Technology-induced Trade Shocks? Evidence from Broadband Expansion in France

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Résumé

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A recent literature has documented the labor market effects of trade shocks by investigating the relationship between regions' labor market outcomes and their exposure to aggregate trade flows as predicted by their sectoral specialization. The large estimated effect has contributed to change the conventional view that trade had only benign effect on the labor markets of advanced economies. While technology was thought to be the key factor driving sectoral employment shares and wage structure over the 1990s, trade, in particular with China and Eastern-European countries seem to have mattered most over the 2000s. In this paper, we investigate the extent to which these national industry trade shocks were facilitated by concomitant technical progress, that is we test for the presence of "technology-induced" trade. To do so, we focus on broadband technology in France and use the staggered roll-out of this information technology to estimate its impact on the importing behavior of affected firms. Using a simple event-study design, we find that broadband expansion leads to an increase in the value of imported goods. We also document change in importing activities along several margins (number of origin countries, of products). Simple counterfactual suggests that absent broadband expansion, the growth in import penetration by Chinese and East-European products would have been between 20 and 25% lower over the 2001-2007 period.

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1 Introduction

Broadband internet is considered a driver of economic growth. Its impact on wages, unemployment and productivity have been widely studied (see Bertschek *et al.*, 2015, for a recent and exhaustive review). While there is a large body evidence on the effecy of broadband internet on firm-level productivity and labor demand (Akerman *et al.*, 2015), little is known regarding the impact of this technology on the international trade behavior of affected firms. This is surprising given that one can think of the economic impact of the internet as primarily a reduction in the information frictions between distant agents.

This paper aims fills this gap by documenting the impact of broadband technology on the importing behavior of firms in France. We exploit the staggered roll-out of broadband internet in France to estimate its impact on the importing behavior of affected firms. Using a simple dynamic difference-in-differences design, we find that broadband expansion leads to an increase in the value of imported goods (+20% after a five year periods). We also document change in importing activities along several margins (number of origin countries, of products). We find that the increase in the overall value of imports is associated with an increase in the number of flows and find no effect on the average value per flow (where a flow is defined as the combination of an importing firm - origin country - HS6 product). We find considerably larger effets on goods imported from China and Eastern European countries and no significant effect for trade with EU-15 countries.

We show that our results are robust to several potential threats to identification. First, our difference-in-differences relies on the usual common trend assumption and the event-study specification allows us to detect pre-expansion differential trends. We find no evidence of such pre-trends. Second, it is well possible that broadband was first deployed in areas experiencing an economic expansion. We show however that controlling for a rich set of city-level lagged covariates on local industry and trade dynamics does barely affect our estimates. We additionally include commuting zone × year fixed-effect, thus flexibly controlling for any changes in local labor market conditions and find that our estimates barely change. Overall the robustness of our results is consistent with the notion that broadband expansion in France was carried out by the historical operator (France Telecom and then Orange) with limited means – which explains that the coverage took several years – and with the objective, determined by national political actors, to maximize population coverage with little consideration for local economic dynamics or potential. We provide more information on the institutional context supporting this interpretation as well as a statistical analysis of the determinants of the timing of broadband expansion in which we show that local economic activity variables have essentially no predictive power and that only initial population density matter significantly.

This paper contributes to the literature in two main ways. First, we are, to the best of our knowledge, the first to investigate the effect of broadband on the importing behavior of firms. This is important as the roll-out of the broadband period (roughly 1999-2007) coincides with a surge in the imports of intermediary inputs. We therefore contribute to assess how innovation in information technology contributed the deepening of globalization. Second, our findings suggest the presence of technology-induced trade and therefore speaks to the literature aiming to assess the relative impact of trade and technology on changes in labor market outcomes among developed economies. While recent work Autor *et al.* (2015) has attempted to distinguish the effect of trade versus that of technology, our results suggest the fact that the magnitude of the rise in import penetration over the 2000s and the associated estimated labor market impacts (Autor *et al.*, 2013; Dauth *et al.*, 2014; Malgouyres, 2017) were to a non-negligible extent driven by concomitant technical change.

The paper is structured as follows. We review the relevant literature in Section 2. We then present the data and in the institutional context in Section 3. We present our empirical approach in Section 4 and provide elements supporting the identifying assumption. We then present baseline results and robustness checks in 5. In Section **??** we compute different counterfactual scenarios in order to gauge the magnitude implied by our estimates. Finally, we conclude.

2 Relation to previous literature

This paper falls within several strands of the literature in international economics and the economic impact of broadband internet. First, it enriches the growing literature about trade-induced technical change and the impact of Broadband Internet expansion. Second, it has implications for the origins of trade shocks whose effects have been extensively studied in recent year. Finally, it contributes to the literature on firm international sourcing choices.

Several works have documents the impact of trade on innovation. Aghion *et al.* (2018) find that expanding exporting opportunities increase the innovative activities of the most productive firms. On the import side, Bloom *et al.* (2016) find that greater Chinese import competition from China is associated with higher firm patenting in a panel of European firms while Autor *et al.* (2016a) find opposite results in the US case. We present empirical evidence on the opposite causal pathway from technology to expanding trade. While technology is often mentioned as an amplifying force of globalization, recent evidence on the matter appears scarce. For instance, in his rich account of the recent wave of globalization and its interplay with technology, Muendler (2017) mentions the possibility of technology induced trade but does not cite empirical papers estimating the impact of ICTs on rising trade flows.[1] A possible reason as to why quasi-experimental evidence on technology-induced trade is rather scarce might be related to the difficulty of measuring technology adoption at the firm-level that varies overtime while simultaneously observing trade patterns. Consequently, most studies documenting a positive impact of ICT on trade are either at the country (Portugal-Perez and Wilson, 2012; Clarke and Wallsten, 2006) or macro-regional level (Barbero and Rodriguez-Crespo, 2018). There are two recent exceptions : Kneller and Timmis (2016) and Akerman *et al.* (2018). Most closely to our paper is Akerman *et al.* (2018) who estimate how broadband local availability affects the effect of distance on trade flows using a panel of bilateral trade flows between Norvegian cities and foreign countries, building on the identification strategy of Akerman *et al.* (2015). While their results display a significant interaction effect between fast internet and distance, they do not find that broadband increases significantly trade flows – although their coefficients are positive, they are imprecisely estimated. We depart from this paper in several ways. First, we do not consider both exports and imports symmetrically but focus on the import side and treat it specifically. Second, our sample size is considerably larger thus allowing us to uncover effects that might be otherwise impossible to detect. Moreover, we analyse several sets of trade outcomes (value of flows, volume, number of origins, number of products) while their work focuses on volumes. We therefore document additional margins of adjustment of trade to broadband expansion. We further adopt a dynamic event-study specification – as opposed to a static panel specification – allowing us to provide evidence on the dynamics effet of broadband expansion. Finally, while Akerman *et al.* (2018) frame their empirical analysis in the gravity literature, aiming at explaining the distance puzzle, we adopt a more agnostic approach and aim primarily at documenting the extent to which technology-induced trade might have contributed to the recent rise in imports penetration across advanced economies, rise whose labor market effect have been increasingly documented.

Indeed, starting with Autor *et al.* (2013), a growing body of evidence has adopted a local labor market approach[2] has documented the impact of trade shocks from emerging countries – in particular China and Eastern European countries, see Dauth *et al.* (2014) – on the local labor

1. Steinwender (2018) provides empirical evidence on the role of ICT on trade during the 1800s by studying the impact of the telegraph on trade between the United Kingdom and the USA.

2. This approach was pioneered by Topalova, 2010 and Kovak, 2013 who analyse the local impact of trade liberalization in India and Brazil respectively.

market outcomes (manufacturing employment, wages or employment rate etc.). Accumulating evidence regarding the adverse local labor market effects of trade shocks has contributed to change economists' conventional wisdom regarding the relative importance of trade and technology in shaping labor market outcomes more generally in advanced economies – see Crozet *et al.* for a recent review with a focus on the French labor market.[3] While trade was thought to have had a fairly small impact on the low-skill and manufacturing employment by the end of the 1990s (Borjas *et al.*, 1997), several articles show that the share played by trade seems to have been larger than initially anticipated over the 2000s, in particular due to the rise of Chinese import competition (see Autor *et al.*, 2016b, for a review). Most studies analyze the effect of trade or technology in isolation, treating the factor left-out as a potential omitted variable to account for. By contrast, Autor *et al.* (2015) attempt to "untangle" the respective effect of trade and technology on several local labor market outcomes, notably the decline in manufacturing employment in the USA. Their analysis however consider sectoral aggregate trade flows as given. We instead make the point that the remarkable increase in trade flows over the period is in part driven by concomitant technical change and we quantify this effect by exploiting the staggered expansion of broadband internet in France. Hence while the local labor market literature estimates the local impact of rising aggregated trade flows over the 2000s, we instead consider such trade flows as dependent variable and use local variation in the timing of technology deployment in order to assess the extent to which they were technology-driven. Our results point to a substantial effect of ICT on trade. Moreover, we also find larger effect for imports sourced from Eastern European countries and China, suggesting that the recent "trade shocks" from the 2000s were, in part, driven by technological change.

