so a constant value is used for each country, taken from the earliest possible edition of the *Yearbook* where data for the country is reported.

Institutions: From Acemoglu, Johnson and Robinson (2002). Indices were coded on a scale of 1 to 7, with larger numbers indicating stronger capitalist institutions. For the constraint on executive index, the coding followed the criteria established in the Marshall and Jaggers (2000) Polity IV dataset: Scores range from a 1 where "there are no regular limitations on the executive's actions", to a 7 where "accountability groups have effective authority equal to or greater than the executive in most activity". The index takes on the Polity IV values for 1800 and 1850, where available. Earlier and missing values were then coded independently by the authors and a research assistant for cross-checking purposes. Separately, Acemoglu, Johnson and Robinson also constructed the protection of capital index. There, the coding criteria range from a 1 for polities where "merchants have no effective protection against

arbitrary confiscation by the ruler”, to a 7 where “the government is formed by and largely influenced or controlled by merchants and middle classes”.

The value of the index in a given year is the average over five observations centered on that year, for example, the 1700 data point is the average over 1680, 1690, 1700, 1710 and 1720. Prior to 1700, the index is only coded at century intervals, so for 1550 and 1650, I use the index value from 1500 and 1600 respectively. The interested reader is directed to Sections 4.1 and 8.3 of Acemoglu, Johnson and Robinson (2002) for more details on the coding of these measures of institutions.

Dummy for Atlantic trade: Equals 1 for cities in England, the Netherlands, France, and Spain. Equals 0 otherwise.

Latitude: From the *Atlas of the World*, Oxford University Press.

Fraction of Years of War: Based on Kohn (1999). Fraction of years in a given 40-year window (1480-1519 for the 1500 data point etc) during which a given country was at war or had a significant domestic rebellion.

Adult literacy: From Allen (2003). Estimates are of the proportion of the adult population that could sign their own name in each country, and are available for 1500, 1600, 1700, 1750 and 1800. Missing years were imputed by geometric interpolation. Data is available for all states except the Ottoman Empire.

Table 1
Institutions and the Real Wages of Skilled Building Craftsmen

Dependent variable = $\ln(\text{Real Skilled Wage})$

	Weighted (1)	Weighted (2)	Weighted Excl London (3)	Unweighted (4)	Weighted Cluster by country (5)	Weighted (6)
Institutions variable ("Constraint" ; "Protection")	0.046** ; 0.050** (0.021) ; (0.021)	0.048** ; 0.055*** (0.019) ; (0.018)	0.074*** ; 0.069*** (0.022) ; (0.022)	0.016 ; 0.043 (0.024) ; (0.027)	0.048* ; 0.055* (0.025) ; (0.025)	0.072*** ; 0.078*** (0.027) ; (0.025)
City population	0.15* ; 0.15** (0.08) ; (0.07)	0.14* ; 0.15** (0.07) ; (0.07)	-0.44 ; -0.24 (0.35) ; (0.34)	0.04 ; 0.07 (0.11) ; (0.11)	0.14* ; 0.15** (0.07) ; (0.06)	-0.29 ; -0.04 (0.39) ; (0.41)
City pop. / Total pop.	-2.31 ; -2.20 (2.03) ; (2.04)	-1.85 ; -1.73 (1.91) ; (1.90)	1.80 ; 0.98 (2.72) ; (2.81)	-0.72 ; -0.79 (2.43) ; (2.36)	-1.85 ; -1.73 (2.89) ; (2.96)	-6.80* ; -7.06* (3.94) ; (3.75)
Agri. land / Total pop.	2.68*** ; 2.76*** (0.91) ; (0.92)	2.87*** ; 2.99*** (0.88) ; (0.88)	3.25*** ; 3.47*** (0.86) ; (0.92)	0.93 ; 1.16 (0.81) ; (0.81)	2.87* ; 2.99* (1.46) ; (1.46)	0.60 ; 0.73 (0.87) ; (0.86)
Dummy for Atlantic trader	—	0.23 ; 0.16 (1.64) ; (1.64)	11.28*** ; 10.18*** (2.15) ; (2.11)	0.11 ; 0.35 (2.28) ; (2.21)	0.23 ; 0.16 (0.82) ; (0.81)	1.89 ; 1.98 (2.03) ; (1.96)
Latitude	—	0.17 ; 0.18 (0.16) ; (0.16)	0.45** ; 0.47** (0.17) ; (0.19)	-0.21 ; -0.19 (0.16) ; (0.16)	0.17 ; 0.18 (0.14) ; (0.14)	-0.18 ; -0.18 (0.27) ; (0.26)
Fraction of years of war	—	0.133 ; 0.144* (0.082) ; (0.084)	0.146* ; 0.161** (0.075) ; (0.079)	0.083 ; 0.078 (0.091) ; (0.089)	0.133 ; 0.144 (0.090) ; (0.097)	-0.030 ; -0.014 (0.087) ; (0.087)
Adult literacy rate	—	—	—	—	—	0.42 ; 0.40 (0.47) ; (0.44)
R^2	0.96 ; 0.96	0.96 ; 0.96	0.92 ; 0.92	0.86 ; 0.87	0.96 ; 0.96	0.92 ; 0.93
Number of observations	118	118	110	118	118	99

Notes: For each column, the first set of coefficients is from the regression that uses the constraint on executive index, while the second set reports results using the protection of capital index. Robust standard errors are reported, clustered within each country-year group (except in Column 5, which is clustered by country only). ***, ** and * indicate significance at the 1%, 5% and 10% levels respectively. All regressions include city and year fixed effects, as well as city-specific linear time trends (city dummies interacted with year of observation). Weighted regressions use the city population in each year as regression weights.

