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DEPARTMENT OF ECONOMICS

Essays on Local Labor Markets

Clément Malgouyres

Thesis submitted for assessment with a view to obtaining the degree of Doctor of Economics of the European University Institute

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European University Institute
Department of Economics

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Summary

This thesis studies empirically several issues regarding the functioning of local labor markets.

In Chapter 1, I follow the methodology developed by Autor, Dorn, and Hanson (2013) to estimate the impact of Chinese imports competition onto French local labor markets, with an emphasis on the spill-overs effects beyond the manufacturing sector on the structure of employment and wages. Local employment and total labor income in both manufacturing and non-manufacturing are negatively affected by rising exposure to imports. Imports competition from China polarized the local structure of employment in the manufacturing sector. Hourly wages distribution is negatively affected but overall wage dispersion is not increased. The non-traded sector even experiences a decrease in lower-tail inequality. Exploiting geographical variation in the bite of the minimum wage, I find evidence suggesting that the minimum wage explains this effect.

In Chapter 2, I use a refinement of empirical strategy in Chapter 1 to look at whether communities suddenly affected by rising economic integration with low-wage countries tended to vote more for the far-right parties over the last four French presidential elections. I find evidence of a small but significantly positive impact of imports competition exposure on votes for the far-right: a one standard-deviation increase in imports-per-worker causes the change in the far-right share to increase by 7 percent of a standard deviation. Further results suggest that this effect has been increasing over the time period considered. We conduct a simple sensitivity test supporting the notion that (i) omitting local share of immigrants is likely to bias our estimate downward, and that (ii) this bias is likely to negligible.

In Chapter 3, co-authored with Camille Hémet, we study the impact of local diversity on labour market outcomes, at two different level of aggregation: local labor market and

immediate neighborhood. We find that employment correlates positively with local labor market diversity, but negatively with neighborhood diversity. Using an instrumental variable approach to deal with local labor market diversity drives the positive correlation to zero, confirming the suspicion of self-selection. Regarding neighborhood diversity, we adopt the strategy of Bayer et al. (2008), taking advantage of the very precise localization of the data: the negative effect of diversity is reinforced. We also show that nationality-based diversity matters more than parents' origin-based diversity, giving insights on the underlying mechanisms.

In Chapter 4, co-authored with Camille Hémet, we exploit some specificities of the French Labor Force Survey, in order to detect the presence of referral networks among neighbors. We show the presence of referral networks, provide extensive robustness checks and investigate two rather understudied issues in the literature: (i) what kind of job transition are local referrals associated with (job-to-job or unemployment-to-job), (ii) how has the strength of local referral effects evolved overtime?

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Chapter 1

The Impact of Chinese Imports Competition on the Local Structure of Employment and Wages: Evidence from France

1.1 Introduction

Rising imports competition from low-wage countries and its impact on employment in the manufacturing sector has been a very widely debated issue across the industrialized world. Among low-wage countries, China stands out as the key player. In one decade (1998 to 2008), China's share of world exports went from 3.3 to 9.5 percent, growing at 15 percent annually in value.[1] Figure 1.1 displays imports and trade balance of France with respect to China and another set of low-wage countries (LWC). China trade's specificity with respect to France, and many high-income countries, stems from the high growth rate of its exports as well as the strong French trade deficit in comparison with other LWCs. Such a sudden rise in imports competition might shrink the manufacturing sector. In turn, decline in manufacturing activity, whether or not triggered by trade shocks, is likely to be associated with a host of local spill-over effects onto local labor markets. Given the large size of the non-traded sector in high-income economies, the question of the transmission of trade shocks outside of manufacturing is of outmost importance for the study of the labor market, especially in its spatial dimension.

[1]This figures are based on author's own calculation based on UN Comtrade. Hanson (2012) presents a complete pictures of the role of emerging economies, particularly China, in world trade over the last three decades.

In this paper, I study the adjustment of local labor markets in France to the massive increase in Chinese imports competition. The aim of the study is to analyze the local impact of China-induced trade shocks on the structure of employment and wages both within and outside of manufacturing. It contributes to the literature on the impact of trade shocks on local labor markets in two main ways. First, it applies the methodology developed by Autor, Dorn, and Hanson (2013) with an emphasis on spill-overs outside of manufacturing caused by Chinese imports competition. The rich administrative dataset used allows to look at a wider array of labor markets outcomes by sector and occupations – employment but also hours worked, total labor earnings. It also investigates the possibly job-polarizing or skill-biased nature of the employment effect of rising Chinese imports competition. Second, it provides estimates of the impact of Chinese imports competition along the local hourly wage distribution inside and outside of the manufacturing sector and different measure of lower- and upper-tail inequality. This sheds light on an important aspect of globalization, the rise in North-South trade (deficit), and its implications in terms of inequality for a high-income country with a highly regulated labor market such as France,[2] notably by relating and contrasting its impact in terms of occupational and wage polarization.

I find substantial effect of direct competition on the manufacturing sector. A $1000 increase in imports exposure per worker causes the manufacturing employment growth to decrease by 6.5 percentage points. Regarding spill-overs to the non-traded sector, I find strong effects on employment, hours worked and labor earnings. A $1000 increase in the imports exposure index is associated with a decline by 4 percentage points of local employment growth rate. Considering that the estimated coefficients capture *absolute changes* and not simply deviations from the aggregate trend, the estimates suggest that, over the period 2001-2007, Chinese imports have led to the destruction of $88,000$ and $190,000$ jobs in the manufacturing and non-traded sector respectively.[3] In both sectors, jobs destructions are concentrated in the low and middle skill occupations. The strongest effect occurs in middling jobs in manufacturing, with a clear polarizing effect of imports competition on the occupational structure. In contrast, in the the non-traded sector, the magnitude of the impact declines monotonically as one considers increasingly skill-intensive occupations.

[2]See e.g. Avouyi-Dovi, Fougere, and Gautier (2013) for more information on French labor market institutions regarding collective bargaining and the minimum wage in particular. Overall, employee protection legislation (EPL) is high in France, particularly for permanent contract. The labor market is characterized by a strong duality following the promotion of temporary working contracts (see e.g.Bentolila et al. (2010).). The minimum wage is strongly binding with a ratio of minimum to median wage equal to 56% in 2000 and 61% in 2007, above the OECD median (source: OECD dataset on minimum relative to average wages of full-time workers).

[3]The assumptions required to formulate aggregate statements are discussed in more details in Section 1.4.

Looking at the impact along the distribution of wages, I find contrasting effects across sectors. Wages are rather uniformly negatively affected in the manufacturing sector. Accordingly there is no increase in wage inequality within the manufacturing sector which is somewhat at odds with what one would have predicted based on the polarizing impact of trade shocks on the occupational structure. In contrast, in the non-tradable sector, hourly wages are affected only in the lower-middle part of the distribution. While no overall impact on the log ratio of the 85 to 15 percentile is found, this absence of impact reflects a decrease in lower-tail inequality (log ratio of the 50 to 15 percentile) and a rise in upper-tail inequality (log ratio of the 85 to 50 percentile). China-induced trade shocks therefore trigger a process of wage polarization within the non-traded sector. The wage polarizing effect of rising Chinese imports competition can be rationalized by the strongly binding minimum wage legislation. While, in the absence of exogenous source of variation in local minimum wage legislation, it is delicate to explicitly test this hypothesis, I provide further evidence backing it by exploiting variation in the degree to which the minimum wage binds at the local level and showing that imports competition caused wage polarization in the non-traded sector only in areas where the minimum wage binds.

This paper belongs to a recent but growing literature using local labor markets as units of observations in order to analyze the impact of change in exposure to imports competition. Typically, within-country cross-sectional variation in changes to trade exposure is obtained by using the fact that local labor markets within a same country differ in their initial sectoral composition and are thus not equally exposed to nation-wide sectoral changes in trade exposure (more detailed are provided in Section 1.3). A first strand of this literature investigates the impact of changes in trade policy, mainly tariffs. Given absence of major changes in tariffs among developed countries, this literature is mainly focused on developing countries (Goldberg and Pavcnik, 2007). Topalova (2010) analyses the impact of India's trade liberalization during the 1990s on poverty in Indian regions. Kovak (2013) frames his analysis in a classical Ricardo-Viner model of capital specific sector and estimates the impact of trade liberalization in the early 1990s on regional (residual) wages in Brazil. My paper shares a similar empirical approach in that it uses local sectoral specialization interacted with nation-wide sectoral shocks. However I study the impact of Chinese imports competition rather than changes in trade policy and allow the wage effects to vary across sectors (traded versus non-traded sector) and along the distribution. Moreover I investigate non-wage outcomes, such as employment and employment-earnings.

Starting with Autor, Dorn, and Hanson (2013), a second strand, to which this paper is the most closely related, estimates the impact of Chinese competition onto local labor markets. Working with US commuting-zone data, Autor, Dorn, and Hanson (2013) interact initial local industrial composition with contemporaneous changes nation-wide sectoral imports to compute an index of exposure to imports competition that captures the value of imports-per-worker faced by each local labor markets. In order to isolate the variation in Chinese exports to the US that is driven by supply factors in China, as opposed to US domestic supply or demand shocks, they use Chinese exports to other high-income countries as an instrument for actual Chinese exports to the USA. Here, I follow the same empirical strategy and carry out a more detailed analysis in terms of the local transmission of trade shocks outside of manufacturing in terms of sector and prevailing wages. Moreover, given the richness of the administrative dataset used, I can look at effects on hourly rather than weekly wages, thus canceling out variation in labor earnings related to working time and focusing on unit factor price. I am also able to see how such effects vary along the distribution, for each sector, rather than looking at average wages. Dauth, Findeisen, and Suedekum (2014) look at the impact of Eastern Europe and China trade on German labor markets. They do not find evidence of strong employment effect of Chinese imports competition either inside or outside manufacturing. These findings must interpreted with the specific context of German-China trade which tends to be much more balanced than the US-China trade. France falls closer to the American case in that it has run a large overall trade deficit and in particular with respect to China ($-billion 26 in 2007, i.e. 4.5 percent of France's overall trade, see Figure 1.1). Lasting trade deficits are likely to be associated with stronger labor market effects as workers and resources, in the absence of a binding balanced-trade condition, need not flow from a subset of the traded sector to another to compensate rising imports.

There has been vibrant debate, both within and outside of academic economics, regarding the impact of globalization on the structure of wages and inequality, however the difficulty to obtain local measure of wage inequality has impeded the application of "local labor market approach" to this issue.[4] Instead most papers on the topic focus on the impact of globalization on job-polarization, that is the disproportional growth of employment in occupations traditionally located at the bottom and top of the wage distribution (see e.g. Autor and Dorn (2013) and Autor, Dorn, and Hanson (2015)). Alternatively, they examine variation in the college wage premium (e.g. Lindley and Machin, 2014). The exhaustive nature of the data as well as information on hours worked is key here in order to obtain reliable statistics for different quantiles of the hourly wage distribution for each employment area considered. Moreover, examining outcomes at the local labor market level (rather than industry- or firm-level analysis) offers the crucial advantage of studying the impact of trade-shocks beyond the manufacturing sector, which, in high-income economies, accounts for a limited percentage of

[4] For instance Harrison, McLaren, and McMillan (2011), in their review of recent theoretical and empirical works on trade and within-country inequality, do not cite any papers looking at the impact of globalization on the distribution of wages using the empirical approach used here.

overall employment and labor income.

There is a large theoretical literature linking trade and wage inequality. The classic Heckscher-Ohlin framework, considering two factors, high-skill and low-skill, posits that trade should increase the wage gap between skill and non-skill labor in the skill-intensive country.[5] More recent analysis based on less stylized models, featuring for instance labor market frictions (Davidson, Martin, and Matusz, 1999), firm-heterogeneity and bargaining (Helpman, Itskhoki, and Redding, 2010) or fair wage considerations (Egger and Kreickemeier, 2009) lead as well to the conclusion that opening to trade increases wage inequality. It must be noted that a mechanism through which trade increases income inequality in models of international trade with labor market frictions is through an increase in unemployment (e.g. Egger and Kreickemeier (2009) and Helpman, Itskhoki, and Redding (2010)). However given the nature of the data available, I focus on the price of employed factors, defined here as hourly wage, and do not consider how unemployment risk or difficultly in working full-time is affected by trade in computing the wage distribution. While this can be an important caveat when evaluating the impact on workers welfare,[6] it remains important to see to which extent the structure of wages is affected by imports competition and how this spill-overs onto the service sector. A motivation to resort to analysis along the wage distribution is based on recent models of international trade which suggests that trade increases inequality in ways that is not captured by the observable skill-premium. For instance, Amiti and Davis (2012) develop a model in which an increase in foreign imports competition drives wages up in input-importing and output-exporting firms and reduce wages in firms serving only the domestic market. In case the correlation between firms' share of high-skill employees and importer or exporter-status is not perfect, such an increase in inequality would not be captured by using a measure of skill-premium as dependent variable. Looking at the whole distribution of wages allows to capture such an effect while remaining agnostic about the specific mechanism driving the change in the wage distribution.

The French case is particularly interesting because unlike other industrialized countries, its wage distribution has become more compressed over the past few decades (Verdugo, 2014).[7] It is therefore relevant to see whether this compression occurred despite a possibly inequalizing effect of trade or whether trade per se did not lead to a rise in wage dispersion.

[5]The empirical predictions of the basic HOS model has however been largely discredited by the simultaneous rises in wage inequality in both low-skill and high-skill intensive countries Harrison, McLaren, and McMillan (2011).

[6]For instance, the model featuring costly labor mobility between sectors by Artuc, Chaudhuri, and McLaren (2010), trade liberalization can trigger a decline in the wages of import competing industries and nevertheless lead to a rise in lifetime income due to the possible reallocation of the workforce towards exports-oriented industries.

[7]See for instance Dustmann, Ludsteck, and Schönberg (2009) for Germany and Goos and Manning (2007) for the United Kingdom.

Results in the non-traded sector shows that trade shocks led a decline in bottom tail inequality, a rise in upper tail inequality leaving the overall level of wage inequality stable. Using geographical variation in the bite of the minimum wage, I show that the decrease in bottom tail inequality only occurred in places where a sizable share of jobs are minimum wage jobs and that, on the contrary, the rise in upper tail inequality is constant across regions with different bite of the minimum wage. Overall, these results suggest that trade contributed to raise wage inequality but that this effect was overall offset by the countervailing effect of the minimum wage, leaving overall wage inequality stable but still modifying (polarizing) the shape of the wage distribution. This exercise provides an illustration of the importance of interactions between shocks and institutions in explaining changes in the structure of wages.

The rest of paper is structured as follow. In Section 4.2, I briefly present the data used in this study. Section 1.3 presents the empirical strategy adopted to identify and estimate the impact of rising Chinese imports competition on a wide array of labor market outcomes. In Section 1.4, I present and discuss the lain results regarding employment inside and beyond manufacturing (Subsection 1.4.1). This section also presents also robustness checks (Subsection 1.4.2) as well as extensions (Subsection 1.4.3), notably regarding the effect of imports competition on local job polarization. The results focusing on the local wage distribution are presented in Section 1.5. The conclusion follows.

1.2 Data

Data for this analysis originates from several sources. Data on employment and wage distribution are drawn from a matched employer-employee dataset, called DADS postes (Déclaration annuelle des données sociales).[8] It contains exhaustive data on non-agricultural salaried job-spells in France. I focus mostly on the competitive sector and do not include workers employed by fully public institutions. Statistics are computed at the "employment zone" level.("Zone d'emploi" in French.) Like the US "commuting area" used in Autor, Dorn, and Hanson (2013), employment zones' definition is based on a criterion of self-contained commuting which limits the acuity of issues usually associated with spatial contagion across administratively defined units. There are 348 such units according to their 1990 definition.

I document the sector to which a job is associated by using a 4-digit NACE (rev.1) code reported by the plant (establishment) where the job is located. This NACE code is itself determined based on what the main activity of the plant is (not that of the firm). Based on this information, I can distinguish between employment in manufacturing and in the

[8] Note that this dataset is exhaustive but only allows to follow workers for two years, unlike the Panel DADS which is a 1/24th sample but has a very long panel dimension.

non-traded sector. More importantly I can construct a very accurate index of exposure to Chine products competition (see next section). Regarding French and China's trade, I use UN Comtrade data on from 1995 to 2007. The Harmonized System (HS) nomenclature of year 1992 is mapped into 4-digit NACE sector codes using conversion tables available on Eurostat's website RAMON. More details are provided in Appendix 1.B. I restrict the sample to jobs occupied by workers aged between 16 and 64, with strictly positive earnings and hours worked.[9] I aggregate data at the non-traded versus traded level for each area-year (1995, 2001 and 2007) and take the first difference of the data and obtain a final dataset of 348 areas observed over two 6-year periods.

Some data regarding private employment during the 1980s (1982 to 1990) and as well as the share of college graduates are taken from public available census data, for the waves 1982, 1990 and 1999. The Census data for 1990 and 1999 are matched to 1995 and 2001, respectively. We compute pre-trend in employment growth between 1982 and 1990 (matched with period 1995-2001) and 1990-1999 (matched with period 2001-2007).

Summary statistics are presented in Table 1.1. The average size (measured by non-auxiliary jobs) of an employment zone was 180,000 in 1995, with a median about half the mean, suggesting some skewness. With almost 2 millions worker, the Paris employment area is a clear outlier in terms of size. All results presented below are robust to the exclusion of this area. Employment in manufacturing declined during both periods, although much more markedly between 2001 and 2007 than between 1995 and 2001. Hours worked per job are on average much higher in the manufacturing sector than in the non-traded sector (1600 versus 1290 in 1995). Hours worked might thus provide a more comparable and relevant measure than job count when comparing impact of Chinese competition across sectors. The variables ΔIPW and ΔDPW stand for changes in, respectively, imports per worker (IPW) and trade deficit per worker (DPW). The precise definition of these variables is given in section 1.3. For now, it is sufficient to see that the two variables are very close, owing to the fact that the rise in French-Chinese trade is mainly driven by France's rising purchases of Chinese products. Moreover, it appears that there has been an acceleration in the pace of imports/trade deficit growth between the two periods.

[9] I only retain jobs if : (a) earnings are more than 3 times the monthly minimum wage or (b) the length of employment is more than 30 days and more than 120 hours and the ratio hours to days is higher than 1.5. This definition matches the definition of the a "non-annex" job implemented by the French National Statistical Institute and that is used in most statistics about employment in France.

1.3 Empirical Strategy: Measurement and identification

In this section I describe the empirical strategy adopted to estimate the direct impact of Chinese imports competition. To measure the local exposure to Chinese imports, I build on Autor, Dorn, and Hanson (2013) and compute a index of imports exposure, called "Imports-per-Worker". This index interacts initial local industrial composition of the manufacturing sector with contemporaneous nation-wide Chinese imports by sector.

Formally, the index ΔIPW is defined according to the following formula:

$$\Delta IPW_{it} = \frac{1}{L_{it}} \sum_{s \in \mathbb{T}} \frac{L_{ist}}{L_{st}} \Delta M_{st} \tag{1.1}$$

where ΔM_{st} stands for the changes in Chinese exports to France between periods t and $t+1$ for sector s, L_{st} is equal sector s employment in France for at time t. $\mathbb{T}$ refers to the set of sectors in the economy that part of manufacturing (We use $\mathbb{N}$ to refer to the set of non-traded sectors.). L_{it} is total employment in area/period i, t while L_{ist} is employment in area i in sector s at time t.[10]

To estimate the impact of Chinese imports penetration on some local labor market outcomes Y (e.g. employment in manufacturing etc.), I use the following baseline specification:[11]

$$\Delta \log Y_{it} = \Delta IPW_{it}\beta + X_{it}'\delta + \eta_t + \varepsilon_{it} \tag{1.2}$$

To fix ideas, consider the case where $\Delta \log Y_{it}$ is the growth rate of employment in the manufacturing sector. There are many plausible reasons why in specification (2.3), ΔIPW might be correlated with the error term ε even after controlling for an extensive set of covariates (discussed more in depth in the next section). Nation-wide sector specific shocks (supply or demand) are partly driving the amount of goods imported in France from abroad. If these shocks affect simultaneously sectoral imports and labor demand, OLS estimates will be biased. To formalize this idea, let us consider the case where the error term ε_{it} can be decomposed between **(i)** a weighted sum of nation-wide sectoral supply and demand shocks (which we denote w_s and x_s respectively) and **(ii)** an error term uncorrelated with any other terms included in the regression. For simplicity we omit the time subscript:

[10] This variable is thus closely related to the widely used Bartik-instrument Bartik (1991) in that it interacts initial sectoral composition and contemporaneous sector-wide trends. Note however, that I use ΔIPW_{it} as a *causal* variable and not as an instrument. In fact, as we will see, there are many outcomes that are affected by ΔIPW_{it} which could raise concerns about the plausibility of the exclusion-restriction when using the Bartik-instrument to instrument, for instance, for changes in local employment or unemployment.

[11] This estimating equation is equivalent of Autor, Dorn, and Hanson (2013). The contribution of the paper lies mainly in exploring different outcomes than those analyzed by them.

$$\varepsilon_i = a_S \sum_s \lambda_{is} w_s + a_D \sum_s \lambda_{is} x_s + \epsilon_i$$

where the parameter a_S and a_D determines the sign and magnitude of the impact of supply and demand shocks, respectively, on manufacturing employment growth and λ_{is} is an unobserved term representing the "importance" of sector s in location i (a simplification could be to set it equal to $\frac{L_{ist}}{L_{it}}$, i.e. the initial employment share of sector s in location i). Collecting sectoral shocks w_s and x_s respectively in vectors $\mathbf{w}$ and $\mathbf{x}$, noting $\mathbf{m}$ the vector containing the changes in imports to initial employment ratios (with typical element $m_s = \frac{\Delta M_s}{L_s}$) and $\theta_{is} = \frac{L_{ist}}{L_{it}}$ the share of sector s in total employment in location i, and omitting exogenous regressors for simplicity, we can rewrite equation (2.3) as:

$$\Delta \log Y_i = \beta \theta_i' \mathbf{m} + \lambda_i'(a_S \mathbf{w} + a_D \mathbf{x}) + \epsilon_i \tag{1.3}$$

This specification is reminiscent of panel model with interactive fixed-effect (Bai, 2009) in the sense that the unobserved heterogeneity term λ_i is multidimensional (the length of vector λ_i is here equal to the number of sectors in the economy) and is allowed to interact with shocks that are common through the rest of the cross-sectional units.

Hence OLS estimation of the main specification will be biased due the covariance between ΔIPW_i and $\lambda_i'(a_S \mathbf{w} + a_D \mathbf{x})$ which we can write as:[12]

$$\mathrm{cov}(\theta_i' \mathbf{m}, \lambda_i'(a_S \mathbf{w} + a_D \mathbf{x})) = a_S \theta_i' \mathrm{cov}(\mathbf{m}, \mathbf{w}) \lambda_i + a_D \theta_i' \mathrm{cov}(\mathbf{m}, \mathbf{x}) \lambda_i$$

If we assume that $s \neq s' \Rightarrow \mathrm{cov}(m_s, w_{s'}) = \mathrm{cov}(x_s, w_{s'}) = 0$ which amounts to ignoring cross-sectors relationships (driven for instance by input-output linkages or substitution in consumption between goods), we get the following expression:

$$\mathrm{cov}(\theta_i' \mathbf{m}, \lambda_i'(a_S \mathbf{w} + a_D \mathbf{x})) = a_S \sum_s \theta_{is} \lambda_{is} \mathrm{cov}(m_s, w_s) + a_D \sum_s \theta_{is} \lambda_{is} \mathrm{cov}(m_s, x_s) \tag{1.4}$$

We expect the covariance between nationwide unobserved sectoral supply shocks and imports-per-worker ($\mathrm{cov}(m_s, w_s)$) to be negative. When French producers in sector s are subject to a negative supply shock ($w_s < 0$ e.g. mandatory nation-wide reduction in weekly working-time with no reduction in monthly wages), one would expect an increase in purchase in goods s from foreign suppliers, including China and other low-wage countries. That suggests $\mathrm{cov}(m_s, w_s) < 0$. On the other hand, as x_s represents demand shocks, one would expect that $\mathrm{cov}(m_s, x_s) > 0$.

[12] In the derivation of this expression I consider θ_i and λ_i as fixed parameter vectors and $\mathbf{m}$ and $\mathbf{w}$ as random vectors.

Positive supply and demand shocks are expected under general conditions to increase employment, hence we can assume $a_S > 0$ and $a_D > 0$. According to this framework, the bias introduced by unobserved sectoral shocks could either be positive or negative depending on the relative magnitude of supply and demand shocks and how they affect imports from low-wage country. Because these nation-wide shocks affect each community differently, through to the vector λ_i, including periods fixed-effects does not solve the issue.

We adapt the instrumental variable strategy developed by Autor, Dorn, and Hanson (2013) to the French case. We instrument actual exports from low-wage countries to France by Chinese exports to a set of high-income countries whose economic cycle is weakly related to that France.[13] The formula for the instrument is the following:

$$\Delta IPW_{it}^{o} = \frac{1}{L_{it}} \sum_{s \in \mathbb{T}} \frac{L_{ist}}{L_{st}} \Delta M_{st}^{o} \tag{1.5}$$

where ΔM_{st}^{o} is Chinese exports to the set of selected other high-income countries. The identifying assumption underpinning the validity of this instrument is that Chinese exports to these countries, i.e. the vector ΔM^{o}, are independent from domestic shocks in France (contained in vectors $\mathbf{w}$ and $\mathbf{x}$ in our example) so that the statistical association between French imports from China and Chinese exports to these high-income countries is only driven by supply-side improvements in China. This assumption seems credible given that China underwent major economic reforms since the 1980s which accelerated over the 1990s, culminating with China's accession to the World Trade Organization in 2001. These reforms were deeply influenced by China's own domestic politics and decided independently from development pertaining specifically to France.[14] The choice of other high-income countries is such that they represent a small percentage of French exports so that the exclusion restriction appears credible.

In the robustness checks section (Section 1.4.2), I show results using an alternative approach, used by Partridge et al. (2013), that attempts to control for the weighted average of sectoral shocks by using a proxy following the same "weighted average structure", known as the Bartik-shock.[15]

Figure 1.2 contain three maps of France regarding IPW and employment growth over

[13] The list of countries is almost the same as in Dauth, Findeisen, and Suedekum (2014), except that it excludes the United Kingdom and includes Denmark, South-Korea, Argentina and Chile. The list includes the following countries: Argentina, Australia, Canada, Chile, Denmark, Japan, New Zealand, Norway, Singapore, Sweden, and South Korea. Note that we excluded all countries from continental Europe which are part of the euro zone.

[14] For an extensive account of Chinese reforms, subsequent growth and increasing economic openness see Brandt and Rawski (2008).

[15] As will be argued later, this approach provides a lower (in asbolute value) bound on the true impact under different identifying assumption than the IV approach and in that sense complements it.

the 2001-2007 period. Table 1.1 shows that decline in manufacturing has taken place over the 2 periods but was much stronger over the second one. Imports and Trade deficit per worker grew faster over the second period as well. Figure 1.3 displays the first-stage, plotting observations from both periods (after taking out period fixed-effect) as well as a best linear fit and its 95 % confidence interval. The instrument is a strong predictor of the endogenous regressor ΔIPW_{it}. The first-stage Kleibergen-Paap F-statistic for the stack specification with no-covariate is 41 way above the critical value of 16 suggested by Stock and Yogo (2005) for 2SLS estimates. The corresponding reduced-forms with respect to manufacturing and non-traded employment are plotted in Figure 1.4.[16]

The period we cover stretches from 1995 to 2007. The choice of the beginning date is driven by data availability regarding local sectoral composition, wages and hours worked. The end of the period is marked by the onset of the Great Recession which deeply affected trade flows as well as GDP in ways that is very likely to be correlated across high-income countries, thus making the identifying assumption less credible over the post-2007 period.[17]

1.4 Results on employment

1.4.1 Employment and Total Earnings

Manufacturing sector

The first specification measures the impact of IPW on employment and total hours worked in the manufacturing sector. Results are displayed in Table 1.2. All specifications include the initial value of overall employment in the employment area.[18] Column (1) implies that a \$1000 increase in ΔIPW is associated with a 6.4 percentage point decrease in manufacturing employment growth rate. We see in Column (2) that instrumenting ΔIPW by ΔIPW^o yields a higher (in absolute value) coefficient of -10. Considering the formula for OLS bias in Equation (1.4), it suggests that, under the maintained hypothesis that the instrument is exogenous, France's increasing imports of Chinese products are for a substantial part explained by French idiosyncratic demand shocks that boost both employment and imports thus causing an upward bias in OLS estimates. This result is similar to what is found for the US case for by Autor, Dorn, and Hanson (2013). In Column (3), we control for the initial share of employment in the manufacturing sector. Under this specification, variation across employment areas comes only from differences in the local specialization within the

[16] Figures OA1 and OA2 in the online appendix display the first stage and the reduced-form in long-differences.

[17] Figure 1.1 shows the decline in imports from China occurring in 2008/2009.

[18] This control accounts for the fact there tends to be a negative relationship between city population and its growth rate, as pointed in Card (2007) and Faggio and Overman (2014).

manufacturing sector and not from differences in terms of share of the manufacturing sector as a wole. It uses the fact that Chinese imports growth has been very uneven across subsets of the manufacturing sector. The inclusion of that term does not affect substantially the estimate, which is equal to -8.[19]

Column (4) and Column (5) test the robustness of the results, relaxing further the identifying assumptions required to interpret the coefficient causally. Column (4) adds a series of additional controls that discards some potential confounding factors. It controls for the initial share of college-educated residents, of women, foreigners, and production workers in the workforce. Including these controls allows each employment area to have a specific trend proportional to each of these initial shares. The rational to include production workers is to account for the potential exposure of employment areas to technical change and automatization that could possibly lead to a decline in labor demand by the manufacturing sector independently from globalization (Autor and Dorn, 2013). The share of college graduates accounts for the fact that there has been an increasing gap in employment and unemployment rates between college and non-college educated individuals. The estimate decrease marginally in magnitude from Column (3) to Column (4) with an absolute value of 7.1. In Column (5) includes "region"-fixed effects[20], thus allowing each region to have a specific trend. Again, we do not see a large decline in the estimate which, at a value of 6.4, remains significant at the 1 percent level.

I now put these results into perspective and gauge their economic significance. It is important to note that the first-difference approach adopted in this paper identifies the *relative impact* of *changes* in trade exposure. In that sense it identifies how local labor markets exposed to Chinese competition deviate from the aggregate trend. In our setting, making aggregate predictions necessitates additional assumptions such that the coefficient associated with ΔIPW reflect *absolute* changes in outcomes and not simply changes *relative* to the aggregate trend. In fact, aggregate prediction requires period fixed-effects not to be a function of the regressor aggregated across employment zones.[21] This assumption is not necessary in order to obtain consistent estimates of imports competition impact on local

[19] Unlike Autor, Dorn, and Hanson (2013), I do not consider employment over population but growth rate in employment (and other outcomes). This choice is mainly dictated by data availability. The years considered (1995, 2001, 2007) do not correspond to a census year. Hence there is no data on overall working age population. Overall adult (above 18) population, as opposed to working age population (which would typically include the 16 to 65 year olds) can be approximated by the number of registered voters for the presidential elections of 1995, 2001 and 2007. In a set of unreported regression, I show that adult population proxied by registered voters do not respond to China-induced trade shocks. The same results hold when using the number of fiscal households which, unlike voters, includes foreigners. Results are available upon request. In the absence of reaction of population adjustment, trade shocks' impact on the change of log employment translates roughly one-to-one to the change in the log ratio of employment to adult population.

[20] There are 22 so called "Regions" in metropolitan France.

[21] This point is also mentioned in Topalova (2010). See Appendix 1.C for a more formal argument.

labor markets but I adopt it in this paragraph in order to, somewhat heuristically, get an order of magnitude of the estimated effect if it was to reflect absolute changes.

Between 1995 and 2007, employment in the manufacturing sector declined by 14.4 percent. It declined by 1.7 percent over the last period (1995-2001) and declined by 13.3 percent over the second period (2001-2007). I predict import-driven changes in growth rate in manufacturing by using our 2SLS estimates times the observed change in import-per-worker at the local level times the share of variation in imports explained by Chinese supply-side factors. I deduce this share from the R-square of a simple bivariate regression of ΔIPW_{it} on ΔIPW^o_{it} (after partialling out all covariates included in Column (5)).[22]This simple computation allows to retrieve the nation-wide effect of Chinese imports penetration on manufacturing employment. I find a nation-wide effect of -0.31 and -1.77 percentage-points for the first and second periods respectively. Hence, under the stated assumptions, we find that Chinese imports are responsible for about 13 percent of the decline in the French manufacturing sector employment over the period 2001-2007. In terms of jobs, that implies that imports Chinese competition destroyed $16,000$jobs over the first period and $88,000$jobs over the second. This exercise is therefore consistent with the notions that (i) trade with China have become increasingly relevant for industrial employment in developed economies, (ii) other factors, such as technological change, lie behind the rapid decline of industrial employment in France.

Columns (6) and (7) apply the same specification as in Column (5) changing only the dependent variable. Columns (6) shows the impact on total hours worked. The estimate is very close to that in Column (5) suggesting no change in average hours worked per job. This is interesting in the light of the widespread notion that imports competition has promoted part-time and unconventional forms of employment. Column (7) looks at overall employment earnings (hours worked times hourly wage). The impact is sensibly larger than that on hours worked suggesting a mild decline in the average hourly wage. This result contrasts with those of Autor, Dorn, and Hanson (2013) who find no effect on average weekly wage within manufacturing. Although downward wage rigidity (nominal or real) is a salient feature of the manufacturing sector, it is not incompatible with this finding. Indeed the present results reflect the effect on wage growth in deviation from aggregate trends and could be explained by low wage for the new hires or simply a lower albeit positive wage growth rate. (The local wage impact of China-induced trade shocks will be analyzed in more detailed in Section 1.5.)

[22]The predicted change for employment zone i at time t is thus equal to: $g_{it} = R^2 \times \beta \times \Delta IPW_{it}$ where R^2 refers to the partial first-stage R-square. The aggregate predicted change is then simply: $\overline{g}_t = R^2 \times \beta \times \overline{\Delta IPW}_t$, where $\overline{x}_t$ means the average of variable x across the cross-section at time t.

