In What Sense Was Samuelson Right? New Estimates on the Distributional Effects of Trade*

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March 17, 2023

Abstract

I venture a Chetverikov-Larsen-Palmer identification framework to evaluate the distributional impact of import competition in Europe. While I do not identify a significant effect for exchanges between EU countries, I find a strongly polarized response to imports from emerging markets. Even with reasonable estimates of gains from trade via other channels, import shocks from the South leave nearly every household better off but for the bottom 30%, who experience a marked decline in pre-tax income. I explore how this sharp non-linearity of the response interacts with social security schemes. Post-redistribution, the impact is flat across the income distribution, yet Samuelson's seminal insight holds if applied to the increased dependency on public benefits.

Keywords: International Trade, Import Competition, Trade Shocks, Distributional Impact of Trade

JEL Codes: F14, F16, F62

*I want to thank Lucas Chancel, Laurent Gobillon, Camille Hémet, Clément Malgouyres, Eric Monnet, Thomas Piketty and Paul-Armand Veillon for their comments or guidance through this research, and for having reviewed part of the findings. I acknowledge the financial support of the *Ecole normale supérieure*-PSL through the CDSN program and of the *Agence Nationale de la Recherche* through the *Investissements d'Avenir* program (ANR-17-EURE-0001). The findings and opinions of this paper are mine only, without any connection to the academic and policy institutions for which I work or have worked. Replication files are available on demand.

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1 Introduction

When the employment effects of growing trade competition from emerging economies first materialised, in the early 1990s, reactions were mixed but overall optimistic. At that time, the main theoretical framework to assess the impact of trade on inequalities remained the Heckser-Ohlin-Samuelson model, from which two classical predictions about the labour impact for Northern economies were derived: 1. Trade openness was doomed to bias Northern exports in favour of capital-intensive products and to increase the share of capital in the remuneration of factors (this is the well known Samuelson-Stolper condition[1]); 2. Openness would simultaneously lead to a redistribution of income in favour of high-skilled workers. In the classical account of [Samuelson 2004], this transfer results from a classical HOS factor-remuneration mechanism, coupled with inefficient redistribution at the national level. Contemporary research stemming from the seminal Krugman-Melitz models ventured a more micro version of this argument: the main feature of models with Chamberlin-differentiation is that trade openness will force less productive firms to exit, [Melitz 2003] arguing that in late 20th c. U.S. context, these exiting firms were more intensive in low-skill labour [Burstein and Vogel 2017]. It has also been noted that managers have more comprehensive skills, and can more easily navigate between sectors, while blue-collar skills are more industry-specific, re-training, to them, being longer and more costly [G. M. Grossman, Helpman, and Kircher 2017].

However, econometricians of the 1990s found it extremely difficult to substantiate these predictions on available trade and labour data of the time. The dreary consequences of trade openness on inequality foretold by the HOS model were at most “elusive” [Krugman, Cooper, and Srinivasan 1995].

One explanation of these equivocal results involves data restrictions. In the early 1990s, import exposure remained relatively limited. It is mainly after the turn of the millennium that China rose to the status of leading manufacturing exporter of the World (in terms of share of international exports of goods, China grew by 1.7 points over 1990-2000, versus 6.6 points over 2000-2010; it surpassed the U.S. in 2006). Arguably, in the early 1990s, it should have been challenging to extract from labour data a massive negative effect of trade competition on unemployment and wage inequality using data from the 1980s. Available methodologies were mostly prospective, based on the factor content of trade and on estimates of the elasticity of substitution between skilled and unskilled labour [Borjas, Friedman, and Katz 1997; Rodrik 1997] or on the relative evolution of product prices [Slaughter 1998], and their conclusions were but mixed: in the great paper by Borjas et alii, trade competition from the South explained at most 6% of the wage premium obtained by college-educated workers in the U.S. over 1980-1995. Even in the maximal strategy of [Acemoglu 2003], who accounted for the indirect impact of trade on skill-biased technological change, import exposure explained 23% at most of these wage premia. This was one of Paul Krugman’s leading line of argument at that time: the purported massive negative distributional impact predicted by HOS-type models and the Stolper-Samuelson theorem were nowhere to be found in actual income data:

> *When a country with a highly skilled labor force increases its trade with a country in which skill is at a greater premium, it can expect a decline in the real wages of its own unskilled workers. [...] All the evidence suggests, however, that this effect will be extremely small. While economic theory suggests that trade between the U.S. and Mexico should involve an exchange of skill-intensive for labor-intensive products, such a bias in trade against low-wage U.S. workers is surprisingly elusive in the actual trade data. Most notably, the widely cited study of NAFTA by Gary Hufbauer and Jeffrey Schott*[2] *finds that U.S. industries that compete with imports from Mexico pay almost exactly the same average wage as industries that export to Mexico. [...] This lack of evidence that trade really does worsen American income distribution is not unique to the Mexican case. Two economists who expected to find a significant effect of trade on wages have concluded that virtually none of the growth in wage inequality in the United States since 1979 is due to international factors*[3]*. A survey by Lawrence Katz reaches the same conclusion*[4]*. As a matter of theory, then, we must concede that NAFTA should be expected to hurt low-skill U.S. workers. In practice however, there is no evidence supporting this belief. [...] NAFTA will neither create nor destroy jobs, but it will make the existing North American labor force more productive. No serious study has failed to find that NAFTA will produce a small net gain for the*

[1] The original wording of which implies very restrictive hypotheses, including perfect inner competition, constant returns to scale, and same number factors and of products.

[2] [Hufbauer et al. 1993]

[3] [R. Lawrence and Slaughter 1993]

[4] [Katz 1992]

United States. [Krugman 1993]

As a consequence, at the end of the 1990s, it was widely assumed that trade had but a limited impact on inequality *within* one region or one country [Autor and Katz 1999]. The new generation of macro trade models indeed predicted a decline in low-skill employment in Northern economies, but it was assumed that these negative effects were largely offset by the positive macro gains in productivity, and that minor adjustments of the monetary and fiscal policy would be enough to tackle them. The consensus on that point was widespread [Wood 2018].

As emphasised by [Dorn and Levell 2021], the persistence of this consensus cannot be imputed exclusively to data availability issues, or to the fact that this literature was relying on old trade figures. Late replications of the factor content or price strategies [Bivens 2007; Edwards and R. Z. Lawrence 2010], even though they relied on data of the early 2000s, still failed to explain more than 10% of the widening in wage premia. The core issue was the specification used, most importantly the outcome variable of interest: the HOS predictions directed research towards an exclusive focus on wage premia for higher-skilled worker, obfuscating the wider context.

In the early 2010s, the new focus in the academic literature on empirical identification [Angrist and Pischke 2010] prompted a new vein of research attempting to gauge the impact of trade openness on inequalities. Across the diverging methodologies of this literature, one might isolate at least three common points:

- *A shift towards more comprehensive definitions of trade exposure* – The factor content or price approaches, widely criticised [Burstein and Vogel 2017], have slowly been replaced by strategies which exploit the variability in exposure to trade across regions of a country, or sectors of a labour market. [Margalit 2011], who focused on trade-driven industrial layoffs in the U.S., or [Topalova 2010], who studied the 1991 Indian liberalisation, are well known precursors within the empirical literature, but concerns for sectoral and regional variability has also become an important feature of trade models [Caliendo, Dvorkin, and Parro 2015; Galle, Rodríguez-Clare, and Yi 2022];
- *The rise of quasi-experimental frameworks* – The credibility revolution in econometrics induced many scholars to turn away from all-encapsulating trade models to focus on specific trade policy events [Kovak 2013; Lopez-Acevedo and Robertson 2012];
- *Looking beyond aggregate effects* – Through its careful exploitation of sectoral and regional variability, that literature was able to prove that, even if at the aggregate level the impact of trade on inequality was relatively moderate, the negative effects of liberalisation had been concentrated on some very specific manufacturing sectors and on a little number of left-behind regions [Helpman et al. 2016], and that these margins of trade liberalisation policies have played a critical role in the evolution of the social and political atmosphere of the last decade [Rodrik 2021].

In this paper, I venture an identification strategy which exploits the potentialities of the IV-quantile regression estimator of [Chetverikov, Larsen, and Palmer 2016] combining *Comtrade* data with new micro-level income series published in [Fournel 2024]. We focus on the case of France at the turn of the millenium; we test several trade partners, but as we'll see, significant results can only be extracted when focusing on the lead emerging market: China.

2 Base specification

Identification strategy Our primary identification strategy, as outlined in [Fournel 2023a], adopts a shift-share approach. This method integrates trade data from the Comtrade database with Census data regarding the local employment structure. Denoting zones with an i and industrial sectors with a j, we establish an index representing the change in import exposure per worker of a zone over the period t to $t+1$, denoted $\Delta IPW_{it,t+1}$. To compute this index, we multiply the change in imports (ΔM) from the trade partner over the time period by the share of region i in the total national workforce of sector j at time t. The summation across sectors yields the individual loss per worker caused by imports over the period, expressed as:

$$\Delta IPW_{it,t+1} = \sum_j \frac{L_{ijt}}{\sum_i L_{ijt}} \frac{\Delta M_{jt}}{L_{it}} \tag{1}$$

Endogeneity issues For the instrumental variable, we utilize a similar index, denoted $\Delta \overline{IPW}_{it,t+1}$, where import figures ΔM are replaced with $\Delta \overline{M}$, representing imports from a control group of advanced economies. Additionally, we employ a lagged start-of-the-period labor force (L) by one period (a decade) to mitigate potential simultaneity bias. The instrumental variable is expressed as:

$$\Delta \overline{IPW} it, t+1 = \sum_j \frac{Lijt-1}{\sum_i L_{ijt-1}} \frac{\Delta \overline{M} jt}{Lit-1} \tag{2}$$

Main specification Moving to the main 2SLS specification, we establish a straightforward framework. Here, the evolution of local manufacturing employment over the decade (ΔL) is regressed on the main exposure index, a time dummy for each decade, and a vector of controls. The regression equation is formulated as follows:

$$\Delta L_{it,t+1} = \beta_1 \Delta IPW_{it,t+1} + X' it \beta_2 + \gamma_t + uit \tag{3}$$

Here, $\Delta IPW_{it,t+1}$ is instrumented by $\Delta \overline{IPW}_{it,t+1}$ as described above.

We refer to [Fournel 2023a] for first stage estimates and other robustness tests of this empirical strategy.

3 Impact on the average output

3.1 Main results

The simplest way to gauge the impact of an import shock on between-inequalities is to evaluate specification (3) using the decadal rise in fiscal income within the geographical unit of interest as the dependent. This *between* approach is the only type of estimates found in [Autor, Dorn, and Hanson 2013] and in almost all of its replications. The results of the stacked decades' estimation[5] are reported in table 7. As far as comparison is possible, the estimates of [Autor, Dorn, and Hanson 2021] are almost similar to the results displayed there in column (2):

Since metropolises and richer cities are overall more exposed, trade shocks have almost no impact on the income rankings of cities and regions. If we build a counterfactual scenario with $\Delta IPW_{1990-2018} = 0$, offering to each city a premium equal to the average exposure over 1990-2018 of the ZE it belongs to times the corresponding effects reported in table 7, replicating it on every issue of the IRCOM series, taking then the ratio top 10% versus bottom 50% of cities according to the average fiscal income of their inhabitants, we see that counterfactual values are barely distinguishable from the actual ones (see figure 7).

3.2 Regional decompozition

These marginal estimates indicate that more exposed regions tend to suffer from an income loss *ceteris paribus*, but they hardly illuminate the general picture about the distributional impact. Two main limitations stand in the way: 1. There are reasons to believe that these marginal effects might vary across types of regions ; 2. A consistent interpretation must juxtapose these marginal effects with the total rise in fiscal income per region, since, as emphasised by [Dorn and Levell 2021] a very same marginal decline could be barely felt in a fast-growing metropolis (which is likely to benefit from other channels of trade exposure), while it might have dreary consequences in a declining city. Figure 8 heeds to these two biases: 1. Relying on the IRCOM data series 1990-2018, we divide cities in weighted-deciles along three great variables: the average fiscal income of their inhabitants, their size (tax unit population) and their distance to the nearest of the 22 metropolises of the country. Over each subgroup, we reestimate marginal effects, multiplying them by the average decadal ΔIPW of the group, which provides us with an estimate of the decadal income loss; 2. We then plot the actual mean decadal rise in fiscal income for each subgroup; summing with the estimated loss provides us with the counterfactual scenario estimates.

As suspected, even though richer cities are slightly more exposed, when we plot the average rise in fiscal income over 1990-2018 over the distribution of cities along major variables (their av. fiscal income, their size, their distance to a metropolis), we systematically get a U-shaped curve; it is the average *commune* (in terms of income,

[5] Since in empirical tests [Autor, Dorn, and Hanson 2021] and econometrical calibrations [Galle, Rodríguez-Clare, and Yi 2022] alike, the impact of the trade shock is fully realised approximately seven years after the start of the exposure, it seems legitimate to estimate decade by decade (under the assumption that the shock of one decade impacts the income evolution of the contemporary decade, and not of the following one).

