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## Basic income and labor supply: Evidence from an RCT in Germany

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# 2123

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#### IMPRESSUM

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# Basic income and labor supply: Evidence from an RCT in Germany

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## Abstract

How does basic income (a regular, unconditional, guaranteed cash transfer) impact labor supply? We show that in search models of the labor market with income effects, this impact is theoretically ambiguous: Employment and job durations might increase or decrease, match surplus might be shifted to workers or employers, and worker surplus might be reallocated between wages and job amenities.

We thus turn to empirical evidence to study this impact. We conducted a pre-registered RCT in Germany, starting 2021, where recipients received 1200 Euro/month for three years. We draw on both administrative and survey data, and find no extensive margin (employment) response, and no impact on on job transitions from either non-employment or employment. We do find a small statistically insignificant intensive margin shift to part-time employment, which implies an excess burden (reduction of government revenues) of ca 7.5% of the transfer. We furthermore observe a small increase of enrollment in training or education.

KEYWORDS: BASIC INCOME, RANDOMIZED CONTROLLED TRIAL, LABOR SUPPLY  
JEL CODES: I38, J22

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# 1 Introduction

Basic income is a much-discussed proposal in debates about the future of welfare states in high income countries (Van Parijs and Vanderborght, 2017; Marinescu, 2018). Basic income is unconditional, paid regularly, and does not involve surveillance or control from authorities. It thus provides an outside option relative to other income sources, reduces uncertainty about future income, and might be less associated with stigma.

Due to its lack of conditionality, basic income furthermore avoids the distortionary incentives of other welfare transfers: Some welfare transfers incentivize excessive labor supply by imposing work requirements or by providing subsidies of low wage work. Other transfers *disincentivize* labor supply, by excluding people from benefits whenever they have other sources of income.

Basic income lacks conditionality of this form, but it might still impact labor supply via income effects. Concerns regarding such income effects play a central role in the wider policy debate about basic income. One concern is that basic income might enable workers to *reduce their labor supply*, thereby reducing the tax base. This is a concern about the deadweight loss implied by the resulting reduction of government revenue. Another, and opposing, concern is that basic income might enable employers to pay lower wages, by *increasing workers' labor supply* at any given level of wages. This is a concern about the incidence of basic income.

This disagreement in the policy debate, about whether a basic income would increase or decrease labor supply, is mirrored in a theoretical ambiguity in dynamic models of the labor market. Standard models of search and matching often assume that there are no income effects (Pissarides, 2000). We introduce a partial equilibrium search model where we allow worker flow utility (1) to depend on endogenous workplace amenities, and (2) to exhibit income effects and declining marginal utility of income. We show that the comparative statics of reservation wages, search durations and employment rates are ambiguous in such a model. We furthermore show that an evaluation of the welfare impact of a basic income in such a setting cannot be expressed in terms of the mechanical transfer alone: Because the presence of search frictions allows for bilateral bargaining, a basic income might lead to a redistribution of match surplus between workers and employers, and to a reallocation of worker surplus between wages and non-wage amenities. The conclusions of the sufficient statistics approach to optimal taxation (Chetty, 2009; Kleven, 2021) thus do not apply; we cannot leverage the envelope theorem (Milgrom and Segal, 2002) to express welfare effects purely in terms of mechanical transfers.

Motivated by these results regarding the theoretical ambiguity of the impact of basic income on labor supply and welfare, we turn to an empirical investigation of these questions. We present evidence from a basic income experiment that we designed in Germany. The experiment was implemented by the German NGO *Mein Grundeinkommen*. This NGO is financed by (mostly small) private donations. The treatment group in the experiment received monthly

payments of EUR 1200 for a total of three years, starting in June 2021.<sup>1</sup> Cash transfers increased baseline annual household income by between 46% and 110%. There were 107 participants in the treatment group, and 1580 participants in the control, where these numbers were determined based on the budget constraints of our implementation partner and the relative cost of treatment and control. Participants were between 21 and 40 years of age, were not unemployed at baseline, and lived in households of size one.<sup>2</sup> We used a stratified randomized experimental design for treatment assignment, and estimate the effect of treatment on changes in outcomes, relative to a pre-treatment baseline. Both stratification and differencing allow us to reduce the variability of our estimates of average treatment effects, which are a concern given the relatively small size of the treated group. We report p-values from permutation tests, as well as standard errors accounting for stratified random assignment.

We draw on two data-sources to measure participant outcomes. The first are administrative data, derived from the *Integrated Labor Market Biographies* dataset constructed by the German *Institute for Employment Research* (IAB), which in turn is based on data reported by employers to the social security system (Schmucker and Vom Berge, 2025). This dataset provides daily records on employment status, wages, unemployment benefits, and location of residence, among others. These data allow us to obtain independently verified measures of labor market outcomes that are missing in many studies of the impact of unconditional cash transfers. They allow us to avoid the risk of experimenter demand effects and other biases in self reports. To complement these administrative data, we conducted semi-annual surveys among participants. These surveys allow us to provide independent validation measures of the outcomes in the administrative data, and to paint a richer picture of participant labor market status. They allow us to capture outcomes not measured in the official data, including weekly work hours and self-employment, as well as stated beliefs and preferences.

Our main findings from this experiment, interpreted in the context of our search model, are as follows. First, there was no extensive margin response of labor supply. Employment rates remain the same in the treatment and control group (our confidence intervals can exclude employment effects outside of  $\pm 5$  percentage points). Correspondingly, there was no effect on unemployment benefit claims (we can exclude effects of more than 22 Euro per participant and month). Second, there was a small intensive margin response of labor supply: About 3% of recipients switched to part-time employment (we can exclude effects of more than 9%), and self-reported working hours declined by about 1.3 hours per week. Third, because of these intensive margin responses, there was a decline of government revenues (social insurance payments and income taxes) of around 90 Euro per participant and month (the confidence

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<sup>1</sup>The experimental design and analysis were pre-registered at <https://www.socialscisceregistry.org/trials/7734>. A companion paper analyzes the effect of basic income on subjective wellbeing and mental health, (Bohmann et al., 2025). Our description of background and experimental design in Section 3 partially overlaps with (Bohmann et al., 2025).

<sup>2</sup>One might conjecture that labor market attachment for this group differs from that of other demographics or other countries. Our own evidence does not allow us to speak to this question of external validity, but our findings are consistent with those from the literature for a range of demographics and contexts, as discussed below.

interval again includes 0). Fourth, and contrary to our expectations, there was no effect on job transitions from either non-employment or employment to other jobs. There was, finally, an increase of the number of participants enrolled in training or education by about 3%.

Interpreted in the context of our theoretical model, these empirical findings imply the following takeaways – assuming the experimental findings extrapolate to the general population. (i) There is no income effect on the extensive margin of labor supply. Reservation wages and search durations of recipient workers appear unaffected. (ii) There is a small income effect at the intensive margin, where some recipients switch to part-time work. There is suggestive evidence that they also choose employers closer to their location of residence. Put differently, there appears to be some reallocation of match surplus from wages to amenities and to reduced work hours. (iii) Based on these intensive margin responses, basic income entails a moderate excess burden of around 7.5%, relative to the amount of income disbursed. Put differently, in a model of optimal taxation, if the welfare weight assigned to recipients thus exceeds the marginal cost of public funds by more than 7.5%, then basic income is optimal. (iv) The beneficiaries of basic income are indeed the nominal recipients, rather than their employers, in contrast to what might be expected for subsidies of low-wage work. General equilibrium effects of a hypothetical basic income disbursed at larger scale are, however, harder to predict.

This paper contributes to a growing literature on the impact of (unconditional) cash transfers, in both higher-income and lower-income countries. Many studies in this literature only find at most a small labor market impact of unconditional cash transfers, in line with our own results. Focusing on the labor market impact, Marinescu (2018) reviews the prior literature and concludes that most studies find no statistically significant effect of an unconditional cash transfer on the probability of working, and that in the studies that do find an effect on labor supply this effect is small. The literature on cash transfers in low and middle income countries comes to similar conclusions. The meta-study Banerjee et al. (2017) finds no systematic evidence that cash transfer programs discourage work. The same holds for (Baird et al., 2018), with the possible exception of cash transfers to the elderly; see also Banerjee et al. (2019). The same conclusion holds true for more recent analyses in high income countries: In a study of the cash dividend from the Alaska Permanent Fund, Jones and Marinescu (2022) find that the dividend had no effect on employment and increased part-time work by 1.8 percentage points. Vivalt et al. (2024), analyzing a recent basic income experiment in the US, find a 3.9 percentage point decrease in labor market participation, and that participants reduced their work hours as a result of the transfers by 1-2 hours/week. Verho et al. (2022), analyzing a Finish experiment that compared unemployment benefits to a basic income, also find no employment effects of basic income.

The rest of this paper is structured as follows. Section 2 discusses a model of job search and bargaining with income effects, and its implications for our setting. Section 3 describes the basic income policy, the sample construction, our experimental design, the administrative and survey data, and our estimators. Section 4 discusses our empirical findings, from both the

administrative and the survey data. Section 5 concludes. Appendix A provides proofs for our theoretical results characterizing the search model. Appendix B provides a detailed description of sampling and treatment assignment, based on our pre-analysis plan. Appendix C describes the definition and construction of our outcome variables, based on the IAB data.

## 2 Basic income in a search model of the labor market

We next discuss a search model of the labor market. This model serves several purposes for our empirical analysis. First, the model shows that key comparative statics of interest are *theoretically ambiguous*, even in a relatively simple model of search: Basic income might improve or worsen workers’ bargaining position relative to employers, it might reduce or increase search time and match quality, and it might shift surplus between wages and non-wage job amenities in different ways. The sign and magnitude of these effects is thus an empirical question.

Second, the model provides *interpretations for our estimands*: In the context of this model, we can meaningfully consider the effects of a basic income on expected match quality, search time, outside options and bargaining power, and the allocation of match surplus between employers and workers and between wages and non-wage job amenities. (Our model is however intentionally stylized, and we do not take it to be a literal description of the labor market. We therefore do not attempt to directly estimate the parameters of the model.)

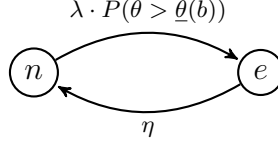
Third, the model allows us to analyze the *welfare impact* of basic income, taking into account endogenous labor market responses: It is common in public finance to assume that the welfare impact of a change in transfers is equal to the direct mechanical impact of such a change (Chetty, 2009; Kleven, 2021), by invoking the envelope theorem (Milgrom and Segal, 2002) for utility maximizing transfer recipients. This conclusion fails in our setting with search frictions, however, because workers and employers get to negotiate over the distribution of match surplus. To estimate the welfare impact of basic income, we thus need to also estimate its impact on wages and amenities.

The remainder of this section is structured as follows. We first introduce the assumptions describing our variation of the standard partial equilibrium model of job search. We then characterize the solution (Bellman equations, bargaining solution) of this model. Based on this solution, we demonstrate the theoretical ambiguity of key comparative statics, in Proposition 1. We next turn to an analysis of the impact of basic income on public revenue and private welfare, which is characterized in Proposition 2. We finally consider a model variation with exogenous amenities and on-the-job search, which allows us to discuss the impact of basic income on job transitions.

### 2.1 A partial equilibrium model of job search with basic income

Our model is based on the search model with bargaining and heterogeneous match quality described in chapter 6 of Pissarides (2000) (and, similarly, in Flinn 2006). Relative to these

Figure 1: Flows between employment states



references, our model is extended by including (1) non-wage job amenities  $a$ , (2) income effects and declining marginal utility of income  $y$ , and (3) an unconditional basic income  $b$ . These three extensions are necessary to yield interesting comparative statics with respect to basic income. Absent income effects, an unconditional basic income has no labor market impact at all. Absent non-wage job amenities, basic income has no effect on job quality that might be mediated by bargaining. Our analysis focuses on partial equilibrium, taking matching rates and job creation as exogenous.<sup>3</sup>

**Assumptions** Workers are either searching (non-employed, subscript  $n$ ) or employed (subscript  $e$ ). Their flow utility equals  $u(y, a)$  for income  $y \in \mathbb{R}$  and job amenities  $a \in \mathbb{R}$ , where the latter are normalized to equal 0 for searching workers. Income is equal to the (exogenous) basic income  $b$  for searching workers, and is equal to the (endogenous) wage  $w$  plus basic income  $b$  for employed workers.

