

Income-Based Fines: Evaluating Rationales for Finnish Speeding Penalties

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Abstract

This paper evaluates rationales for income-based speeding fines. Such fines are famously applied in Finland, where a \$250 offense for a low-income speeder can cost \$100,000 for the rich. We consider four leading policy rationales—externality mitigation, redistribution, equal compliance, and proportional punishment—using linked Finnish administrative data and an original survey. First, using a “job-loss” design, we find that marginal speeding costs *decrease* with offender income, leading us to reject externality mitigation as a rationale. Next, we assess the redistributive rationale. In standard models, speeding fines can aid redistribution only if preferences for speeding co-vary with income. We recover preference heterogeneity by differencing speeding behavior with respect to causal effects of income on speeding, estimated from within-individual earnings variation and inheritance

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shocks. The estimated preferences are *negatively* correlated with earnings, implying that Mirrleesian redistribution rationalizes a *lower* fine on the rich. Finally, we use our survey to assess whether preferences for equal compliance or proportional punishment are motivating rationales. Respondents trade off fixed and income-based speeding fine policies, where we vary the induced across-income distribution of behavior and fine-schedule “steepness” of the latter. Net of redistribution, respondents are on-average willing to forgo €144 million in government revenue to implement income-based policies. However, these valuations are insensitive to induced behavior or schedule-steepness, ruling out our candidate rationales. We conclude that income-based fines cannot be justified using leading economic or fairness-based rationales for their existence.

1 Introduction

Governments use punishments to deter anti-social behavior, be it tax evasion, littering, or murder. In developed countries, the majority of offenses committed are punished with a fine. However, despite their ubiquity, there is substantial heterogeneity in how fines are determined. While fines commonly scale with the severity of the offense, several countries (e.g. Germany, Switzerland, Denmark, Sweden, and Finland) also scale some fines with income. In Finland, the oldest and most comprehensive income-based system, the rich can receive fines in excess of €100,000 for speeding offenses that would cost a few hundred euros for low-income drivers.¹

How can we rationalize income-based fine systems? More broadly, are they effective in achieving their economic and/or policy goals? In this paper, we present the leading economic and fairness-based rationales that have been put forth for these policies and test whether any of these motives can explain the Finnish speeding-fine system.

On the economic side, two frameworks are readily applicable to speeding fines. First, we consider the optimal penalty framework in Becker (1968), which prescribes that fines be set equal to the marginal social cost of crime. In this framework, income-based speeding fines can be rationalized if the marginal social cost of speeding increases with income. Second, we apply a Mirrleesian optimal redistribution framework to our setting. In this framework, the planner’s objective is to redistribute resources from high- to low-ability types, but the un-observability of type means the planner must turn to distortionary taxation. In the canonical Atkinson-Stiglitz result (Atkinson and Stiglitz, 1976), it is most efficient for the planner to use income taxes, rather than commodity taxes, to redistribute resources when consumption preferences are homogeneous. Intuitively, differentially taxing the goods that the rich buy with their income is just as distortionary as taxing their income while also generating a consumption distortion. However, subsequent work (Saez, 2002a; Allcott et al., 2019; Ferey et al., 2024) shows that indirect taxes can lower the efficiency costs of redistribution if preferences for the taxed behavior co-vary with income. Intuitively, if the rich have a fixed preference for a good, differentially taxing that good will not distort their earnings choice.

¹For example, Andres Wiklöf was fined €121,000 for speeding in 2023; in 2002, Nokia executive Anssi Vanjoki received a speeding fine of €116,000.

Building on these results, we show that speeding fines should increase with income if the relative preference for speeding increases with income, giving us a testable benchmark against which to assess redistributive rationales for income-based fines.

Fairness rationales have also featured prominently in the debate surrounding income-based fines. For example, official argumentation in the lead up to the implementation of Finland’s policy cited a desire to equalize the “impact” of fines on people of varying means (Lahti, 2021). One interpretation consistent with this desire is that income-based fines seek to equalize the *utility* cost of crime across incomes. As we show in section 3, with log-utility over consumption, speeding fines that are proportional to net-of-tax income (as in Finland) ensure that marginal utility costs of speeding are equal across incomes. Therefore, if Finland’s income-based fine policy is an instrument for achieving equal utility costs across incomes, then policy preferences should load on the shape of the fine schedule itself. However, under the same log-utility model, proportional fines can also generate equal speeding *rates* across incomes, consistent with scholarly work citing deterrence across incomes as an attractive theoretical basis for income-based systems (Kantorowicz-Reznichenko, 2013). Furthermore, as we will show, income-based speeding fines in Finland are empirically associated with across-income equality in speeding rates. Therefore, cited rationales appear ambiguous: they may be motivated by either a particular fine schedule (e.g., as an instrument to achieve equal utility costs of crime) or a particular behavioral outcome (e.g., equal speeding rates across incomes). We therefore test whether the equal distribution of behavior (hereafter, “equal compliance”) or the proportional schedule itself (hereafter, “proportional-punishment”) is the instrumental objective of Finland’s policy.

To empirically evaluate each of these four rationales, we draw on linked Finnish administrative data and construct an original survey that we administer to a representative sample of Finnish residents.

To evaluate the Beckerian rationale, we measure how the marginal social cost of speeding varies with income using the injuries that result from accidents, as collected in accident report data. Linking this information to data from income-tax returns and crime histories, we leverage cross-sectional variation in rates of speeding-ticket receipt to measure the marginal impact of speeding on accident-related injuries across income levels. To address endogeneity

concerns, we include controls for non-speeding traffic-crime rates, car ownership status, and annual driving distance (measured using odometer data from vehicle inspections). With respect to injuries, we find that the slope of marginal speeding costs is *negative*: the marginal impact of speeding on accident-related injuries is *lower* at high incomes. To translate the marginal speeding cost gradient into a money-metric, we measure the cost of each injury using a “job-loss” design (von Wachter and Bender, 2006); this approach allows us to capture across-victim heterogeneity in costs and to separate fiscal externalities from net-income losses, which may be weighed differently in the planner’s objective. In particular, we compare the earnings trajectories of individuals injured in an accident to those of demographically-matched individuals from the general population to estimate the net-present-value (NPV) cost of each injury. We apply welfare weights (Saez, 2002b) to NPV net-income losses to account for the possibility that the planner weighs a given income loss differently depending on the income of the victim. With our NPV costs in-hand, we re-estimate the slope of marginal speeding costs with respect to income. Regardless of the discount rate we apply to future injury costs or how we welfare-weight net-income losses, we find a consistently *negative* slope. In our preferred estimates, we find that the marginal social cost of speeding decreases by €7.6 (se=4.4) for every €10,000 of income, well below our estimate of the overall slope of the Finnish speeding fine schedule (+12.4 €). We therefore reject the externality-based rationale.

Turning to the redistributive rationale, we measure the extent to which relative speeding preferences are correlated with income. As clarified by Allcott et al. (2019) and Ferey et al. (2024), these preferences can be identified by differencing observed speeding rates with respect to causal earnings effects on speeding, where the latter are identified from variation in *labor supply*. Intuitively, the relative efficiency of using speeding fines rather than income taxes for redistribution depends on the *labor-supply* distortion induced by speeding fines, which is proportional to the causal earnings effect. Previous literature plausibly assumes weak separability between labor supply and consumption (Allcott et al., 2019; Ferey et al., 2024), implying that the earnings effect can be identified using any exogenous income variation. However, we reject this assumption in our setting, meaning that the earnings effect is the sum of a labor supply effect (holding income fixed) *and* an income effect (holding labor supply fixed). To measure the labor supply effect, we use a yearly panel of income and speeding ticket receipt and leverage within-individual

variation in months-employed. We estimate an average labor-supply effect that is about 25% as large as the across-income gradient of speeding ticket receipt. Due to mis-measured labor supply, the coefficient on earnings in this regression is likely upward-biased, so we turn to an inheritance-shock approach to estimate the income effect. Building on Nekoei and Seim (2022), we exploit the differential timing of first parental deaths as an exogenous income shock; to address violations of the exclusion restriction, we also difference with respect to inheritance size, yielding a triple-differences approach. From this approach, we estimate an income effect that is roughly equal to the average labor supply effect. We then difference observed speeding behavior with respect to our earnings effects and find that relative speeding preferences are *negatively* correlated with income, implying that Mirrleesian redistributive motives rationalize a *lower* fine for the rich. Across various assumptions about the planner’s redistributive preferences, we find consistently negative fine gradients, leading us to reject the redistribution-based rationale for income-based fines.

Finally, to evaluate the equal-compliance and proportional-punishment rationales, we turn to our survey. In particular, we ask Finnish residents to make trade-offs between hypothetical income-dependent and fixed-fine policies using government revenue (our willingness-to-pay medium). We vary two key elements of the policy comparison: the distribution of behavior under the income-dependent policy and the “steepness” of the income-dependent fine schedule. We hold total speeding fixed across policies to address safety-related confounds; to address redistributive motives, we difference WTP for income-dependent fines with respect to WTP for income-dependent transfers. Net of redistribution, 56% of respondents have a positive WTP for income-dependent fines, with respondents on-average willing to forgo €144 million in government revenue to implement an income-based policy. However, we find relatively small and insignificant responses to the randomized fine-policy characteristics described above. To assess the robustness of our findings, we explore heterogeneity with respect to other randomized characteristics of the policy comparison and respondents’ background characteristics; across sub-groups, respondents consistently value income-based fines but remain unresponsive to randomized features of the comparison. While the survey results are generally too noisy to reject our hypothesized fairness rationales, we find no significant evidence in their favor.

In summary, our results suggest that leading economic and fairness-based rationales for income-dependent fines are inadequate to account for their existence. Therefore, accounting for the popularity of such policies would require an appeal to either alternative fairness consideration (e.g., procedural fairness) or a non-standard economic model (e.g., income-correlated mis-perceptions). Nonetheless, we find that Finnish residents have a strong residual willingness-to-pay for income-based fines that is orders of magnitude larger than the inefficiencies resulting from externality- and redistribution-based motives. Taking these expressed preferences seriously implies that optimal fines should incorporate *some* income-dependence, but need not exhibit the extremes of the Finnish system.

After discussing related literature and contributions, our paper proceeds as follows. First, section 2 discusses our setting: speeding fines in Finland. We then turn to a theoretical framework (section 3), which clarifies the empirical objects we plan to measure for assessing each of the four rationales we consider. We briefly discuss data (section 4) before testing the externality- (section 5) and redistribution-based (section 6) rationales. We then turn to our survey (section 7), which we use to assess the fairness-based rationales of equal-compliance and proportional-punishment. Section 8 discusses our results and concludes.

1.1 Related Literature

This projects speaks to three main literatures. First, there is an extensive literature that explores the redistributive potential of commodity taxes in Mirrleesian models (Mirrlees, 1971) where income taxes are present. The benchmark result, due to Atkinson and Stiglitz (1976), shows that, under preference homogeneity, commodity taxes are inefficient (relative to income taxation) for redistributing resources. Saez (2002a) clarified conditions under which this result breaks down, and more recent work (Allcott et al., 2019; Ferey et al., 2024) shows how to identify the redistributive value of commodity taxes for optimal tax design using the difference between observed across-income consumption variation and causal effects of earnings on consumption. While we build methodologically on this literature, our paper differs in two key ways. First, empirically, recent work that identifies earnings effects (Allcott et al., 2019; Ferey et al., 2024) plausibly relies on weak separability assumptions between labor supply and consumption, implying

that earnings effects can be identified using any exogenous income variation. However, we test for and reject weak separability in our setting; as a result, our empirical approach involves estimating both labor supply and income effects on consumption. Second, rather than seeking to identify optimal tax rates, as Allcott et al. (2019) does for soda and Ferey et al. (2024) does for savings, we invert the approach, using the optimal redistributive speeding fine as a benchmark against which to test rationales for a given policy.

In relation to this inversion approach, our paper also relates to the “inverse optimum” literature (Christiansen and Jansen, 1978; Bourguignon and Spadaro, 2012; Bargain et al., 2014; Hendren, 2020), which uses analytical results from optimal tax theory and estimates of behavioral elasticities to infer society’s revealed redistributive preferences. This approach has the benefit of removing the difficulty of selecting normative criteria, treating this as an empirical inference exercise. However, such an approach leaves the source of society’s preferences opaque and can lead to results that contradict conventionally held assumptions (Lockwood and Weinzierl, 2016) with little ability to diagnose the source of the inconsistency. Our paper follows this literature in spirit but seeks to open the black box of society’s revealed preferences, enabling us to assess their normative consistency.

Finally, this work relates to a theoretical and experimental literature on domain-specific fairness. On the theoretical side, specific egalitarianism (Tobin, 1970) argues that specific goods should be equally allocated; recent experimental evidence provides support for this view in the domains of health care and legal aid (Caspi et al., 2024). In our setting, the fact that high-income individuals consume more speeding (a good allocated by the justice system) may be viewed as a violation of specific egalitarianism.² Furthermore, in the context of real-stakes games, a wealth of lab evidence has examined under what conditions individuals favor equal allocations, findings that have given rise to numerous fairness theories (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000; Charness and Rabin, 2002; Engelmann and Strobel, 2004). However, work that explicitly links such fairness concerns to observed policy remains limited. Our paper makes progress on this front by taking as its object a policy (income-based speeding fines) that is associated with equality of behavior and eliciting preferences for such equality among individuals subjected to the policy. This approach bears similarities to Rafkin and Soltas

²Repeat offender penalties can also be viewed as part of this objective.

(2025), which uses experimentally elicited social preferences among landlords and tenants to conduct counterfactual analysis of rental assistance policies.

2 Setting: Speeding Fines in Finland

We now emphasize key features of our empirical setting. We use such features to inform our model and empirical approach.

2.1 Day Fine System in Finland

The first iteration of Finland’s income-based fine system was implemented in 1921. Official argumentation in the lead-up to the implementation cited a desire to reduce the number of crimes punished by prison and equalize the “impact” of fines on people with varying means (Lahti, 2021). As a result, except for minor offenses (e.g. littering), all financial punishments are income-based. Each crime is punished with a fixed number of “day units.” The value of each day-unit I depends on an individual’s net-of-tax income according to the following formula:

$$I = \frac{y_m - 255}{60} - 3 \times D \quad (2.1)$$

where y_m is monthly net-of-tax income and D is an individual’s number of dependents. Other than a €255 basic-living deduction, fines are proportional to half-a-day’s net income, hence the “day fine” terminology.

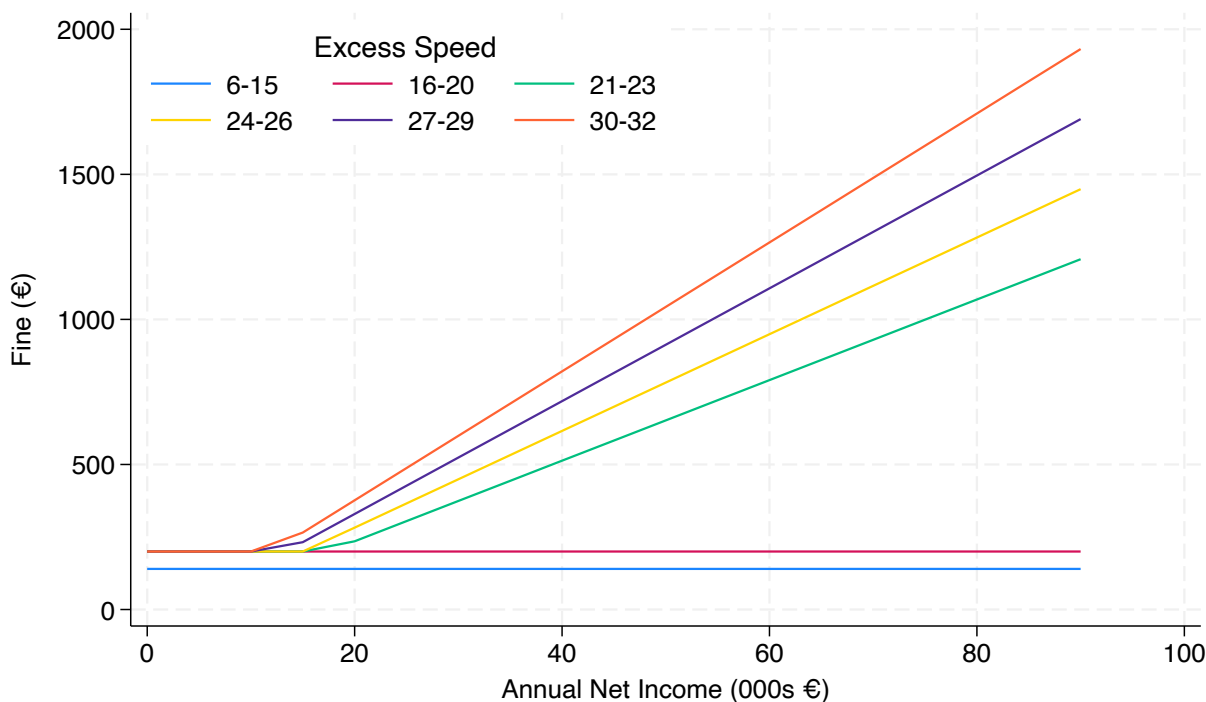
Speeding is partially subjected to the income-based system: drivers are given fixed fines of 140-200 euros for exceeding the limit by 7-20 km/h (minor traffic infractions) and receive income-dependent fines for exceeding the limit by more than 20 km/h.³ To visualize this schedule, figure 2.1 displays hypothetical speeding fines for a 100 km/h limit as a function of net-of-tax income for different levels of “excess speed” (*i.e.* the amount by which a person exceeds the speed limit).⁴ For “minor” speeding crime (7-20 km/h over the limit), fines are constant across incomes; however, for day-fine eligible speeds, fines are essentially proportional to net-of-tax income except for a fine-floor that

³Drivers are given grace for exceeding the limit by no more than 6 km/h.

⁴While the income-dependent cutoff (21 km/h over the limit) is constant across limits, a given excess speed will have larger fines at lower speed limits.

holds at low incomes.⁵ The proportionality is shown explicitly in Appendix figure 9.1: above the fine floor, income-dependent fines constitute a fixed share of monthly net-of-tax income. We also note that there is no ceiling for speeding fines in Finland: individuals can receive arbitrarily large fines at arbitrarily high incomes.

Figure 2.1: Hypothetical Speeding Fine Schedule (100 km/h limit)



Hypothetical speeding-fine schedule, as a function of net income, separately by the amount by which a driver exceeds the limit (in kilometers per hour). Net income consists of earned, capital income, and transfers received, net of tax payments. To construct this schedule, we use equation 2.1 (assuming no dependents) for the monetary value of a day unit, together with information from the Finnish police on the typical number of day units assigned to a given speeding crime.

Because of the income-dependent cutoff, there is a fine discontinuity at 121 km/h, the size of which grows with income.⁶ As shown in Kaila (2024), despite large discontinuities, individuals do not bunch, either in ticket data collected from speeding cameras or in measurements from traffic monitoring stations. Furthermore, while Kaila (2024) shows using a regression-

⁵This floor is inherited from the fixed-fine region of the fine schedule.

⁶In particular, holding income fixed in figure 2.1, the discontinuity is the difference between the red (“16-20”) line and the green (“21-23”) line.

discontinuity-design that individuals who receive larger fines respond by *ex-post* reducing their likelihood of speeding (the extensive margin), there is no evidence that they respond on the *intensive-margin* (i.e. by lowering their speed to avoid the higher fine). We use the latter finding to inform our empirical and model decisions discussed below.

2.2 Relationship Between Income and Speeding

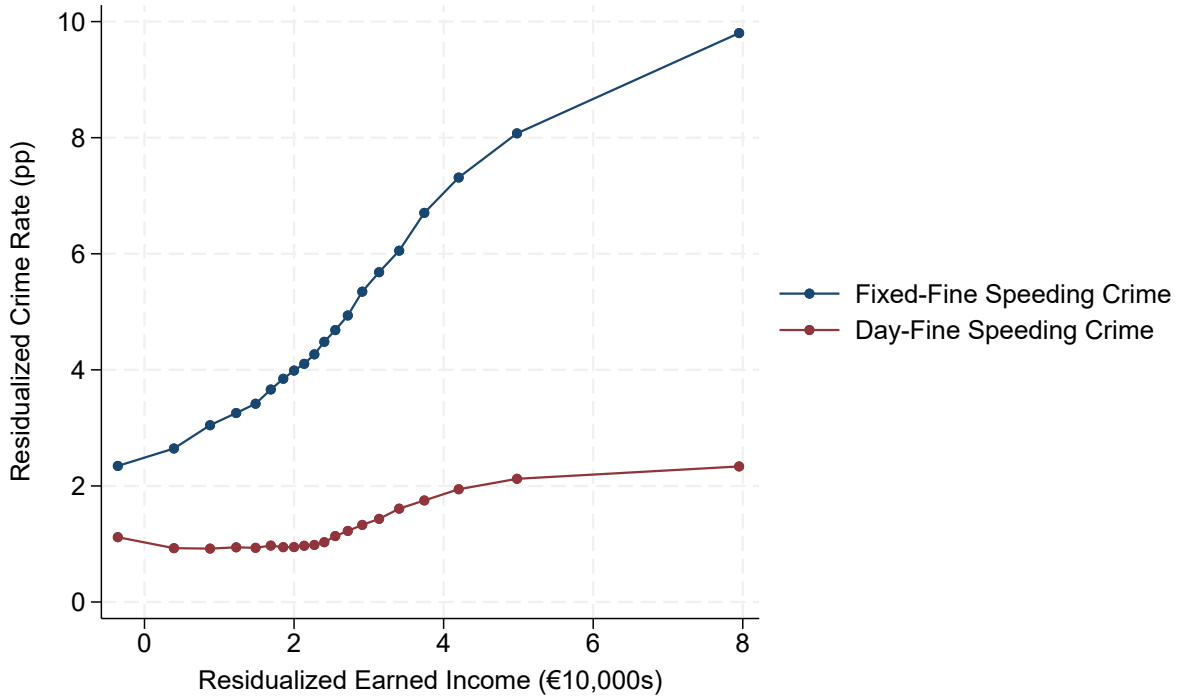
To motivate the potential to use speeding fines as a means for redistribution, figure 2.2 plots the relationship between income and the probability of receiving a speeding ticket in a given year using our crime-income panel described in section 4. We plot the relationship separately for the fixed- and day-fine regions of the fine schedule. We see a strong positive gradient for fixed-fine speeding crime: the fixed-fine speeding rate moves from around 2.5pp to 9pp as we move from the bottom to top earnings decile. While this relationship ostensibly provides a basis to use income-dependent speeding fines as a way to redistribute resources from high- to low-earners, such a possibility needs to be juxtaposed against the option of simply increasing income taxes on high earners, as we discuss in section 3.

We also use figure 2.2 to motivate key features of our survey and elicitation of fairness preferences. In particular, the day-fine region of the speeding-fine schedule is associated with fines that are proportional to net-of-tax income (equation 2.1) and a significantly compressed across-income distribution of behavior. While the latter is not necessarily causal, such an outcome could be an intended “impact” of income-based fines over which society may have fairness preferences. We disentangle the proportionality and equal-compliance channels using our survey (section 7).

2.3 Typical Speeding Crime

In our theoretical and empirical analysis, we use extensive-margin speeding-ticket receipt as our measure of (observed) speeding behavior, collapsing all speeding crime into a single “typical” speeding offense. We justify this simplification in two ways. First, as discussed above, there are no discernible intensive-margin responses to fine size; instead, individuals appear to respond on the extensive-margin (Kaila, 2024). Second, the “typical” speeding crime is similar across incomes. In particular, Appendix figure 9.2 shows, using

Figure 2.2: Binscatter Relationship Between Income and Speeding-Crime Rates



Residualized binscatter relationship between earned income and speeding-crime rates for the population of Finnish adults (*i.e.* individuals who are 18 years or older) from 2006-2019. “Earned income” is defined as taxable earned income, which is the sum of wage/salary income, unemployment insurance payments, and pension income. We say that an individual i committed a “speeding crime” in a given year y if i received a speeding ticket in year y , according to crime-history reports. Fixed-fine speeding crime corresponds to speeding tickets received for driving no more than 20 km/h above the limit, while day-fine speeding crime corresponds to all other speeding crime. We residualize earned income and speeding-crime rates on age-by-year fixed effects.

police speeding-ticket data, that the average speed limit for a given speeding crime is uncorrelated with income (panel 9.2a). While there is a negative correlation between income and excess speed (panel 9.2b), Appendix table 9.1 shows that most of this relationship can be explained by differential police-officer discretion across municipalities (column 3 vs. column 5). We therefore view the typical speeding crime as approximately equivalent across incomes conditional on age and municipality.

2.4 Observed Income-Gradient of Speeding Fines

Given that our goal is to test whether the rationales we consider can justify the speeding-fine schedule in Finland, we need a benchmark fine schedule that accounts for our simplified extensive-margin measure of speeding behavior. To obtain this benchmark, we estimate the observed relationship between speeding fine amounts and income using the speeding offenses in our police ticketing data. In particular, Appendix figure 9.3 shows a binscatter of earnings levels and speeding fine amounts (combining both day- and fixed-fine speeding crimes); while the relationship exhibits a kink at low-incomes,⁷ it is linear for most of the income distribution, with a slope of €12.4 per €10,000.⁸ We take this slope as a benchmark against which to test rationales for Finland’s speeding-fine policy; in particular, for each of the various rationales we consider, we test whether the optimal implied slope of the fine-schedule is positive and large enough to account for the observed fine-schedule gradient.

Appendix figure 9.4 re-estimates the slope between earnings and speeding fine amounts using *only* day-fine speeding crime; we find a gradient of €88 per €10,000 of income. Since this gradient encodes Finland’s income-based speeding-fine policy, we discuss our results in the context of both this slope and the flatter €12.4 slope. However, as we show, even against the straw-man gradient of €12.4, we cannot rationalize the Finnish speeding-fine system.

3 Theoretical Framework

In this section, we build a model of optimal fine-setting in the spirit of an optimal commodity tax framework (Atkinson and Stiglitz, 1976; Saez, 2002a;

⁷The kink arises from the fine floor for income-based fines in figure 2.1.

⁸Under this gradient, the top income decile pays around €100 more on-average than the bottom income decile for a typical speeding crime.

Kaplow, 2006; Allcott et al., 2019), which embeds an optimal commodity tax decision within the redistributive income-tax framework of Mirrlees (1971). Our goal is to clarify the role of each rationale that we consider for income-dependence and set the stage for our empirical approach. In section 3.3, we also make our argument that cited rationales for income-based fines are theoretically ambiguous.

3.1 Setting

Individuals in the economy are indexed by a two-dimensional type $(\theta, \epsilon) \in (\Theta, E)$; θ indexes an individual’s “earnings ability,” *i.e.* their latent wage $w(\theta)$, while ϵ is a shock that governs the benefit of speeding.

There are two periods: in period 0, individuals make a labor supply choice $l(\theta)$ given their type and *ex-ante* uncertainty over ϵ ; this choice generates an earnings level $z(\theta) := w(\theta) * l(\theta)$ for each type. In period 1, ϵ is realized and individuals make an *extensive-margin* speeding decision given their period-0 earnings choice; see section 2.3 for a justification of our decision to focus exclusively on the extensive-margin behavioral margin.

In period 1, an individual with crime decision $s \in \{0, 1\}$, earnings z , earnings ability θ , disposable income c , and shock ϵ receives *ex-post* utility $U(c, s * \epsilon, z; \theta)$. We assume that U is strictly increasing and concave in its first two arguments and strictly decreasing and concave in z . This assumption gives rise to a threshold $\epsilon^*(c, z; \theta)$ such that all type- θ z -earners with $\epsilon \geq \epsilon^*$ will choose to speed. We define $s(z; \theta)$ as the speeding rate for type θ (given earnings z). Given period 1 choices, individuals choose earnings $z(\theta)$ to maximize expected utility.

Taxes and Fines In the spirit of the Mirrlees model, the planner cannot observe individual types θ and therefore must levy taxes on choices z and s , giving rise to distortions.

We ignore any risk associated with speeding ticket receipt and assume that individuals are assessed fines based on their expected speeding rate $s(z; \theta)$. We justify this by noting that, unlike our one-shot static model, individuals in fact make many speeding decisions (e.g. every second they are driving). By central-limit-theorem arguments, as the number of choice occasions grows

large, the variance of fine payments (and therefore utility) will be negligible.

We consider a restricted system of taxes and fines that reflects our empirical setting: an individual earning z with speeding rate $s(z)$ pays income taxes $T(z)$ and speeding fines $\tau(z) * s(z) := (a + b * z) * s(z)$. When $b = 0$, fines are “fixed,” whereas $b > 0$ implies that fines scale with income. We refer to b as the “income-gradient” of the fine schedule.

Externalities Each individual’s speeding behavior imposes a cost on others; this cost can vary with the identity of the offender, e.g. because they interact with certain types of victims. We denote the dis-utility experienced by victims of type θ from others’ speeding as $E(\theta)$ and denote the imposed by offenders of type θ as $\bar{e}(\theta)$.⁹

Standard Planner’s Problem The planner chooses $T(\cdot)$, a , and b to solve:

$$\begin{aligned} \max_{T(\cdot), a, b} \int_{\Theta} \alpha(\theta) G(V(\theta) - E(\theta)) dF_{\theta}(\theta) \\ \text{s.t.} \int_{\Theta} [T(z(\theta)) + (a + b * z(\theta)) * s(z(\theta); \theta)] dF_{\theta}(\theta) \geq R \end{aligned} \tag{3.1}$$

where $G(\cdot)$ is a concave social welfare function, $\alpha(\cdot)$ is a Pareto-weighting function, and R is an exogenous revenue requirement. We make the following assumption.

3.1. *At the optimum: (i) the nonlinear income tax $T(\cdot)$ is smooth; (ii) $z(\theta)$ is smooth and strictly increasing, and $s(z; \theta)$ is a smooth function of z and θ ; and (iii) individuals’ optima are unique and their first- and second-order conditions strictly hold.*

Parts (i)-(iii) of assumption 3.1 allow us to use perturbation-based techniques to derive the optimal tax and fine schedule. Furthermore, this assumption allows us to express all heterogeneity with respect to z , rather than θ , at the optimum; in other words, the tax-and-fine system perfectly separates types. Therefore, in what follows, we suppress dependence on type.

⁹We use overline notation to denote that the imposed cost is aggregated over victims.

3.2 Optimal Fines

Before discussing optimal fine schedules, we define several terms that appear in our formulas. First, the *social marginal welfare weight* for a z -earner $g(z)$ captures the social value of a marginal transfer to a z -earner at the optimum (relative to the value of public funds); these weights embed the planner’s preferences for redistribution.¹⁰ We also define the following behavioral responses:

- Fine (semi-)elasticities: $s'_{\tau_s}(z) = \frac{\partial s(z)}{\partial \tau}$
- Causal earnings effects on speeding: $s'_{inc}(z) := \left. \frac{ds(z;\theta)}{dz} \right|_{\theta=\theta^{-1}(z)}$

Causal earnings effects measure the change in speeding caused by a change in earnings *holding type fixed*; this is distinct from the *cross-sectional* slope $s'(z) := \frac{ds(z)}{dz}$, which measures the difference in observed speeding across individuals with different incomes. These earnings effects are crucial for recovering the earnings ability signal embedded in speeding behavior. The intuition is given in figure 3.1, which shows two adjacent earnings levels, z_0 and z_1 , and associated hypothetical speeding rates, $s(z_0)$ and $s(z_1)$. Under assumption 3.1, those earning z_1 have a higher type (earnings ability) than those earning z_0 ; we index their respective types by θ_1 and θ_0 . The key experiment we use to determine whether speeding behavior is (locally) informative about earnings ability is by inducing those earning z_0 to increase their labor supply and earn z_1 . Despite having the same earnings z_1 , the two types speed at different rates, reflecting differences in underlying earnings ability and/or preferences; the planner can exploit this information for redistributive purposes by targeting fines based on earnings ability. We refer to the earnings ability tag from figure 3.1 as $s'_{het}(z) = s'(z) - s'_{inc}(z)$, following the notation in Ferey et al. (2024).

