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Essays in political economy: Economic shocks and citizens' reactions and attitudes

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Abstract

My PhD thesis focuses on how economic shocks impact social attitudes and public policy preferences. The last two decades have seen considerable economic changes that changed both the structure of economies as well as the political and social landscapes. It is in that context that government have designed public policies. I aim to study this dynamic and in particular to address the following questions: How do people perceive economic shocks? What are people's expectations from the government in times of crisis? What types of public policies do people support or condemn?

I study these research questions in three papers, assembled here in three chapters.

My first paper looks at how Americans view automation via a survey experiment. I investigate why we have not seen an "automation" backlash even though the labor market consequences of automation and globalization have been similar. I find that respondents do not support more government intervention following an automation prime and identify two mechanisms. First, participants do not see automation as particularly unfair to workers and think that firms are justified in automating. Second, the automation treatment I use increases respondents' anxiety regarding automation's impact on American jobs in general but not on their own occupations. Hence, while an automation prime increases average anxiety levels, respondents do not feel personally threatened by robots.

My second paper looks at the impact of a cut in welfare benefits in the UK on a particular segment of the population: young people. In particular it examines whether and how they updated political attitudes following the 2012 Welfare Reform Act. I show that the welfare cuts had a negative effect on young people's opinion of politicians and made them more prone to political disengagement.

Finally, my third paper is co-authored with Laurence Boone and estimates the effects of non-pharmaceutical interventions (NPIs) and the COVID sanitary situation on mobility in advanced OECD countries. Overall, we find that restrictions affected mobility in advanced OECD economies much more than fears of COVID infection. This effect is more visible during the first wave than during the second as the influence of the sanitary situation gained weight during the second wave. This is probably due to (i) a second wave stronger than the first in many countries, (ii) while NPIs were, on average, less stringent during the second wave, leaving more room for voluntary social distancing.

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Introduction

This PhD thesis examines how citizens' react to economic shocks with a special emphasis on social and political attitudes. I focus on advanced economies and look at the effects of three economic shocks: automation, welfare cuts, and the COVID pandemic.

The first chapter studies how Americans perceive automation. Automation and globalization triggered sizeable labor market adjustments in the US. While the backlash against globalization has been well-identified in the political economy literature, this is not the case for automation. I study how Americans update their public policy preferences in the face of automation using a survey experiment. I show they do not support more government intervention and uncover two explanatory channels behind it: fairness considerations and perceived vulnerability to automation. First, respondents do not see automation as particularly unfair to workers and think that firms are justified in automating. Second, my automation treatment increases respondents' anxiety regarding automation's impact on American jobs in general but not on their own occupations. Hence, while an automation prime increases average anxiety levels, respondents do not feel personally threatened by robots. Finally, I find that respondents are less likely to support nationalist policies once they know the cause of the shock is automation. This suggests that misperceptions about the cause of the shock have partly driven the increase in anti-trade and anti-immigration sentiment following robot adoption.

The second chapter uses a difference-in-differences to study the impact of the 2012 British austerity policies on youth political attitudes. I do so by merging longitudinal survey data from Understanding Society with a district-level estimate of the austerity shock each individual was subject to over the years 2013 to 2015. My results suggest that austerity significantly affected political attitudes among the British youth. Young people's political efficacy diminished as they were more likely to believe public officials do not care about them and that they have no say in what the government does. In parallel, their sense of satisfaction with politics, measured, for example, by their perceived political influence, also decreased. Overall, my results show that the British youth were at risk of higher political disenfranchisement following the implementation of the austerity measures. These results provide grounds to see the welfare cuts as a factor in the lower engagement of young people in the Brexit referendum.

Finally, my last chapters analyses the effects of the COVID sanitary situation and non-pharmaceutical interventions (NPIs) on mobility in 29 advanced OECD countries, and compare compare the first wave of COVID to the second one. Overall, my co-author and I show that NPIs were the main explanatory factor behind the mobility reduction in advanced OECD countries during the first wave. The sanitary situation played a more important role during the second wave suggesting (i) a greater awareness of the severity of the health situation and/or (ii) an increase in individual responsibility, which was given more room as restrictions were less severe during the second wave. Focusing on 6 European countries in particular, we observe that those most affected during the first wave display higher elasticities to mobility restrictions, except for Italy where restrictions and the sanitary situation had similar impacts on mobility. Looking at the relative effects of different types of NPIs we see that more stringent measures had more impact on mobility. Nevertheless, we remain cautious regarding these last estimates as the rapid sequencing of NPIs likely implies issues of statistical identification.

Chapter 1

Automation and Public Policy Preferences

1.1 Introduction

Over the last decades, automation and globalization triggered sizeable labor market adjustments in the US. Work by Autor et al. (2013) has shown that rising exposures to Chinese import competition increased unemployment, reduced wages, and lowered labor force participation[1]. Prior to that, an important literature on skill-biased technological change described the phenomenon of job polarization that started in the 1980s (Autor et al., 2003, 2006), and recent estimates show that automation accounts for 50 to 70% of the surge in wage inequality in the US between 1980 and 2016 (Acemoglu and Restrepo, 2022). Overall, import competition and automation have been identified as primary drivers behind the decline in the labor share and the parallel increase in income inequality (Abdih and Danninger, 2017; Aum and Shino, 2020; Elsby et al., 2013).

While the economic consequences of these two shocks have been similar, they have triggered different political reactions. The backlash against globalization has been well-

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[1]Several papers later on confirmed those findings and provided further precisions on the causes and conditions under which trade increases inequality (Antràs et al., 2017; Burstein and Vogel, 2017; Costinot and Rodríguez-Clare, 2014; Ebenstein et al., 2015)

identified in the political economy literature in the form of growing anti-trade and anti-immigrant sentiments [2]. This is not the case for automation where no consensus on its political consequences has emerged.

Previous survey experiments find that automation primes generate limited push for compensation or government intervention (Di Tella and Rodrik, 2020; Gallego et al., 2022; Jeffrey, 2021; Jeffrey and Matakos, 2021; Zhang, 2019). Yet, cross-sectional survey data in Europe and a set OECD countries show that the perception of technology-related employment risks correlates with more demand for redistribution and government protection (Busemeyer et al., 2022; Thewissen and Rueda, 2019). Finally, others highlight that automation increases support for right-wing populist parties as well as limits on immigration and trade (Anelli et al., 2021; Frey et al., 2018; Im et al., 2019; Wu, 2021, 2022)[3].

I investigate this puzzle and provide a unifying framework on automation and public policy preferences that reconciles previous findings. I do so by running a survey experiment on a large representative sample of the US population where I prime them with a newspaper-style text on the restructuring of a company leading to substantial lay-offs. In one treatment arm, the article does not specify the cause of the lay-offs (control), in the other treatment arm, the article specifies that robots will replace workers (automation treatment). After the treatments, I measure support for various social, retaliatory, and nationalist policies as well as views on who should be responsible for helping negatively affected workers. I find an average null effect of the automation treatment: respondents do not update their policy preferences once they know that automation caused job displacement. I uncover two mechanisms behind this result. First, automation is not seen as particularly unfair, and respondents are more likely to think that firms are justified in automating in the automation treatment arm. Second, while respondents become more worried about automation in general, they do not believe that their own occupation is more likely to be automated. This indicates that, on average, they do not feel personally threatened when thinking about automation and, as a result, do not update their policy preferences.

I also test whether the previous experience of unemployment, trust in government, and views on government effectiveness play a mitigating or aggravating role in the demand for government intervention. I find it is not the case for the former two and that favorable views of government effectiveness lower the support for government intervention. Finally, I zoom in on respondents that declare to have been displaced by robots or foreign competition before. I show that they are more likely to support nationalist policies in the control group, but the effect disappears once they know the cause of the shock. This suggests that misperceptions regarding the cause of the shock partly drive the increase in

[2]See for example Autor et al. (2020), Colantone and Stanig (2018), Colantone et al. (2019), and Dorn et al. (2020)

[3]I note that Anelli et al. (2021) also observe that automation exposure increases support for the radical left, but the effect is significantly lower.

anti-trade and anti-immigration sentiments that follows an automation shock.

Contribution to the literature. This paper adds to a small but growing literature that uses survey experiments to study automation and public policy preferences in the US[4]. Zhang (2019) shows that informing respondents about their chances of losing their jobs to automation corrects their views on the threat that automation poses to American jobs. Nevertheless, it does not lead to a change in their preferences on redistribution, immigration and trade policies. Di Tella and Rodrik (2020) prime respondents with different types of labour market shocks and find that trade shocks trigger high demand for protectionism while automation ones have relatively small effects on the demand for transfers and protectionism. Using similar treatments, Wu (2022) obtains an average null impact of an automation prime on support for trade restriction and immigration, with heterogeneous effects along partisan lines. She also finds it increases the demand to restrict companies' use of new technology. Finally, Jeffrey and Matakos (2021) show that knowing about the risk automation poses to inequality has little effect on preferences for redistribution. Nevertheless, their informational treatment appears to change views on redistribution among those that recently experienced a negative labour market shock.

I contribute to the literature in several ways. First, I use a large representative sample of the US population to study the question of attitudes towards automation. With a sample size close to 3000, I can detect an effect size of 0.13 of standard deviation, as specified in the pre-analysis plan[5]. My sample is also representative of the US population in terms of gender, age, race, income, education, and region of residence. This enables us to confirm previous null effect findings of smaller sample sizes or non-representative samples. To my knowledge no other survey experiment of this type uses a representative sample. Jeffrey and Matakos (2021) are the only ones to attempt to do so using post-stratification weights.

Second, I focus on the mechanisms and test for four channels that could explain the lack of backlash against automation: fairness considerations, perceived vulnerability to automation, views on government effectiveness, and trust in government. I measure a range of pre- and post-treatment beliefs to test for heterogeneous effects and belief updating. I also run heterogeneity analysis by demographic characteristics. The aim is to examine whether competing mechanisms are at play.

Third, I benchmark my results against the baseline level of support for government intervention following a labor market shock of unknown cause. Studies so far look at differences between an automation treatment and either a placebo or a control group with no job losses. For instance, Di Tella and Rodrik (2020) and Wu (2022) also prime participants to think about different labor market shocks with newspaper style treatments

[4]Surveys have also been run in the UK and Spain which also show a subdued demand for compensation following an automation prime (see for example Gallego et al. (2022) and Jeffrey (2021)).

[5]With a power of 0.8 and a 0.05 significance level.

that narrate job losses in a manufacturing company. Yet, their control group is not exposed to factory closure or lay-offs. I aim here to isolate the reaction to the cause of the furlough and estimate the reaction to automation net of the effect of job loss itself.

Four, I measure support for a range of public policies and in particular try to see if the lack of demand for social policies is compensated by demand for more regulation or taxes on firms, or nationalist policies. I also test whether respondents believe the responsibility to help affected workers is not the government's but another entity's. This enables us to see whether the limited support for government intervention hides discontentment towards firms.

The paper is structured as follows: section 2 details the experimental design and sample recruitment, section 3 presents the results, and section 4 concludes.

1.2 Experimental Design and Sample

The experiment has three treatment arms: one control and two treatments. I describe the sample and structure of the experiment below.

1.2.1 Sample

I recruited respondents using Dynata, previously Research Now, an online research market company widely used in the literature (Haaland et al., Forthcoming; Stantcheva, 2022). I conducted the survey in August and September 2022[6]. A pre-analysis plan was submitted to the AEA RCT Registry prior to data collection[7], which specified the sample size, hypotheses, and empirical setting.

I applied quota samples to obtain a representative sample of the US population in terms of gender, age, race, income, education, and region. I dropped participants who did not pass the attention check, whose answers to the open-ended questions were non-sensical, flagged by Qualtrics as potential bots or duplicates, and whose completion time is below 3 minutes or above 60 minutes. In total, I end up with 2977 responses with a median completion time of 7.1 minutes. My sample matches the distributions of the US population (table A.1).

1.2.2 Experiment

I start the survey by asking respondents to complete a demographics questionnaire, ask if they have ever been unemployed before, and elicit pre-treatment beliefs on trust in

[6] I ran the survey in a post-pandemic era, which could influence perceptions of the labor market and the government. It should not be an issue to the extent that the treatment and control groups all suffer from the same bias. It could be an issue for the external validity of the results, although the average results are in line with previous findings.

[7] https://www.socialscienceregistry.org/trials/9836

government, government efficiency, and fairness. After eliciting these prior beliefs, I ask them about their political affiliation. Respondents are then randomly assigned to one of three treatment arms: a control, an automation treatment, and an automation treatment preceded by an anchoring of government effectiveness views. Table A.2 shows that my control and two treatment groups are balanced in terms of observable and reject an F-test of joint-significance.

1.2.3 Treatments

1.2.3.1 Treatment 1: Automation shock

Treatment 1 assigns respondents a short newspaper-style article to read. The article describes a food manufacturer that is restructuring and decided to lay off 20% of its labor force. The article specifies that the company will "automate part of its production with machines" and that "many employees [...] will likely become unemployed or have to take lower-paid jobs". Hence, the article clearly links robot adoption and job displacement.

1.2.3.2 Treatment 2: Government effectiveness + Automation shock

Treatment 2 begins by anchoring the level of government effectiveness in the US. Respondents read a short text about a government effectiveness ranking conducted by the World Bank every year [8]. The text describes the different items taken into account to build the ranking. Respondents then estimate how well the US ranked compared to the rest of the world in 2020, the last year of data available. They choose among four options: among the best, better than most, worse than most, and among the worst. After answering, the correct answer is shown to them, which is that the US ranked as one of the most effective governments in the world in 2020. I use a design similar to Kuziemko et al. (2015) where I ask respondents to give an estimate first and then give them the correct answer. This makes the treatment more salient.

After this "government effectiveness treatment", Respondents are shown the same automation newspaper-style article as in Treatment 1.

1.2.3.3 Control

The control group is what Haaland et al. (Forthcoming) describe as an active control group. Respondents are shown a newspaper article similar to treatment 1's, except it does not specify the cause of the lay-off. Hence, respondents are treated with a negative labor market shock but do not know the reason for it.

[8]Data available at `http://info.worldbank.org/governance/wgi/`. See Kaufmann et al. (2010) for more details

1.2.4 Outcomes

1.2.4.1 Self-Reported Policy Preferences

I measure people's preferences on a five-point Likert scale[9] for six policies:

1. Providing re-training programs
2. Providing financial assistance to workers who lose their jobs
3. Increasing taxes for firms
4. Regulating the ways in which firms operate
5. Raising tariffs on foreign goods
6. Restricting immigration to the US

The policies are shown in random order to respondents. To limit the issue of multiple hypothesis testing, I create indices following Anderson (2008). I group policies 1 and 2 as social policies, 3 and 4 as retaliatory ones[10], and 5 and 6 as nationalist ones. In particular, I interpret an increase in support for policies 3 and 4 as ways to punish firms

I also measure people's views on who should be responsible for helping displaced workers. I follow the OECD's Risk that Matters survey (2021) and measure it on a four-point Likert scale[11] for the following entities:

1. Firms, businesses, and employers
2. Civil society groups, such as professional associations, non-profit organizations, and charitable organizations
3. The national government
4. Individual workers themselves

These entities are also displayed in random order to each survey participant.

[9] Strongly oppose, Somewhat oppose, Neither oppose nor support, Somewhat support, Strongly support

[10] I depart slightly from the pre-analysis plan, which interpreted policies 3 and 4 as regulatory. In line with Di Tella et al. (2016), I interpret an increase in support for policies 3 and 4 as ways to punish firms. In particular, I see an increase in support for taxing firms as a retaliatory measure rather than an increase in support for redistribution

[11] Definitely should not be responsible, Probably should not be responsible, Probably should be responsible, Definitely should be responsible

1.2.4.2 Mechanisms

I try to uncover some mechanisms behind the policy preference outcomes and do so by two means. First, I conduct a heterogeneity analysis according to some pre-treatment variables and check whether respondents who view inequality as an issue, have been employed before, trust the government, and have positive views on government efficiency react differently to the treatments. Second, I investigate whether the treatments affect participants' perceptions of fairness, vulnerability to automation and globalization, views of government efficiency, and trust in government. To minimize the risk that respondents realize I are trying to measure whether they update these beliefs, I word the questions differently than the pre-treatment ones (see questions in sub-sections A11 and A12 in section A.3).

1.2.4.3 Definition of outcome variables

I consider all the self-reported measures on a Likert scale as continuous and standardize them. In particular, I subtract the control group mean and divide it by the control group standard deviation for each observation. As specified earlier, I also create indices of the support for different types of public policies. Finally, I code dichotomous variables as dummies.

Details on the treatment and the survey questions are available in Appendix A.3 and a summary chart in Figure A.1.

1.3 Results

1.3.1 Determinants of support for intervention following a negative labor market shock

I first look at the baseline support for each type of public policy following a negative labor market shock of unspecified cause. Figure 1.1 shows that all policies have more supporters than opponents. Retraining programs and financial assistance to displaced workers are the most popular policies, with close to 75% of the sample strongly supporting or somewhat supporting these two policies. The other four policies - regulations, taxes, restrictions on immigration, and tariffs - while not as popular, still exhibit a larger share of respondents that support rather than oppose. They also have a significant percentage of respondents that neither oppose nor support the policy (between 25 and 35%).

I also find that participants' support for different types of public policies varies significantly with their background characteristics (Table 1.1). Republicans and Democrats exhibit pronounced differences, and so do Republicans and Independents, but to a lower extent. Other significant characteristics include gender, age, whether one is born in the

US, whether one is divorced or single, the number of children, and living in a Southern State. The ethnic background does not correlate with the support for some policies, but Hispanic respondents are less likely to favor immigration restrictions. Employment status does not display any strong pattern. I only note that part-time employees and the unemployed are less likely to support tariffs than full-time employees. While currently unemployed respondents do not exhibit a different level of policy support compared to full-time employees, respondents that have been unemployed before are more likely to support financial assistance for displaced workers and more taxes on firms.

Pre-treatment beliefs are strong predictors of supporting specific policies. Trust in government increases support for more regulation and taxes on firms, while people who view the government as efficient are more likely to support all types of policies. Believing that inequality is unfair also correlates with more support for all policies but restrictions on immigration.

I then look at who respondents believe should be responsible for helping workers affected by the adverse labor market shock (Figure 1.2). On average, most respondents believe that the government, civil society, individuals, and firms have a responsibility. Nevertheless, the share of respondents that think firms should be held responsible is significant, with 84% of the sample that answered that firms should probably or definitely be responsible. The government is also seen as responsible by more than 70% of respondents, while the share decreases to 64% for individuals and 56% for civil society.

Fewer background characteristics significantly correlate with views on who should be responsible for helping displaced workers (Table 1.2). Those variables are also more staggered over categories. It is worth noting that those born in the US are less likely to think the government and civil society should be responsible, Blacks and Asians are more likely to declare individuals should be held responsible, and Democrats are more likely to say the government, civil society, and firms should be responsible.

Pre-treatment beliefs have, again, important explanatory power. Trust in government decreases support for firms' responsibility, positive views on government efficiency raise support for all entities' responsibility, and seeing inequality as unfair correlates with more support for the responsibility of the government, civil societies, and firms and less support for an individual one.

Overall, these results show that pre-treatment beliefs are essential in explaining support for the types of public and who implements them. In the rest of the analysis, and as specified in the pre-analysis plan, I control for all the background characteristics and pre-treatment beliefs presented here.

Figure 1.1: Support for public policies following a negative labor market shock

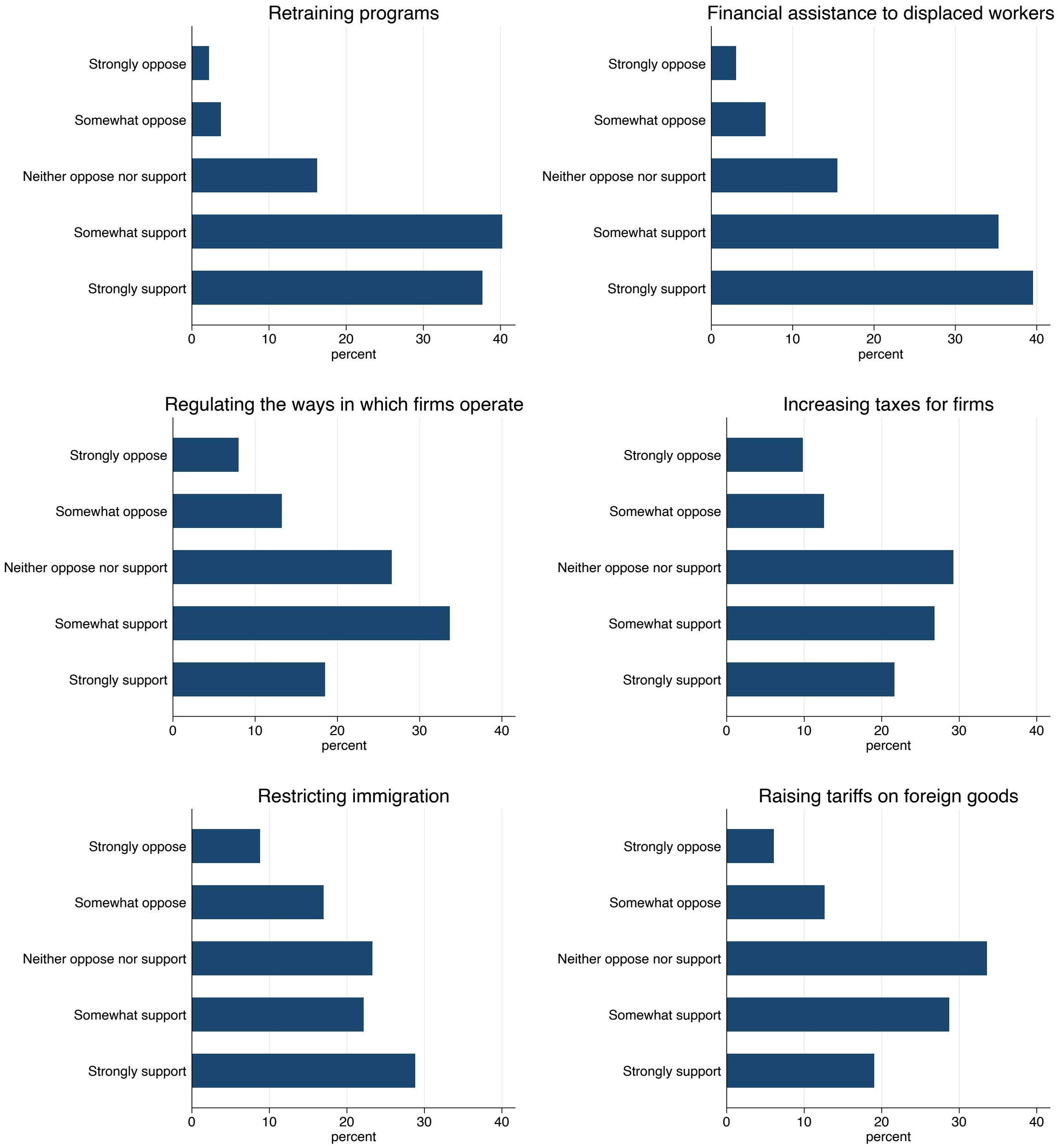


Table 1.1: Covariates of support for public policies following a negative labor market shock

	(1) Retraining	(2) Financial Assistance	(3) Regulate	(4) Tax Firms	(5) Restrict Immigration	(6) Tariffs
Female	0.11*	0.20***	0.11	0.17**	-0.02	0.03
	(1.73)	(3.05)	(1.47)	(2.23)	(-0.26)	(0.34)
Age Category	0.09***	0.03	-0.01	0.05*	0.11***	0.08**
	(3.48)	(1.07)	(-0.43)	(1.72)	(3.02)	(2.53)
Household Income	0.09**	0.03	0.01	0.09*	0.03	0.04
	(2.20)	(0.65)	(0.21)	(1.86)	(0.57)	(0.87)
Highest education level	0.00	-0.04	-0.02	0.05*	-0.03	-0.00
	(0.10)	(-1.57)	(-0.76)	(1.73)	(-0.85)	(-0.16)
Born in the US	-0.27**	-0.20	-0.15	0.04	0.19	0.14
	(-2.04)	(-1.27)	(-0.90)	(0.22)	(1.06)	(0.79)
Ethinicity/Race (ref: White Only)						
Black Only	0.03	0.14	0.02	0.04	-0.02	-0.06
	(0.27)	(1.30)	(0.15)	(0.34)	(-0.16)	(-0.46)
Asian Only	-0.01	-0.02	0.10	-0.06	-0.31	-0.08
	(-0.10)	(-0.10)	(0.50)	(-0.27)	(-1.54)	(-0.47)
Mixed	-0.06	-0.05	0.19	0.22	0.04	0.12
	(-0.54)	(-0.37)	(1.39)	(1.49)	(0.23)	(0.71)
Hispanic	0.09	0.01	-0.02	-0.07	-0.56***	-0.24
	(0.77)	(0.10)	(-0.15)	(-0.45)	(-3.21)	(-1.43)
Marital Status (ref: Married)						
Single	0.16**	0.16*	0.20**	0.12	-0.02	-0.04
	(2.12)	(1.93)	(2.04)	(1.23)	(-0.18)	(-0.45)
Divorced	0.17*	0.18*	0.14	0.22*	-0.05	0.28**
	(1.83)	(1.86)	(1.29)	(1.81)	(-0.45)	(2.47)
Widowed	-0.06	-0.10	0.05	0.19	0.14	0.13
	(-0.35)	(-0.51)	(0.29)	(1.12)	(0.71)	(0.80)
Nb of children	0.06**	0.10***	0.16***	0.02	0.06	-0.01
	(2.05)	(2.78)	(4.34)	(0.56)	(1.24)	(-0.28)
Employment Status (ref: Full-time employee)						
Part-time employee	0.14	-0.04	-0.02	-0.17	-0.17	-0.25*
	(1.41)	(-0.34)	(-0.14)	(-1.29)	(-1.22)	(-1.93)
Self-empl. w dependent workers	0.09	0.06	0.05	-0.01	0.13	-0.14
	(0.58)	(0.32)	(0.27)	(-0.04)	(0.58)	(-0.69)
Self-empl. w/o dependent workers	0.00	-0.00	0.03	-0.10	0.13	-0.14
	(0.02)	(-0.01)	(0.11)	(-0.39)	(0.45)	(-0.43)
Unemployed	0.13	0.14	0.21	-0.14	-0.26	-0.26*
	(0.95)	(0.97)	(1.36)	(-0.91)	(-1.57)	(-1.69)
Student	0.27*	0.12	-0.10	-0.09	-0.19	-0.17
	(1.89)	(0.66)	(-0.40)	(-0.39)	(-0.69)	(-0.72)
Not in the labor force	0.09	0.07	0.08	0.05	-0.08	-0.10
	(1.02)	(0.75)	(0.77)	(0.48)	(-0.70)	(-0.99)
Political Afficliation (ref: Republican)						
Democrat	0.21***	0.33***	0.40***	0.60***	-1.00***	-0.21***
	(3.27)	(4.61)	(5.20)	(7.33)	(-11.39)	(-2.64)
Independent	0.27***	0.27**	0.11	0.15	-0.81***	-0.39***
	(2.78)	(2.38)	(0.89)	(1.16)	(-5.45)	(-3.16)
Other	-0.15	0.04	-0.17	-0.13	-0.76***	-0.67***
	(-0.76)	(0.20)	(-0.86)	(-0.56)	(-3.83)	(-3.51)
Region (ref: MidWest)						
NorthEast	-0.09	-0.01	-0.03	-0.08	0.03	-0.10
	(-1.07)	(-0.06)	(-0.25)	(-0.72)	(0.29)	(-0.88)
West	-0.15	0.01	-0.10	-0.10	-0.05	-0.20*
	(-1.60)	(0.08)	(-0.95)	(-0.85)	(-0.43)	(-1.89)
South	-0.15**	-0.08	-0.16*	-0.15	-0.07	-0.12
	(-2.08)	(-1.01)	(-1.78)	(-1.62)	(-0.80)	(-1.43)
Pre-Treatment Beliefs & Work Experience						
Trusts the govt	0.09	0.09	0.24***	0.17**	0.05	0.01
	(1.37)	(1.20)	(3.18)	(2.06)	(0.56)	(0.10)
Govt is efficient	0.36***	0.42***	0.58***	0.28*	0.57***	0.57***
	(3.32)	(3.41)	(4.58)	(1.91)	(3.29)	(3.72)
Inequality is unfair	0.51***	0.56***	0.60***	0.69***	-0.09	0.15**
	(8.08)	(7.95)	(8.24)	(9.03)	(-1.07)	(2.03)
Unemployed before	0.04	0.15*	0.05	0.22***	0.02	0.05
	(0.53)	(1.85)	(0.58)	(2.66)	(0.20)	(0.66)
Constant	2.30***	2.93***	2.61***	0.88	3.33***	2.71***
	(4.56)	(5.15)	(4.53)	(1.43)	(4.94)	(4.48)
Observations	988	989	989	989	989	989

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 1.2: Responsibility to help displaced workers following a negative labor market shock

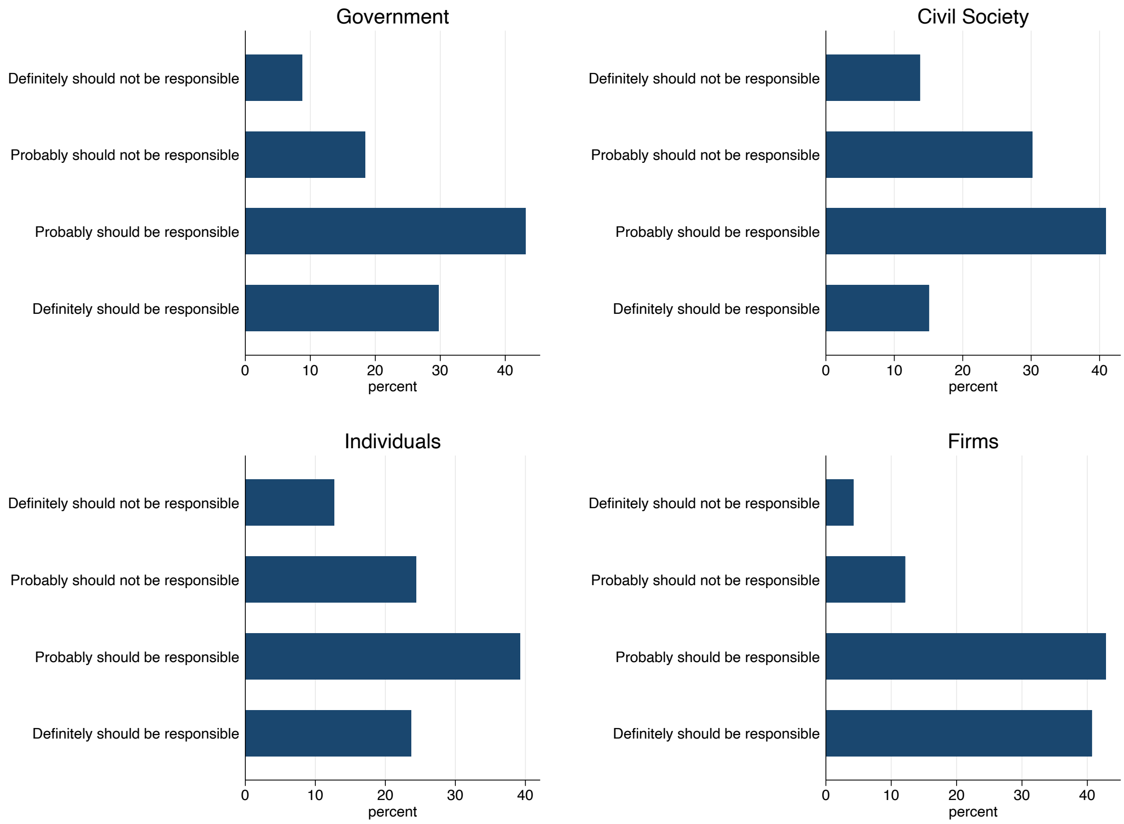


Table 1.2: Covariates of institutional responsibility to help displaced workers following a negative labor market shock

