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Law and Norms: Empirical Evidence[†]

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Abstract

A large theoretical literature argues laws exert a causal effect on norms, but empirical evidence remains scant. Using a novel identification strategy, this paper provides a clean empirical test of this proposition. We use incentivized vignette experiments to directly measure social norms relating to actions subject to legal thresholds. Our large-scale experiments featured around 5,800 subjects drawn from six samples recruited in the UK and China. Results show laws often, but not always, influence norms. Our findings are robust to different methods of measuring norms, and remain qualitatively similar across samples and between two countries with very different legislative environments.

Keywords: Social Norms; Law; Expressive Function of Law .

JEL Classification: C91, C92, D9, K1, K42

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1 Introduction

Legal rules play a vital role in the functioning of societies. Across all walks of life, laws regulate and constrain social behaviors, from the taxes individuals pay to governments, to the way they treat employees at work, or to the public health behaviors they are required to take during a pandemic. However, an emerging literature in behavioral economics shows that many behaviors are also influenced by informal rules of conduct that define what society perceives as socially appropriate or inappropriate. Unlike laws, these *social norms* are not formally codified or sustained by extrinsic reinforcements such as material penalties or fines, yet they are commonly recognized within a given society and informally enforced by means of social sanctions and rewards. Recent research has suggested that norms are an essential determinant of many of the social behaviors that are also regulated by law, such as the untruthful reporting of private information (e.g., Gneezy et al. 2018; Dufwenberg and Dufwenberg 2018; Abeler et al. 2019; Garbarino et al. 2019), tax evasion (e.g., Gërxhani 2004; Lefebvre et al. 2015; Dwenger et al. 2016), bribery and corruption (e.g., Fisman and Miguel 2007; Gneezy et al. 2019), or the expression of discriminatory behaviors or opinions (e.g., Barr et al. 2018; Bursztyn et al. 2020a).

What is the relationship between these two institutions – law and norms – that frequently regulate very similar types of behavior? Do they have independent influence on behavior, one by means of the deterrent power of material incentives, the other by the power of social incentives? Or are there interdependencies in the influence they exert on social behavior? And, more specifically, can lawmakers use the law to affect behavior, not just through the deterring power of incentives, but also through what has been labeled in the literature as *the expressive function of law* (Sunstein 1996), i.e., by shaping the underlying social norms of a society?

This paper presents clean empirical evidence on the causal influence of law on social norms. While this question has attracted the interest of many researchers from multiple disciplines in the last two decades, and a plethora of theoretical mechanisms have been proposed to explain how law may shape norms, the empirical evidence remains scant. This is mainly because the identification of the causal effects of law on norms presents a number of substantive challenges to empirical re-

search. First, for many years social scientists have been struggling to translate the concept of social norm into a measurable construct that can be used in empirical analysis. For this reason, previous empirical research has mostly been limited to studying the influence of legal rules either on *behavior* – arguing that the observed effects cannot be merely explained by deterrence and thus providing indirect evidence for the influence of law on norms (e.g., Funk 2007) – or on *personal opinions* (e.g., Chen and Yeh 2014; Aksoy et al. 2020) – a construct that is related to, but quite distinct from, social norms. In this paper, we exploit recent advancements in empirical research on social norms (e.g., Bicchieri and Xiao 2009; Krupka and Weber 2013; Bursztyn et al. 2020b) and design a series of vignette experiments that allow us to measure, directly and in an incentive-compatible way, the social norms pertaining to a variety of social behaviors. Through these measurements, we can cleanly observe the influence that law exerts directly on norms.

A second, pervasive challenge faced by empirical research in this area concerns the difficulty in establishing a clear direction of causality in the relationship between law and social norms. This is because law and norms co-evolve: they might influence one another and are often simultaneously co-determined by external factors, such as the availability of factual information about the harms of certain behaviors.[1] In this paper, we overcome this identification problem by exploiting a special subclass of laws that regulate behavior by means of *legal thresholds* defining the cut-off point above (or below) which a certain behavior becomes illegal (e.g., speed limit laws; drink drive laws; age of consent laws; etc.). We argue that it is reasonable to assume that, if a social norm exists that governs the same behavior also regulated by a legal threshold, this norm, absent the law, would not make sharp distinctions between behaviors that are arbitrarily close to the threshold (e.g., driving with a blood alcohol content (BAC) of 0.079% or 0.081%, when the limit is 0.08%), since these behaviors are virtually identical to each other in all respects except for their legal status. Thus, if we observe a discrete change in the perceived social appropriateness of behaviors that are just on

[1] Several scholars have argued that norms often precede the law and lead to its creation (e.g., Posner 1997; Chen and Yeh 2013). Indeed, some authors think that the law's effectiveness in regulating behavior crucially depends on whether it reflects the normative intuition of the community that it governs (e.g., Robinson 2000; Stuntz 2000; Acemoglu and Jackson 2017).

either side of a legal threshold, we can causally attribute this difference to the influence of law.[2]

We formalize these ideas in Section 2, where we adapt the theoretical framework proposed by Bénabou and Tirole (2006, 2011) to model a simple mechanism through which the law can induce sharp discontinuities in the perceived social appropriateness of legal and illegal behavior. In our model, social norms are functions that describe the social sanctions ("stigma") and social rewards ("esteem") that accrue to an individual for engaging in behavior that is observable by others. Individuals care about social norms as they gain positive utility from esteem and negative utility from stigma. Individuals also care about the negative externalities they impose on others, i.e. they are "prosocial". We show that, under standard assumptions and in the absence of a law that discriminates between legal and illegal behaviors, actions that produce very similar negative externalities (like driving with a BAC of 0.079% or 0.081%) attract very similar levels of social esteem.

Formalizing an argument previously proposed informally by Posner (1998, 2000, 2002), we then argue that laws introduce sharp payoff discontinuities between legal and illegal behaviors, even when these behaviors are virtually identical to one another. These payoff discontinuities occur both because laws assign material penalties to lawbreakers and because criminal offences are registered in criminal records. Criminal records allow an individual's (illegal) behavior to become known to a wider audience (e.g., future employers who did not directly observe the actions of the individual at the time he/she committed the crime), and this reduces the esteem resulting from illegal behavior.[3] Moreover, the esteem of illegal behavior is further reduced as audiences take into account that

[2] Direction of causality can also be readily established in the context of laboratory experiments, where the researcher tightly controls the decision environment and can introduce exogenous changes in the "rules" that govern behavior in the lab. A number of papers have studied the effects of such "lab laws" using experimental games. These studies show that requirements about specific actions mandated by the experimenter (for instance, a minimum contribution level in a public goods game, or a minimum admissible wage in a gift-exchange game) can affect behavior even if they are supported by weak, non-deterrent material sanctions, and that the effect can last even after the requirement has been lifted (e.g., Falk et al. 2006; Galbiati and Vertova 2008, 2014; Riedel and Schildberg-Hörisch 2013; d'Adda et al. 2017; Galbiati et al. 2018; Barron and Nurminen 2020; Engl et al. 2021; Govindan 2021). Differently from these studies, our paper does not focus on lab laws, but on the effects of laws that regulate behavior outside the lab. This circumvents the issue of external validity that is sometimes raised for experiments that focus on how individuals respond to the legal environment (e.g., Kelman 1985; Arlen and Talley 2008).

[3] A recent paper by Tirole (2021) builds on a similar intuition to analyze the effects of technologies that allow collection of personal data and that can thus widen the number of people able to observe an individual's actions.

someone who engages in criminal behavior is willing to do so despite the payoff discontinuities that exist between legal and illegal actions. Thus, an individual who "marginally" breaks the law (e.g., by driving with a BAC of 0.081%) does not just receive marginally lower esteem than someone who acts within the law (e.g., by driving with a BAC of 0.079%). We interpret this ability of the law to introduce discontinuities in social esteem between legal and illegal behaviors as a manifestation of its "expressive power", which we refer to as the ability of law to shape the social norms that prevail in a society.

In Section 3 we report an experiment designed to provide direct empirical evidence of the existence of discontinuities in our measurements of social norm functions in the presence of legal thresholds. In our vignette experiments, we asked subjects to evaluate the social appropriateness of various behaviors that are regulated by legal thresholds. We consider five types of legal thresholds, pertaining to: sexual activity with minors, the sale of alcohol to minors, undeclared cash imports into a country, drink driving, and speeding. Across several treatments, we presented subjects with slightly modified versions of the vignettes where we described behavior that is either legal or illegal, and either closer or further away from the legal threshold (for example, driving with a BAC of 0.001, 0.002, 0.003 or 0.004 percentage units above or below the legal limit). In each case, we used incentivized experimental techniques to measure the social norm pertaining to the behavior described in the vignette, and thus elicit a "normative function" that expresses the social appropriateness of behavior as a function of age, cash amount imported, blood alcohol content or speed, depending on the type of vignette. We measure the expressive effect of law on each norm by testing for the presence of a discontinuity in the corresponding normative function at the legal threshold.

We ran the experiments with 1,248 UK participants from across three samples: one student sample and two samples representative of the general population. We used two different methods to elicit social norms, using coordination games (Krupka and Weber 2013) as well as a sequential opinion matching method (e.g., Bicchieri and Xiao 2009; Bursztyn et al. 2020b). We report our results in Section 4. In all samples and across both methods, we find clear evidence of marked

discontinuities in the normative functions at the legal thresholds. However, we also observe differences in the expressive power of law across the five types of behavior we consider. In particular, we find strong effects of law on norms associated with having sexual relations with minors, selling alcohol to minors, and importing undeclared cash amounts into a country. We find instead weaker or no effects in the case of laws regulating drink driving and speeding behavior. We provide suggestive evidence that these heterogeneous effects are related to differences across the five domains of law in perceptions of the intentionality of illegal behavior and ability of law enforcement to detect it, which is consistent with the predictions of our model.

To probe the robustness of these results, in Section 5 we report three additional experiments that we ran with another 4,569 subjects where we: (i) conducted placebo tests introducing arbitrary thresholds unrelated to the law to measure whether they create similar discontinuities to the legal thresholds (they do not); (ii) collected more evidence on the proposed mechanism underlying the norm discontinuities by testing whether we also observe discontinuities directly in subjects' perceptions of the trustworthiness, honesty and altruism of someone who engages in legal or illegal behavior (we do); (iii) tested whether our findings generalize to a society (China) with relatively weak rule of law (they do).

Our paper contributes to an interdisciplinary literature, both theoretical and empirical, on the expressive function of law. In theoretical work, scholars have discussed a number of mechanisms to explain the source of the expressive power of law. A prominent approach proposes that the law can act as a public signal containing information that citizens use to update their beliefs about relevant features of the decision environment, such as the prevailing standards of behavior or the distribution of agents' preferences (e.g., McAdams 2000, 2015; Bénabou and Tirole 2011; van der Weele 2012). Another approach suggests that individuals may comply with a norm of legal obedience whereby they feel obliged to follow the law, and therefore automatically consider as appropriate the behaviors that are legal and as inappropriate those that are illegal (e.g., Cooter 1998, 2000; McAdams and Rasmusen 2007). Our paper builds on the theoretical framework introduced by Bénabou and Tirole (2011) to formalize an alternative mechanism through which the law can

exert expressive power. In our model the law conveys no information about the decision environment. Instead, by drawing a "line in the sand" between legal and illegal behaviors, the law allows individuals to reveal private information *about themselves*, based on how they behave in relation to it. This confers discontinuously different "social meaning" to behaviors that fall within or outside the confines of the law – which is the key intuition that drives our empirical strategy.

It is notoriously challenging to design empirical research that establishes a clear direction of causality from laws to norms. This explains why there are only a handful of empirical studies in this literature. Funk (2007), Wittlin (2011) and Rees-Jones and Rozema (2019) show that the law affects behavior beyond what one would expect based on the mere deterrent power of incentives, but cannot establish that these effects are actually mediated by shifts in the underlying social norms, since they do not measure them.[4] Three recent papers (Tankard and Paluck 2017; Casoria et al. 2020; Galbiati et al. 2020) measure directly the impact of law on social norms, exploiting changes in existing laws and, in the case of Casoria et al., using incentivized norm-elicitation techniques similar to those we use in this paper (Tankard and Paluck study the effects of a U.S. Supreme Court ruling in favor of same-sex marriage; Casoria et al. and Galbiati et al. exploit changes in COVID-19 public health regulations in France and the UK, respectively). As with all designs that rely on changes in existing laws for identification, the concern is that such legislative changes may take place simultaneously to other events which have the potential to shape norms (e.g., enhanced media debate of the relevant issues), hence casting doubt on whether changes in norms are necessarily caused by the changes in laws that precede them. The novel identification strategy we use in this paper rests on milder assumptions about causality and complements this existing work by providing first-hand empirical evidence that legal rules have causal power to shape normative intuitions about the behaviors that they regulate.

[4] Funk (2007) shows that the abolition of the legal duty to vote in 4 Swiss Cantons had a detrimental effect on voter turnouts, which is unlikely to be due to (lack of) deterrence since fines for not voting were very low (less than $1 in most cases). Wittlin (2011) shows that differences in seatbelt use across US states cannot be solely explained by state-level variations in penalties for not wearing a seatbelt, and that the enactment of a seatbelt law in one state has spillover effects on neighboring states. Rees-Jones and Rozema (2019) show that the effects of changes in the US cigarette tax law are mediated by the intensity of media coverage, lobbying efforts, and other activities related to the lawmaking process.

As we discuss in Section 6, our paper has broader implications for the body of literature investigating the interactions between formal and informal incentives (e.g., Gneezy and Rustichini 2000; Bénabou and Tirole 2003, 2006, 2011; Fehr and Rockenbach 2003; Fehr et al. 2007; Fehr and Schmidt 2007; Ariely et al. 2009; for recent reviews see Gneezy et al. 2011; Bowles and Polanía-Reyes 2012; Charness et al. 2020). Like our paper, this literature argues that formal incentives and institutions can affect behavior not merely by altering the material payoffs associated with actions, but also by influencing their social meaning. We provide clean and direct evidence that laws can strongly influence the social meaning of human activities by shaping the perception of what is right and wrong in a society.

2 Theoretical framework

2.1 Model

To fix ideas and substantiate our empirical analysis, we start by sketching a simple model of the expressive power of law. The model borrows from the existing literature on social image concerns (e.g., Bernheim 1994; Ellingsen and Johannesson 2008; Andreoni and Bernheim 2009) and in particular the "social esteem" framework proposed by Bénabou and Tirole (2006, 2011).[5]

Externality-generating opportunity. We consider an individual who is presented with a randomly drawn opportunity for material gain $o \in [o_{\min}, o_{\max}]$ that may however impose a negative externality on others. Opportunities differ in the severity of the negative externality $x(o)$ they generate upon being taken. The individual chooses a discrete action $a(o) \in \{0, 1\}$, where $a(o) = 1$ means that they take the opportunity o they are presented with, and $a(o) = 0$ otherwise.

In line with our empirical strategy, we focus on laws that regulate behavior by means of *legal thresholds* that define a clear cut-off point between legal and illegal behavior, such as speed laws, age of consent laws, laws against the sale of alcohol to minors, and so on. For instance, a shopkeeper may face the opportunity to make a material profit by selling alcohol to a young customer

[5] See also Adriani and Sonderegger (2019), Ali and Bénabou (2020) and Tirole (2021) for recent contributions that employ this framework.

who, depending on the draw of o, may be above or below the legal drinking age. The shopkeeper has to decide whether or not to take this opportunity. For concreteness, in the following we model legal thresholds by considering a law that introduces a threshold $\overline{o}$ above which seizing an opportunity becomes illegal.[6]

Utility function. The individual derives utility from material payoff, but also experiences a psychological cost from imposing negative externalities on others. Moreover, the individual also cares about the inferences that others make about the magnitude of the psychological cost he/she suffers for causing externalities. These inferences determine the social esteem that accrue to the individual. Formally, utility is given by:

$$u(o;\theta) = (t - pK\mathbb{I}_{o>\overline{o}} - \theta x(o))a(o) + E\left(\theta \mid \Omega\right) \tag{1}$$

When the individual takes an opportunity o, he/she receives a material gain t, and, if the taken opportunity exceeds the legal threshold (i.e., $o > \overline{o}$), he/she also faces a material penalty $K \geq 0$ with probability $p \in (0, 1]$, the probability of being caught. The individual suffers a psychological cost of size $\theta x(o)$ for imposing externality $x(o)$ on others, where θ is his/her (privately known) type and measures the extent to which the individual cares about causing negative externalities – this subsumes a host of possible prosocial characteristics of the individual, from trustworthiness, to altruism, to honesty. Types are drawn from a distribution with continuous differentiable density $f(.)$ with mean μ_θ and full support $[\theta_{\text{min}}, \theta_{\text{max}}]$, where $\theta_{\text{min}} \geq 0$. An individual characterized by a higher type suffers a higher psychological cost for imposing a negative externality. The individual also cares about *social norms*: utility depends on the social rewards ("esteem") and sanctions ("stigma") that are associated with seizing or leaving an opportunity. This is represented by the last term in (1), measuring the inferences that other people ("observers") make about the individual's type θ upon observing a social signal Ω about the individual's behavior. Similar to Tirole (2021), we consider two types of observers who have access to different information.

[6] It is straightforward to accommodate the case where the law introduces a threshold $\underline{o}$ *below* which seizing an opportunity is illegal, through a simple relabeling exercise.

Close observers. A first class of observers consists of agents who are located in an individual's immediate social network. These agents can perfectly observe the individual's choice as well as the precise characteristics of the opportunity the individual is faced with, regardless of whether an opportunity is legal or illegal. As a result, $\Omega^{close} = \{o, a\}$. The esteem obtained from socially close observers is:

$$S^{close}(o) = \begin{cases} \gamma(o) \equiv E(\theta \mid a(o) = 1) & \text{if individual takes opportunity } o \\ \delta(o) \equiv E(\theta \mid a(o) = 0) & \text{if individual leaves opportunity } o \end{cases}$$

Distant observers. Second, individuals are also concerned about their esteem within "society at large", namely socially distant people who cannot directly observe an individual's action, but can observe his/her *criminal record*, which acts as a public signal about whether the individual has been convicted for seizing an illegal opportunity.[7] This captures the idea, discussed in the legal literature (e.g., Posner 1998, 2000, 2002), that one of the key functions of law, in addition to introducing incentives to deter criminal behavior, is to enhance the visibility of (illegal) behavior within society at large, beyond the circle of people who would normally be able to directly observe an individual's actions.[8]

If an individual is caught seizing an illegal opportunity, the nature of the infraction becomes observable to distant observers, so that $\Omega^{distant} = \{o, a\}$. As a result, the social esteem he/she obtains from distant observers is the same as that obtained from close observers, $\gamma(o)$. If the individual has no criminal conviction, either because he/she has not broken the law or because he/she escaped conviction, $\Omega^{distant} = \emptyset$, and the individual receives esteem $\Psi > \mu_\theta$ from distant observers. This is the expected type conditional on no conviction, and is computed considering all the (infinitely many) possible contexts in which the individual may have been presented with an externality-generating opportunity. Within a given context, Ψ can therefore be taken as exogenous.[9]

[7] For simplicity, we assume that people care equally about the esteem of close and distant observers, but our results can be straightforwardly generalized to allow for different weights.

[8] See also Tirole (2021) for a related approach where individual's behavior can become known to an audience either via direct observation or via publicly disclosed "social scores" that contain information about the individual's behavior in multiple contexts that are not directly observed by the audience.

[9] This is not necessary for the result, but considerably simplifies the analysis. More formally, let the *context* of the opportunity faced by the individual be represented by a real number c drawn from an atomless distribution on the

The social esteem received from distant observers is thus given by:

$$S^{distant}(o) = \begin{cases} \gamma(o) & \text{if the individual is caught taking opportunity } o > \overline{o} \text{ (illegal)} \\ \Psi & \text{otherwise.} \end{cases}$$

The social norm function. The *total* social esteem $S(o)$ an individual receives from society for taking opportunity o is simply the sum of the esteem derived from close and distant observers, $S(o) = S^{close}(o) + S^{distant}(o)$. We can think of it as the "global" social norm that governs the social standing of an individual who engages in an externality-generating activity o.

To sum up, when an individual of type θ faced with opportunity o decides *not* to take the opportunity, his/her utility is:

$$u_{a(o)=0}(o;\theta) = \delta(o) + \Psi$$

independent of θ. If the individual chooses instead to take the opportunity, expected utility is:

$$u_{a(o)=1}(o;\theta) = \begin{cases} t - \theta x(o) + \gamma(o) + \Psi & \text{if } o \leq \overline{o} \text{ (legal)} \\ t - \theta x(o) - pK + \gamma(o)(1+p) + (1-p)\Psi & \text{if } o > \overline{o} \text{ (illegal)} \end{cases}$$

Before proceeding with the analysis, we introduce the following assumption:

Assumption 1 $x(o)$ *is everywhere differentiable.*

Assumption 1 ensures that $x(o)$ is continuous, so that the externality levels generated by seizing very similar opportunities (e.g., selling alcohol to a young customer who is $o - \varepsilon$ years old or $o + \varepsilon$ years old, for ε small) are very close to one another. We think that this assumption applies naturally to the set of decision situations we consider in the empirical part of the paper. The assumption may not naturally extend, however, to other types of situations where, for instance, there may exist discontinuous externalities even between marginally similar actions (e.g., between causing no harm or some harm to an innocent bystander). To minimize notation, without loss of generality we further

unit interval [0,1]. For instance the individual may face a context where he/she has the opportunity to sell alcohol to a minor, the opportunity to drink-drive, and so on. The individual is first randomly assigned to c, and then is presented with opportunity o_c drawn from a context-specific distribution function $g_c(.)$. In the absence of a criminal conviction, distant observers – who cannot observe anything about the context or the opportunity faced by the individual – will assign esteem to the individual taking into account all possible contexts c. Therefore, in any specific context, Ψ can be taken as exogenous. Moreover, $\Psi > \mu_\theta$ follows from the fact that, whenever an individual breaks the law, he/she is caught with positive probability.

impose:

Assumption 1(i) *The externality generated by seizing opportunity o is given by $x(o) = o$.*

2.2 Analysis

We focus on equilibria where opportunities that generate stronger negative externalities are seized by less prosocial types (monotonicity). We also restrict attention to interior solutions.[10] Our equilibrium concept is Perfect Bayesian Equilibrium.

We consider two cases. First, we analyze the social esteem that accrues in equilibrium to an individual who takes opportunity o in the benchmark case where there is no law setting a cut-off point beyond which opportunities become illegal. This analysis characterizes the social norm function that governs behavior in the absence of law. Then, we analyze how introducing a law prohibiting opportunities $o > \overline{o}$ affects the shape of the social norm function. The proofs of the results presented in this section can be found in the Online Supplementary Materials (OSM A).

2.2.1 Social esteem in the absence of law

If there is no law prohibiting opportunities $o > \overline{o}$, then $K = 0$, $p = 0$, and $S^{distant} = \Psi$ regardless of whether the individual takes an opportunity or not. Taking the esteem from close observers as given, the *net* utility from seizing the opportunity is given by $t - \theta o + \gamma(o) - \delta(o)$, decreasing in θ. For each opportunity o, we can therefore identify the highest θ who takes o. Denote this as $\widehat{\theta}_o$. In equilibrium,

$$\gamma(o) = E(\theta \mid \theta < \widehat{\theta}_o) \text{ and } \delta(o) = E(\theta \mid \theta > \widehat{\theta}_o) \tag{2}$$

Let $\mathcal{M}^-(x) \equiv E(\theta \mid \theta < x)$, $\mathcal{M}^+(x) \equiv E(\theta \mid \theta > x)$ and $\Delta(x) \equiv \mathcal{M}^+(x) - \mathcal{M}^-(x)$. We have:

Proposition 2.1. *In the absence of a law prohibiting $o > \overline{o}$, the social esteem from close observers when the individual takes opportunity o is continuous and is given by $\gamma(o) = \mathcal{M}^-(\widehat{\theta}_o)$, where $\widehat{\theta}_o$,*

[10] To guarantee monotonicity, we assume, for all θ, $o_{\min} \geq -\Delta'(\theta) + \mathcal{M}^{-\prime}(\theta)$. This condition ensures that the threshold types $\widehat{\theta}_o$ and $\widetilde{\theta}_o$ (discussed in the next subsections, 2.2.1 and 2.2.2) are continuous and strictly decreasing in o. To rule out corner solutions, we assume $t - o_{\max}\theta_{\min} - \mu_\theta + \theta_{\min} > 0 > t - o_{\min}\theta_{\max} + \mu_\theta - \theta_{\max}$.

the highest type seizing o, satisfies the indifference condition:

$$t - \widehat{\theta}_o o - \Delta(\widehat{\theta}_o) = 0. \tag{3}$$

Under our assumptions the solution of (3) is interior and $\widehat{\theta}_o$ and $\gamma(o)$ are continuously strictly decreasing in o

The following corollary characterizes the total social esteem from seizing an opportunity o.

Corollary 2.1.1. *In the absence of a law prohibiting $o > \overline{o}$, the total social esteem from seizing opportunity o is continuous and given by*

$$S(o) = \mathcal{M}^{-}(\widehat{\theta}_o) + \Psi$$

Proposition 2.1 and Corollary 2.1.1 characterize the social norm function in the absence of law. The key take-away point from this analysis is that, when there are no laws setting a limit on allowed opportunities, the esteem that accrues to an individual for seizing an opportunity is *continuously* decreasing in the magnitude of the negative externality imposed on others. This formalizes the key identifying assumption we will make in the empirical part of the paper, where we will argue that, in the context of the behaviors we consider, absent the law, norms do not make sharp distinctions between behaviors that are arbitrarily close to one another. Formally, the continuity result relies on two features. First, we are focusing on environments where there are no discontinuities in externality-generation, as captured by Assumption 1. Second, the distribution of individual types is continuous with full support. This is a standard assumption which rules out, for instance, that the distribution of types may contain "holes".[11] The next section analyzes how the introduction of law affects the shape of the norm function.

[11] If this was not the case, then the expected type of an individual who seizes opportunity $o - \varepsilon$ could in principle be substantially different from the expected type of an individual who seizes $o + \varepsilon$, even when ε is arbitrarily small (and there is no law prohibiting $o > \overline{o}$).

2.2.2 Social esteem in the presence of law

Suppose now that, for some $\overline{o} \in (o_{\min}, o_{\max})$, an individual who takes opportunities $o > \overline{o}$, breaks the law. Then, with probability $p \in (0, 1]$ the individual is caught and incurs a material penalty $K \geq 0$. Moreover, if the individual is caught breaking the law, this is recorded in his/her criminal record, which distant observers can then observe and use to lower the esteem they confer to the individual. These costs associated with seizing $o > \overline{o}$ have a spillover effect on the esteem the individual gets from close observers. To see why, consider two opportunities, $\overline{o}-\varepsilon$ and $\overline{o}+\varepsilon$, where ε is vanishingly small. Even if the esteem awarded by close observers from seizing these two opportunities was the same, the expected return from seizing $\overline{o} + \varepsilon$ would nonetheless be substantially smaller, due to the additional costs incurred if the individual is caught breaking the law (in the form of the material penalty and the lower esteem from distant observers). An individual who is willing to take a marginally illegal opportunity $\overline{o}+\varepsilon$ is thus not simply a marginally "worse" type, on average, than an individual who seizes $\overline{o} - \varepsilon$. Since the individual is willing to seize an opportunity associated with a substantially smaller return, his/her type must be, on average, substantially lower. Close observers recognize this and take it into account when forming their beliefs about the individual's type. As a result, seizing $\overline{o} + \varepsilon$ carries significantly lower esteem from close observers than seizing $\overline{o} - \varepsilon$, despite the fact that these two opportunities generate very similar externalities. Formally, the social esteem function from close observers experiences a discontinuity at $\overline{o}$, the legal limit.

Proposition 2.2. *In the presence of a law prohibiting $o > \overline{o}$, the social esteem from close observers when the individual takes opportunity o is*

$$\gamma(o) = \begin{cases} \mathcal{M}^{-}(\widehat{\theta}_o) \text{ if } o \leq \overline{o} \\ \mathcal{M}^{-}(\widetilde{\theta}_o) \text{ if } o > \overline{o} \end{cases} \tag{4}$$

where $\widehat{\theta}_o$, the highest type seizing $o \leq \overline{o}$, is defined by (3):

$$t - \widehat{\theta}_o o - \Delta(\widehat{\theta}_o) = 0$$

while $\widetilde{\theta}_o$, the highest type seizing $o > \overline{o}$, is defined (when interior) by

$$t - h(\widetilde{\theta}_o) - \widetilde{\theta}_o o - \Delta(\widetilde{\theta}_o) = 0 \tag{5}$$

for $h(\widetilde{\theta}_o) \equiv p\left[K + \Psi - \mathcal{M}^-(\widetilde{\theta}_o)\right] > 0$. The function $\gamma(o)$ exhibits a downward discontinuity at $\overline{o}$:

$$\lim_{\varepsilon \to 0} \left[\gamma(\overline{o} - \varepsilon) - \gamma(\overline{o} + \varepsilon)\right] = \mathcal{M}^-(\widehat{\theta}_{\overline{o}}) - \mathcal{M}^-(\widetilde{\theta}_{\overline{o}}).$$

The key observation here is that, as proved in OSM A, $\widetilde{\theta}_o$ always lies strictly below $\widehat{\theta}_o$. As a result, the law generates a downward discontinuity in the esteem function $\gamma(o)$ at $\overline{o}$. The following corollary characterizes expected total social esteem.[12]

Corollary 2.2.1. *In the presence of a law prohibiting $o > \overline{o}$, the expected total social esteem from seizing opportunity o is*

$$S(o) = \begin{cases} \mathcal{M}^-(\widehat{\theta}_o) + \Psi & \text{if } o \leq \overline{o} \\ \mathcal{M}^-(\widetilde{\theta}_o)(1+p) + (1-p)\Psi & \text{if } o > \overline{o} \end{cases}$$

The function $S(o)$ exhibits a downward discontinuity at $\overline{o}$:

$$\lim_{\varepsilon \to 0} \left[S(\overline{o} - \varepsilon) - S(\overline{o} + \varepsilon)\right] = \mathcal{M}^-(\widehat{\theta}_{\overline{o}}) - \mathcal{M}^-(\widetilde{\theta}_{\overline{o}}) + p[\Psi - \mathcal{M}^-(\widetilde{\theta}_{\overline{o}})] > 0. \tag{6}$$

Proposition 2.2 and Corollary 2.2.1 contain the key result of our theoretical analysis. While in the absence of law the social esteem function decreases continuously in the magnitude of the externality imposed on others, the introduction of the law introduces a sharp discontinuity in the function at the legal threshold $\overline{o}$. We interpret this as a manifestation of "the expressive power of law": laws shape the social norms that prevail within a society by creating sharp discontinuities in the social rewards and sanctions that accrue to individuals for taking actions that are legal or illegal.

In OSM A we present a number of extensions of our simple model to probe the robustness of our result as well as to study its properties. First, we show that the result also obtains in a more

[12] We talk about expected (or average) total social esteem because illegal behavior is observed by distant observers only with probability p.

standard, Spence-like model where the individual directly chooses the level of externality (instead of taking the binary decision of taking or leaving an opportunity that is exogenously presented to him/her). This is relevant because, while some of the empirical analysis below will focus on cases that are in line with our model of opportunities (e.g., the case of a shopkeeper selling alcohol to a young customer), other cases may be better captured by a model where individuals directly choose the externality (e.g., the case of a person deciding how fast to drive on a motorway).

Second, we show that there is no mechanical relation between the size of the discontinuity at the legal threshold and the prevalence of law breaking (what the social psychology literature calls the "descriptive norm", e.g., Cialdini et al. 1990). It is not the case that laws have expressive power (i.e., generate larger discontinuities) only when they manage to reduce the number of people willing to break the law.

Finally, we examine a series of factors that may affect the size of the discontinuity at the legal threshold. We show that the level of *tolerance* adopted by law enforcement towards law violations (in terms of both strength of the material sanctions and conviction probability) is negatively related to the expressive power of law. We also show that the size of the discontinuity is negatively related to the extent to which law violations may be accidental rather than *intentional*, and to whether illegal behavior can only be measured with a *margin of error*. Both factors weaken the link between existence of a criminal record and esteem from distant observers, which in turn increases the equilibrium value of $\widetilde{\theta}_o$, the highest type willing to take an illegal opportunity. These results are useful as they will guide our interpretation of the differences in expressive power of law that we observe in our empirical analysis across situations characterized by stronger/weaker perceived tolerance, measurement error and intentionality of behavior.

3 Empirical strategy: Main experiment

Our empirical strategy mirrors the analysis of the previous section. We take a novel empirical approach by focusing on a special subset of laws that regulate behavior by setting thresholds to distinguish between legal and illegal actions, such as laws defining the minimum legal age for

the sale of alcohol, the minimum legal age for sexual activities, the maximum speed at which one is allowed to drive, etc.. Focusing on this subset of laws allows us to rely on reasonably mild assumptions to resolve problems of causal identification that are otherwise pervasive in the empirical literature on the topic. While it is, for example, difficult to defend the assumption that the enactment of a law banning the sale of alcohol to minors is independent from pre-existing normative considerations about the appropriateness of consumption of alcoholic beverages by young people, we argue that a much less demanding assumption is that such a norm is unlikely to make a priori sharp distinctions between behaviors that are in all respects very similar to each other. For instance, in the absence of any pre-existing drinking age limit, it is unlikely that a norm would sharply distinguish between selling alcohol to a customer who is 18 years and 1 month old instead of 17 years and 11 months old, such that this would inform the lawmaker's decision to position the legal threshold exactly at 18 years. This is because all factors that may matter for appropriateness judgments (e.g., how harmful it is to drink alcohol at those ages) do not vary sharply across close age groups. In terms of our theoretical framework of Section 2, this corresponds to Assumption 1, namely that opportunities that are similar to one another are characterized by small differences in the magnitude of the externality they imply. If this assumption is valid, one can then consider the existence of a sharp discontinuity in the underlying norm exactly at 18 years as causally determined by the existence of a legal limit at that age.

Our reasoning here is similar to the arguments used to support the local randomization assumption in regression discontinuity designs. As in those designs, we are assuming that the "outcome" variable – in our case, the esteem function $S(.)$ defined in Section 2 – is continuous in the vicinity of the legal threshold, absent an expressive power of the law. If so, we can identify the causal effect of the law on norms by testing for a discontinuity in $S(.)$ at the legal threshold.[13]

More precisely, in our experiment we will use incentivized norm-elicitation procedures, described below, to measure the social appropriateness of a series of actions that vary in the distance

[13] One difference between our design and regression discontinuity designs is that, as we explain below, we obtain measurements of the outcome variable $S(.)$ from individuals who are *randomly assigned* (by us) to either side of the legal threshold. Thus, we do not have to worry about potential manipulations of the "assignment" variable on the part of subjects, which is instead a major concern in regression discontinuity designs.

from a legal threshold $\overline{o}$ (for instance, the appropriateness of selling alcohol to a person aged 17 years and 10 months, 17 years and 11 months, 18 years and 1 month, 18 years and 2 months, when the law prohibits the sale of alcohol to those under 18 years of age). We use the measurements of appropriateness for behavior that stays within the law to estimate the esteem associated with legal behavior, $S(o \mid o \leq \overline{o})$, while we use the measurements of appropriateness for behavior that violates the law to estimate $S(o \mid o > \overline{o})$. Under the assumption that, absent the law, the function $S(o)$ is continuous in o (Corollary 2.1.1), we identify the causal effect of the law on the social norm by estimating:

$$\Big(S(o \mid o \leq \overline{o}) - S(o \mid o > \overline{o}) \mid o = \overline{o}\Big) = lim_{\varepsilon \to 0}\Big[S(o) \mid o = \overline{o} - \varepsilon\Big] - lim_{\varepsilon \to 0}\Big[S(o) \mid o = \overline{o} + \varepsilon\Big] \tag{7}$$

Note that in our empirical analysis we will estimate the "social esteem" that accrues to an individual for taking a certain action by measuring whether that action is perceived as "socially appropriate" or "socially inappropriate". This is in line with the empirical literature on social norm compliance, where shared judgments of appropriateness are used to measure the social approval and disapproval associated with given behavior (e.g., Krupka and Weber 2013; Bicchieri 2016; Görges and Nosenzo 2020). In Section 5, we will report the results of an additional experiment where we use an alternative approach to elicit social esteem that corresponds more closely to the theoretical model of Section 2 (we measure directly the inferences from actions to prosocial traits).

Also, it is worth pointing out that legal thresholds may exert their expressive power on norms in ways that go beyond the discontinuity effects we set out to identify in our empirical analysis. For instance, laws may affect the whole shape of the $S(.)$ function, by changing the way actions that are further away from the legal threshold are evaluated. For example, in many countries people consider it acceptable to speed up to a certain distance from a speeding threshold – for instance, they might consider speeds up to 74mph appropriate if the speed limit is 70mph, but if the limit was reduced to 65mph they may then consider 74mph inappropriate. Our empirical strategy is designed to measure the effect of law exactly at the legal threshold and therefore does not capture

these additional expressive effects of the law that may nevertheless be empirically relevant.

3.1 Experimental Design

In order to directly measure the effect an action's legality has on the social norm pertaining to it, we designed an experiment using *vignettes*. We presented subjects with a series of hypothetical scenarios describing a person behaving in a certain way, and in each vignette elicited subjects' evaluations of the social appropriateness of the person's behavior. We used five vignettes describing situations where the legality of a particular behavior is determined by which side of a legal threshold the behavior falls on. We considered five different types of legal thresholds, concerning: (i) the age of consent; (ii) the legal drinking age; (iii) the maximum amount of cash which it is legal to import into one's country without declaring to customs; (iv) the blood-alcohol content drink driving limit; and (v) the legal speed limit for driving on a motorway.

Each of our five vignettes described the behavior of a person engaged in a situation that involved one of these legal thresholds. The age of consent vignette described the situation of an adult engaging in sexual activity with a younger person that he had met at a party. The alcohol to youth vignette described a shopkeeper selling alcohol to a youth who is known to be a local vandal. In the cash at customs vignette, a person was returning from abroad with a cash amount that he did not declare at customs. In the drink driving vignette, a woman was driving home after drinking on a night out. Finally, the speeding vignette described a woman driving on a motorway. The vignettes are reproduced in the Supplementary Materials (OSM B).

We designed these five vignettes to achieve variation in the severity of the illegal behavior as well as in the extent to which behavior, even if legal, would be deemed socially appropriate. For instance, in the alcohol to youth vignette, we described the young customer as a local vandal in order to reduce the appropriateness of selling him alcohol even when he was legally allowed to buy it.[14] We also chose situations that differed in relevant features of law enforcement, for instance the

[14]Moreover, in this specific vignette we focused on the evaluation of the shopkeeper's behavior, rather than the customer, as we thought this better captures the notion of taking opportunities for material gain that may be harmful to others.

ability to monitor or accurately detect whether a behavior exceeds the legal threshold or not, which we will exploit to shed light on the possible mechanisms that underlie the effects we observe in our experiment

In all cases, we made it clear that the person in the vignette knew what the legal threshold was and could verify which side of the threshold their own behavior would fall on. For example, in the age of consent vignette, the adult checks the younger person's ID card in order to verify whether she is above the age of consent. We deemed this important for two reasons. First, we wanted to subtly remind (or inform) our subjects about the existing legal rules that were relevant for each situation. Second, we wanted our subjects to evaluate the behavior of a person who was knowingly following or breaking the law, so as to remove any ambiguity about a potential "ignorance of the law", which may have affected judgments of appropriateness.

For each situation, we designed 8 (or 4, depending on the sample – see below) different versions of the vignette, which differed only in that they described behaviors falling on either side of the legal threshold and at different distances from it. This included behaviors that were only just legal or only just illegal, so as to measure the appropriateness of actions that were virtually identical in all respects other than their legal status. For instance, for the age of consent situation, we designed versions of the vignette where the younger person was 1, 2, 3, or 4 months above the age of consent, and versions where she was 1, 2, 3, or 4 months below the age of consent.

The different versions of the vignettes were administered according to a between-subject design, so that each subject evaluated the appropriateness of only one behavior per situation. For example, some subjects were (only) described the vignette where the younger person was 1 month above the age of consent, others were (only) described the vignette where she was 2 months above; etc. These between-subject measurements of appropriateness allow us to obtain, for each situation, an estimate of the norm function $S(.)$ that regulates behavior in a neighborhood around the relevant legal threshold. Our identification strategy consists of testing, for each of the five vignettes, whether there is a discontinuity in the norm function $S(.)$ at the corresponding legal threshold.

Our experiments also included ten additional filler vignettes, which, along with the five legal

threshold vignettes that are the focus of the study, were presented in random order (except that the first three vignettes subjects saw were always fillers, for reasons explained in footnote 17). These additional vignettes were included in order to avoid it becoming salient to subjects that we were interested in the evaluation of behaviors regulated by a legal threshold. Thus, the filler vignettes featured a variety of types of behaviors that were either unregulated by law (e.g., a person refusing to give money to a beggar) or that were regulated by law but not by means of legal thresholds (e.g., a person leaving a restaurant without paying the bill). The filler vignettes were not subject to manipulation (i.e. we did not prepare different versions of them), so each one was identical for every subject.

3.2 Incentivization

Subjects received monetary incentives to evaluate the social appropriateness of the behavior described in the vignettes. We used two different types of incentive schemes, corresponding to the two most popular methods used in the empirical literature to elicit social norms. One procedure has been proposed by Krupka and Weber (2013) and we will refer to it as the "Krupka-Weber method". The other procedure has been used in different guises by multiple authors, e.g., Bicchieri and Xiao (2009), d'Adda et al. (2020), Bursztyn et al. (2020b), and Bicchieri et al. (2021). For reasons that will become clear below, we will refer to it as the "opinion matching method".

In the Krupka-Weber method, subjects were required to indicate for each vignette how socially appropriate they thought the described behavior was by selecting one option on a four-point ordered scale: "Very socially appropriate", "Somewhat socially appropriate", "Somewhat socially inappropriate", or "Very socially inappropriate". They were paid a bonus payment in addition to their participation fee only if their evaluation of the behavior in a vignette was the same as that chosen by the most other subjects in the same version of the experiment; otherwise, they were only paid the participation fee.

In the opinion matching method, subjects were randomly assigned to one of two different conditions (between-subject design). In one condition, which was run first, subjects were asked to report

their personal belief of how appropriate the behavior described in the vignette was. Responses, which were not incentivized, were indicated on a four-point scale, as above, but in this case we used the terms "appropriate/inappropriate" rather than "socially appropriate/inappropriate".[15] In the second condition, subjects were asked to guess the most common appropriateness judgment among the first group. These respondents were probabilistically paid a bonus payment in addition to their participation fee if their guess was correct, and were only paid their participation fee otherwise.

Both methods reward subjects for accurately reporting their perception of how appropriate a particular behavior is *commonly* regarded (i.e., second-order beliefs of appropriateness), rather than their own personal evaluation of the behavior.[16] This is important since social norms reflect opinions about what is *collectively approved or disapproved of within a society*, rather than personal opinions about appropriate behaviors (for a discussion of the difference between personal opinions and social norms, see Bicchieri 2006; Krupka and Weber 2013).

The Krupka-Weber method achieves this by transforming the task into a coordination game in which subjects are incentivized to rate behavior in the same way as other participants who are simultaneously taking part in the same experiment. The rationale for this incentive scheme is that, if a norm exists regarding the behavior being evaluated, then this constitutes a particularly salient focal point in the task that subjects can use to successfully coordinate. This being the case, subjects' evaluations of the behavior in the vignette indirectly reveal the underlying social norm pertaining

[15] In each case, we provided subjects with lengthy explanations as to how they should understand these terms. See OSM C for full details. For social appropriateness, the explanation began: "*By socially appropriate, we mean behaviour that you think most people would agree is the "right" thing to do. Another way to think about what we mean is that if someone were to behave in a socially inappropriate way, then other people might be angry at them*". For appropriateness, the wording was similar but dropped "*most people would agree*" and replaced "*other people*" with "*you*".

[16] The opinion matching method also delivers a measure of personal norms (i.e., first-order beliefs of appropriateness) from the first group of participants whose responses are not incentivized. A potential criticism of the Krupka-Weber method is that, conceptually, it is not clear whether it measures second-order or higher-order beliefs of appropriateness. The instructions ask subjects to report their perception of the social norm (i.e., second-order beliefs about what most others think is appropriate). A subject who wants to coordinate with others may however have to form *third-order* beliefs of appropriateness (beliefs about others' second-order beliefs). The logic can be iterated further, e.g., a subject may think that others use third-order beliefs to complete the task, and therefore use *fourth-order* beliefs to coordinate, etc.. However, a counterargument to this criticism is that second-order beliefs are the most salient high-order belief in the task because the instructions are heavily framed in the language of social norms, and, if subject use salient focal points to coordinate (Schelling 1960), they may indeed report their second-order beliefs in the experiment.

to that behavior.

However, one concern with this approach is that in principle subjects may use any other salient focal points – and not the norm – to coordinate, in which case subjects' responses would not be revealing the underlying social norm as intended. In our design this concern may be particularly relevant because our vignettes explicitly indicate what constitutes legal behavior. Legality in itself could be used as a focal principle for coordination. That is, subjects may use the following strategy to coordinate with others: rate actions that are legal as "appropriate" and actions that are illegal as "inappropriate", regardless of whether this is what the underlying social norm truly prescribes. Note that this alternate coordination strategy would also give us a discontinuity at the threshold – albeit for the wrong reason.

To minimize this concern, our experiment was explicitly designed to emphasize the distinction between the concepts of "social appropriateness" and "legality" and to increase the saliency of the former over the latter.[17] Moreover, Section 5 reports further experiments that test the empirical relevance of this issue in our setting, largely confirming that the method is robust to the use of alternate coordination points.

The opinion matching method sidesteps this concern altogether, since subjects were incentivized to guess the most prevalent opinion among a group of other subjects who had already completed the experiment. By removing the strategic component that is present in the Krupka-Weber coordination game, the opinion matching method eliminates the potential for measurement distortions due to the use of alternate coordination strategies that are extraneous to the social norm. However, a possible concern with this method is that subjects are incentivized to match the *unin-*

[17] We included two features of the design to achieve this. First, in the experimental instructions we explicitly told subjects that what constitutes appropriate behavior "... *may not necessarily be made explicit or supported by laws, nor enforced by the threat of legal sanctions. So an action may be 'appropriate' even if it is not legal; or 'inappropriate' even if it is not illegal.*". The inclusion of this passage in the instructions aims to reduce the incentive for subjects to use legality as a coordination device, since it breaks the cycle of beliefs that may support it as a successful coordination strategy (subjects should now be doubtful that others may use legality to coordinate given that they are explicitly told not to do so). Second, the first three vignettes that subjects evaluated in the experiment were always filler vignettes explicitly designed to train subjects to think of social appropriateness as a concept that is distinct from legality. In these vignettes, subjects were described behavior which was unlikely to be considered very inappropriate, but that in one case was regulated by law and legal (a person deciding not to illegally download a movie), in another it was regulated by law and illegal (a person driving very slowly and safely without wearing a seatbelt), and in the third case it was not regulated by law (a person choosing between booking a holiday and giving money to charity).

centivized responses of another group of respondents. If these first-step responses are vulnerable to noise or responding biases, second-step responses may reproduce the same effects.

Given that either method has advantages and disadvantages, in this paper we report norm elicitations based on both empirical procedures. Subjects were randomly assigned to one procedure only (between-subject design). Moreover, if they were assigned to the opinion matching method, they either participated in the non-incentivized or incentivized condition.

3.3 Samples and procedures

Our main experiment was run between September 2017 and March 2021 with a total of 1,248 participants separately recruited in three different UK samples. We used one student sample and two samples of the UK general population. Table 1 summarizes the different samples used in the main experiment.

Table 1: Samples used in the main experiment.

	N	Nationality	Year	Subject Pool Type	Method
Sample 1	197	UK	2017	Students	Krupka-Weber
Sample 2	375	UK	2019	General population (representative: gender, age, yearly income)	Krupka-Weber
Sample 3	676	UK	2021	General population (representative: gender, age, ethnicity)	Opinion matching

The student sample consisted of 197 British students at the University of Nottingham. Subjects were recruited in September 2017 and completed the experiment using the Krupka-Weber method. Subjects were told that, in order to receive the bonus payment from the vignette task, they had to match the responses of other participants of their own sample (i.e., other students at the University of Nottingham). For each vignette with a legal threshold, subjects were randomly assigned to one of 4 possible versions of the vignette. Thus, our estimates of the norm function $S(.)$ rely on 4 distinct measurements (2 legal and 2 illegal) per vignette, with approximately 50 subjects in each version of each vignette. Students completed the experiment online in around 10 minutes, and one-fifth of participants were selected for payment. The selected subjects were paid a £10 participation fee, plus a bonus payment of £30 if they had successfully coordinated in one of the 15 vignettes they

had evaluated (5 target vignettes + 10 fillers), randomly selected at the end of the study.

To probe the generalizability of our findings, we repeated the experiment using two samples of the UK general population. A first sample was recruited in March 2019 and consisted of 375 British participants recruited by the online panel survey company Qualtrics. We set recruitment quotas so as to obtain a sample that was representative of the UK general population along three dimensions: gender (51% female), age (11% aged 18-24; 21% aged 25-34; 23% aged 35-44; 24% aged 45-54; 21% aged 55+), and yearly income (23% less than £20,000; 42% £20,000-£40,000; 20% £40,000-£60,000; 15% more than £60,000). The experiment used the Krupka-Weber method and again subjects were told they had to coordinate within their own sample (other British individuals recruited through Qualtrics). Again, subjects were randomly assigned to one version of each of the five vignettes with legal thresholds. This time, however, we designed 8 different versions of each vignette (4 legal and 4 illegal), so as to increase the precision of our estimate of the norm function $S(.)$. All subjects received a base incentive of approximately £0.40 for participating in the online study. In addition, we randomly selected one-fifth of participants and paid them (through Qualtrics) according to the same rules used for the UK student sample (£30 for successful coordination on one randomly selected vignette).

The second UK general population sample was recruited using a different online sample provider, Prolific. We recruited 676 subjects in March 2021 (but, to keep comparability with the earlier samples, subjects were asked to evaluate behavior in the vignettes in a pre-pandemic world). We again set recruitment quotas to obtain a sample that was representative of the UK general population in respect to gender (51% female), age (9% aged 18-24; 17% aged 25-34; 19% aged 35-44; 17% aged 45-54; 38% aged 55+), and ethnicity (roughly 81% white; 7% Asian; 5% black; 4% mixed; 3% other; however, we are missing ethnicity data for approximately 4% of the subjects in the sample).The experiment used the opinion matching method: 342 subjects were assigned to the unincentivized condition and 334 to the incentivized condition. The first group was asked their personal opinions about the appropriateness of the behavior described in the vignette (first-order beliefs of appropriateness), while the second group was asked to guess, for each vignette, the most

frequent appropriateness judgment of the first group (second-order beliefs). We again used 8 different versions of each vignette (4 legal and 4 illegal). All subjects received a base incentive of £1.88 for participating in the online study. In addition, we randomly selected one-fifth of the subjects in the incentivized condition and paid them (through Prolific) £30 if they matched the most common response in the first group in one randomly selected vignette.

4 Results

Figure 1 plots the norm functions elicited in the five legal threshold situations in our three samples. These functions plot the average (social) appropriateness of the various behaviors that subjects evaluated in the experiment (see OSM D for the full distributions of appropriateness ratings). Following the convention in the social norms literature, we assign evenly-spaced values of +1 to the rating "Very (socially) appropriate", +0.33 to the rating "Somewhat (socially) appropriate", -0.33 to the rating "Somewhat (socially) inappropriate", and -1 to the rating "Very (socially) inappropriate". Thus, the norm functions $S(.)$ assume positive values for actions that, on average, are evaluated as appropriate and negative values for inappropriate actions. The blue circles show the function values for the student sample, while the red squares show the values for the general population samples. Filled squares indicate the responses of the 2019 sample (elicited with the Krupka-Weber method). For the 2021 sample we elicited responses with the opinion matching method that delivers data on both first-order and second-order beliefs of appropriateness. Figure 1 plots both types of beliefs (dotted squares for first-order beliefs; empty squares for second-order beliefs).

In each panel, the dashed black line indicates the position of the legal threshold. Actions to the left of the threshold are legal, while those to the right are illegal. The first three panels of the figure reveal that, in all samples, the legal threshold exerts a very strong influence on the norm function: there is a sharp drop in (social) appropriateness values as we move from the legal to the illegal side of the thresholds. For the age of consent vignette, there is a drop in appropriateness of between 0.96 units (general population 2019) and 0.74 units (students) as the age of the young person in the vignette changes from 16 years and 1 month (legal) to 15 years and 11 months (illegal).

For the vignette where a shopkeeper sells alcohol to a youth, the drop is of between 1.10 units (general population 2021, second-order beliefs) and 0.86 units (general population 2019) as the young customer's age changes from 18 years and 1 month (legal) to 17 years and 11 months (illegal). Finally, in the cash at customs vignette, there is a drop in appropriateness of between 0.97 units (general population 2021, second-order beliefs) and 0.86 units (general population 2021, first-order beliefs) as the person in the vignette imports an amount of cash that is 100 Euros above rather than below the legal threshold. In contrast, it is apparent in all cases that the small increments in the running variables (age and cash amount imported) are inconsequential for behaviors that are both on the legal side of the threshold, or both on the illegal side of the threshold.