Table 1: Exposure to import competition and evolution of between regions inequalities

Dep. : Decadal change in the av. fiscal and disposable income (in pp)			
	Distribution of spatial units		
	Between départements	*Between ZEs*	*Between communes*
	(1)	(2)	(3)
Rise in import exposure:			
Panel A. IRCOM dataset − 1990-2018 − Fiscal income			
β_1	-4.34^{***}	-1.93^{**}	-1.41^{**}
S.E.	(1.08)	(0.82)	(0.59)
R^2	0.85	0.32	0.39
F-stat	44.6^{***}	12.6^{***}	1393^{***}
Obs.	282	912	71520
Panel B. − Filosofi dataset − 2012-2017 − Fiscal income			
β_1		-2.16^{*}	
S.E.		(1.13)	
R^2		0.44	
F-stat		6.9^{***}	
Obs.		304	
Panel C. − Filosofi dataset − 2012-2017 − Disposable income			
β_1		-1.41^{*}	
S.E.		(0.83)	
R^2		0.54	
F-stat		10.6^{***}	
Obs.		304	

Sign. thr. : *p<0.1; **p<0.05; ***p<0.01

Note: Income data are from the IRCOM and Filosofi bases (see [Fournel 2024] for the derivation of micro series). We report the estimation of the main coefficient of model (3), but the dependent variable is the evolution of the related average yearly fiscal or disposable income of the persons living within each geographical unit of interest. The main explanatory variable is the index ΔIPW, described herein above, which provides an estimation of the mean rise in Chinese imports (in value) per worker within each ZE. The instrument is the same ΔIPW, in which French trade data has been replaced by a control group of four countries (Japan, Germany, Spain, Switzerland) and all labour force variables are taken with a decadal lag. When the time period exceeds ten years, decades are stacked with the inclusion of a time dummy. Each specification includes the full vector of controls; when the model is estimated at the level of the *commune*, we ascribe to each city the explanatory and the instrument of the ZE to which it belongs; other controls, and the dependent, are city-specific. Observations are weighted by the start-of-the-period total tax units population. Standard errors are clustered at the level of the INSEE superzones.

of size, of proximity to bigger cities) which grows slower, the marginal impact of trade shocks being even more pronounced among these average cities.

A reduced form approach cannot, *stricto sensu*, construct a counterfactual scenario in autarchy, in the spirit of the recent ones of [Adão et al. 2020; Fajgelbaum and Khandelwal 2016]. In one of their companion articles, Autor, Dorn and Hanson [Autor, Dorn, and Hanson 2021] heed to that issue by juxtaposing their estimates of the negative income impact of import exposure, and some recent results about the aggregate gains from trade, relying on two types of evidence:

- Macro trade models which elaborate the relocation model of [Eaton and Kortum 2002] in order to gauge the aggregate welfare impact of the China shock. [Caliendo, Dvorkin, and Parro 2015], who calibrate their model with the ΔIPW of the original Autor-Dorn-Hanson article, and data about the U.S. in the first decade of the millennium, find a sharp negative impact on wages in the short-run due to local labour effects (with an average decline of -0.4 seven years after the shock), which is offset in the longer term by aggregate gains caused by increased competition, mainly the decline in prices and productivity gains; overall, 13 years after the shock, the average aggregate welfare gains is estimated at $+0.2$ log points (unweighted S.D. of 0.09). [Galle, Rodríguez-Clare, and Yi 2022], because they include a labour immobility factor in their model, find a similar positive impact ($+0.22$ over a 7 years period), but with a far greater variability between regions (unweighted S.D. of 0.31). As far as comparison is possible, [Borusyak and Jaravel 2021] seem to find slightly superior marginal gains of trade; they estimate that a 10% decline of trade costs with China results in a $+0.15$pp net welfare gain per worker; since, over 2000-2010, U.S.-China trade costs have declined by 27% (World Bank's ESCAP figures), the net gains would oscillate around $+0.4$pp;

- Micro evidence about the net gains of purchasing power due to trade competition. [Jaravel and Sager 2019], using estimates drawn from Autor-Dorn-Hanson papers, estimate that a 1% in import penetration within the CZ results in a decline of consumer price of about -1.4%; using a similar approach, [Dorn and Levell 2021] report estimates which are about half the size of Jaravel-Sager ones.

One of the main drawback of this approach lies in the focus on the average outcome, at the expanse of the distributional dimension. The implied bias for the estimation of the gains from trade through consumer prices is negligible according to the most recent micro analyses [Borusyak and Jaravel 2021], which tend to show that the share of goods imported from China within the consumption basket is relatively flat across deciles of the income distribution. For other channels (especially the impact of labour relocations), we might expect considerable differences between deciles; such differences are found even in the most optimistic micro approaches [Borusyak and Jaravel 2021].

Starting from these figures, [Autor, Dorn, and Hanson 2021] proceed with a relatively straightforward strategy; they estimate an extended version of model (3) using as main explanatory $\Delta IPW_{c,2001,2012}$, and as dependent, the variation of the average log personal income of all inhabitants of each CZ over 2001-2019. They retrieve the corresponding $\hat{\beta}_1$. They consider the distribution of the average predicted loss $\hat{\beta}_1 \times \Delta IPW_{c,2001,2012}$, centring it around its national weighted average. Regions, the net loss of which is below the purported decadal gains of trade discussed herinabove might be considered as the losers of trade liberalisation. Here, we emulate that strategy using the very same decades; we are forced to rely on models calibrated over U.S. data, the only framework relying on the Autor-Dorn-Hanson approach applied to European countries being [Caliendo, Dvorkin, and Parro 2015], which provides for France marginal gains of trade slightly superior to the U.S. estimates ($+0.23$).

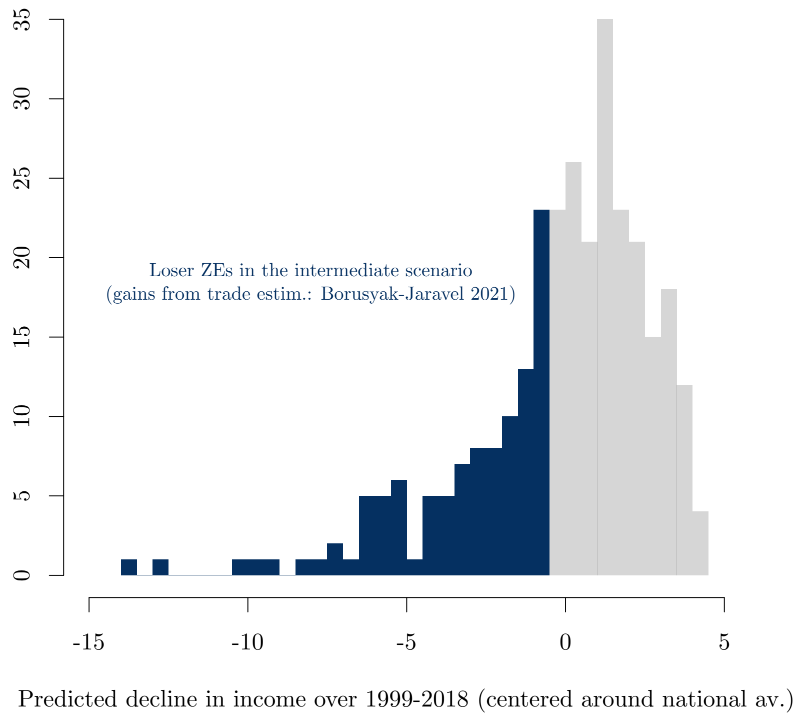
The $\hat{\beta}_1$ we retrieve[6] is -2.27. The weighted average of the corresponding estimated loss in av. fiscal income over 1999-2018 is -4.53 log points, an impact which, as far as comparison is possible, is about one half greater than the one found in [Autor, Dorn, and Hanson 2021]. Figure 1 plots the distribution of this statistic. We then differentiate with multiple estimates of gains of trade to detect the loser ZEs in the sense of Autor, Dorn and Hanson. Two polar scenarios based respectively on [Caliendo, Dvorkin, and Parro 2015] and [Jaravel and Sager 2019] are displayed as maps in 9, and an intermediate scenario based on [Borusyak and Jaravel 2021] in the histogram of figure 1. The variance of our estimated losses is almost twice the size of the Autor-Dorn-Hanson estimates (2.96 versus 1.22), not because of a difference in the marginal effect, but because exposure varies much more across ZEs than across U.S. commuting zones. As a result, the perimeter of the losing regions is much more robust to the choice of gains of trade estimates. In [Autor, Dorn, and Hanson 2013], 38% of the U.S. population lives in a losing CZ in the minimal scenario of based on Caliendo et alii; this figures drops to a tiny 7% in the maximal scenario based on Jaravel-Sager price estimates. The respective values in our setting are 32% and 22%.

4 Distributional impact – Wage and other labour market outcomes

The differential impact through the wage distribution, plotted in figure 10, is in the spirit of the UK estimates of [De Lyon and Pessoa 2021], of the US ones by [Chetverikov, Larsen, and Palmer 2016], of the French ones of [Malgouyres 2017] and of [Autor, Dorn, and Hanson 2013] alike. On mean yearly wages reported in the DADS, the impact of a +\$1000 rise in exposure is negative and 10% significant, at -6.27 log points (see table 2), a figure approximately one half greater than the ones found by [Autor, Dorn, and Hanson 2021] for the U.S. and by [Malgouyres 2017] for 1995-2007 French data. As far as comparison is possible, when we focus on the distributional dimension, the difference in reaction to an import shock between the first and last quartiles of the wage distribution

[6] In this version of model 3, the explanatory is $\Delta IPW_{1999,2008}$, instrumented as described above, the dependent, the evolution of the average log fiscal income of the ZE between 1999 and 2018 (computed over the IRCOM base).

Figure 1: Centred distribution of the income loss caused by a decade of import competition exposure



Note: The unit of interest is the *Zone d'emploi* (2010 INSEE definition). Income data are from [Fournel 2024], and from the IRCOM bases. We estimate model 3, using the import exposure index $\Delta IPW_{1999,2008}$ as the main explanatory variable (instrumented in the way described herein above), the full set of controls, and the variation in fiscal income (in log points) within the ZE over 1999-2018 as the dependent, weighting observations by the start-of-the-period population. We retrieve the corresponding coefficient $\hat{\beta}_1 = -2.27$ (t-stat: 1.95) and multiply it by the exposure of each ZE over 1999-2008, providing an estimate of the income loss caused by a decade of import exposure on local incomes. This histogram plots the distribution of that statistic, centred around the national weighted average. ZEs for which that statistics is below minus the aggregate gains from trade estimates of [Borusyak and Jaravel 2021] are displayed in blue.

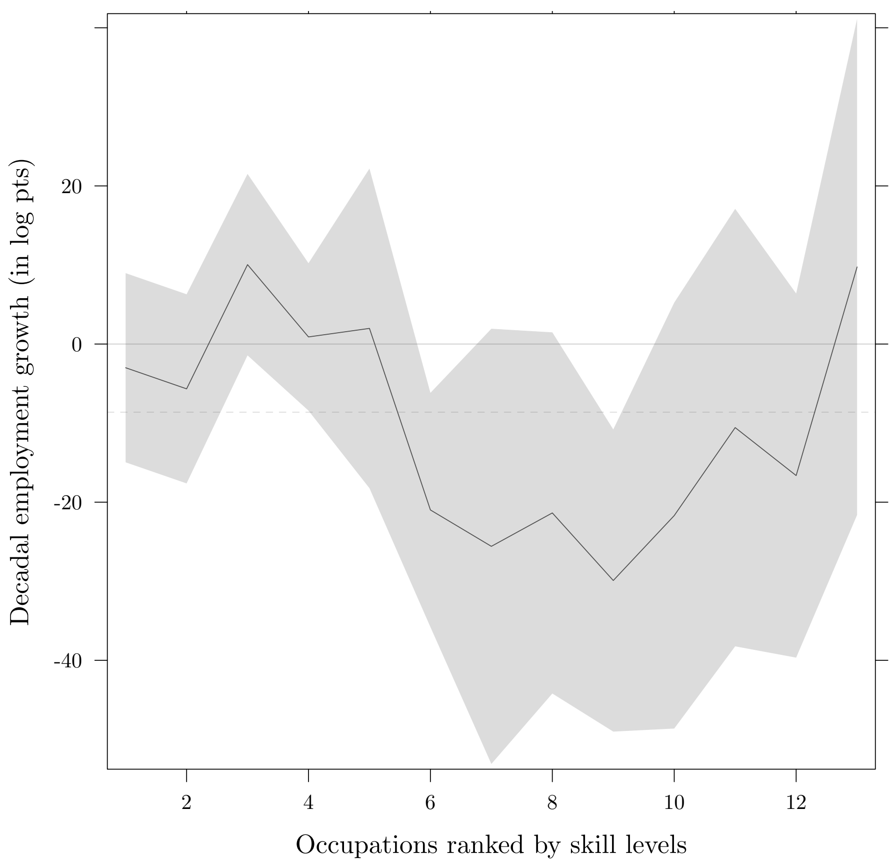
is one half wider than the one reported by [Chetverikov, Larsen, and Palmer 2016]. [Adão et al. 2020] find a much more polarised reaction, but over the labour market of the developing country hardly comparable to our setting.