Outside of manufacturing

I now turn to the effect of Chinese imports penetration employment and hours worked in the non-traded sector as a dependant variable. The results are displayed in Table 1.3. The effect is weaker and less precisely estimated. The weaker effect found on the non-traded sector can be explained by the fact that there is a decline in demand for non-traded output, employers in this sector benefit from a positive local labor supply shocks as a share of workers displaced from the manufacturing sector are likely to be looking for jobs locally. Column (5) implies that a \$1000 increase in ΔIPW is associated with a 4 percentage point decrease in non-tradable sector employment growth rate.[23] We notice that unlike in the case of manufacturing, the impact on hours worked (Column 6) is much weaker (about half as large) than that on jobs (Column 5). This could be explained by the fact that mostly part-time jobs are destroyed. Again the impact on overall earnings is slightly more negative than that on hours suggesting a small decline in average hourly wage, an issue we directly tackle in Section 1.5.

Under the same assumptions as in the previous paragraph, I can get an order of magnitude regarding the aggregate impact of Chinese imports competition on job displacement in the non-traded sector. Using the formula explained in footnote 22 leads to the conclusion that local growth in non-traded employment was reduced by 0.18% over the first period and 1% over the second. This suggests that $190,000$jobs in the non-traded sector were destroyed due to spill-overs associated with China-induced trade shocks between 2001 and 2007. This number is much larger than what is found for the manufacturing sector. While this might seem surprising at first, it is worth stressing that this discrepancy reflects to a great extent the large relative size of the non-traded sector with respect to the manufacturing sector in the economy. We note moreover that the overall trade impact on non-traded sector employment represents only 4 % of overall employment growth in non-traded employment during the entire period. This figure is about 15 % in the manufacturing sector. Therefore the *relative* size of the effect of local exposure to imports competition is much larger in the manufacturing sector.[24] Furthermore, doing the same computation based on hours worked rather than job count reverses the conclusion regarding the *absolute* size of the effect: more "hours worked" have been destroyed by Chinese imports competition in manufacturing than in the non-tradable sector (130 versus 108 millions hours).

The impact of Chinese imports competition onto the non-tradable sector is indirect in that it operates through its impact on the manufacturing sector. Moretti (2010) estimates

[23] Normalizing variables, estimate from Column (5) suggests that a one standard deviation increase in ΔIPW cause a decline in non-traded employment of 0.23 standard deviation.

[24] We also note that temporary workers employed in manufacturing firm could also contribute to explain these results. The data does not allow me to investigate in depth this issue. However available evidence presented online appendix 1.E.3 strongly suggest that they do not drive these results.

the impact of growth in tradable employment on the growth of the non-tradable sector, using the Bartik-shock variable as an instrument for growth in tradable employment. He finds a job-to-job impact of 1.57. Dijk (2014) reexamines Moretti's work and after several corrections concluded to a local multiplier effect roughly 33 % lower (1.02 job-to-job effect). The ratio of the estimates from tables 1.3 and 1.2 is similar to a "local multiplier effect" in the sense that it defines the elasticity of non-tradable employment to employment in the manufacturing sector.[25] Taking estimates of Column 3 from both tables leads to a ratio of 0.58, which given an average ratio of non-traded to manufacturing employment of 2.5 suggest a job-to-job effect of 1.46. This suggests that shocks to local labor demand induced by trade-shocks trigger spill-overs of roughly the same magnitude as those captured by the Bartik-instrument, which encapsulates sectoral shocks of all sources (trade shocks but also domestic demand shock, technological shocks etc.) by using directly nation-wide changes in sectoral employment.[26] While this impact might seem very large at first, it is worth noting that, in the present context, it much more modest when expressed in terms of hours worked – an arguably more informative statistics than job count regarding the state of the local labor market. We see indeed that the non-traded to traded elasticity obtained by taking the ratio of the two coefficients of Columns (6) in Tables 1.2 and 1.3 is 0.29. This is twice smaller than the job-elasticity. This large discrepancy shows that when discussing "local multiplier", conclusions can be very sensitive to the unit of measurement used. Manufacturing to non-tradable job-to-job effect might tend to overestimate the actual increase in labor demand occurring in the non-traded sector following the local creation of a job in the local manufacturing sector.

1.4.2 Robustness Checks

Placebo regressions

In spite of the extensive set of controls, as well as the instrumentation of ΔIPW, there remains the suspicion that results displayed so far could be picking up a secular decline of employment in some local labor markets. I test this possibility by regressing employment

[25] To see this, recall that the coefficients on ΔIPW of Table 1.3 and 1.2 are semi-elasticities of, respectively, non-traded and traded employment with respect to ΔIPW: $\beta_{Non-Traded} = \frac{\partial \Delta \ln(L_{Non-Traded})}{\partial \Delta IPW}$ and $\beta_{Traded} = \frac{\partial \Delta \ln(L_{Traded})}{\partial \Delta IPW}$ where L_s refers to employment in sector s. Therefore the ratio yields an elasticity $\frac{\beta_{Non-Traded}}{\beta_{Traded}} = \frac{\partial \Delta \ln(L_{Non-Traded})}{\partial \Delta \ln(L_{Traded})}$.

[26] A set of regressions where I estimate directly local multiplier using either ΔIPW and *Bartik* as an instrument for growth in non-traded employment. I find statistically indistinguishable estimates. Results are shown in the online appendix (Table OA2). I discuss the relationship between IV estimates and those obtained by Partridge et al. (2013) in Section 1.4.2.

growth on lead values of ΔIPW. If future values of ΔIPW predicts current low job growth, it could could imply that estimates presented until now are picking up the impact of an omitted factor correlated with rising imports competition (for instance labor-saving technological change). I use data from the French Census for periods 1982-1990 and 1990-1999 an compute overall private employment growth and resort to the administrative data, used in the rest of the analysis, for the 2001-2007. Table 1.4 displays the results. In columns (1) and (2), employment growth in the private sector (reexpressed in 6 year period equivalent growth rate) for periods 1990-1999 and 2001-2007 is regressed on ΔIPW for periods 1995-2001 and 2001-2007. In columns (5) and (6), employment growth in the private sector (reexpressed in 6 year period equivalent) for periods 1982-1990 and 1990-1999 is regressed on ΔIPW for periods 1995-2001 and 2001-2007. Focusing on the IV regression results, we can see that while ΔIPW is associated with a decline in contemporaneous employment growth (Column 2), it is associated with higher growth rate in lagged employment, suggesting that the negative coefficients estimated above reflects a causal impact of Chinese imports competition on job growth rather than a secular decline. On the contrary, employment areas with high lead values of ΔIPW were experiencing above average growth in employment during the 1980s and 1990s which suggests that if anything the present estimates are a lower bound on the true effect. Another approach to rule out spurious effect is to include pre-trends as an explanatory variables (rather than dependent) and assess to which extent the coefficients associated with ΔIPW change. In Column (3) and (4), we see that, reassuringly, the coefficients are not substantially affected suggesting pretends are not driving the size of the estimated coefficient. Consistent with the positive coefficient found in Column (6), we find that controlling for pre-trends lead to a stronger estimated impact of Chinese imports competitions, suggesting that estimates presented in section 1.4.1 are most likely a lower-bound for the true effect.

The role of other "emerging" countries

China's exports to France have been particularly fast growing, both in percentage and absolute terms, over the period considered. Moreover, as argued in the introduction, there is a sense that this dramatic increase is largely driven by processes exogenous to the French economy. These two features suggest that the potential impact of Chinese imports competition on French local labor market is both large and arguably easier to estimate. Other low-cost countries however have been increasing their exports towards the French market as well, notably other developing Asian countries and Eastern European countries. Ideally, one would want to control for the rising exposure to the imports competition of these other countries when estimating the impact of China's trade. This runs however into the difficulty of finding a suitable instrument for the import competition of each set of countries. While using exports to OIHCs seems justified in the case of China due to the magnitude and fast pace of its arguably exogenous economic reforms, it is more difficult to justify this approach in the case, for instance, of Eastern Europe whose economies are very much influenced by decisions and shocks occurring in Western European countries.

One way to deal with this "omitted trade" while circumventing the issue of endogeneity is to include the amount imported from these other countries into the computation of the index ΔIPW and resort to the same IV strategy – using Chinese exports to other high income countries as IV for actual imports from France from new set of considered countries. One can then see to which extent the estimates vary.[27] Table A1 displays the results for three sets of countries.

Column (1) repeats the results of Table 1.2. ΔIPW is computed adding to Chinese imports, imports from other developing Asian countries (Column 2), Eastern European countries (Column 3) and the rest of the world (Column 4). The coefficient is only slightly attenuated when adding imports from the rest of developing Asia as well Eastern Europe: the coefficient decrease by about 20% in either case. The conclusions on the economic magnitude of the effect of China competition remains therefore almost unchanged. The first-stages are strong in both cases. This highlights the fact that exposure to imports competition from China, the rest of developing Asia and Eastern Europe are strongly correlated. However, note that Chinese imports competition has grown 2.7 times and 1.9 times more than that of developing Asia and Eastern Europe respectively, thus suggesting that China has had the single greatest impact. We see that the effect becomes virtually 0 when considering imports from the rest of the world. We note however that the first-stage becomes very weak, thus limiting the interpretability of this column.

Alternative identification strategy: using the Bartik-shock as a proxy for domestic shocks

In Section 1.3, we make explicit the more likely sources of bias of an OLS estimator. We reproduce Equation 1.3 for convenience:

$$\Delta \log Y_i = \beta\theta_i'\mathbf{m} + \lambda_i'(a_S\mathbf{w} + a_D\mathbf{x}) + \epsilon_i$$

The bias is akin to an omitted variable bias where the omitted variable is a weighted average of sectoral shocks. An alternative to the instrumental variable approach we have used until now would be to control for this unobserved weighted average by including an observable proxy. Partridge et al. (2013) adopts this approach by using the "Bartik shock".

[27] This exercise amounts to include exposure to non-China imports competition as a control and constraint coefficients on non-China and China imports competition to be equal.

This variable is closely related to ΔIPW in that it interacts aggregate sector-specific trends with local industrial composition. The Bartik variable interacts initial industrial composition with nation-wide sector specific percentage change in employment. Formally, it is defined as follow:

$$Bartik_i = \sum_{s \in \mathbb{T}} \frac{L_{is}}{L_i} \Delta \log L_s = \theta_i' \mathbf{\Delta} l \tag{1.6}$$

where $\mathbf{\Delta} l$ is a vector containing sector specific nationwide growth rate in employment $\Delta \log L_s$. Its inclusion would partially solve the issue highlighted in equation (1.3) to the extent that local employment shares ($\frac{L_{is}}{L_i}$) are proportional to the "true" weight of a sector in the local economy (λ_i) and national employment trend ($\Delta \log L_{st}$) embodies all demand and supply shocks specific to the sector s (vector x_s and w_s).

I assess to which extent including the Bartik-shock as a control gives similar results to the IV approach. Results for the OLS, IV and Bartik are displayed in Table A2. We only display results for the most demanding specification, corresponding to Column (5) of Table 1.2. The first three columns display results regarding the manufacturing sector. Column (1) displays the OLS without controlling for the Bartik, Column (2) the OLS with the Bartik and Column (3) displays the IV estimate. We see that controlling for the Bartik-instrument attenuates the coefficient on ΔIPW in comparison with the OLS. On the contrary, using the IV approach results in a more negative estimate. The same pattern holds for the non-traded sector.

The attenuation is expected as the Bartik-instrument will necessarily capture some of the effect of ΔIPW and in that sense we see this specification as providing a lower (in absolute terms) on the estimate bound under a different set of identifying assumptions than the IV approach.[28] If one considers imports competition as sectoral nationwide shocks that affects each locality depending on the local weight each sector, controlling for the "expected" growth rate of the local economy based on its industrial composition will absorb part of these

[28] Nationwide trends in sectoral employment are themselves affected by changes in imports competition – in fact, it is reasonable to think of the impact of imports competition on local employment to primarily occur through its impact on sectoral nationwide trends. Therefore, using $Bartik_i$ as a proxy for unobserved shocks in a specification including ΔIPW might result in "over-controlling" and attenuate the estimated impact of imports competition. To see this consider that for each sector of the economy nationwide employment growth is generated by the sum of a Chinese import competition shock (m_s which stands for the change in imports per workers at the sectoral level) and an unobserved possibly correlated shock (d_s). That is:

$$\Delta \log L_s = \alpha_0 m_s + \alpha_1 d_s$$

where $d_s = a_S w_s + a_D x_s$. This specification widespread in sector-level studies of trade (see e.g. Federico, 2014). The Bartik-instrument is given by: $Bartik_i = \sum_{s \in \mathbb{T}} \theta_{is} \log L_s = \theta_i' \mathbf{\Delta} l = \alpha_0 \theta_i' \mathbf{m} + \alpha_1 \theta_i' \mathbf{d} = \alpha_0 \Delta IPW_i + \alpha_1 \theta_i' \mathbf{d}$

We consider the main estimating equation (Equation 1.3) where we add the assumption $\lambda_i = \theta_i$ (i.e. the share of local employment for a given sector is an exact proxy for the weight of a shock of that sector in the sectoral shocks.[29]

1.4.3 Extensions

Net imports

Dauth, Findeisen, and Suedekum (2014) emphasizes that local labor markets can be positively affected by trade. Chinese economic growth is associated with a surge in imports. If some local labor markets are specialized in sectors for which Chinese demand grows particularly fast, Chinese growth could stimulate local labor demand. As shown in the introduction and in Figure 1.1, French-Chinese trade is particularly unbalanced. In 2007 for instance, French exports to China amounted \$-bn 14 while its imports amounted to \$-bn 40, thus leaving a deficit of \$-bn 26 equivalent to 4.5 % of French total trade (exports plus imports). In this section, I check whether considering net imports (or trade deficit) as opposed to overall imports lead to substantially different estimates. I do not test exports and imports separately mainly because I do not dispose of a relevant and valid instrument for French exports to China.[30] Table 1.5 based on the same specifications as in Table 1.2 with the only exception that the variable imports per workers has been replaced by a "deficit-per-worker" variable. Coefficients are very close to those in Table 1.2, although of a somewhat larger magnitude. It is logical as the deficit-per-worker nets out any positive effect associated with rise in exports that would not be captured by the gross imports measure.

These estimates provide some information regarding what would have been the impact in

local area) and substitute for $\theta_i'\mathbf{d}$ based on the definition of $Bartik_i$:

$$\Delta \log Y_i = \beta\theta_i'\mathbf{m} + \theta_i'\mathbf{d} + \varepsilon_i = \left(\beta - \frac{\alpha_0}{\alpha_1}\right)\Delta IPW_i + \frac{1}{\alpha_1}Bartik_i + \epsilon_i$$

Given the assumption we made regarding ε_i, we can consistently estimate $\delta_1 := \beta - \alpha_0/\alpha_1$ and $\delta_2 := 1/\alpha_1$. However we can not identify β as α_0 is not identified. Nevertheless we expect α_0 to be negative: at the sectoral level, imports competition from China reduces employment growth. Therefore, provided that our estimate of $1/\alpha_1$ is positive, we know that our estimate for δ_1 is an upward-biased estimate of β. If the true value of β is negative, δ_1 is attenuated. Naturally over-controlling would occur only if α_0 is non-zero, but there is by now large evidence that sectoral shocks in imports competition impact negatively nationwide employment trends (see e.g. Federico, 2014).

[29] I carried two additional robustness checks that are not included in the body of the paper, but available in an online appendix. First, I use the 2010 rather than 1990 definition of employment zones which changes the number of unit from 348 to 304. An other robustness checks consists of omitting the Parisian region (Île de France) from the sample. In both cases, no substantial result is affected. The results are displayed in the Table OA1 in the Online Appendix.

[30] Imports by China of products made in other high income countries could be a possible instrument. The strength of such instrument is however very low, owing to the fact that Chinese product basket of imported goods vary a lot across provenance-countries, much more so than its export basket (which is quite uniform as attested by the strength of the first-stage).

terms of manufacturing employment of balancing trade with respect to China.[31] Given that the average increase in ΔDPW has been \$790 over last period 2001-2007, given a partial R-square of ΔDPW on ΔIPW^o equal to 13%, in the absence of increase in trade deficit, manufacturing employment would have declined about 1 percentage less, representing about 52,000 jobs (based on coefficient displayed in Column 5).

Impact by occupations: did trade shocks lead to local job polarization?

I now look into the skill-specific impact of imports competition on employment. As is usual with administrative data, there is no information on workers' educational achievements, therefore I use a definition of skill based on occupations. Occupations are consistently defined over the period of interest at the 2-digit level (for 18 occupations).[32] While the data available do not allow to define occupation with as much precision as in previous works on occupational structures (e.g. Goos and Manning, 2007, use 3-digit occupations), it has some considerable advantages over the data used in previous work. It is a legal obligation of the employer to document accurately the occupational code of their employees which is likely to result in less measurement error than in self-reported survey data (which is what is typically used in the literature). Moreover, the data being exhaustive, it allows to consider variation in the occupational structure at a rather fine geographical level which would not have been possible with survey data. Before examining the effect of Chinese imports competition on the occupational structure, I present some descriptive facts about the evolution of this structure.

Describing job polarization

A first method to relate occupation to skill is to rank occupations according to their initial median or average wage. "Job polarization" (Goos and Manning, 2007) is then documented by relating the growth rate of employment in each occupation to its initial wage rank. The structure of employment is said to be polarizing when most of employment growth occurs in low and high paying occupations at the expense of middling occupations. Figure 1.5 plots the growth rate of employment in each occupations against their initial (inverse) rank for the manufacturing and non-tradable sector (based on average wage). Within manufacturing it appears that, rather than polarization, there has been a rather monotonic relationship between inverse wage rank and employment growth. On the contrary, job polarization does appear to take place in the non-tradable sector.

[31]Balancing trade is equivalent to setting the average deficit per worker (ΔDPW) from its average value to 0.

[32]More details on the classification can be found in Appendix 1.D.

A clear limitation of ranking occupation by average or median wage is that it ignores within-occupation wage variation which represents a substantial fraction of overall wage dispersion. Summarizing occupation skill-intensity by a one-dimensional index leads to a substantial loss of information. For instance, two occupations with roughly similar median wages could exhibit different degree of wage dispersion such that a decrease in employment in each of these two occupations could have very different implications as far as job polarization is concerned. This limitation is particularly salient in this setting given the low granularity of the occupation classification available which implies that a large share of wage dispersion is occurring within each occupation.

To deal with this issue, we build on the method developed by Juhn, Murphy, and Pierce (1993). It assumes that each occupation combines labor from every percentile of the wage distribution, in varying proportions. These factor shares are computed for an initial period and held constant. A given percentage change in employment for a given occupation will then be diffused across percentiles proportionally to the employment share of this occupation within each percentile. More formally, let's consider an occupation o that, for an initial period, employs L_{op} workers coming from p-wage percentile, such that $L_o = \sum_{p=1}^{100} L_{op}$. Shocks occur at the occupation-level and are denoted $\Delta L_o/L_o$. They affect changes in employment for each percentile denoted as $\Delta L_p/L_p$ according to a weight determined by the fixed employment share occupation o in percentile p: L_{op}/L_p. We can therefore write down the predicted percentage change in employment for percentile p as:

$$\frac{\Delta L_p}{L_p} = \sum_{o \in \mathbb{O}} \frac{L_{op}}{L_p} \frac{\Delta L_o}{L_o} \tag{1.7}$$

where $\mathbb{O}$ refers to the set of all occupations. Equation (1.7) expresses the expected change in employment at wage percentile p as predicted by changes in the occupational structure of employment.[33]

Figure 1.6 plots Equation (1.7) both in terms of jobs and hours worked. We see that while hours worked have not grown as fast as employment over the period, the gap is very stable over wage distribution and we will focus on employment in the reminder of the section. The same conclusions as above broadly hold for both sectors. It is remarkable that in manufacturing, employment growth is predicted to be negative up to the 80th decile. Employment at the 10th decile and median wage are predicted to shrink by 13.5 and 8 %

[33] Juhn, Murphy, and Pierce (1993) show how this formula can be derived from a model in which (i) each occupation is a distinct sector of the economy, (ii) each sector operates at constant returns to scale and (iii) sectors are subject to factor neutral shocks, i.e. the shocks themselves do not affect the sector-specific factor shares (Juhn, Murphy, and Pierce, 1993, p.432). Note that shocks can be factor neutral within a given occupation while being overall skill-biased, for instance if shocks are relatively larger in low-skill intensive occupations.

respectively. The slope becomes much steeper above the 80th decile. In the non-traded sector, polarization is shown to occur although for the most part the slop is rather flat. For instance, predicted employment growth is equal to 20 % for the 10th and 75th wage percentile and is the lowest around the median wage at 17 %. The slope is much steeper in the upper part of the distribution: employment is predicted to grow at 22.5 % and 29.5 % at the 80th and 90th percentile respectively. Interestingly, the change in occupational structure would predict a large increase in wage inequality which is at odds with findings on the actual wage distribution. The variance of log hourly wage which has remain very stable over the period, slightly increasingly in manufacturing and decreasing in the non-traded sector. This suggests that there has certainly been a decline of the between-occupation component of wage dispersion. Section 1.5 analyses changes in the actual wage distribution and contrasts discusses the contrast between changes in the job versus wage structures more at length.

Effect of Chinese imports competition on job polarization

We now consider the effect of trade on job polarization. Retaining the specification of Column (6) from Table 1.2 and 1.3, occupation-sector specific coefficients are estimated (which we denote $\{\hat{\beta}_o\}_{o\in\mathbb{O}}$) and are then reweighed according to the same formula:[34]

$$\widehat{g}_p = \sum_{o\in\mathbb{O}} \frac{L_{op}}{L_p}\hat{\beta}_o \tag{1.8}$$

Figure 1.7 plots the results of Equation (1.8). Interestingly, we see that in manufacturing, the change in occupational structure triggered by imports competition have a clear polarizing effect on employment growth which contrasts with the overall trend described above. The effect is of -3% at the 10th percentile. It becomes more negative along the wage distribution and reaches -7 % at the 70th percentile. It then goes up and ceases to be significant at the 90th percentile. This pattern is somewhat consistent with the process of skill-upgrading caused by Chinese imports competition at the firm-level detected by Mion and Zhu (2013), although in a very skewed manner where upgrading is restricted to the very top of the distribution. In contrast, the non-traded sector's occupational structure is affected more uniformly.[35]

[34]We build standard error for $\widehat{g}_p$ assuming that the estimators for different occupations are independently distributed.

$$\text{Std error}(\widehat{g}_p) = \sqrt{\widehat{Var}(\widehat{g}_p)} = \sqrt{\sum_{o\in\mathbb{O}} \left(\frac{L_{op}}{L_p}\right)^2 \widehat{Var}(\hat{\beta}_o)}$$

The online appendix displays the occupation-specific coefficients simply ranked by average wage (Table OA3).

[35]Occupation specific coefficients as well as a regression table based on a more aggregated definition of

The pattern of the skill-specific impact of trade shocks would tend to predict a rise in wage inequality, particularly in the manufacturing sector. However there might be countervailing forces - reduction in within-occupation dispersion or decline (increase) in the average wage of initially high (low) wage occupations. Without attempting to decompose overall impact of trade on wages into different component, the next section looks directly at the overall impact of imports competition along the local wage distribution.

1.5 Results on the wage impact along the distribution

There is by now a large body empirical work showing that changes in imports exposure has a very heterogenous impact across firms. For instance, Bloom, Draca, and Reenen (2011) find that surge in Chinese imports competition has a negative employment effect that is considerably smaller for innovative firms. Amiti and Davis (2012) shows that, following a trade liberalization episode in Indonesia, large exporters or importers increased their wages relative to firms serving the domestic market only. As explained in the introduction, such heterogeneous effects along firm-level characteristics will be associated with increase in wage dispersion in ways that are unlikely to be fully captured by a variable pertaining to individual-level skill, such as college education or type of education. On the other hand such impact will be reflected in the overall distribution of wages. In this section, I estimate the impact of Chinese imports competition on each decile of the wage distribution, thus capturing overall changes in wage dispersion while remaining agnostic about which the particular mechanism through which Chinese imports competition is affecting inequality.

The upper panel of Table 1.6 displays the results regarding the manufacturing sector. All deciles but the bottom one are significantly negatively affected by imports competition. The estimate is very imprecisely estimated for the 1st decile, resulting in an insignificant coefficient in spite of a very large point estimate. The impact for the median is somewhat lower than the effect on the average wage and imprecisely estimated. The highest estimates are those for the 8th and 9th deciles (although they do not differ significantly from the other coefficients). The distribution of hourly wage in manufacturing is not made less egalitarian by exposure to imports competition, instead it's been rather uniformly decreased. For instance, moving from the 25th to the 75th percentile in terms of increase in Chinese imports competition implies a reduction in wage growth that represents about 8% of the average wage growth at the 20th and 40th percentile and of about 11 % at the 80 and 90th percentile. While most estimates are barely significant, note however that only results based

occupations are presented in the Online Appendix (see Figure OA3 and Table OA3).

on the most stringent specification are displayed. Unreported coefficients based on the same specification without region-fixed yield very similar point estimates with more precision.[36]

The impact of rising Chinese imports competition on the actual local wage distribution within manufacturing contrasts starkly with what one would have predicted based on its impact the occupational structure. While the occupational structure appears to have been strongly polarized, wage dispersion is virtually unaffected. We note that this dual development – job polarization and no increase in wage inequality – holds at the aggregate level. One explanation refers to the large increase in the supply of college educated workers over the period (Verdugo, 2014). This is in a sense the reverse scenario of that developed by Katz and Murphy (1992) which explains the rise in the US college premium over the 1980s by the decline in the relative supply of college graduates. However this explanation is not particularly compelling in the present case. First, note that my estimates are obtained using cross-sectional variation across local labor markets in imports competition. Therefore shocks to the supply of high-skill workers in manufacturing would have to be positively correlated to the raise in imports competition which does not seem particularly plausible. Moreover, controlling for contemporaneous changes in the share of college graduates, an admittedly "bad control" (Angrist and Pischke, 2009), does not affect the results.[37] While exploring the sources of this discrepancy is beyond the scope of this paper, it appears to be an interesting venue for future research.

The lower panel of Table 1.6 displays results for the non-traded sector. We see that the non-significant effect on average wage hides considerable heterogeneity across deciles. There are clearly negative effects in the middle-part of the distribution, between the 2nd and 7th deciles included. The impact on the median wage is much stronger than that on the average wage. The median wage effect is of the same magnitude as in the manufacturing sector and is more precisely estimated. This shows that trade shocks are diffused to the local non-tradable sector. The bottom decile is not significantly affected, with a point estimated very close to zero. A binding minimum wage could be a reason behind this absence of effect (more on this below). There is also no significant impact on the two top deciles. This is consistent with the finding that employment growth in occupations usually located in the top of the distribution was not affected by trade shocks (See the right panel of Figure 1.7 and the discussion in Section 1.4.3). Economically, the impact, while being rather precisely estimated, appears modest. For instance, moving from the 25th to the 75th percentile in terms of increase in Chinese imports competition implies a reduction in wage growth that

[36] The results are available upon request.

[37] The reason why the results are barely affected is because the rise in the share of college graduates has been very uniform across local labor markets. For instance the rank correlation between share of college graduates in 1990 and 1999 is 0.96 while the average aggregate share has gone up from 8 to 13%. The results are available upon request.

represents about 4 % of the average wage growth at the 20th percentile, 7 % at the 40h and 50th percentile and 6 % at the 60th percentile.

In Table 1.7, we report how different measures of wage dispersion/inequality have been affected by China-induced trade shocks. Column (1) displays the estimated effect on changes in the 90-10 log wage differential of the local wage distribution. While the coefficient is positive for manufacturing, it is very imprecisely estimated. It is very close to 0 and insignificant in the non-tradable sector as well. Hence it appears, based on that measure of inequality, that that Chinese imports competition, while it had some notable impact on wage and on employment did not move affected local labor markets away from the general trend towards compression of the French wage distribution over the period considered (Verdugo, 2014). We decompose the change of the log 90-10 differential into the sum of the change of the log of the 50-10 ratio (lower-tail inequality) and the change of the log of the 90-50 ratio (upper tail inequality). In the case of manufacturing, although point estimates suggest a rise in lower-tail dispersion, none is significantly different from zero. In the non-traded sector however, it appears that null effect of imports competition on the 90th to 10th percentile ratio results from a combination of increase in upper-tail dispersion (Column 2) and compression in the lower-tail of the distribution (Column 3). Here again, the binding role of the minimum wage provides a plausible explanation to these findings. I investigate this question further below but first proceed to some robustness checks.

Confounding effect of the reform of working time regulation in France?

A word concerning the possibly confounding impact of the so-called "35-hours legislation" is warranted. Two laws were voted in 1998 and 2000 that implemented a law that reduced the working hours from 39 hours to 35 hours. Increase in hourly wages, notably at the minimum wage level, were granted to ensure the reform was neutral income-wise for the workers. The 35 hours law has an overall compressing effect on the wage distribution, at least during the transition. Moreover, its pace of implementation between 1998 and 2002 was unequal with, for instance large firms on average adopting the legislation faster. It is therefore important to ensure that the measure of trade shocks is not capturing some nation-wide impact that would not be absorbed by period fixed-effects due to the unequal pace of implementation. If the index of exposure to Chinese imports competition was capturing the impact of the reform, we should expect to find a decline of the 90-10 log wage ratio. We do not find however a significant decline neither in the manufacturing sector nor in the non-tradable sector. However, there remains a suspicion that our finding of a decline in the 50-10 log wage ratio in the non-tradable sector could be related to the reform. Importantly for our purpose, earnings per month were not affected by the reform (A fact used in a recent paper by Goux, Maurin, and Petrongolo, 2014). Therefore to assess to which extent the previous results are driven by the confounding impact of the 35 hours reform, I reproduce the analysis using monthly earnings – defined as overall earnings per job divided by the documented length of the contract in months – for full time employees only.[38] Based on Figure 1.9, we see that the two sets of coefficients display a very similar U-shaped pattern. It suggests clearly that the working time reform, which did not have any impact on monthly earnings, is very unlikely to be driving the estimated effect.

The role of the minimum wage

The minimum wage is nationally set in France and there is no variation in the legal definition of the minimum wage across employment areas or sectors of employment.[39] It is however more or less binding, meaning that there is variation in the share of workers working at the minimum wage across locations and sectors. In this subsection, I use this variation to test the hypothesis whether a binding minimum wage explains the "compression" caused by the imports shocks on the bottom tail of the wage distribution in the non-tradable sector and whether places where the minimum wage was not binding experienced an increase in overall inequality following rising exposure to Chinese imports competition. In order to split the sample somewhat equally, I focus on the 15th percentile.[40] We define overall, lower tail and upper tail wage inequality respectively as the 85-15, the 50-15 and the 85-50 log wage ratio.

If in a given employment area and sector, more than 15 % of jobs are paid are the minimum wage, then, one would not expect any impact of trade-induced demand shocks on the 15th percentile of the wage distribution. Inversely, employment areas where less than 15 % of jobs are paid at the minimum wage, the bottom decile of the distribution is exposed to the downward pressure associated with negative labor demand shocks. I introduce an interaction term between ΔIPW and a binary variable equal to 1 if less than 15 % of employees (in manufacturing or the non-tradable sector depending on which sector the dependent variable pertains) in the initial year of the period are paid at the minimum wage. This interaction term is then instrumented by the product of ΔIPW^o and the binary variable. Noting S_{15} the binary variable, the specification we estimate is the following:

[38] Naturally the definition of full-time takes the changes introduced by the reform into account. This restriction is necessary in order to compare hourly and monthly wages in a meaningful way.

[39] While there are some legal exemptions to the minimum wage, they account for a small share of the working population, see Kramarz and Philippon (2001).

[40] Regarding the non-traded sector wages, roughly 40 % of observations are associated with a 15th percentile in the that is above to the minimum wage. Hence the minimum wage is binding at the 15th percentile for roughly 60 % of observations. At the 10th percentile level, the minimum wage is binding for about 95 % of observations.

$$\Delta \log q_{15,it} = \Delta IPW_{it} \cdot \beta_1 + \Delta IPW_{it} \times S_{15,it} \cdot \beta_2 + X'_{it}\delta + \eta_t + \varepsilon_{it} \quad (1.9)$$

Following the line of reasoning exposed above, one would expect β_1 to be close to zero, and β_2 to be negative, meaning that wage losses are concentrated in areas/sector where the minimum wage is not binding for the 15th percentile. There are obvious limitations to this exercise, as the initial share of minimum wage employees could be considered endogenous to posterior growth in the 1st decile of the wage distribution. There could be for instance some unobservable characteristics affecting both minimum wage ratios and subsequent wage growth rate. Note however that intuitively, one would expect such unobservables to cause high minimum wage ratio and lower wage growth. On the contrary, our hypothesis states low minimum wage ratios should be associated with lower wage growth when associated with strong exposure rise in Chinese imports competition. Consequently, endogeneity of low minimum wage local ratio seems likely to introduce an upward bias in estimates that we expect to be negative.

Table 1.8 presents results for the non-tradable sector. Columns 1 to 4 repeat baseline results, considering the 15th rather than the 10 percentile. Column 1 finds a very weak negative effect on the 15th percentile. Column 2 finds a null effect on the 85-15 log ratio. This absence of effect can be unpacked into a positive effect on upper tail inequality (85-50 log ratio, column 3) and a negative effect on lower tail inequality (50-15 log ratio, column 4)

Result in Column (5) show clearly that areas where the 15th percentile was not covered by the minimum wage experienced a negative impact of imports competition, while the others were (unsurprisingly) protected. Column (6) reports results on overall wage inequality. The response of overall wage inequality between places with and without a binding minimum wage is significantly different: inequality grows relatively more in areas where the minimum wage is not binding. Column (7) shows that there is no differences between groups in terms of how upper-tail inequality is affect by Chinese imports competition. The coefficient on the interaction term is very close 0 with a p-value of 0.85. Column (8) shows that on the contrary trade shocks compressed the bottom tail of the distribution in areas with binding minimum wage and had virtually no effect in places where the minimum wage does not bind.

