Preferences for Rights*

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Abstract

Public discourse about in-kind transfers often appeals to "preferences for rights" — for instance, the "right to health care" or "right to counsel" for indigent legal defense. Preferences for rights are "non-welfarist" if the person values the right per se, holding fixed how the right instrumentally affects others' utilities. We test for non-welfarist preferences for rights, and their relationship to redistributive choices, with incentivized online experiments (N = 1,800). Participants face choices about allocating rights goods (lawyers, health care) and benchmark goods (bus passes, YMCA memberships) to tenants facing eviction. We implement a share of choices. In two of three experiments, more than half of participants allocate rights goods in ways that are consistent with preferences for rights and dominated if preferences were entirely welfarist. Dominated behaviors are more common with rights goods than benchmarks. In a fourth experiment, those with preferences for rights also exhibit "anti-targeting," where they redistribute lawyers and health care more universally than benchmark goods to recipients whose incomes differ. At least 26% of participants are non-welfarist, while at most 31% are welfarist.

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Political debates over public provision of in-kind transfers often invoke "rights." Advocates for universal health care appeal to the "right to health care."[1] Since 2017, 17 U.S. cities and four states passed "right to counsel" policies, which give free lawyers to defendants in eviction cases that are not covered by the 6th Amendment.[2] The United Nations recognizes rights as far-reaching as the "right to enjoy the benefits of scientific progress."

Despite rights' prominent role in how people justify policy, economists often restrict attention to "welfarist" allocative preferences. Welfarist preferences solely depend on recipients' utilities.[3] That is, a welfarist person i values allocating in-kind good $y = (y_1, \ldots, y_J)$ among the J others in society only because the good instrumentally enters their utility. Then i's allocative utility is $v_i(y) = f(u_1(y_1), \ldots, u_J(y_J))$ for some function $f(\cdot)$ which aggregates others' utilities $u_j(\cdot)$. This class of welfarist allocative utilities nests standard redistributive motives, Becker (1974)-type altruism, inequity aversion (Fehr and Schmidt, 1999), and paternalism. Welfarist reasons to value rights include that constitutional rights constrain harmful despots, or that behaving as if there are rights increases cooperation (Dal Bó et al., 2010).

Valuing rights intrinsically, and not because they instrumentally enter others' utilities, is "non-welfarist." If i has non-welfarist preferences for rights, her allocative utility is $v_i(y) = f(u_1(y_1), \ldots, u_J(y_J), \phi_i(y))$. Such preferences depend on how allocating y affects rights $\phi_i(y)$, holding fixed how y affects others' utilities. This class of non-welfarist allocative utilities includes the frameworks in Tobin (1970) or Sen (1985).

We study non-welfarist "preferences for rights," distinguishing them from other welfarist concerns for rights or morals. To what extent do people have preferences for rights? How do preferences for rights influence redistributive choices? We answer these questions by conducting experiments in which participants allocate real goods to needy recipients. In several experiments, more than half of participants exhibit behaviors that are consistent with preferences for rights, but which are dominated if participants were exclusively welfarist. Those who exhibit preferences for rights make more universal (less targeted) redistributive choices when recipients differ in need.

Whether people actually have non-welfarist preferences is an empirical question — and one with significant implications for economics. First, economists (should) care about the basic science of measuring allocative/redistributive preferences, as they influence the optimal allocation of scarce resources (Hausman and McPherson, 1993). Second, it is unclear what advocates mean when invoking rights like the "right to shelter." If advocates just mean that shelter is instrumentally

[1] When advocating for the Affordable Care Act, President Barack Obama said, "Health care is not a privilege for the fortunate few — it is a right" (Obama, 2013).

[2] The American Civil Liberties Union argues, "Tenants' right to legal representation in eviction cases is a civil liberties issue, a gender justice, racial justice, and economic justice issue" (ACLU, 2022).

[3] We use the term "allocative preferences" to mean preferences about the allocation of goods to *ex ante* identical others. We use the term "redistributive preferences" to mean preferences about allocation of goods to others whose need differs (e.g., they have different incomes). We use "welfarist"/"non-welfarist" rather than "individualistic"/"non-individualistic" but either applies here.

valuable, then rights are not an extra rationale for the in-kind transfer beyond welfarist arguments (e.g., Currie and Gahvari, 2008). Third, in an influential paper, Kaplow and Shavell (2001) argue that non-welfarist Social Welfare Functions (SWFs) do not satisfy the Pareto principle. However, as Kaplow and Shavell (2001) note (p. 285), Pareto SWFs can accommodate non-welfarist concerns if such concerns enter individuals' utility directly. This paper thus takes up the empirical challenge of testing for non-welfarist preferences, which would salvage SWFs that place value on rights.[4]

We use a simple framework to define non-welfarist preferences and derive testable predictions (Section 1). In the framework, a social planner or Spectator has utility over allocations in society. Allocative utility is separable in a welfarist component that aggregates others' utilities and a non-welfarist component that has a reference-dependent form (Kőszegi and Rabin, 2006). The reference point is the Spectator's views about how society should be (e.g., "everyone should have access to a given good"). If upholding normative views about society matters more for rights than other goods, Spectators place greater value on the non-welfarist component. Because the loss domain is especially costly, the framework predicts "anti-targeting" — that is, the Spectator redistributes rights goods more universally than non-rights goods.

Non-welfarist preferences are hard to study empirically. Once a right legally exists, everyone has it. Little variation remains to identify preferences for the right. For this reason, it is difficult to measure willingness to pay for freedom of speech, or for lawyers in criminal cases which are covered by the 6$^{\text{th}}$ Amendment. Among rights that do not legally exist, it is not obvious how to separate non-welfarist and welfarist preferences, particularly with observational methods.

We overcome these challenges by fielding laboratory experiments that give tight control over the economic environment (Section 2). Although the framework employs special functional forms, the experiments give nonparametric tests. Participants ($N = 1{,}800$ Spectators) face incentivized choices about allocating goods to low-income households. Our key idea is to manipulate what the goods are, isolating non-welfarist concerns with experiments designed to fix instrumental valuations across goods. We contrast goods related to rights (treatment) with goods that are valuable but less related to rights (control). The "rights goods" are lawyers for tenants facing eviction and health care — two goods that feature in controversial policy debates about rights. The "benchmark goods" are YMCA memberships and bus passes, which are less related to rights. The difference in behaviors with rights goods versus benchmarks yields a lower bound on preferences for rights, because Spectators may believe that benchmark goods relate to rights. We implement some choices over lawyers and benchmarks by partnering with a nonprofit.[5]

Our first three experiments test for preferences for features of rights that are motivated by moral psychology (Section 3). These experiments examine Spectators' allocations to *ex ante*

[4] If i herself does not value rights, but others do, and i's allocative utility aggregates others' utilities, then i may be a welfarist who still cares about rights.

[5] Health care choices are always hypothetical. We embed several tests for the impact of incentives with incentivized goods, and reject even small differences in behavior due to lack of incentives.

identical recipients. Experiment 1 studies "inalienability," or the idea that there is a discrete harm from taking a right away. We inform Spectators that a lottery allocated a good to one recipient. Spectators can save money for the nonprofit's future tenant programming by rerunning the lottery, which may take from one recipient and give to another. We elicit Spectators' willingness to pay (WTP) to preserve the lottery, where WTP is measured as the amount of money saved that is required for Spectators to be indifferent to rerunning the lottery. Spectators' WTP does not internalize recipients' reference-dependent utility, as we inform Spectators — and emphasize with confirmation checks — that neither recipient would know about the initial allocation.

Experiment 1 is a natural test for non-welfarist preferences as it modifies the "trolley problem," a classic thought experiment used to probe deontological intuitions (Foot, 1967; Thomson, 1976).[6] Unlike the thought experiment, our experiment is real and incentivized, and we study differences in deontological behavior across goods. Since recipients' utilities depend only on the final allocation, welfarist Spectators should not pay to preserve the lottery.

Yet Spectators do pay to preserve the lottery, even though doing so has no welfarist payoff and actively destroys surplus. Even when the lottery involves the benchmark goods, 46% of Spectators have positive WTP to preserve the lottery. This result may reflect non-welfarist preferences for procedural justice, and aligns with meaningful rates of deontological behaviors in trolley problems (Bénabou et al., 2024, and references therein). Allaying concerns that high levels among the benchmarks merely reflect elicitation errors, we find that these preferences are significantly more concentrated among Spectators who face lotteries over rights goods. With rights goods, 53% of Spectators exhibit a positive WTP (s.e. of difference: 2.5 pp; p-value $= 0.002$). Preferences not to rerun the lottery are similar for health care and lawyers.

Experiment 2 studies "dignity of choice." Consider a welfarist who is certain that a recipient would choose $\$y$ in cash over a lawyer. This welfarist should never be willing to pay to let the recipient choose between the two rather than giving them $\$y$ directly. Experiment 2 elicits Spectators' beliefs that recipients choose $\$y$ in cash over the good. Focusing on Spectators who are certain that the recipient chooses cash, we elicit Spectators' WTP, again measured in dollars saved for future tenant programs, to provide choice rather than $\$y$ directly. Thus, Experiment 2 trades off instrumental costs of providing choice with possible non-instrumental benefits.

Spectators have positive WTP for choice even when certain that the choice will not be exercised. Similar to Experiment 1, we find evidence of non-welfarist preferences even for the benchmark goods. Among Spectators who are at least 90% sure the recipient will choose cash, 39% are still willing to pay for choice, meaning they burn surplus if the choice is not exercised. These preferences are again stronger for rights goods, where 57% of these Spectators have positive WTP

[6]The trolley problem is: Imagine a runaway trolley that is on a track and would kill five people. Should you switch the trolley's track so that it kills just one person? Utilitarians often switch, whereas deontologists often do not.

for choice (s.e. of difference: 2.8, $p < 0.001$).[7] Preferences for choice are stronger for lawyers, but both goods are statistically distinguishable from the benchmarks.

Experiment 3 studies "egalitarianism." Classical welfarists' WTP for providing additional rights goods does not depend on the share already endowed with the good.[8] Non-welfarists' WTP depends on the share already endowed, if they are egalitarians who especially want all of society to get the good. We find that Spectators do have a higher WTP to provide an additional rights good when 9 out of 10 already get it, relative to when smaller shares get it (1 or 5 out of 10). But the increase in WTP is similar between benchmark and rights goods. Our interpretation, to be conservative, is that we only detect evidence of inalienability and dignity of choice. However, the evidence is also consistent with equal egalitarian preferences for rights and benchmarks.

Having found evidence of preferences for rights in Experiments 1 and 2, we next consider their implications for Spectators' redistributive choices and targeting (Section 4). In Experiment 4, Spectators choose how to allocate goods among 10 anonymous recipients with varying need, as indicated by their incomes. Spectators choose between giving the good to everyone, or the good plus cash to people with lower incomes. These choices hold the total redistributive budget fixed. For instance, we price lawyers at \$500 per recipient and fix the budget at \$5,000. Spectators can give 10 lawyers to tenants facing eviction; or give the poorest tenant a lawyer and \$4,500; or give each of the poorest two tenants a lawyer and \$2,000; and so on.

Spectators target rights goods more universally than benchmarks, and preferences for rights may explain why. Spectators are 17 pp (64% of the benchmark mean; s.e.: 2 pp) more likely to provide rights goods to all 10 recipients than to provide benchmarks or cash universally. Propensity to "anti-target" (provide goods universally) is highly correlated with non-welfarist behaviors in Experiment 1 and 2. This result confirms the framework's predicted relationship between normative reference points and redistribution.

To additionally quantify the importance of non-welfarist preferences, we next compute the share of people with welfarist versus non-welfarist preferences. Spectators are at least partially welfarist if their allocation decisions depend on the good's instrumental benefits to recipients. To identify welfarists, we conduct an information-provision experiment that shocks Spectators' beliefs about the instrumental benefits of the rights goods. They then have the choice of revising their initial allocation in the targeting task (Experiment 4). We conservatively label people as (exclusively) non-welfarist if they both exhibit preferences for rights and do not revise given information. We label people who revise given information as (partially) welfarist.

[7] Willingness to pay is 0.51 s.d. higher (s.e.: 0.11, $p < 0.001$) among those who express 100% certainty. Additionally, we contrast the welfarist value of choice, which is the probability the choice is exercised (obtained via incentivized belief elicitations) times the value of choice if exercised (obtained in a separate experiment). This welfarist value of choice is lower than the WTP for choice for 31% of Spectators allocating rights goods and 24% of benchmarks (s.e. of difference: 2.2).

[8] As we discuss when describing the experiment (Section 3.3), non-classical welfarists' WTP also depends on the share if they have inequity aversion (Fehr and Schmidt, 1999).

We find that 26% have exclusively non-welfarist preferences, which is only slightly less than the 31% who have partially welfarist preferences. The remainder cannot be unambiguously classified. This result challenges the prevailing approach in welfare economics of ignoring how people value rights, at least in the domains of health care or lawyers. It suggests that welfarist Social Welfare Functions can still value rights, in a manner consistent with Kaplow and Shavell (2001).

We conclude by showing how preferences for rights correlate with political preferences and support for government policies involving in-kind provision. We find that preferences for rights are uncorrelated with political preferences — we do not merely pick up liberals, for instance. They negatively correlate with income. Preferences for rights predict support for Right to Counsel but not universal health care, perhaps because health care is more politicized.

Related Literature. Philosophers, political scientists, and economists have questioned exclusive focus on recipients' utility in social allocation problems. Economists have proposed non-welfarist frameworks like Tobin (1970)'s specific egalitarianism, Rawls (1971)'s primary goods, Sen (1985)'s capabilities approach, and Saez and Stantcheva (2016)'s generalized social marginal welfare weights.[9] Gasparini and Pinto (2006) consider theoretical properties of non-welfarist social preferences, and Weinzierl (2014, 2017) studies departures from welfarist views. These frameworks, together with distinguished literatures in experimental ethics (Appiah, 2010; Knobe et al., 2012) and moral psychology (Doris et al., 2006), motivate our experiments.

First, we add to a behavioral-economics literature on the foundations of moral preferences and fairness concerns (e.g., Rabin, 1993; Fehr and Schmidt, 1999; Dana et al., 2007). A key finding in the literature is that people often exhibit moral concerns, yet there is heterogeneity (Enke, 2023).[10] Importantly, moral preferences are still welfarist if they emerge from altruism (Fehr and Schmidt, 2006) or (self-)signaling motives (Bénabou and Tirole, 2011), among other reasons. A smaller strand in the literature isolates non-welfarist preferences for morals per se (e.g., Chen and Schonger, 2022; Sullivan, 2023; Bénabou et al., 2024), and is thus especially relevant. Relative to this work, we test for preferences for rights as a new example of non-welfarist moral concerns, in a policy-relevant setting where isolating that the moral concerns are non-welfarist is key. More specifically, we conduct two new experiments for non-welfarist moral concerns (Experiments 1 and 2); show how these moral concerns relate to redistributive preferences (Experiment 4); and manipulate the

[9] An important contribution by Holmes and Sunstein (2000) situates rights within a cost-benefit framework and emphasizes the costs of public provision. We measure the potential benefits to weigh against such costs.

[10] Examples of moral concerns range from preferences not to lie (e.g., Fischbacher and Föllmi-Heusi, 2013; Gneezy et al., 2018), to religion and debt (e.g., Bursztyn et al., 2019), to voting patterns (e.g., Enke, 2020), among many others.

goods to crisply isolate rights from other behavioral or deontological motivations.[11,12]

Second, we build on the behavioral and experimental literature on redistributive decisions (Levitt and List, 2007; List, 2007; Cappelen et al., 2013, 2020; Capozza and Srinivasan, 2024).[13] Recent work pushes beyond standard redistributive preferences to examine paternalism (Ambuehl et al., 2021; Bartling et al., 2023), but still embeds these views within welfarist frameworks. We stress different non-welfarist considerations and conduct experiments to isolate them.

Third, we contribute to the public-finance literature on the rationales for in-kind transfers. Due to Kaplow and Shavell (2001)'s criticism, economists rarely appeal to rights to justify providing assistance in-kind. Instead, economists focus on various classical rationales or non-classical (paternalistic) appeals to internalities (Currie and Gahvari, 2008). Both classical and paternalist rationales are still welfarist, because they view social welfare as exclusively depending on experienced utilities in society (Chetty, 2015) or choices made in welfare-relevant domains (Bernheim and Rangel, 2009; Bernheim, 2016; Bernheim and Taubinsky, 2018).[14] Liscow and Pershing (2022) conduct survey experiments to decompose hypothetical demand for in-kind redistribution. Relative to this work, we conduct experiments that quantify preferences for rights without relying on stated attitudes about rights, and we show how these preferences influence real redistributive decisions.[15] More broadly, we also relate to a recent literature that measures views about normative trade-offs in public finance (Stantcheva, 2021; Cappelen et al., 2023; Hvidberg et al., 2023).

Finally, we add to political-economy research that considers economic justifications for rights or liberal institutions (North, 1991; Acemoglu et al., 2005; Mialon and Rubin, 2008). We test whether people value an institution (i.e., rights) per se, complementing experiments that study how institutions affect outcomes like cooperation (Dal Bó, 2014; Dannenberg and Gallier, 2020).

[11] We build off Polman (2012), who studies demand to change initial allocations for others, but who cannot identify preferences for rights. First, in some of Polman (2012)'s experiments, the recipients know the initial allocation, so the Spectators may be welfarists who aggregate others' loss aversion. Second, several experiments are thought experiments as they involve WTP for non-quantifiable outcomes (e.g., "ability to get dates"). Third, several experiments measure WTP to improve an outcome versus stop the outcome from getting worse. If Spectators perceive recipients as having concave utility, then higher WTP to avoid losses is consistent with welfarist preferences.

[12] Related to Experiment 2, Bartling et al. (2014a) and Bobadilla-Suarez et al. (2017) find subjects value choice for themselves, but do not study how subjects value choice for others. Andreoni et al. (2020) study fairness in the presence of uncertainty and find subjects apply deontological principles when allocating lottery tickets.

[13] Like Fisman et al. (2007), we study how bystanders trade off efficiency for redistributive considerations, but focus on willingness to pay for ensuring rights rather than altruistic giving. Like Alatas et al. (2012), we quantify how people target in-kind goods, but document a different phenomenon — "anti-targeting" of rights goods — and propose explanations. Like Charité et al. (2022), we consider how non-classical forces affect Spectators' choices for others.

[14] Kaplow (2024) discusses the use of welfarist and non-welfarist SWFs in economics.

[15] One of Liscow and Pershing (2022)'s treatments makes the right to in-kind goods salient and studies hypothetical support for in-kind or cash redistribution by the government. They find that the salience framing does not affect redistributive choices. This result may come from people already having preferences for rights in both treatment and control, attenuating the salience treatment: 60% of Liscow and Pershing (2022)'s participants say that rights at least partially drove their choices.

1 Conceptual Framework

1.1 Intuition

In a simple benchmark, the social planner (in our setting, a Spectator in an allocation experiment) redistributes toward the poor. Suppose the Spectator has an exogenous budget to distribute among a population with heterogeneous initial endowments. The Spectator maximizes welfare by allocating goods to those with the highest marginal utility, optionally weighted by social welfare weights. Absent too much complementarity between the good and the endowment, diminishing marginal utility over wealth causes targeting toward those with low endowments.

If the Spectator redistributes relative to a reference point, that can reduce this targeting motive. Suppose the Spectator has loss aversion relative to a reference point for some individuals. Allocating to any individual in the loss domain achieves a higher marginal welfare gain than allocating to those already above the reference point. This motive pushes the Spectator to allocate goods in a "flatter" manner. We model rights as affecting the Spectator's reference point, which then changes how the Spectator targets in-kind provision.

Appendix C provides details and proofs.

1.2 Setup and standard targeting

Setup. Consider Spectator i's preferences for allocating goods to others $j \in \{1, \ldots, J\}$. Allocative utility is welfarist over an allocation of goods $y \equiv (y_1, \ldots, y_J)$ if it can be expressed using the form:

$$v_i(x, y;\ u_j(\cdot)) = f_i\left(\left\{u_j(x_j, y_j)\right\}_j\right), \tag{1}$$

where f_i aggregates others' utilities $u_j(\cdot)$, y_j is the consumption bundle offered to person j, and $x \equiv (x_1, \ldots, x_J)$ captures other aspects of utility or endowments (e.g., income). The subscript j in $u_j(\cdot)$ stresses that social welfare depends on j's experienced utility. Welfarist allocative utilities depend only on the realized utility in society. Utilitarian, redistributive, Rawlsian (maximin), paternalistic, and many other commonly used allocative utilities in economics are welfarist.[16]

Equation (1) does not meaningfully restrict recipients' utilities. Recipients' utilities may themselves be non-classical (e.g., reference-dependent).[17]

To simplify notation, we consider a benchmark with homogeneous utility functions[18] and

[16] Note that allocative utilities and Social Welfare Functions are distinct. Allocative utilities refer to individuals in society's preferences to allocate to others. SWFs capture how the social planner aggregates preferences of individuals in society. SWFs may be welfarist and value rights if individuals themselves are non-welfarist.

[17] The formulation does exclude altruism among recipients, as their utilities depend only on their own bundle. That is, for simplicity, recipients' utilities depend only on (x_j, y_j). We could easily generalize Equation (1) and what follows to allow u_j to depend on the vector (x, y) instead.

