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ABSTRACT

Policymakers are increasingly interested in non-standard policy instruments (NPIs), or "nudges," such as simplified information disclosure and warning labels. We characterize the welfare effects of NPIs using public finance sufficient statistic approaches, allowing for endogenous prices, market power, and optimal or suboptimal taxes. While many empirical evaluations have focused on whether NPIs increase ostensibly beneficial behaviors on average, we show that this can be a poor guide to welfare. Welfare also depends on whether the NPI reduces the variance of distortions from heterogenous biases and externalities, and the average effect becomes irrelevant with zero pass-through or optimal taxes. We apply our framework to randomized experiments evaluating automotive fuel economy labels and sugary drink health labels. In both experiments, the labels increase ostensibly beneficial behaviors but also may decrease welfare in our model, because they increase the variance of distortions.

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A randomized controlled trials registry entry is available at www.socialscienceregistry.org/trials/7460

In recent years, policymakers and academics have become increasingly interested in non-standard policy instruments (NPIs), or "nudges," such as simplified and salient information provision, warning labels, reminders, social comparisons, framing, and choice architecture.[1] Spurred by the book Nudge (Thaler and Sunstein 2008) and the success of the Behavioral Insights Team in the UK, more than 200 public agencies now incorporate NPIs into the policy process (OECD 2017). Hundreds of academic papers evaluate NPIs using randomized experiments (Delmas, Fischlein and Asensio 2013; Hummel and Maedche 2019), and the U.S. government alone has conducted more than 160 randomized evaluations of NPIs (DellaVigna and Linos 2022). NPIs are being used to increase retirement savings, healthy eating, exercise, social program take-up, environmental conservation, organ donation, and other behaviors thought to benefit individuals or society.

When do NPIs increase social welfare? A common argument for NPIs is that they can increase beneficial behaviors at low implementation cost (Benartzi et al. 2017; Loewenstein and Chater 2017), and many empirical evaluations focus on average treatment effects. To what extent are average effects a good proxy for welfare gains? Early arguments for NPIs focused on their potential to increase welfare through "asymmetric paternalism" or "libertarian paternalism": improving choices by biased consumers without affecting those who fully optimize (Camerer et al. 2003; Thaler and Sunstein 2003). How do these arguments apply when NPIs indirectly affect *all* consumers by changing equilibrium market prices?

We address these questions with theoretical tools from public finance and novel randomized experiments evaluating automotive fuel economy labels and sugary drink health labels. Our key conceptual point is this: despite being the focus of much empirical work, *average* treatment effects are an incomplete and potentially misleading way to evaluate NPIs, while effects on the *variance* of distortions are crucial to measure.

In our model, heterogeneous consumers make a binary choice (for example, whether to buy a sugary drink or a sugar-free alternative) in a market with three distortions: consumer biases, externalities, and seller market power. The NPI is a policy instrument that changes each consumer's relative willingness-to-pay (WTP) by a potentially heterogeneous amount τ. The government also has a fully salient uniform tax, which may not be set optimally due to political or technical constraints.

We show that a key determinant of an NPI's total surplus effect is whether it reduces the *variance* of choice distortions caused by bias and externalities. For example, hard information could reduce distortion variance if it has the largest effects on the most misinformed consumers. Alternatively, a graphic warning label that acts through drawing attention might increase distortion variance if consumers who were already the most attentive to harmful attributes are also the most attentive to the label. As another example, if different consumers interpret a label differently, generating idiosyncratic noise in its effects, this would also increase distortion variance.

[1] "NPIs" refers to the same policy instruments usually described as "nudges." We say "NPIs" instead of "nudges" because the word "nudge" suggests a small effect that our analysis does not assume, because these instruments can affect equilibrium prices and choice sets, and because these instruments often directly affect utility (for example, by involving social pressure or guilt). See Bernheim and Taubinsky (2018) for further discussion.

In a market with non-zero pass-through, an NPI's total surplus effect also depends on whether it changes *average* behavior in the "right direction," i.e. if the average of WTP effects τ counteracts the average net distortion from bias, externalities, and market power that is not already internalized by a tax. However, average effects become less important at lower pass-through rates. In the limiting case with zero pass-through, the NPI cannot change aggregate quantity, and all that matters is the efficiency of the allocation of that fixed quantity.[2] Analogously, if the government can optimally control aggregate quantity through an optimal tax, average effects are again irrelevant. Tangibly, this means that if governments can set optimal taxes on goods like gas guzzling cars, sugary drinks, or cigarettes, *it does not matter whether an NPI decreases or increases average consumption*—all that matters is whether the NPI reduces distortion variance.

These results clarify how asymmetric paternalism, libertarian paternalism, and improvements in "deliberative competence" (Ambuehl, Bernheim and Lusardi 2022) translate to market settings and cases where the government can also set taxes. With endogenous producer prices and/or optimal taxes that adjust to NPIs, NPIs can reduce total surplus even if they improve the decisions of some biased consumers without affecting optimizing consumers. For example, a market with homogeneous bias can still achieve the first best if supply is perfectly inelastic or the government can set the optimal tax. In that case, it is heterogeneity in bias that generates misallocation, so NPIs can reduce efficiency if they debias some consumers but not others.

We quantify the importance of these issues by applying the theory to two randomized experiments. In the "cars experiment," consumers make binary choices between hypothetically leasing 23 mile-per-gallon (MPG) and 30 MPG sedans. In the "drinks experiment," consumers make binary choices between sugary and sugar-free drinks with similar flavors. The two experiments have parallel structures: we first measure behavioral bias, then measure baseline relative WTP using a multiple price list (MPL), and finally measure endline WTP while displaying fuel economy or health labels.

We chose labels that have been implemented or proposed by researchers or government agencies. In the cars experiment, we randomized four labels: (i) the MPG component of the fuel economy label required by the U.S. Environmental Protection Agency (EPA), (ii) the average annual fuel cost component of the EPA label, (iii) a personalized annual fuel cost label based on each participant's 2019 miles driven and gas price, and (iv) the EPA's "SmartWay" environmental label. In the drinks experiment, we randomized three labels: (i) a magnified nutrition facts label, (ii) a stop sign health warning label recommended by Grummon et al. (2019b), and (iii) the graphic warning label studied by Donnelly et al. (2018). The label type was randomly assigned across participants, and a control group saw no labels on their endline choices.

Both experiments were incentivized. In the cars experiment, participants' payments depended on how close their relative WTPs were to their "true" relative values implied by earlier survey responses. In the drinks experiment, we randomly selected just over 20 percent of participants and

[2] τ is the NPI's effect on WTP, regardless of price changes. Our sufficient statistics formulas translate these WTP changes into effects on market equilibrium using the pass-through rate and other parameters.

shipped them the drink they had chosen in one of their MPL choices, while deducting the price of the drink from their payment.

In the cars experiment, behavioral bias is the difference between baseline relative WTP and the "true" relative value. As in Ambuehl, Bernheim and Lusardi (2022), these are objective measures of decision quality within our experiment, because bias is defined as the difference between elicited WTP and the WTP that would have maximized the participant's experimental payment. Relative externalities depend on how much each participant drives, the incremental fuel use per mile for the 23-MPG versus 30-MPG car, and the social cost of carbon. In the drinks experiment, we estimate bias from survey measures of nutrition knowledge and self-control using the approach of Allcott, Lockwood and Taubinsky (2019a). We assume a homogeneous health system cost externality for sugary drinks, again following Allcott, Lockwood and Taubinsky (2019a).

The two experiments are complementary in how they trade off internal and external validity. In the cars experiment, we have an objective measure of behavioral bias within the experiment, but the decisions didn't involve actual car purchases. In the drinks experiment, we impose strong assumptions to estimate bias, but participants could actually receive the drinks they chose.

The within- and between-subject variation in labels allows us to measure both average treatment effects and heterogeneity. In the label treatment groups, the relative WTP changes between baseline and endline choices represent the person-specific effect τ plus elicitation noise. The control group WTP changes isolate elicitation noise. The average τ is identified by a simple comparison of average WTP changes in treatment versus control. The variance of τ is identified by the additional dispersion in treatment group WTP changes relative to control. A label's effect on distortion variance depends on how much of the variance in τ represents a covariance with bias and externalities. Our econometric approach addresses potential measurement error in individual-level estimates of bias, externalities, and treatment effects.

In the cars experiment, we estimate small distortions toward the lower-MPG cars, with relative biases and externalities summing to an average of about 3 percent of the typical price. In the drinks experiment, we estimate large distortions toward sugary drinks, with relative biases and externalities summing to 95 percent of price. All fuel economy and sugary drink labels reduce demand for lower-MPG cars and sugary drinks. However, the labels' average effects are much smaller than the average distortion, consistent with the view that NPIs are not sufficiently powerful to completely substitute for taxes (Loewenstein and Chater 2017; Thaler and Sunstein 2021).

We estimate that the labels increase the variance of distortions. The label effects have substantial variance: the estimated coefficients of variation (standard deviation of τ / absolute average τ) are 3.0 and 2.0 when pooling across labels in the cars and drinks experiments, respectively. Fuel economy labels add purely idiosyncratic noise to consumers' decisions: the label effects τ have statistically zero covariance with bias and externalities. Sugary drink labels (especially the stop sign and graphic warnings) are adversely targeted: they reduce WTP for sugary drinks more for consumers estimated to have less bias.

Pooling across labels within each experiment to increase precision, we find that although both fuel economy and health warning labels move average WTP in the "right direction," they do not necessarily increase total surplus in our model. With no tax, hypothetical labels with homogeneous effects equal to the estimated average τ would substantially increase total surplus. However, the increased distortion variance erases much of the surplus gain from sugary drink labels and actually causes fuel economy labels to reduce total surplus. In sensitivity analyses, we find that plausible alternative values of the effects on distortion variance can change the sign of total surplus effects and have enormous implications for the magnitudes. Similarly, alternative pass-through values within the range of previous estimates also significantly affect the results by changing the relative importance of average effects versus distortion variance.

When the government can also set the optimal tax, the average treatment effects are irrelevant, so both fuel economy and sugary drink labels reduce total surplus in our model. This is important because some U.S. cities now tax sugary drinks, and the U.S. uses corporate average fuel economy standards and gasoline taxes to increase demand for higher-MPG cars. If those policies are set close to optimally, our estimates suggest that adding labels might *reduce* total surplus in our model by adding noise to consumer choice.

As an example of how average effects can be misleading, compare sugary drink warning labels to the magnified nutrition facts label. Our point estimates suggest that graphic warning labels reduce average WTP for sugary drinks more than nutrition facts labels, so an evaluation based only on average effects might suggest that graphic warning labels are preferred. However, graphic warning labels cause larger increases in distortion variance than nutrition facts: their treatment effects have higher variance overall (possibly because graphic warnings have more ambiguous interpretations), and their effects are smaller for more biased consumers. Thus, our point estimates suggest that graphic warning labels deliver lower total surplus in our model, despite having larger average effects. Additional survey evidence suggests that graphic warning labels are highly aversive, which further worsens their total surplus effects.

Our theoretical analysis builds on influential work by Farhi and Gabaix (2020), who derive first-order conditions for socially optimal NPI intensity in a perfectly competitive market with constant marginal costs, under parametric assumptions about how NPIs affect utility. We use techniques from Weyl and Fabinger (2013) to study the welfare effects of NPIs with fewer parametric assumptions and in a more general setting with market power and nonlinear production costs. This makes our analysis applicable to the many markets with incomplete pass-through, including cars and sugary drinks (Sallee 2011; Allcott, Lockwood and Taubinsky 2019b). We also go beyond Farhi and Gabaix (2020) by deriving formulas based on empirically measurable sufficient statistics and then estimating those parameters in two applications.

Our empirical work builds on a rich literature studying NPIs in a variety of domains.[3] Our cars

[3]Researchers have studied NPIs in contexts such as charitable giving (Exley and Kessler 2019), education (Hastings and Weinstein 2008; Jensen 2010; Allende, Gallego and Neilson 2019), energy conservation (Schultz et al. 2007; Nolan et al. 2008; Allcott 2011, 2015; Allcott and Rogers 2014; Ito, Ida and Tanaka 2018; Knittel and Stolper 2019), retirement savings (Madrian and Shea 2001; Carroll et al. 2009; Chetty et al. 2014; Beshears et al. 2015; Goldin and Reck 2020), smoking (Gine, Karlan and Zinman 2010; Thrasher et al. 2012; Hammond et al. 2012; Cantrell et al. 2013; Brewer et al. 2016), social program takeup (Bhargava and Manoli 2015; Finkelstein and Notowidigdo 2019),

experiment extends the experimental literature on durable good energy use information disclosure (e.g. Davis and Metcalf 2016; Allcott and Sweeney 2017; Houde 2018; Allcott and Knittel 2019). Our drinks experiment extends the experimental literature on sugary drink warning labels (Donnelly et al. 2018; Moran and Roberto 2018; Grummon et al. 2019b; Grummon et al. 2019a; Grummon and Hall 2020; Hall et al. 2022) and calorie information provision (Bollinger, Leslie and Sorensen 2011). While many of these papers estimate heterogeneous treatment effects, none estimate the parameters that we show are required for welfare analysis. By clarifying the required parameters, we hope that our paper provides a template for moving the literature toward welfare-relevant measurement of heterogeneous treatment effects.

Our paper extends a smaller group of papers that quantitatively evaluate the welfare effects of NPIs (e.g., Carroll et al. 2009; Handel 2013; Chetty et al. 2014; Allcott and Kessler 2019; Thunström 2019; Ambuehl, Bernheim and Lusardi 2022; Butera et al. 2022). These papers study different NPIs than we study, and (with the exception of Ambuehl, Bernheim and Lusardi (2022), who study financial literacy) they do not offer welfare evaluation frameworks like ours that are portable across different NPIs. Our focus on sufficient statistics that translate treatment effect heterogeneity to welfare implications further distinguishes this paper from previous work. This connects to a separate literature studying the targeting of redistributive (instead of corrective) policies (Currie and Gahvari 2008; Alatas et al. 2016; Finkelstein and Notowidigdo 2019).

Most broadly, our work extends the behavioral public economics literature studying optimal policy in the presence of behavioral bias; see Mullainathan, Schwartzstein and Congdon (2012) and Bernheim and Taubinsky (2018) for reviews. Our work shares two limitations with this other work. First, measuring behavioral bias requires assumptions that are sometimes subject to debate. Second, estimating individual-level variation in bias requires empirical designs that are challenging to implement in controlled experiments and especially challenging in naturalistic settings. We use more artefactual designs in part for this reason.

Sections 1–5 present the theoretical framework, experimental designs, parameter estimation, welfare analysis, and conclusion, respectively. All proofs are in Appendix A.3.

1 Theoretical Framework

1.1 Setup

We model a unit mass of consumers who choose whether or not to buy a good that delivers utility v at price p. We assume quasilinear utility, so the utility gain from buying the good is $v - p$. Consumers overestimate their utility from the good by amount γ, due to forces such as inattention, projection bias, and imperfect information. We refer to γ as consumer "bias."

The government has two instruments: a linear tax t on producers and a non-standard policy instrument (NPI). Because the tax is on producers, it affects consumer demand only through producer prices, so it is not heterogeneously perceived due to salience effects, as in Chetty, Looney

vaccinations (Milkman et al. 2021, 2022), and voting (Gerber and Rogers 2009).

and Kroft (2009), Taubinsky and Rees-Jones (2018), and others. The NPI increases willingness-to-pay by amount $\sigma\tau$, where $\sigma \in \mathbb{R}$ is the intensity of the NPI and τ covaries arbitrarily with v and γ. Accounting for the effects of bias and the NPI, consumers buy the good if $v + \gamma + \sigma\tau > p$. We assume that the joint distribution $F(v, \gamma, \tau)$ generates smooth demand curves, but make no other assumptions. Allowing τ to be a function of v and γ would be mathematically equivalent to imposing a particular covariance between τ and v and γ. We define $D(p, \sigma)$ as the demand curve, D'_p as its derivative with respect to p, and $\varepsilon_D = -pD'_p/D$ as its elasticity.

The NPI also imposes a direct psychic cost or benefit. For example, cigarette graphic warning labels might make smokers feel worse, while labels promoting healthy foods might make consumers feel better. We define $\sigma\iota_1$ and $\sigma\iota_0$ to be these direct effects on consumers who buy and don't buy the good, respectively. We define $I(\sigma, p)$ as aggregate psychic benefits, which may indirectly depend on p if $\iota_1 \neq \iota_0$ for some consumers.

On the supply side, we follow the Weyl and Fabinger (2013) model of symmetric competition, where firm j's cost of producing quantity q_j is $c(q_j)$. We limit to symmetric equilibria, giving aggregate quantity q and market price $p(q)$. Weyl and Fabinger (2013) show that a wide range of firm conduct models can be captured by an elasticity-adjusted Lerner index $\theta := \frac{p - c'(q) - t}{p}\varepsilon_D$ that is constant. For example, homogeneous-product Bertrand competition has $\theta = 0$, Cournot competition with n firms has $\theta = 1/n$, monopoly has $\theta = 1$, and the Delipalla and Keen (1992) conjectures model also has constant θ. We define $\rho := \frac{dp}{dt}$ as the pass-through rate.

Total surplus W is the sum of consumer surplus, producer surplus, and government revenue. In an efficient market, all consumers with $v > c'(q)$ would buy the good. We refer to $(\gamma + \sigma\tau - \mu)$ as the "net distortion" to consumer choice caused by bias, the NPI, and the markup. We ignore externalities in this section for simplicity, but without loss of generality. If consumption generates an externality ϕ, then our formulas for distortions and total surplus still hold after replacing γ with $(\gamma + \phi)$.

In our empirical applications, consumers choose between two goods. This framework applies directly if we redefine v, p, γ, and $\sigma\tau$ as the *relative* utility, price, bias, and NPI effect for the first good compared to the second, and let $D(p)$ be the demand for the first good. We consider the general case of many goods with endogenous prices in Appendix A.4.

For any function $X(v, \gamma, \sigma, \tau)$ we define $\mathbb{E}_m[X(v, \gamma, \sigma, \tau)]$ to be the conditional expectation of X over the set of marginal consumers, i.e., the set $\{(v, \gamma, \tau) | v + \gamma + \sigma\tau = p\}$. With a slight abuse of notation, we define

$$\frac{\partial}{\partial \sigma}\mathbb{E}_m[X(v, \gamma, \sigma, \tau)] := \frac{d}{d\sigma'}\int_{\{(v,\gamma,\tau)|v = p - \sigma\tau - \gamma\}} X(v, \gamma, \sigma', \tau) dF\,|_{\sigma' = \sigma}\,. \tag{1}$$

for any function X. The partial derivative notation symbolizes that we take the derivative of the function X, but not of the set of consumers over which the expectation is taken. We use analogous notation for variance and covariance operators.

1.2 Motivating Examples

Before presenting formal results about the welfare effects of NPIs, we use stylized examples to demonstrate the core intuitions. To simplify, we consider the effects of changing σ from 0 to 1. We also assume for the examples that v is distributed uniformly and independently of γ, and that for each tuple $(\gamma, \sigma, \tau, t)$ there is always a consumer on the margin.[4] We abstract away from psychic costs, so that an NPI that exactly offsets the bias (i.e., $\tau = -\gamma$ for all consumers) delivers the social optimum in the absence of market power or taxes. All conclusions in these examples can be derived formally from Proposition 1.

For the first five examples, we assume a competitive market. For the first three, we also assume constant marginal cost.

Example 1. Consider a sugary drink market with two types of consumers. "Oblivious" consumers are inattentive to health harms from sugary drinks ($\gamma_o > 0$) and don't pay attention to health warning labels ($\tau_o = 0$). "Health-conscious" consumers are initially unbiased ($\gamma_h = 0$), but warning labels make them over-sensitized to health ($\tau_h < 0$). On average, consumers over-consume sugary drinks ($\mathbb{E}[\gamma] > 0$), and the label reduces consumption ($\mathbb{E}[\tau] < 0$). Thus, a naive analysis might conclude that the labels increase welfare because they change average behavior in a "good" direction. In reality, however, the label reduces total surplus because it is poorly targeted: it distorts the choices of unbiased consumers without improving the choices of biased consumers.

This example is plausible: psychological theories such as those of Ungemach et al. (2017) predict that product labels can serve as "signposts" that activate "dormant objectives" (Bond, Carlson and Keeney 2008) such as health-consciousness or environmentalism, potentially causing over-sensitivity to these attributes. Value activation could cause poor targeting if health-conscious or environmentalist consumers are less biased because they are already aware of a product's nutritional or environmental characteristics.

