Dunaiski, Maurice; Tukiainen, Janne

Working Paper

Does income transparency affect support for redistribution? Evidence from Finland's tax day

Discussion paper, No. 159

Provided in Cooperation with:
Aboa Centre for Economics (ACE), Turku

Suggested Citation: Dunaiski, Maurice; Tukiainen, Janne (2023) : Does income transparency affect support for redistribution? Evidence from Finland's tax day, Discussion paper, No. 159, Aboa Centre for Economics (ACE), Turku

This Version is available at:
https://hdl.handle.net/10419/298468

Maurice Dunaiski and Janne Tukiainen

Does income transparency affect support for redistribution? Evidence from Finland's tax day

Aboa Centre for Economics

Discussion paper No. 159

Turku

February 2023

The Aboa Centre for Economics is a joint initiative of the economics departments of the University of Turku and Åbo Akademi University.

ISSN 1796-3133

Printed in Uniprint
Turku
February 2023

Maurice Dunaiski and Janne Tukiainen

Does income transparency affect support for redistribution? Evidence from Finland's tax day

Aboa Centre for Economics
Discussion paper No. 159
February 2023

ABSTRACT

This paper examines whether income transparency - the public release of citizens' income information - affects support for redistribution. We leverage a quasi-experiment in Finland, where every year on the so-called tax day, the authorities release income information on Finland's top earners to the public. To identify causal effects we compare respondents who took part in the European Social Survey shortly before and after the event. We find that the tax day increases perceptions that earnings of the top 10% are unfair, but that public support for redistribution remains largely unaffected. A notable exception are top earners, who decrease their support for redistribution, and young people, who increase their support for redistribution. Our results highlight the scope conditions of previous experimental studies, and suggest that increasing exposure to inequality through a real-world policy, rather than experimental treatments, may trigger only marginal changes in support for redistribution.

JEL Classification: D31, D63, D72, D80, H20, H23, H24, H31

Keywords: income transparency, inequality, redistribution, taxes

Contact information

Maurice Dunaiski
UNODC Research.
Email: maurice.dunaiski (at) un.org

Janne Tukiainen
Department of Economics, University of Turku.
Email: janne.tukiainen (at) utu.fi

Acknowledgements

We thank Tiina Heinilä, Anna-Maija Juuso and Lauri Seppälä for excellent research assistance. For helpful comments we thank Johan Ahlback, Dan Berliner, Ulla-Maija Heikkilä, Simon Hix, Rafael Hortala-Vallve, Zhen Jie Im, Anu Kantola, Roxanne Kovacs, Valentino Larcinese, Kostas Matakos, Miroslav Nemcok, Francesco Porcelli, Melissa Sands, Claudia Serra-Sala and Sven Simon, and the audiences at EPCS 2022, IIPF 2022, ZEW-Public Finance 2022.

1 Introduction

> "Finland is unusual, even among the Nordic states, in turning its release of personal tax data into a public ritual of comparison. Though some complain that the tradition is an invasion of privacy, most say it has helped the country resist the trend toward growing inequality that has crept across of the rest of Europe."[1]
>
> *New York Times, 1 Nov 2018*

Income inequality has increased substantially in most industrialised democracies over the past decades (OECD 2011). Figure A6 shows that between 1980 and 2019, the share of total pre-tax income going to the top 10% of US adults increased from 33% to 45%, while the share going to the bottom 50% decreased from 20% to 13%. Similar trends can be observed in Europe, including in Finland. The canonical political economy model predicts that governments will face greater pressure to redistribute income as inequality increases and the distance between the median voters' income and the mean income in society grows (Meltzer & Richard 1981).[2] Yet, contrary to the model's predictions, rising inequality has not led to an increase in public support for redistribution (Ashok et al. 2015, McCall et al. 2017).[3] This disconnect has triggered a large amount of research into the factors that might suppress demand for redistribution, including lack of information (Cruces et al. 2013, Kuziemko et al. 2015, Alesina et al. 2018, Hvidberg et al. 2020), political ideology (Alesina & Fuchs-Schuendeln 2007), fairness beliefs (Alesina & Angeletos 2005), economic insecurity (Rehm et al. 2012) and ethnic heterogeneity (Alesina et al. 1999, 2001, Dahlberg et al. 2012, Alt & Iversen 2017). However, we still know very little about whether and how specific policy

[1] https://www.nytimes.com/2018/11/01/world/europe/finland-national-jealousy-day.html

[2] Most scholars agree that the Meltzer-Richard model has limited explanatory power and that it makes unreasonable assumptions (e.g. that voters have perfect information) (Bredemeier 2014). However, the model continues to be widely used for its analytical tractability, as it sets a clear benchmark for assessing the value of alternative theories (see Cavaille 2020).

[3] Survey evidence from several industrialised democracies suggests that support for redistribution has remained flat or even decreased, depending on the measure used (Ashok et al. 2015, McCall et al. 2017). Redistribution refers to the process of taking material goods from those who need it least, and giving it to those who need it most (Cavaille 2020).

interventions can shift support for redistribution (Trump 2018).[4] Several recent survey- and field experiments (Cruces et al. 2013, Kuziemko et al. 2015, Fernández-Albertos & Kuo 2018, Sands 2017, Karadja et al. 2017, Fehr et al. 2019, Thal 2020, Dietze & Craig 2020, Condon & Wichowsky 2020) try to address the question of what it takes to shift demand for redistribution by manipulating subjects' exposure to inequality. While some find that subjects adjust their redistributive preferences when exposed to inequality (e.g. Cruces et al. 2013), others find that redistributive preferences remain largely unaffected (e.g. Kuziemko et al. 2015). A limitation of these studies is that the experimental manipulations are either customised treatments that rarely occur in the real world (Condon & Wichowsky 2020), or interventions in the field that are difficult to implement on a larger scale, such as randomising the presence of a poor person in a wealthy neighbourhood (Sands 2017).

Our paper advances the literature by studying the effect of a real-world policy (income transparency) on support for redistribution. Income transparency – the public release of citizens' income information – has been promoted as an effective policy intervention to reduce gender and racial pay gaps (Cooney 2018, Baker et al. 2019), tackle excessive executive pay (Mas 2017), and deter tax evasion (Bø et al. 2015). It remains unclear, however, whether income transparency can lead to shifts in public support for redistribution. Despite the lack of evidence, we have good reasons to expect an effect. Income transparency is a policy that increases citizens' exposure to information about income inequality (Perez-Truglia 2020, Reck et al. 2022), and previous survey- and field experiments suggest that such exposure can correct misperceptions about inequality (Hvidberg et al. 2020) and trigger greater demand for redistribution (Cruces et al. 2013, Sands & de Kadt 2020).

We study the effect of an income transparency policy on redistributive preferences in the context of Finland, where public support for redistribution is relatively high and income

[4]The comparative welfare state literature has long been interested in how welfare spending affects public attitudes towards the welfare state (see Busemeyer et al. 2021 for a review). However, the focus has primarily been on explaining long-run trends and cross-country differences in public opinion, rather than identifying the causal effect of specific policy interventions.

inequality is low compared to other industrialised democracies (see Figure A6). To isolate the causal effect of the income transparency policy on citizens' attitudes, we take advantage of a quasi-experiment in Finland, where every year on the first working day of November, the tax authority releases the income information of everyone who earns more than €100,000 per year to the media. The so-called tax day (*Veropäivä*) triggers an annual media spectacle focused on Finland's top earners, celebrities and potential tax dodgers (Barry 2018). Given that the tax day takes place every year, we argue that the event primarily serves to increase the *salience* of inequality in the public debate, rather than providing citizens with much new information about income inequality in Finland. The repeated nature of the tax day also means that the estimates we recover should be interpreted primarily as the effect of one additional exposure to the tax day, rather than the effect of the tax day per se. An exception may apply to the youngest age group, who have less previous exposure to the tax day and may therefore receive a stronger information treatment.

We use media data from 2019 to show that the tax day coincides with a sharp spike in the salience of income inequality in the media. To estimate how the tax day affects citizens' attitudes, we compare respondents who took part in the 2002-2018 European Social Surveys (ESS) shortly before and after the event. To ensure as-if random exposure, we focus on a narrow time window around the tax day, which means we can only assess the short-term effects on citizens' attitudes. We find that the tax day increases perceptions that earnings of the top 10% are unfair. The effects are strongest amongst below-median income earners. Despite these initial reactions, we find that the tax day leaves citizens' support for redistribution largely unaffected. We show that the overall null effect is precisely estimated and unlikely due to ceiling effects, anticipatory effects, or the repeated nature of the tax day. However, the overall null effect also hides substantial heterogeneity. We find that individuals in the top income decile respond to the tax day by decreasing their support for redistribution, while individuals in the youngest age group (15-24 years) respond by increasing their support for redistribution. We find no evidence of heterogeneous effects by

educational status, political interest, political ideology, and partisanship.

We explore potential mechanisms behind the heterogeneous effects by income and age. One explanation is that the tax day suppresses demand for redistribution amongst the top income decile because it triggers a process of motivated reasoning aimed at justifying their privileged position in the income distribution, in line with recent sociological research on Finland's top earners (Kantola & Kuusela 2019, Kantola 2020). The reaction amongst the youngest age group (15-24 years) in turn appears to be driven by the tax day's effect on (mis-)perceptions of relative income status, which we show is stronger amongst the youngest age group compared to older age groups. Taken together, our findings indicate that income transparency can increase citizens' concern about income inequality, but may only marginally affect their support for government action to ameliorate inequality. While the egalitarian context and the repeated nature of the Finland's tax day may limit the generalisability of our findings, they imply that lack of exposure is not the key constraint preventing demand for redistribution from "keeping up" with rising inequality. Instead, citizens may simply fail to connect their concern about inequality with concrete policy measures that would address inequality. Furthermore, citizens' redistributive preferences appear to be rooted in more stable, underlying ideologies, and may be difficult to alter once they are formed in early adulthood.

Our results have important implications. First, they highlight the scope conditions of previous survey- and field experiments (Condon & Wichowsky 2020, Sands 2017, Sands & de Kadt 2020) which have shown that subjects' support for redistribution can be manipulated by exposing them to inequality. Our results indicate that triggering such a response may be more difficult to achieve via real-world policy interventions. Second, lack of exposure to inequality (e.g. due to residential or educational segregation) is frequently put forward as an explanation for why demand for redistribution has not kept up with growing inequality (Condon & Wichowsky 2020). Our results suggest that policy interventions aimed at increasing cross-class exposure may not necessarily be sufficient to address this mismatch.

Related literature - Our paper relates to several strands of literature. First, we contribute to the literature on income transparency by studying, for the first time, its effect on political attitudes. Previous studies have shown that the release of income information can affect individuals' job satisfaction (Card et al. 2012), job retention (Mas 2017), job performance (Blanes-i Vidal & Nossol 2011, Cullen & Perez-Truglia 2018), salary negotiations (Baker et al. 2019, Obloj & Zenger 2022), and tax compliance (Hasegawa et al. 2012, Bø et al. 2015, Slemrod et al. 2022). Most closely related to our paper is a recent study by Perez-Truglia (2020), who finds that income transparency in Norway widened the gap in self-reported happiness between the rich and poor by 29% and increased the life satisfaction gap by 21%. We shift the focus to political outcomes and ask whether income transparency can affect individuals' support for redistribution.

Second, our paper relates to the literature on redistributive preferences. We contribute to a growing body of experimental studies that attempt to manipulate subjects' support for redistribution by exposing them to inequality. A first set of studies do this by providing subjects with new, objective income information in survey experiments, with mixed results (Kuziemko et al. 2015, Cruces et al. 2013, Fernández-Albertos & Kuo 2018, Karadja et al. 2017, Fehr et al. 2019). A second set of studies do not provide new income information, but instead manipulate survey respondents' subjective social status, finding heterogeneous effects on redistributive preferences depending on respondents' objective income status (Brown-Iannuzzi et al. 2015, Condon & Wichowsky 2020, Thal 2020). A third set of studies expose subjects to visible markers of inequality in field experiments. Sands (2017) randomizes the presence of a visibly poor person in wealthy neighborhoods in Boston and finds that wealthy individuals become less supportive of redistribution as a result. Sands & de Kadt (2020) run a field experiment in South Africa where they randomize the presence of an expensive car in a poor neighborhood. They find that passersby who are exposed to an expensive car are more likely to sign a wealth tax petition.

Our findings speak directly to the second and third set of studies, given that our treatment primarily increases the *salience* of inequality, rather than providing new information about inequality. By studying the effect of a real-world policy (income transparency) on support for redistribution, we overcome a limitation of previous survey experiments, which is that individuals' support for redistribution is manipulated via customised treatments that rarely occur in the real world. We also address a limitation of previous field experiments, which is that the experimental manipulations cannot be implemented on a larger scale, leaving the policy implications unclear.[5]

Third, our paper contributes to the literature on perceptions of the income distribution (Cruces et al. 2013, Fernández-Albertos & Kuo 2018, Karadja et al. 2017, Hvidberg et al. 2020). We study the formation of beliefs about the income distribution using naturally occurring variation arising from the Finnish tax day, rather than providing subjects with information in a controlled setting. To our knowledge only two other studies have done this, and both find, as we do, that perceptions of the income distribution can be manipulated "in the wild" (Perez-Truglia 2020, Londoño-Vélez 2022).

2 Background

Finland is a consolidated democracy with one of the most comprehensive welfare systems in the world (Pesonen & Riihinen 2002). Support for redistribution in Finland is relatively high and economic inequality is low compared to other industrialised democracies. Figure A6 shows that, even though income inequality in Finland increased markedly since the early 1990s, it has done so at much lower levels than in the US and the rest of Europe. Support for redistribution in Finland is also higher, on average, than in the rest of Europe. For example, in the ESS data from 2002-18, Finnish respondents are significantly more

[5] A noteable exception is Londoño-Vélez (2022), who uses a quasi-experiment in Colombia to show that exposure to low-income students can increase support for redistribution amongst high-income students.

likely to agree with the statement that the government should take measures to reduce differences in income levels, compared to respondents in the rest of the EU.[6] As in many other Western democracies, the rich in Finland are less supportive of redistribution than the poor. In the Finnish ESS data from 2002-2018, the correlation between respondents' support for redistribution ("the government should take measures to reduce differences in income levels") and respondents' income rank (household income deciles) is negative (β = -0.17) and statistically significant (p < 0.001). Despite repeated exposure to inequality-related information via the tax day, Finnish citizens are not necessarily more knowledgeable about inequality compared to citizens in other industrialised democracies. In 2020, more than 60% of Finnish citizens overestimated the share of national income going to the top 10%. These (mis-)perceptions are comparable to those found, for example, in France and Slovenia (OECD 2021).

Income transparency has a long tradition in Finland and, in many parts of the country, municipal tax records have been publicly available as far back as the 1920s. From the 1960s until the late 1980s, ordinary citizens could purchase so-called tax calendars, which contained the income information of everyone in their municipality. In 2000, new legislation came into force, which allowed the media to purchase lists of individuals with the highest taxable (earned and capital) income in Finland.[7] The tax lists, which include everyone with a pre-tax income of €100,000 or more in the previous tax year, are released to the media on the first working day of November of every year.[8] The specific information released on the tax day includes the person's name, year of birth and province of residence, the total earned and capital income subject to taxation, the total amount of taxes and levies paid, and

[6]Response options range from disagree strongly (1) to agree strongly (5). The mean response in Finland is 3.92 and the mean response in the rest of the EU is 3.86. The difference in means is relatively small but statistically significant (t = -7.576, $p < 0.001$, n = 373,979). Data are from all available ESS rounds between 2002 and 2018. Israel, Russia, Turkey, and Ukraine are excluded.

[7]An English translation of the law is available at: https://finlex.fi/en/laki/kaannokset/1999/19991346

[8]Since 2019, individuals can request to be removed from the list of top earners that is shared with the media on the tax day. In 2019, around 200 top earners had their information removed. In 2020, this rose to around 4,400 as the request could be submitted online via OmaVero. Requests have to be made separately each year.

the total amount of tax refund. The timing of the tax day is unrelated to other important political events in Finland (e.g. national elections), and was chosen because the tax calendar ends in October. Table A1 in the appendix lists the exact dates for all tax days since 2000.

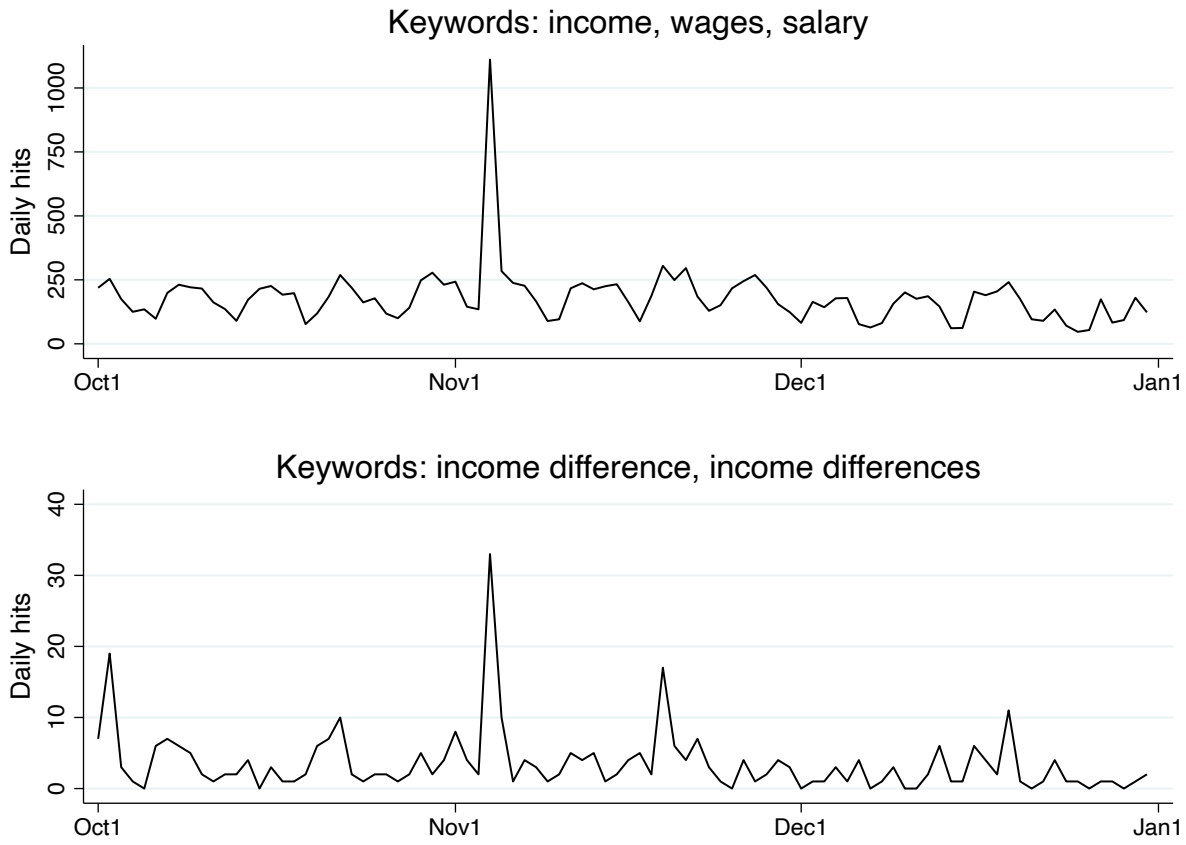
Ever since the law change in 2000, the tax day has become an annual media spectacle focused on Finland's top earners, celebrities and potential tax dodgers (Barry 2018). Historically, income transparency in Finland was justified as a means to ensure tax compliance (Lohiniva-Kerkelä 2003). Today, the tax day is often justified as a means to encourage cross-class comparisons between the rich and poor, and the issue of economic inequality features prominently in the public debate (Yläjärvi 2020, Barry 2018). Several national newspapers such as *Helsingin Sanomat* and *Iltalehti* use the tax day to launch or update databases (*Verokone*) that allow readers to search for the names and incomes of Finland's top earners.[9] The tax day is a highly salient event in Finland and one of the most important media events of the year (Barry 2018). Figure 1 below uses data from 2019 to show that the tax day creates significant spikes in media coverage related to keywords such as salary, income, and inequality during the first few days of November. The spikes are large, but relatively short-lived. Figure A7 in the appendix shows that Google search queries related to the tax day follow the same pattern.

3 Conceptual framework

Given that Finland's tax day has taken place every year since 2000, we do not expect the event to provide the public with much new information about the objective level of income inequality in the country. Instead, we argue that the tax day and the resulting media coverage of Finland's top earners primarily serves to bring the issue of income inequality to

[9]Note that the income information of every Finnish citizen (regardless of their income) can be requested by phone or via customer terminals in local tax offices.

Figure 1: Daily media hits for keywords related to the tax day (Oct–Dec 2019)



Data: LianaMonitor/VATT Institute for Economic Research. *Note:* In 2019, the tax day was on Monday, November 4th. The Finnish keywords for the top panel were "tulot", "palkat", "palkka", and for the bottom panel the Finnish keywords were "tuloero", "tuloerot". LianaMonitor searches all Finnish-language news articles published online within a specified time period. This means that radio and television content is likely underrepresented relative to print media content.

the attention of ordinary citizens. In other words, we expect the tax day to increase the *salience* of income inequality amongst the public.[10] We use the term salience to refer to the degree to which citizens engage with a political issue, in our case income inequality (Moniz & Wlezien 2020).[11] If the tax day increases the degree to which citizens engage with the issue of income inequality, we expect the event to increase perceptions that incomes at the

[10]In Figure 1 we provided some initial evidence that the tax day triggers a spike in the salience of income inequality in the Finnish media. However, we also expect the tax day to increase the salience of income inequality amongst ordinary citizens (not just in the media).

[11]Issue salience can be conceived as being a function of two factors: first, the importance an individual attaches to the issue and, second, the extent to which an individual perceives the issue to be a problem (Moniz & Wlezien 2020).

top are unfairly high, especially amongst the less affluent.

Besides this initial effect, we also expect the tax day to widen the gap in support for redistribution between the rich and the poor. Specifically, we expect that less affluent individuals will respond to the tax day by increasing their support for redistribution, and that affluent individuals will respond by decreasing their support for redistribution.[12] Several mechanisms could explain such divergent effects. First, by increasing the salience of inequality, the tax day might remind the (less) affluent that they would stand to (benefit) lose from redistribution (Sands 2017, Nishi et al. 2015, Côté et al. 2015).[13] Second, by focusing on the incomes of the super-rich, the tax day may reduce the perceived social status of the less affluent, and as a result increase their support for redistribution (Condon & Wichowsky 2020). Amongst the affluent, the tax day may also trigger social status concerns and lead to a "keeping up with the Kardashians" reaction, where the welfare state and the associated tax burden are perceived as standing in the way of catching up with the super-rich (Thal 2020). Finally, the tax day could widen the gap in support for redistribution by correcting individuals' misperceptions about their relative position in the national income distribution, given that individuals at the bottom tend to overestimate their position, while those at the top tend to underestimate their position (Cruces et al. 2013, Karadja et al. 2017, Fehr et al. 2019, Hvidberg et al. 2020). The repeated nature of the tax day makes this mechanism less likely, as most citizens will not receive much new information about their relative income from being exposed to an additional tax day. An exception, however, may be young people, who are less likely to have experienced previous tax days.

[12]The expectation of heterogeneous effects builds on the income transparency literature, which has consistently found that individuals' relative income position is a crucial factor moderating their response to income transparency. For example, Card et al. (2012) find that workers with below-median salaries report lower job satisfaction when incomes are made public, while those earning above the median remain unaffected. Similarly, Perez-Truglia (2020) finds that income transparency widens the subjective well-being gap between the rich and poor.

[13]In a field experiment, Sands (2017) finds that affluent subjects become less supportive of redistribution when randomly exposed to visibly poor person in their local neighbourhood. In a laboratory setting, Nishi et al. (2015) find that visible endowment inequality makes richer participants contribute less to their network. In a survey experiment, Côté et al. (2015) find that affluent participants become less generous (in a dictator game), when they are induced to believe that they lived in an unequal area.

