FONDAZIONE BRUNO KESSLER
INSTITUTE FOR THE EVALUATION OF PUBLIC POLICIES

FBK-IRVAPP Working Paper Series

Working Paper No. 2026-03

The effects of monetary compensation on paid volunteers: Quasi-experimental evidence from Germany

Annalisa Tassi
Lieke Voorintholt

July 2026

FBK-IRVAPP Working Paper No. 2026-03

The effects of monetary compensation on paid volunteers: Quasi-experimental evidence from Germany

Annalisa Tassi
Research Institute for the Evaluation of Public Policies of the Bruno Kessler Foundation (FBK- IRVAPP), Via S. Croce, 77, 38122, Trento, Italy.
atassi@irvapp.it

Lieke Voorintholt
Trier University, Institute for Labour Law and Industrial Relations in the European Union (IAAEU), Behringstr. 21, D-54296 Trier, Germany.
voorintholt@iaaeu.de

FONDAZIONE BRUNO KESSLER
INSTITUTE FOR THE EVALUATION OF PUBLIC POLICIES

Institute for the Evaluation of Public Policies
Fondazione Bruno Kessler
Via Santa Croce, 77
38122 Trento, Italy
https://irvapp.fbk.eu

The effects of monetary compensation on paid volunteers: Quasi-experimental evidence from Germany*

Annalisa Tassi, Lieke Voorintholt

Abstract

In recent decades, volunteering has expanded from completely unpaid work to the possibility of receiving a small (tax-free) monetary compensation for the activity performed, with the goal of stimulating these activities. Since the relationship between monetary compensation and prosocial behavior may differ from standard labor market settings, the effectiveness of monetary and tax incentives in this context is an empirical question. Using data from German income tax returns, this paper investigates the effects of changes in monetary compensations on the duration of voluntary work, donations, and market labor income of paid volunteers. Our empirical analysis leverages a German policy change from 2013 that increased the tax-free threshold for volunteer compensations. Following a difference-in-differences approach in combination with a duration model, we do not find evidence that affected volunteers change the number of years they spend in a certain volunteer position. Our results also show that the compensation increase has no spillover effects on labor earnings or donations. Combining our null findings and insights from our theoretical model, we suggest exploring non-incentive-based measures rather than higher compensations to stimulate paid volunteering.

JEL Classification: D11, D12, D64, H24, J22, J38
Keywords:
DOI:

* Acknowledgements: For useful comments and feedback, we thank Erich Battistin, Gerard van den Berg, Annette Bergemann, Thiess Buettner, Stefano Fiorin, Laszlo Goerke, Olga Gorodetskaya, Daniele Marchesi, Cornelius Schneider, Ryusuke Shinohara, Adriaan Soetevent and audiences of the Seminar in Applied Microeconomics (FAU), ZEW Public Finance Conference, Master and Doctoral Seminar (FAU) (all in 2023), the ESPAnet Low Countries Early Career Research Day, FEBRI PhD conference, Nederlandse Economendag, Global GLO-JOPE Conference, IIPF, and Bavarian MicroWorkshop (all in 2024), WiE Brownbag seminar and 18th Workshop on Social Economy (both in 2025), and Workshop on Labour Economics and SEHO (both in 2026). We are thankful to Hannes Fauser and Raoul Peter from Destatis for excellent support related to our data contract. The research for this article was partially carried out while Lieke Voorintholt was a PhD candidate at the University of Groningen. She is grateful for the support provided by the Graduate School of the Faculty of Economics and Business. Annalisa Tassi thanks the BGPE and the Friedrich-Alexander University of Erlangen-Nuremberg for financial support in accessing the data.

1 Introduction

Although volunteering is traditionally defined as completely unpaid work, it is not uncommon for volunteers to receive a small monetary compensation in the form of a volunteer allowance. For example, in 2014 around 9.6% of German volunteers received some monetary compensation (Hameister et al., 2023). This phenomenon is not specific to Germany, as studies about Italy (Cappellari and Turati, 2004; Fiorillo, 2011), Switzerland (Frey and Goette, 1999), the US (Mesch et al., 1998), and Kenya (Lewis, 2015), among others, testify. These allowances are claimed to be a channel to motivate people to volunteer (*Gesetz zur Stärkung des Ehrenamtes*, 2013). To accommodate such allowances, several countries have even introduced and adapted policies that exempt a predetermined (small) amount of volunteer compensation from tax and social security payments, benefiting both the recipient and the volunteer organization.[1]

From an economic perspective, these developments raise interesting questions, as it is ex ante unclear whether (increases in) monetary compensations to volunteers actually lead to increased volunteering. Standard neoclassical theory suggests that labor supply and wages are positively related. However, the behavioral economics literature has generated some evidence challenging the positive relationship between wages and labor supply (e.g., Camerer et al., 1997).[2] When (labor) supply takes place in the context of prosocial behavior, like in our setting, it becomes even more relevant to question the relationship with monetary compensation, as illustrated by theory (e.g., Frey and Oberholzer-Gee, 1997; Bénabou and Tirole, 2006), although the empirical findings are mixed (Chetty et al., 2014; Lacetera et al., 2014; Wollbrant et al., 2022; Fiorin, 2023). The recent volunteer compensation developments thus motivate investigating the effects of monetary compensation (the volunteer allowances) on the supply of paid voluntary work, both from a behavioral economic and an economic policy perspective.

This paper primarily aims to determine whether the amount of monetary payment affects the duration of voluntary work of existing volunteers. Volunteer duration (due to its direct link with retention) and recruitment jointly determine the size of the volunteer force. Of these two, investments in retention are the most efficient way of sustaining the volunteer force, due to the onboarding efforts that come with high turnover and recruitment rates. However, organizations seem to struggle with retention (McCurley and Lynch, 2007; Holtrop et al., 2024) and so-called "episodic volunteering" is on the rise (Compion et al., 2022), which highlights the relevance of studying factors that influence volunteer duration.

While our primary focus is on the effect of monetary allowances on volunteer duration,

[1] We refer to "volunteer organizations" or "organizations" for simplicity, but these can include various types of organizations and institutions, which are distinct also from a legal perspective. Such "organizations" include clubs and associations, public law entities (like schools and adult education centers), as well as churches and non-profit organizations.

[2] The findings of Camerer et al. (1997) mainly relate to reduced work hours of taxi drivers in situations with high demand (higher wages), though further evidence suggests that this supply effect is driven more by time spent working than by income earned (Farber, 2015).

an increase in the compensation may have broader implications. Volunteering in fact combines elements of both working and giving, motivating why we examine whether the allowance spills over into two close substitute margins: market labor supply and charitable donations. As a first step to guide our understanding of the effects of monetary payments to volunteers, we develop a theoretical model that relates the payment amount to voluntary work, donations, and market labor. Our empirical evaluation is then based on the specific subset of paid volunteers (as opposed to non-compensated volunteers) in Germany.

In Germany, about a third of the population volunteers at least occasionally (Tassi, 2024). Each organization can freely decide whether to pay its volunteers and, if so, how much to pay them. A likely maximum amount is the tax-free threshold set by the German government, for which we empirically observe a large discontinuity. Monetary compensations below this threshold are exempt from taxation and social security payments, so organizations face disproportionately higher costs if they cross the threshold. We therefore define paid volunteers as those individuals who receive a monetary compensation that does not exceed the exemption threshold for an activity that is understood as volunteering according to tax law. In Germany, this threshold is set at €2100 per year before 2013 and €2400 per year since 2013. We explain additional conditions in Section 3.

To determine whether a higher payment binds volunteers more to their organizations, we exploit the exogenous increase in this tax-free threshold by the German government in 2013. To this end, we investigate whether volunteer duration is affected differently in the post-policy period for the paid volunteers (as defined above) who are impacted by the increase because prior to the reform they receive the maximum exempted allowance, relative to the paid volunteers who are less likely to be impacted because they receive a lower allowance and are not constrained by the pre-reform threshold.

Using administrative data from a random 5% subset of the universe of German income tax returns (the so-called Taxpayer Panel, TPP), we set up a duration model and estimate the relevant difference-in-differences (DiD) coefficient. We use a similar DiD strategy in combination with a linear model to estimate the effects of increased volunteer payments on market labor income and donations. Moreover, we make use of the German Survey on Volunteering (*Freiwilligensurvey*, FWS) to explore both the potential mechanisms at work and gain descriptive insights into paid volunteering. In particular, our FWS analyses support our notion that the compensation increase is exogenous to factors such as effort (volunteer hours) and organizational quality (evaluated by the volunteer).

Based on our DiD duration model, for which we use volunteers' spell length and spell ending information, we find no significant effect of additional payment on volunteer duration. This is unlike a typical labor market response, based on which we would expect that additional payment stimulates paid volunteering. We theoretically motivate that the absence of such a positive effect suggests that, like unpaid volunteers, paid volunteers are likely intrinsically motivated and/or value their reputation related to volunteering. These findings align with evidence that certain prosocial behaviors are vulnerable to

motivational crowding out (e.g., Mellström and Johannesson, 2008; Wollbrant et al., 2022).

Our results also show no evidence that higher volunteer compensations affect market labor income or monetary donations. For labor income, this suggests that increased volunteer allowance is unrelated to participation intensity in the labor market, likely reflecting that the increase in the allowance is small relative to labor income. Additionally, the null effect on donations indicates that greater monetary compensation for volunteering does not circulate back to charitable organizations. Additional analyses in which we use a propensity score matching strategy yield the same conclusions for these secondary outcomes.

Our analysis addresses a gap in the behavioral economics literature by focusing on the role of monetary incentives in volunteering. Correlational studies on the relationship between hours volunteered and the amount of the allowance present some suggestive evidence that monetary compensation reduces volunteer work supply in the presence of intrinsic motivation (Frey and Goette, 1999; Cappellari and Turati, 2004; Fiorillo, 2011). Applying propensity score matching between paid and unpaid volunteers at German sports clubs, Dallmeyer et al. (2024) conclude that payment appears to positively affect hours worked, volunteer retention and recruitment. Identification of causal effects is currently limited to a field experiment related to hiring volunteer helpers during a conference (Conrads et al., 2016). However, its findings apply to one-time volunteering as compared to long-term engagement and volunteer retention. A more recent lab experiment designed to reproduce the physical discomfort experienced by volunteers also considers its interplay with monetary compensation and potential charitable benefits (Steward et al., 2026). In general, our study is the first to apply a duration model to volunteer spells, which improves the general understanding of volunteer retention. Moreover, our study uses a causal identification approach, a novel dataset, and a wider selection of outcomes to assess the effect of payment changes on paid volunteers over time.

Our investigation of the outcome volunteer duration relates most closely to the literature on unpaid volunteer exits. This literature analyzes such exits mainly from an organizational or behavioral perspective and has not yet touched on the phenomenon of paid volunteering. A recent meta-analysis by Forner et al. (2024) shows that many different factors, such as volunteer-role characteristics, organizational characteristics, and motivations and attitudes predict volunteer turnover almost equally well. Merrilees et al. (2020) split the volunteer life-cycle into four phases depending on current spell length, which is in line with our methodological use of a duration model that assesses exit probabilities conditional on the concurrent spell duration.

Our research bears important practical insights regarding the use of expanded monetary incentives to attract and retain volunteers. Our findings suggest that raising compensation alone is unlikely to sustain engagement over time, and that volunteer organizations may want to balance modest monetary allowances with strategies that foster intrinsic motivation and reputational benefits (Conrads et al., 2016; Forner et al.,

2024). Furthermore, our evaluation directly addresses the effectiveness of current tax policy structures that exempt volunteer compensations. This is particularly informative, as such tax policy is not only applied in Germany, but also in countries with similar institutional and cultural contexts, like the Netherlands (*Wet op de loonbelasting 1964*, 2025).[3] Finally, by analyzing not only the effect of increasing the tax-free threshold on the duration of volunteer spells, but also potential spillovers on labor supply and donations, our study provides a broader perspective on the overall limited impact of tax incentives in the nonprofit sector.

2 Theoretical framework

2.1 Related theoretical work and novelties

Existing theoretical models that study the relationship between monetary compensation and the supply of voluntary work focus on the contrasting forces of intrinsic motivation, i.e., the decision to engage in some activity for its own sake, and extrinsic motivation, i.e., the decision to engage in some activity for other incentives or benefits (Frey and Goette, 1999; Cappellari and Turati, 2004; Fiorillo, 2011). Fiorillo (2011), for example, shows that the relationship between optimal volunteering and monetary compensation is positive for extrinsically motivated individuals, but could turn negative for intrinsically motivated individuals if the compensation negatively affects their intrinsic motivation.

Carpenter and Myers (2010) set up a model of paid volunteering that includes the reputation for prosociality, based on Bénabou and Tirole (2006). Their comparative static shows an ambiguous net effect of material incentives on volunteer supply by reputation-driven agents. We similarly consider reputational concerns as a channel through which extrinsic incentives might alter volunteering, though we focus on private image instead of public image.

Different from these models that isolate utility from volunteer work and focus on its costs and benefits, our theoretical model considers a broader utility-maximizing framework that next to volunteering utility also includes utility from consumption, donating, and leisure. By considering potential spillovers from volunteering to donating, we build on an extensive literature analyzing the relationship between volunteer work and donations (e.g., Feldman, 2010; Yang, 2016; Brown et al., 2019; Voorintholt, 2023), which suggest that individuals generally consider donating and volunteering as substitutes, since both can be used to derive utility from being charitable. By modeling a monetary budget constraint that is affected by changes in the volunteer allowance, we reflect that the allowance can affect donations through the channel of income dynamics (Auten et al., 2002).

[3] In 2006 the policy has been incorporated in the cited Wage Tax Act and has been updated regularly since then. The initial exempted amount was €1500 a year (https://wetten.overheid.nl/BWBR0002471/2006-01-01/3), while the yearly tax-free volunteer allowance is set at €2200 in 2026 (*Wet op de loonbelasting 1964*, 2025).

In our model, paid volunteering is also connected to a time budget constraint. Various papers indicate that the transition into retirement or out of paid work increases volunteering (e.g., Eibich et al., 2022; Niebuur et al., 2022). We might, therefore, also expect some effects on market labor following shifts in paid volunteering.[4] An additional link to market labor comes from research suggesting that individuals request lower wages to work at firms with a higher social responsibility reputation (Frank, 1996; Nyborg and Zhang, 2013). Tonin (2020) discusses that individuals are willing to accept a lower wage rate to work for a non-profit organization than a higher wage in the for-profit sector. Brown et al. (2019) show empirically that this holds even if individuals could donate the difference in income to charity.

2.2 Theoretical model

We present a theoretical model to guide our hypotheses on the effect of changes in volunteer allowance on volunteering, market labor earnings, and donations. We collapse the utilities from donating and volunteering to those fitting a private consumption model.[5] As in Lilley and Slonim (2014) and Feldman (2010), individuals maximize an additively-separable utility function. They derive utility from private consumption x, discounted volunteering r (as explained below in more detail), donations d, and leisure time l. The utility function has the following form:

$$\begin{aligned} U(x,r,d,l) =& U_x\left[E + w_m m + w_v v - d\right] + U_r\left[\left(1-\rho w_v\right)v\right] + U_d\left[d\right] \\ &+ U_l[T - m - \kappa v], \end{aligned} \tag{1}$$

where U_x, U_r, U_d, and U_l are the subutilities from private consumption, volunteering, donating, and leisure time, respectively.[6] E is the unearned income, m is market labor supply, w_m is the market labor wage, v is volunteering, w_v is the volunteer allowance, and d is the amount of donations. w_m and w_v can be interpreted as yearly compensations ($w_v, w_m > 0$), such that m and v can be interpreted as fractions (between 0 and 1) of the maximum time that can be spent in a position in each of their domains. In fact, volunteering must be a sideline activity for the purposes of the allowance, implying fewer maximum hours than in the case of market labor. Therefore, κ (between 0 and 1) reflects

[4] In contrast, Feldman (2010) and Voorintholt (2023) assume that individuals choose their labor supply independently of their preferences for donating and volunteering and therefore make their charitable giving decisions based on exogenous values of labor hours and income. This simplification is plausible in a setting that considers unpaid volunteering only, but is not suitable for our context of paid volunteering.

[5] In principle, we could also approach the optimization problem dynamically. For instance, on-the-job search models (see Mortensen, 1986, for an overview) could capture transitions from volunteering into paid work. In this paper, however, we abstract from such dynamic models and instead focus on allowances as a source of immediate consumption utility, which motivates our static framework. By contrast, models of retirement or savings decisions are less suitable, since volunteer allowances are typically too small to be relevant for long-term investment.

[6] We assume standard conditions on their first and second derivatives: $U' > 0, U'' < 0, \lim_{x\to 0} U' = \infty$ and $\lim_{x\to\infty} U' = 0$.

the legally maximum time that can be spent on a volunteer position as a share of the time associated with a comparable full-time market labor position.[7] It corrects the relative time differences between m and v relevant for the leisure sub-utility function. T is a transformed measure of an individual's total available time to be allocated between work, volunteering, and leisure. One unit of T corresponds to the yearly time taken up by a full-time paid job in the labor market: T = (yearly hours available)/(yearly hours of a full-time paid job).

Finally, the volunteering discount rate ρ, reflects that the utility from volunteering can be discounted in the presence of extrinsic incentives (the amount of volunteer allowance), for instance, through the crowding out of intrinsic motivation or through negative reputational concerns. This follows the model of Bénabou and Tirole (2006) in which prosocial behavior is derived from three motivations: intrinsic, extrinsic, and reputational. They define the latter as individuals' judgment of their own moral decisions through the lens of an impartial spectator. In our setting this translates to a tension that paid volunteers could experience between the (altruistic) motivations to volunteer and the awareness of not performing a gratuitous act. In our model, monetary extrinsic motivation is captured by U_x, and intrinsic and reputational motivation are embedded in U_r, but not separately identifiable.[8] This matches our empirical application and allows us to accommodate the variety of mechanisms proposed in the literature (Frey and Oberholzer-Gee, 1997; Bénabou and Tirole, 2006; Carpenter and Myers, 2010). We assume that $(1 - \rho w_v) \in [0, 1]$, so that ρ has values in the interval $[0, \frac{1}{w_v}]$ to ensure positive utility arguments.

We solve our model to identify the marginal effect from a change in w_v on the optimal levels of volunteering ($\frac{dv^*}{dw_v}$), labor ($\frac{dm^*}{dw_v}$), and donations ($\frac{dd^*}{dw_v}$). In the evaluation of our model we consider the case $w_v < \kappa w_m$, which implies that the hourly wage for market labor is strictly greater than the hourly wage for the volunteering activity.[9] Since empirically we focus on currently paid volunteers who have the potential for an exogenous change in their allowance, we theoretically consider the case corresponding to the entire range of paid volunteering positions, $0 < v^* \leq 1$. Combining these cases, we find that the sign of each comparative static depends on the volunteering discount rate ρ.

> **Definition 1.** *$\rho_{critical}$ is the value of the volunteering discount rate ρ that marks the threshold between a positive or a negative effect of the volunteer allowance amount on each of the optimal levels of volunteering, labor, and donations. This yields the relevant cases $\rho_{low} < \rho_{critical}$ and $\rho_{high} > \rho_{critical}$. In case $\rho = \rho_{critical}$, the optimal levels of volunteering, market labor supply, and donations are unaffected by changes in the volunteer allowance.*

[7] In Germany, this maximum equals one-third (Federal Ministry of Finance, 2025).

[8] Indeed, some aspects of non-monetary extrinsic motivation, like benefits from networking, might still be captured in U_r.

[9] An examination of the survey data sustains this assumption: Figure D1 shows that the distribution of labor-market wage lies to the right of the hourly allowance distribution.

While we derive $\rho_{critical}$ separately for each comparative static in Appendix A and the resulting expressions involve multiple unknown parameters, our simulations indicate a high degree of convergence among these values.[10] This outcome is intuitive, but not a trivial artifact of the model structure: ρ enters only the utility from volunteering directly, and its relationship to labor and donations is only indirect through the budget and time constraints. Since volunteering responds most directly and in relationship to ρ, the thresholds at which the effects on labor supply and donations change tend to align.

We summarize the comparative statics with respect to the volunteer allowance in two propositions. The first captures the primary behavioral response in volunteering, while the second describes the spillover effects on labor supply and donations.

Proposition 1. *There is a positive (negative) effect of the volunteer compensation on volunteering only when $\rho = \rho_{low}$ ($\rho = \rho_{high}$).*
Proof. See Appendix A.2

The effect of the volunteer allowance on volunteering is direct and central to our analysis. When individuals do not feel a strong tension between the volunteer allowance and their intrinsic or reputational motivation ($\rho = \rho_{low}$), the additional financial incentive from the allowance increases their optimal volunteering level. In contrast, when $\rho = \rho_{high}$, individuals heavily discount the utility they derive from volunteering under monetary compensation. In this case, an allowance increase either crowds out intrinsic motivation or increases self-image concerns. As a result, volunteering decreases.

Proposition 2. *The effect of the volunteer compensation on market labour goes in the opposite direction than the effect on volunteering. The effect on donations is also opposite in the case $\rho = \rho_{high}$, while it is ambiguous if $\rho = \rho_{low}$.*
Proof. See Appendix A.2.

Changes in volunteering behavior also affect how individuals reallocate their time and money across market labor and charitable donations. Given ρ_{low}, when the volunteer allowance increases and volunteering becomes more attractive, individuals tend to reduce market labor supply, and possibly also reduce donations, depending on their preferences and budget constraints. Conversely, when volunteering decreases (under ρ_{high}), the freed-up time is partially reallocated, leading to an increase in market labor supply. Since time spent on market labor is more highly financially rewarded than time spent on volunteering, the consequential increase in income allows for increased donations. These spillover effects arise endogenously in the model and underscore the interdependence of paid volunteering with other prosocial or income-generating activities.

