The Impacts of Guaranteed Basic Income on Crime Perpetration and Victimization*

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Abstract

This paper provides the first experimental evidence on the impact of providing a guaranteed basic income on criminal perpetration and victimization. We analyze a nationwide randomized controlled trial that provided 2,000 unemployed individuals in Finland with an unconditional monthly payment of €560 for two years (2017-2018), while 173,222 comparable individuals remained under the existing social safety net. Using comprehensive administrative data on police reports and district court trials, we estimate no significant impact on criminal perpetration and victimization. Furthermore, point estimates are consistently small across all crime categories, providing little evidence that the reduction in financial strain decreased income-generating property crimes while increasing other forms of criminal behavior. Although estimates for victimization are less precise, our confidence intervals rule out reductions in perpetration larger than 5 percent for police reports and 12 percent for criminal charges.

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1 Introduction

A longstanding question in economics and public policy is whether reducing poverty can also reduce crime. We study this relationship by examining the effects of a large-scale randomized controlled trial that provided a guaranteed basic income to poor individuals in Finland for two years. We leverage this experiment to understand how unconditional monthly cash transfers affect both criminal offending and victimization.

Cash transfers to financially constrained individuals may reduce the incentive to commit financially motivated property crimes, as they no longer need to earn income through illegal means. Greater income stability may also reduce stress and family conflict, lowering the likelihood of violent behavior. On the other hand, other types of criminal activity, such as traffic violations and violence due to increased alcohol consumption, may increase after a cash infusion. In addition, unconditional cash transfers might increase crime among unemployed individuals by reducing incapacitation effects, as recipients are not required to participate in active labor market programs.

The potential effects of a guaranteed basic income on victimization are similarly uncertain. Higher incomes could reduce exposure to risky situations, either by lowering participation in criminal activity or by improving mental and emotional well-being. Consistent with this view, recent work finds that expanding cash assistance programs can reduce domestic violence and abuse (Carr and Packham, 2019, 2021). Alternatively, increasing income might increase victimization if treated individuals engage in riskier behavior. For example, they might be able to afford and consume more alcohol, which can lead to greater victimization risk.

Therefore, whether poor individuals are more or less likely to perpetrate crimes or become victims after receiving a (temporary) guaranteed basic income is an empirical question. To answer these questions, we estimate the impacts of Finland's nationwide randomized basic income experiment. In 2017 and 2018, the Finnish government randomly assigned 2,000 unemployed benefit recipients to receive unconditional cash transfers of €560 per month for two years, while simultaneously eliminating mandatory job search requirements previously imposed by public employment services. The control group (173,222 individuals) remained subject to the existing welfare and taxation system. Verho *et al.* (2022) shows that the experiment increased the treatment group's annual disposable income with minimal impact on labor supply, but does not explore the impact on criminal activity. Consistent with their results, we find the experiment increased disposable income by just over 9 percent for those treated, primarily through the unconditional transfers.

Using high-quality Finnish register data, we study how the basic income experiment impacted crime perpetration and victimization. We link experiment participants to administrative police reports (1996-2023) and district court records (1992-2023). These data capture the universe of reported criminal incidents and judicial proceedings and provide a comprehensive and detailed measurement of criminal behavior across multiple margins: suspected offenses and victimization (from police reports, capturing all reported incidents) as well as formal charges (from court records, representing more serious offenses that proceed through prosecution). In addition, we link the experiment's sample to Statistics Finland's other register data, which provide detailed baseline characteristics for all participants, including socioeconomic indicators (income, employment status, educational attainment), demographic factors (age, gender, marital status), and household composition.

Consistent with the program's goal of targeting low-income individuals, both the treated and control groups had minimal economic resources before the experiment: their average annual labor earnings and disposable incomes were €1,900 and €13,100, respectively, compared with €28,500 and €26,724 in the Finnish population overall. We also document that these individuals were frequent participants in the criminal justice system. In the two years before treatment, 20% of treated individuals appeared in police records as suspected offenders: 13% for traffic crimes, 5% for property crimes, 2.5% for violent crimes, and the remainder for other offenses. Victimization rates were similarly elevated: nearly 10% of the control group were recorded as crime victims by the end of the experiment. These levels of offending and victimization are exceptionally high compared to the general population.

The substantial levels of perpetration and victimization in the targeted population mirror the broader empirical pattern that poverty and crime are tightly linked. In our data, individuals in the bottom 20% of the income distribution are far more likely to commit crimes than those in the top 20%, a well-documented gradient across high-income countries. These elevated offending and victimization rates before the program's introduction also underscore that the basic income experiment disproportionately targeted individuals already deeply involved in the criminal justice system. As such, the program reached precisely the population for whom a guaranteed basic income could have the greatest potential to reduce criminal activity if poverty were a significant causal driver.

In our first set of results, we estimate the impact of being randomized to receive a basic income over two years on crime perpetration. We find no effect on whether treated individuals perpetrate crimes. In the two years following the start of the experiment, individuals in the treatment group were statistically insignificantly 0.5 percentage points more likely to be suspected of a crime ($\beta = 0.005$, standard error = 0.008), representing a 2 percent increase relative to the control group mean of 20 percent. Our estimates rule out relatively modest effects; based on our 95% confidence intervals, we can exclude the possibility that the introduction of the basic income program reduced the probability of being suspected of a crime by more than 5%.

Furthermore, our findings indicate that the null result does not stem from offsetting heterogeneous effects across different crime types. One might expect, for example, that basic income could increase minor traffic offenses while decreasing property or violent crime. However, we find that the intervention produced consistently statistically insignificant effects that are close to zero for each crime category.

We also find that the introduction of basic income for two years had a statistically insignificant impact on the probability of being charged in district court. This second primary outcome captures the effect on more serious offenses, as most petty infractions reported to the police do not result in formal charges. Over the two-year follow-up period, the point estimate suggests that the basic income experiment increased the probability of being charged by an insignificant 0.2 percentage points ($\beta = 0.002$, standard error = 0.005), corresponding to a 4 percent increase relative to the control group mean of 6 percent. Based on the 95 percent confidence interval, we can rule out that the experiment reduced the probability of being charged by more than 12 percent.

Finally, we assess whether basic income affects lower-level offenses, specifically disorderly conduct. These incidents often fall below the threshold for formal police reports or court records, meaning our primary analyses could overlook meaningful changes in minor criminal behavior. To address this possibility, we obtained access to administrative records on disorderly conduct that include individual identifiers, enabling us to link them directly to our study population. The results show no evidence that treated individuals are more likely to engage in disorderly conduct, suggesting that the basic income intervention also does not increase lower-level criminal activity.

In the second part of the paper, we examine whether introducing a guaranteed basic income for two years affected individuals' likelihood of being victims of crime, leveraging the fact that we observe victim IDs in our data. Victimization is relatively common in this population: prior to the experiment, nearly 5 percent of treated individuals appeared in a police report as a victim. Despite this high baseline rate, we find no evidence that introducing a two-year basic income altered victimization risk. During the two-year treatment window, treated and control individuals experienced almost identical rates of victimization, and we can rule out reductions in victimization larger than 20 percent. We also find no effects when we look separately by crime type.

In sum, we find little evidence that the introduction of a temporary basic income affects criminal offending or victimization, despite high baseline rates of both among treated individuals (although the estimates for victimization are less precise). These findings suggest that, at least in Finland, alternative interventions may be more cost-effective at reducing criminal behavior and victimization.

Our paper contributes to three literatures in economics. First, we add to the growing research on guaranteed basic income and unconditional cash transfers in high-income countries. Prior work has found mixed results regarding labor market participation (Balakrishnan *et al.*, 2024; Vivalt *et al.*, 2024; Goodman-Bacon and Palmer, 2024; Verho *et al.*, 2022), health and psychological well-being (Miller *et al.*, 2024; Balakrishnan *et al.*, 2024; Jaroszewicz *et al.*, 2024), and financial security (Bartik *et al.*, 2024; Goodman-Bacon and Palmer, 2024). Two closely related papers examine the relationship between quasi-random cash transfers from Alaska's Permanent Fund and crime. Watson *et al.* (2020) and Dorsett (2021) find that these transfers, which are similar in magnitude to those studied in this paper, result in fewer property crimes and more disorderly conduct, while Bullinger *et al.* (2023) find that these transfers reduce child abuse. In contrast, Deshpande and Mueller-Smith (2022) study substantially larger transfers to a more targeted population (disability recipients) and find substantial crime-reducing effects of such transfers. Similarly, studies from low-income countries often find very positive results of unconditional cash transfers on various outcomes (Crosta *et al.*, 2024; Haarmann *et al.*, 2009), with Attanasio *et al.* (2021) showing that conditional cash transfers were effective at reducing arrests for the treated population in Colombia. Moreover, some research finds that basic income improves subjective well-being in wealthier contexts (Goodman-Bacon and Palmer, 2024; Simanainen and Tuulio-Henriksson, 2021), suggesting possible scope for a guaranteed basic income to reduce crime.

We contribute to this literature by providing the first large-scale randomized experimental evidence of the impacts of guaranteed basic income on crime from a high-income country. Our findings imply that guaranteed basic income, despite its considerable fiscal cost (Hoynes and Rothstein, 2019), does not generate offsetting savings from reduced crime. This result tempers the more optimistic interpretations from prior work and provides a critical empirical benchmark for ongoing policy debates.

Second, we contribute to the literature studying the interaction between economic status, the social security system, and criminal behavior. Studies find that job loss or decreased earnings leads to increased crime (Draca and Machin, 2015; Bennett and Ouazad, 2019; Britto *et al.*, 2022; Rose, 2018), though more generous unemployment benefits can mitigate these effects (Britto *et al.*, 2022; Rose, 2018; Machin and Marie, 2006). Further, access to welfare and higher benefit levels tends to reduce crime (Deshpande and Mueller-Smith, 2022; Dustmann *et al.*, Forthcoming; Yang, 2017; Tuttle, 2019). Additionally, active labor market programs show promise for reducing crime (Andersen, 2021; Fallesen *et al.*, 2018). We add to this literature by showing that unconditional cash transfers are not more efficient at reducing crime than means-tested benefits.

Third and last, we contribute to the literature studying victimization, especially its causes. There has been a recent surge in economic research exploring the impacts of victimization on various outcomes, showing large costs (Adams *et al.*, 2024; Adams-Prassl *et al.*, 2023; Bindler and Ketel, 2022). However, much less is known about the causes of victimization. There is research on the role of colleges (Lindo *et al.*, 2018), schools (Anderson *et al.*, 2013), households (Card and Dahl, 2011), and alcohol (Bindler and Ketel, 2022; Chalfin *et al.*, 2023) in victimization. Our paper adds to the literature by providing some of the first evidence on the impacts of poverty on victimization.

2 Institutional Framework

2.1 Finnish Social Security System for Unemployed Job Seekers

We begin by describing how the social security system works for unemployed job seekers in Finland, which constitutes the primary welfare system that the control group experienced throughout the experiment.

The Finnish unemployment benefit system consists of three types of benefits, with eligibility determined by an individual's employment history and membership in an unemployment fund. An unemployed person can receive only one type of unemployment benefit at a time. All unemployment benefits are taxable and may be supplemented if an unemployed person has children or participates in active labor market programs (ALMPs).

First, individuals are eligible for an earnings-related unemployment benefit if they are members of an unemployment fund and have worked for at least six months during the previous 28 months. They can receive earnings-related benefits for up to 400 weekdays. In 2017, the average monthly earnings-related benefit was approximately € 1,400.

Second, individuals who are not members of an unemployment fund but have worked at least six months during the preceding 28 months are eligible for a flat-rate, non-means-tested unemployment benefit, payable for up to 400 weekdays. In 2017, the monthly flat-rate benefit was approximately € 700.

Finally, individuals who do not meet the employment condition or have exhausted their previous benefits may claim a means-tested flat-rate benefit, subject to a wealth assessment and with no fixed duration. In 2017, this benefit was also approximately € 700 per month.

Many unemployment benefit recipients also receive other social benefits. One of the most common is the housing allowance, which can cover up to 80% of rent. The exact amount of housing allowance varies depending on household income, household size, and the municipality of residence. In 2017, the average housing allowance was approximately € 319. In addition, last-resort social assistance is available to unemployed individuals whose income and assets are insufficient to meet basic needs (e.g., food and clothing).

Only individuals who register as unemployed job seekers with the public employment services are eligible for unemployment benefits. Within two weeks of registration, public employment services invite the job seeker to an initial interview. During the interview, the job seeker and a counselor jointly develop a personalized employment plan outlining the steps to take before the next meeting. The plan is tailored to each person's circumstances and may include objectives such as engaging in an active job search, updating the CV, or enrolling in active labor market programs.

Public employment services check whether the job seeker adheres to the employment plan during follow-up interviews, typically held every three months. If a person does not comply with the plan—for example, by failing to attend ALMPs—their unemployment benefits may be suspended or canceled. For additional details regarding the unemployment benefits system, see Verho *et al.* (2022).

2.2 The Finnish Basic Income Experiment

The basic income experiment was a large-scale initiative undertaken by the Finnish government. The original plan was to include both the unemployed and the employed in the target population, but the experiment was ultimately restricted to unemployed individuals receiving flat-rate benefits. Expanding the target population would have required major changes to tax parameters, which were not feasible given the project's tight schedule.

In the experiment, the treatment group of 2,000 individuals received a guaranteed income of €560 per month (30% of the median monthly income) for two years starting in January 2017. The control group consisted of 173,222 individuals whose welfare benefits and tax rules remained unchanged. Participants were randomly selected from unemployed individuals aged 25–58 who were receiving flat-rate unemployment benefits as of November 2016. Individuals receiving earnings-related benefits were excluded from the experiment. Participation was mandated by law, implying that concerns about non-compliance were minimal.[1] For a description of the benefit system faced by the control group, see subsection 2.1.

The Social Insurance Institution of Finland conducted the randomization on December 15, 2016. Individuals in the treatment group received a letter on December 29, 2016, informing them that they had been selected to receive a basic income of €560 per month for two years. The letter also explained that the basic income transfer was untaxed and unaffected by labor or capital income, and it outlined how it interacted with other social benefits. Individuals in the control group were not notified about their treatment status. The first payment was made on January 9, 2017. Thereafter, participants received the basic income at the beginning of each month for two years.

