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# Deliberating Inequality in a Field Experiment

Franziska Disslbacher
Martin Haselmayer
Lukas Lehner
Severin Rapp
Franziska Windisch

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Franziska Disslbacher*    Martin Haselmayer†    Lukas Lehner‡    Severin Rapp§

Franziska Windisch¶

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## Abstract

This paper investigates how participation in a citizens' assembly affects individuals' redistributive preferences and (perceived) role in democracy. We implement a pre-registered field experiment embedded in a real-world citizens' assembly on wealth inequality in Austria. Using a three-group-design comparing assembly participants, non-selected volunteers, and a population sample, we isolate the causal effects of taking part in a citizens' assembly from self-selection into participation. We find that while participating in the citizens' assembly substantially improves factual knowledge about the wealth distribution and promotes convergence around specific tax policy proposals, notably a EUR 1 million allowance, it has no measurable effect on political efficacy or broader civic engagement. We also document significant political self-selection: individuals willing to participate in the citizens' assembly were already more engaged and supportive of redistribution than the general population. These findings suggest that while deliberative formats can foster informed convergence on policy proposals, their ability to mobilize broader publics is limited – especially if they primarily engage the already supportive and, as in this case, lack institutional anchoring that might facilitate spillover into more institutionalized political arenas.

*Keywords*: Deliberative Democracy, Polarization, Inequality, Redistribution Preferences, Field Experiment, Political Participation, Wealth taxation

*JEL codes:* C93, D31, D63, D71, D72, D73, D83

*Vienna University of Economics and Business. franziska.disslbacher@wu.ac.at.

†University of Applied Sciences Campus Vienna/FORESIGHT/University of Vienna. martin.haselmayer@hcw.ac.at.

‡University of Edinburgh. lukas.lehner@ed.ac.uk.

§Stone Center on Socio-Economic Inequality, Graduate Center at the City University of New York. srapp@gc.cuny.edu.

¶University of Vienna. franziska.windisch@univie.ac.at.

# 1 Introduction

Despite a marked rise in wealth inequality in many advanced economies since the 1980s, advanced capitalist democracies have not responded with stronger efforts to tax the rich (Lierse, 2022; Scheuer and Slemrod, 2021). Although public concern around inequality is widespread (Margalit and Raviv, 2024), it rarely translates into broad political mobilization behind the reintroduction or expansion of wealth taxation (Genschel, Limberg, and Seelkopf, 2023; Schechtl and Tisch, 2023). While affluent groups often lobby effectively against redistributive policies (Emmenegger and Marx, 2019; Klitgaard and Paster, 2021), their non-affluent counterparts remain less engaged (Gallego, 2007), less informed about taxation (Stantcheva, 2021) and less likely to articulate preferences (Elkjær et al., 2025). This political asymmetry raises a fundamental puzzle: how can democratic systems address participatory inequalities and foster more inclusive engagement with complex redistributive policies?

Drawing on a field experiment in a real-world citizens' assembly on wealth inequality and taxation, we assess the potential of deliberative innovations to address these challenges. Our novel three-group design disentangles the causal effects of participating in a citizens' assembly from patterns of self-selection, allowing us to examine both its reach – *who* participates – and its consequences – *whether* participants emerge as confident, politically engaged individuals in support of redistribution. In doing so, we provide the first causal evidence on how such assemblies shape redistributive preferences.

Citizens' assemblies[1] have proliferated globally in recent years (Dryzek et al., 2019; OECD, 2020). They convene randomly selected members of the public to deliberate on complex issues, typically guided by expert input and structured discussion (Smith and Setälä, 2018). Their appeal lies in the promise of empowering ordinary citizens and improving representation in policy debates (Warren, 2008). Thereby, they seek to respond to widespread dissatisfaction with present-day politics (Van Dijk, Werner, and Marien, 2024), especially among those who feel excluded or disillusioned (Pilet et al., 2023). Yet, as participation is voluntary, it often follows patterns of education, political interest, and class (Jacquet, 2017a; Hansen and Andersen, 2004; Grönlund, Setälä, and Herne, 2010), raising doubts about whether such forums can overcome underlying inequalities in political engagement and representation. Deliberative theory nevertheless emphasizes the transformative potential of participation by increasing political knowledge (Barabas, 2004; Grönlund, Setälä, and Herne, 2010), strengthening political efficacy (Gastil, 2000; Morrell, 2005; Pateman, 1970), fostering civic engagement (Carpini, Cook, and Jacobs, 2004; Boulianne, 2019),

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[1]We use the terms "citizens' assembly","citizens' forum", and "mini-public" interchangeably.

shifting policy preferences (Barabas, 2004), and reducing polarization (Fishkin et al., 2021). Yet despite their global spread, causal evidence on citizens' assemblies remains limited. Most existing studies focus on other deliberative formats, such as juries or deliberative polls, and rely on non-experimental or indirect designs that cannot adequately address selection bias.[2]

We embed this design in a pre-registered field experiment conducted within the Good Council for Redistribution, a privately organized citizens' assembly in Austria in 2024. The council convened 50 participants through a state-of-the-art selection algorithm and tasked them with deliberating on wealth inequality and allocating a EUR 25 million endowment. It included expert input, structured discussion and decision-making authority. Efforts were made to lower participation barriers, including financial compensation, on-site interpretation, and childcare. These features make it a compelling case for studying both the reach and the effects of participating in a deliberative mini-public.

**Research design** We assign participation to the citizen assembly using a selection algorithm designed to maximize the competing objectives of demographic representativeness and equal selection probability among people willing to participate (Flanigan et al., 2021b). To disentangle treatment effects from selection, we exploit three comparisons: between randomized participants and non-selected volunteers, between volunteers and a separate population sample, and between participants and the population sample. We collected survey data at three points in times: before the assembly, immediately after, and eight months later. This design and the resulting panel structure allow us to estimate the causal effects of deliberative participation using random assignment, while also comparing participants to the general population to separate selection effects. This is an important advancement for understanding both the effects of deliberation and the broader challenges of the representativeness of deliberative events.

**Main findings** We find that participation significantly increased factual knowledge about wealth inequality and fostered convergence to specific (wealth and inheritance) tax policy designs[3], yet it did not enhance political efficacy, broader civic engagement, or redistributive support (on average). We also document strong selection effects. Individuals interested in participating were more politically engaged and more

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[2]Boulianne (2019) uses a pre-post panel design comparing participants in a citizens' assembly with a representative population sample over time to assess the impact on political efficacy. Wappenhans et al. (2024) employ a field experiment with a large randomly selected treatment group and a broadly matched control group, assessing change in political efficacy and participation outcomes before and after the assembly.

[3]While participants already expressed high levels of support for redistribution before the assembly, deliberation reinforced perceptions that wealth was excessively concentrated and produced broad agreement around key tax design parameters, notably a EUR 1 million tax allowance.

supportive of redistribution than the general population. These results highlight both the potential and the limitations of deliberative processes for shaping redistributive preferences and strengthening democratic engagement.

**Contributions**  This study extends prior research by providing causal evidence on who participates in citizens' assemblies and how participating affects council members' (perceived) role in democracy and their redistributive preferences. Our study makes four key contributions:

First, our three-group experimental design provides strong causal leverage on the individual-level consequences of deliberative participation. By explicitly separating treatment effects from patterns of self-selection, the design addresses long-recognized concerns about the inclusiveness of democratic innovations (Hansen and Andersen, 2004; Luskin, Fishkin, and Jowell, 2002; Grönlund, Setälä, and Herne, 2010; French and Laver, 2009). On a broader scale, the framework can inform the evaluation of various democratic innovations, such as deliberative mini-publics, participatory budgeting and deliberative polling. It can also be applied to field experiments in which selection processes influence observed outcomes.

Second, building on this framework, we demonstrate that demographic representativeness alone does not overcome political self-selection. Despite achieving high levels of sociodemographic representativeness through a state-of-the-art selection algorithm (Flanigan et al., 2021a), participants were more politically engaged and more supportive of the deliberated issue on than the general population. This finding complicates normative claims that sortition amplifies excluded voices (French and Laver, 2009). Demographic quotas, even when carefully implemented (Farrell and Stone, 2020), cannot ensure attitudinal inclusiveness or equal voice in democratic decision-making on their own. Consequently, citizens' assemblies may consolidate support among the already engaged rather than mobilize disengaged constituencies.

Third, we show that structured deliberation (as opposed to passive information provision) deepens knowledge about wealth inequality and fosters an informed consensus on redistributive policy design. This finding highlights the potential of deliberative participation on the formation of policy preferences: in contexts where individuals have little knowledge or inconsistent views, deliberation can enable learning, belief updating, and more coherent attitudes (Barabas, 2004; Zhang, 2019; Muradova, 2021). The promise is especially relevant in the domain of inequality and tax policy, where misperceptions are widespread (Norton and Ariely, 2011; Gimpelson and Treisman, 2018; Stantcheva, 2021) and preferences for concrete policies often remain vague, ambivalent, constrained by fairness norms or fractured by self-interest (Fisman, Jakiela, and Kariv, 2017; Weinzierl, 2014; Perez-Truglia and Yusof, 2024; Schechtl and Tisch, 2023).

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Whereas prior studies of information provision about inequality and taxation often find limited effects (Elkjær et al., 2025; Fisman, Jakiela, and Kariv, 2017; Weinzierl, 2014; Perez-Truglia and Yusof, 2024; Albacete, Fessler, and Lindner, 2025; Alesina, Stantcheva, and Teso, 2018), we show how deliberative contexts can activate information effects that remain muted in isolation.

Fourth, we contribute to existing literature on the democratic effects of deliberative participation. Deliberative theory has long argued that participation in structured political dialogue can increase both internal efficacy (the belief in ones own political competence) and external efficacy (the belief that ones voice matters in the political system), and, consequently, translate into higher levels of political engagement (Pateman, 1970; Gastil, 2000). Recent evidence from citizens assemblies with a formal mandate points more clearly to positive effects (Boulianne, 2019; Wappenhans et al., 2024). By contrast, our findings show that privately organized assemblies without institutional ties struggle to broaden political agency. This underscores an important boundary condition for deliberative democratic theory and highlights the role of institutional design in shaping the reach and impact of democratic innovations.

Taken together, these contributions speak to central debates about participation, representation, democratic responsiveness, and policy preference formation in and beyond the context of redistributive politics. Through methodological innovation and new empirical evidence, our study shows both the potential and the limits of citizens' assemblies in addressing inequalities of voice and influence and establishing consensus in contested policy debates. More broadly, it underscores how institutional design conditions the reach and effects of participatory formats, and provides causal evidence that advances deliberative theory and informs practice on democratic innovation.

## 2 Theorizing the Mobilizing Effects of Citizens' Assemblies

Two key barriers help explain why constituencies for progressive tax policies often remain weak. First, political engagement is systematically stratified by socioeconomic status (Verba, Schlozman, and Brady, 1995). Non-affluent individuals, who are expected to benefit most from progressive taxation, are less likely to participate in both institutionalized forms of participation and non-conventional modes (Gallego, 2007; Dalton, 2017; Marien, Hooghe, and Quintelier, 2010). Macro-level evidence further shows that rising economic inequality itself depresses political engagement, particularly among less affluent groups (Solt, 2008). Second, public understanding of economic inequality and taxation is often limited, especially among

non-affluent groups. Many underestimate the extent of wealth concentration (Norton and Ariely, 2011), misunderstand tax systems (Kuziemko et al., 2015; Sides, 2016), and struggle to link inequality to specific policy instruments (Scheve and Stasavage, 2017; Margalit and Raviv, 2024). Recent work further shows that less affluent individuals are not only less informed about taxation (Stantcheva, 2021; Elkjær et al., 2025), but also more likely to opt out of expressing opinions on tax-related questions altogether (Elkjær and Wlezien, 2024). Taken together, these patterns contribute to a broader disconnect: even where concern about inequality is widespread, the ability and willingness to act on that concern remain uneven.

Deliberative formats such as citizens' assemblies have been proposed as institutional responses to these challenges, explicitly designed to lower participation barriers through sortition procedures and reduce informational asymmetries through structured deliberation. They offer participants a direct experience of political dialogue and decision-making, potentially enhancing political efficacy and reactivating disengaged citizens (Pateman, 1970; Gastil, 2000; Carpini, Cook, and Jacobs, 2004). At the same time, they combine structured discussion with exposure to expert information, offering a sustained opportunity to learn about complex topics, reflect on competing arguments, and articulate more informed and considered preferences (Barabas, 2004; Luskin, Fishkin, and Jowell, 2002; Zhang, 2019; Muradova, 2021).

This suggests that citizens' assemblies may help overcome participatory and informational barriers. We examine whether these promises translate into practice by testing whether participation in a real-world citizens' assembly on wealth inequality fosters political efficacy and engagement and strengthens knowledge of and support for progressive taxation.[4]

## Political Efficacy and Political Participation

Deliberative theory suggests that deliberative participation can strengthen both internal efficacy – defined as the belief in one's own political competence – and external efficacy – defined as the belief that one's voice matters in the political system – which are key drivers of political participation (Dalton, 2017). Assemblies are expected to foster political engagement by offering an experience of meaningful political agency (Pateman, 1970; Gastil, 2000). However, empirical findings on these effects are mixed. Some studies demonstrate increases in efficacy and participation following deliberative events, such as deliberative polls and juries (Fishkin and Luskin, 1999; Grönlund, Setälä, and Herne, 2010; Gastil et al., 2008; Fishkin et al., 2024), while others report more limited effects (Morrell, 2005; Andersen and Hansen, 2007; Munno

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[4]The hypotheses were pre-registered under https://www.socialscienceregistry.org/trials/13874.

and Nabatchi, 2014). Empirical evidence specifically on citizens' assemblies is more consistent: recent studies document moderate increases in both political efficacy and political engagement (Boulianne, 2019; Wappenhans et al., 2024). Against this backdrop, we expect deliberative participation to enhance both internal and external political efficacy and to encourage greater political engagement.

### Redistributive Knowledge and Redistributive Preferences

Participation in deliberative events is expected to influence how individuals assess the policy issues they deliberate on. By combining expert input with peer discussion, citizens' assemblies should help participants to acquire knowledge, form more consistent attitudes, and update their beliefs, especially among those with low prior information and weakly held beliefs (Barabas, 2004; Zhang, 2019; Muradova, 2021). Previous research shows that deliberation increases factual knowledge (Luskin, Fishkin, and Jowell, 2002; Grönlund, Setälä, and Herne, 2010; Suiter et al., 2020), can shift attitudes (Luskin, Fishkin, and Jowell, 2002; Himmelroos and Christensen, 2014) and even foster opinion convergence (Fishkin et al., 2021).

A large body of research highlights widespread misperceptions about inequality and limited understanding of tax systems (Norton and Ariely, 2011; Gimpelson and Treisman, 2018; Stantcheva, 2021). Although concern about inequality is widespread, preferences for specific policy instruments remain ambivalent (Margalit and Raviv, 2024; Elkjær et al., 2025). In response to this, numerous studies have examined whether providing information alters redistributive preferences. However, survey experiments relying on information alone find only limited effects on support for redistributive policies (Kuziemko et al., 2015; Sides, 2016; Fisman et al., 2020; Weinzierl, 2014; Perez-Truglia and Yusof, 2024). Deliberation may offer a more durable alternative because embedding information in structured discussion could activate effects that remain muted when information is provided in isolation. Building on this, we expect deliberative participation to increase knowledge about wealth redistribution. At the same time, while overall support for redistribution may remain largely unchanged, deliberative participation should strengthen support for specific policies such as wealth and inheritance taxation and foster convergence around these proposals.