The coefficient implies an economically large effect. Over the period the 50-15 log ratio has declined by 1.3% on average. In cities with a binding minimum wage at the 15th percentile, the average level of exposure to imports competition implies a reduction of 0.65 % of this ratio (equivalent to 46 % of the average reduction in this ratio). In employment zones where the minimum wage is not binding, the ratio is only reduced by 0.16 %. The minimum wage appears to imply sizable differences in how the bottom of the wage distribution is affected by trade shocks. This difference is also reflected in overall wage inequality (measured by the 85-15 log ratio) given that there is no offsetting effect on the upper-tail of the distribution.

1.6 Conclusion

Local employment and total labor income in the manufacturing sector are reduced in employment areas more exposed to Chinese imports. The effect goes beyond manufacturing sector as non-traded employment is also significantly affected. The estimates suggest that the number of jobs displaced is higher in the non-traded than in the traded sector. However this conclusion is reversed when considering hours worked rather than job count, highlighting the usefulness to account for systematic difference in the types of jobs across sectors when assessing the strength of the local spill-overs.

The employment impact of trade is found to be very uneven across skill categories. There is a negative monotonic relationship between skill and the magnitude of the effect of imports competition on job growth in the non-traded sector. This relationship is U-shaped within manufacturing, implying that Chinese imports competition has polarized the occupational structure of employment in that sector.

Wage rates are found to be negatively affected by Chinese imports competition, although the pattern of the effect differs markedly between sectors. The impact is rather uniform in the manufacturing sector and wage inequality do not rise. Understanding the co-existence of a polarizing effect imports competition on the structure of employment and the absence of such effect on structure of wage in manufacturing appears an interesting venue for future research.

In contrast with manufacturing, the non-traded sector experienced wage polarization: the median wage declines with respect to both the 15th and the 85th percentile. I present evidence suggesting that the compression of the bottom of the wage distribution following a trade shock is attributable the bite of the minimum wage and is not neutralized by offsetting trends in the upper part of the distribution. Therefore wage polarization as a consequence of trade shocks only occurs in areas where the minimum wage is binding.

The present study highlights the rising impact of low-wage competition on local labor markets. The presence of large local multiplier effects associated with large trade-induced displacements combined with evidence that there is little labor mobility in response to shocks in local demand suggest that trade shocks have locally concentrated effects that are likely to be long-lasting. These trends contribute to explain the popularity of place-based policies that generally aim at tempering the local consequences of labor demand shocks. The design and implementation of optimal place-based policies in the presence of strong local labor demand shocks and workers low spatial mobility seem therefore an important area for further research.

1.7 Tables

Table 1.1: Descriptive Statistics

	(1) Period 1995-2001			(2) Period 2001-2007		
	mean	sd	p50	mean	sd	p50
Initial employment in thousands	65.83	101.29	33.63	78.85	118.57	41.53
ΔIPW (in thsds \$, 2001)	0.17	0.12	0.14	1.00	0.65	0.80
ΔDPW (in thsds \$, 2001)	0.15	0.13	0.12	0.79	0.68	0.59
% employment in mfg	28.97	9.44	27.65	24.48	8.72	23.58
% chge in mfg empl.	-1.58	10.13	-0.74	-13.05	9.10	-12.93
% chge in non-tradable sector empl.	25.03	5.23	25.09	7.42	7.69	7.82
Hours worked per job: mfg	1615	69	1619	1496	65	1497
Hours worked per job: non-traded	1299	44	1308	1154	42	1156
90/10 ratio mfg	2.83	0.52	2.71	2.81	0.67	2.64
90/10 ratio non-traded sector	3.01	0.58	2.84	2.88	0.61	2.73
σ log(hrly wages) in mfg	0.45	0.05	0.43	0.45	0.06	0.44
σ log(hrly wages) in tradable sector	0.48	0.06	0.47	0.47	0.06	0.46

Note: See Equation 1.1 for definition of ΔIPW and ΔDPW. Except for the first line, all averages are computed using 1995 total employment as weights.

Table 1.2: Direct impact of Chinese imports competition on manufacturing

	(1) OLS: Jobs	(2) IV	(3) IV	(4) IV	(5) IV	(6) IV: Hrs	(7) IV: Emp. earnings
	b/se	b/se	b/se	b/se	b/se	b/se	b/se
ΔIPW	-5.876***	-8.349***	-6.262***	-6.313***	-6.224***	-6.084***	-8.636***
	(1.242)	(1.618)	(1.782)	(1.789)	(1.667)	(1.621)	(1.885)
% employment in mfg			-0.151***	-0.205***	-0.097	-0.095	0.040
			(0.059)	(0.069)	(0.067)	(0.072)	(0.076)
% college				-0.653***	-0.368**	-0.405***	-0.442***
				(0.174)	(0.143)	(0.148)	(0.157)
% production workers				-0.362***	-0.181	-0.189*	-0.208
				(0.111)	(0.114)	(0.115)	(0.127)
% women				-1.462**	-1.948***	-2.106***	-2.345***
				(0.650)	(0.506)	(0.596)	(0.687)
% foreigners				-0.465**	-0.496**	-0.498**	-0.543**
				(0.213)	(0.193)	(0.211)	(0.231)
KP stat		48.66	31.09	31.72	32.51	32.51	32.51
Region fixed-effect					√	√	√

Note: $N = 696$. Baseline sample is a balanced panel of 348 employment zones. Outcomes variables are expressed in percentage change over six-year period. All specifications include period fixed effect and log of initial total employment. Robust standard errors are clustered at the employment zone level. *p<.10 ** p<.05, *** p<.01.

Table 1.3: Impact of Chinese imports competition on the nontradable sector

	(1) OLS: Jobs b/se	(2) IV b/se	(3) IV b/se	(4) IV b/se	(5) IV b/se	(6) IV: Hrs b/se	(7) IV: Emp. earnings b/se
ΔIPW	-1.845***	-3.640***	-3.937***	-4.071***	-3.645***	-1.765**	-2.363***
	(0.558)	(0.982)	(1.092)	(1.095)	(0.850)	(0.760)	(0.840)
% employment in mfg			0.022	0.003	0.170***	0.146***	0.177***
			(0.046)	(0.049)	(0.048)	(0.046)	(0.049)
% college				-0.360**	-0.099	-0.187*	-0.351***
				(0.143)	(0.117)	(0.113)	(0.121)
% production workers				-0.140	0.135	0.065	0.036
				(0.085)	(0.091)	(0.085)	(0.092)
% women				-0.731	-0.936**	-1.140**	-1.076**
				(0.448)	(0.472)	(0.453)	(0.503)
% foreigners				0.062	-0.082	-0.154	-0.190
				(0.146)	(0.159)	(0.155)	(0.172)
KP stat		48.66	31.09	31.72	32.51	32.51	32.51
Region fixed-effect					√	√	√

Note: $N = 696$. Baseline sample is a balanced panel of 348 employment zones. Outcomes variables are expressed in percentage change over six-year period. All specifications include period fixed effect and log of initial total employment. Robust standard errors are clustered at the employment zone level. *p<.10 ** p<.05, *** p<.01.

Table 1.4: Placebo regression: private employment growth regressed on lead values of import competition

	(1) OLS: Private b/se	(2) IV: Private b/se	(3) OLS: Pre-trend b/se	(4) IV: Pre-trend b/se	(5) OLS: Lagged b/se	(6) IV: Lagged b/se
ΔIPW	-2.495***	-3.755***	-2.383***	-4.512***	-0.785	2.741**
	(0.405)	(0.790)	(0.405)	(0.959)	(0.632)	(1.300)
Pre-trend			0.142***	0.138***		
			(0.038)	(0.037)		
KP stat		32.16		35.52		32.16
All controls	√	√	√	√	√	√
Region fixed-effect	√	√	√	√	√	√

Note: Column 1 and 2 respectively report OLS and IV estimates of specification shown in Equation (2.3) where the dependent variable is current employment growth rate (in equivalent of 6 years) in the overall private sector. Columns 3 and 4 represent the same specification augmented with pre-trend in private sector growth as control. Columns 5 and 6 respectively report OLS and IV estimates of the same specification where employment growth in the private sector is lagged (1982-1990 for period 1995-2001, 1995-2002 for period 2001-2007). In case, long run unobserved factor driving down manufacturing employment in local labor markets is correlated with future exposure to Chinese imports competition, one would expect to find negative coefficients in the Column 5 and 6.

Table 1.5: Trade deficit per worker: impact on manufacturing employment

	(1) OLS: Jobs b/se	(2) IV b/se	(3) IV b/se	(4) IV b/se	(5) IV b/se	(6) IV: Hrs b/se	(7) IV: Emp. earnings b/se
ΔIPW	-4.731***	-9.981***	-7.656***	-7.661***	-7.596***	-7.425***	-10.540***
	(1.192)	(2.298)	(2.464)	(2.471)	(2.287)	(2.167)	(2.654)
% employment in mfg			-0.141**	-0.183**	-0.077	-0.075	0.068
			(0.062)	(0.075)	(0.073)	(0.078)	(0.085)
% college				-0.643***	-0.327**	-0.364**	-0.384**
				(0.174)	(0.146)	(0.151)	(0.164)
% production workers				-0.395***	-0.170	-0.177	-0.192
				(0.113)	(0.116)	(0.117)	(0.132)
% women				-1.547**	-2.019***	-2.176***	-2.445***
				(0.675)	(0.524)	(0.614)	(0.714)
% foreigners				-0.453**	-0.486**	-0.489**	-0.529**
				(0.227)	(0.212)	(0.229)	(0.255)
KP stat		26.38	16.07	16.60	17.42	17.42	17.42
Region fixed-effect					√	√	√

Note: $N = 696$. Baseline sample is a balanced panel of 348 employment zones. Outcomes variables are expressed in percentage change over six-year period. All specifications include period fixed effect and log of initial total employment. Robust standard errors are clustered at the employment zone level. *p<.10 ** p<.05, *** p<.01.

Table 1.6: Impact along the wage distribution in manufacturing and the non-traded sector

	(1) avwage b/se	(2) 10th b/se	(3) 20th b/se	(4) 30th b/se	(5) 40th b/se	(6) 50th b/se	(7) 60th b/se	(8) 70th b/se	(9) 80th b/se	(10) 90th b/se
Mfg sector										
ΔIPW	-2.552***	-2.838	-1.385*	-1.513**	-1.348*	-1.176	-1.261	-1.470*	-2.238**	-2.250**
	(0.809)	(1.969)	(0.769)	(0.769)	(0.775)	(0.782)	(0.798)	(0.862)	(0.880)	(0.972)
	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se
Non-traded sector										
ΔIPW	-0.598**	0.049	-0.714***	-0.883***	-1.137***	-1.068***	-1.014***	-0.934***	-0.429	0.049
	(0.260)	(0.245)	(0.235)	(0.240)	(0.258)	(0.263)	(0.293)	(0.329)	(0.369)	(0.465)
KP stat	32.51	32.51	32.51	32.51	32.51	32.51	32.51	32.51	32.51	32.51
Full set of controls		√	√	√	√	√	√	√	√	√
Region fixed-effect		√	√	√	√	√	√	√	√	√

Note: $N = 696$. Baseline sample is a balanced panel of 348 employment zones. Outcomes variables are expressed in percentage change over six-year period. All specifications include period fixed effect and log of initial total employment. Robust standard errors are clustered at the employment zone level. *p<.10 ** p<.05, *** p<.01. Control variables include: initial share of manufacturing jobs, of female employees, of production employees, of foreign-born employees and of college educated employees. Decile are computed based on jobs reporting positive hours worked and wages, weighing by hours worked.

Table 1.7: Effect of Chinese imports competition on different measures of wage inequality

	(1)	(2)	(3)
	$\Delta \log \frac{q\_90}{q\_10}$	$\Delta \log \frac{q\_90}{q\_50}$	$\Delta \log \frac{q\_50}{q\_10}$
	b/se	b/se	b/se
Manufacturing			
ΔIPW	0.588	-1.074	1.662
	(1.914)	(0.929)	(1.547)
	(1)	(2)	(3)
	b/se	b/se	b/se
Non-traded sector			
ΔIPW	0.000	1.116**	-1.116***
	(0.500)	(0.443)	(0.298)
KP stat	32.51	32.51	32.51
Full set of controls	√	√	√
Region fixed-effect	√	√	√

Note: $N = 696$. Baseline sample is a balanced panel of 348 employment zones. Outcomes variables are expressed in percentage change over six-year period. All specifications include period fixed effect and log of initial total employment. Robust standard errors are clustered at the employment zone level. *p<.10 ** p<.05, *** p<.01. Control variables include: initial share of manufacturing jobs, of female employees, of production employees, of foreign-born employees and of college educated employees. Decile are computed based on jobs reporting positive hours worked and wages, weighing by hours worked.

Table 1.8: Impact along the wage-distribution depending on the "bite" of the minimum wage

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\Delta \log q_{15}$	$\Delta \log \frac{q_{85}}{q_{10}}$	$\Delta \log \frac{q_{85}}{q_{50}}$	$\Delta \log \frac{q_{50}}{q_{15}}$	$\Delta \log q_{15}$	$\Delta \log \frac{q_{85}}{q_{10}}$	$\Delta \log \frac{q_{85}}{q_{50}}$	$\Delta \log \frac{q_{50}}{q_{15}}$
ΔIPW	-0.462**	0.323	0.929**	-0.606***	0.291	-0.166	0.856**	-1.022***
	(0.832)	(0.565)	(0.314)	(0.311)	(0.832)	(0.565)	(0.314)	(0.311)
$\Delta IPW \times S$					-1.341***	0.736**	-0.024	0.760***
					(0.237)	(0.359)	(0.343)	(0.199)
$S := I(\text{Share Min Wage} < 15\%)$					0.213	0.327	0.506*	-0.179
					(0.173)	(0.326)	(0.298)	(0.165)
KP stat	32.51	32.51	32.51	32.51	16.79	16.79	16.79	16.79
Full set of controls (see notes)	√	√	√	√	√	√	√	√
Region fixed-effect	√	√	√	√	√	√	√	√

Note: $N = 696$. Baseline sample is a balanced panel of 348 employment zones. Outcomes variables are expressed in percentage change over six-year period. All specifications include period fixed effect and log of initial total employment. Robust standard errors are clustered at the employment zone level. *p<.10 ** p<.05, *** p<.01. The share of minimum wage workers is computed as the share of workers in a given location and sector who hourly wage (*salaire brut horaire*) is comprised between 85 and 105 % of the legal minimum wage. Observations whose wage is reported below 85 % of the minimum wage are dropped. 60 % of employment zones have a share of minimum wage jobs larger than 15 % in the non-traded sector.

1.8 Figures

Figure 1.1: Imports and trade balance of France with respect to China and other low-wage countries (list based on Auer, Degen, and Fischer (2013))

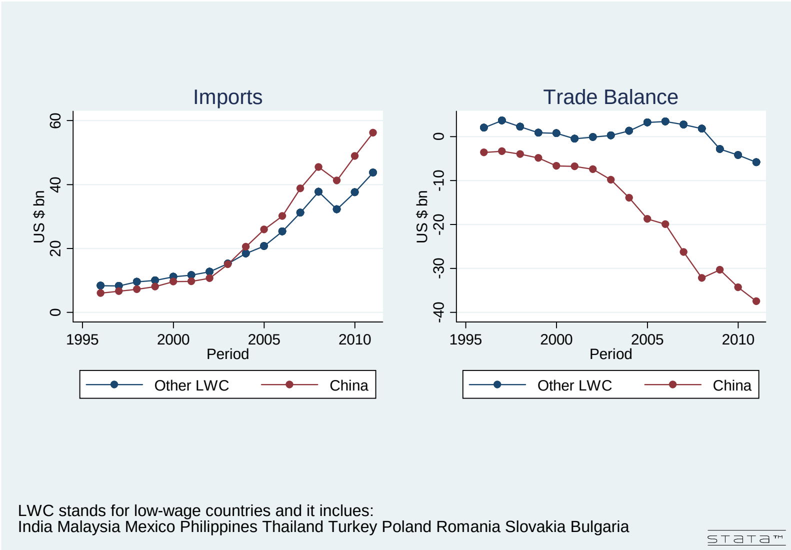


Figure 1.2: Exposure to Chinese imports competition and employment growth (2001-2007)

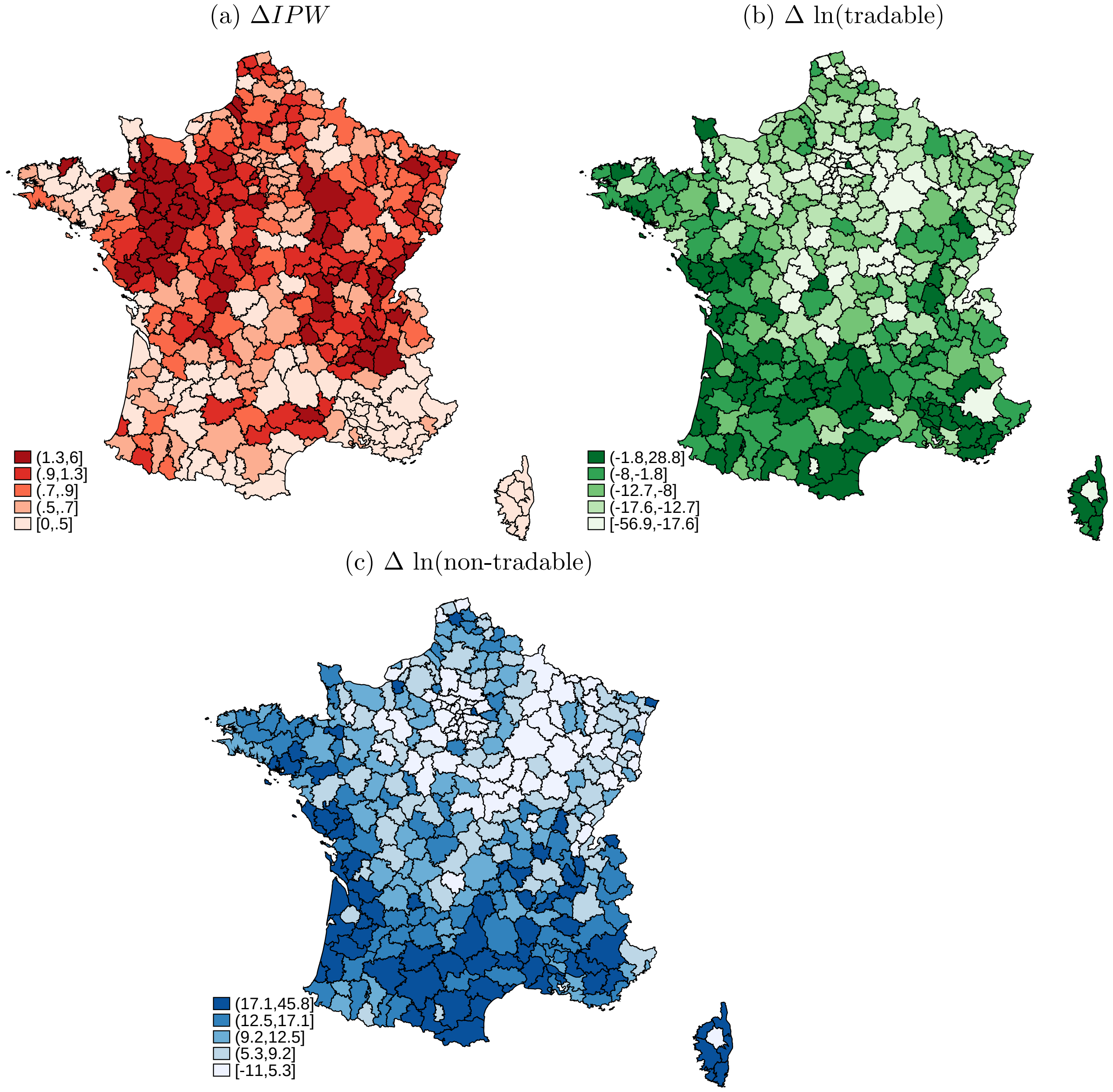
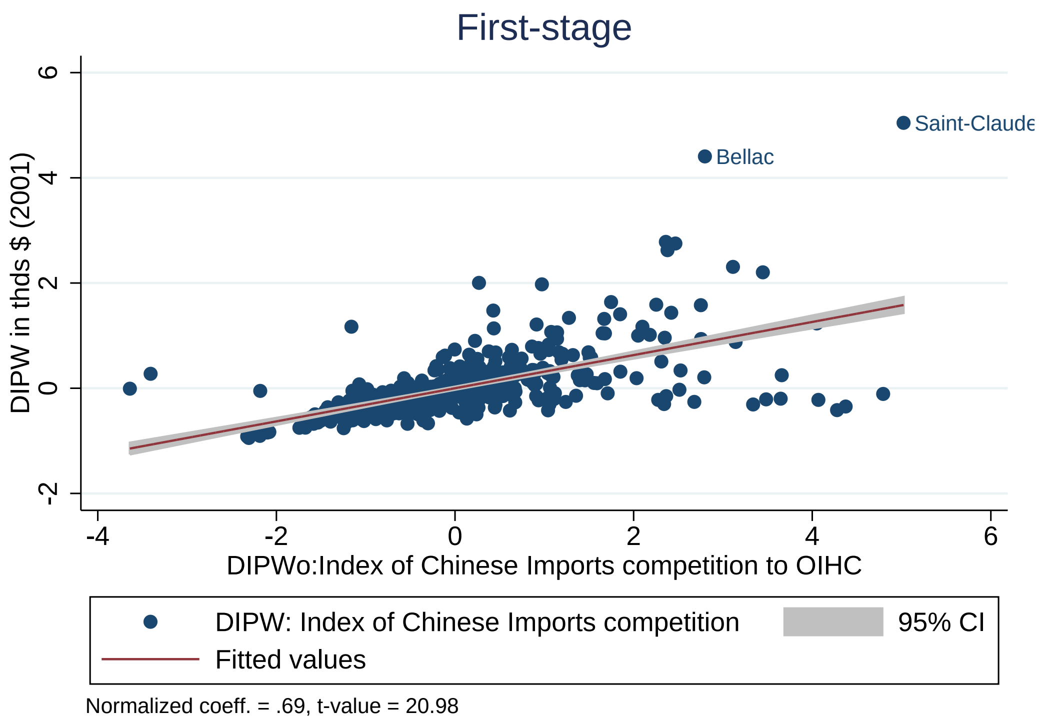
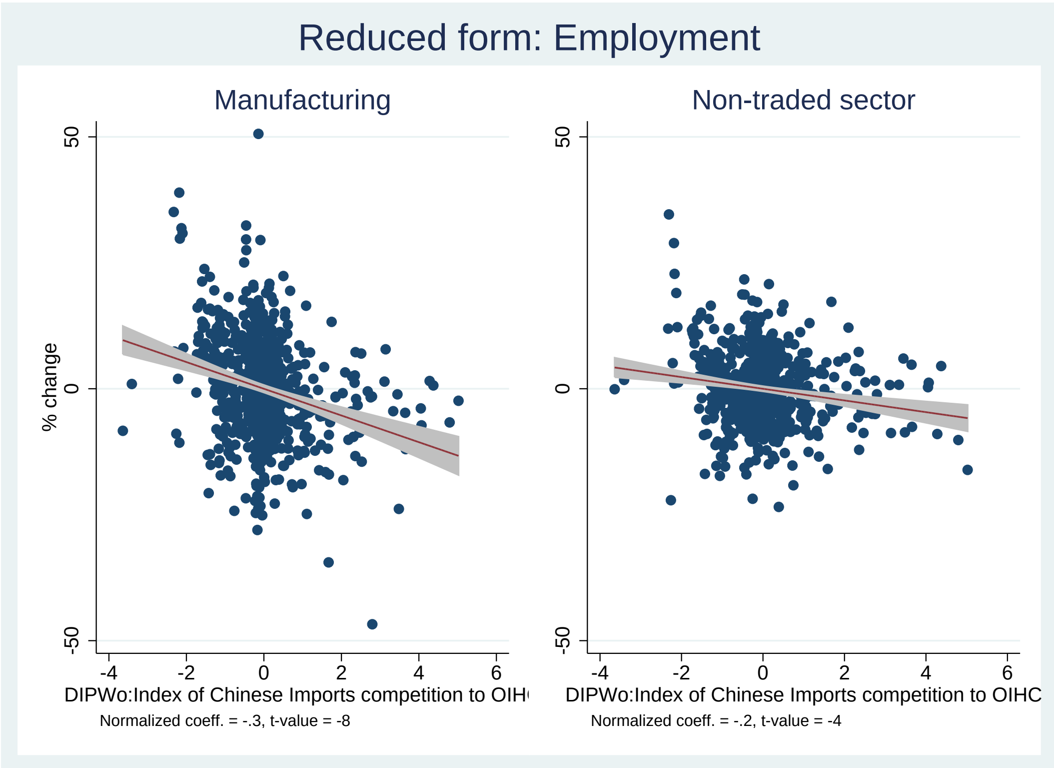


Figure 1.3: First stage



Note: Each dot represents an employment zone for a given period (1995 to 2001, 2001 to 2007). Variables are expressed in deviation from period average. The two outliers in the North/North-West of the graph correspond to two heavily specialized employment areas. The one in the left is specialized in apparel (18% of employment in 2001), the one on the right is specialized in the manufacture of plastic products (12% of employment). In these two sectors, Chinese exports to France grew at a faster than towards others high income countries..

Figure 1.4: Reduced form



Note: Each dot represents an employment zone for a given period (1995 to 2001, 2001 to 2007). Variables are expressed in deviation from period average.

Figure 1.5: Employment growth by occupations

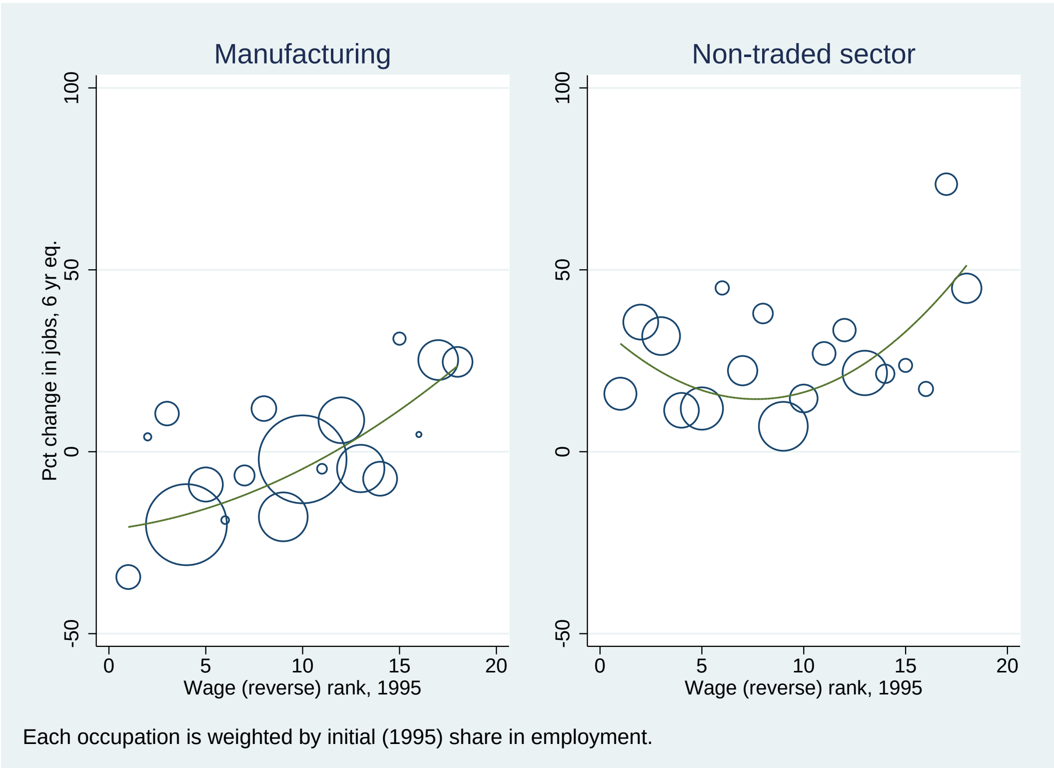
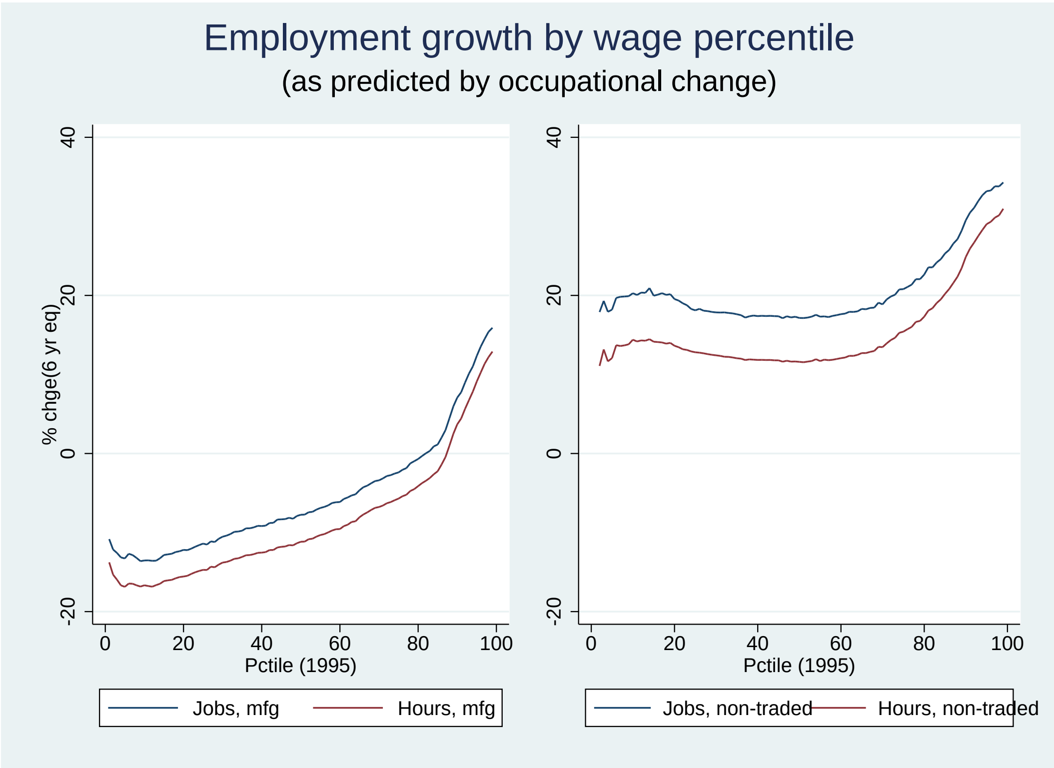
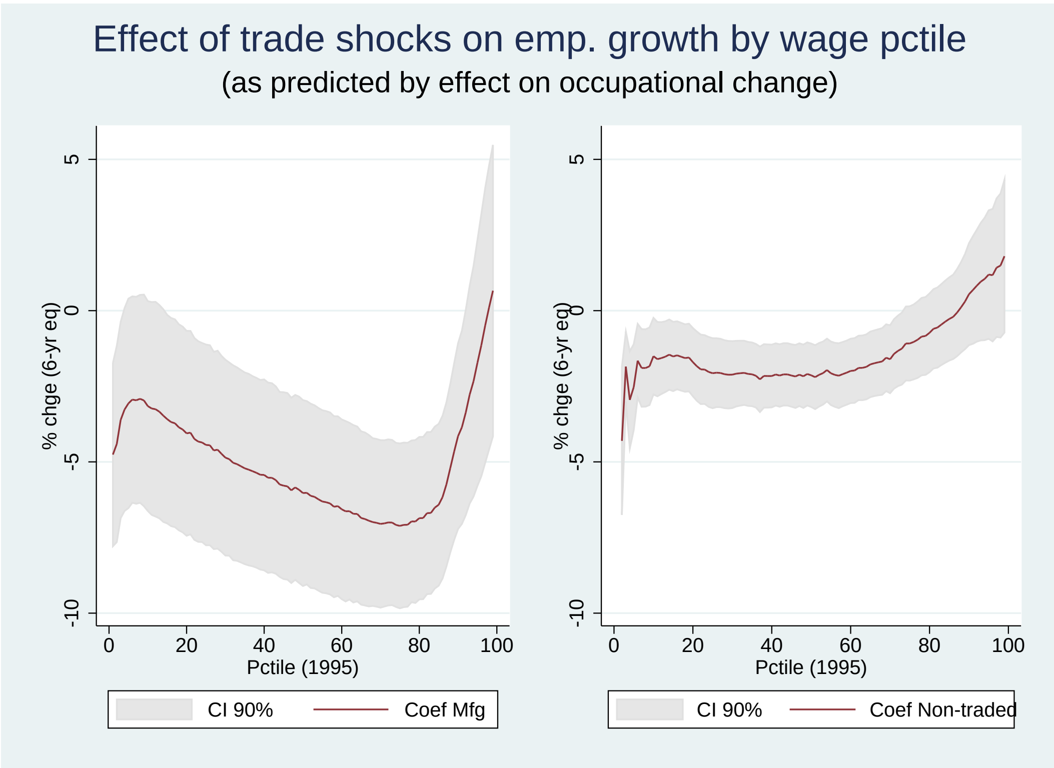


Figure 1.6: Employment growth by wage percentile and occupational change



Note: The main line is obtained by reweighing occupation-sector specific growth rates over the period 1995-2007 (re-expressed in 6 year equivalent growth rates) according to the formula presented in Equation (1.7).