Table 1 below summarizes the direction of these effects for different value ranges of the volunteering discount rate ρ, consistent with Propositions 1 and 2. Our model represents a

[10] In these simulations we impose seemingly realistic values for the parameters in $\rho_{critical}$ that satisfy our model conditions. The corresponding file is available from the authors upon request.

general framework that allows us to study how different degrees of reputational concerns and crowding out of intrinsic motivation affect the interplay between changes in the volunteer allowance and changes in optimal volunteering, donations, and labor market outcomes. With the help of the next sections on the institutional background and on the empirical methods, we explain in what ways our empirical application relates to this general framework.

Table 1: Signs of the comparative statics depending on the volunteering discount rate

	$\frac{\mathrm{d}v^*}{\mathrm{d}w_v}$	$\frac{\mathrm{d}m^*}{\mathrm{d}w_v}$	$\frac{\mathrm{d}d^*}{\mathrm{d}w_v}$
$\rho = \rho_{low}$	> 0	< 0	?
$\rho = \rho_{high}$	< 0	> 0	> 0

3 Institutional background

In Germany, some forms of voluntary activities can be compensated with a small monetary allowance, which is tax-free up to a certain threshold (see *Einkommensteuergesetz*, 2022, Section 3). There are two types of allowance: the general volunteer allowance (*Ehrenamtspauschale*), introduced in 2007, and the instructor allowance (*Übungsleiterpauschale*), in place since 1989. The general allowance applies to charitable, benevolent, or ecclesiastical activities (e.g., serving as treasurer), while the instructor allowance covers voluntary activities with an educational or training purpose (e.g., sports coaching). Initially capped at €500 (general) and €2100 (instructor), the thresholds were increased in 2013 to €720 and €2400 respectively, as part of the "Act to Strengthen Volunteering" (see Figure 1 for a timeline of these reforms).[11]

The 2013 reform was explicitly framed as an incentive to engage in volunteering. Other measures in the Act, such as easing administrative requirements for the organizations, were relatively narrow in scope and are more likely to affect general volunteers in administrative roles than instructors. For this reason, our main analysis focuses on instructor volunteers, while results for the general volunteer sample are reported in Tables C22 and C23 in Appendix C.4 and still confirm the general patterns observed in the main analyses.

It is important to note that these allowances are not meant to be substitutes for low-wage employment. By law, volunteering under these schemes is a sideline activity, capped at one-third of a full-time labor-market equivalent ($\kappa = \frac{1}{3}$ in our model). Even at the maximum annual instructor allowance, the implied maximum hourly rate is far below the statutory minimum wage introduced in 2015 (€3.75 vs. €8.50).[12]

[11]The respective increases (44% for the general volunteer allowance and 14% for the instructor allowance) also exceed German inflation adjustment rates during this period.

[12]The hourly volunteer allowance is calculated based on the maximum tax-free yearly allowance in

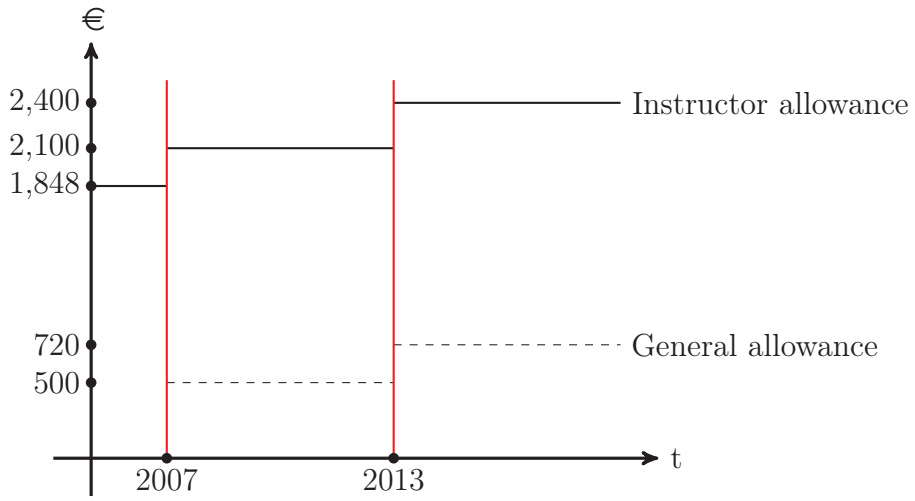


Figure 1: Tax-free allowances in Germany

Notes: The vertical lines in red highlight reform years.

Additionally, the volunteer allowance stands in contrast to so-called Minijobs, which, although also tax-favored, are subject to employer social security contributions and typically involve retail or service work rather than volunteer activities. Additional institutional details, including reporting rules, over-threshold cases, and reimbursement provisions, are presented in Appendix B.

4 Data and descriptives

4.1 Datasets

For our main analyses, we use longitudinal administrative data on the universe of income tax returns in Germany (RDC of the Federal Statistical Office and Statistical Offices of the Federal States, 2022). The so-called Taxpayer Panel (TPP) provides yearly information on individual and couples' gross income and tax deductions, including donations.[13] Importantly, since 2007, the data also includes information on the (tax-free) volunteer allowances and on any amounts exceeding the tax-free threshold.[14]

The TPP (2001–2018, 5% random sample) includes 62,536,172 observations, but we restrict our sample as follows. First, we only consider the years since 2007 as there is no prior information about the volunteer allowances. Second, following the data-preparation instructions, we remove observations only reporting loss assessments and savings allowance cases. We further do not consider taxpayers with residence

2015 (€2400) divided by the maximum number of hours volunteered in a year (640) given the "κ" constraint mentioned above.

[13]The full material excludes individuals who would appear in the panel only once between 2001 and 2018.

[14]Even though the instructor allowance existed before 2007 and could be declared in the tax return, this variable was not included in the dataset.

abroad or cases where only the employer reports payroll tax, as these observations do not contain any information related to the volunteer allowance.[15]

The TPP observations are at the level of the reporting unit, which could be an individual or a couple. Since our analysis focuses on individual supply of voluntary work, we transform the panel from the household to the individual level. The dataset includes information about the allowance amount, which we use to infer volunteer duration, while no information is available on the hours volunteered in this dataset. We use the third-party reported (pre-tax) labor income and the overall deducted donations to proxy labor earnings and donations, respectively. These and the remaining variables of interest are further detailed in Appendix C.1.

Another source of information is represented by the German Survey on Volunteering (*Freiwilligensurvey*, FWS) (Hameister et al., 2023). This dataset is based on phone interviews with a representative sample of the German-speaking population. The data has been collected every five years, most recently in 2009, 2014, and 2019. It represents one of the richest sources of information on volunteering in Germany, as it includes data on the areas of engagement, their intensity, the organizational setting, etc. Moreover, it includes information on both paid and unpaid volunteers. Note that the two datasets cannot be merged.

We use information from the FWS to characterize the differences between compensated and uncompensated volunteers, to gain some insights about the organizations where the volunteers work, and to look into the reasons for quitting volunteering.

4.2 Descriptives

Using TPP data, Figure 2 shows the volunteer allowance distribution for the years 2012 (red) and 2013 (green). The two distributions tend to overlap except for clear spikes at the tax-free thresholds that change around the reform. It is visible that 500 and 2100 are frequent values in 2012, while values between 700 and 800 (likely 720) and 2400 are more frequent in 2013. The particularly high occurrence of values corresponding to the instructor allowance further motivates our choice to focus on this type of volunteer allowance, as compared with the general allowance, for the remainder of the paper.[16] For the full population of tax filers, approximately 15500 volunteers report receiving a €2100

[15]Note that taxpayers earning only income from employment are not required to file a tax return, since the payroll tax is withheld by the employer. These cases correspond to around a third of the taxpayers (RDC of the Federal Statistical Office and Statistical Offices of the Federal States, 2021). However, individuals receiving social security benefits (e.g., unemployment, parental leave) are required to file a tax return (*Einkommensteuergesetz*, 2022, paragraph 46).

[16]Appendix Figures C2 and C3 show the frequency of getting the respective maximum allowance over time, for instructor and general volunteers. After the policy change, it is noticeable that the €2100 allowance immediately becomes less relevant for instructor volunteers, while the €2400 allowance becomes predominant. For general volunteers, it is also the case that the higher allowance becomes more popular, but the €500 value remains the default for many volunteers.

allowance in 2012 and around 12000 volunteers report receiving a €2400 allowance in 2013 (RDC of the Federal Statistical Office and Statistical Offices of the Federal States, 2022).

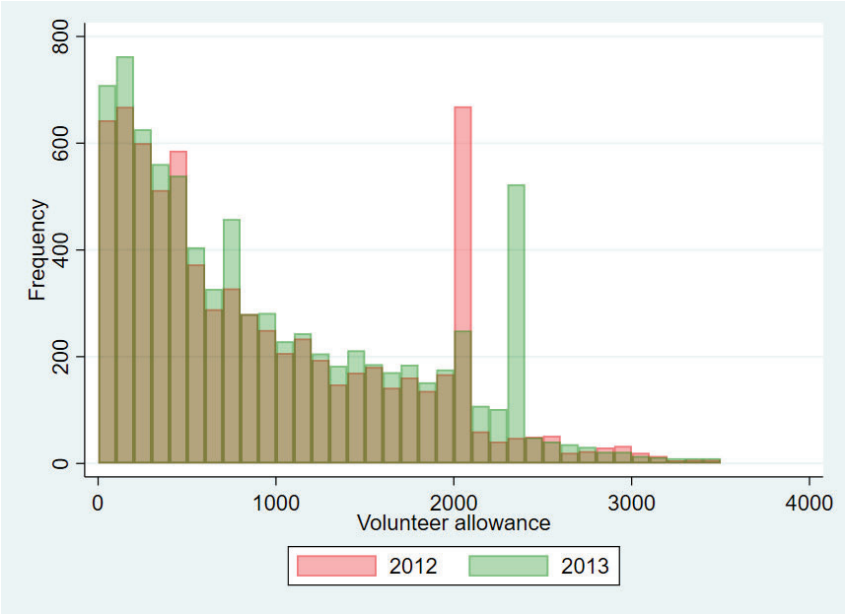


Figure 2: Distribution of the volunteer allowance in € for 2012 and 2013

Source: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, DOI: 10.21242/73111.2018.00.01.2.1.1, own calculations.
Notes: Based on the TPP 5% sample, meaning that multiplying the frequency on the y-axis by 20 gives an estimate of the population frequency.

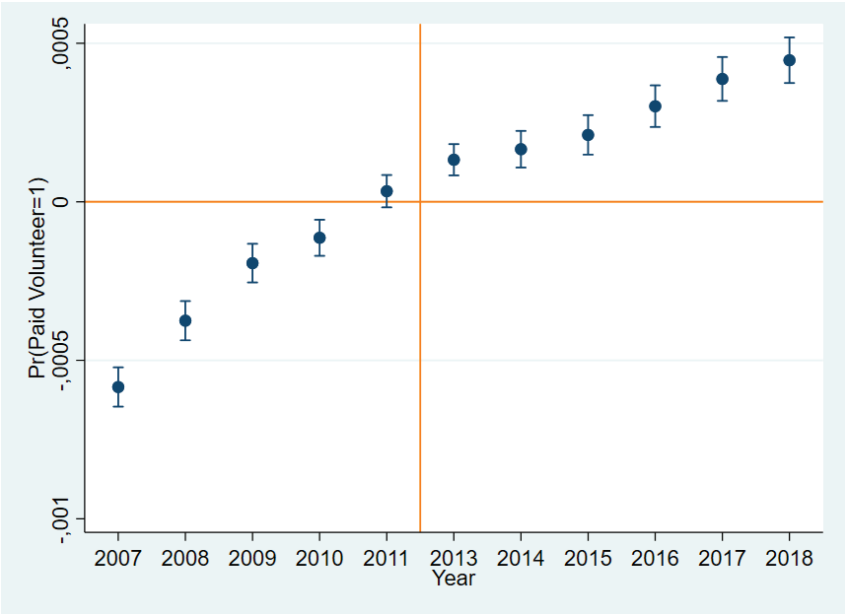


Figure 3: Time trend in probability of being a paid (€1000-3200) volunteer

Source: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, DOI: 10.21242/73111.2018.00.01.2.1.1, own calculations.
Notes: Y-axis measures the change in the probability of volunteering compared to the baseline year 2012. Based on the TPP 5% sample.

Figure 3 shows how the participation rate of paid volunteering in the range of €1000-3200 has progressed over the years 2007 to 2018. The lower bound of this interval clearly excludes general volunteers, whereas the upper bound marks the possibility of receiving both the general and instructor volunteer allowance.[17] Overall, the probability of being a €1000-3200 paid volunteer follows a near-linear yearly increase. The trend before 2013 is slightly more variable than the pattern starting at 2013, which motivates the quantification of a potential deviation from this pre-trend around the year 2013 in Appendix C.3. Table C1 shows that the year effects relative to 2012 are very small but mostly positive after 2012 and mostly negative before. The probability of volunteering is on average higher after 2012 but faces slightly lower increases than before. When looking at entry instead of participation patterns, i.e., focusing only on first-time entry, we also do not notice remarkable disruptions in patterns around the year 2013, see Appendix Figure C4.

From Table 2 we can gain some insights about the observable characteristics of paid volunteers in 2012. We separately show the characteristics of two groups of volunteers, tightly following our identification strategy, that is, the group of volunteers receiving €1000-2000, and those receiving €2100 (see Section 5). The continuous variables reported in Table 3 are winsorized. In our analyses, we work with the original data except for our outcome variables Labor income and Donations, for which we keep using the winsorized values to limit the influence of the 3% top and bottom outliers.[18] This is common practice, especially for donations, (e.g., Meer and Priday, 2021; An et al., 2023), but it is also implemented for labor market outcomes such as for wages and earnings (Angrist and Krueger, 1999; Lachowska et al., 2020; Davis and Krolikowski, 2025).

We observe that the different groups of paid volunteers are very similar with respect to many characteristics: the average age is 49, around 75% are married, 43% do not have children, and 8-9% receive some social security benefits. However, the two groups also display some differences. For example, lower-compensated volunteers are somewhat more likely to have adult versus underage children than higher-compensated volunteers. The latter are in employment or self-employment more often than the former. Nevertheless, lower-compensated volunteers earn more in the labor market (on average over €7500 more per year), but donate at least €240 less on average than the higher-compensated.

We also report the descriptive statistics of several groups observed in the FWS survey data in Table D1. The "paid volunteer" category here also includes many volunteer types (e.g., receiving less than €1000/year) that are not represented in Table 2. Though some characteristics are similar (female, household information), others sketch a quite different image (religion, employment rate, years in volunteer activity). From the FWS we also

[17] Above €3200, we would expect additional allowance to be marked as taxable rather than tax-free volunteer allowance.

[18] We also present results using unwinsorized outcome variables. The 3% cutoff serves as a middle ground between the more commonly used 1% and 5% thresholds in the literature. This choice (i) makes sure that outliers are actually removed in our relatively small sample, while (ii) avoiding the more extensive trimming from winsorizing at the 5th and 95th percentiles.

learn that the most popular form of volunteering is at a club or association (see Table D3), where we indeed expect most of instructor volunteers to be active.

Table 2: Descriptive statistics paid volunteers, 2012

	€1000-2000 comp.		€2100 comp.		Δ	
	Mean	(SD)	Mean	(SD)	b	(SE)
Female	0.42	(0.49)	0.46	(0.50)	-0.04	(0.02)
Age	49.05	(10.79)	48.72	(10.16)	0.33	(0.53)
Married	0.76	(0.43)	0.75	(0.43)	0.01	(0.02)
East Germany	0.36	(0.48)	0.33	(0.47)	0.03	(0.02)
No kids	0.43	(0.50)	0.43	(0.49)	0.01	(0.02)
Kids (<18 y.o.)	0.39	(0.49)	0.43	(0.50)	-0.04	(0.02)
Kids ($\geq$ 18 y.o.)	0.18	(0.38)	0.14	(0.35)	0.04*	(0.02)
Protestant	0.35	(0.48)	0.37	(0.48)	-0.02	(0.02)
Catholic	0.31	(0.46)	0.34	(0.47)	-0.02	(0.02)
Other/not religious	0.34	(0.47)	0.29	(0.46)	0.04	(0.02)
Spell length	1.22	(0.72)	2.02	(1.31)	-0.80***	(0.04)
Labor income	57156	(69667)	49637	(64908)	7518*	(3385)
Donations	596.23	(1042.95)	838.26	(1311.97)	-242.03***	(54.81)
Employed	0.82	(0.39)	0.88	(0.33)	-0.06**	(0.02)
Entrepreneur	0.32	(0.47)	0.40	(0.49)	-0.08***	(0.02)
Landlord	0.18	(0.39)	0.16	(0.37)	0.02	(0.02)
Retired	0.09	(0.28)	0.09	(0.28)	0.00	(0.01)
Receives benefits	0.08	(0.28)	0.09	(0.28)	-0.00	(0.01)
Commuting effort	3967	(6120)	3723	(5762)	244	(298)
Observations	1780		534		2314	

Source: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, DOI: 10.21242/73111.2018.00.01.2.1.1, own calculations.

Notes: Variable derivations are thoroughly explained in Section C.1 in Appendix C. For this table and the rest of the paper, output is based on the TPP 5% sample and the variables Labor income and Donations are winsorized at the 3% level. Just for the statistics reported in this table but not for their further role as controls and in robustness checks, the continuous variables Age, Spell length, and Commuting effort are also winsorized on the 3% level. Commuting effort represents the (return) distance traveled to work per year in km.

5 Empirical methods

We estimate the impact of the increase in volunteer compensation due to the 2013 policy change on the selection of outcomes that are modeled in Section 2: volunteer participation, market labor supply, and donations. While the theoretical model may reflect any exogenous changes in the volunteer allowance w_v, our empirical models focus

only on the exogenous increase in the allowance in 2013 for instructor volunteers previously receiving the maximum tax-free allowance.

Our main indicator for volunteer participation is volunteer duration, allowing us to specifically focus on how a (potential) compensation increase affects the volunteering status of existing paid volunteers by combining our identification strategy with a so-called duration or survival model. We use the same strategy with a linear rather than a duration model for our secondary outcomes: market labor earnings and donations. We begin this section by discussing our main empirical strategy in the context of these secondary outcomes in Section 5.1, followed by the model employed for volunteer duration in Section 5.2. Thus, section 5.2 also highlights the modeling differences when we switch from a linear to a duration model and the corresponding estimation sample differences.

5.1 Difference-in-Differences

Our causal identification strategy compares the policy-induced responses of individuals "at the margin", i.e., receiving the maximum tax-free instructor allowance of €2100 pre-policy (treatment group), to individuals with lower pre-policy allowances between €1000 and €2000 (control group).[19] The volunteers receiving the maximum allowance are more "at risk" of being treated because the reform allows them to expand their monetary compensation without crossing the tax-free threshold, while the latter applies to mid-paid volunteers regardless of the reform. Using these so-defined treatment and control groups, we can test if the between-group differences in labor earnings and donations have changed after the 2013 reform.

We attribute the DiD estimates to the policy reform conditional on a set of assumptions. First, the parallel trends assumption posits that, in the absence of the policy change, volunteers in both groups would have seen similar developments in their regular job earnings and donations. Second, we assume that no external factors or concurrent shocks differentially impacted the treatment and control groups at the time of the policy change (the common shocks assumption). Third, the Stable Unit Treatment Value Assumption (SUTVA) requires that an individual's potential outcomes depend only on their own treatment status, and not on the treatment status of others. We discuss these assumptions in more detail and provide evidence and arguments that they are likely to hold in our setting at various stages of this paper.

Given these assumptions, we study the impact of the increased instructor payment since 2013 on donations and labor earnings using the following regression model:

$$Y_{igt} = \chi_i + \lambda_t + \sum_{j=2013}^{2018} \delta_j \left(max_g \times \mathbb{1}[t = j] \right) + \sum_{k=1}^{K} \beta_k X_{k,it} + \varepsilon_{igt}, \tag{2}$$

[19] In our linear model, we consider individuals' 2012 value as their pre-policy value to determine time-invariant treatment status. This implies that, especially in the years far after the policy, the DiD estimate is less likely to pick up treatment differences, as individuals might have quit their 2012 volunteer spell by that time.

where Y_{igt} is one of our outcomes of interest, labor earnings and donations, for person i in group g and year t. χ_i are individual fixed effects, and λ_t are the year fixed effects capturing time trends common to both groups of volunteers. max_g is equal to one for individuals who were volunteers "at the margin" in 2012, while it is equal to zero for those who were mid-paid volunteers in 2012. The set of DiD estimands $\sum_{j=2013}^{2018} \delta_j \left(max_g \times \mathbb{1}[t = j]\right)$ captures year-specific treatment effects, with δ_j representing the difference in outcomes between treatment and control groups in year $j \geq 2013$ relative to the pre-treatment period 2007-2012. Thus, we estimate the average treatment effect on the treated (ATT) separately for all the post-treatment years 2013-2018, allowing for heterogeneity in yearly treatment effects.[20] $\sum_{k=1}^{K} \beta_k X_{k,it}$ is our controls term, where $X_{k,it}$ denotes the k-th covariate for individual i in year t and β_k is its corresponding estimand. Individual controls proxy labor market status (such as indicators for being retired, receiving benefits), the type of occupation and earnings (from agriculture, real estate, self-employment, employment), and commuting effort. We also control for marital status and having no children or children younger or older than 18, and we include indicators for different religious beliefs, as well as federal state fixed effects.[21] ε_{igt} are errors clustered at the individual level.

5.2 Volunteer duration

We now turn to our primary outcome, volunteer duration. Specifically, we are interested in modeling the probability of a volunteer "spell" ending at the end of analysis year a, conditional on the spell having lasted until that point: its hazard rate. A spell is defined as a continuous period during which an individual receives the same amount of volunteer compensation. As our primary modeling approach, we adopt the Cox proportional hazards-model (Cox, 1972). This semi-parametric model eliminates the need to specify the functional form of the baseline hazard $h_0(a)$, meaning that no stringent assumptions have to be imposed.

