A False Promise for the Left Behind: The Uneven Political Impact of Place-Based Policies

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Abstract

To counter regionally concentrated political frustration, place-based economic policies targeting disadvantaged regions have become increasingly popular among policymakers. This paper examines the effectiveness of place-based policies in mitigating political discontent with a focus on their distributional effects within regions. We argue that the firm-level allocation of place-based funds implies an unequal pattern of exposure: voters with senior-level occupations, higher incomes, and higher educational attainment are most likely to benefit directly. The political effects of such policies are thus concentrated in these groups, while other voters are affected less strongly. Empirically, we study the world's largest place-based policy in the context of the European Union (EU) using both a natural experiment and a randomized survey experiment. First, we construct a 1990-2024 dataset at the individual level (N=1,400,000) across the EU by harmonizing *Eurobarometer* surveys with subnational geocodes and leverage a discontinuity in regional funding eligibility in a regression discontinuity (RD) design. Second, we provide experimental evidence from a pre-registered survey in Germany (N=2,000), in which we present respondents with information about the policy and analyze their responses to closed-ended and open-ended questions. Both analyses show that place-based policies reduce political discontent only among voters with senior-level occupations, high incomes, and high levels of education. Since political frustration is mostly concentrated in other groups, these results highlight how distributional effects limit the effectiveness of place-based policies in addressing political discontent.

Keywords: inequality, place-based policies, political discontent

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1 Introduction

Recent election results in advanced democracies have demonstrated that political discontent is often regionally concentrated. A growing literature has identified a "geography of discontent" in which extremist and populist voting clusters in regions that are economically left-behind (Broz et al., 2021; Ejrnæs et al., 2024; Guriev & Papaioannou, 2022; Rodríguez-Pose, 2018). To counter such regionally concentrated political frustration, place-based economic policies targeting disadvantaged regions have become increasingly popular among policymakers. In the United States, the *CHIPS and Science Act* and the *Inflation Reduction Act* have added new place-based resources to the country's multiple place-based industrial policy programs (Muro, 2023). In the European Union (EU), the *Cohesion Policy* represents the world's largest place-based policy. Although many countries now deploy more place-based spending than ever before, political discontent and anti-establishment voting remains firm in many supported regions.

The evidence on whether place-based policies actually affect political discontent and political behavior more broadly is surprisingly thin.[1] Some studies find them to affect regional voting outcomes (Borz et al., 2022; Gold & Lehr, 2024; Rodríguez-Pose & Dijkstra, 2021), others find only insignificant links (Bayerlein & Diermeier, 2025; Crescenzi et al., 2020; Fidrmuc et al., 2019). While the most recent studies have made substantial progress on understanding the political effects of place-based public spending, the evidence so far focuses mainly on aggregate regional outcomes. An emerging strand in the literature on regional inequality, however, stresses that socio-economic inequalities *within* disadvantaged regions are stark (Bauluz et al., 2023; Königs et al., 2023; Lang et al., 2022). At the same time, place-based policies are typically too small in scale to reach everyone within supported regions and thus have *uneven* economic effects in these regions (Bartik, 2020a; Lang, 2024; Reynolds & Rohlin, 2015). In light of these findings, understanding the political effects of place-based policies requires going beyond the aggregate.

This is why, in this paper, we provide new theory and evidence on the microfoundations of the political effects of place-based policies. Because of their limited region-wide economic effects, substantial sociotropic effects on the political attitudes voters without direct exposure to the policy are unlikely.

[1] The literature review by Guriev and Papaioannou (2022) concludes that "relatively little work has been done on using place-based policies to prevent the rise of populism" (p. 818).

Instead, we argue that place-based policies are more likely to affect opinions of voter groups that are more directly exposed to them. As firms rather than individuals are eligible to apply for place-based funds, their firm-level allocation implies an unequal pattern of exposure. Across firms, place-based funds are more likely to reach the most productive firms. Within firms, the managerial level is more directly involved than rank-and-file staff. We thus hypothesize that place-based policies are more likely to cause changes in political attitudes among voters with senior-level occupations, higher incomes and higher educational attainment.

We test our argument with both a natural experiment and a survey experiment in the context of the world's largest place-based policy, the EU Cohesion Policy. This empirical setting allows us to evaluate effects for a long period of time across multiple countries while providing a consistent operationalization of political discontent: Euroscepticism. First, we assemble a 1990-2024 panel of EU-wide georeferenced survey data by combining existing waves of the *Eurobarometer* and harmonizing geographic identifiers covering more than 1.4 million survey respondents. Leveraging a discontinuity in access to place-based funding, we estimate its effect on political attitudes across different subsets of voters with an RD design. Second, we conduct a pre-registered and geo-coded survey experiment in Germany (N=2,046), in which we expose voters to an information treatment on the place-based policy while collecting detailed data on socio-economic variables and relevant political attitudes.

Empirical results across the two analyses provide consistent evidence for profoundly uneven effects of place-based policies on political discontent. Overall, they reduce political discontent, but only among voters in senior-level occupations as well as highly-skilled and high-income voters. Political attitudes in most other voter groups are barely affected. Our analysis of mechanisms shows that an absence of perceived personal benefit among low-income and low-skilled workers is an important channel. Overall, the results are consistent with voter responses to place-based policies that are *egotropic* rather than *sociotropic*. For voters' political attitudes to be affected by such regional policies, they need to perceive them as beneficial for them personally and not just for their home region.

Against the backdrop of large inequalities within disadvantaged regions, these results point to serious limitations of place-based policies as tools to counter political frustration. The policy fails to increase support for those groups of voters who feel left behind the most and, instead, further reduces discontent among those groups whose political discontent is already limited.

2 Theory

2.1 Regional inequality and political discontent

In recent years, democracies worldwide have experienced a surge of political discontent among their electorate (O'Rourke, 2019). In most countries, such political discontent is regionally concentrated. In particular, former industrial regions that have experienced economic decline have become prone to support populist, anti-establishment politicians that give a voice to voters that are frustrated with mainstream politics (Broz et al., 2021; Colantone and Stanig, 2018b). As underlying root causes for such regional economic decline, scholars have identified the distributional effects of import competition, automation and digitization, structural change more broadly as well as austerity policies (Autor et al., 2013, 2020; Baccini & Sattler, 2025a; Colantone & Stanig, 2018b). This phenomenon of concentrated discontent in economically disadvantaged regions — sometimes labeled as the "places that don't matter" (e.g Rodríguez-Pose, 2018) or "left-behind places" (e.g MacKinnon et al., 2022) — has been observed not only in the United States (Morgan and Lee, 2018), but also in the UK (Colantone and Stanig, 2018a, Becker et al., 2017), in Italy (Urso et al., 2023) and in the EU more broadly (Rodríguez-Pose, 2018).

One reason for this pattern is that many of the individual-level determinants of political discontent are clustered in these regions. Economically disadvantaged areas are often home to older, less-educated, and poorer populations who are more vulnerable to economic shocks resulting from import competition, financial crises, austerity, migration, and technological change (Ahlquist et al., 2020; Baccini & Sattler, 2025a; Dustmann et al., 2019; Goodwin & Heath, 2016). Additionally, voters in these regions, which are often rural and peripheral, tend to uphold traditional values and social hierarchies. As a result, they are more sensitive to perceived social status loss in the face of shifting societal norms and hierarchies that often accompany structural change (Gidron & Hall, 2019; Kurer, 2020). The literature on anti-establishment voting has identified these individual-level characteristics – age, education, income, exposure to economic shocks, social status loss – as significant predictors of political frustration (Algan et al., 2017; Baute & Tober, 2024; Hobolt, 2016; Kriesi et al., 2008; Kurer & Van Staalduinen, 2022; Vasilopoulou, 2016; Ziblatt et al., 2024)

Beyond, individual-level factors, the recent literature increasingly emphasizes the role of aggregate

and structural determinants as important drivers for the rise of populist attitudes (Rickard, 2020; Ballard-Rosa et al., 2021; Alabrese et al., 2019; Albrecht, 2022; Dijkstra et al., 2020). Building on the theory of sociotropic voting, which argues that vote choice depends on voters' assessment of the national economy, a recent literature has found that voters also pay attention to regional economic developments when making vote choices (Rickard, 2020; Johnston and Pattie, 2001, Veiga and Veiga, 2010, Simonovits et al., 2019 ; Huijsmans, 2023; Cremaschi et al., 2025). Relatedly, local deprivation in public services can also fuel political discontent Cremaschi et al. (2024). As public services deteriorate as a result of regional economic decline and lower local tax revenues, this is another channel through which regional economic inequalities drive political discontent (Baccini and Sattler, 2025b; Cremaschi et al., 2024). Against this backdrop, the rise in inequality across regions – which a growing literature in regional economics has documented (Bauluz et al., 2023; Königs et al., 2023; von Ehrlich and Overman, 2020 – may at least partly explain the recent surge in regionally concentrated discontent.

2.2 Political discontent and place-based policies

Over the last years, place-based policies have emerged as a prominent strategy among policymakers seeking to address the economic challenges faced by disadvantaged regions. These policies are frequently conceptualized as a viable approach to enhancing the economic well-being of citizens. By supporting local businesses, bolstering local infrastructure and incentivizing local investments, they aim to address specific local needs. Many researchers have engaged with the question of whether place-based policies indeed can improve local economies in contexts such as the US (Reynolds and Rohlin, 2014, Bartik, 2020a), the UK (Crescenzi et al., 2020, Criscuolo et al., 2019), in Germany (Henkel et al., 2021), in France (Givord et al., 2013) and the EU (Bachtrögler-Unger et al., 2023, Becher and Donnelly, 2013). Multiple studies now find positive average effects of place-based policies on economic outcomes such as GDP (Bachtrögler-Unger et al., 2023), income (Reynolds and Rohlin, 2014) and jobs (Bartik, 2020a). The estimated effects, however, are typically moderate in size and short-lived. Estimates of fiscal multipliers in this literature are often close to one (Becker et al., 2010, 2018; Lang et al., 2022) and estimates of costs per job are close to average wage levels in the respective countries (Busso et al., 2013; Criscuolo et al., 2019). This implies that regional economic gains are of similar magnitude as the

amount of input resources. Except in large-scale "big-push" programs, positive effects are often found to vanish once the policies are discontinued, suggesting an absence of transformational long-term effects (Criscuolo et al., 2019; Garin & Rothbaum, 2025; Kline & Moretti, 2014).

In light of the growing interest among policy-makers in the potential of place-based funds as a tool to support left-behind places, scholars have also sought to understand whether such strategies can effectively address local discontent of such regions. The scholarly debate on whether place-based policies are successful in doing so is ongoing. Several studies find that place-based policies are able to curb the surge of populist voting (Rickard, 2023; Jacobs and Munis, 2019;Gold and Lehr, 2024; Rodríguez-Pose and Dijkstra, 2021; Borz et al., 2022; Ward et al., 2025; Heddesheimer et al., 2024). Other studies on the same question, however, find either mixed or insignificant effects (e.g López-Bazo, 2022, Crescenzi et al., 2020, Fidrmuc et al., 2019, Bayerlein and Diermeier, 2022). One interpretation of this aggregate evidence is that there is considerable variability in the political effects of these policies.

Our theory is an attempt to reconcile these findings in the existing literature. By going beyond the aggregate and studying heterogeneous individual-level effects, we show how positive economic effects can coexist with mixed political effects.

2.3 The uneven effects of place-based policies

The starting point of our argument builds on a recent observation in the literature on regional inequalities. More granular data has helped an emerging strand in this literature to go beyond measuring inequality across regions and also study inequality *within* regions. A key finding in this literature is that inequality within regions is large and a much larger component of overall inequality than inequality across regions. While research on this question has so far focused on European and North American democracies, this pattern holds for all countries that have so far been studied. In most countries, inequality within regions accounts for about 95 percent of overall inequality. Bauluz et al. (2023) find this for the United States, the United Kingdom, Germany, France and Canada. Königs et al. (2023) find similar results for Austria, Belgium, Hungary, Italy, and Slovakia. Their results across countries are consistent with earlier, pioneering work for the UK (Gibbons et al. 2014) and France (Combes et al. 2008) as well as with a study on intra-regional inequality in EU regions (Lang et al., 2022).

In this regard, the literature emphasizes that the effects of place-based policies are conditional on other factors. For instance, findings suggest that, in the case for the UK, place-based policies were successful in decreasing support for Brexit only if they were successful in improving local labor markets (Crescenzi et al., 2020). Furthermore, local demands and the customization of place-based policies matter for political outcomes (Capello and Perucca, 2018). Finally, research on the economic effects of place-based policies suggests that economic outcomes are heterogeneous, and positively biased towards more affluent citizens (Reynolds and Rohlin, 2015, Becher and Donnelly, 2013, Lang et al., 2022). This resonates with findings that larger and more productive firms are more likely to receive place-based policies in the first place (Criscuolo et al., 2019, Bachtrögler-Unger et al., 2023, Slattery and Zidar, 2020). Additionally, place-based policies seem to be more economically effective for regions with higher levels of education (Becker et al., 2013). These findings raise the question of whether place-based policies are an effective tool to decrease political discontent among citizens of all socio-demographic groups.

We argue that voters are more likely to react to place-based policies when they are immediately exposed and directly benefit from them. The greater the exposure to and gain from such policies, the more inclined citizens are to hold more favorable views on the authority responsible for distributing the place-based resources. But how do voters get exposed to place-based policies?

