Immigration vs. poverty: Causal impact on demand for redistribution in a survey experiment ☆

Andrea F.M. Martinangeli [a], Lisa Windsteiger [b],*

[a] *CEREN EA 7477, Burgundy School of Business, Université Bourgogne Franche Comté, Dijon, France*
[b] *Ludwig Erhard ifo Center for Social Market Economy and Institutional Economics, Germany*

ARTICLE INFO

ABSTRACT

We investigate how demand for both the financing and the provision of redistributive policies is affected by information about immigration and poverty. Information about immigration has a positive impact on desired tax progressivity among low-income respondents and a negative one among higher income earners. Information about poverty has no impact. On the provision side, middle- and high-income respondents increase desired public education expenditure in response to poverty, while low-income respondents reduce desired education spending in response to immigration. These heterogeneities are consistent with protectionist reactions to immigration and poverty.

1. Introduction

Immigration has played a central role in the political debate and in nearly every electoral competition taking place in Europe and North America over the past decade, overtaking inequality, poverty and redistribution as a major topic in the political discussion.[1] Many political races culminated in the rise of extremist and populist parties focusing their campaigns around immigration, while political actors promoting redistribution and social welfare expenditures were pushed to the margins, often by those segments of the population whom such policies would benefit most (see, for instance, Lamble (2018) and Ember (2019) for the United Kingdom and United States respectively).[23]

These electoral successes might be explained by the economic and socio-cultural significance of immigration in the eyes of the electorate and its importance relative to other political topics (Hatton, 2017; Coester, 2018; Naumann and Stoetzer, 2018). Erosion of

popular support for the welfare state might thus have been brought about by increases in social diversity, its economic consequences, and cultural conflicts (Gilens, 1995; Lee and Roemer, 2006; Roemer et al., 2007; Eger, 2010; Larsen, 2011; Cappelen and Midtbø, 2016; Bisin and Zanella, 2017).

Despite their recent political misfortune, however, redistribution, income support and poverty relief remain central to the contemporary socio-economic and demographic challenges faced by Western social welfare systems, as demonstrated by the Covid-19 pandemic outbreak and by the disproportionate impact on poor households of the ensuing economic crisis (Stantcheva, 2021). Poverty and redistribution have thus made their return as prominent debate topics in the political discourse. Crucially, so will they remain especially and precisely in connection with the contemporary migratory fluxes which, save for the Covid-19 "intermezzo", paced up dramatically over the past decade (see, e.g., Hatton, 2016): First because immigrants are often perceived as threatening the livelihoods of vulnerable (e.g. low-skilled) native labour force groups, and simultaneously because migrants are often regarded as vulnerable groups requiring welfare support themselves.

Our first aim in this paper is therefore that of investigating how providing information and thus increasing people's awareness about immigration and poverty, two important and recurring themes in the public debate, affects their demand for redistributive policies. Concretely, we run a survey experiment on a representative sample of the German population in which we randomise whether the respondents receive no information (our baseline), information on the extent of immigration (Immigration condition) or the extent of poverty (Poverty condition). Subsequently, the respondents proceed to answer a block of questions eliciting their demand for redistribution. To gain a better insight into how poverty and immigration interact in respondents' minds to shape their redistributive preferences, we complete the design by adding two further interventions. The first provides respondents with information about the extent of both immigration and poverty (Both condition), the second adds information about the extent to which the immigrant and the poor populations overlap (Overlap condition).

Our second aim is that of distinguishing between and investigating, simultaneously but separately, the two components of 'demand for redistribution'. We thus explore how raising awareness about immigration and/or poverty affects demand for (i) the financing and for (ii) the provision of redistributive policies. The first component pertains to how individuals wish to shape the income tax schedule, in terms of its progressivity, to raise public revenue to finance redistributive expenditure. The second pertains instead to their preferred allocation of the public budget over redistributive policies. The separate analysis of these two components allows us to better capture individuals' nuanced preferences over an inherently complex public intervention like redistribution. The distinction between the financing and provision of redistributive policies has been recently found to be consequential in the investigation of individuals' preferences (Cavaillé and Trump, 2015; An and Ye, 2017).

Our investigation targets the effect of exposure to experimentally comparable information about immigration and about poverty on individuals' demand for redistribution. Since (as explained in Section 4) we expected our experimental conditions to work mainly via the "priming" channel, our design allows us to analyse the effect of "thinking about" immigration and poverty on people's attitudes. In a political debate in which both immigration and poverty play a role, how does raising awareness about these topics impact individuals' support for redistribution? Are the effects of the two issues similar in sign and magnitude? Crucially, previous research highlights how demand for redistribution is inherently heterogeneous across income classes, and it is reasonable to expect individuals differently positioned in the income distribution to react differently to changes in the economic environment (Sands, 2017; Alesina et al., 2018a; Naumann et al., 2018; Naumann and Stoetzer, 2018; Bussolo et al., 2021). Thus we also ask: How does the impact of information about immigration and about poverty vary along the income gradient?

Previous research proposes two opposing mechanisms for how thinking about immigration might affect individuals' demand for redistributive policies. According to the so-called *conflict theory*, if they predominantly associate immigration with reduced social cohesion due to ethnic diversity, this would cause natives to withdraw their support for redistribution and income support programs (supposedly) benefiting groups perceived as socially distant (Luttmer, 2001; Alesina et al., 2004). This argument is in accordance with the rise of anti-immigration platforms and parties mentioned in the opening of this article.

The *protection theory* instead distinguishes individuals as net beneficiaries or contributors to the welfare state (e.g. Magni-Berton, 2014; Naumann and Stoetzer, 2018). As (low-skilled) immigration is expected (or perceived) to depress wages and threaten job security in high-immigration sectors (Borjas, 2003), unskilled and low-income earners should increase their demand for redistribution if thinking about immigration reminds them of migration inflows perceived as threatening their job security (Facchini and Mayda, 2009; Iturbe-Ormaetxe and Romero, 2016).[4] High-skilled individuals with high incomes – net contributors to the welfare system – are expected instead to withdraw their support, as they are reminded that their share of the welfare burden is high in the presence of (low-skilled) immigration.

Similar to immigration, thinking about poverty might have two opposite impacts on demand for redistribution according to the placement of the individual in the income distribution (Bussolo et al., 2021; Sands and de Kadt, 2020).[5] It might first of all remind poor individuals of their status as net beneficiaries of the welfare state, thus pushing them to increase their demand for public expenditure on redistributive policies and for progressive income taxation to finance them (findings consistent with this reading are for instance in Sands and de Kadt, 2020; Iturbe-Ormaetxe and Romero, 2016; Andreoli and Olivera, 2020). Second, it might remind the richer net contributors to the welfare state of the expenses they bear to finance social safety nets benefiting other social segments. This is the case for instance in Sands (2017), where the richest strata of the population are found to withdraw their support for

---

[4] Along these lines (Harms and Landwehr, 2020) find low income individuals to support direct democracy referenda if they expect a clear majority to back redistribution.

[5] For accounts of the direct effect of positional concerns on demand for redistribution see Kuziemko et al. (2014), Kim (2019) and Martinangeli and Windsteiger (2021).

redistribution once experimentally primed with poverty.[6] We therefore investigate how the reactions elicited by information about immigration and poverty compare in terms of demand for redistribution once jointly investigated in a unified framework enabling their direct comparison within different portions of the income distribution.

We follow a burgeoning stream of literature using survey experiments to investigate the causal link between socio-economic phenomena of interest on self-reported preferences for redistributive interventions (Kuziemko et al., 2015; Karadja et al., 2017; Barrera Rodriguez et al., 2017; Alesina et al., 2018a; Barton and Pan, 2021). Different from traditional survey-based investigations (Senik et al., 2009; Alesina et al., 2018b), this approach allows us to rely on systematic differences in the responses to target questions induced by exogenous variation in the emphasis placed on specific elements of interest across the sample. That is, by randomly distributing information about poverty and immigration our survey experiment can be taken to randomise the salience given to poverty and immigration by electoral and media debates. Systematic relationships observed between the experimental conditions in the answers to target survey questions can therefore be interpreted as causal effects of the information provided on demand for redistribution.[7]

We find that information about immigration has a sizeable effect on desired tax progressivity and redistributive public expenditure, with opposite signs for high income and low income individuals: High income earners desire *less* tax progressivity, offset by poor individuals requesting *more* progressive taxation. On the provision side, low income respondents want *less* public spending on public education in the presence of information about immigration. Information about poverty has no detectable impact on desired tax progressivity, but *increases* high income respondents' desired education spending.

Taken together, these results first of all offer evidence for protectionist attitudes affecting demand for redistribution, in particular on the financing side, i.e. desired tax progressivity. Similar mechanisms seem however to be at play on the provision side as well. Poor people's lower demand for education expenditure once confronted with information about immigration might reflect the fear that public education would disproportionally benefit immigrants (and their children), thus increasing competition for themselves on the labour market. Conversely, and consistent with the discussion in Bussolo et al. (2021), richer people's increase in desired education spending might result from a belief that education is a relatively painless way to deal with poverty and inequality, accompanied by an expectation that it is likely to, on average, also benefit themselves or their children. Immigration and poverty seem therefore to elicit somewhat different reactions along the income gradient.

The paper is organised as follows: Section 2 summarises related literature. Section 3 describes the survey, followed by an account of the survey experimental method and of the experimental design. Section 4 formulates our hypotheses, Section 5 presents the results and Section 6 concludes.

2. Related literature

There are several recent studies examining the effect of immigration on voting behaviour of natives in a number of European countries. Halla et al. (2017) (Austria), Edo et al. (2019) (France), Dustmann et al. (2018) (Denmark), Becker and Fetzer (2016) (UK) and Bellucci et al. (2019) (Italy) all find a positive relationship between regional immigration shares (refugees in the case of Dustmann et al. (2018), perceived or expected refugee shares in Bellucci et al. (2019)) and voting for (extreme) right-wing parties with strong anti-immigration positions. On the other hand, Steinmayr (2020) shows that direct exposure to refugees (due to temporary refugee accommodations being located in one's home village) weakens this effect.[8]

Another branch of the literature examines the effect of immigration shares on redistributive policies directly. Recent (panel) survey evidence from Dahlberg et al. (2012) (Denmark), Schmidt-Catran and Spies (2016) (Germany) and Alesina et al. (2018b) (16 Western European countries) points to a negative correlation between regional immigration shares and support for redistributive policies.[9] In closely connected research, ethnic diversity is linked to demand for redistribution and public goods provision (Alesina and La Ferrara (2005) and Elsner and Concannon (2020) offer overviews of the recent literature): Luttmer (2001) finds that ethnic diversity lowers desired welfare spending in US neighbourhoods, and Alesina et al. (2001) find that it lowers the size of public transfers. In a theoretical model, Dewan and Wolton (2019) show that, in combination with labour market discrimination against minorities, the majority population will vote for less redistribution.[10]

Attitudes towards immigrants have also been investigated in connection with individuals' economic concerns. Recent examples are Daniele et al. (2020), who show theoretically, and exploit the Covid-19 epidemic to support empirically, that negative economic shocks radicalise anti-immigration sentiments. Naumann et al. (2018), who find that in Europe rich natives prefer highly skilled over low-skilled migration more than low-income respondents do, but that highly skilled migrants are preferred over low-skilled migrants

---

[6] Further support for this conjecture comes from Côté et al. (2015), who find that exposure to high economic inequality is associated with lower generosity among higher income individuals. See also Nishi et al. (2015), who find that richer individuals are less likely to cooperate with others if inequality is (visibly) high.

[7] Section 3 offers a more detailed discussion of the methods.

[8] While these studies are very insightful, we cannot use them to directly assess the effect of immigration on natives' demand for redistribution. First, the examined anti-immigration parties tend to differ in terms of their position on redistributive policies (and often even do not have a clear position on this issue). Second, as we mention in the introduction, parties offer a multi-dimensional policy platform of which redistribution is only one part. It might well be that voters simply value anti-immigration policies more than redistribution and thus vote for the far-right, even though they would oppose their policy proposals concerning redistribution.

[9] However, Auspurg et al. (2019) show that the relationship found in Schmidt-Catran and Spies (2016) does not hold once different time trends are allowed for East and West Germany.

[10] See Mayr (2007) and Gonnot (2022) for models in which natives vote on the political enfranchisement of foreigners.

irrespective of natives' skill levels. On the other hand, Hainmueller and Hiscox (2010) and Hainmueller and Hopkins (2015) find that in the US natives do not differ in their acceptance of immigrants depending on income or skill level.

Our paper is related to the growing literature using survey and field experiments to analyse the effect of information about immigration on attitudes towards immigrants. Facchini et al. (2016) and Grigorieff et al. (2016) find that information improves attitudes towards immigrants in Japan and the US, respectively. Haaland and Roth (2020) find that informing people in the US about research showing no adverse labour market impacts of immigration leads to higher support for immigration (in terms of self-reported attitudes and real petition signatures). Some recent survey experiments examine the effect of immigration on demand for redistribution. Alesina et al. (2018a) find that priming respondents about immigration lowers support for redistribution.[11] Naumann and Stoetzer (2018) find that priming about immigration leads to reduced support for redistributive policies among the well-off and among people with low labour market competition.

Concerning other determinants of demand for redistribution, Kuziemko et al. (2015) examine in a survey experiment in the US how information about inequality and poverty affects redistributive preferences and find that it increases people's concerns about justice and the economy, but does not translate into higher demand for (government) redistribution. A survey experiment conducted by Cruces et al. (2013) in Buenos Aires finds that informing people about their (relative) position in the income distribution increases demand for redistribution among the poor (who otherwise tend to overestimate their relative position). Analogous results with opposite sign for individuals who underestimate their relative position are found in Sweden by Karadja et al. (2017). Windsteiger (2022) relates demand for redistribution to perceived (rather than actual) inequality, which is in turn affected by the degree of (socio-economic) segregation. In a field experiment, Sands (2017) finds that exposure to poverty leads to a reduction of support for a millionaire tax among the rich. Fong (2001) examines data from the Gallup Poll Social Audit Survey and finds that people's beliefs about the causes of poverty determine how they want to react to it: They demand more redistribution if they believe poverty originates from bad luck than if they believe it is due to lack of effort.

The main contribution of our survey experiment is that (to our knowledge) it is the first to investigate the impact of information about both immigration *and* poverty on demand for redistribution. In addition, we show that demand for the financing and for the provision of redistributive policies (Cavaillé and Trump, 2015; An and Ye, 2017) are impacted differently by immigration and poverty, and that the effects are strongly heterogeneous over the income distribution.

3. Experimental method and survey design

3.1. Questionnaire

The survey consisted of 32 questions in total.[12] These included (in order): 5 questions on the respondent's demographics (gender, age, marital status, household composition and income bracket), 2 target questions eliciting the respondents' preferred tax schedule and allocation of public expenditures, 9 questions eliciting attitudes towards redistribution, government intervention and poverty (preferred tax rate on income quantiles, preferred allocation of the public budget, attitude towards inequality, power of the government to tackle inequality, preferred degree of government intervention against inequality, attitudes towards increasing the public budget for (i) schools in poor districts, (ii) social housing, (iii) social security benefits for the poor, trust in the government, luck versus effort as determinant of economic success, attitudes towards poverty in Germany), 10 further background questions (educational attainment, employment status, high immigration professional sector, country of birth, parents born in Germany, German state and region of residence, preferred media, positioning on the political spectrum, voting behaviour, political party preference), and 6 subjective beliefs questions (size of the immigrant population, its origins within or outside of the European Union, size of the poor population, size of te overlap between the poor and the immigrant population, its origins within or outside of the European Union, and perceived job security).[13] Fig. 1 at the end of Section 3 provides a schematic summary of the survey flow.

The first set of 5 demographic questions were elicited early as they formed the basis upon which representativeness of the final sample was ensured. Immediately after these questions, we randomise the display of short videos delivering our experimental conditions (more detail to follow in Section 3.5). Next, the respondents answered our target questions. Our hypotheses rest on the assumption that immediate previous exposure to different experimental conditions will cause differences in the responses to our target questions. We then elicited the respondents' attitudes, and further background questions allowing us to split the analysis across relevant socio-demographic groups and thus further delve into potential heterogeneities. Finally, the beliefs we elicited at the end of the questionnaire serve two main purposes. First, we gain an insight into how such beliefs are distributed in the general population by analysing those collected from the untreated subsample.[14] Second, we are able to gauge whether and to what extent the respondents paid attention to our experimental conditions by comparing the distribution of beliefs collected from the untreated with those collected from the treated respondents. The latter are a priori expected to be more concentrated around the values provided in the experimental conditions than the former.

---

[11] In a very informative follow-up paper, Alesina and Stantcheva (2020) demonstrate in a theoretical model how this effect can result from (mis-)perceptions about immigrants' productivities, biases against immigrants and an interaction of the two.

[12] The survey began with a paragraph summarising the funding sources, the general purpose of the survey, the affiliation of the researchers (kept anonymous) with the Max Planck Society, and the respondents' GDPR rights. The respondent had to approve a declaration of consent in order to participate in the study. See Appendix E for the full questionnaire (translated to English), and https://taxmpg.eu.qualtrics.com/jfe/form/SV_eKU8tuWb3OqHBVH for the German survey.

[13] We elicit job security at the very end of the survey because an early elicitation would have likely interacted with our experimental conditions and biased our results.

[14] However, see the discussion at the beginning of Section 4 and Appendix B.1 for why we think these beliefs are only limitedly informative.

| Demographics (quotas) | The participants are screened to ensure sample representativity (age, gender, income and location) |

| **Experimental conditions**:
 - Neutral
 - Immigration
 - Poverty
 - Both
 - Overlap | The participants receive one of five experimental conditions |

| Target questions:
 - Assignment of tax rates
 - allocation of public budget | We elicit the participants' demand for redistribution |

| Attitude questions:
 - Inequality, poverty, trust in government, government intervention, origin of wealth (luck or effort) | The respondents provide their attitudes and preferences over topics and outcomes related to public sector redistribution |

| Further demographic background questions | We collect further non-quota background characteristics |

| Subjective beliefs elicitation | The participants provide their subjective beliefs about the sizes of the immigrant and poor populations. |

Fig. 1. Flowchart of the subjects' progress through the survey.

3.2. Measuring outcomes

To investigate the effect of our experimental conditions on support for redistribution, we target both the financing and the supply of redistribution policies.[15] We elicit respondents' preferences over how to finance the collection of a public budget of a given size as in Alesina et al. (2018a). This choice prevents our respondents from expressing their potentially confounding preferences over the size of the public budget in addition to those over how to collect it.[16] We ask the respondents to allocate the tax burden (average tax rates) to key quantiles of the income distribution: the bottom 50%, the next 40%, the following 9% and the top 1% via a series of sliders (see Fig. E.2 in Appendix E).[17] A background programme multiplied the chosen tax rates by the total income earned by

---

[15] Correction for multiple hypothesis testing is discussed in Appendix C.7.

[16] While interesting, this distinction does not inform us on the individual's choices of interest. It is hence removed from the respondent's action space to exclude potential confounds and is left for future research.

[17] Having fixed the size of the budget, a more progressive preferred tax schedule will indicate a greater demand for redistributive taxation.

each quantile and returned the public revenue raised by the respondent in real-time. The goal for each respondent was to raise a public budget of a fixed size, roughly corresponding to the revenue collected by the German government via income taxes in 2017. The stated preferred tax rates were then used to construct an index of desired tax progressivity: $\tau = t_{(100)} + t_{(91-99)} - t_{(51-90)}$. This variable consists of the unweighted sum of the desired tax rates on the top 1% and top 10%, minus the desired tax rate on the next 40%. We do not include the bottom 50%, since this group pays already very little in taxes in Germany and can thus be considered irrelevant for the purpose of redistributive taxation.[18] We show in Appendix C.4 that our results hold for different specifications of our index, including when accounting for the bottom half of the income distribution. We further show that our treatment effects are concentrated on tax rates on the top 1% and 51st to 90th percentiles of the income distribution, with no effect found for preferred tax rates on the bottom 50% and upper-middle income group (91st to 99th percentiles).