	(1) Government	(2) Civil Society	(3) Individuals	(4) Firms
Female	0.00	0.05	-0.16**	0.04
	(0.07)	(0.81)	(-2.48)	(0.78)
Age Category	-0.04	-0.03	-0.01	0.03
	(-1.50)	(-1.16)	(-0.37)	(1.27)
Household Income	0.03	-0.02	0.07*	0.04
	(0.69)	(-0.59)	(1.70)	(1.23)
Highest education level	-0.01	0.02	0.02	0.02
	(-0.37)	(0.94)	(0.84)	(0.93)
Born in the US	-0.34***	-0.27*	0.08	-0.07
	(-2.61)	(-1.90)	(0.51)	(-0.56)
Ethinicity/Race (ref: White Only)				
Black Only	0.12	-0.07	0.17*	-0.11
	(1.46)	(-0.73)	(1.73)	(-1.34)
Asian Only	-0.20	-0.09	0.28*	0.01
	(-1.22)	(-0.55)	(1.66)	(0.09)
Mixed	0.04	0.04	-0.16	-0.09
	(0.29)	(0.29)	(-1.13)	(-0.82)
Hispanic	-0.02	-0.06	0.05	0.10
	(-0.17)	(-0.51)	(0.35)	(0.95)
Marital Status (ref: Married)				
Single	0.15**	-0.18**	-0.20**	-0.01
	(2.01)	(-2.28)	(-2.35)	(-0.14)
Divorced	0.15*	-0.08	-0.11	-0.01
	(1.65)	(-0.81)	(-1.02)	(-0.17)
Widowed	-0.21	-0.05	0.06	-0.11
	(-1.23)	(-0.33)	(0.31)	(-0.88)
Nb of children	0.10***	-0.01	-0.06*	-0.00
	(3.38)	(-0.42)	(-1.81)	(-0.13)
Employment Status (ref: Full-time employee)				
Part-time employee	-0.03	-0.12	-0.17	-0.03
	(-0.35)	(-1.21)	(-1.47)	(-0.30)
Self-empl. w dependent workers	-0.14	-0.08	-0.17	-0.34**
	(-0.78)	(-0.56)	(-0.87)	(-2.16)
Self-empl. w/o dependent workers	0.03	0.06	-0.01	-0.02
	(0.21)	(0.28)	(-0.02)	(-0.10)
Unemployed	0.27**	-0.16	-0.29**	-0.01
	(2.35)	(-1.26)	(-2.20)	(-0.07)
Student	0.05	0.08	0.05	0.15
	(0.30)	(0.53)	(0.33)	(1.00)
Not in the labor force	0.05	-0.16*	-0.09	0.01
	(0.60)	(-1.85)	(-0.95)	(0.18)
Political Afficliation (ref: Republican)				
Democrat	0.18***	0.18***	0.06	0.23***
	(2.77)	(2.76)	(0.84)	(4.14)
Independent	0.09	0.07	0.03	0.14
	(0.84)	(0.67)	(0.28)	(1.50)
Other	0.05	-0.38**	-0.01	-0.37*
	(0.23)	(-2.07)	(-0.03)	(-1.90)
Region (ref: MidWest)				
NorthEast	-0.07	-0.14	-0.07	-0.06
	(-0.87)	(-1.62)	(-0.68)	(-0.81)
West	-0.07	-0.10	-0.03	0.01
	(-0.78)	(-1.14)	(-0.32)	(0.11)
South	-0.06	-0.11	0.01	-0.04
	(-0.86)	(-1.54)	(0.07)	(-0.54)
Pre-Treatment Beliefs & Work Experience				
Trusts the govt	0.07	0.10	-0.03	-0.12*
	(1.03)	(1.63)	(-0.36)	(-1.92)
Govt is efficient	0.37***	0.58***	0.39***	0.21**
	(3.79)	(5.57)	(3.24)	(2.43)
Inequality is unfair	0.46***	0.18***	-0.16**	0.39***
	(7.43)	(2.93)	(-2.36)	(6.94)
Unemployed before	-0.01	-0.02	0.08	0.03
	(-0.09)	(-0.24)	(1.10)	(0.60)
Constant	2.58***	3.08***	2.11***	2.28***
	(5.62)	(6.41)	(4.14)	(5.56)
Observations	986	987	987	987

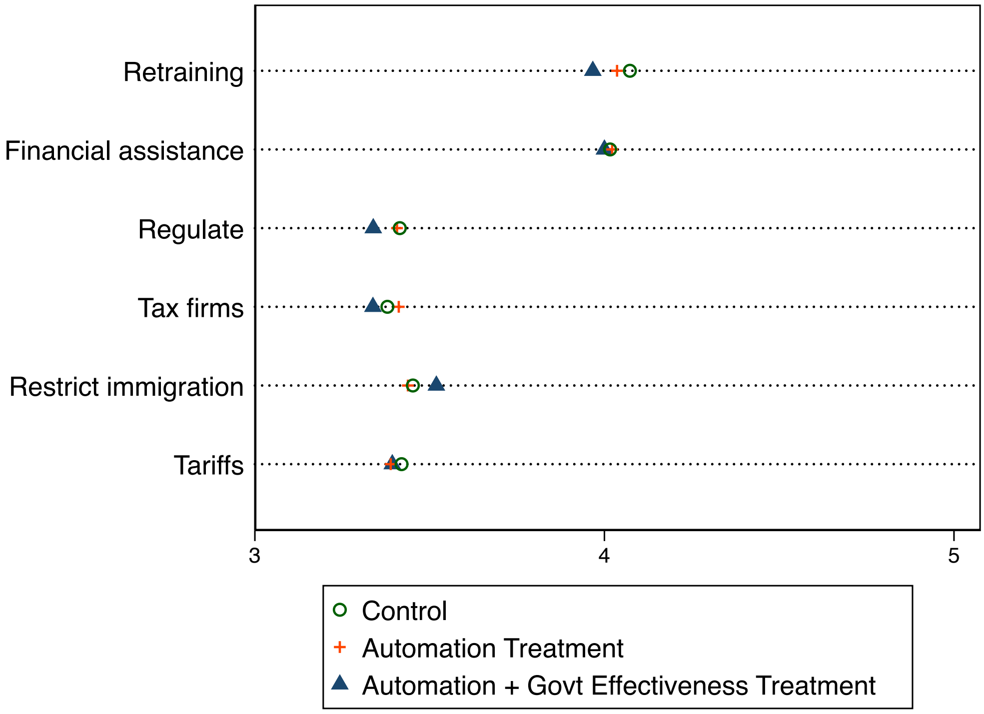
t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

1.3.2 Do participants respond differently to an automation shock?

I investigate whether respondents update their public policy preferences and views on who should be responsible for helping displaced workers when I specify that automation is the cause of the shock.

Figures 1.3 and 1.4 plot the average level of support for each policy and each entity for the control and each treatment. They show minimal differences in means according to the treatment group, suggesting that knowing automation is the cause of the shock has no effect.

Figure 1.3: Average support for public policies by treatment group



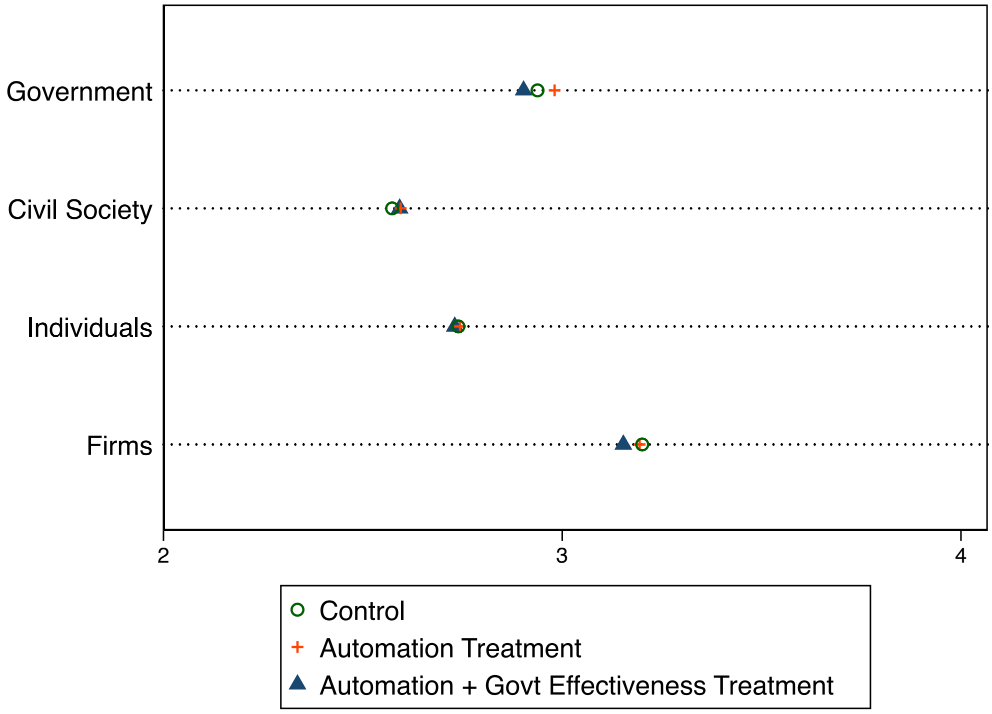
As specified in the pre-analysis plan, I then estimate the following equation:

$$y_i = \alpha_0 + \alpha_1 T1_i + \alpha_2 T2_i + \boldsymbol{A^T X_i} + \epsilon_i \tag{1.1}$$

where

- y_i is the outcome of interest
- $T1_i$ is an indicator for whether respondent i received treatment 1, i.e. only the automation treatment
- $T2_i$ is an indicator for whether respondent i received treatment 2, i.e. the government effectiveness + automation treatment

Figure 1.4: Average support for entities' responsibility by treatment group



- $\boldsymbol{X_i}$ is a vector of pre-specified controls (as described in Section 1.3.1) [12]
- $\epsilon_{j,i}$ is an individual-specific error term. I use robust standard errors for all specifications.

I first focus on the effect of treatment one only, i.e., the automation treatment.

Table 1.3 displays results for the post-treatment support for public policies and Table 1.4 for the views on who should be responsible [13].

I see no significant increase in support for social, retaliatory, or nationalist policies due to the automation treatment. The automation treatment slightly increases the share of people that believe the government should be responsible for helping displaced workers by 0.07 of a standard deviation. Nevertheless, the result is only significant at the 10% level. There is no discernible effect on other entities.

Overall, these results confirm the average null effect of an automation prime on public policy preferences (Jeffrey, 2021; Jeffrey and Matakos, 2021; Wu, 2022; Zhang, 2019). Respondents do not favor any particular type of public policy once knowing that robots displace workers and are marginally more likely to see the government as responsible for helping affected workers. Interestingly, the treatment does not affect views on firms'

[12] I also report the results without controls in the appendix.

[13] A breakdown of the impact for each policy is available in Table A.3. As specified in my pre-analysis plan, I also run the regressions without controls, and the results remain unchanged (see Tables A.4, A.5, and A.6)

responsibility. Some ceiling effects could be at play as more than 80% of the control sample already sees firms as having a responsibility to help in the case of a labor market shock of unspecified causes.

I investigate potential reasons for this lack of reaction in the next section.

Table 1.3: Effect of the automation treatment on public policy preferences

	(1) Social Policies Index	(2) Retaliatory Policies Index	(3) Nationalist Policies Index
Automation Treatment	0.0016	0.032	-0.034
	(0.04)	(0.83)	(-0.82)
Controls	Yes	Yes	Yes
Observations	2976	2977	2977

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.4: Effect of the automation treatment on views on who should be responsible to help affected workers

	(1) Government	(2) Civil Society	(3) Individuals	(4) Firms
Automation Treatment	0.071*	0.039	0.0073	0.0028
	(1.77)	(0.94)	(0.17)	(0.07)
Controls	Yes	Yes	Yes	Yes
Observations	2970	2973	2973	2972

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

1.3.3 Explanatory mechanisms: fairness considerations and perceived vulnerability to automation

I identify two main explanatory mechanisms behind the absence of "automation backlash": fairness consideration and perceived vulnerability to automation.

1.3.3.1 Fairness considerations

I test whether fairness considerations play a role by (i) looking at heterogeneous effects by pre-treatment views on fairness and (ii) measuring fairness considerations after the treatment.

I measure pre-treatment views on fairness by asking respondents how much they agree with the statement, "In the United States, the economic differences between the rich and the poor are unfair". I take this question from Almås et al. (2021)'s survey questionnaire, which was implemented in collaboration with Gallup. Almås et al. (2021)'s paper measures to which extent global inequality differences relate to fairness views of people in each country. I code it as one if the respondent answers "Strongly agree" or "Agree" and zero otherwise. I use this variable as a proxy measure of fairness views in the US.

Tables 1.5 and 1.6 show that those who see inequality as unfair do not respond differently to the automation treatment[14]. I note that while the interaction of pre-treatment fairness views and the automation treatment for civil society is not significant in Table 1.6, it causes the main coefficient to become positive and significant. In other words, respondents who do not see the gap between the rich and poor as unfair are more likely to think civil society should be responsible for helping affected workers in the case of an automation shock. The coefficient is only significant at the 10% level but highlights that these respondents tend to place responsibility on non-governmental organizations rather than the government or firms. Interestingly, civil society is the only variable affected. In particular, respondents who believe inequality is unfair do not view the government and firms as more responsible.

These results suggest that pre-treatment views on fairness do not interact with the automation treatment in any meaningful way. Viewing inequality as unfair does not induce any significant differential reaction to the automation shock.

Table 1.5: Heterogeneous effects by fairness views - Support for policies

	(1) Social Policies Index	(2) Retaliatory Policies Index	(3) Nationalist Policies Index
Automation Treatment	0.021	0.088	0.021
	(0.29)	(1.35)	(0.33)
Automation Treatment × Inequality is unfair=1	-0.031	-0.090	-0.087
	(-0.36)	(-1.12)	(-1.05)
Controls	Yes	Yes	Yes
Observations	2976	2977	2977

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.6: Heterogeneous effects by fairness views - Responsibility to help affected workers

	(1) Government	(2) Civil Society	(3) Individuals	(4) Firms
Automation Treatment	0.093	0.12*	0.030	0.045
	(1.30)	(1.71)	(0.42)	(0.75)
Automation Treatment × Inequality is unfair=1	-0.034	-0.13	-0.037	-0.068
	(-0.40)	(-1.45)	(-0.41)	(-0.93)
Controls	Yes	Yes	Yes	
Observations	2970	2973	2973	2972

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

I then test whether fairness considerations are affected by the treatment. After measuring participants' policy preferences, I measure whether they view the automation shock as more unfair compared to other shocks. I do this with two questions. First, I ask

[14]Results by type of public policy are displayed in Table A.7.

participants to what extent they think it is unfair for those workers to lose their jobs. Participants answer on a five-point Likert scale from completely unfair to completely fair. I adapt this question from a survey question in Hvidberg et al. (2020) 's paper on social positions and fairness views on inequality[15]. Second, I ask participants to choose between two statements: that firms are justified in replacing humans with robots if they can receive better work at a lower cost, or that there should be limits on how many jobs businesses can replace with robots[16]. This question is extracted from Pew's 2017 survey on automation.

Table 1.7 shows that respondents do not see automation as being more unfair than a labor market shock of unknown cause: column 1 displays no effect of the automation treatment on fairness views. In addition, column 2 provides evidence that respondents believe that firms are justified in automating. The automation treatment increases the probability that a respondent declares that firms are justified in automation by 4.2 percentage points. This compares to a 35.7% share of respondents that choose this statement in the control group. Hence it consists of an 11.8% rise compared to the control group, a non-trivial increase.

Overall, these findings indicate that the respondents do not view automation as particularly unfair and are more likely to see just cause for the displacement of workers by robots after the automation treatment. Hence, fairness considerations are one explanatory channel behind the lack of reaction to the automation treatment.

Table 1.7: Effects on post-treatment fairness views

	(1) Unfair to lose job	(2) Firms are justified in automating
Automation Treatment	-0.0014	0.042**
	(-0.03)	(2.06)
Controls	Yes	Yes
Observations	2971	2965

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

1.3.3.2 Perceived vulnerability to automation

I then focus on the role of automation anxiety and whether it plays a mediating role in supporting intervention following an automation shock.

I first try to see whether there are heterogeneous effects according to one's previous experience of economic hardship. Jeffrey and Matakos (2021) find that those displaced or temporarily suspended from work during the COVID-19 pandemic are more likely to

[15]In particular, I adapt it from the question: *On a scale from 1 to 7 where 1 is "Completely fair", 4 is "Neither fair nor unfair" and 7 is "Completely unfair", indicate to what extent you think that it is fair or unfair that there are differences in income among people born the same year as you WITHIN the following groups that you are yourself a part of ?*

[16]See appendix A.3 for details on the questions.

react to a treatment highlighting automation's inequality implications. Therefore, prior to the treatment, I ask respondents whether they have ever been unemployed before. I create a dummy equal to one if the person has been unemployed before and interact it with the automation treatment.

Table 1.8 shows that individuals who have been unemployed before are less likely to support retaliatory policies as a result of the automation treatment. The opposite is true for individuals who have never been unemployed before, as adding the interaction term renders the main coefficient positive. Preferences for social and nationalist policies remain unaffected. I observe no heterogenous effect regarding who should be responsible for helping affected workers (Table 1.9).

A look at the breakdown by types of public policies (Table A.8) shows that the item "Increasing taxes on firms" drives this result. Individuals who have never been unemployed increase their support for the taxation of firms, while others do not (as reflected by the sum of 0.14 - 0.15). Hence, individuals that have never been unemployed seem to retaliate against firms in the form of an increase in support for corporate taxation[17]. Two interpretations are possible regarding the absence of reaction among individuals that have been unemployed before. They do not increase their demand for taxes (i) because they are more fearful of the potential negative consequences of an increase in taxation on their employment prospects or (ii) because they are already supportive of an increase in taxation regardless of the source of the shock. Table 1.1 provides evidence in favor of the latter, as individuals that have been unemployed are more likely to support increasing taxes on firms in the control group.

This result contrasts with Jeffrey and Matakos (2021)'s, who find that respondents affected by the COVID-19 crisis were more likely to support redistributive policies after their treatment. I first note that my results compare how people react to different shocks, one of unknown causes vs. one due to automation. Jeffrey and Matakos (2021) use a placebo control that does not mention any economic shock. Hence, they partly pick up an effect that is a reaction to the negative economic shock rather than automation being the driver. I show in Figure 1.1 that the support for social policies is already high in the control group and that having experienced economic hardship increases the support for financial assistance to workers (Table 1.1). Second, Jeffrey and Matakos (2021) leverage on a recent shock. Their survey takes place during the COVID pandemic and asks participants whether they have been laid off or temporarily suspended due to the COVID crisis. My measure of economic vulnerability is much looser as I include respondents that have been unemployed at any point in their lives. Hence, the distress unemployment can cause is probably less vivid in respondents' minds.

On average, the heterogeneous analysis according to the previous experience of un-

[17]The interpretation of a rise in support for taxation as a retaliatory act is further supported by the lack of heterogeneous effect on the responsibility of firms to take care of displaced workers.

employment suggests it does not lead to more support for intervention in the case of automation.

Table 1.8: Heterogeneous effects by previous unemployment experience - Support for policies

	(1) Social Policies Index	(2) Retaliatory Policies Index	(3) Nationalist Policies Index
Automation Treatment	0.054	0.14**	0.042
	(0.73)	(1.97)	(0.56)
Automation Treatment × Unemployed before=1	-0.078	-0.15*	-0.11
	(-0.88)	(-1.86)	(-1.25)
Controls	Yes	Yes	Yes
Observations	2976	2977	2977

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.9: Heterogeneous effects by previous unemployment experience - Responsibility to help affected workers

	(1) Government	(2) Civil Society	(3) Individuals	(4) Firms
Automation Treatment	0.053	-0.0074	0.0026	0.11
	(0.73)	(-0.10)	(0.03)	(1.42)
Automation Treatment × Unemployed before=1	0.027	0.0100	0.055	-0.15
	(0.31)	(0.11)	(0.61)	(-1.59)
Controls	Yes	Yes	Yes	Yes
Observations	2970	2972	2973	2973

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

I then check whether the treatments increase participants' perceived vulnerability to an automation shock. In particular, I ask them to estimate the impact of new technologies on (i) them and their own job or career and (ii) American workers in general. Participants answer on a five-point Likert scale from very negative (=1) to very positive (=5).

Table 1.10 shows that, following the automation treatment, respondents are more likely to declare that technology negatively impacted American jobs but not their own job or career. Hence, while participants are more anxious following the treatment, they appear to view their own occupation as insulated from an automation shock. It echoes Smith and Anderson (2017) and Zhang (2019) who find that while Americans are worried about automation "in general", they feel relatively optimistic when it comes to their own occupation. Hence, as participants do not feel personally affected, they are less likely to have strong policy demands regarding automation.

Previous literature highlights that Americans appear to misattribute their blame of automation to immigrants and globalization (Wu, 2021; Zhang, 2019). This would be one interpretation as to why robot displacement increases support for radical right parties (Anelli et al., 2021; Frey et al., 2018). My results so far do not point to an increase in demand for nationalist policies as a result of automation. Nevertheless, I also ask participants to evaluate how much globalization impacted their job and American jobs in general. I find that priming respondents about automation does not increase one's perceived vulnerability to import competition (see columns 3 and 4 in Table 1.10). Hence, any increase in anti-globalization sentiment following automation would likely be driven by a misidentification of the cause of the shock.

Finally, I check whether the automation treatment affects one's perception of previous job displacement. I aim to see whether this treatment can bias one's perception of "reality" and make participants more likely to think they have been previously affected by automation or globalization. In particular, I ask participants if they have previously lost their jobs to a robot or a foreign worker. I detect no effect, in line with previous results (Table 1.11).

Table 1.10: Effects on post-treatment views on own job and American jobs' vulnerability to automation and globalization

Impact of ...	(1) ...technology on own job	(2) ...technology on American jobs	(3) ...globalization on own job	(4) ...globalization on American job
Automation Treatment	-0.031 (-0.78)	-0.083** (-2.04)	-0.040 (-0.98)	-0.033 (-0.82)
Controls	Yes	Yes	Yes	Yes
Observations	2945	2945	2931	2939

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.11: Effects on post-treatment declaration of job displacement by robots or foreign workers

	(1) Displaced by robot	(2) Displaced by foreign worker
Automation Treatment	-0.0082 (-0.18)	0.012 (0.27)
Controls	Yes	Yes
Observations	2818	2814

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

I identify two main mechanisms behind the average null effect of the automation treatment: fairness considerations and perceived vulnerability to automation. Participants do not perceive automation as unfair and find that firms are legitimate in replacing workers with robots. In addition, I find that while respondents update their average level of anxiety with regard to automation and its impact on American jobs, they are not more likely to feel personally threatened by robots. There might be a connection between the two mechanisms I identify. In particular, participants' fairness consideration might be more influenced by egotropic concerns rather than sociotropic ones.

1.3.4 Do views on government effectiveness and trust in government play a role?

I zoom in on the impact of government effectiveness and trust in government as additional mechanisms. Previous literature highlights that government efficacy plays a mediating role in the demand for government intervention (Alesina et al., 2018; Algan et al., 2011; Kuziemko et al., 2015; Yamamura, 2014).

I test this assumption and look at the potential impact of (i) views on government effectiveness and (ii) trust in government. I see the former as measuring views on the quality and competence of the government, while the latter measures confidence and lack of (corruption) suspicion. I give details below on how I measure both variables but note that the correlation between views on government effectiveness and trust in government is 0.43. Hence, both variables capture different dimensions of government efficacy.

The focus on government effectiveness tests whether people dismiss government intervention because they see it as inefficient and incompetent or, on the contrary, whether firm beliefs in State efficiency trigger limited additional for public provision when a crisis occurs. Previous literature has already examined the link between public trust and the demand for public action (Kuziemko et al., 2015; Yamamura, 2014). In particular, Kuziemko et al. (2015) provide causal evidence that distrust in government inhibits support for government intervention even when concerns for inequality are high. Consistent with these findings, Table 1.1 shows that trust in government correlates with more support for regulation and taxation on firms.

Table 1.1 also indicates that views on government efficiency are essential determinants of support for public policies. As most research so far has focused on the role of trust in government in demand for redistribution, I emphasize government effectiveness and use a third treatment arm to study its impact.

1.3.4.1 Government effectiveness

In a third treatment arm, I first anchor respondents' views of government effectiveness at a high level and then provide them with the same automation treatment as in section 1.3.2. The second rows of Tables 1.12 and 1.13 display the result of the "automation + government effectiveness treatment"[18].

[18]Breakdown by type of public policy is available in Table A.3

Table 1.12: Effect of the automation treatment on public policy preferences

	(1) Social Policies Index	(2) Retaliatory Policies Index	(3) Nationalist Policies Index
Automation Treatment	0.0016	0.032	-0.034
	(0.04)	(0.83)	(-0.82)
Automation + Govt Effectiveness Treatment	-0.045	-0.020	-0.00031
	(-1.09)	(-0.53)	(-0.01)
Controls	Yes	Yes	Yes
Observations	2976	2977	2977

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.13: Effect of the automation treatment on views on who should be responsible to help affected workers

	(1) Government	(2) Civil Society	(3) Individuals	(4) Firms
Automation Treatment	0.071*	0.039	0.0073	0.0028
	(1.77)	(0.94)	(0.17)	(0.07)
Automation + Govt Effectiveness Treatment	-0.016	0.032	-0.0035	-0.039
	(-0.38)	(0.75)	(-0.08)	(-0.92)
Controls	Yes	Yes	Yes	Yes
Observations	2970	2973	2973	2972

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Adding the government effectiveness treatment does not lead to any significant effect in support of public policies. Nevertheless, the sign flips on the social and retaliatory policies indices and becomes closer to zero for the nationalist policies index. This is indicative evidence that increasing government effectiveness reduces the demand for government intervention. In fact, a look at the breakdown of the effect by type of public policy shows that respondents are significantly less likely to support retraining programs[19]. In addition, anchoring views of government effectiveness at a high level cancels the positive effect of the automation treatment on the government's responsibility. These results highlight a potential mitigating role of government effectiveness in response to automation. I explore this channel further by looking at heterogeneous effects according to pre-treatment views on government efficiency and post-treatment updating of these views.

Before the treatment, I ask respondents to rate the government's efficiency in terms of public services and policy implementation. Respondents choose between not efficient at all, somewhat efficient, or very efficient[20]. I create a dummy variable equal to one if they answer very efficient and look at heterogeneous effects by pre-treatment views on government effectiveness.

[19]It is puzzling that this policy is the only one significantly affected, and I have no clear interpretation of it. One explanation could be that the level of support for retraining is particularly high in the control and would have more room for a negative revision.

[20]I take this question from Robinson et al. (1999)

Table 1.14 shows that reactions to the automation shock exhibit clear heterogeneous effects according to government efficiency views. People that view the government as efficient are less likely to support social, retaliatory, and nationalist policies following the automation treatment. The main coefficient becomes positive in the case of retaliatory policies, indicating that people who do not view the government as efficient are more likely to ask for retaliatory policies. A breakdown of the results by type of public policy (Table 1.16) shows this result is mainly driven by more support for taxing firms rather than regulation. Table 1.16 also indicates a negative interaction effect between automation and government efficiency in the case of tariffs, but not immigration. In other words, people that view the government as efficient are less likely to support the implementation of tariffs following an automation shock. In contrast, their immigration policy views are not differentially affected.

These heterogeneous effects lose significance once I add the government effectiveness treatment, as the gap in pre-treatment views on government efficiency narrows.

Table 1.14: Heterogeneous effects by views on government effectiveness - Support for policies

	(1) Social Policies Index	(2) Retaliatory Policies Index	(3) Nationalist Policies Index
Automation Treatment	0.041	0.070*	-0.0033
	(0.97)	(1.74)	(-0.08)
Automation Treatment × Govt is efficient=1	-0.42***	-0.41***	-0.33*
	(-2.63)	(-2.89)	(-1.83)
Automation + Govt Effectiveness Treatment	-0.020	-0.0061	0.016
	(-0.47)	(-0.15)	(0.38)
Automation + Govt Effectiveness Treatment × Govt is efficient=1	-0.25*	-0.15	-0.16
	(-1.70)	(-1.11)	(-1.02)
Controls	Yes	Yes	Yes
Observations	2976	2977	2977

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.15: Heterogeneous effects by views on government effectiveness - Responsibility to help affected workers

	(1) Government	(2) Civil Society	(3) Individuals	(4) Firms
Automation Treatment	0.083*	0.051	0.0099	0.0067
	(1.96)	(1.17)	(0.21)	(0.15)
Automation Treatment × Govt is efficient=1	-0.12	-0.13	-0.025	-0.043
	(-0.83)	(-0.83)	(-0.16)	(-0.28)
Automation + Govt Effectiveness Treatment	0.0016	0.048	0.0060	-0.041
	(0.04)	(1.09)	(0.13)	(-0.89)
Automation + Govt Effectiveness Treatment × Govt is efficient=1	-0.18	-0.17	-0.097	0.012
	(-1.29)	(-1.14)	(-0.68)	(0.09)
Controls	Yes	Yes	Yes	Yes
Observations	2970	2973	2973	2972

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.16: Breakdown of government efficiency heterogeneous effects by type of public policy

	(1) Retraining	(2) Financial Assistance	(3) Regulate	(4) Tax Firms	(5) Restrict Immigration	(6) Tariffs
Automation Treatment	0.0082	0.060	0.048	0.076*	-0.011	0.0048
	(0.19)	(1.40)	(1.14)	(1.80)	(-0.27)	(0.11)
Automation Treatment × Govt is efficient=1	-0.37**	-0.37**	-0.36***	-0.36**	-0.14	-0.40**
	(-2.35)	(-2.33)	(-2.63)	(-2.30)	(-0.84)	(-2.29)
Automation + Govt Effectiveness Treatment	-0.069	0.027	-0.017	0.0063	0.031	-0.0033
	(-1.51)	(0.62)	(-0.40)	(0.15)	(0.76)	(-0.07)
Automation + Govt Effectiveness Treatment × Govt is efficient=1	-0.25	-0.21	-0.20	-0.062	-0.12	-0.16
	(-1.58)	(-1.42)	(-1.55)	(-0.42)	(-0.75)	(-0.99)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2976	2977	2977	2977	2977	2977

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

I also test whether the treatments affect views on government effectiveness and, for comparison purposes, public trust. To limit experimenter demand, I use a different question from the pre-treatment one and place it toward the end of the survey. Following Pew's American Trends Panel 2019, I ask respondents to choose between two statements: "*Government is almost always wasteful and inefficient*" and "*Government often does a better job than people give it credit for.*" I create a dummy equal to one if respondents choose the latter. This measure of trust in government is very similar to the World Value Survey's and asks respondents how much confidence they have in the federal government on a four-point Likert scale. To reduce experiment demand, I also include other organizations in the list, making it less obvious I focus on trust in government [21].

Table 1.17 provides suggestive evidence that the automation shock lowers views on government effectiveness, although the coefficient fails to be significant at the 10% level.

[21] See appendix A.3 for the exact wording

Interestingly, trust in the government remains unaffected by the automation treatment. Unsurprisingly, the government effectiveness treatment leads to an apparent increase in post-treatment government effectiveness. It also seems to increase trust in the government marginally, but the result is not significant.

Table 1.17: Effects on post-treatment views on government effectiveness and public trust

	(1) Post-treatment govt effectiveness	(2) Post-treatment trust in govt
Automation Treatment	-0.029	-0.015
	(-1.54)	(-0.43)
Automation + Govt Effectiveness Treatment	0.050**	0.043
	(2.53)	(1.21)
Controls	Yes	Yes
Observations	2955	2955

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

These results show that views on government effectiveness play a significant role in the demand for government intervention. In particular, individuals that view the government as efficient are less likely to see a need for government intervention in the face of automation. This mechanism is an unlikely explanation for the low backlash against automation in the US, as less than 10% see the government as "very efficient" in the sample. Nevertheless, this channel could be important in explaining different reactions across countries.

1.3.4.2 Trust in government

As shown in Table 1.17, individuals do not significantly update their level of public trust following the treatments. I also test for heterogeneous effects according to pre-treatment levels of confidence in the government. I use a different question than in Table 1.17 and ask respondents "How much of the time do you think you can trust the federal government in Washington to do what is right?"[22]. I code it as a dummy equal to one if the respondent answers "Most of the time" or "Just about always" and interact it with the treatment variable. I find no heterogeneous effects in Table 1.19 and the first part of Table 1.18. The automation + government effectiveness case results are interesting: the interaction of trust in government with the treatment leads to a negative coefficient. Table 1.20 indicates that views on immigration policies drive this result. In particular, individuals with low trust in government subject to the favorable government effectiveness treatment first and then the automation prime increase their support for restricting immigration by 0.09 of a standard deviation. On the contrary, the average effect for individuals with high trust in government is negative (0.092-0.27). Hence, the interaction of government effectiveness

[22]This question is a classic measure of trust in government used in the General Social Survey, Pew's survey on public trust in government, or the Michigan Public Policy Survey.

and trust appears to play a part in the increase in anti-immigration sentiment following robot displacement.