Figure 1.7: The impact of Chinese imports competition on employment growth by wage percentile (based on occupational change)

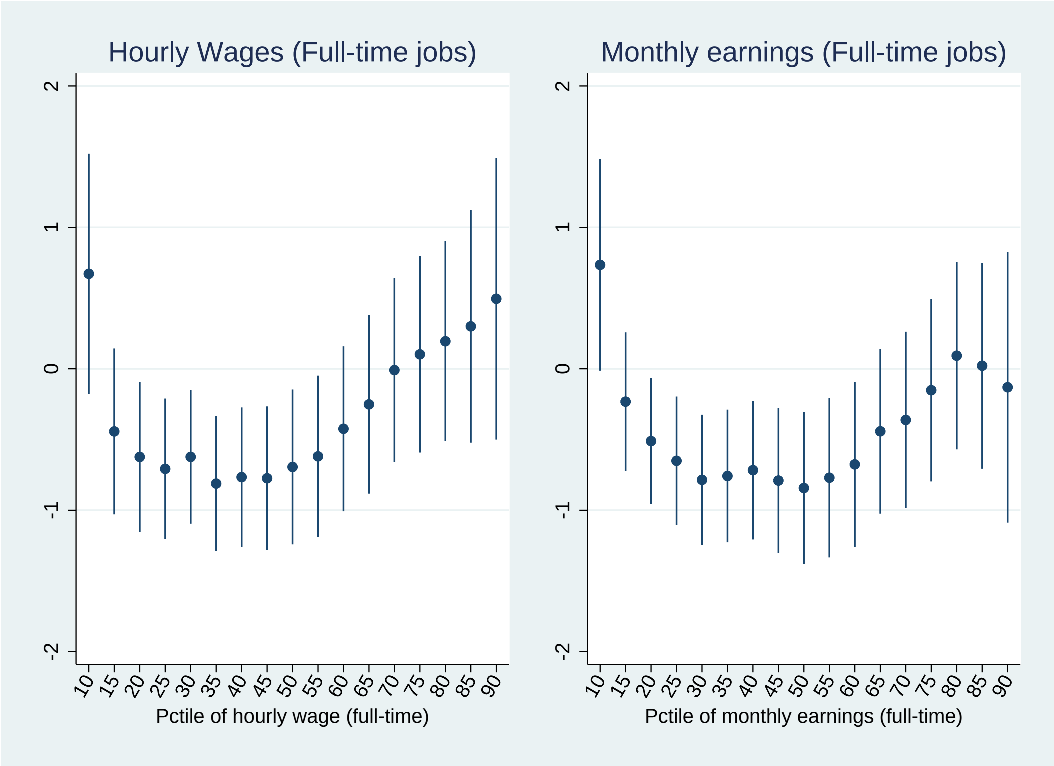


Note: Each coefficients plotted comes from the same specification including of controls and region fixed-effects. Control variables include: initial share of manufacturing jobs, of female employees, of production employees, of foreign-born employees and of college educated employees.

Figure 1.8: The impact of Chinese imports competition along the wage distribution

Note: Each coefficients plotted comes from the same specification, including the full set of controls and region fixed-effects. Control variables include: initial share of manufacturing jobs, of female employees, of production employees, of foreign-born employees and of college educated employees.

Figure 1.9: The impact of Chinese imports competition on hourly wages and monthly earnings



Note: Each coefficients plotted comes from the same specification, including the full set of controls and region fixed-effects. Control variables include: initial share of manufacturing jobs, of female employees, of production employees, of foreign-born employees and of college educated employees. Note that the slight discrepancy between the left panel of this figure and the right panel of Figure 1.8 stem from the restriction to full-time jobs.

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1.A Appendix Tables

Table A1: Imports competition by other trade partners

	(1) China (baseline) b/se	(2) China + DA b/se	(3) China + EE b/se	(4) China + all other b/se
ΔIPW	-6.224***	-4.979***	-5.018***	-0.255**
	(1.667)	(1.268)	(1.240)	(0.126)
KP stat	32.51	46.32	41.66	8.717
Full set of controls	√	√	√	√
Region fixed-effect	√	√	√	√

Note: $N = 696$. Baseline sample is a balanced panel of 348 employment zones. Outcomes variables are expressed in percentage change over six-year period. All specifications include period fixed effect and log of initial total employment. Robust standard errors are clustered at the employment zone level. *p<.10 ** p<.05, *** p<.01.

Table A2: Using Bartik-instrument as a proxy for unobserved sectoral shocks

	(1) OLS	(2) OLS Bartik	(3) IV
Manufacturing			
ΔIPW	-4.784***	-4.784***	-6.224***
	(1.427)	(1.540)	(1.667)
Bartik		0.597***	
		(0.152)	
Nontradable sector			
ΔIPW	-2.052***	-1.550***	-3.645***
	(0.554)	(0.564)	(0.850)
Bartik		0.448***	
		(0.148)	
KP stat			32.51
Full set of controls	√	√	√
Region fixed-effects	√	√	√

Note: $N = 696$. Baseline sample is a balanced panel of 348 employment zones. Outcomes variables are expressed in percentage change over six-year period. All specifications include period fixed effect and log of initial total employment. Robust standard errors are clustered at the employment zone level. *p<.10 ** p<.05, *** p<.01.

1.B Linking trade and employment data

We use data on trade from the website `un.comtrade.org`. The trade data follow the product classification HS 1992 with 6 digit. The data on employment follows the NACE rev 1.1. classification which is equivalent to the 4-digit CPA 2002 classification. To convert HS-1992 6-digit codes into NACE 4-digit codes, we do the following:

1. We use a file available on `un.comtrade.org` to map HS-1992 into HS-2007.
2. We use one file available on `http://ec.europa.eu/eurostat/ramon` to map HS-2007 into CPA 2002. The latter maps n-to-one to the NACE rev 1.1.
3. We obtain a correspondence mapping from HS-1992 into NACE rev. 1.1. All HS-1992 6-digit goods that are not uniquely mapped with a NACE 4-digit sector divided across NACE sectors using weights reflecting each sectors initial "importance" in the economy (the weights are the employment share in 1995). Non-uniquely mapped goods account for about 9 %, 8 % and 6% of French imports from China for years 1995,2001 and 2007 respectively.

Table A3: Total French Imports: Uniquely and Non-Uniquely Mapped ($ millions)

	Total	Uniquely	Non-uniquely	Ratio
1995	5,950	5,385	565	.095
1996	6,833	6,236	597	.0873
1997	7,495	6,874	621	.0828
1998	8,178	7,505	673	.0823
1999	8,943	8,237	706	.079
2000	10,515	9,670	845	.0803
2001	10,450	9,635	815	.078
2002	11,380	10,506	874	.0768
2003	15,850	14,660	1,190	.0751
2004	21,398	19,871	1,527	.0714
2005	26,748	24,737	2,011	.0752
2006	30,968	28,652	2,316	.0748
2007	39,533	37,015	2,518	.0637

Note: A product code HS-1992 is considered "uniquely mapped" if according it is uniquely mapped according to our mapping HS-1992→HS-1996 → NACE built using the conversion tables from RAMON (HS 2007 to CPA) and Comtrade (HS1992 to HS2007). Each observation for product HS1992 that cannot be uniquely mapped to a NACE sector is dropped (either because there is no mapping or the mapping is not unique). Column (4) displays the the trade value non-uniquely matched products as the share of overall imports French imports from China. Trade values are expressed in current dollars.

1.C Formulating aggregate predictions on the basis "deviation from cross-sectional average" estimates

Without an assumption regarding period specific intercept (written η_t in Equation 2.3), finding a negative estimate for β does not allow to distinguish which of the two following statements applies:

1. Trade with China boosts manufacturing employment in France overall but relatively less in places with high exposure to direct Chinese competition.

2. Trade with China reduces manufacturing employment in France overall but relatively more in places with high exposure to direct Chinese competition.

Therefore one cannot make aggregate predictions from micro-estimates without some additional assumption regarding how the period-specific intercept which captures nationwide general equilibrium effects relates to the evolution of Chinese imports competition.

The goal of this appendix is illustrate the the difference between the set of assumptions required to obtain consistent estimate of individual effect and the set of assumptions required to make aggregate predictions. To do so, it focus on a simple univariate linear data generating process.

Let us consider the following data generating process:

$$y_i = \mu + x_i\beta + u_i \tag{1.10}$$

We assume $E(x_i u_i) = 0$, so that we sidestep issues associated with endogenous regressor. For simplicity and without loss of generality, we also assume that $E(x_i) = 0$. Realization of x_i and u_i are assumed to be i.i.d.

Now, we specify μ as being a function of the (un-weighted) sample mean $\overline{x}_N$.[41]

$$\mu := \mu(\overline{x}_N) = \delta + \overline{x}_N\alpha \tag{1.11}$$

So while δ and α are fixed parameters, μ is itself a random variable. Note also that $\overline{x}_N\alpha$ introduces a rough notion of spill-overs between units i's. Hence while x_i and u_i are i.i.d, y_i are not.[42]

In this context, making an "aggregate prediction" is to analyze of the conditional expectation of the cross-sectional average of y_i's conditional on that of x_i's, i.e. $E(\overline{y}_N|\overline{x}_N)$. Under the stated assumption, we have:

$$\frac{\partial E(\overline{y}_N|\overline{x}_N)}{\partial \overline{x}_N} = \alpha + \beta \tag{1.12}$$

This expression contrasts with the individual effect of x_i on the conditional expected value of y_i.

$$\frac{\partial E(y_i|x_i)}{\partial x_i} = \alpha \times \frac{\partial E(\overline{x}_N|x_i)}{\partial x_i} + \beta = \frac{\alpha}{N} + \beta \tag{1.13}$$

Throughout the paper, we include period-fixed effect, which in the context of the DGP above, is akin to using an OLS estimator in deviation from the cross-sectional average.

$$\hat{\beta}_{OLS} = \frac{\sum_{i=1}^{N}(y_i - \overline{y}_N)(x_i - \overline{x}_N)}{\sum_{i=1}^{N}(x_i - \overline{x}_N)^2} \tag{1.14}$$

Substituting Equations 1.10 and 1.11, we obtain:

$$\hat{\beta}_{OLS} = \beta\frac{\sum_{i=1}^{N}(x_i - \overline{x}_N)(x_i - \overline{x}_N)}{\sum_{i=1}^{N}(x_i - \overline{x}_N)^2} + \frac{\sum_{i=1}^{N}(u_i - \overline{u}_N)(x_i - \overline{x}_N)}{\sum_{i=1}^{N}(x_i - \overline{x}_N)^2} = \beta + \frac{\sum_{i=1}^{N}(u_i - \overline{u}_N)(x_i - \overline{x}_N)}{\sum_{i=1}^{N}(x_i - \overline{x}_N)^2} \tag{1.15}$$

[41] The use of a weighted mean does not change the nature of the argument.

[42] It is a special case of a spatial lagged in x model where the weighing matrix contains only $1/N$ as entries. It is straightforward to extend the argument to more general weighing matrix, allowing for instance $\overline{x}_N\alpha$ to be a weighted rather than a simple average, as long as the weights are not i-specific, therefore ensuring that aggregate effects can be properly "taken out" by demeaning.

Clearly under the assumption $E(x_i u_i)$, we have plim $\hat{\beta}_{OLS} = \beta$.

It suggests that in the case where the common intercept is a function of the mean of the regressor, using cross-sectional variation in deviation from the aggregate trend only allows to obtain an approximation of $\frac{\partial E(y_i|x_i)}{\partial x_i}$. However, as N grows to infinity,[43] we have: $\frac{\partial E(y_i|x_i)}{\partial x_i} \longrightarrow \beta = \text{plim } \hat{\beta}_{OLS}$. Hence assuming $\alpha = 0$ is not required to retrieve consistent estimates of $\frac{\partial E(y_i|x_i)}{\partial x_i}$.

However, $\frac{\partial E(\overline{y}_N|\overline{x}_N)}{\partial \overline{x}_N}$ does not become arbitrarily close to β as N grows large. Moreover, α cannot be identified using an estimator based on deviations from the cross-sectional average. As a result, when making aggregate predictions, it becomes necessary to make an assumption on the sign and magnitude of α. I assume $\alpha = 0$ throughout the paper, which appears to lead to conservative predictions as one would expect negative spill-overs across local labor markets, particularly given the absence of reallocation of population across local labor markets (which would be an important channel for positive spill-overs, see footnote 19).

1.D Details on the occupation classification

The DADS postes dataset documents occupation at the two-digit level consistently over the period for 18 occupations which accounts for an overwhelming share of overall employment and hours worked (respectively 93.94 and 94.09 % in 1995). I must exclude workers doing an apprenticeship or an internship from the sample as it is not possible to match them consistently to a two digit occupation over the period. Table A4 displays the different occupations, their labels and their initial share of employment.

In the remainder of the appendix, I detail some example of jobs included in the main occupations that are not self-explanatory.[44]

The first category numbered 68 labelled "Unskilled nonspecialized workers" includes such jobs as cleaners, unskilled construction workers.

Category 56 "Personal service workers" includes professions such as hairdressers, waiters or hotel workers.

Category 55 includes different types of more or less specialized basic retail jobs (e.g. floor-level sales person, cashier).

[43] I focus on asymptotic results because in the paper, estimation is carried out using instrumental variable estimator which has desirable properties asymptotically but not in finite samples. Moreover, given the large number of observations (348 by cross-section) it seems reasonable to consider the case where $\alpha/N \approx 0$.

[44] More details can be found (in French) at the following link: `http://www.insee.fr/fr/methodes/default.asp?page=nomenclatures/pcsese/pcsese.htm`. While the dataset DADS documents occupation at the 4-digit levels, these detailed definition are not consistent overtime, thus constraining us to work at the 2 digit level.

Table A4: Information on occupations

Code	Label	Rank	Share 1995 (%)	Broad-Skill Category
68	Unskilled nonspecialized workers	18	5.15	Low
56	Personal service workers	17	5.48	Low
55	Retail workers	16	6.91	Low
67	Unskilled industrial workers	15	11.67	Low
63	Skilled nonspecialized workers	14	9.07	Low
53	Security workers	13	0.86	Low
64	Drivers	12	4.24	Low
65	Skilled warehouse and handling workers	11	2.33	Low
54	Administrative workers	10	12.94	Low
62	Skilled Industrial Workers	9	10.83	Low
43	Health and social work intermediate profession	8	2.44	Intermediate
47	Technicians	7	4.26	Intermediate
46	Intermediate administrative workers	6	10.96	Intermediate
48	Foremen	5	2.63	Intermediate
35	Information and entertainment professionals	4	0.92	High
34	Scientific professionals	3	4.73	High
38	Engineers and technical professionals	2	0.95	High
37	Sales executives	1	3.61	High

Category 67 “Unskilled industrial workers” includes mainly jobs in the manufacturing sector (production workers in chemistry, textile etc.) and some in non-manufacturing activities, for instance unskilled warehouseman in transport industry or in private postal services.

Category 63 “Skilled nonspecialized workers” includes skilled manual jobs such as plumber, electricians, food related activities (bakers, butchers) and mechanics.

Category 65 “Skilled warehouse and handling workers” includes skilled drivers and warehousemen, for instance those operating heavy equipment (e.g. large trucks).

Category 62 “Skilled industrial workers” includes skilled manual jobs mainly pertaining to manufacturing activities (e.g. welder working with specific types of metals, certain assembly line workers) or construction (e.g. team leader in civil engineering, worker specialized in specific types of concrete).

Category 47 “Technicians” includes many technical professions predominantly employed in manufacturing (e.g. industrial designer) or in construction (e.g. projector, building surveyor).

Category 46 “Intermediate administrative work” includes jobs such as representatives and travelling salesman.

Category 48 “Foremen” includes mainly mid-level managers jobs in manufacturing, in construction and tertiary activities (store manager).

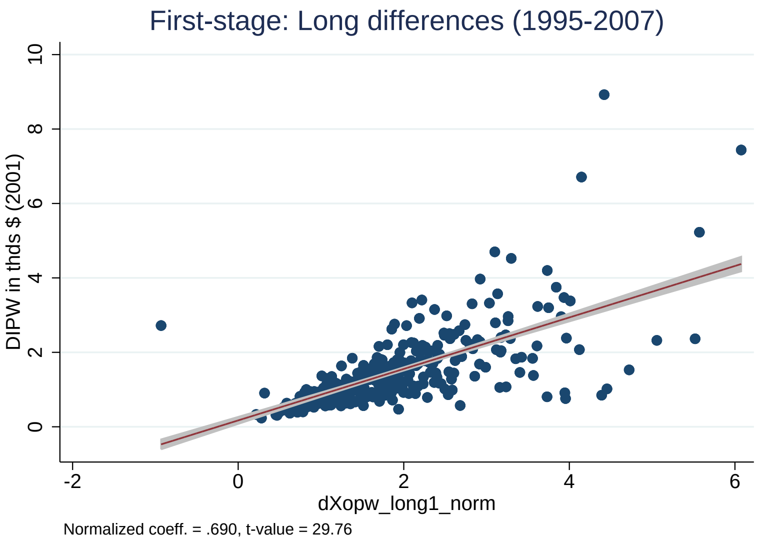
The column broad skill category refers to the categories used in the Table OA3 and is based on a 1-digit classification.

1.E Online Appendix

This online appendix includes additional results and robustness checks.

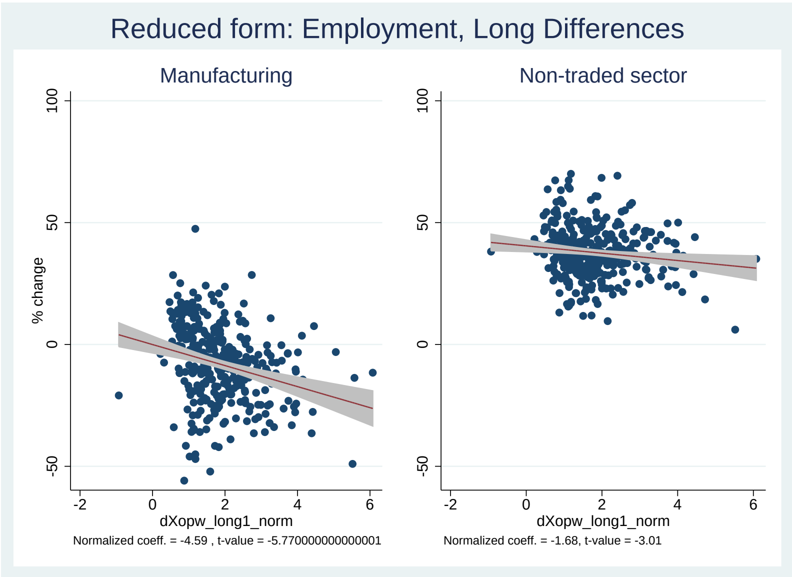
1.E.1 Additional figures

Figure OA1: First stage: Long-differences



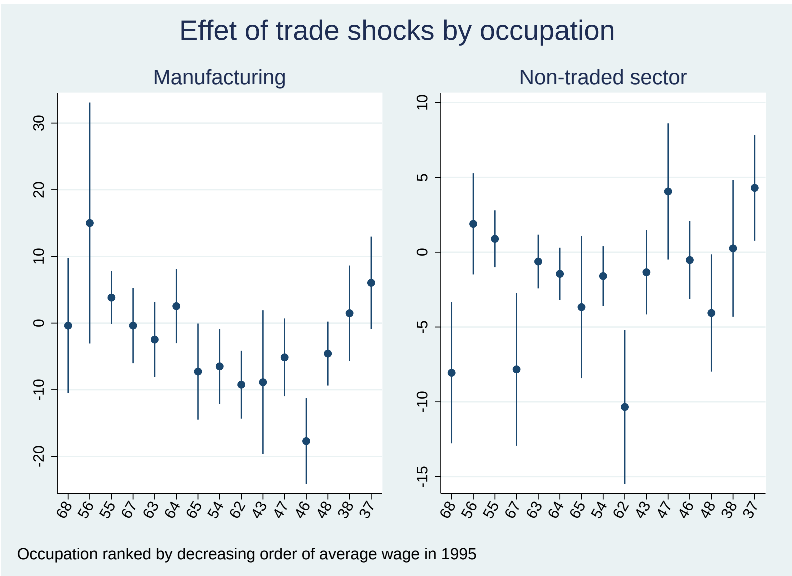
Note: Each dot represents a employment zone over the 12 year period 1995-2007.

Figure OA2: Reduced form: Long-differences



Note: Each dot represents a employment zone over the 12 year period 1995-2007.

Figure OA3: Employment growth by occupations



Labels on the horizontal axis refer to the 2-digit occupations. See Appendix Section 1.D for more details on the occupational classification.

1.E.2 Additional tables

Robustness checks

The table below presents two robustness checks mentioned in foonote 29: (a) using a different zoning (2010 definition of the employment areas), (b) dropping the Parisian region from the sample.

Table OA1: Results (i) using 2010 employment zone definition, (ii) omitting Parisian Region

	ZE 2010		No Parisian Region	
	IV	IV: Hours	IV	IV: Hours
	b/se	b/se	b/se	b/se
Manufacturing				
ΔIPW	-6.394***	-6.233***	-5.258***	-4.878***
	(1.728)	(1.682)	(1.707)	(1.706)
Non-tradable sector				
ΔIPW	-4.054***	-1.894**	-3.905***	-1.837**
	(0.873)	(0.770)	(0.943)	(0.820)
KP stat	30.82	30.82	27.79	27.79
Full set of controls	√	√	√	√
Region fixed-effect	√	√	√	√

Note: For the ZE 2010 panel (two left columns), $N = 304$. More details on the ZE definition are available at http://www.insee.fr/en/methodes/default.asp?page=definitions/zone-emploi.htm. For the two right columns: $N = 308$.

Other tables

Table OA2: Direct estimates of local multipliers

	(1) LM: OLS	(2) RF: ΔIPW^o	(3) FS: ΔIPW^o	(4) IV: ΔIPW^o	(5) RF: Bartik	(6) FS: Bartik	(7) IV: Bartik
$\Delta log(tradable)$	0.232***			0.586***			0.671***
	(0.046)			(0.195)			(0.155)
ΔIPW^o		-1.382***	-2.360***				
		(0.354)	(0.655)				
Bartik					0.508***	0.757***	
					(0.161)	(0.158)	
KP stat				12.99			23.01
Full set of controls	√	√	√	√	√	√	√
Region fixed-effect	√	√	√	√	√	√	√

Note: $N = 696$. Baseline sample is a balanced panel of 348 employment zones. Outcomes variables are expressed in percentage change over six-year period. All specifications include period fixed effect and log of initial total employment. Robust standard errors are clustered at the employment zone level. *p<.10 ** p<.05, *** p<.01.

Table OA3: Impact on employment by skill category: manufacturing and non-traded sector

	(1) Low Skill b/se	(2) Intermediate Skill b/se	(3) High skill b/se	(4) Low Skill b/se	(5) Intermediate Skill b/se	(6) High skill b/se
Manufacturing						
ΔIPW	-3.638**	-13.024***	-2.631	-3.360**	-13.481***	-2.655
	(1.681)	(3.159)	(3.603)	(1.523)	(3.170)	(3.643)
Non-traded sector						
ΔIPW	-5.909***	-2.980*	5.686**	-4.987***	-1.899	4.950**
	(1.420)	(1.662)	(2.513)	(1.099)	(1.386)	(2.400)
KP stat	32.51	32.51	32.51	32.51	32.51	32.51
Controls (see notes)	√	√	√	√	√	√
Region fixed-effect				√	√	√

Note: $N = 696$. Baseline sample is a balanced panel of 348 employment zones. Outcomes variables are expressed in percentage change over six-year period. All specifications include period fixed effect and log of initial total employment. Robust standard errors are clustered at the employment zone level. *p<.10 ** p<.05, *** p<.01. Control variables include: initial share of manufacturing jobs, of female employees, of production employees, of foreign-born employees and of college educated employees. Skill categories are based on occupation. Clerk and unskilled production workers are considered low skill occupations. Intermediate professions and low-rank managers are considered medium skill occupations, while intellectual professions, senior management are considered high-skill occupations.

1.E.3 The sensitivity of aggregate predictions to temporary workers

A word of caution is warranted regarding the role of temporary workers (included in the non-traded sector) in the computation of the aggregate effects of trade shocks on traded and non-traded employment. A subset of the non-traded sector include temporary workers some of which are directly employed in the manufacturing sector. The data does not allow us to distinguish between temporary workers employed in the traded from those employed in the non-traded sector. Moreover identifying “temporary workers” can only be done through the NACE rev 1.1 code “74.50” entitled “Labour recruitment and provision of personel”. This code includes workers directly working for temporary work agencies as well as firms providing “human resources” services (HR consultants etc.). Nevertheless I need to rely on this definition to look into this issue. Therefore I consider all workers employed in the NACE 74.50 as temporary workers.

Temporary workers employed in manufacturing are likely be affected by Chinese imports competition. However these potential job losses are directly related to spill-over effects but should instead be included in the direct impact of imports competition. Although data does not allow to distinguish temporary workers employed in manufacturing from those employed in the non-traded sector, there exists external information on the nationwide share of temporary workers employed in traded versus non-traded sector. In 1996, roughly 4% of workers in the traded sector were temporary workers DARES, 1997.

Using this external information, I can reallocate temporary workers from the non-traded to the traded sector (at the national level) and modify the numbers on the aggregate job destruction accordingly. The ratio is defined as follow $r^T := \frac{L^T_{temp}}{L^T_{perm}+L^T_{temp}} = 4\%$ where L^s_i represent initial employment in sector $s = traded\ (T),\ non - traded\ (N)$ in category $i = permanent,\ temporary$. Rearranging the formula we get:

$$L^T_{temp} = \frac{r^T}{1 - r^T} L^T_{perm} \tag{1.16}$$

In the DADS database all workers classified in the traded sector are permanent workers ($L^T = L^T_{perm}$) and all temporary workers are in the non-traded sector ($L^N_1 = L^N_{perm} + L^N_{temp} + L^T_{temp}$). Using equation 1.16, we define traded employment (L^T_1) and non-traded employment (L^N_1) according to the formula:

$$L^T_1 = L^T_{perm} + \frac{r^T}{1 - r^T} L^T_{perm}$$
$$L^N_1 = L^N_{perm} + L^N_{temp} + L^T_{temp} - \frac{r^T}{1 - r^T} L^T_{perm}$$

Applying the average impact on growth rate to L^T_1 and L^N_1 instead of L^T and L^N only changes mildly the overall picture. Over the second period (2001-2007), imports competition from China is found to have destroyed 95,000 jobs (instead of 88,000) in manufacturing, 186,000 (instead of 193,000 in the non-traded sector). Fulling ignoring temporary workers (i.e. people working in the sector 74.50 are dropped from the overall job count), the impact on the non-traded sector decreases slightly further down to to 170,000 jobs.

Chapter 2

The Impact of Exposure to Low-Wage Country Competition on Votes for the Far-Right: Evidence from French Presidential Elections

2.1 Introduction

Extreme right populist parties (ERPP) have received large electoral support in many West European countries since the 1980s (Ignazi, 2003).[1] These repeated successes, continued over the 2000s and arguably magnified by the crisis, contrast with the experience of radical left which has gone through a relative decline across Europe since 1989 (March and Mudde, 2005). Given the weight of the far-right in the political debate and the radical measures it proposes to implement, understanding the causal factors behind its electoral success appears an important endeavor.[2]

Hostility to immigration is one of the traditional defining features of ERPP (Kitschelt, 1995). Immigration has accordingly been the focus of a large share of the empirical research on the far-right. While many papers have documented a correlation between hostility to for-

[1] Ignazi (2003) p.200 documents that for Austria, Italy, France, Belgium, Norway, and Switzerland such parties have scored about 10 % of votes in national parliamentary elections during the 1990s.

[2] Regarding the influence of the far-right, Bale et al. (2010) document and attempt to explain the how social democratic parties adapt to the rise of far-right populist parties in Northern European countries. Williams (2006) studies and provides evidence regarding the role of far-right as a agenda-setter in France, Germany, and Austria.

eigners and support for the far-right,[3] Halla, Wagner, and Zweimüller (2013) provide the first causal estimate of immigration on the electoral success of ERPPs, for the case of Austria. Controlling for local economic conditions and instrumenting for current level of immigrants' presence at the neighborhood level with historical settlement patterns, they find a substantial positive impact of immigration on the electoral share for the FPO (the Austrian ERPP).

Immigration however is only one dimension of ERPPs' programs. Over the 1990s, ERPPs across Europe have been increasingly vocal detractors of globalization (Zaslove, 2008). That trend has pursued over the 2000s. Academic research has increasingly investigated and supported the notion that the far-right's appeal grew among those who considered themselves as losing out from rising economic integration (Kitschelt, 2007). The transformation of the electoral basis as well as of the economic agenda of the far-right is perhaps most evident in France. The National Front (FN henceforth) has increasingly focused on economic precariousness, pointing at globalization as the main culprit behind the difficulties faced by the workers while proposing increasingly "leftist" policies (Ivaldi, 2013). Meanwhile it has been the main recipient of "working-class" votes during the presidential elections of 2002, 2007 and 2012.[4] While a large literature has described the progressive "proletarization" of ERPPs over the 1990s and 2000s, and analyzed the social and economic correlates of electoral support for the far-right,[5] there is still little knowledge about the causal effect of local economic conditions on its electoral success. This paper aims at filling this gap by treating more carefully the issue of endogeneity of economic shocks with respect to voting outcomes as well as improving the measurement of such shocks.

In this paper, we estimate the impact of trade-induced economic shocks on the local electoral success of the far-right in France.[6] An obstacle to studying empirically the impact of international competition on voting outcomes lies in the difficulty to obtain a measure of imports competition with enough variation without resorting to cross-country regressions from which it is hard to draw causal conclusions.[7] We adopt a different identification strategy than

[3]There are roughly two strands in this literature. The first uses cross-country regressions relating immigration share to electoral success of the far-right. The second uses survey data where respondents document their opinion their opinion about immigration and their political preferences. See

[4]Working-class employees ("ouvriers" in French) made up about 25 percent of the working-age population as of 2005 (INSEE, 2013).

[5]For instance, Jackman and Volpert (1996)'s study finds cross-country evidence that ERPP benefit from high unemployment. This seminal paper has started a large literature on the topic.

[6]This paper thus reverses the focus of Halla, Wagner, and Zweimüller (2013) in the sense that it concentrates on local economic shocks while accounting for the local presence of foreigners. Halla, Wagner, and Zweimüller (2013) proceed the other way around and are not particularly interested in the effect of economic shocks.

[7]For instance, Levine and Zervos (1993) provide, in the context of "growth empirics", an exposition of the general statistical and conceptual issues associated with cross-country empirical studies.

the existing literature by constructing a panel of small geographical units ("cantons", which we will refer to as communities) for France. We circumvent the issue of measurement by interacting the initial sectoral composition of each community with nationwide sector-specific imports originating from low-wage countries , thus obtaining within-country cross-sectional variation in imports competition exposure (This approach was pioneered by Autor, Dorn, and Hanson (2013)). We use a model in first-difference with region-period fixed-effects, and accordingly rely on within-region cross-sectional variation in changes in imports competition and far-right voting share between each election to estimate the parameter of interest.

We find evidence of a positive and significant modest effect of imports competition. On average, an increase in *change* in imports-per-worker (denoted as ΔIPW) of \$ 1000 causes a rise in 0.4 percentage point in *change* in the far-right share of votes between two elections. Due to rising variation in exposure to imports competition over time, this constant coefficient translates into an increasing normalized effect overtime. For the period 1995-2002, 2002-2007 and 2007-2012, increasing of one period-specific standard deviation in imports-exposure causes support for the far-right to rise by, respectively, 2.7 %, 6.6 % and 8.98 % of a standard deviation. Allowing the estimated coefficient to vary over time, we find mixed evidence of an increasing effect overtime. We assess the robustness of our results with respect the local share of immigrants, a well-documented factor of support for the far-right, and conclude that omitting a local measure of migrant presence introduce little bias in our estimate.

France, as many countries of Western Europe, has experienced the increasing electoral success of the far-right since the early 1980s. The far-right was absent of the 1981 French presidential election. It first made an impressive breakthrough in 1984 during the EU elections when it received 11 percent of votes.[8] Ever since, it scored above 14 percent in all but 2007 presidential elections with an average score around 15 percent.[9] The last election 2012 saw the highest score ever achieved by the Front National, at 17.9 percent, 1 percentage point above the score realized by Jean-Marie Le Pen in 2002 (when he qualified for the second round). The period we cover (1995 to 2012) is particularly interesting as it has been marked by a programmatic shift in the economic discourse of the FN. Ivaldi (2013) shows that it has recently moved away from free-market oriented programs towards redistributive,

[8]The noted electoral success of the *new* far-right was during a local by-election in 1983, where the FN and other mainstream right-wing parties formed a coalition to defeat the socialist candidate during the second round (Gaspard, 1990).

[9]During the 2007 election, Nicolas Sarkozy managed to divert a large share of FN voters (Mayer, 2007). He did so by stressing issues usually dear to the core FN electorate, notably national identity and immigration control. However, there is a suspicion that this shift towards the right of the mainstream right parties facilitated, in the medium-run, the so-called de-demonisation of the FN (Mayer, 2013b).

protectionist and social policies. Moreover, economic issues have gained more prominence in the party's discourse. French presidential elections lend themselves well to the study of the impact of local economic conditions on voting behavior as voters across the entire country choose from same set of candidates. This is unlike legislative elections where parties appoint a different candidate in each electoral district, which could lead to confound the effect of trade shocks with that of parties' candidate choice of an optimal candidate for a given district.

The focus on low-wage country imports competition is justified by its tremendous growth over the past two decades. Chinese exports in particular have been growing in constant value at an astounding pace of 15 percent per year. The potential labor market effects of such steep progression has given rise to a heated political debate in most high-income economies. This rapidly rising economic openness has occurred concomitantly to the "proletarianization" of the European far-right parties (both in terms of electoral support and programmatic focus). By documenting some of the political consequences of globalization, we contribute to inform the debate on the cost and benefits of increasing economic integration with low-cost countries.

