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How Institutions Shape Morality

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We present the results of a randomized control trial on the effect of the introduction of formalized property rights on individuals' moral judgments and, in particular, on utilitarian morality. We show that institutions shape morality: being exposed to private property institutions makes individuals *more* utilitarian when confronted with moral dilemmas. Our results shed light on a possible institutional determinant of the variation of moral judgments across the globe and its geographical patterns, and have implications for the consequences of major institutional reforms—both intended, such as land-titling programs, and unintended, such as those following from recent historical events—on moral attitudes. We discuss two possible channels stemming from the inherent features of property rights: the loosening of social ties and the commodification of rights (*JEL* codes: K11; O13; Z10; Z13).

1. Introduction

The car you are driving has sudden brake failure and you have only two options. If you go straight, the car will drive through a pedestrian crossing ahead, which will result in the death of two men. If you swerve, the car

*University of Amsterdam, Amsterdam, The Netherlands. E-mail: gdarimat@uva.nl. The experiment was approved by the Research Ethics Committee Parc de Salut MAR—Barcelona, reference nr. 2018/8015/I. Participants provided informed consent. The empirical strategy was pre-specified in a pre-analysis plan that was registered at the AEA RCT Registry—ID AEARCTR-0005325—before we collected the data, and included specification of the hypothesis to be tested and of the regression approach. We are indebted to Deo-Gracias Houndolo for his support during the fieldwork, and to Dr Charles Ibikounle who provided detailed information on the protection of land in Benin. Ametonou Charmelle, Dossou Fiogbe, Gaston Gnonlonfoun, Issifou Gounou, Colin Henderson, Madeline Holbrook, Nice Hounbegnou, Dorothee Lokossou, Aissath Salifou, Mohamed Sedou, Aparna Sundaram, and Israelia Zannou provided excellent research assistance. We would also like to thank Davide Barrera, Balász Bodó, Ben Depoorter, Davide Morisi, John Donohue, David Enoch, Diego Gambetta, Alon Harel, Keith Hollenberg, Adi Leibovitch, Michael Meurer, Mitch Polinsky, Amber Polk, Chris Robertson, Doron Teichman, Donald Wittman, Kathy Zeiler, Francesco Zucchini, and participants in seminars at Hebrew University of Jerusalem, Stanford Law School, Boston University School of Law, Collegio Carlo Alberto, and the University of Amsterdam. The usual disclaimer applies.

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will drive through a pedestrian crossing in the other lane, which will result in the death of one man.¹ What would you do? Is saving two worth the intentional killing of one?

Normative institutional analysis is often predicated on the premise that there are universal moral principles, such as those derived from deontology, as in Rawls (1971), or utilitarianism, as in Singer (2011).² In contrast, recent empirical research in psychology and anthropology shows that moral judgments vary over time and space, and are socially functional (Haidt 2007).³ To sharply characterize moral dilemmas, philosophers have been extensively using trolley problems in thought experiments since the late 60s (Foot 1967).⁴ Their use has now become commonplace also in empirical research (Christensen and Gomila 2012; Greene 2016), and the hypothetical above is one of them.

Individuals in different societies tend to give markedly different answers when confronted with moral dilemmas. In the most comprehensive survey so far—the *Moral Machine* experiment—Awad et al. (2018) collected 40 million decisions from individuals who were exposed to self-driving-car versions of the dilemma above in 233 different societies. They document both geographical variation and correlation with country-level cultural and institutional indicators. In particular, subjects living in countries with individualistic cultures tend to be more willing to accept utilitarian sacrifice, that is, to swerve in order to spare more lives while intentionally sacrificing some. What explains these patterns? A plausible hypothesis is that, among other factors, legal and economic institutions affect individuals' social environment and hence shape their moral judgments. That is, institutions shape morality.

The notion that morals and, more generally, preferences and culture are endogenous to the institutional setup of society has long been discussed in philosophy (Foucault 1995), sociology (Elias 2000), history (Weber 1976), and economics (Bowles 1998; Tabellini 2008a, 2008b; Bisin and Verdier 2011). More recently, empirical analyses and historical case studies have uncovered the long-term causal effect of institutions on culture (Greif and Tabellini 2017; Lowes et al. 2017; Enke 2019; Schulz et al. 2019; Becker

1. Hypothetical adapted from vignettes in www.moralmachine.net. See below for more details.

2. We provide a definition and an illustration of these concepts below.

3. See also Abarbanell and Hauser (2010) (studying moral dilemmas in a rural Mayan societies and showing differences with respect to what is typically observed in other societies), Haidt and Graham (2007) (arguing that there are five distinct psychological systems shaping an individual's emotional reaction to five distinct sets of issues — harm/care, fairness/reciprocity, ingroup/loyalty, authority/respect, and purity/sanctity — which in turn can be seen as determinants of the world's diverse moral landscape), and Enke (2020) (providing evidence of a demand (by voters) and supply (by politicians) of different moral principles, which in turn shaped support for the various candidates in recent US presidential elections). See also more generally on the global variation in behavioral patterns Henrich et al. (2001, 2005).

4. See also Thomson (1976 and 1984); Kamm (1989); Unger (1996).

et al. 2020; Henrich 2020). While these studies span generations, in this article, we assess the short-term effect of a discontinuous institutional change on moral judgments. In particular, we focus on the effect of private property—one of the most fundamental institutions or, possibly, the most fundamental institution of western societies—on utilitarianism.

Private property, especially of land, has been a feature of western legal cultures since antiquity. It was embedded in the legal systems that shaped western law—Roman and canon law—and was central to the thinking of philosophers from Plato, to Thomas Aquinas, John Locke, Thomas Hobbes, and Adam Smith (Garnsey 2014). Over the past several decades, the West has sought to export law and institutions supporting private property of land to developing countries through a series of reforms.

The question as to the acceptability of utilitarian sacrifice occupies a similarly prominent role in the West's moral tradition, and epitomizes the contrast between utilitarian and deontological moral positions. Utilitarians—such as Bentham (1789 [1907]), Mill (1863), and Singer (2011)—believe that the morality of a choice should be judged solely based on its consequences. From this position, sacrificing a life is morally acceptable if it results in saving more than one life, and is unacceptable otherwise. In contrast, deontologists—such as Rawls (1971) and John Paul II (1995), who sharply oppose utilitarianism—believe that (some) choices are either right or wrong irrespective of their consequences. From this position, sacrificing a life is never morally acceptable. Since most individuals give some weight to both deontological and utilitarian considerations, deontological considerations can be seen as a “moral cost” that constraints the achievement of utilitarian goals. This notion is at the core of our study: To what extent does the introduction of private property affect the balance between the utilitarian calculus and deontological imperatives?

On the policy level, the extent to which one should embrace the utilitarian calculus has been comprehensively discussed and is a fundamental issue behind welfarist analyses of laws and institutions, and the disputation about the compatibility of welfare maximization with notions of fairness (see Posner 1979; Kaplow and Shavell 2001, and the extensive debate that followed summarized by Fabbri and Britto 2017).

While the relationship between private property and morality is not new to economics (see, for instance, Bowles 1998),⁵ assessing empirically its causal effects is challenging because experiments on the introduction of private property are hard to come by. This limitation applies more generally to studies on the effects of institutional changes on culture and, indeed, the extant literature mostly relies on geographical or historical

5. There is a large literature in legal scholarship about how morality (or, more generally, culture) shapes or should shape the law, in general, and property law, in particular (see, for instance, Merrill and Smith 2007; Dagan and Dorfman 2016; Zhang 2016). We focus on the reverse question: how property shapes morality.

discontinuities (Lowe et al. 2017; Schulz et al. 2019).⁶ In contrast, we present the results of a study that bases identification on a randomized control trial (RCT).

To sidestep the difficulties inherent in the randomized introduction of private property, we took advantage of the first case in which a property rights reform was in fact implemented as an RCT in a pool of rural villages in the Republic of Benin. The reform, known as *Plan Foncier Rural* (PFR), consisted in the formalization of previously customary land rights and was implemented in 2010–11. In the winter of 2020, we presented individuals in treated and control villages with a series of moral dilemmas derived from the *Moral Machine* experiment.⁷

In our primary vignette, analogous to the one presented above, each individual was asked whether he or she would do nothing and, consequently, continue straight and kill two individuals, as opposed to swerving and killing only 1. In order to save two lives, the individual had to take action, which resulted inevitably in the death of a passerby. This setup captures the “negative” side of utilitarianism, that is, the acceptability of harm visited upon innocents—which is commonly referred to as “utilitarian sacrifice”—along the path of producing a (greater) good (Kahane et al. 2018).⁸ In a subsequent vignette, the choice was between going straight and killing one man versus swerving and killing two women. In this case, the utilitarian choice was to continue straight. While the former scenario stacks utilitarianism against preferences for inaction, in the latter scenario, utilitarianism is contrasted with gender preferences.⁹

Our results show that, after only 9 years of exposure to the new property rights regime, villagers are significantly more likely to resolve the moral dilemma in a utilitarian way, that is, to kill one rather than two individuals in both vignettes. The estimated effects suggest that, on average, participants in treated villages choose to spare more lives 7–10% more often than those in the control sample. Further analysis shows that the observed effect is driven by participants who were in a position to actually benefit from the reform, that is, those who hold land parcels in the

6. See Alesina and Giuliano (2015) for a review of the literature. There is a large literature—including RCTs—on the effect of market participation on preferences (Jha and Shayo 2019, Margalit and Shayo 2020) and morality (Falk and Szech 2013). We focus on the effects of legal institutions rather than market participation.

7. This study is part of a broader project on the effects of the introduction of formal property rights (Fabbri and Dari-Mattiacci 2020; Fabbri 2020 and 2021). None of the companion papers studies the effect of the reform on morality.