While our paper does not document direct effect on firm-level productivity, it is well documented that importing a wider array of foreign inputs lead to productivity gains, due in part to imperfect substitution between foreign and domestic inputs (Halpern *et al.*, 2015). Our findings therefore relate to the literature on the link between international sourcing, productivity and consumer welfare (Halpern *et al.*, 2015; Blaum *et al.*, 2018). As broadband internet is found to increase the share of spending on foreign inputs as well as the variety of inputs, trade appears to be one of the mechanism through which fast internet boosted productivity and ultimately consumer welfare.

3. Most of the papers use local labor market as unit of observations, however works with findings pointing to a large magnitude of the labor market effect of trade over the 2000s have been carried at the industry-level (Pierce and Schott, 2016) or at the worker-level (Autor *et al.*, 2014).

3 Data and context

3.1 Data

In this paper, we assemble unique data on the expension of Broadband Internet and trade dynamics in France. We mainly combine two sources of information : unique city-level data on Brandband Internet availability and firm-level trade and employment data. We provide a detailed description of our data in the following sections :

3.1.1 Broadband internet data

We manually collected a unique data on each Local Exchange (LE)'s date of upgrade to ADSL in Mainland France. We additionally obtained data from the regulatory agency (ARCEP) regarding the geographical coverage of each LE. The data documents the area of each census block (IRIS) that is covered by a given LE. Each city in France is partitioned into census-blocks. Combining both data, we construct a continuous measure of broadband access of city i at year t . This measure which we denote $\widetilde{Z}_{it}$ is a time-weighted percentage of area covered in city i. It is formally defined as :

$$\widetilde{Z}_{it} = \sum_{b \in i} \underbrace{\frac{\text{day access}_{bt} - \text{Jan 1st year } t}{\#\text{days in year } t}}_{=D_{bt}} \times \underbrace{\frac{\text{area}_{bt}}{\sum_{b \in i} \text{area}_{bt}}}_{A_{bt}} \tag{1}$$

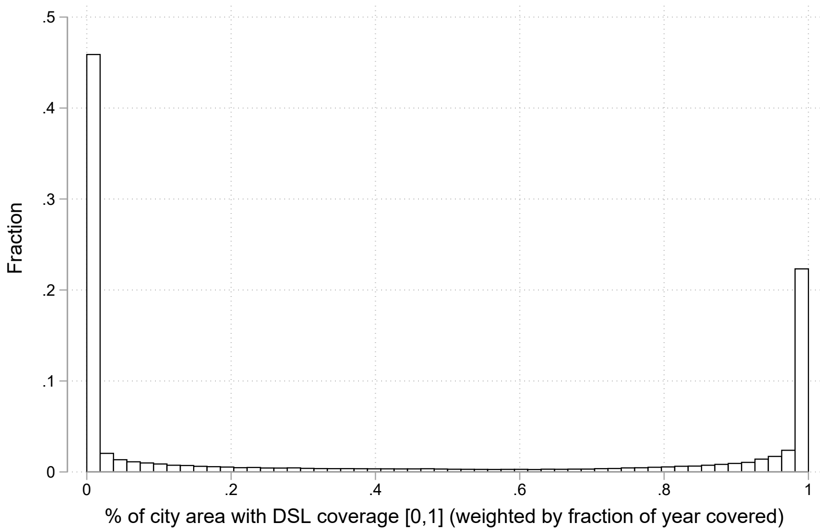
where $b \in i$ refers to census track included in city i.

Discretizing the variable. We see that $\widetilde{Z}_{it}$ is in principle continuous and belongs between 0 and 1. $\widetilde{Z}_{it}$ will be equal to one if all of its areas have had access for the entire year. It will be equal to 1/2 if the entire city has had access to broadband over half the year t

While the continuous measure is useful as it allows to gauge the state of broadband penetration, we do not use it directly in our empirical estimations. Indeed, regressing trade outcome on this measures would assume that the effect of an increase $\widetilde{Z}_{it}$ by, say 0.5, will be the same whether it stems from an coverage of the whole city over half the year or an increase in half the city area of the entire year.

As we do not have strong theoretical reason to think that would be the case but we still believe that the two sources of information are valuable, we discretize the treatment status by setting treatment status to 1 after the city experienced its highest increase in $\widetilde{Z}_{it}$. Formally, we define the year of treatment as $t_{i0} = \text{argmax}_t \Delta \widetilde{Z}_{it}$ and discretized treatment status as $Z_{it} = \mathbb{1}\{t \geq t_{i0}\}$.

Figure 1: Distribution of $\widetilde{Z}_{it}$: 1999-2007



As seen from Figure 1 in practice the empirical distribution of $\widetilde{Z}_{it}$ is heavily concentrated on 0 and 1 and accordingly Z_{it} and $\widetilde{Z}_{it}$ are strongly related ($\rho = 0.90$). An other way to assess the relationship between the continuous and discrete variables is to trace the evolution of $\widetilde{Z}_{it}$ around t_{i0}. Figure A1 plots the coefficients of a regression of $\widetilde{Z}_{it}$ on a set of dummies for each time with respect to t_{i0}. The results show a sharp increase between -1 and 0 from 0 to 0.4 and to 0.9 at +1. The coefficients are estimated with a very high degree of precision which reflects the fact that $\widetilde{Z}_{it}$ while continuous in theory has most of its support in 0 or 1.

To sum up, we choose to discretize the variable as it does not cause us to lose much information and it allows us to use the more transparent event-study approach that will be described in Section 4.

Geographical description of broadband expansion. Figure 2 shows the roll-out for all of France and 3 focus on a specific region.

Figure 2: The progressive roll-out of the DSL technology in France – $\widetilde{Z}$

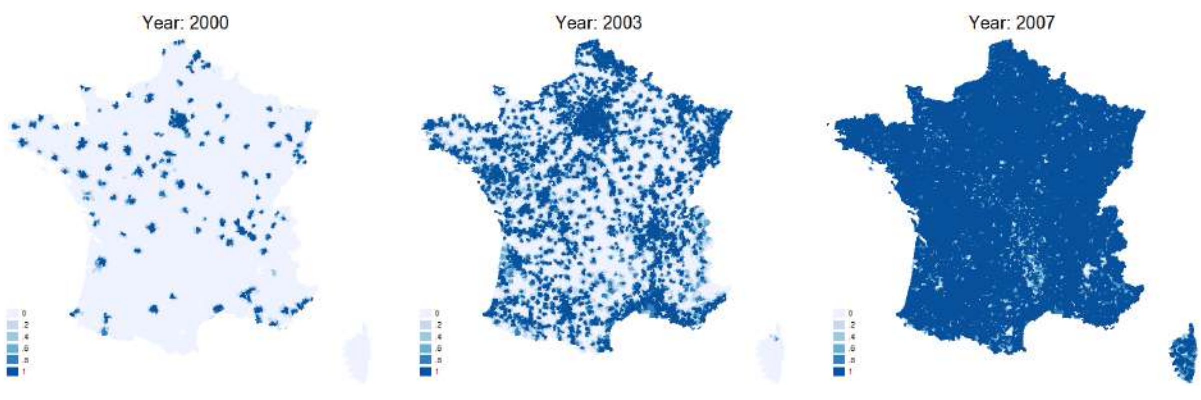
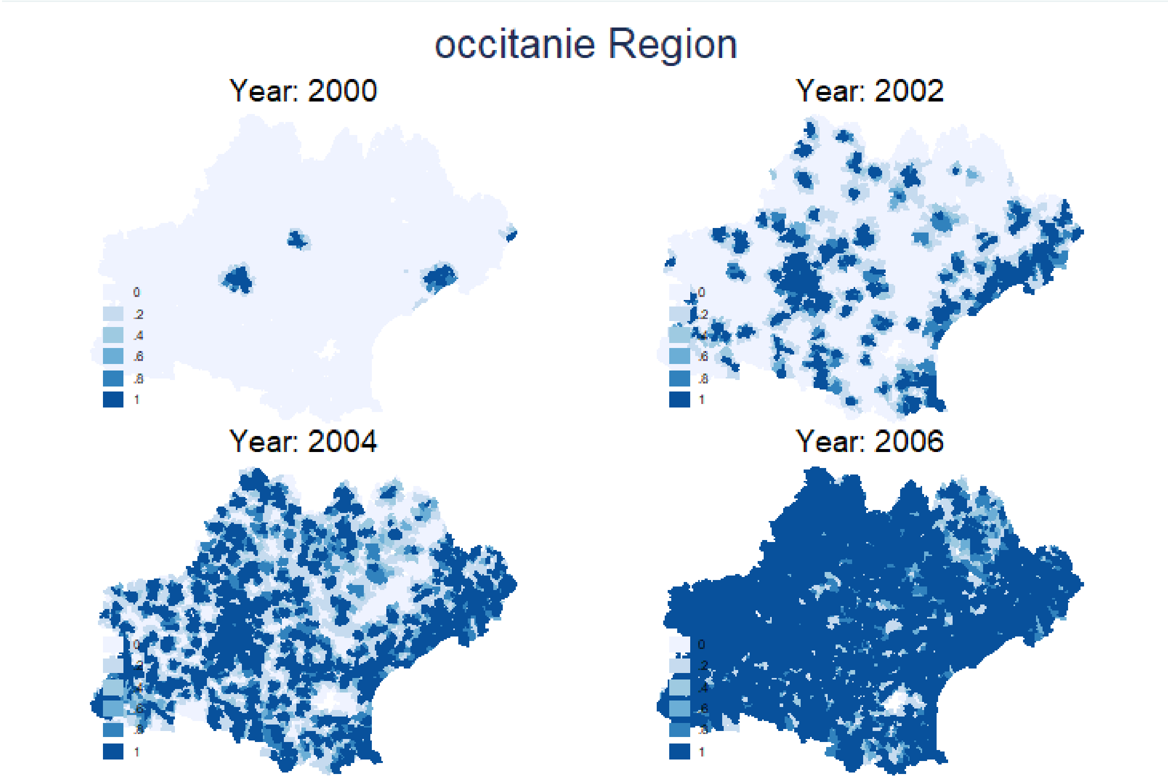


FIGURE 3: The progressive roll-out of the DSL technology in Occitanie – $\widetilde{Z}$



We now go on to describe the data used in order to describe firm-level trade as well as the data used to described the local economic environment.

