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Estimating Agglomeration Economies with History, Geology, and Worker Effects

Pierre-Philippe Combes, Gilles Duranton, Laurent Gobillon, and Sébastien Roux

1.1 Introduction

Productivity and wages are higher in larger cities and denser areas. This fact was first noted by Adam Smith (1776) and Alfred Marshall (1890) and has been confirmed by the modern empirical literature on this topic (see Rosenthal and Strange [2004] for a review). The measured elasticity of local productivity with respect to employment density is typically between 0.04 and 0.10. We confirm this on French data. Panel A of figure 1.1 plots mean log wages against employment density over 1976 to 1996 for 306 French employment areas. The measured density elasticity of wages is 0.05. Panel B of figure 1.1 conducts a similar exercise using log of total factor productivity (TFP) for the same 306 employment areas over 1994 to 2002. The measured density elasticity of TFP is 0.04.

To draw inference from figure 1.1, two fundamental identification prob-

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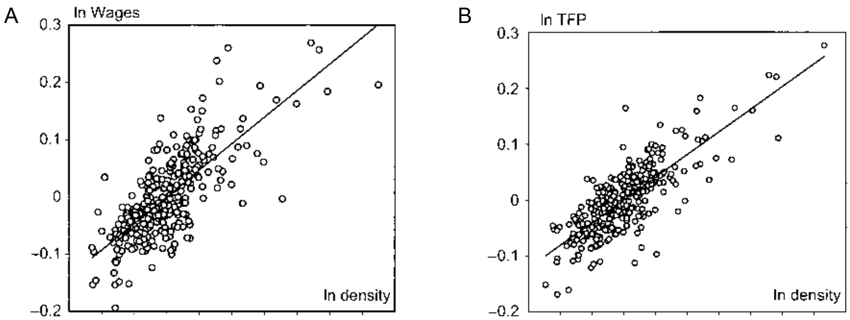

Fig. 1.1 Productivity and employment density in France: *A*, wages and employment density (306 employment areas, 1976 to 1996 average); *B*, TFP (Olley-Pakes) and employment density (306 employment areas, 1994 to 2002 average)

Source: DADS, BRN, RSI, SIREN, and authors' calculations.

Note: All variables are centered around their mean. The R^2 is 56 percent in panel A and 61 percent in panel B. See the rest of the chapter for the details of the calculations.

lems must be dealt with. First, density and measures of productivity (wage or TFP) may be simultaneously determined. This could happen because more productive places tend to attract more workers and as a result become denser. An alternative explanation, albeit equivalent from an econometric perspective, is that there may be a missing local variable that is correlated with both density and productivity. We refer to this issue as the "endogenous quantity of labor" problem. Since Ciccone and Hall (1996), a standard way to tackle this problem has been to use instrumental variables (IV).

The second major identification problem is that more productive workers may sort into denser areas. This may occur for a variety of reasons. For instance, skilled workers may have a stronger preference for high density, perhaps because density leads to better cultural amenities. Alternatively, the productivity benefits of high density may be stronger for skilled workers. These explanations suggest that it is not only density that we expect to be simultaneously determined with productivity but also the characteristics of the local workforce. To make matters worse, we expect characteristics that are not usually observed by the statistician, such as ambition or work discipline, to matter and to be spatially unevenly distributed. For instance, French university professors may have similar observable characteristics everywhere, but a disproportionate fraction of the better ones are working in or around Paris. We refer to this problem as the "endogenous quality of labor" problem. Since Glaeser and Maré (2001), a standard way to tackle this problem has been to use the longitudinal dimension of the data.

One may also be concerned that density affects productivity in a myriad of ways, directly and indirectly (see Duranton and Puga [2004] for a review). Denser markets allow for a more efficient *sharing* of indivisible facilities (e.g., local infrastructure), risks, and the gains from variety and specialization. Next, denser markets also allow for a better *matching* between employers and employees, buyers and suppliers, partners in joint projects, or entrepreneurs and financiers. This can occur through both a higher probability of finding a match and a better quality of matches when they occur. Finally, denser markets can facilitate *learning* about new technologies, market evolutions, or new forms of organization. Some of these mechanisms (e.g., matching) may have instantaneous effects, while others (e.g., learning) may take time to materialize.[1]

Our chapter addresses the issues of endogenous quantity and endogenous quality of labor. We do not attempt to distinguish between the different channels through which density could affect productivity and only aim at estimating a total net effect of density on wages. To deal with the endogenous quantity of labor problem, we take an IV approach, using both history and geology as sources of exogenous variation for population. To deal with the endogenous quantity of labor problem, we proceed as in Combes, Duranton, and Gobillon (2008) and use the longitudinal dimension of extremely rich wage data. We impose individual fixed effects and local time-varying fixed effects in a wage regression. This allows us to separate local from individual effects and to reconstruct some local wages net of individual observed and unobserved effects. Note that both approaches are necessary to identify the effect of density on productivity. Neither approach on its own would be sufficient.

Our main results are the following. The raw elasticity of mean wages to density is slightly below 0.05. Controlling only for the endogenous quantity of labor bias lowers this estimate to around 0.04. Historical and geological instruments lead to roughly the same answer. Controlling only for the endogenous quality of labor bias yields an even lower density elasticity of 0.033. Controlling for both source of biases leads to a coefficient of 0.027. When we also control for the fact that agglomeration economies spill over the spatial units boundaries, our preferred estimate for the elasticity of wages to local density stands at 0.02. These results are broadly confirmed when we use an alternative measure of productivity, TFP, rather than wages.

We draw a number of conclusions from this work. First, even though we control for two major sources of bias, we still find evidence of small but significant agglomeration effects. Second, the sorting of workers across places is a quantitatively more important issue than their indiscriminate agglomeration in highly productive locations. Third, the importance of unobserved labor quality implies that wages should be favored over TFP and other productivity measures, since wage data are our main hope to deal with unobserved worker characteristics.

The rest of this chapter is as follows. Section 1.2 provides a simple model of productivity and wages in cities and discusses the two main estimation issues. Section 1.3 presents the wage data and our approach to the endog-

1. Even if the overall effect is positive, there may also be many negative effects of density on productivity due to crowding or congestion.

enous quality of labor bias. Section 1.4 presents our instruments and discusses the details of our instrumentation strategy. Our results for wages are presented in section 1.5, while those for productivity follow in section 1.6. Finally, section 1.7 concludes.

1.2 Identification Issues when Estimating Agglomeration Effects

We consider a simple theoretical model of the relationship between local characteristics and wages or productivity. Take a competitive firm i operating under constant returns to scale. Its output y_i depends on the amounts of capital k_i and labor l_i it uses and its total factor productivity A_i:

$$y_i = A_i k_i^{\alpha} l_i^{1-\alpha}. \tag{1}$$

If all firms face the same interest rate r, the first-order conditions for profit maximization imply that the wage *rate* is given by:

$$w_i = (1-\alpha)\left(\frac{\alpha}{r}\right)^{\alpha/(1-\alpha)} A_i^{1/(1-\alpha)}. \tag{2}$$

Taking logs directly leads to

$$\ln w_i = \text{Constant} + \frac{1}{1-\alpha}\ln A_i. \tag{3}$$

The whole focus of the agglomeration literature, then, is on how the local characteristics of area a where firm i is located determine productivity. We assume that TFP depends on a vector of local characteristics X_a and (observed and unobserved) firm characteristics μ_i:

$$\ln A_i = X_{a(i)}\varphi + \mu_i. \tag{4}$$

Inserting into equation (3) implies:

$$\ln w_i = \text{Constant} + \frac{1}{1-\alpha}(X_{a(i)}\varphi + \mu_i). \tag{5}$$

This equation can in principle be estimated using wage data and local characteristics. An alternative strategy is to insert equation (4) into equation (1), take logs, and estimate:

$$\ln y_i = \alpha \ln k_i + (1-\alpha)\ln l_i + X_{a(i)}\varphi + \mu_i. \tag{6}$$

Hence, both wage- and firm-level (TFP) data can be used to estimate the coefficients of interest in the vector φ or $\varphi/(1-\alpha)$.[2] The first identification problem when estimating equation (5) or (6) is that the effect of local characteristics, $X_{a(i)}$, on wages and productivity may not be causal (endogenous

2. Combes, Mayer, and Thisse (2008, chapter 11) provide a more complete model of local productivity and a precise discussion of a number of issues, including those that relate to the prices of factors, intermediates, and final output.

quantity of labor bias). In other words, unobserved local determinants of firm productivity that are part of the error term μ_i may well be correlated with $X_{a(i)}$. Second, local characteristics of workers that are not observed and therefore not included in $X_{a(i)}$ may not be comparable across areas (endogenous quality of labor bias). Again, this creates some correlation between μ_i and $X_{a(i)}$.[3]

To provide further justification for equations (5) and (6) and to clarify some issues regarding endogenous quantity of labor bias, we note that the literature on the microfoundations of agglomeration (e.g., Duranton and Puga 2004) typically leads to equilibria, where the wage in area a, w_a, depends on a local productivity shifter, B_a, and the local workforce, N_a:

$$w_a = B_a N_a^{\delta}. \tag{7}$$

This equation is consistent with equation (5) when agglomeration effects are such that $A_i^{1/(1-\alpha)} = B_a N_a^{\delta} e^{\mu_i}$. The variable B_a can be viewed as a short-hand for all variables other than N_a in X_a. With $\delta > 0$, individual wages increase with N_a. If N_a is exogenously determined, it can be part of the vector of local characteristics X_a, and δ can be appropriately estimated with ordinary least squares (OLS). Following Roback (1982) and subsequent literature, we may assume instead that workers choose their city of residence. This choice is determined through utility maximization by the difference between the wage and the local cost of living:

$$U_a = w_a - C_a N_a^{\lambda}. \tag{8}$$

In any city, the cost of living increases with the city workforce and depends on other characteristics such as amenities, which have utility costs (and benefits). A spatial equilibrium equalizes utility across cities.

Assuming $\lambda > \delta$ and normalizing equilibrium utility to zero, the above yields:

$$N_a = \left(\frac{B_a}{C_a}\right)^{1/(\lambda - \delta)} \quad \text{and} \quad w_a = B_a^{\lambda/(\lambda-\delta)} C_a^{-\delta/(\lambda-\delta)} \tag{9}$$

At one extreme, if there are no differences in productivity across cities other than those due to population difference (i.e., $B_a = B$) and only costs vary, the OLS coefficient on log workforce, when regressed against log wage, will be (appropriately) δ. In the opposite case where costs are the same everywhere ($C_a = C$) and only productivity varies, the regression will estimate instead λ. In the general case where both costs and productivity vary, the estimated coefficient on log workforce will be between δ and λ.[4] The intuition

3. In addition, when estimating equation (6), factors might be endogenous as well. This issue is discussed in section 1.6.

4. Using the results from equation (9), it is easy to show that the estimated coefficient for log workforce will be: $[\lambda \text{Var}(\ln B_a) + \delta \text{Var}(\ln C_a) - (\delta + \lambda)\text{Cov}(\ln B_a, \ln C_a)]/[\text{Var}(\ln B_a) + \text{Var}(\ln C_a) - \text{Cov}(\ln B_a, \ln C_a)]$. With zero covariance between B_a and C_a and equal variance, this reduces to $(\delta + \lambda)/2$.

for that result is that if the variation in local workforce comes solely from local costs, it is exogenous, and the proper coefficient is estimated. If instead the workforce is determined by the variation in productivity, wages in equilibrium only reflect the extent to which local costs increase with the size of the workforce. We need to keep this point in mind for our estimation strategy.

This problem actually goes deeper than that. Our model considers only two factors of production—labour and physical capital—the latter of which is mobile and its price can reasonably be taken to be constant everywhere. Then, the term associated with its price r enters the constant and raises no further problem. However, land may also enter as a factor of production. Unlike for capital, the price of land varies across areas. Following again Roback (1982), we expect better consumption amenities to draw in more population and imply higher land prices. Firms will thus use less land. In turn, this lowers the marginal product of labor when land and labor are imperfect substitutes in the production function (as might be expected). Put differently, nonproduction variables may affect both population patterns and may be capitalized into wages. To deal with this problem, we could attempt to control for local variables that directly affect consumer utility and thus land prices. However, our range of controls is limited, and we are reluctant to use a broad range of local amenities, since many of them are likely to be simultaneously determined with wages.

Faced with reverse causality and missing variables that potentially affect both wages and the density of employment, our strategy is to rely on instrumental variables.[5] Hence, we are asking our instrument to deal with both the reverse-causality problem and the missing-variable issue highlighted here.[6]

Turning to the endogenous quality of labor bias, note that the quantity derived in equation (2) and used throughout the model is a wage rate per

5. Alternative approaches may include focusing on groups of workers for which there is an element of exogeneity in their location decision. One could think, for instance, of spouses of military personnel. However, such groups are likely to be very specific. Another alternative may be to look at "natural experiments" that led to large-scale population and employment changes. Such experiments are very interesting to explore a number of issues. For instance, Davis and Weinstein (2008) estimate the effects of the U.S. bombing of Japanese cities during World War II on their specialization to provide some evidence about multiple equilibria. Redding and Sturm (2008) use the division of Germany after World War II to look at the effects of market potential. However, such natural experiments are not of much relevance to study productivity, since the source of any such large-scale perturbation (e.g., the bombing of Japanese cities) is also likely to affect productivity directly, and there is no natural exclusion restriction.