Our approach to analyze the effects on volunteer duration builds on the DiD strategy from Section 5.1. We now compare the pre- and post-reform differences in the hazard rate of maximum-paid volunteers, those that are likely affected by the reform, to medium-paid volunteers, i.e., those that received a payment between €1000 and €2000 pre-reform. Following Hall and Hartman (2010) who face a conceptually similar set-up, we focus on spells starting after the first wave in our data but before the policy change of 2013, i.e., somewhere during the years 2008-12, and follow them until they end or until our dataset finishes in 2018. This implies that our sample also includes mid-paid or max-paid volunteer spells of individuals who have no such spell in the pre-reform year 2012, but

[20] For this multiple periods case, the counterfactual assumption requires the strictest form of parallel trends to hold: For each $t^* > 2012$ and each $t' \leq 2012$: $\mathbb{E}\left[Y^0(t^*) - Y^0(t') \mid max = 1\right] = \mathbb{E}\left[Y^0(t^*) - Y^0(t') \mid max = 0\right]$.

[21] In robustness checks we include group fixed effects instead of individual fixed effects in which case we also control for gender and birth year. In such specifications we can estimate the level difference in Y_{igt} between the treatment and control group.

do have one at an earlier date. These individuals do not contribute to the DiD estimate $\hat{\delta}$ but reduce bias in the estimated level difference $\hat{\gamma}$ between the treatment and control group. Treatment status is based on the allowance level of individuals' most recent spell before 2013. By observing individuals with various spell lengths at the time of the policy change, we can separate the reform effect from a general duration effect.

We thus estimate the following DiD duration model:

$$h_i(a) = h_0(a)exp(\lambda_t + \gamma max_g + \delta\,(max_g \times post_t) + \boldsymbol{X}_i'(a)\boldsymbol{\beta}), \tag{3}$$

where $h_i(a)$ is the probability that the spell ends at the close of an analysis year a, i.e., the hazard rate of subject i, conditional on receiving the maximum volunteering allowance in the most recent spell starting before 2013, max_g, and on a vector of controls varying by analysis time $\boldsymbol{X}_i'(a)$. $h_0(a)$ is the baseline hazard function, which is the hazard rate when all covariates are set to zero. $Post_t$ is an indicator for the calendar years after the 2013 reform, which implies that differently from the linear DiD model in Section 5.1 we only estimate an average post-treatment effect. λ_t are calendar year fixed effects.

In addition to the DiD assumptions already mentioned and due to the special nature of the duration outcome, the Cox proportional-hazards model rests on important assumptions. First, we assume that the treatment indicator and the covariates specified in the model multiplicatively shift the baseline hazard function $h_0(a)$ and that this multiplicative shift is constant over time. Second, while the shape of the baseline hazard function $h_0(a)$ is unspecified, it is assumed to be common across all individuals.[22] Third, because our primary duration model is based on both single- and multiple-spell data, we must in principle assume that observed and unobserved explanatory variables are independent in order to take unobserved heterogeneity into account. However, Van den Berg et al. (2020) show that in settings like ours, with ongoing spells interrupted by a policy change, proportional-hazards estimates remain robust even when unobserved heterogeneity is ignored.[23]

6 Results

6.1 Main results

We begin this section by discussing the findings corresponding to our main outcome of interest, volunteer duration. Table 3 displays the estimates corresponding to our DiD

[22]This assumption can be relaxed in favor of assuming that baseline hazards differ by group (or strata). In our case we include robustness tests in Appendix C in which, following Hall and Hartman (2010), we allow the baseline hazard to be different for volunteers starting their spell in different years.

[23]The intuition is that, conditioning on elapsed duration at the reform date balances treated and control spells, mitigating bias from unobserved heterogeneity (Van den Berg et al., 2020). Hall and Hartman (2010) illustrate this empirically: their results are robust even when using a subset of their sample for multiple-spell analysis, implying no sensitivity to accounting for unobserved heterogeneity. In line with this, the estimates from our own multiple-spell analysis (Table C8) yield a similar conclusion as our main estimates.

regression Equation (3) with small variations in standard errors and control variables. Given the exponentiated coefficients, the estimates can be interpreted as the multiplicative impact (i.e., being different from 1) of a change in the independent variable on the conditional probability of a volunteer spell terminating at the end of a particular analysis year. Our DiD estimate of interest is the interaction of maximum-paid (Max) and Post, which indicates the additional difference in hazard between being in a maximum-paid versus a medium-paid volunteer spell after 2013 versus the period before (2008-2012). The estimate is of similar size and direction in all models, but always insignificantly different from 1, meaning that the policy change of 2013 did not differentially affect the volunteer duration of existing instructor volunteers. The direction of the estimate suggests that, if anything, the policy change relatively shortened rather than lengthened the treatment group spells.

Table 3: Effect of the allowance increase on volunteer duration

	(1)	(2)	(3)	(4)	(5)
Max	0.685***	0.685***	0.684***	0.685***	0.684***
	(0.024)	(0.016)	(0.016)	(0.016)	(0.016)
Max × Post	1.109	1.104	1.111	1.106	1.103
	(0.116)	(0.087)	(0.087)	(0.087)	(0.087)
Controls	No	Labor	Household	Religious	All
Observations	9467	9467	9467	9467	9467

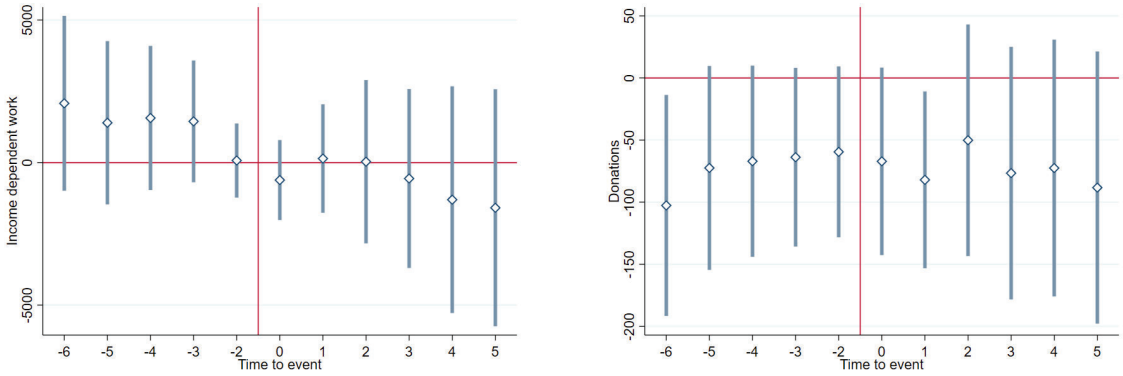
Source: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, DOI: 10.21242/73111.2018.00.01.2.1.1, own calculations.

Notes: Exponentiated coefficients. Based on the TPP 5% sample. Labor market controls consist of dummies for being retired, receiving benefits, and earning income mainly from agriculture, real estate, self-employment, or employment. Household controls consist of dummies for being married, having no kids, or having a kid below 18 y/o. Religious controls consist of dummies for protestant and catholic. Column (5) combines these controls and includes a dummy for gender and birth year as a continuous control. Except for column (1) without any cluster level, standard errors in parentheses are clustered at the individual level. $^{\sim}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Interestingly, we do observe a sizable baseline difference between the maximum- and medium-paid groups of volunteers. The estimate for maximum-paid is significant and stable across models, indicating an approximate 30% lower conditional probability of ending a volunteer spell in comparison to medium-paid volunteers. This is in line with the longer average spell length for the former relative to the latter group as observed in our descriptive Table 2.

Our heterogeneity analyses, reported in Tables C3 and C4 in Appendix C.4, show that the absence of an effect is mostly shared across individuals with different characteristics. The direction of our main estimate appears to be driven by individuals in the lowest half of the income distribution (Table C3). This is not in line with the intuition that the marginal benefit of consumption U'_x is highest for lower-income groups, implying that

these groups would most value extrinsic incentives relative to intrinsic incentives and thus be less prone to crowding out.



(a) Labor income

(b) Donations

Figure 4: Event-study representation of the effects of the increased volunteer allowance

Source: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, DOI: 10.21242/73111.2018.00.01.2.1.1, own calculations.
Notes: The excluded period is -1, the pre-reform year 2012. Individual fixed effects are included. Based on the TPP 5% sample with labor income and donations winsorized at the 3% level.

Moving to our secondary outcomes, Figure 4 displays the results for labor income and donations when we take the year 2012 as our baseline value.[24] The figure shows that labor income and donations do not significantly change in the aftermath of the reform for the maximum-paid volunteers relative to their medium-paid counterparts.

Figure 4 also lends support to the parallel trends assumption, as we perform thorough pretreatment trend analyses, examining whether the two groups exhibited consistent patterns in labor income and donations before 2013. Even though the estimates are not centered around zero in the pre-period, they hover around small amounts (such as €50 in the case of donations) and they are not statistically distinguishable from zero. We conclude that the yearly pre-policy differences between the treatment and control group do not significantly differ from the 2012 baseline year and that there are no anticipatory effects of the reform. This suggests that our results are not explained by an existing trend in the differences between the control and treatment group.

For our main DiD results we focus on the estimates of regression Equation (2). The results are displayed in Table 4. For both outcomes, the year-by-year treatment effects are economically small and not statistically different from zero. For labor income, it appears that being affected by the reform makes volunteers, if anything, substitute away from labor income (columns (1) and (2)). The coefficient estimates for donations are negative or close to zero, but also imprecisely estimated. Overall, the results show no effect of the

[24] We show in Appendix Figure C7 that the inverse hyperbolic sine transformation delivers a similar picture, however this approach is currently criticized in the econometric literature when outcome variables have mass at zero (Chen and Roth, 2024; Mullahy and Norton, 2024). Moreover, Figure C6 shows that indeed, especially for donations, outliers in the unwinsorized and untransformed variables contribute to the instability of the results and add noise to the estimates.

increased volunteer allowance on donations and income from the labor market.

Table 4: Allowance increase effects on labor and donations (individual FE)

	Labor income		Donations	
	(1)	(2)	(3)	(4)
Max × 2013	-1633.536	-1163.728	-8.608	-6.839
	(1122.567)	(1078.583)	(38.296)	(38.265)
Max × 2014	-865.924	-561.257	-23.733	-23.014
	(1326.311)	(1264.743)	(37.526)	(37.409)
Max × 2015	-961.860	-1049.615	7.953	8.525
	(1668.833)	(1549.976)	(48.150)	(48.101)
Max × 2016	-1565.251	-1679.744	-18.570	-18.645
	(1863.651)	(1721.497)	(49.459)	(49.433)
Max × 2017	-2335.664	-2880.399	-13.392	-13.804
	(2268.189)	(2092.766)	(50.556)	(50.799)
Max × 2018	-2583.693	-3438.756~	-30.201	-30.491
	(2324.205)	(2078.401)	(53.277)	(53.267)
Controls	No	Yes	No	Yes
Observations	25512	25512	25512	25512

Source: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, DOI: 10.21242/73111.2018.00.01.2.1.1, own calculations.

Notes: Based on the TPP 5% sample with labor income and donations winsorized at the 3% level. Controls consist of dummies for being retired, receiving benefits, protestant, catholic, married, having no kids, having a kid below 18 y/o, earning income mainly from agriculture, real estate, self-employment, or employment. We also include federal state fixed effects and a continuous proxy for commuting effort. Standard errors in parentheses clustered at the individual level. ~ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

We also perform heterogeneity analyses for these secondary outcomes, where we interact our DiD estimator with various time-invariant characteristics. We find no evidence that the policy response differs by gender or religion (Table C10). Table C11 presents some weak evidence that individuals with children younger than 18 years old have a less negative donation response than individuals with older or no children.

We next split the sample into three groups based on their main source of income in 2012 (labor, capital, or retirement benefits, see Table C12). No major differential patterns emerge from these three groups compared to the baseline. Similarly, we see no differences between East and West Germany (Table C13). Finally, we split our sample into four groups based on their position in the household income distribution. Table C14 shows that the negative direction of our main coefficients is mostly driven by individuals in the highest income quartile, which could be linked to this group experiencing income changes of relatively larger magnitude.

6.2 Alternative matching strategy

A limitation of the above DiD strategy is that we estimate the effect of the strong likelihood of receiving a higher allowance instead of the effect of an actual increase in this allowance. Since not all individuals at the margin were necessarily constrained in their volunteer compensation before the reform, there will be a group whose compensation did not expand to the new tax-free maximum. Therefore, we introduce a second estimation strategy that allows us to compare the difference in outcomes between individuals who receive an increase in their allowance after 2013 and similar individuals who do not. This analysis relies on the pre-2013 maximum-paid volunteers that split into two groups: (i) the group receiving the new maximum (treated) and (ii) the group still receiving the old maximum in 2013 (control). Because of our panel data we can use individual fixed effects to absorb individual time-invariant heterogeneity. Moreover, to address the possibility of endogenous selection into treatment we apply a matching algorithm to compute the treatment effect based on the comparison of similar individuals (in the propensity to be treated).

We estimate propensity scores and limit our sample to matched individuals following the approach originally proposed by Rosenbaum and Rubin (1983) implemented via the `psmatch2` Stata command (Leuven and Sianesi, 2018).[25] The average treatment effect will thus depend on the presence and the distribution of treated and control individuals across these categorical observable characteristics in 2012: being male or female, age, religious affiliation, marital and parental status, type of income sources (self-employment, rental market, agriculture or forestry), being retired, receiving training or benefits, and the federal state of residence.[26] Descriptive statistics for the matched sample are displayed in Appendix Table C2 and support a high degree of similarity between the two groups. Appendix Figure C9 and results from balancing tests show a clear improvement in the comparability between the two volunteer groups after implementing propensity score matching.[27]

We first look into the continuation of volunteers with a payment increase. Given the nature of the sampling and treatment assignment, the assumptions of the Cox duration model are not valid in this setup and would likely lead to biased estimates. We therefore rely on a simple logistic regression model to estimate the likelihood of spell continuation into 2014 based on the (propensity score matched) allowance level in 2013, effectively treating it as a one-year survival outcome. Tables C20 and C21 present the results.

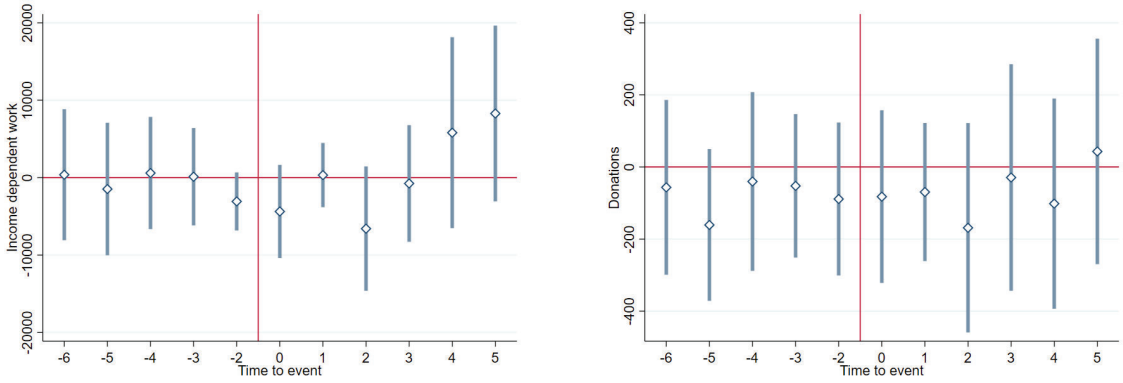
[25] Given that our observable variables are limited in number and have a categorical nature, we also present results based on an alternative matching technique: coarsened exact matching (Blackwell et al., 2009; Iacus et al., 2012). Test results confirm that the imbalance measure between the two groups shrinks after the implementation of the matching algorithm relative to the unmatched baseline comparison of the two groups. Results available upon request.

[26] Ideally we would use information on individuals' volunteer organizations in our regression, but this is not included in the TPP data. However, using the FWS data we can show that there are no significant differences across allowance thresholds with respect to the organizational setting and perceived need for organizational improvement (see Table 6).

[27] Output from balancing tests available upon request.

However, we are cautious to interpret these results, as this "duration" matching approach does not include individual fixed effects, implying that the treatment estimate may still conflate the true causal effect with baseline level differences between the "new maximum" and "old maximum" groups of volunteers.

The matching results of the analysis for our secondary outcomes are displayed in Figures 5a (labor income) and 5b (donations). These Figures also suggest that parallel pre-trends between old-max and new-max individuals are satisfied. The insignificant differences from the 2012-level difference between the treatment and control group indicate that the null results for the labor earnings and donations outcome using our main DiD strategy carry over to this matching approach. These main results are also robust to the application of coarsened exact matching as an alternative matching strategy (see Appendix Figure C10).



(a) Labor income

(b) Donations

Figure 5: Event-study representation of the effects of the increased volunteer allowance based on PS matching strategy

Source: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, DOI: 10.21242/73111.2018.00.01.2.1.1, own calculations.
Notes: The excluded period is -1, the pre-reform year 2012. Individual fixed effects are included. Based on the TPP 5% sample with labor income and donations winsorized at the 3% level.

These graphical patterns are supported by the regression estimates shown in Appendix Table C17. Appendix Table C18 shows that these results are robust to a battery of robustness tests, including clustering standard errors at the household or municipality levels, or using an alternative outcome variable for labor income. Importantly, Appendix Table C19 shows a marginally significant decrease in labor income in the year after the reform, relative to the year prior, but this short-term effect is wiped out in the long run, considering average changes over the whole study period.

6.3 Robustness

We conduct a range of robustness checks to verify that our findings are not driven by incorrect assumptions or measurement choices. Across specifications, our results remain mostly insignificant and qualitatively unchanged. The most notable differences are (i)

larger and significant effects in a short-term linear model of volunteer exits and (ii) slightly stronger negative effects on labor income when accounting for local volunteering environments. Below we provide an overview of the most important checks, splitting their presentation for our primary and secondary outcomes. For convenience, Table 5 synthesizes all the robustness checks and their key findings.

6.3.1 Volunteer duration

To begin with, our DiD strategy assumes that mid-paid volunteers are unconstrained in their pre-policy choice to move to higher-compensated positions. In practice, limited local opportunities could constrain such moves. A plausible concern is thus that after the allowance increase, some mid-paid volunteers are financially motivated enough to quit their current position to take up a €2400-max-paid position elsewhere. We observe no increase in spell exits at the end of 2012 and 2013 among the group of mid-paid volunteers in our sample (Figure C5), relaxing this concern.

A methodological concern related to our main duration model is that the control group has shorter average spells than the treatment group (see Table 2). Because we follow only the spells that started before the 2013 policy change, the shorter control group spells may cause "mechanical depletion", i.e., the control group continuing their volunteer spell after 2013 may be highly selected, potentially affecting our DiD estimate. To address this, we implement a triple difference (DDD) approach based on duplicating the main sample, but cutting it at a date prior to 2012. We then stack the two samples. We test whether the post-cutoff treatment-control hazard difference changes between the two cutoff years through the coefficient estimate for $(max_g \times post_{st} \times 2012\_\text{cutoff}_s)$, where s is also our stratification unit and indicates whether the observation is part of the main estimation sample or the sample that uses a cutoff year prior to 2012.[28] This captures the reform effect net of mechanical depletion, under the identifying assumption that the depletion pattern is stable across cutoff dates. This assumption is not rejected empirically by a pre-trend test comparing the 2010 and 2011 cutoff samples, which yields an insignificant DDD estimate. Table C5 presents results for three cutoff comparisons with symmetric post windows of two, three, and one year(s) respectively. Across all specifications the DDD estimate is relatively close to our main estimate and statistically insignificant, suggesting that our main finding is not driven by mechanical depletion bias.

Additionally, we estimate a simplified short-term linear probability model where the outcome is whether a volunteer quit in 2012 or 2013. The estimates in Table C6 show that the maximum-paid volunteers are on average less likely to exit their volunteering spell, with a magnitude very similar to the one estimated in Table 3. Next, the DiD estimate shows increased hazard for max-paid volunteers in 2013, indicating that this

[28] In which case, the post indicator is a dummy for $t \geq (Z + 1)$, with Z as the alternative cutoff year to 2012. The number of years between Z and 2012 determines the number of post-cutoff years that we include in our estimation samples, to make sure that the pre-2012 cutoff sample does not span post-policy years.

pattern reverts in the post-reform period. This effect is larger in magnitude and much more precisely estimated than our main duration finding, but consistent in sign.

Furthermore, we explore sensitivity to potential violations of censoring and truncation assumptions. We originally treat right censoring by assuming that the 2018 study endpoint is uninformative for the hazard rate. Panel exits before 2018 are treated as spell terminations; mobility abroad or death are the main causes, and these are unlikely to differ systematically across groups. As a check, we re-estimate the duration model on a balanced panel, which delivers a coefficient that is very close to the baseline estimate (Table C7). Left truncation is possible if instructors enter the panel mid-spell. Our main results assume that spell entries coincide with late panel entries. We relax this by (i) excluding entrants after 2007 who volunteer in their first observed year and (ii) excluding individuals under 30 as they might have just entered the labor market and started filing their taxes. The estimates for (i) and (ii) remain unchanged (Table C7). The Cox model's focus on event timing rather than absolute time mitigates further concerns (Cleves et al., 2016).

Finally, we shift to multiple-spell analysis by implementing a stratified partial maximum likelihood estimator with individuals as strata (Ridder and Tunalı, 1999). Since this requires at least one relevant pre-reform and one post-reform spell per individual, we lose roughly one-fourth of observations. Our DiD duration coefficient drops to 0.94 but remains insignificant (Table C8). The similar (to our main result) insignificant difference from 1 suggests that our main results are not driven by dynamic unobserved heterogeneity.

6.3.2 Labor income and donations

Besides checking the sensitivity of the results with respect to the winsorization or transformation of the outcome variables (see Appendix Figures C6 and C7), we conduct several robustness checks. Table C9 reports the quantification of the main results following regression Equation (2) including group fixed effects (GFE) rather than individual FE. These results align with our main estimates and also highlight baseline differences between the two groups: maximum-paid volunteers have, on average, a €7500-10000 lower labor income and donate approximately €200 more. This pattern, reflected in the "Max group" coefficient, is consistent with the descriptive statistics reported in Table 2.