[18] In our setting, as in many allocation problems, the Spectators have no information about (heterogeneous)

where allocative utility is additively separable in recipients' utilities:

$$v(x, y; \gamma) = \sum_{j=1}^{J} \gamma_j u(x_j, y_j) \tag{2}$$

for exogenous welfare weights $\gamma \equiv (\gamma_1, \ldots, \gamma_J)$.

Standard Targeting. Consider allocating m indivisible goods among the population of J recipients. Fixing the vector of welfare weights, the optimal allocation $\{y_j^*\}$ solves

$$\max \sum_{j=1}^{J} \gamma_j u\left(x_j, y_j^*\right), \text{ such that } \sum_j y_j^* \leq m. \tag{3}$$

In this benchmark, the Spectator gives the goods to the $k \leq m$ people where the social marginal welfare gains are largest.[19] If y is not too complementary with x, then the social marginal welfare gain is maximized by providing to those with smaller endowments and higher welfare weights.

1.3 Reference points and kinked utility

We now let allocative utility depend on welfarist and non-welfarist components:

$$v_i = f_i\left(\left\{u_j(x_j, y_j)\right\}_j, \ \phi_{iy}(x, y)\right). \tag{4}$$

Here, $\phi_{iy}(x, y)$ corresponds to rights that enter the Spectator's utility, separately from how (x, y) influence others' utilities. We let ϕ be indexed by y to stress that different goods could have different non-welfarist utilities. As our experiments detect non-welfarist preferences in the context of rights, we often call the non-welfarist utilities in this study "preferences for rights."

We impose homogeneity in recipients' utilities and parameterize the ϕ function with a form of Kőszegi and Rabin (2006)-type reference dependence:

$$v_i(x, y; \gamma, r) = \sum_{j=1}^{J} \gamma_j \left(u(x_j, y_j) + \eta_y \phi\left(y_j \mid r_{ij}\right)\right) \tag{5}$$

$$\phi\left(y_j \mid r_{ij}\right) = \begin{cases} u\left(x_j, y_j\right) - u\left(x_j, r_{ij}\right) & \text{if } y_j > r_{ij} \\ \lambda\left[u\left(x_j, y_j\right) - u\left(x_j, r_{ij}\right)\right] & \text{if } y_j \leq r_{ij} \end{cases}. \tag{6}$$

Here, $r_i \equiv (r_{i1}, \ldots, r_{iJ})$ corresponds to Spectator i's reference points for allocating each $y_j \in y$. The subscript i emphasizes that r_i is Spectator i's reference point for allocating to another person

preferences of the recipients.

[19] Notice that the Spectator can give multiple of the good to the same person. Formally, the Spectator provides goods such that, for all $j, k \leq J$, $\gamma_j\left(u\left(x_j, y_j^*\right) - u\left(x_j, y_j^* - 1\right)\right) \geq \gamma_k(u\left(x_k, y_k^* + 1\right) - u\left(x_k, y_k^*\right))$. A similar result obtains with continuous goods.

j, and not a reference point that enters j's utility. If η_y and $\lambda > 1$, the Spectator i experiences loss aversion over recipient j's utility relative to i's reference point on good y. Notice that this formulation remains agnostic about whether u_j is itself reference-dependent. The case with $\eta_y = 0$ nests welfarist allocative utility.

1.4 Targeting with kinked utility

We introduce a notion of flatness when the Spectator allocates discrete goods.

Definition 1. Let $\mathcal{J}_i$ represent the set of j such that optimal $y_j^* > 0$ under allocative utility v_i. Allocation $\mathcal{J}_2$ is *weakly flatter* than $\mathcal{J}_1$ if and only if $\mathcal{J}_1 \subseteq \mathcal{J}_2$.

Put another way, a set of recipients $\mathcal{J}_2$ is weakly flatter than $\mathcal{J}_1$ if all individuals who receive at least one good under $\mathcal{J}_1$ also receive at least one under $\mathcal{J}_2$. There may be an individual in $\mathcal{J}_2$ who is not in $\mathcal{J}_1$.

We compare optimal allocations when allocative utilities are exclusively welfarist (Equation 2) versus have non-welfarist components (Equation 5). Put the optimal set of recipients $\tilde{\mathcal{J}}$ as the one that maximizes welfarist allocative utility for a given set of welfare weights, utility functions, and endowments and places no value on non-welfarist utility (that is, $\eta_y = 0$). Put the optimal set of recipients $\mathcal{J}^*$ as the one that maximizes allocative utility for the same set of welfare weights, utility functions, and endowments but which also places value on non-welfarist utility with $r_{ij} = 1$ for all j, $\lambda > 1$, and $\eta_y > 0$. Then the Spectator with non-welfarist allocative utility exhibits "anti-targeting," in the following sense:

Proposition 1. *The set of recipients with non-welfarist allocative utility $\mathcal{J}^*$ is weakly flatter than the set of recipients without non-welfarist allocative utility $\tilde{\mathcal{J}}$.*

If allocative utility depends on the Spectator's reference points, then the Spectator allocates goods in a flatter manner. Loss aversion gives a kink in welfare around the reference point which pushes toward more universal provision. This link between reference-dependent allocative utility and redistribution structures our empirical tests.

Connection to Rights. We view rights as mapping onto reference points that the social planner might value. Experiments 1–3, which test for preferences for rights, give joint tests of welfarist allocative utility. They test $H_0 : \eta = 0$ and $\lambda = 1$ and $r_j = 0$, for a particular r in the experiment. Experiment 4 tests the model implication in Proposition 1.

The reference points in our model could relate to any feature of provision. Some may relate to rights. Others could relate to concerns about procedural justice. The model nests welfarist allocative preferences but also lets the social planner evaluate an outcome with respect to rights.

For instance, suppose the Spectator views "freedom to choose a lawyer" as a right. In this model, the Spectator's utility over granting freedom to choose has a reference-dependent form.

Our experiments test for reference-dependent allocative utility over providing rights. They also test whether there is more reference-dependent allocative utility when providing goods associated with rights versus other goods.

2 Experiment Overview

2.1 Sample and Design Overview

Sample and Design Overview. We recruit $N = 1{,}800$ participants from Prolific, a widely used online platform for survey experiments (Appendix D.1). As is common in Prolific studies, participants are higher-income, younger, and more educated than in the U.S. in general (Table 1).

Figure 1 presents the experiment flow. We randomize participants ("Spectators") into one of four goods at the start. They then complete four experiments, each with the same good, before answering several questions about political preferences.[20] To be included in the study, participants needed to pass at least two out of three attention checks throughout the survey (Appendix D.3). 7% fail one of three and are included in the sample. We routinely include comprehension checks after providing task instructions but before doing the elicitation. Participants passed all comprehension checks at rates exceeding 85%. We correct participants who fail.

We ran the experiment on September 11–12, 2023. The survey took 19 minutes on average. We paid $6 for participating, which is 56% above Prolific's suggested wage for a 19-minute survey.

Setup and Goods. We inform participants early in the survey that they will face allocation choices on behalf of a nonprofit in Memphis, Tennessee, which assists tenants facing eviction. All participants are informed that the clients are those facing eviction. We make this choice so that all between-good comparisons hold fixed the need and financial situation of the recipients.

The four goods are: attorneys, who can provide legal assistance to tenants; one year of fully subsidized health care at urgent care; a bus pass containing $350 of prepaid fare; and an annual membership at the local YMCA, which can provide child care and wellness services. All goods except health care can be purchased for about $350. Our main tests compare "rights goods" (attorneys and health care) to "benchmark goods" (bus and YMCA).

The choice of goods is important but challenging. There are many potential rights goods and comparison goods. Once a right legally exists, the experimenter cannot easily manipulate endowing the right in the lab. Even if the experiment involves hypothetical choices, if the right has no analog in the market, participants may have difficulty forming views about willingness to pay for the right. For instance, even a hypothetical choice about willingness to pay for a lawyer may be more ecologically valid than a hypothetical choice about willingness to pay for free speech.

[20] Half the benchmark good participants were randomized into doing Experiment 4 with cash instead of their assigned good.

We pick rights goods relevant to active policy debates about in-kind transfers and where appeals to rights are common in the public discourse. We pick comparison goods that are clearly valuable to low-income recipients, but to which Spectators are unlikely to attach special rights.

The comparison between rights and benchmark goods nets out potential elicitation errors. Otherwise, levels of behaviors in the experiments risk conflating inattentiveness that causes random clicking on Prolific with non-welfarist preferences. Examining differences between goods also addresses potential experimenter demand effects and image concerns, unless they interact with the good (Bénabou et al., 2023).

However, comparing rights to benchmark goods is a conservative exercise that likely leads us to understate the extent of non-welfarist preferences. At one extreme, Spectators could value the "right to property," and therefore exhibit non-welfarist preferences for *any* potential benchmark good. In that case, the comparison between rights and benchmark goods might not be distinguishable from zero. Less extreme versions of this preference, in which Spectators value the "right to transit" or the "right to exercise", would attenuate our results but perhaps not to zero.

Connection to Framework. The experiments yield nonparametric tests. We do not require any functional form assumptions to detect preferences for rights or test whether these preferences correlate with redistributive choices. The special functional forms in Section 1 are useful insofar as they explain why preferences for rights could influence redistributive choices.

Taking the functional forms in Section 1 seriously, the levels of non-welfarist behaviors in the experiments test the joint hypothesis $H_0 : \eta_y = 0$ and $\lambda = 1$ and $r = 0$. The differences between rights and benchmark goods give a notion of whether reference-dependent preferences are "stronger" when allocating rights versus benchmark goods.

For instance, suppose $r = r' > 0$ and $\lambda = \lambda' > 1$ for all goods. Such reference-dependent allocative utility even for benchmark goods could represent either a true allocative preference, or capture as-if preferences that Spectators exhibit due to elicitation error or inattention. Then, our experiments test whether $\eta_{\text{rights}} \neq \eta_{\text{benchmarks}}$.[21] Under this interpretation, our tests of $H_0 : \eta_{\text{rights}} - \eta_{\text{benchmarks}} = 0$ are conservative for testing $H_0 : \eta_{\text{rights}} = 0$, as long as $\eta > 0$ for all goods. If $\eta = 0$, this framework continues to nest welfarist allocative utility.

Ethics. Spectators provide informed consent and face no risk of harm. Through the nonprofit partner, we only allocate goods or cash that have the potential to help tenants. Tenants may also decline the offer of assistance. As all tenants are needy, and there is not enough funding to give all tenants assistance, providing assistance based on the actual choices from some Spectators is a reasonable way of targeting. Indeed, the allocation choices in this study are similar to those in real-world political decisions about means testing, as well as community targeting studies (Alatas

[21] There are other interpretations of the experiments. They could also test whether $r_{\text{rights}} \neq r_{\text{benchmarks}}$ or $\lambda_{\text{rights}} \neq \lambda_{\text{benchmarks}}$. Either way, the framework gives a way of organizing and interpreting non-welfarist behaviors.

et al., 2012) and related work.

2.2 Incentives

A feature of this paper is that we incentivize choices for all goods except health care. We use the strategy method, informing Spectators that there is a chance their choices will be implemented. To implement choices, we provide legal assistance, YMCA memberships, and bus passes via a nonprofit partner in Memphis. Participants are informed and must confirm that they face real choices which could affect allocations for needy recipients. We introduce another incentive by telling all participants (including in the health-care treatment) that the study results could influence the nonprofit's future programming. We also incentivize belief elicitations by paying participants if they are accurate. See Appendix D for details on all incentives.

We embedded several tests to study whether lack of incentives affects results for health care. We find no reasons to be concerned (Section 4.4). That said, we still view incentivization as an important part of this paper. It was unclear that incentives would have small effects *ex ante*, and some readers may (reasonably) have been skeptical of results if all elicitations were hypothetical (see discussion in Sullivan, 2023).

2.3 Main and Secondary Elicitations

We conduct four main experiments, testing for features of rights (Experiments 1–3, Section 3) and redistributive preferences (Experiment 4, Section 4). We also present several secondary experiments/elicitations (pre-registered as such). The first directly elicits (bounds on) the Spectator's indifference point between providing the good and giving cash to an anonymous recipient, using a multiple price list and the strategy method. We interpret this elicitation as a Spectator's willingness to pay WTP_i for the good and use this value in extra tests throughout. The second secondary experiment is an information-provision experiment that tests for welfarism (Section 4).

2.4 Specification, Balance, and Attrition

Specification. Our statistical tests follow the forms:

$$y_i = \beta_0 + \beta \text{Right}_i \ (+X_i\delta) + \varepsilon_i \tag{7}$$

$$y_i = \beta_0 + \beta_l \text{Lawyer}_i + \beta_h \text{HealthCare}_i \ (+X_i\delta) + \varepsilon_i \tag{8}$$

where β is the effect of being a rights good on an outcome y_i in Equation (7), and β_h and β_l are the effects for lawyers and health care respectively in Equation (8). We pool both benchmarks for power. Our main specifications omit controls X_i and compare raw means between rights goods and benchmarks, but robustness checks include them. We use robust standard errors for inference.

Balance and Attrition. Demographics are balanced across rights treatments versus benchmarks (Table 1, joint $p = 0.441$).[22] Attrition rates were 4% (Table A2).

3 Features of Rights (Experiments 1–3)

We present the design and results for each experiment in turn.

3.1 Experiment 1: Inalienability

3.1.1 Design

The logic behind Experiment 1 is that welfarists should care only about the ultimate allocation of a good. Suppose a good first is assigned to one person, then is transferred to an *ex ante* identical person, and neither person is aware of the transfer. The welfarist should not care that the good was transferred. Non-welfarists may dislike transferring goods from one person to another because it requires removing the good from someone who has, in some sense, received it.

Experiment 1 hews closely to this idea. We tell Spectators that Recipient B was assigned a good in a lottery. Spectators have the choice of rerunning the lottery, which has the chance of taking the good from Recipient B and giving it to Recipient A. If they rerun the lottery, the Spectator saves $\$x$ for future programming at the nonprofit. We tell the Spectator that the money saved will assist other tenants. We use a multiple price list to find (bounds on) the point at which Spectators are indifferent between saving $\$x$ and preserving the lottery result. We refer to I_i, the midpoint of the elicited bounds, as willingness to pay (WTP) to preserve or not rerun the lottery, in units of the dollars saved for future programming. If $I_i > 0$, the Spectator is willing to burn I_i of surplus to satisfy a non-welfarist preference.

As the units of I_i are unintuitive, we normalize I_i so that the pooled benchmarks have mean 0 and standard deviation 1. We then compare I_i among the rights goods to the benchmarks. We also study the propensity to have a positive WTP to preserve the lottery (i.e., we form $\mathbb{1}(I_i > 0)$ where I_i is unnormalized).

3.1.2 Results

Spectators have 0.3 s.d. higher WTP I_i to preserve the lottery for rights goods than for benchmarks (Figure 2, s.e. of difference: 0.05). Converted back to units of money saved for future programming, participants' WTP is \$20 higher for rights goods than benchmarks. Inspecting the propensity to pay anything, we find that 53% of participants have positive WTP to preserve the lottery for rights

[22] Table A3 further disaggregates balance across possible treatments and finds $p = 0.857$ for the lawyers treatment against benchmarks and $p = 0.129$ for the health care treatment against benchmarks.

goods, 8 pp higher than benchmarks (s.e. of difference: 2.5). The differences are slightly larger for lawyers than rights goods in the continuous WTP measure, but similar for the extensive margin.

Quotations from free-response questions support our interpretation of these results. For instance, one participant wrote: "It's the principle of the matter. Even if the tenants wouldn't know, you'd know." Another wrote, "If I re-ran the lottery it would feel like I was removing the lawyer from the first winner, and it would feel wrong."

Yet a remarkable 46% of Spectators exposed to benchmark goods still have a positive WTP to preserve the lottery. Why? One explanation is that non-welfarist preferences are present in the allocation of any good, and are just stronger for lawyers and health care. As one participant who saw a benchmark good wrote: "I decided to keep the lottery results for each trial because rerunning the lottery and taking away the original winners' YMCA seems very unfair." Such preferences could owe to a normative respect for procedural justice, for instance. If Spectators have non-welfarist preferences for benchmark goods, comparing rights to benchmark goods implies the true extent of non-welfarist preferences may be closer to the levels who have a positive WTP (i.e., 51% on average).

We aggregate these results in Table 2, which presents outcomes for willingness to pay I_i, an indicator for having a positive WTP, and an indicator for having the maximum WTP that we elicit.

Recipients' Loss Aversion Does Not Explain the Results. Our test cannot be explained as Spectators being welfarists who care about recipients' loss aversion or their endowment effect. This story would require the recipient to be aware of the initial allocation.[23] We take significant steps to inform and remind Spectators that recipients will not know about the initial allocation The experiment text says: "Remember that the **tenants will not know that the lottery was rerun**. They will just learn the final result, and the ultimate allocation will be anonymous" (emphasis in experiment text). We also include a confirmation check that asks participants whether the recipients will know who was originally supposed to receive the good. 98% of participants get the question right. For these participants, we reiterate: "**That is correct**. Tenants will only learn the final result of the lottery." We correct the 2% of participants who get the question wrong: "**That is incorrect**. Tenants will only learn the final result of the lottery."

Interpretation and Connection to Framework. While this experiment does identify non-welfarist utility, whether it identifies "preferences for rights" of the form in Section 1 is more debatable. Viewed through our framework, Spectators place meaningful weight on the reference point of initial allocation of rights goods ($\eta_{\text{rights}} - \eta_{\text{benchmarks}} > 0$). We see "inalienability" as mapping to

[23] Otherwise, to the Spectator, either choice has the same effect on total reference-dependent utility among recipients. That is, suppose participants are *ex-ante* identical and neither participant knows the initial allocation. Then a lottery that takes from B and gives to A has the same effect on reference-dependent utility across B and A as doing nothing. This conclusion holds even if the Spectator has uncertainty about exactly how recipients form reference points, as long the Spectator forms identical beliefs about the reference points.

the reference point $r = 1$, which applies to all goods. The value placed on this reference point is stronger for rights than benchmarks.[24]

Still, we cannot reject alternate models. For instance, if Spectators feel extra responsibility or guilt when allocating lawyers and health care, then they may not intervene.

We cannot rule out these interpretations entirely, but note two points. First, alternate explanations must account for a difference across rights versus benchmark goods. It is not clear why Spectators feel more responsible when intervening in rights goods. Second, differential responsibility or guilt would still imply non-welfarist allocative utility of the form in the introduction.

Robustness: Valuation of the Good. Another concern is that we merely identify a behavioral phenomenon in which Spectators do not like to shuffle valuable goods from one recipient to another. According to this view, our results owe to the fact that lawyers are more valuable than bus passes (say). Such a preference would be still non-welfarist, as switching the goods does not affect recipients' utilities. Nevertheless, it may affect interpretation of our results as a true preference for rights per se.

We reject this concern by controlling for fixed effects in the Spectator's valuation for giving the good directly, WTP_i (Table A4). Intuitively, this test compares Spectators who find rights and benchmark goods equally valuable with respect to cash. We continue to find that Spectators facing the rights good are more likely to pay to preserve the lottery.

3.2 Experiment 2: Dignity of Choice

3.2.1 Design

Design and Main Measure. The logic behind Experiment 2 is that welfarists only value providing recipients with the ability to choose insofar as the choice might be exercised. If welfarists are completely sure a recipient always chooses (a) over (b), then their willingness to pay to provide a choice between (a) and (b), versus giving (a) directly, is zero.[25]

Experiment 2 begins by eliciting Spectators' beliefs about the probability that a recipient facing the choice of $\$y$ versus the good would choose the good, which we denote as p. Then, Spectators face the choice of: (i) providing $\$y$ to the tenant directly and saving $\$x$ for future programming for the nonprofit, versus (ii) giving the recipient the choice between $\$y$ and the good. We elicit (bounds on) the value of $\$x$ that makes Spectators indifferent between (i) and (ii).

We compare C_i, the midpoint of these bounds, among rights and benchmark goods. We focus

[24] Another interpretation is that $r = 0$ for benchmarks, which only amplifies the preferences for rights that obtain with lawyers or health care.

[25] Welfarists may value providing unexercised choices if they project intrinsic values of decisions (Bartling et al., 2014b; Lenk, n.d.) onto recipients. Analogous to procedural justice concerns in Experiment 1, this may explain high levels of WTP among benchmark goods. However, intrinsic values of choices cannot account for differential WTP between benchmark and rights goods, as we find.

on C_i as p approaches 1. When $C_i > 0$ and the Spectator has high beliefs, the Spectator burns surplus to let the recipient choose. As in Section 3.1, we normalize C_i so that it is mean 0, standard deviation 1 among the pooled benchmark goods.