## 3    The Good Council

The citizens' assembly *Good Council for Redistribution* convened 50 Austrian residents over six weekends to deliberate on wealth inequality and redistribution. Austria exhibits high and increasing levels of wealth

concentration alongside low levels of property taxation. As shown in Figure 1, Austria has one of the highest shares of wealth held by the top 1% in Europe, reaching approximately 30% by 2020.[5] At the same time, Austria collects exceptionally low revenue from property taxation, consistently below 1% of GDP. It is currently one of only a few OECD countries that neither levies inheritance taxes nor net wealth taxes.

Figure 1: Wealth Inequality and Taxation Across Advanced Economies
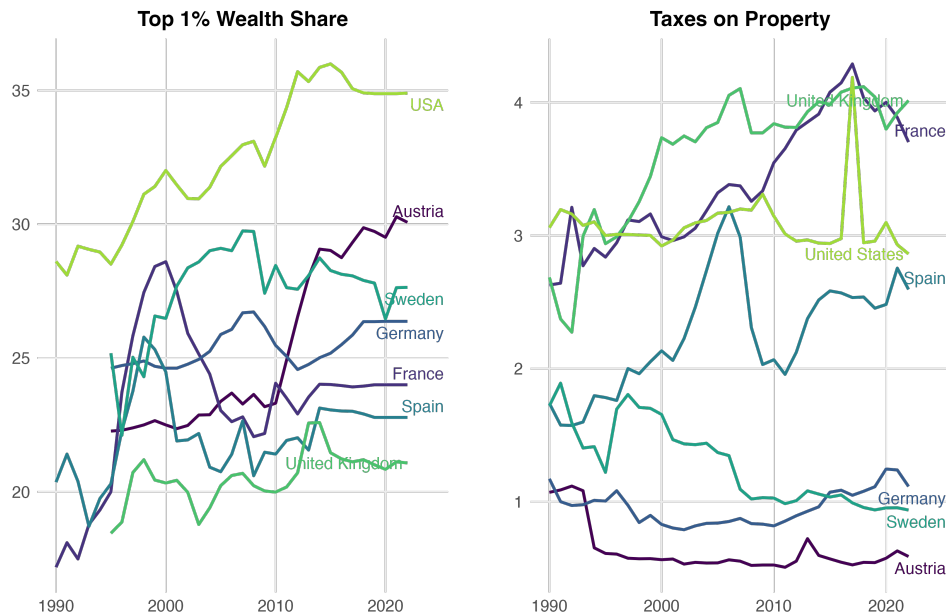
In response to what she saw as the government's inadequate efforts to tackle wealth inequality, Austrian heiress and activist Marlene Engelhorn initiated the citizens' assembly. The council had two main objectives: to foster public debate on the distribution of wealth and to allocate an endowment of EUR 25 million based on the outcomes of the deliberative process. Between March and June 2024, the council met for six weekend sessions. During these meetings, participants received information on inequality and tax policy from scholars, NGOs, and practitioners, and engaged in facilitated discussions to identify priorities and develop concrete proposals for allocating the endowment. Because the funds were held in a legally binding trust, participants were guaranteed that their decisions would be implemented. This distinguishes

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[5]Alternative estimates suggest that the actual share is considerably higher, likely between 40% and 50% (Kennickell, Lindner, and Schürz, 2022; Disslbacher et al., 2023)

the Good Council from most deliberative processes, whose recommendations remain subject to political discretion (Bardhi and Bobkova, 2023). At the same time, a strong democratic mandate and institutional anchoring are often considered important for the broader policy impact and perceived legitimacy of mini-publics (Goldberg and Bächtiger, 2023; Courant, 2022). The Good Council, however, was organized as a private initiative and therefore lacked such formal ties to political institutions.

Table 1: Good Council Overview

| | |
|---|---|
| **Initiator** | Privately organized |
| **Topic** | Wealth distribution and redistribution |
| **Objectives** | 1. To promote dialogue on wealth distribution |
| | 2. To allocate 25 million euros |
| **Selection** | 1. Random selection of 10,000 Austrian residents aged 16+ |
| | 2. Statistical selection of 50 members and 15 alternates |
| | (Flanigan et al., 2021a) |
| **Duration** | March 16 - June 9, 2024 (6 weekends) |

# 4 Research Design

This section outlines our research design. We begin by describing our data sources and sample construction in Subsection 4.1. Subsection 4.3 defines the key outcome variables used in our analysis. Subsection 4.4 discusses our identification strategy, detailing the assumptions necessary for causal interpretation. Finally, Subsection 4.5 explains our approach to statistical inference.

## 4.1 Sample and Data

Selection into the Good Council is based on a random sample of Austrian residents detailed in Appendix Section C. Selection into the Good Council followed a two-stage process in line with established standards for participatory projects (Curato et al., 2021; OECD, 2020). First, 10,000 randomly selected Austrian residents aged 16 and above were invited. From those who responded, 50 members and 15 alternates were selected using the LEXIMIN algorithm (Flanigan et al., 2021a), which maximizes sociodemographic representativeness while ensuring fair inclusion probabilities. The council made strong efforts to mini-

mize barriers to participation: members received EUR 1,200 per weekend in compensation, well above standard rates in comparable initiatives (Carson and Dienel, 2020), and were provided with on-site interpretation and childcare services. Despite initial biases in registration, the final composition of the council closely matched the Austrian population across 35 sociodemographic and attitudinal categories (Haselmayer, 2025).

Our main study sample consists of two groups: participants selected to take part in the Good Council (treatment group) and individuals who registered their interest but were not selected and consented to participate in our study (control group). [6] This yields 50 treated individuals and 305 control individuals, including 13 substitute members selected to step in and replace participants if needed during the first three meetings.[7]

All outcome data were collected through computer-assisted web interviews (CAWI) conducted before and after the citizens' assembly. In addition, we administered a follow-up survey eight months after the Good Council's final event. Table D1 in the Appendix provides detailed results on survey participation rates for both members and the control group across waves. To account for variation in response rates between surveys, the analysis includes only those who participated in the pre-council and the post-council wave (n=241, 38 individuals in the treatment group and 188 individuals in the control group). Beyond surveying the treatment and control group members, we conducted a representative population survey (n=1,002) of Austrian residents aged 16 and above. This survey was fielded between June 21 and July 22, 2024, immediately following the Good Council's final event on June 18, 2024. The survey employed a mixed-mode design combining computer-assisted web interviews (CAWI, 60%) and computer-assisted telephone interviews (CATI, 40%).

Our main analysis compares the treatment and control group using data from the post-council wave collected immediately after the assembly, and contrasts them with the representative population sample surveyed during the same period. As explained in Section 4.4, this design allows us to assess both the effects of participation and the extent of self-selection into deliberation. We further exploit the panel structure of our data in two ways: first, by implementing a difference-in-differences design comparing changes from the pre- to the post-council wave between treatment and control groups as an alternative identification strategy presented in Section F in the Appendix; and second, by examining outcomes in a follow-up wave conducted eight months later to assess longer-term effects presented in Section H in the

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[6]10,000 individuals with Austrian residence were invited to participate, out of those 1,374 individuals registered their interest to participate in the Good Council but were not selected and 305 among those consented to participate in our study (control group).

[7]Out of 15 initial replacement members, one stepped up to replace a member after the first weekend.

Appendix. The main variables are detailed in Section J in the Appendix.

Figure 2: Timeline of the Field Experiment
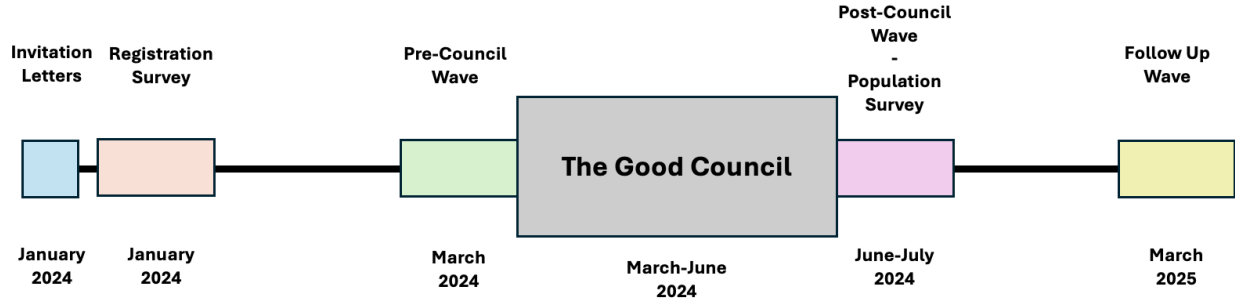


*Note:* The timeline shows the key stages of participant recruitment (invitation letters and registration survey) and survey data collection (pre-council wave, post-council wave and follow-up wave), aligned with the meetings of the Good Council.

Figure 2 summarizes the study timeline. Invitation letters were distributed in January 2024. All individuals completed a brief registration survey at the time of expressing interest in participating in the Good Council. This information was used both to implement the stratified random selection algorithm and to provide baseline covariates for our analysis. The pre-treatment survey wave was conducted in March 2024, shortly before the first Good Council meeting. The citizens' assembly itself took place between March and June 2024. The main post-treatment survey wave was fielded immediately afterwards, at the same time as the Population survey. A final follow-up survey was conducted in March 2025, eight months after the conclusion of the assembly.

**Attrition and survey non-response**   We made an effort to keep attrition to a minimum by offering financial incentives for survey participation. Control group members received EUR 13 for completing the post council survey. Recall that treated participants had already received substantial compensation tied to program participation. For the follow up survey, both treatment and control group members were offered EUR 20. For the treatment group, the survey response rate was 94 percent for the pre-treatment survey, 78 percent for the post-treatment survey, and 56 percent for the follow-up survey. For the control group, the corresponding response rates were 77 percent for the pre-treatment survey, 74 percent for the post-treatment survey, and 63 percent for the follow-up survey. We count only complete responses. Only XX persons in the treatment and XX persons in the control group did not participate in either of the surveys.

We adjust for baseline covariates when comparing individual outcomes across groups to mitigate the

impact of possibly selective non-response (covariate means are reported in Tables A1–A3). We further apply a difference-in-difference approach, which yields robust results (Appendix H).

To test for selective non-response in the post-treatment survey that collects our main outcomes, we furthermore perform balance tests. We do not find any significant differences in covariate means for characteristics that remained balanced between treatment and control groups following assignment, except for the share of respondents from rural areas, which was significantly lower in the control group (Table B1). When comparing treatment and control groups to the population group, we find that the imbalances present at the stage of treatment assignment are replicated (Tables B2–B3).

## 4.2   The Treatment: Deliberating Inequality in the Good Council

The treatment in our study is participation in the Good Council for Redistribution, a citizens assembly that deliberated on wealth inequality and the allocation of EUR 25 million in funds. The Council members met across six weekends in Salzburg, chosen for its central location in Austria. Meetings were moderated and designed by experienced facilitators of citizens assemblies, in collaboration with academic experts from social sciences and philosophy,[8] ensuring balanced information, inclusive participation, and structured deliberation. To guarantee accessibility, childcare, translation services for members not fluent in German, and personal assistance were provided as needed.

Here we provide an overview of the key aspects of the meetings. Further details are available in Appendix E, and the official Good Council website[9] offers comprehensive documentation of each meeting.

**Decision-making rules**   On the first weekend, Council members agreed to using a consensus procedure (consent-based decision-making), whereby participants could signal agreement, raise concerns (that was documented but did not halt the process), or issue serious objections that halted deliberation until resolved. As a special provision for time-limited processes such as a citizens assembly, participants could choose to have serious objections recorded without blocking progress, while retaining the right to revisit them later. In addition, members agreed that funds could not be directed to groups, individuals, or activities that were unconstitutional, hostile to human life, or degrading to human dignity; they could not go to profit-oriented organizations, be used to found or finance political parties, or be paid out to Council members themselves or to closely related persons.

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[8]A full list of expert advisors is provided on the website of the Good Council: https://guterrat.info/en/die-menschen/.
[9]https://guterrat.info/en/einblicke/

**The meetings** During the first two weekends, Council members mainly received expert input on wealth inequality, inheritance, and taxation, and engaged with philosophical perspectives on distributive justice. Building on this foundation, they agreed on six "fields of action" (economic policy, education and media, health and social policy, housing, environment and ecology, and civic participation) and worked within these thematic groups while remaining in regular exchange with the plenary. In the final weekend, members finalized their proposals and allocated the fundsmainly to existing civil society projectsconcluding the process with public presentations of their decisions.

Accordingly, the treatment encompassed several potential mechanisms: (i) informative effects, derived from provided materials, independent preparation, or outside conversations, (ii) deliberative effects, arising from facilitated and informal discussions and (iii) framing or persuasive effects, stemming from the normative environment the citizens' assembly was embedded in. While we estimate the overall impact of deliberative participation, the experimental design does not allow for the disentangling of these distinct channels.

## 4.3 Outcomes of Interest

Our analysis focuses on six primary outcome variables that fall into two broad categories: (i) (perceived) role in democracy, and (ii) (re)distributive views. Unless stated otherwise, the answer "Don't know" is coded as missing. We normalize most outcome variables by subtracting from each value the variable's minimum (across treatment and control groups) and dividing this difference by the range of the variable (difference between maximum and minimum).

*(Perceived) role in democracy:* Political efficacy is measured through a an index that captures both internal efficacy (perceived competence in political engagement) and external efficacy (perceived responsiveness of the political system). Both internal and external efficacy are originally recorded on a four-item scale. To aggregate across dimensions, we take the sum of all responses across variables.[10] Finally, we normalize the resulting scale. Political participation is assessed via a five-item scale evaluating various forms of engagement, including participation in demonstrations, signing petitions, membership in political organizations and online political activism. We take the normalized sum of the responses to these questions, each item coded to unity if it applies and zero otherwise.

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[10]We assume that we can treat the ordered response such that different levels can be expressed as a number ranging from one to five.

*(Re)distributive views:* Knowledge of wealth distribution evaluates respondents' accuracy in estimating the share of wealth held by the top 1% of the population. We code responses to unity if a respondent can put the share in the accurate bracket and 0 otherwise (including cases where respondents select "Don't know"). Support for egalitarian wealth distribution is measured by asking respondents to indicate how much wealth they believe the richest 1% should own. Responses on a five item scale range from "Much more" to "Much less". We assign each response a rank from one to five and normalize the resulting indicator. To measure views on wealth and inheritance taxation, we ask respondents to choose their preferred model of wealth (inheritance) taxation from a list of options. They include a scenario without taxation, a scenario where all wealth (inheritances) are taxed, and several intermediate options that differ in the generosity of tax exemptions. Support for wealth taxation is captured through an indicator assessing support for both net wealth and inheritance taxes. Specifically, we assign unity to an individual who wants both positive wealth and inheritance taxation. We assign 0.5 to those who only support a positive tax rate on one of both taxes, and zero for those opposing both taxes. To investigate polarization, we draw on the full spectrum of responses about preferred tax policy options to both questions. To that end, we identify the most popular tax policy in each experimental group. Then, we assign unity to individuals who agree with the most popular choice on both taxes, 0.5 to those who agree on only one of the taxes with the majority, and zero otherwise. In addition, we study differences between treatment and control groups by analyzing polarization indicators based on responses to the tax policy preference questions. All dependent variables were normalized using min-max scaling to facilitate interpretation and comparison. Appendix Section F provides detailed descriptions of all the question wording and the operationalization of the variables used in the analysis.

The primary independent variable in our analysis distinguishes between assembly participants (treatment group) and interested but unselected individuals (control group). Our regression models incorporate several control variables. These include demographic variables (gender, age, migration background, and degree of urbanization), socioeconomic variables (employment status, income quartile, and education level), and pre-treatment views on wealth inequality, which were measured prior to the assembly to capture baseline differences. We detail the control variables in Table F2 in the Appendix.