Identification relies on the SUTVA, which we believe to be plausible given that treatment assignment is private information and unlikely to affect other individuals' outcomes. However, local volunteering environments could induce spillovers. Including municipality fixed effects and clustering at this level slightly strengthens the estimated negative labor income effect, but does not alter overall conclusions. Clustering standard errors at the household level also does not alter the insignificance of our estimates.

Restricting the estimation to two years around the reform or estimating an average effect for the full post period yields similar patterns (Table C16).

Table 5: Summary of robustness checks

Threat/assumption	Method	Finding
Panel A: Volunteer duration		
Unknown behavior of control group	Describe exits per calendar year	Exits do not increase in 2012 and 2013
Survivor selection bias	DDD approach	Est. insign., hovers around 1
Short-term spells not accurately captured	Short-term LPM	Est. has larger magnitude, same sign as baseline
Baseline hazard differences	Stratify by (2) year of volunteer entry	No change
Household or migration dependence	Cluster at household level; restrict to non-movers	No change
Right censoring	Exclude pre-2018 exiters; balanced panel	No change
Left truncation	Exclude (1) post-2007 entrants and (2) under 30 at entry	No change
Allowance-related differences	Exclude late adopters in control group; exclude individuals with excess allowance	No change
Dynamic unobserved heterogeneity	Stratified partial likelihood on the individual level	Est. drops to insignificant 0.94
Panel B: Secondary outcomes		
Outcome measurement	Use gross income; alternative transformations	No change
Allowance-related reporting issues	Exclude those donating back allowance or with allowance equal to donations	No change
Spillovers	(1) Municipality FE & clustering; (2) exclude people changing municipality of residence; (3) household-level clustering	Slightly stronger negative labor income effect for (1), no change for (2) or (3)
Timing window	Two-year vs. full post period	Similar results
Specification choice	Group instead of individual FE	Similar but noisier estimates

Notes: Output referred to in Panel A is shown in Appendix Tables C5, C6, C7, and C8 and Figure C5. Output referred to in Panel B is available in Appendix Tables C9, C15, and C16.

6.4 Descriptive survey data results

We report a selection of output based on the FWS data in Appendix D. Table D1 shows the descriptive statistics for four different groups: those that never volunteered, unpaid volunteers, paid volunteers, and former volunteers. When looking at the differences between paid and unpaid volunteers, we can notice that paid volunteers work more hours and on average for longer than unpaid volunteers, but the differences are not striking. There are no large differences across the four groups with respect to household composition, citizenship, and residence (East and West Germany).

After showing that the organizational life in Germany has remained rather stable around the reform (see Table D3), we ask if there are systematic differences between the associations where volunteers receive a higher or lower allowance. We first compare volunteers with an allowance up to €600 per year with volunteers with an allowance between €600 and €1800 per year, following the allowance categories cutoffs determined by the FWS.[29] In this way we focus on the general allowance and credibly split the group with a low tax-free allowance from those with a higher one. Secondly, we compare volunteers with an allowance between €600 and €1800 per year with volunteers with an allowance between €1800 and €4200 per year, thus focusing on the group around the instructor allowance maximum.

Table 6 shows the mean differences for perceptions about needs for improvement at the organizational or state level between these two groups in 2014. In column (1) the group with an allowance between €600 and €1800 is associated with a 9.4 percentage point higher likelihood of having paid staff at their organization relative to the group with a lower allowance. However, there are no large nor significant differences with respect to improvement categories, especially with respect to remunerations. In column (2) the only significant difference is also related to the presence of paid staff in the organization, implying that the group of volunteers receiving an allowance between €1800 and €4200 is more likely (by 14 percentage points) to volunteer in an organization with paid staff than the group with a €600–1800 allowance. These pieces of evidence support our expectation that the associations of volunteers who are compensated with different amounts are not systematically different in terms of quality, suggesting that the allowance increase might have an element of randomness.

We look into former volunteers to investigate potential time variation in the reasons for ending the voluntary work.[30] The data allows us to compare over time only two reasons for quitting: voluntarism was ended due to the fact that it took up too much time, and the fact that the activity was limited in time (project-based). Table D4 shows that these are both important reasons why people stop volunteering. Between 2009 and 2014 there are no significant differences in the decision to quit due to time constraints. There is however a significantly higher likelihood of ending volunteering due to being

[29]The FWS cutoffs are expressed in monthly amounts (i.e., twelve times smaller). We use the corresponding yearly values to ease comparison with the TPP data.

[30]We unfortunately cannot explore whether reasons for ending differ between former paid and former unpaid volunteers on the basis of our data.

involved in a project-based activity. This does not appear directly related to the €300 increase, but might be conflated with our effect of interest if the maximum-paid positions are particularly more or less likely to have become project-based.

Table 6: Organizational characteristics and improvement needs related to volunteer payment differences (t-test based on 2014 values)

Binary outcome	(1) Δ Low & medium-paid (SE)	(2) Δ Medium & high-paid (SE)
Paid staff in organization	-0.094**	-0.143*
	(0.035)	(0.058)
Organiz. improvement needs		
Training	-0.023	-0.072
	(0.033)	(0.056)
Recognition by staff	-0.008	-0.047
	(0.033)	(0.055)
Financial remuneration	-0.012	-0.042
	(0.033)	(0.056)
Cost reimbursement	0.014	-0.038
	(0.033)	(0.056)
Equipment	0.029	-0.003
	(0.033)	(0.055)
State improvement needs		
Tax deduction of costs	0.012	-0.056
	(0.033)	(0.055)
Tax exemption of allowance	-0.014	-0.013
	(0.033)	(0.054)
Observations	999	462

Notes: Based on the FWS data. All the reported categories refer to binary variables. (1) Volunteers with allowance up to €600 per year are compared with volunteers with an allowance between €600 and €1800 per year. (2) Volunteers with allowance between €600 and €1800 per year are compared with volunteers with an allowance between €1800 and €4200 per year. $^{\sim}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

We next present some simple regression results using hours volunteered as well as volunteer duration in years as outcome variables. The idea is to test whether the extension of the allowance implied an increase in effort by the volunteers. We therefore interact the allowance categories with a year dummy to test for differences between the pre- and post-reform periods. The results are shown in Table 7. The results for hours volunteered in columns (1) and (2) show no differences except for a small decrease (about 12 minutes per month) for the group receiving €600-1800 per year in the post-period, which does not

speak for an increase in effort. The results for years volunteered in columns (3) and (4) also show no statistically significant differences across the allowance categories between the pre- and post-reform periods. This suggests that volunteers who receive the post-policy allowance increase do not work more as a result.

Table 7: Change in effort: hours volunteered and years of volunteer work.

	Volunteer hours		Volunteer years	
	(1)	(2)	(3)	(4)
600 to 1800 Euro	0.451***	0.438***	-1.606*	-0.542
	(0.069)	(0.070)	(0.715)	(0.641)
1800 to 4200 Euro	0.758***	0.701***	0.185	-0.160
	(0.124)	(0.122)	(1.117)	(0.979)
4200 Euro or more	1.064***	1.059***	0.097	-0.279
	(0.203)	(0.208)	(2.114)	(1.768)
2014	0.003	0.018	-0.626	-0.062
	(0.049)	(0.051)	(0.626)	(0.561)
600 to 1800 Euro × 2014	-0.185*	-0.215*	1.425	-0.268
	(0.094)	(0.096)	(0.991)	(0.891)
1800 to 4200 Euro × 2014	-0.184	-0.197	-1.152	-1.384
	(0.171)	(0.170)	(1.543)	(1.339)
4200 Euro or more × 2014	0.045	0.058	0.731	-0.772
	(0.261)	(0.267)	(2.607)	(2.240)
Controls	No	Yes	No	Yes
Observations	2037	1932	2118	2006

Notes: Based on the FWS data. The outcome for columns (1) and (2) is monthly hours of volunteering. For columns (3) and (4) it is the length of the current volunteer spell in years. The omitted payment category is an allowance up to 600 Euro per month. Controls include dummies for gender, educational level, employment status, religion, number of people in household, municipality size, East-West Germany. Age is included as a continuous variable. Robust standard errors in parentheses. $^{\sim}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Finally, we use a similar set-up to explore the role of values in participating in paid volunteering around the policy change. Table D6 shows that, using very rough proxies for intrinsic and extrinsic motivation, we find no evidence that extrinsically motivated individuals were more likely to be high-paid after 2013 than before.

7 Discussion and conclusion

This paper focuses on the increasingly common phenomenon of paid volunteering, where a low (relative to market labor positions) monetary compensation is used to reward

prosocial behavior. In particular, we examine the effects of an increase in the tax-exempt allowance for instructor volunteers in Germany, a policy intended to foster volunteering.

Our main findings do not show that volunteers receiving an exogenous increase in their monetary compensation alter their volunteer duration, labor earnings or donations. If anything, the probability of quitting a volunteering activity increases rather than decreases with additional monetary compensation. Based on descriptive results, we also see no spike in probability to volunteer after the policy change among our representative population sample. This implies that the reform has not had the desired result of strengthening the volunteer sector through increasing volunteer retention of existing volunteers. At the same time, there are no observable effects of the allowance increase spilling over to declared donations, suggesting overall no significant changes in prosocial behavior.

The close alignment of estimates from our main difference-in-differences (DiD) strategy and an alternative matching strategy provides strong evidence that the absence of effects is a genuine pattern among affected volunteers, rather than an artifact of a specific empirical design. The main DiD approach compares individuals earning the maximum pre-reform tax-free allowance, thus most susceptible to the tax reform, with a control group of mid-earning volunteers. This does not require a post-reform compensation increase in practice,[31] which is nevertheless observed by over 70% of treated individuals (see Table C2). Thus, our main estimates should be interpreted as capturing an Intention-to-Treat (ITT) effect. Given this relatively high compliance rate, we do not expect that any true effect is substantially larger than our ITT estimates. The convergence between DiD and matching estimates also suggests that any downward bias is limited. Furthermore, the robustness of our results across various model specifications, sample restrictions, and measurement adjustments is consistent with the conclusion that large positive effects on volunteer duration or on our secondary outcomes are unlikely, while acknowledging that such effects cannot be ruled out for labor earnings given the large confidence intervals.

From a theoretical perspective, our results are consistent with studies showing that small monetary incentives have limited or even adverse effects on prosocial behavior. Frameworks such as Bénabou and Tirole (2006) suggest that marginal increases in extrinsic incentives may not strengthen prosocial behavior by negatively affecting intrinsic motivation for the action or the public or private image of doing it. In the German paid volunteering context, where the allowance is modest relative to market wages, our evidence indicates that an allowance increase does not produce a positive, but also no negative, volunteer supply response. Relating this empirical finding to our theoretical model from Section 2 suggests, if anything, that the volunteer discount factor ρ does not take an extreme value. We note, however, that an empirical zero could also reflect the modest treatment size and that the theoretical model's focus on volunteering intensity imperfectly matches our duration outcome.

[31] This approach preserves the quasi-experimental nature of the policy change and avoids biases that could arise from conditioning on post-treatment behavior.

Empirically, our findings add to the literature on monetary incentives and prosocial or moral behavior by studying their effect in the "new" context of paid volunteering as an instructor or educator. For example, whereas Wollbrant et al. (2022) and Fiorin (2023), respectively, find that monetary incentives may backfire for behaviors like recycling and reporting the absence of peers, Chetty et al. (2014) and Lacetera et al. (2014), respectively, show that monetary incentives may (temporarily) improve refereeing in an economics journal or donating blood to a bloodbank. Our results for volunteer duration similarly show no permanent effect, while we also find no immediate spillovers on donations or labor earnings. This suggests that either the €300 change in incentive is too small relative to these previous studies to evoke even an immediate effect, or that the positive and negative forces of monetary incentives balance out in the behavior of paid volunteers.

For policy design, our findings suggest that modest increases in volunteer compensation are unlikely to improve retention. Instead, other strategies that involve non-monetary rewards, such as providing participation certificates, seem to encourage participation in a different setting (Conrads et al., 2016). Other forms of recognition that avoid the potential counter-effects of extrinsic incentives, such as fostering organizational attachment (Forner et al., 2024), may be even more effective. Importantly, because the reform primarily increased the compensation for volunteers who were previously below the tax-exemption threshold, rather than decreased above-threshold taxable payments, the policy's fiscal burden for the German government was minimal. By contrast, volunteer organizations bear the additional cost of paying higher allowances without clear benefits, especially since we find that these increases also do not spill over to reported charitable donations. As an additional insight for the design of the tax system and its incentives for prosocial behavior, our results suggest that tax incentives for paid volunteering are far less effective than tax incentives for charitable donations, for which responsiveness is well-documented (e.g., Tiehen, 2001; Han et al., 2024).

We recognize certain limitations of our study, mostly stemming from certain aspects of the data. First, we do not observe hours volunteered directly. Instead, we use survey data to validate that volunteer effort, measured in hours worked, does not change significantly after the policy reform. However, this indirect measure may miss more subtle shifts in volunteer behavior. Second, our inability to identify interpersonal networks within the tax return data, such as volunteers within the same organization and peer effects, means we rely on household and municipality indicators as proxies for these dynamics. While these proxies help address the issue, they may not fully capture the complexities of networks. Future research with more granular data on networks and volunteer hours could provide a more comprehensive understanding of these dynamics. Third, given our yearly data, we cannot perfectly isolate the policy's exact implementation date, which raises some concerns about common shocks. One potential confounder is the "Act to Strengthen Volunteering," rolled out in 2013-2015 (see Appendix B). However, because both treatment and control groups consist of volunteers in educational rather than administrative roles, they are unlikely to be directly affected

by these reforms and we do not expect differential impacts. Last, given the policy's broad reach and our national coverage, the external validity of our study for the German population is very high. We also expect high validity for contexts where paid volunteering is treated similarly, such as the Netherlands. However, extrapolation to settings with different institutional or cultural frameworks should be done with caution.

In light of these points, our study represents a clear contribution to the literature. While earlier studies have examined correlations between pay and volunteer effort or retention, and largely focused on unpaid volunteers, our results indicate that even among volunteers who were already monetarily compensated, small financial incentives do not extend volunteer duration.

References

An, J., Briley, D., Danziger, S., and Levi, S. (2023). The impact of social investing on charitable donations. *Management Science*, *69*(2), 1264–1274.

Angrist, J. D., and Krueger, A. B. (1999). Chapter 23 - Empirical strategies in labor economics. In O. C. Ashenfelter and D. Card (Eds.), (Vol. 3, pp. 1277–1366). Elsevier.

Auten, G. E., Sieg, H., and Clotfelter, C. T. (2002). Charitable giving, income, and taxes: An analysis of panel data. *American Economic Review*, *92*(1), 371-382.

Blackwell, M., Iacus, S., King, G., and Porro, G. (2009). cem: Coarsened exact matching in Stata. *The Stata Journal*, *9*(4), 524–546.

Brown, A. L., Meer, J., and Williams, J. F. (2019). Why do people volunteer? An experimental analysis of preferences for time donations. *Management Science*, *65*(4), 1455–1468.

Bénabou, R., and Tirole, J. (2006). Incentives and prosocial behavior. *American Economic Review*, *96*(5), 1652–1678.

Camerer, C., Babcock, L., Loewenstein, G., and Thaler, R. (1997). Labor supply of New York City cabdrivers: One day at a time. *The Quarterly Journal of Economics*, *112*(2), 407–441.

Cappellari, L., and Turati, G. (2004). Volunteer labour supply: the role of workers' motivations. *Annals of Public and Cooperative Economics*, *75*(4), 619–643.

Carpenter, J., and Myers, C. K. (2010). Why volunteer? Evidence on the role of altruism, image, and incentives. *Journal of Public Economics*, *94*(11-12), 911–920.

Chen, J., and Roth, J. (2024). Logs with zeros? Some problems and solutions. *The Quarterly Journal of Economics*, *139*(2), 891–936.

Chetty, R., Saez, E., and Sandor, L. (2014). What policies increase prosocial behavior? An experiment with referees at the Journal of Public Economics. *Journal of Economic Perspectives*, *28*(3), 169-88.

Cleves, M., Gould, W., and Marchenko, Y. V. (2016). Censoring and truncation. In *An Introduction to Survival Analysis Using Stata* (4th ed., chap. 4). College Station, Texas: Stata Press.

Collischon, M., Cygan-Rehm, K., and Riphahn, R. T. (2021). Employment effects of payroll tax subsidies. *Small Business Economics*, *57*(3), 1201–1219.

Compion, S., Meijs, L., Cnaan, R. A., Krasnopolskaya, I., von Schnurbein, G., and Abu-Rumman, S. (2022). Repeat and non-returning volunteers: The promise of episodic events for volunteer recruitment and retention. *VOLUNTAS: International Journal of Voluntary and Nonprofit Organizations*, *33*, 472–482.

Conrads, J., Irlenbusch, B., Reggiani, T., Rilke, R. M., and Sliwka, D. (2016). How to hire helpers? Evidence from a field experiment. *Experimental Economics*, *19*, 577–594.

Cox, D. R. (1972). Regression models and life-tables. *Journal of the Royal Statistical Society: Series B (Methodological)*, *34*(2), 187–202.

Dallmeyer, S., Breuer, C., and Feiler, S. (2024). To pay or not to pay? The effects of monetary compensation on volunteers in sports clubs. *Nonprofit and Voluntary Sector Quarterly*.

Davis, S. J., and Krolikowski, P. M. (2025). Sticky wages on the layoff margin. *American Economic Review*, *115*(2), 491–524.

Eibich, P., Lorenti, A., and Mosca, I. (2022). Does retirement affect voluntary work provision? Evidence from Europe and the U.S. *Labour Economics*, *76*, 102185.

Einkommensteuergesetz. (2022). Bundesgesetzblatt [Federal Law Gazette]. Consolidated version as of 20 December 2022, Part I, p. 2730. Originally enacted on 8 October 2009. Retrieved from https://www.buzer.de/gesetz/4499/b12067.htm

Farber, H. S. (2015). Why you can't find a taxi in the rain and other labor supply lessons from cab drivers. *The Quarterly Journal of Economics*, *130*(4), 1975–2026.

Federal Ministry of Finance. (2025). *Amtliches Lohnsteuer-Handbuch*. Retrieved 19 June 2026, from https://esth.bundesfinanzministerium.de/lsth/2025/A-Einkommensteuergesetz/II-Einkommen-2-24b/2-Steuerfreie-Einnahmen-3-3c/Paragraf-3/r-3-26.html

Feldman, N. E. (2010). Time is money: choosing between charitable activities. *American Economic Journal: Economic Policy*, *2*(1), 103–130.

Fiorillo, D. (2011). Do monetary rewards crowd out the intrinsic motivation of volunteers? Some empirical evidence for Italian volunteers. *Annals of Public and Cooperative Economics*, *82*(2), 139–165.

Fiorin, S. (2023). Reporting peers' wrongdoing: Evidence on the effect of incentives on morally controversial behavior. *Journal of the European Economic Association*, *21*(3), 1033–1071.

Forner, V. W., Holtrop, D., Boezeman, E. J., Slemp, G. R., Kotek, M., Kragt, D., ... Johnson, A. (2024). Predictors of turnover amongst volunteers: A systematic review and meta-analysis. *Journal of Organizational Behavior*, *45*(3), 434–458.

Frank, R. H. (1996). What price the moral high ground? *Southern Economic Journal*, *63*(1), 1–17.

Frey, B. S., and Goette, L. (1999). *Does pay motivate volunteers?* (Working paper No. 7). University of Zurich, Institute for Empirical Research in Economics.

Frey, B. S., and Oberholzer-Gee, F. (1997). The cost of price incentives: An empirical analysis of motivation crowding-out. *American Economic Review*, *87*(4), 746–755.

Gesetz zur Stärkung des Ehrenamtes. (2013). Bundesgesetzblatt [Federal Law Gazette], Part I, Nr. 15, p. 556. Retrieved from https://www.buzer.de/gesetz/10546/a180006.htm

Hall, C., and Hartman, L. (2010). Moral hazard among the sick and unemployed: Evidence from a Swedish social insurance reform. *Empirical Economics*, *39*(1), 27–50.

Hameister, N., Kelle, N., Kausmann, C., Karnick, N., Arriagada, C., and Simonson, J. (2023). Monitoring civil society. The German survey on volunteering 1999–2019. *SozW Soziale Welt*, *74*(2), 294–314.

Han, X., Hungerman, D. M., and Ottoni-Wilhelm, M. (2024). *Tax incentives for charitable giving: New findings from the TCJA* (Working Paper No. 32737). National Bureau of Economic Research.

Holtrop, D., Soo, C., Gagné, M., Kragt, D., Dunlop, P. D., and Luksyte, A. (2024). Exploring volunteer turnover reasons, intentions, and behavior. *Group & Organization Management*, Article 10596011241237841.

Iacus, S. M., King, G., and Porro, G. (2012). Causal inference without balance checking: Coarsened exact matching. *Political Analysis*, *20*(1), 1–24.

Lacetera, N., Macis, M., and Slonim, R. (2014). Rewarding volunteers: a field experiment. *Management Science*, *60*(5), 1107–1129.

Lachowska, M., Mas, A., and Woodbury, S. A. (2020). Sources of displaced workers' long-term earnings losses. *American Economic Review*, *110*(10), 3231–3266.

Leuven, E., and Sianesi, B. (2018). *PSMATCH2: Stata module to perform full mahalanobis and propensity score matching, common support graphing, and covariate imbalance testing.* Retrieved from https://EconPapers.repec.org/RePEc:boc:bocode:s432001

Lewis, S. (2015). Learning from communities: The local dynamics of formal and informal volunteering in Korogocho, Kenya. *IDS Bulletin*, *46*(5), 69–82.

Lilley, A., and Slonim, R. (2014). The price of warm glow. *Journal of Public Economics*, *114*, 58–74.

McCurley, S., and Lynch, R. (2007). The emerging issue of retaining volunteers. In *Keeping volunteers: A guide to retaining good people* (pp. 1–14). London: Directory of Social Change.

Meer, J., and Priday, B. A. (2021). Generosity across the income and wealth distributions. *National Tax Journal*, *74*(3), 655–687.

Mellström, C., and Johannesson, M. (2008). Crowding out in blood donation: Was Titmuss right? *Journal of the European Economic Association*, *6*(4), 845–863.