Example 2. Now suppose there are two different types of consumers. "Overconsumers" (2/3 of the population) have $\gamma_o > 0$, and an NPI offsets half of their bias ($\tau_o = -\gamma_o/2$). "Underconsumers" (1/3 of the population) have bias of the same magnitude but in the opposite direction ($\gamma_u = -\gamma_o < 0$), and the NPI fully offsets their bias ($\tau_u = -\gamma_u$). On average, consumers overconsume the good ($\mathbb{E}[\gamma] > 0$), but the NPI does not change consumption ($\mathbb{E}[\tau] = 0$). Thus, a naive analysis might conclude that the NPI doesn't increase welfare because it doesn't change average behavior in a "good" direction. In reality, however, the NPI increases total surplus because it is well-targeted to offset both positive and negative biases.

This example is relevant for many information disclosure applications: consumers have wide dispersion in beliefs about product characteristics such as calorie content and energy use, and information is designed to reduce this dispersion (Bollinger, Leslie and Sorensen 2011; Allcott and Taubinsky 2015).

[4] In other words, we assume that there is a value of v in the support of the distribution such that $v - p + \gamma + \sigma\tau = 0$.

Example 3. The previous two examples illustrate how a negative covariance between NPI effects τ and bias γ is key to increasing total surplus. However, this does not guarantee that an NPI increases total surplus. Suppose that the NPI effects are now $\tau = -\gamma + \varepsilon$, where ε is mean-zero noise that is independent of γ. Thus, the covariance between τ and γ is negative and $\mathbb{E}[\tau|\gamma] = -\gamma$, meaning that on average, the NPI fully offsets the bias. Without the NPI, consumers buy the good if $v + \gamma \geq p$, while with the NPI, consumers buy if $v + \varepsilon \geq p$. Thus, the NPI eliminates one distortion (γ) but adds another (ε), and it will decrease allocative efficiency if ε has sufficiently high variance.

This example is relevant in settings where consumers might interpret information differently, or when information is not personalized. For example, energy cost labels on appliances and cars in the U.S. report energy costs at national average utilization and energy prices. This averaging adds noise relative to each consumer's personalized values, as studied by Davis and Metcalf (2016).

Example 4. Suppose that consumers have a homogeneous bias γ. An NPI fully debiases half of consumers ($\tau_1 = -\gamma$) but does not affect that other half ($\tau_2 = 0$). Supply is fixed at some value $q^\dagger$, which implies pass-through $\rho = 0$, since the equilibrium price must always equal the value at which demand meets the fixed supply. Without the NPI, the market is efficient, despite the consumer bias. This is because the share of consumers buying the good always equals $q^\dagger$, and when bias is homogeneous, the consumers buying the good will always be the share $q^\dagger$ of consumers with the highest v. However, with the NPI, the consumers buying the good will be the $q^\dagger$ consumers with the highest $v + \gamma + \tau$, which will not be the $q^\dagger$ consumers with the highest v when $\gamma + \tau$ is heterogeneous. Thus, the NPI reduces total surplus by increasing the variance of the net distortion.

While few markets have fully inelastic supply, we will see that this intuition applies to any market with $\rho < 1$, which is empirically very common.

Example 5. As in the previous example, suppose that consumers have homogeneous γ and the NPI fully debiases half of consumers without affecting the other half. Without the NPI, a Pigouvian tax of $t = \gamma$ can achieve the first best. With the NPI, however, the net distortion $\gamma + \tau$ is heterogeneous, so a uniform tax cannot achieve the first best whenever $\rho > 0$. Thus, the NPI again reduces total surplus by increasing the variance of the net distortion. However, if the tax is constrained to $t = 0$, the NPI increases total surplus for a value of ρ sufficiently close to 1. This illustrates how taxes and NPIs can be substitutes. Alternatively, the tax and NPI could be complements if NPIs make net distortions more homogeneous.

This example is crucial because policymakers control both taxes and NPIs in many markets.

Example 6. Suppose that there is no tax, and producers have market power, giving markup $\mu > 0$. Any bias less than the markup ($0 < \gamma \leq \mu$) reduces the net distortion—the two market failures offset. Thus, any NPI that offsets bias ($-\gamma \leq \tau < 0$) reduces total surplus. At any tax t, the first best obtains if there is homogeneous bias of $\gamma = \mu + t$, so an NPI could only reduce total surplus.

These examples illustrate that the welfare effects of NPIs depend on many factors other than whether they change average behavior in the "right direction." This underscores the importance of a full characterization that accounts for heterogeneity, pass-through, market power, and taxes, which we present below.

1.3 Effects of NPIs on Total Surplus

Proposition 1 formally considers the effects of a marginal increase in the NPI intensity in two cases: exogenous taxes (perhaps constrained by technical or political factors) and optimal taxes.

Proposition 1. *Assume that $\frac{d}{dp}\varepsilon_D$ and $\frac{d}{d\sigma}\varepsilon_D$ are negligible whenever $\mu > 0$. At a fixed tax t, the total surplus change from a marginal increase in NPI intensity is*

$$\frac{dW}{d\sigma} = \frac{1}{2}\left((1-\rho)\frac{\partial}{\partial\sigma}Var_m\left[\gamma+\sigma\tau\right] + \rho\frac{\partial}{\partial\sigma}\mathbb{E}_m\left[(\gamma+\sigma\tau-t-\mu)^2\right]\right)D_p' + \frac{\partial I}{\partial\sigma} + (1-\rho)\mathbb{E}_m[\tau]\frac{\partial I}{\partial p}. \tag{2}$$

At the optimal tax $t^* = \mathbb{E}_m\left[\gamma+\sigma\tau\right]-\mu$, the total surplus change from a marginal increase in NPI intensity is

$$\frac{dW}{d\sigma} = \frac{1}{2}\frac{d}{d\sigma}Var_m\left[\gamma+\sigma\tau\right]D_p' + \frac{\partial I}{\partial\sigma} + \mathbb{E}_m[\tau]\frac{\partial I}{\partial p}. \tag{3}$$

To give intuition for the proposition, we initially assume no psychic costs ($I = 0$), although we return to this issue at the end of the subsection. First, consider the special case of equation (3) with no tax, full pass-through, and no markup ($t = 0$, $\rho = 1$, $\mu = 0$), giving

$$\frac{dW}{d\sigma} = \frac{1}{2}\frac{\partial}{\partial\sigma}\mathbb{E}_m\left[(\gamma+\sigma\tau)^2\right]D_p'. \tag{4}$$

After expanding the second moment, the equation becomes

$$\frac{dW}{d\sigma} = \frac{1}{2}\frac{\partial}{\partial\sigma}\left\{\left(\mathbb{E}_m\left[\gamma\right]+\sigma\mathbb{E}_m\left[\tau\right]\right)^2 + 2\sigma Cov_m\left[\gamma,\tau\right] + \sigma^2 Var_m\left[\tau\right]\right\}D_p'. \tag{5}$$

Since D_p' is negative, more negative values of the terms inside brackets imply more positive total surplus effects. The first term, $\left(\mathbb{E}_m\left[\gamma\right]+\sigma\mathbb{E}_m\left[\tau\right]\right)^2$, captures the standard intuition that NPIs increase total surplus if they change average behavior in the "right direction"—i.e., have the opposite sign of average bias. The second term, $2\sigma Cov_m\left[\gamma,\tau\right]$, generalizes Examples 1 and 2, which showed how NPIs increase surplus when the treatment effects covary negatively with bias. The third term, $\sigma^2 Var_m\left[\tau\right]$, generalizes Example 3, which showed that surplus decreases with treatment effect heterogeneity that does not covary with bias.

Now consider the special case of equation (2) with zero pass-through ($\rho = 0$). The equation becomes identical to equation (3), where the sign of the total surplus effect depends only on $Var_m\left[\gamma+\sigma\tau\right]$. With fixed aggregate supply, the only way for an NPI to improve allocative efficiency is to reduce the variance of the net distortion, as Example 4 illustrated. Equation (2) shows that between the two extremes of constant marginal cost ($\rho = 1$) and fixed supply ($\rho = 0$), the total surplus effect is a convex combination of these extremes, weighted by ρ.

Next, consider equation (3), the effect of an NPI when the government also sets an optimal tax. The intuition is similar to the previous case: when the government can optimally control aggregate quantity by setting a tax equal to the average marginal net distortion $\mathbb{E}_m[\gamma + \sigma\tau - \mu]$, the only way for an NPI to improve allocative efficiency is again to reduce the variance, as Example 5 illustrated.

The net distortion also depends on the markup μ, as Example 6 illustrated. When a positive bias offsets distortions from market power, using an NPI to offset the bias is inefficient. Conversely, when a negative bias depresses demand, this is especially harmful in the presence of market power, and NPIs that offset the bias are especially beneficial. This is reflected in the term $\mathbb{E}_m\left[(\gamma + \sigma\tau - t - \mu)^2\right]$ in equation (2). Because the optimal tax is set equal to the average marginal net distortion (including μ), μ does not appear in equation (3).

Proposition 1 shows that the NPI has two effects on psychic benefits. $\frac{\partial I}{\partial \sigma}$ is the direct effect on psychic benefits holding prices constant. $(1-\rho)\mathbb{E}_m[\tau]\frac{\partial I}{\partial p}$ is an indirect effect: the NPI affects prices, which in turn affect how many consumers incur the psychic benefits associated with purchasing or not purchasing the product, i.e., $\sigma\iota_1$ versus $\sigma\iota_2$. When taxes are endogenous, the NPI affects price in two ways: through its effect on prices holding the tax constant, and through its effect on the optimal tax. Because of this, the factor multiplying $\frac{\partial I}{\partial p}$ given an optimal tax is higher.

1.4 Asymmetric Paternalism, Libertarian Paternalism, and Deliberative Competence in Markets

Our results clarify how asymmetric paternalism, libertarian paternalism, and improvements in "deliberative competence" (Ambuehl, Bernheim and Lusardi 2022) translate to market settings. Camerer et al. (2003, page 1212) write that "a regulation is asymmetrically paternalistic if it creates large benefits for those who make errors, while imposing little or no harm on those who are fully rational." Thaler and Sunstein (2003, page 175) write that paternalism is "libertarian" "if no coercion is involved," and Thaler and Sunstein (2008, page 6) write that "a nudge ... is any aspect of choice architecture that alters people's behavior in a predictable way without forbidding any options or significantly changing their economic incentives."

Except in perfectly competitive markets with fully elastic supply, asymmetric and libertarian paternalism are not possible in markets, because NPIs indirectly change equilibrium prices. In Appendix A.2, we show that the effect of a marginal NPI increase on price is

$$\frac{dp}{d\sigma} = (1-\rho)\mathbb{E}_m[\tau]. \tag{6}$$

NPI-induced price changes harm or benefit all consumers, including those who are fully rational, violating the definition of asymmetric paternalism.[5] Furthermore, this shows how NPIs indirectly affect economic incentives, which is not contemplated in the definition of a "nudge." Note that

[5] Appendix A.2 provides formulas for the effect of NPIs on consumer and producer surplus.

the price change is not "insignificant": equation (6) shows that unless $\rho = 1$, NPIs have first-order effects on prices that scale with the average τ.

A natural modification might be to define an instrument as "asymmetrically paternalistic" if it weakly improves decision quality for all consumers without affecting decisions by unbiased consumers: $|\gamma + \tau| \leq |\gamma|, \forall (\gamma, \tau)$. Relatedly, Ambuehl, Bernheim and Lusardi (2022) say that an NPI improves "deliberative competence" if $\mathbb{E}\left[(\gamma + \sigma\tau)^2\right] < \mathbb{E}\left[\gamma^2\right]$ in our notation. (See Appendix A.1 for more in-depth discussion.) Proposition 1.3 and the preceding examples show that these concepts do not guarantee total surplus gains outside of the special case of no taxation, full pass-through, no markup, and no psychic costs.

2 Experimental Design and Data

Our empirical analyses focus on two markets (automobiles and packaged drinks) that both involve externalities, possible consumer bias, and various types of information labels. There are many models of automobiles and types of packaged drinks, but we simplify the design and analysis to focus on binary choices between closely comparable low- versus high-fuel economy sedans and sugary versus sugar-free drinks.

For cars, the potential biases we study are imperfect information about or inattention to gasoline costs, following Busse, Knittel and Zettelmeyer (2013), Allcott and Wozny (2014), Allcott and Taubinsky (2015), Sallee, West and Fan (2016), Allcott and Knittel (2019), and others. For sugary drinks, the potential biases we study are self-control problems and imperfect information about health costs, following Allcott, Lockwood and Taubinsky (2019a) and others.

We use two randomized experiments to identify utility v, bias γ, and treatment effects τ. The two experiments share a common format:

1. introductory questions,
2. a baseline multiple price list (MPL) that elicits relative valuations of the two choices, and
3. an endline MPL with information labels.

Screenshots from the two experiments are available in Appendix B.

2.1 Cars Experiment

Sample source. We fielded the cars experiment in April 2021 on Amerispeak, an online panel managed by the National Opinion Research Corporation (NORC) at the University of Chicago. AmeriSpeak is a probability-based panel designed to be representative of the U.S. population. NORC randomly selects U.S. households and recruits them by mail, telephone, and in-person visits in an effort to minimize refusals. To minimize fatigue, panelists take only an average of two to three surveys per month. This design makes AmeriSpeak more plausibly representative and higher quality than typical survey panels that allow anyone to opt in and take as many surveys as they like.

Introductory questions. The experiment began by asking people whether they currently have a car; people who did not were screened out. Participants were then asked their state of residence, the number of miles they (and anyone else in their household) drove their primary car in 2019, and the average price they paid for gas in 2019. We asked about 2019 because we wanted to ignore disruptions caused by the coronavirus pandemic. To ensure accurate responses, participants were asked to enter their annual mileage twice, and we asked people to confirm their gas price if their reported price differed from their state's 2019 average by more than $0.50 per gallon.

Participants were told,

For the entire survey, imagine the following scenario:

- *Your primary car broke down and can't be used anymore.*
- *To replace your primary car, you will lease a car for the next 3 years.*
- *Any dealership would offer you the same lease contract, so there's no need to shop around.*
- *You (and anyone else in your household) would drive this car the amount that you told us earlier.*

The experiment elicited yearly WTP to lease four mid-size sedans: the Honda Accord, Nissan Altima, Subaru Legacy, and Ford Fusion. The Accord, Altima, and Legacy each get 30 miles per gallon (MPG), while the Fusion gets 23 MPG.

In the final introductory questions, participants were told, "We'd like to learn how much you would like to have each of the 4 cars on the next 4 screens, setting aside gas costs. In order to tell us that, please **imagine for the next 4 screens that gas is free** and that you'd still drive the [*2019 miles driven*] miles per year you told us earlier." Participants were shown the four cars and asked to write the maximum amount they'd pay per year to lease each one. In this elicitation and again in the MPLs described below, participants were shown each car's picture and key characteristics (including fuel economy), along with a link to more information on the manufacturer's website.

Baseline MPLs. Next, participants were told, "For the rest of the survey, assume that **gas is NOT free**, and the average price for the next three years will be the same as what you told us you paid in 2019." Using two baseline MPLs, we elicited people's relative WTP for the Altima vs. the Fusion and for either the Accord or Legacy vs. the Fusion. We used an adaptive MPL design that offered a series of binary choices between two cars at different prices. We randomized (i) whether the Altima-Fusion or Accord/Legacy-Fusion MPL was first, (ii) which of the Accord/Legacy appeared, and (iii) which car appeared on the left or right for each MPL.

In the adaptive MPL, the price of the car on the right was always $2000 per year. The price of the car on the left began at $2000 per year and would adjust for each subsequent question, reaching as low as $500 or as high as $3500, until a person's relative WTP was determined within $100.

People who were willing to pay less than \$500 or more than \$3500 for the car on the left were asked to report their maximum or minimum WTP in an open-answer box.

Endline MPLs with information labels. The endline MPLs were identical to the baseline MPLs, with two exceptions. First, we exchanged the Accord and Legacy, so people who had valued one at baseline now valued the other. Second, participants were randomly assigned to see fuel economy information labels just below the cars' pictures. Figure A3 presents an example of an endline binary choice screen, including each car's picture, a fuel economy label, and key characteristics.

There were five randomly assigned treatment conditions: full MPG label, average cost label, personalized fuel cost label, SmartWay label, and no label (control). Panel (a) of Figure 1 presents the four labels.

The full MPG and average cost labels were taken from the U.S. Environmental Protection Agency's official "Fuel Economy and Environment" label. In these treatment conditions, participants were told that the labels "are part of the labels that must be posted on car windows at dealerships" and that they "report averages for each car at typical usage patterns." The EPA's annual fuel costs are \$1850 for the 23-MPG Fusion and \$1450 for the 30-MPG Accord, Altima, and Legacy, assuming 15,000 miles driven per year and a gas price of \$2.87 per gallon.

The personalized cost label has the same format as the average cost label, but the numbers were calculated using each respondent's 2019 miles driven and gas price. In this condition, the participants were told that "the fuel cost numbers for each car are based on the annual miles driven and gasoline prices that you told us earlier."

The SmartWay label is an official EPA environmental certification given to vehicles that have relatively low emissions of greenhouse gases and local air pollutants. In this condition, participants were told that "Cars that meet SmartWay Vehicle standards for fuel economy and environmental performance will have SmartWay Vehicle labels. Cars that do not meet these standards will not have SmartWay Vehicle labels." The three 30-MPG cars are all SmartWay vehicles, while the 23-MPG Fusion is not.

Incentivization. Before the MPLs, participants were told that their responses would be incentivized. Specifically, participants were told that they would be paid an amount proportional to the "value" (i.e., consumer surplus) they would receive from an upcoming MPL choice. To compute this consumer surplus, we define f as the WTP if gas is free and g as the annual gas cost implied by the participant's 2019 miles driven and gas price and the vehicle's fuel economy rating. The "true" value implied by those previous survey responses is

$$\hat{v} = f - g, \tag{7}$$

and the consumer surplus at price p is $\hat{v} - p$.

The adaptive MPLs did not elicit choices at all possible price levels. We used the participant's responses to fill out the full "background" MPL that has one row for each possible price level. We then randomly selected one row from all baseline and endline background MPLs and computed the consumer surplus from the participant's choice in that row. Participants were told that it was in their best interest to answer all questions honestly, and were given a link to a page that contained the full instructions of how the adaptive MPL was used to fill out the "background" MPL. We rescaled that consumer surplus to be between \$1 and \$10, where \$1 and \$10 were set to the minimum and maximum possible consumer surplus achievable across all rows of all MPLs. We paid participants in dollar-equivalent "AmeriPoints" through the AmeriSpeak system.

Final sample. Out of 2,595 people who began the survey, 2,089 qualified for inclusion by reporting that they currently owned a car and successfully completing the survey. To increase the proportion of high-quality responses and improve precision in treatment effect variance estimates, we additionally dropped the 822 participants in the top or bottom five percent of annual gas cost, WTP if gas is free, estimated bias, or baseline-endline WTP change. Excluding these outlying responses is conservative: if we do not drop these 822 participants, the $Var[\tau]$ point estimate becomes much larger and $Cov[\phi, \tau]$ becomes more positive, strengthening the qualitative conclusion that fuel economy labels reduce welfare. The final sample size is 1,267 participants.

Appendix Table A1 presents descriptive statistics. To maximize precision, we do not weight the sample in our analyses. The unweighted sample has roughly similar demographics to the US population, although the sample has a higher share of people with a college degree. The average participant reported driving about 10,800 miles and paying \$2.79 per gallon for gas in 2019, both of which are very similar to the national averages. The average participant reported being willing to pay \$2,770 per year to lease one of the four cars, if gas were free, and the average relative WTP for the lower-MPG car from the baseline MPLs is about \$1,550 per year. Appendix Table A3 shows that the treatment groups are balanced on observables, except that the personalized fuel cost group is slightly younger and less white than the control group.

2.2 Drinks Experiment

Sample source. We fielded the experiment in fall 2021, recruiting participants through Facebook ads. See Appendix Figure A4 for an example ad. We recruited participants ourselves because we needed to ship drinks to participants' home addresses, and most online panels don't allow this or would charge prohibitively high prices.

Introductory questions. The experiment was run in two stages separated by three days. The first stage elicited contact information, demographic information, and bias proxies for nutrition knowledge and self-control. The bias proxy survey questions and variable construction are identical to Allcott, Lockwood and Taubinsky (2019a). Nutrition knowledge was measured with 28 questions from the General Nutrition Knowledge Questionnaire (GNKQ), which is widely used in the public health literature.[6] The nutrition knowledge bias proxy variable is the share of the 28 questions that

[6] One example question is, "If a person wanted to buy a yogurt at the supermarket, which would have the least sugar/sweetener?" The four possible responses were "0% fat cherry yogurt," "Plain yogurt," "Creamy fruit yogurt," and "Not sure." A second example is, "Which is the main type of fat present in each of these foods?" The five possible responses were "Polyunsaturated fat," "Monounsaturated fat," "Saturated fat," "Cholesterol," and "Not sure." This question was asked about olive oil (correct answer: monounsaturated), butter (saturated), sunflower oil

the participant answered correctly. Self-control was measured by people's level of agreement with the statement, "I drink soda pop or other sugar-sweetened beverages more often than I should." There were four responses: "Definitely," "Mostly," "Somewhat," and "Not at all." To construct the self-control bias proxy variable, we code those responses as 0, 1/3, 2/3, and 1, respectively. Allcott, Lockwood and Taubinsky (2019a) document that this question has high test-retest reliability, and people's ratings of their own self-control line up well with their spouse's ratings of them.