8. Globally, individuals tend to prefer inaction to action (Awad et al. Smart 1958). This formulation is orthogonal to our discussion.

9. Awad et al. (2018) report a large cross-cultural variance in moral preferences concerning gender. In our data, we found that about one-quarter of the subjects have a preference for sparing a man that is stronger than their preference for inaction; that is, they would rather swerve and spare one man than continue straight and spare one woman (Vignette 3 in Figure 2c, discussed below in the text). As explained below, in the analysis we control for individual gender preferences.

treated area and have higher income levels, stronger market integration, or shorter travel distance from state tribunals. Since having the logistical and financial means to access formal courts is a *de facto* prerequisite to take advantage of the documentary evidence provided by the reform in adverse land claims, these findings reinforce the result that formal property rights affect moral values.¹⁰ We also presented subjects with additional variations of the basic setup in order to capture the moral weight given to age, gender, and social status¹¹ but we detected no effect, which suggests that formal property rights may have a specific effect on utilitarianism.¹²

In the closest study to ours, [Di Tella et al. \(2007\)](#) find that providing formal land titles to a group of squatters in the outskirts of Buenos Aires had a positive effect on individualism—that is, the belief that one can be successful irrespective of the support of a group—and other beliefs associated with a capitalist mindset, such as the role of markets, money, and merit. In contrast, we directly test the effect of the reform on utilitarianism. Although different, our findings are related and largely consistent with theirs as individualism correlates positively both with utilitarianism ([Awad et al. 2018](#)) and with the strength of property rights ([Lehavi and Licht 2011](#); [Dari-Mattiacci and Guerriero 2015](#)) in cross-country studies.¹³

10. In a contribution based on data collected in 2016–2017 across Beninese villages, [Fabbri \(2021\)](#) combines an institutional and legal analysis of the Beninese PFR with survey questions and experimental games to show that the reform increased cooperation and trust choices in social dilemmas, but only for those individuals who face relatively low costs to conduct a legal action. In particular, the author shows that the absence of paved roads and difficulties to reach state tribunals magnify the cost of a legal dispute and jeopardize the use of formal courts. The same inverse relationship between the accessibility of state legal institutions and pro-social behavior is reported in [Fabbri and Dari-Mattiacci \(2020\)](#). The authors conduct a modified Dictator game experiment in Beninese rural villages and show that individuals who were awarded private property through the PFR are less likely to steal from peers' endowment, albeit the effects progressively fade away when distance from roads increases.

11. We presented subjects with two gender-loaded moral dilemmas—one man versus one woman ([Figure 2c](#)) and one man versus two women ([Figure 2b](#))—three age-sensitive dilemmas—one elderly man versus one man ([Appendix Figure A3](#)), one man versus one boy ([Appendix Figure A4](#)), and one elderly man versus one boy ([Appendix Figure A5](#))—and three final dilemmas capturing social and economic status—one male executive versus one man ([Appendix Figure A6](#)), one male doctor versus one man ([Appendix Figure A7](#)), and one male executive versus one male doctor ([Appendix Figure A8](#)). We detected no effect of the reform on those choices. Regression [Appendix Tables A6, A7, and Appendix A8](#) reporting the results are included in [Appendix A](#).

12. [Awad et al. \(2018\)](#) find that individuals living in countries with a smaller gender gap are more willing to spare women and that those living in countries with lower economic inequality are more willing to spare lower-social-status individuals. While formalization of property rights is thought to favor weaker individuals in society, such as women or the poor, the effect, if any, is unlikely to emerge in the short term because it results from subsequent, and possibly slower, social changes.

13. While the squatters in [Di Tella et al. \(2007\)](#) were given a land title, the reform we consider did not directly redistribute land; in contrast, it primarily concerned the formalization and recording of land titles. In addition, the formalization occurred after clearing

The observed effect of formal property rights on utilitarianism can be rationalized by starting from property's fundamental legal features, which in turn impact on both the relationship of the individual with others and the contours of his or her rights and obligations. Formal property rights—differently from rights deriving from contracts—are enforced by a set of state institutions irrespective of any prior relationship (legal or social) between the owner and a potential trespasser. In legal parlance, property rights are rights *in rem*, that is, “in a thing” (Hansmann and Kraakman 2002), are enforceable *erga omnes*, that is, “against the world” (Merrill and Smith 2000), and can be freely transferred (Ayotte and Hansmann 2012). These features of property rights have important practical implications and set them apart from rights that derive from mutual (possibly, multilateral) agreements (Ayotte and Bolton 2011; Arruñada 2012). They point to two distinct but mutually compatible channels through which institutional change may have affected moral judgments, which we refer to as “weakening of social ties” and “commodification,” and examine in turn.

First, in a system of formal property rights, the owner's enjoyment of the asset is independent of his or her network of legal and social relationships within the community and, more specifically, of his or her position within that network. In contrast, customary rights are enforced through recourse to traditional local authorities and hence enforcement is deeply embedded in social relations. Under these circumstances, it is crucial for an individual not to exhibit behaviors that may alienate social connections and consequently weaken the individuals' social standing with possibly negative repercussions on the security of his or her rights. Utilitarian moral attitudes can be potentially damaging to one's social embeddedness, because the willingness to break with deontological principles may be interpreted as a signal of lack of trustworthiness by group members (Awad et al. 2020). The more an individual's success rests on the social support he or she can gather among the local community, the more costly it is to practice utilitarianism. In contrast, formal property rights that are enforced by an impersonal judiciary and transferred through trade make social ties relatively less important and hence unshackle utilitarian attitudes (Bowles 1998: 91–93). Although conjectural, this interpretation is consistent with our finding that those with effective financial and logistical means to access justice in formal courts update their moral positions more strongly in response to the reform, with the fact that, in public goods experiments, participants experiencing the reform increase their contribution to joint projects with strangers outside the village community (Fabbri 2020), and with the literature emphasizing the relationship between kin

possible disputes as to existing entitlements. Lastly, their study is based on a quasi-natural experiment—namely, the fact that some owners disputed the re-allocation of property rights in court while others did not—while our design is grounded in an RCT design that randomized the introduction of formal property rights.

structures, morality, and institutions (Ellickson 1991; Bernstein 1992; Greif 1994; Henrich et al. 2005; Enke 2019; Schulz et al. 2019).

Second, after the reform, land is legally characterized as an object of ownership, a commodity that can be used, pledged, and transferred at will rather than as a collective resource to be used in manners that are collectively agreed upon (Marx 1992 [1867]; Polanyi 1944). The new legal framework makes it easier to put a price on various forms of land use above and beyond its traditional physical use. The (enhanced) ability to pledge land as a collateral and to sell it connect directly with the credit and real estate markets, and provide individuals with a ready metric of “value” and a way to compare different parcels of land. Since land is the main productive asset in the villages affected by the reform and the main source of income for most families in those villages, this process of commodification is most likely highly salient to them. Thus, the second channel through which formal property rights may have affected moral judgments is this process of commodification, as utilitarianism is crucially dependent on an individual’s ability to comfortably compare different outcomes. To say that killing one is “better” than killing two requires the individual to accept that these two outcomes can be compared, and to have some metric for the comparison. The commodification of land may have had a spillover effect on the individuals’ ability to make quantitative comparisons in other important domains of life (André and Platteau 1998; Bowles 1998). This interpretation is consistent with our finding that the effect of the reform on moral judgments is stronger for individuals with high market integration.

This article is the first to measure the effects of a real-life property rights reform on moral judgments. Our results shed light on a possible institutional determinant of the variation of moral judgments across the globe and its geographical patterns, and highlight the moral and, more generally, cultural consequences of large and broadly discussed land-titling programs and of the reforms that followed on from cataclysmic historical events, such as the fall of communism in the late 1980s and early 1990s.

The practical importance of moral judgments and, therefore, of the factors shaping them cannot be overstated. Fundamental and often controversial policy issues—such as whether punishment of criminal acts should be based on deterrence or retribution, the appropriate protection given to constitutional rights, and the scope for cost–benefit analysis prior to the enactment of regulations—commonly hinge on the moral premises of their proponents and detractors, who often belong to either of two camps. Consequentialists, among whom the utilitarians, believe that policy choices should be based on their consequences. In contrast, deontologists believe that some policies are right or wrong irrespective of their consequences (Sunstein 2014).

The remainder of this article is organized as follows. In Section 2, we describe the institutional framework and present the details of the *PFR*.

In Section 3, we present the research design, discussing details of the vignette study, our identification strategy, the data collection procedures, as well as the hypothesis and the empirical specification for testing it. In Section 4, we present our results. In Section 5, we offer a discussion of our findings and their implications and, in Section 6, we conclude with some ideas for future research.

2. Institutional Framework

In recent years, systems of formal land ownership registration have been introduced in nearly every African state. Nonetheless, customary land rights still represent the predominant land-tenure arrangement in most rural areas of the African continent. Customary land rights are characterized by a complex set of tenure principles and regulatory mechanisms, usually defined at the village or local level. While a variety of diverse customary arrangements exists, it is possible to identify a set of common features (Delville 2000). Customary rights consist of socially determined land-use rules, where access to land is an integral part of the social structure and tenure is determined by sociopolitical relationships. Governance and enforcement are in the hands of local customary authorities. The distribution of land rights is based on the sociopolitical local structure and on family relationships.