Job offers arrive at an exogenous rate  $\lambda$ , and the workers' productivity in a job equals  $\theta \in \mathbb{R}$ , where  $\theta$  is a random draw from the distribution  $P$  for each job (i.e., match). Jobs dissolve at an exogenous rate  $\eta$ . The workers' discount factor equals  $\rho$ .<sup>4</sup>

Denote the expected discounted utility of a non-employed worker by  $V_n(b)$ , and of an employed worker for a match of productivity  $\theta$  by  $V_e(\theta, b)$ . The worker will accept a job offer iff  $V_e(\theta, b) \geq V_n(b)$ . Denote the minimal productivity for which a job is accepted by  $\underline{\theta}(b)$ , which is such that

$$V_e(\underline{\theta}(b), b) = V_n(b). \quad (1)$$

To close the model, we need to specify how wages  $w(\theta, b)$  and amenities  $a(\theta, b)$  are determined. Suppose that the employers' utility is given by  $\theta - w - a$ , that their outside option yields utility 0, and that wages and amenities are set by generalized Nash bargaining, where the workers' bargaining power equals  $\alpha$ , so that

$$(w(\theta, b), a(\theta, b)) = \underset{w, a}{\operatorname{argmax}} (u(w + b, a) - \rho V_n(b))^\alpha \cdot (\theta - w - a)^{(1-\alpha)}. \quad (2)$$

<sup>3</sup>This is justified in our empirical context given the small number of recipients: 107 basic income recipients in a country of over 80 million residents.

<sup>4</sup>The exogeneity of  $\lambda$  merits discussion. In contrast to typical search models of the labor market (Pissarides, 2000), we do not endogenize the matching rate (via a matching function), nor the creation of vacancies by firms. This is justified by the fact that we are interested in an empirical setting where the share of treated workers (basic income recipients) is small relative to the size of the economy.

**Bellman equations** Based on these assumptions, we get the following Bellman equations for the expected discounted utility of workers who are searching, and for those who are employed in a match of quality  $\theta$ .

$$\begin{aligned}\rho V_n(b) &= u(b, 0) + \lambda \cdot E[(V_e(\theta, b) - V_n(b)) \cdot \mathbf{1}(\theta > \underline{\theta}(b))] \\ \rho V_e(\theta, b) &= u(w(\theta, b) + b, a(\theta, b)) + \eta \cdot (V_n(b) - V_e(\theta, b)).\end{aligned}\tag{3}$$

The expectation in Equation 3 is taken over the distribution  $P$  of match quality  $\theta$ . Matches are accepted iff match quality exceeds the cutoff value  $\underline{\theta}(b)$ , which depends on the size of the basic income.

Define  $\tilde{\lambda}(b) = \lambda \cdot P(\theta > \underline{\theta}(b))$ , the expected rate of finding *and accepting* a job. With this notation,  $\rho V_n(b) = u(b, 0) + \tilde{\lambda}(b) \cdot (E[V_e(\theta, b) | \theta > \underline{\theta}(b)] - V_n(b))$ . We can then solve the Bellman equations, expressing expected value functions in terms of flow utilities. Denoting

$$A = \frac{\tilde{\lambda}(b)}{\rho + \tilde{\lambda}(b) + \eta}, \quad B = \frac{\rho + \tilde{\lambda}(b)}{\rho + \tilde{\lambda}(b) + \eta},$$

we get

$$\begin{aligned}\rho V_n(b) &= (1 - A) \cdot u(b, 0) + A \cdot E[u(w + b, a) | \theta > \underline{\theta}(b)], \\ \rho E[V_e(\theta, b) | \theta > \underline{\theta}(b)] &= (1 - B) \cdot u(b, 0) + B \cdot E[u(w + b, a) | \theta > \underline{\theta}(b)].\end{aligned}$$

In these equations the dependency of  $w$  and  $a$  on  $\theta$  and  $b$  is omitted for notational brevity. The expectations are over the conditional distribution of  $\theta$  for accepted matches.

**Bargaining solution** The solution to the generalized Nash bargaining problem can be derived in two steps. We can first solve for the optimal  $w$  and  $a$  conditional on the total budget  $t = b + w + a$ . The solution to this problem does not depend on  $\theta$ , given the assumed form of employer utility. This observation allows us to simplify the bargaining problem to a one-dimensional distribution of surplus. Denote  $v(t) = \max_{y,a} u(y, a)$  subject to  $y + a = t$ . We then have to solve in the second step for

$$t(\theta, b) = \operatorname{argmax}_t (v(t) - \rho V_n(b))^\alpha \cdot (\theta + b - t)^{1-\alpha}.\tag{4}$$

The first order condition for  $t(\theta, b)$  is given by

$$v'(t) = \frac{1 - \alpha}{\alpha} \left( \frac{v(t) - \rho V_n(b)}{\theta + b - t} \right).\tag{5}$$

**Comparative statics** The impact of basic income in our simple search model is theoretically ambiguous. This is illustrated by the following proposition, which describes two special cases of our model. Both special cases involve the myopic limit of high discount rates (equivalently,

high transition rates), which reduces value functions to flow utilities. Worker flow utility is furthermore assumed to be separable.

**Proposition 1** (Theoretical ambiguity of comparative statics). *Suppose that worker flow utility is of the form  $u(y, a) = y^\gamma + a^\delta$ . Consider the limits of  $t(\theta, b)$  and  $\underline{\theta}(b)$  in our model as  $\rho \rightarrow \infty$  (holding all other parameters fixed), and their comparative statics with respect to  $b$ .*

1. *If  $\gamma = 1$  and  $\delta \leq 1$ , then  $\underline{\theta}$  does not depend on  $b$  and  $\partial_b t(\theta, b)(b) = 1$ .*
2. *If  $\gamma < 1$  and  $\delta = 1$ , then the dependence of  $\underline{\theta}$  on  $b$  is non-monotonic:*
  - (a) *For  $b$  small,  $\underline{\theta}'(b) > 0$  and  $\partial_b t(\theta, b) > 1$ .*
  - (b) *For  $b$  large,  $\underline{\theta}$  is decreasing in  $b$  and  $\partial_b t(\theta, b)(b) < 1$ .*

*Both the average search duration of the unemployed and the average match quality among the employed are increasing in  $\underline{\theta}$ .*

The proof of Proposition 1 can be found in Appendix A. The theoretical ambiguity of comparative statics, as demonstrated by the examples in this proposition, underscores the need for empirical investigation, as in our experimental evaluation discussed below.

## 2.2 Basic income, public revenue, and private welfare

The preceding discussion showed that the impact of basic income on search durations, match quality, and the allocation of match surplus is ambiguous. We next consider the impact of basic income on public revenue and private welfare. Models of optimal taxation and insurance (e.g. Saez 2001; Chetty 2006; Kleven 2021) typically aim to maximize a (weighted) sum  $SWF$  of individual welfare, subject to a constraint on (net) public revenue  $G$ . The Lagrangian corresponding to this problem is given by

$$\mathcal{L} = SWF + \lambda \cdot G.$$

**Public revenue** To characterize the impact  $\partial_b \mathcal{L}$  of a marginal policy change on this Lagrangian, we need to know its impact on the government budget,  $\partial_b G = M + B$ . This impact has a mechanical component  $M$ , holding individual behavior fixed, and a behavioral component  $B$ , which is due to endogenously changing behavior (e.g., labor supply) that impacts the tax base. The behavioral component  $B$  is the key causal object, or *sufficient statistic* for much of empirical public finance.

In our context,  $M$  is very simple. It corresponds to the monetary cost of a marginal increase of basic income, which is the same for all recipients. The component  $B$  depends on possible changes to labor supply, which include extensive margin changes (non-employment), intensive margin changes (hours, and part-time versus full-time employment), and possible job changes that might impact wage levels. These different margins of labor supply in turn impact social

insurance contributions (owed by both employees and employers in the German system), income taxes, and possible unemployment benefit receipt. Below, we report experimental estimates of basic income receipt on each of these, as well as their combined effect on net government revenue.

**Individual welfare** The impact  $\partial_b SWF$  of a policy change on social welfare is a weighted sum of its impact on individual welfare, which we denote  $\partial_b V_i$  for individual  $i$ . In many settings, we can ignore behavioral responses to marginal tax or price changes, when considering individual welfare. This holds because, by virtue of the envelope theorem Milgrom and Segal (2002), such behavioral responses to marginal tax or price changes have no impact, to first order, on the welfare of individuals, leaving aside possible spillover or equilibrium effects. This insight motivates much of the “sufficient statistics” literature in public finance.

This insight does not directly carry over to our setting with search frictions and Nash bargaining, however: To evaluate the impact of basic income on individual welfare in our setting, we also need to take into account its impact on the distribution of match surplus between workers and employers. Depending on the sign of  $\partial_b t(\theta, b) - 1$ , some of the benefit of increasing basic income might be absorbed by employers (if the sign is negative), or alternatively the benefit of basic income might be amplified due to an improved worker bargaining position (if the sign is positive). In our model, the impact  $\partial_b V_i$  is the expected discounted stream of the impact on flow-utilities, for both non-employed and employed workers, as summarized in the following proposition.

**Proposition 2** (Welfare effect of basic income). *Assume that flow utilities and value functions are differentiable, and that the conditions for exchanging differentiation and integration are satisfied. Then the welfare effect of a marginal increase of basic income  $b$  on non-employed and employed workers is given by*

$$\begin{aligned}\rho \cdot \partial_b V_n(b) &= (1 - A) \cdot \partial_b u(b, 0) + A \cdot E[v'(t) \cdot \partial_b t(\theta, b) | \theta > \underline{\theta}(b)], \\ \rho \cdot E[\partial_b V_e(\theta, b) | \theta > \underline{\theta}(b)] &= (1 - B) \cdot \partial_b u(b, 0) + B \cdot E[v'(t) \cdot \partial_b t(\theta, b) | \theta > \underline{\theta}(b)].\end{aligned}$$

The proof of Proposition 2 can again be found in Appendix A. Notably, in these expressions  $\underline{\theta}$  is held constant; we do not need to take into account its dependence on  $b$ . The impact of  $b$  on individual welfare has two components:

1. The *mechanical* effect, coming directly from receipt of basic income. This effect equals  $\partial_b u(b, 0)$  for the non-employed, and  $v'(t)$  for the employed.
2. The *negotiation* effect  $v'(t) \cdot (\partial_b t(\theta, b) - 1)$ , coming from the re-distribution of match surplus between workers and employers. This effect includes both changes to wages and to amenities; we will provide estimates for some of these in our empirical analysis.

### 2.3 Model variation: Exogenous amenities and on-the-job search

In the baseline model discussed thus far, job amenities are bargained over. Assume now instead that the amenities  $a$  provided by a match are exogenously fixed, rather than being the result of bargaining. Such an amenity  $a$  might for instance reflect how meaningful a given job is to a prospective employee.

In our baseline model, there is furthermore no on-the-job search: Jobs dissolve at an exogenous rate  $\eta$ , and transitions from employment are only to non-employment. Assume now instead that workers also search for a new job while being employed. Suppose that new offers (matches) arrive for employed workers at an exogenous rate  $\lambda^e$ . Match characteristics  $(\theta', a')$  for new offers are i.i.d. draws from some distribution. A new match is preferred to the current one iff  $u(w(\theta', a', b) + b, a) > u(w(\theta, a, b) + b, a)$ , in which case a job transition occurs.