Merrilees, B., Miller, D., and Yakimova, R. (2020). Volunteer retention motives and determinants across the volunteer lifecycle. *Journal of Nonprofit & Public Sector Marketing*, *32*(1), 25–46.

Mesch, D. J., Tschirhart, M., Perry, J. L., and Lee, G. (1998). Altruists or egoists? Retention in stipended service. *Nonprofit Management and Leadership*, *9*(1), 3–22.

Mortensen, D. T. (1986). Job search and labor market analysis. In O. Ashenfelter and R. Layard (Eds.), *Handbook of labor economics* (Vol. 2, pp. 849–919). Amsterdam: North-Holland.

Mullahy, J., and Norton, E. C. (2024). Why transform Y? The pitfalls of transformed regressions with a mass at zero. *Oxford Bulletin of Economics and Statistics*, *86*(2), 417–447.

Niebuur, J., Liefbroer, A. C., Steverink, N., and Smidt, N. (2022). Transitions into and out of voluntary work over the life course: What is the effect of major life events? *Nonprofit and Voluntary Sector Quarterly*, *51*(6), 1233-1256.

Nyborg, K., and Zhang, T. (2013). Is Corporate Social Responsibility associated with lower wages? *Environmental and Resource Economics*, *55*(1), 107–117.

RDC of the Federal Statistical Office and Statistical Offices of the Federal States. (2021). *Finanzen und Steuern. Lohn- und Einkommensteuer 2017* (Fachserie 14 Reihe 7.1). Wiesbaden: Statistisches Bundesamt (Destatis).

RDC of the Federal Statistical Office and Statistical Offices of the Federal States. (2022). *Taxpayer-Panel 2001-2008, Lohn- und Einkommensteuerstatistik H35-73131-03.* Retrieved from https://doi.org/10.21242/73111.2018.00.01.2.1.1

Ridder, G., and Tunalı, I. (1999). Stratified partial likelihood estimation. *Journal of Econometrics*, *92*(2), 193–232.

Rosenbaum, P. R., and Rubin, D. B. (1983). The central role of the propensity score in observational studies for causal effects. *Biometrika*, *70*(1), 41–55.

Steward, G. E., Macis, M., Lacetera, N., Kahn, J. P., and Chib, V. S. (2026). *Incentives and prosocial discomfort: A laboratory experiment* (Working Paper No. 12433). CESifo.

Tassi, A. (2024). Do community needs affect the decision to volunteer? The case of refugees in Germany. *Journal of Economic Behavior & Organisation*, 106758.

Tiehen, L. (2001). Tax policy and charitable contributions of money. *National Tax Journal*, *54*(4), 707–720.

Tonin, M. (2020). Are workers motivated by the greater good? *IZA World of Labor*, Article 138.

Van den Berg, G. J., Bozio, A., and Costa Dias, M. (2020). Policy discontinuity and duration outcomes. *Quantitative Economics*, *11*(3), 871–916.

Voorintholt, L. (2023). Substitutes or complements: a budget-based analysis of the relationship between donating and volunteering. *Oxford Economic Papers*, *75*(4), 1033–1052.

Wet op de loonbelasting 1964. (2025). Consolidated version as of 1 January 2025, Article 2, paragraph 6. Originally enacted on 16 December 1964. Retrieved from https://wetten.overheid.nl/BWBR0002471/2025-01-01

Wollbrant, C. E., Knutsson, M., and Martinsson, P. (2022). Extrinsic rewards and crowding-out of prosocial behaviour. *Nature Human Behaviour*, *6*, 774—781.

Yang, W. (2016). Are contributions of time and money substitutes or complements? *Applied Economics*, *48*(37), 3526–3537.

A Model Appendix

A.1 Solution

The following set of first-order conditions (FOC) defines an agent's equilibrium state:

$$v: \quad U'_x w_v + U'_r(1-\rho w_v) = U'_l \kappa \tag{A1}$$
$$m: \quad U'_x w_m = U'_l \tag{A2}$$
$$d: \quad U'_d = U'_x. \tag{A3}$$

FOC (A1) shows that the optimal level of volunteering is reached when the marginal utility from consumption and the (discounted) marginal utility from volunteering equal the marginal cost of volunteering due to the loss of leisure time utility. For optimal labor supply, FOC (A2) indicates that the marginal consumption utility gained equals the marginal loss of leisure time utility. Finally, FOC (A3) shows that the optimal donation level is characterized by equating the marginal utility of donations to the marginal utility of consumption.

From the FOCs, we compute the implicit derivatives of optimal volunteering, donations, and market labor supply with respect to the volunteering allowance.

$$\begin{aligned}
v: \quad & w_v U''_x \left(w_m \frac{dm^*}{dw_v} + w_v \frac{dv^*}{dw_v} + v^* - \frac{dd^*}{dw_v} \right) + U'_x \\
& + (1-\rho w_v) U''_r \left((1-\rho w_v) \frac{dv^*}{dw_v} - \rho v^* \right) - \rho U'_r - U''_l \kappa \left(-\frac{dm^*}{dw_v} - \kappa \frac{dv^*}{dw_v} \right) = 0 \\
m: \quad & w_m U''_x \left(w_m \frac{dm^*}{dw_v} + w_v \frac{dv^*}{dw_v} + v^* - \frac{dd^*}{dw_v} \right) - U''_l \left(-\frac{dm^*}{dw_v} - \kappa \frac{dv^*}{dw_v} \right) = 0 \\
d: \quad & - U''_x \left(w_m \frac{dm^*}{dw_v} + w_v \frac{dv^*}{dw_v} + v^* - \frac{dd^*}{dw_v} \right) + U''_d \frac{dd^*}{dw_v} = 0
\end{aligned}$$

In matrix notation this becomes:

$$\begin{bmatrix}
w_v^2 U''_x + (1-\rho w_v)^2 U''_r + \kappa^2 U''_l & w_v w_m U''_x + \kappa U''_l & -w_v U''_x \\
w_m w_v U''_x + \kappa U''_l & w_m^2 U''_x + U''_l & -w_m U''_x \\
-w_v U''_x & -w_m U''_x & U''_x + U''_d
\end{bmatrix}
\begin{pmatrix} \frac{dv^*}{dw_v} \\ \frac{dm^*}{dw_v} \\ \frac{dd^*}{dw_v} \end{pmatrix} =$$
$$\begin{pmatrix}
-w_v v^* U''_x - U'_x + (1-\rho w_v)\rho v^* U''_r + \rho U'_r \\
-w_m v^* U''_x \\
v^* U''_x
\end{pmatrix}$$

Solving the resulting system of equations, we arrive at the comparative statics summarized in Table A1 that provide an overview of the marginal effect on the optimal levels of volunteering, labor, and donations from a change in w_v.

Table A1: Comparative statics

$$
\begin{array}{ll}
\frac{dv^*}{dw_v} = & \frac{1}{a}U_l''U_d''U_x''v^*\left(\kappa w_m - w_v\right) \\
& +\frac{1}{a}\Big(-U_x' + U_r'\rho + \left(1-\rho w_v\right)v^*U_r''\rho\Big)\Big(U_l''U_x'' + U_l''U_d'' + U_d''U_x''w_m^2\Big) \\
\\
\frac{dm^*}{dw_v} = & -\frac{1}{a}\Big(U_l''U_d''U_x''v^*\kappa\left(\kappa w_m - w_v\right) + \left(-U_x' + U_r'\rho\right)\left(U_l''U_x''\kappa + U_l''U_d''\kappa + U_d''U_x''w_m w_v\right)\Big) \\
& -\frac{1}{a}v^*(1-\rho w_v)U_r''\Big(w_m U_x''U_d'' + \rho\kappa U_l''(U_x'' + U_d'')\Big) \\
\\
\frac{dd^*}{dw_v} = & \frac{1}{a}\Big(U_l''U_x''\left(U_r''v^*\left(1-\rho w_v\right)\left(1-\rho\kappa w_m\right) - \left(-U_x' + U_r'\rho\right)\left(\kappa w_m - w_v\right)\right)\Big)
\end{array}
$$

Notes: $a = (w_v - \kappa w_m)^2U_x''U_d''U_l'' + (1-\rho w_v)^2U_r''(U_d''U_l'' + U_x''U_l'' + w_m^2U_d''U_x'') < 0.$

A.2 Solving for the critical volunteer discount rate

We now solve for the value of ρ for which the comparative static outcomes equal 0. To ensure internal consistency of the solutions, we still assume that ρ has values in the interval $[0, \frac{1}{w_v}]$ such that the solutions of ρ are not greater than ρ_{max}.

Volunteering: Solving for the value of ρ that gives the comparative static outcome $\frac{dv^*}{dw_v} = 0$, we set

$$
\begin{aligned}
0 = \quad & \frac{1}{a}U_l''U_d''U_x''v^*\left(\kappa w_m - w_v\right) \\
& + \frac{1}{a}\left(-U_x' + U_r'\rho + \left(1-\rho w_v\right)v^*U_r''\rho\right)\left(U_l''U_x'' + U_l''U_d'' + U_d''U_x''w_m^2\right).
\end{aligned}
$$

Expanding and rewriting in terms of ρ yields $A\rho^2 + B\rho + C = 0$, with

$$
\begin{aligned}
A = \quad & -w_v v^*U_r'' > 0 \\
B = \quad & \left(U_r' + v^*U_r''\right) \\
C = \quad & \left(\frac{U_l''U_d''U_x''v^*(\kappa w_m - w_v)}{(U_l''U_x'' + U_l''U_d'' + U_d''U_x''w_m^2)} - U_x'\right) < 0.
\end{aligned}
$$

Using the quadratic formula, we can solve for ρ. Our determinant D is defined as follows:

$$
D = \quad \left(U_r' + v^*U_r''\right)^2 + 4\left(w_v v^*U_r''\right)\left(\frac{U_l''U_d''U_x''v^*(\kappa w_m - w_v)}{U_l''U_x'' + U_l''U_d'' + U_d''U_x''w_m^2} - U_x'\right) > 0.
$$

Then since $\sqrt{D} > |B|$, only one of the two algebraic solutions satisfies the condition $\rho \geq 0$, such that our solution of interest is

$$
\rho^v_{critical} = \frac{-\left(U_r' + v^*U_r''\right) + \sqrt{D}}{2\left(-w_v v^*U_r''\right)}.
$$

Market labor supply: Solving for the value of ρ that gives the comparative static outcome $\frac{dm^*}{dw_v} = 0$, we set

$$\begin{aligned} 0 = \quad & -\frac{1}{a}\Big(U_l''U_d''U_x''v^*\kappa\left(\kappa w_m - w_v\right) + \left(-U_x' + U_r'\rho\right)\left(U_l''U_x''\kappa + U_l''U_d''\kappa + U_d''U_x''w_m w_v\right)\Big) \\ & -\frac{1}{a}\Big(v^*(1-\rho w_v)U_r''\left(w_m U_x''U_d'' + \rho\kappa U_l''(U_x'' + U_d'')\right)\Big). \end{aligned}$$

Expanding and rewriting in terms of ρ yields $A\rho^2 + B\rho + C = 0$, with

$$\begin{aligned} A = \quad & -w_v v^* U_r''\kappa U_l''(U_x'' + U_d'') > 0 \\ B = \quad & U_r'\left(U_l''U_x''\kappa + U_l''U_d''\kappa + U_d''U_x''w_m w_v\right) + v^*U_r''\left(\kappa U_l''(U_x'' + U_d'') - w_m w_v U_x''U_d''\right) \\ C = \quad & U_l''U_d''U_x''v^*\kappa(\kappa w_m - w_v) - U_x'\left(U_l''U_x''\kappa + U_l''U_d''\kappa + U_d''U_x''w_m w_v\right) \\ & + w_m v^* U_r''U_x''U_d'' < 0. \end{aligned}$$

Using the quadratic formula, we can solve for ρ. Our determinant D is defined as follows:

$$\begin{aligned} D = \quad & \left[U_r'\left(U_l''U_x''\kappa + U_l''U_d''\kappa + U_d''U_x''w_m w_v\right) + v^*U_r''\left(\kappa U_l''(U_x'' + U_d'') - w_m w_v U_x''U_d''\right)\right]^2 \\ & + 4\left(w_v v^* U_r''\kappa U_l''(U_x'' + U_d'')\right) \times \\ & \Big[U_l''U_d''U_x''v^*\kappa(\kappa w_m - w_v) - U_x'\left(U_l''U_x''\kappa + U_l''U_d''\kappa + U_d''U_x''w_m w_v\right) + w_m v^* U_r''U_x''U_d''\Big] \\ & > 0. \end{aligned}$$

Then since $\sqrt{D} > |B|$, only one of the two algebraic solutions satisfies the condition $\rho \geq 0$, such that our solution of interest is

$$\rho^m_{crit.} = \frac{-\left[U_r'\left(U_l''U_x''\kappa + U_l''U_d''\kappa + U_d''U_x''w_m w_v\right) + v^*U_r''\left(\kappa U_l''(U_x'' + U_d'') - w_m w_v U_x''U_d''\right)\right] + \sqrt{D}}{2\left(-w_v v^* U_r''\kappa U_l''(U_x'' + U_d'')\right)}.$$

Donations: Solving for the value of ρ that gives the comparative static outcome $\frac{dd^*}{dw_v} = 0$, we set

$$0 = \frac{1}{a}\left(U_r''v^*\left(1-\rho w_v\right)\left(1-\rho\kappa w_m\right) - \left(-U_x' + U_r'\rho\right)\left(\kappa w_m - w_v\right)\right).$$

Expanding and rewriting in terms of ρ yields $A\rho^2 + B\rho + C = 0$, with

$$\begin{aligned} A = \quad & U_r''v^*w_v\kappa w_m < 0 \\ B = \quad & -\left(U_r''v^*(\kappa w_m + w_v) + U_r'(\kappa w_m - w_v)\right) \\ C = \quad & \left(U_r''v^* + U_x'(\kappa w_m - w_v)\right). \end{aligned}$$

Using the quadratic formula, we can solve for ρ. Our determinant $D =$

$$\left(-\left(U_r''v^*(\kappa w_m + w_v) + U_r'(\kappa w_m - w_v)\right)\right)^2 - 4U_r''v^*w_v\kappa w_m\left(U_r''v^* + U_x'(\kappa w_m - w_v)\right).$$

$D > 0$ if and only if $B^2 > -4AC$, which we assume to be the case as well as $\sqrt{D} > |-B|$. In this case only one of the two algebraic solutions satisfies the condition $\rho \geq 0$, such that our solution of interest is

$$\rho^d_{critical} = \frac{\left(U_r''v^*(\kappa w_m + w_v) + U_r'(\kappa w_m - w_v)\right) - \sqrt{D}}{2\left(U_r''v^*w_v\kappa w_m\right)}.$$

B Institutional Appendix

In Germany, volunteers can receive small tax-free allowances for their work as general volunteer or instructor volunteer (see Figure 1). The 2013 reform increased both exemption thresholds, alongside minor administrative changes. While the main text outlines the economic context of these policies, this appendix mostly details how the allowances are reported and administered.

Declaration requirements: Volunteers are supposed to declare allowance income in their annual tax return, even when it is below the exemption threshold. This declaration occurs in a dedicated section of the return, separate from regular employment income. In practice, not all recipients declare exempt amounts, since no tax is due. If total allowance from a specific activity exceeds the exemption limit, the excess is taxable and reported together with other earned income. Such cases are rare because paying above the threshold would trigger social security contributions and income tax withholding for the organization.

Multiple activities: A person can receive both a general and an instructor volunteer allowance if the underlying activities are different. The tax return does not explicitly capture the number of organizations. Survey data from the FWS shows a strong positive relationship between allowance amount and number of activities performed (Appendix Table D5). This supports the idea that multi-role volunteers are more likely to approach the threshold.

Reimbursements vs. allowances: Out-of-pocket expenses/reimbursements are not supposed to be declared in the same field as the allowance in the tax return, even if received alongside allowances. Paid volunteers are more likely to have and exercise the option of having out-of-pocket expenses reimbursed (see Table D2). This suggests that reimbursement and volunteer compensation are rather complementary.

Comparison to market labor: Unlike Minijobs, which are typically part-time positions in retail, services, or hospitality and subject to employer-side social security contributions (Collischon et al., 2021), volunteer allowances are legally capped, tax-exempt (below thresholds), and tied to charitable or educational purposes. The low monetary value and activity-specific nature of volunteer allowances reinforce that they are structurally different from wages.

The 2013 Act to Strengthen Volunteering: The increases in both allowances were part of a broader legislative package, the *Gesetz zur Stärkung des Ehrenamtes* (2013). Beyond the allowance changes, this Act changed (i) the definition of liability of volunteer board members, (ii) the requirements for the formation and use of the organization's reserves (which have been relaxed), and increased (iii) the maximum revenues from sports events, and (iv) the deadline to issue receipts from donations.

C TPP Data Appendix

C.1 Variables definition

- *Volunteer duration* (spell length) is an ordinal variable, from one to twelve, that indicates consecutive periods of voluntary work for people reporting the volunteer allowance. If the allowance changes from one period to the next, we reset the count since an organizational switch might have occurred, indicating a spell end (excluding the old-max to new-max changes in the reform year 2013, which are likely related to the reform). If the person stops reporting the volunteer allowance, we consider it the end of volunteering. For people who do not volunteer, the variable is equal to zero.

- *Donations* is a continuous variable provided by the statistical office, which summarizes all contributions and donations of a reporting unit, following paragraph 10 of the *Einkommensteuergesetz* (2022).

- *Labor income* is a continuous variable that includes the overall income from employment, including income where the payroll tax has already been withheld. Gross labor income also includes income where payroll tax has not been withheld.

- *Age* is the difference between the reported year of birth and the year of the tax return.

- *Female* is an indicator equal to one if the individual is female, zero otherwise. For couples, the indicator is constructed based on the assumption of the statistical office that the first filer, person A, is typically the husband, while person B is the wife. For the variable's definition we take into account information from same-sex couples as well as female first filers when indicated in the data. For further information on the variable's construction, see for example this entry on the Taxpayer Panel Forum: forum.lifbi.de/t/identifikation-des-geschlechts-im-taxpayer-panel/4460/2.

- *Married* is an indicator equal to one if the individual is in a married couple or registered partnership, equal to zero otherwise. The indicator is derived from information on joint or individual filing for a married couple or registered partnership vis-á-vis singles or widow(er)s. This indicator also considers same-sex couples.

- Information about *religion* is mostly related to church taxes for some religious denominations in Germany. We can thus differentiate between Catholics and Protestants and create binary indicators for these religions. Information about other religions is not available.

- Types of income. A person is considered

  - *Employed* if they report labor income or if they report cross-border employment.
  - *Entrepreneur* if they report income as freelancer, sole proprietor, from commerce, or self-employment.
  - *Landlord* if they report income from rent and lease.
  - *Farmer* if they report income from forestry or agricultural activities.

- *Retired* is an indicator equal to one if the individual receives a state or private pension, equal to zero otherwise.

- *Receiving benefits* is an indicator equal to one if the person receives (taxable) benefits including unemployment benefits, parental allowance, maternity leave, short-time allowance; equal to zero otherwise. Unfortunately, given how many benefits are reported together in the same item of the return, it is not possible to clearly identify the source of or reason for the benefit.

- *In training or education* is equal to one if the person reports any expenses related to training or continuing educational activities, equal to zero otherwise.

- *Commuting effort* is a continuous variable that multiplies the number of days commuted with the distance in km (times two to account for the return journey).

- *No kids*, *Kids (<18 years old)*, and *Kids ($\geq$ 18 years old)* are indicators for individuals having no kids, minor kids (below age of 18), or kids above 18.

C.2 Sample derivation

An important premise is that the tax-return data are provided at the level of the filing unit, which might be an individual or a couple (made up by person A and person B, with or without dependent children). We follow the assumption of the Statistical Office that Person A is the husband and Person B is the wife, unless otherwise self-declared and adjusting for same-sex couples. While saving information on household composition and its members, we transform our data from the household to the individual level, following the subsequent steps.

1. Drop unusual returns as recommended in the documentation provided by the German Statistical Office (see "Hinweise zu LEST"). This procedure enables us to only consider "usual" tax returns filed by households in contrast to very specific tax-filing cases that would not contain the information relevant for our analysis, like loss assessments and savings allowance cases. In particular, we do not consider tax-return information only coming from the employer side and not filled in by households (available from 2012), as these do not contain self-reported volunteer allowances by construction.

2. Drop the 2010 ID duplicates for person A, an issue that has been identified by the German Statistical Office, which reported this issue for about 10 cases in 2010 with the same ID.

3. The data as delivered by the Statistical Office includes a unique ID at the filing-unit level. Individual IDs for persons A and B are only introduced from the reporting year 2010. To take the example of a married couple observed since 2007, this implies that they have missing individual IDs until 2010, but these can be easily carried backward within the individuals in the household. Similarly, we can carry backward the ID of single individuals that we observe before and after 2010. We otherwise create random IDs for individuals or couples exiting the panel before 2010. Following this logic and using multiple conditions, including the change in marital status, we harmonize the information for couples in which IDs are accidentally swapped between Persons A and B over time (this seems especially relevant for widow/ers).

4. Finally, after appending the panel at the individual level, we take care of few duplicate ID-year observations (less than 0.02% of the observations). The phenomenon of duplicated ID-year observations seems related to the pre-2012 period and mostly to changes in marital status (widow/ers, new couples or divorcées). We "limit our losses" by recoding the ID, considering changes in marital status and sex information.

Despite our attempts to carefully transform the data from the household to the individual level, the way the data is collected at source leads to some limitations, which cannot be easily overcome. For example, women getting married in 2010 are linked to the pre-existing husband record (i.e., person A appears single until 2010 and married afterwards). However, the fact that person A's and B's ID were only introduced in 2010 leads to the fact that we cannot follow the wife in the years prior to her marriage.