Nevertheless, some peculiarities of the French case must be heeded to:

- *A seemingly negative impact on innovation within firms, even near the technological frontier* – The literature provides conflicting results when it comes to the issue of the innovation impact of the China shock. In the U.S. context, Autor and his coauthors argue that the shock had a significant depressing impact on innovation input (R&D spending) and output (patents) [Autor, Dorn, Hanson, et al. 2016]; over European data on the contrary, [Bloom, Draca, and Van Reenen 2015] find that the Multifiber agreement bolstered patenting among more exposed firms. The usual argument to reconcile these findings involves the inverted-U-curve of [Aghion, Bloom, et al. 2005], i.e. a rise in competition on markets which used to be highly regulated shall have a positive impact on innovation, but exacerbated competition and high variance of the level of technological advancement might induce *laggard* firms to drop out of the innovation contest. Congruent with this setting, Aghion and his coauthors recently identified among French firms a general negative impact of the China shock on innovation output, an impact largely concentrated onto firms which have a prior productivity disadvantage and were lagging far behind the technological frontier [Aghion, Bergeaud, et al. 2021]. Actually, Aghion's findings draw a more disturbing picture: firms which are more exposed to Chinese import competition tend to invest less, compensating with: 1. A Ricardian focus on products for which France had a prior comparative advantage (an effect identified among all types of firms) ; 2. A rise of the Chamberlin differentiation, with an acceleration of the pace at which new products are introduced and old ones discontinued (among firms which are nearer to the technological frontier only). In a sense, the shock bolsters the demand for creative and innovative jobs at the very top of the wage and skill hierarchy (an impact clearly seen in figures 3 and 2), while those intermediate jobs which are usually needed to implement new innovations are needed less;

- *Downgrading polarisation in the non-exposed sector* – [Harrigan, Reshef, and Toubal 2020] find in French context that trade shocks foster job polarisation within service firms with hikes in demand for either very low-skilled jobs (like retail workers) or very high-skilled ones (what they call *techies*), at the expense of lower-intermediate blue-collar jobs (this corresponds to the median worker in our fig. 3b, or to the longer vocational programs in fig. 2, the decline of which is primarily driven by non-manufacturing sectors);

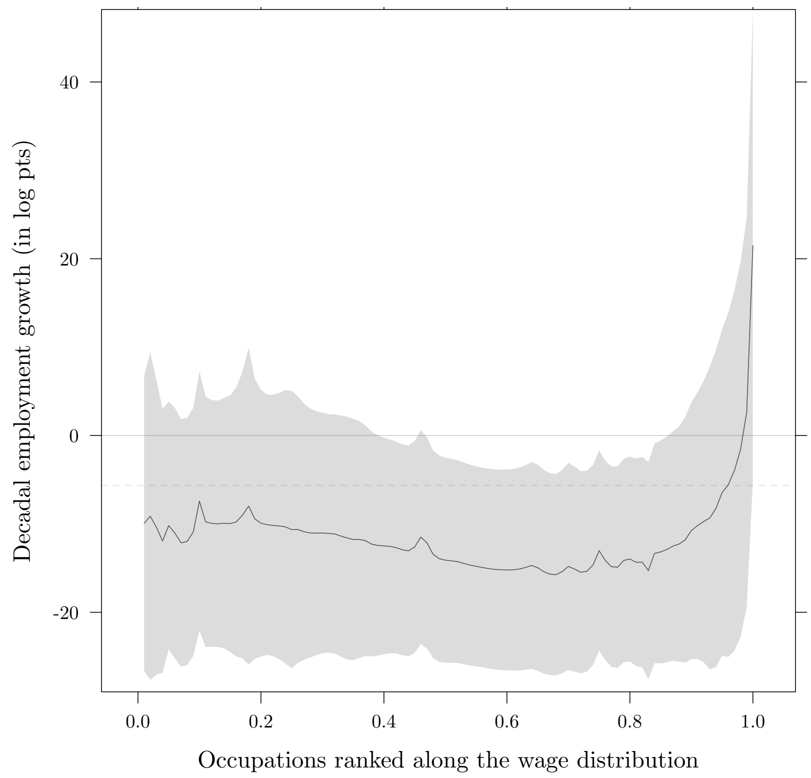
Figure 2: Import shocks and labour market response − Job polarisation (skills)



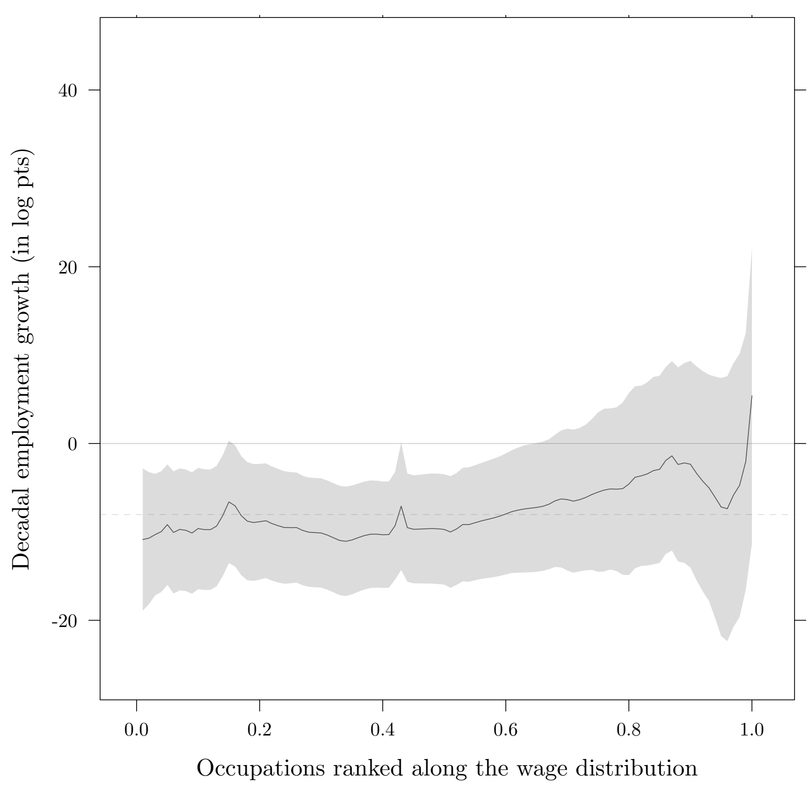
Note: The unit of interest is the *département*. Employment data are from the 1/12 microsample of the DADS. The main specification is still 3 with the full vector of controls and the Chinese imports' exposure index ΔIPW as the main explanatory, but this time the dependent is the decadal evolution (in log points) of the total stock of employment for a specific occupation. The decade estimated is 2008-2018, and we take the exposure index of 1999-2008 as the explanatory, with the corresponding instrumentation. Occupations are then ranked as follows: We group occupations using the PCS scale at the 4-digits level. We consider the individual diploma scale of the INSEE with slight modifications (1 - No schooling / 2 - No high school and no diploma / 3 - Some high school but no diploma / 4 - Middle-school diplomas (BDC-BEPC-CEP-DFEO) / 5 - Vocational education, short diplomas (CAP-BEP) / 6 - Vocational education, long diplomas (Bac-Tech-Bac-Pro) / 7 - High school diplomas (Bac) / 8 - Vocational higher education (BTS-DUT-DEUST) / 9 - Undergraduate short diplomas (DEUG-L1-L2) / 10 - Undergraduate long diplomas (L3-M1) / 11 - Graduate (M2) / 12 - Gradute "grande école" / 13 - PhD). Using the 2009 issue of the INSEE-ECMOSS dataset, we reconstruct the distribution of diplomas of the members of one specific occupation. We then compare that distribution to the nationwide distribution of diplomas of all workers, and we ascribe to that occupation the diploma which is most overrepresented compared to the national structure of diplomas for all workers. We estimate our main specification with, as dependent, the employment growth for the set of workers to which that specific diploma has been ascribed in each *département*, weighting observations by the start-of-the-decade log total employment, clustering standard errors at the level of the INSEE's superzones.

Figure 3: Import shocks and labour market response − Job polarisation (wages)

(a) Employment response by wage percentile − Manufacturing



(b) Employment response by wage percentile − Non-manufacturing



Note: The unit of interest is the *département*. Employment data are from the 1/12 microsample of the DADS. The main specification is still 3 with the full vector of controls and the Chinese imports' exposure index ΔIPW as the main explanatory, but this time the dependent is the decadal evolution (in log points) of the total stock of employment for a specific occupation. The decade estimated is 2008-2018, and we take the exposure index of 1999-2008 as the explanatory, with the corresponding instrumentation. Occupations are then ranked as follows: We estimate our main specific using as dependent the employment growth for each occupation of the PCS scale at the 3-digits level within each *départeent* (with a required minimum of 100 workers nationwide), weighting observations by the start-of-the-decade log occupation-specific employment, clustering standard errors at the level of the INSEE's superzones. The grouping method then applied is exactly similar to the one used by [Malgouyres 2017] to construct its figure 7: we calculate how many workers in each occupation belong to the x-th percentile of the start-of-the-decade wage distribution. Then for each percentile, we construct a specific coefficient which is the weighted sum of the occupation-specific betas retrieved before. Standard errors are similarly reconstructed assuming independence between occupation-specific coefficients. The final coefficient plotted for each percentile is meant to provide an estimation of the job loss at a certain location of the start-of-the-decade wage distribution.

Table 2: Within-firm impact of the shock on wages, hours works, and part-time jobs

	*Dep. : Decadal change of corresponding variable (*département*-level)*										
	Period of estimation: 2002-2008 & 2008-2018										
Restriction		Gender		Age			Type of job			Firm size	
		Women	*Men*	*30−*	*31-50*	*51+*	*Blue-c.*	*Menial*	*Sup.*	*249−*	*250+*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Rise in import exposure											
Panel A. Impact on the av. yearly wage income (log pts)											
	-6.27^{*}	-6.09^{**}	-8.66	-17.3^{*}	-6.61	-5.58	-6.99^{**}	-8.56^{*}	-9.19	-5.16^{*}	-5.83
	(3.66)	(2.84)	(6.01)	(9.25)	(4.05)	(6.75)	(3.11)	(4.82)	(6.84)	(2.91)	(4.59)
Panel B. Impact on the total nb of hours worked (log pts)											
	-0.57	-2.04^{**}	0.09	-5.44	-0.04	2.13	0.51	-3.94^{*}	1.09	0.71	-4.09
	(0.92)	(0.86)	(1.04)	(3.64)	(1.05)	(1.24)	(1.15)	(2.02)	(0.78)	(0.63)	(3.23)
Panel C. Impact on the share of part-time jobs											
	2.05	3.02^{*}	1.87	4.53	1.46	-0.58	0.96	1.59	1.51	1.04	5.21
	(1.64)	(1.71)	(1.68)	(2.88)	(1.49)	(1.21)	(1.71)	(1.39)	(2.18)	(1.02)	(3.55)

Sign. thr. : *p<0.1; **p<0.05; ***p<0.01

Note: The unit of observation is the *département* (oversee dép. and territories excluded). The dependent variable is the evolution of the related average yearly wage within the *département*, computed over the 1/12 subsample of the *Déclarations annuelles des données sociales* (DADS). The main explanatory variable is the index ΔIPW, described herein above, which provides an estimation of the mean rise in Chinese imports (in value) per worker within each ZE. The instrument is the same ΔIPW, in which French trade data has been replaced by a control group of four countries (Japan, Germany, Spain, Switzerland) and all labour force variables are taken with a decadal lag. We stack the two decades, including a time dummy. Each specification includes the full vector of controls. Observations are weighted by the start-of-the-decade total employment. Standard errors are clustered at the level of the INSEE superzones.

- *Polarisation and skill-upgrading in the exposed sector* − Most of the European estimates suggest that the employment, wage, and skill impact of the China shock is straightforwardly regressive. Exposure to competition on final goods is doomed to hurt primarily low-skill-low-pay workers [Biscourp and Kramarz 2007; Dauth, Findeisen, and Suedekum 2021], while offshoring and the correlative rise of intermediate imports shall boost the skill intensity and the productivity of the remaining workforce, especially at the top of the wage distribution [Mion and Zhu 2013; Costa, Dhingra, and Machin 2019; Harrigan, Reshef, and Toubal 2020]. This seems congruent with a general narrative where international competition forces unproductive firms out of the market, and fosters reallocation of labour towards more productive ones, in the spirit of the [Melitz 2003] model, or of models which interpret offshoring as a task-trading mechanism [Feenstra and Hanson 2001; G. Grossman and Rossi-Hansberg 2006]. However, as emphasised by [Malgouyres 2017], applying an Autor-Dorn-Hanson framework to the employer-employee matched DADS datasets of the INSEE yields results which are not perfectly consistent with this general story: over DADS data for manufacturing firms, it's primarily upper-middle jobs that are destroyed (fig. 3a), while jobs at the two tails of the distribution are preserved, but both experience sharp wage cuts (fig. 3b). This result[7] is less surprising if we heed to the fact that Chinese imports have become more and more technology-intensive, to the point that, as emphasised by [Rodrik 2006] and [Schott 2008], China can hardly be used as a real-world equivalent of the low-income trade partner in dual HOS models. Our labour market findings are in fact relatively consistent with a setting *à la* Aghion in which firms lagging far behind the technological frontier rescind their innovation investments (hence the decline of these upper-middle jobs which are critical to the implementation of new innovations), while those firms which are still innovating hire only the most qualified creative jobs (the impact being clearly seen in

[7] These findings are congruent with the French estimates of [Malgouyres 2017] and the Danish estimates of [**kellerutar2016polaris**], but also, in a sense, with the original Autor-Dorn-Hanson framework: in, [Autor, Dorn, and Hanson 2013], the sectoral pattern of the China shock is clear-cut: in the manufacturing sector, there are massive layoffs because of import exposure, but no decline of the average wage, while for non-manufacturing they find the reverse reaction (no employment decline, but a sharp negative impact on the wages of lower-paid service employees), i.e., in the exposed industrial sector, the adjustment is made through layoffs; in the non-exposed sectors on the contrary, the adjustment involves primarily a decline of wages; concurrently, there seems to be no impact of the China shock on the share of labour in the remuneration of factors [Autor, Dorn, Katz, et al. 2020], while it is the case French context according to [Aghion, Bergeaud, et al. 2021].

fig. 3a and 2).

5 Distributional impact – Household income

5.1 Regressive effects on pre-redistribution, but not on post-redistribution income

A specificity of French tax and income data is that they do not only provide averages for each region, but also detailed information about the distribution of income within each zone (at the level of the city since 1990, at the level of the *département* since the early 19th c.), most of the time in the form of a piece-wise function, from which income distribution interpolation methods pioneered by [Blanchet, Fournier, and Piketty 2017] can derive a continuous distribution with all its parameters.

When it comes to the estimation strategy for the second impact (the distributional effect of the shock), there are very few items in the econometrical literature which address the specific problem we are faced with. In the absence of individual income data, the best approach that we know of is a group-level-treatment IV quantile regression, a framework which was still used informally in the early 2000s, most notably by [Angrist and Lang 2004], and which has been recently formalised by [Chetverikov, Larsen, and Palmer 2016]. It applies to experimental settings where the treatment is group-specific (whether that group is a school, a firm, a city, a region or else) and where endogeneity concerns involve the group, not the individual dimension. When micro data is missing, the first step of that estimation strategy consists merely in the retrieval of quantiles of the distribution of the dependent variable within each group of interest.

Using our ZEs (or another geographical unit) as groups, we can replicate specification (3), using now as the dependent variable, the evolution of each quantile of the *within*-distribution of income. The results of two group-level-treatment IV quantile regressions (at the city and ZE levels[8]) are provided in figure 4. We also have recourse to a strategy based on the shares of the total fiscal income of the region held by each *within*-decile, the outcome of which is plotted in figure 11.

Consistent with our wage estimates, we find that the impact on fiscal income is largely concentrated on the first three deciles, a finding strikingly similar to the one of Chetverikov, Larsen, and Palmer 2016.