Next, we elicit the respondents' demand for the supply of redistributive policies. To this end, we ask them about their preferred allocation of a public budget of a fixed size to different public policies and public goods, again as in Alesina et al. (2018a). We asked each individual to state what proportion of the budget they wished to allocate to (i) *public infrastructure*, (ii) *domestic security*, (iii) *public education*, (iv) *social insurance, disability support and pensions*, (v) *unemployment and welfare benefits*, (vi) *public healthcare* and (vii) *affordable housing*. We constrained the sum of all stated shares to sum up to 100 (see Fig. E.3 in Appendix E).[19]

3.3. Sample

We surveyed 4000 individuals from a representative sample of the German adult population.[20] The questionnaire was distributed online by the panel provider Respondi.[21] Random assignment with equal probability to the five experimental conditions yields a target sample size per experimental condition of 800 individuals.[22] Effective sample sizes per condition and sample balance tests are reported in Appendix A.

3.4. Causality and demand effects

Experimental survey studies (e.g. Alesina et al. (2018a), Naumann and Stoetzer (2018) and Kuziemko et al. (2015)) have emerged as an important investigation strategy to overcome the endogeneities inherent in responses to traditional surveys (Cappelen and Midtbø, 2016). Survey experiments allow for the identification of causal relationships between the experimental conditions under which the respondents answered the questionnaire and their responses to target questions within the survey. Specifically, our respondents are first randomly assigned to different groups of roughly equal size. Each group is then given (if at all) information about the extent of either immigration or of poverty in Germany. After having received the information, all respondents are asked about their preferences for specific redistributive policy interventions the state could enact, their attitudes towards government intervention and towards poverty. Because they were randomly administered over the whole sample, any systematic relationship uncovered between the experimental conditions and the respondents' preferences for specific policy measures can thus be interpreted as causal.

A common concern, especially with unincentivised survey experiments, is the potential presence of experimenter demand effects: The risk that any effect of the experimental intervention might be due to the respondents systematically reacting to an implicit "demand" on behalf of the experimenter (Zizzo, 2010). Mummolo and Peterson (2019) show however that experimenter demand effects largely leave results of online survey experiments unaffected. Further, as argued in Alesina et al. (2018a), our variables of interest are sufficiently disconnected from the information presented in the Immigration condition to not give rise to serious concerns about demand effects. In addition, it is unclear, from a respondents' perspective, what the "correct" way to behave would be Zizzo (2010). Demand effects might ex-ante be of greater concern in the Poverty condition. Our results are however at odds with the behavioural patterns that could potentially arise from demand effects (see Section 5). Such concerns are further reduced by the implementation of a between-subject design, also reducing the risk of any social desirability bias arising. Finally, de Quidt et al. (2018) show that responses do not differ systematically across incentivised and non-incentivised online experimental designs.

---

[18] See for instance https://www.iwkoeln.de/studien/iw-kurzberichte/beitrag/martin-beznoska-wer-zahlt-wie-viel-einkommensteuer-in-deutschland-405957.html.

[19] Analogous to the elicitation of preferred tax schedules, we thus keep the total size of the public budget constant. By allocating a larger percentage of funds to redistributive items (such as unemployment and welfare benefits) compared to less redistributive ones (such as infrastructure), respondents can vary the degree of redistribution that is implied by their chosen spending allocation.

[20] The sample is representative with respect to gender, age, income and geography. Sample balance analysis is presented in Table A.2 in Appendix A. Notice that we did not set quotas to ensure representativeness along the education dimension. However, the distribution of educational attainment in our sample is roughly consistent, allowing for differences in sample restrictions, with that reported by the Statistisches Bundesamt (available at https://www.destatis.de/EN/Themes/Society-Environment/Education-Research-Culture/Educational-Level/_node.html).

[21] https://www.respondi.com/EN/.

[22] With such sample size we are able to detect differences of size $d = 0.15$ at alpha $= 0.05$ with a power $p > 0.8$ in pairwise experimental condition comparisons of the means of summary indices of desired tax progressivity using the Neutral condition as a baseline, scaled to unit standard deviations.

3.5. The experimental conditions

The respondents were shown a short video exposing them to the experimental condition they were randomly assigned to.[23] Specifically, the information respondents received in each condition was as follows.[24]

Neutral condition. Respondents in the Neutral condition were provided no information about neither immigration nor poverty. Instead, the video only reminded them of the total size of the resident population in Germany (82 Millions).

Immigration condition. Respondents in the Immigration Condition were reminded of the total size of the resident population in Germany, and in addition were informed about the size of the resident population that was *born abroad* (13.2 Millions).[25]

Poverty condition. Respondents in the Poverty Condition were reminded of the total size of the resident population in Germany, and in addition were informed about the size of the poor population (13.7 Millions). The poverty line was transparently defined as an income equalling 60% of median income.

Notice how our experimental design relies on the approximate equal size of the immigrant and poor populations in Germany (13.2 and 13.7 millions, respectively) in 2018. We can thus exclude that any systematic impact of the two pieces of information be attributed to the numeric difference between population sizes.

Further, this feature allows us to complement our design with two additional experimental conditions allowing us to dig deeper into the relationship between immigration and poverty and their impact on demand for redistribution. These conditions address the following questions: How is demand for redistribution impacted by the simultaneous presentation of information about immigration and poverty? Do the effects cancel out and what is their interplay in various segments of the income distribution? We therefore expand our design by including two conditions, described below, in which the information about the size of the immigrant and of the poor populations is presented jointly, one of which also includes information about the extent to which the two populations overlap.

Both condition. Respondents in the Both condition were reminded of the total size of the resident population in Germany. In addition, they were informed about both the size of the resident population that was born abroad, *and* about the size of the resident population living on an income falling below the poverty line. We control for order of presentation effects by randomising the placement (left or right on screen) in which the information about immigration and about poverty was presented.

Overlap condition. Finally, respondents in the Overlap condition were reminded of the total size of the resident population in Germany, they were informed about both the size of the resident population that was born abroad, *and* about the size of the resident population living on an income falling below the poverty line. In addition, they received information about the size of the resident population which was both *born abroad and lives on an income below the poverty line* (3.2 Millions). We control for order of presentation effects by randomising the placement in which the information about immigration and about poverty was presented. Information about the overlap was presented at the centre of the screen.

4. Hypotheses

Our interventions might operate via two channels: First, by mentioning immigration and/or poverty, we (temporarily) increase the salience/awareness of those issues and this alone can affect the way people think about redistribution later on, irrespective of the actual information (in terms of numbers) that is delivered via our intervention. Put simply, if immigration and poverty matter for people's demand for redistribution, by making those topics salient right before asking our questions of interest, we emphasise their effect on people's support for redistribution (compared to the Neutral condition). Second, our interventions also deliver factual information about poverty and immigration. The effect of this information on people's demand for redistribution might depend on their prior beliefs and on the direction in which they are updated.

The information in our experimental conditions was however most likely one that most people would not have any clear prior beliefs about. Respondents might think that immigrants are "(too) many" or that poverty is "(too) high" irrespective of actual figures and will hardly have thought about a precise number (in Million people). Eliciting prior beliefs (in terms of numbers) about these topics will thus most likely lead to random guesses by our respondents and yield very little useful information.

Moreover, asking respondents to state prior beliefs in terms of concrete numbers when they have never previously had concrete numbers in mind can even harm the survey experiment, as it might lead to (i) the formation of *ad hoc* beliefs with no solid grounds which risk being interpreted and used as meaningful, and (ii) anchoring effects or unresponsiveness caused by respondents' desire

---

[23] Screenshots of the information provided and links to the information videos can be found in Appendix D. Each respondent viewed only one of the videos.

[24] Sources: Statistisches Bundesamt (see https://www.destatis.de/DE/Presse/Pressemitteilungen/2018/08/PD18_282_12511.html), and Poverty Report 2018 (Deutscher Paritʾatischer Wohlfahrtsverband, see https://www.der-paritaetische.de/fileadmin/user_upload/Schwerpunkte/Armutsbericht/doc/2018_armutsbericht.pdf).

[25] We can define immigrants in two ways: By citizenship (i.e. all people living in the country who do not have citizenship) or by country of birth (i.e. all residents who were born abroad). Following Alesina et al. (2018a), we chose the latter for our experimental conditions. This definition provides a simple and general wording, abstracting from immigrants' idiosyncratic choice of whether or not to apply for citizenship (a choice which also depends, to a large extent, on their country of origin: For instance EU citizens can indefinitely reside in Germany without naturalisation). We however acknowledge that the respondents might have reacted differently to the treatment had we emphasised that a subset of the 13.2 million immigrants have obtained German citizenship, requiring German language tests and proof of attachment to the country.

to remain consistent later on in the survey (e.g. Tversky and Kahneman, 1974; Koehler and Harvey, 2008; Epley and Gilovich, 2006; Mochon and Frederick, 2013). Providing respondents with the correct numbers can hence hardly be interpreted as "correcting" their beliefs upwards or downwards, since there probably were no precise (quantitative) beliefs about these numbers in the first place (see also Appendix B for a more detailed discussion).

Finally, recent studies such as Alesina et al. (2018a) and Naumann and Stoetzer (2018) have demonstrated that awareness and not information provision (correction of biased beliefs) matters in driving individuals' responses to immigration in terms of support for redistributive policies.

For these reasons we believe our experimental conditions to operate primarily through the awareness channel, we refrain from eliciting quantitative prior beliefs, and we formulate our hypotheses accordingly. Nevertheless, comparisons of posterior beliefs across groups might illuminate on how effective our experimental conditions were in focusing our respondents' attention on the topics of interest. We thus adopt a posterior-only design to perform manipulation assessments.[26]

How our experimental conditions affect demand for redistributive policies also depends on whether individuals maximise their own material utility or total (or average) social welfare (and if the latter, on which relative weights they attach to different social groups, for instance immigrants or natives). In formulating our hypotheses, we assume that respondents put their own material interests first (see for instance Harms and Landwehr (2020)). That is to say, if their own and society's interests are at odds, we assume that personal economic interests will dominate. For this reason, we formulate different hypotheses for different income groups.

Note furthermore that, while our survey experiment examines the effect of *making people think about* immigration and poverty, our hypotheses are (to some extent) also informed by empirical or theoretical findings about the impact of *actual* immigration and poverty on demand for redistribution. If thinking about immigration or poverty reminds people of the (expected) effect of these phenomena on themselves (and on welfare state finances and society as a whole), this approach will yield predictions which can be brought to the data to illuminate on the specificities of exposing individuals to a social discourse on these topics.

Finally, we generally expect our treatments to affect desired tax progressivity and demand for various public spending items in different ways. We thus formulate separate hypotheses for our treatments' impact on these different aspects of demand for redistribution.

4.1. Immigration condition

4.1.1. Immigration and desired tax progressivity

Alesina and Stantcheva (2020) propose a concise theoretical model to explain the impact of immigration on demand for redistributive taxation. In their model, natives maximise a generalised social welfare function. How demand for redistribution reacts to immigration depends on (i) the relative weights assigned to immigrants compared to natives in the welfare function and (ii) the degree to which immigrants are, relative to natives, perceived as more or less reliant on the welfare state ("freeloading") rather than as participating in the labour market. If natives are not biased against immigrants per se (i.e. if they do not assign them lower weights in the welfare function) and do not perceive them as more likely to be "freeloaders", immigration should not affect their demand for redistribution. However, should natives (mis-)perceive immigrants as more likely to be "freeloaders" than natives, then higher immigration rates will be associated with lower demand for redistribution. This effect will be exacerbated if in addition natives are biased against immigrants, i.e. if they put lower weight on immigrants in the social welfare function regardless of them being considered "freeloaders" or not.

Because in Alesina and Stantcheva (2020) people maximise a generalised social welfare function, these effects are the same for all natives, irrespective of their own income. If we assume that people maximise their own economic interests instead, a native's bias against immigrants (i.e. a lower weight for immigrants in the social welfare function) becomes irrelevant, while the perception of immigrants as "freeloading" more often than natives remains relevant. Crucially, the effect of immigration on natives' demand for redistribution will also vary with income.

Thus, we expect that natives with low incomes will demand more redistributive taxation when primed with immigration. The reason is the following: Recent studies suggest that when people think about immigration they tend to think mostly of low-skilled immigration (see e.g. Alesina et al., 2018a). Furthermore, (low-skilled) immigrants are likely to be substitutes to low-skilled/low-income natives in the labour market (see for instance Iturbe-Ormaetxe and Romero, 2016). Thinking about influxes of cheap labour (perceived as) exerting downward pressure on their wages and threatening their jobs is thus going to increase low-income natives' demand for redistribution because they expect to be more dependent on welfare benefits. This effect will be exacerbated by the expectation that immigrants rely on the welfare state disproportionately more than natives: An influx of "freeloaders" would increase the need for welfare state funding via progressive taxation.

On the other hand, middle- and high-income earners do not view immigrants as a threat to their jobs. If at all, (low-skilled) immigrants are complements in the labour market and immigration is expected to increase high-skilled native's wages. High-income natives expect their status as net contributors to the welfare system to be entrenched by immigration (thus benefiting even less from the welfare system on net than in the absence of immigration). Thus, making them think about immigration should lower their demand for progressive taxation. Again, this effect should be stronger if immigrants are additionally expected to be "freeloaders" in need of welfare state benefits.

These considerations lead to the formulation of the following hypothesis concerning the effect of immigration on desired tax progressivity:

---

[26] See Fuster and Zafar (2022) for a discussion.

Hypothesis 1 (*Immigration and Desired Tax Progressivity*)**.** Desired tax progressivity is higher in the Immigration condition compared to the Neutral condition for low-income respondents, and lower for higher-income respondents.

4.1.2. Immigration and desired public spending allocation

Generally, immigrant children need more educational support than natives on average, for instance due to language barriers (OECD, 2018). For the quality of public schooling to remain constant, public education expenditure should thus increase with immigration rates. In countries where public schooling can easily be substituted by a sufficient amount of good private schools, natives who can afford to do so will "flee" to the private sector to protect their children from the decline in the quality of public education. As a result, middle- to high-income individuals for whom the substitution of public with private education is a viable option, should reduce their demand for public spending on education if primed with immigration (Speciale, 2012). Low income individuals will instead demand greater public education spending to contain the drop in the quality of public education on which they still depend (Tanaka et al., 2018). Private schools are however not common in Germany. High-income natives cannot easily "flee" to the private sector, and will most likely react to immigration by demanding higher education spending to ensure the schooling quality remains at least constant. For this reason, we expect an increase in demand for public education spending for both rich and poor natives when primed with immigration.

Overall, immigration into Germany is expected to have a positive effect on the sustainability of the pension system because immigrants are on average younger than natives and thus the number of people in the labour force contributing to the pay-as-you-go pension system increases (Biber and Stegmann, 2019). Thus, less spending on pensions is required due to immigration. A similar argument holds for disability benefits if immigrants are on average "fitter" and more likely to be in the labour force than natives, thus more likely to be net contributors to the social insurance system. These facts should lower desired pension spending and concerns about the sustainability of the social insurance system for all natives (irrespective of income) when thinking about immigration and we thus expect to see a lower preference for spending on social insurance, disability benefits and pensions in the Immigration condition for all tiers of the income distribution.[27]

Conversely, we expect the effect of thinking about immigration on unemployment and welfare benefits and affordable housing to differ depending on natives' incomes. Low-income respondents would want more spending on these items because they expect to be more reliant on them if low-skilled immigrants endanger their jobs and if they expect competition for welfare benefits to increase following their uptake on behalf of immigrants. On the other hand, middle- to high-income earners should want less public resources spent on these items because they expect to be even less likely to be in need of benefits due to immigration. Rather, they expect their status as net contributors to the welfare system to be entrenched due to the gains they reap from low-skilled immigration and the greater reliance on welfare of the low income population.

We do not expect demand for any of the other spending items to be directly affected by (thinking about) immigration, thus we do not formulate any hypotheses pertaining to the effect of our Immigration condition on those items.[28]

These considerations lead to the formulation of the following hypotheses concerning the effect of our Immigration condition on desired public spending.

Hypothesis 2 (*Immigration and Desired Public Spending Allocation*)**.**

  a. Desired education spending is higher in the Immigration condition compared to the Neutral condition for both low-income and higher-income respondents.
  b. Desired spending on social insurance, disability benefits and pensions is lower in the Immigration condition compared to the Neutral condition for both low-income and higher-income respondents.
  c. Desired spending on unemployment and welfare benefits and on affordable housing is higher in the Immigration condition compared to the Neutral condition for low-income respondents and lower for higher-income respondents.

4.2. Poverty condition

4.2.1. Poverty and desired tax progressivity

We expect low income respondents' demand for progressive redistribution to increase in the Poverty condition, as they are made aware (or reminded) of the fact that redistribution from rich to poor is needed to ensure those in need of the welfare state (like

---

[27] However, these arguments necessitate that immigrants are perceived to be *less* likely to be freeloaders and more likely to be net contributors to social insurance (and it is not clear whether the general public perceives them as such).

[28] Demand for public health spending would only be affected if natives perceive immigrants to be disproportionally more likely to be ill than natives and thus disproportionally less likely to be net contributors to the health care system, such that thinking about immigration makes them fear that public healthcare would be underfunded without an increase in public spending. We do not find evidence that immigrants are perceived as overusing the healthcare system compared to natives. In fact, the opposite is the case in most industrialised countries, including Germany: immigrants use public health care services less often than natives (see for instance https://www.welt.de/politik/deutschland/article172556088/Gesetzliche-Krankenkassen-Zuwanderer-fuehren-zu-einem-doppelten-Entlastungseffekt.html). Hence, if at all, the effect should be the opposite: Natives demanding less health care spending because of the "stabilising" effect of immigrants on the health care system due to their lower usage while contributing to social insurance. We however do not think that this effect of immigration on the financial sustainability of public health care is accounted for by natives.

themselves) are adequately supported (Sands and de Kadt, 2020; Iturbe-Ormaetxe and Romero, 2016; Andreoli and Olivera, 2020).[29] Conversely, high income individuals should desire less redistribution if the Poverty condition reminds them of their status as net contributors to the welfare system.[30]

Hypothesis 3 (*Poverty and Desired Tax Progressivity*). Desired tax progressivity is increased by the Poverty condition compared to the Neutral condition among lowest income respondents, and reduced among higher income respondents.

4.2.2. Poverty and desired public spending allocation

Our hypotheses about demand for social insurance, disability benefits and pensions, unemployment and welfare benefits, and affordable housing, follow the same logic as those about desired tax progressivity: High-income earners in the Poverty condition should want less spending on these items due to the reminder about their status as net contributors to the welfare system who benefit less from public spending on those items. Conversely, low income earners should want to increase public spending on those items when reminded of being themselves net receivers and of the demand for those services among the low-income population.

Were the public health care system predominantly used by the poor while the rich fled to private healthcare, the logic should remain the same as above: The poor should want more spending on public health and the rich should want less. However, for many treatments patients cannot simply "flee" to private healthcare in Germany.[31] We therefore do not expect significant effects on desired health care spending in any direction. Similarly, we do not expect an effect of our Poverty condition on desired education spending if everybody just maximises their own material utility.[32]

We thus formulate the following hypotheses concerning the effect of poverty on desired public spending.

Hypothesis 4 (*Poverty and Desired Public Spending Allocation*). Desired spending on social insurance, disability benefits and pensions, on unemployment and welfare benefits and on affordable housing is higher in the Poverty condition compared to the Neutral condition for low-income respondents and lower for higher-income respondents.

4.3. Both and overlap conditions

The effect of the Both and Overlap Conditions, in which we present information on both poverty and immigration, will be a combination of the individual effects, and will depend on which of these dominates whenever they point in opposite directions. Note that in formulating the hypotheses for these conditions we assume that the effects of thinking about immigration and poverty simply add up when presented jointly. We abstract from the possibility that their joint presentation might lead to non-linear effects.

Since we expect the Immigration and the Poverty condition to affect desired tax progressivity in the same way, this also holds for our expectation when they are presented jointly.

Hypothesis 5 (*Effect of Both and Overlap on Desired Tax Progressivity*). Desired tax progressivity is expected to increase for low-income respondents and decrease for higher-income respondents in the Both and Overlap conditions compared to the Neutral condition.

The same argument holds for unemployment and welfare benefits and for affordable housing. We do not expect any effect of the Poverty condition on education spending, hence the effect of the Both and Overlap conditions on this item should simply be identical to the one in the Immigration condition. We hypothesise that both thinking about immigration and thinking about poverty should decrease desired spending on social insurance, disability benefits and pensions among high-income respondents.

The hypothesised effects of the Immigration and the Poverty condition on social insurance spending differ for low-income respondents. The joint effect of the two in the Both and Overlap condition should hence depend on which individual effect dominates.