This system implies that rights held by individuals are the result of a social and political process of negotiations arbitrated by customary local authorities. This enforcement process has an inherently procedural nature. Rules governing customary arrangements do not provide a precise codification of each landholder's rights. Instead, they only state procedures by which an individual obtains access to the land. Therefore, the informal nature of customary rules might be an obstacle to the establishment of secure and well-defined property rights on land. Population growth and the consequent increasing pressure on natural resources create serious concerns for the functioning of informal customary arrangements. Scholars have noticed that the absence of written documentation regarding land use tends to give rise to increasing conflicts over inheritance and disputes over land use (Deininger and Castagnini 2006).

In Benin, the policy response to problems due to tenure insecurity has been a land-tenure reform known as the PFR. The reform consists of socio-land surveys run at the village level to identify right holders, their rights, and parcel boundaries. Rights and associated right holders are then recorded in public registries, and a process of land demarcation takes place. The process allows for public objection to the proposed registration of rights and requires that right holders and neighbors publicly sign survey records (Hounkpodote 2007).

According to the PFR roadmap, following the processes of land demarcation and public registry recording, each local administration created a

public land registry.¹⁴ Given these characteristics, the PFR reform in Benin brought about a major change in the institutional contours of ownership over land by creating a system akin to formalized private property rights (Fabbri and Dari-Mattiacci 2020).

Due to a lack of resources, the Beninese PFR remained on paper until the Millennium Challenge Corporation (MCC) subsidized an implementation program completed between 2010 and 2011. In agreement with the World Bank that designed and carried on the impact evaluation of the reform, the implementation followed an RCT process involving hundreds of rural villages. In fact, this is the first case of a large-scale land-tenure reform implemented in this manner (Goldstein et al. 2018).

In the preliminary phase of the project, rural villages were informed of the possibility to have the reform implemented and invited to apply for being included in the RCT pool. As a second step, each application was examined to verify whether the village met certain eligibility criteria.¹⁵ A total of 575 eligible villages were included in the RCT. Once this pool of villages was identified, a subsample of 300 of them was selected via public lottery, and in these villages, the PFR was implemented. The villages that were not selected for the PFR did not receive any intervention and, as of today, continue to have customary land rights. Figure 1 shows a map of the communes and villages where the reform took place.

3. Research Design

The empirical strategy was pre-specified in a pre-analysis plan that was registered at the American Economic Association's Registry for RCTs before we collected the data. The pre-analysis plan included the specification of the hypothesis to be tested, the regression approach, and the dimensions to be studied in the heterogeneity analysis.¹⁶

14. In the original formulation as stated in the Rural Land Act 2007-003, the local administration would also issue the "Certificat Foncier Rural," that is, land certificates that required registration to assign land ownership titles ("Titre Foncier"). Remarkably, in the original formulation of the PFR once the land-demarkation intervention and the recording of rights in a public registry have taken place, the subsequent process of releasing land certificates is purely administrative and does not require further action by landholders. The release of land certificates was de facto interrupted with the enactment of the new Rural Land Law 2013-01 creates a unique ownership document, the "Certificat de Propriete Foncier," that reunifies land certificates and ownership titles. However, as clarified also by Benin State Law 2017-15, even in cases when the local administration has not yet released the certificates, the recorded rights that constitute the basis for the land-demarkation process assign to right holders the use of rights recognized by courts.

15. The criteria for eligibility were poverty index, potential for commercial activities, regional market integration, local interest in promoting gender equality, infrastructure for economic activities, adherence to the PFR application procedure, the incidence of land conflicts, and the production of main crops.

16. See AEA RCT Registry—ID AEARCTR-0005325.

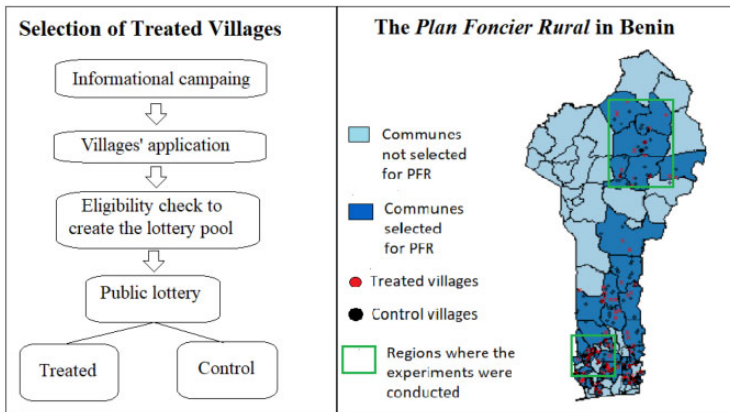


Figure 1. From Fabbri (2021: 33, Appendix A). Left Panel: The mechanism for selecting treated villages. Right Panel: Distribution of treated and control villages after the RCT implementation. The green square identifies the provinces where the data collection took place.

3.1 The Moral Dilemmas

To capture utilitarianism in moral judgments, we used a series of vignettes that were produced using the *Design* function on www.moralmachine.net (Awad et al. 2018). The vignettes presented subjects with moral dilemmas consisting of a binary choice between inaction and action, both of which resulted in the death of one or more individuals with varying characteristics. Each vignette presented the heading “The car in the pictures below has sudden brake failure: What would you do if you were driving the car? Choose one of the two options below,” followed by two graphic representations of the two options available: *Continue straight*—“In this case the car will drive through a pedestrian crossing ahead, which will result in the death of:...”—or *Swerve*—“In this case the car will drive through a pedestrian crossing in the other lane, which will result in the death of:...”

Our measure of utilitarianism in moral preferences is based on two vignettes constructed by varying the number and characteristics of the individuals put on either path before the car. In Vignette 1—depicted in Figure 2a—the choice was between two men (if *Continue straight* was chosen) and one man (if *Swerve* was chosen). Given that individuals tend to attach greater moral weight to action (but see also Abarbanell and Hauser 2010), choosing to act has a moral cost for the individual, which is greater the greater the weight is given to deontological constraints. Therefore, individuals who choose the *Swerve* option are those with relatively weak moral constraints along the path toward a greater good, that is, they are utilitarian (Kahane et al. 2018).

The utilitarian dilemma presented in Vignette 2 (Figure 2b) depicts the choice between one man (if *Continue straight* was chosen) and two women (if *Swerve* was chosen). In this case, the utilitarian thing to do is not to act. However, there is evidence that in some cultures subjects have gender

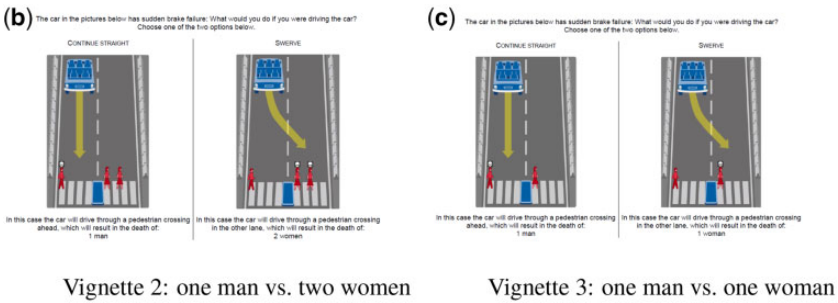
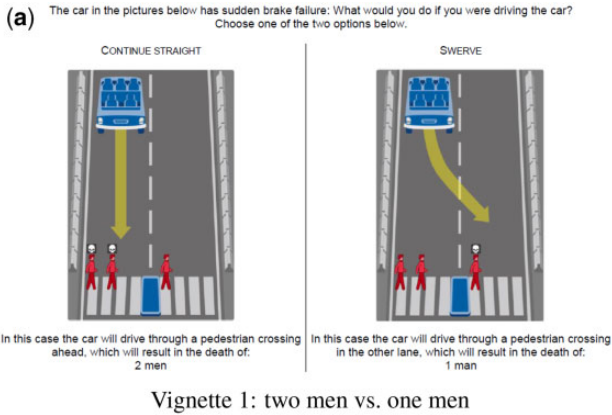


Figure 2. The Vignettes Used in the Study. (a) Vignette 1: two men versus one man. (b) Vignette 2: one man versus two women. (c) Vignette 3: one man versus one woman.

preferences for sparing a man (Awad et al. 2018). In this case, the utilitarian choice of going straight is countered by a moral cost deriving from the individual’s gender preferences. To identify the portion of utilitarian individuals in the population, we combined the answers to Vignettes 1 and 2 by classifying an individual as utilitarian only if he or she had chosen to spare two individuals in both cases, and as non-utilitarian otherwise.

As we specified in the pre-analysis plan, in the analysis we also account for the individual propensity to spare men rather than women displayed by some of the participants in our sample. We assessed the extent of these preferences in Vignette 3 (Figure 2c, which staked one man against one woman) and we used this measure as a control in the regression analysis. The idea behind introducing the control for gender preferences is that it captures whether an hypothetical decision to swerve in Vignette 2 (i.e. sparing one man versus two women) was only due to utilitarian motives or whether it was additionally influenced by gender consideration (the analysis presented in Section 4 shows that excluding this pre-registered control has no effects on the results).¹⁷

17. In addition to the three vignettes just discussed, during the data collection participants were presented with six other vignettes presenting different moral dilemmas in the

Table 1. Utilitarian Participants.