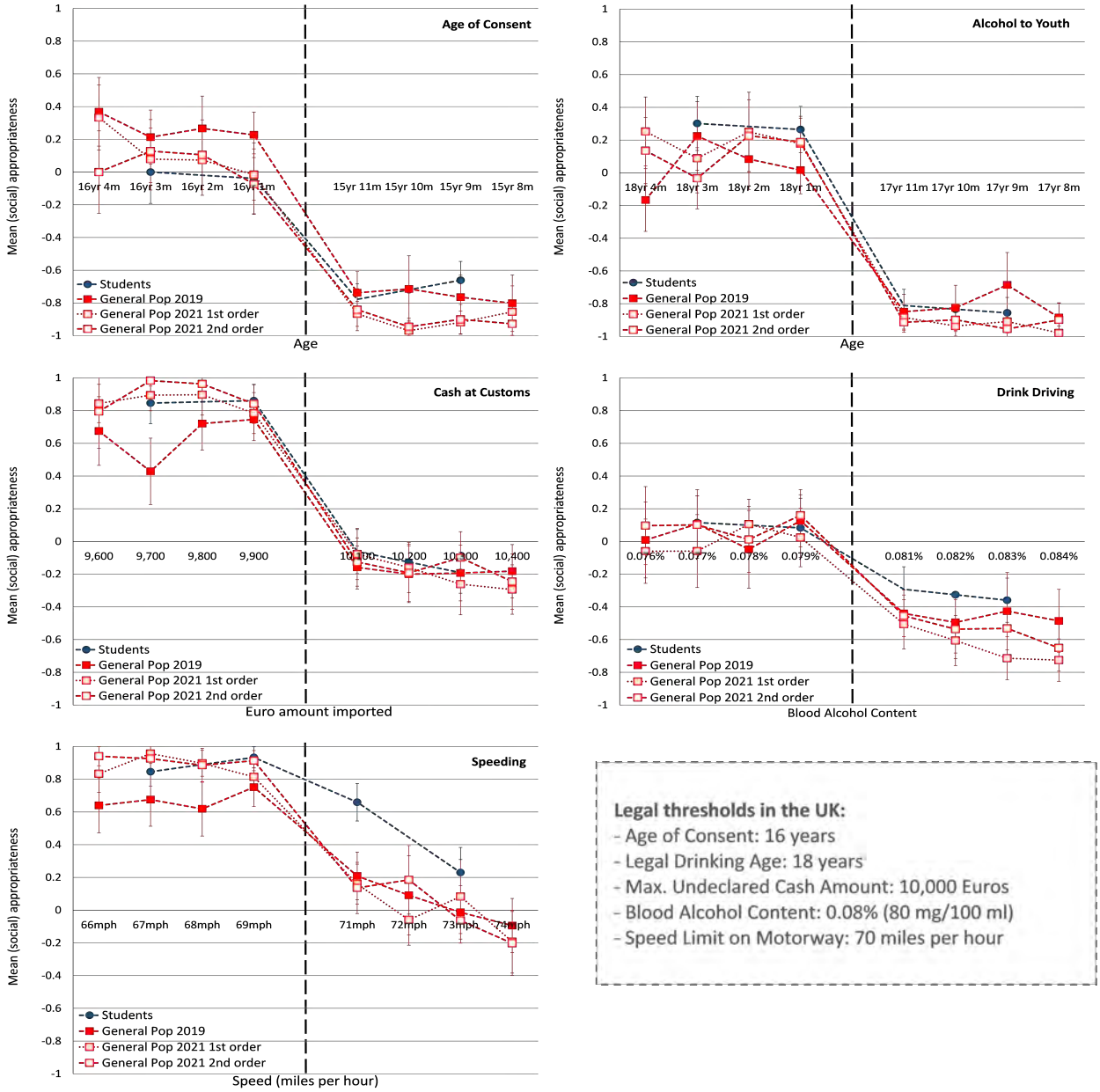
The drop in appropriateness values at the legal threshold is, however, smaller in the drink driving and speeding vignettes, for both the student and general population samples. Here, the functions tend to decrease over the range of behavior measured in the experiment, but there are not such sharp discontinuities at the threshold. For the drink driving vignette, the drop in appropriateness varies between 0.61 units (general population 2021, second-order beliefs) and 0.38 units (students) as the blood alcohol concentration changes from 0.079% (legal) to 0.081% (illegal). For the speeding vignette, the appropriateness drops between 0.78 units (general population 2021, second-order beliefs) and 0.27 units (students) as the speed changes from 69mph (legal) to 71mph (illegal).

We formally examine these patterns using regression analysis. Based on the identification strategy sketched in equation 7, we estimate the following regression model for each vignette:

$$s(o_i) = \alpha + \beta_1(T - o_i) + \beta_2 Illegal_i + \beta_3(T - o_i) * Illegal_i + \epsilon_i \tag{8}$$

where $s(o_i)$ is subject i's evaluation of appropriateness of behavior in the vignette describing opportunity o_i, $(T - o_i)$ measures the distance between the legal threshold and opportunity o_i, $Illegal_i$ is a dummy that takes value 1 if subject i evaluated a version of the vignette that contained illegal behavior and 0 otherwise, and ϵ_i is the error term. Note that this model allows the slope of the relationship between appropriateness and distance from the threshold to differ between legal and illegal opportunities. The coefficient β_1 measures the relationship for legal opportunities, i.e., the

Figure 1: Norms in the five legal threshold situations, UK samples



Note: Each panel plots the average (social) appropriateness of actions at various distance from a legal threshold (1 = very (socially) appropriate; -1 = very (socially) inappropriate). The dashed black line indicates the position of the legal threshold in each situation (values of the legal thresholds are reported in the bottom-right box). Actions to the left of the threshold are legal, actions to the right are illegal. Bars are 95% confidence intervals.

slope of the esteem function $S(.)$ below the legal threshold.[18] The coefficient β_3 measures how this slope changes for illegal, rather than legal, opportunities, i.e. it allows us to derive the slope of $S(.)$ for opportunities that exceed the legal threshold. The coefficient of most interest is β_2, which measures the difference between the estimates of the norm function for opportunities just above or just below the legal threshold T and thus captures the discontinuity of the norm at the legal threshold, i.e. the causal effect of law on normative considerations.

We estimated the regression equation 8 separately for each sample and each vignette, using OLS regressions with heteroscedasticity-robust standard errors. Table 2 shows the results, in Panel A for the students sample, in Panel B for the 2019 general population sample, and in Panels C (first-order beliefs) and D (second-order beliefs) for the 2021 general population samples.[19]

Starting with Panel A (students), the estimate of the coefficient β_2 is negative and highly significant in models A1, A2 and A3 (the age of consent, alcohol to youth, and cash at customs vignettes), indicating the existence of strong discontinuities at the legal thresholds for these situations (the magnitude of β_2 ranges from -0.778 to -1.035 across the three vignettes). In contrast, the estimates of β_2 are much smaller in models A4 and A5 (the drink driving and speeding vignettes). The coefficient is in fact not significantly different from zero for the speeding vignette, and only significant at the 10% level for the drink driving vignette (p = 0.068).

Similar patterns emerge in the general population samples (Panels B, C and D). Here we also find strong discontinuities in the norm functions for the age of consent, alcohol to youth, and cash at customs vignettes (coefficients ranging from -0.803 to -1.137), but weaker effects in the drink driving and speeding vignettes, where the coefficients are roughly half the magnitude of those of the other three vignettes (ranging from -0.461 to -0.592), although they are always strongly significant.

In all samples, a series of Chow tests confirm that there are no significant differences between

[18] In two of our five vignettes (age of consent and alcohol to youth) opportunities below the threshold are illegal, while in the other three opportunities in excess of the threshold are illegal. To ease interpretation, we code our variable $(T - o_i)$ so that positive values are always assigned to legal opportunities and negative values to illegal opportunities. In other words, the variable is actually defined as $(o_i - T)$ for the age of consent and alcohol to youth vignettes, while it is defined as $(T - o_i)$ for the other three vignettes.

[19] For the general population samples we also have data on participants' age, gender and income, which we use as controls in the regressions (not shown in Table 2). We did not collect any socio-demographic data from the students.

the coefficients of the Illegal variable in the first three vignettes, $\beta_2^{consent}$, $\beta_2^{alcohol}$, and β_2^{cash} (all p $\geq$ 0.135), or between the estimates of $\beta_2^{drink-drive}$ and $\beta_2^{speeding}$ (all p $\geq$ 0.347).[20] We instead find statistically significant differences between the estimates of the first and second group of coefficients. Specifically, among students, we find differences that are significant in all such comparisons (all p $\leq$ 0.027) except between $\beta_2^{drink-drive}$ and $\beta_2^{consent}$ (p = 0.124); in the 2019 general population sample, we find significant differences in all such comparisons (all p $\leq$ 0.089); in the 2021 general population sample we find significant differences in first-order beliefs between $\beta_2^{alcohol}$ and both $\beta_2^{drink-drive}$ and $\beta_2^{speeding}$ (both p $\leq$ 0.009), and in second-order beliefs in all comparisons (all p $\leq$ 0.055), except those involving $\beta_2^{consent}$ (both p = 0.256).

Overall, these results show that the law can have a strong influence in shaping the norms that govern the behaviors that are targeted by the law. However, the results also show that the expressive power of law does not hold uniformly across all situations. In particular, our data show that, in the UK, laws related to driving behaviors seem to hold a weak power on the underlying social norms. In the Supplementary Materials (OSM E) we explore potential explanations for this variability in the expressive power of law. In particular, we show, using data from follow-up questions that we included at the end of our general population experiments, that illegal behavior in the speeding and drink-driving vignettes is perceived to be relatively difficult for law-enforcement to accurately measure, and relatively likely to occur unintentionally. We also show that the estimated effects of laws on norms are generally weaker among subjects who believe that illegal behavior may be unintentional or difficult to measure. Overall, this analysis supports the prediction of our model that the perceived intentionality and measurability of behavior are moderators of the expressive power of law, and that these factors can explain some of the between-vignette variability in our estimated discontinuities. We also find some support for police tolerance towards illegal behavior being another moderator – which our model also predict – though here the evidence is rather weaker.

[20] The p-values we report for the Chow tests have been adjusted to take into account the multiple comparison problem. P-values are adjusted using the Benjamini-Hochberg False Discovery Rate method (Benjamini and Hochberg 1995; Simes 1986).

Table 2: OLS regressions, UK samples

Panel A: Students	**(A1)** Age of consent	**(A2)** Alcohol to youth	**(A3)** Cash at customs	**(A4)** Drink driving	**(A5)** Speeding
$(T - o_i)$	0.019 (0.071)	0.019 (0.054)	-0.007 (0.039)	0.016 (0.061)	-0.044 (0.027)
$Illegal$	-0.778*** (0.184)	-1.035*** (0.138)	-0.866*** (0.132)	-0.326* (0.178)	-0.103 (0.107)
$(T - o_i) * Illegal$	-0.078 (0.081)	0.004 (0.064)	0.072 (0.061)	0.017 (0.077)	0.258*** (0.055)
$Constant$	-0.058 (0.167)	0.246** (0.114)	0.868*** (0.079)	0.067 (0.141)	0.977*** (0.053)
Controls	No	No	No	No	No
R^2	0.293	0.613	0.567	0.139	0.319
N.	197	197	197	197	197

Panel B: Gen. pop. 2019	**(B1)** Age of consent	**(B2)** Alcohol to youth	**(B3)** Cash at customs	**(B4)** Drink driving	**(B5)** Speeding
$(T - o_i)$	0.026 (0.038)	-0.029 (0.039)	-0.055 (0.038)	-0.014 (0.043)	-0.039 (0.033)
$Illegal$	-0.890*** (0.128)	-0.920*** (0.118)	-0.948*** (0.124)	-0.522*** (0.143)	-0.461*** (0.127)
$(T - o_i) * Illegal$	-0.001 (0.051)	0.034 (0.045)	0.058 (0.051)	0.024 (0.058)	0.145*** (0.049)
$Constant$	0.216 (0.140)	0.264** (0.119)	0.596*** (0.145)	-0.040 (0.153)	0.623*** (0.126)
Controls	Yes	Yes	Yes	Yes	Yes
R^2	0.467	0.405	0.373	0.160	0.263
N.	375	375	375	375	375

Panel C: Gen. pop. 2021 1st order	**(C1)** Age of consent	**(C2)** Alcohol to youth	**(C3)** Cash at customs	**(C4)** Drink driving	**(C5)** Speeding
$(T - o_i)$	0.096** (0.039)	0.003 (0.041)	0.016 (0.029)	-0.041 (0.041)	0.010 (0.023)
$Illegal$	-0.803*** (0.119)	-1.047*** (0.116)	-0.820*** (0.129)	-0.487*** (0.143)	-0.592*** (0.097)
$(T - o_i) * Illegal$	-0.107** (0.047)	0.016 (0.044)	0.055 (0.046)	0.132** (0.052)	0.094** (0.040)
$Constant$	-0.323** (0.141)	0.299** (0.145)	0.750*** (0.129)	0.204 (0.149)	1.063*** (0.101)
Controls	Yes	Yes	Yes	Yes	Yes
R^2	0.509	0.610	0.554	0.281	0.525
N.	332	332	332	332	332

Panel D: Gen. pop. 2021 2nd order	**(D1)** Age of consent	**(D2)** Alcohol to youth	**(D3)** Cash at customs	**(D4)** Drink driving	**(D5)** Speeding
$(T - o_i)$	0.041 (0.047)	-0.042 (0.040)	0.011 (0.035)	-0.019 (0.044)	0.013 (0.015)
$Illegal$	-0.813*** (0.138)	-1.137*** (0.113)	-0.971*** (0.127)	-0.542*** (0.139)	-0.577*** (0.109)
$(T - o_i) * Illegal$	-0.030 (0.053)	0.044 (0.043)	0.015 (0.050)	0.078 (0.054)	0.108*** (0.041)
$Constant$	-0.350** (0.158)	0.224* (0.125)	0.879*** (0.120)	0.161 (0.145)	0.979*** (0.094)
Controls	Yes	Yes	Yes	Yes	Yes
R^2	0.478	0.595	0.568	0.253	0.549
N.	334	334	334	334	334

Note: Dependent variable is the evaluation of appropriateness of the behavior described in a vignette. Robust standard errors in parentheses. Regressions with bootstrapped standard errors yield very similar results. Controls (age, gender, and income) are included in the regressions of Panel B, C and D, but are not reported in the Table. In Panel C, we have 332 observations (instead of 342) because 10 subjects have missing values for some of the control variables. *** = 1% significance level; ** = 5% significance level; * = 10% significance level.

To conclude this section, we briefly discuss two additional interesting results that emerge from our data. First, from a methodological point of view, it is remarkable that the results obtained with the student sample are successfully replicated using representative samples of the broader population. This result contributes to the ongoing debate about the generalizability of results of standard economic experiments (see, e.g., Levitt and List 2007; Camerer 2015).

Another interesting result is that our data show remarkable similarities between the two methods used to measure social norms. We observe similar discontinuities across the five vignettes both when we use the Krupka-Weber method and the opinion matching method. Moreover, results are similar regardless of whether we consider first-order or second-order beliefs of appropriateness. This suggests that it is unlikely that our results are driven by subjects using legality as a coordination strategy in the experiment. If they had done so, we would expect to observe weaker discontinuities in the norm functions elicited with the opinion matching method, since in this case the incentives to coordinate are muted.[21] We further explore the robustness of our results below where we present the results of three additional experiments.

5 Robustness analysis: Additional experiments

We conducted three additional experiments to further probe the robustness of our results. Table 3 presents an overview of these experiments. In Experiment 1 we conducted *placebo tests* to rule out alternative explanations for the discontinuities we observed at the legal thresholds in the main experiment. In Experiment 2 we designed an alternative version of the experiment where, instead of measuring the esteem function $S(.)$ through subjects' perceptions of the social appropriateness of actions, we directly elicited their inferences about a person's *prosocial traits*, upon observing his/her actions. Finally, in Experiment 3 we probed the generalizability of our results by studying

[21] Further corroborating evidence comes from the fact that we see a series of systematic patterns in our data that are inconsistent with the notion that subjects used legality as a coordination strategy. First, Figure 1 shows that, in all samples, there are several instances of legal behaviors being evaluated as inappropriate and illegal behaviors being evaluated as appropriate, which is in contradiction of an alternative coordination strategy based on legality. Second, we do not see uniform effects of the law on norms, as we would instead expect to observe if the discontinuities were driven by such an alternative strategy.

the expressive power of law in a country (China) where the *rule of law* is relatively weak compared to the UK, where all our other experiments were run.

Table 3: Overview of the additional experiments.

	N	Nationality	Year	Subject Pool Type	Method
Experiment 1 (Placebo thresholds)	1,554	UK	2021	General population	Opinion matching & Krupka-Weber
Experiment 2 (Prosocial traits)	2,767	UK	2021	General population	Opinion matching
Experiment 3 (Weaker rule of law)	248	China	2017	Students	Krupka-Weber

5.1 Experiment 1: Placebo thresholds

One concern with the results presented in Section 4 is that the downward discontinuities at the legal threshold in the social norm functions may be caused by two alternative mechanisms that are distinct from our preferred interpretation (i.e., the law produces discontinuous social esteem at the threshold through its impact on the inferences drawn about a perpetrator's "type"). The first mechanism is especially relevant for the Krupka-Weber method and is related to the concern, discussed earlier, that the legal threshold may give subjects a salient focal point to coordinate their responses in the experiment. Despite the precautions adopted in the design of the experiment (see Section 3.2) and the encouraging results reported at the end of the previous section, one may still be concerned that subjects may have coordinated by rating actions that are illegal as "inappropiate" and actions that are legal as "appropriate", irrespective of whether this truly corresponds to their perception of the social norm. Such a coordination strategy would produce a downward discontinuity at the legal threshold, even if the law had no expressive power.

The second mechanism is related to the notion that subjects may believe that the exact position of the legal threshold chosen by a government reflects information, which is privy to the government, about the existence of sharp variations at the threshold in some relevant aspects of the decision situation. For instance, the government may set the legal drinking age limit at 18 years based on information that the harms of alcohol are discontinuously higher below that age. As ex-

plained in Section 3, our identification strategy rules out this possibility by assumption. We believe the assumption is justified: although considerations about the harms of alcohol at various ages are undoubtedly factored in in the choice of the legal drinking age limit, it is very unlikely that this information sharply discriminates between points that are very close to the threshold (e.g., 18 years and 1 month vs 17 years and 11 months). Nevertheless, subjects may *believe* that this is the case and thus react to the position of the threshold *as if* it was indeed carrying information about some sharp variation in the function $S(.)$ exactly at that point, that would exist even in the absence of a law. If this were the case, it would mean that the expressive power of law we identified in the main experiments could be at least in part driven by a similar information transmission mechanism to those discussed in McAdams (2000, 2015), Bénabou and Tirole (2011) and Bursztyn et al. (2020a).

To probe the robustness of our results against these alternative explanations, we designed an experiment where we introduced a *placebo threshold* in each of our five vignettes. The placebo threshold was always positioned at a close distance from the actual legal threshold (five or six "units" above or below the legal threshold).[22]

We introduced the placebo thresholds in the vignettes using narratives in which a group of people advocated an alternative limit to the behavior described in the vignette, either above (i.e., more permissive) or below the actual legal threshold. For instance, in the speeding vignette, the person in the vignette recalls hearing about "a petition to raise speed limits on motorway to 75mph". Across vignettes, we changed the narratives to create variation in the extent to which subjects could interpret the placebo thresholds as conveying information about the decision situation. The idea is that subjects may be more likely to believe the placebo threshold has informational value if the alternative limit is advocated by a group of people that is more representative of society and/or has more expertise of the situation. Therefore, the "high informational content" narratives described the placebo thresholds as proposed by experts and/or relatively large lobbying groups, such as "a

[22] We set the placebo thresholds at 75mph for the speeding vignette (legal threshold: 70mph), at 0.075% BAC for the drink driving vignette (legal threshold: 0.08%) and at 10,500 Euros for the custom vignette (legal threshold: 10,000 Euros). For thresholds based on age, we felt it was more natural to place the placebo threshold at a distance of half-a-year from the actual legal threshold. We therefore placed them six months below (age of consent) or above (legal drinking age) the current legal threshold in the UK (16 and 18 years, respectively).

panel of scientists" (drink driving vignette), a public "petition" (speeding vignette), or a "campaign group" (alcohol to youth vignette).[23] The "low informational content" narratives revolved instead around the opinions of a smaller number of unqualified people, such as a "group of friends" (age of consent vignette) and "custom officials working in the airport" (cash at custom vignette).

In all cases, we explicitly mentioned that the people advocating the alternative limit believed that the placebo threshold neatly separated appropriate from inappropriate behavior. For example, in the speeding vignette we said that the petition argued that "it is appropriate to drive at speeds up to 75mph, and inappropriate at higher speeds". We did this to maximize the salience of the placebo threshold as a focal coordination point. In this way, the vignette explicitly spelled out a strategy subjects could use to coordinate with others, by rating behavior as appropriate if it was below the placebo threshold, and inappropriate otherwise.

Our test consists of measuring whether the placebo thresholds produce discontinuities in the social norm function analogous to those produced by the legal thresholds. If we were to systematically observe similarly-sized discontinuities at the placebo thresholds, this would raise questions about our interpretation of the results of the main experiment. The mechanism we propose is that discontinuities in the social norm function are driven by payoff discontinuities at the legal threshold that separate sharply between individuals who take marginally legal and illegal actions. These payoff discontinuities do not arise at the placebo thresholds (exceeding the placebo limit does not trigger any criminal record and/or material sanction). Thus, observing systematic discontinuities in the $S(.)$ function at the placebo threshold would suggest that at least part of the effect identified in Section 4 is driven by alternative mechanisms. We can quantify the observed influence of these alternative mechanisms by comparing the magnitude of the discontinuities at the legal and placebo thresholds (if any).

We recruited 1,554 subjects from the UK general population via the online platform Prolific between May and June 2021. We elicited appropriateness judgments using both the Krupka-Weber

[23] We avoided directly using the government or parliamentary committees as advocates of the alternative limits because we feared that this may signal that a change of the law is imminent, which would have introduced a different reason to respond to the placebo threshold.

method (653 subjects) and the opinion matching method (901 subjects; 260 assigned to the non-incentivized condition and 641 to the incentivized condition).[24] Other than the inclusion of the placebo thresholds in the 5 target vignettes, the experiment was in all important respects identical to that described in Section 3.[25] The incentives were also similar to those of the earlier experiments (£1.88 participation fee plus, for a randomly-selected fifth of those assigned to the incentivized tasks, a £30 bonus payment for matching the most common response in one randomly selected vignette).

Figure 2 shows the main results, both from the Krupka-Weber method and the opinion matching method (we present only the second-order beliefs data)[26]. The new experiments reproduce the patterns of discontinuities at the legal thresholds we observed in Section 4. We observe strong discontinuities for the age of consent, alcohol to youth and cash at customs vignettes, both when we elicit norms using the Krupka-Weber method and the opinion matching method. We instead observe more modest discontinuities at the legal threshold in the drink driving and speeding vignettes.

In nearly all cases, the placebo thresholds have a markedly smaller influence on the norm functions, regardless of the method used to elicit them. For the age of consent, alcohol to youth and cash at customs vignettes, Figure 2 shows that there is hardly any discontinuity at the placebo threshold. For the speeding vignette, the discontinuity measured with the opinion matching method seems to go in the opposite direction than what one would expect (exceeding the placebo threshold increases appropriateness). Only in the drink driving vignette do we observe a discontinuity at the placebo threshold that is roughly of the same (small) magnitude as that observed at the legal threshold.

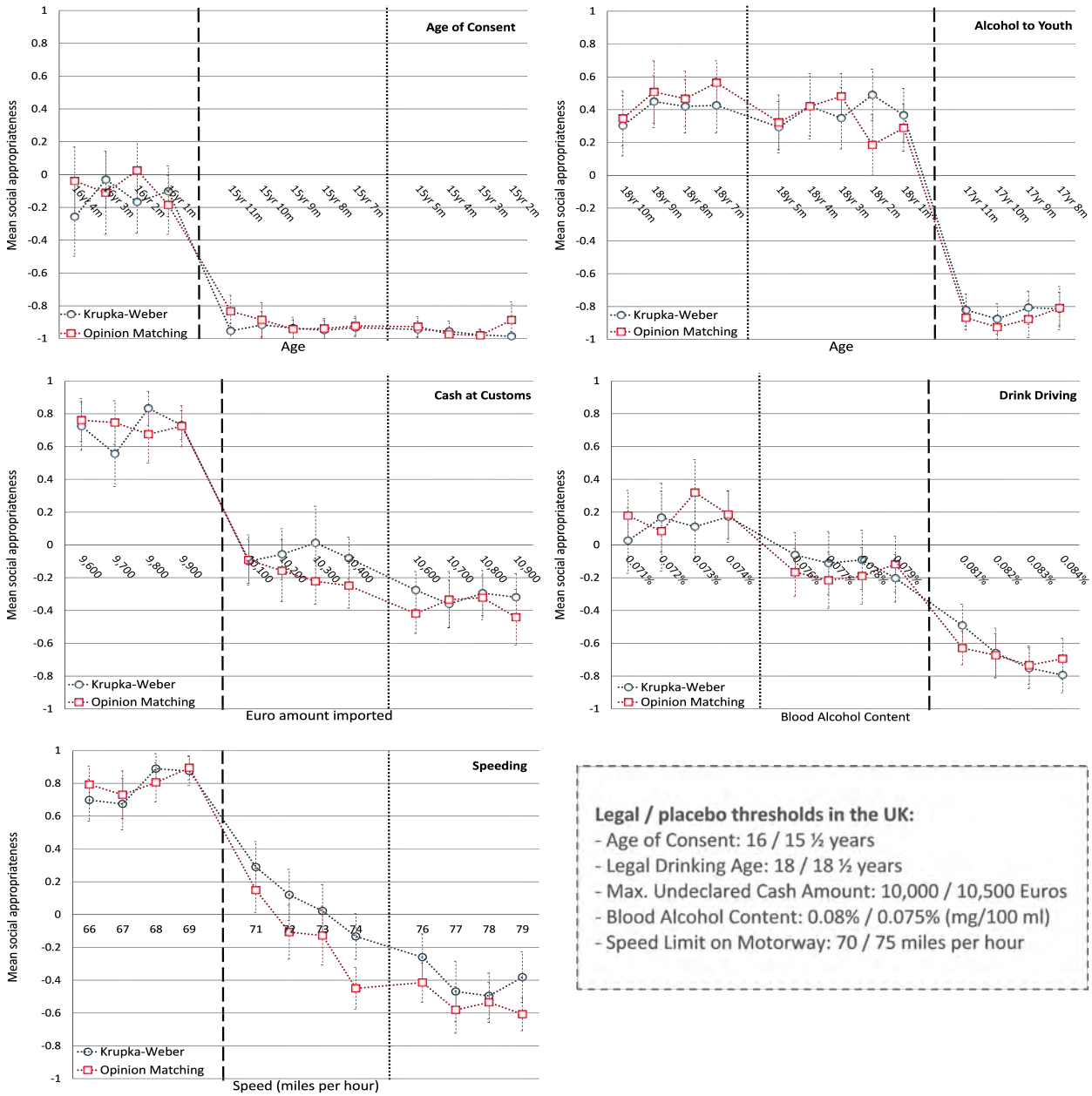
We formally analyze these effects using regression analysis. The regression models are similar to those presented earlier in equation 8, except that we also include a dummy variable for the placebo

[24] We did not recruit a representative sample for any of the robustness experiments because this constraint would have made it impossible to recruit enough subjects, given our budget and the platform pool of volunteers. Also for budget reasons, we recruited a smaller sample in the non-incentivized condition, only with the purpose of using it to incentivize the elicitation of second-order beliefs.

[25] The complete wordings of the placebo vignettes can be found in OSM B, while details of minor changes to the preliminary screens of the experimental instructions, relative to our main experiment, are available in OSM C.

[26] Although they are not of primary interest, in OSM D, we also report the complete distributions of responses in the first-order beliefs condition. This broadly demonstrates that patterns of responses to the two conditions of the opinion matching method were similar (i.e., second-order beliefs elicited in the incentivized condition tended to be well calibrated)

Figure 2: Legal and placebo thresholds



Note: Each panel plots the average social appropriateness of actions at various distance from a legal and a placebo threshold (1 = very socially appropriate; -1 = very socially inappropriate). The dashed black line indicates the position of the legal threshold in each situation; the dotted line indicates the position of the placebo threshold (values of the legal and placebo thresholds are reported in the bottom-right box). Bars are 95% confidence intervals.

threshold and corresponding interaction with the distance from threshold variable. We report more details on the model and the full regression estimates in the Supplementary Materials (OSM F).

The regressions reproduce our earlier results in terms of the effects of the legal thresholds on the norm functions. Regarding the placebo thresholds, when we use the Krupka-Weber method, we find that the placebo produces a marginally significant discontinuity only in the case of the cash at customs vignette ($p = 0.054$). The size of the placebo discontinuity is 0.22 compared to 0.88 at the legal threshold, and the latter is significantly larger than the former ($p = 0.000$).

When we use the opinion matching method, we find only two cases where there is a significant discontinuity at the placebo threshold. In the speeding vignette there is an increase in appropriateness of 0.22 after the placebo threshold, significant at the 5% level ($p = 0.042$). The magnitude of the discontinuity at the corresponding legal threshold (0.61) is significantly larger than that at the placebo ($p = 0.026$). For the drink driving vignette, we observe a statistically significant discontinuity at the placebo ($p = 0.000$) that is roughly of the same size as that observed at the legal threshold ($p = 0.878$).

Overall, out of ten tests (five vignettes by two methods), in seven cases we find no evidence of a statistically significant discontinuity at the placebo threshold. In two cases, we find a significant discontinuity at the placebo threshold that is nevertheless significantly smaller than the discontinuity observed at the corresponding legal threshold. Only in one case do we find evidence of a placebo effect that is similar to the effect detected at the legal threshold.

Taken together, the results from the placebo experiments and our main results provide a coherent set of evidence that corroborates our preferred interpretation for the observed effects. These results provide instead little support for the notion that subjects exploit focal points alternative to the norm to coordinate their responses in the Krupka-Weber task. The placebo thresholds offer a very salient focal point, since we explicitly state how subjects can use them to coordinate. Yet, we find very little evidence of discontinuities at the placebo thresholds when we use the Krupka-Weber method. This result is interesting from a methodological perspective as it suggests that the Krupka-Weber method – which has become an influential tool to elicit norms in the literature – is robust to the

presence of norm-unrelated focal points (also see Fallucchi and Nosenzo 2021, for further evidence on this point).

We also do not find much evidence that the effects might be driven by a mechanism whereby laws transmit information about relevant features of the decision environment. We fail to detect significant discontinuities in four out of six cases where the placebo thresholds have high informational content (Krupka-Weber: drink driving, speeding, alcohol to youth; opinion matching: alcohol to youth). In one case we observe a discontinuity that goes against the informational content of the placebo (it is more appropriate to drive at a speed higher than lower than the placebo limit). The only piece of evidence that is in line with this type of information transmission mechanism occurs when we use the opinion matching method in the drink driving vignette. However, this is also the vignette – together with speeding – in which we generally find evidence that the law has relatively weak expressive power across all our experiments, which reinforces our interpretation that this may be an area of law where our proposed mechanism (the law produces discontinuous social esteem at the legal limit) has little bite.

5.2 Experiment 2: Prosocial traits

To further probe our interpretation of the main result, we designed another experiment where, instead of measuring the social appropriateness of the behaviors described the vignettes, we asked subjects to report their perception of the prosocial traits of individuals who engage in those behaviors. This may be seen as a more direct test of the mechanism we propose in the model of Section 2, where the social esteem $S(.)$ that accrues to a person for engaging in a certain behavior is determined by the inferences that observers make about the person's "type", defined in terms of their prosociality (i.e., how much they care about affecting others' payoffs).

We therefore designed an alternative version of our main experiment where, after reading a vignette, subjects had to report the likelihood that the person in the vignette would engage in three different types of prosocial behaviors, involving *trustworthiness*, *honesty* and *altruism*. Trustworthiness was captured by eliciting the perceived likelihood that the person would keep a promise

made to a friend. Honesty was measured by eliciting the likelihood that the person would spontaneously return excess change they were accidentally given by a cashier. Altruism was measured by asking for the likelihood that the person would volunteer for a charitable organization. In each case, subjects responded using a four-point ordered scale ("Very likely", "Somewhat likely", "Somewhat unlikely", "Very unlikely") that mirrored the scale used in our other experiments.

Subjects were also asked to report the likelihood of three additional behaviors that we included in the experiment as fillers to distract subjects from the true objective of the study. We chose behaviors that are socially desirable (as the target behaviors arguably are), but unrelated to prosociality. The behaviors were: exercising regularly to keep fit; keeping a healthy diet; and reading at least two books per month.

To make the task more manageable for subjects and to further reduce the scope for experimenter demand, each subject was presented with only one target vignette plus three filler vignettes.[27] The three filler vignettes were always presented first and in random order, and the target vignette was presented last. We used 8 different versions of each target vignette, varying whether the described behavior was legal or illegal and its distance from the threshold (4 versions legal and 4 illegal). Each subject was randomly assigned to only one version of each target vignette.

To incentivize responses, we used the opinion matching method. We recruited a total of 2,767 subjects and randomly assigned 783 of them to a non-incentivized condition where we simply asked them to report their personal opinion of the likelihood that the person in the vignette would engage in each of the six behaviors. The remaining 1,984 subjects were asked to guess the most common response to each of the six questions among the first group. The experiments were run in June 2021 using a sample of the UK general population recruited on Prolific. All subjects were paid a £0.94 participation fee. Moreover, we randomly selected one-tenth of the subjects in the second group (incentivized condition) and paid them £30 if their response to one randomly selected question matched the most common response among the non-incentivized group.

Our test consists of measuring whether, in each of the five target vignettes, we detect a dis-

[27] The three filler vignettes were the three vignettes that subjects saw at the beginning of all our experiments, and that we use to emphasize the difference between legality and appropriateness; for details see footnote 17.

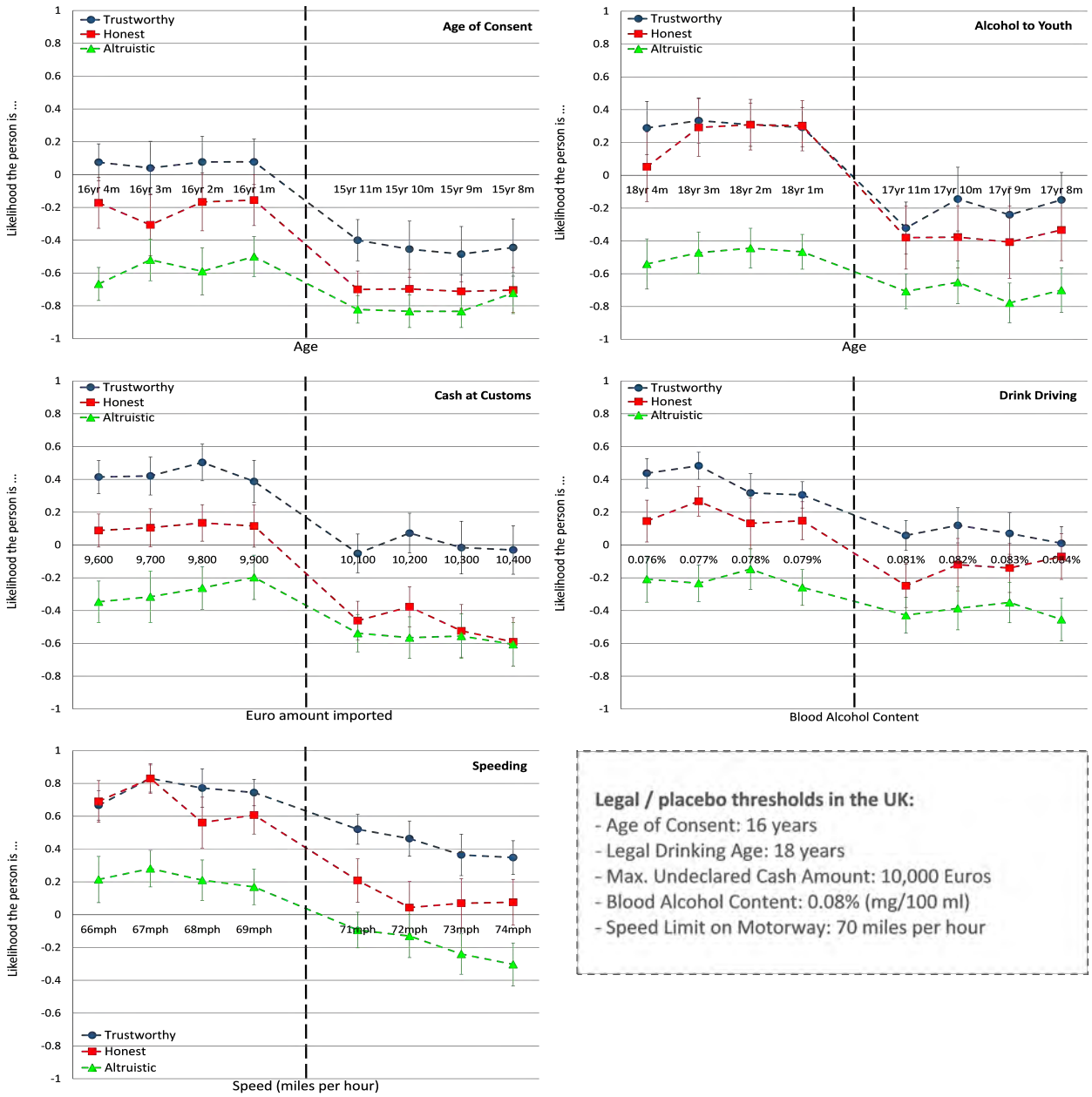
continuity at the legal threshold in the inferences subjects make about a person's trustworthiness, honesty and altruism. Figure 3 reports the main results, based on the responses of subjects in the incentivized condition.[28] To construct the figure, we assigned evenly-spaced values of +1 to the response that a person is "Very likely" to engage in a certain prosocial behavior, +0.33 to the response "Somewhat likely", -0.33 to the response "Somewhat unlikely", and -1 to the response "Very unlikely". Thus, the figure uses the same scale as our previous appropriateness figures. Positive values indicate that, on average, a person is evaluated as likely to engage in prosocial behavior, while negative values indicate the opposite. In each panel, we show separate functions for the likelihood that the person engages in trustworthy (blue circles), honest (red squares), and altruistic behavior (green triangles).

Several interesting results emerge from the figure. First, in all three vignettes where our main experiment found that the expressive power of law is strongest (age of consent, alcohol to youth and cash at customs), we observe marked discontinuities in perceived trustworthiness, honesty and (to a lesser extent) altruism. Across the three vignettes, the size of the discontinuities ranges between 0.61 and 0.44 for trustworthiness, between 0.68 and 0.54 for honesty, and between 0.34 and 0.24 for altruism. Our regression analysis, which mirrors the analysis of the main experiment and is reported in full in the Supplementary Materials (OSM G), indicates that the discontinuities are statistically significant in all cases (all $p \leq 0.009$).

Second, we observe somewhat smaller discontinuities in the two vignettes where we have found weaker expressive power of law (speeding and drink driving). For speeding, the size of the discontinuities is 0.40 for honesty, 0.26 for altruism, and 0.22 for trustworthiness. The altruism discontinuity is significant at the 5% level ($p = 0.045$), while the discontinuities in the other two behavioral traits are significant at the 1% level ($p \leq 0.002$). For drink driving, the discontinuities are 0.40 for honesty, 0.25 for trustworthiness, and 0.17 for altruism. The effect is insignificant in the case of trustworthiness ($p = 0.130$), marginally significant for altruism ($p = 0.079$), and significant at the

[28] As in the case of the placebo thresholds experiment, our budget only allowed to recruit a smaller number of subjects in the non-incentivized condition with the purpose of using their data to incentivize the second group. In OSM D, we also report the complete distributions of responses to the non-incentivised condition., This shows that, like in our other experiments, second-order beliefs were generally well calibrated.

Figure 3: Legal thresholds and perceived trustworthiness, honesty and altruism



Note: Each panel plots the average perception that the person in the vignette engages in trustworthy, honest and altruistic behavior based on opportunities at various distance from the legal threshold (1 = very likely to be trustworthy/honest/altruistic; -1 = very unlikely). The dashed black line indicates the position of the legal threshold in each situation (values of the legal thresholds are reported in the bottom-right box). Bars are 95% confidence intervals.

1% level for honesty (p = 0.000).

These results mirror our previous findings and corroborate the interpretation we proposed earlier: laws that set limits for age of consent, drinking age, and cash imported at customs have strong and discontinuous effects on the inferences observers make about a person's prosocial "type". In contrast, the influence of law is somewhat weaker for speeding and especially drink driving laws. In addition, these findings shed further light on the *specific dimensions of prosociality* that these inferences revolve around. It seems that observers strongly update their beliefs about a person's trustworthiness and honesty, and only to a lesser extent about their altruism (which in the experiment was measured in terms of volunteering for a charity).

5.3 Experiment 3: Weaker rule of law

At the same time as the UK student experiment described in Section 3, we also collected data from a second student sample comprised of 248 Chinese students at the University of Nottingham Ningbo China. The main interest of this additional experiment was to probe the generalizability of findings by testing the effects of laws on norms in a very different legislative environment, one where the rule of law is relatively weak compared to the UK (for instance, according to the 2016 Rule of Law Index of the World Justice Project, the UK ranked 10th out of 113 countries while China ranked 80th).

Procedures used in the Chinese experiment were similar to those used in the UK student sample experiment. Instructions were first translated into Chinese and then back-translated in English, as per usual practice. The Chinese vignettes were further slightly adjusted to reflect cross-country differences in the law (although laws regulating the five behavior under study exist in both countries, the cutoff values of the thresholds differ).[29] Incentives were converted using a PPP exchange rate

[29] Other aspects of the real-world legal frameworks, regulating the actions featuring in the vignettes, may of course also have differed between the two countries. For instance, some laws may carry heavier punishments or be more strongly enforced in one country or the other. Therefore, while we kept all procedural features of the UK and China experiments as close as possible, our aim is not to conduct a fully controlled cross-cultural comparison of the effect of law on norms. Rather, we consider identifying the expressive power of laws in each country to be of independent interest. We can also comment on whether the results are qualitatively similar between the two countries.

of £1 = 6.2RMB, and the payment rules were the same as those in the UK students experiment.[30]

Figure 4 shows the norm functions estimated from the responses of the Chinese students. The figure has the same structure of the previous figures. Table 4 contains the regression estimates of this data, using the same models shown in equation 8.

Table 4: OLS regressions, China sample

	(1) Age of consent	(2) Alcohol to youth	(3) Cash at customs	(4) Drink driving	(5) Speeding
$(T - o_i)$	0.050	0.046	-0.023	0.097*	0.028
	(0.061)	(0.049)	(0.054)	(0.053)	(0.050)
$Illegal$	-0.495***	-0.410*	-1.078***	-0.215	-0.751***
	(0.156)	(0.160)	(0.158)	(0.151)	(0.157)
$(T - o_i) * Illegal$	-0.006	0.029	0.044	-0.019	-0.006
	(0.069)	(0.066)	(0.074)	(0.069)	(0.068)
$Constant$	-0.243*	-0.068	0.690***	-0.161	0.472***
	(0.132)	(0.117)	(0.105)	(0.108)	(0.117)
Controls	No	No	No	No	No
R^2	0.301	0.285	0.468	0.213	0.396
N.	248	248	248	248	248

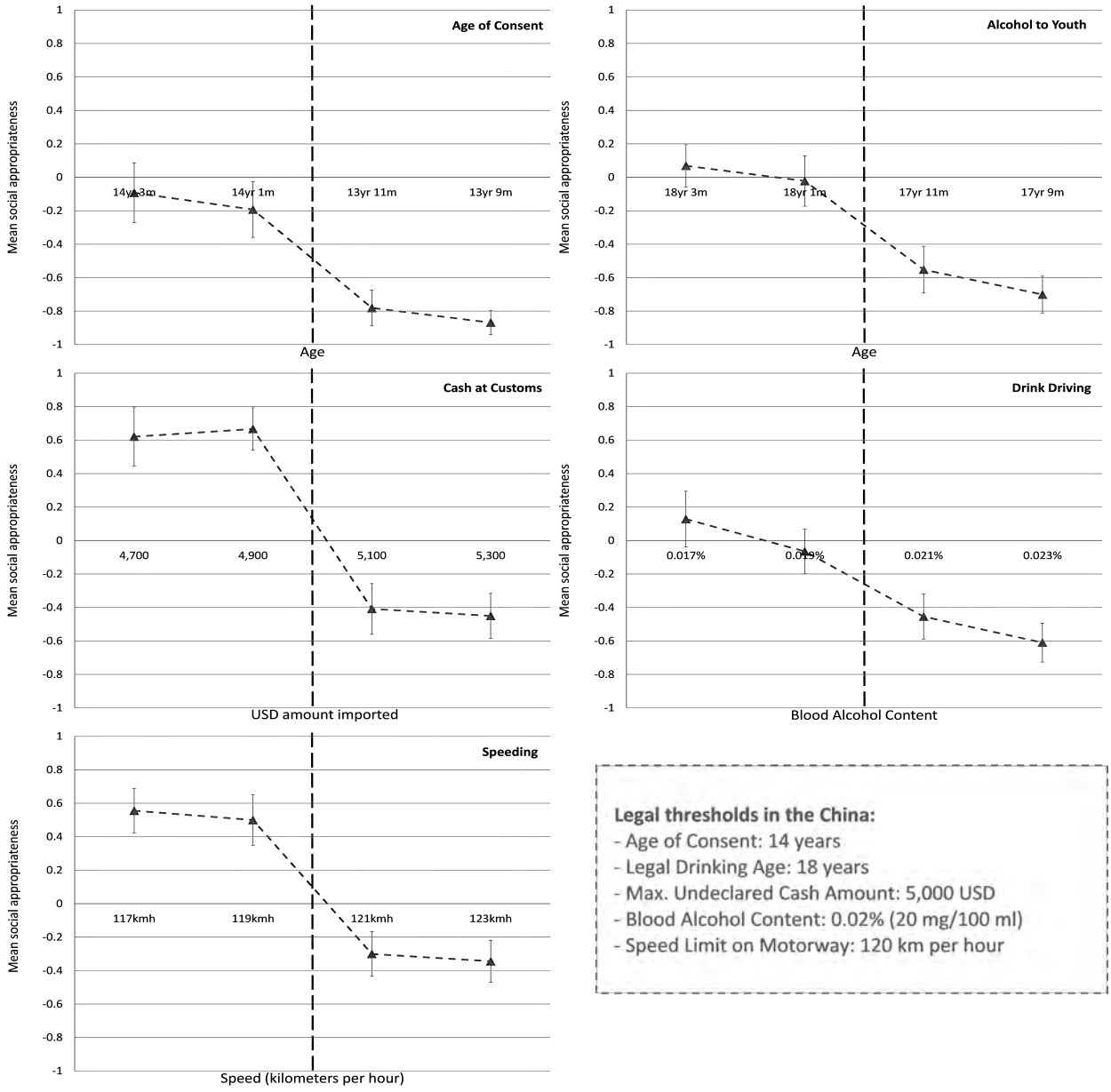
Note: Dependent variable is the evaluation of appropriateness of the behavior described in a vignette. Robust standard errors in parentheses. Regressions with bootstrapped standard errors yield very similar results. *** = 1% significance level; ** = 5% significance level; * = 10% significance level.

In the Chinese sample, we observe that the law also exerts expressive power on norms, albeit again the effect is not uniform across the five situations. In contrast to the UK case, in China the law seems to have its strongest effects on norms in the case of the cash at customs and speeding vignettes. The effect is weaker for the age of consent and alcohol to youth vignettes, and statistically insignificant for the drink driving vignette. A series of Chow tests confirm that the law tends to carry different expressive power in the cash at customs and speeding situations compared to the other three situations.[31]

[30] Monetary amounts in the vignettes were also adjusted according to PPP exchange rate (with rounding), except in the cash at customs vignette where the amounts were dictated by different legal thresholds between the UK and China. Conversions, subject to rounding, were also made between imperial and metric units, where relevant.

[31] Specifically, we find that β_2^{cash} and $\beta_2^{speeding}$ are not significantly different from each other (p = 0.233), β_2^{cash}

Figure 4: Norms in the five legal threshold situations, China sample



Note: Each panel plots the average social appropriateness of actions at various distance from a legal threshold (1 = very socially appropriate; -1 = very socially inappropriate). The dashed black line indicates the position of the legal threshold in each situation (values of the legal thresholds are reported in the bottom-right box). Actions to the left of the threshold are legal, actions to the right are illegal. Bars are 95% confidence intervals.

Thus, although there are some differences between the UK and Chinese samples, particularly in the type of situations characterized by strong effects of the law, which may reflect inherent differences in culture as well as in the specifics of the law and law enforcement between the two countries, the main result that the law can have expressive power, but that this varies across situations, carries over to the Chinese sample. It is interesting to note that this data was collected in a very different legislative environment, characterized by markedly weaker rule of law compared to the UK. This shows that the expressive power of law does not require a strong rule of law to take hold.

6 Conclusions

For some years scholars from across the social sciences have asserted that legal rules carry expressive power, i.e. they have the ability to shape the social norms within a society. However, because societal laws and norms typically co-evolve, it has been difficult to design empirical strategies to establish a clear causal effect from laws to norms, which explains the paucity of empirical work on this topic. In this paper we have proposed a novel empirical strategy that enables us to cleanly identify the causal influence of laws on norms. Our design –- taking advantage both of recent advances in methods to estimate norms, and vignettes with laws characterized by thresholds –- allows us to conclude that the legal status of an action does causally influence its normative appropriateness.

Our results have important implications for the effectiveness of laws and formal institutions more generally. They imply that the impact of formal rules on behavior is greater than their mere deterrent effect alone, which is the standard mechanism through which economists have traditionally argued laws and institutions take effect (e.g., Becker 1968). Instead, our findings show that laws can also affect behavior by strengthening the social disapproval towards illegal actions. The effects on behavior of this expressive power may be substantial: in some of our vignettes we find the effect of law on norms to be not just statistically significant, but of a quantitatively large mag-

is significantly different from the other three coefficients (all $p \leq 0.027$), and $\beta_2^{speeding}$ is significantly different from $\beta_2^{drink-drive}$ ($p = 0.034$). All other comparisons are statistically insignificant. All p-values are corrected using the Benjamini-Hochberg False Discovery Rate method.

nitude.

As discussed above, different mechanisms have been proposed for why laws would have an expressive power on norms. Theories that laws transmit information about "community standards" (e.g., Bénabou and Tirole 2011), though empirically possible, cannot be tested in our design because our empirical strategy rules out by assumption the possibility that legal thresholds carry informational content (an assumption which we further corroborate in our placebo experiments). In light of our results, we can however comment on the meta-norm explanations of Cooter (1998, 2000) and McAdams and Rasmusen (2007) – that obeying the law is itself a norm. While our results do not rule out this possibility, they cannot be fully explained by a meta-norm alone. We would expect this meta-norm to produce an expressive power of law which is constant across contexts – we therefore need a further explanation as to why it is not (i.e., why the strength of the meta-norm itself would be context-dependent).

We argue that this can be provided, at least in part, by the signaling theory we sketched in the paper, which uses a "social image" framework (Bénabou and Tirole 2006, 2011) to formalize the arguments proposed by Posner (1998, 2000, 2002). Intuitively, illegality can make behavior less appropriate because of the signal it sends about the person committing it. In scenarios where the illegality can be unintentional or difficult to observe, one would expect the signal conveyed to be weaker and therefore less impactful upon norms. Our model corroborates this intuition and our experiments confirm that laws regulating behaviors that are perceived to be more difficult to measure and more likely to imperfectly reflect the actor's intentions, such as motoring laws, have a weaker influence on the corresponding social norm.

More broadly, our findings speak to the theoretical and empirical literature on the relation between formal and informal incentives that regulate behavior in social settings (e.g., Gneezy and Rustichini 2000; Frey and Jegen 2001; Bénabou and Tirole 2003, 2006, 2011; Fehr and Rockenbach 2003; Fehr et al. 2007; Fehr and Schmidt 2007; Houser et al. 2008; Ariely et al. 2009; Gneezy et al. 2011; Bowles and Polanía-Reyes 2012; Lacetera et al. 2012; Stagnaro et al. 2017; Birke 2020; Charness et al. 2020). This literature has often highlighted how formal incentives can crowd out

the social incentives to engage in prosocial behavior, suggesting the two types of incentives can act as *substitutes*. Our paper provides evidence of a mechanism that produces instead a *complementarity* between formal and social incentives: by shaping social norms, laws can harness the power of social incentives to reinforce the deterrent effect of formal incentives. An interesting question for further research would be to explore the interplay between these substitution and complementarity effects and investigate whether they might be systematically related to the nature of the institution setting the formal incentives, e.g., whether they are governments (as in our experiment) or private companies and organizations (as in much of the crowding-out literature).

Our results also raise interesting new questions – both for theory and empirical work – about the scope for laws and formal institutions to initiate societal change. Take, for instance, gender gaps in social and economic outcomes, which are believed to be partly driven by gender norms perpetuating socio-economic inequality between men and women (e.g., Akerlof and Kranton 2000; Bertrand et al. 2015; Gangadharan et al. 2016; Exley et al. 2020). What is the scope for law to influence and shape norms so as to correct these gender inequalities? The answer to this question is far from obvious, as it depends on whether or not laws have the *same* expressive power across heterogeneous sub-groups of the population, such as gender or racial groups. The mechanism sketched in our model suggests that this needs not be the case: the effect of law on norms may systematically differ across men and women, or across Whites and Nonwhites. For instance, if there are systematic differences in law enforcement between different groups (e.g., wrongful convictions are more prevalent among certain groups than others), then a person's criminal record will be a more or less noisy signal of his/her type for individuals belonging to different sub-groups of the population, leading to smaller or larger discontinuities in the norm function. In follow-up work, we are exploring these conjectures both theoretically and empirically (Görges et al. 2021).