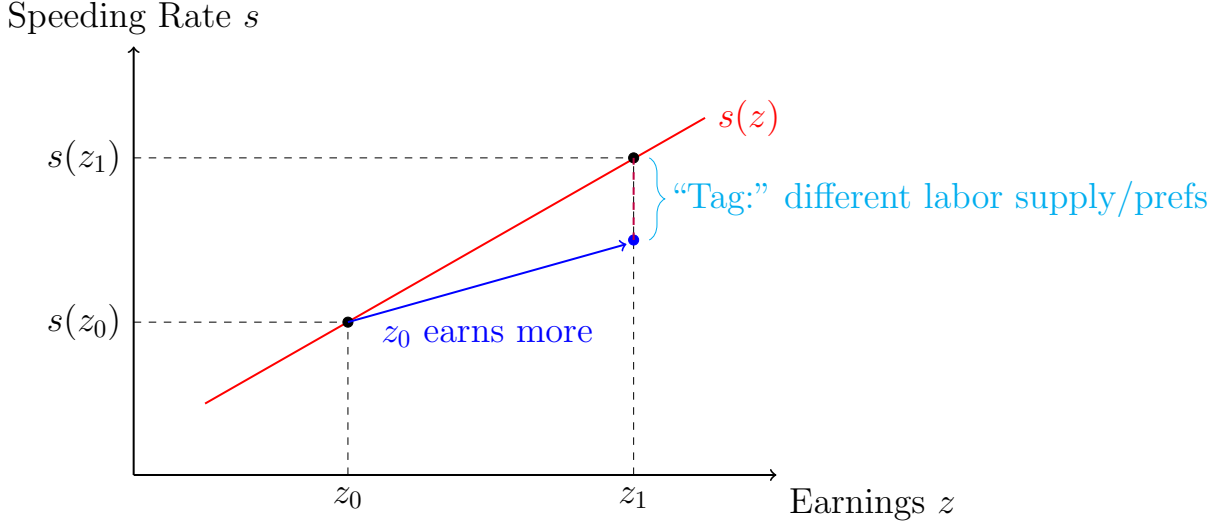
With this intuition in-hand, we now present the first result regarding the size of the fine gradient b , which is the key focus of our paper. Expressions for the optimal fixed component a and marginal income-tax rates $T'(\cdot)$ are shown in Appendix section 9.1.

¹⁰Formally:

$$g(z) := \frac{\alpha(z)G'(V(z) - E(z))V'_c(z)}{\lambda}$$

where λ is the multiplier on the government budget constraint.

Figure 3.1: Latent Earnings Ability Tag



Theorem 1. *Suppose assumption 3.1 holds, the income tax schedule $T(\cdot)$ is optimal, the fixed component $a^*(b)$ is optimal, and $s'_{\tau_s}(z) \perp z$. Then, at the optimum, the income-gradient of the fine schedule, b , satisfies the following:*

$$b^* = \frac{\text{Cov}[\bar{e}'(z)s'_{\tau_s}(z), z] + \text{Cov}\left[\hat{g}(z), \int_0^z (y - \bar{z})s'_{het}(y)dy\right]}{\bar{s}'_{\tau_s} \mathbb{V}[z]}$$

where $\hat{g}(z)$ are augmented social welfare weights.¹¹

Under conventional welfare weights that are monotonically declining in z , the optimal income gradient of the fine schedule depends essentially on how deterrence effects $\bar{e}(z)*s'_{\tau_s}(z)$ and earnings ability tags $s'_{het}(z) := s'(z) - s'_{inc}(z)$ co-vary with income. With respect to deterrence, we consider the special case where the externality $\bar{e}(z) = \alpha_e + \beta_e z$ is affine in z . This implies:

$$b^* = \beta_e + \frac{\text{Cov}\left[\hat{g}(z), \int_0^z (y - \bar{z})s'_{het}(y)dy\right]}{\bar{s}'_{\tau_s} \mathbb{V}[z]}$$

In words, an optimally corrective fine gradient (ignoring redistribution) would simply set the fine gradient equal to the slope of marginal speeding costs,

¹¹The augmented weight $\hat{g}(z)$ captures the full social value (including fiscal externalities) of a marginal transfer to an individual with a given earnings level z .

$b^* = \beta_e$.¹² In section 5, we estimate the slope of marginal speeding costs β_e in order to test the externality-driven rationale for income-based speeding fines.

With regards to the redistributive term $\frac{\text{Cov}\left[\hat{g}(z), \int_0^z (y-\bar{z})s'_{het}(y)dy\right]}{\bar{s}'_{\tau_s}\mathbb{V}[z]}$, we refer to the numerator as the “redistributive value:” it captures the social value of using the ability tags $s'_{het}(z)$ to move resources between individuals with different welfare weights $\hat{g}(z)$ through changes in the fine slope b . However, since speeding behavior is a *mutable* tag, the redistributive value is dissipated by a fiscal externality term (*i.e.* a consumption distortion) that is proportional to the average fine semi-elasticity \bar{s}'_{τ_s} .

It can be shown that, when $s'_{het}(z) = \alpha_{het} + \beta_{het} * z$ is affine with $\beta_{het} > 0$, the redistributive term will be positive. In words, if the earnings ability tag $s'_{het}(z)$ is *more* informative at higher income levels, then fines that scale with income ($b^* > 0$) will have an instrumental redistributive value. The relationship between earnings z and the tag $s'_{het}(z)$ is therefore the key metric that informs our test of the redistributive rationale; in section 6, we estimate the earnings effects $s'_{inc}(z)$ that are used to recover the tag $s'_{het}(z)$. To quantify the consumption distortion, we use estimates of extensive-margin fine elasticities from Kaila (2024).

3.3 Incorporating Fairness

We now formalize the fairness rationales we consider and show how they enter our expression for the optimal fine gradient. As discussed in section 2, official argumentation for income-based fines in Finland cites a desire to equalize the “impact” of fines across incomes (Lahti, 2021). One interpretation of this rationale, consistent with recent legal theory (Kantorowicz-Reznichenko, 2013), is that income-based fines are intended to equalize the (marginal) utility cost of crime across incomes. In particular, with log-utility over consumption, fines that are proportional to net-of-tax income (such as day-fines in Finland) precisely generate equal utility costs of crime.

To see this, let $y = z - T(z)$ be net income and $u(c) = \ln(y - \tau(y) * s)$ be the sub-utility of income; suppose $\tau(y) = b * y$, *i.e.* fines are proportional to

¹²The intercept term would be captured by the fixed component of the fine schedule: $a^* = \alpha_e$.

net income. Then, the marginal utility cost of speeding is given by:

$$u'_s(y - \tau(y) * s) = \frac{-b}{1 - b * s}$$

In words, when $\tau(y)$ is proportional to net-income, the marginal utility cost of speeding is independent of income y at all speeding levels s . Thus, under log utility, Finland’s income-proportional fine policy equalizes the marginal utility cost of speeding across incomes; if the latter is the instrumental objective of income-based fines, then preferences over different fine policies will depend on the shape of the fine schedule *per se*.

However, with log-utility of consumption, proportional fines *also* generate across-income equality in speeding rates under any monotonic and increasing sub-utility of speeding $v(s)$. As we showed in figure 2.2, the income-based speeding fine system is associated with rough equality of speeding rates across incomes, in contrast to the inequality within the fixed-fine region of the schedule. Thus, income-based fines may instead be an instrument for achieving a particular behavioral outcome (equal speeding rates across incomes), rather than marginal deterrence. Given this ambiguity, and motivated by theories of fairness that emphasize equal allocations/behavior,¹³ we consider two separate fairness-driven effects of a given fine policy: “behavioral fairness,” which depends on the distribution of behavior induced by a policy, and “fine fairness,” which depends only on the fine schedule itself.

To formalize these preferences within our optimal tax framework, we model each rationale in a reduced-form (sufficient-statistic) manner. In particular, suppose there is a prevailing fine schedule $\tau(z) = a^*(b) + b * z$, where $a^*(b)$ is optimal given a slope parameter b . Given this schedule, we define the fine fairness term $\bar{e}_{\tau_s}(z)$ as the social value of a marginal speeding fine *increase* for z -earners; similarly, we define the behavioral fairness term $\bar{e}_s(z)$ as the social value of a marginal *decrease* in the speeding rate among z -earners. The term $\bar{e}_s(z)$ captures local preferences over induced behavior, while $\bar{e}_{\tau_s}(z)$ captures local preferences over the shape of the fine schedule.

Because the behavioral fairness term $\bar{e}_s(z)$ is proportional to behavioral re-

¹³For example, specific egalitarianism emphasizes equality in the allocation of certain goods (such as health insurance); it may be natural to extend this to recent empirical evidence (Caspi et al., 2024) provides support for this theory in certain domains.

sponses $s'_{\tau_s}(z)$, it enters the expression for the optimal fine gradient in precisely the same manner as a standard externality. By contrast, $\bar{e}_{\tau_s}(z)$ produces effects proportional to the size of the fine change; as a result, it enters the optimal fine gradient in a manner similar to the redistributive value. To see this most clearly, suppose that both $\bar{e}_s(z) = \alpha_s + \beta_s * z$ and $\bar{e}_\tau(z) = \alpha_\tau + \beta_\tau * z$ are affine. Then, if $s'_{\tau_s}(z) \perp z$, our expression for the optimal fine gradient becomes:

$$b^* = \beta_e + \beta_s + \frac{-\beta_\tau \mathbb{V}[z] + \text{Cov}\left[\hat{g}(z), \int_0^z (y - \bar{z}) s'_{het}(y) dy\right]}{\bar{s}'_{\tau_s} \mathbb{V}[z]} \quad (3.2)$$

Considering the fairness mechanisms in isolation, if society has a stronger desire to decrease speeding rates among the rich ($\beta_s > 0$), the optimal gradient b^* will be positive. Furthermore, if society has a stronger desire to increase fines among the rich ($\beta_\tau > 0$), then the optimal gradient will be positive.¹⁴ In our survey, we test whether $\beta_s > 0$ by eliciting subjects' value for policies that lead to more equality in across-income speeding rates (relative to a distribution where high-income individuals speed more); similarly, we test whether $\beta_\tau > 0$ by varying the “steepness” of the fine schedule (*i.e.* how much larger the fine is at high incomes relative to low incomes).

Appendix equation 9.3 presents a generalized version of equation 3.2 when, rather than assuming $s'_{\tau_s}(z) \perp z$, we assume a constant fine elasticity ζ_τ ; this is the equation we use to derive the optimal redistributive fine slope in section 6. There are two differences relative to equation 3.2. First, \bar{z} is replaced by a consumption-weighted average earnings level \bar{z}^{rw} ; second, the fine-fairness intercept α_τ appears in the optimal gradient. While our survey design described in section 7 only allows us to estimate the slope term β_τ , this will be sufficient to determine the sign that fine-fairness contributes to the optimal gradient b^* if α_τ is not *too* large (see assumption 9.1 in the Appendix for details).¹⁵ Since our survey respondents are relatively unresponsive to randomized fine-schedule steepness (and, if anything, slightly dislike steeper schedules), we focus our discussion on the slope term β_τ .

¹⁴This follows from the fact that $\bar{e}_\tau(z) = \alpha_\tau + \beta_\tau * z$ and fine-fairness enters the optimal gradient as $\frac{-\beta_\tau}{\bar{s}'_{\tau_s}}$. Because \bar{s}'_{τ_s} is negative, $\beta_\tau > 0$ implies the result.

¹⁵The claim that β_τ is determinative under assumption 9.1 is dependent on the joint distribution of fines, speeding rates, and earnings. Essentially, if $s(z)/\tau(z)$ is weakly increasing in z (as is the case in Finland when we consider overall speeding behavior and its associated fines), then this claim will hold under any constant fine elasticity.

We now discuss the administrative data sources that we use to estimate the marginal speeding cost gradient β_e and causal earnings effects on speeding $s'_{inc}(z)$.

4 Administrative Data

We now briefly describe the administrative data we use to assess the Beckerian and redistributive rationales for income-dependent speeding fines.

Our primary dataset is an annual panel of earnings, speeding-ticket receipt, and accident costs from 2006-2019. We construct this panel using a population register, income-tax register, crime reports, and accident reports. We use taxable earned income as our earnings measure, though we also make use of information on transfers received and taxes paid from the tax returns. Using the crime reports, we construct a 0-1 indicator for speeding-ticket receipt in a given year as our measure of speeding behavior; from this data, we also construct an indicator for receipt of a non-speeding traffic ticket. We restrict this panel to individuals who are 18 years or older in a given year.

The accident report data is crucial for testing whether the marginal cost of speeding varies with income. To measure accident costs for a given “offender” i in year t , we count the number of *injuries* (both fatal and non-fatal) incurred by “externality-relevant” participants in accidents involving i in year t . In our baseline results, we assign all participants *outside* of i ’s vehicle for a given accident to the externality-relevant group; implicitly, we assume that the driver i internalizes the cost to everyone in their own vehicle. We also consider an alternative definition where all other participants (except the driver i) constitute the externality-relevant group; qualitatively, this does not change our results.

We supplement this primary panel dataset using several additional registers. First, we make use of the vehicle register (2013-2023), which we use to measure each individual’s annual driving distance from vehicle-inspection odometer information (see Appendix section 9.2.1 for details); this measure serves as an important control in our estimate of the marginal speeding cost gradient. We also draw on the inheritance tax register (2013-2022), the uni-

verse of parent-child links, and the cause-of-death register (2013-2022) to construct inheritance-shock events around first parental deaths, which we use to measure income effects on speeding.

5 Externalities

We now discuss our approach for estimating the income-gradient of marginal speeding externalities, β_e , in Finland; in the absence of other concerns, an estimate of β_e allows us to directly evaluate the externality-based rationale via equation 3.2. Our approach to measuring β_e has two key features. First, we exploit cross-sectional variation in speeding ticket receipt-rates to measure the effect of speeding on accident-related injuries. Second, we leverage a “job-loss” design to calculate the monetary cost of injuries, which we use in tandem with our cross-sectional estimates to measure money-metric marginal speeding costs. We begin with the cross-sectional approach.

5.1 Cross-Sectional Approach

As discussed in our theoretical framework (section 3), *observed* speeding behavior (*i.e.*, speeding ticket receipt) is the correct behavioral margin for determining optimal speeding fines when speeding enforcement is fixed.¹⁶ Since accidents are rare, we elect to exploit *cross-sectional* variation in speeding ticket receipt (conditional on observables); this yields more powerful tests than behavioral variation induced by, say, price instruments.

In particular, we exploit cross-sectional variation in speeding rates \bar{s}_i , defined as the number of years in which i receives a ticket divided by the number of years i appears in our panel; exploiting variation in speeding *rates* (as opposed to ticket receipt in a given year $s_{i,t}$) mitigates attenuation due to the stochastic nature of ticket receipt. We collapse our panel of earnings, speeding-ticket receipt, and accident-related injuries into an individual-level dataset with the following key variables:

- average taxable earned income, \bar{z}_i
- speeding rate, \bar{s}_i

¹⁶Since we do not observe enforcement probabilities $p(z)$, it is difficult to make use of outside estimates for speeding externalities, as in van Benthem (2015).

- average number of injuries incurred by externality-relevant parties in accidents involving driver i , \bar{I}_i^{ext}
- average money-metric social cost of injuries from accidents involving driver i , \bar{M}_i^{ext} (discussed in section 5.3)

For individuals who are never in an accident that causes an externality-relevant injury, \bar{I}_i^{ext} and \bar{M}_i^{ext} will be equal to 0. As discussed in section 4, we take individuals who are outside the focal driver’s vehicle (hereafter, “other-party” victims) as our externality-relevant group when calculating \bar{I}_i^{ext} and \bar{M}_i^{ext} ; for robustness, we also consider all other accident participants besides the driver (hereafter, “other-person” victims) as the externality-relevant group. Summary statistics for this cross-sectional dataset are available in Appendix table 9.2.

In the results below, we estimate the gradient of marginal speeding costs, measured in terms of both injuries and money; we therefore distinguish between β_e^I —the gradient in terms of *injuries*—and $\beta_e^M := \beta_e$ —the *money-metric* gradient that we eventually use to test the externality-based rationale. For a given outcome Y , we estimate the following regression:

$$\bar{Y}_i^{ext} = \rho_e^Y * \bar{z}_i + \alpha_e^Y * \bar{s}_i + \beta_e^Y * \bar{z}_i * \bar{s}_i + \Gamma * (\bar{X}_i + \bar{X}_i * \bar{z}_i) + \gamma_{a(i)} + \epsilon_i \quad (5.1)$$

where $\gamma_{a(i)}$ is a set of birth-cohort fixed-effects and \bar{X}_i is a set of covariates discussed below. We weight each individual by the number of years they appear in the population register from 2006-2018.

The coefficient ρ_e^Y measures the cost-gradient for non-speeders, while α_e^Y measures the intercept of marginal social costs (corresponding to the constant in the optimal fine slope); our primary object of interest is β_e^Y , which measures how the marginal effect of speeding varies with income.

Our cross-sectional approach relies on two assumptions: conditional on income \bar{z}_i and covariates \bar{X}_i

5.1. *Speeding-rate variation is driven by behavior (rather than enforcement)*

5.2. *Speeding rates \bar{s}_i are uncorrelated with other determinants of accident costs*

To address 5.1, we assess robustness to including fixed-effects for municipality of residence, under the presumption that, conditional on income, enforcement is unlikely to vary within-municipality. To address 5.2, we include the following variables in \overline{X}_i :

- Average car ownership rates (for both an individual and their spouse), measured using the population register from 2006-2018
- Average annual driving distance, measured using successive odometer readings from vehicle inspections during 2013-2018
- Average non-speeding traffic-crime rates, constructed using crime reports from 2006-2018
- Individual i 's number of accident-related injuries and non-injurious accidents from 2006-2018

Car ownership serves as a baseline proxy of driving distance (and therefore accident exposure); we supplement this variable with a measure of annual driving distance constructed using odometer readings. We control for non-speeding traffic-crime rates to absorb a driver's "general riskiness," which may be correlated with their speeding behavior. Given the cross-sectional nature of our main dataset, we also control for each individual i 's *own* accident-related injuries and their involvement in non-injurious accidents, both of which serve to address reverse causality concerns.¹⁷ As an additional safeguard against reverse causality concerns, we drop individuals involved in an accident from 2003-2005, prior to the 2006-2018 period we use to create our cross-sectional dataset; we discuss reverse-causality concerns further in section 5.5.

Note that our identification assumption is not confounded by *across*-income differences that may affect marginal speeding costs. For example, if income is positively correlated with vehicle mass, the marginal social cost of speeding may be positive correlated with income since speeding with a heavier car leads to more severe accidents; more generally, unpriced characteristics or behaviors that are correlated with income may generate "second-best" reasons for income-based speeding fines. Alternatively, even if the marginal

¹⁷As we show with our job-loss design, accident-related injuries lead to earnings losses (and may have an impact on how likely someone is to subsequently speed). This can confound the relationship between income and marginal speeding costs.

social cost of *latent* speeding behavior is constant across incomes, lower enforcement rates for rich drivers would imply that the marginal social cost of *observed* speeding is *higher* for rich drivers.¹⁸ While we cannot entirely disentangle between “second-best” and enforcement-based drivers with our data, we provide suggestive evidence below that correlations between income and traffic-density can explain our qualitative findings.

5.2 Marginal Speeding Gradient (Injuries)

To motivate our main results, we start by estimating equation 5.1 using injuries \bar{I}_i^{ext} as the outcome; our estimates are displayed in table 5.1. In column 1, we include only reverse-causality controls and birth-cohort fixed effects. Reassuringly, the baseline speeding effect on injuries (row 1) is positive; however, the gradient (row 2) is negative, implying that the marginal effect of speeding on the likelihood of an external injury decreases by 0.07 percentage points for every €10,000 increase in driver earnings. Motivated by concerns that conditional variation in speeding rates may be driven by enforcement rather than behavior, column 2 includes fixed effects for a driver’s municipality of residence; compared to column 1, our results are nearly unchanged, mitigating concerns about assumption 5.1. Column 3 restricts to individuals who have a non-missing driving-distance variable,¹⁹ and column 4 adds the remaining covariates in \bar{X}_i described above. The combination of sample selection and covariate-inclusion causes the marginal cost intercept and gradient to attenuate substantially, though the signs are unchanged. Column 4 is our preferred specification, implying that the marginal effect of speeding on the likelihood of an external injury decreases by 0.02 percentage points for every €10,000 increase in driver earnings. In Appendix table 9.3, we replicate table 5.1 using “other-person victims” (*i.e.* including individuals in the driver’s vehicle) as the externality-relevant group; we find somewhat more negative marginal speeding gradients, consistent with a correlation between income and car safety.

Appendix table 9.4 uses information from the accident reports to explore

¹⁸In particular, under a lower enforcement rate, each speeding ticket implies more underlying speeding behavior.

¹⁹This variable is frequently missing since its construction requires an individual to be the primary user on a vehicle that undergoes multiple inspections between 2013 and 2018; see Appendix section 9.2.1 for details. In our regressions, we weight each individual by the number of years for which annual driving distance is non-missing.

Table 5.1: Income Gradient of Marginal Social Costs (Injuries)

	(1)	(2)	(3)	(4)
	Other-Party Injuries (per 1,000 drivers)			
Speeding Rate (MSC Intercept)	8.484*** (0.163)	8.374*** (0.164)	6.249*** (0.215)	1.370*** (0.217)
Speeding X Earnings (MSC Gradient)	-0.741*** (0.0380)	-0.742*** (0.0380)	-0.435*** (0.0504)	-0.219*** (0.0513)
Earned Income (10,000s €)	0.122*** (0.00411)	0.131*** (0.00422)	0.0664*** (0.00678)	0.138*** (0.0242)
Constant	0.304*** (0.0104)	0.289*** (0.0105)	0.735*** (0.0222)	-0.0373 (0.0783)
<i>N</i>	5033597	5033597	2223285	2223285
Cohort FE	X	X	X	X
Reverse-Causality Controls	X	X	X	X
Municipality FE		X		
Non-Missing Distance			X	X
Car Ownership & Other Traffic Crime Controls				X
Driving Distance Control				X

Estimates of equation 5.1 using the cross-sectional dataset described in section 5.1. Other-party injuries are injuries incurred by accident participants outside of the focal individual’s vehicle; we multiply other-party injuries by 1,000 for interpretability. An individual i ’s “speeding rate” is defined as the number of years from 2006-2018 in which i receives a speeding ticket, divided by 13; earned income refers to an individual’s average taxable earned income from 2006-2018. Column 2 restricts to individuals who receive at least one speeding ticket from 2006-2018; column 3 includes fixed-effects for municipalities of residence in 2006 and 2018. For details on the construction of control variables, see section 5.1. The car-ownership rate, non-speeding traffic-crime rate, and driving-distance controls are also interacted with earnings, when included. In columns 1-4, we weight each individual by the number of years in which they appear in the population register from 2006-2018; in column 5, we use a compound weight which is the product of our baseline weight and the number of years for which driving distance information is available from 2013-2018. Heteroskedasticity-robust standard errors are in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

the mechanisms underlying the findings in table 5.1. In particular, we show that, *conditional on an accident*, the negative marginal speeding cost gradient dramatically attenuates and becomes insignificant when we control for the number of participants involved in an accident.²⁰ While we caution that this conditional analysis cannot be used to explain the full unconditional cost gradient in table 5.1, our results suggest that high-income drivers tend to drive on busier roads (e.g. during rush hour), where *other* drivers’ behavior plays a larger relative role in the severity of an accident.²¹

Taking stock, the results in table 5.1 suggest that optimal externality-motivated fines may actually be *decreasing* in income, under the presumption that the cost of an external injury is unrelated to the driver’s income. However, to motivate why such costs may not be constant, Appendix figure 9.5 uses accident-level data from the accident reports to plot the relationship between driver income and victim income, both for “other-party” victims (panel 9.5a) and “other-person” victims (panel 9.5b). We see modest positive relationships in both figures, implying that higher-income drivers are involved in accidents with higher-income victims; to the extent that injury costs are correlated with income (e.g. due to lost labor time), this could weaken or even flip the negative marginal injury cost gradient. More generally, victims’ injury costs could be correlated with driver income for a number of reasons, e.g. if the injuries resulting from accidents with high-income drivers are more severe. We therefore turn to a “job-loss”-style design (von Wachter and Bender, 2006) that enables us to measure earnings losses associated with individual injuries.

5.3 Money-Metric Costs of Injuries

Our approach to measuring the monetary costs of injuries relies on comparisons between individuals who are injured or killed in an accident and matched individuals from the general population. In particular, for each individual in-

²⁰Compare column 1 to column 2 of table 9.4. In unreported results, we examine a number of alternative controls in this conditional sample, including municipality of the accident, prevailing speed limit, collision type (E.g. head-to-head collision, side-to-side collision, etc.), and vehicle type. We find a persistently negative and significant gradient across these controls; among these, only collision-type meaningfully attenuates the baseline conditional gradient from column 1 of table 9.4 (by around 42%).

²¹In Appendix table 9.4, we also see that the positive relationship between earnings and external injuries among non-speeders (row 3) effectively disappears when we control for the number of accident participants. This substantiates our claim that traffic density explains our results: high income drivers *in general* (regardless of their speeding behavior) seem to drive on busier roads, leading to accidents that involve more participants.

jured or killed in an accident involving a motor vehicle (the treatment group), we assign two random control individuals from the population register with the same birth year, sex-at-birth, and education level in the year prior to the accident.²² We then construct an event-time panel for each match-group around the accident event year.²³

To illustrate this approach, we begin by estimating parsimonious event-study models of the form:

$$Y_{i,t,k} = \beta_0 * \text{Treat}_{i,t} + \gamma_k + \sum_{k=-4, k \neq -1}^{2023-t} \beta_k * \text{Treat}_i * I(t+k \geq t) + \epsilon_{i,t,k} \quad (5.2)$$

where $Y_{i,t,k}$ is an outcome for individual i in year $t+k$, $\text{Treat}_{i,t}$ is indicator for whether or not individual i is treated in year t , $I(t+k \geq t)$ is a post-period indicator, and γ_k is a set of event-time dummies.²⁴

Figure 5.1 shows event-study coefficients from equation 5.2, separately for pre- and post-tax income. For both series, we see no discernible pre-trends and immediate income losses in the year of the accident, followed by a gradual divergence between the treatment and control groups. Appendix figure 9.9 displays event-study estimates using only non-fatal injuries; we see little-to-no decline in post-tax income for several years following the accident despite declines in pre-tax income, implying that the tax-and-transfer system is able to insure against initial income losses. Nonetheless, after several years, we see a divergence pattern similar to that in figure 5.1 emerge.

To account for the gradual income divergence between treatment and control groups, we show in Appendix figure 9.7 that around 4% of individuals who are non-fatally injured drop out of the labor force within a few years of the accident and never return. Since the average injured individual is in their prime earnings-growth years (≈ 45 years old), it is unsurprising that income gradually diverges between the treatment and control group.

²²We use four education categories (based on highest completed qualification): less than upper-secondary, upper secondary or post-secondary non-tertiary, short-cycle tertiary, and bachelor's or above.

²³We have reliable accident report data going back to 2003 and are able to follow individuals until 2023.

²⁴Due to the nature of our data, in which we lose one accident-year cohort in each successive post-period, the event-time dummies are equivalent to including accident-year fixed-effects for the post-period.

Figure 5.1: Event Study Effects of Accident-Related Injury/Death on Income



Estimates of β_k from equation 5.2, using disposable income and pre-tax income as outcomes. Disposable income is the sum of earned income, capital income, and transfers received, net of taxes paid; pre-tax income is the sum of capital and earned income. For details on sample construction, see section 5.3. We restrict to accident events that involve a motor vehicle (cars, trucks, buses, vans, mopeds, motor bikes, and motorcycles); injuries from accidents involving a snow mobile, trailer, tram, train, tractor, or other unidentified motor vehicles are excluded. We also restrict to individuals who are at least 18 years old in the year prior to the accident.

To motivate our approach for computing money-metric injury costs, note that post-tax income changes capture the consumption loss that victims experience (corresponding to the red-shaded region in figure 5.1). However, the government (through increased transfer payments and reduced tax receipts) also bears a fiscal externality from accident-related injuries, as shown by the purple-shaded region in figure 5.1.²⁵ While fiscal costs are valued dollar-for-dollar by the planner, income losses to victims may have different social costs depending on the income of the victim. For example, a €10,000 income loss for someone earning €40,000 may be weighed very differently than an equal-sized income loss for someone earning €100,000, e.g. because of diminishing marginal utility. Our earnings-loss approach allows us to separate these two components of injury costs and apply income-dependent weights to victims' net-income losses; this decomposition may prove important in light of the correlations we find between driver- and victim-income (Appendix figure 9.5).²⁶

Concretely, to calculate the cost of each accident-related injury in our data, we start by calculating net-present-value (NPV) fiscal externalities and net-income losses by aggregating period-specific effects within each match-group (see Appendix section 9.2.2 for details). The results are shown in Appendix table 9.5, separately for injuries and deaths. Under our baseline discount rate of 3%, non-fatal injuries generate around €10,600 in NPV costs to the government and €6,500 in NPV net-income losses to individuals, based on a 13-year treatment horizon. For the small number of fatal injuries, we find cost *savings* for the government, though these are swamped in magnitude by nearly €200,000 of NPV net-income losses through 13 years. Unsurprisingly, given that injuries cause permanent declines in labor supply (Appendix figure 9.7), we find in Appendix figure 9.10 that fiscal externalities and net-income losses are positively correlated with victims' pre-period income.

To aggregate injury-cost components, we apply a standard set of welfare weights g_i (Saez, 2002b) to NPV net-income losses:

$$g_i \propto y_i^{-\nu}$$

where y_i is i 's disposable income in the year prior to the accident and ν is a

²⁵For a decomposition of this fiscal externality into transfer payments and tax receipts, see Appendix figure 9.8.

²⁶Our earnings-loss approach also implicitly captures the severity of victims' injuries, which may vary with income.

CRRA parameter that encodes the planner’s distaste for inequality. Under this set of weights, the planner weighs money-metric losses to each individual in proportion to the marginal utility from a CRRA utility function; the larger is ν , the larger is the weight placed on a given monetary loss to low-income individuals relative to high-income individuals.²⁷ In our baseline results, we set $\nu = 1$, though we assess robustness to higher and lower degrees of inequality aversion.

The total cost of a given injury is the sum of NPV fiscal externalities and welfare-weighted NPV net-income losses. We use these injury-specific costs to calculate \overline{M}_i^{ext} , the average money-metric social cost of accidents involving driver i ; we use this outcome to estimate the marginal speeding cost gradient in section 5.4 below. For this context, we note two important features of our cost estimates. First, we do not project injury costs forward. Within our cross-sectional design, this choice is unlikely to bias our estimate of the money-metric *gradient* β_e^M as long as accident year (which determines the treatment-horizon in our earnings-loss design) is not correlated with offender income;²⁸ indeed, we find that this correlation is quantitatively negligible. Second, we elect to use our earnings-loss estimates to impute the cost of both non-fatal *and* fatal injuries, rather than relying on outside value-of-a-statistical-life (VSL) estimates for the cost of fatal injuries. We take this approach so that the costs of both injury types are determined on a similar basis: as long as our approach appropriately captures the *relative* severity of fatal and non-fatal injuries, our results are unlikely to be biased.