## 4.4 Experimental Design and Identification

**Experimental Design**  The core tension in designing citizens' assemblies lies between two objectives: demographic representativeness and equal selection probability. Because willingness to participate varies systematically across population groups, these goals are inherently in conflict. The design challenge is thus to construct representative panels while offering as equal as possible selection probabilities to all who are willing to participate. The Good Council addressed this trade-off using the LEXIMIN algorithm by Flanigan et al. (2021a), which seeks to balance these objectives: approximating demographic representativeness while minimizing variance in individual selection probabilities.

The respective trade-off also complicates evaluation. Non-selected volunteers, constituting of those not assigned to participate, differ systematically from both the treatment group and the general population along characteristics overrepresented among willing participants. Reweighting these groups often requires assigning extreme weights to individuals from underrepresented subpopulations, undermining robustness and increasing sensitivity to model specification. An alternative is to construct a population-based control group that corresponds to the treatment group on the basis of observables. Yet such groups inevitably include individuals who would not have volunteered, introducing potential bias on unobservables.

**Identification**  To address these challenges, we identify three treatment contrasts that separate causal effects from selection patterns. Comparing the treatment and control groups isolates causal effects among those who volunteered to participate. Comparing the control group to the broader population captures the selection gradient, indicating how volunteers differ from the general population. Finally, comparing the treatment group to the population provides an overall measure that combines treatment and selection effects. We begin by comparing outcomes between individuals randomly assigned to treatment and control groups, with stratification on key covariates (see Appendix Table A1 for the baseline balance across groups). Randomization follows the algorithm of Flanigan et al. (2021a), designed specifically for citizens' assemblies. This approach ensures internal validity, though it applies only to individuals who self-selected into the assembly. The sample thus differs systematically from the general population, as all participants expressed prior interest in participating in the Good Council.

Second, we compare the control group to a separate sample drawn from the general Austrian population.[11] This population sample is constructed using non-probability methods and surveyed on the same outcomes

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[11]In our analysis involving the population sample, we use weights to improve the population representativeness of the sample.

(see Appendix Table A2 for the baseline balance across groups). This contrast captures the extent of selection into the Good Council, providing a benchmark for evaluating external validity.

Third, we compare the treatment group to the population sample. This contrast combines the treatment effect and the selection effect, offering a summary measure of how treated participants differ from the broader population (see Appendix Table A3 for the baseline balance across groups).

**Interpretation of Estimands**  To facilitate interpretation of our estimates, we introduce a formal framework following loosely Graham (2008) and Kasy and Lehner (2025). Let $Y_i$ denote an outcome for individual $i$, such as preferences for redistribution. Let $T_i$ denote assignment to the Good Council, and $S_i$ selection into the pool of individuals who expressed interest in participating. Let finally $\epsilon_i$ be a vector of unobserved individual characteristics, which are unaffected by the Good Council. We can then assume that

$$Y_i = g(T_i, S_i, \epsilon_i), \tag{1}$$

where $g$ is a structural function determining counterfactual outcomes. The dependence of $g$ on $T$ captures treatment effects and the dependence on $S_i$ captures selection effects. Let expectations average over the distribution of unobserved heterogeneity $\epsilon_i$ for those who selected into the pool of individuals who expressed interest in participating in the Good Council.

Using this notation, we can now express the identified averages from our three identified contrasts in structural terms. Table 2 maps each of these averages to its corresponding structural notation. Correspondingly, Table 3 maps the contrasts discussed thus far to their respective average structural effects. The experimental comparison between the treatment and control groups identifies the **average treatment effect** under the assumption of no differential selection between the groups. The comparison between the control and the population groups identifies the **average selection effect**, under the plausible assumption that individuals in the control group are unaffected by any spillovers resulting from non-selection into the Good Council. Lastly, the comparison between the treatment and the population groups identifies the **average total effect**, capturing both treatment and selection effects. While our empirical set up allows us to estimate all three contrasts separately, it follows that the average total effect corresponds to the sum of the average treatment effect and the average selection effect:

$$E[g(1,1,\epsilon_i) - g(0,0,\epsilon_i)] = E[g(1,1,\epsilon_i) - g(0,1,\epsilon_i)] + E[g(0,1,\epsilon_i) - g(0,0,\epsilon_i)] \tag{2}$$

Table 2: Identified averages

| | |
|---|---|
| Treatment Group | $E[g(1,1,\epsilon_i)]$ |
| Control Group | $E[g(0,1,\epsilon_i)]$ |
| Population Group | $E[g(0,0,\epsilon_i)]$ |

Table 3: Identified effects

| Contrast | Identified effect | Interpretation | Figures & Tables |
|---|---|---|---|
| Treatment Group vs. Control Group | $E[g(1,1,\epsilon_i) - g(0,1,\epsilon_i)]$ | Average treatment effect | Table 4 |
| Control Group vs. Population Group | $E[g(0,1,\epsilon_i) - g(0,0,\epsilon_i)]$ | Average selection effect | Table 5 |
| Treatment Group vs. Population Group | $E[g(1,1,\epsilon_i) - g(0,0,\epsilon_i)]$ | Average total effect | Table 6 |

## 4.5   Estimation and Inference

As the randomization conditional on observable characteristics in the treatment and control groups induces imbalances between i) the treatment and control group and ii) the control group and the population sample, we use regressions to take differences in observable characteristics into account. To increase precision, we estimate parametric regressions using the following estimation regression:

$$Y_{i,t} = \beta_0 + \beta_1 T_{i,t} + \mathbf{X_{i,t}} + \epsilon_{i,t} \tag{3}$$

where $Y_i$ refers to the outcome variable for individual i. Depending on the contrast, the control group or the general population sample serves the reference groups. Further, we include all stratification variables specified in Table F2 and control variables measured before treatment, which is represented by $\mathbf{X_i}$ in the specification above. Depending on the scale of the outcome variable, we use an OLS (continuous) or a logit (binary) regression.

## 5   Findings

We report three contrasts: the average treatment effect, the average selection effect, and the total effect, followed by robustness checks. Each is estimated using stratified regressions with pre-treatment covariates.

## 5.1 Average Treatment Effect

Table 4 shows the estimated average treatment effect of participation, comparing treatment and control groups after the Good Council. The table contains OLS estimates for the six outcomes. In addition, Table 4 reports mean outcomes in the control group and the number of observations that feature in each regression. Due to missing values or cases where respondents answered with "Don't know", the number of observations differs across specifications. Finally, the table also features goodness-of-fit measures for each model. Each model controls for the stratification variables $\mathbf{X}_{i,t}$ (Table F2). Table 4 only reports the estimate of the average treatment effect while dropping the coefficient estimates on control variables.

Table 4: Average Treatment Effect

| | *Dependent variable:* | | | | | |
|---|---|---|---|---|---|---|
| | Political efficacy | Political participation | Knowledge wealth distr. | More wealth redist. | Support W u. IH tax | Preference concentration |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Member | $-0.052^{**}$ | $-0.105^{**}$ | $0.306^{***}$ | $0.034^{*}$ | $0.0002$ | $0.407^{***}$ |
| | (0.026) | (0.049) | (0.072) | (0.019) | (0.036) | (0.063) |
| Mean dep. var (ref group) | 0.47 | 0.50 | 0.14 | 0.08 | 0.93 | 0.38 |
| N Control | 201 | 187 | 203 | 196 | 199 | 203 |
| N Treatment | 37 | 36 | 38 | 34 | 38 | 38 |
| $R^2$ | 0.285 | 0.184 | 0.216 | 0.164 | 0.226 | 0.287 |

*Note:* The table reports average treatment effects from comparing treatment and control groups. Estimates use the baseline specification in Section 4.5, which includes covariates used as stratification variables and control variables, both specified in Table F2. Table report OLS estimates for continuous outcomes on standardized outcome variables. Outcomes are measured in the post treatment survey following the Good Council. See Section F for outcome definitions of columns 1–5 and Section G for column 6. Standard errors in parentheses: $^{*}$p<0.1; $^{**}$p<0.05; $^{***}$p<0.01.

Council membership is associated with a negative and statistically significant effect on political efficacy (0.052, p<0.05). The estimated effect on political participation is also negative and marginally significant (0.105, p<0.05). Tables I2 and I3 in Appendix I.2 include disaggregated effects of deliberative participation on council members' (perceived) role in democracy. Both internal and external efficacy show negative point estimates, with only internal efficacy reaching statistical significance at the 10%-level. For political participation, effects differ across activities, with significantly negative estimates for signing a petition and supporting a public cause (p<0.05).

Among the treated, the proportion of individuals who correctly estimate the share of wealth held at the top of the distribution is significantly higher than in the control group. Council membership is positively and statistically significantly associated with knowledge of the wealth distribution (0.306, p<0.01), suggesting a strong informational effect. Figure 3 illustrates group differences in estimates of wealth held by the top 1%, showing more accurate responses among participants. Among council members, the share with an

18

Figure 3: Knowledge of Wealth Distribution By Council Membership

**How much wealth do the richest 1% of households in Austria own?**
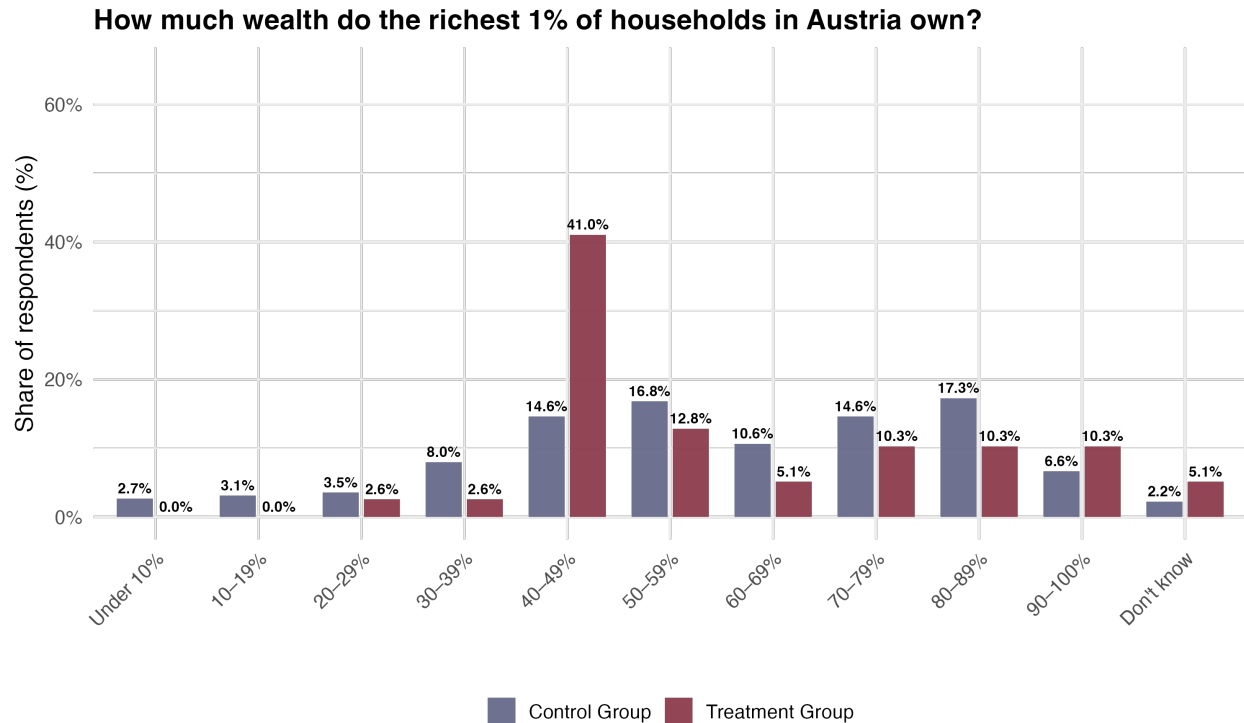


*Note:* Survey question: "What do you think? How much wealth do the richest 1% of households in Austria own?" Treatment Group (Council Members): 39 respondents; Control Group (Interested Invitees): 226 respondents.

accurate estimate in the 40-49% bracket is slightly above 40%, while it is only 14.6% among members of the control group.

Despite marginally more favorable views on wealth redistribution among treated individuals (0.034, p<0.1), this does not translate into increased support for wealth and inheritance taxation. The indicator variable, summarizing wealth and inheritance taxation (measuring whether respondents completely reject taxation of wealth or inheritances), is not significantly different in the treatment group than it is among members of the control group. However, the results point to meaningful differences in preferences regarding the design of taxation. The coefficient in the sixth column captures whether respondents support the most common tax schedule, defined by a particular exemption threshold, within their respective group. The average treatment effect is large and statistically significant (0.407, p < 0.01), indicating that treated individuals are substantially more likely to converge around a shared design for taxing wealth and inheritances. A more detailed analysis of distributional convergence, using measures of inequality, polarization, and agreement, is provided in the Appendix in Table G1.

Figure 4: Preferences for Tax Reforms By Council Membership



**Proposal: Wealth Tax**

| | |
|---|---|
| Don't know | 0.0% / 1.8% |
| No taxes | 2.6% / 3.5% |
| From €500,000 | 10.3% / 31.4% |
| From €50,000 | 0.0% / 4.0% |
| From €100,000 | 0.0% / 15.9% |
| From €1,000,000 | 87.2% / 40.7% |
| All wealth | 0.0% / 2.7% |

Percentage of Respondents (%)

Control Group    Treatment Group

**Proposal: Inheritance Tax**

| | |
|---|---|
| Don't know | 0.0% / 0.4% |
| No taxes | 7.7% / 10.6% |
| From €500,000 | 7.7% / 31.4% |
| From €50,000 | 0.0% / 5.8% |
| From €100,000 | 7.7% / 12.4% |
| From €1,000,000 | 76.9% / 32.3% |
| All inheritances | 0.0% / 7.1% |

Percentage of Respondents (%)

Control Group    Treatment Group

*Note: Left panel:* Proposal for a Wealth Tax  Survey question: There has been a lot of discussion in the political arena about the idea of a wealth tax.  What do you think about the following proposals? *Right panel:* Proposal for an Inheritance Tax  Survey question: And how about an inheritance tax?  What do you think about the following proposals?; Treatment Group (Council Members): 39 respondents; Control Group (Interested Invitees): 226 respondents.

Figure 4 sheds more light on the tax preferences formation induced by the treatment. The figure illustrates support for wealth and inheritance taxes, comparing the treatment to the control group. It plots the share of respondents (separately for the treatment and control group) that support a tax with different exemption levels on the x-axis. The graphical evidence offers two important insights that complement the findings in the fifth and sixth column of Table 4. First, it shows that it is only a small minority that rejects the taxation of wealth and inheritances completely, and that the share of respondents who oppose taxation is higher for inheritance taxation than it is for wealth taxation. Second, the figure illustrates that preferences on taxation for both wealth and inheritances in the treatment group strongly converge to one model that involves a generous tax allowance of EUR 1,000,000 – a design choice already presented on the assembly homepage, repeatedly discussed during deliberation, and ultimately endorsed by a large majority of participants. In the treatment group, 87.2% favor this threshold for wealth taxation and 76.9% for inheritance taxation.

## 5.2 Average Selection Effect

Table 5 presents results based on the contrast between the control group and the random population sample. The population serves as a reference group in Table 5. The specifications reported in table parallel those in Table 4, and we report only the coefficient that indicates membership in the control group versus the general population sample.