Our paper is related to a large empirical literature on the contextual factors of support for the far-right. As pointed out by Arzheimer (2009): *"Research on the voters of the extreme right in Western Europe has become a minor industry..."*. However, although there exist many studies relating some measure of economic hardship to far-right voting, there is, to our knowledge, no previous study looking at the impact of international competition on the electoral support for ERPPs that deal explicitly with the issue of endogeneity. This is surprising given the impressive increase in imports penetration by low-wage countries (most notably China) over past two decades and the large consequences this process has had on the labor market markets in high-income countries, especially low-skill workers, i.e. the category the most inclined to support the far-right.[10] The paper that falls the closest to ours in terms of substantive question (rather than method) is Swank and Betz (2003). The authors assess the effect of trade-openness on the electoral success of ERPPs and investigate how this effect varies with welfare state institutions (that are likely to mitigate the redistributive impact

[10] There is a large literature regarding the labor market effect of low-wage country competition. Most relevant to us is the work by Autor, Dorn, and Hanson (2013) which shows that locations intensive in Chinese-import competing industries in the USA tend to have lower employment rate. This effect outside of manufacturing affects primarily low-skill workers. For Europe, Bloom, Draca, and Reenen (2011) show that firms facing strong Chinese competition tend innovate more but cut employment, especially among production jobs. This study backs the notion that a share of the observed skill-biased technical change is in fact induced by trade with low-wage countries and finds no evidence of such phenomenon associated with other high-income countries. Mion and Zhu (2013) use data on Belgian manufacturing firms and show that Chinese imports are particularly disruptive, due perhaps to their high degree of sophistication conditional on Chinese wage rates (Rodrik, 2006) and cause firm to reduce employment, upgrade the average skill of their workforce and increase their share of non-production workers.

of trade liberalization). They use cross-country variation in trade-openness (measured as trade-to-GDP ratio), ERPP voting shares and welfare-state institutions within Western Europe during the 1980s and 1990s. They find no direct effect of trade-openness and a negative interaction effect between high degree of social protection and trade-openness, supporting the notion that increase in public spending allows to compensate losers from trade liberalization (Rodrik, 1998). It is difficult to draw causal statements about the effects of trade shocks from cross-country data as countries differ in many other dimensions and policies. Hence, while Swank and Betz (2003) include many control variables, it remains unclear that no not-controlled-for mechanisms drives differences in aggregate ERPP electoral share. While working-class/production worker status predicts strongly support for the FN, Oesch (2008) shows that electors of the Front National, for the year 2002, cared mostly about social issues (cultural homogeneity) rather than economic issues. This result is confirmed by Mayer (2013a) for the 2012 election. Naturally, given the issues associated with survey data regarding the self-identification as far-right voters, it seems plausible that only the most radicalized part of the electorate self-identifies themselves as far-right voters thus resulting in a selected sample.[11] Our findings suggest that the documented correlation between working-class status and support for the FN does not purely reflect the heterogeneity of preference for cultural homogeneity across socio-economic categories, but that instead economic shocks have been, by themselves, causing a rise in the support for the far-right.

This paper is also related to the literature on the impact globalization on voting outcomes. A large of share of this literature is framed within the Heckscher–Ohlin or Viner-Ricardo models and aims at testing their predictions in terms of attitude towards trade-openness using survey data. Scheve and Slaughter (2001) look at whether individuals' skills and/or industry of employment matter for trade-policy preferences and finds support for the HO model in that factor-type is a more decisive determinants of trade-policy preferences than industry of employment. They also find that, independently of factor-type or industry of employment, homeowners in areas whose industries are intensively exposed to international competition are more likely to hold protectionist view. This last finding is interesting in that it suggests that beyond people directly employed in imports-competing industries, communities are susceptible to vote for anti-globalization parties through the home-owning channel, industrial decline leading ultimately to lower land prices. Mayda and Rodrik (2005) roughly confirms the predictions of a standard HO model for skilled individuals but not for low-skill ones. However, measurement error in the industry of employment is a large

[11] For instance, as shown in Oesch (2008), 11.4 percents only of the respondents to the 2002/2003 wave of the European Social Survey acknowledged to have voted for the Front National while the FN gathered 16.9 percents of valid votes during the first round of the presidential election of May 2002.

issue in both of these studies as, because it is not directly documented, it has to be inferred from individuals'occupation and education which limits the reliability of their test of the specific-factor model. Interestingly for our purpose, they find that even after controlling for socio-economic situation, individuals more hostile to trade-openness tend also to hold nationalistic/chauvinistc opinion. This finding is corroborated by Mansfield and Mutz (2009). This could imply that following a trade-shock, impacted voters deciding to vote for a protectionist party are likely to support far-right party.

More closely related to our paper is Margalit (2011) which analyzes electoral outcomes at the local level. The author uses an innovative measure of trade-induced layoffs in the United-States that he constructed by collecting the number of assistance requests under the Trade Adjustment Act for each county from 1996 to 2004. He then looks at the impact of such requests on the on the share of votes for the incumbent in the 2004 Presidential elections. The author finds that trade-induced job destruction has a negative effect on the incumbent share. Moreover, he finds that this effect exists above and beyond local economic conditions as it is still present after controlling for local unemployment. We depart from this paper by looking at the electoral effect of a general measure of trade-exposure (as opposed to focusing on layoffs). Moreover, we study on electoral support for the FN, a platform with clear anti-immigration and anti-globalization platform as opposed to looking at incumbent effect.

The rest of the paper is organized as follow. Section 2.2 presents some background on the far-right in France and introduce the data and measurement of imports-exposure. Section 2.3 presents the specification and some descriptive evidence. Section 2.4 shows the results and discusses them. The conclusion follows.

2.2 Data and measurement

We intend to measure the impact of exposure to imports competition from low-wage countries on the propensity of communities to vote for the far-right. Hence the paper resorts to data on vote, trade and local sectoral composition of employment. The data on votes come from the Interior Ministry and are at the municipality level (there are about 36,000 municipalities in France). The DADS (Déclaration annuelle de données sociales) dataset is an exhaustive matched employer-employee administrative dataset containing information for all employees of the non-farm private sector in France. We use it to measure the sectoral structure of local employment. Publicly available data on bilateral trade flows are used to measure imports (UN Comtrade). More details on the mapping of products (Comtrade) into sectors (DADS) is provided in the appendix 2.A.

Formally, we start by computing a index of imports exposure, called "Imports-per-Worker" according the following formula:

$$\Delta IPW_{it} = \frac{1}{L_{it}} \sum_{s} \frac{L_{ist}}{L_{st}} \Delta M_{st} \tag{2.1}$$

where M_{st} stands for imports from low-wage countries to France for sector/period st, L_{st} is equal to employment in France for sector/period st, L_{it} is total employment in area/period it, and Δx_t refers to changes in variable x between periods t and $t+1$.

We use the list of manufacturing intensive low-wage countries as established by Auer, Degen, and Fischer (2013). The list includes six-countries: China, India, Malaysia, Mexico, the Philippines and Thailand.[12]

We improve measurement by explicitly accounting for interdependence between cantons by using the shares of workers initially commuting from one canton to another. That is, for each canton i we compute a weighted average of all cantons imports exposure (ΔIPW_{jt} for $j = 1, ..., N$ when N is the number of cantons) using commuting shares between canton i and j during the initial year as weights. Formally, the index to be used in the estimation is defined as the following:

$$\Delta \overline{IPW}_{it} = \sum_{j} \eta_{ijt} \Delta IPW_{jt} \tag{2.2}$$

, where η_{ijt} is the share of workers living in canton i and working in canton j at time t (beginning of the period).

2.2.1 Descriptive statistics and geographical overview

Table 2.1 presents some community-level statistics. The median electoral size of a "canton" is 7,000 registered voters. The change in FN voting share between the first rounds of 1995 and 2012 is 3.18 percentage point on average with substantial variation across areas (standard deviation on 4.70 percent). The simple and commuting-adjusted measures of imports exposure have roughly the same mean[13], the simple measure has a much larger standard

[12] Auer, Degen, and Fischer (2013) is a extension to Europe of the work on the deflationist impact of low-wage country imports for the United-states by Auer and Fischer (2010). The results are not very sensitive to the choice of low-wage country group. We expand the definition and use Bernard, Jensen, and Schott (2006) with roughly similar results. INCLUDE RESULTS IN APPENDIX.

[13] The reason why ΔIPW and $\Delta \overline{IPW}$ do not exactly have the same mean is because the statistics displayed here are computed with weights on each communities equal to the number of registered voters, if

deviation than commuting-adjusted measure. It stems from the fact that it is computed as a convex combination of the simple measure. In economic terms, it conveys the notion that employment opportunities do not vary as dramatically across space as the local presence of jobs, because of the possibility of workers to commute to different communities to take up jobs.

Figure 2.1 shows the spatial distribution of FN votes. The blue lines corresponds to province (or département) boundaries. The left panel (a) show the vote in 1995. The strongest concentrations of FN votes in the South-East and along the Mediterranean coast, around Lyon, in the East (Alsace and Moselle) and North and East of Paris. A regression of FN shares on a full set of province dummies yield an R-square of 70 percent, suggesting there is not much within-province variation. The right panel (b) displays the change in local shares of FN votes between 1995 and 2012. The average increase is 3.8 points but there is a lot of variation around the mean. The most impressive gains were realized in the very North and in the center and West of France. Gains in the East (Alsace) were limited but 1995-level was very high as most communities there had FN shares comprised between 20 and 30 percent in 1995. The R-square of a regression of *change* in FN voting share on a full set of province dummies is equal to 50 percent implying that within-province variation is higher in changes than in levels.

Figure 2.2 displays the spatial distribution of the imports-exposure index. Panel (a) shows the simple index while (b) shows the commuting-adjusted index. A comparison of the Southern most province along the Western side between Panel (a) and (b) illustrates the difference between the two measure. While Panel (a) shows only a couple of very exposed communities (colored in dark red) surrounded by unexposed ones (colored in light pink or white), adjusting for commuting patterns in Panel (b) diffuses the exposure and smoothes it, with many communities facing intermediate level of exposure (colored in darker shades of pink).

The crux of our empirical approach is to relate the within-province (entities delimited by the blue lines) variation from Panel (b) Figure 2.1 to within-province variation from Panel (b) Figure 2.2.

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the weights where the total employed salaried population, the mean would identical.

2.3 Empirical approach

2.3.1 Baseline specification

We adopt the following specification:

$$\Delta FN_{it} = \beta \Delta \overline{IPW}_{it} + X'_{it}\delta + \gamma_{d(i),t} + \varepsilon_{it} \tag{2.3}$$

,where it refers to canton i during period t to $t+1$. ΔFN_{it} is the change in voting share of the FN. $\Delta \overline{IPW}_{it}$ has been defined above. X_{it} is a set of demographic controls from the Census. It includes: total population, a set of share of total population for sex s and age category a with 7 different age categories (0-14 year old, 15-29 year old, ..., 90 year old and more). The data come from the Census of 1990 (associated with the 1995-2002 period), 1999 (associated with the 2002-2007 period), 2006 (associated with the 2007-2012 period). ε_{it} is an idiosyncratic shock that we assume uncorrelated with the regressors. The term $\gamma_{d(i),t}$ represents a province-period fixed effect.

Adopting a first-difference model allow us to control for time-invariant heterogeneity. Given the inclusion of period × province (or département in French) dummies, the identifying variation under this specification comes from *within-province cross-sectional* variation in *changes* in exposure to low-wage country imports. Yet, there is a suspicion that domestic nation-wide sectoral shocks might driving imports from low-wage countries. To the extent that these shocks might also be affecting or correlated to support in the far-right, they will bias the OLS estimates of Equation 2.3. In the next subsection, we present the instrumental variable approach we follow to deal with this issue.

2.3.2 Instrumental variable approach to deal with the endogeneity of sectoral imports

Nation-wide sector specific shocks (supply or demand) are partly driving the amount of goods imported in France from abroad. If these shocks affect simultaneously sectoral imports and votes for the far-right, through their impact on labor market outcomes for instance, OLS estimates will be biased. As sectoral shocks are likely to affect each community differently depending on local characteristics, period fixed-effects do not absorb such shocks. In this section, we formalize this argument to motivate and explain our instrumental variable approach.[14]

[14] This approach was pioneered by Autor, Dorn, and Hanson (2013).

Consider the following data generating process (for simplicity, we ignore covariates, the cross-commuting shares weighing and omit the time subscript):

$$\Delta FN_i = \alpha_0 \Delta IPW_i + e_i \tag{2.4}$$

We recall the definition of ΔIPW_i.

$$\Delta IPW_i = \sum_i \frac{L_{is}}{L_i}\frac{\Delta M_s}{L_s} = \sum_s \theta_{is}\frac{\Delta M_s}{L_s} = \sum_s \theta_{is} m_s$$

The share of employment in sector s in location i is denoted θ_{is} and m_s is the change in imports to initial employment ratio for sector s. For simplicity, we consider it as a parameter (i.e. non stochastic) here.
We consider the case where the error terms e_{it} is composed of **(i)** a weighted sum of nation-wide sectoral supply and demand shocks (which we denote w_s and x_s respectively) and **(ii)** a proper error term uncorrelated with any other terms included in the regression.

$$e_{it} = a_S \sum_s \lambda_{is} w_s + a_D \sum_s \lambda_{is} x_s + \varepsilon_i$$

where the parameter a_S and a_D determines the sign and magnitude of the impact of supply and demand shocks, respectively, on the changes in the voting share of the FN and λ_{is} is an unobserved term representing the "importance" of sector s in location i (hence it is expected to be highly similar to θ_{is}). We can rewrite equation (2.4) as:

$$\Delta FN_i = \alpha_0 \theta_i' \mathbf{m} + \lambda_i'(a_S \mathbf{w} + a_D \mathbf{x}) + \varepsilon_i \tag{2.5}$$

where $\mathbf{w}$ and $\mathbf{x}$ are vectors respectively containing the nation-wide sector-specific supply and demand shocks, and $\mathbf{m}$ is the vector containing the changes in imports to initial employment ratios. This specification is reminiscent of panel model with interactive fixed-effect (Bai, 2009) in the sense that the unobserved heterogeneity term λ_i is multidimensional (the length of vector λ_i is here equal to the number of sectors in the economy) and is allowed to interact with shocks that are common through the rest of the cross-sectional units.

Hence OLS estimation of the main specification will be biased due the covariance between ΔIPW_i and $\lambda_i'(a_S\gamma + a_D\psi)$ which we can write as:

$$\text{cov}(\theta_i'\mathbf{m}, \lambda_i'(a_S\mathbf{w} + a_D\mathbf{x})) = a_S\theta_i'\text{cov}(\mathbf{m}, \mathbf{w})\lambda_i + a_D\theta_i'\text{cov}(\mathbf{m}, \mathbf{x})\lambda_i$$

We assume that $s \neq s' \Rightarrow \text{cov}(m_s, w_{s'}) = \text{cov}(x_s, w_{s'}) = 0$ which amounts to ignoring cross-sectors relationships (driven for instance by input-output linkages or substitution in consumption between goods). Therefore we get the following expression:

$$\text{cov}(\theta_i'\mathbf{m}, \lambda_i'(a_S\mathbf{w} + a_D\mathbf{x})) = a_S \sum_s \theta_{is}\lambda_{is}\text{cov}(m_s, w_s) + a_D \sum_s \theta_{is}\lambda_{is}\text{cov}(m_s, x_s)$$

We expect the covariance between nationwide unobserved sectoral supply shocks and imports-per-worker ($\text{cov}(m_s, w_s)$) to be negative. When French producers in sector s are subject to a negative supply shock ($w_s < 0$ e.g. mandatory nation-wide reduction in weekly working-time with no reduction in monthly wages), one would expect an increase in purchase in goods s from foreign suppliers, including China and other low-wage countries. That suggests $\text{cov}(m_s, w_s) < 0$. On the other hand, as x_s represents demand shocks, one would expect that $\text{cov}(m_s, x_s) > 0$.

Economic hardship is supposed to increase the support for the far-right, hence we have $a_S < 0$ and $a_D < 0$. According to this framework, the bias introduced by unobserved sectoral shocks could either be positive or negative depending on the relative magnitude of supply and demand shocks and how they affect imports from low-wage country. Because these nation-wide shocks affect each community differently, through to the vector λ_i, including periods fixed-effects does not solve the issue.

We resort to an instrumental variable strategy whereby we instrument actual exports from low-wage countries to France by Chinese exports to a set of high-income countries whose economic cycle is weakly related to that France.[15] The formula for the instrument is the following:

$$\Delta IPW_{it}^o = \frac{1}{L_{it}} \sum_s \frac{L_{ist}}{L_{st}} \Delta M_{st}^o \tag{2.6}$$

where ΔM_{st}^o is Chinese exports to the set of selected other high-income countries. The identifying assumption underpinning the validity of this instrument is that Chinese exports to these countries are independent from domestic shocks in France and that the correlation between French imports from low-wage countries and Chinese exports to these countries is only driven by supply-side improvements in China or common to China and other low-wage countries.

[15] These countries are the same as in Dauth, Findeisen, and Suedekum (2014) and include: Australia, Canada, Japan, New Zealand, Norway, Singapore, Sweden, United Kingdom. We excluded all countries from continental Europe which are part of the euro zone.

2.3.3 Discussion of the treatment effect captured by $\overline{IPW}$

A variable that is closely related to $\overline{IPW}_{it}$ is the Bartik-instrument (Bartik, 1991). It interacts initial sectoral shares with nation-wide sectoral trends in employment growth. The Bartik-instrument is typically used an instrument for local unemployment or shift in local labor demand. For our purpose, i.e. estimating the impact of trade shocks on the voting share of the far-right, we do not think $\overline{IPW}_{it}$ can be used as an instrument for a specific labor market outcome, such as local unemployment rate. Indeed, there are many potential channels other than unemployment through which imports exposure can negatively affect labor market outcomes (downward pressure on wages, increase in working flexibility and rates etc.), thus suggesting that the exclusion restriction is not satisfied. Hence, our estimates should be interpreted as the reduced-form impact of increase in low-wage imports competition. Isolating the causal channels will the object of further research. As illustrated by Margalit (2011), it could be that trade shocks have an electoral impact beyond its impact on the labor market. Previous work with French data suggest that local labor demand shocks likely to push youth unemployment up are associated with rise in crime (Fougère, Kramarz, and Pouget, 2009). This could be another channel.

2.4 Results

2.4.1 Baseline results

We present the baseline results from OLS estimation in Table 2.2. Column (1) display the coefficient for a specification with no other covariates. The introduction of demographic structure roughly halves the estimated coefficients (Column (2)). The introduction of controls for education diminishes further the estimated relationship as can be seen in Column (3) with a point estimate of 0.27. All three estimates are significantly different than 0 at the 1 percent confidence level.

It is no entirely clear at first whether education should be included in our regression. Indeed, it is likely that the share of college workers is affected by trade shocks. There is an important literature documenting the mobility differential in response to local labor market shocks which concludes, roughly, that college educated workers are much more likely to move out of declining labor market than lower educated workers (Notowidigdo, 2011).[16] As result, communities struck by large increase in imports exposure are likely to see their share of

[16]Most of this literature is based on US data. We are not aware of studies documenting this fact for France.

college educated workers decrease, relatively to other communities. To deal with this issue, I use the share in the adult population which completed a college degree for the Census year 1990, that is before imports from low-wage countries represented a significant share of French external trade.

As discussed above, nation-wide sector imports might reflect not only the rise of competitiveness of low-wage countries but also domestic demand and supply shocks. In order to isolate the supply-driven change in exposure to low-wage country imports, we instrument $\Delta\overline{IPW}_{it}$ with $\Delta\overline{IPW}^{o}_{it}$. The results are displayed in Table 2.3.

We see Column (2) that the IV estimate with no covariate exceed its OLS counter-part by about third with a point estimate of 0.945. The point estimate is twice lower when demographic controls are included and is not affected much by the inclusion of the 1990 college share.

Column (3) estimate of 0.41 implies that on average over the period 1995-2012, an increase in $\overline{IPW}_{it}$ by one cross-sectional standard deviation raise the change in electoral support for the far-right by about 7 percent of a standard deviation.

2.4.2 Local presence of immigrants

Instrumenting for the current presence of migrants

Much of the literature on the contextual factors driving votes for the far-right is concerned with the effect of immigration. In our empirical setting and given data availability, it would be possible to control for the local share of employees that are foreigners and/or born abroad. However, as argued convincingly by Halla, Wagner, and Zweimüller (2013), the local presence of foreigners is most likely endogenous with-respect-to the voting share of the far-right. There are many channels, notably the local supply of public housing in the case of France, through which a community that is particularly prejudiced against immigrants and hence very likely to massively vote for the FN, could manipulate the presence of immigrants. Moreover, immigrants might be reluctant to settle in areas where they face a high level of discrimination.

To deal with this issue, Halla, Wagner, and Zweimüller (2013) uses historical settlement patterns of foreigners as an instrument for current immigration. This approach is ubiquitous in the literature on the labor market effect of migration (e.g. Altonji and Card (1991)). The assumption is that while past settlements are highly correlated with current immigration, because migrants of a given nationality tend to locate in the host country where co-nationals are already established, they are unrelated to the current outcome of interest. While the application of this instrument to the study of the labor market is rather uncontroversial as there is little suspicion that past immigration affect current wage through any other channel than the current level of immigration, there are reasons to doubt that the exclusion restriction underpinning the validity of the instrument is plausible when the dependent variable is a voting outcome. Historical settlement patterns, even if they are initially exogenous, are likely to affect the opinion of natives with respect to migrants over the entire period between the initial settlement and the period at which the election is held, through the presence of migrants over the entire intermediary period. Hostility to migrants might arise at some point in time years ahead of the studied election, which might in turn affect the propensity of migrants to locate in this community. Appendix 2.C.1 contains a simple formal example of a data-generating process that illustrates the issue with this instrumental variable approach when hostility to migrants and local migrants presence are allowed to affect each other dynamically.

Taking stock of the difficulty of directly dealing with the endogeneity of the share of foreigners in the context of electoral studies, rather than including it in our regressions, we use it to bound the effect of interest for different assumptions regarding the magnitude of the true effect of immigration on vote for the far-right.

Assessing the bias for different assumptions on the effect of immigration

Consider our main specification:

$$\Delta FN_{it} = \beta_1 \Delta IPW_{it} + \beta_2 \Delta I_{it} + X_{it}'\delta + \gamma_{d(i),t} + \varepsilon_{it}$$

We can rewrite the equation in terms of residuals as:

$$\widetilde{\Delta FN}_{it} = \beta_1 \widetilde{\Delta IPW}_{it} + \beta_2 \widetilde{\Delta I}_{it} + \epsilon_{it} \tag{2.7}$$

where $\widetilde{x}_{it}$ refers to the residual from the regression of x_{it} on the set of exogenous regressors (i.e. X_{it}' and $\gamma_{d(i),t}$). Estimating Equation (2.7) using $\widetilde{\Delta IPW}^{o}_{it}$ as an instrument and omitting $\widetilde{\Delta I}_{it}$ from the regression yield an estimator for β_1 which converges in probability towards:

$$\text{plim}\left(b_1^{IV}\right) = \frac{\text{cov}\left(\widetilde{\Delta FN}, \widetilde{\Delta IPW}^{o}\right)}{\text{cov}\left(\widetilde{\Delta IPW}, \widetilde{\Delta IPW}^{o}\right)} = \beta_1 + \beta_2 \frac{\text{cov}\left(\widetilde{\Delta I}, \widetilde{\Delta IPW}^{o}\right)}{\text{cov}\left(\widetilde{\Delta IPW}, \widetilde{\Delta IPW}^{o}\right)} \tag{2.8}$$

Notice that here we use the maintained assumption that $\text{cov}\left(\epsilon, \widetilde{\Delta IPW}^{o}\right) = 0$, i.e. our instrumental approach to deal with the endogeneity of imports to nation-wide sectoral imports is valid. Given our sample estimates for **(i)** $\text{cov}\left(\widetilde{\Delta I}, \widetilde{\Delta IPW}^{o}\right)$ and **(ii)**

$\mathrm{cov}\left(\widetilde{\Delta IPW}, \widetilde{\Delta IPW}^o\right)$, we can compute the bias for different values of the unknown causal coefficient β_2. We report results for the specification of Column (3) from Table 2.3 in Table 2.4. The results clearly show that given that the ratio $\frac{\mathrm{cov}\left(\widetilde{\Delta I}, \widetilde{\Delta IPW}^o\right)}{\mathrm{cov}\left(\widetilde{\Delta IPW}, \widetilde{\Delta IPW}^o\right)}$, even assuming strong positive effect of immigration on far-right votes, the implied bias is very low. Our estimates therefore appear quite robust to the exclusion of the immigration variable. In the next subsection, we provide evidence that our estimate is robust to whether communities included where subject to high or low immigrants presence in 1982.

Heterogeneity across communities: past level of immigration

In this paragraph, we present results where the sample has been split according to whether a community was above or below the median presence of immigrants in 1982 (a time at which the FN was still a rather obscure political party). Table 2.5. We see the effect is roughly similar for both half of the sample (Columns (2) and (3)). If anything, the effect appears to be stronger in the bottom half, a pattern confirmed by results for the bottom and top terciles of the distribution, displayed in columns (4) and (5). Note however that the difference between the estimates is not statistically different.

2.4.3 Further results

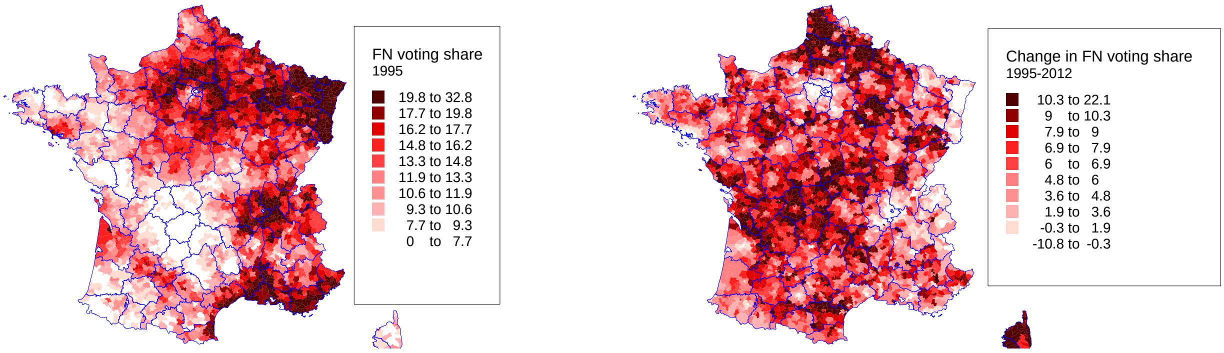
We check whether this impact has changed overtime. We proceed in two ways: (i) we restrict controls to have the same effect across periods, (ii) we estimate the model for each period. Results are of the first approach are presented in Table 2.6. It shows that for all specifications, only the last period is driving the results. This is consistent with the notion that the economic crisis triggered in 2008 and recent FN's programmatic shift have contributed to increase the marginal impact economic shock on far-right voting. Estimates in columns (2), (3) suggest a normalized effect of 14.8 and 13.7 percent of a standard deviation respectively (Notice that the normalized impact goes up in comparison with results from Table 2.3 both because of the change in estimate and because the ratio $\sigma(\Delta\overline{IPW})/\sigma(\Delta FN)$ is higher in the last period than on average).

2.5 Discussion and conclusion

In this paper, we study the impact of imports-exposure of local communities in France on their propensity to vote for the far-right. Investigating this issue is important to gauge the relevance of economic factors in the vote for the far-right. As the National Front is promoting policies that, for some of them, question the founding values of liberal democracy, while some others, can be considered, more mundanely, poor economics (protectionism, tax cuts and spending increases, uncoordinated exit of the euro area etc.), it is highly relevant in terms of public policy and social welfare to assess whether local trade-shocks foster its electoral success. Our focus on low-wage imports competition is justified by the rapid growth of such competition over the past 20 years and the stronger impact on the labor market it has been shown to have.

The results assembled in this paper suggest a small but significant effect of imports exposure on the propensity of communities to vote for the FN. Over the last four presidential elections in France, a one standard deviation increase in imports-exposure has been associated on average with a 7 percent increase in the change in the far-right voting share. We have shown evidence suggesting that not controlling for local degree of immigration is unlikely to cause substantial bias in our findings. The effect of industrial decline on vote have changed over time, benefitting the far-right mainly during the last period (2007-2012) which can be interpreted as the combined effect of the Great Recession and the focus of the National Front on economic hardship issues.

Figure 2.1: Spatial distribution of FN votes in 1995 and changes between 1995 and 2012



(a) 1995

(b) 1995-2012

2.6 Tables and figures

Table 2.1: Descriptive statistics

Variables	Mean	Sd	p10	p50	p90
Registered voters	10,527	14,495	2,482	7,160	20,562
FN	14.21	4.38	8.53	14.00	20.06
ΔFN	0.87	6.07	-7.82	1.40	8.69
ΔIPW	0.6823	1.058	0.053	0.34	1.62
$\Delta \overline{IPW}$	0.675	0.540	0.140	0.570	1.40
Share Foreigners	10.79	7.21	2.29	9.08	21.52
Δ Share Foreigners	-0.00	6.70	-7.77	-0.00	9.46

Figure 2.2: Spatial distribution of ΔIPW and $\Delta\overline{IPW}$ 1995 to 2012

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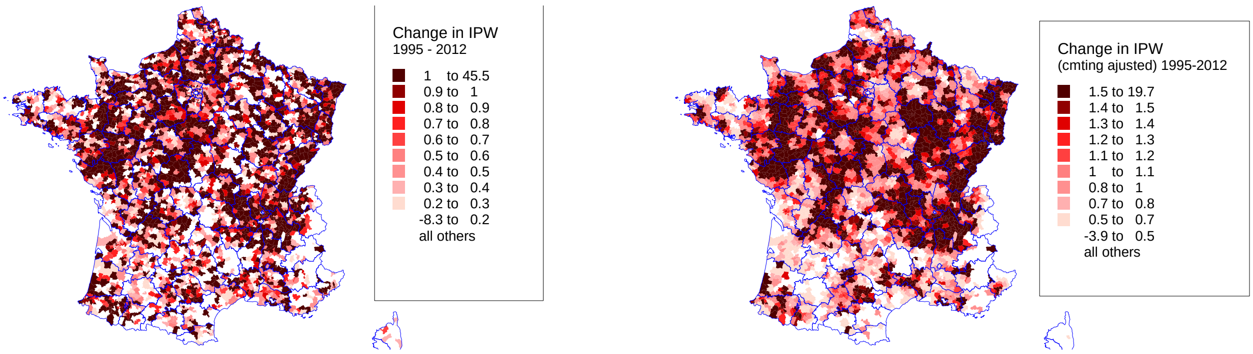

(a) ΔIPW (b) $\Delta\overline{IPW}$

Table 2.2: OLS: First-Difference with Departement-Year FE, $\Delta\overline{IPW}$

	(1) OLS	(2) OLS	(3) OLS	(4) OLS
$\Delta\overline{IPW}$	0.660***	0.335***	0.324***	0.341***
	(0.0743)	(0.0529)	(0.0537)	(0.0537)
Region-Year FE	$\surd$	$\surd$	$\surd$	$\surd$
Demographic structure ^{a}		$\surd$	$\surd$	$\surd$
College share 1990			$\surd$	$\surd$
Working Class Share 1990 ^{b}				$\surd$
N	10140	10140	10140	10140

Notes: The sample consists of 3380 cantons, observed over 3 periods: 1995-2002-2007-2012. Robust clustered standard errors are reported between brackets. Corsica is excluded from the sample. ΔIPW is expressed in thousands of dollar. ^{a}: Demographic controls include the age-sex distribution for 7 different categories (0-15 year old, 16-24, ... ,74-90, 90 and more), population and population-squared. ^{b}: Share of population with a higher education degree and share of population whose occupation is manual worker ("ouvrier") as 1990. Regressions are weighted by 1990 Census population. ^{a}: Demographic controls include the age-sex distribution for 7 different categories (0-15 year old, 16-24, ... ,74-90, 90 and more), population and population-squared. ^{b}: share of population whose occupational status is manual worker ("ouvriers"). Regressions are weighted by 1990 Census population. *p<.10 ** p<.05, *** p<.01.

Table 2.3: IV: First-Difference with Departement-Year FE, $\Delta\overline{IPW}$

	(1) OLS b/se	(2) IV b/se	(3) IV b/se	(4) IV b/se	(5) IV b/se
$\Delta\overline{IPW}$	0.660***	0.945***	0.401***	0.351***	0.340***
	(0.074)	(0.113)	(0.082)	(0.082)	(0.082)
Region-Year FE	$\surd$	$\surd$	$\surd$	$\surd$	$\surd$
Demographic structure [a]			$\surd$	$\surd$	$\surd$
College Share 1990				$\surd$	$\surd$
Working Class Share 1990 [b]					$\surd$
Cragg-Donald Stat		7594.5	7097.4	7015.4	6588.1
KP stat		365.4	343.8	343.1	324
N	10140	10140	10140	10140	10140

Notes: The sample consists of 3380 cantons, observed over 3 periods: 1995-2002-2007-2012. Robust clustered standard errors are reported between brackets. Corsica is excluded from the sample. ΔIPW is expressed in thousands of dollar. [a]: Demographic controls include the age-sex distribution for 7 different categories (0-15 year old, 16-24, ... ,74-90, 90 and more), population and population-squared. Regression are weighted by 1990 Census population. *p<.10 ** p<.05, *** p<.01

Table 2.4: Estimated bias of b_1^{IV} for different assumptions regarding the causal effect of immigration

Value for β_2	-5	-3	0	3	5	10
Table 3 Col 3 ($b_1^{IV} = 0.41$, $r = -0.002621$)	0.0131	0.0078	0	-0.0078	-0.0131	-0.0262
Table 3 Col 4 ($b_1^{IV} = 0.35$, $r = -0.00263$)	0.0131	0.0079	0	-0.0079	-0.0131	-0.0263
Table 3 Col 5 ($b_1^{IV} = 0.34$, $r = -0.002241$)	0.0120	0.00723	0	-0.007235	-0.0120	-0.0241

Notes:
The table display the value of $\beta_2 \text{cov}\left(\widetilde{\Delta I}, \widetilde{\Delta IPW}^o\right) / \text{cov}\left(\widetilde{\Delta IPW}, \widetilde{\Delta IPW}^o\right)$ given empirical estimates $\text{cov}\left(\widetilde{\Delta I}, \widetilde{\Delta IPW}^o\right)$ and $\text{cov}\left(\widetilde{\Delta IPW}, \widetilde{\Delta IPW}^o\right)$ where residuals are computed from a regression including demographic controls and province×year fixed effect. for different values of β_2.