Table 1.18: Heterogeneous effects trust in government - Support for policies

	(1) Social Policies Index	(2) Retaliatory Policies Index	(3) Nationalist Policies Index
Automation Treatment	0.029	0.059	-0.031
	(0.60)	(1.30)	(-0.66)
Automation Treatment × Trusts the govt=1	-0.10	-0.099	0.0019
	(-1.13)	(-1.17)	(0.02)
Automation + Govt Effectiveness Treatment	-0.029	0.0073	0.057
	(-0.60)	(0.16)	(1.24)
Automation + Govt Effectiveness Treatment × Trusts the govt=1	-0.054	-0.098	-0.21**
	(-0.58)	(-1.16)	(-2.26)
Controls	Yes	Yes	Yes
Observations	2976	2977	2977

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.19: Heterogeneous effects trust in government - Responsibility to help affected workers

	(1) Government	(2) Civil Society	(3) Individuals	(4) Firms
Automation Treatment	0.066	0.044	-0.013	-0.034
	(1.35)	(0.89)	(-0.26)	(-0.67)
Automation Treatment × Trusts the govt=1	0.021	-0.016	0.076	0.14
	(0.25)	(-0.17)	(0.79)	(1.46)
Automation + Govt Effectiveness Treatment	-0.010	0.048	-0.018	-0.064
	(-0.21)	(0.95)	(-0.34)	(-1.24)
Automation + Govt Effectiveness Treatment × Trusts the govt=1	-0.021	-0.060	0.049	0.084
	(-0.23)	(-0.65)	(0.53)	(0.91)
Controls	Yes	Yes	Yes	Yes
Observations	2970	2973	2973	2972

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.20: Breakdown of trust heterogeneous effects by type of public policy

	(1) Retraining	(2) Financial Assistance	(3) Regulate	(4) Tax Firms	(5) Restrict Immigration	(6) Tariffs
Automation Treatment	-0.0016	0.049	0.050	0.055	-0.0079	-0.042
	(-0.03)	(1.00)	(1.04)	(1.15)	(-0.17)	(-0.84)
Automation Treatment × Trusts the govt=1	-0.093	-0.088	-0.13	-0.043	-0.046	0.045
	(-1.00)	(-0.96)	(-1.48)	(-0.48)	(-0.48)	(0.46)
Automation + Govt Effectiveness Treatment	-0.078	0.019	-0.0075	0.021	0.092**	0.0082
	(-1.50)	(0.39)	(-0.16)	(0.44)	(2.02)	(0.17)
Automation + Govt Effectiveness Treatment × Trusts the govt=1	-0.052	-0.043	-0.099	-0.074	-0.27***	-0.10
	(-0.53)	(-0.46)	(-1.16)	(-0.83)	(-2.85)	(-1.04)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2976	2977	2977	2977	2977	2977

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Overall, I find that public trust plays little role in explaining participants' reaction to automation, while positive views on government efficiency reduce the demand for public policies. The latter suggests that institutional settings matter when citizens formulate their response to an automation shock.

1.3.5 The case of nationalist policies

My results show that automation does not affect the demand for government intervention, and, in particular, I find no increase in support for nationalist policies. This contrasts with Anelli et al. (2021), Frey et al. (2018), Gingrich (2019), and Im et al. (2019) who identify a relation between automation and support for extreme right-wing candidates at the macro level. The result on the interactive effect of government effectiveness with trust in government provides a first element of response, yet it is unlikely to be the main explanation. Instead, I focus on the misattribution hypothesis highlighted by Zhang (2019) and Wu (2021). Indeed, the gap between the average null effect I obtain and observational studies could be due to respondents misidentifying the source of their displacement. In other words, respondents displaced by robots might mistakenly think that immigration or globalization is the cause of their job loss. Such a hypothesis appears likely as automation is not as easily identifiable as an import competition shock and as there is a strong interdependence between globalization and technological advancement (Wang, 2021).

I use the questions on post-treatment declaration of job displacement to try to answer this question. In particular, I restrict the survey sample to the control group and regress the dummy variables for individuals that declare to have been displaced by robots or by foreign workers on nationalist policies, along with the usual set of controls[23]. Table

[23] I note that post-treatment declaration of job displacement by robots or foreign workers are "bad controls" as they might have been affected by the treatment. Yet I am not worried about that for two reasons. First, I capture an effect in the control group, not the treated groups. Second, Table 1.11 shows that the treatments do not affect these declarations.

1.21 shows a clear positive relation between robot displacement and anti-immigration and anti-trade sentiments in the control group. The effect is sizeable. Individuals that declare to have been displaced by robots are 0.42 of a standard deviation (p<0.01) more likely to support restrictions on immigration and 0.27 of a standard deviation more likely to support tariffs (p<0.1). A correlation is also visible for those declaring foreign workers have displaced them, and the coefficients are respectively 0.25 of standard deviation (p<0.01) and 0.28 of standard deviation (p<0.01).

The significant correlation disappears once I specify that the source of the shock is automation. Tables 1.22 and 1.23 present the results for the automation and the automation + government effectiveness treatment group. Figures 1.5 and 1.6 show the average support for nationalist policies for displaced respondents and non-displaced participants by treatment group. The figures show how the gap between displaced and non-displaced workers closes once I provide them with the information that automation caused the layoffs. This is evidence that part of the effect captured in macro-level observational studies is probably driven by misperceptions about the cause of the shock among those displaced by globalization or automation.

Table 1.21: Covariates of support for nationalist policies in the control group

	(1) Nationalist Policies Index	(2) Nationalist Policies Index	(3) Restrict Immigration	(4) Restrict Immigration	(5) Tariffs	(6) Tariffs
Displaced by robot	0.41***		0.42***		0.27*	
	(2.97)		(3.42)		(1.89)	
Displaced by foreign worker		0.32***		0.25***		0.28***
		(3.20)		(2.59)		(2.63)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	906	869	906	869	906	869

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.22: Covariates of support for nationalist policies in the automation group

	(1) Nationalist Policies Index	(2) Nationalist Policies Index	(3) Restrict Immigration	(4) Restrict Immigration	(5) Tariffs	(6) Tariffs
Displaced by robot	0.20		0.16		0.17	
	(1.37)		(1.24)		(1.20)	
Displaced by foreign worker		0.17		0.26**		0.044
		(1.55)		(2.41)		(0.39)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	904	874	904	874	904	874

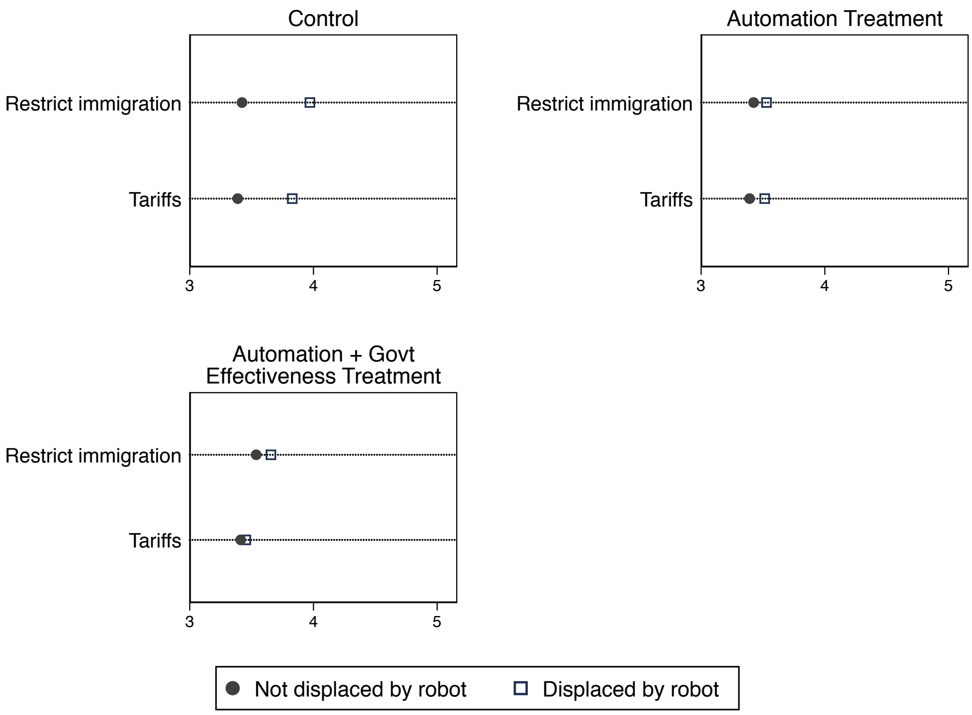
t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.23: Covariates of support for nationalist policies in the automation + government effectiveness group

	(1) Nationalist Policies Index	(2) Nationalist Policies Index	(3) Restrict Immigration	(4) Restrict Immigration	(5) Tariffs	(6) Tariffs
Displaced by robot	0.053		0.12		-0.024	
	(0.39)		(1.00)		(-0.16)	
Displaced by foreign worker		0.030		0.075		-0.021
		(0.29)		(0.73)		(-0.18)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	902	882	902	882	902	882

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 1.5: Average support for nationalist policies for respondents displaced by robots and non-displaced participants by treatment group



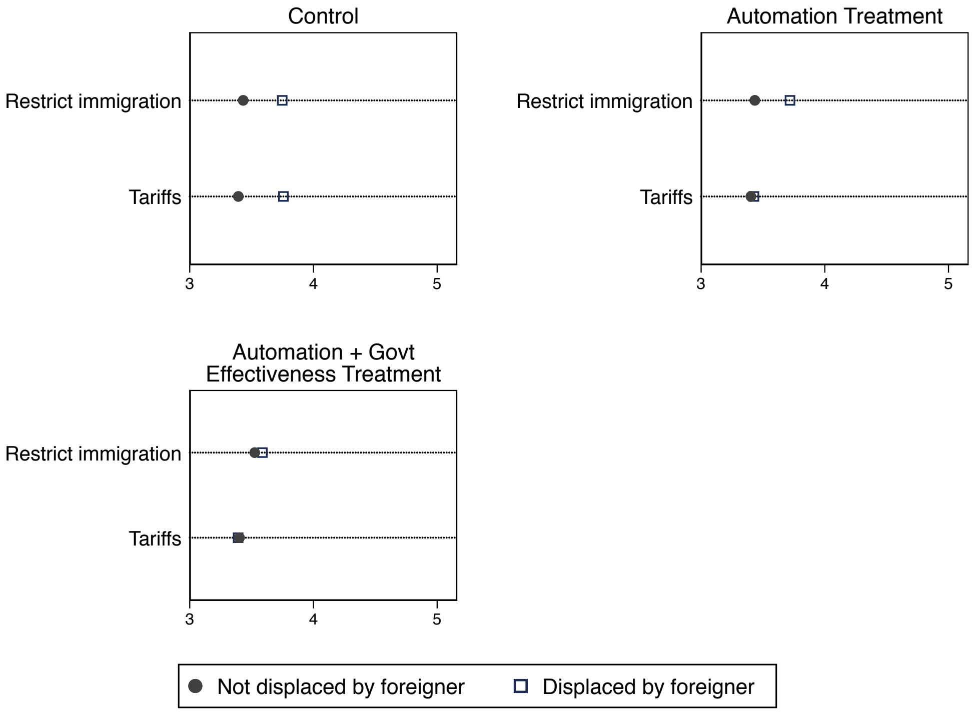
1.3.6 Additional heterogeneous effects

As specified in the pre-analysis plan, I also investigate whether survey participants respond differently to the treatments according to political affiliation and education level.

As visible in Table 1.24, Democrats and Independents are less likely to support social policies in the case of the automation + government effectiveness treatment but not the automation one[24]. This can seem counter-intuitive as they are more likely to support

[24] The breakdown by type of public policy is included in Table A.9

Figure 1.6: Average support for nationalist policies for respondents displaced by foreigner workers and non-displaced participants by treatment group



social policies to address labor shocks in general (Table 1.1). One interpretation could be that they are more receptive to the government effectiveness treatment and therefore are less likely to see a need to demand social policies when the government is effective. Unsurprisingly, Democrats are less likely than Republicans to think the responsibility to help affected workers falls on the individual following the automation treatment (Table 1.25). The effect disappears once the government effectiveness treatment is added.

I find no evidence of heterogeneous effects by education level in Tables 1.26 and 1.27. A look at the breakdown by type of public policies shows striking heterogeneous reactions regarding taxes (Table A.10). Respondents with no college degree are more likely to support taxes on firms after the automation treatment, while those with a college degree or a postgraduate degree are less likely. The government effectiveness treatment reduces the effect.

Table 1.24: Heterogeneous effects by political affiliation - Support for policies

	(1) Social Policies Index	(2) Retaliatory Policies Index	(3) Nationalist Policies Index
Automation Treatment	0.083	0.029	0.011
	(1.20)	(0.44)	(0.17)
Automation Treatment × Democrat	-0.14	-0.039	-0.069
	(-1.60)	(-0.46)	(-0.78)
Automation Treatment × Independent	-0.13	0.15	-0.17
	(-0.85)	(1.10)	(-1.16)
Automation + Govt Effectiveness Treatment	0.098	0.0085	0.021
	(1.42)	(0.13)	(0.33)
Automation + Govt Effectiveness Treatment × Democrat	-0.21**	-0.086	-0.082
	(-2.33)	(-1.02)	(-0.93)
Automation + Govt Effectiveness Treatment × Independent	-0.38***	0.14	0.097
	(-2.83)	(1.02)	(0.71)
Controls	Yes	Yes	Yes
Observations	2976	2977	2977

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.25: Heterogeneous effects by political affiliation - Responsibility to help affected workers

	(1) Government	(2) Civil Society	(3) Individuals	(4) Firms
Automation Treatment	0.097	0.041	0.11*	0.090
	(1.52)	(0.70)	(1.82)	(1.33)
Automation Treatment × Democrat	-0.059	-0.086	-0.16**	-0.12
	(-0.73)	(-1.14)	(-1.98)	(-1.37)
Automation Treatment × Independent	-0.047	-0.11	-0.029	-0.15
	(-0.35)	(-0.88)	(-0.21)	(-1.04)
Automation + Govt Effectiveness Treatment	-0.021	0.040	0.063	0.043
	(-0.32)	(0.66)	(1.00)	(0.63)
Automation + Govt Effectiveness Treatment × Democrat	-0.011	-0.13*	-0.093	-0.070
	(-0.14)	(-1.75)	(-1.11)	(-0.78)
Automation + Govt Effectiveness Treatment × Independent	0.098	-0.14	0.076	-0.12
	(0.77)	(-1.19)	(0.56)	(-0.86)
Controls	Yes	Yes	Yes	Yes
Observations	2970	2972	2973	2973

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.26: Heterogeneous effects by education - Support for policies

	(1) Social Policies Index	(2) Retaliatory Policies Index	(3) Nationalist Policies Index
Automation Treatment	-0.00094	0.067	-0.045
	(-0.02)	(1.38)	(-0.87)
Automation Treatment × College degree	-0.025	-0.069	0.060
	(-0.27)	(-0.81)	(0.65)
Automation Treatment × Postgraduate degree	0.090	-0.13	-0.056
	(0.66)	(-0.94)	(-0.37)
Automation + Govt Effectiveness Treatment	-0.037	0.022	0.019
	(-0.71)	(0.46)	(0.37)
Automation + Govt Effectiveness Treatment × College degree	-0.032	-0.13	-0.036
	(-0.34)	(-1.52)	(-0.39)
Automation + Govt Effectiveness Treatment × Postgraduate degree	0.015	-0.050	-0.081
	(0.11)	(-0.37)	(-0.58)
Controls	Yes	Yes	Yes
Observations	2976	2977	2977

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.27: Heterogeneous effects by education - Responsibility to help affected workers

	(1) Government	(2) Civil Society	(3) Individuals	(4) Firms
Automation Treatment	0.041	-0.0085	0.021	-0.017
	(0.85)	(-0.19)	(0.43)	(-0.31)
Automation Treatment × College degree	0.061	0.056	0.012	0.068
	(0.74)	(0.71)	(0.14)	(0.72)
Automation Treatment × Postgraduate degree	0.037	-0.046	0.091	0.060
	(0.32)	(-0.44)	(0.76)	(0.46)
Automation + Govt Effectiveness Treatment	0.0077	-0.0099	0.027	-0.023
	(0.16)	(-0.22)	(0.53)	(-0.42)
Automation + Govt Effectiveness Treatment × College degree	-0.044	-0.056	-0.035	0.077
	(-0.51)	(-0.68)	(-0.40)	(0.82)
Automation + Govt Effectiveness Treatment × Postgraduate degree	-0.096	-0.071	0.099	-0.028
	(-0.80)	(-0.70)	(0.82)	(-0.23)
Controls	Yes	Yes	Yes	Yes
Observations	2970	2972	2973	2973

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

1.3.7 Threats to validity and robustness checks

1.3.7.1 Experimenter demand

Issues regarding experimenter demand effects (EDE) in survey settings have gained increasing attention over recent years (De Quidt et al., 2018; Mummolo and Peterson, 2019). It seems unlikely that this survey experiment suffers from EDE for two reasons. The first is that the average main result is a null effect. Had the survey experiment triggered EDE, I should have seen some positive coefficients in section 1.3.1. Second, I use an active control group in which participants are treated with a negative labor market shock. This reduces the probability of EDE in this setting as highlighted by Haaland et al. (Forthcoming).

1.3.7.2 Within-survey attrition

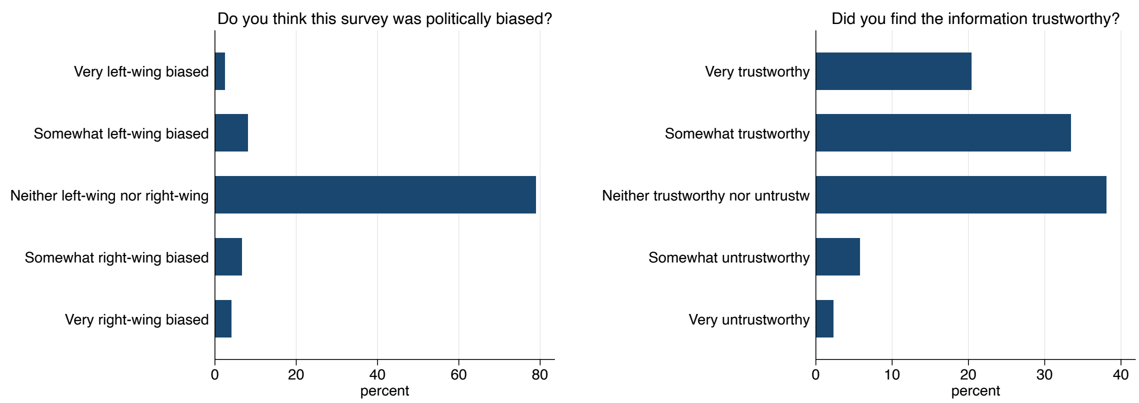
There is attrition as participants progress toward the end of the survey. The attrition rate remains very low: 23 participants fail to answer all questions after the treatment. This corresponds to a 0.77% attrition rate. Attrition does not correlate with the assignment to a specific treatment group, and my results are robust to restricting the sample only to those that answered all questions.

1.3.7.3 Perception of the research setting

At the end of the survey, I measure participants' perceptions of the treatment and survey questions.

The vast majority of respondents do not think that the survey was politically biased as close to 80% of respondents answer that the survey was "neither left-wing nor right-wing" (see left panel of figure 1.7). In addition, more than 90% of participants found the information provided in the survey "Very trustworthy", "Somewhat trustworthy", or "Neither trustworthy nor untrustworthy" (figure 1.7, right panel).

Figure 1.7: Perception of the survey



I also test whether respondents' views differ by treatment group and do not find evidence of it (Table 1.28).

Table 1.28: Perception of the survey by treatment group

	(1) Survey Political Bias	(2) Survey Trustworthiness
Automation Treatment	-0.03	0.05
	(-1.15)	(1.29)
Automation + Govt Effectiveness Treatment	-0.01	0.05
	(-0.52)	(1.29)
Controls	Yes	Yes
Observations	2954	2954

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

1.3.7.4 Multiple hypothesis testing

In my pre-analysis plan, I highlighted the risk of multiple hypothesis testing as I looked at the treatments' effect on six types of public policies and four entities that could be held responsible. I intended to address this issue by creating indices a la Anderson (2008) and controlling for the false discovery rate.

On average, I do not obtain any significant effect in my baseline specification, which enables us to discard the issue of multiple hypothesis testing.

1.4 Conclusion

Using a large representative sample of the US population, my results confirm an average null effect of an automation prime on public policy preferences. I test for different mechanisms and identify fairness considerations and perceived vulnerability to automation as explanatory channels.

I show that respondents do not see automation as particularly unfair to workers and think that firms are justified in automating. In addition, the automation treatment increases respondents' anxiety regarding automation's impact on American jobs in general but not on their own occupations. Hence, while an automation prime increases average anxiety levels, respondents do not feel personally threatened by robots.

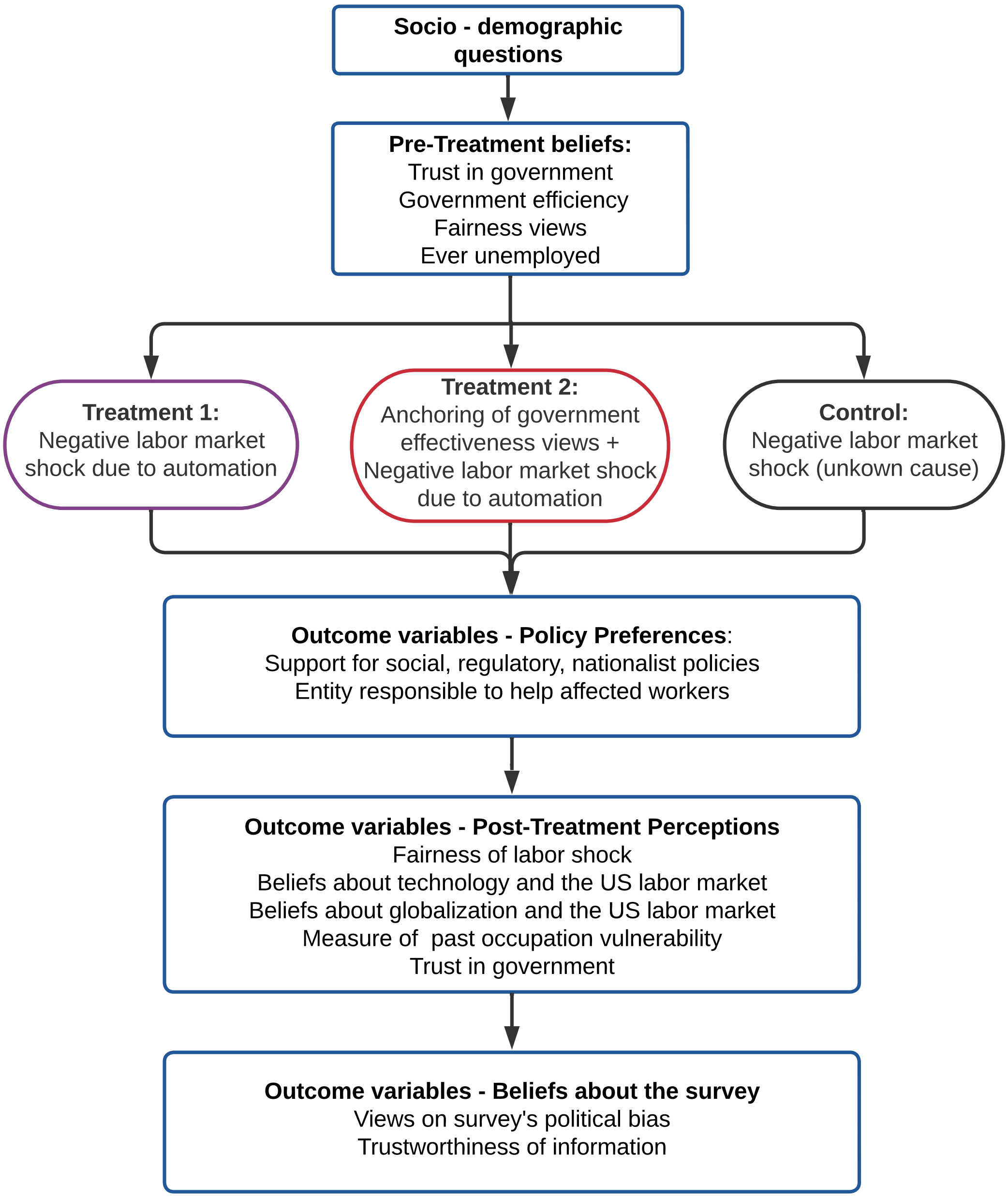
Finally, I look at the case of nationalist policies and find that respondents are less likely to support nationalist policies once they know the cause of the shock is automation. This suggests that misperceptions about the cause of the shock have partly driven the increase in anti-trade and anti-immigration sentiment following automation.

Overall, this study bridges a gap in the literature by identifying explanatory channels behind the limited demand for more redistribution and increased support for nationalist policies in the face of automation.

Appendices - Chapter 1

A.1 Figures

Figure A.1: Summary of the survey flow



A.2 Tables

Table A.1: Socio-demographic characteristics of the sample compared to the American Community Survey

Demographic Characteristics	Own Survey	ACS 5-Year Estimates (2020)
Female	50.3%	50.8%
18-25 y.o.	9%	12%
25-34 y.o.	17.5%	18%
35-44 y.o.	18%	16.3%
45-54 y.o.	19%	16.4%
55-64 y.o.	17.5%	16.6%
$\geq$65 y.o.	18.7%	20.7%
White	76.8%	75.1%
Black	12.7%	14.2%
Asian	5%	6.8%
Other	7.2%	9.6%
Less than HS or HS only	35.6%	38%
Some college, no degree	24.9%	21%
Associate or college degree	28.1%	28%
Graduate or professional degree	11.41%	12.7%
$0 to $14,999	11.5%	9.9%
$15,000 to $34,999	19.3%	17.1%
$35,000 to $49,999	12.5%	12%
$50,000 to $74,999	18.3%	17.2%
$75,000 to $99,999	12.7%	12.8%
$100,000 to $149,999	14.5%	15.6%
$150,000 or more	10.2%	15.4%
North East	15.7%	13.6%
Mid West	23.5%	28.1%
West	17.8%	15.8%
South	43.5%	42.3%

Table A.2: Balance across the treatment and control groups

	(1) Control		(2) Treatment 1		(3) Treatment 2		T-test P-value		
Variable	N	Mean	N	Mean	N	Mean	(1)-(2)	(1)-(3)	(2)-(3)
Female	989	0.485	996	0.508	992	0.510	0.312	0.271	0.927
Age	989	47.220	996	47.984	992	47.395	0.283	0.805	0.406
Household Income	989	73393	996	72275	992	71164	0.659	0.372	0.657
No college	989	0.607	996	0.591	992	0.610	0.487	0.884	0.400
College degree	989	0.288	996	0.296	992	0.268	0.695	0.320	0.165
Postgraduate degree	989	0.105	996	0.112	992	0.122	0.602	0.238	0.509
Born in the US	989	0.945	996	0.949	992	0.945	0.736	0.934	0.674
Caucasian/White	989	0.772	996	0.747	992	0.784	0.184	0.528	0.050
Black/African American	989	0.133	996	0.134	992	0.112	0.997	0.143	0.142
Asian/Asian American	989	0.042	996	0.056	992	0.049	0.157	0.462	0.496
Hispanic	989	0.091	996	0.083	992	0.081	0.545	0.411	0.827
Married	989	0.518	996	0.527	992	0.512	0.675	0.803	0.503
Single	989	0.325	996	0.303	992	0.330	0.305	0.810	0.205
Divorced	989	0.116	996	0.131	992	0.114	0.335	0.869	0.258
Widowed	989	0.041	996	0.039	992	0.044	0.795	0.750	0.563
Nb of children	989	0.645	996	0.605	992	0.585	0.371	0.172	0.639
Full-time employee	989	0.405	996	0.398	992	0.390	0.721	0.486	0.733
Part-time employee	989	0.104	996	0.098	992	0.092	0.671	0.353	0.613
Self-empl. w dependent workers	989	0.033	996	0.034	992	0.046	0.924	0.139	0.165
Self-empl. w/o dependent workers	989	0.018	996	0.021	992	0.024	0.644	0.355	0.641
Unemployed	989	0.077	996	0.077	992	0.091	0.969	0.265	0.281
Student	989	0.024	996	0.026	992	0.028	0.794	0.582	0.771
Not in the labor force	989	0.338	996	0.345	992	0.329	0.719	0.668	0.430
Republican	989	0.376	996	0.387	992	0.399	0.633	0.293	0.564
Democrat	989	0.503	996	0.487	992	0.442	0.488	0.007	0.042
Independent	989	0.091	996	0.101	992	0.130	0.432	0.006	0.046
Mid West	989	0.226	996	0.232	992	0.235	0.773	0.658	0.876
North East	989	0.174	996	0.146	992	0.155	0.085	0.263	0.547
West	989	0.179	996	0.176	992	0.173	0.849	0.745	0.892
South	989	0.421	996	0.447	992	0.436	0.240	0.476	0.644
Trusts the govt	989	0.295	996	0.244	992	0.258	0.010	0.064	0.469
Govt is efficient	989	0.096	996	0.089	992	0.099	0.607	0.838	0.472
Inequality is unfair	989	0.632	996	0.624	992	0.619	0.731	0.550	0.799
Unemployed before	989	0.697	996	0.648	992	0.661	0.020	0.092	0.521
F-test of joint significance (p-value)							0.484	0.203	0.793

Table A.3: Breakdown by type of public policy

	(1) Retraining	(2) Financial Assistance	(3) Regulate	(4) Tax Firms	(5) Restrict Immigration	(6) Tariffs
Automation Treatment	-0.027	0.026	0.015	0.043	-0.024	-0.032
	(-0.64)	(0.62)	(0.36)	(1.05)	(-0.60)	(-0.74)
Automation + Govt Effectiveness Treatment	-0.093**	0.0066	-0.036	0.00040	0.020	-0.019
	(-2.12)	(0.16)	(-0.90)	(0.01)	(0.49)	(-0.43)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2976	2977	2977	2977	2977	2977

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.4: Effect of the automation treatment on public policy preferences- No controls

	(1) Social Policies Index	(2) Retaliatory Policies Index	(3) Nationalist Policies Index
Automation Treatment	-0.017	0.011	-0.024
	(-0.38)	(0.25)	(-0.53)
Automation + Govt Effectiveness Treatment	-0.068	-0.056	0.015
	(-1.54)	(-1.28)	(0.33)
Controls	No	No	No
Observations	2976	2977	2977

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.5: Breakdown by type of public policy - No controls

	(1) Retraining	(2) Financial Assistance	(3) Regulate	(4) Tax Firms	(5) Restrict Immigration	(6) Tariffs
Automation Treatment	-0.039	0.0057	-0.0068	0.026	-0.011	-0.028
	(-0.88)	(0.13)	(-0.15)	(0.59)	(-0.24)	(-0.63)
Automation + Govt Effectiveness Treatment	-0.11**	-0.015	-0.065	-0.034	0.052	-0.024
	(-2.44)	(-0.35)	(-1.48)	(-0.76)	(1.15)	(-0.54)
Controls	No	No	No	No	No	No
Observations	2976	2977	2977	2977	2977	2977

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.6: Responsibility to help displaced workers - No controls

	(1) Government	(2) Civil Society	(3) Individuals	(4) Firms
Automation Treatment	0.047	0.024	0.0043	-0.0079
	(1.08)	(0.54)	(0.10)	(-0.18)
Automation + Govt Effectiveness Treatment	-0.038	0.021	-0.0094	-0.058
	(-0.86)	(0.47)	(-0.21)	(-1.30)
Controls	No	No	No	No
Observations	2970	2973	2973	2972

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.7: Breakdown of fairness heterogeneous effects by type of public policy

	(1) Retraining	(2) Financial Assistance	(3) Regulate	(4) Tax Firms	(5) Restrict Immigration	(6) Tariffs
Automation Treatment	0.022	0.016	0.057	0.10	0.025	0.011
	(0.29)	(0.22)	(0.84)	(1.50)	(0.41)	(0.16)
Automation Treatment × Inequality is unfair=1	-0.077	0.015	-0.067	-0.092	-0.078	-0.069
	(-0.85)	(0.17)	(-0.80)	(-1.10)	(-0.97)	(-0.78)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2976	2977	2977	2977	2977	2977

t statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.8: Breakdown of economic hardship heterogeneous effects by type of public policy

	(1) Retraining	(2) Financial Assistance	(3) Regulate	(4) Tax Firms	(5) Restrict Immigration	(6) Tariffs
Automation Treatment	-0.027	0.11	0.088	0.16**	0.054	0.018
	(-0.35)	(1.53)	(1.22)	(2.14)	(0.76)	(0.23)
Automation Treatment × Unemployed before=1	-0.00032	-0.13	-0.11	-0.17*	-0.12	-0.074
	(-0.00)	(-1.46)	(-1.26)	(-1.90)	(-1.36)	(-0.78)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2976	2977	2977	2977	2977	2977

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.9: Breakdown of political heterogeneous effects by type of public policy

