

# Globalization, Compensation, and the Rise of the Far Right

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International economic integration and the ensuing economic shocks increase voters' support for far right populist parties. Can compensation stem this tide? I investigate this question using novel, geo-located data on globalization-induced job losses. Using a difference-in-differences estimation strategy, I find that additional compensation for globalization-related job losses has no robust effect on far right parties' vote shares.

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How does globalization affect politics? In recent years, globalization has fundamentally altered politics in many countries by motivating the rise of far-right populist parties. The rise of far right parties raises two key questions. First, what factors contribute to the emergence of far right political parties (Kriesi et al. 2006)? Second, what policies could curtail the rise of extreme right parties?

To date, virtually all research has focused on the first question. Studies find that voters in areas hit hard by economic shocks shift their support to far-right parties (Colantone and Stanig 2018, Milner 2019, Dippel, Gold and Heblich 2016, Malgouyres 2017). This pattern holds in various developed countries including France (Malgouyres 2017) and Germany (Dippel, Gold and Heblich 2016).

In contrast, however, virtually no studies address the second question. What, if anything, can mainstream parties do to stem the rise of the far right? Can compensation programs designed to offset the costs of globalization slow the rise of the extreme right? If governments provided greater compensation to voters harmed by economic shocks, might this keep voters from turning to the extreme right?

Existing theories offer conflicting answers. A long-standing theory, often referred to as embedded liberalism, suggests the answer is yes. The “bargain of embedded liberalism” (Ruggie 1982), established after WWII, recognizes that mass support for globalization can be maintained by government transfers that tax the winners from economic integration in order to fund a social safety net for the losers. If compensation can offset the costs of globalization, it may in turn stem the rise of far right political parties. Compensation via redistribution appears to have worked to mute preferences for extremism in the past (e.g. Burgoon 2009, Garrett 1998).

But some scholars have begun to question the effectiveness of compensation as opposition to globalization grows (e.g. Gingrich 2019). Gidron and Hall (2017) maintain that people today prefer recognition over redistribution. As a result, individuals who feel left behind by globalization cannot be placated with compensation. What distinguishes today's "losers" from other voters is a feeling that they have not been treated with respect and a sense that they are not fully valued members of society. They care as much, or even more, about recognition as about redistribution (Gidron and Hall 2017, 26).

Despite the emerging debate about the effectiveness of compensation, its impact on voters' support for the far right remains unclear. This lacuna stems from challenges of research design. Research studying the individual-level determinants of support for far right parties has overwhelmingly relied on cross-sectional survey data (Margalit 2019). Beyond survey data, researchers investigating actual election outcomes employ both single-country and multi-country studies. But such studies are ill equipped to examine the role of compensation on far-right voting.

In single-country studies, compensation is typically uniform and invariant. In effect, all of the losers from globalization within a given country are "treated" – that is, they all receive compensation, and often the same amount of compensation, for trade-related job losses. As a result, no control group exists with which to compare the effects of compensation in single-country studies.

Multi-country studies could examine the effectiveness of compensation by leveraging the variation in states' welfare systems and safety nets. However, most cross-national studies include country fixed effects that soak up cross-national variation, including the variation in compensation policies. And most cross-national studies do not interpret or even report the country-specific intercepts. Furthermore, myriad factors vary across countries including party

systems, electoral institutions, and executive-legislative relations. As a result, multi-country studies do not provide the ideal opportunity to test the causal effect of compensation on far right parties' electoral support.

I overcome these challenges by leveraging within-country variation in compensation in a single-country study. In France, some workers receive extra compensation for trade-related job losses while others do not. This within-country variation allows me to estimate the effect of (extra) compensation on support for far right populist parties, holding country-specific characteristics constant.

Using novel, geo-located data on globalization-induced job losses, I estimate a difference-in-differences model that compares the far right party's vote shares over time in "treated" and untreated areas, where treated areas are those in which workers made unemployed by globalization received extra compensation. I find that extra compensation does not significantly change the far right's vote share relative to areas without extra compensation. In other words, extra compensation is ineffective in stemming the rise of the far right. Simply doing "more of the same" does not stem the growing popularity of the far right.