Baseline MPLs. Three days later, we sent emails inviting participants to complete the second stage. We included this three-day delay after the bias proxy questions because we did not want participants to have nutrition and self-control problems at top of mind as we elicited their valuations of sugary drinks. The second stage began by asking people to rate six sugary drinks from most favorite to least favorite: Minute Maid Lemonade, Coca-Cola, Pepsi, Seagram's Ginger Ale, Sprite, and Crush. Participants were told that they would be offered a series of choices between their three most favorite drinks and three substitutes.

Using three baseline MPLs, we elicited WTP for each participant's three favorite drinks relative to a sugar-free substitute with similar flavor.[7] As in the cars experiment, we used an adaptive MPL design that was still incentive-compatible because it was used to fill out a background MPL. We randomized the order of the three MPLs. Each choice screen showed the pictures of the drinks and a link to the nutrition facts.

In the adaptive MPL, the price of the drink on the right was always $4, which is close to the typical market price of a 12-pack. The price of the drink on the left began at $4 and would adjust for each subsequent question, reaching as low as $1.20 or as high as $6.80, until a person's relative WTP was determined within $0.40. People who were willing to pay less than $1.20 or more than $6.80 were asked to report their maximum or minimum WTP in an open-answer box.

Endline MPLs with information labels. The endline MPLs were identical to the baseline MPLs, except that participants were randomly assigned to see magnified labels next to the drinks' pictures. There were four randomly assigned treatment conditions: nutrition facts label, stop sign warning label, graphic warning label, and no label (control). Panel (b) of Figure 1 presents the three labels.

In the nutrition facts label condition, participants were told that "we will add the official nutrition facts label to each drink." In the stop sign and graphic warning label conditions, participants were told, "Now, we will add nutritional warning labels to drinks with added sugar. Drinks without added sugar will not have warning labels." The labels state, "WARNING: Drinking beverages with added sugar contributes to obesity, diabetes, and tooth decay." Both warning labels are taken from prior studies that found that they reduced demand for sugary drinks (Donnelly et al. 2018; Grummon et al. 2019a). Grummon et al. (2019b) recommend the version of the stop sign label we use because it appears to have larger effects on demand than other designs.

(polyunsaturated), and eggs (cholesterol).

[7]The pairings of sugary drinks and sugar-free alternatives were: Minute Maid Lemonade and LaCroix Lemon, Coca-Cola and LaCroix Cola, Pepsi and LaCroix Cola, Seagram's Ginger Ale and 365 Ginger Sparkling Water, Sprite and LaCroix Lime, and Crush Orange and Bubly Orange.

Finally, we included several questions to measure the labels' direct psychic benefits or costs. Participants were asked, "Imagine that drink manufacturers could print the above label on some drink containers. Also imagine that the computer selects you to receive the 12 pack of [a randomly chosen sugary drink]. Would you prefer to receive drink containers with that label or without it?" The survey then asked participants why they did or did not want the labels and used an MPL to elicit WTP to receive or avoid the labels.

Incentivization. Participants were told that they would receive a $15 completion payment via online gift card, and that their responses would be incentivized. Specifically, participants were told, "20 percent of respondents will be randomly selected to receive a 12-pack of drinks they chose in one of the survey questions plus an online gift card for the difference between $15 and the price of the drinks. The remaining respondents will receive their entire $15 reward in the form of an online gift card. For those who are selected to receive drinks, we will send the drinks to your address directly from either Walmart or Amazon." We initially selected one-third of participants for incentivization but later reduced the proportion due to logistical difficulties. Overall, we shipped drinks to 22 percent of participants.

Final sample. Out of the 5,744 people who consented to participate, 3,653 completed the first stage (including passing the attention check) and were invited to take the second survey. Of those, 2,619 completed the second stage (including passing the second attention check); this is our final sample size.

Table A2 presents descriptive statistics. The sample is wealthier and better educated than the US population. The average participant had a nutrition knowledge score of 0.70 and a self-control rating of 0.41. This latter number is lower than the US population average reported in Allcott, Lockwood and Taubinsky (2019a), perhaps because our recruitment ads were designed to recruit soda drinkers who would want to receive drinks as part of the experiment. Appendix Table A3 shows that the treatment groups are balanced on observables, except that the stop sign label group is slightly younger than the control group.

3 Experimental Results

This section presents reduced-form analyses of our two experiments and calculates sufficient statistics used in the welfare analysis in Section 4. We index consumers by i, product pairs (e.g., Altima & Fusion, or Coke & LaCroix Cola) by j, and choice occasions (baseline or endline MPLs) by $s \in \{1, 2\}$. All relative values between two goods (valuation v, bias γ, etc.) are for the more harmful relative to the less harmful good—i.e., the 23-MPG car minus the 30-MPG car, and the sugary drink minus the zero-calorie drink.

We estimate population-level parameters instead of limiting to marginal consumers. In addition to the precision gains, this may increase generalizability, as market prices (and thus marginal consumers) vary over time and place. The parameter estimates are similar when we limit the sample to observations with WTP closer to the market prices at the time of the experiment; see

Appendix Table A5.

3.1 Bias and Externalities

Both experiments deliver bias and externality estimates, which we denote as $\hat{\gamma}_{ij}$ and $\hat{\phi}_{ij}$, respectively. We assume that these are noisy measures of the true γ_{ij} and ϕ_{ij}, with mean-zero measurement error.

3.1.1 Cars Experiment

In the cars experiment, we estimate biases from imperfect information about or inattention to gasoline costs. A key advantage of our design is that we have an objective measure of bias within the experiment: bias is the extent to which people fail to maximize their expected experimental incentive by valuing the lower-MPG too high or too low on the MPLs. We define w_{ij1} as the baseline relative WTP for the lower-MPG car in product pair j, and $\hat{v}_{ij}$ is the relative "true" value defined in equation (7): WTP if gas is free minus gas costs. The bias estimate is

$$\hat{\gamma}_{ij} = w_{ij1} - \hat{v}_{ij}. \tag{8}$$

$\hat{\gamma}_{ij} = 0$ corresponds to a relative WTP that maximizes the hypothetical consumer surplus from leasing a car; this also corresponds to MPL choices that maximize the actual experimental incentive. $\gamma_{ij} > 0$ reflects a relative WTP above the incentive-maximizing level, while $\hat{\gamma}_{ij} < 0$ reflects a relative WTP that is too low.

Our externality proxy is the annual climate change damages from the additional gasoline consumption from driving the 23-MPG car instead of the 30-MPG car, based on each participant's 2019 miles driven.[8] We assume a \$51 social cost of carbon (SCC), following the U.S. Government Interagency Working Group on Social Cost of Greenhouse Gases (2021), and we define $\chi = 8.887 \times 10^{-3}$ metric tons CO2 per gallon as the CO2 content of gasoline. The externality estimate is

$$\hat{\phi}_i = \left(\frac{m_i}{23} - \frac{m_i}{30}\right) \cdot \chi \cdot SCC. \tag{9}$$

3.1.2 Drinks Experiment

In the drinks experiment, we estimate biases related to imperfect information and self-control problems. We follow Allcott, Lockwood and Taubinsky (2019a) in constructing bias estimates from survey measures of nutrition knowledge and self-control. We define $\hat{\boldsymbol{b}}_i$ as a two-part vector containing participant i's nutrition knowledge and self-control variables from our survey. Let $\boldsymbol{b}^V$ be the nutrition knowledge and self-control of an unbiased consumer. Following Allcott, Lockwood and Taubinsky (2019a), we assume that unbiased consumers have nutrition knowledge of 0.92 (the

[8] This calculation requires the assumption that depreciation is driven entirely by age, not mileage. Roughly consistent with that, Knittel and Sallee (2019) find that the effect of age on depreciation is about three times the effect of mileage.

average value for nutritionists and dieticians in their data) and self-control of 1 (i.e. they respond "not at all" when asked if they "drink soda pop or other sugar-sweetened beverages more often than I should"). Allcott, Lockwood and Taubinsky (2019a) show that under certain assumptions, the bias estimate is

$$\hat{\gamma}_i = \left(\boldsymbol{b}^V - \hat{\boldsymbol{b}}_i \right) \kappa, \tag{10}$$

where $\kappa > 0$ is an empirical constant.[9] This equation implies larger bias for consumers with lower nutrition knowledge and self-control.

Again following Allcott, Lockwood and Taubinsky (2019a), we assume a relative externality from sugary drinks of $\hat{\phi} = 0.85$ cents per ounce.[10] We assume that this is constant across people and product pairs.

3.1.3 Bias and Externality Estimates

Figure 2 presents the distribution of bias estimates $\hat{\gamma}_{ij}$ for each experiment. This dispersion can reflect dispersion in (true) bias γ_{ij}, measurement error, or both. For the cars experiment, the average $\hat{\gamma}_{ij}$ is \$135 per year, and the standard deviation is \$735. The average gas cost savings from the higher-MPG car (given reported 2019 miles driven and gas prices) is about \$304 per year, so the average $\hat{\gamma}_{ij}$ implies inattention to about 44 percent of gas costs. This is more inattention than implied by other papers using market data or field experiments (Busse, Knittel and Zettelmeyer 2013; Allcott and Wozny 2014; Sallee, West and Fan 2016; Grigolon, Reynaert and Verboven 2018; Allcott and Knittel 2019). The average externality proxy $\hat{\phi}_{ij}$ is \$50 per year, as reported in a separate vertical line, and the standard deviation of $\hat{\phi}_{ij}$ is about \$22.

For the drinks experiment, the average $\hat{\gamma}_i$ is \$2.56 per 12-pack, and the standard deviation is about \$1.24. The relative externality $\hat{\phi}$ is a constant \$1.22 per 12-pack.

These results imply that NPIs change average behavior in the "right direction" if they reduce demand for lower-MPG cars and sugary drinks, but the heterogeneity foreshadows the importance of reducing distortion variance.

[9] Specifically, $\kappa = \hat{\boldsymbol{\lambda}} p^s / \hat{\zeta}^c$, where $\hat{\boldsymbol{\lambda}}$ is the association between bias proxies and sugary drink consumption, p^s is the price of sugary drinks, and $\hat{\zeta}^c > 0$ is the compensated elasticity of demand. We take $\hat{\boldsymbol{\lambda}} = [0.854, 0.825]$ from column 1 of Allcott, Lockwood, and Taubinsky's (2019b) Table V, and we take p^s = \$0.0363/ounce (\$5.23/12-pack) and $\hat{\zeta}^c = 1.39$ from their Table VI. Intuitively, bias $\hat{\gamma}_i$ (in dollar units) is larger if the bias proxies are more strongly associated with consumption ($\hat{\boldsymbol{\lambda}}$ is larger) or if a given consumption change corresponds to more dollars ($p/\hat{\zeta}^c$ is larger). See Allcott, Lockwood and Taubinsky (2019a) for discussion of required assumptions, including linearity and unconfoundedness.

[10] Their estimate of $\hat{\gamma}_i^e$ is derived from several earlier studies. Wang et al. (2012) use epidemiological simulation models to estimate that soda consumption increases health care costs by an average of approximately one cent per ounce. Yong et al. (2011) estimate that for people with employer-provided insurance, about 15 percent of health costs are borne by the individual, while 85 percent are covered by insurance. Similarly, Cawley and Meyerhoefer (2012) estimate that 88 percent of the total medical costs of obesity are borne by third parties, and obesity is one of the primary diseases thought to be caused by SSB consumption.

3.2 Average Treatment Effect of Labels, Variance, and Covariance with Bias and Externalities

In this section, we estimate the average treatment effects of labels, the variance of treatment effects, and the covariance of treatment effects with bias and externalities.

3.2.1 Empirical Model

We define T_i as a treatment indicator, with $T_i = 1$ for a label treatment condition and $T_i = 0$ for control. (We pool all label conditions in some analyses, while in other analyses we compare an individual label condition against control.) Following the framework in Section 1, participant i's relative WTP for the more harmful good in product pair j elicited on choice occasion s depends on utility v_{ijs}, bias γ_{ij}, and NPI effect τ_{ij}.[11] We decompose the utility as $v_{ijs} = v_{ij} + \epsilon_{ijs}$, where v_{ij} is a stable person-specific component and ϵ_{ijs} is an idiosyncratic shock capturing taste variation, elicitation noise, or (in the cars experiment) the WTP difference between the Accord and Legacy. This implies that relative WTP is

$$w_{ijs} = v_{ij} + \gamma_{ij} + \tau_{ij} \cdot T_i \cdot \mathbf{1}[s = 2] + \epsilon_{ijs}. \tag{11}$$

We use "~" to indicate differences between the baseline and endline choice occasions. For example, $\tilde{w}_{ij} \coloneqq w_{ij2} - w_{ij1}$. Differencing equation (11) gives

$$\tilde{w}_{ij} = \tau_{ij} \cdot T_i + \tilde{\epsilon}_{ij}. \tag{12}$$

We make the following assumptions, which deliver a standard mixed effects model.

Assumption 1. The measurement errors in bias and externality, $\hat{\gamma} - \gamma$ and $\hat{\phi} - \phi$, are mean-zero, normally distributed, and independent of γ, ϕ, and τ.

Assumption 2. The treatment is randomly assigned: $T \perp \tilde{\epsilon}$.

Assumption 3. The idiosyncratic WTP change is orthogonal to the treatment effect : $\tilde{\epsilon} \perp \tau$.

Assumption 4. τ, γ, ϕ, and $\tilde{\epsilon}$ are normally distributed.

Perhaps the strongest assumption is the part of Assumption 1 that requires measurement error $\hat{\gamma} - \gamma$ to be independent of treatment effects τ. For example, this implies that the treatment effects of nutrition facts labels don't covary with misperceptions of calorie or sugar content that are not predicted by our nutrition knowledge and self-control bias proxies. In the cars experiment, we have an error-free measure of failure to optimize within the experiment, but Assumption 1 would be required to consider potential bias in actual car purchase decisions.

An alternative strategy in Appendix D.2 delivers $Cov[\gamma, \tau]$ in the drinks experiment without assuming normality; the estimate is very similar.

[11] In the cars experiment, recall that we randomized whether the Accord or Legacy was valued at baseline instead of endline. Accord-Fusion at baseline followed by Legacy-Fusion at endline is defined as a separate product-pair from Legacy-Fusion followed by Accord-Fusion.

Assumptions 1 and 4 together imply that $\hat{\gamma}$ and $\hat{\phi}$ are normally distributed. Since τ is also normal, τ_{ij} can be written as

$$\tau_{ij} = \eta_{ij} + \alpha_1 \hat{\gamma}_{ij} + \alpha_2 \hat{\phi}_{ij}, \tag{13}$$

where η is normal and independent of $\left(\hat{\gamma}, \hat{\phi}\right)$, $\alpha_1 = Cov\left[\hat{\gamma}, \tau\right] / Var\left[\hat{\gamma}\right]$, and $\alpha_2 = Cov\left[\hat{\phi}, \tau\right] / Var\left[\hat{\phi}\right]$. By Assumption 1, $Cov\left[\hat{\gamma}, \tau\right] = Cov\left[\gamma, \tau\right]$ and $Cov\left[\hat{\phi}, \tau\right] = Cov\left[\phi, \tau\right]$, and thus

$$Cov\left[\gamma, \tau\right] = \alpha_1 Var\left[\hat{\gamma}\right] + \alpha_2 Cov\left[\hat{\gamma}, \hat{\phi}\right] \tag{14}$$

$$Cov\left[\phi, \tau\right] = \alpha_2 Var\left[\hat{\phi}\right] + \alpha_1 Cov\left[\hat{\gamma}, \hat{\phi}\right]. \tag{15}$$

Combining equations (12) and (13) gives

$$\tilde{w}_{ij} = \eta_{ij} T_i + \alpha_1 \hat{\gamma}_{ij} T_i + \alpha_2 \hat{\phi}_{ij} T_i + \tilde{\epsilon}_{ij}. \tag{16}$$

A linear projection of $\tilde{\epsilon}_{ij}$ on $\hat{\gamma}_{ij}$ and $\hat{\phi}_{ij}$ gives

$$\tilde{\epsilon}_{ij} = \beta_1 \hat{\gamma}_{ij} + \beta_2 \hat{\phi}_{ij} + \beta_{0i} + \nu_{ij}, \tag{17}$$

where β_{0i} is an individual-level constant and the residual ν_{ij} is mean-zero.[12] Our assumptions imply that $\beta_{0i} + \nu_{ij}$ is normally distributed, and we additionally assume that β_{0i} and ν_{ij} are each normally distributed. Combining equations (16) and (17) gives a mixed effects model:

$$\tilde{w}_{ij} = \eta_{ij} T_i + \alpha_1 \hat{\gamma}_{ij} T_i + \alpha_2 \hat{\phi}_{ij} T_i + \beta_1 \hat{\gamma}_{ij} + \beta_2 \hat{\phi}_{ij} + \beta_{0i} + \nu_{ij}. \tag{18}$$

Using Assumptions 1 and 2, we estimate $\mathbb{E}\left[\tau\right]$ using equation (12) with fixed τ in ordinary least squares, i.e. by simply regressing $\tilde{w}_{ij}$ on T_i and a constant. Using orthogonality and normality from Assumptions 2 and 3, we estimate $Var\left[\tau\right]$ using equation (12) in a standard mixed effects maximum likelihood estimator. Using orthogonality and normality from Assumptions 1–4, we also estimate equation (18) via mixed effects. Then, using the estimated regression coefficients $\{\alpha_1, \alpha_2\}$ and the variances and covariance $\left\{Var[\hat{\gamma}], Var\left[\hat{\phi}\right], Cov\left[\hat{\gamma}, \hat{\phi}\right]\right\}$ computed directly from the sample distribution of bias and externality estimates, we then recover the covariances $\{Cov\left[\gamma, \tau\right], Cov\left[\phi, \tau\right]\}$ using equations (14) and (15).

Because equations (12)–(18) also hold conditional on controls, we can improve precision by adding controls. In all regressions, we include indicators for product pairs j and MPL order.

[12] The coefficients are $\beta_1 = Cov\left[\hat{\gamma}, \tilde{\epsilon}\right] / Var\left[\hat{\gamma}\right]$ and $\beta_2 = Cov\left[\hat{\phi}, \tilde{\epsilon}\right] / Var\left[\hat{\phi}\right]$.

3.2.2 Descriptive Figures and Intuition for Identification

Before presenting formal regression results, we present figures that illustrate the identification. Figure 3 presents the treatment and control group distributions of WTP changes $\tilde{w}_{ij}$ across people and product pairs in each experiment, pooling treatment group data across all labels. In both experiments, the treatment distributions are shifted to the left, implying that the labels reduced WTP for lower-MPG cars (relative to higher-MPG cars) and for sugary drinks (relative to sugar-free drinks). The treatment distributions are also more dispersed, with less weight on \$0 change and more weight throughout the left tails. In both experiments, we should thus expect $\mathbb{E}[\tau] < 0$ and a significant $Var[\tau]$, since the effect of the label on dispersion of $\tilde{w}$ is what identifies $Var[\tau]$.

Figure 4 presents average treatment effect (ATE) estimates using the OLS version of equation (12), for each label in each experiment. The left-most bars for each label are the average treatment effects $\mathbb{E}[\tau]$. The remaining bars present conditional ATEs within the sample of participant-by-product pair observations with below-median or above-median bias or externality proxies. Holding the ATE constant, a more negative effect for above-median distortions $\hat{\delta}$ would imply a more negative $Cov[\delta, \tau]$, and thus good targeting.

For the cars experiment, the ATEs and conditional ATEs are statistically indistinguishable across labels. For the personalized cost label, the cost differences presented are larger for heavy drivers and smaller for light drivers, which could have generated larger effect sizes for heavy drivers, analogous to Davis and Metcalf (2016). Our point estimates are consistent with this, but the difference is highly statistically insignificant.

For the drinks experiment, there is some suggestive evidence of tradeoffs. The graphic warning label has the largest ATE, but it has smaller effects for more biased consumers, implying a positive $Cov[\hat{\gamma}, \tau]$. The nutrition facts label has smaller ATE but larger point estimates for more biased consumers. These differences are not statistically significant, but they suggest that the nutrition facts label might generate larger total surplus gains despite having smaller average effects. When pooling across the three sugary drink labels in the right-most set of results, the point estimates suggest slightly smaller effects for more biased consumers, implying slightly positive $Cov[\hat{\gamma}, \tau]$. Recall that for sugary drinks, we assume that the externality ϕ is constant across consumers.