As we do not expect an effect of priming immigration or poverty on any of the other spending items (infrastructure, defense and public health care), we do not expect those items to be affected by the Both and Overlap conditions either.

We thus expect the following effects of our Both and Overlap conditions on desired public spending.

Hypothesis 6 (*Effect of Both and Overlap on Desired Public Spending Allocation*).

a. Desired spending on unemployment and welfare benefits as well as on affordable housing is expected to increase for low-income respondents and decrease for higher-income respondents in the Both and Overlap conditions compared to the Neutral condition.

---

[29] Note that the Poverty condition does not inform respondents about the extent of inequality. Providing such information would also convey information about how much they could gain from redistribution. Our design can therefore be taken to be capturing a lower bound on the impact of the comprehensive discourse on inequality (subsuming poverty) on demand for redistribution.

[30] Hypothesis 3 presumes that people care mainly about their own income. If, in addition to their own income, people also care about poverty per se, reminding them or making them aware of poverty might make them desire more redistribution to relieve poverty, even if it implies sacrificing their own income in case they are net contributors to the welfare system.

[31] Private healthcare in Germany often means that patients pay higher monthly fees to get preferential treatment in some respects (like shorter waiting times). Generally speaking, underfunding of the public healthcare system means that "private healthcare" suffers too.

[32] However, if people think "something should be done" to combat poverty then this is the least painful option for everybody, as everyone benefits, to some degree, from higher education spending. Education is in fact very often mentioned as the best way to combat poverty in the public discourse (see for instance the discussion in Bussolo et al. (2021)).

   b. Desired education spending is higher in the Both and Overlap conditions compared to the Neutral condition for both low-income and higher-income respondents.

   c. Desired spending on social insurance, disability benefits and pensions is lower in the Both and Overlap conditions compared to the Neutral condition for higher-income respondents. The direction of effect for low-income respondents depends on whether the effect of the immigration prime dominates (thus lowering desired spending on this item), or whether the effect of the poverty prime prevails (thus increasing desired spending on this item for low-income respondents).

Because the Both Condition omits information about the intersection between poverty and immigration, the effects of conditions Both and Overlap are expected to differ if (i) beliefs about the overlap are different in the two treatment groups and (ii) if beliefs about the overlap matter for respondents' demand for redistribution. Following our arguments above, beliefs about the overlap should matter for desired tax progressivity, for unemployment and welfare benefits and for affordable housing: These are the items for which immigrants' reliance on the welfare system matters for natives' redistribution and spending preferences. If natives overestimate the proportion of poor immigrants in need of welfare state support, the effects (increasing demand for redistribution among low-income respondents, decreasing demand for redistribution among high-income respondents) should be less pronounced in the Overlap condition compared to the Immigration and the Both condition (and vice versa if natives underestimate the share of poor immigrants).

5. Experimental evidence on immigration, poverty and demand for redistribution

Before proceeding with our main results, we check whether respondents paid attention to our experimental conditions, a precondition for observing the expected effects. The next section presents evidence that our respondents were attentive by showing that post-experimental subjective beliefs about poverty and immigration in Germany are systematically more concentrated around the correct values among the treated individuals compared to the untreated.

5.1. Manipulation success

Figs. 2(a) and 2(b) compare the distributions of beliefs about immigration and poverty in the Immigration and Poverty conditions (respectively) with those of beliefs elicited in the Neutral condition. Figs. 2(c) and 2(d) apply a Gaussian kernel density smoothing function to the belief data. What stands out immediately is that, as expected, untreated beliefs about immigration and poverty are very dispersed. Most respondents seem to have no clear idea about the number of immigrants and poor in Germany. From Figs. 2(b) and 2(d), untreated individuals (in the Neutral condition) underestimate the number of individuals living below the poverty line. A majority in this group believes that at most five million people live below the poverty line. The distribution of beliefs of those who received the Poverty condition peaks instead at around 14 million, matching the information that 13.7 million people in Germany live below the poverty line.

For the Immigration condition, the difference in the distribution of beliefs between treatment and control group is even starker. From Figs. 2(a) and 2(c) an absolute majority of the respondents in the Immigration condition believes that around 13 million foreign-born individuals are living in Germany. This directly matches the number provided in our experimental condition (13.2 million). Respondents in the control group do not systematically over- or underestimate the number of immigrants: The relative frequency of beliefs in the control group is higher both above and below 13 million than those in the Immigration condition.

It is furthermore remarkable that the Immigration condition seems to have 'better' attracted our respondents' attention than the Poverty condition - posterior beliefs are here much more precisely concentrated around the true value.[33] These observations are aligned with the fact that immigration was a more salient topic than poverty at the time the survey experiment was rolled out, making sure that people would be more impacted when presented with information about immigration than when presented with information about poverty.[34]

We complement the graphical results so far reported with Probit regressions of a binary variable indicating whether a respondent's belief lies within the (plus or minus) one, three or five millions range of the correct number. The results of these analyses, reported in Table B.1 in Appendix B, confirm that respondents indeed state beliefs much closer to the true values if they were exposed to the respective experimental conditions. We can see clearly that beliefs about poverty are significantly more likely to be correct relative to the Neutral condition in all cases except for the Immigration condition (which is the only one not allowing respondents to update these beliefs). Vice versa, beliefs about immigration are close to the true value in all conditions except for the Poverty condition. Together with the graphical results, we thus have strong evidence that our experimental conditions were paid attention to.

Placebo tests. Finally, we can check whether the Immigration condition had an effect on beliefs about poverty and vice versa in placebo tests. As expected, the kernel densities of beliefs that were not targeted by our experimental conditions and the beliefs reported by the control group almost perfectly overlap in Fig. 3.

---

  [33] This observation holds for both high and low income respondents (results available on request).

  [34] An alternative interpretation of these observations is that individuals' beliefs about poverty may be more strongly held than those about immigration, and may hence be harder to manipulate. However, we deem this possibility rather unlikely since, as mentioned in Section 4, we do not think that people have strong and precise numerical beliefs about poverty (or immigration). The uncertainty around these topics is confirmed by the dispersion of the distribution of untreated beliefs. Notice moreover that the scope for updating poverty beliefs is lower than that for immigration beliefs, as the former appear to be more concentrated on low values than the latter (see the discussion in Appendix B.1).
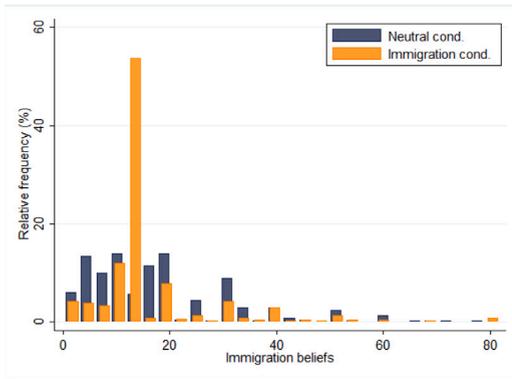
(a) Relative frequency of responses to question about how many people living in Germany were born abroad (in Million)
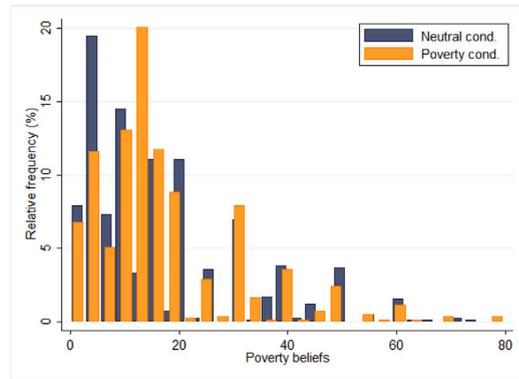
(b) Relative frequency of responses to question about how many people live below the poverty line in Germany (in Million)
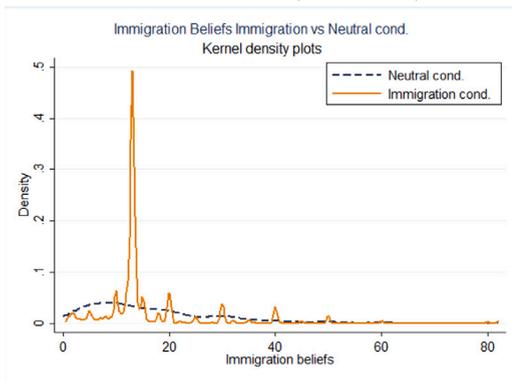
(c) Gaussian kernel density of beliefs about how many people living in Germany were born abroad (in Million)
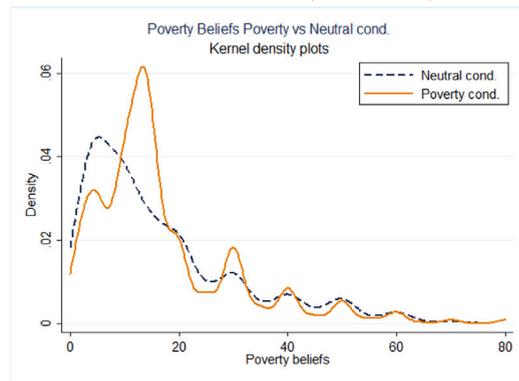
(d) Gaussian kernel density of beliefs about the number of people living below the poverty line in Germany (in Million)

Fig. 2. Beliefs about the number of immigrants and poor in the Immigration and Poverty conditions compared to those in the Neutral condition.
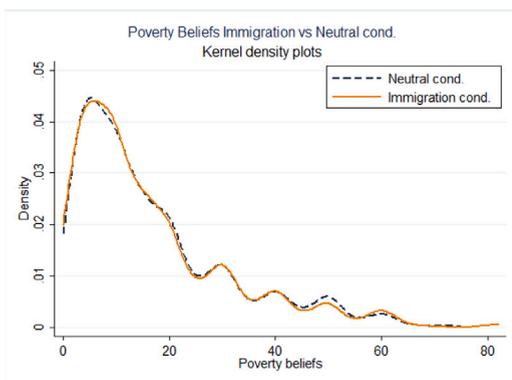
(a) Gaussian kernel density of beliefs about the number of people living below the poverty line (in Million) in the Immigration condition.
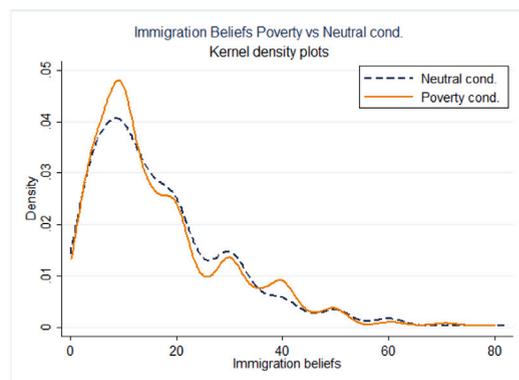
(b) Gaussian kernel density of beliefs about the number of immigrant people living in Germany (in Million) in the Poverty condition.

Fig. 3. Placebo comparison of untargeted beliefs in the Immigration and Poverty conditions with the untreated beliefs in the Neutral condition.

5.2. Treatment effects on demand for redistribution

We will now present results from simple OLS regressions of our measures of demand for redistributive taxation and redistributive public spending on the experimental conditions and a vector of individual and regional covariates. The tables report the point estimates of the effects of our experimental conditions and, for the sake of clarity, omit any other coefficients.

We estimate the following OLS regression model:

$$y_i = \alpha_i + \beta c_i + \gamma'_1 X_i + \gamma'_2 W_i + \gamma_3 Land_i + \varepsilon_i \quad , \tag{1}$$

where y_i denotes the (standardised) outcome variable of interest, c a categorical variable uniquely identifying each of our conditions with the Neutral condition as a baseline, X_i and W_i are vectors respectively containing respondent i's sociodemographic and the background characteristics of i's region ("Landkreis", corresponding to NUTS level 3 geographic areas) of residence. $Land$ denotes the respondent German state of residence (corresponding to NUTS level 1). ε_i denotes a random error component which we cluster at Landkreis level in our analyses.[35] The individual socio-demographic background variables, not displayed in the output but always controlled for, are: the respondent's household income, whether the respondent holds a university degree, the respondent's (self-) placement in the political spectrum with 1 indicating extreme left and 10 indicating extreme right, an indicator taking value 1 if the respondent works or has ever worked in a sector employing a high number of unskilled immigrant labour.[36] Further, the respondent's age (6 classes: 18–25, 26–35, 36–45, 46–55, 56–65, 66+), gender, marital status (single/in couple), voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany. Regional background variables, again not displayed in the output, are region GDP and the share of resident immigrants. Our coefficient of interest is β, capturing the fixed effects of having received each of our experimental conditions. Wherever relevant, the output from OLS regressions will report p-values (instead of standard errors) corrected for multiplicity of tested hypotheses using the method described in List et al. (2016) and implemented by Barsbai et al. (2020).[37]

5.2.1. Aggregate analyses

Table 1 presents the effects of our experimental conditions on aggregate level desired tax progressivity, which, though all our hypotheses are specific to income groups, we present for completeness. As a reminder, we elicit desired average tax rates on key quantiles of the income distribution to obtain a measure of respondents' desired tax progressivity (see Section 3.2). We construct our index τ as the (unweighted) sum of the desired tax rates on the top 1% (denoted $t_{(100)}$) and next 9% ($t_{(91-99)}$), minus the desired tax rates on the next 40% ($t_{(51-90)}$): $\tau = t_{(100)} + t_{(91-99)} - t_{(51-90)}$.[38] As mentioned in Section 3.2, we leave out the tax rate applied to the lowest 50% of the income distribution in this analysis because this group, for the largest part, does not pay much in taxes in Germany. For a given size of the public budget, the taxes on the middle 40% must be lowered in order to increase taxes on the rich (i.e. the top 10%).[39][40]

None of our experimental conditions appear to have any detectable effect at the aggregate level. All coefficients throughout columns 1 to 4 are small in magnitude and not significant at conventional levels (regardless of multiplicity correction) over a variety of specifications.

Observation 1. *Our conditions do not affect respondents' demand for progressive taxation at the aggregate level.*

Next, we analyse how our conditions affect the respondents' desired allocation of a fixed public budget over defence, infrastructure, education, social insurance and pensions, unemployment and welfare benefits, health care and housing support.

Table 2 reveals a positive impact of the Poverty condition on desired public education expenditure. This increase amounts to roughly 12% of the baseline sample's standard deviation.

Observation 2. *The Poverty condition increases respondents' average desired public expenditure on education.*

Our analyses uncover a significant effect on desired expenditure for public education, though no systematic effect emerges on the other desired public spending items (see Tables C.22 to C.27 in Appendix C). These findings suggest that the Poverty condition's

---

[35] We cluster standard errors to account for potential correlation in the responses due to the geographic clustering of poverty and immigration phenomena. Our results hold for different clustering levels or in the absence of clustering.

[36] Elicited in the survey by asking for sectors in which respondents have worked, and then in the regressions classifying them as "low-skilled high immigration sectors" (1) or not (0).

[37] See Appendix C.7 for details.

[38] Summary statistics are reported in A.1 in Appendix A.

[39] Recall that we asked the respondents to shift tax rates on our quantiles in order to collect (roughly) the amount of income tax revenue Germany raised in 2017.

[40] We will avoid overburdening the reader with notation and refer to the elicited tax rates and constructed indices in words, confining the use of mathematical notation to tables and results.

Table 1

OLS regression of desired tax progressivity. The square brackets contain p-values corrected for multiplicity of tested hypotheses (Barsbai et al., 2020). Omitted individual controls include: age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany.

Variables	Desired tax progressivity			
Neutral condition: baseline				
Immigration condition	−0.068	−0.067	−0.068	−0.073
	[0.345]	[0.372]	[0.347]	[0.284]
Poverty condition	−0.035	−0.027	−0.026	−0.030
	[0.482]	[0.587]	[0.594]	[0.551]
Both condition	−0.034	−0.029	−0.028	−0.033
	[0.644]	[0.728]	[0.727]	[0.676]
Overlap condition	−0.095	−0.087	−0.088	−0.090
	[0.163]	[0.195]	[0.204]	[0.178]
Constant	✓	✓	✓	✓
Individual controls 1	✓	✓	✓	✓
Individual controls 2		✓	✓	✓
Regional controls			✓	✓
Land fixed eff.				✓
Observations	3952	3952	3952	3952
R-squared	0.011	0.030	0.031	0.035

Robust p-values corrected for multiple hypotheses in square brackets.
Corrected p-values significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2

OLS regression of standardised preferred share of public budget allocated to education. The square brackets contain p-values corrected for multiplicity of tested hypotheses (Barsbai et al., 2020). Omitted individual controls include: age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany.

Variables	Preferred public education expenditure			
Neutral condition: baseline				
Immigration condition	−0.004	−0.001	−0.002	0.003
	[0.870]	[0.906]	[0.881]	[0.967]
Poverty condition	0.115*	0.112*	0.113*	0.120**
	[0.070]	[0.060]	[0.065]	[0.042]
Both condition	0.012	0.015	0.016	0.017
	[0.976]	[0.963]	[0.949]	[0.941]
Overlap condition	0.037	0.035	0.033	0.035
	[0.827]	[0.849]	[0.855]	[0.846]
Constant	✓	✓	✓	✓
Individual controls 1	✓	✓	✓	✓
Individual controls 2		✓	✓	✓
Regional controls			✓	✓
Land fixed eff.				✓
Observations	3955	3955	3955	3955
R-squared	0.021	0.038	0.040	0.046

Robust p-values corrected for multiple hypotheses in square brackets.
Corrected p-values significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

effect on desired education expenditure is, at the aggregate level, uniformly compensated for across the other public expenditure items.[41][42]

Note however that due to the expected stark heterogeneity of treatment effects across income groups as per Hypotheses 1 to 6, these results can only be of limited use for gaining insight into how immigration and poverty affect society's demand for redistribution on aggregate. As we expect our treatments to work in opposite directions for different income groups, it is difficult to generalise to how their effects will balance each other out in the German population as a whole, for instance before an election.

---

[41] Summary statistics are reported in A.1 in Appendix A.

[42] Notice moreover that we find no correlation between demand for tax progressivity and the size of the budget allocated to the public expenditure items save for a positive correlation with social insurance ($\beta = 0.05$, $p = 0.021$ in a regression analysis). Moreover, we elicited whether the respondents believe personal success should be attributed prevalently to individual effort or to luck. This variable is however potentially endogenous, because it was elicited after the experimental conditions were delivered. When investigating *within treatment* correlations with desired tax progressivity we uncover a negative and significant correlation among the participants in the Overlap condition ($\beta = -0.33$, $p = 0.01$). We find moreover weighing luck more than effort is correlated with a lower budget being assigned to public education expenditure in the Poverty ($\beta = -0.25$, $p = 0.059$) and Both ($\beta = -0.41$, $p = 0.008$) conditions.

Table 3

OLS regression of desired tax progressivity. The square brackets contain p-values corrected for multiplicity of tested hypotheses (Barsbai et al., 2020). Individual controls include: university education, political placement, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany. Regional controls include GDP and the share of immigrant population.

Variables	Desired tax progressivity	
	Low income	High income
Neutral condition: baseline		
Immigration condition	0.273*	−0.160***
	[0.089]	[0.008]
Poverty condition	0.113	−0.068
	[0.649]	[0.583]
Both condition	0.063	−0.061
	[0.820]	[0.607]
Overlap condition	−0.041	−0.104
	[0.743]	[0.295]
Constant	✓	✓
Individual controls	✓	✓
Regional controls	✓	✓
Land fixed eff.	✓	✓
Observations	787	3165
R-squared	0.065	0.035

Robust p-values corrected for multiple hypotheses in square brackets.
Corrected p-value significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

We cannot in fact claim our sample to be a true accurate image of the German population with all its characteristics. Though we have tried (successfully) to achieve approximate representativeness along four relevant dimensions, other factors which we cannot account for and which could be unequally (or unrepresentatively) distributed across income groups (e.g. turnout) will affect voting behaviours.