This paper makes two contributions. First, it contributes to the burgeoning literature on the rise of far right populist parties. Right-wing populist parties are the driving force behind political transformations in many developed countries (Kriesi et al. 2006). Given this, it is important to understand the factors that influence the strength of this particular type of party (Kriesi et al. 2006). To date, many studies focused on estimating the effects of import shocks on far right vote shares in either single country studies, where compensation is constant, or multi-country studies using fixed effects that soak up cross-national variation. As a result, these studies are largely mute on the potential role of compensation in moderating

the rise of the far right. In contrast, this study exploits novel data on within-country variation in compensation to estimate the effectiveness of compensation in muting voters' reactions to economic shocks. I find no evidence to suggest that compensation works to keep voters from turning to the extreme right in response to negative economic shocks.

Second, this study contributes to the emerging debate over the effectiveness of compensation in offsetting the costs of globalization today. Despite the long intellectual history of embedded liberalization, some scholars have recently suggested that compensation is no longer effective to sustain public support for economic openness. Given the growing backlash against compensation, this assertion seems reasonable. The novel findings reported here provide suggestive evidence that compensation is increasingly ineffective.

### **Globalization and the rise of the far right**

Existing research demonstrates a clear relationship between negative economic shocks and increased support for extreme political parties, particularly on the right. Autor, Dorn and Hanson (2013) and Autor et al. (2016) show that districts in the US most affected by surges in imports from China tend to remove moderate members of congress and vote in more extreme ones, especially on the right. Colantone and Stanig (2018b) show that increased trade, especially Chinese imports, leads voters to increase support for extreme right parties in 15 west European countries from 1988-2007. Milner (2019) extends and updates Colantone and Stanig's (2018b) analysis and finds similar results. Dippel, Gold and Heblich (2016) demonstrate that German voters in areas most exposed to import shocks tend to increase their support for extreme right parties. Malgouyres (2017) finds that, in France over the 1995-2012 period, exposure to low-wage country import competition increases the share of votes for Front National. In sum, a robust relationship exists between negative economic shocks and far right voting.

Can compensation stem the rise of the far right by offsetting the costs of negative economic shocks? Most existing research on the electoral effects of globalization's economic shocks are silent on the effects of compensation. Single-country studies are generally unable to examine the effects of compensation because all persons negatively impacted by globalization are "treated" – that is, all trade-displaced workers are compensated via national welfare programs. As a result, no comparable treatment and control groups exist to test the effects of compensation on voters made worse off by globalization.

In multi-country studies, country fixed effects are typically employed, which "soak up" the cross-national variation in welfare state generosity. Existing studies generally do not examine the substantive effects of country-specific dummies that could speak to the role of compensation in mediating globalization's impacts on support for far right parties.

However, one prominent view associates rising support for populism with a failure to provide adequate levels of economic compensation to people on whom international trade has imposed concentrated losses (e.g., Asatryan et al., 2014, Roubini 2016, Obstfeld 2016). But increased compensation may not be enough to assuage supporters of radical right parties (Kurer 2017). Voters for far right parties are not especially strong supporters of redistribution. What distinguishes them from voters for mainstream parties is the feeling that they have not been treated with respect – a sense that they are not fully valued by society. They care as much about recognition as about redistribution (Gidron and Hall 2017). People who feel socially-marginalized may want decent well-paid jobs more than they want social benefits or social reassurance.

In an observational study, Gingrich (2019) finds that the compensation has weak or inconsistent effects on how voters responded to new economic pressures – in some cases shoring up support for both the mainstream left and right populists. She concludes that

compensation does not stop voters from supporting populist parties following economic shocks. Similarly, Kurer (2017) finds that it is a perception of relative decline and concomitant anxiety about one's position in society — not poverty or acute material hardship — that drives support for right-wing populist parties. This suggests that no amount of compensation will suppress voters' response to negative economic shocks.

### **Innovation**

Identifying the causal effects of compensation for trade-related job losses on election outcomes is challenging. One challenge is the lack of within-in country variation in compensation. Another challenge comes from the difficulty of measuring globalization-related job losses. Most existing studies calculate the exposure of sub-national geographic areas to (Chinese) imports and then assert, or sometimes show, that local employment is reduced in areas more exposed to (Chinese) imports. But even studies that regress imports on employment do not measure the actual number of job losses due to globalization.

In this study, I aim to overcome these challenges. First, I compile novel, geo-located data that directly measures globalization-induced job losses and locates these job losses in geographic space. I compile these data using documents from the European Union and the European Globalization Adjustment Fund (EGF). The EGF was set up by the European Union in 2007 to support workers who lose their jobs as a result of globalization and changing trade patterns. The main aim of the fund is to support redundant workers who experience hardship because of globalization.

