

Why Don't Donors Deduct? Social Norms and the Limits of Tax Incentives*

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Abstract

Many Austrian donors leave tax benefits unclaimed even when doing so requires minimal effort and yields meaningful financial rewards. Qualitative findings from a representative survey point to confusion about how to access these benefits and to misperceived social norms on the moral appropriateness of deducting donations as the main drivers of this gap. We test concise information on deductibility and a one-sentence norm cue in an online experiment ($n = 483$), a door-to-door field experiment with address-level randomization ($n = 6,728$), and a radio-based campaign spanning two Austrian federal states. We find that almost all donors deduct when donating through the anonymous online tool. By contrast, during face-to-face fundraising, where social-image concerns are salient, fewer than 1 in 100 donors choose to do so. Across settings, information on deductibility alone leaves deduction unchanged, whereas adding the norm cue increased take-up by 0.36 standard deviations in the door-to-door setting. Our findings show that financial incentives can falter when clashing with misperceived norms in social settings, unless paired with campaigns that reshape those norms.

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1 Introduction

In 2017, Austria massively simplified the deduction process by introducing automatic electronic reporting of donations. Previously, donors had to request a receipt from the charity and manually include it in their tax return. Since the reform, charities are required to collect identifying information from deducting donors and transmit it to the tax authority, which automatically prefills the deduction in taxpayers' returns. Yet, despite this simplification, the share of taxpayers deducting donations rose only by 1.8 percentage points (from 19.8% to 21.6%), leaving roughly four-fifths of taxpayers still foregoing the benefit. The 2017 Austrian tax reform provides a rare opportunity to study the role of altruistic motivations and the limits of tax incentives when administrative barriers are removed almost entirely.

This limited response raises a broader concern. Tax incentives are a central tool for policymakers to steer taxpayer behavior toward socially desirable ends, including charitable giving. By lowering the effective cost of donations, governments aim to encourage greater support for causes deemed of public value.¹ Yet when such incentives clash with misperceived norms in socially observable settings, take-up can remain low even when the administrative cost is near zero. Understanding this interaction is essential for designing effective policy.

A complementary and equally puzzling pattern emerges when comparing deduction rates across donation channels for one of Austria's largest and best-established charities. Among anonymous online donors, 86% claim the tax incentive. In contrast, during a nationwide door-to-door campaign where donors interact directly with local volunteers, only 0.7% claim the deduction, even though donations frequently exceed 50 EUR. This two-orders-of-magnitude gap underscores the depth of the puzzle and suggests that the determinants of deduction behavior extend beyond filing costs.

Observational data alone cannot resolve this puzzle. Even with rich administrative records, disentangling mechanisms is impossible because of persistent concerns about selection and endogeneity. The 2017 reform illustrates this limitation: it lowered filing costs but did not reveal why deduction rates remain so low or why take-up differs so starkly across contexts. Such aggregate shocks only reveal average changes and cannot identify underlying mechanisms. To identify causal effects, we implement a sequence of experiments that systematically vary key factors and allow us to isolate how each influences deduction behavior. Field experiments are especially valuable in this regard. They allow us to manipulate single features of the natural decision environment, such as information or norm cues, while holding all other aspects constant.

Our paper makes five main contributions. First, we provide the first systematic evaluation of Austria's 2017 reform that automated the reporting of charitable donations. Despite its ambition to eliminate filing costs, the reform increased deduction rates by less than 2 percentage points, from 19.8% to 21.6%. Second, we document a striking puzzle: while the reform barely moved deduction rates, we observe a two-order-of-magnitude difference between anonymous online donations and socially observable donations in the field. Third, we provide novel evidence on the mechanisms behind this puzzle by combining administrative data with a representative survey that uncovers large knowledge gaps and systematic misperceptions of social norms. Fourth, we design and implement a sequence of field, online, and laboratory experiments that allow us to causally identify the role of these frictions, varying both the availability of procedural information and the activation of social norms through visibility. Finally, our findings speak to broader debates on the limits of financial incentives when they clash with social norms, highlighting the need for complementary norm-shaping

¹See, for example, Republik Österreich (2023) for Austria, Joint Committee on Taxation (2013) for the United States, HM Treasury (2015) for the United Kingdom, République Française (2003) for France, and Deutscher Bundestag (2007) for Germany.

policies in domains of public interest.

We explore two main explanations for this puzzle, both suggested by prior work on taxation and charitable giving: (i) donors often lack information about how to deduct, and (ii) behavioral barriers, such as social-image concerns and misperceived norms, limit the effectiveness of monetary incentives. Lack of procedural knowledge is a well-documented friction in tax settings, often reducing the effectiveness of policy incentives. Taxpayers frequently misperceive key features of the tax code—for example, they overestimate the share of individuals subject to top rates or misunderstand the degree of progressivity (Stantcheva 2021). Insufficient knowledge has also been shown to constrain take-up of tax benefits, while simplified or more accessible information can significantly improve it (Bhargava and Manoli 2015; Chetty et al. 2013; Chetty and Saez 2013).

A complementary literature highlights the role of social norms and image concerns. Misperceptions about prevailing norms can directly shape behavior, as recent work shows (Bursztyn and Yang 2022). Such mechanisms operate across domains — from wage aspirations (Bursztyn et al. 2017) and educational effort (Bursztyn et al. 2019) to voting (DellaVigna et al. 2016). Charitable giving, in particular, is highly sensitive to observability and solicitation: donors avoid fundraisers and respond strongly to visibility (Andreoni et al. 2017; DellaVigna et al. 2012). These patterns are consistent with models of signaling and identity (Bénabou and Tirole 2006, 2011), and with recent evidence on reputational returns to prosocial acts (Exley 2018). Together, this evidence shows that informational frictions and social norms can strongly shape the extent to which individuals respond to policy incentives.

We begin by examining taxpayers’ knowledge and perceptions through a representative survey, which informs the design of our interventions. We conducted a quota-based representative survey in Austria to establish baseline knowledge of the deduction system and perceptions of relevant norms. Respondents reported what qualifies for deduction (including donations and eligible charities), what steps donors must take, how morally appropriate they find deducting donations, and how they believe others view it. They were also asked to provide brief open-text reasons for and against deducting.

Two patterns emerge. First, more than 60% lack essential procedural knowledge required to deduct. Second, we observe a large gap between respondents’ beliefs and reality: over 75% think fewer people view deducting as morally appropriate than is actually the case. By contrast, we find little evidence that privacy concerns about sharing personal data or views about the government’s use of non-claimed resources play a major role. These diagnostics align with the literature on tax misperceptions (Stantcheva 2021) and pluralistic ignorance (Bursztyn and Yang 2022) and directly inform our interventions.

We use these survey insights to design two treatments that we implement across all subsequent experiments. The *Information* treatment provides clear instructions on how to deduct, while the *Morality* treatment combines the same instructions with a short norm cue about others’ moral views on deducting. This staggered design enables us to distinguish between responses driven by closing the knowledge gap and those arising from correcting misperceived norms.

We test the role of knowledge gaps and misperceived norms in a socially observable setting that covers a broad donor population and minimizes selection concerns. In collaboration with a large Austrian charity, we delivered short written treatment texts prior to a nationwide door-to-door event. Randomization was implemented at two levels: address-level assignment in three municipalities, covering 6,728 treated addresses, and municipality-level assignment in another 29 municipalities. At the address level we do not detect significant effects, while at the municipality level we find that providing a norm cue increased the number of deductors by about 7.6 per treated ZIP code, corresponding to an effect size of 0.36 standard deviations. Procedural information alone had no measurable effect. This demonstrates

that in a natural, high-visibility setting with active social norms, providing moral norm cues to correct misperceptions can shift take-up, while procedural information by itself does not.

To reach a broader population and assess the role of treatment timing and delivery medium, we conducted a second field experiment one year later with the same partner organization. We delivered treatments via radio advertisements, broadcast four times on the same day across two stations in each of two federal states. The broadcast day coincided with the median day of deduction activity as observed in previous years. Based on station reports, the two stations in Upper Austria (*Morality* treatment) reach a daily audience of about 567,000 listeners, while those in Carinthia (*Information* treatment) reach about 558,000 listeners. Outcomes are constructed from charity administrative data aggregated at the ZIP level. We find no statistically significant effect of either treatment. Compared to Field Experiment 1, the use of a federal state-level medium rather than local outlets might have made norm cues less salient, as individuals might primarily be concerned about the views of their local peers. In addition, legal constraints prevented us from mentioning the partner charity in the spots, which may have further diluted treatment strength.

To study the underlying behavior and treatment effects in a comparable, yet fully anonymous setting, we implemented an online experiment with Austrian and German participants. Participants earned money through real effort, were taxed, and could donate part of their earnings to real charities, some of which were deductible. They then faced a deduction decision. The baseline treatment yields a very high deduction rate of 85%, closely matching our charity partner’s online donation data and alleviating concerns that high online take-up reflects selection bias. Relative to this baseline, we find no significant treatment effects. This shows that when social norms are deactivated by anonymity, deduction rates are high and stable—replicating the stark gap between anonymous and observable settings.

To causally vary the degree to which social norms are active, we designed a laboratory experiment that randomized the visibility of deduction decisions. Participants earned money through real effort, faced income taxation, could donate to a real charity, and then made a deduction choice. In the *Anonymity* condition, deduction was made privately, whereas in the *Observability* condition, the decision was made under the gaze of an experimenter or lab assistant standing nearby. In both cases, other participants never had access to another’s decision, ensuring that only experimenters could observe choices when assigned to *Observability*. The sample comprises 473 participants. Being observed reduces deduction by about 12 percentage points, but the study is not powered to detect effects smaller than 23 percentage points at conventional significance levels. Open-ended responses describing deduction choices point in the same direction. Taken together, these findings provide suggestive evidence that varying visibility can activate or deactivate the influence of social norms on the use of tax incentives.

Taken together, our experiments show that procedural instructions alone leave deduction behavior unchanged. By contrast, moral norm cues can shift take-up when social norms are active in observable settings. High deduction rates in anonymous online contexts confirm that selection is not driving the puzzle, while the laboratory experiment—where other participants are present in the same room but decisions can be shielded—yields intermediate deduction rates around 53%. This pattern underscores the central role of visibility in activating norms. By combining field, online, and laboratory evidence, we triangulate mechanisms across complementary settings and provide causal evidence that misperceived norms—not information frictions alone—explain why financial incentives often fail to deliver their intended effect.

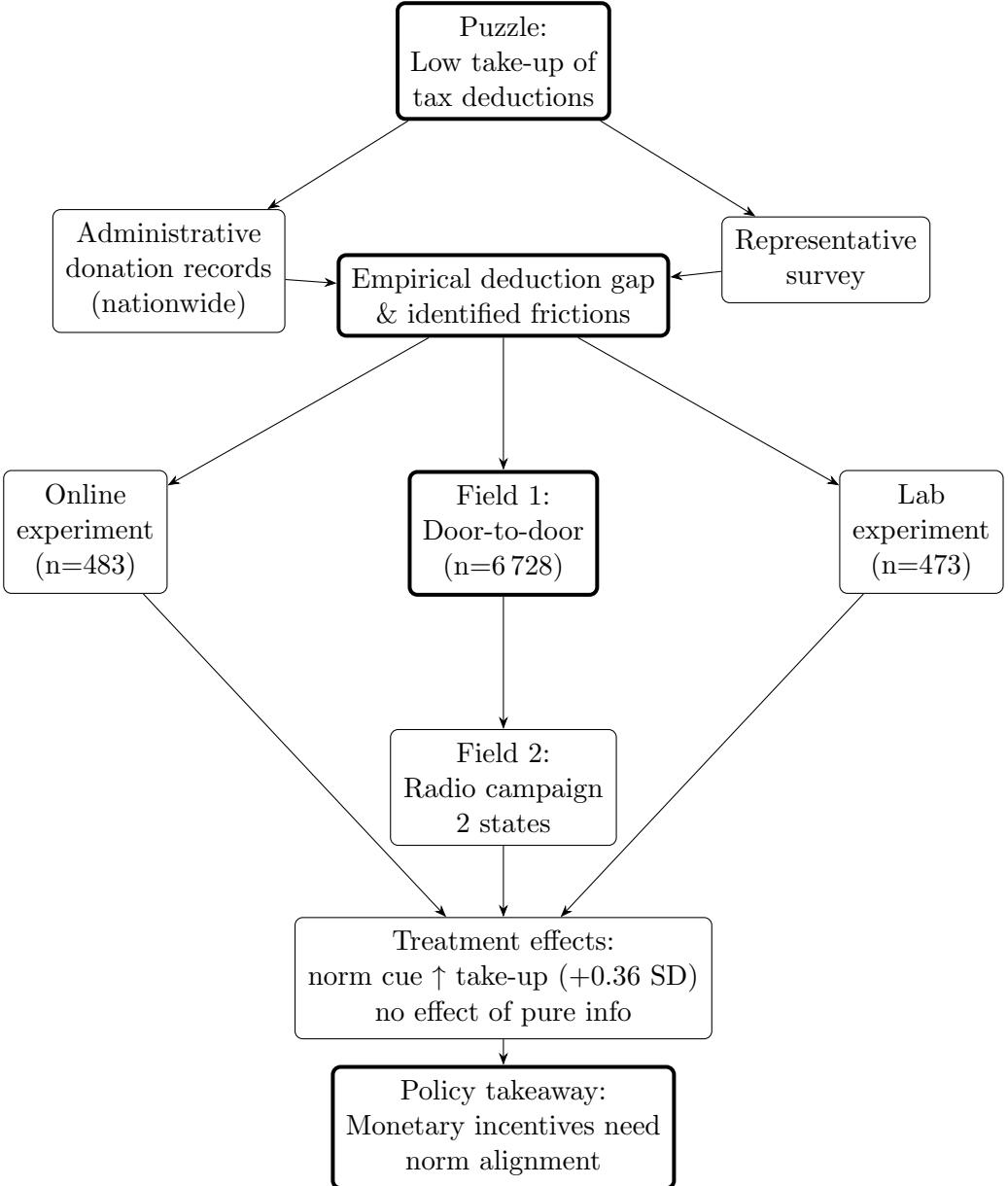


Figure 1: Overview of empirical and experimental components.

2 Empirical Evidence on Deduction Behavior and Underlying Mechanisms

2.1 The Carol Singers' Charity Drive: A Door-to-Door Tradition

To study why donors often fail to claim available tax deductions, we need a setting that combines three features: (i) broad population coverage to capture the behavior of a diverse donor base, (ii) a high degree of social interaction so that social norms and image concerns are activated, and (iii) access to reliable administrative records that allow us to track deduction behavior. The Austrian Carol Singers' Charity Drive (*Dreikönigsaktion*) offers exactly such a setting. As one of Austria's ten largest NGOs by donation revenue, it operates a standardized nationwide campaign with deep local roots through the Catholic parish structure.² Founded in 1954, the organization has become a widely recognized part

²In Austria, parishes are local Catholic communities that typically overlap geographically with municipalities and serve as the organizational base for the campaign. Outside the major cities, parish and municipal



Figure 2: Group of Austrian carol singers (© Dreikönigsaktion)

of Austria's charitable landscape, ensuring both visibility and credibility. Most importantly, its door-to-door fundraising format creates direct, face-to-face interactions between donors and local volunteers, making it an ideal environment to examine how observability shapes tax deduction behavior.

The event takes place annually between December 26 and January 6, when groups of children and adults dressed as kings and queens visit households, sing songs, and collect donations (Figure 2). Over time, the drive has become one of Austria's most visible and anticipated charitable traditions, with households across the country expecting the singers' annual visit.

As part of our collaboration, we obtained access to detailed administrative data on both donations and deductions and were granted permission to embed experimental treatments during the annual event.

2.2 Large Differences in Deduction Behavior Across Settings

Studying deduction behavior in charitable giving is challenging because large-scale data from different donation channels are rarely available. In particular, field data from in-person interactions are typically limited to deductors only, making it difficult to assess how many donors forgo the incentive. Our collaboration with the Carol Singers' Charity Drive provides a rare opportunity to overcome this challenge. We combine two complementary data sources that allow us to compare deduction behavior across highly observable and anonymous contexts.

First, we use administrative records from the door-to-door campaign, where donations are made in face-to-face interactions with local volunteers. Since 2017, Austrian NGOs eligible

boundaries largely coincide. Carol Singing covers all households independent of religious affiliation and is not perceived as a religious activity, making the parish effectively equivalent to a municipality for our purposes.

for donation deductibility have been required to collect and transmit donor information to the tax authority. During the drive, this means that deductors provide their personal details (name, date of birth, address, amount donated) to the accompanying adult, while non-deductors simply hand over their donation without any paperwork. To capture the behavior of this otherwise invisible group, we organized a complementary data-collection effort: in five municipalities, accompanying adults recorded anonymous tick marks for each donation received. This yielded around 4,500 ticks from 10,500 households, corresponding to a donation volume of 78,000 EUR (see the tick-list template reproduced as Figure 32 in Online Appendix B.1). These tick lists allow us to construct the denominator needed to calculate the deduction rates in the field.

Second, we analyze administrative data from the charity’s online donation platform,³ where donors face the same incentive but make their decision without social interaction. Online donations are heavily concentrated during the same period as the drive (see Figure 8 in Appendix A.1). Unlike the field data, the online platform records both deductors and non-deductors, including information on personal characteristics and donation amounts.

Taken together, these two sources provide a unique window into deduction behavior across contrasting social environments. Consistent with anecdotal evidence and prior behavioral research, deduction rates differ sharply across settings. Almost nine out of ten online donors claim a deduction, whereas in the field setting only about one in two hundred do so. Table 1 summarizes these patterns, reporting both average donation amounts and approximate donor counts. For the field, we estimate a total of about 800,000 cash donors nationwide per year, while online figures are drawn directly from the charity’s administrative records.

	Share of deductors	# Donors	Average donation (deductors)	Average donation (non-deductors)
Field	0.66%	~800,000	71.72 EUR	17.32 EUR
Online	86%	19,042	54.31 EUR	36.46 EUR

Table 1: Deduction propensities and average donations by deduction status across settings.

2.3 Uncovering Mechanisms: Norm Perceptions and Information

To probe why such stark differences emerge across settings, we complement these administrative data with a representative survey on taxpayers’ knowledge and perceptions. We conducted an online survey of Austrian taxpayers ($n=314$) in April 2023 via Bilendi, an online research panel provider that offers quota-based national representativeness in terms of gender, age, and education. The survey included several components. First, participants reported whether they had donated in the past and, conditional on donating, whether they had claimed a deduction. Second, we assessed their procedural knowledge of the tax deduction process by asking about four key steps: providing personal data when donating, submission of information by the charity to the financial authorities, automatic consideration of the deduction in the annual tax declaration, and automatic transfer of the reimbursement. Third, we elicited whether they personally considered it appropriate to deduct and beliefs about the share of others finding it appropriate to do so. Finally, we asked for open-ended reasons in favor of and against deducting.

The survey also contained a vignette designed to probe perceptions of generosity in the presence of tax deductions. Respondents were told about “Lukas,” who considers either (a) donating 30 EUR without deducting, or (b) donating 35 EUR and claiming an 8 EUR

³See the Dreikönigsaktion’s online donation portal at <https://www.dka.at/spenden/online>.

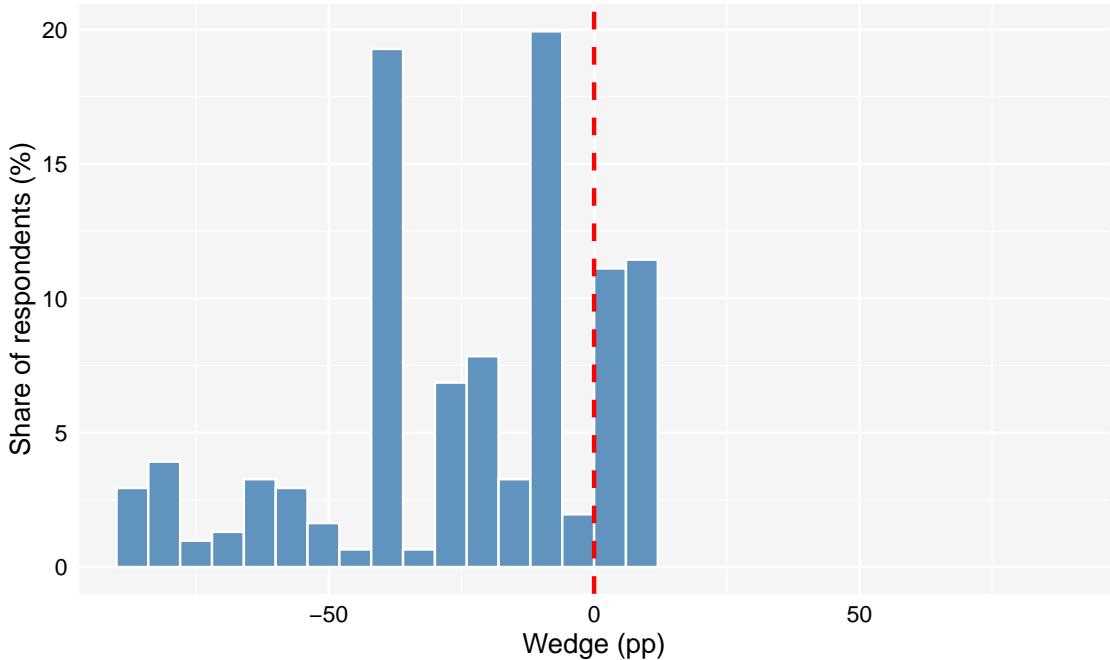


Figure 3: Distribution of perceived social support for deducting donations

The figure shows the deviation between respondents' beliefs about others' views and the actual average view in the sample. Negative values indicate underestimation, positive values overestimation. The dashed red line marks the actual average view.

reimbursement via deduction. Participants indicated which option they regarded as more generous and provided a short justification. Results from this vignette are presented in Appendix A.2.

The main results reveal two principal dimensions. First, substantial informational gaps exist, with 64% of respondents failing to correctly identify at least one step in the deduction process. Second, social image concerns are evident as we observe a 24 percentage point gap between respondents' own views of moral appropriateness and their beliefs about how others view deduction.

As additional descriptive patterns, we find that moral attitudes toward deducting strongly predict behavior. Respondents who considered tax-deducting donations "appropriate" were substantially more likely to deduct themselves, whereas those who judged it "inappropriate" were predominantly non-donors or non-deductors (see Appendix A.2, Figure 14). Likewise, perceptions of procedural effort align with experience: deductors reported lower expected time costs than non-donors or non-deductors, consistent with beliefs being shaped by prior exposure and misperceptions (Appendix A.2, Figure 16).

Figure 3 visualizes the distribution of wedges, defined as the difference between each respondent's belief about others' views and the actual average view. The bulk of the mass lies to the left of zero, showing that more than 75% of respondents underestimate support for deducting. This systematic misperception suggests that norm-related frictions, not just lack of procedural knowledge, are key to understanding low take-up of tax incentives.

3 Field Experiments: Observability and Social Interaction

3.1 Field Experiment 1: Community-Journal Articles

To test whether providing procedural information or correcting misperceived norms can shift deduction behavior, we embedded an intervention in the 2023/24 Carol Singers' charity event. This setting is uniquely suited to our research question since donation and deduction decisions occur in face-to-face interactions, where social norms are highly salient and behavior is highly observable, and administrative deduction records can be linked to treatment assignment.

We delivered our interventions through the final annual issue of local parish journals, which reach virtually all households in parishes and are directly tied to the charity event. The journals carried a feature article marking the 70th anniversary of the event. Depending on treatment assignment, this article was followed by one of two treatment messages. The *Information* treatment outlined the four procedural steps required to obtain a deduction: (i) providing personal details when donating, (ii) the charity transmitting this information to the tax authority, (iii) automatic pre-filling of the deduction in the annual tax return, and (iv) the subsequent refund being transferred to the donor's account. The *Morality* treatment combined these instructions with a concise norm cue: "88% of participants in a recent representative survey found it morally appropriate to deduct charitable donations." The exact wording of both treatments is provided in Appendix A.4.

The intervention covered 30 parishes or parish unions with a combined population of roughly 235,000 residents. Parish unions are administrative clusters of several parishes and function similarly to municipality unions. In 29 units, randomization occurred at the parish level, with all households in a unit receiving the same journal version. In one parish union, we implemented address-level randomization by manually delivering nearly 10,000 copies over ten days. We blocked assignment by carol-singing routes to ensure comparability across neighborhoods. Each route consists of a small number of adjacent streets, and households within a route are typically similar in socioeconomic status. Blocking at the route level therefore ensures that treatment assignment was balanced across key observables. This fine level of randomization was only possible in one parish union since the delivery was carried out entirely by the two researchers, which put a strict cap on the number of addresses that could be covered in this way.

Parishes were recruited through referrals from the charity's state-level coordinators, personal contacts, and targeted cold-calling. Treatment assignment was guided by three balancing criteria: geographic dispersion across federal states, population size, and the degree of urbanization. Achieving balance required navigating rolling recruitment and parish-specific publication deadlines. Some parishes had to be assigned before others were even recruited, which limited perfect ex-ante balance but allowed all units to be included on time. Figure 4 displays the geographic distribution of treatment units. While the map shows an uneven spread across Austria, identification relies on randomized assignment at the parish level and on difference-in-differences comparisons of changes in the number of deductors between treated and untreated parishes.