5.4 Marginal Speeding Gradient (Money-Metric)

To estimate the money-metric gradient of marginal speeding costs, β_e^M , we implement equation 5.1 using the cost estimates \overline{M}_i^{ext} from the previous section as our outcome.²⁹ Table displays the results from using a 3% discount

²⁷We set the proportionality constant such that $\bar{g} = 1$, meaning the planner is indifferent between a €1 increase in revenue and distributing €1 equally among the population.

²⁸In particular, while we may understate the marginal social cost of speeding by failing to project the cost of injuries, our estimate of the gradient will be unaffected if the level of understatement is uncorrelated with offender income.

²⁹While the individual-specific \overline{M}_i^{ext} are estimated, we do not currently account for estimation uncertainty when implementing equation 5.1; this introduces additional noise and inflates our standard errors. However, as we will see, our standard errors are precise enough to rule out meaningfully positive estimates for β_e^M .

rate and no welfare-weighting (*i.e.*, $\nu = 0$) to estimate NPV injury-costs; columns 1-3 use other-person victims as the externality-relevant group, while columns 4-6 use other-party victims. Columns 1 and 4 present estimates with only our reverse-causality and birth-cohort controls. We note that the baseline marginal speeding cost (MSC intercept α_e^M) is €169 for other-person victims in column 1; this cost is nearly identical to the average fixed speeding fine in the police ticketing data (approximately €167), suggesting that our empirical approach does a reasonable job of capturing the marginal cost of speeding. Columns 2 and 5 condition on individuals with non-missing driving distance, while columns 3 and 6 introduce the rest of our preferred covariates. These additional controls attenuate the MSC intercept term (row 1) but do little to change the marginal speeding cost gradient (row 2). For other-party victims (our preferred externality-relevant group), our estimates in column 6 indicate that fines should *decrease* by almost €19 for every €10,000 of income, well below the estimated fine-schedule gradients of +€12.4 (all speeding crime combined) and +€88 (day-fine speeding crime) from section 2.

To see how discount rates and welfare weights affect this baseline estimate, we re-estimate the marginal social cost gradient under varying discount rates $\delta \in \{0.03, 0.05, 0.07\}$ and inequality-aversion parameters $\nu \in \{0, 0.25, 1, 4\}$. Table 5.3 displays the results using other-party victims. We first note that, across all parameter values, we find consistently negative estimates and, with 95% confidence, can reject marginal social cost gradients larger than +1 € per €10,000 of earnings. Appendix Table 9.7 shows nearly identical quantitative results when we use other-person victims. Second, we see that inequality aversion attenuation of the gradient, primarily when moving from $\nu = 0.25$ to $\nu = 1$. We do not consider inequality-aversion parameters above $\nu = 4$ because $1 < \nu < 4$ implies a roughly flat relationship between victim income and injury costs,³⁰ in contrast to the positive relationship for unweighted costs. We view constant across-income costs as a normatively appealing stopping-point.

To summarize, regardless of how we specify the externality-relevant group (other-party or other-person), we fail to find a positive marginal speeding cost gradient across a range of discount rates and levels of inequality-aversion; furthermore, our estimates are precise enough to rule out meaningfully positive gradients. We therefore reject externality-based motives as a rationale

³⁰See Appendix table 9.6.

Table 5.2: Income Gradient of Money-Metric Marginal Social Costs: 3% Discount Rate

	Other-Person Costs			Other-Party Costs		
	(1)	(2)	(3)	(4)	(5)	(6)
Speeding Rate (MSC Intercept)	168.8*** (25.49)	146.0*** (33.80)	84.39* (34.35)	118.1*** (22.54)	116.3*** (30.80)	75.43* (31.15)
Speeding X Earnings (MSC Gradient)	-27.19*** (5.978)	-23.16** (8.108)	-21.45* (8.425)	-17.54** (5.372)	-17.31* (7.429)	-18.60* (7.712)
Earned Income (10,000s €)	0.925 (0.681)	1.573 (1.115)	0.160 (4.222)	2.100*** (0.617)	1.952 (1.017)	2.929 (3.743)
Constant	6.338*** (1.698)	7.943* (3.555)	-10.06 (13.08)	2.189 (1.507)	5.328 (3.220)	-21.38 (11.44)
<i>N</i>	5033597	2223285	2223285	5033597	2223285	2223285
Cohort FE	X	X	X	X	X	X
Reverse Causality Controls	X	X	X	X	X	X
Distance Non-missing		X	X		X	X
Car Ownership & Other Traffic Crime Controls			X			X
Driving Distance Control			X			X

Estimates of equation 5.1 using the cross-sectional dataset described in section 5.1 and estimates of money-metric injury costs described in sections 5.3 and 5.4. Columns 1 and 2 use the cost of accident-related injuries incurred by “other-person” victims as the outcome; columns 3 and 4 use costs incurred by “other-party” victims as the outcome. Other-person injuries are injuries incurred by all accident participants excluding the focal individual; other-party injuries are injuries incurred by accident participants outside of the focal individual’s vehicle. An individual i ’s “speeding rate” is defined as the number of years from 2006-2018 in which i receives a speeding ticket, divided by 13; earned income refers to an individual’s average taxable earned income from 2006-2018. For details on the construction of control variables, see section 5.1. The car-ownership rate, non-speeding traffic-crime rate, and driving-distance controls are also interacted with earnings, when included. In columns 1 and 3, we weight each individual by the number of years in which they appear in the population register from 2006-2018; in columns 2 and 4, we use a compound weight which is the product of our baseline weight and the number of years for which driving distance information is available from 2013-2018. Heteroskedasticity-robust standard errors are in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

for the Finnish speeding fine system.

5.5 Robustness

Causal Injury Effects To measure the cost of accident-related injuries in section 5.3, we rely on a standard parallel-trends assumption. Even in the absence of pre-trends, this assumption could be violated if negative shocks *specific to the year of the accident* simultaneously cause both an accident and an earnings decline. For example, an individual who loses their job might experience an earnings decline and, as a result, begin to drive more recklessly.

Table 5.3: Income Gradient of Money-Metric Marginal Social Costs (Other-Party Victims), by Discount Rate and Degree of Inequality Aversion

	Discount Rate		
	3%	5%	7%
Degree of Inequality Aversion			
None ($\nu = 0$)	-18.6* (7.712)	-15.89* (6.694)	-13.67* (5.875)
Low ($\nu = 0.25$)	-16.07* (7.477)	-13.73* (6.504)	-11.84* (5.717)
Middle ($\nu = 1$)	-7.617 (4.392)	-6.486 (3.839)	-5.568 (3.393)
High ($\nu = 4$)	-7.55 (4.371)	-6.421 (3.821)	-5.507 (3.376)

Estimates of β_e^M from equation 5.1 using the cross-sectional dataset described in section 5.1 and estimates of money-metric injury costs described in sections 5.3 and 5.4; for details on the inequality-aversion parameter and the discounting procedure, see section 5.3. Other-party victims are accident participants outside of the focal individual's vehicle who incur an injury. For details on the construction of control variables, see section 5.1; the car-ownership rate, non-speeding traffic-crime rate, and driving-distance controls are also interacted with earnings. We weight each individual by the number of years in which they appear in the population register from 2006-2018, multiplied by the number of years for which an individual's driving distance information is available from 2013-2018. Heteroskedasticity-robust standard errors are in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

To evaluate this concern, Appendix figure 9.6 shows injury effects on post-tax income separately for vehicle drivers and passengers: the simultaneous-shocks hypothesis predicts that earnings declines should be larger for drivers. The two trajectories are extremely similar, suggesting that simultaneous negative shocks do not confound our estimates.

Reverse Causality As discussed in section 5.1, our main cross-sectional sample is vulnerable to reverse-causality issues. To assuage these concerns, we build an alternative dataset using 2006-2011 averages for independent variables and 2012-2022 averages for accident costs. Appendix table 9.8 shows estimates of the money-metric gradient from equation 5.1 using this sample for various discount rates and inequality-aversion parameters; we do not control for driving distance since the vehicle register is contemporaneous with our dependent variables. Unsurprisingly, results are attenuated. Nonetheless, we continue to estimate negative gradients, and our standard errors are small enough to reject meaningfully positive gradients.

Other Accident Costs Our main results only consider earnings losses associated with accident-related injuries as the cost of speeding. There are two dimensions we ignore: (1) other costs associated with injuries and (2) non-injury costs of accidents. Regarding (1), we note that other injury costs—including healthcare and psychological costs—are unlikely to vary with victims’ income, conditional on injury severity. However, if the speeding of high income drivers generates higher-severity accidents, omitted injury costs may bias our gradient downwards. To evaluate this possibility, we note that higher-severity accidents should be more likely to result in injuries. However, Appendix table 9.4 shows that, conditional on an accident, the marginal speeding effect on injury-likelihood is *decreasing* with offender income, implying that our gradient is likely to be biased *upwards*. Regarding (2), we expect non-injury costs—e.g. property damage—to co-vary primarily with the likelihood of an accident; however, we find in unreported results that the marginal speeding effect on accident likelihood is decreasing with offender income, implying that marginal non-injury costs are also likely to decrease with income.

6 Redistribution

We now turn to our empirical test of the redistributive rationale for income-based speeding fines. As elaborated in Section 3, our objective is to recover causal earnings effects on speeding behavior; these effects serve as instruments for evaluating the tagging potential of speeding behavior in a Mirrleesian model of redistribution. We begin by providing a decomposition of the causal earnings effect into labor-supply and income effects (section 6.1). To measure the labor-supply effect, we exploit within-individual variation in months employed, holding earnings fixed (section 6.2). Since income effects estimated using this method are likely upward-biased, we turn to an inheritance-shock design for measuring the income effect (section 6.3). We combine these two distinct estimates to implement our test of the redistributive rationale in section 6.4.

6.1 Earnings Effects and Weak Separability

Recall from figure 3.1 that the earnings effect is identified from labor-supply driven changes in earnings; intuitively, the redistributive potential of speeding fines depends on whether speeding fines distort labor supply to the same degree as income taxes. As a result, the earnings effect is composed of both a labor-supply effect and an income effect.

To clarify this decomposition, we index individuals by their implicit wage w . Given some tax-and-fine system, an individual with wage w chooses a speeding rate $s(z, l)$ as a function of pre-tax income $z = w * l$ and labor supply l . Taking the derivative with respect to l and re-scaling by $\frac{1}{w}$ yields:

$$\frac{ds(z, l)}{dz} \Big|_w := s'_{inc}(z) = \underbrace{\frac{\partial s}{\partial z}}_{\text{Pre-tax income effect}} + \underbrace{\frac{\partial s}{\partial l} * \frac{1}{w}}_{\text{Re-scaled labor supply effect}}$$

The earnings effect is composed of a (pre-tax) income effect and a wage-scaled labor-supply effect. Previous work that empirically estimates earnings effects (Allcott et al., 2019; Ferey et al., 2024) assumes weak separability, *i.e.* that the labor-supply effect is zero. Under this assumption, earnings effects can be identified from any exogenous income variation. While weak separability is a reasonable assumption for the contexts studied in previous papers—*i.e.* soda

consumption and savings behavior—it seems more likely that labor-supply has independent effects on *speeding* behavior.³¹ Indeed, as we show in the next section, individuals speed more in years where they are employed for more months during the year. As a result, we reject weak separability and separately estimate labor-supply and income effects.

6.2 Labor Supply Effects

Our approach for measuring the labor supply effect is motivated by a test of weak separability: weak separability is violated if, for a given individual, variation in labor supply has an independent effect on speeding behavior (holding income fixed). To demonstrate this approach, in this section we estimate a single labor supply effect for the general population; in section 6.4 we estimate the labor supply effect separately by income in order to construct the local earnings effects $s'_{inc}(z)$ that enter our test of the redistributive rationale (equation 3.2).

Concretely, we use our annual population panel from 2006-2019 and model ticket receipt $s_{i,t}$ for individual i in year t as:

$$s_{i,t} = \beta_1 * z_{i,t} + \beta_2 * \bar{w} * l_{i,t}^m + \gamma_i + \kappa_{a(i),t} + \epsilon_{i,t} \quad (6.1)$$

where $z_{i,t}$ is taxable earned income,³² $\bar{w} * l_{i,t}^m$ is months employed during the year, re-scaled into a money-metric using the average monthly wage;³³ γ_i is an individual fixed-effect; and $\kappa_{a(i),t}$ are year-by-cohort fixed effects. Our coefficient of interest is β_2 , the independent effect of months-employed on speeding-ticket receipt holding earnings fixed. We expect that, since months-employed is a noisy measure of *true* labor supply, our estimate of β_2 will be attenuated; we therefore view our labor-supply effects as conservative, *i.e.* biased in favor of finding a redistributive role for income-based fines.

³¹For example, additional labor supply may imply additional commuting time.

³²This measure includes some taxable transfers like unemployment benefits and pensions. Since these transfers insure against within-lifetime income risk, rather than across-individual income risk, we view this measure as corresponding to the static z in our model framework.

³³We set \bar{w} equal to the 2012 average wage in order to facilitate comparability with our inheritance sample in section 6.4. Note that we cannot use an individual's imputed wage in this regression due to co-linearity; we elect for a cross-sectional average wage since it is well-measured (as opposed to an individual's average wage) and less likely to be endogenous. When we estimate labor supply effects separately by income group, we calculate separate average wages for each group.

Column 2 of table 6.1 displays our estimates from equation 6.1. For comparison, column 1 shows the cross-sectional relationship between earnings and speeding rates: each €10,000 increase in annual earnings is associated with a 1.2 percentage point increase in the annual probability of receiving a speeding ticket. In column 2, we find a significant labor supply effect: a €10,000 increase in wage-scaled months-employed causes a 0.29 percentage point increase in the likelihood of receiving a speeding ticket.³⁴ Based on this estimate, we can say that the wage-scaled labor-supply effect explains around 25% of the overall cross-sectional gradient in column 1.

Table 6.1: Income Gradient of Speeding Behavior & Labor Supply Effect on Speeding

	Speeding Rate (pp)	
	(1)	(2)
	Cross-section	Within-person
Earned Income (€10,000s)	1.166*** (0.00609)	0.499*** (0.0106)
Re-scaled Labor Supply (€10,000s)		0.288*** (0.00768)
<i>N</i>	15207808	15172401
<i>R</i> ²	0.019	0.196
Pop. Sample	25%	25%
Cohort-Year FE	X	X
Individual FE		X

Estimates of equation 6.1 using a 25% sample of Finnish adults (*i.e.* individuals 18 years of age or older) that appear in the population register from 2006-2019. The “speeding rate” refers to the share of individuals who receive a speeding ticket in a given year, according to crime-history reports. “Earned income” is defined as taxable earned income, which is the sum of wage/salary income, unemployment insurance payments, and pension income. “Re-scaled labor supply” refers to an individual’s months employed during the year, multiplied by the average imputed monthly wage (taxable earned income in 2012 divided by months employed in 2012) among our sample. Standard errors (clustered at the individual level) are in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note that the coefficient on earnings ostensibly yields an estimate of the income effect, *i.e.* the effect of income on speeding rates holding labor supply

³⁴Concretely, this is the effect of increasing months employed by the amount necessary to generate an additional €10,000 in earnings under the average wage.

fixed. However, due to mis-measured labor supply, variation in earnings likely includes variation in labor supply; given the positive labor-supply effect we estimate, the coefficient on earnings would likely be an upward-biased estimate of the income effect. Since larger income effects make it harder to find a redistributive role for speeding fines, we use an alternative approach for estimating income effects.

6.3 Income Effects

Our identification approach for estimating income effects builds on Nekoei and Seim (2022), who leverage the differential timing of parental deaths to explore how inheritances contribute to wealth inequality. Taking this approach off-the-shelf poses a problem: parental deaths may have concomitant effects on speeding that are unrelated to the associated wealth shock, *e.g.* due to grieving or caregiving. As we show, individuals who receive little-to-no inheritance indeed exhibit speeding trends around parental death. Therefore, relative to Nekoei and Seim (2022), we add an additional difference across inheritance size to address non-wealth effects of parental death.

First Difference: Timing of Parental Deaths As in Nekoei and Seim (2022), the treatment event in our paper is the first parental death that a child experiences. We identify all such deaths from 2013-2022 using the cause-of-death register and parent-child links. We compare heirs whose first parental death occurs in year t (treated) to heirs whose first parental death occurs in $t + \delta$ (control). To address observable differences between parental-death cohorts, Nekoei and Seim (2022) apply DFL-reweighting (DiNardo et al., 1996) to homogenize the distribution of education and age across cohorts. We follow a similar approach, re-weighting each cohort with respect to sex, age, and education; since we are interested in population-level estimates, we normalize using the distribution of demographics in the 2012 population register.

In our baseline results, we set $\delta = 4$ and follow individuals for four years ($t = 0, 1, 2, 3$) post-treatment; we also examine robustness to using 5 post-periods. This “fixed-control” method (one control cohort per treatment cohort) generates a clean comparison that is balanced across periods and avoids any contamination from using early-treated observations as controls for late-treated observations. To create our event-time dataset, we construct treatment and control groups for each treatment year $y \in \{2013, 2014, 2015, 2016\}$;

we then stack observations across treatment years. Observations are therefore indexed by an individual i , a treatment year y , and an event time t (defined relative to y). We cluster all standard errors at the individual level and apply the demographic weights described above.

Second Difference: Inheritance Size To formally motivate our second difference, let β^{wealth} be the effect of a given wealth shock on speeding behavior; similarly, let α^{death} be the effect of a parental death on speeding behavior. A first-difference across parental death years measures the sum of both effects. However, assuming that the death effect α^{death} is common across inheritance size, a second comparison between those who receive *positive* inheritances $\Delta w > 0$ and those who receive *no* inheritance $\Delta w = 0$ will remove the death effect:

$$\underbrace{\underbrace{(\beta^{\text{wealth}} + \alpha^{\text{death}})}_{\text{DID}|\Delta w > 0} - \underbrace{\alpha^{\text{death}}}_{\text{DID}|\Delta w = 0}}_{\text{Triple Difference}}$$

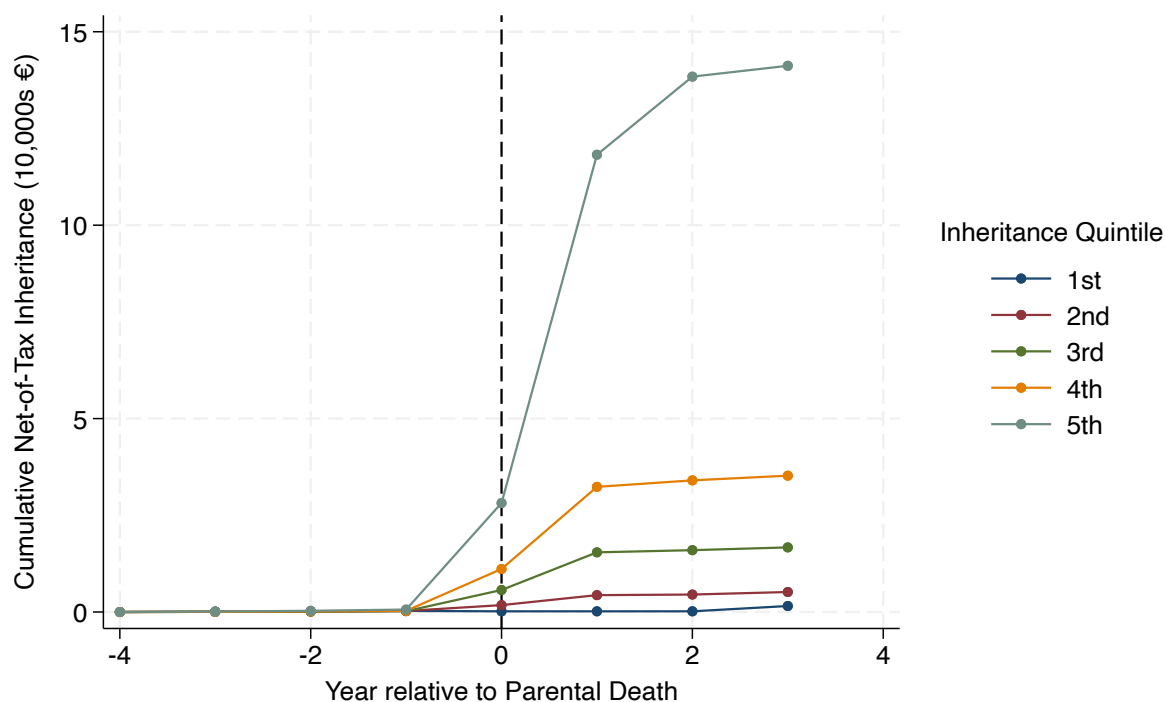
We discuss tests of the common-death-effect assumption in section 6.5. To construct the $\Delta w > 0$ and $\Delta w = 0$ groups (hereafter referred to as the “top” and “bottom” inheritance-size groups), we use cumulative inheritance flows following parental death; we show quintiles of these flows in figure 6.1. There are essentially no inheritance flows prior to the first parental death and the majority of flows occur within a year of the treatment event;³⁵ we use the flows during the four-year period from $t = 0$ through $t = 3$ as our first-stage, *i.e.* the denominator in our estimate of the income-effect. We assign the bottom two flow quintiles to the “bottom” group and the top three quintiles to the “top” group.³⁶

Estimating Income Effects To motivate our triple-difference approach,

³⁵The estate inventory must be filed with the tax authority within 3 months of the parent’s death, after which the tax authority assesses the inventory and makes a tax decision (which usually takes 6-12 months). Therefore, most inheritance decisions are made in the year following the parental death; inheritance flows in later years can be generated by a combination of above-average tax-processing times and second parental deaths.

³⁶In theory, the bottom group should receive zero inheritance; however, the bottom two quintiles receive around €2,300 on-average. We include some positive inheritances in the bottom group to improve power. In theory, this can lead to bias if income effects differ across inheritance size. We show in Appendix section 9.2.3 that the bias disappears as $\frac{\Delta W^{\text{top}} - \Delta W^{\text{bottom}}}{\Delta W^{\text{top}}} \rightarrow 1$; given that, on-average, the top group receives around €63,000, we expect this bias is negligible.

Figure 6.1: Cumulative Inheritance Flows Around First Parental Death



Cumulative net-of-tax inheritance flows around the year of first parental death for children whose first parent dies between 2013 and 2020, separately by quintile of flows; cumulative flows are normalized to 0 in $t - 5$. Net-of-tax inheritances refer to the monetary value of the inheritance received—according to the inheritance-tax return—net of inheritance taxes due. Heirs are allocated to inheritance-flow quintiles within each year-of-death cohort d based on the cumulative inheritances received in $[d, d + 2]$ (*i.e.* using 3 years of post-death data). We restrict to children who are 18 years of age or older in the year of the parental death event.

we first show how speeding behavior evolves around parental death for each inheritance-size group. Figure 6.2 plots (re-weighted) event study estimates from the following regression:

$$s_{i,y,t} = \gamma_t + \rho * I_{YOD(i)=y} + \sum_{k=-3}^4 \beta^k * I_{YOD(i)=y} * I_{t=k} + \epsilon_{i,y,t} \quad (6.2)$$

where $s_{i,y,t}$ is an indicator for whether i received a speeding ticket in year $y + t$, γ_t are event-time fixed effects, $I_{YOD(i)=y}$ is an indicator for whether i 's first parental death occurred in year y , and β^k is the treatment coefficient for event year k . We use $t - 4$ as the omitted year in order to show anticipation effects, which appear for both inheritance-size groups in year $t - 1$ of figure 6.2. Following this anticipation effect, the two groups diverge, with the top group exhibiting higher speeding rates.

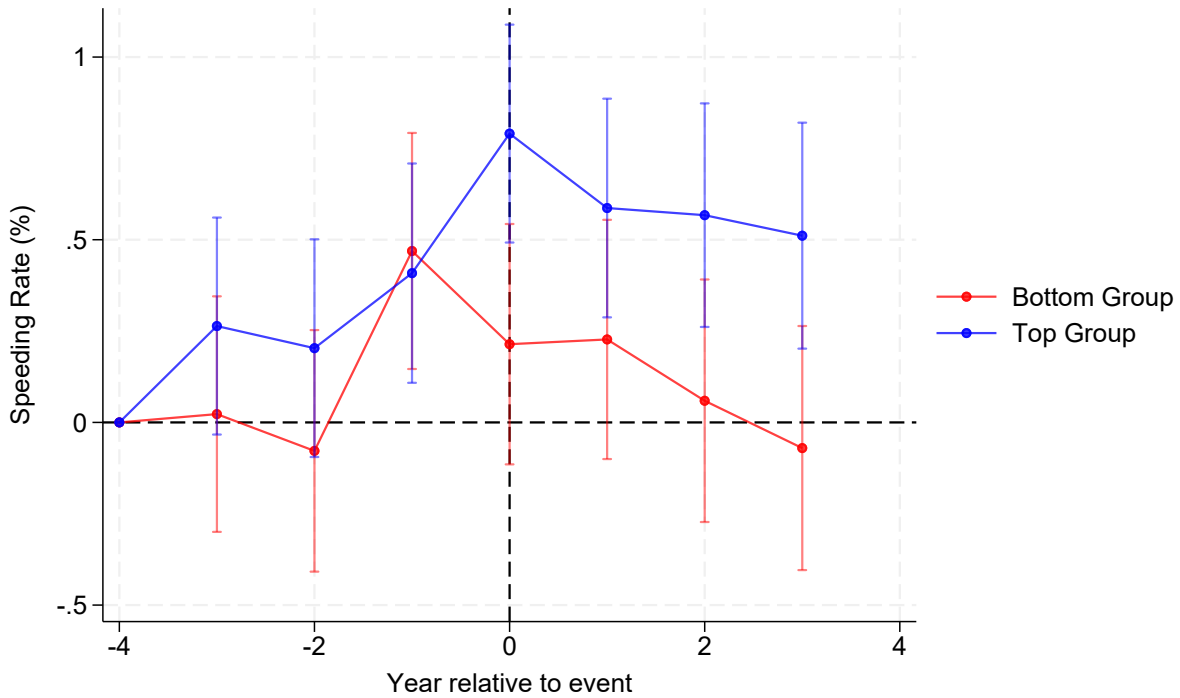
Given that the common anticipation effect in $t - 1$ is present for the group receiving little-to-no inheritance, this spike is unlikely to be driven by anticipated wealth shocks. To bolster this claim, Appendix figures 9.11 and 9.12 show that there are no anticipatory effects on earnings or car ownership. As a result, we want to difference this anticipation effect out of our income-effect estimates.³⁷ Figure 6.3 plots the difference in event-study coefficients, $\beta_{\text{top}}^t - \beta_{\text{bottom}}^t$, from figure 6.2. While the series is somewhat noisy, we see a clear level difference between the pre- and post-treatment periods, as well as the absence of statistically significant pre-trends.

To estimate income effects, we use the reduced-form speeding effects from figure 6.3 and the first-stage inheritance flows from figure 6.1 in a triple-difference IV approach; our estimating equations are shown in Appendix section 9.2.4. Implicitly, our approach assumes that the extent to which the inheritance shock will be “spent” on speeding fines is confined to our four-year treatment horizon; we view this approach as somewhat conservative (*i.e.* leading to smaller income effects).³⁸ While we are limited in our ability to explore longer treatment horizons, we do find that our income effect is somewhat larger with a 5-year horizon (section 6.5). However, we

³⁷This anticipation effect could be driven by caregiving-related travel.

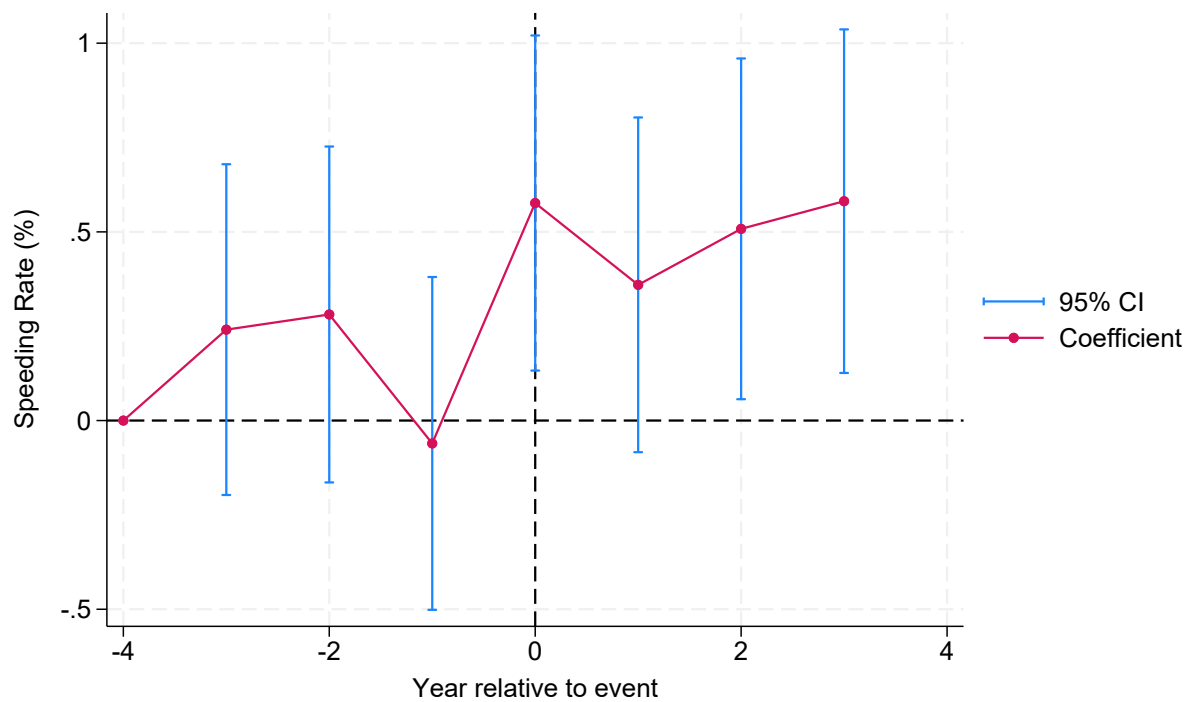
³⁸At least two other forces push towards a conservative interpretation: some inherited assets are less liquid than cash and inheritances are, to some extent, anticipated. Unfortunately, we do not have access to the asset composition of inheritances.

Figure 6.2: Event Study Effects of First Parental Death on Speeding Rates, by Inheritance-Size Group



Estimates of β^k from equation 6.2, separately by inheritance-size group. The treatment group consists of heirs whose first parent dies in [2013,2016]; the control group consists of heirs whose first parent dies in [2017,2020]. The bottom inheritance group, for a given parental death-cohort d , consists of heirs in the bottom two quintiles of total inheritance flows received in $[d, d + 2]$; the top inheritance group for a given death-cohort consists of heirs in the top three quintiles of total inheritance flows received in $[d, d + 2]$. We restrict to heirs who are 18 years-of-age or older in the year of the parental death event. Each death-cohort-by-inheritance-size group is reweighted to match the 2012 distribution of age, sex, and education in the population register. Standard errors are clustered at the individual level.