To simplify the following discussion, we finally assume again that  $u$  is separable, so that  $\partial_y \partial_a u = 0$ , and consider the myopic limit  $\rho \rightarrow \infty$ , where  $\rho V_n(b) \rightarrow u(b, 0)$  and  $\rho V_e(\theta, a, b) \rightarrow u(w(\theta, a, b) + b, a)$ .

**Indifference curves** A match in this model variation is characterized by the exogenously given tuple  $(\theta, a)$ . Correspondingly, wages  $w$  (as determined by bargaining) are a function of  $(\theta, a, b)$ , and the same holds for the utility of employed workers. A match leads to employment iff there exists a wage such that both worker and employer are better off than under their outside option. The maximum wage for which the employer is indifferent equals  $\theta - a$ , implying worker income  $y = \theta - a + b$ , so that worker indifference between a match and non-employment holds iff  $u(\theta - a + b, a) = u(b, 0)$ . The slope of the indifference curve between matches  $(\theta, a)$  and the outside option of non-employment is given by

$$\partial_a \theta = 1 - \frac{\partial_a u}{\partial_y u} < 1.$$

Note also that (in the myopic limit) non-employment is equivalent to a match  $(\theta, a) = (0, 0)$ , so that the indifference curve between matches and non-employment goes through the origin. This implies the following comparative statics of job acceptance with respect to an increase of basic income  $b$ .

**Proposition 3** (Comparative statics of job acceptance). *Under the assumptions of the search model with exogenous amenities and on-the-job search, consider a marginal increase of  $b$ .*

*The slope  $\partial_a \theta$  of the indifference curve relative to non-employment is decreasing in  $b$ . Therefore there exist matches with  $a > 0$  which are not accepted prior to an increase of  $b$ , but are accepted after the increase, and reversely for some matches with  $a < 0$ .*

The proof of Proposition 3 can once again be found in Appendix A. We have characterized transitions of the unemployed to employment. What about on-the-job transitions? Workers are indifferent between different matches  $(\theta, a)$  yielding the same flow utility  $u(w(\theta, a, b) + b, a) = \bar{u}$ .

Because marginal utility is positive for both income and amenities, indifference requires higher income  $w + b$  for matches with lower amenities  $a$ . Because marginal utility is decreasing in income, and holding  $w$  fixed, the effect on utility of an increase of basic income  $b$  is smaller for those with higher income  $w + b$ . This would suggest that an increase of basic income makes jobs with higher amenities and lower wages relatively more desirable. It can be expected that the same holds after taking into account the endogenous adjustment of wages, so that an increase of basic income makes matches with higher  $a$  and lower  $\theta$  relatively more desirable, leading to corresponding job-to-job transitions, but the specifics depend on the functional form of  $u$ .

To summarize: An increase of basic income shifts the relative value of different matches, and the value of matches relative to the outside option of unemployment. Under plausible assumptions, there is an income effect, where increased basic income raises the value of job amenities, relative to the value of match productivity. This in turn impacts both the acceptance decisions of the unemployed, and potential job transitions of the employed (in the presence of on-the-job search). The extent to which either effect is present is again a question that we will investigate empirically.

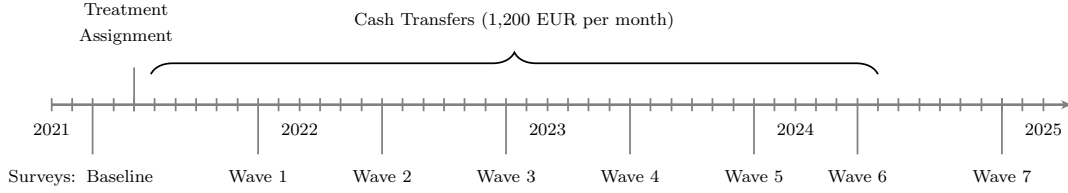


Figure 2: Timeline of the RCT

### 3 The basic income experiment

We estimate the effect of a basic income program on recipients’ labor market outcomes in a pre-registered (<https://www.socialscisceregistry.org/trials/7734>) randomized controlled trial (RCT) in Germany. The following description is based on this pre-registration. The German NGO *Mein Grundeinkommen*, which is funded through small private donations, paid for the cash transfers.<sup>5</sup> Treated participants in our experiment received tax-free cash transfers of EUR 1200, paid monthly, over the course of three years. There were no conditions attached to receiving the cash transfers, apart from completing six semi-annual online surveys.

**Timeline and sample construction** The timeline of our experiment and data collection is depicted in Figure 2. We advertised the RCT in a public call in August 2020. 2,048,370 applicants registered online and filled out a brief survey collecting basic socio-economic variables. From this set of applicants, we constructed a baseline sample of 20,000 eligible candidates. Applicants were eligible if, at baseline, (i) they were between 21 and 40 years of age, (ii) had a personal, monthly income between EUR 1,100 and 2,600, (iii) were not unemployed for more than one year (if at all) and (iv) lived in households of size one.

**Experimental design: Block randomization** From the set of applicants, 20,000 were invited to complete a baseline survey. There were 8,971 respondents to this invitation who satisfied the eligibility criteria and provided sufficient demographic information. These were then sorted into homogeneous blocks of 32 observations each. Blocks were chosen to minimize the total sum of distances between pairs of observations within blocks, where distance is measured as the Mahalanobis distance on 28 baseline variables. From these blocks, 53 were selected, to maximize representativeness for the target population. Within each block, 2 individuals were then randomly assigned to receive 1200 Euro monthly for three years (treatment), and the remainder to the control group, resulting in 107 treated and 1,580 control participants. Further details on sampling and treatment assignment can be found in Appendix B. Table 1 shows average outcomes and balance tests for the covariates used in treatment assignment. Section B.3 in the appendix provides a more detailed description of these variables.

<sup>5</sup>Prior to the RCT, *Mein Grundeinkommen* made regular cash transfers of EUR 1000 per month for a single year, which were allocated by lottery; we do not have access to these prior transfers and hence do not evaluate

Table 1: Balance of baseline covariates in the study sample

Outcome	Treated	Control	Difference	SE	t-statistic	p-value
Age 29-32	0.355	0.331	0.024	0.048	0.498	0.619
Age 33-40	0.336	0.373	-0.036	0.048	-0.757	0.449
Female	0.477	0.412	0.065	0.050	1.290	0.197
German citizen	0.981	0.981	0.000	0.014	0.014	0.989
UBI proponent	0.505	0.547	-0.042	0.050	-0.837	0.403
Tenure	0.766	0.766	0.000	0.043	0.005	0.996
Education: Hauptschule	0.037	0.038	0.000	0.019	-0.020	0.984
Education: Realschule	0.215	0.214	0.001	0.041	0.035	0.972
Education: Fachabitur	0.243	0.241	0.002	0.043	0.044	0.965
Education: Abitur	0.037	0.054	-0.016	0.019	-0.843	0.399
Net monthly income	1944.888	1925.767	19.121	40.181	0.476	0.634
Monthly saving	271.607	296.407	-24.800	24.742	-1.002	0.316
Wealth	25327.103	25392.157	-65.054	4450.093	-0.015	0.988
Debt	10170.374	9077.122	1093.252	2655.173	0.412	0.681
High financial security	0.327	0.312	0.016	0.047	0.329	0.742
Working for money	0.935	0.944	-0.010	0.025	-0.383	0.702
In training or education	0.178	0.151	0.027	0.038	0.691	0.489
In vocational training	0.411	0.432	-0.021	0.050	-0.421	0.674
Searching work	0.037	0.038	0.000	0.019	-0.020	0.984
Sick days	7.776	10.850	-3.075	1.152	-2.669	0.008
Weekly hours worked	37.826	37.346	0.480	1.458	0.329	0.742
Political preferences (PC1)	0.015	0.142	-0.127	0.142	-0.893	0.372
Political preferences (PC2)	0.164	0.053	0.112	0.125	0.893	0.372
Subjective wellbeing (PC1)	-0.360	-0.129	-0.231	0.183	-1.263	0.207
Body mass index	24.656	25.452	-0.797	0.490	-1.627	0.104
Transfers to others	363.551	330.733	32.819	103.753	0.316	0.752
Donations in 2020	100.664	96.562	4.101	21.002	0.195	0.845
Binary gender	1.000	1.000	0.000	0.000	—	—

*Notes:* This table shows averages of baseline covariates for the treated and control group in our study sample, as well as their difference. Section B.3 in the appendix provides a more detailed description of these variables. The table additionally shows “naive” standard errors (ignoring blocked assignment), as well as the corresponding t-statistic and p-value. As this table shows, we were able to achieve a very high degree of balance for almost all variables.

This block-randomized design allows us to (i) minimize sampling variability, by ensuring similarity of treated and control individuals within each block, while (ii) still allowing us to calculate standard errors (since there are 2 treated observations in each block). As can be seen in Table 1, this procedure resulted in a well-balanced assignment, where treatment and control group are very similar in terms of pre-determined covariates: Sick days are the only variable that has a t-statistic larger than 2.

**Administrative and survey data** The German Institute for Employment Research provided administrative data for all those participants in the experiment who agreed to merge their survey data with administrative data (99 participants in the treated group, and 1278 in the control group). The Integrated Labor Market Biographies (Schmucker and Vom Berge, 2025) consist of data reported by employers to the social security system. The dataset also provides daily records in spell format on employment status (full-time, part-time, registered unemployed and active labor market policy program participation), daily gross wages, unemployment benefits, geographical information on the district of residence as well as the district of work, and an identifier for the business establishment for employed individuals. The data do not include information on civil servants or self-employed persons.

In addition to these administrative data, we also collected survey data on a range of outcomes. Treatment recipients were required to fill out six semi-annual surveys. Members of the control group were similarly asked to complete these surveys. For every completed survey, control participants received an incentive payment of EUR 10, plus an additional payment of EUR 30 if they completed all six surveys. This allowed us to limit attrition, and led to response rates of about 80-90% for each wave, where 81% of participants completed at least 4 waves of the survey. A professional survey provider implemented the surveys and was in contact with the respondents, which ensured that respondents were not in direct contact with *Mein Grundeinkommen* and reduced the risk of experimenter demand effects. We also invited treatment and control members to participate in a final survey six months after the cash transfer program. All participants received EUR 20 for completing the final survey.

**Estimation and inference** We next briefly describe our estimators and inference procedures. We denote individual treatment status by  $D$  and outcomes by  $Y$ . Throughout, our primary object of interest is the sample average treatment effect  $\Delta = \sum_i (Y_i^1 - Y_i^0)$ , for various individual-level outcomes  $Y_i$  for individuals  $i$ , with corresponding potential outcomes  $Y_i^0, Y_i^1$ .

Our estimates  $\hat{\Delta}$  are based on block-level differences of mean outcomes, averaged across blocks  $b$ :

$$\bar{Y}_b^1 = \frac{1}{n_b^1} \sum_{i: b_i=b} D_i Y_i, \quad \bar{Y}_b^0 = \frac{1}{n_b^0} \sum_{i: b_i=b} (1 - D_i) Y_i, \quad \hat{\Delta}_b = \bar{Y}_b^1 - \bar{Y}_b^0, \quad \hat{\Delta} = \frac{1}{N} \sum \hat{\Delta}_b, \quad (6)$$

---

them here.

where  $n_b^1$  and  $n_b^0$  are the number of treated and untreated individuals in block  $b$ , and  $N$  is the number of blocks.

For our headline results, we define  $Y_i$  (and correspondingly  $Y_i^0, Y_i^1$ ) as *changes* relative to baseline outcomes, prior to treatment assignment. Since baseline outcomes are balanced in expectation, this does not change the definition of the average treatment effect (relative to estimation in levels). Estimation in differences, blockwise random assignment, and estimation using block-level differences all serve to increase the precision of our estimates and the power of our tests. Additionally, these approaches help address any concerns about possible selective non-response in our surveys (which have a very high response rate, in any case) by adjusting for pre-determined heterogeneity.