3.1.1 Results

We evaluate treatment effects using both the parish-level assignments and the one parish union where address-level randomization was implemented. For the parish-level analysis, we observe deduction data for the December 2022/January 2023 event (pre-treatment) and the

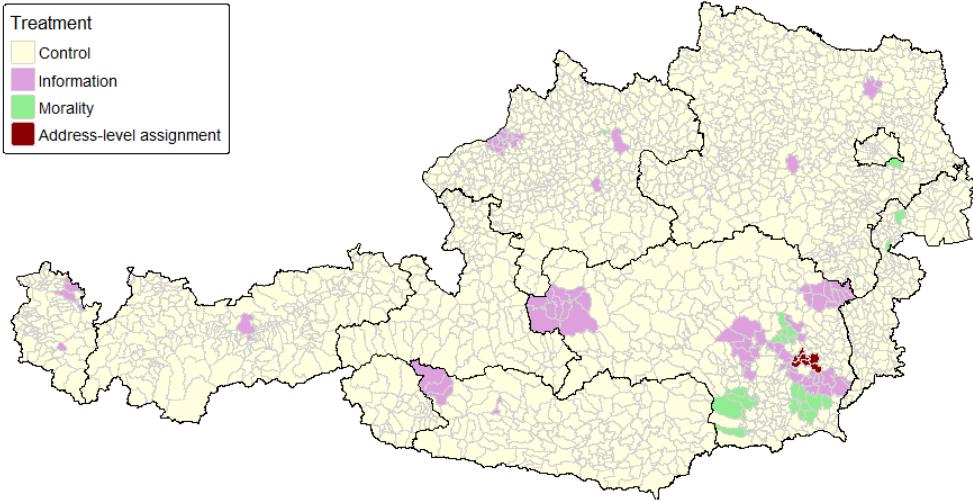


Figure 4: Treatment area of parish-journal intervention.

December 2023/January 2024 event (post-treatment). This allows a difference-in-differences design that compares treated and untreated units before and after the intervention, while at the same time correcting for any location-specific factors that are constant over time. The outcome variable is the number of deductors in a given parish–year or ZIP–year cell. Because the parish-level analysis was not part of our preregistration, we report all plausible specifications for transparency.⁴

The most precise specification is the active ZIP sample, defined as ZIPs with at least one deduction in either year. In this specification, the *Morality* treatment increases the number of deductors by about 7.6 per ZIP ($p < 0.01$), corresponding to an effect size of 0.36 standard deviations. By contrast, the *Information* treatment shows no measurable impact. The other three specifications (all ZIPs, active parishes, all parishes) yield smaller positive point estimates for the *Morality* treatment, with two reaching marginal significance at the 10 percent level (Table 2, Figure 5).

At the address level, where assignment occurred within one parish union, the study is underpowered to detect effects of the magnitude we estimate at the ZIP level. Baseline take-up was only about 0.66% of households, so the *Morality* arm contained roughly 12–13 deductors before the intervention. An effect size comparable to the ZIP-level estimate would translate into about 5 additional deductors in this arm. Yet with about 2,200 addresses per arm, the minimum detectable effect size at 80% power ($\alpha = 0.05$) is roughly +0.68 percentage points, or about 15 deductors. The expected effect therefore falls well below the study’s detection threshold, which explains the null finding in the address-level data. By contrast, the ZIP-level analysis covers nearly 4,400 units observed over two years, with 1,563 active ZIP–year observations in the preferred sample. The outcome variable, the number of deductors per unit, is more continuous and less sparse than binary household-level take-up, which provides substantially greater statistical power. This explains why moderate effects that are undetectable at the address level can nevertheless be estimated with precision in the ZIP-level analysis.

We also test for effects on the intensive margin by analyzing the average donation amount among deductors. Across all specifications, we find no statistically significant changes (Appendix A.4). This pattern suggests that newly converted deductors adjusted their contributions upward to the higher donation levels typical of established deductors, leaving the

⁴The study was preregistered with a pre-analysis plan focused on the address-level randomization, available at <https://osf.io/5ew3j>.

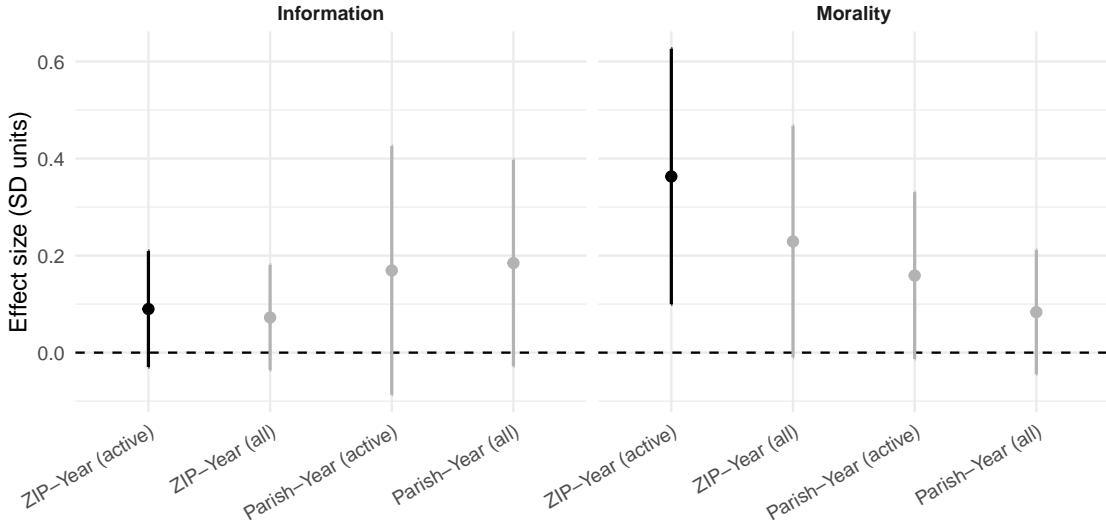


Figure 5: Coefficient plot of treatment effects from Field Experiment 1.

The figure displays point estimates and 95% confidence intervals for both the *Information* and *Morality* treatments across all four specifications (ZIP-Year active, ZIP-Year all, Parish-Year active, Parish-Year all). The coefficients are expressed in standard deviation units of the pre-treatment distribution of deductors. Estimates for active ZIP-Year are highlighted in black; all others are shown in grey.

average donation among deductors unchanged.

	ZIP-Year (active)	ZIP-Year (all)	Parish-Year (active)	Parish-Year (all)
Information	1.699 (1.153) (0.141)	1.369 (1.047) (0.191)	4.892 (3.778) (0.196)	5.070* (2.978) (0.089)
Morality	7.642*** (2.828) (0.007)	4.823* (2.561) (0.060)	4.392* (2.425) (0.070)	1.945 (1.521) (0.201)
Num.Obs.	1640	4472	1587	4318
R2	0.954	0.923	0.954	0.924
SE	ZIP	ZIP	Parish	Parish
FE	Unit	Unit	Unit	Unit
	+	+	+	+
	Event-Year	Event-Year	Event-Year	Event-Year

Table 2: Difference-in-differences estimates of treatment effects on the number of deductors.

The outcome variable is the number of tax-deducting donors per unit-year (ZIP or parish). “Active” samples are restricted to units with at least one deduction in either the pre- (Dec 2022/Jan 2023) or post-period (Dec 2023/Jan 2024). Point estimates represent the absolute change in the number of deductors per treated unit relative to controls. Standard errors (clustered on unit level) and p-values in parentheses. All models include two-way fixed effects (unit and event-year). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

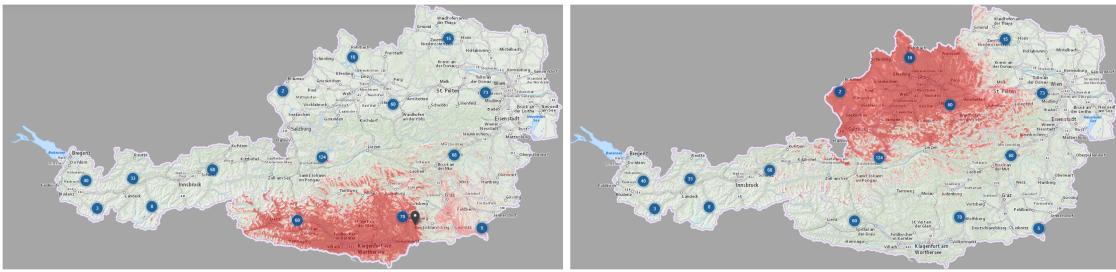


Figure 6: Treatment area of radio-ad intervention.

The figure shows the radio broadcast coverage areas for the *Information* treatment (left panel) and the *Morality* treatment (right panel).

Taken together, the evidence from Field Experiment 1 shows that providing a moral-norm cue increases deduction take-up, while procedural instructions alone have no effect. The results highlight that social-image considerations, when activated in a natural high-visibility setting, can meaningfully shift behavior even in a context with long-standing and entrenched donation habits. This contribution is twofold. First, we provide causal evidence on how social norms affect the use of tax incentives in real-world giving. Second, we demonstrate a scalable, low-cost way of correcting misperceived norms to increase take-up, while showing that purely informational interventions—i.e., efforts that only inform the public about the availability and mechanics of tax deductibility—are ineffective in such settings.

3.2 Field Experiment 2: Radio broadcasts

To address the limits of scalability in our first field experiment and the concern that effects may have diluted over the weeks between treatment delivery and data collection, we implemented a second field experiment using radio broadcasts. This medium allowed us to deliver treatments at the precise moment when deduction decisions were being made and to reach a much larger audience within a single day.

We implemented the intervention on January 4, 2025, during the peak of the Carol Singers’ charity drive. While the drive formally spans December 26 to January 6, past data show that in Upper Austria and Carinthia nearly all deduction activity occurs between January 2 and 6, with January 4 as the median day. We therefore selected this date for broadcasting. Each of the two states was assigned one treatment: the *Morality* message in Upper Austria and the *Information* message in Carinthia. To ensure broad demographic coverage, we recruited two radio stations: a state-level public broadcaster (ORF) with the largest daily audience and a private station (Kronehit) targeting younger listeners. In both states, the spot was aired four times on January 4 on each station.⁵ Because Austrian broadcasting regulations require that radio spots disclose their institutional source, we collaborated with the Vienna University of Economics and Business to include its name as the sender. Referring to an Austrian university ensured credibility and avoided potential suspicion that might have arisen from naming a German institution.

Alongside the radio campaign, we created a dedicated website to provide additional information and tools for potential donors. The site explained the deduction process, featured a searchable registry of all eligible Austrian charities (linked to their official websites), and included a two-way calculator allowing users to estimate either the refund they would receive by deducting or the gross amount they could donate for a given net cost. The website URL was mentioned in both radio spots as the main reference for further information. The site was live from mid-December 2024 through mid-January 2025, used an Austrian domain, and was

⁵Daily audiences are approximately 567,000 in Upper Austria and 558,000 in Carinthia.

indexed through Google services to ensure visibility in search results. Online Appendix B.5 provides screenshots of the website's pages.

Identification builds on two complementary sources of variation. First, within treated states, the timing of carol-singing routes across January 2–6 is determined primarily by the availability of volunteer groups rather than household or neighborhood characteristics, providing quasi-random assignment of households to pre- versus post-broadcast periods. This allows for a difference-in-differences (DiD) design comparing deduction behavior in January 2–3 (pre) versus January 4–6 (post) within each state. Second, exposure drops discontinuously at the predicted edge of the radio signal. This supports a geographic regression discontinuity design (RDD) that compares municipalities just inside and just outside the coverage area.

Difference-in-Differences (DiD). Our first strategy exploits within-state timing around the single broadcast day, using two years of data (2024 and 2025). The unit of observation is the municipality-day, and the analysis is run separately for each treated state (Upper Austria for *Morality*, Carinthia for *Information*). We define January 2–3 as the pre-period and January 4–6 as the post-period in both years, with January 4 marking the broadcast date in 2025. Identification comes from the quasi-random timing of carol-singing routes: within municipalities, volunteer groups schedule visits based on their own availability and route logistics rather than household or neighborhood characteristics. This generates as-if random assignment of households to pre- vs. post-broadcast days.

Our main specification is a Poisson difference-in-differences model,

$$Y_{mty} = \alpha_m + \delta_y + \gamma \text{Post}_t + \beta (\text{Post}_t \times \text{Year}_{2025}) + \varepsilon_{mty},$$

where Y_{mty} is the count of deductors in municipality m on day t of year y , α_m are municipality fixed effects, δ_y year dummies, and β captures the treatment effect of the 2025 broadcast by comparing the pre-post change in 2025 to the same pre-post change in 2024. Standard errors are clustered at the day level.

As extensions, we estimate (i) an event-time specification probing pre-trends, (ii) a two-way fixed-effects model absorbing all municipality and date heterogeneity, and (iii) a triple-difference design that further compares treated states to the seven non-treated federal states.

Regression Discontinuity (RDD). Complementing our DiD analysis, we implement a geographic RDD that leverages the discontinuous change in exposure at the predicted broadcast border. Official signal propagation data from the Austrian broadcast control agency are geo-referenced and linked to municipality boundaries to classify exposure.⁶ The running variable is the signed distance (in kilometers) from the municipality centroid to the broadcast border. We estimate local linear regressions on either side of the cutoff with triangular kernel weights. Our main specification uses a ± 20 km bandwidth, and we report additional estimates for ± 10 km and ± 30 km as robustness checks. We also implement a donut RDD excluding municipalities within 1 km of the border. Identification requires continuity of potential outcomes at the border; we assess this through smoothness tests on pre-treatment deduction rates and observables, density diagnostics, and the donut exclusion. The coefficient of interest is the discontinuity in deduction rates at the cutoff, which under these assumptions captures the causal effect of broadcast exposure. Taken together, the DiD and RDD strategies exploit complementary sources of quasi-experimental variation providing a robust framework to evaluate the causal impact of radio exposure on deduction behavior.

⁶<https://senderkataster.rtr.at>

3.2.1 Results

Because individual-level data are available only for deducting donors, we deviate from the preregistration.⁷

For Upper Austria, placebo checks support the parallel trends assumption and thereby allow causal interpretation of the DiD estimates. Consistent with the RDD, these results show no detectable effect of the *Morality* treatment (Table 3). For Carinthia, the placebo test suggests a pre-trend, invalidating the DiD design for causal identification. We therefore rely on RDD estimates as the primary evidence, while reporting the DiD output together with placebo tests in Appendix A.5. Across different bandwidths, the RDD estimates consistently indicate a null effect of the *Information* treatment (Table 4). Further robustness checks and analyses of dynamic effects are provided in Appendix A.5.

Two factors likely contribute jointly to this outcome. First, regulatory restrictions on radio-based public service announcements prohibited explicit mention of both the charity name and the event, making it harder for listeners to connect the information to the actual donation opportunity. Second, discussions with our media agency partner and the participating radio stations suggested that a minimum broadcast period of about two weeks would normally be required to achieve sufficient audience reach and recall, whereas budget constraints limited us to a single day of broadcasts. Beyond these structural constraints, it is also possible that information about social norms carries greater weight when communicated through local-level outlets such as parish journals, where it is more naturally perceived as a signal about one's immediate community, rather than through state-wide radio campaigns that convey norms of a more anonymous mass audience. This interpretation is necessarily tentative, but it highlights that the perceived social context of a message may shape its effectiveness.

# Deductors (percent change)	
Upper Austria (Morality)	-33.7
	(25.5)
	(0.284)
Num.Obs.	10
Pseudo R2	0.295
SE	date
FE	state + date

Standard errors and p-values in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Poisson DiD estimates of *Morality* treatment effects on daily deductors

⁷The preregistration is available at <https://osf.io/x45kv>.

	Carinthia (Information)	Upper Austria (Morality)
# Deductors (percent change)	-38.4 (20.7) (0.124)	6.4 (28.2) (0.807)
Num. Obs.	n_L/n_R : 18 / 24	n_L/n_R : 29 / 178

Table 4: RDD estimates of the change in the number of deductors (2025)

Standard errors and p-values in parentheses. n_L and n_R denote the number of observations to the left (outside treatment area) and right (inside treatment area) of the cutoff with 20km bandwidth. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Finally, website engagement during the campaign period was very low, yielding insufficient data for formal analysis.

4 Online Experiment: Anonymity and Norms

4.1 Motivation

To benchmark deduction take-up under conditions where social-image concerns are absent, we implemented an online experiment. Our results so far suggest that social observability strongly affects deductibility: while the majority of donors deduct in the anonymous online setting, very few do so in the field. In the latter context, our field experiments indicate that moral-norm cues can shift behavior, likely because social-image concerns hold donors back. What remains unclear is whether such interventions matter at all when anonymity rules out observability pressures. The online experiment therefore provides a clean test of whether information or norm cues retain any power in the absence of social-image concerns.

4.2 Design

We conducted the study on Prolific in October 2023 ($n=483$) using oTree (Chen et al. 2016), recruiting Austrian and German participants.⁸ Participants first completed two real-effort encoding tasks to earn money. A flat 30% tax was levied on earnings and transferred into a shared fiscal pool, creating an externality without requiring interaction.

Afterwards, participants could decide whether to donate part of their earnings. Donors could allocate funds across nine real Austrian NGOs. Exactly three were tax-deductible, while six were not. All organizations were presented under neutral labels, but participants could click to view short descriptions including whether the charity was eligible. Actual money was transferred to the selected NGOs after the experiment.

Conditional on donating to at least one deductible NGO, participants were asked whether they wished to deduct. Doing so required them to enter their Prolific ID, mimicking the real-world step of providing personal information. Refunds were financed from the shared fiscal pool, ensuring real monetary consequences. Participants were randomly assigned to one of three conditions. In the *Control* group, deductibility was described minimally: donors were told that if they opted to deduct, they would have to complete the required steps and receive back 30% of their donation. The *Information* treatment added procedural detail by

⁸See <https://osf.io/mruz4> for the preregistration and pre-analysis plan.

explaining where eligibility information could be found and an explanation that entering the Prolific ID sufficed to complete the process. The *Morality* treatment included all of these elements as well. In addition, it stressed that deductibility enables donors to support those in need more strongly and noted that in a previous study 88% of participants considered deducting donations morally appropriate.

4.3 Results

Neither the *Information* treatment nor the *Morality* treatment produced a statistically detectable effect on deduction propensity. This pattern reflects a ceiling effect: because only donors to at least one deductible charity face the deduction decision (Control n=94, Information n=92, Morality n=101) and take-up in the *Control* group is 85%, the minimum detectable effect size at 80% power is about 11.6–11.8 percentage points per treatment–control comparison.⁹

Treatment messages also left donation behavior unchanged: both the incidence of giving and the allocation between deductible and non-deductible NGOs were unaffected (see Appendix A.3 Figures 17 and 18). Thus, in an anonymous environment, deduction was already close to universal, leaving limited scope for either procedural information or norm cues to shift behavior.

5 Lab Experiment: Observability and Norms

5.1 Motivation

Deduction take-up varies sharply across contexts: in anonymous online settings, the vast majority of donors deduct, whereas in face-to-face field settings, very few do so. This striking gap suggests that social visibility may suppress deduction, rather than differences in the charitable decision itself. Field interventions, however, cannot cleanly disentangle observability from other features of in-person interaction, since the legal framework imposes strict constraints on how deduction can be implemented.¹⁰ To isolate the role of visibility, we therefore implemented a laboratory experiment that directly manipulated whether deduction decisions were observable to others. This setting allows us to causally test whether making the deduction choice visible activates social-image concerns and thereby shifts behavior.

5.2 Design and Treatments

The structure of the lab experiment closely followed the online study while adding experimental control over observability. Participants completed two real-effort tasks to earn income, which was taxed at a flat rate of 30 percent. They could donate part of their earnings to a real charity, Dreikönigsaktion, the same partner as in the field studies. Donations were transferred for real, and deductions were reimbursed at the marginal rate from a shared tax pool. This setup created realistic stakes and preserved the trade-off between effort and refund.

The design combined two treatment dimensions in a 2×2 structure. To vary observability, participants moved to a separate terminal after completing the donation stage. In the

⁹≈12.5 pp with a Bonferroni adjustment.

¹⁰Austrian charities are legally required to verify that the amount claimed for deduction matches the donation actually received. This prevents experimental variation in whether deduction choices are visible, as any attempt to conceal information would risk enabling tax fraud.

Anonymity condition they made their deduction choice alone, while in the *Observability* condition the experimenter escorted them to the terminal, explained that further tasks would be completed there, and remained standing behind them with full view of the screen. Other participants never saw anyone’s decision, which ensured that the manipulation focused exclusively on the presence of a neutral observer. To vary exposure to norms, participants in the *Morality* condition saw a short message on their original terminal immediately before moving to the deduction station: “*In a recent representative survey, 88% of respondents said it is morally appropriate to deduct charitable donations.*” The experimenter was blind to this assignment. The experiment was implemented in oTree (Chen et al. 2016) and coordinated through ORSEE (Greiner 2015) across two Vienna economics laboratories.

5.3 Results

Following our preregistration and pre-analysis plan, we first compared deduction rates between *Anonymity* and *Observability* using a one-sided Boschloo test.¹¹ We do not reject the null ($p = 0.672$). Likewise, we find no significant effect of the moral message, either among anonymously deducting donors ($p = 0.756$) or among donors whose decision was observed ($p = 0.857$). Because treatment was assigned before the donation decision, the resulting donor samples in the four treatment cells were not fully balanced in demographics even though no treatment content was revealed pre-donation. Summary statistics show modest differences in age and gender (Appendix A.6, Table 18). To address potential covariate imbalance and improve precision, we complement the preregistered tests with a Lin-style OLS linear probability model (Lin 2013) that retains the *Observability* \times Moral-message interaction and adds full interactions between each treatment indicator and the covariates (gender, age, education), while including session fixed effects and HC1 standard errors clustered at the session level. In this specification, deduction rates were 13.9 percentage points lower under *Observability* compared to *Anonymity*, but the effect is imprecisely estimated ($p = 0.196$). The moral message produced small and statistically insignificant effects in both visibility conditions. Figure 7a displays the average marginal effects for *Observability* and *Moral message* as well as the interacted versions for *Observability*.

A post-hoc calculation shows that the minimum detectable effect given the effective sample size we had with 80 percent power and $\alpha = 0.05$ is roughly 23.6 percentage points for a one-sided test (26.6 pp two-sided). This threshold exceeds the observed effect and explains why statistical significance is not reached. Deduction rates in the lab averaged about 59 percent under *Anonymity* and 47 percent under *Observability*, with a mean of roughly 53 percent across both conditions. These levels sit between the high take-up observed in the online experiment and our charity partner’s online data (around 86%) and the very low levels in the field (around 0.7%). The contrast in the lab was inevitably weaker as the *Anonymity* condition was less private than at-home giving, since participants remained in a lab environment, and the *Observability* condition involved a neutral lab assistant rather than charity-affiliated volunteers. Together with lower donation incidence in the lab (about 39% compared to roughly 50% online), these features limited power. We therefore view the lab results as a consistency check. While the estimates do not reject the null at conventional levels, they are directionally aligned with the broader evidence that visibility and thus active social norm concerns discourage deduction.

To further probe inference, we complement conventional cluster-robust standard errors with a randomization inference (RI) procedure. RI tests the sharp null of no treatment effect by repeatedly reassigning the realized treatment labels within sessions and recalculating the test statistics. This approach exploits the actual randomization scheme, thereby providing

¹¹See <https://osf.io/uc7jw> for the preregistration and pre-analysis plan.

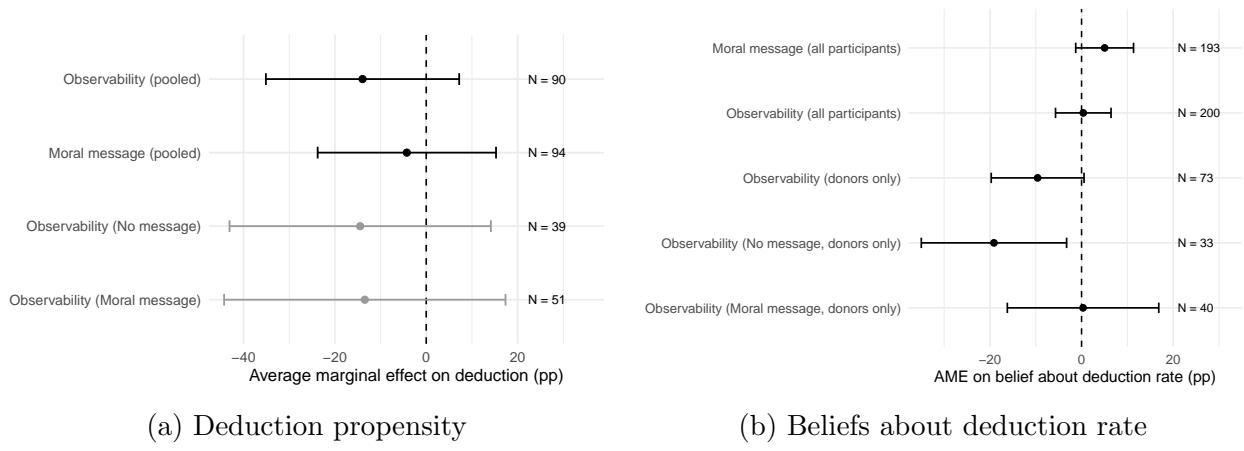


Figure 7: Treatment effects on behavior and beliefs

Estimates are Lin-adjusted average marginal effects from an OLS linear probability model. The Lin adjustment (Lin 2013) augments the specification with full interactions between each treatment indicator and the covariates (gender, age, education), while retaining the treatment \times treatment term (Observability/Anonymity \times Moral message/No message) and session fixed effects. Standard errors are cluster-robust at the session level (HC1). Points are average marginal effects (percentage points); whiskers are 95% confidence intervals. Differences in sample sizes result from belief block being added only after the first few sessions.

exact finite-sample p-values that are robust to small-sample and heteroskedasticity concerns. In our data, RI yields lower p-values for the effect of *Observability* (0.088 versus 0.196 from the asymptotic model), but not for the *Moral message* (0.596 versus 0.670). This pattern is consistent with differences in effect heterogeneity across clusters: when treatment effects are more homogeneous across sessions, the permutation distribution is tighter and the observed estimate appears more extreme, reducing the RI p-value. By contrast, with more variability across sessions, the permutation distribution widens and the RI p-value remains close to the conventional one. In our case, this suggests that reactions to the *Observability* treatment were relatively consistent across clusters, whereas responses to the *Moral message* varied more strongly. We report the full RI results in Appendix A.6, Table 16.¹²

To shed light on perceived descriptive norms, we next examine elicited beliefs about the share of peers who deduct, using the same specification as above.