	Model 1	Model 2	Model 3	Model 4
Treated	0.380** (0.167)	0.465** (0.200)	0.479** (0.199)	0.580** (0.230)
Sparing-man	—	−1.545*** (0.270)	−1.559*** (0.272)	−1.490*** (0.255)
Dland	—	—	0.240 (0.337)	0.188 (0.335)
Standard-Controls	Y	Y	Y	Y
Wealth-Controls	N	N	N	Y
Constant	0.923 (0.876)	2.214** (1.025)	2.189** 2.511* (1.037)	(1.285)
No. of observations	576	576	576	576

Notes: Dummy *utilitarian* equal to 1 if the participant opted for sparing the two men in Vignette 1 and the two women in Vignette 2. Logit regression. Standard errors robust for clustering at the village level. Standard-controls included in all regressions: age, gender, religion, marital status, number of family members, participation in household finance management, years of education, whether the village of participation is also the village of birth, years of residence in the village, self-reported weekly income, incentivized measure of risk preferences, village population, distance from paved roads, and whether the village is located in the South. Model 2 adds a dummy for whether the participant took action to spare one man and sacrifice a woman. Model 3 additionally controls for whether participants possess individual land rights; Model 4 adds wealth-controls for: hectares of land owned, whether the house has a cement floor, whether the household possesses a radio or a television, whether the household possesses a motorbike or car, whether somebody in the household holds a bank account or a credit card, whether the house has electricity, whether the house has running water, and a self-reported rank of socio-economic status within the village (1–10). Symbols ***, **, and * indicate significance at the 1, 5, and 10% level, respectively.

3.2 Identification Strategy

Our research design is based on the RCT implementation of the reform. From the list of all villages in two regions that were included in the PFR lottery pool, we randomly selected 32 villages where we performed the data collection from a sample of 576 participants. For our identification strategy to work, some caveats are in order. First, it is required that there were no pre-treatment differences in moral preferences between treatment groups and that our selection of participants resulted in a balanced sample. We do not have pre-treatment data on measures of moral preferences for participants in treated and control villages included in our sample. This is a limitation of our study. Nonetheless, we believe that our design still provides a credible identification strategy to the extent that we can show that the implementation of the reform was based on a random selection of treated and control villages within the lottery pool and that, when analyzing data from our sample of participants, we pass the robustness checks described below.

With respect to the RCT implementation of the reform across Beninese villages, a thorough impact evaluation of the reform carried on by the

form of *Continue straight* versus *Swerve*. These additional vignettes—depicted in Appendix Figures A3–A8—were used to capture the moral weight given to age, gender, and social status as discussed in footnote 11 in the Introduction section.

Table 2. Utilitarian Choices—Heterogeneity Analysis

	Model 1 Road distance	Model 2 Low	Model 3 Market integration	Model 4 Low	Model 5 High Income	Model 6 Low
Sample:	High	Low	High	Low	High	Low
Treated	0.452 (0.331)	814*** (0.299)	1.368*** (0.432)	-0.135 (0.280)	0.820*** (0.313)	0.089 (0.277)
Spare-man	-2.051*** (0.406)	-1.248*** (0.413)	-0.556 (0.444)	-2.186*** (0.330)	-1.600*** (0.341)	-1.552*** (0.375)
Dland	-0.002 (0.022)	0.039 (0.036)	0.065 (0.044)	0.001 (0.025)	0.012 (0.014)	-0.007 (0.018)
Controls	Y	Y	Y	Y	Y	Y
Constant	1.858** (0.795)	0.903 (1.256)	3.080** (1.300)	1.472 (1.376)	0.630 (0.870)	2.286* (1.309)
No. of observations	270	306	188	388	288	288

Notes: Dummy *utilitarian* equal to 1 if the participant opted for sparing the two men in Vignette 1 and the two women in Vignette 2. Logit regression. Standard errors robust for clustering at the village level. Controls included in all regressions: age, gender, religion, marital status, number of family members, participation in household finance management, years of education, whether the village of participation is also the village of birth, years of residence in the village, self-reported weekly income, incentivized measure of risk preferences, village population, distance from paved roads, and whether the village is located in the South. Models 1, 3, and 5 include only the participants living closer to paved roads than the sample median, purchasing more than half of the total calorie intake on the market, and reporting a level of income higher than the sample median, respectively. Models 2, 4, and 6 include only the participants not included in Models 1, 2, and 5. Symbols ***, **, and * indicate significance at the 1, 5, and 10% level, respectively.

World Bank's Gender Innovation Lab returns evidence that the randomization determined by the lottery was successful (Omondi 2019). In particular, the World Bank team made use of both a rich set of pre- and post-treatment survey data collected by a national agency, as well as of administrative monitoring and evaluation data independently collected by the MCC-Benin. The impact evaluation, resulting from a cross-evaluation performed by using these independently collected data sources, show pre-intervention balance on outcome variables between treatment groups and dispels residual concerns regarding the randomization performed by the PFR lottery selection (Goldstein et al. 2016; Omondi 2019).

Concerning our sample of participants, we collected data from residents of 32 villages randomly selected among those in the RCT pool. In Appendix Table A1, we report descriptive statistics relative to 20 socio-demographic characteristics for the subjects who took part in the vignette survey. Participants in the treated group are on average older (40 versus 36.8), more likely to be married (89% versus 83%) and to have running water at home (26% versus 18%), and show a slightly higher literacy rate (40% versus 33%, marginally significant) than those in the control group. Notwithstanding that the sample is balanced on the remaining indicators—including income, acres of land owned, and seven other proxies for household's wealth—these individual characteristics might be associated with affluence and the imbalance might signal that affluent individuals are over-represented among treated.

We summarize here the strategies we adopted to reduce concerns that the results we estimated are biased by affluent individuals self-selecting into the treated group, and we report the details of these robustness checks in the next section. First, we show that in our sample participants' age, marital status, having running water, and more generally income levels are *not* associated with a larger propensity to make utilitarian choices. Second, we employ a Lasso post-double-selection methodology (Belloni et al. 2014) for appropriately selecting the controls to be included in the regression when accidental imbalances in the sample occur, so as to improve the robustness of our causal inference (Chernozhukov et al. 2018b). Finally, we also perform a heterogeneity analysis comparing categories of participants with virtually identical income levels across treatment groups. When comparing these individuals, the analysis still confirms that the treatment effects on moral preferences are concentrated on those who were affected by the reform the most.

Furthermore, to dispel concerns regarding self-selection of participants, it is also necessary to show that subjects have not migrated into treated villages following the PFR randomization. As explained Section 4, we have verified that migrating out of the village of origin is rare for participants in our sample and that migration flows are the same across treatment branches.

3.3 Data Collection Procedures

Survey participants were recruited during fieldwork sessions in Beninese rural villages. A team of research assistants visited 32 villages that have been randomly selected among the list of villages included in the PFR for the regions of Couffou and Mono (in the South of the country) and Alibori and Borgou (in the North). The day before the experiment a research assistant visited the village and requested voluntary participation in the research study to the local population. Among the villagers who showed up at the convened time, we randomly recruited 18 households (9 males and 9 females, older than 18 years old, and maximum of one participant per household) for each village, for a total of 576 participants. Nonselected individuals were paid a show-up fee equal to XOF 500 (approximately \$0.85) and were requested to leave.

Each of the 18 participants received a flat participation fee equal to XOF 500 for taking part in the study. The participants took part in the vignette survey to elicit the moral judgments described above, in a post-experimental survey, and in other tasks not related to this project. In addition, we collected the participants' incentivized measure of risk preferences and socio-demographic information regarding age, gender, religion, marital status, number of family members, participation in household finance management, literacy, village of birth, years of residence in the village, income, and nine additional proxies for individual wealth.¹⁸

18. Specifically: hectares of land owned, whether the house has cement floor, whether the household possesses a radio or a television, whether the household possesses a

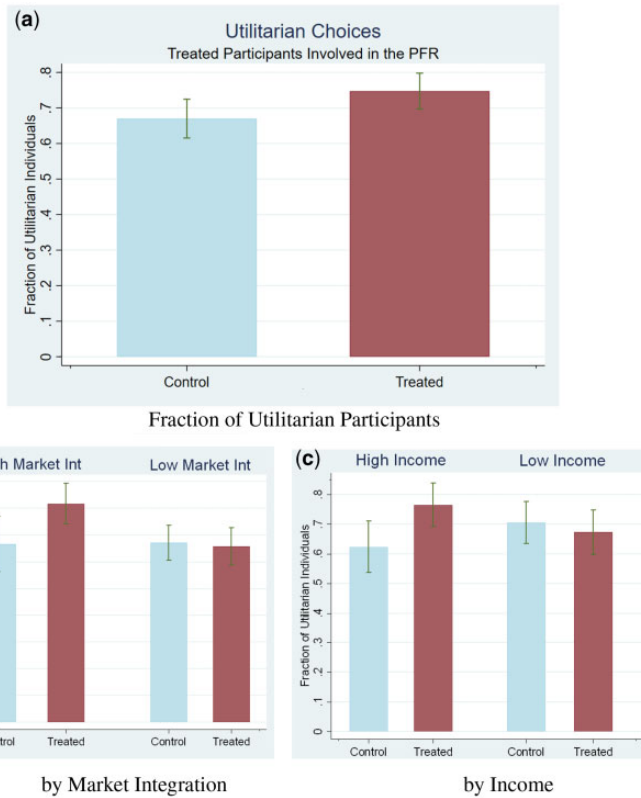


Figure 3. Utilitarian Participants Across Treatments and Conditions. (a) Fraction of utilitarian participants. (b) By market integration. (c) By income.

3.4 Hypothesis and Empirical Specification

The main hypothesis tests whether the introduction of formal property rights determined a shift toward utilitarian moral attitudes. This in turn implies a preference for choosing the option that results in sparing the larger number of lives. Therefore, we hypothesize that, in the decisions presented above:

- In Vignette 1 (Figure 2a), villagers who experienced the reform will spare more often the two men than participants in control villages.
- In Vignette 2 (Figure 2b), villagers who experienced the reform will spare more often the two women than participants in control villages.

motorbike, whether the household possesses a car, whether in the household somebody holds a bank account or a credit card, whether the house has electricity, whether the house has running water, and a self-reported rank of socio-economic status within the village.