Inference in this paper is based on two alternative methods, both of which yield valid inference for the sample average treatment effect: Standard errors and confidence intervals based on a normal approximation, and randomization inference. To calculate a *standard error* for  $\hat{\Delta}$ , as an estimator of  $\Delta$ , we calculate block-level standard-errors (allowing for arbitrary heteroskedasticity), and aggregate across blocks:

$$\begin{aligned}\hat{\sigma}_b^{21} &= \frac{1}{n_b^1 - 1} \sum_{i: b_i=b} D_i \cdot (Y_i - \bar{Y}_b^d)^2, & \hat{\sigma}_b^{20} &= \frac{1}{n_b^0 - 1} \sum_{i: b_i=b} (1 - D_i) \cdot (Y_i - \bar{Y}_b^d)^2, \\ \hat{\sigma}_b^2 &= \frac{1}{n_b^1} \hat{\sigma}_b^{21} + \frac{1}{n_b^0} \hat{\sigma}_b^{20}, & \hat{\sigma}^2 &= \frac{1}{N} \sum_b \hat{\sigma}_b^2.\end{aligned}\tag{7}$$

Confidence intervals for  $\Delta$  with 95% (asymptotic) coverage are then calculated as  $CI = [\hat{\Delta} - 1.96 \cdot \hat{\sigma}^2, \hat{\Delta} + 1.96 \cdot \hat{\sigma}^2]$ . (Neyman) p-values are similarly based on these standard errors and the normal approximation for the distribution of  $\hat{\Delta}$ .

Our second method for calculating (Fisher) p-values is based on permutations of treatments, that is, based on *randomization inference*. This approach allows us to test the null hypothesis that the intervention had no effect of any kind, that is,  $Y_i^1 = Y_i^0$  for all individuals  $i$  and potential outcomes  $Y_i^1, Y_i^0$ . We re-assign treatment at random *within* each of the blocks  $b$ . For this counterfactual treatment assignment, we re-calculate any given test-statistic. Repeating this process many times, we can calculate the share of re-assignments for which the test-statistic is bigger than the realized value of the test-statistic. This share is the p-value for the null hypothesis of no effects.

Table 2: Average effects, changes relative to baseline

Outcome	Treated	Control	ATE	SE	t-stat	p-val (N)	p-val (F)	n treated	n control
<b>Government revenue</b>									
Income tax	88.011	129.790	-41.779	24.271	-1.721	0.085	0.102	99	1278
SI contributions (employee + employer)	106.980	156.086	-49.106	37.116	-1.323	0.186	0.212	99	1278
Unemployment benefits	-1.532	-4.145	2.613	12.732	0.205	0.837	0.830	99	1278
Government Revenues	200.719	294.374	-93.655	66.854	-1.401	0.161	0.180	99	1278
<b>Earnings and commute</b>									
Employer costs	314.879	460.125	-145.246	111.294	-1.305	0.192	0.222	99	1278
Net earnings (excl. tax and SI)	104.225	158.054	-53.829	50.593	-1.064	0.287	0.322	99	1278
Distance to employer	-5.193	-1.743	-3.450	6.266	-0.551	0.582	0.588	93	1212
<b>Extensive and intensive margin</b>									
Employed	-0.045	-0.042	-0.003	0.025	-0.104	0.917	0.914	99	1278
Employed full-time	-0.056	-0.005	-0.051	0.027	-1.848	0.065	0.070	99	1278
Employed part-time	-0.005	-0.035	0.030	0.031	0.994	0.320	0.328	99	1278
<b>Job transitions</b>									
Initial employment	-0.050	-0.091	0.041	0.041	0.996	0.319	0.366	99	1278
New employer	0.208	0.248	-0.040	0.033	-1.205	0.228	0.270	99	1278

*Notes:* This table reports estimates of the effect of basic income on outcomes from the IAB administrative data. Outcomes are averaged over the three years during which basic income was disbursed. Monetary outcomes are in Euro per month. Changes are relative to the average outcome over the 12 months preceding the experiment. *Treated* and *Control* columns report average changes in either group. Estimation of the *ATE* uses within-block differences, as discussed in Section 3. Column *p-val (F)* are Fisher p-values based on permutation inference, and *p-val (N)* are Neyman p-values based on asymptotic normality. Table 9 in the appendix reports analogous estimates in levels, rather than changes.

## 4 Empirical findings

We now turn to a discussion of our empirical findings. Table 2 summarizes the effect of basic income receipt on a variety of outcomes. Outcomes are averaged over the three years during which basic income was disbursed, and are measured using administrative data derived from German social security records; see Appendix C for variable definitions. This table reports estimates using *changes* relative to the baseline level of these outcomes, averaged over the 12 months prior to the start of the experiment. Table 9 in the appendix reports analogous estimates in levels. Additional findings from survey data will be discussed below. In the following, we present figures reporting estimates of the effect of basic income receipt on each of the outcomes in Tables 9 and 2, for each month of the experiment.

All effects discussed in this section are estimated using our block-randomized design, and using changes relative to baseline values, as in Table 2. Both block-level estimation and the use of differences relative to the baseline minimize the statistical variability of our estimates. This is reflected in the smaller standard errors in Table 2, using differenced estimation, relative to Table 9, using estimation in levels.

We will first consider the (behavioral) effects  $B$  of basic income on government revenues and expenses. These effects are central for assessing the “excess burden” of basic income, that is, the effective social cost of the program. We then consider the effect on monetary and

Table 3: Average effects, changes relative to baseline, survey evidence

Outcome	Treated	Control	ATE	SE	t-stat	p-val (N)	p-val (F)	n treated	n control
<b>Labor supply</b>									
Not unemployed	-0.009	-0.016	0.007	0.024	0.282	0.778	0.774	107	1477
Self-employed	0.014	0.011	0.003	0.024	0.130	0.897	0.896	107	1477
Working Hours	1.429	2.776	-1.347	1.016	-1.326	0.185	0.192	106	1464
Net earnings	339.241	436.871	-97.630	50.055	-1.950	0.051	0.070	106	1452
Training or education	-0.029	-0.055	0.026	0.027	0.981	0.327	0.328	107	1531

Table 4: Average effects on stated labor supply, levels, survey evidence

Outcome	Treated	Control	ATE	SE	t-stat	p-val (N)	p-val (F)	n treated	n control
<b>Acceptance thresholds</b>									
Net wage to accept new job	2423.872	2496.888	-73.016	83.989	-0.869	0.385	0.446	107	1471
Amenities to accept new job	7.080	7.050	0.029	0.129	0.227	0.820	0.822	107	1476
<b>Search effort</b>									
Job application sent last 6 months	0.975	1.514	-0.539	0.212	-2.545	0.011	0.052	107	1477
Hours per week searching other jobs	0.542	0.895	-0.353	0.140	-2.520	0.012	0.056	107	1477
<b>Current job</b>									
Satisfaction w. amenities	6.300	6.107	0.193	0.148	1.306	0.191	0.200	107	1467
% wage cut before you quit	10.598	10.766	-0.168	0.766	-0.219	0.826	0.836	107	1477
<b>Outside options</b>									
Wage at a new, similar job	128.434	158.975	-30.541	30.983	-0.986	0.324	0.344	107	1474
Weeks to find a new, similar job	7.847	8.139	-0.293	1.092	-0.268	0.789	0.810	107	1477

*Notes:* These tables report estimates of the effect of basic income on stated labor supply from survey data. Outcomes are averaged over the three years during which basic income was disbursed and are reported. Table 3 reports effects on changes relative to the baseline survey 3 months prior to the experiment. Table 4 reports effects on levels, because these outcomes were not collected at baseline.

non-monetary aspects of recipient well-being. This includes the effect on earnings, on leisure time, and on commute distance. As discussed in Section 2, these effects might matter for recipient welfare in a way not captured by the direct mechanical effect of transfers. We finally discuss the effect of basic income on job transitions into and out of employment, and between employers. These job transitions matter for an understanding of the equilibrium labor market impact of basic income.

All these effects are estimated using administrative data. We complement the administrative evidence with survey evidence, which allows us to capture additional dimensions of labor supply, and to validate our administrative estimates with independent measurement instruments.<sup>6</sup> Table 3 reports average effects on the relevant labor market outcomes in our surveys, over the duration of the experiment, while Table 4 reports effects on dimensions of stated labor supply. Figures 8 and 9 below show corresponding estimates for each survey wave.

<sup>6</sup>A more comprehensive analysis of these survey data can be found in our companion paper on the welfare effects of basic income, Bohmann et al. (2025).

**Government revenue and expenses** Consider now the effect of basic income disbursement on government revenue and expenses. Recall from Section 2 that we can decompose this effect into a mechanical component  $M$  and a behavioral component  $B$ . The mechanical component  $M$ , which in our case is borne by the NGO *Mein Grundeinkommen*, is simply equal to the total amount of basic income disbursed, 1200 Euro per month and recipient.

The behavioral component  $B$  is driven by labor supply decisions. This behavioral component matters, from the perspective of optimal taxation, because the “excess burden” of basic income is determined by the magnitude of  $B$ . In the context of the German tax and social insurance system,  $B$  has multiple components, including social insurance contributions, income taxes, and unemployment benefits.

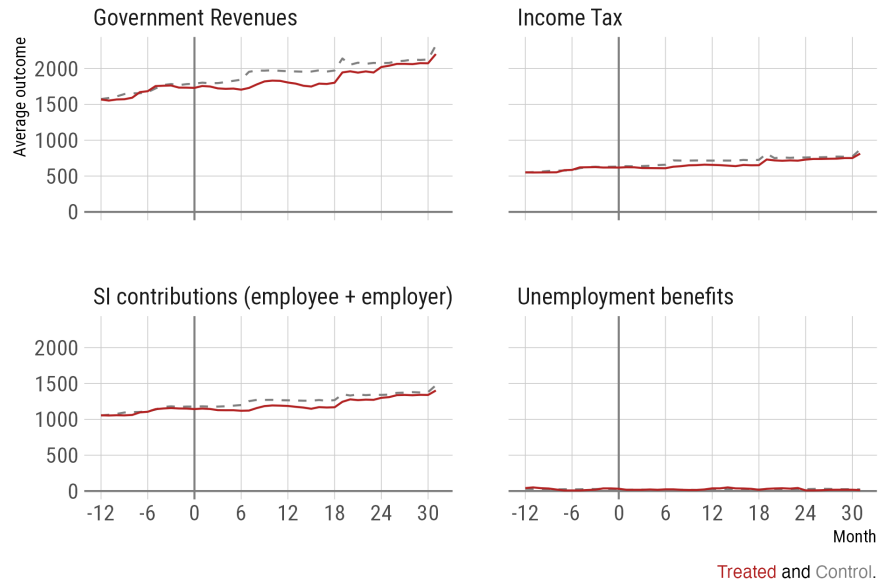
Let us start with *social insurance (SI) contributions*. Earnings, up to a maximum earnings level (which has increased over time) are subject to proportional insurance contributions totalling around 40%; see Appendix C for details. This social insurance contribution is split equally between employees and employers. We report the combined effect on both employee and employer contributions. As can be seen in Table 2 and Figure 3, the estimated average effect on SI contributions is around -49 Euros per month, over the duration of the experiment, with a standard error of 37. As shown by the monthly time series of effects, this average effect is driven by a slightly larger reduction of contributions between month 7 and month 24 of the experiment, while effects at the start and the end were smaller. This is consistent with the implications of dynamic models of labor supply behavior, in the presence of search frictions and foresight. Furthermore, as shown by the time series of the level of contributions, the effect is driven by a (temporarily) slower growth of SI contributions among recipients over time, rather than by a reduction of the level of SI contributions.