In contrast to people-based policies like social security schemes or family benefits, eligibility to place-based policies is determined by place rather than by individual-level attributes and the eligible entities in a given region are typically firms rather than individuals. While some place-based schemes such as "special economic zones" lower taxes and regulations for *all* firms in an eligible region, our focus in on place-based investments policies that only subsidize firms after they have successfully applied with an eligible project. Most place-based funding then comes in the form of co-financing of investments or tax credits because most place-based policies do not fund the full project volume. Voters are thus more likely to get in direct contact with place-based policies if they work for firms that successfully apply for place-based funding. At the level of the firm, this requires knowledge of the policy, human and financial resources needed for a (successful) application, and financial resources for co-financing the project. These prerequisites are more likely to be available to larger and more productive firms. In fact, larger firms often have specialized units focusing on attracting such funds and

there is evidence suggesting that larger firms receive more place-based funds (Bachtrögler et al., 2019; Bartik, 2020b; Slattery & Zidar, 2020).

But in addition to differences in exposure *across* firms, there is also an *intra*-firm component. Within firms, applications for and management of place-based funds are typically handled at the managerial level. In larger firms, upper management may be supported by teams of specialized highly-skilled employees. In smaller firms, business owners are typically most heavily involved. If proposals are successful, the most direct benefits in the form of increased resources, new responsibilities and larger teams thus accrue to managers and business owners and employees in leadership roles. In addition, supported firms often hire new employees as a result of the place-based subsidies and these new jobs can be at all levels of the intra-firm hierarchy. While such additional employees are also beneficiaries of the subsidies, firms often cannot (and do not) clearly connect specific hires to specific funding sources. Some employees may be hired in the context of a specific project that is explicitly connected to the place-based subsidy. But in general, newly hired employees do not necessarily know whether their job is among the marginally added jobs due to a specific subsidy.

As a result, we expect the benefits of place-based subsidies to be most directly felt by voters with specific profiles. They have management functions within their firms or are at the upper levels of a firm's hierarchy. Across firms, they work for more productive companies and thus receive higher wages, and are more highly skilled than average. By contrast, the benefits are less direct and visible for workers at lower levels of a firm's hierarchy and for employees of smaller, less productive firms. These firms typically offer lower incomes and employ a less skilled workforce. To the extent that place-based funds affect political attitudes, we thus hypothesize that these effects depend on occupation, income, and education. They will be concentrated among voters with more senior roles in firms, with higher incomes, and with higher skills. For voters with other jobs, lower incomes and lower skills, we expect more limited effects. We expect null effects for voters that are not part of the labor force.

3 Research Design I: EU-wide Observational Data

3.1 EU Cohesion Policy

To test our hypotheses, we study the world's largest place-based policy, the EU Cohesion Policy. It has been in place since 1989 and in its most recent funding period from 2021-2027 entails spending of up to 392 billion euros. Its main objective is to promote economic convergence across Europe's sub-national regions. It does so by co-financing projects proposed and implemented by private firms and public agencies. Eligibility for support is subject to the submission of an application to the relevant local authority, which is responsible for allocating its share of the funds. All EU member states and regions receive support under this scheme, but the amount of funding a region receives depends on its population size and its level of economic development. For instance, German regions receive a total of around 37 billion Euro for the funding period 2021-2027. In the same period, Italian regions receives around 73 billion Euro and regions in Poland up to 91 billion Euro. Funding is distributed across a broad spectrum of beneficiaries and project categories, and the success of these applications depends in part on the design of national operational programs. These programs needs to align with a national strategic reference framework that is designed by the member states and accepted by the European Commission.

EU Cohesion Policy provides a compelling case for examining the political effects of place-based policies, given its substantial scale and its role as a major redistributive instrument across European regions. Furthermore, the policy frequently has been framed as a tool to tackle political discontent across EU member states. Examples of political parties fueling hopes of the Cohesion Policy to mitigate political discontent include a recent press release of statements by S&D MPs of the European Parliament stating that "killing cohesion policy may only fuel the anti-European narrative of the extremists with their talks on how Europe does not think and contribute to improving the lives of all Europeans by limiting the disparities" (Socialists & Democrats Group, 2025). Also, in an interview the then-chair of the parliament's regional development committee Iskra Mihaylova (ALDE) stated: "Cohesion policy is the most powerful weapon to fight populism and to promote European integration and solidarity." (The Parliament Magazine, 2018).

We collect data on funding allocation at the so-called NUTS-2 level. NUTS-2 regions are subna-

tional units with 800.000 to 3 million inhabitants. They correspond, for instance, to the *regione* in Italy and to the *Regierungsbezirk* in Germany. While some EU-level spending is allocated at the member state level, we concentrate on those funding schemes that are allocated on the NUTS-2 level: the European Regional Development Fund (ERDF) and the European Social Fund (ESF). The volume of both funds that is allocated to regions depends on the regions' GDP per capita. According to an institutional rule that has been in place since 1989, regions whose GDP per capita is below 75 percent of the EU average receive substantially more resources than regions with a higher GDP per capita. This threshold creates a quasi-exogenous discontinuity in the volume of place-based funds across regions that a regression discontinuity (RD) design can leverage for causal identification.

This quasi-exogenous source of variation has first been leveraged by Becker et al. (2010) and has since been repeatedly used in the related literature (Becker et al., 2013, 2018; Borin et al., 2021; Gold & Lehr, 2024). In contrast to most of this literature, we do not use the statistics on regional GDP that are currently available from *Eurostat*. These figures have repeatedly been revised and do not necessarily correspond to the GDP figures that were available at the time the decision on funding eligibility was made. Instead, we use the historical data used by the European Commission for the eligibility decisions collected by Lang et al. (2022). This allows constructing a running variable that enables a cleaner identification of the effect at the threshold. Furthermore, the outcomes to which we are matching these data are not regional aggregates; rather, they are measured at the individual level.

3.2 Euroscepticism as a cross-national measure of political discontent

Political discontent is often understood as a multidimensional concept that consists of three main dimensions on which citizens can indicate discontent: They can be discontented by the political institutions, by their political actors, or by their performance (Ejrnæs et al., 2024). Another dimension concerns the level of governance, as these phenomena can manifest across multiple levels, including supranational, national, regional, and local contexts (ibid.). We focus on one specific dimension of political discontent: dissatisfaction with the political institution of the European Union. We do so by using a measure of EU support as a proxy for political discontent.

Why is this our preferred measure? First, and most importantly, examining EU support as a dimension of political discontent is most plausible for our design, as we investigate the EU's place-based

policies. Financial support by the EU should most likely change political discontent towards the EU. This way, we investigate the same level of governance on both sides of the equation.

Second, our approach treats the European Union as a political institution in its own right. This perspective is consistent with established scholarship on Euroscepticism, which commonly distinguishes between scepticism towards policies and scepticism towards the EU as an institution (e.g. Szczerbiak and Taggart, 2008). Similarly, more recent distinctions between regime and policy scepticism (Hobolt and De Vries, 2016; De Vries, 2018) conceptualize the European Union as a standalone political institution toward which citizens may develop discontent. In this literature, consequently, scholars frame EU attitudes as a dimension of political discontent (Fetzer, 2019; Rodríguez-Pose and Dijkstra, 2021; Abts and Baute, 2022; Krouwel and Abts, 2007).

Third, recent scholarship finds a strong association between skepticism against the EU and other forms of political discontent (Rodríguez-Pose and Dijkstra, 2021; Dijkstra et al., 2020; Krouwel and Abts, 2007; Treib, 2014; Rooduijn et al., 2016). Similarly, Treib (2014) argues that the 2014 European Parliament elections provided voters with an opportunity to express dissatisfaction with their domestic governments. Moreover, Gold and Lehr (2024) also discusses the results of their analysis on the effects of EU funding on EU discontent, highlighting that domestic and EU-level discontent are often intertwined. Most notably, De Vries and Edwards (2009) demonstrates that EU skepticism is present at both the extreme right and the extreme left of the political spectrum, albeit for different reasons. While right-wing Eurosceptic parties tend to mobilize around national identity, left-wing Eurosceptic parties primarily criticize the EU's economic outcomes. This shows that also the motives of supporting EU skeptic parties often correspond with motives associated with broader political discontent. Together, these findings suggest that attitudes toward the EU can serve as a meaningful proxy for broader political discontent.

3.3 Data: Harmonization of Eurobarometer Surveys

To measure political discontent with the EU for as large a sample of EU citizens as possible and for the longest possible period, we engaged in a comprehensive harmonization of *Eurobarometer* surveys from 1990 to 2024. In total, these data contain up to 2.14 million observations and include variables such as gender, age, and occupation. A main feature of our data harmonization is the consistent cod-

ing of subnational regions according to the NUTS-2 classification. While the subnational coding in Eurobarometer waves varies over time and is based on different subnational units, our harmonization allows subnational analyses at the NUTS-2 level. For this, we decided to use the NUTS-2 classifications of 2016 as the reference. For harmonization, we went through all NUTS-2 classifications of observations and adapted their codes so that they correspond to the NUTS-2 classification scheme. To our knowledge, we are the first to provide such a harmonized set of *Eurobarometer* surveys to the research community.

A second important feature of our harmonization is the coding of a consistent measure of Euroscepticism. Some *Eurobarometer* items, especially those asking about support for the EU, have changed over the long observation period. Therefore, we combine different measures of EU support to create a harmonized variable and to increase the sample size. To do this, we code a variable set to one if a respondent supports the EU and to zero if the respondent has a neutral or unfavorable opinion of the EU. The variable harmonizes three different measures of support for the EU. First, we use a standard EU question asking whether respondents evaluate the EU as a bad thing, a good thing, or neither. We code EU support as one if respondents perceive it as good. If this variable is missing, we use a second variable that measures the perceived benefit of the EU membership of the respondent's country on a binary scale (benefited/no benefit). Benefit is coded as EU support. The last variable, used if both previous variables are missing, is based in the respondent's EU image on a scale from 1 (very bad) to 5 (very good). Here we code only respondents as EU supporters, who have a "good" or "very good" image of the EU. We collected and harmonized each wave of the *Eurobarometer* that includes at least one of these questions. Moreover, the appendix provides a full table on the number of observations per country in Table A1 and a summary about all included Eurobarometer waves and observations per year in Table A2 in the appendix. Summary statistics on each of the variables used to harmonize our dependent variable can be found in Table A3. The full harmonized dataset includes the 1989-2024 period. As we only aim to analyze completed funding periods (1989-1993, 1994-1999, 2000-2006, 2007-2013, 2014-2020), we analyze data until 2019. Eligibility for the ongoing 2021-2027 funding period was already determined in 2020, which is why we exclude observations from 2020 onward from our analysis.

3.4 Method: Regression Discontinuity Design

For identifying the causal effect of place-based EU funds on EU support at the individual level, we match the individual-level survey data from harmonized *Eurobarometer* waves with data on the Cohesion Policy at the NUTS-2 level using the geographic identifiers. We then leverage the discontinuity in regions' funding volumes at the threshold of 75% of the EU's mean GDP per capita. The discontinuity in eligibility at the cutoff based on the running variable is visualized in Figure A1 in the appendix.

For implementing the RD design, we use GDP per capita relative to the EU average as the running variable. We then estimate local linear regressions with robust bias-corrected confidence intervals (Calonico et al., 2017) to obtain RD estimates. Specifically, we estimate:

$$Y_{irt}^{s} = \beta_s c_{rt} + \gamma_1 gdp_{rt}^{EU} + \gamma_2 c \cdot gdp_{rt}^{EU} + X_i' \mu + \mu_c + \tau_t + e_{rt}, \quad \forall s \in S \tag{1}$$

In the baseline, Y_{rts} denotes *EU support* of respondent i living in region r and belonging to occupational group s in year t. The cutoff variable c_{rt} indicates *treated* regions below the cutoff. The running variable is gdp_{rt}^{EU}. X_i are individual-level socio-demographic control variables. Country fixed effects and year fixed effects are denoted by μ_c and τ_t. e_{rt} is the error term, and we cluster standard errors on the NUTS-2 level. We use a triangular kernel so that observations close to the 75% cutoff are weighted more. To test our key hypotheses, we estimate this regression separately for different occupational groups s.