Pooling all participants, *Observability* increases beliefs by 0.37 pp ($p=0.90$), while the *Moral message* shifts beliefs downwards by 5.04 pp ($p=0.12$). Neither of the effects is statistically significant at conventional levels. We then focus on donors, because they are the only participants for whom a deduction decision is observed and thus the relevant group for linking beliefs to behavior. Among donors, *Observability* leads to a change of -9.59 pp ($p=0.06$) in perceived prevalence. Splitting donors by message clarifies where this comes from. In the *No message* condition, our cleanest read on the effect of visibility and thus active social-norm concerns without any explicit cue, *Observability* lowers beliefs by -19.13 pp ($p=0.02$), whereas in the *Moral message* condition the estimate is 0.33 pp and indistinguishable from zero ($p=0.97$) (Figure 7b).

These descriptive belief patterns are transparent evidence consistent in direction with the behavioral estimate reported above for *Observability* on deduction propensity. We do not estimate causal mediation because beliefs were elicited after the deduction decision; any decomposition would be non-causal and is therefore omitted.

¹²Using a List et al. (2024)-style cross-fitted AIPW estimator with session-blocked propensities and random-forest outcome models (folds at the session level), we estimate an effect of -11.9 pp (SE 8.41, N=164) among donors. Given the small covariate set (gender, age, degree) and modest N, this machine-learning adjustment is primarily precision-oriented; conclusions are unchanged. For more details, see Appendix A.6, Table 17.

6 Conclusion

Analysis of Austria’s 2017 reform, designed to eliminate filing frictions, reveals a persistent puzzle. Before the reform, 19.6% of taxpayers claimed a deduction for their charitable donations. Trend-adjusted estimates indicate that the reform raised the deduction share by only 1.8 percentage points, a 9% increase relative to the baseline. At the same time, the average deducted amount fell by about 34.90 EUR, corresponding to a 16.9% decline relative to the pre-reform mean of 206.90 EUR. Even after filing frictions were largely eliminated, the majority of donors still forewent the available tax benefit, and those who did deduct claimed smaller amounts on average. To our knowledge, this is the first systematic evaluation of the reform.

The contrast across donation channels is sharper still. About 86% of donors deduct when giving through the charity’s anonymous website, whereas only 0.7% do so in the face-to-face door-to-door drive. A representative survey points to two plausible explanations: limited procedural knowledge and social-image concerns, reflected in a gap between what individuals view as morally appropriate and what they believe others expect. Motivated by these patterns, we designed an anonymous online experiment and two field experiments that directly target information and perceived norms. In the online setting, providing procedural information alone has no effect. Adding a one-sentence norm correction also leaves behavior unchanged, plausibly because deducting is already near saturation, as a baseline rate of 85% leaves little room for further increases at our sample size.

In a socially observable setting (Field Experiment 1), printing the combined information-and-norm paragraph once in local parish journals several weeks before the 2023/24 drive increases the number of deductors in our main specification—active ZIPs—by 0.36 standard deviations, which corresponds to about +7.6 deductors per treated ZIP. Two complementary parish-based specifications and the all-ZIPs variant show smaller, less precise positives. By contrast, an information-only paragraph has no detectable effect across any of the four pre-specified variants.

Field Experiment 2 stress-tests scalability and timing by delivering one-day radio spots during the drive itself in two federal states. Neither the *Morality* treatment in Upper Austria nor the *Information* treatment in Carinthia has a detectable effect on deduction propensity. Two features likely contribute to these nulls: public-service rules prohibited naming the charity or its event, dampening relevance at the moment of choice, and the campaign was limited to a single day with only a handful of plays, whereas professional guidance suggests sustained multi-week exposure is typically required. A more speculative reading is that norm cues may carry more weight when delivered by local outlets that feel community-proximate, as in parish journals, than by broad state-level broadcasts.

We causally investigate the role of observability in a lab experiment. Participants complete donation choices at their desk and move to a separate terminal for deduction and belief reporting. Under *Anonymity* they are alone at the terminal; under *Observability* a neutral experimenter escorts them and can view the screen. Following our preregistration, pooled donor data show no rejection of the null; the short moral-norm message has no detectable effect either. Because randomization preceded the donation decision, donor composition across the four cells is not guaranteed to balance perfectly. We therefore report average marginal effects from a logit with demographic controls and session fixed effects: *Observability* reduces deduction by 13.94 pp ($p = 0.196$, one-sided), and the moral message remains small and imprecise. The post-hoc minimum detectable effect at 80% power (one-sided, $\alpha = 0.05$) is about 26.8 pp, above the observed estimate. Deduction rates line up naturally with our broader evidence: about 59% under *Anonymity* and 47% under *Observability*, yielding an overall rate of ~53%, which falls between the online benchmark (~85%) and the field drive

($\sim 0.7\%$).

Taken together, the results point to a common thread about the role of social norms. In settings of full anonymity, norms are effectively deactivated, and deduction rates rise close to their natural ceiling; under such conditions, neither procedural information nor a brief norm cue has room to matter. When deduction is decided in socially observable settings, norms become salient and often suppress take-up. Here, correcting misperceived norms in a credible, community-anchored channel such as parish journals read in advance of face-to-face solicitation increases deduction. By contrast, a one-day, state-wide broadcast does not shift behavior. This null is consistent with the limited number of broadcasting slots, which made exposure easy to miss and left no possibility of delayed treatment; in contrast, a printed journal can be read at any convenient moment before the drive, allowing the message to sink in and to be perceived as a local cue. Across all three studies, the pattern is coherent: what matters is not only whether donors know the procedural steps, but whether social norms are activated, and if so, whether they are corrected in ways that feel credible to the community in which giving takes place.

Our findings on charitable tax-deductibility suggest that the interaction between incentives and social norms is not confined to this setting but generalizes more broadly. Beyond giving, the same mechanisms shape behavior in other domains. Vehicle choice is a salient case: in many countries, large fuel-intensive cars continue to function as markers of status, which may blunt the effect of purchase rebates for electric vehicles. Policy can counter this by reshaping the norm of what ownership signals, for instance through visible public EV fleets or campaigns that portray electric vehicles as modern and aspirational. Home energy investments show a similar pattern. Subsidies for insulation or heat pumps may achieve little because such improvements are invisible to neighbors, whereas rooftop solar spreads once panels become a visible neighborhood standard. Publishing block-level retrofit rates or awarding plaques for energy-efficient homes can therefore complement financial incentives by shifting the local norm. Even the take-up of earned-income credits may be constrained when claiming is seen as stigmatizing. Letters that emphasize how many peers file or testimonials that frame claiming as responsible financial behavior can change the perception of what the action communicates. Across these settings, the broader lesson is consistent: to move behavior at scale, policy must align the private payoff with the social image, either by correcting misperceived norms or by reshaping them directly.

Limitations. First, all field data come from Austria, a high-income country with standardized electronic filing since 2017 and an average refund around 40%; settings with payroll deduction or lower tax coverage may respond differently. Second, the main field setting is a door-to-door drive in which children and adults perform short songs or verses as part of the solicitation, and effects may be smaller in more impersonal fundraising channels. Third, while our data cover all deductions linked to the drive, they do not capture possible spillovers to other charities or substitution across giving opportunities.

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A Robustness and Heterogeneity

This appendix presents supplementary robustness checks and heterogeneity analyses that support the empirical and experimental findings reported in the main text. Sections A.1 and A.2 revisit the administrative and survey evidence with alternative specifications and additional event-study plots. Sections A.3, A.4, A.5, and A.6 provide further evidence for the online, field, and lab experiments, including subgroup analyses by gender, prior giving, and baseline deduction behavior.

Unless otherwise indicated, we use the same variable definitions and estimation strategies as in the main text. All confidence intervals are two-sided and clustered at the treatment unit (ZIP code or individual) level.

A.1 Empirical Data

This subsection supplements Section 2 with additional detail on online-giving behavior and on the effect of Austria’s 2017 tax reform on deduction outcomes.

Tables 5 and 6 present summary statistics by deduction status and demographic characteristics. Figure 8 documents the pronounced seasonal clustering of online donations, justifying our focus on the annual drive period.

Figures 10 and 11 report event-study estimates of the reform’s impact on (i) deduction propensity and (ii) average deducted amounts. Tables 7 and 8 present trend-adjusted interrupted time series (ITS) estimates for deduction propensity and the average deducted volume. Further heterogeneity results are provided in Online Appendix B.1.

Identification and trend adjustment. Because the reform was implemented nationally on January 1, 2017, there is no contemporaneous untreated control group. Moreover, the bracket-level event study for the extensive margin (share of deductors) exhibits a clear non-zero pre-trend in 2013–2016, implying that a simple level-shift comparison to 2016 would overstate the reform’s impact (Figure 10). To address this, we construct a trend-adjusted counterfactual by estimating, for each bracket cell, a linear trend on the pre-reform years (2013–2016, weighted by the number of taxpayers). We then extrapolate this cell-specific trend to 2017–2019 and define the detrended outcome as the difference between the observed series and the pre-trend prediction. Regressing the detrended series on a post-reform indicator with cell fixed effects (and clustering by cell) yields an average post-2017 deviation from the counterfactual of 1.77 percentage points (s.e. 0.18 pp.) (Table 7). Relative to the pre-reform mean of 19.6%, this corresponds to a 9% increase in the share of taxpayers who deduct donations.

For average deducted amounts, an event study relative to 2016 does not satisfy parallel pre-trends (joint test of 2013–2015 leads: $p < 10^{-26}$). We therefore use a trend-adjusted ITS. For each bracket cell, we estimate a linear trend on 2013–2016 weighted by the number of deductors, extrapolate this trend to 2017–2019, and define the detrended outcome as the difference between the observed series and the pre-trend prediction. Regressing the detrended series on a post-2017 indicator with cell fixed effects and cluster-robust standard errors yields an average post-reform deviation of -34.90 EUR (s.e. 1.70 EUR), relative to a pre-reform mean of 206.90 EUR (Table 8). This corresponds to a -16.9% reduction in average deducted amounts. Figure 12 shows that the downward shift is also visible in the full distribution of average deducted amounts. These ITS estimates correct for the observed pre-trends and attribute the post-2017 changes in deduction outcomes to the reform under the assumption that cell-specific linear pre-trends would have continued absent the policy.

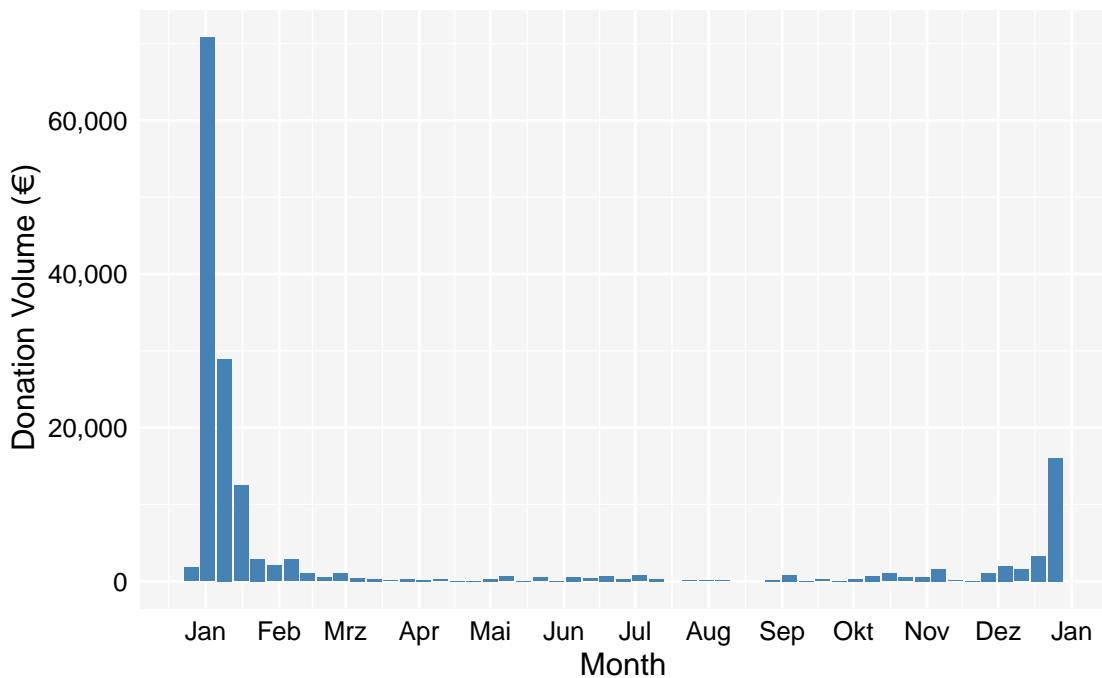


Figure 8: Distribution of incoming donations through the charity's online tool (2022).

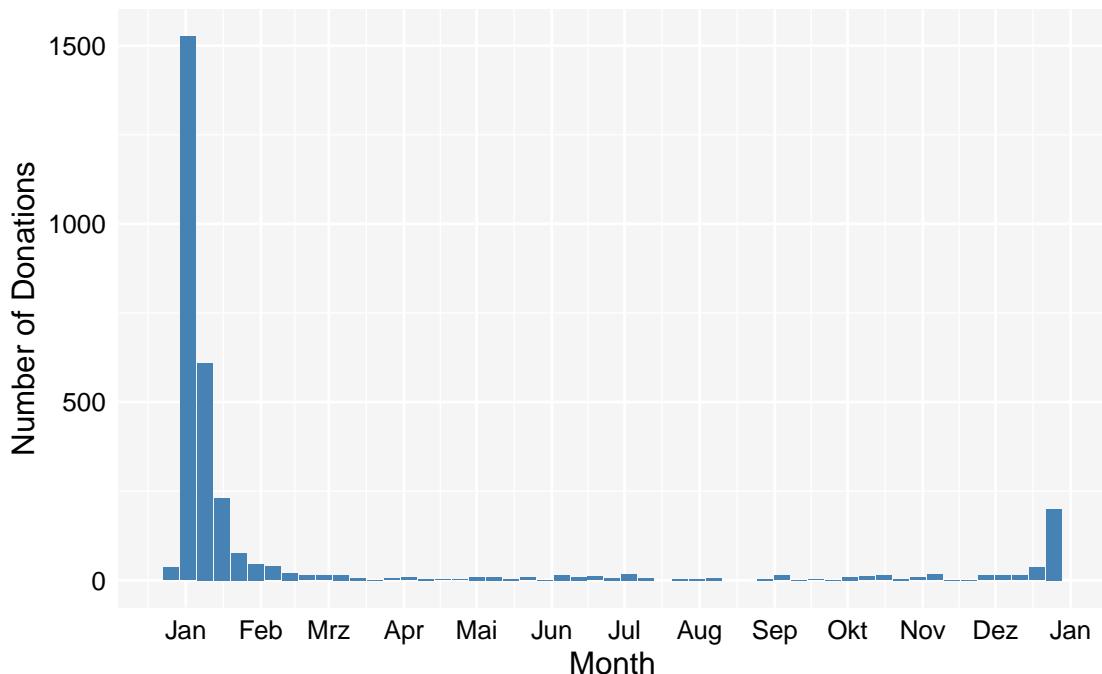


Figure 9: Count of online donations by week (2022).

Deduction status	Observations	Mean (€)	SD (€)
Deductor	16321	54.30	90.00
Non-deductor	2721	36.50	126.10
Full sample	19042	51.80	96.20

* Deductor refers to donors who requested to tax-deduct their donation.

Table 5: Summary statistics of online donation data by deduction status.

Gender	N (D)	Mean € (D)	SD € (D)	N (ND)	Mean € (ND)	SD € (ND)
Female	6608	48.65	87.26	1471	32.68	52.59
Male	9086	57.05	84.50	1124	32.49	54.64
Unknown	627	74.12	161.88	126	116.00	528.95
Age group	N (D)	Mean € (D)	SD € (D)	N (ND)	Mean € (ND)	SD € (ND)
≤24	185	41.73	57.15	55	44.08	136.94
25-34	1249	49.73	94.59	154	23.71	20.75
35-44	2812	48.79	80.99	192	29.32	33.47
45-54	3760	54.47	90.18	262	32.48	81.59
55-64	4128	54.34	71.08	270	37.93	100.18
65-74	2667	59.06	105.13	206	38.48	37.13
75+	1243	53.68	74.66	138	37.08	28.47
NA	277	93.78	220.79	1444	38.58	160.25
Federal state	N (D)	Mean € (D)	SD € (D)	N (ND)	Mean € (ND)	SD € (ND)
Burgenland	484	37.90	42.73	57	28.32	27.57
Carinthia	924	44.34	44.76	137	28.49	23.62
Lower Austria	2924	53.64	115.14	310	39.06	97.78
Salzburg	743	50.11	65.64	84	31.80	25.37
Styria	2957	48.89	58.19	435	39.33	239.52
Tyrol & Vorarlberg	1672	55.33	73.43	211	29.10	28.74
Upper Austria	3265	57.79	99.83	262	36.73	59.96
Vienna	3350	61.83	102.69	323	54.26	191.39
NA	2	20.00	0.00	902	31.62	48.74

Note: D = Deductor, ND = Non-deductor.

Table 6: Summary statistics of empirical online donation data.

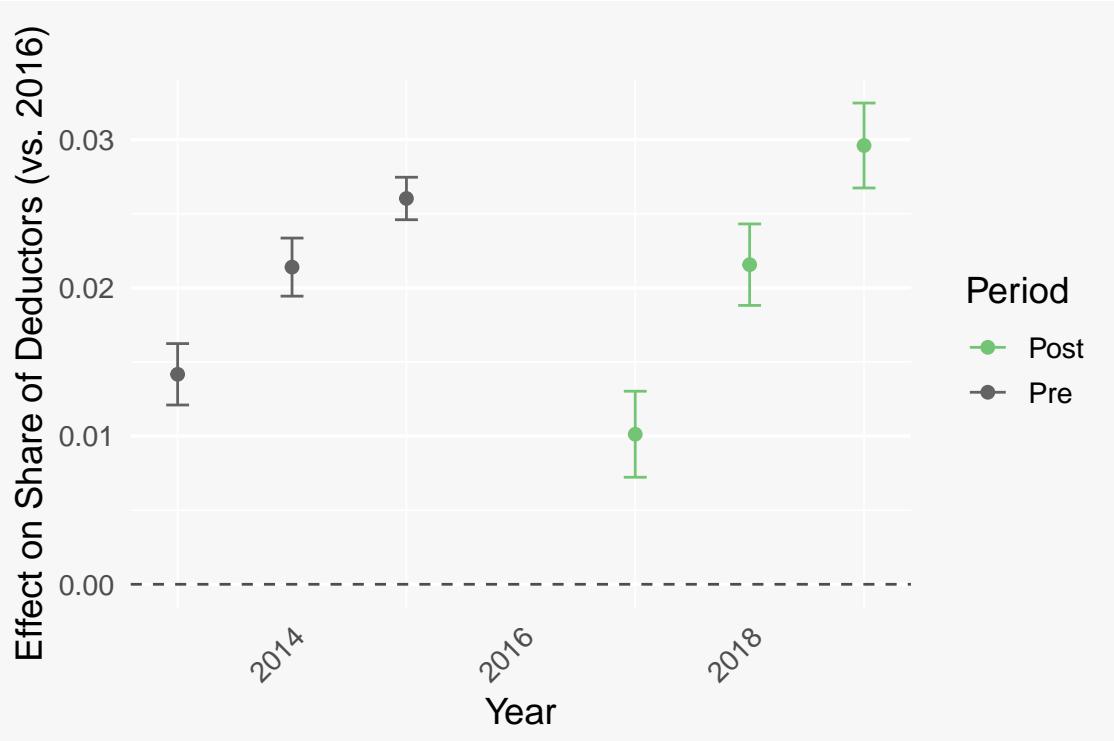


Figure 10: Event-study estimates of deduction propensity showing non-zero pre-trend.

Dependent Variable:	Detrended Share of Deductors
<i>Variables</i>	
Post-2017	0.0177*** (0.0018)
FE	brackets
Observations	
R ²	68,260
Within R ²	0.52837
	0.04199

Clustered (bracket-level) standard-errors in parentheses.

Brackets are defined as low-level aggregates for subgroups.

(e.g. active men of age 20-29 earning 20-25k EUR a year who reside in Vienna.)

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

Table 7: Tax-reform effect on deduction propensity (trend-adjusted ITS).

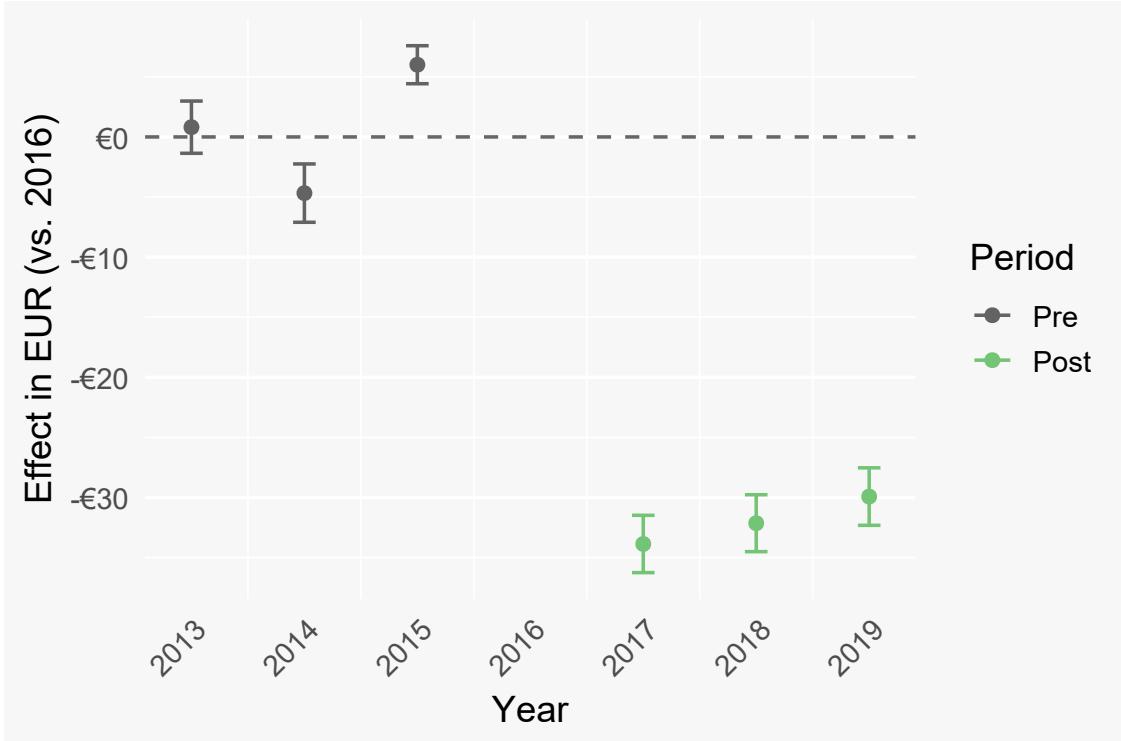


Figure 11: Event-study estimates of average deducted amount showing unstable pre-trend.

Dependent Variable:	Detrended Avg. Deducted Amount (EUR)
<i>Variables</i>	
Post-2017	-34.90*** (1.701)
FE	brackets
Observations	
R ²	49,158
Within R ²	0.44768
0.01667	

Clustered (bracket-level) standard-errors in parentheses.

Brackets are defined as low-level aggregates for subgroups.

(e.g. active men of age 20-29 earning 20-25k EUR a year who reside in Vienna.)

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

Table 8: Tax-reform effect on average deducted amount (trend-adjusted ITS).

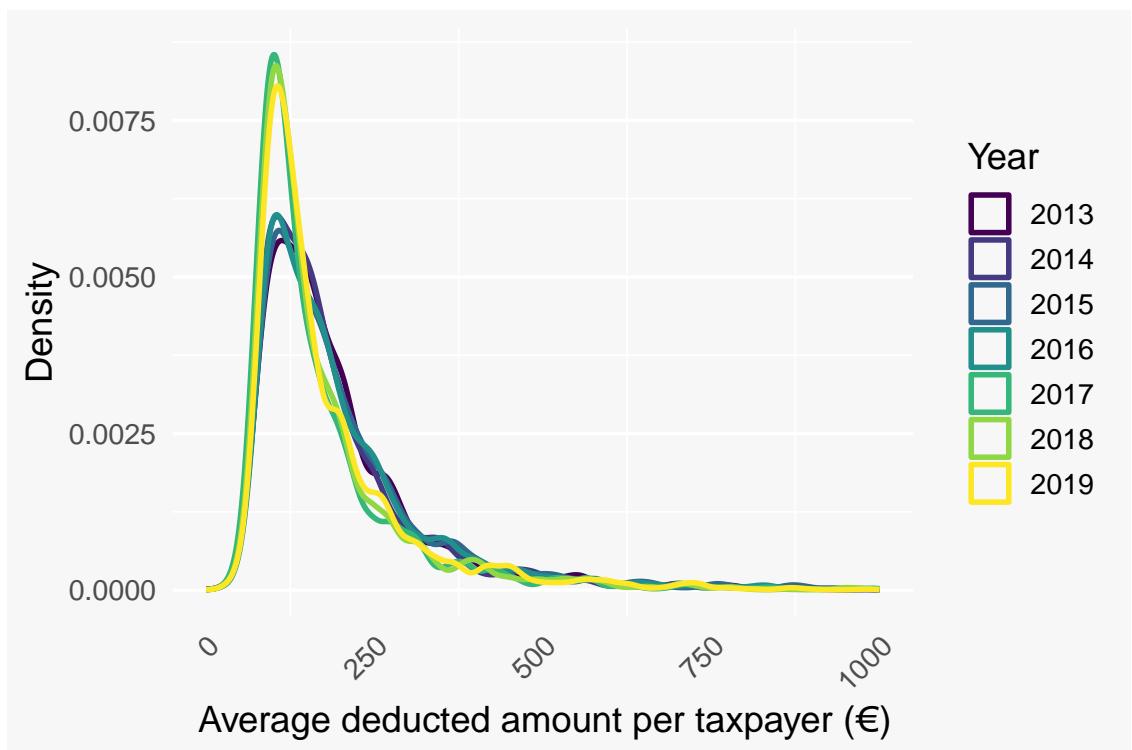


Figure 12: Year-by-year distribution of average deducted amounts, weighted by the number of deductors per cell.