3.1.2 Trade and employment data

(i) Firm-level trade data. The data on firm-level trade is produced by the customs office, and compiles the exported values and quantities for each firm-destination-product combination over the period considered (1997-2007).

(ii) Firm-level employment and location data. The administrative dataset DADS (Déclarations Annuelles de Données Sociales) comes from firms' social security records. For the period 1997-2007 and for every plant with at least one employee, it provides the number of workers, overall wage bill and the city of localisation. This is a plant-level dataset where a firm can have several plants.

(iii) City level covariates. Several covariates (population, share of college educated workers, labor force) come from 1999 census files aggregated at the city level and directly provided by the French statistical institute (INSEE) on its website. Additional covariates (number of fiscal households and overall fiscal income) come from fiscal files aggregated at the city level (Fichier communal de l'impôt sur le revenu).

3.1.3 Construction of the estimating sample

We assemble those different data sources to construct a final city-level estimating sample. We start with the administrative dataset DADS to keep in our working sample only mono-city and mono-establishment firms.This is done to precisely identify the impact of Broandband Internet availability on trade outcomes (We want that our treatment and our outcomes to be at the same micro level). Indeed, while Broandband Internet expansion may occur at different moment for a same firm which owns several establishments in several cities, we only observe aggregated trade at the firm-level (the firm headquarter) and not the establishment-level. Keeping only mono-establishment or mono-city firms adequatly address this potential measurement issue. We compute the value of imports, the quantity of imports, the number of importing flows and the number of different products imported for mono-city firms aggregated at the city-level and for several geographic areas and products categories (as defined by the BEC). We then merge this trade database with Broadband internet data. Finally, we use firm-level employment and location data, data on population, share of college educated workers, labor force from the 1999 census files as well as number of fiscal households and overall fiscal income from fiscal files aggregated at the city level to construct a large fiscal and demographic control database that we merge with our trade working sample.

3.2 Context : The Broadband Internet Expansion if France

[TO BE COMPLETED]

During the 1990s, many OECD countries were planning the expansion of services related to information and communications technology. Innovation and adoption of new technologies was seen by public authorities as a key objective in order to retain competitiveness and achieve high standards of living in a global economy.

In France, we can find evidence of this concern in the debate within the French Parliament ("Sénat") regarding the expansion of broadband internet (cf. Question écrite n. 30844 de M. Rene Tregouet (Sénat, 01/02/2001)) and, more precisely, about the agreement between the French governement (The State Secretary to the Industry) and France Télécom, which was the monopolistic telecom supplier at the time and continues to be the main supplier today. France Télécom was mandated to ensure that a certain fraction of French households throughout the country had access to broadband internet at a reasonable and uniform price and until a certain date.

The deployment of the technology was very progressive. France Télécom started the experimentation of the ADSL technology in 1998. They first started it in limited geographic zones with its subsidiary "France Télécom Interactive" (FTI) specialized in internet access technology. The FTI became Wanadoo from the 24 of September 2001. A second phase was engaged in mid-1999 with the marketing of an unlimited broadband Internet access by France Télécom, which was named Netissimo 1 (with 500kbits/s) and Netissimo 2 (with 1 Mbit/s).
An important point discussed by the French Governement and France Télécom was the amount of investment in infrastructure that was needed to modernise the physical telecom network and reach objectives set by the State Secretary to the Industry. Indeed, all broadband infrastructure was owned and operated by France Télécom but not the entire network was eligible to broadband internet. By the end of 2000, only 11 million telephone lines were eligible to the ADSL and an estimation carried this number to 16 million by the end of 2001. This represented a third of the total number of telephone lines in 2000 and half in 2001. France Télécom's ADSL clients (through Netissimo directly or indirectly though the resale of an FAI) where 67,000 by the end of 2000, 408,000 by the end of 2001 and 1,400,000 by the end of 2002.
Under the impulse of the government, the historical operator France Telecom pledged in 2002 to cover 98% of the French (metropolitan) population by the end of 2006.

4 Empirical approach

As mentioned in the Section 3, our empirical setting does not fit the standard difference-in-differences setting because all units are ultimately treated, i.e. connected to broadband internet over the period we consider. We therefore exploit variation in the timing of treatment which was staggered overtime.

4.1 Baseline specification

In light of the issues associated with the static two way fixed-effects in the staggered treatment setting (Goodman-Bacon, 2018; Borusyak and Jaravel, 2017), we estimate a dynamic specification where we allow treatment to vary with time-from-treatment. We drop two indicator variables for d_0 and 1 year prior to broadband expansion, thus. assuming that the trend between these two years are flat. That restriction is necessary to avoid multi-collinearity and to identify the fully-dynamic underlying data generating process in the staggered design (Gross *et al.*, 2018; Borusyak and Jaravel, 2017). To ensure that this restriction is not driving our results, we display results with alternative normalisations in the robustness section. The main estimating equation is as follows :

$$Y_{it} = \sum_{\substack{d=-d_0 \\ d\neq -1}}^{d_1} \beta_d \times \mathbb{1}\{d + t_{0i} = t\} + \mathbf{x}'_{it}\delta + \alpha_i + \psi_{d(i),t} + \varepsilon_{it} \tag{2}$$

The specification presented in equation 2 includes leads and lags. The inclusion of leads allows us to assess the presence of pre-trends. However, in order to maximize efficiency, we also include a semi-dynamic specification where only the lags of the treatment are included, as presented in equation (3) :

$$Y_{it} = \sum_{d=0}^{d_1} \beta_d \times \mathbb{1}\{d + t_{0i} = t\} + \mathbf{x}'_{it}\delta + \alpha_i + \psi_{d(i),t} + \varepsilon_{it} \tag{3}$$

This empirical model enables us to assess the dynamics of the treatment effect in the short and medium run (up to five periods after the expansion).

Interpretation : The event-study coefficients $\widehat{\beta_d}$ in equation 2 can be interpreted causally under the identifying assumption that, conditional on receiving broadband over the period considered and conditional on city fixed-effects, the timing of broadband roll-out is unrelated to the outcome. The event study specification presented in equation (2) allows us to examine patterns in outcomes in the years leading up to the broadband expansion.

The identifying assumption that there is no systematic local factor driving both broadband and the trade outcome variables is extensively investigated by including flexible controls for shocks at the city level and assess the sensitivity of the coefficients.

Identification in the presence of cohort, time and time to ADSL effects : Note that we face collinearity between time since broadband expansion, calendar time, and indicator for broadband expansion cohort. As a broadband cohort proceeds through time, both time since broadband expansion and calendar time increase at the same rate. This is akind to the standard cohort age-time-problem in event-study research designs. Our empirical model can identifiy the full path of coefficients up to a linear transformation (shift in intercept and rescaling). We therefore need to normalize to 0 two coefficients instead of one as in classical DiD setting. We then check whether we see deviation away from the flat pre-trends between the two normalized coefficients and we further also assess the sensitivity of our results to the choice of the coefficients to be normalized. Moreover, note that the semi-dynamic specifications are not subject (by definition) to the under-idenfication problem.

4.2 Validation of the research design

4.2.1 Explaining broadband expansion

Our identification strategy hinges on the assumption that the coverage of cities is mostly determined by city population density – which is mostly fixed over time – and did not take into account underlying trends in trading / importing activities. As a result, conditional on city and year fixed-effects, we consider the variation in broadband access as good as random. In order to assess the validity of this assumption, we explore the extent to which broadband coverage over time is predicted by different types of lagged city-level covariates. We group explanatory variables into several groups :

1. **Density** : population in 1999 per square km (log), interacted with a full of year dummy variables.
2. **Industry dynamics** : shares of employment in 10 economic sector at $t-1$ as well as changes in shares between $t-1$ and $t-2$.
3. **Trade** : asinh(number of transactions), asinh(value of imports) in $t-1$, changes in these two variables between $t-1$ and $t-2$.

We estimate following specification :

$$\tilde{Z}_{it} = \mathbf{x}'_{it}\delta + \alpha_i + \psi_{d(i),t} + \varepsilon_{it} \tag{4}$$

where $\tilde{Z}_{it}$ is the time-weighed share of city i that is covered by broadband internet as described in Equation 1. As we are mostly interested in the explanatory power of thee different groups of observable variables we only report the R-square of different set of regressions. Individual coefficients are presented in the appendix.

We start by regressing internet coverage on all three sets of observable covariates with out including any time or city fixed-effect. As indicated in Column (1), we obtain a R-square of 56% indicating that these variables capture a substantial share of the variation in treatment status. Column (2) presents the R-square of two-way fixed-effect model including city and province $\times$ year fixed effects. This model absorbs 82% of the variance in treatment intensity. Column (3) presents the same model including 1999 measure of density interacted with year dummies, the fit of the model increase by 2.6 pp. Interestingly, the set of industry dynamic or trade variables barely increase the fit of the model, indicating that conditional on city and province-year fixed effect, they are roughly unrelated to the timing of internet coverage.