For this reason, we now focus our investigation on analyses illuminating on how different income classes react, perhaps in opposite ways, to the same stimuli. We believe this approach to yield more instructive insights than the aggregate results by highlighting each income group's specific reaction to our conditions. In order to ease the interpretation and elaboration of our estimates, we resort to split-sample analyses rather than interactions, as we are not focused on the relative effects across groups but rather on the direction and size of effects for each group separately.[43]

5.2.2. Split sample analyses

We split our sample according to the respondents' placement with respect to the poverty line. As argued in Section 4, we expect low-income respondents' reaction to immigration and poverty to differ compared to middle- and high-income respondents because they feel at risk of competition with low- or unskilled immigrant workers both in the labour market and in the welfare system. Relevant from a respondent's perspective is the likelihood with which they will be competing with low- or unskilled immigrant labour force, and the extent to which they perceive they belong to a low income group. It is a priori unclear whether individuals have a correct picture of where they stand in terms of being net receivers or contributors to the welfare state, in particular with earnings around the median income (for instance: Cruces et al., 2013; Karadja et al., 2017; Windsteiger, 2022). Because of these considerations, we feel the income split chosen is the best suited to generate and capture these effects.[44] Moreover, this split matches the definition of "Poverty" mentioned in our experimental conditions.

Desired tax progressivity. Table 3 displays the result of OLS regressions of desired tax progressivity on condition indicators (the Neutral condition serving as a baseline) and on individual and regional controls by income-splits as discussed above.[45] Because the dependent variable has been standardised into z-scores, the results can be interpreted in terms of the unit standard deviation of the control group.

We observe strong effects of our Immigration condition on desired tax progressivity. High income earners demand a less progressive tax schedule upon receiving information about immigration compared to the baseline group: The coefficient is large (roughly 16% of the baseline group's standard deviation) and significant at the 1% level. Conversely, low income individuals increase the progressivity of their desired tax schedule. The coefficient is large (roughly 27% of the baseline group's standard deviation) and significant at the 10% level. This analysis reveals that low income individuals and the rest of the income distribution are affected in opposite directions, thus uncovering evidence in support for Hypothesis 1.

---

[43] Interaction analyses confirm the findings reported here (see Table C.1 in Appendix C).

[44] We explore different income splits and the inclusion of a third, "middle income" group in Appendix C.3, confirming the findings here reported.

[45] Table C.1 in Appendix C.1 confirms all these findings in an interacted regression.

Result 1. *Low income (Higher income) respondents increase (decrease) their demand for progressive taxation in the Immigration condition.*

On the other hand, the Poverty condition does not have any detectable effect on either of our income splits: The estimated coefficients are small and not significant.

Result 2. *The Poverty condition has no significant impact on desired tax progressivity across the entire income distribution.*

Looking at income-specific effects in the Both and Overlap conditions, we observe no significant effect on desired tax progressivity for either low- or higher-income respondents.

Result 3. *The Both and Overlap conditions have no significant impact on desired tax progressivity across the entire income distribution.*

Result 2 is at odds with Hypothesis 3, which predicts that high-income earners should demand lower tax progressivity and low income earners should want a more progressive tax schedule in response to information about poverty.

The reason why we do not find an effect of the Poverty condition on middle- and high income respondents could be because individuals are not purely self-interested (as we assume in the formulation of our hypotheses) but also care about minimising poverty (or inequality) per se. Then the Poverty condition would make people in the upper quantiles of the income distribution not only aware that they are net contributors to the welfare system (which would lower their demand for redistribution) but also of poverty per se (which could lead to an increase in their support for redistribution). These opposite effects might cancel out on average.

Another explanation for the insignificant effect of the Poverty condition on desired tax progressivity could be that 'poverty' as an issue is simply less salient than immigration and thus our brief experimental condition does not suffice to shift people's tax preferences. Support for this conjecture comes from looking at posterior beliefs about poverty, less concentrated around the correct number than those concerning immigration and suggesting that people might have paid less attention to our experimental condition focusing on poverty. It is further supported by the fact that desired tax progressivity is also not affected for low income respondents, who should react by demanding higher tax progressivity in response to poverty irrespective of whether they are purely self-interested or care also about poverty per se.

Result 3 does not offer support for Hypothesis 5 on the impact of the Both and Overlap conditions on the preferred tax schedule of the two income groups. Moreover, even taking into account that our Poverty condition did not succeed in shifting respondents' desired tax progressivity, Result 3 is at odds with the strong effects observed for the Immigration condition and supports our arguments in Appendix B about the potential interaction of the two pieces of information: Immigration might become less salient due to the simultaneous presentation of information about poverty. Thus, while poverty in itself does not significantly affect respondents' demand for redistribution, it might lower the effects of immigration when presented jointly. This finding suggests the possibility of non-linear interactions between the two treatments. While a detailed investigation of these interactions and the underlying mechanisms is beyond the scope of this paper, it presents interesting material for future research.

Allocation of the public budget. Table 4 reports the results from two OLS regressions of the respondents' demanded budget for public education on income splits. The table reports p-values corrected for multiplicity of income groups and treatment conditions.[46][47]

We again observe opposite responses among respectively low and higher income respondents in our experimental conditions. Middle- and high income earners would like to increase spending on education in the Poverty condition, while the low income group exhibit a strong negative response in the Immigration condition.

Result 4. *The Immigration condition decreases demand for public education expenditure among low-income respondents.*

Result 5. *The Poverty condition increases demand for public education expenditure among middle- and high income respondents.*

The evidence reported by Result 4 is contrary to Hypothesis 2, where desired education spending is expected to increase among both low and higher income respondents in response to the Immigration condition. Moreover, Result 5 displays evidence of a significant increase in desired public education spending on behalf of higher income groups which is not expected as of our hypotheses in Section 4.2. Notice however that these findings are consistent with the heuristics described in Section 4.2 about the common perception of education being a viable poverty reduction strategy. We expand on this argument below.

Point estimates for the Both and the Overlap conditions are aligned in sign and magnitude with the estimated impact of information about immigration and poverty so far discussed for low and high income respondents, though only the negative impact of the Both condition on low income respondents survives multiple hypothesis correction. These findings consequently also are at odds with Hypothesis 6, predicting an increase in desired education spending across the income gradient.

Result 6. *The Both condition reduces desired public education expenditure among low income respondents. No statistically significant effect is uncovered among higher income respondents.*

---

[46] The estimates in Table 4 are robust to correction for multiple outcomes, the results of which are available upon request.

[47] Table C.1 in Appendix C.1 again confirms all of these findings in an interacted regression.

Table 4

OLS regression of standardised preferred share of public budget allocated to education. The square brackets contain p-values corrected for multiplicity of tested hypotheses (Barsbai et al., 2020). Individual controls include: university education, political placement, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany.

Variables	Desired education spending	
	Low income	High income
Neutral condition: baseline		
Immigration condition	−0.315**	0.089
	[0.019]	[0.277]
Poverty condition	−0.150	0.191***
	[0.192]	[<0.001]
Both condition	−0.393***	0.105
	[<0.001]	[0.257]
Overlap condition	−0.208	0.108
	[0.227]	[0.155]
Constant	✓	✓
Individual controls	✓	✓
Regional controls	✓	✓
Land fixed eff.	✓	✓
Observations	787	3165
R-squared	0.120	0.042

Robust p-values corrected for multiple hypotheses in square brackets.
Corrected p-value significance: *** p < 0.01, ** p < 0.05, * p < 0.1.

Testing for the equality of the coefficients on Immigration and Both conditions on the low income subsample fails to reject the null hypothesis of equality (t-test p-value = 0.478). This observation together with the absence of a statistically significant effect of the Poverty Condition suggest the effect of the Both Condition might be driven solely and entirely by the information about immigration. This conjecture is also supported by the fact that results uncorrected for multiplicity (presented in Table C.1) suggest low income respondents may react negatively whenever they are presented with information about immigration. Worth of mention is that robustness results obtained using different outcomes measuring the perceived severity of poverty and inequality as a social problem, and the support for public intervention for poverty reduction support most of these findings (see Appendix C.6).

The effects of our experimental conditions on desired education spending are at odds with our hypotheses in Section 4. We feel however that a note of caution is due because of the specific nature of the public expenditure item, education, for which effects are uncovered. First, as mentioned in footnote 30, if higher income respondents feel the need to react to poverty in some way (even if they mainly care about their own material utility), increasing spending on education might seem like the "least painful option" for them, since they or their children will likely also benefit from this increase. In fact, it is not even entirely clear whether public education is a strongly redistributive public intervention in Germany. While education can help increase social mobility, previous empirical analyses have found that the German public education system (with its high degree of selection starting at a very young age) is currently set up to benefit children from higher-income (and higher-education) backgrounds *more* than children with low income, low education backgrounds (see e.g. Krüger et al. (2010) and Geißler and Weber-Menges (2010)). Moreover, education spending is often mentioned as an effective way to reduce poverty (see e.g. Bussolo et al. (2021)). Both of these aspects might play a role in the observed increase of desired education spending in response to poverty on behalf of higher-income individuals. Even more crucially, what matters here is what people perceive the effect of education to be irrespective of what it actually is. If, coherently with the dominant cultural discourse about the socially and individually beneficial effects of increased education, rich individuals think the burden placed on their wallets will be reduced by more educated low income and immigrant groups, they will favour its expansion. Similarly, if low income individuals believe the benefits of an expanded public education system will accrue primarily to immigrant children at the expense of theirs, they will retrench their support.[48] These arguments are consistent with the findings reported in both Results 4 and 5.

We further investigate our findings on demand for public education expenditure, with a particular eye on the negative effect of information about immigration on the poor's desired education spending, by exploring any heterogeneity with respect to the respondent's education level (measured as having at least a university level degree or not) and of the presence of at least one child in the respondent's household (see Appendix C.2).[49] We observe no robust evidence for heterogeneities with respect to the respondent's education level, nor with respect to the presence of children in the household.

Analogous to the aggregate analysis, we do not find significant effects on any other public spending item in our split sample analyses (see Appendix C.5). This suggests that the increased demand for public spending is compensated for uniformly across the other spending items not only across the whole income distribution but also among low income and middle- to high income respondents separately.

---

[48] See, e.g., the analyses on immigrant children's education outcomes in Dustmann et al. (2012).

[49] Appendix C.2 also investigates, and finds no evidence for, heterogeneity with respect to political preferences.

Note that in Section 4 we hypothesise about the effect of our conditions on all spending items separately, as if respondents could decide to increase or decrease spending on each of them, irrespective of the effect this has on aggregate spending. However, our question was in fact asked in such a way that the total amount of spending had to remain constant, i.e. we asked respondents to assign percentages to each spending item, the total of which was constrained to sum up to 100%. This means that, in addition to calculating the individual effect of our conditions on their spending preferences, respondents would have to also adjust spending on the remaining items accordingly and potentially prioritise preferred spending increases according to which items they found most important (in case they would have preferred to increase spending on many or all items).

We thus acknowledge that we presented our respondents in this question with a potentially very complex task. While our research design does not allow us to say how exactly respondents made their choices, our treatment effect patterns are consistent with them having resorted to the heuristic of simply demanding the highest (or lowest) percentage of spending to the item deserving the highest attention in their opinion (which, for a majority of respondents, was public education spending), and then adjusting accordingly across all other items (or randomly across a subset of those). We thus caution against interpreting the lack of significant treatment effects for all the other spending items as immigration and/or poverty not mattering for desired spending on these items. It might simply be that they matter *most* for desired education spending compared to all the other items.

6. Conclusion

Our paper investigates information about immigration and poverty as drivers of demand for both the financing and supply sides of the welfare state (Cavaillé and Trump, 2015) in a unified framework. We achieve this goal by exploiting the near equality in the size of the immigrant (defined as being born abroad) and of the poor populations (defined as living on an income lower than the poverty line) in Germany (respectively 13.2 and 13.7 million individuals) to design experimental conditions delivered to the respondents in a survey experiment.

Our findings reveal that information about immigration and poverty impact demand for redistribution in different ways across income groups. We observe strong and opposite reactions on behalf of poor and high income individuals to information about immigration in terms of the progressivity of their preferred income tax schedule, respectively higher and lower for poorer and richer respondents . This evidence points towards a protective approach to desired taxation on behalf of natives facing migration waves (the so called *protection theory*) when investigating the financing side of redistribution.

Conversely, delivering information about poverty has no detectable impact (within our framework) on our respondents' demand for redistributive taxation. This finding contrasts with our expectation that making individuals focus on the poverty problem would increase the awareness of their reliance on social welfare systems among the poorest and the awareness of their status of net contributors to the welfare state among richer respondents, respectively increasing and decreasing demand for redistribution.

When turning to redistributive public expenditures, we observe strong responses to both information about immigration and about poverty in terms of their demand for publicly financed education, but no systematic effects on demand for the other redistributive public expenditure items. Reactions are again of opposite sign for low and high income individuals.

In the Immigration condition, poor respondents *decrease* their demanded expenditure on public education, while high income respondents seem to exhibit no systematic reaction. Conversely, low income respondents do not appear to react to information about poverty, while high income respondents strongly increase their demand for expenditure on public education . These findings are at odds with our predictions of a positive and null effect of respectively information about immigration and poverty on public education expenditure among both income groups. However, they are consistent with a perception of public education being a viable way to reduce poverty and to counteract the supposedly negative impact of immigration flows.

Summing up, the lowest and higher income earners seem to be on fundamental disagreement in terms of their reaction to information about the size of the immigrant and, to a lesser extent, of the poor population. At the same time, they appear to be largely in agreement about the underlying motivations for their opposite reactions: that of protecting themselves from what they perceive as threats to their livelihoods. These findings expand upon recent investigations by delivering a more fine-grained picture on the channels along which the protectionist attitude manifests itself.

The answer to the questions posed in the opening of this article is therefore not a straightforward one. Does immigration reduce or increase demand for redistribution, and why? Does immigration induce protectionist attitudes to redistribution, and does this attitude fully explain the observed reactions? How does the impact of immigration compare with the impact of poverty, a very related social factor which redistribution is directly intended to address? Our study highlights how the link between information about immigration, about poverty, and welfare state support is a complex phenomenon deserving further scholarly attention. When striving to understand how current phenomena impact the social and political spheres, it is vital to understand to which degree and in which direction each relevant segment of the electorate is impacted and with which consequences in order to increase our understanding of the social and political consequences of the phenomenon.

The investigation of demand for the provision side of redistribution, i.e. public spending in its different components (public education being only one of them) is itself a promising avenue for future research Redistribution is a manifold system of policies which do not necessarily form a cohesive body of public interventions in the electorate's mind. Support for one class of interventions therefore does not necessarily translate into support for another one, such that demand for redistribution might be eroded on one dimension (e.g. fiscal instruments) and not on others (e.g. education). Crucial for our future understanding of how demand for redistribution is determined is hence to clearly grasp which classes of public interventions are affected by current socio-cultural, political, demographic and natural events and their impact on current socio-political debates.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Data will be made available on request.

Appendix A. Summary statistics and sample balance

See Tables A.1–A.4.

Table A.1

Summary statistics of tax progressivity index and desired public education expenditure by condition and sample income split.

	Full sample		Low income		High income	
	Mean	sd	Mean	sd	Mean	sd
Condition neutral						
Tax progressivity τ	45.01	22.29	42.32	23.90	45.76	21.78
Desired public educ exp	17.42	7.83	18.36	9.19	17.15	7.39
Condition poverty						
Tax progressivity τ	43.31	23.60	46.92	24.70	42.24	23.18
Desired public educ exp	17.37	8.22	15.95	8.06	17.79	8.22
Condition immigration						
Tax progressivity τ	44.08	23.26	44.42	25.57	44.00	22.73
Desired public educ exp	18.30	8.06	17.03	8.31	18.58	7.99
Condition both						
Tax progressivity τ	44.16	21.73	43.19	22.82	44.37	21.50
Desired public educ exp	17.58	7.96	15.78	7.64	17.96	7.97
Condition overlap						
Tax progressivity τ	43.03	23.70	40.78	27.11	43.58	22.78
Desired public educ exp	17.65	7.99	16.38	8.48	17.96	7.84

Table A.2

Balance table. Individual and geographical average characteristics in each of the conditions are compared pairwise with the average in the Neutral condition. "Delta" is, for each variable, the difference between a condition average and the average in the Neutral condition. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

	Neutral	Poor	Delta	Immigrant	Delta	Both	Delta	Overlap	Delta
Household income	1914.786	1942.515	27.729	1891.536	−23.249	2007.349	92.564*	1973.337	58.551
	(989.198)	(977.425)	(0.574)	(957.903)	(0.632)	(969.141)	(0.058)	(1052.283)	(0.251)
Job security	0.593	0.598	0.005	0.556	−0.037	0.586	−0.007	0.591	−0.002
	(0.492)	(0.491)	(0.850)	(0.497)	(0.136)	(0.493)	(0.764)	(0.492)	(0.941)
Age	44.319	43.865	−0.454	44.776	0.457	44.238	−0.081	43.732	−0.586
	(14.024)	(14.295)	(0.523)	(14.041)	(0.514)	(14.070)	(0.908)	(14.005)	(0.402)
Female	0.495	0.490	−0.005	0.475	−0.020	0.518	0.023	0.471	−0.024
	(0.500)	(0.500)	(0.854)	(0.500)	(0.422)	(0.500)	(0.359)	(0.499)	(0.340)
Single	0.387	0.381	−0.007	0.344	−0.043*	0.360	−0.027	0.380	−0.007
	(0.487)	(0.486)	(0.790)	(0.475)	(0.071)	(0.480)	(0.261)	(0.486)	(0.770)
Rightwing	5.060	5.229	0.170**	5.006	−0.053	5.046	−0.014	5.128	0.068
	(1.742)	(1.695)	(0.050)	(1.709)	(0.536)	(1.762)	(0.874)	(1.784)	(0.438)
Voted	1.104	0.983	−0.121	0.969	−0.135	0.842	−0.262	0.983	−0.122
	(0.306)	(3.591)	(0.341)	(3.551)	(0.281)	(4.975)	(0.136)	(3.553)	(0.333)
Social Media	0.450	0.464	0.014	0.459	0.009	0.451	0.002	0.451	0.002
	(0.498)	(0.499)	(0.578)	(0.499)	(0.716)	(0.498)	(0.948)	(0.498)	(0.950)
Born in Germany	0.974	0.955	−0.019**	0.965	−0.009	0.961	−0.013	0.964	−0.010
	(0.159)	(0.207)	(0.044)	(0.184)	(0.298)	(0.195)	(0.130)	(0.187)	(0.240)
High immigr. sector	0.660	0.683	0.023	0.691	0.031	0.652	−0.008	0.649	−0.011
	(0.474)	(0.465)	(0.325)	(0.462)	(0.182)	(0.477)	(0.742)	(0.478)	(0.634)
GDP at kreis level	39,721.621	40,297.973	576.349	39,227.848	−493.776	40,400.563	678.941	39,409.145	−312.476
	(16,750.947)	(17,562.801)	(0.505)	(17,574.648)	(0.567)	(17,622.289)	(0.430)	(16,585.020)	(0.709)
German State	197.172	195.761	−1.411	192.375	−4.796	188.436	−8.736	193.130	−4.042
	(119.256)	(115.631)	(0.811)	(115.959)	(0.414)	(115.286)	(0.135)	(117.792)	(0.495)
Has a university degree	0.285	0.307	0.021	0.306	0.021	0.317	0.032	0.299	0.013
	(0.452)	(0.461)	(0.356)	(0.461)	(0.359)	(0.466)	(0.167)	(0.458)	(0.555)
Observations	806	783	1589	800	1606	811	1617	800	1606

Table A.3
Balance table, above the poverty line. Individual and geographical average characteristics in each of the conditions are compared pairwise with the average in the Neutral condition. "Delta" is, for each variable, the difference between a condition average and the average in the Neutral condition. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