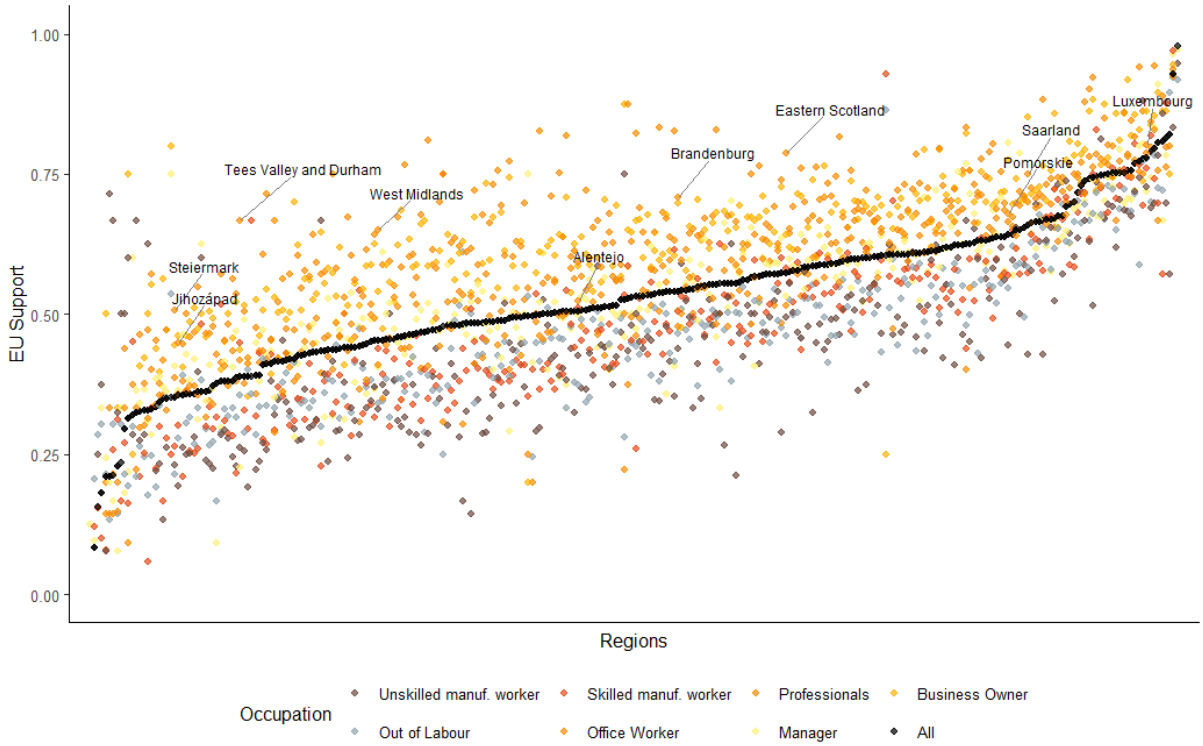
In the baseline, we use an RD bandwidth of 20 percentage points above and below the threshold of 75% percent of the mean EU GDP per capita. Figure A1 in the appendix shows that the cutoff constitutes an almost perfect compliance with the allocation rule. Only very few, exceptional regions received funding, even though they were above the 75% cutoff. In robustness tests, we drop these exceptional cases and estimate a sharp RD (Figure A4). To challenge the robustness of our bandwidth selection, we also report results of a more conservative automated bandwidth selection using the package provided by Calonico et al., 2017. Results of these estimations can be found in Table 1 and in Figure A3. For these estimations, each sub-sample in the analysis uses different bandwidths, implying different samples. In contrast, the constant bandwidth of 20 above and below the threshold holds constant the sample of regions. Our results remain largely unchanged across both approaches. Furthermore, a common threat to causal identification in RD designs is a bias resulting from sorting effects. There-

fore, in Figure A2 in the Appendix, we test whether regions systematically sort themselves into the treated group. We find no evidence for this bias in our setup.

4 Results I: EU-wide Observational Data

4.1 Descriptive Evidence

Figure 1: EU Support across Subnational Regions by Occupation



Note: The figure plots the mean value of *EU support* across region and occupation groups. Data source: harmonized *Eurobarometer* waves 1990-2019.

Our empirical analysis begins with descriptive evidence. Figure 1 plots the degree of EU support by region and occupational group. There is substantial variation in political discontent with the EU both across and within European regions. The plot is ordered by mean EU support in the region — plotted in black – showing that this measure varies between 30 percent to 70 percent in most regions. The colored dots indicate EU support by occupational group. Differences across across occupational group within regions are substantial. Across virtually all regions, business owners, managers and professionals indicate higher EU support than average. Unskilled manufacturing workers and people out of labor exhibit the lowest values for this measure. Given such large differences in political attitudes

across groups within regions, the question of how place-based policies affect such groups differently seems all the more pressing.

4.2 RD Results

We start our regression analysis by presenting the RD results for the full sample in Panel A of 1. We present two types of bandwidth selection for four samples. The first column reports results from the automated optimal bandwidth selection (Calonico et al., 2017). The second column reports results from a selection that includes observations from regions with 20 percentage points below and above the 75% cutoff. The estimated point estimates imply that eligibility for the largest volumes of place-based funding increase EU support by around 7 percentage points on average. The effect is statistically significant ($p < 0.01$) and is in line with multiple recent studies focusing on aggregate political outcomes at the regional level (e.g Gold and Lehr, 2024; Rickard, 2023; Borz et al., 2022).

Next, we turn to our main analysis of sub-samples to evaluate how this effect differs across occupational groups. In Panel B, we estimate the same specifications for the sub-sample of managers. Here, we find a substantially stronger effect of a 14-16 percentage-point increase in EU support. Effects are similarly strong by business owners (Panel C). Panels D and E turn to an occupational group for which our hypotheses predict more limited effects: unskilled manufacturing workers and people out of labor. For both groups, we estimate smaller point estimates that are statistically insignificant. To examine the differences between effect sizes, we use Wald tests to evaluate whether the treatment effects are significantly different across Panels. We compare the coefficients of managers and business owners with those of unskilled manufacturing workers and respondents out of labor. The results are reported in Table A5 in the appendix. The difference between Managers and business owners to citizens out of labor is sizable and statistically significant. However, comparing these coefficients to those of unskilled manufacturing worker reveal that, while coefficients for managers and business are almost twice as large, the differences do not reach statistical significance at conventional levels.

Table 1: RD: Main Results

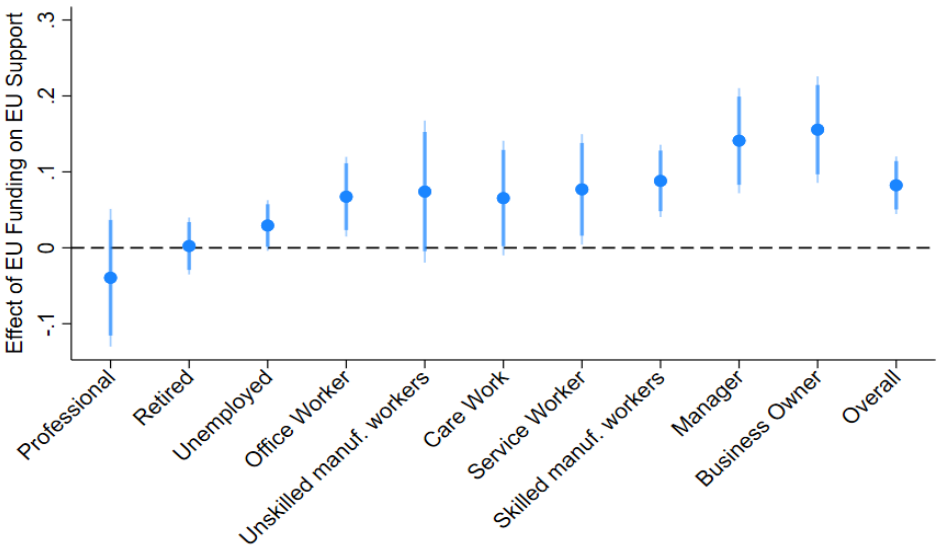
	(1) Optimal bandwidth	(2) Bandwidth 20
Panel A: Full Sample		
Robust	0.07	0.08
	(0.03)**	(0.02)**
Observations (below/above cutoff)	120,377/ 56,714	289,684/ 174,039
Full sample N	1,265,106	1,265,106
BW	11	20
Panel B: Manager		
Robust	0.16	0.14
	(0.04)**	(0.04)**
Observations (below/above cutoff)	8,041/ 4,543	16,047/ 12,691
Full sample N	99,290	99,290
BW	12	20
Panel C: Business Owner		
Robust	0.22	0.16
	(0.08)**	(0.04)**
Observations (below/above cutoff)	6,565/ 2,510	17,160/ 8,106
Full sample N	62,430	62,430
BW	9	20
Panel D: Unskilled Manufacturing Worker		
Robust	0.01	0.07
	(0.05)	(0.05)
Observations (below/above cutoff)	7,269/ 4,639	9,680/ 6,966
Full sample N	43,841	43,841
BW	15	20
Panel E: Out of labour		
Robust	0.02	0.03
	(0.02)	(0.02)
Observations (below/above cutoff)	52,028/ 30,110	123,079/ 76,478
Full sample N	526,432	526,432
BW	11	20
Country Fixed-effect	✓	✓
Year Fixed-effect	✓	✓

Note: This table presents the main results of our regression discontinuity analysis. All models include country and year fixed-effects and clustered standard error on the Nuts-2 level. It reports the results of four panels and two different sets of bandwidth selections. The first column "Optimal BW" reports results from the automated optimal bandwidth selection (Calonico et al., 2017). The second coloumn "BW:20" reports results from a selection including observations from regions with 20% below and above the 75% cutoff. * denotes statistical significance at the 5% level and ** denotes statistical significance at the 1% level.

As a next step, we repeat the analysis for each occupation category in the harmonized version of the Eurobarometer. Specifically, based on the indicated occupation of respondents in the Eurobarometer, we summarize respondents into several occupation categories and run RDs for each category. Figure 2 depicts the results of these estimations ordered by estimated effect size. In line with our hypotheses, the largest effects are estimated for business owners and managers; small and insignificant effects are

estimated for the unemployed, the retired; in between are office workers, unskilled manufacturing workers, and service workers. Somewhat surprisingly, the smallest effects are estimated for the category of professionals (lawyers, doctors, architects, auditors etc.). Given that this group is generally most supportive of the EU (see Figure 1), the absence of an effect here, could also be due to a ceiling effect.

Figure 2: Effects of EU Funding by Occupation



Note: This coefficient plot shows the RD estimates based on equation 1 for different occupation groups as sub-samples and the full sample. The analysis is conducted with a bandwidth of 20 above and below the threshold. The outcome variable is *EU support*. The analysis includes the full sample from 1990-2019.

For robustness, we reiterate this analysis by employing an automated bandwidth selection selection following Calonico et al. (2017). The results of this estimation are shown in Figure A3 and largely remain the same in interpretation.

We also run a similar analysis, but differentiate by education. To obtain the educational attainment of respondents, we utilize the Eurobarometer's imperfect measurement of education that indicates the age at which an individual completed their highest level of education. This is one of the few variables consistently included across all waves of the Eurobarometer survey and serves as a proxy for respondents' educational level. For the education analysis, we employ the same RD specification utilized in the occupation analysis presented above. The results of this can be found in Figure A5 in the appendix. There is no significant impact of EU funds on EU support. However, as educational levels

increase, the influence of EU funds on EU support becomes more pronounced, reaching its peak in the highest education category. Given the limitations of this measure, we suggest being cautious when interpreting it. In our experimental study, we reiterate this analysis with a more accurate measurement of education.

Taken together, the observational Eurobarometer findings provide evidence that the impact of EU place-based funding on EU support is heterogeneous and depends on individuals' socio-demographic characteristics. The results indicate that EU place-based policies are especially effective in enhancing EU support for voters with more senior-level occupations and jobs that require more education.

4.3 Mechanisms

Our argument suggests that these heterogeneous effects are due to differences in personal benefits. In this section we aim to provide empirical evidence for this mechanism testing whether perceived benefits are indeed heterogeneous in a way that explains the main results.

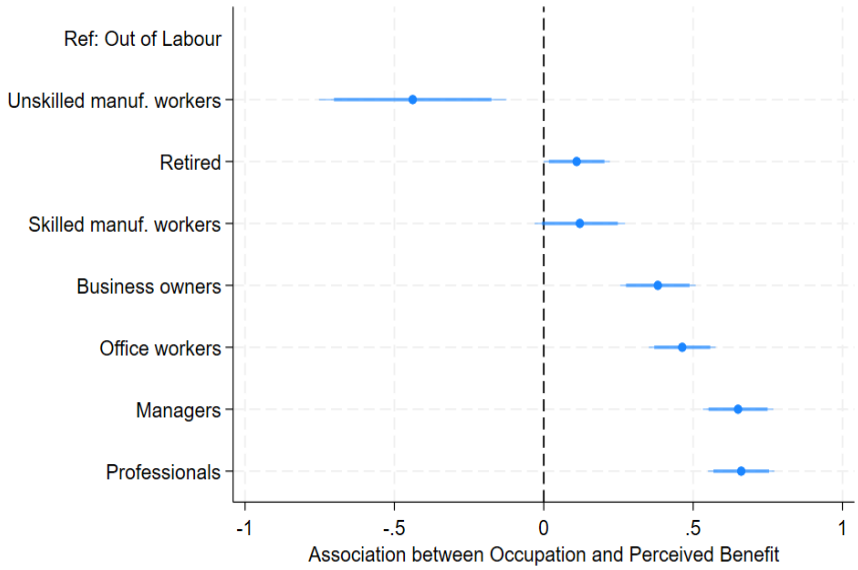
We examine three additional waves of the *Flash-Eurobarometer* from 2015 (European Commission, 2023), 2017 (European Commission, 2020) and 2019 (European Commission, 2015). These three waves do not include the variables we use to measure EU support. Instead they ask specifically about the perceived benefit from the policy, which is why they are a good fit to test our mechanism. Altogether these three waves include approximately 80,000 respondents across all EU member states.

For Figure 3 we examine how perceived personal benefits differs by occupation groups. The dependent variables measures whether a respondent reports benefit from the EU Cohesion Policy (1) or not (0). We include year and country fixed-effect and control for age and gender as well as awareness of EU funding. Importantly, this implies that differences in reported benefit are not driven by different levels of awareness across occupational groups. In line with our expectations, we find that business owners, skilled office workers, professionals, and managers have a significantly higher positive evaluation of personal benefits of the EU Cohesion policy in comparison to citizens out of labor.

The strong perception of personal benefits among professionals supports the view that the absence of a positive main effect above could indeed be driven by a ceiling effect.

These differences are substantial in size. They provide empirical support for the hypothesized mechanism, indicating that difference in perceived benefit explain heterogeneity in political outcomes.

Figure 3: Association of Occupation and personal benefit of EU funds



Note: This plot shows the coefficients from the analysis with the Flash Eurobarometer 2015, 2017 and 2019. It shows the estimated effects sizes of occupation categories on perceived benefit by the Cohesion policy compared to citiziens who are out of the labour force. The analysis include year and country fixed-effect as well as a EU funding awareness control.