Table 5: Average Selection Effect

| | Dependent variable: | | | | | |
|---|---|---|---|---|---|---|
| | Political efficacy | Political participation | Knowledge wealth distr. | More wealth redist. | Support W u. IH tax | Preference concentration |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Member | 0.017 | 0.107*** | 0.050* | 0.101*** | 0.106*** | 0.106*** |
| | (0.013) | (0.023) | (0.028) | (0.017) | (0.022) | (0.035) |
| Mean dep. var (ref group) | 0.49 | 0.35 | 0.09 | 0.25 | 0.76 | 0.31 |
| N Population | 921 | 887 | 944 | 793 | 863 | 944 |
| N Control | 201 | 187 | 203 | 196 | 199 | 203 |
| $R^2$ | 0.201 | 0.170 | 0.051 | 0.206 | 0.131 | 0.078 |

*Note:* The table reports average selection effects from comparing control and population groups. Estimates use the baseline specification in Section 4.5, which includes covariates used as stratification variables and control variables, both specified in Table F2. Table report OLS estimates for continuous outcomes on standardized outcome variables. Outcomes are measured in the post treatment survey following the Good Council. See Section F for outcome definitions of columns 1–5 and Section G for column 6. Standard errors in parentheses: *$p<0.1$; **$p<0.05$; ***$p<0.01$.

Compared to the general population, members of the control group exhibited similar levels of political efficacy. The respective coefficient is small and statistically insignificant, suggesting that individuals interested in participating in the Good Council did not exhibit higher levels of perceived political influence. However, they reported significantly higher levels of political participation (0.107, p<0.01). Those who showed interest in taking part in the Good Council are already more politically engaged than the average Austrian resident. They also demonstrated greater factual knowledge about wealth inequality, with a significantly higher probability of correctly identifying the concentration of wealth among the top 1% (0.050, p<0.1). This suggests that more knowledgeable individuals were more likely to show interest in the assembly, or that those who did may have proactively engaged with relevant information. The control group expressed stronger support for a more egalitarian wealth distribution (0.106, p<0.01), as well as for wealth and inheritance taxation (0.106, p<0.01). These attitudinal differences highlight that those interested in taking part in the assembly were more predisposed toward redistribution than the general public. Finally, their views on wealth and inheritance taxation were significantly less polarized (0.106, p<0.01).

## 5.3 Average Total Effect

Table 6 provides estimates of the average total effect, by comparing outcomes between the treatment group and the general population sample conditional on covariates. All specifications are the same as those we use to estimate the average treatment effect and the average selection effect.

Table 6: Average Total Effect

| | Political efficacy | Political participation | Knowledge wealth distr. | More wealth redist. | Support W u. IH tax | Preference concentration |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Member | −0.032 | 0.026 | 0.350*** | 0.202*** | 0.138*** | 0.538*** |
| | (0.029) | (0.048) | (0.078) | (0.029) | (0.036) | (0.057) |
| Mean dep. var (ref group) | 0.49 | 0.35 | 0.09 | 0.25 | 0.76 | 0.31 |
| N Population | 921 | 887 | 944 | 793 | 863 | 944 |
| N Treatment | 37 | 36 | 38 | 34 | 38 | 38 |
| $R^2$ | 0.201 | 0.148 | 0.101 | 0.176 | 0.112 | 0.173 |

*Dependent variable:*

*Note:* The table reports average total effects from comparing treatment and population groups. Estimates use the baseline specification in Section 4.5, which includes covariates used as stratification variables and control variables, both specified in Table F2. Table report OLS estimates for continuous outcomes on standardized outcome variables. Outcomes are measured in the post treatment survey following the Good Council. See Section F for outcome definitions of columns 1–5 and Section G for column 6. Standard errors in parentheses: *$p<0.1$; **$p<0.05$; ***$p<0.01$.

Political efficacy in the treatment group closely resembles that of the general population. Since the corresponding estimates for treatment and control groups point in opposite directions, we see a modest statistically significant negative treatment effect. Levels of political participation in the treatment group are similar to those in the general population. This stands in contrast to the negative and significant treatment effect observed in the control comparison, suggesting that this effect is primarily driven by selection as the total effect is close to zero. By contrast, members of the treatment group are substantially more knowledgeable about wealth concentration than the general population (0.350, p<0.01). This suggests a clear learning effect: participants acquire additional information beyond what selection alone would predict. Support for a more egalitarian wealth distribution is also significantly higher among treatment group members (0.202, p<0.01). Combined with the main results, this indicates that deliberation slightly strengthens redistributive preferences even among those already supportive of a more egalitarian wealth distribution. Support for wealth and inheritance taxation is also higher in the treatment group (0.138, p<0.01). Although this estimate slightly exceeds the selection effect, the overall treatment effect remains statistically insignificant. Finally, views on wealth and inheritance taxation are significantly less polarized in the treatment group compared to the general population (0.538, p<0.01), confirming that participation fosters convergence on specific policy proposals.

## 5.4 Alternative Identification Strategy & Robustness Checks

We look into an alternative identification strategy and carry out several robustness checks. First, we explore the role of unobserved heterogeneity that may persist despite randomization. This could arise if individuals in the control group do not respond to our survey at the end of the council based on unobserved heterogeneity that affects the outcomes we measure. To investigate these concerns, we leverage data from the survey that was administered shortly before the Good Council convened for the first time but after the treatment assignment. This allows us to compare changes in views and preferences between the treatment and control group in a differences-in-differences design. The results of this exercise are reported in the Appendix Section H. All our results hold qualitatively, except the statistically significant and negative effects estimated for political participation. The results from the earlier survey suggest that members of the treatment group were less likely to be politically active already before the Good Council convened, a difference that did not change over time.

Second, we run placebo tests on changes in outcomes that are unrelated to the treatment. These include views on carbon taxation, unemployment benefits, and tuition fees. Our results in Section K in the Appendix suggest that these outcomes remain unaffected.

Third, we study dynamic effects in Section J in the Appendix. Several months after the field experiment ends, we find that the treatment group continues to be better informed about the concentration of wealth than the control group. Moreover, the treatment group maintains more similar views on wealth and inheritance taxation compared to the control group. In terms of political efficacy, political participation, demand for more wealth distribution and overall support for wealth and inheritance taxation, no differences between the treatment and control group persist in the long run.

Fourth, we implement our baseline estimation approach in Table 4 on a reduced sample of observations with no missing outcome variables. This analysis in Appendix Table I1 shows that differences across outcomes are not driven by fluctuations in the sample across specifications, which are due to missing values on each of the six main outcomes.

# 6 Summary & Discussion of the Findings

Figure 5 and Figure 6 summarize estimated treatment, selection, and total effects across democratic and redistributive outcomes (based on Table 4, Table 5 and Table 6).

Figure 5: Effects of Deliberative Participation on Council Members' (Perceived) Role in Democracy



**Main Findings: (Perceived) Role in Democracy**
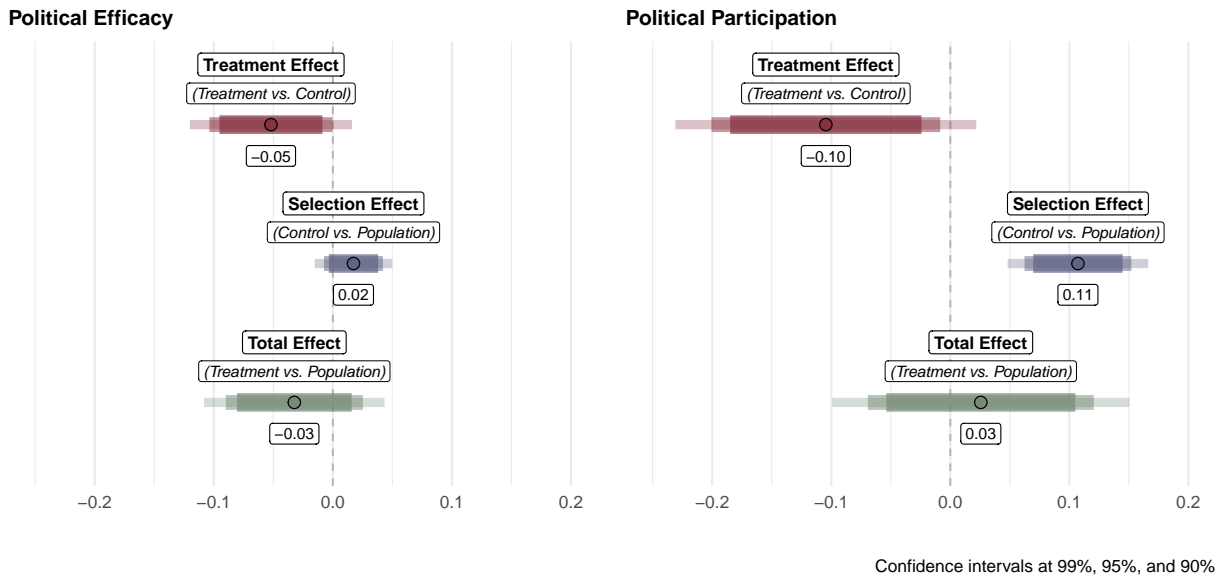
*Note:* The plot shows the average treatment effect (treatment vs. control), the average selection effect (control vs. population) and the average total effect (treatment vs. population) for the outcomes political efficacy and political participation. The underlying models can be found in Table 4, Table 5, and Table 6.

As shown in Figure 5, treatment effects on political efficacy and participation are negligible. Participation in the citizens' assembly did not enhance internal or external efficacy, nor did it increase participants' involvement in other political activities. These results hold both immediately after the assembly and several months later (Table J1 in the Appendix). Selection effects, by contrast, are pronounced for political participation: individuals who were willing to take part in the assembly already exhibited higher levels of political participation than the general population. This yields no significant total effects for either political efficacy or political participation.

This is a notable finding, as prior work has often highlighted democratic empowerment as one of the core promises of deliberative participation. The absence of institutional anchoring may help explain the lack of effects: participants had no clear indication that their proposals would be taken up by political decision-makers, possibly limiting their sense of external efficacy. Without such a link to formal politics, the assembly may also have lacked the momentum to inspire further engagement in more institutionalized forms of political participation. Normative theories of deliberative democracy emphasize the importance of institutional coupling: deliberative sites are expected to be most effective when embedded in political structures that can act on their outputs (Hendriks, 2016). However, the absence of an effect on internal

efficacy – the sense of one's own political competence – is somewhat more surprising. Participants engaged in sustained, small-group discussion, learned from experts, deliberated over competing proposals, and reached group-level consensus. Yet, this experience did not enhance their confidence in their own political capacities. This suggests that even under favorable conditions – informational input leading to substantial learning, peer discussion resulting in measurable opinion convergence, and participation culminating in tangible outputs – deliberative participation did not increase individuals' political self-efficacy.

Figure 6: Effects of Deliberative Participation on Council Members' Redistributive Views

**Main Findings: Redistributive Views**



Confidence intervals at 99%, 95%, and 90%

*Note:* The plot shows the average treatment effect (treatment vs. control), the average selection effect (control vs. population) and the average total effect (treatment vs. population) for the outcomes knowledge of the wealth distribution, support for more wealth distribution, support for wealth and inheritance taxation and preference concentration. The underlying models can be found in Table 4 Table 5, and Table 6.

As shown in Figure 6, we observe a strong and statistically significant treatment effect on factual knowledge: participation in the citizens' assembly improved council members' factual knowledge of how wealth

is distributed in Austria. Selection effects are minimal, suggesting that interested invitees did not differ substantially from the general population in their initial knowledge levels. As a result, the total effect is largely driven by participation itself, indicating that deliberative formats, especially when paired with expert input, can effectively promote learning about complex economic issues.

For support for redistribution and support for taxation, however, treatment effects are small and not statistically significant. Deliberative participation did not meaningfully shift council members attitudes in favor of a more wealth distribution or increased support for wealth and inheritance taxes. In these two domains, we instead observe substantial selection effects: individuals in the control group already supported redistribution and taxation more strongly than the general population. This suggests that individuals with more progressive fiscal preferences were more likely to accept the invitation to participate in the assembly, leaving limited room for more attitudinal change. As a result, the total effects for redistribution and taxation are modest but largely reflect pre-existing differences, not opinion change triggered by deliberative participation. Finally, we find a robust treatment effect on preference concentration. Treated individuals were significantly more likely to support the modal tax schedule chosen by the group, suggesting that deliberation facilitated opinion alignment. This result is robust across regression-based specifications and distributional measures. Selection effects for this outcome are minimal, although statistically significant and the resulting total effect is primarily driven by participation. This result is robust across regression-based specifications and distributional measures.

Taken together, the findings show that participation in the Good Council primarily served to inform participants and foster convergence on specific tax models. Political efficacy and participation, by contrast, remained unchanged. Crucially, many of the most pronounced differences arise not from the treatment itself, but from who chooses to participate. While the assembly was demographically representative, it still attracted a politically distinct subset of citizens. These results highlight a central challenge for deliberative democratic reform: stratified random selection may succeed in demographic representation, but it does not eliminate self-selection based on prior political attitudes and behavior. As a result, deliberation may refine existing beliefs rather than change them, and speak most clearly to those already predisposed to listen.

# 7 Conclusion

Despite a marked rise in economic inequality, broad mobilization behind progressive tax reforms remains weak (Genschel, Limberg, and Seelkopf, 2023). This reflects participatory and informational inequalities: non-affluent citizens are less engaged in politics, less informed about taxation, and less likely to articulate redistributive preferences. Citizens' assemblies have been proposed as institutional responses to these challenges, seeking to empower disengaged citizens by offering a direct experience of political agency and to reduce informational asymmetries through structured deliberation.

Our study examined whether these promises hold by testing how participation in a privately organized citizens' assembly on wealth inequality affected participants' (perceived) role in democracy and their redistributive views. Using a novel three-group experimental design that separates causal effects of participation from patterns of self-selection, we identify both potentials and limits of deliberative innovation.

We find that while deliberative participation increased factual knowledge and fostered convergence around a specific tax policy design, it did not enhance political efficacy, broader civic engagement, or redistributive support. These limited effects reflect two boundary conditions: strong political self-selection, with participants already more supportive of redistribution and more politically engaged than the general public, and weak institutional anchoring, which likely reduced the assembly's broader political relevance.

These findings refine deliberative democratic theory in two ways. First, they reinforce longstanding claims that structured, expert-supported deliberation can enhance knowledge and reduce policy disagreement (Pateman, 1970; Gastil, 2000; Barabas, 2004), while also cautioning against assumptions that such experiences routinely generate empowerment or engagement (Morrell, 2005). Second, they highlight the institutional constraints of privately organized forums: without ties to political institutions, citizens' assemblies may foster informed consensus but struggle to broaden democratic participation (Courant, 2022; Goldberg and Bächtiger, 2023). Methodologically, our three-group design demonstrates the importance of distinguishing selection from treatment effects and provides a framework applicable not only to the study of democratic innovations but also to policy evaluations and field experiments in which selection processes shape observed outcomes.

Our results also carry important practical implications. By separating who participates from what participation does, we show that political self-selection constitutes a structural boundary for the transformative potential of citizens' assemblies. Even when recruitment procedures secure high levels of socio-

demographic representativeness, such forums may still predominantly attract those already engaged and supportive. Addressing this requires refining how representation is conceptualized and evaluated (see Spada and Peixoto (2025) for a careful discussion). A more comprehensive approach that accounts not only for socio-demographic background but also for political attitudes, levels of engagement, and motivational barriers is essential to improve the deliberative potential of these institutions (Germann, 2025). Future research can build on this insight by testing how alternative recruitment strategies might reach politically disengaged or skeptical citizens, and by investigating whether embedding citizens assemblies within formal political institutions amplifies not only informational but also participatory outcomes (Wappenhans et al., 2024; OECD, 2020).