A.2 Representative Survey

This subsection supplements Section 2.3 with additional evidence from the representative survey on taxpayers' beliefs and attitudes toward tax-deductibility of donations.

Figure 13 shows that beliefs about the share of taxpayers who deduct donations differ by respondents' own donation and deduction behavior. Deductors, on average, overestimate the prevalence of deduction relative to non-donors and non-deductors. The actual sample share of deductors was 31.5%.

Figure 14 illustrates flows from first-order moral beliefs to deduction behavior. Respondents who considered tax-deducting donations "appropriate" were substantially more likely to deduct themselves, whereas those who judged it "inappropriate" were predominantly non-donors or non-deductors. This visualizes the close alignment between moral attitudes and actual behavior.

Figure 15 compares the distribution of second-order beliefs (beliefs about others' views) by respondents' own first-order stance. Those who judged deduction inappropriate systematically believed that others would also disapprove, whereas respondents viewing deduction as appropriate held more dispersed second-order beliefs. A Kolmogorov–Smirnov test confirms that the two distributions differ significantly ($p = 0.002$).

Figure 16 documents perceived procedural frictions. Respondents systematically misestimated the time needed to claim a deduction, with strong differences across groups (Kruskal–Wallis $p < 0.001$). Deductors reported lower expected time costs than non-donors or non-deductors, consistent with experience and misperceptions shaping beliefs about procedural effort.

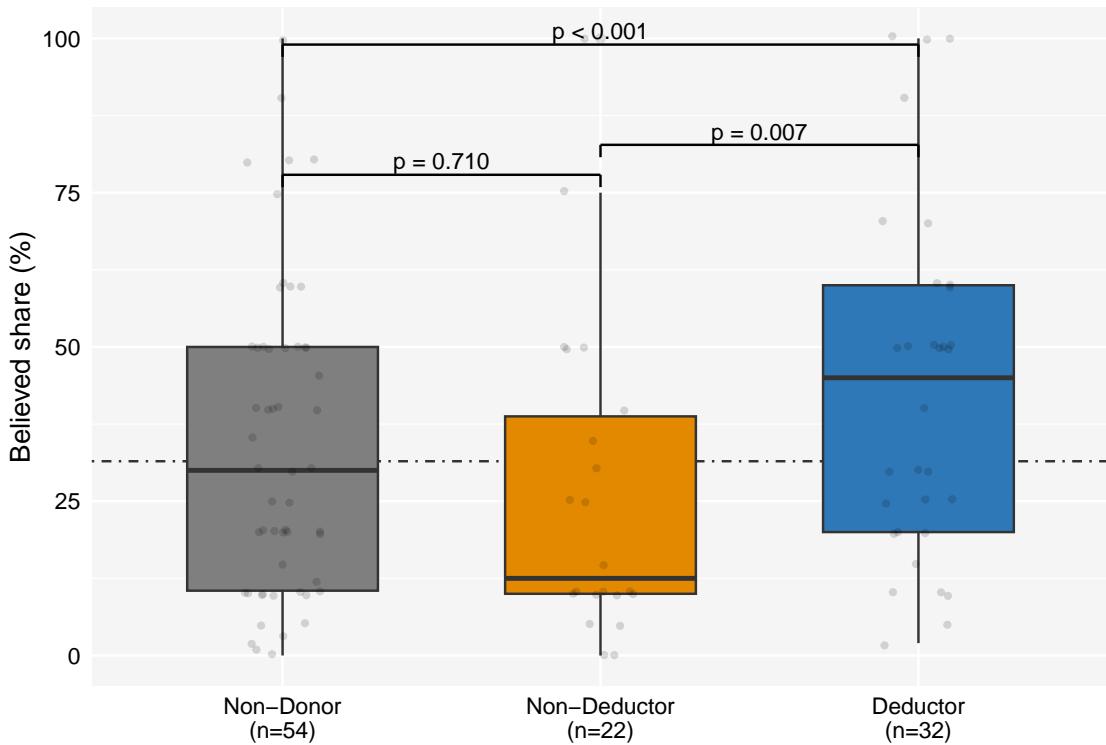


Figure 13: Beliefs about the share of deductors by respondents' own donation and deduction behavior. Reference line marks actual share (31.5%).

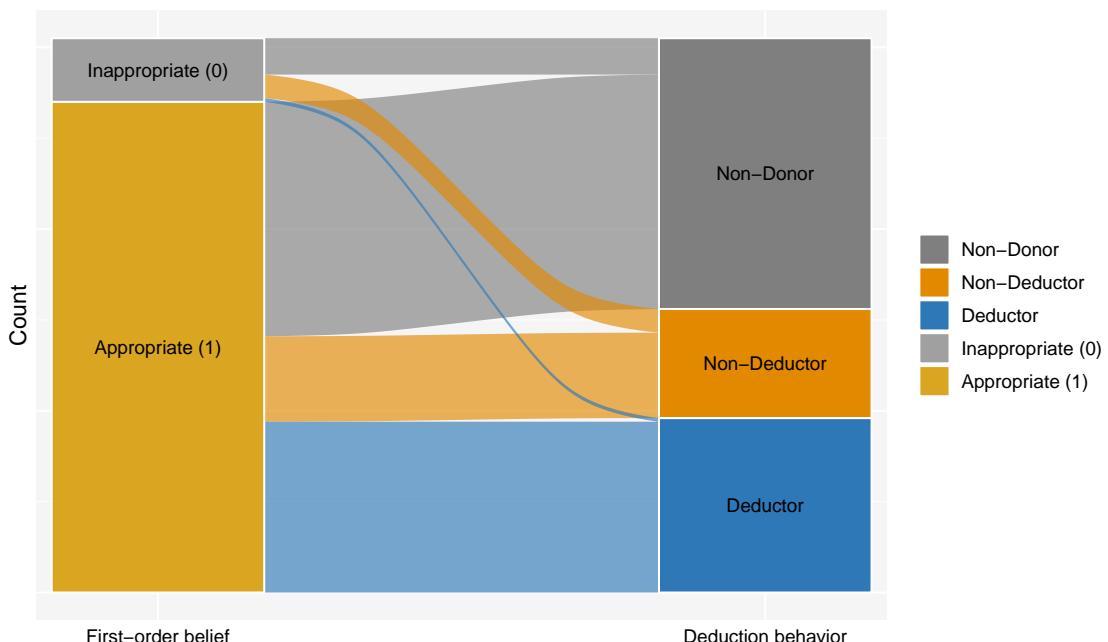


Figure 14: Flows from first-order moral beliefs (appropriateness of deduction) to deduction behavior.

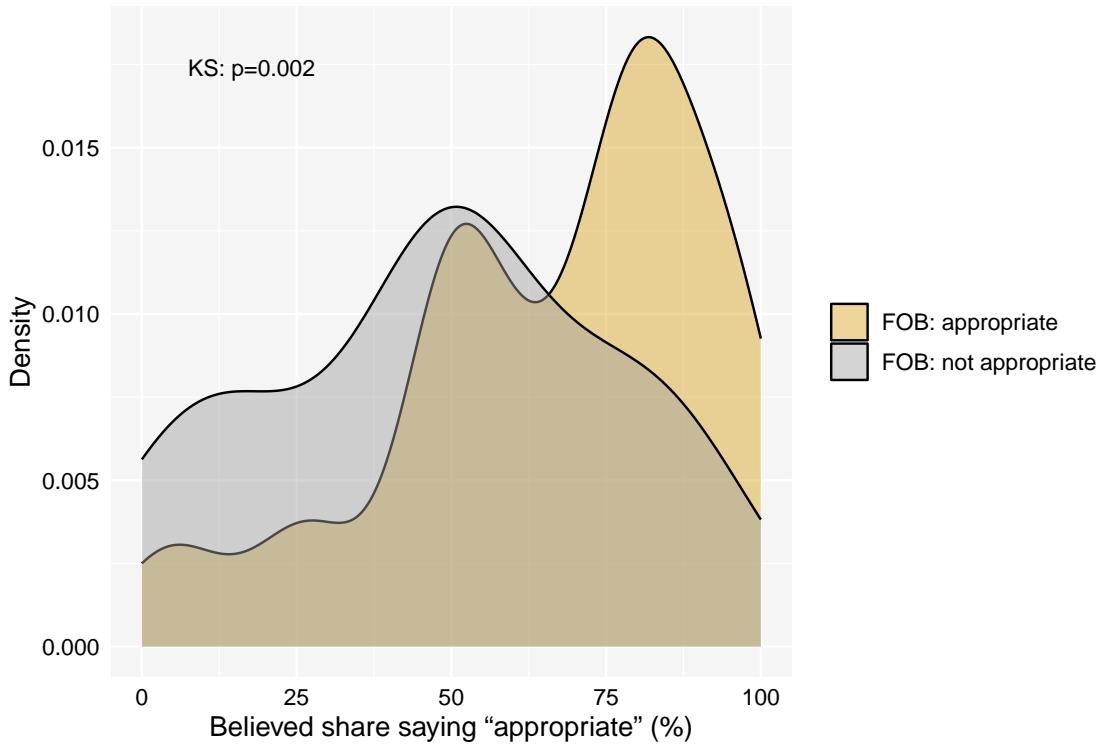


Figure 15: Second-order beliefs about others’ views by respondents’ own first-order stance (Kolmogorov–Smirnov $p = 0.002$).

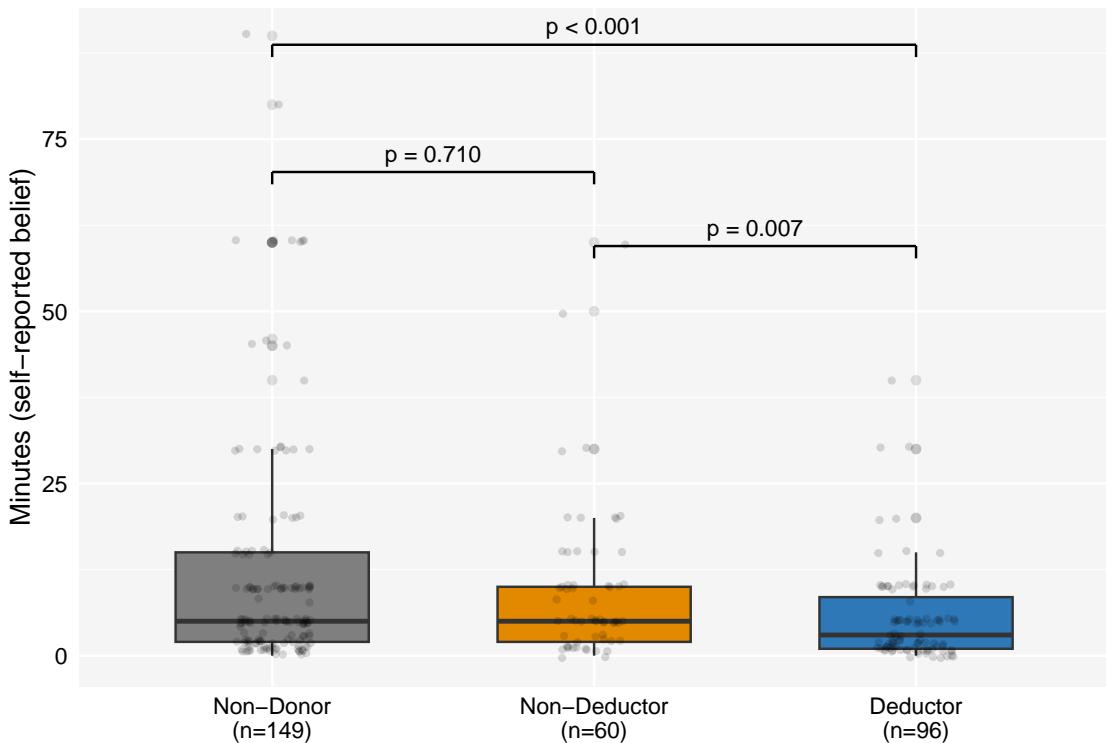


Figure 16: Perceived time required to deduct donations, by respondents’ own behavior (Kruskal–Wallis $p < 0.001$).

A.3 Online Experiment

This subsection complements Section 4 with descriptive checks from the online experiment. Figure 17 confirms that treatment messages left donation incidence unchanged—rates are statistically indistinguishable across the *Control*, *Information*, and *Morality* groups. Figure 18 examines allocation across tax-deductible and non-deductible charities. Participants in every treatment donated substantially more to eligible charities, but the share given to eligible causes did not differ by treatment. Finally, Figure 19 shows that among participants who donated to at least one eligible charity, roughly 85–90% claimed the deduction irrespective of treatment, underlining the high baseline take-up rate. Taken together, the evidence indicates that while deductibility shapes allocation choices, our messages did not affect whether or how much participants donated, nor the decision to claim a deduction.

A.3.1 Power Analysis for the Online Experiment

Because only donors to at least one deductible NGO face the deduction decision, the relevant sample sizes are smaller than the full experimental $n = 483$: Control ($n = 94$), Information ($n = 92$), and Morality ($n = 101$). The baseline deduction rate in the Control group was 85%.

We compute the minimum detectable effect size (MDES) for a two-sample test of proportions with two-sided $\alpha = 0.05$ and 80% power. For unequal group sizes n_1 (Control) and n_2 (Treatment), the MDES $\Delta = |p_2 - p_1|$ solves

$$\Delta = z_{1-\alpha/2} \sqrt{p.(1-p.) \left(\frac{1}{n_1} + \frac{1}{n_2} \right)} + z_{0.80} \sqrt{\frac{p_1(1-p_1)}{n_1} + \frac{p_2(1-p_2)}{n_2}},$$

with $p_1 = 0.85$, $p_2 = p_1 + \Delta$, $p. = (p_1 + p_2)/2$, $z_{1-\alpha/2} = 1.96$, and $z_{0.80} = 0.842$.

Results:

- Information vs. Control ($n_2 = 92$ vs. $n_1 = 94$): MDES ≈ 11.8 percentage points.
- Morality vs. Control ($n_2 = 101$ vs. $n_1 = 94$): MDES ≈ 11.6 percentage points.
- Applying a Bonferroni correction for two simultaneous tests ($\alpha = 0.025$) increases the MDES to about 12.5 percentage points.

This means that while the experiment was well-powered to detect large treatment effects, it had limited ability to identify more modest shifts around the already high baseline. The absence of significant differences between treatment and control is therefore consistent with either a true null effect or effects smaller than this detection threshold.

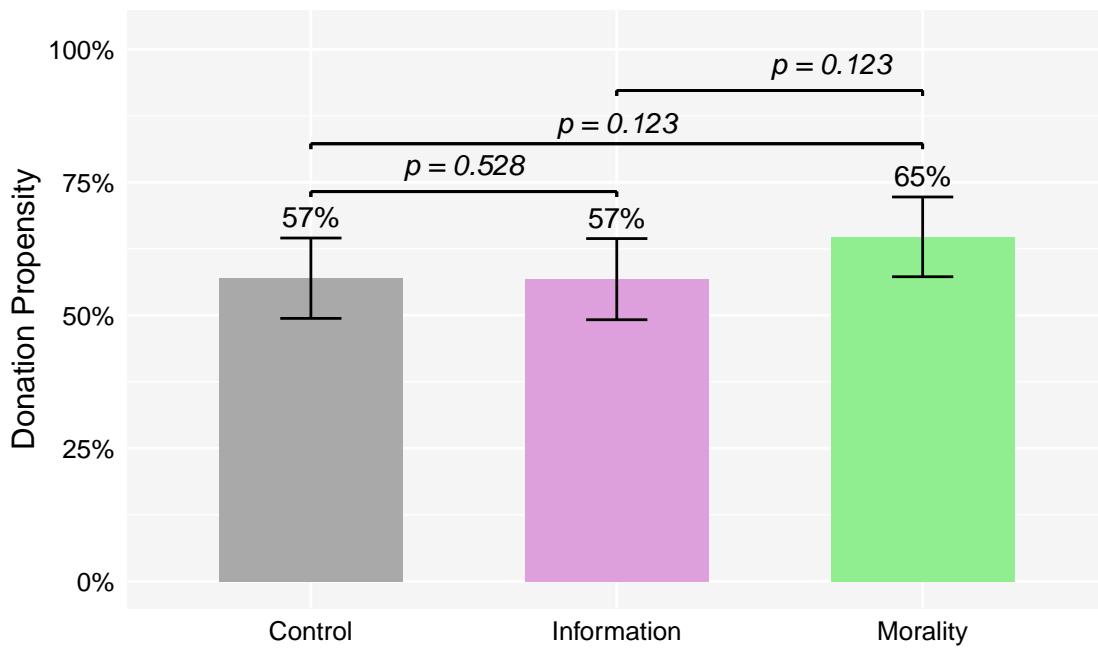


Figure 17: Donation propensity by treatment in the online experiment.

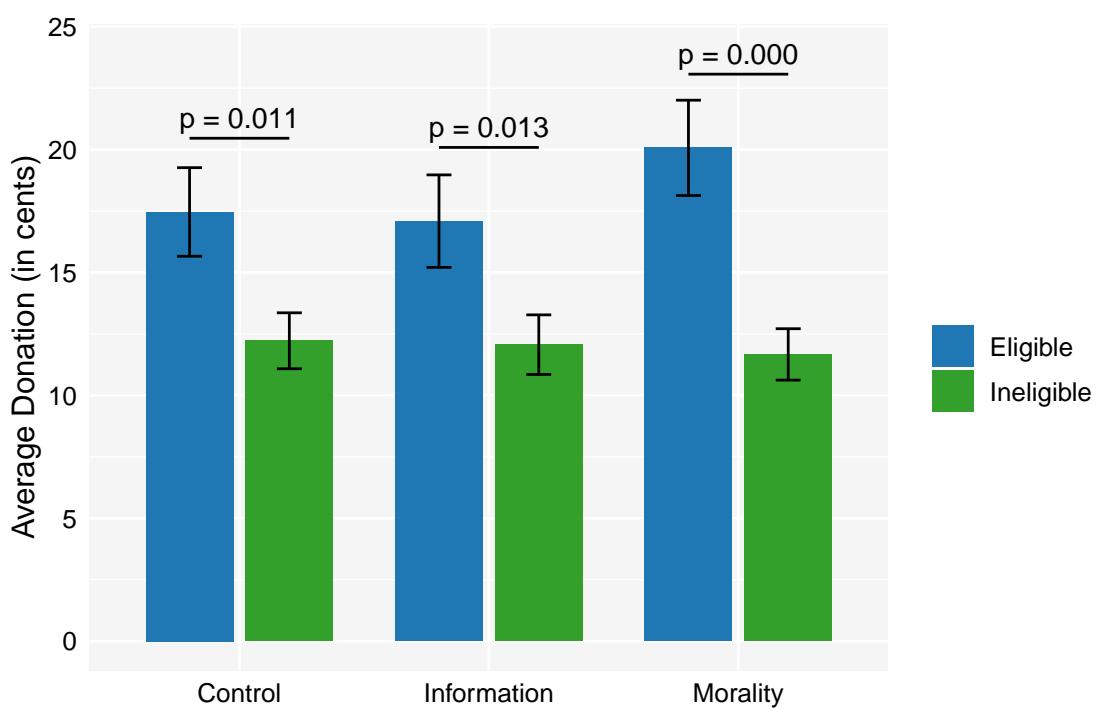


Figure 18: Average donation to eligible vs. ineligible charities by treatment.

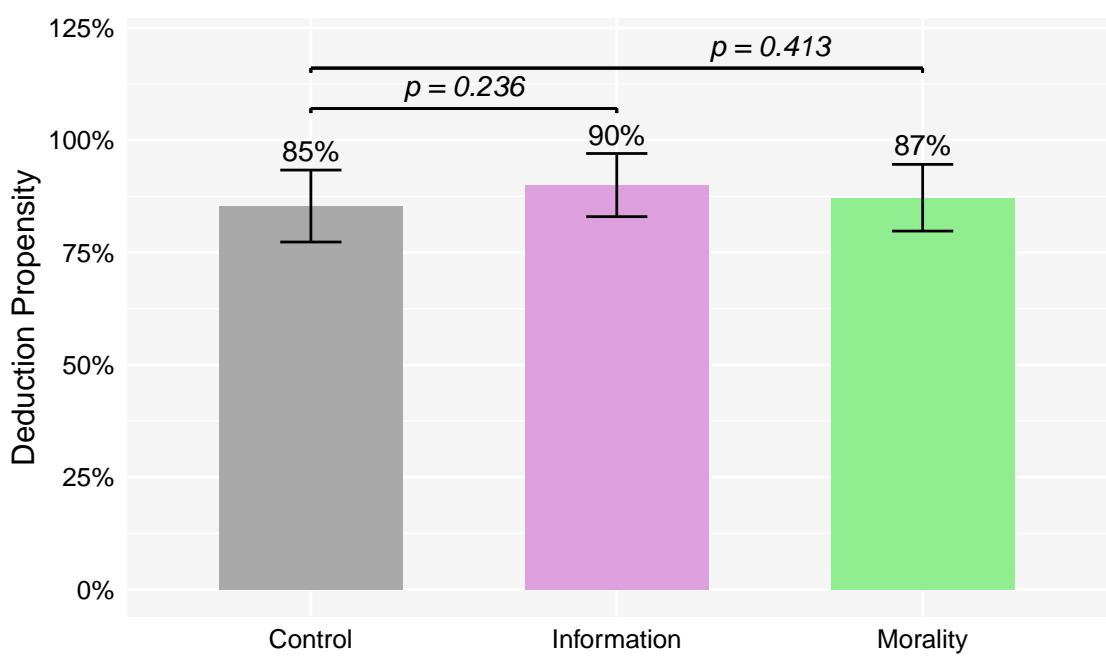


Figure 19: Deduction propensity among participants who donated to at least one eligible charity, by treatment.

A.4 Field Experiment 1

This subsection expands on Section 3.1.1 by probing the robustness of the *Information* and *Morality* treatments under alternative aggregation rules. We move from treatment–unit–level analysis (parish-union or parish instances) that covers all Austrian ZIP codes to ZIP-code-level analysis restricted to active ZIP codes—that is, codes with non-zero deduction activity in at least one of the two study years. This switch distinguishes parishes within the same parish union and removes zero-activity cells that dilute treatment effects.

Figures 20 and 21 visualize the distribution of treatment effects in the instance-level data and the adjusted share of deductors for the address-level data.

	ZIP-Year (active ZIPs)	Parish-Year (active)
Information	-4.221 (5.866) (0.472)	-0.392 (5.890) (0.947)
Num.Obs.	1626	1576
R2	0.828	0.827
Morality	-4.118 (2.932) (0.160)	-11.774* (6.545) (0.072)
Num.Obs.	1563	1560
R2	0.826	0.826
Std.Errors	by: ZIP	by: Parish
Fixed effects	ZIP + Drive-Year	Parish + Drive-Year

Only active-ZIP specifications displayed; all-ZIP specifications are identical because ZIPs with no donations have NA volume and are dropped. * p <0.1, ** p <0.05, *** p <0.01

Table 9: Treatment effects on donation volume

The table displays only active-ZIP specifications. All-ZIP specifications are identical because ZIPs with no donations have NA volume and are dropped. * p <0.1, ** p <0.05, *** p <0.01

A.4.1 Minimum Detectable Effect Size at the Address Level

To assess the power of the address-level experiment, we compute the minimum detectable effect size (MDES) under simple binomial assumptions. Each treatment arm contained approximately 2,200 addresses, with a baseline deduction rate of about 0.66%. The standard error for a difference in proportions between two arms is given by

$$SE \approx \sqrt{p(1-p) \left(\frac{1}{n_T} + \frac{1}{n_C} \right)},$$

where p is the baseline probability and n_T , n_C are the number of observations per arm. Plugging in $p = 0.0066$ and $n_T = n_C \approx 2,200$ yields $SE \approx 0.00242$ (0.242 percentage

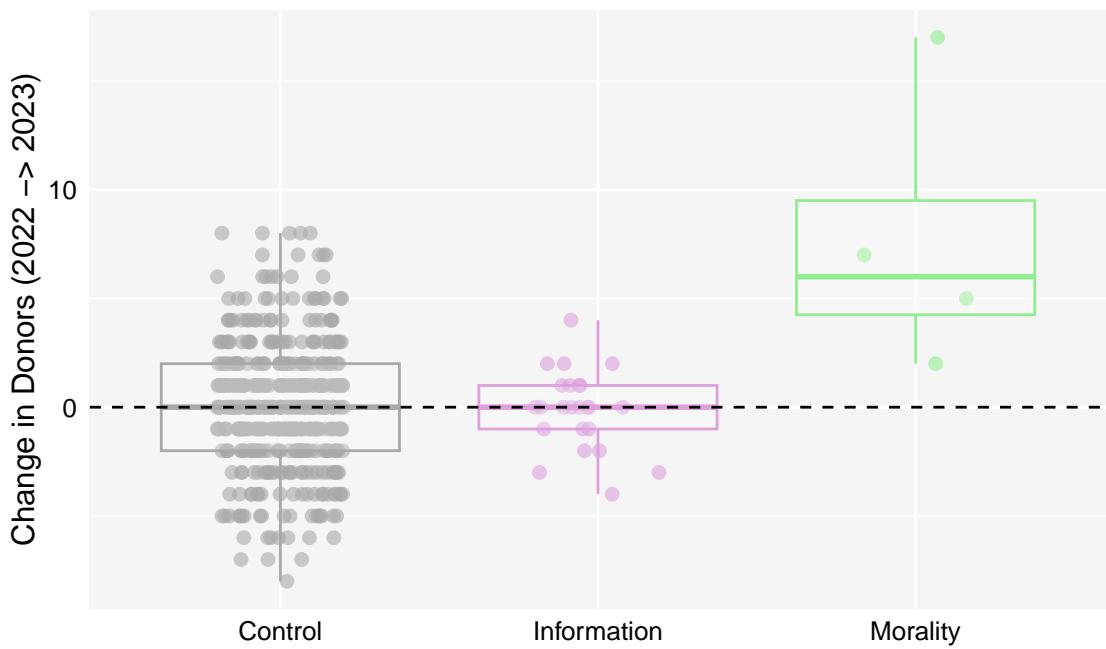


Figure 20: Distribution of treatment effects on the change in deducting donors.