So far, we have provided quasi-experimental evidence on the causal impact of EU funding on political discontent as well as correlational evidence on the mechanism of differential personal benefit. In the following section, we present the results of a survey experiment that we pre-registered after having conducted these analyses. The experiment has two main objectives. The first is to challenge our results by replicating them in a more controlled setting. The second is to provide more evidence on the proposed mechanisms.

5 Research Design: Experimental Data

We complement this analysis with evidence from a survey experiment in Germany (N = 2,000). The survey was preregistered in June 2025 and was in the field in July 2025. The survey experiment was embedded in a larger questionnaire and items regarding our analysis were asked at the very end of the survey. The questionnaire comprised a series of questions regarding the perception of EU funds and their benefits, as well as demographic controls and three dependent variables measuring EU support. We chose to examine Germany for a case study as it receives relatively little EU money per capita. Estimates based on German respondents are most likely lower bounds and we would expect larger

effects in countries with more access to the place-based EU budget.

The survey relies on a between-subjects design and primes one main treatment. The treatment condition includes information on EU place-based policies, an exemplary picture of a sign of an EU-funded project, and an open-ended response field in which participants were supposed to write down their thoughts on the scheme. The exact treatment condition that treated respondents saw is shown in Figure 4. Respondents in the control group received no information and were immediately directed to the remainder of the questionnaire.

We summarized our expectations in the pre-registration as follows. First, we expect that reported views toward the EU and its place-based policies will be more favorable among the respondents in the treatment group. Second, we expect that this effect will not be homogeneous. It will depend on respondents' socio-demographic characteristics such as occupation, income group, and level of education: The effect will be stronger among respondents who report to work in more senior-level occupations, have higher incomes, and are more educated. As dependent variables we aimed to be close to the dependent variable of our observational study, but at the same time examine additional perspectives of political discontent. We do that by examining a set of three dependent variables: 1) Trust in the European Union, 2) support for further EU integration, and 3) support for EU Cohesion policy. We pre-registered the analysis of several dimensions of socio-demographic categories. We present results on objective and subjective income, education and occupation. While we will present all pre-registered analyses, we will only include the most relevant results in the main text and present all other results in the Appendix.

Figure 4: Treatment in Survey

Note: This figure illustrates the treatment condition that treated subjects in the survey were exposed to. Note that the original version was in German. This version was translated by the authors for presentation.

6 Results II: Experimental Data

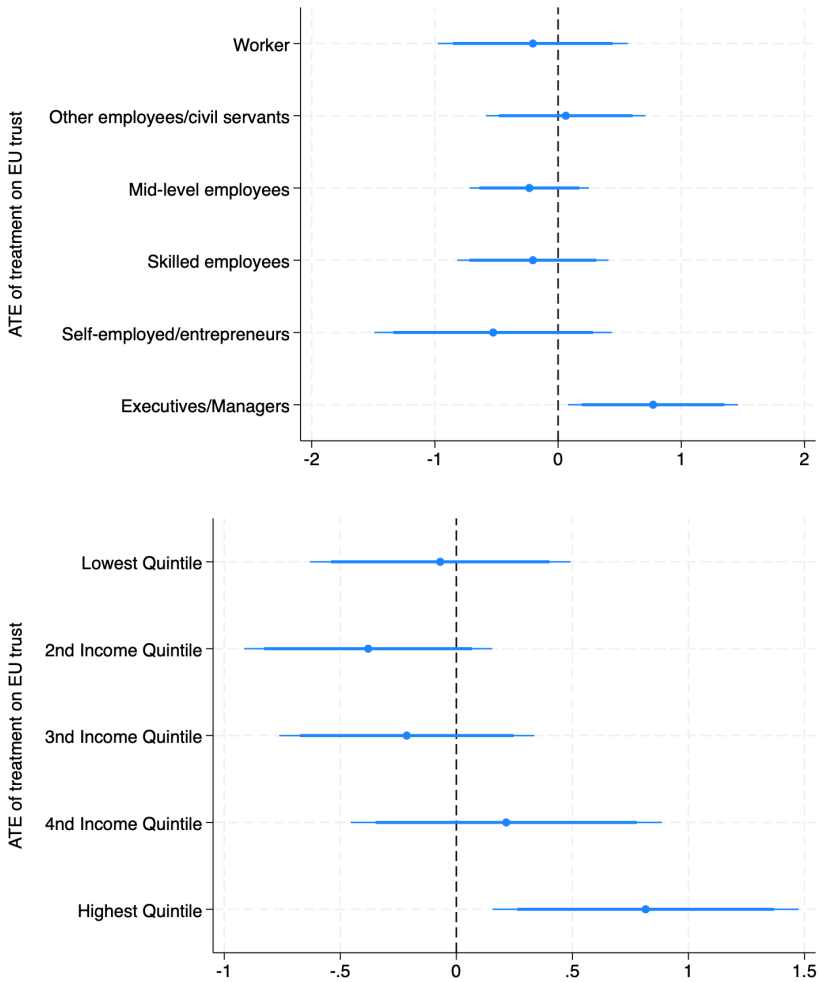
6.1 Experimental Results

We begin this analysis by examining the treatment effect without considering differences between socio-demographic variables. In all regressions for this analysis, we include controls for gender and age as indicated in the pre-registration. In addition to these, we add state fixed effects to control for contexts of citizens increasing statistical precision. We estimate a linear model by OLS using heteroskedasticity robust standard errors.

Contrary to our preregistered expectations, the treatment, on average, did not change attitudes towards the EU. This result is reported in Table A7 and Figure A9. On the one hand, this result suggests that is hard to affect attitudes toward the EU by exposing survey respondents to information about EU place-based policies. On the other hand, they further highlight the importance of considering heterogeneity across socio-economic groups.

To examine this heterogeneity, we asked respondents about their income, education, and occupation, grouped respondents accordingly, and estimated group-specific average treatment effects. The results are presented in Figure 7. First, we present the treatment effect of EU funding on EU trust across occupation categories. The treatment effect is only positive and statistically significant for executives and managers ($p < 0.05$) and insignificant for all other occupational groups.

Figure 5: Marginal Effect of the Survey Treatment on EU Support by Group



Note: This figure shows coefficient of heterogeneous treatment effects on EU support across sub-groups in terms of income, and occupation. It includes age, gender, and funding controls.

With respect to income, the treatment effects are statistically insignificant across all income groups except for the highest quintile. Thus, exposure to information about European place-based policies increases trust in the EU only among respondents in the top income quintile. We repeat the same analysis for education and find a similar pattern. Only respondents with a tertiary education indicate a significant ($p < 0.05$) treatment effect. These results are reported in Figure A10 in the appendix.

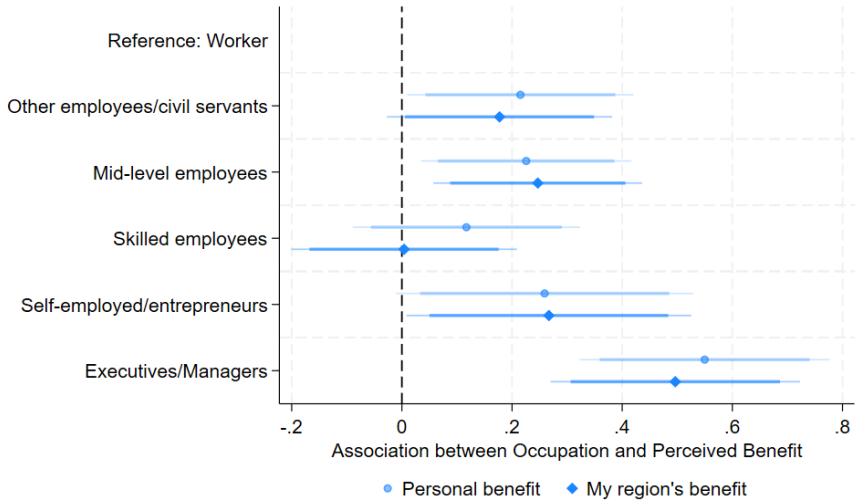
We also estimate regression models that include interactions between the information treatment and socio-economic categories, using the lowest category as the reference group. This allows us to test whether the treatment effect is significantly stronger in higher categories relative to the lowest category. The results of these estimations are presented in Figure A12 in the appendix. The top 20% income group exhibits a significantly stronger treatment effect compared to the lowest 20% income group. Similarly, respondents with tertiary education report a significantly stronger treatment effect than those with vocational training. Finally, managers also exhibit a stronger treatment effect than workers; however, this difference is only statistically significant at the $p < 0.1$ level.

We repeat all analyses steps using two alternative dependent variables, 1) support for EU Cohesion Policy and 2) support for EU integration. Results are presented in Figure A13. We do not find any positive treatment effects for any socio-economic variable for these dependent variables. We do, however, find limited evidence for potential backlash in EU integration support. Respondents with vocational training have a negative treatment effect ($p < 0.1$). The same is true for respondents in the second lowest income category ($p < 0.1$). Thus, our findings suggest differences across dependent variables. Overall, the results indicate that information about EU funding does not change attitudes toward specific policies but does affect attitudes toward the EU as an institution.

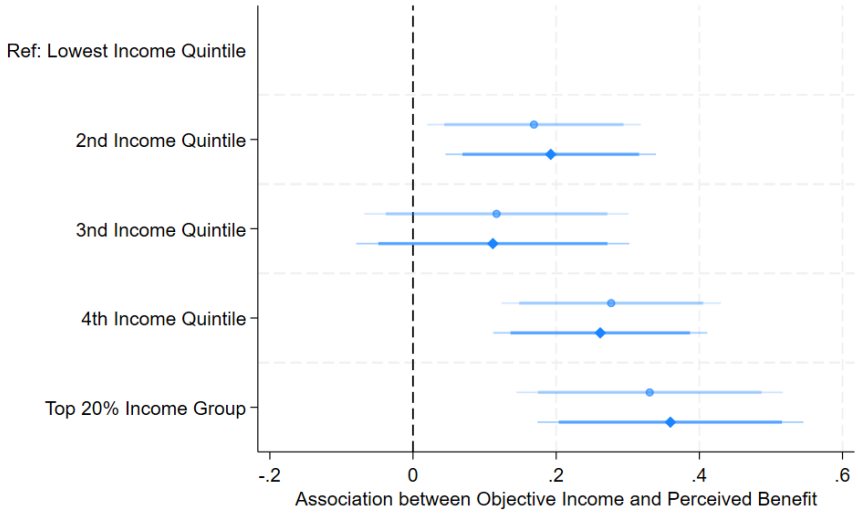
6.2 Mechanisms

To further study the proposed mechanism of personal benefit, we included the following questions in the survey: "Do you agree with the following statement? EU-funded projects have a very positive impact on my region" and "Do you agree with the following statement? 'I personally benefit greatly from projects funded by the EU in my region." Answer categories range from strongly disagree (1) to strongly agree (5).

Figure 6: Perceived Benefit of EU Cohesion Policy by Income and Occupation



(a) Occupation



(b) Income

Note: This plot shows the coefficients from the analysis of perceived benefits with the experimental data. It examines personal and regional perceived benefits from the EU Cohesion Policy. It includes age, gender, and funding controls.

Similar to the preceding analysis based on the Eurobarometer, we examine perceived personal and regional benefits across groups. Figure 6 shows the results of this analysis. For occupation, workers are chosen as the reference category and for income, the lowest category is used as the reference group. The results reveal a clear pattern in which entrepreneurs and managers report a significantly higher perception of personal and regional benefits from the EU Cohesion Policy than workers ($p < 0.05$). We find a similar association for mid-level employees, although the effect is strongest among managers.

Furthermore, the estimates show that respondents with higher income significantly ($p < 0.05$) evaluate personal and regional benefits of EU place-based policies higher than respondents in the lowest income category. Associations for income groups in the middle of the distribution are in between. We reiterate this analysis with subjective income and education. The results are reported in the appendix and depict a very similar pattern (Figure A14 and Figure A15).

Overall, these results support the proposed theoretical mechanism whereby individuals with senior-level occupations, high levels of education and high incomes perceive greater benefits from EU Cohesion Policy and, as a result, are more likely to shift their political attitudes in a pro-EU direction.