Our test requires conditioning on people with high beliefs p, which could lack power. To increase Spectators' beliefs, we provide all Spectators with truthful information from a randomly selected pilot sample. The information says that all tenants in the pilot sample chose cash over the good (see Appendix D.5 for details on the treatment). This information raises power by increasing the number of Spectators with high beliefs.[26]

Second Measure. We also form a second measure of a non-welfarist willingness to pay for choice. Welfarists value providing the choice of good g and cash $\$y$ versus cash as:

$$C_i^w = p(-i \text{ chooses g}) \times E[f(u_{-i}(g)) - f(u_{-i}(y)) \mid -i \text{ chooses } g]. \tag{9}$$

This expression says that welfarists value choice at their WTP to provide the good, times the probability of exercising the choice. We obtain the value $E[f(u_{-i}(g)) - f(u_{-i}(y)) \mid -i \text{ chooses } g]$ by eliciting Spectators' willingness to provide g versus cash to a recipient. We assume small selection on gains (supported by a direct test below), such that

$$E[f(u_{-i}(g)) - f(u_{-i}(y))] \approx E[f(u_{-i}(g)) - f(u_{-i}(y)) \mid -i \text{ chooses } g]. \tag{10}$$

We study the effect of rights goods on Δ_i, the difference between actual WTP for choice C_i and welfarist implied WTP for choice C_i^w:

$$\Delta_i(t) := \mathbb{1}(C_i - C_i^w - t > 0). \tag{11}$$

Setting tolerance $t = 0$ lets us examine whether the elicited WTP for choice is exactly equal to the welfarist WTP for choice. We focus on $t \gg 0$, to conservatively account for trembles (e.g., imperfect ability to scale WTP by beliefs) and selection on gains.[27]

The advantage of the second measure relative to the first measure is that it does not require us to condition on having high beliefs. The disadvantage is that we lack a principled way to choose t.

[26] Conditioning on beliefs could, in theory, affect experimental balance. Randomization does not guarantee that participants who have high beliefs p for one good have the same potential outcomes as those who have high beliefs for a different good. First, randomization is not required for this test. Any positive WTP as $p \to 1$ (or difference across goods) still indicates non-welfarist preferences. Second, Table A5 shows that balance persists conditioning on beliefs. Third, our second measure of the value of choice is not subject to this concern.

[27] Setting $t > 0$ also accounts for minor elicitation differences between C_i and C_i^w. In particular, C_i is WTP in units of dollars of future programming for the nonprofit. C_i^w is WTP in units of dollars of money provided directly to that tenant. Crucially, no matter how large we set t, we find differences in $\Delta_i(t)$ across goods. Relatedly, both C_i and C_i^w are subject to top-coding in the multiple-price list. To be conservative and push against finding a large $\Delta_i(t)$, we top code the maximum direct WTP at \$1,500, whereas we top code C_i at \$950. Note that top coding across does not introduce bias unless differential by good.

If t is too small and perceived selection on gains is large, the test is invalid. We use $t = \$250$ and show robustness to this decision.

3.2.2 Results

Spectators exhibit differential preferences for the dignity of choice among rights goods relative to benchmark goods (Figure 3A). Focusing on rights goods (blue series), we reassuringly find that WTP C_i is decreasing in beliefs that recipients will choose the good over cash. This negative relationship reflects that choice is less valuable if it is unlikely to be exercised.

However, for every bin of beliefs, willingness to pay for rights goods is higher than for benchmark goods. Among Spectators with beliefs larger than 0.9, WTP is 0.5 sd (s.e.: 0.05) higher for pooled rights goods than benchmarks. Converted back to units of money for future programming, Spectators are willing to pay \$395 on average for pooled rights goods (\$223 for benchmarks).[28] Spectators have differentially higher WTP for lawyers than health care, but both differ from the benchmarks (see formal tests in Table A6). These differences persist even if we condition on having beliefs larger than 0.95 (farthest right whiskers).

Rights good versus benchmark differences also persist on the extensive margin, when we examine having a positive WTP for choice at all (Figure 3B). For instance, among Spectators who think there is at least a 95% chance that the recipient will choose cash, 52% have positive WTP for choice with a rights good (versus 37% who provide a benchmark good; s.e. of difference: 3.5 pp).

As in Experiment 1, Spectators exposed to the benchmarks still exhibit high levels of non-welfarist preferences (Figure 3, orange series). These levels may reflect welfarist or non-welfarist valuations of giving choice even in non-rights cases (Bartling et al., 2014a). The difference between benchmarks and rights goods rule out elicitation errors, so we stress differences to be conservative.

Quotations again support our interpretation of the results. A Spectator seeing lawyers who had the maximum WTP wrote, "I think the tenant has a right to choose what assistance to accept." Another wrote, "The tenant has a right to choose, no matter what the monetary consequences."

As one way of summarizing these accounts, we ask Spectators a qualitative question about why they made their decision in the experiment. The share of Spectators who say that recipients have the right to choose when facing a rights good is about 40%, which is 13.2 pp (48%) more likely than with benchmarks (Table A7, Column 5).[29] Reassuringly, the share of Spectators whose self-described motivations include the right to choose is only somewhat smaller than the share who are willing to pay for choice (40% versus 52%).

[28] The scales of the elicitation in Experiments 1 and 2 differ, since providing choice could have large instrumental benefits if, say, lawyers help tenants win an eviction case.

[29] After eliciting WTP, we ask participants "Which of the following reasons motivated your choice(s)? Select all that apply." Options included: "I thought anyone who would choose the [good] would really want it"; "I did not think anyone would choose the [good] in reality"; "Saving is my priority"; "All tenants should be entitled to the choice of a [good]" (the right to choice option); and "None of the above."

Formal tests reinforce these results (Table 3). Pushing our experiment to its logical conclusion, the effect on the overall WTP is large and highly significant even if we examine only the 454 Spectators who say there is a 100% chance the recipient will choose cash (Column 9).[30] The effect on an indicator for the extensive margin attenuates if we consider only those with posteriors of 100% (to 7.2 pp, s.e.: 4.7). The gap between the overall WTP and extensive margin results is driven by a large effect of rights goods on willingness to pay the maximum to ensure choice (Columns 5 and 8). That is, more Spectators appear to value providing choice very highly for rights goods than for benchmarks.

The second measure of dignity of choice corroborates the primary measure (Figure 3C). We find that rights goods have larger $\Delta_i(t)$ for all tolerances between \$0 and \$500. For instance, focusing on a tolerance of \$250, we find that Spectators exposed to rights goods are 6.4 pp more likely to have a WTP for choice that exceeds their instrumentalist WTP by \$250 or more (s.e.: 1.7). The value $\Delta_i(t)$ is guaranteed to decrease in t. But the difference in $\Delta_i(t)$ for rights versus benchmark goods remains large as a share of benchmark goods' $\Delta_i(t)$.

Testing Selection on Gains. A concern is that we do not account for selection on gains. Suppose Spectators believe that recipients who choose g over \$$y$ benefit substantially from it, but also believe that most recipients choose \$$y$ over g and would not benefit from g. Then Spectators may: (i) have a low average WTP for lawyers; (ii) have a high WTP for choice.

Our primary tests above address this concern. Selection on gains is relevant only if p is mismeasured, since selection on gains still vanishes from welfarist's utility as $p \to 1$. However, it is reasonable to worry that Spectators' beliefs are mismeasured due to noise, lack of numeracy, or elicitation issues.

We embed another test to directly examine selection on gains. In particular, we randomize the value of the bundle $y \in \{\$200,\ \$300\}$. Intuitively, randomizing the bundle traces a supply curve to provide choice. If this supply curve is upward sloping, holding beliefs about the share who choose the good over \$$y$ constant, that suggests selection on gains. In fact, we find no evidence that this supply curve is upward sloping in \$$y$. Appendix D.6 explains formally how this subexperiment tests for selection on gains.

Connection to Framework. To view this experiment through the lens of our framework, consider the choice itself as a good y. Suppose also that $r = 1$ for providing choice, that $\lambda = \lambda'$ for all goods, and that u_j does not itself depend on choice. Then our experiments test $H_0 : \eta_{\text{rights}} = \eta_{\text{benchmarks}}$, which is conservative for testing $H_0 : \eta_{\text{rights}} = 0$ as long as $\eta_{\text{benchmarks}} > 0$. Here, η's relate to

[30] Our incentive scheme rewards people equally if they had posteriors of 96–100%, and we find large effects on the extensive margin if we focus on posteriors of 95% or above (Figure 3B). We can detect an effect on the extensive margin limiting to Spectators with 100% posteriors if we use the machine learning method of Chernozhukov et al. (2018) to select controls (Table A7). Moreover, the extensive margin is still distinguishable for lawyers versus benchmarks among Spectators with 100% posteriors (Table A6, Column 7). Spectators with 100% posteriors are 11 pp more likely to select that people have a right to choice when doing the experiment with a rights good (Table A7, Column 4).

non-welfarist concerns over providing choice.

One complication is if u_j depends on choice. Conditioning on $p \to 1$ intends to restrict to the subset of people for whom choice is not instrumentally valuable. But if recipients value choice even when choice is not exercised (Bartling et al., 2014a), then welfarist Spectators may also value choice. Differencing with respect to benchmarks still provides a valid test of $H_0 : \eta_{\text{rights}} = \eta_{\text{benchmarks}}$ if: (i) u_j is additively separable in the intrinsic value of choice and other parts of utility, and (ii) the intrinsic value of choice is equal for goods of equal value.

3.3 Experiment 3: Egalitarianism

3.3.1 Design

The logic behind Experiment 3 is that welfarists' utility should not depend on the share of people in society who already get a good. To be concrete, suppose z out of 10 people get a lawyer regardless. Welfarists' WTP to provide the $(z+1)$th recipient a lawyer should not vary with z differentially for rights goods.

We operationalize this idea by informing participants that z out of 10 *ex ante* identical and anonymous recipients were selected to receive lawyers. We then elicit participants' willingness to pay to provide the $(z+1)$th person with a lawyer. In this case, the outside option is a donation to a food bank.[31] We randomize $z \in \{1, 5, 9\}$.

We estimate the following difference-in-differences specification:

$$y_i = \delta_0 \text{Right}_i + \delta_1 \mathbb{1}(z_i = 9) + \beta_0 (\text{Right}_i \times \mathbb{1}(z_i = 9)) + \varepsilon_i \tag{12}$$

$$y_i = \delta_0 \text{Lawyer}_i + \delta_1 \mathbb{1}(z_i = 9) + \delta_2 \text{HC}_i + \beta_l (\text{Law}_i \times \mathbb{1}(z_i = 9)) + \beta_h (\text{HC}_i \times \mathbb{1}(z_i = 9)) + \varepsilon_i. \tag{13}$$

The coefficients of interest are β_0, β_h, and β_l.

The difference-in-differences specification addresses an important concern that inequity aversion (Fehr and Schmidt, 1999, still a welfarist concern) generates a higher willingness to pay for *any* good if $z_i = 9$. One way of thinking about this experiment is that it essentially examines whether inequity aversion differs by good. As another test of inequity aversion, we augment Equations (12) and (13) with a control $g(\text{WTP})_i$, which is a flexible function of the directly elicited WTP to provide the good. As inequity aversion still depends on realized utilities, controlling for the value $g(\text{WTP})_i$ isolates the non-instrumental role of differential z_i.

[31] Had the outside option been "saving for future programs" as in Experiments 1–2, then choices in Experiment 3 could never be egalitarian. Tenants later would not be assisted. Put another way, "future programs" would have raised the denominator from 10 to an unknown number.

3.3.2 Results

We find no evidence of differentially egalitarian preferences for lawyers or health care compared to benchmark goods (Figure A3). We find that across all goods, rights and benchmark, preferences become more egalitarian as z rises. This null result can be interpreted several ways. First, non-welfarist egalitarian preferences may extend to all four goods, including benchmarks. Alternatively, as there is no differential egalitarianism across rights versus benchmarks, these tests cannot reject the presence of welfarist but inequity-averse preferences. Elicitation errors or inattention are a less persuasive explanation for the spike at $z = 9$ as they are unlikely to differ at $z = 9$ versus $z = 5$ and $z = 1$.[32]

Egalitarian preferences were *ex ante* reasonable to examine. Equal rights are a fundamental tenet of liberalism. However, they are also challenging to manipulate in the lab. We cannot control the share of people in society who have access to the good. One explanation for the null result could be that Spectators internalize that, no matter their choice, many people will still lack lawyers or health care.

Multiple Hypothesis Corrections. As we find results consistent with our hypotheses in two of three experiments, we perform multiple hypothesis corrections for inalienability, dignity of choice, and egalitarianism (Table A9). Romano-Wolf adjusted p-values for the continuous WTP measures in Experiments 1–2 remain significant at $p < 0.001$.

4 Implications for Targeting and Political Preferences

Having found evidence of preferences for rights, we now turn to their implications. First, we show that they correlate with redistribution decisions, using a novel redistribution experiment that is suitable for this setting. Second, we use this task to quantify the share of people with welfarist versus non-welfarist preferences. Finally, we consider preferences for rights and support for in-kind provision. As we find evidence only for the features of inalienability and dignity of choice, we focus on how these correlate with the outcomes of interest.

4.1 Anti-Targeting (Experiment 4)

4.1.1 Design

We want to measure redistributive preferences over indivisible goods where recipients only benefit from provision at the extensive margin. It is not obvious how to do that. To see why, suppose there are 10 people who need lawyers; they can be uniquely sorted by income (i.e., no ties); there

[32] Formal tests of the difference-in-differences — including or excluding a control for the direct WTP for the good — also fail to detect evidence of differential egalitarianism (Table A8). If anything, we find that the coefficient is negative (and significant at $p < 0.05$ with the WTP control), which implies more differential egalitarianism for the *benchmarks*.

are $\ell < 10$ lawyers; and no one benefits from multiple eviction lawyers. Anyone with progressive redistributive preferences gives the lawyers to the ℓ poorest people. Thus, we cannot simply ask Spectators how they would allocate ℓ lawyers among 10 people.

Experiment 4 introduces smoothness into the problem as follows. We truthfully tell Spectators that 10 tenants with annual household incomes ranging from \$0 to \$36,000, in increments of \$4,000, have applied for assistance. Spectators may give all tenants the good g, again randomized across the four goods. Alternatively, Spectators may give the poorest $\ell \in \{1, 2, \ldots, 9\}$ people the good as well as cash. The value of the cash is decreasing in ℓ. Thus, Spectators face a trade-off between (i) giving more money and the good to fewer, needier households, versus (ii) less money and the good to more households, where the marginal household is less needy.

To ensure that every choice considers the same budget, we fix the total redistributive budget for this choice at B. Good g's price is $p_g = B/10$. Any money not spent on "purchasing" the good is divided equally among tenants who receive the good. The Spectator faces a choice of giving $\frac{B - p_g \ell}{\ell}$ dollars and the good g to ℓ tenants, or all 10 tenants the good. We use multiple price lists to identify the number of tenants at which a Spectator is indifferent between giving the good to everyone and money plus the good to fewer recipients.

As an example for one good, we price lawyers at \$500 and consider a total redistributive budget $B = \$5{,}000$. First, Spectators choose between giving (i) lawyers to everyone, versus (ii) five tenants a lawyer and \$500 in cash each. If they choose (i), they face the choice of giving lawyers to everyone versus six tenants a lawyer and \$333 in cash each. We iterate on these questions until we find R_i, the Spectator's preferred value, for $R_i \in \{1, \ldots, 10\}$.

Design Considerations. It is important to choose the price of each good (equivalently, the budget) correctly. To see why, suppose p_{lawyer} were \$1 and p_{bus} were \$100. Then, since not much cash can be redistributed by giving lawyers to fewer people, and lawyers may be very effective, most Spectators would likely choose to give 10 tenants the lawyer. We price health care at \$600, lawyers at \$500, YMCA at \$300, and bus passes at \$250. We selected these prices to be the median of pilot WTP elicitations. Notice that making rights goods more expensive pushes toward allocating them less universally. This choice is conservative for our ultimate conclusions. As one check, we find they are similar but not identical to the direct WTP we elicit for each good.

In addition to conducting this exercise with rights and benchmark goods, we also randomize some of the Spectators assigned to benchmark goods into doing this exercise with cash, at a total budget of $B = \$5,000$. Spectators in this elicitation choose the value ℓ at which they are indifferent between giving B/ℓ in cash to ℓ people or $B/10$ to 10 people.

4.1.2 Results

Spectators are more likely to "anti-target" — that is, give goods universally (to all 10 tenants) with rights goods than benchmarks (Figure 4). Pooling lawyers and health care, 43% of Spectators who allocate rights goods anti-target, compared to 26% of Spectators allocating benchmarks or cash (s.e. of difference: 2.3 pp). Both lawyers and health care are significantly different than the benchmarks and cash. Spectators are more likely to anti-target with lawyers than health care. Lawyers are 26.3 pp more likely to be anti-targeted than benchmarks or cash (s.e. of difference: 2.7 pp), whereas health care is 7.1 pp more likely to be anti-targeted (s.e. of difference: 2.6 pp). Table 4 aggregates these tests and shows similar results with R_i, a continuous measure of the number of tenants allocated assistance.

One should not interpret these results as suggesting that Spectators would give goods universally no matter the recipient population. All 10 recipients are quite needy. However, when facing the same group of needy tenants, Spectators' targeting preferences are flatter when distributing rights goods versus benchmarks and cash.

4.1.3 Non-Welfarist Preferences and Anti-Targeting

Our framework predicts that preferences for rights lead to flatter allocation of goods (Proposition 1). Thus we expect that Spectators who demonstrate preferences for rights in Experiments 1 and 2 provide rights goods to more recipients in Experiment 4.[33]

We find support for this prediction (Figure 5). WTP for preserving the lottery (I_i) and choice (C_i) are both significantly higher among Spectators providing goods universally. Table 5 presents bivariate and multivariate regressions of anti-targeting on I_i and C_i. Multivariate regressions of anti-targeting on both WTPs reveal that C_i is more predictive than I_i when considered jointly. The predictiveness of C_i on Spectators' anti-targeting propensity persists when considering each right separately. Further supporting this result, the propensity to exhibit non-welfarist preferences for benchmarks in Experiment 1 and 2 also predicts anti-targeting of benchmarks (Figure A5).

Addressing Objections. As noted above, a key concern about this exercise is whether we set the "price" correctly for rights goods versus benchmark. We purposefully set a price for each good that is conservative with respect to generating anti-targeting of rights goods — that is, Spectators could give more cash to the poorest if they chose not to anti-target lawyers and health care. Despite these efforts, the concern remains reasonable, as WTP in the experiment for lawyers exceeds that from pilots. In particular, average WTPs for YMCA, bus, health care, and lawyers are: \$328, \$373, \$507, and \$765 respectively.

[33]The framework does not map literally onto this experiment because Proposition 1 is based on providing any number of a one-dimensional good. Experiment 4 provides a good with two dimensions, cash and the single right or benchmark. Allocations change both dimensions. The fixed budget in Experiment 4 constrains allocations in a similar way to the fixed number of goods in Proposition 1.

To further test this point, we control for Spectators' elicited WTP (Table A10). Rights still predict anti-targeting propensity, although the effect attenuates modestly in the pooled sample. Lawyers still predict anti-targeting even when we control for WTP, even though lawyers are the sole good where elicited WTP exceeds the implied price (which would push toward more universal provision). Moreover, the correlation between I_i and C_i and universal provision persists even with controls for direct WTP (Table A11). Indeed, controlling for direct WTP is a highly conservative exercise here, as preferences for rights may generate high direct WTPs, so controlling for direct WTP risks being a "bad control" (Angrist and Pischke, 2009).

A related objection involves paternalism. If Spectators believe low-income participants will misuse cash, they may prefer universal provision of any good. Rights goods versus benchmark differences in universal provision address this concern.

4.2 Quantifying Welfarist and Non-Welfarist Preferences

We embedded a sub-experiment into Experiment 4 to identify potential welfarists. By doing so, we can compare the share of non-welfarists to welfarists.

Design. The idea behind the experiment is that welfarists change redistributive choices based on surprising information about the instrumental effects of providing a good. For instance, if welfarists learn that lawyers are ineffective, they should be less inclined to provide them rather than cash. In this experiment, we give truthful information about the efficacy of lawyers and health care. Then we ask Spectators if they want to change redistributive choices based on the information. We label Spectators as "welfarist" if they do revise redistributive choices when this information conflicts with beliefs.

We implement this design as follows (Appendix D.7 gives full details). Spectators assigned to lawyers or health care are randomized into seeing information that the good is effective or ineffective. There is no equivalent experiment for the benchmark goods. For health care, we show either a positive or null treatment effect about health care from the Oregon Health Insurance Experiment (Allen et al., 2013; Baicker et al., 2013). For lawyers, we show either a large or small treatment effect from an ongoing RCT of providing lawyers to tenants facing eviction in Memphis, TN (Caspi and Rafkin, 2023). Before giving information, we elicit prior beliefs about the efficacy of lawyers and health care.

After providing information, we let Spectators choose whether to revise their targeting choice in Experiment 4. (The anti-targeting results in Section 4.1 all report Spectators' *initial* choices.) In particular, we ask Spectators assigned to lawyers: "Previously, you made choices distributing a limited budget across hiring lawyers and giving tenants cash. Given this information, would you like to revise any of your choices?" Spectators who say they want to revise their choice then do the same targeting elicitation from Experiment 4. The setup is similar for health care.

Our goal is to label Spectators as welfarist or non-welfarist. We focus on Spectators who had beliefs about lawyers/health care efficacy that disagree with information we provided them in the treatment. Among these Spectators, we label them as *welfarist* if they revise their targeting decision.[34] Among the same group of Spectators, we label them as *non-welfarist* if they do not revise their targeting decision and do exhibit either positive WTP not to rerun the lottery or positive WTP for choice. Because Spectators may have positive WTP for choice for instrumental/welfarist reasons (e.g., selection on gains), robustness tests restrict to Spectators who also have high beliefs that the tenant will choose cash in Experiment 2 ($p \to 1$, in the notation of Section 3.2).

The way we label Spectators is conservative. Anyone who revises targeting in Experiment 4 is labeled as welfarist, even if they also make non-welfarist choices in Experiments 1 or 2. We only label Spectators as non-welfarist if they *both* forgo the chance to make a welfarist revision to targeting in Experiment 4 *and* make a non-welfarist decision in one of Experiments 1 or 2.