4 Empirical strategy

4.1 Methods

The unique institutional setting of Finland's tax day makes it possible to identify the causal effect of income transparency on individuals' attitudes in a before-and-after type research design. We take advantage of the fact that the tax day coincides with the implementation period of the ESS in Finland. Whether respondents took part in the ESS shortly before or after the tax day can be considered as-if-random, so we can estimate the causal effect of the tax day by comparing responses shortly before and after the event. This approach is sometimes referred to as *Unexpected Event during Survey Design*, and has been used to study the effect of events such as terrorist attacks (Finseraas & Listhaug 2013, Legewie 2013, Muñoz et al. 2020), election victories (Giani & Méon 2019), leadership transitions (Mikulaschek et al. 2020), and football victories (Depetris-Chauvin et al. 2020). Even though our event is not unexpected, it is likely to meet the identification assumptions of this design. Valid identification relies on two key assumptions: temporal ignorability and excludability (Muñoz et al. 2020). Temporal ignorability means that the moment at which each respondent is interviewed during the fieldwork is independent from the timing of the tax day. Balance tests on pre-determined covariates (age, gender, education, etc.) suggest that this assumption is plausible within a 10-day window around the tax day (see Figure A8 in the appendix). Further away from the tax day, as-if random treatment assignment is less plausible given that respondents who are harder to reach are more likely to be interviewed later in the fieldwork period (see Figure A10 in the appendix). Using an even narrower window around the tax day in turn makes our estimates susceptible to bias from day-to-day variation in the number and types of respondents interviewed each day (see Figure 2 below).[14]

[14]Despite similarities in the approach and estimation, we follow Muñoz et al. (2020) in not considering this as a regression discontinuity (RD) design. Nonetheless, we note that a 10-day window minimises the mean-squared error (MSE) of the local polynomial RD point estimator (MSE-optimal bandwidth = 11.6).

Excludability means that the timing of the survey interview only affects the outcome of interest through the respondent's exposure to the tax day. Threats to identification can arise from time trends in the outcome variable and from simultaneous events (Muñoz et al. 2020). While the excludability assumption cannot be directly tested, we present results from several placebo tests, which support our identification strategy. First, we show that the tax day has no significant effects on a placebo outcome (attitudes towards gays and lesbians). Second, we re-run our main analysis on ESS respondents from Sweden and find null effects. Third, we test for effects of "fake" tax days prior to the actual tax day and find null effects (see Section G in the appendix). Finally, it is possible that changes in citizen's attitudes are driven by the framing of the tax day by political parties or the media rather than the event per se, which can be considered a problem of "imprecise treatment" (Muñoz et al. 2020).[15] We find no evidence that the tax day affects support for redistribution differently depending on whether respondents have high- or low media exposure (Figure A31), which casts some doubts on the idea that framing by political actors drives people's response to the tax day.

We estimate the effect of the tax day using the following OLS model:

$$Y_{it} = \beta_1 Treatment_{it} + \beta_2 Days_{it} + \beta_3 (Treatment_{it} \times Days_{it}) + \gamma_t + \epsilon_{it} \quad (1)$$

where Y_{it} refers to the outcome of interest (support for redistribution, unfairness perception), $Treatment_{it}$ is a dummy equal to one on and after the tax day, and zero before the tax day, $Days_{it}$ is a running variable indicating the number of days before and after the tax day (with zero on the tax day itself), and γ_t refers to survey year fixed effects. The main coefficient of interest is β_1, which captures the size of the discontinuity in the outcome on the tax day. The coefficient on the interaction term β_3 in turn indicates whether the treatment effect changes

[15] We did not find any official statements by political parties on the tax day. However, we found some differences in terms of how left- and right-leaning politicians and media outlets frame the tax day (with the former focusing on the fact that wealth is not reported, while the latter focus on the tax burden of the top income earners).

(weakens or strengthens) as time goes by after the tax day. Finally, β_2 captures linear time trends in the outcome variable prior to the tax day. We follow Muñoz et al. (2020) and use conventional standard errors, as they have a very similar setup with ESS data from a single country.[16]

In the baseline model, we restrict the sample to a 10-day window around the tax day in a given year, as covariate balance tests suggest that as-if-random treatment assignment is plausible within this window (see Figure A8). As a robustness check, we also present estimates for alternative bandwidths of 5 to 30 days around the tax day. In the baseline model, we furthermore exclude all respondents who were interviewed in the three days prior to the tax day. We do this to allow for the possibility that media coverage of the tax day builds up for a few days before the event, so these respondents may have already been "treated" by the tax day. We find some evidence for a build-up in media coverage in the number of tax day-related keyword hits that we observe just before the event (see Figure 1). As a robustness check, we also present results for alternative exclusion windows just before the tax day.

4.2 Data

We use data from all available rounds of the Finnish ESS (2002-2018).[17] The ESS is a nationally representative survey that has been implemented in Finland every two years since 2002. The fieldwork period is typically from September to December, with a few interviews also conducted into the next year. Figure 2 shows that the tax day falls roughly into the middle of the fieldwork period, and that there is no obvious bunching of respondents before or after the event.[18] We interpret this as further evidence in support of the temporal ignorability

[16] For completeness we also report the main results with robust standard errors clustered at the level of the running variable (see Appendix C).

[17] The ESS data are available at: https://www.europeansocialsurvey.org

[18] Figure A9 in the appendix provides a more fine-grained picture by zooming in on the 40-days around the tax day.

assumption. Figure 2 also shows that survey enumerators conducted fewer interviews on Fridays, Saturdays and Sundays. Given that the tax day usually occurs on the first working day of November, we therefore record substantially fewer respondents in the 2-3 days just before the event.[19] These respondents may differ systematically from other respondents, for example because the ESS fieldwork guidelines require that unsuccessful interview attempts must be followed-up at the weekend.[20] While excluding three days prior to the tax day goes some way to address this concern, we also show that our results are robust to including day-of-the-week fixed effects in our regression models (Appendix E).

The fieldwork is implemented by Statistics Finland in collaboration with the Department of Social Research at the University of Turku. The data are collected through face-to-face computer assisted personal interviews (CAPI) in Finnish or Swedish. The sample is selected by one-stage random sampling and is representative of all persons aged 15 and over who reside in private households.[21] Quota sampling and substitution of non-responding households or individuals are not permitted. The ESS aims for a response rate of at least 70% and the Finnish sample typically includes around 2000 respondents per survey round. The ESS fieldwork guidelines require at least four personal visits to each sample unit before it is abandoned as non-productive.[22] Figure A10 in the appendix plots the relationship between the number of attempted contacts with sampled units and the fieldwork day when the interview was completed. It shows that respondents who are harder to reach are more likely to be interviewed later in the fieldwork period. This suggest that our strategy of focusing on a narrower 10-day window around the tax day is advisable to avoid potential biases related to reachability (Muñoz et al. 2020).

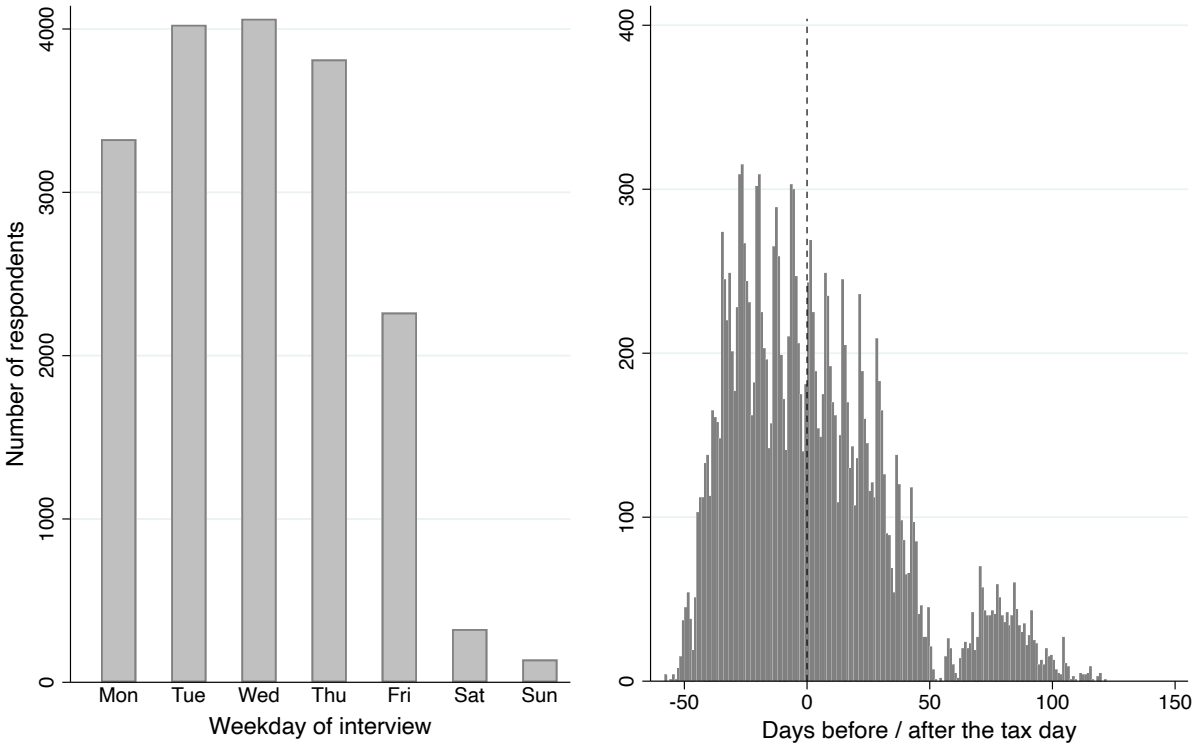
[19] This is confirmed by a non-parametric density test (see Cattaneo et al. 2018), which rejects the null hypothesis that there is no discontinuity in the density of the running variable at the threshold (t=1.9; p=0.06).

[20] Field Procedures in the European Social Survey Round 9: Guidelines for Enhancing Response Rates and Minimising Nonresponse Bias (p.10).

[21] https://www.europeansocialsurvey.org/about/country/finland/finnish/methods.html

[22] See Field Procedures in the European Social Survey Round 9: Guidelines for Enhancing Response Rates and Minimising Nonresponse Bias (p.10).

Figure 2: Number of respondents by weekday and interview date (ESS 2002-18)



Data: ESS Finland 2002-18. *Note:* The left panel shows the number of survey respondents by weekday on which the interview was conducted. The right panel shows the number of respondents by interview date relative to the tax day. Exact dates for the tax days are found in Table A1. Figure A9 zooms in on the 40 days before and after the tax day.

Table 1 provides summary statistics for the dependent variables used in the analysis. Our main dependent variable (support for redistribution) captures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from one (disagree strongly) to five (agree strongly). This measure of support for redistribution is quite general,[23] and may tap into respondents' attitudes towards the appropriate size of government. As a robustness check, we therefore use four alternative measures of support for redistribution, which capture, respectively, respondents' support for unemployment benefits, their support for public childcare, their preference for economic equality,

[23]For example, it does not distinguish between redistribution "from the rich" and "to the poor" (Cavaillé & Trump 2015).

and their support for a social safety net (see Table A2 for details).

Table 1: Summary statistics

	Years	Obsv.	Mean	SD	Min	Max
Support for redistribution	2002-18	17766	3.92	0.99	1	5
Unfairness perception (of top 10% incomes)	2018	1681	5.70	1.53	1	9

Data: ESS Finland 2002-18. *Note:* Support for redistribution captures the extent to which respondents agree that the government should take measures to reduce differences in income levels. Unfairness perception captures how fair respondents think the incomes of the top 10% in Finland are, with high values reflecting higher unfairness perceptions.

To our knowledge, available national surveys implemented in Finland during the relevant time period do not include standard issue salience questions, such as what respondents think is the most important problem facing the country (Moniz & Wlezien 2020). We therefore use an 2018 ESS survey question on unfairness perceptions of top incomes as a proxy to capture the salience of income inequality at the individual level. The survey item prompts respondents to think about the 10% of employees working full-time in Finland who earn more than €6000 per month, and whether they consider these incomes unfairly low, fair, or unfairly high. Possible responses range from one to nine, with high values reflecting perceptions that top 10% incomes are unfairly high.

Our measure of household income is based on respondents' self-placement into national income deciles, which are pre-determined for each ESS round and calculated using income data from the Finnish tax registry. In the ESS 2002-6, EU-wide income categories were used instead of national income deciles. For these years, we impute national income deciles by assigning each respondent a household income that is drawn from a uniform random distribution of values between the lower and upper cut-off values of the EU-wide income bracket that they placed themselves in. For outcomes that are available for several survey years (support for redistribution), we have sufficient observations to disaggregate the analysis by income deciles. For outcomes that are only available for one survey year (unfairness perceptions), we have relatively small sample sizes (see Table 1), so we only distinguish

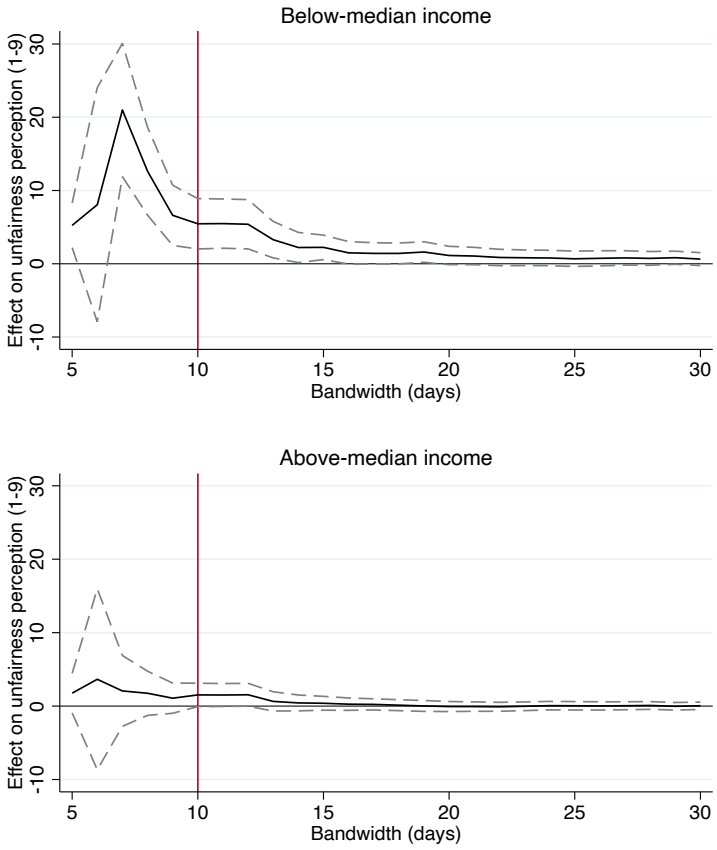
between respondents with below- and above-median household income.

5 Results

We find that the tax day increases perceptions that the incomes of the top 10% are unfairly high. Figure 3 plots the estimated effect of the tax day on unfairness perceptions separately for below- and above-median income earners and for different bandwidths of up to 30 days around the tax day. The red vertical line marks the default 10-day bandwidth. For below-median income earners, we observe a significant positive effect for all bandwidths up to 21 days. For above-median income earners, we only observe a significant positive effect for bandwidths between 10 and 13 days. Table A3 in the appendix presents the corresponding regression results from our baseline model with a 10-day bandwidth. This suggests that the effect of the tax day on unfairness perceptions is driven by the reactions of below-median income earners, for whom the effect is around 3.5 times larger than for above-median income earners. For the whole sample (including both income groups), we observe a positive and statistically significant effect of 2.5 (see Table A5 in the appendix), which amounts to an increase in unfairness perceptions of more than 1.5 standard deviations.

Despite these initial effects, we find that the tax day leaves individuals' support for redistribution largely unaffected. For the whole sample (including all income groups), the estimated effect of the tax day on support for redistribution is statistically insignificant (see Table A5 in the appendix). The overall null effect is precisely estimated. We can rule out increases in overall support for redistribution that are larger than 0.2 standard deviations and decreases that are larger than -0.16 standard deviations – which are small effect sizes. Given that support for redistribution was measured in every ESS round since 2002, we can disaggregate the analysis by income deciles. Figure 4 plots the estimated effect of the tax day on support for redistribution for each income decile, using the default 10-day bandwidth. The effect of

Figure 3: Effect of the tax day on unfairness perception (varying bandwidths)



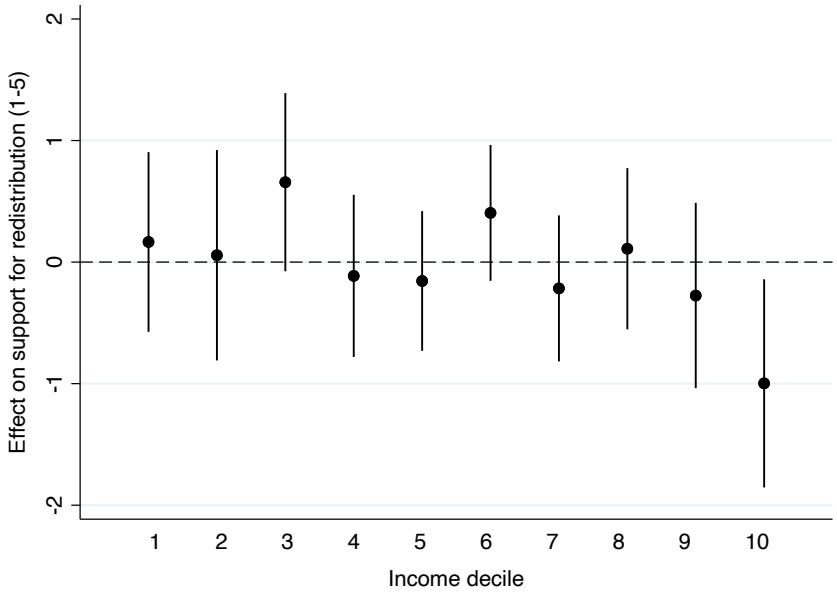
Data: ESS Finland 2018. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' unfairness perception for varying bandwidths (days) around the tax day. Unfairness perception ranges from 1 to 9, with high values reflecting perceptions that top 10% incomes are unfairly high. The results are presented separately for below-median income earners and above-median income earners. The red vertical line marks the default bandwidth of 10 days around the tax day.

the tax day is indistinguishable from zero for all income groups except for the top income decile ($n = 314$), where it is negative ($\beta =$ -1) and statistically significant ($p < 0.05$). The point estimate suggests that amongst the top income decile, the tax day decreases support for redistribution by around one standard deviation, which is comparable to the difference in average support for redistribution between the most left-wing and most right-wing respondents (on a 10-point self-placed ideology scale). Taken together, the results suggest that the tax day leaves support for redistribution largely unaffected, except amongst Finland's top

earners, where the event triggers a relatively strong negative response.

The result for the top income decile withstands several robustness checks. First, the result holds when we include day-of-the-week fixed effects (see Figure A18). Second, the result is relatively robust to alternative bandwidth choices. We find significant negative effects for all bandwidths between 7 and 13 days around the tax day (see Figure A19). Third, the result holds when using alternative exclusion windows from 1 to 5 days prior to the tax day (rather than the default 3 days) although the estimates are in some cases only significant at 90% (see Figure A20).

Figure 4: Effect of the tax day on support for redistribution by income decile

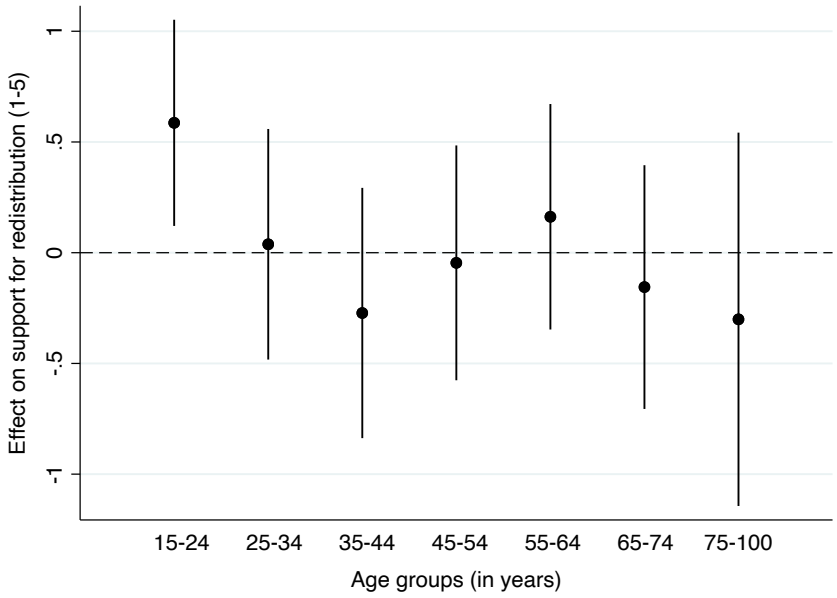


Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' support for redistribution by income decile. Vertical lines represent 95% confidence intervals. Estimates are from our baseline model with 10-day bandwidths, fitted separately on each income decile. The three days prior to the tax day are excluded from the analysis. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly). Corresponding regression results are in Table A7.

To further investigate possible heterogeneous effects, we also disaggregate the analysis by respondents' educational status (Figure A14), level of political interest (Figure A15), political

ideology (Figure A17), and partisanship (Figure A16). We find no evidence of heterogeneous effects in these sub-groups. However, we find that the tax day has different effects depending on respondents' age. Figure 5 plots the estimated effect of the tax day on support for redistribution for different age groups, using the default 10-day bandwidth specification. For most age groups, the estimates are indistinguishable from zero. A notable exception, however, are the youngest age group (15-24 years), where the tax day triggers an increase in support for redistribution of more than 0.5 standard deviations ($\beta = 0.58$; $p = 0.01$). This effect amounts to roughly half the difference in average support for redistribution between the most left-wing and most right-wing respondents.

Figure 5: Effect of the tax day on support for redistribution by age group



Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' support for redistribution by age group, using 9-year bins. Vertical lines represent 95% confidence intervals. Estimates are from our baseline model with 10-day bandwidths, fitted separately on each age group. The three days prior to the tax day are excluded from the analysis. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly). Corresponding regression results are in Table A9.

The result for the youngest age group withstands several robustness checks. First, the

result holds when we include day-of-the-week fixed effects (see Figure A21). Second, the result is robust to alternative bandwidth choices. We find significant positive effects for all bandwidths of up to 15 days around the tax day (see Figure A22). Third, we consistently find positive effects for the youngest age group when using alternative exclusion windows from 1 to 5 days prior to the tax day. We note, however, that the estimates for exclusion windows of 4 and 5 days do not reach conventional significance (see Figure A23). Fourth, we find the same pattern (with noisier estimates) when controlling for household income (see Figure A24), which suggests that age matters independently of income. Finally, we run the analysis using smaller 4-year bins to determine age groups (see Figure A25). The estimates are noisier due to the smaller sample sizes, but they point in the same direction as the main results. The tax day triggers a positive and statistically significant ($\beta = 0.74$; $p = 0.01$) increase in support for redistribution amongst the youngest age group (15-19 years). The estimated effect for the 20-24 year-old's is also positive, but smaller and non-significant ($\beta = 0.28$; $p > 0.05$), which suggests that the reaction is concentrated amongst the very youngest respondents.

6 Mechanisms

In the supplementary material we explore potential mechanisms behind the overall null effect as well as the heterogeneous effects by income and age. First, we investigate why the tax day leaves public support for redistribution largely unaffected. We provide evidence which suggests that the overall null effect on support for redistribution is unlikely due to ceiling effects in the outcome variable or the repeated nature of the tax day, which could result in a "weak treatment" as well as information saturation in the population (see Appendix A.1). A related concern is that the overall null result may reflect a change in respondents' redistribution attitudes occurring in the period leading up to tax day as they anticipate

the event. Results from a placebo treatment analysis (Figure A36) as well as descriptive evidence from a Regression Discontinuity type plot (Figure A2) suggest that there are no such anticipatory effects. The coefficient for the regression slope below the cutoff, which is indistinguishable from zero for the main outcome, also suggests no anticipatory effects (Table A5).

Taken together, this evidence reassures us that the muted effect of the tax day on support for redistribution is not due to the specific nature of the event or the study design. Instead, there are several possible explanations for why the redistributive preferences of most Finnish citizens remain unaffected by the tax day, even when their concerns about income inequality increase. In a seminal study, Kuziemko et al. (2015) find similar null effects on redistributive preferences when exposing US citizens to inequality information in a survey experiment. Kuziemko et al. (2015) argue that distrust in government could explain why preferences for redistribution are so inelastic. In the US, politicians are simply not trusted to be able to address inequality with public policies. In Finland, we think this explanation carries less weight, given that Finns trust their government more than citizens of most other industrialised democracies (OECD 2022). In-group biases have also been proposed as an explanation for the public's limited support for redistribution in the face of rising inequality (Shayo 2009, Dahlberg et al. 2012, Alesina et al. 2019). However, in Finland, relatively low levels of immigration and high levels of ethnic and religious homogeneity make this a less plausible explanation for why redistributive preferences are so inelastic (Matakos et al. 2020).