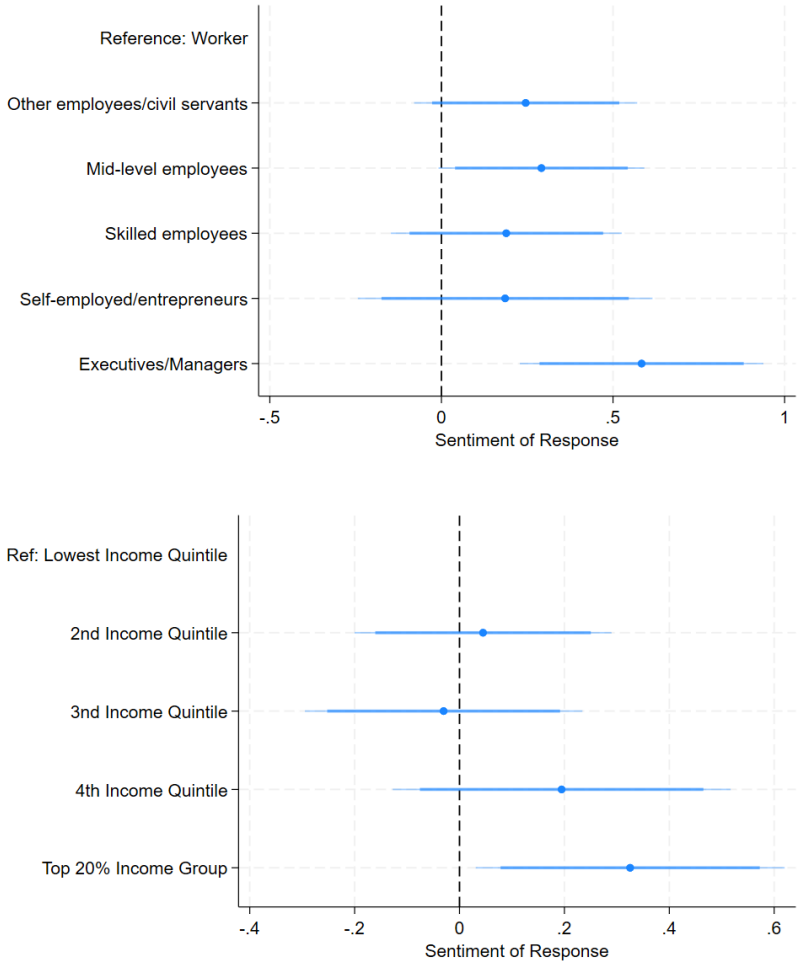
6.3 Sentiment Analysis of Open-Ended Survey Question

In an additional exploratory analysis of the open-ended survey responses of our treated observations, we further aim to validate our results by examining respondents' sentiment after receiving the treatment. Our argument suggests that citizens with higher socio-economic status respond to the treatment with a more positive sentiment. In turn, we expect neutral or even negative sentiment by low-skilled, low-income individuals. This analysis can give insights into the immediate responses of citizens to information about the Cohesion Policy, and as such provides novel evidence on this matter. This analysis was not preregistered.

For this analysis, we hand-coded the sentiment of all responses into 5 categories from 1 "very negative" to 5 "very positive". Statements such as "Terrible", or "Lies, everything are lies, the EU should be abolished" are examples of the lowest category. An exemplary statement for the highest categories is "Very good, I have my current job because of EU funding!" Neutral positions are statements such as "I have not thought about that yet." We then regress these sentiments on occupation and income.

Our results suggest that managers and respondents in the top 20% income category respond more positively to the information treatment than workers or citizens in the lowest income category. This corroborates results from Figure 6 and suggests that not only are perceived benefits higher for higher socio-economic classes, but also written statements by respondents on the Cohesion policy mirror this bias.

Figure 7: Sentiment of open-ended survey responses to treatment condition



Note: This plot shows the coefficients from the analysis of the sentiments of the written statements of treated respondents with our experimental data. The dependent variables measure sentiment on a scale from 1 (very negative) to 5 (very positive) and were hand-coded. It includes age, gender, and funding controls.

7 Conclusion

This paper examines the microfoundations of the political effects of place-based policies with a focus on its distributional and heterogeneous effects. Relying on both a quasi-experimental observational analysis and a survey-experimental analysis, it analyzes heterogeneities in the effect of place-based spending on political attitudes. The results of the two studies provide consistent support for the argument that place-based policies primarily benefit individuals with higher socio-economic status in terms of occupation, education, and income. The evidence is consistent with stronger perceptions of personal benefit from the place-based policy in these groups.

These results have implications for the scholarly understanding of the role of policies for mitigating political discontent. Since research points to a connection between fundamental socio-economic disparities across regions and regionally concentrated political frustration, place-based policies are often portrayed as a key remedy for countering political extremism. Our results suggest that this policy instrument has an important limitation. It does not reach the most vulnerable groups within the affected regions. In contrast, its distributional effects within regions may even exacerbate feelings of left-behindness. More generally, the results thus contribute to explaining why contemporary political discontent remains strong in many regions despite large financial efforts (Broz et al., 2021).

Overall, these findings carry important policy implications. They indicate that place-based policies alone are insufficient to address political discontent among groups with lower levels of education and in lower-status occupations. This is particularly relevant given that citizens with lower education and lower income are especially likely to hold negative views of the EU, a pattern well established in the literature (e.g., Baute and Tober, 2024, Hobolt, 2016; Algan et al., 2017). The current design of placed-based policies in the EU tends to exacerbate rather than weaken the strong educational and class-based divide in support for the EU .

An important limitation of our analysis is that it only studies one place-based policy. Further research could focus on differences across policies in order to identify mechanisms through which place-based policies can circumvent the distributional effects and heterogeneities that we identify.

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A1 Description: Eurobarometer Analysis

First, we provide a more detailed overview of the harmonized Eurobarometer dataset. For this depiction, we decided to present the dataset that is already merged with the Cohesion Policy funding data from 1990 to 2019. This constitutes our analysis dataset. As a consequence, from our original 2.4 million observations, a significant number of observations are dropped, because they do not contain funding data. Oftentimes this is due to missing Nuts 2 information for observations. For instance, large countries as Germany our the UK, only contain data until 2002.

Table A1 depicts all observations per country. It shows, that for each country a significant number of observations is included. This is important as our analyses include country fixed-effects and thus need a sufficient amount of observations per country to examine within country changes. The table also shows that our dataset includes almost all EU countries. Only Cyprus, Malta, and Slovenia are missing from our dataset. This provides a comprehensive analyses of the political consequences of EU place-based policies.

Table A2 in turn depicts the number of observations per year. Similarly, our analysis includes year fixed-effects and thus a sufficient amount of observations per year is relevant as well. In addition to observation per year, for transparency we also include the Eurobarometer ID that we used for harmonization. For the observations from 1990-2002 we used the Mannheimer Eurobarometer Trendfile provided by Schmitt et al., 2008. We then continued the harmonization by including Eurobarometer waves that include one of the EU support measurements depicted in Table A3. As can be seen in Table A3, later years include more waves. This is because in later years the Eurobarometer started to implement more fragmented modules and waves.

Table A1: Observations per Country

Country code	Observations
AT	96,068
BE	113,101
BG	46,129
CZ	58,944
DE	95,295
DK	57,455
EE	32,305
ES	110,775
FI	81,780
FR	111,625
GB	42,175
GR	27,844
HR	32,938
HU	58,734
IE	80,944
IT	48,802
LT	43,397
LU	27,658
LV	43,335
NL	112,025
PL	57,485
PT	111,059
RO	52,528
SE	80,805
SK	59,446
Total	1,682,652

Note: This table shows the number of observations per country of our analysis sample. The sample includes all observations from 1990-2019 that were able to be matched to funding information on the Nuts 2 level.

Table A3, illustrates our key variables with which we construct the political discontent measurement. We consider three key variables for the construction of our dependent variable. First, whether respondents consider the EU as a good thing or not, second, whether they perceive themselves to benefit from it, and last, their image of the EU. Sometimes, in one wave, a respondent has answered several questions, whereas in others, only one of the items is included. To construct a harmonized EU support variable, we first coded EU support (1) if a respondent said that the EU is a good thing and no support (0) if that is not the case. We used the EU as the basis for our variable, as it is the most widely used EU support measure in the Eurobarometer.

Table A2: Eurobarometer ID per Year

Year	Eurobarometer ID	Number of Observations
1990	3521	29,122
1991	3521	33,310
1992	3521	44,184
1993	3521	33,229
1994	3521	33,549
1995	3521	71,496
1996	3521	41,816
1997	3521	55,958
1998	3521	41,429
1999	3521	53,040
2000	3521	39,212
2001	3521	93,613
2002	3521	52,386
2003	3904, 3905, 3938	25,358
2004	4056, 4229, 4231	32,760
2005	4411, 4414	23,964
2006	4506, 4526	23,562
2007	4530, 4565	27,701
2008	4744, 4819	27,577
2009	4971, 4973	41,583
2010	5234, 5449	31,040
2011	5481, 5567, 5596	51,748
2012	5612, 5613, 5685	51,735
2013	5689, 5875, 5876	51,821
2014	5913, 5928, 5932, 5933	78,633
2015	5964, 5965, 5998, 6595, 6596, 6643, 6644	143,144
2016	6693, 6694, 6695, 6788, 6791	122,191
2017	6863, 6925, 6927, 6928	82,072
2018	6963, 7482, 7483, 7484, 7488, 7489, 7556	143,069
2019	7561, 7562, 7576, 7580, 7601	102,350
Total		1,682,652

Note: This table shows the number of observations per year of our analysis sample. The sample includes all observations from 1990-2019 that were able to be matched to funding information on the Nuts 2 level. Moreover, the table includes the Eurobarometer ID of each included wave in our harmonization. We include waves that contain measurements of political discontent, measured as support for the EU.

Second, we coded a citizen to support the EU if she reported perceiving a benefit from the EU. Last, if a respondent has a positive EU image (4 or 5) we code EU support. This way, we have a total number of observations of 1.4 million respondents that we can assign a value for EU support, 753,301 of which reported to be supporters of the EU, and 721,316 did not.

Table A3: Frequencies for EU Support Variables

Variable	Category	Freq	Percent
EU good thing	Bad thing	125,877	15.34
	Neither	208,234	25.37
	Good thing	486,685	59.29
EU benefit	No	204,424	32.74
	Yes	419,925	67.26
EU image	1 (Very negative)	35,540	3.61
	2	146,793	14.91
	3	391,474	39.76
	4	355,870	36.15
	5 (Very positive)	54,845	5.57

Note: This table shows the three EU support measurement whith which we construct our political discontent variable. The sample includes all observations from 1990-2019 that were able to be matched to funding information on the Nuts 2 level.

Table A4 now turns to the harmonization of the occupation variable. As can be seen, for the analysis in Table 1 and Figure 2, we rely on a summarized version of the occupation variable since many categories of the original Eurobarometer coding would be redundant. For instance, we refer to the owners of shops and other businesses as shop owners. Additionally, in Table 1 of the main analysis, we classify retired, unemployed, and care workers as out of labour. Nevertheless, in Figure 2, we also present results for the sub-categories of out of labour. For the category of professionals, we decided to put self-employed profesionals not as shop owners but to professionals. We do so because EU Cohesion support, in most cases, does not finance lawyers or medical practitioners, which is why we argue that, effectively, for our analysis, their self-employment is not reason enough to assign them to shop owners.

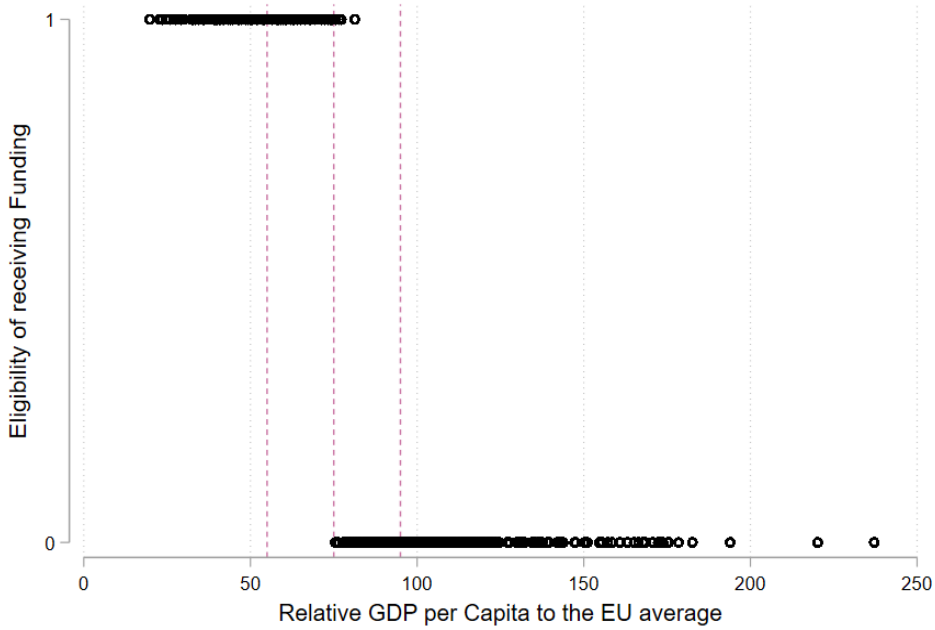
Table A4: Mapping of Occupation Codes to Occupation Groups

Summary Occupation	**Detailed Labels (Original)**
Shop owners	Owner of shop
	Business Proprietor
Managers	General management
	Middle management
Skilled manuf. Worker	Skilled manufacturing worker
Unskilled manuf. Worker	Other Unskilled Worker
Professionals	Self-employed Professionals (lawyer, medical practitioner, ...)
	Employed Professionals (lawyer, medical practitioner, ...)
Office Job	Employed desk positions
Service Worker	Employed service position
Out of labour	Retired
	Unemployed
	Care Work

Note: This table shows how we summarize the occupation for the Eurobarometer sub-sample analysis. The original labels stem from the Eurobarometer occupation labels.