6. The issue with instrumenting is that the number of possible instruments is small, while there are potentially dozens of (endogenous) variables that can describe a local economy. In view of this problem, our strategy is to consider parsimonious specifications with no more than one or two potentially endogenous variables. The drawback is that the exclusion restriction for the instruments (i.e., lack of correlation between the instruments and the error) is more difficult to satisfy than with a greater number of controls. Despite this, we think that a more demanding exclusion restriction is preferable to the addition of inappropriate and possibly endogenous controls.

efficiency unit of labor. Even if we are willing to set aside the issue that different types of labor should be viewed as different factors of production, not all workers supply the same number of efficiency units of labor per day. However, the data for individual workers is about their daily earnings—that is, their wage *rate* times the efficiency of their labor. For worker j employed by firm i, it is convenient to think of their earnings as being $W_j = w_{i(j)} \times s_j$, where their level of skills s_j is assumed to map directly into the efficiency of their labor. Hence, individual skills must be conditioned out from the regression to estimate equation (5) properly. Otherwise, any correlation between local characteristics and the skills of the local workforce will lead to biased estimates for agglomeration effects. Put differently, the quality of workforce in an area is likely to be endogenous. Previous work on French data (Combes, Duranton, and Gobillon 2008) leads us to be believe that this is a first-order issue.

To deal with this problem of endogenous labor quality, a number of approaches can be envisioned. The first would be to weigh the workforce by a measure of labor quality at the area level and try to instrument for labor quality just like we instrument for labor quantity. Instruments for labor quality are very scarce. The only reasonable attempt is by Moretti (2004), who uses land-grant colleges in U.S. cities to instrument for the local share of workers with higher education. In any case, this is unlikely to be enough, because we also expect unobservables such as ambition or work discipline to matter and to be spatially unevenly distributed (Bacolod, Blum, and Strange 2009).

To tackle sorting head-on, previous literature has attempted to use area characteristics at a different level of spatial aggregation. For instance, Evans, Oates, and Schwab (1992) use metropolitan characteristics to instrument for school choice, while Bayer, Ross, and Topa (2008) use location at the block level and assume an absence of sorting conditional on neighborhood choice.[7] In our data, although we know location at the municipal level, we are loathe to make any strong spatial identifying assumption of that sort. A more satisfactory alternative would be to estimate a full system of equations, modeling explicitly location choice. Unfortunately, due to both the difficulty of finding meaningful exclusion restrictions and to the complications introduced by the discrete nature of the choice among many locations, this is a difficult exercise. Dahl (2002) proposes a new approach to this problem, but this can be applied to cross-sectional data only.

The last existing approach is to use the longitudinal dimension of the data, as in Glaeser and Maré (2001), Moretti (2004), and Combes, Duranton, and

7. Opposite spatial identifying assumptions are made. In Evans, Oates, and Schwab (1992), the choice of the more aggregate area is assumed to be exogenous, while location choice at a lower spatial level is not. Bayer, Ross, and Topa (2008) assume instead that randomness prevails at the lower level of aggregation and not at the higher level of aggregation.

Gobillon (2008). This is the approach we follow. The details of our methodology are described in the next section.

1.3 Sorting and Wage Data

1.3.1 Choice of Spatial Zoning, Sectoral Aggregation, and Explanatory Variables

The choice of geographical units could in principle be of fundamental importance. With the same data, there is no reason why a partial correlation that is observed for one set of spatial units should also be observed for an alternative zoning. In particular, the shape of the chosen units may matter. However, Briant, Combes, and Lafourcade (2007) compare the results of several standard exercises in spatial economics using both official French units, which were defined for administrative or economic purposes, and arbitrarily defined ones of the same average size (i.e., squares on a map). Their main finding is that to estimate agglomeration effects, the localization of industries, and the distance decay of trade flows across areas, the *shape* of units makes no difference.

With respect to our choice of units, we opt for French employment areas ("zones d'emploi"). Continental France is fully covered by 341 employment areas, whose boundaries are defined on the basis of daily commuting patterns. Employment areas are meant to capture local labor markets, and most of them correspond to a city and its catchment area or to a metropolitan area. This choice of relatively small areas (on average 1,500 km^2) is consistent with previous findings in the agglomeration literature that agglomeration effects are in part very local (Rosenthal and Strange 2004). Nevertheless, we are aware that *different spatial scales* may matter with respect to agglomeration effects (see Briant, Combes, and Lafourcade [2007] and previous literature). We need to keep this important issue in mind when deciding on a specification.

Turning to the level of sectoral aggregation, a key question regards whether the benefits from agglomeration stem from the size of the overall local market (*urbanization economies*) or from geographic concentration at the sector level (*localization economies*). Although we want to focus on overall scale effects, sector effects cannot be discarded. Previous results for France suggest that they matter, although they are economically far less important than overall scale effects (Combes, Duranton, and Gobillon 2008). In the following, we work at the level of 114 three-digit sectors.[8]

The main explanatory variable we are interested in is employment den-

8. We view this level of aggregation as a reasonable compromise. On the one hand, we need finely defined sectors in wage regressions and for TFP estimation. On the other hand, localization economies are expected to be driven by similarities in customers, suppliers, workers, and technology, and thus take place at a fairly broad level of sectoral aggregation.

sity. It is our favorite measure of local scale. Since Ciccone and Hall (1996), density-based measures have often been used to assess overall scale effects. Their main advantage compared to alternative measures of size, such as total employment or total population, is that density-based measures are more robust to the zoning. In particular, Greater Paris is divided into a number of employment areas. The true economic scale of these Parisian employment areas is much better captured by their density than by any absolute measure of employment.

To repeat, French employment areas are relatively small and are determined by commuting patterns. On the other hand, input-output linkages may not be limited by commuting distances. Hence, we expect some agglomeration effects to take place at a scale larger than employment areas. There is by now a lot of evidence that the market potential of an area matters (Head and Mayer 2004). Thus, in some regressions, we also consider the market potential of an area that we define as the sum of the density of the other areas, weighted by the inverse distance to these areas.[9] Experimenting with other measures leads to very similar results.

1.3.2 Main Wage Data

We use an extract from the Déclarations Annuelles des Données Sociales (DADS) or the Annual Social Data Declarations database from the French statistical institute (INSEE). The DADS are collected for pension, benefits, and tax purposes. Establishments must fill a report for each of their employees every year. An observation thus corresponds to an employee-establishment-year combination. The extract we use covers all employees in manufacturing and services working in France and born in October of even-numbered years.

For each observation, we know the age, gender, and occupation at the two-digit level. Except for a small subsample, education is missing. We also know the number of days worked but not hours for all years so that we restrict ourselves to full-time employees for whom hours are set by law. For earnings, we focus on total labor costs, deflated by the French consumer price index. We refer to the real 1980 total labor cost per full working day as the wage. The data also contains basic establishment-level information such as location and three-digit sector.

The raw data contains 19,675,740 observations between 1976 and 1996 (1981, 1983, and 1990 are missing). The details of the cleaning of the data is described in Combes, Duranton, and Gobillon (2008). After selecting only full-time workers in the private sector, excluding outliers, dumping a number of industries with reporting problems, and deleting observations

9. We retain a simple specification for market potential and do not aim to derive it from a "new economic geography" model (Head and Mayer 2004). Alternative specifications for market potential are highly correlated with the one we use. See Head and Mayer (2006) for further evidence and discussion of this fact.

with coding problems, we end up with 8,826,422 observations. For reasons of computational tractability, we keep only six points in time (every four years: 1976, 1980, 1984, 1988, 1992, and 1996), leaving us with 2,664,474 observations.

Using this data, we can construct a number of variables for each year. Our main explanatory variable, employment density, can be readily calculated from the data.[10] So can market potential. For each area and sector, we also compute the number of establishments, the share of workers in professional occupations, and the share of the sector in local employment. As controls, we also use three amenities variables. These amenities variables are the share of population located on a sea shore, mountains, and lakes and waterways. These variables come from the French inventory of municipalities. We aggregate them at the level of employment areas, weighting each municipality by its population.[11] Table 1.1 reports a number of descriptive statistics for French employment areas.

1.3.3 Three Wages

The simplest way to implement equation (5) is to compute the mean wage for each area and year and take its log:

$$W^1_{at} \equiv \ln \overline{w}_{at} \equiv \ln\left(\frac{1}{N_{at}} \sum_{j \in (a,t)} w_{jt}\right), \tag{10}$$

where w_{jt} is the wage of worker j and year t, and N_{at} is the number of workers in area a and year t.

We can then use W^1_{at} as the dependent variable to be explained by local employment density and other local characteristics in equation (14). Using a simple log mean like W^1_{at} throws a number of problems. First, when using mean wages, we do nothing regarding the endogenous quality of labor bias. Second, we do not condition out sector effects.[12]

To deal with these two problems, a first solution is to use all the available observables about workers and proceed as follows. We first compute a mean wage per employment area, sector, and year:

$$\overline{w}_{ast} \equiv \frac{1}{N_{ast}} \sum_{j \in (a,s,t)} w_{jt}. \tag{11}$$

10. We keep in mind that the years are not the same for the wage and TFP regressions. For each set of regressions, the explanatory variables are constructed from the corresponding data sources.

11. Each employment area contains on average more than one hundred municipalities.

12. One further (minor) issue needs to be mentioned. We take the log of mean wages rather than the mean of log (individual) wages. When viewing local wages as an aggregate of individual wages, the log of mean wages is not the proper aggregate to consider. Mean log wages should be used instead. However, the former is easier to implement than the latter, especially for those who do not have access to microdata. In any case, this issue is empirically unimportant, since the correlation between log mean wages and mean log wages is 0.99.

Table 1.1 Summary statistics for our main variables (averages across 306 employment areas)

	Mean	Standard deviation
Mean wage (1976–1996, in 1980 French francs, per day)	207.9	15.8
W^1	5.3	0.074
W^2	5.2	0.070
W^3	–0.04	0.049
Employment density (workers per sq. km)	64.4	543.0
Ln employment density	2.4	1.2
Market potential (workers per sq. km)	108.1	139.9
Ln market potential	4.4	0.7
1831 Urban population density (inhabitants per sq. km)	38.2	419.8
1881 Urban population density (inhabitants per sq. km)	106.8	1232.3
Sea (average % municipalities on a coast line)	8.8	21.1
Lake (average % municipalities on a lake)	17.2	12.9
Mountain (average % municipalities on a mountain)	9.8	19.7

Source: DADS for the first eight lines, historical censuses for the next two, and 1988 municipal inventory for the last three. For sea, lake, and mountain, we have for each employment area the percentage of municipalities on a coast, with a lake, or on a mountain. We average this quantity across employment areas.

This wage can then be regressed on a number of (mean) characteristics of the workers and the local sector. More specially, we can estimate the following first-step regression:

$$\ln \overline{w}_{ast} = W^2_{at} + \gamma_s + X_{ast}\varphi + \varepsilon_{ast}. \tag{12}$$

In this equation, γ_s is a sector dummy, and X_{ast} is a set of characteristics for sector s in area a and year t and the workers employed therein. To capture sector effects, we use in X_{ast} the (log) share of local employment in sector s and the (log) number of local establishments in this sector. Also in X_{ast}, the mean individual characteristics are the age, its square, and the shares of employment in each of six skill groups.[13] In equation (12), the coefficient of interest is W^2_{at}, a fixed effect for each employment area and year. When estimating equation (12), all local sector and mean individual characteristics are centered, and the observations are weighted by the number of workers in each cell to avoid heteroscedasticity.

The coefficients W^2_{at} can, in a second step, be regressed on local employment density and other local characteristics, as stipulated by equation (5). While further details and justifications about the estimation of equation (12) are given in Combes, Duranton, and Gobillon (2008), three important

13. The shares of each skill in local sector employment capture the effects of both individual characteristics at the worker level and the interactions between workers. The two cannot be separately identified with aggregate data.

issues need to be briefly discussed. First, the approach described here first estimates local fixed effects before using them as the dependent variable in a second step. We prefer this two-step approach to its one-step counterpart for reasons made clear next.

Below, estimating equation (12) with OLS may condition out sectoral effects, but it does not take care of the possible simultaneity between mean sector wages and local sector characteristics. A high level of specialization in a certain sector may induce high wages in this sector. Alternatively, high local wages may simply be a reflection of strong local advantage, also leading to a high level of specialization. We acknowledge this concern at the sector level, but we do not deal with it. The main reason is that the coefficients for local specialization and the number of establishments, although significant, only explain a very small part of the variation in equation (12) (Combes, Duranton, and Gobillon 2008).