A more plausible explanation in our view is that citizens have trouble connecting their concerns about inequality (which are triggered by the tax day) with concrete measures that could ameliorate inequality such as, for example, redistributive policies. In the US context, Kuziemko et al. (2015) find support for this mechanism. They show that exposing subjects to inequality information together with concrete examples of redistributive policies can increase

support for redistribution. We think this mechanism may also apply in the Finnish context, where the public discussion around the tax day primarily focuses on the problem (inequality) rather than possible solutions to the problem (Barry 2018). Finally, we think that political socialisation plays a role in explaining why preferences for redistribution are inelastic. Alesina & Fuchs-Schuendeln (2007) show that the political environment in which people live (in their case East German Communism) instils in them certain preferences for redistribution and that these preferences do not change quickly, even with major changes in the environment (in their case the collapse of Communism). This argument seems plausible in our case and is supported by our finding that only the youngest age group respond to the tax day by increasing their support for redistribution. This suggests that preferences for redistribution are rooted in relatively stable political ideologies that are most "malleable" in early adulthood.

Next, we examine potential explanations for why the tax day reduces support for redistribution amongst respondents in the top income decile (see Appendix A.2). We provide evidence which indicates that this effect may operate via a process of motivated reasoning, whereby top earners - in response to the tax day - re-affirm their belief that income inequality in Finland is justified, for example because high incomes are rewards for effort or talents. This explanation also finds support in recent sociological research on Finland's top earners, who often have to justify their privileged position in the face of strong egalitarian norms (Kantola & Kuusela 2019, Kantola 2020). Other explanations, focused on the potential effect of the tax day on top earners' misperception of their relative income status, their tax burden or status concerns, find less support in the data.

Finally, we investigate why respondents in the youngest age group (15-24 years) increase their support for redistribution in response to the tax day (see Appendix A.3). We provide evidence which suggests that the reaction amongst the youngest is driven by the tax day's effect on misperceptions of relative income status, which we show is stronger amongst the youngest age group compared to older age groups. The information effect of the tax day

might be stronger for the youngest age group, either because they are less likely to have been exposed to information from previous tax days, or because they are less likely to interpret the information through the lens of a consolidated political worldview (see Dinas 2013). To disentangle these two mechanisms, we further split the sample into respondents who report being interested in politics and those not interested. We find that the positive effect of the tax day is concentrated amongst those young people with low interest in politics - who are less likely to have been "pre-treated" - which suggests that lack of previous exposure may explain some of the age group results (Figure A4). A leftward shift in redistributive preferences is in turn plausible because young people are disproportionately represented at the lower end of the income distribution, where individuals tend to overestimate their income status (Hvidberg et al. 2020). An alternative explanation focused on differences in media diets between younger and older age groups finds less support in the data.

7 Discussion

Every year, on the first working day of November, Finland's tax authorities release the income information of everyone who earns more than €100,000 a year to the public. We propose that the so-called tax day increases the salience of income inequality in the public debate and expect it to widen the gap in support for redistribution between the rich and the poor. We use media data to show that the tax day coincides with a significant spike in media coverage related to income inequality. Using nationally representative survey data and a before-and-after type research design, we also show that the tax day increases perceptions that the incomes of the top 10% are unfairly high. However, despite these initial effects, we find that the tax day leaves public support for redistribution largely unaffected. We can rule out increases in support for redistribution that are larger than 0.2 standard deviations and decreases that are larger than -0.16 standard deviations. We explore possible explanations for

the public's muted response to the tax day, and present evidence that the overall null effect on support for redistribution is unlikely due to the repeated nature of the tax day, ceiling effects, or anticipatory effects. Importantly, we show that the overall null effect hides substantial heterogeneity between income and age groups. We find that the tax day suppresses support for redistribution amongst individuals in the top income decile, and we present evidence that this effect likely operates via changes in top earners' fairness beliefs. Furthermore, we find that the tax day increases support for redistribution amongst the youngest age group (15-24 years), and we provide evidence that this effect likely operates through changes in young people's perceived relative income status.

Regarding the external validity of our findings, we note that Finland is one of the most equal societies in Europe (OECD 2011) and support for redistribution is relatively high in comparison to other countries in Europe.[24] Although we present evidence that our results are unlikely due to ceiling effects, it is possible that the impact of income transparency is more pronounced in other contexts, where baseline support for redistribution is lower and income differences are larger. Furthermore, although several countries have adopted income transparency policies (see e.g. Bø et al. 2015, Slemrod et al. 2022), Finland's tax day is unique in that it creates an annual media spectacle focused on the nation's top earners (Barry 2018). In Norway, for example, the availability of tailored search apps that were integrated into social media platforms such as *Facebook* meant that Norwegians primarily used income transparency to compare their own income with that of their neighbours, friends, and co-workers, rather than the nation's top earners (Perez-Truglia 2020, Reck et al. 2022). While it is difficult to rule out that the muted response to Finland's tax day is due to the specific nature of the event (rather than the nature of redistributive preferences), we provide evidence that the tax day has similar effects on citizens' subjective well-being as a comparable income transparency intervention in Norway (Perez-Truglia 2020). Furthermore, Slemrod et al. (2022) find that the annual release of income tax information in Pakistan has

[24] See footnote 6

positive effects on citizens' tax compliance several years after the initial release, which implies that repeated events like the Finnish tax day can continue to affect individual-level outcomes even when their novelty effect may have subsided. Taken together, this reassures us that the muted effect of income transparency on support for redistribution in Finland is not solely determined by the specific institutional context of the tax day.

A limitation of our research design is that we primarily focus on respondents who were interviewed within 10 days of the tax day. We do this because the assumption of as-if random exposure to the tax day is most plausible in such a narrow window. This approach comes at a cost in that we can only assess the short-term effects of the tax day. However, we note that this limitation is not unique to our study and pertains to most previous experimental studies that manipulate subjects' support for redistribution by exposing them to inequality (Cruces et al. 2013, Karadja et al. 2017, Sands 2017, Sands & de Kadt 2020, Condon & Wichowsky 2020).[25] Another limitation of our research design is that we only have data from 2002 onward, which is two years after the tax authorities first started releasing the income information of Finland's top earners to the media. We disaggregate our analysis by survey round and find no evidence that the effect of the tax day on support for redistribution was more pronounced in early rounds. However, we cannot rule out entirely that the tax day had a significant effect on citizens' redistributive preferences when it first took place in 2000, and that the null effects we observe from 2002 onward are due to information saturation.

Overall, our findings suggest that income transparency can trigger greater concern about income inequality amongst ordinary citizens, but that it may only lead to marginal changes in public support for redistribution. This interpretation corresponds well with the study by Kuziemko et al. (2015), who show that randomly exposing survey respondents to information on income inequality has large effects on their views about inequality, but only slightly moves their support for redistributive policies. Our results contrast, however, with several recent

[25] An exception are Kuziemko et al. (2015), who re-survey respondents after one month and find that 58% of the initial effect size remains (p.1493).

survey- and field experiments (Condon & Wichowsky 2020, Sands 2017, Sands & de Kadt 2020), which find that subjects' support for redistribution can be shifted by exposing them to inequality. We contribute to this experimental research, by studying the effect of a real-world policy on support for redistribution in the context of a large-scale quasi-experiment. While the debate on the "malleability" of redistributive preferences is far from settled, our results indicate that triggering significant shifts in support for redistribution may be more difficult to achieve outside the controlled experimental setting.

An important implication of our findings is that lack of cross-class exposure may not be the main reason why public support for redistribution has failed to "keep up" with rising inequality in Western democracies. Scholars and political commentators, especially in the US context, frequently point to limited cross-class exposure due to residential or educational segregation as an explanation for the disconnect between rising inequality and lack of support for redistribution (e.g. Reardon & Bischoff 2011, Minkoff & Lyons 2019, Condon & Wichowsky 2020). In Finland, income transparency is also frequently justified as a mechanism to encourage cross-class comparisons and resist the trend towards growing inequality (Barry 2018, Yläjärvi 2020). Our findings cast some doubts on the idea that increasing cross-class exposure is all it takes for redistributive demand to catch up with rising inequality. Of course, income transparency is just one of many policy interventions that might increase cross-class exposure. For example, residential integration programmes like the *Moving to Opportunity* initiative (see Chetty et al. 2016, 2014) are likely to expose participants to visible markers of inequality. Exposure to inequality in local neighbourhoods may in turn have much more far-reaching consequences for redistributive demand than the relatively abstract exposure to the super-rich triggered by Finland's tax day (see Sands & de Kadt 2020). Exploring the attitudinal effects of other policies that might increase citizens' exposure to inequality is a promising avenue for further research.

It is beyond the scope of this paper to explore in detail *why* citizens' redistributive preferences

are relatively inelastic to increasing exposure to inequality. However, we think that one plausible explanation is that people simply fail to connect their concerns about inequality (which are triggered by the tax day) with concrete measures that would help to tackle inequality, such as redistributive policies. Another explanation is that citizens' redistributive preferences are rooted in stable political worldviews that are difficult to alter once they are crystallized during the "impressionable years" of adolescence and early adulthood (Neundorf & Smets 2017). Our finding that the youngest age group are more responsive to the tax day than older age groups supports this view. Our data do not allow us to establish precisely why young people attach more weight to the income information revealed by the tax day, but one plausible channel is that individuals in this age group are less likely than older age groups to interpret the information through the lens of a consolidated political worldview (see Dinas 2013). Another channel, for which we find indicative evidence, is that young people are less likely to have been previously exposed to the information revealed during the tax day. Further research is needed to unpack the precise mechanisms behind young people's heightened sensitivity to attitudinal change.

References

Alesina, A. & Angeletos, G.-M. (2005), 'Fairness and Redistribution', *American Economic Review* **95**(4), 960–980.
URL: *https://pubs.aeaweb.org/doi/10.1257/0002828054825655*

Alesina, A., Baqir, R. & Easterly, W. (1999), 'Public Goods and Ethnic Divisions', *The Quarterly Journal of Economics* **114**(4), 1243–1284.
URL: *https://doi.org/10.1162/003355399556269*

Alesina, A. & Fuchs-Schuendeln, N. (2007), 'Good-Bye Lenin (or Not?): The Effect of Communism on People's Preferences', *American Economic Review* **97**(4).

Alesina, A., Glaeser, E. L. E. L. & Sacerdote, B. (2001), 'Why Doesn't the United States Have a European-Style Welfare State?', *Brookings Papers on Economic Activity* **2001**(2), 187–277.
URL: *http://muse.jhu.edu/content/crossref/journals/brookings$_p$apers$_o$n$_e$conomic$_a$ctivity/v*2001/2001.2

Alesina, A., Murard, E. & Rapoport, H. (2019), Immigration and Preferences for Redistribution in Europe, Technical Report w25562, National Bureau of Economic Research, Cambridge, MA.
URL: *http://www.nber.org/papers/w25562.pdf*

Alesina, A., Stantcheva, S. & Teso, E. (2018), 'Intergenerational Mobility and Preferences for Redistribution', *American Economic Review* **108**(2), 521–554.
URL: *https://pubs.aeaweb.org/doi/10.1257/aer.20162015*

Alt, J. & Iversen, T. (2017), 'Inequality, Labor Market Segmentation, and Preferences for Redistribution', *American Journal of Political Science* **61**(1), 21–36.
URL: *http://doi.wiley.com/10.1111/ajps.12264*

Ashok, V., Kuziemko, I. & Washington, E. (2015), '2015 Ashok et al - Support for redistribution in an age of increasing inequality.pdf', *Brookings Papers on Economic Activity* **Spring 2015**.

Baker, M., Halberstam, Y., Kroft, K., Mas, A. & Messacar, D. (2019), Pay Transparency and the Gender Gap, Technical Report w25834, National Bureau of Economic Research, Cambridge, MA.
URL: *http://www.nber.org/papers/w25834.pdf*

Barry, E. (2018), 'Happy 'National Jealousy Day'! Finland Bares Its Citizens' Taxes (Published 2018)', *The New York Times* .
URL: *https://www.nytimes.com/2018/11/01/world/europe/finland-national-jealousy-day.html*

Blanes-i Vidal, J. & Nossol, M. (2011), 'Tournaments Without Prizes: Evidence from Personnel Records', *Management Science* **57**(10), 17.

Bredemeier, C. (2014), 'Imperfect information and the Meltzer-Richard hypothesis', *Public Choice* **159**(3), 17.

Brown-Iannuzzi, J. L., Lundberg, K. B., Kay, A. C. & Payne, B. K. (2015), 'Subjective Status Shapes Political Preferences', *Psychological Science* **26**(1), 15–26.
URL: *http://journals.sagepub.com/doi/10.1177/0956797614553947*

Busemeyer, M. R., Abrassart, A. & Nezi, R. (2021), 'Beyond Positive and Negative: New

Perspectives on Feedback Effects in Public Opinion on the Welfare State', *British Journal of Political Science* **51**(1), 137–162.
URL: *https://www.cambridge.org/core/product/identifier/S0007123418000534/type/journal*$_a$*rticle*

Bø, E. E., Slemrod, J. & Thoresen, T. O. (2015), 'Taxes on the Internet: Deterrence Effects of Public Disclosure', *American Economic Journal: Economic Policy* **7**(1), 36–62.
URL: *https://pubs.aeaweb.org/doi/10.1257/pol.20130330*

Card, D., Mas, A., Moretti, E. & Saez, E. (2012), 'Inequality at Work: The Effect of Peer Salaries on Job Satisfaction', *American Economic Review* **102**(6), 2981–3003.
URL: *https://pubs.aeaweb.org/doi/10.1257/aer.102.6.2981*

Cattaneo, M. D., Jansson, M. & Ma, X. (2018), 'Manipulation Testing Based on Density Discontinuity', *The Stata Journal* **18**(1), 234–261. Publisher: SAGE Publications.
URL: *https://doi.org/10.1177/1536867X1801800115*

Cavaille, C. (2020), *Fair Enough? Support for Redistribution in the Age of Inequality.*

Cavaillé, C. & Trump, K.-S. (2015), 'The Two Facets of Social Policy Preferences', *The Journal of Politics* **77**(1), 146–160.
URL: *https://www.journals.uchicago.edu/doi/10.1086/678312*

Chetty, R., Hendren, N. & Katz, L. F. (2016), 'The Effects of Exposure to Better Neighborhoods on Children: New Evidence from the Moving to Opportunity Experiment', *American Economic Review* **106**(4), 855–902.
URL: *https://pubs.aeaweb.org/doi/10.1257/aer.20150572*

Chetty, R., Hendren, N., Kline, P. & Saez, E. (2014), 'Where is the land of Opportunity? The Geography of Intergenerational Mobility in the United States', *The Quarterly Journal of Economics* **129**(4), 1553–1623.
URL: *https://academic.oup.com/qje/article/129/4/1553/1853754*

Condon, M. & Wichowsky, A. (2020), 'Inequality in the Social Mind: Social Comparison and Support for Redistribution', *The Journal of Politics* **82**(1), 149–161.
URL: *https://www.journals.uchicago.edu/doi/10.1086/705686*

Cooney, S. (2018), 'Should You Share Your Salary With Co-Workers? Here's What Experts Say'.
URL: *https://time.com/5353848/salary-pay-transparency-work/*

Cruces, G., Perez-Truglia, R. & Tetaz, M. (2013), 'Biased perceptions of income distribution and preferences for redistribution: Evidence from a survey experiment', *Journal of Public Economics* **98**, 100–112.
URL: *https://linkinghub.elsevier.com/retrieve/pii/S004727271200117X*

Cullen, Z. & Perez-Truglia, R. (2018), 'How much does your boss make?', *NBER Working Paper No. 24841* .
URL: *http://www.nber.org/papers/w24841*

Côté, S., House, J. & Willer, R. (2015), 'High economic inequality leads higher-income individuals to be less generous', *Proceedings of the National Academy of Sciences* **112**(52), 15838–15843.
URL: *http://www.pnas.org/lookup/doi/10.1073/pnas.1511536112*

Dahlberg, M., Edmark, K. & Lundqvist, H. (2012), 'Ethnic Diversity and Preferences for Redistribution', *Journal of Political Economy* **120**(1), 41–76. Publisher: The University

of Chicago Press.
URL: *https://www.journals.uchicago.edu/doi/abs/10.1086/665800*

Depetris-Chauvin, E., Durante, R. & Campante, F. (2020), 'Building Nations through Shared Experiences: Evidence from African Football', *American Economic Review* **110**(5), 1572–1602.
URL: *https://pubs.aeaweb.org/doi/10.1257/aer.20180805*

Di Tella, R., MacCulloch, R. J. & Oswald, A. J. (2003), 'The Macroeconomics of Happiness', *The Review of Economics and Statistics* **85**, 20.

Dietze, P. & Craig, M. A. (2020), 'Framing economic inequality and policy as group disadvantages (versus group advantages) spurs support for action', *Nature Human Behaviour* .
URL: *http://www.nature.com/articles/s41562-020-00988-4*

Dinas, E. (2013), 'Opening "Openness to Change": Political Events and the Increased Sensitivity of Young Adults', *Political Research Quarterly* **66**(4), 868–882.
URL: *http://journals.sagepub.com/doi/10.1177/1065912913475874*

Fehr, D., Mollerstrom, J. & Perez-Truglia, R. (2019), 'Your Place in the World: The Demand for National and Global Redistribution', *NBER Working Paper No. 26555* p. 40.
URL: *http://www.nber.org/papers/w26555*

Fenton, G. (2020), 'How Elastic are Preferences for Redistribution? New Results on Partisan Polarization', *SSRN Electronic Journal* .
URL: *https://www.ssrn.com/abstract=3538441*

Fernández-Albertos, J. & Kuo, A. (2018), 'Income Perception, Information, and Progressive Taxation: Evidence from a Survey Experiment', *Political Science Research and Methods* **6**(1), 83–110.
URL: *https://www.cambridge.org/core/product/identifier/S2049847015000734/type/journal*$_a$*rticle*

Ferrer-i Carbonell, A. (2005), 'Income and well-being: an empirical analysis of the comparison income effect', *Journal of Public Economics* p. 23.

Finseraas, H. & Listhaug, O. (2013), 'It can happen here: the impact of the Mumbai terror attacks on public opinion in Western Europe', *Public Choice* **156**(1-2), 213–228.
URL: *http://link.springer.com/10.1007/s11127-011-9895-7*

Giani, M. & Méon, P.-G. (2019), 'Global Racist Contagion Following Donald Trump's Election', *British Journal of Political Science* pp. 1–8.
URL: *https://www.cambridge.org/core/product/identifier/S0007123419000449/type/journal*$_a$*rticle*

Hasegawa, M., Hoopes, J. L., Ishida, R. & Slemrod, J. B. (2012), 'The Effect of Public Disclosure on Reported Taxable Income: Evidence from Individuals and Corporations in Japan', *SSRN Electronic Journal* .
URL: *http://www.ssrn.com/abstract=1653948*

Hvidberg, K., Kreiner, C. & Stantcheva, S. (2020), Social Position and Fairness Views.
URL: *http://www.nber.org/papers/w28099.pdf*

Kantola, A. (2020), 'Gloomy at the Top: How the Wealthiest 0.1% Feel about the Rest', *Sociology* **54**(5), 904–919.
URL: *http://journals.sagepub.com/doi/10.1177/0038038520910344*

Kantola, A. & Kuusela, H. (2019), 'Wealth Elite Moralities: Wealthy Entrepreneurs' Moral Boundaries', *Sociology* **53**(2), 368–384.

URL: *http://journals.sagepub.com/doi/10.1177/0038038518768175*

Karadja, M., Mollerstrom, J. & Seim, D. (2017), 'Richer (and Holier) Than Thou? The Effect of Relative Income Improvements on Demand for Redistribution', *The Review of Economics and Statistics* **99**(2), 201–212.

Korpi, S. (2021), 'Varakkaiden tienesti- ja verotiedot mediassa: Yhteinen asia vai yksityisasia?'.

Kuziemko, I., Norton, M. I., Saez, E. & Stantcheva, S. (2015), 'How Elastic Are Preferences for Redistribution? Evidence from Randomized Survey Experiments', *American Economic Review* **105**(4), 1478–1508.
URL: *https://pubs.aeaweb.org/doi/10.1257/aer.20130360*

Legewie, J. (2013), 'Terrorist Events and Attitudes toward', **118**(5), 1199–1245.

Lohiniva-Kerkelä, M. (2003), *Verosalaisuus*, Edita Publishing Oy.
URL: *https://www.booky.fi/tuote/lohiniva$_k$erkela/verosalaisuus/*9789513739287

Londoño-Vélez, J. (2022), 'The impact of diversity on perceptions of income distribution and preferences for redistribution', *Journal of Public Economics* **214**, 104732.
URL: *https://linkinghub.elsevier.com/retrieve/pii/S0047272722001347*

Mas, A. (2017), 'Does Transparency Lead to Pay Compression?', *Journal of Political Economy* **125**(5), 39.

Matakos, K., Savolainen, R. & Tukiainen, J. (2020), 'Refugee Migration and the Politics of Redistribution: Do Supply and Demand Meet?', *SSRN Electronic Journal* .
URL: *https://www.ssrn.com/abstract=3544184*

Matikainen, J. (2020), 'Media ja yleisön luottamuksen ulottuvuudet : instituutiot, journalismi ja mediasuhde'.
URL: *https://helda.helsinki.fi/handle/10138/319153*

McCall, L., Burk, D., Laperrière, M. & Richeson, J. A. (2017), 'Exposure to rising inequality shapes Americans' opportunity beliefs and policy support', *Proceedings of the National Academy of Sciences* **114**(36), 9593–9598.
URL: *http://www.pnas.org/lookup/doi/10.1073/pnas.1706253114*

Meltzer, A. H. & Richard, S. F. (1981), 'A Rational Theory of the Size of Government', *Journal of Political Economy* **89**(5), 914–927.
URL: *https://www.journals.uchicago.edu/doi/10.1086/261013*

Mikulaschek, C., Pant, S. & Tesfaye, B. (2020), 'Winning Hearts and Minds in Civil Wars: Governance, Leadership Change, and Support for Violent Groups in Iraq', *American Journal of Political Science* **64**(4), 773–790.
URL: *https://onlinelibrary.wiley.com/doi/10.1111/ajps.12527*

Minkoff, S. L. & Lyons, J. (2019), 'Living With Inequality: Neighborhood Income Diversity and Perceptions of the Income Gap', *American Politics Research* **47**(2), 329–361.
URL: *http://journals.sagepub.com/doi/10.1177/1532673X17733799*

Moniz, P. & Wlezien, C. (2020), Issue Salience and Political Decisions, *in* 'Oxford Research Encyclopedia of Politics', Oxford University Press.
URL: *https://oxfordre.com/politics/view/10.1093/acrefore/9780190228637.001.0001/acrefore-9780190228637-e-1361*

Muñoz, J., Falcó-Gimeno, A. & Hernández, E. (2020), 'Unexpected Event during Survey Design: Promise and Pitfalls for Causal Inference', *Political Analysis* **28**(2), 186–206.
URL: *https://www.cambridge.org/core/product/identifier/S1047198719000275/type/journal*$_a$*rticle*

Neundorf, A. & Smets, K. (2017), *Political Socialization and the Making of Citizens*, Vol. 1, Oxford University Press.
URL: *http://oxfordhandbooks.com/view/10.1093/oxfordhb/9780199935307.001.0001/oxfordhb-9780199935307-e-98*

Nishi, A., Shirado, H., Rand, D. G. & Christakis, N. A. (2015), 'Inequality and visibility of wealth in experimental social networks', *Nature* **526**(7573), 426–429.
URL: *http://www.nature.com/articles/nature15392*

Obloj, T. & Zenger, T. (2022), 'The influence of pay transparency on (gender) inequity, inequality and the performance basis of pay', *Nature Human Behaviour* **6**(5), 646–655. Number: 5 Publisher: Nature Publishing Group.
URL: *https://www.nature.com/articles/s41562-022-01288-9*

OECD (2011), Divided We Stand: Why Inequality Keeps Rising, Technical report, OECD, Paris.