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Online Supplementary Materials

"Law and Norms: Empirical Evidence"

Tom Lane, Daniele Nosenzo, Silvia Sonderegger

Contents

A. Theory appendix

A.1. A Spence-like model

Suppose that, rather than seizing or leaving an opportunity that they are (exogenously) presented with, agents can choose the level of negative externality they impose on others. Let $\overline{o} \gg 0$ indicate the legal threshold. An individual who selects action $o \geq 0$ obtains utility

$$\begin{cases} to - \theta\frac{o^2}{2} + \gamma(o) + \Psi & \text{if } o \leq \overline{o} \text{ (within the law)} \\ to - \theta\frac{o^2}{2} + \gamma(o) + p(\gamma(o) - K) + (1-p)\Psi & \text{if } o > \overline{o} \text{ (outside of law)} \end{cases}$$

where t is the marginal (material) return from o, while $\theta\frac{o^2}{2}$ reflects the psychological cost for an individual of type θ from imposing a negative externality on others. We restrict attention to

$$\theta_{\max} - K - \Psi < 0 \tag{a.1}$$

which ensures that, conditional on breaking the law, an individual's expected payoff is decreasing in p, the probability of detection. As we show in OSM A4, in equilibrium $\overline{o}$ is chosen by at most one type. For brevity of exposition, we assume that $\overline{o}$ is selected in equilibrium.[1]

Proposition A1: *Denote as θ_{thres} the type who selects $\overline{o}$ in equilibrium. In any D1-refined equilibrium, the difference in total social esteem from choosing $\overline{o} - \varepsilon$ and $\overline{o} + \varepsilon$ where $\varepsilon \to 0$ is given by,*

$$\lim_{\varepsilon \to 0} \left[S(\overline{o} - \varepsilon) - S(\overline{o} + \varepsilon) \right] = p(\Psi - \theta_{thres}) \tag{a.2}$$

Proof: See OSM A4.

Thus, a discontinuity in the total social esteem function emerges even in a model where individuals directly choose the level of the negative externality they impose on others. Here the effect is totally driven by esteem from distant observers. Note that a sufficient condition for (a.2) to be positive is that $\theta_{thres} < \mu_\theta$. If $f(\theta)$ is symmetric, $\theta_{thres} < \mu_\theta$ implies that the majority of people select legal actions in equilibrium. Finally, clearly enough, the case where the law is absent is here isomorphic to a situation where $p \to 0$. In that case, Proposition A1 shows that the social esteem function exhibits no discontinuity.

[1] If this is not the case, Proposition A1 extends straightforwardly. It is easy to show that, in the D1-refined equilibrium, $\lim_{\varepsilon \to 0} \left[S(\overline{o} - \varepsilon) - S(\overline{o} + \varepsilon) \right] = p(\Psi - \theta_{min})$.

A.2. Descriptive norm

An interesting question that follows from our model is whether the size of the esteem discontinuity is simply a reflection of the prevalence of law breaking behavior. For instance, if many people are willing to marginally break the law, does that imply that we should observe a smaller downward discontinuity in esteem at the legal limit?

We now argue that this is not necessarily the case by means of a numerical illustration. Let $t = 1$, $p = 1/2$, $K = 1/20$ and $\overline{o} = 1$. We compare two distributions: (1) θ is uniformly distributed on $[0, z + y]$ (2) θ is uniformly distributed on $[x, y]$.[2] To fix Ψ in each of these two cases, we consider the benchmark case where Ψ is arbitrarily close to the mean of the distribution: $\Psi \to \mu_\theta$. This case applies for instance if, when we aggregate across all contexts, the probability that an individual may be confronted with an illegal opportunity is arbitrarily small.

With distribution 1), the size of the downward discontinuity in $\gamma(o)$ at $\overline{o}$ is 0.1 while the size of the discontinuity in $S(o)$ is 0.275. The share of individuals willing to marginally break the law is 30%. It is easy to find parameter values such that distribution 2) has *both* a lower incidence of marginal law-breaking *and* a smaller downward discontinuity in esteem than distribution 1) in both $\gamma(o)$ and $S(o)$. This occurs e.g. if $x = 0.3$, $y = 0.75$ and $z = 0.25$.[3] On the other hand, it is also possible to construct examples where distribution 2) generates less marginal law-breaking and a bigger discontinuity at the legal limit. This happens for instance if $x = 0.3$, $y = 0.5$ and $z = 0.5$.

These examples illustrate the general point that the size of the downward discontinuity in esteem from seizing a marginally illegal opportunity is not simply a mechanical reflection of the share of individuals willing to marginally break the law. Rather, the relationship between the two is more subtle and depends on the characteristics of the underlying type distribution.

A.3. Comparative Statics

We now explore how the size of the discontinuity in social esteem varies with various characteristics of law enforcement and the context. We start by looking at *police tolerance* and explore how the probability of enforcing a law and the size of the penalty associated with it affect the magnitude of the discontinuity in social esteem. We then turn to two characteristics of the context in which opportunities arise. We start by investigating the effects of the material return from taking an opportunity. We then ask whether behaviors that are less likely to be *measured accurately* and/or

[2] When θ is uniform on $[a, a + b]$, we have $\widehat{\theta}_{\overline{o}} = t - b/2$ and $\widetilde{\theta}_{\overline{o}} = (2t - b - 2p\Psi - 2Kp + pa) / (2 - p)$. The downward discontinuity in $\gamma(o)$ at $\overline{o}$ is found by computing $\frac{t-\frac{b}{2}+a}{2} - \frac{2t-b-2p\Psi-2Kp+pa+a(2-p)}{2(2-p)}$. The share of individuals who are willing to marginally break the law is given by $\frac{\widetilde{\theta}_{\overline{o}}-a}{b} = \frac{2t-b-2p\Psi-2Kp+pa-a(2-p)}{b(2-p)}$.

[3] Note that in this case distribution 2) first-order stochastically dominates distribution 1), since $F_1(\theta) = \theta > F_2(\theta) = \frac{\theta-0.3}{0.75}$.

taken intentionally are subject to larger or smaller discontinuities in social esteem.

Strength of sanctions

The discontinuity in social esteem at the legal limit is increasing in the strength of sanctions. Recall from the results in our main body that $\widehat{\theta}_{\overline{o}}$ is the highest type seizing $\overline{o}$ while $\widetilde{\theta}_{\overline{o}}$ is the highest type seizing $\overline{o}+\varepsilon$ for $\varepsilon \to 0$. From (5), a higher K decreases $\widetilde{\theta}_{\overline{o}}$ whilst leaving $\widehat{\theta}_{\overline{o}}$ unchanged. Since $\mathcal{M}^{-}(.)$ is an increasing function, this implies that $\mathcal{M}^{-}(\widehat{\theta}_{\overline{o}})-\mathcal{M}^{-}(\widetilde{\theta}_{\overline{o}})$, the downward discontinuity in the social esteem function $\gamma(o)$ at $\overline{o}$, is now more pronounced. The same applies to the discontinuity in $S(o)$, as can be seen from (6). Note that our main result carries through even if $K=0$. In this case, the social esteem discontinuity is entirely driven by esteem concerns.

Enforcement probability

The discontinuity in social esteem at the legal limit is increasing in the probability of sanctions against law-breaking being enforced. If $p=0$ then $\widehat{\theta}_{\overline{o}}$ and $\widetilde{\theta}_{\overline{o}}$ coincide and hence the discontinuity in esteem at the legal limit disappears. As p increases, $\widetilde{\theta}_{\overline{o}}$ decreases while $\widehat{\theta}_{\overline{o}}$ remains the same. As a result, the downward discontinuity in $\gamma(o)$ at the legal limit becomes more pronounced when p is larger, and similarly for the downward discontinuity in $S(o)$. Note that, in the case of the discontinuity in $S(o)$, there is also a direct effect of a higher p, as shown by (6), going in the same direction as the indirect effect operating through $\widetilde{\theta}_{\overline{o}}$. Intuitively, this is because a higher p makes it more likely that seizing an illegal opportunity will result in a conviction and will thus be observed by distant observers, causing a drop in esteem from those observers.

Material returns from the opportunity

The effect of a higher t for the discontinuity in $\gamma(o)$ is ambiguous. Intuitively, higher t makes seizing any given externality-generating opportunity more attractive. As a result, both $\widehat{\theta}_{\overline{o}}$ and $\widetilde{\theta}_{\overline{o}}$ increase, exerting countervailing forces on the size of the discontinuity. This implies that our theory does not deliver unambiguous predictions about the effect of a change in t for the downward esteem discontinuity. We can however make predictions that are contingent of the underlying distribution of θ. For instance, if θ is uniformly or normally distributed, then a higher t unambiguously implies a smaller discontinuity in $\gamma(o)$ and $S(o)$ at the legal limit.

Intentionality/measurability of behavior

Sometimes individuals may cause a negative externality unintentionally. To capture the key effects introduced by this possibility, suppose that, whenever an individual takes or leaves an opportunity

o, there is a probability that this might have happened *inadvertently*. Set $p = 1$ for simplicity. Suppose that close observers are able to observe intentionality, and therefore assign esteem μ_θ if an individual seizes o unintentionally and esteem $\gamma(o)$ if the action is intentional. Distant observers, on the other hand, are not able to distinguish intentionality. When an individual is caught breaking the law, distant observers cannot establish whether this happened intentionally or inadvertently.[4] This implies that the esteem this individual obtains from distant observers is now given by

$$\gamma^{dis}(o) = (1-q)\,\mathcal{M}^{-}(\theta_o) + q\mu_\theta \tag{a.3}$$

where q is the probability that the opportunity was taken/left unintentionally.

Consider then the decision of an individual to intentionally seize o. The case analyzed in the main text above is a special case of this more general framework, in which $q = 0$. In this more general setup, $\widetilde{\theta}_o$ is defined by:

$$t - l(\widetilde{\theta}_o) - \widetilde{\theta}_o o - \Delta(\widetilde{\theta}_o) = 0. \tag{a.4}$$

where $l(\widetilde{\theta}_o) \equiv K + \Psi - (1-q)\,\mathcal{M}^{-}(\widetilde{\theta}_o) - q\mu_\theta > 0$. It is easy to show that $\widetilde{\theta}_o$ is increasing in q, while $\widehat{\theta}_o$ is given by (3) as before and is thus independent of q. Allowing for $q > 0$ affects social esteem in two ways. First, behavior is now less informative about an individual's underlying type and, as a result, esteem from distant observers depends less strongly on behavior, as highlighted by (a.3). Second, since esteem depends less strongly on behavior, people are now willing to (intentionally) take opportunities characterized by a higher o, which they would have left if $q = 0$. In other words, equilibrium behavior changes, which further affects the esteem consequences from seizing a given opportunity.

Proposition A2 *Both* $\lim_{\varepsilon\to 0}\left[\gamma\left(\overline{o}-\varepsilon\right) - \gamma\left(\overline{o}+\varepsilon\right)\right]$ *and* $\lim_{\varepsilon\to 0}\left[S\left(\overline{o}-\varepsilon\right) - S\left(\overline{o}+\varepsilon\right)\right]$ *are strictly decreasing in* q.
Proof: See OSM A.4.

Note that, as highlighted in proposition A2, q also affects the esteem from close observers, who, differently from distant observers, are *perfectly aware* that the subject seizing the opportunity is doing so in a fully intentional manner. Intuitively, although close observers forming beliefs about an individual's prosociality parameter know that the individual is seizing the opportunity intentionally, the fact that *distant observers* are unable to discern the individual's intentions changes the individual's behavior. This is reflected in threshold values depending on q, as described in (a.4). As a result, the downward discontinuity in $\gamma(o)$ at the legal limit is also decreasing in q.

[4] Alternatively we could assume that both close and distant observers cannot observe intentionality. Our results would continue to apply under intuitive restrictions on the distribution of θ (e.g., that θ is normally or uniformly distributed).

The setup we have sketched here can also be used to describe an environment where illegal behavior can only be measured with a margin of error, implying that, when an individual is convicted for breaking the law, distant observers know that, with some probability q, this may be the result of a measurement error.

A.4. Proofs

Proof of Proposition 2.1 Taking Δ as given, the net payoff from seizing o is strictly decreasing in θ. Condition (3) then follows straightforwardly from the indifference requirement. To prove that $\widehat{\theta}_o$ is interior, suppose first that all types seize opportunity o, so that $\Delta \geq \theta_{\max} - \mu_\theta$.[5] For $\theta_{\max}$, the net utility from seizing o is $t - \theta_{\max} o + \mu_\theta - \theta_{\max} < 0$, where the inequality follows from our assumption that $t - \theta_{\max} o_{\min} + \mu_\theta - \theta_{\max} < 0$. This leads to a contradiction. Consider now the case where all types leave opportunity o, so that $\Delta \leq \mu_\theta - \theta_{\min}$.[6] For $\theta_{\min}$, the net utility from seizing o is $t - \theta_{\min} o - \mu_\theta + \theta_{\min} > 0$, where the inequality follows from our assumption that $t - \theta_{\min} o_{\max} - \mu_\theta + \theta_{\min} > 0$, again a contradiction. Note that, since the distribution of θ has full support, $\widehat{\theta}_o$ is a continuous function of o and hence $\gamma(o)$ is also continuous. Finally, to show that $\widehat{\theta}_o$ is strictly decreasing in o, note that by total differentiation of (3) we obtain

$$\frac{d\widehat{\theta}_o}{do} = -\frac{\widehat{\theta}_o}{o + \Delta'(\widehat{\theta}_o)} < 0$$

where the inequality follows from our assumption that, for all θ, $o_{\min} + \Delta'(\theta) - \mathcal{M}^{-\prime}(\theta) > 0$ and the observation that $\mathcal{M}^{-\prime}(\theta) > 0$.[7] This last observation also proves that $\gamma(o)$ is strictly decreasing in o. ■

Proof of Proposition 2.2 We only have to prove that $\widehat{\theta}_o > \widetilde{\theta}_o$ for all $p > 0$. Note that, evaluated at $p = 0$, $\widetilde{\theta}_o = \widehat{\theta}_o$. To prove that $\widehat{\theta}_o > \widetilde{\theta}_o$ it is thus sufficient to prove that $\widetilde{\theta}_o$ is decreasing in p. Total differentiation of (5) gives

$$\frac{d\widetilde{\theta}_o}{dp} = -\frac{\widetilde{\theta}_o}{o + \Delta'(\widetilde{\theta}_o) - p\mathcal{M}^{-\prime}(\widetilde{\theta}_o)} < 0$$

where the inequality follows from our assumption that, for all θ, $o_{\min} + \Delta'(\theta) - \mathcal{M}^{-\prime}(\theta) > 0$ and the observation that $\mathcal{M}^{-\prime}(\theta) > 0$.. ■

Proof of Proposition A1 The proof is adapted from Birke (2020) and is divided in steps.

[5] In this case, leaving o is an out-of-equilibrium action and hence beliefs following it are not well defined, implying that Δ is also not well defined. However, to prove the result it is sufficient to show that it holds for Δ's upper bound.

[6] Similar to the case above, here seizing o is an out-of-equilibrium action, and hence Δ is not well defined. However, it is sufficient to show that the result holds for Δ's lower bound.

[7] Note that, as proved in Jewitt (2004), although $\Delta'(\theta)$ can be negative, its absolute value is bounded.

Step 1 Incentive Compatibility requires that, in equilibrium, $o(\theta)$ is non-increasing: for any θ_1 and θ_0 that satisfy $\theta_1 > \theta_0$, we have $\sup o(\theta_1) \leq \inf o(\theta_0)$. As a result, $\gamma(o)$ is also non-increasing.
Proof of step 1: let $\theta_1 > \theta_0$, and consider o_1 and o_0 that satisfy $o_1 \in o(\theta_1)$ and $o_0 \in o(\theta_0)$. For $i \in \{0,1\}$, let $r_i = 0$ if $o_i \leq \overline{o}$ and $r_i = p$ if $o_i > \overline{o}$. IC requires

$$u(o_0, r_0; \theta_0) \geq u(o_1, r_1; \theta_0)$$
$$u(o_1, r_1; \theta_1) \geq u(o_0, r_0; \theta_1)$$

where $u(o, r; \theta) \equiv to - \theta\frac{o^2}{2} + \gamma(o) + r(\gamma(o) - K) + (1-r)\Psi$. Rearranging, this can be rewritten as

$$\frac{1}{2}\theta_1(o_0^2 - o_1^2) \geq G \geq \frac{1}{2}\theta_0(o_0^2 - o_1^2) \tag{a.5}$$

where $G \equiv t(o_0 - o_1) + (1 + r_0)\gamma(o_0) - (1 + r_1)\gamma(o_1) + (\Psi + K)(r_1 - r_0)$. Condition (a.5) implies $(\theta_1 - \theta_0)(o_0^2 - o_1^2) \geq 0$ and, hence, $o_0 \geq o_1$.

Step 2 $\theta_1 > \theta_0$ has a lower incentive than θ_0 to deviate to $o' > o_0$, and a larger incentive than θ_0 to deviate to $o'' < o_1$.
Proof of step 2: Consider first a deviation to $o' > o_0$. The net gain obtained by θ_0 from the deviation is

$$u(o', r'; \theta_0) - u(o_0, r_0; \theta_0) \tag{a.6}$$

The net gain obtained by θ_1 from the deviation is

$$\begin{aligned} & u(o', r'; \theta_1) - u(o_0, r_0; \theta_1) + u(o_0, r_0; \theta_1) - u(o_1, r_1; \theta_1) && \text{(a.7)} \\ \leq\ & u(o', r'; \theta_1) - u(o_0, r_0; \theta_1) && \text{(a.8)} \end{aligned}$$

where the inequality follows since, by incentive compatibility, $u(o_0, r_0; \theta_1) - u(o_1, r_1; \theta_1) \leq 0$. To prove the result it remains to show that

$$\begin{aligned} u(o', r'; \theta_0) - u(o_0, r_0; \theta_0) &\geq u(o', r'; \theta_1) - u(o_0, r_0; \theta_1) && \text{(a.9)} \\ \text{i.e. } u(o', r'; \theta_0) - u(o', r'; \theta_1) &\geq u(o_0, r_0; \theta_0) - u(o_0, r_0; \theta_1) && \text{(a.10)} \end{aligned}$$

Substituting, this requires

$$\frac{1}{2}(o')^2(\theta_1 - \theta_0) \geq \frac{1}{2}o_0^2(\theta_1 - \theta_0)$$

which always holds since $\theta_1 - \theta_0 > 0$ and $o' > o_0$. This proves that θ_1 has a lower incentive than θ_0 to deviate to $o' > o_0$. The proof to show that θ_1 has a higher incentive than θ_0 to deviate to $o'' < o_1$ is analogous and is therefore omitted.

Step 3 In equilibrium, there cannot be pooling at any $o > 0$.
Proof of step 3: Suppose that there is $o > 0$ at which types $[\theta_0, \theta_1]$ pool. Consider $o - \varepsilon$, where ε is arbitrarily small. If this action is taken in equilibrium, then $\gamma(o - \varepsilon) > \theta_1 > \gamma(o)$. If this action is not taken in equilibrium, then by the argument in step 2, the D1-refined belief following a deviation to $o - \varepsilon$ is that $\theta \geq \theta_1$. In both cases, types in $[\theta_0, \theta_1]$ would have an incentive to deviate to $o - \varepsilon$. This proves that any $o > 0$ is selected by at most one type in equilibrium.

Step 4 There exists $\varepsilon_0 > 0$ such that, in equilibrium, nobody selects $\overline{o} + \varepsilon$ for all $\varepsilon < \varepsilon_0$.
Proof of step 4: Suppose that this is not the case. For any type θ, the net gain from selecting $\overline{o}$ instead of $\overline{o} + \varepsilon$ is

$$[t\overline{o} - \theta\frac{\overline{o}^2}{2} + \gamma(\overline{o}) + \Psi] - [t(\overline{o} + \varepsilon) - \theta\frac{(\overline{o} + \varepsilon)^2}{2} + \gamma(\overline{o} + \varepsilon) + p(\gamma(\overline{o} + \varepsilon) - K) + (1 - p)\Psi]$$

When $\varepsilon \to 0$ this becomes

$$\gamma(\overline{o}) - \gamma(\overline{o} + \varepsilon) + p[\Psi + K - \gamma(\overline{o} + \varepsilon)] > 0 \tag{a.11}$$

where the inequality follows from (a.1) and the fact that, by step 1 and step 3, $\gamma(\overline{o}) > \gamma(\overline{o} + \varepsilon)$. This proves that, for ε sufficiently small, any type θ who selects $\overline{o} + \varepsilon$ would have an incentive to deviate to $\overline{o}$.

Denote as θ_{thres} the type who selects $\overline{o}$ in equilibrium. If $\theta_{thres} > \theta_{min}$, then there must be a $o_{thres} > \overline{o}$ such that θ_{thres} is indifferent between $\overline{o}$ and o_{thres}, and randomizes between the two in equilibrium. In this case the out-of-equilibrium belief upon observing $\overline{o} + \varepsilon < o_{thres}$ is not covered by step 2 since $\overline{o} + \varepsilon$ is both higher than $\overline{o}$ and lower than o_{thres}. Conditional on $\theta > \theta_{thres}$, the net gain from deviating to $\overline{o} + \varepsilon$ is

$$\begin{aligned} &t\overline{o} - \theta\frac{\overline{o}^2}{2} + \gamma(\overline{o} + \varepsilon) + p(\gamma(\overline{o} + \varepsilon) - K) + (1 - p)\Psi \\ &-[to(\theta) - \theta\frac{o(\theta)^2}{2} + \theta + \Psi] \end{aligned} \tag{a.12}$$

where $o(\theta) < \overline{o}$ is θ's equilibrium action and $\gamma(o) = \theta$. The derivative of (a.12) with respect to θ is

$$\frac{o(\theta)^2}{2} - \frac{\overline{o}^2}{2} - 1 + (\theta o(\theta) - t)\, o'(\theta) \tag{a.13}$$

By optimality, $o(\theta)$ satisfies $t - \theta o + \gamma'(o) = 0$, implying that $\theta = \frac{t+\gamma'(o)}{o}$ and, hence, (since $\gamma(o) = \theta$)

$$\theta o = t + \frac{d\theta}{de} \text{ so that } o'(\theta) = \frac{1}{\theta o(\theta) - t} < 0.$$

Substituting in (a.13) we obtain $\frac{o(\theta)^2}{2} - \frac{\overline{o}^2}{2} < 0$, implying that θ_{thres} experiences a greater net gain from deviating to $\overline{o} + \varepsilon$ than any other $\theta > \theta_{thres}$. Consider now $\theta < \theta_{thres}$. The net gain from deviating to $\overline{o} + \varepsilon$ is

$$t\overline{o} - \theta\frac{\overline{o}^2}{2} + \gamma(\overline{o} + \varepsilon)(1 + p) - [to(\theta) - \theta\frac{o(\theta)^2}{2} + \theta(1 + p)] \tag{a.14}$$

where $o(\theta) > o_{thres}$ is θ's equilibrium action and $\gamma(o) = \theta$. The derivative of (a.14) with respect to θ gives

$$\frac{o(\theta)^2}{2} - \frac{\overline{o}^2}{2} - (1 + p) + (\theta o(\theta) - t)\, o'(\theta) \tag{a.15}$$

By optimality, $o(\theta)$ satisfies $t - \theta o + \gamma'(o)(1 + p) = 0$, implying that $\theta = \frac{t+\gamma'(o)(1+p)}{o}$ and, hence,

$$o'(\theta) = \frac{1 + p}{\theta o(\theta) - t} < 0.$$

Substituting in (a.15) delivers $\frac{o(\theta)^2}{2} - \frac{\overline{o}^2}{2} > 0$, implying that θ_{thres} has a greater net gain from deviating to $\overline{o} + \varepsilon$ than any other $\theta < \theta_{thres}$. This proves that the D1-refined belief upon observing $\overline{o} + \varepsilon$ is that $\theta = \theta_{thres}$. We now prove that the D1-refined belief upon observing $\overline{o} - \varepsilon$ tends to θ_{thres} as $\varepsilon \to 0$. If $\overline{o} - \varepsilon$ is played in equilibrium this is straightforward and follows from full separation and monotonicity of $o(\theta)$. If $\overline{o} - \varepsilon$ is not played in equilibrium, then the result is derived by applying Step 2 above. Overall, this implies that

$$\begin{aligned} \lim_{\varepsilon \to 0} [S(\overline{o} - \varepsilon) - S(\overline{o} + \varepsilon)] &= \Psi - p\theta_{thres} - (1 - p)\Psi \\ &= p(\Psi - \theta_{thres}) \end{aligned}$$

as described in the proposition. ■

Proof of Proposition A2: We denote the threshold types in the modified setup with $q > 0$ as $\widehat{\theta}_o^I$ and $\widetilde{\theta}_o^I$. For close observers,

$$\lim_{\varepsilon \to 0} \left[\gamma \left(\overline{o} - \varepsilon \right) - \gamma \left(\overline{o} + \varepsilon \right) \right] = \mathcal{M}^{-}(\widehat{\theta}_o^I) - \mathcal{M}^{-}(\widetilde{\theta}_o^I)$$

The derivative wrt q is

$$-\mathcal{M}^{-\prime}(\widetilde{\theta}_{\overline{o}}^I) \frac{\partial \widetilde{\theta}_{\overline{o}}^I}{\partial q}$$

where $\mathcal{M}^{-\prime}(.) \geq 0$. For the expression to be negative, we require $\frac{\partial \widetilde{\theta}_{\overline{o}}^I}{\partial q} > 0$. By totally differentiating (a.4) we obtain

$$\frac{\partial \widetilde{\theta}_{\overline{o}}^I}{\partial q} = \frac{\mu_\theta - \mathcal{M}^{-}(\widetilde{\theta}_{\overline{o}}^I) + \Delta(\widetilde{\theta}_{\overline{o}}^I)}{\overline{o} + (1-q) \left[\Delta'(\widetilde{\theta}_{\overline{o}}^I) - \mathcal{M}'^{-}(\widetilde{\theta}_{\overline{o}}^I) \right]} > 0$$

where the inequality follows from our assumption that, for all θ, $o_{\min} + \Delta'(\theta) - \mathcal{M}^{-\prime}(\theta) > 0$. We now look at total esteem.

$$\lim_{\varepsilon \to 0} \left[S \left(\overline{o} - \varepsilon \right) - S \left(\overline{o} + \varepsilon \right) \right] = \left[\mathcal{M}^{-}(\widehat{\theta}_o^I) - \mathcal{M}^{-}(\widetilde{\theta}_o^I) \right] - (1-q) \mathcal{M}^{-}(\widetilde{\theta}_o^I) + \Psi - q\mu_\theta$$

We have established that the expression in square brackets is decreasing in q. Consider now $-(1-q)\mathcal{M}^{-}(\widetilde{\theta}_o^I) + \Psi - q\mu_\theta$. The derivative wrt q is

$$\mathcal{M}^{-}(\widetilde{\theta}_{\overline{o}}^I) - \mu_\theta - (1-q)\, \mathcal{M}^{-\prime}(\widetilde{\theta}_{\overline{o}}^I) \frac{\partial \widetilde{\theta}_{\overline{o}}^I}{\partial q} \tag{a.16}$$

which is again negative given $\mathcal{M}^{-}(\widetilde{\theta}_{\overline{o}}^I) < \mu_\theta$. ■.

B. Vignettes used in the experiments

The five vignettes we chose to investigate describe different types of behavior, all of which are illegal only if particular thresholds are crossed. The five behaviors to be evaluated were: 1) an older adult having sex with a person just below or above the legal age of consent; 2) selling alcohol to a youth who is known to be a vandal who is just below or above the age at which a person can legally be sold alcohol; 3) entering one's country with an amount of cash just below or above the threshold at which it must legally be declared to customs, and not declaring it; 4) driving with a blood-alcohol level just below or above the legal limit; 5) driving at a speed just below or above the legal speed limit.

These behaviors were chosen because each is subject to a legal threshold in both the UK and China (although for some of them the threshold is set at different levels in each country). Moreover, we wanted to select behaviors which, in their legal version, would cover a range of positions across the social appropriateness scale. For instance, while it may be viewed as morally dubious – even when such behavior is legal – for an older adult to have sex with a younger person, or for someone to sell alcohol to a youth (especially if they are a known local vandal), it is unlikely that anyone would consider it inappropriate to drive just below the legal speed limit, or carry a large but legal amount of cash undeclared through customs.

All five of the vignettes are constructed such that subjects are made aware of the legal threshold, and in all cases the characters whose behavior they are evaluating also know whether their behavior is legal or illegal.

B.1. Main experiment, Prosocial traits experiment and Rule of law experiment

Below, we present the vignettes from the main experiment (Section 3 of the main paper), prosocial traits experiment (Section 5.2) and rule of law experiment (Section 5.3). We present first the wordings of the vignettes, which were consistent across these experiments apart from the minor changes indicated and the fact that in second-order beliefs versions of experiments the vignettes were preceded by a statement informing subjects that this text had previously been shown to another set of participants. We then display the questions which were posed to subjects underneath the vignettes, which differed across the experiments.

Where two wordings appear in parentheses, the wording on the left applies to the UK experiments and the wording to the right to the China (rule of law) experiment.

AGE OF CONSENT VIGNETTE

Wording: A (20/18) year old man meets a girl at a party. The man invites the girl to come to his

home, and she agrees. At his home, the man tells the girl he wants to have sex with her, but that she looks young, and asks whether she is above the legal age of consent of (16/14) years. The girl tells the man that she is aged (Age)*, and shows him an ID card which confirms this. She tells the man that she wants to have sex with him. The man then has sex with the girl.

** The possible value of (Age) were: 16 years and 3 months, 16 years and 1 month, 15 years and 11 months or 15 years and 9 months for the UK student sample; 16 years and 4 months, 16 years and 3 months, 16 years and 2 months, 16 years and 1 month, 15 years and 11 months, 15 years and 10 months, 15 years and 9 months or 15 years and 8 months for the other UK samples; 14 years and 3 months, 14 years and 1 month, 13 years and 11 months or 13 years and 9 months for the China sample.*

Questions

Main experiment, Krupka-Weber method / Rule of Law experiment: How socially appropriate would most people think it is for the man to have sex with the girl? (Possible answers: Very socially appropriate; Somewhat socially appropriate; Somewhat socially inappropriate; Very socially inappropriate)

Main experiment, Opinion matching method, first-order beliefs: How appropriate do you think it is for the man to have sex with the girl? (Possible answers: Very appropriate; Somewhat appropriate; Somewhat inappropriate; Very inappropriate)

Main experiment, Opinion matching method, second-order beliefs: Participants were asked: "How appropriate do you think it is for the man to have sex with the girl?"

What do you predict was the most common answer to that question? (Possible answers: Very appropriate; Somewhat appropriate; Somewhat inappropriate; Very inappropriate)

Prosocial traits experiment, first-order beliefs: Please answer the following questions.

1) How likely is this man to keep a promise made to a friend? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

2) How likely is it that this man reads at least two books per month? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

3) How likely is it that this man keeps a healthy diet, avoiding fatty foods and refined sugar? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

4) If a cashier accidentally gave this man more change than he was due, how likely would the man be to return the extra change? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

5) How likely is it that this man keeps fit by regularly going to the gym? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

6) How likely is it that this man volunteers for a charitable organisation? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

Prosocial traits experiment, second-order beliefs: Please answer the following questions.

1) Participants were asked: ”How likely is this man to keep a promise made to a friend?”

What do you predict was the most common answer to that question? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

2) Participants were asked: ”How likely is it that this man reads at least two books per month?”

What do you predict was the most common answer to that question? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

3) Participants were asked: ”How likely is it that this man keeps a healthy diet, avoiding fatty foods and refined sugar?”

What do you predict was the most common answer to that question? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

4) Participants were asked: ”If a cashier accidentally gave this man more change than he was due, how likely would the man be to return the extra change?”

What do you predict was the most common answer to that question? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

5) Participants were asked: ”How likely is it that this man keeps fit by regularly going to the gym?”

What do you predict was the most common answer to that question? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

6) Participants were asked: ”How likely is it that this man volunteers for a charitable organisation?”

What do you predict was the most common answer to that question? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

ALCOHOL TO YOUTH VIGNETTE

Wording: A youth enters a local shop with the intention of buying some beer. He sees a sign in the shop reminding customers that in (Britain/China) it is illegal for shopkeepers to sell alcohol to people younger than 18 years. The shopkeeper knows the youth personally, and knows that he is aged (Age)*. The shopkeeper knows that the youth often gets drunk and vandalises property in his neighbourhood. The youth, who appears sober, asks to buy a box containing 20 alcoholic beers, and the shopkeeper sells it to him.

**The possible value of (Age) were: 18 years and 3 months, 18 years and 1 month, 17 years and 11 months or 17 years and 9 months for the UK student and China samples; 18 years and 4 months, 18 years and 3 months, 18 years and 2 months, 18 years and 1 month, 17 years and 11 months, 17 years and 10 months, 17 years and 9 months or 17 years and 8 months for the other UK samples.*

Questions

Main experiment, Krupka-Weber method / Rule of Law experiment: How socially appropriate would most people think it is for the shopkeeper to sell the beers to the youth? (Possible answers: Very socially appropriate; Somewhat socially appropriate; Somewhat socially inappropriate; Very socially inappropriate)

Main experiment, Opinion matching method, first-order beliefs: How appropriate do you think it is for the shopkeeper to sell the beers to the youth? (Possible answers: Very appropriate; Somewhat appropriate; Somewhat inappropriate; Very inappropriate)

Main experiment, Opinion matching method, second-order beliefs: Participants were asked: "How appropriate do you think it is for the shopkeeper to sell the beers to the youth?"

What do you predict was the most common answer to that question? (Possible answers: Very appropriate; Somewhat appropriate; Somewhat inappropriate; Very inappropriate)

Prosocial traits experiment, first-order beliefs: Please answer the following questions.

1) How likely is this shopkeeper to keep a promise made to a friend? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

2) How likely is it that this shopkeeper reads at least two books per month? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

3) How likely is it that this shopkeeper keeps a healthy diet, avoiding fatty foods and refined sugar? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

4) If a cashier accidentally gave this shopkeeper more change than the shopkeeper was due, how likely would the shopkeeper be to return the extra change? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

5) How likely is it that this shopkeeper keeps fit by regularly going to the gym? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

6) How likely is it that this shopkeeper volunteers for a charitable organisation? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

Prosocial traits experiment, second-order beliefs: Please answer the following questions.

1) Participants were asked: "How likely is this shopkeeper to keep a promise made to a friend?"

What do you predict was the most common answer to that question? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

2) Participants were asked: "How likely is it that this shopkeeper reads at least two books per month?"

What do you predict was the most common answer to that question? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

3) Participants were asked: "How likely is it that this shopkeeper keeps a healthy diet, avoiding fatty foods and refined sugar?"

What do you predict was the most common answer to that question? (Possible answers: Very

likely; Somewhat likely; Somewhat unlikely; Very unlikely)

4) Participants were asked: ”If a cashier accidentally gave this shopkeeper more change than the shopkeeper was due, how likely would the shopkeeper be to return the extra change?”

What do you predict was the most common answer to that question? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

5) Participants were asked: ”How likely is it that this shopkeeper keeps fit by regularly going to the gym?”

What do you predict was the most common answer to that question? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

6) Participants were asked: ”How likely is it that this shopkeeper volunteers for a charitable organisation?”

What do you predict was the most common answer to that question? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

CASH AT CUSTOMS VIGNETTE

Wording: A man is returning to (Britain/China) from an overseas holiday. In his suitcase he is carrying cash worth (Amount)*. In the airport he notices a sign informing passengers that it is illegal to bring cash worth more than (10,000 Euros into Britain / 5,000 US dollars into China) from overseas without declaring it to customs. After reading the sign, the man enters the country without declaring the cash to customs.

**The possible values of (Amount) were 9,700, 9,900, 10,100 or 10,300 Euros for the UK student sample; 9,600, 9,700, 9,800, 9,900, 10,100, 10,200, 10,300 or 10,400 Euros for the other UK samples; $4,700, $4,900, $5,100 or $5,300 for the China sample.*

Questions

Main experiment, Krupka-Weber method / Rule of Law experiment: How socially appropriate would most people think it is for the man to enter the country without declaring the cash to customs? (Possible answers: Very socially appropriate; Somewhat socially appropriate; Somewhat socially inappropriate; Very socially inappropriate)

Main experiment, Opinion matching method, first-order beliefs: How appropriate do you think it is for the man to enter the country without declaring the cash to customs? (Possible answers: Very appropriate; Somewhat appropriate; Somewhat inappropriate; Very inappropriate)

Main experiment, Opinion matching method, second-order beliefs: Participants were asked: ”How appropriate do you think it is for the man to enter the country without declaring the cash to customs?”

What do you predict was the most common answer to that question? (Possible answers: Very appropriate; Somewhat appropriate; Somewhat inappropriate; Very inappropriate)

Prosocial traits experiment, first-order beliefs: Please answer the following questions.

1) How likely is this man to keep a promise made to a friend? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

2) How likely is it that this man reads at least two books per month? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

3) How likely is it that this man keeps a healthy diet, avoiding fatty foods and refined sugar? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

4) If a cashier accidentally gave this man more change than he was due, how likely would the man be to return the extra change? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

5) How likely is it that this man keeps fit by regularly going to the gym? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

6) How likely is it that this man volunteers for a charitable organisation? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

Prosocial traits experiment, second-order beliefs: Please answer the following questions.

1) Participants were asked: ”How likely is this man to keep a promise made to a friend?”

What do you predict was the most common answer to that question? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

2) Participants were asked: ”How likely is it that this man reads at least two books per month?”

What do you predict was the most common answer to that question? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

3) Participants were asked: ”How likely is it that this man keeps a healthy diet, avoiding fatty foods and refined sugar?”

What do you predict was the most common answer to that question? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

4) Participants were asked: ”If a cashier accidentally gave this man more change than he was due, how likely would the man be to return the extra change?”

What do you predict was the most common answer to that question? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

5) Participants were asked: ”How likely is it that this man keeps fit by regularly going to the gym?”

What do you predict was the most common answer to that question? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

6) Participants were asked: ”How likely is it that this man volunteers for a charitable organisation?”

What do you predict was the most common answer to that question? (Possible answers: Very

likely; Somewhat likely; Somewhat unlikely; Very unlikely)

DRINK DRIVING VIGNETTE

Wording: A woman works for a company which manufactures state-of-the-art breathalysers, machines which can measure a person's blood alcohol content with extremely high accuracy. One day, after drinking in a bar in (City)*, the woman remembers she has one of the breathalysers in her bag, and wonders whether her blood alcohol content is below (0.08% / 0.02%), the maximum level at which a person can legally drive in (England/China). She tests herself and discovers that her blood alcohol content is (Percentage)**. The woman then drives home.

**(City) was 'Nottingham' for the UK student sample, 'Ningbo' for the China sample, and 'a city in England' for the other UK samples. Note that we specified 'England' rather than 'Britain' because, unlike the laws featured in the other scenarios, drink-driving laws differ across the constituent countries of the UK. **The possible values of (Percentage) were: 0.077%, 0.079%, 0.081% or 0.083% for the UK student sample; 0.076%, 0.077%, 0.078%, 0.079z%, 0.081%, 0.082%, 0.083% or 0.084% for the other UK samples; 0.017%, 0.019%, 0.021% or 0.023% for the China sample.*

Questions

Main experiment, Krupka-Weber method / Rule of Law experiment: How socially appropriate would most people think it is for the woman to drive home? (Possible answers: Very socially appropriate; Somewhat socially appropriate; Somewhat socially inappropriate; Very socially inappropriate)

Main experiment, Opinion matching method, first-order beliefs: How appropriate do you think it is for the woman to drive home? (Possible answers: Very appropriate; Somewhat appropriate; Somewhat inappropriate; Very inappropriate)

Main experiment, Opinion matching method, second-order beliefs: Participants were asked: "How appropriate do you think it is for the woman to drive home?"

What do you predict was the most common answer to that question? (Possible answers: Very appropriate; Somewhat appropriate; Somewhat inappropriate; Very inappropriate)

Prosocial traits experiment, first-order beliefs: Please answer the following questions.

1) How likely is this woman to keep a promise made to a friend? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

2) How likely is it that this woman reads at least two books per month? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

3) How likely is it that this woman keeps a healthy diet, avoiding fatty foods and refined sugar? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

4) If a cashier accidentally gave this woman more change than she was due, how likely would the

woman be to return the extra change? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

5) How likely is it that this woman keeps fit by regularly going to the gym? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

6) How likely is it that this woman volunteers for a charitable organisation? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

Prosocial traits experiment, second-order beliefs: Please answer the following questions.

1) Participants were asked: ”How likely is this woman to keep a promise made to a friend?”

What do you predict was the most common answer to that question? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

2) Participants were asked: ”How likely is it that this woman reads at least two books per month?”

What do you predict was the most common answer to that question? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

3) Participants were asked: ”How likely is it that this woman keeps a healthy diet, avoiding fatty foods and refined sugar?”

What do you predict was the most common answer to that question? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

4) Participants were asked: ”If a cashier accidentally gave this woman more change than she was due, how likely would the woman be to return the extra change?”

What do you predict was the most common answer to that question? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

5) Participants were asked: ”How likely is it that this woman keeps fit by regularly going to the gym?”

What do you predict was the most common answer to that question? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

6) Participants were asked: ”How likely is it that this woman volunteers for a charitable organisation?”

What do you predict was the most common answer to that question? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

SPEEDING VIGNETTE

Wording: A woman is driving between two cities in order to attend a meeting. She turns onto a road and notices a sign informing motorists that the legal speed limit on the road is (70 miles per hour / 120 kilometres per hour). The woman drives for the next five minutes at (Speed)*, before turning onto a different road.

*The possible values of (Speed) were: 67, 69, 71 or 73 miles per hour for the UK student sample; 66, 67, 68, 69, 71, 72, 73 or 74 miles per hour for the other UK samples; 117, 119, 121, 123 kilometers per hour for the China sample.

Questions

Main experiment, Krupka-Weber method / Rule of Law experiment: How socially appropriate would most people think it is for the woman to drive at (Speed)? (Possible answers: Very socially appropriate; Somewhat socially appropriate; Somewhat socially inappropriate; Very socially inappropriate)

Main experiment, Opinion matching method, first-order beliefs: How appropriate do you think it is for the woman to drive at (Speed)? (Possible answers: Very appropriate; Somewhat appropriate; Somewhat inappropriate; Very inappropriate)

Main experiment, Opinion matching method, second-order beliefs: Participants were asked: "How appropriate do you think it is for the woman to drive at (Speed)?"

What do you predict was the most common answer to that question? (Possible answers: Very appropriate; Somewhat appropriate; Somewhat inappropriate; Very inappropriate)

Prosocial traits experiment, first-order beliefs: Please answer the following questions.

1) How likely is this woman to keep a promise made to a friend? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

2) How likely is it that this woman reads at least two books per month? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

3) How likely is it that this woman keeps a healthy diet, avoiding fatty foods and refined sugar? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

4) If a cashier accidentally gave this woman more change than she was due, how likely would the woman be to return the extra change? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

5) How likely is it that this woman keeps fit by regularly going to the gym? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

6) How likely is it that this woman volunteers for a charitable organisation? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

Prosocial traits experiment, second-order beliefs: Please answer the following questions.

1) Participants were asked: "How likely is this woman to keep a promise made to a friend?"

What do you predict was the most common answer to that question? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

2) Participants were asked: "How likely is it that this woman reads at least two books per month?"

What do you predict was the most common answer to that question? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

3) Participants were asked: ”How likely is it that this woman keeps a healthy diet, avoiding fatty foods and refined sugar?”

What do you predict was the most common answer to that question? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

4) Participants were asked: ”If a cashier accidentally gave this woman more change than she was due, how likely would the woman be to return the extra change?”

What do you predict was the most common answer to that question? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

5) Participants were asked: ”How likely is it that this woman keeps fit by regularly going to the gym?”

What do you predict was the most common answer to that question? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

6) Participants were asked: ”How likely is it that this woman volunteers for a charitable organisation?”

What do you predict was the most common answer to that question? (Possible answers: Very likely; Somewhat likely; Somewhat unlikely; Very unlikely)

B.2. Placebo experiment

The wordings of the vignettes were identical across the three samples among which we conducted the placebo experiment (Section 5.1 of the main paper), except that for the second-order beliefs sample of the opinion matching version we first explained that the text had been presented to an earlier set of participants. The questions asked to subjects differed across versions, as indicated below.

AGE OF CONSENT VIGNETTE

Wording: A 20 year old man meets a girl at a party. The man invites the girl to come to his home, and she agrees. At his home, the man tells the girl he wants to have sex with her, but that she looks young, and asks whether she is above the legal age of consent of 16 years. The girl tells the man that she is aged (Age)*, and shows him an ID card which confirms this. She tells the man that she wants to have sex with him. The man remembers a recent conversation with a group of friends, who expressed the opinion that it would be appropriate to have sex with any girl so long as she was above the age of 15 years and 6 months, and inappropriate otherwise. The man then has sex with the girl.

** The possible value of (Age) were 16 years and 4 months, 16 years and 3 months, 16 years and 2 months, 16 years and 1 month, 15 years and 11 months, 15 years and 10 months, 15 years and 9 months or 15 years and 8 months, 15 years and 7 months, 15 years and 5 months, 15 years and 4 months, 15 years and 3 months, 15 years and 2 months.*

Questions

Krupka-Weber method: How socially appropriate would most people think it is for the man to have sex with the girl? (Possible answers: Very socially appropriate; Somewhat socially appropriate; Somewhat socially inappropriate; Very socially inappropriate)

Opinion matching method, first-order beliefs: How appropriate do you think it is for the man to have sex with the girl? (Possible answers: Very appropriate; Somewhat appropriate; Somewhat inappropriate; Very inappropriate)

Opinion matching method, second-order beliefs: Participants were asked: ”How appropriate do you think it is for the man to have sex with the girl?” What do you predict was the most common answer to that question? (Possible answers: Very appropriate; Somewhat appropriate; Somewhat inappropriate; Very inappropriate)

ALCOHOL TO YOUTH VIGNETTE

Wording: A youth enters a local shop with the intention of buying some beer. He sees a sign in the shop reminding customers that in Britain it is illegal for shopkeepers to sell alcohol to people younger than 18 years. The shopkeeper knows the youth personally, and knows that he is aged (Age)*. The shopkeeper knows that the youth often gets drunk and vandalises property in his neighbourhood. The youth, who appears sober, asks to buy a box containing 20 alcoholic beers. The shopkeeper remembers recently visiting the website of a campaign group, who were arguing that it is appropriate to allow people to drink alcohol when at least 6 months have passed after their 18th birthday, and inappropriate otherwise. The shopkeeper then sells the beers to the youth.

**The possible value of (Age) were 18 years and 10 months, 18 years and 9 months, 18 years and 8 months, 18 years and 7 month, 18 years and 5 months, 18 years and 4 months, 18 years and 3 months, 18 years and 2 months, 18 years and 1 month, 17 years and 11 months, 17 years and 10 months, 17 years and 9 months, 17 years and 8 months.*

Questions *Krupka-Weber method*: How socially appropriate would most people think it is for the shopkeeper to sell the beers to the youth? (Possible answers: Very socially appropriate; Somewhat socially appropriate; Somewhat socially inappropriate; Very socially inappropriate)

Opinion matching method, first-order beliefs: How appropriate do you think it is for the shop-keeper to sell the beers to the youth? (Possible answers: Very appropriate; Somewhat appropriate; Somewhat inappropriate; Very inappropriate)

Opinion matching method, second-order beliefs: Participants were asked: ”How appropriate do

you think it is for the shopkeeper to sell the beers to the youth?" What do you predict was the most common answer to that question? (Possible answers: Very appropriate; Somewhat appropriate; Somewhat inappropriate; Very inappropriate)

CASH AT CUSTOMS VIGNETTE

Wording: A man is returning to Britain from an overseas holiday. In his suitcase he is carrying cash worth (Amount)*. In the airport he notices a sign informing passengers that it is illegal to bring cash worth more than 10,000 Euros into Britain from overseas without declaring it to customs. After reading the sign, the man overhears one customs official telling a colleague about a focus group that had previously been run among customs officials working in the airport, which had asked the officials what they thought the legal limit should be. The customs official said that most of the officials in the focus group thought the legal limit should be above 10,500 Euros, and told her colleague that she would therefore consider it appropriate if someone failed to disclose bringing in any amount less than 10,500 Euros, but inappropriate if they failed to disclose bringing any larger amount. After hearing this conversation, the man then enters the country without declaring the cash to customs.

**The possible values of (Amount) were 9,600, 9,700, 9,800, 9,900, 10,100, 10,200, 10,300, 10,400, 10,600, 10,700, 10,800, 10,900 Euros.*

Questions

Krupka-Weber method: How socially appropriate would most people think it is for the man to enter the country without declaring the cash to customs? (Possible answers: Very socially appropriate; Somewhat socially appropriate; Somewhat socially inappropriate; Very socially inappropriate)

Opinion matching method, first-order beliefs: How appropriate do you think it is for the man to enter the country without declaring the cash to customs? (Possible answers: Very appropriate; Somewhat appropriate; Somewhat inappropriate; Very inappropriate)

Opinion matching method, second-order beliefs: Participants were asked: "How appropriate do you think it is for the man to enter the country without declaring the cash to customs?" What do you predict was the most common answer to that question? (Possible answers: Very appropriate; Somewhat appropriate; Somewhat inappropriate; Very inappropriate)

DRINK DRIVING VIGNETTE

Wording: A woman works for a company which manufactures state-of-the-art breathalysers, machines which can measure a person's blood alcohol content with extremely high accuracy. One day, after drinking in a bar in a city in England, the woman remembers she has one of the breathalysers in her bag, and wonders whether her blood alcohol content is below 0.08%, the maximum level at which a person can legally drive in England. She tests herself and discovers that her blood

alcohol content is (Percentage)*. The woman remembers reading the previous day about a panel of scientists who were arguing that it is appropriate to drive with a blood alcohol content below 0.075%, and inappropriate otherwise. The woman then drives home.

**The possible values of (Percentage) were 0.071%, 0.072%, 0.073%, 0.074%, 0.076%, 0.077%, 0.078%, 0.079%, 0.081%, 0.082%, 0.083%, 0.084%.*

Questions *Krupka-Weber method*: How socially appropriate would most people think it is for the woman to drive home? (Possible answers: Very socially appropriate; Somewhat socially appropriate; Somewhat socially inappropriate; Very socially inappropriate)

Opinion matching method, first-order beliefs: How appropriate do you think it is for the woman to drive home? (Possible answers: Very appropriate; Somewhat appropriate; Somewhat inappropriate; Very inappropriate)

Opinion matching method, second-order beliefs: Participants were asked: "How appropriate do you think it is for the woman to drive home?" What do you predict was the most common answer to that question? (Possible answers: Very appropriate; Somewhat appropriate; Somewhat inappropriate; Very inappropriate)

SPEEDING VIGNETTE

Wording: A woman is driving between two cities in order to attend a meeting. She turns onto a road and notices a sign informing motorists that the legal speed limit on the road is 70 miles per hour. The woman remembers hearing the previous week about a petition to raise speed limits on motorways, arguing that it is appropriate to drive at speeds up to 75mph, and inappropriate at higher speeds. The woman drives for the next five minutes at (Speed)*, before turning onto a different road.