Finally, controlling for observable labor market characteristics including one-digit occupational categories (for lack of control for education) attenuates concerns about the endogenous quality of labor bias. However, they do not eradicate them entirely.

A more powerful way to deal with the endogenous quality of labor bias is to estimate:

$$(13) \qquad \ln w_{jt} = W^3_{a(jt)t} + \gamma_{s(jt)} + X^1_{a(jt)s(jt)t}\varphi^1_{s(jt)} + X^2_{jt}\varphi^2 + \theta_j + \varepsilon_{jt}.$$

This equation is estimated at the level of individual workers and contains a worker fixed effect θ_j, which controls for all fixed individual characteristics.[14] The use of individual data also allows us to control for individual characteristics X^2_{jt} (age and its square) separately from (centered) local industry characteristics X^1_{ast}. The latter contain the share of local employment of the sector, the local number of firms in the sector, and the local share of professional workers. The coefficient of interest in equation (13) is the wage index W^3_{at} for each area and year after conditioning out sector effects, observable time-varying individual characteristics, and all fixed individual characteristics. If we ignore again the possible endogeneity of local sector characteristics, the main issue when estimating equation (13) regards the endogeneity of location or sector choices. However, because we have sector effects and time-varying local effects, W^3_{at}, problems only arise when we have spatial or sector sorting based on the worker-specific errors. In particular, there is no bias when sorting is based on the explanatory variables, *including individual, area-year, and industry fixed effects.* More concretely, there is a bias when the location decision is driven by the exact wage that the worker can get at locations in a given year, but there is no bias when workers base their location decision on the average wage of other workers in an area and

14. Equation (13) is identified from both the movers (to identify the difference between W^3_{at} and $W^3_{a't+1}$) and the stayers (to identify the difference between W^3_{at} and W^3_{at+1}).

their own characteristics (i.e., when they make their location decision on the basis of their expected wages). See Combes, Duranton, and Gobillon (2008) for further discussion.

Note that we prefer this two-step approach, which first estimates equation (12) or (13) before regressing W_{at}^2 or W_{at}^3 on local characteristics, to its corresponding one-step counterpart for three reasons. First, we can properly take into account correlations between area-sector variables and error terms at the area level. Second, a two-step approach allows us to account for area-specific error terms when computing the standard errors for the coefficients we estimate. Doing so is important, because Moulton (1990) shows that standard errors can be seriously biased otherwise. Accounting for area-specific errors with a one-step approach is not possible, given that workers can move across areas. Third, we can conduct a variance decomposition for the second stage.[15]

Finally, to avoid identifying out of the temporal variation, we average the three wage variables and all the explanatory variables across the six years of data we use.[16] Before turning to our results, it is interesting to note that these three local wage variables are strongly correlated with one another. The correlation between W^1 and W^2 is 0.87, the correlation between W^1 and W^3 is 0.81, and the correlation between W^2 and W^3 is 0.91. Table 1.1 reports a number of descriptive statistics for French employment areas.

1.4 Instruments

That the estimation of agglomeration economies could be plagued by simultaneity was first articulated by Moomaw (1981). To preview our IV approach, we note first that using historical variables such as long lags of population density to instrument for the size or density of local population has been standard since Ciccone and Hall's (1996) pioneering work. To the extent that (a) there is some persistence in the spatial distribution of population and (b) the local drivers of high productivity today differ from those of a long-gone past, this approach is defensible. An alternative strategy is to use the nature of soils, since geology is also expected to be an important determinant of settlement patterns. Some soils are more stable than others and thus can support a greater density of people. More fertile lands may have also attracted people in greater numbers, and so forth. To the extent that

15. It is also true that using as the dependent variable a coefficient estimated in a previous step introduces some measurement error. The procedure used in Combes, Duranton, and Gobillon (2008) to control for this problem shows that it makes no difference, because the coefficients are precisely estimated at the first step.

16. These averages are weighted by the number of workers in the area for each year to obtain a wage index for the average worker in the area over time. By contrast, our final regressions for the cross-section of employment areas assess whether denser areas make their average firm more productive. There is no longer any reason to weigh the observations (by the number of workers) in these regressions.

geology affects the distribution of population (i.e., labor supply) and does not otherwise cause productivity (i.e., labor demand) because fertile lands are no longer a relevant driver of local wealth, it can provide reasonable instruments to explain the distribution of employment. Except by Rosenthal and Strange (2008) in a slightly different context, geology has not been used to instrument for the distribution of population.

1.4.1 Description of the Instruments

Our first set of instruments is composed of historical populations from early French censuses. For twenty-six French censuses prior to our earliest year of data (1976), we know the urban population for each municipality. Among available censuses, we choose the earliest one from 1831 and another from 1881, fifty years later.[17] We also experimented with other years. Unfortunately, urban population in historical censuses is only reported above a threshold of 5,000. For 1831, there are thirty-five employment areas for which no municipality had an urban population above 5,000. A small majority of them are rural areas, while the others are densely populated employment areas with strong municipal fragmentation. We think of this as being measurement error. To minimize weak instrument problems, we drop these thirty-five employment areas.

Our second group of instruments is composed of geological variables from the European Soil Database (ESDB) compiled by the European Soil Data Centre. The data originally come as a raster data file with cells of 1 km per 1 km. We aggregated it at the level of each employment area.[18] Given that soil characteristics are usually discrete, we use the value that appears most often in each area. To take an illustrative example, the initial and transformed data for the water capacity of the subsoil are represented in figure 1.2. For a small number of densely populated employment areas in Greater Paris, the most important category is sometimes missing. When this is the case, we turn to the second-most important category. In the rare instances where the information is missing from all the pixels in an employment area, we impute the value of a neighboring area (chosen because it takes similar values for other soil characteristics). For instance, the water capacity of the subsoil in Central Paris is missing. We impute the value of its close neighbor Boulogne-Billancourt.

In total, we generate twelve variables from the ESDB.[19] The first four

17. Because they are in log, using these two variables together allows us to instrument for both past 1831 level and past growth between 1831 and 1881.

18. To aggregate the information from 1-km-by-1-km pixels to employment areas, the zonal statistics tool from ArcGIS 9 software was used. The tool uses the zones defined in the zone data set (in our case, French employment areas) and internally converts the vectors into a zone raster, which it aligns with the value raster data set for soils.

19. The ESDB (v2.0 Raster Archive) contains many more characteristics. For France, some of them, like the soil code according to the standard Food and Agricultural Organization classification, are poorly reported. A large number of characteristics also contain categories

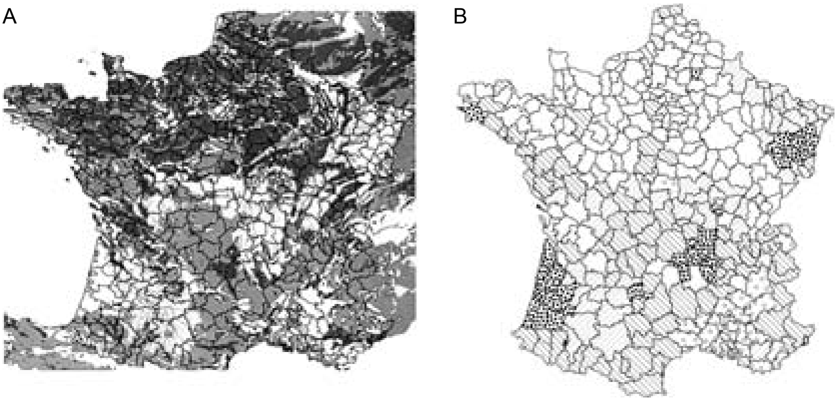

Fig. 1.2 Geological characteristics—water capacity of the subsoil: *A*, original data; *B*, transformed data

Source: European Soil Database.

Note: Panel A represents the initial raster data. Panel B represents the transformed version of the same data after imputation of the missing values for seven employment areas in Greater Paris. In panel A, the darkest shade of gray corresponds to "very high" (i.e., above 190 mm), the second-darkest shade corresponds to "high" (between 140 and 190 mm), followed by "medium" (100–140 mm), "low" (5–100 mm), and "very low" (0–5 mm). Missing values in panel A (around Paris) are in white.

describe the nature of the soils, according to the mineralogy of their subsoil (three categories) and topsoil (four categories) and the nature of the dominant parent material at a broad level of aggregation (six categories) and at a finer level (with twenty categories). More precisely, the mineralogy variables describe the presence of various minerals in the topsoil (the first layer of soil, usually 5 to 15 cm deep) and the subsoil (the intermediate layer between the topsoil and the bedrock). The dominant parent material of the soil is a description of the underlying geological material (the bedrock). Soils usually get a great deal of structure and minerals from their parent material. The more aggregate dominant parent material variable (in six categories) contains entries such as igneous rocks, glacial deposits, or sedimentary rocks. Among the latter, the detailed version of the same variable (with twenty categories) distinguishes between calcareous rocks, limestone, marl, and chalk.

The next seven geological characteristics document various characteristics of the soil, including the water capacity of the subsoil (five categories) and topsoil (three categories), depth to rock (four categories), differentiation (three categories), erodibility (five categories), carbon content (four catego-

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that refer to land use (e.g., "urban" or "agriculture") and are thus not appropriate here. More generally, characteristics a priori endogenous to human activity were discarded. Finally, some characteristics such as the secondary dominant parent material struck us as anecdotal and unlikely to yield relevant instruments.

ries), and hydrogeological class (five categories). Except for the hydrogeological class, which describes the circulation and retention of underground water, the meaning of these variables is relatively straightforward. Finally, we create a measure of local terrain ruggedness by taking the mean of maximum altitudes across all pixels in an employment area minus the mean of minimum altitudes. This variable thus captures variations of altitude at a fine geographical scale.

1.4.2 Relevance of the Instruments

Following equations (5) and (6), the specifications we want to estimate are:

$$\ln W_a = \text{Constant} + X_a \varphi^W + \mu_a^W \tag{14}$$

and

$$\ln \text{TFP}_a = \text{Constant} + X_a \varphi^{\text{TFP}} + \mu_a^{\text{TFP}}, \tag{15}$$

where μ_a^W and μ_a^{TFP} are the error terms for the wage and TFP equations. The vector of the dependent variables X_a contains the three amenity variables previously discussed, (log) employment density, and sometimes market potential. These last two variables are suspected of being simultaneously determined with wages and TFP.

Estimating the effect of employment density and market potential on local wages and productivity using instrumental variables can yield unbiased estimates, provided that the instruments satisfy two conditions: relevance and exogeneity. Formally, these conditions are

$$\text{Cov}(\text{Density}_a, Z_a|.) \neq 0, \quad \text{Cov}(\text{MarketPotential}_a, Z_a|.) \neq 0, \tag{16}$$

and

$$\text{Cov}(\mu_a^X, Z_a) = 0 \quad \text{for } X = W \text{ and } X = \text{TFP, respectively,} \tag{17}$$

where Z denotes the set of instruments. We begin by discussing the ability of our instruments to predict contemporaneous employment density and market potential conditionally to the other controls.

The stability of population patterns across cities over time is a well-documented fact (see Duranton [2007] for a recent discussion). This stability is particularly strong in France (Eaton and Eckstein 1997). The raw data confirm this. Table 1.2 presents pairwise correlations between our four historical instruments and current employment density and market potential.[20] For the sake of comparison with the following geology variables, we also report the R^2s of the corresponding univariate regressions. We can see that the log urban population densities of 1831 and 1881 are good predictors

20. We use the measures of density used for our wage regressions (1976 to 1996). Our measures of density for the TFP regressions differ slightly, since they are calculated from a slightly different source and cover different years.

of current employment density. Past market potentials computed from 1831 and 1881 urban populations also predict current market potential extremely well.

Turning to geological characteristics, we expect the nature of soils and their characteristics to be fundamental drivers of population settlements. Soil characteristics arguably determine their fertility. Since each soil characteristic is described by several discrete variables, it is not meaningful to run pairwise correlations as with historical variables. Instead, table 1.3 reports the R^2 when regressing employment density and market potential against various sets of dummies for soil characteristics. The results show that some geological characteristics such as the dominant parent material or the depth to rock have good explanatory power. Other soil characteristics such as their mineralogy or their carbon content are less powerful predictors of current population patterns. Note also that soil characteristics tend to be better at explaining the variations of market potential than employment density. This is not surprising, since most soil characteristics vary relatively smoothly over

Table 1.2 **R^2s of univariate regressions and pairwise correlations: Historical versus density and market potential (1976 to 1996)**

	Ln (employment density)	Ln (market potential)
Ln (1831 density)	0.57 (0.75)	0.05 (0.24)
Ln (1881 density)	0.78 (0.88)	0.10 (0.33)
Ln (1831 market potential)	0.21 (0.46)	0.96 (0.98)
Ln (1881 market potential)	0.22 (0.47)	0.99 (0.99)

Note: 306 observations; adjusted R^2 in plain text, and pairwise correlations between parentheses.