OECD (2021), *Does Inequality Matter?*
URL: *https://www.oecd-ilibrary.org/content/publication/3023ed40-en*

OECD (2022), *Building Trust to Reinforce Democracy: Main Findings from the 2021 OECD Survey on Drivers of Trust in Public Institutions*, Organisation for Economic Co-operation and Development, Paris.
URL: *https://www.oecd-ilibrary.org/governance/building-trust-to-reinforce-democracy*$_b$*407f99c − en*

Perez-Truglia, R. (2020), 'The Effects of Income Transparency on Well-Being: Evidence from a Natural Experiment', *American Economic Review* **110**(4), 1019–1054.
URL: *https://pubs.aeaweb.org/doi/10.1257/aer.20160256*

Pesonen, P. & Riihinen, O. (2002), *Dynamic Finland: The Political System and the Welfare State*, Finnish Literature Society / SKS. Accepted: 2018-07-03 19:08:58 ISSN: 0355-8924 Journal Abbreviation: The Political System and the Welfare State.
URL: *https://library.oapen.org/handle/20.500.12657/29744*

Reardon, S. F. & Bischoff, K. (2011), 'Income Inequality and Income Segregation', *American Journal of Sociology* **116**(4), 1092–1153.
URL: *https://www.journals.uchicago.edu/doi/10.1086/657114*

Reck, D., Slemrod, J. & Vattø, T. E. (2022), 'Public disclosure of tax information: Compliance tool or social network?', *Journal of Public Economics* **212**, 104708.
URL: *https://linkinghub.elsevier.com/retrieve/pii/S0047272722001104*

Rehm, P., Hacker, J. S. & Schlesinger, M. (2012), 'Insecure Alliances: Risk, Inequality, and Support for the Welfare State', *American Political Science Review* **106**(2), 386–406.
URL: *https://www.cambridge.org/core/product/identifier/S0003055412000147/type/journal*$_a$*rticle*

Sands, M. L. (2017), 'Exposure to inequality affects support for redistribution', *Proceedings of the National Academy of Sciences* **114**(4), 663–668.
URL: *http://www.pnas.org/lookup/doi/10.1073/pnas.1615010113*

Sands, M. L. & de Kadt, D. (2020), 'Local exposure to inequality raises support of people of

low wealth for taxing the wealthy', *Nature* **586**(7828), 257–261. Number: 7828 Publisher: Nature Publishing Group.
URL: *https://www.nature.com/articles/s41586-020-2763-1*

Shayo, M. (2009), 'A Model of Social Identity with an Application to Political Economy: Nation, Class, and Redistribution', *American Political Science Review* **103**(2), 147–174. Publisher: Cambridge University Press.
URL: *https://www.cambridge.org/core/journals/american-political-science-review/article/abs/model-of-social-identity-with-an-application-to-political-economy-nation-class-and-redistribution/7C3B4760571E6986D32973AF22060FFF*

Slemrod, J., Rehman, O. U. & Waseem, M. (2022), 'How Do Taxpayers Respond to Public Disclosure and Social Recognition Programs? Evidence from Pakistan', *The Review of Economics and Statistics* **104**(1), 116–132.
URL: *https://doi.org/10.1162/rest*$_{a0}$0929

Suhay, E., Klasnja, M. & Rivero, G. (2020), 'Ideology of Affluence: Rich Americans' Explanations for Inequality and Attitudes toward Redistribution', *The Journal of Politics* .
URL: *https://www.journals.uchicago.edu/doi/10.1086/709672*

SVT (2017), 'Vapaa-ajan osallistuminen'.

Thal, A. (2020), 'The Desire for Social Status and Economic Conservatism among Affluent Americans', *American Political Science Review* **114**(2), 426–442.

Trump, K.-S. (2018), 'Income Inequality Influences Perceptions of Legitimate Income Differences', *British Journal of Political Science* **48**(4), 929–952.
URL: *https://www.cambridge.org/core/product/identifier/S0007123416000326/type/journal*$_a$*rticle*

Yläjärvi, E. (2020), 'Pääkirjoitus: Verotietojen avoimuus uhkaa kaventua merkittävästi', *ltalehti* .
URL: *https://www.iltalehti.fi/paakirjoitus/a/684d446b-4d73-4951-acf1-19f2291d895e*

8 Appendix

A Mechanisms

A.1 Why does the tax day leave support for redistribution largely unaffected?

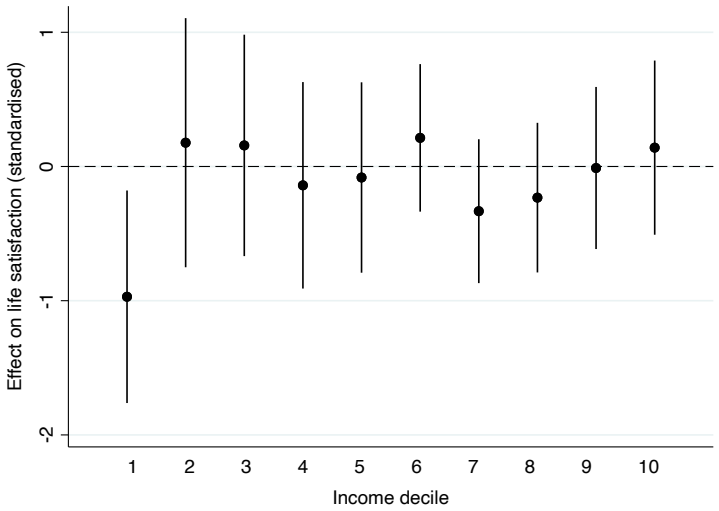
Weak treatment - Given that the tax day takes place every year, it is plausible that the "treatment" of the tax day is simply not strong enough to shift individuals' attitudes in a manner that is comparable to the effect of income transparency interventions in other contexts (Card et al. 2012, Perez-Truglia 2020). As an external validity check, we therefore assess whether the tax day affects individuals' subjective well-being. We look at subjective well-being given that this outcome has been previously studied in the very similar context of Norway, where citizens' tax records became easily accessible online in 2001 (Perez-Truglia 2020). We measure subjective well-being using responses to the following question, which was included in all ESS rounds since 2002: "All things considered, how satisfied are you with your life as a whole nowadays?" Possible responses range from one (extremely dissatisfied) to 11 (extremely satisfied). Following Perez-Truglia (2020), we use the Probit-OLS method to assign values to each response option and then standardise the variable to a mean of 0 and a standard deviation of 1.[26] While self-reported measures of well-being have some limitations (e.g. due to social desirability bias), they have been shown to be significantly correlated with objective measures of well-being (Di Tella et al. 2003).

Based on previous findings from Norway (Perez-Truglia 2020), we expect the tax day to increase the subjective well-being of the affluent and decrease the subjective well-being of the less affluent. We find some evidence for this, although reactions to the tax day appear to be concentrated at the bottom of the income distribution. Figure A1 plots the estimated effect of the tax day on self-reported life satisfaction by income decile, using the default 10-day bandwidth specification. For most income deciles, the tax day leaves self-reported life satisfaction unaffected. However, for the bottom income decile, we observe a sizeable and statistically significant decrease in life satisfaction in response to the tax day (-0.9 SD; $p = 0.02$). In the appendix, we present evidence to suggest that a plausible mechanisms linking the tax day to decreased life satisfaction amongst the poorest is via its effect on perceived income status (see Figure A27). We interpret this as evidence that the tax day "treatment" is strong enough to shift attitudes in a manner that is similar to the effect of income transparency interventions studied in other contexts (Card et al. 2012, Perez-Truglia 2020).[27]

[26] The Probit-OLS method assigns values to match the distribution of responses to a normal distribution (Ferrer-i Carbonell 2005). The standardised Probit-OLS adjusted measure has 11 discrete values and runs from -3.2 to 1.8. See Figure A12 for a histogram of the re-scaled life satisfaction variable.

[27] Note that our estimates are nor directly comparable, as these studies do not disaggregate their results by income decile. In Norway, Perez-Truglia (2020) finds that income transparency increased the life satisfaction–income gradient by 21%. In California, Card et al. (2012) find that income transparency decreased self-reported job satisfaction by 0.22 standard deviations amongst respondents in the lowest income quartile.

Figure A1: Effect of the tax day on life satisfaction by income decile



Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' life satisfaction by income decile. Vertical lines represent 95% confidence intervals. Estimates are from our baseline model with 10-day bandwidths, fitted separately on each income decile. The three days prior to the tax day are excluded from the analysis. Life satisfaction is measured with the following question: "All things considered, how satisfied are you with your life as a whole nowadays?" Response options range from 1 (extremely dissatisfied) to 11 (extremely satisfied). The values are adjusted using the Probit-OLS method and standardised to have a mean of 0 and a standard deviation of 1.

Findings from a similar transparency policy in Pakistan also reassure us that the repeated nature of the tax day does not necessarily imply a "weak" treatment effect. In 2012, the Pakistani government started publishing a tax directory every year, which reveals the name, tax identifier, and tax liability of every citizen in the country. The directory is posted online annually and can be accessed freely by anyone. In a recent study, Slemrod et al. (2022) show that the income tax release in Pakistan increased tax compliance amongst citizens even several years after the initial release of information in 2012. We interpret this as indicative evidence that a repeated transparency event such as the Finnish tax day can affect individual-level outcomes even when the novelty effect of the initial information release may have worn off.

To our knowledge, there are no previous studies on the Finnish tax day and how it might affect behavioural or attitudinal outcomes in the population. However, descriptive evidence from a Finnish media analysis lends additional support to our argument that the tax day is a highly salient event, to which most Finnish citizens are likely exposed in one way or another. Korpi (2021) shows that, related to the 2020 tax day alone, the public broadcasting company *YLE* published 12 online articles on the topic, on top of its extensive TV coverage. The biggest newspaper *Helsingin Sanomat* also published 14 articles and the major evening newspaper *Iltalehti* 43 articles related to the tax day. Given that 77% of the Finnish popula-

tion over 10 years report following newspapers regularly (SVT 2017) and given that trust in media is high (Matikainen 2020), we would argue that this flood of tax day reporting in the media makes it unlikely that our null results appear because the tax day is not sufficiently prominent or noticeable.

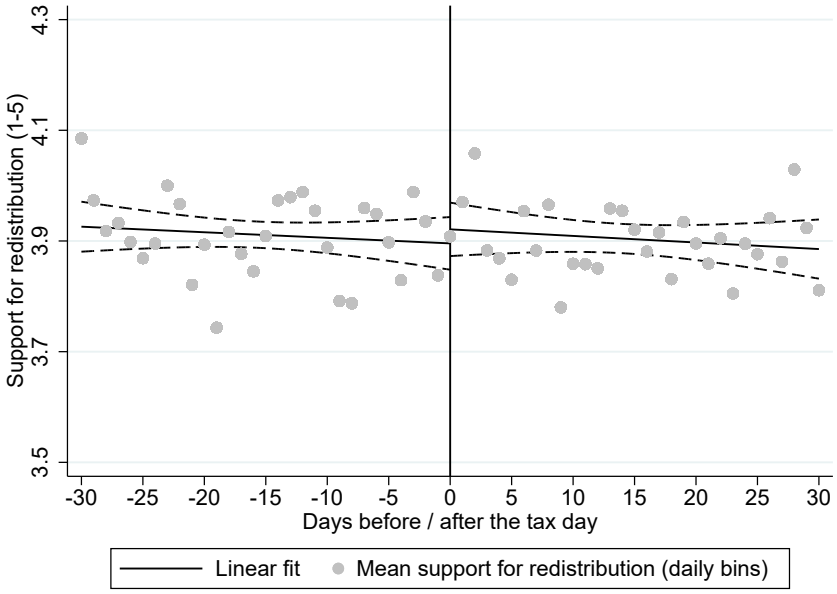
Information saturation - A related explanation for the overall null effect is that, because the tax day has taken place every year since 2000, most citizens' do not obtain any new information from the event. If this is the case, we would expect to not observe null effects in early rounds of the ESS, where the tax day arguably still revealed more new information to Finnish citizens. Figure A26 plots the effect of the tax day on support for redistribution separately for each survey round between 2002 and 2018. To avoid small sample sizes, we only distinguish between below- and above-median income earners. We find no evidence that the tax day triggers stronger attitudinal reactions in early rounds of the ESS. While this evidence is only suggestive, we interpret it to mean that information saturation is unlikely to be the main reason why we observe an overall null effect of the tax day on support for redistribution.

Ceiling effects - Another plausible explanation for the overall null effect is that support for redistribution in Finland is quite high to begin with (Figure A11). To address concerns about ceiling effects, we turn to four alternative measures of support for redistribution, where responses are less clustered at the higher end of the scale (Figure A13). As for our main outcome, we find that the tax day leaves these alternative measures of support for redistribution largely unaffected (Table A11). This suggests that our main null result is unlikely to be driven by ceiling effects.

Anticipatory effects - A key concern is that, as tax day is approaching, citizens may have already changed their views regarding redistribution as they consider the upcoming event. If this is the case, the overall null results may reflect an earlier change in redistribution attitudes that occurred in the period leading up to tax day. To address concerns about anticipatory effects, we conduct a placebo treatment analysis, where we impose a "fake" tax days up to 50 days before and after the actual tax day. The consistent null effects prior to the actual tax day indicate that there are no anticipatory effects (Figure A36). A descriptive Regression Discontinuity-style plot of mean support for redistribution amongst respondents interviewed before and after the tax day also suggests no anticipatory effects (Figure A2). Finally, the coefficient for the regression slope below the cutoff, which is indistinguishable from zero for the main outcome ($\beta = 0.004$, se $= 0.014$), also suggests no anticipatory effects on support for redistribution (Table A5).

Partisanship - It is also possible that the overall effect of the tax day on support for redistribution is limited because many citizens identify with a specific political party and follow the cues of their preferred political party on the issue of redistribution (Cavaille 2020). If this is the case, we would expect to observe null effects amongst citizens with partisan attachments, but to observe changes in support for redistribution amongst citizens with *no* partisan attachments. While the majority of our sample (56%) do indeed identify with a specific political party, we find no evidence that their response to the tax day is more muted

Figure A2: Mean support for redistribution amongst respondents interviewed before and after the tax day (daily bins)



Data: ESS Finland 2002-18. *Note:* The graph shows the mean support for redistribution amongst respondents interviewed before and after the tax day using daily bins. Solid lines represent linear lines of best fit, estimated separately below and above the cutoff. Dotted lines represent 95% confidence intervals.

than the response of non-partisans (see Figure A16). While this evidence does not rule out that partisanship might explain the muted overall response to the tax day, it casts some doubts on this explanation.

Political ideology - Finally, it is possible that the overall null effect of the tax day on support for redistribution hides divergent reactions amongst left- and right-wing respondents. This idea is supported by recent research (Karadja et al. 2017, Fenton 2020), which suggests that political ideology can be an important moderating factor determining whether and how much citizens' adjust their support for redistribution when exposed to information about inequality. Figure A17 in the appendix shows the estimated effect of the tax day on support for redistribution separately for left- and right-wing respondents.[28] There is no evidence that the overall null result is driven by divergent reactions amongst left- and right-wing respondents.

[28]The correlation between ideological self-placement (on a 10-point left-right scale) and support for redistribution (on a 5-point scale) is negative and statistically significant (r = -0.28; $p < 0.01$).

A.2 Why does the tax day decrease support for redistribution amongst the top income decile?

There are at least four plausible mechanisms that could explain why respondents in the top income decile respond to the tax day by decreasing their support for redistribution. While the available data do not allows us to confirm or rule out any of these complementary mechanisms, we do find some evidence in support of one mechanism (motivated reasoning) and little evidence in support of the others. We discuss each potential mechanism in turn.

Correcting misperceptions - The first potential mechanism is that the tax day corrects misperceptions amongst the top 10% about their relative position in the income distribution. We might expect a stronger corrective information effect amongst the top 10% because the very rich tend to underestimate their relative income status more than any other income group (Hvidberg et al. 2020).[29] However, we think that this mechanism is unlikely to explain our results for the following reasons. The tax day focuses on the incomes of the super-rich who earn more than €100,000 a year, so perceptions of relative income status amongst the top 10% should become even more biased downwards, if anything. Furthermore, we find no evidence that respondents in the top income decile adjust their perceived relative income status in response to the tax day. Unfortunately, we do not have data on perceived income rank comparable to Hvidberg et al. (2020), so we cannot directly quantify misperceptions about relative income. However, since 2002, the ESS includes an item on perceived income adequacy, which we use as a proxy to measure perceived income status. Figure A27 in the appendix shows that the tax day has no significant effect on perceived income adequacy amongst the top income decile. While this evidence is only suggestive, it speaks against the idea that the top 10% reduce their support for redistribution in response to the tax day because the event corrects their (mis-)perceptions about their relative income status.

Salience of tax burden - A second possible mechanism is that the tax day suppresses support for redistribution amongst the top income decile because it reminds these individuals of their relatively high tax burden. This is plausible because the tax day reveals the total amount of taxes paid by Finland's top earners and the newspapers' search engines (*Verokone)* usually report their tax rate as well.[30] In this scenario we would expect the tax day to decrease the proportion of respondents in the top income decile who agree with the statement that "higher earners should pay a higher share of earnings in tax". We find no evidence that this is the case (see Figure A28). However, the null result may be driven by the small number of observations, as the relevant item was only included in one survey round.

Status concerns - A third plausible mechanism is that the tax day suppresses support for redistribution amongst the top income decile because it triggers a "keeping up with the Kardashians" reaction. Such a reaction is plausible because the tax day facilitates upward

[29] Hvidberg et al. (2020) use survey and administrative data from Denmark to show that people with incomes above the 95th percentile overestimate the average income in the 95th percentile by 50%.

[30] See e.g. https://www.iltalehti.fi/verokone. Finland's top earners typically pay between 30-50% taxes on earned and capital income.

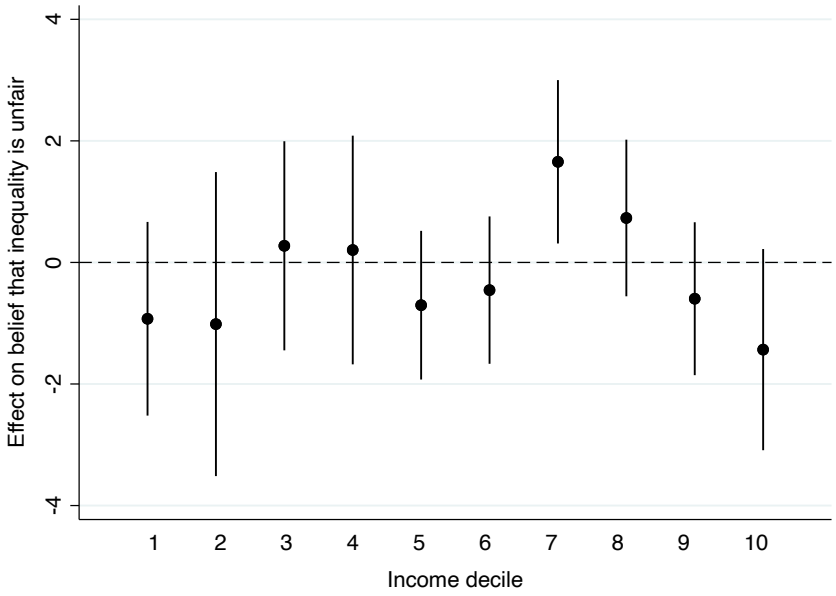
income comparisons with the super-rich who earn more than €100,000 a year.[31] If status concerns are the mechanism linking the tax day to reduced support for redistribution amongst the top income decile, we would expect the event to increase the extent to which the top 10% report valuing money and other status goods. However, we find no evidence that the tax day increases the proportion of respondents in the top income decile who agree with the statement that it is "important to be rich, have money and expensive things" (see Figure A29). In this case, the relevant item was included in all ESS rounds, so the null effect is unlikely due to small sample size.

Motivated reasoning - A fourth plausible mechanism is that the tax day suppresses demand for redistribution amongst the top income decile because the event triggers a process of motivated reasoning. Given that the tax day focuses on the incomes of Finland's top earners, individuals in the top income decile may feel unfairly targeted or singled-out by the tax day and may therefore (re-)affirm their beliefs that income inequality in Finland is justified, for example because high incomes are rewards for effort or talents.[32] This explanation finds support in recent sociological research from Finland (Kantola & Kuusela 2019, Kantola 2020), which shows that the nation's top earners tend to construct self-identities based on hard work in order to justify their wealth in the face of strong egalitarian norms. We find some suggestive evidence for the motivated reasoning mechanism. Figure A3 shows that, amongst the top income decile, the tax day decreases support for the statement that "for a fair society, differences in standard of living should be small." The estimate for the top income decile is negative and statistically significant (β = -1.43; $p = 0.08$), and amounts to a reduction of 1.5 standard deviations. The relevant item is only included in two ESS rounds, so the small sample size in the top income decile ($n = 83$) might explain the relatively large confidence interval. However, despite the noisy estimates, Figure A3 shows a rightward shift in fairness beliefs in response to the tax day amongst individuals at the top of the income distribution. Reassuringly, we find a very similar pattern when using an alternative measure of respondents' beliefs that inequality is justified or fair. The relevant item was included in two ESS rounds and asks respondents to what extent they disagree that "large differences in income are acceptable to reward talents and efforts", with higher values on the 5-point scale reflecting higher levels of disagreement. Again, although the estimates are noisy, they indicate that the tax day boosts beliefs amongst top earners that inequality can be justified to reward talents or effort (see Figure A30).

[31] In line with this expectation, experimental research from the US shows that exposing affluent subjects to other people's success causes them to hold more economically conservative views (Thal 2020)

[32] In line with this mechanism, Suhay et al. 2020 find that affluent Americans are more likely than other income groups to attribute economic success to intelligence and hard work. They also find that "individual-blaming" attributions for economic success are more strongly predictive of economic conservatism amongst the affluent than amongst other income groups.

Figure A3: Effect of the tax day on belief that inequality is unfair by income decile



Data: ESS Finland 2008 & 2016. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' belief that inequality is unfair by income decile. Vertical lines represent 95% confidence intervals. Estimates are from an OLS model with 10-day bandwidths, fitted separately on each income decile. The three days prior to the tax day are excluded from the analysis. To increase precision of the estimates, we include controls for age, gender, education, and labour market status (in paid job, unemployed, student, retired, doing housework). Belief that inequality is unfair measures respondents' support for the statement that "for a fair society, differences in standard of living should be small." Response options range from 1 (disagree strongly) to 8 (agree strongly) so that higher values reflect more left-wing beliefs.

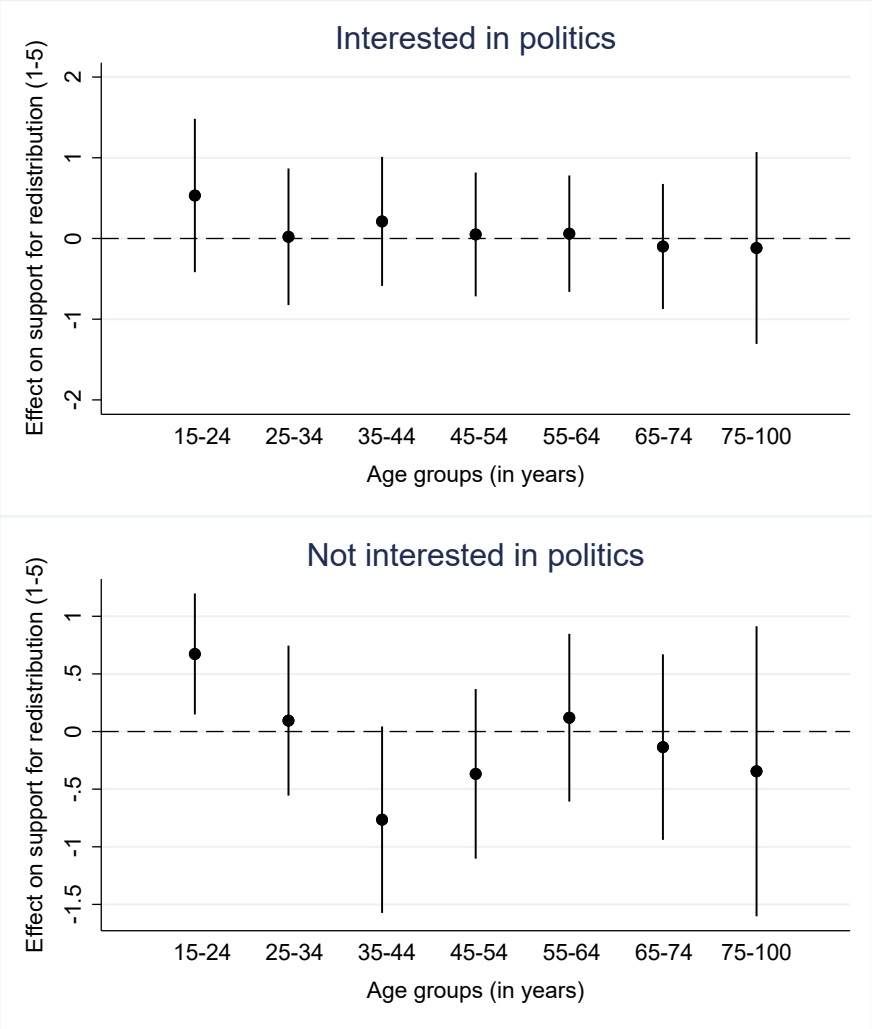
A.3 Why does the tax day increase support for redistribution amongst young people?