TABLE 1: Explaining city broadband coverage : panel analysis

	(1) Covariates	(2) Twoway FE	(3) (2)+density	(4) (2)+indus.	(5) (2)+trade.	(6) (2)+ all cova.
R^2	0.563	0.786	0.812	0.786	0.786	0.812

Comment on variation in broadband access : While regression results in Table A4 show statistically significant impacts of some city characteristics on the timing of broadband roll-out, in particular density interacted with year dummies, most of the variation remains unexplained. Moreover, trade and industry lagged level and dynamics are almost all insignificant once fixed-effects are introduced (column 6 of A4). Finally, it is remarkable that once one conditions on city-fixed effects, the full-set of explanatory variables beyond density contributes essentially nothing to the R-square (see Table 1).

We consider the low predictive power of observable variables as supporting our identification strategy, as a large share of the variation in timing of the broadband expansion seems to be idiosyncratic in nature.

Our baseline specification will allow for differential (linear and quadratic) trends in outcomes based on initial density. We will further assess the sensitivity of coefficients to the inclusion of observables and show that results are little affected by their inclusion. Naturally, this test does not imply that unobservables are not biasing our estimated coefficients. However for this to be the case, these unobservables should be time-varying, correlated with the timing of broadband expansion and yet uncorrelated to the rich set of observable variables included whose inclusion we show does not affect our estimated coefficients. While this remains a possibility we cannot completely rule out, given the battery of robustness tests we provide, we view it as unlikely.

5 Baseline Results

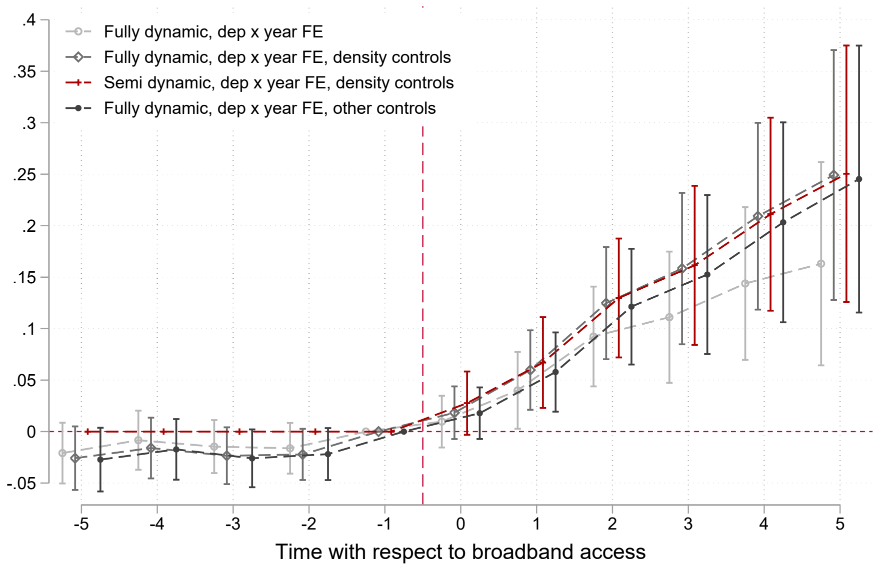
The main variable we consider is the (log of) value of imports by a given city. We show the results for different specifications and assess the robustness of the results. We then turn to different margin of trade (extensive and sub-extensive margins).

5.1 Value of imports

Figure 4 displays the main results of our paper. It represents estimated coefficients from Equation (2). The associated table is presented in Appendix Table A3.

The light grey coefficients are from a specification with no controls except for year-département and city fixed-effects. Wee see a flat trend prior to -1 and a break in the trend after broadband expansion. The coefficient for $d = 5$ is equal to 0.162 suggesting the the broadband expansion increased the overall value of city-level import by about 16 % 5 years after the period of largest expansion (Colum 2 of Table A3).

FIGURE 4: Main specification : Log of the value of imports



Notes : This figure plots estimates for specification in equation (2 – fully dynamic) and (3 – semi dynamic). Controls include: (i) 1999 share of college educated workers interacted with year-dummies, (ii) 1999 population density at the city level defined as #. of inhabitant divided by city area interacted with quadratic and linear trends. 95 % confidence interval are presented. Standard errors clustered at the province (département) level. The sample include all cities with a positive trade flow (import).

Given the relevance of density in the decision making process leading to broadband expansion, it seems natural to control for it. Our baseline specification included controls for density (linear and quadratic trends). The dark grey coefficients show the results. Here again, we see no deviation from the underlying linear trend prior to broadband expansion and a regular growth afterward. The estimate effects is considerably larger with a coefficient of 0.25 after five years. The dark coefficients displayed results once controls for dynamics in sectoral composition and population are included. Results are remarkably stable to this inclusion and are virtually identical to the density-controls only specification. The estimated lead coefficients appear equally supportive of the absence of pre-trends. Finally, the last set of coefficients in red represent the semi-dynamic results when controls for density are included. They present the advantage to be in theory more efficiently estimated – as the number of paramters to be estimated is lower –, however the standard errors turnout to be very close to the fully dynamic specification.

Comments on the temporal pattern of effect : We see that the estimated effects (independently of the chosen specification) are growing with time since ADSL expansion in a somewhat linear fashion. The effects grow in magnitude as time passes which suggests that they are structural and might affect the path of the local economy and not simply its level. Given our reduced-form approach, this pattern might reflect the combination of two effects, the time lag between local availability of broadband and its adoption by firms as well an internal lag between firm-level adoption and changes in importing behavior. In the absence of data on local adoption by firms, it is not possible to disentangle these two sets of effets and our estimated coefficients reflect the products of both which might explain this pattern of linearly growing effects. Note that the time period included in our analysis finished in 2007. This choice is dictated by the advent of the great recession which has a major negative impact on trade flows (Baldwin, 2009) and would add noise to the estimation. [4] Unfortunately it limites our ability to investigate the longer run effect of broadband expansion and whether it levels-off after a given period of exposure.

Magnitude computation :

In this section, we try to get a sense of the quantitative implications of our findings by constructing counterfactual series for trends in aggregate imports if broadband expansion had not occurred. A counterfactual outcome is measured as the actual outcome minus the predicted effect of broadband availability on the outcome, taking into account to the dynamics of the effects as captured by our semi-dynamic specification.

More specifically, we define the year specific average effect as $\bar{b}_t = \sum_{t_0=1999}^{2007} w_{t_0}^y \widehat{\beta}_{t-t_0}$ where where $w_{t_0}^y$ represents the national trade share for outcome y in year t_0 of firms located in cities where broadband became available at year t_0. We consider the national time series for a given trade flow y_t. We postulate that the observed trade flow is given by a baseline level $y_t(0)$ that would have occurred in the absence of broadband diffusion times the predicted effect : $y_t(1) = \exp(\bar{b}_t) y_t(0)$. We obtain the counterfactual series by inverting this relationship : $y_t(0) = \exp(-\bar{b}_t) y_t(1)$.

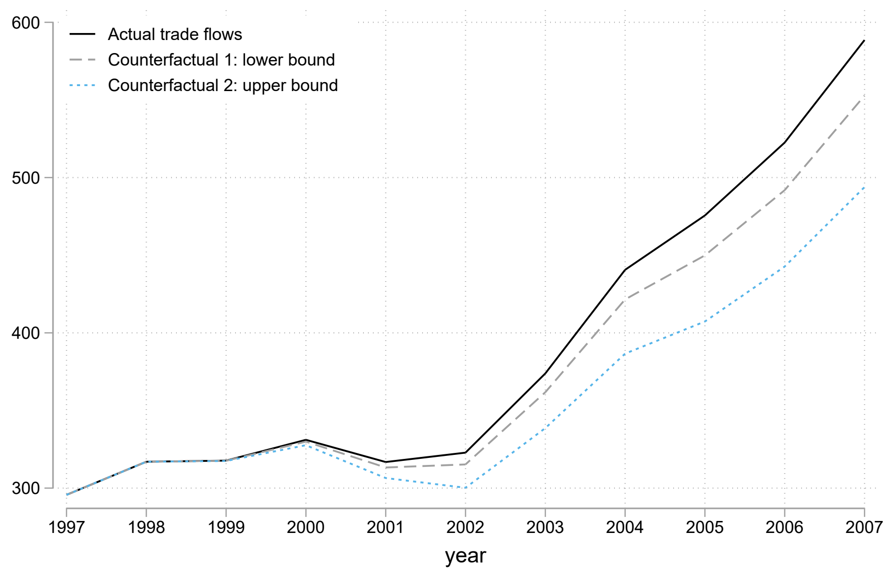
We present two sets of counterfactual time-series in imports which corresponds to different sets of weights. The first one is obtained by considering the share of the estimating sample, which contains single-city firm only, in overall imports. The weights therefore do not sum to 1 for any given year but instead sum to the share of single-city firms in national imports.