The basic income provided to participants was roughly equal to the net unemployment benefits received by individuals in the control group. In some cases, the basic income transfer was lower than the unemployment benefits an individual would have received in the control group. This difference arose because the basic income did not automatically include the supplements attached to unemployment benefits for individuals with children or those participating in ALMPs. However, participants in the treatment group were entitled to these supplements, provided they registered as job seekers and participated in ALMPs. Furthermore, individuals in the treatment group could still apply for other social

[1] Authorities suspended payments when individuals moved abroad, were incarcerated, entered military service, or began receiving other benefits such as retirement, study, or childcare allowances.

benefits, such as the housing allowance, but these were adjusted to account for their basic income.

The basic income experiment had two major implications for the treatment group. First, it eliminated most of the bureaucracy associated with unemployment benefit applications. The treatment group was exempt from job-search monitoring and mandatory re-employment services, which are typically required for individuals receiving unemployment assistance. Because the basic income was roughly equal to the unemployment benefit received by the control group, the main effect for treated individuals who did not start working was to reduce their bureaucratic burden.

Second, the experiment reduced marginal tax rates and increased disposable income among participants who started working. Because the basic income was tax-free and was not withdrawn when participants started working, their marginal effective tax rates on earnings fell substantially, particularly at higher earnings levels. This reduction in marginal effective tax rates increased disposable income for individuals who started working. For a more detailed discussion of participation tax rates and work incentives, see Verho *et al.* (2022).

One institutional feature potentially relevant for criminal behavior in Finland is the use of income-based fines, particularly for traffic and other lower-level offenses. These fines are calculated using taxable earned income rather than total disposable income. Because the guaranteed basic income transfer in the experiment was untaxed and did not affect labor earnings (as we will show), the program did not mechanically increase the size of income-based fines faced by treated individuals. Moreover, for some participants, the basic income replaced taxable unemployment benefits with a non-taxable transfer, which would theoretically reduce expected fines rather than increase them. However, given the extremely low levels of taxable income in the study population, most individuals were already near the lower bound of the income-based fine schedule, implying that any such effects are likely quantitatively very small, although we will examine heterogeneity by predicted income to directly test this mechanism.

2.3 The Criminal Justice Process in Finland

A criminal investigation begins in one of two ways: the police receive a report of a suspected crime, or the authorities identify grounds for suspicion through surveillance. We construct our crime report and victimization outcomes using cases recorded by the police.

If the police suspect a crime, they initiate a pre-trial investigation to determine what happened and gather evidence through questioning and inquiries. Once sufficient evidence is collected, the police prepare a pre-trial investigation report and close the investigation. At this stage, the police decide whether to refer the case to the prosecutor. If the police do not forward the case to the prosecutor, it is typically because no crime occurred, the matter is deemed minor, or a minor committed the offense. Further, in minor crimes such as petty theft, the police may impose a fine on the perpetrator, thereby closing the case.

After the prosecutor receives the criminal case from the police, the prosecutor decides whether to bring charges in the district court. The prosecutor files charges if they conclude that probable grounds exist to support the suspect's guilt, and there are no other reasons to waive charges. Also, in less severe cases where the suspect has confessed, the prosecutor may impose a fine on the perpetrator without a trial.

If the prosecutor decides to bring charges, the case proceeds to a district court, where a judge or a panel of judges hears the case. We construct our second main crime outcome—criminal charges—using cases that proceed to district courts. Following the hearing, the judge or panel delivers a verdict and determines the appropriate punishment. In most cases, the primary punishment is an income-based fine, which accounts for approximately 50 percent of all cases. Probation and prison sentences are assigned in approximately 22 percent and 11 percent of cases, respectively. In about 5 percent of cases, the defendant is acquitted. The remaining cases receive other punishments, such as community service and juvenile penalties. If the convicted person or prosecutor is dissatisfied with the verdict and punishment, they may appeal to a court of appeals.

The Finnish Legal Register Center (LRC) and the Prison and Probation Service of Finland (RISE) are responsible for implementing punishments. The LRC oversees the enforcement of pecuniary penalties, while RISE manages community service, probation, and prison sentences.

3 Data

3.1 Data Sources

To study how the basic income experiment affected crime, we use rich Finnish administrative data from multiple registers, which we link at the individual level using pseudonymized identifiers. These data allow us to track participants in the basic income experiment from the first police report to the court system until a final verdict. The data also provide detailed information about the income and demographics of individuals in the control and treatment groups. We draw data from four pre-specified data sources: the Basic Income Experiment register, police data, court data, and the FOLK modules. We describe each data source in turn below.

Basic Income Experiment Register Kela's (The Finnish Social Insurance Institution) Basic Income Experiment Register provides information about participants in the basic income experiment. The dataset includes all participants, their treatment status, the benefits they received, and the timing of those benefits. It also contains pseudonymized personal identifiers, which we use to link the Basic Income Experiment Register to other Finnish administrative data.

Police Data We link the Basic Income Experiment Register to Statistics Finland's police register data, which contain all crime reports recorded by the Finnish Police between 1996 and 2023. This dataset provides information about the type of suspected crime, the date and location of the crime, and whether the case was solved and referred to the prosecutor. Using this data, we can identify the suspected perpetrator and potential victim and link them to other registers.

Court Data Our second source of crime data is Statistics Finland's District Court Data. This data set includes all criminal cases decided in Finnish district courts between 1992 and 2023, recorded at the individual charge level. Unique case identifiers allow us to link all charges associated with the same case. Additionally, the dataset provides information on crime codes that identify crime types, district court identifiers, crime dates, and decision dates. Finally, the court data allow us to observe the conviction, the type of punishment, or whether the individual was acquitted.

Taken together, the police and court data allow us to observe the start and end of the judicial process. However, it is worth noting that many cases do not proceed to the district courts. Most minor offenses result in fines and are thus settled prior to reaching a court case. Moreover, in some cases, the police discontinue the investigation or the prosecutor decides not to file charges, for example, when there is insufficient evidence to proceed.

Income and Demographic Information Finally, we link the Basic Income Experiment register, police data, and court data to Statistics Finland's FOLK basic and income data modules. The FOLK modules provide individual-level information on earnings, transfers, age, sex, education, marital status, number of children, municipality of residence, and other background variables. We primarily use this data to evaluate the validity of the experimental setting through a series of balance checks and to construct control variables that enhance statistical precision.

Analysis Sample Our main sample includes participants in the basic income experiment, including those who receive basic income (the treatment group) and the control group. We can follow both the treatment and control groups in the register data, before, during, and after the basic income experiment took place.

The use of administrative register data minimizes attrition. We are able to match 1,999 of the 2,000 treated individuals and 173,041 of the 173,222 control individuals to Statistics Finland's register data in 2016. The only sources of attrition are when participants move away from Finland or die, as these events remove them from Statistics Finland's registers. Appendix Figure A4 shows that the attrition is minimal and balanced evenly across treatment and control groups.

3.2 Main Outcome Variables

We examine three families of outcomes, following our pre-analysis plan.[2] Each family contains one primary outcome and several secondary outcomes. For each family, we adjust for multiple hypothesis testing across the secondary outcomes, as described in more detail when we present the main estimates.

Primary Outcomes The paper focuses on three primary outcomes. The first is an indicator variable that equals 1 if an individual is suspected of any crime between 2017 and 2018. To increase statistical power, we construct this primary variable using police reports. A substantial proportion of these crime reports do not proceed to district court, as prosecutors or police can impose fines for minor offenses. Consequently, this crime variable encompasses less severe offenses, such as speeding.

[2]The pre-analysis plan is available here. Our pre-analysis plan initially included an indicator for domestic violence as a fourth outcome. We did not obtain victimization data for cohabiting partners. Given the statistically insignificant impacts on violent and other crimes documented below, any effect on domestic violence would necessarily be small.

Our second primary outcome is an indicator variable that equals 1 if an individual faces district court charges for crimes committed during 2017-2018, and 0 otherwise. This variable captures more severe criminal behavior. As explained in Section 2.3, the police and prosecutors can resolve minor cases by issuing fines, meaning only more serious cases proceed to the district court.

Our third primary outcome is criminal victimization. We create an indicator variable that equals 1 if an individual appears as a complainant in any crime report during 2017-2018, and 0 otherwise.

In addition to these three primary outcomes, we added a fourth outcome, police apprehension due to disorderly conduct. This was not specified in our pre-analysis plan, as we only became aware of the data after receiving access to our main analysis sample. These cases are not necessarily linked to criminal behavior, but rather encompass preemptive measures taken by the police to prevent accidents or crimes due to public drunkenness or to remove a person from premises where he or she has caused a disturbance. The Police Act allows for a maximum of 24 hours of apprehension on these grounds. This outcome is also measured as a binary indicator variable equal to 1 if an individual was apprehended at least once during 2017-2018.

Secondary Outcomes Our secondary outcomes examine specific crime categories. Based on our pre-analysis plan, we categorize crimes into: 1. Traffic Crimes, 2. Property Crimes, 3. Violent Crimes, and 4. Other Crimes. Our approach closely follows Statistics Finland's two-figure categorization (shown in Appendix Table A10), which has seven classes. However, to increase statistical power, we combine some Statistics Finland categories as specified in our pre-analysis plan. First, we pool together offenses against life and health and sexual offenses as violent crimes. Second, we merge traffic offenses (category EE) and offenses under the Road Traffic Act and the Vehicle Act, along with the Driving Licenses Act (subcategories of category GG), into traffic crimes. Finally, we pool together Statistics Finland's categories of offenses against public authority (DD), other offenses against the penal code (FF), and offenses against other acts (GG, excluding the traffic cases) as other crimes. The crime data contains detailed six-digit crime codes that we can use to categorize crimes.

4 Empirical Specification

To study the causal effect of the basic income experiment on crime, we estimate the following equation.

$$Y_i = \alpha + \beta T_i + \gamma \mathbf{X_i} + \epsilon_i, \quad (1)$$

where α is the constant, T_i is the treatment indicator taking value one for the treatment group, $\mathbf{X_i}$ is a vector of controls listed in Table 1 which we add to increase statistical precision, and ϵ_i refers to the error term. We control the family-wise error rate (FWER) for multiple hypothesis testing when presenting secondary outcomes (e.g., effects across crime subcategories).

The main parameter of interest is β, which captures the causal impact of basic income on crime, assuming that the treatment is independent of potential outcomes. Our approach closely follows that of Verho *et al.* (2022).

Randomization Check and Descriptive Statistics Our key identification assumption is that the treatment assignment is independent of potential outcomes. This assumption implies that the treatment and control groups are comparable across both observed and unobserved dimensions.

Table 1 provides evidence that the randomization was successful. Columns 1 and 2 report the means of predetermined background variables. In column 3, we report the difference in means between the control and treatment groups, obtained using equation 1. Finally, column 4 shows the p-value for each difference in the mean estimate.

Table 1 shows that all the differences are very small and statistically insignificant. Further, we test whether the coefficients are jointly significant by regressing the treatment indicator on all the variables listed in the table. We also fail to reject the null hypothesis that the coefficients are jointly equal to zero.[3]

Impacts on Income Before presenting our main estimates in the next section, we first show the impact of the experiment on incomes. We summarize these results in Figure 1(b),

[3]The Social Insurance Institution of Finland conducted randomization and assigned 2000 and 173,222 individuals to treatment and control groups. As shown in Table 1, we can match 99% of the experiment's participants to Statistics Finland's register data.

Table 1: Descriptive Statistics and Balance Check

	Treated (1)	Controls (2)	Difference (3)	*p-value* (4)
Demographics				
Disposable Income	13,151.876	13,194.334	-42.458	0.744
Labor Earnings	1,893.447	1,940.970	-47.523	0.622
Unemployment Months	7.136	7.024	0.111	0.280
Female	0.477	0.475	0.002	0.883
Age	40.807	40.448	0.360	0.107
Basic Education	0.334	0.352	-0.018	0.086
Secondary Education	0.463	0.461	0.002	0.830
Post Secondary Education	0.203	0.187	0.016	0.080
Married	0.349	0.339	0.009	0.387
N. of Children	0.799	0.782	0.017	0.532
Foreign Language	0.244	0.252	-0.009	0.359
Capital Region	0.303	0.302	0.001	0.887
Criminal Background				
Any report, t-1 to t-2	0.198	0.200	-0.002	0.821
Any charge, t-1 to t-2	0.067	0.068	-0.001	0.855
Any traffic report	0.132	0.133	-0.001	0.873
Any property report	0.050	0.052	-0.002	0.668
Any violent report	0.030	0.033	-0.004	0.371
Any other report	0.060	0.061	-0.001	0.826
Joint F-test			0.568	0.917
N	1,999	173,041		

Note: The table shows the predetermined characteristics for the treatment and control groups, and a balance check. Column 1 shows the means of background variables for the treatment group. Column 2 presents the means of background variables for the control group. Column 3 shows the mean difference between the control and treatment groups obtained using equation 1. Column 4 shows the p-value of the mean difference. The joint F-test statistic is obtained by regressing the treatment indicator on the listed background variables. Data and variables are constructed as explained in Section 3 which shows that the introduction of basic income substantially increased the disposable incomes of treated individuals, as expected.

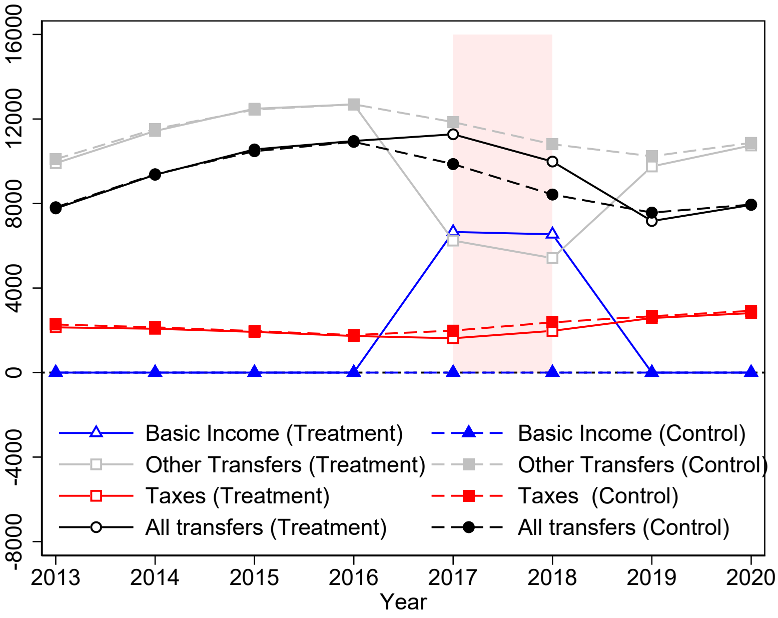
Figure 1 Panel A illustrates the evolution of transfers and taxes for the treatment and control groups before, during, and after the experiment. White triangles indicate that the basic income transfer amounted to approximately 6,600 euros annually. The grey squares show that income from other transfers was lower in the treatment group than in the control group. This primarily reflects that the basic income replaced the unemployment benefits the treatment group had received before the experiment began.