3.2.3 Parameter Estimates

Table 1 presents results with data pooled across all labels. Columns 1 and 2 present fixed coefficient (OLS) versions of equations (12) and (16), respectively, while columns 3 and 4 present the full random coefficient (mixed effects) models. The coefficients are very similar between OLS and mixed effects.

From column 1, the average treatment effect $\mathbb{E}[\tau]$ for fuel economy labels is a reduction in relative WTP for the lower-MPG car of \$59 per vehicle-year. Using the demand slope reported in Section 4.2, a homogeneous treatment effect of that magnitude would reduce the lower-MPG car's market share by about $\mathbb{E}[\tau]\, D'_p \approx 4$ percentage points. This is within the confidence intervals of the (statistically insignificant) effects of fuel cost information provision on actual vehicle purchases from the experiment in Allcott and Knittel (2019).

The ATE for sugary drink labels is a reduction in relative WTP for the sugary drink of \$0.43 per 12-pack. Using the demand slope, a homogeneous treatment effect of that magnitude would reduce the sugary drink's market share by about 6 percentage points. This is smaller than the 10 and 17 percentage point effects of stop sign and graphic warning labels on sugary drink demand in Grummon et al. (2019a) and Hall et al. (2022), respectively.

Both fuel economy and health labels change average behavior in the "right direction," in the sense that the average effects offset the suboptimally high purchases of low-MPG vehicles and sugary drinks caused by bias and externalities. This offset is only partial: labels offset $59/(135+50) \approx 32\%$ of the bias + externality in the cars experiment, and labels offset only $0.43/(2.56+1.22) \approx 11\%$ in the drinks experiment. This is consistent with the view that in markets with material externality problems, NPIs don't generate enough behavior change to eliminate the need for taxation (e.g., Loewenstein and Chater 2017; Thaler and Sunstein 2021).

From column 3, the variance of fuel economy label effects is $Var[\tau] \approx$ 30,428 (\$/vehicle-year)^2, implying a standard deviation of \$174 per vehicle-year, and thus a coefficient of variation ($\sqrt{Var[\tau]}/\mathbb{E}[\tau]$) of about 3.0. The variance of sugary drink label effects is 0.74 (\$/12-pack)^2, implying a standard deviation of \$0.86 and a coefficient of variation of about 2.0.

Column 4 presents the primary estimate of $\{\alpha_1, \alpha_2\}$, the coefficients on bias or externality interacted with T_i, in mixed effects. For the cars experiment, $\alpha_1 \approx -0.01$ and $\alpha_2 \approx -0.007$, both of which are statistically indistinguishable from zero, and $Cov\left[\hat{\gamma}, \hat{\phi}\right] \approx$ 2,384. From Section 3.1, the standard deviations of $\hat{\gamma}$ and $\hat{\phi}$ are \$735 and \$22 per year. Thus, using equations (14) and (15), the covariance point estimates are $Cov[\gamma, \tau] \approx$ -7,744 and $Cov[\phi, \tau] \approx$ -37 (\$/vehicle)^2.

For the drinks experiment, $\alpha_1 \approx 0.08$, which is statistically significantly different from zero. From Section 3.1, the standard deviation of $\hat{\gamma}$ is \$1.24 per 12-pack. Thus, the covariance point estimate is $Cov[\gamma, \tau] \approx 0.08 \times 1.24^2 \approx 0.13$ (\$/12-pack)^2.

Appendix D.1 presents versions of Table 1 specific to each of the seven labels across the two experiments. One key result that differs across labels is that the graphic warning label has significantly more positive $Cov[\gamma, \tau]$ than the nutrition facts label, consistent with the suggestive graphical results in Figure 4.

3.3 Psychic Benefits and Costs

For the cars experiment, the labels are not very evocative, so we assume zero psychic benefit ($\Delta I = 0$). For the drinks experiment, we use the MPL questions eliciting hypothetical WTP to receive or avoid receiving labels on a 12-pack of sugary drinks. We assume that those who buy sugar-free drinks experience no psychic benefits or costs ($\iota_0 = 0$), so aggregate psychic benefits with the labels are $I(\sigma, p) = \sigma\bar{\iota}_1 q^*(\sigma = 1)$, where $\bar{\iota}_1$ is the average psychic benefit conditional on buying the sugary drink, and $q^*(\sigma = 1)$ is the equilibrium quantity. ι_1 could be negative if the label is disgusting or otherwise aversive, or it could be positive if consumers value the hard information or reminders about health even after buying the drinks.

Figure 5 presents average WTP by label. The average person in our sample wants to receive the nutrition label and reported being willing to pay \$2 for it on a 12-pack. Average WTP is also positive for the stop sign label. The average person in our sample reported being willing to pay about \$1 to avoid receiving a 12-pack with the graphic warning labels. Pooling across all three labels, average WTP is almost exactly zero.

Appendix Figure A8 presents the distribution of reasons why people did or did not want to receive each label, as elicited from multiple choice questions. People who wanted to receive the nutrition and stop sign labels mostly reported that this was because "the information on the label is useful," while the minority of people who wanted to receive the graphic warning label mostly reported that this was because "it would remind me to drink less." The modal person who wanted to avoid receiving the labels reported that this was because "the label makes me feel bad," although a large minority of people reported that "I don't need the government to tell me what to drink." Another large minority of people who wanted to avoid the graphic warning labels reported that this was because "the label is gross."

4 Welfare Analysis

4.1 Implementation of Proposition 1.3

In our empirical applications, we consider a binary NPI intensity, $\sigma \in \{0, 1\}$, corresponding to the question of whether or not to have labels. To easily apply Proposition 1 to this case, we assume that the moments are locally linear in σ.[13] We define a difference operator $\Delta X(\sigma) \coloneqq X(\sigma = 1) - X(\sigma = 0)$ for any function $X(\sigma)$.

As noted in Section 3, we increase precision and generalizability by assuming that the population parameters approximate the marginal parameters.

We explicitly add an externality ϕ to the bias γ, and we define $\delta \coloneqq \gamma + \phi$ as their sum. Because covariances are bilinear, $Cov\left[\delta, \tau\right] = Cov\left[\gamma, \tau\right] + Cov\left[\phi, \tau\right]$. As described in Section 1.1, Proposition 1 holds after replacing γ with δ. We also assume zero psychic benefits/costs ($I = 0$) for our primary analyses.

When we make the assumptions above, expand the second moment, and rearrange, Proposition 1 implies that the total surplus changes from the NPI at $t = 0$ and at optimal tax $t^* = \mathbb{E}\left[\delta + \sigma\tau\right] - \mu$, respectively, are:

$$\Delta W(t = 0) \approx \frac{1}{2}\left\{\rho\left(\left(\mathbb{E}\left[\tau\right] + \mathbb{E}\left[\delta\right] - \mu\right)^2 - \left(\mathbb{E}\left[\delta\right] - \mu\right)^2\right) + 2Cov\left[\delta, \tau\right] + Var\left[\tau\right]\right\} D_p' \tag{19}$$

$$\Delta W(t = t^*) \approx \frac{1}{2}\left\{2Cov\left[\delta, \tau\right] + Var\left[\tau\right]\right\} D_p'. \tag{20}$$

[13] Without this assumption, we would need to integrate the first-order condition in Proposition 1 across all values of $\sigma \in [0, 1]$.

We take $\mathbb{E}[\delta]$, $\mathbb{E}[\tau]$, $Var[\tau]$, and $Cov[\delta, \tau]$ from Section 3. Below, we discuss the remaining parameters and then present total surplus effects.

4.2 Demand Slope

We pool across all product pairs to construct an average demand function for the more harmful good, as illustrated in Appendix Figure A7. The average demand slopes for the fuel economy and drinks experiments are -0.00060 market share per \$/vehicle-year and -0.14 market share per \$/12-pack, respectively.

4.3 Pass-Through and Markup Assumptions

When there are two or more goods, it is the difference in markups and pass-through rates that form the sufficient statistics for welfare analysis; see Appendix A.4. The pass-through rate ρ is the pass-through of a tax on the more harmful good to the price of the more harmful good relative to the less harmful good. The markup μ is the relative markup on the more versus less harmful good.

4.3.1 Cars Experiment

There are no papers that specifically study how taxes on 23-MPG sedans are passed through to those sedans vs. 30-MPG sedans. As a proxy, we collect estimates from three papers studying pass-through rates for hybrid vehicle subsidies and manufacturers' customer rebates, which use the prices of other vehicles as controls.[14] The average pass-through rate is approximately 0.8, so we assume $\rho \approx 0.8$ as a rough benchmark.

Since prices and cost structures are similar for our 23-MPG versus 30-MPG sedans, we assume $\mu = 0$.

4.3.2 Drinks Experiment

For sugary drinks, we collect estimates from seven papers that study city-level sugary drink taxes in the U.S.[15] The average pass-through rate to taxed beverages is 0.85, and the average pass-through rate to untaxed beverages is 0.05, giving $\rho \approx 0.85 - 0.05 \approx 0.80$.

Three of these seven papers report prices for both sugary drinks and water.[16] The prices are relatively similar. While there is no public information on the marginal costs of 12-packs of sugary drinks versus sparkling water, they have very similar cost structures. Thus, we assume similar markups on the two goods, implying a relative markup of $\mu = 0$.

[14] The papers are Busse, Silva-Risso and Zettelmeyer (2006), Sallee (2011), and Gulati, McAusland and Sallee (2017).

[15] The papers are Falbe et al. (2015), Silver et al. (2017), Cawley et al. (2018), Cawley et al. (2020), Bleich et al. (2021), Seiler, Tuchman and Yao (2021), Petimar et al. (2022)

[16] The three papers are Falbe et al. (2015), Cawley et al. (2020), and Seiler, Tuchman and Yao (2021).

4.4 Results

Table 2 summarizes the sufficient statistics and resulting total surplus effects obtained from equations (19) and (20). We initially pool across labels in each experiment to simplify and increase precision.

To demonstrate the computation, consider the total surplus effect of fuel economy labels with no tax. Using $\mu = 0$, $\mathbb{E}[\delta] = \mathbb{E}[\gamma] + \mathbb{E}[\phi] = 135 + 50$, and $Cov[\delta, \tau] = Cov[\gamma, \tau] + Cov[\phi, \tau] =$ -7,744 $+ -37$, equation (19) is

$$\Delta W(t=0) \approx \frac{1}{2}\left\{\rho\left((\mathbb{E}[\tau] + \mathbb{E}[\delta])^2 - (\mathbb{E}[\delta])^2\right) + 2Cov[\delta, \tau] + Var[\tau]\right\} D'_p \tag{21}$$

$$\approx \frac{1}{2}\left\{0.80\left(\ (-59 + 184)^2\ \ - (184)^2\right) + 2 \times (-7,781) + 30,428\right\} \times (-0.00060) \tag{22}$$

$$\approx -0.07 \ \ \$/\text{vehicle-year}.$$

Thus, the point estimates suggest that fuel economy labels decrease total surplus in our experiment.

Three special cases reported near the bottom of Table 2 illustrate the importance of the three terms inside brackets, which correspond to the three economic forces described in Section 1.3. First, labels with homogeneous effects ($Cov[\delta, \tau] = Var[\tau] = 0$) and the same estimated $\mathbb{E}[\tau]$ would increase total surplus by \$4.36 per vehicle-year. Second, labels with zero average effect ($\mathbb{E}[\tau] = 0$) and the same estimated $Cov[\delta, \tau]$ and $Var[\tau]$ would decrease total surplus by \$4.43 per vehicle-year. Third, "pure noise" labels with zero average effect and $Cov[\delta, \tau] = 0$ and the same estimated $Var[\tau]$ would decrease total surplus by \$9.07 per vehicle-year. The first case shows the importance of average behavior change, which drives the overall total surplus gain. The latter two cases show that the labels add substantial noise to consumer choice in our experiment, and this misallocation is quantitatively important for welfare.

The final row of Table 2 shows that fuel economy labels reduce total surplus when they are paired with the optimal tax. Intuitively, since the tax allows the government to optimally control aggregate quantity, the labels' average effect is irrelevant, and the increase in distortion variance reduces total surplus. In all cases, the estimated total surplus effects are small relative to lease prices of \$5,600 per vehicle-year (Brozic 2022). This is driven by the modest demand slope D'_p and small average treatment effects.

In the drinks experiment, the point estimates of the variance and covariance are much smaller compared to the bias and externality. With no tax, health labels increase total surplus in our model, again primarily because the labels change average behavior in the "right direction." At the optimal tax, the labels reduce total surplus in our model because they increase the distortion variance, due to their high variance and positive covariance with bias.

Figures 6 and 7 illustrate how the total surplus effects of labels and taxes depend on different parameter values. Assuming locally linear demand, the total surplus gain from the optimal tax t^* is $-\frac{1}{2}t^{*2}D'_p$. In both experiments, the labels reduce the net distortion, so taxes and labels are substitutes: total surplus gains from optimal taxes are smaller with the labels, and total surplus gains from labels are smaller with optimal taxes. Total surplus gains from optimal taxes depend only on D'_p and the average marginal net distortion $\mathbb{E}[\delta + \sigma\tau] - \mu$, so those lines are flat in Figure 6 with respect to $Var[\tau]$ and $Cov[\delta, \tau]$.

The left panels in Figure 6 illustrate one basic insight from Proposition 1 by plotting ΔW as a function of $Var[\tau]$, holding constant the other parameters in Table 2. The total surplus gains from labels are declining in $Var[\tau]$: a smaller $Var[\tau]$ corresponds to better targeting, because a larger share of the assumed variance represents covariance with bias and externalities. In both experiments, total surplus gains differ significantly for plausible values of $Var[\tau]$. In the cars experiment, the negative $Cov[\delta, \tau]$ implies that fuel economy labels might even be preferred to optimal taxes at small values of $Var[\tau]$. By contrast, at twice the estimated $Var[\tau]$, fuel economy labels generate substantial misallocation, reducing total surplus by about the amount that the optimal tax would increase it.

In the drinks experiment, optimal taxes are preferred to labels at most plausible values because the average net distortion is large relative to the average treatment effect of the labels. In the drinks experiment at $t = 0$, labels increase total surplus at small values of $Var[\tau]$ because the decrease in average demand reduces the average net distortion, but they decrease total surplus at larger values of $Var[\tau]$.

The right panels in Figure 6 plot ΔW as a function of $Cov[\delta, \tau]$, holding constant the other parameters. The total surplus gains from labels are declining in $Cov[\delta, \tau]$, because a more negative $Cov[\delta, \tau]$ implies that a larger share of the assumed variance is well-targeted. In both experiments, plausible differences in $Cov[\delta, \tau]$ again substantially affect total surplus. In the cars experiment, if $Cov[\delta, \tau]$ were twice as large as our point estimates, fuel economy labels would be preferred to optimal taxes. If $Cov[\delta, \tau]$ had the same magnitude but opposite sign, the fuel economy labels would significantly reduce total surplus.

The left panels in Figure 7 show how total surplus effects depend on pass-through assumptions. At $\rho = 0$, implying perfectly inelastic supply, taxes do not affect equilibrium quantity or total surplus. The optimal tax increases total surplus as ρ increases, especially without labels, again because optimal taxes and labels are substitutes at our parameter values. At the optimal tax, pass-through does not affect the total surplus effect of labels because the government uses the tax to optimally set aggregate quantity. At $t = 0$, pass-through does matter because the labels decrease average demand, offsetting the distortion from bias and externalities. Our three auto market pass-through estimates (from footnote 14) range from 0.57 to 1, while different combinations of estimates from the seven soda tax papers (from footnote 15) could imply relative pass-through from $\rho = 0.16$ to $\rho = 1.37$. In both experiments when $t = 0$, using estimates from the lower versus higher ends of those ranges would significantly change the magnitude of total surplus effects and could even change the sign.

The right panels in Figure 7 show how the markup affects total surplus. To ease interpretation, we divide the markup difference μ by benchmark estimates of average prices of each good: \$5,600 to lease a car for one year (Brozic 2022) and \$4 for a 12-pack of drinks. The welfare effects of optimal taxes are quadratic in μ because μ directly determines the net distortion, and the minimum total surplus effect is zero at $t^* = 0$. At the optimal tax, the total surplus effects of labels do not depend on the markup, because the tax rate can be set to offset any markup difference. At $t = 0$, the total surplus effect of labels is decreasing in μ, as the average demand decrease caused by the labels is more beneficial with an additional pre-existing distortion that decreases the price of the more harmful good.

Appendix Table A13 presents label-specific point estimates. The parameters and total surplus effects are mostly statistically indistinguishable across labels. The point estimates suggest that although graphic warning labels on sugary drinks have larger ATEs than the nutrition facts labels, the increased distortion variance (larger $Var[\tau]$ and significantly larger $Cov[\gamma, \tau]$) implies that the graphic labels generate lower total surplus gains. Adding the large psychic benefits and costs quantified in Section 3.3 would imply large total surplus gains from nutrition facts labels and large total surplus losses from graphic warning labels.

5 Conclusion

Simplified information provision, warning labels, and other NPIs have become increasingly popular over the past 15 years. Much of the public discussion and empirical work focuses on whether these instruments change average behavior in the "right direction," and the original ideas of asymmetric paternalism and libertarian paternalism has not been formalized in market settings. Our theoretical framework fleshes out these ideas and clarifies how average behavior change, asymmetric paternalism, deliberative competence, and other concepts matter for welfare. Our randomized experiments demonstrate both the challenges and opportunities in empirically evaluating NPIs in a theoretically grounded framework. The results highlight the importance of measuring both aversiveness and the variance of distortions, illustrating how average treatment effects can be an incomplete and possibly misleading proxy for welfare.

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Table 1: **Average Treatment Effects, Variance, and Covariance**

(a) **Cars Experiment**

	(1) OLS	(2) OLS	(3) Mixed effects	(4) Mixed effects
Treated	-59.10**	-55.81	-58.70**	-56.69
	(23.30)	(57.44)	(23.42)	(57.42)
Bias × Treated		-0.00		-0.01
		(0.04)		(0.04)
Externality × Treated		-0.06		-0.01
		(1.06)		(1.06)
Cov(bias, treatment effect)		-1,106		-7,744
(standard error)		20,902		21,348
Cov(externality, treatment effect)		-35		-37
(standard error)		513		514
Var(treatment effect)			30,428	
(standard error)			11,925	
Number of participants	1,267	1,267	1,267	1,267
Number of observations	2,534	2,534	2,534	2,534

(b) **Drinks Experiment**

	(1) OLS	(2) OLS	(3) Mixed effects	(4) Mixed effects
Treated	-0.43***	-0.65***	-0.43***	-0.65***
	(0.04)	(0.09)	(0.04)	(0.09)
Bias × Treated		0.08**		0.08**
		(0.04)		(0.04)
Cov(bias, treatment effect)		0.130		0.129
(standard error)		0.055		0.055
Var(treatment effect)			0.743	
(standard error)			0.187	
Number of participants	2,619	2,619	2,619	2,619
Number of observations	7,857	7,857	7,857	7,857

Notes: Panels (a) and (b), respectively, present estimated ATEs, variances, and covariances for the cars experiment and drinks experiment, pooling across all labels. Columns 1 and 2 present fixed coefficient (OLS) versions of equations (12) and (16), respectively, while columns 3 and 4 present the full random coefficient (mixed effects) models. All regressions also include controls for bias and externality as well as indicators for product pairs j and MPL order.

Table 2: **Parameters and Welfare Analysis: Pooled Labels**

Parameter	Description	(1) Cars experiment	(2) Drinks experiment
D'_p	Demand slope (share of purchases/($/unit)	-0.00060	-0.14
$\mathbb{E}[\gamma]$	Average bias ($/unit)	135	2.56
		(17.6)	(0.02)
$\mathbb{E}[\phi]$	Average externality ($/unit)	50	1.22
		(0.62)	(0.00)
$\mathbb{E}[\tau]$	Average treatment effect ($/unit)	-59	-0.43
		(23)	(0.04)
$Var[\tau]$	Treatment effect variance (($/unit)^2	30,428	0.74
		(11,925)	(0.19)
$Cov[\gamma, \tau]$	Bias and treatment effect covariance (($/unit)^2)	$-7,744$	0.13
		$(21,348)$	(0.05)
$Cov[\phi, \tau]$	Externality and treatment effect covariance (($/unit)^2)	-37	0
		(514)	
ρ	Pass-through (unitless)	0.80	0.80
μ	Markup ($/unit)	0	0
$\Delta W(t=0)$	Total surplus effect with no tax ($/unit)	-0.07	0.11
	special case: $Cov[\delta, \tau] = Var[\tau] = 0$ (homogeneous)	4.36	0.18
	special case: $\mathbb{E}[\tau] = 0$	-4.43	-0.07
	special case: $\mathbb{E}[\tau] = Cov[\delta, \tau] = 0$ (pure noise)	-9.07	-0.05
$\Delta W(t=t^*)$	Total surplus effect with optimal tax ($/unit)	-4.43	-0.07

Notes: This table presents parameter estimates and total surplus effects, pooling across labels in each experiment. $\Delta W(t=0)$ and $\Delta W(t=t^*)$ are computed using equations (19) and (20), given the parameters reported above. "Unit" is "vehicle-year" for cars and "12-pack" for sugary drinks. Standard errors are in parentheses.