**The possible values of (Speed) were 66, 67, 68, 69, 71, 72, 73, 74, 76, 77, 78, 79 miles per hour.*

Questions

Krupka-Weber method: How socially appropriate would most people think it is for the woman to drive at (Speed)? (Possible answers: Very socially appropriate; Somewhat socially appropriate; Somewhat socially inappropriate; Very socially inappropriate)

Opinion matching method, first-order beliefs: How appropriate do you think it is for the woman to drive at (Speed)? (Possible answers: Very appropriate; Somewhat appropriate; Somewhat inappropriate; Very inappropriate)

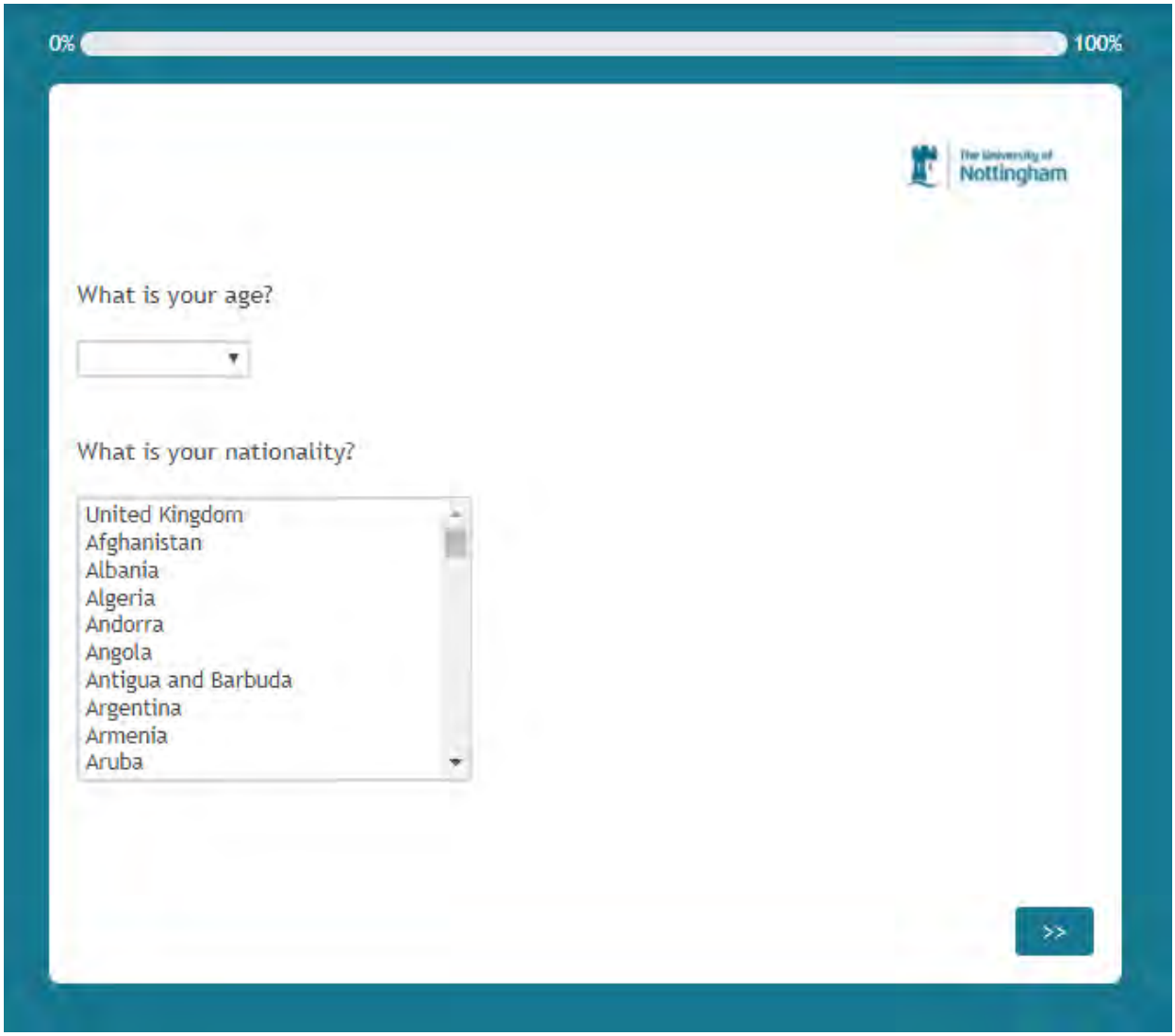
Opinion matching method, second-order beliefs: Participants were asked: "How appropriate do you think it is for the woman to drive at (Speed)?" What do you predict was the most common answer to that question? (Possible answers: Very appropriate; Somewhat appropriate; Somewhat inappropriate; Very inappropriate)

C. Screenshots of experimental instructions

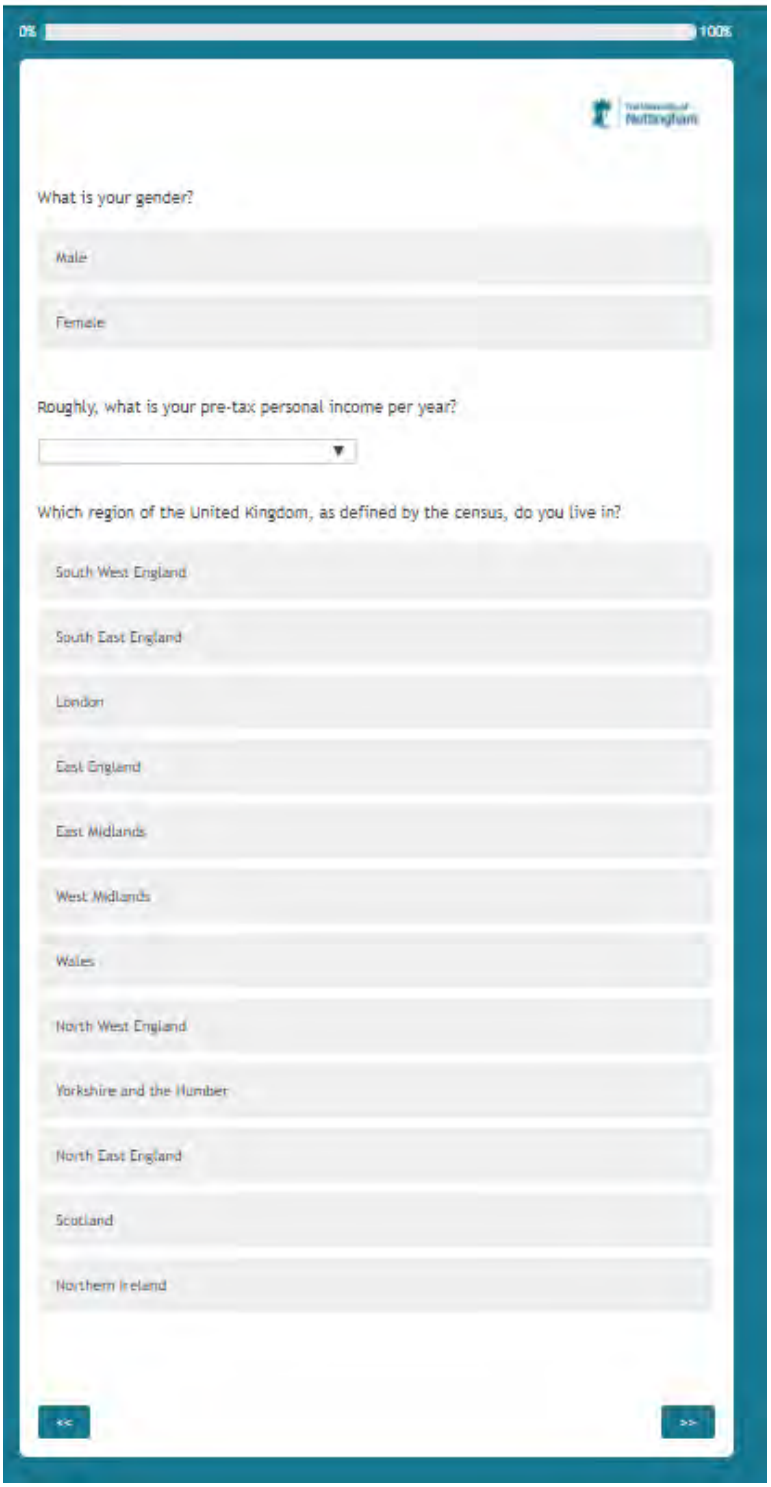
We first present screenshots from the UK experiments and then from the China experiment. The size has been adjusted so that each screen fits on one page – in the experiment itself subjects could scroll up and down.

UK experiments

Screenshots are taken from the main experiment, Sample 2 version (UK General population, Krupka-Weber method). Where the other UK experiments differed, this is explained below (or occasionally above) each screenshot.



- This was not included in the student experiment.
- The Main experiment opinion-matching version, Placebo experiment and Prosocial traits experiment also required subjects to enter their Prolific ID at the top of this screen.



- Not included in the student experiment.
- The Placebo experiment and Prosocial traits experiment also asked for subjects' ethnicity.

- Following this screen, the second-order beliefs opinion-matching versions of the Main experiment and Placebo experiment, and the Krupka-Weber version of the Placebo experiment, had the below message.

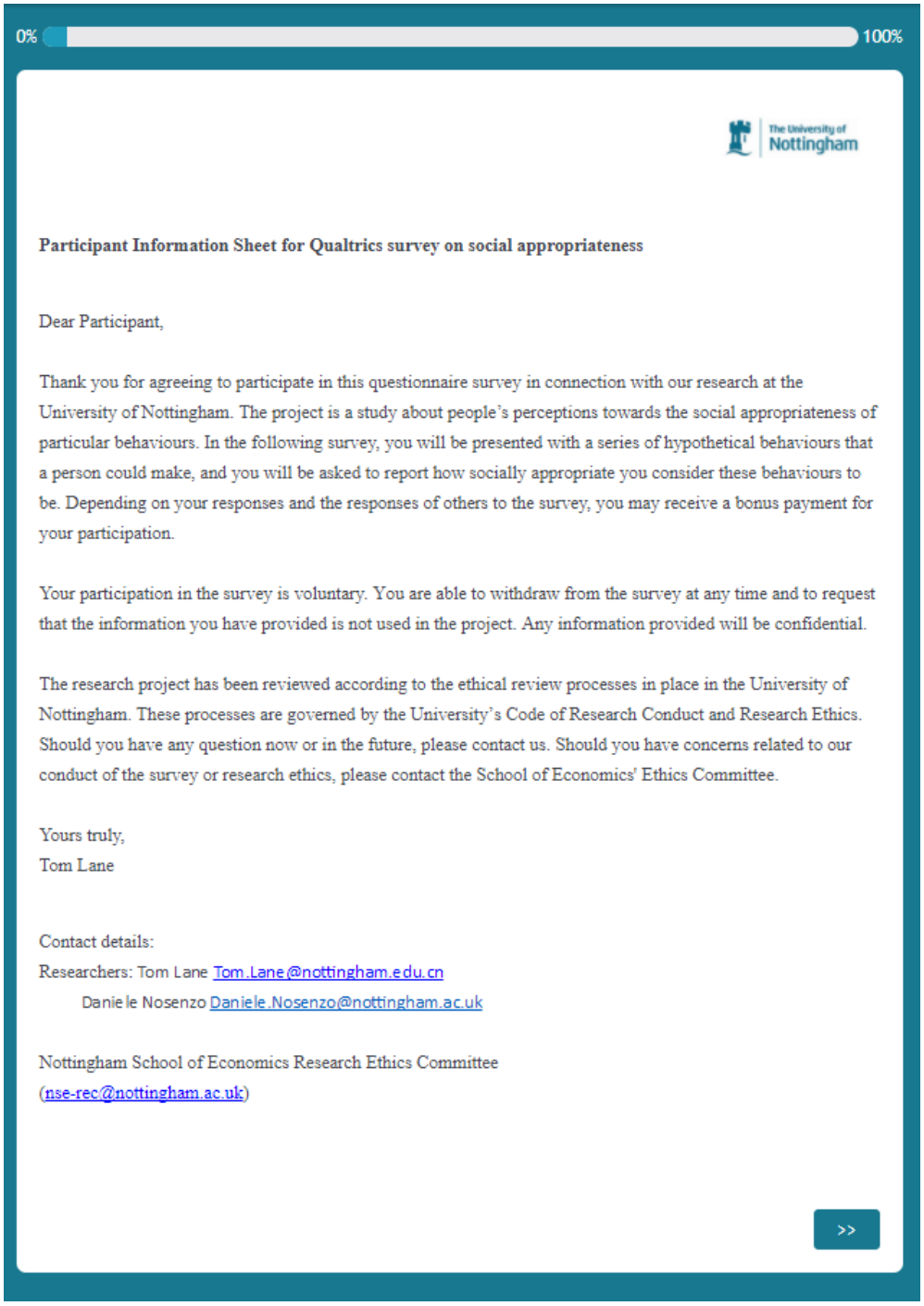
Welcome!

Thank you for accepting our study.
Including the time for reading these instructions, the study will take about 15 minutes to complete.

To complete this study you will receive a guaranteed participation fee of **£1.88** plus a bonus of up to **£30** that will depend on your decisions during the study.

At the end of the study, you will receive a link taking you back to Prolific, which you must click on to receive your payment.

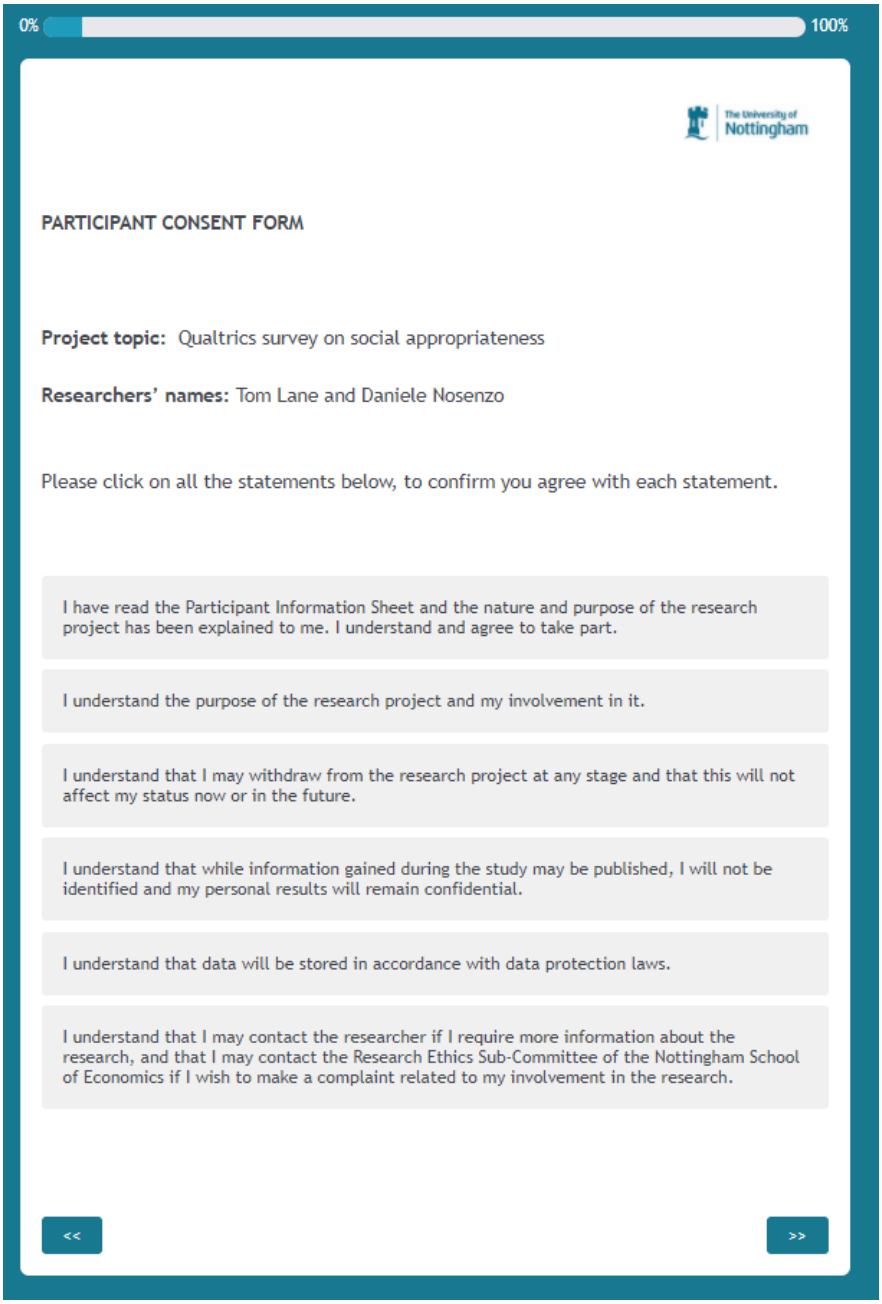
- The same message, excluding the mention of possible bonus payment, was included in the first-order beliefs versions of these experiments. The equivalent messages (respectively with and without the mention of possible bonus payment) were presented to subjects in the Prosocial traits experiment, except that the guaranteed participation fee was £0.94 and the stated study duration was 5-10 minutes.



- The title was changed to *Qualtrics survey about behaviour* for the Prosocial traits experiment.
- *Socially appropriate* was replaced by *appropriate* for the first-order versions of the opinion-matching method Main and Placebo experiments.
- The sentence beginning *The project is a study about…* was replaced by *This project is a study about your predictions towards other people's opinions* in the second-order

versions of the opinion matching method Main and Placebo experiments, and second-order version of the Prosocial traits experiment. In the first-order version of the Prosocial traits experiment, it was replaced by *The project is a study about people's perceptions towards those who engage in particular behaviours*.

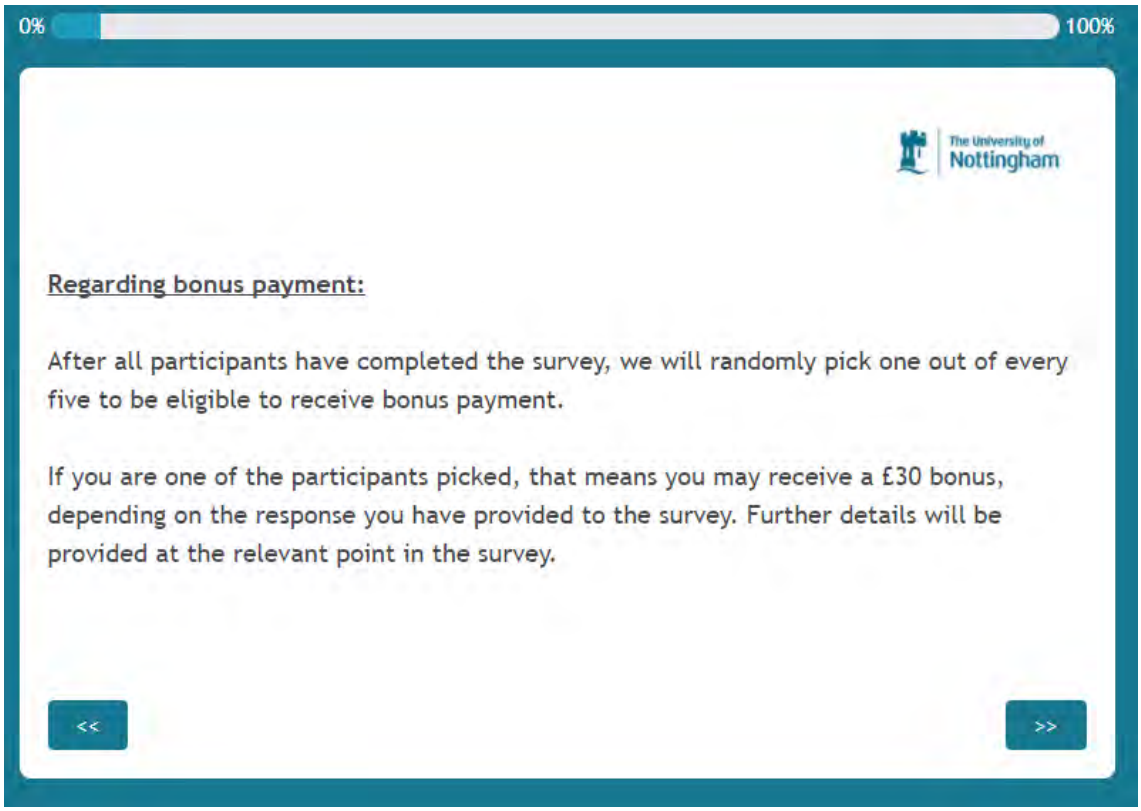
- The sentence beginning *In the following survey…* was replaced by *In the following survey, you will be presented with a series of questions that were asked to respondents of a previous survey, and you will be asked to report what you believe the most common answers to these questions were* in the second-order versions of the opinion-matching method Main and Placebo experiments, and second-order version of the Prosocial traits experiment. In the first-order version of the Prosocial traits experiment, it was replaced by *In the following survey, you will be presented with a series of hypothetical people's behaviour, and you will be asked to report how these people would be likely to behave in other contexts*.
- The sentence beginning *Depending on your responses...* was not included in any first-order beliefs experiments. For second-order beliefs experiments, the text referred to the responses of *other participants* rather than *others to the survey*.
- In the student experiment, following *Any information provided will be confidential*, were the additional sentences: *Your student ID number will be taken so that we can contact participants who are selected to receive payment, but when stored the data will be anonymized as quickly as possible, and your identity will not be revealed to any third party*.
- In response to a request from our ethics committee, all experiments run in 2021 except the first-order beliefs version of the opinion matching Main experiment contained a warning on this screen that the survey would (or, in the Prosocial traits experiment, might) contain questions about sexual behaviour.



- There was an additional screen at this point in the student experiment, reading:

This survey should take around 45 minutes to complete. If you need to stop, you can save your responses and return to the survey later.

First, please enter your student ID number. Make sure you enter this correctly, as we will use it to contact you regarding payment. (followed by box to enter ID number)

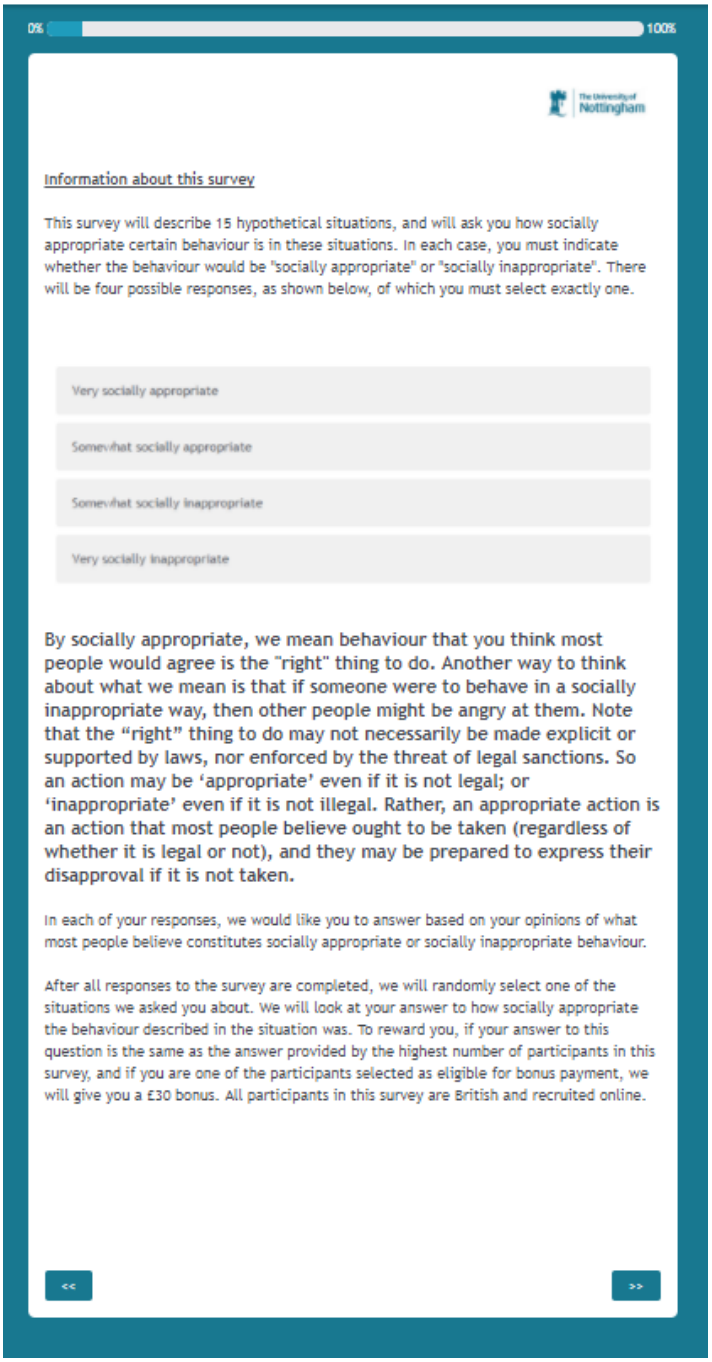


- This screen was not included in the first-order beliefs versions of the Main, Placebo and Prosocial traits experiments.
- In the second-order beliefs version of the Prosocial traits experiment, subjects were instead told we would randomly pick one out of every ten to be eligible to receive bonus payment.
- In the student experiment, this screen read:

Regarding payment:

After all participants have completed the survey, we will randomly pick one out of every five to receive payment. We will email all participants by September 28 to notify them whether they have been selected for payment or not. Participants selected for payment will then be able to collect their money from the Clive Granger Building on University Park Campus. If you have any questions regarding payment for this survey, please email Tom.Lane@nottingham.edu.cn.

If you are selected for payment, you will receive a participation fee of £10. Based on your response to the survey, you may also receive an additional £30. Further details will be provided at the relevant point in the survey.



- In the first-order beliefs version of the opinion matching Main and Placebo experiments, the term *appropriate* was used instead of *socially appropriate*. In these experiments, the final three paragraphs were replaced by:

  By appropriate, we mean behaviour that you think is the "right" thing to do. Another way to think about what we mean is that if someone were to behave in an inappropriate way, then you might be angry at them. Note that the "right" thing to do may not necessarily be made explicit or supported by laws, nor enforced by the threat of legal sanctions. So you may think that an action is 'appropriate' even if it is not legal; or 'inappropriate' even if it is not illegal. Rather, an appropriate action is an action that you believe ought to be taken (regardless of whether it is legal or not), and you may be prepared to express your disapproval if it is not taken.

  In each of your responses, we would like you to answer based on what you believe constitutes appropriate or inappropriate behaviour.

- In the second-order opinion matching version of the Main and Placebo experiments, subjects were told:

  In this survey your task is to guess the most common answers given to questions in a previous survey.

  This previous survey described 15 hypothetical situations, and we asked respondents how appropriate certain behaviour is in these situations. In each case, respondents had to indicate whether the behaviour was "appropriate" or "inappropriate". There were four possible responses, as shown below, of which respondents had to select exactly one.

  Subjects then saw a picture of the four options available to respondents of the first-order beliefs version. They then saw the final paragraphs of the screen that had been presented to first-order beliefs subjects, preceded by the statement *We gave the following explanation to respondents about what we meant by "appropriate":*.

  Finally, subjects in these versions saw the following paragraphs:

  All participants in the previous survey were British and recruited online on Prolific.

  After you have completed this survey, we will randomly select one of the questions in which we asked you to predict respondents' answers in the previous survey. We will look at your prediction as to what the most common answer was to how appropriate the behaviour described in the situation was. To reward you, if you correctly predicted the

most common answer for this situation, and if you are one of the participants selected as eligible for bonus payment, we will give you a £30 bonus.

- In the first-order beliefs version of the Prosocial traits experiment, subjects were told:

  This survey will describe 4 hypothetical people's behaviour, and in each case will ask your opinion about how this person would behave in other contexts. For each person, we will list possible behaviours in these other contexts and ask you how likely it is that this person would do them. For each question there will be four possible responses, as shown below, of which you must select exactly one.

  The four options they were shown were *(Very likely, Somewhat likely, Somewhat unlikely, Very unlikely)*. The last three paragraphs were dropped.

- In the second-order beliefs version of the Prosocial traits experiment, subjects were told:

  In this survey your task is to guess the most common answers given to questions in a previous survey.

  This previous survey described 4 hypothetical people's behaviour, and in each case we asked respondents their opinions about how this person would behave in other contexts. For each person, we listed possible behaviours in these other contexts and asked respondents how likely it is that this person would do them. For each question, there were four possible responses, as shown below, of which respondents had to select exactly one.

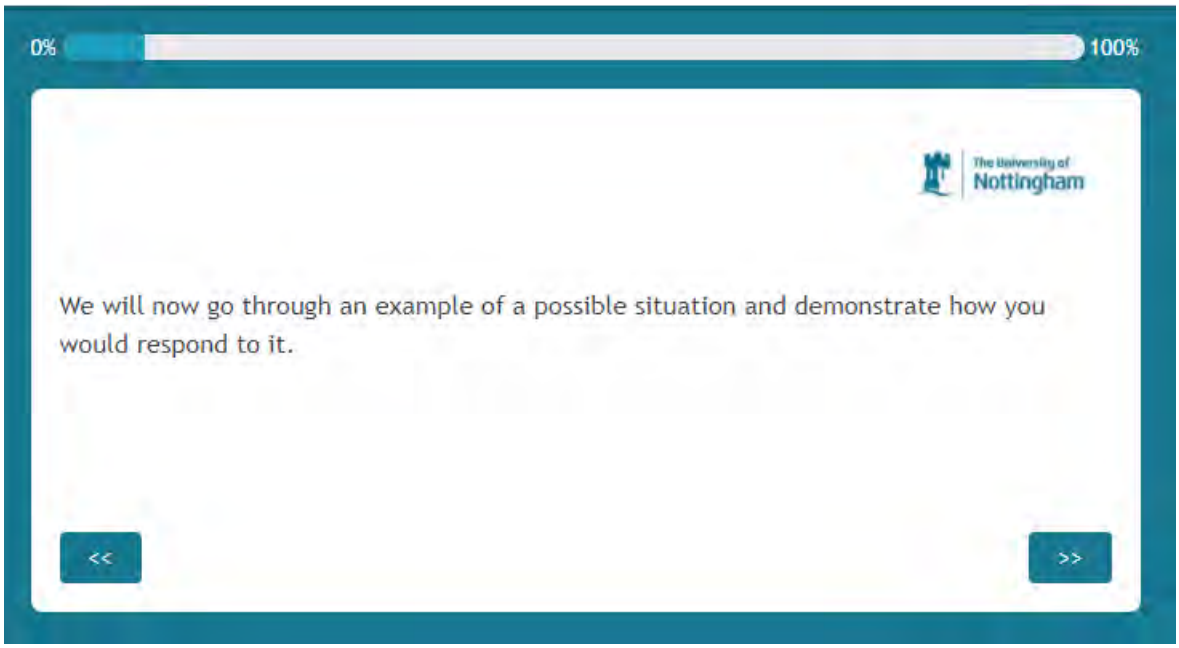
  After seeing the four available options, they were told:

  All participants in the previous survey were British and recruited online on Prolific.

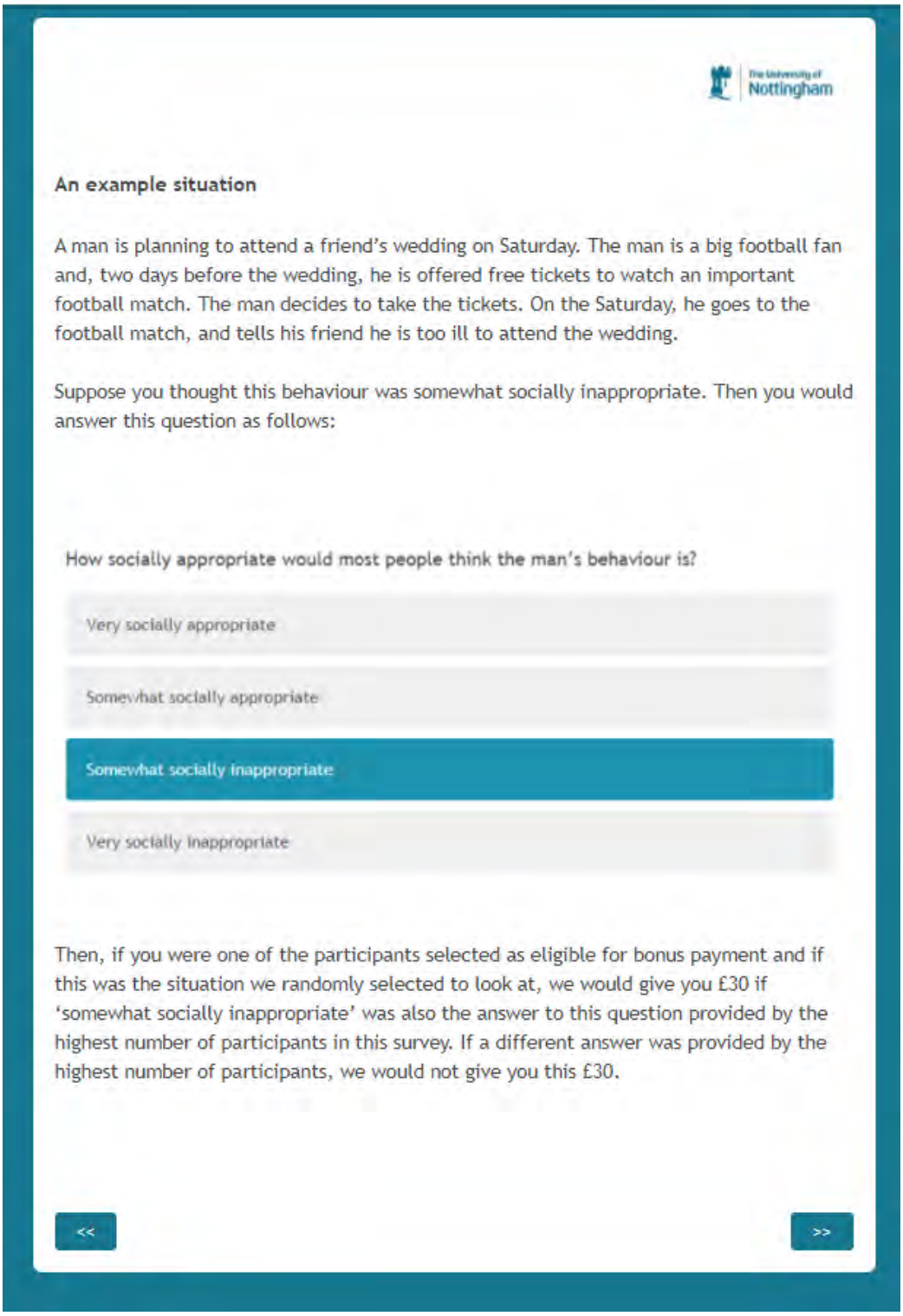
  After you have completed this survey, we will randomly select one of the questions in which we asked you to predict respondents' answers in the previous survey. We will look at your prediction as to what the most common answer was regarding how likely the person described was to behave in a particular way. To reward you, if you correctly predicted the most common answer to this question, and if you are one of the participants selected as eligible for bonus payment, we will give you a £30 bonus.

- In the student experiment, the final two sentences read: *To reward you, if your answer to this question is the same as the answer provided by the highest number of participants in this survey, and if you are one of the participants selected for payment, we will give you*

£30 in addition to your participation fee. All participants in this survey are British and studying at the University of Nottingham.



- In the second-order opinion matching version of the Main and Placebo experiments, *situation* was replaced by *question*.
- In the first-order version of the Prosocial traits experiment, the sentence was replaced by *We will now go through an example of a possible person's behaviour and demonstrate how you would respond to questions about the person.*
- In the second-order version of the Prosocial traits experiment, the sentence read *We will now go through an example of a possible set of questions and demonstrate how you would respond to them.*



- In the first-order beliefs opinion matching version of the Main and Placebo experiments, *socially appropriate/inappropriate* was replaced by *appropriate/inappropriate*. The

question in the example was *How appropriate do you think the man's behaviour is?* The paragraph at the bottom of the screen was not present in these versions.

- In the second-order beliefs opinion matching version of the Main and Placebo experiments, the heading was changed to ***An example question***. The vignette was preceded by the sentence *Suppose that we presented participants of the previous survey with the following scenario:*. The paragraph beginning *Suppose you thought this behaviour*…was replaced by:

  Suppose participants were asked: "How appropriate do you think the man's behaviour is?"

  Suppose you thought the most common answer was that the behaviour was somewhat inappropriate. Then you would answer this question as follows:

  The question *How socially appropriate would most people think the man's behaviour is?* was replaced by *What do you think was the most common answer to the question "How appropriate would most people think the man's behaviour is?"* (in the possible answers, *socially appropriate* was replaced by *appropriate*)

  The final paragraph read:

  Then, if you were one of the participants selected as eligible for bonus payment and if this was the situation we randomly selected to look at, we would give you £30 if 'somewhat inappropriate' was the answer to this scenario provided by the highest number of participants in the previous survey. If a different answer was provided by the highest number of participants, we would not give you this £30.

- In the first-order beliefs version of the Prosocial traits experiment, the heading was replaced by ***An example of a person's behaviour***. The paragraph beginning *Suppose you thought this behaviour*…was replaced by:

  Suppose we asked you:

  (1) "How likely, in future years, is this man to tell his friend the true reason why he missed the wedding?"

  (2) "How likely is this man to watch the football World Cup Final?"

Suppose you thought the answer to the first question was "Somewhat unlikely" and the answer to the second question was "Very likely". Then you would answer the questions as follows:

The screenshot showing how the questions would be answered was altered accordingly, with the possible answers *Very likely*, *Somewhat likely*, *Somewhat unlikely* and *Very Unlikely*. The final paragraph of the screen was not included in this version.

- In the second-order beliefs version of the Prosocial traits experiment, the heading was replaced by ***Example questions***. The vignette was preceded by *Suppose that we presented participants of the previous survey with the following hypothetical person's behaviour:*

  The paragraph beginning *Suppose you thought this behaviour*…was replaced by:

  Suppose participants were asked:

  (1) "How likely, in future years, is this man to tell his friend the true reason why he missed the wedding?"

  (2) "How likely is this man to watch the football World Cup Final?"

  Suppose you thought the most common answer to the first question was "somewhat unlikely" and the most common answer to the second question was "very likely". Then you would answer these questions as follows:

  The screenshot showing how the questions would be answered was altered accordingly, with the questions in the pictures commencing with *What do you think was the most common answer in the previous survey to the question*…

  In this version, the final paragraph of the screen was replaced by:

  Then, if you were one of the participants selected as eligible for bonus payment and if we randomly selected the second question about this person to look at, we would give you £30 if 'very likely' was the answer to this scenario provided by the highest number of participants in the previous survey. If a different answer was provided by the highest number of participants, we would not give you this £30.

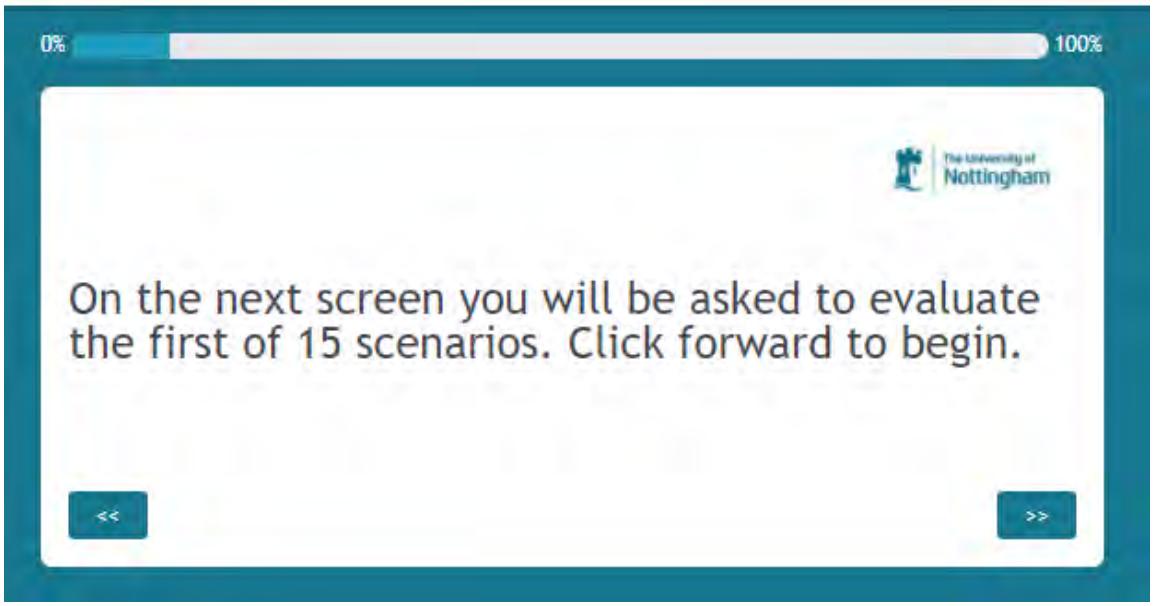
- In the student experiment, *selected as eligible for bonus payment* was replaced by *selected for payment*.

- Before the next screen, an additional screen was added to all experiments conducted in 2021. For the first-order opinion matching versions of the Main and Placebo experiments, and the Krupka-Weber version of the Placebo experiment, this screen read:

  Note: When providing your answers, you should imagine that these situations are all taking place BEFORE the existence of Covid-19. So it is not necessary for the people in these hypothetical situations to practice social distancing or take any pandemic-related precautions.

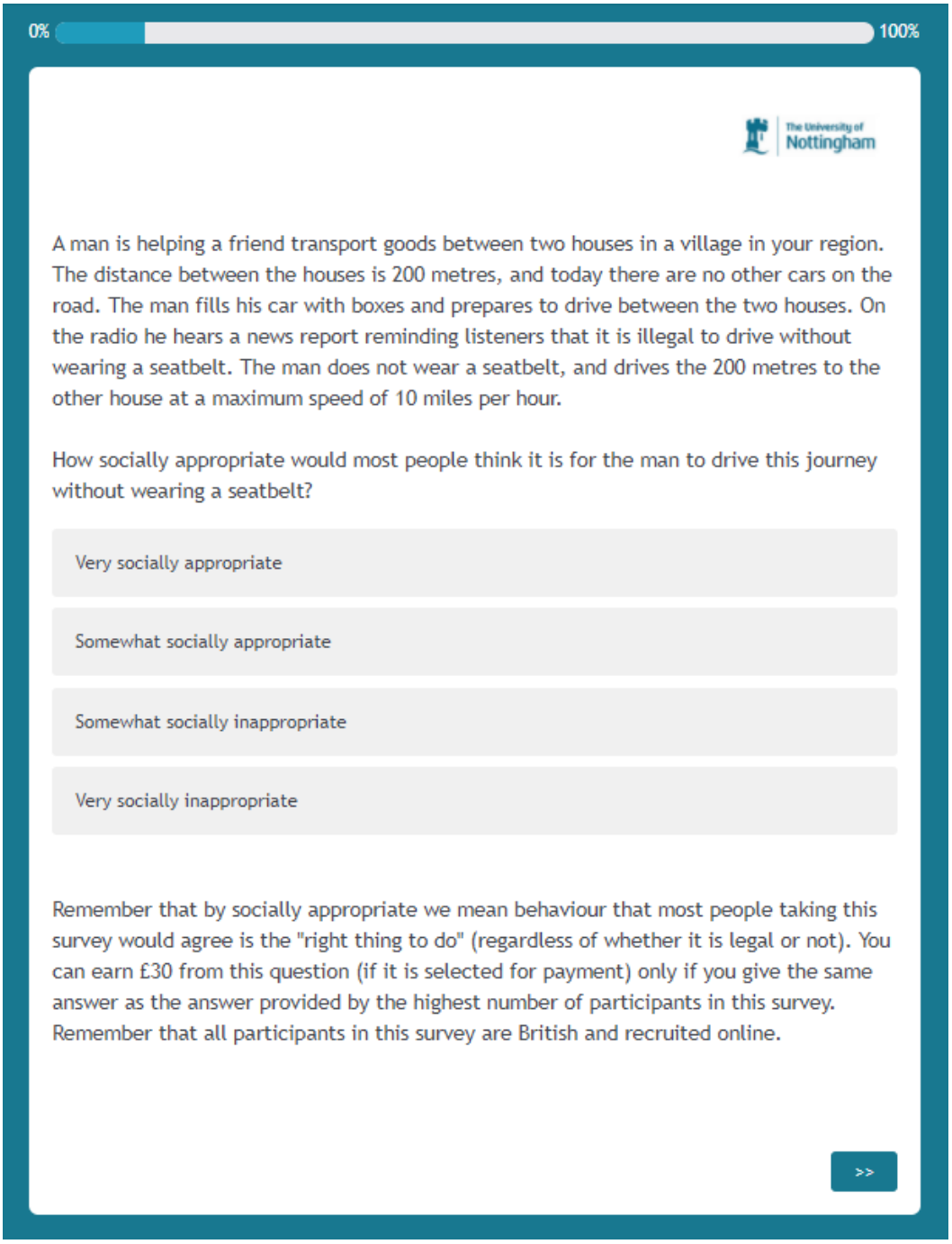
  For the first-order Prosocial traits experiment, the screen was as above except ***situations*** was replaced by ***behaviours*** and ***the people in these hypothetical situations*** was replaced by ***these hypothetical people***.

  The second-order beliefs versions of the opinion matching Main and Placebo experiments, and Prosocial traits experiment, contained the equivalent text to the first-order versions, except that ***Note*** was replace by ***Note that we told participants in the previous survey***.



- For the second-order beliefs versions of the opinion matching Main and Placebo experiments, the first sentence was replaced by ***On the next screen you will be asked to make your first of 15 predictions about the answers from the previous survey***. Such language was also used in the second-order beliefs versions of the Prosocial traits experiment, except for referring to 4 sets of predictions rather than 15 predictions.
- For the first-order beliefs version of the Prosocial traits experiment, the sentence was replaced by ***On the next screen you will be asked to evaluate the first of 4 people***.

(The following three vignettes were presented in random order)



Changes were made to the screen above, and all subsequent vignette screens, as follows:

- In the first-order beliefs opinion matching version of the Main and Placebo experiments, *socially appropriate* was replaced by *appropriate.* In these versions, questions asked *How appropriate do you think* rather than *How socially appropriate would most people think…* The paragraph at the bottom of each screen in these versions read *Remember that by appropriate we mean behaviour that you think is the "right thing to do" (regardless of whether it is legal or not)*.
- In the second-order beliefs opinion matching version of the Main and Placebo experiments, the vignettes were preceded by *Participants in the previous survey were presented with the following situation:*

  In these versions, the questions took the format:

  Participants were asked: "(wording from first-order beliefs version)"

  What do you predict was the most common answer to that question? (possible answers: Very appropriate, Somewhat appropriate, Somewhat inappropriate, Very inappropriate)

  The bottom paragraph of each screen read:

  Remember that we explained to participants that, by appropriate, we mean behaviour that they think is the "right thing to do" (regardless of whether it is legal or not). You can earn £30 from this question (if it is selected for payment) only if you correctly predict the most common response to the question in the previous survey. Remember that all participants in the previous survey were British and recruited online on Prolific.

- In the first-order beliefs version of the Prosocial traits experiment, the one question was replaced by the set of six questions indicated in OSM B.1. The questions were always the same, only changing to reflect the gender of the person in the vignette. The paragraph at the bottom of each vignette screen was not present in this version.
- In the second-order beliefs version of the Prosocial traits experiment, all vignettes were preceded by *Participants in the previous survey were presented with the following hypothetical person's behaviour:* The one question was replaced by the set of six questions indicated in OSM B.1. The questions were always the same, only changing to reflect the gender of the person in the vignette. The bottom paragraph on each screen read:

  Remember that, if one of these questions is selected for payment, you can earn £30 from it only if you correctly predict the most common response to the question in the previous

survey. Remember that all participants in the previous survey were British and recruited online on Prolific.

- In the Krupka-Weber version of the Placebo experiment, the last sentence on each screen read: *Remember that all participants in this survey are British and recruited online on Prolific.*
- In the student experiment, the last sentence on each screen read: *Remember that all participants in this survey are British and studying at the University of Nottingham.*

- On the above screen of the student experiment, *village in your region* was replaced by *village near Nottingham*.

0% 100%

The University of Nottingham

A woman has saved up £2,000 which she intends to spend on a luxury beach holiday. Just before she books the holiday, she reads a news report about a charity providing aid for hungry people in an impoverished African country. The woman decides she should donate the £2,000 to the charity instead of booking the beach holiday. However, she then changes her mind and books the beach holiday, and does not donate any money to charity.

How socially appropriate would most people think it is for the woman to book the beach holiday and not donate any money to charity?

- Very socially appropriate
- Somewhat socially appropriate
- Somewhat socially inappropriate
- Very socially inappropriate

Remember that by socially appropriate we mean behaviour that most people taking this survey would agree is the "right thing to do" (regardless of whether it is legal or not). You can earn £30 from this question (if it is selected for payment) only if you give the same answer as the answer provided by the highest number of participants in this survey. Remember that all participants in this survey are British and recruited online.