After accounting for all income sources, we find that the experiment increased the treatment group's disposable income substantially compared to the control group. Specifically, Figure 1 Panel B shows how incomes evolved for the control and treatment groups. As Verho *et al.* (2022) showed, we also find that the experiment did not affect labor earnings. However, due to the basic income transfers, the treatment group's disposable income was approximately 1500 euros higher than the control group's income during the experiment. This is equivalent to an increase in income of approximately 9%-11% relative to the control group.[4]

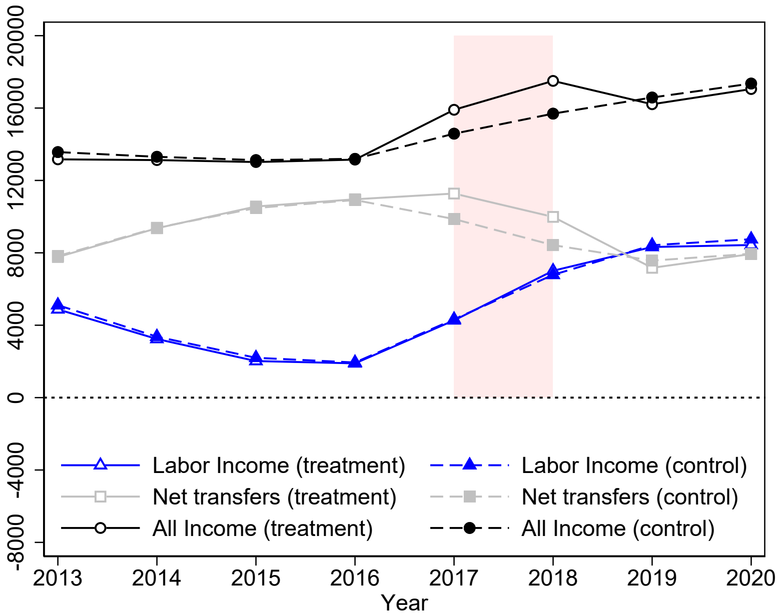
[4] Appendix Table A8 shows the impact on yearly disposable income, labor earnings, employment, and unemployment months. In both Figure 1 and Appendix Table A8, disposable income includes labor earnings, taxes, all transfers (including basic income transfer), entrepreneurial income, and capital income.

Figure 1: Income Streams by Treatment Status

(a) Transfers



(b) All Income Sources



Note: The figure illustrates the evolution of income sources for the treatment and control groups. Panel (a) plots the average yearly transfers and taxes. White triangles indicate the value of the basic income transfer received by the treatment group. Grey squares show all other transfers (excluding basic income), while red squares indicate average yearly taxes paid. The dots represent the net sum of all transfers and taxes—i.e., total net transfers for each group. Panel (b) focuses on income composition. Blue triangles plot the average labor income for the treatment and control groups. Dots capture total disposable income, including labor earnings, taxes, all transfers (including the basic income transfer), entrepreneurial income, and capital income. The shaded area indicates the experimental period (2017–2018). Data and variables are constructed as explained in Section 3.

5 Impact of Basic Income on Criminal Perpetration

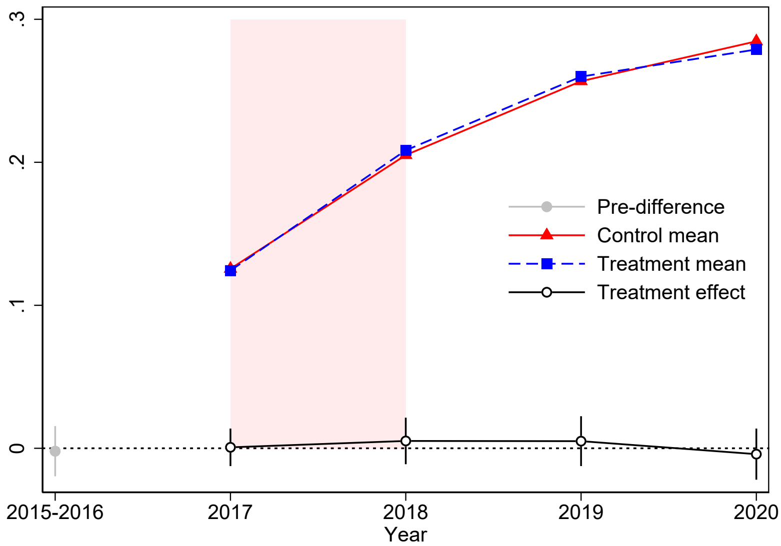
Impact on Crime Reports Figure 2 presents the main results of the paper for crime perpetration, graphing the impacts of the basic income experiment on both crime reports (Panel A) and charges that end up in court (Panel B). Blue squares and red triangles show the raw cumulative means for the treatment and control groups, respectively. White dots plot the treatment effect we obtain using equation 1 with controls. The gray dot from 2015-2016 shows the pre-treatment difference in the cumulative probability of crime report (Panel A) and probability of charges (Panel B) two years before the start of the experiment, demonstrating no differences between treatment and control observations before the experiment starts.

We focus first on Panel 2(a), which estimates the impact on police reports (i.e., the experiment's effect on the probability of being suspected of a crime). The raw means, depicted by the dotted blue line and the solid red line, demonstrate that the control and treatment groups' raw criminality evolve strikingly similarly during the experiment (red area) and after the experiment concluded (the years 2019 and 2020). The white dots plot the simple difference in cumulative means between the control and treatment groups, i.e., reporting the estimates obtained from equation 1. Based on these estimates, we do not detect any statistically significant differences in means during or after the experiment, and the point estimates are approximately zero. This zero effect is especially striking, given the high rates of police reports in both the control and treatment groups during the experiment, with more than 10% committing crimes in 2017 and 20% committing crimes between 2017–2018 (see also Appendix Figure A1).

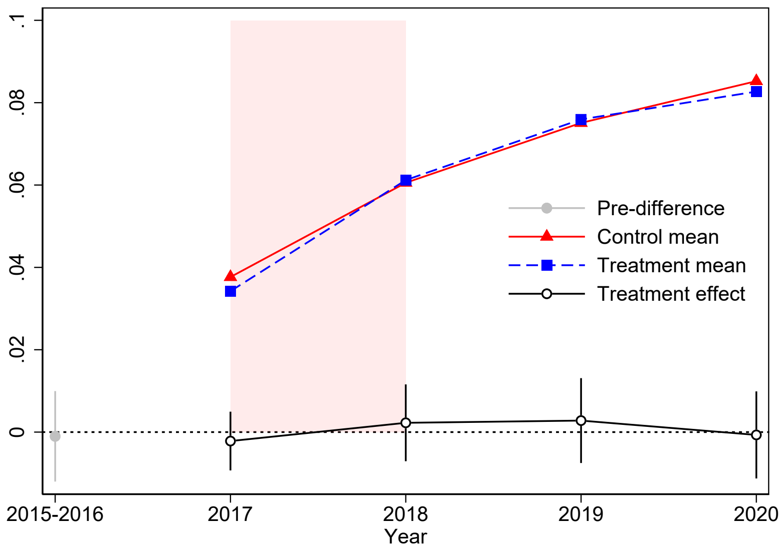
To summarize these initial results, we find no evidence that introducing a two-year basic income reduced criminality among treated individuals, who were just as likely to commit crimes during and after the experiment. This is true despite the large infusion of cash they received during the experimental window indicated in red.

Figure 2: The Impact of Basic Income Experiment on Crime Reports and Charges

(a) Crime Reports



(b) Charges



Notes: Figure shows the impact of basic income experiment on crime. In Panel A (Panel B), the outcome is the cumulative probability of being suspected (charged) of a crime. Blue squares and red triangles display the raw cumulative means for the treatment and control groups. White dots plot the estimated treatment effects obtained using equation 1 with control variables. Vertical lines denote the 95% confidence intervals. The grey dots represent pre-treatment differences in the cumulative probability of a crime report (Panel A) and charges (Panel B) two years before the start of the experiment. The shaded area indicates the experimental period (2017–2018). Data and outcomes are constructed as explained in Section 3.

Next, Table 2 Panel A reports the impact of the experiment on our first primary outcome, the probability of being suspected of a crime at any point during the experiment. Column 1 shows that being assigned to the treatment group increases the probability of being suspected of a crime by a statistically insignificant 0.5 percentage points during the two-year follow-up period. Compared with the control group mean of 20.5% appearing in a police report, this point estimate translates to an *increase* in the probability of being suspected of a crime of about 2 percent. However, this increase is not statistically significant. Our 95 percent confidence interval allows us to reject relatively small effects. We can rule out that the experiment reduced the probability of a crime report by more than 5 percent relative to the control-group mean.

One potential explanation for the null results is that the experiment had heterogeneous effects across crime types that offset each other. For instance, the introduction of a basic income may increase the likelihood of traffic offenses, such as speeding, because as treated individuals become richer, they are more likely to own cars and commit such crimes (i.e., an income effect). At the same time, the introduction of a basic income may reduce property or violent crime through improved financial stability or greater income predictability. These opposing effects could cancel each other out, resulting in no detectable aggregate impact, but still producing important and significant effects on subcategories of crime.

Columns 2–5 of Table 2 rule out this explanation for the aggregate null effect. They show no statistically significant effects for any specific crime type (traffic, property, violent, and other). Although the confidence intervals for specific crime types are wider than for our primary outcome (any crime), all point estimates remain consistently close to zero.

Moreover, in Appendix Table A1, we examine driving-under-the-influence offenses, drug-related crimes, and income-generating crimes, motivated by the possibility that a guaranteed basic income could affect these categories differently. For example, additional disposable income could reduce financially motivated property crime while increasing alcohol- or drug-related offending. We find no statistically significant impacts across any of these categories. Point estimates remain economically small and close to zero, providing no evidence that the null aggregate effect masks offsetting responses across different types of criminal behavior.

Table 2: The Impact of Basic Income on Being Suspected or Charged with a Crime

Outcome:	Any Crime Report (1)	Type of Crime: Traffic (2)	Property (3)	Violent (4)	Other (5)
Panel A: Crime Report					
Treatment Effect (controls)	0.0051	0.0052	-0.0002	0.0019	0.0044
	(0.0083)	(0.0075)	(0.0041)	(0.0037)	(0.0046)
Treatment Effect (no controls)	0.0034	0.0047	-0.0014	0.0008	0.0030
	(0.0092)	(0.0081)	(0.0046)	(0.0039)	(0.0052)
Control Mean	0.2051	0.1481	0.0444	0.0296	0.0521
Conventional *p-value*		0.4905	0.9602	0.6036	0.3295
FWER *p-value*		0.8601	0.9590	0.8601	0.7961
Observations	172,850	172,850	172,850	172,850	172,850
Panel B: Criminal Charge					
Treatment Effect (controls)	0.0023	0.0024	0.0003	-0.0008	-0.0004
	(0.0048)	(0.0033)	(0.0028)	(0.0025)	(0.0027)
Treatment Effect (no controls)	0.0007	0.0020	-0.0004	-0.0015	-0.0009
	(0.0054)	(0.0035)	(0.0032)	(0.0026)	(0.0029)
Control Mean	0.0606	0.0223	0.0211	0.0147	0.0181
Conventional *p-value*		0.4506	0.9085	0.7509	0.8992
FWER *p-value*		0.9025	0.9880	0.9810	0.9880
Observations	172,850	172,850	172,850	172,850	172,850

Note: The table shows results estimating the impact of basic income on the cumulative probability of being suspected of crime (panel A) or being charged in district court with a crime (panel B). In column 1, the outcome is an indicator equal to 1 if the individual was suspected or charged of any crime during the experiment (years 2017 and 2018). Columns 2-5 report analogous results for specific crime categories as outcomes. The "treatment effect" coefficient equals the β from equation 1. The brackets show heteroskedasticity robust standard errors. The table also presents conventional and family-wise error rate (FWER) corrected p-values. Specifications in the first rows include controls to increase the precision of the estimates, while those without controls are reported directly below.

Impact on Charges Next, we examine the impact of the basic income experiment on our second primary outcome: the probability of being charged with a crime committed during the experimental period. Table 2 Panel B presents the results of this analysis. Column 1 pools all crime types, while columns 2–5 report estimates separately by crime category. Compared to the first primary outcome, this outcome captures the effect of basic income on more serious offenses, as it excludes minor infractions that typically do not result in formal charges or proceed to court.

Similarly to the results based on crime reports, we find no evidence that the basic income experiment affected the probability of being charged in district court during the experimental period (2017–2018). The estimated treatment effect is statistically insignificant ($\beta = 0.0023$, standard error = 0.0048), corresponding to approximately a 4 percent increase relative to the control group mean. We cannot rule out effects as small as those we could for police reports. However, based on the 95 percent confidence interval, we can exclude decreases in the probability of being charged larger than 12 percent. Moreover, we find no evidence of an impact across any specific crime categories: all point estimates are small in magnitude and statistically insignificant.

Impact on Disorderly Conduct We also find no significant impacts on our additional outcome variable, police apprehension due to disorderly conduct. Appendix Figure A6 shows that while the levels of disorderly conduct appear to be marginally higher in the treatment group in the pre-intervention period (less than one percentage point), this gap does not increase during the treatment years. Moreover, estimates of the experiment's impact on disorderly conduct are not statistically significant in any year and decline to near zero by 2020.