Second, I exploit within-country variation in compensation to estimate its impact on far right parties' vote shares. Not all workers who lose their jobs because of globalization receive extra compensation from EGF (Rickard 2019). As a result, compensation for globalization-related job losses varies within a single-country. I exploit this within-country

variation to examine whether extra compensation mediates the electoral effects of negative economic shocks. In this sense, extra compensation is the “treatment”. Although this treatment may not be random, I could not find any robust predictors of which geographic areas receive EGF money. Arguably, this is because the extra compensation is not allocated based on geography. As a result, at the geographic level the treatment may be considered to be “as if” random.

This within-country variation allows me to estimate the effect of extra compensation on support for right-wing populists. To do so, I use a difference-in-differences estimation strategy and compare changes in votes for the far-right party over time in “treated” and control areas. I identify treated areas as those in which workers displaced by globalization received extra compensation. Displaced workers directly benefit from the extra compensation. The local area also benefits because the local economy is boosted by re-employment, a better qualified and better educated work force, and fewer unemployed persons. Local labor markets, rather than individuals, are therefore the correct unit of analysis.

### **Case selection**

I focus on France for several reasons. First and foremost, some French workers receive extra compensation for their job losses while others do not. This within-country variation makes it possible to estimate the effect of (extra) compensation on support for far right populist parties, while holding country-specific characteristics constant. France was the first country to receive money from the EGF in 2007. In 2007, France applied for EGF assistance for 267 workers who had been employed by Peugeot suppliers and lost their jobs due to trade. Since 2007, France has filed 9 successful applications. These applications cover workers in 18 sub-national regions (departments). These workers had been employed in

various industries including air transport, automotive, road transport, and slaughterhouses. The number of affected jobs range from 267 to 4744 per case, with an average of 2215 jobs lost per case. The amount of extra assistance per person ranged from 1208 to 9581 euros with an average of 5062 euros per case.

Several additional reasons exist to study France. First, France exhibits exceptionally diverse economic developments across regions (Combes, Lafourcade and Mayer (2005), Frocrain, Pierre-Noël Giraud 2017). As a result, globalization hits some areas harder than others. Second, France experienced the forceful mobilization of a far right populist party (Kriesi et al. 2006). France's far right party is the oldest and probably the most successful new party of the populist right (Kriesi et al. 2006). Third, the Front National tends to fare better in French localities exposed to low-wage country import competition (Malgouyres 2017). Over the 1995-2012 period, exposure to low-wage country import competition increases the share of votes for Front National.

### **Research Design**

The appropriate unit of analysis to study the effects of international economic integration and ensuing economic shocks is the community, not the individual. Economic shocks have strong local effects. In France, for example, local employment and total labor income in both manufacturing and nonmanufacturing are negatively affected by rising exposure to imports (Malgouyres 2017). Trade shocks have local effects beyond manufacturing. In areas hit hard by foreign imports, workers are laid off and local businesses of all kinds suffer. Additionally, young people leave, real estate values plummet, and social services decline (Broz, Frieden and Weymouth 2019).

Local conditions affect the attitudes of individuals in at least two ways. First, people glean much of their information about the state of the economy, and of society more

generally, from their neighbors and their community. Second, individuals have a social, material, and psychological stake in their communities: they care about how their neighbors and fellow citizens fare, whether for altruistic or self-interested reasons (Ansolabehere et al. 2014, and Kiewiet and Lewis-Beck 2001).

Because globalization has local economic effects, I focus on sub-national geographic units. More precisely, départements are my unit of analysis. Départements are administrative sub-national geographic units. The original design of departments was economically motivated. The size of each département had to be such that it would be possible from any point inside the département to reach its capital city (usually centrally located) and come back within 48 hours. This meant, at a time when horses were the fastest mean of transport, that départements were typically organized within a radius of 30 to 40 kilometers around their capitals. Today, departments vary in size but are relatively small with a median population of 538,750.