Table 2.5: Heterogeneity based on lagged (1982) share of immigrants

	(1) All b/se	(2) < 50th pctile b/se	(3) > 50th pctile b/se	(4) < 33rd pctile b/se	(5) > 66th pctile b/se
$\Delta\overline{IPW}$	0.401***	0.405***	0.347***	0.492***	0.282**
	(0.082)	(0.094)	(0.102)	(0.105)	(0.122)
Region-Year FE	√	√	√	√	√
Demographic structure [a]	√	√	√	√	√
KP stat	334.8	223.9	196.7	201.8	137.7
Cragg-Donald stat	7876.3	3295.7	4226.1	2339.2	2873.5
N	10140	5028	5112	3279	3552

Notes: The sample consists of 3380 cantons, observed over 3 periods: 1995-2002-2007-2012. Robust clustered standard errors are reported between brackets. Corsica is excluded from the sample. ΔIPW is expressed in thousands of dollar. [a]: Demographic controls include the age-sex distribution for 7 different categories (0-15 year old, 16-24, ... ,74-90, 90 and more), population and population-squared. [b]: Share of population whose occupation is manual worker ("ouvrier") as 1990. Regressions are weighted by 1990 Census population. *p<.10 ** p<.05, *** p<.01

Table 2.6: Period specific effect with controls coefficient restricted to be constant

	(1) OLS b/se	(2) IV b/se	(3) IV b/se	(4) IV b/se
$\Delta\overline{IPW}\times$ I(period = 95-02)	0.251	0.171	0.057	-0.013
	(0.248)	(0.393)	(0.389)	(0.392)
$\Delta\overline{IPW}\times$ I(period = 02-07)	0.37***	0.165	0.123	0.116
	(0.128)	(0.136)	(0.113)	(0.138)
$\Delta\overline{IPW}\times$ I(period = 07-12)	0.319***	0.655***	0.605***	0.596***
	(0.080)	(0.129)	(0.118)	(0.123)
Region-Year FE	√	√	√	√
Demographic structure [a]	√	√	√	√
College Share 1990			√	√
Working Class Share 1990 [b]				√
AP stat		60/263/275	60/261/273	60/254/264
Cragg-Donald stat		1415.9	1410.5	
N	10140	10140	10140	10140

Notes: The sample consists of 3380 cantons, observed over 3 periods: 1995-2002-2007-2012. Robust clustered standard errors are reported between brackets. Corsica is excluded from the sample. ΔIPW is expressed in thousands of dollar. [a]: Demographic controls include the age-sex distribution for 7 different categories (0-15 year old, 16-24, ... ,74-90, 90 and more), population and population-squared. [b]: Share of population with a higher education degree and share of population whose occupation is manual worker ("ouvrier") as 1990. Regressions are weighted by 1990 Census population.

Table 2.7: Period-by-period

	(1) All Periods b/se	(2) 1995-2002 b/se	(3) 2002-2007 b/se	(4) 2007-2012 b/se
ΔIPW	0.401***	0.094	0.495***	0.268**
	(0.082)	(0.388)	(0.125)	(0.113)
Region-Year FE	√	√	√	√
Demographic structure [a]	√	√	√	√
KP stat	343.8	60.42	248.2	258.1
Cragg-Donald stat	7876.3	1436.5	3465.6	2188.0
N	10140	3380	3380	3380

Notes: The sample consists of 3380 cantons, observed over 3 periods: 1995-2002-2007-2012. Robust clustered standard errors are reported between brackets. Corsica is excluded from the sample. ΔIPW is expressed in thousands of dollar. [a]: Demographic controls include the age-sex distribution for 7 different categories (0-15 year old, 16-24, ... ,74-90, 90 and more), population and population-squared. [b]: Share of population with a higher education degree and share of population whose occupation is manual worker ("ouvrier") as 1990. Regressions are weighted by 1990 Census population. * p<.10 ** p<.05, *** p<.01

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2.A Trade and Employment Data

We use data on trade from the website `un.comtrade.org`. The trade data follow the product classification HS 1992 with 6 digit. The data on employment follows the NACE rev 1.1. classification which is equivalent to the 4-digit CPA 2002 classification. To convert HS-1992 6-digit codes into NACE 4-digit codes, we do the following:

1. We use a file available on `un.comtrade.org` to map HS-1992 into HS-2007.
2. We use one file available on `http://ec.europa.eu/eurostat/ramon` to map HS-2007 into CPA 2002. The latter maps n-to-one to the NACE rev 1.1.
3. We obtain a correspondence mapping from HS-1992 into NACE rev. 1.1. All HS-1992 6-digit goods that are not uniquely mapped with a NACE 4-digit sector divided across NACE sectors using weights reflecting each sectors initial "importance" in the economy (the weights are the employment share in 1995). Non-uniquely mapped goods account for about 9 %, 8 % and 6% of French imports from China for years 1995,2001 and 2007 respectively.

Table 2.8: Total French Imports: Uniquely and Non-Uniquely Mapped ($ millions)

	Total	Uniquely	Non-uniquely	Ratio
1995	5,950	5,385	565	.095
1996	6,833	6,236	597	.0873
1997	7,495	6,874	621	.0828
1998	8,178	7,505	673	.0823
1999	8,943	8,237	706	.079
2000	10,515	9,670	845	.0803
2001	10,450	9,635	815	.078
2002	11,380	10,506	874	.0768
2003	15,850	14,660	1,190	.0751
2004	21,398	19,871	1,527	.0714
2005	26,748	24,737	2,011	.0752
2006	30,968	28,652	2,316	.0748
2007	39,533	37,015	2,518	.0637

Note: A product code HS-1992 is considered "uniquely mapped" if according it is uniquely mapped according to our mapping HS-1992→HS-1996 → NACE built using the conversion tables from RAMON (HS 2007 to CPA) and Comtrade (HS1992 to HS2007). Each observation for product HS1992 that cannot be uniquely mapped to a NACE sector is dropped (either because there is no mapping or the mapping is not unique). Column (4) displays the the trade value non-uniquely matched products as the share of overall imports French imports from China. Trade values are expressed in current dollars.

2.B Selection of countries

We primarily use the list by Auer, Degen, and Fischer, 2013 which includes: China, India, Malaysia, Mexico, Philippines, Thailand, Turkey, Poland, Romania, Slovakia and Bulgaria.

2.C Some elements regarding identification

2.C.1 The difficulty of instrumenting for local presence of immigrants using historical settlement pattern: a simple example

Consider the simplified data-generating process (in levels as historical settlement patterns of migrants are used to instrument for the share of migrants across each cross-section):

$$FN_{it} = \alpha_1^1 I_{it} + h_{it} + \varepsilon_{it}^1 \tag{2.9}$$

$$I_{it} = \alpha_1^2 I_i^{init} + \alpha_2^2 h_{it-1} + \varepsilon_{it}^2 \tag{2.10}$$

$$h_{i,t} = \alpha_1^3 I_{i,t-1} + \alpha_2^3 \cdot h_i^{init} + \alpha_3^3 h_{i,t-1} + \varepsilon_{it}^3 \tag{2.11}$$

where FN_{it} is the electoral share of the far-right, I_{it} is the current share of immigrants and h_{it} is an unobserved variable summarizing hostility to migrants. I_i^{init} is the initial settlement pattern. The three error terms ε_{it}^j are assumed independent from the other variables.

The bias of OLS estimates of α_1^1 comes from the possibility in Equation 2.10 that $\alpha_2^2 < 0$: unobserved hostility to migrants tends to reduce the local share of migrants resulting in a negative bias of the OLS estimate of α_1^1. The issue with the instrument I_i^{init} comes from the possibility that it affects unobserved hostility to migrants through past presence (here at $t-1$) of foreigners. This will be the case in our DGP if $\alpha_1^3 > 0$.

I now compute the asymptotic bias of the OLS and IV estimator under this DGP. We consider the case where $\alpha_3^3 = 0$. The IV estimator converge towards:

$$\frac{cov(FN, I^{init})}{cov(I, I^{init})} = \alpha_1^1 + \frac{cov(h, I^{init})}{cov(I, I^{init})} = \alpha_1^1 + \alpha_1^3 \frac{cov(I_{-1}, I^{init})}{cov(I, I^{init})} + \alpha_2^3 \frac{cov(h^{init}, I^{init})}{cov(I, I^{init})}$$

Assuming $cov(h^{init}, I^{init}) = 0$ (i.e. historical settlement patterns are unrelated to historical level of prejudice towards immigrants), we develop the bias further:

$$\frac{cov(FN, I^{init})}{cov(I, I^{init})} - \alpha_1^1 = \alpha_1^3 \frac{cov(I_{-1}, I^{init})}{cov(I, I^{init})} = \alpha_1^3 \left(\alpha_1^2 \frac{var(I^{init})}{cov(I, I^{init})} + \alpha_2^2 \frac{cov(h_{-2}, I^{init})}{cov(I, I^{init})} \right)$$

Hence even if I_i^{init} is exogenous in the sense that it is independent from h_i^{init} (i.e. the initial level of hostility to immigrants), it might not be a suitable instrument as long as $\alpha_1^3 \neq 0$.

The sign of the bias is not obvious. According to the line of reasoning exposed above, we expect $\alpha_1^3 > 0$ and $\alpha_2^2 < 0$. Therefore the IV estimator will be upward (downward) bias if

$$\alpha_2^1 var(I^{init}) > (<) - \alpha_2^2 cov(h_{-2}, I^{init})$$

.

In comparison, the OLS bias is equal to:

$$\frac{cov(h, I)}{var(I)} = \alpha_2^2 \frac{cov(h, h_{-1})}{var(I)}$$

which is negative if α_2^2 is negative and $cov(h, h_{-1})$ exhibits positive serial correlation.

Chapter 3

Diversity and Employment Prospects: Neighbors Matter!

Joint with Camille Hémet

3.1 Introduction

Western economies are facing intensified flows of immigration due to trade development and economic integration, and consequently have to cope with increasingly mixed populations. This feature is particularly salient in the European Union in following the recent enlargement process. The economic and social implications of higher heterogeneity are therefore central issues. In particular, public opinion is generally hostile to immigrants that are often perceived as a threat to job security and wages, although this is not clear from empirical research. In the latest paper on the topic, Ottaviano and Peri (2012) adopt a general equilibrium approach and show that the massive immigration to the US over the 1990-2004 period actually increased natives wages, contradicting the influential paper by Borjas (2003). In contrast to the large literature studying the economic impact of immigration on natives, papers looking at the labor market effect of diversity *per se* are scarce. Using US cities data, Ottaviano and Peri (2006) jointly estimate a wage and a rent equations and find that diversity is positively associated to both variables. They conclude that diversity has a net positive impact on US-born workers' productivity. Using a similar setting, Prarolo et al. (2009) replicate these results for European regions. To the best of my knowledge, these are the only two papers in the diversity literature dealing with labor market outcomes, although not directly with employment.

This paper intends to fill this gap by assessing the impact of local diversity on individuals' employment prospects. It asks the following question: to what extent people living in heterogeneous neighborhoods have different employment probabilities than those living in more homogeneous areas? In other words, this work looks at how individuals cope with increasing levels of diversity, and in particular how this affects their employment prospects. It is relevant in the current context of high unemployment, especially in high immigration countries such as France, Italy and more recently Spain. At the micro level, if different ethnic or cultural groups are hermetic to each other, in the sense that no interaction takes place across groups, then diversity can act as a barrier to communication and in particular to job information transmission. Given the importance of personal networks in the job search process (see Ioannides and Datcher Loury, 2004), diversity would therefore reduce the chance of finding a job. On the other hand, if communication across groups is not an issue, then mixing people conveying non-redundant pieces of information (e.g. due to different backgrounds) can certainly improve employment prospects. At a more aggregate level, diversity can affect employment probability through its impact on productivity, which is ambiguous as well. On the bright side, diversity can be beneficial to productivity due to complementarity in workers' skills (see Lazear, 1999; Alesina and La Ferrara, 2005). On the downside, heterogeneity can hinder productivity by preventing social capital formation (Coleman, 1988).[1]

As we see from this brief discussion, the question of the role of diversity on employment is not trivial. By addressing the issue of diversity and employment at a local level, we intend to show how diversity directly affects workers as individuals, in addition to impacting them indirectly via firms' productivity. We am able to deal with this question using detailed geolocalized French employment data that allow me to measure diversity at very low geographic levels. More precisely, we measure diversity using several definitions of origins and at various geographic levels, so as to understand as precisely as possible the mechanisms lying behind the diversity-employment relationship. In addition, we adopt several identification strategies in order to bypass the endogeneity issue that is likely to flaw any estimate of the impact of diversity. These three methodological elements allowing me to answer the central question of this paper are detailed below.

First, the level of diversity is measured at two different geographic levels. As discussed above, diversity could impact employment prospects locally through networks and on a larger scale through productivity. In order to account for both effects, we compute diversity at a very local neighborhood level and at the local labor market level. As far as we know, it is the first time that various geographic scales for diversity are simultaneously considered. In

[1]Not only is the impact of diversity on productivity unclear, but the impact of productivity on employment is ambiguous as well: Nordhaus (2005) finds that more rapid productivity growth leads to increased rather than decreased employment in manufacturing, a sector that recently experienced a large employment decline. On the contrary, Michelis, Estevão, and Wilson (2013) find a strong negative relationship between TFP growth and labor input.

addition, this and Algan, Hémet, and Laitin (2015) are the first studies conducted at such disaggregated levels. The results reveal that employment probability is negatively correlated with neighborhood diversity, but positively correlated with employment zone diversity, suggesting a negative effect through networks and a positive one through productivity.

Second, we rely on three alternative definitions of origins to measure diversity, namely nationality, birth country, and parents' origins, while the existing literature mostly relies on ethno-linguistic and religious groups. The measure based on parents' origin encompasses first- and second-generation immigrants and is therefore more closely related to the standard ethnic classification of individuals. By contrast, defining kinship according to nationality introduces the notion of immigrants' integration through naturalization. This distinction allows me to draw conclusions on whether the cultural or the ethnic dimension of diversity prevails. An important finding of this paper is that diversity based on nationality has a larger impact than diversity based on birth country, which is itself more relevant than diversity based on parents' origins, suggesting a prominent role of cultural over ethnic diversity.

Third, we tackle the endogeneity issue that is pervasive in the literature on ethnic diversity. An important concern is that individuals have a preference for living close to their co-ethnics and thus tend to gather along ethnic lines, biasing any measure of the effect of diversity. Another issue is that of reverse causality that can arise if immigrants decide to settle in more economically dynamic areas. The endogeneity of employment zones diversity is handled through a traditional instrumental variable approach, where two different instruments are proposed. Following Card (2001) and Saiz (2007), we construct the predicted level of diversity in each employment zone based on the distribution of each origin group across employment zones in 1968 and the current number of individuals from each origin in France at the time of the study (2007-2010). An alternative and more innovative instrument is the level of diversity within the public housing tenants of the employment zone. It builds on Algan, Hémet, and Laitin (2015) who show that the allocation of households across public housing units in France does not take their origins or their preference for diversity into account, so that public housing diversity can be considered as exogenous. Interestingly, once employment zone diversity is instrumented using any of these two variables, its positive relationship with employment is driven down to zero, confirming the intuition that the effect was actually driven by selection. To deal with the endogeneity of local neighborhoods diversity, we follow Bayer, Ross, and Topa (2008) and assume that although households are able to select the precise area in which they want to live, they are, however, unable to pinpoint an exact neighborhood within this given area. Therefore, after controlling for sorting in a larger area, the assignment of individuals to a specific neighborhood is essentially random and provides a useful source of variation to identify the effect of diversity. It turns out that the effect of local diversity on employment is corrected downward, *i.e.* becomes more negative.

The rest of the paper proceeds as follows. Section 3.2 discusses more extensively the channels through which diversity can affect employment prospects. Section 3.3 presents the data and the various measures of diversity. The relationship between diversity and employment status is investigated in Section 3.4. Section 3.5 corrects for endogeneity. Results are interpreted in Section 3.6. Section 3.7 concludes.

3.2 Diversity and employment prospects

The interest in the effect of diversity on economic performance and social peace has been rooted in economic research since the seminal paper by Easterly and Levine (1997) showing that Africa's high levels of ethnic diversity help understand its "tragic growth performance". The subsequent literature covers a very broad set of issues. Diversity is generally found to reduce public good provision, because the threat of sanction to punish defectors is not credible across groups, or because different groups do not share the same preferences and cannot agree on the type of public good to be produced. This result holds in developing countries and developed countries alike (see Miguel and Gugerty (2005) for Kenya, Alesina, Baqir, and Easterly (1999) for the US and Algan, Hémet, and Laitin (2015) for France).[2] Another trend of the literature focuses rather on the social impact of diversity, and shows that it is associated with lower participation to civic life or community activities (Alesina and La Ferrara (2000), Costa and Kahn (2003)) and reduced trust (Alesina and La Ferrara (2002)).

The present paper focuses on employment and is therefore more closely related to the branch of the literature that studies productivity. At the macro level, diversity can affect employment through its effect on productivity. A large part of the literature supports the idea that diversity has a positive impact on diversity related to skills complementarity, that dominates the negative effects linked to coordination issues. Indeed, workers from different origins are more likely to have been exposed to diverse cultures and distinct school systems (especially if they come from different countries), acquiring various skills and learning different approaches to the same problem, so that their collaboration can increase productivity and facilitate innovation. More formally, Hong and Page (2001) develop a model showing that team work may benefit more from low-skilled but cognitively diverse workers than from homogenous high-skilled workers. In a different theoretical setting, Lazear (1999) shows that when multicultural workers are complementary in the sense that they can exchange

[2] An exception is Glennerster, Miguel, and Rothenberg (2013) who do not find any particular effect of diversity in Sierra Leone villages.

non-redundant and relevant information, the benefits from diversity offset its costs (e.g. barriers to communication).

Several recent papers also bring empirical support to the beneficial impact of diversity on productivity and economic performance more generally. Using data from 160 metropolitan areas in the US, Ottaviano and Peri (2006) jointly estimate a wage and a rent equations and find that diversity, measured in terms of birth countries, is positively associated to both variables. These results are robust to the inclusion of many confounding factors proxying for productivity and amenity shocks across cities, as well as to the instrumentation of diversity to correct for endogeneity. They conclude that diversity has a net positive impact on US-born workers' productivity. A similar methodology is adopted by Prarolo et al. (2009) who reach the same conclusion for European regions. Finally, Alesina, Harnoss, and Rapoport (2013) investigate the relationship between birth country-based diversity and economic development in a cross-section of countries. Potential endogeneity due to reverse causality is addressed through instrumental variable estimation. They compute a predicted measure of immigrants diversity by estimating a gravity model based on exogenous geographic and cultural bilateral variables. They find that while standard ethno-linguistic fractionalization is detrimental to economic success, the impact of diversity in terms of birth countries is positive, especially in more developed countries.[3]

At a more micro level, diversity can affect individuals' employment prospects through the channel of networks and job information transmission. There is considerable evidence that information transmission plays a key role on the labor market.[4] Many empirical studies conducted over various time periods and on diverse countries agree that relying on friends and family is a very popular job search method and that on average half of jobs are found through social networks (Corcoran, Datcher, and Duncan (1980), Granovetter (1995), Holzer (1988), Wahba and Zenou (2005)). Theoretically as well, Calvó-Armengol and Jackson (2004) show that employment probability increases both with the number of links an agent has, and with the employment rate in the individual's network. In particular, several papers focus on the role of ethnic and immigrant networks. A recent paper by Battu, Seaman, and Zenou (2011) shows that ethnic minorities in the UK rely extensively on personal networks when searching for a job, although this does not necessarily lead to better employment prospects. The sociology literature also emphasizes the importance of ethnic networks in business relations and entrepreneurship, through an increased capacity to cooperate due to common languages and values (Light and Rosenstein (1995), Light (2005)).

[3] This paper also provides a comprehensive review of the literature on the costs and benefits of diversity.

[4] Ioannides and Datcher Loury (2004) and Ioannides and Topa (2010) provide comprehensive surveys on the topic.

Because communication across ethnic groups may be hindered by a tendency to self-segregate, by different religious beliefs and culture, and above all by differences in the languages spoken, diversity may prevent network formation and information transmission, thus having a negative impact on individuals' labor market performances. This effect could be amplified if diversity exists at the neighborhood level, as networks tend to be very local (see for instance Wellman (1996)). In particular, a few recent studies have shown that local social interactions within neighborhoods do affect employment and wage outcomes. For instance, Weinberg, Reagan, and Yankow (2004) show that a one standard deviation increase in neighborhood employment is associated with a 6.1% increase in annual hours worked for adult males on average. Bayer, Ross, and Topa (2008) estimate that living in the same block increases by more than 33% the probability to work at the same location. In a paper dealing explicitly with ethnic networks, Patacchini and Zenou (2012) show that the individual probability of finding a job increases with the number of ties, but that the magnitude of the effect decreases with distance. To summarize, if individuals are unable to create social ties within their neighborhood because they live in a diverse environment, this might hinder their ability to search and find job through the network.

3.3 Data and descriptive statistics

The main dataset used in this paper is the French Labor Force Survey (*Enquête Emploi*, INSEE, hereafter the LFS), which has been conducted quarterly since 2003. One sixth of the sample is renewed each quarter, so that the survey takes the form of a quasi-panel, as each household is surveyed for six consecutive waves before leaving the sample. Each wave of the survey comprises about 72,000 respondents aged 15 years-old or older. The sampling strategy of the LFS makes it particularly valuable for studying neighborhood effects. To put it simply, France is divided into areas made up of twenty homes on average. The sample is then drawn from a random selection of these areas, in which all the households will be surveyed.[5] As a consequence, we are able to characterize the immediate neighborhood of each surveyed individual. In particular, it is possible to measure the precise level of diversity and the unemployment rate within these 20 households units.

The LFS contains all the relevant information about individuals' labor market situation: employment status, wage, type of contract, tenure, job search methods and socio-economic category. It also provides detailed individual information, such as age, gender, education, and marital status. Individuals' ethnic background can be inferred from their birth country,

[5] Refer to INSEE documentation for more details on the sample composition and selection.

their nationality, and their parents' origins. Because we are interested in individuals' employment status we restrict the sample to working-age individuals (16 to 65 years-old) and we drop the students. In addition, the information about parents which is used to characterize individuals' origins is mostly missing before 2007, we restrict my sample to the 2007 to 2010 period. Table 3.1 summarizes the main employment-related individual characteristics for this sub-sample. We then define three different measures of origins. Two measures are simply based on individuals' nationality and country of birth, while a third measure combines the nationality and birth country of both individuals' and their parents'. The survey contains two variables with 28 categories describing individuals' nationality and birth country respectively, which are used as two different indicators of origin of their own.[6] The information about parents' nationality and birth country is given by four variables with 9 categories, for each parent's (mother's and father's) nationality *at birth* and birth country.[7] This enables me to build a measure of origins that takes second generation immigrants into account. More precisely, for an individual to be sorted in a given group, it must be the case that at least one of her or his parents belongs to this group. For instance, a French person who is born in France but whose parents were born with a Maghrebian nationality is allocated to the "Maghreb" group. Note however that this third type of classification of origins can contain at most 9 categories. Because the measure of diversity (see next paragraph) is sensitive to the number of categories used, the alternative measures of origins should contain the same number of categories to allow comparison. In addition, some of the groups considered, such as "Northern European", represent such small shares of the population living in France that we decide to aggregate them further. We eventually build three measures of origins based on nationality, birth country and individual and parents origins that are divided into the following 6 categories: France, Southern Europe, other European countries, Maghreb, other African countries and rest of the world. Table 3.2 describes the sample along the three dimensions of origins.

Using these various classifications of origins, we am able to compute several measures of diversity. The level of diversity in a given area reflects the probability that two randomly drawn individuals from the population belong to two different groups. Formally, it is computed using the standard fractionalization index used in the literature (see Alesina et al. (2003) for an extensive description):

[6] The 28 possible nationalities or birth countries correspond to the following countries: France, Algeria, Tunisia, Morocco, Other African countries, Vietnam / Laos / Cambodia, Italy, Germany, Belgium, Netherlands, Luxembourg, Ireland, Denmark, United Kingdom, Greece, Spain, Portugal, Switzerland, Austria, Poland, Yugoslavia, Turkey, Norway, Sweden, other European countries (including former USSR), USA / Canada, Latin American countries and other countries.

[7] The 9 categories correspond to France, Northern Europe, Southern Europe, Eastern Europe, Maghreb, rest of Africa, Middle East, Vietnam / Laos / Cambodia and rest of the world.

$$DIV_j = \sum_{i=1}^{N} s_{ij}\,(1 - s_{ij}) = 1 - \sum_{i=1}^{N} s_{ij}^2 \tag{3.1}$$

where s_{ij} is the share of individuals from group i *(i=1, ..., N)* in geographic area j. This index takes its minimum at 0 when the population living in area j is fully homogeneous, and it converges to 1 as the population heterogeneity increases. Note that DIV_j can increase for two reasons: it will increase with the number of groups, and it will increase the more equal the size of the groups.

In the context of this paper, the groups considered are alternatively nationalities, birth countries and parents' origins, as defined above. Looking at various measures of diversity enables me to capture various dimensions of diversity. We argue that parents' origins-based diversity is the best proxy for *ethnic* diversity as it is more likely to reflect color of skin. For instance, a person whose parents are Senegalese is very likely to be black, even though s/he is French and born in France. This measure of diversity is therefore the closest to the ethnic diversity used in the literature and in particular in the US. On the other hand, diversity based on nationality reflects *cultural* rather than *ethnic* diversity. Indeed, two individuals sharing the same nationality are more likely to speak a common language and to share other cultural traits. This can be true for two native individuals, as well as for naturalized French who spent some time in France, learning French before being naturalized. Given what precedes, it is then reasonable to rank birth country diversity in-between.

Let me now present the various types of geographic areas for which we assess diversity. The first type of area considered is the local neighborhood made of around twenty contiguous households (LFS sampling unit). Measuring diversity at such a local geographic level enables one to indirectly account for really local interactions between immediate neighbors and to test whether diversity acts as a barrier to job information transmission. The second area used to measure diversity is the employment zone, which is a local labor market. More precisely, it is a geographical area within which most of the labor force lives and works, and in which establishments can find most of the labor force necessary to occupy the offered jobs. There are about 300 employment zones in metropolitan France. Measuring diversity at a level corresponding to a consistent local labor market is particularly useful to challenge the idea that diversity increases productivity hence being beneficial at more aggregate levels.

Table 3.3 describes the levels of diversity prevailing in individuals' local neighborhoods and employment zones. It is immediate to see that diversity is the lowest when measured in terms of nationalities, and the highest when computed based on individuals' and parents' origins, which is not surprising. Consider for instance an area made of three French individuals, one born in France from French parents (e.g. native French), one born in France from Vietnamese parents (e.g. second generation immigrant), and one born in Morocco from Moroccan parents (e.g. first generation immigrant). This population is completely homogeneous (DIV_j=0) if we consider the individuals' nationality only. However, diversity is larger (0.44) once birth countries are taken into account, and even more (0.67) once parents' origins are considered. Note also that employment zone diversity is slightly larger than neighborhood diversity, while the latter takes more extreme values (larger maxima) than the former.

3.4 Analysis

In this section, diversity is considered as exogenous, and its impact on employment status is estimated through the following equation:

$$EMP_{ijt} = \alpha + \beta DIV_{jt} + \eta Z_{jt} + \gamma X_{it} + \phi_g + \phi_t + \varepsilon_{ij} \tag{3.2}$$

where EMP_{ijt} is the employment status of individual i living in area j at time t, DIV_{jt} is the level of diversity in area j at time t, Z_{jt} is a vector of characteristics of area j at time t, and X_{it} is a set of individual control variables. We also include geographic fixed effects ϕ_g, generally *départements* fixed effects, along with time fixed effects ϕ_t (quarter dummies). Finally, ε_{ij} is an error term. The main coefficient of interest is β. Individuals' employment status can either be *employed*, *unemployed* or *inactive*. In what follows, the dependent variable used will be a dummy variable equal to 1 if the individual is employed, and 0 otherwise (unemployed or inactive).[8] The results presented in this section derive from OLS estimates, with robust standard errors clustered at the neighborhood level. Logistic regressions lead to qualitatively similar results, but OLS estimates are displayed for simplicity. At some point, multinomial logit estimates will be presented, to take into account the three possible employment statuses, without altering the main result.

The various measures of diversity (nationality-based, birth country-based and parents' origin-based) are included separately, in different regressions. However, both neighborhood and employment zone diversity (based on the same origin groups) might be included in a given regression. The set of individual controls X_{it} comprises the standard socio-demographic variables: age (quadratic form), gender, origin, education, socio-economic category and potential experience (quadratic function). The origin variable can take 6 different values:

[8] An alternative dummy variable considered takes value 1 if the individual is employed, and 0 if s/he is unemployed, letting aside inactive individuals. Using this alternative dependent variable does not significantly alter the estimated coefficients.

France, South Europe, rest of Europe, Maghreb, rest of Africa and rest of the world. Specifically, we alternatively include nationality, birth country and parents-based origin indicators when diversity is measured based on nationality, birth country and parents' origin respectively. The education variable describes the highest degree obtained by the individual, which can be one of the following: *No diploma*, end of junior high school degree (9th grade) (*BEPC*), early vocational training degree (*CAP*), *Technical degree*, technical or vocational senior high school degree (*Tech. & Pro. Baccalauréat*), general senior high school degree (*Baccalauréat*), *Undergraduate* diploma (two years after the *Baccalauréat*), *Bachelor's* degree (three years after the *Baccalauréat*), *Graduate* diploma (four years after the *Baccalauréat*), and higher degree (*Master's & PhD*). Finally, there are 6 possible socio-economic categories: *Farmer*, *Craftsman or Shopkeeper*, *Executive* or other high position, *Intermediate* occupation, *Employee* and *(Factory) worker*. Potential experience is measured as the number of years since the highest degree was awarded. Finally, we also control for the unemployment rate prevailing in an individual's neighborhood, so as to account for peer effects. Note that a given individual is excluded when computing the unemployment rate in his/her neighborhood.

Table 3.4 presents the estimates obtained from regressing the employment dummy on neighborhood-level and employment zone-level diversity based on nationality. Each column corresponds to an alternative specification, starting from no control in column 1 to the full set of controls in column 4. The sample is restricted to the non-student, working-age population (16 to 65 years-old individuals). In addition, we keep the first observation of each individual, so that an individual appears only once in the sample.[9] The estimates reported in the first column directly reveal that local diversity is negatively associated to the probability of being employed, while the correlation with employment zone diversity is positive.[10] These results seem to be in line with the idea that diversity can have an adverse effect on job finding locally by preventing communication, but that at a more aggregate level, diversity has positive effect on productivity and hence on employment probability. An alternative interpretation could be that when diversity is high, individual's networks lie in a larger area than their direct neighborhood. The estimates presented in column 2 are obtained controlling for the set of individual characteristics. The coefficients for the two measures of diversity are significantly reduced (in absolute terms), but we still have a negative coefficient for neighborhood diversity and a positive one for employment zone diversity. Turning to individuals' nationality, it is quite interesting to see that South European perform better than French in terms of

[9] The panel aspect of the data is ignored for the time being.

[10] This is also true when local and employment zone diversity are included in separate regressions. In this case, both coefficients are smaller (in absolute terms), but are still significant at the 1% level.

employment, while individuals of any other nationality are more likely to be unemployed or inactive than French. The positive coefficient of South European can be attributed to Portuguese who have a much lower unemployment rate than France average. Coefficients for education levels, socio-economic categories, gender, age and experience all have the expected signs. Column 3 adds quarters and *départements* fixed effects to the previous specification. The most notable change is for employment zone diversity which is reduced further. Finally, the results displayed in column 4 are obtained when neighborhood unemployment rate is added to the other controls. Including this variable significantly decreases (in absolute terms) the estimated effect of diversity, especially that of neighborhood diversity. Obviously, the coefficient for local unemployment rate is strongly negative.

The estimated effects of the various types of diversity are summarized in Table 3.5. Each column corresponds to a different specification, as in Table 3.4. The first two lines display the estimates for nationality-based diversity that were already shown in the previous table. The second and third sets of estimates correspond to birth country- and parents' origin-based diversity. As previously, we observe that the coefficient is always negative for neighborhood diversity, and always positive for employment zone diversity, no matter how diversity is measured. Note also that the negative effect of local neighborhood diversity always dominates the positive effect of employment zone diversity, revealing that close neighbors are more important than further neighbors when it comes to job finding. A substantial result emerges from comparing the coefficients for the various measures of diversity. In any specification, the estimated effect of nationality-based diversity is larger (in absolute terms) than that of birth country-based diversity, which is also larger than parents' origins-based diversity. To put it differently, living in a context where people have different nationalities matters more for employment than living in a context where people were born in different countries, and even more than living close to people whose parents are from different origins. As discussed in the previous section, parents' origins more likely reflect ethnicity than nationality is, the latter being rather associated to common values and language. A direct interpretation of the results is therefore that neighborhood diversity lowers the probability of employment because of cultural differences, most likely including language differences, rather than ethnic differences. This reinforces the intuition that diversity affects job finding by limiting job information transmission between neighbors.

Finally, Table 3.6 reports the estimates for the various types of diversity obtained with multinomial logits. This enables me to look at the effect of diversity on the three possible employment statuses. The two results put forward in the preceding tables hold in this case. First, living in a more diverse neighborhood reduces one's employment prospects, while the effect of living in a more diverse employment zone goes in the opposite direction. Second, cultural diversity, embedded by diversity based on nationality matters more than ethnic diversity, embedded by parents' origins diversity. The additional information contained in this table is that when neighborhood diversity is found to decrease employment probability, it corresponds to an increase in both unemployment and inactivity, the former being two to three times larger than the former.

3.5 Results with endogenous diversity

The analysis presented in the previous section assumes that diversity is exogenous. However, there are several reasons to suspect that diversity might actually be endogenous. First, individuals who have a taste for diversity might both self-select into high diversity areas and be more able or willing to communicate with their neighbors. In this case, people living in more diverse area should be more likely to communicate with each other and the negative effect found previously would be overestimated (less negative than the true effect). Reverse causality could also be a problem if immigrants are attracted by more economically dynamic places, where jobs are more abundant. The issue of endogeneity related to the non-random location of individuals is addressed in this section.

3.5.1 Employment zone diversity: an instrumental variable approach

The first part of this section deals with the endogeneity of employment zones diversity. Because employment zones are quite large areas (there are about 300 employment zone in mainland France), the assumption made in the previous subsection cannot hold and the above strategy cannot be applied. Instead, we will rely on more standard instrumental variable estimation. A plausible instrument should be correlated with employment zone level of diversity (e.g. employment zone population composition), but uncorrelated to labor market outcomes. In what follows, we propose two different instruments.