Heterogeneity by Age, Gender, Criminal History, and Income In Appendix Figures A7-A8, we explore the heterogeneity of our main impacts by the age, gender, income, and criminal history of the individual. Starting with age, younger individuals—particularly young men—are much more likely to commit crimes. In the control group, those below the median age are almost twice as likely to appear in court for criminal charges as those above it. Yet, despite this baseline gap, we find small and statistically insignificant effects of the basic income for both age groups. In other words, while younger people are more criminally active overall, the guaranteed basic income does not appear to mitigate this behavior.

Moving to gender, we find that men are approximately three times more likely to appear in police reports or face criminal charges in court. However, we again find no statistically significant impact of the basic income for either gender, with treatment effects close to zero for both men and women. Splitting the sample by median income reveals more minor pre-treatment differences in baseline criminality, and, as with age and gender, no meaningful treatment effects emerge.

Last, when we look at criminal history, we observe stark differences in the control group: while 10% of those with no criminal history appear in a police report, over 40% of those with a criminal history do. Similarly, while fewer than 2% of those without a criminal history appear in court for criminal charges, over 15% of those with a criminal history do so. Yet despite these stark differences in the control means, we observe no significant treatment effects for those with a criminal history (or those without a criminal history).

We conclude that there is no evidence of offsetting or heterogeneous impacts: the guaranteed basic income in Finland appears to have had no measurable effect on criminal behavior across a wide range of individual characteristics and crime types. These conclusions are reinforced by our causal forest analysis, which likewise detects no systematic heterogeneity in treatment effects (Appendix C).

Heterogeneity by Predicted Income Impact of the Guaranteed Basic Income As Section 2.2 explains, the experiment's effects on administrative burden and disposable income partly depended on whether individuals started working. The basic income transfer was roughly equal to the unemployment benefits received by jobless individuals in the control group. Hence, for treated individuals who remained jobless during the experiment, the main change was a reduction in administrative burden. In contrast, for treated individuals who started working, disposable income increased substantially. This was because the basic income transfer, unlike standard unemployment benefits, was not withdrawn as labor earnings increased.

To investigate whether the effects on crime vary depending on whether individuals were exposed primarily to a reduction in bureaucratic burden or to a positive disposable-income shock, we examine treatment-effect heterogeneity by predicted labor income. The idea is that individuals with low predicted labor income mainly face a reduction in bureaucratic burden, as they are unlikely to work, whereas those with high predicted labor income experience a large disposable-income shock.

Appendix Figures 10(a) and 10(b) illustrate the idea by plotting disposable income, including basic income transfer, as a function of predicted labor income. To do so, we first predict average labor income during the experiment using predetermined characteristics, and then split individuals into groups based on these predictions.[5] For the bottom predicted income groups, average disposable income increases by approximately 0–1,000 euros per year. For the top predicted income groups, the disposable income shock ranges from 2,500 to 4,000 euros per year.

[5] We predict each individual's average labor income using only the control group and a regression model in which the independent variables are those listed in Table 1 as well as labor earnings and the number of months unemployed between 2013 and 2015.

Appendix Figure 10(c) then examines whether the impact on police reports during the experiment varies with the level of the disposable-income shock. We construct the figure by running equation 1 separately for each income group, with the outcome being any police report during the experiment (y-axis) and average disposable income during the experiment (x-axis). The figure provides suggestive evidence that the impact on crime does not vary with the level of the disposable-income shock. The estimated treatment effect is very similar for the bottom group, who mainly experience a reduction in bureaucratic burden, and for the top group, who face a large disposable-income shock.

These results also help disentangle the mechanisms through which the treatment might have impacted crime. As outlined above, the program affects individuals along three key margins: reducing administrative burden, increasing disposable income, and lowering marginal tax rates on labor earnings. The predicted-income heterogeneity analysis allows us to differentially weight these channels across groups. For individuals in the lowest predicted income groups who we find are unlikely to enter employment, the treatment primarily operates through a reduction in bureaucratic burden, with only modest changes in disposable income and no meaningful change in marginal tax rates. In contrast, for individuals in the highest predicted income groups who are already largely working, the treatment yields substantial increases in disposable income and reduces effective marginal tax rates, with a comparatively small role for administrative simplification. Despite these stark differences in how the treatment is experienced, we consistently find null effects on criminal behavior. This pattern suggests that the absence of an overall treatment effect is unlikely to be driven by offsetting impacts of these three channels. Instead, the results indicate that each of these mechanisms, whether operating in isolation or in combination, has a limited influence on criminal behavior in this setting.

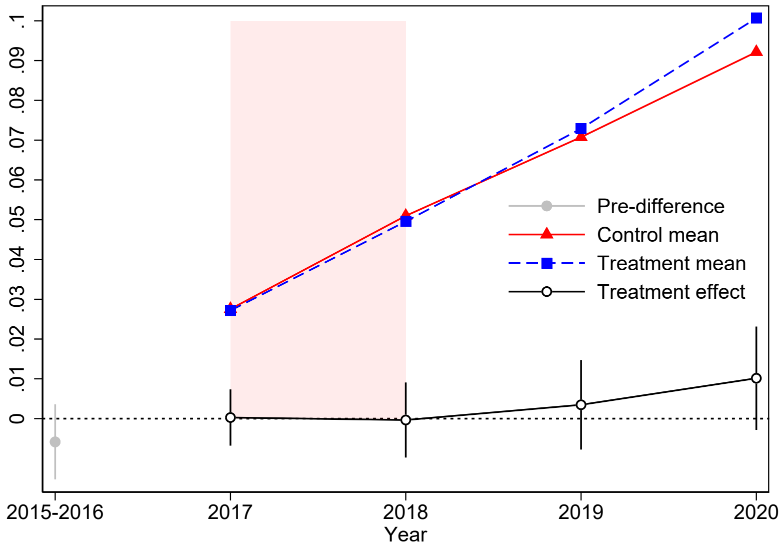
Robustness We demonstrate that our null results are robust to alternative definitions of the main variables and specifications. Appendix Tables A2 - A3 show that results are similar when the main outcome is the number of police reports and the number of charges during the experiment. The main specification includes controls to increase the precision of the estimate. Appendix Tables A5 - A6 illustrate that estimates are also very similar when we use a specification without control variables.

6 Impact of Basic Income on Victimization

While the guaranteed basic income experiment had no discernible impacts on the perpetration of crime, it could still have reduced the likelihood of victimization. Recipients may have experienced less household stress around finances, reducing domestic violence. Beyond the household, recipients may have been less inclined to engage in risky behavior. To address this question, we examine whether treated individuals were more or less likely to show up as victims in the administrative police data.

We report estimates in Figure 3. We find no meaningful evidence that the intervention affected rates of victimization. Although victimization is relatively common in this population—nearly 10% of the control group appear in a police report as victims by 2020 (see also Appendix Figure A3)—there are no discernible differences between treatment and control groups during the treatment period (marked by the red shaded area). By 2018, we can statistically rule out effects as large as a 20% reduction in victimization. By 2020, the treatment group appears slightly more likely to experience victimization, but this difference is small and not statistically significant.

Figure 3: The Impact of Basic Income Experiment on Victimization



Notes: The figure shows the impact of the basic income experiment on crime victimization. The outcome is the cumulative probability of being victimized, based on police reports. Blue squares and red triangles display the raw cumulative means for the treatment and control groups. White dots plot the estimated treatment effects obtained using equation 1 with control variables. Vertical lines denote the 95% confidence intervals. The grey dot represents the pre-treatment difference in the cumulative probability of victimization two years before the start of the experiment. The shaded area indicates the experimental period (2017–2018). Data and outcomes are constructed as explained in Section 3.

We report the estimated impact on whether the treated individual was recorded as a victim in either 2017 or 2018 (the years of the experiment) in Table 3. Consistent with the figure, we find null effects for victimization overall in column 1. The table also reports impacts for crime subcategories, as specified in the pre-analysis plan. It could be that while property and violent victimization decrease, traffic victimization increases, and these countervailing effects cause the null overall effect. However, the estimates in columns 2–5 show that the absence of treatment effects holds consistently across all major crime types.

Table 3: The Impact of Basic Income on Victimization During the Experiment

Outcome	Any Victimization	**Type of Victimization** Traffic	Property	Violent	Other
	(1)	(2)	(3)	(4)	(5)
Treatment Effect	-0.0003	0.0014	-0.0023	0.0011	-0.0001
	(0.0048)	(0.0020)	(0.0027)	(0.0035)	(0.0025)
Control Mean	0.0510	0.0068	0.0166	0.0260	0.0125
Conventional *p-value*		0.4410	0.4343	0.7506	0.9819
FWER *p-value*		0.8851	0.8851	0.9315	0.9815
Observations	172,850	172,850	172,850	172,850	172,850

Note: The table reports the estimated impacts of the basic income experiment on the cumulative probability of being reported as a victim in police data. In column 1, the outcome is an indicator equal to 1 if the individual was reported as a victim in a police report during the experiment (years 2017 and 2018). Columns 2–5 report analogous results for specific crime categories as outcomes. The "treatment effect" coefficient equals the β from equation 1. The brackets show heteroskedasticity robust standard errors. The table also presents conventional and family-wise error rate (FWER) corrected p-values. All the specifications include controls to increase the precision of the estimates. The data and outcomes are constructed as explained in Section 3.

We further find that these null results are robust to using the number of victimizations as the main outcome (Appendix Table A4) and to excluding the control variables (Appendix Table A7).

Appendix Figure A9 examines whether the effects of the basic income experiment on victimization vary across key demographic and socioeconomic subgroups. The figure shows that baseline rates of victimization differ meaningfully across groups, albeit less starkly when compared with perpetration. In particular, young individuals and those with a prior criminal history experience substantially higher victimization rates in the control group. Despite these differences in underlying risk, the estimated treatment effects are consistently close to zero across all subgroups. For each characteristic examined, the confidence intervals are tight enough to rule out moderate reductions in victimization.

In the appendix, we also explore whether treatment effects differ based on individuals' exposure to changes in disposable income during the experiment, using a dosage-style exercise as we did with perpetration. Results are captured in Appendix Figure A11. Because the basic income primarily reduced administrative burden for individuals who remained out of work, but generated sizable increases in disposable income for those who began working, the experiment produced meaningful variation in the magnitude of the income shock across participants. Using predicted labor income to proxy for this variation, the dosage analysis shows that individuals facing larger positive income shocks did not experience different victimization impacts than those facing only a reduction in bureaucratic burden. Across the entire gradient of predicted income gains, estimated treatment effects remain very close to zero, and no systematic pattern emerges linking the magnitude of the income shock to improvements or deteriorations in victimization risk.

Together, these analyses strengthen the conclusion that a guaranteed basic income over the two-year experiment did not meaningfully alter victimization outcomes. The absence of heterogeneous effects across demographic groups or across individuals with different exposure to income gains suggests that neither financial stability nor reductions in administrative stress were sufficient to influence victimization risk within the two-year time horizon of the experiment. The results reinforce the findings for criminal perpetration: while victimization is common in this low-income population, a (temporary) guaranteed basic income alone does not shift the likelihood of becoming a victim of crime.

7 Conclusion

In this paper, we estimate the impacts of a randomized controlled experiment that provided two years of guaranteed basic income to treated individuals at the bottom of the income distribution in Finland. While the original experiment was designed to evaluate the behavioral effects of basic income on employment, we examine its effects on criminal perpetration and victimization. Our results indicate that a guaranteed basic income does not alter criminal behavior. We find no significant effects for either perpetration or victimization. We can rule out modest (more than 5%) decreases in the probability of being suspected of a crime, although the victimization estimates are less precise. This is true despite high rates of perpetration and victimization in this population, with almost 30% of control individuals appearing in police reports and nearly 10% of control individuals experiencing a victimization event by 2020.

These results are unique to Finland, which already had a very generous social support system. However, they suggest that governments, especially those that already provide generous support, should consider focusing on other interventions with a track record of success to reduce crime more effectively. For example, prior economics research suggests that investments in policing (Di Tella and Schargrodsky, 2004), education (Huttunen *et al.*, 2023; Anders *et al.*, 2023; Machin and Sandi, 2024; Rose *et al.*, 2022), and direct interventions with perpetrators (Doleac, 2023; Shem-Tov *et al.*, 2024) can all lead to significant reductions in crime and, as such, could all be more effective investments for governments focused on making their communities safer.