More broadly, our findings shed light on the enduring puzzle of redistributive politics: even as economic inequality rises, mobilization behind progressive tax reforms remains weak. Citizens' assemblies can foster informed consensus among the politically distinct subset of individuals who choose to participate, but without institutional embedding and inclusive recruitment, they are unlikely to activate disengaged constituencies or build broader coalitions for redistributive change.

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# Appendix

## Table of Contents

# A  Treatment Assignment

Table A1: Balance Table, Treatment Group and Control Group

| Section | Description | Label | Treatment | Control | Difference | p-value |
|---------|-------------|-------|-----------|---------|------------|---------|
| Gender | Share female | gender | 0.52 | 0.57 | -0.05 | 0.5 |
| Age | Mean in years | age | 47.2 | 42.8 | 4.4 | 0.11 |
| | Share max 29 years | agebucket1 | 0.2 | 0.18 | 0.02 | 0.8 |
| | Share 30-44 years | agebucket2 | 0.28 | 0.42 | -0.14 | 0.05 |
| | Share 45-59 years | agebucket3 | 0.22 | 0.23 | -0.01 | 0.92 |
| | Share 60+ years | agebucket4 | 0.3 | 0.17 | 0.13 | 0.06 |
| Education | Share Max compulsory | edu_red1 | 0.18 | 0.05 | 0.13 | 0.02 |
| | Share Apprenticeship | edu_red2 | 0.34 | 0.15 | 0.19 | 0.01 |
| | Share Middle school | edu_red3 | 0.16 | 0.13 | 0.03 | 0.59 |
| | Share High school | edu_red4 | 0.14 | 0.2 | -0.06 | 0.31 |
| | Share Higher education | edu_red5 | 0.18 | 0.47 | -0.29 | 0 |
| Employment | Share Unemployed | employ_red1 | 0.04 | 0.04 | 0 | 0.94 |
| | Share In training | employ_red2 | 0.08 | 0.1 | -0.02 | 0.59 |
| | Share Employed | employ_red3 | 0.44 | 0.62 | -0.18 | 0.02 |
| | Share Self-employed | employ_red4 | 0.1 | 0.1 | 0 | 0.93 |
| | Share Retired | employ_red5 | 0.28 | 0.12 | 0.16 | 0.02 |
| | Share Other | employ_red6 | 0.06 | 0.02 | 0.04 | 0.3 |
| NUTS-3 | Share East | nuts31 | 0.44 | 0.48 | -0.04 | 0.64 |
| | Share South | nuts32 | 0.22 | 0.2 | 0.02 | 0.69 |
| | Share West | nuts33 | 0.34 | 0.33 | 0.01 | 0.88 |
| Migration background | Share Austria | mig_first_gen_red21 | 0.78 | 0.8 | -0.02 | 0.73 |
| | Share Other | mig_first_gen_red22 | 0.06 | 0.05 | 0.01 | 0.74 |
| | Share EU (West) | mig_first_gen_red23 | 0.04 | 0.07 | -0.03 | 0.36 |
| | Share EU (East) | mig_first_gen_red24 | 0.06 | 0.05 | 0.01 | 0.74 |
| | Share Turkey, Western Balkan | mig_first_gen_red25 | 0.06 | 0.03 | 0.03 | 0.47 |
| Household income quartile | Share Quartile 1 | inc_quart1 | 0.24 | 0.13 | 0.11 | 0.08 |
| | Share Quartile 2 | inc_quart2 | 0.24 | 0.16 | 0.08 | 0.2 |
| | Share Quartile 3 | inc_quart3 | 0.28 | 0.36 | -0.08 | 0.23 |
| | Share Quartile 4 | inc_quart4 | 0.24 | 0.35 | -0.11 | 0.09 |
| Urbanization | Share Cities | urb1 | 0.36 | 0.45 | -0.09 | 0.25 |
| | Share Towns and suburbs | urb2 | 0.3 | 0.29 | 0.01 | 0.94 |
| | Share Rural areas | urb3 | 0.34 | 0.26 | 0.08 | 0.27 |
| Is wealth dist. fair? | Wealth inequality perception | Q11.norm | 0.29 | 0.2 | 0.09 | 0.04 |

*Note:* This table presents the comparison of various demographic and socioeconomic variables between the members and control groups. The columns show the variable, measurement type, shares and mean values for treatment and control groups, the difference between these shares and means, and the p-value indicating the significance of the difference.

Table A2: Balance Table Control Group v. Population Sample

| Section | Description | Label | Control | Population | Difference | p-value |
|---|---|---|---|---|---|---|
| Gender | Share female | gender | 0.57 | 0.51 | 0.07 | 0.01 |
| | Mean in years | age | 42.8 | 48.48 | -5.68 | 0 |
| | Share max 29 years | agebucket1 | 0.18 | 0.19 | 0 | 0.91 |
| Age | Share 30-44 years | agebucket2 | 0.42 | 0.24 | 0.17 | 0 |
| | Share 45-59 years | agebucket3 | 0.23 | 0.27 | -0.05 | 0.09 |
| | Share 60+ years | agebucket4 | 0.17 | 0.3 | -0.13 | 0 |
| | Share Max compulsory | edu_red1 | 0.05 | 0.23 | -0.18 | 0 |
| | Share Apprenticeship | edu_red2 | 0.15 | 0.32 | -0.17 | 0 |
| Education | Share Middle school | edu_red3 | 0.13 | 0.14 | -0.01 | 0.64 |
| | Share High school | edu_red4 | 0.2 | 0.16 | 0.03 | 0.64 |
| | Share Higher education | edu_red5 | 0.47 | 0.15 | 0.33 | 0 |
| | Share Unemployed | employ_red1 | 0.04 | 0.04 | 0 | 1 |
| | Share In training | employ_red2 | 0.1 | 0.05 | 0.05 | 0.01 |
| Employment | Share Employed | employ_red3 | 0.62 | 0.52 | 0.1 | 0 |
| | Share Self-employed | employ_red4 | 0.1 | 0.06 | 0.04 | 0.72 |
| | Share Retired | employ_red5 | 0.12 | 0.26 | -0.14 | 0 |
| | Share Other | employ_red6 | 0.02 | 0.07 | -0.04 | 0.95 |
| | Share East | nuts31 | 0.48 | 0.44 | 0.04 | 0.22 |
| NUTS-3 | Share South | nuts32 | 0.2 | 0.21 | -0.01 | 0.66 |
| | Share West | nuts33 | 0.33 | 0.35 | -0.03 | 0.35 |
| | Share Austria | mig_first_gen_red21 | 0.8 | 0.9 | -0.1 | 0 |
| | Share Other | mig_first_gen_red22 | 0.04 | 0.03 | 0.02 | 0.15 |
| Migration background | Share EU (West) | mig_first_gen_red23 | 0.07 | 0.04 | 0.03 | 0.07 |
| | Share EU (East) | mig_first_gen_red24 | 0.04 | 0.01 | 0.03 | 0.01 |
| | Share Turkey, Western Balkan | mig_first_gen_red25 | 0.04 | 0.02 | 0.02 | 0.06 |
| | Share Quartile 1 | inc_quart1 | 0.13 | 0.16 | -0.03 | 0.53 |
| Household income quartile | Share Quartile 2 | inc_quart2 | 0.16 | 0.16 | -0.01 | 0.72 |
| | Share Quartile 3 | inc_quart3 | 0.37 | 0.3 | 0.07 | 0.03 |
| | Share Quartile 4 | inc_quart4 | 0.35 | 0.38 | -0.03 | 0.15 |
| | Share Cities | urb1 | 0.27 | 0.21 | 0.06 | 0.2 |
| Urbanization | Share Towns and suburbs | urb2 | 0.4 | 0.35 | 0.05 | 0.75 |
| | Share Rural areas | urb3 | 0.33 | 0.44 | -0.11 | 0.39 |
| Is wealth dist. fair? | Wealth inequality perception | Q11.norm | 0.2 | 0.33 | -0.13 | 0 |
| Population weight | Population weight | GEW | 1 | 1.22 | -0.22 | 0.81 |

*Note:* This table presents the comparison of demographic and socioeconomic variables between the Control and Population groups. The columns show the variable, definition, shares and mean values for the Members and Population groups, and the difference between these shares and means.

Table A3: Balance Table Treatment Group v. Population Sample

| Section | Description | Label | Treatment | Population | Difference | p-value |
|---|---|---|---|---|---|---|
| Gender | Share female | gender | 0.52 | 0.51 | 0.01 | 0.62 |
| Age | Mean in years | age | 47.2 | 48.48 | -1.28 | 0.57 |
| | Share max 29 years | agebucket1 | 0.2 | 0.19 | 0.01 | 0.76 |
| | Share 30-44 years | agebucket2 | 0.28 | 0.24 | 0.04 | 0.56 |
| | Share 45-59 years | agebucket3 | 0.22 | 0.27 | -0.05 | 0.37 |
| | Share 60+ years | agebucket4 | 0.3 | 0.3 | 0 | 0.98 |
| Education | Share Max compulsory | edu_red1 | 0.18 | 0.23 | -0.05 | 0.81 |
| | Share Apprenticeship | edu_red2 | 0.34 | 0.32 | 0.02 | 0.55 |
| | Share Middle school | edu_red3 | 0.16 | 0.14 | 0.02 | 0.72 |
| | Share High school | edu_red4 | 0.14 | 0.16 | -0.02 | 0.41 |
| | Share Higher education | edu_red5 | 0.18 | 0.15 | 0.03 | 0.58 |
| Employment | Share Unemployed | employ_red1 | 0.04 | 0.04 | 0 | 0.94 |
| | Share In training | employ_red2 | 0.08 | 0.05 | 0.03 | 0.45 |
| | Share Employed | employ_red3 | 0.44 | 0.52 | -0.08 | 0.31 |
| | Share Self-employed | employ_red4 | 0.1 | 0.06 | 0.04 | 0.94 |
| | Share Retired | employ_red5 | 0.28 | 0.26 | 0.02 | 0.9 |
| | Share Other | employ_red6 | 0.06 | 0.07 | -0.01 | 0.29 |
| NUTS-3 | Share East | nuts31 | 0.44 | 0.44 | 0 | 0.94 |
| | Share South | nuts32 | 0.22 | 0.21 | 0.01 | 0.88 |
| | Share West | nuts33 | 0.34 | 0.35 | -0.01 | 0.83 |
| Migration background | Share Austria | mig_first_gen_red21 | 0.78 | 0.9 | -0.12 | 0.04 |
| | Share Other | mig_first_gen_red22 | 0.06 | 0.03 | 0.03 | 0.32 |
| | Share EU (West) | mig_first_gen_red23 | 0.04 | 0.04 | 0 | 0.94 |
| | Share EU (East) | mig_first_gen_red24 | 0.06 | 0.01 | 0.05 | 0.16 |
| | Share Turkey, Western Balkan | mig_first_gen_red25 | 0.06 | 0.02 | 0.04 | 0.22 |
| Household income quartile | Share Quartile 1 | inc_quart1 | 0.24 | 0.16 | 0.08 | 0.12 |
| | Share Quartile 2 | inc_quart2 | 0.24 | 0.16 | 0.08 | 0.24 |
| | Share Quartile 3 | inc_quart3 | 0.28 | 0.3 | -0.02 | 0.8 |
| | Share Quartile 4 | inc_quart4 | 0.24 | 0.38 | -0.14 | 0.02 |
| Urbanization | Share Cities | urb1 | 0.26 | 0.21 | 0.05 | 0.71 |
| | Share Towns and suburbs | urb2 | 0.28 | 0.35 | -0.07 | 0.05 |
| | Share Rural areas | urb3 | 0.46 | 0.44 | 0.02 | 0.15 |
| Is wealth dist. fair? | Wealth inequality perception | Q11.norm | 0.29 | 0.33 | -0.04 | 0.26 |
| Population weight | Population weight | GEW | 1 | 1.22 | -0.22 | 0.81 |

*Note:*   This table presents the comparison of demographic and socioeconomic variables between the Members and Population groups. The columns show the variable, definition, shares and mean values for the Members and Population groups, and the difference between these shares and means.

# B   Covariate balance for survey respondents

Table B1: Covariate Balance for Survey Respondents, Treatment Group and Control Group

| Section | Description | Treatment | Control | Difference | p-value |
|---|---|---|---|---|---|
| Gender | Share Male | 0.37 | 0.41 | -0.05 | 0.60 |
| | Share Female | 0.61 | 0.58 | 0.02 | 0.78 |
| | Share Diverse | 0.03 | 0.00 | 0.02 | 0.42 |
| Age | Mean in years | 49.05 | 43.80 | 5.25 | 0.08 |
| | Share max 29 years | 0.16 | 0.17 | -0.01 | 0.82 |
| | Share 30-44 years | 0.26 | 0.39 | -0.13 | 0.10 |
| | Share 45-59 years | 0.26 | 0.25 | 0.02 | 0.83 |
| | Share 60+ years | 0.32 | 0.19 | 0.13 | 0.11 |
| Education | Share Max compulsory | 0.13 | 0.06 | 0.07 | 0.24 |
| | Share Apprenticeship | 0.37 | 0.16 | 0.21 | 0.01 |
| | Share Middle school | 0.18 | 0.11 | 0.07 | 0.29 |
| | Share High school | 0.13 | 0.17 | -0.04 | 0.56 |
| | Share Higher education | 0.18 | 0.50 | -0.31 | 0.00 |
| Employment | Share Unemployed | 0.03 | 0.02 | 0.01 | 0.81 |
| | Share In training | 0.03 | 0.12 | -0.09 | 0.01 |
| | Share Employed | 0.53 | 0.60 | -0.07 | 0.40 |
| | Share Self-employed | 0.08 | 0.09 | -0.01 | 0.76 |
| | Share Retired | 0.29 | 0.15 | 0.14 | 0.07 |
| | Share Other | 0.05 | 0.02 | 0.03 | 0.38 |
| NUTS-3 | Share East | 0.34 | 0.46 | -0.12 | 0.17 |
| | Share South | 0.26 | 0.21 | 0.06 | 0.47 |
| | Share West | 0.39 | 0.33 | 0.06 | 0.49 |
| Migration background | Share Austria | 0.79 | 0.85 | -0.06 | 0.38 |
| | Share Other | 0.05 | 0.03 | 0.02 | 0.55 |
| | Share EU (West) | 0.05 | 0.08 | -0.03 | 0.52 |
| | Share EU (East) | 0.08 | 0.02 | 0.05 | 0.23 |
| | Share Turkey, Western Balkan | 0.03 | 0.01 | 0.01 | 0.67 |
| Household income quartile | Share Quartile 1 | 0.18 | 0.11 | 0.07 | 0.29 |
| | Share Quartile 2 | 0.21 | 0.16 | 0.05 | 0.50 |
| | Share Quartile 3 | 0.34 | 0.34 | 0.00 | 0.98 |
| | Share Quartile 4 | 0.26 | 0.38 | -0.12 | 0.13 |
| Urbanization | Share Cities | 0.16 | 0.28 | -0.12 | 0.08 |
| | Share Towns and suburbs | 0.32 | 0.41 | -0.09 | 0.26 |
| | Share Rural areas | 0.53 | 0.32 | 0.21 | 0.02 |
| Household size | Household size (count) | 2.55 | 2.68 | -0.13 | 0.55 |
| Is wealth dist. fair? | Very unfair | 0.58 | 0.58 | 0.00 | 0.98 |
| | Unfair | 0.29 | 0.35 | -0.07 | 0.42 |
| | Fair | 0.05 | 0.05 | 0.00 | 0.93 |
| | Very fair | 0.03 | 0.00 | 0.03 | 0.31 |
| | Dont know | 0.05 | 0.02 | 0.03 | 0.38 |

*Note:* This table presents the comparison of various demographic and socioeconomic variables between the treatment and control groups. The columns show the variable, measurement type, shares and mean values for treatment and control groups, the difference between these shares and means, and the p-value indicating the significance of the difference.