C.3 Probability of paid volunteering

In principle, the policy change affects all individuals who could be engaged in paid volunteering. Therefore, we use a relatively simple setup to quantify whether participation in the policy year 2013 (and the following post-policy period) shows a deviation from the participation level in the pre-policy years. We use a linear probability model (LPM) of the form

$$\Pr(V_{it} = 1) = \sum_{t \neq 2012} \mu_t + X_{it}\beta + \varepsilon_{it},$$

where $\Pr(V_{it} = 1)$ represents the probability that individual i is a paid volunteer (€1000-3200) in year t, while μ_t are time dummies that capture year-specific shocks relative to our baseline year 2012. The vector X_{it} includes an intercept term and control variables specific to individual i in year t with corresponding coefficients denoted by β.

Finally, ε_{it} is the error term with standard errors clustered at the individual level. In an alternative specification, we test for a break in linear time trends by including the continuous variable year_t and an interaction between time trends and the post-2013 period.

A limitation of this strategy is that we cannot claim that any estimated correlation between post-policy years and the outcome is exclusively attributable to the policy change of our interest. Other events correlated to the post-policy period that impact paid volunteering participation will also be reflected in μ. A potentially relevant confounding event is the introduction of other policies within the 2013's "Act to Strengthen Volunteering" as discussed in Section 3. The empirical DiD and matching strategies that we present in Section 5 overcome this limitation by comparing differentially affected groups. The same approach is not applicable for studying volunteer participation in our setting.

As a result, we believe our LPM estimates are meaningful to grasp whether there has been a break or a change in the existing level of instructor volunteer participation if we keep in mind that they might not perfectly reflect the effect of the increased tax-free volunteer compensation.

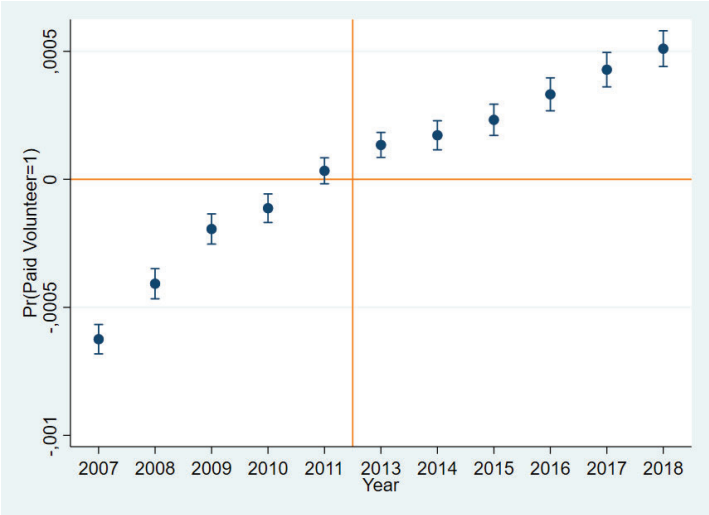
The results are shown in Table C1 and Figure C1 and are estimated out of the full sample, i.e., including never-volunteers and volunteers getting any positive allowance. From columns (1) and (2) of Table C1 we can see that, relative to 2012, the probability of being a paid volunteer increases in the years that follow, but the change is small. Columns (3) and (4) show that relative to the pre-policy period 2007-2012, in the years 2013-2018 people are 9-to-10% more likely to be paid volunteers. However, the slope actually marginally decreases in the aftermath of the policy. A similar picture is revealed by the different specifications reported in Figure C1.

Taken together, these findings suggest that the policy did not generate a strong shift in the extensive margin of paid volunteering. This lends support to our decision to focus the main analysis on the more relevant subpopulation of existing volunteers at the time of the policy change. If there had been a sharp upward break in the volunteer participation trend, our approach that ignores new volunteers would risk missing important spillover effects and would raise concerns about external and internal validity.

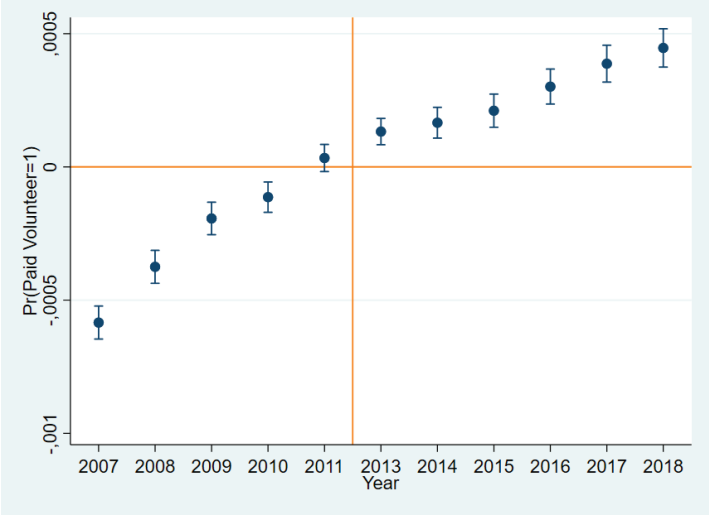
C.4 Results TPP 5% sample

Source: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, DOI: 10.21242/73111.2018.00.01.2.1.1, own calculations.

(a) Yearly effects



(b) Yearly effects and individual FE



(c) Yearly effects and time trends

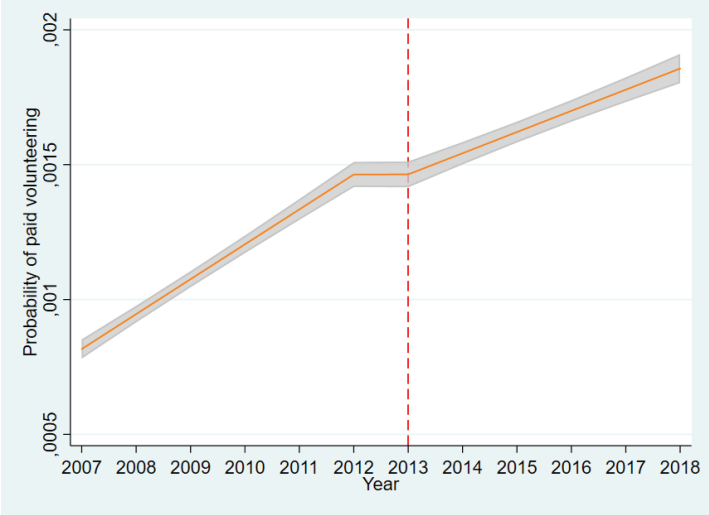


Figure C1: Probability of being a paid volunteer with a €1000-3200 allowance

Notes: Standard errors clustered at the individual level. Omitted year for panels (a) and (b) is 2012.

Table C1: Allowance increase effects on the probability to volunteer.

	(1)	(2)	(3)	(4)
2007	-0.001***	-0.001***		
	(0.000)	(0.000)		
2008	-0.000***	-0.000***		
	(0.000)	(0.000)		
2009	-0.000***	-0.000***		
	(0.000)	(0.000)		
2010	-0.000***	-0.000		
	(0.000)	(0.000)		
2011	0.000	0.000***		
	(0.000)	(0.000)		
2013	0.000***	0.000***		
	(0.000)	(0.000)		
2014	0.000***	0.000***		
	(0.000)	(0.000)		
2015	0.000***	0.000***		
	(0.000)	(0.000)		
2016	0.000***	0.000***		
	(0.000)	(0.000)		
2017	0.000***	0.000***		
	(0.000)	(0.000)		
2018	0.001***	0.001***		
	(0.000)	(0.000)		
post			0.102***	0.092***
			(0.019)	(0.020)
year			0.000***	0.000***
			(0.000)	(0.000)
post × year			-0.000***	-0.000***
			(0.000)	(0.000)
Controls	No	Yes	No	Yes
Observations	25,225,593	25,221,466	25,225,593	25,221,466

Notes: The outcome is a dummy for reporting allowance in the range €1000-3200 in a particular calendar year. Post is an indicator for the years from 2013, year is included as a time trend. Controls include gender, marital status, indicators for no kids or having kids below age of 18, age, age squared, employment status, earnings source, religion, commuting effort, federal state fixed effects. Standard errors clustered at the individual level in parentheses. $^{\sim}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C2: Descriptive statistics matched paid volunteers, 2012

	Earns €2100 in 2012 and in 2013		Earns €2100 in 2012 €2400 in 2013	
	Mean	SD	Mean	SD
Female	0.45	(0.50)	0.47	(0.50)
Age	49.29	(8.60)	50.33	(9.87)
Married	0.78	(0.42)	0.75	(0.43)
East	0.40	(0.49)	0.29	(0.46)
No kids	0.40	(0.49)	0.44	(0.50)
Kids (<18 y.o.)	0.48	(0.50)	0.38	(0.49)
Kids ($\geq$ 18 y.o.)	0.12	(0.33)	0.18	(0.39)
Protestant	0.38	(0.49)	0.34	(0.48)
Catholic	0.34	(0.48)	0.41	(0.49)
Other/not religious	0.27	(0.45)	0.24	(0.43)
Spell length	2.21	(1.41)	2.31	(1.48)
Labor income	54695	(72284)	45598	(65527)
Donations	1173	(1612)	923	(1337)
Employed	0.86	(0.35)	0.90	(0.30)
Entrepreneur	0.40	(0.49)	0.40	(0.49)
Landlord	0.14	(0.35)	0.18	(0.38)
Retired	0.07	(0.25)	0.12	(0.32)
Receives benefits	0.08	(0.28)	0.05	(0.22)
Commuting effort	3886	(5927)	3330	(5552)
Observations	73		177	

Notes: Unweighted statistics for the matched sample. For the control variables, sample means are never statistically different for the two groups. The means further converge once matching weights are taken into account (material available upon request). Variable derivations are thoroughly explained in Section C.1 in Appendix C. The variables Labor income and Donations are winsorized at the 3% level. Just for the statistics reported in this table but not for their further role as controls and in robustness checks, the continuous variables Age, Spell length, and Commuting effort are also winsorized on the 3% level. Commuting effort represents the (return) distance traveled to work per year.

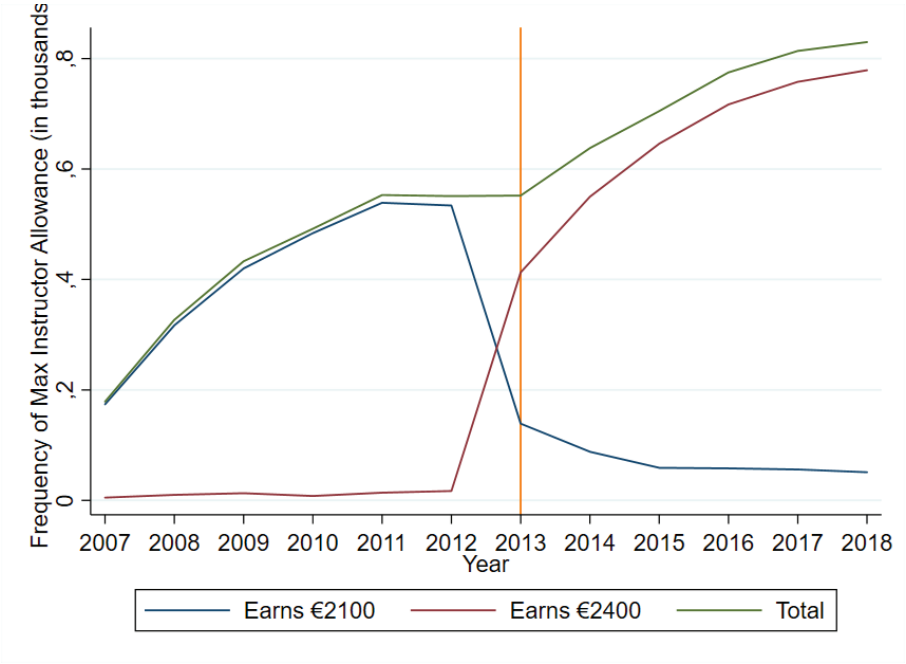


Figure C2: Evolution of the maximum-paid instructor volunteers

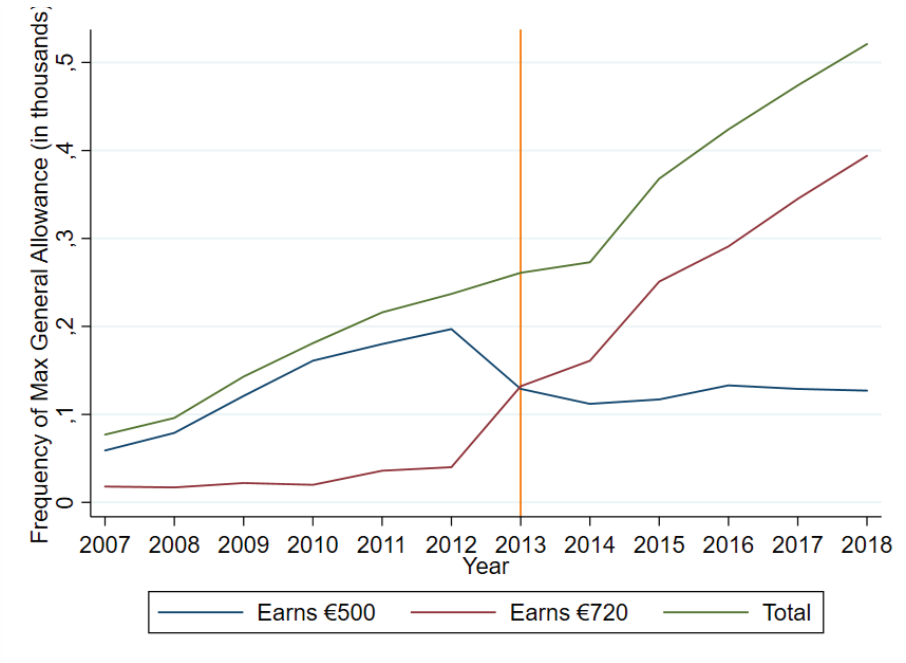


Figure C3: Evolution of the maximum-paid general volunteers

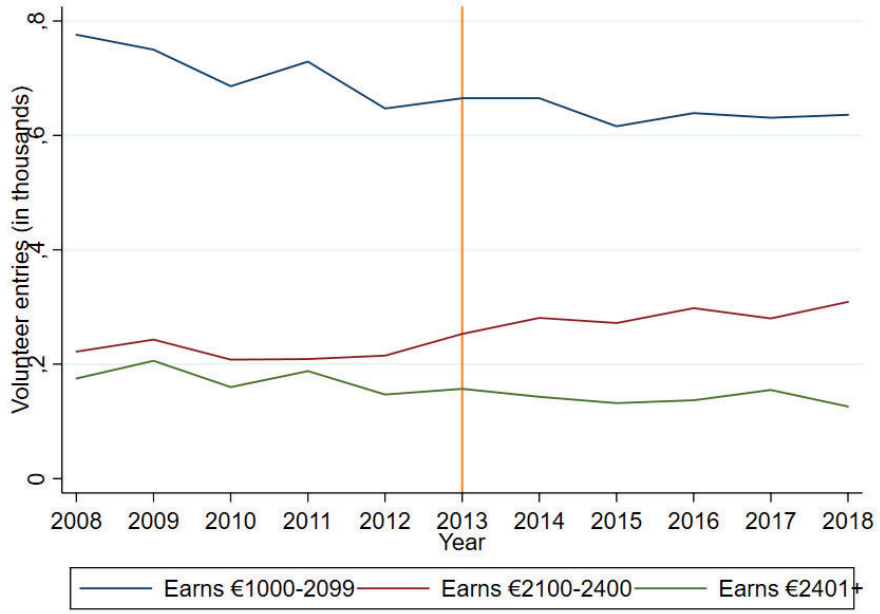


Figure C4: Number of first-ever entry into paid volunteering

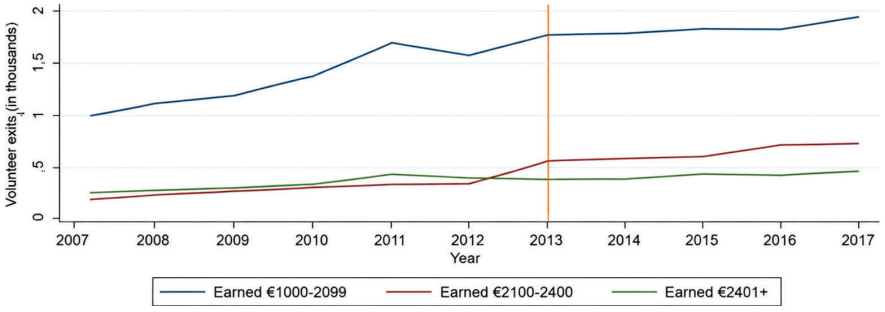
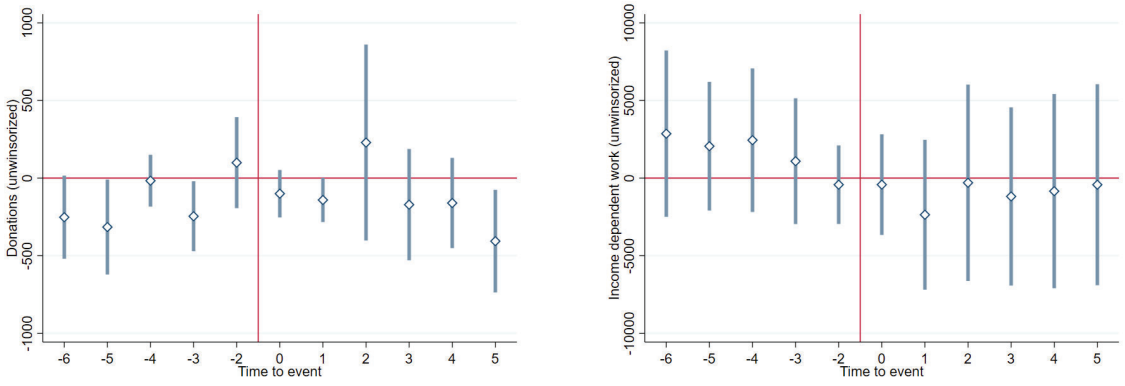


Figure C5: Spell exits by volunteer type

Table C3: Effect of the allowance increase on volunteer duration split by East/West, income source, and household income quartiles

	East	West	Labor	Capital	Retirement
Panel A					
Max	0.670***	0.671***	0.656***	0.771**	0.634***
	(0.027)	(0.019)	(0.018)	(0.076)	(0.061)
Max × Post	1.020	1.151	1.158~	0.878	0.844
	(0.138)	(0.112)	(0.102)	(0.355)	(0.221)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	3321	5734	7103	472	565
	Quart. (1)	Quart. (2)	Quart. (3)	Quart. (4)	
Panel B					
Max	0.681***	0.667***	0.701***	0.606***	
	(0.041)	(0.029)	(0.026)	(0.032)	
Max × Post	1.245	1.285~	1.049	0.945	
	(0.267)	(0.192)	(0.144)	(0.150)	
Controls	Yes	Yes	Yes	Yes	
Observations	1114	2192	3399	2350	

Notes: Exponentiated coefficients. Income quartiles are defined on the basis of the whole sample before pre-selecting on individuals in the control or treatment groups, hence the uneven distribution after. Controls consist of dummies for female, retirement, receiving benefits, earning income mainly from agriculture, real estate, self-employment, or employment, being married, having no kids, having a kid below 18 y/o, being protestant, or catholic. Birth year and a proxy for commuting effort are included as continuous control variables. Standard errors in parentheses are clustered at the individual level. ~ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.



(a) Donations (b) Labor income

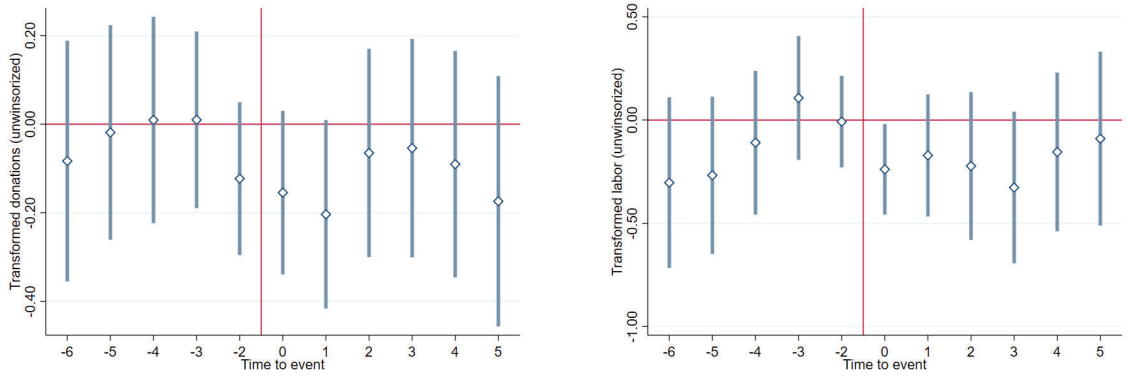
Figure C6: Event-study representation of the effects of the increased volunteer allowance, unwinsorized outcome variables

Notes: The excluded period is -1, the pre-reform year 2012.

Table C4: Effect of the allowance increase on volunteer duration interacted with gender, religion and having minor children

	(1)	(2)	(3)
Max	0.671***	0.684***	0.684***
	(0.016)	(0.016)	(0.016)
Max × Post	1.166	1.109	1.062
	(0.112)	(0.096)	(0.141)
Max × Post × Woman	0.907		
	(0.106)		
Max × Post × Kids (<18 y.o.)		0.988	
		(0.128)	
Max × Post × Christian			1.054
			(0.146)
Woman	1.003	0.973*	0.973*
	(0.012)	(0.013)	(0.013)
Kids (<18 y.o.)	1.006	1.003	1.002
	(0.019)	(0.020)	(0.020)
Christian	1.008	1.011	1.010
	(0.013)	(0.014)	(0.013)
Observations	9055	9467	9467

Notes: Exponentiated coefficients. Controls consist of dummies for female, retirement, receiving benefits, earning income mainly from agriculture, real estate, self-employment, or employment, being married, having no kids, having a kid below 18 y/o, being protestant, or catholic. Birth year and a proxy for commuting effort are included as continuous control variables. Standard errors in parentheses are clustered at the individual level. ~ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.



