Chinese Competition and the European Industrial Shift*

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Abstract

Amid mounting alarms over a looming 'second China shock', I investigate the impact of Chinese imports on employment in the EU so far. Reviewing the evidence, I focus on the one methodology which predicts the worst, but is also the most challenged by recent contributions from econometricians: shift-share instrumentation. Applying these contributions to French trade and industrial data, I estimate that a marginal rise in Chinese import exposure is associated with a -4.02 pp decadal decline in total manufacturing employment. That effect is exclusive to final goods, robust across instrumentations and spatial econometrics' tests. We are thus able to explain at least 25% of the manufacturing employment decline over 1990-2018. The elasticity of non-manufacturing employment to the shock is estimated at 1.37, just below U.S. equivalent results. I also substantiate concerns about the increase in the technology-content of Chinese exports, pondering potential policy responses from the EU.

Keywords: international competition, import competing, trade shocks, manufacturing decline, manufacturing employment

JEL Codes: F14, F16, F66, L60, R12, R23

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1 Introduction

When imports from emerging markets to the North really took up in the late 1990s, the empirical literature was still struggling to identify the massive employment effects predicted by the Heckscher–Ohlin factor proportions model [Heckscher 1919; Ohlin 1933], especially on non-technology-intensive industries which, according to the HOS framework, should have suffered the most from this new supply shock [Wood 2018].

Empirical evidence seemed to run against predictions of a HOS framework on the issue of differentiation; emerging markets were not using *between*-specializing on certain goods or services, but rather focusing on *within*-differentiation at a lower level of the quality ladder [Schott 2004]; it was argued at that time that the European focus on high-value-added-high-quality production provided strong protections against that emerging competition [Fontagné, Gaulier, and Zignago 2008].

Very early on, however, trade economists were warning policymakers about the specificity of China [Schott 2008], the fact that its exports were gradually becoming more technology-intensive, and its trade connection with the West less and less consistent with a classical HOS North-South setting [Rodrik 2006]. We should almost apply to this connection the Chamberlain-differentiation-based models in the vein of [Melitz 2003] and expect a decisive impact of the exit rate of firms with low productivity, the onus of the shock being concentrated on industrial and service firms of the West highly intensive in low-skill labour [Burstein and Vogel 2017].

The most popular identification strategies in the late 1990s were the factor-content approach to trade [Borjas, Friedman, and Katz 1997; Rodrik 1997] and the study of the relative evolution of product prices [Slaughter 1998]. These approaches failed to ascribe to trade with the South a sizeable impact on the employment rate or wage dynamics of low-skilled workers in the North; this remained true even with late replications of these strategies [Bivens 2007; Edwards and Lawrence 2010]. Besides, these approaches were criticized from a purely methodological standpoints, and replications were hazardous; to take the example of France, depending on the parameters chosen, the number of industrial jobs destroyed by competition from emerging markets, estimated in factor-content based strategy, could vary by a factor 1 to 4 [Aubert and Sillard 2005; Demmou 2010]. Some approaches relying on individual panel data retrieved relatively higher results [Saeger 1997].

The credibility revolution of the 2010s [Angrist and Pischke 2010] shifted the focus of the literature towards the issue of endogeneity:

- Part of the literature exploited specific trade events and exogeneous shocks to reestimate the impact of trade openness. Well studied episodes include trade liberalisations in emerging economies like Brazil [Kovak 2013], the fall of the USSR, the ensuing abnormally high trade surpluses of Russia and their inequality background [Novokmet, Piketty, and Zucman 2018], paralleled by a rise of competition from Eastern countries in Europe [Dauth, Findeisen, and Suedekum 2021]; more recently, the end of the Multi-fiber Agreement [Lopez-Acevedo and Robertson 2012], unexpected currency events like the appreciation of the Swiss Franc following the end of the Euro peg in 2015 [Kaufmann and Renkin 2018] or the fall of the British pound the day of the Brexit referendum [Costa, Dhingra, and Machin 2019], the protectionist policy of the Trump administration [Fajgelbaum et al. 2019], and even retrospective papers on key events of trade history, for instance to gauge the effect of the NAFTA [Hakobyan and McLaren 2016], of 19th c. colonial exclusive zones [Cogneau, Dupraz, and Mesplé-Somps 2018, or even of the *Zollverein* [Wolf 2009];

- Firm-level strategies exploiting micro data have focused on identifying the increasing return impact of openness [Bloom, Draca, and Van Reenen 2015; Mion and Zhu 2013; De Lyon and Pessoa 2021];

- A vein of this literature has exploited space variability in exposure to trade competition; [Margalit 2011] and [Topalova 2010] are the most well-known precursors, but the contribution which has stemmed the richest vein of research is [Autor, Dorn, and Hanson 2013], with responses ranging from empirical replications, structural models meant to support [Caliendo, Dvorkin, and Parro 2015; Galle, Rodríguez-Clare, and Yi 2022] or question [Borusyak and Jaravel 2021] their results, plus a vast range of econometrical publications gauging the reliability of their setting [Adão, Kolesár, and Morales 2019; Goldsmith-Pinkham, Sorkin, and Swift 2020; Borusyak, Hull, and Jaravel 2021].

Actually, that last vein of research is growing rapidly, but seems to show scant regard for the repeated warning of econometricians about the extreme sensibility of results to parameters of this type of space-based settings. In this paper, I focus on the French case, where employment figures and distributional income data are available at extremely thin spatial levels.

2 Empirical framework

2.1 Measure of import exposure

In shift-share settings applied to trade, two types of data are combined: 1. Trade data, classically from the U.N. *Comtrade* base, which provides a detailed breakdown of exports and imports between countries by subtypes of exchanged products; 2. Regional data about the sectoral employment structure of each commuting zone. From this, it is straightforward to build an index of exposure to import competition for each region. Denoting zones with an i and industrial sectors with a j, that main index is the change in import exposure per worker of a zone over a period $t, t+1$, denoted $\Delta IPW_{it,t+1}$. In the setting of [Autor, Dorn, and Hanson 2013], the exposure index is computed for each sector, taking the imports from the trade partner over the time period, ΔM, times the share of region i in the total national workforce of sector j ($L_{ijt}/\sum_i L_{ijt}$) at time t (the beginning of the time period of interest), the grand total being normalised by the total number of workers in region i. Summing across sectors yields the individual loss per worker caused by imports over the period:

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

2.2 Limits and counfounding factors

A specification using ΔIPW as the main explanatory variable, and some measure of industrial decline as the dependent, implies several limitations: 1. Such a reduced-form framework focuses exclusively on one channel connecting a trade shock to employment dynamics, i.e., on the impact of competition from foreign products within the home market, ignoring home exports to the foreign partner, and competition from the foreign partner on other foreign markets that the home firms serve; 2. That specification relies on the strong assumption that there is zero mobility of labour between regions within the home country; i.e., that marginal effects that will be estimated will encapsulate employment decline, not outmigration, Autor, Dorn and Hanson arguing that even in U.S. context, it is a relatively realistic assumption, especially when it comes to blue-collar workers, which are known to be less mobile [Wozniak 2010]; 3. The reduced-form approach assumes a unique direct effect of exposure on job losses. However, in our setting, there is evidence of important retroactive impacts through wage decline [Fournel 2023a], and of spatial spillovers between sub-regions.

More important still are endogeneity issues. Since the explanatory variable essentially captures the local industrial structure, we rely on the assumption that this structure has been predetermined long before trade began with the foreign partner, and that the employment decline isolated is caused by exposure only, and does not encapsulate some region-specific or sector-specific employment shock.

The usual way out is a shift-share instrument strategy, which consists in rebuilding the main explanatory using trade data for a pool of advanced economies similar to the home country, under the assumption that there is no correlation between countries in import demand shocks.

2.3 Instrumentation strategy

In the Autor-Dorn-Hanson framework, these Bartik instruments are built like the ΔIPW index, with two slight differences: 1. Import figures ΔM are replaced with $\Delta\overline{M}$, the imports from the partner to a control group of advanced economies ; 2. The start-of-the-period labour force L is taken with a lag of one period (here, a decade). The authors use this lag to counter the simultaneity bias. Each instrument writes:

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

There is little questioning about the relevance of these instruments: in [Autor, Dorn, and Hanson 2013] and there alike (see our tables 1 and 3), the first stage yields very satisfying results. Using an alternative control group (see table 6) does not change the general picture.

On the contrary, the proper proof of the orthogonality of shift-share instruments is a contested issue, with a growing body of formal literature, some items of which are directly focused on the framework we'll be using, most importantly [Goldsmith-Pinkham, Sorkin, and Swift 2020], [Adão, Kolesár, and Morales 2019] and more recently [Borusyak, Hull, and Jaravel 2021]. Like most of the existing literature, we'll be clustering our standard errors at a meta-regional level, here, at the level of the INSEE's superzones or ZEAT (*Zones d'études et d'aménagement du territoire*); this should discard part of the risks at identifying a merely regional shock[1]. As to the risk involving sectoral shocks, we'll conduct below several robustness checks in the spirit of [Goldsmith-Pinkham, Sorkin, and Swift 2020] and [Adão, Kolesár, and Morales 2019].

2.4 Main specification

The main 2SLS specification is a very straightforward framework, in which 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:

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

$\Delta IPW_{it,t+1}$ being instrumented by $\Delta \overline{IPW}_{it,t+1}$ as described above. It is not a first-difference model: the multiple decades are simply stacked with a specific time dummy.

2.5 Data

Applying this setting to French data is not straightforward, especially when it comes to combining Comtrade data with the sectorial classifications, all the more since we rely, for employment variables, not on social security declarations (DADS) but on the INSEE's Census. The main comparative features are the following:

- The period of estimation extends to three decades: 1990-1999, 1999-2008 and 2008-2018. Most of the U.S. literature uses as unit of interest the commuting zone (CZ), which has on average 400k inhabitants; in our setting, the main unit is the INSEE's *Zone d'emploi* (ZE, 2010 definition), with an average of 200k inhabitants, and sometimes the *département* (average of 600k inhabitants);

- Our instrument $\Delta \overline{IPW}_{it,t+1}$ is built on two non-EU economies (Japan and Switzerland) and two EU countries (Germany and Spain). We'll see in our robustness checks that using an alternative instrument based on non-EU countries only does not change the main results. Descriptively, over 1999-2008, exports from China to France have expanded a bit slower than exports to our instrumental group; once quantities are re-scaled to France's population, we get a +36.1 billion USD in imports to France versus +47.1 for the instrumental group. French imports are relatively inferior to the control group's trend for steelworks (+2.2 vs. +3.9) and computers (+11.1 vs +15.6), slightly superior for textiles (+8.4 vs +7.9).