Départements continue to represent meaningful lines of demarcation inside France for both economic activity and networks (Combes, Lafourcade, Mayer 2005). Each département usually has a chamber of commerce and industry. The départements with the largest cities or industrial bases have usually two. These chambers represent the “commercial and industrial interests of their jurisdiction” to the public authorities and are elected by the local business community. They provide services to local firms, including assistance with administrative procedures for the creation of a firm, data and expertise on local markets and potential suppliers, relationships with local authorities. They are consulted in the making of local public policies on numerous economic-related subjects (Combes, Lafourcade, Mayer 2005).

### **Empirical expectations and model**

My units of analysis are departments. In order to generate conservative estimates of the electoral returns to extra compensation, I construct a binary treatment indicator that indicates whether trade-displaced workers in a department received extra compensation for their job loss. More precisely, this variable takes the value of zero for control departments that did not receive any extra compensation and the value of one for departments in which trade-displaced workers received extra compensation. To construct the treatment indicator, I compile novel data from EU documents, GIS maps, and data from the Bureau van Dijk's Orbis database.

I consider  $i = (1, \dots, 96)$  departments in France. I examine the first round of voting in presidential elections for year  $t = (2007, 2012)$ . For each department, I calculate the change in vote shares for the far right party National Rally (RN) (*Rassemblement National*), which until June 2018 was known as the National Front (*Front National*). Because the elections I examine pre-date the party's name change, I refer to it throughout the paper as the National Front (*Front National*) or FN.

Given the myriad possible correlates of a party's vote shares, I use a difference-in-differences estimation strategy. The difference-in-differences estimator does not require the voting preferences in both groups of departments to be the same. The estimator compares the change in the choices of voters in treated departments between two elections with the change in the choices of voters in control departments. If extra compensation is provided to trade-displaced workers between the two elections and this event has an influence on voting decisions, a change in the voting patterns in treated departments will be observed but not in control departments. In other words, the difference-in-differences estimator compares the change in voting choices by both groups over time instead of comparing both groups directly in a particular period of time, which helps to rule out alternative explanations.

For a difference-in-differences estimator to be valid, several assumptions must hold – each of which I investigate empirically. First, I show that the treatment is unrelated to the outcome at baseline. In other words, the allocation of extra compensation was not determined by the far right’s vote share in the pre-treatment period. The FN’s vote shares in 2007 (and 2002) were not significantly different in treated and untreated departments, as illustrated by the balance tests in Table 2.

Second, the parallel trends assumption must hold for the difference-in-differences estimator to be valid. To probe the plausibility of this assumption, I first plot the FN’s average vote shares over time in treated and untreated departments. These trends are displayed in Figure 1 and show that the identification assumption holds. Prior to treatment, trends in the FN’s vote shares were parallel in treated and control groups. Second, I conduct a falsification test and estimate a placebo difference-in-differences regression with a similar specification for the earlier 2002 and 2007 pre-treatment elections. These results are reported in Table 1. I find that the FN’s vote shares in the treated and untreated departments follow a virtually identical trend prior to the treatment period.

The FN on average lost about 5.8 percentage points nationwide, but this decrease is identical in districts that are eventually treated between 2007 and 2012 and control districts that are not. The placebo effect is not statistically different from zero. The strikingly parallel trend of FN vote share in both groups in the pre-treatment period increases confidence in the identification assumption. Given the parallel trends in the pre-treatment period, it seems plausible to assume that, in the absence of any treatment, the group of treated and untreated districts would have continued on approximately parallel trends in the post-treatment period

## **Results**

The treated and untreated districts continue on parallel trends in the posttreatment period, as illustrated in Figure 1. The treatment had virtually no impact on FN vote shares in

treated departments. The treatment's null effect is confirmed by results reported in Table 3, which shows show the difference-in-differences estimates of the electoral impact of extra compensation for trade-related job losses as measured by the FN's vote shares from the first round of voting in the 2007 to 2012 presidential elections. Extra compensation does not significantly change the far right's vote share relative to areas without extra compensation. This null result holds for the benchmark fixed-effects equation, which includes fixed effects for departments. It also holds after a set of time-varying covariates are added to the benchmark fixed-effects equation to account for changes in observed district-level characteristics. They includes controls for sociodemographic shocks such as increased population. I also include a battery of controls for economic voting, such as the employment rate and GDP per capita. The null compensation effect remains even after the inclusion of these covariates. The estimated ATT is virtually identical across all of the estimated models. Although the estimated ATT is negatively signed, it never reaches conventional levels of statistical significance indicating that the far right's vote shares in treated and untreated departments continued on approximately parallel trends post-treatment.