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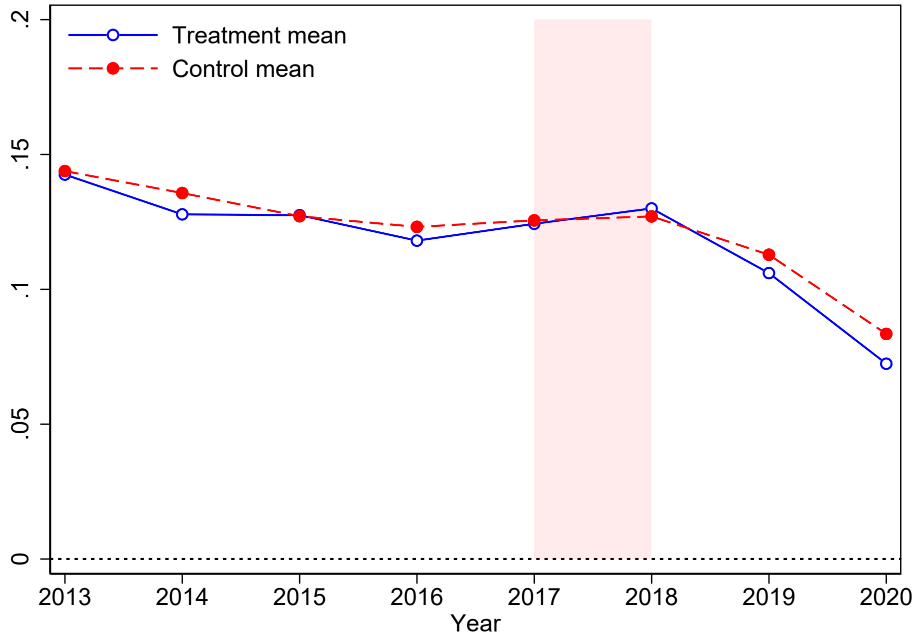
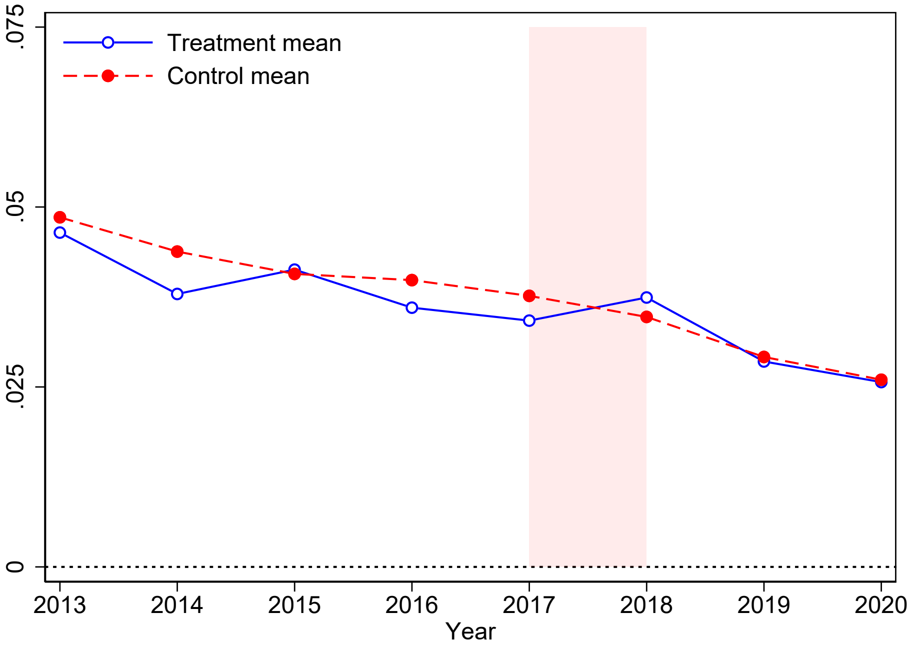
A Appendix Figures

Figure A1: Annual Share of Individuals Recorded as Suspected Perpetrators in Crime Reports



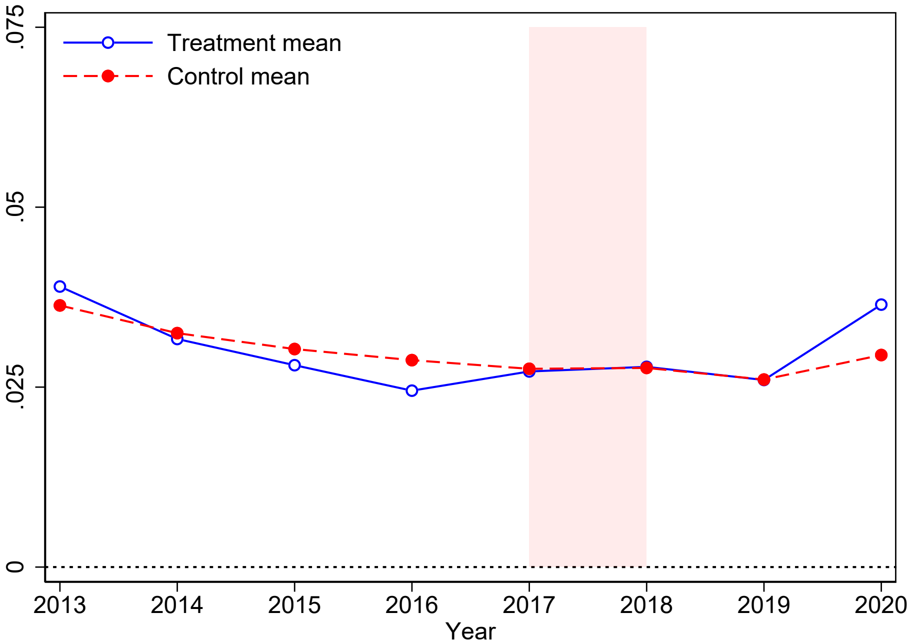
Notes: The figure shows the annual share of individuals recorded as suspects in police crime reports. Red dots indicate the shares for the control group, while white dots indicate the shares for the treatment group. The shaded area marks the experimental period (2017–2018). Data and outcomes are constructed as explained in Section 3.

Figure A2: Annual Share of Individuals with a Criminal Charge



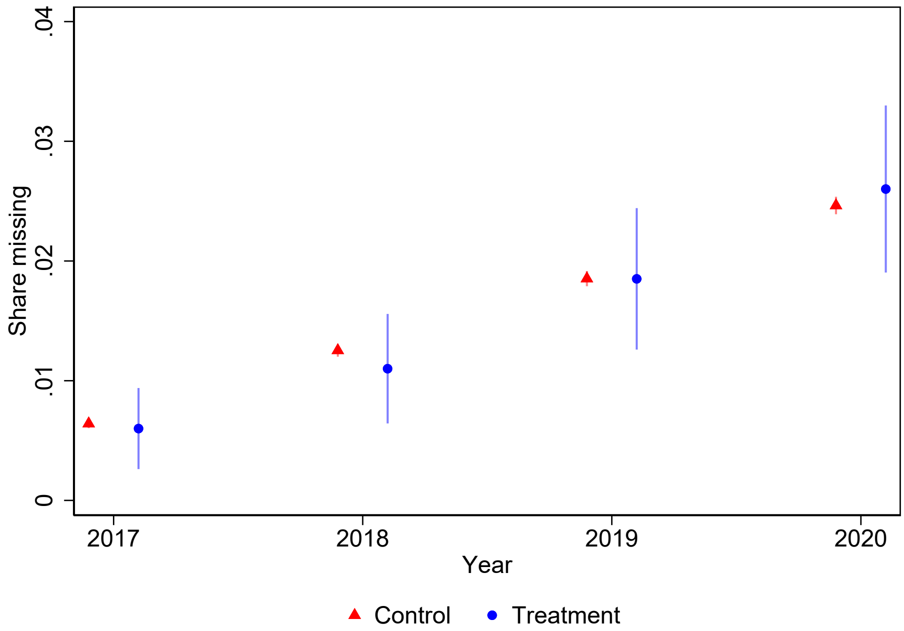
Notes: The figure shows the annual share of individuals with at least one criminal charge in district court. Red dots indicate the shares for the control group, while white dots indicate the shares for the treatment group. The shaded area marks the experimental period (2017–2018). Data and outcomes are constructed as explained in Section 3.

Figure A3: Annual Share of Individuals Victimized



Notes: The figure shows the annual share of individuals experiencing victimization. Red dots indicate the shares for the control group, while white dots indicate the shares for the treatment group. The shaded area marks the experimental period (2017–2018). Data and outcomes are constructed as explained in Section 3.

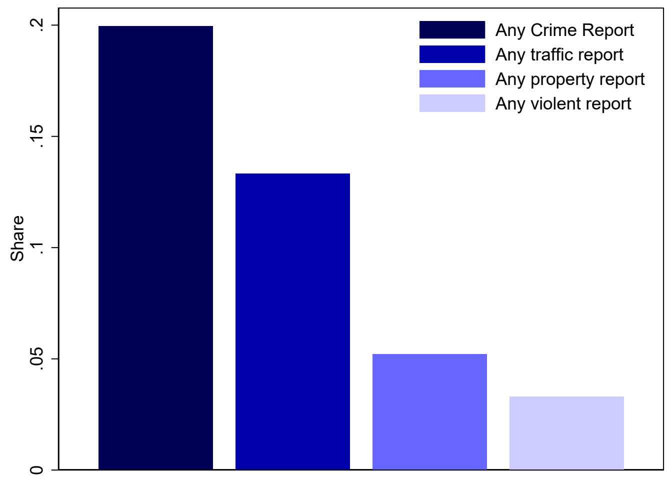
Figure A4: Annual Attrition Rates by Treatment Status



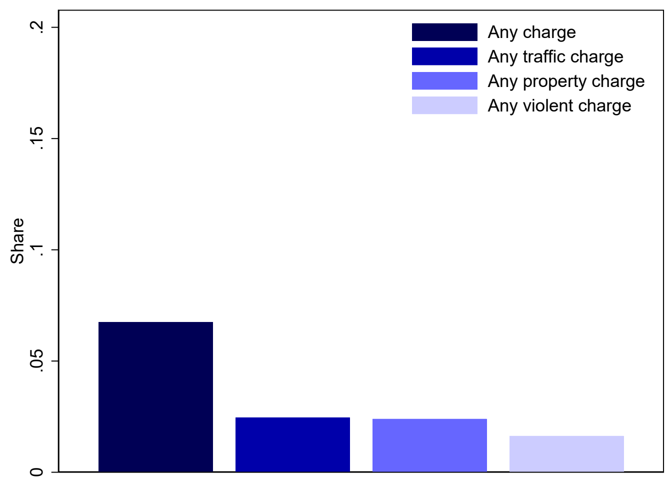
Notes: The figure plots the share of individuals in the treatment and control groups who are missing from the administrative register data. Triangles denote the control group. Dots denote the treatment group. Lines indicate 95% confidence intervals. Data and outcomes are defined as described in Section 3.

Figure A5: The Distributions of Crime Reports and Charges 1-2 years before the treatment

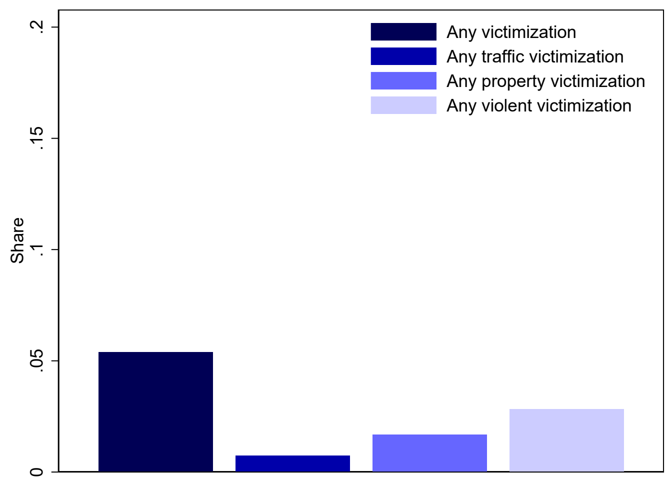
(a) Crime Reports



(b) Charges

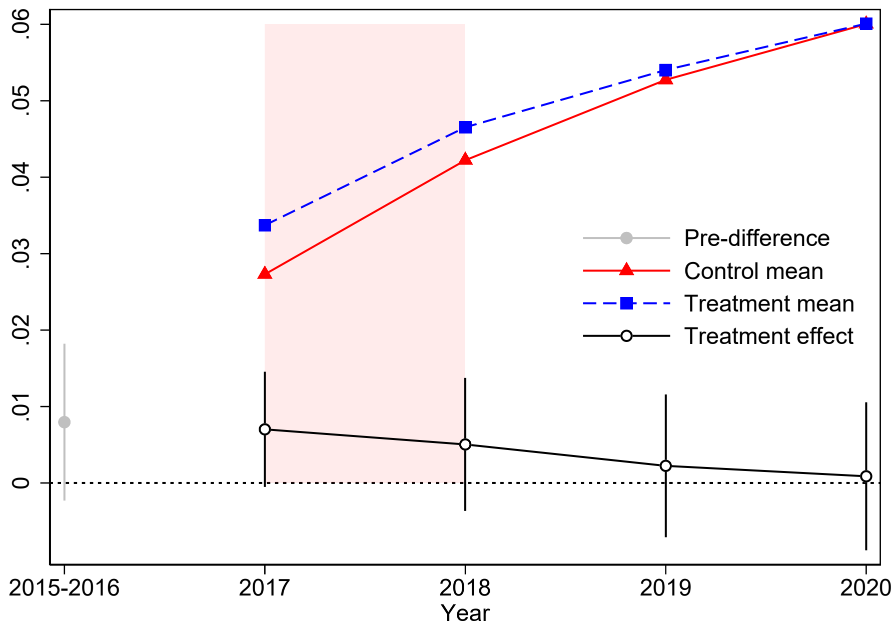


(c) Victims



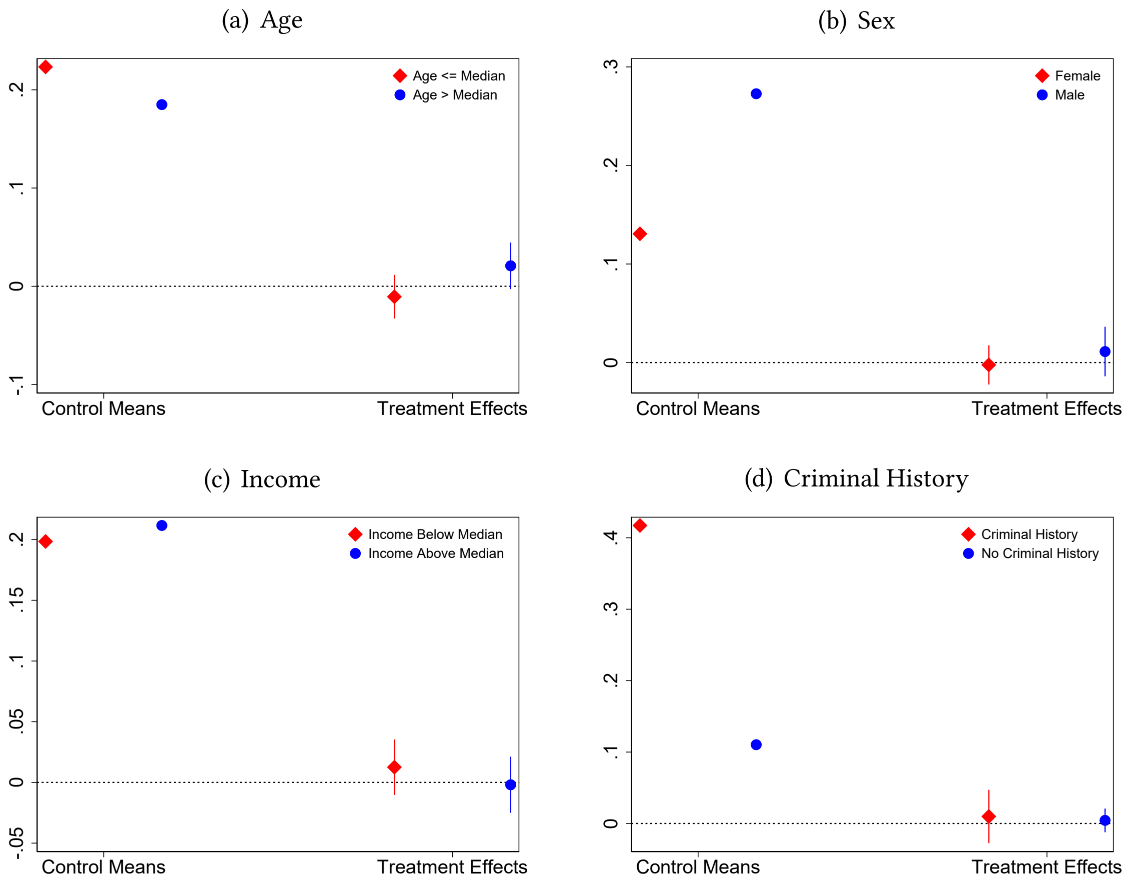
Notes: The figure reports the share of individuals with a criminal history, pooling together the treatment and control groups. Panel (a) shows the share recorded as suspected offenders in a police report in the two years preceding the start of the basic income experiment. Panel (b) shows the share with at least one criminal charge filed in district court in the two years preceding the start of the experiment. Panel (c) shows the share of individuals recorded as victims in the police data. Data and outcomes are defined as described in Section 3.