Figure 6.3: Event Study Effects of First Parental Death on Speeding Rates: Triple Difference



Differences in event-study coefficients, $\beta_{\text{top}}^t - \beta_{\text{bottom}}^t$, between top and bottom inheritance-size groups from equation 6.2. See figure 6.2 for separate estimates of β_{top}^t and β_{bottom}^t , as well as details on sample construction. Each death-cohort-by-inheritance-size group is reweighted to match the 2012 distribution of age, sex, and education in the population register. Standard errors are clustered at the individual level.

maintain a 4-year horizon to improve precision when estimating income-effect heterogeneity, which is important for evaluating the redistributive potential of income-based fines (see equation 3.2).

Table 6.2 shows our estimate of the average income effect; we discuss effect heterogeneity in section 6.4. The DDD-IV estimate in column 3 implies that an additional €10,000 inheritance shock causes a 0.26 pp increase in the likelihood of receiving a speeding ticket in the post-period; this effect is similar in magnitude to the average labor supply effect we estimate in section 6.2. As a result, the average earnings effect (labor supply effect + income effect) explains nearly one-half of the cross-sectional relationship between income and speeding from table 6.1.

Table 6.2: DDD-IV Estimates of the Income Effect

	(1)	(2)	(3)
	Reduced-Form	First-Stage	IV
DDD Coefficient	0.391*** (0.115)	1.511*** (0.0344)	
Inherit. Shock (10,000s €)			0.259*** (0.0765)
Constant	7.316*** (0.0540)	0.00701*** (0.000927)	7.314*** (0.0541)
N	4912424	4912424	4912424

Estimates of β from equation 9.5 (column 1), θ from equation 9.4 (column 2), and β/θ (column 3). The treatment group consists of heirs whose first parent dies in [2013,2016]; the control group consists of heirs whose first parent dies in [2017,2020]. The bottom inheritance group, for a given parental death-cohort d , consists of heirs in the bottom two quintiles of total inheritance flows received in $[d, d + 2]$; the top inheritance group for a given death-cohort consists of heirs in the top three quintiles of total inheritance flows received in $[d, d + 2]$. We restrict to heirs who are 18 years-of-age or older in the year of the parental death event. Each death-cohort-by-inheritance-size group is reweighted to match the 2012 distribution of age, sex, and education in the population register. Standard errors (clustered at the individual level) are in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

We now discuss across-income heterogeneity in the labor-supply and income effects, which we use to evaluate the redistributive rationale for income-based fines.

6.4 Optimal Redistributive Fine Gradient

To test whether a Mirrleesian redistributive rationale can justify income-based fines, we assess whether our estimate of the optimal redistribution-driven gradient $\hat{b}_{\text{redist}}^*$ matches the observed speeding-fine gradient in Finland. In our setting, the key determinant of whether b_{redist}^* is positive or negative is the covariance between welfare weights $g(z)$ and relative speeding preferences $s'_{\text{het}}(z) := s'(z) - s'_{\text{inc}}(z)$. When this covariance is *negative*, fines should *increase* with income: intuitively, the planner should use speeding fines to raise revenue from individuals with low marginal utility (low welfare weights) and small distortionary costs (strong preferences for speeding). Conditional on this covariance, the magnitude of b_{redist}^* is also shaped by the (average) speeding fine elasticity $\bar{\zeta}_\tau$: when behavior is highly responsive to fines, the redistributive role of speeding fines is attenuated.³⁹ As a result, b_{redist}^* is determined by three key inputs:⁴⁰ welfare weights $g(z)$, speeding preferences $s'_{\text{het}}(z)$, and the fine elasticity $\bar{\zeta}_\tau$. We discuss our estimates of each object in turn.

Welfare Weights We take the same approach detailed in section 5.3 and use welfare weights $g(z) \propto y^{-\nu}$, where y is net income (Saez, 2002b). In our baseline results, we set $\nu = 1$.

Speeding Preferences We first estimate heterogeneity in our labor supply and income effects to obtain earnings effects $s'_{\text{inc}}(z)$. To make these two sets of estimates internally consistent, we estimate the labor supply effects using the individuals from the top inheritance-size group—the group for which we can identify income effects—and apply the demographic weights from our inheritance-shock design.⁴¹ We split this sample into 8 equal-sized groups based on 2012 age-adjusted earnings and estimate the wage-scaled labor supply effect (equation 6.1) separately for each group.⁴² For income-effect hetero-

³⁹In some cases, changes in the fine-schedule slope can generate revenue on the margin. In such cases, our logic reverses: a negative covariance between $g(z)$ and $s'_{\text{het}}(z)$ implies a negative fine gradient. However, with a constant or uncorrelated fine elasticity, our intuition will hold if $s(z)/\tau(z)$ is increasing in income, which is true in our setting when considering overall speeding crime (fixed and day-fine crime combined). In general, our intuition holds when fines are more distortionary (on the margin) for above-average earners.

⁴⁰The magnitude also depends on the joint-distribution of earnings, speeding behavior, and observed fine amounts. Statistics of this distribution can be found in Appendix table 9.14.

⁴¹Appendix table 9.13 estimates the cross-sectional gradient and average labor-supply effect for this weighted sample using our within-individual design from section 6.2; the results are extremely similar to those in table 6.1 for the general population, suggesting that our estimates are representative.

⁴²For these estimates, we use the group-specific average wage to scale labor supply. We choose 2012 as

geneity, we are more limited by power considerations and therefore estimate separate effects only for below- and above-median earnings groups;⁴³ however, despite a large earnings difference between these two groups, we find only a small difference (0.076 pp) for the income effect, suggesting that heterogeneity is minimal. Given these two sets of estimates, we measure the earnings effect $s'_{inc}(z_j)$ for each income group $j \in \{1, 2, \dots, 8\}$ as the sum of group-specific labor-supply and income effects.

To obtain speeding preferences $s'_{het}(z_j)$, we use the 2012 cross-sectional profile of speeding rates $s(z_j)$ to calculate across-income variation $s'(z_j)$; we then subtract our earnings effects $s'_{inc}(z_j)$. Figure 6.4 displays our results (see Appendix table 9.14 for exact estimates). Cross-sectional variation $s'(z)$ (blue dashed line) initially increases with earnings before declining, reflecting the s-shaped cross-sectional profile $s(z)$ (see Appendix figure 9.18). By contrast, the earnings effects $s'_{inc}(z)$ (red-dashed line) are relatively homogeneous; as a result, the profile of speeding preferences $s'_{het}(z)$ (solid purple line) exhibits an inverted u-shape.

Fine Elasticity We assume that fine elasticities are uncorrelated with income and use the average elasticity estimate $\hat{\zeta}_\tau = -0.157$ from Kaila (2024), which uses a regression-discontinuity-design around the income-based fine cutoff to measure the effect of larger speeding fines on future recidivism.⁴⁴

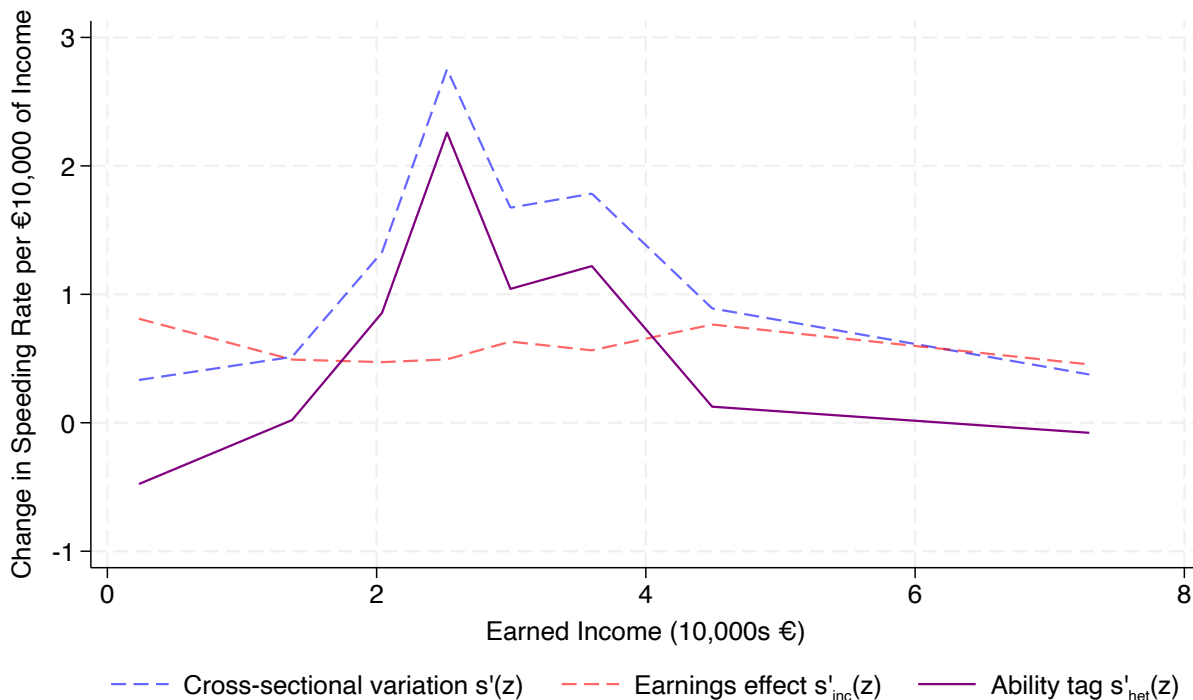
Optimal Redistributive Gradient Figure 6.5 plots our estimates of speeding preferences $\hat{s}'_{het}(z)$ alongside our baseline profile of welfare weights; the two series move in opposite directions at low incomes but co-move for a large segment of the earnings distribution. The resulting covariance is *positive*, implying that our estimate of the optimal fine gradient \hat{b}^*_{redist} will be *negative*. Using the fine elasticity estimate from Kaila (2024), we estimate an optimal redistributive gradient of -12€ per €10,000 of earnings, implying that fines should *decrease* by €12 for every €10,000 of income at the optimum. Using

our benchmark year since it is one year prior to the first cohort of parental deaths in our sample.

⁴³To increase power, we use the set of parental death cohorts $[y + 1, y + 4]$ as a control group for cohort y , in contrast to our baseline approach which uses only the $y + 4$ cohort as a control. In unreported results, we find that expanding the control group and re-estimating the average income effect yields a result nearly identical to that of table 6.2.

⁴⁴We do not assume an uncorrelated *semi*-elasticity, as in equation 3.2; instead, we use a generalized expression for the optimal fine gradient (Appendix equation 9.3) and assume a fine elasticity that is uncorrelated with income.

Figure 6.4: Relationship between Speeding Preferences and Earnings

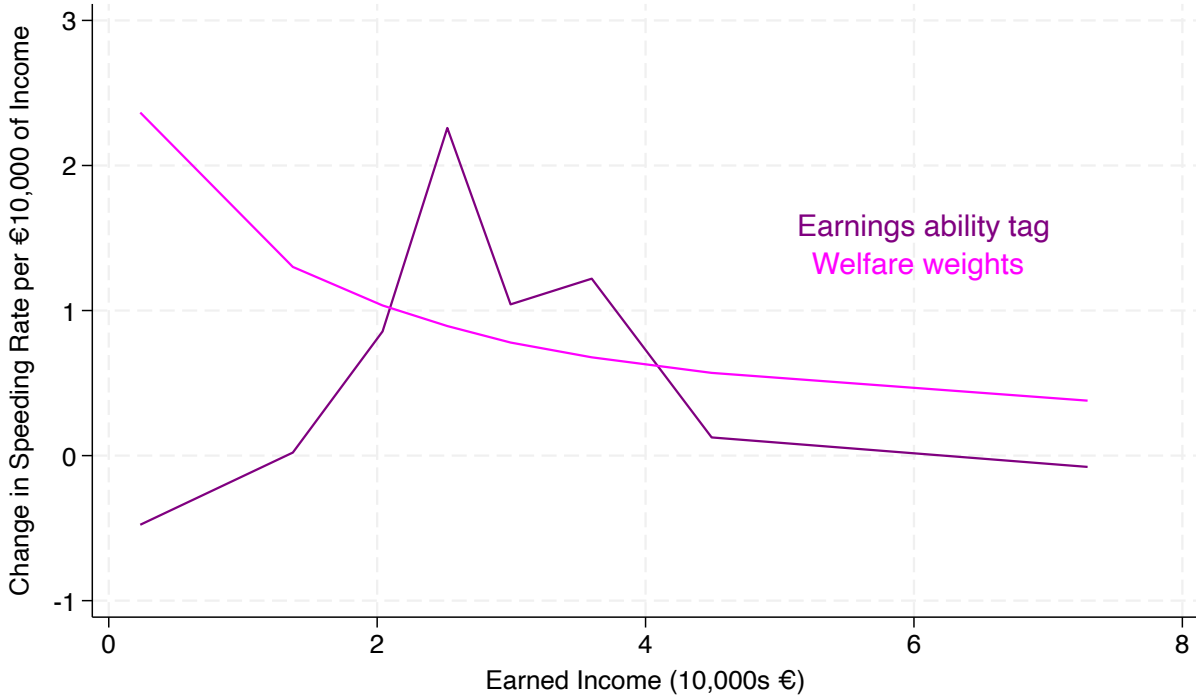


Estimates of $s'(z_j)$, $s'_{inc}(z_j)$, and $s'_{het}(z_j)$ for each income group $j \in \{1, 2, \dots, 8\}$ among the sample of heirs in the top inheritance-size group. Individuals are reweighted to match the 2012 distribution of age, sex, and education in the population register. Income groups are assigned based on 2012 age-adjusted earnings. To calculate $s'(z_j)$, we estimate age-adjusted speeding rates in 2012 for each income group $s(z_j)$ (Appendix figure 9.18); then, for each income group $j < 8$, $s'(z_j) = \frac{s(z_{j+1}) - s(z_j)}{z_{j+1} - z_j}$. For the top income group $j = 8$, we estimate $s'(z_8)$ using a regression of speeding ticket receipt on earnings, as in column 1 of table 6.1. To estimate $s'_{inc}(z_j)$, we combine estimates of β_2 from equation 6.1 (the labor-supply effect) and β/θ from equations 9.4 and 9.5 (the income effect); for details on sample construction and the implementation of these equations, see section 6.4. Appendix table 9.14 shows the estimates that underlie this graph.

this estimate, we cannot rationalize the steepness of either the day-fine gradient (+88 €) or the combined-fine gradient (+12.4 €) that we estimated in section 2.

To assess the robustness of this result, Appendix figure 9.19 shows a heatmap of estimated optimal fine gradients \hat{b}_{redist}^* as a function of the fine elasticity $|\bar{\zeta}| \in \{0.1, 0.157, 0.2\}$ and the degree of inequality aversion $\nu \in \{0.25, 1, 4\}$. Across all considered values, we find a negative gradient, with the strongest (*i.e.* most negative) gradients arising from weaker fine elasticities and stronger redistributive preferences (higher ν). While the former result is mechanical, the latter reflects the logic of tagging: when the planner only places weight on

Figure 6.5: Covariance between Welfare Weights and Ability Tags



Estimates of welfare weights $g(z_j)$ and earnings ability tags $s'_{\text{het}}(z_j)$ for each income group $j \in \{1, 2, \dots, 8\}$ among the sample of heirs in the top inheritance-size group. Individuals are reweighted to match the 2012 distribution of age, sex, and education in the population register. For details on the estimation of $s'_{\text{het}}(z_j)$, see section 6.4. To construct welfare weights, we use $g(z_j) \propto y(z_j)^{-1}$, where $y(z_j)$ is the average net income of the j th income group; see Appendix table 9.14 for the estimates of $y(z_j)$ and $s'_{\text{het}}(z_j)$ underlying this graph.

the welfare of the poorest individuals (*i.e.* ν is large), the government seeks to maximize revenue collection for the purpose of redistribution. The strong negative relationship between preferences and earnings for middle-to-high incomes (figure 6.5) implies that such revenue is maximized using a strongly negative fine gradient.

In summary, we find that relative speeding preferences are negatively correlated with earnings overall, implying that the efficiency costs associated with using speeding fines—rather than income taxes—to redistribute are increasing with income. As a result, optimally redistributive speeding fines would *decrease* with income under any conventional parameterization of welfare weights. We therefore reject the redistributive rationale for income-based speeding fines in Finland.

6.5 Robustness

Labor Supply/Earnings Responses to Inheritance Shocks Our estimates of income effects using inheritance shocks should hold labor supply fixed. Furthermore, to the extent that earnings respond to inheritance receipt, we may be mis-measuring the denominator in our income effect. Appendix figures 9.13 and 9.14 show event-studies for months-employed and earnings respectively using our triple-difference approach. There is no discernible labor supply effect, and earnings effects are small: in any given period, earnings decrease by no more than €400. Given the size of the inheritance shocks in our first stage (table 6.2), this potential source of contamination is quantitatively negligible.

Inheritance-Shock Effect Horizon We use a 4-year effect horizon to measure income effects (section 6.4). While longer effect horizons are under-powered to measure income-effect heterogeneity, we examine robustness to including an additional post-treatment year in Appendix table 9.12. We estimate an income effect 0.3 pp, about 15% higher than our baseline estimate in table 6.2. An event-study with 5 post-periods (Appendix figure 9.17) shows a decline in speeding effects after four years. These results suggest that our four-year effect horizon yields slightly conservative estimates.

Comparability of Parental Death Effects A key assumption we use to identify income effects in section 6.3 is that death effects are common across inheritance-size groups. In Appendix section 9.2.5, we provide evidence against several potential violations of this assumption: differences in parental cause-of-death, differences in speeding preferences, and differences in estate complexity.

7 Fairness Preferences

We now turn to our survey, which we use to evaluate the two fairness-driven rationales for income-based speeding fines outlined in section 3. We posit that income-based fines may be valuable to achieve “equal compliance” (*i.e.* equal speeding rates across incomes) or “proportional punishment,” where the latter may instantiate a preference to equalize the utility costs of speeding (see section 3). We build our survey to quantify both of these forces and any residual value that respondents may place on income-based fine policies. In

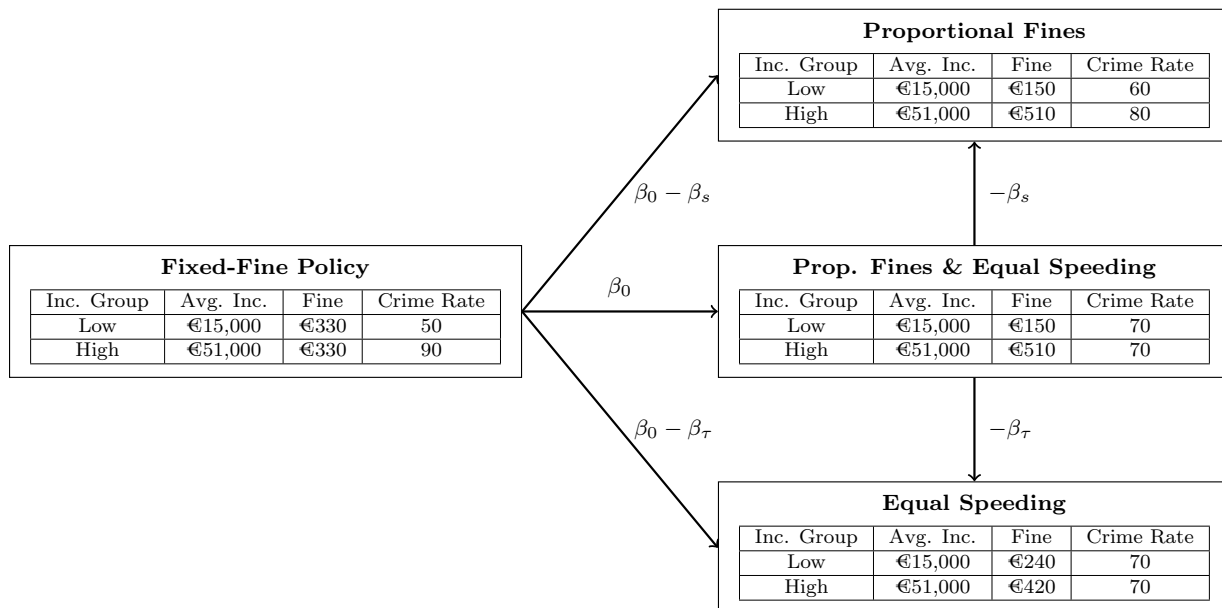
particular, the key question in our survey asks respondents to hypothetically compare a given fixed-fine policy to a randomly-varied income-dependent policy; we explore how respondents valuations (in terms of government revenue) vary with these randomized characteristics. We begin by discussing the fine-policy comparison in some detail, before turning to other aspects of our survey design.

7.1 Identification Approach

In our main survey question, respondents compare a fixed-fine policy to an income-based policy. The simplest such policies that allow us to test the equal-compliance and proportional-punishment rationales are four-dimensional: they assign a fine and speeding rate to each of two distinct income groups, as shown by the policies in figure 7.1. To increase our power, we restrict fine policies to have the same (unweighted) average fine level and the same average speeding rate. Policies are therefore differentiated along two key dimensions: (1) the difference in speeding rates $s_{\text{high}} - s_{\text{low}}$ between the high- and low-income groups and (2) the difference in fines $\tau_{\text{high}} - \tau_{\text{low}}$ between the high- and low-income groups.

Our identification approach is illustrated in figure 7.1. Respondents are shown a fixed-fine policy where fines are uniform and speeding rates are higher for the high-income group, as in the fixed region of the fine schedule in Finland (figure 2.2). For comparison, a given respondent is shown a randomly selected income-based fine policy. These income-based policies randomly deviate from a baseline policy that levies proportional fines and induces equal speeding rates, as in the income-based region of Finland’s fine schedule (figure 2.2). This baseline policy is shown as the middle policy in the right column of figure 7.1. We independently vary the difference in speeding rates and the difference in fine levels across respondents, as illustrated by the right column of figure 7.1. To test the equal-compliance rationale, we randomize whether the behavior difference under the income-based policy $s_{\text{high}}^{\text{inc}} - s_{\text{low}}^{\text{inc}}$ is positive, zero, or negative; the equal-compliance rationale predicts that income-based policies are less desirable when $s_{\text{high}}^{\text{inc}} - s_{\text{low}}^{\text{inc}}$ is non-zero. Similarly, to test the proportional-punishment rationale, we randomize whether the income-based schedule $\{\tau_{\text{low}}^{\text{inc}}, \tau_{\text{high}}^{\text{inc}}\}$ is proportional, steeper than proportional (“above-

Figure 7.1: Survey Identification



proportional”), or flatter than proportional (“below-proportional”);⁴⁵ the proportional-punishment rationale predicts that income-based policies are less desirable when $\{\tau_{\text{low}}^{\text{inc}}, \tau_{\text{high}}^{\text{inc}}\}$ is not proportional.

Figure 7.1 shows that we can also use our design to test whether fairness preferences justify the observed fine gradient in Finland. In particular, suppose the behavioral- and fine-fairness terms are affine (as in section 3): $\bar{e}_s(z) = \alpha_s + \beta_s * z$ and $\bar{e}_\tau(z) = \alpha_\tau + \beta_\tau * z$. Under this assumption, β_s and β_τ are the fairness parameters that enter the expression for the optimal fine gradient b^* (equation 3.2). Our design identifies β_s and β_τ when the low- and high-income groups are equal-sized.⁴⁶ We discuss our approach in more detail in more detail in section 7.5.3.

Note that, because we hold the overall speeding rate constant across policies,

⁴⁵Formally, income-proportional fines satisfy $\tau_{\text{high}}^{\text{inc}}/\tau_{\text{low}}^{\text{inc}} = \bar{z}_{\text{high}}/\bar{z}_{\text{low}}$. Below-proportional fines are more equal than the income ratio ($\tau_{\text{high}}^{\text{inc}}/\tau_{\text{low}}^{\text{inc}} < \bar{z}_{\text{high}}/\bar{z}_{\text{low}}$) and above-proportional fines are more unequal than the income ratio ($\tau_{\text{high}}^{\text{inc}}/\tau_{\text{low}}^{\text{inc}} > \bar{z}_{\text{high}}/\bar{z}_{\text{low}}$).

⁴⁶This arises because, under our randomization, each income-based policy can be expressed as an equal-and-opposite reform $\{\{-d\tau, d\tau\}, \{ds, -ds\}\}$ of the proportional-and-equal policy. Thus, when the two income groups are equal-sized, such reforms eliminate the α intercept terms, implying that the fairness differences across income-based policies depend only on the β slope terms. For formal derivations, see Appendix table 9.15.

safety-related costs should not be a confound when respondents make policy comparisons.⁴⁷ However, three other confounds might arise.

First, the comparison across income-dependent policies within the right column of figure 7.1 is confounded by the behavior *change* relative to the fixed-fine policy. We therefore randomize the size of the behavior change induced by a given income-based policy. Implicitly, this introduces another test of behavioral fairness since, for a given income-dependent policy, increasing the between-policy behavior change implies more behavioral inequality in the fixed-fine policy: if respondents value increases in behavioral equality, they should place a higher value on income-dependent policies that induce larger behavior changes.

Second, income-based policies may be desirable because of liquidity concerns, a preference that is distinct from the fairness concerns we study. To deal with this concern, we randomize the average fine level across respondents:⁴⁸ if liquidity drives demand for income-based fines, then demand should decrease as we lower the average fine level.

Third, fine-policy comparisons may be confounded by redistributive preferences: respondents may favor income-dependent fines simply because such policies implicitly transfer money to infra-marginal low-income individuals at the expense of inframarginal high-income individuals. To deal with this confound, we separately ask respondents to compare a fixed transfer and an income-dependent transfer, where the difference between the two transfers captures the implicit (first-order) redistribution implied by the fine-policy comparison.⁴⁹ We then difference out respondents' valuations for the income-dependent transfer from their valuations of the income-dependent fine policy, yielding a net-of-redistribution valuation for income-dependent fines.

Appendix table 9.16 details the set of values we use in our randomization; we discuss how we choose the average speeding rate, fine levels, and group

⁴⁷See section 7.2 for how we make this aspect salient to respondents. We also conduct robustness checks using free-response data (section 7.6).

⁴⁸By average, we mean an unweighted average across statutory fine levels, *i.e.* not a behavior-weighted average.

⁴⁹To first-order, increasing the speeding fine by $d\tau$ for an individual i with a speeding rate s_i is equivalent to taking $d\tau * s_i$ € away from i . For additional details on how we construct these transfers, see Appendix section 9.2.6.

incomes in Appendix section 9.2.7.

7.2 Survey Design

The fine policy comparison consists of four parts: (1) an explanation of speeding-crime rates + a comprehension question to ensure that respondents understand this concept (see Appendix figure 9.20);⁵⁰ (2) a binary choice between fixed and income-dependent fine policies; (3) a slider that respondents use to indicate how much government revenue they would be willing to forgo in order to implement their preferred fine policy; and (4) an open-text question that asks respondents to describe the factors that influenced their choice in (2) (see Appendix figure 9.21).

In the binary choice, respondents are told that their own speeding fine is unaffected by their policy choice and to make a choice based on what would be better for Finland overall. To make it clear that overall speeding is unaffected by the policy choice, each policy description includes the (constant) number of speeding tickets that would arise and we emphasize that this is held constant in the instructions. Instructions and policy descriptions are shown in Appendix figure 9.22.

After making the binary choices, we tell respondents that the government could hypothetically save on administrative costs by implementing their dis-preferred system (see Appendix figure 9.23 for exact wording). Respondents then use a slider (Appendix figure 9.24) to indicate the minimum annual savings (WTP) that would compel them to switch to their dis-preferred policy. We display the savings in both total and per-capita terms; the slider is capped at an annual savings of €275 per-capita,⁵¹ which corresponds to a total annual savings of €1.238 billion. Finally, respondents use a text box to describe the factors that influenced their fine-policy choice; we use these responses to evaluate the persistence of confounds in section 7.6.

For the hypothetical transfer comparison described in 7.1, respondents answer an analogous set of questions: a binary choice (Appendix figure 9.25), a slider, and a text response.⁵² We randomize whether or not the transfer

⁵⁰We force respondents to correctly answer this question before proceeding.

⁵¹In our survey, “per-capita” means per Finnish adult, of which there are around 4.5 million.

⁵²One difference in the wording between the two policy comparisons is that the transfer is motivated as

questions appear before the fine-policy questions and present an attention check between the two sets of questions.⁵³ Each respondent completes only one set of responses (*i.e.*, one fine-policy comparison and one transfer comparison); this mitigates survey fatigue and ensures that respondents do not see mutually inconsistent policies.⁵⁴ Our identification therefore relies on across-respondent randomization.

7.3 Survey Recruitment

We collected 3,038 survey responses from a survey pool of Finnish residents maintained by Norstat. To create representative survey samples, Norstat monitors the demographic distribution of respondents as surveys are completed and dynamically adjusts its contact rate for different groups. As a result, our survey is approximately representative on income, age, and gender, based on demographic information we collect from respondents at the end of the survey (Appendix table 9.17). With respect to rates of speeding-ticket receipt and car-ownership, our sample is also consistent with statistics from administrative data.⁵⁵

Consistent with our pre-registration, we drop respondents who fail the attention check (104 respondents) or choose the maximum slider value in *both* the

a one-time occurrence, which is a more natural story for transfers. Because the government savings being considered in the transfer scenario are also one-time savings—as opposed to annual savings in the fine-policy comparison—the distinction does not matter for comparing valuations across the two policy types.

⁵³We use the following attention check, taken from Rees-Jones and Taubinsky (2019):

Sometimes, respondents to online surveys do not read all the instructions carefully and proceed too quickly. With this question, we want to make sure that respondents to this survey pay attention to the instructions provided. We now ask you to simply press the “Next” button at the bottom of the page. Do not select any of the options listed below. You can now move forward by pressing “Next”.

Respondents are provided three answer choices, “Never,” “Sometimes,” and “Always;” any respondent who fills in one of these answers fails the attention check.

⁵⁴The survey as-is takes the median respondent around 13 minutes to complete and the fine policy questions in particular are highly involved. Additional fine-policy comparisons would also require additional transfer comparisons. On the second point, given that our survey is hypothetical, showing respondents mutually inconsistent policies (e.g. income-dependent policies with the same schedules but different speeding rates) could undermine the perceived legitimacy of a given policy.

⁵⁵In our administrative data on speeding ticket receipt, available through 2019, around 7% of individuals receive a ticket in a given year. In our survey, we find that the share of respondents who report receiving a ticket in the last 2 years is slightly above 7%, though it is well known that enforcement rates have declined following the implementation of a new camera system in 2020. For car ownership rates, a 2016 household survey (see here) found that around 74% of households have at least one car; for our survey respondents, this rate is around 80%.

fine and transfer comparisons (210 respondents).⁵⁶ The summary statistics for our main sample are shown Appendix table 9.17; differences between the main sample and the full set of respondents are negligible.

7.4 Estimation

Our main dependent variable of interest is net-of-redistribution WTP for income-dependent fines WTP_i^{net} for each respondent i . To calculate this variable, we take $WTP_i^{\text{inc-fine}}$, respondent i 's WTP for the income-based fine policy, and subtract $WTP_i^{\text{inc-transfer}}$, respondent i 's WTP for the income-based transfer.⁵⁷ Our main regression assesses how net WTP responds to the randomized features of the fine-policy comparison discussed in section 7.1.