Table 2
Institutions and the Real Wages of Unskilled Building Laborers

Dependent variable = ln(Real Unskilled Wage)

	Weighted (1)	Weighted (2)	Weighted Excl London (3)	Unweighted (4)	Weighted Cluster by country (5)	Weighted (6)
Institutions variable ("Constraint" ; "Protection")	0.085*** ; 0.088*** (0.023) ; (0.025)	0.087*** ; 0.093*** (0.022) ; (0.022)	0.116*** ; 0.108*** (0.025) ; (0.026)	0.063** ; 0.090*** (0.027) ; (0.029)	0.087** ; 0.093** (0.032) ; (0.030)	0.098*** ; 0.104*** (0.031) ; (0.029)
City population	0.23** ; 0.23** (0.10) ; (0.10)	0.23** ; 0.23** (0.10) ; (0.09)	-0.43 ; -0.11 (0.41) ; (0.40)	0.06 ; 0.11 (0.11) ; (0.11)	0.23** ; 0.23** (0.10) ; (0.08)	-0.58 ; -0.25 (0.52) ; (0.55)
City pop. / Total pop.	-1.86 ; -1.62 (2.26) ; (2.30)	-1.42 ; -1.15 (2.08) ; (2.10)	2.60 ; 1.29 (3.13) ; (3.31)	0.91 ; 0.75 (2.32) ; (2.27)	-1.42 ; -1.15 (3.29) ; (3.40)	-6.36 ; -6.71* (4.14) ; (3.91)
Agri. land / Total pop.	2.66** ; 2.79** (1.16) ; (1.20)	2.84** ; 3.02** (1.18) ; (1.21)	3.30*** ; 3.63*** (1.18) ; (1.30)	0.89 ; 1.24 (0.72) ; (0.77)	2.84 ; 3.02 (2.01) ; (2.06)	0.02 ; 0.19 (1.07) ; (1.09)
Dummy for Atlantic trader	—	0.63 ; 0.57 (1.72) ; (1.73)	10.58*** ; 8.81*** (3.01) ; (2.91)	1.29 ; 1.28 (1.69) ; (1.74)	0.63 ; 0.57 (1.01) ; (0.97)	-3.64 ; -3.52 (2.90) ; (2.86)
Latitude	—	0.01 ; 0.03 (0.19) ; (0.19)	0.49** ; 0.52** (0.20) ; (0.22)	-0.15 ; -0.12 (0.16) ; (0.16)	0.01 ; 0.03 (0.17) ; (0.18)	-0.17 ; -0.16 (0.34) ; (0.33)
Fraction of years of war	—	0.125 ; 0.143 (0.102) ; (0.106)	0.145 ; 0.168 (0.096) ; (0.102)	0.067 ; 0.075 (0.078) ; (0.078)	0.125 ; 0.143 (0.080) ; (0.089)	-0.044 ; -0.023 (0.104) ; (0.103)
Adult literacy rate	—	—	—	—	—	0.49 ; 0.47 (0.73) ; (0.68)
R^2	0.95 ; 0.95	0.95 ; 0.95	0.90 ; 0.90	0.85 ; 0.86	0.95 ; 0.95	0.91 ; 0.92
Number of observations	118	118	110	118	118	99

Notes: See notes for Table 1.