Table 1.3 **R^2s when regressing density and market potential on soil characteristics**

	Ln (employment density)	Ln (market potential)
Subsoil mineralogy (2 dummies)	0.02	0.06
Topsoil mineralogy (3 dummies)	0.02	0.06
Dominant parent material (5 dummies)	0.11	0.31
Dominant parent material (19 dummies)	0.13	0.48
Topsoil water capacity (2 dummies)	0.03	0.23
Subsoil water capacity (3 dummies)	0.01	0.32
Depth to rock (3 dummies)	0.10	0.35
Soil differentiation (2 dummies)	0.07	0.19
Erodibility (4 dummies)	0.04	0.19
Carbon content (3 dummies)	0.04	0.04
Hydrogeological class (4 dummies)	0.01	0.04
Ruggedness	0.05	0.10

Note: Adjusted R^2s; 306 observations.

fairly large spatial scales, while variations in density are more abrupt and take place at smaller spatial scales.

While the correlations and R^2s reported in tables 1.2 and 1.3 are interesting, equation (16) makes clear that the relevance of an instrument depends on the *partial* correlation of the instrumental variables and the endogenous regressor. To assess these partial correlations, table 1.4 presents the results of OLS regressions of log density on our instrumental variables and controls. Table 1.5 reports results for a similar exercise with market potential.

Column (1) of table 1.4 examines the partial correlation between employment density and 1831 population density while conditioning out amenities (sea, lake, and mountain). Column (2) performs a similar regression using 1881 instead of 1831 population density. In both columns, the coefficient on past density is highly significant and close to unity. In columns (3) to (9), we regress contemporaneous employment density on a series of soil dummies concerning their mineralogy, dominant parent material, water capacity, carbon content, depth to rock, and soil differentiation. For lack of space, we do not report all the coefficients, but it must be noted that at least one dummy is significant at 5 percent in each regression.

The comparison of R^2s in columns (1) to (2) versus (3) to (9) immediately shows that long lags of population density explain a greater share of the variations in contemporaneous employment density than soil characteristics. To make a more formal assessment of the relevance of our instruments, we turn to the weak instrument tests developed by Stock and Yogo (2005).[21] Table 1.4 presents the relevant F-statistics. The two lagged density instruments in columns (1) and (2) have F-statistics close to 400 and 1,000, respectively. This makes them very strong in light of the critical values reported by Stock and Yogo (2005) in their tables 1 through 4. The soil instruments are weaker by comparison and fall below the critical values of Stock and Yogo (2005) with two-stage least squares (TSLS). To avoid the pitfalls of weak instruments, a number of possible strategies can be envisioned. First, it would be possible to increase the strength of some soil instruments by considering only the more relevant dummies and by dropping insignificant ones. In absence of a well-articulated theory of how soils affect economic development, we acknowledge an element of "data mining" in our use of soil characteristics. We are nonetheless reluctant to push it to such extremes. Second, we experiment next with estimation strategies that are less sensitive to weak instruments, such as limited information maximum likelihood

21. Stock and Yogo (2005) provide two tests for weak instruments. They are both based on a single F-statistic of the instrumental variables but use different thresholds. The first one tests the hypothesis that the two-stage least square (TSLS) small sample bias is small relative to the OLS endogeneity bias ("bias test"). Second, an instrument is considered strong if, from the perspective of the Wald test, its size is close to its level for all possible configurations of the IV regression ("size test"). Note that instruments may be weak in one sense but not another, and instruments may be weak in the context of TSLS but not when using limited information maximum likelihood (LIML).

Table 1.4 First stage: Density

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Ln (1831 density)	0.906								
	(0.046)***								
Ln (1881 density)		0.924							
		(0.030)***							
Ruggedness			–0.710						
			(0.224)***						
Subsoil mineralogy	N	N	N	Y	N	N	N	N	N
Dominant parent material (6 categories)	N	N	N	N	Y	N	N	N	N
Subsoil water capacity	N	N	N	N	N	Y	N	N	N
Soil carbon content	N	N	N	N	N	N	Y	N	N
Depth to rock	N	N	N	N	N	N	N	Y	N
Soil differentiation	N	N	N	N	N	N	N	N	Y
R^2	0.58	0.78	0.07	0.07	0.17	0.06	0.10	0.15	0.11
F-test (H_0—all instruments zero)	395.7	1,018.8	10.0	5.5	9.1	1.7	6.5	12.6	12.3
Partial R^2	0.57	0.77	0.03	0.04	0.13	0.02	0.06	0.11	0.08

Note: Dependent variable: ln (employment density); 306 observations for each regression. All regressions include a constant and three amenity variables (sea, lake, and mountain). Standard errors in parentheses.

***Significant at the 1 percent level.

Table 1.5 First stage: Market potential

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Ln (1831 market potential)	1.026								
	(0.012)***								
Ln (1881 market potential)		0.970							
		(0.007)***							
Ruggedness			–0.339						
			(0.111)***						
Subsoil mineralogy	N	N	N	Y	N	N	N	N	N
Dominant parent material (6 categories)	N	N	N	N	Y	N	N	N	N
Subsoil water capacity	N	N	N	N	N	Y	N	N	N
Soil carbon content	N	N	N	N	N	N	Y	N	N
Depth to rock	N	N	N	N	N	N	N	Y	N
Soil differentiation	N	N	N	N	N	N	N	N	Y
R^2	0.97	0.99	0.23	0.24	0.43	0.41	0.28	0.44	0.31
F-test (H_0—all instruments zero)	7,106.47	21,503.0	9.4	7.3	23.1	26.3	11.3	41.2	24.0
Partial R^2	0.96	0.99	0.03	0.05	0.28	0.26	0.10	0.29	0.14

Note: Dependent variable: ln (market potential); 306 observations for each regression. All regressions include a constant and three amenity variables (sea, lake, and mountain). Standard errors in parentheses.

***Significant at the 1 percent level.

(LIML), as advocated by Andrews and Stock (2007). Third, we repeat the same regressions with different sets of soil instruments and see how this affects the coefficient(s) of interest. Obtaining the same answer over and over again would be reassuring.

In table 1.5, we repeat the same exercise with market potential using lagged values of that variable and the same set of soil instruments as in table 1.4. Both historical and soil variables are much stronger instruments for market potential than for employment density. For historical variables, the reason is that market potential is computed as a weighted mean of employment density. As a result, this washes out much idiosyncratic variation and naturally yields higher R^2s. Put differently, soil variables are better in replicating the smooth evolution of market potential than the spikes of employment density. The facts that in column (1), the coefficient on 1831 market potential is essentially 1 and that the partial R^2 is 95 percent also indicate that we should not expect much difference between OLS and TSLS.

Because both market potential and soil characteristics vary smoothly over space, one may worry that the good explanatory power of soil characteristics may be spurious. This will be the case if some large areas with particular soil characteristics spuriously overlap with areas of particularly high or low market potential. However, a detailed reading of the coefficients on soil dummies (not reported in table 1.5) indicates that this is not the case. For instance, areas for which the dominant parent material is conditionally associated with the lowest market potential are eolian sands, molasse (sand stone), and ferruginous residual clay. Sands, which drain very fast, and ferruginous clay, a heavy soil which does not drain at all, do not lead to very fertile soils. On the other hand, the parent materials associated with a high market potential are loess, a notably fertile type of soil, and chalk, a stable and porous soil, which can be very fertile, provided it is deep enough. Similarly, a high water capacity of the subsoil is associated with a higher market potential, as could be expected.

1.4.3 Instrument Exogeneity

Equation (17) gives the second condition that must be satisfied by a valid instrument: orthogonality to the error term. Intuitively, the difficulty in inferring the effect of density and market potential on wages and TFP arises because of the possibility that a missing local characteristic or some local shocks might be driving both population location and economic outcomes. To overcome this problem, we require instruments that affect wages and TFP only through the spatial distribution of population. That is, as made clear previously, we need our instruments to affect the supply of labor but not directly productivity. We now discuss the a priori arguments for why our instruments may (or may not) satisfy this exogeneity condition.

We begin with historical variables dating back to 1831. Long-lagged values of the same variable obviously remove any simultaneity bias caused by contemporaneous local shocks. For such simultaneity to remain, we would need these shocks to have been expected in 1831 and to have determined population location at the time. This is extremely unlikely. However, endogeneity might also arise because of some missing permanent characteristic that drives both past population location and contemporaneous productivity. A number of first-nature geographic characteristics such as a coastal location may indeed explain both past population location and current economic outcomes. In our regressions, we directly control for a number of such first-nature characteristics (coast, mountain, lakes and waterways).

Hence, the validity of long population lags rests on the hypothesis that the drivers of population agglomeration in the past are not related to modern determinants of local productivity after controlling for first-nature characteristics of places. The case for this relies on the fact that the French economy in the late twentieth century is very different from what it was in 1831. First, the structure of the French economy in the late twentieth century differs a lot from that of 1831. In 1831, France was only starting its industrialization process, whereas it is deindustrializing now. Manufacturing employment was around 3 million in 1830, against more than 8 million at its peak in 1970 and less than 6 million today (Marchand and Thélot 1997). Then, agriculture employed 63 percent of the French workforce against less than 5 percent today. Since 1831, the workforce has also doubled. Second, the production techniques in agriculture, manufacturing, and much of the service industries are radically different today from what they were more than 150 years ago. With technological change, the location requirements of production have also changed considerably. For instance, the dependance of manufacturing on sources of coal and iron has disappeared. Third, the costs of shipping goods and transporting people from one location to another have declined considerably. Actually, 1831 coincides with the construction of the first French railroads. Subsequently, cars, trucks, and airplanes have further revolutionized transport. At a greater level of aggregation, trade has also become much easier because of European integration over the last fifty years. Fourth, other drivers of population location not directly related to production have changed as well. With much higher standards of living, households are arguably more willing to trade greater efficiency against good amenities (Rappaport 2007). Some previously inhospitable parts of the French territory such as its Languedocian coast in the south have been made hospitable and are now developed, and so on. Finally, since 1831, France has been successively ruled by a king, an emperor, and presidents and prime ministers from five different republics. The country also experienced a revolution in 1848, a major war with Germany in 1870, and two world wars during the twentieth century.

With so much change, a good case can indeed be made that past determinants of population location are not major drivers of current productivity. As a result, historical variables have been the instrument of choice for current population patterns since Ciccone and Hall (1996). They have been widely used by the subsequent literature.

Although the a priori case for historical instruments is powerful, nothing guarantees that it is entirely foolproof. The fact that long lags of the population variables usually pass overidentification tests and other ex post diagnostics may not constitute such a strong argument in favor of their validity. Population variables are often strongly correlated with one another so that any permanent characteristics that affect both measures of past population location and contemporaneous productivity may go unnoticed due to the weak power of overidentification tests when the instruments are very similar and thus highly correlated.

We now consider geological characteristics. The a priori case for thinking that geological characteristics are good instruments hinges first on the fact that they have been decided mostly by nature and do not result from human activity. This argument applies very strongly to a number of soil characteristics we use. For instance, soil mineralogy and their dominant parent material were determined millions of years ago. Other soil characteristics might seem more suspect in this respect. For instance, a soil's depth to rock or its carbon content might be an outcome of human activity. In the very long run, there is no doubt that human activity plays a role regarding these two characteristics. Whether recent (in geological terms) economic activity can play an important role is more doubtful (e.g., Guo and Gifford 2002). A second caveat relates to the measurement of some soil characteristics. In particular, it is hard to distinguish between a soil's intrinsic propensity to erodibility from its actual erosion (see Seybold, Herrick, and Brejda [1999]). In relation to these two worries, our wealth of soil characteristics implies that we can meaningfully compare the answers given by different soil characteristics as instruments in different regressions. We can also use overidentification tests to assess this issue more formally.

Nonetheless, that soils predate patterns of human settlement does not ensure that any soil characteristics will automatically satisfy the condition in equation (17) and be valid instruments. More specifically, we expect soil characteristics to have been a major determinant of local labor demand in the past. The main argument for the validity of geological instruments, then, is that soil quality is no longer expected to be relevant in an economy where agriculture represents less than 5 percent of employment. We also exclude agricultural activities from our data. Put differently, the case for geological characteristics relies on the fact that this important, though partial, determinant of past population location is now largely irrelevant. Hence, like with historical instruments, the a priori case for geological instruments is strong, but there is no way to be entirely sure.

It is important to note that the cases for the validity of historical and geological variables as instruments differ. Historical variables are broad determinants of current population location. Soil characteristics are narrower but more fundamental determinants of current population location. Put differently, although we expect soils to have determined history, they were not the sole determinants of population patterns in 1831. Geological characteristics also explain current patterns of employment density over and above past employment density. If one group of instruments fails, it is unlikely that the second will do so in the same way. Finally, it is also important to keep in mind that these two sets of instruments can only hope to control for the endogenous quantity of labor bias. That a higher density can lead to the sorting of better workers in these areas is not taken care of by these instruments. Put differently, we expect the endogenous quality of labor bias to remain. Moving from crude measures of wage such as W^1 to more sophisticated ones (W^2, and most of all, W^3) is designed to tackle this second issue.