Figure 1: **Labels**

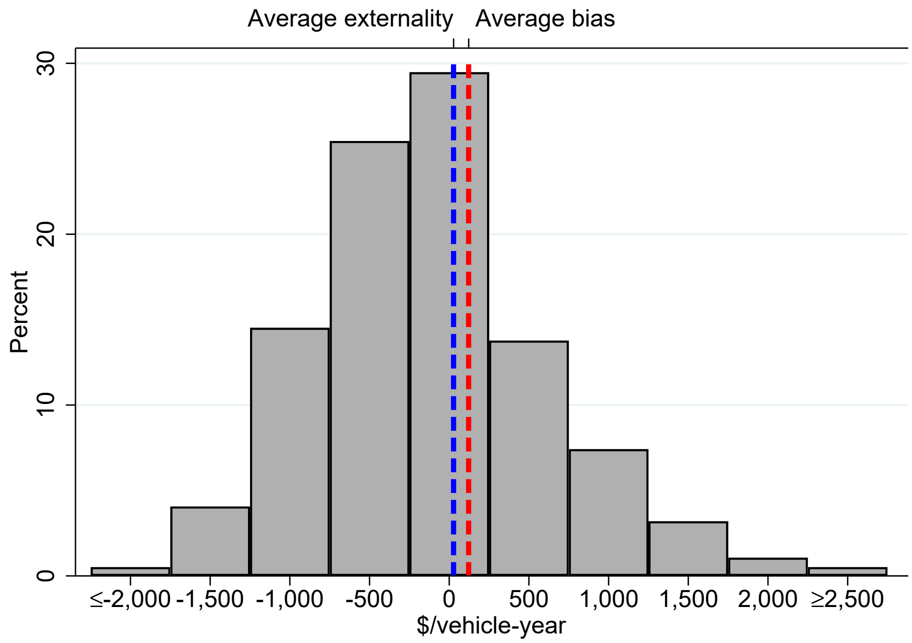
(a) **Cars Experiment**

(b) **Drinks Experiment**

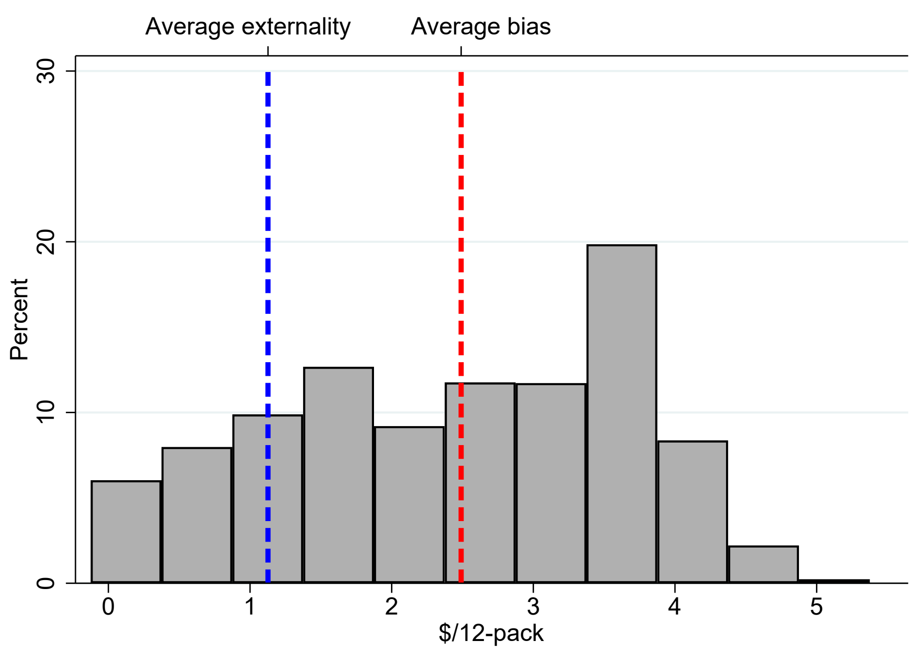
Notes: Panels (a) and (b), respectively, present the labels used in the cars experiment and drinks experiment.

Figure 2: **Distributions of Bias Proxies**

(a) **Cars Experiment**



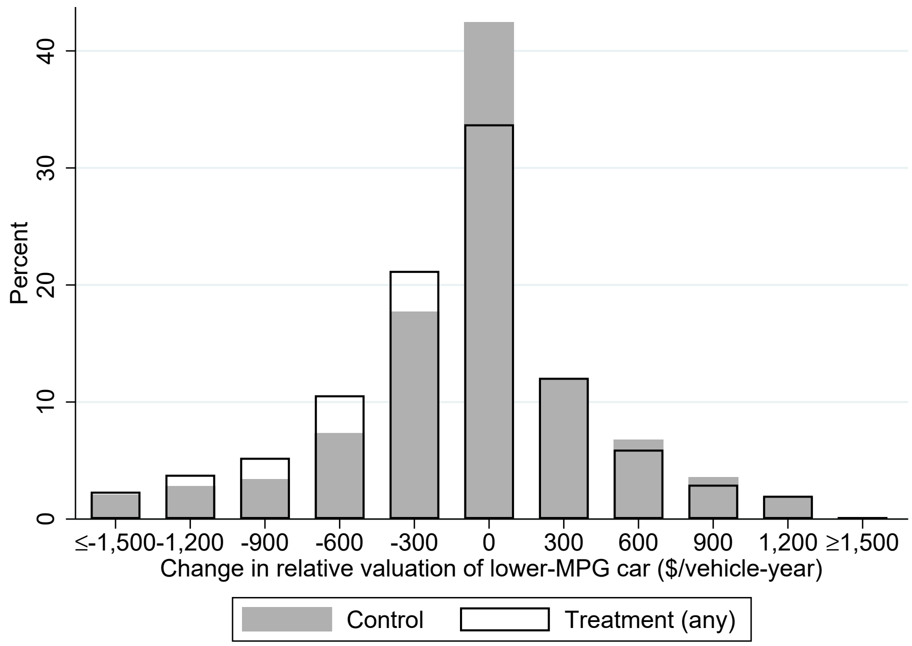
(b) **Drinks Experiment**



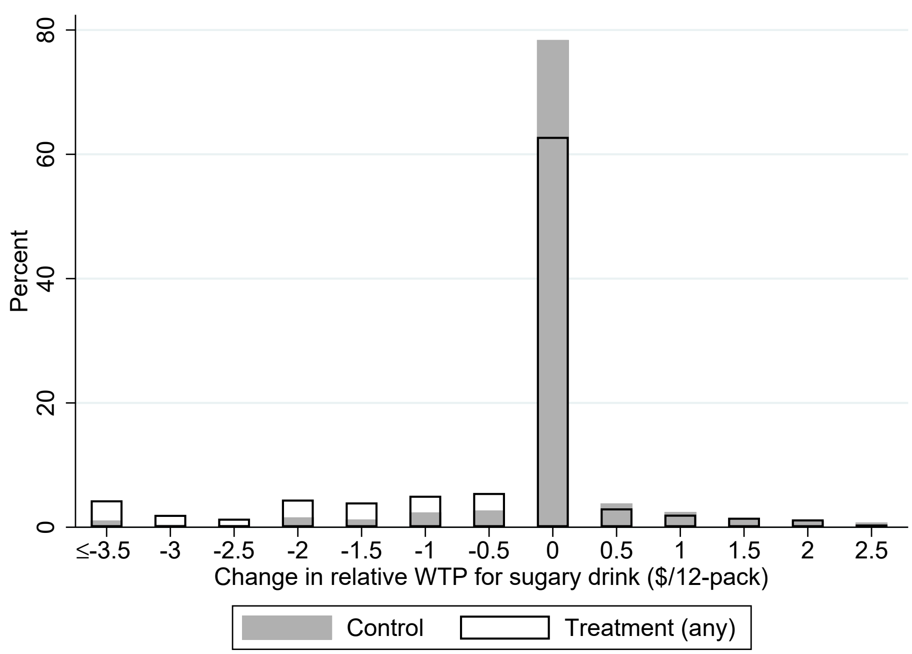
Notes: Panels (a) and (b), respectively, present the distributions of bias proxies across participants in the cars experiment and drinks experiment. The vertical lines are the average bias and externality.

Figure 3: **Changes in Willingness-to-Pay**

(a) **Cars Experiment**



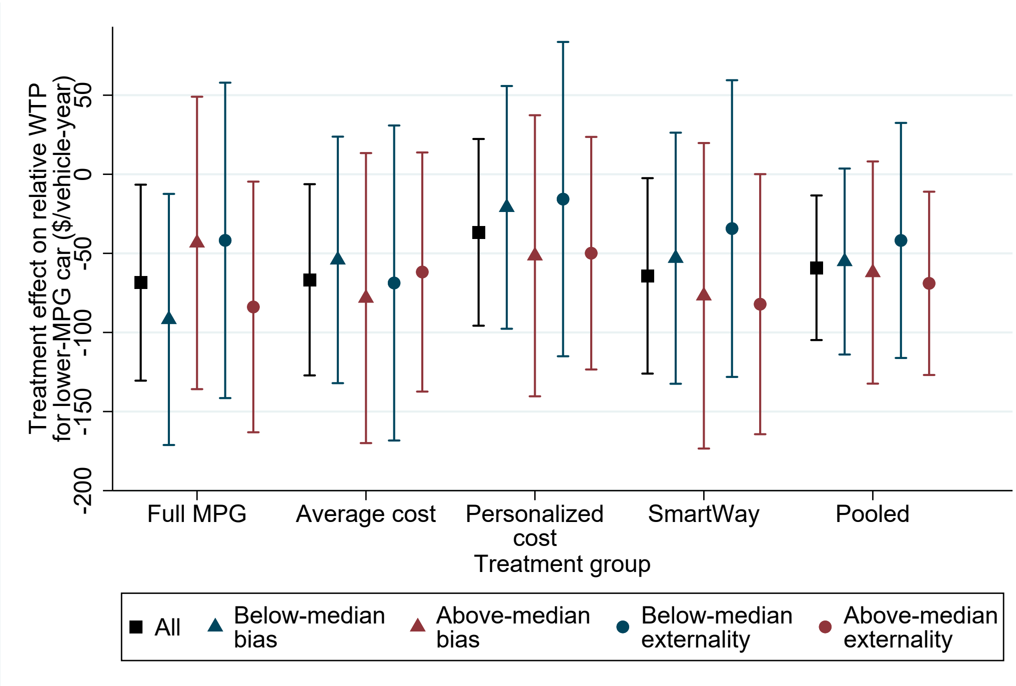
(b) **Drinks Experiment**



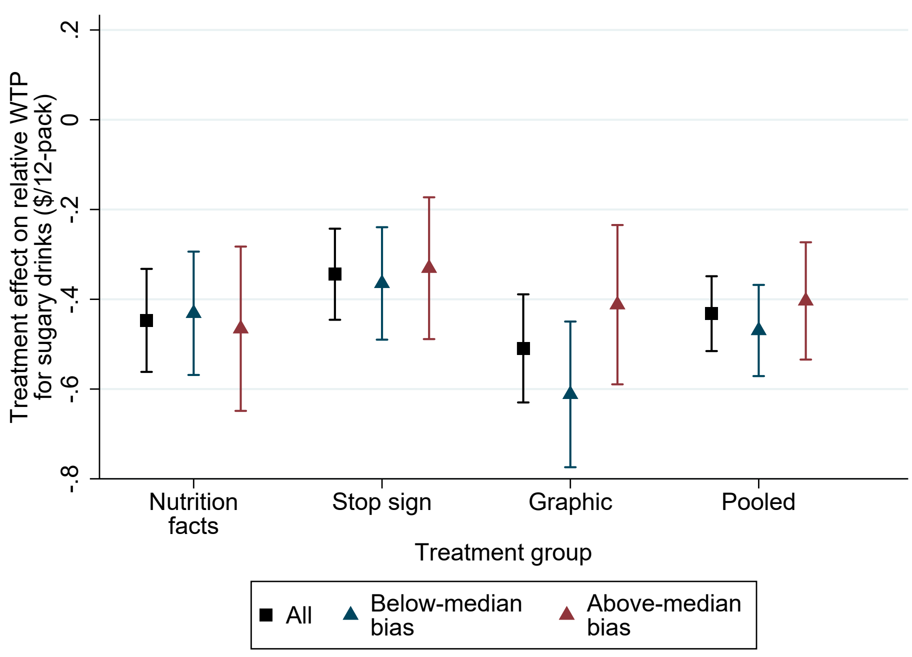
Notes: Panels (a) and (b), respectively, present histograms of baseline-endline willingness-to-pay changes in the cars experiment and drinks experiment.

Figure 4: **Average Treatment Effects and Heterogeneity**

(a) **Cars Experiment**

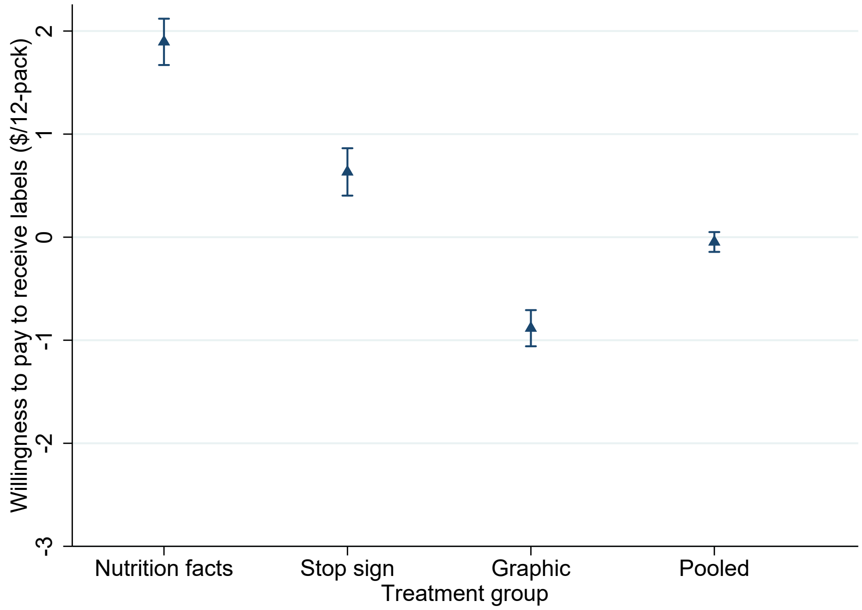


(b) **Drinks Experiment**



Notes: Panels (a) and (b), respectively, present estimates of equation (12) for the cars experiment and drinks experiment, also controlling for product pair and choice order indicators. For each label, the coefficients on the left are average treatment effects in the full samples. The remaining coefficients are heterogeneous effects in subgroups of observations with above- or below-median bias or externality proxies. In the drinks experiment, we assume homogeneous externalities, so there are no above- and below-median externality subgroups.

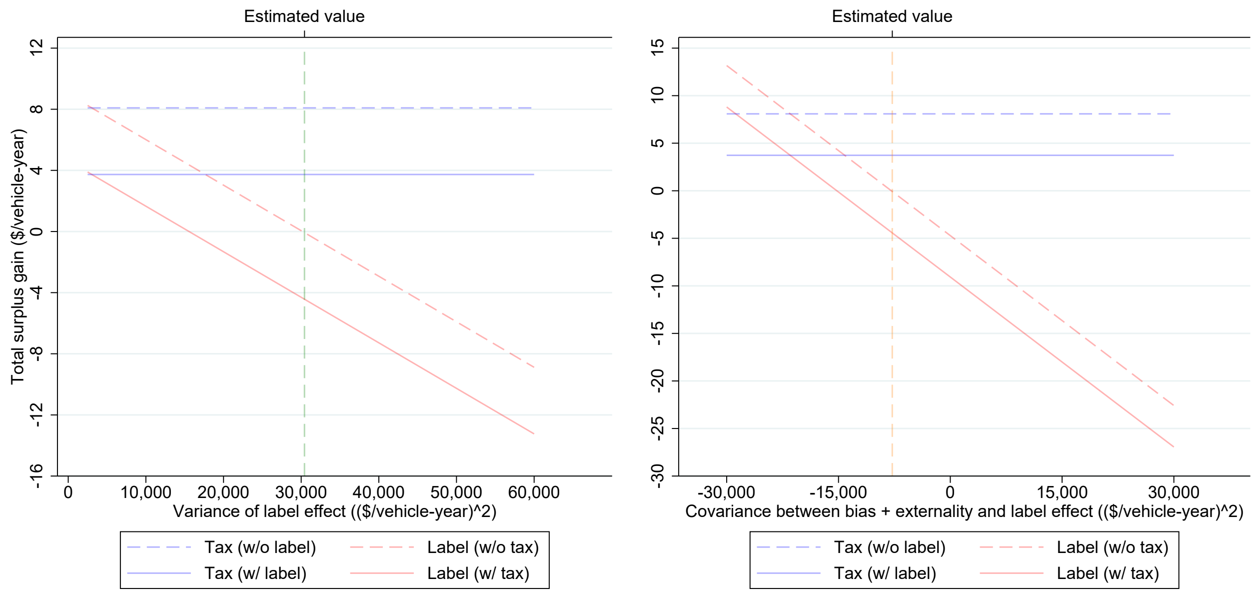
Figure 5: **Willingness-to-Pay to Receive Labels**



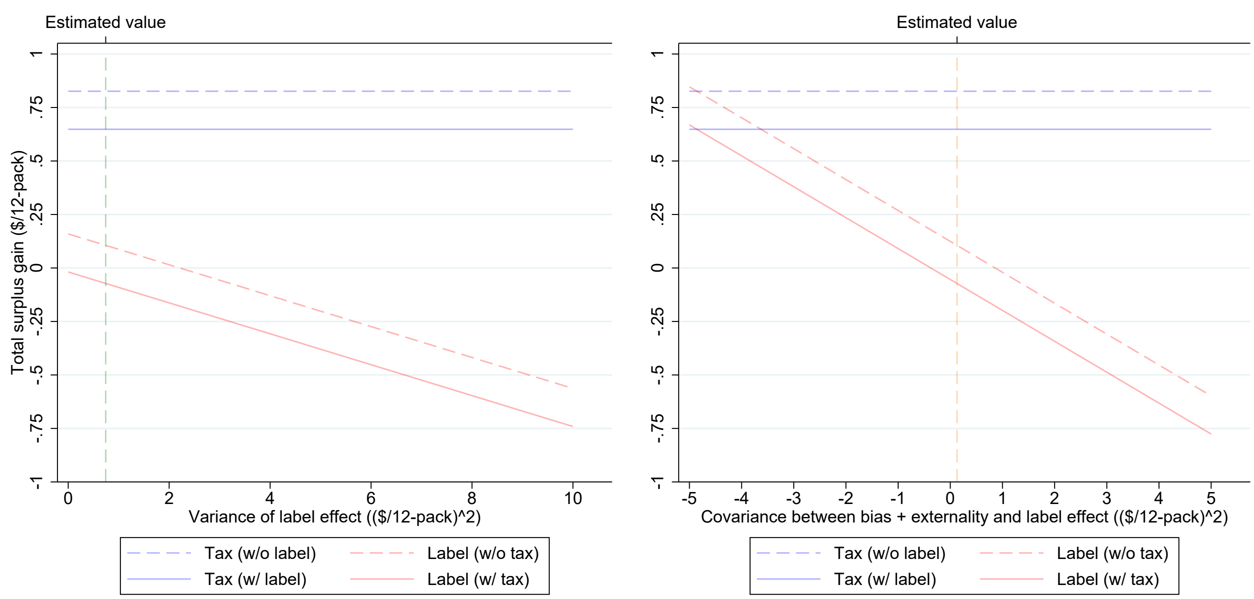
Notes: For this survey question, participants were told to assume that they had been selected to receive 12-packs of sugary drinks. The survey then elicited their hypothetical willingness-to-pay to receive or avoid receiving labels on the packages. This figure presents the average hypothetical WTP to receive labels.