Table B2: Covariate Balance for Survey Respondents Control Group v. Population Sample

| Section | Description | Control | Population | Difference | p-value |
|---|---|---|---|---|---|
| Gender | Share Male | 0.41 | 0.48 | -0.07 | 0.02 |
| | Share Female | 0.58 | 0.51 | 0.07 | 0.02 |
| Age | Mean in years | 43.80 | 48.52 | -4.72 | 0.00 |
| | Share max 29 years | 0.17 | 0.18 | -0.01 | 0.88 |
| | Share 30-44 years | 0.39 | 0.25 | 0.15 | 0.00 |
| | Share 45-59 years | 0.25 | 0.27 | -0.03 | 0.42 |
| | Share 60+ years | 0.19 | 0.30 | -0.11 | 0.00 |
| Education | Share Max compulsory | 0.06 | 0.24 | -0.18 | 0.00 |
| | Share Apprenticeship | 0.16 | 0.32 | -0.16 | 0.00 |
| | Share Middle school | 0.11 | 0.14 | -0.03 | 0.18 |
| | Share High school | 0.17 | 0.15 | 0.01 | 0.77 |
| | Share Higher education | 0.50 | 0.14 | 0.36 | 0.00 |
| | Education missing | 0.00 | 0.00 | 0.00 | 0.32 |
| Employment | Share Unemployed | 0.02 | 0.04 | -0.02 | 0.16 |
| | Share In training | 0.12 | 0.05 | 0.07 | 0.00 |
| | Share Employed | 0.60 | 0.51 | 0.10 | 0.01 |
| | Share Self-employed | 0.09 | 0.06 | 0.04 | 0.94 |
| | Share Retired | 0.15 | 0.27 | -0.12 | 0.00 |
| | Share Other | 0.02 | 0.07 | -0.05 | 0.67 |
| | Employment missing | 0.00 | 0.02 | -0.02 | 0.00 |
| NUTS-3 | Share East | 0.46 | 0.44 | 0.02 | 0.61 |
| | Share South | 0.21 | 0.20 | 0.00 | 0.93 |
| | Share West | 0.33 | 0.35 | -0.02 | 0.65 |
| Migration background | Share Austria | 0.85 | 0.91 | -0.05 | 0.04 |
| | Share Other | 0.03 | 0.03 | 0.00 | 0.81 |
| | Share EU (West) | 0.08 | 0.04 | 0.04 | 0.08 |
| | Share EU (East) | 0.02 | 0.01 | 0.01 | 0.18 |
| | Share Turkey, Western Balkan | 0.01 | 0.01 | 0.00 | 0.91 |
| Household income quartile | Share Quartile 1 | 0.11 | 0.16 | -0.05 | 0.18 |
| | Share Quartile 2 | 0.16 | 0.17 | 0.00 | 0.90 |
| | Share Quartile 3 | 0.34 | 0.29 | 0.05 | 0.17 |
| | Share Quartile 4 | 0.38 | 0.38 | 0.00 | 0.73 |
| Urbanization | Share Cities | 0.28 | 0.22 | 0.06 | 0.38 |
| | Share Towns and suburbs | 0.41 | 0.34 | 0.07 | 0.85 |
| | Share Rural areas | 0.32 | 0.44 | -0.13 | 0.30 |
| Household size | Very unfair | 0.58 | 0.29 | 0.29 | 0.00 |

*Note:* This table presents the comparison of demographic and socioeconomic variables between the Control and Population groups. The columns show the variable, definition, shares and mean values for the Members and Population groups, and the difference between these shares and means.

Table B3: Covariate Balance for Survey Respondents Treatment Group v. Population Sample

| Section | Description | Treatment | Population | Difference | p-value |
|---|---|---|---|---|---|
| Gender | Share Male | 0.37 | 0.48 | -0.11 | 0.11 |
| | Share Female | 0.61 | 0.51 | 0.09 | 0.18 |
| Age | Mean in years | 49.05 | 48.52 | 0.53 | 0.95 |
| | Share max 29 years | 0.16 | 0.18 | -0.03 | 0.76 |
| | Share 30-44 years | 0.26 | 0.25 | 0.02 | 0.83 |
| | Share 45-59 years | 0.26 | 0.27 | -0.01 | 0.89 |
| | Share 60+ years | 0.32 | 0.30 | 0.02 | 0.87 |
| Education | Share Apprenticeship | 0.37 | 0.32 | 0.05 | 0.36 |
| | Share Middle school | 0.18 | 0.14 | 0.04 | 0.57 |
| | Share High school | 0.13 | 0.15 | -0.02 | 0.44 |
| | Share Higher education | 0.18 | 0.14 | 0.04 | 0.77 |
| | Education missing | 0.00 | 0.00 | 0.00 | 0.32 |
| Employment | Share Unemployed | 0.03 | 0.04 | -0.01 | 0.72 |
| | Share In training | 0.03 | 0.05 | -0.02 | 0.44 |
| | Share Employed | 0.53 | 0.51 | 0.02 | 0.75 |
| | Share Self-employed | 0.08 | 0.06 | 0.02 | 0.72 |
| | Share Retired | 0.29 | 0.27 | 0.02 | 0.85 |
| | Share Other | 0.05 | 0.07 | -0.02 | 0.45 |
| | Employment missing | 0.00 | 0.02 | -0.02 | 0.00 |
| NUTS-3 | Share East | 0.34 | 0.44 | -0.10 | 0.23 |
| | Share South | 0.26 | 0.20 | 0.06 | 0.47 |
| | Share West | 0.39 | 0.35 | 0.04 | 0.60 |
| Migration background | Share Austria | 0.79 | 0.91 | -0.12 | 0.09 |
| | Share Other | 0.05 | 0.03 | 0.02 | 0.48 |
| | Share EU (West) | 0.05 | 0.04 | 0.01 | 0.81 |
| | Share EU (East) | 0.08 | 0.01 | 0.07 | 0.13 |
| | Share Turkey, Western Balkan | 0.03 | 0.01 | 0.01 | 0.64 |
| Household income quartile | Share Quartile 1 | 0.18 | 0.16 | 0.02 | 0.57 |
| | Share Quartile 2 | 0.21 | 0.17 | 0.04 | 0.52 |
| | Share Quartile 3 | 0.34 | 0.29 | 0.05 | 0.51 |
| | Share Quartile 4 | 0.26 | 0.38 | -0.12 | 0.08 |
| Urbanization | Share Cities | 0.16 | 0.22 | -0.06 | 0.16 |
| | Share Towns and suburbs | 0.32 | 0.34 | -0.03 | 0.28 |
| | Share Rural areas | 0.53 | 0.44 | 0.08 | 0.04 |
| Household size | Household size (count) | 2.55 | 2.50 | 0.05 | 0.68 |

*Note:*
This table presents the comparison of various demographic and socioeconomic variables between the members group and the population sample. The columns show the variable, measurement type, shares and mean values for Members and Control groups, the difference between these shares and means, and the p-value indicating the significance of the difference.

# C   Selection of participants of the Good Council

10,000 randomly selected Austrian residents received an invitation (letter) to participate in the "Good Council". 1,424 of these residents expressed their interest in participating in the Good Council (see Table C1) [12]. The response rate of 14.2% was notably higher than the anticipated ±5%, as indicated by both national and international studies (Devillers et al., 2021; Jacquet, 2019; Jacquet, 2017b; Austria, 2021). Consequently, despite the absence of institutional ties, individuals were more than twice as likely to engage with the Good Council as compared to the government-initiated Council for Climate Action.

Table C1: Registration for the Good Council

| Invitations | Registrations | Response Rate % | Cancellations | % of Registrations |
|:---:|:---:|:---:|:---:|:---:|
| 10,000 | 1,424 | 14.2 | 16 | 1.1 |

*Note:* Data based on Haselmayer, 2024.

The short registration survey, as recommended in other studies (**oecd_innovative_2020**), collected sociodemographic information. Respondents were asked about their gender, education level, employment status, postal code (to determine geographic regions and urbanization levels), and household income (including household size and number of children to calculate per capita income). Importantly, it also included a question on the perceived fairness of the distribution of wealth in Austria. This question aimed at ensuring a diversity of opinions on the core topic of deliberation. Ultimately, nine variables with 35 categories were incorporated into the selection process.

Empirical evidence from other deliberative mini-publics (Ryfe and Stalsburg, 2012; Fournier et al., 2011) shows a significant correlation between the willingness to participate and socioeconomic status. A similar registration-bias exists for the Good Council. Individuals with higher education and above-average income were more inclined to register. Furthermore, attitudes towards wealth distribution played a significant role for the willingness to participate; those who believed that wealth was unevenly distributed in Austria were more likely to participate in the Good Council (Haselmayer, 2024). Despite the high overall response rate, the Good Council thus was subject to registration selectivity.

The second stage of the selection process was specifically designed to mitigate these biases. To achieve this, an algorithm commonly used for assembling mini-publics and as developed by Flanigan et al. (2021a) was employed. This approach aims at selecting a representative panel while maximizing the likelihood of each individual being included in the final group (the selected participants). This "LEXIMIN" algorithm requires a predefined set of selection variables and quotas, and it calculates various panel configurations that satisfy these quotas. The final selection is made through a random draw from the qualifying options (for further details, see Flanigan et al. (2021a)). This methodology,

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[12]Out of these, two individuals had to be removed from the pool due to incomplete survey responses. The statistical selection thus drew on 1,422 registrations.

combined with the substantial number of registered citizens, allowed the composition of the Good Council to closely reflect the actual demographics of the Austrian population across nine selected variables, ensuring fair opportunities for all interested participants.

In Table C2, the composition of the final panel is displayed, comparing the mean values of various categories between members of the panel and the general population, along with the differences.

Table C2: Balance Table Treatment Group v. Universe Austrian Residents

| Section | Description | Label | Treatment | Population | Difference |
|---|---|---|---|---|---|
| Gender | Share female | gender | 0.52 | 0.51 | 0.01 |
| Education | Share Max compulsory | edu_red1 | 0.18 | 0.24 | -0.06 |
| | Share Apprenticeship | edu_red2 | 0.34 | 0.31 | 0.03 |
| | Share Middle school | edu_red3 | 0.16 | 0.14 | 0.02 |
| | Share High school | edu_red4 | 0.14 | 0.16 | -0.02 |
| | Share Higher education | edu_red5 | 0.18 | 0.16 | 0.02 |
| Employment | Share Unemployed | employ_red1 | 0.04 | 0.04 | 0 |
| | Share In training | employ_red2 | 0.08 | 0.09 | -0.01 |
| | Share Employed | employ_red3 | 0.44 | 0.45 | -0.01 |
| | Share Self-employed | employ_red4 | 0.1 | 0.06 | 0.04 |
| | Share Retired | employ_red5 | 0.28 | 0.27 | 0.01 |
| | Share Other | employ_red6 | 0.06 | 0.09 | -0.03 |
| NUTS-3 | Share East | nuts31 | 0.44 | 0.44 | 0 |
| | Share South | nuts32 | 0.22 | 0.2 | 0.02 |
| | Share West | nuts33 | 0.34 | 0.36 | -0.02 |
| Migration background | Share Austria | mig_first_gen_red21 | 0.78 | 0.76 | 0.02 |
| | Share Other | mig_first_gen_red22 | 0.06 | 0.05 | 0.01 |
| | Share EU (West) | mig_first_gen_red23 | 0.04 | 0.04 | 0 |
| | Share EU (East) | mig_first_gen_red24 | 0.06 | 0.06 | 0 |
| | Share Turkey, Western Balkan | mig_first_gen_red25 | 0.06 | 0.08 | -0.02 |
| Household income quartile | Share Quartile 1 | inc_quart1 | 0.24 | 0.25 | -0.01 |
| | Share Quartile 2 | inc_quart2 | 0.24 | 0.25 | -0.01 |
| | Share Quartile 3 | inc_quart3 | 0.28 | 0.25 | 0.03 |
| | Share Quartile 4 | inc_quart4 | 0.24 | 0.25 | -0.01 |
| Urbanization | Share Cities | urb1 | 0.26 | 0.31 | -0.05 |
| | Share Towns and suburbs | urb2 | 0.28 | 0.3 | -0.02 |
| | Share Rural areas | urb3 | 0.46 | 0.38 | 0.08 |

*Note:* This table presents the comparison of demographic and socioeconomic variables between the Members of the Good council and the entire Austrian population (not only individuals in the random population sample). The columns show the variable, definition, shares and mean values, and the difference between these shares and means. Data sources: Population data: Statistics Austria (2021-2023).

# D   Response Rates by Group and Wave

Table D1 provides detailed results on survey participation rates for both members and the control group across waves. In the first wave, 94% of the treatment group and 77% of the control group participated in the survey. The surveys achieved a response rate of 78% for the treatment group and 73% for the control group among eligible participants who consented to the study in the second wave of the survey. In the follow-up wave, we reached 56% of the treatment group and 62.6% of the control group.

Table D1: Response Rates by Group and Wave

| Group | Wave 1 | Wave 2 | Wave 3 |
|---|---|---|---|
| Control | 235/305 (77.0%) | 224/305 (73.4%) | 191/305 (62.6%) |
| Treatment | 47/50 (94.0%) | 39/50 (78.0%) | 28/50 (56.0%) |

# E   The Treatment: Deliberating Inequality in the Good Council

This appendix provides a detailed overview of the treatment, namely participation in the Good Council for Redistribution. The Council consisted of 50 members who met across six weekends in Salzburg to deliberate on wealth inequality and the allocation of EUR 25 million in funds. Meetings were moderated by experienced facilitators of citizens assemblies and structured in collaboration with academic experts. Each weekend combined expert input, group deliberation, and plenary exchange, with the process gradually shifting from building a shared knowledge base to formulating concrete proposals and, ultimately, reaching consensus on the allocation of funds. The following summary outlines the central activities and discussions of each weekend.

- **Weekend 1:** Members were introduced to the purpose of the Good Council and got to know each other. Content included presentations by academic experiments on wealth inequality in Austria, the distribution of inheritances, and (wealth) taxation[13]. Core concepts such as income, wealth, redistribution via government spending and taxation, tax evasion and avoidance, were clarified. The weekend concluded with a "marketplace" session, where participants recorded impressions, open questions, and discussed initial redistribution ideas.

- **Weekend 2:** Deliberation deepened: participants discussed philosophical and normative perspectives on inequality, and together began to develop criteria for how to distribute the endowment.

- **Weekend 3:** Experts from academia and practice, in particular from NGOs, presented to the Council. Afterwards, members broke into thematic working groups based the six "fields of action" that emerged during prior discussion: economic policy, education & media, health & social policy, housing, environment & ecology, and civic participation.

- **Weekend 4:** The thematic working groups developed concrete proposals for redistribution and deliberate with the entire plenary regularly. Researchers provided background information as needed, and participants started to identify potential existing civil society organizations they could potentially partner-up with.

- **Weekend 5:** Participants refined their proposals and crafted a communication strategy to present the Councils decisions to the public. At that point, latest, it became clear that they were going to distribute the endowment towards already existing organizations.

- **Weekend 6:** The final weekend focused on decision-making. Members finalized fact sheets for each proposal,

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[13]These input lectures are available on the website: `https://guterrat.info/en/einblicke/`

deliberated within action groups, and reached consensus on how to allocate funds across chosen organizations. The process ended with public presentations.