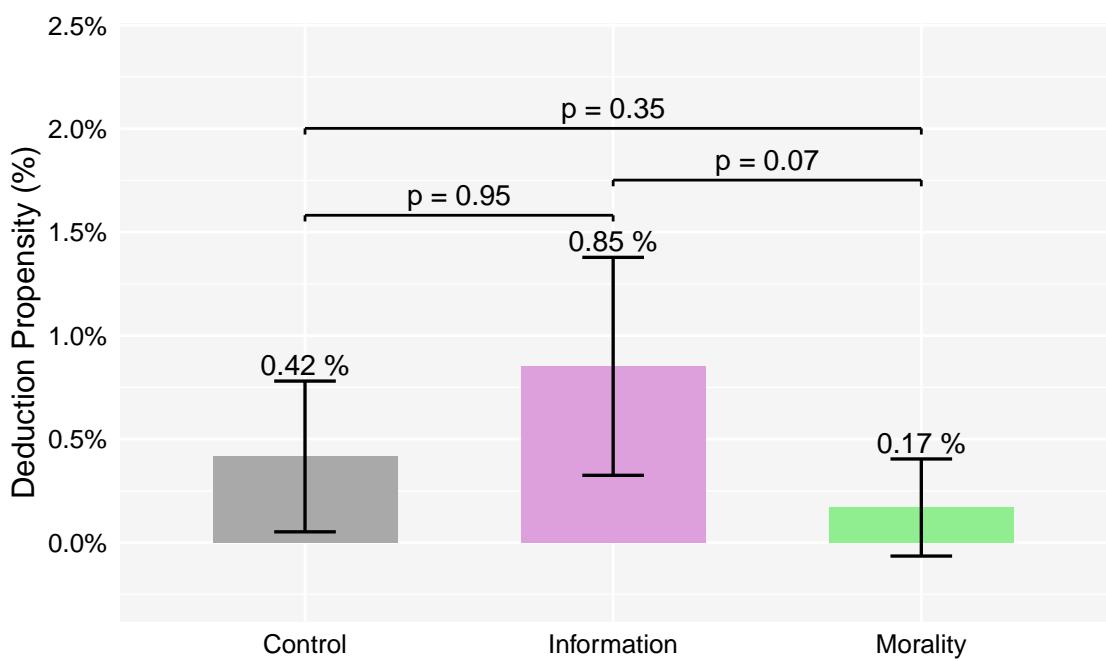


Figure 21: Share of deductors in the address-level data adjusted by donation propensity.

points). A two-sided test at the 5% level with 80% power requires an effect of approximately $(1.96 + 0.84) \times SE \approx 0.0068$, i.e., 0.68 percentage points. This corresponds to about 15 additional deductors in the Morality arm. By comparison, the effect implied by the ZIP-level analysis is only about 0.22 percentage points (≈ 5 deductors). The address-level study was therefore underpowered to detect effects of the magnitude observed at the ZIP level.

A.4.2 *Information* treatment

For 70 years, children and adults in Austria have been going from door to door as carol singers. Although cars have now become an important means of transport for bridging the sometimes large distances between houses, sleds were still used in the early years depending on snow conditions. The forms of payment have also evolved over time; in some parishes it is now possible to donate by debit card. Yet even though minor aspects of carol singing have continually adapted to the spirit of the times, the core of the campaign has remained unchanged: doing good for people in poor countries around the world. One important change, however, occurred in 2009. Since then, your donations to the carol singers have been tax-deductible. To claim the deduction, simply give your name and date of birth to the adult accompanying the carol-singing group, who then transmits your details directly to the tax office.

The German original can be inferred from Figure 40 in Online Appendix B.4: it is the leaflet text minus the final paragraph that is unique to the Morality treatment.

A.4.3 *Morality* treatment

The Morality treatment reproduced the same text as the Information treatment and added the following paragraph:

Being able to deduct donations even makes it possible to provide greater support to those in need. A recent survey also found that 88 % of respondents consider it morally appropriate to deduct donations.

The full German version, including this additional paragraph, is also visible in Figure 40 in Online Appendix B.4.

A.5 Field Experiment 2

This appendix documents the alternative specifications and robustness checks that underpin the null result in Field Experiment 2 (subsection 3.2). We report (i) DiD event-time dynamics and alternative DiD estimators, (ii) RDD estimates across bandwidths ($\pm 10, \pm 20, \pm 30$ km) including a 1 km donut exclusion and density/continuity diagnostics, and (iii) placebo tests using pre-treatment periods. Across all exercises, estimates remain small and statistically indistinguishable from zero, reinforcing the main-text conclusion.

A.5.1 Difference-in-Differences (DiD)

Table 3 in the main text (subsection 3.2) reports the core two-way fixed effects Poisson pseudo-maximum-likelihood DiD estimates. Here, we provide additional specifications and placebo checks:

- Table 10 extends the analysis to the 2022–2025 window.

- Tables 11 and 12 report placebo DiD estimates, documenting the violation of parallel trends in Carinthia in 2024 but not in earlier years.
- Figure 22 visualizes placebo DiD effects across years, and Figure 23 shows dynamic triple-DiD estimates.

	# Deductors (percent change)
<i>Information</i> (Carinthia)	-49.1 (21.9) (0.116)
<i>Morality</i> (Upper Austria)	8.6 (22.3) (0.689)
Num.Obs.	180
Pseudo R2	0.657
Std.Errors	by: date
FE: state	X
FE: date	X

Standard errors and p-values in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 10: TWFE PPML DiD — extended sample (2022–2025; state \times date FE; clustered by date).

	# Deductors (percent change)
<i>Information</i> (Carinthia)	-43.6** (15.7) (0.040)
<i>Morality</i> (Upper Austria)	-0.4 (13.1) (0.975)
Num.Obs.	90
Pseudo R2	0.785
Std.Errors	by: date
FE: state	X
FE: date	X

Standard errors and p-values in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 11: Placebo DiD (2024 vs 2023; state \times date FE; clustered by date).

	# Deductors (percent change)
<i>Information</i> (Carinthia)	-21.4 (50.8) (0.655)
<i>Morality</i> (Upper Austria)	93.2 (109.1) (0.176)
Num.Obs.	90
Pseudo R2	0.785
Std.Errors	by: date
FE: state	X
FE: date	X

Standard errors and p-values in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 12: Placebo DiD (2023 vs 2022; state \times date FE; clustered by date).

A.5.2 Regression Discontinuity Design (RDD)

The main RDD results are in section 3.2.1, Table 4. We complement these with robustness checks (donut specification, asinh transform) and alternative bandwidths:

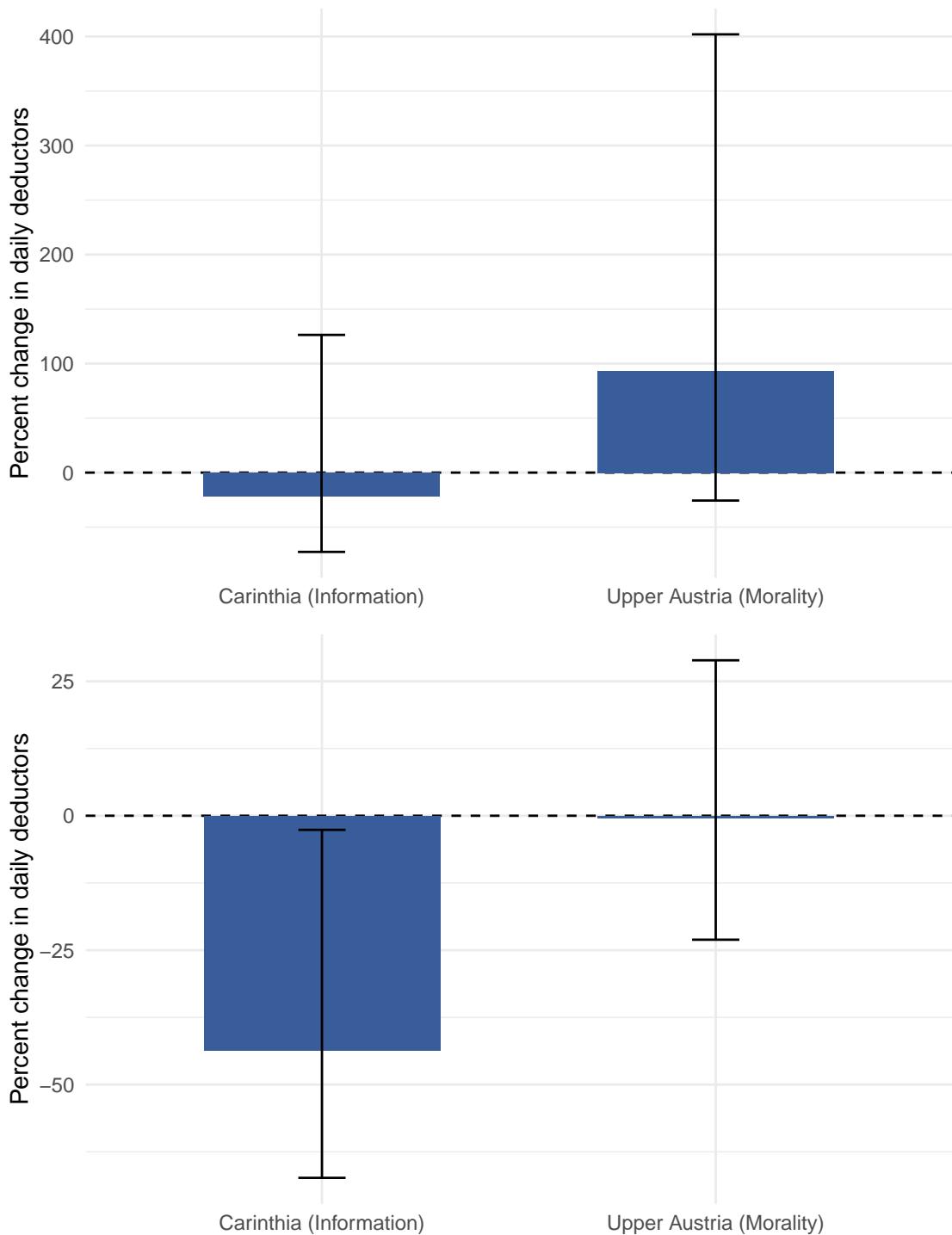


Figure 22: Placebo DiD plots: 2023 vs 2022 (top) and 2024 vs 2023 (bottom).

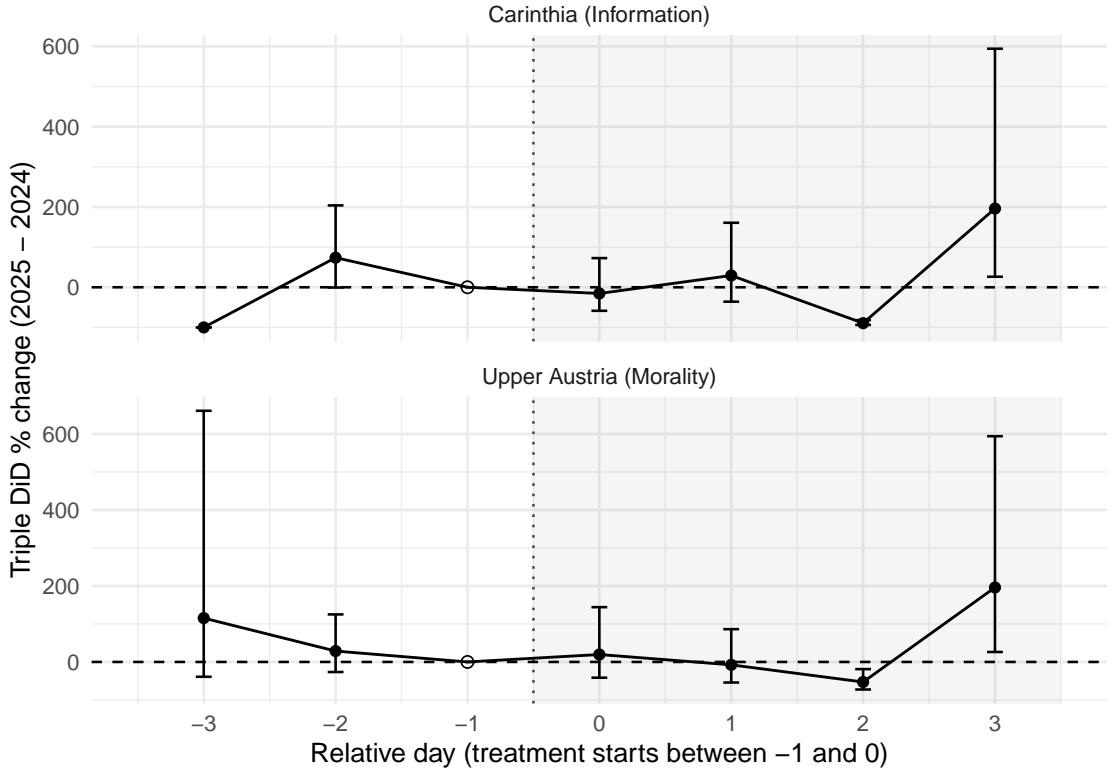


Figure 23: Triple DiD (daily) with state-clustered confidence intervals.

	<i>Information</i> (Carinthia)	<i>Morality</i> (Upper Austria)
# Deductors (level)	-6.589* (3.7) (0.072)	3.909 (5.2) (0.451)
Num. Obs.	$n_L/n_R: 18 / 19$	$n_L/n_R: 26 / 155$

Table 13: RDD robustness checks: donut exclusion and alternative transformations.

Standard errors and p-values in parentheses. n_L and n_R denote the number of observations to the left (outside treatment area) and right (inside treatment area) of the cutoff with 20km bandwidth that are not within a 1km-donut of the broadcast border. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

	<i>Information</i> (Carinthia)	<i>Morality</i> (Upper Austria)
# Deductors (level)	−26.313 (16.3) (1.000)	3.145 (3.1)) (1.000)
<i>Num. Obs.</i>	n_L/n_R : 21 / 22	n_L/n_R : 52 / 181

Table 14: Placebo RDD estimates (2024 only; Jan 2–6).

Standard errors and p-values in parentheses. n_L and n_R denote the number of observations to the left (outside treatment area) and right (inside treatment area) of the cutoff with 20km bandwidth. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

	h=10km	h=20km	h=30km
<i>Information</i> (Carinthia)	−0.456 (0.5) (0.357)	−0.485 (0.3) (0.124)	−0.478* (0.3) (0.087)
<i>Num. Obs.</i>	n_L/n_R : 7 / 21	n_L/n_R : 18 / 24	n_L/n_R : 31 / 24
<i>Morality</i> (Upper Austria)	−0.030 (0.2) (0.899)	0.062 (0.3) (0.807)	−0.001 (0.2) (0.997)
<i>Num. Obs.</i> (n_L/n_R)	n_L/n_R : 16 / 139	n_L/n_R : 29 / 178	n_L/n_R : 40 / 179

Table 15: RDD results across bandwidths (h = 10, 20, 30 km)

Standard errors and p-values in parentheses. n_L and n_R denote the number of observations to the left (outside treatment area) and right (inside treatment area) of the cutoff with indicated bandwidth. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

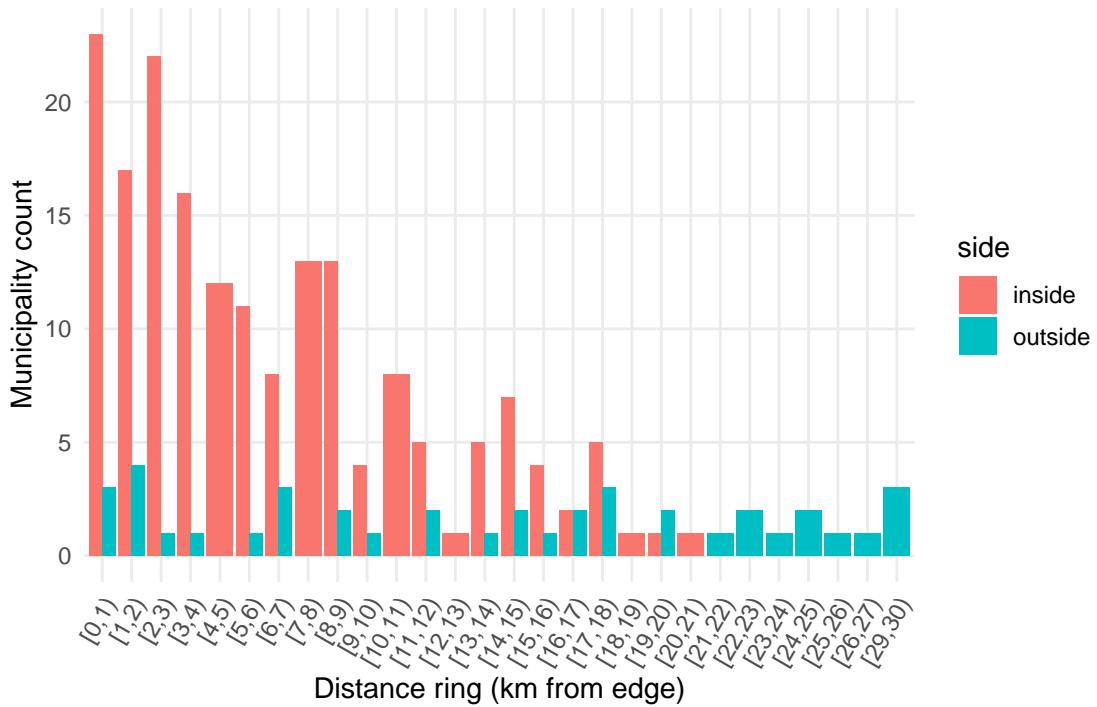
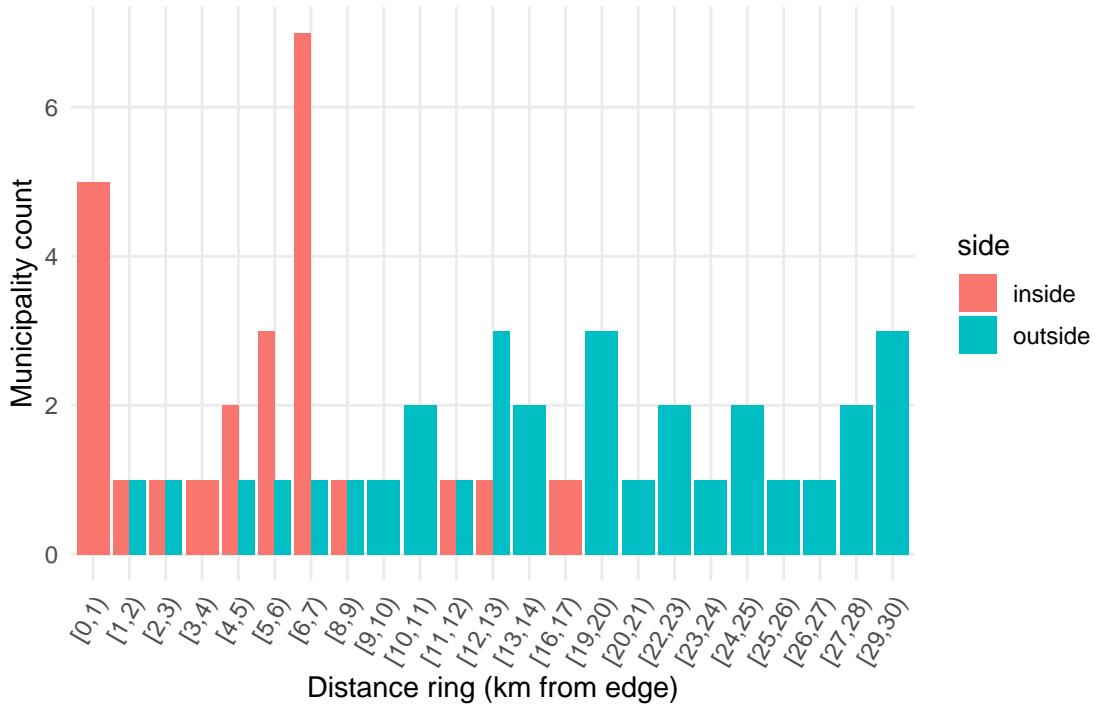


Figure 24: Municipality counts by 1 km ring around treatment cutoff (Carinthia top, Upper Austria bottom).

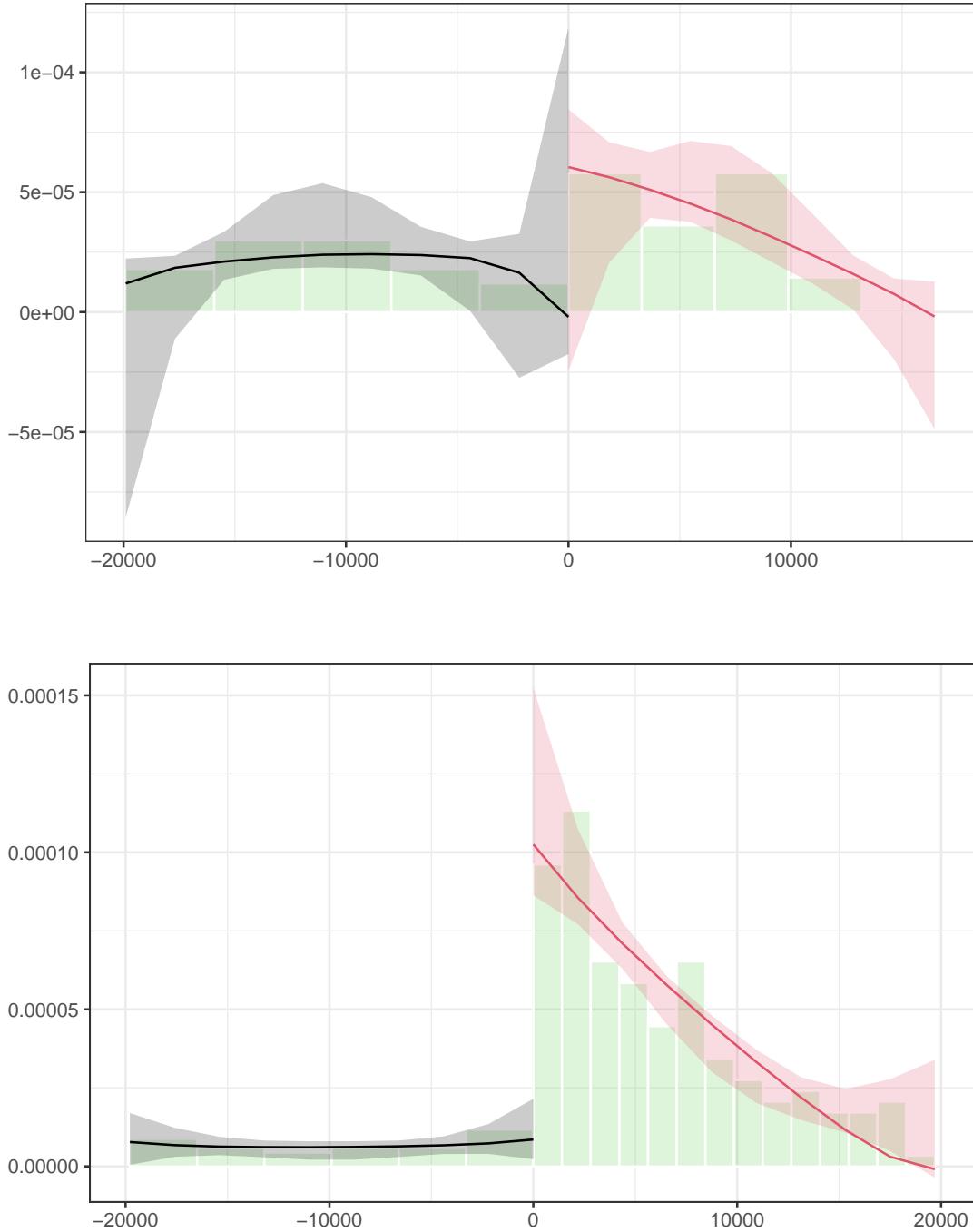


Figure 25: McCrary density tests at the cutoff (Carinthia top, Upper Austria bottom).

We implement McCrary density tests to assess whether observations may have been selectively sorted around the predicted signal cutoff. In Upper Austria, we observe a visible discontinuity, with higher density just above the threshold than below, suggesting potential sorting or measurement error that complicates the validity of the RDD in this state. In Carinthia, by contrast, the density appears smooth across the cutoff, providing no evidence of manipulation. Overall, these results advise caution in interpreting the Upper Austria RDD estimates, while supporting the use of the design in Carinthia.

A.6 Lab experiment

This subsection presents the Lin-adjusted regression results (Table 16) and the List et al. (2024) flexible regression adjustment procedure (Table 17) and supplements Section 5 with balance checks, power calculations, and heterogeneity analyses.

	Deduction propensity — AME (pp)
Observability (overall, Lin-adjusted)	-13.94 (10.79) (0.196)
<i>RI p-value</i>	0.088
Moral message (overall, Lin-adjusted)	-4.25 (9.96) (0.670)
<i>RI p-value</i>	0.596
Num.Obs.	185
Clusters (session)	52
Std.Errors	session
FE	session
Controls	gender, age, degree

Table 16: Lin (2013)-adjusted average marginal effects on deduction propensity

Standard errors and conventional p-values in parentheses. RI p-values are based on 10,000 iterations and appear on the third line below each effect. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Flexible regression adjustment (AIPW robustness)

As a robustness check we implement a cross-fitted augmented inverse-probability (AIPW) estimator that respects the session-blocked randomization. We split the donors' sample by *session*, fit flexible outcome models $m_w(X) = \mathbb{E}[Y | X, W=w]$ on training folds using random forests (covariates: gender, age, degree, and the other randomized arm), predict on held-out folds, and combine these predictions with *session-specific* treatment propensities $p_s = \Pr(W=1 | \text{session})$ to form influence scores. Sessions with no within-session variation in treatment are excluded. The ATE equals the mean influence score; we report HC1 standard errors clustered by session. Among donors, this AIPW estimator yields an effect of -11.9 pp (SE 8.41, $N = 164$), consistent in sign and magnitude with the Lin-adjusted estimate reported in the main text. Given the small set of covariates and modest sample size, we view this ML adjustment as a precision-oriented robustness rather than a separate headline result.