Figure A6: The Impact of Basic Income Experiment on Disorderly Conduct



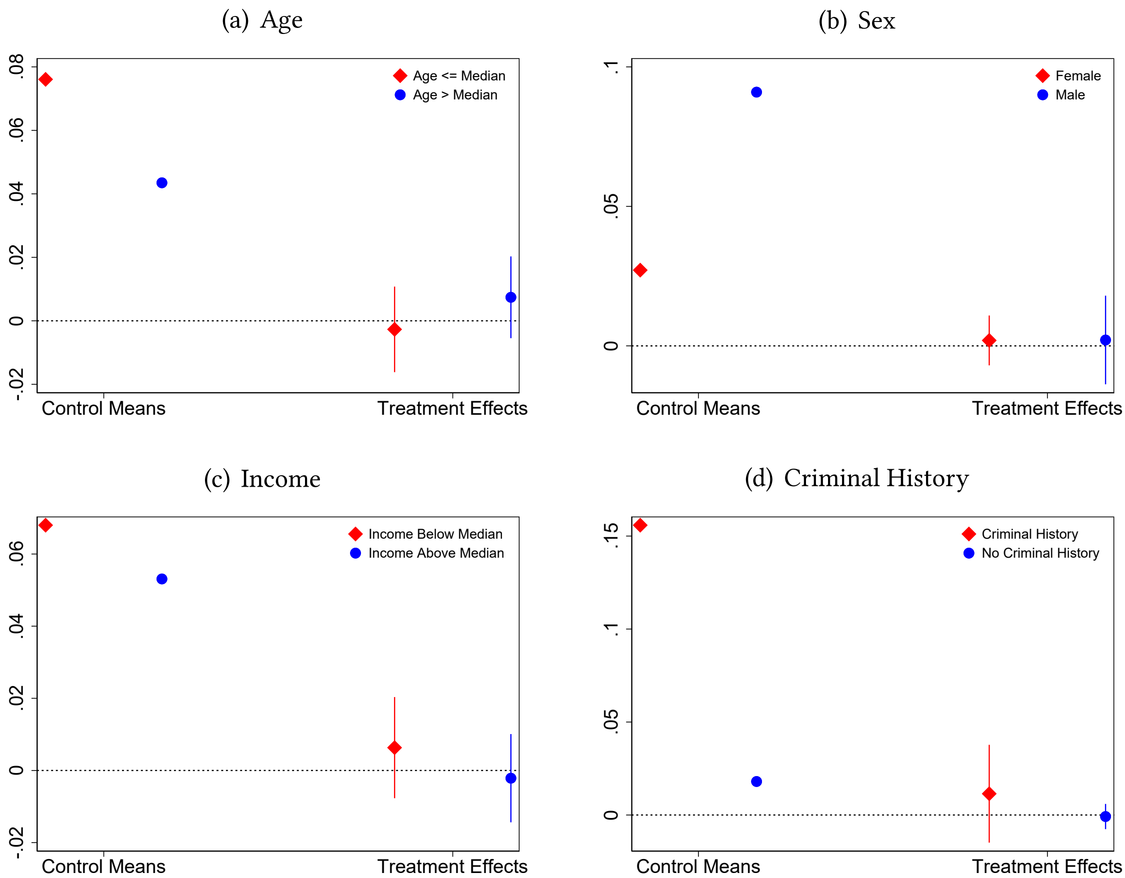
Notes: Figure shows the impact of the basic income experiment on disorderly conduct. Blue squares and red triangles display the raw cumulative means for the treatment and control groups. White dots plot the estimated treatment effects obtained using equation 1 with control variables. Vertical lines denote the 95% confidence intervals. The grey dots represent the pre-treatment difference in the cumulative probability of disorderly conduct two years before the start of the experiment. The shaded area indicates the experimental period (2017–2018). Data and outcomes are constructed as explained in the Section 3.

Figure A7: Heterogeneity in the Impacts on Police Reports



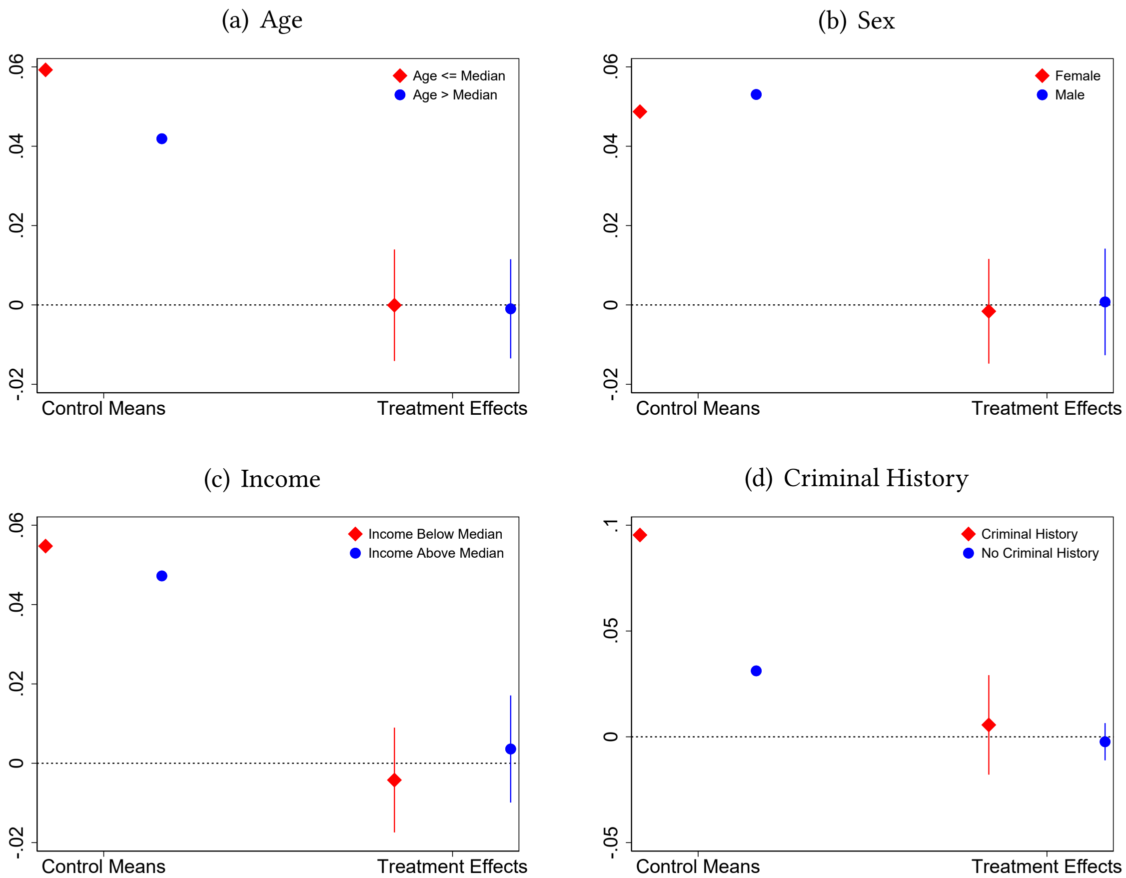
Note: The figure presents heterogeneity in the effect of basic income on the probability of being recorded as a suspected offender in police reports during the experimental period (2017-2018). Panel (a) reports estimates by age, panel (b) by sex, panel (c) by income, and panel (d) by criminal history (defined using police report data). Estimates on the left of each panel show the control and treatment means of the outcome. The estimates on the right show the treatment-effect estimates from equation (1) with controls included. The outcome is the cumulative probability of being a suspected offender in a police report over the experimental period. Lines indicate 95% confidence intervals. Data and outcomes are defined as described in Section 3.

Figure A8: Heterogeneity in the Impacts on Charges



Note: The figure presents treatment-effect heterogeneity in the impact of basic income on the probability of being charged in district court during the experimental period (2017-2018). Panel (a) reports estimates by age, panel (b) by sex, panel (c) by income, and panel (d) by criminal history (defined using police report data). Estimates on the left of each panel show the control and treatment means of the outcome. The estimates on the right show treatment-effect estimates from equation (1) that include controls. The outcome is the cumulative probability of being charged in the district court during the experimental period. Lines indicate 95% confidence intervals. Data and outcomes are defined as described in Section 3.

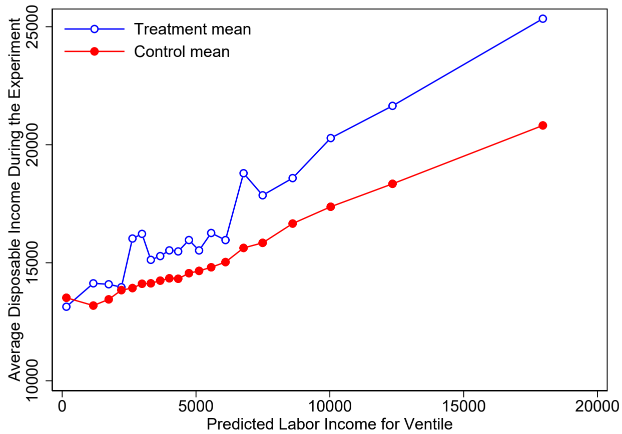
Figure A9: Heterogeneity in the Impacts on Victimization



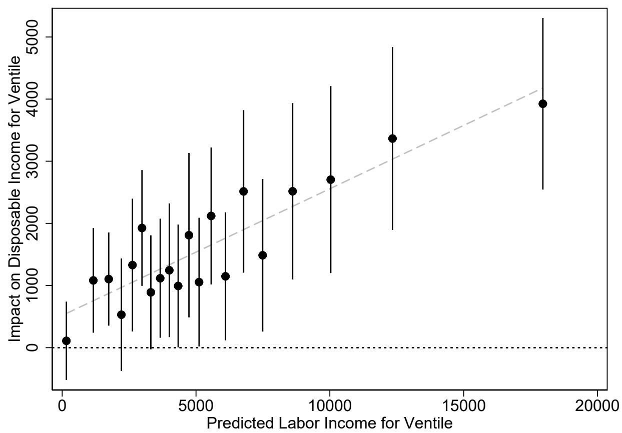
Note: The figure presents treatment-effect heterogeneity in the impact of basic income on the probability of victimization during the experimental period (2017-2018) Panel (a) reports estimates by age, panel (b) by sex, panel (c) by income, and panel (d) by criminal history (defined using police report data). Estimates on the left of each panel show the control and treatment means of the outcome. The estimates on the right show treatment-effect estimates from equation (1) that include controls. The outcome is the cumulative probability of being a victim during the experimental period. Lines indicate 95% confidence intervals. Data and outcomes are defined as described in Section 3.

Figure A10: Heterogeneity in the Impacts on Reports by Predicted Income

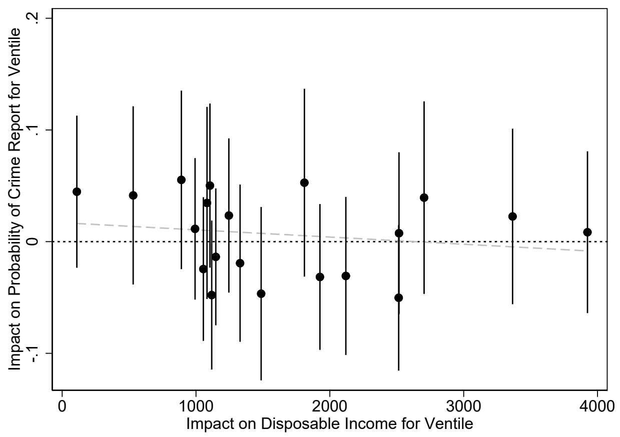
(a) Predicted Labor Income and Observed Disposable Income



(b) Impact on Disposable Income and Predicted Income



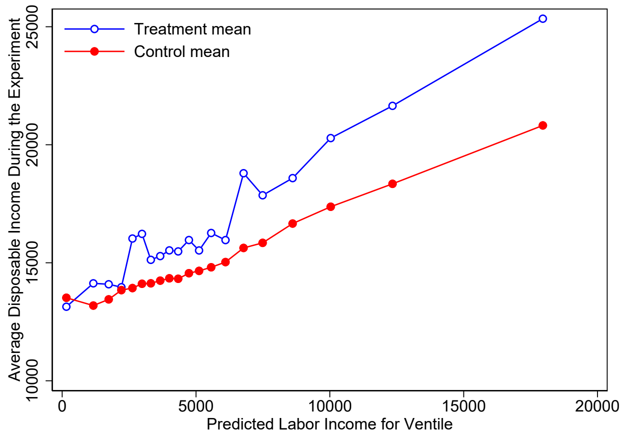
(c) Impact on Reports and Impact on Disposable Income



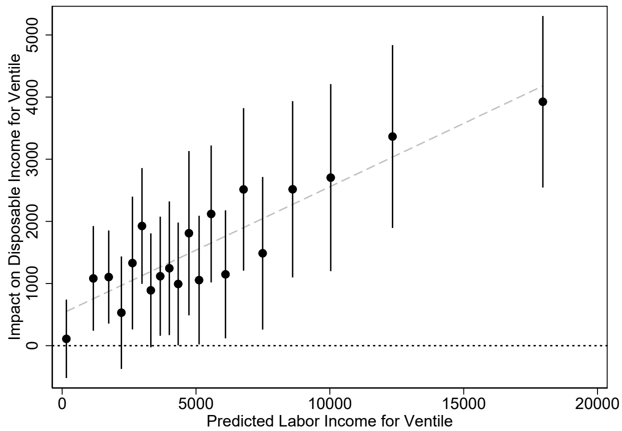
Notes: Panel (a) shows mean disposable income during the experiment for the treatment and control groups across predicted labor-income ventiles. Panel (b) shows estimated treatment effects on mean disposable income during the experiment by predicted labor-income ventiles. Panel (c) plots the relationship between ventile-specific treatment effects on police reports and ventile-specific treatment effects on disposable income during the experiment. We predict labor income for all individuals using a regression model estimated only on the control group, with average labor income during the experiment as the dependent variable. The regressors are the covariates listed in Table 1, as well as labor earnings and the number of months unemployed between 2013 and 2015. To construct predicted ventiles, we split individuals into 20 equal-sized groups based on the predicted values. Treatment Effects on disposable income and police reports are estimated using equation 1 with controls. For panels (b) and (c), each dot represents a separate regression estimated within a ventile.