	Neutral	Poverty	Delta	Immigration	Delta	Both	Delta	Overlap	Delta
Household income	2238.207	2222.605	−15.602	2205.72	−32.492	2266.03	27.82	2264.016	25.810
	(875.894)	(833.496)	(0.747)	−886.444	−0.512	−871.04	−0.567	(965.953)	(0.618)
Job security	0.684	0.657	−0.027	0.662	−0.022	0.657	−0.027	0.674	−0.010
	(0.465)	(0.475)	(0.317)	−0.473	−0.412	−0.475	−0.303	(0.469)	(0.711)
Age	44.688	45.641	0.952	44.147	−0.541	44.733	0.045	43.986	−0.702
	(13.700)	(13.532)	(0.217)	−14.122	−0.489	−13.901	−0.954	(13.652)	(0.360)
Female	0.498	0.481	−0.017	0.477	−0.02	0.51	0.012	0.461	−0.036
	(0.500)	(0.500)	(0.548)	−0.5	−0.47	−0.5	−0.663	(0.499)	(0.194)
Single	0.326	0.269	−0.057**	0.33	0.004	0.322	−0.004	0.328	0.002
	(0.469)	(0.444)	(0.027)	−0.471	−0.871	−0.468	−0.891	(0.470)	(0.948)
Rightwing	5.084	5.049	−0.036	5.229	0.145	5.135	0.051	5.109	0.025
	(1.692)	(1.667)	(0.707)	−1.732	−0.133	−1.735	−0.595	(1.768)	(0.800)
Voted	1.083	0.903	−0.180	0.931	−0.152	0.783	−0.3	0.924	−0.159
	(0.276)	(4.033)	(0.265)	−3.97	−0.34	−5.483	−0.171	(3.953)	(0.315)
Social Media	0.444	0.437	−0.007	0.455	0.011	0.439	−0.005	0.429	−0.016
	(0.497)	(0.496)	(0.793)	−0.498	−0.691	−0.497	−0.857	(0.495)	(0.573)
Born in Germany	0.973	0.960	−0.013	0.95	−0.023**	0.96	−0.013	0.963	−0.010
	(0.162)	(0.197)	(0.189)	−0.218	−0.033	−0.197	−0.182	(0.190)	(0.301)
High immigr. sector	0.615	0.652	0.037	0.659	0.044	0.625	0.01	0.610	−0.005
	(0.487)	(0.477)	(0.177)	−0.474	−0.107	−0.484	−0.713	(0.488)	(0.854)
GDP at kreis level	40,786.617	39,737.773	−1,048.843	40904.47	117.85	41,133.25	346.63	39,844.703	−941.914
	(17,222.656)	(18,328.346)	(0.300)	−18,024.91	−0.906	−18,335.51	−0.727	(16,770.205)	(0.325)
German State	190.436	186.900	−3.537	193.763	3.327	187.938	−2.498	189.356	−1.081
	(118.622)	(114.286)	(0.592)	−116.13	−0.614	−116.049	−0.702	(118.448)	(0.871)
Has a university degree	0.326	0.358	0.032	0.338	0.012	0.337	0.011	0.342	0.016
	(0.469)	(0.480)	(0.238)	−0.473	−0.647	−0.473	−0.663	(0.475)	(0.553)
Observations	629	618	1247	639	1268	667	1296	644	1273

Table A.4
Balance table, below the poverty line. Individual and geographical average characteristics in each of the conditions are compared pairwise with the average in the Neutral condition. "Delta" is, for each variable, the difference between a condition average and the average in the Neutral condition. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

	Neutral	Poverty	Delta	Immigration	Delta	Both	Delta	Overlap	Delta
Household income	765.454	767.359	1.905	774.564	9.111	812.764	47.310**	773.353	7.899
	(189.221)	(193.281)	(0.925)	(188.194)	(0.667)	(189.353)	(0.027)	(192.137)	(0.706)
Job security	0.271	0.214	−0.057	0.313	0.041	0.257	−0.014	0.250	−0.021
	(0.446)	(0.411)	(0.210)	(0.465)	(0.419)	(0.438)	(0.774)	(0.434)	(0.662)
Age	43.006	41.841	−1.165	42.611	−0.395	41.944	−1.061	42.686	−0.320
	(15.089)	(15.324)	(0.469)	(15.025)	(0.816)	(14.658)	(0.526)	(15.377)	(0.848)
Female	0.486	0.456	−0.030	0.549	0.063	0.556	0.070	0.513	0.027
	(0.501)	(0.499)	(0.573)	(0.499)	(0.265)	(0.499)	(0.215)	(0.501)	(0.625)
Single	0.605	0.599	−0.006	0.604	−0.000	0.535	−0.070	0.596	−0.008
	(0.490)	(0.491)	(0.914)	(0.491)	(0.995)	(0.501)	(0.210)	(0.492)	(0.877)
Rightwing	4.972	4.863	−0.109	5.231	0.259	4.629	−0.342	5.206	0.235
	(1.911)	(1.841)	(0.582)	(1.523)	(0.189)	(1.830)	(0.105)	(1.854)	(0.258)
Voted	1.181	1.192	0.012	1.215	0.034	1.118	−0.063	1.224	0.044
	(0.386)	(0.395)	(0.780)	(0.412)	(0.441)	(0.324)	(0.121)	(0.419)	(0.324)
Social Media	0.469	0.533	0.064	0.500	0.031	0.507	0.038	0.545	0.076
	(0.500)	(0.500)	(0.226)	(0.502)	(0.581)	(0.502)	(0.499)	(0.500)	(0.168)
Born in Germany	0.977	0.984	0.006	0.979	0.002	0.965	−0.012	0.968	−0.009
	(0.149)	(0.128)	(0.676)	(0.143)	(0.915)	(0.184)	(0.514)	(0.177)	(0.597)
High immigr. sector	0.819	0.824	0.005	0.792	−0.028	0.778	−0.041	0.808	−0.012
	(0.386)	(0.382)	(0.903)	(0.408)	(0.536)	(0.417)	(0.357)	(0.395)	(0.788)
GDP at kreis level	35,880.254	37,468.883	1588.628	37,609.035	1728.781	36,995.133	1114.880	37,599.039	1718.785
	(14,325.594)	(14,590.530)	(0.304)	(15,112.606)	(0.298)	(13,367.616)	(0.479)	(15,715.324)	(0.302)
German State	221.205	210.940	−10.265	204.611	−16.593	190.736	−30.468**	208.712	−12.493
	(118.740)	(119.929)	(0.416)	(113.367)	(0.205)	(112.055)	(0.020)	(114.099)	(0.331)
Has a university degree	0.141	0.132	−0.009	0.167	0.025	0.222	0.081*	0.122	−0.019
	(0.349)	(0.339)	(0.797)	(0.374)	(0.530)	(0.417)	(0.059)	(0.328)	(0.602)
Observations	177	182	359	144	321	144	321	156	333

Table A.1 reports the mean and standard deviation of the tax progressivity index and desired level of public expenditure discussed in Section 5 for each experimental condition and income split.

Table A.2 reports the comparison of the subsamples which received each of our experimental conditions with that which received the Neutral condition (our baseline) along all the observables. "Delta" refers, for each observable, to the difference between its average in a given condition and the average in the Neutral condition. Tables A.3 and A.4 repeat this exercise, respectively, for the high and low income groups.

Appendix B. Further manipulation checks

Beliefs in the Both and in the Overlap conditions depict similar pictures to those presented in Section 5.1. Respondents in these conditions updated their beliefs about both poverty and immigration. Fig. B.1 plots beliefs about the number of poor and the number of immigrants reported by the respondents in the Both condition in comparison with those reported in the Neutral condition. Figs. B.1(b) and B.1(d) report respectively the relative frequencies and the kernel densities of beliefs about poverty. Figs. B.1(a) and B.1(c) report analogously on beliefs about immigration. As can be observed in Fig. B.1, beliefs about both the number of poor and the number of immigrants are concentrated around the appropriate values.

Analogous conclusions can be drawn by observing Fig. B.2, which focuses on the beliefs about poverty and immigration reported in the Overlap condition.

Noteworthy is the observation that in the Both and Overlap conditions we do not observe the same extreme concentration in beliefs about immigration found in the Immigration condition as compared to the concentration of beliefs about poverty in the Poverty condition. Rather, the information about immigration and about poverty have similar effects on posterior beliefs. We interpret this asymmetry as a product of the decreased salience of (or decreased attention received by) the information about immigration when presented jointly with information about poverty. This effect can be due, for instance, to information overload
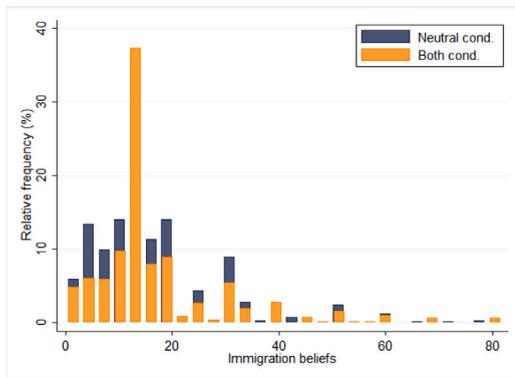
(a) Relative frequency of responses to question about how many people living in Germany were born abroad (in Million)
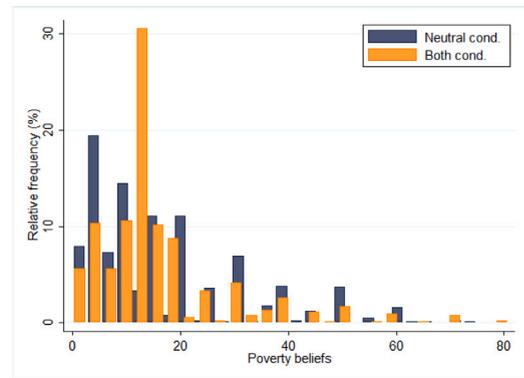
(b) Relative frequency of responses to question about how many people live below the poverty line in Germany (in Million)
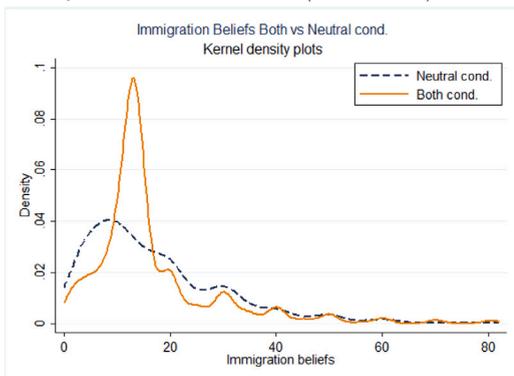
(c) Gaussian kernel density of beliefs about how many people living in Germany were born abroad (in Million)
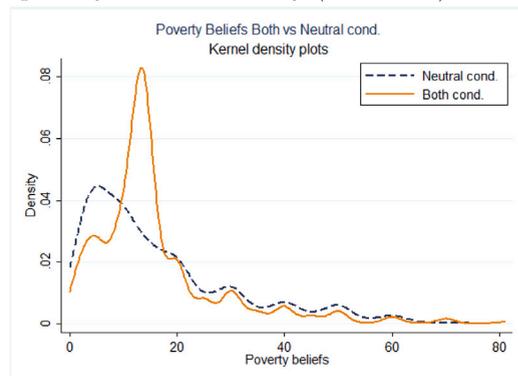
(d) Gaussian kernel density of beliefs about the number of people living below the poverty line in Germany (in Million)

Fig. B.1. Beliefs about the number of immigrants and poor in the Both condition compared to those in the Neutral condition.

(a) Relative frequency of responses to question about how many people living in Germany were born abroad (in Million)

(b) Relative frequency of responses to question about how many people live below the poverty line in Germany (in Million)

(c) Gaussian kernel density of beliefs about how many people living in Germany were born abroad (in Million)

(d) Gaussian kernel density of beliefs about the number of people living below the poverty line in Germany (in Million)

Fig. B.2. Beliefs about the number of immigrants and poor in the Overlap condition compared to those in the Neutral condition.

or to the decreased importance assigned to the immigration topic by respondents who are simultaneously treated with information about poverty. Distinguishing between these two mechanisms is beyond the scope of this paper and hence left for future research.

Table B.1 complements the manipulation analysis presented in Section 5.1. The response variable is an indicator taking value 1 if the respondent's belief about the number of poor and of immigrants lies within a range r from the true value reported (or not) in the experimental conditions, and 0 otherwise. Such indicator is regressed on the condition indicators and on individual and regional controls. As can be seen from Table B.1, having received information about poverty or immigration significantly increases the probability of the responses lying within a range r from the true value, with r taking values 1 Million, 3 Millions and 5 Millions.

Moreover, in line with our salience argument, the impact on posteriors of information about immigration is significantly reduced by its simultaneous presentation with information about poverty. The same analysis for the impact on posteriors of information about poverty yields much less clear cut results, and its simultaneous presentation with information about immigration seems to even strengthen its impact.

B.1. Information vs. priming

The comparison of posterior beliefs across conditions provides clear evidence that beliefs about immigration and poverty were affected by the respective experimental conditions. Notice, however, that posterior beliefs elicited at the end of the survey (rather than in an – ideally obfuscated – follow-up survey 1–2 weeks later, see Haaland et al. (2020)) should be interpreted with caution. Recent investigations have pointed out that posterior beliefs asking respondents to restate the information provided in an information provision experiment, rather than providing their updated posterior beliefs, perform a memory exercise by mechanically restating the number they were provided regardless of the degree to which they believed the information (for instance: Fuster and Zafar (2022)). These numbers might thus not be very informative about people's true beliefs about the size of immigration and/or poverty, but

Table B.1

Probit regression of an indicator taking value 1 if the respondent's belief about the extent of immigration and about the extent of poverty lies within 1 million (columns 1 and 4), 3 million (columns 2 and 5) and 5 million (columns 3 and 6) individuals from the correct value. Individual controls include: age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany. Regional controls include GDP and the share of immigrant population. Pairwise comparisons of the coefficients and their relative size $\Delta\%$ are reported below the estimated coefficients.

	(1) Immigration bias	(2)	(3)	(4) Poverty bias	(5)	(6)
	≤1 ml	≤3 ml	≤5 ml	≤ 1 ml	≤3 ml	≤5 ml
Neutral condition: baseline						
Immigration condition	2.291***	1.268***	1.001***	0.153	−0.005	0.016
	(0.143)	(0.067)	(0.060)	(0.169)	(0.079)	(0.071)
Poverty condition	0.091	−0.099	0.068	1.408***	0.588***	0.469***
	(0.186)	(0.074)	(0.063)	(0.145)	(0.071)	(0.068)
Both condition	1.848***	0.859***	0.637***	1.759***	0.830***	0.617***
	(0.138)	(0.070)	(0.067)	(0.136)	(0.068)	(0.062)
Overlap condition	1.757***	0.662***	0.456***	1.752***	0.754***	0.481***
	(0.125)	(0.067)	(0.063)	(0.137)	(0.066)	(0.063)
Constant	−2.575***	−1.339***	−0.811***	−2.120***	−1.281***	−0.871***
	(0.255)	(0.207)	(0.191)	(0.282)	(0.187)	(0.181)
Individual controls	✓	✓	✓	✓	✓	✓
Regional controls	✓	✓	✓	✓	✓	✓
Land fixed eff.	✓	✓	✓	✓	✓	✓
H_0: Imm. - Both = 0	p < 0.01	p < 0.01	p < 0.01			
$\Delta\%$	19.3	32.2	36.2			
H_0: Imm. - Overlap = 0	p < 0.01	p < 0.01	p < 0.01			
$\Delta\%$	23.2	47.8	54.4			
H_0: Pov. - Both = 0				p < 0.01	p < 0.01	p = 0.019
$\Delta\%$				−25.0	−41.2	−31.4
H_0: Pov. - Overlap = 0				p < 0.01	p < 0.01	p = 0.853
$\Delta\%$				−24.4	−28.3	−0.2
Observations	3955	3955	3955	3955	3955	3955

Robust standard errors in parentheses.

*** p < 0.01, ** p < 0.05, * p < 0.1.

rather reflect how well they paid attention to these numbers when we provided them at the beginning of the survey. This is in fact how we interpret and analyse them in Section 5.

A natural question is whether the effect of our conditions is the product of (upwards or downwards) belief correction or of 'induced attention focus' on these phenomena (thus consisting in essence of *priming* conditions). In Section 4 we argue that our experimental conditions have worked primarily via the awareness channel. It is possible however to draw a few insights concerning how our treatments factually focused *treated* individuals' attention towards the mentioned number. Analysing beliefs in the control group (and assuming that prior beliefs in the respective experimental conditions were distributed similarly on average) reveals that the Poverty condition on average corrected the respondents' average underestimation of the incidence of poverty. The effect of correcting the underestimation of poverty is in this case the same as that expected from priming the respondent with the poverty topic. Distinguishing between the priming effect and that of correcting the respondents' underestimation is outside of the scope of this paper.[50]

Conversely, untreated respondents appear to over- and underestimate the size of the immigrant population roughly equally, such that our intervention is on average expected to correct both over- and underestimations of equal sizes. Assuming that support for redistributive policies would be affected in opposite directions for over- and underestimations, the effect we would observe would be the average of two opposing forces, which could potentially (to some extent) cancel out. The results presented in Section 5 however do not support this conclusion. In fact, we find some of the most pronounced effects precisely in the Immigration condition.

We are thus reassured in our assessment that the experimental conditions worked predominantly via the priming channel.

Appendix C. Robustness and further analyses

C.1. Interacted regressions

In Section 5, we presented regressions of our index of desired tax progressivity and desired public expenditure on education programmes on sample splits across income brackets (Tables 3 and 4). Here we present analogous OLS regressions in which the

---

[50] Notice however that the greater sluggishness with which the beliefs about the poverty appear to have been manipulated relative to beliefs about immigration in Fig. 2 suggests that our findings concerning the effect information about poverty might be a lower bound for the effects which would have been observed under stronger belief correction.

Table C.1

Combination of interaction coefficient estimates from an OLS regression of our standardised progressivity measure of the respondents' preferred tax schedule (column 1) and of preferred share of public budget allocated to education (column 2).

Combination of estimated coefficients: $\beta_{1|c} + \beta_{3|c,x}$	(1) Tax progressivity	(2) Public exp. on educ.
Condition × Low income		
Immigration condition	0.242**	−0.317***
	(0.112)	(0.111)
Poverty condition	0.108	−0.161
	(0.116)	(0.116)
Both condition	0.063	−0.343***
	(0.119)	(0.114)
Overlap condition	−0.054	−0.241*
	(0.123)	(0.123)
Condition × High income		
Immigration condition	−0.163***	−0.094
	(0.049)	(0.062)
Poverty condition	−0.069	0.194***
	(0.053)	(0.057)
Both condition	−0.062	−0.108*
	(0.046)	(0.059)
Overlap condition	−0.104*	0.109**
	(0.054)	(0.052)
Individual controls	✓	✓
Regional controls	✓	✓
Land fixed eff.	✓	✓
Observations	3952	3955
R-squared	0.0329	0.048

Robust standard errors in parentheses.

*** p < 0.01, ** p < 0.05, * p < 0.1.

condition indicators are interacted with the same income brackets. To ease comparison with Tables 3 and 4, we present the results as the combination of the coefficients estimated on the interaction term and on the condition indicators. Table C.1 reports such combinations for the respondents' preferred tax schedule (column 1) and preferred share of public budget allocated to education (column 2).

The condition indicators $c = \{$Neutral, Poverty, Immigration, Both, Overlap$\}$ are interacted with income bracket indicators $x = \{$below the poverty line, above the poverty line (high income)$\}$. We estimate $y_i = \alpha + \beta_1 c + \beta_2 x + \beta_3 (c \times x) + \gamma_1' X_i + \gamma_2' W_i + \gamma_3 Land_i + \varepsilon_i$ (see Eq. (1)). The combined coefficient $\beta_{1|c} + \beta_{3|c,x}$, reported in Table C.1, yields the effect of condition c on individuals in income bracket x.

Individual controls include: household income, university education, political preferences, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany. Regional controls include GDP and the share of immigrant population.

C.2. Further heterogeneities: Education level and children, and political orientation

As discussed in Section 5, heterogeneity with respect to the presence of children and individual education level might illuminate on the findings on the impact of information about immigration and poverty summarised in Results 4 and 5. First, the respondents' education level might plausibly impact their understanding of the policies we investigate and, more importantly, their understanding of the connection between the content of our experimental conditions and said policies. Second, effects might also differ depending on whether the respondent does or does not have young children, in particular concerning desired education spending, as this group would plausibly be the most affected by changes in the allocations to public education.

Table C.2 therefore explores the heterogeneity of our condition effects with respect to the respondents' education level (university degree compared to lower) and whether they have children or not. Each of these two variables separately does not seem to drive any heterogeneity in experimental effects.

Tables C.3–C.6 repeat this analysis by further splitting according to the respondents' income as elsewhere in this article. We again find no evidence for strong heterogeneities in experimental condition effects according to the respondent's education level and presence of children.

Tables C.7–C.9 investigate heterogeneities along the political dimension, finding no relevant heterogeneities there either.

Table C.2

OLS regression of standardised desired tax progressivity and public spending on education. The experimental condition dummies are interacted with an indicator taking value College = 1 for people with a university degree (columns 1 and 2), and with an indicator taking value Anychild = 1 for individuals who have at least one child in their household. Individual controls include: household income, political placement, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany. Regional controls include GDP and the share of immigrant population. Standard errors are clustered at Kreis level.