In Table 3, the control group includes all departments that did not receive extra compensation. This includes departments without job losses as well as those with job losses that were not additionally compensated above and beyond the normal national levels. A fairer test of extra compensation may be comparing treated departments with those that experienced job losses but didn't receive any extra compensation. I therefore restrict the control group to include only those departments that experienced net job losses from 2007 to 2012 but did not receive any additional compensation. As an additional robustness check, I restrict the control group to only those departments that experienced a reduction in the employment rate between 2007 and 2012 but didn't receive any additional compensation. These results are reported in Tables 4 and 5 respectively.

As before, the FN's vote shares in treated and control districts continue on approximately parallel trends in the post-treatment period. Extra compensation does not significantly change the far right's vote share relative to areas without extra compensation – even those that experienced net job losses or lower employment rates.

Several other results deserve mention. Increases in departments' GDP per capita between 2007 and 2012 are negatively correlated with FN vote shares. Similarly, increases in the local employment rate between 2007 and 2012 are negatively correlated with FN vote shares. In sum, the far right appears to do relatively better with voters in areas experiencing poor economic conditions. Additional compensation to offset these negative economic shocks does not, however, retard the rise of the far right's electoral support.

### **Conclusion**

Increased compensation for globalization-related job losses appears to be ineffective in staunching the rise of far right parties. This finding provides an answer to the question – why didn't mainstream parties expand compensation to see off the far right? (Broz, Frieden and Weymouth 2019). My results suggest a simple response: expanded compensation does not work to see off the far right.

Several reasons may explain why compensation does not stem rising support for the far right. First, compensation may be insufficient to fully offset the costs of globalization. Even the extra compensation studied here is not overly generous. The median amount per job loss equals €5,720. And among worker that receive the additional assistance, only 60% of them found new jobs within 2 years (EC 2019).

Second, voters may worse off by globalization may not want compensation. Jobs have meaning beyond the income they provide (Kurer 2017). When people lose their jobs, they lose not only income but the meaning provided by their jobs. Job losses may therefore result

in status anxiety, which cannot be ameliorated by compensation. Compensation only deals with the material implications of negative income changes from redundancy – not the status anxiety.

If compensation no longer assuages voters made worse off by globalization, the “bargain of embedded liberalism” needs to be revisited. Embedded liberalism suggested that governments could compensate their citizens for the costs of economic openness and sustain public support for globalization by doing so. Yet citizens in areas hit hard by trade-related job losses increase their support for the far right. They do so even when additional compensation is provided to trade-displaced workers. The apparent inability of compensation to fully offset the costs of trade may explain why compensation has become less effective in sustaining public support for globalization in recent years.

Figure 1: FN vote shares over time, first round presidential election results

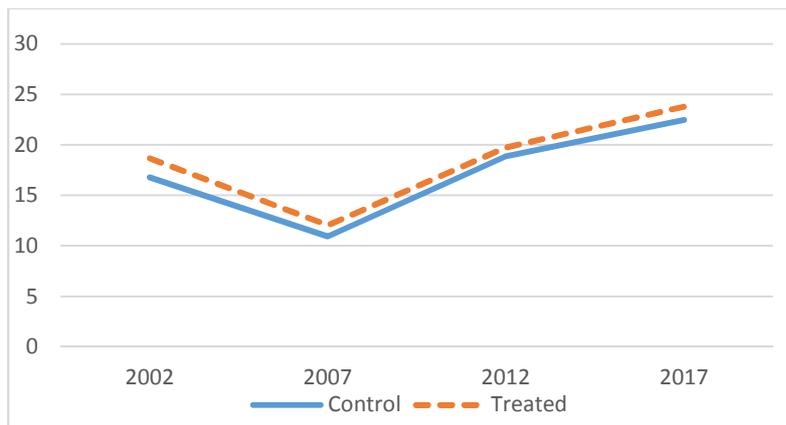


Table 1: Placebo test using 2007-12 treatment on 2002 and 2007 elections

	(1)	(2)	(3)	(4)	(5)
Treated 07-12	-0.790 (0.677)	-1.064 (0.717)	-1.089 (0.750)	-1.047 (0.758)	-1.020 (0.779)
Post period	-5.826*** (0.257)	-5.065*** (0.366)	-4.829*** (0.481)	4.289*** (0.584)	4.425*** (0.680)
Population (nl)		-22.383* (11.464)	-16.627 (14.925)	-25.359 (15.651)	-27.279 (17.198)
GDP (nl)			-4.762 (7.329)	4.745 (10.129)	6.640 (10.212)
GDP per capita				-0.000 (0.000)	-0.000 (0.000)
Employment rate					-0.117 (0.388)
Constant	16.929*** (0.121)	309.660** (149.985)	344.628** (157.441)	246.724 (177.496)	232.725 (163.997)
Observations	192	192	192	192	192
R-squared	0.864	0.872	0.872	0.875	0.876
Number of departments	96	96	96	96	96