Correcting misperceptions - A possible mechanism linking the tax day to increased support for redistribution amongst the youngest age group is through its effect on perceptions of relative income status. The information effect of the tax day might be stronger for the youngest age group, either because they are less likely to have been exposed to previous tax days, or because they are less likely to interpret the information through the lens of a consolidated political worldview (see Dinas 2013).[33] To disentangle these two mechanisms, we further split the sample into respondents who report being interested in politics and those not interested. We find that the positive effect of the tax day is concentrated amongst those young people with low interest in politics - who are less likely to have been "pre-treated" -

[33]15-24 year-olds are also least likely to have experience paying taxes. Only 26% of 15-24 year-olds are in paid work, compared with 73% of 25-65 year-olds.

which is indicative evidence that lack of previous exposure can explain the reaction amongst the youngest age group (Figure A4).

Figure A4: Effect of the tax day on support for redistribution by age group and interest in politics

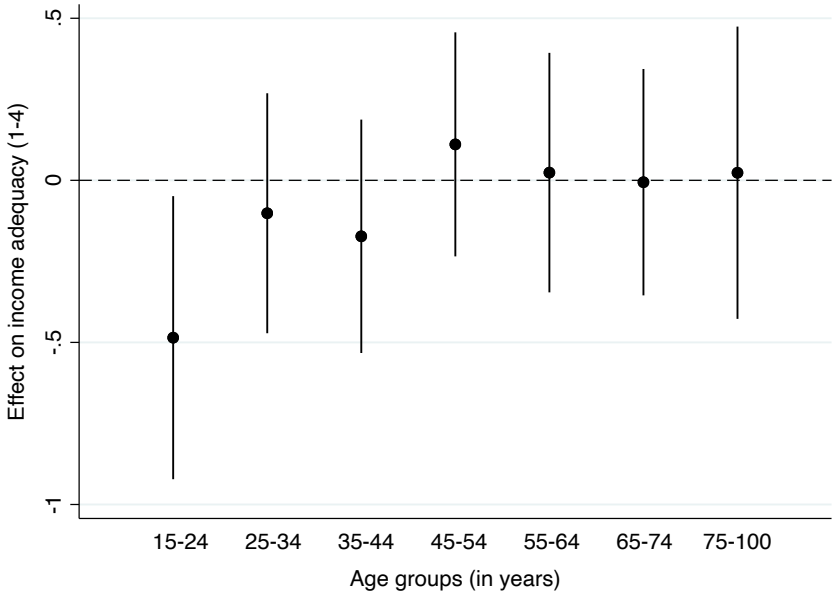


Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' support for redistribution by age group. The sample is split into respondents who report being "very" or "quite" interested in politics and respondents who report being "hardly" or "not at all" interested". Vertical lines represent 95% confidence intervals. Estimates are from an OLS model with 10-day bandwidths, fitted separately on each age group. The three days prior to the tax day are excluded from the analysis.

Either way, we would expect the youngest age group to adjust their perceptions of relative income status more than older age groups. In this case, a leftward shift in redistributive preferences is plausible because young people are disproportionately represented at the lower end of the income distribution, where individuals tend to overestimate their relative income

status (Hvidberg et al. 2020). We find some evidence in support of this explanation. As earlier, we use perceived income adequacy as a proxy for perceived income status. In line with our expectations, we find that the tax day decreases perceived income adequacy amongst those in the youngest age group, and only here (see Figure A5). The negative effect is sizeable and statistically significant (-0.7 SD; $p = 0.02$). While this evidence is only suggestive, it speaks for the idea that the tax day leads to increased support for redistribution amongst the youngest age group because it corrects (mis-)perceptions about their relative income status.

Figure A5: Effect of the tax day on perceived income adequacy by age group



Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' perceived income adequacy by age group. Vertical lines represent 95% confidence intervals. Estimates are from an OLS model with 10-day bandwidths, fitted separately on each age group. The three days prior to the tax day are excluded from the analysis. To increase precision of the estimates, we include controls for age, gender, education, and labour market status (in paid job, unemployed, student, retired, doing housework). Perceived income adequacy captures how respondents feel about their household's income nowadays, with response options ranging from 1 (finding it very difficult) to 4 (living comfortably).

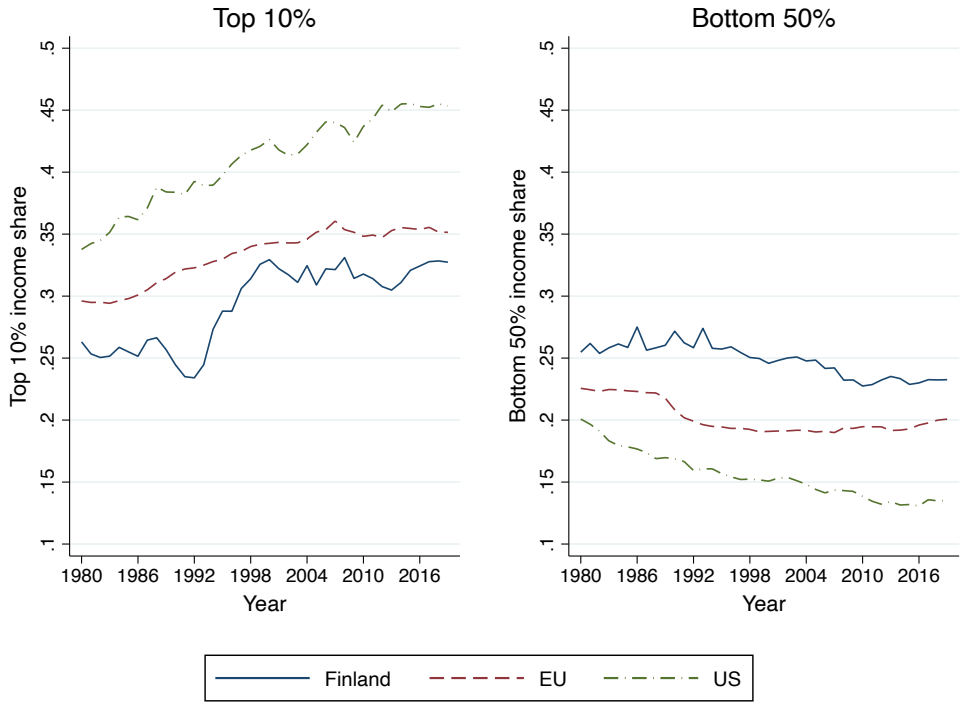
Media diets - An alternative explanation for the stronger reaction amongst the youngest age group is that young people systematically expose themselves to different media coverage of the tax day than older age groups (e.g. coverage that is more critical of income inequality). Unfortunately, the ESS does not ask respondents which newspapers they read or television channels they watch. However, we can measure *how much* respondents are exposed to news about politics and current affairs. We use an ESS item which asks respondents how much time they spend on a typical day watching, reading or listening to news about politics and current affairs (in minutes),[34] and divide the sample into respondents with high media

[34]This item was included in only two ESS rounds (2016 & 2018).

exposure (>60 minutes per day) and low media exposure (<60 minutes per day).[35] We find that young people are significantly less likely to be exposed to news about politics and current affairs compared to older age groups. For example, only 31% of 15-24 year-old's have high media exposure compared with 73% of 75-100 year-old's.[36] This suggests that young people do indeed have a different media diet than older age groups. However, we find no evidence that the tax day affects support for redistribution differently depending on whether respondents have high- or low media exposure (see Figure A31). While this evidence is only suggestive, it casts some doubts on the idea that systematically different media diets explain why young people react more strongly to the tax day.

B Supplementary descriptive material

Figure A6: Income inequality in Finland, the EU and the US (1980-2019)



Data: World Inequality Database. *Note:* The graph shows the share of total pre-tax income going to the top 10% of adults (left panel) and the bottom 50% of adults (right panel) in Finland (solid), the EU (dashed) and the US (dash-dotted) for the years 1980-2019.

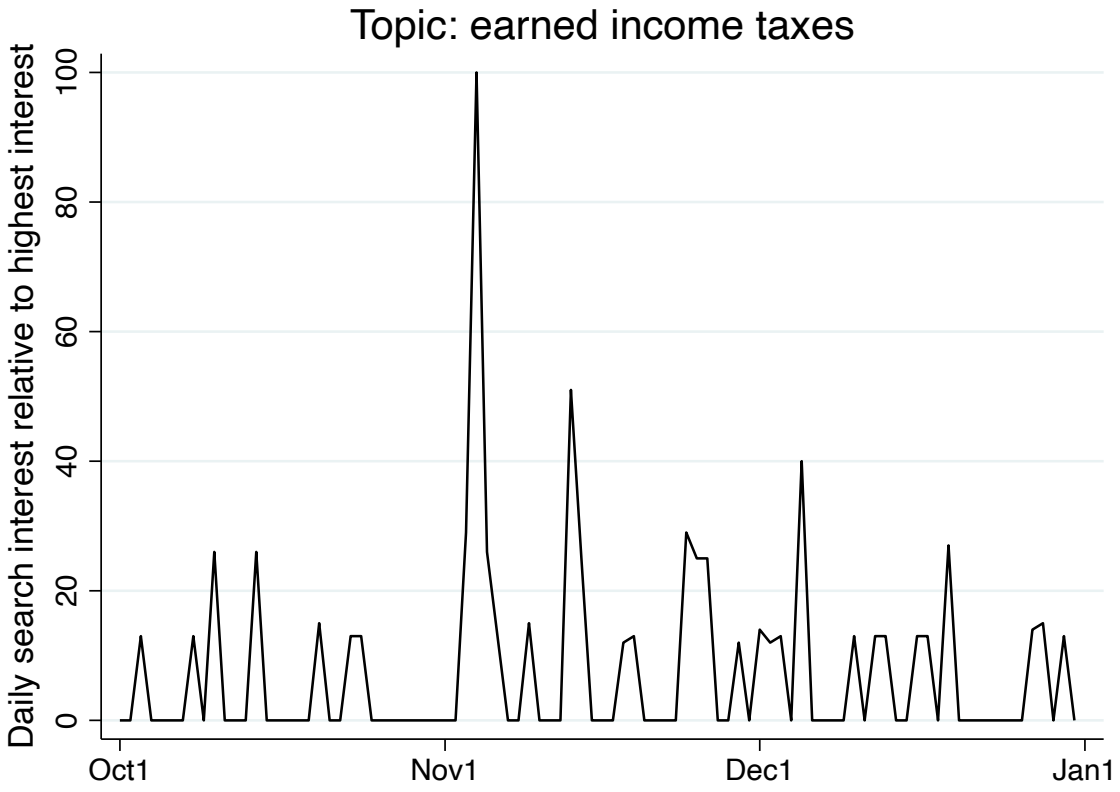
[35] 60 minutes is the median and mean response.

[36] The differences between the youngest age group and all older age groups are statistically significant.

Table A1: Finland's tax days (2000-2020)

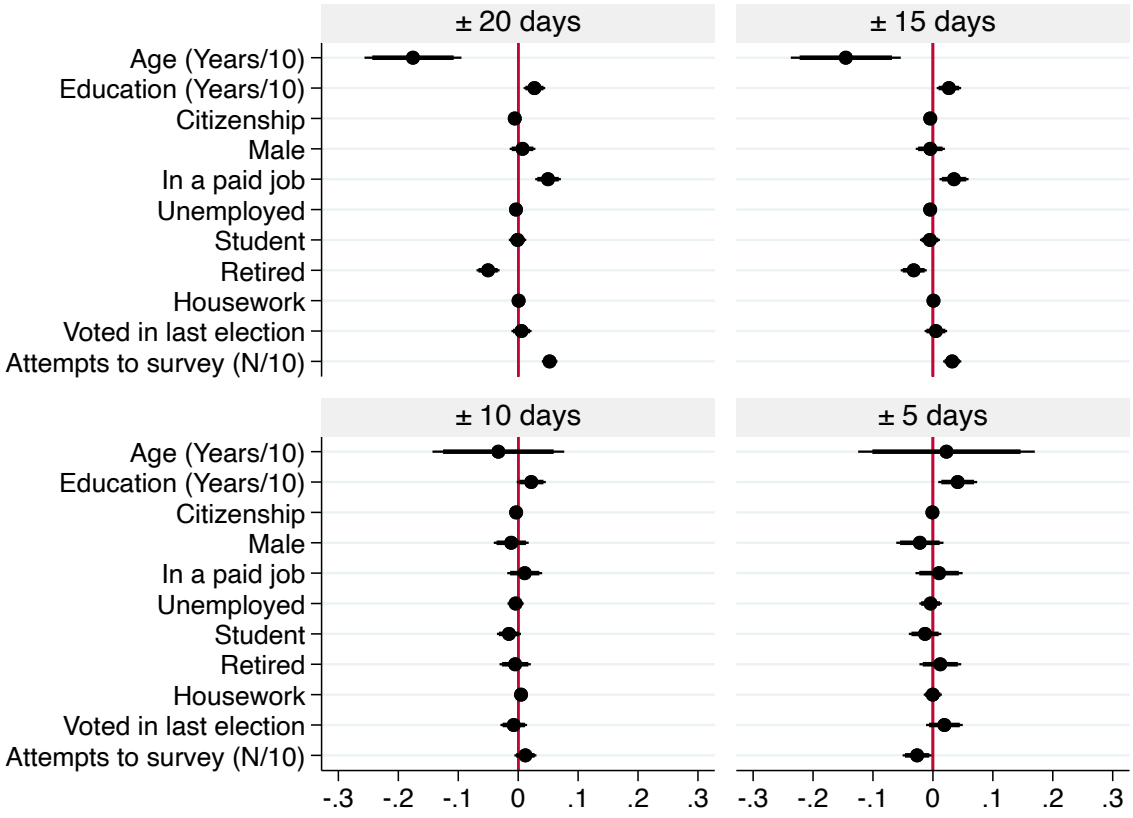
Year	Date	Day of the week
2000	Nov 1	Wednesday
2001	Nov 1	Thursday
2002	Nov 1	Friday
2003	Nov 3	Monday
2004	Nov 1	Monday
2005	Nov 1	Tuesday
2006	Nov 1	Wednesday
2007	Nov 1	Thursday
2008	Nov 3	Monday
2009	Nov 2	Monday
2010	Nov 1	Monday
2011	Nov 1	Tuesday
2012	Nov 1	Thursday
2013	Nov 1	Friday
2014	Nov 3	Monday
2015	Nov 2	Monday
2016	Nov 1	Tuesday
2017	Nov 1	Wednesday
2018	Nov 1	Thursday
2019	Nov 4	Monday
2020	Nov 3	Tuesday

Figure A7: Google search queries for the "earned income taxes" topic (Oct–Dec 2019)



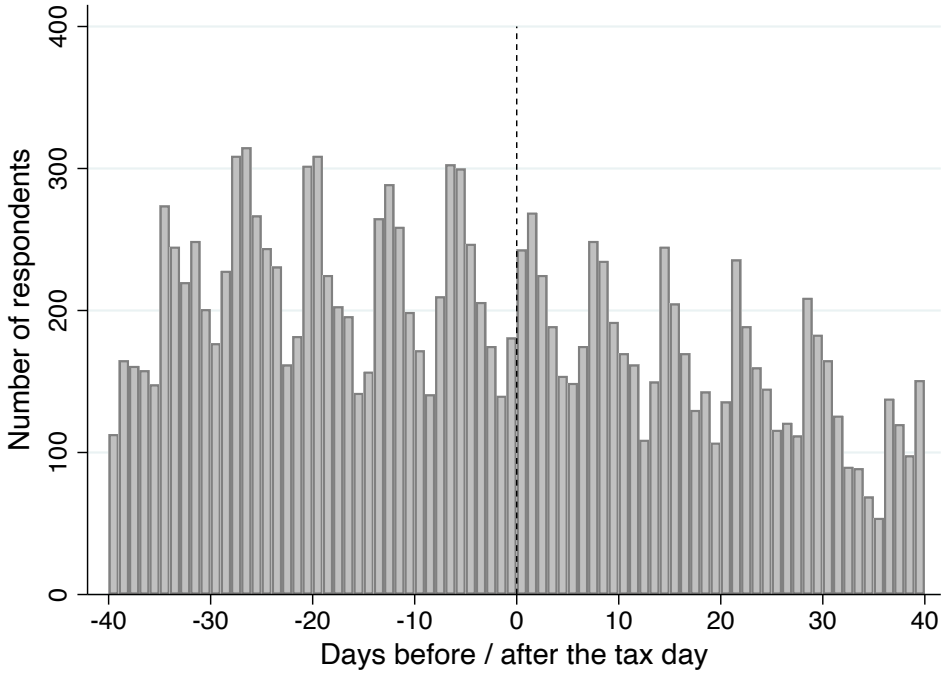
Data: Google Trends. *Note:* Numbers represent search interest relative to the highest point on the chart for the given region and time. A value of 100 is the peak popularity for the term. A value of 50 means that the term is half as popular. A score of 0 means that there was not enough data for this term. In 2019, the tax day was on Monday, November 4^{th}.

Figure A8: Balance tests on covariates (ESS 2002-18)



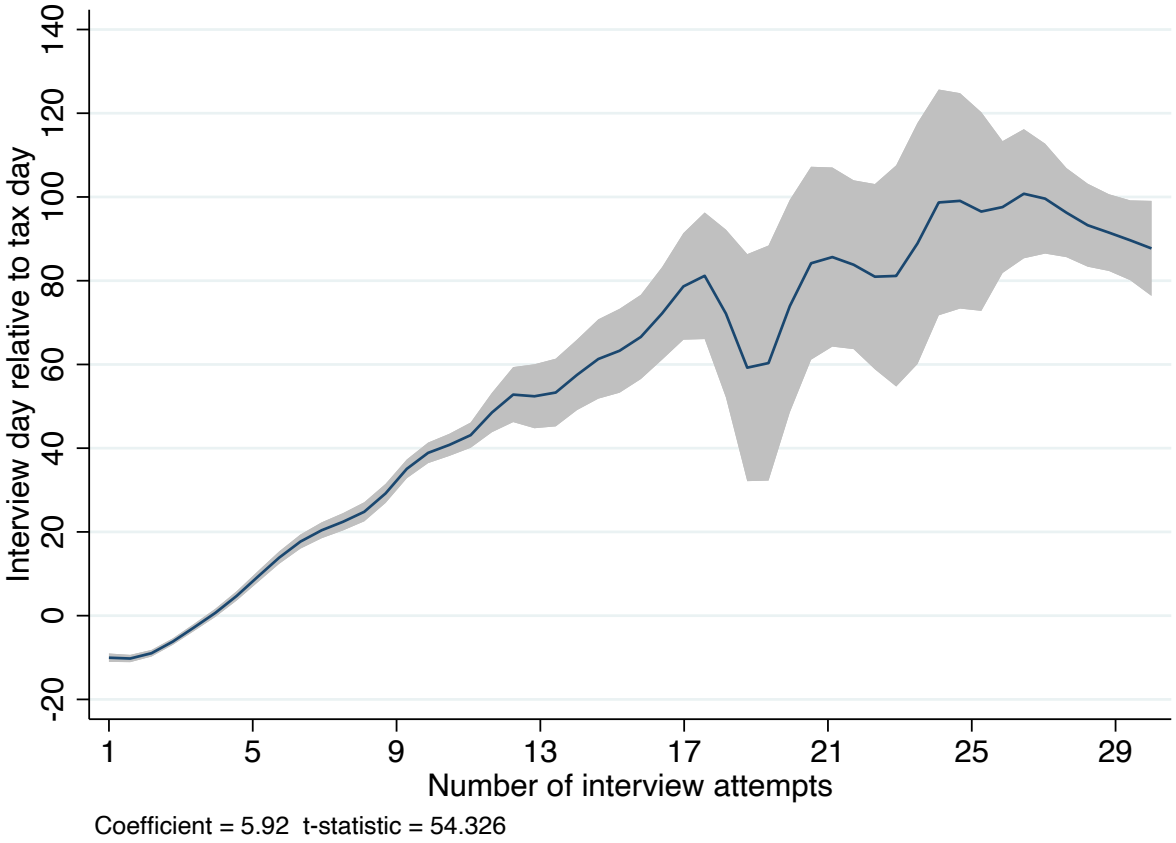
Data: ESS Finland 2002-18 for all covariates except "attempts to survey", which is from ESS rounds 2008-2018. *Note:* Entries report the difference in the mean of the covariates between the treatment and control groups for various bandwidths (days) around the tax day. Thick and thin lines are 90% and 95% confidence intervals, respectively.

Figure A9: Number of respondents by interview date in 40-day window around the tax day (ESS 2002-18)



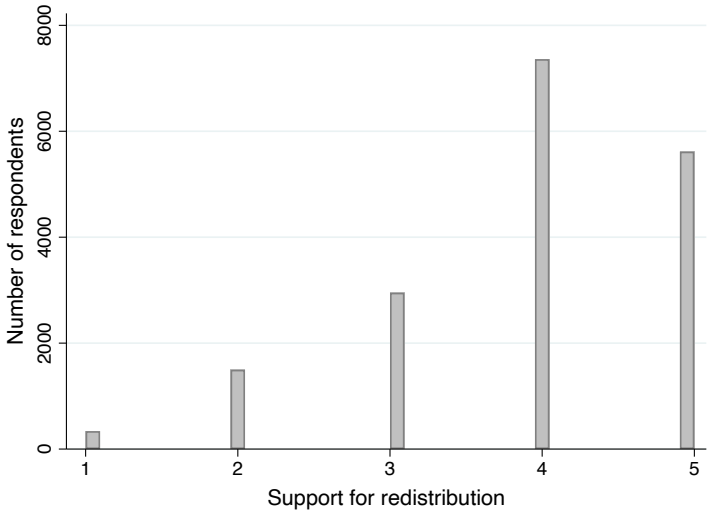
Data: ESS Finland 2002-18. *Note:* Exact dates for the tax days are in Table A1.

Figure A10: Number of attempts to survey by interview day relative to tax day (ESS Paradata 2008-18)



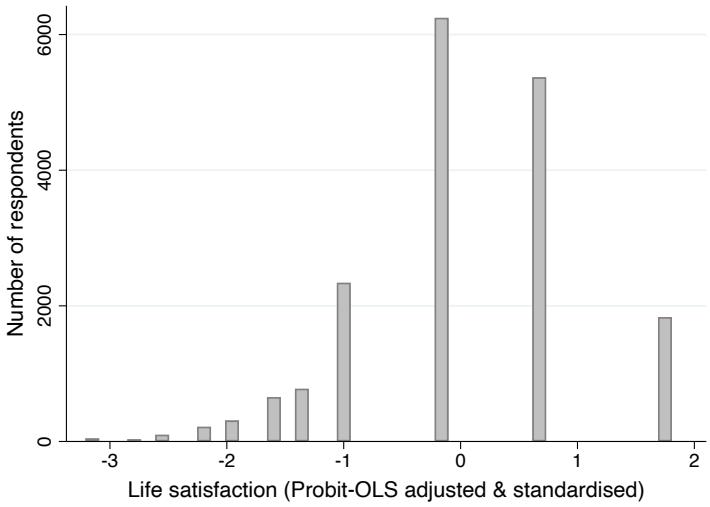
Data: ESS Paradata 2008-18 for Finland. Earlier rounds are not included because the coding of interview outcomes in the ESS Paradata changed from 2006 to 2008. *Note:* The figure plots the number of attempts to survey before interview completion by fieldwork day when the interview was completed (relative to the tax day). We use kernel-weighted local polynomial smoothing with 95% confidence intervals, as Muñoz et al. (2020). The coefficient and t-statistic are from an OLS model regressing fieldwork day when interview was completed on the number of attempts to survey.

Figure A11: Histogram of support for redistribution



Data: ESS Finland 2002-18. *Note:* Support for redistribution captures the extent to which respondents agree that the government should take measures to reduce differences in income levels (1-disagree strongly, 5-agree strongly).