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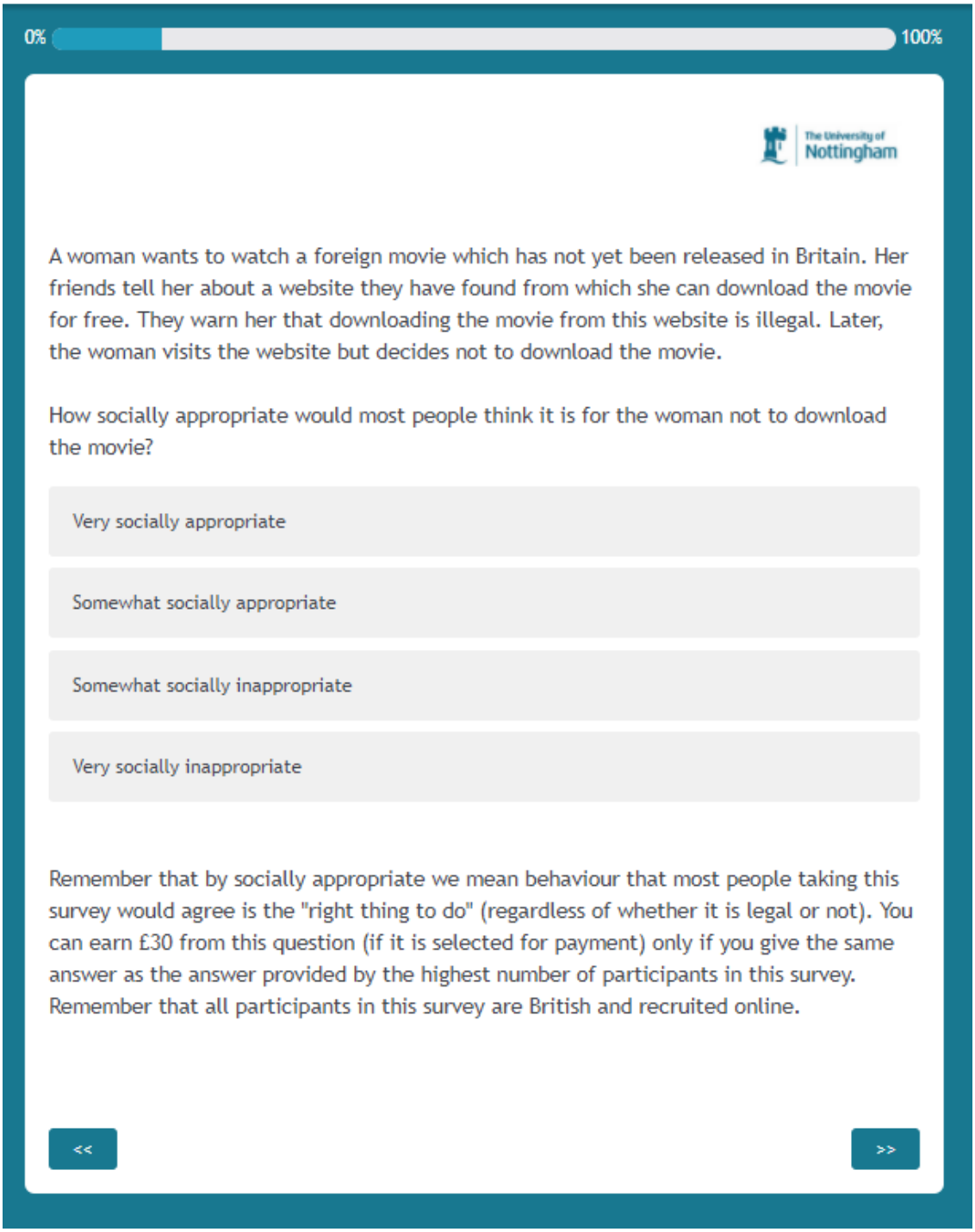
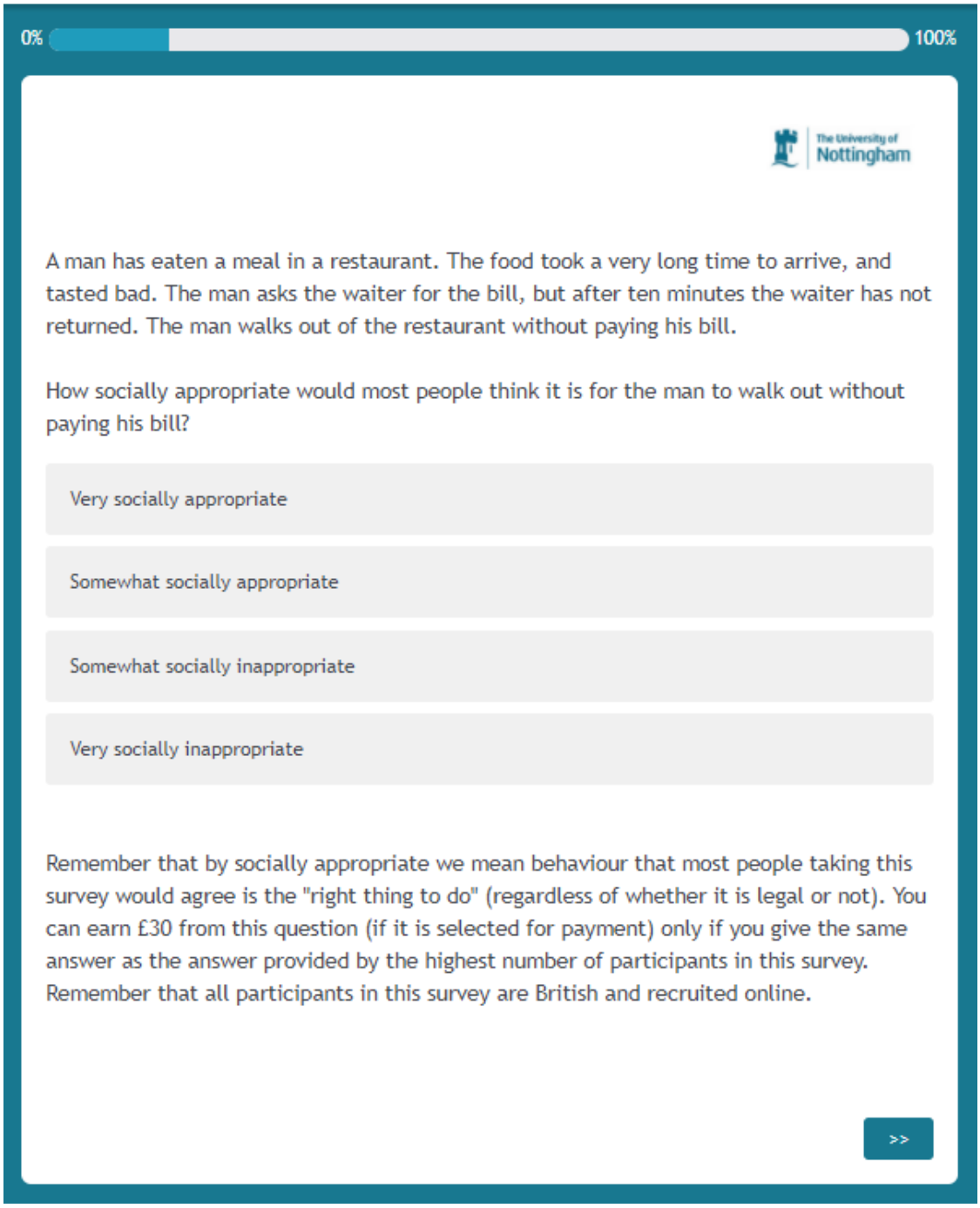


(The following 12 vignettes were presented in random order, except in the Prosocial traits experiment, in which only one of the five threshold vignettes was randomly selected to be presented and the other filler vignettes were not presented)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The University of Nottingham
A man has eaten a meal in a restaurant. The food took a very long time to arrive, and tasted bad. The man asks the waiter for the bill, but after ten minutes the waiter has not returned. The man walks out of the restaurant without paying his bill.
How socially appropriate would most people think it is for the man to walk out without paying his bill?
Very socially appropriate
Somewhat socially appropriate
Somewhat socially inappropriate
Very socially inappropriate
Remember that by socially appropriate we mean behaviour that most people taking this survey would agree is the "right thing to do" (regardless of whether it is legal or not). You can earn £30 from this question (if it is selected for payment) only if you give the same answer as the answer provided by the highest number of participants in this survey. Remember that all participants in this survey are British and recruited online.
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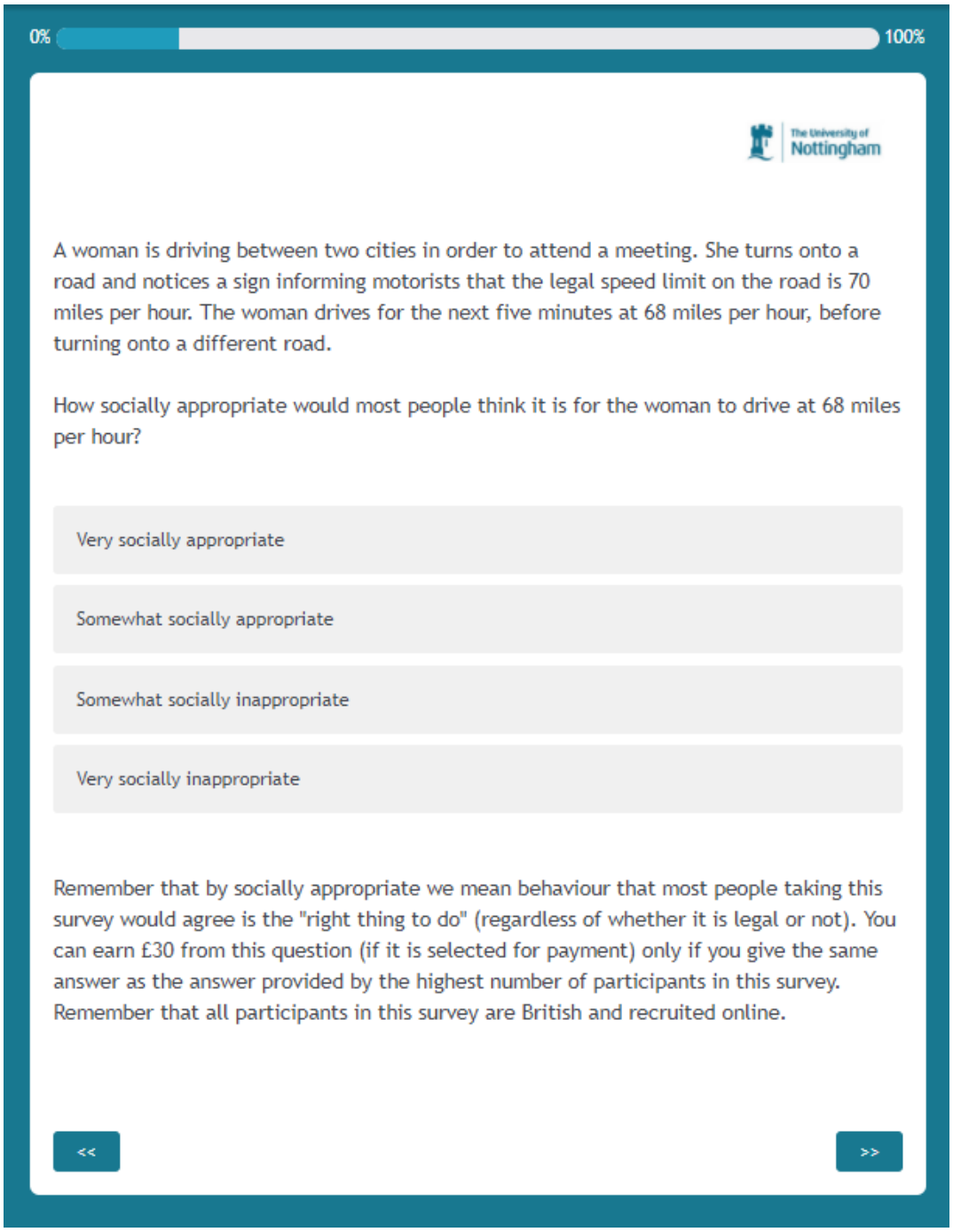
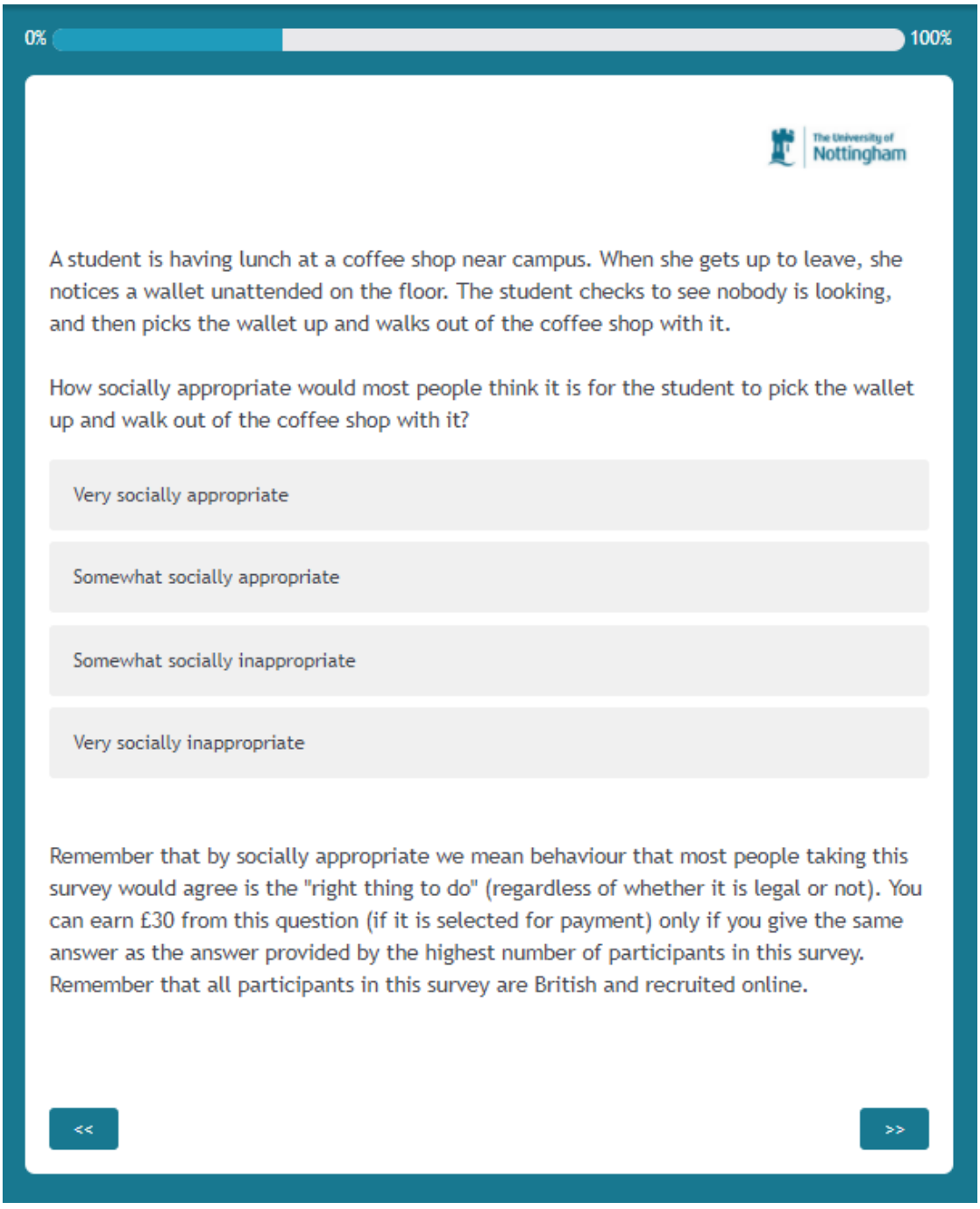


- The wording of this vignette was modified in the Placebo experiments, as indicated in OSM B.2.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The University of Nottingham
A student is having lunch at a coffee shop near campus. When she gets up to leave, she notices a wallet unattended on the floor. The student checks to see nobody is looking, and then picks the wallet up and walks out of the coffee shop with it.
How socially appropriate would most people think it is for the student to pick the wallet up and walk out of the coffee shop with it?
Very socially appropriate
Somewhat socially appropriate
Somewhat socially inappropriate
Very socially inappropriate
Remember that by socially appropriate we mean behaviour that most people taking this survey would agree is the "right thing to do" (regardless of whether it is legal or not). You can earn £30 from this question (if it is selected for payment) only if you give the same answer as the answer provided by the highest number of participants in this survey. Remember that all participants in this survey are British and recruited online.
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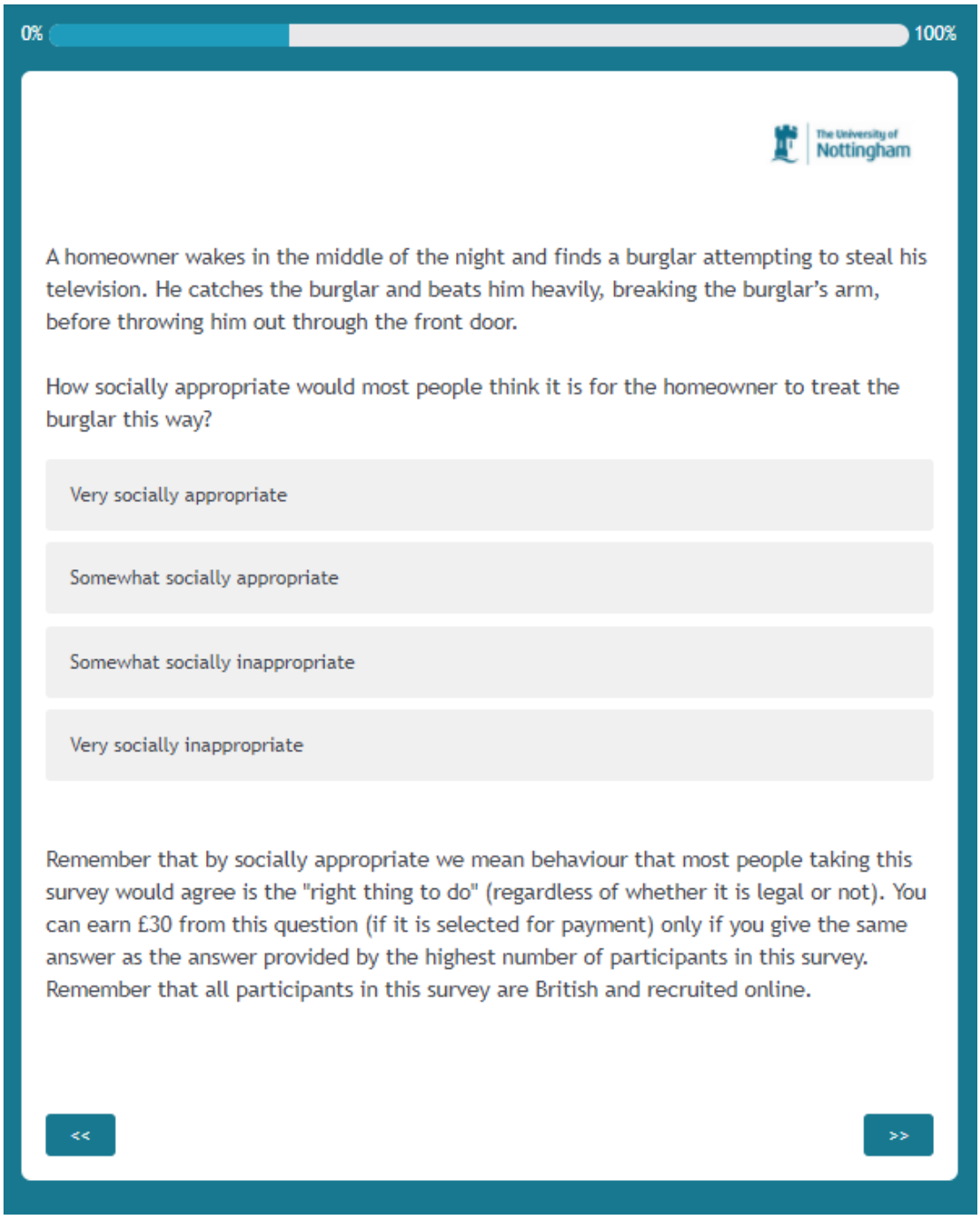
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The University of Nottingham
A homeowner wakes in the middle of the night and finds a burglar attempting to steal his television. He catches the burglar and beats him heavily, breaking the burglar's arm, before throwing him out through the front door.
How socially appropriate would most people think it is for the homeowner to treat the burglar this way?
Very socially appropriate
Somewhat socially appropriate
Somewhat socially inappropriate
Very socially inappropriate
Remember that by socially appropriate we mean behaviour that most people taking this survey would agree is the "right thing to do" (regardless of whether it is legal or not). You can earn £30 from this question (if it is selected for payment) only if you give the same answer as the answer provided by the highest number of participants in this survey. Remember that all participants in this survey are British and recruited online.
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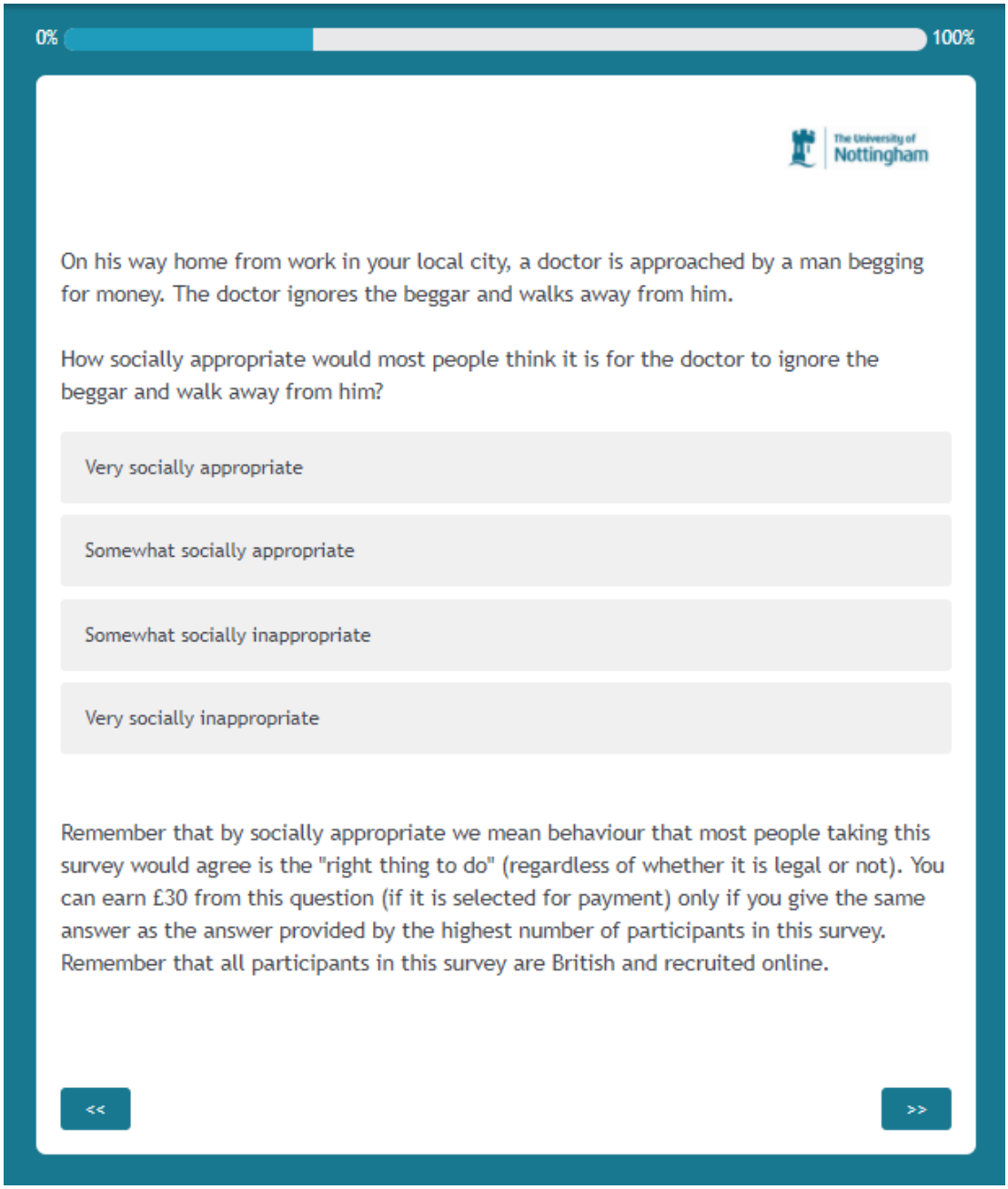
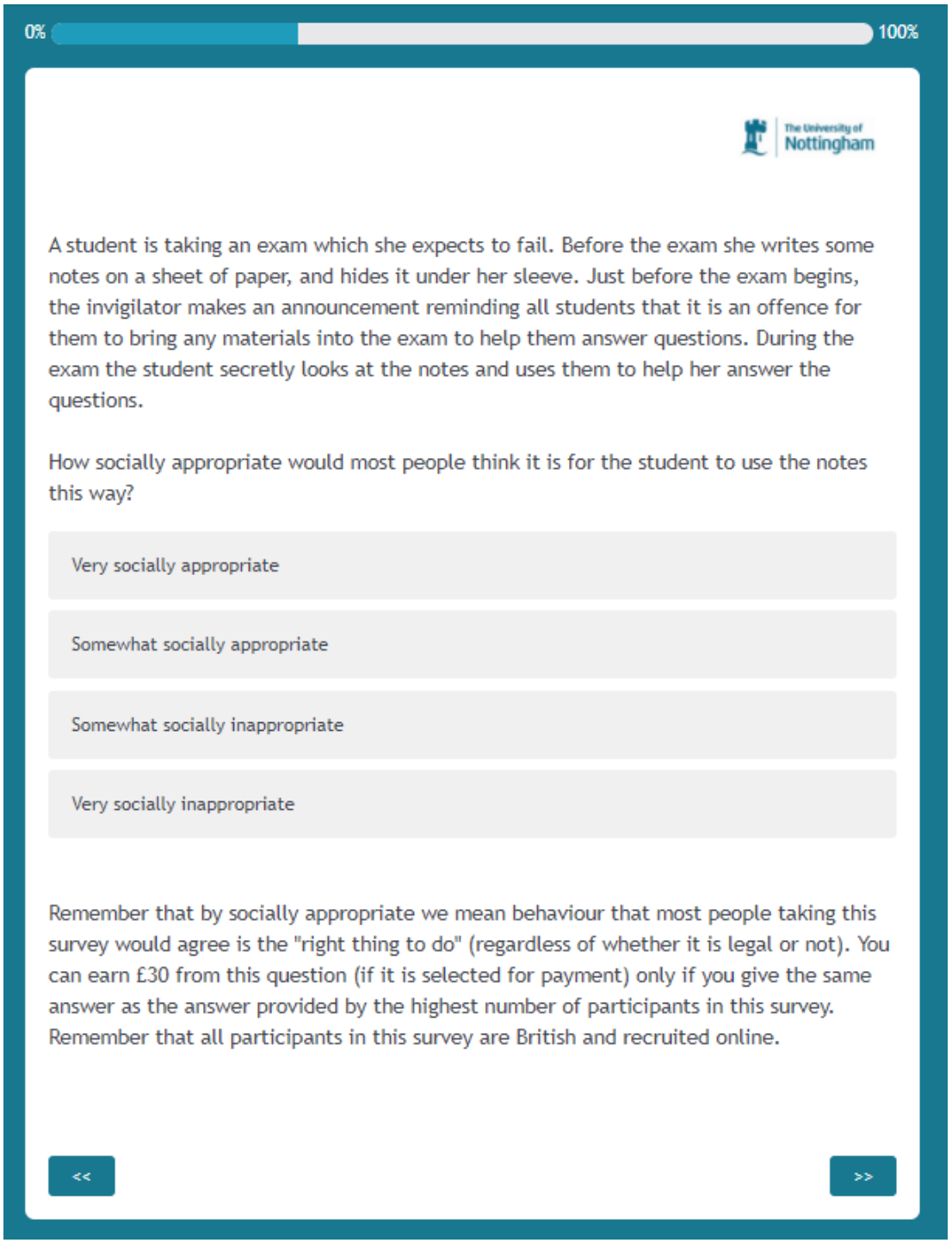


- In the student experiment, *your local city* was replaced by *Nottingham*.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The University of Nottingham
A student is taking an exam which she expects to fail. Before the exam she writes some notes on a sheet of paper, and hides it under her sleeve. Just before the exam begins, the invigilator makes an announcement reminding all students that it is an offence for them to bring any materials into the exam to help them answer questions. During the exam the student secretly looks at the notes and uses them to help her answer the questions.
How socially appropriate would most people think it is for the student to use the notes this way?
Very socially appropriate
Somewhat socially appropriate
Somewhat socially inappropriate
Very socially inappropriate
Remember that by socially appropriate we mean behaviour that most people taking this survey would agree is the "right thing to do" (regardless of whether it is legal or not). You can earn £30 from this question (if it is selected for payment) only if you give the same answer as the answer provided by the highest number of participants in this survey. Remember that all participants in this survey are British and recruited online.
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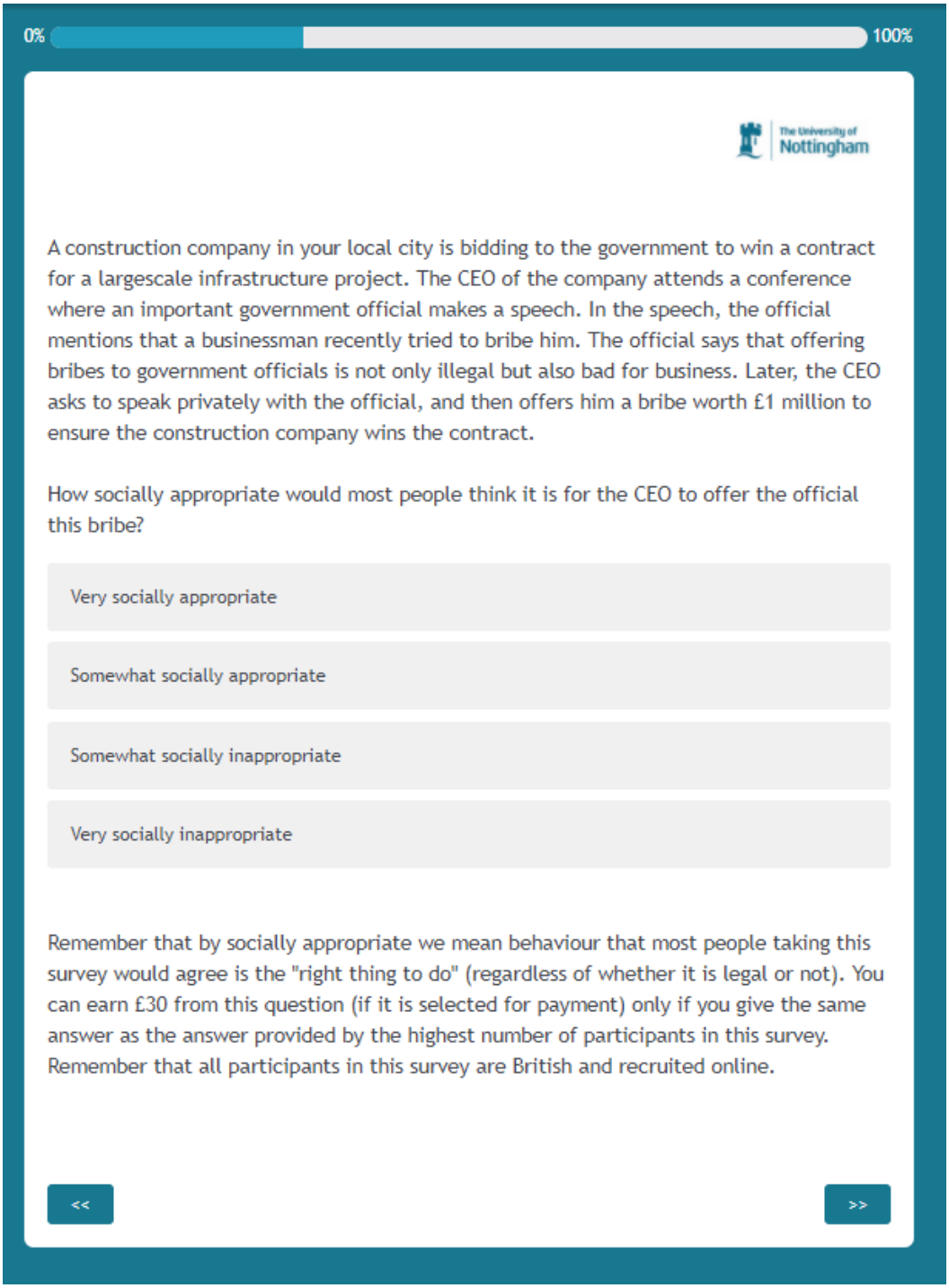
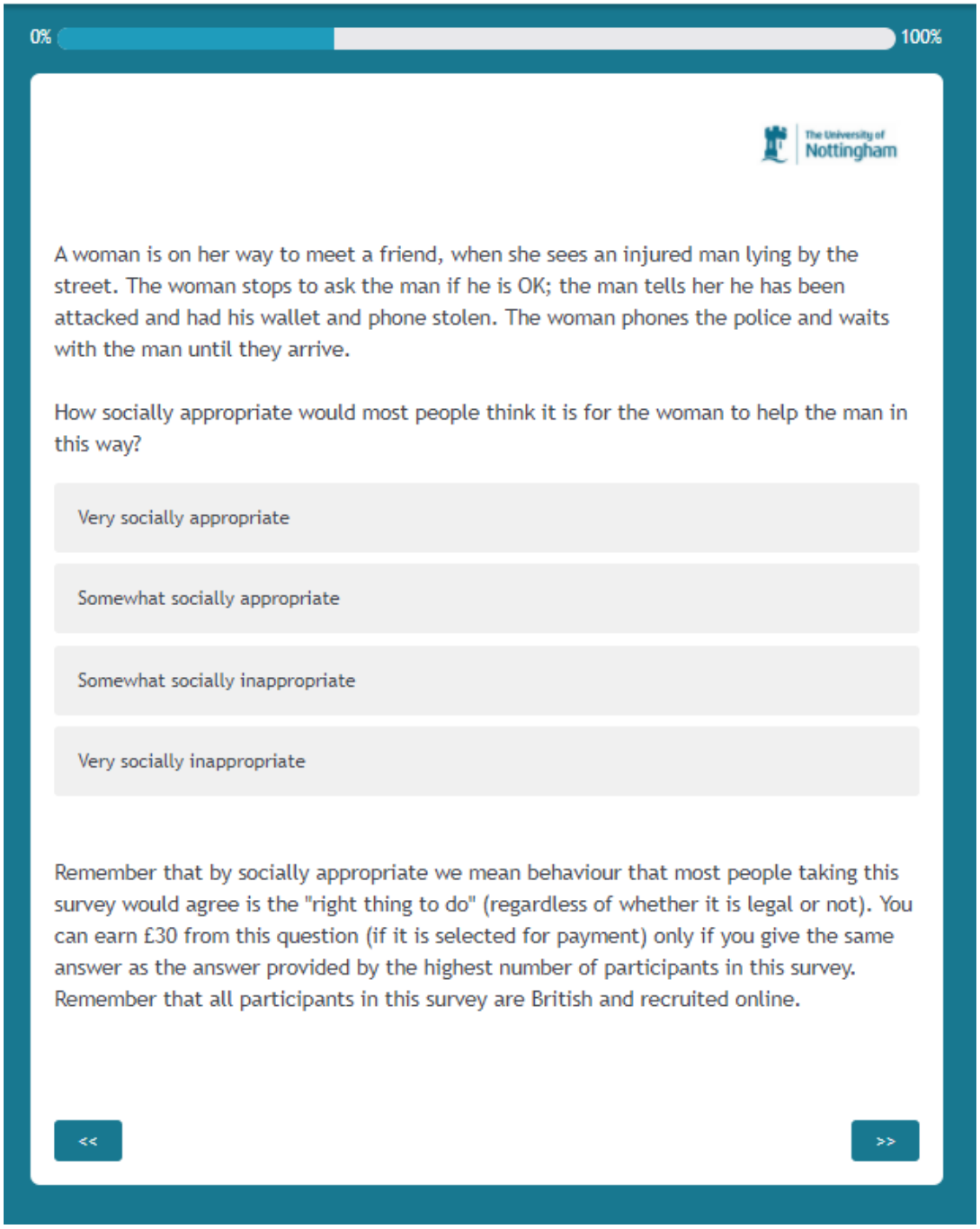


- In the student experiment, *your local city* was replaced by *Nottingham*.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The University of Nottingham
A woman is on her way to meet a friend, when she sees an injured man lying by the street. The woman stops to ask the man if he is OK; the man tells her he has been attacked and had his wallet and phone stolen. The woman phones the police and waits with the man until they arrive.
How socially appropriate would most people think it is for the woman to help the man in this way?
Very socially appropriate
Somewhat socially appropriate
Somewhat socially inappropriate
Very socially inappropriate
Remember that by socially appropriate we mean behaviour that most people taking this survey would agree is the "right thing to do" (regardless of whether it is legal or not). You can earn £30 from this question (if it is selected for payment) only if you give the same answer as the answer provided by the highest number of participants in this survey. Remember that all participants in this survey are British and recruited online.
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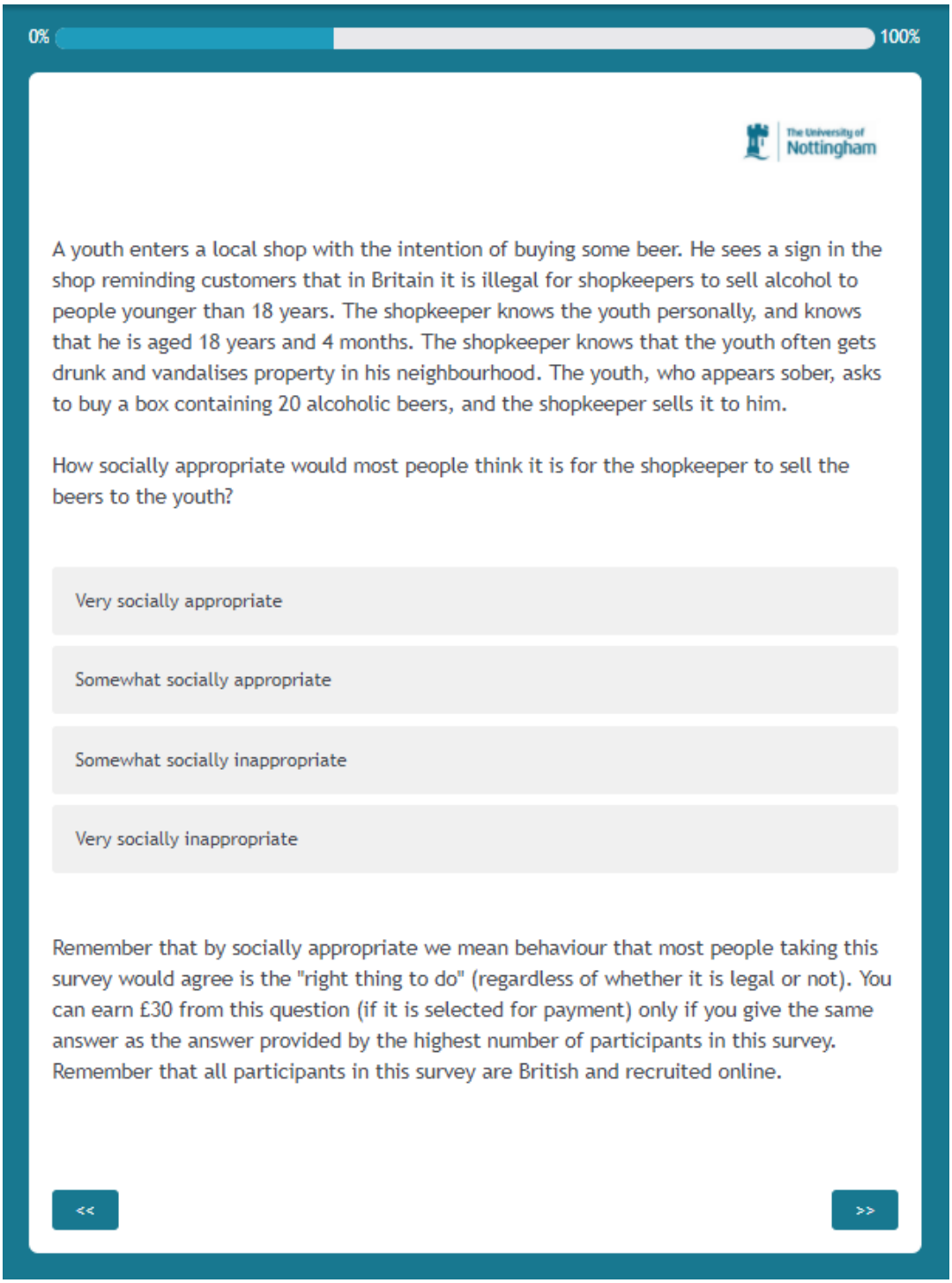
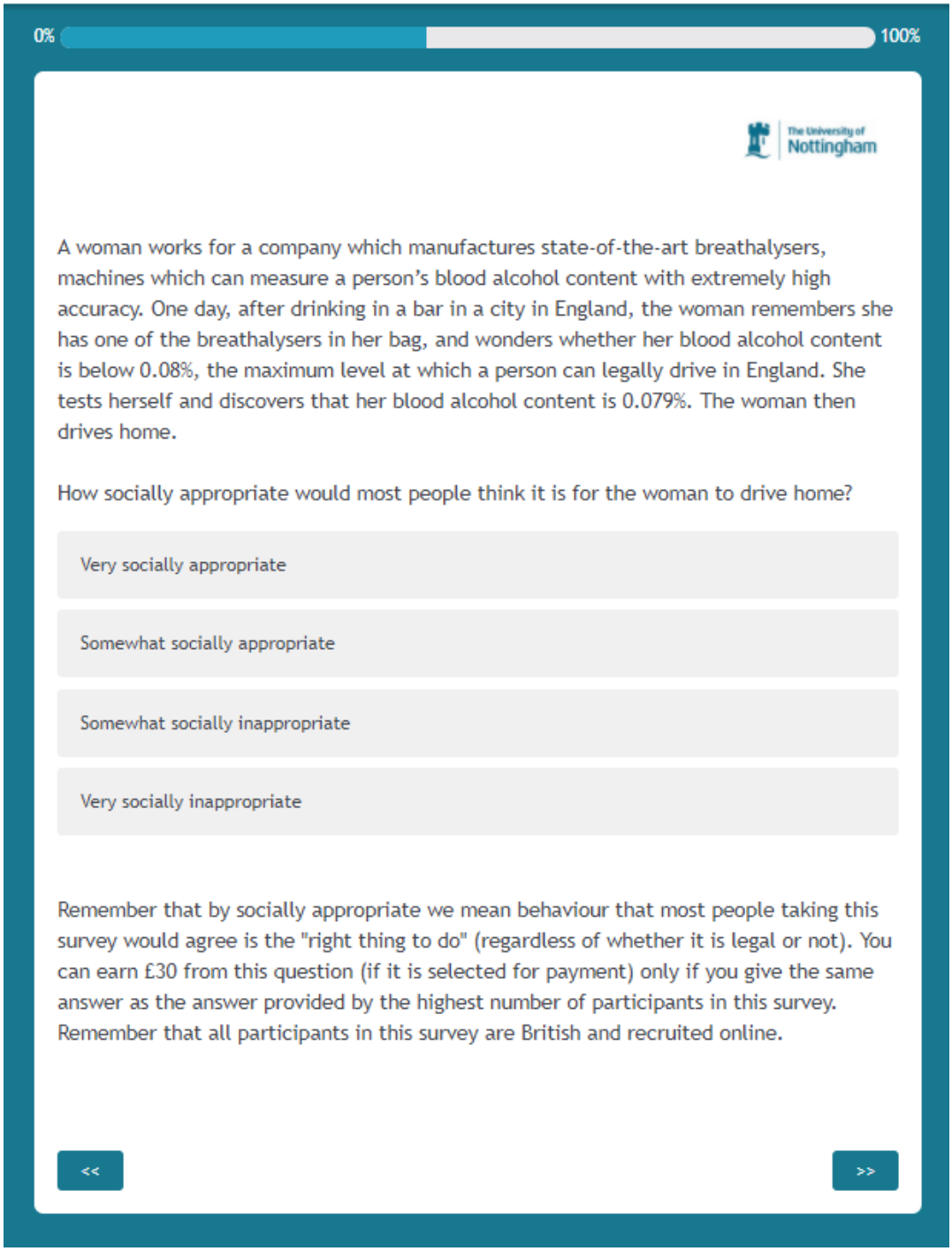
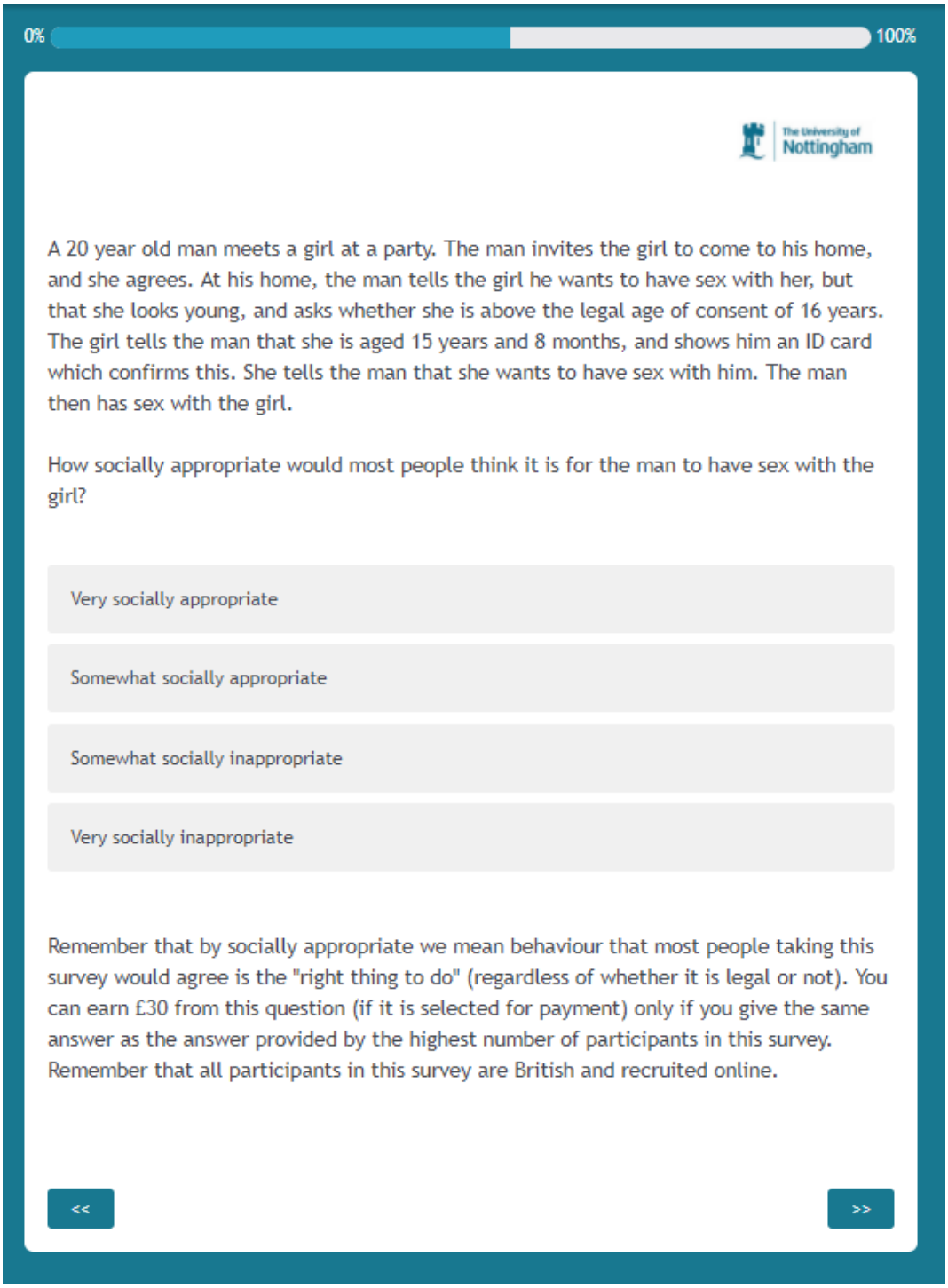


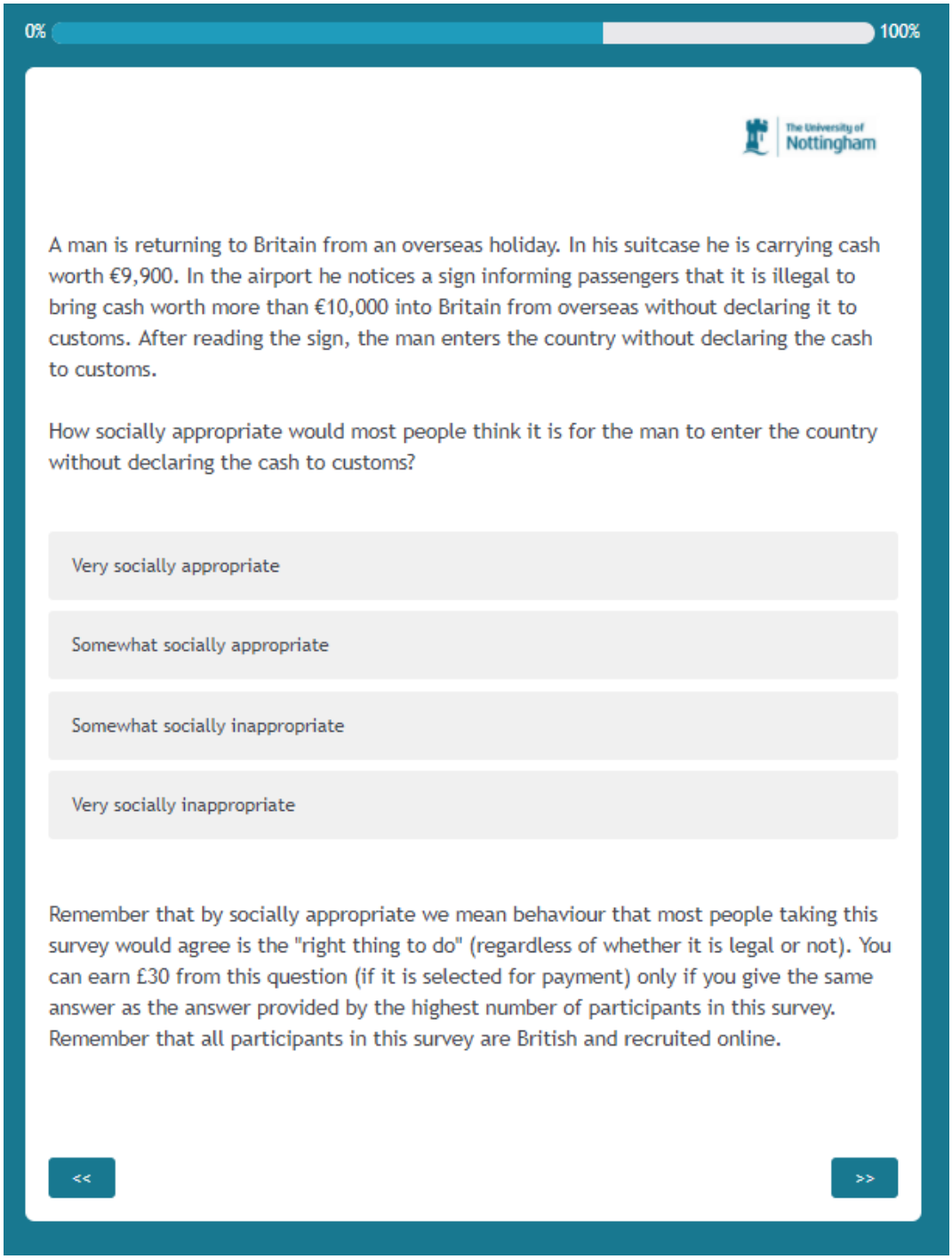
- The wording of this vignette was modified in the Placebo experiments, as indicated in OSM B.2.



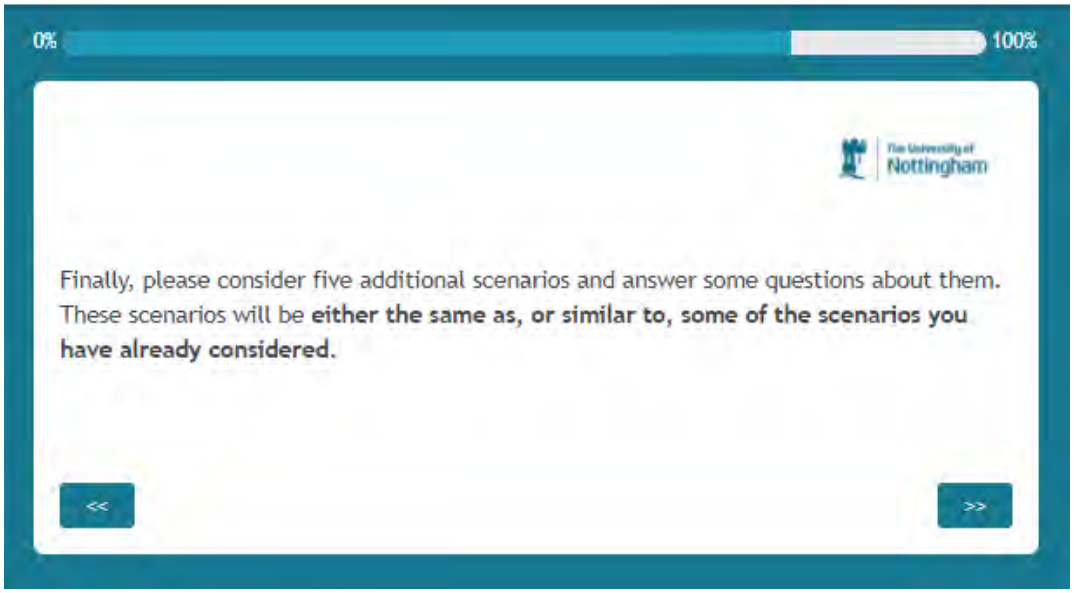
- In the student experiment, *a city in England* was replaced by *Nottingham*.
- The wording of this vignette was modified in the Placebo experiments, as indicated in OSM B.2.



- The wording of this vignette was modified in the Placebo experiments, as indicated in OSM B.2.



- The wording of this vignette was modified in the Placebo experiments, as indicated in OSM B.2.



- Not included in the student or Placebo experiments.
- In the second-order beliefs opinion matching version of the Main experiment, the following sentence was added at the bottom of the screen: ***In the following questions we are asking about your own opinions, not your predictions of how others would answer them.***
- In the first-order Prosocial traits experiment, this screen read:

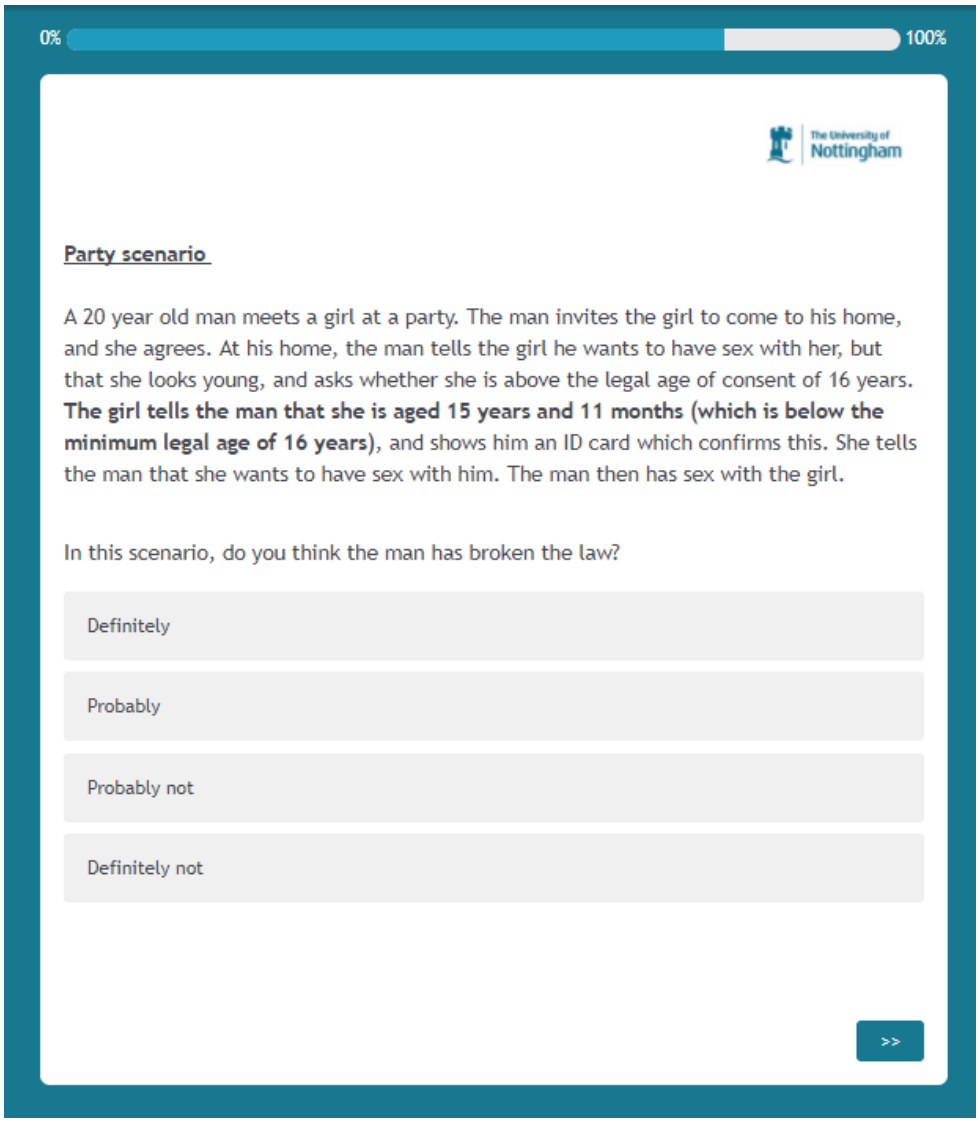
  Finally, please consider one additional hypothetical person and answer some questions about their described behaviour. This behaviour will be ***either the same as, or similar to, the behaviour of one of the people you have already considered****.*

- In the second-order Prosocial traits experiment, it read:

  Finally, please consider one additional hypothetical person and answer some questions about their described behaviour. This behaviour will be ***either the same as, or similar to, the behaviour of one of the people you have already considered****.*

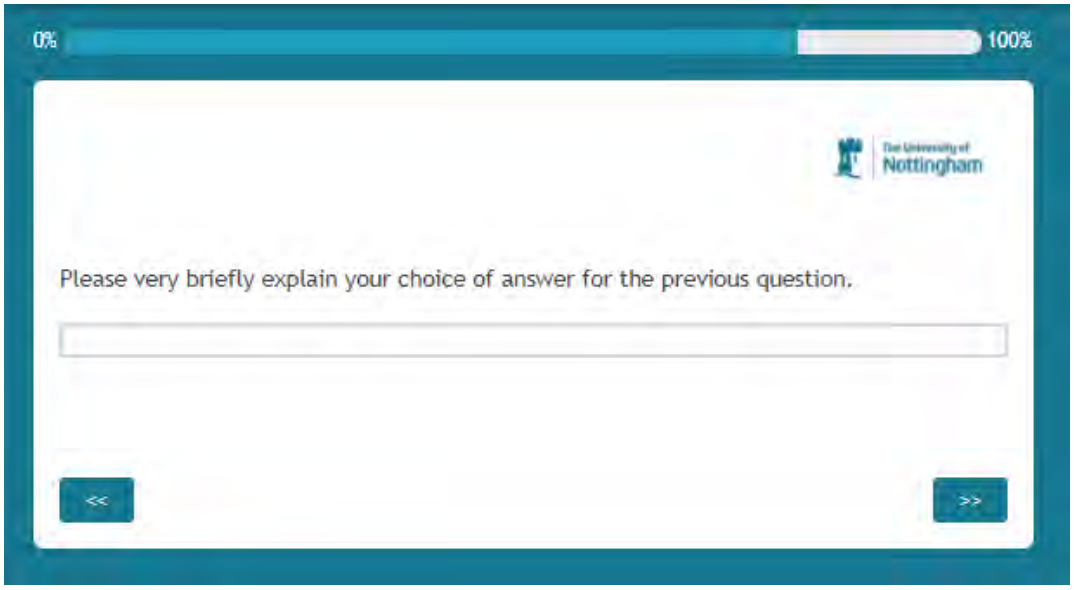
  In these questions, you should answer by stating your own opinions, rather than predicting the answers of others.

(The following questions about the five scenarios were presented in random order in the Main experiment, except for the student version where they were not included. In the Prosocial traits experiment, only the questions about the vignette that was randomly presented to subjects were displayed.)