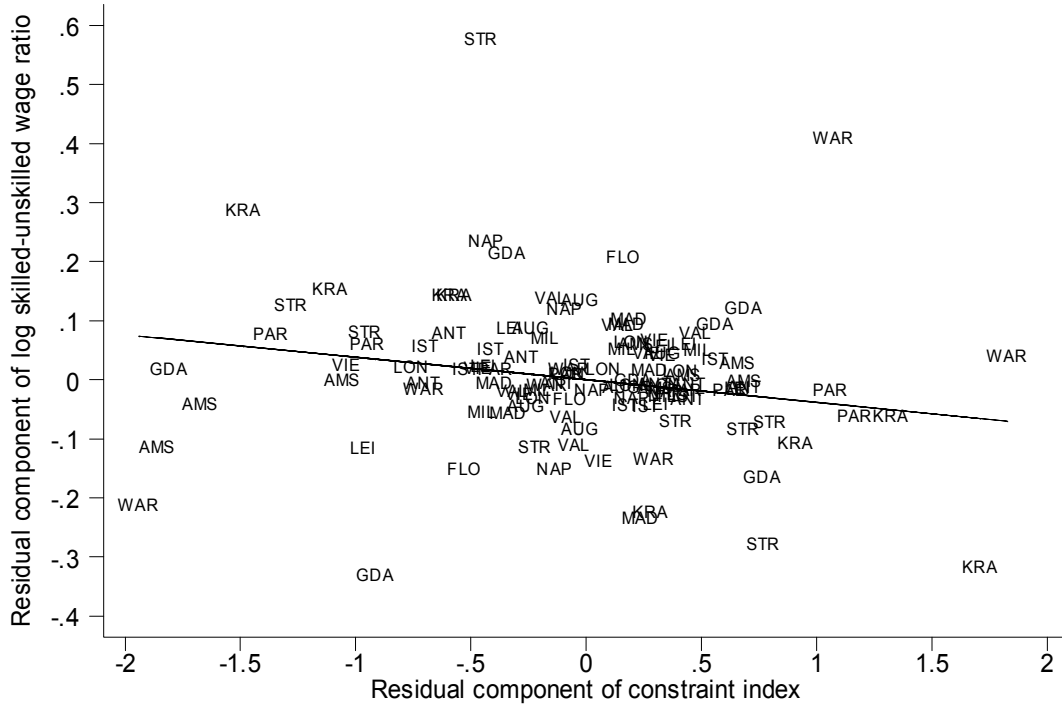
Table 3
Institutions and Wage Inequality

Dependent variable = $\ln(\text{Real Skilled Wage} / \text{Real Unskilled Wage})$

	Weighted (1)	Weighted (2)	Weighted Excl London (3)	Unweighted (4)	Weighted Cluster by country (5)	Weighted (6)
Institutions variable ("Constraint" ; "Protection")	-0.038*** ; -0.038*** (0.010) ; (0.010)	-0.038*** ; -0.038*** (0.009) ; (0.010)	-0.042*** ; -0.038*** (0.012) ; (0.011)	-0.047** ; -0.047** (0.019) ; (0.020)	-0.038** ; -0.038** (0.014) ; (0.015)	-0.025 ; -0.027 (0.018) ; (0.017)
City population	-0.09** ; -0.08** (0.04) ; (0.04)	-0.09** ; -0.08** (0.04) ; (0.04)	-0.01 ; -0.13 (0.15) ; (0.14)	-0.03 ; -0.04 (0.07) ; (0.07)	-0.09* ; -0.08* (0.04) ; (0.04)	0.29 ; 0.20 (0.32) ; (0.34)
City pop. / Total pop.	-0.45 ; -0.58 (0.78) ; (0.81)	-0.43 ; -0.58 (0.79) ; (0.81)	-0.80 ; -0.31 (1.23) ; (1.23)	-1.63 ; -1.54 (1.15) ; (1.18)	-0.43 ; -0.58 (0.90) ; (1.00)	-0.44 ; -0.35 (2.15) ; (2.16)
Agri. land / Total pop.	0.02 ; -0.03 (0.55) ; (0.56)	0.03 ; -0.03 (0.57) ; (0.58)	-0.05 ; -0.16 (0.67) ; (0.70)	0.05 ; -0.08 (0.52) ; (0.58)	0.03 ; -0.03 (0.71) ; (0.75)	0.58 ; 0.53 (0.77) ; (0.78)
Dummy for Atlantic trader	—	-0.40 ; -0.41 (0.97) ; (0.97)	0.70 ; 1.37 (1.61) ; (1.56)	-1.19 ; -0.93 (1.86) ; (1.82)	-0.40 ; -0.41 (0.49) ; (0.50)	5.53** ; 5.50** (2.30) ; (2.30)
Latitude	—	0.17* ; 0.15 (0.10) ; (0.10)	-0.04 ; -0.05 (0.16) ; (0.11)	-0.06 ; -0.07 (0.16) ; (0.16)	0.17* ; 0.15* (0.07) ; (0.08)	-0.02 ; -0.02 (0.26) ; (0.25)
Fraction of years of war	—	0.008 ; 0.001 (0.042) ; (0.043)	0.001 ; -0.008 (0.045) ; (0.046)	0.015 ; 0.003 (0.041) ; (0.040)	0.008 ; 0.001 (0.029) ; (0.025)	0.014 ; 0.009 (0.053) ; (0.053)
Adult literacy rate	—	—	—	—	—	-0.08 ; -0.07 (0.47) ; (0.45)
R^2	0.78 ; 0.77	0.78 ; 0.77	0.76 ; 0.76	0.61 ; 0.60	0.78 ; 0.77	0.74 ; 0.74
Number of observations	118	118	110	118	118	99

Notes: See notes for Table 1.

Figure 1
The Relationship between the Log Relative Wage and the “Constraint” Index



Notes: Slope of regression line is -0.038, significant at the 1% level, using city population as regression weights, and clustering the standard errors within each country-year group. The residual values plotted are from a regression of the log skilled-unskilled wage ratio or the constraint index respectively on: city population, the ratio of city to total population, the ratio of agricultural land area to total population, the Atlantic dummy, latitude, fraction of years of war, city fixed effects, year fixed effects, and city-specific linear time trends, weighted by city population.