Deduction propensity — AME (pp)		
Observability (donors, AIPW-RF, blocked)		-11.91
		(8.41)
		(0.156)
Num.Obs.		164
Sessions		40

Standard errors (clustered by session, HC1) and p-values in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 17: List et al. (2024)-based average marginal effects on deduction propensity

Treatment cell	N	Mean age	Share female (%)
Anonymity \times Moral message	43	23.8	58.1
Anonymity \times No message	52	25.0	59.6
Observability \times Moral message	51	24.5	52.9
Observability \times No message	39	25.9	51.3

Table 18: Summary statistics of donor characteristics by treatment.

Effect	AME (pp)	SE (pp)	MDE (one-sided, pp)	MDE (two-sided, pp)
Observability	-13.94	10.79	26.8	30.2
Moral message	-4.25	9.96	24.8	27.9

Table 19: Post-hoc minimum detectable effects (80% power, $\alpha=0.05$; session-clustered HC1).

MDEs assume 80% power and $\alpha = 0.05$. One-sided uses $z_{1-\alpha} = 1.645$, two-sided uses $z_{1-\alpha/2} = 1.96$. Computed as $(z_+ + z_{0.8}) \times \text{SE}$ with HC1 cluster-robust SEs (sessions).

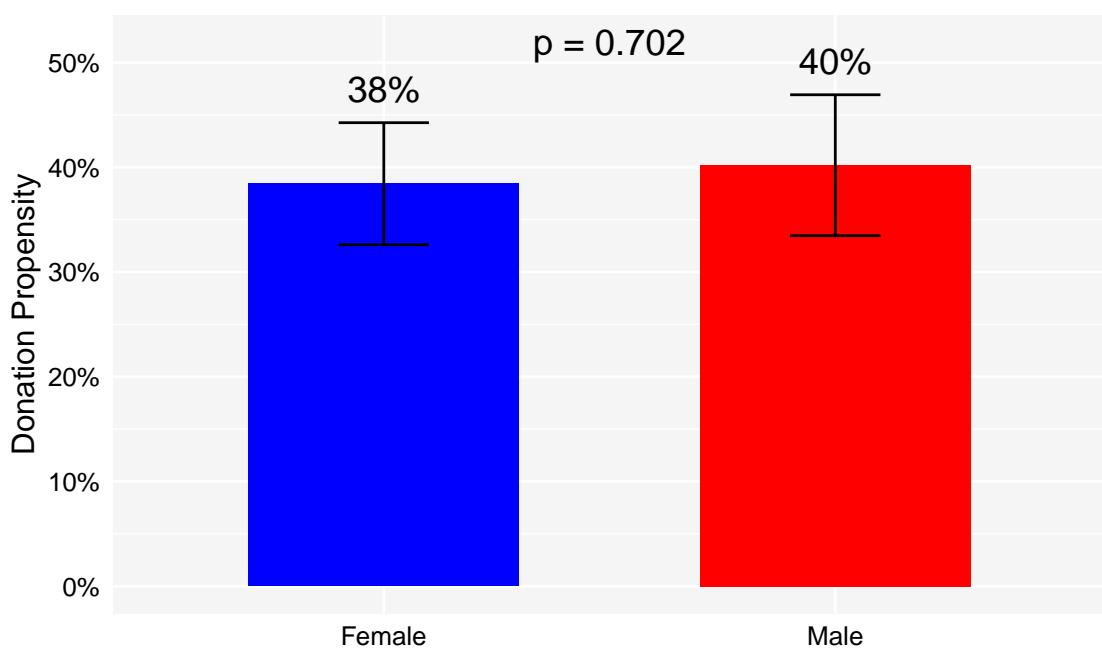


Figure 26: Donation rate by gender across treatments.

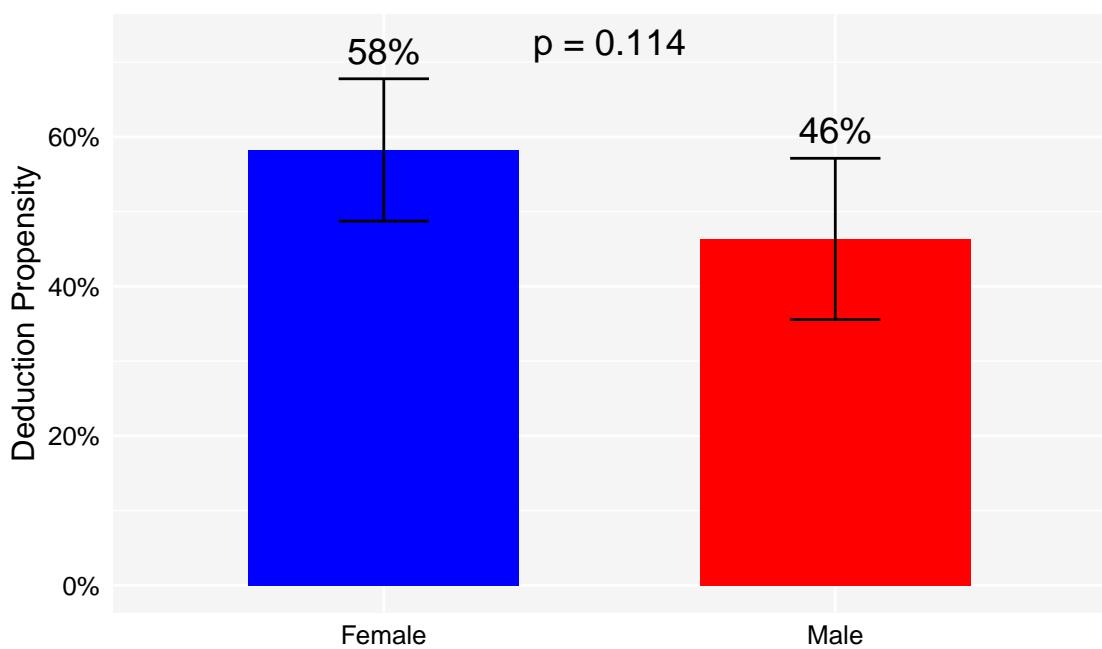


Figure 27: Deduction rate by gender across treatments.

B Online Appendix

B.1 Empirical Data

2017 tax reform: heterogeneity analysis

This subsection reports how the 2017 tax-reform effect on deduction rates and average deducted volume varies by income, gender, age, and federal state. Tables 20 to 22 present fixed-effects (FE) regressions for deduction propensity, while Tables 23 to 25 show the corresponding estimates for average deducted amounts. Figures 28 to 31 visualize the same estimates.

	(1)
Post-2017	0.005*** (0.001)
Post-2017 × Rich	0.007*** (0.002)

* p <0.1, ** p <0.05, *** p <0.01, standard errors in parentheses. “Post” is an indicator variable capturing post-reform observations. “Rich” is an indicator for above-median income.

Table 20: FE regression of 2017 tax reform effect on deduction propensity by income

	(1)
Post-2017	0.013*** (0.002)
Post-2017 × Male	-0.013*** (0.002)

* p <0.1, ** p <0.05, *** p <0.01, standard errors in parentheses. “Post” is an indicator variable capturing post-reform observations.

Table 21: FE regression of 2017 tax reform effect on deduction propensity by gender

	(1)
Post-2017	0.012*** (0.001)
Post-2017 \times Young	-0.017*** (0.002)

* p <0.1, ** p <0.05, *** p <0.01, standard errors in parentheses. “Post” is an indicator variable capturing post-reform observations. “Young” is an indicator for below-median age (40) in the pre-reform period.

Table 22: FE regression of 2017 tax reform effect on deduction propensity by age

	(1)
Post-2017	-24.690*** (1.086)
Post-2017 \times Rich	-37.588*** (3.528)

* p <0.1, ** p <0.05, *** p <0.01, standard errors in parentheses. “Post” is an indicator variable capturing post-reform observations. “Rich” is an indicator for above-median income.

Table 23: FE regression of 2017 tax reform effect on average deducted donation volume by income

	(1)
Post-2017	-26.978*** (1.064)
Post-2017 × Male	-10.295*** (1.999)

* p <0.1, ** p <0.05, *** p <0.01, standard errors in parentheses. “Post” is an indicator variable capturing post-reform observations.

Table 24: FE regression of 2017 tax reform effect on average deducted donation volume by gender

	(1)
Post-2017	-40.435*** (1.130)
Post-2017 × Young	37.501*** (1.420)

* p <0.1, ** p <0.05, *** p <0.01, standard errors in parentheses. “Post” is an indicator variable capturing post-reform observations.
“Young” is an indicator for below-median age (40) in the pre-reform period.

Table 25: FE regression of 2017 tax reform effect on average deducted donation volume by age

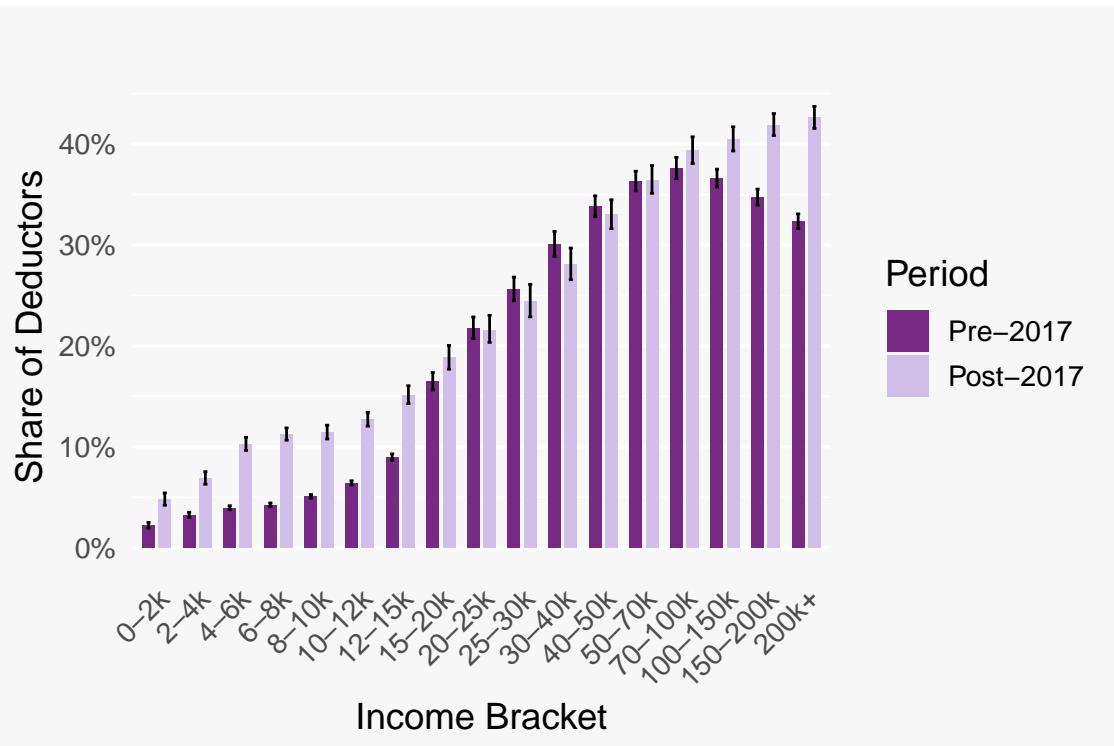


Figure 28: Effect of 2017 tax reform on deduction propensity by income

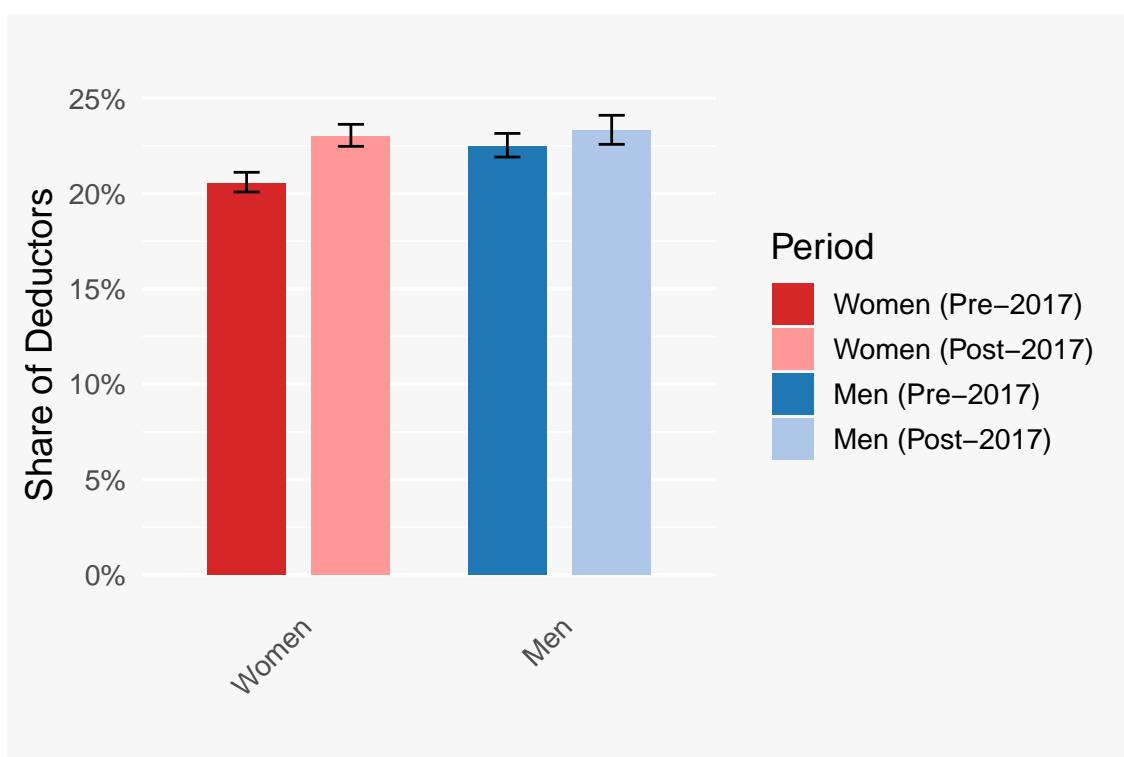


Figure 29: Effect of 2017 tax reform on deduction propensity by gender

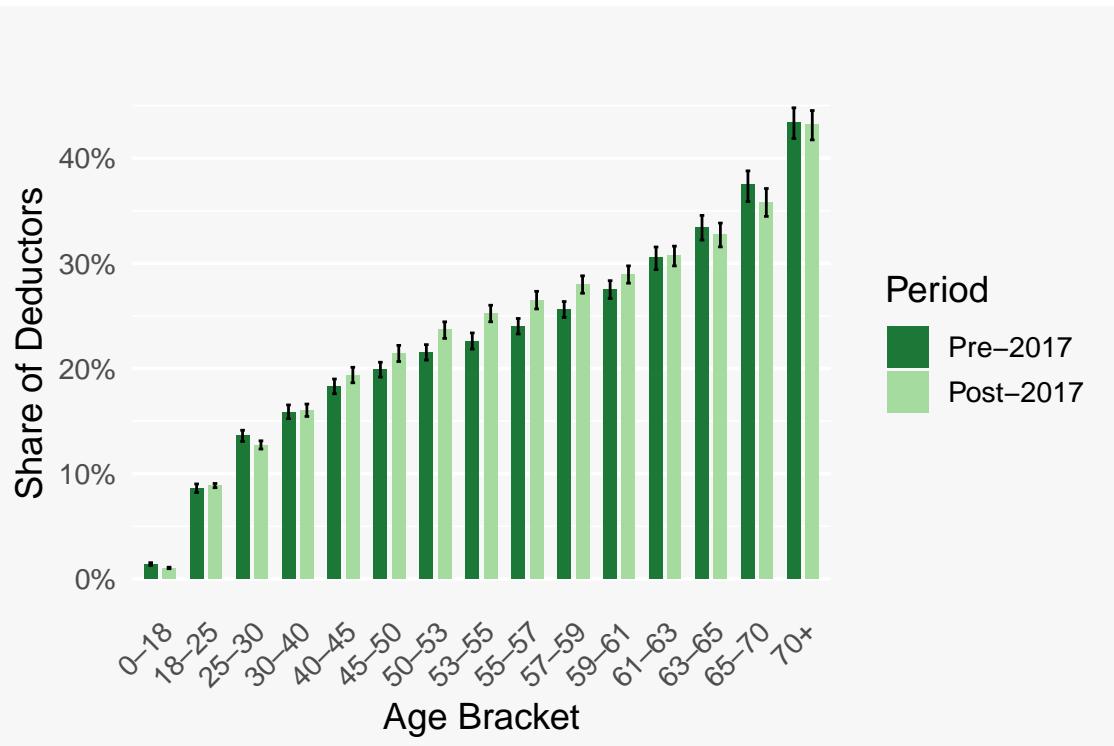


Figure 30: Effect of 2017 tax reform on deduction propensity by age

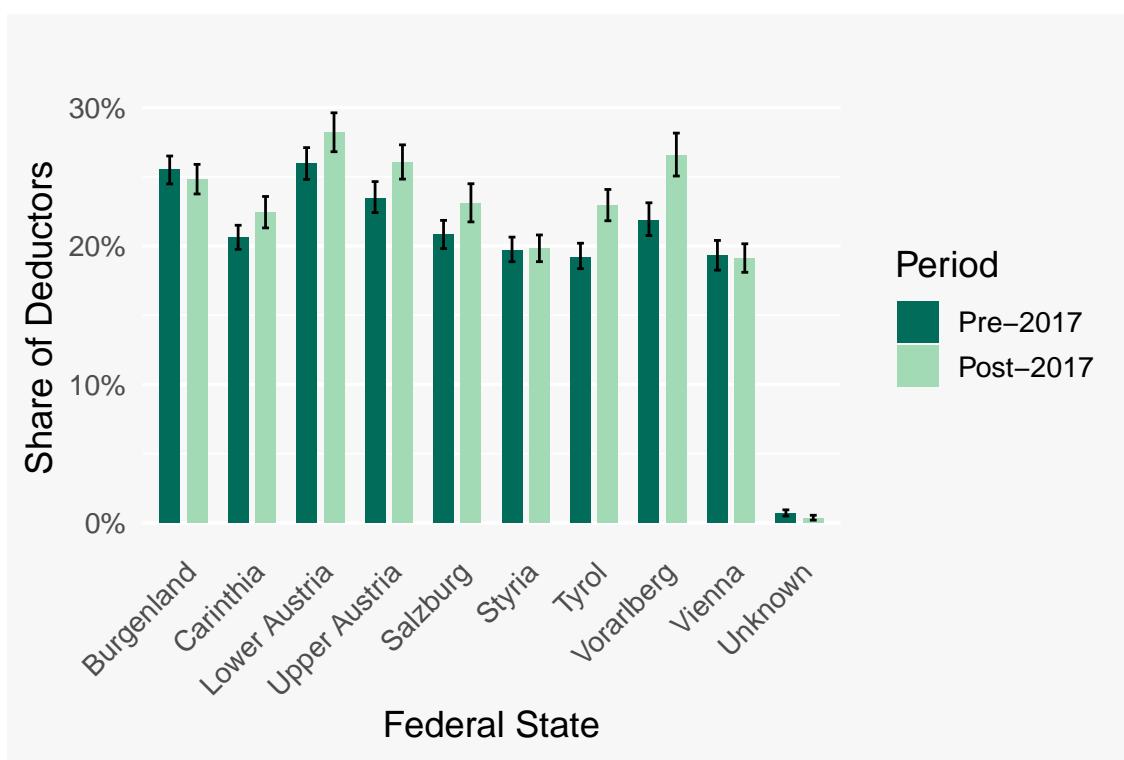


Figure 31: Effect of 2017 tax reform on deduction propensity by federal state

DIÖZESE: PFARRE: GRUPPENNUMMER: DATUM: NAME BEGLEITPERSON: TELEFONNUMMER BEGLEITPERSON:																
<div style="display: flex; justify-content: space-between;"> <div style="width: 45%;"> <p>ANLEITUNG</p> <p>Machen Sie bitte für jede Person, die Ihrer Gruppe eine Spende übergibt, einen Strich in den Strichlistekästchen. Sobald Sie fünf Striche in einem Kästchen gemacht haben, verwenden Sie bitte das nächste Kästchen. Nachstehend finden Sie ein Beispiel, in welchem sieben Personen gespendet haben:</p> <div style="text-align: right; margin-top: 10px;"> <input type="checkbox"/> <input type="checkbox"/> <input type="checkbox"/> <input type="checkbox"/> <input type="checkbox"/> <input type="checkbox"/> </div> <p>SPEZIALFÄLLE</p> <ul style="list-style-type: none"> • <i>Zwei Personen aus dem selben Haushalt spenden getrennt: zwei Striche</i> • <i>Ein Elternteil gibt seinen zwei Kindern jeweils 10€, die sie den Sternsingern überreichen: ein Strich</i> • <i>Niemand öffnet, jedoch hängt ein Kuvert mit Geld für die Sternsinger an der Tür: ein Strich</i> </div> <div style="width: 45%;"> <p>STRICHLISTE</p> <table border="1" style="width: 100%; border-collapse: collapse;"> <tr><td style="width: 33.33%; height: 30px;"></td><td style="width: 33.33%; height: 30px;"></td><td style="width: 33.33%; height: 30px;"></td></tr> <tr><td style="width: 33.33%; height: 30px;"></td><td style="width: 33.33%; height: 30px;"></td><td style="width: 33.33%; height: 30px;"></td></tr> <tr><td style="width: 33.33%; height: 30px;"></td><td style="width: 33.33%; height: 30px;"></td><td style="width: 33.33%; height: 30px;"></td></tr> <tr><td style="width: 33.33%; height: 30px;"></td><td style="width: 33.33%; height: 30px;"></td><td style="width: 33.33%; height: 30px;"></td></tr> <tr><td style="width: 33.33%; height: 30px;"></td><td style="width: 33.33%; height: 30px;"></td><td style="width: 33.33%; height: 30px;"></td></tr> </table> <p>GESAMT:</p> </div> </div>																

Figure 32: Tick list template used to estimate the average donation by non-deducting donors.

B.2 Representative Survey

Demographics / Screening

Age numeric (18–99)

Gender Male • Female • Diverse

Education No qualification • Apprenticeship • Vocational diploma • High-school diploma (Matura) • Higher

Annual gross income €0–25k • €25k–50k • €50k–75k • €75k–100k • €100k+

Postal code numeric (1000–9999)

Federal state Burgenland • Carinthia • Styria • Lower Austria • Upper Austria • Salzburg • Tyrol • Vorarlberg • Vienna

Knowledge & Perceptions

Aware that certain *expenses* are tax-deductible Yes • No

Aware that certain *donations* are tax-deductible Yes • No

Estimated share of donors who deduct numeric (0–100 %)

Estimated time to claim a deduction numeric (minutes)

“Step 1 (give name & date of birth)” – “Step 4” of deduction process (automatic refund)
I knew this • I didn’t know this

Reasons for / against deducting

Reasons *against* claiming a deduction open-ended text

Reasons *for* claiming a deduction free text

Attitudes & Behavior

Is it morally acceptable to deduct donations? Yes • No

Estimated share answering “Yes” numeric (0–100 %)

Filed a tax return at least once No • Yes, self-filed • Yes, hired someone

Deducted donations in recent years No donations • Donated but did not deduct • Yes, deducted

Vignette “Lukas”

Lukas would like to donate to the Sternsinger campaign. Option A: donate €30 and not claim a deduction. Option B: donate €35, claim a deduction and receive €8 back.

Which option is more generous? A • B • Both equally generous

Reason free text

Estimated share choosing Option A numeric (0–100 %)

Estimated share choosing Option B numeric (0–100 %)

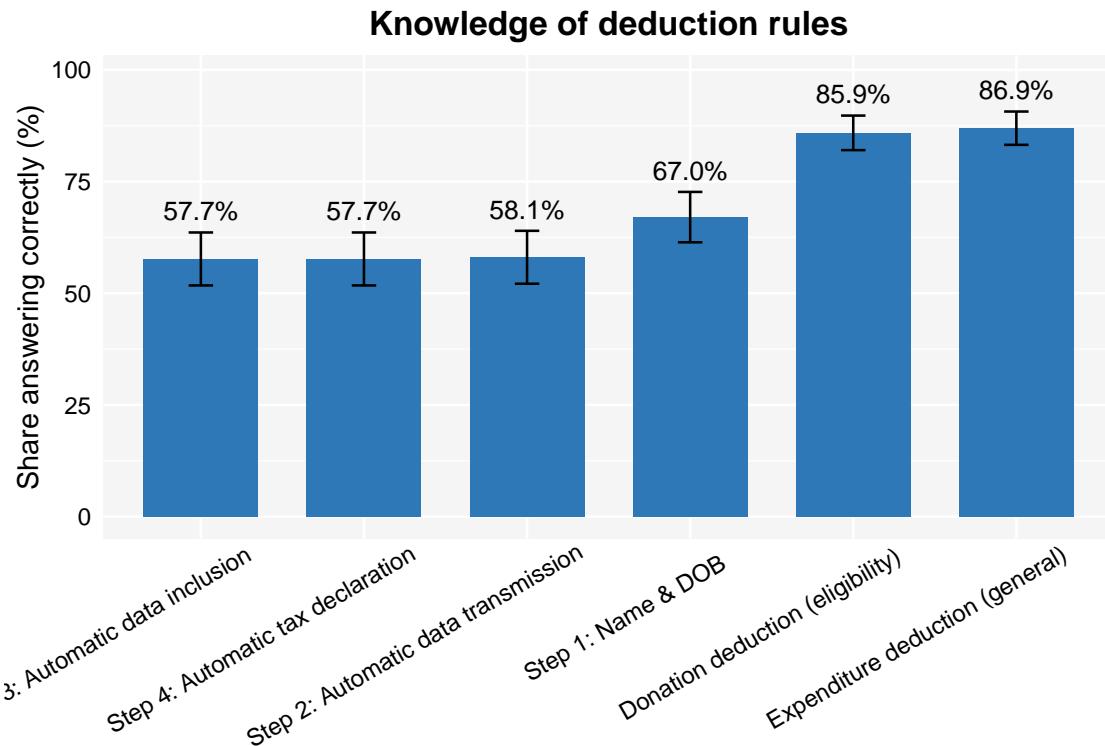


Figure 33: Knowledge of deduction rules among survey respondents.