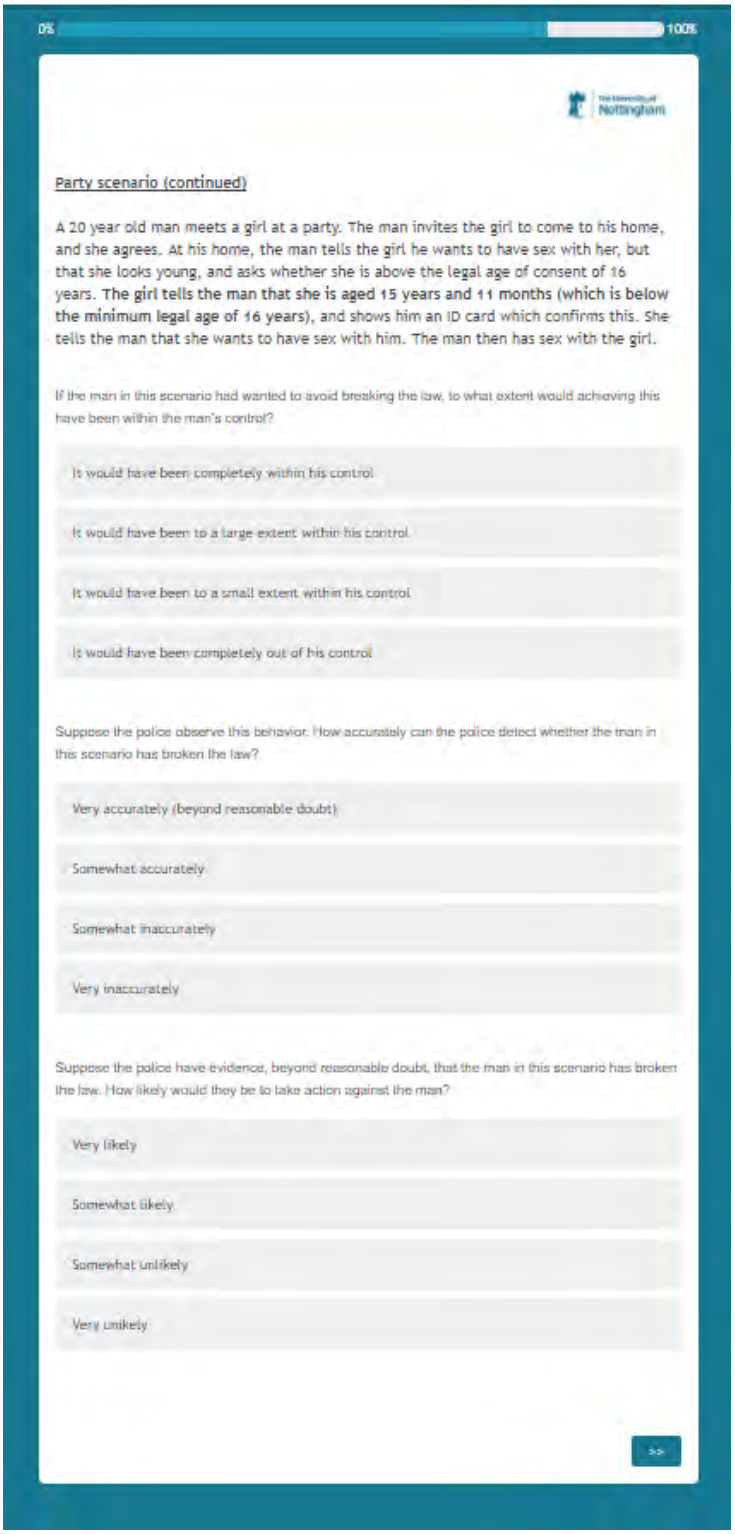


- This screen, and the screen after next, were changed slightly after reviewing the responses to our soft launch data (N=35) from the Krupka-Weber version of the Main experiment, which revealed some subjects were misunderstanding these follow up questions relating to each of the scenarios, incorrectly believing the questions were asking about the version of the vignette they evaluated earlier. The sentence beginning *The girl tells the man…*was not initially in bold, and the parentheses *(which is below the*

minimum legal age of 16 years) was absent. Other experiments in which these screens were present always featured the updated versions.



- Not included if subject answered *Definitely* to previous question)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Party scenario (continued)
A 20 year old man meets a girl at a party. The man invites the girl to come to his home, and she agrees. At his home, the man tells the girl he wants to have sex with her, but that she looks young, and asks whether she is above the legal age of consent of 16 years. The girl tells the man that she is aged 15 years and 11 months (which is below the minimum legal age of 16 years), and shows him an ID card which confirms this. She tells the man that she wants to have sex with him. The man then has sex with the girl.
If the man in this scenario had wanted to avoid breaking the law, to what extent would achieving this have been within the man's control?
It would have been completely within his control
It would have been to a large extent within his control
It would have been to a small extent within his control
It would have been completely out of his control
Suppose the police observe this behavior. How accurately can the police detect whether the man in this scenario has broken the law?
Very accurately (beyond reasonable doubt)
Somewhat accurately
Somewhat inaccurately
Very inaccurately
Suppose the police have evidence, beyond reasonable doubt, that the man in this scenario has broken the law. How likely would they be to take action against the man?
Very likely
Somewhat likely
Somewhat unlikely
Very unlikely
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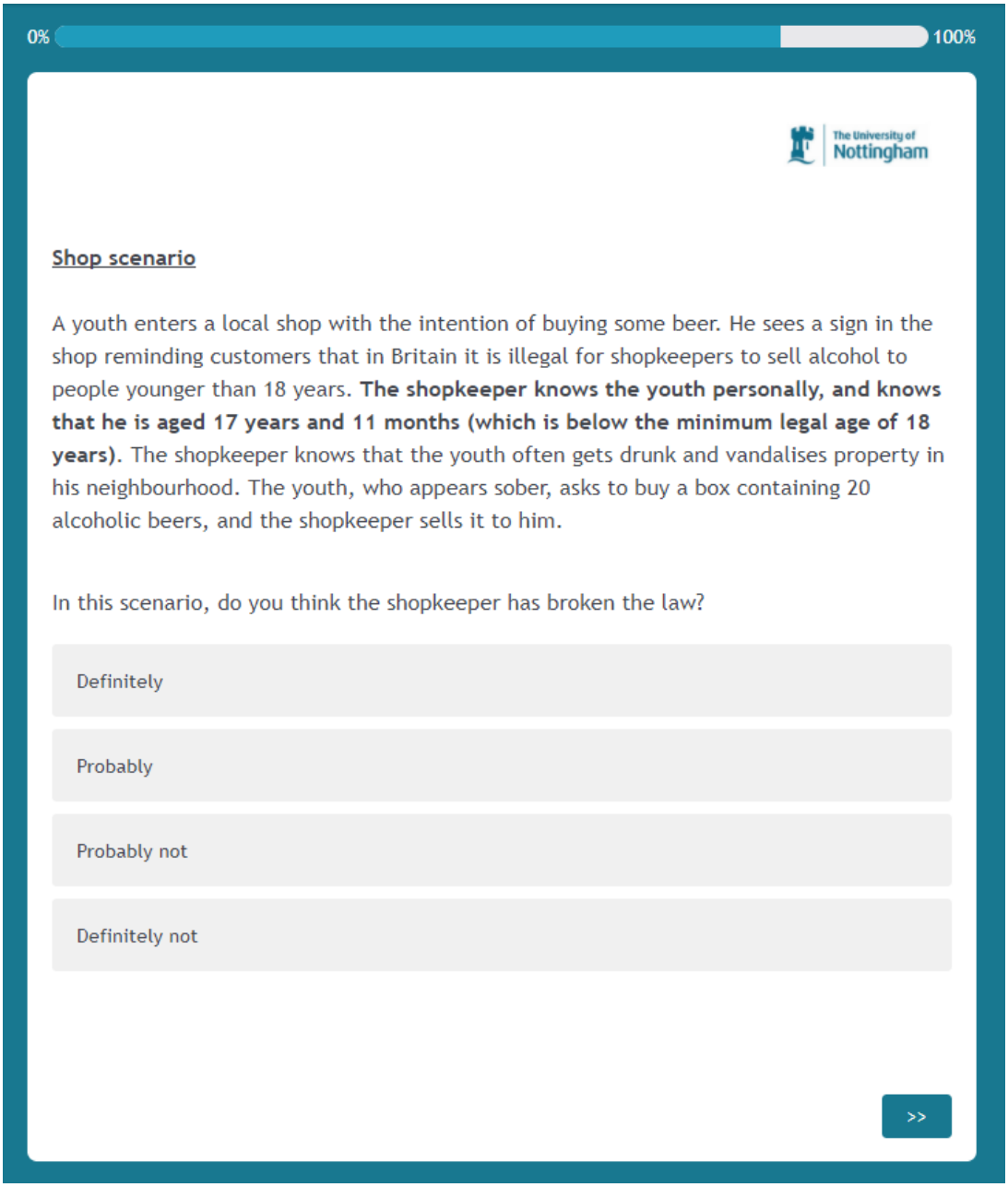
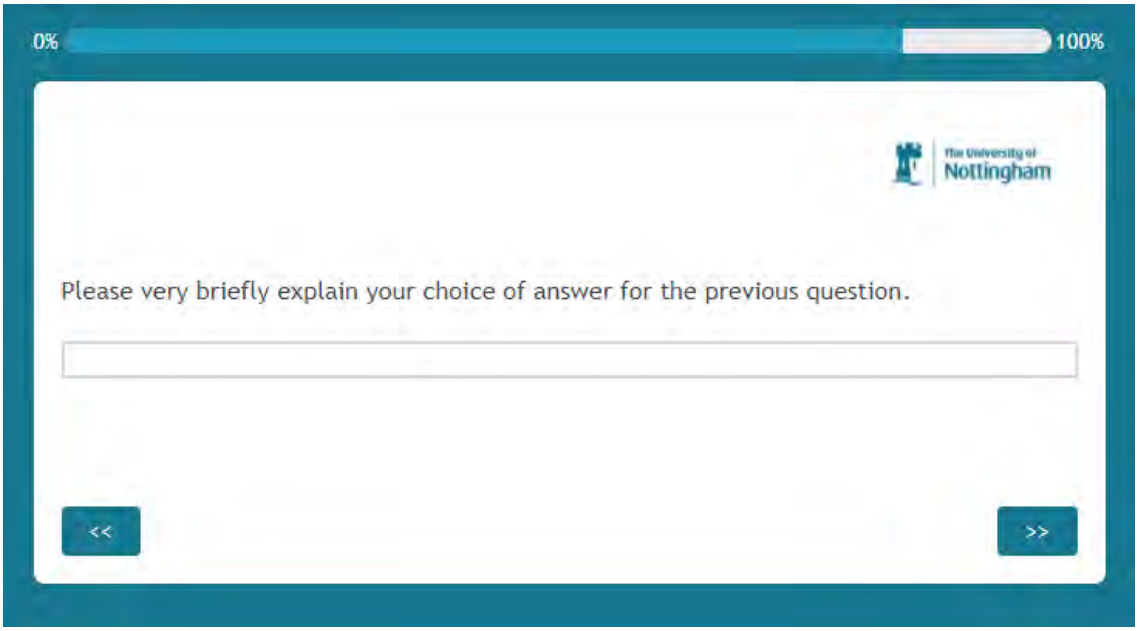
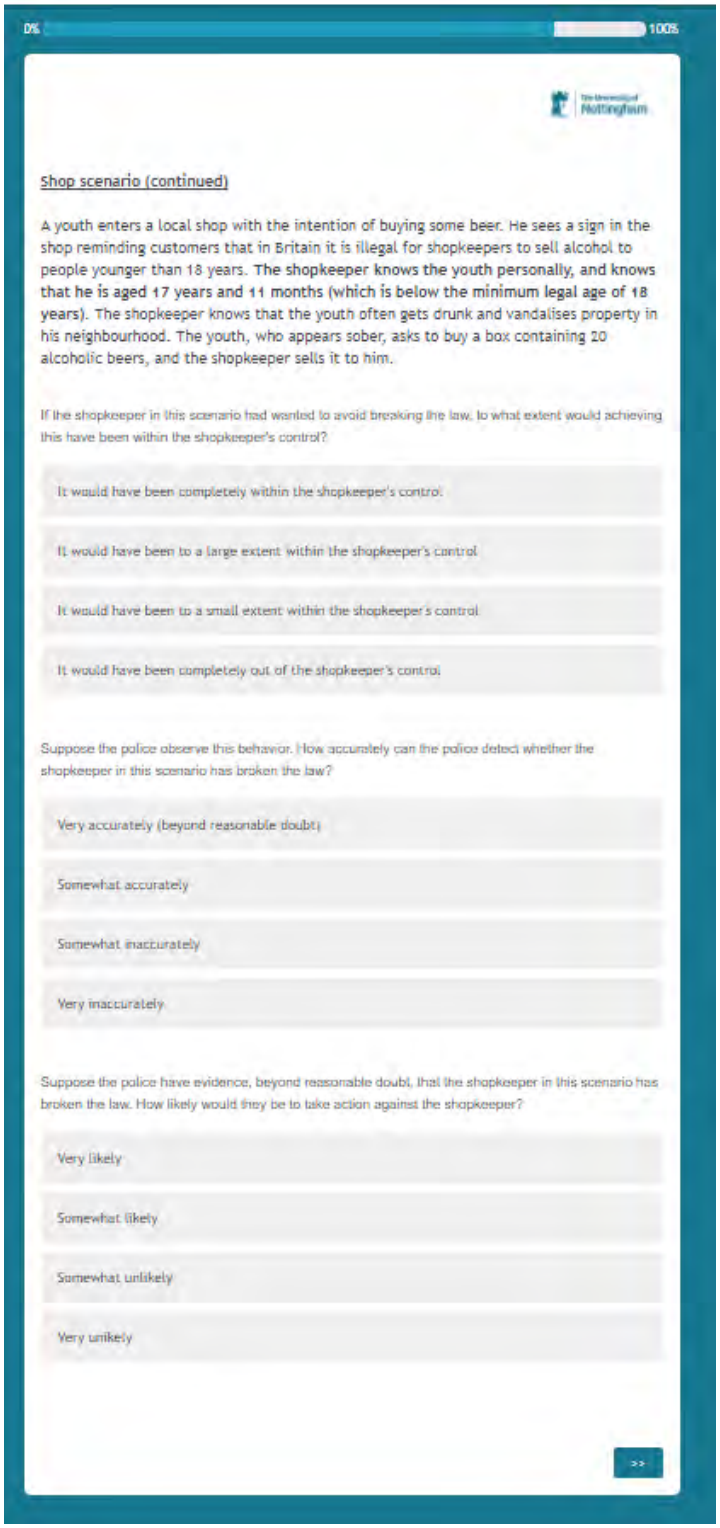


- This screen, and the screen after next, were changed slightly after reviewing the responses to our soft launch data (N=35) from the Krupka-Weber version of the Main experiment, which revealed some subjects were misunderstanding these follow up questions relating to each of the scenarios, incorrectly believing the questions were asking about the version of the vignette they evaluated earlier. The sentence beginning *The shopkeeper knows the youth…*was not initially in bold, and the parentheses *(which is below the minimum legal age of 18 years)* was absent. Other experiments in which these screens were present always featured the updated versions.



- Not included if subject answered *Definitely* to previous question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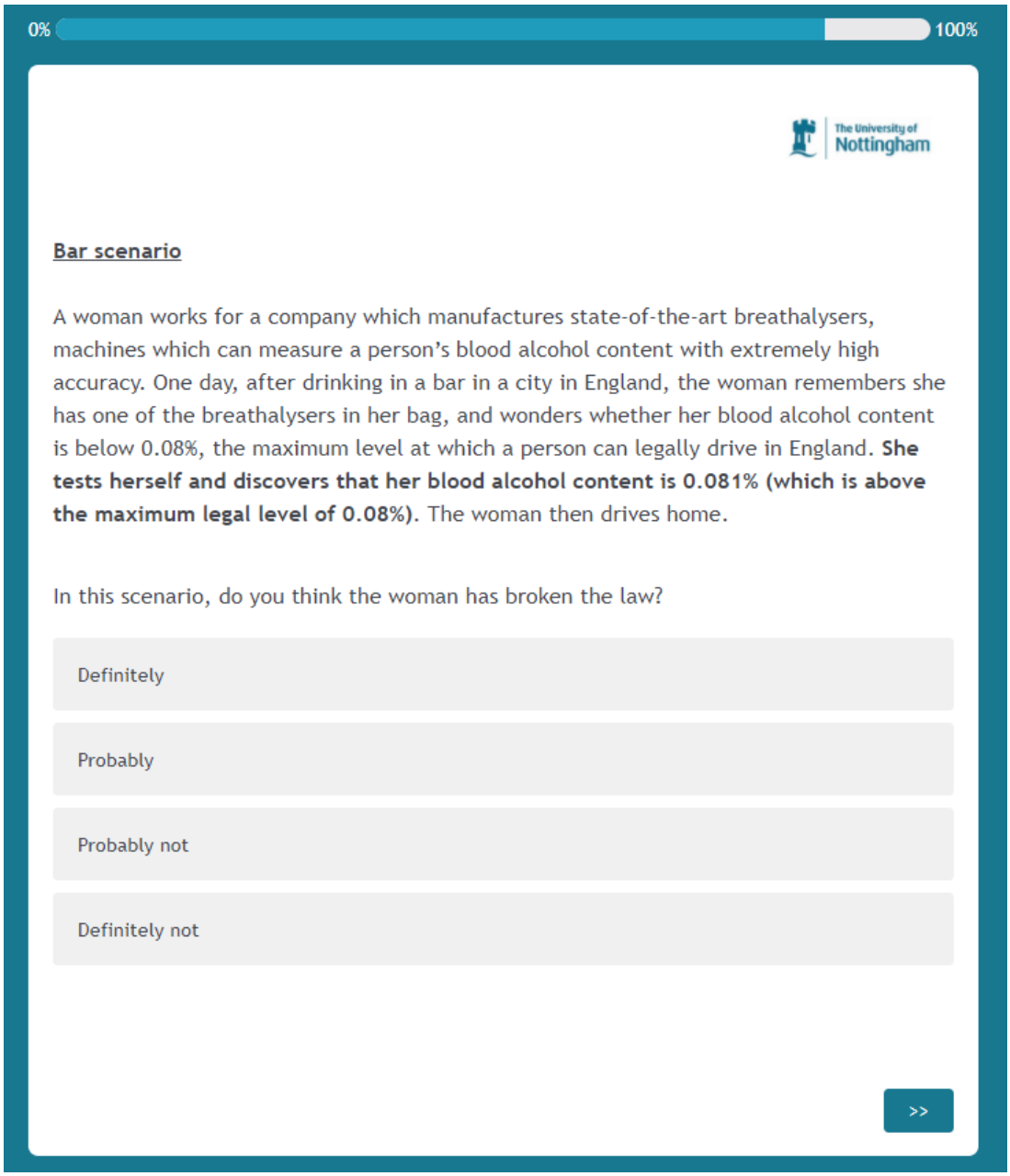
Shop scenario (continued)
A youth enters a local shop with the intention of buying some beer. He sees a sign in the shop reminding customers that in Britain it is illegal for shopkeepers to sell alcohol to people younger than 18 years. The shopkeeper knows the youth personally, and knows that he is aged 17 years and 11 months (which is below the minimum legal age of 18 years). The shopkeeper knows that the youth often gets drunk and vandalises property in his neighbourhood. The youth, who appears sober, asks to buy a box containing 20 alcoholic beers, and the shopkeeper sells it to him.
If the shopkeeper in this scenario had wanted to avoid breaking the law, to what extent would achieving this have been within the shopkeeper's control?
It would have been completely within the shopkeeper's control
It would have been to a large extent within the shopkeeper's control
It would have been to a small extent within the shopkeeper's control
It would have been completely out of the shopkeeper's control
Suppose the police observe this behavior. How accurately can the police detect whether the shopkeeper in this scenario has broken the law?
Very accurately (beyond reasonable doubt)
Somewhat accurately
Somewhat inaccurately
Very inaccurately
Suppose the police have evidence, beyond reasonable doubt, that the shopkeeper in this scenario has broken the law. How likely would they be to take action against the shopkeeper?
Very likely
Somewhat likely
Somewhat unlikely
Very unlikely
>>



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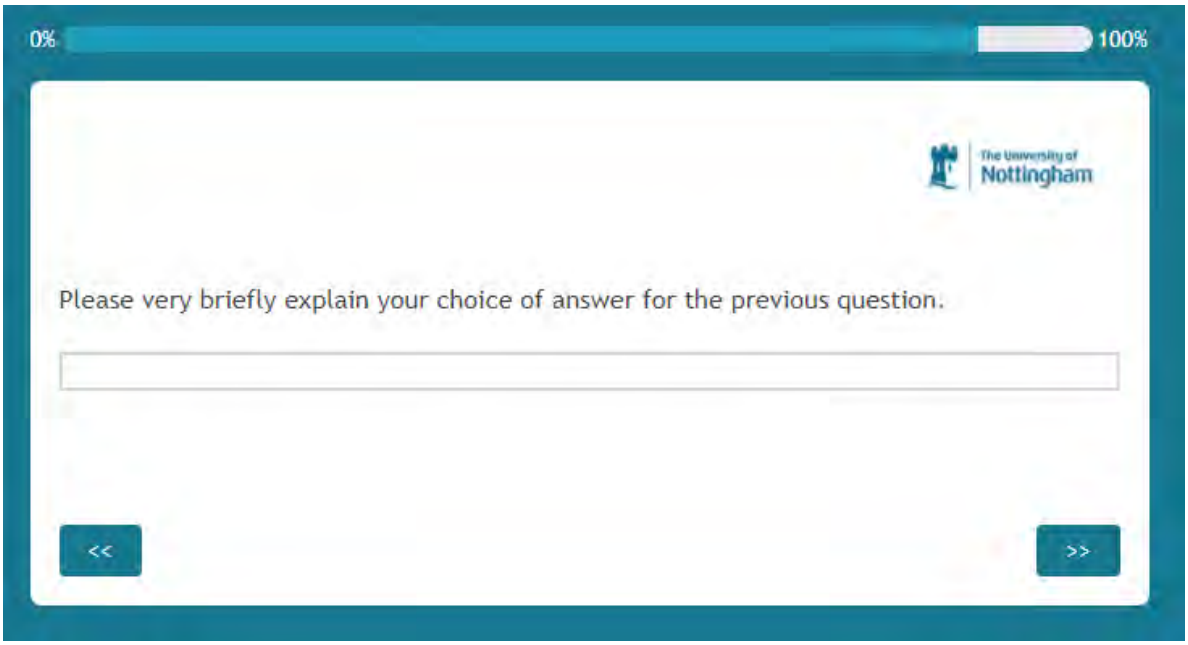
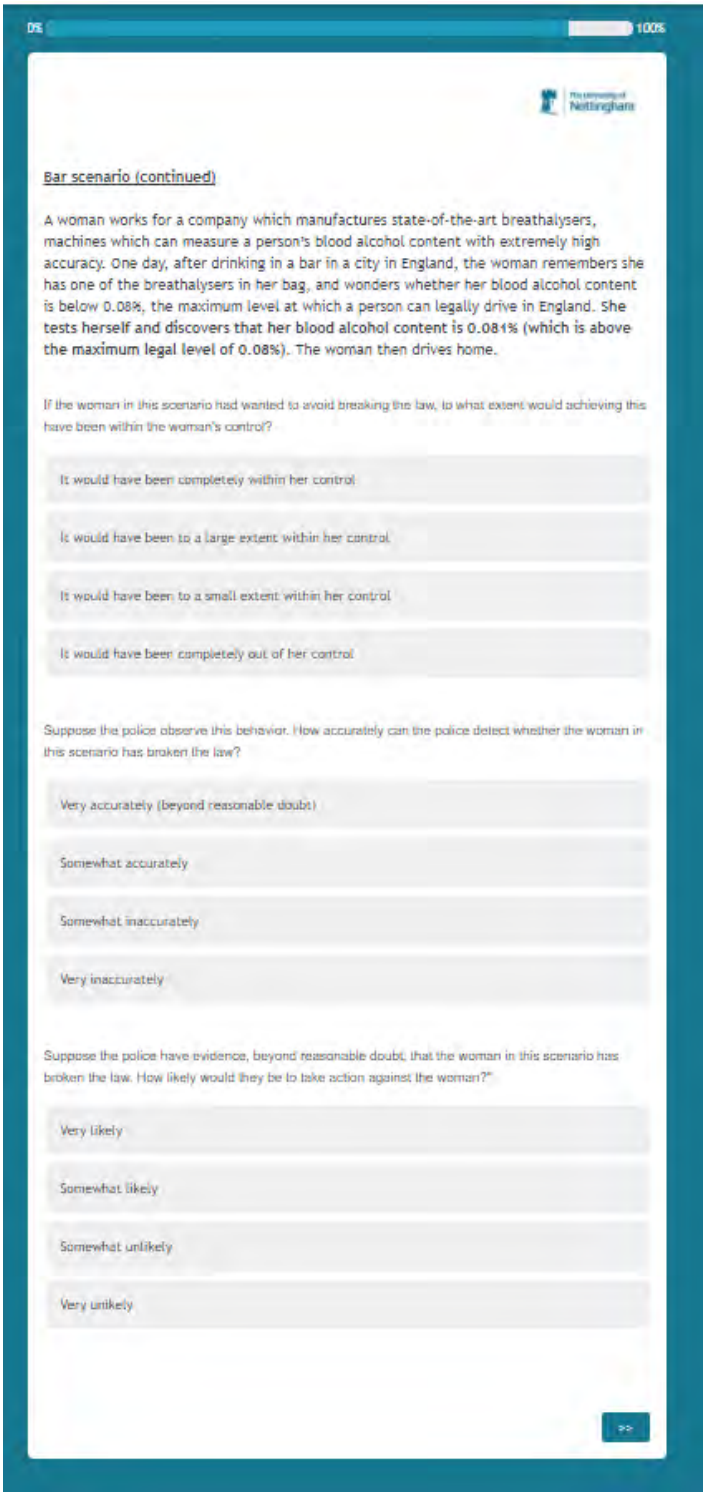


- Not included if subject answered *Definitely* to previous question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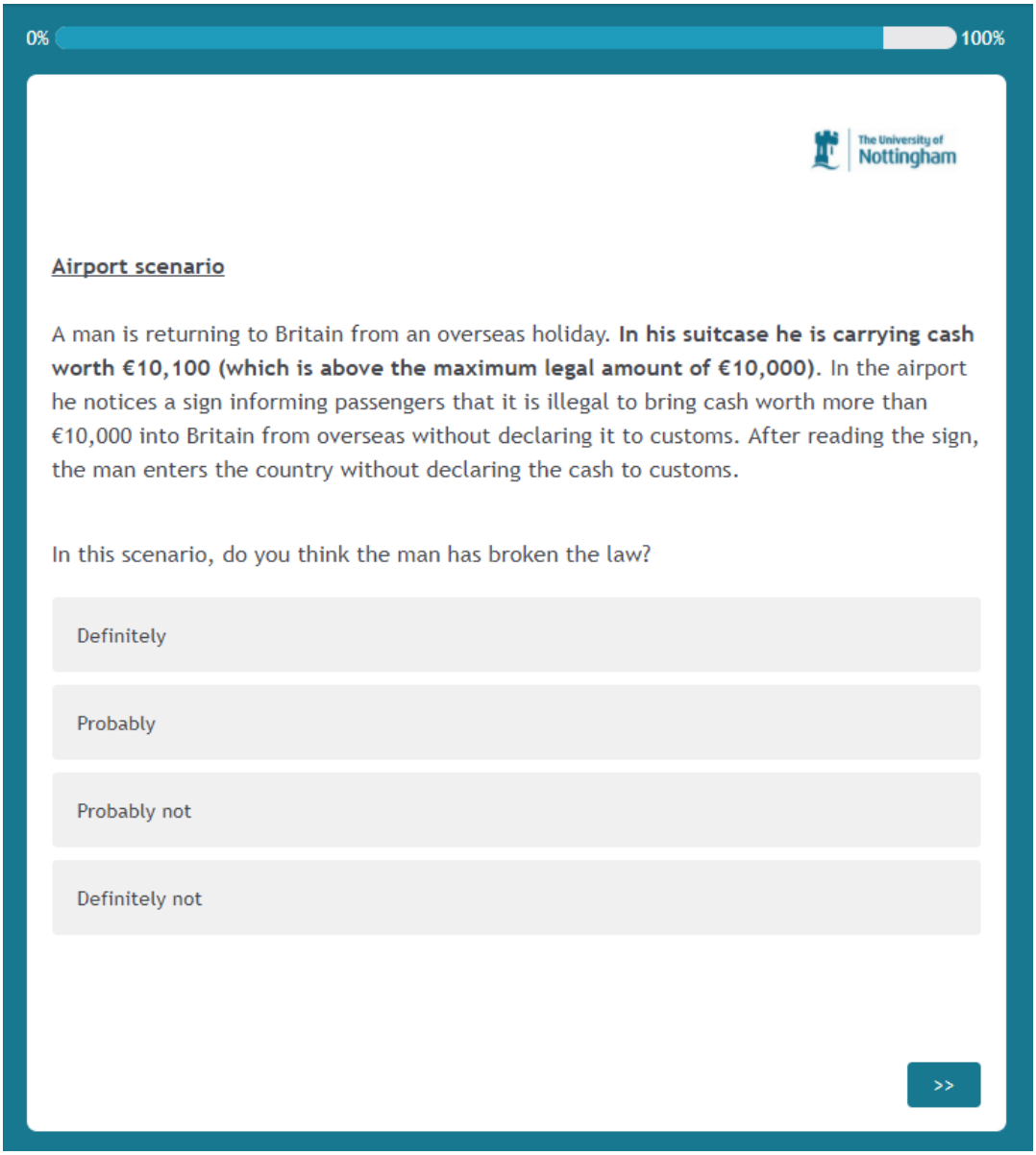
Bar scenario (continued)
A woman works for a company which manufactures state-of-the-art breathalysers, machines which can measure a person's blood alcohol content with extremely high accuracy. One day, after drinking in a bar in a city in England, the woman remembers she has one of the breathalysers in her bag, and wonders whether her blood alcohol content is below 0.08%, the maximum level at which a person can legally drive in England. She tests herself and discovers that her blood alcohol content is 0.081% (which is above the maximum legal level of 0.08%). The woman then drives home.
If the woman in this scenario had wanted to avoid breaking the law, to what extent would achieving this have been within the woman's control?
It would have been completely within her control
It would have been to a large extent within her control
It would have been to a small extent within her control
It would have been completely out of her control
Suppose the police observe this behavior. How accurately can the police detect whether the woman in this scenario has broken the law?
Very accurately (beyond reasonable doubt)
Somewhat accurately
Somewhat inaccurately
Very inaccurately
Suppose the police have evidence, beyond reasonable doubt, that the woman in this scenario has broken the law. How likely would they be to take action against the woman?*
Very likely
Somewhat likely
Somewhat unlikely
Very unlikely
>>



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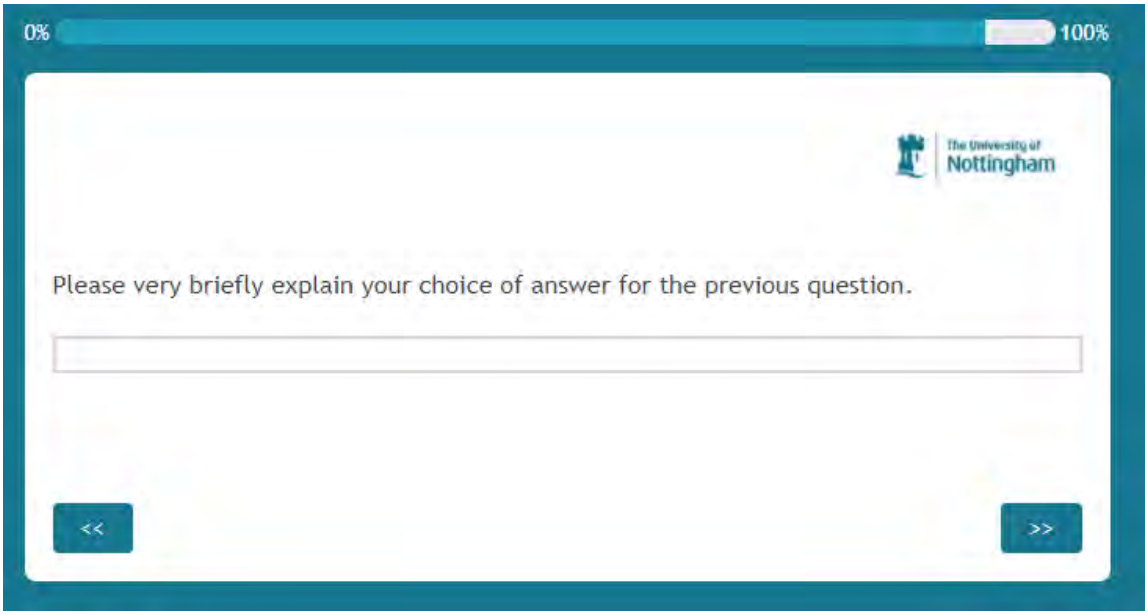
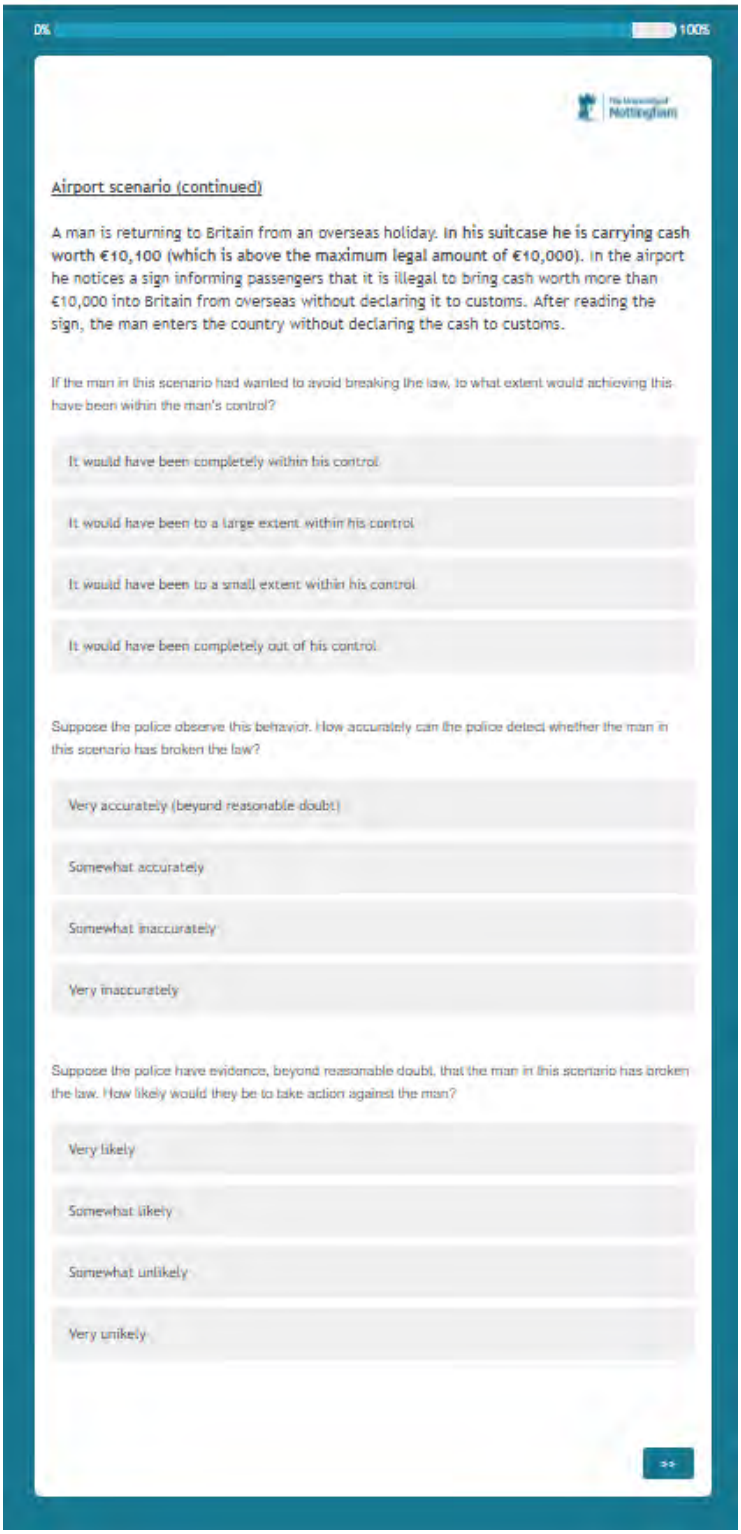


- Not included if subject answered *Definitely* to previous question.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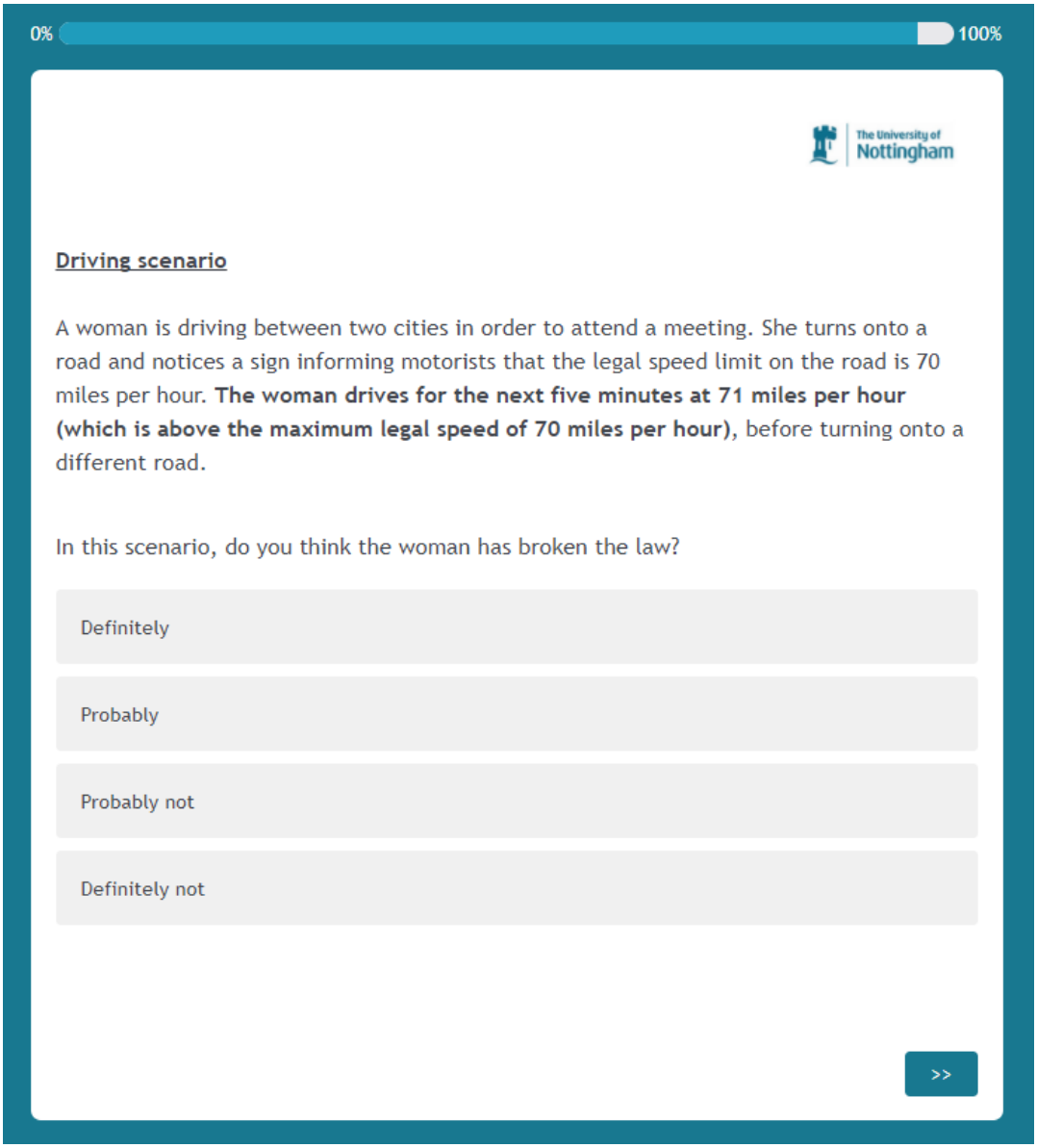
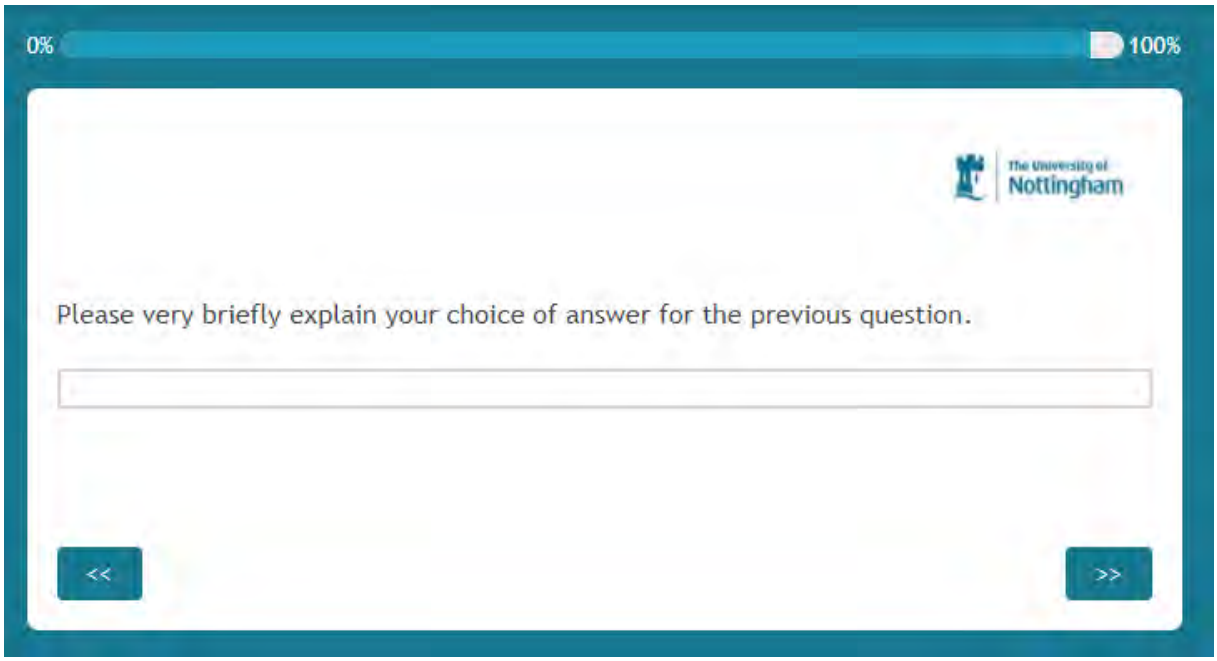
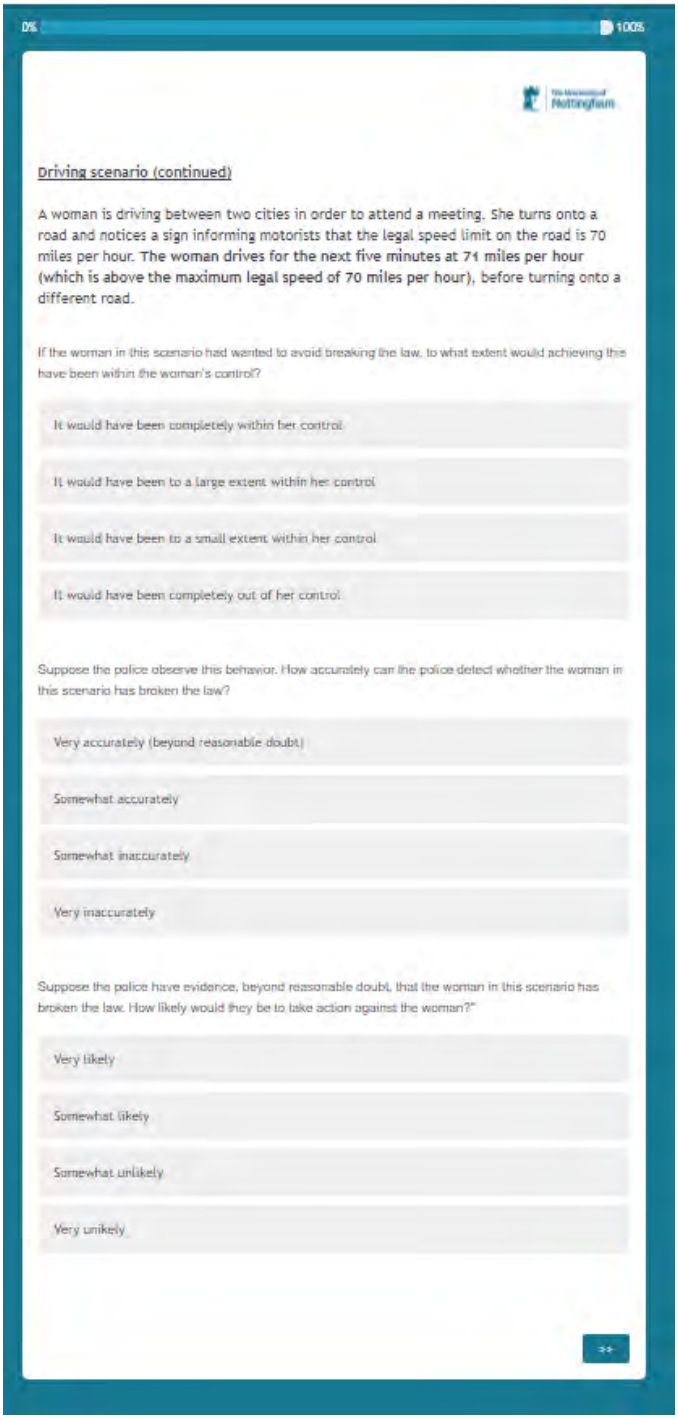
Airport scenario (continued)
A man is returning to Britain from an overseas holiday. In his suitcase he is carrying cash worth €10,100 (which is above the maximum legal amount of €10,000). In the airport he notices a sign informing passengers that it is illegal to bring cash worth more than €10,000 into Britain from overseas without declaring it to customs. After reading the sign, the man enters the country without declaring the cash to customs.
If the man in this scenario had wanted to avoid breaking the law, to what extent would achieving this have been within the man's control?
It would have been completely within his control
It would have been to a large extent within his control
It would have been to a small extent within his control
It would have been completely out of his control
Suppose the police observe this behavior. How accurately can the police detect whether the man in this scenario has broken the law?
Very accurately (beyond reasonable doubt)
Somewhat accurately
Somewhat inaccurately
Very inaccurately
Suppose the police have evidence, beyond reasonable doubt, that the man in this scenario has broken the law. How likely would they be to take action against the man?
Very likely
Somewhat likely
Somewhat unlikely
Very unlikely
>>



- This screen, and the screen after next, were changed slightly after reviewing the responses to our soft launch data (N=35) from the Krupka-Weber version of the Main experiment, which revealed some subjects were misunderstanding these follow up questions relating to each of the scenarios, incorrectly believing the questions were asking about the version of the vignette they evaluated earlier. The sentence beginning *The woman drives…*was not initially in bold, and the parentheses *(which is above the maximum legal speed of 70 miles per hour)* was absent. Other experiments in which these screens were present always featured the updated versions.