	(1) Retraining	(2) Financial Assistance	(3) Regulate	(4) Tax Firms	(5) Restrict Immigration	(6) Tariffs
Automation Treatment	0.054	0.089	-0.0031	0.056	0.024	-0.0038
	(0.75)	(1.23)	(-0.04)	(0.81)	(0.40)	(-0.05)
Automation Treatment × Democrat	-0.13	-0.12	0.012	-0.082	-0.070	-0.047
	(-1.40)	(-1.33)	(0.14)	(-0.93)	(-0.82)	(-0.50)
Automation Treatment × Independent	-0.16	-0.064	0.086	0.19	-0.14	-0.14
	(-1.04)	(-0.43)	(0.58)	(1.28)	(-0.95)	(-0.97)
Automation + Govt Effectiveness Treatment	0.038	0.13*	-0.023	0.039	0.045	-0.0067
	(0.53)	(1.80)	(-0.35)	(0.59)	(0.73)	(-0.10)
Automation + Govt Effectiveness Treatment × Democrat	-0.17*	-0.20**	-0.047	-0.11	-0.076	-0.063
	(-1.74)	(-2.20)	(-0.54)	(-1.22)	(-0.88)	(-0.67)
Automation + Govt Effectiveness Treatment × Independent	-0.47***	-0.22	0.077	0.17	0.090	0.075
	(-3.26)	(-1.59)	(0.55)	(1.20)	(0.64)	(0.54)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2976	2977	2977	2977	2977	2977

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.10: Breakdown of education heterogeneous effects by type of public policy

	(1) Retraining	(2) Financial Assistance	(3) Regulate	(4) Tax Firms	(5) Restrict Immigration	(6) Tariffs
Automation Treatment	-0.029	0.026	-0.011	0.16***	-0.053	-0.040
	(-0.56)	(0.46)	(-0.18)	(2.59)	(-0.78)	(-0.66)
Automation Treatment × College degree	-0.012	-0.031	0.088	-0.25**	0.074	0.049
	(-0.13)	(-0.31)	(0.83)	(-2.28)	(0.64)	(0.45)
Automation Treatment × Postgraduate degree	0.065	0.095	0.021	-0.32*	-0.0027	-0.098
	(0.50)	(0.66)	(0.13)	(-1.70)	(-0.01)	(-0.57)
Automation + Govt Effectiveness Treatment	-0.074	0.0075	-0.040	0.093	0.061	-0.015
	(-1.40)	(0.14)	(-0.67)	(1.46)	(0.94)	(-0.24)
Automation + Govt Effectiveness Treatment × College degree	-0.030	-0.025	-0.024	-0.27**	-0.088	0.0047
	(-0.33)	(-0.26)	(-0.23)	(-2.45)	(-0.75)	(0.04)
Automation + Govt Effectiveness Treatment × Postgraduate degree	-0.030	0.058	0.025	-0.14	-0.11	-0.062
	(-0.22)	(0.40)	(0.16)	(-0.81)	(-0.59)	(-0.39)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2976	2977	2977	2977	2977	2977

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.3 Survey Experiment Questions

A.3.1 Default demographic questions

- What is your gender?
  - Male
  - Female
  - Non-binary / third gender
  - Prefer not to say
- How old are you?
  - 18 to 24
  - 25 to 34
  - 35 to 44
  - 45 to 54
  - 55 to 64
  - above 64
- How would you describe your ethnicity/race? Please check all that apply.
  - White
  - African American/Black

  - Hispanic/Latino
  - Asian/Asian American
  - Other (please specify)

- In which state do you currently reside? [DROPDOWN LIST]
- Were you born in the United States?
  - Yes
  - No
- Please indicate your marital status
  - Married
  - Single
  - Divorced
  - Widowed
- How many children (aged 18 y.o. or younger) live with you in your household?
  - None
  - 1
  - 2
  - 3
  - 4
  - 5 or more

A.3.2 Education and Occupation questions

- What is the highest level of school you have completed or the highest degree you have received?
  - Less than high school
  - High school graduate (high school diploma or equivalent including GED)
  - Some college but no degree
  - Associate degree in college (2-year)
  - Bachelor's degree in college (4-year)
  - Master's degree
  - Doctoral degree

- Professional degree (JD, MD, MBA)

- Have you ever been unemployed in the past?
  - Yes
  - No

- Which statement best describes your current employment status?
  - Full-time employee
  - Part-time employee
  - Self-employed or small business owner with dependent workers
  - Self-employed or small business owner without dependent workers
  - Unemployed and looking for work
  - Student
  - Not in labor force (for example: retired, full-time parent, or disabled)

- Which of the following best describes your employer? If you hold more than one job describe the one at which you worked the most hours last week. If you're unemployed describe your most recent employment.
  - Private sector: For-profit company or organization
  - Private sector: Non-profit organization (including tax-exempt and charitable organizations)
  - Local government (for example: city or county school district)
  - State government
  - Active duty US Armed Forces or Commissioned Corps
  - Federal government

- Which of the following industries most closely matches the one in which you are employed, or were employed most recently?
  - Agriculture, forestry, fishing, or hunting
  - Mining, quarrying, or oil and gas extraction
  - Construction
  - Manufacturing
  - Wholesale trade
  - Retail trade

  - Transportation or warehousing
  - Utilities
  - Information
  - Finance or insurance
  - Real estate or rental and leasing
  - Professional, scientific or technical services
  - Management of companies or enterprises
  - Admin, support, waste management or remediation services
  - Educational services
  - Health care or social assistance
  - Arts, entertainment or recreation
  - Accommodation or food services
  - Other services (except public administration)
  - Public Administration
  - Other (please specify)

- What is your main occupation? (For example: 4th grade teacher, entry-level plumber etc.) If unemployed, enter the main occupation you had most recently? [OPEN TEXT BOX]

- What was your household income before taxes in 2020?

  - Less than $10,000
  - $10,000 to $14,999
  - $15,000 to $24,999
  - $25,000 to $34,999
  - $35,000 to $49,999
  - $50,000 to $74,999
  - $75,000 to $99,999
  - $100,000 to $149,999
  - $150,000 to $199,999
  - $200,000 or more
  - Prefer not to say

A.3.3 Pre-treatment: Political and social attitudes

- How much of the time do you think you can trust the federal government in Washington to do what is right?
  - Never
  - Only some of the time
  - Most of the time
  - Just about always
- Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?
  - Need to be very careful
  - Most people can be trusted
- Thinking about the quality of public services and policy implementation in the U.S., how efficient do you think the government is?
  - Not efficient at all
  - Somewhat efficient
  - Very efficient
- How much do you agree with the following statement: "In the United States, the economic differences between the rich and the poor are unfair."
  - Strongly disagree
  - Disagree
  - Neither disagree nor agree
  - Agree
  - Strongly agree
- In politics today do you consider yourself a Republican, a Democrat, or an Independent?
  - Republican
  - Democrat
  - Independent
  - Other
- As of today, do you lean more to the Democratic Party or the Republican Party?

- Republican Party
- Democratic Party
- Don't Know

A.3.4 Control

Now we'd like you to take the time to read this short article.

National food manufacturer to cut down 20% of its job force

HCYH food manufacturer, which employs more than 100,000 workers in the U.S., is facing changes this year. The company announced it would make adjustments to keep its plants competitive and lay off 20% of its workforce.

Contacted by email, a company spokesperson specified: "These adjustments are a further step towards restructuring the company", but did not give further details.

We spoke with an employee from one affected plant. Having worked there for fifteen years, the employee asked to stay anonymous and complained that the company did not communicate these changes adequately to the workers. Many employees from HCYH will likely become unemployed or have to take lower-paid jobs.

A.3.5 Treatment Automation

Now we'd like you to take the time to read this short article.

National food manufacturer to cut down 20% of its job force HCYH food manufacturer, which employs more than 100,000 workers in the U.S., is facing changes this year. The company announced it would make adjustments to keep its plants competitive and lay off 20% of its workforce.

Contacted by email, a company spokesperson specified: "These adjustments are a further step towards restructuring the company. New technologies enable us to produce more efficiently. As a result, we decided to automate part of our production with machines."

We spoke with an employee from one affected plant. Having worked there for fifteen years, the employee asked to stay anonymous and complained that the company did not communicate these changes adequately to the workers. Many employees from HCYH will likely become unemployed or have to take lower-paid jobs.

A.3.6 Treatment Government effectiveness + Automation

Each year the World Bank, an international organization based in the United States, ranks countries according to their level of government effectiveness.

The ranking takes into account: - the quality of public services, - the quality of the civil service and the degree of its independence from political pressures, - the quality of policy formulation and implementation, and - the credibility of the government's commitment to such policies. Now, think about government effectiveness in the United States and how it compares to other countries in the world.

- According to you, how well did the United States rank in terms of government effectiveness in 2020 compared to the rest of the world?
  - Among the best: The U.S. has one of the most effective governments.
  - Better than most: The U.S. has a government that is more effective than most other countries.
  - Worse than most: The U.S. has a government that is less effective than most other countries.
  - Among the worst: The U.S. has one of the least effective governments.
- You answered: [ANSWER FROM RESPONDENT]

  The correct answer is:

  In 2020, the United States ranked among the top countries in the world in terms of government effectiveness.

Now we'd like you to take the time to read this short article.

National food manufacturer to cut down 20% of its job force HCYH food manufacturer, which employs more than 100,000 workers in the U.S., is facing changes this year. The company announced it would make adjustments to keep its plants competitive and lay off 20% of its workforce.

Contacted by email, a company spokesperson specified: "These adjustments are a further step towards restructuring the company. New technologies enable us to produce more efficiently. As a result, we decided to automate part of our production with machines."

We spoke with an employee from one affected plant. Having worked there for fifteen years, the employee asked to stay anonymous and complained that the company did not communicate these changes adequately to the workers. Many employees from HCYH will likely become unemployed or have to take lower-paid jobs.

A.3.7 Attention Check

- This is a simple color test to check your attention, when asked for your favorite color you must enter the word "puce" in the text box below. What is your favorite color? [OPEN TEXT BOX]

A.3.8 Post treatment: Policy Views

We'd like to know what you think the response of the government should be, if any.

- Would you support or oppose the government implementing the following policies? [Strongly oppose, Somewhat oppose, Neither oppose nor support, Somewhat support, Strongly support]
  - Providing re-training programs
  - Providing financial assistance to workers who lose their jobs
  - Increasing taxes for firms
  - Restricting immigration to the US
  - Raising tariffs on foreign goods
  - Regulating the ways in which firms operate
- More generally, to what extent do you think each of the following should or should not be responsible for dealing with the potential negative effects workers face? [Definitely should not be responsible, Probably should not be responsible, Probably should be responsible, Definitely should be responsible]
  - The national government
  - Firms, businesses, and employers
  - Civil society groups, such as professional associations, non-profit organizations, and charitable organizations
  - Individual workers themselves
- To what extent do you think that it is fair or unfair for these workers to lose their jobs?
  - Completely unfair
  - Somewhat unfair
  - Neither unfair nor fair
  - Somewhat fair
  - Completely fair
- Which of the following statements best describes how you feel, even if neither is exactly right?
  - If businesses can receive better work at lower cost by replacing humans with robots and computers, they are justified in doing so
  - There should be limits on how many jobs businesses can replace with robots and computers, even if they can do those jobs better and more cheaply than humans can

A.3.9 Post Treatment: Beliefs about technology and the US labor market

The following questions are about your labor market experiences.

- Thinking about new technologies, what kind of impact have they had on... [Very negative, Mostly negative, Neither negative, nor positive, Mostly positive, Very positive]
  - You and your job or career
  - American workers in general

A.3.10 Post Treatment: Beliefs about globalization and the US labor market

- Thinking about globalization, what kind of impact has it had on... [Very negative, Mostly negative, Neither negative, nor positive, Mostly positive, Very positive]
  - You and your job or career
  - American workers in general

A.3.11 Post Treatment: Measure of occupation vulnerability

- Have you yourself ever lost a job because your employer replaced your position with a machine, robot or computer program?
  - Yes
  - No
  - I'm not sure
  - Not applicable
- What was your occupation then? [OPEN TEXT BOX]
- Have you yourself ever lost a job because your employer replaced your position with a cheaper worker in another country?
  - Yes
  - No
  - I'm not sure
  - Not applicable
- What was your occupation then? [OPEN TEXT BOX]

A.3.12 Post Treatment: Beliefs about institutions

Now we'd like to get your views about different organizations/institutions.

- Here is a pair of statements that will help us understand how you feel about the government. Please choose the statement that comes closer to your own views – even if neither is exactly right.
  - Government is almost always wasteful and inefficient
  - Government often does a better job than people give it credit for
- We are going to list six organizations/groups. For each one, could you tell us how much confidence you have in them: [None at all, Not very much confidence, Some confidence, A great deal of confidence]
  - Your neighbors
  - Lawyers
  - Major companies
  - The press
  - The federal government
  - The local government

A.3.13 Post Treatment: Beliefs about the survey

Finally, we'd like to have your opinion about this survey.

- Do you think this survey was politically biased?
  - Very left-wing biased
  - Somewhat left-wing biased
  - Neither left-wing nor right-wing biased
  - Somewhat right-wing biased
  - Very right-wing biased
- Did you find the information we provided you with trustworthy or untrustworthy?
  - Very untrustworthy
  - Somewhat untrustworthy
  - Neither trustworthy nor untrustworthy
  - Somewhat trustworthy
  - Very trustworthy

A.3.14 End Survey

- You've reached the end of this survey. We thank you for your time. Please enter any comment you might have about the survey in the box below. [OPEN TEXT BOX]

Chapter 2

Austerity and Youth Political Attitudes in the UK

2.1 Introduction

"*The age of irresponsibility is giving way to the age of austerity*", declared David Cameron in his speech to the 2009 Conservative Party forum in Cheltenham [1]. A year before the British general elections, the Conservative Party set up a campaign that denounced the "Labor's Debt Crisis" and aimed to restore the country's finances. The Great Recession had triggered a considerable increase in public spending, preceded by some years of deficit. By 2010, British public debt to GDP reached 75 percent and peaked at 87 percent in 2015. As Europe started to grapple with a sovereign debt crisis, a Conservative-led coalition was elected in 2010 in the United Kingdom. Then-Prime Minister David Cameron established the Office for Budget Responsibility and began to implement severe austerity measures to deliver on its promise to provide "more for less".

While aiming to enhance work incentives and simplify the welfare system, the cuts were sizeable. Data from the OECD Economic Outlook 106 shows that the cyclically adjusted primary balance as a percentage of potential GDP improved from -6.4 in 2009 to 0.2 in 2017. This represents a fiscal consolidation of 6.6 percent of potential GDP over eight years. Innes and Tetlow (2015) estimate that between 2009-10 and 2014-15, English local authorities cut net service spending by 23.4 percent per person on average (in real terms). As a consequence, Fetzer (2019) argues that support for UKIP rose and that austerity played a role in the subsequent Brexit vote. Those results echo similar research

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[1] Speech available here: https://conservative-speeches.sayit.mysociety.org/speech/601367

on the link between austerity and social unrest in Europe (Ponticelli and Voth, 2020) or the rise of the Nazi party in Germany (Galofré-Vilà et al., 2017). Additional research by Stuckler et al. (2017) shows the social costs of these policies by highlighting that austerity in the U.K. correlated with an increase in suicides.

This paper aims to study how the 2012 Welfare Reform Act impacted youth political attitudes and their sense of political representation. Rather than focusing on the political cost and turnover austerity can cause, I shed light on whether and how austerity can influence individual political attitudes, particularly young people's. I show that the welfare cuts that started to be implemented in 2013 in the U.K. had a negative effect on young people's opinion of politicians and made them more prone to political disengagement.

The paper has three main contributions. First it adds the literature on the political consequences of austerity policies and shows that welfare cuts can have political repercussions. Second it provides further evidence that negative income shocks interact with the life cycle, and that young people are more prone to updating their political views in the face of economic hardship. Finally, it highlights trade-offs that government should take into account when facing budget imbalances. In particular, it emphasizes the need to evaluate potential unintended consequences of fiscal policies on certain parts of the population.

My analysis relies on longitudinal survey data from Understanding Society (UnderSoc), which follows a representative sample of 40,000 households from 2009 to 2019 (University of Essex, 2019). I focus on five outcome variables: (i) whether one is interested in politics, (ii) whether one agrees that public officials don't care, and (iii) whether one agrees to have no say in what the government does, (iv) whether one feels a sensation of satisfaction when they vote, and (v) whether they believe their vote will make a difference. The survey includes each respondent's local area district of residence, a unit of analysis that is smaller than counties. This allows me to merge survey responses with a district-level estimate of each respondent's austerity shock. Similarly to Fetzer (2019), I use a difference-in-differences (DiD) with Beatty and Fothergill (2016)'s estimates of regional welfare cuts. They are a proxy of the intensity of the austerity shock in each district. With this, I can compare over time the answers of young individuals from districts exposed to different austerity shocks while controlling for individual and regional characteristics.

On average, my results suggest that austerity significantly affected political attitudes among the British youth. Young people's political efficacy diminished as they were more likely to believe public officials do not care about them and that they have no say in what the government does. In parallel, their sense of satisfaction with politics, measured, for example, by their perceived political influence, also decreased. These results provide grounds to see the welfare cuts as a factor in the lower engagement of young people in the Brexit referendum.

My paper is structured as follows: Section 2.2 provides details on the context of my empirical setting, its policy relevance, and previous literature, section 2.3 describes the data used, section 2.4 presents my empirical strategy, section 2.5 displays the results, finally section 2.6 concludes.

2.2 Context, Research Question, and Literature Review

The Brexit vote was followed by commentaries on young people's low turnout at the poll. Even though the link between age and lower vote turnout has been established in the literature, the low turnout surprised as young people's future was more likely to be affected by the outcome of the referendum and as a majority of them supported the Remain side[2]. Some Op-Eds emerged, with titles in the vein of the FT's"*Young people feel betrayed by Brexit but gave up their voice*" (2016).

In parallel, estimates of youth turnout varied and were revised over time, as no exit poll was conducted. Sky Data, Sky News polling branch, initially evaluated the 18-24 turnout at 36%, but polls conducted later by IpsosMori and Opinium estimated, respectively, that 60% and 64% of registered voters aged 18-24 voted. While not as striking as earlier figures, these numbers are still well below the average turnout of 72% for all age groups. The BBC further documented that counties with a higher share of young people had lower voting turnout during the referendum (BBC, 2021). These numbers are disconcerting given the expected repercussions of Brexit for young people: those aged 18 to 24 will have to live with the consequences of the Brexit vote for an average of 69 years, compared with 16 years for those over 65 (Generation Citizen, 2016). In light of this evidence, one could wonder whether some economic factors played a role in the lower than anticipated turnout for young people. In particular, substantial welfare cuts took place before the Brexit vote that seems to have played a role in the referendum's results (Fetzer, 2019).

A wealth of research examines how political attitudes are developed. A number of studies focus on childhood factors, and in particular the effect of early forces in the political socialization of individuals. They highlight the role played by parents' political orientation on a child's political attitude formation (Jennings, 1996; Jennings and Niemi, 1968; Jennings et al., 2009; Maccoby et al., 1954) as well as the educational system and peers' (Campbell, 1980; Jennings and Niemi, 1974; Tedin, 1980), or even genetic influences (Alford et al., 2005). Another area of research focuses on the influence of recent or contemporary events on political attitudes. Specifically, several papers have demonstrated how economic circumstances can affect political attitudes and electoral outcomes (Durr, 1993; Fiorina, 1978; Kinder and Kiewiet, 1981; Kramer, 1983; Lewis-Beck and Stegmaier, 2000)

[2]More than 70% of the young voters cast a ballot in favor of staying in the EU.

Margalit (2019) lays out four possible outcomes of the impact of economic shocks on political behavior:

- they can lead to an increase in support for left parties and redistributive policies
- the losers of the economic shock can embrace anti-establishment contenders and far-right parties
- they can trigger a vote against the incumbent
- they can lead to a reduced interest in politics and lower the voting turnout

The rise of populism that followed the Great Recession led to considerable research on the second point (see, for example, Algan et al. (2017), Dustmann et al. (2017), and Rhodes-Purdy et al. (2020)). However, recent papers also highlight the role of abstention in the rise of populist and radical right parties (Guiso et al., 2017). Yet, out of the four outcomes Margalit (2019) lists, abstention and a decrease in the interest in politics have been the least developed topic. In fact, evidence on the effect of economic shocks on turnout appears to be still limited and so far points to an interaction with the life-cycle.

Indeed, two papers, Finseraas (2017) and Emmenegger et al. (2017), highlight that economic shocks that take place when young affect political interest and participation. Exploiting the discovery of oil outside the Norwegian county of Rogaland, Finseraas (2017) finds that cohorts that experienced a positive shock in family income during their childhood are more likely to vote. Emmenegger et al. (2017) find a symmetrical effect when analyzing whether being unemployed interacts with the life-cycle stage in depressing political interest. They use German panel data to show that unemployment spells among young adults trigger a drop in political interest, which is not visible later in life. They explain this finding by young people being more malleable by the economic conditions they live in while forming their opinion. This is consistent with the "impressionable years" or "formative years" theory from the psychology literature, according to which adolescence and early adulthood are more sensitive to major life events and can lead someone to revise their preferences (Krosnick and Alwin, 1989; Sears, 1983). That susceptibility decreases thereafter and remains low during the rest of the life cycle.

Hence, this paper adds to a small but growing literature on how the experience of economic shock during formative years increases young people's detachment from politics. In particular, I test the following hypotheses:

- *H1: The welfare cuts led to decreased political efficacy/sense of political representation among young people.*
- *H2: Such effect was not visible in other age groups.*

I treat the welfare cuts as a negative income shock and interpret my results in line with the cited literature. However, I should note that the decrease in political efficacy I observe among young people could also reflect a political disappointment following the government's decision to cut benefits. I am not able to disentangle these two effects.

Another limitation of my paper is its focus on attitudes towards politicians and voting, which I measure with survey data. Therefore I can only see how declared political beliefs evolve and do not evaluate any attitudinal feedback on real-life political outcomes. Nevertheless, although political interest and attitudes towards politicians are conceptually different from political behavior, they remain important determinants of political participation and views of the political system (Brady et al., 1995; Powell, 1986).

Finally, this paper contributes to two additional strands of literature. The first one is in labor economics and suggests economic shocks have heterogeneous effects depending on when they occur in a person's life. Negative labor shock when young have been shown to have long-term consequences in terms of earnings (Oreopoulos et al., 2012; Schwandt and Von Wachter, 2019), the likelihood of facing poverty (Schwandt and Von Wachter, 2019), many social outcomes ranging from fertility decisions, marriage, and divorce to criminal activities, risky alcohol consumption, and lower health outcome (Von Wachter, 2020), as well as the choice of major and line of job (Cotofan et al., 2020). Secondly, it adds to the debate on the political cost of austerity. While some papers find no link between austerity and a fall in popularity of governments and/or electoral defeat (Alesina and Ardagna, 2010; Alesina et al., 1998; Arias and Stasavage, 2019), others observe a relation between fiscal consolidations and social instability (Ponticelli and Voth, 2020; Vegh and Vuletin, 2014). Finally, and as cited above, Fetzer (2019) shows that austerity in the U.K. caused a rise in the popularity of UKIP, the pro-Brexit political party. My results support the latter side of the debate.

2.3 Data

The post-recession period saw the implementation of severe austerity measures in the UK, triggered by the election of a Conservative-led coalition government in 2010. Fetzer (2019) describes three implementation phases. The first one consisted of budget cuts across most Westminster departments in 2010. Local governments experienced a decrease in funding to conduct their daily activities. Nominal wage freezes were then implemented for public sector employees from 2011 to 2013, with a public-sector wage growth cap set at 1 percent in 2014. Finally, a reform of the welfare state took place, with the 2012 Welfare Reform Act. This reform is the bulk of the austerity plan advocated by the ruling government and consisted of substantial welfare cuts among several dimensions.

Beatty and Fothergill (2016)[3] provides a quantitative estimate of these welfare cuts,

[3]Fetzer (2019) uses Beatty and Fothergill (2013)'s estimates, which were expected cuts. I use the

focusing on ten measures. These measures encompass changes in housing benefits, non-dependent deductions, household benefit caps, council tax benefits, disability living allowances, incapacity benefits, child benefits, tax credits, and the reduction in annual up-rating of working-age benefits. While estimates, their figures are “deeply rooted in official statistics" and come, for example, from “the Treasury's own estimates of the financial savings, the government's Impact Assessments, and benefit claimant data" (Beatty and Fothergill, 2016, p.6). Overall, they calculate that these measures yielded savings of almost £14 billion a year by 2016. Taken as a share of the benefit claimants, this amounts to an average welfare loss of £345 per working-age adult, per annum[4].

This average hides considerable variation between districts. Indeed, the estimated annual financial losses per working-age adult range from £100 in the City of London to £720 in Blackpool, with a standard deviation of £84. Figure 2.1 displays the geographical variation of the austerity shock, per annum, across districts in the UK. I use this estimate as a proxy for the regional intensity of the austerity measures voted in 2012. In particular, I compare individuals across districts subject to different austerity shocks.

There are two important caveats to my analysis. First, the austerity shock's regional variation is driven by the heterogeneity in the distribution of the benefits claimants over the country. This could imply a bias in my estimation as the districts most affected by the welfare cuts are mainly populated by vulnerable households. To address this issue, I include individual and district-level fixed effects in my estimation.

Second, the austerity shock provided by Beatty and Fothergill (2016) contains a set of measures implemented before the election of the Conservative-led government. In particular, the Labor party introduced the Employment and Support Allowance (ESA). The Conservatives-led coalition then added new elements to it, such as the time-limiting and non-means-tested ESA. Therefore, my austerity shock is partly “contaminated" by additional measures that pre-date the 2012 Welfare Reform Act. Nevertheless, the vast majority of the austerity cuts took place as part of the Welfare Reform Act. In addition, I expect the austerity cuts to increase negative attitudes towards politicians and politics in general. Therefore, an overestimation of the fiscal shock will likely lead to underestimating the elasticity of political attitudes to austerity. Hence, my estimates should be interpreted as lower bounds.

I combine this regional austerity shock with survey data from the Understanding Society (UnderSoc) database (University of Essex, 2019). The UnderSoc is a longitudinal survey run by the Institute for Social and Economic Research at the University of Essex, which replaced the British Household Panel Survey (BHPS) in 2009. It follows a representative sample of 40,000 British households across nine waves, from 2009 till 2019, and

updated estimates from Beatty and Fothergill (2016), which were revised downward by £5 billion.

[4]The average amount is of £363 per working-age adult, per annum, if you include Northern Ireland. Because the sample of respondents from Northern Ireland is small, I drop it from the analysis.

Figure 2.1: Distribution of the austerity shock in the UK - Source: Beatty and Fothergill (2016)

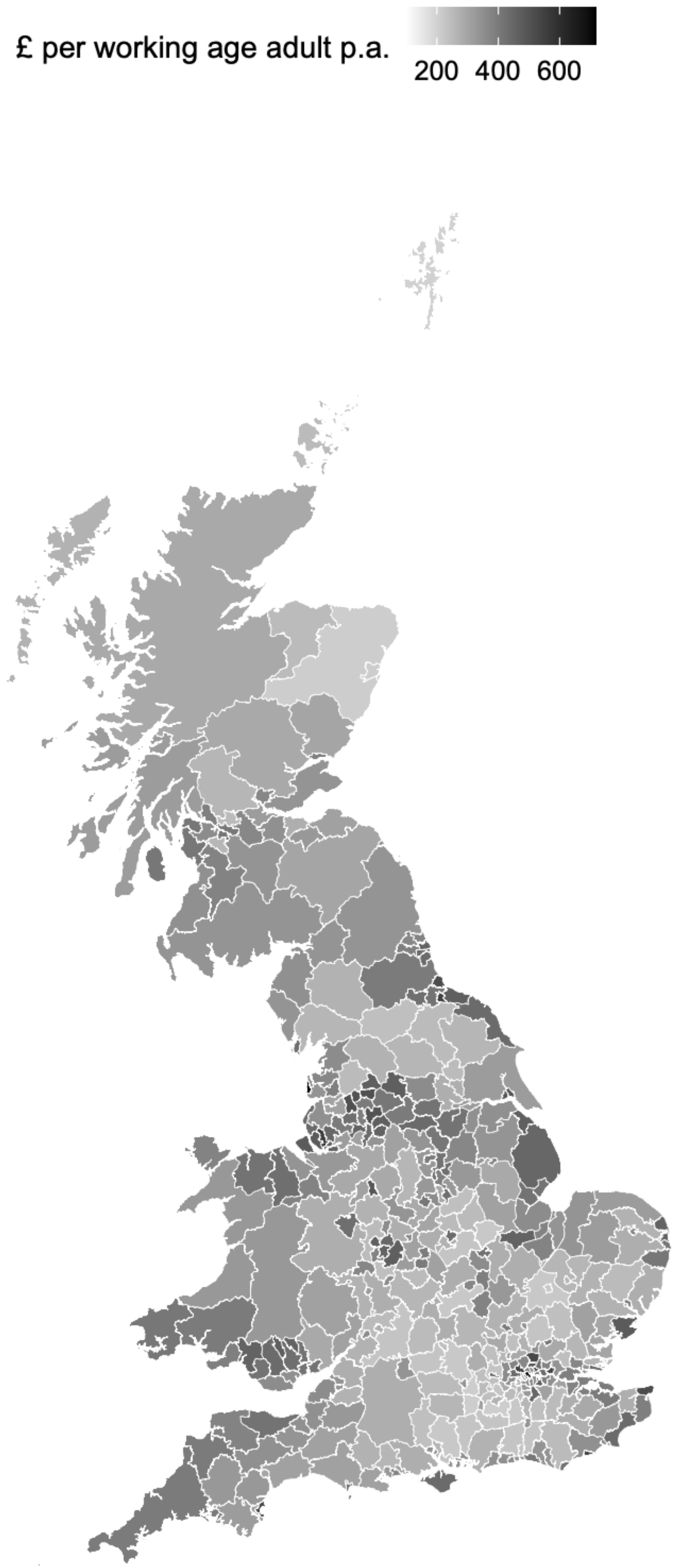


Table 2.1: UnderSoc Waves and Corresponding Years

Wave number	Year
1	Jan 2009- Jun 2011
2	Jan 2010- Jun 2012
3	Jan 2011- Jun 2013
4	Jan 2012- Jun 2014
5	Jan 2013- Jun 2015
6	Jan 2014- Jun 2016
7	Jan 2015- Jun 2017
8	Jan 2016- Jun 2018
9	Jan 2017- Jun 2019

interviews them on socio-economic issues. One particularity, compared to BHPS, is that the interviews are conducted over 2.5 years and that the waves overlap. Table 2.1 maps each wave to its corresponding years. In general, each individual is interviewed around the same time every year, yet I structure my dataset as a wave-individual panel rather than a year-individual panel. The UnderSoc database provides the local authority district of residence of each respondent. Therefore I can map each individual to its corresponding regional austerity shock, my main explanatory variable.

I use five dependent variables of political attitudes from this survey:

1. **Public officials don't care** stems from the question *"How far do you agree or disagree with the following statements? Public officials don't care much about what people like me think".* Respondents can strongly agree, agree, neither agree/disagree, disagree, or strongly disagree. It is coded 1 if the respondent answered strongly agree or agree.

2. **No say in what government does** follows the same format as above: people are asked whether they agree with the statement *"People like me don't have any say in what the government does".* It is coded 1 if the respondent answered strongly agree or agree.

3. **Interest in Politics** is created from the question *"How interested would you say you are in politics? Very interested, fairly interested, not very interested, or not at all interested".* It is coded 1 if the respondent answered very interested or fairly interested.

4. **Personal benefit from voting** asks respondents whether they strongly agree, agree, neither agree nor disagree, disagree, or strongly disagree with the following statement "*I feel a sense of satisfaction when I vote*". It is coded 1 if the respondent answered strongly agree or agree.

5. **Perceived political influence** asks respondents “On a scale from 0 to 10, where 0 means very unlikely and 10 means very likely, *how likely is it that your vote will make a difference* in terms of which party wins the election in this constituency at the next general election?" and was entered as is in the regression.

These variables correspond to standard measures of political efficacy and sense of political representation. In particular, question 1 has been used as a measure of external political efficacy in the past, while questions 2 and 5 are usually used to measure internal political efficacy (Craig and Maggiotto, 1982; Craig et al., 1990; Miller and Traugott, 1989; Niemi et al., 1991)[5]. Question 1 and 4 are additional measures of political attitudes that correlate with voting.

While the first question is asked at all waves, the second and third ones are asked at waves 3, 6, and 9, and the latter two at waves 2, 3, 6, and 9[6]. I choose to exclude the waves that occurred after wave 6 for two reasons. First, the Conservatives voted for new austerity measures in 2015, and hence the austerity shock is different starting in 2016. In addition, the Brexit vote took place in June 2016 and likely led to significant adjustments in political attitudes. Indeed, intense political turmoil took place following the Brexit vote, and polls highlighted that some Brexiters regretted their voting choice[7]. Therefore, I choose to focus on the period pre-Brexit and post-Welfare Reform Act to estimate how austerity affected political attitudes in the UK.[8]

I use the longitudinal nature of the survey and run a difference-in-differences (DiD) where 2013 is the start of my treatment [9]. I use the longitudinal weights provided by *Understanding Society* to ensure that my analysis remains representative of the British population over the different waves. Further details about my empirical strategy are provided in section 2.4.