The first instrument relies on the "shift-share" methodology initiated by Card (2001) and more recently used by Saiz (2007) and Ottaviano and Peri (2006) in a setting close to this paper's. It builds on the idea that new migrants to a country tend to settle where former migrants from the same origin previously settled, i.e. ethnic enclaves (Munshi (2003), Winters, Janvry, and Sadoulet (2001)). Using past settlements of immigrants from various countries across French employment zones, it is possible to construct a predicted measure of current diversity in each employment zone. More precisely, we use the 1968 population census data to compute the distribution of each origin group across French employment zones. Because employment zones did not exist in 1968, and because their frontiers evolved over time, we apply the 2010 employment zones borders to the 1968 population. The origin groups considered are limited by the information contained in the 1968 census. In particular, no information about parents is available. We alternatively rely on nationalities and birth countries, grouped into the six categories defined previously. Then, for each origin group, we apply the 1968 distribution of groups across employment zones to the current (2007 to 2010) total population in France. Doing so, we compute the expected number of individuals from each origin in each employment zone, solely based on the ethnic enclaves pull factor. From this, we can deduce the predicted composition of each employment zone population. Once the predicted shares of each group are computed, we can eventually construct the predicted level of diversity in each employment zone over the 2007 to 2010 period. By construction, the predicted measure of diversity depends only on 1968 population settlements and not on any employment zone-specific shock (e.g. productivity shock), and can thus be used to instrument actual diversity. A more formal description how the predicted level of diversity is computed can be found in Appendix 3.A.

The alternative instrument is more innovative and builds on Algan, Hémet, and Laitin (2015). In this paper, the authors provide evidence that the allocation of households across public housing units in France does not take their origins or their preference for diversity into account, so that diversity can be considered as exogenous within the public housing sector. Not only do legal rules prohibit housing allocation based on ethnic backgrounds, but the characteristics of the public housing sector, which is very tight and highly regulated, also make it very complicated to bypass the law in practice. In addition to presenting these general arguments, the authors conduct a variety of formal statistical tests to verify the absence of self-sorting on ethnic characteristics. In particular, they show that the observed spatial distribution of residents across public housing blocks is not statistically different from a random distribution. Now that the exogeneity of diversity is acknowledged, we argue that the level of diversity within the public housing sector of a given area is necessarily correlated to the global level of diversity of this area. Indeed, because the public housing population is part of the total population, fractionalization based only on this sub-population is mechanically proportional to fractionalization based on the population as a whole. Also, it is reasonable to think that individuals living in the private housing market, and who are therefore less constrained upon their location choice, are influenced by the composition of the public housing population. Otherwise stated, people from a given group might be attracted by an area where some of their co-ethnics were located through the public housing allocation process, so that diversity in the area is likely to reflect diversity in the area's public housing sector.

In a nutshell, public housing diversity can be considered as exogenous, and it is correlated to general diversity both mechanically and through a magnet effect on immigrants living in the private sector. It can therefore reasonably be used to instrument the general level of diversity.

We now estimate the effect of diversity on employment status using a two-stage least square procedure, where the two instrumental variables described above are alternatively employed. Aside from the inclusion of an instrument, the specification corresponds to the full specification presented in Section 3.4, where we control for individual characteristics, local neighborhood unemployment, and *département* fixed effects. The results are summarized in Table 3.9, where we only report the coefficients and statistics of interest.[11] For the sake of comparability between OLS and IV estimates, we report the OLS estimates in the first column. Column 2 displays the results obtained using the predicted measure of employment zone diversity as an instrument for employment zone diversity. More precisely, in the first part of the table, which deals with diversity based on nationalities, the predicted diversity is also based on nationalities. In the second part of the table dealing with birth country-based diversity, we use the predicted diversity based on birth countries instead.[12] In both cases, we observe that the coefficient for employment zone diversity loses its significance once it is instrumented by the predicted level of diversity. The magnitude of the coefficient drops significantly and is driven down to zero (especially in the birth country regression), so that the lack of significance is not just a consequence of larger standard errors. The first-stage statistics reported at the bottom of Table 3.9 illustrate the strength of the excluded instrument. The F-statistics testing the hypothesis that the excluded instrument is equal to zero in the first stage are much larger than the rule-of-thumb value of 10 indicated by the literature on weak instruments (e.g. Stock and Yogo, 2002). In addition, the partial R^2 confirm the large correlation between the excluded instrument and the endogenous variable. The results obtained using the second instrument, namely diversity within the public housing sector of the employment zones, are reported in column 3. The first-stage statistics also reveal that this instrument is strong, and its use leads to the same results as with the first instrument: the coefficient for employment zone diversity is basically annihilated in the second stage.[13] To summarize, these results show that employment zone diversity does not

[11]The coefficients of the other variables are almost unchanged, and are available upon request.

[12]Given that we do not have any information about parents' origins in the 1968 census, we am unable to compute the predicted level of diversity based on this particular measure of origins. As a consequence, we have alternatively instrumented parents' origins-based diversity by the predicted level of diversity based on nationality and on birth country. The results are comparable to those reported in the table for the other measures of diversity, and are available upon request.

[13]A comment on local neighborhood diversity is in order here. As noted in the previous subsection, the estimates reported in Table 3.9 are likely to be biased, as we only control for *département* fixed effects. However, we already know that correcting for this bias by the introduction of smaller area fixed effects (e.g. municipality) reduces these coefficient further.

have any causal impact individuals' employment status. This confirms the suspicion that the naive estimates derived in the previous section were upwardly biased, due to a selection of immigrants into more economically dynamic areas.

3.5.2 Local neighborhood diversity: a local approach

The second part of this section deals with the endogeneity of local neighborhoods (*aires*) diversity. The approach adopted here builds on the very local nature of the data. It follows Bayer, Ross, and Topa (2008) who study the role of neighbors on work location. The idea is that although households are able to select a given area in which they want to live, they are, however, unable to select a precise neighborhood within this given area. This assumption means that even if households are able to choose a given residential area, there will not be any correlation in unobserved factors affecting employment probability among individuals living in the same neighborhood within the larger selected area.

Let me now present a few arguments supporting this assumption. First, because the housing market is very tight, it is reasonable to think that an individual targeting a given area is very unlikely to have a choice over the precise neighborhood where s/he will end up in this area. This would indeed require that at least one housing unit satisfying the other decision criteria of the individual (e.g. size) be vacant in each of the neighborhoods within the target area at the time when the individual is searching a new place. A second consideration is that it may be difficult for prospecting individuals to identify neighborhood-by-neighborhood variation in neighbors and contextual characteristics, prior to moving into the neighborhood. To put it differently, although the individual may have a realistic ex-ante view on the characteristics of the targeted area, it is less likely that s/he is actually able to identify differences in these characteristics across the various neighborhoods of the area. This is particularly reasonable when the neighborhood considered consists in about twenty households. Finally, the neighborhoods studied here (the labor force survey units called *aires*) do not correspond to any administrative or official frontiers. People do not know where the borders are, and more generally do not even know what an *aire* is, as it is only used as the sampling unit of the LFS. For those reasons, it is close to impossible that French households purposely decide to live in a given *aire* rather than the next one. All these arguments support the validity of the assumption that there should be no correlation in unobserved factors affecting employment among neighbors living in the same neighborhood (*aire*) within the larger targeted area.

As a consequence, once we control for the characteristics of the larger area selected by the individual, the remaining spatial variance of diversity across neighborhoods within the larger area is supposed to be exogenous. This is done through the inclusion of fixed effects of larger areas than the neighborhood under study (*aire*). Yet, one cannot know for sure which is the larger area initially selected by an individual prior to moving in a new home. We therefore run several regressions where we successively control for smaller and smaller areas fixed effects. The results are summarized in Table 3.7, which reports the coefficients for local neighborhood diversity. Each column corresponds to a separate regression, with the full specification, but controlling for the different larger areas fixed effects. Note also that as we want to focus on local neighborhood diversity here, we exclude employment zone diversity from these regressions.[14] In the first column, we control for *départements* characteristics, as in the regressions presented in the previous section. As *départements* are quite large areas, it is very likely that individuals actually target a more precise location. Hence, we control for employment zone fixed effects in column 2. We can see that the coefficients are slightly more negative than in the *département* fixed effects specification, comforting the idea that the previous estimates of neighborhood diversity were indeed overestimated. Employment zones still being rather large areas, we go one step further and include municipalities (i.e. cities) fixed effects in column 3. In particular, the *arrondissements* of Paris, Lyon and Marseille are municipalities of their own. Again, the estimated effects of diversity are even lower than in the previous set of regressions, as we control for the characteristics of a more precise area in which individuals are more likely to self-select. We finally control for the characteristics of the *sector* where the individual lives, which is the smallest identifiable area after the *aire* (the 20 homes neighborhoods of interest in this paper). More precisely, a sector is an area delimited by topographical elements such as streets, roads, railways and rivers, containing between 120 and 240 homes and hence between 6 and 11 *aires*, out of which 6 are randomly selected to be included in the labor force survey sample over its total lifespan. The last column reports the estimates of diversity when *sector* fixed effects are included. The estimates are still significantly negative. All in all, these results confirm that local diversity has indeed a strong negative causal impact on the probability to be employed, and that, if anything, this effect was underestimated in the previous section.

As explained above, our results are only valid to the extent that there is no sorting on unobservables within each sector. While this assumption is not directly testable, the presence of sorting on pre-determined observables (such as age, education or nationality) within-sector would make it dubious while the absence of such sorting would make it seem more plausible. To test for the presence of within-sector sorting on observables, we carry

[14]The coefficients for local neighborhood diversity are slightly larger when we control for employment zone diversity, but the changes related to the inclusion of alternative large neighborhood fixed effects are similar. On the other hand, the coefficients for employment zone diversity lose their significance once fixed effects for smaller areas are included. This reinforces the intuition that the naive estimates presented in the previous section were actually upward biased. This is addressed in the following subsection.

out a test in the spirit of Bayer, Ross, and Topa (2008). We start by randomly selecting one individual i per area. We regress some observable x_i, say years of education, on the neighborhood average of that same variable $x_{a(i)}$ (excluding the selected individual and her household in the computation of the area-average) and repeat the same procedure including sector fixed-effect, i.e. regressing $\widetilde{x}_i$ on $\widetilde{x}_{a(i)}$ where $\widetilde{x}$ refers to deviation from sector-average (i.e. residual from a regression on a full set of sector indicators). Table 3.8 displays the results of this sorting test. We see that while we detect a substantial amount of sorting, along variables such as French or North-African nationality or education (graduate degree in particular), taking sector-fixed effect drastically reduces the explanatory power of area-level average on individual characteristics. For instance, the R-square for the regression of the French nationality dummy is divided by 8 when taking sector-fixed effect. The same pattern holds for other variables pertaining to education or age, which suggests that very little sorting within sector is occurring.

3.6 Interpretation of the results

So far, we have shown that there is a positive relationship between diversity and individuals' employment probability at the employment zone level, but that it is merely due to self-selection, and does not correspond to any causal relationship from the former to the latter. By contrast, we have also established that living in a diverse neighborhood actually implies a lower employment probability. This section is an attempt to understand why local diversity reduces individuals' employment prospects. As mentioned at the beginning of the paper, one of the channels that come to mind when thinking about the relationship between neighborhood diversity and employment is the channel of communication and job information transmission between agents. Specifically, if neighbors from different origins do not communicate, e.g. because they do not speak the same language, then information about job opportunities or about how to register to an employment agency does not circulate across groups. One of the results obtained in this paper, namely that the negative effect of neighborhood diversity is stronger for nationality-based diversity than for birth country- or parents' origin-based diversity is a first evidence supporting this intuition.

In order to address this question more formally, we look at the correlation between local diversity and the nature of neighborhood relationships using the 2002 French Housing Survey. Surveyed individuals are asked to qualify the relationships with their neighbors, which can either be *good*, *average*, *bad*, or *nonexistent*. In addition, we know the precise (block level) place of living of the individuals, and we are able to match it with representative block level measures of diversity computed using the 1999 population census. The results of multinomial logit regressions of the quality of neighborhood relationships on neighborhood diversity are presented in Table 3.10. Each line corresponds to a separate regression: the first line displays the estimated coefficients of diversity based on nationality at birth, those for birth country-based diversity being reported in the second line. Each regression controls for individual characteristics (age, gender, origin, employment status, education, household income), block level unemployment rate, department fixed effects and a detailed indicator of the social and economic composition of neighborhood in 27 categories.[15] The results reveal that individuals living in more diverse neighborhoods are less likely to report having good relationships with their neighbors. In particular, they are more likely to report having bad relationships than average relationships than no relationship at all. These simple results tend to support the idea that communication can be hindered in more diverse neighborhoods due to the poor quality of the relationships between neighbors.

An alternative test of this intuition is to see how employment status is affected by the presence of people from the same origin group. Presumably, if the negative effect of neighborhood diversity is due to limited information transmission across groups, then living close to people from the same origin should conversely be related to better employment prospects. Using the LFS data, we compute, for each individual, the share of the local neighborhood population belonging to the same origin group (excluding the reference individual from the computation). We then run simple OLS regressions of the employment status dummy used in Sections 3.4 and 3.5 on this variable, using the same set of controls as in the full specification. However, because we want to avoid the bias due to endogenous location selection, we include municipalities rather than *département* fixed effects. The results are presented in Table 3.11. The estimates presented in Column 1 show that the larger the share of neighbors from one's own origin group, the higher one's employment probability. This is especially true when the individual's nationality determines his/her origin group. To put it differently, when communication is free from cultural or language barriers with a larger share of individuals, employment prospects are improved. Mechanically, more diversity implies smaller group shares, contributing to the negative effect of diversity. As a matter of fact, once we control for neighborhood diversity in column 2, the estimates of the share of people from the same group are strongly reduced (and lose their significance except for nationality).

A more direct and natural way to dig into the hypothesis that job information transmission bridges the gap between diversity and employment, is to focus on the role of personal networks in job search and job finding. The LFS data first provides information about the

[15]The socio-economic classification of French neighborhoods into 27 groups is realized by Martin-Houssart and Tabard (2002).

methods used by individuals who are looking for a job. Job seekers, whether unemployed or not can indicate which methods they use among 15 possible methods. For the purpose of the present study, we focus on the use of friends and family network as a job search method. We consider two variables: a dummy indicating whether the job seeker relies on personal networks or not, possibly combined with other job search methods and a dummy equal to one if the person uses exclusively his/her network. Simple OLS regressions including the full set of controls used throughout the paper (individual characteristics including employment status, neighborhood unemployment rate, quarter and municipality fixed effects), reveal that neighborhood diversity does not relate to these variables, as shown in Table 3.12. Yet, individuals' origin matter to some extent in explaining the use of networks to search for a job. People with Mediterranean, Maghrebian and other African origins (taking 2nd generation into account) are more likely to rely on personal networks than natives (column 3). Interestingly, European (other than South European) and African citizens are also more likely to rely exclusively on networks (column 4), revealing a low level of integration for those particular groups. For those minority groups that heavily rely on networks to look for jobs, living in more diverse areas and hence being cut from the bulk of their friends and family might therefore hinder their job search efforts.

Alternatively, the LFS asks employed workers to indicate the main channel through which they found their current job. We build a variable equal to 1 if the individual found his/her job through personal contacts, and 0 otherwise, which we regress on diversity using the same specification as for job search methods. Table 3.13 reports the estimates of neighborhood diversity, which do not significantly differ from zero, suggesting that living in a more diverse environment do not influence the chance to find a job through contacts. However, any employed foreigner is more likely to have found his/her job using networks than French citizens (column 1). This is especially true for individuals of South European and Rest of the World nationalities. The coefficients decrease (or even vanish) as other measures of origins are considered, suggesting that networks are particularly helpful for the least integrated people, i.e. those who are of foreign origin but who have not yet been naturalized. Although these results are to be interpreted with caution because they do not correct for selection and do not control for the search methods that were actually used, they suggest that friends and family network is an important vector of employment for foreign individuals. Therefore, even if diversity is not directly involved in the use of networks to search and find jobs, it might still be an issue for minorities if they live in diverse areas, isolated from the core of their network.

3.7 Conclusion

The findings of this paper bring new insights to the literature on diversity. First, measuring diversity based on various definitions of origins reveals that diversity in terms of nationalities matters more than diversity in terms of parents origins. This is a key result, as it means that diversity of origins plays a role through the variety of cultures and languages rather than through ethnic diversity *per se*. This speaks in favor of the idea that diversity affects employment prospects by altering job information transmission. Second, measuring diversity at different geographic levels reveals that this effect is not independent from the level of observation. Neighborhood diversity reduces employment prospects, while employment zone diversity is neutral, after correcting for endogenous sorting. This implies that the mechanisms through which diversity hinders employment at a local level are counterbalanced at a more aggregate level. In particular, job seekers might be unable to develop efficient networks in their own neighborhood because of diversity, but they might instead rely on a network established in a larger area. More generally, this work calls for a new approach of the literature on diversity, as it shows that (i) the notion of diversity hides various aspects that can influence the outcome considered in different ways, and that (ii) the effect of diversity can vary according to the geographical level considered.

Although part of this paper is devoted to tests the hypothesis that the negative impact of local diversity on employment prospects is related to job information transmission, much remains to be done in this direction. In addition, a natural subsequent question is that of the quality of the job found in terms of tenure or wage for instance. These issues remain open for future research.

Table 3.1: Sample Description: 16-65 y.o. individuals, 2007-2010

	[Min-Max]	Mean	(Std Dev)
Male	[0-1]	49.30	(50.00)
Age	[16-65]	42.92	(12.71)
Experience (years)	[0-63]	24.00	(13.82)
Employment Status			
Employed	[0-1]	0.714	(0.452)
Unemployed	[0-1]	0.066	(0.249)
Inactive	[0-1]	0.220	(0.414)
Socio-Economic Category			
Farmer	[0-1]	0.018	(0.133)
Craftsman, shopkeeper	[0-1]	0.059	(0.236)
Executive or other high position	[0-1]	0.143	(0.350)
Intermediate occupation	[0-1]	0.225	(0.417)
Employee	[0-1]	0.304	(0.460)
(Factory) worker	[0-1]	0.240	(0.427)
Unemployed never employed	[0-1]	0.011	(0.106)
Level of Education			
Master, PhD, schools	[0-1]	0.077	(0.267)
Graduate (bac+4)	[0-1]	0.030	(0.170)
Under-graduate (bac+3)	[0-1]	0.037	(0.188)
Lower under-grad (bac+2)	[0-1]	0.125	(0.331)
General Baccalaureat	[0-1]	0.081	(0.272)
Techno. / Pro. Baccalaureat	[0-1]	0.075	(0.263)
bretech	[0-1]	0.020	(0.140)
cap	[0-1]	0.253	(0.435)
bepc	[0-1]	0.082	(0.274)
No Diploma	[0-1]	0.222	(0.415)
Employed workers characteristics			
Hourly wage (log)	[-5.02-6.828]	2.276	(0.445)
Tenure (months)	[0-792]	136.0	(125.6)
Public servant	[0-1]	0.280	(0.449)
Part time job	[0-1]	0.165	(0.371)
Permanent contract	[0-1]	0.835	(0.371)

These figures are obtained using a sample of 920,388 individuals aged between 16 and 65 years old. It consists in the observations from the 16 successive waves of the labor Force Survey from 2007 to 2010.

Table 3.2: Distribution of individuals' origins, 2007-2010 (in %)

	Nationality	Birth Country	Parents
France	93.65	87.09	80.31
Southern Europe	1.51	2.18	6.30
Rest of Europe	1.07	1.81	3.31
Maghreb	1.86	4.86	5.66
Rest of Africa	0.81	1.79	1.80
Rest of the World	1.10	2.27	2.62
N	920,235	920,346	905,241

Reading: among the 15-65 y.o. individuals living in France, 1.86 % are of Maghrebian nationality, 4.86 % are born in Maghreb and 5.66 % have a Maghrebian origin, either by their nationality or through their parents'.

Table 3.3: Diversity in individuals' living environment

	[Min-Max]	Mean	(Std Dev)	Median
Neighborhood diversity				
Nationality	[0-0.771]	0.089	(0.131)	0.034
Birth Country	[0-0.803]	0.175	(0.162)	0.132
Parents	[0-0.818]	0.280	(0.207)	0.246
Employment Zone diversity				
Nationality	[0-0.559]	0.099	(0.080)	0.078
Birth Country	[0-0.731]	0.190	(0.117)	0.166
Parents	[0-0.735]	0.325	(0.171)	0.317

Reading: Individuals live in neighborhoods where diversity in terms of nationality amounts to 8.9 % on average. They live in employment zones where diversity in terms of birth country amounts to 19 % on average. Alternatively: there is a 32.5 % chance that two individuals living in the same employment zone are from different origin background.

Table 3.4: Employment Status and Diversity by Nationality

	No controls	Individual Characteristics	Ind. charac., Time & Geo. FE,	Ind. charac., Time & Geo. FE, Local Unemployment
	(1)	(2)	(3)	(4)
Diversity by Nationality				
Local Neighborhood	-0.364***	-0.172***	-0.181***	-0.105***
	(0.016)	(0.013)	(0.013)	(0.011)
Employment Zone	0.447***	0.189***	0.122***	0.090***
	(0.024)	(0.018)	(0.028)	(0.025)
Nationality (Ref.: French)				
South European		0.103***	0.098***	0.087***
		(0.009)	(0.009)	(0.009)
Other European		-0.125***	-0.124***	-0.132***
		(0.013)	(0.013)	(0.013)
Maghrebian		-0.143***	-0.139***	-0.130***
		(0.011)	(0.011)	(0.011)
Other African		-0.122***	-0.126***	-0.125***
		(0.017)	(0.017)	(0.017)
Other nationality		-0.111***	-0.115***	-0.117***
		(0.014)	(0.014)	(0.014)
Education (Ref: Baccalauréat)				
Master, PhD & schools		0.049***	0.048***	0.047***
		(0.006)	(0.006)	(0.006)
Graduate (bac+4)		0.034***	0.033***	0.034***
		(0.007)	(0.007)	(0.007)
Under-graduate (bac+3)		0.044***	0.045***	0.045***
		(0.006)	(0.006)	(0.006)
Lower under-grad (bac+2)		0.039***	0.039***	0.038***
		(0.005)	(0.005)	(0.005)
Techno. & Pro. Baccalauréat		0.037***	0.038***	0.037***
		(0.005)	(0.005)	(0.005)
Technical degree		0.013*	0.012	0.012
		(0.008)	(0.008)	(0.008)
cap		-0.004	-0.004	-0.003
		(0.005)	(0.005)	(0.005)
bepc		-0.039***	-0.037***	-0.035***
		(0.006)	(0.006)	(0.006)
No diploma		-0.082***	-0.080***	-0.073***
		(0.005)	(0.005)	(0.005)

Table 3.4: Employment Status and Diversity by Nationality (C'ed)

	No controls	Individual Characteristics	Ind. charac., Time & Geo. FE,	Ind. charac., Time & Geo. FE, Local Unemployment
	(1)	(2)	(3)	(4)
Socio-economic category (Ref:)				
Craftsman, shopkeeper		0.204***	0.206***	0.205***
		(0.010)	(0.010)	(0.010)
Executive or other high position		0.191***	0.191***	0.190***
		(0.010)	(0.010)	(0.010)
Intermediate occupation		0.162***	0.163***	0.163***
		(0.010)	(0.010)	(0.010)
Employee		0.150***	0.152***	0.154***
		(0.010)	(0.010)	(0.009)
(Factory) worker		0.098***	0.101***	0.105***
		(0.010)	(0.009)	(0.009)
Other individual characteristics				
Male		0.087***	0.087***	0.087***
		(0.002)	(0.002)	(0.002)
Age		0.068***	0.068***	0.067***
		(0.002)	(0.002)	(0.001)
Age^2		-0.001***	-0.001***	-0.001***
		(0.000)	(0.000)	(0.000)
Experience		0.007***	0.007***	0.007***
		(0.001)	(0.001)	(0.001)
Experience^2		-0.000***	-0.000***	-0.000***
		(0.000)	(0.000)	(0.000)
Local unemployment rate				-0.272***
				(0.014)
Intercept	0.682***	-0.651***	-0.608***	-0.570***
	(0.003)	(0.027)	(0.031)	(0.030)
Quarter F.E.	No	No	Yes	Yes
Department F.E.	No	No	Yes	Yes
N	162,097	151,053	151,053	150,913
Adj. R^2	0.009	0.254	0.257	0.260

The dependent variable indicates the employment status of an individual in a given quarter. It takes value 1 if the individual is employed, and 0 otherwise (unemployed or inactive). It is regressed on diversity by nationality based on the 6-categories nationality variable. Each column corresponds to a different specification. In column (1), the employment dummy is regressed on neighborhood and employment zone diversity, without any other control. Column (2) controls for individual characteristics: origin group (6 categories), gender, quadratic function of age, education (10 categories), socio-economic category (6 categories), quadratic function of experience. Column (3) = (2) + quarter fixed effects + *département* fixed effects. Column (4) = (3) + unemployment rate in the local neighborhood (excluding the individual). The sample is made of the first observation of each individual. Standard errors clustered at the neighborhood level are reported in parentheses.
* p<0.10, ** p<0.05, *** p<0.001

Table 3.5: Employment status and diversity: summary of the results (OLS)

	No controls	Individual Characteristics	Ind. charac., Time & Geo. FE,	Ind. charac., Time & Geo. FE, Local Unemployment
	(1)	(2)	(3)	(4)
1. Diversity by Nationality				
Local Neighborhood	-0.364***	-0.172***	-0.181***	-0.105***
	(0.016)	(0.013)	(0.013)	(0.011)
Employment Zone	0.447***	0.189***	0.122***	0.090***
	(0.024)	(0.018)	(0.028)	(0.025)
2. Diversity by Birth Country				
Local Neighborhood	-0.346***	-0.149***	-0.156***	-0.092***
	(0.014)	(0.011)	(0.011)	(0.010)
Employment Zone	0.381***	0.151***	0.120***	0.088***
	(0.018)	(0.014)	(0.023)	(0.020)
3. Diversity by Parents Origins				
Local Neighborhood	-0.234***	-0.089***	-0.096***	-0.053***
	(0.011)	(0.009)	(0.009)	(0.008)
Employment Zone	0.215***	0.091***	0.056***	0.041**
	(0.013)	(0.010)	(0.016)	(0.014)
Individual controls	No	Yes	Yes	Yes
Quarter F.E.	No	No	Yes	Yes
Département F.E.	No	No	Yes	Yes
Local unemployment rate	No	No	No	Yes

The dependent variable indicates the employment status of an individual in a given quarter. It takes value 1 if the individual is employed, and 0 otherwise (unemployed or inactive). It is regressed on diversity by nationality in the first set of regressions (**1.**), on diversity by birth country and by parents' origins in the second (**2.**) and third (**3.**) sets of regressions respectively. Fractionalization indices are based on the 6-categories origin variables. Each column corresponds to a different specification. In column (1), the employment dummy is regressed on neighborhood and employment zone diversity, without any other control. Column (2) controls for individual characteristics: origin group (6 categories), gender, quadratic function of age, education (10 categories), socio-economic category (6 categories), quadratic function of experience. Column (3) = (2) + quarter fixed effects + *département* fixed effects. Column (4) = (3) + unemployment rate in the local neighborhood (excluding the individual). The sample is made of the first observation of each individual. Standard errors clustered at the neighborhood level are reported in parentheses. * p<0.10, ** p<0.05, *** p<0.001

Table 3.6: Employment status and diversity: summary of the results (Multinomial logit)

Dep Var:	Employment Status (Ref: Employed)	
	Unemployed	Inactive
1. Diversity by Nationality		
Local Neighborhood	0.921***	0.463***
	(0.090)	(0.092)
Employment Zone	-0.566**	-0.628**
	(0.228)	(0.207)
1. Diversity by Birth Country		
Local Neighborhood	0.929***	0.351***
	(0.083)	(0.078)
Employment Zone	-0.580**	-0.603***
	(0.187)	(0.167)
1. Diversity by Parents' Origins		
Local Neighborhood	0.687***	0.201**
	(0.069)	(0.062)
Employment Zone	-0.387**	-0.273**
	(0.129)	(0.114)
Individual controls	Yes	
Local unemployment rate	Yes	
Quarter *dep.* F.E.	Yes	

The dependent variable indicates the employment status of an individual in a given quarter. It takes value 1 if the individual is employed (reference category), 2 if s/he is unemployed and 3 if s/he is inactive. It is regressed on diversity by nationality based on the 6-categories origin variables. It is regressed on diversity by nationality in the first regression (**1.**), on diversity by birth country and by parents' origins in the second (**2.**) and third (**3.**) regressions respectively. The results come from a multinomial logit estimation, using the full specification. In each regression, the following controls are included: individual characteristics (origin group, gender, quadratic function of age, education, socio-economic category, quadratic function of experience), local unemployment rate, and quarter and *département* fixed effects. The sample is made of the first observation of each individual. Standard errors clustered at the neighborhood level are reported in parentheses. * p<0.10, ** p<0.05, *** p<0.001

Table 3.7: Employment status and local diversity: considering within area variation

	Département FE	Employment Zone FE	Municipality FE	Sector FE
	(1)	(2)	(3)	(4)
Local Neighborhood Diversity				
1. By Nationality	-0.094***	-0.109***	-0.140***	-0.110***
	(0.010)	(0.010)	(0.013)	(0.017)
2. By Birth Country	-0.079***	-0.094***	-0.120***	-0.080***
	(0.009)	(0.009)	(0.011)	(0.016)
3. By Parents Origins	-0.047***	-0.054***	-0.075***	-0.060***
	(0.007)	(0.007)	(0.009)	(0.012)

The dependent variable indicates the employment status of an individual in a given quarter. It takes value 1 if the individual is employed, and 0 otherwise (unemployed or inactive). The sample is made of the first observation of each individual. Fractionalization indices are based on the 6-categories origin variables. Each regression controls for the full set of individual characteristics, quarter and department fixed effects and local neighborhood unemployment rate. However, compared to the previous specification, employment zone diversity is not included so as to focus on the changes of local neighborhood diversity to the inclusion of the alternative fixed effects. Standard errors clustered at the neighborhood level are reported in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.001$

Table 3.8: Assessing within-sector sorting

	R^2 unconditional	R^2 conditional
Nationality		
French (CAT=1)	2.28%	0.27%
Mediteranean (CAT=2)	0.14%	0.02%
Other European countries (CAT=3)	0.15%	0.04%
Maghreb (CAT=4)	1.52%	0.27%
Other African countries (CAT=5)	0.20%	0.03%
Rest of the world (CAT=6)	0.42%	0.11%
Education		
Graduate degree	4.73%	0.17%
Undergraduate	0.17%	0.03%
High School Professional	0.18%	0.08%
Drop out	3.71%	0.25%
Age		
Age	2.84%	0.70%

Notes: We randomly select one individual per *area*. For the set of displayed variables, we regress the variable on the area average (leaving the selected individual and her household out) without and with sector level fixed effect. We repeat this procedures 100 times and report the average R-squares, without (Column (1)) and with sector fixed-effect (Column (2)).