1.5 Main Wage Results

Table 1.6 presents the results of three simple regressions for our three wages: W^1, the mean local wage as computed in equation (10); W^2, the wage index after conditioning out sector effects and observable individual characteristics as estimated in equation (12); and W^3, the wage index from equation (13), which also conditions out individual fixed effects. In columns (1), (2), and (3), these three wages are regressed on log employment density, controlling for three amenity variables (coast, lakes and waterways, mountain) using OLS. The measured density elasticity of mean wages is at 0.048. This is very close to previous results in the literature (Ciccone and Hall 1996; Ciccone 2002). Controlling for sector effects in column (2) implies a marginally higher estimate of 0.051 for the density elasticity and significantly improves the explanatory power of employment density. This suggests that although the local characteristics of the sector of employment matter, conditioning out sector effects does not affect our estimates of the density elasticity. Controlling also for unobserved individual characteristics yields a significantly lower elasticity of 0.033. This suggests that a good share of measured agglomeration effects are in fact attributable to the unobserved characteristics of the workforce. More specifically, workers who command a higher wage on labor market sort in denser areas.

In columns (4), (5), and (6), we perform the same regressions as in columns (1), (2), and (3), but we instrument employment density with 1831 urban population density. Compared to their corresponding OLS coefficients, the TSLS coefficients for employment density are marginally lower. The instrument is very strong, with a first-stage F- (or Cragg-Donald) statistic close to 400. In columns (6), (7), and (8), we add 1881 population density as a second instrument for employment density. The results are virtually undistinguishable from those of columns (4), (5), and (6). With two instruments, it is also possible to run Sargan tests of overidentification. They are passed

Table 1.6 **Local wages as a function of density: OLS and historical instruments**

Variable	W^1 OLS (1)	W^2 OLS (2)	W^3 OLS (3)	W^1 TSLS (4)	W^2 TSLS (5)	W^3 TSLS (6)	W^1 TSLS (7)	W^2 TSLS (8)	W^3 TSLS (9)
Ln (density)	0.048	0.051	0.033	0.040	0.042	0.026	0.040	0.044	0.027
	(0.002)***	(0.002)***	(0.001)***	(0.003)***	(0.002)***	(0.002)***	(0.003)***	(0.002)***	(0.002)***
Instruments used:									
Ln (1831 density)	—	—	—	Y	Y	Y	Y	Y	Y
Ln (1881 density)	—	—	—	—	—	—	Y	Y	Y
First-stage statistics	—	—	—	395.7	395.7	395.7	518.7	518.7	518.7
Overidentification test p-value	—	—	—	—	—	—	0.99	0.19	0.21
R^2	0.56	0.72	0.65	—	—	—	—	—	—

Note: 306 observations for each regression. All regressions include a constant and three amenity variables (sea, lake, and mountain). Standard errors in parentheses.

***Significant at the 1 percent level.

in all three cases with *p*-values above 10 percent. However, we can put only a limited weight on this test, because the correlation between 1881 and 1831 density is high at 0.75.

If we think of table 1.6 as our baseline, a number of findings are worth highlighting. The density elasticity of mean wages is 0.048 (column [1]). Controlling for the endogenous quality of labor bias through a fixed effect estimation reduces the size of the coefficient by about one-third to 0.033 (column [3]). Controlling for the endogenous quantity of labor bias using long historical lags as instruments reduces it by another one-fifth to 0.027 (column [9]). Hence, this table provides evidence about both the quality and quantity of labor being simultaneously determined with productivity. It also suggests that the endogenous quality of labor bias is more important than the quantity bias.

Next, table 1.7 reports results for a number of regressions, which all use geological characteristics as instruments for employment density. Following the results of table 1.4, we expect geological instruments to be on the weak side. Furthermore, table 1.5 also makes clear that geological characteristics appear to explain market potential better than employment density. Hence, we need to keep in mind that our geological instruments are correlated with a variable—market potential—that is (for the time being) missing from the regression and suspected to have an independent effect on wages. As a consequence, IV estimations that rely solely on geological characteristics may not perform very well and should be interpreted with caution.

In each of the regressions in table 1.7, we use two different soil characteristics. Except for ruggedness, because each soil characteristic is documented with a series of dummy variables, we could technically run overidentification tests while instrumenting for only one characteristic. However, such tests may not be economically meaningful, since we would end up testing for overidentification using the particular categorization of the ESDB. We experimented extensively with soil characteristics. The results we report in the table are representative of what is obtained using any combination of the soil characteristics listed in the table. With them, overidentification tests are usually passed. This is not the case with the other soil characteristics.

More precisely, in column (1) of table 1.7, we regress mean wages on density and other controls using subsoil mineralogy and ruggedness as instruments for employment density. We obtain a density elasticity of 0.042, which is consistent with what we find in table 1.6 when we use historical variables. We repeat the same regression in columns (2) and (3) using W^2 and W^3 as dependent variables. In column (3), the coefficient is slightly above its OLS counterpart rather than slightly below when using historical instruments. The difference, nonetheless, is not significant. Before going any further, note that the low first-stage statistics in columns (1) through (3) raise some questions about the strength of these geological instruments. With weak instruments, a number of authors (e.g., Stock and Yogo 2005) now argue for

Table 1.7 **Local wages as a function of density: Geological instruments**

Variable	W^1 TSLS (1)	W^2 TSLS (2)	W^3 TSLS (3)	W^3 LIML (4)	W^3 LIML (5)	W^3 LIML (6)	W^3 LIML (7)	W^3 LIML (8)
Ln (density)	0.042	0.047	0.038	0.038	0.048	0.050	0.048	0.047
	(0.010)***	(0.008)***	(0.006)***	(0.006)***	(0.005)***	(0.005)***	(0.005)***	(0.005)***
Instruments used:								
Subsoil mineralogy	Y	Y	Y	Y	Y	N	N	N
Ruggedness	Y	Y	Y	Y	N	N	N	N
Depth to rock	N	N	N	N	Y	Y	Y	N
Soil carbon content	N	N	N	N	N	Y	N	N
Topsoil water capacity	N	N	N	N	N	N	Y	Y
Dominant parent material (6 categories)	N	N	N	N	N	N	N	Y
First-stage statistics	6.2	6.2	6.2	6.2	8.3	8.2	8.1	6.8
Overidentification test p-value	0.99	0.90	0.67	0.67	0.45	0.12	0.34	0.15

Note: 306 observations for each regression. All regressions include a constant and three amenity variables (sea, lake, and mountain). Standard errors in parentheses.

***Significant at the 1 percent level.

the superiority of the LIML estimator to the TSLS estimator. Column (4) of table 1.7 reports the LIML estimate for a specification similar to column (3). The TSLS and LIML results are the same.[22]

In columns (5) to (8), we report LIML results regarding our preferred measure of wages, W^3, for further combinations of instruments. The coefficient on employment density is positive and highly significant in all cases. However, it is above its OLS counterpart rather than below, even more so than in column (4). This discrepancy between the IV results using history in table 1.6 and those using geology in table 1.7 is due to the fact that soil variables are not only correlated with the employment density but also with the market potential, which is missing. As a result, the density elasticities in table 1.7 may be biased upward. To see this, note that in column (4), the correlation between the predicted values of employment density obtained from the instrumental regression and actual density is 0.29. The correlation between predicted density and actual market potential (omitted from the regression) is nearly as high at 0.27. In column (5), the problem is even worse, since the correlation between predicted and actual density is 0.37, while the correlation between predicted density and market potential is 0.48.[23]

To explore this problem further, we now consider historical and geological instruments at the same time. Table 1.8 reports the results for a number of regressions using both 1831 density and some soil characteristics. In all cases, the instruments are strong because of the presence of 1831 density. Subsoil mineralogy (along with 1831 density) is used in columns (1) to (3) to instrument for density and explain W^1, W^2, and W^3. The results are the same as those of columns (4) to (6) of table 1.6, which use only 1831 density, while they differ more with those of columns (1) to (3) of table 1.7, which use subsoil mineralogy (together with ruggedness) but not 1831 density. This is unsurprising, given that 1831 density is a much stronger instrument. Using a generalized method of moments (GMM) IV estimation rather than TSLS in column (4) does not change anything. Using ruggedness or hydrogeological class in columns (5) to (7) also implies a similar coefficient on density. With these three soil characteristics (and 1831 density), the overidentification test is passed. For the other soil characteristics, however, this test is failed. An example is given in column (8) with topsoil water capacity. This is in line with the results of the previous table that a majority of soil characteristics do not give the same answer as 1831 density when used as instruments to estimate the density elasticity of wages.

To confirm that this problem is due to the strong correlation between soil

22. In the other regressions, the differences in the point estimates and standard errors between TSLS and LIML remain small. The differences with respect to the overidentification tests are sometimes more important. This is due to the greater power of the Anderson-Rubin test under LIML relative to the Sargan test used with TSLS.

23. This is consistent with the fact that overidentification tests are passed only for the small set of regressions reported in the table.

Table 1.8 Local wages as a function of density: Historical and geological instruments

Variable	W^1 TSLS (1)	W^2 TSLS (2)	W^3 TSLS (3)	W^3 GMM (4)	W^3 TSLS (5)	W^3 TSLS (6)	W^3 TSLS (7)	W^3 TSLS (8)
Ln (density)	0.040	0.042	0.027	0.027	0.027	0.027	0.027	0.027
	(0.003)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***
Instruments used:								
Ln(1831 density)	Y	Y	Y	Y	Y	Y	Y	Y
Subsoil mineralogy	Y	Y	Y	Y	N	Y	N	N
Ruggedness	N	N	N	N	Y	Y	Y	N
Hydrogeological class	N	N	N	N	N	N	Y	N
Topsoil water capacity	N	N	N	N	N	N	N	Y
First-stage statistics	138.7	138.7	138.7	116.2	208.7	108.1	69.8	76.2
Overidentification test p-value	0.98	0.83	0.15	0.13	0.31	0.21	0.53	0.02

Note: 306 observations for each regression. All regressions include a constant and three amenity variables (sea, lake, and mountain). Standard errors in parentheses.

***Significant at the 1 percent level.

characteristics and market potential, table 1.9 reports results for a number of regressions in which market potential is added as a control. In columns (1) to (3), our measures of wages W^1, W^2, and W^3 are regressed on density and market potential using OLS. The measured elasticity of wages with respect to market potential is between 0.01 and 0.03. It is also interesting to note that the density elasticity is slightly lower than in columns (1) to (3) of table 1.6, where market potential is omitted. In columns (4) to (9), we instrument employment density with 1831 density and a range of soil characteristics. The density elasticity is very stable at 0.02, while the market potential elasticity is also very stable at 0.034. Importantly, the overidentification tests are passed (whereas they fail without market potential as a control). More generally, the overidentification test is passed for most combinations of geological instruments and 1831 density. The main systematic failure occurs when the dominant parent material dummies are used. It should be noted that 1831 density is a much stronger instrument, and as a result, it does most of the work in generating the predicted density at the first stage. This greater strength of past density may explain the stability of the coefficients. Nonetheless, in each of the IV regressions of table 1.9, at least one soil dummy (and usually more) is significant (and usually highly so). This implies that we can run meaningful overidentification tests. The fact that their *p*-values are usually well above 10 percent is strongly suggestive that 1831 density and a broad range of soil characteristics all support this 0.02 estimate for the density elasticity of wages.

Finally, in table 1.10, we consider that market potential could also be endogenous. In columns (1) to (3), we use only historical instruments: 1831 and 1881 density, as well as 1831 market potential. The results for W^3 in column (3) are similar to the IV results in table 1.8. In columns (4) to (9), we use 1831 employment density in each regression with two different soils characteristics among erodibility, carbon content, subsoil water capacity, depth to rock, ruggedness, and soil differentiation. The overidentification test is always passed in the table. Although the results are not reported here, this test is also passed for all the other pairwise combinations of these characteristics (except the combination of soil differentiation and carbon content for which the test marginally fails). For our preferred concept of wage, W^3, the coefficients on density and market potential are very stable and confirm the estimates of column (3) with historical instruments and those of the previous table, where market potential is taken to be exogenous. This stability across columns (3) to (9) is interesting, because, as instruments in columns (4) to (9), geological variables and past density are not as strong as the combination of past density and past market potential. Our preferred estimate for the elasticity of wages with respect to employment density is 0.02. With respect to market potential, our preferred estimate is at 0.034.