Table 2: Pre-treatment balance

Variable	Year	Mean		Diff in Means	
		Control	Treatment	T-stat	P-value
Employment rate	2006	40.0	39.7	0.108	0.914
GDP per capita	2006	23999.9	24275.4	-0.097	0.922
GDP (nl)	2006	23.16	23.85	-2.13	0.036
Population (nl)	2006	13.05	13.7	-2.41	0.018
FN vote share	2002	16.77	18.67	-1.19	0.236
FN vote share	2007	10.94	12.06	-0.963	0.338

Table 3: Estimated effect of extra compensation on FN's vote shares, 2007-2012

	(1)	(2)	(3)	(4)	(5)
ATT	-0.218 (0.569)	-0.232 (0.564)	-0.319 (0.467)	-0.331 (0.495)	-0.324 (0.494)
Post period	7.924*** (0.170)	7.977*** (0.189)	7.428*** (0.355)	8.437*** (0.354)	8.466*** (0.413)
Population (nl)		-2.312 (5.513)	11.322 (8.064)	-20.286* (10.801)	-19.651* (10.745)
GDP (nl)			-8.454** (3.541)	20.631*** (7.695)	20.032*** (7.594)
GDP per capita				-0.001*** (0.000)	-0.001*** (0.000)
Employment rate					0.056 (0.261)
Constant	11.04*** (0.081)	41.35 (72.29)	59.07 (74.88)	-174.96* (93.77)	-171.74* (92.61)
Observations	192	192	192	192	192
R-squared	0.962	0.962	0.965	0.972	0.972
Number of department	96	96	96	96	96

Table 4: Control group = departments with reductions in employment rate, 2007-2012

	(1)	(2)	(3)	(4)	(5)
ATT	-0.219 (0.569)	-0.219 (0.568)	-0.248 (0.462)	-0.269 (0.489)	-0.286 (0.481)
Post period	7.925*** (0.169)	7.927*** (0.182)	7.293*** (0.391)	8.256*** (0.337)	7.909*** (0.434)
Population (nl)		-0.058 (5.891)	12.715 (7.944)	-16.412 (10.199)	-20.486* (10.414)
GDP (nl)			-9.297** (4.133)	18.196** (7.348)	21.058*** (7.338)
GDP per capita				- 0.001*** (0.000)	-0.001*** (0.000)
Employment rate					-0.438* (0.242)
Constant	10.96*** (0.081)	11.72 (76.95)	60.39 (80.31)	-171.04* (97.77)	-166.0* (97.83)
Observations	174	174	174	174	174
R-squared	0.966	0.966	0.969	0.976	0.976
Number of nuts3eucode	87	87	87	87	87

Table 5: Control group = departments with net job losses, 2007-2012

	(1)	(2)	(3)	(4)	(5)
ATT	-0.159 (0.598)	-0.165 (0.606)	-0.174 (0.548)	-0.047 (0.499)	0.028 (0.505)
Post period	8.086*** (0.156)	8.068*** (0.152)	7.741*** (0.375)	8.379*** (0.437)	7.730*** (0.470)
Population (nl)		2.092 (9.545)	9.489 (12.917)	-21.969 (20.883)	-30.125 (19.954)
GDP (nl)			-4.254 (4.349)	24.579 (16.897)	26.166 (15.903)
GDP per capita				-0.001 (0.001)	-0.001 (0.001)
Employment rate					- 0.627*** (0.234)
Constant	11.67*** (0.078)	-15.27 (123.01)	-12.95 (122.99)	-238.13 (181.29)	-146.51 (171.66)
Observations	106	106	106	106	106
R-squared	0.981	0.982	0.982	0.983	0.984
Number of departments	53	53	53	53	53