Figure 3: Government revenues

EFFECT BY MONTH



AVERAGE IN TREATED AND CONTROL GROUP



*Notes:* This figure plots estimates of the effect of basic income on government revenues, for each month of the experiment. Basic income receipt started at month 0. The top figure shows estimated effects, using within-block differences relative to baseline, with 95% confidence bands based on standard errors calculated as in Equation (7). The bottom figure shows average outcomes for treatment and control group.

Consider next the effect of basic income receipt on *income taxes*. Earnings are subject to a progressive tax schedule, which depends on the household composition of workers. Appendix C describes the parameters of this tax schedule. We estimate an average effect on income tax contributions of participants of around -42 Euro per month, with a standard error of 24.

Consider third, and finally, the effect of basic income on *unemployment benefit receipt*.<sup>7</sup> If basic income leads to an extensive-margin labor supply response, then unemployment benefits might rise. As Figure 3 shows, however, there was a precisely estimated zero effect of basic income on unemployment benefit receipt. Benefit receipt was negligible in both the treatment and control group, and in particular there does not seem to have been any extensive margin labor supply response to basic income receipt. Nobody entered unemployment because of the basic income.

Summarizing and adding up these different margins, we obtain an estimate of  $B$  of around 94 Euro; we cannot statistically exclude an effect of 0. Put differently, every Euro spent on basic income, if it were paid for by the government, would have a cost for the government of around 1.08 Euro. This suggests a fairly small excess burden of basic income, which appears to reflect limited income effects at either the extensive or intensive margin of labor supply.

In the search model discussed in Section 2, the term  $\frac{\partial}{\partial b}t(\theta, b) - 1$  captures the welfare impact on basic income recipients beyond the mechanical effect of basic income. This effect exists due to the presence of search frictions, which create room for bilateral bargaining once a match between employer and worker is formed. Depending on preferences and production technologies, some of the surplus of a match might be shifted either to workers or to employers. Conditional on the surplus going to workers, there might furthermore be a shift between earnings and amenities. We can recover some aspects of these endogenous welfare effects by measuring the impact of basic income on (net) earnings, on hours worked / part-time work, and on non-monetary amenities, including commute distance.

**Net earnings and employer costs** Consider first net earnings, that is earnings after social insurance contributions and income taxes; see Appendix C for the calculation of these. Figure 4 shows the effect on net earnings. We find an average decline of net earnings of 54 Euros per month due to basic income receipt. This effect is, again, not significantly different from 0. Employer costs, which are the sum of net earnings, social insurance contributions, and taxes, decline by 145 Euros. As was the case for social insurance contributions and for income taxes, these effects are concentrated in the middle phase of the experiment, with smaller effects at the beginning and towards the end.

Using survey data, we obtain very similar estimates; see Table 3 and Figure 8. Self reported net earnings are reduced, on average, by 98 Euros per month, with a standard error of 50. The dynamics over time furthermore show a similar pattern to that seen in the administrative data.

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<sup>7</sup>Basic income recipients are legally entitled to unemployment benefits during the first year of unemployment, under the same conditions as other beneficiaries, so that these estimates are in fact meaningful.

Figure 4: Earnings



*Notes:* This figure plots estimates of the effect of basic income on earnings, for each month of the experiment. Basic income receipt started at month 0. The top figure shows estimated effects, using within-block differences relative to baseline, with 95% confidence bands. The bottom figure shows average outcomes for treatment and control group.

**Extensive and intensive margin responses** We now turn to an examination of the impact of basic income on both the intensive and extensive margins of labor supply, starting with the decision to work part time or full time. Figure 5 shows the effect of basic income on the share of recipients working full-time and the share of recipients working part-time. For basic income recipients, the share of days spent in full-time employment was reduced by .05. The share of days spent in part-time employment, however, increased by 0.03. Both effects are again concentrated during the middle of the duration of the experiment, with smaller effects in the first 6 months and the last 6 months.

Between these two opposing movements, total employment does not decline at all.<sup>8</sup> Our

<sup>8</sup>Note that there is a small gap of about 2% of our sample between the sum of part-time and full-time employment, and total employment. This gap is due to workers in so-called “mini-jobs” who are not subject to social insurance payments, but are counted in total employment.

estimated average effect on employment equals  $-.003$ , with a standard error of  $.025$ . The estimated effect by month is furthermore consistently close to 0 throughout the experiment; there are no dynamic adjustments at the beginning or end of this period. This explains the null effect on unemployment benefit receipts discussed above. There are no *extensive* margin labor supply responses to basic income. It is thus only the *intensive* margin shift from full-time to part-time work for about 3% of recipients which explains the small decline in social insurance and tax contributions discussed earlier.

Our survey data again paint a very similar picture. While the administrative social security data do not record hours, only part-time status, we do have reported work hours in the survey. Reported work time is reduced by 1.3 hours per week for basic income recipients.

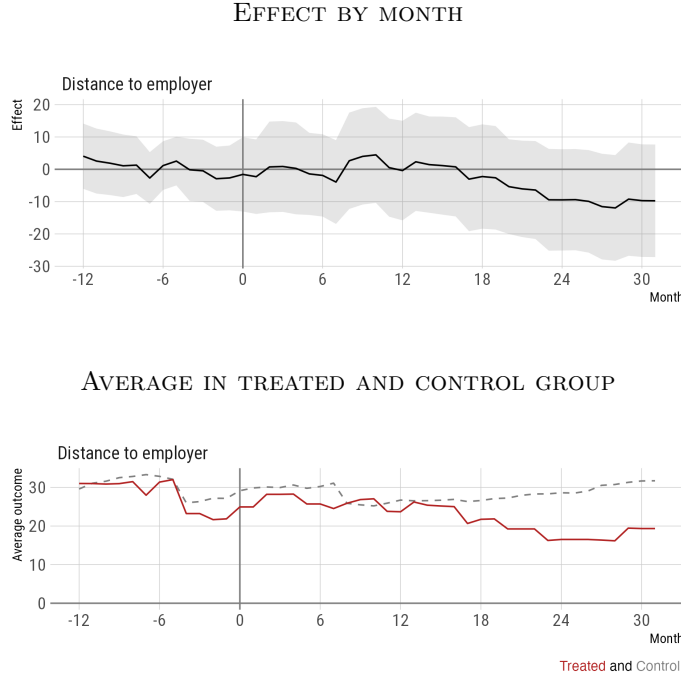
Figure 5: Extensive and intensive margin



*Notes:* This figure plots estimates of the effect of basic income on the extensive and intensive margin of labor supply, for each month of the experiment. Basic income receipt started at month 0. The top figure shows estimated effects, using within-block differences relative to baseline, with 95% confidence bands. The bottom figure shows average outcomes for treatment and control group.

**Distance to work** In the context of the model of Section 2, we might subsume part-time work, and work hours more broadly, as a component of the amenities  $a$  that affect worker utility for a given employment relationship. Another related amenity margin along which jobs might adjust is commute distance, shown in Figure 6. If there are positive income effects on the value of (non-work) time, then we might expect basic income to not only lead to an increase of part-time work, but also to a choice of employers that require a shorter commute. This prediction is consistent with our data, though the estimate is very imprecise: The (straight line) distance between employer location and home address is reduced by 3.4 km, on average (with a large standard error). This effect is furthermore increasing over time, as we would expect in the presence of search frictions in the labor market.

Figure 6: Commute distance



*Notes:* This figure plots estimates of the effect of basic income on commute distance, for each month of the experiment. Basic income receipt started at month 0. The top figure shows estimated effects, using within-block differences relative to baseline, with 95% confidence bands. The bottom figure shows average outcomes for treatment and control group.

**Job transitions** We finally turn to the effect of basic income on job transitions. In the model discussed in Section 2, transitions out of unemployment might be affected by basic income because the reservation match quality  $\underline{\theta}$  is shifted, reflecting income effects on the relative utility of additional income, work amenities, and leisure. As shown in Proposition 1, the sign of this effect is theoretically ambiguous, and depends on the shape of flow utility.

(Such ambiguity would also apply to a model with endogenous search effort.) In our model extension that allows for on-the-job search, income effects might also shift the relative utility of different matches with different combinations of productivity and amenities, and might thus affect on-the-job search. Signs and magnitudes of these effects are again theoretically ambiguous.

Figure 7 shows our empirical estimates of the effect of basic income on job transitions. Consider first the effect of basic income on the probability of being employed at the same employer as at the start of the experiment. It appears that basic income slightly increased the probability of staying with the initial employer. This estimated effect is far from significant, however. Correspondingly, the probability of working with a new employer appears to be slightly reduced by basic income, but again this is far from significant.<sup>9</sup> There is furthermore some catch-up in transitions towards the end of the experiment.

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<sup>9</sup>For both these probabilities we defined the baseline as the outcome at time 0, rather than the average over the prior year, which would not be well-defined for these outcomes.

Figure 7: Job transitions



*Notes:* This figure plots estimates of the effect of basic income on job transitions, for each month of the experiment. Initial employment equals 1 for those who are employed and whose employer ID is equal to that of month 0. New employer equals 1 for those who are employed with a different employer. Basic income receipt started at month 0. The top figure shows estimated effects, using within-block differences relative to baseline (month 0, in this plot), with 95% confidence bands. The bottom figure shows average outcomes for treatment and control group.

**Self-employment and education** The administrative data only capture activities (employment) subject to social insurance contributions. This excludes, in particular, both self-employment and education or training. Here again the survey evidence helps.

As shown in Table 3 and Figure 8, there was no average increase of the share of recipients in self-employment, over the duration of the experiment. That said, Figure 8 suggests a possible rise of this share over time. By contrast, there was a (statistically insignificant) increase in the share of recipients who were in training or education. There appears to be an initial spike education or training by recipients, followed by slower increases over later periods. Conceivably, basic income enabled recipients to stay in educational programs longer than they otherwise

would have, explaining the more pronounced initial effect.

Figure 8: Labor outcomes, survey



*Notes:* This figure plots estimates of the effect of basic income on labor outcomes from our surveys, for each wave of the survey. Basic income receipt started at month 0. The plots show estimated effects, using within-block differences relative to the baseline survey (at month -3), with 95% confidence bands.

**Stated labor supply** We have thus far focused on actual labor supply decisions. To gain further insight into the underlying preferences, beliefs, and evaluations motivating these decisions, we asked survey respondents a range of subjective questions regarding their labor supply. Treatment effects on these variables are shown in Table 4 (average over the treatment period) and Figure 9 (separately for each wave). Because these questions were not asked in the baseline survey, the corresponding effects are estimated in levels, rather than differences.