Figure A12: Histogram of life satisfaction



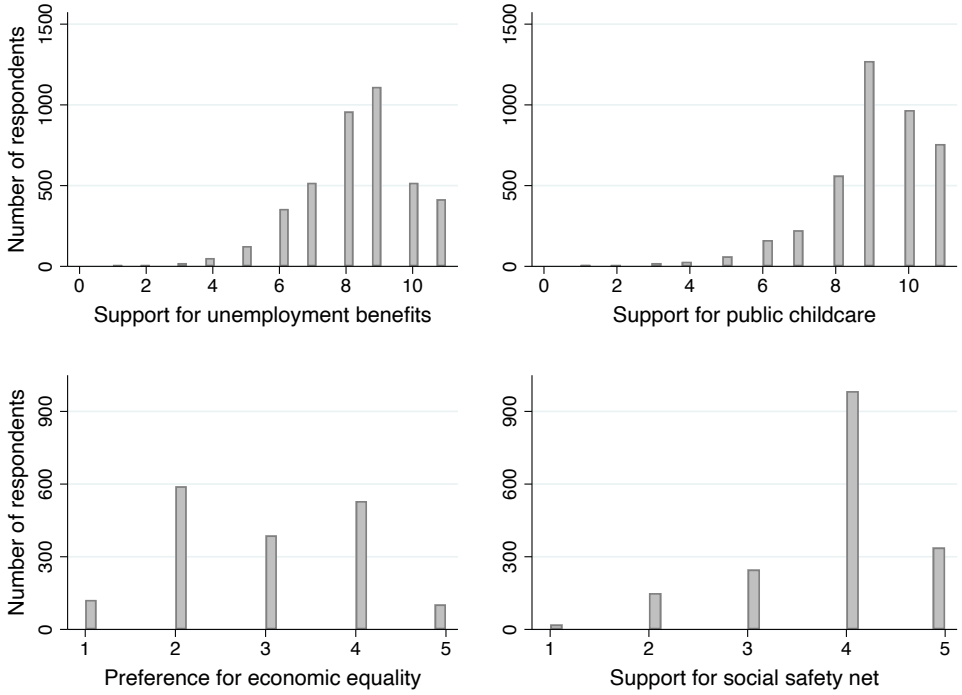
Data: ESS Finland 2002-18. *Note:* Life satisfaction is measured with the following question: "All things considered, how satisfied are you with your life as a whole nowadays?" Response options range from 1 (extremely dissatisfied) to 11 (extremely satisfied). The values are adjusted using the Probit-OLS method and standardised to have a mean of 0 and a standard deviation of 1.

Table A2: Summary statistics for alternative measures of support for redistribution

	Years	Obsv.	Mean	SD	Min	Max
Support for unemployment benefits	2008 & 2016	4092	8.34	1.68	1	11
Support for public childcare	2008 & 2016	4074	9.09	1.59	1	11
Preference for economic equality	2018	1734	2.94	1.08	1	5
Support for social safety net	2018	1739	3.85	0.87	1	5

Data: ESS Finland 2008, 2016, 2018. *Note:* Support for unemployment benefits captures the extent to which respondents think that it should be governments' responsibility to ensure a reasonable standard of living for the unemployed (1-not at all, 11-entirely). Support for public childcare captures the extent to which respondents think that it should be governments' responsibility to ensure sufficient child care services for working parents (1-not at all, 11-entirely). Preference for economic equality captures how much respondents agree or disagree with the statement that a society is fair when income and wealth are equally distributed among all people (1-disagree strongly, 5-agree strongly). Support for social safety net captures how much respondents agree or disagree with the statement that a society is fair when it takes care of those who are poor and in need regardless of what they give back to society (1-disagree strongly, 5-agree strongly).

Figure A13: Histograms of alternative measures of support for redistribution



Data: ESS Finland 2008, 2016, 2018. *Note:* Support for unemployment benefits captures the extent to which respondents think that it should be governments' responsibility to ensure a reasonable standard of living for the unemployed (1-not at all, 11-entirely). Support for public childcare captures the extent to which respondents think that it should be governments' responsibility to ensure sufficient child care services for working parents (1-not at all, 11-entirely). Preference for economic equality captures how much respondents agree or disagree with the statement that a society is fair when income and wealth are equally distributed among all people (1-disagree strongly, 5-agree strongly). Support for social safety net captures how much respondents agree or disagree with the statement that a society is fair when it takes care of those who are poor and in need regardless of what they give back to society (1-disagree strongly, 5-agree strongly).

C Regression tables

Table A3: Effect of the tax day amongst below- and above-median income earners

	(1) Unfairness perception	(2) Support for redistribution
	Below-median income	
Treatment	5.461***	0.168
	(1.732)	(0.156)
Days	-0.622***	-0.017
	(0.203)	(0.021)
Treatment × Days	0.657***	0.006
	(0.219)	(0.023)
Survey-year FE	-	✓
Observations	94	1,606
R-squared	0.119	0.007
Survey years	2018	2002-18
	Above-median income	
Treatment	1.524*	-0.076
	(0.806)	(0.149)
Days	-0.133	0.024
	(0.093)	(0.020)
Treatment × Days	0.049	-0.034
	(0.108)	(0.022)
Survey-year FE	-	✓
Observations	153	2,051
R-squared	0.030	0.005
Survey years	2018	2002-18

Note: Estimates are from OLS regressions with standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The *Treatment* coefficient captures the effect of the tax day on the outcome. The sample is restricted to a 10-day window around the tax day in a given year and the 3 days prior to the tax day are excluded. The top panel shows estimates for the less affluent sub-sample (below-median income) and the bottom panel shows estimates for the affluent sub-sample (above-median income). Unfairness perception ranges from 1 to 9, with high values reflecting perceptions that top 10% incomes are unfairly high. Support for redistribution captures the extent to which respondents agree that the government should take measures to reduce differences in income levels (1-disagree strongly, 5-agree strongly).

Table A4: Effect of the tax day amongst below- and above-median income earners (clustered standard errors)

	(1) Unfairness perception	(2) Support for redistribution
	Below-median income	
Treatment	5.461*	0.140
	(3.070)	(0.095)
Days	-0.622*	-0.012
	(0.344)	(0.013)
Treatment × Days	0.657*	0.001
	(0.349)	(0.017)
Survey-year FE	-	✓
Observations	94	1,606
R-squared	0.119	0.007
Survey years	2018	2002-18
	Above-median income	
Treatment	1.524***	-0.055
	(0.312)	(0.128)
Days	-0.133***	0.020
	(0.042)	(0.017)
Treatment × Days	0.049	-0.031
	(0.045)	(0.019)
Survey-year FE	-	✓
Observations	153	2,051
R-squared	0.030	0.005
Survey years	2018	2002-18

Note: Estimates are from OLS regressions with robust standard errors clustered at the *Days* level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The *Treatment* coefficient captures the effect of the tax day on the outcome. The sample is restricted to a 10-day window around the tax day in a given year and the 3 days prior to the tax day are excluded. The top panel shows estimates for the less affluent sub-sample (below-median income) and the bottom panel shows estimates for the affluent sub-sample (above-median income). Unfairness perception ranges from 1 to 9, with high values reflecting perceptions that top 10% incomes are unfairly high. Support for redistribution captures the extent to which respondents agree that the government should take measures to reduce differences in income levels (1-disagree strongly, 5-agree strongly).

Table A5: Effect of the tax day (whole sample)

	(1) Unfairness perception	(2) Support for redistribution
Treatment	2.538***	0.043
	(0.793)	(0.106)
Days	-0.268***	0.004
	(0.092)	(0.014)
Treatment × Days	0.240**	-0.015
	(0.104)	(0.016)
Survey-year FE	-	✓
Observations	247	3,789
R-squared	0.043	0.003
Survey years	2018	2002-18

Note: Estimates are from OLS regressions with standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The *Treatment* coefficient captures the effect of the tax day on the outcome. The sample is restricted to a 10-day window around the tax day in a given year and the 3 days prior to the tax day are excluded. Unfairness perception ranges from 1 to 9, with high values reflecting perceptions that top 10% incomes are unfairly high. Support for redistribution captures the extent to which respondents agree that the government should take measures to reduce differences in income levels (1-disagree strongly, 5-agree strongly).

Table A6: Effect of the tax day (whole sample, clustered standard errors)

	(1) Unfairness perception	(2) Support for redistribution
Treatment	2.538***	0.043
	(0.767)	(0.092)
Days	-0.268***	0.004
	(0.085)	(0.011)
Treatment × Days	0.240**	-0.015
	(0.090)	(0.013)
Survey-year FE	-	✓
Observations	247	3,789
R-squared	0.043	0.003
Survey years	2018	2002-18

Note: Estimates are from OLS regressions with robust standard errors clustered at the *Days* level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The *Treatment* coefficient captures the effect of the tax day on the outcome. The sample is restricted to a 10-day window around the tax day in a given year and the 3 days prior to the tax day are excluded. Unfairness perception ranges from 1 to 9, with high values reflecting perceptions that top 10% incomes are unfairly high. Support for redistribution captures the extent to which respondents agree that the government should take measures to reduce differences in income levels (1-disagree strongly, 5-agree strongly).

Table A7: Effect of the tax day on support for redistribution by income decile

Income decile	1st	2nd	3rd	4th	5th	6th	7th	8th	9th	10th
Mean outcome	4.046	4.047	4.080	4.063	4.088	4.027	3.972	3.814	3.665	3.350
Treatment	0.165	0.056	0.657*	-0.114	-0.155	0.405	-0.216	0.110	-0.275	-0.998**
	(0.376)	(0.440)	(0.373)	(0.339)	(0.292)	(0.284)	(0.305)	(0.337)	(0.387)	(0.435)
Days	-0.035	0.049	-0.085	0.021	0.016	-0.049	0.037	-0.022	0.071	0.157***
	(0.049)	(0.060)	(0.052)	(0.045)	(0.040)	(0.038)	(0.042)	(0.044)	(0.051)	(0.057)
Treatment × Days	0.042	-0.095	0.050	-0.013	-0.013	0.042	-0.044	0.018	-0.094*	-0.170***
	(0.055)	(0.066)	(0.057)	(0.050)	(0.043)	(0.042)	(0.047)	(0.048)	(0.055)	(0.063)
Survey-year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	302	276	321	329	371	442	404	382	359	314
R-squared	0.040	0.034	0.063	0.021	0.020	0.036	0.008	0.023	0.055	0.063

Data: ESS Finland 2002-18. *Note:* Estimates are from OLS regressions with standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The *Treatment* coefficient captures the effect of the tax day on support for redistribution for each income decile. The sample is restricted to a 10-day window around the tax day in a given year and the 3 days prior to the tax day are excluded. Support for redistribution captures the extent to which respondents agree that the government should take measures to reduce differences in income levels (1-disagree strongly, 5-agree strongly).

Table A8: Effect of the tax day on support for redistribution by income decile (clustered standard errors)

Income decile	1^{st}	2^{nd}	3^{rd}	4^{th}	5^{th}	6^{th}	7^{th}	8^{th}	9^{th}	10^{th}
Treatment	0.165	0.056	0.657**	-0.114	-0.155	0.405**	-0.216	0.110	-0.275	-0.998***
	(0.207)	(0.348)	(0.233)	(0.224)	(0.154)	(0.173)	(0.333)	(0.217)	(0.198)	(0.239)
Days	-0.035	0.049	-0.085**	0.021	0.016	-0.049**	0.037	-0.022	0.071**	0.157***
	(0.024)	(0.042)	(0.030)	(0.034)	(0.022)	(0.020)	(0.038)	(0.034)	(0.031)	(0.026)
Treatment × Days	0.042	-0.095*	0.050	-0.013	-0.013	0.042*	-0.044	0.018	-0.094**	-0.170***
	(0.029)	(0.049)	(0.044)	(0.039)	(0.030)	(0.022)	(0.047)	(0.042)	(0.034)	(0.031)
Survey-year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	302	276	321	329	371	442	404	382	359	314
R-squared	0.040	0.034	0.063	0.021	0.020	0.036	0.008	0.023	0.055	0.063

Data: ESS Finland 2002-18. *Note:* Estimates are from OLS regressions with robust standard errors clustered at the *Days* level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The *Treatment* coefficient captures the effect of the tax day on support for redistribution for each income decile. The sample is restricted to a 10-day window around the tax day in a given year and the 3 days prior to the tax day are excluded. Support for redistribution captures the extent to which respondents agree that the government should take measures to reduce differences in income levels (1-disagree strongly, 5-agree strongly).

Table A9: Effect of the tax day on support for redistribution by age group

Age group (years)	15-24	25-34	35-44	45-54	55-64	65-74	75-100
Treatment	0.586**	0.038	-0.272	-0.046	0.162	-0.155	-0.301
	(0.237)	(0.265)	(0.288)	(0.270)	(0.259)	(0.280)	(0.428)
Days	-0.055*	0.038	0.047	0.020	-0.046	0.037	0.031
	(0.032)	(0.036)	(0.039)	(0.036)	(0.036)	(0.037)	(0.057)
Treatment × Days	0.027	-0.087**	-0.063	-0.018	0.072*	-0.047	-0.038
	(0.035)	(0.040)	(0.042)	(0.039)	(0.039)	(0.042)	(0.063)
Survey-year FE	✓	✓	✓	✓	✓	✓	✓
Observations	530	544	590	625	698	495	307
R-squared	0.024	0.024	0.014	0.012	0.035	0.016	0.019

Data: ESS Finland 2002-18. *Note:* Estimates are from OLS regressions with standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The *Treatment* coefficient captures the effect of the tax day on support for redistribution for each age group. The sample is restricted to a 10-day window around the tax day in a given year and the 3 days prior to the tax day are excluded. Support for redistribution captures the extent to which respondents agree that the government should take measures to reduce differences in income levels (1-disagree strongly, 5-agree strongly).

Table A10: Effect of the tax day on support for redistribution by age group (clustered standard errors)

Age group (years)	15-24	25-34	35-44	45-54	55-64	65-74	75-100
Treatment	0.586**	0.038	-0.272*	-0.046	0.162	-0.155	-0.301
	(0.247)	(0.219)	(0.134)	(0.265)	(0.165)	(0.228)	(0.356)
Days	-0.055*	0.038	0.047**	0.020	-0.046	0.037	0.031
	(0.028)	(0.035)	(0.020)	(0.037)	(0.029)	(0.033)	(0.048)
Treatment × Days	0.027	-0.087**	-0.063**	-0.018	0.072**	-0.047	-0.038
	(0.031)	(0.039)	(0.022)	(0.041)	(0.031)	(0.034)	(0.052)
Survey-year FE	✓	✓	✓	✓	✓	✓	✓
Observations	530	544	590	625	698	495	307
R-squared	0.024	0.024	0.014	0.012	0.035	0.016	0.019

Data: ESS Finland 2002-18. *Note:* Estimates are from OLS regressions with robust standard errors clustered at the *Days* level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The *Treatment* coefficient captures the effect of the tax day on support for redistribution for each age group. The sample is restricted to a 10-day window around the tax day in a given year and the 3 days prior to the tax day are excluded. Support for redistribution captures the extent to which respondents agree that the government should take measures to reduce differences in income levels (1-disagree strongly, 5-agree strongly).

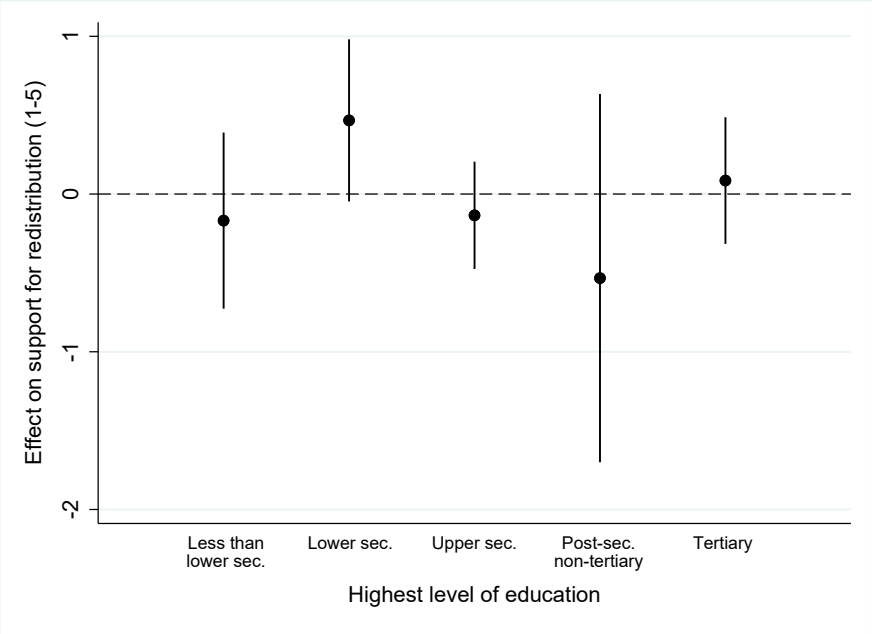
Table A11: Effect of the tax day on alternative measures of support for redistribution

	(1) Support for unemployed	(2) Support for childcare	(3) Pref. for econ. equality	(4) Support for safety net
		Below-median income		
Treatment	-0.105	-0.192	-0.812	1.223
	(0.648)	(0.669)	(1.147)	(0.787)
Days	-0.006	0.017	0.174	-0.134
	(0.102)	(0.105)	(0.134)	(0.092)
Treatment × Days	0.006	-0.003	-0.207	0.115
	(0.106)	(0.109)	(0.145)	(0.099)
Survey-year FE	✓	✓		
Observations	426	422	97	97
R-squared	0.018	0.030	0.061	0.025
		Above-median income		
Treatment	0.503	0.333	1.298*	-0.149
	(0.460)	(0.463)	(0.662)	(0.528)
Days	-0.089	-0.072	-0.129*	0.031
	(0.070)	(0.070)	(0.076)	(0.061)
Treatment × Days	0.084	0.054	0.051	-0.034
	(0.074)	(0.074)	(0.089)	(0.071)
Survey-year FE	✓	✓		
Observations	574	570	158	158
R-squared	0.003	0.011	0.036	0.004

Data: ESS Finland 2008, 2016, 2018. *Note:* Estimates are from OLS regressions with standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The *Treatment* coefficient captures the effect of the tax day on the outcome. The sample is restricted to a 10-day window around the tax day in a given year and the 3 days prior to the tax day are excluded. The top panel shows estimates for the less affluent sub-sample (below-median income) and the bottom panel shows estimates for the affluent sub-sample (above-median income). Support for unemployment benefits captures the extent to which respondents think that it should be governments' responsibility to ensure a reasonable standard of living for the unemployed (1-not at all, 11-entirely). Support for public childcare captures the extent to which respondents think that it should be governments' responsibility to ensure sufficient child care services for working parents (1-not at all, 11-entirely). Preference for economic equality captures how much respondents agree or disagree with the statement that a society is fair when income and wealth are equally distributed among all people (1-disagree strongly, 5-agree strongly). Support for social safety net captures how much respondents agree or disagree with the statement that a society is fair when it takes care of those who are poor and in need regardless of what they give back to society (1-disagree strongly, 5-agree strongly).

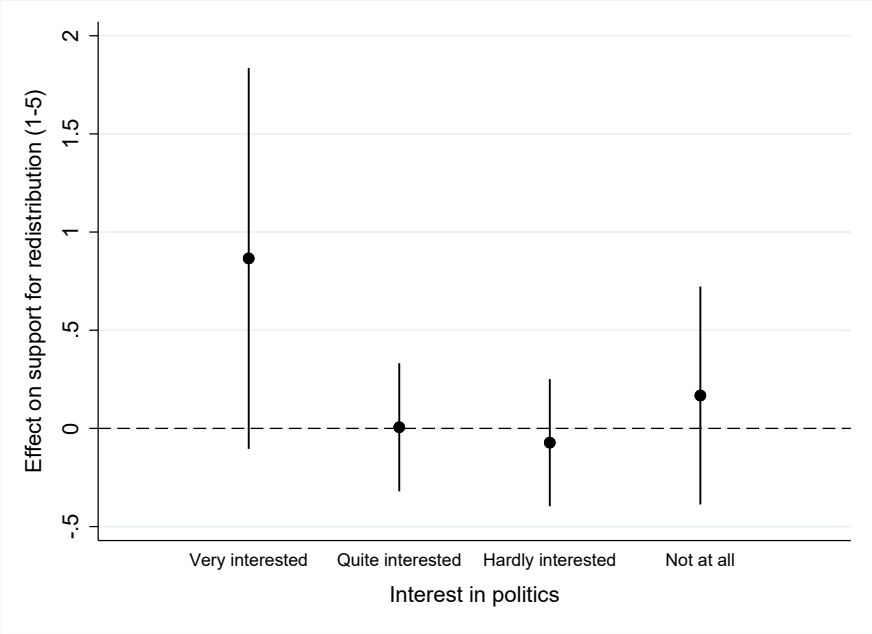
D Supplementary figures on heterogeneous effects

Figure A14: Effect of the tax day on support for redistribution by highest level of education



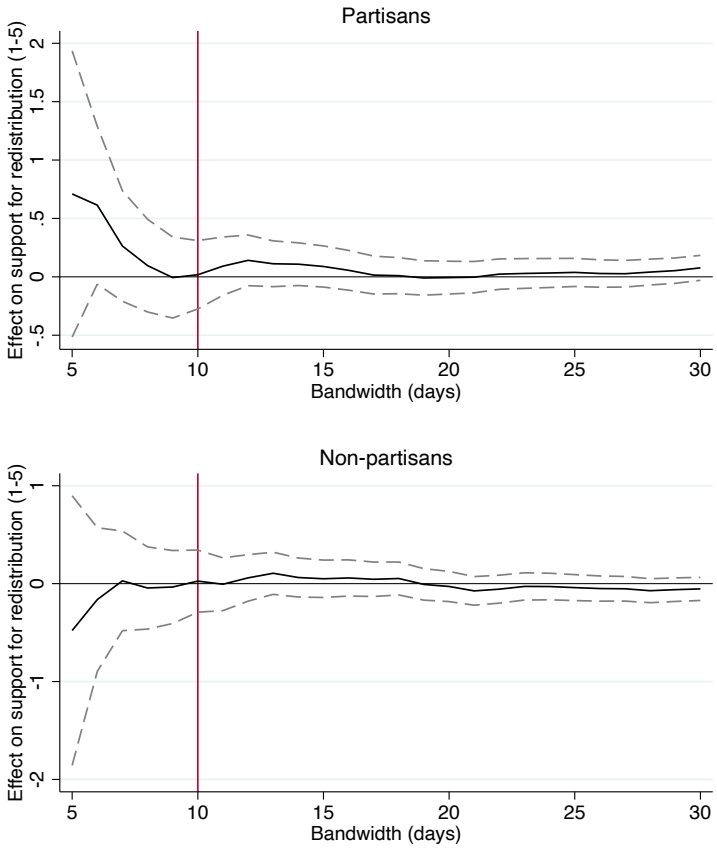
Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' support for redistribution by respondents' highest level of education. Vertical lines represent 95% confidence intervals. Estimates are from our baseline model with 10-day bandwidths, fitted separately on each educational status group. The three days prior to the tax day are excluded from the analysis. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly).

Figure A15: Effect of the tax day on support for redistribution by interest in politics



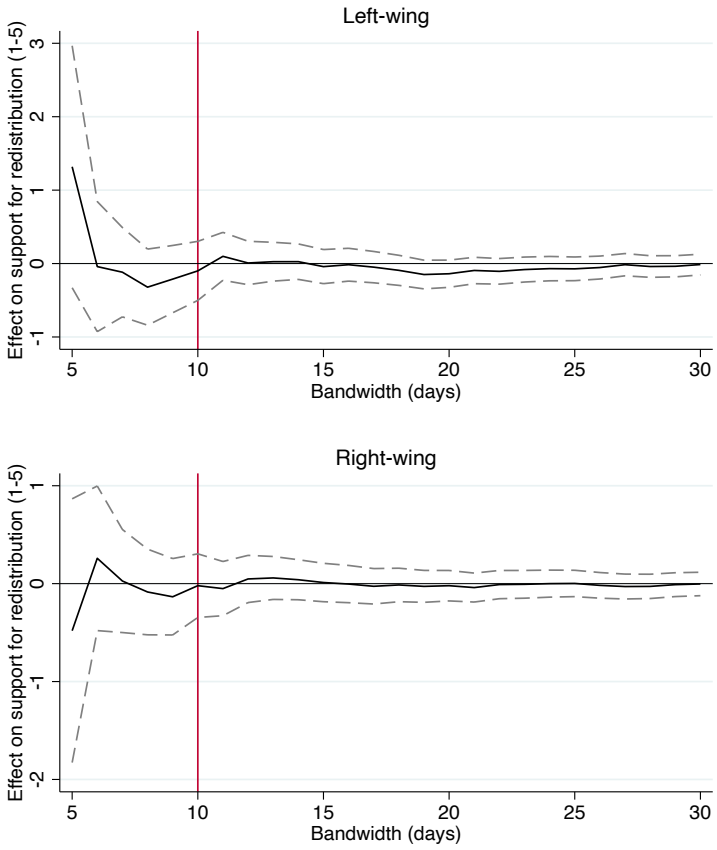
Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' support for redistribution by respondents' level of interest in politics. Vertical lines represent 95% confidence intervals. Estimates are from our baseline model with 10-day bandwidths, fitted separately on each level of political interest. The three days prior to the tax day are excluded from the analysis. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly).

Figure A16: Effect of tax day on support for redistribution (partisans vs non-partisans)



Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' support for redistribution for varying bandwidths (days) around the tax day. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly). The results are presented separately for partisans (top panel) and non-partisans (bottom panel). Partisanship is measured the with the following item: "Is there a particular political party you feel closer to than all the other parties?" (Yes = 1; No = 0). The red vertical line marks the default bandwidth of 10 days around the tax day. All models control for individuals' household income rank (in deciles).