Results. Upon receiving information, updates are fairly rare. About 40% choose to update their allocation if they receive information about lawyers, and less than 30% update if they receive information about health care (Figure A4). Conditional on updating, Spectators tend to update in the direction of the information shown (e.g., they provide more lawyers if they get positive information about lawyers).

Despite conservative classification choices, we observe comparable magnitudes of welfarism and non-welfarism (Figure 6A).[35] Restricting only to Spectators who do the experiments with rights goods, 81% exhibit preferences for rights in at least one of Experiments 1–2, and 39% exhibit preferences for rights in both experiments. Meanwhile, 31% are welfarist, meaning that they revise targeting choices after receiving information. If we label people as having preferences for rights only if they make non-welfarist choices in both Experiments 1–2 and do not make a welfarist choice, we find that 26% have non-welfarist preferences.

We therefore decisively reject that non-welfarist preferences are not quantitatively meaningful. In fact, we find that they are just 18% less prevalent than welfarist preferences, even though assuming welfarist preferences is by far the norm in welfare economics.[36]

Inattention. A natural concern is that we mislabel Spectators as being non-welfarist or unclassified if they actually just fail to update due to inattention. Measurement error from inattention is undoubtedly present, but unlikely to change our conclusions that non-welfarist preferences are

[34]The beliefs we elicited for lawyers exactly correspond to the information provided in the treatment. Because it was difficult to elicit beliefs that were identical to the information we provided for health care, we label people as having priors that exceed the information if their prior about how health care vouchers increase the percent of tenants with improved health outcomes 1 year later exceed the analogous percent increase for lawyers (80% for high information, 20% for low information) (Appendix D).

[35]The figure restricts to a constant sample of Spectators whom we could have observed as welfarist. That is, if the information does not conflict with priors, then we cannot identify Spectators either way and they are not included.

[36]The difference of 4.8 pp (s.e.: 2.5, $p = 0.058$) indicates that welfarist preferences are more common than this stringent classification of non-welfarist preferences, but not overwhelmingly so.

prevalent. First, we designed the belief updating task to require an active choice to update or not. Participants must select either: "**Yes**, I would like to revise my choices and give more people lawyers"; "**Yes**, I would like to revise my choices and give more people cash"; or "**No**, I would not like to revise my choices." It is not obvious that inattentive participants would choose not to update versus choose to update. In fact, inattention might work in the other direction, by leading true non-welfarists not to exhibit preferences for rights in both Experiments 1 and 2.

Second, participants are attentive overall (Section 2). This concern thus requires attention to lapse at precisely this elicitation and essentially nowhere else.

Finally, as our results are large in magnitude, an implausible amount of measurement error is required to undo them entirely. Suppose a full 50% of the Spectators whom we label as non-welfarists are actually inattentive welfarists (who pass two other attention checks). Even then, the share of non-welfarists to welfarists would still be quantitatively meaningful (about 30%).

Correlations With Anti-Targeting. Having categorized Spectators as welfarist and non-welfarist, we now return to whether these preferences predict anti-targeting. While Figure 5 suggests behaviors in Experiments 1–2 predict anti-targeting, it is not guaranteed that non-welfarist preferences, which also require not updating based on relevant information, are still predictive. Yet this concern is unfounded (Figure 6B, see Table A12 for standard errors and hypothesis tests). 32% of anti-targeters are non-welfarist versus 23% of non-anti-targeters (p-value of difference $= 0.006$). Moreover, we also find a sharp drop in welfarist preferences among anti-targeters (19% versus 38%, p-value of difference < 0.001).[37] These correlations cast doubt on random elicitation errors as explaining the large share of non-welfarists, since noise would not predict other choices.

4.3 Support for In-Kind Redistribution and Heterogeneity

Support for In-Kind Redistribution. We conclude the study by asking Spectators if they support (i) "right to counsel" policies that provide lawyers to tenants facing eviction, (ii) rent control, and (iii) universal health care. We then regress support for these policies on exhibiting preferences for rights in both Experiments 1–2. For right to counsel and rent control, we conduct this exercise among Spectators who did the experiments with lawyers. For universal health care, we conduct the exercise among Spectators who did the experiments with health care.

Rights preferences predict support for right to counsel and rent control, but not universal health care (Figure 7). For right to counsel and rent control, the relationship survives adding a

[37]The result that non-welfarism is more prevalent among those who anti-target is mostly robust to alternative definitions and dropping those with low posteriors in Experiment 2 (Table A12), and vice-versa with welfarism. Results are driven by lawyers, and are not significant for non-welfarist preferences if we focus on health care alone (Panels C–D). Some correlations between non-welfarism and targeting attenuate with different definitions of non-welfarism (Columns 1–2). This attenuation relative to Figure 5 is caused by: (1) focusing on the extensive margin (those with I_i or C_i larger than 0), as continuous measures are robustly correlated with anti-targeting (Table A10), and (2) the fact that the sample of people whom we can unambiguously label as welfarist or non-welfarist is smaller.

control for whether the Spectator believes the policy would be effective, as well as for whether the person is a liberal.[38] Thus, preferences for rights, at least in the context of providing lawyers, predict policy support on top of welfarist/instrumental views about whether policy will help people. One explanation for why preferences for rights could be less predictive of support for universal health care is that this issue is more politicized.

As a final test, we ask Spectators if they agree there is a right to several types of in-kind goods. Strongly agreeing there is a right to in-kind goods like food, education, and housing is robustly correlated with having rights preferences (Figure A7). The one exception is agreement with the view that there is a right to a lawyer in criminal cases.[39]

Demographic Heterogeneity. If the people who have preferences for rights were mostly rich, then non-utilitarian SWFs that aggregate preferences might still place a small value on preferences for rights. To the contrary, Spectators with preferences for rights are, if anything, less likely to be rich or well-educated (Figure A6). We regress an indicator for expressing non-welfarist preferences in both Experiments 1–2 on demographics. Households with incomes larger than $60,000 per year have significantly smaller preferences for rights, driven by their choices over allocating lawyers.

Other correlations are small. Notably, we find no correlation between having preferences for rights and being a self-reported liberal. Thus, heterogeneity does not support the hypothesis that preferences for rights reflec polarization or preferences among Democrats vs. Republicans.

4.4 Robustness Checks

Incentives. As health care was not incentivized, we embedded two complementary tests to see how much incentives might matter (see details in Appendix D.2). First, in our WTP elicitation where we ask Spectators to choose between giving a recipient cash or the good, we randomly assign half of the participants assigned to the three incentivized goods to have explicitly hypothetical framing. We reject even small effects of incentives for this elicitation (Table A14).

Second, we randomize benchmark goods into being incentivized throughout all experiments (as in lawyers) or not incentivized (as in health care). In particular, we randomize benchmark goods into receiving identical language as those who see health care. Minor parts of the introduction to the study were different for health care to ensure truthfulness regarding incentivization. Comparing benchmark participants who see the health care language to those who see the lawyer language therefore jointly tests for the importance of incentives and the other language differences. We find no effect on behaviors among benchmark participants (Table A13). As a consequence,

[38] We elicit beliefs about policy efficacy by asking whether people in the U.S. would be on average worse or better off with the policy.

[39] One explanation is that Spectators may view that question as a factual matter. It is the only good we ask about which is actually guaranteed in the U.S.

Table A13 also shows that rights goods versus benchmark goods differences also persist using only incentivized or only unincentivized benchmark participants.

The advantages of the first test are that: (i) it provides evidence that behaviors about allocating a treatment good (lawyers) are not affected by incentives, and (ii) the only difference in wording pertains to hypothetical versus not. The advantage of the second test is that it provides direct evidence about incentives for our main experiments, but only among the control group, and it bundles additional wording differences. We did not randomize incentives among lawyers in the main experiments because we did not know *ex ante* that incentives would have small effects, and we did not want to reduce power if they proved important. Together, these tests and results contribute to a conflicted literature on the importance of incentives in laboratory experiments (Andersen et al., 2011; Charness et al., 2021; Danz et al., 2022; Sullivan, 2023).

Other Tests. Results do not change if we use double/debiased machine learning (Chernozhukov et al., 2018) to select among the demographic controls in Table 1 (Table A15).

5 Conclusion

Using several experiments, we document that preferences for rights are almost as common as the welfarist preferences that are the default in welfare economics. These preferences for rights are correlated with preferences to provide in-kind assistance universally. Our results may salvage the rights-based justification for in-kind assistance. Social Welfare Functions that depend only on the preferences of individuals in society still value rights in the domains of eviction defense lawyers and health care. Our experimental techniques are portable to other settings with potential non-welfarist preferences, for instance right to shelter or sustenance.

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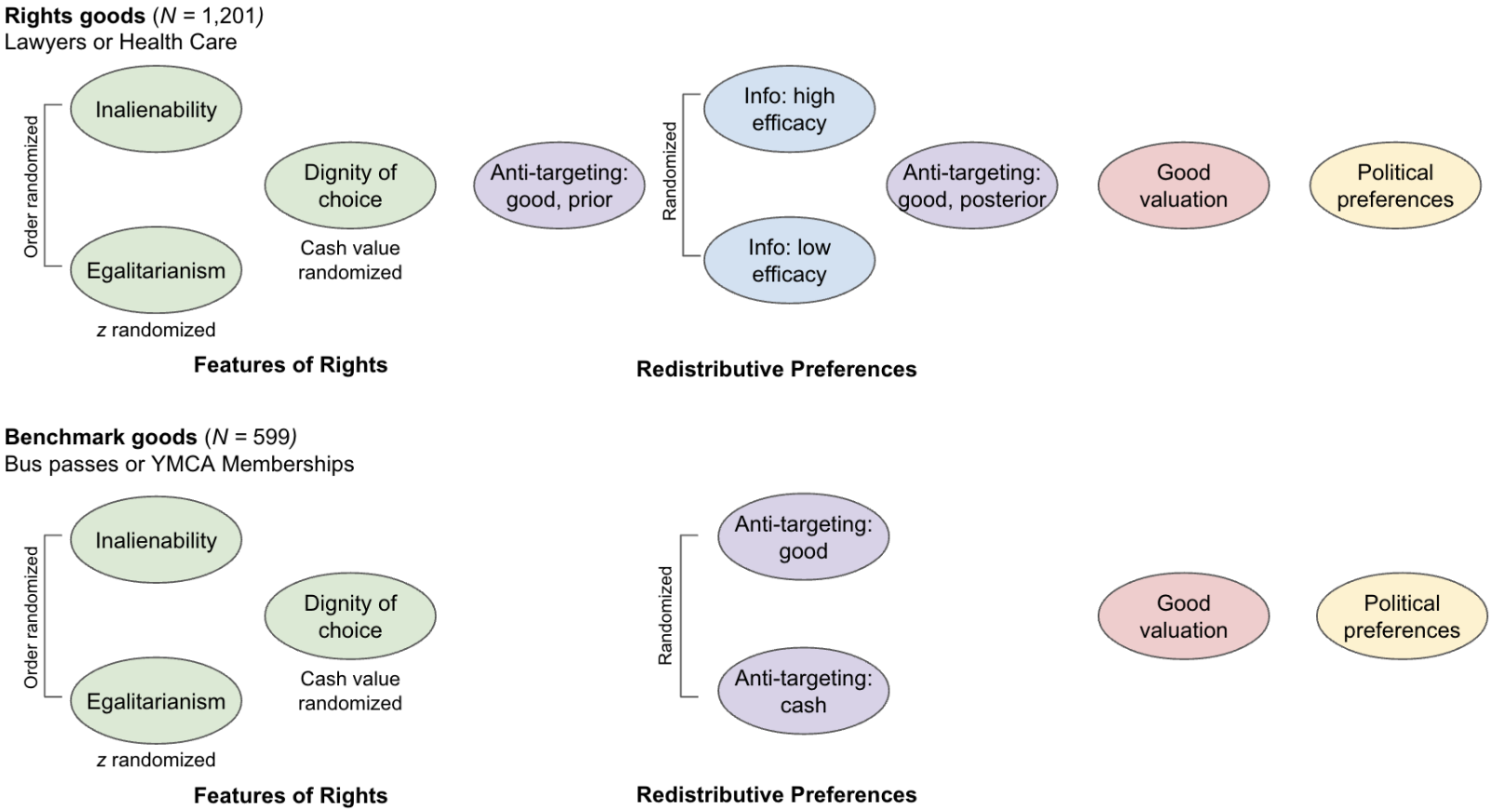
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6 Figures

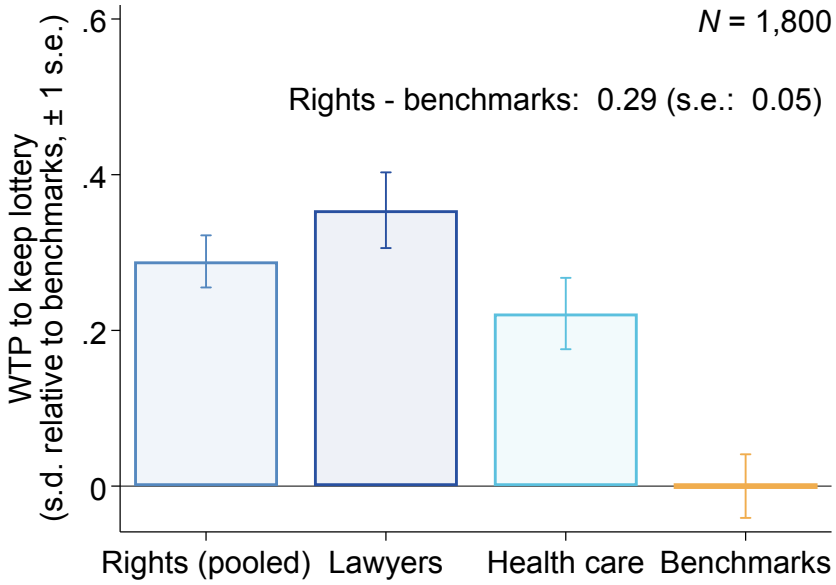
Figure 1: Survey Flow



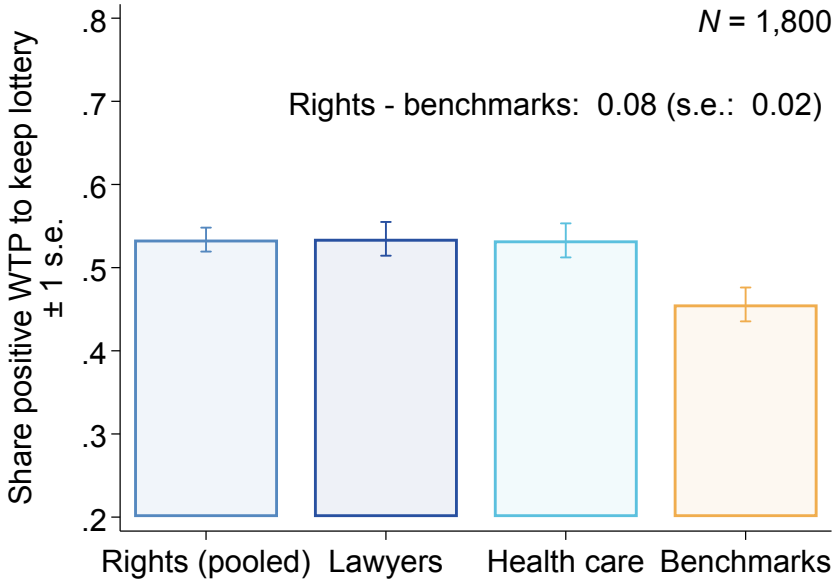
Note: This figure presents the flow of the survey, differentiating between those who saw rights goods (2/3) and those who saw benchmark goods (1/3). Within each arm, participants were randomly shown one of the two potential goods. After introductory information, participants first completed experiments related to features of rights. The order of *Inalienability* and *Egalitarianism* was randomized, followed by *Dignity of Choice*. Next, participants answered questions about *Anti-Targeting*; those assigned to rights goods also saw an information treatment. Finally, we elicited participants WTP for the good they were assigned, demographic information, and political preferences. Participants were required to pass two out of three attention checks.

Figure 2: Experiment 1: Inalienability

(a) WTP to keep lottery



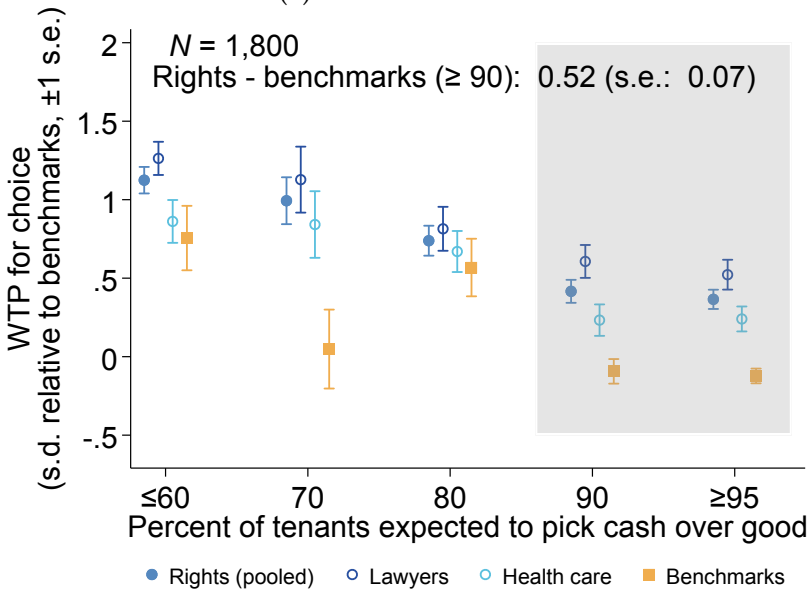
(b) Share with positive WTP to keep lottery



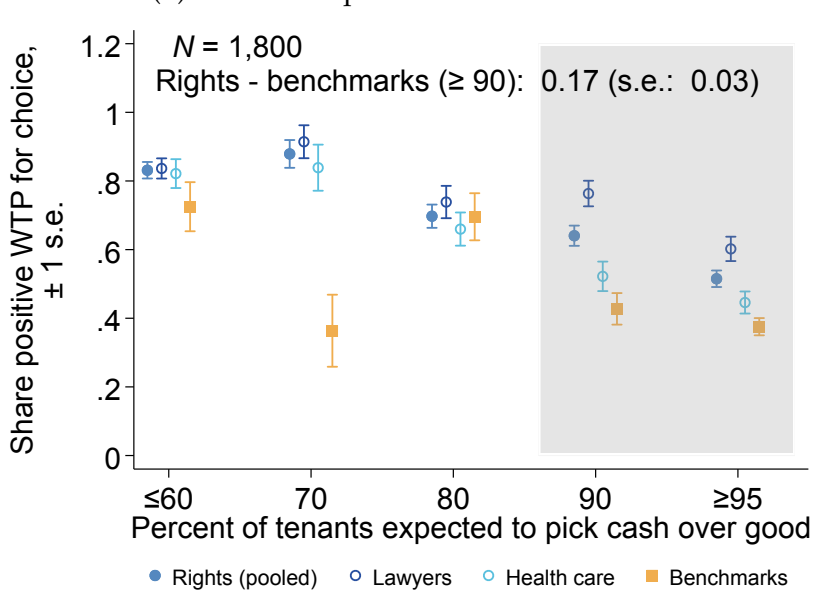
Note: This figure shows participants' decisions about keeping or rerunning a lottery in order to save money for future programs in Experiment 1 (Section 3.1). In panel A, we show WTP to keep the initial lottery result in standard deviations relative to the benchmark goods. The far left blue bar shows both rights goods pooled, while the next two bars show results for lawyers and health care disaggregated. Panel B shows the share of respondents with positive WTP to keep the lottery by good. Both panels include ± 1 standard errors. See Table 2 for detailed regression results.