5.2 A rise in dependence to redistribution

Concurrently with [Autor, Dorn, and Hanson 2013] and with our disposable income findings, we identify a clear marginal impact of import exposure on the rise of the share of social transfers within the final income, illustrated in table 3, concentrated on minimum income. When we decompose that aggregate impact by decile of the *within*-ZE disposable income distribution, we find that the rise in shares is concentrated on deciles 2 and 3.

6 Discussion and interpretation

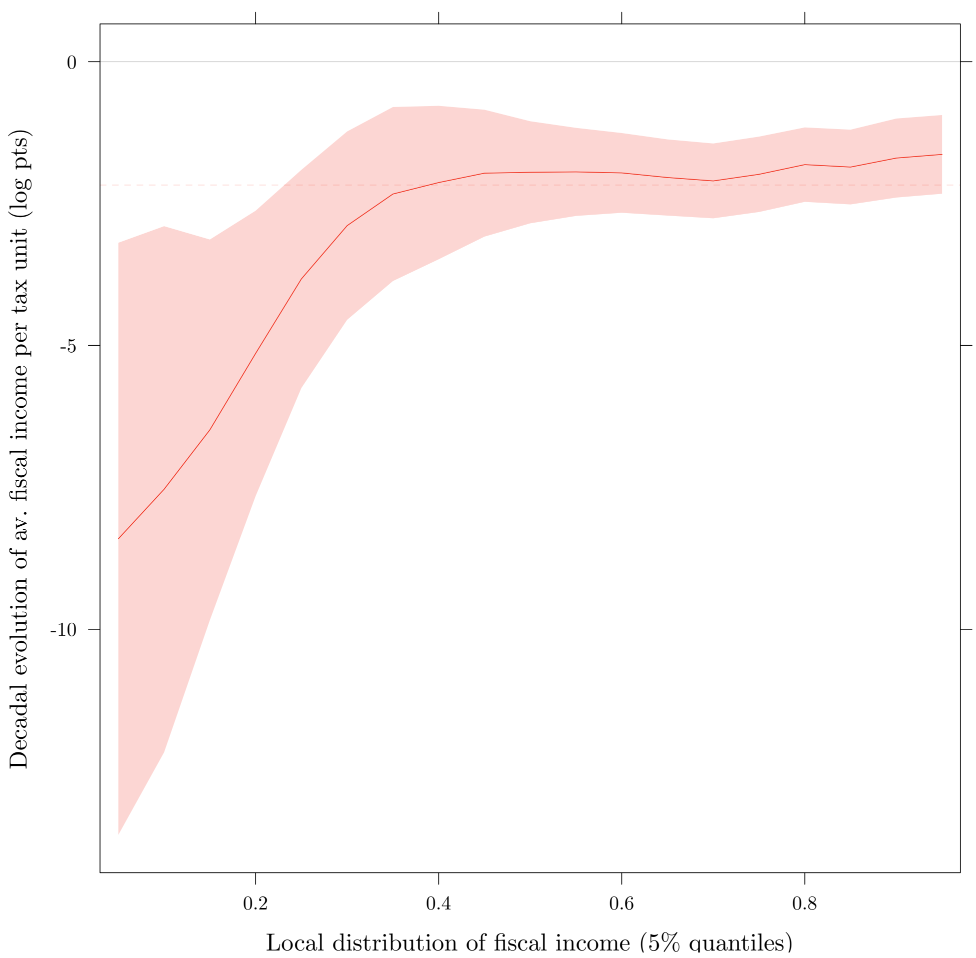
Several results stand out compared to usual results of the literature:

- *A distributional impact less regressive than expected*, at least considering the few existing individual-level estimates [Adão et al. 2020]; for the top 70%, the income impact is relatively flat, and remains sufficiently low so that, if we rely on usual estimates of gains of trade, these families might be surmised to be better-off once the trade shock is fully realised;
- *A sharp discontinuity of the fiscal income impact below the third decile* – Our tests indicate a flat and insensitive impact down till somewhere around the 3rd decile or first quarter, where the estimated loss brutally jumps down. As a consequence, the bottom 30% experience considerable fiscal income losses, up to −25pp over 1999-2008, which is far beyond any estimates of gains of trade. The implied decadal impact on the national ratio T10/B50 is +0.66 for the 1999-2008 decade, +0.16 for the 2008-2018 one. This finding

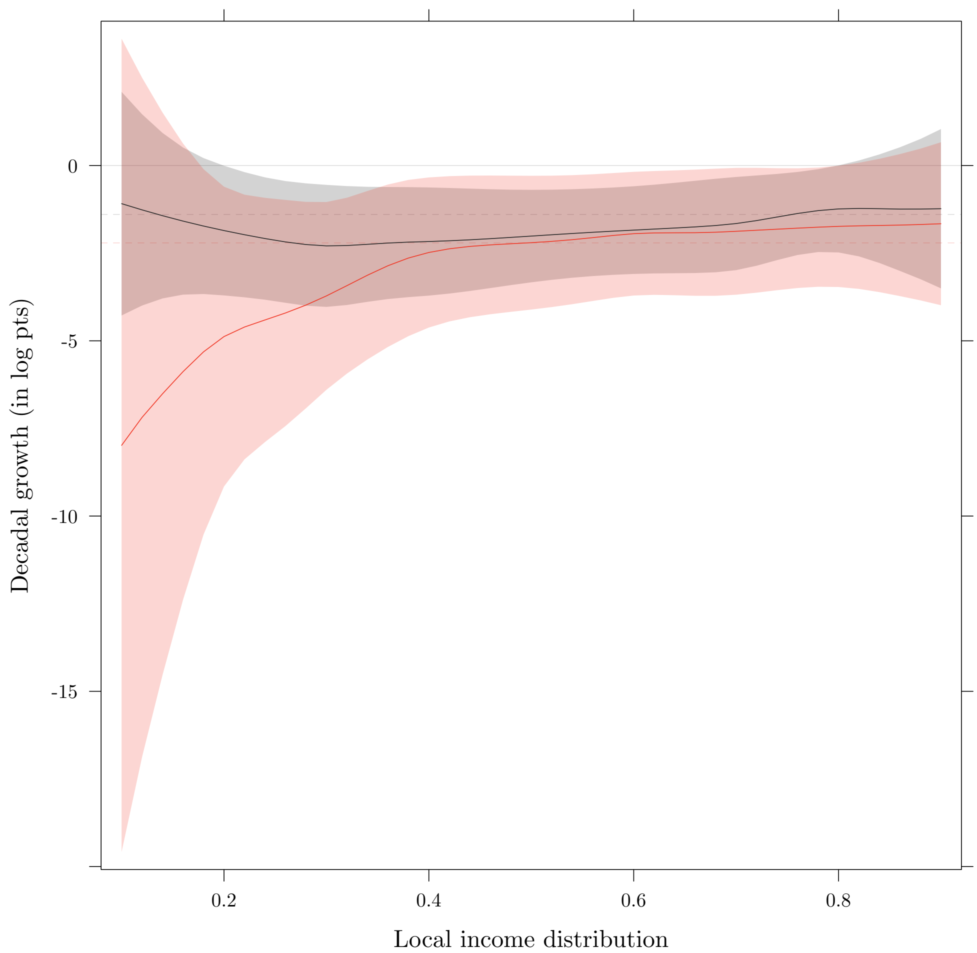
[8] In the estimation strategy based on the IRCOM income figures, the quantiles of each local distribution must be computed with the *gpinter* algorithm of [Blanchet, Fournier, and Piketty 2017]. To check for possible biases created by the interpolation, in the second panel of figure 4, we use non-interpolated quantiles, those which are provided in the Filosofi dataset. Note however that these quantiles are not directly computed by the INSEE over micro data; they are also reconstructed. See [Fournel 2024] for more details.

Figure 4: Distributional impact of a trade shock − Group-level-treatment IV quantile regressions using the Chetverikov-Larsen-Palmer estimator [Chetverikov, Larsen, and Palmer 2016]

(a) Impact on the distribution of fiscal income



(b) Impact on the distribution of fiscal (*red*) and disposable (*grey*) income



Note: The unit of interest is the city-*commune* and the commuting zone-ZE (*Zone d'emploi*, 2010 INSEE definition). The main source for income variables are respectively the IRCOM-R2 and Filosofi (see [Fournel 2024] for more details about the construction of these series). The specification is similar to 3, but this time the dependent variable is the decadal-equivalent evolution (in log points) of each quantile of the local distribution of fiscal (red) and disposable (grey) incomes. The main explanatory variable is the index ΔIPW, described herein above, which provides an estimation of the mean rise in Chinese imports (in value) per worker within each zone. The instrument is the same ΔIPW, in which French trade data has been replaced by a control group of four countries (Japan, Germany, Spain, Switzerland) and all labour force variables are taken with a decadal lag. The period of estimation is 2012-2017 for ZEs, 2001-2018 for cities. All specifications include the full vector of controls; for the city-level strategy, we ascribe to each city the indexes ΔIPW, the routine, offshorability, and machine penetration indexes of the ZE to which it belongs, other controls being city-specific. Observations are weighted by the start-of-the-decade total population of the zone reported in the IRCOM or Filosofi base. Standard errors are clustered at the level of the INSEE's superzones. The main line denotes the coefficient $\hat{\beta}_1$, with the corresponding 95% conf. interval; dashed line provide the corresponding $\hat{\beta}_1$ when the mean rise in fiscal income in the zone is the dependent, i.e. for the first panel -2.17 (t=5.38), and for the second one, respec. -2.13 (t=1.77) and -1.51 (t=1.64).

Table 3: Exposure to import competition and evolution of reliance on social transfers

	Dep. : Decadal change in average share of social transfers in final income					
	Period of estimation: 2012-2017					
		Types of transfers			*Restrictions*	
	All transfers	*Minimum inc.*	*Family allow.*	*Housing benefits*	*Top 10 ZEs*	*Bottom 50 ZEs*
	(1)	(2)	(3)	(4)	(5)	(6)
Rise in imports from China per worker:						
+Full vector of controls:	0.511**	0.411**	0.083	0.036	1.07***	0.469
	(0.24)	(0.19)	(0.05)	(0.04)	(0.21)	(0.37)
R^2	0.68	0.78	0.61	0.57	0.88	0.56
F-stat	18.2***	29.7***	13.2***	10.5***	6.1***	4.9***
Obs.	304	304	304	304	31	152

Sign. thr. : *p<0.1; **p<0.05; ***p<0.01

Note: The unit of observation is the ZE (*Zone d'emploi*, definition of 2010) . The dependent variable is the average evolution (in pp) of the share of each type of transfers within the final disposable (after-redistribution) income of a tax unit within the ZE of interest, as reported in the Filosofi database of the INSEE. The main explanatory variable is the index ΔIPW, described herein above, which provides an estimation of the mean rise in Chinese imports (in value) per worker within each ZE. The instrument is the same ΔIPW, in which French trade data has been replaced by a control group of four countries (Japan, Germany, Spain, Switzerland) and all labour force variables are taken with a decadal lag. All specifications include the full vector of controls (at the exception of offshorability and machine penetration indexes due to data limitation). Observations are weighted by the start-of-the-decade total population of households reported in the Filosofi database. Standard errors are clustered at the level of the INSEE superzones.

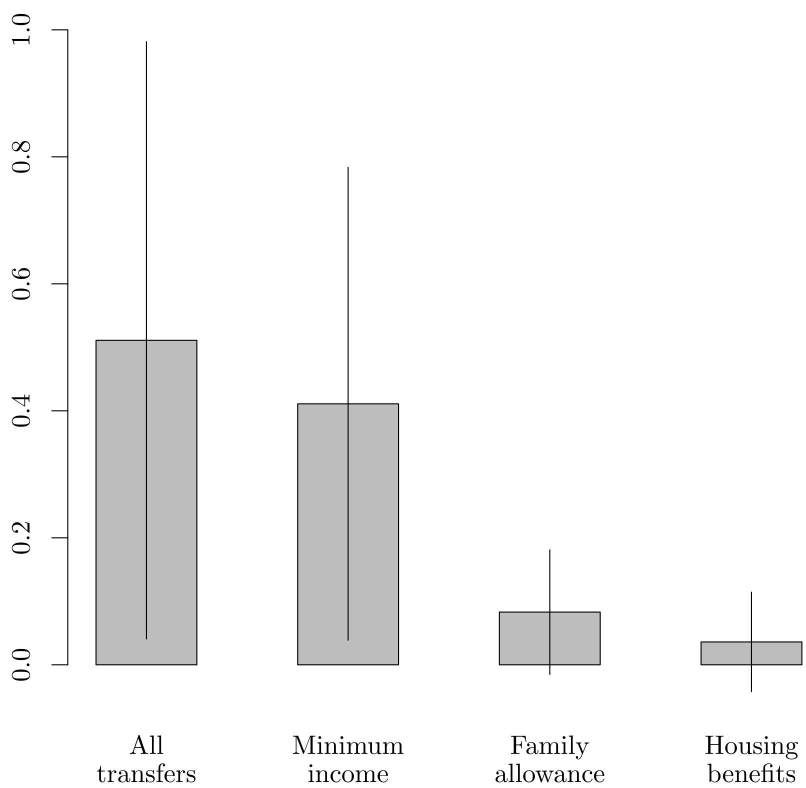
is consistent with other estimates based on labour income [Chetverikov, Larsen, and Palmer 2016] or final income [Adão et al. 2020] data[9]; concurrently, aggregate growth impact curves at the European level [Blanchet, Chancel, and Gethin 2019] display non-linear behaviours below the 3rd decile which might encapsulate some long-term effects of the shocks driven by international economic integration;

- *A quite different picture whether we focus on fiscal or on disposable income*; We had evidence, in European context, of redistributive policies being powerful enough to counter the depressing income effects of trade shocks, so that the final aggregate impact on personal income be nonsignificant [Utar 2018]. However, to our knowledge, the distributive dimension of this fiscal versus disposable difference had never been investigated before. It shows that, if at the aggregate level, redistribution reduces the total ratio T10/B50 by a rough one third margin [Chancel 2019], in the case of trade shocks, it seems surprisingly efficient at suppressing almost the entirety of its inequality impact. However, there remains a margin of lower-middle-class households which fall within the hotspot of the employment and income impact of the import shock, but outside of the usual perimeter of many social transfers. More detailed individual data would be necessary to fully illuminate that point, but it is a crucial indication for the interpretation of the social and political side-effects mentioned hereinafter;

- *Little impact of the between-region divide* – If we replicate the Theil breakdown exercise of [Blanchet, Chancel, and Gethin 2019] at the nation's level, using inequalities of fiscal income *within* and *between* ZEs in 2012 over the Filosofi database, we find that *between*-zones inequalities account for at most 6% of the total (the *between*-Theil index is 0.013, compared to an average *within*-Theil index of 0.203). In our estimation at the ZE level, the first shock (the *between*-zones impact) has paradoxically a slight negative effect on the ratio T10/B50 (-0.006), consistent with the idea that more exposed regions are overall slightly richer. I.e., if regional variability is crucial to our identification strategy, in itself, inequality between regions mediates but a very marginal part of the income impact of shocks caused by international economic integration;

[9] Studying the last decade, [Adão et al. 2020] finds a decadal impact of the order of +1.7 to the ratio T10/B50 of income (in Ecuadorian contexts, where the initial ratio is twice greater than European figures).