Figure A11: Heterogeneity in the Impacts on Victimization by Predicted Income

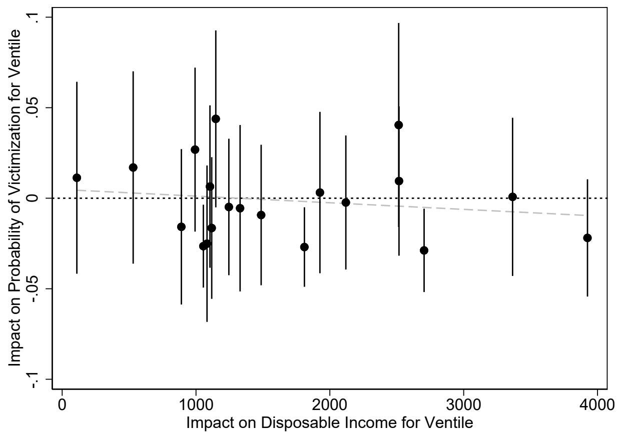
(a) Predicted Labor Income and Observed Disposable Income



(b) Impact on Disposable Income and Predicted Income



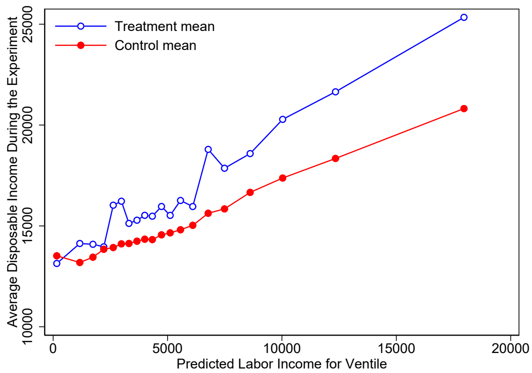
(c) Impact on Victimization and Impact on Disposable Income



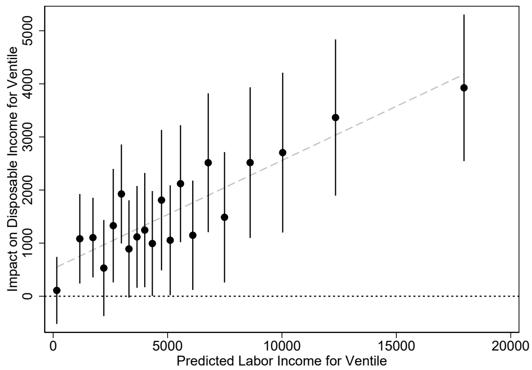
Notes: Panel (a) shows mean disposable income during the experiment for the treatment and control groups across predicted labor-income ventiles. Panel (b) shows estimated treatment effects on mean disposable income during the experiment by predicted labor-income ventiles. Panel (c) plots the relationship between ventile-specific treatment effects on victimization and ventile-specific treatment effects on disposable income during the experiment. We predict labor income for all individuals using a regression model estimated only on the control group, with average labor income during the experiment as the dependent variable. The regressors are the covariates listed in Table 1, as well as labor earnings and the number of months unemployed between 2013 and 2015. To construct predicted ventiles, we split individuals into 20 equal-sized groups based on the predicted values. Treatment Effects on disposable income and police reports are estimated using equation 1 with controls. For panels (b) and (c), each dot represents a separate regression estimated within a ventile.

Figure A12: Appendix Figure Heterogeneity in the Impacts on Property Crime Reports by Predicted Income

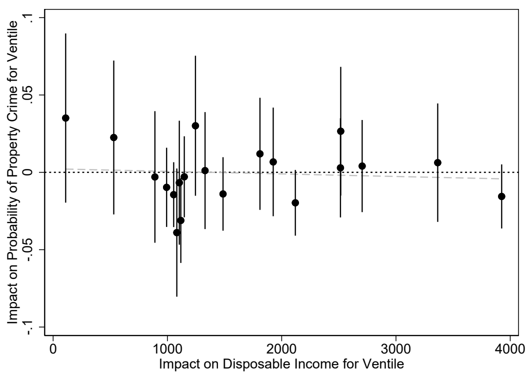
(a) Predicted Labor Income and Observed Disposable Income



(b) Impact on Disposable Income and Predicted Income



(c) Impact on Property crime Reports and Impact on Disposable Income



Notes: Panel (a) shows mean disposable income during the experiment for the treatment and control groups across predicted labor-income ventiles. Panel (b) shows estimated treatment effects on mean disposable income during the experiment by predicted labor-income ventiles. Panel (c) plots the relationship between ventile-specific treatment effects on police reports in which the suspected crime is property crime and ventile-specific treatment effects on disposable income during the experiment. We predict labor income for all individuals using a regression model estimated only on the control group, with average labor income during the experiment as the dependent variable. The regressors are the covariates listed in Table 1, as well as labor earnings and the number of months unemployed between 2013 and 2015. To construct predicted ventiles, we split individuals into 20 equal-sized groups based on the predicted values. Treatment effects on disposable income and police reports are estimated using equation 1 with controls. For panels (b) and (c), each dot represents a separate regression estimated within a ventile.

B Appendix Tables

Table A1: The Impact of Basic Income on Driving Under the Influence, Drug-related, and Income-Generating Crimes

	(1) Police reports	(2) Charges
Panel A: Driving under the influence		
Treatment Effect	0.0022	0.0019
	(0.0031)	(0.0030)
Control Mean	0.0211	0.0182
Observations	172,850	172,850
Panel B: Drug cases		
Treatment Effect	0.0001	0.0020
	(0.0031)	(0.0023)
Control Mean	0.0234	0.0100
Observations	172,850	172,850
Panel C: Income-Generating cases		
Treatment Effect	-0.0002	0.0003
	(0.0041)	(0.0028)
Control Mean	0.0444	0.0211
Observations	172,850	172,850

Note: The table shows results estimating the impact of basic income on the cumulative probability of being suspected of or charged with the crimes specified in each panel. Panel A reports outcomes related to driving under the influence. Panel B reports outcomes related to drug offenses. Panel C reports outcomes related to income-generating offenses, primarily property offenses. Note that our income-generating offenses are almost exactly overlapping with the crimes listed as financially motivated in Tuttle (2019), and the specific crime categories are listed in Appendix Table A10. Column 1 uses police reports as the outcome (an indicator equal to 1 if the individual was suspected of the relevant crime during the experiment, years 2017 and 2018), and column 2 uses charges in district court (an indicator equal to 1 if the individual was charged with the relevant crime during the same period). The treatment effect coefficient equals the β from equation 1. Brackets report heteroskedasticity-robust standard errors. Control mean is the mean of the outcome among the control group. All specifications include controls.

Table A2: The Impact on the Number of Crime Reports During the Experiment

Outcome	Number of Crime Reports	Type of Crime Report: Traffic	Property	Violent	Other
	(1)	(2)	(3)	(4)	(5)
Treatment Effect	0.0536	0.0204	0.0337	0.0049	0.0134
	(0.0480)	(0.0175)	(0.0376)	(0.0063)	(0.0146)
Controls	✓	✓	✓	✓	✓
Control Mean	0.4822	0.2379	0.1534	0.0400	0.1068
Conventional *p-value*		0.2547	0.3425	0.4439	0.4151
FWER *p-value*		0.6272	0.6367	0.6367	0.6367
Observations	172,850	172,850	172,850	172,850	172,850

Note: The table presents results from an analysis similar to that in Table 2 Panel A, but with the number of crime reports as the outcome. In column 1, the outcome is the number of crime reports in which the individual is recorded as a suspect during the experiment (years 2017 and 2018). Columns 2–5 report analogous results for distinct categories of crime. The "treatment effect" coefficient equals the β from equation 1. The brackets show heteroskedasticity robust standard errors. The table also presents conventional and family-wise error rate (FWER) corrected p-values. All the specifications include controls to increase precision. The data and outcomes are constructed as explained in Section 3

Table A3: The Impact on the Number of Charges During the Experiment

Outcome	Number of Charges	Type of Charge: Traffic	Property	Violent	Other
	(1)	(2)	(3)	(4)	(5)
Treatment Effect	0.0086	0.0044	0.0033	-0.0000	0.0009
	(0.0114)	(0.0056)	(0.0073)	(0.0032)	(0.0043)
Controls	✓	✓	✓	✓	✓
Control Mean	0.1055	0.0292	0.0372	0.0165	0.0225
Conventional *p-value*		0.3699	0.6640	0.9889	0.8124
FWER *p-value*		0.8496	0.9575	0.9895	0.9705
Observations	172,850	172,850	172,850	172,850	172,850

Note: The table presents results from an analysis similar to that in Table 2 Panel B, but with the number of criminal charges as the outcome. In column 1, the outcome is the number of criminal charges filed against the individual for crimes committed during the experimental period (2017–2018). Columns 2-5 report similar results from similar analyses, but the outcomes are different crime types. The "treatment effect" coefficient equals the β from equation 1. The brackets show heteroskedasticity robust standard errors. The table also presents conventional and family-wise error rate (FWER) corrected p-values. All the specifications include controls to increase precision. The data and outcomes are constructed as explained in Section 3.

Table A4: The Impact on the Number of Victimization Reports During the Experiment

Outcome	Number of Victimizations	Type of Victimization: Traffic	Property	Violent	Other
	(1)	(2)	(3)	(4)	(5)
Treatment Effect	0.0070	0.0018	0.0006	0.0024	0.0044
	(0.0084)	(0.0022)	(0.0042)	(0.0049)	(0.0044)
Control Mean	0.0645	0.0069	0.0184	0.0326	0.0148
Conventional *p-value*		0.3448	0.8547	0.6441	0.1901
FWER *p-value*		0.7611	0.8921	0.8921	0.6507
Observations	172,850	172,850	172,850	172,850	172,850

Note: The table presents results from an analysis similar to that in Table 3, but with the number of victimization reports as the outcome. Column 1 reports results when the outcome is the number of victimization reports in which the individual was identified as a victim during the experimental period (2017–2018). Columns 2–5 report analogous results for different categories of crime. The "treatment effect" coefficient equals the β from equation 1. The brackets show heteroskedasticity robust standard errors. The table also presents conventional and family-wise error rate (FWER) corrected p-values. All the specifications include controls to increase precision. The data and outcomes are constructed as explained in the section 3

Table A5: The Impact of Basic Income on Probability of Being Suspected of Crime During the Experiment Using a Specification without Controls

Outcome	Any Crime Report	Type of Crime Report: Traffic	Property	Violent	Other
	(1)	(2)	(3)	(4)	(5)
Treatment Effect	0.0034	0.0047	-0.0014	0.0008	0.0030
	(0.0092)	(0.0081)	(0.0046)	(0.0039)	(0.0052)
Control Mean	0.2051	0.1481	0.0444	0.0296	0.0521
Conventional *p-value*		0.5591	0.7570	0.8429	0.5443
FWER *p-value*		0.9535	0.9535	0.9535	0.9535
Observations	172,850	172,850	172,850	172,850	172,850

Note: The table presents results from an analysis similar to that in Table 2 Panel A, but using a specification that excludes control variables. Column 1 reports results using a binary outcome equal to 1 if the individual was suspected of any crime during the experimental period (2017–2018). Columns 2–5 report analogous results for different categories of crime. The "treatment effect" coefficient equals the β from equation 1. The brackets show heteroskedasticity robust standard errors. The table also presents conventional and family-wise error rate (FWER) corrected p-values. The data and outcomes are constructed as explained in the section 3.