Figure 3: Experiment 2: Dignity of Choice

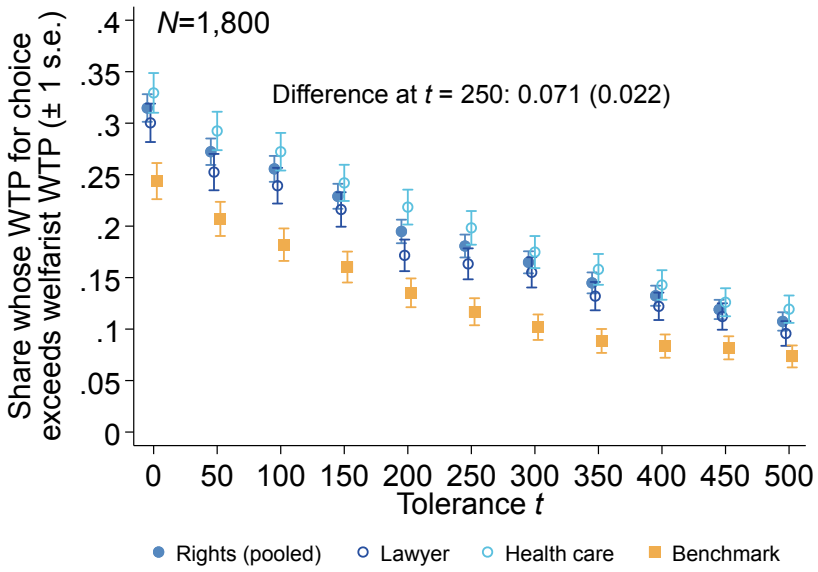
(a) WTP for Choice



(b) Share with positive WTP for Choice

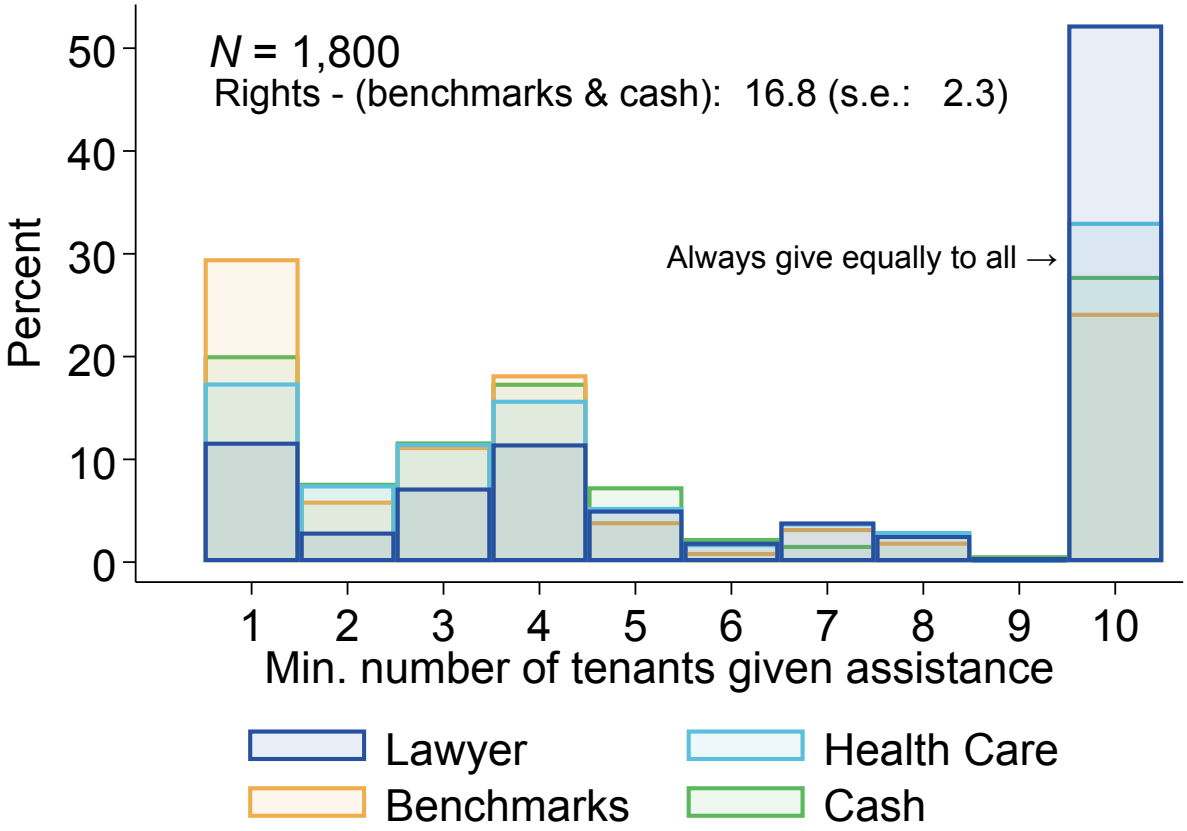


(c) Share with WTP for Choice Exceeding Welfarist WTP



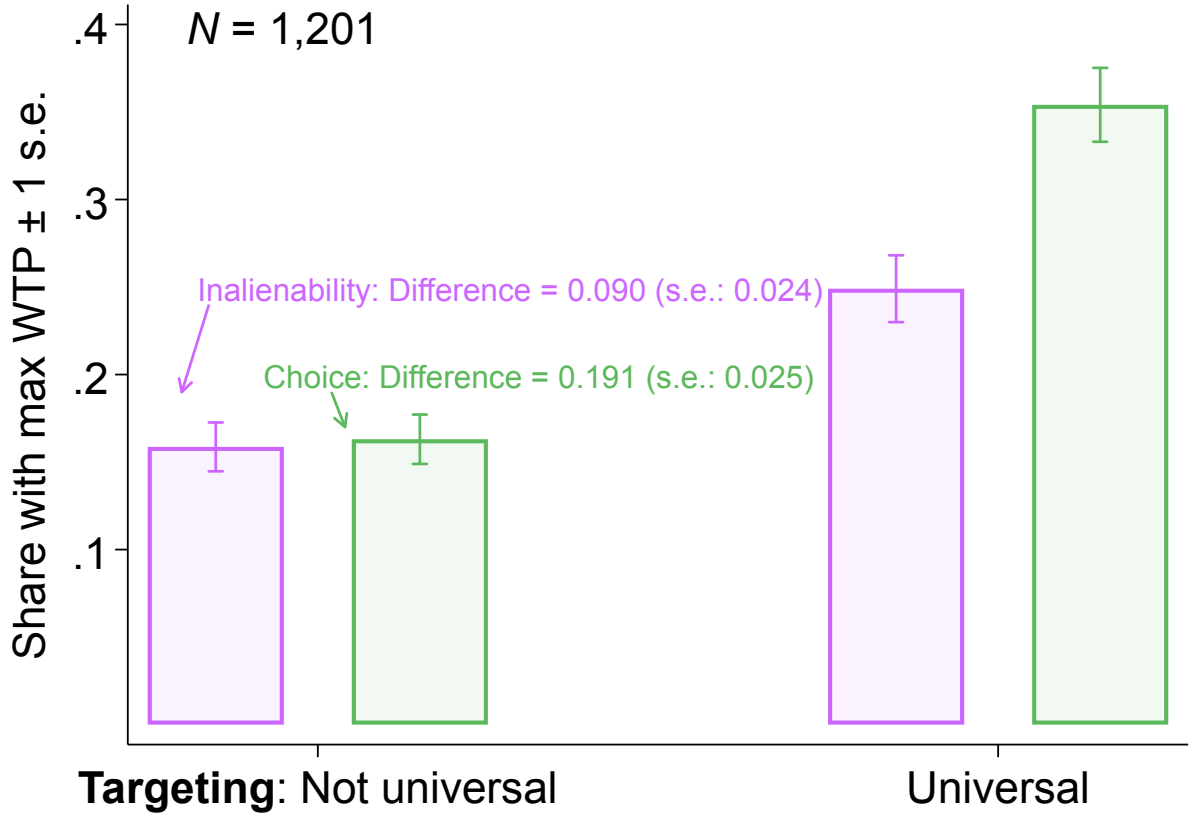
Note: This figure shows participants' decisions about giving tenants a choice between cash and a good, versus saving money for future programs in Experiment 2 (Section 3.2). Panel A plots participants' WTP to give tenants a choice in standard deviations relative to the benchmark goods (±1 s.e.). We show this for different posterior beliefs about the percent of tenants expected to pick cash over the good. The shaded gray area emphasizes those with high posteriors (≥ 90%). See Table 3 for detailed regression results. Panel B plots the same for the share with positive WTP. Panel C shows the share of participants whose WTP for choice exceeds their welfarist WTP (Equation 11) as a function of the tolerance t.

Figure 4: Anti-Targeting



Note: This figure shows the distribution of the minimum number of tenants given assistance in Experiment 4 (Section 4.1). For example, if a participant chose to give two tenants the good and cash, but to give everyone a lawyer rather than giving one tenant the good and cash, the minimum number of tenants given assistance is two. If the minimum is 10, the participant always chose to give the good to everyone. The blue series show distributions for lawyers and health care, the orange series shows the distribution for benchmark goods, and the green series shows the distribution for cash; half of participants who saw benchmark goods throughout the survey were asked about distributing cash in this experiment. See Table 4 for detailed regression results.

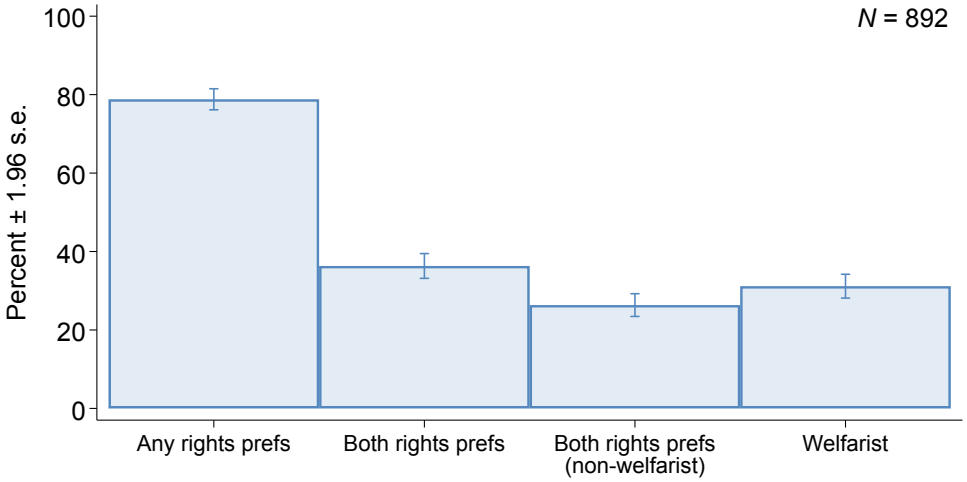
Figure 5: Correlations Between Universalism and Features of Rights



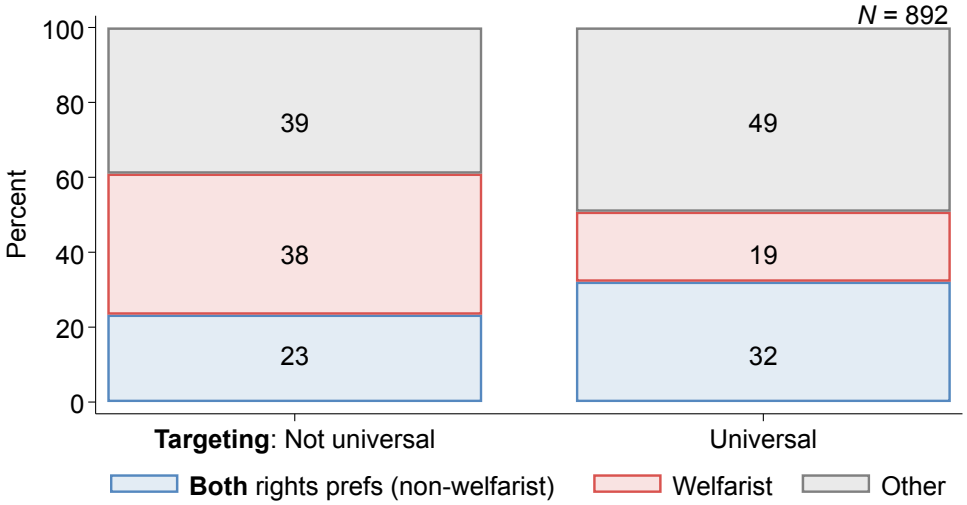
Note: This figure shows the share of participants with the maximum possible WTP in the inalienability (pink) and dignity of choice (green) experiments. Shares are split by whether participants distributed the good universally in Experiment 4. The sample here is restricted to those who saw rights goods (lawyers or health care). See Table 5 for detailed regression results.

Figure 6: Rights Preferences and Welfarism

(a) Prevalence

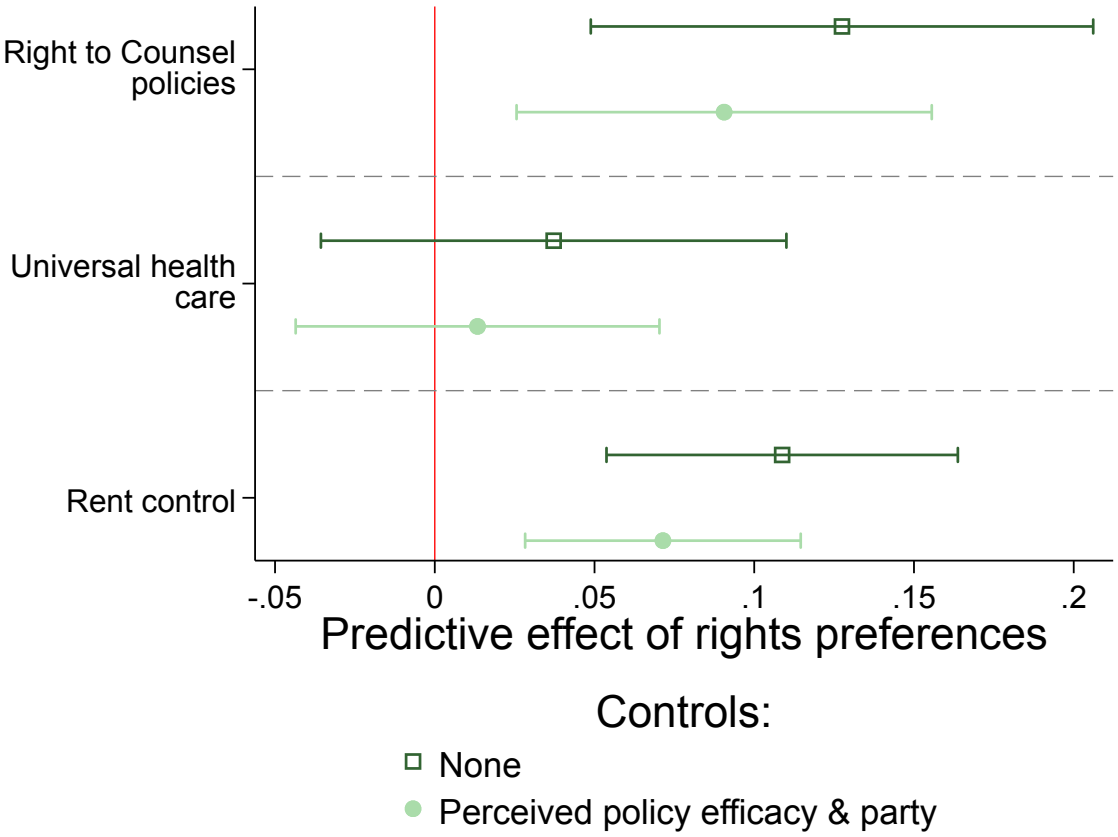


(b) Rights Preferences, Welfarism, and Universalism



Note: Panel A presents the prevalence of rights based preferences and welfarism according to several definitions. First, *Any rights prefs* includes those that have a positive WTP in either Experiment 1 or 2. Second, *Both rights prefs* refers to those who with positive WTP in *both* Experiment 1 and 2. Third, *Both rights prefs (Non-Welfarist)* adds the restriction of non-welfarism. *Welfarist* participants, fourth, are those who change their choices about targeting when information about efficacy of lawyers or health care disagrees with their priors. Panel B plots the share of *Both rights prefs (Non-Welfarist)*, *Welfarist*, and *Other* participants among those who did and did not distribute the good universally in Experiment 4. See Table A12 for detailed regression results. Both panels restrict to a constant sample of participants who saw rights goods, and further to those we can classify as welfarist or non-welfarist based on their prior beliefs.

Figure 7: Political Preferences



Note: This figure shows the predictive effect of rights preferences—having positive WTP in both Experiment 1 and 2—on support for specific policies. We show results for *Right to Counsel policies* among those who did the experiment with lawyers, *Universal health care* among those who did the experiment with health care, and *Rent control* among those who did the experiment with either lawyers or health care. Estimates shown with ± 1.96 standard errors.

Table 1: Demographics and Balance

	(1) U.S.	(2) Experimental sample	(3) Rights -Benchmarks
White non-Hispanic	0.66	0.71	-0.01
	[0.47]	[0.45]	(0.02)
Income $>$ 60k	0.24	0.48	-0.03
	[0.43]	[0.50]	(0.02)
Less than Bachelor's	0.67	0.45	-0.00
	[0.47]	[0.50]	(0.02)
Female	0.51	0.51	-0.02
	[0.50]	[0.50]	(0.02)
Less than age 40	0.33	0.56	0.00
	[0.47]	[0.50]	(0.02)
Liberal		0.57	0.05
		[0.49]	(0.02)
Legal case without a lawyer		0.16	0.01
		[0.36]	(0.03)
Urgent health issue without HC		0.31	-0.02
		[0.46]	(0.03)
F-statistic			.996
p-value			0.437
Observations	2,624,206	1,800	1,800

Note: This table shows the composition of our experimental sample relative to all U.S. adults (18+, a requirement on Prolific) in the 2021 ACS (Ruggles et al., 2023). Column (3) show differences in participants assigned to lawyers and health care compared to those assigned to benchmarks from an OLS regression. The F-statistic is from a joint test of significance for the listed demographic variables. Brackets show standard deviations. Parentheses show robust standard errors.

Table 2: Tests of Inalienability

	(1) Non-zero WTP (= 1)	(2) Max WTP (= 1)	(3) WTP (s.d.)	(4) WTP (s.d.)	(5) WTP (s.d.)
Rights good (= 1)	0.078	0.080	0.289		
	(0.025)	(0.017)	(0.053)		
	[0.002]	[0.000]	[0.000]		
Lawyers (= 1)				0.354	
				(0.064)	
				[0.000]	
Health care (= 1)					0.222
					(0.061)
					[0.000]
Raw mean (benchmarks)			58.4	58.4	58.4
Raw s.d. (benchmarks)			69.9	69.9	69.9
Mean (benchmarks)	0.456	0.117	-0.000	-0.000	-0.000
Observations	1,800	1,800	1,800	1,205	1,194

Note: This table shows the effects of being assigned to a rights good on three measures of WTP for inalienability (Equations 7 and 8). Columns (1)-(3) pool lawyers and health care, while column (4) shows results for lawyers and column (5) shows results for health care. Parentheses show robust standard errors. In columns (3)-(5), WTP is reported in standard deviations relative to the benchmark goods. Parentheses show robust standard errors. Brackets show p-values.

Table 3: Tests of Dignity of Choice

				Posterior ≥ 0.9			Posterior $= 1$		
	(1) Non-zero WTP (= 1)	(2) Max WTP (= 1)	(3) WTP (s.d.)	(4) Non-zero WTP (= 1)	(5) Max WTP (= 1)	(6) WTP (s.d.)	(7) Non-zero WTP (= 1)	(8) Max WTP (= 1)	(9) WTP (s.d.)
Rights good (= 1)	0.171	0.123	0.495	0.166	0.139	0.518	0.072	0.153	0.516
	(0.025)	(0.018)	(0.057)	(0.030)	(0.020)	(0.067)	(0.047)	(0.033)	(0.109)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.122]	[0.000]	[0.000]
Posterior	-0.442	-0.244	-1.046						
	(0.044)	(0.047)	(0.135)						
	[0.000]	[0.000]	[0.000]						
Raw mean (benchmarks)			223.0			223.0			223.0
Raw s.d. (benchmarks)			277.4			277.4			277.4
Mean (benchmarks)	0.432	0.092	0.000	0.432	0.092	0.000	0.432	0.092	0.000
Observations	1,800	1,800	1,800	1,058	1,058	1,058	459	459	459

Note: This table shows the effects of being assigned to a rights good on three measures of WTP for dignity of choice (Equation 7). Columns (1)-(3) show results for the whole sample controlling for posterior beliefs about the percent of tenants who will choose cash over the good. Columns (4)-(6) restrict to those with posteriors beliefs greater than or equal to 90% and columns (7)-(9) restrict to those with posteriors of 100%. In columns (3), (6) and (9), WTP is reported in standard deviations relative to the benchmark goods. Parentheses show robust standard errors. Brackets show *p*-values.

Table 4: Tests of Anti-Targeting

	(1) Universal (= 1)	(2) No. Tenants	(3) Universal (= 1)	(4) No. Tenants	(5) Universal (= 1)	(6) No. Tenants
Rights good (= 1)	0.168	1.419				
	(0.023)	(0.176)				
	[0.000]	[0.000]				
Lawyers (= 1)			0.263	2.140		
			(0.027)	(0.201)		
			[0.000]	[0.000]		
Health care (= 1)					0.071	0.685
					(0.026)	(0.204)
					[0.007]	[0.001]
Mean (benchmarks)	0.260	4.855	0.260	4.855	0.260	4.855
Observations	1,800	1,800	1,205	1,205	1,194	1,194

Note: This table shows the effects of being assigned to a rights good on choices in the anti-targeting experiment (Equations 7 and 8). The outcome columns (1), (3) and (5) is an indicator for whether the participant provided the good universally. The outcome columns (2), (4) and (6) is the minimum number of tenants the participant distributed to, which ranges between 1 (preferring to giving the poorest tenant the good and cash) and 10 (always preferring universal provision). Columns (1)-(2) pool lawyers and health care, while columns (3)-(4) shows results for lawyers and column (5)-(6) shows results for health care. Parentheses show robust standard errors. Brackets show p-values.