While regressing mean wages on employment density leads to a measured

Table 1.9 Local wages as a function of density and (exogenous) market potential: Historical and geological instruments

Variable	W^1 OLS (1)	W^2 OLS (2)	W^3 OLS (3)	W^3 TSLS (4)	W^3 TSLS (5)	W^3 TSLS (6)	W^3 TSLS (7)	W^3 TSLS (8)	W^3 TSLS (9)
Ln (density)	0.042	0.048	0.026	0.020	0.020	0.020	0.020	0.020	0.020
	(0.003)***	(0.002)***	(0.001)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***
Ln (market potential)	0.024	0.012	0.027	0.034	0.034	0.034	0.034	0.034	0.034
	(0.006)***	(0.004)***	(0.003)***	(0.003)***	(0.003)***	(0.003)***	(0.003)***	(0.003)***	(0.003)***
Instruments used:									
Ln (1831 density)	—	—	—	Y	Y	Y	Y	Y	Y
Subsoil mineralogy	—	—	—	Y	N	N	N	N	N
Ruggedness	—	—	—	N	Y	N	N	N	N
Subsoil water capacity	—	—	—	N	N	Y	N	N	N
Depth to rock	—	—	—	N	N	N	Y	N	N
Erodibility	—	—	—	N	N	N	N	Y	N
Soil differentiation	—	—	—	N	N	N	N	N	Y
First-stage statistics	—	—	—	128.5	191.6	80.9	96.9	77.9	130.0
Overidentification test p-value	—	—	—	0.72	0.82	0.42	0.54	0.37	0.10
R^2	0.59	0.73	0.73	—	—	—	—	—	—

Note: 306 observations for each regression. All regressions include a constant and three amenity variables (sea, lake, and mountain). Standard errors in parentheses.

***Significant at the 1 percent level.

Table 1.10 **Local wages as a function of density and (endogenous) market potential: Historical and geological instruments**

Variable	W^1 TSLS (1)	W^2 TSLS (2)	W^3 TSLS (3)	W^3 TSLS (4)	W^3 TSLS (5)	W^3 TSLS (6)	W^3 TSLS (7)	W^3 TSLS (8)	W^3 TSLS (9)
Ln (density)	0.033	0.040	0.020	0.018	0.019	0.020	0.020	0.020	0.020
	(0.003)***	(0.003)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.003)***	(0.002)***
Ln (market potential)	0.034	0.020	0.034	0.048	0.039	0.036	0.036	0.033	0.034
	(0.006)***	(0.005)***	(0.003)***	(0.007)***	(0.005)***	(0.005)***	(0.006)***	(0.010)***	(0.007)***
Instruments used:									
Ln (1831 density)	Y	Y	Y	Y	Y	Y	Y	Y	Y
Ln (1881 density)	Y	Y	Y	N	N	N	N	N	N
Ln (1831 market potential)	Y	Y	Y	N	N	N	N	N	N
Erodibility	N	N	N	Y	N	N	N	N	Y
Soil carbon content	N	N	N	Y	Y	N	N	N	N
Subsoil water capacity	N	N	N	N	Y	Y	N	N	N
Depth to rock	N	N	N	N	N	Y	Y	N	N
Ruggedness	N	N	N	N	N	N	Y	Y	N
Soil differentiation	N	N	N	N	N	N	N	Y	Y
First-stage statistics	298.0	298.0	298.0	8.3	17.0	23.0	19.8	8.3	10.5
Overidentification test p-value	0.57	0.36	0.67	0.62	0.19	0.36	0.54	0.11	0.14

Note: 306 observations for each regression. All regressions include a constant and three amenity variables (sea, lake, and mountain). Standard errors in parentheses.

***Significant at the 1 percent level.

elasticity of 0.05, adding further controls and correcting for the endogenous quality and quantity of labor biases bring this number down to about 0.02.

1.6 TFP

1.6.1 Firm and Establishment Data

To construct our establishment-level data, we proceed as follows. We first put together two firm-level data sets: the BRN (Bénéfices Réels Normaux) and the RSI (Régime Simplifié d'Imposition). The BRN contains the balance sheet of all firms in the traded sectors with a turnover above 730,000 euros. The RSI is the counterpart of the BRN for firms with a turnover below 730,000 euros. Although the details of the reporting differ, for our purpose, these two data sets contain essentially the same information. Their union covers nearly all French firms.

For each firm, we have a firm identifier and detailed annual information about its output and its consumption of intermediate goods and materials. This allows us to construct a reliable measure of value added. To estimate TFP (see the following), we use a measure of capital stock based on the sum of the reported book values of productive and financial assets.[24] We also experiment with TFP estimations using the cost of capital rather than assets values, following the detailed methodology developed by Boutin and Quantin (2006).

Since firms can have many establishments at many locations, we also use the SIREN data (Système d'Identification du Répertoire des ENtreprises), which is an exhaustive registry of all establishments in the traded sectors. For each establishment and year, SIREN reports both a firm and an establishment identifier, a municipality code, and total employment. Finally, note that BRN, RSI, and SIREN only report total employment and not hours worked.

To obtain information about hours, we return to the DADS, which reports them after 1993. Hence, for 1994 to 2002, we use another, this time exhaustive, DADS data set.[25] Using the individual information about hours and two-digit occupations that this source contains, we can aggregate it at the establishment level to obtain the hours for all employees and by skill group. We emphasize this because of the suspected importance of labor quality.

24. In this respect, we proceed like Syverson (2004). Nevertheless, valuing assets at their historical costs is not without problems. We minimize them by estimating TFP at the three-digit level with 114 sectors. Indeed, the capital stocks of firms within the same sector are likely to be of the same vintage when sectors are more narrowly defined. We also use year dummies. An alternative would be to deflate assets using economic criteria. However, our panel is rather short, which makes it difficult to trace the original investments. Our procedure also differs from that of Olley and Pakes (1996), who use a permanent inventory method.

25. Unfortunately, this data cannot be used for our wage regressions, because the different years have not been linked up.

To avoid estimating too many coefficients for different types of labor, we aggregate two-digit occupational categories into three groups: high-, intermediate- and low-skill workers, following the classification of Burnod and Chenu (2001).

To merge these four data sets, we extend the procedure of Aubert and Crépon (2004). At the establishment level, we first match SIREN with DADS using the establishment identifier present in both data sets. This establishment-level data set (sector and hours by skill group) is needed later to create a number of local characteristics. Next, we aggregate this establishment data set at the firm level using the firm identifier. Finally, we merge this firm data with RSI and BRN to recover firm-level information. For each firm between 1994 and 2002, we end up with its value added, the value of its assets, and total hours worked by establishment and skill group. The total number of observations for 1994 is 942,506. This number rises slowly over the period.

Finally, to avoid dealing with the complications of TFP estimation for multiestablishment firms for which capital and output are known only at the firm level, we restrict our attention to single-establishment firms to estimate TFP.[26] Because the information about very small firms tends to be noisy, we only retain firms with more than five employees.

1.6.2 Constructing Area-Year Measures of TFP

We now turn to TFP and start by constructing productivity measures for each employment area and year from TFP regressions. We estimate TFP for 114 sectors separately. For simplicity, we ignore sector subscripts for the coefficients. For firm i in a given sector, its value added va_{it} is specified as:

$$\ln \text{va}_{it} = \alpha \ln k_{it} + \beta \ln l_{it} + \sum_{m} \beta_m^S q_{imt} + \phi_t + \varepsilon_{it}, \tag{18}$$

where k_{it} is the capital of firm i, l_{it} is its labor (in hours), q_{imt} is the share of labor hours in skill group m, ϕ_t is a year fixed effect, and ε_{it} is an error term measuring firm TFP. The way we introduce skill shares is justified in Hellerstein, Neumark, and Troske (1999).

Three important issues are worth highlighting at this stage. First, we face the same problem as with wages regarding input quality, and more particularly, labor. Unfortunately, workers characteristics are typically scarce in firm- or establishment-level data. We use the strategy used in equation (18) based on occupational categories to control for labor quality.[27] This is obvi-

26. With multiestablishment firms, we need to impute the same residual estimated from a firm-level production function to all establishments of the same firm. This is a strong assumption that we would rather not make. In results not reported here, we nonetheless experimented with TFP estimated from multiestablishment firms.

27. An obvious way to deal with the unobserved quality of the workforce is to use fixed effects, but unfortunately, their use is often problematic with firm-level data because of the

ously a less powerful set of controls than the individual fixed effects used in the preceding wage regressions.

Second, we can hope to control for the two main factors of production—capital and labor—but not for other factors—and in particular.[28] As argued previously, the price of land is expected to affect the consumption of land and thus production while at the same time to be correlated with other local characteristics. Again, instrumenting for these local characteristics is the solution we consider here. Furthermore, output prices are unobserved and are likely to be correlated with local characteristics as well. To the extent that we think of our work as looking into the determinants of local value added rather than pure productivity, this need not bother us much here.[29]

The third issue about TFP estimation is related to the fact that input choices are expected to be endogenous. This issue has received a lot of attention in the literature (see Ackerberg, Caves, and Frazer [2006], for a recent contribution). For our purpose, this endogeneity bias matters only when it differs across areas. Our main TFP results were estimated using Olley and Pakes (OP; 1996). See appendix A for details about the OP approach. This approach allows us to recover r_{it}, an estimator of ε_{it}. We then average it within sectors, areas, and years:

(19)
$$r_{ast} \equiv \frac{1}{L_{ast}} \sum_{i \in (a,s,t)} l_{i,t} r_{i,t},$$

where $L_{ast} \equiv \Sigma_{i \in (a,s,t)} l_{i,t}$ is the total number of hours worked in area a, sector s, and year t. A first measure of the local productivity of the average firm in area a and year t, denoted TFP^1_{at}, is obtained by averaging equation (19) across sectors, within areas and years, with weights equal to the number of firms:

(20)
$$\text{TFP}^1_{at} \equiv \frac{1}{n_{at}} \sum_{s \in (a,t)} n_{ast} r_{ast},$$

where n_{ast} and n_{at} are the total numbers of firms for area a, sector s, and year t, and for area a and year t, respectively.

This measure of TFP does not control for the local sector structure. To control for the fact that high productivity sectors may have a propensity to locate in particular areas, we regress r_{ast} on a full set of sector fixed effects, γ_s:

sluggish adjustment of capital. See Fox and Smeets (2007) for a more thorough attempt to take (observable) input quality into account when estimating TFP. Like us, they find that measures of labor quality are highly significant, but taking labor quality into account does not reduce the large dispersion of TFP across firms.

28. We also expect the functional form to matter, although we limit ourselves to simple specifications here.

29. In a different context where one is interested in distinguishing between price and productivity effects, such benign neglect may not be warranted. See, for instance, Combes et al. (2007). Note that this issue also applies to wages.

(21) $$r_{ast} = \gamma_s + \iota_{ast}.$$

This equation is estimated with weighted least squares, (WLS), where the weights are the number of establishments associated with each observation.[30] To estimate a productivity index TFP^2_{at}, we average the estimated residuals of equation (21) for each area and year:

(22) $$\text{TFP}^2_{at} \equiv \frac{1}{n_{at}} \sum_{s \in (a,t)} n_{ast} \hat{\iota}_{ast}.$$

The variable TFP^2_{at} can thus be interpreted as a productivity index net of sector effects.

We finally compute a third local productivity index, TFP^3_{at}, controlling for variables at the area and sector level, X_{ast}. For that purpose, we estimate the equation:

(23) $$r_{ast} = \text{TFP}^3_{at} + \gamma_s + X_{ast}\varphi + \varepsilon_{ast}.$$

This equation is estimated with WLS, where weights are once again the number of establishments associated with each observation. It mimics equation (12) for wages and uses the same (centered) local characteristics (same sector specialization, number of firms, share of professionals, average age, and average squared age). The main difference, however, is that these characteristics are constructed using the TFP data and not the wage data.

For comparison, we also estimate equation (18) with OLS. Denote $\hat{\varepsilon}_{it}$ the estimated residual for firm i. We then define:

(24) $$r^{\text{OLS}}_{ast} \equiv \frac{1}{L_{ast}} \sum_{i \in (a,s,t)} l_{it} \hat{\varepsilon}_{it},$$

the OLS counterpart to equation (19). It is possible to recompute our three measures of local productivity, TFP^1_{at}, TFP^2_{at}, and TFP^3_{at}, using equation (24) rather than equation (19). Next, we compare the coefficients in our main regressions using local productivity indices computed from OP and OLS.

One aspect of the simultaneity bias at the area level is that establishments may produce more and grow larger in areas where the local productivity is higher. It is possible to control for that by introducing area and year fixed effects g_{at} in equation (18):

(25) $$\ln \text{va}_{it} = \alpha \ln k_{it} + \beta \ln l_{it} + \sum_m \beta^S_m q_{imt} + \phi_t + g_{at} + \varepsilon_{it}.$$

This equation is estimated with OLS. Since this equation is estimated for each sector, the area-year fixed effects depend on the sector and can be rewritten with a sector subindex, g_{ast}. We can then define $r^{\text{FE}}_{ast} \equiv g_{ast}$, the fixed

30. These weights give more importance to sectors and areas for which a larger number of $r_{i,t}$ are considered when constructing $r_{s,a,t}$. For these area-sector-years, the sampling error on $r_{s,a,t}$ is usually smaller. Weighing should thus reduce the impact of the sampling error on the dependent variable that comes from the first-stage estimation.

effect counterpart to equations (19) and (24), and construct once again our three measures of local productivity.[31]

Finally, we average our estimates across years as we did for wages to avoid identifying out of the temporal variation.[32] Before going to our results, note that our local productivity variables are strongly correlated with one another. Using OP estimates, the correlation between TFP^1 and TFP^2 is 0.93, the correlation between TFP^1 and TFP^3 is also 0.93, and the correlation between TFP^2 and TFP^3 is 0.98. For TFP^3, the correlation between OP and OLS estimates is 0.96, the correlation between OP and fixed effects estimates is 0.91, and the correlation between OLS and fixed effects is also 0.91. Finally, the correlation between TFP^3 estimated with OP and mean wages (W^1) is 0.77.[33] This correlation rises to 0.88 after correcting wages of sector effects (W^2) or to 0.87 after correcting wages of sector and worker effects (W^3).