Let $s_X^{\text{inc}}(i)$ refer to the speeding rate of group $X \in \{\text{low}, \text{high}\}$ under the income-based fine policy shown to respondent i . We define $\Delta s_i := s_{\text{low}}^{\text{inc}} - s_{\text{low}}^{\text{fixed}}$ as the behavior change between the fixed and income-based fine policies.⁵⁸ Echoing our notation elsewhere in the paper, let b_i refer to the fine gradient of the income-based policy; we designate b_{prop} as the proportional fine gradient. Our main regression is given by:

$$\begin{aligned} WTP_i^{\text{net}} = & \beta_0 + \beta_s^{\text{high}} * I(s_{\text{low}}^{\text{inc}}(i) < s_{\text{high}}^{\text{inc}}(i)) + \beta_s^{\text{low}} * I(s_{\text{low}}^{\text{inc}}(i) > s_{\text{high}}^{\text{inc}}(i)) \\ & + \beta_s^{\text{change}} * \Delta s_i + \beta_\tau^{\text{below}} * I(b_i < b_{\text{prop}}) + \beta_\tau^{\text{above}} * I(b_i > b_{\text{prop}}) + \epsilon_i \end{aligned} \quad (7.1)$$

where $I(\cdot)$ is the indicator function. The constant term β_0 measures net WTP for the omitted policy with proportional fines, equal speeding rates, and a baseline behavior change $\Delta s = 20$ (see Appendix table 9.16).⁵⁹ The parameters β_s^{low} and β_s^{high} measure the effect on net WTP of deviating from an equal distribution of speeding rates; the equal-compliance rationale predicts that both of these parameters are negative. If respondents generally

⁵⁶For respondents that choose the maximum slider value in both policy comparisons, we cannot infer the sign of WTP for income-dependent fines after differencing out redistributive motives.

⁵⁷For individuals that prefer fixed fines or fixed transfers, the policy-specific WTP measures are negative.

⁵⁸As shown in Appendix table 9.16, this variable is always strictly positive, implying that the policy comparison uses semi-elasticities that are strictly negative.

⁵⁹Given the average earnings levels of the low- and high-income groups, the income-based fine schedules we consider can be grouped into three discrete categories—proportional, below-proportional, and above-proportional—regardless of the overall fine level, which we vary for the purpose of testing liquidity-based rationales (see discussion in section 7.1).

value decreases in behavioral inequality, then we would expect $\beta_s^{\text{change}} > 0$: as discussed in section 7.1, larger behavior changes Δs imply more behavioral inequality under the fixed-fine policy, making the income-based policy relatively more attractive.

Turning to fine-schedule effects, $\beta_\tau^{\text{below}}$ $\beta_\tau^{\text{above}}$ measure the effect on net WTP of deviating from a proportional fine schedule; the proportional-punishment rationale predicts that both of these parameters are negative.

All β_s coefficients in equation 7.1 measure the effect of changing $s_{\text{high}} - s_{\text{low}}$ by 20 tickets per 1,000; the β_τ coefficients measure the effect of changing $\tau_{\text{high}}^{\text{inc}} - \tau_{\text{low}}^{\text{inc}}$ by €145.⁶⁰ We discuss the mapping between regression estimates and optimal fine-gradient parameters β_s and β_τ in section 7.5.3.

7.5 Results

7.5.1 Baseline Estimates

We begin by discussing respondents' extensive-margin preferences for income-based fine policies. Table 7.1 shows the share of our main sample that strictly prefers the income-dependent fine policy (row 1), strictly prefers the income-dependent transfer (row 2), and strictly prefers income-based fines after accounting for redistributive preferences (row 3).⁶¹ More than 3/4 of our sample strictly prefers the income-dependent fine policy; for the income-dependent transfer, that share is slightly lower, but still more than 2/3. On net, 56% of our sample strictly prefers the income-dependent fine policy after accounting for redistributive preferences expressed in the transfer comparison.

Turning to our WTP measure, table 7.2 shows that respondents are on-average willing to forgo €75 per-capita of government revenue in order to implement an income-based fine policy. Given an average WTP for income-dependent transfers of €43 per-capita, average net-of-redistribution WTP for income-based fines is €32 per-capita, which corresponds to a total WTP of €144 million. Median net WTP is about 25% as large as the average, though we can reject that the median is equal to 0 and the implied total

⁶⁰The variation in $\tau_{\text{high}}^{\text{inc}} - \tau_{\text{low}}^{\text{inc}}$ depends on the average fine level (see Appendix table 9.16). Therefore, €145 is the average of variation across fine-levels. However, when we condition on a given fine level, this implies a different scaling for the fine changes.

⁶¹We say a respondent has a strict preference for a policy when they indicate a positive WTP for it.

Table 7.1: Extensive-Margin Preferences for Hypothetical Survey Policies

	Share WTP > 0
$\text{WTP}^{\text{inc-fine}}$	0.768
$\text{WTP}^{\text{inc-transfer}}$	0.678
WTP^{net}	0.558
$\Delta(\text{WTP}^{\text{inc-fine}}, \text{WTP}^{\text{net}})$	0.209*** (0.01)
N	2727

Share of the survey sample (see section 7.3) with positive willingness-to-pay for income-based fines (row 1) and income-based transfers (row 2). Row 3 displays the share of respondents with positive net-of-redistribution willingness-to-pay for income-based fines; see section 7.4 for details on this willingness-to-pay measure. Standard errors are in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

WTP (€36 million) is still substantial. Together with our extensive-margin findings in table 7.1, these results indicate substantial fairness-driven support for income-based fines on-average; furthermore, they provide reassurance that elicited WTPs are not pure noise.⁶²

Table 7.3 explores how WTP responds to randomized fine-policy characteristics using equation 7.1. The first row shows per-capita WTP for the omitted proportional-and-equal fine policy, rows 2-4 show the effects of randomized behavior characteristics, and rows 5-6 show the effects of randomized fine-schedule steepness. We begin with column 1, which presents OLS estimates using $\text{WTP}^{\text{inc-fine}}$ —*i.e.*, WTP prior to accounting for redistribution—as the outcome. We see a very high per-capita WTP (€88) for the proportional-and-equal policy in row 1; however, responses to randomized behavior and fine-schedule characteristics are insignificant and small relative to WTP for the omitted policy. Column 2 presents OLS estimates using our primary outcome measure, net-of-redistribution WTP; while net WTP for the omitted policy is lower than gross WTP in column 1, the responses to randomized

⁶²In particular, respondents can rank policies using our WTP measure.

Table 7.2: Mean & Median WTP for Hypothetical Survey Policies

	Per-Capita	Aggregate
Mean		
Income-Based Fines	€75.25	€339 million
Income-Based Transfers	€43.27	€195 million
Net WTP	€31.98	€144 million
Median		
Net WTP	€8	€36 million
<i>N</i>	2727	

Mean willingness-to-pay for income-based fines (row 1) and income-based transfers (row 2) using the survey sample described in section 7.3; row 3 displays the average difference between these measures and row 4 displays the median difference.

fine-policy characteristics are largely unchanged. In order to examine whether the top-coding of slider values drives our results, column 3 presents OLS estimates using an alternative net WTP measure that assigns larger WTP values to respondents who max out the slider.⁶³ However, the results using this alternative measure are nearly unchanged relative to column 2. Column 4 presents estimates from a quantile (median) regression with net WTP as the outcome and finds qualitatively similar results. In Appendix table 9.18, we replicate the same pattern of findings using extensive-margin outcomes.

In summary, respondents value income-based fines but are insensitive to both the distribution of behavior they induce and the steepness of the fine schedule. While noisy, our findings provide no evidence in favor of either the equal-compliance or proportional-punishment rationales. We now probe the heterogeneity of these baseline results.

7.5.2 Heterogeneity

Table 7.4 presents OLS estimates of equation 7.1 separately for different randomized characteristics: the average fine level, the size of behavior change

⁶³In our baseline net WTP measure, we assign the maximum value, €275, as the WTP for those who max out the slider. In our alternative measure, we assign a value of €325.

Table 7.3: Effects of Fine-Policy Characteristics on Hypothetical WTP

	Per-Capita WTP for Income-Based Fines			
	(1)	(2)	(3)	(4)
	Gross	Net	Net (alt.)	Net
Proportional Fines, Equal Speeding, $\Delta s = 20$	87.56*** (9.557)	45.74*** (11.36)	47.09*** (11.99)	16* (6.337)
Behavior				
Inequality ($s_{\text{low}}^{\text{inc}} < s_{\text{high}}^{\text{inc}}$)	7.483 (6.104)	3.473 (7.256)	2.906 (7.659)	2 (4.048)
Inequality ($s_{\text{low}}^{\text{inc}} > s_{\text{high}}^{\text{inc}}$)	-7.899 (6.109)	-7.090 (7.262)	-6.567 (7.666)	-4 (4.051)
Behavior Change Δs	-4.823 (4.951)	-8.496 (5.886)	-8.618 (6.213)	-6 (3.283)
Fine Schedule				
Below-Proportional ($b < b_{\text{prop}}$)	-6.191 (6.044)	5.022 (7.185)	5.961 (7.584)	4 (4.008)
Above-Proportional ($b > b_{\text{prop}}$)	-8.429 (6.137)	-4.641 (7.295)	-5.081 (7.700)	2 (4.069)
N	2727	2727	2727	2727
Model	OLS	OLS	OLS	Quantile

Estimates of equation 7.1 using the survey sample described in section 7.3. Standard errors are in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Δs , and the question order. Throughout, we focus our attention on the effects of randomized behavior and fine-schedule characteristics on net WTP. Beginning with the fine level (columns 1 and 2), we see relatively precise responses to randomized behavior characteristics when the average fine is €200. However, the signs for these effects are generally the *opposite* of those predicted by the equal-compliance rationale: respondents prefer income-based policies with $s_{\text{low}}^{\text{inc}} < s_{\text{high}}^{\text{inc}}$ to policies with equal speeding rates (row 2, column 1) and prefer to *increase* behavioral inequality under the fixed-fine policy (row 4, column 1).⁶⁴ Furthermore, despite some effects that emerge in column 1, only

⁶⁴We also see in column 1 that respondents dislike above-proportional fines when the average fine is €200. However, this result may be driven by the small value for the low-income fine (€35) under such a policy.

one of the coefficient differences between columns 1 and 2 is significant at the 5% level. In columns 3 and 4, we examine results conditional on different levels of behavior change and find neither significant effects nor discernible differences across the two columns. In columns 5 and 6, we examine the role of question order to test how salience and/or learning effects might influence our estimates; within this comparison, we again find no significant differences.

Table 7.4: Heterogeneous Effects of Fine-Policy Characteristics on Hypothetical WTP (Randomized Features)

	Per-Capita Net WTP for Income-Based Fines					
	Fine Level		Behavior Change Δs		Question Order	
	(1) €200	(2) €330	(3) 20	(4) 30	(5) Transfer Q 1st	(6) Fine Q 1st
Prop. Fines, Equal Speeding	59.92*** (15.81)	29.57 (16.26)	43.05*** (9.951)	23.46* (9.220)	44.54** (15.07)	46.70** (16.83)
Behavior						
Inequality ($s_{low}^{inc} < s_{high}^{inc}$)	16.12 (10.04)	-8.938 (10.43)	1.682 (10.41)	4.707 (10.15)	-5.135 (9.520)	10.59 (10.87)
Inequality ($s_{low}^{inc} > s_{high}^{inc}$)	-4.643 (9.916)	-8.067 (10.59)	-16.02 (10.49)	1.623 (10.09)	-17.13 (9.649)	1.208 (10.75)
Behavior Change Δs	-18.96* (8.055)	3.472 (8.566)			-5.181 (7.758)	-11.21 (8.770)
Fine Schedule						
Below-Proportional ($b < b_{prop}$)	-1.513 (9.863)	10.20 (10.41)	1.168 (10.26)	9.167 (10.07)	19.85* (9.517)	-7.014 (10.68)
Above-Prop. ($b > b_{prop}$)	-22.85* (9.937)	14.31 (10.64)	-6.743 (10.47)	-2.981 (10.19)	2.912 (9.494)	-11.72 (11.00)
<i>N</i>	1358	1369	1356	1371	1313	1414

Estimates of equation 7.1 using the survey sample described in section 7.3. Standard errors are in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Overall, across the 28 behavior and fine-schedule coefficients in table 7.4, only 3 are significant at the 5% level; furthermore, only one of the 17 across-group differences is significant at the 5% level.⁶⁵ In Appendix table 9.19,

Note that we do not see any response to above-proportional fine schedules with when the average fine level is €330, in which case the low-income fine is €60.

⁶⁵This includes across-group differences in net WTP for the proportional-and-equal policy.

we explore heterogeneity with respect to both background characteristics— income, age, and education—and respondents’ beliefs about others’ driving behavior (see Appendix section 9.2.8 for details). Overall, we find remarkably little heterogeneity across sub-groups: net WTP for the proportional-and-equal policy is consistently large and responses to randomized features are not significantly different across sub-groups.

7.5.3 Implied Fine-Gradient

We now translate our estimates into implied fairness-driven fine gradients, which can be used to assess whether our estimates are sufficiently small and precise to reject fairness-driven rationales for income-based fines. Our procedure involves two steps. First, we use our survey estimates to recover the fairness parameters β_s and β_τ that enter the expression for the optimal fine gradient (equation 3.2). Second, while β_s directly enters the expression for the optimal gradient, β_τ must be transformed into the appropriate units.⁶⁶ To accomplish the latter, we leverage the empirical moments from section 6.4 that we used to derive the optimal redistributive fine gradient.⁶⁷

Starting with β_s , note that, intuitively, β_s captures the relative value of deterring high-income speeding: if society has a stronger preference for deterrence at high incomes than low incomes, β_s and the implied fine gradient are both positive. In our survey, this preference would appear as a higher WTP for policies that lower the speeding rate for the high-income group relative to the low-income group. In our main results from column 2 of table 7.3, we actually find that preferences for lowering the high-income speeding rate are uniformly *negative*.⁶⁸ Therefore, to maximize power, we combine the three sources of behavioral variation to recover an estimate of β_s .⁶⁹

⁶⁶In particular, welfare effects arising from behavioral fairness are proportional to behavioral responses, whereas welfare effects arising from fine fairness are proportional to fine changes. The latter must therefore be normalized by the fine elasticity; see equation 3.2.

⁶⁷In particular, we use the general expression for the optimal fine gradient (Appendix equation 9.3), the average fine elasticity from Kaila (2024), and the distributional statistics from panel c of Appendix table 9.14.

⁶⁸This is immediate for rows 3 and 4 of table 7.3. The coefficient in row 2 measures the effect of increasing the high-income speeding rate and therefore needs to be multiplied by -1 .

⁶⁹In particular, our independent variable becomes $\underbrace{s_{\text{low}}^{\text{inc}} - s_{\text{low}}^{\text{fixed}}}_{\text{Behavior change}} + \underbrace{s_{\text{low}}^{\text{inc}} - 0.06}_{\text{Behavior distribution}}$, which captures both dimensions (behavior change and induced distribution) that we use to increase the speeding rate of the low-income group relative to the high-income group. To properly scale this parameter, we use the identification results in Appendix table 9.15.

By contrast, the parameter β_τ captures the relative value of increasing fines for the high-income group. In our survey, a positive β_τ appears as a stronger preference for policies that create a steeper fine schedule. Because the effect of steeper schedules in column 2 of table 7.3 is also uniformly negative, we take an analogous approach to identify β_τ .

Table 7.5 shows results from regressing respondents' net WTP on the unit-adjusted behavior and fine-schedule variation in our survey; the resulting coefficients yield estimates of implied fine-schedule gradients. Column 1 presents OLS results, while column 2 uses a quantile (median) regression.⁷⁰ We note five main findings. First, consistent with our baseline findings in table 7.3, the implied slopes are negative: respondents express a slight distaste for policies with steeper schedules and lower speeding rates among the high-income group. Second, the implied slopes are extremely large; this results because respondents are willing to sacrifice several euros *per-capita* for policies that ultimately affect a small share of the population.⁷¹ Third, after combining the various behavioral effects, we have enough power to reject behavior-related motives for income-based fines, though we caution that the combination of these coefficients was not pre-registered. Fourth, even after assuming a constant fine-schedule effect, we cannot reject a schedule-motivated fairness rationale: our standard errors are such that Finland's day-fine gradient (€88, see Appendix figure 9.3) falls within our confidence interval. Fifth, when we combine both fairness rationales to estimate a total implied gradient (last row of table 7.5), the day-fine gradient of €88 lies just outside our 95% confidence interval (though we cannot reject the overall fine gradient of €12.4).

In summary, while our results tentatively reject a behavioral-fairness rationale, our survey findings are noisy. Therefore, our read is that the survey evidence overall is inconsistent with either behavior- or schedule-based fairness motives, though we cannot reject them outright.

⁷⁰To ensure that policy comparisons do not occur across average fine levels, we include an indicator for the fine level in our regressions. If we did not do this, we would confound changes in the fine schedule with comparisons across different fine levels.

⁷¹As is well-known, hypothetical WTP elicitation may be subject to inflation bias. While our differencing approach deals with some of this, some residual likely remains. E.g., if a respondent likes the income-dependent transfers half as much as the income-dependent fine policy, then inflation will persist if the base elicitation (how much they like the fine policy) is inflated.

Table 7.5: Implied Fairness-Driven Fine Gradients

	Optimal Fine Gradient b^* (per €10,000 of income)	
	(1)	(2)
Behavioral Fairness	-331.9* (168.0)	-222.2* (98.11)
Fine Fairness	-268.8 (307.5)	0 (179.6)
N	2727	2727
Fine-Level Control	X	X
Estimation	OLS	Quantile
Total Gradient	-600.7 (350.2)	-222.2 (204.5)

Estimates of the optimal fairness-driven fine gradients (equation 9.3) implied by our survey results (table 7.3); see section 7.5.3 for details on the construction of these estimates. Standard errors are in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

7.6 Robustness

Safety Concerns While we emphasize to respondents that total speeding behavior is constant in the fine-policy comparison, respondents might nonetheless believe that road safety differs between the two policies.⁷² To address this potential confound, we draw upon free-text responses use to explain why they made their fine-policy choice.⁷³ In particular, we tag all responses that include one of the following strings: {safe, danger, accident}. In total, only 113 responses (4%) include one of these strings; furthermore, average net WTP among this group is €28, not meaningfully different from all other responses (€32). In unreported results, we re-estimate equation 7.1 after dropping this group and find little difference in our coefficients. This evidence suggests that safety-related confounds do not drive our findings.⁷⁴

⁷²For example, they may believe that a particular groups' speeding behavior is more dangerous (e.g. leads to more accidents).

⁷³*E.g.* someone who chooses the income-based fine policy sees the following prompt: “Please use the text box below to describe the factors that influenced your decision to choose Income-based fines over Fixed fines.”

⁷⁴For an expanded notion of safety (which may also include fairness-motivated rationales), we additionally include responses with the string “deter;” this increases the number of safety-related responses to 230, nearly 10% of our sample. Again, this group has similar net WTP to the rest of the sample, and re-estimating

8 Discussion and Conclusion

In this paper, we evaluate rationales for a controversial policy: income-based fines. Using the Finnish speeding fine system as our laboratory, we first consider the Beckerian motive: does the marginal social cost of speeding scale with the income of the offender? Drawing on linked data from accident reports, income tax returns, and crime histories, we exploit cross-sectional variation in speeding histories conditional on income, together with a job-loss design, to recover the income-gradient of marginal social speeding costs. We find that this gradient is *negative*, leading us to reject this rationale.

We then evaluate a standard motivation in the public finance literature: in a Mirrleesian redistributive framework, fines can be exploited to lower the distortionary costs of redistribution if speeding is an informative signal for latent earnings ability. As clarified by recent work in optimal commodity taxation, this signal can be identified by differencing across-income variation in speeding rates with respect to causal earnings effects on speeding. To identify these causal earnings effects, we leverage two complementary strategies: within-individual earnings variation and differences in the timing and size of inheritance shocks. We find that the estimated earnings ability signal is *negatively* correlated with income, implying that Mirrleesian redistributive rationales can only justify a *lower* speeding fine for the rich.

Finally, we consider two fairness rationales alluded to in theoretical and statutory motivations for the policy: equal compliance across incomes and proportional punishment. To evaluate these motives, we use data from an original survey in which Finnish residents make trade-offs between fixed- and income-based fine policies, where we randomly vary the latter with respect to both schedule “steepness” and the induced across-income distribution of speeding rates. We find that income-based fines are popular: net of redistribution, respondents are willing to forgo €144 million in government revenue in order to implement an income-based speeding fine policy. However, these valuations are insensitive to both the distribution of behavior induced by fines and the steepness of the fine schedule itself. Therefore, our evidence is inconsistent with the equal-compliance and proportional-punishment rationales.

Overall, our evidence suggests that oft-cited rationales for income-based fines

equation 7.1 without these responses makes little difference.

are insufficient to justify their popularity. Furthermore, we explicitly reject two natural economic objectives for such policies: externality mitigation and redistribution. Therefore, accounting for the popularity of income-based fines requires an appeal to either non-standard economic motives (e.g. income-correlated mis-perceptions) or fairness preferences that are not tied to salient observable features of such policies.

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9 Appendix

9.1 Expressions for Fines and Taxes

Given $\tau(z) = a + b * z$, the optimal marginal income tax rate satisfies the following at each earnings level z^0 :

$$T'(z^0) + b * s(z^0) + [\tau(z^0) - \bar{e}'(z^0)]s'_{inc}(z^0) = \frac{\int_{z \geq z^0} (\hat{g}(z) - 1) dH(z)}{h(z^0)z'_{T_z}(z^0)} \quad (9.1)$$

where $z'_{T_z}(z^0)$ is the (semi-)elasticity of labor supply with respect to the marginal income tax rate and $h(\cdot)$ is the earnings density function. Under an optimal income-tax schedule, if assumption 3.1 holds, then the fixed component of fines satisfies the following for any given b :

$$a^*(b) = \frac{\mathbb{E}[s'_{\tau_s}(z) * \bar{e}'(z)] + \text{Cov}[s'(z) - s'_{inc}(z), \hat{g}(z)] - b * \mathbb{E}[s'_{\tau_s}(z) * z]}{\bar{s}'_{\tau_s}} \quad (9.2)$$

We now write the general expression for the optimal fine gradient that under a constant fine elasticity ζ_τ (to replace the semi-elasticities in our previous equations). To reflect our empirical approaches, we make the following assumption:

9.1. *Externality $\bar{e}(z)$, behavioral fairness $\bar{e}_s(z)$, and fine fairness $\bar{e}_\tau(z)$ are all composed of intercept and slope terms:*

$$\begin{aligned} \bar{e}(z) &= \alpha_e + \beta_e * z \\ \bar{e}_s(z) &= \alpha_s + \beta_s * z \\ \bar{e}_\tau(z) &= \alpha_\tau + \beta_\tau * z \end{aligned}$$

Furthermore, either $\alpha_\tau \geq 0$ or there exists some earnings level z_0 such that $\bar{e}_\tau(z_0) = 0$.

The last part of assumption 9.1 guarantees that β_τ determines the sign that fine-fairness contributes to the optimal fine gradient by ensuring that the intercept α_τ is not too negative.

Then, if the income tax schedule $T(z)$ satisfies equation 9.1, the fixed-fine component $a^*(b)$ satisfies equation 9.2, and the fine elasticity ζ_τ is constant, then assumption 9.1 implies that the optimal fine gradient satisfies:

$$b^* = \beta_e + \beta_s + \frac{-\beta_\tau \int_{\mathcal{Z}} z * (z - \bar{z}^{rw}) dH(z) - \alpha_\tau (\bar{z} - \bar{z}^{rw}) + \text{Cov} \left[\hat{g}(z), \int_0^z (y - \bar{z}^{rw}) s'_{het}(y) dy \right]}{\zeta_\tau \int_{\mathcal{Z}} z * (z - \bar{z}^{rw}) \frac{s(z)}{\tau(z)} dH(z)} \quad (9.3)$$

where $\bar{z}^{rw} := \frac{\mathbb{E}[\frac{s(z)}{\tau(z)} * z]}{s/\tau}$ is response-weighted average income.

9.2 Empirical Appendix

9.2.1 Imputing Driving Distance Using Odometer Data

We start with a quarterly panel of vehicles from the vehicle registration data and keep only quarters in which we observe an odometer reading for a vehicle. We then group our data into vehicle-by-primary-user cells. Each of these cells constitutes a panel where we observe odometer readings over time for each vehicle-user. Between any two successive odometer readings for a vehicle-user cell, we calculate distance driven between inspections. We then uniformly assign this distance across quarters between the two inspection dates, assuming that inspections occur at the beginning of each quarter.⁷⁵ Aggregating across vehicles and quarters, we obtain a user-by-year panel of driving distance. Driving distance is non-missing for a given user-year (i, t) in our panel if and only if at least one quarter in year t falls between two inspections for a given vehicle registered to user i . Note that, if a user switches from an old vehicle h to a new vehicle h' , we will not be able to calculate distance driven between the last inspection date for the old vehicle h and the first inspection date for the new vehicle h' .

9.2.2 Procedure for Calculating Money-Metric Cost of Injuries and Deaths from Accidents

We proceed as follows

1. For each driver j involved in an accident a_t in year t , we identify the set of externality-relevant accident victims $\{v_1(j, a_t), v_2(j, a_t), \dots, v_n(j, a_t)\}$.

⁷⁵Assuming the inspections occur at the end of the quarter makes no difference for our results.

2. For each victim $v_m(j, a_t)$ we randomly select two “control” individuals, $c_1(v_m)$ and $c_2(v_m)$, from the FOLK population register who have the same birth year, sex, and education level as v_m in year $t - 1$
3. We compute disposable income differences for each victim-year

$$\{\Delta\hat{y}_{v_m,t-1}, \Delta\hat{y}_{v_m,t}, \dots, \Delta\hat{y}_{v_m,2023}\}$$

until 2023, where

$$\Delta\hat{y}_{v_m,t+r} := y_{v_m,t+r} - [(y_{c_1(v_m),t+r} + y_{c_2(v_m),t+r})/2]$$

for all $r \in \{-1, 0, 1, \dots, 2023 - t\}$

4. Similarly, we compute differences in net transfers received for each victim-year

$$\{\Delta\hat{T}_{v_m,t-1}, \Delta\hat{T}_{v_m,t}, \dots, \Delta\hat{T}_{v_m,2023}\}$$

until 2023, where $\Delta\hat{T}_{v_m,t+r} := T_{v_m,t+r} - [(T_{c_1(v_m),t+r} + T_{c_2(v_m),t+r})/2]$ for all $r \in \{-1, 0, 1, \dots, 2023 - t\}$

5. We calculate victim-specific DID net-income losses in each event year $r \geq 0$ as $\Delta\hat{y}_{v_m,t+r}^{DID} := \Delta\hat{y}_{v_m,t+r} - \Delta\hat{y}_{v_m,t-1}$. We analogously calculate victim-specific DID for net transfer received, $\Delta\hat{T}_{v_m,t+r}^{DID}$
6. We calculate a welfare-weighted, NPV cost for each victim v_m as

$$M_{v_m} := \sum_{k=0}^{2023-t} \delta^k \left[\underbrace{\Delta\hat{T}_{v_m,t+k}^{DID}}_{\text{Transfers Paid by Govt.}} - \underbrace{g(y_{v_m,t-1}) * \Delta\hat{y}_{v_m,t+k}^{DID}}_{\text{Welfare-weighted income gain}} \right]$$

under some exponential discount rate δ and $g(\cdot)$ denotes a set of social marginal welfare weights

7. The total welfare-weighted external income loss imposed by driver j in accident a_t is $M_j^{ext}(a_t) := \sum_{m=1}^n M_{v_m}$

9.2.3 Bias in Income Effects Estimated Using Inheritance Shocks

Let α be the death effect (assumed homogeneous), η_q be the income effect for quintile q , and ΔW_q be the size of the total inheritance shock received by

quintile q . Then, for a given q , the total effect of parental death is $\alpha + \eta_q * \Delta W_q$. Differencing effects across two quintiles q and q' yields $\eta_q * \Delta W_q - \eta_{q'} * \Delta W_{q'} = (\eta_q - \eta_{q'}) * \Delta W_q + \eta_{q'} (\Delta W_q - \Delta W_{q'})$. Dividing by $\Delta W_q - \Delta W_{q'}$ (*i.e.* the first-stage) yields $\eta_{q'} + (\eta_q - \eta_{q'}) * \frac{1}{1 - \Delta W_{q'} / \Delta W_q}$, which equals η_q if $\Delta W_{q'} = 0$, $\eta_{q'} = \eta_q$, or $\eta_{q'} = 0$. Furthermore, as $\Delta W_{q'} / \Delta W_q \rightarrow 0$, this expression converges to η_q .

9.2.4 Triple-Difference Equations for Estimating Income Effects

Observations are indexed by an individual i , parental-death treatment cohort y , and event time t . We specify the following system:

$$\begin{aligned} \Delta W_{i,y,t} = & \theta_0 + \theta_1 * I_{t \geq 0} + \theta_2 * I_{YOD(i)=y} + \theta_3 * \text{Top}(i) + \theta_4 * I_{t \geq 0} * I_{YOD(i)=y} \\ & + \theta_5 * I_{t \geq 0} * \text{Top}(i) + \theta_6 * I_{YOD(i)=y} * \text{Top}(i) \\ & + \theta * I_{t \geq 0} * I_{YOD(i)=y} * \text{Top}(i) + \epsilon_{i,y,t} \end{aligned} \quad (9.4)$$

$$\begin{aligned} s_{i,y,t} = & \beta_0 + \beta_1 * I_{t \geq 0} + \beta_2 * I_{YOD(i)=y} + \beta_3 * \text{Top}(i) + \beta_4 * I_{t \geq 0} * I_{YOD(i)=y} \\ & + \beta_5 * I_{t \geq 0} * \text{Top}(i) + \beta_6 * I_{YOD(i)=y} * \text{Top}(i) \\ & + \beta * I_{t \geq 0} * I_{YOD(i)=y} * \text{Top}(i) + \nu_{i,y,t} \end{aligned} \quad (9.5)$$

where $\Delta W_{i,y,t}$ is the wealth shock for individual i in year $y + t$, $\text{Top}(i)$ is an indicator for whether i is in the top inheritance group (quintiles 3-5), and $I_{t \geq 0}$ is a post-event indicator. We estimate the income effect $\hat{\beta} / \hat{\theta}$ via two-stage least-squares.

9.2.5 Comparability of Parental Death Effects

One reason why death effects may differ across inheritance groups is that the likelihood of a transport-related cause-of-death (e.g. a car crash) is systematically correlated with inheritance size: heirs whose parent dies in a transport-related accident may exhibit differential speeding responses to parental death, potentially leading to bias in our DDD-IV approach. Indeed, using the cause-of-death register, we find in unreported results that parents of heirs in the top inheritance-size group are more likely to die from a transport-related accident.⁷⁶ However, in our inheritance sample transport-

⁷⁶Transport-related accidents correspond to Chapter V deaths in the cause-of-death code.

related accidents are quite rare, constituting no more than 0.6% of any given death cohort’s causes-of-death. Therefore, when we re-estimate the income effect after dropping all heirs whose parent dies in a transport-related accident (Appendix table 9.9), our estimate is unchanged relative to our baseline.

Another potential way in which this assumption could be violated is that different inheritance-size groups have different speeding preferences. Indeed, column 1 of Appendix table 9.10 shows that the bottom inheritance group has a speeding rate that is 0.342 percentage points larger than the top group at event time $t = -4$. While this difference is small relative to baseline speeding rates (slightly above 7%), we consider an alternative to our baseline assumption that death effects are constant in levels: we assume that death affects are a constant proportion of baseline speeding rates. In particular, we measure speeding effects for each inheritance-size group relative to its baseline speeding rate; we then difference the relative effects across inheritance-size groups and calculate the implied level effect from this difference.⁷⁷ Appendix figure 9.15 compares these adjusted effects to the baseline event study effects from figure 6.3. The effects are almost perfectly aligned, suggesting that preference differences across inheritance groups are unlikely to drive our results.