Figure 5: Impact on the share of transfers in final income



Note: The unit of interest is the ZE. The main source is the Filosofi database. The dependent variables are the evolution of each type of transfer as a share of disposable income. The main explanatory variable is the index ΔIPW, described herein above, which provides an estimation of the mean rise in Chinese imports (in value) per worker within each *département*. The instrument is the same ΔIPW, in which French trade data has been replaced by a control group of four countries (Japan, Germany, Spain, Switzerland) and all labour force variables are taken with a decadal lag. The period of estimation is 2012-2017. All specifications include the full vector of controls (at the exception of offshorability and machine penetration indexes due to data limitation). Observations are weighted by the start-of-the-decade total population of the ZE. Standard errors are clustered at the level of the INSEE superzones. Bars denote the main coefficient, with the corresponding 95% conf. interval.

- *Investigating the U.S. versus Europe divide* – We have seen that import exposure in French ZEs over 1990-2008 was very similar in magnitude to the ones of U.S. CZs. Yet both countries have followed quite different patterns of inequality dynamics in the last three decades. Actually, shocks caused by international economic integration might have put national social security schemes to test; European systems, as shown in our setting or in European equivalents [Utar 2018], have been surprisingly efficient at deadening the negative income impact of trade shocks, while we might doubt the ability of the U.S. system to do so. Besides, a common feature of the French, but also of the German case [Dauth, Findeisen, and Suedekum 2021] is that a large part of the most exposed regions are overall more export-oriented, more integrated and more dynamic, while in the articles of Autor, Dorn and Hanson, the general portrait of the most exposed zones draws a consistent picture of declining industrial bastions for which the import shock is the final straw;

- *Other channels* – To our final income estimates, we should add the distributional impact of other effects of trade exposure, most notably the export channel (which is slightly progressive in the most recent estimates of [Adão et al. 2020]) and the expenditure channel (for which there is a heated debate to know whether [Fajgelbaum and Khandelwal 2016] or not [Borusyak and Jaravel 2021] lower income families' consumption patterns are more intensive in imported goods).

A distributional impact smoother than could be expected ; effects which are concentrated on the middle city and on the lower-middle-class worker; a rise of redistribution which counteracts the negative effects of the import shock, but fails to bring full compensation for families lying around the 3rd and 4th quantile of the national income distribution. It is quite obvious that such a structural income impact is doomed to spawn social and political reactions which will not be focused on distributional concerns; on the contrary, it seems that all is set to obfuscate the political consciousness of these issues. The discontinuity found in our estimations seems to create a breach at the bottom of the income distribution which might appear ominous for the financial and political stability of European-style social security system [Dauth, Findeisen, and Suedekum 2021;Fournel 2023b]

7 Summary of the findings and conclusion

There are few topics in empirical economics which might better exemplify the shortcomings of an approach focused on the aggregate macro effects of shocks than the issue of trade integration. Arguably, it is very easy to extract from this thesis the main line of argument of the pervasive consensus of the 1990s on trade and inequality reviewed by [Wood 2018]:

- In French context, trade shocks involving a developing partner like China have but insensitive effects on *between*-region inequality; there is a horizontal redistribution between more and less exposed zones (with a marginal commuting-zone-level impact of -2.16pp), but more exposed ones are overall a bit more affluent, making the distributional impact across regions neutral;

- The impact on the distribution of disposable income is almost flat, and moderate enough (decadal zone-level marginal impact of -1.41pp) so that there is little doubt that other channels of the trade shock leave a very large majority of households better off once the total impact of trade openness is realised;

- Like in many European equivalent settings [Dauth, Findeisen, and Suedekum 2021], we find evidence that the most socially vulnerable regions (declining heavy-industry bastions of North-East, or deep rural districts) are relatively protected from such import shocks, and that on the contrary the most exposed regions might benefit from exposure to international trade dynamics through other channels; we find no evidence of import exposure hurting the provision of public goods, or the quality of public services, or more generally the attractivity of a region (since we even isolate in-migration fluxes to the most exposed zones);

Yet the most interesting set of results involves the much commented specific nature of Chinese exports [Rodrik 2006; Schott 2008], their becoming increasingly technology-intensive over the last two decades, and the fact that Chinese competition hardly fits into a traditional North-South HOS framework:

- Over the INSEE's data, the negative impact of the China shock over manufacturing employment (a decadal marginal effect of -4.02pp at the commuting zone-ZE level) is maximal around the 7th decile of the wage distribution, and for workers with undergraduate degrees, a finding at variance the popular image of Chinese imports hurting mainly declining light industries: in a sectoral approach, textiles, but also steelworks, microelectronics and computers, are the main drivers of the effect. These results are consistent with a job polarisation story [Mion and Zhu 2013; Malgouyres 2017], but also with the idea that the China shock might drive little firms lagging behind the technological frontier out of the innovation competition [Aghion, Bergeaud, et al. 2021];

- The sharpest negative wage response (overall marginal impact of -6.27log points) is found around the 2nd decile of the wage distribution in both the exposed and non-exposed sectors, driven mainly by female workers faced with a rise of part-time work and a decline in hours worked. That wage impact is however negative for almost everyone along the whole distribution; we then fail to isolate statistically significant wage premia for top jobs, though we find a sizeable impact of the shock on the demand for the most productive workers (in exposed and non-exposed firms alike);

- If the impact of the shock on post-redistribution income is neutral, the effect on pre-redistribution income is highly regressive, with an overall decadal marginal response of -2.16pp, with a sharp discontinuous effect around the 3rd decile (the marginal impact being estimated at -8.01pp at decile 1);

- The neutral impact on disposable income comes at the expense of a marked rise of the share of transfers within the final income ($+0.51$pp), these families which lose the most in terms of disposable income being found at the 3rd decile, within the hotspot of the fiscal income impact but outside of the scope of many transfer schemes;

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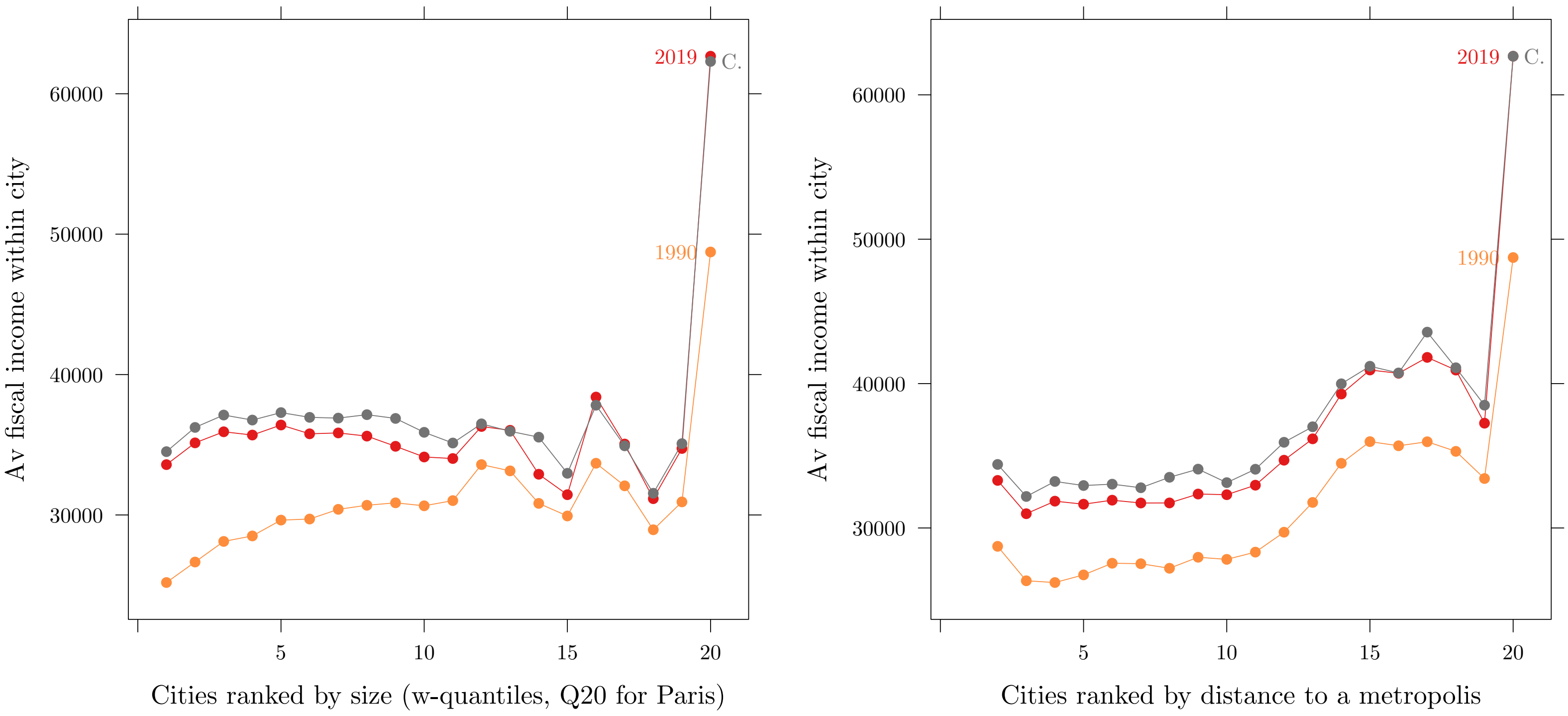
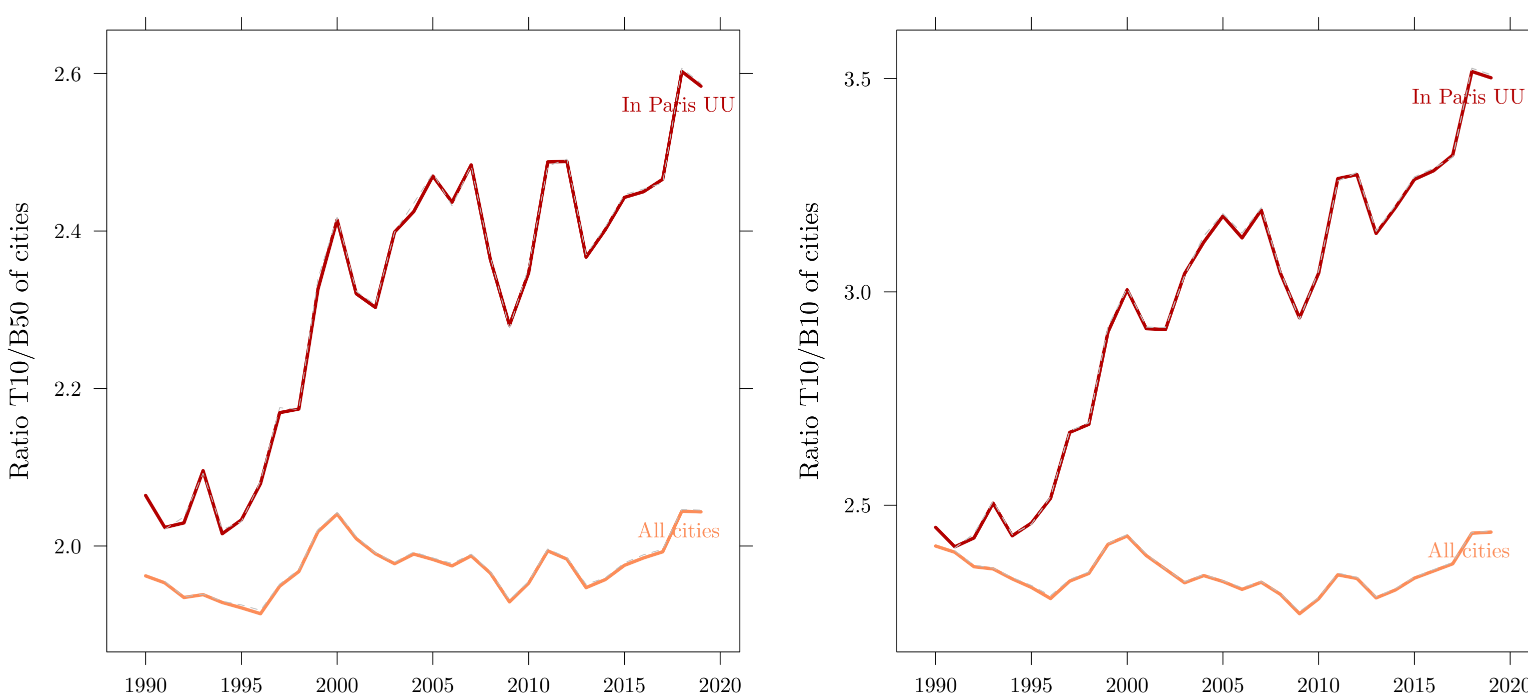
8 Annex A – Supplementary figures

Figure 6: Counterfactual scenario with no exposure ($\Delta IPW_{1990-2018} = 0$) – Impact on the distribution of cities