Hypothesis 1 *The introduction of formal property rights increases the frequency of utilitarian moral judgments.*

To test the hypothesis, we construct the dummy variable *utilitarian*, which takes the value one if an individual chooses to spare more lives in the two situations just described, and zero otherwise. We then apply a one-sided z -test and a χ^2 test¹⁹ comparing participants in the treated and control samples and a regression analysis that controls for a set of pre-specified socio-demographic characteristics. Specifically, we use a Logit regression model implementing the following specification:

$$\text{utilitarian}_i = \alpha + \delta_T T_i + \mathbf{X}_i + \epsilon_i \quad (1)$$

where T_i is a dummy equal to 1 for subjects in treated villages, and \mathbf{X}_i is a vector the individual characteristics elicited in the post-experimental survey. As specified in the pre-analysis plan and motivated in Section 3.1, in some model specifications, we also added as a control a dummy regarding the outcome of the decision in Vignette 3 (Figure 2c), which identifies those individuals who take a positive action in order to swerve and spare one man instead of one woman (in the analysis presented in the next section, we will show that excluding this control has no effects on the qualitative results).

4. Results

4.1 Main Analysis

We start by verifying that, after the reform implementation, participants had not self-selected through migration into one of the treatment branches. To do so, we collected data regarding the participants' village of origin, the number of years they have been living in the village, and—in case of migration into the village—the reason for migration. The vast majority of participants reside in the same village where they were born (69% in treated and 72% in control, χ^2 test, $p > 10\%$). The majority of migrations were reported by female participants with marriage as the most common reason. The likelihood of having migrated is the same across treatment branches. Finally, we verified that there is no statistically significant difference between treatments in the fraction of adult life a participant has spent in the village where he or she took part in the data collection (two-sided t -test,

19. In the pre-analysis plan, we mentioned that “[S]ince our hypothesis specifies a clear prior regarding the direction of the reform effects, we will apply one-sided tests.” Pre-registering that we have theoretical reasons to use a one-sided test would make sense if we are implementing a z -test for proportions. However, in the same passage, we also mentioned that we would use a χ^2 test, which is essentially a one-sided test (the use of the test as two-sided might generate a fit that is too good, as for instance in the controversy related to the famous Mendel’s pea experiments). In practice, the two-tail probability beyond $\pm z$ for the standard normal distribution equals the right-tail probability above z -squared for the chi-squared distribution with $df=1$ (Agresti 2018: 11). For completeness, we report both the results of a one-sided z -test and of a χ^2 test.

$p > 10\%$). This is consistent with the very low migration rate (less than 1% of surveyed subjects) registered in villages included in the PFR lottery pool during the period 2006–15 (Omondi 2019).

We begin the analysis with a raw comparison of how many participants report to prefer the two utilitarian options—that is, sparing the greater number of lives—in villages interested by the land-tenure reform and in control villages. Out of a balanced sample consisting of 288 participants for each treatment branch, we have 206 individuals displaying utilitarian choices in treated villages against 193 in control villages. A z -test and a χ^2 test show that the difference is not statistically significant. However, a closer look at the post-experimental survey reveals that, among the residents in treated villages who took part to the data collection, 82 individuals do not own land parcels that were included in the land-tenure reform and, hence, they had not experienced directly the formalization of property rights. This could happen either because an individual does not possess land at all or because she holds use-rights over land parcels that are located outside of the administrative boundaries of the treated villages, thus in an area not interested by the PFR, which applied exclusively to land parcels within the borders of the treated village (Goldstein et al. 2018). If we exclude from the sample the 82 residents in treated villages who were not affected by the reform, we will be left with 154 individuals making utilitarian choices, a fraction significantly larger than in the control (one-sided z -test, $p = 3\%$; χ^2 test, $p = 6\%$), as shown graphically in Figure 3a.

We test our hypothesis in a Logistic regression framework. The dummy *utilitarian*—which is equal to one when a participant spares the largest number of lives in both the decisions described above—is regressed on the treatment dummy and a set of socio-demographic controls,²⁰ clustering standard errors at the village level. Model 1 in Table 1 reports the results. The coefficient of the dummy *treated* is positive and statistically significant at the conventional level, indicating that a larger fraction of villagers in the treated sample is engaging in utilitarian choices. The marginal effects of the estimated coefficient suggest that experiencing the reform increases the likelihood of choosing the utilitarian option by roughly 7%.

We continue the analysis by verifying whether controlling for gender preferences over the individuals to be spared affects the results. Indeed, we know from Awad et al. (2018) that there are cross-country and cross-cultural variations in preferences for sparing men versus women. In our setting, to be classified as utilitarian, a participant must spare the largest number of lives irrespective of whether the individuals to be saved are two

20. The controls include: age, gender, religion, marital status, number of family members, participation in household finance management, years of education, whether the village of participation is also the village of birth, years of residence in the village, self-reported weekly income, incentivized measure of risk preferences, village population, distance from paved roads, and whether the village is located in the South.

males (Vignette 1 in Figure 2a) or two women (Vignette 2 in Figure 2a). As specified in the pre-analysis plan and discussed in Section 3.1, we account for a participant's gender preferences in the moral dilemma choice by recording data from Vignette 3 in Figure 2c where participants choose to either sacrifice a male or swerve and consequently kill a woman. In Model 2, we then insert as a control a dummy equal to 1 for those participants who decide to take action in order to save the man and sacrifice the woman. The qualitative results do not change compared to the basic specification. The point estimate of the treatment variable becomes slightly larger, suggesting that experience with the property rights reform increases the likelihood of being classified as utilitarian by approximately 9%. In Model 3, we further add as a control the dummy *dland* equal to one for participants who took part in the survey but do not own land parcels affected by the PFR. The coefficient remains qualitatively and quantitatively similar to the one presented above.

In the context of low- or medium-income countries, self-reported income is not always an appropriate proxy for individual wealth (Moser and Felton 2007; Arrow et al. 2012). Therefore, in Model 4, we additionally include as controls a set of nine proxies for individual wealth.²¹ The coefficient of the treatment variable remains significant at the conventional level, and the point estimate substantially increases, indicating that participants in villages interested by the reform are on average 10.5% more likely than those in control villages to make utilitarian choices. In connection to this, we further verify the robustness of our results with respect to the measures of individual wealth we control for. In Appendix Table A3, we re-estimate the specification presented in Model 4 by proposing four different proxies of individual wealth. Results remain qualitatively the same and quantitatively very similar to those reported in Table 1.

As discussed in Section 3.2, a potential problem for our empirical strategy is that participants across treatment groups lack balance for some individual characteristics (age, marital status, having running water at home) that tend to be positively associated with affluence. First, we address this concern by showing that in our sample those who have access to running water at home, are married, older, or in general more affluent than the sample median do not display a higher frequency of utilitarian choices (two-sided *z*-test for the four comparisons, $p = 0.15$; $p = 0.78$; $p = 0.33$; $p = 0.85$, respectively). Moreover, participants who are older than the median and over-represented in our treated sample—if anything—report on average *lower income and lower utilitarianism* as compared to younger subjects, albeit in both cases the difference is not statistically significant.

21. The wealth controls include: hectares of land owned, whether the house has cement floor, whether the household possesses a radio or a television, whether the household possesses a motorbike, whether the household possesses a car, whether somebody in the household holds a bank account or a credit card, whether the house has electricity, whether the house has running water, and a the self-reported rank of socio-economic status within the village (1–10).

Second, to mitigate residual concerns, we implement a machine learning tool to select the appropriate controls to be included in the regression analysis. Specifically, we re-estimate the models presented in [Table 1](#) by employing the lasso post-double selection approach proposed by [Belloni et al. \(2014\)](#) and the cross-fit partialing-out using cross-validation lasso regression developed by [Chernozhukov et al. \(2018a\)](#). This methodology has been proved useful to select the controls to be included in a regression when accidental imbalances in the sample occur in a principled way ([Belloni et al. 2017](#); [Chernozhukov et al. 2018b](#)). [Appendix Table A2](#) reports the results. The coefficient of the treatment dummy is confirmed to be significant at the conventional level for both approaches. The magnitude of the estimated effect is similar to the one reported in the main analysis, thus adding confidence that the estimated increase in utilitarianism in villages where the reform was implemented is not driven by self-selection of more affluent individuals into the treated group. In [Section 4.2](#), we will come back to this point and provide additional evidence that, holding constant affluence levels, experiencing the reform first-hand is what drives the observed changes.

Finally, the land demarcation and property rights registration process involved in the reform might have affected the rate of conflicts experienced by participants and, as a consequence, this could have an impact on the moral judgments displayed by the individuals. Participants in treated villages reported a total of 28 conflict episodes in the period following the reform, a marginally larger number compared to the 14 episodes reported in the control group. We re-estimate the regression models presented in [Table 1](#) after adding a control for conflicts experienced and we report results in [Appendix Table A4](#). The results remain qualitatively unchanged and point estimates very similarly.

4.2 Heterogeneity Analysis

We now perform a heterogeneity analysis with respect to a set of villagers' socio-demographic characteristics that could shed some light on the channels through which formalized property rights affect moral preferences. We know from previous research ([Casaburi et al. 2013](#); [Asher et al. 2018](#); [Banerjee et al. 2020](#); [Fabbri 2021](#)) and from the survey evidence discussed above that road access is an important determinant of the possibility to reach government institutions and state tribunals and, consequently, to effectively enforce the formalized property rights assigned by the reform. As a first step, we divide subjects into two categories according to the distance of their village from the closest paved road. We consider only the subsample of participants living in villages that are more distant from paved roads than the sample median. For this subsample of participants, we estimate the specifications presented in [Model 4](#) of [Table 1](#). We then repeat the comparison for the subsample of participants living at or closer than the sample median distance from paved roads. Results are summarized graphically in [Figure 2b](#) and reported in [Models 1](#) and [2](#) of [Table 2](#), respectively.