Another reason that death effects may not be comparable across inheritance size is because the amount of effort involved in disbursing the estate varies across inheritance-size groups in a way that systematically affects the likelihood of speeding. For example, larger inheritances may be more complex and therefore require more travel to-and-from the parent’s residence. To evaluate the role of inheritance complexity, we examine two features of the estate: (1) the number of (potential) child heirs and (2) the number of tax revisions we observe for an heir’s inheritance. Appendix figure 9.16 shows that inheritance size is negatively correlated with the number of potential child heirs (panel 9.16a) and positively correlated with the number of tax revisions (panel 9.16b). To examine the relevance of these differences, Appendix table 9.11 shows income-effect estimates from our DDD-IV model when we include “placebo” controls for the number of child heirs (column 1) or the number of tax revisions (column 2);⁷⁸ our income effect estimates are virtually un-

⁷⁷The level effect is obtained by multiplying the differenced relative effect by the baseline speeding rate for the top group; this is the group for which we can measure income effects.

⁷⁸In particular, this set of controls consists of all possible interactions between the estate complexity measure, the treatment indicator $I_{YOD(i)=y}$, and the post-indicator $I_{t \geq 0}$. In other words, we add a set of controls that substitutes the top-inheritance group indicator for the estate complexity variable, allowing us

changed, even increasing slightly after controlling for the number of child heirs. We therefore do not view estate complexity as a meaningful confound in our estimates.

9.2.6 Construction of Survey Transfer Policies

Let $\{\bar{s}_{below}^{inc}, \bar{s}_{above}^{inc}\}$ be speeding rates under the income-dependent fine policy and $d\tau = \tau_{above}^{inc} - \tau_{above}^{fixed}$ be the fine difference for the high-income group. We ask individuals to compare a fixed transfer policy that gives 25 € to all Finnish adults with an income-dependent transfer that allocates $25 + d\tau * s_{below}^{inc}$ to the low-income group and $25 - d\tau * s_{above}^{inc}$ to the high-income group. We use the speeding rates under the income-dependent policy, rather than those under the fixed-fine policy, since this choice guarantees that revenue effects are the same for income-dependent transfer and fine policies. To see this, note that the revenue effect of the income-dependent fine policy is given by:

$$\Delta R_{fine} = 0.5[s_{below}^{inc} * \tau_{below}^{inc} + s_{above}^{inc} * \tau_{above}^{inc}] - 0.5 * \tau^{fixed} * [s_{below}^{fixed} + s_{above}^{fixed}]$$

Since total speeding is the same under both policies, we can replace $s_{below}^{fixed} + s_{above}^{fixed}$ with $s_{below}^{inc} + s_{above}^{inc}$ and it immediately follows that:

$$\Delta R_{fine} = 0.5 * d\tau * [s_{above}^{inc} - s_{below}^{inc}]$$

Thus, increasing the low-income transfer by $d\tau * s_{below}^{inc}$ (*i.e.* spending an additional $d\tau * s_{below}^{inc}$ per low-income person) and lowering the transfer by $d\tau * s_{above}^{inc}$ for the high-income group yields an equivalent revenue effect.

In general, our method for constructing the income-dependent transfer will tend to inflate the role of redistribution when (1) “altruism” weights α on each group are positive and (2) $\alpha_{below} > \alpha_{above}$. In particular, for baseline speeding rates $\{s_{below}, s_{above}\}$, the welfare effect of transfers $\{d\tau * s_{below}, -d\tau * s_{above}\}$ is given by:

$$dW = d\tau * \alpha_{below} * (s_{below} - \frac{\alpha_{above}}{\alpha_{below}} * s_{above})$$

With $\alpha_{above} > 0$ and $\alpha_{above} < \alpha_{below}$, this value will be maximized when s_{below} is large and s_{above} is small. Since the income-dependent policies we consider increase the speeding rate for the low-income and decrease it for the

to test whether estate complexity is the true underlying treatment dimension that explains our results.

high-income group, dW will be larger if we use the speeding rates under the income-dependent policy to construct the income-dependent transfer, making our estimates for the value of income-dependent fines more conservative. This result also holds if $\alpha_{above} < 0$ with $\alpha_{below} > |\alpha_{above}| > 0$.

9.2.7 Choice of Fine Policy Parameters

Average Incomes: We set the above- and below-median incomes (€15,000 and €51,000 respectively) using the 2023 distribution of earned income; we lower the income of the low-income group relative to these calculations in order to make proportional fines more apparent in the income-based policy.

$\tau^{\text{fixed}} = \mathbf{330}$: We calculate the higher fixed-fine of €330 as the average of the proportional fines €150 and €510; these proportional fines, which are 1% of average income for each group, are reflective of the degree of income-dependence we estimate on-average within the day-fine region of the Finnish fine schedule (see figure 9.3).

$\tau^{\text{fixed}} = \mathbf{200}$: The fixed-fine of €200 is close to the average speeding fine received in Finland (around 215 €). We calculate proportional income-based fines relative to this average fine level using a slightly lower proportionality rate of 0.6%, which guarantees that the fine difference between the fixed and income-based policies is the same in relative terms for both levels of the average fine. In particular, $(200-90)/200 \approx (330-150)/330$.

$d\tau$: We choose the size of the fine perturbations $d\tau$ (€90 for $\tau^{\text{fixed}} = 330$ and €55 for $\tau^{\text{fixed}} = 200$) around the proportional policy to be roughly equivalent relative to the level of the fixed fine.

Speeding Rate: We choose the average speeding rate to reflect the average in Finland in 2019, our last complete year of data.

9.2.8 Survey Belief Elicitations

Fine Awareness We ask respondents to indicate how often (from never to always) they believe “drivers are aware of the fine they would receive if they were caught speeding.” In Appendix table 9.19, we separate respondents into those who answer “rarely” or “never” (column 3) and those who answer at least as often as “sometimes” (column 4); see Appendix figure 9.26 for the

distribution of responses.

Fine Responsiveness We ask respondents to indicate how much they agree with the following statement:

“Drivers will speed less if speeding fines are increased.”

In Appendix table 9.19, we group respondents into {“strongly disagree,” “disagree,” “neither agree nor disagree,” “agree,” “strongly agree”} (column 5) and {“agree,” “strongly agree”} (column 6); the distribution of responses is shown in Appendix figure 9.27.

9.2.9 Counterfactual Analysis

When the fixed component of the fine schedule $a^*(b)$ is optimal and the income tax $T(\cdot)$ is optimal, then a small increase in the fine slope b yields a welfare effect (ignoring fairness considerations) given by:

$$dW = (b - \beta^M) * db * \int_Z s'_{\tau_s}(z) * z * (z - \bar{z}^{rw}) dH(z) \\ - db * \text{Cov}[\hat{g}(z), \int_{z_{min}}^z (y - \bar{z}^{rw})(s'(y) - s'_{inc}(y))]]$$

where β^M is the income-gradient of the marginal social cost of speeding (assumed constant). To calculate the revenue, efficiency, and redistributive consequences of a change in the slope from b to b^{alt} , we integrate the expression above.

9.3 Supplemental Tables

Table 9.1: Income-Gradient of Excess Speed

	Excess Speed				
	(1)	(2)	(3)	(4)	(5)
Earned Income (€10,000s)	-0.342*** (0.00358)	-0.0175*** (0.00371)	-0.0436*** (0.00366)	-0.00708** (0.00224)	-0.00959** (0.00348)
Constant	15.66*** (0.0164)	14.46*** (0.0166)	14.56*** (0.0163)	11.23*** (0.0103)	14.43*** (0.0155)
<i>N</i>	753881	753877	753876	603762	753876
Age FE		X			
Age-Year FE			X	X	X
Camera Only				X	
Municipality FE					X

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9.2: Summary Statistics for Collapsed Accident Panel

	Mean (SD)
Earned Income (10,000s €)	2.576 (1.788)
Car Ownership Rate	0.508 (0.446)
Speeding Rate	0.0573 (0.104)
Ever Receive Speeding Ticket (%)	35.12 (47.73)
Other Traffic Crime Rate	0.0238 (0.0657)
Ever Receive Other Traffic Ticket (%)	18.17 (38.56)
Ever in Accident (%)	7.965 (27.08)
Ever in Injurious Accident (%)	1.765 (13.17)
Ever Injured in Accident (%)	0.798 (8.898)
<i>N</i>	5033598

Table 9.3: Marginal Speeding Gradient (Other-Person Injuries)

	(1)	(2)	(3)	(4)
	Other-Person Injuries (per 1,000 drivers)			
Speeding Rate (MSC Intercept)	10.70*** (0.198)	10.58*** (0.199)	7.581*** (0.251)	1.498*** (0.254)
Speeding X Earnings (MSC Gradient)	-1.103*** (0.0445)	-1.105*** (0.0445)	-0.637*** (0.0578)	-0.253*** (0.0593)
Earned Income (10,000s €)	0.145*** (0.00479)	0.157*** (0.00491)	0.0850*** (0.00787)	0.176*** (0.0289)
Constant	0.338*** (0.0128)	0.316*** (0.0129)	0.833*** (0.0261)	-0.124 (0.100)
<i>N</i>	5033597	5033597	2223285	2223285
Cohort FE	X	X	X	X
Reverse-Causality Controls	X	X	X	X
Municipality FE		X		
Non-Missing Distance			X	X
Car Ownership & Traffic Crime Controls				X
Annual Driving Distance				X

*Heteroskedasticity-robust standard errors in parentheses. Car ownership rates, non-speeding crime rates, and driving distance are also interacted with earnings when included. Each individual is weighted by the number of years they appear in the population register. In column 5, this weight is multiplied by the number of years for which driving distance information is available from 2013-2018. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$*

Table 9.4: Conditional Marginal Speeding Gradient

	(1)	(2)
	Other-Party Injuries (per 1,000 accidents)	
Speeding Rate (MSC Intercept)	18.64 (9.811)	2.581 (8.853)
Speeding X Earnings (MSC Gradient)	-10.85*** (2.626)	-3.872 (2.370)
Pre-Period Earnings (10,000s €)	5.235** (1.965)	-0.471 (1.773)
Other-Party Participants		153.7*** (0.574)
Constant	129.9*** (6.855)	-7.789 (6.207)
<i>N</i>	315026	315026
Age FE	X	X
Year FE	X	X
Reverse Causality	X	X
Car Ownership & Other Traffic Crime	X	X
Vehicle Mass	X	X

Sample consists of the first accident in which individuals from our cross-sectional sample (described in section 5.1) are involved as the driver of a motor vehicle (car, truck, bus, moped, motorbike, motorcycle, or van) from 2006-2018. Earnings are measured in the year prior to the accident. Vehicle mass is measured as the average mass of vehicles registered to a given individual from 2013-2018. “Reverse Causality Controls” refers to a control for the total number of accidents in which an individual is involved from 2006-2018. For descriptions of the remaining variables, see section 5.1. Car ownership rates, non-speeding crime rates, driving distance, and vehicle mass controls include both a baseline effect and an interaction of these variables with earnings. Each individual is weighted by the number of years they appear in the population register from 2006-2018, multiplied by the number of years for which vehicle mass information is available from 2013-2018. Standard errors are in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9.5: Cumulative Social Costs of Accidents

	(1)	(2)
	Non-Fatal Injuries	Fatal Injuries
# of Post-periods	12.98	12.91
Age at accident	44.43	50.92
Cumulative Fiscal Cost	€13,250	-€41,957
Cumulative Net Inc. Loss	€8,791	€244,961
$\delta = 0.03$		
Discounted Fiscal Cost	€10,564	-€35,261
Discounted Net Inc. Loss	€6,473	€199,547
$\delta = 0.05$		
Discounted Fiscal Cost	€9,226	-€31,817
Discounted Net Inc. Loss	€5,358	€176,700
$\delta = 0.07$		
Discounted Fiscal Cost	€8,149	-€28,973
Discounted Net Inc. Loss	€4,486	€158,157
N	78378	3489

Table 9.6: Income Loss Gradients, Separately by ν

	Cumulative Net Income Losses			
	(1)	(2)	(3)	(4)
	$\nu = 0$	$\nu = 0.25$	$\nu = 1$	$\nu = 4$
Pre-Period Net Income	3.707*** (0.591)	2.497*** (0.230)	1.826** (0.601)	-1.176 (1.363)
Constant	-46385.8*** (9882.7)	-30074.9*** (3743.1)	-39834.9* (16552.4)	45111.5 (49712.7)
N	100761	99803	99803	99803
R^2	0.027	0.028	0.007	0.004
Age-Year FE	X	X	X	X

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9.7: Marginal Speeding Cost Gradients by Discount Rate and Inequality Aversion: Other-Person Victims

Degree of Inequality Aversion	Discount Rate		
	3%	5%	7%
$\nu = 0$	-21.45* (8.425)	-18.45* (7.302)	-15.99* (6.398)
$\nu = 0.25$	-18.06* (8.146)	-15.55* (7.079)	-13.5* (6.215)
$\nu = 1$	-9.027 (4.809)	-7.74 (4.193)	-6.703 (3.696)
$\nu = 4$	-9.064 (4.783)	-7.761 (4.169)	-6.713 (3.674)

Heteroskedasticity-robust standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9.8: Other-Party Marginal Speeding Gradients, by Discount Rate and Degree of Inequality Aversion (Alternative Dataset)

	Discount Rate		
	3%	5%	7%
Degree of Inequality Aversion			
$\nu = 0$	-5.424 (3.626)	-4.823 (3.253)	-4.321 (2.941)
$\nu = 0.25$	-5.211 (3.535)	-4.686 (3.184)	-4.218 (2.882)
$\nu = 1$	-1.605 (2.080)	-1.379 (1.882)	-1.198 (1.714)
$\nu = 4$	-1.741 (2.045)	-1.505 (1.851)	-1.318 (1.687)
N	3557565	3557565	3557565
Cohort FE	X	X	X
Car Ownership Rates	X	X	X
Non-Speeding Traffic-Crime	X	X	X

Heteroskedasticity-robust standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9.9: DDD-IV Income Effect Estimates, Excluding Transport-Related Causes-of-Death

	(1)	(2)	(3)
	Reduced-Form	First-Stage	IV
DDD Coefficient	0.311*** (0.0818)	1.424*** (0.0118)	
Inherit. Shock (10,000s €)			0.219*** (0.0575)
Constant	7.361*** (0.0348)	0.00648*** (0.000552)	7.359*** (0.0349)
<i>N</i>	10491513	10491513	10491513

Standard errors (clustered at the individual level) in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9.10: Speeding Rate Differences Across Inheritance-Size Groups

	Speeding Rate (pp) (1)
I(Bottom Group)	0.342*** (0.0851)
Constant	7.259*** (0.0575)
N	614053

Description

Table 9.11: DDD-IV Income Effect Estimates with Controls

	(1)	(2)
	Number of Heirs	Number of Revisions
Inheritance (€10,000s)	0.279*** (0.0831)	0.258** (0.0876)
Constant	6.944*** (0.0944)	7.313*** (0.0541)
<i>N</i>	4912424	4912424
DDD-IV	X	X

Table 9.12: DDD-IV Income Effect Estimates: 5-year Effect Horizon

	(1)	(2)	(3)
	Speeding Rate (pp)	Inheritance (€10,000s)	IV
DDD Coefficient	0.364*** (0.108)	1.219*** (0.0272)	
Inheritance (€10,000s)			0.299*** (0.0892)
Constant	7.358*** (0.0401)	0.00471*** (0.000674)	7.356*** (0.0402)
<i>N</i>	10179538	10179538	10179538

We use individuals whose first parent dies in $[y + 1, y + 5]$ as a control group for individuals whose first parent dies in y in order to increase power.

Table 9.13: Labor Supply Effect: Top Inheritance Group

	(1)	(2)
	Speeding Rate (pp)	
Taxable Earned Income (10,000s €)	1.165*** (0.0133)	0.474*** (0.0267)
Re-scaled Labor Supply (10,000s €)		0.281*** (0.0227)
Constant	3.285*** (0.0389)	4.426*** (0.0751)
<i>N</i>	9593075	9593075
<i>R</i> ²	0.014	0.206
Cohort-Year FE	X	X
Individual FE		X

Standard errors (clustered at the individual level) in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9.14: Redistributive Value: Sufficient Statistics

	<u>Earnings Group</u>							
	1	2	3	4	5	6	7	8
Panel (a): Earnings Effects & Cross-Sectional Variation								
<i>Inherit. Income Effect</i>	0.211 (0.0866)	0.211 (0.0866)	0.211 (0.0866)	0.211 (0.0866)	0.287 (0.105)	0.287 (0.105)	0.287 (0.105)	0.287 (0.105)
<i>FE Labor Supply Effect</i>	0.599 (0.0917)	0.281 (0.0619)	0.261 (0.0778)	0.283 (0.0852)	0.345 (0.0936)	0.277 (0.0840)	0.478 (0.0845)	0.167 (0.0576)
$s'_{inc}(z)$	0.810	0.492	0.472	0.494	0.632	0.564	0.765	0.454
$s'(z)$	0.333	0.513	1.328	2.753	1.675	1.784	0.890	0.376
Panel (b): Earnings Ability Tag								
$s'_{het}(z) := s'(z) - s'_{inc}(z)$	-0.477	0.021	0.856	2.259	1.043	1.220	0.125	-0.078
Panel (c): Additional Moments (2012)								
Avg. Earnings (€)	2350	13720	20405	25231	29943	35992	44927	72940
Avg. Disposable Income (€)	8143	14803	18600	21561	24707	28423	33764	50834
Speeding Rate (%)	4.368	4.747	5.090	5.731	7.028	8.041	9.635	12.129
Observed Fine Level (€)	173	188	196	202	208	215	226	261
\bar{z}^{rw}	€35,124							

Table 9.15: Fairness Effects of Equal-and-Opposite Fine Reforms

	$z < z_{\text{median}}$	$z > z_{\text{median}}$	Total
Fine Changes	$-d\tau$	$d\tau$	0
Fine-Fairness Effects	$-0.5(\alpha_\tau + \beta_\tau \bar{z}_{\text{low}})d\tau$	$0.5(\alpha_\tau + \beta_\tau \bar{z}_{\text{high}})d\tau$	$\beta_\tau * d\tau * 0.5 * \Delta\bar{z}$
Behavioral Responses	ds	$-ds$	0
Behavioral-Fairness Effects	$-0.5(\alpha_s + \beta_s \bar{z}_{\text{low}})ds$	$0.5(\alpha_s + \beta_s \bar{z}_{\text{high}})ds$	$\beta_s * ds * 0.5 * \Delta\bar{z}$

Table 9.16: Survey Randomization

<u>Characteristic</u>	<u>Values</u>
Behavior (crimes per 1,000)	
Behavior Change $\Delta s := s_{\text{low}}^{\text{inc}} - s_{\text{low}}^{\text{fixed}}$	{20, 30}
Induced Speeding Distribution $(s_{\text{low}}^{\text{inc}}, s_{\text{high}}^{\text{inc}})$	{(60, 80) ; (70, 70) ; (80, 60)}
Fines (€)	
Fixed-fine $\tau^{\text{fixed}} = 200$	
Income-Based Fines $(\tau_{\text{low}}^{\text{inc}}, \tau_{\text{high}}^{\text{inc}})$	{ $\underbrace{(145, 255)}$; $\underbrace{(90, 310)}$; $\underbrace{(35, 365)}$ }
	Below-Proportional Proportional Above-Proportional
Fixed-fine $\tau^{\text{fixed}} = 330$	
Income-Based Fines $(\tau_{\text{low}}^{\text{inc}}, \tau_{\text{high}}^{\text{inc}})$	{ $\underbrace{(240, 420)}$; $\underbrace{(150, 510)}$; $\underbrace{(60, 600)}$ }
	Below-Proportional Proportional Above-Proportional

Table 9.17: Survey Respondent Summary Statistics

	(1)	(2)
	All Responses	Main Sample
Income Group (%)		
0 € - 14,999 € (1st quintile)	20.3	19.6
15,000 € - 24,999 € (2nd quintile)	16.3	16.4
25,000 € - 34,999 € (3rd quintile)	20.9	21.0
35,000 € - 49,999 € (4th quintile)	22.3	22.7
50,000 € or above (5th quintile)	20.2	20.2
Education completed (%)		
Primary Education or lower	7.0	6.2
Secondary Education	48.1	47.7
Higher Education	44.9	46.1
Age group (%)		
18-34 (1st quartile)	27.1	26.3
35-50 (2nd quartile)	26.0	25.9
51-65 (3rd quartile)	21.3	21.3
Over 65 (4th quartile)	25.6	26.4
Gender (%)		
Man	49.9	49.3
Woman	49.3	49.9
Other/decline to answer	0.8	0.8
Driving frequency (%)		
Never	13.0	12.3
Less than once per month	10.6	10.4
Monthly	6.8	6.8
Weekly	69.5	70.5
I(Speeding ticket in last 2 years) (%)	7.27	7.00
HH Car Ownership Rate (%)	79.3	79.9
Median completion time (mins.)	12.9	13.05
<i>N</i>	3038	2727

Table 9.18: Extensive Margin Regressions

	Share WTP > 0 (pp)	
	(1)	(2)
	WTP ^{inc-fine}	WTP ^{net}
Proportional Fines, Equal Speeding, $\Delta s = 20$	79.69*** (3.125)	58.26*** (3.674)
Behavior		
Inequality ($s_{\text{low}}^{\text{inc}} < s_{\text{high}}^{\text{inc}}$)	2.922 (1.996)	2.625 (2.346)
Inequality ($s_{\text{low}}^{\text{inc}} > s_{\text{high}}^{\text{inc}}$)	-1.888 (1.998)	-2.130 (2.348)
Behavior Change Δs	-2.018 (1.619)	-2.831 (1.903)
Fine Schedule		
Below-Proportional ($b < b_{\text{prop}}$)	0.537 (1.976)	3.791 (2.323)
Above-Proportional ($b > b_{\text{prop}}$)	-1.395 (2.007)	0.929 (2.359)
N	2727	

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9.19: Heterogeneity by Background Characteristics

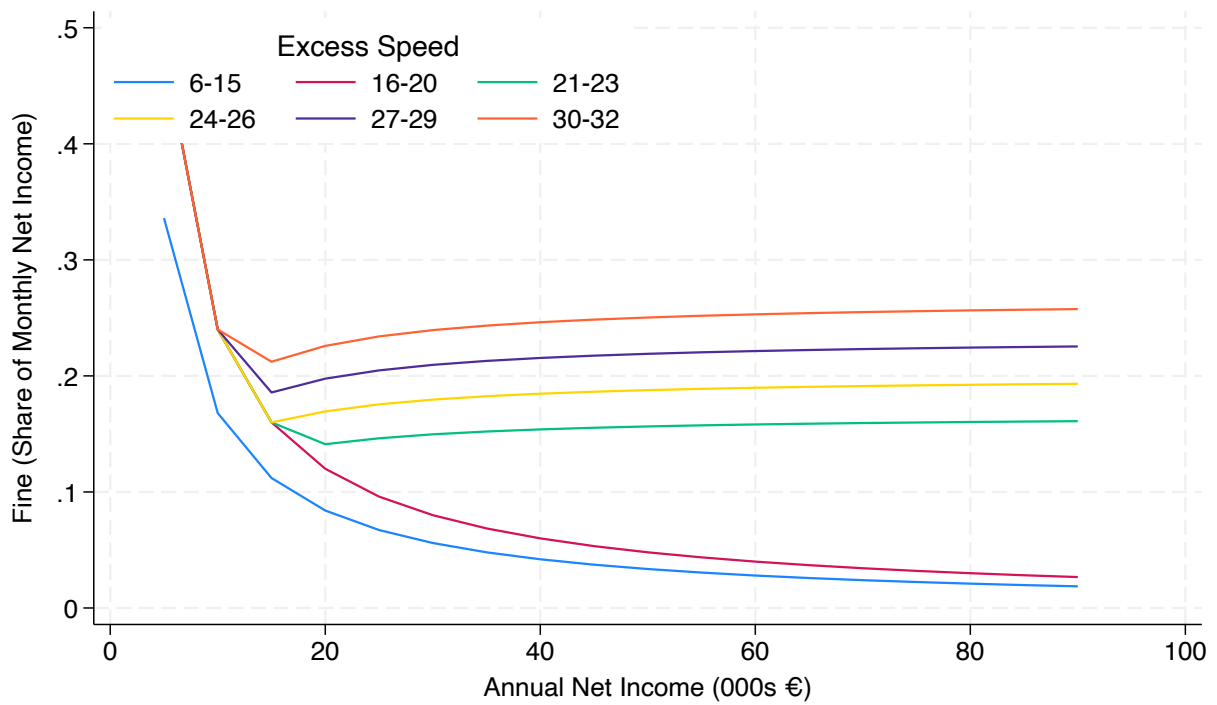
	Outcome: Net WTP									
	Income			Fine Awareness Beliefs			Fine Responsiveness Beliefs			Age
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	<35,000 €	≥35,000 €	Unaware	Aware	Unresponsive	Responsive	≤ 50	> 50	Secondary Educ.	Higher Educ.
Proportional Fines & Equal Speeding	52.19*** (14.90)	38.59* (17.51)	45.76* (18.08)	44.71** (14.60)	34.32 (17.81)	53.96*** (14.68)	32.46* (14.83)	61.11*** (17.50)	38.65* (16.63)	52.54** (17.05)
Behavior										
Inequality ($s_{low}^{inc} < s_{high}^{inc}$)	-1.240 (9.572)	10.72 (11.11)	-6.412 (11.61)	10.27 (9.313)	2.134 (11.37)	5.123 (9.376)	14.33 (9.405)	-9.088 (11.14)	7.467 (10.43)	-1.205 (10.82)
Inequality ($s_{low}^{inc} > s_{high}^{inc}$)	-11.07 (9.434)	-2.505 (11.35)	-10.46 (11.79)	-4.758 (9.216)	-16.16 (11.26)	0.907 (9.463)	-1.558 (10.03)	-13.72 (10.82)	-6.580 (10.48)	-8.769 (11.02)
Behavior Change Δs	-5.707 (7.712)	-12.60 (9.092)	-9.041 (9.411)	-7.361 (7.553)	-14.56 (9.147)	-3.927 (7.650)	-3.668 (7.817)	-13.91 (8.924)	-0.723 (8.370)	-15.84 (8.912)
Fine Schedule										
Below-Proportional ($b < b_{prop}$)	6.203 (9.487)	1.836 (11.00)	2.291 (11.46)	7.086 (9.223)	15.94 (11.07)	-4.450 (9.409)	9.195 (9.350)	-0.299 (10.97)	12.59 (10.60)	1.197 (10.67)
Above-Proportional ($b < b_{prop}$)	-2.974 (9.617)	-8.436 (11.20)	9.145 (11.77)	-13.78 (9.293)	21.97 (11.33)	-24.52** (9.494)	-7.855 (9.559)	-0.887 (10.85)	4.913 (10.37)	-19.78 (10.91)
N	1555	1172	1110	1617	1117	1610	1425	1302	1300	1257

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

9.4 Supplemental Figures

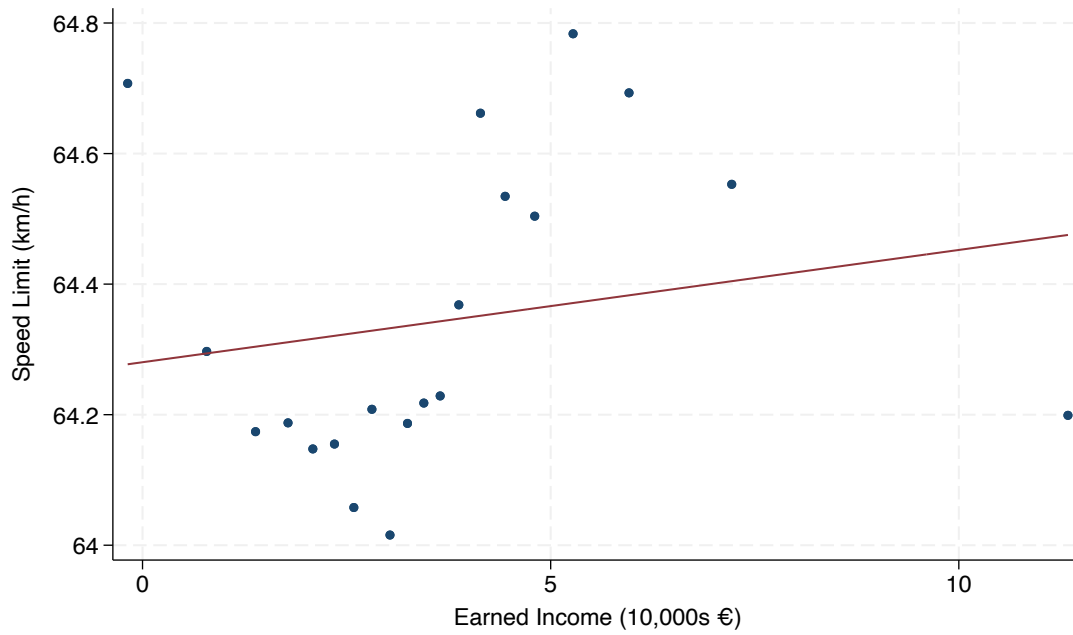
Figure 9.1: Hypothetical Fine Schedule (100 km/h limit)



Net-of-tax income consists of earned and capital income, as well as any transfers received, after the payment of taxes. We assume no dependents for the formula in equation 2.1.