Figure 6: **Total Surplus Effects Under Alternative Targeting Assumptions**

(a) **Cars Experiment**



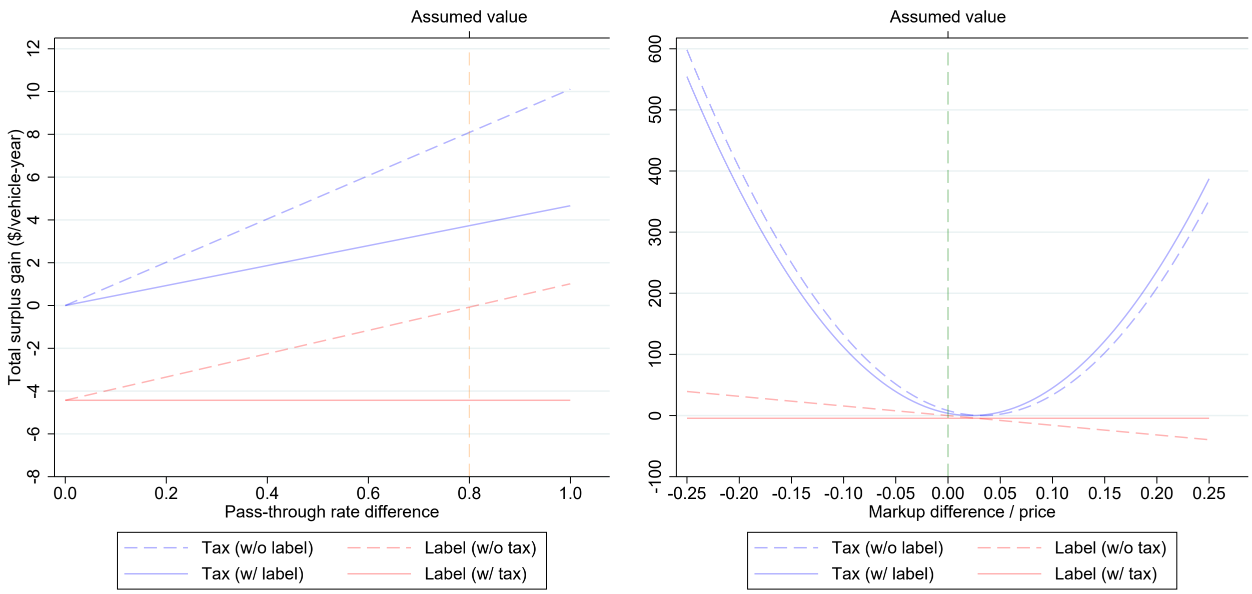
(b) **Drinks Experiment**



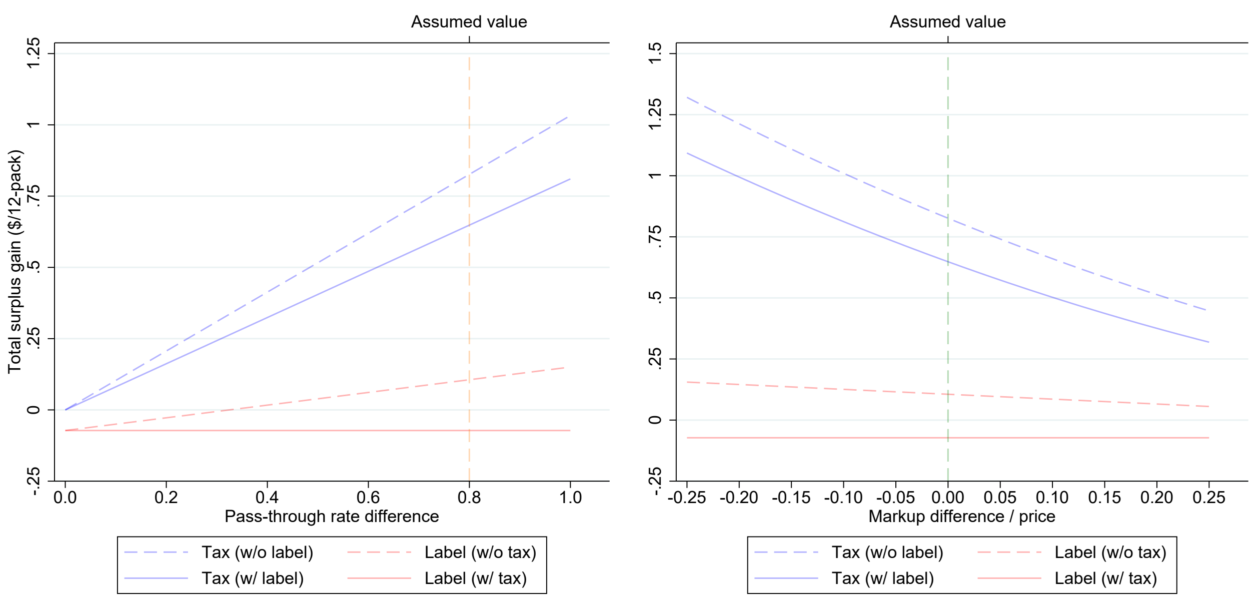
Notes: This figure presents the effects of labels and taxes on total surplus under alternative assumptions for $Var(\tau)$ and $Cov(\tau, \delta)$. The total surplus effects are computed using equations (19) and (20), given the other parameters reported in Table 2. The total surplus gain from the optimal tax $t^* = \mathbb{E}[\delta + \sigma\tau] - \mu$ is $-\frac{1}{2}t^{*2}D'_p$.

Figure 7: **Total Surplus Effects Under Alternative Pass-Through and Markup Assumptions**

(a) **Cars Experiment**



(b) **Drinks Experiment**



Notes: This figure presents the effects of labels and taxes on total surplus under alternative assumptions for ρ and μ. The total surplus effects are computed using equations (19) and (20), given the other parameters reported in Table 2. The total surplus gain from the optimal tax $t^* = \mathbb{E}\left[\delta + \sigma\tau\right] - \mu$ is $-\frac{1}{2}t^{*2}D_p'$.

Online Appendix

When Do “Nudges” Increase Welfare?

Hunt Allcott, Daniel Cohen, William Morrison, and Dmitry Taubinsky

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A Theory Appendix

A.1 Relation to Deliberative Competence Metrics of Ambuehl, Bernheim and Lusardi (2022)

Ambuehl, Bernheim and Lusardi (2022) propose measures of deliberative competence, which they use to evaluate financial literacy interventions. Ambuehl et al. evaluate frames that affect choice without directly affecting utility, which in our terminology is equivalent to NPIs with $\iota \equiv 0$. Ambuehl et al. also consider situations where all distortions come from consumer bias, and not externalities. Under these assumptions, they propose the following metrics:

Definition 1. An NPI improves deliberative competence under the L1 metric if $\mathbb{E}[|\gamma+\tau|] < \mathbb{E}[|\gamma|]$ and it improves deliberative competence under the L2 metric if $\mathbb{E}[(\gamma+\tau)^2] < \mathbb{E}[\gamma^2]$.

There are several differences between our welfare metrics and these definitions. First, because Ambuehl et al. study environments with an ex-ante unknown price, their metrics apply to the full population, rather than to marginal consumers. By contrast, our welfare formulas concern markets with observed producer prices. Thus, if the NPI affects the population versus the marginal consumers differentially, there will be a fundamental disconnect between our metrics and theirs.

Of course, one can adapt their definition to marginal consumers as well to make it more comparable, and we now focus on this more comparable definition:

Definition 2. Choosing an NPI with intensity $\sigma = 1$ rather than $\sigma = 0$ improves deliberative competence under the L1 metric if $\mathbb{E}_m[\gamma+\sigma\tau]$ is decreasing in $\sigma \in [0,1]$, and it improves deliberative competence under the L2 metric if $\mathbb{E}_m[(\gamma+\sigma\tau)^2]$ is decreasing in $\sigma \in [0,1]$.

Under this definition, minimizing the L2 metric corresponds to the special case of Proposition 1 under the assumptions that markets are perfectly competitive, that the pass-through parameter is $\rho = 1$, that the tax is $t = 0$, and that there is no aversiveness. If one of those assumptions fails, Proposition 1 shows that improvements in deliberative competence don't correspond to improvements in social surplus. These various failures are illustrated in Examples 4, 5, and 6, where the NPI improves deliberative competence but does not increase social surplus. For similar reasons, minimizing the L2 metric need not correspond to increases in consumer surplus, which is formalized in Proposition 2 below. Finally, it is clear that minimizing the L1 metric need not correspond to increases in social or consumer surplus under an even larger set of assumptions.

A.2 Impacts on Consumer and Producer Surplus

Lemma 1. *Suppose that $\mu\frac{d\varepsilon_D}{d\sigma} = -\mu\mathbb{E}_m[\tau]\frac{d\varepsilon_D}{dp}$. The equilibrium market price p varies with the NPI intensity σ as follows:*

$$\frac{dp}{d\sigma} = (1-\rho)\mathbb{E}_m[\tau]. \tag{23}$$

The assumption of the lemma holds when $\mu = 0$ or when the demand elasticity is approximately constant in σ and p. When $\mu > 0$ and the elasticity is not approximately constant, the assumption also mechanically holds when τ is homogeneous. Another example of when the assumption holds is when the demand curves D^{τ} that correspond to each set of consumers that experience a given treatment effect τ have the same elasticity.

The lemma shows that the lower is the pass-through of taxes to final consumer prices, the larger is the impact of NPIs on producer prices. When $\rho < 1$, any NPI that increases demand for a product will lead to higher producer prices, and thus potentially harm all consumers, irrespective of their bias. Thus, even if an NPI stimulates demand in a socially efficient way, it might do so by transferring surplus from consumers toproducers, with the size of the transfer potentially larger than the efficiency gain itself. Conversely, NPIs that increase social efficiency by depressing demand also lead to lower equilibrium prices, which generates additional benefits to consumers beyond improvements to decision quality.

It may also help to explicitly note that this effect on prices is not in some informal sense "second order" relative to the other effects of the NPIs, so that the effects on prices can be argued to be negligible relative to the conjectured benefits of "light-touch" interventions that have relatively small effects on behavior. We formalize this below by quantifying the effects on consumer surplus in markets with taxes fixed at $t = 0$.

Proposition 2. *Let q^* denote the equilibrium quantity purchased in the market. With a fixed tax t, the impacts of the NPI on consumer surplus W_C and producer surplus W_P are respectively given by*

$$\frac{dW_C}{d\sigma} = \frac{1}{2}\left((1-\rho)\frac{\partial}{\partial\sigma}Var_m\left[\gamma+\sigma\tau\right] + \frac{1}{2}\rho\frac{\partial}{\partial\sigma}\mathbb{E}_m\left[(\gamma+\sigma\tau)^2\right]\right)D'_p \tag{24}$$

$$- (1-\rho)\mathbb{E}_m[\tau]q^* + \frac{\partial I}{\partial\sigma} + (1-\rho)\mathbb{E}_m[\tau]\frac{\partial I}{\partial p} \tag{25}$$

$$\frac{dW_P}{d\sigma} = (1-\rho)\mathbb{E}_m[\tau]q^* - \mu\rho\mathbb{E}_m[\tau]D'_p \tag{26}$$

For example, when $\mu = I \equiv 0$, the impact on consumer surplus can be written as $\frac{dW_C}{d\sigma} = \frac{dW}{d\sigma} - (1-\rho)\mathbb{E}_m[\tau]q^*$; i.e., the impact on social surplus minus the impact on prices. The example in Section 1.2 has shown that even NPIs that only "debias" consumers can decrease social surplus. Proposition 2 thus shows that such NPIs can have an even more negative effects on consumer surplus if they increase demand for the product and therefore raise prices.

A.3 Proofs of Lemma 1) and Propositions 1 and 2

This appendix presents a series of derivations that together contain the proof of Proposition 1. Proofs of Lemma 1 and Proposition 2 are intermediate results derived in Appendices A.3.1 and A.3.3, respectively.

A.3.1 Pass-Through Formula (Proof of Lemma 1)

Consider the Lerner index $\theta := \frac{p-c'(q)-t}{p}\varepsilon_D$, which we assumed to be constant. Differentiating the equation $\theta p = (p - c'(q) - t)\varepsilon_D$ with respect to σ yields

$$\theta \frac{dp}{d\sigma} = \left(\frac{dp}{d\sigma} - c''(q)\frac{dq}{d\sigma}\right)\varepsilon_D + (p - c'(q) - t)\frac{d\varepsilon_D}{d\sigma}. \tag{27}$$

Now the equilibrium demand $\frac{dq}{d\sigma}$ is

$$\frac{dq}{d\sigma} = \frac{\partial D}{\partial \sigma} + \frac{\partial D}{\partial p}\frac{dp}{d\sigma} = -\frac{\partial D}{\partial p}\mathbb{E}_m[\tau] + \frac{\partial D}{\partial p}\frac{dp}{d\sigma}. \tag{28}$$

It thus follows that

$$\theta \frac{dp}{d\sigma} = \left(\frac{dp}{d\sigma} - c''(q)(-D'_p\mathbb{E}_m[\tau] + D'_p\frac{dp}{d\sigma})\right)\varepsilon_D + (p - c'(q) - t)\frac{d\varepsilon_D}{d\sigma}, \tag{29}$$

and thus

$$\frac{dp}{d\sigma}\left(1 - \theta - c''(q)D'_p\right) = -c''(q)D'_p\mathbb{E}_m[\tau] - (p - c'(q) - t)\frac{d\varepsilon_D}{d\sigma}, \tag{30}$$

or

$$\frac{dp}{d\sigma} = \frac{-c''(q)D'_p\mathbb{E}_m[\tau] - \mu\frac{d\varepsilon_D}{d\sigma}}{1 - \theta - c''(q)D'_p}. \tag{31}$$

Analogously, a tax t_c on consumers changes producer prices as follows (noting that in this case a tax is just a special case of an NPI with $\tau \equiv 1$):

$$\frac{dp}{dt_c} = \frac{c''(q)D'_p - \mu\frac{d\varepsilon_D}{dp}}{1 - \theta - c''(q)D'_p}. \tag{32}$$

The pass-through is $\frac{dp}{dt} = \rho = 1 + \frac{dp}{dt_c}$. Thus, if $\mu\frac{d\varepsilon_D}{d\sigma} = -\mu\mathbb{E}_m[\tau]\frac{d\varepsilon_D}{dp}$, then

$$\frac{dp}{d\sigma} = -\frac{dp}{dt}\mathbb{E}_m[\tau] = (1-\rho)\mathbb{E}_m[\tau]. \tag{33}$$

For example, $\mu\frac{d\varepsilon_D}{d\sigma} = -\mu\mathbb{E}_m[\tau]\frac{d\varepsilon_D}{dp}$ holds with constant-elasticity demand or homogeneous treatment effects. This establishes Lemma 1.

A.3.2 Optimal Tax Formula

The tax must maximize

$$W = \int_{v \geq p(t) - \gamma - \sigma\tau} v dF - c(q^*) + I. \tag{34}$$

Differentiating yields

$$\frac{dW}{dt} = -c'(q^*)\frac{dq^*}{dt} \tag{35}$$

$$- \int_{v=p(t)-\gamma-\sigma\tau} f(p(t)-\gamma-\sigma t)(p(t)-\gamma-\sigma\tau)(p'(t)) + \frac{\partial I}{\partial p}\frac{dp}{dt} \tag{36}$$

$$= -c'(q^*)\frac{dq^*}{dt} + D_p p'(t)\left(p(t) - \mathbb{E}_m[\gamma+\sigma\tau]\right) + \rho\frac{\partial I}{\partial p} \tag{37}$$

$$= \frac{dD}{dt}\left(p(t) - \mathbb{E}_m[\gamma+\sigma\tau]\right) - c'(q^*)) + \rho\frac{\partial I}{\partial p}, \tag{38}$$

where $q^* = Pr(v \geq p(t) - \gamma - \sigma\tau)$.

Substituting $p(t) - c' = \mu + t$ implies that

$$W'(t) = \frac{dD}{dt}\left(\mu + t - \mathbb{E}_m[\gamma+\sigma\tau]\right) + \rho\frac{\partial I}{\partial p}. \tag{39}$$

Setting $W'(t^*) = 0$ thus implies that

$$t^* = \mathbb{E}_m[\gamma+\sigma\tau] - \mu - \rho\frac{\frac{dI}{dp}}{\frac{dD}{dt}}, \tag{40}$$

or alternatively,

$$t^* = \mathbb{E}_m[\gamma+\sigma\tau] - \mu - \sigma\mathbb{E}_m[\Delta\iota], \tag{41}$$

where $\sigma\mathbb{E}_m[\Delta\iota]$ is the average difference in psychic costs that consumers on the margin obtain from purchasing the good versus not.

Under the assumption that terms of order $\frac{d^2D}{dt^2}t^2$ and $\frac{d}{dt}\frac{\partial}{\partial p}Dt^2$ are negligible, the welfare impact of the optimal tax is

$$W''(t^*)t^{*2}/2 = -\frac{dD}{dt}_{t=0} t^{*2}/2. \tag{42}$$

A.3.3 Impacts on Consumer, Producer, and Social Surplus in the Absence of Taxes

Consumer surplus is given by $W_C = \int_{v\geq p-\gamma-\sigma\tau} vdF - pq^* + I$, producer surplus is given by $W_P = p - c(q^*)$, and social surplus is given by $W = \int_{v\geq p-\gamma-\sigma\tau} vdF - c(q^*) + I$.

Equation (28) and Lemma 1 imply that the impact of σ on equilibrium quantity q^* is

$$\frac{dq^*}{d\sigma} = -\frac{\partial D}{\partial p}\mathbb{E}_m[\tau] + \frac{\partial D}{\partial p}(1-\rho)\mathbb{E}_m[\tau] = -\rho\mathbb{E}_m[\tau]D_p'. \tag{43}$$

Thus

$$\frac{d}{d\sigma}c(q^*) = c'(q^*)\rho\mathbb{E}_m[\tau]D_p'. \tag{44}$$

Using the multidimensional Leibniz rule, we have that

$$\frac{d}{d\sigma}\int_{v\geq p-\gamma-\sigma\tau} vdF = -\int_{v=p-\gamma-\sigma\tau}(p-\gamma-\sigma\tau)\left(-\tau+\frac{dp}{d\sigma}\right)dF \tag{45}$$

$$= \mathbb{E}_m[(p-\gamma-\sigma\tau)(-\tau+(1-\rho)\mathbb{E}_m[\tau])]D'_p \tag{46}$$

$$= -p\rho\mathbb{E}_m[\tau]D'_p - \mathbb{E}_m[(\gamma+\sigma\tau)(-\tau+(1-\rho)\mathbb{E}_m[\tau])]D'_p \tag{47}$$

$$= -p\rho\mathbb{E}_m[\tau]D'_p + \rho\mathbb{E}_m[(\gamma+\sigma\tau)\tau]D'_p \tag{48}$$

$$+ (1-\rho)\left(\mathbb{E}_m[(\gamma+\sigma\tau)\tau] - \mathbb{E}_m[\gamma+\sigma\tau]\mathbb{E}_m[\tau]\right)D'_p. \tag{49}$$

Now observe that for each τ,

$$\frac{1}{2}\frac{d}{d\sigma}(\gamma+\sigma\tau)^2 = \gamma\tau+\sigma\tau^2 = \tau(\gamma+\sigma\tau). \tag{50}$$

Thus,

$$\frac{1}{2}\frac{\partial}{\partial\sigma}\mathbb{E}_m[(\gamma+\sigma\tau)^2] = \mathbb{E}_m[\tau(\gamma+\sigma\tau)] \tag{51}$$

and

$$\frac{1}{2}\frac{\partial}{\partial\sigma}Var_m[(\gamma+\sigma\tau)^2] = \mathbb{E}_m[(\gamma+\sigma\tau)\tau] - \mathbb{E}_m[(\gamma+\sigma\tau)]\mathbb{E}_m[\tau]. \tag{52}$$

Substituting into our derivations of $\frac{d}{d\sigma}\int_{v\geq p-\gamma-\sigma\tau} vdF$ above we have that

$$\frac{d}{d\sigma}\int_{v\geq p-\gamma-\sigma\tau} vdF = -p\rho\mathbb{E}_m[\tau]D'_p + \frac{1}{2}(1-\rho)\frac{\partial}{\partial\sigma}Var_m\left[\gamma+\sigma\tau\right]D'_p + \frac{1}{2}\rho\frac{\partial}{\partial\sigma}\mathbb{E}_m\left[(\gamma+\sigma\tau)^2\right]D'_p. \tag{53}$$

The impact on consumer surplus is thus given by

$$\frac{dW_C}{d\sigma} = \frac{1}{2}(1-\rho)\frac{\partial}{\partial\sigma}Var_m\left[\gamma+\sigma\tau\right]D'_p + \frac{1}{2}\rho\frac{\partial}{\partial\sigma}\mathbb{E}_m\left[(\gamma+\sigma\tau)^2\right]D'_p \tag{54}$$

$$-\left(\frac{dp}{d\sigma}q^* + p\frac{dq^*}{d\sigma}\right) - p\rho\mathbb{E}_m[\tau]D'_p + \frac{dI}{d\sigma} \tag{55}$$

$$= \frac{1}{2}(1-\rho)\frac{\partial}{\partial\sigma}Var_m\left[\gamma+\sigma\tau\right]D'_p + \frac{1}{2}\rho\frac{\partial}{\partial\sigma}\mathbb{E}_m\left[(\gamma+\sigma\tau)^2\right]D'_p - (1-\rho)\mathbb{E}_m[\tau]q^* + \frac{dI}{d\sigma}. \tag{56}$$

The impact on producer surplus is given by

$$\frac{dW_P}{d\sigma} = \frac{dp}{d\sigma}q^* + p\frac{dq^*}{d\sigma} - \frac{d}{d\sigma}c(q^*) \tag{57}$$

$$= (1-\rho)\mathbb{E}_m[\tau]q^* - p\rho\mathbb{E}_m[\tau]D'_p - c'(q^*)\rho\mathbb{E}_m[\tau]D'_p \tag{58}$$

$$= (1-\rho)\mathbb{E}_m[\tau]q^* - (p-c'(q^*))\rho\mathbb{E}_m[\tau]D'_p \tag{59}$$

$$= (1-\rho)\mathbb{E}_m[\tau]q^* - \mu\rho\mathbb{E}_m[\tau]D'_p. \tag{60}$$

Putting this together, the impact on social surplus $W = W_C + W_P$ is

$$\frac{dW}{d\sigma} = \frac{1}{2}(1-\rho)\frac{\partial}{\partial\sigma}Var_m\left[\gamma+\sigma\tau\right]D'_p + \frac{1}{2}\rho\frac{\partial}{\partial\sigma}\mathbb{E}_m\left[(\gamma+\sigma\tau-\mu)^2\right]D'_p + \frac{dI}{d\sigma}. \tag{61}$$

Finally, to obtain the statement of Proposition 1, note that

$$\begin{aligned}\frac{dI}{d\sigma} &= \frac{\partial I}{\partial\sigma} + \frac{dp}{d\sigma}\frac{\partial I}{dp} \\ &= \frac{\partial I}{\partial\sigma} + (1-\rho)\mathbb{E}_m[\tau]\frac{\partial I}{\partial p}. \end{aligned}\tag{62}$$

A.3.4 Impacts on Consumer and Producer Surplus with a Fixed Tax

Our formulas for $\frac{dW_P}{d\sigma}$ and $\frac{dW_C}{d\sigma}$ are identical if there is instead a fixed tax t on producers. The reason is that on the producer side, the tax t can be considered to simply be part of the cost function, in which case all calculations are identical. On the consumer side, the tax t on producers does not independently affect consumers, given a producer price p.