- Employment figures are drawn from the *Recensement* of the INSEE. Prior to 2008, the Census relied on a classification of sectors known as NES-AE, which was specific to France, but closely connected to the United Nations' ISIC-rev.3 categorization. The 1999 Census provides, for each *commune*, the employment structure classified within successively 4, 36, 60 and 114 subsectors. The 1975, 1982 and 1990 issues provide a prior version of the 36 breakdown ; the 2006 & 2007 issues provide the 36 breakdown in its definitive version. The 2008 issue introduced the INSEE's new system, the NAF-rev.2, parallel to the ISIC-rev.4 and NACE classifications; from 2008 onward, the Census provides a breakdown in 38 activities, 16 of which belong to the industrial sector. We apply conversions between the rev.3 and 4 of the ISIC according to the concordance tables of the OECD [OECD 2017]. As to trade data, they are drawn from the UN Comtrade database; these are imports in value (in 2022 USD), classified using the 6-digit HS system. We rely then on concordance tables between HS and ISIC classifications provided by the WITS software of the World Bank.

2.6 Summary statistics and overview

The most interesting aspect of the whole picture is how quickly the structure of imports from China is shifting from light industry products to more technology-intensive goods: over 1999-2008, imports of computers and electrical

[1] Clustering at an inferior level, like the pre-2015 *régions*, does not change the general results.

devices grew 1.3 times faster than textile imports in value; over 2008-2018, it was 2.9 times faster, the shift being even more pronounced in the control group. The main drivers are, on the one hand, a sharp decline of textile imports from China after the Great Recession (partly because suppliers have offshored production units to more competitive countries), and massive hikes in imports of electronic devices. The average ΔIPW over the whole period 1990-2018 is slightly superior to 3,000 dollars per worker. In the U.S. framework, the median ΔIPW was 1.25 thousand USD per head over 1990-2000 and 2.9 over 2000-2007.

Overall, the most protected ZEs are mostly touristic resorts of the West and South (+146 in Corte in 1999-2008), regions with genuine product differentiation like wine valleys (+755 in Jonzac over 1999-2008), and the West, thanks to its specific mix of primary sector + STEM (566 over the whole period for Carhaix). On the other hand, the general portrait of the exposed regions tends to evolve over time. Consistent with our picture of Chinese exports swiftly transitioning to heavy industries, over 2008-2018, we see regions with a highly technology-intensive industrial base enter the top rankings of the most exposed zone (for instance, +1616 in Grenoble, +1283 in Saclay). Conversely, because of the decline of textile imports, many textile regions tend to transition from the top to the bottom of the rankings between the two periods. The rankings are but barely altered when we build our lagged instrument; for the first lagged decade (1982-1990), one of the main differences is the very high exposure of regions in which extractive industries had an historical role (especially among Northern or Northeastern mining or steelworks bastions).

3 Results

3.1 A consistent negative impact of import exposure on manufacturing employment

From now on, and for much of the remainder, the trade partner of interest will be France's main developing partner, China. Some results will be reported for Germany (see table 6), for Turkey and Poland; but as we'll see, beyond the industrial dimension, the massive economic and social impact of the China shock that a shift-share framework can identify are extremely difficult to replicate for alternative partners [Fournel 2023b].

The decade-per-decade estimation of model (3) for China is displayed in table 1. Unsurprisingly, the bulk of the depressing effect is concentrated in the second decade, the one characterised by the sharpest rise in imports from China.

3.2 Controlling for alternative theories of industrial decline (skill-biased technological change, offshoring, automation)

Aside of the usual regional controls about gender, ethnicity, education, insecure jobs[2] and the share of manufacturing within the total workforce, we'll be using throughout a set of three supplementary control variables meant to capture the great alternative explanations of industrial job decline ventured in the literature of the last three decades:

- *Automation and the rise of robots* – We replicate the methodology of one of the most classical articles on the issue of automation : [Acemoglu and Restrepo 2020]. The intuition is to use data about the stock of newly-bought robots within each sector in each country, from which an index of average penetration of robots in sector j is derived:

$$APR_{j,t_0,t_1} = \frac{R_{j,t_1} - Rj,t_0}{L_{j,1990}} - g_{j,t_0,t_1}\frac{Rj,t_0}{L_{j,1990}} \tag{4}$$

  R being the stock of robots in industry j at time t, L total employment, and g the growth rate of the sector over the time interval. The average APR of a commuting zone i is then computed as:

$$\text{Exposure to automation}_{i,t_0,t_1} = \sum_j l_{j,1990} \times APR_{j,t_0,t_1} \tag{5}$$

  Where l is the share of each sector in the total workforce[3].

[2] Namely, the share of internships, State-sponsored jobs, short-term *CDD* contracts, and contingent work within total employment, as reported in the INSEE's Census.

[3] The original dataset used by [Acemoglu and Restrepo 2020] for the computation of APRs, the annual report of the International Federation of Robotics, is not available to the public; we are therefore forced to use the data provided by D. Acemoglu on his personal website; he computes sector-by-sector APRs over a set of five European countries used as a control group for the U.S., namely Denmark, Finland, France, Italy and Sweden. The only option we are left with is to use these APRs and to map them to the sectoral structure of

Table 1: Exposure to import competition from China and change in manufacturing employment at the ZE level (I)

	Dep. : Decadal change in total manufacturing employ. (in pp)					
Type	OLS			2SLS		
	1990-1999	1999-2008	2008-2018	1990-1999	1999-2008	2008-2018
	(1)	(2)	(3)	(4)	(5)	(6)
Rise in imports from China per worker	-2.99	-5.62^{***}	-2.79	-3.36^{**}	-6.28^{***}	-3.27
over the decade (in 2022 kUSD)	(1.85)	(1.37)	(2.05)	(1.7)	(1.34)	(2.59)
R^2	0.09	0.22	0.02	0.09	0.21	0.02
F-stat	31.1^{***}	81.1^{***}	3.6^{*}	15.8^{***}	79.3^{***}	3.5^{*}
First stage: Instrumenting by the rise				0.41^{***}	0.93^{***}	0.58^{***}
in imports to a group of control countries				(0.06)	(0.09)	(0.05)
R^2				0.41	0.79	0.71
F-stat				203^{***}	1117^{***}	738^{***}
Obs.	304	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). The dependent variable is the change (in pp) of total manufacturing employment within the ZE. The main explanatory variable is the index ΔIPW, described herein above, which provides an estimation of the mean rise in Chinese imports 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. No other control is applied. Observations are weighted by the start-of-the-decade total Census population of the ZE. Standard errors are clustered at the level of the INSEE superzones.

- *Skill-biased technological change* – If, as we'll see, the seminal intuition of [Acemoglu 2003] about trade acting as a buffer of technological-change driven job polarisation has been substantiated many times since, even very recently on French employer-employee matched datasets [Harrigan, Reshef, and Toubal 2020], the major earliest theorists of skill-biased technological change and polarisation tended to play down the role of trade competition, imputing much of the decline of industrial employment to the *routinisation* hypothesis, with the idea that, because of accelerated progresses in STEM sectors, jobs markets of the North had produced extra demand for either creative and innovative jobs on the one hand, or for social interaction jobs (*care* workers, delivery jobs) on the other, hence the decline of routine jobs like the intermediate office tasks or mechanised activities of the industry. One way to test that hypothesis is to include an index for routine jobs, as defined by [Autor, Levy, and Murnane 2003] and [Autor, Katz, and Kearney 2006][4];

- *Offshoring* – Some jobs are lost because of direct trade competition from foreign firms, but some other jobs are lost through offshoring from home firms. The most natural empirical strategy to implement this distinction is to isolate within trade data the intermediate products, as opposed to final, consumption goods. A more straightforward approach relies on a specifically-built *offshorability* index of local jobs, taken from [Firpo, Fortin, and Lemieux 2011]. Those jobs are not easily offshorable that require face-to-face contact and direct on-site monitoring[5]. There's almost no correlation between the skill and routine dimension of a job

manufacturing employment in each ZE in the INSEE's Census. Note that the original data from the IFR is not detailed at all, giving stocks of robots for 13 industrial sectors only.

[4] Autor and his coauthors have recourse to the US Department of Labor's *Dictionary of Occupational Titles*, defining routine jobs as those where repetitive tasks (cognitive or manual) are highly frequent. It is then possible to build a share of routine jobs within each industrial sector. Here, we rely on the averages by great industrial sector found in the replication provided on Daron Acemoglu's personal web-page for his article [Acemoglu and Restrepo 2020], mapping them to the industrial structure of each ZE in the INSEE's Census.

[5] We construct this index using the methodology described by [Autor and Dorn 2013]; they had recourse to the widely used O*Net base of the U.S. Dep. of Labor to build a standardised *offshorability* index of sectors. An activity is said to be offshorable when : 1. It does not require face-to-face contact; 2. It does not require on-site work. Following [Firpo, Fortin, and Lemieux 2011], they define

and its offshorability: a highly qualified activity like coding softwares ranks among the most offshorable ones (because it does not require face-to-face contact or geographic proximity); conversely, nurses and first-aid workers are among the most protected;

Table 2: Raw correlation, at the employment zone (ZE) level, between key indexes (weighted by 1990 total pop.)

Exposure to imports from China	*1990-1999*	1.00							
à la [Autor, Dorn, and Hanson 2013]	*1999-2008*	0.44	1.00						
Share of routine jobs	*1990*	−0.03	0.01	1.00					
à la [Autor, Katz, and Kearney 2006]	*1999*	−0.03	0.01	0.86	1.00				
Offshorability of manuf. jobs	*1990*	0.16	0.25	−0.04	−0.04	1.00			
à la [Firpo, Fortin, and Lemieux 2011]	*1999*	0.23	0.33	−0.06	−0.07	0.06	1.00		
Expansion of automation	*1990-1999*	−0.15	−0.07	0.48	0.39	−0.09	−0.03	1.00	
à la [Acemoglu and Restrepo 2020]	*1999-2008*	−0.11	−0.05	0.57	0.49	−0.17	−0.05	0.51	1.00

These three indexes are relatively uncorrelated between one another. Over U.S. and French data alike, automation is slightly negatively connected with trade exposure and offshorability (compare table A3 in [Acemoglu and Restrepo 2020] and our table 2); textiles, typically, is a sector which has experienced high trade competition but limited automation. Conversely, at the ZE level, we find a positive correlation between routine jobs and automation.

In simple descriptive statistics, the variables which are most correlated with the decline of industrial employment at the ZE level evolve over time. Over 1990-1999, we find a slight negative correlation with the share of routine jobs and the penetration of robots; over 1999-2008 on the contrary, we find a strong correlation with offshorability and exposure to China trade. It is relatively consistent with our general picture of China imports being more and more skill & technology intensive as we move to more recent data[6].