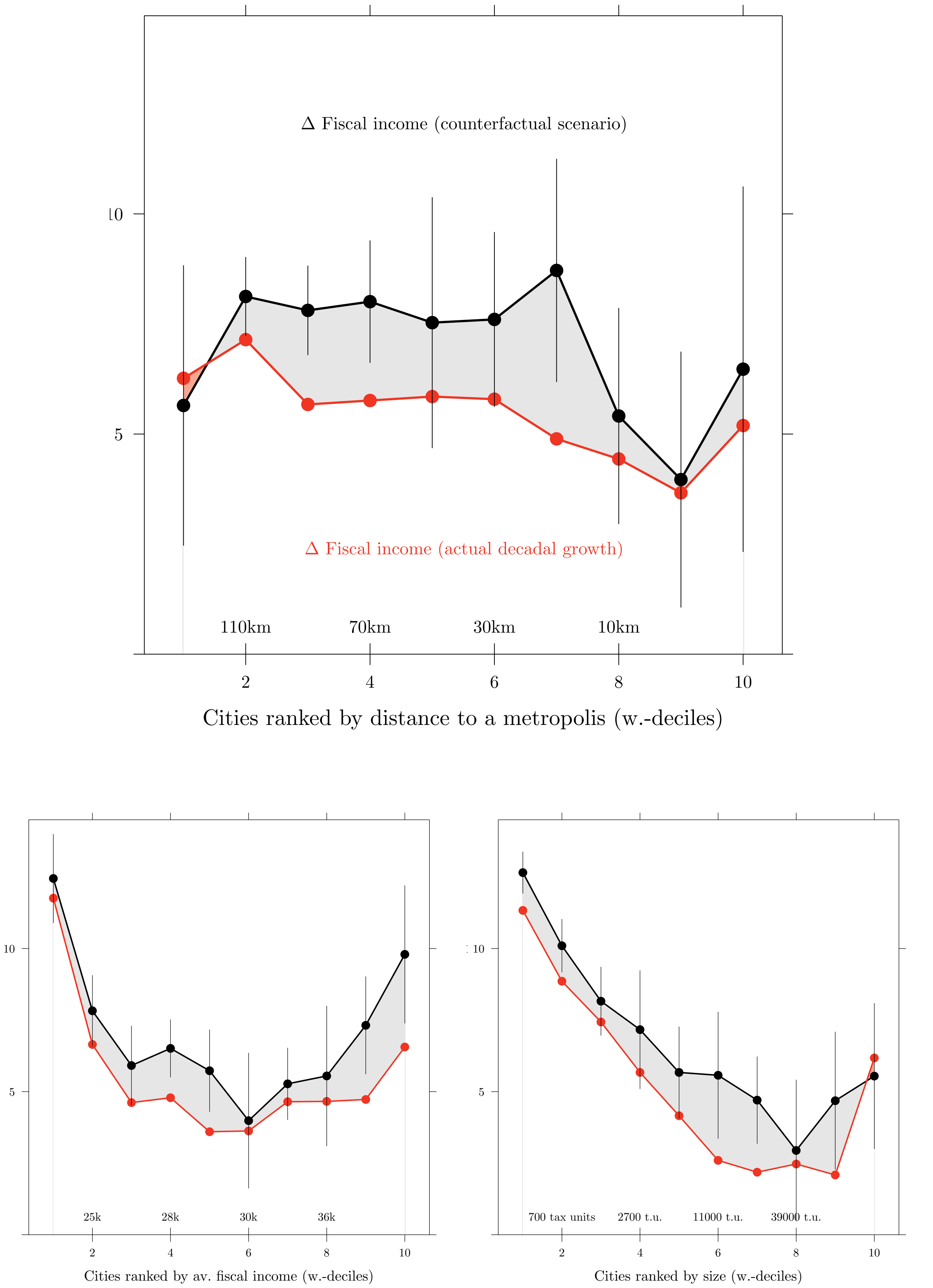
Note: The unit of interest is the *commune*. Cities are ranked by 5% total-population-weighted quantiles of : 1. City size (tax unit pop.); 2. Haversine distance from the centroid of the city to the centroid of the nearest metropolis. Reported statistics is the average fiscal income within the commune, as reported in the IRCOM database (subsample R0, i.e. all cities with a tax unit pop. above 11). Actual values are reported in colour, counterfactual values with import exposure 1990-2018 set to zero, in grey.

Figure 7: Counterfactual scenario with no exposure ($\Delta IPW_{1990-2018} = 0$) – Impact of between-cities inequality



Note: The unit of interest is the *commune*. Data are from the IRCOM database, over restriction 0 (i.e. over all *communes*, at the exception of those with fewer than 11 inhabitants). We consider each city as an individual which earns the average fiscal income of its inhabitants, and we compute the ratio of the average income of the 10% richest cities, over the 10% (or 50%) poorest; fractiles are computed with tax unit population weights, i.e. the 10% richest cities are not the 3600 richest *communes*, but the richest communes in which 10% of the national population lives. Through this computation, the *arrondissements* of Paris, Lyon and Marseille are taken as individual cities (the Western *arrondissements* of Paris always fall within our top 10%, while the Northeastern ones do not). See [Fournel 2024] for more details about the construction of these series. For the counterfactual scenario without trade exposure (plotted as a dashed grey line), we compute the marginal yearly equivalent of the decadal impacts reported in table providing to each city an income premium scaled by the exposure of the ZE it belongs to over 1990-2018.

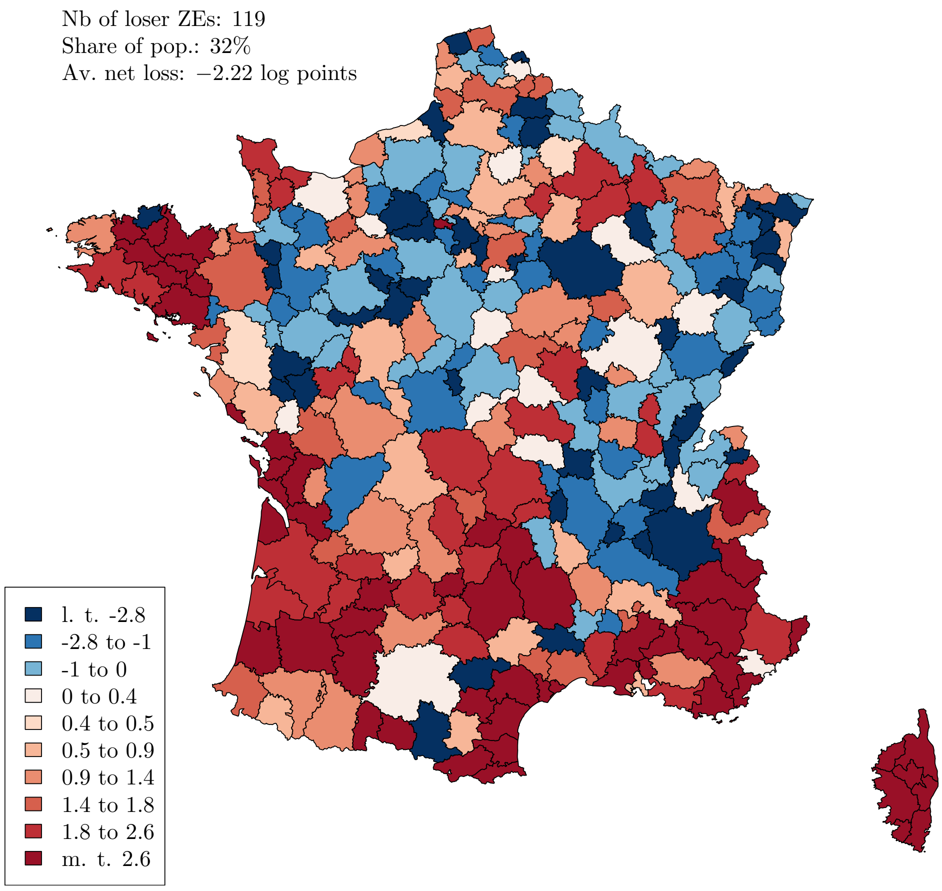
Figure 8: Growth of av. fiscal income across types of cities − Actual growth *vs* Counterfactual scenario with no exposure ($\Delta IPW_{1990,2018} = 0$)



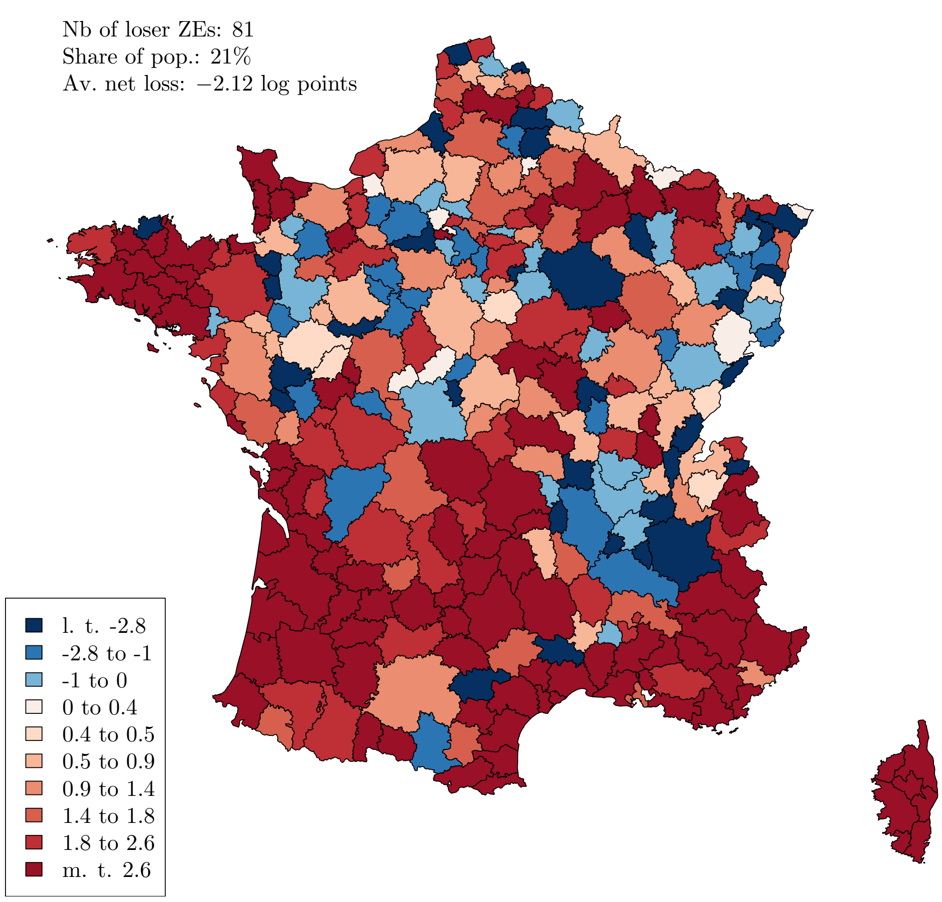
Note: Cities are divided by 10% total-tax-unit-population-weighted quantiles of the corresponding ranking variable. Specification 3 is estimated over each of these subgroups, for three decades (1990-1999, 1999-2008 and 2008-2018), using the full set of controls, and the decadal rise in fiscal income of the inhabitants of the city as the dependent. All growth rates are in pp. We ascribe to each city the ΔIPWs of the ZE to which it belongs; other controls are city-specific. We report the descriptive statistics average rise in fiscal income over the decade, plus the estimated loss of income with its 95% conf. interval. In all specifications, observations are weighted by the start-of-the-period population, and S.E. are clustered at the level of the INSEE superzones.

Figure 9: Regions losing from import exposure in the sense of [Autor, Dorn, and Hanson 2021] using different estimates of gains of trade (marginal deviation of local incomes from national av. in log points)

(a) Aggregate gains for France estimated by [Caliendo, Dvorkin, and Parro 2015]



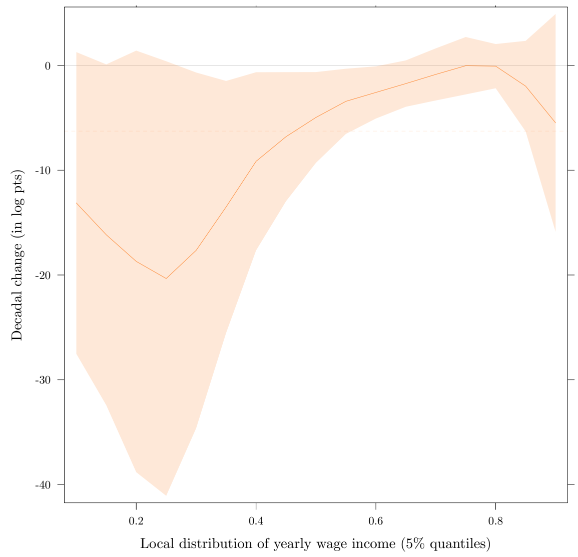
(b) Marginal gains from price effects estimated by [Jaravel and Sager 2019]



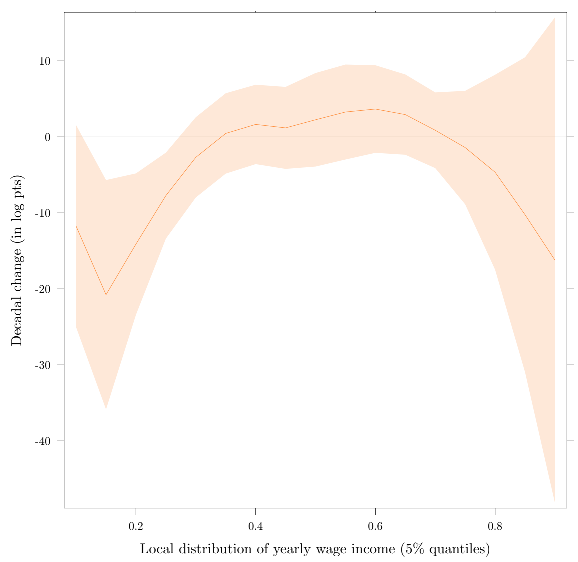
Note: The unit of interest is the *Zone d'emploi* (2010 INSEE definition). Income data are from the IRCOM. The reported statistics, described in figure 1, is the average loss in the average fiscal income of the ZE caused by a decade (1999-2008) of exposure to import imputed to competition from China, expressed in deviation from the national average (log points). Two estimates of gains of trade are added to that statistic; we plot the resulting values.