Table 2.2 provides summary statistics of my five dependent variables for waves 2, 3, and 6, for the whole sample and young people. On average, half of the respondents declare

[5]As defined by Miller and Traugott (1989), internal political efficacy measures “individuals' self-perceptions that they are capable of understanding politics and competent enough to participate in political acts such as voting", while external efficacy measures “expressed beliefs about political institutions rather than perceptions about one's own abilities[...] The lack of external efficacy [...] indicates the belief that the public cannot influence political outcomes because the government leaders and institutions are unresponsive to their needs".)

[6]I note that these two questions in wave 2 were asked only of the following samples: Ethnic Minority Boost, General Population Comparison sample or Low Density Ethnic Minority Area sample. However, in wave 3, these questions were asked only of people not in these samples, hence these questions were asked over two waves. I use data from both wave 2 and 3 and use longitudinal weights to correct for it when I conduct my analysis. The questions was asked to the entire sample of UnderSoc in wave 6

[7]See for example the EURef2 Poll of Polls that take the average share of the vote for ‘Leave' and ‘Remain' in the six most recent polls of how people would vote if they were to be presented once again with the choice of either leaving the EU or remaining a member (available at `https://whatukthinks.org/eu/opinion-polls/euref2-poll-of-polls-2/`).

[8]Wave 6 is conducted from January 2014 till June 2016. UnderSoc database records the date of the interview and I am able to see that none of the responses from wave 6 were recorded after the 23rd of June 2016, date of the Brexit Referendum.

[9]While the Welfare Reform Act was voted in 2012, its implementation started in 2013

they are interested in politics, agree that public officials don't care and that they have no say in what the government does, and feel a sense of satisfaction from voting. The mean perceived political influence is 3.22 (out of 10). Those numbers are lower for younger people, implying that their sense of political representation is lower than the population's average. This is consistent with lower turnout rates and lower electoral registration among young people. The standard deviations in table 2.2 are large as the variables are dummies or categorical.

Table 2.2: Dependent Variables - Summary Statistics

	Mean	S.D.	N
Whole sample			
Public officials don't care	0.49	0.500	37539
No say in what govt does	0.50	0.500	37741
Interest in politics	0.47	0.499	56722
Personal benefit from voting	0.52	0.500	37816
Perceived political influence	3.22	3.210	36946
Young people			
Public officials don't care	0.42	0.493	5281
No say in what govt does	0.45	0.498	5320
Interest in politics	0.37	0.482	8187
Personal benefit from voting	0.36	0.479	4537
Perceived political influence	2.91	2.972	4828

Estimated with longitudinal weights.

I construct indices following a principal component analysis to reduce the risk of multiple hypothesis testing and because my dependent variables are correlated (see table2.3). I group the variables according to their level of correlation and create a first index that measures the level of low political efficacy in my sample with the variables *Public officials don't care* and *No say in what government does*, and a second one that measures the level of satisfaction with politics with the variables *Interest in Politics*, *Personal benefit from voting*, and *Perceived political influence.* Grouping the variables this way will also make the interpretation easier as the first and second indices correlate negatively.

Table 2.3: Correlations between the outcome variables

	Public officials don't care	No say in what govt does	Interest in politics	Personal benefit from voting	Perceived pol. influence
Public officials don't care	1				
No say in what govt does	0.56	1			
Interest in politics	-0.09	-0.13	1		
Personal benefit from voting	-0.09	-0.13	0.34	1	
Perceived political influence	-0.14	-0.17	0.20	0.30	1

2.4 Empirical strategy

2.4.1 DiD

I first run a standard DiD specification (equation 2.1) for young people and "non-young" people separately, using the austerity shock as treatment.

$$y_{i,d,w,t} = \alpha_0 + \alpha_1 \times \mathbf{1}(Year \geq 2013) \times Austerity_d + \lambda_i + \gamma_t + \nu_d + \epsilon_{i,d,w,t} \tag{2.1}$$

where $y_{i,d,w,t}$ corresponds to one of my outcome variables of political attitudes for individual i, living in district d, at wave w and time t; α is a constant intercept across all individuals, districts, wave, and periods; λ_i is an individual fixed effect; γ_t is a year fixed effect; ν_d is a district-level fixed effect. I cluster the standard errors at the year and district levels.

My main explanatory variable, $Austerity_d$, takes the value of the austerity shock in district d (described in figure 2.1) when the year is superior or equal to 2013. This is equivalent to having it take the value of the austerity shock for wave 6 only. One particularity of my estimation is that the treatment is a continuous variable that applies to all individuals after 2013. My results come from the variation in the intensity of the treatment and imply linearity in reaction to the treatment.

Equation 2.1 is similar to Fetzer (2019)'s, with the exception that my explanatory variable is at the district level while my measure of political attitude is at the individual level. Hence, I aim to capture how macroeconomic conditions influence one's views on government rather than individual conditions. Nevertheless, my measure of austerity stems from the heterogeneous distribution of benefit claimants over the UK. This implies that districts with more welfare beneficiaries will encounter a higher austerity shock and might be more prone to adverse consequences on political attitudes as their inhabitants rely more on the welfare state. To address this issue, I use fixed effects at the district and the individual level. The former controls for regional specificities that might explain why some regions react differently than others to the austerity shock, while the latter controls for the same characteristics for individuals. In particular, my district fixed effects allow me to control for the impact of secular structural shocks at the regional level, such as import competition or automation, that could impact political attitudes. The use of individual fixed effects enables me to compare the evolution of an individual's reaction to the austerity shock, controlling for all the individual fixed characteristics that determine her/his preferences.

One should note that the fixed effects control for individual and community characteristics that may influence political engagement and may be correlated with the austerity shock, but not for the heterogeneous impacts of the austerity shock. Indeed, the impact on political attitudes could come from (i) districts with larger cuts having more people affected by the cuts, (ii) districts with larger cuts experiencing larger cuts in benefits, or (iii) people living in districts with larger cuts reacting more intensely to the cuts. I cannot distinguish between the three channels here.

I define young people as individuals that are 25 years old or younger[10]. Running equation2.1 on the sample of young people will give us an estimate of how young people updated their political attitudes following the austerity shock. I can then compare it to the estimate I obtain when running the same equation on the sample of individuals that are older than 25.

For my results to be credible estimates, I need to assume that the parallel assumption holds between people living in different districts. This means assuming that the expected variation in political attitudes of people living the most affected districts and those living in the least affected ones would have been the same. I cannot test for the parallel trend assumption here and return to this issue in section 2.4.3. Comparing the estimates I get for young and non-young enables me to relax this assumption, providing that the bias is the same for the two groups living in the same district.

2.4.2 DiD with interaction terms

Another way to look at the effect of austerity on young people is to add interaction terms. I run a new specification, similar to equation 2.1, where I interact the continuous treatment variable interacted with a dummy for young people. In practice, I add the terms $\beta_2 \times \mathbf{1}(Year \geq 2013) \times Young_i \times Austerity_d$ and $Young_i \times \gamma_t$ to equation 2.1. It takes the following form:

[10] I also test the sensitivity of my results to the age threshold later on.

$$
\begin{aligned}
y_{i,d,w,t} = \beta_0 + \beta_1 \times \mathbf{1}(Year \geq 2013) \times Austerity_d \\
+\beta_2 \times \mathbf{1}(Year \geq 2013) \times Young_i \times Austerity_d \\
+\lambda_i + \gamma_t + Young_i \times \gamma_t + \nu_d + \epsilon_{i,d,w,t}
\end{aligned} \tag{2.2}
$$

where $Young_i$ is a dummy for individuals younger than 25. I also cluster the standard errors at the year and district levels.

This second equation enables me to check whether young people have been more affected by the welfare cuts than their older peers. If significant, my coefficient β_2 would provide additional evidence of an heterogeneous effect of austerity on the British youth. This would be while controlling for the impact of the austerity shock in general with the term $\beta_1 \times \mathbf{1}(Year \geq 2013) \times Austerity_d$ and for the "usual" evolution of young people's political attitudes over time with $Young_i \times \gamma_t$.

While the specification is close to a triple DiD, it displays several important distinctions. First, a triple DiD would also include the interaction of each of the three components with each other and each component as a fixed effect. In this case, it means adding the interaction $Young \times Austerity_d$ and a fixed effect for young people. Yet, including these items risks saturating my model as the individual fixed effects already control for these two items. Second, the treatment here affects everyone, with a different intensity. Ideally, with a triple DiD, the causal effect estimate will come from changes in the treatment units, and not changes in the control units. For this identification strategy to be a perfect triple DiD, I would need to have an austerity treatment that only affects young people, and not older cohorts.

Hence, specification 2.1 gives me an estimate of how people updated their attitudes following the austerity shock by comparing them to individuals from the same age category that lived in a different location. On the other hand, specification 2.2 provides an estimate of how young people reacted differently than elder peers subsequently to the welfare cuts, while controlling for a number of time variant specificities.

2.4.3 Potential Threats to Validity

There are several threats to the validity of my results. First, respondents might move between the second, third, and sixth waves of the survey. This could bias my results as the evolution of one's political attitudes could change following their move to a different district. This is particularly relevant in this setting as young people are more prone to moving than other age cohorts. To control for it, I re-run my analysis, restricting my sample to non-movers only. These results, presented in section 2.5.3, show that there is no significant difference whether I use my sample of non-movers or all survey respondents. This is because few people moved between wave 2 or 3 and wave 6.

Second, as I compare three or two waves, I only have two to three data points per individual. In addition to the fact that I use a continuous treatment, this means I cannot test for the parallel trend assumption. A potential alternative would be to perform a falsification test with an alternative dependent variable. In my case, finding an appropriate alternative variable is complicated as it is unlikely that austerity influences political attitudes without affecting other social variables. This is in combination with another drawback of my empirical strategy: I cannot use district $\times$ year fixed effects to control for district-specific trends. Indeed, they would be collinear to my explanatory variable. Therefore, my identification relies on using the correct functional form and sufficient controls with individual, time, and district fixed effects. This means that I postulate that no other shock at the district level is collinear or dependent on the austerity shock I use. Hence, I cannot claim to identify any clear causal effect, especially as the effects of the Great Recession were still lingering.

Thirdly, I consider that the austerity shock started in 2013, which raises two issues. First, some respondents in wave 3 were interviewed in 2013, which can lead to a downward bias in my coefficients. I drop these respondents in a robustness check and show that it does not change my results (section 2.5.3). In fact, most of the interviews take place during the first two years of each wave, and respondents from wave 3 interviewed in 2013 account for only 6 percent of the wave's sample. Secondly, the Conservatives' austerity plan included three phases and started in 2010, as described earlier. The 2012 Welfare Reform Act is the latest component, which means that political attitudes might have been affected by austerity measures earlier to waves 2 and 3. As a result, my coefficient should be seen as a lower bound of the effect of these austerity measures on political attitudes.

Overall, equations 2.1 and 2.2 are a very demanding specification that include about 20,000 fixed effects. As discussed above, a more accurate identification strategy should control for district-specific trends, this is not possible in this context. Therefore, while I control for many factors, my results can only be seen as suggestive and not causal.

2.5 Results

2.5.1 Main specification

Table 2.4 presents the standardized coefficients of the DiD using the principal component indices[11]. We see that, on average, the index of low political efficacy increases by 0.44 for a one standard deviation increase in the welfare cuts. In contrast, the index of satisfaction with politics decreases by 0.35. It shows that young people affected by the welfare cuts were significantly more likely to update their political attitudes. We compare these results with those for "non-young" people, i.e., individuals older than 25. Table 2.5 shows no

[11] All variables are normalized before constructing the principal component indices.

significant impact, even though the sample is much bigger. Hence, the association between the welfare cuts and lower political efficacy and satisfaction with politics is only visible among young adults.

One drawback of principal component indices is that they make interpreting the results more difficult. Hence, we re-run our analysis on the sub-components that comprise the indices. Table B.1 and table B.2 display the results for young people and older generations, respectively. We see that an increase in welfare cuts increases the probability that a young person feels public officials do not care and that they have no say in what the government does by 0.13 of an s.d. and 0.18 of an s.d., respectively. In parallel, it decreases their perceived level of political influence by 0.76 of an s.d. The coefficients for the variables *Interest in Politics* and *Personal benefit from voting* are much smaller and not significant. Nevertheless, they exhibit negative signs and are almost significant at the 15% level and 10% level, respectively. It indicates that the results obtained on the index of satisfaction with politics in table 2.4 are not just driven by the variable measuring one's perceived political influence[12].

The results are also quite sizeable, especially compared to the dependent variables' means for young people before austerity, which are 0.37 for *Interest in politics*, 0.41 for *Public officials don't care*, 0.45 for *No say in what the government does*, 0.34 for *Personal benefit from voting*, and 2.87 for *Perceived political influence.*

When focusing on individuals older than 25 (table B.2), the results disappear. I also run the same specification on different age cohorts separately. Tables B.4, B.5, B.6, B.7, and B.8 display the results for the age categories 25-35, 35-45, 45-55, 55-65, and those 65 and over. The results are not significant, except for individuals aged 65 and over who exhibit a decrease in political efficacy following the cuts (at the 10% level), but no associated effect on their satisfaction with politics. Running the same equation on the sub-component of the index (see table B.13) indicates it is likely driven by an increase in the view they have no say in what the government does (at the 10% level). In parallel, we can note that the perceived benefit from voting increases (also at the 10% level). This could imply that a political backlash from the austerity cuts was more likely to emanate from individuals older than 65.

Tables B.9 to B.12 display the breakdown results for the other categories. Unsurprisingly it paints a very similar picture. Two things stand out. First, individuals between 55 and 65 are less likely to declare that public officials don't care following the welfare cuts. This could suggest some support for the welfare cuts in this age category. Second, the coefficient on *Public officials don't care* for the 35-45 category is significant and positive (see table B.10). It is the only significant coefficient for that age category, and the coefficient's magnitude is about half that for young people. There is no clear interpretation

[12]The sample of respondents for column 4 is also much smaller which partially explain the lower significance.

of it. The result could mean this age category displayed lower levels of political efficacy following the cuts. The result could also come from multiple hypothesis testing. In fact, the index of low political efficacy remains non-significant for this age group.

Overall, no other age category exhibit the same patterns as young people. The significance and magnitude of the coefficients imply that the 2012 austerity policies impacted youth political attitudes. It suggests that the effects of the benefit cuts interact with the life-cycle stage, and in particular young people were more prone to feel politically marginalized following the austerity. These results echoes those of Emmenegger et al. (2017) and imply that (i) young people's attitudes are more sensitive to economic shocks and that (ii) negative income shock lowers their feeling of political efficacy and satisfaction with politics.

Table 2.4: Impact of the regional welfare cuts on young people's political attitudes - Indices

	(1) Index - Low Political Efficacy	(2) Index - Satisfaction with Politics
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$	0.440***	-0.345***
	(0.1388)	(0.1243)
Constant	-0.208***	-0.399***
	(0.0024)	(0.0039)
Individual FE	Yes	Yes
District FE	Yes	Yes
Year FE	Yes	Yes
Observations	2000	1194

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table 2.5: Impact of the regional welfare cuts on individuals older than 25 - Indices

	(1) Index - Low Political Efficacy	(2) Index - Satisfaction with Politics
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$	0.0442	0.0359
	(0.0342)	(0.0324)
Constant	0.0385***	0.104***
	(0.0000)	(0.0000)
Individual FE	Yes	Yes
District FE	Yes	Yes
Year FE	Yes	Yes
Observations	32106	30701

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

2.5.2 Specification with interaction terms

I then look at the heterogeneous effect of the welfare cuts to estimate whether young people have been differentially affected by the austerity cuts compared to older cohorts. Table 2.6 presents the results using the index variables and table B.3 for each sub-component. We can see from both tables that young people have been differentially affected. They were more likely to express feelings of low political efficacy and lower satisfaction with politics following the austerity cuts. The only variable that remains non-significant is *Interest in politics* from table B.3, which is consistent with table B.1's results.

The estimate of our coefficient of interest, β_2 from equation 2.2, is much smaller than the estimate of α_1 in tables 2.4 and B.1. This is not surprising as β_2 measures the differential response to the austerity cut between young and "non-young" people from the same districts, where the austerity cuts impacted both control and treatment units.

Table 2.6: Differential impact of the regional welfare cuts on young people's political attitudes - Indices

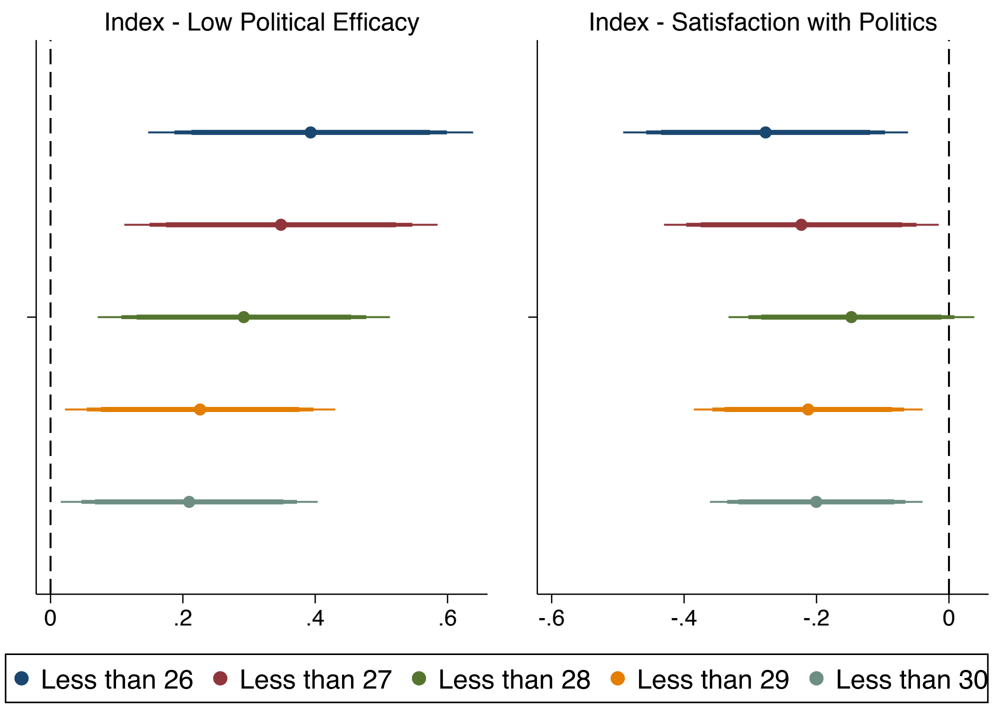
	(1) Index - Low Political Efficacy	(2) Index - Satisfaction with Politics
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$	0.0438	0.0383
	(0.0334)	(0.0315)
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$ $\times$ Young	0.123***	-0.0891***
	(0.0445)	(0.0302)
Constant	-0.000932	0.0605***
	(0.0045)	(0.0030)
Individual FE	Yes	Yes
District FE	Yes	Yes
Year FE	Yes	Yes
Young $\times$ $\mathbf{1}(Year \geq 2013)$	Yes	Yes
Observations	34940	32666

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

I perform the same exercise on other age groups to see if the differential reaction observed among young people holds for older cohorts. Tables B.14, B.15, B.16, B.17, and B.18 in the appendix, show that the austerity shock did not lead to a differential response in other age-groups compared to the average. The coefficient on $\mathbf{1}(Year \geq 2013) \times Austerity_d$ becomes positive for the Low Political Efficacy Index once the sample of young people is added in[13]. This implies that young people drive the result.

[13] They are not "taken out" of the average coefficient on $\mathbf{1}(Year \geq 2013) \times Austerity_d$ by the interaction term anymore.

Figure 2.2: Coefficient stability of the austerity shock when varying the upper age threshold for young people, Indices



We can also note that the 55-65 y.o. appear to be affected differently but in the opposite direction. They are less likely to express a low political efficacy following the welfare cuts compared to the population average, in line with the results from table B.10.

Overall, these results convey that young people were more affected by the welfare cuts than the rest of the population. Austerity seems to have led to increased political disengagement among the British youth, a subpopulation already prone to lower political turnout and voting registration[14]. Other age cohorts do not appear to have been affected the same way by the welfare cuts.

2.5.3 Robustness Checks

I perform several robustness checks to test the validity of my results.

I first re-run equation 2.2 using different upper thresholds when I define who is young. In particular, I re-run my equation for individuals younger than 26, 27, 28, 29, and 30. Figure 2.2 and B.1 display how stable the estimate of the coefficient α_1 is for different samples of young people. Overall, the coefficients of the index variables (figure 2.2) remain stable over time but tend to lose some significance as I expand the sample to older individuals. The same applies when I focus on each sub-component (figure B.1) with less stability for the coefficients *Interest in politics* and *Personal benefit from voting*.

[14]See for example Commission (2014) for an estimate of voting registration by age in the U.K.

I also re-run equation 2.2 excluding responses from 2013 to see if the downward bias on my coefficients is important. Table 2.7, with the indices, and table B.24, with each variable, show my results remain stable. I note however that column 4 from table B.24, *Personal benefit from voting*, becomes significant. Overall, these results suggest there is no significant downward bias from including data from the year 2013. This is in line with expectations as only 6 percent of wave 6's interviews took place in 2013.

Table 2.7: Impact of the regional welfare cuts on young people's political attitudes - Excluding responses from 2013 - Indices

	(1) Index - Low Political Efficacy	(2) Index - Satisfaction with Politics
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$	0.449***	-0.348***
	(0.1410)	(0.1244)
Constant	-0.227***	-0.407***
	(0.0024)	(0.0039)
Individual FE	Yes	Yes
District FE	Yes	Yes
Year FE	Yes	Yes
Observations	1950	1178

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

As stated previously, Conservatives voted for new austerity measures in 2015, which changes the austerity shock individuals are subject to from 2016 onwards. A few of the respondents in wave six were interviewed in 2016, which could bias my results. Hence, I conduct the same analysis, dropping respondents that answered in 2016. Table 2.8 shows it does not change my results, which is not surprising as respondents interviewed in 2016 account fo 3% of the sample[15].

Another potential bias arises from people moving to another local area district between wave 2 or 3, and wave 6. For example, an individual who used to live in a community that suffered from substantial welfare cuts might update her political attitudes even though she might now live in a lightly affected area. The opposite could apply to a person moving from a lightly affected area to a heavily affected one. I address this issue by restricting my sample to non-movers and keeping respondents who reside in the same district in waves 2 and 6 and 3 and 6. Table 2.9 shows that my results hold when I restrict my sample to non-movers[16]. This is consistent with only 6.2 percent of survey respondents moving to another district between wave 3 and wave 6, and 7.6 percent between wave 2 and 6.

[15]See table B.25 for a breakdown of the results by variable.

[16]See table B.26 for a breakdown of the results by variable. District fixed effects are dropped as they become collinear with individual fixed effects.

Table 2.8: Impact of the regional welfare cuts on young people's political attitudes - Excluding responses from 2016 - Indices

	(1) Index - Low Political Efficacy	(2) Index - Satisfaction with Politics
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$	0.436***	-0.350***
	(0.1427)	(0.1244)
Constant	-0.201***	-0.376***
	(0.0023)	(0.0037)
Individual FE	Yes	Yes
District FE	Yes	Yes
Year FE	Yes	Yes
Observations	1894	1142

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table 2.9: Impact of the regional welfare cuts on young people's political attitudes - Sample of non-movers only - Indices

	(1) Index - Low Political Efficacy	(2) Index - Satisfaction with Politics
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$	0.450***	-0.341***
	(0.1448)	(0.1283)
Constant	-0.147***	-0.441***
	(0.0025)	(0.0037)
Individual FE	Yes	Yes
District FE	Yes	Yes
Year FE	Yes	Yes
Observations	1706	1035

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Finally, I check whether some outliers are driving my results and drop observations for which the austerity shock is in the 99th ($\geq$£714) or 1st ($\leq$£263) percentile of the distribution. In other words, I discard individuals that were subject to the strongest and and weakest intensity of my austerity shock to see if they are driving my estimates. Table 2.10 shows that my results do not change much when I discard the highest values of the austerity shock[17]. Table 2.11 show a similar picture when I discard the lowest values of the austerity shock[18]. Overall, this provides ground to believe outlier values of the austerity shock do not drive my results.

Table 2.10: Impact of the regional welfare cuts on young people's political attitudes - Discarding largest values of austerity shock - Indices

	(1) Index - Low Political Efficacy	(2) Index - Satisfaction with Politics
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$	0.430***	-0.300**
	(0.1511)	(0.1278)
Constant	-0.210***	-0.397***
	(0.0027)	(0.0041)
Individual FE	Yes	Yes
District FE	Yes	Yes
Year FE	Yes	Yes
Observations	1992	1191

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

[17]See table B.27 for a breakdown of the results by variable.

[18]See table B.28 for a breakdown of the results by variable. I note that the variables *Interest in politics* and *Personal benefit from voting* become significant at the 5% and 10% significance levels. This remains close to the results from table B.1 with coefficients of comparable magnitudes, same signs, and that were close to being significant at the 15% and 10% level.

Table 2.11: Impact of the regional welfare cuts on young people's political attitudes - Discarding smallest values of austerity shock - Indices

	(1) Index - Low Political Efficacy	(2) Index - Satisfaction with Politics
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$	0.410**	-0.440***
	(0.1610)	(0.1583)
Constant	-0.177***	-0.421***
	(0.0019)	(0.0046)
Individual FE	Yes	Yes
District FE	Yes	Yes
Year FE	Yes	Yes
Observations	1764	1069

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

2.6 Conclusion

These results provide further evidence of the social and political costs associated with the 2012 Welfare Reform Act. Specifically, it demonstrates the significant impact of the welfare cuts on young people's political attitudes in the UK. The study reveals that austerity measures have adversely affected their views on politics and politicians, as well as their sense of political representation. These findings suggest that welfare cuts can contribute to the political marginalization of certain groups in society and, in this specific context, of those who are already exhibiting lower levels of political participation and pronounced feelings of political disenfranchisement.

Given that political disaffection can translate into lower political participation, it is essential that governments consider the attitudinal and political costs associated with austerity policies. For instance, ex-ante assessments of the social impact of austerity policies could be conducted to better evaluate the trade-offs associated with welfare cuts. Such exercises could help prevent the further ostracization of parts of the population, which can undermine political legitimacy.

Furthermore, this paper contributes to the literature on the political economy of austerity, providing additional evidence that welfare cuts affect political attitudes. It suggests that some of the previous studies that reported average null results may be hiding important variations in the impact of welfare cuts on different groups of people.

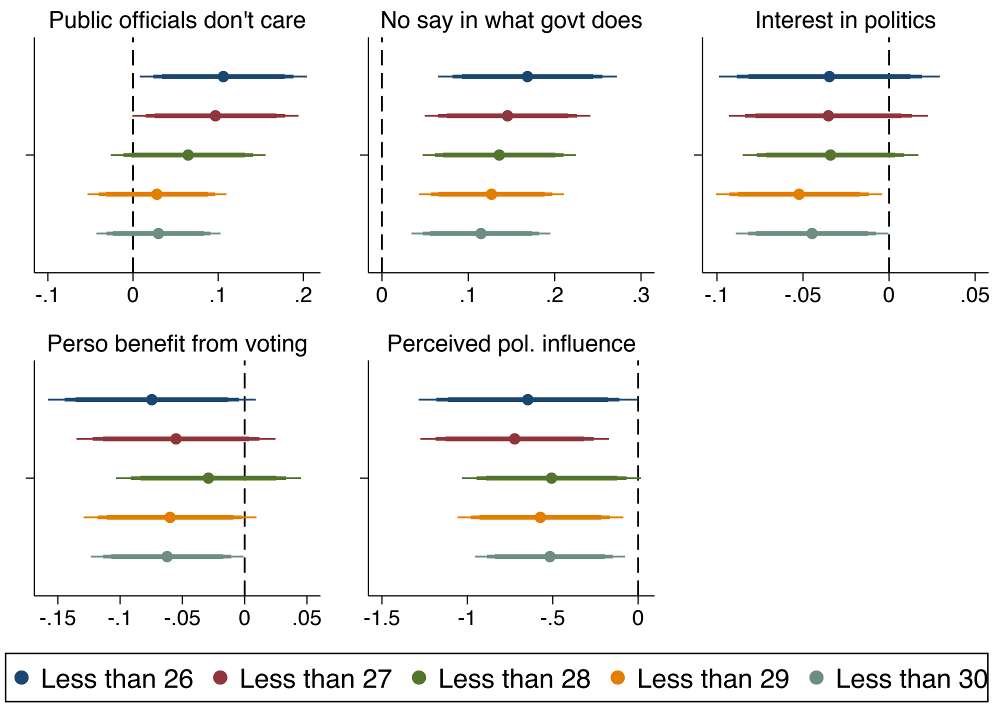
Lastly, this study adds to the research on economic shocks, political turnout, and their interaction with the life-cycle. It offers further evidence that young people react differently to some economic shocks, in this case welfare cuts, and sheds light on some of the mechanisms underlying the low levels of political engagement of young people.

It implies that policies aimed at re-equilibrating public finances can have considerable political impacts on the younger segment of society. Overall, these findings underscore the need for an assessment of the heterogeneous effects of some economic policies and their consequences on youth political engagement.