Variables	(1) Desired tax prog.	(2) Desired education spending	(3) Desired tax prog.	(4) Desired education spending
Immigration	−0.123**	−0.000	−0.098*	0.052
	(0.062)	(0.065)	(0.055)	(0.059)
Poverty	−0.037	0.086	−0.089	0.119**
	(0.057)	(0.060)	(0.058)	(0.053)
Both	−0.081	0.006	−0.095*	0.032
	(0.058)	(0.066)	(0.050)	(0.053)
Overlap	−0.114*	0.029	−0.164***	0.053
	(0.059)	(0.060)	(0.054)	(0.054)
College	−0.054	0.135*		
	(0.072)	(0.082)		
Immigration × College	0.169*	0.013		
	(0.100)	(0.101)		
Poverty × College	0.027	0.112		
	(0.106)	(0.124)		
Both × College	0.163	0.040		
	(0.104)	(0.116)		
Overlap × College	0.083	0.020		
	(0.124)	(0.102)		
Anychild			−0.321***	0.238**
			(0.083)	(0.093)
Immigration × Anychild			0.104	−0.193
			(0.120)	(0.117)
Poverty × Anychild			0.246**	−0.016
			(0.116)	(0.116)
Both × Anychild			0.261**	−0.073
			(0.114)	(0.116)
Overlap × Anychild			0.296**	−0.069
			(0.124)	(0.131)
Constant	0.040	0.271*	0.145	0.141
	(0.162)	(0.155)	(0.156)	(0.152)
Observations	3952	3955	3952	3955
R-squared	0.031	0.047	0.035	0.051

Robust standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

C.3. Different income splits

In Section 5.2.2 we examine the effect of our experimental conditions on people with low or higher incomes. For this purpose, we split our sample into two groups. The low income group defined as people earning (equivalent household) income below the poverty line (defined as 60% of median income, and amounts to 1096 Euros for 2018), and the high income group including everyone earning (equivalent household) income above the poverty line. It might be argued that despite being the least arbitrary choice, both in terms of consistency with our experimental design (the Poverty Condition defines the poor as those living below the poverty line) and conceptually (it is how people perceive their position in the income distribution which will drive their demand for redistribution), such split might be too rough at higher incomes and might be hiding potential heterogeneities in higher segments of the income distribution from view.

To gain a more nuanced insight on the impact of income on our effects we now expand our analysis to three income groups, thus adding a middle income group to our analyses. Different from the definition of the low income group, we cannot rely on an objective and universally acceptable discriminant between the middle and highest income groups. To reduce the risk of obtaining results solely due to arbitrariness in our upper income split we therefore follow three strategies which yield very similar results and support our findings.

First, people living below the poverty line make up roughly 16 percent of the population. For this reason, we define the high income earners as those who are the top 16% income earners (which amounts to earning more than 2800 Euros for a one-person household) thus obtaining roughly equally sized groups of rich and poor. The remainder, i.e. the people in between, make up the middle income group. Results are presented in Tables C.10 and C.11.

Second, the German government defines the middle class ("Mittelschicht") very broadly as people earning between 60% and 200% of median income. For our analysis this would imply the same definition of low income, but a much higher cut-off for the rich (at roughly 3600 Euros). The results from adopting this definition are presented in Tables C.12 and C.13.

Table C.3

OLS regression of desired tax progressivity by income split. The experimental condition dummies are interacted with an indicator taking value College = 1 for people with a university degree. Individual controls include: household income, political placement, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany. Regional controls include GDP and the share of immigrant population. Standard errors are clustered at Kreis level.

Variables	(1) Desired tax progressivity	(2)
	Low income	High income
Immigration	0.304**	−0.274***
	(0.134)	(0.068)
Poverty	0.117	−0.087
	(0.129)	(0.064)
Both	0.028	−0.126**
	(0.141)	(0.063)
Overlap	−0.022	−0.149**
	(0.123)	(0.071)
College	0.166	−0.122
	(0.226)	(0.081)
Immigration × College	−0.238	0.328***
	(0.359)	(0.108)
Poverty × College	−0.030	0.067
	(0.298)	(0.117)
Both × College	0.142	0.200*
	(0.279)	(0.112)
Overlap × College	−0.218	0.133
	(0.431)	(0.134)
Constant	−0.136	0.029
	(0.468)	(0.179)
Controls	✓	✓
Observations	787	3165
R-squared	0.062	0.036

Robust standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.4

OLS regression of public spending on education by income split. The experimental condition dummies are interacted with an indicator taking value College = 1 for people with a university degree. Individual controls include: household income, political placement, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany. Regional controls include GDP and the share of immigrant population. Standard errors are clustered at Kreis level.

Variables	(1) Desired education spending	(2)
	Low income	High income
Immigration	−0.301**	0.103
	(0.120)	(0.081)
Poverty	−0.159	0.164**
	(0.125)	(0.074)
Both	−0.444***	0.120
	(0.130)	(0.078)
Overlap	−0.186	0.107
	(0.131)	(0.068)
College	0.345	0.135
	(0.233)	(0.088)
Immigration × College	−0.032	−0.028
	(0.293)	(0.114)
Poverty × College	0.118	0.083
	(0.344)	(0.132)
Both × College	0.285	−0.039
	(0.300)	(0.129)
Overlap × College	−0.188	0.016
	(0.315)	(0.111)
Constant	1.161***	0.171
	(0.390)	(0.180)
Controls	✓	✓
Observations	787	3168
R-squared	0.117	0.037

Robust standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.5

OLS regression of preferred tax progressivity by income split. The experimental condition dummies are interacted with an indicator taking value Anychild = 1 for people with at least one child in the household. Individual controls include: household income, political placement, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany. Regional controls include GDP and the share of immigrant population. Standard errors are clustered at Kreis level.

Variables	(1)	(2)
	Desired tax progressivity	
	Low income	High income
Immigration	0.226*	−0.183***
	(0.126)	(0.056)
Poverty	0.020	−0.121**
	(0.137)	(0.061)
Both	−0.106	−0.097*
	(0.136)	(0.055)
Overlap	−0.139	−0.165***
	(0.148)	(0.059)
Anychild	−0.324*	−0.317***
	(0.191)	(0.089)
Immigration × anychild	0.194	0.097
	(0.265)	(0.136)
Poverty × anychild	0.405	0.222*
	(0.271)	(0.122)
Both × anychild	0.767***	0.161
	(0.275)	(0.127)
Overlap × anychild	0.447	0.239*
	(0.327)	(0.127)
Constant	0.165	0.270
	(0.445)	(0.180)
Controls	✓	✓
Observations	787	3165
R-squared	0.074	0.041

Robust standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.6

OLS regression of preferred public spending on education by income split. The experimental condition dummies are interacted with an indicator taking value Anychild = 1 for people with at least one child in the household. Individual controls include: household income, political placement, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany. Regional controls include GDP and the share of immigrant population. Standard errors are clustered at Kreis level.

Variables	(1)	(2)
	Desired education spending	
	Low income	High income
Immigration	−0.210*	0.136**
	(0.118)	(0.067)
Poverty	−0.049	0.169***
	(0.126)	(0.063)
Both	−0.344***	0.114*
	(0.121)	(0.062)
Overlap	−0.100	0.110*
	(0.122)	(0.062)
Anychild	0.477**	0.193*
	(0.233)	(0.100)
Immigration × Anychild	−0.456	−0.152
	(0.308)	(0.126)
Poverty × Anychild	−0.442	0.081
	(0.288)	(0.125)
Both × Anychild	−0.209	−0.036
	(0.285)	(0.127)
Overlap × Anychild	−0.458	0.007
	(0.306)	(0.142)
Constant	0.676*	0.103
	(0.380)	(0.168)
Controls	✓	✓
Observations	787	3168
R-squared	0.128	0.047

Robust standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.7

OLS regression of standardised desired tax progressivity and preferred education spending along the political orientation split. The square brackets contain p-values corrected for multiplicity of tested hypotheses (List et al., 2016). Individual controls include: household income, university education, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany. Regional controls include GDP and the share of immigrant population.

Variables	Desired tax progressivity		Preferred education spending	
	Leftwing	Rightwing	Leftwing	Rightwing
Neutral condition: baseline				
Immigration condition	−0.101	−0.029	0.042	−0.100
	[0.464]	[0.684]	[0.912]	[0.673]
Poverty condition	−0.135	0.113	0.133	0.075
	[0.258]	[0.589]	[0.124]	[0.961]
Both condition	−0.029	−0.035	0.036	−0.052
	[0.930]	[0.879]	[0.814]	[0.940]
Overlap condition	−0.129	−0.041	0.068	−0.045
	[0.272]	[0.962]	[0.839]	[0.647]
Constant	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓
Regional controls	✓	✓	✓	✓
Land fixed eff.	✓	✓	✓	✓
Observations	2530	1427	2532	1428
R-squared	0.027	0.033	0.045	0.034

Robust p-values corrected for multiple hypotheses in square brackets.
Corrected p-values significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.8

OLS regression of standardised desired tax progressivity along the political orientation split, by income bracket. The square brackets contain p-values corrected for multiplicity of tested hypotheses (List et al., 2016). Individual controls include: household income, university education, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany. Regional controls include GDP and the share of immigrant population.

Variables	Desired tax progressivity			
	Low income		High income	
	Leftwing	Rightwing	Leftwing	Rightwing
Neutral condition: baseline				
Immigration condition	0.054	0.648*	−0.134	−0.216
	[0.879]	[0.091]	[0.493]	[0.153]
Poverty condition	−0.240	0.719***	−0.107	−0.045
	[0.631]	[<0.001]	[0.622]	[0.960]
Both condition	−0.084	0.265	−0.016	−0.133
	[0.933]	[0.882]	[0.795]	[0.595]
Overlap condition	−0.343	0.425	−0.079	−0.176
	[0.464]	[0.528]	[0.806]	[0.505]
Constant	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓
Regional controls	✓	✓	✓	✓
Land fixed eff.	✓	✓	✓	✓
Observations	533	257	1997	1170
R-squared	0.083	0.152	0.031	0.037

Robust p-values corrected for multiple hypotheses in square brackets.
Corrected p-values significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

A statistically meaningful income split that might illuminate on the meaningfulness of the socio-economic definitions here used is that identifying the bottom quartile as "low income", the second and third quartiles as "middle income" and the top quartile as "high income". The corresponding (equivalent household) income cutoffs would be 1200 and 2500 Euro. Tables C.14 and C.15 display our main results with these splits. As with the previous splits, the results remain qualitatively the same though our result for tax progressivity disappears once we correct for multiplicity. One general regularity from all the different income splits is that if we use a broader definition for the low income group, the effect of immigration on desired tax progressivity is still positive, but the coefficient loses significance, likely due to the fact that expanding the low income group means classifying respondents from the

Table C.9

OLS regression of standardised desired allocation of the public budget to public education along the political orientation split, by income bracket. The square brackets contain p-values corrected for multiplicity of tested hypotheses (List et al., 2016). Individual controls include: household income, university education, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany. Regional controls include GDP and the share of immigrant population.

Variables	Preferred education expenditure			
	Low income		High income	
	Leftwing	Rightwing	Leftwing	Rightwing
Neutral condition: baseline				
Immigration condition	−0.159	−0.636**	0.098	0.036
	[0.772]	[0.023]	[0.798]	[0.829]
Poverty condition	−0.156	−0.124	0.205**	0.131
	[0.819]	[0.783]	[0.010]	[0.752]
Both condition	−0.281*	−0.564	0.120*	0.056
	[0.082]	[0.669]	[0.671]	[0.922]
Overlap condition	−0.197	−0.310	0.140**	0.028
	[0.743]	[0.767]	[0.348]	[0.916]
Constant	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓
Regional controls	✓	✓	✓	✓
Land fixed eff.	✓	✓	✓	✓
Observations	533	257	1999	1171
R-squared	0.081	0.170	0.039	0.034

Robust p-values corrected for multiple hypotheses in square brackets.
Corrected p-values significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.10

OLS regression of desired tax progressivity with income splits using the German government definition of middle class. The square brackets contain p-values corrected for multiplicity of tested hypotheses (List et al., 2016). Individual controls include: university education, political placement, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany. Regional controls include GDP and the share of immigrant population.

Variables	Desired tax progressivity		
	Low income	Middle income	High income
Neutral condition: baseline			
Immigration condition	0.273	−0.174**	−0.076
	[0.198]	[0.044]	[0.931]
Poverty condition	0.113	−0.046	−0.133
	[0.905]	[0.913]	[0.850]
Both condition	0.063	−0.075	0.023
	[0.936]	[0.837]	[0.840]
Overlap condition	−0.041	−0.084	−0.135
	[0.925]	[0.828]	[0.841]
Constant	✓	✓	✓
Individual controls	✓	✓	✓
Regional controls	✓	✓	✓
Land fixed eff.	✓	✓	✓
Observations	787	2456	709
R-squared	0.065	0.037	0.071

Robust p-values corrected for multiple hypotheses in square brackets.
Corrected p-values significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

middle income group who on average react in the opposite way as having a low income. The results concerning education spending are very robust (also in terms of significance) to the different splits.

C.4. Different tax progressivity indices

Section 5 analyses the effect of our experimental conditions on desired tax progressivity by constructing an index τ capturing such desired progressivity as the (unweighted) sum of the desired tax rates on the top 1% and top 10%, minus the desired tax rates

Table C.11

OLS regression of desired public expenditure on education with income splits using the German government definition of middle class. The square brackets contain p-values corrected for multiplicity of tested hypotheses (List et al., 2016). Individual controls include: university education, political placement, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany. Regional controls include GDP and the share of immigrant population.

Variables	Preferred education spending		
	Low income	Middle income	High income
Neutral condition: baseline			
Immigration condition	−0.315*	0.134	−0.075
	[0.053]	[0.397]	[0.800]
Poverty condition	−0.150	0.245**	0.013
	[0.606]	[0.011]	[0.907]
Both condition	−0.393***	0.148	−0.037
	[0.004]	[0.364]	[0.905]
Overlap condition	−0.208	0.168	−0.101
	[0.397]	[0.117]	[0.709]
Constant	✓	✓	✓
Individual controls	✓	✓	✓
Regional controls	✓	✓	✓
Land fixed eff.	✓	✓	✓
Observations	787	2458	710
R-squared	0.120	0.045	0.113

Robust p-values corrected for multiple hypotheses in square brackets.
Corrected p-values significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.12

OLS regression of desired tax progressivity with income splits using the German government definition of middle class. The square brackets contain p-values corrected for multiplicity of tested hypotheses (List et al., 2016). Individual controls include: university education, political placement, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany. Regional controls include GDP and the share of immigrant population.

Variables	Desired tax progressivity		
	Low income	Middle income	High income
Neutral condition: baseline			
Immigration condition	0.273	−0.176***	0.002
	[0.160]	[<0.001]	[0.993]
Poverty condition	0.113	−0.094	0.268
	[0.829]	[0.458]	[0.634]
Both condition	0.063	−0.089	0.294
	[0.932]	[0.448]	[0.484]
Overlap condition	−0.041	−0.128	0.179
	[0.922]	[0.227]	[0.785]
Constant	✓	✓	✓
Individual controls	✓	✓	✓
Regional controls	✓	✓	✓
Land fixed eff.	✓	✓	✓
Observations	787	2934	231
R-squared	0.065	0.036	0.170

Robust p-values corrected for multiple hypotheses in square brackets.
Corrected p-values significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

on the next 40%: $\tau = t_{(100)} + t_{(91-99)} - t_{(51-90)}$ (see Section 5). This seems the most obvious measure of tax progressivity, given that the bottom 50% of income earners do not pay a lot of income taxes in Germany.[51] As the size of the public budget is fixed, in order to increase taxes on the rich (i.e. the top 10%), the taxes on the next 40% must therefore be lowered.[52] The results in Tables C.16–C.19,

---

[51] See for instance https://www.iwkoeln.de/studien/iw-kurzberichte/beitrag/martin-beznoska-wer-zahlt-wie-viel-einkommensteuer-in-deutschland-405957.html.

[52] Recall that we programmed the tax-rate slider task such that background computation add up all the collected taxes with the aim for the respondents to collect the budget actually collected through German income taxes in 2017.

Table C.13

OLS regression of desired public expenditure on education with income splits using the German government definition of middle class. The square brackets contain p-values corrected for multiplicity of tested hypotheses (List et al., 2016). Individual controls include: university education, political placement, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany. Regional controls include GDP and the share of immigrant population.

Variables	Preferred education spending		
	Low income	Middle income	High income
Neutral condition: baseline			
Immigration condition	−0.315**	0.112	−0.177
	[0.034]	[0.445]	[0.753]
Poverty condition	−0.150	0.204***	0.059
	[0.637]	[0.004]	[0.802]
Both condition	−0.393***	0.132	−0.201
	[0.007]	[0.286]	[0.728]
Overlap condition	−0.208	0.120	−0.049
	[0.384]	[0.267]	[0.953]
Constant	✓	✓	✓
Individual controls	✓	✓	✓
Regional controls	✓	✓	✓
Land fixed eff.	✓	✓	✓
Observations	787	2936	232
R-squared	0.120	0.042	0.197

Robust p-values corrected for multiple hypotheses in square brackets.
Corrected p-values significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.14

OLS regression of desired tax progressivity with income splits as bottom quartile, middle two quartiles and top quartile. The square brackets contain p-values corrected for multiplicity of tested hypotheses (List et al., 2016). Individual controls include: university education, political placement, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany. Regional controls include GDP and the share of immigrant population.

Variables	Desired tax progressivity		
	Low income	Middle income	High income
Neutral condition: baseline			
Immigration condition	0.213	−0.157	−0.090
	[0.418]	[0.105]	[0.871]
Poverty condition	0.117	−0.052	−0.134
	[0.840]	[0.854]	[0.825]
Both condition	0.046	−0.074	0.030
	[0.901]	[0.794]	[0.767]
Overlap condition	−0.063	−0.068	−0.157
	[0.899]	[0.858]	[0.794]
Constant	✓	✓	✓
Individual controls	✓	✓	✓
Regional controls	✓	✓	✓
Land fixed eff.	✓	✓	✓
Observations	885	2343	724
R-squared	0.056	0.037	0.071

Robust p-values corrected for multiple hypotheses in square brackets.
Corrected p-values significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

reporting separate split sample analyses for each desired tax rate, confirm in fact that all the "action" occurs on the tax rate applied to the top 1% income earners and to the 51st to 90th percentiles.

We further present the effect of our conditions on alternative indices of tax progressivity. Table C.20 shows the effect on a tax progressivity index $\hat{\tau}$ defined as the sum of taxes on the top 1% and next 9% minus taxes on the next 40%: $\hat{\tau} = t_{(100)} + t_{(91-99)} - t_{(51-90)} - t_{(0-50)}$. This index therefore includes the bottom 50%.

Table C.21 further shows the effect of our conditions on a desired tax progressivity index $\tilde{\tau}$ defined simply as the difference between taxes on the top 1% and the 51st to 90th percentiles ("the next 40%"), thus ignoring the 9% between the 90th and 99th percentiles: $\tilde{\tau} = t_{(100)} - t_{(51-90)}$. Our results are robust to all these specifications.

Table C.15

OLS regression of desired public expenditure on education with income splits as bottom quartile, middle two quartiles and top quartile. The square brackets contain p-values corrected for multiplicity of tested hypotheses (List et al., 2016). Individual controls include: university education, political placement, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany. Regional controls include GDP and the share of immigrant population.

Variables	Preferred education spending		
	Low income	Middle income	High income
Neutral condition: baseline			
Immigration condition	−0.344***	0.170	−0.103
	[<0.001]	[0.191]	[0.620]
Poverty condition	−0.124	0.251***	−0.018
	[0.613]	[0.007]	[0.855]
Both condition	−0.334**	0.160	−0.079
	[0.021]	[0.199]	[0.697]
Overlap condition	−0.227	0.202**	−0.133
	[0.226]	[0.019]	[0.557]
Constant	✓	✓	✓
Individual controls	✓	✓	✓
Regional controls	✓	✓	✓
Land fixed eff.	✓	✓	✓
Observations	885	2345	725
R-squared	0.123	0.047	0.115

Robust p-values corrected for multiple hypotheses in square brackets
Corrected p-values significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.16

Split sample OLS regression of desired tax rate on the Top 1%. The square brackets contain p-values corrected for multiplicity of tested hypotheses (List et al., 2016). Individual controls include: university education, political placement, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany. Regional controls include GDP and the share of immigrant population.