(a) Donations

(b) Labor income

Figure C7: Event-study representation of the effects of the increased volunteer allowance, transformed outcomes

Notes: Inverse hyperbolic sine transformation applied to outcome variables. The excluded period is -1, the pre-reform year 2012.

Table C5: DDD estimate of effect allowance increase on volunteer duration

	(1) Main (2010v2012)	(2) 2009v2012	(3) 2011v2012
Max	0.687***	0.666***	0.687***
	(0.021)	(0.025)	(0.018)
Post	1.261**	1.221~	1.104
	(0.108)	(0.125)	(0.090)
Max × Post	1.157	0.996	1.273*
	(0.117)	(0.108)	(0.145)
Max × 2012 cutoff	0.997	1.027	0.996
	(0.024)	(0.035)	(0.016)
Post × 2012 cutoff	0.934	0.774	1.139
	(0.130)	(0.152)	(0.122)
Max × Post × 2012 cutoff	1.043	1.112	0.955
	(0.141)	(0.155)	(0.146)
Observations	14397	12894	15880

Notes: Exponentiated coefficients. The alternative cutoff years to 2012 are displayed under the column numbering. Controls included in all specifications consist of birth year (continuous) and dummies for being retired, receiving benefits, and earning income mainly from agriculture, real estate, self-employment, or employment, dummies for being married, having no kids, or having a kid below 18 y/o, dummies for protestant and catholic, gender. Stratification is on the sub-sample level, e.g., in column 1 on whether the year 2010 or 2012 is taken as the cutoff. Standard errors in parentheses are clustered at the individual level. ~ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C6: Probability of ending the volunteer spell, linear probability model

	(1)	(2)
Max	0.676***	0.673***
	(0.016)	(0.016)
Max × 2013	1.524***	1.498***
	(0.092)	(0.090)
2013	0.770***	0.786***
	(0.021)	(0.022)
Controls	No	Yes
Observations	2751	2751

Notes: Linear coefficient estimates are transformed to exponentiated coefficients for easy comparison with the hazard model estimates. Columns (1) and (2) show results for the two years around the reform, i.e., 2012 and 2013. Controls consist of birth year (continuous) and dummies for being retired, receiving benefits, and earning income mainly from agriculture, real estate, self-employment, or employment, dummies for being married, having no kids, or having a kid below 18 y/o, dummies for protestant and catholic, gender. Standard errors in parentheses clustered at the individual level. ~ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C7: Effect of the allowance increase on volunteer duration, robustness checks.

	(1)	(2)	(3)	(4)
Panel A				
Max	0.685***	0.687***	0.683***	
	(0.016)	(0.016)	(0.016)	
Max × Post	1.109	1.169~	1.096	
	(0.088)	(0.093)	(0.086)	
Controls	No	No	Yes	
Observations	9467	9467	9467	
Panel B				
Max	0.684***	0.679***	0.683***	0.666***
	(0.016)	(0.016)	(0.016)	(0.018)
Max × Post	1.114	1.138	1.124	1.135
	(0.091)	(0.092)	(0.089)	(0.103)
Controls	No	No	No	No
Observations	8370	8985	9326	6677
Panel C				
Max	0.684***	0.673***	0.667***	0.685***
	(0.016)	(0.018)	(0.017)	(0.016)
Max × Post	1.099	1.152~	1.107	1.109
	(0.087)	(0.097)	(0.092)	(0.087)
Controls	No	No	No	No
Observations	9342	8448	8370	9467

Notes: Panel A column (1) standard errors clustered at household level, (2) strata by start year of volunteering, (3) includes federal state of residence fixed effects. *Panel B* column (1) drops individuals exiting panel before 2018, (2) excludes individuals entering the panel after 2007 with current volunteering spell in first period, (3) excludes individuals younger than 30 in the first observed year, (4) includes only the balanced panel. *Panel C* column (1) excludes individuals who might be donating back the allowance, (2) excludes individuals with an excess, taxable allowance, (3) drops individuals who move, (4) excludes late adopters of allowance increase. Controls consist of dummies for female, retirement, receiving benefits, earning income mainly from agriculture, real estate, self-employment, or employment, being married, having no kids, having a kid below 18 y/o, and being Christian. Birth year and a proxy for commuting effort are included as continuous control variables. Standard errors in parentheses clustered at the individual level if not otherwise stated. ~ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C8: Effect of the allowance increase on volunteer duration, multiple-spell analysis.

	(1)	(2)
Max	0.892**	0.896**
	(0.036)	(0.037)
Max × Post	0.942	0.941
	(0.045)	(0.045)
Controls	No	Yes
Observations	7008	7008

Notes: All specifications include strata at the individual level. Controls consist of dummies for being retired, receiving benefits, married, having no kids, having a kid below 18 y/o, earning income mainly from agriculture, real estate, self-employment, or employment. We also include a continuous proxy for commuting effort. Robust standard errors in parentheses. ~ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

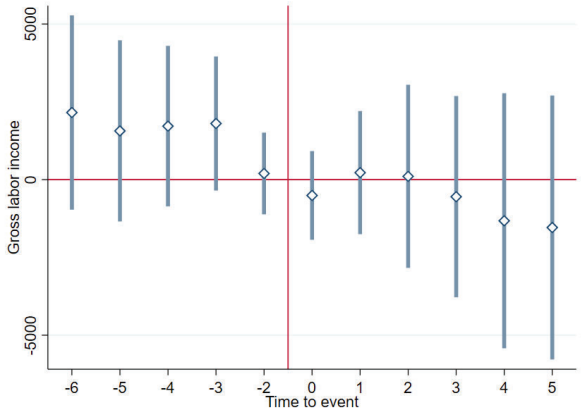


Figure C8: Event-study representation of the effects of the increased volunteer allowance on gross labor income

Notes: The excluded period is -1, the pre-reform year 2012.

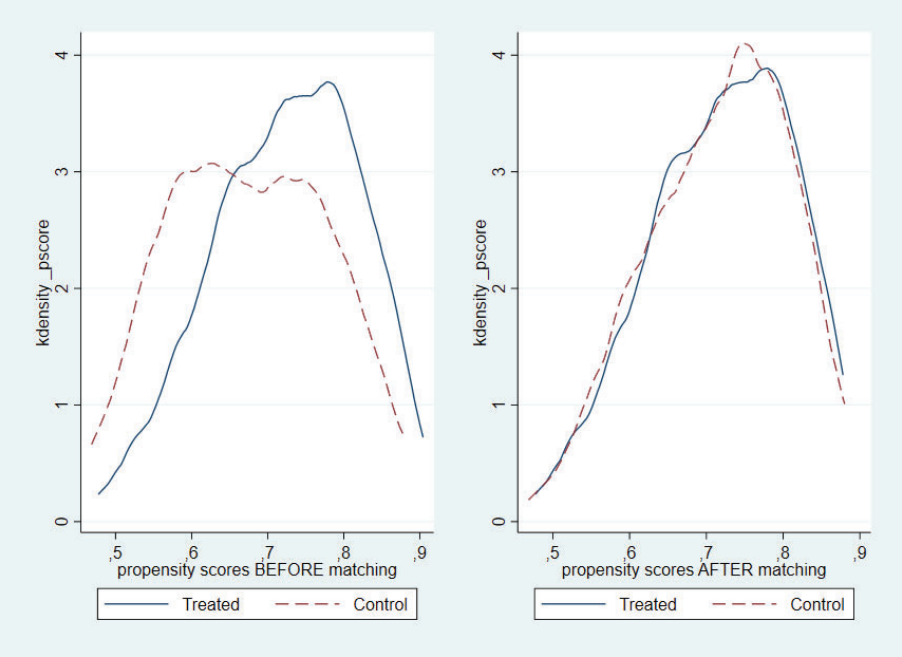
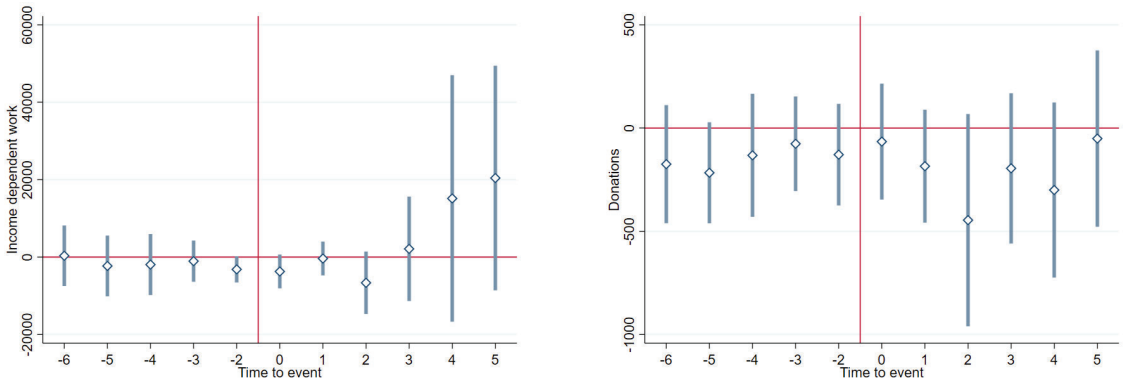


Figure C9: Distribution of the propensity score before and after matching

Notes: Treated indicates volunteers receiving the maximum allowance in 2012 and switching to the new maximum allowance in 2013. Control indicates volunteers receiving the maximum allowance in 2012 and remaining at that level also in 2013.



(a) Labor income

(b) Donations

Figure C10: Event-study representation of the effects of the increased volunteer allowance based on coarsened exact matching strategy

Notes: The excluded period is -1, the pre-reform year 2012. Labor income and donations are winsorized at the 3% level.

Table C9: Allowance increase effects on labor and donations with group fixed effects (GFE)

	Labor income		Donations	
	(1)	(2)	(3)	(4)
Max × 2013	-975.072	-300.847	-30.957	-20.066
	(1167.095)	(1228.929)	(38.296)	(38.663)
Max × 2014	-382.197	451.746	-46.868	-37.784
	(1379.436)	(1430.321)	(38.207)	(38.746)
Max × 2015	-189.399	385.598	-14.651	-2.018
	(1767.614)	(1750.580)	(49.099)	(49.335)
Max × 2016	-706.411	794.573	-45.577	-19.178
	(1969.570)	(1888.358)	(50.451)	(50.537)
Max × 2017	-1053.220	-139.453	-32.291	-15.074
	(2355.291)	(2188.085)	(52.172)	(52.353)
Max × 2018	-1295.757	-969.323	-47.795	-32.844
	(2418.375)	(2240.192)	(54.935)	(55.351)
Max group	-7557.834*	-10656.97***	205.917***	192.720***
	(3165.665)	(2625.187)	(58.968)	(57.279)
Controls	No	Yes	No	Yes
Observations	25514	25514	25514	25514

Notes: Labor income and donations are winsorized at the 3% level. Controls consist of dummies for being retired, receiving benefits, protestant, female, catholic, married, having no kids, having a kid below 18 y/o, earning income mainly from agriculture, real estate, self-employment, or employment. We also include federal state fixed effects, a continuous proxy for commuting effort, and birth year. Standard errors in parentheses clustered at the individual level. $^{\sim}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C10: Allowance increase effects on labor and donations interacted with gender and religion.

	Labor income		Donations	
	(1)	(2)	(3)	(4)
Panel A				
Max × 2013 × Woman	-64.641	-378.442	-8.679	-9.714
	(2089.237)	(2029.759)	(75.075)	(75.139)
Max × 2014 × Woman	-886.563	-467.098	-2.326	-3.317
	(2458.975)	(2351.181)	(74.311)	(74.349)
Max × 2015 × Woman	96.334	-128.022	10.264	16.093
	(3067.773)	(2861.721)	(93.445)	(93.271)
Max × 2016 × Woman	-364.945	851.610	71.906	76.198
	(3413.141)	(3180.886)	(96.398)	(95.963)
Max × 2017 × Woman	985.917	2006.681	56.290	60.251
	(4125.893)	(3827.136)	(99.601)	(99.228)
Max × 2018 × Woman	279.620	1790.350	44.276	47.373
	(4243.092)	(3811.549)	(103.954)	(103.610)
Panel B				
Max × 2013 × Christian	-969.074	-1880.444	-87.589	-73.466
	(2429.195)	(2379.297)	(90.510)	(90.044)
Max × 2014 × Christian	-1228.123	-1204.061	18.925	29.524
	(2822.704)	(2672.495)	(76.613)	(76.752)
Max × 2015 × Christian	-1187.413	-1264.212	33.760	45.291
	(3626.830)	(3291.421)	(103.963)	(102.845)
Max × 2016 × Christian	-1339.642	-1236.129	17.157	29.832
	(4019.656)	(3693.441)	(113.654)	(112.480)
Max × 2017 × Christian	-4305.294	-3518.713	-20.876	-14.406
	(4831.336)	(4463.704)	(111.869)	(111.084)
Max × 2018 × Christian	-3645.512	-3777.965	-79.112	-73.951
	(4914.956)	(4414.720)	(117.156)	(116.179)
Controls	No	Yes	No	Yes
Observations	25512	25512	25512	25512

Notes: Labor income and donations are winsorized on the 3% level. Controls consist of dummies for being retired, receiving benefits, protestant, catholic, married, having no kids, having a kid below 18 y/o, earning income mainly from agriculture, real estate, self-employment, or employment. We also include federal state fixed effects, a continuous proxy for commuting effort. Standard errors clustered at the individual level in parentheses. $^{\sim}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C11: Allowance increase effects on labor and donations specifically for individuals with non-adult children.

	Labor income		Donations	
	(1)	(2)	(3)	(4)
Max × 2013	-365.728	-123.853	-64.404	-61.553
	(1296.424)	(1237.045)	(46.022)	(45.558)
Max × 2014	462.620	1060.784	-76.973~	-77.617~
	(1517.365)	(1432.743)	(46.071)	(45.945)
Max × 2015	-1478.368	-1265.023	-105.115~	-104.985~
	(1981.625)	(1801.004)	(56.253)	(56.219)
Max × 2016	-1322.312	-950.191	-90.819	-94.019
	(2222.355)	(2048.256)	(57.057)	(57.291)
Max × 2017	-2635.214	-3071.644	-90.478	-93.279
	(2860.697)	(2641.475)	(60.604)	(61.178)
Max × 2018	-3521.232	-3696.605	-57.045	-62.012
	(2835.460)	(2472.847)	(64.603)	(64.761)
Max × 2013 × Kids (<18 y.o.)	-3558.241	-2563.100	125.126	122.503
	(2323.722)	(2249.376)	(78.716)	(78.675)
Max × 2014 × Kids (<18 y.o.)	-4059.463	-4082.572	116.680	121.156
	(2749.132)	(2626.065)	(76.707)	(76.564)
Max × 2015 × Kids (<18 y.o.)	10.746	109.815	257.278**	259.910**
	(3405.451)	(3246.222)	(99.022)	(98.545)
Max × 2016 × Kids (<18 y.o.)	-1860.933	-2010.341	155.921	165.536
	(3811.494)	(3610.447)	(102.207)	(101.957)
Max × 2017 × Kids (<18 y.o.)	-892.385	34.295	167.946	176.510~
	(4565.578)	(4335.956)	(103.751)	(103.921)
Max × 2018 × Kids (<18 y.o.)	270.962	-42.389	51.662	65.526
	(4664.625)	(4343.287)	(109.532)	(109.599)
Controls	No	Yes	No	Yes
Observations	25512	25512	25512	25512

Notes: Labor income and donations are winsorized on the 3% level. Controls consist of dummies for being retired, receiving benefits, protestant, catholic, married, having no kids, having a kid below 18 y/o, earning income mainly from agriculture, real estate, self-employment, or employment. We also include federal state fixed effects, a continuous proxy for commuting effort. Standard errors clustered at the individual level in parentheses. ~ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C12: Allowance increase effects on labor and donations split by income source.

	Labor	Capital	Retirement	Labor	Capital	Retirement
		Labor income			Donations	
Max × 2013	-1398.103	-256.136	-125.143	7.990	101.248	-106.055
	(1240.558)	(2265.275)	(1368.389)	(42.639)	(224.550)	(153.308)
Max × 2014	-680.570	-1710.510	829.763	16.118	-109.454	-129.690
	(1456.684)	(2827.978)	(1441.164)	(40.726)	(197.071)	(167.761)
Max × 2015	-962.233	-1962.461	178.806	24.764	227.615	-262.389
	(1808.923)	(3115.969)	(1545.595)	(47.461)	(394.610)	(199.523)
Max × 2016	-1888.975	-1839.665	-45.730	-22.706	487.905	-107.282
	(2025.632)	(2840.977)	(1510.980)	(50.573)	(368.219)	(192.702)
Max × 2017	-3510.431	-1859.172	824.227	9.146	136.711	-206.267
	(2494.092)	(3062.580)	(1468.546)	(50.673)	(433.312)	(218.422)
Max × 2018	-3896.518	55.161	872.078	-20.115	345.429	-133.389
	(2442.177)	(3321.189)	(1576.796)	(53.444)	(453.530)	(205.848)
Observations	20247	1243	1439	20247	1243	1439

Labor income and donations are winsorized on the 3% level.

Standard errors clustered at the individual level in parentheses.

All models are estimated with individual fixed effects and control variables.

$^{\sim}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table C13: Allowance increase effects on labor and donations split by East- and West-Germany.

	West	East	West	East
	Labor income		Donations	
Max × 2013	-2111.679	556.459	-30.125	31.811
	(1440.108)	(1459.544)	(49.317)	(58.887)
Max × 2014	-593.024	-364.847	-49.642	31.086
	(1645.073)	(1844.946)	(46.795)	(62.688)
Max × 2015	-622.480	-1510.310	12.012	2.235
	(1987.120)	(2377.857)	(64.385)	(67.322)
Max × 2016	-2007.783	-735.368	-45.694	36.859
	(2195.989)	(2679.395)	(65.252)	(71.535)
Max × 2017	-2888.003	-2583.368	-15.634	-6.920
	(2769.026)	(2904.648)	(64.083)	(82.694)
Max × 2018	-4129.812	-1519.745	-11.862	-71.093
	(2734.576)	(2892.545)	(67.981)	(85.220)
Observations	16479	9031	16479	9033

Notes: Labor income and donations are winsorized on the 3% level. Controls consist of dummies for being retired, receiving benefits, protestant, catholic, married, having no kids, having a kid below 18 y/o, earning income mainly from agriculture, real estate, self-employment, or employment. We also include federal state fixed effects, a continuous proxy for commuting effort. Standard errors clustered at the individual level in parentheses. $^{\sim}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C14: Allowance increase effects on labor and donations, split by household income quartiles.

	Quart. (1)	Quart. (2)	Quart. (3)	Quart. (4)
Panel A: Labor income				
Max × 2013	-2221.774	-159.664	-426.856	-1722.432
	(2942.859)	(744.308)	(1482.965)	(3079.145)
Max × 2014	-655.284	855.543	38.507	-3502.450
	(3255.614)	(930.999)	(1872.974)	(3533.941)
Max × 2015	-1907.302	380.948	3442.168	-1.05e+04*
	(3189.101)	(1212.682)	(2302.391)	(4533.071)
Max × 2016	-2920.389	1543.794	2562.443	-1.19e+04*
	(3332.262)	(1273.971)	(2457.353)	(5279.030)
Max × 2017	-3633.288	1264.597	2979.564	-1.75e+04**
	(3527.377)	(1328.958)	(2920.815)	(6496.251)
Max × 2018	-5262.475	2362.028	1779.225	-1.79e+04**
	(3686.464)	(1478.006)	(3100.860)	(5870.104)
Panel B: Donations				
Max × 2013	-35.451	55.803	64.618	-163.319
	(94.093)	(42.457)	(62.417)	(102.409)
Max × 2014	-121.440	48.951	42.251	-141.388
	(90.960)	(49.696)	(61.917)	(95.843)
Max × 2015	-104.018	33.620	55.550	-9.448
	(119.560)	(60.702)	(75.827)	(126.130)
Max × 2016	13.256	20.052	58.516	-171.721
	(105.830)	(59.690)	(84.651)	(126.162)
Max × 2017	-66.999	38.344	16.746	-73.721
	(125.697)	(71.817)	(79.301)	(131.750)
Max × 2018	16.561	66.995	-69.980	-68.386
	(125.869)	(73.541)	(83.911)	(138.391)
Observations	3132	5898	9928	6554

Notes: All models are estimated with individual fixed effects and control variables. Quartiles are defined on the basis of the whole sample before pre-selecting on individuals in the control or treatment groups, hence the uneven distribution after. Labor income and donations are winsorized on the 3% level. Standard errors clustered at the individual level in parentheses. $^{\sim}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C15: Allowance increase effects on labor and donations, robustness checks.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Max × 2013	-1673.467	-1167.126	-1163.728	-6.839	-1988.893~	-9.685
	(1132.860)	(1086.440)	(1132.998)	(42.401)	(1069.948)	(39.797)
Max × 2014	-927.614	-599.612	-561.257	-23.014	-1299.478	-23.070
	(1361.716)	(1296.245)	(1329.216)	(41.798)	(1187.332)	(39.914)
Max × 2015	-1033.109	-1096.872	-1049.615	8.525	-1732.774	7.205
	(1698.418)	(1573.939)	(1630.956)	(52.772)	(1627.026)	(50.610)
Max × 2016	-1700.912	-1760.275	-1679.744	-18.645	-2567.758	-23.844
	(1894.874)	(1744.767)	(1808.141)	(54.145)	(1792.097)	(51.307)
Max × 2017	-2564.696	-3072.810	-2880.399	-13.804	-3766.169~	-22.939
	(2338.290)	(2154.565)	(2197.901)	(55.273)	(2110.480)	(52.881)
Max × 2018	-2682.154	-3529.392~	-3438.756	-30.491	-4666.739*	-39.638
	(2356.342)	(2096.709)	(2179.072)	(58.066)	(2170.986)	(58.524)
Controls	No	Yes	Yes	Yes	Yes	Yes
Observations	25512	25512	25512	25512	25314	25314
Panel B						
Max × 2013	-1068.831	-11.184	-479.078	9.813	-1789.121~	-14.479
	(1098.273)	(38.440)	(1188.997)	(43.831)	(1054.974)	(40.991)
Max × 2014	-498.617	-16.198	240.161	-34.997	-646.184	-32.719
	(1288.099)	(37.268)	(1359.230)	(41.467)	(1272.222)	(40.726)
Max × 2015	-1067.379	16.649	-10.706	20.566	-1523.263	11.380
	(1577.183)	(48.245)	(1643.058)	(55.348)	(1573.650)	(53.122)
Max × 2016	-1711.123	-12.077	-702.377	-15.087	-2088.906	-22.224
	(1752.109)	(49.440)	(1801.767)	(56.212)	(1741.125)	(54.574)
Max × 2017	-2943.530	-2.820	-1549.825	-0.904	-3730.182~	-26.086
	(2130.232)	(50.920)	(2136.623)	(57.886)	(2102.452)	(55.838)
Max × 2018	-3307.487	-17.480	-2649.697	-4.626	-4734.901*	-41.651
	(2107.075)	(53.242)	(2172.854)	(60.937)	(2119.515)	(57.628)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	25223	25223	23096	23096	22377	22377

Notes: Panel A, columns (1), without controls, and (2), with controls, show the results for gross labor income instead of our normal labor income measure; columns (3) and (4) show results for labor income and donations, respectively, where standard errors are clustered at the household level; columns (5) and (6) show results for labor income and donations, respectively, where standard errors are clustered at the municipality level and municipality fixed effects are included. *Panel B* shows results for labor income in columns (1), (3), and (5) and for donations in (2), (4), and (6). Columns (1) and (2) exclude individuals who might be donating back the allowance, columns (3) and (4) focus on individuals without a taxable allowance, while columns (5) and (6) focus only on individuals who have not moved between municipalities during the sample period. Results are relative to the baseline period 2007-12. All specifications include year and individual fixed effects. Labor income and donations are winsorized on the 3% level. Control variables include marital status, indicators for no kids or having kids below age of 18, employment status, earnings source, religion, commuting effort, federal state fixed effects. Standard errors clustered at the individual level in parentheses if not stated otherwise. ~ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C16: Allowance increase effects on labor and donations, short-term and average results.