If however we stack periods to ensure comparability with the existing literature, we find that exposure to China trade remains the key explanatory variable:

In our preferred specification, i.e. column 6 of table 3, a $1000 rise in import exposure per worker within the ZE is associated within a decline of total manufacturing employment by 4.02 percentage points[7]. This is almost similar to the marginal impacts reported in table 5 of [Autor, Dorn, and Hanson 2013]. Compared to existing French estimates, it is but very slightly inferior to the effects reported in the regional strategy of [Malgouyres 2017]. As far as comparison is possible, industry-based strategies find impacts which are half this magnitude [Aghion et al. 2021] while firm-level estimates are of the order of ten times lower [Biscourp and Kramarz 2007; Aghion et al.

face-to-face activities as the simple average of O*NET variables face-to-face discussions, establishing and maintaining interpersonal relationships, assisting and caring for others, performing for or working directly with the public, and coaching and developing others. On-site activities is similarly defined as the simple average of the variables inspecting equipment, structures, or material, handling and moving objects, operating vehicles, mechanised devices, or equipment, and the mean of repairing and maintaining mechanical equipment and repairing and maintaining electronic equipment. The next step consists in mapping each occupation of the SOC classification to the equivalent categories of the Census to get an index of the offshorability of local manufacturing employment for each zone. In our setting, we use concordance tables of the ILO to shift from the SOC to the international ISCO classification of the ILO. We then have recourse to the *Enquête emploi* (aggregated 1990-2002 issue) which provides, for a very large sample of 2.5 million workers, the ISCO code of their occupation, their industrial sector and their SES (the INSEE's PCS). Each worker is ascribed the offshorability index of its ISCO occupation; weighted averages are then computed along the INSEE's sectoral classifications and the INSEE's PCS scale; mapping to the Census values is then straightforward.

[6] We must heed to the fact that these differential correlations are also dependent on : 1. The quality of available data; 2. The choices made in the construction of each index. 1. Overall, our indexes of offshorability and of trade exposure are much more precise than those derived from D. Acemoglu's data. The original data of the IFR provides but a very coarse decompozition, with 13 subsectors, compared with the 53 and 648 subsectors used to build the offshorability index over 1990-1999 and 1999-2008 respectively; 2. We rely on indexes which are widely used in the literature, but some of them might be criticised; the offshorability index of [Firpo, Fortin, and Lemieux 2011] for instance is highly efficient in isolating jobs which are indeed protected against offshoring (nurses, doctors), but it yields some surprising results when it comes to endangered jobs. Because of the prevalence of on-site work, many industrial jobs get an artificially low level of offshorability, while many STEM jobs get an artificially high level. This might explain the low-correlation with the decline of industrial employment over 1990-1999 and the higher one over 1999-2008.

[7] If we focus on the first two decades (see for instance table), or on the second only (see for instance figure 4), we find marginal impacts which are systematically superior.

Table 3: Exposure to imports from China and change in manufacturing employment at the ZE level (II)

	Dep. : Decadal change in total manufacturing employ. (in pp)					
	1990-2018					
	(1)	(2)	(3)	(4)	(5)	(6)
Rise in imports from China per worker	-6.27^{***}	-6.47^{***}	-4.22^{***}	-4.35^{***}	-3.38^{*}	-4.02^{***}
over the decade (in 2022 kUSD)	(1.32)	(1.21)	(1.48)	(1.16)	(1.96)	(1.33)
Extra controls:						
Share of employ. in manufacturing		0.04	-0.04	-0.23	-0.02	-0.15
		(0.14)	(0.08)	(0.18)	(0.16)	(0.22)
Share of women in lab. force				-0.91^{**}		-0.77^{*}
				(0.38)		(0.35)
Share of foreign-born in pop.				-1.05^{***}		-1.09^{***}
				(0.25)		(0.24)
Share of higher educ. in pop.				0.27		0.31
				(0.21)		(0.19)
Share of insecure jobs				-0.57^{**}		-0.66^{**}
				(0.21)		(0.23)
Share of routine jobs					-0.11	-1.09^{*}
					(0.75)	(0.59)
Offshorability of manuf. jobs					-1.99^{*}	-1.36^{**}
					(1.13)	(0.51)
Penetration of robots					-0.58	-0.38
					(1.2)	(1.36)
Regional dummies			X	X	X	X
R^2	0.28	0.28	0.49	0.57	0.51	0.58
F-stat	115^{***}	86.6^{***}	32.1^{***}	37.9^{***}	30.1^{***}	35.7^{***}
First stage: Instrumenting by the rise	0.83^{***}	0.81^{***}	0.8^{***}	0.81^{***}	0.76^{***}	0.77^{***}
in imports to a group of control countries	(0.07)	(0.07)	(0.08)	(0.09)	(0.07)	(0.07)
R^2	0.89	0.9	0.89	0.9	0.91	0.91
F-stat	2565^{***}	1949^{***}	302^{***}	263^{***}	299^{***}	266^{***}
Obs.	912	912	912	912	912	912

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 change (in pp) of total manufacturing employment within the ZE. 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 two decades (1990-1990 and 1999-2008) and include a time dummy for the second decade. Observations are weighted by the start-of-the-decade total population of the ZE. Regional dummies denote the pre-2015 reform French *régions*. Standard errors are clustered at the level of the INSEE superzones.

2021] and in some specifications nonsignificant [Harrigan, Reshef, and Toubal 2020]. Equivalent European regional strategies provide estimates slightly superior to ours [De Lyon and Pessoa 2021; Dauth, Findeisen, and Suedekum 2021]. Note that among our special control variables, the only one on which we find a consistent negative impact is the offshorability index, a results consistent with European estimates of the impact of offshoring on manufacturing employment [Biscourp and Kramarz 2007; Mion and Zhu 2013; Hummels et al. 2014] and with our intermediate goods estimates (see below).

3.3 A third of manufacturing job losses could be explained by trade competition

To provide an intuition of these results, table 4 compares them to the Autor-Dorn-Hanson framework of 2013 (which we will abbreviate ADH from now on). Exposure of French industries came later, but was slightly more pronounced. As a result, the predicted industrial decline to Chinese competition over the two decades is very similar. An important difference lies in the fact that, though the Hausman tests still lead us to use a 2SLS specification in all contexts, our 2SLS and OLS estimates are not as divergent as the ones of ADH; in the variance breakdown exercise (detailed in the corresponding annex) meant to isolate the supply-driven from the demand-driven dimension of the China shock, we ascribe a very tiny 7% share of the shock to demand-driven factors, meaning that our final estimate of the share of manufacturing employment destroyed by import competition from China is barely altered (from 34 to 32%) and overall superior to the U.S. figures.

Table 4: Comparing results of table 3 with those of [Autor, Dorn, and Hanson 2013]

	US Data	French Data
Main coefficient (marginal impact of exposure)	-4.23^{***}	-4.02^{***}
(tab. 5-col. 1 in origin. art.; here tab. 3-col. 6)	(1.05)	(1.33)
Av. rise in exposure to China trade per worker		
First decade	+\$1,140	+\$281
Second decade	+\$1,839	+\$2,029
Third decade		+\$711
Total	+\$2,979	+\$3,021
Implied growth of manuf. empl. (1990-2008)	−12.6	
Actual growth of manuf. empl. (1990-2008)	−25.61	
Implied growth of manuf. empl. (1990-2018)		−12.44
Actual growth of manuf. empl. (1990-2018)		−36.33
Percentage explained (raw)	49%	34%
Percentage explained (supply-driven shock only)	24%	32%

3.4 Labour force impact: Lingering unemployment, multiplicative effects in non-manufacturing

The depiction of the population impact of this industrial decline in [Autor, Dorn, and Hanson 2013], and in companion papers (most notably [Autor, Dorn, and Hanson 2016] and [Autor, Dorn, Hanson, and Song 2014]) hinges round the argument that the rise of employment in the service sector is not sufficient to offset the decline of local manufacturing jobs, that outmigrations are far too sluggish compared to the usual predictions of matching models, and that, as a result, the bulk of the adjustment is borne by unemployment and early retirement. Here, we'll be using the *Mobilité* database of the INSEE to check for migration patterns (i.e., we focus on persons having moved between 1990 and 1999, and 2001 and 2006 resp.).

The main differences with the U.S. context might be summarised as such:

- *More exposed regions experience in-migrations* – It is a common feature of ADH and of its European equivalents, notably [De Lyon and Pessoa 2021], that an import exposure shock bolsters sectoral, but not regional mobility, i.e. workers adjusts to the shock by moving more frequently from one firm to another, but they rarely leave their home region. Here, we find a significant positive impact on the local population of exposed

Table 5: Exposure to imports from China and evolution of occupations within each ZE

Dep. : Decadal change in population log counts or shares of total adult population (1990-2008)

	Population evolution			Adult population breakdown				
	Total change	*Natural increase*	*Migration increase*	Working (manuf.)	Working (tert.)	Unempl.	Retired	Other inactivity
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Change in log counts								
Rise in imports from China per worker:								
No controls:	2.19^{***}	-0.01	2.19^{***}					
	(0.56)	(0.09)	(0.55)					
Full vector of controls:	3.04^{***}	0.31	2.73^{***}					
	(0.62)	(0.22)	(0.71)					
Panel B. Change in shares of adult pop.								
Rise in imports from China per worker:								
No controls:				-0.14^{***}	-0.24^{**}	0.12^{*}	0.14	0.22
				(0.03)	(0.12)	(0.07)	(0.19)	(0.22)
Full vector of controls:				-0.22^{***}	-0.14^{**}	0.13^{**}	-0.02	0.38
				(0.05)	(0.07)	(0.05)	(0.17)	(0.25)
Obs.	608	608	608	608	608	608	608	608

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). We estimate model 3 with and without the full vector of controls mentioned in table 3, using as dependent variable a set of changes in population, expressed either in log counts, or in shares in adult (15 y.o. or more) population; data for these dependent variables are drawn from the *Mobilités* datasets 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. We stack two decades (1990-1990 and 1999-2008) and include a time dummy for the second decade. 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.