Table A6: The Impact of Basic Income on Criminal Charges During the Experiment Using a Specification without Controls

Outcome	Any Charge	**Type of Criminal Charge** Traffic	Property	Violent	Other
	(1)	(2)	(3)	(4)	(5)
Treatment Effect	0.0007	0.0020	-0.0004	-0.0015	-0.0009
	(0.0054)	(0.0035)	(0.0032)	(0.0026)	(0.0029)
Control Mean	0.0606	0.0223	0.0211	0.0147	0.0181
Conventional *p-value*		0.5555	0.9050	0.5759	0.7702
FWER *p-value*		0.9545	0.9545	0.9545	0.9545
Observations	172,850	172,850	172,850	172,850	172,850

Note: The table presents results from an analysis similar to that in Table 2 Panel B, but using a specification that excludes control variables. Column 1 reports results using a binary variable equal to 1 if the individual was charged in district court for a crime committed during the experimental period (2017–2018). Columns 2–5 report analogous results for different crime categories as outcomes. The "treatment effect" coefficient equals the β from equation 1. The brackets show heteroskedasticity robust standard errors. The table also presents conventional and family-wise error rate (FWER) corrected p-values. The data and outcomes are constructed as explained in Section 3

Table A7: The Impact of Basic Income on Victimization During the Experiment Using a Specification without Controls

Outcome	Any Charge	**Type of Victimization Charge** Traffic	Property	Violent	Other
	(1)	(2)	(3)	(4)	(5)
Treatment Effect	-0.0014	0.0014	-0.0025	0.0003	-0.0003
	(0.0049)	(0.0020)	(0.0027)	(0.0036)	(0.0025)
Control Mean	0.0510	0.0068	0.0166	0.0260	0.0125
Conventional *p-value*		0.4638	0.3888	0.9282	0.8894
FWER *p-value*		0.8561	0.8541	0.9880	0.9880
Observations	172,850	172,850	172,850	172,850	172,850

Note: The table presents results from an analysis similar to that in Table 3, but using a specification that excludes control variables. Column 1 reports results using a binary variable equal to 1 if the individual was reported as a victim in a police report during the experimental period (2017–2018). Columns 2–5 report analogous results for different categories of crime as outcomes. The "treatment effect" coefficient equals the β from equation 1. The brackets show heteroskedasticity robust standard errors. The table also presents conventional and family-wise error rate (FWER) corrected p-values. The data and outcomes are constructed as explained in Section 3

Table A8: The Impact of the Basic Income Experiment on Labor Market Outcomes

Outcome:	**Disposable Income** (1)	**Labor Income** (2)	**Employed** (3)	**Unemp. Months** (4)
Panel A: Year 2017				
Treatment Effect	1,347.153	-55.725	0.025	-0.113
	(122.154)	(148.464)	(0.010)	(0.091)
Control Mean	14,599.002	4,325.774	0.259	5.900
Observations	173,918	173,918	173,918	173,918
Panel B: Year 2018				
Treatment Effect	1,821.673	217.228	0.023	-0.404
	(162.822)	(216.317)	(0.010)	(0.099)
Control Mean	15,712.712	6,788.194	0.334	4.753
Observations	172,850	172,850	172,850	172,850

Note: The table reports the impact of the basic income experiment on labor market outcomes. Columns (1)–(4) report results when outcomes are: 1. disposable income (including the basic income transfer), 2. labor earnings, 3. an employment indicator that equals 1 if employed at year-end, and 3. total months unemployed during the year. Panel A reports results for 2017. Panel B reports results for 2018. The treatment effect equals β from Equation 1. Heteroskedasticity-robust standard errors are reported in brackets. Data and variable definitions are described in Section 3

C Heterogeneity Using Causal Forest

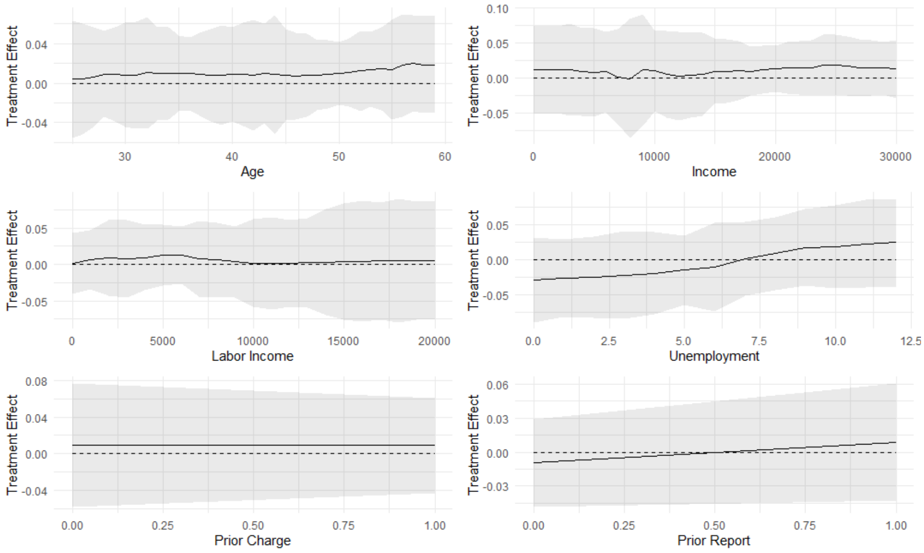
In addition to testing treatment effect heterogeneity through interactions in linear probability models, we also, consistent with our pre-analysis plan, estimate causal forests as an exploratory way to see if we can detect any subgroups for whom the treatment would have been particularly effective. These models were run using the R library "grf" (Tibshirani, Athey, Sverdrup, Wager).

The causal forest algorithm requires a reasonable balance of treatment and control observations, which is not the case in our raw data, where only 1.1 percent of observations belong to the treatment group. For this reason, we did not use the entire control group, but rather drew a random sample of 10,000 controls for this specification. We only used observations for whom there were no missing values for any of the background variables, which reduced the size of the treatment group to 1,977 individuals. We included 14 potential effect modifiers: the same 12 sociodemographic variables listed in Table 1 and two variables measuring criminal background (any charge or any report in the two years preceding the reform).

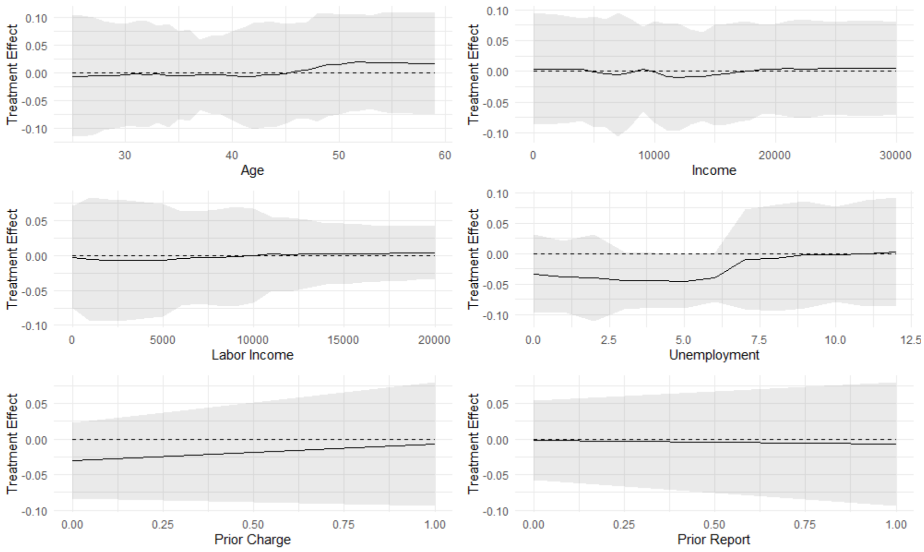
For the "any report" outcome, the variable importance plots (share of trees in a random forest using this variable as a split) suggest that the most important background variables that could explain effect heterogeneity are total income, age, prior charge, months unemployed, labor income, and prior police report. However, the test of significance of overall effect modification suggests that no significant effect heterogeneity in terms of the examined variables is present. A similar result is obtained by rank-weighted average treatment effects (RATE), calculated as the area under the targeting operating characteristic curve (AUTOC). This finding is confirmed by examining effect modification for the individual background variables. Higher age, lower income, and longer unemployment before the reform appear to be associated with worse outcomes. Still, the overwhelming conclusion from the heterogeneity analysis is that the main effect and all subgroup effects are zero.

Figure A13

(a) Crime Reports



(b) Charges



Notes: The figure presents the mean predicted conditional average treatment effects over different individual characteristics obtained using the method of Athey *et al.* (2019). In panel (a), the outcome is the police report during the experiment. In panel b, the outcome is a charge during the experiment.

D Variable Definitions

D.1 Crime Variables

We construct our crime groups using Statistics Finland's two-figure categories (Appendix Table A10). Our crime groups are

1. Traffic crime (EE + Road Traffic Act (103§, 105a§), Vehicles Act (96§), Driving Licences Act)
2. Property Crime (AA)
3. Violent Crime (BB + CC)
4. Other (Other offenses)

Table A9: Variable Definitions

Variable	Definition
Demographics	
Disposable Income	Consists of earned, entrepreneurial, and property income as well as of transfers received after taxes and tax-deductible expenses. (*kturaha_k*).
Unemployment months	Number of months unemployed during the calendar year. 0 if no unemployment months. (*tyke*)
Female	Binary indicator equal to 1 if the individual is female, 0 otherwise. *sukup* in folk module.
Age	Age in years on the last day of the year. (*ika*).
Secondary Education	Binary indicator equal to 1 if the highest completed education is secondary level, 0 otherwise. (*ututku_aste* = 3)
Any Post Secondary Education	Binary indicator equal to 1 if highest completed education is tertiary level, 0 otherwise (*ututku_aste* > 3)
Married	Binary indicator equal to 1 if married or in registered partnership, 0 otherwise (*sivs* = 2)
N of Children	Number of children in family, 0 is missing. (*lkm_k*)
Foreign Language	Binary indicator equal to 1 if native language is not Finnish or Swedish, 0 otherwise ((*kieli_k = 1 or kieli_k = 2*)
Capital Region	Binary indicator equal to 1 if residing in the Helsinki metropolitan area, 0 otherwise (*mkunta* = 1)
Criminal Background	
Any crime report, year t-1 to -2	An indicator variable equal to 1 if the individual was a crime suspect during two years before the experiment, and 0 otherwise.
Any charge, year t-1 to -2	An indicator variable equal to 1 if the individual faced any criminal charges during two years before the reference year, and 0 otherwise.
Any traffic crime report, t-1 to -2	Binary indicator equal to 1 if individual suspected traffic-related crime reports two years before the experiment.
Any property crime report, t-1 to -2	Binary indicator equal to 1 if individual suspected any property-related crime reports two years before the experiment.
Any violent crime report, t-1 to -2	Binary indicator equal to 1 if individual suspected any violent crime reports two years before the experiment.
Any other crime report, t-1 to -2	Binary indicator equal to 1 if individual suspected any other type of crime reports two years before the experiment.

The first column lists the variable name. The second column offers a brief explanation of how the variable is constructed. The name in parentheses denotes the variable in the folk module used to construct the variable used in the analysis.

Table A10: Statistics Finland's Two-figure Category of Offences 2017

Code	Offense Category
AA Offences against property	
00	Theft, aggravated theft (28:1-2, 34a:1)
01	Petty theft (28:3)
02	Embezzlement (28:4-6)
03	Fraud, insurance fraud, means of payment fraud (36:1-4, 37:8-11)
04	Tax and subsidy offences (29:1-8)
05	Robbery, extortion (31:1-4)
06	Criminal damage (35:1-3, 34a:1/3)
07	Unauthorised use (28:7-9)
08	Receiving offence, money laundering (32:1-10)
09	Other offences against property (28:10-12, 36:5-7, 39:1-6, 46:1-12)
0A	Stealing of a motor vehicle for temporary use (28:9, 34a:1§11/3)
BB Offence against life and health	
10	Manslaughter, Murder, Killing, Infanticide (21:1-4, 34a:1§1/2,6,7)
11	Aggravated assault, brawling (21:6,12)
12	Assault, petty assault (21:5,7)
15	Negligent homicide (21:8-9)
18	Negligent bodily injury (21:10-11)
19	Other offence against life and health (12:6a,13-14, 22:1-6, 34a:1§1/2)
CC Sexual offence	
20	Sexual abuse of a child (20:6-7)
21	Rape, aggravated rape (20:1-2)
22	Other sexual offence (20:4-5, 8-9)
DD Offences against public authority and perjury	
30	Resistance to a public official (16:1-2)
31	Obstruction of a public official (16:3)
32	False statement, giving false identifying information, providing false documents to a public authority (15:1-5, 16:5,8)
33	Other offences against public authority and perjury (15:6-11, 16:4, 6-7, 9-17, 17:1-22, 34a:1§1/2)

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Table A10 – continued from previous page

Code	Offense Category
EE Traffic offences	
80	Causing a traffic hazard (23:1)
81	Causing a serious traffic hazard (23:2)
50	Driving while intoxicated (23:3)
51	Driving while seriously intoxicated (23:4)
52	Other traffic intoxication (23:5-7)
53	Relinquishing a vehicle to an intoxicated person (23:8)
84	Other traffic offences (23:9-11)
FF Other offences against the Penal Code	
41	Weapons offences, offences endangering health and safety (41, 44 chapter)
60	Criminal mischief, criminal traffic mischief (34:1-3, 34a:1§1/3,5)
61	Forgery, counterfeiting (33:1-5, 37:1-7)
65	Military offences (45 chapter)
66	Employment offences (47 chapter)
67	Environmental offences (48 chapter, 34a:1§1/4)
76	Alcohol offences (50(a) chapter)
74	Narcotics offences (50 chapter)
62	Other (11-14,18,24,25,30,38,40,48a,49,51 chapters, 34:4-11, 34a:1§1/1,3-5, 34a:2-5)
GG Offences against other Acts	
73	Alcohol Act (50a:4,6)
83	Road Traffic Act (103§, 105a§), Vehicles Act (96§), Driving Licences Act (93§, 94§)
90	Security Stewards Act (26§), Assembly Act (26§)
91	Code of Judicial Procedure (17:36), Criminal Procedure Act (8:4-5)
93	Offences against other Acts and Decrees
99	Offence category unknown

E Changes from the Pre-Analysis Plan

We did not design the experiment, but we wrote a pre-analysis plan before being granted access to the data. The pre-analysis plan is available here. The analysis in the paper follows the pre-analysis plan closely, but there are a few instances where we made small changes:

- The pre-analysis plan states that we would also examine the impact on domestic violence. However, Statistics Finland granted us access only to data that contains individuals who participated in the experiment or are found in police reports or charge data. Therefore, we were unable to conduct the analysis. We noted in the pre-analysis that there was some uncertainty around victimization data, implying it was unclear whether we could study domestic violence. Further, our results on victimization suggest that there is no reason to expect that the experiment affected domestic violence, as we find no impacts on violent crime victimization.

- The pre-analysis plan states that we would have a graph showing the mean monthly outcomes for the treatment and control groups. However, the monthly outcomes are very noisy. Hence, we decided to plot yearly outcomes.

- We wrote that we would examine heterogeneity in the effects using machine learning tools proposed by Athey *et al.* (2019) and Chernozhukov *et al.* (2023). However, as the results we obtained using the method suggested by Athey *et al.* (2019) indicated that there is very little heterogeneity, we decided not to conduct the analysis using the approach of Chernozhukov *et al.* (2023).

- The pre-analysis plan stated that when we study the impact on secondary outcomes, we would correct for multiple hypothesis testing using both Family-Wise Error Rate (FWER) and False Discovery Rate. However, we only implemented FWER correction as it's often more conservative.