Variables	Desired tax rate on top 1%	
	Low income	High income
Neutral condition: baseline		
Immigration condition	0.232	−0.167***
	[0.268]	[<0.001]
Poverty condition	0.089	−0.079
	[0.815]	[0.509]
Both condition	0.048	−0.069
	[0.893]	[0.502]
Overlap condition	−0.032	−0.114
	[0.801]	[0.304]
Constant	✓	✓
Individual controls	✓	✓
Regional controls	✓	✓
Land fixed eff.	✓	✓
Observations	787	3165
R-squared	0.064	0.034

Robust p-values corrected for multiple hypotheses in square brackets.
Corrected p-values significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

C.5. Other public expenditure items

We here report analyses analogous to those reported in Tables 4 and 2 for preferences for public education on the other public expenditure items available to the respondents. As done for public education, we report results from split sample regressions along the income dimension (Tables C.22 to C.25). These results reveal no systematic variations that can be attributed to the experimental intervention. We therefore can conclude that while the experimental intervention causes responses to systematically vary their preferred allocation of public money to education programmes, they do not seem to be systematically shifting to or away from other expenditure items.

Table C.17

Split sample OLS regression of desired tax rate on the Next 9%. The square brackets contain p-values corrected for multiplicity of tested hypotheses (List et al., 2016). Individual controls include: university education, political placement, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany. Regional controls include GDP and the share of immigrant population.

Variables	Desired tax rate on next 9%	
	Low income	High income
Neutral condition: baseline		
Immigration condition	0.162	−0.058
	[0.669]	[0.858]
Poverty condition	0.085	0.006
	[0.953]	[0.921]
Both condition	0.024	−0.007
	[0.998]	[0.986]
Overlap condition	−0.081	−0.045
	[0.933]	[0.924]
Constant	✓	✓
Individual controls	✓	✓
Regional controls	✓	✓
Land fixed eff.	✓	✓
Observations	787	3165
R-squared	0.047	0.027

Robust p-values corrected for multiple hypotheses in square brackets.
Corrected p-values significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.18

Split sample OLS regression of desired tax rate on the Next 40%. The square brackets contain p-values corrected for multiplicity of tested hypotheses (List et al., 2016). Individual controls include: university education, political placement, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany. Regional controls include GDP and the share of immigrant population.

Variables	Desired tax rate on next 40%	
	Low income	High income
Neutral condition: baseline		
Immigration condition	−0.308**	0.144**
	[0.021]	[0.019]
Poverty condition	−0.124	0.079
	[0.580]	[0.595]
Both condition	−0.101	0.059
	[0.588]	[0.573]
Overlap condition	−0.020	0.070
	[0.857]	[0.619]
Constant	✓	✓
Individual controls	✓	✓
Regional controls	✓	✓
Land fixed eff.	✓	✓
Observations	787	3165
R-squared	0.047	0.019

Robust p-values corrected for multiple hypotheses in square brackets.
Corrected p-values significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

C.6. Further outcomes

The survey also allows us to cross-check some of the findings presented in this article using additional outcomes we collected for this purpose. In particular, our respondents answered questions about whether they think that poverty and inequality are a serious problem in contemporary Germany, whether they think the government should intervene to reduce inequality, and whether they would support government interventions aimed at increasing the number of schools in poor districts, providing housing to those in need and increasing income support programmes. The point estimates are reported in Tables C.28 and C.29 for, respectively, the low income and high income respondents.

Table C.19

Split sample OLS regression of desired tax rate on the Bottom 50%. The square brackets contain p-values corrected for multiplicity of tested hypotheses (List et al., 2016). Individual controls include: university education, political placement, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany. Regional controls include GDP and the share of immigrant population.

Variables	Desired tax rate on the bottom 50%	
	Low income	High income
Neutral condition: baseline		
Immigration condition	0.055	−0.029
	[0.956]	[0.982]
Poverty condition	0.022	0.033
	[0.886]	[0.976]
Both condition	−0.033	0.027
	[0.970]	[0.954]
Overlap condition	−0.178	−0.060
	[0.869]	[0.859]
Constant	✓	✓
Individual controls	✓	✓
Regional controls	✓	✓
Land fixed eff.	✓	✓
Observations	787	3165
R-squared	0.061	0.043

Robust p-values corrected for multiple hypotheses in square brackets.
Corrected p-values significance: *** p < 0.01, ** p < 0.05, * p < 0.1.

Table C.20

Split sample OLS regression of desired tax progressivity defined as the sum of the tax rates on the top 1% and next 9% minus the tax rates on the next 40% and bottom 50%. The square brackets contain p-values corrected for multiplicity of tested hypotheses (List et al., 2016). Individual controls include: university education, political placement, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany. Regional controls include GDP and the share of immigrant population.

Variables	Desired tax progressivity: $\hat{\tau}$	
	Low income	High income
Neutral condition: baseline		
Immigration condition	0.231	−0.134**
	[0.316]	[0.04]
Poverty condition	0.095	−0.042
	[0.797]	[0.865]
Both condition	0.038	−0.039
	[0.777]	[0.909]
Overlap condition	−0.088	−0.100
	[0.755]	[0.305]
Constant	✓	✓
Individual controls	✓	✓
Regional controls	✓	✓
Land fixed eff.	✓	✓
Observations	787	3165
R-squared	0.068	0.042

Robust p-values corrected for multiple hypotheses in square brackets
Corrected p-values significance: *** p < 0.01, ** p < 0.05, * p < 0.1.

Starting with the low income group, Table C.28 is aligned with most of the results reported in Section 5. In particular, we see that in both tables, the perception of poverty as a problem is not sensitive to any of our experimental conditions (crucially, not even to the poverty condition itself). On the other hand, weak increases in the perceived severity of inequality as a problem can be observed in all conditions, including and especially in reaction to the *Both* Condition, suggesting some degree of additivity. To interpret this finding notice that, conversely, our respondents seem not to increase their support for government intervention nor poverty alleviation programmes in response to our experimental conditions. Perceptions of the severity of poverty and inequality as a problem need not be accompanied by the conviction that the government should necessarily intervene on it. Notice that similar

Table C.21

Split sample OLS regression of desired tax progressivity defined taxes on the top 1% minus taxes on the 51–90 percentile ("the next 40%"). The square brackets contain p-values corrected for multiplicity of tested hypotheses (List et al., 2016). Individual controls include: university education, political placement, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany. Regional controls include GDP and the share of immigrant population.

Variables	Desired tax progressivity: $\tilde{\tau}$	
	Low income	High income
Neutral condition: baseline		
Immigration condition	0.270	−0.172***
	[0.160]	[<0.001]
Poverty condition	0.105	−0.084
	[0.721]	[0.420]
Both condition	0.067	−0.071
	[0.808]	[0.463]
Overlap condition	−0.019	−0.109
	[0.870]	[0.212]
Constant	✓	✓
Individual controls	✓	✓
Regional controls	✓	✓
Land fixed eff.	✓	✓
Observations	787	3165
R-squared	0.063	0.031

Robust p-values corrected for multiple hypotheses in square brackets
Corrected *p*-value significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.22

OLS regression of standardised preferred share of public budget allocated to domestic security. The square brackets contain p-values corrected for multiplicity of tested hypotheses (List et al., 2016). Omitted individual controls include: university education, political placement, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany.

Variables	Domestic security	
	Low income	High income
Neutral condition: baseline		
Immigration condition	0.082	−0.054
	[0.712]	[0.794]
Poverty condition	0.207	−0.098
	[0.613]	[0.370]
Both condition	0.086	−0.065
	[0.795]	[0.724]
Overlap condition	0.052	−0.099
	[0.593]	[0.385]
Constant	✓	✓
Individual controls	✓	✓
Regional controls	✓	✓
Land fixed eff.	✓	✓
Observations	787	3165
R-squared	0.082	0.095

Robust p-values corrected for multiple hypotheses in square brackets
Corrected *p*-value significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

to what observed in Section 5 systematic reactions for the four outcomes pertaining to government intervention are observed in correspondence to information about immigration (noticeably with a negative sign when public schooling is concerned).

Not much emerges from an analysis of these outcomes on the high income sample.

C.7. Correction for multiple hypothesis testing

We corrected the standard errors on all our coefficients of interest wherever computationally possible. The statistical package we used was designed by Barsbai et al. (2020) to implement the method reported in List et al. (2016). This package does not

Table C.23

OLS regression of standardised preferred share of public budget allocated to public infrastructure development. The square brackets contain p-values corrected for multiplicity of tested hypotheses (List et al., 2016). Omitted individual controls include: university education, political placement, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany.

Variables	Infrastructure	
	Low income	High income
Neutral condition: baseline		
Immigration condition	−0.162	0.032
	[0.625]	[0.991]
Poverty condition	−0.013	0.027
	[0.918]	[0.987]
Both condition	0.018	0.034
	[0.985]	[0.995]
Overlap condition	−0.061	−0.019
	[0.993]	[0.986]
Constant	✓	✓
Individual controls	✓	✓
Regional controls	✓	✓
Land fixed eff.	✓	✓
Observations	787	3165
R-squared	0.090	0.050

Robust p-values corrected for multiple hypotheses in square brackets
Corrected *p*-value significance: *** p < 0.01, ** p < 0.05, * p < 0.1.

Table C.24

OLS regression of standardised preferred share of public budget allocated to public social insurance. The square brackets contain p-values corrected for multiplicity of tested hypotheses (List et al., 2016). Omitted individual controls include: university education, political placement, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany.

Variables	Social insurance and pensions	
	Low income	High income
Neutral condition: baseline		
Immigration condition	0.051	−0.037
	[0.959]	[0.956]
Poverty condition	0.024	−0.024
	[0.818]	[0.952]
Both condition	0.069	−0.091
	[0.973]	[0.517]
Overlap condition	0.034	−0.075
	[0.932]	[0.802]
Constant	✓	✓
Individual controls	✓	✓
Regional controls	✓	✓
Land fixed eff.	✓	✓
Observations	787	3165
R-squared	0.074	0.030

Robust p-values corrected for multiple hypotheses in square brackets
Corrected *p*-value significance: *** p < 0.01, ** p < 0.05, * p < 0.1.

report corrected standard errors, but only corrected p-values (the relevant information in this context). For this reason, and to avoid overburdening the reader, we reported the p-values directly in the tables in place of the commonly reported standard errors. Notice moreover, that the package only reports the corrected p-values for the hypotheses explicitly corrected for in the command syntax. The estimates not explicitly corrected for are not reported (e.g. the constant, the control variables). For this reason, we report only the relevant point estimates for the sake of clarity. Full regression outputs and the do-files are available upon request. In practical terms, we corrected for the simultaneous testing of multiple treatments and outcomes.

Table C.25

OLS regression of standardised preferred share of public budget allocated to public unemployment insurance. The square brackets contain p-values corrected for multiplicity of tested hypotheses (List et al., 2016). Omitted individual controls include: university education, political placement, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany.

Variables	Unemployment and welfare benefits	
	Low income	High income
Neutral condition: baseline		
Immigration condition	0.166	−0.036
	[0.711]	[0.927]
Poverty condition	0.206	0.062
	[0.744]	[0.727]
Both condition	0.204	−0.009
	[0.609]	[0.872]
Overlap condition	0.056	0.026
	[0.904]	[0.908]
Constant	✓	✓
Individual controls	✓	✓
Regional controls	✓	✓
Land fixed eff.	✓	✓
Observations	787	3165
R-squared	0.076	0.058

Robust p-values corrected for multiple hypotheses in square brackets
Corrected p-values significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.26

OLS regression of standardised preferred share of public budget allocated to public health insurance. The square brackets contain p-values corrected for multiplicity of tested hypotheses (List et al., 2016). Omitted individual controls include: university education, political placement, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany.

Variables	Health care	
	Low income	High income
Neutral condition: baseline		
Immigration condition	0.115	−0.011
	[0.803]	[0.876]
Poverty condition	−0.136	−0.110
	[0.738]	[0.318]
Both condition	0.126	0.023
	[0.740]	[0.893]
Overlap condition	0.109	0.053
	[0.774]	[0.728]
Constant	✓	✓
Individual controls	✓	✓
Regional controls	✓	✓
Land fixed eff.	✓	✓
Observations	787	3165
R-squared	0.063	0.020

Robust p-values corrected for multiple hypotheses in square brackets
Corrected p-values significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.27

OLS regression of standardised preferred share of public budget allocated to public housing. The square brackets contain p-values corrected for multiplicity of tested hypotheses (List et al., 2016). Omitted individual controls include: university education, political placement, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany.

Variables	Housing	
	Low income	High income
Neutral condition: baseline		
Immigration condition	0.047	0.035
	[0.891]	[0.914]
Poverty condition	−0.197	−0.026
	[0.398]	[0.947]
Both condition	−0.108	0.039
	[0.894]	[0.922]
Overlap condition	0.013	0.050
	[0.930]	[0.933]
Constant	✓	✓
Individual controls	✓	✓
Regional controls	✓	✓
Land fixed eff.	✓	✓
Observations	787	3165
R-squared	0.046	0.036

Robust p-values corrected for multiple hypotheses in square brackets
Corrected p-values significance: *** p < 0.01, ** p < 0.05, * p < 0.1.

Table C.28

OLS regression of our additional outcome measures for the low income sample. Individual controls include: household income, political placement, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany. Regional controls include GDP and the share of immigrant population.

Low income						
Variables	Poverty	Inequality	Ineq (gov int)	+ schools	+ housing	+ soc prog
Neutral c.: baseline						
Immigration c.	0.110	0.187*	0.169*	−0.247**	0.079	0.012
	(0.100)	(0.111)	(0.094)	(0.097)	(0.095)	(0.095)
Poverty c.	0.047	0.208*	−0.096	−0.192	−0.085	−0.102
	(0.098)	(0.115)	(0.121)	(0.121)	(0.103)	(0.095)
Both c.	0.127	0.328***	0.134	0.033	0.146	0.119
	(0.104)	(0.110)	(0.106)	(0.109)	(0.094)	(0.090)
Overlap c.	0.089	0.144	−0.040	−0.211*	−0.093	−0.153
	(0.106)	(0.115)	(0.112)	(0.114)	(0.113)	(0.105)
Constant	−0.790*	0.197	0.160	0.863**	−0.033	0.280
	(0.427)	(0.357)	(0.380)	(0.396)	(0.400)	(0.381)
Observations	787	787	787	787	787	787
R-squared	0.148	0.119	0.094	0.074	0.099	0.110

Robust standard errors in parentheses
*** p < 0.01, ** p < 0.05, * p < 0.1.

Table C.29

OLS regression of our additional outcome measures for the high income sample. Individual controls include: household income, political placement, employment in high-immigration sectors, age, gender, marital status, voting behaviour in previous election, social media consumption, and whether the respondent was born in Germany. Regional controls include GDP and the share of immigrant population.

High income						
Variables	Poverty	Inequality	Ineq (gov int)	+ schools	+ housing	+ soc prog
Neutral c.: baseline						
Immigration c.	0.013	−0.067	−0.050	0.004	0.025	0.020
	(0.052)	(0.051)	(0.057)	(0.058)	(0.060)	(0.051)
Poverty c.	0.008	−0.063	−0.052	0.034	0.042	0.100*
	(0.055)	(0.053)	(0.056)	(0.053)	(0.054)	(0.055)
Both c.	−0.065	−0.112**	−0.069	0.067	0.099*	0.022
	(0.053)	(0.054)	(0.058)	(0.052)	(0.053)	(0.058)
Overlap c.	−0.023	−0.034	−0.054	0.033	0.050	0.030
	(0.056)	(0.047)	(0.059)	(0.050)	(0.053)	(0.052)
Constant	0.096	0.396**	0.396**	0.537***	0.409**	0.860***
	(0.162)	(0.158)	(0.177)	(0.174)	(0.159)	(0.154)
Observations	3168	3168	3168	3168	3168	3168
R-squared	0.075	0.081	0.086	0.081	0.105	0.114

Robust standard errors in parentheses

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix D. Experimental conditions

We here present screenshots of the experimental condition content. The videos are available from the authors upon request.

Neutral condition (Baseline)

Derzeit leben in Deutschland 82 Millionen Menschen

''82 Million people are currently living in Germany''.

Immigration condition

Derzeit leben in Deutschland 82 Millionen Menschen

13,2 Millionen
Menschen sind
im Ausland geboren

"82 Million people are currently living in Germany.
13.2 Million people were born abroad".

Poverty condition

Derzeit leben in Deutschland 82 Millionen Menschen

13,7 Millionen
Menschen leben
unterhalb der Armutsgrenze*
*60% des Medianeinkommens

"82 Million people are currently living in Germany.
13.7 Million people live below the poverty line*.
*60% of the median income".

Both condition

Derzeit leben in Deutschland 82 Millionen Menschen

13,7 Millionen
Menschen leben
unterhalb der Armutsgrenze*
*60% des Medianeinkommens

13,2 Millionen
Menschen sind
im Ausland geboren

"82 Million people are currently living in Germany.
13.7 Million people live below the poverty line*.
*60% of the median income.
13.2 Million people were born abroad".

The order in which the two poverty and immigration components of this experimental condition were presented was randomised across individuals to control for order effects.

Overlap condition

Derzeit leben in Deutschland 82 Millionen Menschen

13,7 Millionen
Menschen leben
unterhalb der Armutsgrenze*
*60% des Medianeinkommens

13,2 Millionen
Menschen sind
im Ausland geboren

3,2 Millionen
sind
im Ausland geboren
und
leben unterhalb der Armutsgrenze

"82 Million people are currently living in Germany.
13.7 Million people live below the poverty line*.
*60% of the median income.
13.2 Million people were born abroad.
3.2 Million were born abroad and live below the poverty line".

The order in which the two poverty and immigration components of this experimental condition were presented was randomised across individuals to control for order effects.

Appendix E. Full questionnaire in English

1. See Fig. E.1.
2. What is your gender?
   Female; Male

We are a non-partisan group of academic researchers from the Max Planck Society. We would like to know your personal views on some policy matters.

Please note that it is very important for the success of our research that you **answer honestly**, providing your true opinion or the true answer, and r**ead all the questions very carefully** before answering. Any time you don't know an answer, just give your best guess. However, please be sure to spend enough time reading and understanding the question. To ensure the quality of survey data, your responses will be subject to statistical control methods, which can detect incoherent or rushed answers. **Responding without adequate effort or skipping many questions may result in your responses being flagged for low quality and you may not receive your payment.**

It is also very important for the success of our research project that you **complete the entire survey**, once you have started. This survey should take (on average) about 15 minutes to complete.

Notes: Your participation in this study is purely voluntary. Your name will never be recorded by researchers. Results may include summary data, but you will never be identified. The data will be stored on our servers and will be kept confidential. The collected anonymous data may be made available to other researchers for replication purposes.

Yes, I would like to take part in this study.

No, I would not like to participate.

Fig. E.1. First page of the survey (translated to English).

3. Please indicate your age:

4. What is your marital status?
   Single (Never Married/Widowed/Separated/Divorced); Married /Civil partnership/Cohabiting

5. Please indicate how many people live in your household (including yourself): Adults:_ Children:_

6. What was your total monthly household income after tax in 2018? (in Euros) [Please include all your household income sources: salaries, scholarships, pension and Social Security benefits, dividends from shares, income from rental properties, child support and alimony etc. We are not interested in the type of income source, only in the total annual income earned by all the members of your household together.]
   <1000; 1000–1999; 2000–2999; 3000–3999; 4000–4999; 5000–7500; >7500

7. We will now ask your opinion on some important policy questions. Please watch the following video before continuing: *See description of our conditions in Appendix D.*

8. See Fig. E.2.

9. See Fig. E.3.

10. Before proceeding to the next set of questions, we want to ask for your feedback about the responses you provided so far. It is vital to our study that we only include responses from people who devoted their full attention this study. This will not affect in any way the payment you will receive for taking this survey. In your honest opinion, should we use your responses, or should we discard your responses since you did not devote your full attention to the questions so far?
    Yes, I have devoted my full attention to these questions and I think you should use my answers for your research.; No, I have not devoted my full attention to these questions and I think you should not use my answers for your research.