The second one is normalizing the previous shares so that they sum to 1. Applying the first set of weights implicitly assumes that multi-city firms were not affect by broadband expansion in their importing behavior, while applying the second set is equivalent to assuming that they reacted in

4. It is standard for studies of empirical studies of international trade to exclude the great recession from their time window.

the same way as single-city firms. Therefore we see the first counterfactual as a lower while the second is more likely to be an upper bound. Indeed, to the extent that multi-city firms are larger and might be able to invest in technology and commercial networks that decrease their reliance on broadband technology to engage in international trade, the impact of broadband internet on their importing behavior is probably lower than the same effect for smaller, single-city firms.

FIGURE 5: Counterfactual aggregate trends in overall import



Notes : Description counterfactual.

5.2 Further Robustness checks

[TO BE COMPLETED]

Local labor market dynamics. We saw above that including results on sectoral employment dynamics leave the results virtually unaffected. It could be however that our proxies do not fully capture local labor market shocks. In order the gauge the sensitivity of our results to the type of controls included, we control non-parametrically for any development at the local labor market level by including commuting-zone × year fixed effects. Commuting zone are defined based on criterion of self-contained commuting and are the usual unit to study local labor market developments. The estimated effects are displayed in Column 5 of Table A3 and are somewhat attenuated but remain significant and exhibit the same trend.

Restricting the sample period in order to obtain a proper control group. We check that using the continuous measure in a simple panel-fixed effect model over the period 1997-

2004 leads to qualitatively comparable results. The restriction of this subsample period allows us to consider cities that were never treated and therefore implement a classical difference-in-differences estimation.

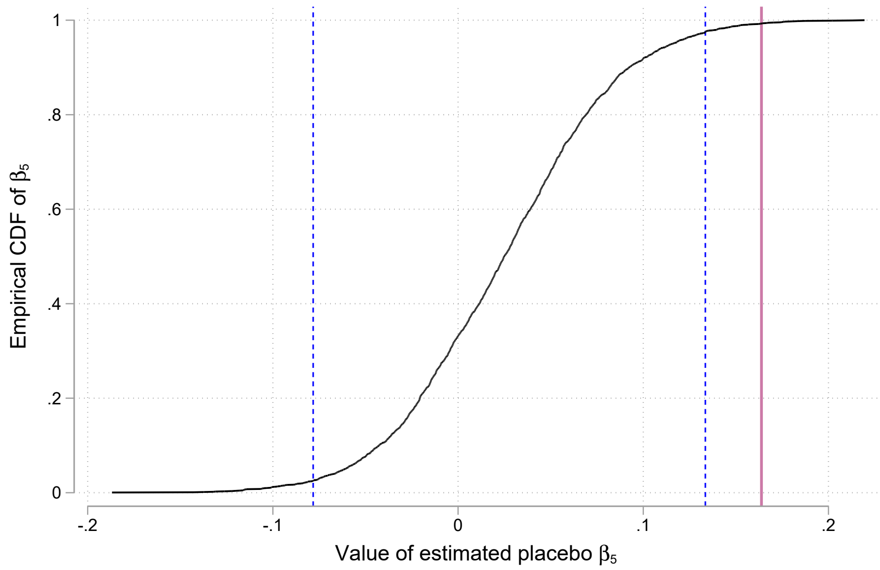
Accounting for zero-flows. Our results so far do not include observations where ln(imports) is not defined because imports is null. As such there might be an intensive margin at the city-level that we are therefore missing. A simple way to accomodate observations where city-level imports are null is to resort to the asinh(import) on a balanced sample of cities (**?**). Ashin() is defined as : $asinh(z) = \ln(z + \sqrt{1 + z^2})$. [5] The results are displayed in Figure XX and in Table A2 of the Appendix. Coefficients are larger (0.60 instead of 0.25 for the baseline specification), suggesting a role for extensive margin at the city level. They are however harder to interpret.

Placebo inference. Bertrand *et al.* (2004) show that serial correlations can bias inference in difference-in-differences studies leading to serious over-rejection of the null hypothesis. We take this issue into account in our main analysis by constructing standard error clustered at the province-level. These standard errors should lead to unbiased inference even in the presence of serial correlation within cities as well as cross-sectional dependence in the error term across cities within province. The inference in the baseline results relies on robust standard errors clustered at the province level. Therefore the standard errors are robust to serial correlation in the error term at the city level as well as autocorrelation across cities within a province – although part of this autocorrelation is absorbed by the province - year fixed effects.

In order to validate the inference provided by our clustered standard errors (which are only valid asymptotically) we implement Chetty *et al.* (2009)'s non-parametric permutation test of $\beta_d = 0$ for $d = 1, 2, 3, 4, 5$. To do so, we randomly reallocate the date of broadband expansion across cities and proceed to estimate equation (2). We repeat this process 2000 times and build an empirical CDF for $\widehat{\beta}_d$ which we denote $\widehat{F}()$. If broadband expansion had a truly significant positive effect on the dependent variable, say log of value of imports, one would expect the estimated coefficient to be in the very upper tail of the estimated empirical CDF based on permutations.

5. It is such that for $z \geq 2$, $asinh(z) \approx ln(z) + 2$ but with $asinh(0) = 0$

FIGURE 6: Distribution of Placebo Estimates : Log Imports, β_5



Notes : This figure plots the empirical cumulative distribution function of placebo estimated effects broadband on log imports, where date of broadband expansion is randomly reallocated across cities. Draws are with replacement and may include the correct date of treatment. The CDF is constructed from 2000 estimates of β_5 using the specification in equation (2) without observable controls. The solid line (in red) corresponds to the actual estimate of the matching specification. It lies outside of the 95% confidence interval that is delineated by the dash lines (in blue).

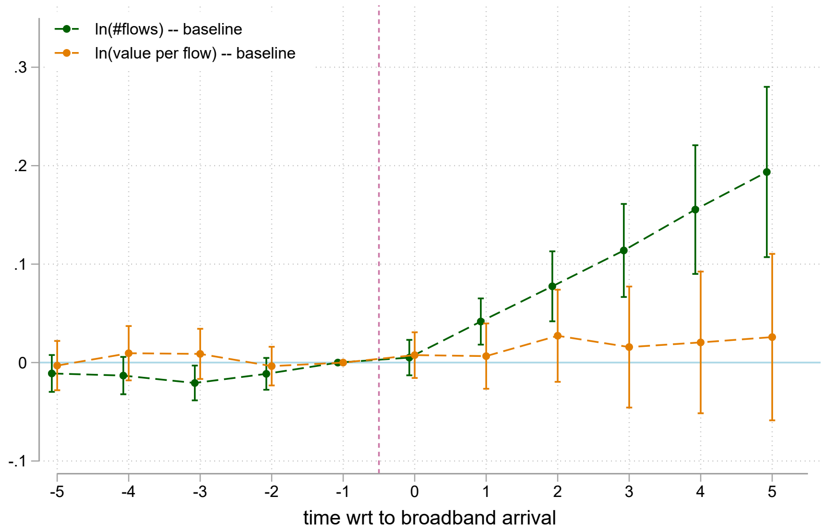
Denoting $\widehat{\beta}_5^M$ the point estimate obtained in Figure 4 based on the log value of imports, we get $1 - \widehat{F}(\widehat{\beta}_5^M) = 0.005$. Results are presented in Figure 6. Although this p-value is larger than the one using the t-statistics based on asymptotically-valid clustered standard errors (0.001), they confirm that the broadband internet led to an abnormally large increase in the value of imports. The same holds for β_4 (see Figure A2 in the appendix).

5.3 Intensive, extensive and sub-extensive margins

The previous sub-section established a strong effect of broadband internet on the value of imports at the city level. This pattern could be consistent with an increase in the number of importing flows, defined as a firm-origin country-product tuple, or an increase in the average value per flow or some combination of both.

Figure 7 show the results on both outcome for the baseline specification. We see that the value per flow is virtually inaffected and that the effect on value is entirely driven by the increase in the number of flows.

Figure 7: Number of flows and average value per flow

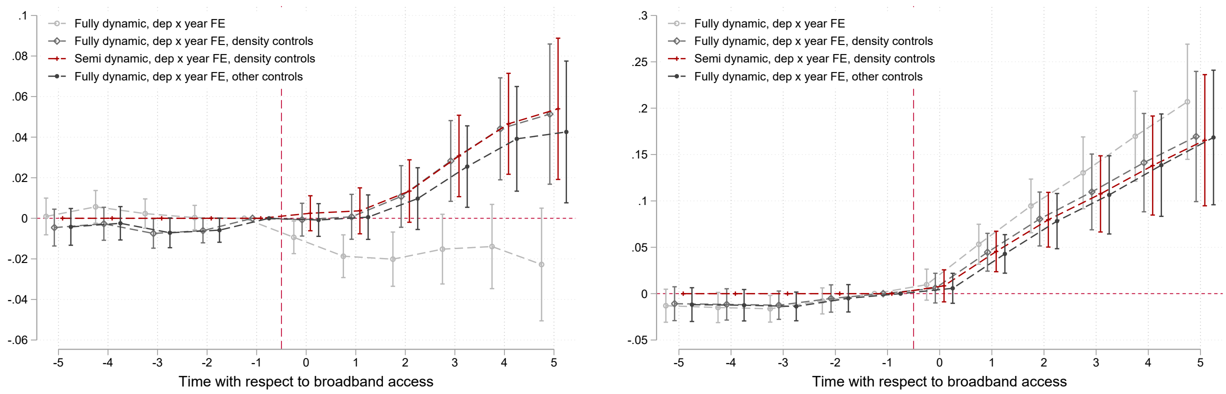


The increase in the number of flows could in turn reflect either an increase in the number of importing firms or in the number of importing flows-per-firm. We show that the first impact (extensive margin at the firm-level) is very small in magnitude (abour 6 %) albeit significant in the baseline specification (see Figure 8). This implies that the second effect dominates : the "sub-extensive margin" (Gopinath and Neiman, 2014) explains most of the causal impact of broadband internet on the value of imports.