In Model 1, the coefficient of the treatment variable becomes statistically not different from zero. This suggests that, in our subsample of subjects living farther away from paved roads, the effect of experiencing the reform on developing utilitarian preferences is negligible. Conversely, in Model 2, in which we focus on subjects living close to paved roads, the coefficient of the treatment dummy is strongly statistically significant. The point estimate suggests an increase of roughly 16% in the likelihood of making utilitarian choices for treated individuals compared to control ones. Notice that the result reported in Model 2 cannot be driven by higher affluence of participants in the treated group, since we are comparing a sample of participants reporting similar levels of income in the treated and control samples—if anything, participants in control who live close to roads report on average higher income than those in treated (XOF 8800 versus 6800, respectively; two-sided t -test, $p > 10\%$).

We continue the analysis by verifying whether two additional interrelated characteristics, market integration, and income levels, interact with the effects of the property rights reform on moral preferences.²² As a proxy for the level of market integration, we collected data on the share of calories coming from self-produced products versus food purchased on the market by each household. We then classify those participants who report to purchase more than half of their total calories intake on the market as highly market-integrated. In Models 3 and 4, we isolate the effects of experiencing the reform on utilitarian choices for the subsamples of participants characterized by high and low levels of market integration, respectively. In the high-market integration subsample, the formalization of property rights significantly and substantially increases utilitarianism. The point estimate suggests an increase of about 19% for treated participants in the likelihood of being classified as utilitarian compared to control ones. Notice that also in this instance the estimated difference in utilitarian choices cannot be due to differences in income, since we are comparing a subsample of treated and control participants with roughly the same income levels (XOF 10,000 versus 9200 respectively; two-sided t -test, $p > 10\%$). Conversely, in the low-market integration subsample, the estimated effect of the reform on utilitarian morality is zero.

The heterogeneity analysis with respect to income levels points to a similar story, as also summarized graphically in [Figure 3c](#). In Model 5, which isolates the effects of the reform on participants characterized by income levels above the sample median, the coefficient of the treatment dummy is positive and strongly statistically significant, suggesting an estimated 16% increase in the likelihood of making utilitarian choices for participants experiencing formalized property rights. Once more, by construction here, we are comparing a subsample of participants

22. One reason why markets and income levels can interact with property in shaping moral preferences is that access to formal courts is costly and hence enforcement of property rights is de facto only available to higher-income individuals.

characterized by virtually the same income levels between treatment groups (XOF 16,100 for treated versus 15,300 for control; two-sided t -test, $p > 10\%$), thus suggesting that the difference in the levels of utilitarianism observed between the two groups in this subsample does not depend on the participants' affluence. Conversely, the estimated effect is statistically indistinguishable from zero for subjects in the low-income subsample, as shown by the coefficient estimate of Model 6.

5. Discussion

Formal private property rights have a long history in law and legal theory and, for centuries, they have understood as rights *in rem*, that is, as rights “in a thing” (Talamanca 1995: 386–387; Hansmann and Kraakman 2002). This legal definition has fundamental implications for the two crucial events for property holders: enforcement against potential trespassers and transfer.

First, while (possibly multilateral) contractual agreements create rights that are typically good only against the contractual counterparties, a property right can be enforced against *any* third party who comes in contact with the asset (Merrill and Smith 2000). Crucially, this is true irrespective of prior legal relationships between the trespasser and the owner (Hansmann and Kraakman 2002; Ayotte and Bolton 2011). In legal parlance, property rights can be enforced *erga omnes*—that is, “against the world”—and a set of property-specific institutions, such as registries, notaries, and remedies has evolved to ensure both the effectiveness of the enforcement and the provision of reliable and readily accessible information (notice) for all potential third parties as to the contours and the holders of property rights (Hansmann and Kraakman 2002; Arruñada 2012).

The reform we study is no exception and, in fact, the introduction of a property registry and recourse to state courts was two of its central features. At a very fundamental level, these institutions make enforcement of property rights *impersonal*, that is, fully dependent on objective records and disconnected from prior (legal or social) relationships with others in the local community. This is in sharp contrast with the prior system of community enforcement of collective use rights. In this setting, individual rights were inherently embedded in a process that required continual support by the community and was arbitrated by traditional local authorities, which were also fully embedded in the local social network.

Second, while contractual rights are typically non-assignable, property rights include the right to transfer them, fully or partially (Ayotte and Hansmann 2012). Sale and pledge as a collateral are the two most salient ways in which property rights can be transferred and the reform was explicitly aimed at facilitating these forms of transferability. In turn, transferability makes it possible for the owner to easily recover the market value of his or her property by looking at the price of comparable land parcels in the real estate market or at the rates of loans secured on analogous parcels in the credit market. Land, which was previously an invaluable and

virtually inalienable communal resource is now, legally, a “thing” that can be priced and transferred.

To sum up, the preexisting system of communal rights may, somewhat loosely, be described as a multilateral agreement at the village level and gave individuals temporary use rights on a communal resource. These rights could be enforced and transferred only with the agreement of the community, arbitrated by traditional local authorities. Against this background, the introduction of formal property rights set off two major shifts: it transformed enforcement in an impersonal process and facilitated transferability in a market setting. These two features map onto two conjectures as to the possible channels through which property shapes morality, which we will discuss momentarily. Next, we will comment on the fact that the reform did not result in an increase in altruism, and explore the implications of our results for the co-evolution of institutions and morality.

5.1 Conjectural Channel 1: The Loosening of Social Ties

Here we propose and discuss a conjecture that follows from the fact that enforcement under the PFR is impersonal: Formal property rights protected by state courts loosen the web of social ties that support the allocation and enforcement of customary rights; in turn, individuals are freed from the need to refrain from socially alienating moral positions, such as utilitarianism. The link between small communities with strong social ties and their ability to operate internal rules of enforcement and conflict resolution outside and sometimes even in opposition to formal legal systems has been well documented in several settings (Ellickson 1991; Bernstein 1992) and carries with it the opposite implication, that close social ties can be detrimental to the development of impersonal institutions (Greif 1994; Greif and Tabellini 2017). Here we add a twist to this line of argumentation by suggesting that the introduction of impartial institutions can make social ties less relevant and, as a result, loosen their grip on individuals’ moral standings.

To start with, replies to the post-experimental survey by our participants show that the reform resulted in an important increase in material security. We summarize the relevant survey questions and responses in [Appendix Table A5](#). The vast majority of individuals in treated villages appreciate the importance of the reform as protecting rights on land through a system of record-keeping and adjudication in formal courts. Importantly, villagers demonstrate to be well aware of the existence and function of the newly established PFR registries, including their physical location, the possibility to consult them, and their importance as evidence in case of conflicts. Roughly half of the participants have in fact consulted the registry or know somebody who has. Respondents reported to believe that the court system affords effective protection even in the event of a conflict with a more powerful or wealthier individual, suggesting that the reform provided tools for a more effective protection of property rights.

Consistent with this contention, we find that survey responses varied depending on the distance from the closest paved road, a key determinant of effective access to the formal legal system (Fabbri 2021). Individuals in villages closer to paved roads—that is, closer than the median distance—face substantially lower costs of access to formal courts (with a ratio of 1:3), and report to have already had the experience of resolving a land-related dispute in a formal court more often when compared to those farther away. Indeed, easier access to paved roads is associated with substantially more positive responses to the reform along most of the dimensions indicated above. Moreover, individuals closer to paved roads reported a more diffused belief that decisions by formal courts overrule those by customary courts, that disputes should be adjudicated before formal courts rather than before customary courts, and that formal courts are less corrupt than customary courts, as compared to the beliefs held by individuals farther away from paved roads.

Overall, responses to the survey evidence a shared belief that the reform increased material security for those with effective access to formal courts. It has been argued that the lack of material security is a key factor contributing to an individual's parochialism, that is, his or her tendency to favor a close circle of family, kin or friends over strangers (Hruschka and Henrich 2013).²³ In turn, an improvement in material security of the kind we discuss above is thought to encourage interactions with strangers outside one's own kith-and-kin community and the expansion of social networks.²⁴ Indeed, experimental results by one of us (Fabbri 2020) document increased levels of cooperation with anonymous strangers belonging to other village communities for individuals who experienced the PFR reform.

The next step in our conjecture is to link the loosening of social ties to an increase in utilitarianism. Awad et al. (2020) argue that utilitarianism is related to relational mobility, that is, the independent position of an individual vis-à-vis the social network in which he or she lives and operates. Using the *Moral Machine* platform, they collected the responses of 70,000 subjects in 42 countries to three versions of the classic trolley problem.²⁵

23. On how a change in institutions may affect kinship structures see also Bau (2021).

24. A possible caveat as to the long-term effects of private property rights on social ties comes from the observation that the introduction of private property rights in a system with weak state enforcement may foster conflicts and pave the way for the emergence of local (and possibly criminal) organizations with the power to arbitrate in disputes and guarantee their resolution (Gambetta 1996; Bandiera 2003). While, on the one hand, local enforcement may cause a reversal in the importance of social ties, on the other hand, diffuse levels of distrust due to widespread illegal activities may contribute a factor toward their further weakening.