ZEs, even once controlled for a rich set of variables, and a positive impact which is driven primarily by in-migrations. Consistent with this figure, we also find that a marginal shock is associated with -0.22pp decline in the share of vacant accommodation ($t = 1.68$). This result is less surprising if we heed to the fact that more exposed regions are not traditional heavy-industry bastions: as we'll see, when the China shock materialised in the mid-1990s, the old manufacturing industries predominant in North-Eastern ZEs had been declining for decades; most manufacturing jobs had still been destroyed; in the *Nord* region, even for the first decade 1990-1999, Lille and Dunkerque are the only ZEs which are above the national average of the $\Delta IPW_{1990,1999}$ index; former steelworks or mining centres (Lens, Hénin, Bruay) are all far below. At the nation's level, more exposed ZEs are far from being the deprived and declining onesIn U.S. econometrical calibrations like [Caliendo, Dvorkin, and Parro 2015], California and New York are among the U.S. States with the highest ΔIPW.. All in all, these marginal effects on in-migrations might encapsulate two types of reaction: 1. As we move on in time, Chinese exports become more technology-intensive, and ZEs like Saclay or Grenoble, known for their high shares of STEM jobs, enter the top tear of the exposure rankings. These are regions which, even if they are hurt at the margin, will benefit from trade exposure in many other ways, making them attractive to young workers (the average person which migrates to a ZE with a $\Delta IPW_{1999,2008}$ above national average is slightly younger and more educated than the national population); 2. Another type of mechanism, involving less attractive ZEs, is the default migration described by [Taffin and Debrand 2005; Vignal 2005; Davezies 2012], i.e., faced with an employment shock in their own district, low-income families, highly constrained on the housing market, are unable to move to the fastest-growing areas, and opt for a neighbouring region where the impact of local employment shocks has been less pronounced; descriptively, people who move to the 152 most exposed zones over 1990-2008 are more likely to come from the same region (63%) that migrants moving to the 152 least exposed ZEs (59%);

- *Persistent unemployment* – Contrary to ADH, we find little sign of increased transition to inactivity. The impact on local unemployment, on the contrary, is in the spirit of the American figures. The just-identified sector-by-sector instruments of [Goldsmith-Pinkham, Sorkin, and Swift 2020] allow for a sectoral analysis of this result. As mentioned above, the bulk of the employment dynamics of the shock is driven by a triad of sectors: microelectronics-textiles-steelworks; yet the highest marginal impacts on the job stock and on unemployment figures are found for all extractive industries and for plastics;

- *A sharp negative multiplicative impact on other sectors* – But one of the most interesting aspects is the multiplicative impact connecting the decline of industrial employment to job dynamics in the non-manufacturing, non-exposed sectors. Such spillovers are difficulty identified in [Autor, Dorn, and Hanson 2013], and [Autor, Dorn, and Hanson 2021] still fail to find any negative reaction of non-manufacturing employment to an import exposure shock. In European context, on the contrary, such multiplicative effects are found almost everywhere [Dauth, Findeisen, and Suedekum 2021; Citino and Linarello 2021], France included [Malgouyres 2017]. In these estimates, or in ours [Fournel 2023a], the implied local multipliers (the elasticity of local non-manufacturing employment to manufacturing employment shocks) are in the spirit of [Moretti 2010] (for the 2008-2018 DADS strategy for instance, it is 1.37 compared to Moretti's original U.S. figure of 1.57) and very similar to other European estimates. When we try to decompose this multiplicative effect, it seems like extractive industries in the earlier decades, and electronics later on, are the main drivers, light industries like textiles having a secondary role. The precise mechanism behind that multiplicative effect is however difficult to gauge: if we restrict ourselves to trade figures about final-consumption goods, the coefficient on manufacturing employment is multiplied by more than two (in tab. 11, col. 4) while the non-manufacturing coefficient becomes nonsignificant. There is a suspicion that the retroactive effect on service jobs is in fact capturing a foreign-for-domestic low-skill labour substitution mechanism through offshoring in the spirit of [Melitz 2003]. Our next section will attempt to determinate the specific nature of these multiplicative employment responses.

4 Time dynamics

A critical dimension of these trade shocks is the responsiveness and pace of the reaction of home employment and wage values.

In figure 4, we plot the actual ZE-level longer term effects of the China shock, estimating model 3 using the 1999-2008 exposure as the main explanatory, and the evolution of some major employment variables as the dependent, in the spirit of [Acemoglu, Autor, et al. 2016]. When the shock is fully realised, at the end of the decade[8], it brings a -7.85 log points decline to the manufacturing employment stock, and a -0.51 log points decline for to the entire stock. We see that the impact is fully realised approximately 12 years after the beginning of the exposure decade, a result consistent with structural [Caliendo, Dvorkin, and Parro 2015] and reduced form approaches [Autor, Dorn, and Hanson 2021] alike. The impact on industrial employment decline is considerable, with no sign of recovery in the longer term. The short-term reaction of total employment and unemployment are in the spirit of the Blanchard-Katz model, but their longer term fortunes are not[9]; total employment reverts to its pre-exposure values, while the marginal impact on unemployment continues to be felt 20 years after the start of the shock. It is like industrial employment is becoming an autonomous, desynchronised section of the economy, a king of sectoral hysteresis[10].

5 Robustness checks I

Here's a brief overview of the robustness checks provided in the corresponding annex:

- *1.1. Testing for the relevance of shift-share instruments – Computation of the Rotemberg weights of [Goldsmith-Pinkham, Sorkin, and Swift 2020]* – Goldsmith-Pinkham and alii's initial argument about the assessment of

[8] In most empirical calibrations inspired by [Autor, Dorn, and Hanson 2013], most importantly [Caliendo, Dvorkin, and Parro 2015], the full impact is deemed realised between 7 and 12 years after the start of the exposure period.

[9] It is mainly because we fail to find any sign of outmigration reactions following a trade shock. In American context, the standard correlation with the local demographic increase through migration is 0.9 [Turek 1985] and this reaction is generally deemed to sluggish by macro models [Davis, Fisher, and Veracierto 2021]; in French context, the general figure is around 0.4, 0.7 for the Paris metropolis.

[10] Local multipliers we get over the whole period 1990-2018, as we saw, are in the spirit of [Moretti 2010; Dijk 2016], but on the 1999-2008 interval, the reaction seems less pronounced.

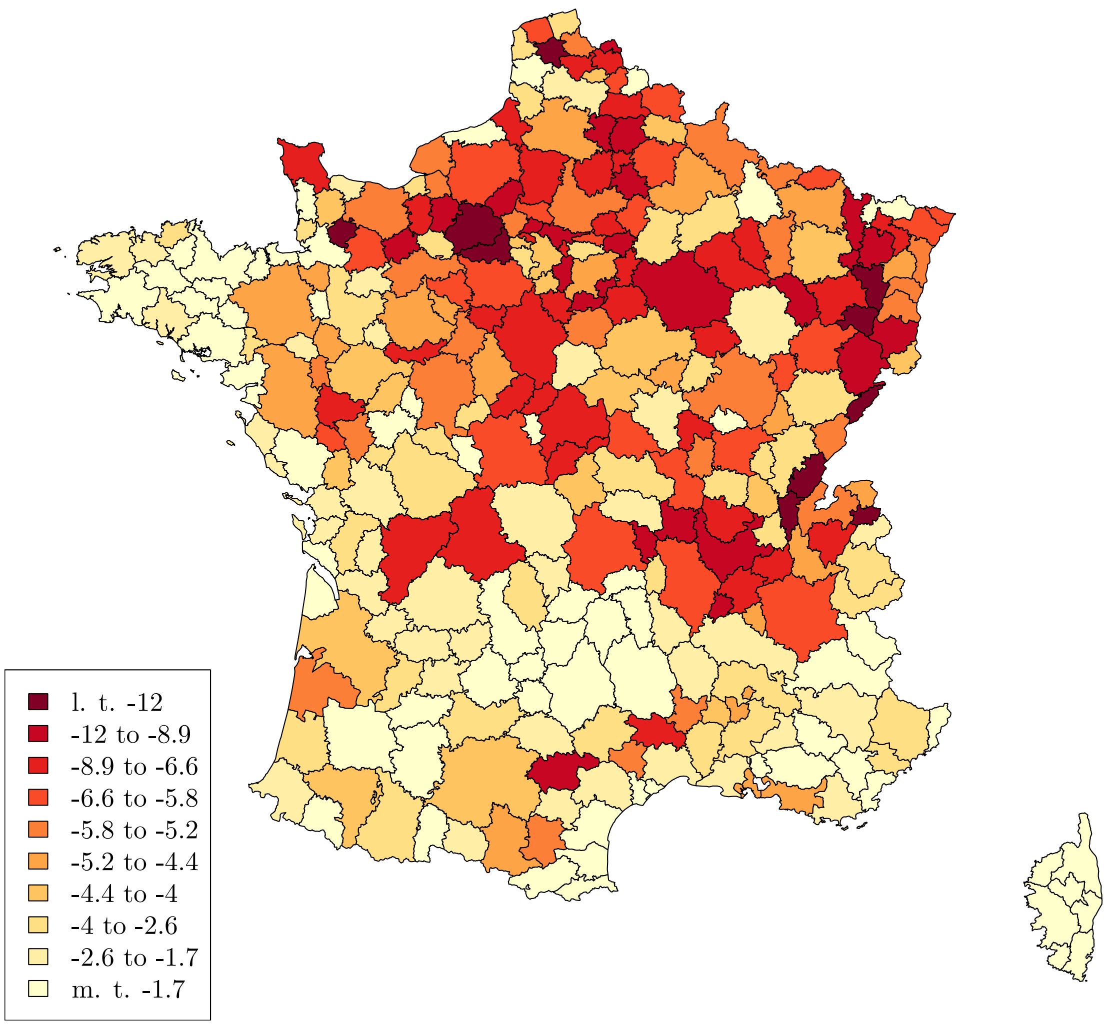


Figure 1: Industrial decline over 1990-2018 − Evolution of total manufacturing employment within each ZE (INSEE, *Zone d'emploi 2010*) as a ratio of the working age population, in percentage points [INSEE, *Recensement*]