Table 5: Tests of Correlations with Universalism

	Dep. Var.: Universal (= 1)			
	(1)	(2)	(3)	(4)
Panel A. Rights goods (pooled)				
WTP for inalienability (s.d.)	0.031			0.006
	(0.012)			(0.012)
	[0.013]			[0.626]
WTP for dignity of choice (s.d.)		0.086		0.085
		(0.011)		(0.011)
		[0.000]		[0.000]
Mean WTP (s.d.)			0.098	
			(0.014)	
			[0.000]	
Panel B. Lawyers				
WTP for inalienability (s.d.)	0.031			-0.002
	(0.017)			(0.017)
	[0.067]			[0.911]
WTP for dignity of choice (s.d.)		0.094		0.095
		(0.015)		(0.015)
		[0.000]		[0.000]
Mean WTP (s.d.)			0.100	
			(0.019)	
			[0.000]	
Panel C. Health care				
WTP for inalienability (s.d.)	0.021			0.009
	(0.018)			(0.018)
	[0.238]			[0.612]
WTP for dignity of choice (s.d.)		0.057		0.056
		(0.016)		(0.016)
		[0.000]		[0.001]
Mean WTP (s.d.)			0.069	
			(0.021)	
			[0.001]	
Mean	0.428	0.428	0.428	0.428
Observations	1,201	1,201	1,201	1,201

Note: This table shows the effect of WTP for inalienability, WTP for dignity of choice, and mean WTP on universal provision in the anti-targeting experiment. All WTPs are in terms of s.d. relative to the benchmark goods. The outcome is an indicator for whether the participant distributed the good universally. The sample is those who did the experiment with rights goods: Panel A shows pooled results, while Panels B and C show lawyers and health care, respectively. Parentheses show robust standard errors. Brackets show p-values.

Appendices for Online Publication

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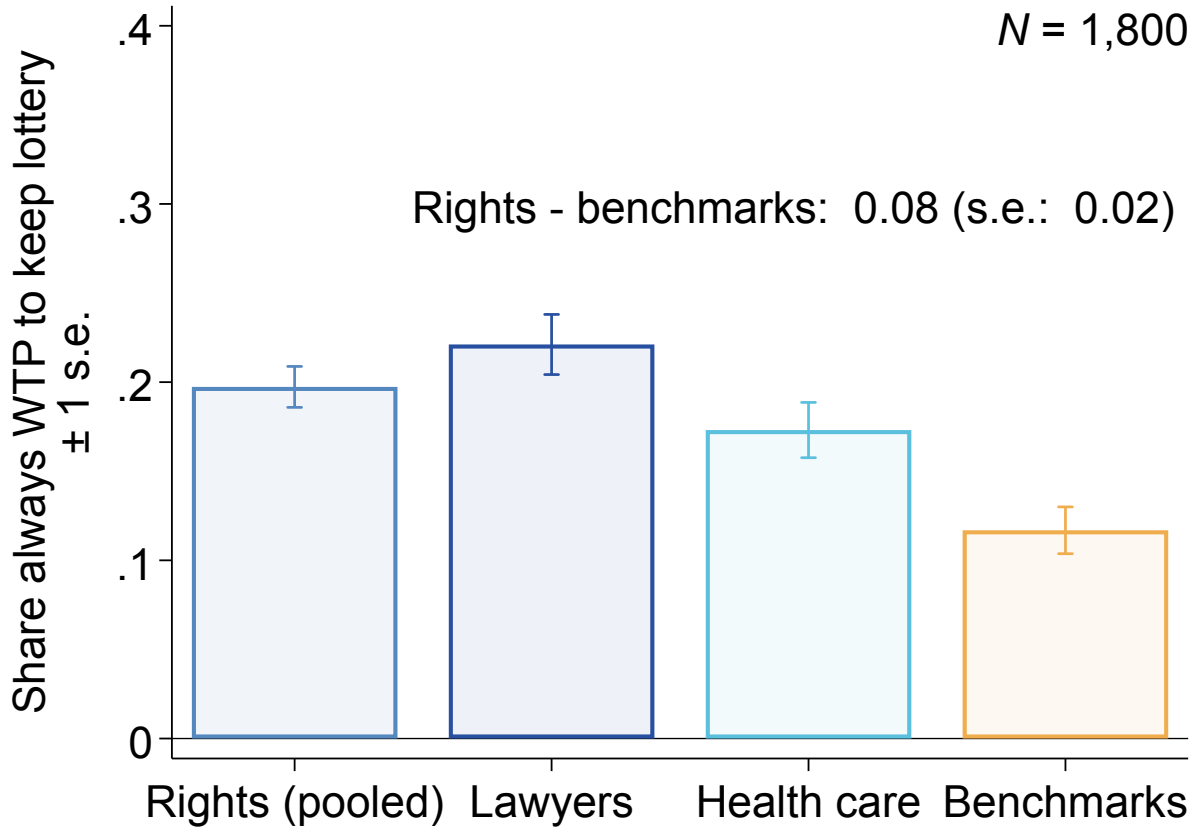
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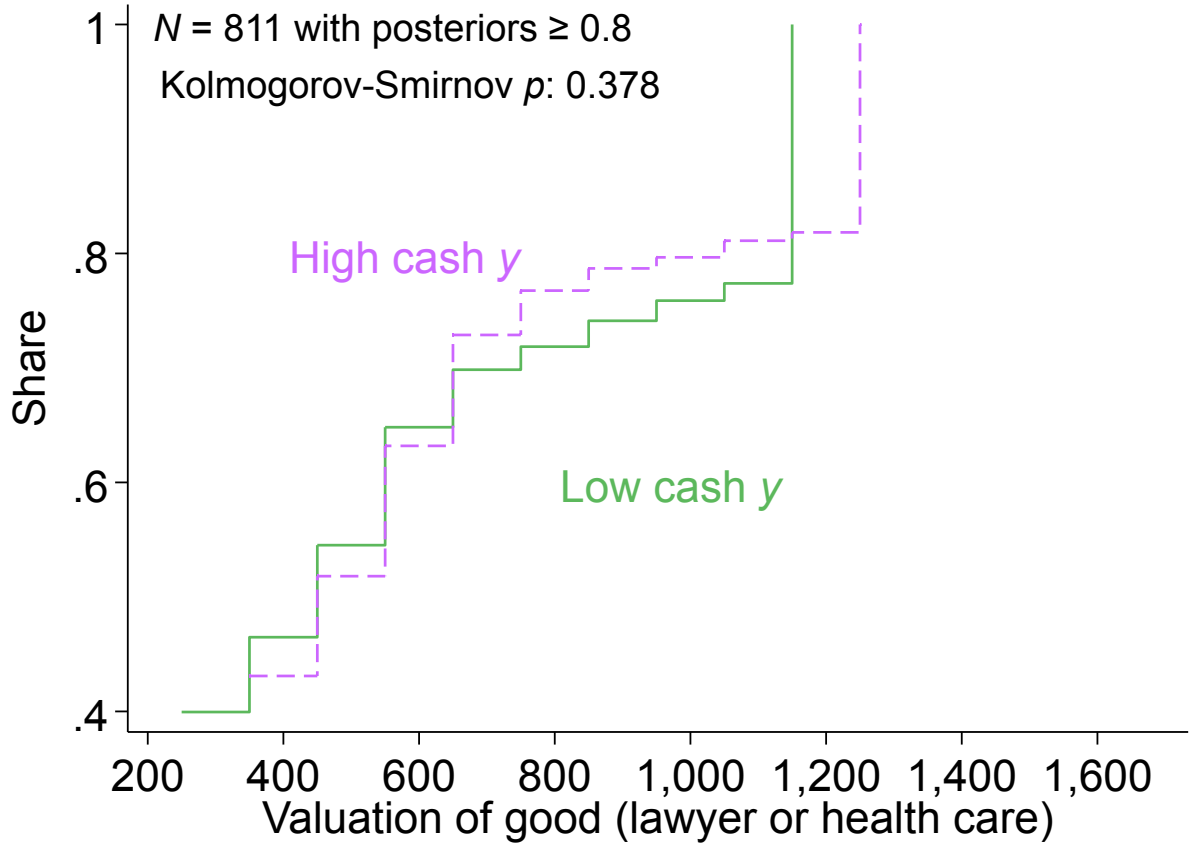
A Additional Figures

Figure A1: Share Always Willing to Pay to Keep Lottery



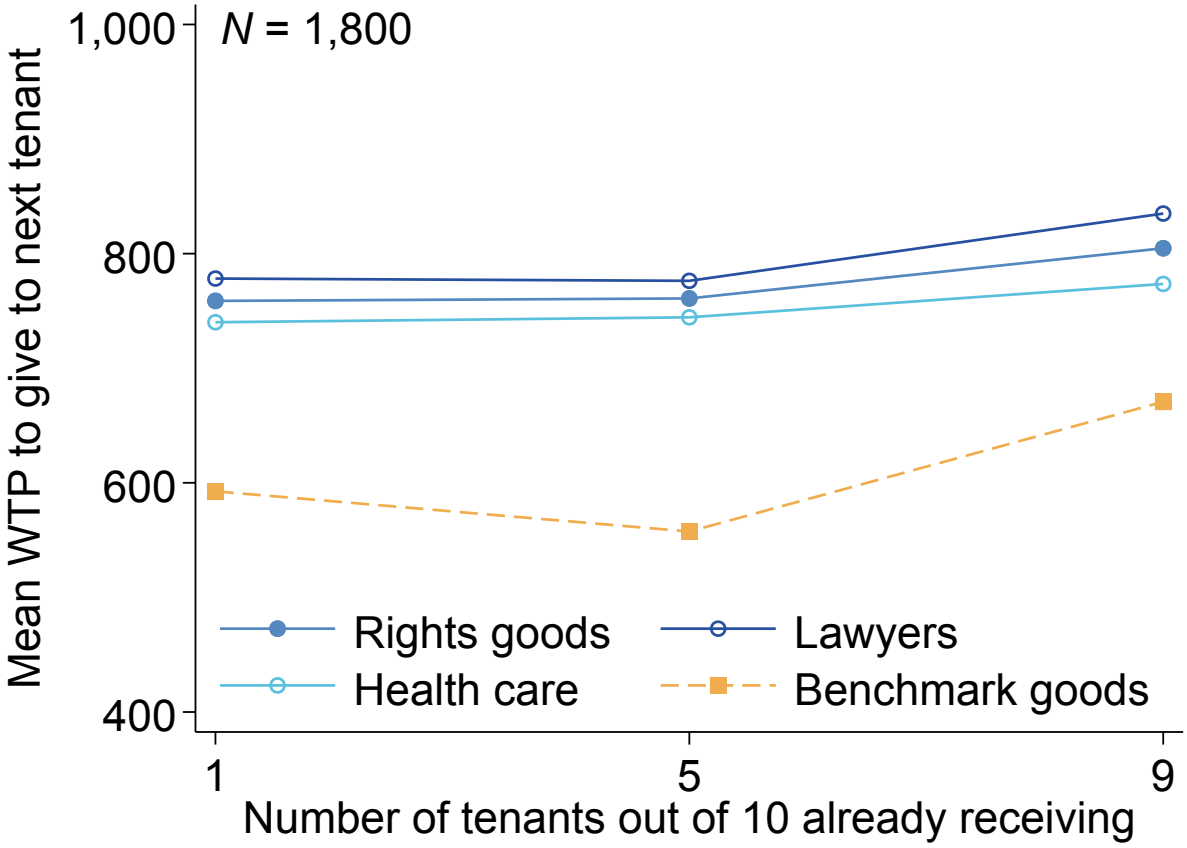
Note: This figure shows participants' decisions about keeping or rerunning a lottery in order to save money for future programs in Experiment 1 (Section 3.1). The y-axis is the share of participants who are willing to pay the maximum value we elicit to keep the lottery. The far left blue bar shows both rights goods pooled, while the next two bars show results for lawyers and health care disaggregated.

Figure A2: WTP for Dignity of Choice by Cash Value



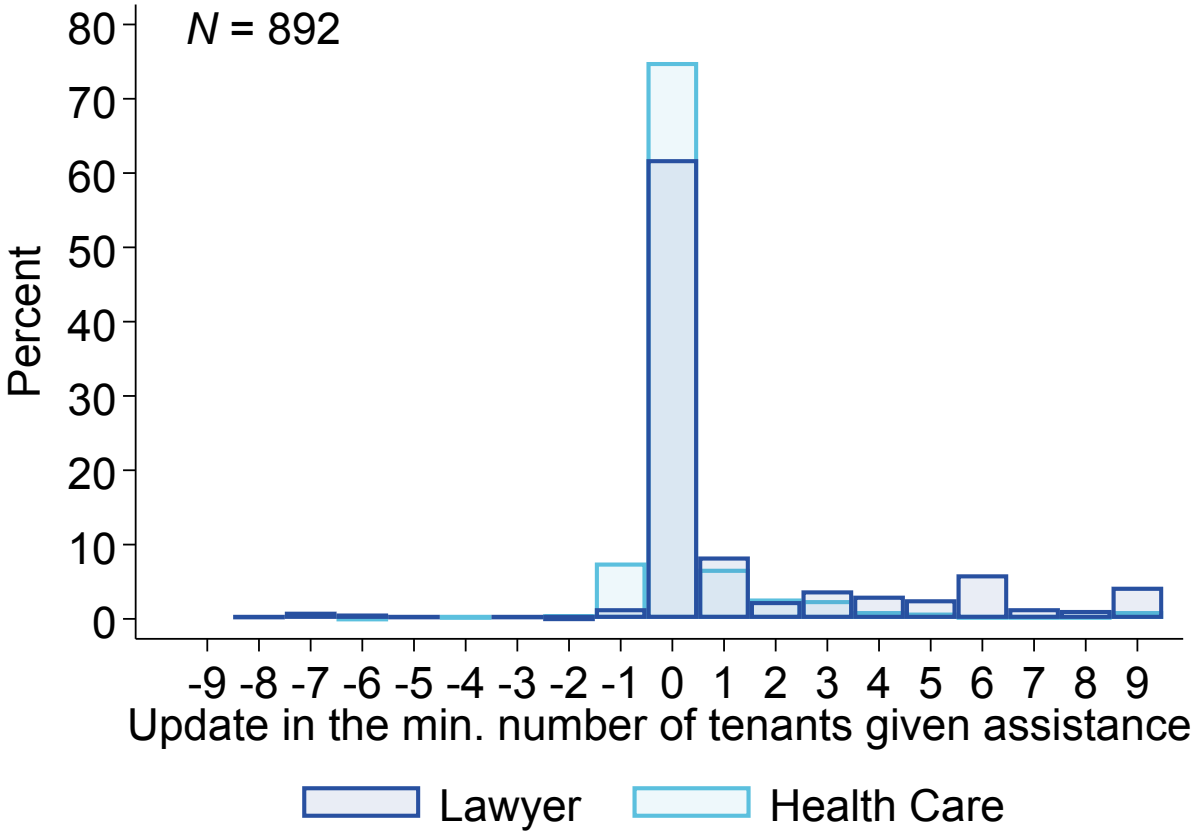
Note: This figure plots the distributions of participants' valuation of the good by the randomized cash value seen in Experiment 2. The high cash $y = \$300$ (dashed pink line) and the low cash $y = \$200$ (solid green line). The sample is restricted to those who did the experiments with rights goods and with posteriors about the percent of tenants expected to choose cash over the good $\geq 80\%$.

Figure A3: Egalitarianism



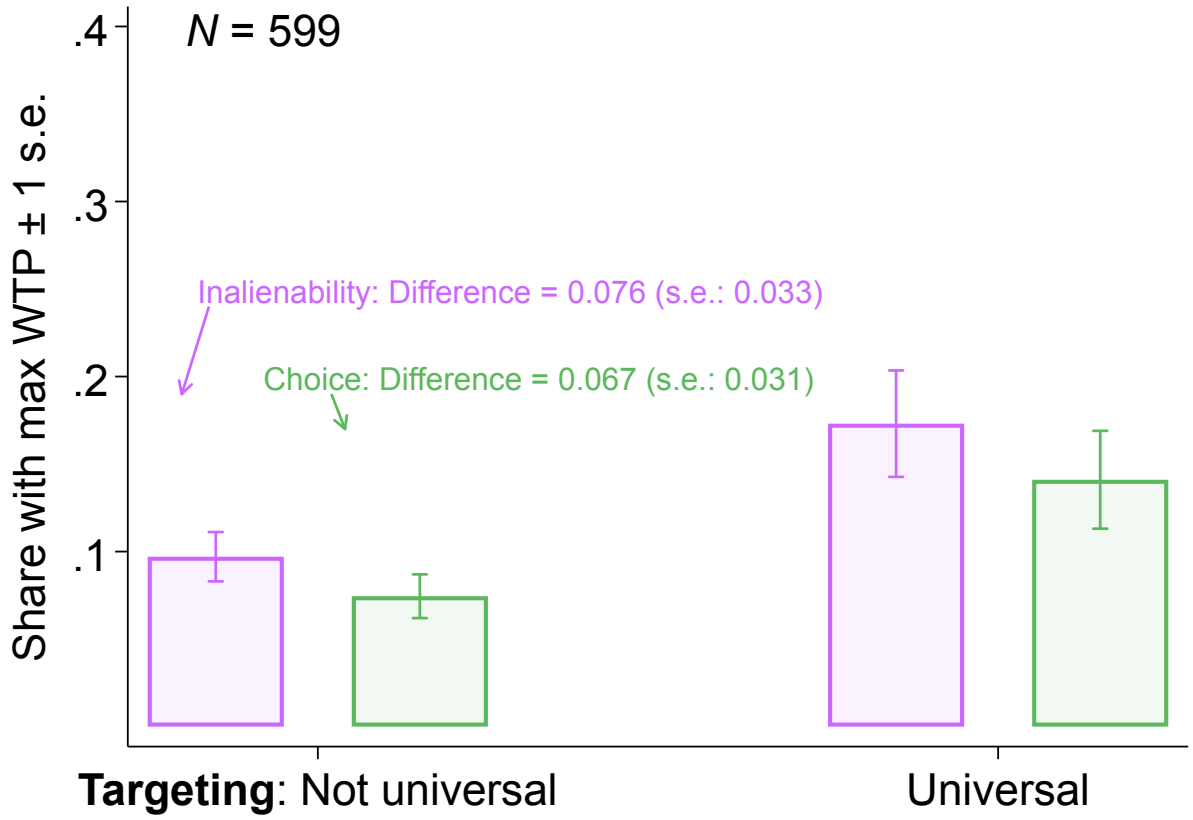
Note: This figure plots mean WTP to give the good to the next tenant in Experiment 3 (Section 3.3). Along the x-axis is the number of tenants out of 10 already assigned to receive the good. Rights goods are presented both pooled and disaggregated by lawyers health care.

Figure A4: Anti-Targeting: Updates After Information Treatment



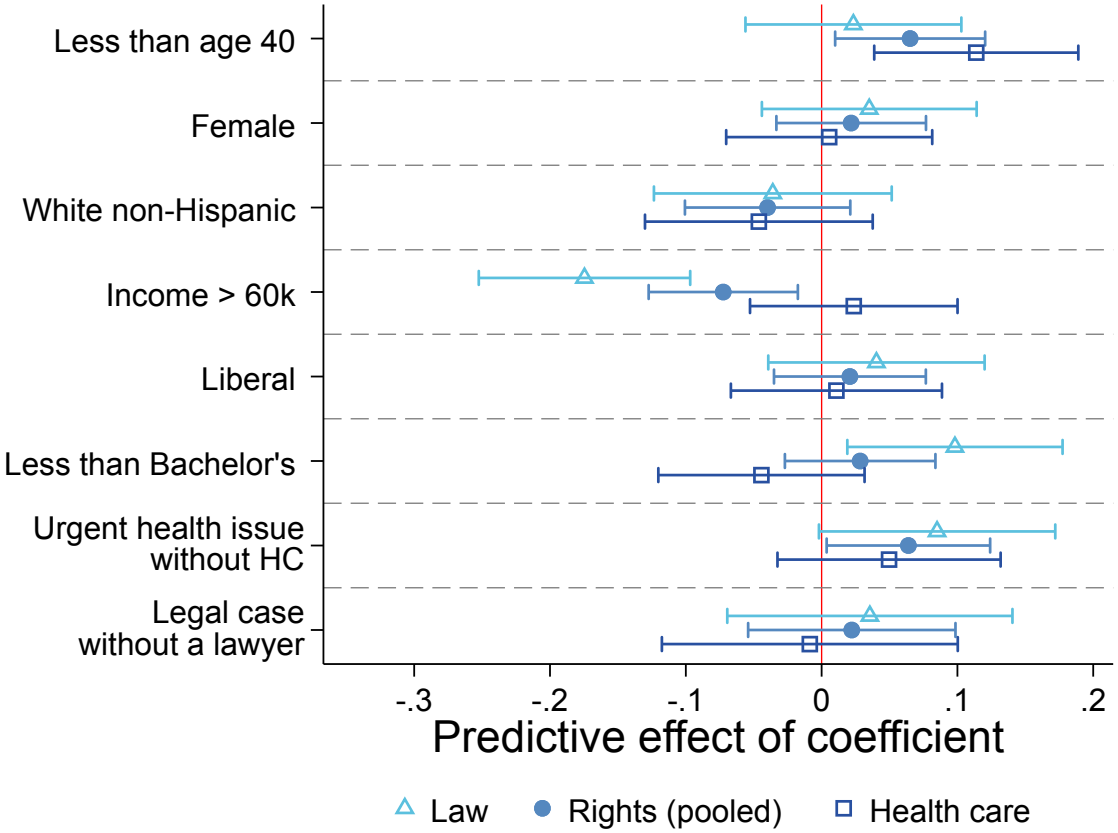
Note: This figure shows the distribution of updates in the minimum number of tenants given assistance (Figure 4) after being shown either a high or low information treatment (Appendix D.7). Only participants in lawyer or health care treatments saw information and were offered the chance to update. We restrict the sample to participants who saw rights goods, and further to those we can classify as welfarist or non-welfarist based on their prior beliefs. Zero indicates the participant declined to update. Positive numbers indicate the participant chose to update in the direction of information shown, relative to their priors.