The first two outcomes describe acceptance thresholds for accepting a new job: In order to accept a new job, what wage level would you need to accept, or how satisfied would you have to be with the amenities? These questions map onto our discussion at the end of Section 2. We conjectured that a basic income would lead to a greater preference over jobs with higher amenities  $a$  (and lower wages  $w$ ), relative to lower amenities (and higher wages). The signs of the corresponding effects in Table 4 appear to confirm this hypothesis, but the effects are not

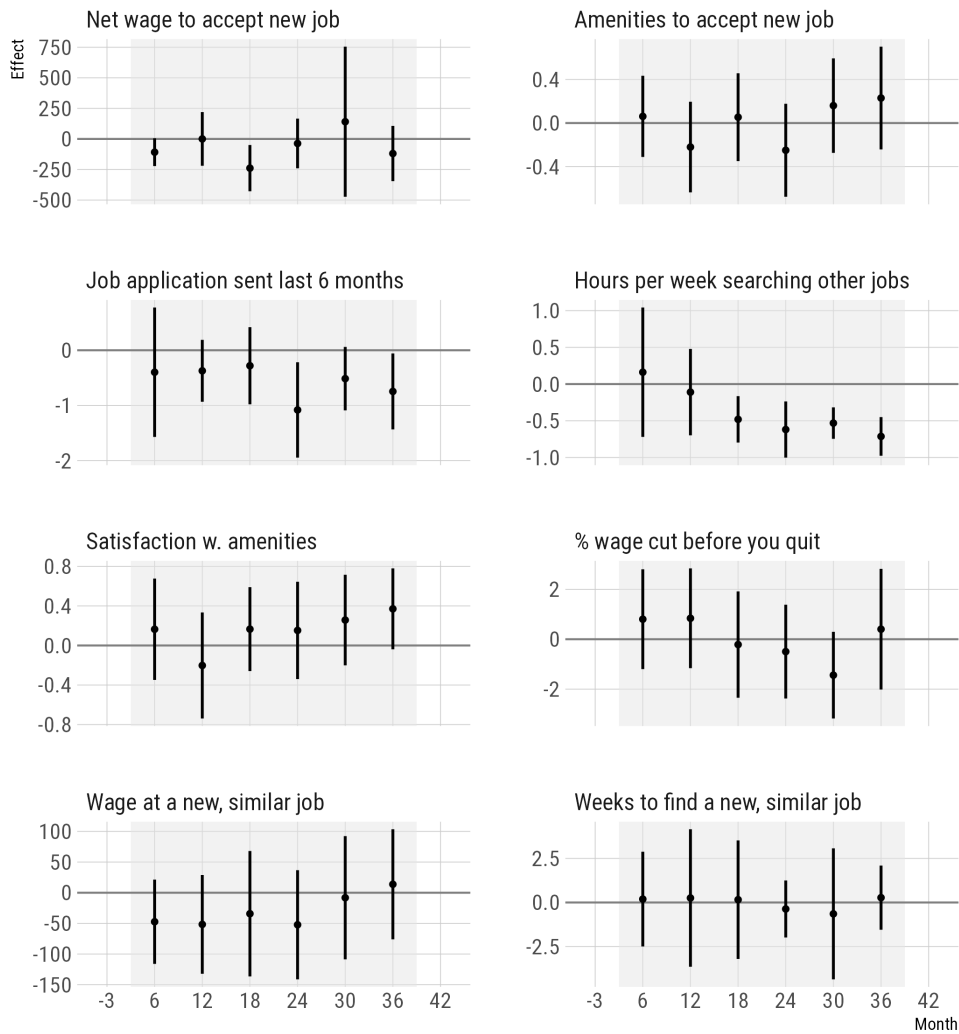
significant, and seem to fluctuate over time.

The next two questions correspond to on-the-job search behavior, reflected in the rate  $\lambda^e$  in our model. We find that basic income receipt reduces search, both in terms of the number of applications sent, and in terms of the amount of time spent searching. These effects are increasing over time. These effects are statistically significant, and align with our estimates discussed above indicating that basic income (slightly) reduced the probability of transitioning to new employers.

The following two questions describe respondents' assessment of their current job. Satisfaction with amenities appears to increase (insignificantly), which might suggest renegotiation or internal job transition, driven by the income effect of basic income. This effect is increasing over time. The acceptable wage cut before quitting might be interpreted as a measure of the match surplus  $t$  relative to the outside option. There appears to be little effect on this measure. Taken at face value, this suggests that the welfare impact of basic income is, in fact, close to its mechanical impact; cf. Proposition 2.

The last two questions describe respondents' assessment of their outside options. These subjective beliefs might be important for bargaining, and have received some attention in the recent literature on employer monopsony power (e.g. Jäger et al. 2024). The treated group appears to expect a shorter search duration to find a new job, but also lower wages (both insignificant), which might again reflect income effects.

Figure 9: Stated labor supply



*Notes:* This figure plots estimates of the effect of basic income on stated labor supply, preferences, and beliefs from our surveys, for each wave of the survey. Basic income receipt started at month 0. The plots show estimated effects, using within-block differences and *levels* of outcomes, with 95% confidence bands.

## 5 Conclusion

Is the introduction of a basic income, involving regular, unconditional and guaranteed cash payments, desirable? This question has caused much controversy. Part of the controversy relates to the labor supply effect of a potential basic income: Would the introduction lead to a decline of labor market participation, and a corresponding decline of earnings tax and social insurance contributions? Or would it, reversely, lead to an increase in labor supply that allows employers to lower wages and absorb part of the transfer? In this paper we shed light on this aspect of the debate, discussing the impact of basic income on labor market outcomes both theoretically and empirically, in the context of a randomized controlled trial that we designed and evaluated in Germany.

Our findings broadly align with those of the prior literature estimating the impact of unconditional cash transfers on labor supply in high income countries (e.g, Marinescu 2018; Jones and Marinescu 2022; Vivalt et al. 2024; Verho et al. 2022): Basic income does not lead to an extensive margin response on labor market participation. It however leads to a (small and statistically insignificant) intensive margin response, with a (small) rise of part-time work and a (small) decline of weekly working hours. Correspondingly, we estimate an excess burden of declining government revenues of around 7.5% of the value of the cash transfer. Contrary to what our theoretical considerations suggested, we do not observe an increase of job-to-job transitions towards jobs with higher amenities and lower wages.

These findings suggest that neither concerns about the excess burden of a basic income, due to declining labor supply, nor concerns about the incidence of a basic income, due to increasing labor supply, are born out in practice – at least in the context and for the population from which our study sample was drawn. This assessment might change, of course, if (i) basic income were scaled up to the population level, instead of a small sample, so that general equilibrium effects would matter, and (ii) depending on the nature of taxes used to finance such a basic income. Future policy pilots at a larger scale might help to speak to these questions.

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## A Proofs

### A.1 Proof of Proposition 1 (Theoretical ambiguity of comparative statics)

To prove these claims, we first characterize the model in the myopic limit of high discounting and/or low transition rates, for general  $u$ . In this limit, value functions reduce to flow utilities. We then consider the CES functional form for  $u$ .

**Myopic limit** We can solve the Bellman equations to express the value functions in terms of flow utilities:

$$\begin{aligned}\rho V_n(b) &= (1 - A) \cdot u(b, 0) + A \cdot E[u(w + b, a) | \theta > \underline{\theta}(b)], \\ \rho E[V_e(\theta, b) | \theta > \underline{\theta}(b)] &= (1 - B) \cdot u(b, 0) + B \cdot E[u(w + b, a) | \theta > \underline{\theta}(b)],\end{aligned}$$

where

$$A = \frac{\tilde{\lambda}(b)}{\rho + \tilde{\lambda}(b) + \eta}, \quad B = \frac{\rho + \tilde{\lambda}(b)}{\rho + \tilde{\lambda}(b) + \eta}.$$

Taking the limit as  $\rho \rightarrow \infty$  gives  $A \rightarrow 0$  and  $B \rightarrow 1$  (since  $\tilde{\lambda}(b)$  is upper bounded by  $\lambda$ ), so that

$$\begin{aligned}\rho V_n(b) &\rightarrow u(b, 0), \\ \rho E[V_e(\theta, b) | \theta > \underline{\theta}(b)] &\rightarrow E[u(w + b, a) | \theta > \underline{\theta}(b)],\end{aligned}$$

and thus

$$\rho V_e(\theta, b) = u(w(\theta, b) + b, a(\theta, b)) + \eta \cdot (V_n(b) - V_e(\theta, b)) \rightarrow u(w(\theta, b) + b, a(\theta, b)).$$

Correspondingly, the limit of the first order condition for  $t(\theta, b)$  is given by

$$v'(t) = \frac{1 - \alpha}{\alpha} \left( \frac{v(t) - u(b, 0)}{\theta + b - t} \right). \quad (8)$$

The minimal productivity for which a job is accepted solves  $V_e(\underline{\theta}(b), b) = V_n(b)$ . In the limit, therefore,

$$u(b, 0) = v(\underline{\theta}(b) + b).$$

For the following examples, we consider worker utility functions of the form  $u(y, a) = y^\gamma + a^\delta$ , for  $\gamma, \delta \leq 1$ .

**(I) No income effects, declining marginal utility of amenities** ( $\gamma = 1, \delta \leq 1$ ) Assume that  $u(y, a) = y + a^\delta$  for some  $\delta \leq 1$ , so that there are no income effects (as in the standard model discussed in Pissarides 2000). Under this assumption,  $u(b, 0) = b$ . Efficiency of the bargaining solution implies the first order condition  $\delta a^{\delta-1} = 1$ , and thus the optimal level of  $a$  is independent of  $t$  and equal to  $\bar{a} = \delta^{1-\delta}$ . This implies  $v(t) = t - \bar{a} + \bar{a}^\delta$ ,  $v'(t) = 1$ , and  $v(t) - u(b, 0) = t - b - \bar{a} + \bar{a}^\delta$ . The job acceptance threshold is then given by

$$\underline{\theta}(b) = \bar{a} - \bar{a}^\delta.$$

Employer cost  $t = y + a$  is finally given by the solution to the bargaining first order condition

$$\frac{\alpha}{1-\alpha} = \left( \frac{t - b - \bar{a} + \bar{a}^\delta}{\theta + b - t} \right).$$

Since the left hand side does not depend on  $b$ , given  $\theta$ , neither does the right hand side. Therefore  $\partial_b t(\theta, b) = 1$ . Basic income thus has no impact on the labor market. There is, furthermore, no heterogeneity of amenities  $a$  across matches of different quality  $\theta$ .

**(II) Declining marginal utility of income** ( $\gamma < 1, \delta = 1$ ) Assume now instead that, for some  $\gamma < 1$ ,  $u(y, a) = y^\gamma + a$ . Under this assumption,  $u(b, 0) = b^\gamma$ . Efficiency of the bargaining solution implies the first order condition  $\gamma y^{\gamma-1} = 1$ , and thus

$$y = \bar{y} = \gamma^{-\frac{1}{1-\gamma}}, \quad v(t) = t - \bar{y} + \bar{y}^\gamma, \quad v(t) - u(b, 0) = t - b^\gamma - \bar{y} + \bar{y}^\gamma$$

$t$  is given by the solution to the bargaining first order condition

$$\frac{\alpha}{1-\alpha} = \left( \frac{t - b^\gamma - \bar{y} + \bar{y}^\gamma}{\theta + b - t} \right),$$

and thus

$$t(\theta) = \alpha \cdot (\theta + b) + (1 - \alpha) \cdot (b^\gamma + \bar{y} - \bar{y}^\gamma).$$

In particular,

$$\partial_b w = -1, \quad \partial_b a = \partial_b t, \quad \partial_b t = \alpha + (1 - \alpha) \cdot \gamma \cdot b^{\gamma-1}.$$

Any increase in the basic income will be compensated by a corresponding decrease in wages. For small  $b$ ,  $\partial_b t > 1$ , so that the improved outside option leads to a redistribution of match surplus from the employer to the worker. This redistribution is on top of the increased worker utility due to the direct effect of the basic income. For larger  $b$ , this is reversed, and surplus is redistributed from the worker to the employer. Ultimately a share of  $1 - \alpha$  of the basic income is absorbed by the employer.