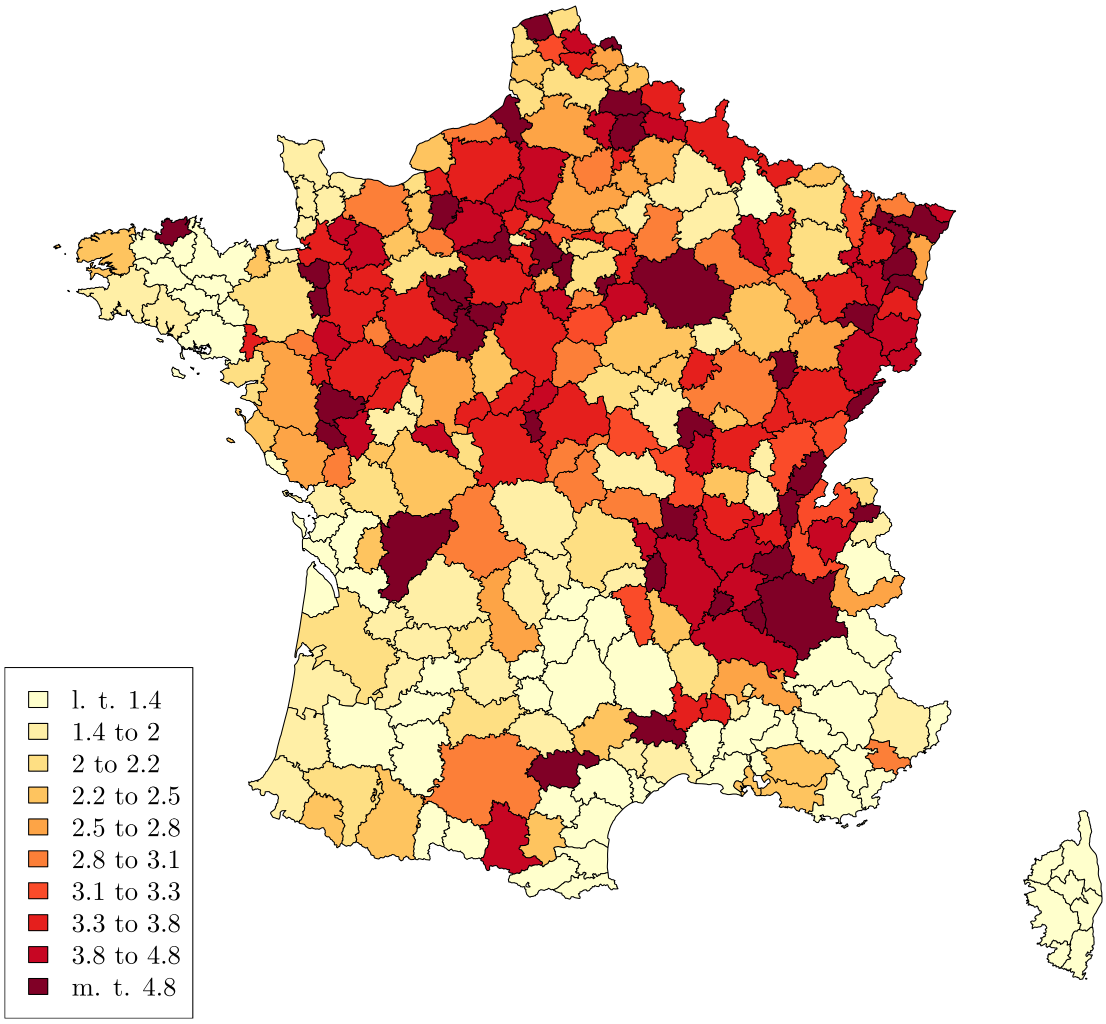
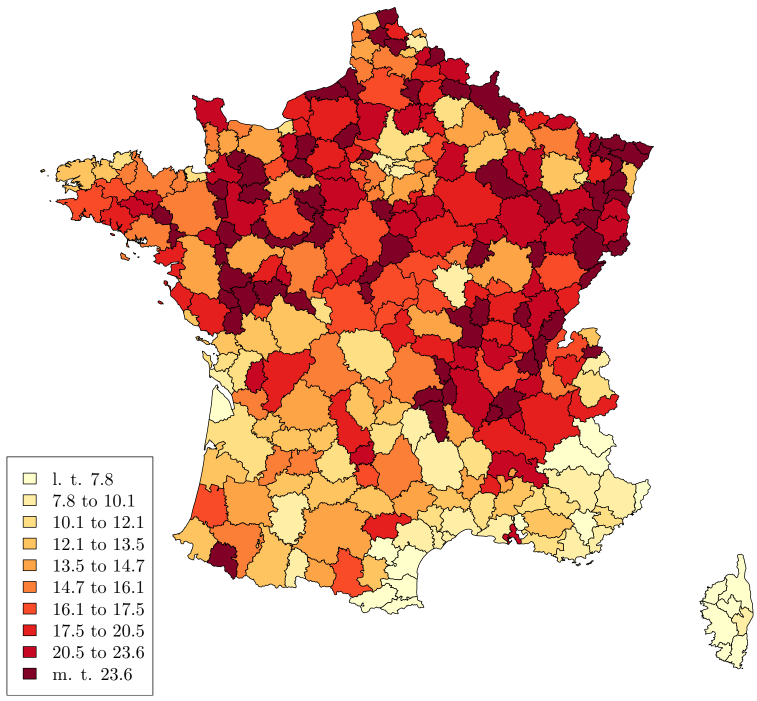


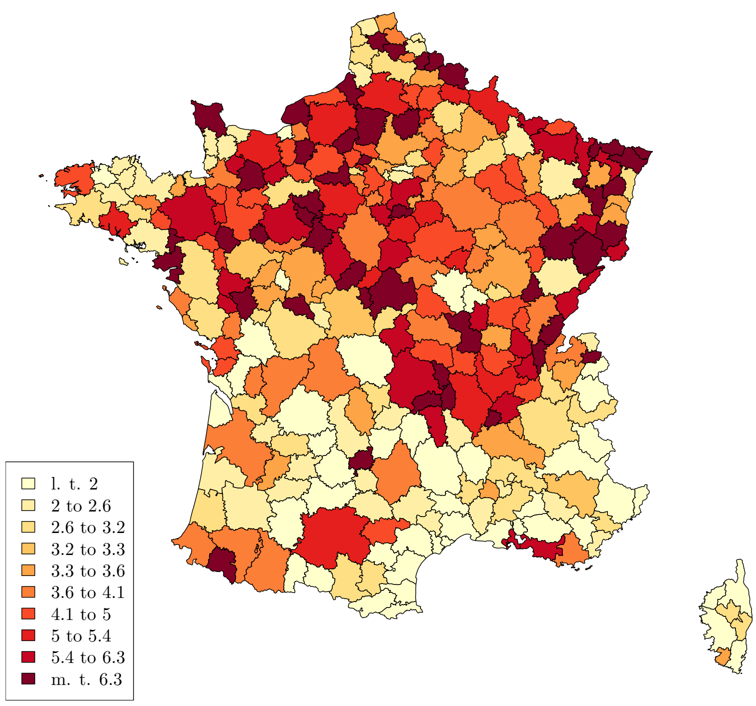
Figure 2: Main explanatory var.: Chinese imports competition exposure (1990-2018) − Av. loss of manuf. output per worker through Chinese imports (in th. of 2022 U.S. dollars) [UN, *Comtrade*; INSEE, *Recensement*]

Figure 3: Some major control variables

(a) Share of manufacturing in total employment in 1990 [INSEE, *Rec.*]



(b) Rise in the number of manufacturing robots per worker (1990-2018) [Acemoglu and Restrepo 2020; INSEE, *Rec.*]



(c) Offshorability index in 1990 [Autor and Dorn 2013; INSEE, *Rec.*; INSEE, *E.E.*]

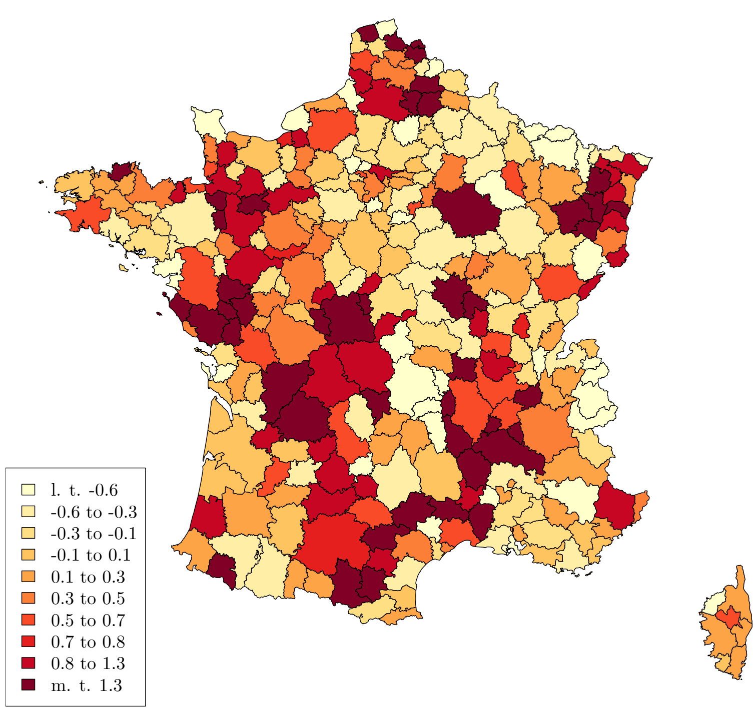
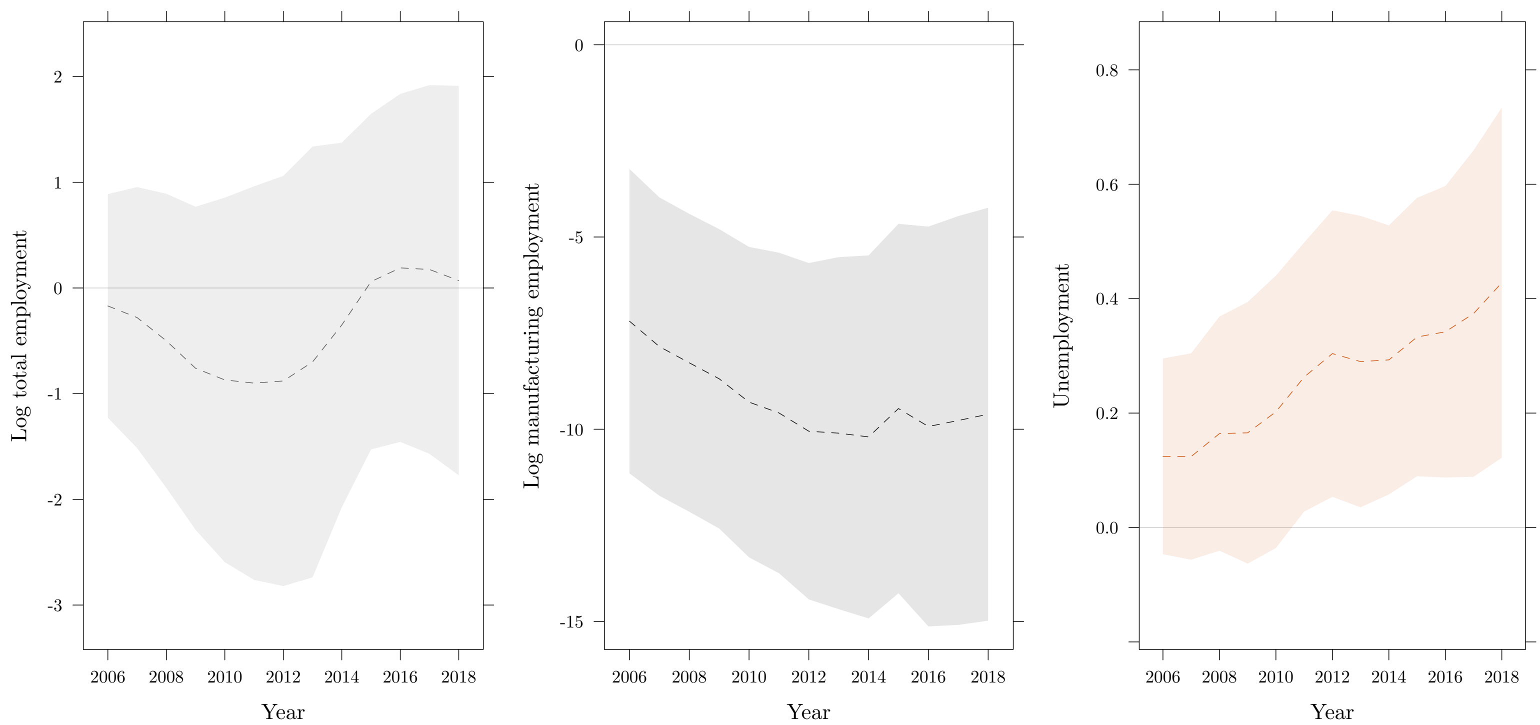


Figure 4: Long-term impact of the China shock (1999-2008 import exposure) on employment within ZEs