Table 3.9: Effect of diversity on employment status: IV regressions

Instrument used:	OLS	IV: Expected Diversity	IV: Public Housing Diversity
	(1)	(2)	(3)
1. Diversity by Nationality			
Local Neighborhood	-0.105***	-0.092***	-0.104***
	(0.011)	(0.014)	(0.012)
Employment Zone	0.090***	-0.028	0.025
	(0.025)	(0.070)	(0.054)
First stage			
Expected Diversity		0.468***	
		(0.020)	
Public Housing Diversity			0.197***
			(0.006)
F-stat (excl. instr.)		537.20	937.02
Partial R^2 (excl. instr.)		0.133	0.252
2. Diversity by Birth Country			
Local Neighborhood	-0.092***	-0.081***	-0.092***
	(0.010)	(0.011)	(0.011)
Employment Zone	0.088***	0.006	0.046
	(0.020)	(0.044)	(0.046)
First stage			
Expected Diversity		0.723***	
		(0.024)	
Public Housing Diversity			0.209***
			(0.007)
F-stat (excl. instr.)		922.28	848.12
Partial R^2 (excl. instr.)		0.227	0.230

The dependent variable indicates the employment status of an individual in a given quarter. It takes value 1 if the individual is employed, and 0 otherwise (unemployed or inactive). The sample is made of the first observation of each individual. Fractionalization indices are based on the 6-categories origin variables. Each regression controls for the full set of individual characteristics, quarter and department fixed effects and local neighborhood unemployment rate. Standard errors clustered at the neighborhood level are reported in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.001$

Table 3.10: Quality of the relationships with neighbors and diversity

Dep. Var.:	Quality of Neighborhood Relationships (Ref: Good)		
	Average	Bad	No relationship
1. Diversity by nationality	1.434***	1.708***	0.882***
	(0.235)	(0.506)	(0.179)
2. Diversity by birth country	1.617***	1.958**	1.206***
	(0.273)	(0.610)	(0.207)

Each line reports the coefficients from a separate multinomial logit regression. The dependent variable indicates opinion about the relationships with the neighbors. It takes value 1 if the surveyed individual declares having good relationships with his/her neighbors (reference category), 2 if the relationships are average, 3 if they are bad, and 4 if there is no relationship at all. The main variable of interest is the level of diversity, computed at the block level, based on nationalities in the first regression and on birth countries in the second one. In each specification, the following controls are included: individual characteristics (age, gender, origin, employment status, education, household income), block level unemployment rate, department fixed effects and a detailed indicator of the social and economic composition of the neighborhood (27 categories). The data come from the 2002 French Housing Survey and the 1999 population census (INSEE). Standard errors clustered at the neighborhood level are reported in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.001$

Table 3.11: Effect of the share of neighbors from own origin group on employment probability

Dep. Var.: Employment status (employed vs unemployed or inactive)		
	(1)	(2)
1. Origins: Nationality		
Neighborhood share of same origin	0.154***	0.046**
	(0.016)	(0.023)
Neighborhood Diversity		-0.115***
		(0.018)
2. Origins: Birth country		
Neighborhood share of same origin	0.073***	-0.014
	(0.013)	(0.163)
Neighborhood Diversity		-0.127***
		(0.014)
3. Origins: Parents		
Neighborhood share of same origin	0.037***	0.002
	(0.008)	(0.010)
Neighborhood Diversity		-0.074***
		(0.011)

Each column and each set of results (1., 2. and 3.) report the coefficients from a separate OLS regression. The dependant variable indicates whether the individual is employed (1) or unemployed or inactive (0). In each specification, the following controls are added to the variables displayed: individual characteristics (origin group, gender, quadratic function of age, education, socio-economic category, quadratic function of experience), neighborhood unemployment rate (excluding the individual), quarter dummies and municipalities fixed effects. The sample is made of the first observation of each individual. Standard errors clustered at the neighborhood level are reported in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.001$

Table 3.12: Job search method: use of friends and family networks

Dep Var:	Use of networks			Exclusive use of networks		
Origins:	Nationality	Birth country	Parents	Nationality	Birth country	Parents
	(1)	(2)	(3)	(4)	(5)	(6)
Neighborhood diversity	-0.027	-0.035	-0.005	-0.007	-0.006	-0.004
	(0.025)	(0.023)	(0.019)	(0.007)	(0.006)	(0.005)
Origin Group: (Ref: France)						
South Europe	0.024	0.027	0.033**	0.008	-0.003	-0.003
	(0.024)	(0.021)	(0.011)	(0.007)	(0.006)	(0.003)
Rest of Europe	-0.006	0.018	0.017	0.026***	0.022***	0.012**
	(0.021)	(0.017)	(0.014)	(0.006)	(0.005)	(0.004)
Maghreb	0.018	0.015	0.028**	0.005	0.000	0.001
	(0.014)	(0.010)	(0.009)	(0.004)	(0.003)	(0.002)
Rest of Africa	0.022	0.027*	0.030**	0.012**	0.003	0.007*
	(0.019)	(0.014)	(0.014)	(0.005)	(0.004)	(0.004)
Rest of World	0.049**	0.039**	0.033**	0.018***	0.009**	0.008**
	(0.019)	(0.015)	(0.013)	(0.005)	(0.004)	(0.004)

In the first three columns, the dependent variable indicates whether the individual relied on personal networks to search for a job, possibly combined with other job search methods. In the last three columns, the dependent variable indicates whether the individual relied on personal networks to search for a job, excluding the use of any other job search method. Fractionalization indices are based on the 6-categories origin variables. The estimates come from OLS regressions. In addition to diversity and origin group (which differ in each column), each regression controls for employment status, gender, age, age squared, education, SEC, experience, experience squared, neighborhood unemployment rate, quarter dummies and municipality fixed effect are also included. The sample is made of the first observation of each individual. * $p<0.10$, ** $p<0.05$, *** $p<0.001$

Table 3.13: Main method through which the job was found: networks

Dep Var:	Job found through networks		
Origins:	Nationality	Birth country	Parents
	(1)	(2)	(3)
Neighborhood diversity	0.012	-0.003	-0.012
	(0.016)	(0.015)	(0.012)
Origin Group: (Ref: France)			
South Europe	0.141***	0.103***	0.057***
	(0.011)	(0.009)	(0.006)
Rest of Europe	0.040**	0.023**	0.034***
	(0.015)	(0.011)	(0.008)
Maghreb	0.031**	0.006	-0.003
	(0.012)	(0.008)	(0.007)
Rest of Africa	0.038**	-0.000	-0.002
	(0.016)	(0.011)	(0.011)
Rest of World	0.178***	0.114***	0.103***
	(0.014)	(0.010)	(0.009)

The dependent variable indicates whether the currently employed individual found his/her job through personal network. The sample is made of the first observation of each employed individual, and excludes civil servants. Fractionalization indices are based on the 6-categories origin variables. The estimates come from OLS regressions. In addition to diversity and origin group (which differ in each column), each regression controls for gender, age, age squared, education, SEC, experience, experience squared, neighborhood unemployment rate, quarter dummies and municipality fixed effect are also included. * p<0.10, ** p<0.05, *** p<0.001

3.A Appendix

3.A.1 Construction of the predicted level of employment zone diversity

In this appendix, we describe more formally the construction of the "shift-share" instrumental variable. Denote $N^g_{France,1968}$ the number of individuals from origin group $g = 1, ..., g_{max}$ in France in 1968 and $N^g_{EZ_j,1968}$ the number of individuals from origin group $g = 1, ..., N_g$ in employment zone $j = 1, ..., N_j$ in 1968. Then, the share of group g individuals, living in employment zone j in 1968 (out of the total number of group g individuals in France in 1968) can be computed as follows:

$$S^g_{EZ_j,1968} = \frac{N^g_{EZ_j,1968}}{N^g_{France,1968}} \tag{3.3}$$

with $\sum_{j=1}^{N_j} S^g_{EZ_j,1968} = 1$, for any group g.

Then, the expected number of group g individuals living in employment zone j in year $t = 2007, ..., 2010$ is given by:

$$\widehat{N^g_{EZ_j,t}} = S^g_{EZ_j,1968} * N^g_{France,t} \tag{3.4}$$

From this, we can deduce the expected share of group g individuals in employment zone j in year t (out of the total number of individuals living in employment zone j in t, all groups included):

$$\widehat{s^g_{EZ_j,t}} = \frac{\widehat{N^g_{EZ_j,t}}}{\sum_{g=1}^{N_g} \widehat{N^g_{EZ_j,t}}} \tag{3.5}$$

with $\sum_{g=1}^{N_g} \widehat{s^g_{EZ_j,t}} = 1$, for any employment zone j.

Finally, the predicted measure of diversity in employment zone j in t is obtained as follows:

$$\widehat{DIV_{EZ_j,t}} = 1 - \sum_{g=1}^{N_g} \widehat{s^g_{EZ_j,t}}^2 \tag{3.6}$$

References

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Chapter 4

The (declining) strength of local referral networks

Joint with Camille Hémet

4.1 Introduction

A large and growing body of empirical research has shown that informal networks play a pervasive role in modern labor markets. Studies based on survey data typically find that most unemployed resort to informal methods to search for jobs and that a large share of employees states having found their jobs through such methods. For instance, the seminal study by Holzer (1988) shows, based on a sample of unemployed youth in the United-States, that 90 % of them are searching for a job through a friend or relative while 50 % only rely on state agency.[1] A more recent strand of the literature relies on administrative data and matched employer-employee datasets to indirectly detect informal referral networks. The seminal work by Bayer, Ross, and Topa (2008) shows that direct neighbors, i.e. people living in the same census block, are much more likely to work in the same census block than residents of nearby blocks. The tendency of close neighbors to display an excess clustering in the same workplace is confirmed by Hellerstein, McInerney, and Neumark (2011) (who use firm rather than work-location to determine coworkers). This evidence suggests that individuals share employment opportunities with their immediate neighbors and that overall, referral networks have an important spatial dimension.

[1] Job search methods of are not mutually exclusive. The author distinguished five different search methods: friends and relatives, responses to newspaper advertisements, state employment agencies, direct applications to employers, and "other" methods. These categories are those commonly used in labor force surveys, see e.g. Addison and Portugal (2002).

In this paper, we present new evidence on the role of neighborhood-based referral networks in the case of France. Using specific features of the Labor Force Survey in France, we are able to use the approach developed in Bayer, Ross, and Topa (2008) to assess the strength of referral network. Based on a very fine definition of housing block (20 to 40 households), we measure the strength of informal hiring network as the differential propensity of individuals living in the same block to work in the same firm in comparison with residents of a contiguous block, living so to speak "across the street". (More details on the definition of neighborhoods will be given in Section 4.2)

We contribute to the literature on local social interactions in three main ways. First, we present evidence on how the strength of local referral network has evolved over time. In a review of the literature, Topa (2011) points out that little evidence has been produced regarding the changes over time of the strength of referral networks. This is particularly true of local interactions. Using retrospective data on the time of entry in the currently held job, we can cover the 1970 to 2010 period. This period has been characterized by many disruptions, rising level of residential mobility, the advent of the internet as a job search tool etc., which should presumably lead to lower levels of local social interactions. Second, by using the employer ID included in the LFS, this paper examines explicitly the comparability between the indirect approach to detect informal referral network and the more classic approach relying on survey questionnaires regarding job finding method. Moreover, it allows us to compare the evolution overtime of the strength of residential referral networks with that of referral networks in general. Finally, using specific features of the French LFS and housing market, we are able to provide several robustness checks of our results regarding two main threats to identification, sorting and reverse causality. Notably, using the short panel dimension of the data, we can focus part of the analysis on job transitions and analyse in which type of transition (job-to-job versus out of non-employment) the use of local referral is the most intensive. It is also noteworthy that most evidence on the existence of neighborhood-based referral network is based on American data. Evidence for Continental Europe and particularly France is much more scarce and we contribute to fill this gap.[2]

Our empirical strategy to detect local referral network follows Bayer, Ross, and Topa (2008) and Schmutte (2015). We use a very fine definition of nested neighborhood. The primary unit is called *area* and generally contains between 20 and 40 households. *Areas* are grouped by *sector* which each contains 6 areas, and therefore between 120 and 240 house-

[2] An exception is Hawranek and Schanne (2015) who study residential job referral networks in Germany using matched employer-employee data but do not consider the evolution overtime of the strength of such networks and do not consider job transitions.

holds. The identifying assumption rules out the possibility of *within-sector* sorting but allows for the possibility of sorting *across sectors*. We discuss this assumption later in the paper and carry out a test in the spirit of Altonji, Elder, and Taber (2005).

We find that being close neighbors increases the probability of being coworkers relatively to neighbors living in nearby area by 0.35 % on average, which represent about a third of the baseline probability. We assess the robustness of our results with respect to several threats to identification. Our local fixed-effect approach requires the absence of sorting within *sector*. We show that while the raw data exhibits a large amount of sorting on observables, conditional on sector-fixed effect there is virtually no correlation between individual and neighborhood-level predetermined variables (such as educational achievement or being foreigner). The absence of within-sector sorting on observables supports the validity of the local-fixed effect approach.

In order to deal with the issue of reverse causality, i.e. the possibility that people might be meeting at work and referring each other on the housing market, we proceed to two robustness checks. First, we estimate this effect restricting the sample to public housing tenants. Given the shortage of public housing as well as the allocation mechanism of public housing units, the risk of reverse causality is non-existent in this setting. Second, we exploit the short-longitudinal dimension of the panel to measure the role of local referral network on labor market transitions. Looking at individuals changing jobs rules out the problem of reverse causality albeit at the cost of a substantial reduction in sample size.

Going further, we use information on date of arrival within current firm and find that the strength of local referral network has considerably decreased overtime. We document a steep decline of the effect of being *area*-neighbors on the probability of being coworker. It progressively goes down from 0.85% for people employed in their jobs since the 1970 and 1974 to 0.11 % for people who found their jobs after 2010. We contrast this decline with the stable share over job finding periods of individuals stating having found their job through their individual networks. This suggests that referral networks have become less localized over time.

4.2 Data and institutional setting

4.2.1 The French Labor Force Survey and its clustered sampling scheme

In this paper, we use the French Labor Force Survey (*Enquête Emploi*, hereafter the LFS), which is conducted quarterly by the French statistical institute (INSEE)since 2003. One sixth of the sample is renewed each quarter, so that the survey takes the form of a rotating-panel, as each household is surveyed for six consecutive waves before leaving the sample. Each wave of the survey comprises about 72,000 respondents aged 15 years-old or older. The sampling strategy of the LFS makes it particularly valuable for studying neighborhood effects: instead of drawing a random sample of homes over the French territory, the INSEE draws a random sample of neighborhoods made of twenty households on average [3], which are then exhaustively surveyed. To be more precise, France is initially divided into primary units for the needs of this survey, which are themselves split into sectors (*Secteur* in French.). A sector is defined so as to contain between 120 and 240 homes, and its boundaries are following urban boundaries such as roads and railways. One sector is selected in each primary unit, with a probability proportional to its size in terms of the number of homes. Each selected sector is then divided into neighborhoods of twenty homes (*aires*), and six neighborhoods are randomly picked within each sector. One of these six neighborhoods will be exhaustively surveyed over six quarters, at which point a second neighborhood of the same sector enters the sample, and so on, until all six neighborhoods of a given sector are surveyed.[4] This sampling strategy, designed to save money and time, implies that we can perfectly observe all individuals living in these neighborhoods and hence characterize the socio-economic and demographic environment of surveyed individuals.

An important issue given our purpose is that sector are sampled one area at a time. This implies that no individuals belonging to the *same* sector and *different* areas are interviewed on the same quarter. To cope with this issue with minimum measurement error, we consider only the first and last wave of interrogation of each area, and redefine a sector as a two distinct areas belonging to the same sector and interviewed one quarter apart. Such a definition results in a rather small reference neighborhood (relative to base neighborhood) which might limit our ability to detect small effect but ensure that the subcomponents of each sector are very similar. Moreover, focusing on areas sampled one quarter apart limits the problems caused by firm mortality over time which would lead us to over-estimate the

[3] The French term is *aires*. We are going to refer to *aires* as *areas*

[4] For more details on the sample composition and selection, please refer to INSEE documentation

impact of being area-neighborhood on working in the same firm.[5]

4.2.2 Public housing in France

Several previous studies have used the fact that given the relative shortage of public housing in France (resulting in long waiting lines), individuals are very unlikely to be picky regarding which particular unit they get allocated, which in turns makes the social make-up of their immediate neighbors as good as random (see e.g. Goux and Maurin, 2007; Algan, Hémet, and Laitin, 2015). A subset of our analysis focuses on public tenants. Unlike previous work, we do not rely on the assumption that neighbors are exogenous within public housing block. Instead, we focus on public tenants because they are much less exposed to the issue of reverse causality. Given the allocation mechanism of public housing, it is highly unlikely that individuals working for the same employer will meet in the work place and refer each other on the housing market.[6]

4.2.3 Description of the sample and descriptive statistics

Our main sample consists of individuals between the age of 25 and 59 employed in a firm with a correct employer ID at the time of the survey between the first quarter of 2003 and and the last quarter of 2012. The unit of observations is a "couple" of neighbors living in the same *sector*. Individuals belonging to the same household are not considered as neighbors. We focus on the first and last interrogations of each individual, so that they can be matched residents from another *area* belonging to the same *sector* with a gap of only quarter in the period of observation.

In our final sample, areas and sectors contain on average about 15 and 30 employed individuals respectively. The final sample contains about 3,2 millions pairs of sector-level neighbors, who live in about 10,000 different areas and about 3000 sectors. Table 4.1 some key summary statistics regarding the sample of individuals and pairs.

[5]Every year a substantial share of firms in France go through changes in their official ID-number. Such changes do not always imply that these firms are actually closing or opening. In fact, a majority of changes in ID-number are related to firm demographic operations which are associated with very little discontinuities in the labor market situation of its employees Picart (2008). As a robustness check, we restrict use retrospective job tenure information to restrict the sample to individuals who have been working in the same firm for more than a quarter, see Section 4.4.1.

[6]See Algan, Hémet, and Laitin (2015) for a more in depth discussion of assignment to public housing units.

4.3 Empirical Approach

4.3.1 Baseline

We start by estimating the following specification:

$$W_{ij} = \alpha R^a_{ij} + \rho_{s(ij)} + \varepsilon_{ij} \quad (4.1)$$

where W_{ij} and R^a_{ij} are binary variables equal to one if, respectively, individuals i and j work in the same firm and live in the same area $a(ij)$. The term $\rho_{s(ij)}$ is a sector-fixed effect associated with the sector $s(ij)$ that includes the area $a(ij)$. For most of our estimations, we define neighborhood such that there are two areas per sector (more details are included section 4.2).

In equation 4.1, the coefficient α capture the extent to which area-neighbors tend to work in the same firm relatively to sector-neighbors. Given the small size of sectors and the geographical contiguity of areas within a sector, one can safely rule out that differences in the propensity to work in the same firm arises because of different access to employment or transport infrastructure, factor which have been shown to be important determinants of labor market outcomes in urban settings.[7] Therefore the crux of the approach is to neutralize most obvious determinants of neighbors clustering within the same firm by using sector-level fixed-effect, and interpret the remaining within-sector, between areas differences in propensity to work in the same firm as evidence of informal hiring network.

This strategy therefore hinges on two mains assumptions. The first is that local interactions between neighbors that shape referral-networks are more local than other factors affecting the likelihood of two individuals to work in the same firm. A violation of that assumption would lead us to find no effect. The second assumption is that there is no within-sector sorting on unobservable characteristics that could be driving the probability of two area-neighbors to work in the same firm. A violation of this assumption would lead us to overestimate the important of local referral network. While this assumption is intrinsically unverifiable, we sketch in the next section a natural test based on observed covariates. Indeed, the presence of sorting on observables would make the assumption of no sorting on unobservables particularly untenable while its absence would strengthen its plausibility.

4.3.2 Sorting within-*sector*

We rewrite the baseline model in 4.1 in terms of deviations from sector-average:

[7]See Gobillon, Magnac, and Selod (e.g. 2011) for an analysis of spatial mismatch in the Parisian region.

$$\widetilde{W}_{ij} = \alpha \widetilde{R}^a_{ij} + \widetilde{\varepsilon}_{ij} \tag{4.2}$$

where $\widetilde{z}$ refers to the residual of from a regression of z on a set of sector-dummies. The identifying assumption, which is not directly testable, writes as:

$$Corr(\widetilde{\varepsilon}_{ij},\ \widetilde{R}^a_{ij}) = 0 \tag{4.3}$$

To support this assumption, in the spirit of Schmutte (2015), we attempt to test to which extent individuals seems to be sorting on observable characteristics. To test this, we start by randomly selecting one individual per area. We regress some observable x_i, say years of education, on the neighborhood average of that same variable $x_{a(i)}$ (excluding selected individuals in the computation of the area-average) and repeat the same procedure including sector fixed-effect, i.e. regressing $\widetilde{x}_i$ on $\widetilde{x}_{a(i)}$.

A large degree of sorting across sectors, i.e. large value of $R^2(x_i, x_{a(i)})$, seems plausible and is not incompatible with our identification strategy, however we expect a very low degree of within sector sorting, that is low values for $R^2(\widetilde{x}_i, \widetilde{x}_{a(i)})$.

Table 4.2 displays the results for several variables of interest. We see clearly that college-educated individuals tend to live in areas with a high share of college educated neighbors. The R-square is on average over the simulations of 6.83 %. However including sector fixed effects divides the R-square by a factor of about 10. It seems therefore that the amount of within-sector sorting based on college-education is negligible. The same holds for other levels of education and a dummy variable for foreign nationality.

4.4 Baseline Results

4.4.1 Baseline results on residential referral networks

Table 4.3 present the baseline results. Column (1) shows that being area neighbors is associated with a 0.35% increase in the probability of being coworker. Given an unconditional probability of about 1, the effect represents roughly a third of the baseline probability, a magnitude very similar to what has been found on US data by Bayer, Ross, and Topa, 2008.

Column (2) to Column (6) constitutes a set of robustness checks. Column (2) focuses only on workers who have been employed in their firms for a quarter of more, such solving issues related to systematic measurement error due to inter-quarter firm mortality which would tend to bias upward our estimator of the referral network effect.

Column (3) to (5) use information on tenure and/or occupational status to restrict the sample to pairs (i, j) such that i has a shorter tenure (Column 3) than j, a lower occupational status

j (Column 4), or both (Column 5). The coefficients are very stable over the three columns. Column (6) focuses on public tenants. Again the rationale for focusing on these tenants is that causality within this subsample is very unlikely the run the other way. We find larger referral effect in this subsample. However expressed as a percentage of the baseline probability the magnitude of the effect is virtually identical to the first column.

Table 4.4 is a first look at the temporal evolution of this relationship. It splits the sample between the period prior and during the Great Recession (before and after 2009Q1). It seems that the strength of local social interactions is lower in the post-2009 period. Below, we analyze further the evolution overtime of the strength of local interactions by leveraging retrospective information on the date of entry in the current job.

4.4.2 Job transitions

Using the short longitudinal dimension of the data set, we run a second set of estimations based on the sample of individuals taking up a new job during between two quarters. Looking at transitions allows to circumvent the issue of reverse causality, although at the expense of sample size.

The baseline equation is identical to equation 4.1, except that the dependent variable TW_{ij} now is equal to one when individual i find a new job in the same company as j:

$$TW_{ij} = \alpha R^a_{ij} + \rho_{s(ij)} + \varepsilon_{ij} \tag{4.4}$$

In order to preserve a sample of size that is large enough, we consider all labor market transitions even if they do not correspond to the first or last quarter of observation of the area. We consider sector-neighbors of i all individuals j living in the same sector and sampled within a gap of 3 quarters or less with respect to i's transitions.

Theoretical predictions regarding whether referral network use is more prevalent for out-of-unemployment versus job-to-job transitions is ambiguous. Referral network as often considered as a way to reduce the noise around the signal relating to the productivity of an applicant (Dustmann, Glitz, and Schönberg, 2011; Brown, Setren, and Topa, 2016). Being employed itself conveys information about a worker's productivity and implies that she has access to a professional network. According to this line of argument, one would expect residential referral network to be more intensively used by individuals who are currently unemployed. However, being unemployed could also be associated with a stigma among neighbors thus causing referrals to be harder to obtain once unemployed. The data at hand allow us determine whether residential referral network are more intensely used for job-to-job or unemployment-to-job transitions.

We restrict the sample on individuals who were either inactive or unemployed prior to finding their current job.

$$TW_{ij} = \alpha_0 R^a_{ij} + \alpha_1 J_i R^a_{ij} + \alpha_2 J_i + \rho_{s(ij)} + \varepsilon_{ij} \tag{4.5}$$

Results in Table 4.5 display the results. We find evidence of local social interaction: local referral network seems to be more relevant for unemployment to job transitions than job-to-job transitions. A lot of studies based on administrative data are focused on job-to-job transitions due the poor quality of data for people outside of employment (e.g. Schmutte, 2015). Our finding that suggests they might be missing an important role of local labor market referrals.

The presence of local referrals is a possible way to explain the presence of well-documented neighborhood effects. For instance, Solignac and To (2014) focus on young graduates in France and find that local (neighborhood) employment rate affects positively job access. Such positive neighborhood effect in terms of labor market entry might be partially driven by the direct referrals we detect here.

4.5 The declining strength of local referrals network

We now attempts to assess the evolution overtime of the strength of local referral network. At the moment, we use data from 2003 to 2012 (we are in the process of producing results with LFS from the 1993 onwards). We use retrospective information on the year at which workers entered in their current position. We denote this date e. We augment Equation 4.1 to allow the effect of area-neighborhood on the probability of being coworkers α to depend on e:

$$W_{ij} = \alpha(e_i) \times R^a_{ij} + \delta' X_i + \rho_{s(ij)} + \varepsilon_{ij} \tag{4.6}$$

We use two main specifications for $\alpha(e)$. First we allow α to vary discretely by interval of 5 years $\alpha(e_i) = \sum_h 1\{e_i \in b_h\}\alpha_{b_h}$. Then we model α as a polynomial of e: $\alpha(e_i) = \alpha_0 + \sum_h \delta^h e_i^h$. We focus on polynomials of order 2.

The vector X_i includes controls for job tenure and age (both with a quadratic term) as well as dummy variable for job finding periods. Note that the variable $e(i)$ is not collinear with tenure because we use cross-sections over several years.

We report the results of the first specification graphically in Figure 4.1. The graph plots the estimated $\hat{\alpha}_b$ where $p1970$ correspond to the 1970 to 1974 period, $b1975$ to the 1975 to 1979 period and so forth until the last period $p2010$ which corresponds to 2010-2012. It shows a clear decline overtime of the local referrals network, with a point estimate of 0.84

% for the 1970-1974 period down to a 0.14 % effect for the 2010-2012. We compare this strong decline with the share of individuals stating having found their jobs through their "personal network". This definition of network is not based on neighborhood but includes family members, friends or acquaintances. Figure 4.2. display this share by half-decade of job finding date.

4.6 Conclusion

In this paper, we document a set of new facts regarding the evolution of referral network overtime. Adopting the approach developed by Bayer, Ross, and Topa (2008) and applying it to the French Labor Force Survey, we find evidence of strong local referral network effects. We show that this effect is robust to numerous checks regarding the issue of sorting and reverse causality. We detect the presence of strong referral networks. We find that they are most intensively used for unemployment-to-job transition rather than job-to-job transitions. Taking advantage of the rich retrospective information from the LFS, we are able to track the evolution of this effect overtime. We document a steady decline in the strength of local referral network over time of entry in the current form. This decline cannot be explained by differences in terms of age or tenure of the workers. This decline contrasts with the stability in the share of individuals stating having found their job through their personal network, suggesting that while referral networks remained an essential feature of the labor market, networks themselves have become less spatially concentrated.

4.7 Tables and figures

Table 4.1: Descriptive statistics on individuals and pairs

Variable	mean (in %)
Individuals	
No degree	29.45
Prof. Degree	33.24
High-school	16.52
College	20.80
Foreign	3.88
Male	55.37
25-34 year old	27.70
35-44 year old	31.63
45-59 year old	40.67
Informal Job Finding method	22.70
# individuals	116,859
# pairs	3,262,782
Coworker present in the sector	1.06
# neighbors in the sector	49.63
# *areas*	10,081
# *sectors*	116,859

Notes: Individuals statistics are computed based on one observation by individual. Coworker present in sector is computed based on all pairs of workers.

Table 4.2: Sorting on observables, with and without "sector" fixed-effect: R^2 of bivariates regressions (in %)

Variable	$R^2(x, x_a)$	$R^2(\widetilde{x}, \widetilde{x}_a)$
No degree	2.74	0.53
High-school	1.91	0.31
College	6.83	0.72
Foreign	2.22	0.67

Notes: We randomly select one individual per *area*. For the set of displayed variables, we regress the variable on the area average (leaving the selected individual out) without and with sector level fixed effect. We repeat this procedures 100 times and stored the average R-squares, without (Column (1)) and with fixed-effect (Column (2)).

Table 4.3: Baseline results on direct referrals

	(1)	(2)	(3)	(4)	(5)	(6)
	$Baseline$	$Ten. > 1Q$	$LowTen.$	$LowOcc.$	$LowOcc.Ten.$	$Pub.Hsg$
R	0.355***	0.364***	0.335***	0.388***	0.375***	0.493***
	(8.589)	(8.51)	(8.34)	(8.89)	(8.19)	(4.80)
$\overline{W}$	1.061	1.082	1.035	1.145	1.104	1.36
Obs.	3262782	3170135	1601471	2552779	1263868	332926

Notes: Obs. refers to the number of pairs of neighbors used in the estimation. Standard errors are cluster at the sector-level. Column (1) presents results based on the entire sample. (2) restrict the sample to individuals who have a firm-tenure longer than 1 quarter. (3) restricts the sample to pairs such that i has a lower job tenure than j. (4) restricts the sample to pairs such that i has a hierarchically-lower occupation than j. (5) Subsample that fits the intersection of the conditions in 3 and 4. (6) restricts the sample to public housing tenants.

Table 4.4: Results on direct referrals: before and during the Great Recession

	(1)	(2)	(3)	(4)	(5)	(6)
Prior						
	Baseline	$Ten. > 1Q$	*LowTen.*	*LowOcc.*	*LowOcc.Ten.*	*Pub.Hsg*
R	0.431***	0.441***	0.407***	0.471***	0.452***	0.490***
	(7.01)	(6.93)	(6.84)	(7.28)	(6.61)	(4.22)
$\overline{W}$	1.179	1.203	1.149	1.274	1.149	1.389
Obs.	2043211	1985444	1002988	1599986	792634	227671
During						
	Baseline	$Ten. > 1Q$	*LowTen.*	*LowOcc.*	*LowOcc.Ten.*	*Pub.Hsg*
R	0.229***	0.236***	0.215***	0.252***	0.245***	0.499**
	(5.85)	(5.83)	(4.53)	(4.97)	(4.89)	(2.47)
$\overline{W}$	0.863	0.88	0.844	0.93	0.844	1.298
Obs.	1219571	1184691	598483	952793	471234	105255

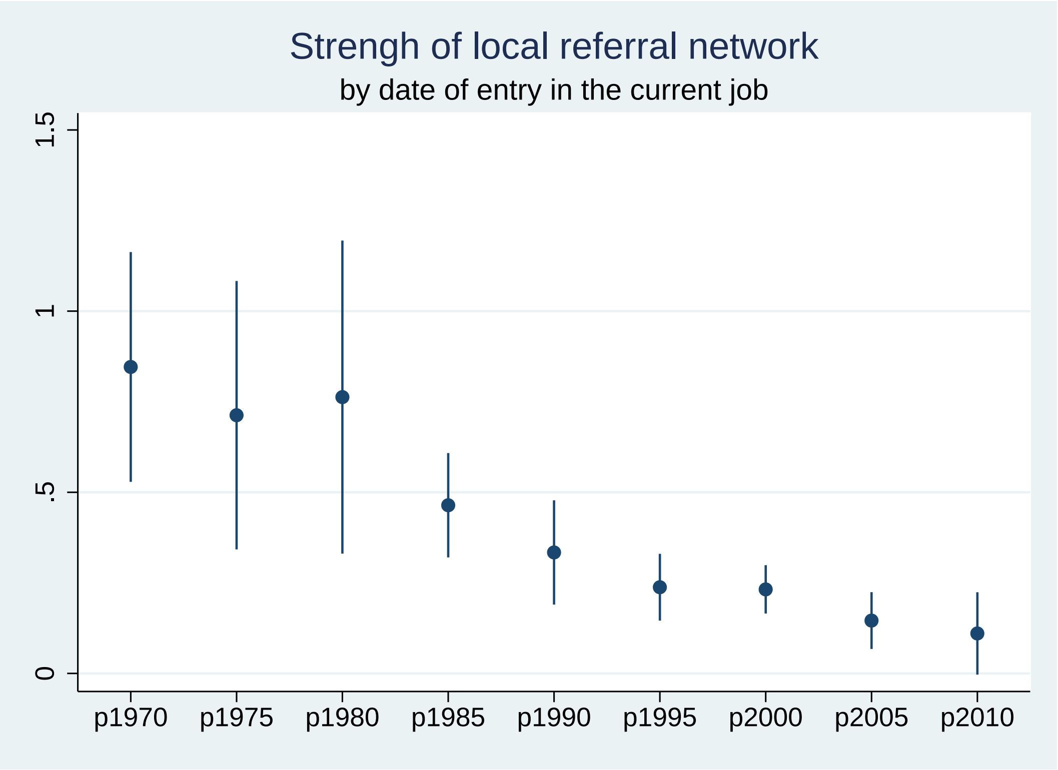
Notes: Obs. refers to the number of pairs of neighbors used in the estimation. Standard errors are cluster at the sector-level. The onset of the Great Recession is timed at 2009Q1. Column (1) presents results based on the entire sample. (2) restrict the sample to individuals who have a firm-tenure longer than 1 quarter. (3) restricts the sample to pairs such that i has a lower job tenure than j. (4) restricts the sample to pairs such that i has a hierarchically-lower occupation than j. (5) Subsample that fits the intersection of the conditions in 3 and 4. (6) restricts the sample to public housing tenants.

Table 4.5: Results on direct referrals: job-transitions

	(1)	(2)	(3)
	Baseline	J-to-J	U-to-J
R	0.123**	0.0411	0.342***
	(2.09)	(0.61)	(2.67)
$\overline{W}$	0.432	0.373	0.563
Obs.	88968	61621	27347

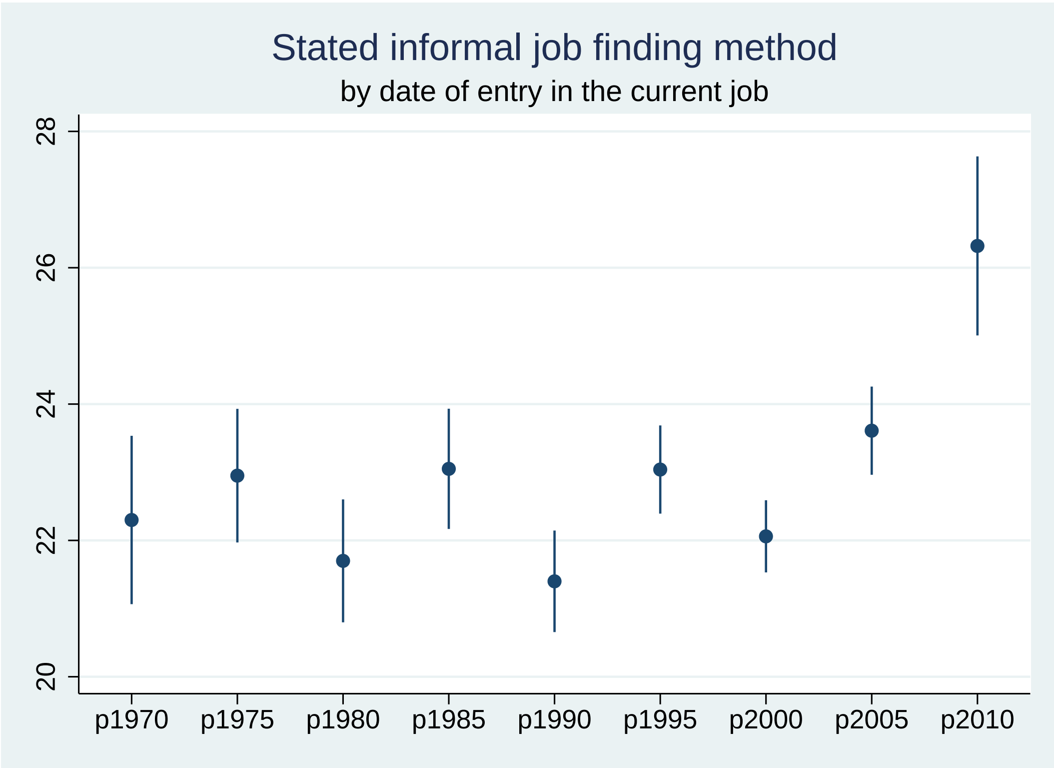
Notes: Results based on transitions. Areas-neighbors are observed contemporaneously and sector-neighbors are observed with a gap of at most 3 quarters with respect to quarter t when the transition occurs between t and $t + 1$.

Figure 4.1: The strength of local social interactions overtime



p1970 refers to job found during the 1970-1974 interval, p1975 refers to job found during the 1975-1979 interval and so forth. Each coefficient corresponds to a period-dummy interacted with a area-neighborhood dummy.

Figure 4.2: Share of individuals stating having found their job via their network (including non-local)



p1970 refers to job found during the 1970-1974 interval, p1975 refers to job found during the 1975-1979 interval and so forth. Each coefficient corresponds to the share of individuals stating that they have used an informal job finding method.

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