25. Subjects were asked whether it was morally acceptable to kill one in order to spare five in three different scenarios. In the *Switch* scenario a trolley is heading toward five workers and can be stopped by activating a switch that sends the trolley on a side track, away from the five but en route to one person who will consequently die. In the *Loop* scenario the side track is a loop and the killing of the one is instrumental in saving the five: his body will

They find variation across societies in the acceptability of utilitarian sacrifice and document an association with relational mobility. In societies where individuals have few opportunities to make new connections and break loose from their original social network, it may be more important to refrain from holding opinions that could potentially alienate friends. Holding a permissive moral position as to the acceptability of utilitarian sacrifice may be considered as a signal of lack of trustworthiness and hence have negative social consequences. As a result, condemnation of utilitarian sacrifice is the more important the more valuable stable social ties are. In this view, morality is deeply socially functional (Haidt 2007) and hence is sensitive to the institutional setup of society.

Other studies also support this point. Schulz et al. (2019) show that church policy banning cousin marriage in the middle ages broke kinship ties, and that kinship ties are negatively related to individualism, which, in turn is shown to be correlated with utilitarian choices (Awad et al. 2018). This suggests that policies that loosen kinship ties tend to result in more individualistic (and possibly utilitarian) attitudes. Similarly, Enke (2019) shows that societies with historically loose kinship ties evolved moral attitudes—universal moral values, internalized guilt, and altruistic punishment—that are different from those typical of societies with strong kinship ties.

While we stress that this is only a conjecture, we note that the evidence we collected and a broad strand of literature seem to align well with the view that the formalization of property rights resulted in increased utilitarianism through a change in how strongly individuals relate to (and rely on) social connections within their local community.

5.2 Conjectural Channel 2: Commodification

An alternative, but compatible, channel through which property may have affected morality is the process of commodification of values that the PFR set off. Comparing values—such as when choosing between sacrificing one life and sacrificing two lives—generate a moral dilemma mainly because fundamental values are difficult to compare. Philosophers have pointed to incommensurability as an obstacle to utilitarian choices (Raz 1988; Chang 1997). Markets, in contrast, are grounded in the idea that the goods traded can be compared by means of a common medium, money, and carry with them a psychology of commensurability (Schwartz 1986).

The property rights reform we study in this article may be thought as having transformed a resource—the access to which was regulated through a complex system of social control accounting for family needs and redistribution—into a commodity that can be freely traded on the

stop the trolley. Finally, in the *Footbridge* scenario, to save the five a large man must be pushed in front of the trolley. They find a universal qualitative ranking of the acceptability of utilitarian sacrifice in these three scenarios: Switch is more acceptable than Loop, which is more acceptable than Footbridge. They also found, however, quantitative variation as illustrated in the text.

market (Marx 1992 [1867]; Polanyi 1944), and hence has an easily recoverable monetary value. Because land is one of the—if not the—most important productive asset in rural Benin, the reform may have changed the way individuals regard not only land ownership but all things of value more generally, making value comparisons easier or more natural and, in turn, utilitarian judgments more acceptable.

Indeed, the effects we measure are more pronounced in individuals who are more reliant on markets. These considerations suggest an additional vector of moral change, which is based on the individuals' psychology and their relationship with goods and values. This channel is consistent—and possibly concurrent—with the loosening of social ties discussed above, which instead focuses on how individuals relate to each other.

5.3 Utilitarian Sacrifice Versus Altruism

Utilitarianism entails the maximization of aggregate welfare. The implication is that, if to maximize aggregate welfare some harm must be done, which is less than the aggregate gain, then that harm is permissible. A purely utilitarian calculus is unconstrained, that is, it admits the production of *any* amount of harm as long as there is a corresponding offsetting benefit that balances it. In contrast, deontological moral principles identify harms that are inadmissible no matter the benefit. In this article, we focus on this specific implication of utilitarianism, that is, individuals' acceptance of utilitarian sacrifice. However, utilitarianism, as a normative ethical theory, is richer than that, which raises the question whether one can extrapolate from an individual's acceptance of utilitarian sacrifice to infer his or her adherence to the broader prescriptions of the theory. The answer is most likely negative.

Kahane et al. (2018) distinguish between the general principle of welfare maximization, or the doing of good, which is termed “positive utilitarianism,” and its “negative” side, which is related to the production of harm. Put differently, the positive side of utilitarianism embeds a form of moral altruism, where the moral imperative is to accept a personal loss if that allows for the creation of a greater good for somebody else. In contrast, the negative side of utilitarianism embeds the absence or overcoming of deontological constraints to the production of harm for others whenever that harm is instrumental to the generation of a greater good. Kahane et al. (2018) emphasize that the positive and negative sides of utilitarianism are both philosophically and empirically distinguished, and that moral dilemmas, such as the trolley problems used in this study, are designed to exclusively capture the negative side (acceptability of harm) and carry no weight for its positive counterpart (altruism).

Consistently with this approach, we let our participants play a Dictator game framed as a donation to an orphanage in Benin. As shown in Appendix Figure A2, average donations do not differ statistically between treatment groups—if anything, participants in the control sample donate slightly more. This finding is consistent with the results reported in a

companion paper on a previous wave of incentivized experiments in different Beninese villages in which we found that the reform had no effect on individuals' altruism (Fabbri and Dari-Mattiacci 2020).

5.4 Co-evolution of Institutions and Morality

A large literature has emerged in economics around the idea that morality—or, more generally, preferences—and institutions co-evolve (Bisin and Verdier 2011; Mueller 2018). While we only document one side of this relationship and, namely, that formal institutions affect morality, others have emphasized the reverse effect, that of morality on institutions.

Greif (1994) contrasts the close-kin relationships of the Maghribi traders and the more individualistic attitudes of the Genovese traders. While close-kin relationships provided a short-term advantage in terms of enforcement of claims, they also impaired the formation of third-party enforcement systems which eventually favored the Genovese. Enke (2019) emphasizes that the relationship between morality, kinship structure, and economic outcomes amplified over time because societies with loose kinship structures developed moral attitudes that facilitated economic development, which in turn furthered the loosening of social ties.

With respect to private property, two studies (Lehavi and Licht 2011; Dari-Mattiacci and Guerriero 2015) found that an individualistic culture tends to result in stronger property rights. A particular characteristic of the language spoken by the plurality group—license to drop the first-person pronoun in a sentence—is used as an instrument for individualism.²⁶ Since languages evolved before the formalization of property rights, these studies conclude that culture affects the structure of property rights. Given the cross-country correlation between a utilitarian morality and an individualistic culture (Awad et al. 2018), their and our results suggest that there may be a self-reinforcing co-evolutionary process of utilitarian moral attitudes and formal property rights. Finally, these findings are in line with recent accounts of the institutional, psychological, and moral peculiarities of western populations (Henrich 2020).

6. Conclusion

In this article, we have shown that the randomized introduction of formal property rights in a pool of Beninese villages resulted in a measurable moral drift toward utilitarianism. Since formal property rights are a key feature of Western economies, our results relate to recent studies on the moral and psychological peculiarities of Westerners (Henrich 2020), and may contribute a factor explaining the patterns of geographical variation in moral attitudes across the globe (Awad et al. 2018). While our results

²⁶ Languages that do not allow dropping the first-person pronoun (such as English) put more emphasis on the individual than languages (such as Italian) where pronoun-drop is allowed, which in turns correlates with survey measures of individualism.

focus on land, the past several decades have seen an expansion of property rights in (and property rights narratives concerning) intangible assets, such as data and ideas. To the extent that the effects we document are common to other forms of property, these trends may be contributing to the causes pushing western societies toward becoming even more utilitarian, as documented by [Hannikainen et al. \(2018\)](#). In addition, since property rights reforms have been popular over the past few decades, our results imply that they may have also set the reforms recipients on a path of changing moral values.

Our results may also have implications as to the effects of unintended institutional changes. One of the largest and most cataclysmic events in recent history is the fall of communism in the late 1980s and early 1990s and the demise of the USSR. [Alesina and Fuchs-Schündeln \(2007\)](#) show that living under communism for 50 years in East Germany made individuals believe more strongly that social conditions determine one's fortunes and favor state intervention as compared to the control group of comparable West Germans. They also show that preferences are quickly (re-) converging so that the two groups are predicted to be indistinguishable within two generations. One could take the opposite perspective and investigate the effect that the fall (rather than the introduction) of communism had on preferences (and morality). Conspicuously, former communist countries reverted back to formal private property. Our results suggest that morality may have changed in former communist countries after the demise of communism in the same way as it changed in rural Beninese villages.

The effect of property on utilitarianism that we document was measured only 9 years after the reform. This is a remarkably short period of time if compared with the notion of an innate psychology of ownership acquired in early childhood ([Nancekivell et al. 2019](#)) and with studies evidencing that moral change in society occurs especially across generations, with limited effects within generations ([Hannikainen et al. 2018](#)). What explains the short-term effects we detect? Is the long-term impact of the reform even larger or has it already climaxed? While we offer a conjecture as to the channels through which formal property affects morality, more research is needed in order to clarify the psychological and sociological mechanisms at work.

Conflict of interest

The authors declare no competing interests.

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Appendix A Supplementary Analysis

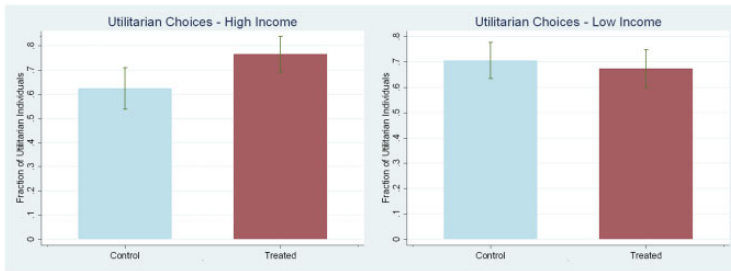


Figure A1. Utilitarian Choices Comparing Across Treatment Groups Participants with High Income (left panel) and Low Income (right panel).