# F  Description of Main Variables

## F.1  Main Outcome Variables

Table F1: Main Outcome Variables (Overview)

| Variable | Description | Source |
|---|---|---|
| Political efficacy | Normalized index (4 items) | Wave 2 |
| Political participation | Normalized index (5 items) | Wave 2 |
| Knowledge of wealth distribution | Normalized index (1 item) | Wave 2 |
| Support for wealth redistribution | Normalized index (1 item) | Wave 2 |
| Support for wealth taxation | Binary indicator (2 items) | Wave 2 |

1. **Political Efficacy:**

   **Q: To what extent do the following statements apply to you?**

   (a) I can understand and assess important political issues.

   (b) I feel confident participating in conversations about political topics.

   (c) Politics cares about what people like me think.

   (d) Politics tries to stay in close contact with the people.

   **Response options:** Fully agree, Rather agree, Neither-nor, Rather not agree, Don't agree, Don't know

2. **Political Participation:**

   **Q: There are various ways in which people can engage politically or advocate for an issue. In the past six months, have you**

   (a) participated in a demonstration or rally?

   (b) signed a petition, including online?

   (c) been active in a political party, association, or citizens initiative?

   (d) supported a cause or another person among friends or in your neighborhood?

   (e) liked, shared, or commented on political content online?

   **Response options:** Yes, No, Don't know

3. **Knowledge of the Wealth Distribution:**

   **Q: What do you estimate: What share of total wealth in Austria is owned by the richest 1% of households?**

   **Response options:** Under 10%, 10 - 20%, 21 - 30%, 31 - 40%, 41 - 50%, 51 - 60%, 61 - 70%, 71 - 80%, 81 - 90%, 91 - 100% of total wealth, Don't know

4. **Support for Wealth Redistribution:**

   **Q: And how much *should* the richest 1% of households own of Austrias total wealth?**

   **Response options:** Much less, Somewhat less, Same, Somewhat more, Much more, Don't know

5. **Support for Wealth Taxation:**

   **Q: In politics, various ideas for taxing wealth are being discussed. What do you think of the following proposals which would you support?**

   **Response options:** Tax all wealth, Wealth above EUR 50.000, Wealth above EUR 100.000, Wealth above EUR 500.000, Wealth above EUR 1.000.000, Wealth should not be taxed, Don't know

6. **Support for Inheritance Taxation:**

   **Q: And what is your opinion on inheritance taxes? Which proposal would you support?**

   **Response options:** Tax all inheritances, Inheritances above EUR 50.000, Inheritances above EUR 100.000, Inheritances above EUR 500.000, Inheritances above EUR 1.000.000, Inheritances should not be taxed, Don't know

## F.2 Stratification and Control Variables

1. **Perceived Fairness of Wealth Distribution:**

   **Q: What do you think: How fair is the current distribution of wealth in Austria? (Wealth includes, for example, real estate, cash, savings, stocks and bonds, jewelry, art collections, etc.)**

   **Response options:** Very fair, Rather fair, Rather unfair, Very unfair, Dont know.

Table F2: Stratification and Control Variables

| Variable | Categories | Source |
|---|---|---|
| Gender | Men, women, diverse | Pre-survey |
| Age group | 16–29, 30–44, 45–59, 60+ | Pre-survey |
| Educational attainment | Compulsory, apprenticeship, BMS, high school, university | Pre-survey |
| Employment status | Unemployed, education, employed, self-employed, retired, other | Pre-survey |
| Region | East, South, West | Pre-survey |
| Migration background | None, Other, EU West/East, Turkey/Balkans | Pre-survey |
| Income quartile[1] | 1st to 4th quartile | Pre-survey |
| Equivalence scale[1] | Square root Equivalence scale | Pre-survey |
| Urban-rural region | City, town/suburb, rural | Pre-survey |
| Perceived Fairness | Normalized index (1 item) | Pre-survey |

[1] Disposable household income unadjusted for size. Quantiles are constructed using quantile thresholds from the EU-SILC, provided by Austria (2023). Missing values interpolated using income and education.

# G    Opinion Polarization

In addition to regression-based inference, we employ weighting methods to estimate treatment effects beyond the mean. This is necessary to quantify differences between treatment and control groups in terms of opinion polarization. The weights used in this study are Horvitz–Thompson weights defined on all participants in the experiment, but not on the representative population sample (Horvitz and Thompson, 1952). Let $e(x)$ be the probability of participating in the Good Council given $\mathbf{X_i} = x$ conditionally on registering interest. We estimate these probabilities using a logit model. Then, the weights for treatment and control groups denoted by $\omega_1$ and $\omega_0$ respectively are as follows:

$$\omega_1 = \frac{1}{e(x)} \quad \omega_0 = \frac{1}{1 - e(x)} \tag{1}$$

The validity of the reweighting approach relies on common support. The tables A1 and A2 in the Appendix show that the control group features observations in each category for discrete variables, and suggests that differences in averages for continuous variables are small. To obtain valid standard errors, we bootstrap after weighting.

Table G1 explores the polarization of preferences on wealth and inheritance taxation in more detail. In contrast to the measure in Table 4, we do not summarize different response options, but maintain the policy options reported on the x-axis in Figure 4. Rather than comparing differences in share of individuals who favor the most popular policy between treatment and control groups, we compute indicators of inequality, polarization and agreement for the treatment and control group separately. As detailed in Subsection 4.5,

we compare the indicators between groups only after balancing to adjust for differences in observables.

Table G1 suggests that the distribution of responses is much more compressed in the treatment group than it is in the control group, across different measures and irrespective of whether we consider wealth or inheritance taxation. The Gini index of responses is 0.079 in the treatment group for wealth taxation, and 0.183 in the control group. The bootstrapped standard errors suggest that this difference is significant. The same holds for inheritance taxation, where the index corresponds to 0.132 (0.24) in the treatment (control group). As the Gini index is not an ideal measure for ordinal data, we proceed by considering the Esteban Ray polarization measure (Esteban and Ray, 1994). Polarization of tax preferences is much lower in the treatment group for wealth taxation (0.132) than in the control group (0.283). Inheritance taxation is slightly more divisive in the treatment group (0.196) than wealth taxation, although polarization is lower than in the control group (0.290). Finally, we turn to the van der Eijk measure of agreement, which is another indicator that is suitable for ordinal data (Eijk, 2001). In contrast to the Gini index and the Esteban Ray polarization measure, this indicator increases the more individuals agree. As a result, we observe a higher value in the treatment group for both wealth taxation (0.902) and inheritance taxation (0.799) than in the control group (0.586 and 0.435 respectively). The changes in each measure are consistent with the previous findings in Table 4 that are based on whether individuals agree with the most popular model in their group.

Table G1: Polarization in views on tax policy

| Polarization measure | Wealth tax | | Inheritance tax | |
|---|---|---|---|---|
| | Treatment | Control | Treatment | Control |
| Gini | 0.079 | 0.183 | 0.132 | 0.240 |
| | (0.021) | (0.010) | (0.031) | (0.012) |
| Esteban Ray polarization | 0.132 | 0.283 | 0.196 | 0.290 |
| | (0.091) | (0.009) | (0.080) | (0.010) |
| Van der Eijk agreement | 0.902 | 0.586 | 0.799 | 0.435 |
| | (0.063) | (0.032) | (0.089) | (0.045) |

*Note:*
Support for different models of wealth and inheritance taxation. Inverse probability weighting used for covariate balancing. Bootstrapped standard errors (1000 iterations) in parenthesis. For Esteban and Ray (1994) Polarisation measure, alpha and beta are set to unity.

# H  Differences-in-Differences

We supplement our estimates of the treatment effect in Section 5 with an analysis of the changes in outcomes. This follows a Differences-in-Differences (DiD) approach, comparing the changes in outcomes between the first meeting of the Good Council and the end of deliberation across the treatment and control group. We add a post treatment variable $P_t$, which we interact with the treatment variable $T_i$, which yields Equation 1:

$$Y_{i,t} = \gamma_0 + \gamma_1 P_{i,t} + \gamma_2 T_{i,t} + \gamma_3 (P_{i,t} \times T_{i,t}) + \mathbf{X_{i,t}}\gamma + \zeta_{i,t} \tag{1}$$

The other variables in the specification remain identical to Equation 3.

Table H1 reinforces the results in Table 4. The table refers to the specification detailed in Equation 1, drawing on data on outcomes collected before and after the Good Council convened and concluded. The coefficient reported as "Member*W2" refers to $\gamma_3$, while the "Member" row is the estimate of $\gamma_2$. Again, control variables are dropped from the table, though they feature in all specifications. The number of observations fluctuates, due to the dropping of observations that do not answer the question or choose "Don't know".

Table H1: DiD results W1 and W2

| | Political efficacy | Political participation | Knowledge wealth distr. | More wealth redist. | Support W u. IH tax | Preference concentration |
|---|---|---|---|---|---|---|
| | *Dependent variable:* | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Member | 0.059 | −0.177** | −0.145 | −0.110* | −0.004 | −0.303** |
| | (0.048) | (0.089) | (0.136) | (0.063) | (0.069) | (0.135) |
| Member*W2 | −0.060* | 0.037 | 0.213** | 0.079** | 0.005 | 0.354*** |
| | (0.032) | (0.059) | (0.092) | (0.035) | (0.043) | (0.083) |
| Observations | 536 | 496 | 527 | 525 | 538 | 554 |
| $R^2$ | 0.241 | 0.175 | 0.128 | 0.165 | 0.159 | 0.182 |

*Note:* The table reports average total effects from comparing changes within the treatment and population groups before and after deliberation. Estimates use the specification in Section H, which includes covariates used as stratification variables and control variables, both specified in Table F2. Table report OLS estimates for continuous outcomes on standardized outcome variables. See Section F for outcome definitions of columns 1–5 and Section G for column 6. Standard errors in parentheses: *p<0.1; **p<0.05; ***p<0.01.

The difference-in-differences estimate of the treatment effect on political efficacy is negative and statistically significant at the 10% level (-0.06, p<0.1). This is in accordance with the findings reported in Table 4. Rather than empowering participants, the assembly experience appears to have modestly decreased their sense of

political agency.

The second column refers to political participation. The difference-in-differences analysis indicates a slight positive effect of participation (0.037), although this effect lacks statistical significance. While participants showcase higher overall participation, being part of the assembly did lead to higher levels political participation in other dimensions. However, the timing of measurement may matter since participants are already engaged in a form of political participation through the assembly, potentially showing reduced activity in other forms during this period.

The third column appraises the treatment effect on knowledge about wealth inequality comparing the treatment and the control groups over time. The difference-in-difference analysis shows that participation in the Good Council substantially increased knowledge, an effect that is highly significant (0.213, $p<0.05$). These findings broadly align with our results reported in Section 5.1.

The fourth column investigates changes in support for a more egalitarian wealth distribution between participants and the control group over time. The difference-in-difference analysis shows participation increased support for egalitarian distribution (0.079, $p<0.05$) significantly, although the effect size is marginal. The fifth column refers to changes in support for wealth and inheritance taxes. Being part of the assembly did not lead to increased support for wealth and inheritance taxes.

Finally, the sixth column estimates changes in the polarization of views on wealth and inheritance taxation between the treatment and the control group. The difference-in-difference estimate reinforces the results in the main analysis, as polarization falls dramatically. The difference-in-difference analysis shows a strong decrease in dispersion (0.354), which is highly statistically significant ($p<0.01$). This estimate refers to a significantly higher share of individuals in the treatment group who agree with the majority of their peers on capital taxation.

# I   Additional Descriptive Statistics and Results

## I.1   Results with Complete Cases

Table I1: Main results W2: Complete cases

| | Political efficacy | Political participation | Knowledge wealth distr. | More wealth redist. | Support W u. IH tax | Preference concentration |
|---|---|---|---|---|---|---|
| | *Dependent variable:* | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Member | −0.059** | −0.069 | 0.267*** | 0.034* | −0.020 | 0.363*** |
| | (0.030) | (0.051) | (0.074) | (0.020) | (0.041) | (0.069) |
| Mean dep. var (ref group) | 0.47 | 0.50 | 0.14 | 0.08 | 0.95 | 0.41 |
| N Control | 176 | 176 | 176 | 176 | 176 | 176 |
| N Treatment | 32 | 32 | 32 | 32 | 32 | 32 |
| $R^2$ | 0.298 | 0.207 | 0.228 | 0.168 | 0.236 | 0.273 |

*Note:* The table reports average total effects from comparing treatment and population groups. We drop all obsvertations with missing or "Don not know" answers on any question. Estimates use the baseline specification in Section 4.5, which includes covariates used as stratification variables and control variables, both specified in Table F2. Table report OLS estimates for continuous outcomes on standardized outcome variables. Outcomes are measured in the post treatment survey following the Good Council. See Section F for outcome definitions of columns 1–5 and Section G for column 6. Standard errors in parentheses: *p<0.1; **p<0.05; ***p<0.01.

## I.2 Results by Component: Political Efficacy & Political Participation

Figure I1 plots two of our main outcomes – political efficacy and political participation – by component. The figure is based on Table I2 and Table I3.
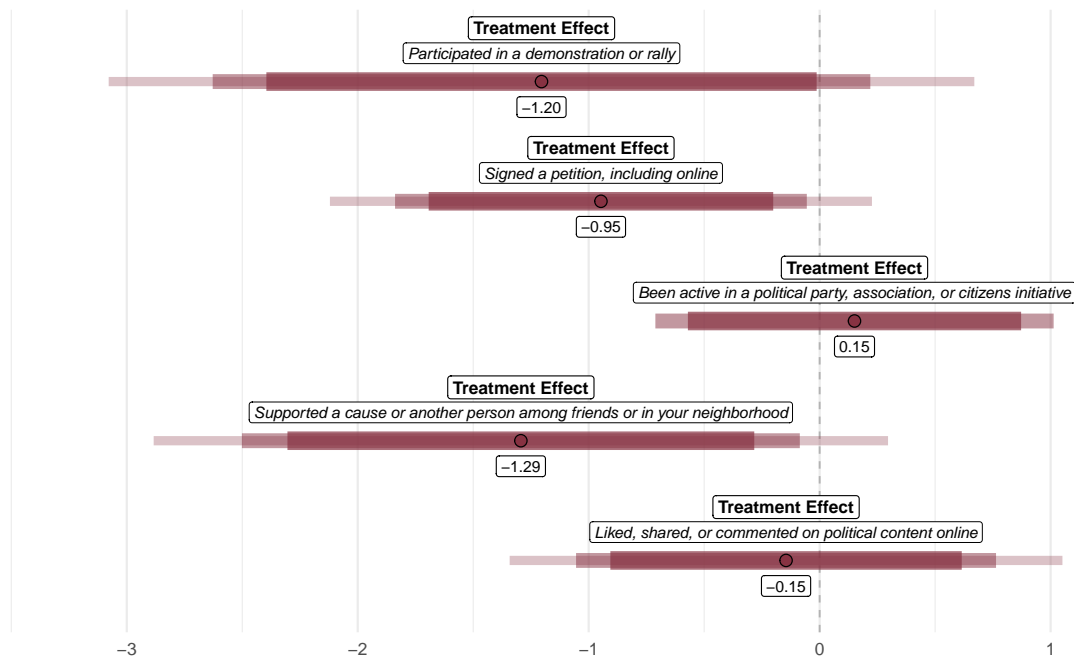
Figure I1: Disaggregated Effects of Deliberative Participation on Council Members' (Perceived) Role in Democracy.