Figure A5: Correlations Between Universalism and Features of Rights for Benchmark Goods



Note: This figure replicates Figure 5 for the sample of those who saw benchmark goods (YMCA membership or bus pass). It shows the share of participants with the maximum possible WTP in the inalienability (pink) and dignity of choice (green) experiments. Shares are split by whether participants distributed the good universally in Experiment 4. The

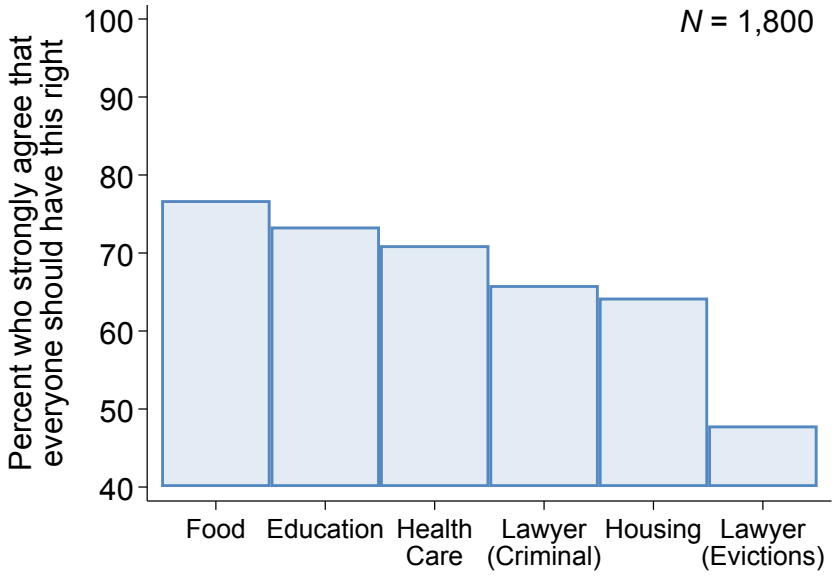
Figure A6: Demographics and Rights Preferences



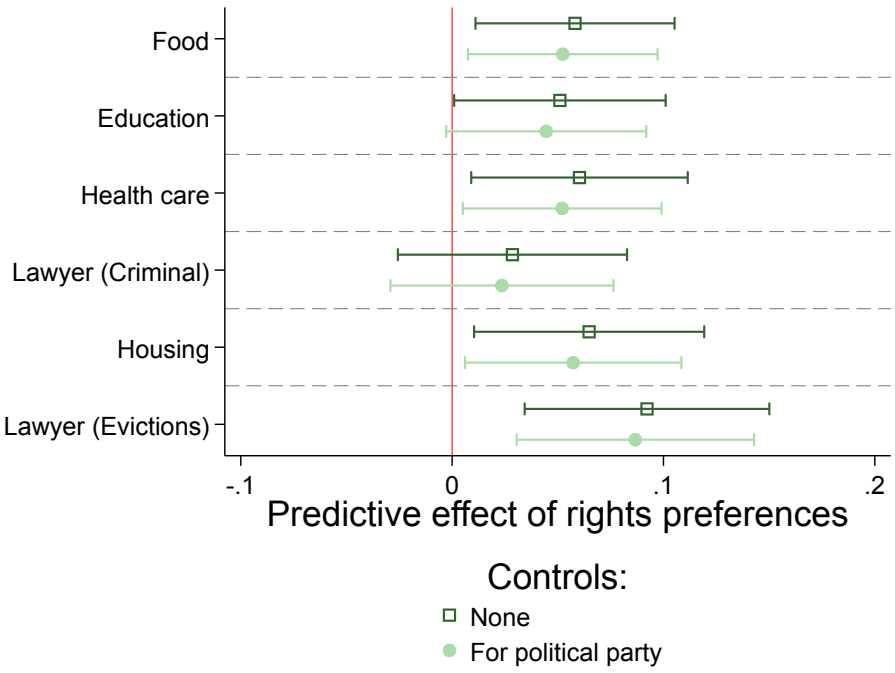
Note: This figure shows estimates of rights preferences for different demographics. Here, rights preferences are defined as positive WTP in both Experiment 1 and Experiment 2. We present estimates for rights good (pooled) as well as for lawyers and health care. *Urgent health issue without HC* captures responses to the question "Have you ever needed urgent medical care that you did not seek because of the cost?" and *Legal case without a lawyer* captures responses to "Have you ever been involved in a legal matter without a lawyer representing you?" Estimates shown with ± 1.96 standard errors.

Figure A7: Rights to Other In-Kind Goods

(a) Strong Agreement with Rights to Other In-Kind Goods



(b) Rights Preferences and Rights to Other In-Kind Goods



Note: This figures plots results from asking Spectators if they agree there is a right to several types of in-kind goods. Panel A plots the percent who "strongly agree" that each good should be a right. Panel B shows the predictive effect of having positive WTP in both Experiments 1 and 2 on strongly agreeing each good should a right. Estimates shown with ± 1.96 standard errors.

B Additional Tables

Table A1: Main Study: Balance

	Rights goods			Benchmark goods			p-values		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Pooled	Lawyers	Health care	Pooled	YMCA	Bus pass	1=4	2=4	3=4
White non-Hispanic	0.71	0.71	0.70	0.72	0.72	0.72	0.72	0.85	0.71
	[0.46]	[0.45]	[0.46]	[0.45]	[0.45]	[0.45]			
Liberal	0.59	0.57	0.61	0.54	0.53	0.55	0.03	0.23	0.01
	[0.49]	[0.50]	[0.49]	[0.50]	[0.50]	[0.50]			
Income $>$ 60k	0.47	0.49	0.45	0.50	0.53	0.48	0.15	0.59	0.05
	[0.50]	[0.50]	[0.50]	[0.50]	[0.50]	[0.50]			
Less than Bachelor's	0.45	0.45	0.45	0.45	0.40	0.49	0.96	0.79	0.76
	[0.50]	[0.50]	[0.50]	[0.50]	[0.49]	[0.50]			
Legal case without a lawyer	0.16	0.17	0.14	0.16	0.15	0.16	0.72	0.25	0.55
	[0.36]	[0.38]	[0.35]	[0.36]	[0.36]	[0.37]			
Urgent health issue without HC	0.31	0.30	0.32	0.32	0.30	0.33	0.43	0.20	0.96
	[0.46]	[0.46]	[0.47]	[0.47]	[0.46]	[0.47]			
Female	0.50	0.51	0.50	0.52	0.53	0.51	0.42	0.67	0.31
	[0.50]	[0.50]	[0.50]	[0.50]	[0.50]	[0.50]			
Less than age 40	0.57	0.55	0.58	0.56	0.54	0.57	0.97	0.90	0.82
	[0.50]	[0.50]	[0.49]	[0.50]	[0.50]	[0.50]			
F-statistic							0.996	0.510	1.586
p-value							0.437	0.849	0.124
Observations	1,201	606	595	599	299	300	1,800	1,205	1,194

Note: This table expands on Table 1. Columns (1)-(6) show demographic characteristics for different goods, pooled and separately. Columns (7)-(9) show the *p*-values of the differences between pooled benchmarks and each rights goods column. The *F*-statistic is from a joint test of significance for the listed demographic variables. Brackets show standard deviations.

Table A2: Main Study: Attrition

	Number of participants			
	(1) Lawyers	(2) Health care	(3) YMCA	(4) Bus pass
Started	627	623	309	316
Egalitarianism⋆	615	605	306	310
Passed at least 1 of 2 attenion checks⋆	614	604	303	309
Inalienability⋆	614	603	306	309
Dignity of Choice	612	600	302	306
Anti-targeting	610	599	302	306
Passed at least 2 of 3 attenion checks	607	596	300	300
Good valuation	607	596	300	300
Demographics & political preferences	606	595	299	300

Note: This table shows the attrition in our survey. Rows show the number of Spectators participating, for each good, at each stage of our survey flow. Rows marked with ⋆ were presented in a randomized order.

Table A3: Follow-up Study: Balance and Attrition

	Effectiveness			Stakes			Identity			p-values		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Pooled	High Stakes	Low Stakes	Pooled	High Stakes	Low Stakes	Pooled	High Stakes	Low Stakes	2=3	5=6	8=9
White non-Hispanic	0.63	0.62	0.63	0.62	0.62	0.62	0.62	0.62	0.61	0.90	0.95	0.53
	[0.48]	[0.48]	[0.48]	[0.49]	[0.49]	[0.49]	[0.49]	[0.49]	[0.49]			
Liberal	0.51	0.53	0.49	0.52	0.52	0.51	0.52	0.51	0.52	0.08	0.57	0.79
	[0.50]	[0.50]	[0.50]	[0.50]	[0.50]	[0.50]	[0.50]	[0.50]	[0.50]			
Income $>$ 60k	0.56	0.57	0.56	0.55	0.54	0.57	0.55	0.56	0.55	0.83	0.13	0.82
	[0.50]	[0.50]	[0.50]	[0.50]	[0.50]	[0.50]	[0.50]	[0.50]	[0.50]			
Less than Bachelor's	0.43	0.42	0.43	0.44	0.44	0.44	0.44	0.43	0.45	0.68	0.66	0.50
	[0.49]	[0.49]	[0.50]	[0.50]	[0.50]	[0.50]	[0.50]	[0.50]	[0.50]			
Female	0.57	0.57	0.58	0.56	0.56	0.57	0.57	0.56	0.58	0.52	0.76	0.55
	[0.49]	[0.50]	[0.49]	[0.50]	[0.50]	[0.50]	[0.50]	[0.50]	[0.49]			
Less than age 40	0.57	0.57	0.57	0.57	0.58	0.55	0.56	0.56	0.55	0.91	0.27	0.67
	[0.49]	[0.49]	[0.50]	[0.50]	[0.49]	[0.50]	[0.50]	[0.50]	[0.50]			
Attrition Rate (%)	0.03	0.04	0.03	0.03	0.04	0.02	0.03	0.03	0.03	0.947	0.103	0.355
F-statistic										0.643	0.739	0.279
p-value										0.696	0.618	0.947
Observations	1,984	984	1,000	2,004	1,012	992	2,012	1,000	1,012	1,984	2,004	2,012

Note: Columns (1)-(9) show demographic characteristics for by arms in follow-up study, pooled and separately. Columns (10)-(12) show the *p*-values of the differences between high and low stakes for each comparison. The *F*-statistic is from a joint test of significance for the listed demographic variables. Brackets show standard deviations.

Table A4: Inalienability Robustness: Controlling for Valuation of Good

	(1) Non-zero WTP (= 1)	(2) Max WTP (= 1)	(3) WTP (s.d.)	(4) WTP (s.d.)	(5) WTP (s.d.)
Rights good (= 1)	0.065	0.064	0.248		
	(0.027)	(0.019)	(0.058)		
	[0.017]	[0.001]	[0.000]		
Lawyers (= 1)				0.255	
				(0.077)	
				[0.001]	
Health care (= 1)					0.235
					(0.064)
					[0.000]
WTP for good FE	✓	✓	✓	✓	✓
Raw mean (benchmarks)			58.4	58.4	58.4
Raw s.d. (benchmarks)			69.9	69.9	69.9
Mean (benchmarks)	0.456	0.117	-0.000	-0.000	-0.000
Observations	1,800	1,800	1,800	1,205	1,194

Note: This table replicates Table 2 with added fixed effects for valuation of the good. Parentheses show robust standard errors. Brackets show *p*-values.

Table A5: Balance Among High Posterior Participants

	(1) Rights-Benchmark goods
White non-Hispanic	-0.01
	(0.03)
Liberal	0.05
	(0.03)
Income $>$ 60k	-0.03
	(0.03)
Less than Bachelor's	0.02
	(0.03)
Legal case without a lawyer	0.01
	(0.04)
Urgent health issue without HC	-0.03
	(0.03)
Female	-0.01
	(0.03)
Less than age 40	0.02
	(0.03)
F-statistic	0.750
Observations	1,198

Note: This table shows balance for respondents with high posteriors ($\geq$ 90%) about the percent of tenants expected to pick cash over the good. The *F*-statistic is from a joint test of significance for the listed demographic variables. Parentheses show robust standard errors.

Table A6: Dignity of Choice Robustness: Lawyers and Health Care

				Posterior ≥ 0.9			Posterior $= 1$		
	(1) Non-zero WTP (= 1)	(2) Max WTP (= 1)	(3) WTP (s.d.)	(4) Non-zero WTP (= 1)	(5) Max WTP (= 1)	(6) WTP (s.d.)	(7) Non-zero WTP (= 1)	(8) Max WTP (= 1)	(9) WTP (s.d.)
Panel A. Lawyers									
Lawyers (= 1)	0.250	0.168	0.642	0.263	0.180	0.677	0.131	0.237	0.752
	(0.029)	(0.024)	(0.072)	(0.037)	(0.029)	(0.089)	(0.060)	(0.050)	(0.157)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.029]	[0.000]	[0.000]
Posterior	-0.351	-0.278	-1.085						
	(0.055)	(0.059)	(0.170)						
	[0.000]	[0.000]	[0.000]						
Panel B. Health Care									
Health Care (= 1)	0.099	0.079	0.349	0.086	0.106	0.384	0.032	0.095	0.354
	(0.029)	(0.020)	(0.067)	(0.036)	(0.024)	(0.080)	(0.054)	(0.037)	(0.124)
	[0.001]	[0.000]	[0.000]	[0.016]	[0.000]	[0.000]	[0.547]	[0.011]	[0.004]
Posterior	-0.517	-0.166	-0.932						
	(0.065)	(0.059)	(0.179)						
	[0.000]	[0.005]	[0.000]						
Raw mean (benchmarks)			223.0			189.3			213.0
Raw s.d. (benchmarks)			277.4			247.5			268.3
Mean (benchmarks)	0.432	0.092	0.000	0.382	0.066	-0.121	0.438	0.087	-0.036
Observations: lawyers	606	606	606	273	273	273	102	102	102
Observations: health care	595	595	595	327	327	327	149	149	149
Observations: benchmarks	599	599	599	458	458	458	208	208	208

Note: This table replicates Table 3 separately for lawyers (Panel A) and health care (Panel B). Parentheses show robust standard errors. Brackets show p-values.

Table A7: Dignity of Choice Robustness: High Posteriors

	Posterior = 1				Posterior ≥ 0.9
	(1) Non-zero WTP (= 1)	(2) Max WTP (= 1)	(3) WTP (s.d.)	(4) Entitled to Choice (= 1)	(5) Entitled to Choice (= 1)
Rights good (= 1)	0.077	0.155	0.547	0.115	0.132
	(0.048)	(0.034)	(0.110)	(0.045)	(0.033)
	[0.111]	[0.000]	[0.000]	[0.011]	[0.000]
Raw mean (benchmarks)			213.0		
Raw s.d. (benchmarks)			268.3		
Mean (benchmarks)	0.438	0.087	-0.036	0.308	0.276
DDML	✓	✓	✓		
Observations	459	459	459	459	804

Note: This table shows robustness checks for dignity of choice using the sub-sample of respondents who believe 100 out of 100 tenants would choose cash over the good. Columns (1)-(3) replicate columns (7)-(9) of Table 3 with double/de-biased machine learning (Chernozhukov et al., 2018). The model selects from the demographic controls reported in Table 1: race, income, education, gender, age, political beliefs, having experienced a legal case without a lawyer, and having had an urgent health issue without access to health care. The outcome in column (4) is a dummy that indicates a respondent selected "All tenants should be entitled to the choice of a [good]" as one reason for their decisions about giving the tenant a choice. Parentheses show robust standard errors. Brackets show p-values.

Table A8: Tests of Egalitarianism

	(1) Non-zero WTP (= 1)	(2) Max WTP (= 1)	(3) WTP (s.d.)	(4) Non-zero WTP (= 1)	(5) Max WTP (= 1)	(6) WTP (s.d.)
Rights good=1	0.082	0.193	0.562	0.074	0.144	0.483
	(0.020)	(0.033)	(0.066)	(0.019)	(0.034)	(0.068)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
$\mathbb{1}(z = 9)$	0.043	0.124	0.291	0.055	0.122	0.307
	(0.023)	(0.038)	(0.079)	(0.023)	(0.038)	(0.079)
	[0.068]	[0.001]	[0.000]	[0.017]	[0.001]	[0.000]
Rights good=1 $\times$ $\mathbb{1}(z = 9)$	-0.046	0.011	-0.154	-0.055	0.011	-0.170
	(0.026)	(0.047)	(0.090)	(0.025)	(0.047)	(0.090)
	[0.075]	[0.809]	[0.087]	[0.027]	[0.815]	[0.058]
WTP for good FE				✓	✓	✓
Raw mean (benchmarks, $\mathbb{1}(z < 9)$)			575.3			575.3
Raw s.d. (benchmarks, $\mathbb{1}(z < 9)$)			328.4			328.4
Mean (benchmarks, $\mathbb{1}(z < 9)$)	0.888	0.258	0.000	0.888	0.258	0.000
Observations	1,800	1,800	1,800	1,800	1,800	1,800

Note: This table reports estimates from the differences-in-differences specification from Equation 12. $\mathbb{1}(z < 9)$ indicates the Spectator saw 9 out of 10 tenants had already received the good, rather than 1 or 5 tenants. Outcomes are the extensive margin (*Non-zero WTP*), an indicator for having the maximum WTP that we elicit (*Max WTP*), and the intensive margin (*WTP*). WTP is reported in terms of standard deviations relative to benchmark goods. Columns (4)-(6) add fixed effects for WTP for the good directly. Parentheses show robust standard errors. Brackets show *p*-values.

Table A9: Robustness: Multiple Hypothesis Testing Corrections

	Inalienability	Dignity of Choice	Egalitarianism
	(1) WTP (s.d.)	(2) WTP (s.d.)	(3) WTP (s.d.)
Rights good	0.289	0.518	0.562
	(0.053)	(0.067)	(0.066)
	[0.000]	[0.000]	[0.000]
Rights good & $\mathbb{1}(z = 9)$			-0.154
			(0.090)
			[0.087]
Romano-Wolf p-value	< 0.001	< 0.001	0.096
Posterior ≥ 0.9		✓	
Observations	1,800	1,058	1,800

Note: This table shows robustness to multiple hypothesis testing corrections for inalienability, dignity of choice, and egalitarianism with Romano-Wolf p-values (Clarke et al., 2020) with 1,000 iterations. Parentheses show robust standard errors. Brackets show p-values.

Table A10: Anti-Targeting Robustness: Controlling for Valuation of Good

	(1) Universal (= 1)	(2) No. Tenants	(3) Universal (= 1)	(4) No. Tenants	(5) Universal (= 1)	(6) No. Tenants
Rights good (= 1)	0.044	0.458				
	(0.024)	(0.187)				
	[0.067]	[0.015]				
Lawyers (= 1)			0.116	1.007		
			(0.034)	(0.252)		
			[0.001]	[0.000]		
Health care (= 1)					0.013	0.215
					(0.026)	(0.204)
					[0.621]	[0.293]
WTP for good FE	✓	✓	✓	✓	✓	✓
Mean (benchmarks)	0.260	4.855	0.260	4.855	0.260	4.855
Observations	1,800	1,800	1,205	1,205	1,194	1,194

Note: This table replicates Table 4 with added fixed effects for valuation of the good. Parentheses show robust standard errors. Brackets show p-values.

Table A11: Correlations with Universalism Robustness: Controlling for Valuation of Good

	Dep. Var.: Universal (= 1)			
	(1)	(2)	(3)	(4)
Panel A. Rights goods (pooled)				
WTP for inalienability (s.d.)	0.022			0.008
	(0.012)			(0.012)
	[0.058]			[0.490]
WTP for dignity of choice (s.d.)		0.053		0.051
		(0.011)		(0.011)
		[0.000]		[0.000]
Mean WTP (s.d.)			0.062	
			(0.014)	
			[0.000]	
Panel B. Lawyers				
WTP for inalienability (s.d.)	0.022			-0.001
	(0.016)			(0.017)
	[0.184]			[0.958]
WTP for dignity of choice (s.d.)		0.072		0.072
		(0.016)		(0.017)
		[0.000]		[0.000]
Mean WTP (s.d.)			0.074	
			(0.020)	
			[0.000]	
Panel C. Health care				
WTP for inalienability (s.d.)	0.017			0.010
	(0.017)			(0.017)
	[0.314]			[0.554]
WTP for dignity of choice (s.d.)		0.033		0.032
		(0.016)		(0.016)
		[0.032]		[0.047]
Mean WTP (s.d.)			0.044	
			(0.021)	
			[0.035]	
WTP for good FE	✓	✓	✓	✓
Mean	0.428	0.428	0.428	0.428
Observations	1,201	1,201	1,201	1,201

Note: This table replicates Table 5 with added fixed effects for valuation of the good. Parentheses show robust standard errors. Brackets show p-values.