Note: The unit of interest is the ZE. Employment data are from the INSEE's Census. 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 of the mentioned employment variable (log total employment, log manufacturing employment, unemployment rate) between 1999 and the year mentioned on the x-axis. Estimation is done for each year over 2006-2018, weighting observations by the total population of the ZE, and clustering S.E. at the INSEE superzones level. We report 95% conf. intervals.

the exogeneity assumption for Bartik instruments being reducible to a proof of the exogeneity of the initial shares (in this setting, the initial shares of each industry within each subzone) is applied to the Autor-Dorn-Hanson framework with the finding that STEM and technology-intensive-sectors account for an abnormally high part of the identifying variation, with the risk that the initial shares for these specific industries be correlated with important drivers of recent employment dynamics (typically here, with the over-representation of higher educated workers within each zone). We use the replication codes provided by [Goldsmith-Pinkham, Sorkin, and Swift 2020] to reconstruct the equivalent of their table 4 panel D. Their pivotal finding concerning the Autor-Dorn-Hanson framework was that, among the very large number of instruments (J industries times T years), 1% of them accounted for 49.5% of the absolute sum of the Rotemberg weights (in their setting, the final main coefficient is decomposed into a weighted sum of several just-identified instrumental variable estimators, one for each industry, allowing to gauge which industry contributes the more to the final aggregate marginal impact). In our replication, as far as comparison is possible, it is 11% of the instruments which account for 53% of the Rotemberg weights' sum. More important still, Goldsmith-Pinkham and alii's censured [Autor, Dorn, and Hanson 2013] for what they consider is an interpretative gap: Autor-Dorn-Hanson put the emphasis on light industries, among which are indeed found, in sheer descriptive statistics, sectors which have experienced the highest hikes in exposure to Chinese import competition (rubber, apparel, footwear...) and for which the exclusion restriction seems more than reasonable; yet once the breakdown is applied and the Rotemberg weights computed, these light industries account for a very low share of the final negative marginal impact, while the bulk of the effect is provided by technology-intensive sectors for which the exogeneity assumption is much more questionable. Once again, as far as comparison is possible, we find less extreme results in our replication: the top 3 instruments in terms of Rotemberg weight are in order (all three for the decade 1999-2008), "computers, micro-electronics and optics", "apparel, leather and footwear" and "steelworks (machinery and equipment excluded)"; in our setting, it seems there is more congruence between the descriptive statistics of the China shock and the estimated weights.;

- *1.2. Testing for the relevance of shift-share instruments – Alternative sectoral-regional clustering* – [Adão, Kolesár, and Morales 2019], constructing randomised sectoral placebo shocks and then applying a shift-share instrumentation strategy exactly similar to the Autor-Dorn-Hanson framework, find that the null hypothesis of zero employment effect of that random shock is frequently rejected. They venture two alternative methods to construct shift-share-designs dedicated standard errors. We applied these two methods, using the INSEE's superzone as the meta-regional level, and the ISIC rev. 4 two digits subdivisions as the meta-sectoral level;

if we focus on the first coefficient in column (6) of tab. 3, the raw standard error without clustering is 0.72; the S.E. we use throughout this paper, clustered at the superzone level, is 1.33; S.E. using their AKM and $AKM0$ methods are respectively 1.204 and 1.494, with corresponding p-values all below 0.05;

- *2. From 2SLS to OLS* – In [Autor, Dorn, and Hanson 2013], the authors find a strong difference between the OLS and 2SLS estimates (their main coefficient of interest, equivalent our first line in model (6) of table 3, is reduced from -0.596 to -0.17 when switching back to an OLS specification), indicating that there was a sizeable problem of endogeneity because of local demand shocks. In our setting, we rarely find such large gaps, even if we always reject the null in the Hausman test, indicating that 2SLS estimation is still to be preferred;

- *3.1. Placebo tests for reverse causality on ancient data* – The intuition is to regress *past* changes in manufacturing employment (1975-1990) on *future* trade exposure indexes (from 1990 to 2018); no correlation of any kind is observed;

- *3.2. Placebo tests for reverse causality on contemporary data* – The same exercise is applied on recent data with the first (1990-1999) and the second decade (1999-2008), finding no correlation;

- *4.1. Alternative sources* – The matched employer-employee DADS datasets provided by the INSEE are localised (in decreasing order of size) at the level of the *région*, of the *département*, and of the *Zone d'emploi - ZE*; yet the definition of these geographical unit has changed over time (regions in 2015 and ZEs twice, in 2010 and 2020). Using DADS data therefore implies, either a restriction to a shorter time period in order to remain at the ZE level (this is the choice of [Malgouyres 2017]) or on the contrary the choice of a larger geographical unit of interest like the *département* in order to maintain a longer time period. Yet we'll be using Census instead of DADS data; issues of the *Recensement* provide since 1962 a sectoral decompozition at the *commune*-city level, from which it is possible to aggregate to any geographical unit of interest; besides, there is no more sectoral granularity over the DADS than over the Census data we access to;

- *4.2. Alternative explanatory and dependent variables* – [Autor, Dorn, and Hanson 2013] discuss the relevance of an alternative explanatory, i.e., the very same ΔIPW index, but normalised, not by the whole employment of the zone, but by manufacturing employment only, or to tell it plainly, the exposure, not by worker, but by industrial worker. As to alternative dependent variables, we tested the evolution of manufacturing employment divided by the total working-age population (the dependent of [Autor, Dorn, and Hanson 2013]) or by the total labour force, keeping the evolution of total industrial employment in pp or in log points to ensure comparability with existing European estimates. In our annex, we report a wide range of results using an alternative specification with the alternative ΔIPW described above (and the corresponding instrument) and, as dependent, the evolution of manufacturing employment divided by the total labour force;

- *4.3. Alternative instrument* – In tab. 6, we have recourse to an alternative instrument with exclusively non-UE countries (Japan, Australia, Canada & New Zealand). The estimation is unaltered;

- *4.4. Alternative trade partners* – Autor, Dorn & Hanson find similar negative impacts of trade exposure for other emerging economies which are key trade partners of the U.S., particularly Mexico. We therefore replicate our whole setting with the main European developing trade partner of France – Poland – and the main non-European developing partner behind China – Turkey. In both cases, we find some significant effects, but which are not robust across specifications. Since this might be due to the fact that these countries are minor partners in terms of sheer trade value compared to China, we replicate our setting with France's key trade partner: Germany (using our extra-EU instrument). Over 1990-2008, we find evidence of a significant negative marginal impact of exposure to imports from Germany, approximately half the size of our estimates for China. In decade-per-decade breakdown, we manage to explain 23% of the industrial decline over 1990-2008 through German competition on imports (see table 6). This computation is however somewhat artificial, since competition from European partners comes mainly from competition on export market shares.

- *5. Variance breakdown exercise* – Having computed OLS and 2SLS coefficients, we can implement a variance breakdown exercise to discriminate the share of variance in industrial decline explained by exogenous changes in trade exposure (the Chinese supply shock) from endogenous changes (the home demand channel); we find that the latter one accounts for 93% of the total for the 1990-2008 period;

- *6. Intermediate versus final goods* – Over our UN-Comtrade data, we have recourse to the BEC classification to distinguish imports of intermediate and of final consumption goods; the former one only can be considered as exemplifying true import competition, the latter one being a consequence of offshoring. When we replicate specification (6) of table 3 with final goods only, we get $\hat{\beta}_1 = -6.37$ ($t = 3.43$); on intermediate goods, we get a nonsignificant positive coefficient;

- *7. Spatial spillovers* – As opposed to [Autor, Dorn, and Hanson 2021] and concurrently with [Adão, Arkolakis, and Esposito 2019], we find in the spatial econometrics exercise of (see our tab. 7), some evidence of strong spillover effects across neighbouring ZEs when it comes to the employment effect of the China shock. [Dorn and Levell 2021] impute these reactions to a local suppliers' channel, i.e., the industrial decline in one region is doomed to hurt the suppliers of nearby zones.

6 Robustness checks II – A Manski setting

With almost all our empirical models, when we apply the Moran test to the residuals, we systematically reject the null, meaning there is strong spatial correlation, which is not surprising for the type of phenomena we are studying. Our correction strategy, congruent with what the literature advises in such cases (see especially [Loonis and Bellefon 2018]), consists in modelising our main specifications as a peer effects model *à la* [Manski 1993], distinguishing zones by their level of proximity to one another.

Towards a spatial VAR model

Building a the weight matrix

In order to do so, we need a weight matrix. There is a wide debate in the econometric literature about the influence of the choice of the weight matrix on the estimation strategy, though recent research suggests that the sensibility of estimates to this choice is generally overestimated [Lesage and R. Pace 2014]. As a precaution, we shall use four different ones, built with the following options:

- A Delaunay triangulation on the coordinates of the centroids of the zones;
- A matrix based on contiguities between zones[11];
- A matrix based on the 3 nearest neighbours[12];
- A matrix based on distance (when the unit of interest in the ZE, for instance, the weights decrease by the square of the distance, and are equal to zero beyond a radius of 150km);

A spatial overview of the three first methods is provided in figure 5. For all types of matrices, weights are normalised row-wise so that the sum of each line is equal to one. The matrix of weights is always denoted W.

Main specification

The Manski framework has been recently transcribed in spatial econometrics, notably by [Elhorst 2010]. In this setting, the model is specified in matrix form as:

$$\begin{aligned} Y &= \rho \cdot WY + X \cdot \beta + WX \cdot \theta + u \\ u &= \lambda \cdot Wu + \varepsilon \end{aligned} \tag{6}$$

Where Y is the matrix of the dependent variable (Δu in our case), X the matrix of explanatory and control variables, and W the weight matrix defined herein above. The interpretation of the coefficients is derived from Manski:

- ρ captures the *endogenous effects* (the retroactive impact of the choice variable; in our main specification 3, it would be a decline of industrial employment in one ZE caused by the industrial decline of neighbouring ZEs);

[11] In our computations, two districts are considered to be contiguous if they have at least one point in common; this rule is called the *queen* contiguity in spatial econometrics, a metaphorical comparison with the moves of the queen in a chess game.

[12] For these three first types of matrix, if one district has n neighbours, each neighbouring district gets a weight of $1/n$, the remaining ones, a zero weight.