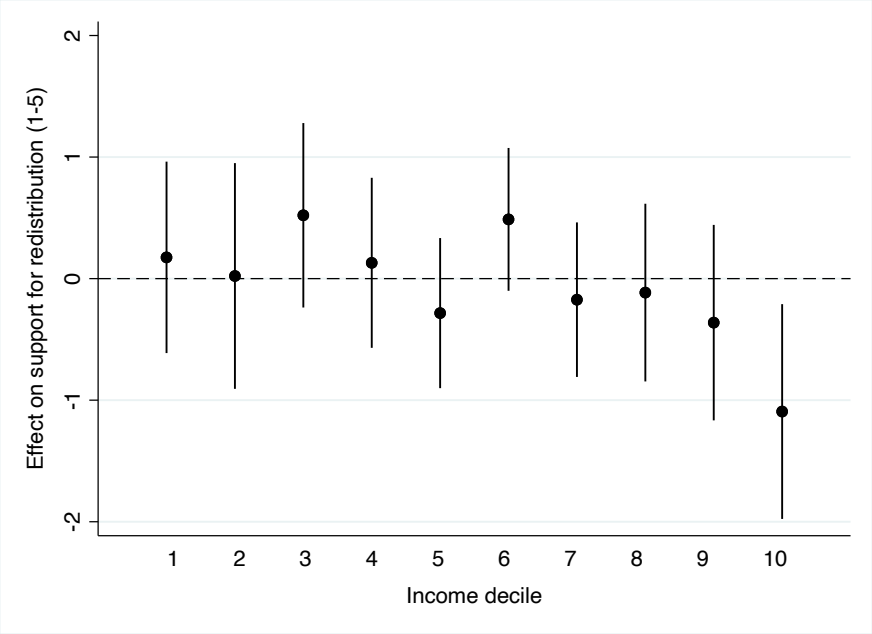
Figure A17: Effect of tax day on support for redistribution (left- vs right-wing respondents)



Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' support for redistribution for varying bandwidths (days) around the tax day. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly). The results are presented separately for left-wing (top panel) and right-wing respondents (bottom panel). A respondent is left-wing if she scored 4 or lower on a 0-10 left-right self-placement scale, and right-wing if she score 6 or higher. The red vertical line marks the default bandwidth of 10 days around the tax day. All models control for individuals' household income rank (in deciles).

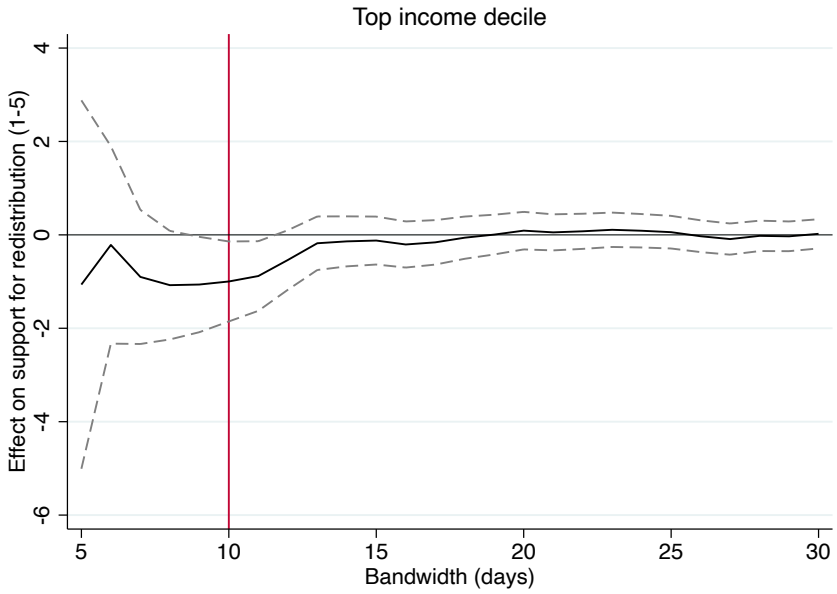
E Robustness checks

Figure A18: Effect of the tax day on support for redistribution by income decile (with day-of-the-week fixed effects)



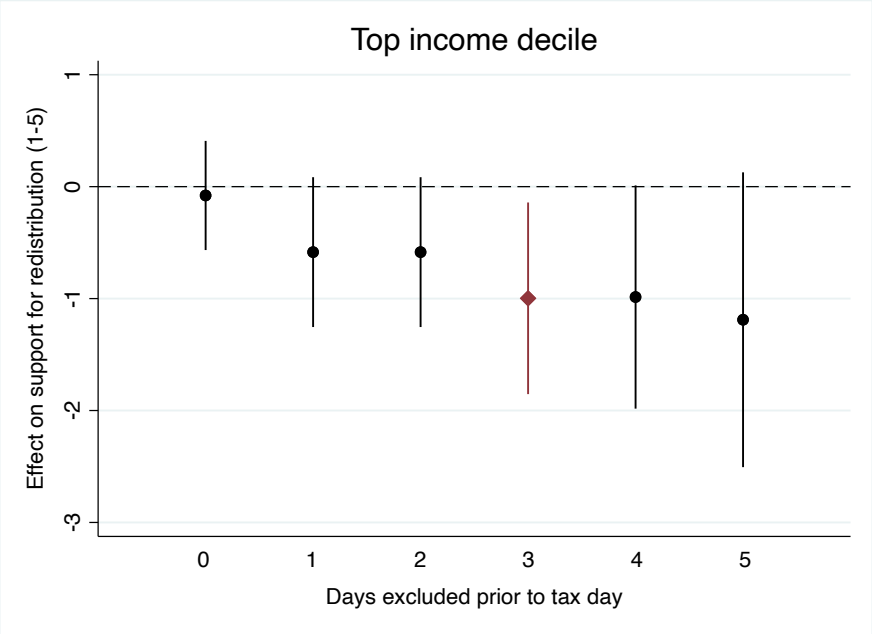
Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' support for redistribution by income decile. Estimates are from our baseline model with 10-day bandwidths and day-of-the-week fixed effects, fitted separately on each income decile. Vertical lines represent 95% confidence intervals. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly).

Figure A19: Effect of the tax day on support for redistribution amongst the top income decile (varying bandwidths)



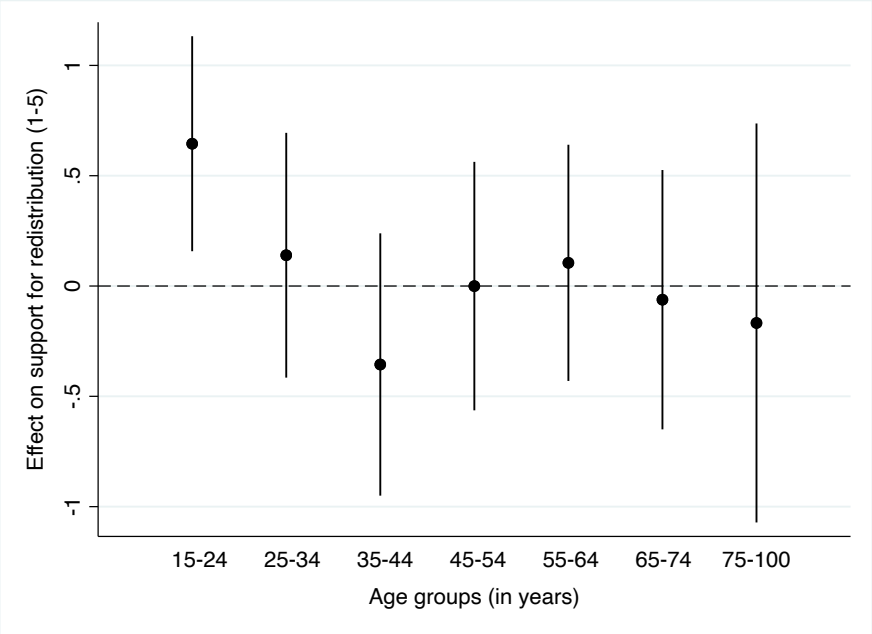
Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on support for redistribution in the top income decile for varying bandwidths (days) around the tax day. Support for redistribution measures to extent to which respondents agree that the government should take measures the reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly). The red vertical line marks the default bandwidth of 10 days around the tax day.

Figure A20: Effect of the tax day on support for redistribution amongst the top income decile (alternative exclusion windows prior to the tax day)



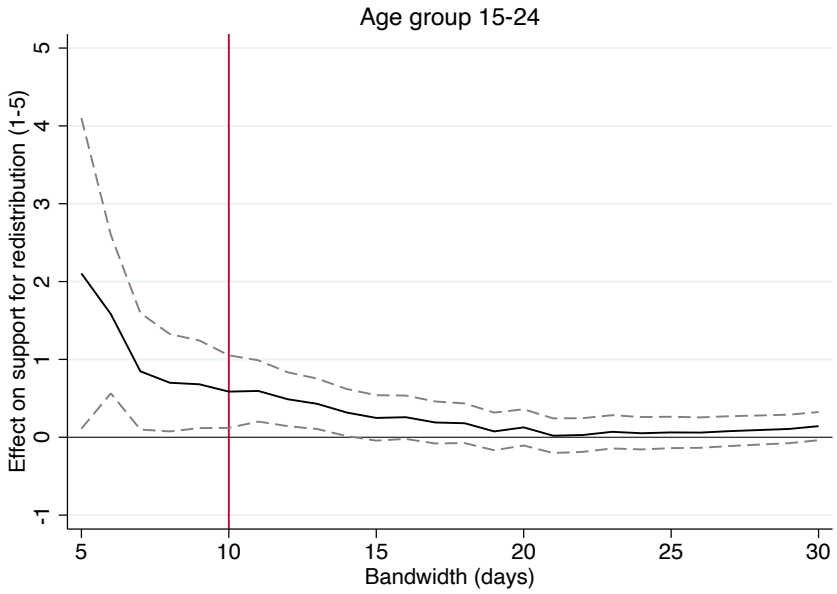
Data: ESS Finland 2002-18. *Note:* Point estimates (with 95% confidence intervals) show the effect of the tax day on support for redistribution depending on how many days prior to the event are excluded from the analysis. The default is 3 days and highlighted in red. Estimates are based on OLS with survey-year fixed effects. The sample is restricted to the top income decile and a 10-day window around the tax day in a given year. Support for redistribution captures the extent to which respondents agree that the government should take measures to reduce differences in income levels (1-disagree strongly, 5-agree strongly).

Figure A21: Effect of the tax day on support for redistribution by age group (with day-of-the-week fixed effects)



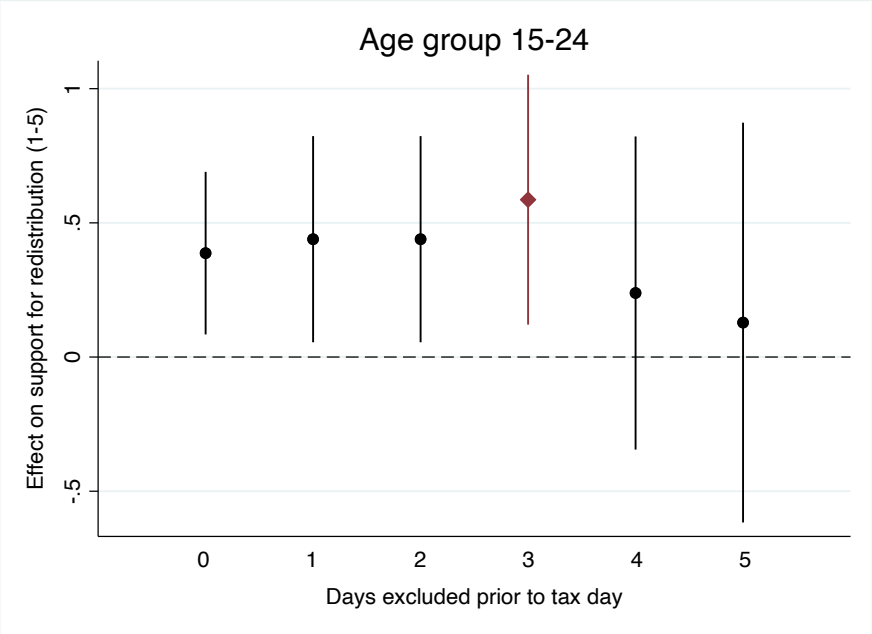
Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' support for redistribution by age group, using 9-year bins. Estimates are from our baseline model with 10-day bandwidths and day-of-the-week fixed effects, fitted separately on each age group. Vertical lines represent 95% confidence intervals. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly).

Figure A22: Effect of the tax day on support for redistribution amongst the the youngest age group (varying bandwidths)



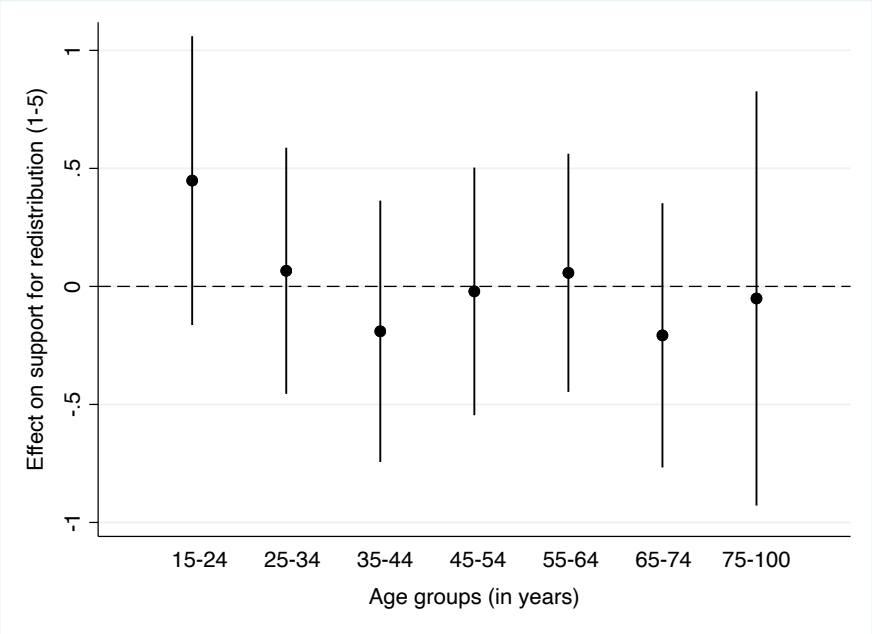
Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on support for redistribution in the youngest age group (15-24 years) for varying bandwidths (days) around the tax day. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly). The red vertical line marks the default bandwidth of 10 days around the tax day.

Figure A23: Effect of the tax day on support for redistribution amongst the youngest age group (alternative exclusion windows prior to the tax day)



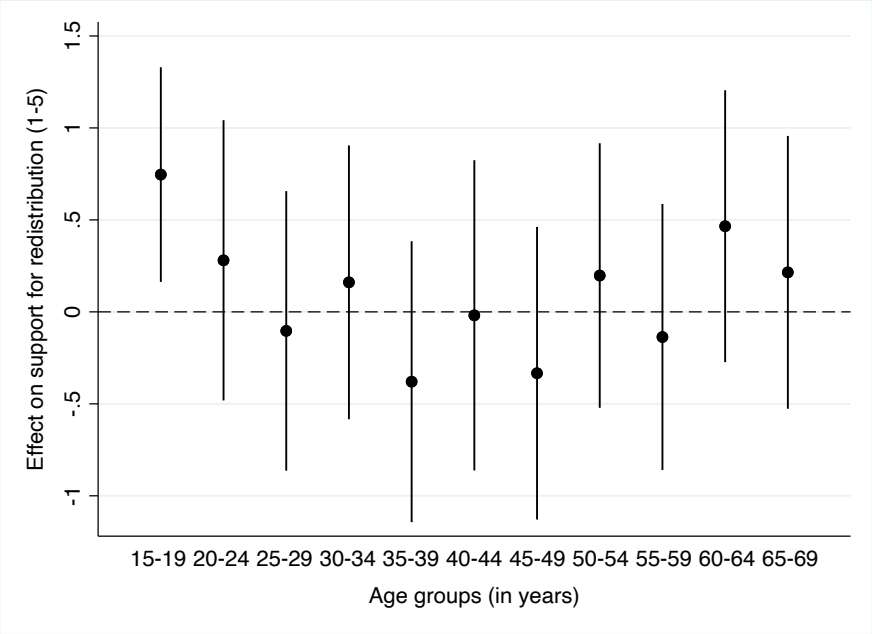
Data: ESS Finland 2002-18. *Note:* Point estimates (with 95% confidence intervals) show the effect of the tax day on support for redistribution depending on how many days prior to the event are excluded from the analysis. The default is 3 days and highlighted in red. Estimates are based on OLS with survey-year fixed effects. The sample is restricted to the youngest age group (15-25 years) and a 10-day window around the tax day in a given year. Support for redistribution captures the extent to which respondents agree that the government should take measures to reduce differences in income levels (1-disagree strongly, 5-agree strongly).

Figure A24: Effect of the tax day on support for redistribution by age group (controlling for household income)



Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' support for redistribution by age group, controlling for respondents' household income rank (in deciles). Vertical lines represent 95% confidence intervals. Estimates are from our baseline model with 10-day bandwidths, fitted separately on each age group. The three days prior to the tax day are excluded from the analysis. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly).

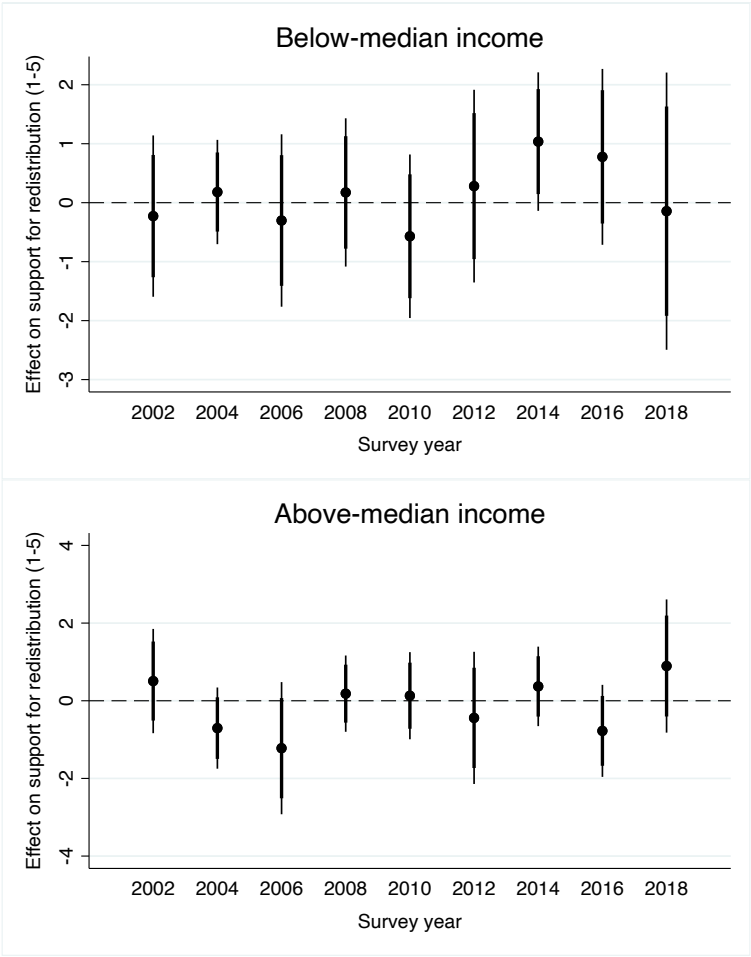
Figure A25: Effect of the tax day on support for redistribution by age group (4-year bins)



Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' support for redistribution by age group, using 4-year bins. Vertical lines represent 95% confidence intervals. Estimates are from our baseline model with 10-day bandwidths, fitted separately on each age group. The three days prior to the tax day are excluded from the analysis. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly).

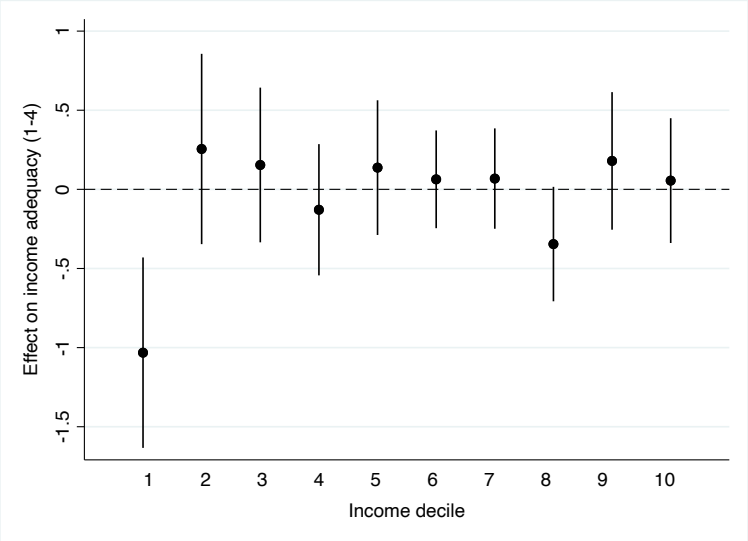
F Supplementary figures on mechanisms

Figure A26: Effect of tax day on support for redistribution (estimated separately for each survey year)



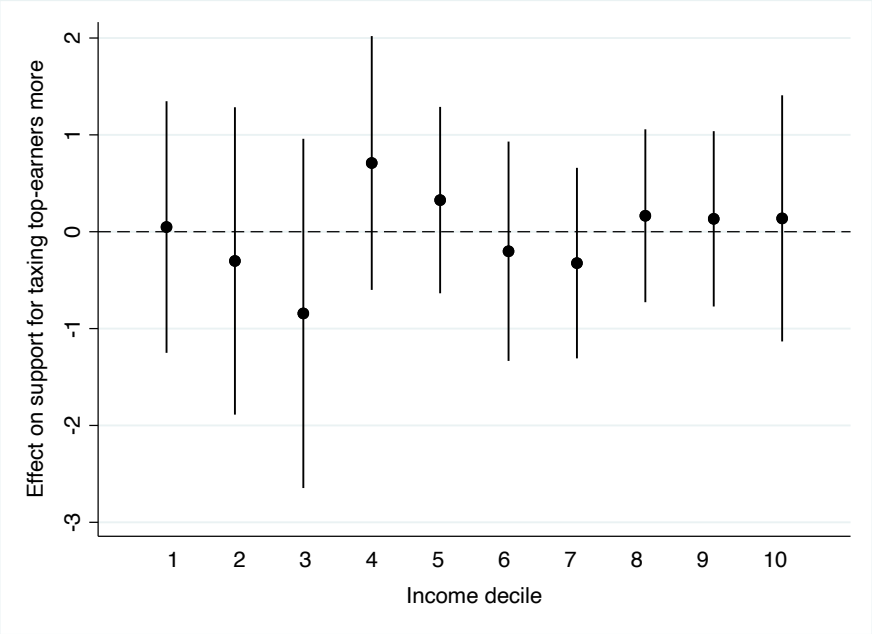
Data: ESS Finland 2002-18. *Note:* Point estimates show the effect of the tax day on support for redistribution, estimated separately for each available survey year using OLS. Vertical lines represent 95% (thick) and 99% (thin) confidence intervals. The sample is restricted to a 10-day window around the tax day in a given year and the 3 days prior to the tax day are excluded. The top panel shows estimates for below-median income earners and the bottom panel shows estimates for the above-median income earners. Support for redistribution captures the extent to which respondents agree that the government should take measures to reduce differences in income levels (1-disagree strongly, 5-agree strongly).