Figure 33 provides additional detail on factual knowledge of the deduction process. While awareness was high for basic requirements (eligibility of donations, need to provide name and date of birth), knowledge was weaker for more technical rules (distinguishing between expenses and donations, and awareness of automatic transmission). This illustrates that some gaps remain even among a representative sample.

Figure 34 summarizes responses to the Lukas vignette. A majority judged the option with deduction (Option B) as more generous, and responses were strongly aligned with both respondents' own moral stance on deductibility and their past deduction behavior. This confirms that attitudes toward deduction extend into judgments of others' generosity.

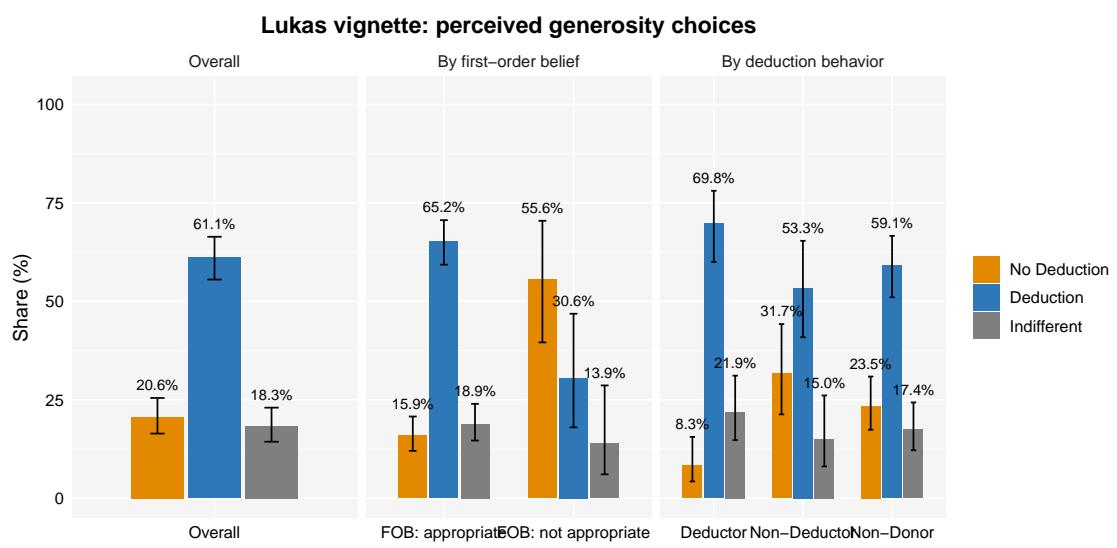


Figure 34: Perceived generosity choices in the Lukas vignette, overall and by respondents' beliefs and deduction behavior.

B.3 Online Experiment

This section reports supplementary figures from the online experiment. Figure 35 provides the anonymized charity descriptions shown to participants. The remaining figures confirm that treatment messages did not affect donation behavior, allocation, or moral beliefs.

Figure 36 shows that average donation levels (pooled across eligible and ineligible charities) were virtually identical across treatment arms. Figure 37 confirms that the share of donations allocated to eligible charities was unaffected by treatment. Finally, Figure 38 shows that first-order beliefs about the moral appropriateness of deducting did not vary across groups.

Karitative Organisationen

Name	Tätigkeiten	Beschreibung	Spendenhöhe
Karitative Organisation 1	Tierrechte, Naturschutz	Beschreibung	0
Karitative Organisation 2	Artenschutz, Naturschutz	Beschreibung	0
Karitative Organisation 3	Tierrechte, Tierethik	Beschreibung	0
Karitative Organisation 4	Bildung, soziale Arbeit	Beschreibung	0
Karitative Organisation 5	Bildung, nachhaltige Entwicklung	Beschreibung	0
Karitative Organisation 6	Humanitäre Hilfe, Bildung	Beschreibung	0
Karitative Organisation 7	Menschenrechte, freie Meinungsäußerung	Beschreibung	0
Karitative Organisation 8	Menschenrechte, Diskriminierung	Beschreibung	0
Karitative Organisation 9	Menschenrechte, Flüchtlingsarbeit	Beschreibung	0

Figure 35: Anonymized descriptions of the nine charities in the online experiment.

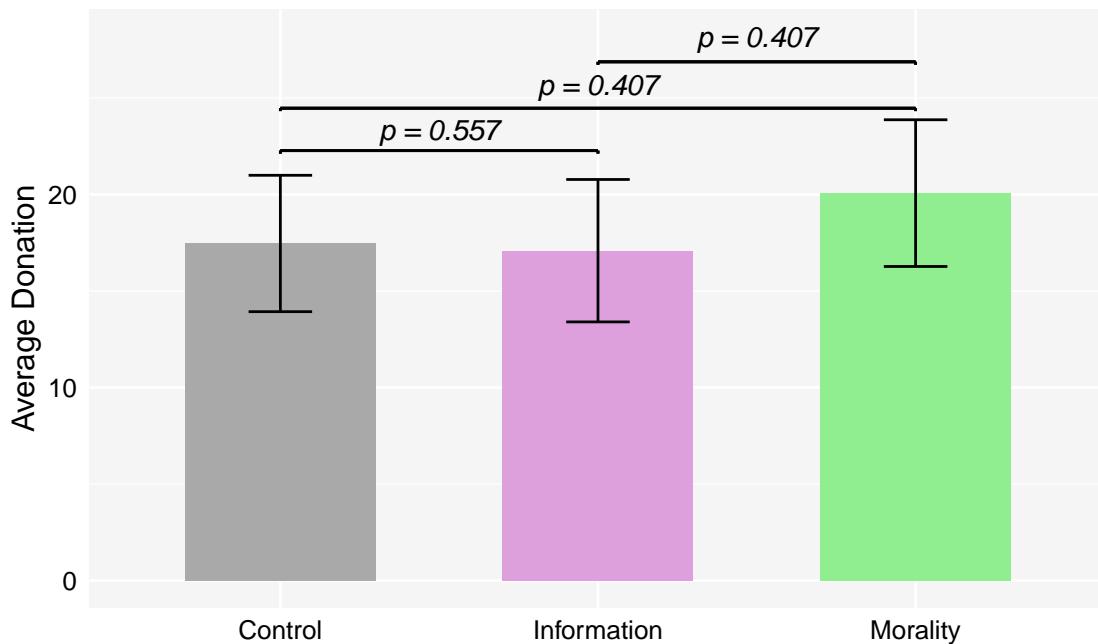


Figure 36: Average donation (pooled across eligible and ineligible charities) by treatment.

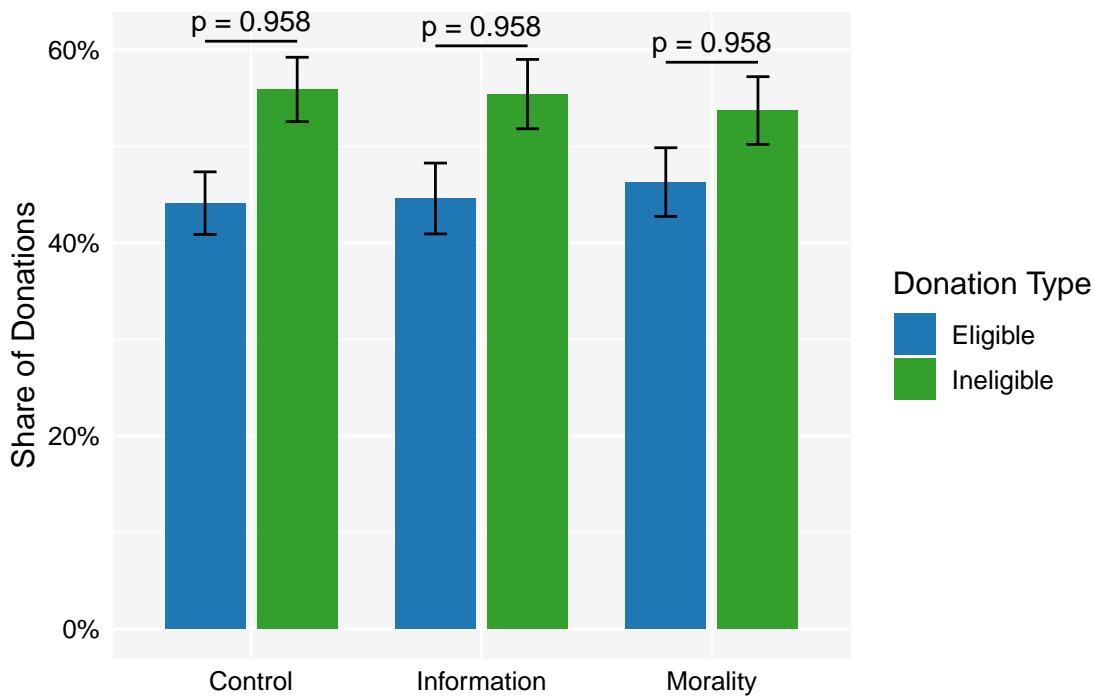


Figure 37: Share of donations allocated to eligible vs. ineligible charities by treatment.

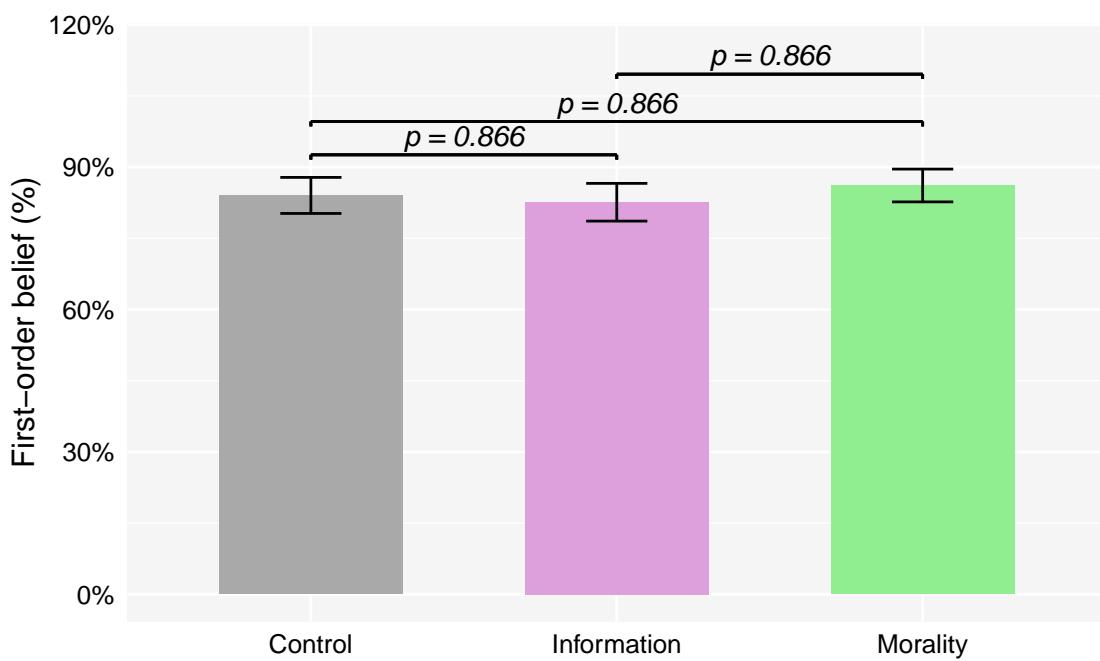


Figure 38: First-order beliefs about the moral appropriateness of deducting by treatment.

B.4 Field experiment 1

JOSEF FINK

Ehemaliger Pfarrer und Dechant

Auf über 60 Priesterjahre kann der beliebte ehemalige Pfarrer und Dechant von Gleisdorf zurückblicken. Bei unserem Besuch bei ihm in Bad Gleichenberg erzählt er von seiner Priesterweihe im Jahr 1960 im Grazer Dom, von seinen Kaplan-Jahren in St.Veit am Vogau und in Rinnbach. Aus seiner Zeit in Gleisdorf (1965 – 1993) sind ihm neben vielen persönlichen Begegnungen/ Begleitungen als Seelsorger auch „weltliche“ Erlebnisse in Erinnerung: Jagd, Geselligkeit am Hohenberg, Marizell-Wallfahrten und die legendären Krapfen- bzw. Märrbälle.



HANS WALLNER

Pensionierter Pfarrer von St. Ruprecht

Seelsorge ist für mich, für die Menschen da zu sein und sie durch alle Lebensabschnitte zu begleiten, im Besonderen in der Feier der gemeinsam gestalteten Gottesdienste. Die Feier der Taufe, der Erstkommunion, der Firmung, der Trauung und der Krankensalbung sind Lebensstationen der Menschen. Als Priester bin ich mit den Menschen in Freude und Leid verbunden. Gottes Segen und alles Gute!



**VORSTELLUNG
PASTORALER MITARBEITER**



Mein Name ist Bernd Kubin-Aber und ich komme aus Graz. Meinen Dienst im Seelsorgeraum Gleisdorf habe ich am 1. September des heurigen Jahres begonnen. Jesus Christus logischer Wissenschaft* im Jahr 2021 habe ich den ersten Schritt gesetzt, mir das notwendige Fundament für diese Berufung zu schaffen; mit der Anstellung im Seelsorgeraum Gleis-

70 JAHRE STERNSINGEN

Wir feiern ein Jubiläum!

Liebe Kinder und Erwachsene, die kommende Sternengeneration steht unter einem besonderen Stern, feiern wir doch ihr 70-jähriges Bestehen! Wie schon damals ziehen wir auch heute von Tür zu Tür, um allen Leuten mit unseren Liedern und Sprüchen eine Freude zu bereiten und Spenden für arme Menschen in anderen Ländern zu sammeln. Wir laden euch alle, egal ob groß oder klein, ob Bewohner oder erholender Teilnehmende einer mit Freunden zusammen oder mehrere Tage als Königinnen und Könige unterwegs zu uns mit dem großen Ziel, allen Menschen in unserer Pfarre einen Sternsingerebesuch zu ermöglichen. Du hast Interesse in königliche Gewänder zu schlüpfen, als Begleiterperson unterwegs zu sein oder eine Sternsingerguppe zum Mittagessen einzuladen? Dann komm' zu den Sternsingernproben und melde dich dort an oder nimm Kontakt mit dem verantwortlichen Vorbereitungsteam auf!

Sternsingernproben in Gleisdorf: 2., 9. und 16.12., jeweils um 09:30 Uhr im Pfarrzentrum. Eine Anmeldung ist vor Ort bei der ersten Probe oder unter dka.gleisdorf@gmail.com möglich.

Sternsingernproben in Hartmannsdorf: 3., 10., und 17.12., jeweils um 09:30 Uhr im Pfarrhof. Eine Anmeldung ist vor Ort bei der ersten Probe oder über den QR-Code möglich.

Sternsingernproben in Sinaibellkirchen: 3., 10. und 17.12., jeweils um 09:30 Uhr im Pfarrsaal. Eine Anmeldung ist vor Ort bei der ersten Probe oder über den QR-Code möglich.

FIRM-ANMELDUNG

Für die Firmung sind Jugendliche angesprochen, die im Jahr der Firmung auch die Anmeldung dazu erfolgt über die Website. Die Anmeldung

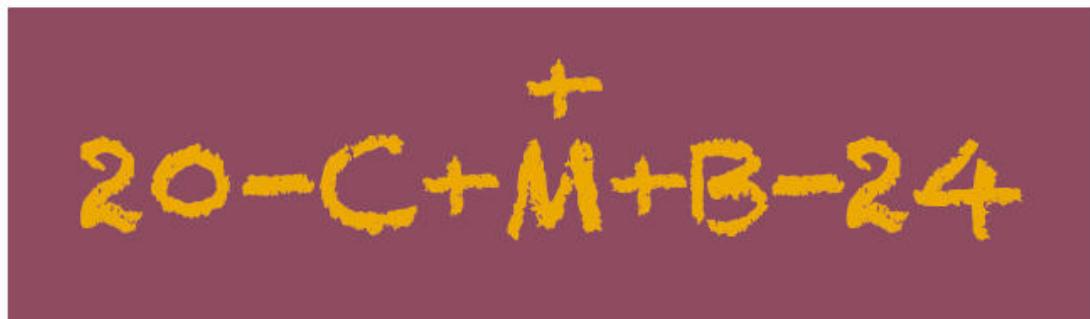


WORT DES SEELSORSORGERS
César Cabeza

Priester und Flugzeuge: Träger der Liebe und der Versöhnung

Einmal sagte Papst Franziskus zu den östlichen Kardinälen: „Priester sind wie Flugzeuge, sie machen nur dann Schlagzeilen, wenn sie abstürzen“. Soweit ich mich erinnere, endet der Spruch folgendermaßen: „Niemand erinnert sich an die, die weiterfliegen“. Das sind aber, Gott sei Dank, die meisten. In der Steiermark sind über 300 Priester ihrer Weile treu geblieben. Der Begriff „Weile“ bedeutet dem Wortlaut nach so viel wie „aussondern“ oder „trennen“, d.h. für gottesherrliche Zwecke aussondern. Das übliche deutsche Wort für das Sakrament der Ordination betont die Heiligkeit des Rituals und die Funktion der Geweihten. Erinnern wir uns einfach an die Wörter Weihwasser, Wehrach oder Weihnachten. Oft hört man auch, dass die Priester „nur“ Sakramentspender sind.

Figure 39: Sample page of a parish journal.



70 Jahre Sternsingen

In Österreich ziehen Kinder und Erwachsene seit 70 Jahren als SternsingerInnen von Tür zu Tür. Wenn auch Autos mittlerweile ein wichtiges Transportmittel sind um die teils großen Distanzen zwischen den Häusern zu überwinden, so kamen in den Anfangsjahren je nach Schneelage noch Schlitten zum Einsatz.

Auch hinsichtlich der Zahlungsmedien hat sich über die Jahre einiges getan; inzwischen kann man in einigen Pfarren bereits mit Bankomatkarde spenden. Doch selbst wenn sich laufend kleinere Aspekte des Sternsingens an die Zeichen der Zeit angepasst haben, ist der Kern der Aktion unverändert geblieben: Gutes tun für Menschen in armen Ländern der Welt.



Sternsinger im Jahr 1954 (© Kindermissionswerk)



Sternsinger im Jahr 2019 (© Martin Mittermair)

Eine wichtige Veränderung hat es jedoch im Jahr 2009 gegeben. Seit diesem Jahr sind Ihre Spenden an die SternsingerInnen steuerlich absetzbar.

Um Ihre Spende abzusetzen wenden Sie sich einfach an die Begleitperson der Sternsingergruppe. Die Begleitperson trägt Ihren Namen und Ihr Geburtsdatum in die Absetzliste ein, die im Anschluss direkt an das Finanzamt gemeldet wird.

Steuerliches Absetzen macht es sogar möglich, jenen, die Hilfe benötigen, stärker unter die Arme zu greifen. Auch fanden es 88% der Befragten einer aktuellen Umfrage moralisch angebracht Spenden abzusetzen.

Figure 40: Leaflet used in the address-randomised parish union (German original).

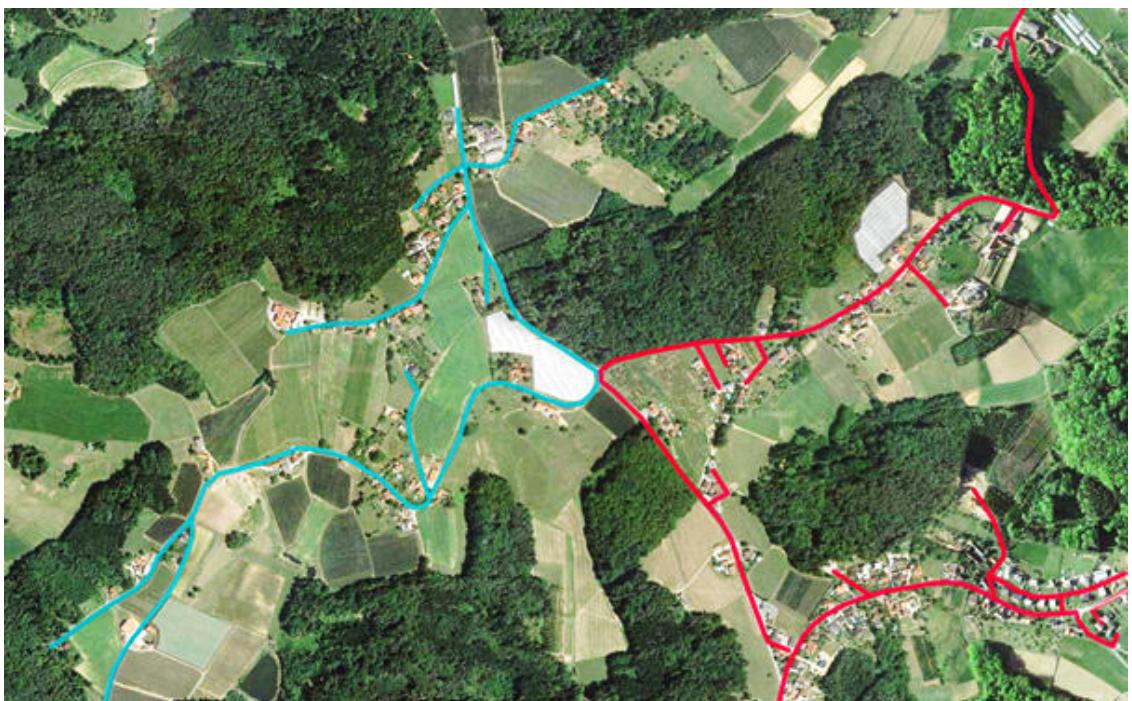


Figure 41: Two example carol-singing routes (red, green).

B.5 Field experiment 2

This section provides supplementary descriptive evidence for Field Experiment 2 as well as scripts and additional materials. The presented figures are not central to identification but offer additional context on donor composition.

Figure 42 compares the age distribution of donors in Carinthia and Upper Austria, showing broadly similar demographic profiles across the two states. Figure 43 plots the distribution of donation amounts, again showing no major systematic differences between treatment and control states.

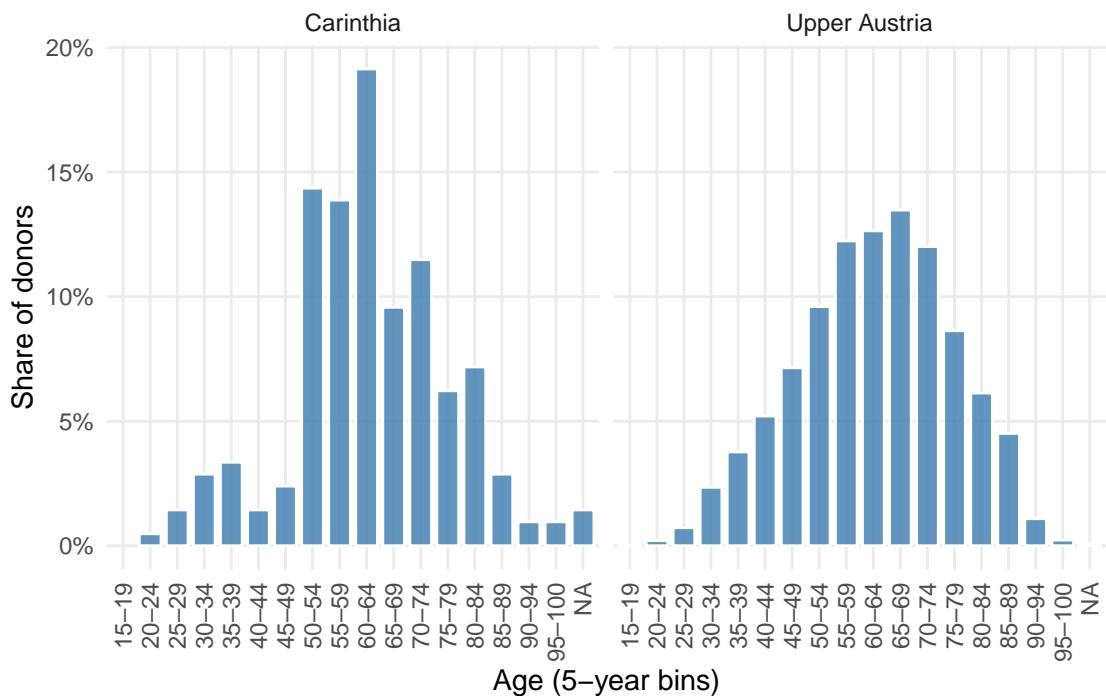


Figure 42: Age distribution of donors in Carinthia vs. Upper Austria.