Appendices - Chapter 2

B.1 Figures

Figure B.1: Coefficient stability of the austerity shock when varying the upper age threshold for young people



B.2 Tables

Table B.1: Impact of the regional welfare cuts on young people's political attitudes

	(1) Public officials don't care	(2) No say in what govt does	(3) Interest in politics	(4) Perso benefit from voting	(5) Perceived pol. influence
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$	0.127**	0.182***	-0.0514	-0.0811	-0.761**
	(0.0566)	(0.0567)	(0.0363)	(0.0497)	(0.3511)
Constant	0.410***	0.431***	0.334***	0.315***	2.882***
	(0.0010)	(0.0010)	(0.0021)	(0.0014)	(0.0090)
Individual FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	2022	2040	3941	1405	1661

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table B.2: Impact of the regional welfare cuts on individuals older than 25

	(1) Public officials don't care	(2) No say in what govt does	(3) Interest in politics	(4) Perso benefit from voting	(5) Perceived pol. influence
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$	0.0137	0.0173	-0.00258	0.0168	-0.0229
	(0.0140)	(0.0128)	(0.0085)	(0.0113)	(0.1026)
Constant	0.508***	0.507***	0.492***	0.553***	3.309***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0001)
Individual FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	32266	32580	50741	33418	31295

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table B.3: Differential impact of the regional welfare cuts on young people's political attitudes

	(1) Public officials don't care	(2) No say in what govt does	(3) Interest in politics	(4) Perso benefit from voting	(5) Perceived pol. influence
Austerity × $\mathbf{1}(Year \geq 2013)$	0.0110	0.0193	-0.00388	0.0169	-0.0334
	(0.0135)	(0.0126)	(0.0085)	(0.0112)	(0.1010)
Austerity × $\mathbf{1}(Year \geq 2013)$ × Young	0.0345*	0.0510***	-0.0118	-0.0305**	-0.193*
	(0.0185)	(0.0176)	(0.0107)	(0.0126)	(0.1044)
Constant	0.493***	0.494***	0.470***	0.529***	3.279***
	(0.0019)	(0.0018)	(0.0009)	(0.0013)	(0.0108)
Individual FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Young × $\mathbf{1}(Year \geq 2013)$	Yes	Yes	Yes	Yes	Yes
Observations	35128	35460	55288	35679	33758

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table B.4: Impact of the regional welfare cuts on 25-35 y.o.'s political attitudes - Indices

	(1) Index - Low Political Efficacy	(2) Index - Satisfaction with Politics
Austerity × $\mathbf{1}(Year \geq 2013)$	0.0907	0.00457
	(0.1146)	(0.0908)
Constant	-0.183***	-0.106***
	(0.0001)	(0.0009)
Individual FE	Yes	Yes
District FE	Yes	Yes
Year FE	Yes	Yes
Observations	3072	2803

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table B.5: Impact of the regional welfare cuts on 35-45 y.o.'s political attitudes - Indices

	(1) Index - Low Political Efficacy	(2) Index - Satisfaction with Politics
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$	0.0749	0.0818
	(0.0669)	(0.0733)
Constant	-0.116***	-0.0131***
	(0.0005)	(0.0006)
Individual FE	Yes	Yes
District FE	Yes	Yes
Year FE	Yes	Yes
Observations	4608	4360

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table B.6: Impact of the regional welfare cuts on 45-55 y.o.'s political attitudes - Indices

	(1) Index - Low Political Efficacy	(2) Index - Satisfaction with Politics
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$	0.0464	0.0745
	(0.0828)	(0.0580)
Constant	0.00782***	0.0198***
	(0.0002)	(0.0002)
Individual FE	Yes	Yes
District FE	Yes	Yes
Year FE	Yes	Yes
Observations	4828	4689

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table B.7: Impact of the regional welfare cuts on 55-65 y.o.'s political attitudes - Indices

	(1) Index - Low Political Efficacy	(2) Index - Satisfaction with Politics
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$	-0.116	0.00636
	(0.0737)	(0.0667)
Constant	0.0549***	0.167***
	(0.0004)	(0.0005)
Individual FE	Yes	Yes
District FE	Yes	Yes
Year FE	Yes	Yes
Observations	4310	4365

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table B.8: Impact of the regional welfare cuts on 65 and over's political attitudes - Indices

	(1) Index - Low Political Efficacy	(2) Index - Satisfaction with Politics
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$	0.0993*	0.0176
	(0.0583)	(0.0534)
Constant	0.312***	0.338***
	(0.0003)	(0.0003)
Individual FE	Yes	Yes
District FE	Yes	Yes
Year FE	Yes	Yes
Observations	7216	6901

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table B.9: Impact of the regional welfare cuts on 25-35 y.o.'s political attitudes

	(1) Public officials don't care	(2) No say in what govt does	(3) Interest in politics	(4) Perso benefit from voting	(5) Perceived pol. influence
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$	0.0139	0.0436	-0.0207	-0.0105	-0.00984
	(0.0455)	(0.0437)	(0.0240)	(0.0333)	(0.2793)
Constant	0.422***	0.437***	0.436***	0.442***	3.222***
	(0.0001)	(0.0001)	(0.0013)	(0.0003)	(0.0027)
Individual FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	3086	3114	6463	3129	2930

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table B.10: Impact of the regional welfare cuts on 35-45 y.o.'s political attitudes

	(1) Public officials don't care	(2) No say in what govt does	(3) Interest in politics	(4) Perso benefit from voting	(5) Perceived pol. influence
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$	0.0658**	-0.00768	0.0120	0.0107	0.212
	(0.0281)	(0.0291)	(0.0175)	(0.0262)	(0.2692)
Constant	0.457***	0.449***	0.457***	0.474***	3.409***
	(0.0002)	(0.0002)	(0.0001)	(0.0002)	(0.0020)
Individual FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	4622	4674	9331	4724	4475

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table B.11: Impact of the regional welfare cuts on 45-55 y.o.'s political attitudes

	(1) Public officials don't care	(2) No say in what govt does	(3) Interest in politics	(4) Perso benefit from voting	(5) Perceived pol. influence
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$	0.0107	0.0259	0.0107	0.0306	0.0683
	(0.0365)	(0.0288)	(0.0143)	(0.0268)	(0.1795)
Constant	0.499***	0.496***	0.482***	0.526***	3.149***
	(0.0001)	(0.0001)	(0.0000)	(0.0001)	(0.0004)
Individual FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	4842	4876	9369	5028	4763

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table B.12: Impact of the regional welfare cuts on 55-65 y.o.'s political attitudes

	(1) Public officials don't care	(2) No say in what govt does	(3) Interest in politics	(4) Perso benefit from voting	(5) Perceived pol. influence
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$	-0.0574**	-0.0346	0.00439	0.00629	-0.201
	(0.0280)	(0.0358)	(0.0233)	(0.0308)	(0.2745)
Constant	0.523***	0.503***	0.538***	0.591***	3.223***
	(0.0002)	(0.0002)	(0.0003)	(0.0002)	(0.0019)
Individual FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	4324	4354	8832	4623	4403

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table B.13: Impact of the regional welfare cuts on 65 and over's political attitudes

	(1) Public officials don't care	(2) No say in what govt does	(3) Interest in politics	(4) Perso benefit from voting	(5) Perceived pol. influence
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$	0.0256	0.0434*	0.00934	0.0433*	-0.218
	(0.0251)	(0.0224)	(0.0168)	(0.0235)	(0.1888)
Constant	0.602***	0.603***	0.535***	0.682***	3.453***
	(0.0001)	(0.0001)	(0.0000)	(0.0001)	(0.0012)
Individual FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	7274	7370	11158	7611	7004

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table B.14: Differential impact of the regional welfare cuts on 25-35 y.o.'s political attitudes - Indices

	(1) Index - Low Political Efficacy	(2) Index - Satisfaction with Politics
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$	0.0742**	0.0190
	(0.0317)	(0.0315)
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$ $\times$ 25-35	0.00379	-0.00219
	(0.0375)	(0.0319)
Constant	0.0110**	0.0518***
	(0.0044)	(0.0037)
Individual FE	Yes	Yes
District FE	Yes	Yes
Year FE	Yes	Yes
25-35 $\times$ $\mathbf{1}(Year \geq 2013)$	Yes	Yes
Observations	34940	32666

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table B.15: Differential impact of the regional welfare cuts on 35-45 y.o.'s political attitudes - Indices

	(1) Index - Low Political Efficacy	(2) Index - Satisfaction with Politics
Austerity × $\mathbf{1}(Year \geq 2013)$	0.0765**	0.00560
	(0.0330)	(0.0327)
Austerity × $\mathbf{1}(Year \geq 2013)$ × 35-45	-0.00224	0.0461
	(0.0338)	(0.0340)
Constant	0.0114***	0.0528***
	(0.0010)	(0.0009)
Individual FE	Yes	Yes
District FE	Yes	Yes
Year FE	Yes	Yes
35-45 × $\mathbf{1}(Year \geq 2013)$	Yes	Yes
Observations	34940	32666

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table B.16: Differential impact of the regional welfare cuts on 45-55 y.o.'s political attitudes - Indices

	(1) Index - Low Political Efficacy	(2) Index - Satisfaction with Politics
Austerity × $\mathbf{1}(Year \geq 2013)$	0.0886***	0.0105
	(0.0322)	(0.0362)
Austerity × $\mathbf{1}(Year \geq 2013)$ × 45-55	-0.0378	0.0239
	(0.0407)	(0.0336)
Constant	0.0104***	0.0521***
	(0.0012)	(0.0008)
Individual FE	Yes	Yes
District FE	Yes	Yes
Year FE	Yes	Yes
45-55 × $\mathbf{1}(Year \geq 2013)$	Yes	Yes
Observations	34940	32666

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table B.17: Differential impact of the regional welfare cuts on 55-65 y.o.'s political attitudes - Indices

	(1) Index - Low Political Efficacy	(2) Index - Satisfaction with Politics
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$	0.113***	0.0139
	(0.0361)	(0.0332)
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$ $\times$ 55-65	-0.129***	0.0167
	(0.0398)	(0.0275)
Constant	0.00619***	0.0522***
	(0.0016)	(0.0010)
Individual FE	Yes	Yes
District FE	Yes	Yes
Year FE	Yes	Yes
55-65 $\times$ $\mathbf{1}(Year \geq 2013)$	Yes	Yes
Observations	34940	32666

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table B.18: Differential impact of the regional welfare cuts on 65 y.o. and over's political attitudes - Indices

	(1) Index - Low Political Efficacy	(2) Index - Satisfaction with Politics
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$	0.0651*	0.0224
	(0.0360)	(0.0339)
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$ $\times$ 65 +	0.0309	-0.0105
	(0.0372)	(0.0322)
Constant	0.0130***	0.0511***
	(0.0018)	(0.0014)
Individual FE	Yes	Yes
District FE	Yes	Yes
Year FE	Yes	Yes
65 + $\times$ $\mathbf{1}(Year \geq 2013)$	Yes	Yes
Observations	34940	32666

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table B.19: Differential impact of the regional welfare cuts on 25-35 y.o.'s political attitudes

	(1) Public officials don't care	(2) No say in what govt does	(3) Interest in politics	(4) Perso benefit from voting	(5) Perceived pol. influence
Austerity × $\mathbf{1}(Year \geq 2013)$	0.0249*	0.0281**	-0.00356	0.0130	-0.0699
	(0.0134)	(0.0121)	(0.0086)	(0.0118)	(0.1015)
Austerity × $\mathbf{1}(Year \geq 2013)$ × 25-35	-0.0134	0.0119	-0.00783	-0.00452	-0.0298
	(0.0144)	(0.0161)	(0.0085)	(0.0134)	(0.1105)
Constant	0.498***	0.498***	0.470***	0.526***	3.262***
	(0.0017)	(0.0019)	(0.0008)	(0.0016)	(0.0124)
Individual FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
25-35 × $\mathbf{1}(Year \geq 2013)$	Yes	Yes	Yes	Yes	Yes
Observations	35128	35460	55288	35679	33758

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table B.20: Differential impact of the regional welfare cuts on 35-45 y.o.'s political attitudes

	(1) Public officials don't care	(2) No say in what govt does	(3) Interest in politics	(4) Perso benefit from voting	(5) Perceived pol. influence
Austerity × $\mathbf{1}(Year \geq 2013)$	0.0141	0.0378***	-0.00546	0.0104	-0.130
	(0.0138)	(0.0130)	(0.0092)	(0.0122)	(0.0977)
Austerity × $\mathbf{1}(Year \geq 2013)$ × 35-45	0.0233*	-0.0198	-0.00258	0.00269	0.171
	(0.0141)	(0.0158)	(0.0092)	(0.0141)	(0.1189)
Constant	0.497***	0.499***	0.469***	0.526***	3.263***
	(0.0004)	(0.0004)	(0.0002)	(0.0004)	(0.0033)
Individual FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
35-45 × $\mathbf{1}(Year \geq 2013)$	Yes	Yes	Yes	Yes	Yes
Observations	35128	35460	55288	35679	33758

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table B.21: Differential impact of the regional welfare cuts on 45-55 y.o.'s political attitudes

	(1) Public officials don't care	(2) No say in what govt does	(3) Interest in politics	(4) Perso benefit from voting	(5) Perceived pol. influence
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$	0.0254**	0.0352***	-0.00900	0.00720	-0.0997
	(0.0126)	(0.0132)	(0.0094)	(0.0121)	(0.1059)
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$ $\times$ 45-55	-0.0141	-0.00886	0.00755	0.0107	0.0528
	(0.0175)	(0.0154)	(0.0078)	(0.0138)	(0.1015)
Constant	0.496***	0.499***	0.470***	0.526***	3.260***
	(0.0005)	(0.0004)	(0.0002)	(0.0004)	(0.0026)
Individual FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
45-55 $\times$ $\mathbf{1}(Year \geq 2013)$	Yes	Yes	Yes	Yes	Yes
Observations	35128	35460	55288	35679	33758

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table B.22: Differential impact of the regional welfare cuts on 55-65 y.o.'s political attitudes

	(1) Public officials don't care	(2) No say in what govt does	(3) Interest in politics	(4) Perso benefit from voting	(5) Perceived pol. influence
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$	0.0347**	0.0453***	-0.00715	0.0101	-0.102
	(0.0148)	(0.0135)	(0.0083)	(0.0116)	(0.0971)
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$ $\times$ 55-65	-0.0494***	-0.0452**	0.00313	0.00248	0.0629
	(0.0161)	(0.0175)	(0.0089)	(0.0134)	(0.1208)
Constant	0.494***	0.498***	0.470***	0.526***	3.261***
	(0.0007)	(0.0007)	(0.0003)	(0.0005)	(0.0047)
Individual FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
55-65 $\times$ $\mathbf{1}(Year \geq 2013)$	Yes	Yes	Yes	Yes	Yes
Observations	35128	35460	55288	35679	33758

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table B.23: Differential impact of the regional welfare cuts on 65 y.o. and over's political attitudes

	(1) Public officials don't care	(2) No say in what govt does	(3) Interest in politics	(4) Perso benefit from voting	(5) Perceived pol. influence
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$	0.0155	0.0297**	-0.00976	0.00680	-0.0657
	(0.0140)	(0.0146)	(0.0090)	(0.0120)	(0.1148)
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$ $\times$ 65 +	0.0146	0.00712	0.00636	0.0127	-0.0549
	(0.0147)	(0.0158)	(0.0094)	(0.0153)	(0.1272)
Constant	0.497***	0.500***	0.470***	0.526***	3.256***
	(0.0007)	(0.0008)	(0.0004)	(0.0007)	(0.0059)
Individual FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
65 + $\times$ $\mathbf{1}(Year \geq 2013)$	Yes	Yes	Yes	Yes	Yes
Observations	35128	35460	55288	35679	33758

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table B.24: Impact of the regional welfare cuts on young people's political attitudes - Excluding responses from 2013

	(1) Public officials don't care	(2) No say in what govt does	(3) Interest in politics	(4) Perso benefit from voting	(5) Perceived pol. influence
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$	0.137**	0.184***	-0.0550	-0.0992**	-0.760**
	(0.0576)	(0.0578)	(0.0368)	(0.0497)	(0.3543)
Constant	0.402***	0.424***	0.330***	0.317***	2.892***
	(0.0010)	(0.0010)	(0.0020)	(0.0014)	(0.0091)
Individual FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	1970	1988	3815	1375	1631

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table B.25: Impact of the regional welfare cuts on young people's political attitudes - Excluding responses from 2016

	(1) Public officials don't care	(2) No say in what govt does	(3) Interest in politics	(4) Perso benefit from voting	(5) Perceived pol. influence
Austerity × $\mathbf{1}(Year \geq 2013)$	0.126**	0.181***	-0.0593	-0.0830*	-0.753**
	(0.0575)	(0.0578)	(0.0366)	(0.0502)	(0.3564)
Constant	0.411***	0.435***	0.337***	0.319***	2.899***
	(0.0010)	(0.0009)	(0.0020)	(0.0014)	(0.0088)
Individual FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	1916	1932	3726	1339	1583

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table B.26: Impact of the regional welfare cuts on young people's political attitudes - Sample of non-movers only

	(1) Public officials don't care	(2) No say in what govt does	(3) Interest in politics	(4) Perso benefit from voting	(5) Perceived pol. influence
Austerity × $\mathbf{1}(Year \geq 2013)$	0.131**	0.184***	-0.0507	-0.0809	-0.739**
	(0.0574)	(0.0591)	(0.0379)	(0.0515)	(0.3465)
Constant	0.426***	0.458***	0.309***	0.294***	2.819***
	(0.0010)	(0.0010)	(0.0024)	(0.0014)	(0.0086)
Individual FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	1726	1744	3222	1194	1423

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table B.27: Impact of the regional welfare cuts on young people's political attitudes - Discarding largest values of austerity shock

	(1) Public officials don't care	(2) No say in what govt does	(3) Interest in politics	(4) Perso benefit from voting	(5) Perceived pol. influence
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$	0.116*	0.185***	-0.0345	-0.0726	-0.783**
	(0.0606)	(0.0613)	(0.0348)	(0.0533)	(0.3839)
Constant	0.409***	0.430***	0.336***	0.315***	2.883***
	(0.0011)	(0.0011)	(0.0020)	(0.0016)	(0.0102)
Individual FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	2014	2032	3937	1402	1656

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Table B.28: Impact of the regional welfare cuts on young people's political attitudes - Discarding smallest values of austerity shock

	(1) Public officials don't care	(2) No say in what govt does	(3) Interest in politics	(4) Perso benefit from voting	(5) Perceived pol. influence
Austerity $\times$ $\mathbf{1}(Year \geq 2013)$	0.132*	0.158**	-0.0823**	-0.119*	-0.719*
	(0.0710)	(0.0650)	(0.0405)	(0.0609)	(0.4149)
Constant	0.422***	0.441***	0.331***	0.307***	2.873***
	(0.0009)	(0.0008)	(0.0025)	(0.0016)	(0.0102)
Individual FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	1784	1796	3789	1258	1474

Standard errors in parentheses and clustered at the district and individual level. Coefficients standardized by one s.d. of the austerity shock. * p<0.1, ** p<0.05, *** p<0.01

Chapter 3

Fear of COVID and non-pharmaceutical interventions: An analysis of their economic impact among 29 advanced OECD countries

Chapter co-authored with Laurence Boone

3.1 Introduction

The COVID epidemic saw the implementation of lockdowns to limit the spread of the disease. At the time, a debate was taking place in the academic and public policy circles on whether citizens reduced their mobility because of the governmental restrictions or out of fear of infection. This chapter adds to this literature, focusing on two COVID waves between February 15, 2020 and January 15, 2021. It aims to estimate the effects of non-pharmaceutical interventions (NPIs) and the COVID sanitary situation on the economy of advanced OECD countries, using Google mobility data as a proxy for economic activity. Our estimates attempt to shed light on four questions:

1. What is the average effect of NPIs, i.e. mobility restrictions, and individual perceptions of the sanitary situation on mobility?
2. Are the elasticities of economic activity to mobility restrictions different between the first and the second wave?

This paper was written as part of the French national independent Mission on the assessment of the Covid-19 crisis management and on the anticipation of pandemic risks. It was published as Covid Economics Paper (issue 73, March 2021). We are grateful to Antoine Armand, Philippe Burnel, Balazs Égert, Jean Pisani-Ferry, Yvan Guillemette, Phillipe Martin, Romain Martischang, Pierre Parneix, Alexia Pastré, Didier Pittet, and Dave Turner for interesting discussions and valuable comments.

3. Are these elasticities different between countries, in particular between France, Germany, Italy, Spain, the United Kingdom, and Switzerland?

4. What are the relative effects of different types of NPIs?

We find that the effect is more visible during the first wave than during the second as the influence of the sanitary situation gained weight during the second wave. This is probably due to (i) a second wave stronger than the first in many countries, (ii) while NPIs were, on average, less stringent during the second wave, leaving more room for voluntary social distancing.

An analysis by country shows that the most affected countries during the first wave display higher elasticities to mobility restrictions, except for Italy where restrictions and the sanitary situation had similar impacts on mobility. Looking at the relative effects of different types of NPIs we see that more stringent measures had more impact on mobility. Nevertheless, we remain cautious regarding these last estimates as the rapid implementation in the sequence of NPIs likely implies issues of statistical identification.

Our work echoes many publications on COVID and we can only mention a few here. Overall, studies have shown that both mobility restrictions and fears of infections lead people to reduce their activity. Thus Demirgüç-Kunt et al. (2020) show that stricter lockdowns are associated with more important falls in economic activity. Looking at the US, Gupta et al. (2020) find that individuals primarily adjusted their mobility voluntarily as the sanitary situation deteriorated, but the introduction of restrictions at the local level further reduced it. Égert et al. (2020) and the IMF (2020) also use mobility as a proxy for economic activity and show that containment measures, as well as a degraded sanitary situation, are associated with significant economic costs. However, our results differ slightly from the IMF's which finds, on a comparable sample of advanced countries, that NPIs and the sanitary situation had an equivalent influence on the decline in mobility during the first wave[1]. Our results suggest that NPIs played a more important role in the decrease in economic activity than voluntary distancing. The difference comes from a different treatment of daily deaths, the variable used to estimate the elasticity of voluntary social distancing. The IMF (2020) uses the logarithm of the daily death toll as an independent variable while our analysis uses the daily death toll per 1 million, as in Égert et al. (2020). Using the logarithm assumes that people are responding to changes in the death toll and implies that people understand the exponential dynamics of the epidemic. Instead, we assume that people react to daily announcements. There is no evidence that one hypothesis is superior to the other and we should note that this variable also captures other effects linked to the perception of crisis management.

[1]Their paper also highlights the differences in effect between advanced countries and emerging and developing countries with regard to containments and closures of workplaces. However, their analysis does not make a distinction according to the level of intensity of stay-at-home requirements and workplace closures in advanced countries.

In parallel, country-level studies have found that mobility or consumption reductions are mainly caused by fears of infections and voluntary social distancing. Thus Andersen et al. (2020) use data from bank card transactions to show that the decline in consumption in Sweden was almost equivalent to that in Denmark despite much lower restrictions on movement. Goolsbee and Syverson (2021) find that individuals in the US reduced their movements voluntarily rather than as a reaction to the implementation of lockdowns. These results remain compatible with ours. They suggest that in the absence of restrictions people adjust their behaviour, and underline the existence of cultural differences between countries, which have also been highlighted by other studies (Bargain and Aminjonov, 2020; Becher et al., 2021). Indeed, our estimates apply to the average of advanced OECD countries, behind which some country heterogeneity exists and on which we shed some light. Besides, our estimates for the second wave show that the sanitary situation plays a greater role. This suggests that when NPIs are less stringent the impact of the sanitary situation on mobility gains weigh. Another interpretation could be that there is a learning curve according to which behaviours adapt in face of the virus. Overall, our results are in line with the idea, advanced for example by Chetty et al. (2020), that addressing the health concerns themselves is a pre-requisite for the resumption of mobility, and therefore for an economic recovery.

Our paper is structured as follows: in section 3.2 we describe the data used and the methodological framework, section 3.3 presents the average elasticity associated with NPIs and voluntary social distancing in advanced OECD economies, section 3.4 compares estimates for six European countries, and section 3.5 evaluates which NPIs appear to be more effective at reducing mobility.

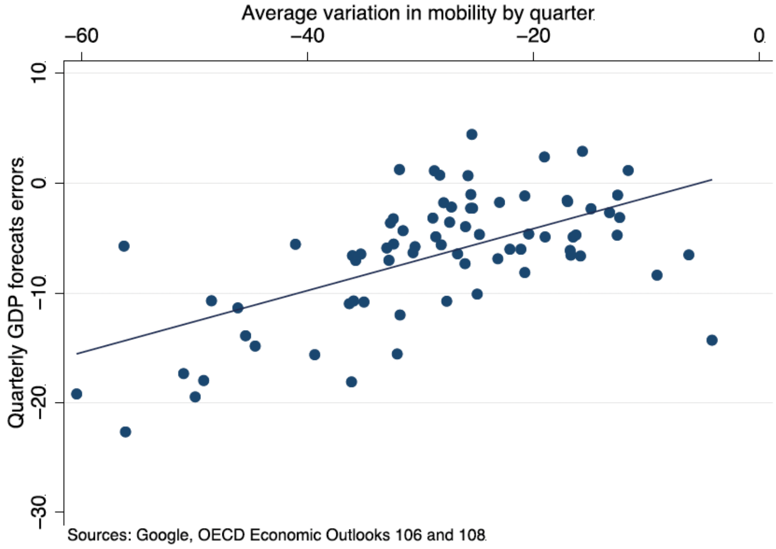
3.2 Data and methodological framework

Our analysis relies on variables that are measured at a daily frequency, and available for a sample of 29 advanced OECD countries[2], to estimate the impact of both government restrictions and the health situation:

- Mobility, published by Google, is used as a proxy for economic activity. This measure has the advantage of being available at high frequency (daily), very quickly (the data is published a few days after being recorded by Google), and for a large number of countries. Its strong correlation with economic activity makes it a credible instrument for estimating economic activity. This measure has been widely used since the start of the crisis by national statistical institutes (INSEE, ONS), inter-

[2]The 29 countries are Australia, Austria, Belgium, Canada, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Ireland, Israel, Italy, Japan, Latvia, Lithuania, Luxembourg, New Zealand, Norway, Netherlands, Portugal, Slovakia, Slovenia, Spain, Sweden, Switzerland, the United Kingdom, and the United States.

Figure 3.1: Correlation between the changes in mobility measured by Google and the GDP forecast errors among the 29 advanced OECD countries selected



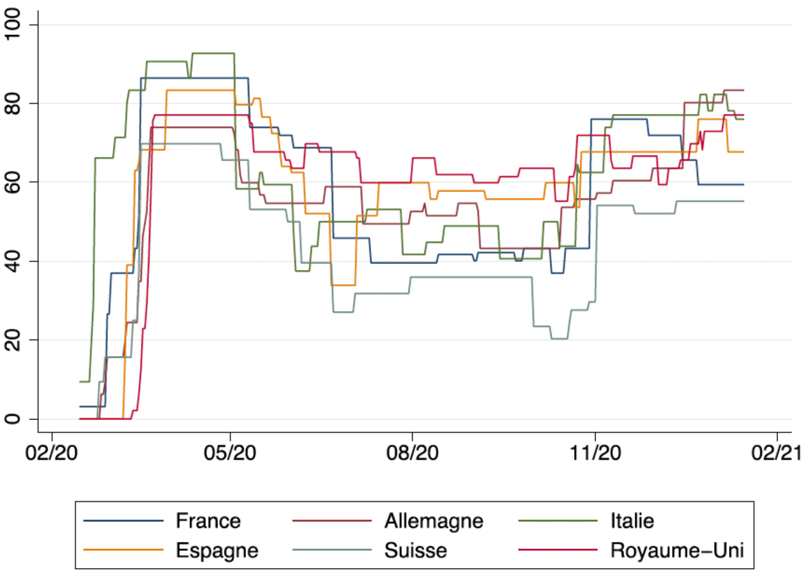
Note: Estimates for the first three quarters of 2020. The average variation in mobility during the quarter is calculated compared to the period from January 3 to February 6, 2020. Mobility is the arithmetic mean of mobility in the workplace, in retail and leisure businesses, in transit stations

national organisations (Chen et al., 2020; Égert et al., 2020; IMF, 2020; Maloney and Taskin, 2020), and in academic studies (Barbieri and Bonini, 2020; Bargain and Aminjonov, 2020; Chernozhukov et al., 2021). Google provides mobility data on travels to specific places, including workplaces, retail and recreational places, transit stations, groceries and pharmacies, parks, and residences. The data they publish is the change in mobility in percentage between one day and its baseline value. This baseline is the median value of the same day of the week over the 3rd January to 6th February 2020 period, at which time the pandemic was not proven elsewhere than in China. There are limitations to this measure. In particular, this variable measures the mobility of Google Maps users who have authorised Google to track their movements. It is therefore likely that parts of the population are not represented in these data.

- The Oxford Stringency Index is an aggregate measure of the level of stringency of the NPIs implemented in a country. It is published by the University of Oxford and available at a daily frequency for a large number of countries. This index is calculated according to the following steps.

  First, the type of NPI implemented is classified into one of eight categories defined by the University of Oxford: school closure, workplace closure, cancellation of public events, restrictions on gatherings, closure of public transport, stay-at-home requirements, restrictions on internal movement, and international travel controls. Once categorised, a score ranging from 1 to 3 (or 1 to 4) is assigned to the NPI depending

Figure 3.2: Trends in the Oxford stringency index in France, Germany, Italy, Spain, Switzerland, and the United Kingdom from Feb 15, 2020, to Jan 15, 2021



on the severity of the measure. Table C.1 synthesises the different scores and their meanings for each type of NPI. Second, a sub-index from 0 to 100 is calculated for each category of NPI. Thus, if a school closing of level 2, out of 3, has been implemented in a country, a sub-index of 66 is assigned to the corresponding days (2/3*100). In the case of a regional or sectoral measure, as opposed to a national measure, a scalar of 0.5 is subtracted from the score. The overall stringency index is then calculated as the arithmetic mean of the eight sub-indices[3], [4].

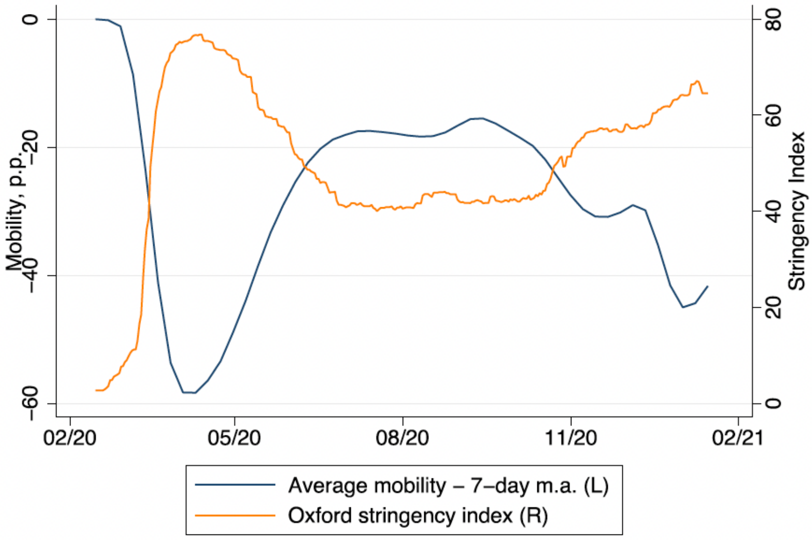
This index makes it possible to synthesise a lot of information simply: an index that is close to 100 means that many high-intensity NPIs have been implemented, and therefore that mobility restrictions are significant. Conversely, an index equal to 0 means that there are no restrictions. Figure 3.2 shows the evolution of the average index over time in France, Germany, Italy, Spain, Switzerland, and the United Kingdom. We can see that NPIs increased since mid-February 2020 in these 6 countries, although unevenly, and started to be lifted in May. New restrictions were then put in place from November 2020.

Like any synthetic index, certain country differences in the implementation of NPIs are not taken into account. Thus, a scoring scale from 1 to 3, or 1 to 4, does

[3]The "official" Oxford stringency index is the average of these 8 indices of containment measures plus one index that synthesises the implementation of a public health campaign on COVID. As in IMF (2020), we focus on the 8 indices of containment measures and exclude the index of public health campaigns. Indeed, we include a second independent variable on daily COVID deaths which should capture the reaction to the sanitary situation. Excluding the public health campaign index is therefore necessary to avoid collinearity. Nevertheless, as a robustness check we also run our analysis using the "official" Oxford stringency index and our results remain unchanged.

[4]For more information see Hale et al. (2020))

Figure 3.3: Average change in the Oxford stringency index and mobility in advanced OECD countries studied from Feb 15, 2020, to Jan 15, 2021

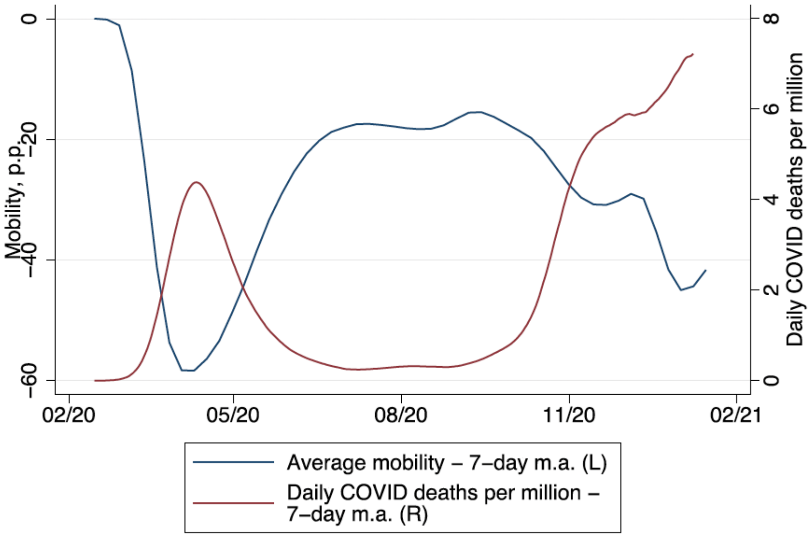


not capture certain specificities that differentiate two NPIs of the same score in two different countries. Despite these drawbacks, the correlation between mobility and the aggregate stringency index is apparent and shows that individuals strongly decrease their mobility as a result of the adoption of NPIs (see Figure 3.3).

- Finally, the daily number of confirmed COVID deaths, as a share of one million inhabitants, is used to control for the sanitary situation of a country (Figure 3.4). We use data from the University of Johns Hopkins and interpret this variable's coefficient as a measure of the fear or awareness of the sanitary situation in the country and its ensuing voluntary social distancing. It is important to note that the reality of the sanitary situation and the death toll are better approximated by the excess mortality over the period. Nevertheless, it can only be calculated a posteriori and does not reflect the state of knowledge at the time. Therefore, daily COVID deaths seem a better way to measure individuals' reactions to the reality presented to them at time t.

From these data series, OLS panel regressions are estimated on our sample of 29 advanced OECD countries on a period that spans from February 15, 2020, to January 15, 2021. We then divide the data into two sub-periods, to compare the first wave (whose period is defined here as February 15, 2020, to June 30, 2020) to the second wave (September 1, 2020, to January 15, 2021). Using daily frequency data enables us to have approximately 9,700 observations for the entire period. We choose to focus on advanced OECD countries to have a sample of countries homogenous in their characteristics that are not linked to the pandemic.

Figure 3.4: Average daily COVID deaths and mobility in advanced OECD countries from Feb 15, 2020, to Jan 15, 2021



3.3 Average impact of NPIs and daily COVID deaths on mobility in advanced OECD countries

In this section, we present estimates of the effects of containment measures and voluntary social distancing on mobility across our sample of 29 advanced OECD countries.