11. Do you think income differences between rich and poor are:
    Not a problem at all; A small problem; A serious problem; A very serious problem

12. To reduce income differences between rich and poor people, the government (at the local, state, or federal level) has the ability and the tools to do:
    Nothing at all; Not much; Some; A lot

13. Some people think that the government (at the local, state, or federal level) should not care about income differences between rich and poor people. Others think that the government should do everything in its power to reduce income inequality.
    Please rate on a scale of 1 to 7 on how you feel about this issue, with 1 being the government should not concern itself with income inequality and 7 being the government should do everything in its power to reduce income inequality.

14. Here are several things that the local, state, or federal government might do to reduce income differences between rich and poor people. Please indicate if you favour or oppose them. Keep in mind that, naturally, to finance an expansion of any of these policies, other types of spending (like spending on infrastructure and defense, for example) would have to be scaled down or taxes would have to be raised.

The government currently raises a certain amount of revenue through the income tax in order to sustain the current level of public spending. In your view, what would be a fair split of the tax burden to sustain public spending?

The average income tax rate* is the percentage of your income that you pay in income tax. For example, if your monthly gross income is €3000 and you pay €300 in income taxes, your average income tax rate is 10%.

Please use the sliders below to tell us how much you think each of the following groups should pay as a percentage of their total income.

While you adjust the four sliders for each group, you will see below the sliders how much of the current revenue you have been able to raise so far. You will receive a warning message as long as you have not raised enough revenue, or if you have raised more money than necessary.
You will only be able to move to the next question when you meet the revenue target exactly.

*If you receive a regular paycheck, the income tax is part of the amount that is automatically taken out. It is different from other amounts that are also automatically taken out, such as the health insurance contribution.
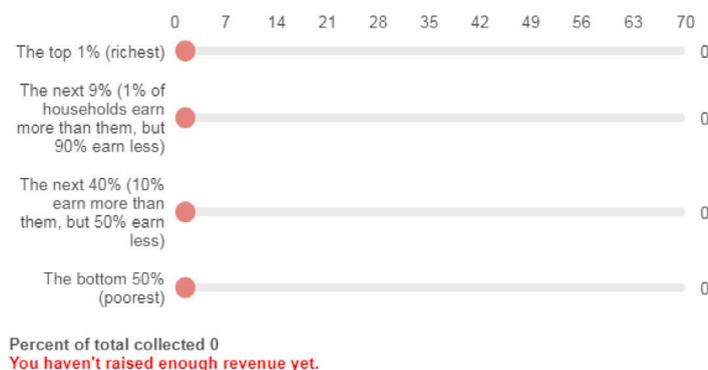

Fig. E.2. Question on preferred tax rates (translated to English).

(a) Would you say that you strongly favour, favour, neither favour nor oppose, oppose or strongly oppose spending more money on schools in poor neighborhoods?
Strongly favour; Favour; Neither favour nor oppose; Oppose; Strongly oppose

(b) Would you say that you strongly favour, favour, neither favour nor oppose, oppose or strongly oppose spending more money to provide decent housing for those who can?t afford it?
Strongly favour; Favour; Neither favour nor oppose; Oppose; Strongly oppose

(c) Would you say that you strongly favour, favour, neither favour nor oppose, oppose or strongly oppose increasing income support programs for the poor?
Strongly favour; Favour; Neither favour nor oppose; Oppose; Strongly oppose

15. How much of the time do you think you can trust our government to do what is right?
Almost always; A lot of the time; Not very often; Almost never

16. Some people say that people get ahead by their own hard work, others say that lucky breaks or help from other people are more important. Which do you think is most important?
Mostly luck; Both equally; Mostly hard work

17. Do you think poverty in today's Germany is:
Not a problem at all; A small problem; A serious problem; A very serious problem

18. What is the highest level of education you have completed?
Less than high school; High school; Realschule ("Mittlere Reife"); Degree from polytechnische Oberschule; Fachhochschulreife; A-levels; Apprenticeship; Specialized school degree ("Fachschulabschluss"); degree from "Fachhochschule"; Bachelor; Master; Diploma/Magister or comparable degree; Doctoral degree

19. What is your current employment status?
Full-time employee, Part-time employee, Self-employed or small business owner, Unemployed and looking for work, Student/apprentice, Retired, Not in labour force

We now ask you how you would like to spend the total government budget, which comprises the local, state and federal government budgets. Suppose that you are the person deciding on the German budget for the next year. You can choose how you want to divide the budget (in percent) between the following 7 categories:

1) **Defense and Internal Security,** which refers to the costs of the Defense department and the costs of supporting security operations in Germany and in foreign countries, and also includes the costs of the police.

2) **Public Infrastructure,** which includes, among others, transport infrastructure like roads, bridges and airports, and water infrastructure.

3) **Spending on Schooling, Higher Education and research,** including help for children from low income families to attend school and university (e.g. via BAfÖG).

4) **Social Security, Disability Insurance and pensions.**

5) **Arbeitslosengeld II and social benefits (Hartz IV).** This covers help to the unemployed (through unemployment insurance) and help for low income families.

6) **Public Spending on Health,** including health insurance and research funding.

7) **Affordable Housing.** This includes subsidies to make housing more affordable for low income families and funds to build and manage public housing.

Defense and National Security	0
Public Infrastructure	0
Schooling	0
Social security	0
Arbeitslosengeld II and social benefits (Hartz IV)	0
Health	0
Affordable Housing	0
Total	0

Fig. E.3. Question on preferred government spending (translated to English).

20. *(If respondent is working)* Do you work in one of the following sectors?
    Transport, logistics, protection and security; Commodity production and manufacturing; Commercial services, trade, sales, hotels and tourism; Construction, architecture, surveying and mapping, facility technology; None of these
21. Where were you born? *(Select from list of countries.)*
22. *(If respondent was born in Germany)* Were both of your parents born in Germany? *Yes; No*
23. Please choose your state and region (Landkreis):
24. Which media do you most frequently get information on world happenings from? Check each that apply.
    TV News; Social media (social networks, blogs); Radio/podcasts; Online newspaper/Newspaper App; Print newspaper; I don't follow the news.
25. Where do you see yourself on the political spectrum, where 1 represents the left and 10 represents the right? *(Select 1–10)*
26. Did you vote in the previous *(Bundestag)* elections? *Yes; No*
27. Which party would you vote for if there were elections on Sunday?
    CDU/CSU; SPD; FDP; Bündnis 90/Die Grünen; Die Linke; AfD; Andere
28. According to your best estimate, how many of the 82 Million people living in Germany at the moment were born abroad? _ *million people*
29. Of those people living in Germany and born abroad, do you think the majority were born inside or outside of the EU? *Inside the EU; Outside of the EU*
30. According to your best estimate, how many of the 82 Million people in Germany live below the poverty line? _ *million people*
31. According to your best estimate, how many of the people living below the poverty line* in Germany at the moment were born abroad? (*The poverty line is a measure of relative poverty and is computed as 60% of median income.) _ *million people*

32. Do you think the majority of the people living below the poverty line in Germany at the moment and born abroad were born inside or outside of the EU? *Inside the EU; Outside of the EU*
33. *(If respondent is working)* How secure do you think your current job is?
   Very secure; Relatively secure; Not very secure; Not secure at all

References

Alesina, A., Glaeser, E., Glaeser, E.L., 2004. Fighting Poverty in the US and Europe: A World of Difference. Oxford University Press.

Alesina, A., Glaeser, E., Sacerdote, B., 2001. Why doesn't the United States have a European-style welfare state? Brook. Pap. Econ. Act. 187–278.

Alesina, A., La Ferrara, E., 2005. Ethnic diversity and economic performance. J. Econ. Lit. 43 (3), 762–800.

Alesina, A., Miano, A., Stantcheva, S., 2018a. Immigration and Redistribution. Working Paper 24733, National Bureau of Economic Research.

Alesina, A., Murard, E., Rapoport, H., 2018b. Immigration and Preferences for Redistribution in Europe. Working Paper.

Alesina, A.F., Stantcheva, S., 2020. Diversity, Immigration, and Redistribution. NBER Working Paper W26620.

An, W., Ye, M., 2017. Mind the gap: Disparity in redistributive preference between political elites and the public in China. Eur. J. Political Econ. 50, 75–91.

Andreoli, F., Olivera, J., 2020. Preferences for redistribution and exposure to tax-benefit schemes in Europe. Eur. J. Political Econ. 63, 101880.

Auspurg, K., Brüderl, J., Wöhler, T., 2019. Does immigration reduce the support for welfare spending? A cautionary tale on spatial panel data analysis. Am. Sociol. Rev. (Forthcoming).

Barrera Rodriguez, O.D., Guriev, S.M., Henry, E., Zhuravskaya, E., 2017. Facts, alternative facts, and fact checking in times of post-truth politics. SSRN Electron. J..

Barsbai, T., Licuanan, V., Steinmayr, A., Tiongson, E., Yang, D., 2020. Information and the Formation of Social Networks. NBER Working Paper 27346.

Barton, J., Pan, X., 2021. Movin' on up? A survey experiment on mobility enhancing policies. Eur. J. Political Econ..

Becker, S.O., Fetzer, T., 2016. Does Migration Cause Extreme Voting? Technical report.

Bellucci, D., Conzo, P., Zotti, R., 2019. Perceived immigration and voting behavior. In: ESt Working Paper Series 15/19.

Biber, U., Stegmann, M., 2019. Auslaendische versicherte und rentenbezieher in der gesetzlichen rentenversicherung. In: Deutsche Rentenversicherung April 2019 (German Only).

Bisin, A., Zanella, G., 2017. Time-consistent immigration policy under economic and cultural externalities. Econ. Policy 32 (91), 415–446.

Borjas, G.J., 2003. The labor demand curve is downward sloping: Reexamining the impact of immigration on the labor market. Q. J. Econ. 118 (4), 1335–1374.

Bussolo, M., Ferrer-i Carbonell, A., Giolbas, A., Torre, I., 2021. I perceive therefore I demand: The formation of inequality perceptions and demand for redistribution. Rev. Income Wealth 67 (4), 835–871, _eprint: https://onlinelibrary.wiley.com/doi/pdf/10.1111/roiw.12497.

Cappelen, C., Midtbø, T., 2016. Intra-EU labour migration and support for the norwegian welfare state. Eur. Sociol. Rev. 32 (6), 691–703.

Cavaillé, C., Trump, K.-S., 2015. The two facets of social policy preferences. J. Politics 77 (1), 146–160, Publisher: The University of Chicago Press.

Coester, C., 2018. Rise of the right: Europe's left turns right on immigration. Handelsblatt Today.

Côté, S., House, J., Willer, R., 2015. High economic inequality leads higher-income individuals to be less generous. Proc. Natl. Acad. Sci. 112 (52), 15838–15843.

Cruces, G., Perez-Truglia, R., Tetaz, M., 2013. Biased perceptions of income distribution and preferences for redistribution: Evidence from a survey experiment. J. Public Econ. 98, 100–112.

Dahlberg, M., Edmark, K., Lundqvist, H., 2012. Ethnic diversity and preferences for redistribution. J. Polit. Econ. 120 (1), 41–76.

Daniele, G., Martinangeli, A.F., Passarelli, F., Sas, W., Windsteiger, L., 2020. Fear and Loathing in Times of Distress - Causal Impact of Social and Economic Insecurity on Anti-Immigration Sentiment. Working Paper of the Max Planck Institute for Tax Law and Public Finance No. 2020-17.

de Quidt, J., Johannes, H., Christopher, R., 2018. Measuring and bounding experimenter demand. Amer. Econ. Rev. 108 (11), 64.

Dewan, T., Wolton, S., 2019. A political economy of social discrimination. Available at SSRN.

Dustmann, C., Frattini, T., Lanzara, G., 2012. Educational achievement of second-generation immigrants: an international comparison*. Econ. Policy 27 (69), 143–185.

Dustmann, C., Vasiljeva, K., Piil Damm, A., 2018. Refugee migration and electoral outcomes. Rev. Econom. Stud. 86 (5), 2035–2091.

Edo, A., Giesing, Y., Öztunc, J., Poutvaara, P., 2019. Immigration and electoral support for the far-left and the far-right. Eur. Econ. Rev. 115, 99–143.

Eger, M.A., 2010. Even in Sweden: The effect of immigration on support for welfare state spending. Eur. Sociol. Rev. 26 (2), 203–217.

Elsner, B., Concannon, J., 2020. Immigration and Redistribution. SSRN Scholarly Paper 3691389, Social Science Research Network, Rochester, NY.

Ember, S., 2019. Bernie sanders, once the progressive outlier, joins a crowded presidential field. N.Y. Times.

Epley, N., Gilovich, T., 2006. The anchoring-and-adjustment heuristic: Why the adjustments are insufficient. Psychol. Sci. 17 (4), 311–318, Publisher: SAGE Publications Inc.

Facchini, G., Margalit, Y., Nakata, H., 2016. Countering Public Opposition to Immigration: The Impact of Information Campaigns. IZA Discussion Papers Series No. 10420.

Facchini, G., Mayda, A.M., 2009. Does the welfare state affect individual attitudes toward immigrants? Evidence across countries. Rev. Econ. Stat. 91 (2), 295–314.

Fong, C., 2001. Social preferences, self-interest, and the demand for redistribution. J. Public Econ. 82 (2), 225–246.

Fuster, A., Zafar, B., 2022. Survey Experiments on Economic Expectations. Technical Report w29750, National Bureau of Economic Research, Cambridge, MA, p. w29750.

Geißler, R., Weber-Menges, S., 2010. Bildungsungleichheit–eine deutsche altlast. Die bildungssoziologische perspektive. In: Handbuch Bildungsfinanzierung. Springer, pp. 155–165.

Gilens, M., 1995. Racial attitudes and opposition to welfare. J. Politics 57 (4), 994–1014.

Gonnot, J., 2022. Taxation with representation: Understanding natives' attitudes to foreigners' voting rights. Eur. J. Political Econ. 71, 102060.

Grigorieff, A., Roth, C., Ubfal, D., 2016. Does Information Change Attitudes Towards Immigrants? Representative Evidence from Survey Experiments. Technical report, IZA Discussion Papers.

Haaland, I., Roth, C., 2020. Labor market concerns and support for immigration. J. Public Econ. 191, 104256.

Haaland, I., Roth, C., Wohlfart, J., 2020. Designing information provision experiments.

Hainmueller, J., Hiscox, M.J., 2010. Attitudes toward highly skilled and low-skilled immigration: Evidence from a survey experiment. Am. Political Sci. Rev. 104 (1), 61–84.

Hainmueller, J., Hopkins, D.J., 2015. The hidden American immigration consensus: A conjoint analysis of attitudes toward immigration. Am. J. Political Sci. 59 (3), 529–548.

Halla, M., Wagner, A.F., Zweimüller, J., 2017. Immigration and voting for the far right. J. Eur. Econom. Assoc. 15 (6), 1341–1385.

Harms, P., Landwehr, C., 2020. Is money where the fun ends? Material interests and individuals' preference for direct democracy. Eur. J. Political Econ. 61, 101818.

Hatton, T.J., 2016. Immigration, public opinion and the recession in Europe. Econ. Policy 31 (86), 205–246, Publisher: Oxford Academic.

Hatton, T.J., 2017. Refugees and asylum seekers, the crisis in Europe and the future of policy. Econ. Policy 32 (91), 447–496.

Iturbe-Ormaetxe, I., Romero, J.G., 2016. Financing public goods and attitudes toward immigration. Eur. J. Political Econ. 44, 159–178.

Jenkins, S., 2018. From Sweden to Brexit, immigration is the issue dividing Europe | Simon Jenkins. Guardian.

Karadja, M., Mollerstrom, J., Seim, D., 2017. Richer (and holier) than thou? The effect of relative income improvements on demand for redistribution. Rev. Econ. Stat. 99 (2), 201–212.

Kim, D.G., 2019. Positional concern and low demand for redistribution of the poor. Eur. J. Political Econ. 56, 27–38.

Koehler, D.J., Harvey, N., 2008. Blackwell Handbook of Judgment and Decision Making. John Wiley & Sons, Google-Books-ID: s73eYl1DRHUC.

Krüger, H.-H., Kramer, R.-T., Budde, J., Rabe-Kleberg, U., 2010. Bildungsungleichheit Revisited: Bildung und Soziale Ungleichheit vom Kindergarten bis zur Hochschule, Vol. 30. Springer-Verlag.

Kuziemko, I., Buell, R.W., Reich, T., Norton, M.I., 2014. "Last-place aversion": Evidence and redistributive implications. Q. J. Econ. 129 (1), 105–149.

Kuziemko, I., Norton, M.I., Saez, E., Stantcheva, S., 2015. How elastic are preferences for redistribution? Evidence from randomized survey experiments. Amer. Econ. Rev. 105 (4), 1478–1508.

Lamble, L., 2018. Jeremy Corbyn puts new focus on inequality but the old challenges loom. Guardian.

Larsen, C.A., 2011. Ethnic heterogeneity and public support for welfare: Is the American experience replicated in Britain, Sweden and Denmark? Scand. Political Stud. 34 (4), 332–353.

Lee, W., Roemer, J.E., 2006. Racism and redistribution in the United States: A solution to the problem of American exceptionalism. J. Public Econ. 90 (6), 1027–1052.

List, J.A., Shaikh, A.M., Xu, Y., 2016. Multiple Hypothesis Testing in Experimental Economics. NBER Working Paper Series.

Luttmer, E.F.P., 2001. Group loyalty and the taste for redistribution. J. Polit. Econ. 109 (3), 500–528.

Magni-Berton, R., 2014. Immigration, redistribution, and universal suffrage. Public Choice 160 (3), 391–409.

Martinangeli, A.F.M., Windsteiger, L., 2021. Last word not yet spoken: a reinvestigation of last place aversion with aversion to rank reversals. Exp. Econ. 24 (3), 800–820.

Mayr, K., 2007. Immigration and income redistribution: A political economy analysis. Public Choice 131 (1), 101–116.

Mochon, D., Frederick, S., 2013. Anchoring in sequential judgments. Organ. Behav. Human Decis. Process. 122 (1), 69–79.

Mummolo, J., Peterson, E., 2019. Demand effects in survey experiments: An empirical assessment. Am. Political Sci. Rev. 113 (2), 517–529.

Naumann, E., Stoetzer, L.F., 2018. Immigration and support for redistribution: survey experiments in three European countries. West Eur. Politics 41 (1), 80–101.

Naumann, E., Stoetzer, L., Pietrantuono, G., 2018. Attitudes towards highly skilled and low-skilled immigration in Europe: A survey experiment in 15 European countries. Eur. J. Political Res. 57 (4), 1009–1030.

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.

OECD, 2018. The Resilience of Students with an Immigrant Background. p. 296.

Roemer, J.E., Lee, W., van der Straeten, K., 2007. Racism, Xenophobia, and Distribution: Multi-Issue Politics in Advanced Democracies. Harvard University Press.

Rydgren, J., 2018. The Oxford Handbook of the Radical Right. Oxford University Press.

Sands, M.L., 2017. Exposure to inequality affects support for redistribution. Proc. Natl. Acad. Sci. 114 (4), 663–668.

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.

Schmidt-Catran, A.W., Spies, D.C., 2016. Immigration and welfare support in Germany. Am. Sociol. Rev. 81 (2), 242–261.

Senik, C., Stichnoth, H., Van der Straeten, K., 2009. Immigration and natives and attitudes towards the welfare state: Evidence from the European social survey. Soc. Indic. Res. 91 (3), 345–370.

Speciale, B., 2012. Does immigration affect public education expenditures? Quasi-experimental evidence. J. Public Econ. 96 (9–10), 773–783.

Stantcheva, S., 2021. Inequalities in the times of a pandemic. Econ. Policy.

Steinmayr, A., 2020. Contact versus exposure: Refugee presence and voting for the far-right. Rev. Econ. Stat. 1–47, Publisher: MIT Press.

Tanaka, R., Farre, L., Ortega, F., 2018. Immigration, assimilation, and the future of public education. Eur. J. Political Econ. 52, 141–165.

Tversky, A., Kahneman, D., 1974. Judgment under uncertainty: Heuristics and biases. Science 185 (4157), 1124–1131, Publisher: American Association for the Advancement of Science.

Windsteiger, L., 2022. The redistributive consequences of segregation and misperceptions. Eur. Econ. Rev. 144, 104073.

Zizzo, D.J., 2010. Experimenter demand effects in economic experiments. Exp. Econ. 13 (1), 75–98.