Figure 10: Import shocks and labour market response − Wages

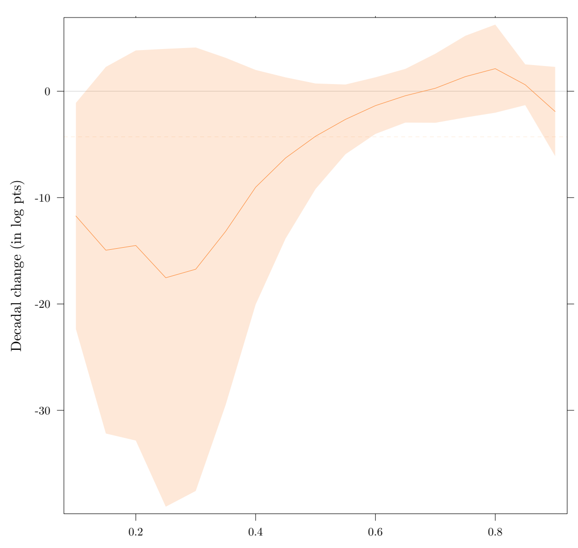
(a) Wage response − All sectors



(b) Wage response − Manufacturing



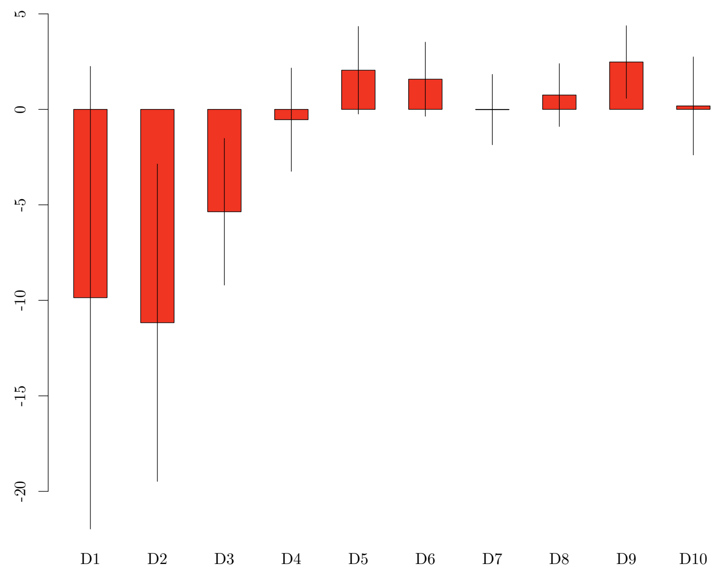
(c) Wage response − Non-manufacturing



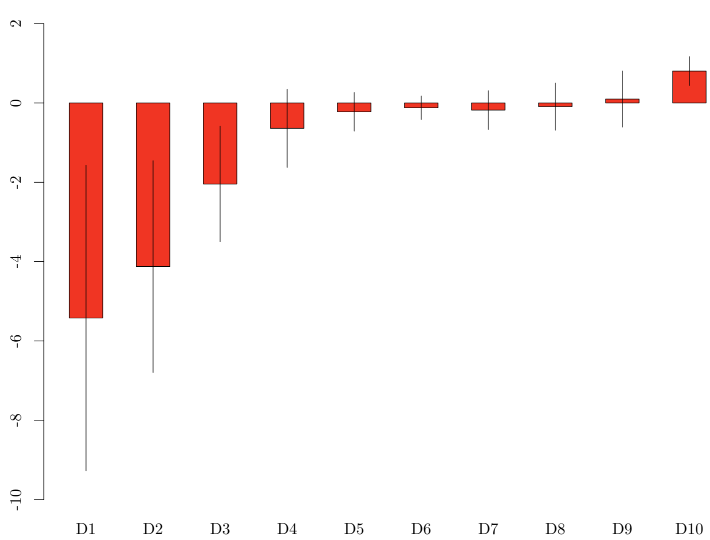
Note: The unit of interest is the *département*. The main source is the 1/12 microsample of the DADS. The dependent variable is the evolution (in log points) or quantiles of the average yearly wage distribution of the *département*. The main explanatory variable is the index ΔIPW, described herein above, with the corresponding instrumentation. The period of estimation is 2002-2008 and 2008-2018; the two decades are stacked with the addition of a time dummy. All specifications include the full vector of controls. Observations are weighted by the start-of-the-decade total employment. Standard errors are clustered at the level of the INSEE superzones. The main line denotes the main coefficients, with the corresponding 95% conf. interval. A dashed line plots $\hat{\beta}_1$ when the dependent is the rise in log points of the av. yearly wage within the *département*, i.e. respec. -6.27 (t=1.71), -6.19 (t=1.07) and -4.29 (t=1.54).

Figure 11: Distributional impact of the shock − Shares estimates

(a) Decadal growth of the shares of total local fiscal income held by each *within*-decile of *départements* (in pp)

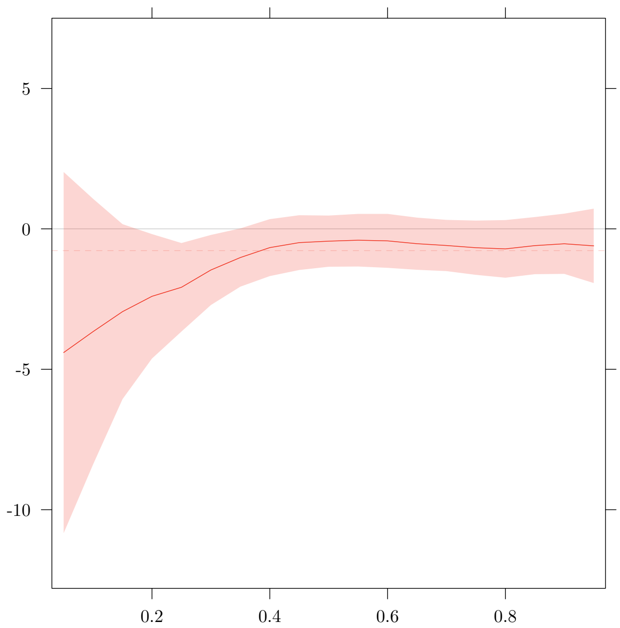


(b) Decadal growth of the share of total local fiscal income held by each *within*-decile of *communes* (in pp)

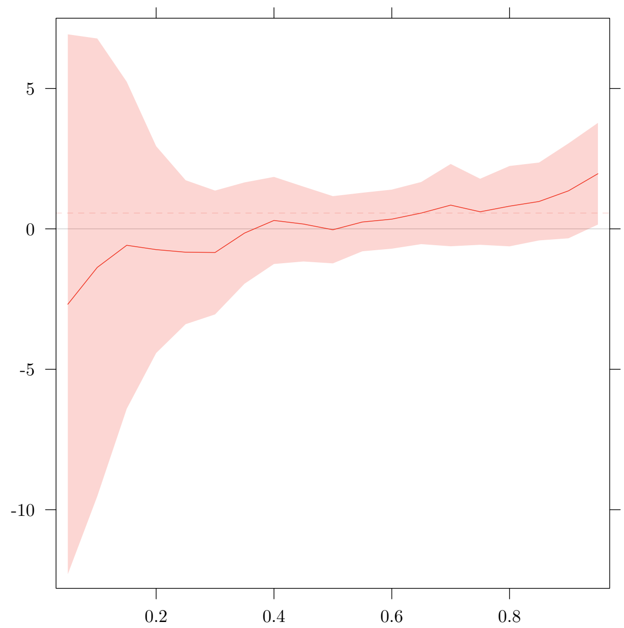


Note: The unit of interest is the *département* or the city-*commune*, the main source being the IRCOM database (restriction 0 for the latter case, restriction 2 for cities). The dependent variable is the evolution (in pp), for each within-region-decile of the fiscal income distribution, of its share of total regional income as a ratio of the initial share. The main explanatory variable is the index ΔIPW, described herein above, which provides an estimation of the mean rise in Chinese imports (in value) per worker within each *département*. The instrument is the same ΔIPW, in which French trade data has been replaced by a control group of four countries (Japan, Germany, Spain, Switzerland) and all labour force variables are taken with a decadal lag. The period of estimation is 2001-2018, i.e. two decades, and we estimate decade by decade (i.e. the ΔIPW of decade t is meant to have a causal impact on the income evolution of decade t only, not on the evolution in $t+1$). All specifications include the full vector of controls. Observations are weighted by the start-of-the-decade total population of the *département*. Standard errors are clustered at the level of the INSEE superzones. Bars denote the main coefficient, with the corresponding 95% conf. interval.

Figure 12: Isolating the role of offshoring in the fiscal income response



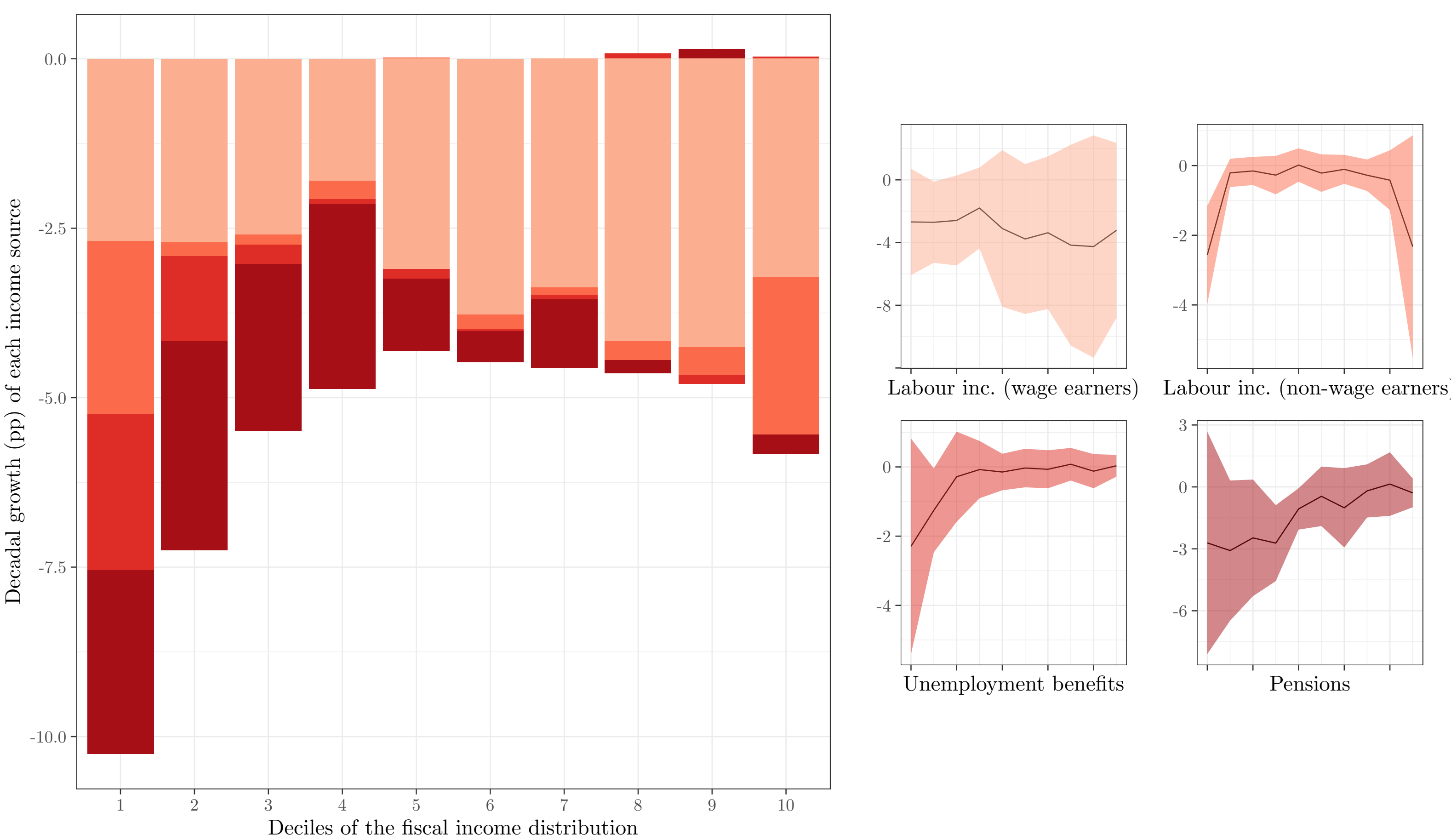
(a) Restriction to consumption goods



(b) Restriction to intermediate products

Note: These two figures are replicates of fig. 4a (the only difference being the removal of the offshoring index as a control variable), this time using as explanatory the exposure index ΔIPW described herein above and the corresponding instrument, but over a restriction to goods and products which are considered within the BEC-*Broad Economic Categories classification* as either intermediate or consumption goods. We use the concordance tables between the HS, STIC and BEC scales provided by the UN Statistics division: note that all products cannot be mapped from the latter to the former one.

Figure 13: Breakdown of the fiscal income impact of the shock



Note: The unit of interest is the ZE. The main source is the Filosofi database (2nd-breakdown datasets). The dependent variables are the decadal evolution (in percentage points) of the four exclusive main sources of fiscal (pre-redistribution) income − labour incomes of wage earners or of independent workers, unemployment benefits, and pensions − for each household weighted by its number of *unités de consommation*. The main explanatory variable is the index ΔIPW, described herein above, which provides an estimation of the mean rise in Chinese imports (in value) per worker within each *département*. The instrument is the same ΔIPW, in which French trade data has been replaced by a control group of four countries (Japan, Germany, Spain, Switzerland) and all labour force variables are taken with a decadal lag. The period of estimation is 2012-2017. All specifications include the full vector of controls. Observations are weighted by the start-of-the-decade total household population of the ZE. Standard errors are clustered at the level of the INSEE superzones. Lines denote the main coefficient, with the corresponding 95% conf. interval.

9 Annex B – Time analysis

Figure 14 plots the long-term fiscal income effect of the China shock (1999-2008 decade of exposure) for all households.