**Detailed Results: (Perceived) Role in Democracy**

**Political Efficacy**
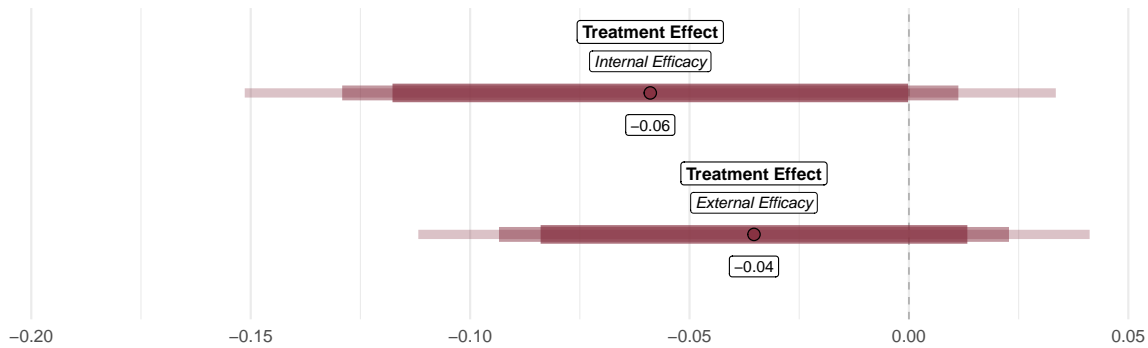


**Political Participation**



Estimates for Political Participation are log–odds coefficients from logistic regression models.

Confidence intervals at 99%, 95%, and 90%

*Note:* The plot shows the average treatment effect (treatment vs. control group) on internal and external efficacy, as well as on five different forms of political participation. The underlying models can be found in Table I2 and Table I3 in the Appendix.

Table I2: Political efficacy by component

| | Internal efficacy (W2) | External efficacy (W2) | Internal efficacy (DiD) | External efficacy (DiD) |
|---|---|---|---|---|
| | *Dependent variable:* | | | |
| | (1) | (2) | (3) | (4) |
| Member | −0.059* | −0.035 | −0.014 | 0.124** |
| | (0.036) | (0.030) | (0.059) | (0.059) |
| Member*W2 | | | −0.028 | −0.084** |
| | | | (0.041) | (0.038) |
| Observations | 239 | 240 | 544 | 544 |
| $R^2$ | 0.273 | 0.211 | 0.227 | 0.173 |
| Adjusted $R^2$ | 0.164 | 0.093 | 0.177 | 0.119 |
| Residual Std. Error | 0.191 (df = 207) | 0.178 (df = 208) | 0.183 (df = 510) | 0.170 (df = 510) |
| F Statistic | 2.505*** (df = 31; 207) | 1.789*** (df = 31; 208) | 4.546*** (df = 33; 510) | 3.233*** (df = 33; 510) |

*Note:* The table reports average total effects from comparing treatment and population groups. Estimates use the baseline specification in Section 4.5 in columns 1–2 and the specification in Section H in columns 3–4. Both include covariates used as stratification variables and control variables, specified in Table F2. Table report logit estimates for binary outcome variables. Outcomes are measured in the post treatment survey following the Good Council in columns 1–2 and in the pre and post treatment survey before and after deliberation in columsn 3–4. See Section F for outcome definitions of columns 1–4 Standard errors in parentheses: *p<0.1; **p<0.05; ***p<0.01.

Table I3: W2: Political participation by component (Logistic regressions)

| | Demonstration | Collect signature | Pol. orga member | Active for sth or sb | Internet |
|---|---|---|---|---|---|
| | *Dependent variable:* | | | | |
| | (1) | (2) | (3) | (4) | (5) |
| Member | −1.204* | −0.947** | 0.151 | −1.294** | −0.145 |
| | (0.726) | (0.455) | (0.440) | (0.616) | (0.464) |
| Observations | 238 | 236 | 240 | 233 | 239 |
| Log Likelihood | −99.207 | −147.459 | −126.794 | −68.141 | −140.332 |
| Akaike Inf. Crit. | 262.413 | 358.917 | 317.589 | 200.281 | 344.664 |

*Note:* The table reports average total effects from comparing treatment and population groups. Estimates use the baseline specification in Section 4.5, which includes covariates used as stratification variables and control variables, both specified in Table F2. Table report logit estimates for binary outcomes variables. Outcomes are measured in the post treatment survey following the Good Council. See Section F for outcome definitions of columns 1–5. Standard errors in parentheses: *p<0.1; **p<0.05; ***p<0.01.

## J  Long-term outcomes

To explore the lasting effects of participation in the citizens' assembly, we conducted a third wave of the survey in March 2025, approximately eight months after the Good Council ended. The treatment and control groups report similar levels of political efficacy. External efficacy is substantially higher than internal efficacy, with only slight differences between the treatment and control groups (see Figure J1). In the follow-up wave, the treatment group was more likely to have participated in demonstrations and political groups, while the control group reported higher levels of online participation and petition signing (see Figure J2). Knowledge about wealth distribution remained elevated in the treatment group compared to the control group, suggesting that informational gains persisted over time (see Figure J3). Support for a more egalitarian wealth distribution was equally high among both groups (see Figure J4). Preferences regarding tax thresholds continued to center around the EUR 1 million allowance for both wealth and inheritance taxes in the treatment group, while the control group exhibited more dispersed preferences regarding tax design (see Figure J5 and Figure J6). Table J1 replicates the analysis in Table 4 for the third wave and compares the treatment to the control group. The table supports the conclusion that it was knowledge about the wealth distribution and the convergence to a specific tax policy model that remains in the long run.

Table J1: Main results W3

|  | Dependent variable: | | | | | |
| --- | --- | --- | --- | --- | --- | --- |
|  | Political efficacy | Political participation | Knowledge wealth distr. | More wealth redist. | Support W u. IH tax | Preference concentration |
|  | (1) | (2) | (3) | (4) | (5) | (6) |
| Member | 0.049 | −0.084 | 0.163* | 0.007 | 0.006 | 0.314*** |
|  | (0.048) | (0.067) | (0.094) | (0.029) | (0.041) | (0.086) |
| Mean dep. var (ref group) | 0.60 | 0.52 | 0.10 | 0.04 | 0.94 | 0.48 |
| N Control | 167 | 167 | 160 | 161 | 153 | 153 |
| N Treatment | 25 | 25 | 21 | 21 | 21 | 21 |
| $R^2$ | 0.242 | 0.245 | 0.150 | 0.400 | 0.190 | 0.181 |

*Note:* The table reports average total effects from comparing treatment and population groups. Estimates use the baseline specification in Section 4.5, which includes covariates used as stratification variables and control variables, both specified in Table F2. Table report OLS estimates for continuous outcomes on standardized outcome variables. Outcomes are measured in the follow-up survey in March 2025. See Section F for outcome definitions of columns 1–5 and Section G for column 6. Standard errors in parentheses: *$p<0.1$; **$p<0.05$; ***$p<0.01$.

## Figure J1: Political Efficacy at Follow-Up Wave
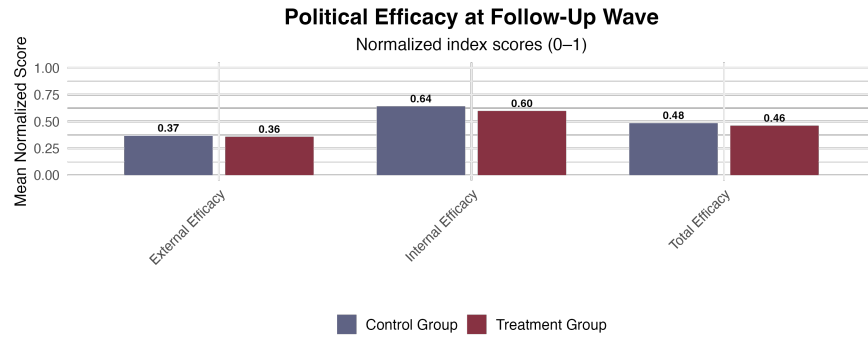


The plot displays normalized mean scores (0–1) for internal, external, and total political efficacy for both the treatment and the control group. Treatment Group (Council Members): 22 respondents; Control Group (Interested Invitees): 191 respondents.

## Figure J2: Political Participation at Follow-Up Wave
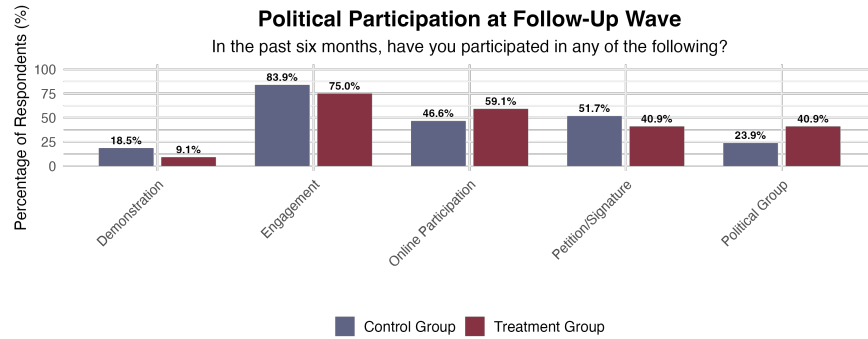


The plot shows mean values for both the treatment and the control group across different forms of political participation. Treatment Group (Council Members): 22 respondents; Control Group (Interested Invitees): 191 respondents.

## Figure J3: Knowledge of Wealth Distribution at Follow-Up Wave
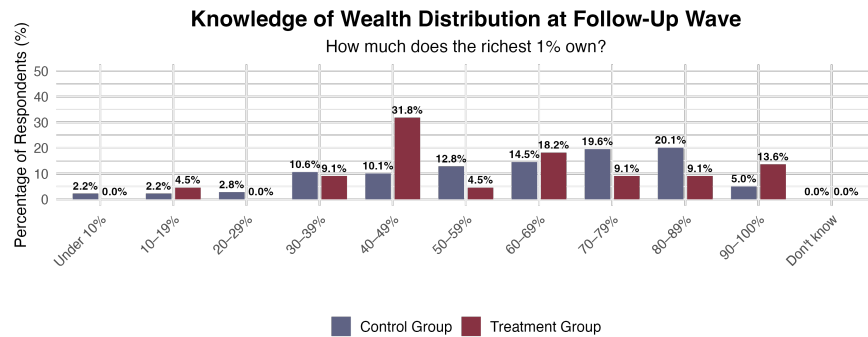


Respondents were asked: "How much does the richest 1% own?". Bars show the share of respondents choosing each category, separately for the treatment group and the control group. Treatment Group (Council Members): 22 respondents; Control Group (Interested Invitees): 181 respondents.

Figure J4: Support for Egalitarian Distribution at Follow-Up Wave



**Support for Egalitarian Distribution at Follow-Up Wave**
How much SHOULD the richest 1% own?
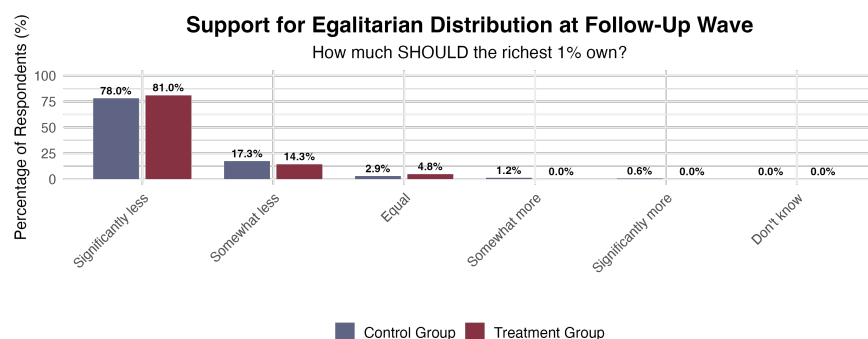
Respondents were asked: "How much *should* the richest 1% own?". Bars show the share of respondents choosing each category, separately for the treatment group and the control group. Treatment Group (Council Members): 21 respondents; Control Group (Interested Invitees): 173 respondents.

Figure J5: Preferences for Tax Reforms at Follow-Up Wave, Proposal: Wealth Tax



**Preferences for Tax Reforms at Follow-Up Wave**
Proposal: Wealth Tax
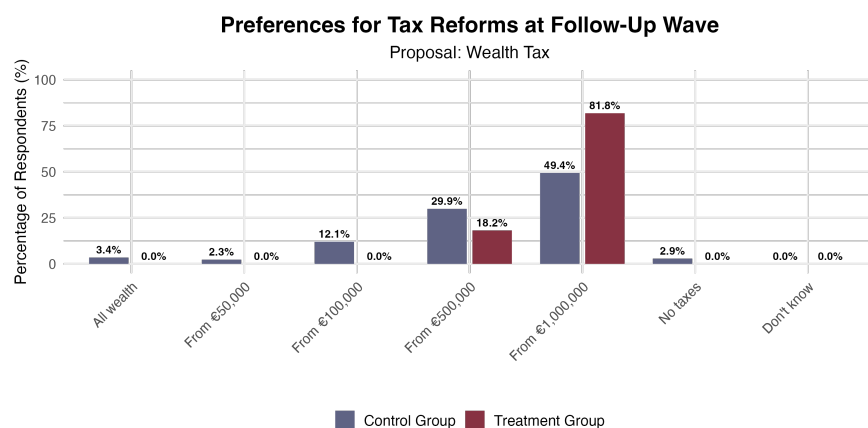
Respondents indicated their preferred wealth tax proposal. Bars show the share of respondents choosing each option, separately for the treatment group and the control group. Treatment Group (Council Members): 22 respondents; Control Group (Interested Invitees): 174 respondents.

Figure J6: Preferences for Tax Reforms at Follow-Up Wave, Proposal: Inheritance Tax

**Preferences for Tax Reforms at Follow-Up Wave**

Proposal: Inheritance Tax
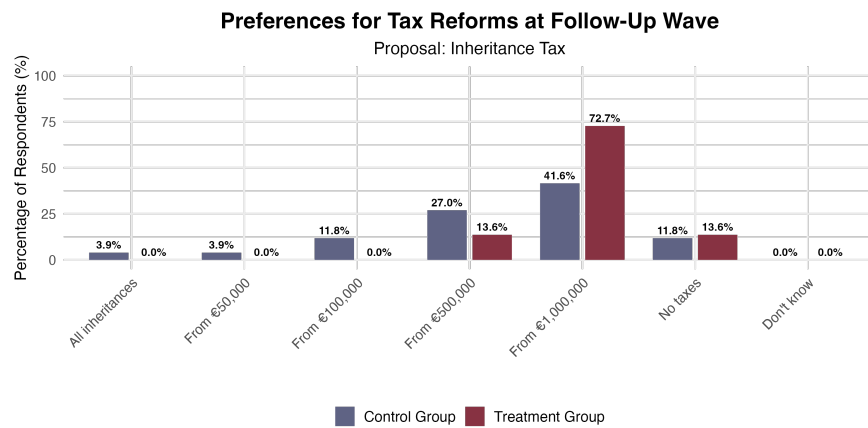


Respondents indicated their preferred inheritance tax proposal. Bars show the share of respondents choosing each option, separately for the treatment group and the control group. Treatment Group (Council Members): 22 respondents; Control Group (Interested Invitees): 178 respondents.

# K   Placebo analysis

Table K1: Placebo outcomes W2

| | Carbon tax | Unemployment benefit | | Tuition fee |
|---|---|---|---|---|
| | *Dependent variable:* | | | |
| | (1) | (2) | | (3) |
| Member | 0.057 | 0.062 | | 0.042 |
| | (0.038) | (0.049) | | (0.036) |
| | | | | |
| Observations | 241 | 241 | | 241 |
| $R^2$ | 0.141 | 0.160 | | 0.218 |
| Adjusted $R^2$ | 0.014 | 0.036 | | 0.102 |

*Note:* The table reports average total effects from comparing treatment and population groups. Estimates use the baseline specification in Section 4.5, which includes covariates used as stratification variables and control variables, both specified in Table F2. Table report OLS estimates for continuous outcomes on standardized outcome variables. Outcomes are measured in the post treatment survey following the Good Council. Standard errors in parentheses: *$p<0.1$; **$p<0.05$; ***$p<0.01$.