HERE PUT EXTENSIVE EFFECT FOR BASELINE SPECIFICATION + SUB-EXTENSIVE IN THE SAME GRAPH

Figure 8: Extensive and sub-extensive margins

(a) Extensive margin (at the firm level) (b) Sub-extensive margins : flows per importer



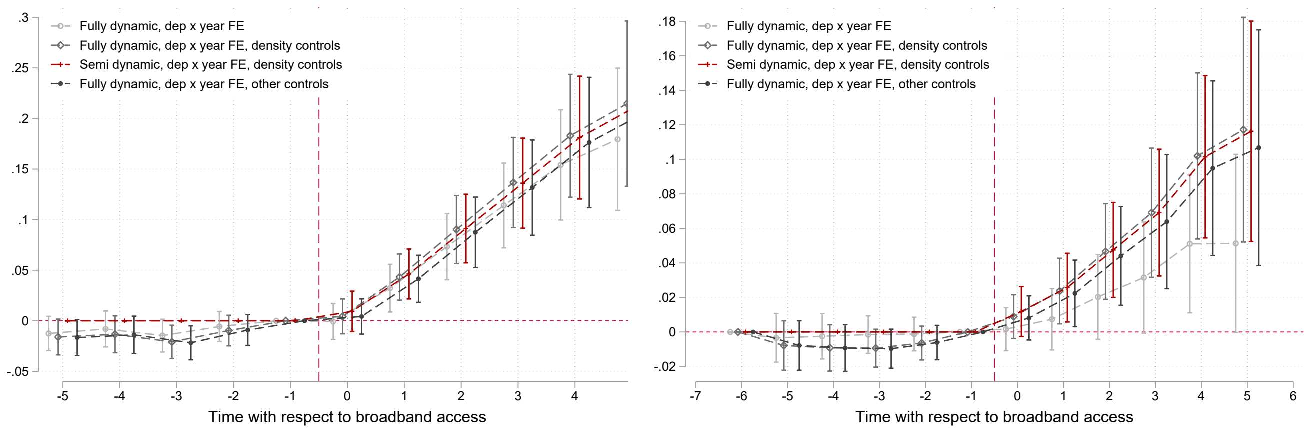
Notes : This figure plots estimates for specification in equation (2 – fully dynamic) and (3 – semi dynamic). Controls include: (i) 1999 share of college educated workers interacted with year-dummies, (ii) 1999 population density at the city level defined as #. of inhabitant divided by city area interacted with quadratic and linear trends. 95 % confidence interval are presented. Standard errors clustered at the province (département) level. The sample include all cities with a positive trade flow (import).

Our findings therefore support the notion that broadband internert caused an increase in import value, mostly by causing firms that were already importing starting to import more goods and from a wider array of origin countries while keeping the amount per flow roughly unaffected. The increase in the amount spent on spent on imports is roughly equal to the increase in the number of varieties imported – if one defines a variety as the unique combination of a product and a sourcing country as generally done in the literature (REF).

The increase in the total number of flows reflect a rise in the number of products or an increase in the number of origin countries. Figure 9 shows that both margins appear relevant.

Figure 9: Number of products (nc6) and sourcing countries

(a) Number of products (nc6) (b) Number of sourcing-countries



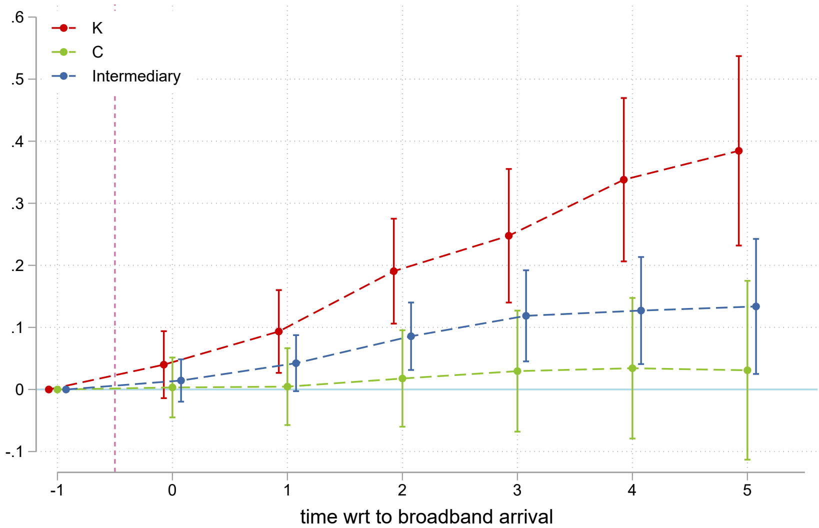
Notes : This figure plots estimates for specification in equation (2 – fully dynamic) and (3 – semi dynamic). Controls include: (i) 1999 share of college educated workers interacted with year-dummies, (ii) 1999 population density at the city level defined as #. of inhabitant divided by city area interacted with quadratic and linear trends. 95 % confidence interval are presented. Standard errors clustered at the province (département) level. The sample include all cities with a positive trade flow (import).

6 Heterogeneity

6.1 Origin-country

6.2 Type of goods : capital, intermediary and consumption

FIGURE 10: Value of imports by type of goods



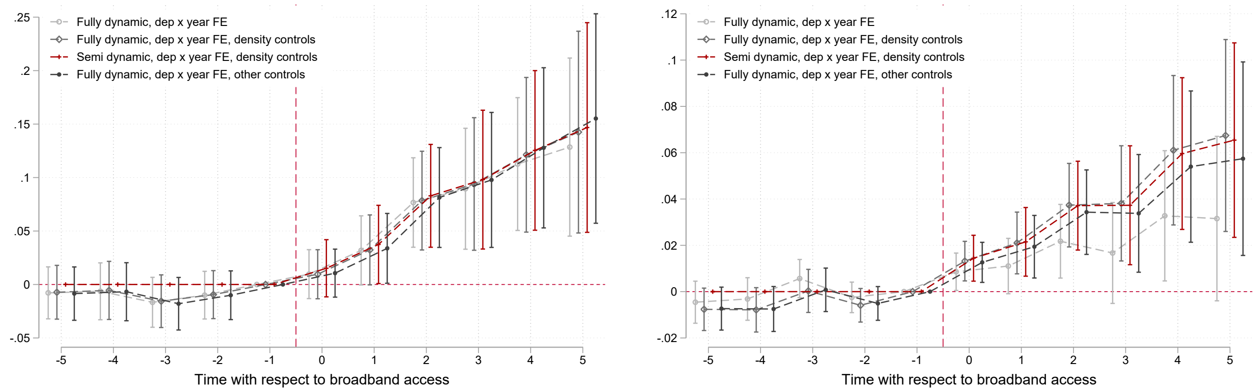
Notes : This figure plots estimates ($t \geq 0$) for specification in equation (3 – semi dynamic) and for goods grouped. Controls include: (i) 1999 share of college educated workers interacted with year-dummies, (ii) 1999 population density at the city level defined as #. of inhabitant divided by city area interacted with quadratic and linear trends. 95 % confidence interval are presented. Standard errors clustered at the province (département) level. The sample include all cities with a positive trade flow (import).

7 Impact on firm-performance

While trade and import in particular are important outcomes in and of themselves, it is interesting to see whether increasing the participation of firms in the import market is associated with expansion of business as captured in the sales or value-added. The results, displayed in Figure 11, show a 15 % increase in sales and a 6 % in value-added. We see that results for value-added are sensitive to the inclusion of density controls.

Figure 11: Value-added

(a) Sales (b) Value-added

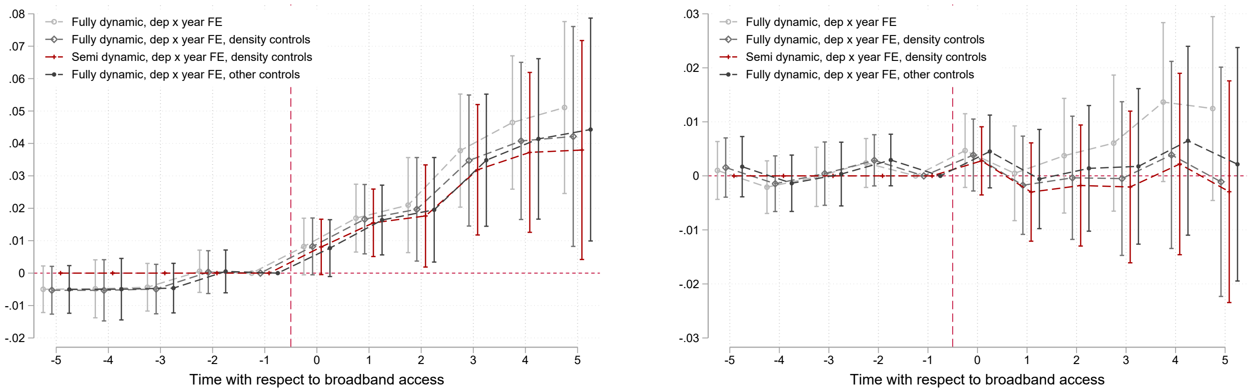


Notes : This figure plots estimates for specification in equation (2 – fully dynamic) and (3 – semi dynamic). Controls include: (i) 1999 share of college educated workers interacted with year-dummies, (ii) 1999 population density at the city level defined as #. of inhabitant divided by city area interacted with quadratic and linear trends. 95 % confidence interval are presented. Standard errors clustered at the province (département) level. The sample include all cities with a positive trade flow (import).