Table A12: Tests of Universalism and Welfarism

	(1) Any rights prefs	(2) Both rights prefs	(3) Both rights prefs (non-welfarist)	(4) Welfarist
Panel A. All rights goods				
Universal (=1)	0.068	0.026	0.088	-0.190
	(0.028)	(0.034)	(0.032)	(0.030)
	[0.014]	[0.447]	[0.006]	[0.000]
Constant	0.765	0.354	0.233	0.376
	(0.018)	(0.020)	(0.017)	(0.020)
	[0.000]	[0.000]	[0.000]	[0.000]
Observations	892	892	892	892
Panel B. Posteriors $\geq$ 90				
Universal (=1)	0.081	0.064	0.131	-0.229
	(0.039)	(0.042)	(0.038)	(0.039)
	[0.037]	[0.126]	[0.000]	[0.000]
Constant	0.699	0.266	0.146	0.412
	(0.025)	(0.024)	(0.019)	(0.027)
	[0.000]	[0.000]	[0.000]	[0.000]
Observations	533	533	533	533
Panel C. Lawyers				
Universal (=1)	0.049	0.047	0.133	-0.255
	(0.037)	(0.051)	(0.048)	(0.046)
	[0.187]	[0.352]	[0.005]	[0.000]
Constant	0.812	0.410	0.244	0.474
	(0.024)	(0.030)	(0.026)	(0.031)
	[0.000]	[0.000]	[0.000]	[0.000]
Observations	417	417	417	417
Panel D. Health care				
Universal (=1)	0.079	-0.003	0.042	-0.140
	(0.041)	(0.045)	(0.043)	(0.039)
	[0.050]	[0.943]	[0.325]	[0.000]
Constant	0.726	0.308	0.224	0.296
	(0.025)	(0.026)	(0.023)	(0.026)
	[0.000]	[0.000]	[0.000]	[0.000]
Observations	475	475	475	475

Note: This table offers formal tests for Figure 6. Each panel shows estimates of an indicator for universal provision in Experiment 4 on four outcomes. The outcome in column (1) in an indicator for having positive WTP in either Experiment 1 or 2, in column (2) is an indicator for positive WTP in *both* experiments, and in Column (3) is an indicator for positive WTP in *both* experiments and non-welfarism. Finally, the outcome in column (4) is an indicator for welfarist preferences. The sample is Spectators who saw rights goods and who can be classified as welfarist or non-welfarist based on their prior beliefs. Panel A pools both rights goods, Panel B restricts the sample in Panel A to those with posteriors $\geq$ 90%, Panel C shows results for lawyers, and Panel D shows results for health care. Parentheses show robust standard errors. Brackets show p-values.

Table A13: Robustness: Benchmark Incentives and Features of Rights

	Inalienability				Dignity of Choice				Anti-targeting			
	(1) WTP (s.d.)	(2) WTP (s.d.)	(3) WTP (s.d.)	(4) WTP (s.d.)	(5) WTP (s.d.)	(6) WTP (s.d.)	(7) WTP (s.d.)	(8) WTP (s.d.)	(9) No. Tenants	(10) No. Tenants	(11) No. Tenants	(12) No. Tenants
Unincentivized (= 1)	-0.002				-0.052				-0.422			
	(0.096)				(0.095)				(0.326)			
	[0.980]				[0.586]				[0.195]			
Rights good (= 1)		0.289	0.288	0.290		0.518	0.506	0.557		1.419	1.312	1.734
		(0.053)	(0.058)	(0.090)		(0.067)	(0.071)	(0.096)		(0.176)	(0.195)	(0.299)
		[0.000]	[0.000]	[0.001]		[0.000]	[0.000]	[0.000]		[0.000]	[0.000]	[0.000]
Benchmarks Only	✓				✓				✓			
Incentivized Benchmarks	✓	✓	✓		✓	✓	✓		✓	✓	✓	
Unincentivized Benchmarks	✓	✓		✓	✓	✓		✓	✓	✓		✓
Observations	599	1,800	1,648	1,353	458	1,058	952	706	599	1,800	1,648	1,353

Note: This table shows the results of incentivization of benchmark goods on Spectators' decisions in Experiments 1, 2 and 4. Column (2) replicates column (3) of Table 2, column (6) replicates column (6) of Table 3, and column (10) replicates column (2) of Table 4, including all benchmark good observations. Columns (3), (7), and (11) restrict benchmark observations to only those who were incentivized, while column (4), (8), and (12) restrict benchmark observations to only those who were unincentivized. Columns (5)-(8) restrict to those with posteriors ≥ 90. Among those who saw incentivized goods (lawyers, YMCA memberships, bus passes), half randomly had their WTP for the good incentivized while the other half did not. Parentheses show robust standard errors. Brackets show p-values.

Table A14: Robustness: Incentives and Valuation of Good

	Dep. Var.: WTP for good	
	(1) Lawyers	(2) Benchmarks
Unincentivized	-11.448	-7.180
	(26.478)	(28.301)
	[0.666]	[0.800]
Intercept	771.382	341.391
	(18.891)	(20.087)
	[0.000]	[0.000]
Observations	606	303

Note: This table shows the results of differential incentivization on Spectators' WTP for the good. Column (1) presents the effect of incentivization for lawyers and column (2) presents the same for benchmark goods. Among those who saw incentivized goods (lawyers, YMCA memberships, bus passes), half randomly had their WTP for the good incentivized while the other half did not. Parentheses show robust standard errors. Brackets show p-values.

Table A15: Robustness: Double/debiased Machine Learning

	Inalienability		Dignity of Choice		Anti-targeting	
	(1) Non-zero WTP (= 1)	(2) WTP (s.d.)	(3) Non-zero WTP (= 1)	(4) WTP (s.d.)	(5) Universal (= 1)	(6) No. Tenants
Rights good (= 1)	0.073	0.290	0.167	0.512	0.177	1.492
	(0.025)	(0.054)	(0.031)	(0.067)	(0.023)	(0.174)
	[0.004]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Posterior $\geq$ 0.9			✓	✓		
Observations	1,800	1,800	1,058	1,058	1,800	1,800

Note: This table shows our main results using double/debiased machine learning for inalienability, dignity of choice, and anti-targeting (Chernozhukov et al., 2018). The model selects from demographic controls. Eligible demographic controls are those reported in Table 1: race, income, education, gender, age, political beliefs, having experienced a legal case without a lawyer, and having had an urgent health issue without access to health care. Column (1) modifies column (3) of Table 2, column (2) modifies column (6) of Table 3, and column (3) modifies column (2) of Table 4. Parentheses show robust standard errors. Brackets show p-values.

C Framework Appendix

C.1 Proof of Proposition 1

Proof. Consider an allocation $\tilde{Y} \equiv (\tilde{y}_1, \dots \tilde{y}_j)$ that maximizes welfarist social welfare for a given set of welfare weights, utility functions, and incomes. Thus $\tilde{Y}$ solves

$$\max \sum_{j=1}^{J} \gamma_j u(x_j, y_j), \text{ such that } \sum_j y_j \leq m. \tag{14}$$

Put $\Delta u(y_j = k) \equiv u(x_j, y_k) - u(x_j, y_{k-1})$, which is the difference in private utility generated from providing the kth good to individual j. Put $\Delta v(y_j = k) \equiv \gamma_j \Delta u(y_j = k)$, which the difference in welfarist social welfare from providing the kth good to individual j.

Now consider the $Y^* \equiv (y_1^*, \dots y_J^*)$ which solves

$$\max \sum_{j=1}^{J} \gamma_j \left(u(x_j, y_j) + \eta_y \phi(y_j, r_{ij}) \right), \text{ such that } \sum_j y_j \leq m, \tag{15}$$

where $\phi(\cdot)$ is as in Equation (5).

As in the text, let $\tilde{\mathcal{J}} \equiv \{j \leq J : \tilde{y}_j > 0\}$ and $\mathcal{J}^* \equiv \{j \leq J : y_j^* > 0\}$. We want to show that $\tilde{\mathcal{J}} \subseteq \mathcal{J}^*$.

As is standard, we assume $\eta > 0, \lambda > 1$, and that $u(\cdot)$ is weakly concave in its inputs (thus, $\Delta u(y_j = 1) \geq \Delta u(y_j = k)$ for all $k > 0$). We also assume $r_j = 1$ for all j. Comparing the maximand in Equation (15) to Equation (14), the marginal welfare generated by the first y provided to any individual $j \notin \tilde{\mathcal{J}}$ increased by $\eta\lambda\Delta v(y_j = 1)$ and the marginal welfare generated by providing $k > 1$ to any individual $j \in \tilde{\mathcal{J}}$ increased by $\eta \Delta v(y_j = k)$.

There are two possible cases. First, $\tilde{Y}$ is already "flat," that is, $\max_{j \leq J}(\tilde{y}_j) = 1$. we show Y^* must also be flat. Second, $\tilde{Y}$ is not flat. Then, either (i) there exists a reallocation such that $\tilde{\mathcal{J}} \subsetneq \mathcal{J}^*$, or (ii) there is no reallocation and $\tilde{\mathcal{J}} = \mathcal{J}^*$.

Case 1. Consider reallocating $y = 1$ from j' to j, for $j', j \in \tilde{\mathcal{J}}$. Notice that the relative welfare value of providing a kth y to any individual $j \in \tilde{\mathcal{J}}$ relative to a first y to j' has decreased by $\eta \left(\lambda \Delta v(y_{j'} = 1) - \Delta v(y_j = k) \right)$, comparing the maximand in Equation (15) to Equation (14). Thus, if $\tilde{Y}$ is flat, then Y^* is also flat.

Case 2. Comparing the maximand in Equation (15) to Equation (14), the marginal welfare generated by providing the first good to each j' for whom $\tilde{y}_{j'} = 0$ has increased by $\eta\lambda\Delta v(y_{j'} = 1)$. Let $\underline{j} \equiv \arg\min_j \{\Delta v(\tilde{y}_j = k)\}$ for $k > 0$. That is, $\underline{j}$ is the individual to whom providing the marginal unit of y gives the least amount of social welfare, yet received at least one unit of y under $\tilde{Y}$. The marginal social welfare generated by providing the last good to $\underline{j}$ has increased by only $\eta \Delta v(y_{\underline{j}} = \tilde{y}_{\underline{j}})$. Thus, the welfare gain from flattening provision by allocating toward any $j' \notin \tilde{\mathcal{J}}$

has increased by $\eta\left(\lambda\Delta v(y_{j'}=1)-\Delta v(y_{\underline{j}}=\tilde{y}_{\underline{j}})\right)$. If that increase is sufficiently large relative to the initial gap in marginal social welfare, then Y^* will involve reallocating toward individuals in the loss domain and $\mathcal{J}^*$ will be strictly flatter than $\tilde{\mathcal{J}}$. Otherwise, the allocation does not change and is weakly flatter. □

D Experiment Details

D.1 Survey Recruitment

We recruited participants on Prolific. We advertised for an 18-minute survey entitled "Research Study," with $6 compensation. We restricted potential participants to fluent English speakers in the U.S.

D.2 Incentives

Rights Good Incentivization and Framing. We incentivize choices for rights goods in two ways. First, for Spectators who see lawyers, we use the strategy method. Second, we inform Spectators seeing both lawyers and health care that the nonprofit will be informed about participants' choices, which may impact their policies. Specifically, we inform Spectators doing experiments with lawyers:

> "Sometimes we will ask you what type of assistance to provide to tenants facing eviction. Please take these questions very seriously. Some participants will be randomly chosen to have their answers made in real life. **If you are chosen, your answers here will have significant impacts on the lives of real people**, so please take your time and respond truthfully."

We implement choices via a nonprofit partner in Memphis that works with tenants. Health care is not incentivized because it was not possible to implement. Instead, we tell these Spectators:

> "We will present you with a series of hypothetical scenarios and ask you what type of assistance to provide to tenants facing eviction. Please take these questions very seriously. A Memphis nonprofit who helps tenants facing eviction will be informed of participants' opinions on resource allocation."

Spectators then see contextual information about either lawyers[40] or health care[41], and then do

[40] We tell these Spectators: "Evictions often end up in court. In court, **tenants usually do not have lawyers**, because they are usually low-income and cannot afford them. **Landlords usually do have lawyers."** The government guarantees attorneys to anyone charged with a crime, but usually does not provide lawyers in civil settings, including eviction cases."

[41] We tell these Spectators: "Most low-income households in the United States say that they or a family member in their household delayed or went without some type of medical or dental care in the past year because they had difficulty affording the cost."

a comprehension check about that fact. We then introduce the types of assistance the nonprofit provides: either lawyers *or* health care depending on which good they see, bus passes, YMCA memberships, and cash.

Benchmark Incentivization. Participants doing experiments with YMCA memberships or bus passes are randomized into seeing lawyers or health care as an alternate assistance option that the nonprofit provides. Based on which framing they see, their incentives follow that of lawyers or health care. Half of Spectators who see the lawyers framing are randomized into hypothetical good valuation.

Incentivization Tests.

First, we test the effect of incentivization on WTP in our 3 main experiments among benchmark Spectators. There is no effect of incentivization on outcomes in inalienability, dignity of choice, or anti-targeting experiments (Table A13). Moreover, our main results looks similar using either only incentivized or unincentivized Spectators as the control group.

Our second test of incentivization is within incentivized participants. Half of Spectators seeing lawyers or benchmark goods are disincentivized in their choices about their WTP for the good. We inform selected participants:

> "At the beginning of the survey, we told you some choices will be randomly selected to be implemented for real tenants. In this section, all choices will be purely hypothetical. However, your choices are still important and the nonprofit will be informed of the results."

This is the last incentive-eligible section of the survey. We compare Spectators' valuations and find no difference by incentivization (Table A14).

Belief Elicitations. We further incentivize both belief elicitations that are in the survey. At the start of the survey, we inform only incentivized participants:

> "Sometimes we will ask you to predict what choices tenants have made or the impacts programs affecting tenants have had. Please take these questions very seriously. Some participants will be randomly chosen to be paid bonuses if the answers they give are close enough to the truth. **If you are chosen, your answers could increase your participation bonus**, so please take your time and respond truthfully."

First, in Experiment 2, we elicit prior beliefs and posterior beliefs after information about the percent of tenants expected to choose $y in cash over the good. All incentivized participants (seeing lawyers, or seeing YMCA or bus passes with lawyers framing) have this belief incentivized. Second, in Experiment 4, we elicit prior beliefs and posterior beliefs after information about the efficacy of lawyers or health care. Here, incentivization is only for Spectators who see experiments

about lawyers. In both modules, participants are informed: "Choose your responses carefully. You can earn bonuses for correct answers! You may request more details if you are curious about how the payment works." Interested participants saw that they would be enrolled in a lottery with a 10% chance of winning; for one up their upcoming predictions, they are compensated $1 in Spring 2024 if their answer is within 4 percentage points of the correct answer. We were not able to issue bonus payments at the time of survey because the accuracy of correct answers is determined by future events. In practice, all bonuses for selected participants are issued based on accuracy of prior beliefs, before participants were shown information.

D.3 Attention Checks

We include three attention checks. The first attention check asks participants to select a specific multiple choice option. The second two checks provide a list of cities and their populations, and ask participants to rank them from most to least populous.

Participants exit the survey if they fail two attention checks. 12 participants began our survey and were dropped because of this restriction (Table A2). 7% of our final sample of 1,800 participants failed only one attention check. 41 failed the first check, 52 failed the second check, and 36 failed the third check.

We have several additional comprehension checks throughout the survey, which cannot be the basis for dropping participants per Prolific policy. Participants are always informed of the correct answer after completing a comprehension check.

D.4 Experiment 1 Details

To elicit participants' WTP for inalienability, we set up a scenario where 2 eligible, comparably needy tenants are entered in a lottery for one available good. We explain that: "After the lottery takes place, but before the tenants are informed of the outcome, the nonprofit reserves the ability to rerun the lottery in some cases. Sometimes prices change and money can be saved by rerunning the lottery and assigning the [good] to whoever wins the second lottery." This sets up the choice between either (1) leaving the lottery results as they are or (2) taking the good away from the original winner, giving it to whoever wins the second lottery, and saving some amount of money for future programs. We then reinforce this setup by asking confirmation questions emphasizing, first, money is saved when the lottery is rerun, and second, the tenants will not know the original allocation if the lottery is rerun.

We first ask Spectators if they would prefer to keep the lottery results or rerun the lottery and save $20. We repeatedly ask Spectators the same question in increments of $20, until either they elect to rerun the lottery or they prefer to keep the results over rerunning and saving $200.

D.5 Experiment 2 Details

Information Treatment. We begin by eliciting beliefs about the percent of tenants expected to choose cash y over the good. The cash value is randomized $y \in \{\$200, \$300\}$. We ask participants to guess how many tenants, among 100 tenants who apply for assistance, would choose $\$y$ over the good. We then truthfully inform participants: "Researchers who work with the nonprofit asked 10 tenants whether they would choose a [good] or [$\$y$] in cash. All of them chose to receive [$\$y$] over a [good]." We ensured the truthfulness of this information treatment using additional Prolific experiments, where we screened for tenants and asked about their preferences for all combinations of goods and cash y values. At least 10 tenants for each combination preferred cash. After sharing this information, we elicit posterior beliefs.

Elicitation. Following the information treatment and posterior elicitation, we proceed with the elicitation of WTP for dignity of choice. We begin by informing tenants that the good typically costs \$350, and that the current budget of the nonprofit allocates y in cash to the tenant and saves the rest for future programs. We first ask Spectators if they would prefer to give the tenant $\$y$ and save \$100, or give the tenant the choice between $\$y$ and the good. We repeatedly ask Spectators the same question in increments of \$100, until either they elect to give $\$y$ and save or they prefer to give the tenant the choice over giving $\$y$ and saving \$900.

D.6 Experiment 2: Selection on Gains Details

We test for selection on gains by randomizing the value of the bundle $\$y \in \{200, 300\}$ and presenting Spectators' implied valuation of the good (Figure A2). Intuitively, randomizing $\$y$ identifies Spectators' supply curve for providing choice. If the distribution of the valuation of g does not vary with $\$y$, holding beliefs fixed, then that argues against substantial perceived selection on gains.

To see this, observe that:

$$E[\text{choice}|\$y = 200]_i := \int_{200}^{\infty} a(u(g|g \succ \$y))_i f(g|g \succ \$y)_i dy = C_i + 200 \tag{16}$$

$$E[\text{choice}|\$y = 300]_i := \int_{300}^{\infty} a(u(g|g \succ \$y))_i f(g|g \succ \$y)_i dy = C_i + 300. \tag{17}$$

In these expressions, $a(u(g|r \succ \$y))_i$ refers to i's allocative utility f from giving the recipient utility u from choosing g over y. The probability distribution function $f(\cdot)$ embeds the chance that i would choose g over y.

The key idea of our test is that fixing $f(\cdot)$,

$$E[\text{choice}|\$y = 300]_i - E[\text{choice}|\$y = 200]_i \equiv E[a(u(r|r \succ 300)) - a(u(r|r \succ 200))]_i. \tag{18}$$

This expression says that, holding constant beliefs about the probability of choosing g, the difference in the expected value of choice is equivalent to the difference in selection on gains.

We observe $f(\cdot)$ directly. Thus, we can consider the distribution of $C_i + \$y$ for beliefs within a small range of $f(\cdot)$.

We find no reason to be concerned about selection on gains (Figure A2). Kolmogorov-Smirnov tests do not permit us to reject equality, and indeed, the distribution of valuations for high $\$y$ lies *below* that for low $\$y$, which suggests selection on *losses*. Such selection would only amplify our results.

D.7 Experiment 4 Details

Information Treatment. Following the targeting elicitation (described in detail in Section 4.1, we randomly provide Spectators doing experiments with rights goods either a high or low information treatment about the instrumental benefit of the good. For lawyers, we elicit priors in two parts: first asking how many out of 100 tenants without a lawyer receive an eviction judgement, and second asking the same for tenants *with* lawyers. We then tell Spectators in the [high/low] treatments: "Researchers studied a program that is providing lawyers to tenants facing eviction in Memphis. Among 100 of the tenants, having a lawyer led to a [80/20]% reduction in eviction rates. About 55% of tenants who did not receive a lawyer from the program were evicted in court, but only about [15/45]% of tenants who did receive one were." These estimates are based on an ongoing RCT of providing lawyers to tenants facing eviction in Memphis, TN (Caspi and Rafkin, 2023).

For health care, we ask Spectators how many of 100 tenants without health care vouchers will have improved health outcomes 1 year later. We tell Spectators positive and null results from the Oregon Health Insurance Experiment (Baicker et al., 2013; Allen et al., 2013). In the high information treatment, Spectators see: "Researchers studied Medicaid expansion in Oregon and found that among people who newly gained access to Medicaid, rates of depression fell by 9 percentage points and increased the likelihood of self-reporting health as good, very good, or excellent by 13 percentage points." In the low information treatment, Spectators see: "Researchers studied Medicaid expansion in Oregon and found that among people who newly gained access to Medicaid, it did not have a significant effect on measured blood pressure or cholesterol."

Identifying Welfarists. Following the information, we give Spectators the opportunity to revise their targeting choices. We use participants' decision whether or not to revise their allocation when information conflicts with beliefs to identify welfarists. For lawyers, elicited priors are directly comparable with the information provided and we classify each participant as seeing information (80% or 20%) above or below their priors. We analogously classify health care participants, comparing their priors about improved health outcomes one year later to an 80% effect for the

"high" information and 20% for the "low" information. Therefore, welfarists are those who chose to revise their choices and either: initially distribute universally and see information below priors, initially distribute only to the poorest tenant and see information above priors, or initially distribute to the $R_i \in \{2, \ldots, 9\}$ poorest tenants. We cannot classify those who initially distribute universally and see information above priors or initially distribute only to the poorest tenant and see information below priors as either welfarist or non-welfarist.

D.8 Direct WTP Elicitation Details

To address the potential for anchoring, we randomize the initial choice participants see when eliciting their WTP directly for their assigned good. We ask if they would prefer to give one tenant the good or cash, randomizing the initial cash value from {\$300, \$500, \$700}. We ask the same question in increments of \$100 until we identify their indifference point.