Consider next the dependence of the cutoff  $\underline{\theta}(b)$  on  $b$ . Specializing  $u(b, 0) = v(\underline{\theta}(b) + b)$ , we

get  $b^\gamma = \underline{\theta}(b) + b - \bar{y} + \bar{y}^\gamma$  and thus

$$\underline{\theta}(b) = b^\gamma - b + \bar{y} + \bar{y}^\gamma,$$

which is similarly increasing in  $b$  for small  $b$ , but decreasing for larger  $b$ .  $\square$

## A.2 Proof of Proposition 2 (Welfare effect of basic income)

Recall that, by definition,  $v(t(\theta, b)) = u(w(\theta, b) + b, a(\theta, b))$ . With this notation, the Bellman equations (3) can be written as

$$\begin{aligned} \rho V_n(b) &= u(b, 0) + \lambda \cdot E[(V_e(\theta, b) - V_n(b)) \cdot \mathbf{1}(\theta > \underline{\theta}(b))] \\ \rho V_e(\theta, b) &= v(t(\theta, b)) + \eta \cdot (V_n(b) - V_e(\theta, b)). \end{aligned}$$

The reservation match quality  $\underline{\theta}(b)$  is a choice variable of the non-employed worker. By optimality of  $\underline{\theta}(b)$ ,  $E[(V_e(\theta, b) - V_n(b)) \cdot \mathbf{1}(\theta > \underline{\theta})]$  is maximized at  $\underline{\theta} = \underline{\theta}(b)$ , and thus

$$\partial_{\underline{\theta}} E[(V_e(\theta, b) - V_n(b)) \cdot \mathbf{1}(\theta > \underline{\theta})] \big|_{\underline{\theta} = \underline{\theta}(b)} = 0.$$

(This is an instance of the envelope theorem, Milgrom and Segal 2002.) Therefore

$$\begin{aligned} \rho \cdot \partial_b V_n(b) &= \partial_b u(b, 0) + \lambda \cdot E[(\partial_b V_e(\theta, b) - \partial_b V_n(b)) \cdot \mathbf{1}(\theta > \underline{\theta}(b))] \\ \rho \cdot \partial_b V_e(\theta, b) &= v'(t(\theta, b)) \cdot \partial_b t(\theta, b) + \eta \cdot (\partial_b V_n(b) - \partial_b V_e(\theta, b)). \end{aligned}$$

Solving for  $\rho \cdot \partial_b V_n(b)$  and  $\rho \cdot E[\partial_b V_e(\theta, b) | \theta > \underline{\theta}(b)]$ , and recalling the definitions  $\tilde{\lambda}(b) = \lambda \cdot P(\theta > \underline{\theta}(b))$ ,  $A = \frac{\tilde{\lambda}(b)}{\rho + \tilde{\lambda}(b) + \eta}$  and  $B = \frac{\rho + \tilde{\lambda}(b)}{\rho + \tilde{\lambda}(b)}$  yields the claim.  $\square$

## A.3 Proof of Proposition 3 (Comparative statics of job acceptance)

Consider the indifference curve relative to non-employment, with slope  $\partial_a \theta = 1 - \frac{\partial_a u}{\partial_y u}$ . An increase of basic income  $b$  does not affect the numerator  $\partial_a u$ , given separability of  $u$ , but it does decrease the denominator  $\partial_y u$ , given concavity of  $u$  in income. Therefore  $\partial_a \theta$  is decreasing in  $b$ : For higher basic income, a greater increase in productivity is required to compensate for a reduction in amenities. The indifference curve to non-employment necessarily crosses the origin  $(\theta, a) = (0, 0)$ . Since the slope  $\partial_a \theta$  is decreasing in  $b$ , there exist matches with  $a > 0$  which were not previously accepted but now are, and reversely for matches with  $a < 0$ .  $\square$

## B Sampling and treatment assignment

This section is an extract of our pre-analysis plan, as pre-registered at <https://www.socialscisceregistry.org/trials/7734>. In the following we describe in detail the multi-step sampling and treatment assignment procedure used to construct our study sample. The steps in this procedure are (i) a public call and voluntary registration of potential participants, (ii) selection of a subsample based on demographic and economic eligibility criteria, (iii) stratified sampling of eligible registrants to construct a representative baseline sample, members of which were then invited to fill out a longer baseline survey, (iv) blocking of participants in the baseline sample who have a completed survey, based on a rich set of baseline covariates, and random assignment to treatment within blocks, and (v) selection of a representative subsample of blocks based on the budget constraints of the study.

### B.1 Sampling

**Signup call and registrations** In August 18, 2020, MG and the German Institute for Economic Research (DIW Berlin) publicly announced the launch of the RCT during Spring/Sommer 2021 and made a public call to register to participate in the RCT. The announcement included a description of the main features of the study: Selected participants of the study would be randomly assigned to a treatment group or a control group; treatment and control groups would participate in biannual online surveys; members of the treatment group would receive monthly payments of 1,200.00 EUR for three years; members of the control group would receive monetary incentives to complete the surveys; additional research activities may be offered. During signup, we collected the following demographic and socioeconomic information: Age, gender, education, monthly net income, number of people living in their household, number of kids, zip code, and their general attitude towards universal basic income. Between August 18 and December 10 in 2020, 2,048,370 potential participants registered in response to this public signup call.

**Eligibility criteria** We then invited a subsample of registered individuals (called “baseline sample”) to complete the baseline survey. Selection into the baseline sample is based on the following eligibility criteria with respect to participants’ demographic and socioeconomic characteristics. These eligibility criteria were largely determined by our implementation partner, MG.

1. Participants have to be between 21 and 40 years old.
2. Households of size greater than one, and individuals with dependent children, are excluded from participation.

Participants of our study whose household size changes, or who have a child, will, however, not lose their participation status.

3. Participants are required to be German residents and to have a monthly net income between 1,100.00 and 2,600.00 EUR.
4. Individuals who, at the time of the baseline survey, were receiving social benefits for long term unemployment are excluded from participation.<sup>10</sup>

Participants of our study who transition to unemployment and receipt of social transfers will, however, not lose their participation status.

**Baseline sample** Among the potential participants who satisfied these criteria, our implementation partner next sampled 20,000 individuals who were invited to participate in a baseline survey. Sampling of these individuals was based on the following criteria. First, the sample was supposed to contain an equal number of proponents and opponents of a universal basic income. Second, potential participants in both of these groups were sampled using a weighted sampling procedure to generate a sample that is close to being representative for the (eligible) German population, and similar across both groups, in terms of age, gender, income, education, employment status, and state (“Bundesland”).<sup>11</sup>

**Baseline survey** Before the invitations to the baseline survey were sent out, one person requested to be excluded from the RCT. The baseline survey resulted in 14,420 completed surveys. Of the remaining invitations,

- 51 invitations were sent to recipients with multiple registrations. These participants were in turn excluded since potential participants were allowed to register only once.
- 3,359 invitations were sent to recipients who subsequently never started the baseline survey.
- 328 invitations were sent to recipients who then started but did not complete the baseline survey.
- 1,841 recipients completed the survey, but did not sign the required data sharing consent forms.

Amongst the 14,420 individuals who completed the baseline survey and gave consent, 8,971 participants are considered in the randomized block assignment discussed next. The remaining 5,449 individuals are dropped because their eligibility status with respect to their characteristics listed above in criteria 1-4 changed and/or they had missing responses in baseline variables that were used in the randomized block assignment.<sup>12</sup>

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<sup>10</sup>Given current benefit eligibility rules, such social benefits would have been cut by up to the full amount of the cash transfer by MG, if these individuals were to participate in our study. The net transfer to such individuals would thus have been significantly below the expenditure for MG.

<sup>11</sup>The exact sampling procedure is unknown to us. This does not affect, however, the internal validity or correctness of inference for the study design described below.

<sup>12</sup>Additionally, our implementation partner selected a group of 15 individuals who will be treated (that is,

## B.2 Blocking and treatment assignment

**Blocking** We use the answers to the baseline survey to sort participants into homogenous blocks. Pairwise distances between observations are calculated using the Mahalanobis distance.<sup>13</sup> We construct blocks containing 32 observations each. The blocks are chosen to minimize the total sum of distances between pairs of observations within blocks. We do so using the R package *blockTools* (Moore and Schnakenberg, 2016). We then discard all blocks with a maximum within-block distance greater than 14 (to avoid poorly matched observations), as well as one block with less than 32 observations.

**Random assignment within blocks** Within each block, treatment is assigned uniformly at random. We assign 2 out of the 32 observations in a block to the treatment group, 26 observations to the control group, and the remaining 4 observations to a “reserve,” which is to be sampled in case of attrition of observations from the treatment or control group.

These numbers are chosen based on the following considerations: We want two treated units per block, in order to be able to calculate standard errors for the sample average treatment effect; cf. Athey and Imbens (2017) and our discussion of inference below. We don’t want more treated units per block, to keep blocks as homogenous as possible. The budget constraints of our implementation partner are furthermore such that we can survey 13 control units for every treated individual.

Lastly, because we have 107 treated individuals in total (an odd number), one additional individual from one block is chosen at random to participate in the treatment.

**Weighted sampling of blocks** This procedure results in 273 blocks, while our project budget allows for 53 blocks. These blocks are furthermore not fully representative for the baseline sample, because not all individuals who were invited to participate in the baseline survey passed eligibility and had non-missing responses in the questions we used for blocking (see above) and because of our discarding of poorly matched blocks.

In order to obtain a representative sample of blocks, we create block level sampling weights. These weights are chosen so as to match the distribution of gender, education groups, and income groups of eligible participants in the screening survey. We then draw a sample of 53 blocks from the 273 available blocks using these sampling weights, to obtain a representative subsample. This results in 107 individuals assigned to treatment, 1377 assigned to the control group, and 212 individuals assigned to the “reserve,” distributed evenly across 53 blocks.

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who will receive the basic income). These additional individuals indicated in the baseline survey that they were willing to participate in qualitative surveys (which are not conducted by the authors of this preregistration and are not part of this preregistration) and in interviews with journalists to publicly share their own experiences with the basic income *during* the RCT. Since any public appearance of these participants *may* bias their responses in our online surveys, we exclude these “media participants” from our study.

<sup>13</sup>The Mahalanobis distance of two covariate vectors  $x_1$  and  $x_2$  that are realizations of a random vector  $X$  is given by  $d(x_1, x_2) = \sqrt{(x_1 - x_2) \cdot \text{Var}(X)^{-1} \cdot (x_1 - x_2)}$ .

**The resulting treatment assignment** Table 1 in the main text summarizes the resulting study sample. The second and third columns show covariate averages for the 28 covariates used for blocking, for the treated and control group. This table drops observations in the reserve. The remaining columns show standard errors, confidence intervals, and p-values as discussed below.<sup>14</sup> As can be seen from this table, we have achieved an extraordinary degree of balance between the treated and control group. At this point, the selected participants in the treatment group and control group were informed about their treatment status. 7 individuals in the control group wanted to be excluded from the study sample, 1 individual in the treatment group resigned his/her spot in the treatment group because of a job opportunity outside of Germany, and 1 individual in the treatment group could not be reached. For each of these missing individuals, we sampled one individual from the replacement sample within the same block, to receive the corresponding treatment status.

### B.3 Baseline variables used for treatment assignment

- Age 29-32: Dummy, 1 if individuals' age is between 29 and 32 years, 0 if individuals' age is below 29 or above 32 years.
- Age 33-40: Dummy, 1 if individuals' age is between 33 and 40 years, 0 if individuals' age is below 32 years.
- Female: Dummy, 1 if individuals' gender is female, 0 if individuals' gender is not female.
- German citizen: Dummy, 1 if individual is a german citizen, 0 if not
- UBI proponent: Dummy, 1 if individuals' general attitude towards universal basic income is positive, 0 if it is negative.
- Tenure: Dummy, 1 if the individual has (at least one) tenured job, 0 if the individual has no tenured job.
- Education: Hauptschule: Dummy, 1 if highest education level qualifies for vocational training, 0 if not.
- Education: Realschule: Dummy, 1 if highest education level qualifies for high school, 0 if not.
- Education: Fachabitur: Dummy, 1 if highest education level qualifies for vocational academy, 0 if not.
- Education: Abitur: Dummy, 1 if highest education level qualifies for university, 0 if not.  
(Note that the omitted education category is college or more.)

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<sup>14</sup>Inference should not be taken literally here, and is only including for illustration. In particular, because of our blocked assignment procedure, which aims for balance, p-values are expected to be systematically larger than suggested by the uniform distribution under the "null" of no effect.