Table 6: Robustness checks − Alternative instrument, alternative trade partner

Dep. : Decadal change in total manuf. employ. over 1990-2008 (in pp)						
	China				Germany	
	(1)	(2)	(3)	(4)	(5)	(6)
Rise in import exposure from specified exporter						
Panel A. OLS estimates						
β_1	-6.03^{***}	-3.78^{***}	-6.03^{***}	-3.78^{***}	-1.45^{**}	-1.31^{**}
SE	(1.39)	(0.89)	(1.39)	(0.89)	(0.62)	(0.51)
R^2	0.28	0.61	0.28	0.61	0.21	0.43
F-stat	116^{***}	27.5^{***}	116^{***}	27.5^{***}	75.9^{***}	45.2^{***}
Panel B. 2SLS estimates						
β_1	-6.83^{***}	-4.18^{***}	-6.81^{***}	-4.61^{***}	-1.57^{**}	-1.56^{***}
SE	(1.41)	(1.21)	(1.61)	(0.72)	(0.77)	(0.49)
R^2	0.27	0.61	0.27	0.604	0.19	0.61
F-stat	114.6^{***}	27.3^{***}	113.2^{***}	27.5^{***}	74.7^{***}	29.5^{***}
First-stage:						
Original instrument	0.96^{***}	0.88^{***}				
	(0.05)	(0.09)				
Extra-EU instrument			0.72^{***}	0.75^{***}	2.61^{***}	2.28^{***}
			(0.03)	(0.04)	(0.09)	(0.18)
R^2	0.91	0.92	0.92	0.92	0.86	0.91
F-stat	5801^{***}	210^{***}	5326^{***}	211^{***}	3686^{***}	191^{***}
Controls		X		X		X
Obs.	608	608	608	608	608	608
Av. national $\Delta IPW_{1999,2008}$	2.289	2.289	2.289	2.289	3.608	3.608
Explained share of manuf. decline		0.36		0.39		0.23

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 change (in pp) of total manufacturing employment within the ZE. 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 (in 2022 kUSD) within each ZE. The original 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 extra-EU instrument is similarly built with a control group made out of Japan, New Zealand, Australia & Canada. 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. The shares explained are computed using the variance breakdown exercise reported in the corresponding annex.

- θ captures the *exogenous effects* of each explanatory variable (the impact of neighbouring district's characteristics on the choice variable ; here, it would an industrial decline in one ZE driven by the change in some control variable of another ZE);
- λ captures the *correlated effects* (the impact of the wider context);

Choice of the relevant spatial model

It is well know that Manski models cannot be estimated and interpreted directly, and require extra hypotheses about the nullity of at last one of those coefficients.

From now on, we'll be relying on a simple OLS version of model (3), with the evolution of import exposure over the second decade $\Delta IPW_{1999,2008}$ as the main explanatory variable, plus the full set of start-of-the-period values of controls mentioned in table 3; the default dependent for now is the evolution of manufacturing employment within the ZE over 1999-2008 (in pp).

The usual strategy, drawing from [Elhorst 2010] is to start with two robust Lagrange multiplier tests, taking $\rho = 0$ and $\lambda = 0$ as null hypotheses. With our main specification, at a 5% risk, we reject the second but not the first null. In this case, it is advised to estimate separately the framework as a SDM - Spatial Durbin Model (in which we assume that $\lambda = 0$, but allow ρ and θ to be non-null) and as a SEM - spatial error model (based on the hypothesis that ρ and θ are equal to zero). In the ensuing likelihood ratio test, we reject the hypothesis of a common factor between these two models (the null $\theta = -\rho\beta$). We therefore opt for a SDM model.

In such a specification, it is impossible to interpret directly coefficients. In a model where $\rho \neq 0$, there is a contamination effect of the industrial decline in one ZE in the decline in the neighbouring ones. Similarly, when $\theta \neq 0$, the industrial decline of a zone can be driven by the control variables of neighbouring zones. Formally, if we use subscript r to index our explanatory variables, that specification writes:

$$\begin{aligned}
&Y = \rho \cdot WY + X \cdot \beta + X \cdot \theta + \varepsilon \\
\Longleftrightarrow &(1-\rho W)Y = X \cdot \beta + WX \cdot \theta + \varepsilon \\
\Longleftrightarrow &Y = (1-\rho W)^{-1} X \cdot \beta + (1-\rho W)^{-1} WX \cdot \theta + (1-\rho W)^{-1}\varepsilon \\
&Y = \sum_{r=1}^{R} (1-\rho W)^{-1}\beta_r X_r + \sum_{r=1}^{R} (1-\rho W)^{-1} W\theta_r X_r + (1-\rho W)^{-1}\varepsilon \\
&Y = \sum_{r=1}^{R} (1-\rho W)^{-1}(I_n\beta_r + W\theta_r) X_r + (1-\rho W)^{-1}\varepsilon
\end{aligned}$$

The matrix $(1-\rho W)^{-1}(I_n\beta_r + W\theta_r)$ is specific to each explanatory variable r; we can rename it $S_r(W)$. It is of size $N \times N$, providing, for that variable, for each district, its direct impact, and indirect impact on any other district. Formally:

$$Y = \begin{pmatrix} \Delta u_{1,t-10,t} \\ \Delta u_{2,t-10,t} \\ \vdots \\ \Delta u_{N,t-10,t} \end{pmatrix}, X_r = \begin{pmatrix} x_{1,r} \\ x_{2,r} \\ \vdots \\ x_{N,r} \end{pmatrix}, S_r(W) = \begin{pmatrix} S_r(W)_{1,1} & S_r(W)_{1,2} & \dots & S_r(W)_{1,N} \\ S_r(W)_{2,1} & S_r(W)_{2,2} & \dots & \vdots \\ \vdots & \vdots & \ddots & \vdots \\ S_r(W)_{N,1} & S_r(W)_{N,2} & \dots & S_r(W)_{N,N} \end{pmatrix}$$

Following [Lesage and R. K. Pace 2009], the usual interpretative solution is to rely on two measures: 1. The average of the diagonal coefficient (or equivalently, the trace of the matrix divided by N) which provides the *average direct impact* of one district on itself for one explanatory variable; 2. The average of all coefficients, which provides the average total impact; differenced with the average direct impact, it gives the *average indirect impact*; intuitively, it provides, for one average district, the average impact of the N marginal changes of r in all zones, minus the average direct impact.

The usual estimation strategy relies on Markov chain Monte Carlo methods to approximate the distribution of these two effects; with 1000 repetitions, we can build reliable confidence intervals, which are displayed in table 7.

Results of the estimation of the spatial Durbin model

As we see, with almost all choices of matrices of weights, we find a sizeable negative direct impact of the non-instrumented ΔIPW on the local industrial decline, an impact which is significantly different from zero with a 2.5% risk. As to indirect/retroactive impacts, their significance is critically dependent on the choice of the weight matrix; they become significant when we use a more restrictive definition of neighbourhood.

Table 7: Controlling for spatial autocorrelation

	Dep. var.: Change in total manuf. employment (pp)			
Matrix of weighs	Delaunay	Contiguity Q	Closest 3	Distance
	(1)	(2)	(3)	(4)
Exposure to import competition per worker				
OLS estimates	−1.42	−1.42	−1.42	−1.42
	(0.96)	(0.96)	(0.96)	(0.96)
- *Moran test stat.*	7.42***	8.67***	7.51***	11.13***
Spatial Durbin model				
- *Av. direct impact*	**−3.06**	**−3.03**	**−3.67**	**−3.48**
	[−5.16,−0.87]	[−5.03,−0.89]	[−5.57, −1.75]	[−5.64,−1.33]
- *Av. indirect-retroactive impact*	−2.91	0.53	**−5.99**	**−16.08**
	[−11.46, 5.36]	[−7.85, 8.47]	[−10.93,−0.95]	[−32.39, −0.65]

Note: The main specification in an OLS version of model (3), with the evolution of import exposure over the second decade $\Delta IPW_{1999,2008}$ as the main explanatory variable, plus the full set of start-of-the-period values of controls mentioned in table 3; the dependent is the evolution of manufacturing employment within the ZE over 1999-2008 (in pp)., and rewritten as a Spatial Durbin model (where we allow the value of the dependent variable of one district to be influenced by the values of the dependent and the explanatory variables of neighbouring districts), with four different matrices of weights indicated in each column. We use 1000 iterations of Markov chain Monte Carlo methods to produce an estimation of the average direct and indirect (or retroactive) impact of our main explanatory on the local evolution of the upper-group population. We reported in brackets the confidence interval for the estimated impact, for a 2.5% risk. We put in bold coefficients, the confidence interval of which does not comprise zero.

7 Summary of findings and conclusion

This study focused on analyzing the impact of import competition, on manufacturing employment in Europe, taking the example of France. We applied a shift-share strategy to exploit variability in exposure to import competition from several key partners. China stands out as the only emerging market on which it is possible to isolate a significant employment response at home. In our prefered setting, a marginal rise in Chinese import exposure cripples manufacturing employment by a −4.02 pp margin over the decade.

The bulk of the impact is observed in the early 2000s, coinciding with a significant rise in imports from China. Equivalent outcomes are harsh to replicate for emerging trade partners of lesser importance. We investigate the relevance of this setting for intra-EU trade.

Results are robust to controls from alternative theories of industrial decline such as skill-biased technological change, offshoring, and automation. Heeding to the growing concerns from econometricians on shift-share instruments and their reliability, we applied several key decompozition exercises ventured by recent contributions. We also tested for a wide variety of spatial settings to check for potential confounders.

The most interesting piece of evidence, however, does not involve aggregate outcomes, but rather the striking similarity between the decomposition of the impact of Chinese exports on U.S. employment in [Adão, Kolesár, and Morales 2019] and the one found there in European context, i.e. the fact that the employment impact of Chinese exports is increasingly concentrated on technology-intensive sectors. At at time of intense debates on how EU's

Trade Defence Instruments (TDIs) are to be reformed [European Comission 2022] to counter China's dual policy of internal State-sponsored investments and FDI to emerging markets (the so-called *Go out policy*),

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Annex

Robustness checks

Table 8: Exposure to imports from some countries and change in manufacturing employment at the ZE level - Alternative import partner

Dep. : Decadal change in total manuf. employ. per working age pop. 1990-2008 (in pp)	China		Turkey	Poland	Germany
	(1)	(2)	(3)	(4)	(5)
Panel A. OLS estimates					
Rise in imports from specified exporter	-0.15^{***}	-0.15^{***}	0.09	0.43	0.02
per worker (in 2022 kUSD)	(0.05)	(0.05)	(0.32)	(0.46)	(0.02)
Panel B. 2SLS estimates					
Rise in imports from specified exporter	-0.19^{***}	-0.14^{***}	0.98	1.04	0.08
per worker (in 2022 kUSD)	(0.06)	(0.05)	(1.1)	(0.63)	(0.05)
R^2	0.49	0.48	0.46	0.46	0.46
F-stat	16.7^{***}	16.6^{***}	16.1^{***}	16.4^{***}	16.1^{***}
First-stage:					
Original instrument	0.83^{***}				
	(0.08)				
Extra-EU instrument		0.68^{***}	10.1^{***}	2.3^{***}	2.28^{***}
		(0.06)	(1.06)	(0.31)	(0.32)
R^2	0.91	0.98	0.98	0.97	0.91
F-stat	266^{***}	251^{***}	81.1^{***}	579^{***}	188^{***}
Obs.	608	608	608	608	608

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 change (in pp) of total manufacturing employment within the ZE, as a ratio of the total working age population of the zone. 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 (in 2022 kUSD) within each ZE. The original 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 extra-EU instrument is similarly built with a control group made out of Japan, New Zealand, Australia & Canada). Observations are weighted by the start-of-the-decade total population of the ZE. All specifications contain the full vector controls of the main model. Standard errors are clustered at the level of the 10 INSEE superzones.