A.3.5 Impacts on Social Surplus with a Fixed Tax

A fixed tax t on producers has the same social welfare effect as a tax $t_c = t$ on consumers. Now imposing a fixed tax t_c on consumers is equivalent to assuming that bias is given by $\gamma' = \gamma - t_c$.

From above, we thus trivially have that when the tax is fixed at some value t,

$$\frac{dW}{d\sigma} = \frac{1}{2}(1-\rho)\frac{\partial}{\partial\sigma}Var_m\left[\gamma+\sigma\tau\right]D'_p + \frac{1}{2}\rho\frac{\partial}{\partial\sigma}\mathbb{E}_m\left[(\gamma+\sigma\tau-t-\mu)^2\right]D'_p \tag{63}$$

where $\mu = p - c'(q) - t$, and where we use that $Var_m\left[\gamma+\sigma\tau\right] = Var_m\left[\gamma+\sigma\tau-t\right]$.

A.3.6 Impact on Social Surplus with Optimal Tax

By the envelope theorem, $\frac{dW}{d\sigma} = \frac{\partial W}{\partial\sigma}$, where the partial derivative treats the optimal tax $t^* = \mathbb{E}_m[\gamma+\sigma\tau] - \mu - \mathbb{E}_m[\Delta\iota]$ as fixed.

Now at the optimal tax,

$$\rho\mathbb{E}_m[(\gamma+\sigma\tau-t^*-\mu)\tau] = \rho\mathbb{E}[(\gamma+\sigma\tau-\mathbb{E}[\gamma+\sigma\tau])\tau] + \rho\mathbb{E}_m[\tau]\mathbb{E}_m[\Delta\iota] \tag{64}$$

$$= \frac{1}{2}\rho\frac{\partial}{\partial\sigma}Var_m\left[\gamma+\sigma\tau\right] + \rho\mathbb{E}_m[\tau]\mathbb{E}_m[\sigma\Delta\iota]. \tag{65}$$

It thus follows that at the optimal tax,

$$\frac{dW}{d\sigma} = \frac{\partial}{\partial\sigma}Var_m\left[\gamma+\sigma\tau\right]D'_p + \rho\mathbb{E}_m[\tau]\frac{\partial I}{\partial p} + \frac{dI}{d\sigma} \tag{66}$$

where we use that $\frac{\partial I}{\partial p} = \mathbb{E}_m[\sigma\Delta\iota]D'_p$. Substituting the expression in equation (62) gives

$$\frac{dW}{d\sigma} = \frac{\partial}{\partial\sigma}Var_m\left[\gamma + \sigma\tau\right]D'_p + \frac{\partial I}{\partial\sigma} + \mathbb{E}_m[\tau]\frac{\partial I}{\partial p}. \tag{67}$$

A.4 Generalization to Many Goods

More generally, suppose that there are J different types of products, indexed by $j = \{1, \ldots, J\}$, and that each consumer must buy at least one of the products. The model in the body of the paper corresponds to the special case with two products, where product $j = 2$ is an outside good with a fixed price.

A given consumer's set of valuations and biases is given by the vectors $v = (v_1, \ldots, v_j)$ and $\gamma = (\gamma_1, \ldots, \gamma_J)$. For simplicity, we assume that the NPI only directly affects valuations of a single product, which we label product 1 without loss of generality, and we let $\sigma\tau$ continue denoting the distribution of treatment effects on this product, where σ is the strength of the NPI. The demand curve for product j is $D_j(p, \sigma)$, where p is the vector of prices. Denote the own-price elasticity of demand for product j by $\varepsilon_D^j = -\frac{\partial D_j}{dp_j}\cdot\frac{p_j}{D_j}$, and denote the elasticity of demand of product i with respect to the price p_j of product j by ε_D^{ij}. The general case where the NPI affects multiple goods is an immediate corollary that is obtained by taking the sum of the effects on each good.

Each firm produces only one type of product, at cost $c_j(q)$ to for q units, and pays tax t_j per unit. Let $\theta_j := \frac{p - c'_j(q) - t_j}{p}\varepsilon_D^j$ denote the market conduct parameter for product j, and assume that it is constant. We let $\mu_j = p - c'_j(q) - t_j$ denote the markup.

We define ρ_{jk} to be the impact of a tax on producers of j on the price of product k. We denote by $\Delta p_{1j} = p_1 - p_j$ the relative price of product 1 to product j, and we let $\Delta\rho_{1j} := \rho_{11} - \rho_{1j}$ denote the pass through of t_1 to Δp_{1j}.

For any function $X(v, \gamma, \sigma, \tau)$ we define $\mathbb{E}_{ij}[X(v, \gamma, \sigma, \tau)]$ to be the conditional expectation of X over the set of consumers who are on the margin of buying either product i or j. We define $\mathbb{E}_1[X] = \sum_j \mathbb{E}_{1j}[X]$ as the expectation over the set of consumers who are on the margin for buying product 1 versus any other product. We utilize analogous notation for the covariance and variance operators. With a slight abuse of notation, we define

$$\frac{\partial}{\partial\sigma}\mathbb{E}_{1j}[X(v, \gamma, \sigma, \tau)] := \frac{d}{d\sigma'}\int_{\{(v,\gamma,\tau)|v_1 - p_1 - \sigma\tau = v_2 - p_2\}} X(v, \gamma, \sigma', \tau)dF\,|_{\sigma'=\sigma}\ . \tag{68}$$

for any function X.

A.4.1 Impact of NPI on prices

Analogous to Lemma 1,

$$\frac{\partial D_j}{\partial\sigma} = -\frac{\partial D_j}{\partial p_1}\mathbb{E}_1[\tau]. \tag{69}$$

Similarly, consider a consumer tax t_c on product 1. The impact of a marginal change in σ on prices is equivalent to a marginal change of $\mathbb{E}_1[\tau]$ in t_c. Thus, since $\rho_{jj} = 1 + \frac{dp_j}{dt_c}$, we have that

$$\frac{dp_1}{d\sigma} = -\frac{dp_1}{dt_c}\mathbb{E}_1[\tau] = (1-\rho_{11})\mathbb{E}_1[\tau], \tag{70}$$

and more generally

$$\frac{dp_j}{d\sigma} = -\frac{dp_j}{dt_c}\mathbb{E}_{1j}[\tau] = (1-\rho_{1j})\mathbb{E}_{1j}[\tau]. \tag{71}$$

A.4.2 Impact of NPI on Welfare

Calculations analogous to the proof of Proposition 1 imply the following:

Proposition 3. *Assume that $\frac{d}{dp_k}\varepsilon_D^{ij}$ and $\frac{d}{d\sigma}\varepsilon_D^{1j}$ are negligible in the case where $\mu > 0$ for all i, j, k. Define $\Delta\gamma_{1j} = \gamma_1 - \gamma_j$ and $\Delta\mu_{1j} = \mu_1 - \mu_j$. Then the marginal change in social surplus from an NPI in a market with taxes t_j on products j is*

$$\frac{dW}{d\sigma} = -\sum_j \left[\frac{1}{2}(1-\Delta\rho_{1j})\frac{\partial}{\partial\sigma}Var_{1j}\left[\Delta\gamma_{1j}+\sigma\tau\right] + \frac{1}{2}(\Delta\rho_{1j})\frac{\partial}{\partial\sigma}\mathbb{E}_{1j}\left[(\Delta\gamma_{1j}+\sigma\tau - t_1 + t_j - \Delta\mu_{1j})^2\right]\right]\frac{\partial}{\partial p_1}D_j \tag{72}$$

$$+\frac{\partial I}{\partial\sigma} + (1-\rho_{11})\mathbb{E}_1[\tau]\frac{\partial I}{\partial p_1}. \tag{73}$$

For intuition, note that when there are only two goods, the general expression above reduces to an expression almost identical to the one in the body of the paper. The main difference is that when the price of the outside good is exogenous, the key parameter is the pass-through of the tax on good 1 to the relative price, $p_1 - p_2$, of good 1. The key bias statistic is how much people overvalue good 1 relative to good 2, $\gamma_1 - \gamma_2$. And the interaction with market power is now captured by the difference $\mu_1 - \mu_j$. The general formula for many goods is obtained by taking the sum of the welfare impacts corresponding to each pair of good 1 and some other good j.

B Experimental Design Appendix

B.1 Cars Experiment

Figure A1: **Cars Experiment: Valuation if Gas is Free**

2020 Subaru Legacy

- 182 horsepower
- 30 miles/gallon
- 2.5 L, 4-cylinder
- 5 seats
- CVT transmission

Details: click here

If gas is free, the maximum I'd pay per year to lease the Subaru Legacy is:

$ ______ per year

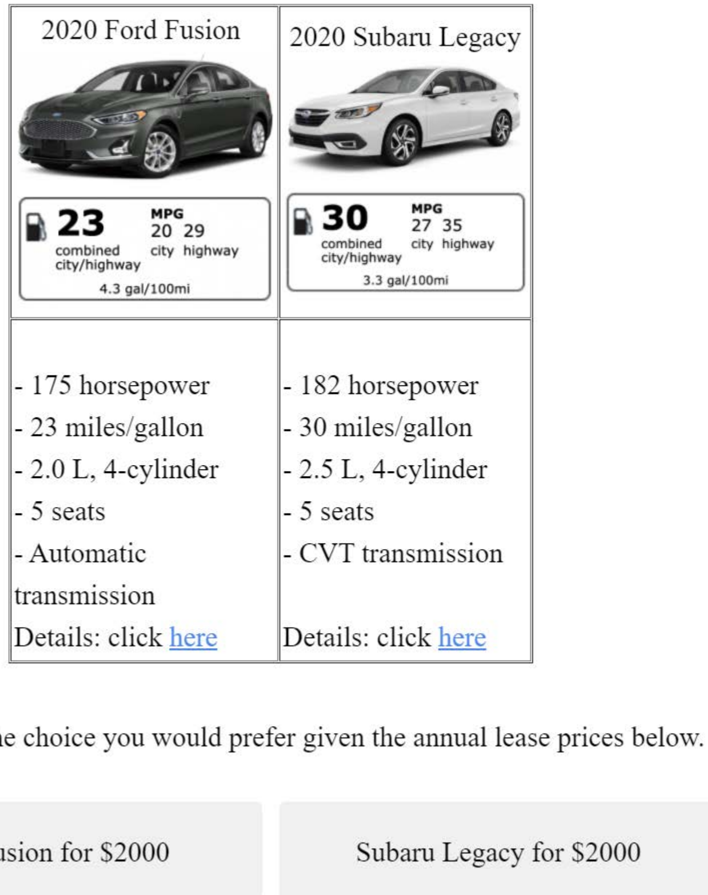
Figure A2: **Cars Experiment: Baseline Multiple Price List**

2020 Ford Fusion	2020 Subaru Legacy
	
- 175 horsepower - 23 miles/gallon - 2.0 L, 4-cylinder - 5 seats - Automatic transmission Details: click here	- 182 horsepower - 30 miles/gallon - 2.5 L, 4-cylinder - 5 seats - CVT transmission Details: click here

Please click on the choice you would prefer given the annual lease prices below.

Ford Fusion for $2000	Subaru Legacy for $2000

Figure A3: **Cars Experiment: Endline Multiple Price List with Full MPG Label**



B.2 Drinks Experiment

Figure A4: **Drinks Experiment: Recruitment Ad**

Figure A5: **Drinks Experiment: Baseline Multiple Price List**

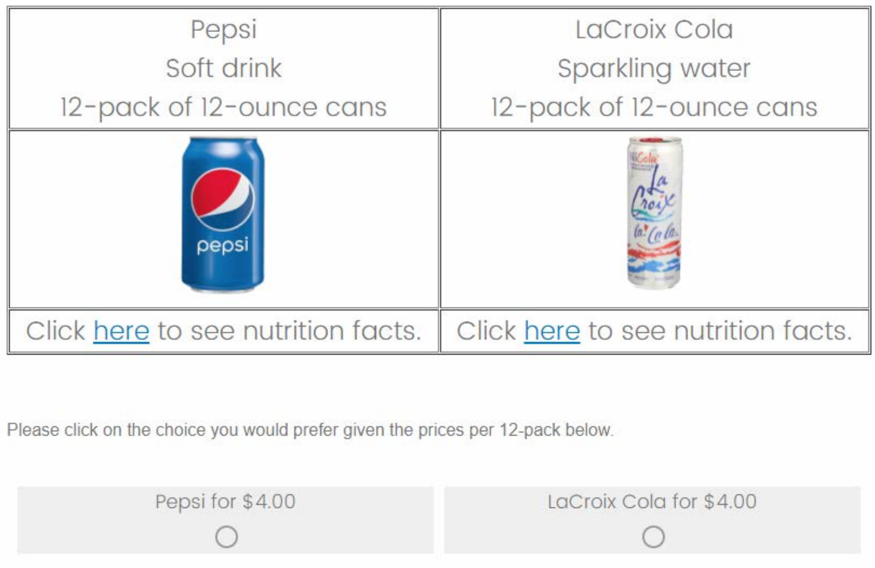


Figure A6: **Drinks Experiment: Endline Multiple Price List with Stop Sign Label**

C Data Appendix

Table A1: **Cars Experiment Descriptive Statistics**

	(1) Experiment sample	(2) US population
Income under $50,000	0.37	0.39
College degree (for age $\geq$ 25)	0.41	0.33
Male	0.53	0.49
White	0.70	0.75
Under age 45	0.41	0.44
2019 miles driven	10,803	11,131
2019 gas price ($/gallon)	2.79	2.60
Average WTP if gas is free ($/vehicle-year)	2,771	
Average baseline WTP ($/vehicle-year)	1,553	

Notes: US population averages for demographic variables are from the 2016–2020 American Community Surveys. US population average *2019 miles driven* and *2019 gas price* are from the 2017 National Household Travel Survey (Oak Ridge National Laboratory undated) and U.S. Energy Information Administration (2020), respectively. *Average WTP if gas is free* is the respondent's average valuation of the Accord, Altima, Fusion, and Legacy in the baseline questions when told to imagine that gas is free. The experiment sample includes 1,267 participants.

Table A2: **Drinks Experiment Descriptive Statistics**

	(1) Experiment sample	(2) US population
Income under $50,000	0.63	0.39
College degree (for age $\geq$ 25)	0.41	0.33
Male	0.47	0.49
White	0.84	0.75
Under age 45	0.40	0.44
Nutrition knowledge	0.70	0.70
Self-control	0.41	0.77

Notes: US population averages for demographic variables are from the 2016–2020 American Community Surveys. *Nutrition knowledge* is the share correct out of 28 questions from the General Nutrition Knowledge Questionnaire (Kliemann et al. 2016). *Self-control* is level of agreement with the statement, "I drink soda pop or other sugar-sweetened beverages more often than I should." Responses were coded as "Definitely" = 0, "Mostly" = 1/3, "Somewhat" = 2/3, and "Not at all" = 1. National averages are as reported in Allcott, Lockwood and Taubinsky (2019a). The experiment sample includes 2,619 participants.

Table A3: **Cars Experiment: Covariate Balance**

Variable	(1) Control Mean/SD	(2) Full MPG Mean/SD	(3) Fuel cost Mean/SD	(4) Personalized fuel cost Mean/SD	(5) SmartWay Mean/SD	T-test P-value (1)-(2)	(1)-(3)	(1)-(4)	(1)-(5)
Household income ($000s)	74.53	75.80	76.25	72.86	74.01	0.67	0.56	0.58	0.86
	(47.81)	(46.87)	(48.68)	(48.11)	(48.21)				
College degree	0.40	0.42	0.34	0.41	0.45	0.67	0.05**	0.72	0.13
	(0.49)	(0.49)	(0.48)	(0.49)	(0.50)				
Male	0.54	0.52	0.56	0.51	0.53	0.57	0.40	0.38	0.95
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)				
White	0.72	0.69	0.73	0.63	0.73	0.32	0.57	0.00***	0.77
	(0.45)	(0.46)	(0.44)	(0.48)	(0.45)				
Age	50.05	49.78	50.86	48.22	50.46	0.78	0.42	0.07*	0.68
	(16.42)	(15.45)	(16.38)	(16.25)	(15.95)				
N	530	494	516	492	502				
F-test of joint significance (p-value)						0.87	0.19	0.05*	0.67
F-test, number of observations						1024	1046	1022	1032

Notes: This table presents tests of covariate balance between treatment conditions in the cars experiment. The first five columns present means and standard deviations. The final four columns present p-values of t-tests of equality between each treatment condition and the control group.

Table A4: **Drinks Experiment: Covariate Balance**

Variable	(1) Control Mean/SD	(2) Nutrition Mean/SD	(3) Stop sign Mean/SD	(4) Graphic Mean/SD	T-test P-value (1)-(2)	(1)-(3)	(1)-(4)
Household income ($000s)	47.10	47.73	47.53	47.14	0.61	0.72	0.97
	(38.60)	(39.35)	(38.46)	(38.70)			
College degree	0.41	0.41	0.40	0.41	0.80	0.69	0.88
	(0.49)	(0.49)	(0.49)	(0.49)			
Male	0.47	0.47	0.45	0.48	0.87	0.13	0.73
	(0.50)	(0.50)	(0.50)	(0.50)			
White	0.84	0.85	0.83	0.84	0.50	0.29	0.95
	(0.36)	(0.36)	(0.37)	(0.36)			
Age	48.97	48.06	47.52	48.51	0.09*	0.01***	0.38
	(16.59)	(16.82)	(16.49)	(16.57)			
N	2001	1923	1980	1953			
F-test of joint significance (p-value)					0.45	0.02**	0.97
F-test, number of observations					3924	3981	3954

Notes: This table presents tests of covariate balance between treatment conditions in the drinks experiment. The first four columns present means and standard deviations. The final three columns present p-values of t-tests of equality between each treatment condition and the control group.