3.3.1 Specification

A first analysis consists of estimating the elasticity of mobility to the adoption of NPIs, and daily COVID deaths using the following panel equation:

Our main equation is the following:

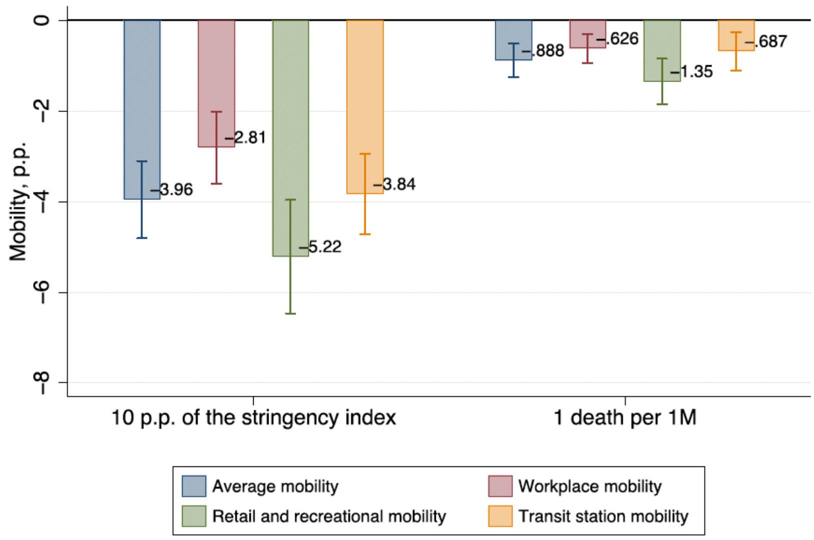
$$Mobility_{i,t} = c + \alpha_i + \tau_t + \beta \times Stringency_{i,t} + \gamma \times Deaths_{i,t-1} + \epsilon_i, t \tag{3.1}$$

where $Mobility_{i,t}$ is the change in mobility as measured by Google in country i at time t; $Stringency_{i,t}$ is the average stringency level of the containment measures in country i at time t; $Deaths_{i,t-1}$ is the seven-day rolling average of the daily number of confirmed COVID deaths per million people in country i at time t-1; α_i and θ_t are country and time fixed effects; c is a constant; standard errors are aggregated at country and weekly level.

We estimate two coefficients of interest from Equation 1:

1. β, which is the elasticity of mobility to the adoption of NPIs. It measures how much individuals adjust their mobility following the implementation of containment measures.

Figure 3.5: Impact of an increase of 10 p.p. of the stringency index and 1 death per 1M on mobility among advanced OECD countries



Note: 95% confidence interval. Detailed regression results available in the appendix, Table C.2.

2. γ, which corresponds to the elasticity of mobility to daily deaths and which we interpret as the voluntary adjustment of individual mobility to the sanitary situation of the country and the level of fear associated with it.

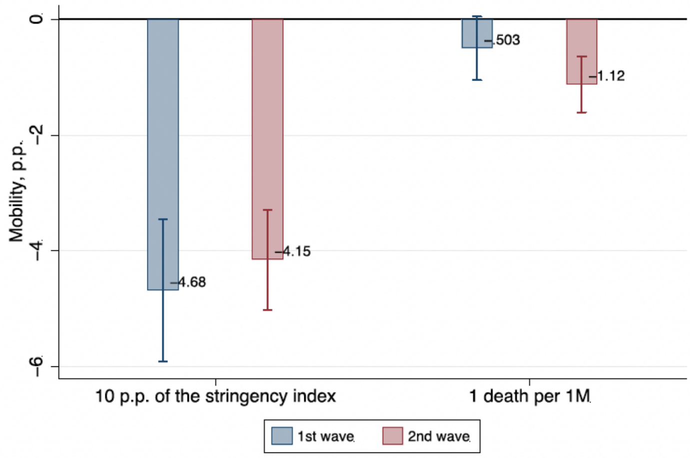
We use country-fixed effects to absorb the effect of certain country specificities on mobility, which could be institutional or cultural. Similarly, time fixed effects enable us to capture common effects across countries for specific dates. For example, they control for specific variations in mobility on Christmas Day or the evolution of the global sanitary situation. Finally, we aggregate our standard errors at the country and week level to take into account correlation effects within a country and/or during the same week.

3.3.2 Results

Figure 3.5 displays our results for Equation 3.1 using the whole sample, that is on the period that spans from February 15, 2020, to January 15, 2021. We look at four estimates of mobility across our 29 countries: mobility in workplaces, in retail and recreational places, in transit stations, and an average of the previous three types of mobility.

The results are consistent with the general intuition, in the context of an overall decrease in mobility. Thus, we observe that a 10 percentage point (p.p.) increase in the stringency index is associated with a decrease in average mobility of 4 p.p. In parallel an increase in daily COVID deaths of one for a million is associated with a decrease in mobility close to 0.9 p.p. These effects are quite large when compared to the variables' standard deviations over the period – February 15, 2020, to January 15, 2021 – which are 24% for the stringency index and 4 for daily COVID deaths per million. It is also

Figure 3.6: Evolution of the elasticities of the stringency index and daily COVID deaths between the first and second waves for advanced OECD countries



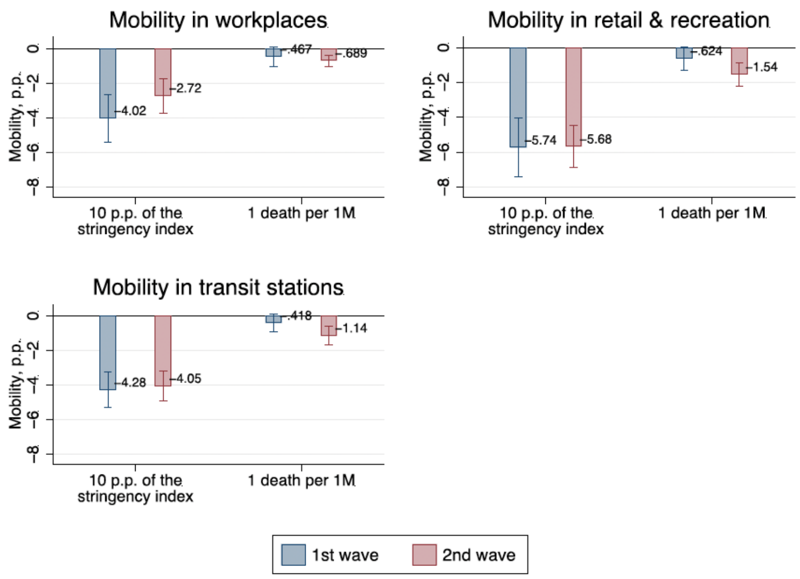
Note: 95% confidence interval. Detailed regression results available in the appendix, Tables C.3 and C.4.

interesting to see that the decline in mobility is greatest in retail and recreational businesses, followed by mobility in transport and mobility in the workplace. It shows that non-essential travels have decreased the most.

Equation 3.1 is then re-estimated over two different periods: the first wave period which spans from February 15, 2020, to June 30, 2020, and the second wave period, from September 1, 2020, to January 15, 2021. Figure 3.6 shows that the elasticity of NPIs does not change much between the first and the second wave and remains stable between -4.65 and -4.18 for a 10 p.p. increase in the stringency index. Conversely the elasticity of daily COVID deaths increases (in absolute value): while COVID deaths do not have a significant effect on mobility in the first wave, the coefficient becomes significant in the second wave, with an elasticity of -1.12. This suggests greater awareness of the pandemic during the second wave and means that the effect of deaths represented in Figure 3.5 is driven by data from the second wave. A breakdown by type of mobility shows that this difference is particularly visible for mobility in retail and recreational businesses and transport (see Figure 3.7), in line with our previous interpretation. Besides, we see that while the elasticities of NPIs in retail and transport do not change much from the first to the second wave, this is not the case for mobility at work. Indeed, the decline in mobility at work is less significant during the second wave, which is consistent with the idea that containment measures were less severe on workers during the second wave [5].

[5]Note that this analysis ends on January 15, 2021, date at which the second wave is not finished. Workplace restrictions may become more severe in the following weeks or months.

Figure 3.7: Evolution of the elasticities of the stringency index and daily COVID deaths between the first and second waves for advanced OECD countries, by type of mobility



Note: 95% confidence interval. Detailed regression results available in the appendix, Tables C.3 and C.4.

3.4 Differential effects according to the country

We now turn to an analysis by country and compare the evolution of mobility in six European countries: France, Germany, Italy, Spain, Switzerland, and the United Kingdom. We show that countries react differently to the sanitary situation and the implementation of mobility restrictions. Figures 3.8 and 3.9 display the evolution of mobility, stringency index, and COVID deaths over time for each of these countries. We see that during the first wave mobility fell in all countries and did not return to its pre-crisis level afterwards. In particular, the more severe the restrictions, the more important the decline in mobility was. Those were also accompanied by important mortality peaks (France, Italy, Spain, and the United Kingdom). Germany and Switzerland are characterized by drops in mobility that are smaller than in other countries. This is consistent with lower death numbers and a lower stringency index as these two countries have been less affected by the first wave.

During the summer of 2020, mobility rebounded in all countries as NPIs were lifted. However, the United Kingdom stood out from other countries as mobility picked up more slowly and stabilised at a lower level than the other countries. This is due to a more limited lifting of NPIs and could also be linked to a less precise estimation of the Oxford stringency index there, as England, Wales, Scotland, and Northern Ireland had different policies in place[6].

[6]Overall, restrictive measures taken at the regional level increased between Wave 1 and Wave 2 but remain proportionately low. Indeed, they represent 4% of our sample during the first wave against 6.3% during the second. However, in the United Kingdom the proportion of regional measures increases from

Dynamics during the second wave were less homogeneous among countries. Spain faced a relatively linear rise in COVID deaths during the summer, accompanied by an increase in NPIs from July which did not result in any visible change in mobility. France and the United Kingdom experienced a significant increase in the number of daily COVID deaths in September. This increase quickly became exponential and was followed, in France, by the implementation of additional NPIs. In parallel, the United Kingdom adopted additional NPIs that were less strict than in France, as they were already at a high level of restrictions. Consequently, mobility in the United Kingdom decreased less than in France, having also stabilised at a relatively low level during the summer. However, after stabilising in November-December, the death toll increased exponentially in the United Kingdom from the end of December, probably because of the emergence of the new variant. It was followed by a sharp increase in restrictive measures and a marked drop in mobility. Italy followed a development similar to France's, with a lag, as its second wave started at the end of October. Its second wave nevertheless appears to be as deadly as the first, although under-testing was probably an issue during the first wave. Switzerland, relatively spared by the first wave, saw a sudden and brutal increase in its daily death toll. However, mobility did not react much to it, reflecting fewer restrictions. It then decreased with some delay compared to the observed sanitary situation. The same delay was observed for the decrease in the number of deaths and mobility in January. Finally, Germany was hit by a second wave of COVID relatively late in 2020 compared to the other countries. However, the increase in the daily death toll became significant quite rapidly. As of January 15, 2021, the peak of daily deaths did not appear to have been reached. Greater mobility restrictions than in the first wave were put in place. Mobility fell in parallel to these restrictions and the abrupt deterioration of the health situation.

3.4.1 Specification

To quantify these country differences, we estimate the same equation as in part I and add two elements to capture each country's differential impact:

$$\begin{aligned} Mobility_{i,t} = c + \alpha_i + \tau_t + \beta_1 \times Stringency_{i,t} + \gamma_1 \times Deaths_{i,t-1} + \beta_2 \times CountryDummy_i \\ \times Stringency_{i,t} + \gamma_2 \times CountryDummy_i \times Deaths_{i,t-1} + \epsilon_i, t \end{aligned} \tag{3.2}$$

Where $CountryDummy_i = 1$ if country i is France, Germany, Italy, Spain, Switzerland, or the United Kingdom. Equation 3.2 is therefore estimated six times, and the coefficients of each estimation are compared to each other.The coefficients α_i and τ_t make it possible to estimate the differential impact of, respectively, the increase in NPIs and the number of daily COVID deaths in country i compared to the average of advanced OECD

5.7% to 11.6%. The Oxford team notably dedicated a specific research paper to this question, with England, Scotland, Wales and Northern Ireland having more diverged in their management of the health situation during the second vague (see Cameron-Blake et al. (2020)).

Figure 3.8: Evolution of mobility and the stringency index among six selected European countries

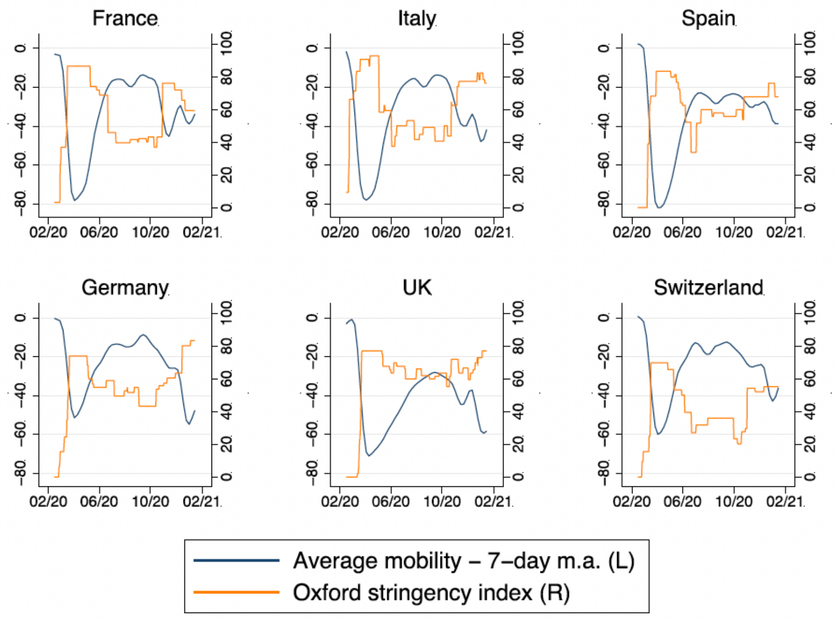
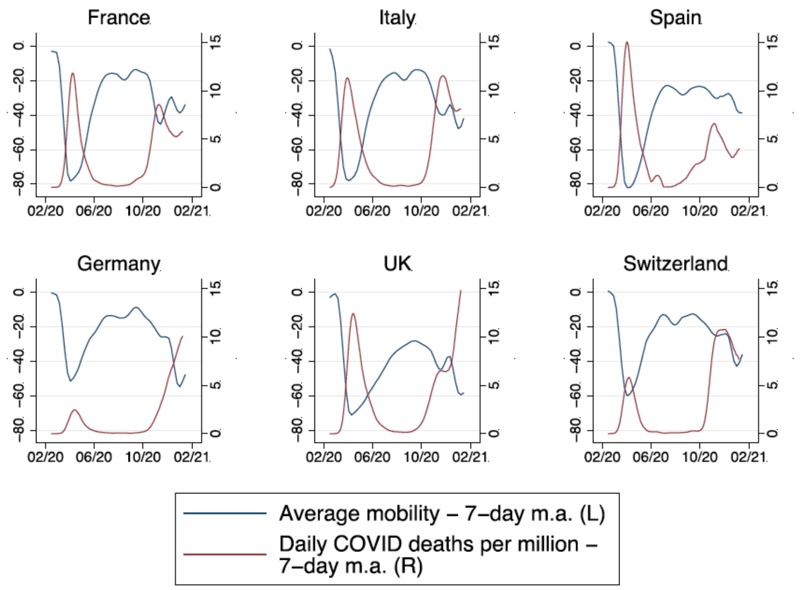


Figure 3.9: Evolution of mobility and the daily COVID deaths per million among six selected European countries



countries. The sum of the coefficients β_1 and β_2 can be seen as an estimate of the average impact of an increase in the stringency index in the country for which $CountryDummy_i$= 1, and the sum of the coefficients γ_1 and γ_2 as that of the average impact of an increase in the daily death toll in the same country.One should note that as we use different country dummies for each estimation, therefore the coefficients β_1 and γ_1 do not represent the same average of countries each time. In other words, when $CountryDummy_i$= 1 and country i is France, the coefficients β_1 and γ_1 are averages for our sample of OECD countries minus France. This implies that our coefficients β_2 and γ_2 must be interpreted as the differential effects in country i compared to the advanced OECD countries minus country i. Nevertheless, the relative stability of the coefficients β_1 and γ_1 over the six regressions (see Tables C.5 and C.6) makes us believe that the approximation proposed in the previous paragraph is acceptable[7].

3.4.2 Results

First Wave

Figure 3.10 shows the effects associated with an increase of 10 p.p. in the stringency index and of 1 death per million for each country during the first wave. We can see that France, Spain, and the United Kingdom reacted in a similarly strong fashion to the implementation of NPIs. Conversely, Germany and Italy have coefficients that are below the average of advanced OECD countries. Finally, Switzerland is in line with the average.

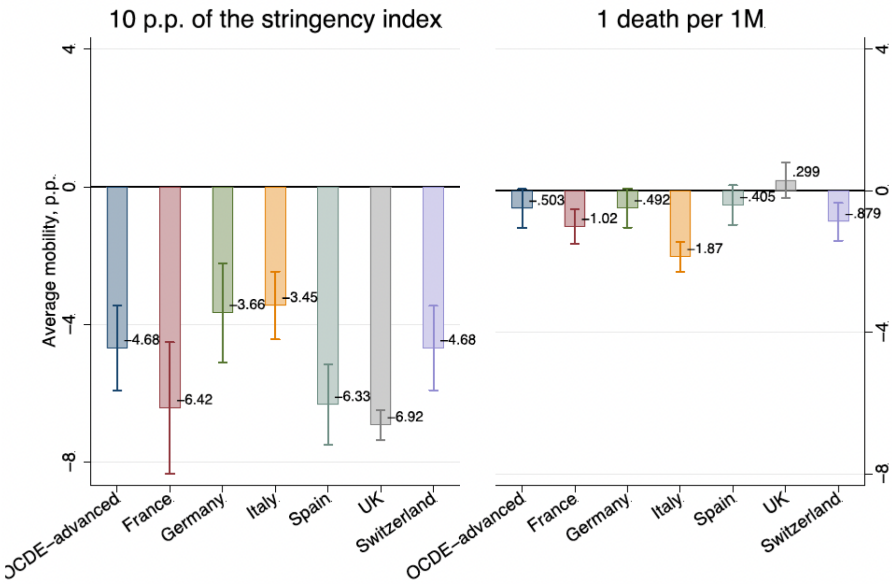
The elasticity of NPIs differs from country to country and tends to be greater in the countries most affected by the first wave of COVID. Switzerland and France display larger elasticities to daily deaths than the average, but less so than Italy. Germany, Spain, and the United Kingdom have non-significant elasticities. In these three countries, mobility appears to be affected only through NPIs.

The case of Italy is particularly interesting. While its elasticity to NPIs is lower than the average for OECD countries, the elasticity to daily deaths is particularly high: it is more than three times the average elasticity of advanced OECD countries (although that the latter is only significant at 10%). This is probably due to Italy being the first European country affected by the pandemic. Several weeks separated the identification of the first epidemic outbreaks and the adoption of a national lockdown, and those weeks were marked by a strong sense of panic. Consequently, mobility had already fallen before the most drastic NPIs were adopted (see Figure C.1).

This suggests that isolating the effects of voluntary distancing from government interventions might be difficult as they tend to occur at the same time. It is, therefore,

[7]We also perform a robustness check where we the interactions of the 6 countries are included at the same time in our equation. The country estimates in Figures 10 and 11 remain unchanged, but our aggregate for advanced OECD countries minus these 6 countries is no longer significant. This implies that the inclusion of these 6 countries (together) for the analysis of the average of advanced OECD countries is important.

Figure 3.10: Effect of the NPIs and daily COVID deaths on mobility, by country, during the first wave



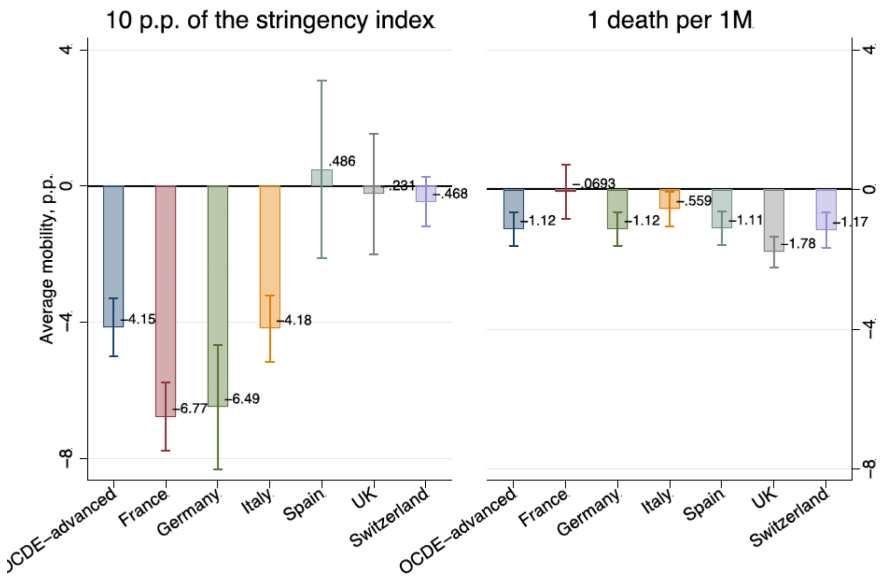
necessary to interpret our results as correlations and associations rather than causal relations.

Second Wave

Figure 3.11 presents the effects associated with a 10 p.p. increase in the Oxford stringency index and an increase of 1 death per million for each country during the second wave. France and Germany are now the two countries with the highest elasticities to NPIs. The elasticity of France is similar to that observed in the first wave, a sign that people have reacted similarly to government-imposed restrictions. Conversely, Germany now has a higher coefficient, which means that individuals adjusted more their mobility to the restrictions than in the first wave, consistent with a more important wave than in the first episode. Italy falls within the average of advanced OECD countries while Spain, the United Kingdom, and Switzerland have non-significant coefficients. Spain and the United Kingdom's results may be due, in part, to more numerous and different regional restrictions, whose measurement by the stringency index is less precise. The results in Switzerland seem to reflect the subdued reaction of mobility following the implementation of restrictive measures during the second wave. It also suggests that fears of infections played a bigger role and that mobility reacted more to death figures (see Figure 3.8).

The elasticities to daily deaths are, overall, greater than during the first wave. France and Italy stand out with an elasticity that is not significant and lower than the average of advanced OECD countries. These results are particularly striking as these two countries were the only ones with greater than average elasticities in the first wave. They can suggest two things. First, that these two countries implemented restrictions earlier than in the first wave, before a rapid rise in daily deaths. Thus, the majority of the effects of

Figure 3.11: Effect of the NPIs and daily COVID deaths on mobility, by country, during the second wave



the decrease in mobility are captured by the coefficient on the stringency index. Second, it can also reflect that individuals have grown used to the health situation, and do not display the same sense of panic as during the first wave.

Germany, Spain, and Switzerland have elasticities on daily deaths that are in line with the average of advanced OECD countries. Yet, their elasticities are greater than during the first wave, a sign that individuals adjusted more their movements according to the sanitary situation. This is consistent with our descriptive evidence for Germany and Switzerland where the second wave was much stronger. Finally, the United Kingdom shows an elasticity to daily deaths that is above average and larger than during the first wave. This could reflect a particularly deteriorated sanitary situation during the second wave. Overall, these results suggest that the sanitary situation had more of an effect on mobility in the second wave (in 4 of the 6 countries examined here) while at the same time, the reaction to NPIs was less homogeneous among countries.

To better compare the associated effect of NPIs on mobility with that of daily deaths, we x-standardise the coefficients estimated above. In other words, each coefficient is multiplied by the standard deviation of its variable for country i. These standardised coefficients allow to visualise the weight of each independent variable on mobility during the first wave (Figure 3.12) and the second (Figure 3.13) for each country.

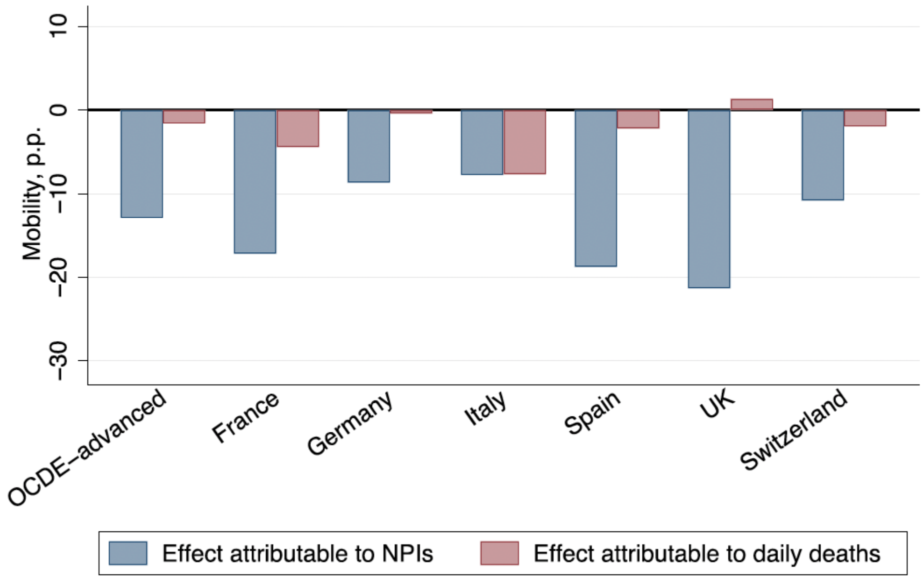
Three observations can be made from Figures 3.12 and 3.13. First, the standardised coefficients of the stringency index are larger in the first wave than the second for all countries. This reflects a second wave of NPIs that is less strict than the first. Thus, if the elasticities associated with the stringency index for the average of advanced OECD countries are very close between the first and the second wave (with respective values of

- 4.68 and - 4.15, see Figure 6), the standardised coefficients are different. It is -12 p.p. in the first wave and -8 p.p. in the second on average for OECD advanced economies. The standard deviation of the stringency index is lower during the second wave, which is consistent with the fact that most restrictions remained during the first and second waves. Meanwhile, the standardised coefficients of daily deaths gained more explanatory weight on the mobility decrease in the second wave. When looking at the average of advanced OECD countries, daily deaths correspond to 16% of the effect of NPIs during the first wave and nearly 75% during the second wave. This is due to both a higher elasticity and a higher standard deviation of daily deaths in the second wave on aggregate (see Figures 3.10 and 3.11 for the evolution of the elasticity). However, results by countries detailed below show some heterogeneity across countries.

Second, in the first wave, the greater effect of restrictions on mobility relative to daily deaths is visible in each country selected. Only Italy stands out from the other countries: its standardised coefficients for NPIs and daily deaths are equal (see below). This suggests that the sanitary situation weighed as heavily as the mobility restrictions on Italy's decrease in mobility during the first wave. Conversely, in the United Kingdom, Spain, and Germany the daily number of deaths had almost no effect on mobility in the first wave (the positive effect in the case of the United Kingdom is not significant), as suggested by their non-significant elasticities. Switzerland, whose elasticity is significantly different from zero, nevertheless has a very low standardised coefficient. This indicates that daily variations in deaths were low during the first wave in Switzerland. In France, even if the sanitary situation did have a significant effect on mobility, NPIs have about 5 times more impact on mobility. France's sharp decrease in mobility during the first wave is therefore mainly linked to the implementation of restrictions on movements.

Finally, in the second wave, all the countries studied display lower standardised coefficients on NPIs than their first wave's, and in the case of Germany an equal one. This reflects, as highlighted above, mobility restrictions that are, on average, less stringent during the second wave. Nonetheless, as this sample end on January 15, 2021, it is possible that Germany and the United Kingdom reach higher levels of restrictions in the rest of their second wave. In a context of uncertainty around new COVID variants, restrictions in the second wave might exceed that of the first for some countries in the future. At the same time, countries that suffered from a second wave larger than the first - Germany, the United Kingdom, and Switzerland - saw an increase in their standardised coefficient of the number of daily deaths. Spain has standardised coefficients close to zero, which reflect its limited variations in mobility during the second wave, as well as a second wave peak well below the first one. In Italy, the effect of daily deaths is less important during the second wave. As explained above, the climate of anxiety that was present during the first wave might have eased. A similar interpretation can be made to explain the lack of effect of daily deaths on mobility in France, which comes from a non-significant elasticity.

Figure 3.12: Standardised coefficients of NPIs and COVID deaths on mobility, by country, during the first wave



Thus, people might have reacted less to the deterioration of the sanitary situation in these countries during the second wave. This is perhaps due to the communication around these deaths that became less vocal or to increased confidence in the effectiveness of individual protective measures, such as masks or hand washing.

3.5 Effects of different NPI categories on mobility

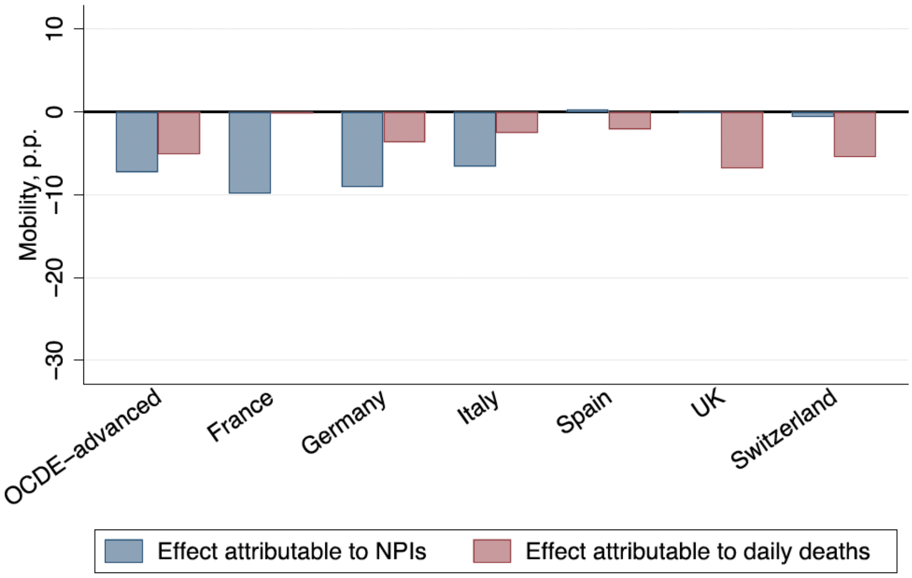
3.5.1 Specification

The differences in elasticities according to the country suggest that the composition of NPIs behind the increase in the stringency index may matter. Hence, the type of NPI, as well as the degree of severity of the measures, could have different effects. To explore these alternatives, we estimate two other equations. A first equation (Equation 3.3) breaks down the Oxford stringency index in its eight sub-indices (see Table C.1). This makes it possible to compare, for example, the impact of the closure of schools on mobility to the implementation of restrictions on gatherings.

$$Mobility_{i,t} = c + \alpha_i + \tau_t + \sum_{p=1}^{8} \beta_p \times SubIndex_{p,i,t} + \gamma \times Deaths_{i,t-1} + \epsilon_i, t \qquad (3.3)$$

Secondly, we introduce the dummies that the sub-indices are built from directly in the equation (Equation 3.4). Unlike previously, Equation 3.4 gives the same weight to each intervention, regardlessof its level of intensity. This allows us to evaluate the role of each

Figure 3.13: Standardised coefficients of NPIs and COVID deaths on mobility, by country, during the second wave



NPI's intensity and see if the introduction of a level 1 NPI is associated with the same elasticity as a level 2 or level 3 NPI.

$$Mobility_{i,t} = c + \alpha_i + \tau_t + \sum_{p=1}^{23} \beta_p \times NPIdummy_{p,i,t} + \gamma \times Deaths_{i,t-1} + \epsilon_i, t \qquad (3.4)$$

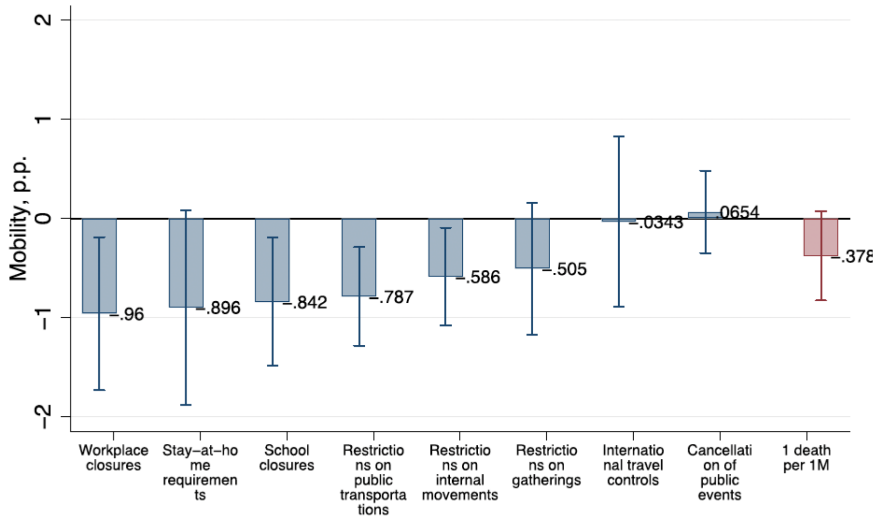
Thus, Equation 3.3 can identify the kind of NPI that tends to reduce mobility while Equation 3.4 identifies which levels of NPI are associated with greater reductions in mobility.