Figure 9.2: Binscatter Relationships Between Income and Speeding Crime Characteristics

(a) Speed Limit



(b) Excess Speed

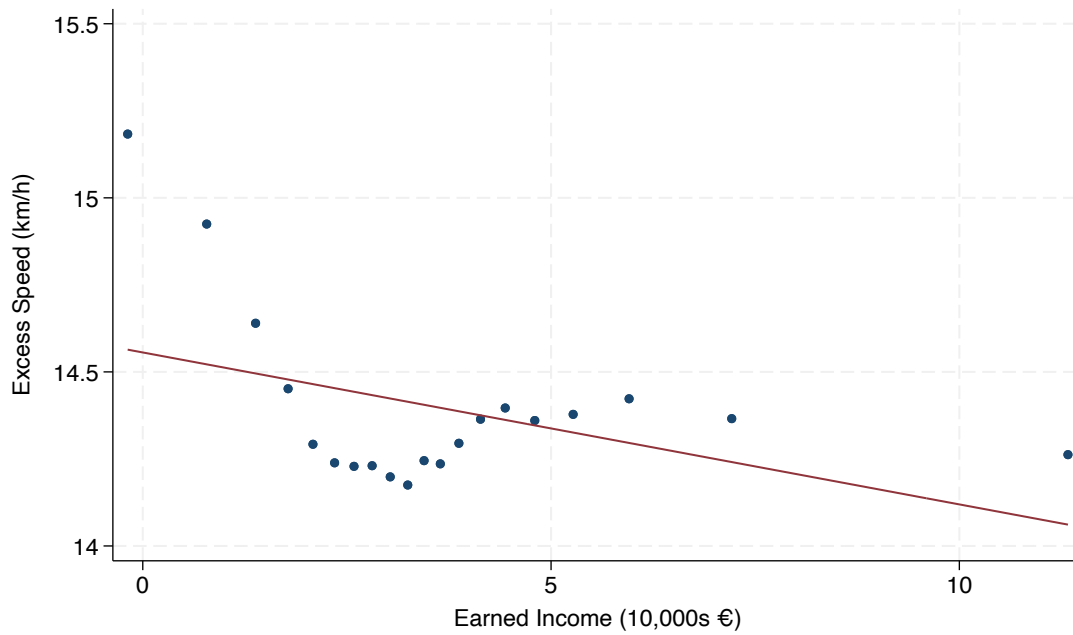
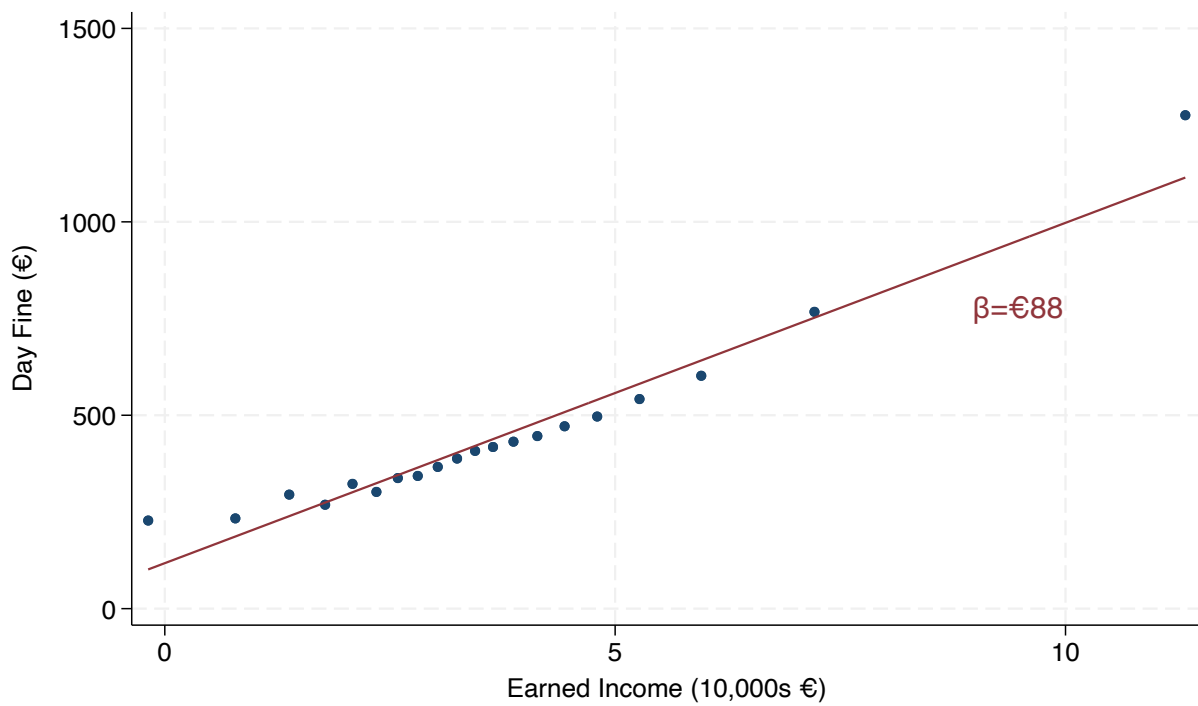
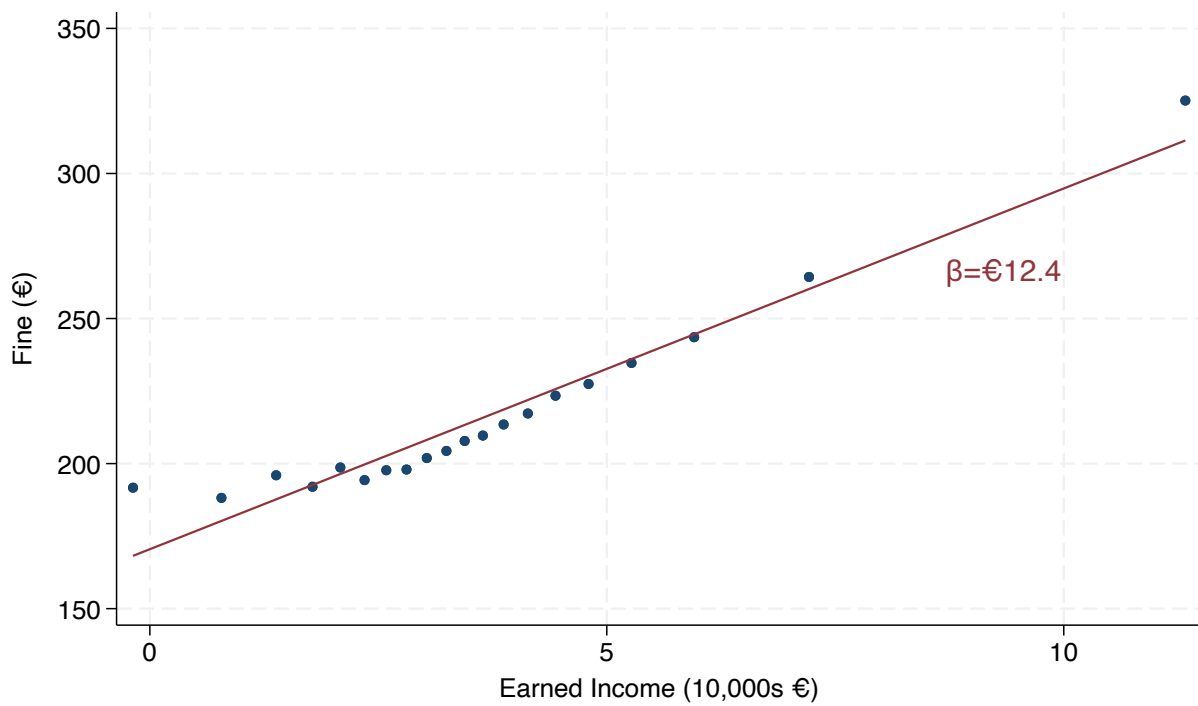


Figure 9.3: Day Fine Slope



Binscatter based on police ticket data from 2018-2020. Income and the fine amount are residualized on age-by-year fixed-effects.

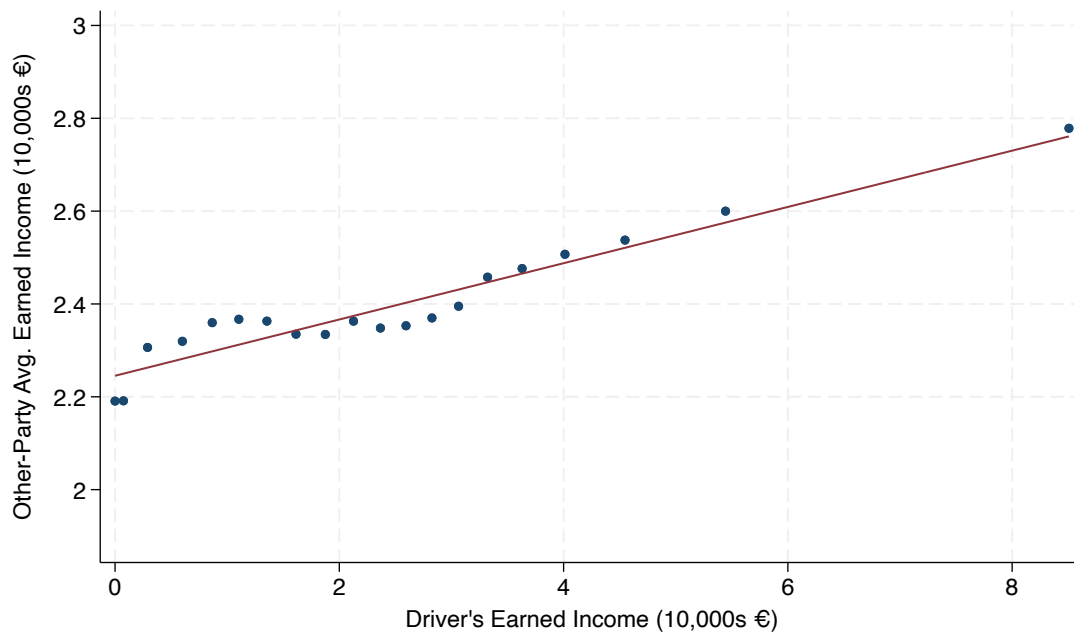
Figure 9.4: Overall Fine Slope



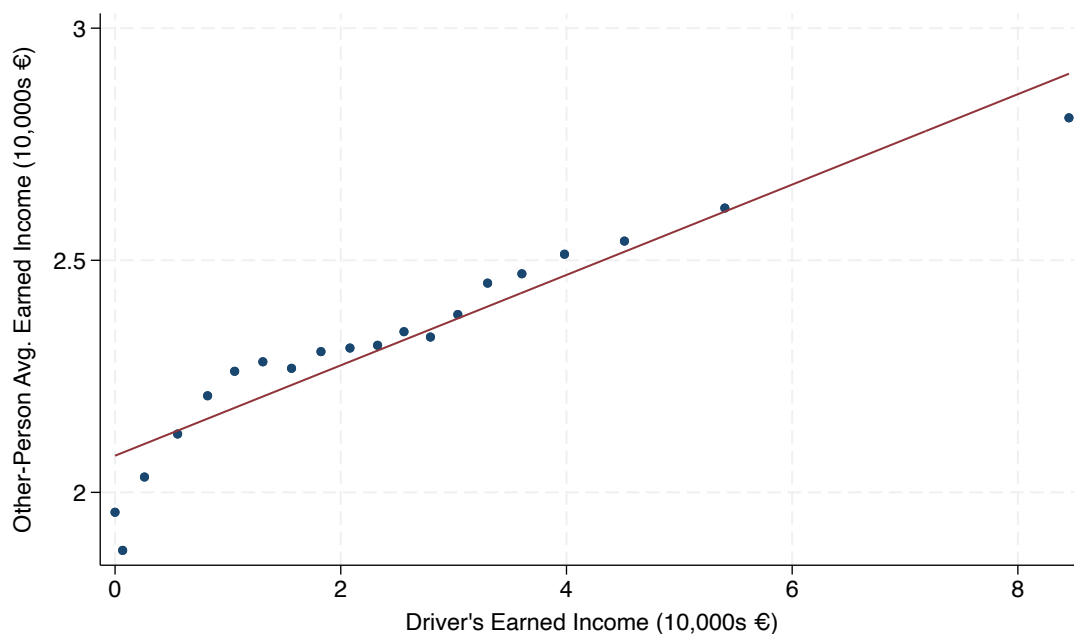
Binscatter based on police ticket data from 2018-2020. Income and the fine amount are residualized on age-by-year fixed-effects.

Figure 9.5: Binscatter Correlations Between Driver and Victim Income

(a) Other-Party Victims



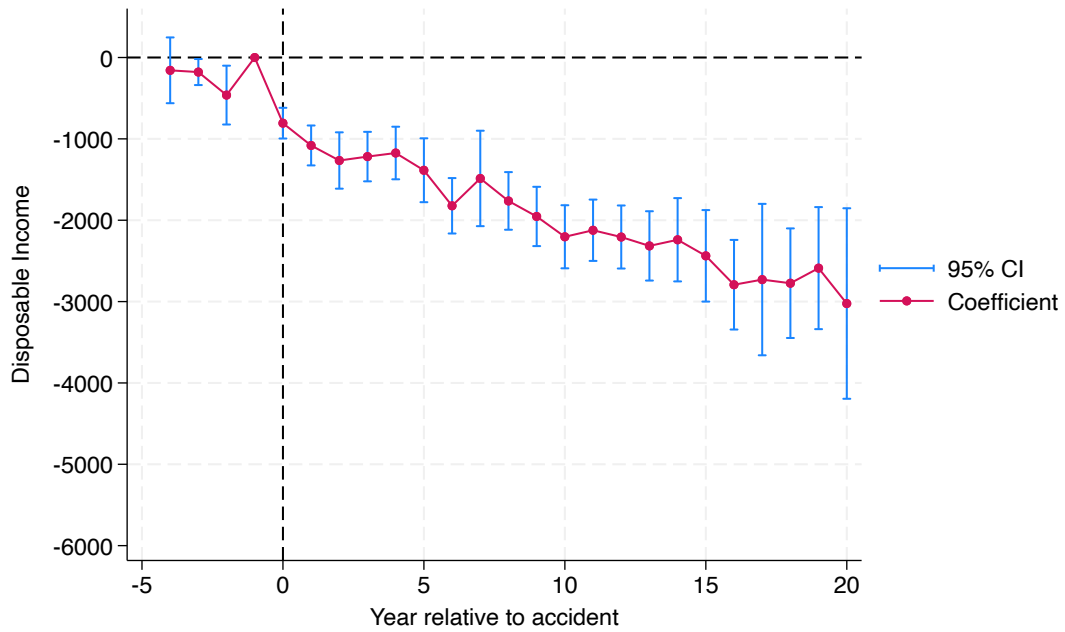
(b) Other-Person Victims



Binscatter relationships between driver income and victim income. We use the sample of individuals described in 5.1 and restrict to the first accident in which an individual is involved as a driver from 2006-2018. We further restrict to drivers of a car, truck, bus, moped, motor bike, motorcycle, or van and exclude accidents involving trains, trams, tractors, snow mobiles, trailers, or other unidentified motor vehicles. Earnings are measured in the year prior to the accident.

Figure 9.6: Event Study Effects of Injury/Death on Disposable Income

(a) Drivers



(b) Passengers

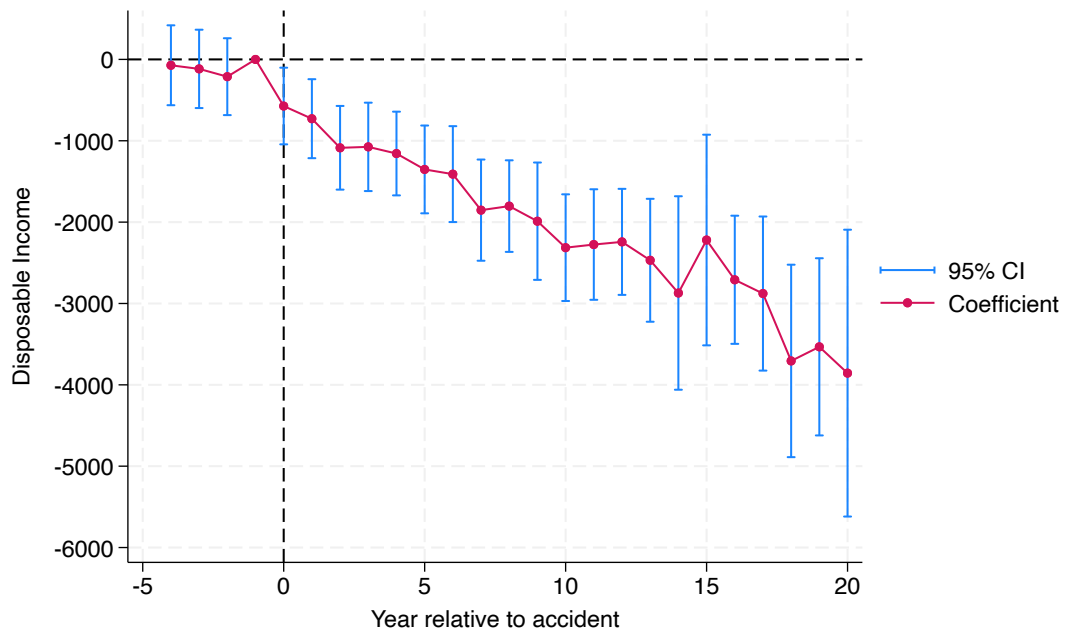
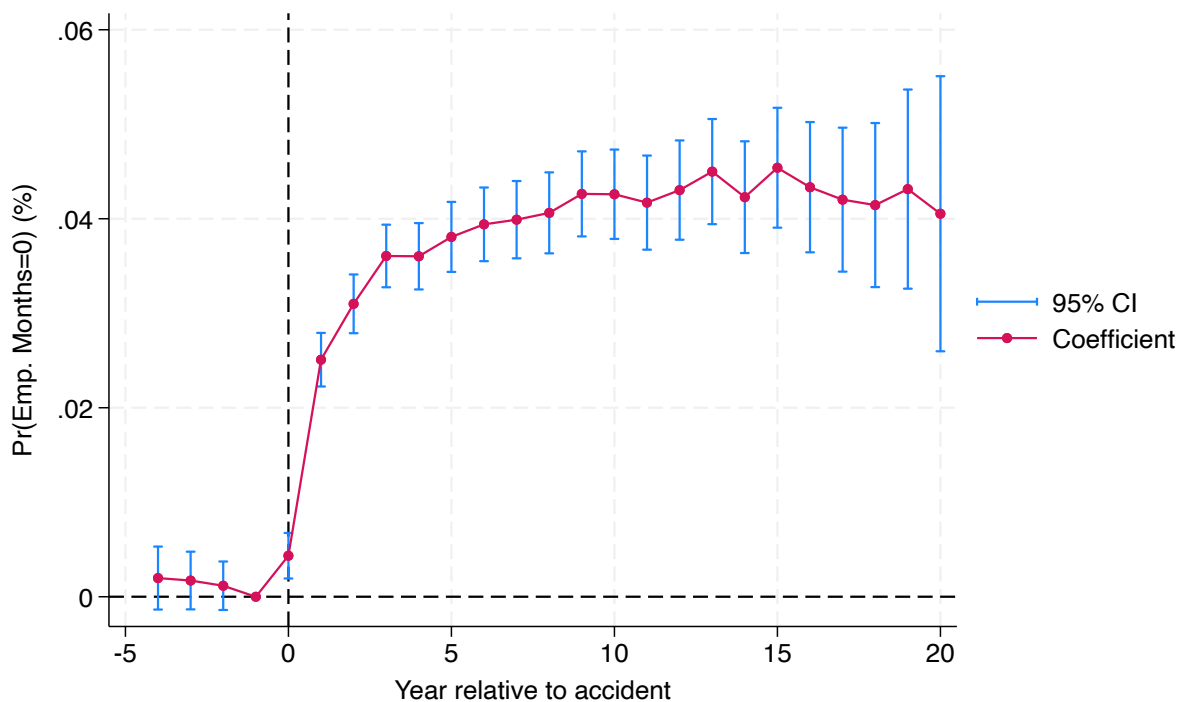
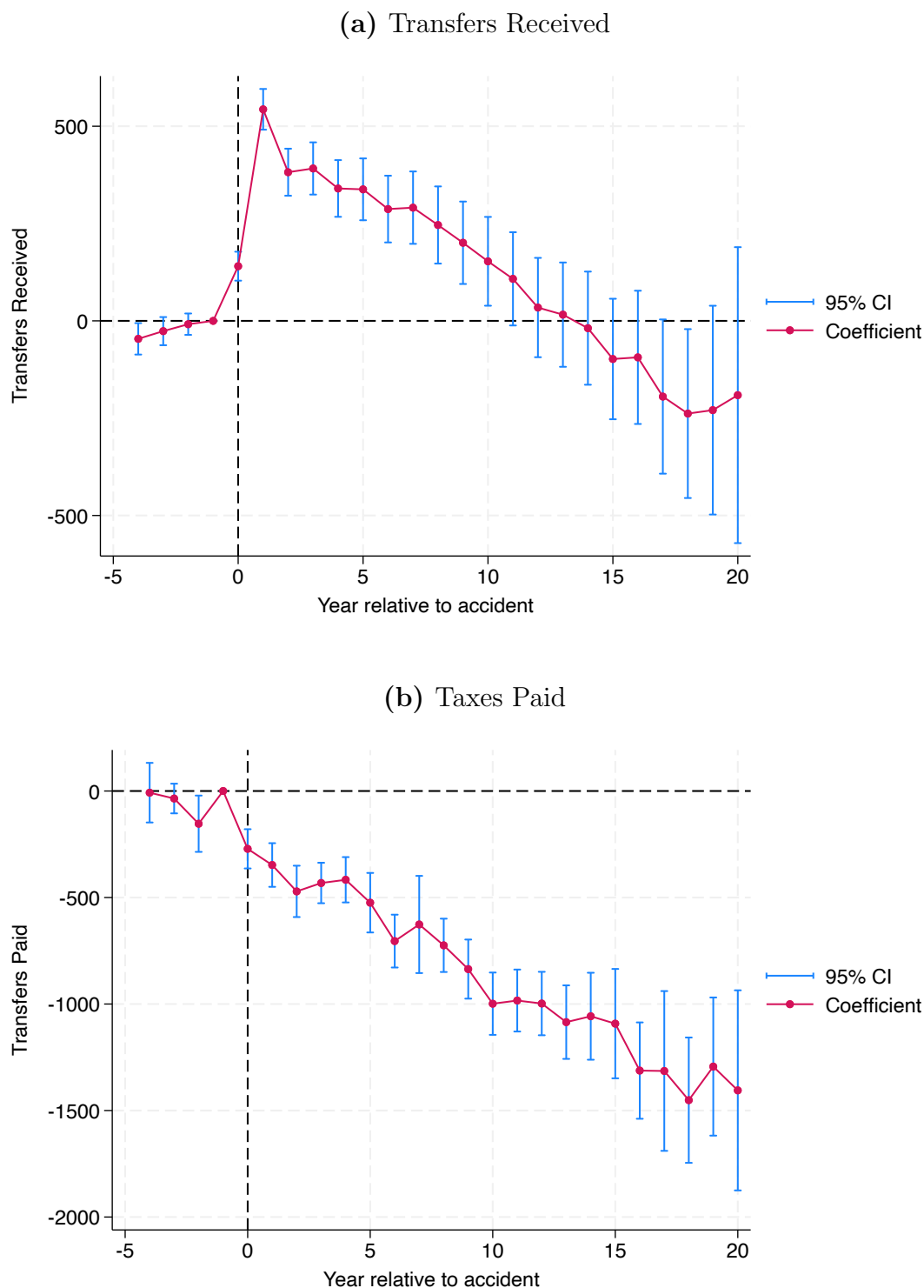


Figure 9.7: Event Study Effects of Non-Fatal Injuries on Probability of Working



Estimates of β_k from equation 5.2 using an indicator for whether an individual is employed for zero months during the year as the outcome. The sample includes all accident-related non-fatal injuries from 2003-2022, restricted to accidents that include a motor vehicle (cars, trucks, buses, vans, mopeds, motor bikes, and motorcycles). Injuries from accidents involving a snow mobile, trailer, tram, train, tractor, or other unidentified motor vehicles are excluded.

Figure 9.8: Event Study Effects of Injury/Death on Transfers/Taxes



Estimates of β_k from equation 5.2. The sample includes all accident-related injuries from 2003-2022, restricted to accidents that include a motor vehicle (cars, trucks, buses, vans, mopeds, motor bikes, and motorcycles). Injuries from accidents involving a snow mobile, trailer, tram, train, tractor, or other unidentified motor vehicles are excluded.

Figure 9.9: Event Study Effects of Non-Fatal Injuries on Income

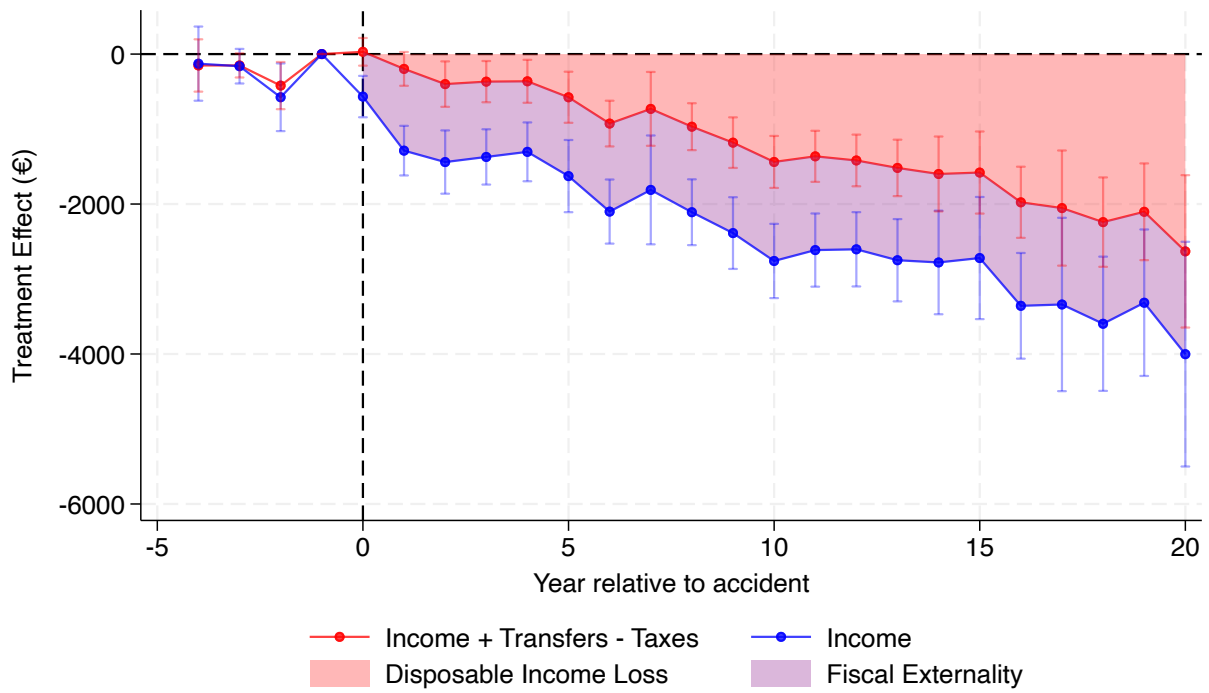
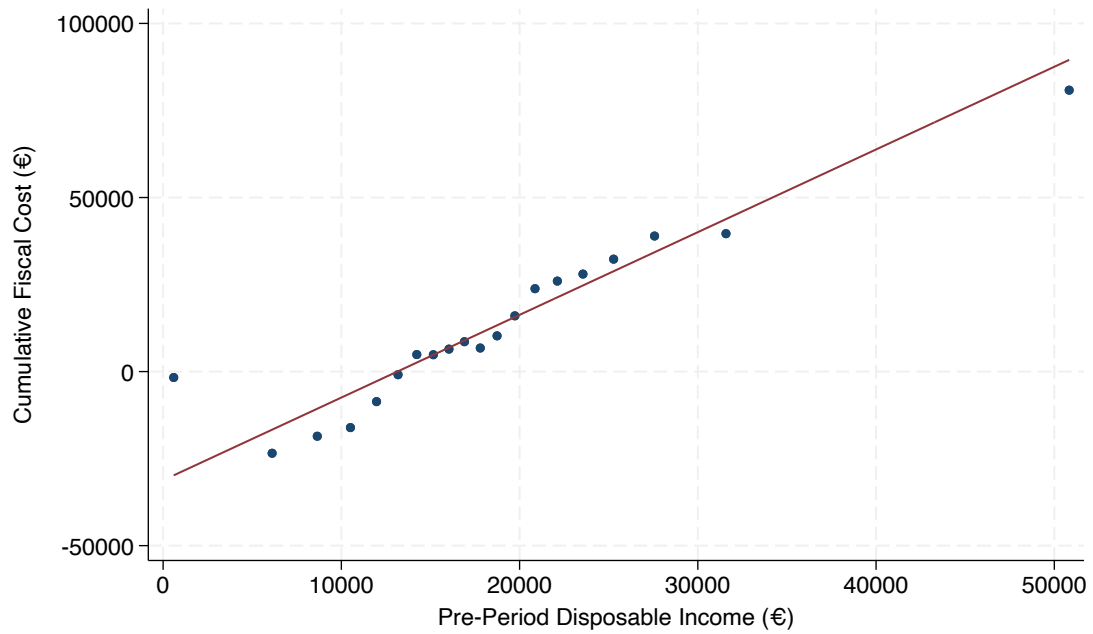