Beyond the scale of economic activity, access to a broader range of inputs have been documented to increase productivity. As most of the evidence focuses on the manufacturing sector (e.g. Halpern *et al.*, 2015; Blaum *et al.*, 2018) we split the analysis of productivity between the manufacturing sector and the non manufacturing sector. Results in Figure 12 shows a positive effect of broadband expansion on the productivity of the manufacturing sector, but essentially no impact outside of the manufacturing sector where value-added and employment grew in the same proportions.

FIGURE 12: Value-added per worker

(a) Manufacturing sector   (b) Non manufacturing sector



<u>Notes :</u> This figure plots estimates for specification in equation (2 – fully dynamic) and (3 – semi dynamic). Controls include: (i) 1999 share of college educated workers interacted with year-dummies, (ii) 1999 population density at the city level defined as #. of inhabitant divided by city area interacted with quadratic and linear trends. 95 % confidence interval are presented. Standard errors clustered at the province (département) level. The sample include all cities with a positive trade flow (import).

8 Conclusion

[TO BE COMPLETED]

We find that broadband expansion have increased importing activities by French small and mediums firms over the 2000s.

These findings have implications. The recent literature on the local labor markets impact of trade shocks shows that trade has strongly reshaped the structure of employment in advanced economies of the 2000s, and seems to have mattered more than technical change as far as manufacturing employment is concerned (Autor *et al.*, 2015). Nevertheless, we provide within-country quasi-experimental evidence that a specific instance of technological change, namely broadband internet, has largely determined the size of the trade shocks that this literature analyses. Baseline estimates suggest that the rise in the import penetration of Eastern European and Chinese products in France would have been 20% smaller absent the deployment of broadband internet over the 1997-2007 period. In the presence of technology-induced trade shocks, it is hard to disentangle trade from technology.

Furthermore, our findings suggest that the positive effect of broadband internet on productivity might be in part driven by the rise in the value and the variety of imported inputs.

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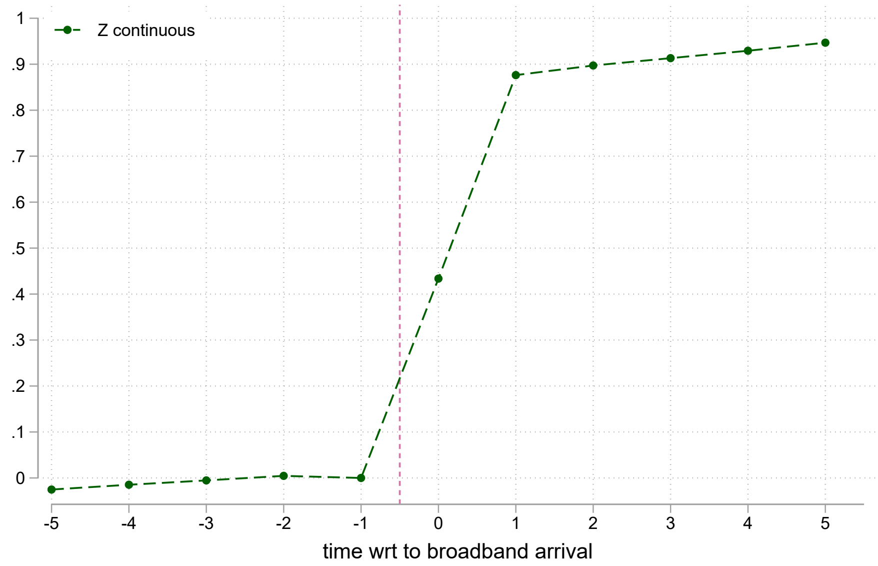
A Data appendix

B Additional tables and figures

B.1 Pseudo first-stage

FIGURE A1: The evolution of continuous measure of broadband coverage ($\widetilde{Z}_{it}$) around the (discrete) year of the largest increase in $\widetilde{Z}_{it}$ (t_{0i}).



Notes : This figure plots estimates from regression of $\widetilde{Z}_{it}$ on set of time to broadband expansion dummies with year fixed effect. The time at -1 is normalized to 0. The sample include all cities with a positive trade flow (import).

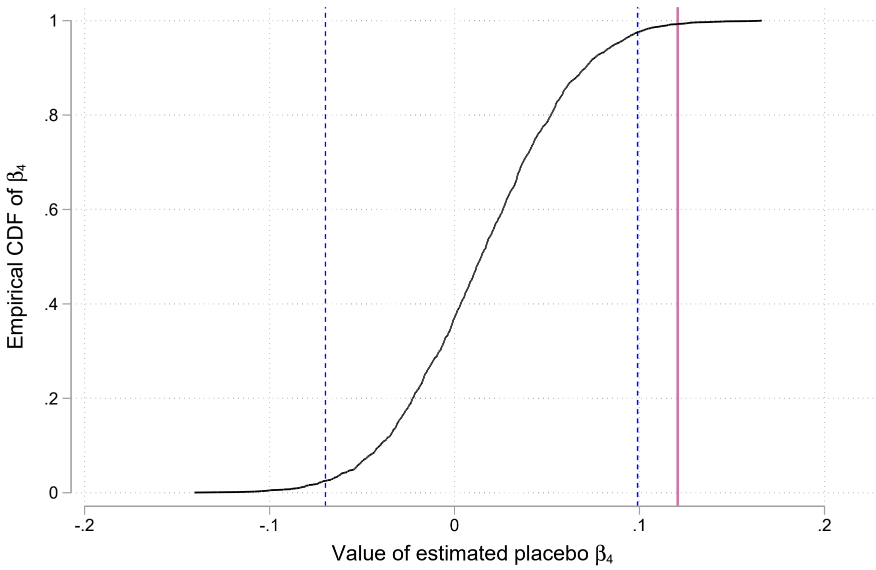
B.2 Robustness checks

[TO BE COMPLETED]

Sensitivity with respect to omission of a different period

Permutation-based inference

Figure A2: Distribution of Placebo Estimates : Log Imports, β_4



Notes : This figure plots the empirical cumulative distribution function of placebo estimated effects broadband on log imports, where date of broadband expansion is randomly reallocated across cities. Draws are with replacement and may include the correct date of treatment. The CDF is constructed from 2000 estimates of β_4 using the specification in equation (2) without observable controls. The solid line (in red) corresponds to the actual estimate of the matching specification. It lies outside of the 95% confidence interval that is delineated by the dash lines (in blue).

Exclusion the 3 largest urban areas in France (Paris, Lyon, Marseille)

Regression table : ln(value of imports)

Table A1: Specification Check : ln(value of imports)

	Ln (Values of Imports)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	No Dep.-Year FE	Dep.-Year FE	Baseline	(3)+Addit. controls	CZ-year FE	SD : no controls	SD baseline
-7	-0.019	-0.024	-0.027	-0.064	-0.025		
	(0.027)	(0.027)	(0.027)	(0.045)	(0.029)		
-6	-0.007	-0.013	-0.020	-0.043	-0.013		
	(0.025)	(0.026)	(0.027)	(0.044)	(0.026)		
-5	-0.013	-0.018	-0.027	-0.030	-0.018		
	(0.025)	(0.026)	(0.028)	(0.039)	(0.027)		
-4	0.004	0.001	-0.008	-0.005	-0.002		
	(0.017)	(0.018)	(0.020)	(0.030)	(0.019)		
-3	-0.001	-0.007	-0.015	-0.016	-0.009		
	(0.015)	(0.016)	(0.018)	(0.023)	(0.018)		
-2	-0.008	-0.012	-0.017	-0.020	-0.011		
	(0.013)	(0.013)	(0.014)	(0.014)	(0.014)		
0	0.000	0.004	0.012	0.012	0.011	0.005	0.015
	(0.012)	(0.014)	(0.014)	(0.016)	(0.014)	(0.016)	(0.016)
1	0.018	0.029	0.046**	0.044*	0.033	0.028	0.046**
	(0.019)	(0.021)	(0.022)	(0.026)	(0.021)	(0.023)	(0.023)
2	0.058**	0.075**	0.104***	0.100**	0.089***	0.072**	0.101***
	(0.027)	(0.030)	(0.034)	(0.040)	(0.031)	(0.028)	(0.030)
3	0.055	0.084**	0.127***	0.123**	0.093**	0.079**	0.120***
	(0.035)	(0.039)	(0.045)	(0.054)	(0.040)	(0.037)	(0.041)
4	0.066	0.111**	0.171***	0.169**	0.118***	0.104**	0.162***
	(0.040)	(0.044)	(0.054)	(0.066)	(0.045)	(0.041)	(0.049)
5	0.068	0.125**	0.205***	0.206**	0.130**	0.115**	0.193***
	(0.054)	(0.058)	(0.070)	(0.085)	(0.057)	(0.054)	(0.064)
Year FE	Yes	No	No	No	No	No	No
Year x Dep. FE	No	Yes	Yes	Yes	No	Yes	Yes
Year x CZ FE	No	No	No	No	Yes	No	No
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Density controls	No	No	Yes	Yes	No	No	Yes
Fiscal + Sector + Educ. controls	No	No	No	Yes	No	No	No
Spec.	D	D	D	D	D	SD	SD
Observations	109816	109816	109816	99589	109814	109816	109816