A2 Decision rule of Cohesion Policy

Figure A1: Allocation rule of Objective-1 Funding

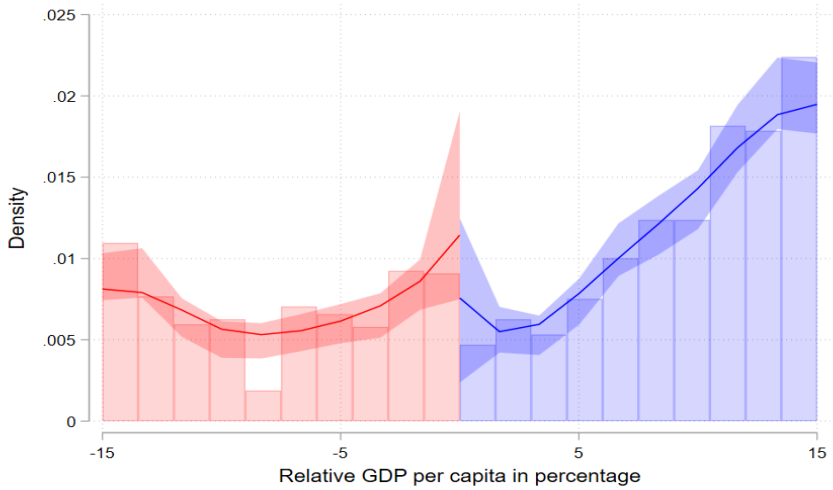


Note: This plot visualized the allocation rule of EU funding. Regions below 75% of the relative GDP per capita are in the treated condition, whereas regions above this threshhold are not being allocated funding.

Finally, Figure A1 turns to the Cohesion policy decision rule for financial support under the "convergence objective". There are two treatment conditions, 1 being eligible for financial support, and 0 not being eligible. The x-axis in Figure A1, illustrates that this eligibility depends on the relative GDP per Capita to the EU average. If a region has a GDP per capita that is below 75% it obtains eligibility for financial. If it is above this threshold, it is not eligible. Thus, the x-axis represents the running variable in our regression discontinuity design. The red dotted lines indicate the 75% threshold in the middle, and the bandwidths of 20 above and below the threshold that we use for our main analysis in Figure 2. In the section below, we also vary this bandwidth to a even more conservative estimation using the automated optimal bandwidth selection by Calonico et al., 2017. Both approaches have advantages and disadvantages. While a set bandwidth can ensure that the examined sample remains the same throughout all sub-samples of the analysis, a more restrictive and automated selection of the bandwidth can provide robustness for RD assumptions.

A2.1 Test for Sorting

Figure A2: Test of Sorting of Nuts-regions



Note: This plot shows the test for sorting into the treatment condition. The analysis is done on the regional level, as funding is allocated on that level.

An important assumption that we need to test in our RD setting is sorting. Since being treated in our case constitutes a favorable outcome, regions might have motivation to sort themselves into the treatment. We test for a significant jump in observation density directly at the cutoff of in our running variable. We do not find a significant difference. This indicates that regions did not proactively sorted themselves to be treated.

A3 Eurobarometer: Additional Results

To provide a deeper insight into the results of the Eurobarometer data, in this section, we present additional results of our main analysis.

A3.1 Difference of Outcomes

Table A5: Differences in Outcomes

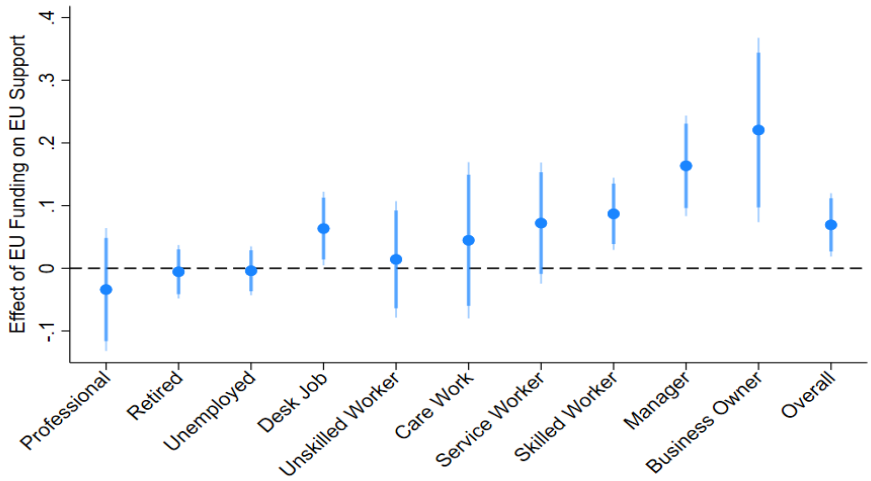
Wald test:	Diff.	SE	z	p
Compared to Manager				
Unskilled manuf. worker	.07	.06	1.13	.26
Out of labour	.11	.02	2.7	.01
Compared to Business Owner				
Unskilled manuf. worker	.08	.06	1.37	.17
Out of labour	.12	.04	3.03	0

Note: This tables shows the results of wald tests that compare the effect sizes of EU funding on EU support between manager and business owner with those of unskilled manuf. workers and citizens out of labour. Diff. indicates the difference in effect size, and p indicates whether this difference is statistically significant to the effect size of manager or business owner.

First, to assess the significance of the effect sizes between the coefficients of Table 1 in the main text, we conduct Wald tests of a set of dyads of occupation. We use the same specifications of models for the input of the Wald-test as used for the results in Figure 2 in the main text. Specifically, we assess the differences between the effect sizes for the categories Manager and Out of labour, and Manager and Unskilled manufacturing worker. In addition we reiterate this process by comparing the same categories with business owners. Table A5 shows the results of these comparisons. The results suggest that the effect sizes of EU funding on EU support for managers and business owners are significantly higher ($p < 0.01$) than the effect size of citizens that are out of labour. Comparing this to the effect size of Unkskilled manuf. worker the results are not that clear. The table shows that while both manager and business owner indicate a stronger coefficient and thus a stronger effect of EU funding on EU support, the Wald tests suggest that the difference do not reach statistical significance.

A3.2 Alternative Bandwidth

Figure A3: Effect of EU funding on EU support by occupation (BW: optimal bandwidth)

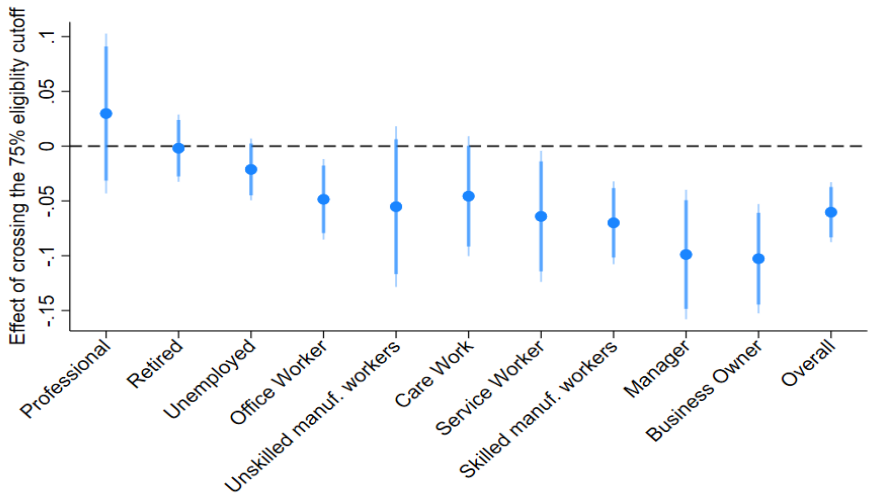


Note: This figure shows the coefficients of our RD estimates for each EU support variable individually, using automated optimal bandwidth selection. We include, country and year-fixed effects and cluster the standard errors on NUTS-2 level. The x-axis shows the different occupations for which we run the estimations.

First, Figure A3 presents the same analysis as in Figure 2 in the main text, but now uses automated optimal bandwidth selection by Calonico et al., 2017. The results also show that, with an alternative set of bandwidths, the results from Figure 2 largely remain steady with a much more conservative estimation. Table 1 in the main text, provides insights into the bandwidths. For the manager and respondents out of labour, a bandwidth of 7 above and below the cutoff is selected. For an unskilled manufacturing worker, a bandwidth of 10 below and above the threshold is selected. With the new bandwidths, business owners now indicate the strongest effect of EU funding on EU support. Nevertheless, as in Figure 2, only skilled manufacturing workers, managers, and business owners, as well as office workers, indicate a statistically significant and positive effect.

A3.3 Sharp RD without non-compliers

Figure A4: Effect of EU funding on EU support by occupation (BW: 20)

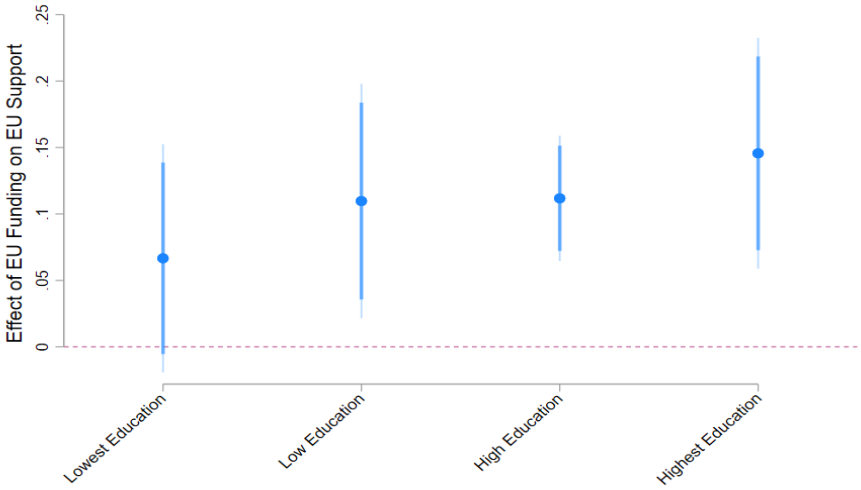


Note: This figure shows the coefficients of our RD estimates for each EU support variable individually with a bandwidth of +- 20 below/above the threshold. This estimations use sharp RD estimates and exclude non-compliers in the sample. We include, country and year-fixed effects and cluster the standard errors on NUTS-2 level. The x-axis shows the different occupations for which we run the estimations. Since, the estimations compute the sharp RD results, the coefficients indicate the effects of not being treated.

In Figure A4 we reiterate the same analysis in Figure 2, including sharp RD estimates and the exclusion of non-complying regions. As discussed in the main text, some regions are eligible for funding even though they surpass the 75% cutoff of the allocation rule. In our main analysis, we keep these observations because they are eligible and therefore treated. Nevertheless, Figure A4 shows that the results of Figure 2 are independent of this decision, as the results do not substantially change. When interpreting the results, it is important to consider that the coefficient indicates the results of crossing the 75% eligibility cutoff. This is why, now a negative coefficient indicates a positive treatment effect in A4. This is different from the main results, which reports the coefficients of being treated.

A3.4 Education

Figure A5: Effects of EU Funding by Education



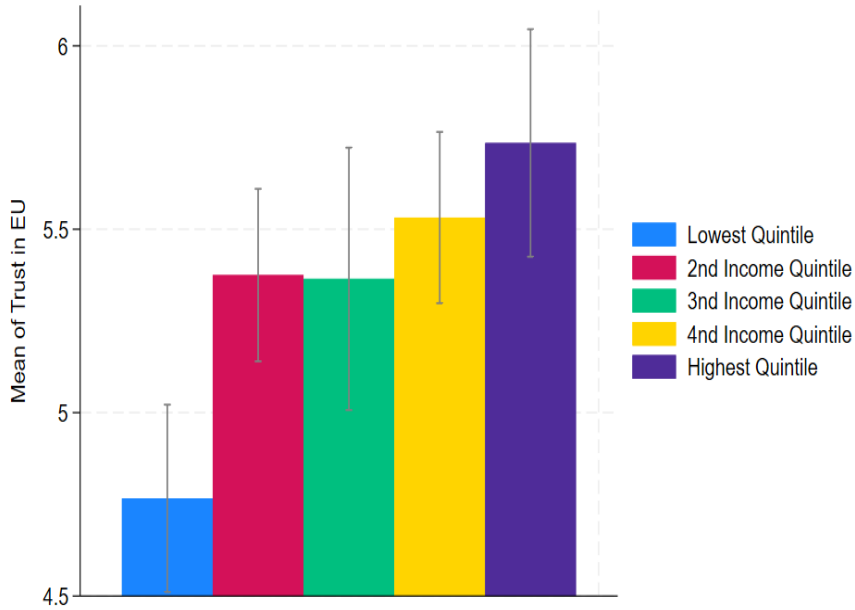
Note: This coefficient plot shows the fuzzy RD estimates with a bandwidth of +- 20 below/above the threshold. The x-axis shows the age at which respondents finished their education.

Next, we also examine education as a dependent variable. To this end, we harmonized a variable indicating when respondents completed their highest educational degree. While a variable measuring actual degrees would be more precise, due to data availability, we have to rely on a proxy using the age of graduation of the highest educational degree. Overall, this includes 1,357,220 in our sample. We categorize the variable into four categories: Lowest education (no education or finishing a degree with 14), Low education (finished with 15 or 16), High education (finished from 17 to 19), and Highest education (finished with an age older than 19). With this variable we than reiterate the analysis of Figure 2, with education as the dependent variable. The results suggest that EU funding has a stronger positive effect on EU support for higher education categories. This finding is in line with our theoretical expectations. Nevertheless, given the fuzzy measurement of education, the results should be interpreted carefully.