- Not included if subject answered *Definitely* to previous question.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Driving scenario (continued)
A woman is driving between two cities in order to attend a meeting. She turns onto a road and notices a sign informing motorists that the legal speed limit on the road is 70 miles per hour. The woman drives for the next five minutes at 71 miles per hour (which is above the maximum legal speed of 70 miles per hour), before turning onto a different road.
If the woman in this scenario had wanted to avoid breaking the law, to what extent would achieving this have been within the woman's control?
It would have been completely within her control
It would have been to a large extent within her control
It would have been to a small extent within her control
It would have been completely out of her control
Suppose the police observe this behavior. How accurately can the police detect whether the woman in this scenario has broken the law?
Very accurately (beyond reasonable doubt)
Somewhat accurately
Somewhat inaccurately
Very inaccurately
Suppose the police have evidence, beyond reasonable doubt, that the woman in this scenario has broken the law. How likely would they be to take action against the woman?"
Very likely
Somewhat likely
Somewhat unlikely
Very unlikely
>>

China experiment

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关于在社会上的得当性Qualtrics问卷调查的参与者信息表

尊敬的参与者:

感谢你愿意做这份与我们在宁波诺丁汉大学的研究项目相关的问卷调查。此项目研究人们对特定行为的在社会上的得当性认知。在以下的调查中，我们将向你说明一系列某人可能做出的假定行为，并要求你报告你觉得这些行为在社会上的得当程度如何。根据你的回答和其他参与者对调查的回答，你可能会收到参与酬金。

你参与这项调查出于自愿。你可以随时退出调查，并且可以请求不将所提供的信息用于该项目。所提供的任何信息将予以保密。你必需填写学生证号码，以便我们联系被选中的参与者收取酬金，但在储存资料时，我们将尽快使其匿名化，且不会向任何第三方披露你的身份。

该研究项目已根据伦理审查流程在宁波诺丁汉大学审查到位。该等流程受到大学研究行为守则和研究道德的管制。目前或今后你若有任何疑问，请联系我们。你若对我们进行的调查或研究道德有任何顾虑，请联系大学的伦理委员会。

致礼，
Tom Lane

联系方式:
研究者: Tom Lane Tom.Lane@nottingham.edu.cn
Daniele Nosenzo Daniele.Nosenzo@nottingham.ac.uk

诺丁汉大学研究道德委员会秘书：Ms Joanna Huang
(Joanna.Huang@nottingham.edu.cn)

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参与者同意表

项目主题： 关于在社会上的得当性的Qualtrics问卷调查

研究者姓名： Tom Lane和Daniele Nosenzo

请点选以下所有声明，以确认你同意各项声明。

本人已阅读声明，项目组织者已经我解释了研究项目的性质和宗旨。本人理解并同意参与。

本人理解项目的目的和在项目中的参与作用。

本人明白可以在研究项目的任何阶段退出，不会因此影响现在以及将来的状况

本人明白研究过程中信息可能会被公开，但本人身份不会被确认，个人的调查结果始终是被保密。

本人了解数据会根据数据保护相关法律进行存储

本人理解，如果需要更多有关研究的信息，可以联系研究者。并且如果想就本人与研究的牵涉提出投诉，可以联系宁波诺丁汉大学研究道德委员会。

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完成这份调查大约需要45分钟。如果需要暂停，你可以保存你的回答，以后再继续作答。
首先，请输入你的学生证号码。确保准确输入，我们将通过该号码就酬金事宜与你联系。
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关于酬金：
所有参与者完成调查后，我们会随机在每五位参与者中挑选一位获取酬金。我们将在9月28日前发电子邮件给每位参与者，告知其是否被选中获取酬金。被选中的参与者届时可以到UNNC校园的行政楼领取酬金。若对这份调查的酬金有任何疑问，请发送电邮至 Tom.Lane@nottingham.edu.cn.
你若被选中获得酬金，你将收到62元的参与费。根据你对调查问卷的答复，你还有可能获得186元的额外奖金。在调查问卷的相关问题上会提供更多详情。
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关于这份调查的信息
这份调查将描述15个假设情境，然后就这些情景询问你觉得某种行为在社会上的得当程度如何。在每种情景下，你必须在这种行为“在社会上被认为是得当的”还是“在社会上被认为是不得当的”，将会有四个可能的回应（如下所示），你必须从其中选择一个。
在社会上被认为是非常得当的
在社会上被认为是有些得当的
在社会上被认为是有些不得当的
在社会上被认为是非常不得当的
我们用“在社会上被认为是得当的”描述的行为是指你认为大多数人会认同这是应该做的“正确”事。从另一个角度考虑，如果某人做出的行为在社会上是不得当的，那么其他人可能会对他表示生气。要注意的是，该做的“正确”事不一定在法律上有明确规定或受到法律支持，也不一定受法律制裁的威胁而必须遵守。所以，即使某行为违反法律，也有可能是‘得当的’；又或者即使其并没有违法，也有可能是‘不得当的’。更确切地说，一个得当的行为指的是大多数人认为这是一个应该要做的行为（不论其是否合法）；如果某人不做此行为，其他人可能会随时准备表达他们的不满。
在你作出每个回应时，你将要考虑大多数人认为该行为是得当的还是不得当的。
完成所有调查问卷的回答后，我们将随机挑选你被问及的其中一个情境。对于该情境所描述的行为，我们会了解你对该行为在社会上的得当程度如何所作出的回答。如果你对该问题的回答和在本次调查中被参与者们选中最多次的回答一致，并且你是被选中的有酬参与者，则除了参与酬金外，我们将给你发放180元的奖金。本次调查的所有参与者均为中国人并且在宁波诺丁汉大学学习。
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现在，我们将举个可能情境的示例，并演示如何作答。
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示例情境

有位男子计划星期六去参加一个朋友的婚礼。他是个大球迷，在婚礼开始两天前，有人给他一场重要球赛的免费门票。他决定收下门票。周六时，他去观看了足球赛，并告诉他朋友因为生病不能参加婚礼。

假设你认为该行为在社会上是有些不得当的。那么你将作出如下回答：

大多数人会如何看待该男子的行为，这在社会上是否得当？

在社会上被认为是非常得当的

在社会上被认为是相对得当的

在社会上被认为是有些不得当的

在社会上被认为是非常不得当的

那么，如果你是其中一位被选中获得酬金的参与者，并且这就是我们随机选中查看的情景，我们将会给予你186元酬金，若"在社会上被认为是不恰当的"正是那个在本次调查中被参与者们选中最多的回答。如果被参与者们选中最多的是一个另外的回答，我们将不会给予你这笔186元的酬金。

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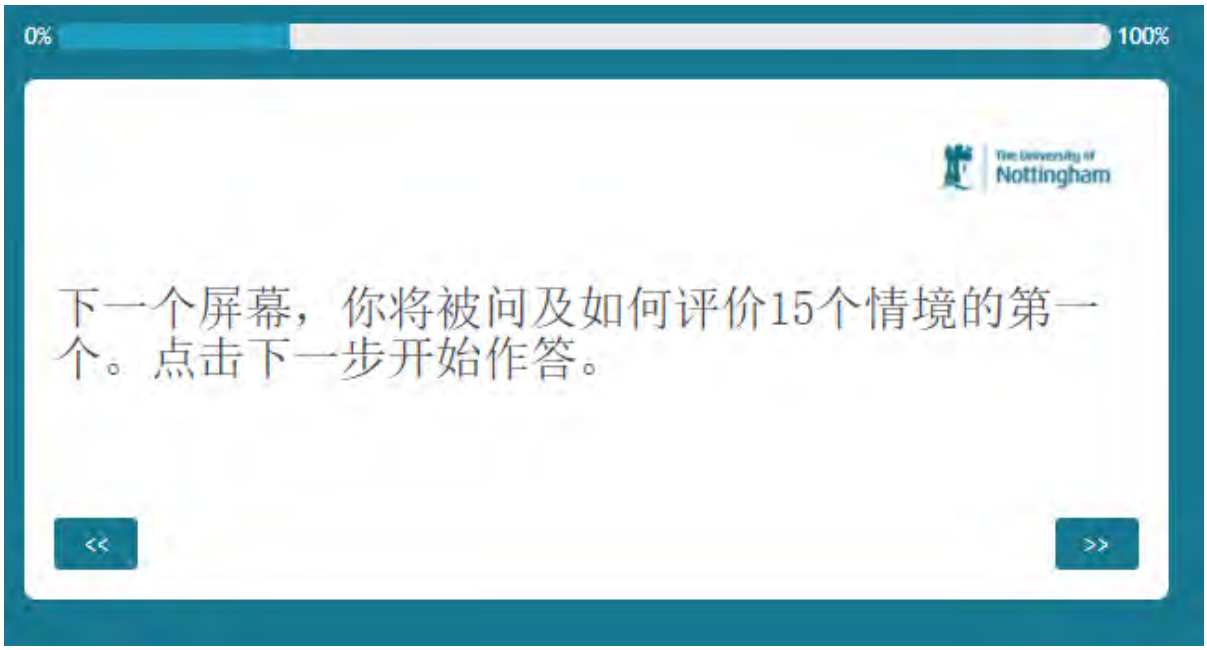
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下一个屏幕，你将被问及如何评价15个情境的第一个。点击下一步开始作答。
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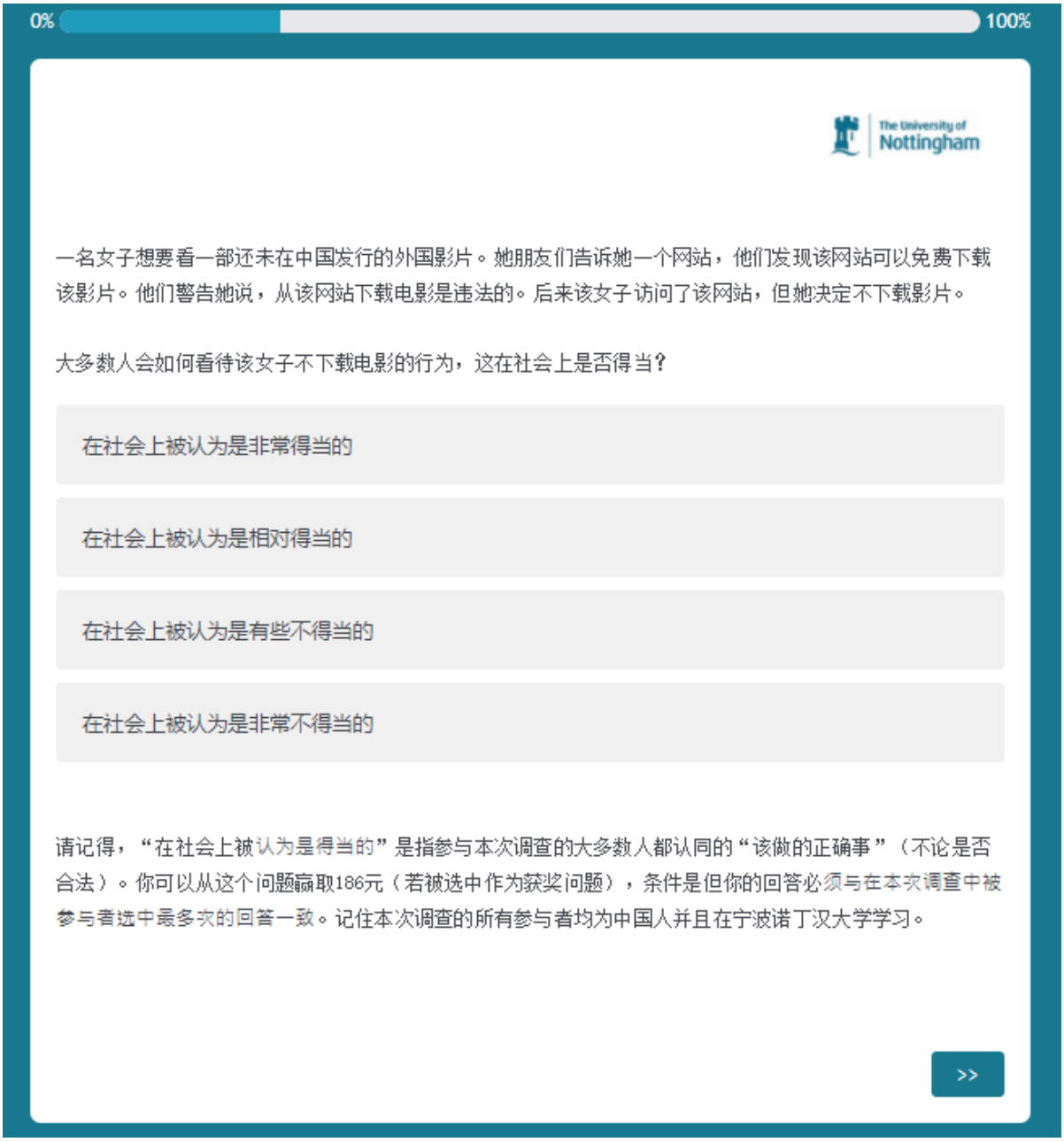
(The following three vignettes were presented in random order)



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在宁波附近的一个村庄，一名男子正帮一个朋友在两栋住所之间搬运货物。两栋住所间的距离是200米，而且今天路上没有其他车辆。该男子将货箱装上车后，准备在两栋住所之间行驶。他在收音机中听到一则新闻报道，提醒听众未系安全带开车是违法的。该男子并没有系安全带，然后以每小时15公里的最大速度行驶200米到另一处住所。

大多数人会如何看待该男子未系安全带行驶这趟旅程，这在社会上是否得当？

在社会上被认为是非常得当的

在社会上被认为是相对得当的

在社会上被认为是有些不得当的

在社会上被认为是非常不得当的

请记得，“在社会上被认为是得当的”是指参与本次调查的大多数人都认同的“该做的正确事”（不论是否合法）。你可以从这个问题赢取186元（若被选中作为获奖问题），条件是但你的回答必须与在本次调查中被参与者选中最多次的回答一致。记住本次调查的所有参与者均为中国人并且在宁波诺丁汉大学学习。

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一名女子存储了12,000元，她打算用这笔钱度一个豪华海滨假期。就在她预订假期前，读到一则新闻报道称一个慈善机构正在为某个贫困的非洲国家的饥民提供援助。该女子决定不预订海滨假期，而是向慈善机构捐出了12,000元。但是，她又改变了主意，然后预订了海滨假期，而没有向慈善机构捐款。

大多数人会怎么看待该女子预订海滨假期却不向慈善机构捐款的行为，这在社会上是否得当？

在社会上被认为是非常得当的

在社会上被认为是相对得当的

在社会上被认为是有些不得当的

在社会上被认为是非常不得当的

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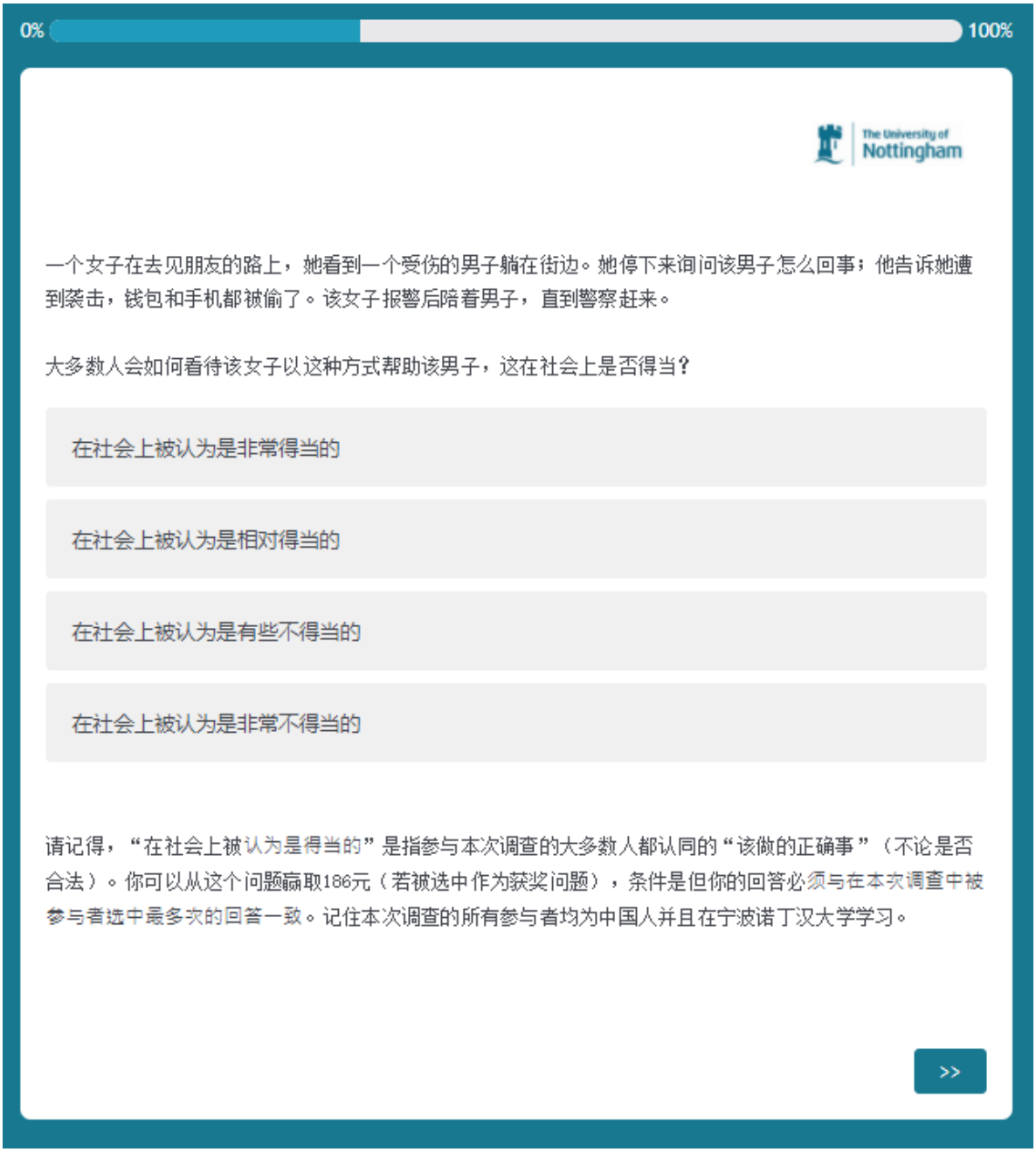
(The following 12 vignettes were presented in random order)



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一名学生正在校园附近某咖啡馆吃午饭。她起身准备离开时，发现地上有一个被丢的钱包。该学生看看周围无人注意，于是捡起钱包并带着离开了咖啡馆。

大多数人会如何看待该学生捡起钱包并带着离开咖啡馆的行为，这在社会上是否得当？

在社会上被认为是非常得当的

在社会上被认为是相对得当的

在社会上被认为是有些不得当的

在社会上被认为是非常不得当的

请记得，“在社会上被认为是得当的”是指参与本次调查的大多数人都认同的“该做的正确事”（不论是否合法）。你可以从这个问题赢取186元（若被选中作为获奖问题），条件是但你的回答必须与在本次调查中被参与者选中最多次的回答一致。记住本次调查的所有参与者均为中国人并且在宁波诺丁汉大学学习。

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一个青年人去当地一家店买了几瓶啤酒。他看到店里的一个标牌提醒顾客：在中国店主向未满18岁的青年出售含酒精的饮料是违法行为。店主认识该青年，并且知道他17岁零9个月。店主知道该青年经常喝醉并且破坏周边小区的财产。该名看上去头脑清醒的青年人要求买一箱（20瓶）啤酒，店主就卖了一箱啤酒给他。

大多数人会如何看待店主向该青年出售啤酒的行为，这在社会上是否得当？

在社会上被认为是非常得当的

在社会上被认为是相对得当的

在社会上被认为是有些不得当的

在社会上被认为是非常不得当的

请记得，“在社会上被认为是得当的”是指参与本次调查的大多数人都认同的“该做的正确事”（不论是否合法）。你可以从这个问题赢取186元（若被选中作为获奖问题），条件是但你的回答必须与在本次调查中被参与者选中最多次的回答一致。记住本次调查的所有参与者均为中国人并且在宁波诺丁汉大学学习。

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宁波的一家建筑公司在向政府投标，为赢得一个大型基建项目的合同。公司首席执行官（CEO）参加了一个有政府重要官员进行演讲的会议。该官员在演讲中提及最近有位商人试图向他行贿。该官员说向政府官员行贿不仅是违法行为，而且对公司不利。会后，该CEO要求与该官员私下交谈，然后向他行贿600万元，以确保该建筑公司能够中标。

大多数人会如何看待该CEO向官员行贿的行为，这在社会上是否得当？

在社会上被认为是非常得当的

在社会上被认为是相对得当的

在社会上被认为是有些不得当的

在社会上被认为是非常不得当的

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一个女子为一家生产先进的呼气测醉器的公司工作，该仪器能测量人体血液酒精含量，准确率极高。某天，在宁波某酒吧喝酒后，该女子记起包里有一个呼气测醉器，想测一测她血液中的酒精含量是否低于0.02%。在中国超过该含量开车属于非法行为。她自测后发现血液中的酒精含量为0.019%。随后她驾车回家。

大多数人会怎么看待该女子驱车回家的行为，这在社会上是否得当？

在社会上被认为是非常得当的

在社会上被认为是相对得当的

在社会上被认为是有些不得当的

在社会上被认为是非常不得当的

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一名女子为参加一个会议在两座城市之间行驶。她转到一条马路上，看到一个标牌指示驾驶员该路段的合法限速为每小时120公里。接下来的五分钟内她以每小时123公里的速度行驶，直到拐入另一条路。

大多数人会如何看待该女子以每小时123公里的速度行驶的行为，这在社会上是否得当？

在社会上被认为是非常得当的

在社会上被认为是相对得当的

在社会上被认为是有些不得当的

在社会上被认为是非常不得当的

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一名男子结束了海外旅行准备返回中国。他在行李箱中携带数额4,900美元的现金。他在机场注意到一个标牌告知旅客：携带数额超过5,000美元的现金从国外入境中国但未向海关申报属于违法行为。读过标牌后，该男子未向海关申报就入境了。

大多数人会如何看待该男子未向海关申报现金就入境的行为，这在社会上是否得当？

在社会上被认为是非常得当的

在社会上被认为是相对得当的

在社会上被认为是有些不得当的

在社会上被认为是非常不得当的

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宁波的一名医生在下班回家的路上，被一名男子靠近乞讨金钱。该医生不理乞丐并径自走开了。

大多数人会如何看待医生不理乞丐径自走开的行为，这在社会上是否得当？

在社会上被认为是非常得当的

在社会上被认为是相对得当的

在社会上被认为是有些不得当的

在社会上被认为是非常不得当的

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一名房主在半夜醒来，发现一名窃贼想偷他家的电视机。他抓住窃贼并下重手打他，打断了窃贼的胳膊，然后从前门将他仍出去。
大多数人会如何看待该房主这样对待窃贼，这在社会上是否得当？
在社会上被认为是非常得当的
在社会上被认为是相对得当的
在社会上被认为是有些不得当的
在社会上被认为是非常不得当的
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一名学生在参加一个她预计会失利的考试。考前她在一页纸上写下一些笔记，然后藏在袖子里。考试开始前，监考人宣布并提醒所有学生：携带任何材料进入考场帮助答题是违规的。考试期间，该学生偷看笔记并用以帮助她作答。

大多数人会如何看待该学生以这种方式使用笔记，这在社会上是否得当？

在社会上被认为是非常得当的

在社会上被认为是相对得当的

在社会上被认为是有些不得当的

在社会上被认为是非常不得当的

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一男子在一家餐馆吃饭。上菜很慢，并且味道较差。他让服务员结账，但十分钟过后服务员还未回来。该男子未付款就离开了餐馆。

大多数人会如何看待该男子未结账就走掉的行为，这在社会上是否得当？

在社会上被认为是非常得当的

在社会上被认为是相对得当的

在社会上被认为是有些不得当的

在社会上被认为是非常不得当的

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一名18岁的男子在一个聚会上结识了一个女孩。他邀请女孩来家里，女孩也接受了邀请。到家后，男子告诉女孩：他想和她发生性关系，但女孩看起来很小。男子继而问女孩是否年满14岁即法律允许的性行为的最低年龄。女孩告诉男子她的年龄是14岁零1个月，并向她出示了身份证确认。她告诉男子想和他发生性关系。随后该男子就与她发生了性关系。

大多数人会如何看待该男子与那女孩发生性关系的行为，这在社会上是否得当？

在社会上被认为是非常得当的

在社会上被认为是相对得当的

在社会上被认为是有些不得当的

在社会上被认为是非常不得当的

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D. Distributions of appropriateness ratings in the experiments

D.1. Main experiment

TABLE D1: APPROPRIATENESS OF SEX IN AGE OF CONSENT VIGNETTE: DISTRIBUTION OF RATINGS – MAIN EXPERIMENT

		Very (socially) inappropriate	Somewhat (socially) inappropriate	Somewhat (socially) appropriate	Very (socially) appropriate
Sample 1: UK Students, Krupka-Weber method (N = 197)					
Age of girl (years, months)	16, 3	16.7	35.4	29.2	18.8
	16, 1	23.3	27.9	30.2	18.6
	15, 11	66.7	33.3	0.0	0.0
	15, 9	59.0	31.2	9.8	0.0
Sample 2: UK General population, Krupka-Weber method (N = 375)					
Age of girl (years, months)	16, 4	5.3	26.3	26.3	42.1
	16, 3	8.0	24.0	46.0	22.0
	16, 2	10.0	17.5	45.0	27.5
	16, 1	6.4	23.8	49.2	20.6
	15, 11	73.8	16.4	6.6	3.3
	15, 10	77.1	8.6	8.6	5.7
	15, 9	76.5	13.7	7.8	2.0
	15, 8	83.8	8.1	2.7	5.4
Sample 2: UK General population, Opinion matching – first order beliefs (N = 342)					
Age of girl (years, months)	16, 4	5.1	25.6	33.3	35.9
	16, 3	16.7	16.7	54.8	11.9
	16, 2	14.6	34.2	26.8	24.4
	16, 1	16.9	33.9	33.9	15.4
	15, 11	81.7	16.7	1.7	0.0
	15, 10	95.5	4.6	0.0	0.0
	15, 9	87.8	12.2	0.0	0.0
	15, 8	87.5	6.3	3.1	3.1
Sample 2: UK General population, Opinion matching – second order beliefs (N = 334)					
Age of girl (years, months)	16, 4	21.9	25.0	34.4	18.8
	16, 3	10.3	28.2	43.6	18.0
	16, 2	14.6	29.3	31.7	24.4
	16, 1	22.2	33.3	27.8	16.7
	15, 11	83.3	11.9	2.4	2.4
	15, 10	91.8	8.2	0.0	0.0
	15, 9	87.5	10.0	2.5	0.0
	15, 8	89.2	10.8	0.0	0.0

Notes: Table D1 displays the percentages of subjects, by sample and treatment, who chose each evaluation in the Age of consent vignette. In each case, the modal evaluation is shaded.

TABLE D2: APPROPRIATENESS OF SALE IN ALCOHOL TO YOUTH VIGNETTE: DISTRIBUTION OF RATINGS – MAIN EXPERIMENT

		Very (social)y inappropriate	Somewhat (socially) inappropriate	Somewhat (socially) appropriate	Very (socially) appropriate
Sample 1: UK Students, Krupka-Weber method (N = 197)					
Age of youth (years, months)	18, 3	2.4	26.2	45.2	26.2
	18, 1	1.7	32.8	39.7	25.9
	17, 11	73.9	23.9	2.2	0.0
	17, 9	82.4	13.7	3.9	0.0
Sample 2: UK General population, Krupka-Weber method (N = 375)					
Age of youth (years, months)	18, 4	20.0	45.0	25.0	10.0
	18, 3	11.6	25.6	30.2	32.6
	18, 2	12.5	33.3	33.3	20.8
	18, 1	11.5	36.1	41.0	11.5
	17, 11	83.0	13.2	1.9	1.9
	17, 10	83.3	9.5	4.8	2.4
	17, 9	72.2	13.9	8.3	5.6
	17, 8	86.5	9.6	3.9	0.0
Sample 2: UK General population, Opinion matching – first order beliefs (N = 342)					
Age of youth (years, months)	18, 4	6.1	27.3	39.4	27.3
	18, 3	10.0	30.0	46.7	13.3
	18, 2	9.4	28.1	28.1	34.4
	18, 1	10.0	25.0	43.3	21.7
	17, 11	85.5	13.0	1.5	0.0
	17, 10	90.5	9.5	0.0	0.0
	17, 9	83.4	13.6	0.0	0.0
	17, 8	96.9	3.1	0.0	0.0
Sample 2: UK General population, Opinion matching – second order beliefs (N = 334)					
Age of youth (years, months)	18, 4	8.1	35.1	35.1	21.6
	18, 3	15.0	35.0	40.0	10.0
	18, 2	0.0	40.0	36.0	24.0
	18, 1	7.3	29.1	41.8	21.8
	17, 11	87.0	13.0	0.0	0.0
	17, 10	84.8	15.2	0.0	0.0
	17, 9	93.2	6.8	0.0	0.0
	17, 8	87.9	9.1	3.0	0.0

Notes: Table D2 displays the percentages of subjects, by sample and treatment, who chose each evaluation in the Alcohol to youth vignette. In each case, the modal evaluation is shaded.

TABLE D3: APPROPRIATENESS OF NON-DECLARATION IN CASH AT CUSTOMS VIGNETTE: DISTRIBUTION OF RATINGS – MAIN EXPERIMENT

		Very (socially) inappropriate	Somewhat (socially) inappropriate	Somewhat (socially) appropriate	Very (socially) appropriate
Sample 1: UK Students, Krupka-Weber method (N = 197)					
Amount imported (Euros)	9,700	2.6	0.0	15.4	82.0
	9,900	0.0	4.2	12.5	83.3
	10,100	5.6	57.4	27.8	9.3
	10,300	12.5	57.1	26.8	3.6
Sample 2: UK General population, Krupka-Weber method (N = 375)					
Amount imported (Euros)	9,600	10.3	2.6	12.8	74.4
	9,700	9.5	11.9	33.3	45.2
	9,800	4.7	4.7	18.6	72.1
	9,900	3.2	7.9	12.7	76.2
	10,100	14.8	49.2	31.2	4.9
	10,200	17.8	55.6	15.6	11.1
	10,300	18.4	44.7	34.2	2.6
	10,400	18.2	45.5	31.8	4.6
Sample 2: UK General population, Opinion matching – first order beliefs (N = 342)					
Amount imported (Euros)	9,600	2.1	2.1	12.8	83.0
	9,700	0.0	2.6	10.5	86.8
	9,800	0.0	3.9	7.7	88.5
	9,900	4.8	1.6	14.5	79.0
	10,100	11.5	50.0	26.9	11.5
	10,200	13.0	54.4	26.1	6.5
	10,300	24.3	46.0	24.3	5.4
	10,400	17.7	58.8	23.5	0.0
Sample 2: UK General population, Opinion matching – second order beliefs (N = 334)					
Amount imported (Euros)	9,600	7.7	0.0	7.7	84.6
	9,700	0.0	0.0	2.5	97.5
	9,800	0.0	2.8	0.0	97.2
	9,900	4.8	1.6	6.4	87.3
	10,100	14.8	50.8	23.0	11.5
	10,200	12.1	60.6	21.2	6.1
	10,300	5.4	62.2	24.3	8.1
	10,400	15.8	63.2	13.2	7.9

Notes: Table D3 displays the percentages of subjects, by sample and treatment, who chose each evaluation in the Cash at customs vignette. In each case, the modal evaluation is shaded.

TABLE D4: APPROPRIATENESS OF DRIVING IN DRINK DRIVING VIGNETTE: DISTRIBUTION OF RATINGS – MAIN EXPERIMENT

		Very (socially) inappropriate	Somewhat (socially) inappropriate	Somewhat (socially) appropriate	Very (socially) appropriate
Sample 1: UK Students, Krupka-Weber method (N = 197)					
Blood alcohol content	0.077%	8.2	32.7	42.9	16.3
	0.079%	12.5	31.3	37.5	18.8
	0.081%	18.4	61.2	16.3	4.1
	0.083%	25.5	54.9	17.7	2.0
Sample 2: UK General population, Krupka-Weber method (N = 375)					
Blood alcohol content	0.076%	21.6	24.3	35.1	18.9
	0.077%	22.6	18.9	28.3	30.2
	0.078%	25.7	20.0	40.0	14.3
	0.079%	15.5	23.9	36.6	23.9
	0.081%	45.3	32.0	16.0	6.7
	0.082%	42.4	42.4	12.1	3.0
	0.083%	50.0	25.0	13.9	11.1
	0.084%	42.9	42.9	8.6	5.7
Sample 2: UK General population, Opinion matching – first order beliefs (N = 342)					
Blood alcohol content	0.076%	15.4	41.0	30.8	12.8
	0.077%	20.6	29.4	38.2	11.8
	0.078%	2.4	41.5	43.9	12.2
	0.079%	14.8	37.0	27.8	20.4
	0.081%	46.3	37.0	13.0	3.7
	0.082%	52.3	40.9	2.3	4.6
	0.083%	64.3	28.6	7.1	0.0
	0.084%	61.8	35.3	2.9	0.0
Sample 2: UK General population, Opinion matching – second order beliefs (N = 334)					
Blood alcohol content	0.076%	16.1	25.8	35.5	22.6
	0.077%	10.9	32.6	37.0	19.6
	0.078%	11.1	33.3	48.2	7.4
	0.079%	6.0	34.0	40.0	20.0
	0.081%	36.4	45.5	18.2	0.0
	0.082%	56.5	23.9	13.0	6.5
	0.083%	51.4	32.4	10.8	5.4
	0.084%	59.5	28.6	11.9	0.0

Notes: Table D4 displays the percentages of subjects, by sample and treatment, who chose each evaluation in the Drink driving vignette. In each case, the modal evaluation is shaded.

TABLE D5: APPROPRIATENESS OF SPEED IN SPEEDING VIGNETTE: DISTRIBUTION OF RATINGS – MAIN EXPERIMENT

		Very (socially) inappropriate	Somewhat (socially) inappropriate	Somewhat (socially) appropriate	Very (socially) appropriate
Sample 1: UK Students, Krupka-Weber method (N = 197)					
Speed (miles per hour)	67	0.0	1.9	19.2	78.9
	69	0.0	0.0	10.0	90.0
	71	0.0	4.3	42.6	53.2
	73	5.2	31.0	37.9	25.9
Sample 2: UK General population, Krupka-Weber method (N = 375)					
Speed (miles per hour)	66	4.0	14.0	14.0	68.0
	67	2.7	5.4	29.7	62.2
	68	2.4	11.9	26.2	59.5
	69	0.0	7.8	21.6	70.6
	71	5.1	30.5	42.4	22.0
	72	12.1	39.4	21.2	27.3
	73	14.0	34.0	42.0	10.0
	74	15.1	47.2	24.5	13.2
Sample 2: UK General population, Opinion matching – first order beliefs (N = 342)					
Speed (miles per hour)	66	0.0	2.8	19.4	77.8
	67	0.0	0.0	6.5	93.6
	68	0.0	0.0	15.4	84.6
	69	0.0	0.0	27.8	72.2
	71	2.7	38.4	41.1	17.8
	72	5.1	56.4	30.8	7.7
	73	12.5	31.3	37.5	18.8
	74	18.4	52.6	18.4	10.5
Sample 2: UK General population, Opinion matching – second order beliefs (N = 334)					
Speed (miles per hour)	66	0.0	0.0	8.9	91.1
	67	0.0	0.0	11.1	88.9
	68	0.0	2.9	11.4	85.7
	69	0.0	0.0	13.0	87.0
	71	7.4	33.3	40.7	18.5
	72	5.6	38.9	27.8	27.8
	73	7.1	47.6	42.9	2.4
	74	22.0	48.8	17.1	12.2

Notes: Table D5 displays the percentages of subjects, by sample and treatment, who chose each evaluation in the Speeding vignette. In each case, the modal evaluation is shaded.

D.2. Placebo experiment

TABLE D6: APPROPRIATENESS OF SEX IN AGE OF CONSENT VIGNETTE: DISTRIBUTION OF RATINGS – PLACEBO EXPERIMENT, KRUPKA-WEBER METHOD (N = 653)

		Very socially inappropriate	Somewhat socially inappropriate	Somewhat socially appropriate	Very socially appropriate
Age of girl (years, months)	16, 4	40.0	17.1	34.3	8.6
	16, 3	11.4	43.2	34.1	11.4
	16, 2	22.7	40.9	25.0	11.4
	16, 1	23.6	30.6	33.3	12.5
	15, 11	93.0	7.0	0.0	0.0
	15, 10	87.5	12.5	0.0	0.0
	15, 9	92.3	5.8	1.9	0.0
	15, 8	92.3	7.7	0.0	0.0
	15, 7	91.4	7.1	1.4	0.0
	15, 5	93.0	5.6	1.4	0.0
	15, 4	93.3	6.7	0.0	0.0
	15, 3	96.7	3.3	0.0	0.0
	15, 2	97.9	2.1	0.0	0.0

Notes: Table D6 displays the percentages of subjects, by treatment, who chose each social appropriateness evaluation in the Age of consent vignette. In each case, the modal evaluation is shaded.

TABLE D7: APPROPRIATENESS OF SALE IN ALCOHOL TO YOUTH VIGNETTE: DISTRIBUTION OF RATINGS – PLACEBO EXPERIMENT, KRUPKA-WEBER METHOD (N = 653)

		Very socially inappropriate	Somewhat socially inappropriate	Somewhat socially appropriate	Very socially appropriate
Age of youth (years, months)	18, 10	9.3	14.0	48.8	27.9
	18, 9	2.1	17.4	41.3	39.1
	18, 8	5.6	16.7	37.0	40.7
	18, 7	6.0	14.0	40.0	40.0
	18, 5	11.9	13.4	43.3	31.3
	18, 4	6.5	15.2	37.0	41.3
	18, 3	4.6	27.3	29.6	38.6
	18, 2	2.0	19.6	31.4	47.1
	18, 1	8.2	18.0	34.4	39.3
	17, 11	76.8	21.4	0.0	1.8
	17, 10	83.7	14.0	2.3	0.0
	17, 9	71.1	29.0	0.0	0.0
	17, 8	75.9	22.2	0.0	1.9

Notes: Table D7 displays the percentages of subjects, by treatment, who chose each social appropriateness evaluation in the Alcohol to youth vignette. In each case, the modal evaluation is shaded.

TABLE D8: APPROPRIATENESS OF NON-DECLARATION IN CASH AT CUSTOMS VIGNETTE: DISTRIBUTION OF RATINGS – PLACEBO EXPERIMENT, KRUPKA-WEBER METHOD (N = 653)

		Very socially inappropriate	Somewhat socially inappropriate	Somewhat socially appropriate	Very socially appropriate
Amount imported (Euros)	9,600	3.8	7.6	15.1	73.6
	9,700	5.1	15.4	20.5	59.0
	9,800	0.0	7.1	10.7	82.1
	9,900	0.0	8.5	23.4	68.1
	10,100	8.3	58.3	23.3	10.0
	10,200	9.8	43.9	41.5	4.9
	10,300	11.1	37.0	40.7	11.1
	10,400	10.6	48.5	33.3	7.6
	10,600	18.6	57.1	21.4	2.9
	10,700	26.5	55.1	14.3	4.0
	10,800	21.6	52.9	23.5	2.0
	10,900	25.5	46.8	27.7	0.0

Notes: Table D8 displays the percentages of subjects, by treatment, who chose each social appropriateness evaluation in the Cash at customs vignette. In each case, the modal evaluation is shaded.

TABLE D9: APPROPRIATENESS OF DRIVING IN DRINK DRIVING VIGNETTE: DISTRIBUTION OF RATINGS – PLACEBO EXPERIMENT, KRUPKA-WEBER METHOD (N = 653)

		Very socially inappropriate	Somewhat socially inappropriate	Somewhat socially appropriate	Very socially appropriate
Blood alcohol content	0.071%	22.0	22.0	36.0	20.0
	0.072%	16.7	22.9	29.2	31.3
	0.073%	15.7	29.4	27.5	27.5
	0.074%	9.7	27.4	40.3	22.6
	0.076%	17.1	35.5	36.8	10.5
	0.077%	22.9	35.4	27.1	14.6
	0.078%	18.2	36.4	36.4	9.1
	0.079%	24.7	44.2	18.2	13.0
	0.081%	40.7	44.1	13.6	1.7
	0.082%	62.2	26.7	8.9	2.2
	0.083%	70.6	23.5	3.9	2.0
	0.084%	71.4	26.2	2.4	0.0

Notes: Table D9 displays the percentages of subjects, by treatment, who chose each social appropriateness evaluation in the Drink driving vignette. In each case, the modal evaluation is shaded.

TABLE D10: APPROPRIATENESS OF SPEED IN SPEEDING VIGNETTE: DISTRIBUTION OF RATINGS – PLACEBO EXPERIMENT, KRUPKA-WEBER METHOD (N = 653)

		Very socially inappropriate	Somewhat socially inappropriate	Somewhat socially appropriate	Very socially appropriate
Speed (miles per hour)	66	0.0	11.3	22.6	66.0
	67	2.3	9.3	23.3	65.1
	68	0.0	4.2	8.3	87.5
	69	1.6	1.6	10.9	85.9
	71	4.9	29.5	32.8	32.8
	72	5.7	39.6	35.9	18.9
	73	14.5	35.5	32.3	17.7
	74	18.3	36.7	41.7	3.3
	76	25.0	45.3	23.4	6.3
	77	49.0	30.6	12.2	8.2
	78	34.2	58.5	4.9	2.4
	79	32.7	49.1	10.9	7.3

Notes: Table D10 displays the percentages of subjects, by treatment, who chose each social appropriateness evaluation in the Speeding vignette. In each case, the modal evaluation is shaded.

TABLE D11: APPROPRIATENESS OF SEX IN AGE OF CONSENT VIGNETTE: DISTRIBUTION OF RATINGS – PLACEBO EXPERIMENT, OPINION-MATCHING METHOD

		Very inappropriate	Somewhat inappropriate	Somewhat appropriate	Very appropriate
First-order beliefs (N = 260)					
Age of girl (years, months)	16, 4	10.5	52.6	31.6	5.3
	16, 3	27.8	33.3	33.3	5.6
	16, 2	16.7	50.0	33.3	0.0
	16, 1	25.9	33.3	33.3	7.4
	15, 11	85.7	10.7	3.6	0.0
	15, 10	95.2	4.8	0.0	0.0
	15, 9	100.0	0.0	0.0	0.0
	15, 8	87.5	6.3	6.3	0.0
	15, 7	84.0	16.0	0.0	0.0
	15, 5	92.9	7.1	0.0	0.0
	15, 4	94.4	5.6	0.0	0.0
	15, 3	100.0	0.0	0.0	0.0
	15, 2	94.1	5.9	0.0	0.0
Second-order beliefs (N = 641)					
Age of girl (years, months)	16, 4	18.6	37.2	25.6	18.6
	16, 3	33.3	16.7	33.3	16.7
	16, 2	15.4	30.8	38.5	15.4
	16, 1	31.5	22.2	38.9	7.4
	15, 11	82.4	11.8	4.4	1.5
	15, 10	85.7	11.4	2.9	0.0
	15, 9	91.3	8.7	0.0	0.0
	15, 8	90.7	9.3	0.0	0.0
	15, 7	89.9	8.7	1.5	0.0
	15, 5	90.6	7.8	1.6	0.0
	15, 4	96.0	4.0	0.0	0.0
	15, 3	97.1	2.9	0.0	0.0
	15, 2	89.4	6.3	2.1	2.1

Notes: Table D11 displays the percentages of subjects, by treatment, who chose each evaluation in the Age of consent vignette. In each case, the modal evaluation is shaded.

TABLE D12: APPROPRIATENESS OF SALE IN ALCOHOL TO YOUTH VIGNETTE: DISTRIBUTION OF RATINGS – PLACEBO EXPERIMENT, OPINION-MATCHING METHOD

		Very inappropriate	Somewhat inappropriate	Somewhat appropriate	Very appropriate
First-order beliefs (N = 260)					
Age of youth (years, months)	18, 10	0.0	21.4	71.4	7.1
	18, 9	12.5	12.5	37.5	37.5
	18, 8	8.7	21.7	30.4	39.1
	18, 7	0.0	0.0	30.8	69.3
	18, 5	13.9	13.9	27.8	44.4
	18, 4	0.0	18.8	31.3	50.0
	18, 3	6.7	26.7	46.7	20.0
	18, 2	4.0	8.0	36.0	52.0
	18, 1	3.6	28.6	28.6	39.3
	17, 11	73.9	26.1	0.0	0.0
	17, 10	82.4	17.7	0.0	0.0
	17, 9	92.3	7.7	0.0	0.0
	17, 8	61.5	38.5	0.0	0.0
Second-order beliefs (N = 641)					
Age of youth (years, months)	18, 10	4.0	26.0	34.0	36.0
	18, 9	7.1	9.5	33.3	50.0
	18, 8	2.5	15.0	42.5	40.0
	18, 7	0.0	12.2	40.8	46.9
	18, 5	11.1	15.9	36.5	36.5
	18, 4	7.9	10.5	42.1	39.5
	18, 3	3.7	9.3	48.2	38.9
	18, 2	11.1	22.2	44.4	22.2
	18, 1	8.0	22.7	37.3	32.0
	17, 11	82.9	15.8	0.0	1.3
	17, 10	88.9	11.1	0.0	0.0
	17, 9	86.8	7.9	5.3	0.0
	17, 8	77.1	17.1	5.7	0.0

Notes: Table D12 displays the percentages of subjects, by treatment, who chose each evaluation in the Alcohol to youth vignette. In each case, the modal evaluation is shaded.

TABLE D13: APPROPRIATENESS OF NON-DECLARATION IN CASH AT CUSTOMS VIGNETTE: DISTRIBUTION OF RATINGS – PLACEBO EXPERIMENT, OPINION-MATCHING METHOD

		Very inappropriate	Somewhat inappropriate	Somewhat appropriate	Very appropriate
First-order beliefs (N = 260)					
Amount imported (Euros)	9,600	0.0	13.6	22.7	63.6
	9,700	0.0	6.9	10.3	82.8
	9,800	7.1	14.3	28.6	50.0
	9,900	0.0	0.0	18.8	81.2
	10,100	21.4	50.0	17.9	10.7
	10,200	8.3	45.8	41.7	4.2
	10,300	25.0	62.5	6.3	6.3
	10,400	38.1	28.6	28.6	4.8
	10,600	35.5	38.7	22.6	3.2
	10,700	17.4	52.2	13.0	17.4
	10,800	47.4	15.8	31.6	5.3
	10,900	35.3	41.2	23.5	0.0
Second-order beliefs (N = 641)					
Amount imported (Euros)	9,600	3.8	1.9	20.8	73.6
	9,700	3.2	9.5	9.5	77.8
	9,800	4.9	7.3	19.5	68.3
	9,900	5.3	5.3	14.7	74.7
	10,100	13.8	48.3	25.9	12.1
	10,200	15.8	52.6	21.1	10.5
	10,300	12.5	64.6	16.7	6.3
	10,400	20.6	52.4	20.6	6.4
	10,600	30.7	53.2	14.5	1.6
	10,700	31.1	42.2	22.2	4.4
	10,800	25.9	48.3	24.1	1.7
	10,900	37.8	40.5	21.6	0.0

Notes: Table D13 displays the percentages of subjects, by treatment, who chose each evaluation in the Cash at customs vignette. In each case, the modal evaluation is shaded.

TABLE D14: APPROPRIATENESS OF DRIVING IN DRINK DRIVING VIGNETTE: DISTRIBUTION OF RATINGS – PLACEBO EXPERIMENT, OPINION-MATCHING METHOD

		Very inappropriate	Somewhat inappropriate	Somewhat appropriate	Very appropriate
First-order beliefs (N = 260)					
Blood alcohol content	0.071%	15.0	15.0	35.0	35.0
	0.072%	6.3	18.8	62.5	12.5
	0.073%	17.7	23.5	29.4	29.4
	0.074%	18.5	33.3	33.3	14.8
	0.076%	25.9	37.0	25.9	11.1
	0.077%	33.3	41.7	8.3	16.7
	0.078%	21.4	53.6	25.0	0.0
	0.079%	14.3	42.9	25.0	17.9
	0.081%	54.8	32.3	12.9	0.0
	0.082%	60.0	20.0	13.3	6.7
	0.083%	42.9	50.0	7.1	0.0
	0.084%	84.0	12.0	4.0	0.0
Second-order beliefs (N = 641)					
Blood alcohol content	0.071%	4.7	27.9	53.5	14.0
	0.072%	22.5	22.5	25.0	30.0
	0.073%	14.9	8.5	40.4	36.2
	0.074%	6.4	31.8	39.7	22.2
	0.076%	23.5	35.3	33.8	7.4
	0.077%	23.5	45.1	21.6	9.8
	0.078%	25.5	35.3	31.4	7.8
	0.079%	27.7	26.1	32.3	13.9
	0.081%	52.8	38.9	8.3	0.0
	0.082%	60.4	34.0	1.9	3.8
	0.083%	62.5	35.0	2.5	0.0
	0.084%	62.5	29.2	8.3	0.0

Notes: Table D14 displays the percentages of subjects, by treatment, who chose each evaluation in the Drink driving vignette. In each case, the modal evaluation is shaded.

TABLE D15: APPROPRIATENESS OF SPEED IN SPEEDING VIGNETTE: DISTRIBUTION OF RATINGS – PLACEBO EXPERIMENT, OPINION-MATCHING METHOD

		Very inappropriate	Somewhat inappropriate	Somewhat appropriate	Very appropriate
First-order beliefs (N = 260)					
Speed (miles per hour)	66	5.3	10.5	31.6	52.6
	67	4.8	0.0	9.5	85.7
	68	7.7	0.0	23.1	69.2
	69	0.0	12.5	12.5	75.0
	71	2.3	30.2	44.2	23.3
	72	16.7	33.3	41.7	8.3
	73	35.7	35.7	14.3	14.3
	74	4.4	47.8	43.5	4.4
	76	30.0	56.7	3.3	10.0
	77	39.1	34.8	26.1	0.0
	78	38.1	47.6	9.5	4.8
	79	32.0	64.0	4.0	0.0
Second-order beliefs (N = 641)					
Speed (miles per hour)	66	0.0	4.4	22.2	73.3
	67	2.1	8.5	17.0	72.3
	68	0.0	8.3	12.5	79.2
	69	0.0	1.8	12.3	86.0
	71	4.6	36.9	40.0	18.5
	72	18.9	37.7	34.0	9.4
	73	12.8	53.9	23.1	10.3
	74	36.5	46.0	15.9	1.6
	76	33.8	47.3	16.2	2.7
	77	48.8	39.5	11.6	0.0
	78	42.9	44.6	12.5	0.0
	79	43.1	54.9	2.0	0.0

Notes: Table D15 displays the percentages of subjects, by treatment, who chose each evaluation in the Speeding vignette. In each case, the modal evaluation is shaded.

D.3. Prosocial traits experiment

TABLE D16: TRUSTWORTHINESS IN AGE OF CONSENT VIGNETTE: DISTRIBUTION OF RATINGS

		Very unlikely	Somewhat unlikely	Somewhat likely	Very likely
First order beliefs (N = 149)					
Age of girl (years, months)	16, 4	5.0	25.0	65.0	5.0
	16, 3	20.0	20.0	53.3	6.7
	16, 2	7.1	42.9	42.9	7.1
	16, 1	6.1	36.4	51.5	6.1
	15, 11	27.3	45.5	22.7	4.6
	15, 10	35.3	52.9	5.9	5.9
	15, 9	46.7	46.7	0.0	6.7
	15, 8	30.8	23.1	46.2	0.0
Second order beliefs (N = 408)					
Age of girl (years, months)	16, 4	4.8	33.9	56.5	4.8
	16, 3	12.0	32.0	44.0	12.0
	16, 2	11.5	26.9	50.0	11.5
	16, 1	8.3	33.3	46.7	11.7
	15, 11	30.0	51.7	16.7	1.7
	15, 10	43.2	34.1	20.5	2.3
	15, 9	45.5	34.1	18.2	2.3
	15, 8	36.1	47.2	13.9	2.8

Notes: Table D16 displays the percentages of subjects, by treatment, who chose each evaluation regarding the likelihood of the man in the Age of consent vignette keeping a promise to a friend. In each case, the modal evaluation is shaded.

TABLE D17: HONESTY IN AGE OF CONSENT VIGNETTE: DISTRIBUTION OF RATINGS

		Very unlikely	Somewhat unlikely	Somewhat likely	Very likely
First order beliefs (N = 149)					
Age of girl (years, months)	16, 4	20.0	30.0	45.0	5.0
	16, 3	20.0	53.3	26.7	0.0
	16, 2	28.6	50.0	21.4	0.0
	16, 1	9.1	48.5	30.3	12.1
	15, 11	50.0	40.9	9.1	0.0
	15, 10	58.8	29.4	5.9	5.9
	15, 9	53.3	46.7	0.0	0.0
	15, 8	46.2	38.5	15.4	0.0
Second order beliefs (N = 408)					
Age of girl (years, months)	16, 4	24.2	35.5	32.3	8.1
	16, 3	32.0	46.0	8.0	14.0
	16, 2	23.1	40.4	25.0	11.5
	16, 1	21.7	38.3	31.7	8.3
	15, 11	61.7	33.3	3.3	1.7
	15, 10	59.1	36.4	4.6	0.0
	15, 9	56.8	43.2	0.0	0.0
	15, 8	61.1	33.3	5.6	0.0

Notes: Table D17 displays the percentages of subjects, by treatment, who chose each evaluation regarding the likelihood of the man in the Age of consent vignette returning excess change to a cashier. In each case, the modal evaluation is shaded.

TABLE D18: ALTRUISM IN AGE OF CONSENT VIGNETTE: DISTRIBUTION OF RATINGS

		Very unlikely	Somewhat unlikely	Somewhat likely	Very likely
First order beliefs (N = 149)					
Age of girl (years, months)	16, 4	35.0	50.0	15.0	0.0
	16, 3	53.3	33.3	13.3	0.0
	16, 2	42.9	42.9	14.3	0.0
	16, 1	24.2	48.5	27.3	0.0
	15, 11	77.3	13.6	9.1	0.0
	15, 10	82.4	5.9	5.9	5.9
	15, 9	80.0	20.0	0.0	0.0
	15, 8	61.5	30.8	7.7	0.0
Second order beliefs (N = 408)					
Age of girl (years, months)	16, 4	54.8	40.3	4.8	0.0
	16, 3	38.0	54.0	6.0	2.0
	16, 2	51.9	38.5	5.8	3.9
	16, 1	40.0	45.0	15.0	0.0
	15, 11	75.0	23.3	1.7	0.0
	15, 10	77.3	20.5	2.3	0.0
	15, 9	77.3	20.5	2.3	0.0
	15, 8	61.1	36.1	2.8	0.0

Notes: Table D18 displays the percentages of subjects, by treatment, who chose each evaluation regarding the likelihood of the man in the Age of consent vignette volunteering for charity. In each case, the modal evaluation is shaded.

TABLE D19: TRUSTWORTHINESS IN ALCOHOL TO YOUTH VIGNETTE: DISTRIBUTION OF RATINGS

		Very unlikely	Somewhat unlikely	Somewhat likely	Very likely
First order beliefs (N = 149)					
Age of youth (years, months)	18, 4	0.0	30.0	60.0	10.0
	18, 3	0.0	8.3	66.7	25.0
	18, 2	0.0	25.0	58.3	16.7
	18, 1	0.0	10.0	66.7	23.3
	17, 11	20.6	50.0	29.4	0.0
	17, 10	9.5	47.6	42.9	0.0
	17, 9	15.8	42.1	31.6	10.5
	17, 8	18.2	36.4	45.5	0.0
Second order beliefs (N = 391)					
Age of youth (years, months)	18, 4	6.7	15.6	55.6	22.2
	18, 3	2.1	18.8	56.3	22.9
	18, 2	3.7	16.7	59.3	20.4
	18, 1	4.6	15.4	61.5	18.5
	17, 11	31.6	42.1	19.3	7.0
	17, 10	23.9	37.0	26.1	13.0
	17, 9	16.7	58.3	19.4	5.6
	17, 8	15.0	47.5	32.5	5.0

Notes: Table D19 displays the percentages of subjects, by treatment, who chose each evaluation regarding the likelihood of the shopkeeper in the Alcohol to Youth vignette keeping a promise to a friend. In each case, the modal evaluation is shaded. This analysis excludes 7 subjects in the first-order beliefs treatment who, due to an experimental glitch, were presented with the word 'appropriate' instead of 'likely' in one of the possible responses..

TABLE D20: HONESTY IN ALCOHOL TO YOUTH VIGNETTE: DISTRIBUTION OF RATINGS

		Very unlikely	Somewhat unlikely	Somewhat likely	Very likely
First order beliefs (N = 156)					
Age of youth (years, months)	18, 4	20.0	30.0	30.0	20.0
	18, 3	16.7	16.7	50.0	16.7
	18, 2	10.5	36.8	31.6	21.1
	18, 1	3.3	10.0	43.3	43.3
	17, 11	38.2	44.1	14.7	2.9
	17, 10	42.9	33.3	14.3	9.5
	17, 9	15.8	47.4	26.3	10.5
	17, 8	18.2	36.4	36.4	9.1
Second order beliefs (N = 391)					
Age of youth (years, months)	18, 4	22.2	17.8	40.0	20.0
	18, 3	4.2	31.3	31.3	33.3
	18, 2	7.4	14.8	51.9	25.9
	18, 1	9.2	16.9	43.1	30.8
	17, 11	49.1	21.1	17.5	12.3
	17, 10	43.5	23.9	28.3	4.4
	17, 9	44.4	30.6	16.7	8.3
	17, 8	32.5	40.0	22.5	5.0

Notes: Table D20 displays the percentages of subjects, by treatment, who chose each evaluation regarding the likelihood of the shopkeeper in the Alcohol to Youth vignette returning excess change to a cashier. In each case, the modal evaluation is shaded.

TABLE D21: ALTRUISM IN ALCOHOL TO YOUTH VIGNETTE: DISTRIBUTION OF RATINGS

		Very unlikely	Somewhat unlikely	Somewhat likely	Very likely
First order beliefs (N = 156)					
Age of youth (years, months)	18, 4	30.0	60.0	10.0	0.0
	18, 3	25.0	33.3	41.7	0.0
	18, 2	15.8	57.9	26.3	0.0
	18, 1	16.7	43.3	36.7	3.3
	17, 11	55.9	38.2	5.9	0.0
	17, 10	28.6	57.1	14.3	0.0
	17, 9	26.3	63.2	10.5	0.0
	17, 8	54.6	27.3	18.2	0.0
Second order beliefs (N = 391)					
Age of youth (years, months)	18, 4	48.9	33.3	17.8	0.0
	18, 3	33.3	54.2	12.5	0.0
	18, 2	29.6	59.3	9.3	1.9
	18, 1	32.3	55.4	12.3	0.0
	17, 11	61.4	33.3	5.3	0.0
	17, 10	54.4	41.3	2.2	2.2
	17, 9	69.4	27.8	2.8	0.0
	17, 8	62.5	30.0	7.5	0.0

Notes: Table D21 displays the percentages of subjects, by treatment, who chose each evaluation regarding the likelihood of the shopkeeper in the Alcohol to Youth vignette volunteering for charity. In each case, the modal evaluation is shaded.

TABLE D22: TRUSTWORTHINESS IN CASH AT CUSTOMS VIGNETTE: DISTRIBUTION OF RATINGS

		Very unlikely	Somewhat unlikely	Somewhat likely	Very likely
First order beliefs (N = 156)					
Amount imported (Euros)	9,600	0.0	5.3	63.2	31.6
	9,700	0.0	13.0	69.6	17.4
	9,800	11.1	11.1	50.0	27.8
	9,900	0.0	9.1	68.2	22.7
	10,100	3.9	38.5	57.7	0.0
	10,200	0.0	35.7	64.3	0.0
	10,300	0.0	40.0	50.0	10.0
	10,400	0.0	50.0	42.9	7.1
Second order beliefs (N = 367)					
Amount imported (Euros)	9,600	2.0	2.0	77.6	18.4
	9,700	0.0	7.9	71.1	21.1
	9,800	0.0	6.4	61.7	31.9
	9,900	2.0	12.2	61.2	24.5
	10,100	7.7	42.3	50.0	0.0
	10,200	2.2	39.1	54.4	4.4
	10,300	9.5	40.5	42.9	7.1
	10,400	9.1	40.9	45.5	4.6

Notes: Table D22 displays the percentages of subjects, by treatment, who chose each evaluation regarding the likelihood of the man in the Cash at customs vignette keeping a promise to a friend. In each case, the modal evaluation is shaded.