Figure 14: Long-term impact of the China shock (1999-2008 import exposure) on mean fiscal income within ZEs

Note: The unit of interest is the ZE. Income data are from [Fournel 2024]. We estimate model (3) using the full vector of controls, taking as explanatory the import competition exposure index $\Delta IPW_{1999-2008}$ instrumented in the way described herein above, and as dependent, the evolution (in pp) of the average fiscal income reported in the zone between 1998 and the year mentioned on the x-axis. Estimation is done for each year, weighting observations by the total population of the ZE, and clustering S.E. at the INSEE superzones level. We report 90% confidence intervals.

Up till now, we have been relying on a decade-by-decade approach, under the assumption that the exposure of one decade impacts the income evolution of the same decade. Yet fig. 14 suggests no sign of recovery in the longer term dynamics of fiscal income following an import shock. In a sense, fiscal income exhibits a reaction quite similar to the employment stock in the original Blanchard-Katz model, a pattern imputable, not much to purported local multiplicative effects through the income channel (since, as shown before, redistribution deadens much of the income impact of the shock), but rather to the rise in local dependence to social transfers. It's primarily that increased dependence to redistribution which encapsulates, once the shock is realised, much of the impact of the multiple distortion mechanisms documented by New Keynesian models: multiplicative effects across labour markets [Bartik 1991], sluggish outmigrations, and regional hysteresis.

Annex C– Alternative dependent and explanatory var.

Some of our results are replicated with: 1. An altern. dep. var: the evo. of the ratio of total manuf. employ. over the total 15-64 y.o. labour force of the ZE; 2. An altern. exp. var: ΔIPW, this time normalised, not by total employment, but by manufacturing employment (in this comput., ΔIPW is about 3 times larger).

Table 4: Exposure to import competition and evolution of wages

	Dep. : Decadal change in the average yearly wage (expressed in 2022 euros) within the département *(in pp)*					
	Period of estimation: 2002-2008 & 2008-2018					
	All *départements*			Restrictions		
	All wages	*Manufacturing sector*	*Non-manuf. sectors*	*Top 10 dép.*	*Middle 40 dép.*	*Bottom 50 dép.*
	(1)	(2)	(3)	(4)	(5)	(6)
Rise in imports from China per worker:						
+ Full vector of controls:	-0.579^{***}	-0.721^{***}	-0.498^{**}	-1.48^{***}	-0.248^{***}	-0.245^{**}
	(0.17)	(0.22)	(0.21)	(0.11)	(0.07)	(0.102)
R^2	0.88	0.44	0.91	0.75	0.98	0.95
F-stat	89.12^{***}	8.4^{***}	108.3^{***}	2.3^{*}	95.6^{***}	62.7^{***}
Obs.	192	192	192	20	74	96

Sign. thr. : *p<0.1; **p<0.05; ***p<0.01

Note: The unit of observation is the *département*. Dep. var. is the evo. of the av. y. wage, computed over the 1/12 subsample of the DADS. Main exp. var. is the index ΔIPW, described herein above, instrumented as described above. Two decades are stacked tog. with a time dummy, including the full vector of controls (sectoral ones excluded) plus the st.-of-dec. mean wage. Obs. are weight. by tot. pop. and SEs clust. at ZEAT level.

Table 5: Exposure to import competition and evolution of reliance on social transfers

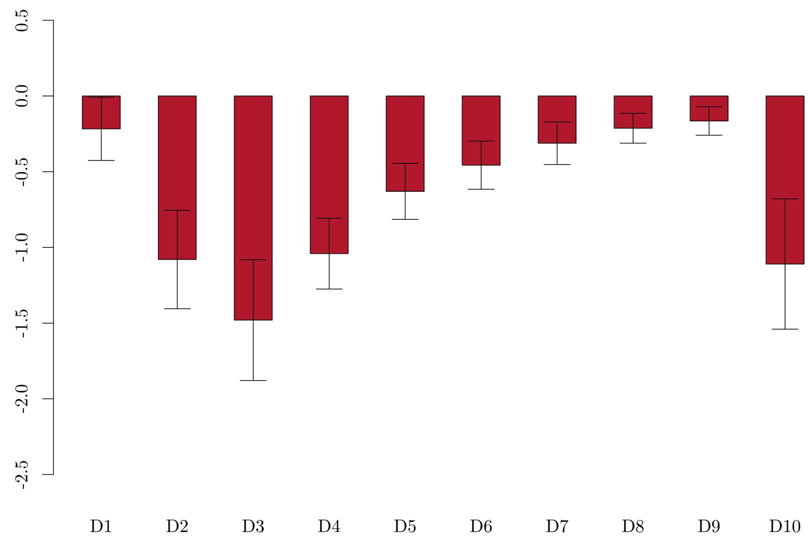
	Dep. : Decadal change in average share of social transfers in final income					
	Period of estimation: 2012-2019					
	All ZEs				*Restrictions*	
	All transfers	*Family allow.*	*Housing benefit*	*Minimum income*	*Top 10 ZEs*	*Bottom 50 ZEs*
	(1)	(2)	(3)	(4)	(5)	(6)
Rise in imports from China per worker:						
+Full vector of controls:	0.081^{**}	0.018	0.013^{**}	0.052^{**}	0.108^{***}	0.024
	(0.032)	(0.012)	(0.006)	(0.019)	(0.02)	(0.06)
R^2	0.68	0.58	0.56	0.78	0.86	0.81
F-stat	18.9^{***}	12.3^{***}	11.5^{***}	31.8^{***}	6.1^{***}	7.1^{***}
Obs.	304	304	304	304	31	152

Sign. thr. : *p<0.1; **p<0.05; ***p<0.01

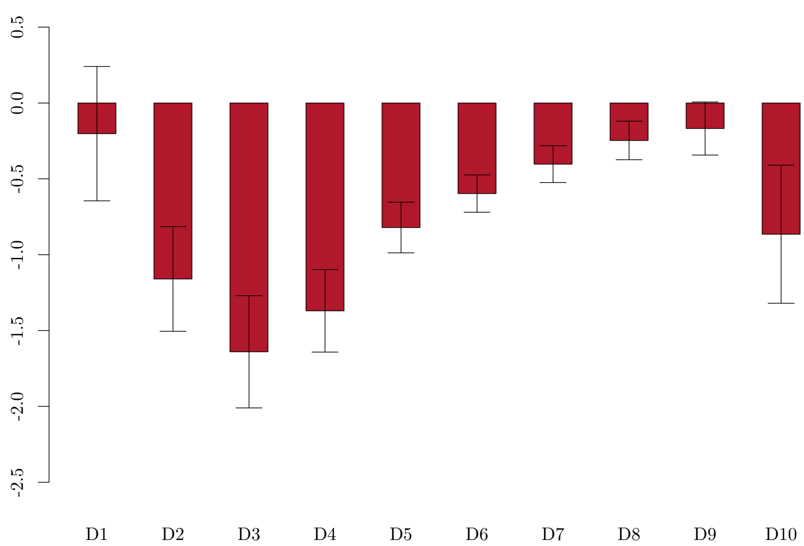
Note: The unit of observation is the ZE (*Zone d'emploi*, definition of 2010) or the *département*. The dependent variable is the average evolution (in pp) of the share of each type of transfers within the final disposable (after-redistribution) income of a tax unit within the ZE of interest, as reported in the Filosofi database of the INSEE. Exp. var. ΔIPW and corr. instr. have been described herinabove. All specifications include the full vector of controls (at the exception of offshorability and machine penetration indexes due to data limitation). Observations are weighted by the start-of-the-decade total population of the ZE. Standard errors are clustered at the level of the INSEE superzones.

Figure 15: Exposure to import competition and evolution of wages (*within*-inequality) − Predicted impact of a +$1000 rise in imports per head exposure within the *département*, on the average yearly wage of each quantile (decline in pp)

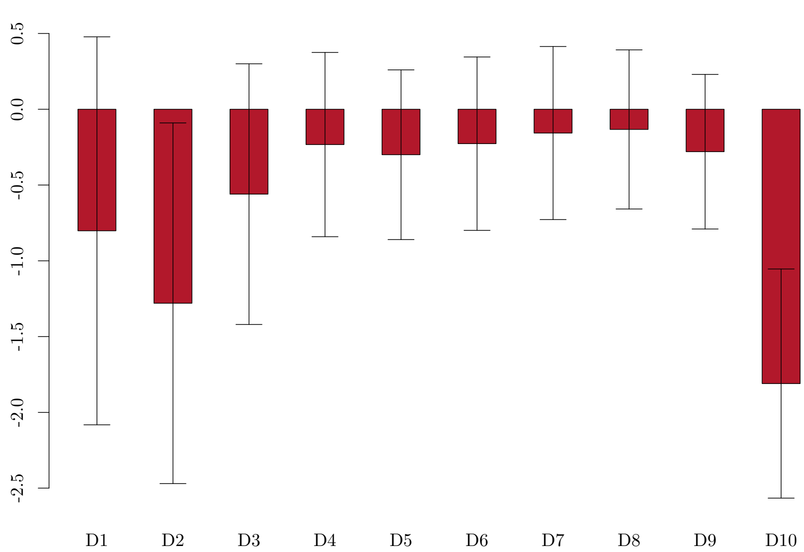
(a) All sectors



(b) Non-manufacturing



(c) Manufacturing



Note: The unit of interest is the *département*. The main source is the 1/12 microsample of the DADS. The dependent variable is the evolution (in pp) of the average yearly wage reported in the DADS, computed over each decile of the distribution of wages of each *département*. Exp. var. ΔIPW and corr. instr. have been described herinabove. All specifications include the full vector of controls (at the exception of offshorability and machine penetration indexes due to data limitation). Observations are weighted by the start-of-the-decade total population of the *département*. Standard errors are clustered at the level of the INSEE superzones. Bars denote the main coefficient, with the corresponding 95% conf. interval.

Table 6: Exposure to import competition and evolution of incomes (*between*-inequality)

	Dep. : Decadal change in average yearly fiscal income (expressed in 2022 euros) within the spatial unit of interest (in pp)								
	Period of estimation: 2000-2008 and 2008-2018								
	Fiscal income						... of which labour income		
	Département-level			*ZE*-level			*ZE*-level		
	All dép.	*Top 10 dép.*	*Bottom 50 dép.*	All ZEs	*Top 10 ZEs*	*Bottom 50 ZEs*	All ZEs	*Top 10 ZEs*	*Bottom 50 ZEs*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Rise in imports from China per worker:									
+ Full vector of controls:	-0.68^{***}	-0.71^{*}	-0.35^{*}	-0.94^{***}	-1.61^{**}	-0.24	-0.47^{***}	-0.78^{**}	-0.15
	(0.17)	(0.36)	(0.21)	(0.25)	(0.74)	(0.27)	(0.14)	(0.39)	(0.09)
R^2	0.82	0.95	0.93	0.48	0.86	0.49	0.54	0.86	0.38
F-stat	24.1^{***}	11.2^{***}	35.1^{***}	16.6^{***}	15.8^{***}	9.1^{***}	21.1^{***}	16.7^{***}	5.9^{***}
Obs.	192	20	96	608	62	304	608	62	304

Sign. thr. : *p<0.1; **p<0.05; ***p<0.01

Note: The unit of observation is the ZE (*Zone d'emploi*, definition of 2010) or the *département*. The dependent variable is the evolution (in percentage points) of the related average yearly income per tax unit within the ZE or dép. as reported in the IRCOM database. Data about all incomes are computed over restriction zero (i.e. all cities, excluding those with fewer than 11 inhabitants). Exp. var. ΔIPW and corr. instr. have been described herinabove. All specifications include the full vector of controls (at the exception of offshorability and machine penetration indexes due to data limitation). For restrictions, we rank *départements* and ZEs according to their mean wage at the beginning of the period of estimation (2000) and distinguish between the top decile versus the five lowest deciles. Observations are weighted by the start-of-the-decade total population of the ZE. Standard errors are clustered at the level of the INSEE superzones.

Table 7: Exposure to import competition and evolution of fiscal income (*within*-inequality)

	Dep. : Decadal change in fiscal income (in pp)				
	Evolution of fiscal income			*Ratio T10/B50*	
	All incomes	Incomes of decile 1 to 5	Incomes of decile 10	Start-of-the-period value	Evolution
	(1)	(2)	(3)	(4)	(5)
Panel A – Estimated at the level of the département *(2012-2019)*					
Rise in imports from China per worker:					
+Full vector of controls:	-0.541^{***}	-0.885^{***}	-0.0121	-0.21^{***}	0.026^{*}
	(0.146)	(0.25)	(0.24)	(0.05)	(0.015)
R^2	0.86	0.79	0.87	0.96	0.87
F-stat	14.5^{***}	8.9^{***}	14.6^{***}	67.2^{***}	15.2^{***}
Obs.	304	304	304	304	304
Panel B – Estimated at the level of the ZE *(2012-2017)*					
Rise in imports from China per worker:					
+Full vector of controls:	-0.658^{***}	-0.818^{**}	-0.501^{**}	-0.162^{*}	0.003
	(0.181)	(0.31)	(0.23)	(0.84)	(0.01)
R^2	0.49	0.79	0.87	0.79	0.68
F-stat	8.3^{***}	8.9^{***}	9.1^{***}	33.6^{***}	18.7^{***}
Obs.	304	304	304	304	304

Sign. thr. : *p<0.1; **p<0.05; ***p<0.01

Note: The unit of observation is the ZE (*Zone d'emploi*, definition of 2010) or the *département.* The dependent variable is the average evolution (in pp) of the share of each type of transfers within the final disposable (after-redistribution) income of a tax unit within the ZE of interest, as reported in the Filosofi database of the INSEE. The main explanatory variable is the index ΔIPW, described herein above, which provides an estimation of the mean rise in Chinese imports (in value) per worker of the industrial sector within each ZE. The instrument is the same ΔIPW, in which French trade data has been replaced by a control group of four countries (Japan, Germany, Spain, Switzerland) and all labour force variables are taken with a decadal lag. All specifications include the full vector of controls (at the exception of offshorability and machine penetration indexes due to data limitation). Observations are weighted by the start-of-the-decade total population of the ZE. Standard errors are clustered at the level of the INSEE superzones.