A4 Survey Analysis

A4.1 Descriptives of Survey data

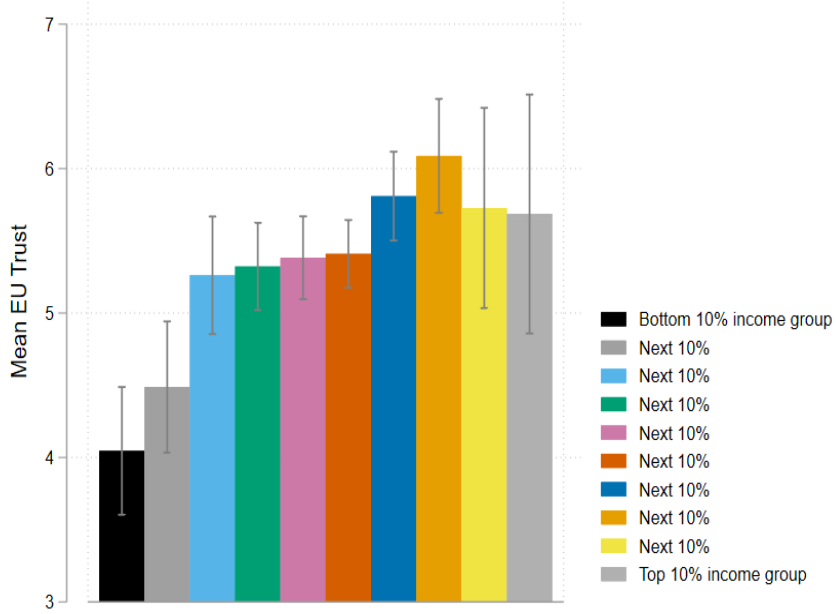
Figure A6: Trust in the EU and Income position



Note: This figure shows the mean level of trust for each income category.

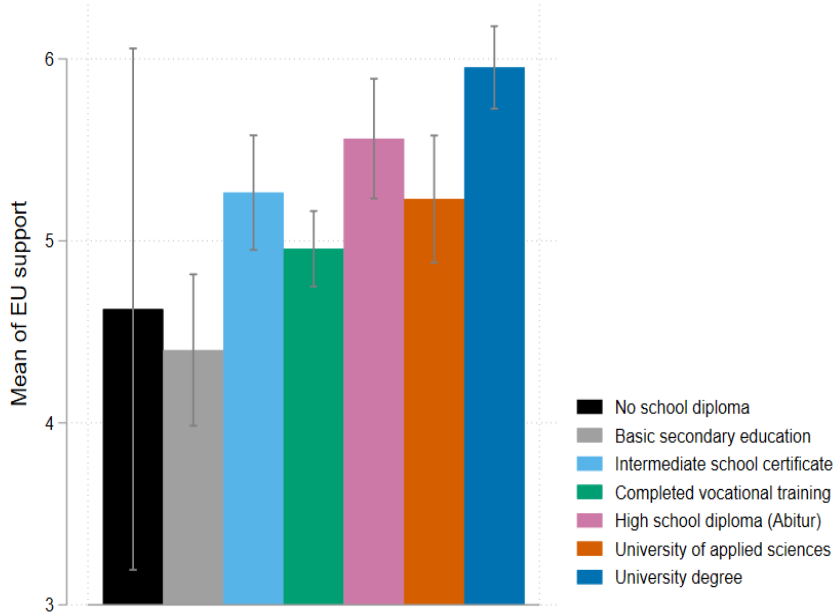
We start to summarize our survey data by presenting descriptive statistics on the level of trust in the EU. Trust in the EU is the primary dependent variable in the main analysis, as it best reflects our understanding of political discontent. Figure A6 shows that the higher the income position, the higher the trust in the EU. This mirrors findings by Hobolt, 2016, which show that citizens with low socioeconomic status tend to oppose the EU. We also find the same pattern when looking at subjective income in Figure A7 or education in Figure A8. These findings underline once again, that to efficiently decrease significant political discontent, it is citizens from the low socio-economic strata that need to be reached.

Figure A7: Trust in the EU and subjective income position



Note: This figure shows the mean level of trust for each subjective income category.

Figure A8: Trust in the EU and education



Note: This figure shows the mean level of trust for each education category.

A4.2 Balance Test of treatment condition

Table A6: Balance Table for Information Treatment

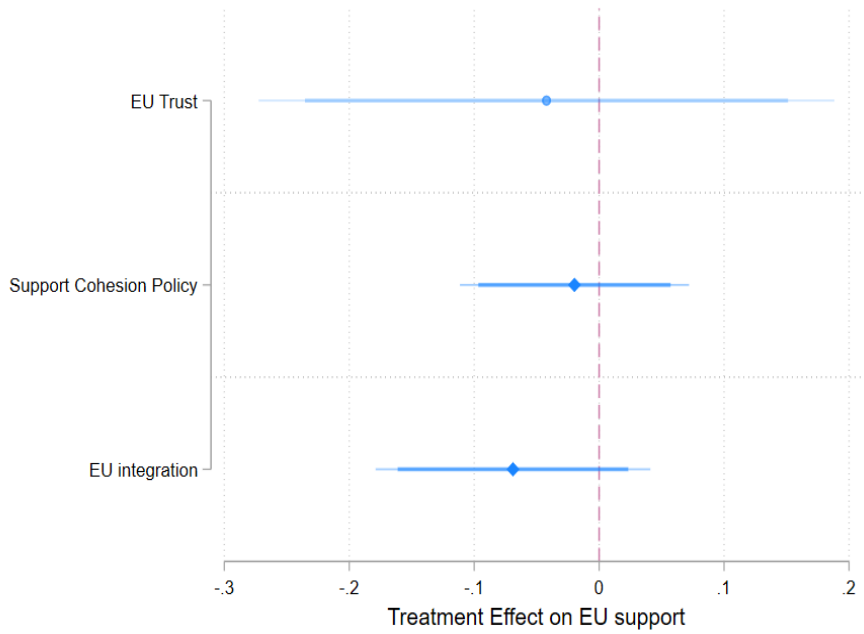
	Control			Treatment				
Variable	N	Mean	SD	N	Mean	SD	Diff	p-value
Education	921	4.62	1.32	926	4.56	1.28	-0.06	0.30
Gender	920	2.51	0.51	927	2.48	0.51	-0.03	0.24
Income	839	6.15	3.24	871	6.04	3.29	-0.12	0.46
Age	921	51.45	18.15	927	51.62	18.11	0.17	0.84
Left-Right ideology	904	5.03	1.98	901	5.01	2.06	-0.02	0.84

Note: This balance table compares the treatment and control groups of the information treatment across pre-treatment covariates. The *Diff* column reports the difference in means (Treatment−Control). The p-values stem from bivariate OLS regressions where each covariate is regressed on the treatment indicator.

Table A6 presents a pre-registered balance table of five key socio-demographic variables. We aim to test whether these variables significantly differ across treatment and control group. If that is not the case this would suggest a working randomization process in our survey. The difference between the mean values between the treatment and control groups is statistically insignificant across all variables.

A4.3 Treatment effect

Figure A9: Treatment effect overall



Note: This figure presents the overall treatment effect for all our dependent variables. The results controls for age and gender and includes state fixed-effects.

First, we present the results of the overall treatment effect. We control for age and gender and include state fixed effects. Given significant heterogeneity along a socio-economic dimension, we do not necessarily expect significant effects in this analysis, as they are likely to cancel each other out. We find no statistically significant effect for any of our dependent variables.

Table A7: Treatment Effects on EU Attitudes

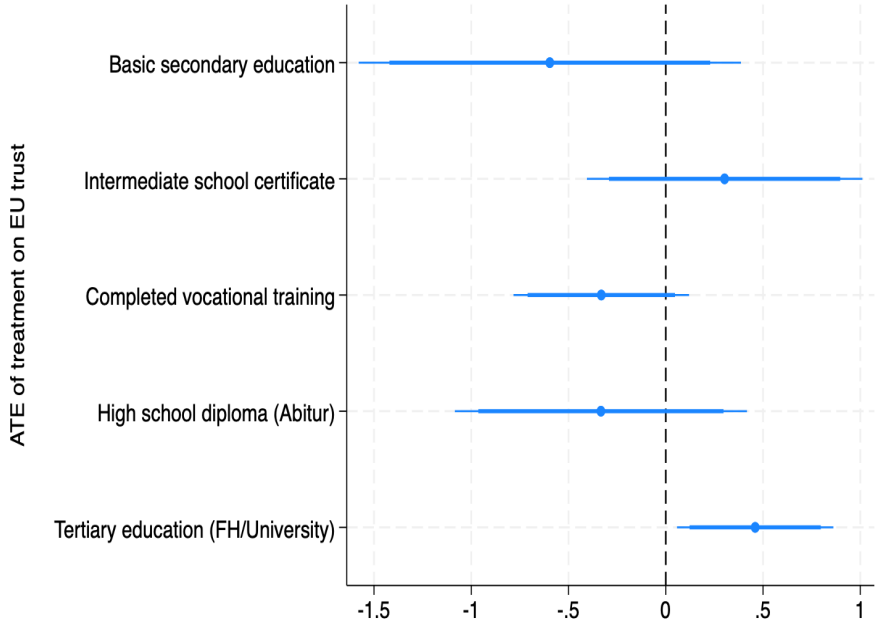
	(1) EU Trust	(2) More Cohesion	(3) EU Integration
Treatment	0.01	-0.05	-0.09
	(0.13)	(0.05)	(0.06)
Age	-0.11***	-0.02**	-0.05***
	(0.02)	(0.01)	(0.01)
Age^2	0.00***	0.00***	0.00***
	(0.00)	(0.00)	(0.00)
Gender (ref.: diverse)			
Male	0.72	-0.68*	0.07
	(1.23)	(0.41)	(0.52)
Female	0.82	-0.64	0.09
	(1.23)	(0.41)	(0.52)
Constant	7.10***	4.40***	4.41***
	(1.29)	(0.43)	(0.54)
Supported Region Fixed	✓	✓	✓
Observations	1510	1572	1570
R^2	0.02	0.02	0.02

Note: Standard errors in parentheses. Levels of significance are denoted with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. This table presents the overall treatment effect for all three dependent variables. The results controls for age and gender and includes supported region fixed-effects.

Table A7 shows the full regression table of our pre-registered analysis from A9.

A4.4 Education

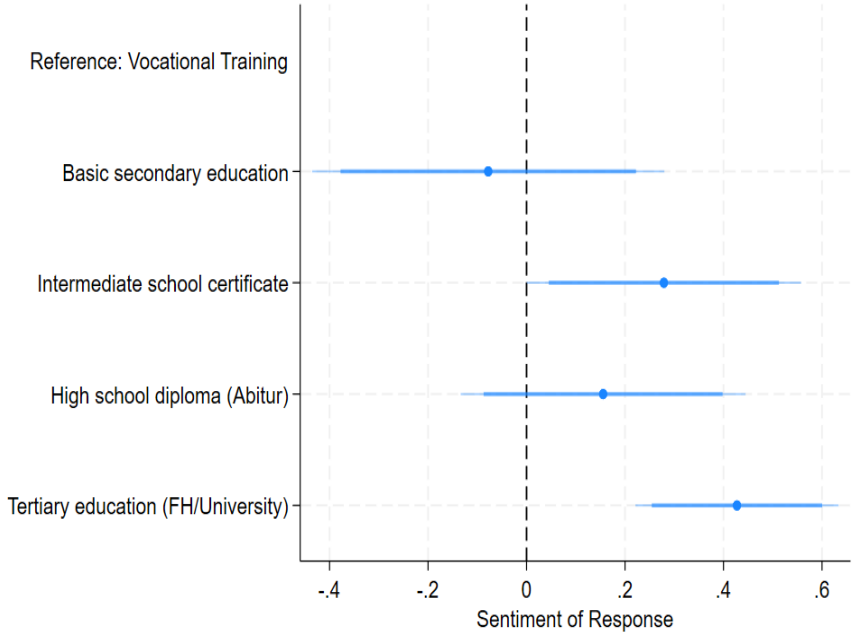
Figure A10: Treatment effect overall



Note: This figure presents treatment effect on EU trust across education categories. The results controls for age and gender and includes state fixed-effects.

In addition to occupation and income, we also present results across education categories. Figure A10 reports the results of the treatment condition on EU trust across education categories. Only respondents of the highest education category (tertiary education), report a statistically significant ($p < 0.05$) positive effect of treatment exposure. This underlines the results from Figure A5 with the Eurobarometer data, which has used an imperfect education proxy variable for a similar analysis, but finds a similar pattern.