- Net monthly income: net monthly income available to the individual.
- Monthly saving: amount of money saved per month.
- Wealth: individuals' level of wealth.
- Debt: individuals' level of debt.
- High financial security: Dummy, 1 if individual states that she could finance herself (with help of others but absent social security benefits) for one year without receiving any income, 0 if not.
- Working for money: Dummy, 1 if individual works and receives a financial compensation in return, 0 if not.
- In training or education: Dummy, 1 if individual is in vocational training or receives higher education (undergraduate, graduate, or doctoral level), 0 if not.
- In vocational training: Dummy, 1 if individual is in vocational training, 0 if not.
- Searching work: Dummy, 1 if looking for a job, 0 if not looking for a job.
- Sick days: number of workdays missed because of health.
- Weekly hours worked: number of hours worked per week
- Political preferences (PC1): first component of a principle component analysis that is based on an individual's response to how likely (in percent) it is that they vote for either party currently in the German parliament.
- Political preferences (PC2): second component of a principle component analysis that is based on an individual's response to how likely (in percent) it is that they vote for either party currently in the German parliament.
- Subjective wellbeing (PC1): first component of a principle component analysis that is based on an individual's responses to questions related to several dimensions of their subjective well-being (life satisfaction, emotional wellbeing, depression, eudaimonie, and subjective health).
- Body mass index.
- Transfers to others: how much money did the individual give to family members or friends (or others) in 2020.
- Donation in 2020: how much money was donated in 2020.
- Binary gender: Dummy, 1 if binary gender, 0 if not

Table 5: Month correspondence

Month in	experiment	Calendar Year
$(-\infty,$	$-16]$	2019
$(-16,$	$-5]$	2020
$(-4,$	$7]$	2021
$(8,$	$19]$	2022
$(20,$	$31]$	2023
$(32,$	$36]$	2024

## C General description of administrative data

The German Institute for Employment Research provided administrative data for all participants in the experiment who agreed to merge their survey data with administrative data. The Integrated Employment Biographies (Schmucker and Vom Berge, 2025) consist of daily spell data reported by employers to the social security system, data on the receipt of unemployment benefits and participation on active labor market programs. The data contain daily information on gross daily wages, occupation, industry, a dummy for full-time or part-time work, differentiate between minor employment and employment that is subject to social security insurance, and socio-demographic characteristics such as date of birth, sex, nationality, qualifications, and place of residence and place of work. The data do not include information on civil servants or self-employed persons.

The data preparation transferred the daily spell data into monthly panel data, as well as average aggregated data for the pre- and post treatment periods. In particular, we create two cross-sectional data sets with aggregated information over a period of twelve months before treatment assignment and a period of 36 (currently 30) months after treatment assignment. The monthly panel data set contains monthly aggregated information, covering the the same periods pre- and post assignment (see Table 5).

### C.1 Employment variables

Both analysis data sets (average, and monthly) contain the following information. “Share of days” in the following refers to the relevant pre- and post-treatment periods for average data, and the relevant month for monthly data.

**Employed:** Share of days for which person was employed (employment subject to social security contributions). This includes full-time, part-time, and minor employment.

**Employed fulltime:** Share of days for which the person was employed fulltime. Full-time employment includes employment subject to social security contributions. Fulltime is not further specified in the source data. Employers are only required to differentiate ‘fulltime’ and

‘parttime’ contracts.

**Employed parttime:** Share of days for which the person was employed parttime.

**Initial employment:** Share of days for which the person was employed at the business establishment in which they were employed at the day of the random assignment.

## C.2 Monetary variables

In the source data, only daily gross wages for the respective employment spell are available. We use this information on daily gross earnings to calculate a number of intermediary and outcome variables:

**Net earnings:** Net monthly earnings are calculated by deducting monthly employee social security contribution and monthly income tax from gross monthly earnings. In Germany employers and employees nominally pay equal shares of the social insurance contributions. Employee social security contributions are therefore calculated as the product of  $0.5 \times$  the total contribution rate (as defined in Table 6) and the monthly gross wage.

**Gross earnings (intermediary variable not used in paper):** For the average data, we calculate the average monthly earningy, by taking the sum of total earnings over the (pre- and posttreatment assignment period, and divide it by the number of months in the pre- and posttreatment assignment period. For the monthly data, we calculate gross monthly earnings as the product of daily wage and an average of 30 days per month.

**Income tax:** We approximate the monthly income tax, based on the assumption that all of our respondents are unmarried and have no children, i.e. Tax Class I in the German income tax system. The German tax system consists of a stepwise linear function specified in § 32a EStG (see Equation 9)<sup>15</sup> with different progression zones which are that results in a progressive tax schedule. The cutoff points for the different progression zones are redefined on an annual basis by the Federal Ministry of Finance and are given in Table 8. Because the income tax is based on annual earnings, we first calculate the hypothetical annual income of each respondent if the employment spell of a particular month covered the entire year, and divide the resulting annual income tax by 12 to receive the monthly income tax. This is the monthly income tax we use in the monthly data. For the average data, we take a monthly average over all pretreatment months and posttreatment months, respectively.

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<sup>15</sup>see <https://esth.bundesfinanzministerium.de/esth/2023/home.html> for § 32a EStG in the income tax regulations of the respective years, to find the cutoff-points for the respective years.

**Social Insurance Contributions (SI-Contributions) (Employee + Employer):** Social security contributions are calculated as the product of the total contribution rate, which contains contributions to different statutory social insurances, as defined in Table 6 and the smoothed monthly gross wage, or the average monthly gross wage for the pre- and post treatment period. Note: we currently ignore the contribution assessment ceilings for health and pension insurance contributions (see Table 7).

**Employer costs:** Employer costs are calculated as the sum of smoothed monthly or averaged monthly gross wage and monthly employer social security contributions. Employer social security contributions are calculated as the product of  $0.5 \times$  the total contribution rate (as defined in Table 6) and the monthly gross wage.

**Unemployment benefits:** The source data provide information on the daily unemployment benefits. For the average dataset, we use the total sum of unemployment benefits received in the pre- and posttreatment period, respectively, and divide it by the number of days in registered unemployment in the respective period. For the monthly dataset, we use the total sum of unemployment benefits received in the respective month and divide it by the number of days in registered unemployment in the respective month, and multiply it by 30 to obtain a smoothed curve.

Table 6: Social Insurance Contributions (employee and employer)

Component	2019	2020	2021	2022	2023
Pension Insurance (RV)	18.6%	18.6%	18.6%	18.6%	18.6%
Long-Term Care Insurance (PV)	3.05%	3.05%	3.05%	3.05%	3.05%
Unemployment Insurance (ALV)	2.5%	2.4%	2.4%	2.4%	2.6%
Health Insurance (base rate)	14.6%	14.6%	14.6%	14.6%	14.6%
Average Additional Health Contribution	0.9%	1.1%	1.3%	1.3%	1.6%
<b>Total Contribution Rate</b>	<b>39.65%</b>	<b>39.75%</b>	<b>39.95%</b>	<b>39.95%</b>	<b>40.45%</b>

**Note:** The respective contribution shares have been obtained from Federal Ministry of Labor and Social Affairs Publication of Factor F for all years in the survey

Table 7: Contribution Assessment Ceilings

Year	KV (Health)		RV West (Pension)		RV East (Pension)	
2019	4,537.50	EUR	6,700	EUR	6,150	EUR
2020	4,687.50	EUR	6,900	EUR	6,450	EUR
2021	4,837.50	EUR	7,100	EUR	6,700	EUR
2022	4,987.50	EUR	7,050	EUR	6,750	EUR
2023	5,175.00	EUR	7,300	EUR	7,100	EUR
2024	5,512.50	EUR	7,550	EUR	7,550	EUR
2025	5,512.50	EUR	8,050	EUR	8,050	EUR

Table 8: Income tax cutoff points

Symbol	Description	2019	2020	2021	2022	2023	2024
$G$	Income tax free allowance	9168	9408	9744	10347	10908	11604
$B_2$	Upper bound for bracket 2	14254	14532	14753	14926	15999	17005
$B_3$	Upper bound for bracket 3	55961	57052	57919	58597	62810	66761
$B_4$	Upper bound top tax rate	265326	270500	274612	277825	277825	277825
$a_2$	Coefficient for bracket 2	980.14	972.87	995.21	1088.67	979.18	922.98
$a_3$	Coefficient for bracket 3a	216.16	212.02	208.85	206.43	192.59	181.19
$c_3$	Constant in bracket 3b	965.58	972.79	950.96	869.32	966.53	1025.38
$c_4$	Offset in 42% bracket	8780.90	8963.74	9136.63	9336.45	9972.98	10602.13
$c_5$	Offset in 45% bracket	16740.68	17078.74	17364.99	17671.20	18307.73	18936.88

$$T(E) = \begin{cases} 0 & \text{if } E \leq G \\ (a_2 \cdot (\frac{E-G}{10^4}) + 1400) \cdot (\frac{E-G}{10^4}) & \text{if } G < E \leq B_2 \\ (a_3 \cdot (\frac{E-B_2}{10^4}) + 2397) \cdot (\frac{E-B_2}{10^4}) + c_3 & \text{if } B_2 < E \leq B_3 \\ 0.42 \cdot E - c_4 & \text{if } B_3 < E \leq B_4 \\ 0.45 \cdot E - c_5 & \text{if } E > B_4 \end{cases} \quad (9)$$

Once  $T(E)$  is computed, the following variables are derived:

$$\begin{aligned}
\text{inctax\_perc} &= \frac{T(E)}{E} \quad (\text{annual tax rate}) \\
\text{m\_inctax} &= \frac{T(E)}{12} \quad (\text{monthly tax}) \\
\text{m\_inctax\_perc} &= \frac{\text{m\_inctax}}{\text{m\_earnings}} \quad (\text{monthly tax rate}) \\
\text{m\_inc\_net} &= \text{m\_earnings} - \frac{\text{m\_socins}}{2} - \text{m\_inctax} \quad (\text{monthly net labor income})
\end{aligned}$$

**Commuting distance:** The source data provides information on the location of the employer and the place of residence of the employee at the district level (Kreise). We use a distance matrix by the Federal Institute for Research on Building, Urban Affairs and Spatial Development that provides the air distance between the geographical midpoints of each district to calculate the distance traveled one-way between home and main employer in each month. For the average datasets, we take the average of the monthly one-way distance for all pre- and post-treatment assignment months respectively. For months in which no employment is registered, the distance is recorded as 0.

## D Additional empirical findings

Table 9: Average effects, levels

Outcome	Treated	Control	ATE	SE	t-stat	p-val (N)	p-val (F)	n treated	n control
<b>Government revenue</b>									
Income tax	679.552	727.569	-48.017	38.572	-1.245	0.213	0.212	99	1278
SI contributions (employee + employer)	1221.703	1291.282	-69.579	56.094	-1.240	0.215	0.230	99	1278
Unemployment benefits	24.350	21.449	2.901	9.759	0.297	0.766	0.780	99	1278
Government Revenues	1876.784	1997.439	-120.656	96.464	-1.251	0.211	0.228	99	1278
<b>Earnings and commute</b>									
Employer costs	3685.289	3886.915	-201.625	167.694	-1.202	0.229	0.232	99	1278
Net earnings (excl. tax and SI)	1750.104	1833.531	-83.427	73.325	-1.138	0.255	0.260	99	1278
Distance to employer	21.615	27.555	-5.940	6.524	-0.910	0.363	0.370	99	1278
<b>Extensive and intensive margin</b>									
Employed	0.835	0.863	-0.029	0.029	-0.988	0.323	0.330	99	1278
Employed full-time	0.641	0.682	-0.040	0.038	-1.070	0.285	0.270	99	1278
Employed part-time	0.175	0.168	0.007	0.034	0.196	0.845	0.854	99	1278
<b>Job transitions</b>									
Initial employment	0.627	0.616	0.011	0.040	0.280	0.780	0.800	99	1278
New employer	0.208	0.248	-0.040	0.033	-1.205	0.228	0.228	99	1278

*Notes:* These tables report estimates of the effect of basic income on outcomes from the IAB administrative data. Outcomes are averaged over the three years during which basic income was disbursed. Monetary outcomes are in Euro per month. In Table 2, changes are relative to the average outcome over the 12 months preceding the experiment. Estimation uses within-block differences, as discussed in Section 3. p-val (F) are Fisher p-values based on permutation inference.