1.6.2 Results

Table 1.11 presents the results of three regressions for our three measures of local OP productivity: TFP^1, the mean productivity computed in equation (20); TFP^2, the local productivity controlling for sector fixed effects as estimated in equation (21); and TFP^3, the local productivity estimated in equation (23), which conditions out a broader set of sector effects. This table mirrors the wage table 1.6 for productivity. In columns (1), (2), and (3), these three measures of local productivity are regressed on log employment density controlling for amenities using OLS. The mean elasticity of TFP with respect to density is at 0.04 for mean productivity, 0.041 when taking out sector effects, and 0.047 when also controlling for the local sector structure. In columns (4), (5), and (6), we instrument employment density with 1831 urban population density. The TSLS coefficients for employment density are marginally lower than in columns (1), (2), and (3). In columns (7), (8), and (9), we add 1881 population density to instrument for contemporaneous employment density. Although the Sargan test of overidentification marginally fails in column (7) with a p-value of 7 percent, the results are very close to those of columns (4), (5), and (6).

Comparing these results to those of table 1.6 for wages, we note the following. First, instrumenting for contemporaneous employment density with deep historical lags lowers the coefficients in roughly the same proportion in both cases. This confirms our finding of a mild simultaneity bias regarding the quantity of labor. Second, controlling for sector effects in TFP^3 compared to TFP^1 raises the coefficient on employment density, just like it does when considering W^2 instead of W^1 (although the increase is slightly more

31. We also experimented with a number of alternative TFP approaches, such as GMM, cost shares, IV cost shares, Levinsohn and Petrin (2003), and so forth.

32. Like with wages, these averages are now unweighted.

33. Recall that the years over which TFP and wages are computed are not the same.

Table 1.11 **Local TFP (Olley-Pakes) as a function of density: OLS and historical instruments**

Variable	TFP^1 OLS (1)	TFP^2 OLS (2)	TFP^3 OLS (3)	TFP^1 TSLS (4)	TFP^2 TSLS (5)	TFP^3 TSLS (6)	TFP^1 TSLS (7)	TFP^2 TSLS (8)	TFP^3 TSLS (9)
Ln (density)	0.040	0.041	0.047	0.031	0.034	0.038	0.033	0.035	0.039
	(0.002)***	(0.002)***	(0.002)***	(0.003)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***
Instruments used:									
Ln (1831 density)	—	—	—	Y	Y	Y	Y	Y	Y
Ln (1881 density)	—	—	—	—	—	—	Y	Y	Y
First-stage statistics	—	—	—	371.4	371.4	371.4	429.1	429.1	429.1
Overidentification test *p*-value	—	—	—	—	—	—	0.07	0.18	0.37
R^2	0.63	0.70	0.75	—	—	—	—	—	—

Note: 306 observations for each regression. All regressions include a constant and three amenity variables (sea, lake, and mountain). Standard errors in parentheses.

***Significant at the 1 percent level.

important here).[34] A stronger effect of density after conditioning out sector effects is consistent with the notion that sectors located in less-dense areas may gain less from overall density and perhaps more from same sector specialization or another sector characteristics that are conditioned out in TFP^3.[35] Third, it is also interesting to note that when a direct comparison is possible, the density elasticities for wages tend to be above those for TFP. From the theoretical framework developed previously (and particularly equations [5] and [6]), we actually expect the coefficients on employment density to be higher for wages by a factor equal to the inverse of the labor share ($1/[1 - \alpha]$). With labor coefficients typically between 0.5 and 0.75, the difference between the two sets of estimates is of the right magnitude, although a bit smaller than expected.

To assess the sensitivity of our results to the approach used to estimate TFP, we reproduce in table 1B.1 of appendix B some of the regressions of table 1.11 using alternative local productivity indices. These measures of local TFP are constructed from the OLS estimates of equation (18) and from equation (25), which computes local productivity fixed effects. When TFP is estimated with OLS instead of OP, the coefficients on density are close, though not exactly the same.[36] When TFP is estimated with local fixed effects instead of OP, we find lower coefficients on density. At this stage, our best estimate of the density elasticity of TFP is at 0.04.[37]

Turning to geological instruments, table 1.12 mirrors for TFP what table 1.7 does for wages.[38] Columns (1) to (3) use subsoil mineralogy and ruggedness to instrument for employment density using our three measures of TFP as dependent variables. The coefficients on density are higher than with historical instruments in table 1.11. Such a difference between geological

34. While TFP^1 may be taken to be the counterpart of W^1, TFP^3 corresponds to W^2. Because we cannot control for input quality well, there is no TFP concept that corresponds to W^3.

35. This higher coefficient on density with TFP^3 is also consistent with possible correlations between unobserved input quality and the local structure of production.

36. When TFP is estimated with OP, we must drop the first year of data and firms with no investment. Estimating TFP with OLS on the same sample of firms as with OP makes no difference with respect to OLS estimates of local productivity.

37. Comparing these results to Henderson (2003), the main study about agglomeration effects using TFP data in the literature, is not easy. First, Henderson (2003) uses very different U.S. data for which value added cannot be measured directly, and he focuses on five industries only. Second, he concentrates on sector effects and uses as a key independent variable the number of plants in the local industry. We focus instead on total local employment, conditioning out local industry shares (among others) in some TFP measures. Third, he estimates TFP and the effects of local characteristics in one stage using a different specification for productivity, which includes firm fixed effects. Finally, he tackles endogeneity problems using a GMM approach. Despite these differences, his findings of strong heterogeneity across industries and modest to high scale effects at the industry level are consistent with ours.

38. That is, aside from the difference in dependent variables, the regressions are exactly the same. The values taken by employment density differ very slightly because of the differences in years between the wage and TFP data and the difference in data source.

Table 1.12 **Local TFP (Olley-Pakes) as a function of density: Geological instruments**

Variable	TFP^1 TSLS (1)	TFP^2 TSLS (2)	TFP^3 TSLS (3)	TFP^3 LIML (4)	TFP^3 LIML (5)	TFP^3 LIML (6)	TFP^3 LIML (7)	TFP^3 LIML (8)
Ln (density)	0.054	0.041	0.045	0.045	0.048	0.047	0.045	0.046
	(0.009)***	(0.007)***	(0.007)***	(0.007)***	(0.005)***	(0.005)***	(0.007)***	(0.005)***
Instruments used:								
Subsoil mineralogy	Y	Y	Y	Y	Y	N	N	N
Ruggedness	Y	Y	Y	Y	N	N	N	N
Depth to rock	N	N	N	N	N	Y	N	N
Soil carbon content	N	N	N	N	N	Y	Y	N
Topsoil water capacity	N	N	N	N	N	N	Y	Y
Dominant parent material (6 categories)	N	N	N	N	Y	N	N	Y
First-stage statistics	5.5	5.5	5.5	5.5	5.9	7.4	5.2	6.0
Overidentification test p-value	0.58	0.60	0.38	0.38	0.26	0.19	0.15	0.22

Note: 306 observations for each regression. All regressions include a constant and three amenity variables (sea, lake, and mountain). Standard errors in parentheses.

***Significant at the 1 percent level.

and historical instruments is also observed with wages.[39] To repeat, it reflects the fact that geological instruments have a larger correlation with market potential than local density. The results of column (3) are confirmed in column (4), when LIML rather than TSLS is used, and in columns (5) to (8), when different sets of instruments are used. It is interesting to note that the overidentification tests are passed for the same specifications as with wages (and they also fail for the same unreported regressions).

To mirror again the analysis performed with wages, table 1B.2 of appendix B performs the regressions of table 1.8 with TFP rather than wages, using historical and geological instruments at the same time. The results are again extremely consistent with the wage results. The coefficients on employment density in table 1B.2 with both sets of instruments are the same as those that use historical instruments only in table 1.11. This near equality also holds with wages. Furthermore, overidentification tests appear to be passed or failed with the same combinations of instruments. An exception is column (8) with dominant parent material and topsoil water capacity. The test is passed with wages with a *p*-value of 15 percent, while it is failed with TFP (*p*-value of 5 percent).

In table 1B.3 of appendix B, we add market potential as the explanatory variable, just as we do with wages in table 1.9. We again use the exact same specifications as with wages. Adding market potential to the OLS specifications lowers the coefficient on employment density for TFP. The elasticity of TFP with respect to market potential is about half the density elasticity. These two results closely mirror what happens in our wage regressions when we add market potential as an explanatory variable. In the second part of table 1B.3, we instrument employment density with 1831 density and a range of soil characteristics. The coefficient on density declines by about 0.01 point to 0.033, while that on market potential increases by about the same amount to 0.027. This again is very close to what happens in the wage regressions. Interestingly, the same combinations of instruments pass the overidentification tests with both wages and TFP. The failure of the Sargan test in the last column of table 1B.3 is an exception.

Finally, in table 1.13, market potential is also assumed to be endogenous. As with wages in table 1.10, we instrument density and market potential with historical and soil variables. The main result is that instrumenting for market potential leaves its coefficient unchanged. The IV coefficient on employment density is also unchanged. This is the same outcome as with wages. In tables 1B.4 and 1B.5 of appendix B, we repeat the same exercise but use TFP indices estimated with OLS and with local fixed effects, as in equation (25). As in previous comparisons, the results for OLS and OP TFP are very close. With (local) fixed effect TFP, the density and market potential elasticities are

39. As previously, the coefficients on density are also slightly above those obtained with wages for similar regressions.

Table 1.13 **Local TFP (Olley-Pakes) as a function of density and (endogenous) market potential: Historical and geological instruments**

Variable	TFP^1 TSLS (1)	TFP^2 TSLS (2)	TFP^3 TSLS (3)	TFP^3 TSLS (4)	TFP^3 TSLS (5)	TFP^3 TSLS (6)	TFP^3 TSLS (7)	TFP^3 TSLS (8)	TFP^3 TSLS (9)
Ln (density)	0.028	0.030	0.035	0.034	0.034	0.034	0.034	0.035	
	(0.003)***	(0.002)***	(0.002)***	(0.003)***	(0.003)***	(0.003)***	(0.003)***	(0.004)***	(0.003)***
Ln (market potential)	0.025	0.027	0.026	0.021	0.023	0.021	0.022	0.024	0.018
	(0.005)***	(0.004)***	(0.004)***	(0.009)***	(0.007)***	(0.006)***	(0.008)***	(0.013)***	(0.009)***
Instruments used:									
Ln (1831 density)	Y	Y	Y	Y	Y	Y	Y	Y	Y
Ln (1881 density)	Y	Y	Y	N	N	N	N	N	N
Ln (1831 market potential)	Y	Y	Y	N	N	N	N	N	N
Erodibility	N	N	N	Y	N	N	N	N	Y
Soil carbon content	N	N	N	Y	Y	N	N	N	N
Subsoil water capacity	N	N	N	N	Y	Y	N	N	N
Depth to rock	N	N	N	N	N	Y	Y	N	N
Ruggedness	N	N	N	N	N	N	Y	Y	N
Soil differentiation	N	N	N	N	N	N	N	Y	Y
First-stage statistics	230.6	230.6	230.6	8.2	16.4	22.8	20.2	8.0	10.5
Overidentification test *p*-value	0.16	0.43	0.17	0.68	0.90	0.60	0.29	0.04	0.16

Note: 306 observations for each regression. All regressions include a constant and three amenity variables (sea, lake, and mountain). Standard errors in parentheses.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

lower than with OLS and OP TFP. However, we observe the same stability in the coefficients across regressions. This suggests that the method used to estimate TFP matters with respect to the point estimates for the density and market potential elasticities (though by only 0.01). However, the choice of TFP estimation does not matter otherwise.

1.7 Conclusions

We revisit the estimation of local scale effects using large-scale French wage and TFP data. To deal with the "endogenous quantity of labor" bias (i.e., urban agglomeration is a consequence of high local productivity rather than a cause), we take an instrumental variable approach and introduce a new set of geological instruments in addition to standard historical instruments. To deal with the "endogenous quality of labor" bias (i.e., cities attract skilled workers so that the effects of skills and urban agglomeration are confounded), we take a worker fixed effect approach.

Our first series of findings relates to the endogenous quantity of labor bias. Long lags of our endogenous explanatory variables make for strong instruments. Geological characteristics are more complicated instruments to play with. Nevertheless, geological and historical instruments lead to similar answers once the regression is properly specified: the simultaneity problem between employment density and local wages/productivity is relatively small. It reduces the impact of density by around one-fifth.