Figure A11: Treatment effect sentiment

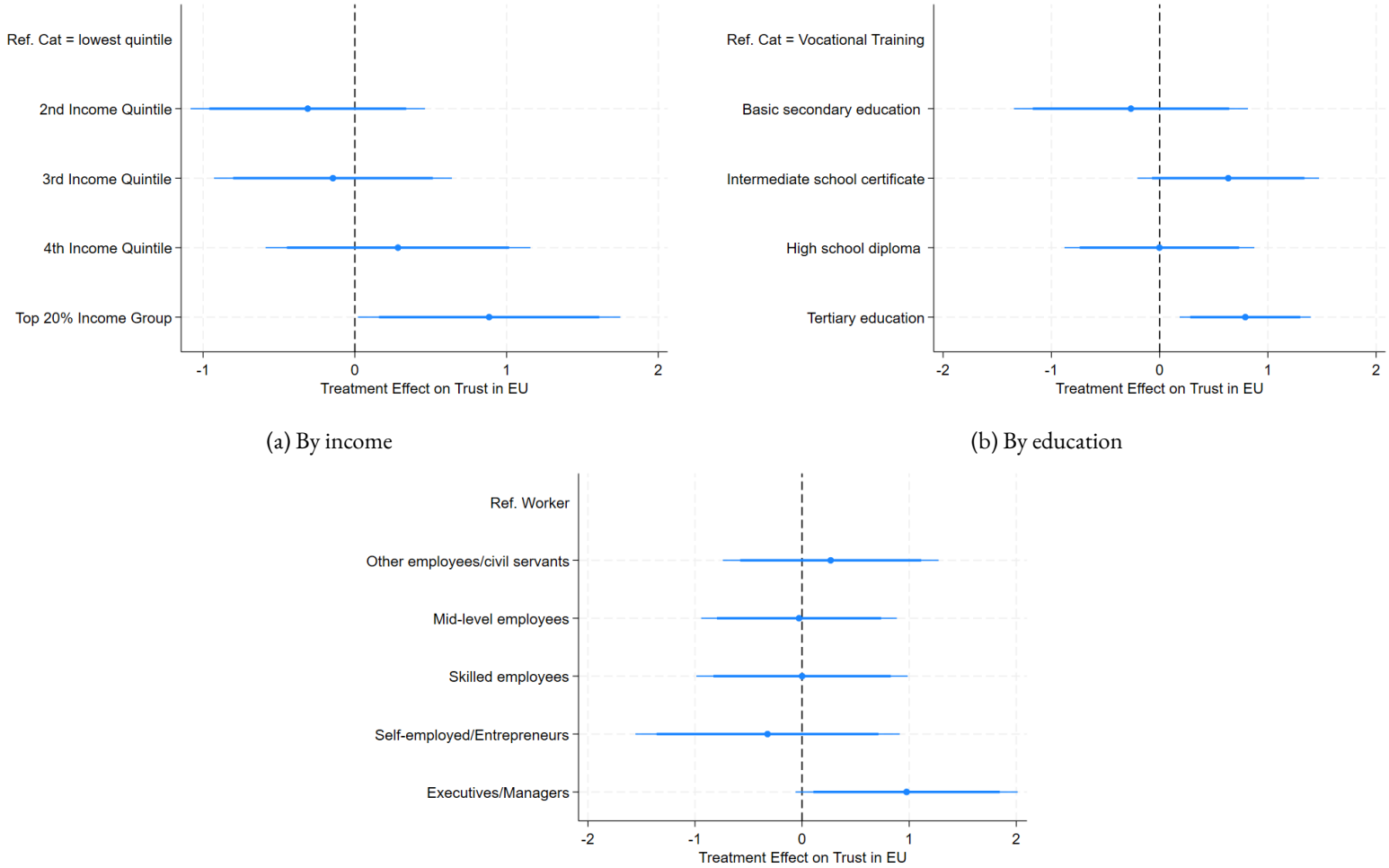


Note: This figure presents the association between education and sentiment to the response to the treatment condition. The results control for age and gender and include state fixed-effects.

Figure A11 shows how the written sentiment of treated individuals differs across education in comparison to respondents with no education. Similar to the findings from Figure 7, treated individuals of the highest education category report a more positive sentiment than respondents who have vocational training.

A4.5 Marginal Effects of survey analysis

Figure A12: Marginal Effect on Trust in EU by Sub-Groups with reference categories



(a) By income

(b) By education

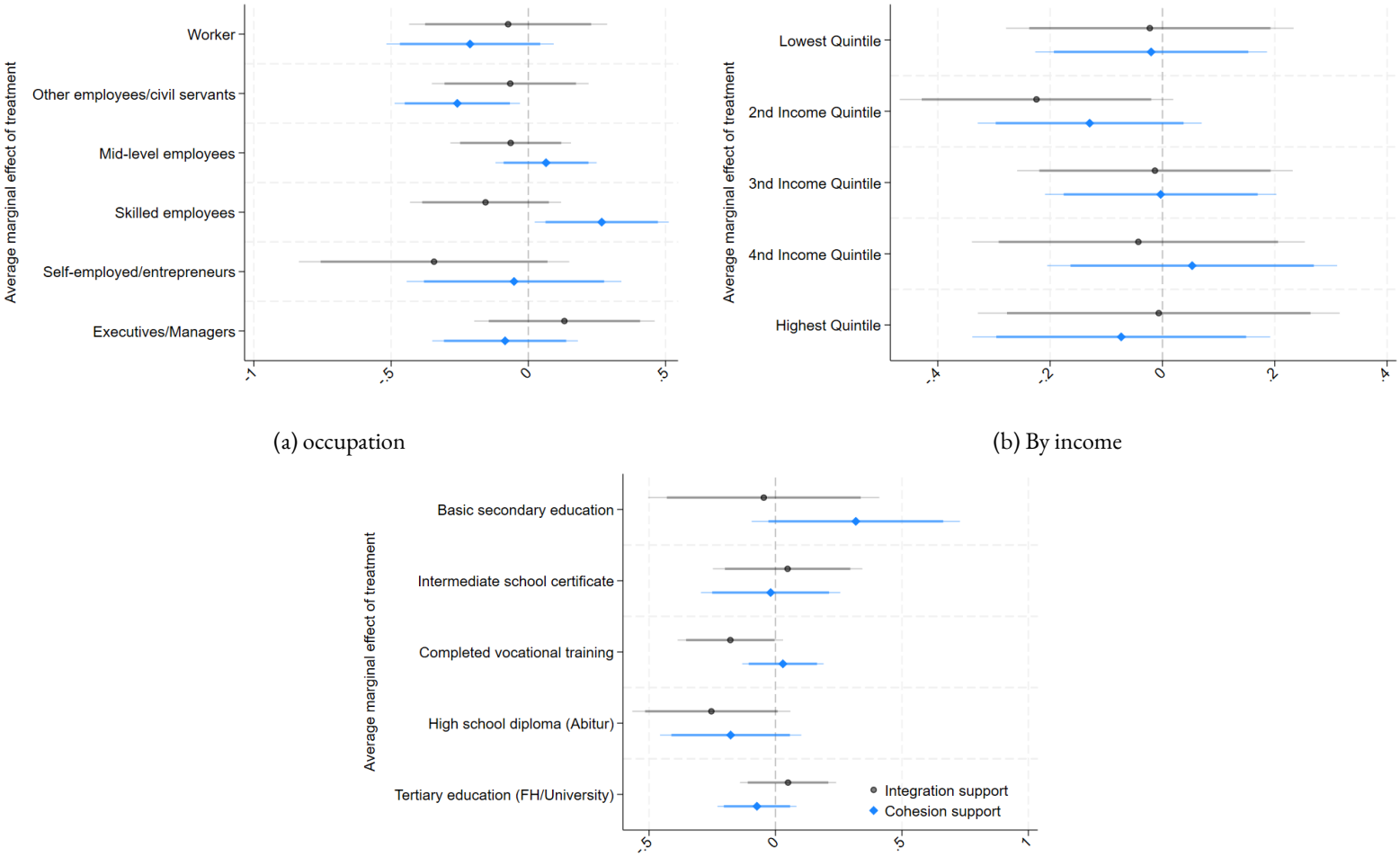
(c) By occupation

Note: This figure displays the marginal effects of the treatment across categories, using the lowest category as the reference group. It includes age, gender, and funding controls.

Next, we present the analysis of marginal effects to evaluate whether the treatment effect of the top socio-demographic categories is significantly stronger than that of the lowest category. For this analysis, we use interaction models, using the lowest categories of income, education and occupation (lowest income quintile, vocational training, worker) as the reference category. Indeed, we find that the treatment effect is significantly stronger for the highest-income and highest-education group than for their respective reference categories ($p < 0.05$). For occupation, we find a similar relationship, however, it only reaches a significance level of $p < 0.1$.

A4.6 EU integration & Cohesion Policy

Figure A13: Marginal Effect on EU integration and Cohesion policy by sub-groups



(a) occupation

(b) By income

(c) By education

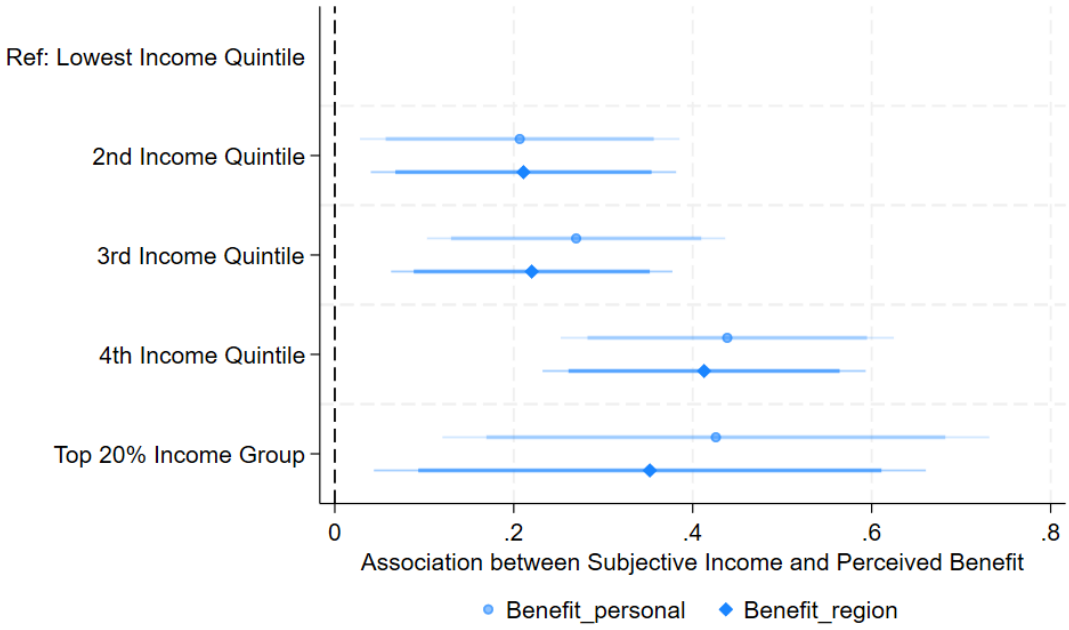
Note: This figure displays the marginal effects of the treatment across categories. It includes age, gender, and funding controls.

Figure A13 shows the results reiterating the analysis from Figure 4, but with different dependent variables and including education. While the specifications of the models are the same, the models now examine the treatment effect on 1) EU integration support and 2) support for Cohesion policy. Thus, while the dependent variable in the main text, EU trust, measured a more general attitude towards the EU, these two variables are closer to the analysis of support for actual policy changes. The results show little evidence of heterogeneous treatment effects across occupational, income, and educational groups. This outcome contradicts our initial theoretical expectations. At the same time, these findings shed light on important constraints in the political effectiveness of EU funding. We argue that the absence of significant effects could be attributed to two main reasons. First, they might suggest

that EU funding is limited to alter support for general attitudes towards the EU, but is not capable of changing support for actual policy changes. A second reason could be the case selection of our experiment. As Germany contributes a significant share to the EU budget, the results from our survey experiment should be considered conservative estimates. Thus, we suggest that future research should engage with possible differences of EU funding on multiple dependent variables.

A4.7 Perceived Benefit

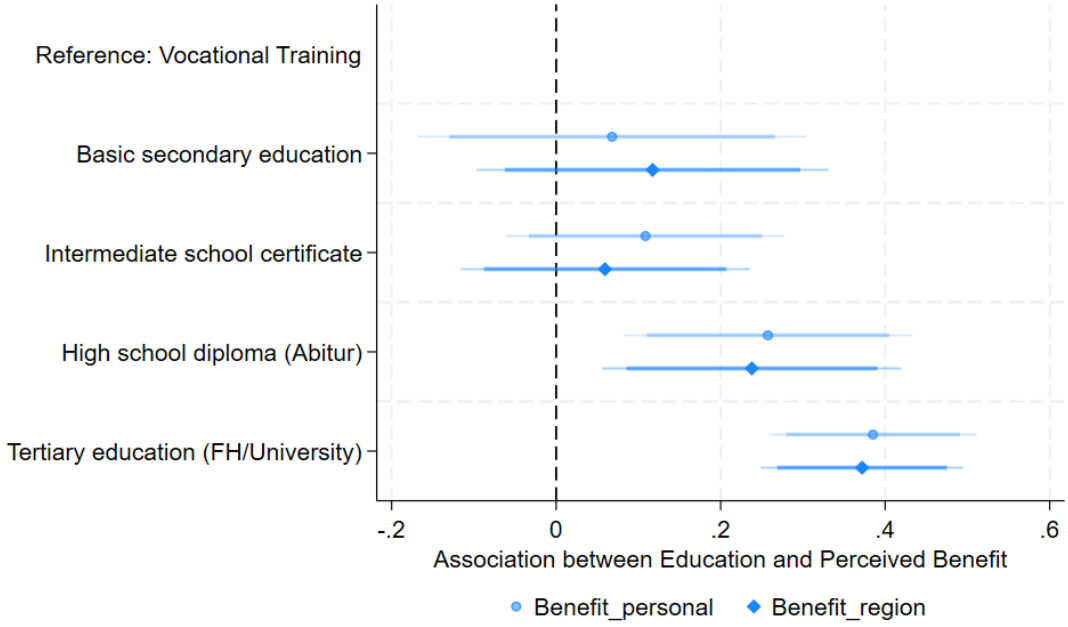
Figure A14: Perceived Benefit of EU regional policy by subjective income



Note: This plot depicts the perceived benefit of EU regional policy by subjective income category. Categories are compared to the lowest category of subjective income.

Figure A14 complements the results from Figure 6 by differentiating the association between the perceived benefit of the EU Cohesion Policy and the subjective income of respondents. In comparison to the results from objective income in Figure 6, now even all income categories show a significantly stronger association to perceived benefit than the lowest income category.

Figure A15: Perceived Benefit of EU regional policy by education



Note: This plot depicts the perceived benefit of EU regional policy by education. Categories are compared to the lowest category of education.

Figure A15 also presents the results of the association with the perceived benefit of the EU Cohesion policy across education categories. The results are in line with the other analyses of socio-economic classes and perceived benefit. The higher the education category, the higher the perceived benefit of the EU Cohesion policy.