Regression table : asin(value of imports)

Table A2: Specification Check : asinh(value of imports)

	Asinh (Values of Imports)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	No Dep.-Year FE	Dep.-Year FE	Baseline	(3)+Addit. controls	CZ-year FE	SD : no controls	SD baseline
-7	0.167**	0.167**	0.154**	0.161	0.158**		
	(0.066)	(0.067)	(0.069)	(0.131)	(0.069)		
-6	0.161**	0.138**	0.113	0.127	0.131*		
	(0.063)	(0.066)	(0.070)	(0.113)	(0.067)		
-5	0.087	0.058	0.029	0.058	0.040		
	(0.059)	(0.063)	(0.067)	(0.103)	(0.065)		
-4	0.069	0.044	0.013	0.066	0.037		
	(0.051)	(0.054)	(0.057)	(0.084)	(0.056)		
-3	0.052	0.033	0.005	0.043	0.021		
	(0.038)	(0.042)	(0.046)	(0.062)	(0.043)		
-2	0.067**	0.055*	0.039	0.053	0.057		
	(0.031)	(0.033)	(0.035)	(0.040)	(0.034)		
0	0.051	0.067	0.090**	0.076	0.069	0.072*	0.104**
	(0.042)	(0.042)	(0.044)	(0.049)	(0.042)	(0.042)	(0.045)
1	0.054	0.077	0.127*	0.102	0.061	0.097*	0.153**
	(0.057)	(0.057)	(0.064)	(0.077)	(0.060)	(0.057)	(0.063)
2	0.086	0.132*	0.215**	0.176	0.130*	0.167**	0.253***
	(0.075)	(0.073)	(0.085)	(0.110)	(0.077)	(0.073)	(0.082)
3	0.100	0.176*	0.294**	0.242	0.187*	0.226**	0.344***
	(0.098)	(0.098)	(0.116)	(0.147)	(0.102)	(0.097)	(0.111)
4	0.135	0.247**	0.408***	0.337*	0.247*	0.316***	0.475***
	(0.122)	(0.124)	(0.149)	(0.186)	(0.126)	(0.120)	(0.139)
5	0.199	0.325**	0.534***	0.431*	0.327**	0.410***	0.616***
	(0.146)	(0.141)	(0.174)	(0.217)	(0.149)	(0.139)	(0.167)
Year FE	Yes	No	No	No	No	No	No
Year x Dep. FE	No	Yes	Yes	Yes	No	Yes	Yes
Year x ZE FE	No	No	No	No	Yes	No	No
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Density controls	No	No	Yes	Yes	No	No	Yes
Fiscal + Sector + Educ. controls	No	No	No	Yes	No	No	No
Spec.	D	D	D	D	D	SD	SD
Observations	174613	174613	174602	158317	174612	174613	174602

Table A3: Specification Check : ln(value of imports)

	Ln (Values of Imports)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	No Dep.-Year FE	Dep.-Year FE	Baseline	(3)+Addit. controls	CZ-year FE	SD : no controls	SD baseline	(7)+Addit. controls
-5	-0.019	-0.021	-0.016	0.000	-0.014			
	(0.015)	(0.015)	(0.016)	(0.021)	(0.018)			
-4	-0.006	-0.008	-0.006	0.009	-0.010			
	(0.014)	(0.015)	(0.016)	(0.021)	(0.018)			
-3	-0.008	-0.014	-0.015	-0.008	-0.017			
	(0.012)	(0.013)	(0.015)	(0.017)	(0.016)			
-2	-0.012	-0.016	-0.017	-0.015	-0.015			
	(0.012)	(0.013)	(0.013)	(0.013)	(0.014)			
0	0.005	0.009	0.014	0.011	0.019	0.016	0.021	0.019
	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)	(0.016)	(0.016)	(0.016)
1	0.030	0.039**	0.047**	0.042**	0.047**	0.046**	0.054**	0.051**
	(0.018)	(0.019)	(0.019)	(0.019)	(0.019)	(0.022)	(0.021)	(0.021)
2	0.075***	0.092***	0.099***	0.088***	0.107***	0.097***	0.105***	0.099***
	(0.024)	(0.025)	(0.027)	(0.028)	(0.028)	(0.027)	(0.028)	(0.028)
3	0.082***	0.110***	0.118***	0.102**	0.120***	0.114***	0.123***	0.115***
	(0.030)	(0.032)	(0.038)	(0.041)	(0.040)	(0.036)	(0.040)	(0.040)
4	0.099***	0.143***	0.156***	0.134***	0.153***	0.146***	0.162***	0.149***
	(0.036)	(0.038)	(0.048)	(0.050)	(0.050)	(0.041)	(0.049)	(0.049)
5	0.107**	0.162***	0.186***	0.157**	0.178***	0.165***	0.191***	0.174***
	(0.048)	(0.050)	(0.064)	(0.065)	(0.064)	(0.054)	(0.065)	(0.064)
Year FE	Yes	No	No	No	No	No	No	No
Year x Dep. FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Year x ZE FE	No	No	No	No	Yes	No	No	No
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Density controls	No	No	Yes	Yes	Yes	No	Yes	Yes
Fiscal + Sector + Educ. controls	No	No	No	Yes	No	No	No	Yes
Spec.	D	D	D	D	D	SD	SD	SD
Observations	103015	103015	103001	96620	102991	103015	103001	96620

Table A4: Explaining variation in internet coverage : full panel regressions

	(1) Covariates	(2) Twoway FE	(3) (2)+density	(4) (2)+indus.	(5) (2)+trade.	(6) (2)+ all cova.
L.(mean) share_primary	-0.0206			-0.0702		-0.0932
	(-1.27)			(-1.00)		(-1.28)
L.(mean) share_construction	-0.0158			-0.198		-0.102
	(-0.47)			(-1.66)		(-0.96)
L.(mean) share_auto	0.0301***			-0.0185		-0.0154
	(7.01)			(-0.98)		(-0.83)
L.(mean) share_transport	0.0370***			0.0131		-0.00123
	(7.74)			(0.74)		(-0.07)
L.(mean) share_hotel	0.0136			-0.0156		-0.0000880
	(1.78)			(-0.42)		(-0.00)
L.(mean) share_telecom	0.0279***			0.0267		0.0175
	(4.02)			(1.14)		(0.75)
L.(mean) share_service_pro	0.0278***			-0.0148		-0.0292
	(8.24)			(-0.86)		(-1.80)
L.(mean) share_service_pers	0.0372***			0.0256		0.0108
	(3.60)			(0.72)		(0.29)
LD.(mean) share_primary	-0.0238			0.0240		0.0175
	(-0.39)			(0.42)		(0.31)
LD.(mean) share_construction	-0.111			0.0852		0.0528
	(-1.51)			(1.27)		(0.85)
LD.(mean) share_auto	-0.00375			0.0104		0.00857
	(-0.25)			(0.72)		(0.60)
LD.(mean) share_transport	-0.0142			-0.00782		-0.00356
	(-0.82)			(-0.48)		(-0.22)
LD.(mean) share_hotel	-0.0507			-0.0183		-0.0219
	(-1.93)			(-0.72)		(-0.89)
LD.(mean) share_telecom	-0.00426			-0.0165		-0.00775
	(-0.18)			(-0.90)		(-0.43)
LD.(mean) share_service_pro	-0.0248			-0.00740		0.0108
	(-1.91)			(-0.62)		(0.90)
LD.(mean) share_service_pers	-0.0384			-0.0169		-0.0218
	(-1.40)			(-0.61)		(-0.80)
L.asinhn_flowsM	0.00728***				-0.00511*	-0.00184
	(8.04)				(-2.44)	(-0.92)
L.asinhvaleurM	0.000263				0.000680	-0.0000708
	(0.95)				(1.38)	(-0.15)
LD.asinhn_flowsM	-0.00278				0.00104	0.000209
	(-1.74)				(0.70)	(0.15)
LD.asinhvaleurM	0.0000509				0.0000843	0.000381
	(0.15)				(0.28)	(1.33)
year=1999 × Ln Density 1999	-0.0417***		-0.0357***			0
	(-45.67)		(-6.92)			(.)
year=2000 × Ln Density 1999	-0.0274***		0			0.0358***
	(-29.91)		(.)			(6.95)
year=2001 × Ln Density 1999	0.0117***		0.0785***			0.114***
	(12.74)		(15.51)			(26.32)
year=2002 × Ln Density 1999	0.0487***		0.132***			0.168***
	(53.15)		(17.32)			(34.48)
year=2003 × Ln Density 1999	0.0831***		0.140***			0.175***
	(91.00)		(13.20)			(26.05)
year=2004 × Ln Density 1999	0.123***		0.113***			0.149***
	(134.21)		(9.14)			(17.57)
year=2005 × Ln Density 1999	0.161***		0.0566***			0.0923***
	(175.68)		(4.98)			(12.38)
year=2006 × Ln Density 1999	0.185***		0.00315			0.0388***
	(200.79)		(0.37)			(8.27)
year=2007 × Ln Density 1999	0.193***		-0.0177*			0.0180***
	(209.08)		(-2.57)			(6.00)
R^2	0.563	0.786	0.812	0.786	0.786	0.812