D Empirical Results Appendix

Table A5: **Average Treatment Effects, Variance, and Covariance for "Marginal" Consumers**

(a) **Cars Experiment**

	(1) OLS	(2) OLS	(3) Mixed effects	(4) Mixed effects
Treated	-85.18***	-82.04	-85.01***	-82.08
	(32.91)	(77.30)	(32.78)	(77.04)
Bias × Treated		-0.08		-0.09
		(0.06)		(0.06)
Externality × Treated		0.43		0.47
		(1.36)		(1.35)
Cov(bias, treatment effect)		-24,083		-26,085
(standard error)		18,988		19,177
Cov(externality, treatment effect)		-11		-7
(standard error)		652		650
Var(treatment effect)			33,254	
(standard error)			18,691	
Number of participants	482	482	482	482
Number of observations	964	964	964	964

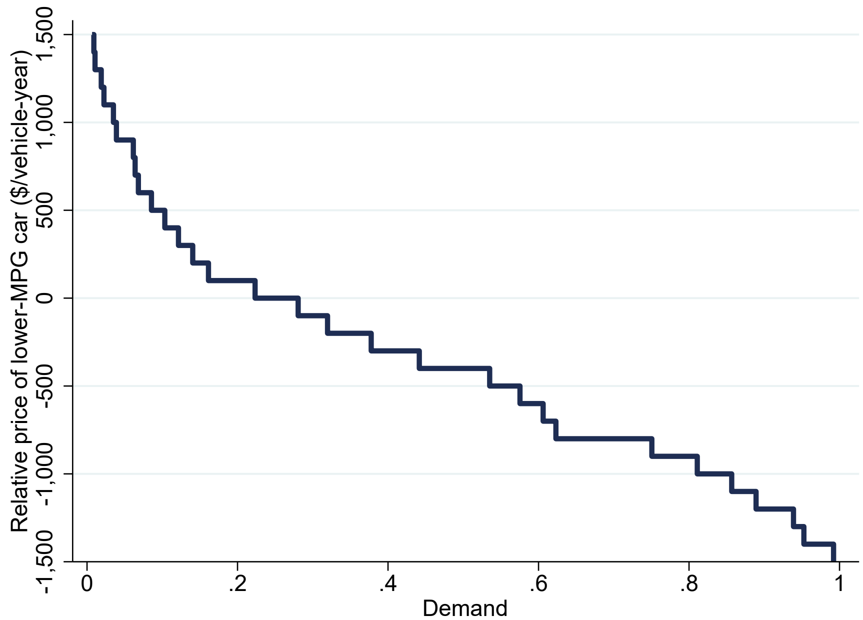
(b) **Drinks Experiment**

	(1) OLS	(2) OLS	(3) Mixed effects	(4) Mixed effects
Treated	-0.43***	-0.65***	-0.43***	-0.65***
	(0.04)	(0.09)	(0.04)	(0.09)
Bias × Treated		0.08**		0.08**
		(0.04)		(0.04)
Cov(bias, treatment effect)		0.130		0.129
(standard error)		0.055		0.055
Var(treatment effect)			0.743	
(standard error)			0.187	
Number of participants	2,619	2,619	2,619	2,619
Number of observations	7,857	7,857	7,857	7,857

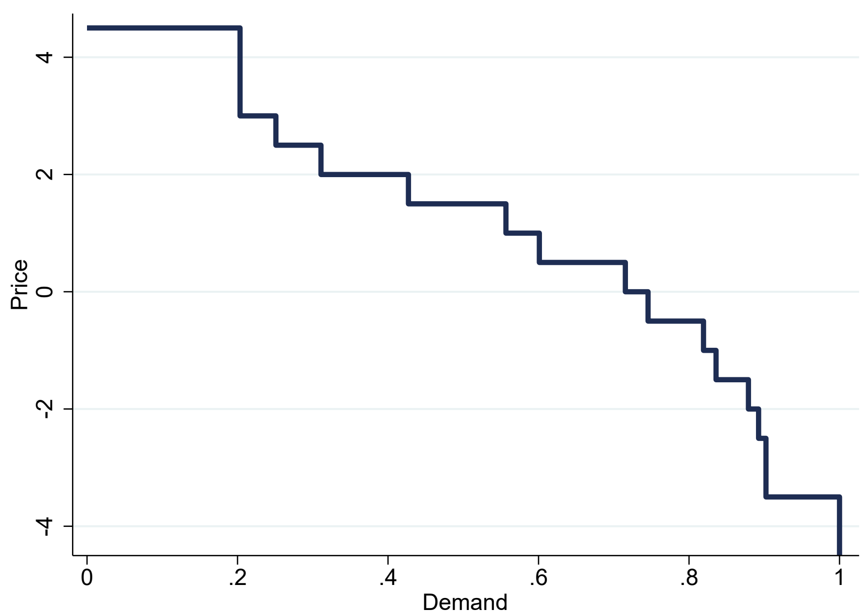
Notes: Panels (a) and (b), respectively, present estimated ATEs, variances, and covariances for the cars experiment and drinks experiment, pooling across all labels. Columns 1 and 2 present fixed coefficient (OLS) versions of equations (12) and (16), respectively, while columns 3 and 4 present the full random coefficient (mixed effects) models. The samples are limited to participants with below-median absolute value of relative WTP for both product pairs.

Figure A7: **Baseline Demand Curves**

(a) **Cars Experiment**



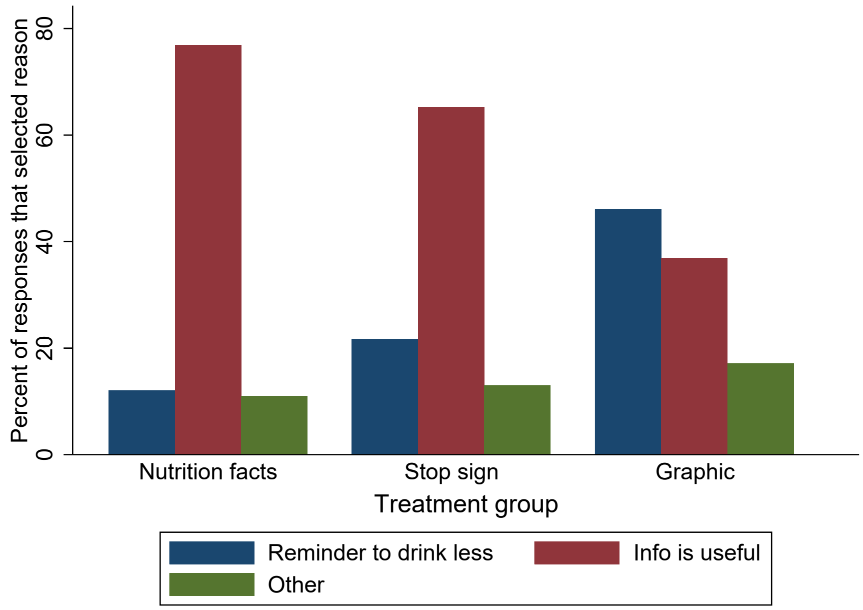
(b) **Drinks Experiment**



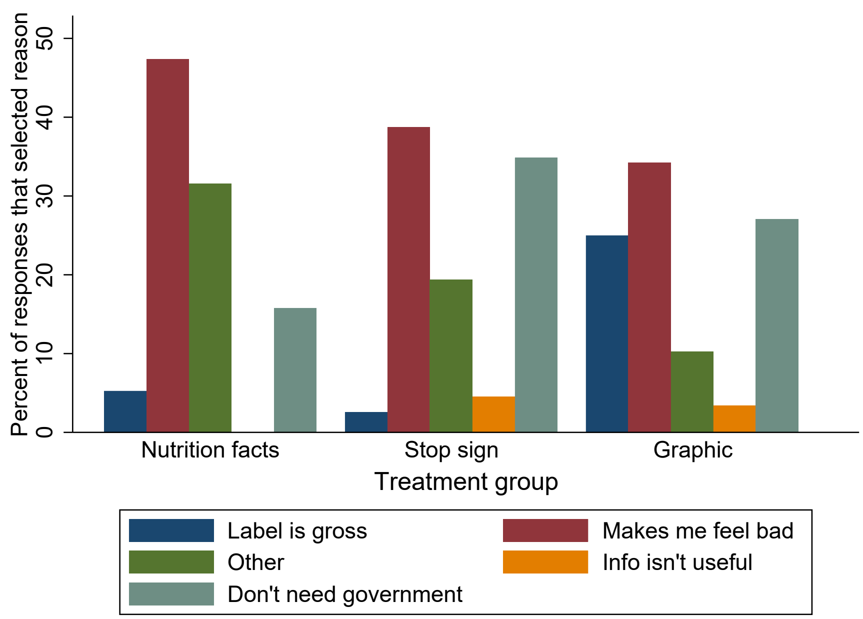
Notes: Panels (a) and (b), respectively, present the baseline demand curves for the cars experiment and drinks experiment.

Figure A8: **Reasons for Wanting or Not Wanting Sugary Drink Labels**

(a) **Reasons for Wanting Labels**



(b) **Reasons for Not Wanting Labels**



Notes: For this survey question, participants were told to assume that they had been selected to receive 12-packs of sugary drinks. The survey then asked if they would prefer to receive drink containers with or without the label shown to their treatment group. Panels (a) and (b), respectively, present the distribution of responses to questions about why participants wanted to receive drink containers with and without the labels.

D.1 Estimates of Table 1 by Label

Table A6: **Average Treatment Effects, Variance, and Covariance for Full MPG Label (Cars Experiment)**

	(1) OLS	(2) OLS	(3) Mixed effects	(4) Mixed effects
Treated	-68.52**	-46.45	-67.85**	-48.22
	(31.53)	(77.46)	(31.57)	(77.48)
Bias × Treated		0.03		0.02
		(0.05)		(0.05)
Externality × Treated		-0.54		-0.46
		(1.46)		(1.46)
Cov(bias, treatment effect)		17,438		12,019
(standard error)		27,033		27,883
Cov(externality, treatment effect)		-176		-165
(standard error)		691		691
Var(treatment effect)			40,201	
(standard error)			17,339	
Number of participants	512	512	512	512
Number of observations	1,024	1,024	1,024	1,024

Notes: Table presents estimated ATEs, variances, and covariances for the Full MPG label in the cars experiment. Columns 1 and 2 present fixed coefficient (OLS) versions of equations (12) and (16), respectively, while columns 3 and 4 present the full random coefficient (mixed effects) models. All regressions also include controls for bias and externality as well as indicators for product pairs j and MPL order.

Table A7: **Average Treatment Effects, Variance, and Covariance for Average Cost Label (Cars Experiment)**

	(1) OLS	(2) OLS	(3) Mixed effects	(4) Mixed effects
Treated	-66.72**	-68.19	-66.16**	-66.70
	(30.77)	(77.22)	(30.93)	(77.31)
Bias × Treated		-0.01		-0.03
		(0.05)		(0.05)
Externality × Treated		0.06		0.10
		(1.41)		(1.41)
Cov(bias, treatment effect)		-6,702		-18,247
(standard error)		27,589		28,034
Cov(externality, treatment effect)		6		-19
(standard error)		681		682
Var(treatment effect)			30,179	
(standard error)			17,155	
Number of participants	523	523	523	523
Number of observations	1,046	1,046	1,046	1,046

Notes: Table presents estimated ATEs, variances, and covariances for the Average Cost label in the cars experiment. Columns 1 and 2 present fixed coefficient (OLS) versions of equations (12) and (16), respectively, while columns 3 and 4 present the full random coefficient (mixed effects) models. All regressions also include controls for bias and externality as well as indicators for product pairs j and MPL order.

Table A8: **Average Treatment Effects, Variance, and Covariance for Personalized Cost Label (Cars Experiment)**

	(1) OLS	(2) OLS	(3) Mixed effects	(4) Mixed effects
Treated	-36.72	-18.67	-35.92	-16.92
	(30.04)	(76.28)	(30.07)	(75.99)
Bias × Treated		-0.00		-0.01
		(0.05)		(0.05)
Externality × Treated		-0.35		-0.35
		(1.43)		(1.42)
Cov(bias, treatment effect)		-3,308		-7,453
(standard error)		28,016		28,448
Cov(externality, treatment effect)		-171		-184
(standard error)		663		662
Var(treatment effect)			13,550	
(standard error)			15,610	
Number of participants	511	511	511	511
Number of observations	1,022	1,022	1,022	1,022

Notes: Table presents estimated ATEs, variances, and covariances for the Personalized Cost label in the cars experiment. Columns 1 and 2 present fixed coefficient (OLS) versions of equations (12) and (16), respectively, while columns 3 and 4 present the full random coefficient (mixed effects) models. All regressions also include controls for bias and externality as well as indicators for product pairs j and MPL order.

Table A9: **Average Treatment Effects, Variance, and Covariance for SmartWay Label (Cars Experiment)**

	(1) OLS	(2) OLS	(3) Mixed effects	(4) Mixed effects
Treated	-64.23**	-85.65	-64.43**	-89.12
	(31.45)	(75.55)	(31.42)	(75.64)
Bias × Treated		-0.03		-0.04
		(0.05)		(0.05)
Externality × Treated		0.50		0.58
		(1.38)		(1.39)
Cov(bias, treatment effect)		-15,166		-20,555
(standard error)		29,008		29,360
Cov(externality, treatment effect)		160		174
(standard error)		676		677
Var(treatment effect)			36,295	
(standard error)			20,825	
Number of participants	516	516	516	516
Number of observations	1,032	1,032	1,032	1,032

Notes: Table presents estimated ATEs, variances, and covariances for the SmartWay label in the cars experiment. Columns 1 and 2 present fixed coefficient (OLS) versions of equations (12) and (16), respectively, while columns 3 and 4 present the full random coefficient (mixed effects) models. All regressions also include controls for bias and externality as well as indicators for product pairs j and MPL order.

Table A10: **Average Treatment Effects, Variance, and Covariance for Nutrition Facts Label (SSB Experiment)**

	(1) OLS	(2) OLS	(3) Mixed effects	(4) Mixed effects
Treated	-0.45***	-0.52***	-0.45***	-0.52***
	(0.06)	(0.13)	(0.06)	(0.13)
Bias × Treated		0.03		0.03
		(0.05)		(0.05)
Cov(bias, treatment effect)		0.046		0.046
(standard error)		0.078		0.078
Var(treatment effect)			0.796	
(standard error)			0.224	
Number of participants	1,308	1,308	1,308	1,308
Number of observations	3,924	3,924	3,924	3,924

Notes: Table presents estimated ATEs, variances, and covariances for the nutrition facts label in the SSB experiment. Columns 1 and 2 present fixed coefficient (OLS) versions of equations (12) and (16), respectively, while columns 3 and 4 present the full random coefficient (mixed effects) models. All regressions also include controls for bias and externality as well as indicators for product pairs j and MPL order.

Table A11: **Average Treatment Effects, Variance, and Covariance for Stop Sign Warning Label (SSB Experiment)**

	(1) OLS	(2) OLS	(3) Mixed effects	(4) Mixed effects
Treated	-0.34***	-0.53***	-0.34***	-0.53***
	(0.05)	(0.11)	(0.05)	(0.11)
Bias × Treated		0.07*		0.07*
		(0.04)		(0.04)
Cov(bias, treatment effect)		0.108		0.108
(standard error)		0.063		0.063
Var(treatment effect)			0.355	
(standard error)			0.212	
Number of participants	1,327	1,327	1,327	1,327
Number of observations	3,981	3,981	3,981	3,981

Notes: Table presents estimated ATEs, variances, and covariances for the stop sign warning label in the SSB experiment. Columns 1 and 2 present fixed coefficient (OLS) versions of equations (12) and (16), respectively, while columns 3 and 4 present the full random coefficient (mixed effects) models. All regressions also include controls for bias and externality as well as indicators for product pairs j and MPL order.

Table A12: **Average Treatment Effects, Variance, and Covariance for Graphic Warning Label (SSB Experiment)**

	(1) OLS	(2) OLS	(3) Mixed effects	(4) Mixed effects
Treated	-0.51***	-0.90***	-0.51***	-0.90***
	(0.06)	(0.15)	(0.06)	(0.15)
Bias × Treated		0.15***		0.15***
		(0.05)		(0.05)
Cov(bias, treatment effect)		0.234		0.233
(standard error)		0.082		0.082
Var(treatment effect)			1.080	
(standard error)			0.264	
Number of participants	1,318	1,318	1,318	1,318
Number of observations	3,954	3,954	3,954	3,954

Notes: Table presents estimated ATEs, variances, and covariances for the graphic warning label in the SSB experiment. Columns 1 and 2 present fixed coefficient (OLS) versions of equations (12) and (16), respectively, while columns 3 and 4 present the full random coefficient (mixed effects) models. All regressions also include controls for bias and externality as well as indicators for product pairs j and MPL order.

D.2 Alternate Covariance Estimation Strategy for the Drinks Experiment

In this appendix, we consider an alternative strategy to estimating $Cov\left[\tau,\gamma\right]$: we estimate $Cov\left[\tilde{w},\hat{\gamma}\right]$, the sample covariance between WTP change and bias. This strategy does not require the the normality assumptions from our primary strategy in Section 3.2.1.

To see when $Cov\left[\tilde{w},\hat{\gamma}\right]=Cov\left[\tau,\gamma\right]$, we substitute the model for $\tilde{w}_{ij}$ from equation (12) into $Cov\left[\tilde{w},\hat{\gamma}\right]$:

$$Cov\left[\tilde{w}_{ij},\hat{\gamma}_{ij}\right]=Cov\left[\tau_{ij}\cdot T_i+\tilde{\epsilon}_{ij},\hat{\gamma}_{ij}\right] \tag{74}$$

$$=Cov\left[\tau_{ij},\hat{\gamma}_{ij}\right]+Cov\left[\tilde{\epsilon}_{ij},\hat{\gamma}_{ij}\right]. \tag{75}$$

From this equation, we see that two conditions are sufficient for $Cov\left[\tilde{w},\hat{\gamma}\right]=Cov\left[\tau,\gamma\right]$: (i) $Cov\left[\tau,\hat{\gamma}\right]=Cov\left[\tau,\gamma\right]$ and (ii) $Cov\left[\tilde{\epsilon},\hat{\gamma}\right]=0$. Condition (i) follows from Assumption 1 in Section 3.2.1, so this strategy is no more restrictive than our primary strategy. In the cars experiment, $\hat{\gamma}_{ij}$ is constructed using baseline WTP w_{ij1}, and is thus mechanically correlated with $\tilde{\epsilon}_{ij}$, violating condition (ii). In the drinks experiment, however, $\hat{\gamma}_{ij}$ is constructed independently of w_{ij}, and thus condition (ii) is plausible.

In the data, $Cov\left[\tilde{w},\hat{\gamma}\right]\approx 0.11$. This is very similar to our primary estimate of 0.13.

E Welfare Analysis Appendix

Table A13: **Parameters and Welfare Analysis: Individual Labels**

(a) **Cars Experiment**

Parameter	(1) Full MPG	(2) Average cost	(3) Personalized cost	(4) SmartWay
$\mathbb{E}[\tau]$	-69	-67	-37	-64
	(32)	(31)	(30)	(31)
$Var[\tau]$	$40,201$	$30,179$	$13,550$	$36,295$
	$(17,339)$	$(17,155)$	$(15,610)$	$(20,825)$
$Cov[\gamma,\tau]$	$12,019$	$-18,247$	$-7,453$	$-20,555$
	$(27,883)$	$(28,034)$	$(28,448)$	$(29,360)$
$Cov[\phi,\tau]$	-165	-19	-184	174
	(691)	(682)	(662)	(677)
$\Delta W(t=0)$	-14.14	6.69	3.42	5.99
$\Delta W(t=t^*)$	-19.04	1.89	0.51	1.33

(b) **Drinks Experiment**

Parameter	(1) Nutrition facts	(2) Stop Sign warning	(3) Graphic warning
$\mathbb{E}[\tau]$	-0.45	-0.34	-0.51
	(0.06)	(0.05)	(0.06)
$Var[\tau]$	0.80	0.35	1.08
	(0.22)	(0.21)	(0.26)
$Cov[\gamma,\tau]$	0.04	0.11	0.24
	(0.08)	(0.06)	(0.08)
$Cov[\phi,\tau]$	0.00	0.00	0.00
$\Delta W(t=0)$	0.12	0.10	0.10
$\Delta W(t=t^*)$	-0.06	-0.04	-0.11

Notes: This table presents parameter estimates and total surplus effects separately for each label in each experiment. Bias $\mathbb{E}[\gamma]$, externality $\mathbb{E}[\phi]$, demand slope D'_p, pass-through ρ, and markup μ are as reported in Table 2. $\Delta W(t=0)$ and $\Delta W(t=t^*)$ are computed using equations (19) and (20), given the parameters reported above. “Unit” is “vehicle-year” for cars and “12-pack” for sugary drinks. Standard errors are in parentheses.