Figure 9.10: Cumulative Social Costs of Injury/Death, by Pre-Period Income

(a) Fiscal Externality



(b) Disposable Income Loss

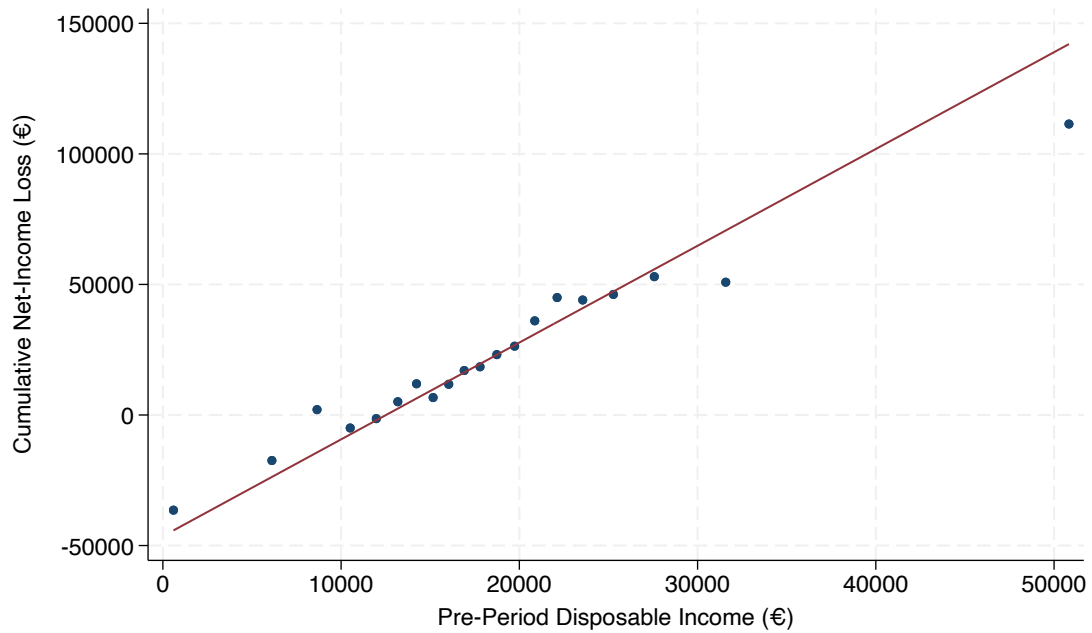


Figure 9.11: Event Study of Taxable Earned Income around Parental Death

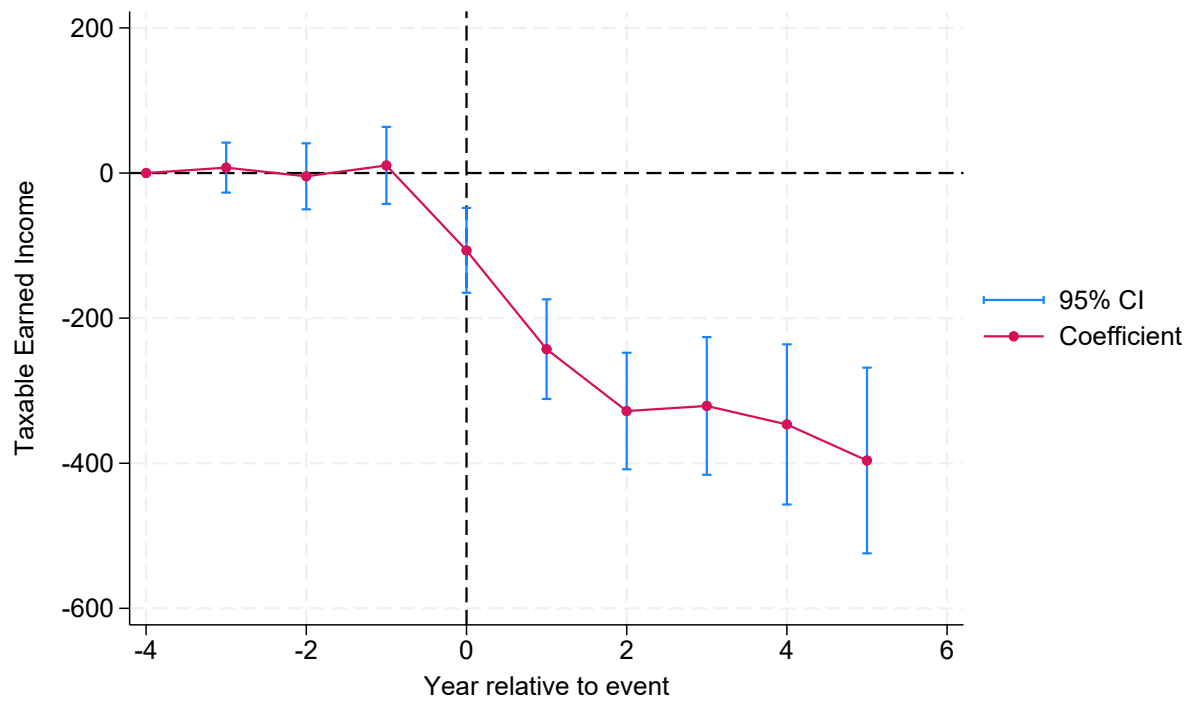


Figure 9.12: Event Study of Car Ownership around Parental Death

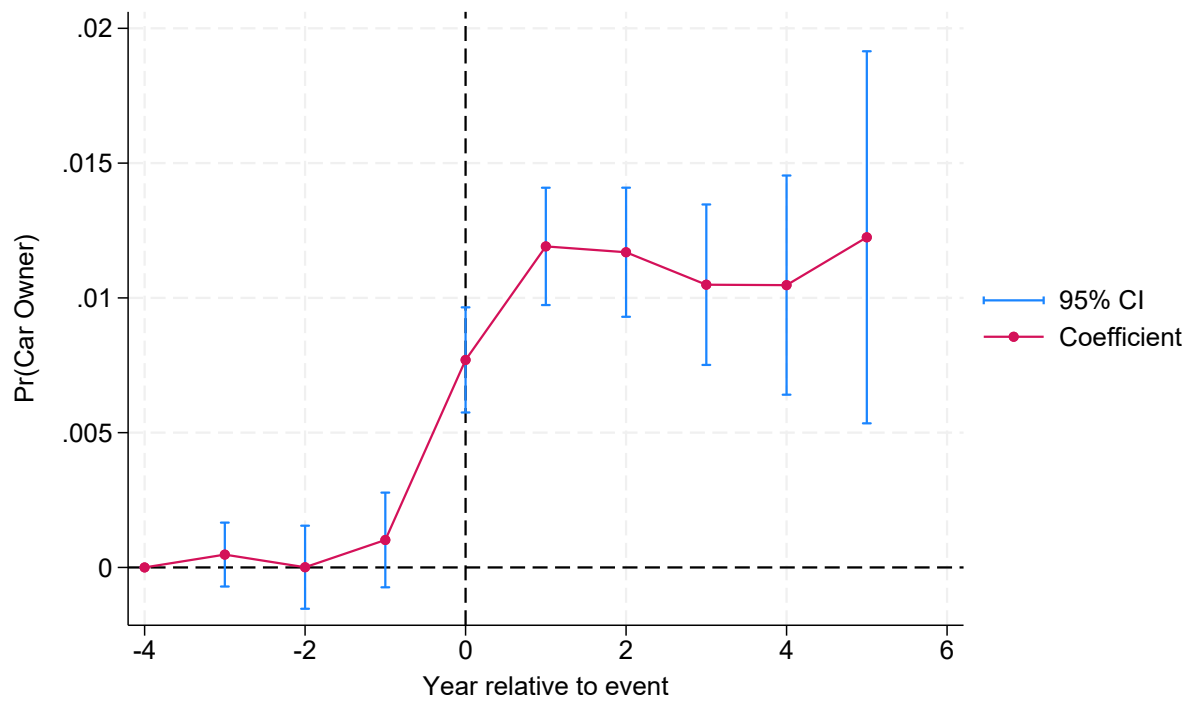


Figure 9.13: Triple-Difference Effects on Labor Supply

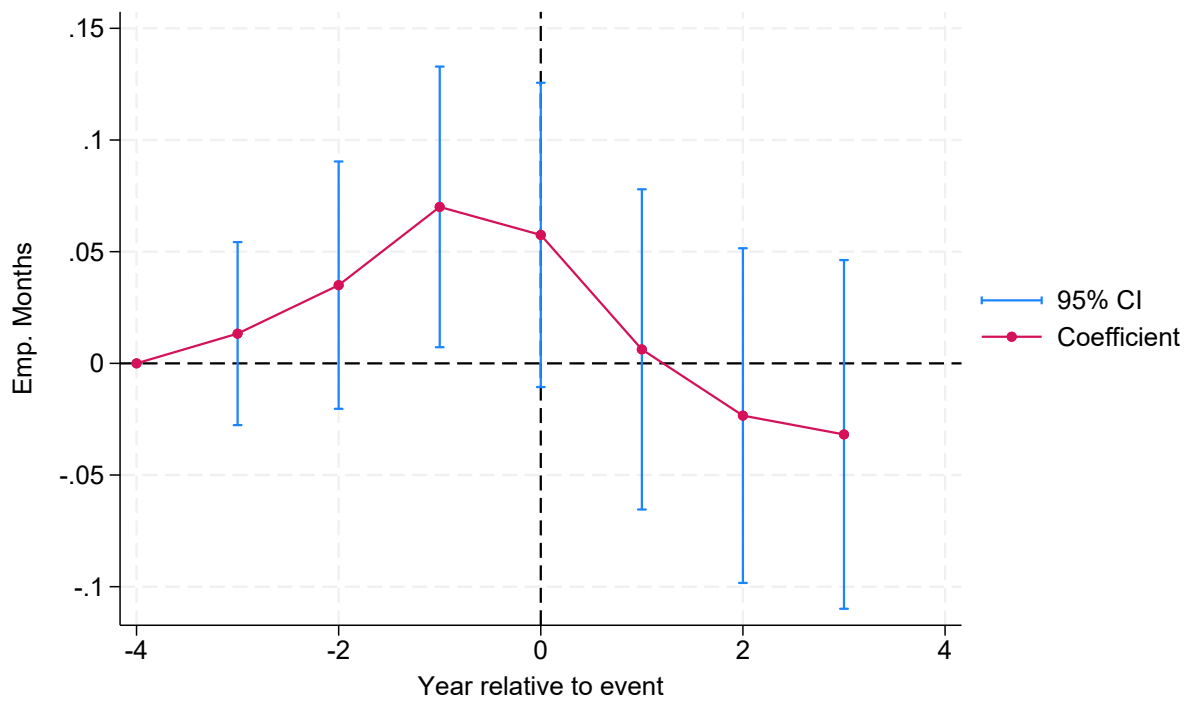


Figure 9.14: Triple-Difference Effects on Earnings

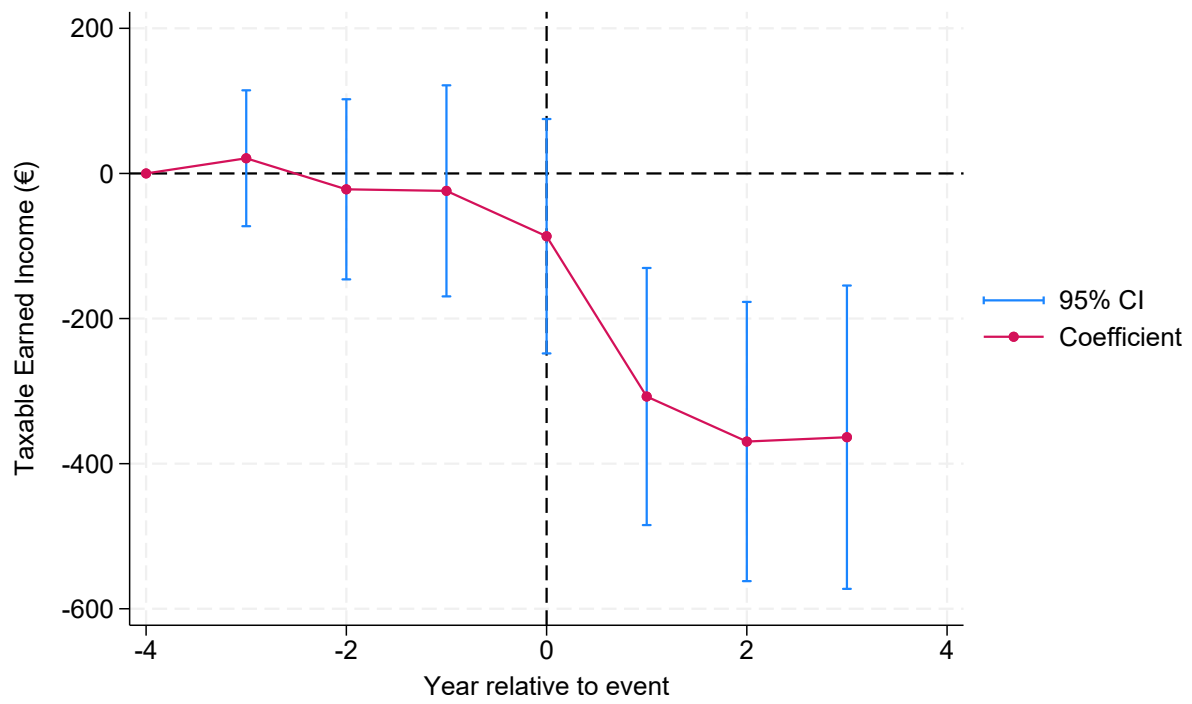
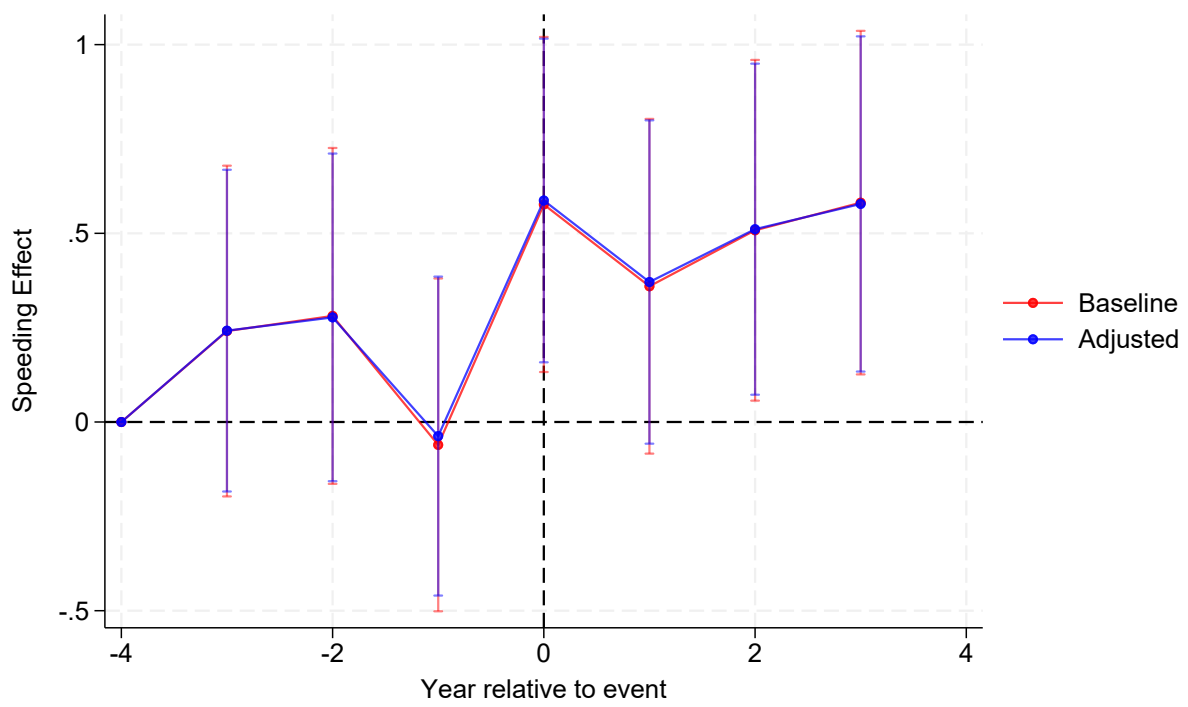


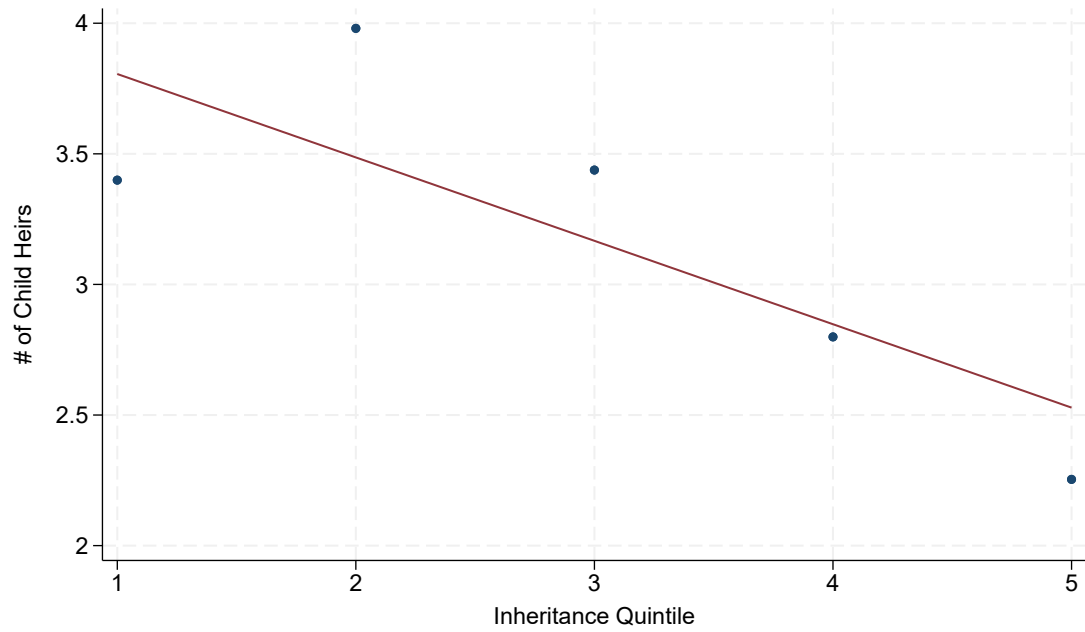
Figure 9.15: DDD Event Study of Speeding Rates around Parental Death: Relative vs. Level Effects



Caption

Figure 9.16: Potential Child Heirs and Tax Revisions by Inheritance Quintile

(a) Child Heirs



(b) Tax Revisions

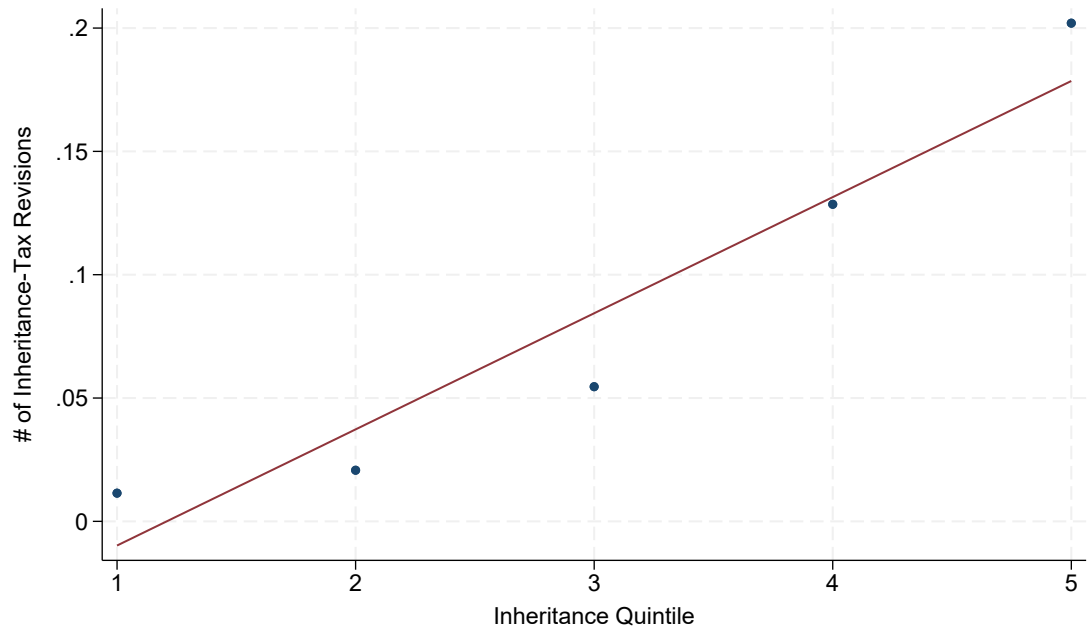


Figure 9.17: DDD Event Study of Speeding Rates around Parental Death: 5-year Event Horizon

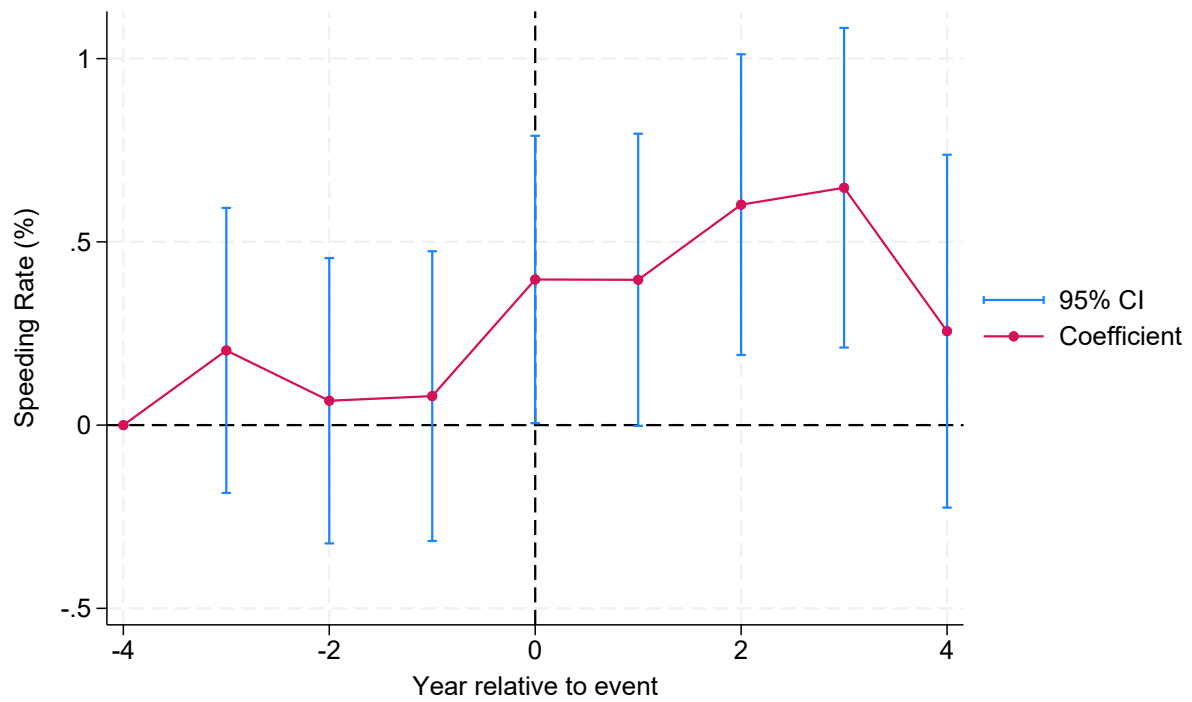


Figure 9.18: Relationship between Earnings and Speeding Rates (2012)

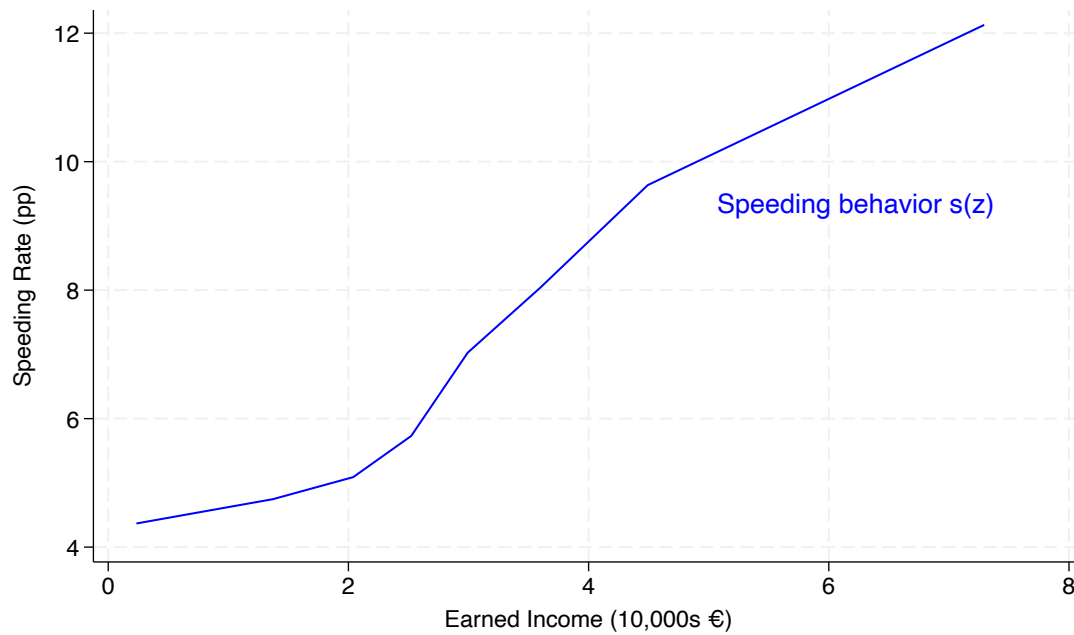


Figure 9.19: Heatmap of Optimal Redistributive Gradients

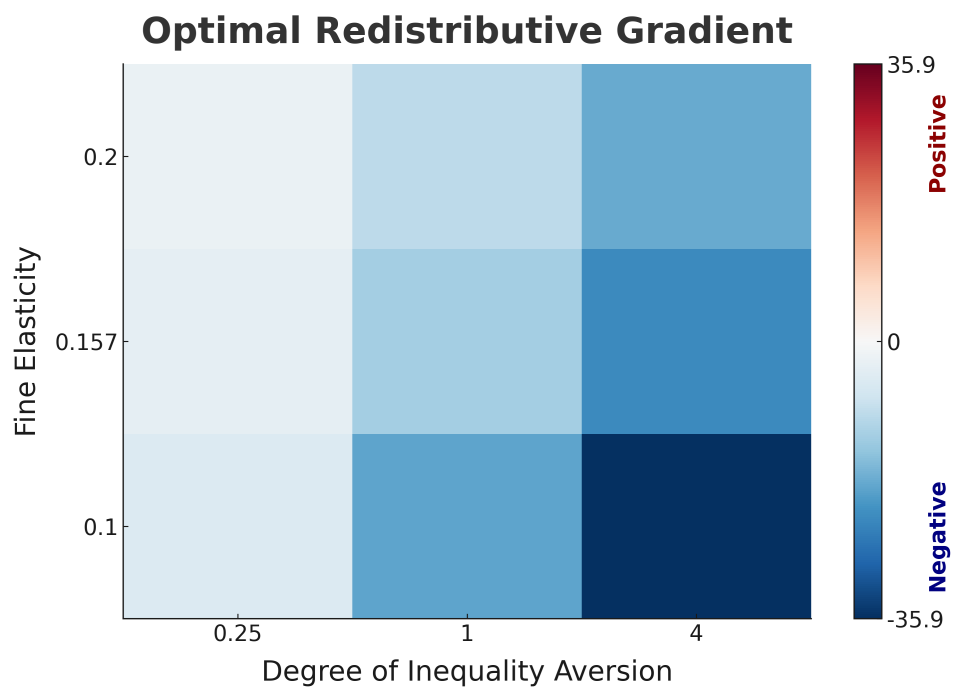


Figure 9.20: Speeding Rate Comprehension

The next questions concern hypothetical speeding fines that different income groups might have to pay. We describe how common speeding is in each income group by reporting how many speeding fines the group receives per 1,000 people per year.

For example, if an income group has 2,000 people and a total of 20 of them receive a speeding fine in a year, then proportionally this group receives 10 fines per 1,000 people per year.

If group A receives more speeding fines per 1,000 people than group B, then group A receives proportionally more fines.

The next question ensures that you understand what we mean by proportional (per-capita) numbers.

Suppose that a hypothetical group receives a total of 500 speeding fines per year and its size is 5,000 people. Estimate how many speeding fines the group receives per 1,000 people per year.

Figure 9.21: Fine-Policy Reasoning

We would appreciate if you could describe the basis on which you initially chose the Income-based fines policy. The tables below present the policies again.

Fixed fines		
<u>Income Group</u>	<u>Speeding Fine</u>	<u>Fines per 1,000 people</u>
Low (Average Income: 15 000 €)	330 €	40
High (Average Income: 51 000 €)	330 €	100
<u>Total speeding tickets per year: 315 000</u>		

Income-based fines		
<u>Income Group</u>	<u>Speeding Fine</u>	<u>Fines per 1,000 people</u>
Low (Average Income: 15 000 €)	150 €	70
High (Average Income: 51 000 €)	510 €	70
<u>Total speeding tickets per year: 315 000</u>		

Please use the text box below to describe the factors that influenced your decision to choose Income-based fines rather than Fixed fines.

Figure 9.22: Example Fine-Policy Comparison

This question compares two hypothetical speeding ticket systems that could be introduced in Finland:

1. **Fixed fines** - the fines are the same for everyone.
2. **Income-based fines** - the size of the fine depends on whether a person belongs to a low or high income group.

We ask you to choose **which system would be better for Finland overall**; suppose that **the fine YOU would have to pay would not be determined by the systems described above.**

The tables below present these two systems for two different income groups with different income levels. **Each income group covers half of the adult population in Finland. The low-income group includes the half of the population with the lowest income. The high-income group includes the half with the highest income.**

Fixed fines		
Income Group	Speeding Fine	Fines per 1000 people
Low (Average Income: 15 000 €)	330 €	50
High (Average Income: 51 000 €)	330 €	90
Total speeding tickets per year: 315 000		

Income-based fines		
Income Group	Speeding Fine	Fines per 1000 people
Low (Average Income: 15 000 €)	150 €	70
High (Average Income: 51 000 €)	510 €	70
Total speeding tickets per year: 315 000		

Which system do you prefer?

Please note that the systems differ in how much speeding is done in different income groups and how large the fines are. **However, the overall speeding rate is the same in both systems.** The systems are otherwise completely similar.

Figure 9.23: Survey: Transition from Comparison to Slider

Let's imagine that the Finnish government could save on administrative costs by adopting the system that you did *not* choose in the previous question (Fixed fines).

Next, we ask you to assess how large the *minimum* annual savings would need to be for you to be willing to switch to the system you did not choose in the previous question.

Click the "Next" button to move forward.

Figure 9.24: Slider for Fine Policy Comparison

Consider options A and B below.

<u>Option A</u>	<u>Option B</u>
Income-based fines are implemented	Fixed fines are implemented and Savings of Y € per adult (X million € in total) annually

The tables below present the two fine policies again. What would be the minimum annual savings required for you to choose option B?

I would choose option B if the savings were
at least Y € per adult (X million € in total) annually



*(Remember, the **fine YOU would have to pay is not determined by the policies described below.** If the smallest total savings that would make you switch is greater than the values shown on the slider, choose the highest possible value.)*

Figure 9.25: Transfer Comparison Question

Imagine that the Finnish government receives a **one-time** unexpected revenue increase. The government plans to distribute some of this extra revenue to the Finnish adult population in the form of cash transfers. There are two alternative systems for distributing the funds:

1. **Fixed cash transfers** - every adult living in Finland receives the same amount.
2. **Income-based cash transfers** - the amount distributed to a person depends on whether they belong to a low- or high-income group.

We ask you to choose **which system would be better for Finland overall**. Suppose that your choice between the two ways of distributing the funds **wouldn't affect you: YOU would not receive a cash transfer in either case**.

The table below shows the two ways of distributing the funds described above. **Each income group covers half of the adult Finnish population. The low-income group includes the half of the population with the lowest incomes; the high-income group includes the half with the highest incomes.**

	Cash transfer to each individual in a group	
<u>Income Group</u>	Fixed cash transfers	Income-based cash transfers
Low (<i>Average Income:</i> 15 000 €)	25 €	32 €
High (<i>Average Income:</i> 51 000 €)	25 €	20 €

Which system do you prefer?

Please note that the systems differ in how much cash they distribute to different income groups. In addition, the systems do not necessarily distribute the same amount of transfers in total. The systems are otherwise completely similar.

Figure 9.26: Frequency of Fine Awareness Beliefs

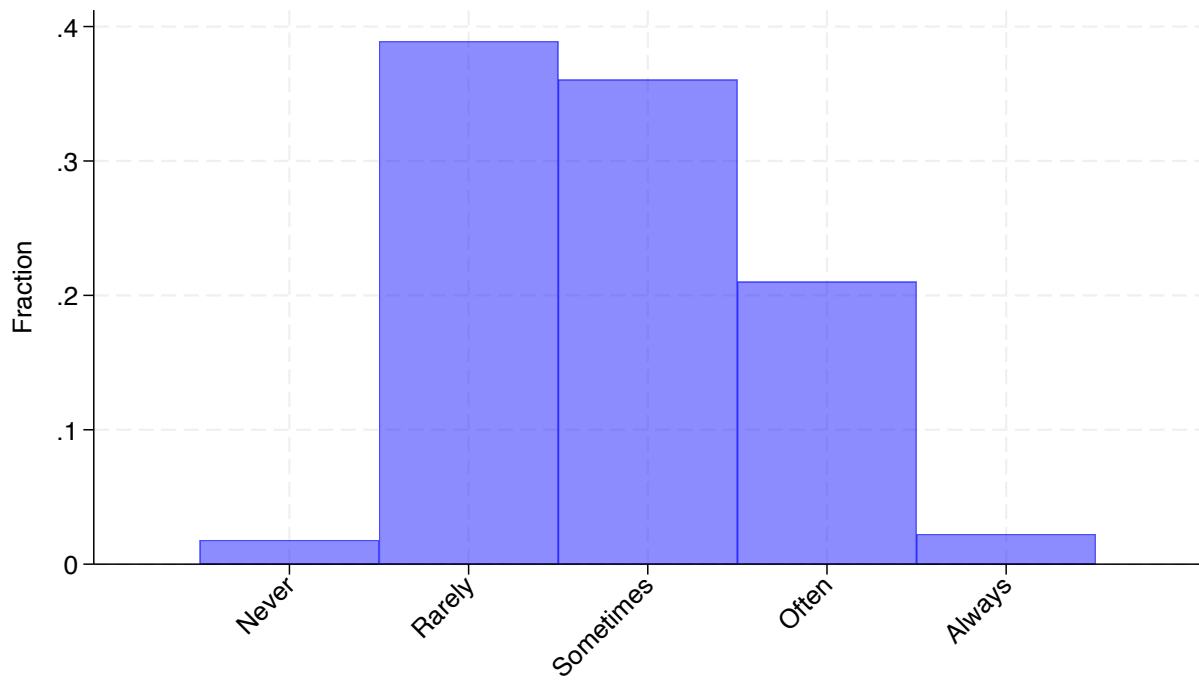


Figure 9.27: Frequency of Fine Responsiveness Beliefs