Our second finding relates to the endogenous quality of labor bias. Better workers are located in more productive areas. This sorting of workers by skills (observed and unobserved) is quantitatively more important than the endogenous quantity of labor bias. In our regressions, we address sorting using the panel dimension of our wage data. The density elasticity is divided by almost 2. Applying this type of approach to TFP is problematic. We thus put more weight on our wage results than we do on our TFP results. Nonetheless, the high degree of consistency between wage and TFP results is reassuring.

We believe the priority for future work should be to develop more sophisticated approaches to deal with the sorting of workers across places. Awaiting progress on this issue, our preferred estimates for the elasticity of wages to density is at 0.02 and is around 0.035 for the density elasticity of TFP. For market potential, we find elasticities around 0.035 for wages and 0.025 for TFP. Finally, our result about the relative importance of the two biases raises an interesting question. To what extent does it reflect particular features of the French housing and labor market institutions? One may imagine that in a country like the United States with greater labor mobility and a much flatter housing supply curve (in at least part of the country), the endogenous quantity of labor bias might dominate. Further research should inform this question.

Appendix A

Implementation of Olley and Pakes (1996)

The error term in equation (18) is rewritten as $\varepsilon_{it} \equiv v_{it} + \xi_{it}$, where v_{it} is the part of the error term that influences the decision of the firm regarding its factors, and ξ_{it} is an independent noise. The crucial assumption is that capital investment, I_{it}, can be written as a function of the error term, v_{it}, and current capital: $I_{it} \equiv f_t(k_{it}, v_{it})$, with $\partial f_t / \partial_{it} > 0$. The investment function can be inverted to yield: $v_{it} = f_t^{-1}(k_{it}, I_{it})$. Equation (18) can then be rewritten as:

$$\text{(A1)} \quad \ln \text{va}_{it} = \alpha \ln k_{it} + \beta \ln l_{it} + \sum_m \beta^S_m q_{imt} + \phi_t + f_t^{-1}(k_{it}, I_{it}) + \xi_{it}.$$

This equation can be estimated in two stages. Denote $\Phi_t(k_{it}, I_{it}) \equiv f_t^{-1}(k_{it}, I_{it}) + \alpha \ln k_{it} + \phi_t$. Equation (A1) becomes:

$$\text{(A2)} \quad \ln \text{va}_{it} = \beta \ln l_{it} + \sum_m \beta^S_m q_{imt} + \Phi_t(k_{it}, I_{it}) + \xi_{it}.$$

This equation can be estimated with OLS after approximating $\Phi_t(k_{it}, I_{it})$ with a third-order polynomial, crossing k_{it}, I_{it}, and year dummies. Its estimation allows us to recover some estimators for the labor- and skill-share coefficients ($\hat{\beta}$ and $\hat{\beta}^S_m$). It is then possible to construct $z_{it} \equiv \ln \text{va}_{it} - \hat{\beta} \ln l_{it} - \Sigma_m \hat{\beta}^S_m q_{imt}$. Furthermore, the error v_{it} is rewritten as the projection on its lag and an innovation: $v_{it} \equiv h(v_{it-1}) + \zeta_{it-1}$. Using $v_{it-1} = f_{t-1}^{-1}(k_{it-1}, I_{it-1}) = \Phi_{t-1}(k_{it-1}, I_{it-1}) - \alpha \ln k_{j,t-1} - \phi_{t-1}$, the value added equation then becomes:

$$\text{(A3)} \quad z_{it} = \alpha \ln k_{it} + \phi_t + h\,[\hat{\Phi}(k_{it-1}, I_{it-1}) - \alpha \ln k_{jt-1} - \phi_{t-1}] + \psi_{it},$$

where ψ_{it} is a random error. The function $h(.)$ is approximated by a third-order polynomial, and equation (A3) is estimated with nonlinear least squares. It allows us to recover some estimators of the capital coefficient $\hat{\alpha}$ and the year dummies $\hat{\phi}_t$. Firm TFP is then defined as $r_{it} \equiv z_{it} - \hat{\alpha} \ln k_{it} - \hat{\phi}_t$. It is an estimator of ε_{it}. For further details about the implementation procedure in stata used in our chapter, see Arnold (2005).

Although the OP method allows us to control for simultaneity, it has some drawbacks. In particular, we need to construct investment from the data: $I_{it} = k_{it} - k_{it-1}$. As a consequence, it can be computed only for firms that are present in two consecutive years. Other observations must be dropped. Furthermore, the investment equation $I_{it} = f_t(k_{it}, v_{it})$ can be inverted only if $I_{it} > 0$. Hence, we can keep only observations for which $I_{it} > 0$. This double selection may introduce a bias, for instance, if (a) there is greater "churning" (i.e., entry and exits) in denser areas and (b) age and investment affect productivity positively. Then, more establishments with a low productivity may be dropped in high-density areas. In turn, this may increase the measured difference in local productivity between areas of low and high density. Reestimating OLS TFP on the same sample of firms used for OP shows that fortunately, this is not the case on French data.

Appendix B

Further Results

Table 1B.1 Local TFP (OLS and fixed effects) as a function of density: OLS and historical instruments

	TFP estimated with OLS				TFP with fixed effects			
Variable	TFP^1 OLS (1)	TFP^3 OLS (2)	TFP^1 TSLS (3)	TFP^3 TSLS (4)	TFP^1 OLS (5)	TFP^3 OLS (6)	TFP^1 TSLS (7)	TFP^3 TSLS (8)
Ln (density)	0.035	0.049	0.029	0.042	0.027	0.040	0.018	0.033
	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***
Instruments used:								
Ln (1831 density)	—	—	Y	Y	—	—	Y	Y
Ln (1881 density)	—	—	Y	Y	—	—	Y	Y
First-stage statistics	—	—	432.3	432.3	—	—	432.3	432.3
Overidentification test *p*-value	—	—	0.21	0.10	—	—	0.85	0.75
R^2	0.63	0.75	—	—	0.45	0.67	—	—

Note: 306 observations for each regression. All regressions include a constant and three amenity variables (sea, lake, and mountain). Standard errors in parentheses.

***Significant at the 1 percent level.

Table 1B.2 Local TFP (Olley-Pakes) as a function of density: Historical and geological instruments

Variable	TFP^1 TSLS (1)	TFP^2 TSLS (2)	TFP^3 TSLS (3)	TFP^3 TSLS (4)	TFP^3 GMM (5)	TFP^3 TSLS (6)	TFP^3 TSLS (7)	TFP^3 TSLS (8)
Ln (density)	0.031	0.034	0.038	0.038	0.039	0.039	0.039	0.038
	(0.003)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***
Instruments used:								
Ln (1831 density)	Y	Y	Y	Y	Y	Y	Y	Y
Subsoil mineralogy	Y	Y	Y	Y	N	Y	N	N
Ruggedness	N	N	N	N	Y	Y	Y	N
Hydrogeological class	N	N	N	N	N	N	Y	N
Topsoil water capacity	N	N	N	N	N	N	N	Y
First-stage statistics	129.7	129.7	129.7	103.3	194.1	100.3	64.5	125.7
Overidentification test p-value	0.14	0.56	0.78	0.66	0.14	0.46	0.33	0.05

Note: 306 observations for each regression. All regressions include a constant and three amenity variables (sea, lake, and mountain). Standard errors in parentheses.

***Significant at the 1 percent level.

Table 1B.3 Local TFP (Olley-Pakes) as a function of density and (exogenous) market potential: Historical and geological instruments

Variable	TFP[1] OLS (1)	TFP[2] OLS (2)	TFP[3] OLS (3)	TFP[3] TSLS (4)	TFP[3] TSLS (5)	TFP[3] TSLS (6)	TFP[3] TSLS (7)	TFP[3] TSLS (8)	TFP[3] TSLS (9)
Ln (density)	0.036	0.037	0.043	0.033	0.033	0.034	0.033	0.033	0.033
	(0.002)***	(0.002)***	(0.002)***	(0.003)***	(0.003)***	(0.003)***	(0.003)***	(0.003)***	(0.003)***
Ln (market potential)	0.017	0.019	0.017	0.027	0.027	0.027	0.028	0.027	0.028
	(0.004)***	(0.004)***	(0.004)***	(0.004)***	(0.004)***	(0.004)***	(0.004)***	(0.004)***	(0.004)***
Instruments used:									
Ln (1831 density)	—	—	—	Y	Y	Y	Y	Y	Y
Subsoil mineralogy	—	—	—	Y	N	N	N	N	N
Ruggedness	—	—	—	N	Y	N	N	N	N
Subsoil water capacity	—	—	—	N	N	Y	N	N	N
Depth to rock	—	—	—	N	N	N	Y	N	N
Erodibility	—	—	—	N	N	N	N	Y	N
Soil differentiation	—	—	—	N	N	N	N	N	Y
First-stage statistics	—	—	—	115.0	170.6	71.4	86.6	70.7	116.1
Overidentification test p-value	—	—	—	0.27	0.50	0.69	0.32	0.42	0.05
R^2	0.64	0.72	0.76	—	—	—	—	—	—

Note: 306 observations for each regression. All regressions include a constant and three amenity variables (sea, lake, and mountain). Standard errors in parentheses.

***Significant at the 1 percent level.

Table 1B.4 Local TFP (OLS) as a function of density and (endogenous) market potential: Historical and geological instruments

Variable	TFP^1 TSLS (1)	TFP^2 TSLS (2)	TFP^3 TSLS (3)	TFP^3 TSLS (4)	TFP^3 TSLS (5)	TFP^3 TSLS (6)	TFP^3 TSLS (7)	TFP^3 TSLS (8)	TFP^3 TSLS (9)
Ln (density)	0.023	0.022	0.037	0.035	0.035	0.036	0.037	0.036	0.035
	(0.002)***	(0.002)***	(0.002)***	(0.003)***	(0.003)***	(0.003)***	(0.003)***	(0.004)***	(0.003)***
Ln (market potential)	0.030	0.030	0.024	0.022	0.026	0.019	0.012	0.020	0.020
	(0.004)***	(0.004)***	(0.004)***	(0.010)**	(0.007)***	(0.007)***	(0.008)	(0.013)	(0.009)**
Instruments used:									
ln (1831 density)	Y	Y	Y	Y	Y	Y	Y	Y	Y
ln (1881 density)	Y	Y	Y	N	N	N	N	N	N
ln (1831 market potential)	Y	Y	Y	N	N	N	N	N	N
Erodibility	N	N	N	Y	N	N	N	N	Y
Soil carbon content	N	N	N	Y	Y	N	N	N	N
Subsoil water capacity	N	N	N	N	Y	Y	N	N	N
Depth to rock	N	N	N	N	N	Y	Y	N	N
Ruggedness	N	N	N	N	N	N	Y	Y	N
Soil differentiation	N	N	N	N	N	N	N	Y	Y
First-stage statistics	232.2	232.2	232.2	8.2	16.4	22.9	20.2	8.0	10.5
Overidentification test *p*-value	0.54	0.56	0.04	0.73	0.29	0.05	0.14	0.38	0.87

Note: 306 observations for each regression. All regressions include a constant and three amenity variables (sea, lake, and mountain). Standard errors in parentheses.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

Table 1B.5 **Local TFP (fixed effects) as a function of density and (endogenous) market potential: Historical and geological instruments**

Variable	TFP^1 TSLS (1)	TFP^2 TSLS (2)	TFP^3 TSLS (3)	TFP^3 TSLS (4)	TFP^3 TSLS (5)	TFP^3 TSLS (6)	TFP^3 TSLS (7)	TFP^3 TSLS (8)	TFP^3 TSLS (9)
Ln (density)	0.011	0.013	0.028	0.030	0.029	0.030	0.030	0.030	0.030
	(0.002)***	(0.002)***	(0.002)***	(0.003)***	(0.003)***	(0.003)***	(0.003)***	(0.004)***	(0.003)***
Ln (market potential)	0.032	0.027	0.022	0.013	0.015	0.011	0.008	0.012	0.010
	(0.004)***	(0.004)***	(0.004)***	(0.009)	(0.007)**	(0.007)	(0.008)	(0.013)	(0.009)
Instruments used:									
Ln (1831 density)	Y	Y	Y	Y	Y	Y	Y	Y	Y
Ln (1881 density)	Y	Y	Y	N	N	N	N	N	N
Ln (1831 market potential)	Y	Y	Y	N	N	N	N	N	N
Erodibility	N	N	N	Y	N	N	N	N	Y
Soil carbon content	N	N	N	Y	Y	N	N	N	N
Subsoil water capacity	N	N	N	N	Y	Y	N	N	N
Depth to rock	N	N	N	N	N	Y	Y	N	N
Ruggedness	N	N	N	N	N	N	Y	Y	N
Soil differentiation	N	N	N	N	N	N	N	Y	Y
First-stage statistics	232.1	232.1	232.1	8.2	16.4	22.9	20.2	8.0	10.5
Overidentification test *p*-value	0.63	0.15	0.89	0.68	0.79	0.56	0.38	0.33	0.57

Note: 306 observations for each regression. All regressions include a constant and three amenity variables (sea, lake, and mountain). Standard errors in parentheses.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

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