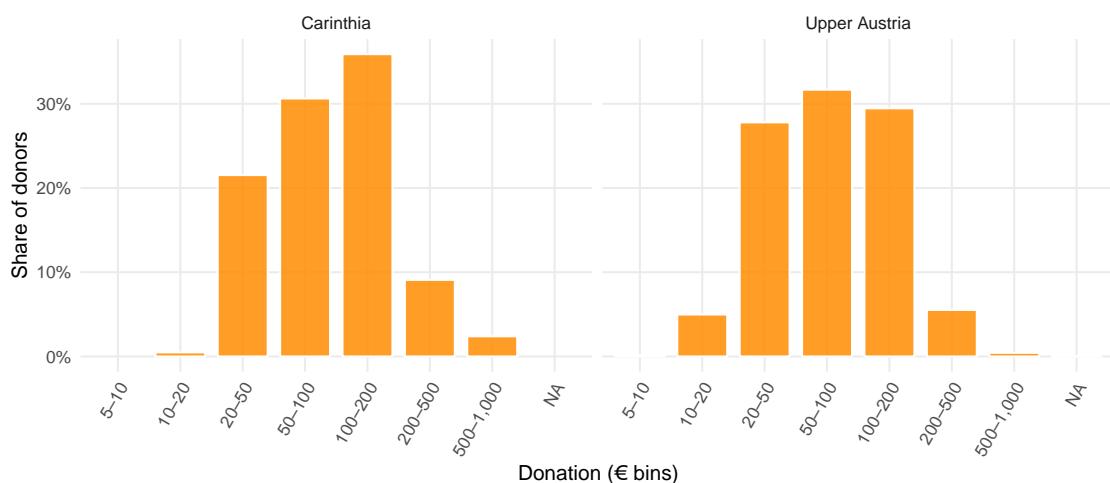


Figure 43: Donation amount distribution (in EUR bins) of donors in Carinthia vs. Upper Austria.

Radio-spot scripts. Original German and English translation of the *Information* and *Morality* treatments used in the 4 January 2025 public-radio campaign.

B.5.1 Treatment texts

Information treatment

Man hört ja Vieles - aber das hier ist wirklich wichtig!

Ab sofort gelten vereinfachte Regeln für das steuerliche Absetzen von Spenden.

Ob Onlinespende, Banküberweisung oder Haustürspende – die steuerliche Abschreibung funktioniert immer. Name und Geburtsdatum angeben, online oder an der Haustür, und die Spendenorganisation leitet die Daten für Ihre Steuererklärung ans Finanzamt weiter, und Sie erhalten einen Teil des Geldes zurück. Einfach so!

Weitere Infos auf [einfachabsetzen.at!](#)!

Eine entgeltliche Information der WU Wien.

YOU HEAR A LOT OF THINGS — BUT THIS IS REALLY IMPORTANT!

From now on, simplified rules apply for claiming a tax deduction on donations.

Whether you give online, by bank transfer, or at the door, the tax credit always works the same way. Just enter your name and date of birth online or tell the solicitor at your door; the charity forwards this information to the tax office for your return, and you get part of the money back. It's that simple!

More information at [einfachabsetzen.at!](#)

A public service announcement by Vienna Business University.

Morality treatment.

Man hört ja Vieles - aber das hier ist wirklich wichtig!

Jetzt können Sie gleich doppelt Gutes tun - Helfen Sie Menschen in Not – und setzen Sie Ihre Spenden steuerlich ab!

Ob Onlinespende, Banküberweisung oder Haustürspende – die steuerliche Abschreibung funktioniert immer. Name und Geburtsdatum angeben, online oder an der Haustür und die Spendenorganisation leitet die Daten für Ihre Steuererklärung ans Finanzamt weiter, und Sie erhalten einen Teil des Geldes zurück. Einfach so!

Helfen Sie Menschen, nutzen Sie die steuerlichen Möglichkeiten und informieren Sie auch Freunde & Bekannte über diese Möglichkeiten zu helfen.

Weitere Infos auf [einfachabsetzen.at!](#)

Eine entgeltliche Information der WU Wien.

YOU HEAR A LOT OF THINGS — BUT THIS IS REALLY IMPORTANT!

Now you can do twice as much good: **help people in need & claim a tax deduction for your donation.**

Whether you give online, by bank transfer, or at the door, the tax credit always works the same way. Just enter your name and date of birth online or tell the solicitor at your door; the charity forwards this information to the tax office for your return, and you get part of the money back. *It's that simple!*

Help people, make use of the tax benefits, and let friends and family know about this opportunity.

More information at **einfachabsetzen.at**!

A public service announcement by Vienna Business University.

B.5.2 Website screenshots

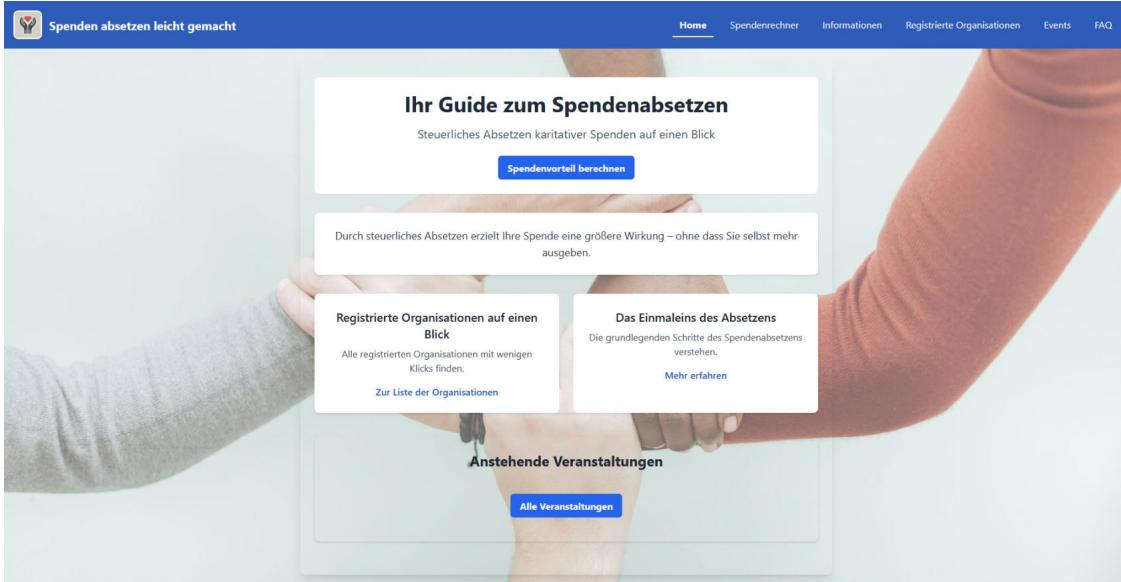


Figure 44: Landing page of the website.

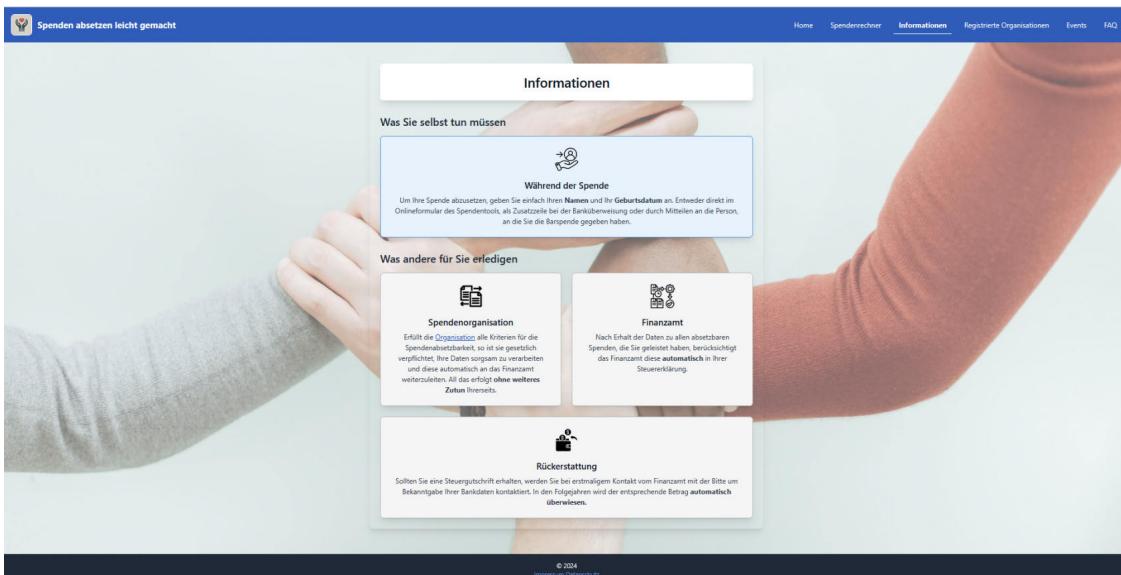


Figure 45: Procedural information page of the website.

Berechnen Sie Ihre Rückerstattung

Berechnen Sie, wie viel Sie durch steuerliches Absetzen **zurückbekommen** können.

Spenden lassen sich von der Steuer absetzen. Wählen Sie, ob Sie sich das Geld vom Finanzamt zurückstatten lassen oder Ihre Spende erhöhen möchten.

[Rückerstattung erhalten](#) [Wirkung vergrößern](#)

Beabsichtigte Spende (EUR):

Monatliches Bruttoeinkommen (EUR):

[Berechnen](#)

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Figure 46: Reimbursement-mode calculator page of the website.

Vergrößern Sie Ihre Wirkung

Ermitteln Sie, wie viel **mehr** Sie durch die steuerliche Absetzbarkeit **spenden können**.

Spenden lassen sich von der Steuer absetzen. Wählen Sie, ob Sie sich das Geld vom Finanzamt zurückstatten lassen oder Ihre Spende erhöhen möchten.

[Rückerstattung erhalten](#) [Wirkung vergrößern](#)

So viel möchte ich selbst ausgeben (EUR):

Monatliches Bruttoeinkommen (EUR):

[Berechnen](#)

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Figure 47: Additional-donation-mode calculator page of the website.

Name	Absetzbarkeit seit	Ende Gültigkeit	Link
0-9-16 ailes, außer gewöhnlich- Verein für individuelles Lernen 1.ASC 20-2016	14.09.2023	• Absetzbar	Besuchen
1. American Sports Club Vienna Knights, kurz Vienna Knights oder 1.ASC Vienna Knights 20-2016	02.10.2024	• Absetzbar	Besuchen
1. Tisch-Turnierclub GOLD WEISS Innsbruck 20-2016	13.09.2024	• Absetzbar	Besuchen
1000aue gemmütige GmbH 20-2016	01.01.2024	• Absetzbar	Besuchen
24education e.V. 20-1000	10.02.2022	• Absetzbar	Besuchen
4Life 20-1100	15.06.2021	• Absetzbar	Besuchen
A. & K. Kinderhaus der Barmherzigen Brüder Salzburg 20-1010	31.07.2017	• Absetzbar	Besuchen
Ad. Kinderhaus St. Vinzenz Betriebs GmbH 20-1020	13.01.2010	• Absetzbar	Besuchen
ABZ+AUSTRIA Verein zur Förderung von Arbeit, Bildung und Zukunft von Frauen 81-2205	01.01.2024	• Absetzbar	Besuchen
Academie Europäische Akademie der Wissenschaften und Künste, European Academy for Sciences and Arts or Académie Européenne des Sciences et des Arts vereinbart werden. Kurzbezeichnung: Academia Europaea, Europäische Akademie, European Academy oder Académie Européenne. Firmenname	18.11.1996	• Absetzbar	Besuchen

Seite 1 von 569

Figure 48: Charity registry page of the website.

Weihnachtsmarkt der Krebshilfe

• Absetzbar
2.12.2024 - Wien
Glühwein trinken und Gutes tun für die Krebshilfe Wien.

St. Anna Kinderkrebsforschung - Benefizsoirée

• Absetzbar
9.12.2024 - Wien
Klassische Musik genießen und einen Beitrag zur Kinderkrebsforschung leisten. [Link](#)

Weihnachtswichteln SOS Kinderdorf

• Absetzbar
12.12.2024 - Österreichweit
Kinderaugen mit einem kleinen Geschenk zum Leuchten bringen. [Link](#)

Vorige Seite 1 von 4 Nächste

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Figure 49: Event page of the website.

Häufige Fragen

Allgemeines

- Was ist der Zweck dieser Webseite?

Absetzbarkeit

- Sind alle karitativen Spenden in Österreich absetzbar?
- Ist die Art der Spende entscheidend?
- Muss ich meine Steuererklärung selbst machen?
- Bin ich aufgrund meiner abgesetzten Spende verpflichtet eine Steuererklärung zu machen?

Weitere Ressourcen

- Wo kann ich weitere Informationen finden?

Links

Figure 50: FAQ page of the website.

B.5.3 Radio-spot timing.

Federal state	Radio station	Treatment	Time slots (hh:mm)
Carinthia	Radio Kärnten	Information	07:12, 08:20, 12:05, 14:59
	Kronehit Kärnten	Information	06:49, 07:49, 08:49, 09:49
Upper Austria	Radio Oberösterreich	Morality	08:17, 10:17, 13:17, 15:17
	Kronehit Oberösterreich	Morality	06:49, 07:49, 08:49, 09:49

Table 26: Radio advertising slots on 4 January 2025, by federal state, station and treatment.
All times UTC+1.

B.6 Lab experiment

B.6.1 Treatment text

Base announcement (all participants)

Important Announcement

Your participant ID: *[displayed only at the University of Vienna; participants copied it to their IBAN slip]*

This is the final page you will see on this terminal.

To conclude the experiment, please proceed to the dedicated computer **when it is your turn**. At that computer you will make a final decision about whether to claim a deduction for your donation.

[If the session is at WU Vienna: “Further, you will input your IBAN credentials there.”]

Stay at your place until you are asked to go to the dedicated computer.

As a participant, you have the option to **deduct your donation** from the tax pool. Here's how it works:

Reimbursement Rate: You can recover 30 % of your donation as a reimbursement.

Tax Pool Contribution: The reimbursement is funded by contributions to the tax pool from all participants.

Voluntary Choice: Deciding to claim the deduction is entirely your choice and does not affect your donation amount.

Additional cue: Observability treatment

A proctor will be available at the dedicated computer to assist you with entering your information.

Additional cue: Moral-message treatment

Did you know? In a recent survey conducted in Austria (*Aman Hild & Hilweg-Waldeck, WP*), over **80 % of respondents** indicated that they consider it morally appropriate to tax-deduct charitable donations.

Additional Figures

The following plots provide descriptive splits of deduction rates across treatment arms. They complement the regression-based results in the appendix by illustrating the same null effects in a more granular fashion.

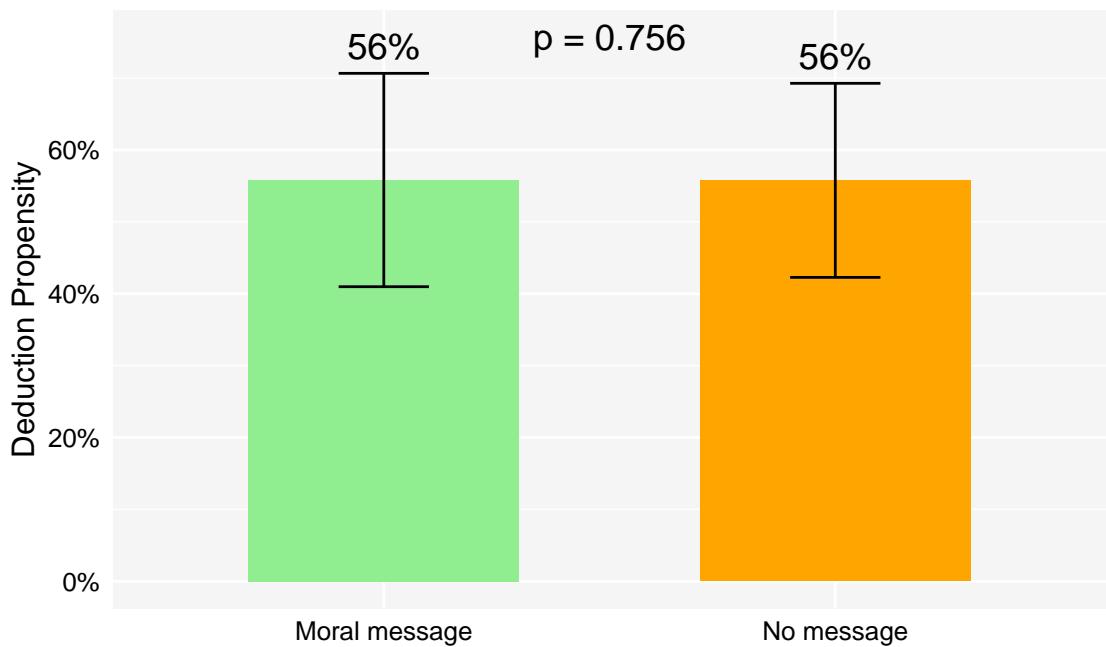


Figure 51: Deduction rate in the anonymity condition, by moral-message treatment.

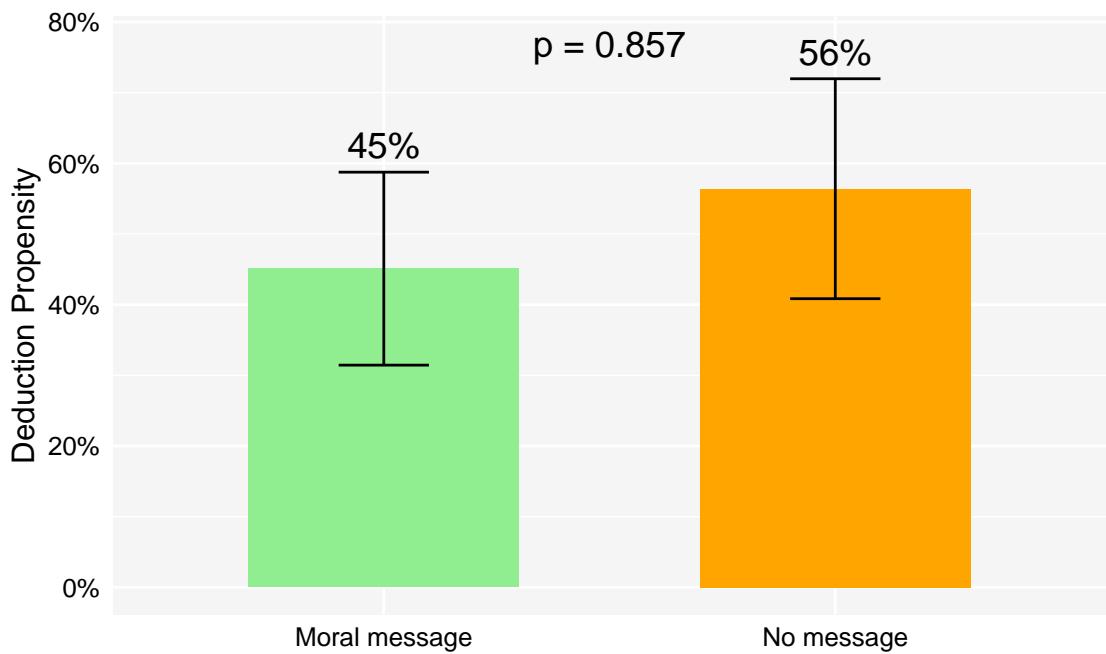


Figure 52: Deduction rate in the observability condition, by moral-message treatment.

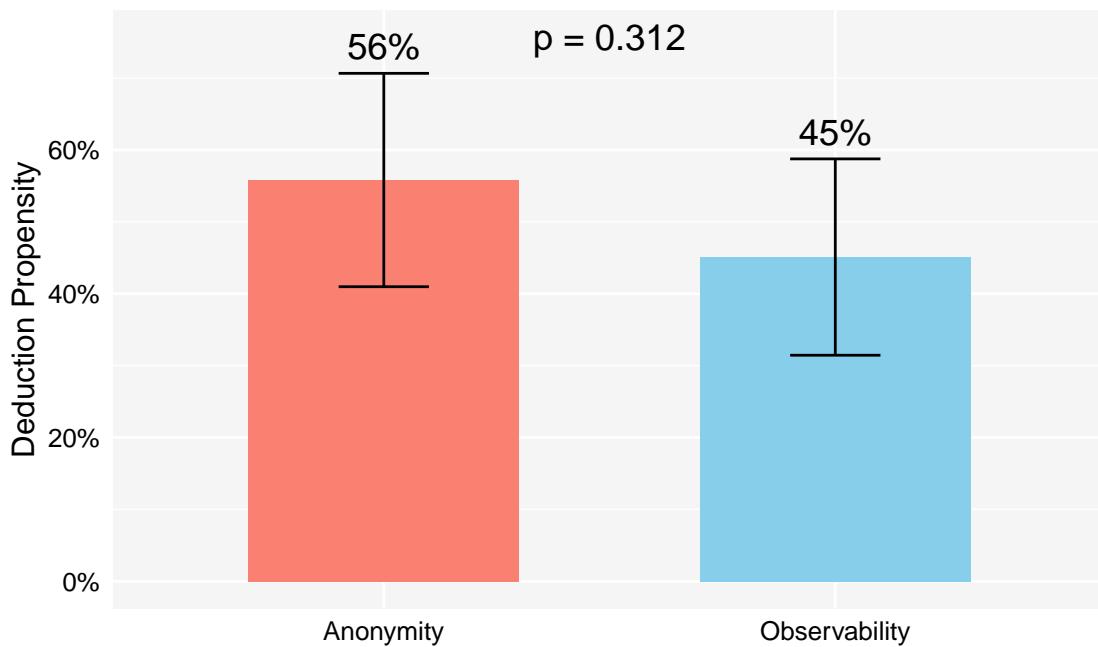


Figure 53: Deduction rate in the moral-message treatment, by observability condition.

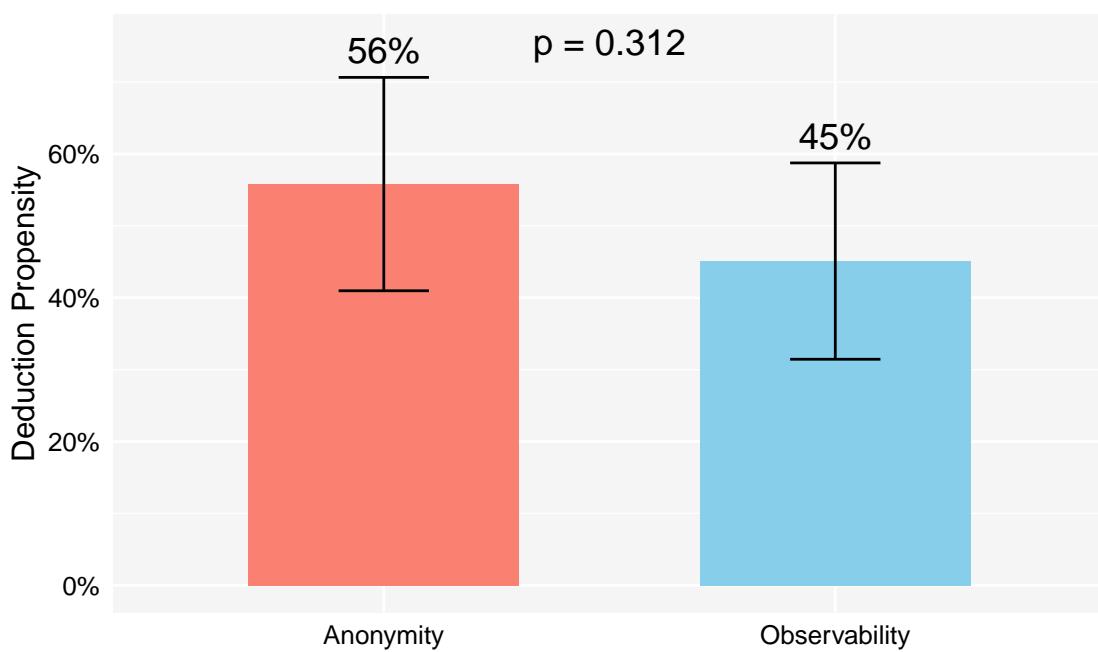


Figure 54: Deduction rate in the no-message condition, by observability condition.

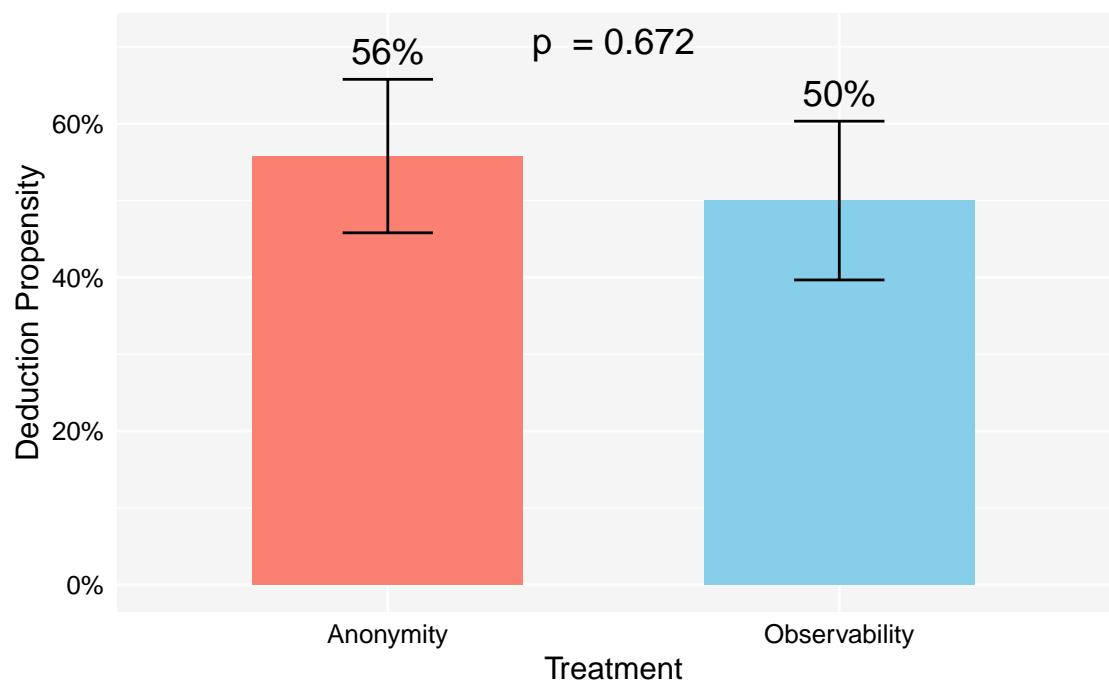


Figure 55: Deduction rate pooled across message treatments, by observability condition.