3.5.2 Results

Equations 3.3 and 3.4 are estimated over the first and second waves. Figure 3.14 shows the results for the first wave. These results suggest the closure of workplaces and stay-at-home requirements have had the most effect on the decrease in mobility during the first wave. Stay-at-home requirements, however, are only significant at the 90th percentile level. Similar results appear for restrictions on public transports, school closures, restrictions on gatherings (at the 90th percentile level), and, to a lesser extent, restrictions on internal movement. Border controls and cancellations of public events do not appear to have affected mobility.

These results should be interpreted with caution given their rapid implementation in sequence. Indeed, Figure 3.15 shows that, during the first wave, the different types of NPIs were set up quickly one after the other in our sample. On average, a few days

Figure 3.14: Effects of different types of NPIs and COVID deaths on mobility during thefirst wave (increase of 10 p.p. for each sub-index)



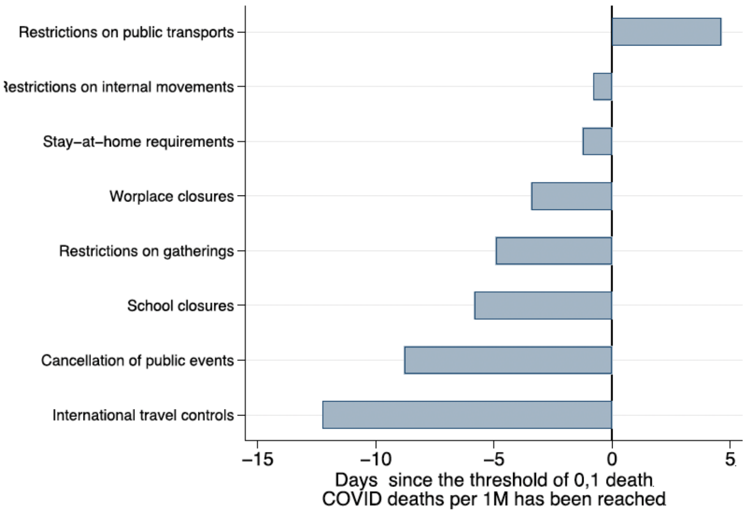
Note: 95% confidence interval. Detailed regression results available in the appendix, Table C.7.

separate their implementation over a total window of 17 days[8]. Thus, distinguishing for example the effect of restrictions on internal movements from the effect of containment is difficult as 24 hours separate their implementation on average. Besides, these coefficients can be interpreted as marginal effects associated with the addition of each measure rather than as absolute value effects. This interpretation is also underlined by the IMF (2020) and would mean that the coefficients displayed here under-estimate the effects of each restriction individually. In any case, it seems difficult to draw definitive conclusions about the relative impact of different types of NPIs on mobility.

During the second wave, stay-at-home orders no longer have a significant effect on mobility and workplace closures remain the most effective measure (Figure 3.16). This is consistent with the generalisation of teleworking during this period and the lower prevalence of stringent lockdown. They are followed by school closures (significant at the 10% level) and restrictions on internal movements. Cancellations of public events, restrictions on gatherings, and restrictions on public transport do not have a significant effect, which is likely due to less variation in these types of NPIs in the second wave. Most of these NPIs were implemented during the first wave and never lifted. International travel controls have a positive and significant effect. This last result could come from a correlation between the resumption of economic activities in certain countries and the implementation of health protocols to travel. Indeed, screenings and quarantines are types of international travel controls. Hence, their implementation might have enabled a resumption of some tourism and business travel activities, and therefore an increase in mobility in some countries.

[8]This tight sequencing for these 29 countries is surprising as countries faced different COVID shocks during the first wave. Yet, many countries adopted preventive NPIs then. In addition, some researchers have highlighted a phenomenon of mimicry between countries, which would explain the diffusion of homogeneous NPI when the situations were heterogeneous (Sebhatu et al., 2020).

Figure 3.15: Average sequencing of the implementation of NPIs during the first wave, bytype of NPI, in advanced OECD countries



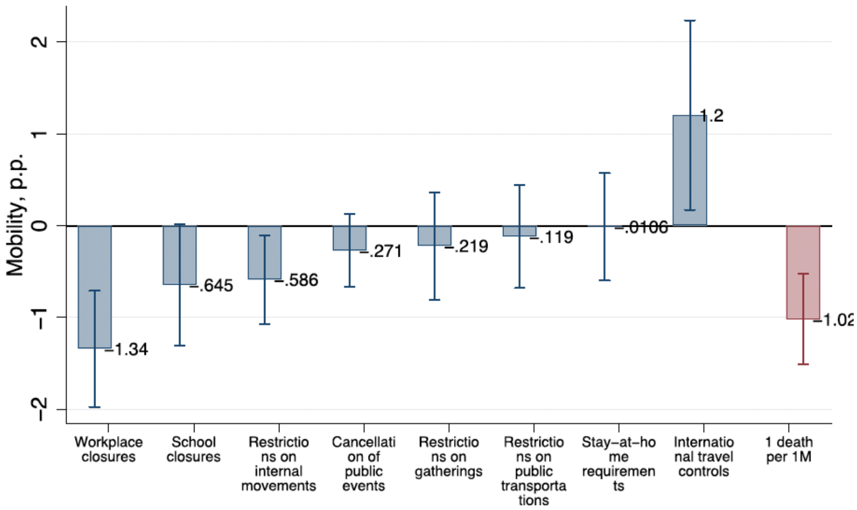
Note: The threshold of 0,1 daily deaths to 1 M was arbitrarily chosen and represents a threshold from which thereality of the COVID crisis is evident in the country. This sequencing is only shown for the first wave as thechronology of the second wave has been more heterogeneous. Besides, several NPIs put in place during the first wavehave never been lifted afterwards (for example restrictions on gatherings or event cancellations).

In addition, there has been a temporary increase in air travel during the summer and Christmas holidays that might have been captured in our coefficient (see Figure C.3).

These early results seem to show that the most stringent NPIs - the closure of workplaces and stay-at-home requirements in the first wave and the restrictions on internal movement and teleworking requirements in the second wave - had more important effects. To validate this hypothesis, we now study the effect of different NPI levels on mobility. Similarly, to Égert et al. (2020), dummy variables for each level of NPI are included in our estimations (see Equation 3.4). Results are presented for the first wave (Figure 3.17) and the second wave (Figure 3.18). Figure 3.17 is particularly revealing and shows that only high-level NPIs have a significant effect on mobility. Thus, during the first wave, stay-at-home requirements of levels 1 and 2 appear to have had little effect on mobility. Conversely, the stay-at-home orders of level 3 had the strongest effects: they are associated with a decrease in mobility over 10 p.p.

Several interpretations to this result can be put forward. The first is that the use of time fixed effects may prevent us from capturing the effects of low-level NPIs. We, therefore, re-estimate our results without time fixed effects (see Figures C.4 and C.5). As a result, some coefficients become significant, such as cancelled public events of level 2 or international travel controls of level 4. The magnitude of our effects also increases for some NPIs, in particular for stay-at-home requirements of level 3. However, these results are close to those that include time fixed effects and several low-level NPIs remain insignificant.

Figure 3.16: Effects of different types of NPIs and COVID deaths on mobility during the second wave (increase of 10 p.p. for each sub-index)



Note: 95% confidence interval. Detailed regression results available in the appendix, Table C.7.

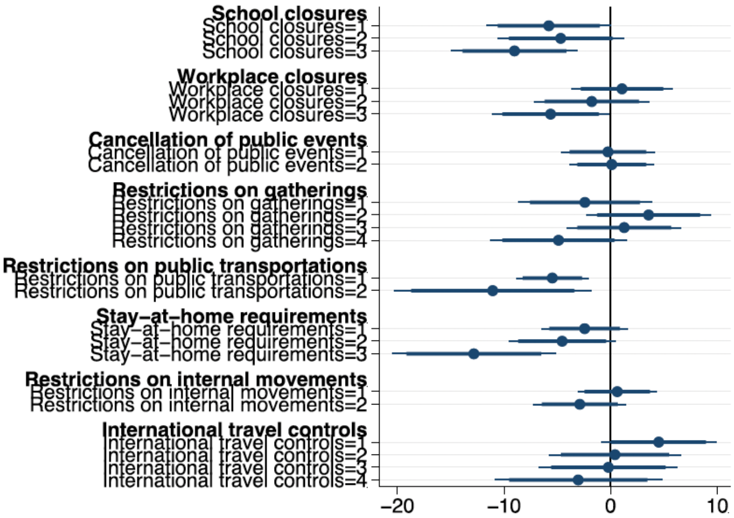
The second is that there are too few low-level NPIs in our sample. Thus, results close to zero for scores of 1 or 2 could be due to low statistical power. This hypothesis would also explain why certain NPIs which were significant in Figure 14, for example restrictions on internal movements, are no longer once broken down by level. Nevertheless, the large majority of countries implemented stay-at-home requirements of level 1 or 2 during the first wave. So, while lack of statistical power might play a role, it cannot be the only factor.

A third is that individuals may adjust their mobility more following the adoption of a range of compulsory measures. In fact, a package of government restrictions was often adopted at the same time. For example, in France, a lockdown, the generalisation of teleworking, and the closure of schools were announced concomitantly during the first wave. Thus, certain dummy variables could absorb both the effect associated with their dummy and others that were implemented at the same time.

Finally, non-linearity may exist and the effect of an individual NPI may only be felt at a high level of restriction. At first glance, the Égert et al. (2020)'s results, who use a larger sample of countries, do not seem to validate this. Their results show that stay-at-home requirements and workplace closures of levels 1 and 2 affect mobility. However, the other types of NPI follow the same pattern as ours. The differences observed in stay-at-home requirements and workplace closures can partly be explained by the use of a different sample of countries. Égert et al. (2020) include 128 advanced, emerging, and developing countries and different behavioural responses might have occurred in advanced and developing economies[9]. Therefore, the hypothesis of a non-linearity cannot be ruled

[9]Their paper also highlights the differences in effect between advanced countries and emerging and developing countries with regard to containments and closures of workplaces. However, their analysis does not make a distinction according to the level of intensity of stay-at-home requirements and workplace

Figure 3.17: Effects of different levels of NPIs on mobility during the first wave



Note: 90% and 95% confidence intervals. Detailed regression results available in the appendix, Table C.8.

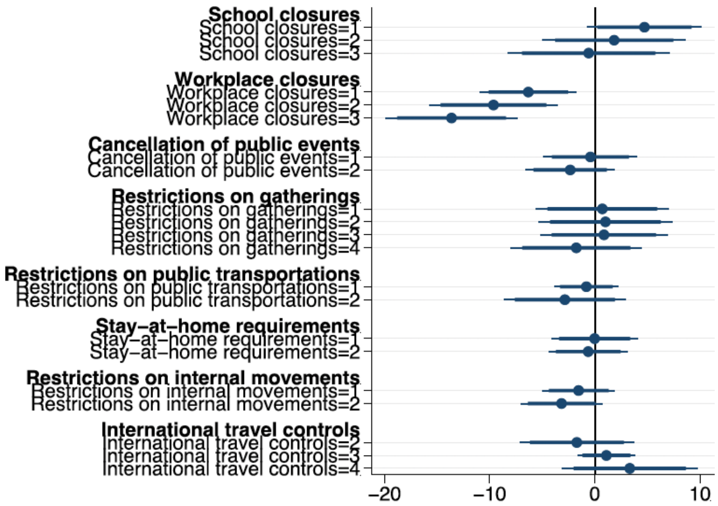
out.

These last two points are not exclusive. Despite possible identification issues, non-linearity effects may exist and the more stringent NPIs may be the only ones to have a significant effect on mobility. This interpretation would also be consistent with the country results presented in Part II. In particular, it would partly explain Germany's low elasticity during the first COVID wave. If this interpretation was confirmed and level 1 and 2 measures were sufficient to slow the epidemic significantly while having a limited effect on mobility, then a strategy where mobility restrictions are less strict could become a first-best option. This is what Égert et al. (2020) advocate in their analysis.

Our analysis by NPI level has so far focused on the first wave. However, the results from the second wave enable us to draw interesting lessons. Figure 18 depicts a situation where only workplace closures affect mobility. This time, the workplace closures of levels 1 and 2 are significant, a sign of greater adoption of teleworking even when this instruction is only recommended. These results echo those presented in Figure 16, where the closure of workplaces and restrictions on internal movements were the only significant measures, the latter being at the limit of significance (and not significant when broken down by intensity level).The absence of stay-at-home requirements of level 3 shows that NPIs during the second wave were not as stringent as during the first wave. However, the zero effects of level 1 and 2 stay-at-home requirements are puzzling. Indeed, curfews – which are a type of stay-at-home requirements – have been widely used during the second wave, and their effect on mobility has been observed, for example, in Ile-de-France (Valdano et al., 2020). This result appears to be due to the way curfews are measured in the stringency index. In particular, during the second wave, the lockdown implemented in France was

closures in advanced countries.

Figure 3.18: Effects of different levels of NPIs on mobility during the second wave



Note: 90% and 95% confidence intervals. Detailed regression results available in the appendix, Table C.8.

assigned a score of 2 in the stay-at-home category. The switch to a curfew at 8 p.m. on December 15 is a relaxation of the measure and was followed by a resumption of mobility. However, the Oxford stringency index also assigns a score of 2 to an 8 pm curfew in the stay-at-home category. Thus, while the measures were eased on December 15 in France, the stringency index does not reflect it. Hence, this choice of measurement prevents us from estimating the effects of lockdowns and curfews adequately during the second wave. This issue of categorisation might become more important as restrictions are implemented at increasingly granular levels.

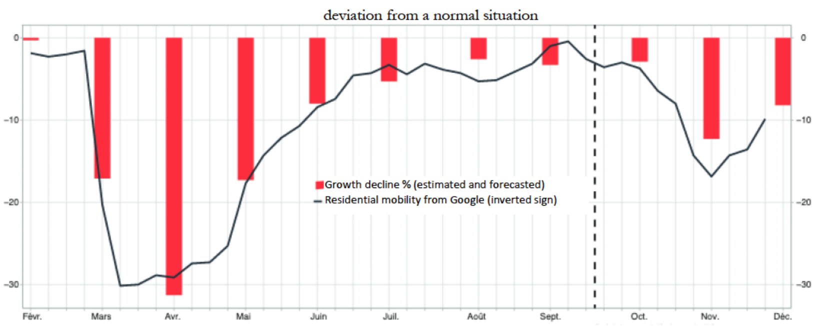
3.6 Conclusion

Overall, our results show that NPIs were the main explanatory factor behind the mobility reduction in advanced OECD countries during the first wave. Voluntary distancing then played a more important role during the second wave suggesting (i) a greater awareness of the severity of the health situation and/or (ii) an increase in individual responsibility, while restrictions were less severe during the second wave.A more detailed analysis of selected countries shows that during the first wave the effect of NPIs on mobility was stronger in France, Spain, and the United Kingdom than in the average of advanced OECD countries. This is probably due to the magnitude of the shock in these countries. Interestingly, Italy stands out as the only country in which mobility decreased both as a consequence of government restrictions and the sanitary situation. The fall in activity in the first half of 2020 was more marked in these four countries.Reactions across countries were less homogeneous during the second wave. The explanatory weight of NPI in the mobility decline is higher in France, Germany, and Italy, which is explained by the implementation of restrictive measures, and in particular second lockdowns, in these countries. Conversely, countries where the restrictions have been less strict, in part because decisions were adopted at the regional level, see the sanitary situation as the main explanatory factor. It is interesting to note that in France, the daily COVID deaths toll had zero weight on the decline in mobility during the second wave, which could suggest either a lower anxiety-inducing perception of the virus, for example, due to less negative communication, or greater confidence in the respect of individual protective measures, such as mask-wearing and handwashing.Finally, identifying the most effective type of NPI appears difficult to us. Indeed, the tight sequencing of the implementation of the measures during the first wave leads us to be careful in the interpretation of our results. Besides, the estimates obtained from the second wave, and in particular the null coefficient associated with stay-at-home measures, seem to say more about the categorization of NPIs than their effect. As countries adopt increasingly more granular NPIs the Oxford categorisation might prevent us from identifying precise impact.More research is needed on the subject: if it were confirmed that looser restrictions give room to greater awareness and empowerment of the population, then another equilibrium than tight mobility restrictions could exist. This would be conditional on the effectiveness of more targeted and less restrictive measures on limiting the circulation of the virus. Yet, if true, an effective strategy to fight the virus could emerge, reconciling health and economic imperatives more effectively than in the first wave.

Appendices - Chapter 3

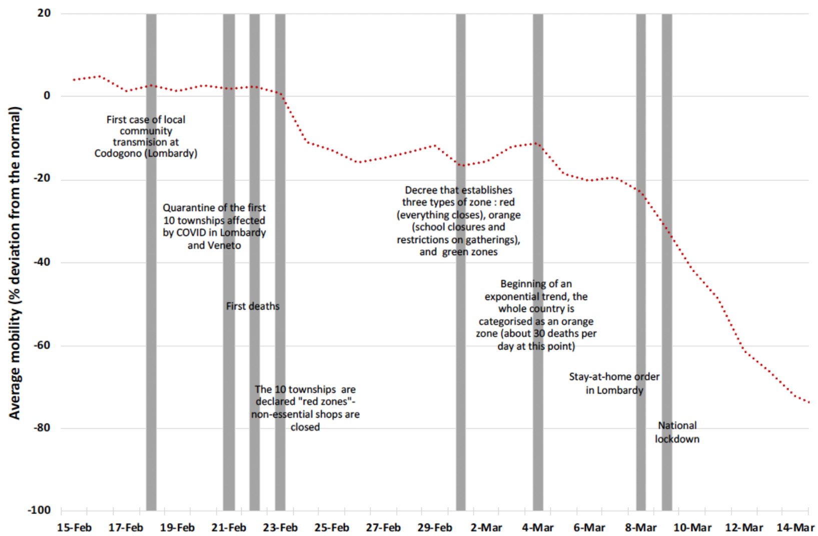
C.1 Figures

Figure C.1: Residential mobility and monthly decline in economic activity, estimated and forecasted by the INSEE (extracted from INSEE's "note de conjoncture de décembre 2020")



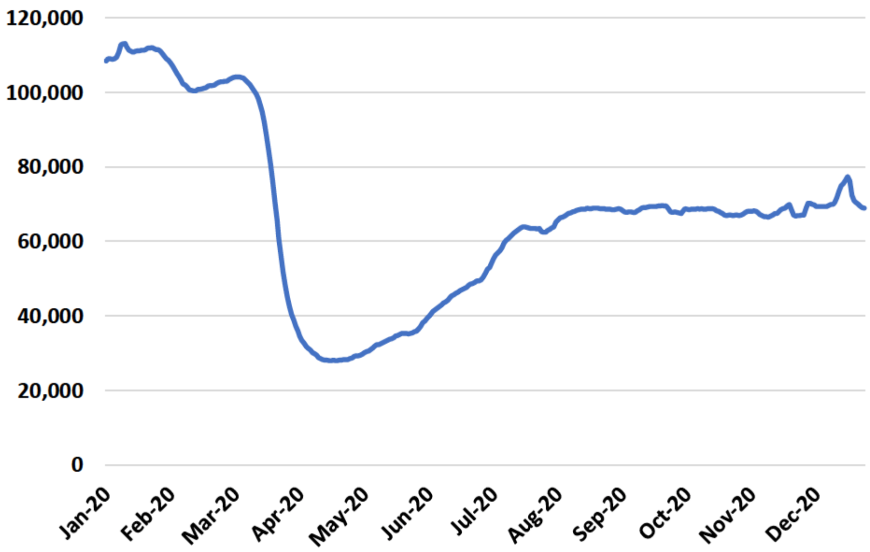
Reading indication: during the first week of December, people spent 10 % more time at home compared to a normal situation.
Note: Residential mobility data stops on Dec 6th. Weekly values are the averages of the week.
Source: Google, INSEE

Figure C.2: Evolution of mobility in Italy during the first month of the pandemic)



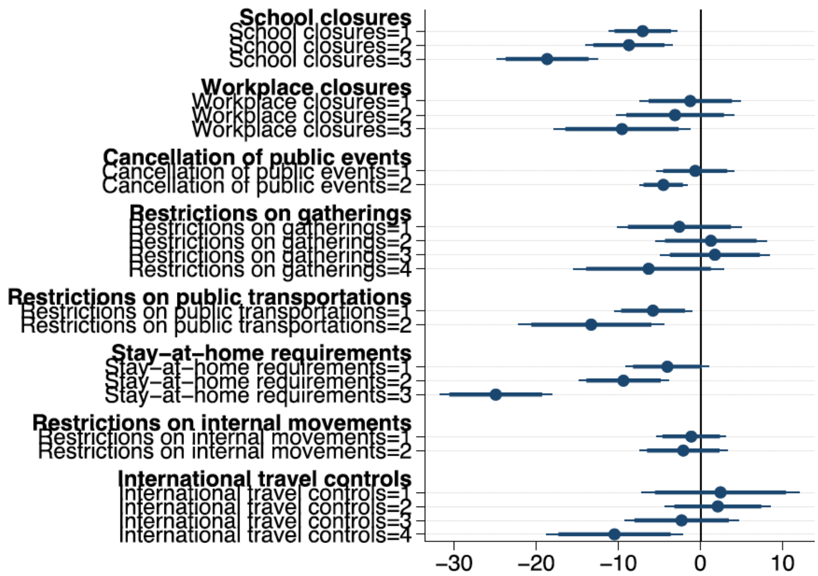
Source Google

Figure C.3: Daily commercial flights in the world (7-day moving average)



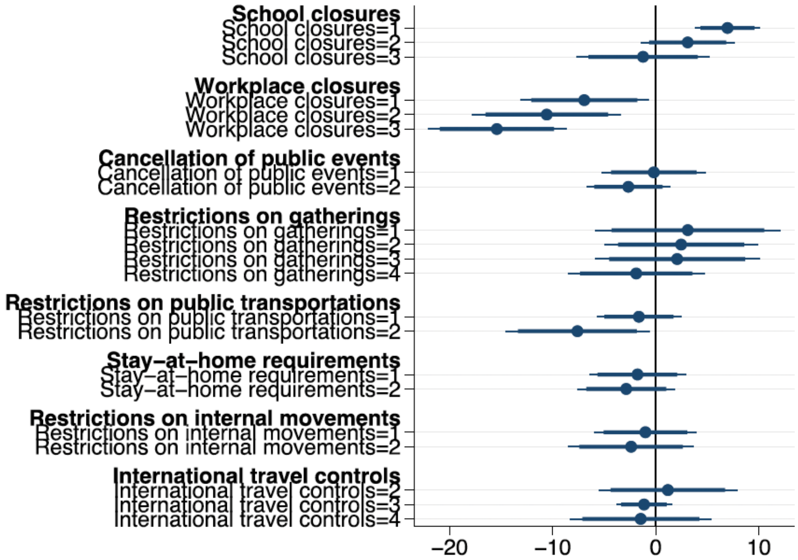
Source: Flightradar24

Figure C.4: Effects of different levels of NPIs on mobility during the first wave, without date fixed effects)



Note: 90% and 95% confidence intervals.

Figure C.5: Effects of different levels of NPIs on mobility during the second wave, without date fixed effects)



Note: 90% and 95% confidence intervals.

C.2 Tables

Table C.1: Categories of non-pharmaceutical interventions that compose the Oxford stringency index

Type of NPI	**Stringency score**
School closure	0 No measure 1 Recommended 2 Required for some categories (just high schools or just public schools) 3 Required for all
Workplace closure	0 No measure 1 Recommended (or work from home) 2 Required for some sectors or categories of workers (or work from home) 3 Required for all (or work from home) but essential workplaces
Cancellation of public events	0 No measure 1 Recommended 2 Required
Restrictions on gatherings	0 No measure 1 Restrictions on gatherings above 1000 people 2 Restrictions on gatherings between 101 and 1000 people 3 Restrictions on gatherings between 11 and 100 people 4 Restrictions on gatherings of 10 people or less
Closure of public transports	0 No measure 1 Recommended (or significant reduction of the means of transport available) 2 Required (or prohibits most citizens from using it)
Stay-at-home order	0 No measure 1 Recommended not to leave the house 2 Required not to leave the house with the exceptions of daily exercise, grocery shopping, and essential trips 3 Required not to leave the house with few exceptions (e.g. one person at a time, only once a week)
Restrictions on internal movements	0 No measure 1 Recommended not to travel between regions 2 Restrictions on movements between regions
International travel controls	0 No measure 1 Screening 2 Quarantines for arrivals from high-risk regions 3 Ban on arrivals from some regions 4 Ban on all regions or total border closure

Source: Hale et al. (2020)

Table C.2: Estimation results from equation 3.1 over the whole period

	(1) Average mobility	(2) Workplace mobility	(3) Retail and recreational mobility	(4) Transit station mobility
Oxford stringency index	-0.396***	-0.281***	-0.522***	-0.384***
	(0.0416)	(0.0386)	(0.0613)	(0.0435)
L.Deaths per million	-0.888***	-0.626***	-1.352***	-0.687***
	(0.182)	(0.155)	(0.245)	(0.205)
Constant	-6.165***	-11.36***	4.261	-11.39***
	(1.950)	(1.780)	(2.974)	(2.133)
Country FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Adj-R2	0.822	0.758	0.818	0.813
N	9496	9496	9499	9499

Standard errors in parentheses, clustered at the country and week level
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.3: Estimation results from equation 3.1 over the first wave period

	(1) Average mobility	(2) Workplace mobility	(3) Retail and recreational mobility	(4) Transit station mobility
Oxford stringency index	-0.468***	-0.402***	-0.574***	-0.428***
	(0.0585)	(0.0660)	(0.0800)	(0.0486)
L.Deaths per million	-0.503*	-0.467*	-0.624*	-0.418*
	(0.262)	(0.267)	(0.311)	(0.241)
Constant	-7.881**	-7.994**	-2.246	-13.40***
	(2.948)	(3.322)	(4.052)	(2.528)
Country FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Adj-R2	0.877	0.810	0.867	0.877
N	3754	3754	3754	3754

Standard errors in parentheses, clustered at the country and week level
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.4: Estimation results from equation 3.1 over the second wave period

	(1) Average mobility	(2) Workplace mobility	(3) Retail and recreational mobility	(4) Transit station mobility
Oxford stringency index	-0.415***	-0.272***	-0.568***	-0.405***
	(0.0412)	(0.0481)	(0.0580)	(0.0411)
L.Deaths per million	-1.125***	-0.689***	-1.543***	-1.142***
	(0.231)	(0.157)	(0.325)	(0.260)
Constant	-1.953	-7.745**	9.873***	-7.980***
	(2.036)	(2.525)	(2.858)	(2.078)
Country FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Adj-R2	0.786	0.732	0.792	0.801
N	3917	3917	3918	3918

Standard errors in parentheses, clustered at the country and week level
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.5: Estimation results from equation 3.2 over the first wave period

	(1) France	(2) Germany	(3) Italy	(4) Spain	(5) United Kingdom	(6) Switzerland
Oxford stringency index	-0.467***	-0.468***	-0.480***	-0.460***	-0.451***	-0.468***
	(0.0588)	(0.0585)	(0.0537)	(0.0607)	(0.0570)	(0.0585)
Oxford stringency index × Country==1	-0.175***	0.102***	0.135***	-0.172***	-0.241***	-0.00184
	(0.0324)	(0.0201)	(0.0135)	(0.0179)	(0.0478)	(0.0250)
L.Deaths per million	-0.403	-0.492*	-0.414	-0.405	-0.495*	-0.498*
	(0.258)	(0.261)	(0.258)	(0.266)	(0.280)	(0.263)
L.Deaths per million × Country==1	-0.616***	0.414	-1.457***	-0.104	0.794**	-0.381**
	(0.193)	(0.459)	(0.286)	(0.230)	(0.335)	(0.169)
Constant	-7.603**	-8.104**	-7.509**	-8.082**	-8.470***	-7.852**
	(3.039)	(2.963)	(2.721)	(3.050)	(2.790)	(2.955)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj-R2	0.879	0.877	0.878	0.878	0.878	0.877
N	3754	3754	3754	3754	3754	3754

Standard errors in parentheses, clustered at the country and week level
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.6: Estimation results from equation 3.2 over the second wave period

	(1) France	(2) Germany	(3) Italy	(4) Spain	(5) United Kingdom	(6) Switzerland
Oxford stringency index	-0.410***	-0.409***	-0.418***	-0.415***	-0.420***	-0.423***
	(0.0427)	(0.0418)	(0.0422)	(0.0405)	(0.0424)	(0.0403)
Oxford stringency index × Country==1	-0.267***	-0.240***	-0.0631	0.464***	0.397***	0.376***
	(0.0863)	(0.0705)	(0.0511)	(0.0835)	(0.0839)	(0.0443)
L.Deaths per million	-1.127***	-1.125***	-1.139***	-1.109***	-1.116***	-1.166***
	(0.231)	(0.231)	(0.235)	(0.232)	(0.236)	(0.238)
L.Deaths per million × Country==1	1.058***	0.255	0.581***	0.445	-0.666***	0.182
	(0.361)	(0.149)	(0.184)	(0.414)	(0.208)	(0.173)
Constant	-1.820	-1.827	-1.707	-3.098	-2.543	-1.983
	(2.018)	(2.033)	(2.067)	(2.189)	(2.009)	(1.892)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj-R2	0.786	0.787	0.786	0.787	0.786	0.790
N	3917	3917	3917	3917	3917	3917

Standard errors in parentheses, clustered at the country and week level
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.7: Estimation results from equation 3.3 over the first and second wave periods

	(1) Mobility 1st wave	(2) Mobility 2nd wave
Stay-at-home requirements	-0.0896*	-0.00106
	(0.0467)	(0.0279)
School closures	-0.0842**	-0.0645*
	(0.0307)	(0.0314)
Workplace closures	-0.0960**	-0.134***
	(0.0366)	(0.0302)
Cancellation of public events	0.00654	-0.0271
	(0.0198)	(0.0189)
Restrictions on gatherings	-0.0505	-0.0219
	(0.0318)	(0.0279)
Restrictions on public transports	-0.0787***	-0.0119
	(0.0237)	(0.0268)
Restrictions on internal movements	-0.0586**	-0.0586**
	(0.0234)	(0.0231)
International travel controls	-0.00343	0.120**
	(0.0407)	(0.0493)
L.Deaths per million	-0.378*	-1.020***
	(0.214)	(0.236)
Constant	-12.06***	-15.72***
	(3.794)	(4.287)
Country FE	Yes	Yes
Time FE	Yes	Yes
Adj-R2	0.883	0.792
N	3754	3943

Standard errors in parentheses, clustered at the country and week level
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.8: Estimation results from equation 3.4 over the first and second wave periods

	(1) Mobility 1st wave	(2) Mobility 2nd wave
School closures=1	-5.803**	4.726*
	(2.761)	(2.588)
School closures=2	-4.685	1.842
	(2.803)	(3.249)
School closures=3	-9.019***	-0.581
	(2.808)	(3.666)
Workplace closures=1	1.068	-6.320***
	(2.237)	(2.191)
Workplace closures=2	-1.771	-9.643***
	(2.555)	(2.906)
Workplace closures=3	-5.632**	-13.63***
	(2.607)	(2.993)
Cancellation of public events=1	-0.250	-0.412
	(2.070)	(2.125)
Cancellation of public events=2	0.104	-2.343
	(1.869)	(2.011)
Restrictions on gatherings=1	-2.414	0.713
	(2.974)	(3.013)
Restrictions on gatherings=2	3.560	1.015
	(2.782)	(3.046)
Restrictions on gatherings=3	1.278	0.870
	(2.542)	(2.869)
Restrictions on gatherings=4	-4.894	-1.762
	(3.031)	(2.971)
Restrictions on public transports=1	-5.466***	-0.798
	(1.598)	(1.442)
Restrictions on public transports=2	-11.06**	-2.851
	(4.408)	(2.752)
Stay-at-home requirements =1	-2.442	-0.0168
	(1.910)	(1.949)
Stay-at-home requirements =2	-4.550*	-0.626
	(2.375)	(1.778)
Stay-at-home requirements =3	-12.83***	.
	(3.637)	(.)
Restrictions on internal movements =1	0.622	-1.538
	(1.768)	(1.644)
Restrictions on internal movements =2	-2.896	-3.162
	(2.053)	(1.853)
International travel controls =1	4.506*	.
	(2.560)	(.)
International travel controls =2	0.404	-1.709
	(2.926)	(2.579)
International travel controls =3	-0.213	1.101
	(3.087)	(1.311)
International travel controls =4	-3.033	3.327
	(3.733)	(3.085)
L.Deaths per million	-0.345	-1.114***
	(0.205)	(0.233)
Constant	-16.70***	-13.41**
	(4.478)	(4.705)
Country FE	Yes	Yes
Time FE	Yes	Yes
Adj-R2	0.890	0.794
N	3753	3908

Standard errors in parentheses, clustered at the country and week level
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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