	Labor income		Donations	
	(1)	(2)	(3)	(4)
Panel A				
Max × 2013	-528.273	-234.901	-67.492~	-64.669~
	(715.970)	(692.733)	(38.598)	(39.044)
Controls	No	Yes	No	Yes
Observations	4518	4518	4518	4518
Panel B				
Max × Post	-1642.509	-1761.041	-14.290	-13.870
	(1505.636)	(1370.208)	(37.064)	(37.046)
Controls	No	Yes	No	Yes
Observations	25512	25512	25512	25512

Notes: Panel A shows short-term results based on one year before and one after the reform, for the outcome variables specified in the header. *Panel B* shows average treatment effects for the whole period (2007-18) for the outcome variables specified in the header. All specifications include year and individual fixed effects. Labor income and donations are winsorized on the 3% level. Controls include marital status, indicators for no kids or having kids below age of 18, employment status, earnings source, religion, commuting effort, federal state fixed effects. Standard errors clustered at the individual level in parentheses. ~ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C17: Allowance increase effects on labor and donations for matched volunteers.

	Labor income		Donations	
	(1)	(2)	(3)	(4)
Increased allowance × 2013	-3776.057	-2554.534	-17.554	-9.474
	(4083.508)	(3443.371)	(121.001)	(123.254)
Increased allowance × 2014	920.270	894.359	-4.841	-12.342
	(3158.271)	(2900.656)	(95.300)	(98.169)
Increased allowance × 2015	-5998.411	-6132.434	-104.081	-101.136
	(3966.245)	(3890.815)	(144.367)	(148.573)
Increased allowance × 2016	-166.966	-186.983	35.454	38.872
	(4472.085)	(4302.396)	(152.481)	(155.370)
Increased allowance × 2017	6393.534	6829.216	-42.378	-44.302
	(6482.716)	(5924.741)	(128.251)	(134.287)
Increased allowance × 2018	8875.362	10132.202~	107.913	97.786
	(6232.698)	(5468.371)	(157.489)	(162.407)
Married		-3154.829		92.604
		(5949.918)		(100.624)
Employed		11957.753***		-142.098~
		(3236.078)		(84.072)
No kids		-7185.231**		-62.718
		(2444.730)		(91.563)
Kids (<18 y.o.)		-5737.620~		-96.003
		(3315.455)		(73.248)
Commuting effort		0.496*		0.002
		(0.200)		(0.005)
Controls	No	Yes	No	Yes
Observations	2784	2784	2784	2784

Notes: Results are relative to the baseline period 2007-12. All specifications include year and individual fixed effects. Labor income and donations are winsorized on the 3% level. Controls include marital status, indicators for no kids or having kids below age of 18, employment status, earnings source, religion, commuting effort, federal state fixed effects. Standard errors clustered at the individual level in parentheses. ~ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C18: Allowance increase effects on labor and donations for matched volunteers, robustness checks.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Incr. allowance × 2013	-3882.118	-2560.884	-2554.534	-9.474	-2138.097	-4.686
	(4135.652)	(3463.603)	(3641.641)	(134.545)	(3290.475)	(106.727)
Incr. allowance × 2014	790.009	941.706	894.359	-12.342	1487.234	-5.453
	(3189.862)	(2934.547)	(3051.561)	(110.048)	(3098.639)	(107.373)
Incr. allowance × 2015	-6136.354	-6109.800	-6132.434	-101.136	-5618.378	-91.975
	(4025.528)	(3889.517)	(4078.731)	(161.206)	(4002.756)	(158.657)
Incr. allowance × 2016	-374.203	-255.364	-186.983	38.872	301.335	53.383
	(4514.664)	(4338.628)	(4514.391)	(167.888)	(4414.771)	(165.011)
Incr. allowance × 2017	6327.273	6981.114	6829.216	-44.302	7271.735	-34.158
	(6617.295)	(6076.110)	(6214.703)	(146.089)	(6348.976)	(139.726)
Incr. allowance × 2018	8538.707	9925.962$^{\sim}$	10132.202$^{\sim}$	97.786	10701.973$^{\sim}$	105.102
	(6280.136)	(5522.092)	(5729.746)	(175.660)	(5894.216)	(173.578)
Controls	No	Yes	Yes	Yes	Yes	Yes
Observations	2784	2784	2784	2784	2776	2776
Panel B						
Incr. allowance × 2013	-1035.427	39.320	-1590.163	59.039	4981.795	-26.085
	(2807.115)	(115.368)	(2996.331)	(126.810)	(3629.386)	(201.209)
Incr. allowance × 2014	1552.110	-38.040	1366.686	-45.252	-1772.181	153.380
	(2763.172)	(93.916)	(2872.808)	(103.760)	(11097.967)	(383.050)
Incr. allowance × 2015	-4618.826	-168.543	-5451.801	-142.048	-274.329	7.853
	(3632.285)	(147.736)	(3824.212)	(161.933)	(8241.815)	(230.782)
Incr. allowance × 2016	1606.547	-20.889	2182.320	39.306	-6740.626	-319.098
	(4473.547)	(161.270)	(4638.147)	(173.750)	(11228.333)	(358.722)
Incr. allowance × 2017	6702.565	-47.741	7442.204	28.438	-4505.149	-344.667**
	(6300.385)	(148.583)	(6591.422)	(164.729)	(11492.681)	(126.356)
Incr. allowance × 2018	9715.124$^{\sim}$	53.005	10012.524$^{\sim}$	115.574	-1424.028	-175.569
	(5742.865)	(169.759)	(5929.045)	(187.241)	(13187.615)	(144.873)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2791	2791	2437	2437	402	402

Notes: Panel A, columns (1), without controls, and (2), with controls, show the results for gross labor income; columns (3) and (4) show results for labor income and donations, respectively, where standard errors are clustered at the household level; columns (5) and (6) show results for labor income and donations, respectively, where standard errors are clustered at the municipality level and municipality fixed effects are included. *Panel B* shows results for labor income in columns (1), (3), and (5) and for donations in (2), (4), and (6). Columns (1) and (2) exclude individuals who might be donating back the allowance, columns (3) and (4) focus on individuals without a taxable allowance, while columns (5) and (6) only include individuals reporting a taxable allowance. Results are relative to the baseline period 2007-12. All specifications include year and individual fixed effects. Labor income and donations are winsorized on the 3% level. Control variables include marital status, indicators for no kids or having kids below age of 18, employment status, earnings source, religion, commuting effort, federal state fixed effects. Standard errors clustered at the individual level in parentheses if not stated otherwise. $^{\sim}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C19: Allowance increase effects on labor and donations for matched volunteers, short-term and average results.

	Labor income		Donations	
	(1)	(2)	(3)	(4)
Panel A				
Increased allowance × post	-4367.654	-4271.381~	-82.248	-101.592
	(3047.503)	(2562.234)	(121.389)	(128.631)
Controls	No	Yes	No	Yes
Observations	498	498	498	498
Panel B				
Increased allowance × post	874.591	1224.491	-5.169	-6.358
	(3486.792)	(3081.905)	(99.117)	(102.953)
Controls	No	Yes	No	Yes
Observations	2784	2784	2784	2784

Notes: Panel A shows short-term results based on one year before and one after the reform, for the outcome variables specified in the header. *Panel B* shows average treatment effects for the whole period (2007-18) for the outcome variables specified in the header. All specifications include year and individual fixed effects. Labor income and donations are winsorized on the 3% level. Controls include marital status, indicators for no kids or having kids below age of 18, employment status, earnings source, religion, commuting effort, federal state fixed effects. Standard errors clustered at the individual level in parentheses. ~ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C20: Probability of exit for matched volunteers.

	IPW: Logit		IPW: LPM	
	(1)	(2)	(3)	(4)
Increased allowance	-0.238***	-0.264***	-0.238**	-0.257***
	(0.072)	(0.063)	(0.073)	(0.067)
Controls	No	Yes	No	Yes
Observations	249	249	249	249

Notes: Non-exponentiated coefficients using a logit model or linear probabiliy model and inverse-probability weighting. Controls include marital status, indicators for no kids or having kids below age of 18, employment status, earnings source, religion, commuting effort, federal state fixed effects, gender, and birth year. Standard errors clustered at the individual level in parentheses. ~ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C21: Robustness checks: Probability of exit for matched volunteers.

	(1)	(2)	(3)	(4)
Panel A				
Increased allowance	0.795***	0.776***	0.800***	0.789***
	(0.051)	(0.044)	(0.053)	(0.051)
Controls	No	Yes	No	Yes
Observations	249	249	209	208
Panel B				
Increased allowance	0.795***	0.791***	0.776***	0.809**
	(0.051)	(0.051)	(0.051)	(0.055)
Controls	No	No	No	No
Observations	249	249	226	229
Panel C				
Increased allowance	0.786***	0.730	0.798***	0.778***
	(0.053)	(0.177)	(0.052)	(0.049)
Controls	No	No	No	No
Observations	216	33	245	242

Notes: Exponentiated coefficients using a logit model and inverse-probability weighting, except if differently stated. *Panel A*, in columns (1) and (2) we report the same results as in Table C20 columns (1) and (2), but presenting exponentiated coefficients instead of marginal effects. Column (3) shows the results based on coarsened exact matching without control variables, which are included in column (4). *Panel B*, in column (1) errors are clustered at the household level, in column (2) we control for the first year of volunteering, in column (3) we drop individuals exiting the TPP before 2018, in column (4) we exclude individuals entering the panel after 2007 with current volunteering spell in first period. *Panel C*, in column (1) we exclude individuals who have a taxable allowance, in column (2) we focus only on individuals who have a taxable allowance, in column (3) we exclude individuals who might be donating back the allowance, in column (4) we exclude individuals younger than 30. Controls include marital status, indicators for no kids or having kids below age of 18, employment status, earnings source, religion, commuting effort, federal state fixed effects, gender, and birth year. If not differently stated, standard errors clustered at the individual level in parentheses. $^{\sim}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C22: Effect of the allowance increase on volunteer duration for general volunteers

	(1)	(2)	(3)	(4)	(5)
Max general	0.745***	0.743***	0.742***	0.744***	0.740***
	(0.042)	(0.026)	(0.026)	(0.027)	(0.026)
Max general × Post	1.287*	1.281*	1.286*	1.285*	1.277*
	(0.204)	(0.150)	(0.150)	(0.151)	(0.149)
Observations	7833	7833	7833	7833	7833

Notes: Exponentiated coefficients based on the estimation of equation (3). The Max general group consists of volunteers receiving €500 during their last spell before 2013 and is compared to the Mid general group receiving €100-400 during their last spell before 2013. Labor market controls consist of dummies for being retired, receiving benefits, and earning income mainly from agriculture, real estate, self-employment, or employment. Household controls consist of dummies for being married, having no kids, or having a kid below 18 y/o. Religious controls consist of dummies for protestant and catholic. Column (5) combines these controls and includes a dummy for gender and birth year as a continuous control. Except for column (1) without any cluster level, standard errors in parentheses are clustered at the individual level. $^{\sim}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C23: Allowance increase effects on labor and donations for general volunteers.

	Labor income		Donations	
	(1)	(2)	(3)	(4)
Max general × 2013	-1281.714	-240.014	70.594	66.806
	(2264.836)	(2190.411)	(44.502)	(44.412)
Max general × 2014	-2574.443	-1453.683	61.064	56.662
	(2450.857)	(2340.286)	(53.248)	(53.369)
Max general × 2015	-4005.222	-3168.797	67.025	63.370
	(2781.691)	(2564.752)	(57.983)	(58.151)
Max general × 2016	-3296.177	-2044.453	11.178	8.158
	(3059.975)	(2780.582)	(58.993)	(59.712)
Max general × 2017	-5521.725	-3314.845	-7.240	-6.739
	(3715.984)	(3288.517)	(64.150)	(64.775)
Max general × 2018	-8546.004*	-6320.968~	-32.072	-33.697
	(4203.546)	(3611.671)	(69.680)	(70.401)
Controls	No	Yes	No	Yes
Observations	22357	22357	22357	22357

Notes: Results are relative to the baseline period 2007-12. The Max general group consists of volunteers receiving €500 in 2012 and is compared to the Mid general group receiving €100-400 in 2012. All specifications include year and individual fixed effects. Labor income and donations are winsorized on the 3% level. Controls include marital status, indicators for no kids or having kids below age of 18, employment status, earnings source, religion, commuting effort, federal state fixed effects. Standard errors clustered at the individual level in parentheses. ~ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

D FWS Data Appendix

Table D1: Descriptive statistics, by group

	Not vol. Mean (SD)	Unpaid vol. Mean (SD)	Paid vol. Mean (SD)	Former vol. Mean (SD)
Male	0.403	0.470	0.544	0.464
	(0.491)	(0.499)	(0.498)	(0.499)
Highly educated	0.320	0.507	0.554	0.443
	(0.467)	(0.500)	(0.497)	(0.497)
Employed	0.464	0.540	0.506	0.460
	(0.499)	(0.498)	(0.500)	(0.498)
Nr. people in household	2.234	2.515	2.500	2.070
	(1.197)	(1.243)	(1.237)	(1.062)
Age	48.759	48.317	45.655	53.649
	(19.080)	(17.422)	(17.827)	(17.814)
German citizenship	0.932	0.973	0.979	0.962
	(0.253)	(0.163)	(0.145)	(0.190)
Religious	0.488	0.625	0.591	0.519
	(0.500)	(0.484)	(0.492)	(0.500)
West Germany	0.640	0.701	0.694	0.623
	(0.480)	(0.458)	(0.461)	(0.485)
City dweller	0.720	0.674	0.663	0.710
	(0.449)	(0.469)	(0.473)	(0.454)
Donates	0.448	0.637	0.611	0.583
	(0.497)	(0.481)	(0.488)	(0.493)
Volunteer hours/week		1.357	1.684	
		(0.763)	(0.980)	
Volunteer duration (years)		10.058	10.715	
		(10.603)	(10.512)	
Observations	17,813	17,223	2,195	11,342

Notes: The table displays pooled results for the years 2009 and 2014, weighted. *Highly educated* is an indicator with value 1 if the person has completed at least the A-levels. *Employed* is an indicator with value 1 if the person has a full- or part-time job. *City dweller* is an indicator with value 1 if the person lives in a municipality with at least 100,000 inhabitants.

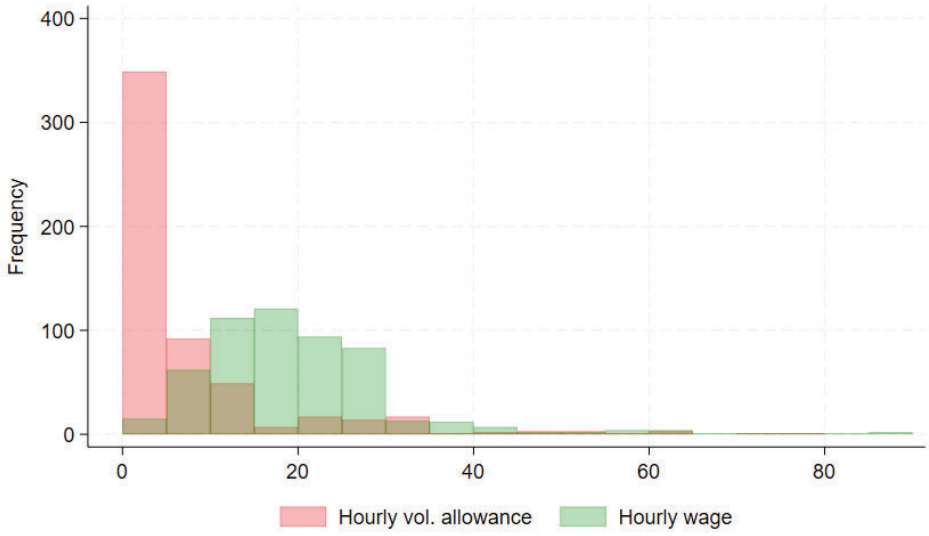


Figure D1: Distribution of positive hourly wage and hourly volunteer allowance in 2014 (for cases with payments below 100)

Table D2: Reimbursement option and use as a predictor of (high) paid volunteering

	(1)	(2)	(3)
	High paid volunteer		Paid volunteer
Reimbursement is possible	0.074**		
	(0.023)		
Reimbursement N/A (no expenses)	-0.017		
	(0.031)		
Regularly uses reimbursement		0.135**	0.305***
		(0.048)	(0.026)
Occassionally uses reimbursement		0.030	0.103***
		(0.042)	(0.019)
Controls	Yes	Yes	Yes
Observations	935	605	2392

Notes: High-paid volunteering implies receiving an allowance of at least €1801 per year versus €1-1800. Controls include gender, age, education, employment status, religion, number of people in household, municipality size, East-West Germany. Robust standard errors in parentheses. ~ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table D3: Percentage of paid volunteers by organizational type

	2009	2014
Club or association	55.9%	49.3%
Church or religious association	4.6%	7.3%
State or municipal institution	17.7%	17.7%
Other formally organized institution	12.5%	13.3%
Self-organized group	8.9%	11.1%
Observations	990	1205

Notes: We do not report other possible answers like "refused to reply" or "does not know," which represent very few cases.

Table D4: Differences in reasons for ending.

	2009	2014	t-test
	(1)	(2)	(3)
No time	0.397	0.405	-0.007
	(0.489)	(0.491)	(0.009)
Project-based activity	0.247	0.430	-0.183***
	(0.431)	(0.495)	(0.009)

Notes: Includes former paid and unpaid volunteers. $N = 4713$ in column 1. In column 2, $N = 6576$ for the first item, while $N = 6550$ for the second. In column 3, $N = 11312$. Standard deviations (columns 1 and 2) and standard errors (column 3) in parentheses. $^{\sim}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table D5: Number of activities regressed on allowance amount

	(1)	(2)
600 to 1800 Euro	0.196*	0.181*
	(0.076)	(0.078)
1800 to 4200 Euro	0.511**	0.489**
	(0.160)	(0.161)
4200 Euro or more	0.678**	0.747**
	(0.240)	(0.231)
2014	0.303***	0.320***
	(0.063)	(0.064)
600 to 1800 Euro × 2014	-0.075	-0.077
	(0.113)	(0.116)
1800 to 4200 Euro × 2014	-0.319	-0.324
	(0.206)	(0.205)
4200 Euro or more × 2014	0.078	-0.064
	(0.329)	(0.326)
Controls	No	Yes
Observations	2121	2007

Notes: The omitted category is an allowance up to 600 Euro per month. Controls include dummies for gender, education level, employment status, religion, number of people in household, municipality size, East-West Germany. Age is included a a continuous variable. Robust standard errors in parentheses. ~ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table D6: Selection into high compensation given underlying values

	(1)	(2)	(3)	(4)
	Payment category (1-4)		High-paid volunteer	
2014	-0.070	-0.080	0.026	0.011
	(0.220)	(0.226)	(0.089)	(0.092)
Values: Help people (1-5)	0.047~	0.045~	0.025*	0.023*
	(0.027)	(0.027)	(0.011)	(0.011)
2014 × Help people (1-5)	0.013	0.021	-0.005	-0.004
	(0.038)	(0.039)	(0.015)	(0.016)
Values: Enjoying the good things (1-5)	-0.002	-0.008	-0.001	0.001
	(0.030)	(0.030)	(0.012)	(0.013)
2014 × Enjoying the good things (1-5)	0.007	0.007	0.000	0.004
	(0.041)	(0.042)	(0.017)	(0.018)
Controls	No	Yes	No	Yes
Observations	2111	1998	2111	1998

Notes: Respondents were asked to rate the importance of the values "To help people" and "Enjoying the good things" for them on a scale from 1 (not important) to 5 (extremely important). The four outcome categories (treated as continuous scale) in columns (1) and (2) are up to €600, €600-1800, 1800-4200€, and €4200 or more. The outcome in columns (3) and (4) is a binary variable that takes value 1 if an individual is compensated €1800+ per year and value 0 when compensated up to €1800 per month. Controls include dummies for gender, education level, employment status, religion, number of people in household, municipality size, East-West Germany. Age is included as a continuous variable. Robust standard errors in parentheses. ~ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.