Table 9: Testing for reverse causality (Present exposure to China trade versus Past industrial employment decline) - 2SLS estimates

	Dep. : Decadal change in total manuf. employ. per working age pop. 1975-1990 (in pp)		
	(1)	(2)	(3)
Rise in imports from China per worker:			
1990-1999	3.4		
	(3.1)		
1999-2008		$-.32$	
		(.21)	
2008-2018			.21
			(.66)
R^2	0.07	0.002	0.001
F-stat	3.2^{*}	5.2^{**}	0.16

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 change (in pp) of total manufacturing employment within the ZE, divided by the total working age population of the zone. The main explanatory variable is the index ΔIPW, described herein above, which provides an estimation of the mean rise in Chinese imports per worker of the industrial sector (in 2022 kUSD) 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. No other control is applied. Observations are weighted by the start-of-the-decade total population of the ZE. Standard errors are clustered at the level of the 10 INSEE superzones.

Table 10: Testing for reverse causality (Exposure to China trade and industrial decline with a decadal lag) - 2SLS estimates

	Dep. : Decadal change in total manuf. employ. per working age pop. 1990-1999 (in pp)			
	Exposed ZEs		All ZEs	
	(1)	(2)	(3)	(4)
Current period exposure (1990-1999)				
Rise in imports from China per worker	-1.1	0.18	-1.59	-0.49
	(1.4)	(1.1)	(1.7)	(1.4)
Start-of-the-period manuf. empl. share		-0.19^{***}		-0.22^{***}
		(0.03)		(0.04)
Future period exposure (1999-2008)				
Rise in imports from China per worker	-0.22	-0.22^{*}	-0.04	-0.05
	(0.13)	(0.12)	(0.11)	(0.11)
Start-of-the-period manuf. empl. share		-0.08		-0.01
		(0.08)		(0.05)

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 change (in pp) of total manufacturing employment within the ZE, divided by the total working age population of the zone. The main explanatory variable is the index ΔIPW, described herein above, which provides an estimation of the mean rise in Chinese imports per worker of the industrial sector (in 2022 kUSD) 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. Exposed ZEs are the top quartile of ZEs ranked according to their $\Delta IPW^{fr}_{1999-2008} - \Delta IPW^{fr}_{1990-1999}$. Observations are weighted by the start-of-the-decade total population of the ZE. Standard errors are clustered at the level of the 10 INSEE superzones.

Alternative indices

Table 11: Exposure to imports from China and evolution of occupations within each ZE

	Dep. : Decadal change in population log counts or shares of total adult population							
	Population evolution			Population breakdown				
	Total change	*Natural increase*	*Migration increase*	In labour force	*Employ. in services*	*Unemploy.*	Retired	Other inactivity
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Change in log counts								
Rise in imports from China per worker:								
No controls:	-0.08	-0.25^{***}	0.16	-0.04	-0.28	1.29^{***}	1.15^{***}	-1.37^{**}
	(0.19)	(0.04)	(0.21)	(0.21)	(0.26)	(0.44)	(0.42)	(0.61)
Full vector of controls:	0.43	-0.21^{**}	0.46^{*}	0.31	0.13	1.83^{***}	1.52^{***}	-0.42
	(0.26)	(0.08)	(0.27)	(0.22)	(0.21)	(0.48)	(0.55)	(0.72)
Panel B. Change in shares of adult pop.								
Rise in imports from China per worker:								
No controls:				0.01	-0.05	0.08^{***}	0.27^{***}	-0.17^{**}
				(0.11)	(0.06)	(0.015)	(0.06)	(0.07)
Full vector of controls:				-0.08	-0.11	0.07^{***}	0.24^{***}	-0.11
				(0.14)	(0.08)	(0.015)	(0.08)	(0.09)
Obs.	608	608	608	608	608	608	608	608

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 evolution of the related population, either in log counts, or expressed in shares of the total adult population (i.e. all individuals aged 15 y.o. or more) of the ZE. Exp. var. ΔIPW and corr. instr. have been described herinabove. We stack two decades (1990-1990 and 1999-2008) and include a time dummy for the second decade. 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.

Variance breakdown exercise

Autor, Dorn & Hanson's intuition to discriminate the supply-driven from the demand-driven dimension of the China shock relies on a comparison between the OLS and 2SLS estimates of the marginal impact of exposure. If we rewrite the main model and drop covariates, we get:

$$\Delta L_{it} = \beta \Delta IPW_{it} + u_{it} \tag{7}$$

Coefficients are then straightforwardly estimated with:

$$\hat{\beta}_{OLS} = \frac{Cov(\Delta IPW, \Delta L)}{Var(\Delta IPW)}, \qquad \hat{\beta}_{2SLS} = \frac{Cov(\Delta IPW_{IV}, \Delta L)}{Var(\Delta IPW_{IV})}$$

Our instrumentation is meant to isolate the exogenous from the endogenous dimension of the change in China trade exposure:

$$\Delta IPW = \Delta IPW_{IV} + \Delta IPW_{Endo}$$

In 7, an OLS estimation will therefore yield:

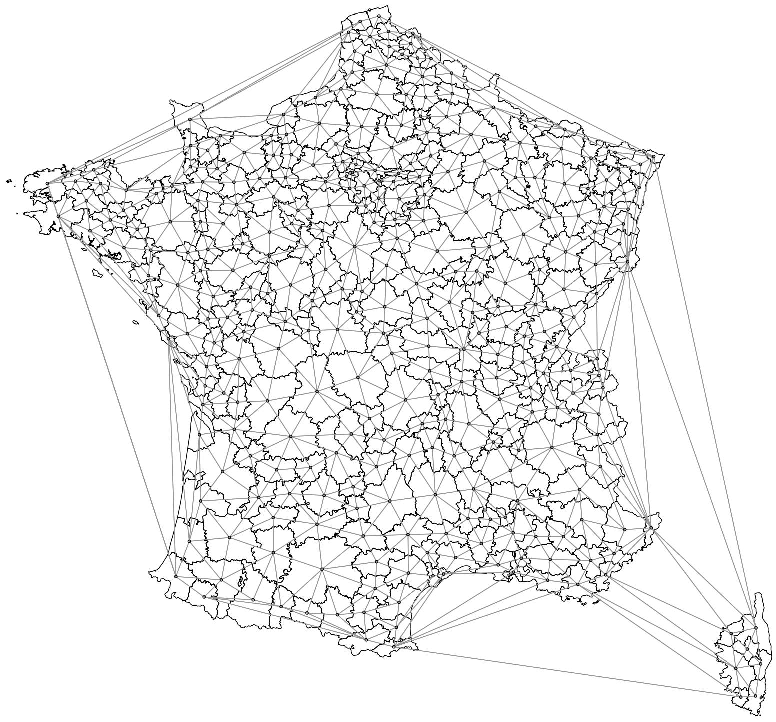
$$\begin{aligned}
\hat{\beta}_{OLS} =& \frac{Cov(\Delta IPW_{IV} + \Delta IPW_{Endo}, \Delta L)}{Var(\Delta IPW_{IV} + \Delta IPW_{Endo})} \\
=& \frac{Cov(\Delta IPW_{IV}, \Delta L) + Cov(\Delta IPW_{Endo}, \Delta L)}{Var(\Delta IPW_{IV}) + Var(\Delta IPW_{Endo}) + 2 \times Cov(\Delta IPW_{IV}, \Delta IPW_{Endo})} \\
=& \frac{Cov(\Delta IPW_{IV}, \Delta L) + Cov(\Delta IPW_{Endo}, \Delta L)}{Var(\Delta IPW_{IV}) + Var(\Delta IPW_{Endo})}
\end{aligned}$$

The simplification of the last lign being operated through the orthogonality of construction between the endogenous and exogenous part of the breakdown of ΔIPW. If we substitute with our estimated coefficients, and define a similar endogenous β, we get:

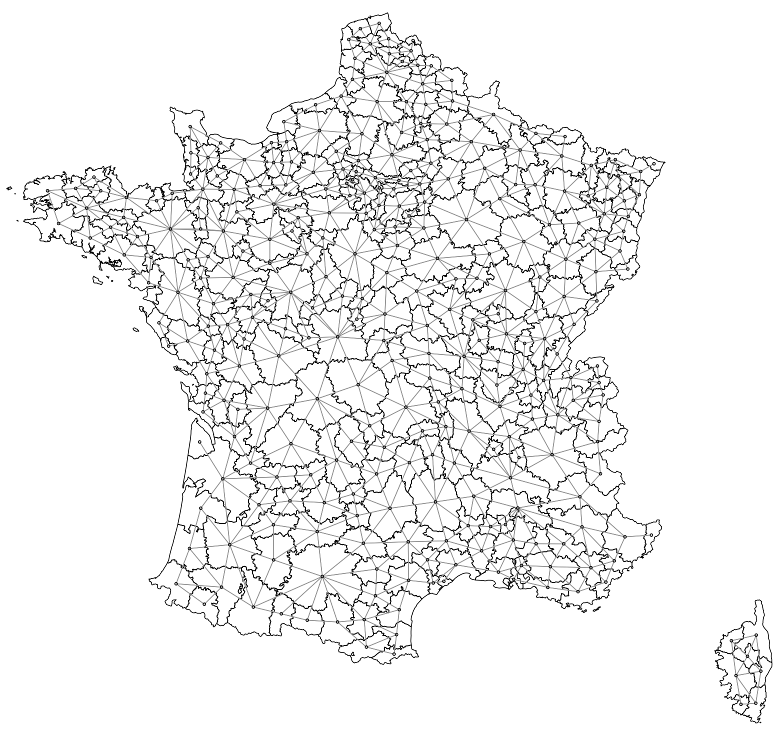
$$\hat{\beta}_{OLS} = \hat{\beta}_{IV} \times \frac{Var(\Delta IPW_{IV})}{Var(\Delta IPW_{IV}) + Var(\Delta IPW_{Endo})} + \hat{\beta}_{Endo} \times \frac{Var(\Delta IPW_{Endo})}{Var(\Delta IPW_{IV}) + Var(\Delta IPW_{Endo})}$$

Figure 5: Three methods to build a weight matrix for the proximity of ZEs

(a) Delaunay triangulation of the centroids



(b) Contiguity - Common border



(c) Closest three neighbours

The corresponding coefficients are then taken from the data. For instance, over the decades 1999-2018, in the simplest specification with no controls, we obtained $\hat{\beta}_{OLS} = -5.66$ and $\hat{\beta}_{2SLS} = -6.27$ and we can compute $\hat{\beta}_{Endo} = 2.07$. From this, it follows that $\frac{Var(\Delta IPW_{IV})}{Var(\Delta IPW_{IV})+Var(\Delta IPW_{Endo})} \approx 0.93$.

That figure is superior to the one of [Autor, Dorn, and Hanson 2013], but very similar to the ones found in other European replications.