TABLE D23: HONESTY IN CASH AT CUSTOMS VIGNETTE: DISTRIBUTION OF RATINGS

		Very unlikely	Somewhat unlikely	Somewhat likely	Very likely
First order beliefs (N = 156)					
Amount imported (Euros)	9,600	5.3	26.3	52.6	15.8
	9,700	8.7	26.1	52.2	13.0
	9,800	5.6	38.9	50.0	5.6
	9,900	9.1	36.4	54.6	0.0
	10,100	38.5	50.0	11.5	0.0
	10,200	28.6	50.0	21.4	0.0
	10,300	40.0	30.0	30.0	0.0
	10,400	35.7	50.0	14.3	0.0
Second order beliefs (N = 367)					
Amount imported (Euros)	9,600	12.2	30.6	38.8	18.4
	9,700	13.2	15.8	63.2	7.9
	9,800	12.8	14.9	61.7	10.6
	9,900	10.2	24.5	53.1	12.2
	10,100	40.4	42.3	13.5	3.9
	10,200	34.8	41.3	19.6	4.4
	10,300	50.0	33.3	11.9	4.8
	10,400	54.6	31.8	11.4	2.3

Notes: Table D23 displays the percentages of subjects, by treatment, who chose each evaluation regarding the likelihood of the man in the Cash at customs vignette returning excess change to a cashier. In each case, the modal evaluation is shaded.

TABLE D24: ALTRUISM IN CASH AT CUSTOMS VIGNETTE: DISTRIBUTION OF RATINGS

		Very unlikely	Somewhat unlikely	Somewhat likely	Very likely
First order beliefs (N = 156)					
Amount imported (Euros)	9,600	15.8	63.2	21.0	0.0
	9,700	0.0	65.2	26.1	8.7
	9,800	11.1	66.7	22.2	0.0
	9,900	18.2	54.6	27.3	0.0
	10,100	34.6	53.9	11.5	0.0
	10,200	50.0	35.7	14.3	0.0
	10,300	50.0	40.0	10.0	0.0
	10,400	28.6	50.0	14.3	7.1
Second order beliefs (N = 367)					
Amount imported (Euros)	9,600	20.4	63.3	14.3	2.0
	9,700	21.1	57.9	18.4	2.6
	9,800	14.9	61.7	21.3	2.1
	9,900	12.2	59.2	24.5	4.1
	10,100	38.5	53.9	7.7	0.0
	10,200	43.5	47.8	8.7	0.0
	10,300	42.9	47.6	9.5	0.0
	10,400	50.0	40.9	9.1	0.0

Notes: Table D24 displays the percentages of subjects, by treatment, who chose each evaluation regarding the likelihood of the man in the Cash at customs vignette volunteering for charity. In each case, the modal evaluation is shaded.

TABLE D25: TRUSTWORTHINESS IN DRINK DRIVING VIGNETTE: DISTRIBUTION OF RATINGS

		Very unlikely	Somewhat unlikely	Somewhat likely	Very likely
First order beliefs (N = 156)					
Blood alcohol content	0.076%	0.0	20.0	73.3	6.7
	0.077%	0.0	15.8	57.9	26.3
	0.078%	0.0	66.7	0.0	33.3
	0.079%	0.0	14.3	67.9	17.9
	0.081%	2.8	33.3	58.3	5.6
	0.082%	0.0	36.8	36.8	26.3
	0.083%	14.3	28.6	57.1	0.0
	0.084%	4.6	31.8	59.1	4.6
Second order beliefs (N = 403)					
Blood alcohol content	0.076%	0.0	9.4	65.6	25.0
	0.077%	0.0	7.5	62.5	30.0
	0.078%	4.7	18.6	51.2	25.6
	0.079%	0.0	22.2	59.7	18.1
	0.081%	4.8	39.7	47.6	7.9
	0.082%	4.0	30.0	60.0	6.0
	0.083%	7.9	39.5	36.8	15.8
	0.084%	3.0	45.5	48.4	3.0

Notes: Table D25 displays the percentages of subjects, by treatment, who chose each evaluation regarding the likelihood of the woman in the Drink driving vignette keeping a promise to a friend. In each case, the modal evaluation is shaded. This analysis excludes 12 subjects in the first-order beliefs treatment who, due to an experimental glitch, were presented with the word 'appropriate' instead of 'likely' in one of the possible responses.

TABLE D26: HONESTY IN DRINK DRIVING VIGNETTE: DISTRIBUTION OF RATINGS

		Very unlikely	Somewhat unlikely	Somewhat likely	Very likely
First order beliefs (N = 168)					
Blood alcohol content	0.076%	6.7	20.0	46.7	26.7
	0.077%	5.3	15.8	68.4	10.5
	0.078%	6.7	13.3	46.7	33.3
	0.079%	3.6	21.4	57.1	17.9
	0.081%	5.6	38.9	50.0	5.6
	0.082%	10.5	31.6	47.4	10.5
	0.083%	28.6	35.7	21.4	14.3
	0.084%	13.6	40.9	36.3	9.1
Second order beliefs (N = 403)					
Blood alcohol content	0.076%	7.8	28.1	48.4	15.6
	0.077%	2.5	22.5	57.5	17.5
	0.078%	2.3	34.9	53.5	9.3
	0.079%	6.9	31.9	43.1	18.1
	0.081%	12.7	63.5	22.2	1.6
	0.082%	14.0	46.0	34.0	6.0
	0.083%	18.4	42.1	31.6	7.9
	0.084%	3.0	57.6	36.4	3.0

Notes: Table D26 displays the percentages of subjects, by treatment, who chose each evaluation regarding the likelihood of the woman in the Drink driving vignette returning excess change to a cashier. In each case, the modal evaluation is shaded.

TABLE D27: ALTRUISM IN DRINK DRIVING VIGNETTE: DISTRIBUTION OF RATINGS

		Very unlikely	Somewhat unlikely	Somewhat likely	Very likely
First order beliefs (N = 168)					
Blood alcohol content	0.076%	0.0	46.7	46.7	0.0
	0.077%	10.5	47.4	31.6	10.5
	0.078%	13.3	40.0	40.0	6.7
	0.079%	10.7	35.7	50.0	3.6
	0.081%	16.7	55.6	27.8	0.0
	0.082%	26.3	57.9	15.8	0.0
	0.083%	35.7	42.9	21.4	0.0
	0.084%	27.3	31.8	36.3	4.6
Second order beliefs (N = 403)					
Blood alcohol content	0.076%	10.9	59.4	29.7	0.0
	0.077%	17.5	50.0	32.5	0.0
	0.078%	11.6	55.8	25.6	7.0
	0.079%	15.3	58.3	26.4	0.0
	0.081%	23.8	66.7	9.5	0.0
	0.082%	18.0	72.0	10.0	0.0
	0.083%	34.2	39.5	21.1	5.3
	0.084%	30.3	57.6	12.1	0.0

Notes: Table D27 displays the percentages of subjects, by treatment, who chose each evaluation regarding the likelihood of the woman in the Drink driving vignette volunteering for charity. In each case, the modal evaluation is shaded.

TABLE D28: TRUSTWORTHINESS IN SPEEDING VIGNETTE: DISTRIBUTION OF RATINGS

		Very unlikely	Somewhat unlikely	Somewhat likely	Very likely
First order beliefs (N = 124)					
Speed (miles per hour)	66	0.0	0.0	60.0	40.0
	67	0.0	0.0	52.6	47.4
	68	0.0	0.0	42.9	57.1
	69	0.0	3.6	46.4	50.0
	71	0.0	8.7	91.3	0.0
	72	0.0	0.0	100.0	0.0
	73	0.0	0.0	73.7	26.3
	74	0.0	16.7	77.8	5.6
Second order beliefs (N = 415)					
Speed (miles per hour)	66	0.0	0.0	50.0	50.0
	67	0.0	0.0	25.5	74.5
	68	0.0	2.6	29.0	68.4
	69	0.0	1.4	35.6	63.0
	71	0.0	4.7	62.5	32.8
	72	0.0	6.5	67.4	26.1
	73	2.3	9.3	69.8	18.6
	74	2.3	4.6	81.8	11.4

Notes: Table D28 displays the percentages of subjects, by treatment, who chose each evaluation regarding the likelihood of the woman in the Speeding vignette keeping a promise to a friend. In each case, the modal evaluation is shaded. This analysis excludes 30 subjects in the first-order beliefs treatment who, due to an experimental glitch, were presented with the word 'appropriate' instead of 'likely' in one of the possible responses.

TABLE D29: HONESTY IN SPEEDING VIGNETTE: DISTRIBUTION OF RATINGS

		Very unlikely	Somewhat unlikely	Somewhat likely	Very likely
First order beliefs (N = 154)					
Speed (miles per hour)	66	0.0	15.4	30.8	53.9
	67	10.5	5.3	31.6	52.6
	68	0.0	14.3	28.6	57.1
	69	0.0	10.7	46.4	42.9
	71	0.0	19.4	77.4	3.2
	72	0.0	25.0	66.7	8.3
	73	0.0	31.6	52.6	15.8
	74	0.0	44.4	44.4	11.1
Second order beliefs (N = 415)					
Speed (miles per hour)	66	1.8	7.1	26.8	64.3
	67	0.0	2.0	21.6	76.5
	68	2.6	5.3	47.4	44.7
	69	2.7	6.9	37.0	53.4
	71	4.7	28.1	48.4	18.8
	72	10.9	30.4	50.0	8.7
	73	7.0	32.6	53.5	7.0
	74	6.8	29.6	59.1	4.6

Notes: Table D29 displays the percentages of subjects, by treatment, who chose each evaluation regarding the likelihood of the woman in the Speeding vignette returning excess change to a cashier. In each case, the modal evaluation is shaded.

TABLE D30: ALTRUISM IN SPEEDING VIGNETTE: DISTRIBUTION OF RATINGS

		Very unlikely	Somewhat unlikely	Somewhat likely	Very likely
First order beliefs (N = 154)					
Speed (miles per hour)	66	0.0	7.7	76.9	15.4
	67	0.0	31.6	47.4	21.0
	68	7.1	7.1	64.3	21.4
	69	0.0	32.1	53.6	14.3
	71	3.2	61.3	35.5	0.0
	72	8.3	66.7	25.0	0.0
	73	5.2	68.4	15.8	10.5
	74	0.0	66.7	27.8	5.6
Second order beliefs (N = 415)					
Speed (miles per hour)	66	1.8	35.7	41.1	21.4
	67	2.0	15.7	70.6	11.8
	68	0.0	26.3	65.8	7.9
	69	5.5	23.3	61.6	9.6
	71	7.8	50.0	40.6	1.6
	72	8.7	54.4	34.8	2.2
	73	11.6	62.8	25.6	0.0
	74	18.2	59.1	22.7	0.0

Notes: Table D30 displays the percentages of subjects, by treatment, who chose each evaluation regarding the likelihood of the woman in the Speeding vignette volunteering for charity. In each case, the modal evaluation is shaded.

D.4. Rule of law experiment

TABLE D31: APPROPRIATENESS OF SEX IN AGE OF CONSENT VIGNETTE: DISTRIBUTION OF RATINGS – CHINA SAMPLE (N = 248)

		Very socially inappropriate	Somewhat socially inappropriate	Somewhat socially appropriate	Very socially appropriate
Age of girl (years, months)	14, 3	24.6	32.8	24.6	18.0
	14, 1	24.6	40.4	24.6	10.5
	13, 11	76.6	14.1	9.4	0.0
	13, 9	81.8	16.7	1.5	0.0

Notes: Table D31 displays the percentages of subjects, by treatment, who chose each social appropriateness evaluation in the Age of consent vignette. In each case, the modal evaluation is shaded.

TABLE D32: APPROPRIATENESS OF SALE IN ALCOHOL TO YOUTH VIGNETTE: DISTRIBUTION OF RATINGS – CHINA SAMPLE (N = 248)

		Very socially inappropriate	Somewhat socially inappropriate	Somewhat socially appropriate	Very socially appropriate
Age of youth (years, months)	18, 3	3.2	46.0	38.1	12.7
	18, 1	11.7	43.3	31.7	13.3
	17, 11	52.2	34.3	7.5	6.0
	17, 9	62.1	31.0	6.9	0.0

Notes: Table D32 displays the percentages of subjects, by treatment, who chose each social appropriateness evaluation in the Alcohol to youth vignette. In each case, the modal evaluation is shaded.

TABLE D33: APPROPRIATENESS OF NON-DECLARATION IN CASH AT CUSTOMS VIGNETTE: DISTRIBUTION OF RATINGS – CHINA SAMPLE (N = 248)

		Very socially inappropriate	Somewhat socially inappropriate	Somewhat socially appropriate	Very socially appropriate
Amount imported (USD)	4,700	8.6	10.3	10.3	70.7
	4,900	5.0	8.8	17.5	68.8
	5,100	35.9	43.4	17.0	3.8
	5,300	36.8	45.6	15.8	1.8

Notes: Table D33 displays the percentages of subjects, by treatment, who chose each social appropriateness evaluation in the Cash at customs vignette. In each case, the modal evaluation is shaded.

TABLE D34: APPROPRIATENESS OF DRIVING IN DRINK DRIVING VIGNETTE: DISTRIBUTION OF RATINGS – CHINA SAMPLE (N = 248)

		Very socially inappropriate	Somewhat socially inappropriate	Somewhat socially appropriate	Very socially appropriate
Blood alcohol content	0.017%	9.6	30.8	40.4	19.2
	0.019%	13.0	46.8	27.3	13.0
	0.021%	40.9	39.4	16.7	3.0
	0.023%	49.1	43.4	7.6	0.0

Notes: Table D34 displays the percentages of subjects, by treatment, who chose each social appropriateness evaluation in the Drink driving vignette. In each case, the modal evaluation is shaded.

TABLE D35: APPROPRIATENESS OF SPEED IN SPEEDING VIGNETTE: DISTRIBUTION OF RATINGS – CHINA SAMPLE (N = 248)

		Very socially inappropriate	Somewhat socially inappropriate	Somewhat socially appropriate	Very socially appropriate
Speed (kilometers per hour)	117	4.0	13.3	28.0	54.7
	119	1.9	17.3	34.6	46.2
	121	24.6	49.2	23.0	3.3
	123	23.3	58.3	15.0	3.3

Notes: Table D35 displays the percentages of subjects, by treatment, who chose each social appropriateness evaluation in the Speeding vignette. In each case, the modal evaluation is shaded.

E. Moderators of the expressive power of law

The results of Section 4 have shown that the expressive power of law varies across the five legal threshold situations. In the case of the experiments in the UK, results suggest the existence of a systematic separation between the age of consent, alcohol to youth, and cash at customs situations on the one hand, and the drink driving and speeding situations on the other. This separation does not seem related to the legal nature of the offence described in the vignette (the UK legal system differentiates between "summary" and "indictable" offences, but this does not organize the data; for instance, both selling alcohol to minors and speeding are summary offences), nor does it appear to be positively correlated with the severity of the legal penalties (for instance, importing undeclared cash at customs is subject to a fine of up to £5,000, while a drink-driving offence is subject to up to 6 months' imprisonment, an unlimited fine, and a driving ban for at least 1 year).

However, the separation could be related to aspects of the situations that determine the strength of the signal that illegal behavior sends about the type of person who violates the law, which our model identifies as potential moderators of the expressive power of law (see Online Supplementary Materials A), and which may differ across vignettes. Specifically, these aspects are: 1) whether illegal behavior can be measured accurately or with a margin of error (which we refer to as "measurability" below); 2) the level of tolerance adopted by law enforcement towards law violations ("tolerance"); and 3) the extent to which law violations may be accidental rather than intentional ("intentionality"). For instance, we hypothesized that (small) violations of the speed law may be perceived as subject to possible measurement error and potentially accidental and tolerated by the police, compared to transgressions of, e.g., the age of consent law. If this is the case, we would expect that speed law violations may provide a weaker signal about a person's type relative to violations of the age of consent law, and, according to our model, this could explain why speeding laws have a weaker effect on norms compared to age of consent laws.

To probe whether these aspects do moderate the expressive power of law, for Samples 2 and 3 of the main experiment (with the UK general population, using the Krupka-Weber and opinion matching methods respectively), we included a series of follow-up questions designed to estimate perceived measurability, tolerance and intentionality for each situation. After participants had completed the evaluations of the 15 vignettes, we asked them to consider, in random order, five additional scenarios which were similar to the five legal threshold situations they had already evaluated except that in all cases the scenarios now described an instance where the behavior was just on the illegal side of the threshold. In each case, we asked them (in non-incentivized questions) to report the extent to which they agreed that: 1) the police could accurately measure the legality of the behavior; 2) if the police were sure the person had broken the law, they would be likely to take action against them; 3) avoiding breaking the law would have been within the person's control. As these

beliefs were all recorded on a four-point ordered scale, we transform the answers onto an evenly spaced numerical scale, with 1 indicating the highest level of agreement and -1 the lowest. (Note that we made minor changes to the presentation of these questions after we had collected our first 35 observations. Details are available next to the relevant screenshots in OSM C. Excluding these 35 observations makes negligible differences to the results we outline below).

We use the responses to these questions in two ways. First, we check whether there is indeed variability in the perceptions of measurability, tolerance and intentionality across the five situations – a necessary condition for these factors to be candidate sources of between-vignette variability in the expressive power of law. Second, having established this, we check whether the effect of law on norms differs across subjects who hold different perceptions about each of these factors – which would indicate that they are indeed mediators of the expressive power of law.

Figure E.1 shows, for each vignette, the mean perceptions of measurability of behavior, tolerance and intentionality. Data is pooled from all subjects from all versions of the main experiment in which we employed these follow-up questions (Samples 2 and 3, N = 1,051). We observe clear differences across vignettes in each of the three factors. Of particular interest are the differences between the two groups of situations between which we observed differences in the expressive power of law (speeding and drink-drive on one hand; age of consent, alcohol to youth and cash at customs on the other).

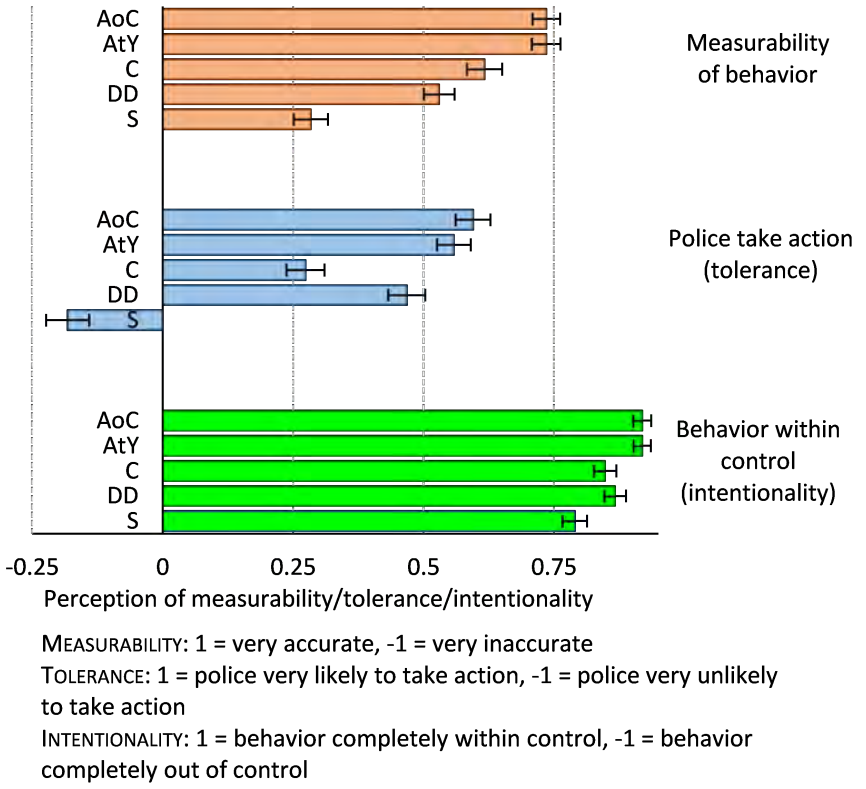
The significance of cross-vignette differences is tested by the OLS regressions reported in Table E.1. These contain the numerically-transformed response as the dependent variable, with vignette dummies along with demographic control variables.

Regarding speeding, as expected, we find that subjects perceive lower accuracy in measuring behavior, lower likelihood of police intervention, and lower intentionality in breaking the law in the speeding vignette compared to the three vignettes with stronger expressive power of law (the differences are all statistically significant at the 1% level).

Regarding drink-driving, the evidence is more mixed. We do find that, compared to violations of the age of consent and sale of alcohol to minors laws, drink-driving offences are perceived to be less accurately measurable, less likely to be prosecuted, and less intentional (all significant at the 1% level). However, when comparing the drink-driving and cash at customs vignettes, we find drink-driving has significantly lower perceived measurability, but significantly higher perceived tolerance (both at the 1% level), while the difference in perceived intentionality is insignificant.

In spite of this mixed evidence, this first analysis suggests that any of these three factors can potentially explain some of the between-vignette variability in the expressive power of law. To investigate whether they systematically moderate the influence that the law exerts on social norms in the five situations, we conduct an effect heterogeneity analysis – that is, we examine whether the magnitude of the discontinuity of the norm functions at the legal threshold varies across subjects

Figure E.1: Variation in measurability of behavior, police tolerance and intentionality across vignettes



Note: The figure plots the perceived measurability of behavior, police tolerance and intentionality in each vignette. AoC = Age of Consent; AtY = Alcohol to Youth; C = Cash at Customs; DD = Drink Drive; S = Speeding. Bars are 95% confidence intervals.

Table E.1: OLS regressions, Differences between vignettes

	(1)	(2)	(3)
	Measurability	Lack of Tolerance	Intentionality
Age of consent	0.450***	0.779***	0.129***
	(0.021)	(0.024)	(0.014)
Alcohol to youth	0.450***	0.743***	0.129***
	(0.021)	(0.023)	(0.013)
Cash at customs	0.332***	0.455***	0.058***
	(0.022)	(0.021)	(0.015)
Drink driving	0.244***	0.651***	0.076***
	(0.018)	(0.023)	(0.014)
Constant	0.210***	-0.142***	0.677***
	(0.036)	(0.048)	(0.025)
Controls	Yes	Yes	Yes
R^2	0.108	0.190	0.053
N.	5,205	5,205	5,205
Linear restriction tests (adjusted p-values)			
Drink driving vs *Age of consent*	<0.001	<0.001	<0.001
Drink driving vs *Alcohol to youth*	<0.001	<0.001	<0.001
Drink driving vs *Cash at customs*	<0.001	<0.001	0.202
Cash at customs vs *Age of consent*	<0.001	<0.001	<0.001
Cash at customs vs *Alcohol to youth*	<0.001	<0.001	<0.001
Age of consent vs *Alcohol to youth*	0.997	0.078	0.997

Note: Dependent variables are the numerically-transformed responses to the questions asking about the degree of measurability of, (lack of) tolerance towards, and intentionality of illegal behavior. The omitted vignette dummy is *Speeding*. Robust standard errors (with clustering at the individual level) in parentheses. Controls (age, gender, and income) are included in the regressions but not reported in the Table. The p-values from linear restriction tests have been adjusted using the Benjamini-Hochberg False Discovery Rate method. We have 5,205 instead of 5,255 observations because we have missing values in some control questions for 10 subjects. *** = 1% significance level; ** = 5% significance level; * = 10% significance level.

who hold different perceptions of measurability, tolerance and intentionality.

To do so, for each follow-up question we divide subjects into two categories, depending on whether or not they expressed the highest possible level of agreement (that is, they said that in a given vignette behavior was very accurately measurable, police were very likely to take action against violators, and behavior was completely within the control of the individual). We then estimate modified versions of the regression models from Table 2, including dummies capturing a subject's category and interacting these with the Illegal dummy. This is done separately for measurability, tolerance and intentionality (each model pools data from all versions of the experiment in which the follow-up questions were asked). These regressions (reported in Table E.2) test whether the magnitude of the discontinuities of the norm functions differ between subjects who express the highest possible level of agreement to the given question and those who do not.

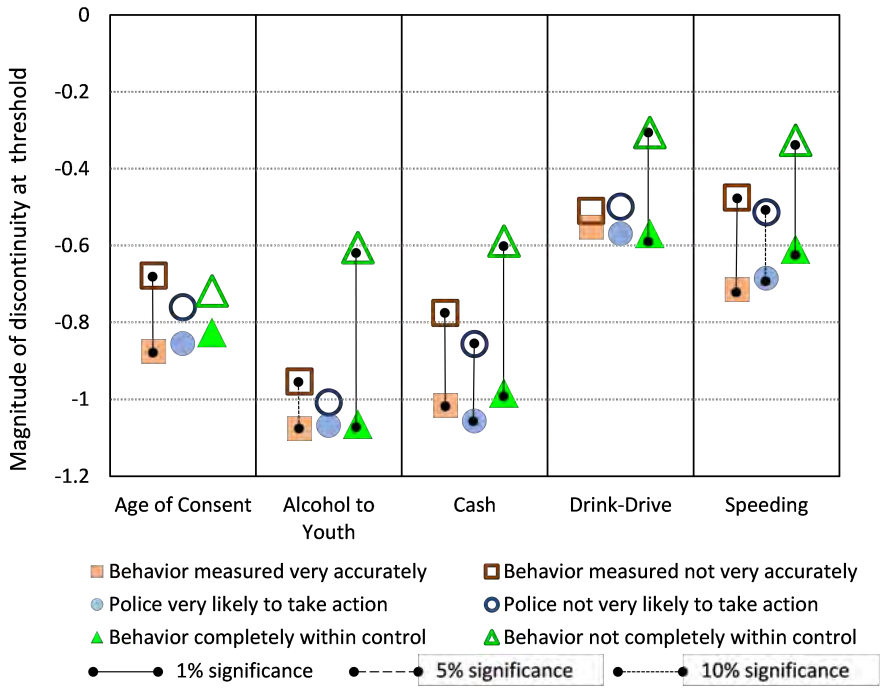
Figure E.2 presents the estimates of the magnitude of these discontinuities in each vignette for subjects belonging to either group. A number of interesting results emerge from this analysis. First, in all cases, the effect of the law on norms is larger among subjects who rate the illegal behavior described in the vignette as highly measurable, very likely to be prosecuted, and completely within the control of the person.

Second, the significance of these differences varies across the three factors. For police tolerance, the differences are significant only in the cash at custom vignette (at the 1% level) and speeding vignette (at the 10% level). Thus, although perceptions of police tolerance differ widely across vignettes (see Figure E.1), this factor alone cannot explain the observed differences in expressive power of the law across situations since it does not necessarily moderate the effect of law on norms.

In contrast, both measurability and intentionality of behavior are significant moderators of the effect of the law on norms in all cases except for measurability in the drink-driving vignette and intentionality in the age of consent vignette. Since we also observe differences in perceptions of measurability and intentionality of behavior between the speeding vignette and the three vignettes with relatively strong expressive power of law (see Figure E.1), these two factors can partly explain the differences in expressive power of law between these situations. In the speeding vignette, subjects think that small violations of the law are measured more inaccurately and are poorer reflections of a person's intentions than in the other three cases, and this reduces the influence that the law has on shaping the underlying norm of conduct.

Intentionality may also partially explain why the drink-driving vignette has relatively weak expressive power of law, since it is lower in this vignette than in the age of consent and alcohol to youth vignettes, and it significantly moderates the effect of the law on norms in the drink-driving and alcohol to youth vignettes. Although measurability is perceived to be lower in the drink-driving vignette than in all three vignettes with relatively strong expressive power of law, it is not a signif-

Figure E.2: Measurability of behavior, police tolerance and intentionality as mediators of the effect of legal thresholds on norms



Note: The figure plots the estimated magnitude of the discontinuity in the norm function at the threshold for each vignette, disaggregated between subjects who think that: 1) behavior can or cannot be measured very accurately (full or hollow square), 2) police are or are not very likely to take action upon detection of a crime (full or hollow circle), 3) the individual has or has not complete control of their behavior in the situation (full or hollow triangle). The black connectors between markers indicate whether the corresponding difference is significant at the 1%, 5% or 10% in the OLS regressions reported in Table E.2.

icant moderator of the effect of the law in the drink-driving vignette (and only is at the 10% level in the alcohol to youth vignette), so the evidence that measurability is an important determinant of the comparatively mild expressive power of law in the drink-driving vignette is relatively weak.

Overall, this analysis shows that contextual differences in the measurability and intentionality of behavior can partially explain the differences in the expressive power of laws observed in the main experiment. These results provide suggestive support for the type-signaling mechanism underlying our model. In situations where the illegality of behavior is difficult to observe, or may be accidental, it conveys a weaker signal about the type of person who engages in such behavior, and should therefore not be expected to impact as strongly on the norms regulating that behavior.

Table E.2: OLS regressions, Mediation analysis

	(1) Age of consent	(2) Alcohol to youth	(3) Cash at customs	(4) Drink driving	(5) Speeding
$(T - o_i)$	0.056**	-0.024	-0.014	-0.022	-0.004
	(0.024)	(0.023)	(0.019)	(0.024)	(0.015)
$Illegal$	-0.876***	-1.076***	-1.016***	-0.553***	-0.715***
	(0.078)	(0.070)	(0.078)	(0.093)	(0.084)
$(T - o_i) * Illegal$	-0.042	0.028	0.047*	0.074**	0.116***
	(0.029)	(0.026)	(0.028)	(0.031)	(0.025)
$Measurement\ Error$	-0.039	-0.033	-0.092**	0.156***	-0.000
	(0.056)	(0.056)	(0.045)	(0.057)	(0.041)
$Measurement\ Error * Illegal$	0.197***	0.122*	0.240***	0.042	0.239***
	(0.070)	(0.066)	(0.065)	(0.074)	(0.073)
$Constant$	-0.156*	0.269***	0.783***	0.012	0.886***
	(0.090)	(0.077)	(0.077)	(0.093)	(0.073)
Controls	Yes	Yes	Yes	Yes	Yes
R^2	0.478	0.525	0.486	0.188	0.421
N.	1,041	1,041	1,041	1,041	1,041

	(1) Age of consent	(2) Alcohol to youth	(3) Cash at customs	(4) Drink driving	(5) Speeding
$(T - o_i)$	0.055**	-0.025	-0.012	-0.026	-0.004
	(0.024)	(0.023)	(0.019)	(0.024)	(0.015)
$Illegal$	-0.855***	-1.068***	-1.057***	-0.570***	-0.686***
	(0.079)	(0.074)	(0.084)	(0.092)	(0.103)
$(T - o_i) * Illegal$	-0.042	0.029	0.051*	0.080**	0.113***
	(0.029)	(0.026)	(0.027)	(0.031)	(0.025)
$Tolerance$	-0.013	0.006	0.065	0.117**	0.063
	(0.054)	(0.055)	(0.047)	(0.056)	(0.054)
$Tolerance * Illegal$	0.095	0.060	0.201***	0.071	0.172*
	(0.065)	(0.062)	(0.069)	(0.073)	(0.094)
$Constant$	-0.152*	0.260***	0.729***	0.026	0.848***
	(0.088)	(0.082)	(0.077)	(0.093)	(0.082)
Controls	Yes	Yes	Yes	Yes	Yes
R^2	0.474	0.524	0.493	0.232	0.269
N.	1,041	1,041	1,041	1,041	1,041

	(1) Age of consent	(2) Alcohol to youth	(3) Cash at customs	(4) Drink driving	(5) Speeding
$(T - o_i)$	0.056**	-0.020	-0.012	-0.025	-0.004
	(0.024)	(0.023)	(0.019)	(0.025)	(0.015)
$Illegal$	-0.826***	-1.065***	-0.984***	-0.569***	-0.609***
	(0.075)	(0.066)	(0.073)	(0.083)	(0.067)
$(T - o_i) * Illegal$	-0.044	0.028	0.046*	0.074**	0.111***
	(0.029)	(0.026)	(0.028)	(0.031)	(0.025)
$Non - Intentionality$	0.166**	-0.183**	-0.225***	0.024	-0.111**
	(0.081)	(0.078)	(0.070)	(0.074)	(0.046)
$Non - Intentionality * Illegal$	0.108	0.459***	0.395***	0.262***	0.283***
	(0.075)	(0.114)	(0.088)	(0.097)	(0.069)
$Constant$	-0.183	0.279***	0.793***	0.068	0.911***
	(0.086)	(0.074)	(0.075)	(0.089)	(0.066)
Controls	Yes	Yes	Yes	Yes	Yes
R^2	0.480	0.532	0.490	0.231	0.419
N.	1,041	1,041	1,041	1,041	1,041

Note: Dependent variable is the evaluation of appropriateness of the behavior described in a vignette. Robust standard errors in parentheses. Regressions with bootstrapped standard errors yield very similar results. Controls (age, gender, and income) are included in the regressions but not reported in the Table. *Measurement Error* = 0 if subject reports that police can measure illegal behavior very accurately, =1 if they do not. *Tolerance* = 0 if subject reports that police are very likely to take action against person breaking law, =1 if they do not. *Non-Intentionality* = 0 if subject reports that avoiding breaking law completely within person's control, =1 if they do not. We have 1,041 instead of 1,051 observations because we have missing values in some control questions for 10 subjects. *** = 1% significance level; ** = 5% significance level; * = 10% significance level.

F. Placebo thresholds experiment, regression results

We formally analyze the results of our placebo thresholds experiment (see Section 5.1 of the main paper) by running the following regression model, separately for each vignette:

$$s(o_i) = \alpha + \beta_1 Illegal_i + \beta_2 Placebo_i + \beta_3(T - o_i) + \beta_4(T - o_i) * Illegal_i + \beta_5(P - o_i) * Placebo_i + \epsilon_i$$

where $s(o_i)$ is subject i's evaluation of the appropriateness of taking opportunity o_i that was described in the vignette the subject was randomly assigned to. The key explanatory variables are:

1. The dummy $Illegal_i$ that takes value 1 if subject i evaluated a version of the vignette that contained illegal behavior, and 0 otherwise;

2. The dummy $Placebo_i$ that takes value 1 if subject i evaluated a version of the vignette that contained behavior deemed inappropriate according to the placebo threshold P, and 0 otherwise;

3. The variable $(T - o_i)$ measuring the distance between the legal threshold T and opportunity o_i;

4. The interaction between $Illegal_i$ and $(T - o_i)$;

5. The interaction between $Placebo_i$ and $(P - o_i)$, a variable measuring the distance between the placebo threshold P and opportunity o_i.

The coefficients β_3, β_4 and β_5 can be used to calculate the slope of the relationship between appropriateness and the vignette's running variable (age, blood-alcohol content, etc.) for opportunities that are, respectively, to the left of the leftmost threshold, between the two thresholds, or to the right of the rightmost threshold. The coefficients of interest are β_1 and β_2. The former captures the discontinuity of the norm at the legal threshold, while the latter captures the discontinuity at the placebo threshold.

Table F.1 reports the results of OLS regressions run separately for the experiments that used the Krupka-Weber method (Panel A) and the opinion matching method (Panel B). For the latter, the analysis focuses on second-order beliefs, since we only measured first-order beliefs using a small sample of subjects with the purpose of incentivizing second-order beliefs and so we lack power to perform meaningful statistical analysis. Nevertheless, for completeness, we report regressions of first-order beliefs in Table F.2. All regressions also include controls for gender, age and income (not reported in the table).

Starting with β_1, the regressions reproduce the results of our main experiment. We observe large discontinuities at the legal threshold for the age of consent, alcohol to youth and cash at customs

Table F.1: OLS regressions, Placebo thresholds experiment

Panel A: Krupka-Weber	**(A1)** Age of consent	**(A2)** Alcohol to youth	**(A3)** Cash at customs	**(A4)** Drink driving	**(A5)** Speeding
$Illegal$	-0.884***	-1.297***	-0.880***	-0.203*	-0.547***
	(0.107)	(0.105)	(0.103)	(0.121)	(0.111)
$Placebo$	0.001	-0.176	-0.219*	-0.219	-0.024
	(0.045)	(0.134)	(0.113)	(0.135)	(0.127)
$(T - o_i)$	-0.026	-0.032	-0.024	-0.037	-0.074***
	(0.041)	(0.038)	(0.027)	(0.040)	(0.025)
$(T - o_i) * Illegal$	0.023	0.016	0.014	0.049	0.212***
	(0.042)	(0.035)	(0.040)	(0.042)	(0.042)
$(P - o_i) * Placebo$	0.022	0.007	0.023	0.084	-0.100**
	(0.014)	(0.046)	(0.041)	(0.051)	(0.047)
$Constant$	-0.248**	0.813**	0.615***	0.466	0.967***
	(0.113)	(0.336)	(0.085)	(0.299)	(0.084)
Controls	Yes	Yes	Yes	Yes	Yes
R^2	0.507	0.524	0.466	0.243	0.460
N.	653	653	653	653	653
Test $Illegal = Placebo$, p-value	*0.000*	*0.000*	*0.000*	*0.918*	*0.000*

Panel B: Opinion matching (2nd order)	**(B1)** Age of consent	**(B2)** Alcohol to youth	**(B3)** Cash at customs	**(B4)** Drink driving	**(B5)** Speeding
$Illegal$	-0.677***	-1.184***	-0.740***	-0.485***	-0.612***
	(0.122)	(0.098)	(0.124)	(0.121)	(0.100)
$Placebo$	-0.008	-0.202	-0.082	-0.463***	0.223**
	(0.055)	(0.123)	(0.119)	(0.132)	(0.110)
$(T - o_i)$	0.032	-0.055	0.016	-0.022	-0.047**
	(0.043)	(0.034)	(0.029)	(0.035)	(0.023)
$(T - o_i) * Illegal$	-0.010	-0.038	0.035	0.038	0.230***
	(0.045)	(0.034)	(0.044)	(0.044)	(0.037)
$(P - o_i) * Placebo$	-0.035	0.077*	-0.054	0.006	-0.125***
	(0.024)	(0.043)	(0.046)	(0.050)	(0.039)
$Constant$	-0.191	0.919***	0.641***	0.488*	0.956***
	(0.125)	(0.291)	(0.104)	(0.268)	(0.078)
Controls	Yes	Yes	Yes	Yes	Yes
R^2	0.451	0.552	0.472	0.280	0.598
N.	641	641	641	641	641
Test $Illegal = Placebo$, p-value	*0.000*	*0.000*	*0.000*	*0.878*	*0.026*

Note: Dependent variable is the evaluation of appropriateness of the behavior described in a vignette. Robust standard errors in parentheses. Regressions with bootstrapped standard errors yield very similar results. Controls (age, gender, and income) are included in the regressions but not reported in the Table. *** = 1% significance level; ** = 5% significance level; * = 10% significance level.

vignettes, regardless of the method used to elicit social norms. The size of the discontinuities is comparable to that measured in the main experiment. We observe generally smaller discontinuities for the drink driving and speeding vignette, which is also consistent with what is reported in Section 4 of the main paper. For the drink driving vignette, the discontinuity is only marginally significant in the experiment that used the Krupka-Weber method (Panel A, model A4).[8]

Turning to β_2, in Panel A the coefficient is not statistically different from zero in four out of five regressions. The only case where we observe a marginally significant discontinuity is in the cash at customs vignette (model A3). The magnitude of the placebo discontinuity, however, is significantly smaller than that of the discontinuity at the legal threshold (see linear restriction test reported in the last row of Panel A, p = 0.000). In fact, the discontinuity at the placebo threshold is significantly smaller than that at the legal threshold in all cases except the drink driving vignette where we fail to detect a large discontinuity at both the legal and placebo thresholds.

We find similar results in Panel B where we report regressions run with the data from the opinion matching experiment. We find no discontinuity at the placebo threshold for the age of consent, alcohol to youth and cash at customs vignettes. For the speeding vignette (model B5), we find a small discontinuity that is significant at the 5% level, but in the opposite direction of what we would have expected (exceeding the placebo limit *increases* appropriateness). The absolute magnitude of the placebo discontinuity is significantly smaller than absolute magnitude of the discontinuity at the legal threshold (linear restriction test, p = 0.026). In the drink driving vignette (model B4), we observe a significant discontinuity at the placebo threshold that is roughly the same size of the discontinuity at the legal threshold (linear restriction test, p = 0.878).

[8] In Panel A, a series of Chow tests find that the β_1 coefficients of the age of consent, alcohol to youth and cash at customs vignettes, $\beta_1^{consent}$, $\beta_1^{alcohol}$, and β_1^{cash}, are significantly larger than the coefficients of the speeding and drink driving vignettes, $\beta_1^{drink-drive}$ and $\beta_1^{speeding}$, (all $p \leq 0.034$). We also find that $\beta_1^{alcohol}$ is significantly larger than both $\beta_1^{consent}$ and β_1^{cash} (both $p \leq 0.008$), and that $\beta_1^{drink-drive}$ is significantly smaller than $\beta_1^{speeding}$ ($p = 0.034$). We find fewer significant differences in Panel B. $\beta_1^{alcohol}$ is significantly larger than both $\beta_1^{drink-drive}$ and $\beta_1^{speeding}$ (both $p \leq 0.001$) as well as larger than both $\beta_1^{consent}$ and β_1^{cash} (both $p \leq 0.013$). None of the other bilateral comparisons reach statistical significance (all $p \geq 0.265$). All reported p-values from Chow tests have been adjusted for multiple comparisons using the Benjamini-Hochberg FDR procedure.

Table F.2: OLS regressions, Placebo thresholds experiment (first-order beliefs)

Opinion matching (1st order)	**(1)** Age of consent	**(2)** Alcohol to youth	**(3)** Cash at customs	**(4)** Drink driving	**(5)** Speeding
$Illegal$	-0.714***	-1.133***	-0.892***	-0.421**	-0.507***
	(0.160)	(0.158)	(0.194)	(0.187)	(0.190)
$Placebo$	-0.045	-0.410**	0.146	-0.176	-0.038
	(0.077)	(0.194)	(0.214)	(0.213)	(0.186)
$(T - o_i)$	0.011	-0.148***	-0.025	0.114*	-0.048
	(0.053)	(0.050)	(0.045)	(0.063)	(0.058)
$(T - o_i) * Illegal$	-0.010	0.010	0.091	0.125*	0.169**
	(0.057)	(0.053)	(0.071)	(0.068)	(0.072)
$(P - o_i) * Placebo$	-0.001	0.131**	-0.044	-0.166*	-0.071
	(0.028)	(0.065)	(0.077)	(0.085)	(0.059)
$Constant$	-0.092	1.598***	0.789***	-0.535	0.742***
	(0.175)	(0.458)	(0.171)	(0.461)	(0.189)
Controls	Yes	Yes	Yes	Yes	Yes
R^2	0.497	0.515	0.430	0.262	0.443
N.	260	260	260	260	260
Test $Illegal = Placebo$, p-value	*0.000*	*0.000*	*0.028*	*0.303*	*0.047*

Note: Dependent variable is the evaluation of appropriateness of the behavior described in a vignette (measured using first-order beliefs). Robust standard errors in parentheses. Regressions with bootstrapped standard errors yield very similar results. Controls (age, gender, and income) are included in the regressions but not reported in the Table. *** = 1% significance level; ** = 5% significance level; * = 10% significance level.

G. Prosocial traits experiment, regression results

We formally analyze the results of our prosocial traits experiment (see Section 5.2 of the main paper) by running the following regression model, separately for each vignette and for each prosocial trait:

$$s(o_i) = \alpha + \beta_1(T - o_i) + \beta_2 Illegal_i + \beta_3(T - o_i) * Illegal_i + \epsilon_i$$

where $s(o_i)$ is subject i's evaluation of the likelihood that the person taking opportunity o_i is trustworthy, honest or altruistic. The key explanatory variables are:

1. The dummy $Illegal_i$ that takes value 1 if subject i evaluated a version of the vignette that contained illegal behavior, and 0 otherwise;

2. The variable $(T - o_i)$ measuring the distance between the legal threshold T and opportunity o_i;

3. The interaction between $Illegal_i$ and $(T - o_i)$;

The coefficient of interest is β_2 which captures the discontinuities in the trait perceptions at the legal threshold. The coefficients β_1 and β_3 measure the slope of the relationship between trait perception and the vignette's running variable (age, blood-alcohol content, etc.), allowing for different slopes between legal and illegal actions.

Table G.1 reports the results of OLS regressions run separately for each vignette and for each prosocial trait. The regressions use data on second-order beliefs collected using the opinion matching method, since we measured first-order beliefs using only a small sample of subjects with the purpose of incentivizing second-order beliefs, and so we lack power to perform meaningful statistical analysis. Nevertheless, for completeness, we report regressions of first-order beliefs in Table G.2. All regressions also include controls for gender, age and income (not reported in the table).

For trustworthiness (Panel A), we find evidence of significant discontinuities at the legal threshold in all vignettes ($p \leq 0.002$), except the drink driving vignette where the discontinuity is smaller and statistically insignificant ($p = 0.130$). A series of Chow tests indicate that there are no significant differences between the Illegal coefficients of the age of consent, alcohol to youth and cash at customs vignettes, $\beta_2^{consent}$, $\beta_2^{alcohol}$, and β_2^{cash} (all $p \geq 0.393$), or between the estimates of $\beta_2^{drink-drive}$ and $\beta_2^{speeding}$ ($p = 0.549$).[9] There are instead significant differences between the former group of coefficients ($\beta_2^{consent}$, $\beta_2^{alcohol}$ and β_2^{cash}) and the latter ($\beta_2^{drink-drive}$ and $\beta_2^{speeding}$). Specifically, we find that $\beta_2^{drink-drive}$ is significantly different from $\beta_2^{alcohol}$ ($p = 0.030$), and marginally

[9] All p-values from Chow tests reported in this Appendix are adjusted for multiple comparisons using the Benjamini-Hochberg FDR procedure, as described in the main text

different from both $\beta_2^{consent}$ and β_2^{cash} (both p = 0.092). Instead, $\beta_2^{speeding}$ is significantly different from $\beta_2^{alcohol}$ (p = 0.039), but not from $\beta_2^{consent}$ and β_2^{cash} (both p $\geq$ 0.146).

For honesty (Panel B), we observe significant discontinuities at the legal threshold in all vignettes (p $\leq$ 0.001). The differences in magnitude between the coefficients in the various vignettes are directionally similar as those discussed above, but less pronounced. In fact, a series of Chow tests find no significant differences between the coefficients of the five vignettes in any bilateral comparison (all p $\geq$ 0.174).

For altruism (Panel C), we observe generally smaller discontinuities relative to trustworthiness and honesty. They are statistically significant in the age of consent, alcohol to youth and cash at customs vignettes (all p $\leq$ 0.009). The discontinuity is significant at the 5% level for speeding (p = 0.045) and at the 10% level for drink-driving (p = 0.079). A series of Chow tests find no significant differences between the coefficients of the five vignettes in any bilateral comparison (all p $\geq$ 0.435).

When we compare the size of the discontinuities in trustworthiness, honesty and altruism in each vignette, we find that the discontinuities in altruism are nearly always smaller than those in honesty and trustworthiness. A series of Chow tests show that these differences are statistically significant in the alcohol to youth vignette (honesty vs altruism, p = 0.003; trustworthiness vs altruism, p = 0.029) and marginally significant in the drink driving vignette (honesty vs altruism, p = 0.057). In the drink driving vignette we also detect a marginally significant difference between trustworthiness and honesty (p = 0.053). None of the other differences reach statistical significance.

Table G.1: OLS regressions, Prosocial traits experiment (second-order beliefs)

Panel A: Trustworthiness	**(A1)** Age of consent	**(A2)** Alcohol to youth	**(A3)** Cash at customs	**(A4)** Drink driving	**(A5)** Speeding
$(T - o_i)$	-0.005 (0.029)	0.004 (0.032)	-0.004 (0.025)	0.050** (0.022)	-0.026 (0.019)
$Illegal$	-0.491*** (0.121)	-0.626*** (0.126)	-0.449*** (0.104)	-0.151 (0.100)	-0.237*** (0.076)
$(T - o_i) * Illegal$	0.021 (0.044)	-0.051 (0.047)	-0.001 (0.039)	-0.034 (0.038)	0.086*** (0.028)
$Constant$	0.015 (0.120)	0.436*** (0.110)	-0.208** (0.010)	0.129 (0.093)	0.543*** (0.076)
Controls	Yes	Yes	Yes	Yes	Yes
R^2	0.194	0.208	0.235	0.127	0.238
N.	408	391	367	403	415

Panel B: Honesty	**(B1)** Age of consent	**(B2)** Alcohol to youth	**(B3)** Cash at customs	**(B4)** Drink driving	**(B5)** Speeding
$(T - o_i)$	-0.014 (0.034)	-0.073* (0.040)	-0.015 (0.038)	0.007 (0.030)	0.041 (0.027)
$Illegal$	-0.539*** (0.120)	-0.814*** (0.153)	-0.514*** (0.135)	-0.425*** (0.110)	-0.375*** (0.107)
$(T - o_i) * Illegal$	0.015 (0.044)	0.065 (0.059)	0.060 (0.051)	-0.060 (0.042)	-0.002 (0.040)
$Constant$	-0.375*** (0.122)	0.282* (0.143)	-0.087 (0.132)	0.071 (0.110)	0.133 (0.010)
Controls	Yes	Yes	Yes	Yes	Yes
R^2	0.201	0.205	0.240	0.102	0.326
N.	408	391	367	403	415

Panel C: Altruism	**(C1)** Age of consent	**(C2)** Alcohol to youth	**(C3)** Cash at customs	**(C4)** Drink driving	**(C5)** Speeding
$(T - o_i)$	-0.041 (0.025)	-0.023 (0.029)	-0.054* (0.029)	0.012 (0.023)	0.013 (0.027)
$Illegal$	-0.418*** (0.092)	-0.261*** (0.099)	-0.371*** (0.107)	-0.160* (0.091)	-0.198** (0.099)
$(T - o_i) * Illegal$	0.011 (0.034)	0.032 (0.039)	0.070* (0.040)	-0.013 (0.037)	0.058 (0.038)
$Constant$	-0.605*** (0.091)	-0.465*** (0.098)	-0.315*** (0.106)	-0.318*** (0.097)	-0.080 (0.100)
Controls	Yes	Yes	Yes	Yes	Yes
R^2	0.107	0.069	0.119	0.051	0.213
N.	408	391	367	403	415

Note: Dependent variable is the likelihood that a person taking the behavior described in a vignette is trustworthy (Panel A), honest (Panel B) and altruistic (Panel C). Data are based on second-order beliefs. Robust standard errors in parentheses. Regressions with bootstrapped standard errors yield very similar results. Controls (age, gender, and income) are included in the regressions but not reported in the Table. *** = 1% significance level; ** = 5% significance level; * = 10% significance level.

Table G.2: OLS regressions, Prosocial traits experiment (first-order beliefs)

Panel A: Trustworthiness	**(A1)** Age of consent	**(A2)** Alcohol to youth	**(A3)** Cash at customs	**(A4)** Drink driving	**(A5)** Speeding
$(T - o_i)$	0.021	-0.046	-0.027	-0.019	-0.002
	(0.043)	(0.046)	(0.042)	(0.037)	(0.046)
$Illegal$	-0.355*	-0.757***	-0.303*	-0.214	-0.369***
	(0.195)	(0.161)	(0.161)	(0.138)	(0.127)
$(T - o_i) * Illegal$	-0.013	-0.017	-0.046	0.043	-0.019
	(0.077)	(0.068)	(0.061)	(0.052)	(0.054)
$Constant$	-0.130	0.458***	0.325**	0.306*	0.600***
	(0.186)	(0.147)	(0.159)	(0.170)	(0.062)
Controls	Yes	Yes	Yes	Yes	Yes
R^2	0.154	0.280	0.137	0.067	0.198
N.	149	149	156	156	124

Panel B: Honesty	**(B1)** Age of consent	**(B2)** Alcohol to youth	**(B3)** Cash at customs	**(B4)** Drink driving	**(B5)** Speeding
$(T - o_i)$	-0.028	-0.201***	0.074	-0.007	0.004
	(0.053)	(0.070)	(0.048)	(0.053)	(0.052)
$Illegal$	-0.538***	-1.263***	-0.427**	-0.217	-0.297**
	(0.186)	(0.191)	(0.172)	(0.067)	(0.149)
$(T - o_i) * Illegal$	0.021	0.067	-0.103	0.052	0.023
	(0.073)	(0.089)	(0.066)	(0.071)	(0.064)
$Constant$	-0.311	0.296	-0.011	0.157	0.505***
	(0.206)	(0.193)	(0.197)	(0.094)	(0.187)
Controls	Yes	Yes	Yes	Yes	Yes
R^2	0.193	0.268	0.255	0.093	0.147
N.	149	156	156	168	154

Panel C: Altruism	**(C1)** Age of consent	**(C2)** Alcohol to youth	**(C3)** Cash at customs	**(C4)** Drink driving	**(C5)** Speeding
$(T - o_i)$	-0.065	-0.088	0.022	0.017	0.040
	(0.043)	(0.055)	(0.043)	(0.047)	(0.042)
$Illegal$	-0.529***	-0.603***	-0.366**	-0.339**	-0.356**
	(0.168)	(0.169)	(0.171)	(0.160)	(0.146)
$(T - o_i) * Illegal$	0.048	0.029	-0.065	0.006	-0.054
	(0.065)	(0.073)	(0.071)	(0.066)	(0.056)
$Constant$	-0.302	-0.101	0.125	-0.304*	0.254
	(0.188)	(0.174)	(0.165)	(0.177)	(0.106)
Controls	Yes	Yes	Yes	Yes	Yes
R^2	0.142	0.124	0.160	0.101	0.207
N.	149	156	156	168	154

Note: Dependent variable is the likelihood that a person taking the behavior described in a vignette is trustworthy (Panel A), honest (Panel B) and altruistic (Panel C). Data are based on first-order beliefs. Robust standard errors in parentheses. Regressions with bootstrapped standard errors yield very similar results. Controls (age, gender, and income) are included in the regressions but not reported in the Table. The analysis excludes 30 subjects who, due to an experimental glitch, were presented with the word "appropriate" instead of "likely" in one of the possible responses. *** = 1% significance level; ** = 5% significance level; * = 10% significance level.

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