Figure A27: Effect of the tax day on perceived income adequacy by income decile



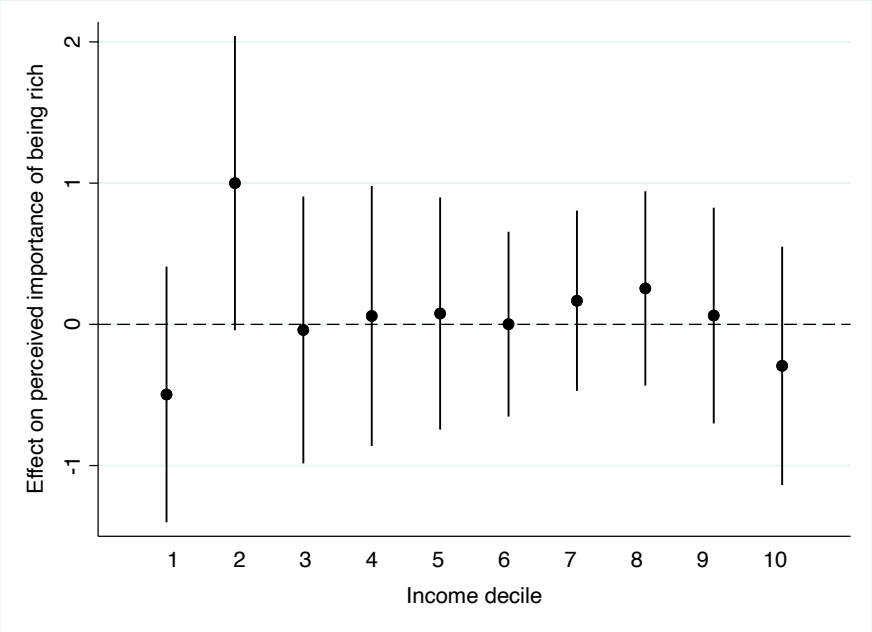
Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' perceived income adequacy by income decile. Vertical lines represent 95% confidence intervals. Estimates are from our baseline model with 10-day bandwidths, fitted separately on each income decile. The three days prior to the tax day are excluded from the analysis. Perceived income adequacy captures how respondents feel about their household's income nowadays, with response options ranging from 1 (finding it very difficult) to 4 (living comfortably).

Figure A28: Effect of the tax day on support for taxing high-earners more by income decile



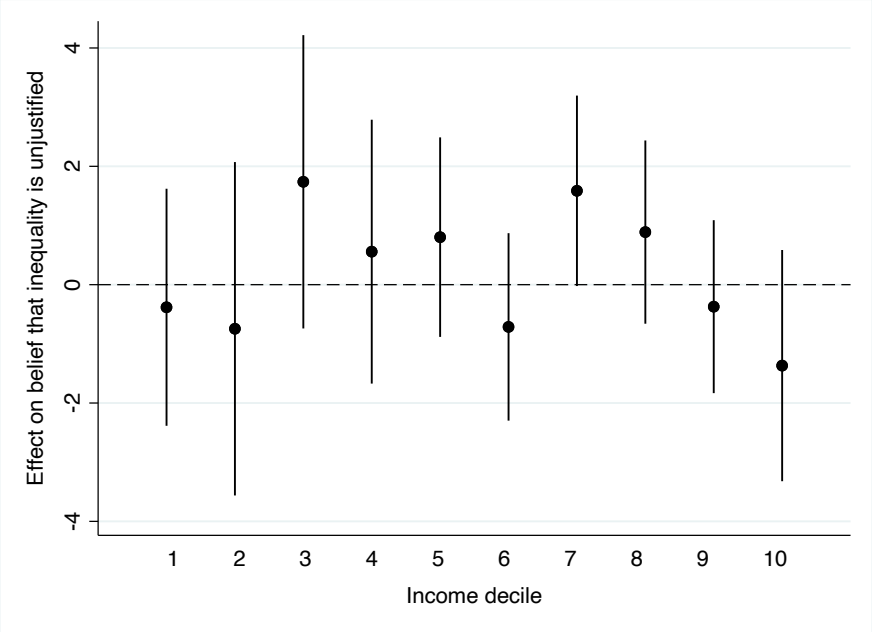
Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' support for taxing top earners more by income decile. Vertical lines represent 95% confidence intervals. Estimates are from an OLS model with 10-day bandwidths, fitted separately on each income decile. The three days prior to the tax day are excluded from the analysis. To increase precision of the estimates, we include controls for age, gender, education, and labour market status (in paid job, unemployed, student, retired, doing housework). Support for taxing high-earners more is a binary variable which records whether respondents agree that "higher earners should pay a higher share (a higher %) of their earnings in taxes".

Figure A29: Effect of the tax day on perceived importance of being rich by income decile



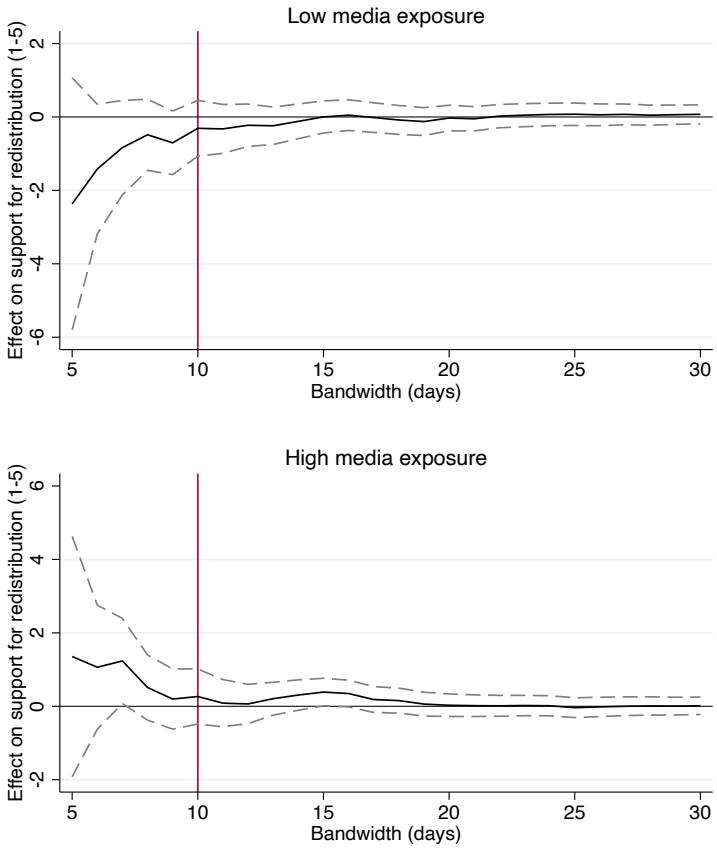
Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' perception that being rich is important by income decile. Vertical lines represent 95% confidence intervals. Estimates are from an OLS model with 10-day bandwidths, fitted separately on each income decile. The three days prior to the tax day are excluded from the analysis. To increase precision of the estimates, we include controls for age, gender, education, and labour market status (in paid job, unemployed, student, retired, doing housework). Perceived importance of being rich measures how much respondents' think they are like a person who values being rich, having money and expensive things. Response options range from 1 (very much like me) to 6 (not at all like me).

Figure A30: Effect of the tax day on belief that inequality is unjustified by income decile



Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' belief that inequality is unjustified by income decile. Vertical lines represent 95% confidence intervals. Estimates are from an OLS model with 10-day bandwidths, fitted separately on each income decile. The three days prior to the tax day are excluded from the analysis. To increase precision of the estimates, we include controls for age, gender, education, and labour market status (in paid job, unemployed, student, retired, doing housework). Belief that inequality is unjustified measures respondents' support for the statement that "large differences in people's incomes are acceptable to properly reward differences in talents and efforts." Response options range from 1 (agree strongly) to 8 (disagree strongly) so that higher values reflect more left-wing beliefs.

Figure A31: Effect of the tax day on support for redistribution (low vs high media exposure)



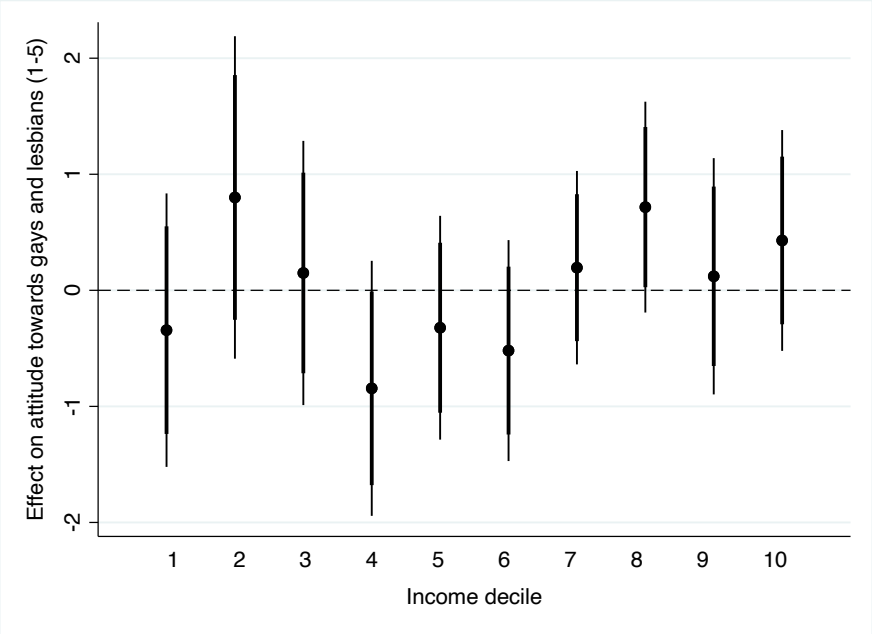
Data: ESS Finland 2018. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' support for redistribution for varying bandwidths (days) around the tax day. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly). The results are presented separately for respondents with high media exposure (>60 minutes per day) and low media exposure (<60 minutes per day). Media exposure refers to how much time respondents spend on a typical day watching, reading or listening to news about politics and current affairs (in minutes). The red vertical line marks the default bandwidth of 10 days around the tax day.

G Placebo tests

We conduct several placebo tests to assess the plausibility of the excludability assumption (see Muñoz et al. 2020). First, we show that the tax day has no significant effects on a placebo outcome (attitudes towards gays and lesbians) that is conceptually close to our main outcome of interest, but should in theory remain unaffected by the event.[37] We find null effects regardless of whether we disaggregate the analysis by income decile (Figure A32) or age group (Figure A33). Second, to rule out that global shocks or time trends are behind our findings, we run the main analysis on ESS respondents from Sweden, who should in theory remain unaffected by the tax day. Reassuringly, we find null effects regardless of whether we disaggregate the analysis by income decile (Figure A34) or age group (Figure A35). Third, to check for problematic time trends, we conduct a placebo treatment analysis, where we impose "fake" tax days for up to 50 days before and after the actual tax day. We find null effects throughout, which suggests that there are no anticipatory effects of the tax day on support for redistribution (Figure A36). Finally, we follow Muñoz et al. (2020) and split the control group sub-sample (who were interviewed before the tax day) at its empirical mean (23 days before the tax day) and test for the absence of a placebo effect at that point. Again, we find null effects regardless of whether we disaggregate the analysis by income decile (Figure A37) or age group (Figure A38).

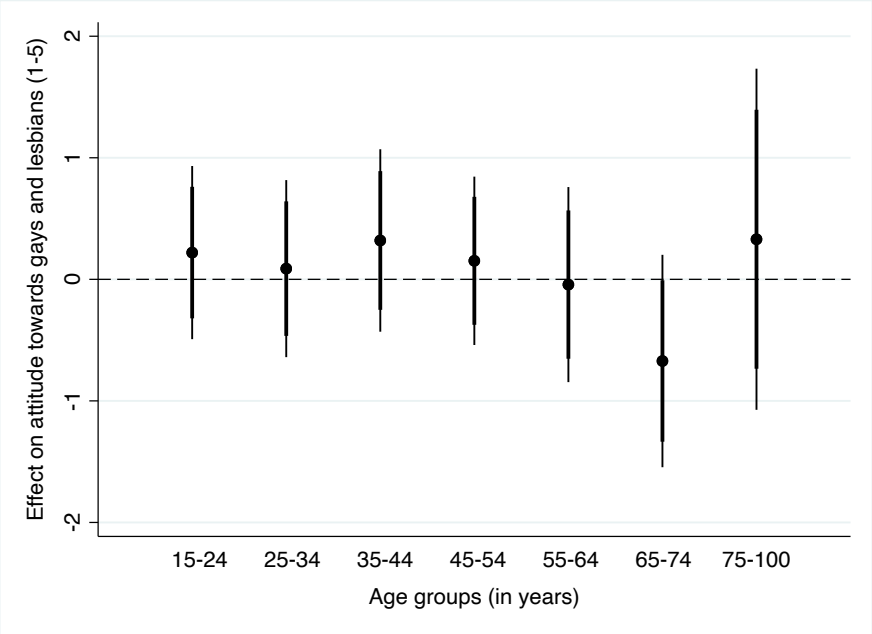
[37] Support for redistribution reflects the classical left-right dimension in politics, whilst attitudes towards gays and lesbians taps into the "second" dimension of socio-cultural politics.

Figure A32: Placebo test - Effect of the tax day on attitude towards gays and lesbians by income decile



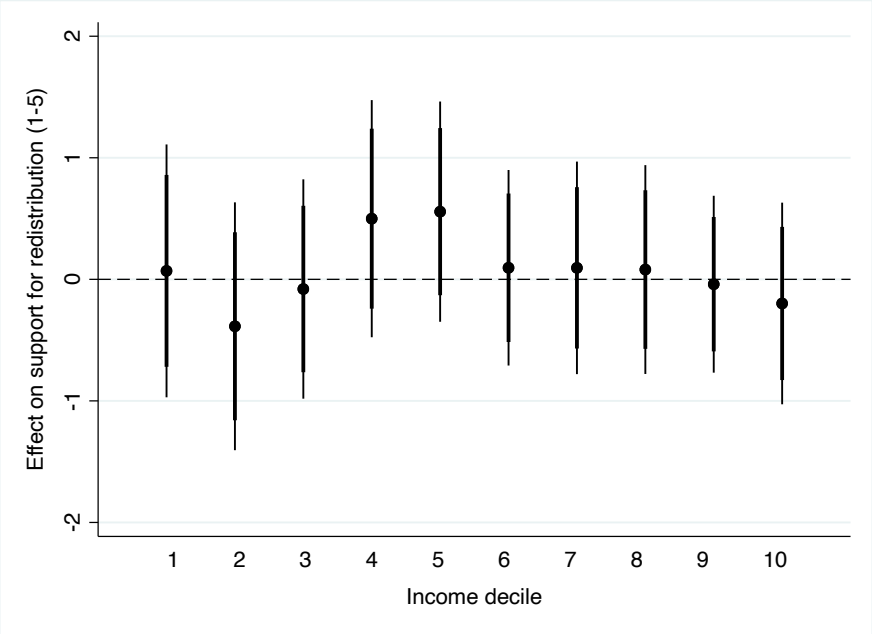
Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' attitude towards gays and lesbians by income decile. Vertical lines represent 95% (thick) and 99% (thin) confidence intervals. Estimates are from an OLS model with 10-day bandwidths, fitted separately on each income decile. The three days prior to the tax day are excluded from the analysis. Attitude towards gays and lesbians measures respondents' agreement with the statement that "gay men and lesbians should be free to live their own life as they wish". Response options range from 1 (Disagree strongly) to 5 (Agree strongly).

Figure A33: Placebo test - Effect of the tax day on attitude towards gays and lesbians by age group



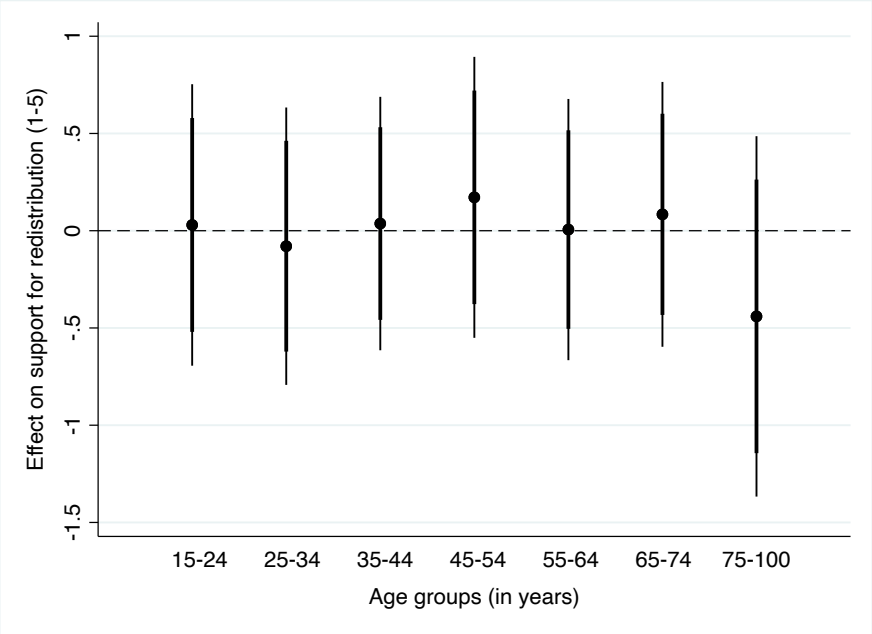
Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' attitude towards gays and lesbians by age group. Vertical lines represent 95% (thick) and 99% (thin) confidence intervals. Estimates are from an OLS model with 10-day bandwidths, fitted separately on each age group. The three days prior to the tax day are excluded from the analysis. Attitude towards gays and lesbians measures respondents' agreement with the statement that "gay men and lesbians should be free to live their own life as they wish". Response options range from 1 (Disagree strongly) to 5 (Agree strongly).

Figure A34: Placebo test - Effect of the tax day on support for redistribution amongst Swedish ESS respondents (by income decile)



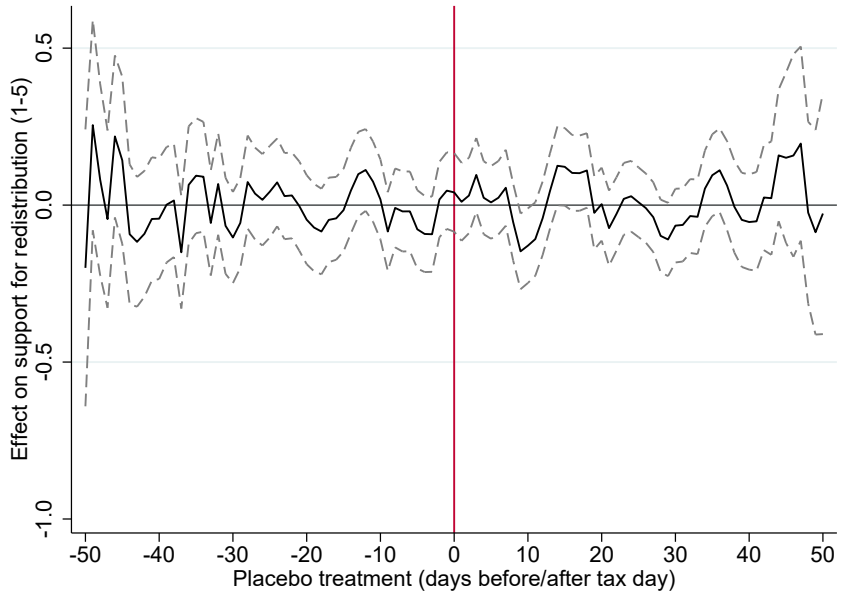
Data: ESS Sweden 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on Swedish respondents' support for redistribution by income decile. Vertical lines represent 95% (thick) and 99% (thin) confidence intervals. Estimates are from our baseline model with 10-day bandwidths, fitted separately on each income decile. The three days prior to the tax day are excluded from the analysis. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly).

Figure A35: Placebo test - Effect of the tax day on support for redistribution amongst Swedish ESS respondents (by age group)



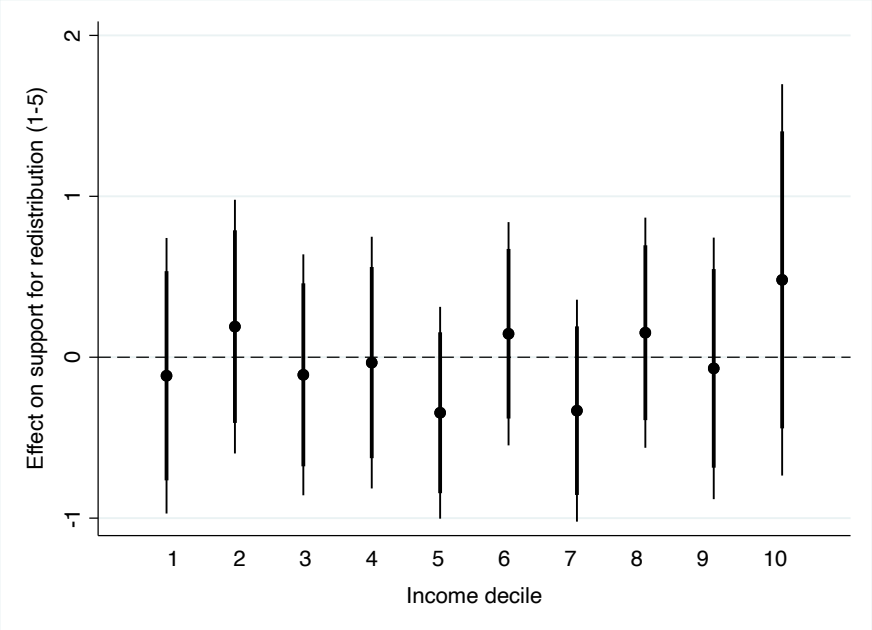
Data: ESS Sweden 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on Swedish respondents' support for redistribution by age group. Vertical lines represent 95% (thick) and 99% (thin) confidence intervals. Estimates are from our baseline model with 10-day bandwidths, fitted separately on each age group. The three days prior to the tax day are excluded from the analysis. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly).

Figure A36: Placebo test - Effect of fake tax days on support for redistribution (whole sample)



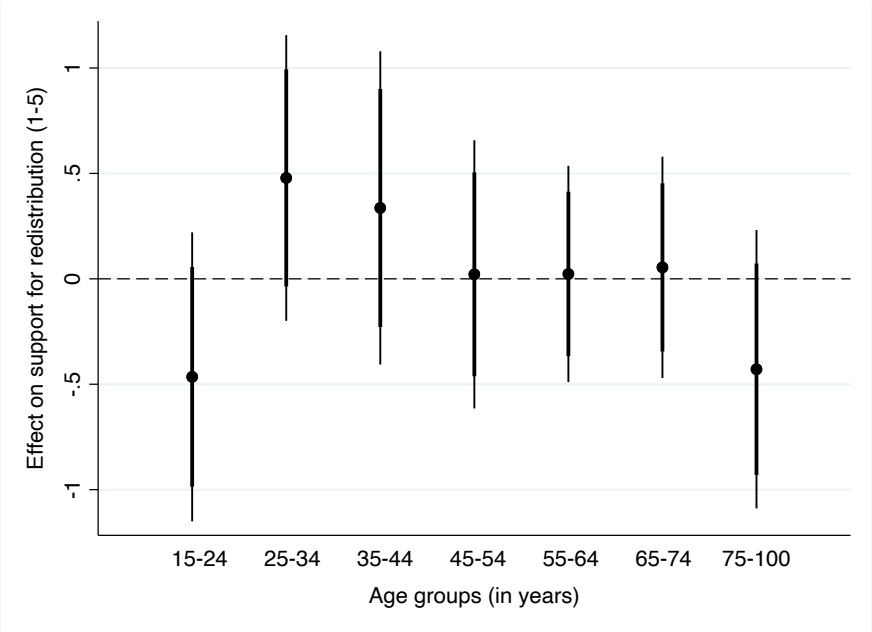
Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of fake tax days (the coefficient on the *Treatment* indicator) on respondents' support for redistribution for up to 50 days before and after the actual tax day. Dotted lines represent 95% confidence intervals. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly).

Figure A37: Placebo test - Effect of fake tax day on support for redistribution by income decile



Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of a fake tax day (the coefficient on the *Treatment* indicator) on respondents' support for redistribution by income decile. Vertical lines represent 95% (thick) and 99% (thin) confidence intervals. The fake tax day is located at the empirical mean (23 days before the tax day) of the control group (who were interviewed before the tax day). The sample is restricted to respondents in the control group. Estimates are from our baseline model with 10-day bandwidths, fitted separately on each income decile. The three days prior to the fake tax day are excluded from the analysis. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly).

Figure A38: Placebo test - Effect of fake tax day on support for redistribution by age group



Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of a fake tax day (the coefficient on the *Treatment* indicator) on respondents' support for redistribution by age group. Vertical lines represent 95% (thick) and 99% (thin) confidence intervals. The fake tax day is located at the empirical mean (23 days before the tax day) of the control group (who were interviewed before the tax day). The sample is restricted to respondents in the control group. Estimates are from our baseline model with 10-day bandwidths, fitted separately on each age group. The three days prior to the fake tax day are excluded from the analysis. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly).

The **Aboa Centre for Economics (ACE)** is a joint initiative of the economics departments of the Turku School of Economics at the University of Turku and the School of Business and Economics at Åbo Akademi University. ACE was founded in 1998. The aim of the Centre is to coordinate research and education related to economics.

Contact information: Aboa Centre for Economics, Department of Economics, Rehtorinpellonkatu 3, FI-20500 Turku, Finland.

www.ace-economics.fi

ISSN 1796-3133