Table A1. Balance of observables across treatment groups (*t*-test two-sided for continuous variable and chi-square test for dummy variables)

Variable	PFR Reform (<i>n</i> = 288)	Control (<i>n</i> = 288)	Difference (<i>p</i> -value)
Male	0.49	0.51	0.73
Age	40.0	36.8	0.01
Muslim	0.45	0.41	0.27
Vodoun	0.19	0.18	0.91
Married	0.89	0.83	0.02
Household nr	9.8	10.0	0.68
Manage finance	0.95	0.95	0.99
Literate	0.40	0.33	0.08
Born village	0.69	0.72	0.41
Years in village	32.3	30.9	0.24
Weekly income (XOF)	9026	8468	0.59
Land use (Hect)	5.47	5.10	0.65
Concrete floor	0.64	0.59	0.23
Electricity	0.36	0.36	0.99
Water	0.26	0.18	0.02
Radio-TV	0.63	0.63	0.99
Car	0.09	0.07	0.28
Moto	0.77	0.78	0.69
Bank-acc	0.33	0.27	0.12
Social-rank	4.45	4.36	0.56

Table A2. Utilitarian Participants—Lasso Post-Double-Selection and Cross-Fit Partialing-Out Methodologies for Selection of Controls

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Treated	0.076** (0.034)	0.078** (0.033)	0.087** (0.039)	0.093** (0.039)	0.084** (0.039)	0.089** (0.039)
Confidence Interval	0.010– 0.143	0.013– 0.143	0.010– 0.164	0.016– 0.171	0.007– 0.161	0.012– 0.166
<i>p</i> -value	0.024	0.018	0.028	0.018	0.033	0.024
No. of observations	576	576	576	576	576	576

Notes: Dependent variable: Dummy *utilitarian* equal to 1 if the participant opted for sparing the two men in Vignette 1 and the two women in Vignette 2. Models 1 and 2: Regularized post-double selection lasso regression (Belloni et al. 2014); Models 3–6: cross-fit partialing-out using cross-validation lasso regression (Chernozhukov et al. 2018a). In Models 3 and 4, the moment conditions are solved using the observations in each fold to produce *K* different estimates and then averages these *K* estimates to produce the final estimate for the coefficients of interest. In Models 5 and 6 all the observations are used to solve the moment conditions to produce a single final estimate for the coefficients of interest. High-dim controls included: age, gender, religion, marital status, number of family members, participation in household finance management, years of literate, whether the village of participation is also the village of birth, years of residence in the village, self-reported weekly income, and incentivized measure of risk preferences. Models 2, 4, and 6 always include as a control whether participants possess individual land rights. Standard errors robust for clustering at the village level. Symbols ***, **, and * indicate significance at the 1, 5, and 10% level, respectively.

Table A3. Utilitarian Participants—Different Wealth Measures

	Model 1	Model 2	Model 3	Model 4
Treated	0.475** (0.196)	0.557** (0.231)	0.498** (0.212)	0.476** (0.206)
Constant	2.814** (1.266)	2.247** (1.086)	2.277* (1.183)	2.439** (1.165)
No. of observations	576	576	576	576

Notes: Dummy *utilitarian* equal to 1 if the participant opted for sparing the two men in Vignette 1 and the two women in Vignette 2. Logit regression. Standard errors robust for clustering at the village level. Controls included in all regressions: age, gender, religion, marital status, number of family members, participation to household finance management, years of education, whether the village of participation is also the village of birth, years of residence in the village, incentivized measure of risk preferences, village population, distance from paved roads, and whether the village is located in the South, a dummy for whether the participant took action to spare one man and sacrifice a woman. As a proxy for wealth, Model 1 uses self-reported rank of socio-economic status within the village (1–10); Model 2 uses hectares of land owned, whether the house has cement floor, whether the house has electricity, whether the house has running water; Model 3 uses whether the household possesses a radio or a television, whether the household possesses a motorbike or car, whether somebody in the household holds a bank account or a credit card; Model 4 uses a compounded index of income (whether above or below the median) and all the elements in Models 1–3. Symbols ***, **, and * indicate significance at the 1, 5 and 10% level, respectively.

Table A4. Utilitarian Participants—Controlling for Conflicts Experienced

	Model 1	Model 2	Model 3	Model 4
Treated	0.382** (0.168)	0.468** (0.201)	0.500** (0.198)	0.436** (0.197)
Conflicts	-0.048 (0.322)	-0.085 (0.350)	0.001 (0.348)	0.053 (0.341)
Sparing-man	—	-1.547*** (0.260)	-1.558*** (0.250)	-1.638*** (0.271)
Dland	—	—	0.300 (0.334)	0.338 (0.331)
Wealth controls	N	N	N	Y
Constant	1.044 (0.902)	2.329** (1.074)	2.771** (1.252)	3.280*** (1.210)
No. of observations	576	576	576	576

Notes: Dummy *utilitarian* equal to 1 if the participant opted for sparing the two men in Vignette 1 and the two women in Vignette 2. Logit regression. Standard errors robust for clustering at the village level. Controls included in all regressions: age, gender, religion, marital status, number of family members, participation to household finance management, years of education, whether the village of participation is also the village of birth, years of residence in the village, self-reported weekly income, incentivized measure of risk preferences, village population, distance from paved roads, and whether the village is located in the South. Model 2 adds a dummy for whether the participant took action to spare one man and sacrifice a woman. Model 3 additionally controls for whether participants possess individual land rights; Model 4 adds controls for: hectares of land owned, whether the house has cement floor, whether the household possesses a radio or a television, whether the household possesses a motorbike or car, whether somebody in the household holds a bank account or a credit card, whether the house has electricity, whether the house has running water, and a self-reported rank of socio-economic status within the village (1–10). Symbols ***, **, and * indicate significance at the 1, 5, and 10% level, respectively.

Table A5. Post-Experimental Survey (treated villages sample)

	Road distance		
	High (%)	Low (%)	diff
Importance of PFR			
1. Believes PFR certificate protects from expropriation	91	94	—
2. Believes PFR certificate helps prevail in a conflict with powerful individual	89	89	—
3. Willing to contest eviction due to PFR	44	40	—
4. Would ask for PFR certificate before buying land	96	97	—
Knowledge and beliefs about PFR			
5. Knows where to consult PFR registry	26	37	**
6. Knows somebody who consulted PFR	24	39	***
7. Thinks conflicts should be solved before formal court	15	53	***
8. Thinks formal court decisions are more important than customary court decisions	66	88	***
9. Thinks formal courts are more effective in resolving conflicts than customary courts (scale: 1–7)	3.36	3.76	***
10. Thinks that formal courts are as corrupt as or less corrupt than customary courts	20	45	***

Continued

Table A5. Continued

	Road distance		
	High (%)	Low (%)	diff
11. Thinks formal courts can be used by the rich to subvert decisions taken by customary courts Experience with PFR	86	80	—
12. Initiated procedure in formal courts rather than customary court	16	40	*
13. Knows somebody who solved a conflict in formal courts	9	41	***
14. Costs of conflict resolution before formal courts (thousand XOF)	1233	382	**

Notes: For each question $N=276$, except for questions 12 and 14 where $N=38$ (these questions were posed only to respondents reporting to have had land-related conflicts). Symbols ***, **, and * indicate significance at the 1, 5, and 10% level, respectively.

Table A6. Moral Preferences for Gender

	Model 1	Model 2	Model 3	Model 4
Treated	0.007 (0.096)	0.007 (0.096)	0.006 (0.096)	0.042 (0.122)
Dland	— —	— —	-0.027 (0.199)	-0.120 (0.216)
Wealth controls	N	N	N	Y
Cut 1	-0.174 (0.461)	-0.174 (0.461)	-0.180 (0.460)	-0.132 (0.556)
Cut 2	0.609 (0.453)	0.609 (0.453)	0.603 (0.451)	0.703 (0.554)
No. of observations	576	576	576	576

Notes: Dependent variable: sparing-women $\in \{0; 2\}$. Ordinal Probit regression. Standard errors robust for clustering at the village level. Controls included in all regressions: age, gender, religion, marital status, whether polygamist, number of family members, participation in household finance management, years of education, whether the village of participation is also the village of birth, years of residence in the village, self-reported weekly income, incentivized measure of risk preferences, village population, distance from paved roads, and whether the village is located in the South. Model 3 additionally controls for whether participants possess individual land rights; Model 4 adds controls for: hectares of land owned, whether the house has cement floor, whether the household possess either a radio or a television, a motorbike or car, whether in the household somebody holds a bank account or a credit card, whether the house has electricity, whether the house has running water, self-reported rank of socio-economic status within the village (1–10). Symbols ***, **, and * indicate significance at the 1, 5, and 10% level, respectively.



Figure A2. Coins donated in a Dictator game framed as a donation to an orphanage.

Table A7. Moral Preferences Related to Age

	Model 1	Model 2	Model 3	Model 4
Treated	-0.040 (0.085)	-0.018 (0.110)	-0.007 (0.109)	0.058 (0.117)
Sparing-man	—	-1.087*** (0.134)	-1.101*** (0.131)	-1.050*** (0.133)
Dland	—	—	0.246 (0.154)	0.207 (0.145)
Wealth controls	N	N	N	Y
Cut 1	-1.279*** (0.365)	-2.189*** (0.410)	-2.146*** (0.423)	-2.437*** (0.450)
Cut 2	0.127 (0.350)	-0.592* (0.348)	-0.539 (0.365)	-0.790* (0.439)
Cut 3	0.918** (0.358)	0.308 (0.350)	0.365 (0.366)	0.154 (0.429)
No. of observations	576	576	576	576

Notes: Dependent variable: sparing-young $\in \{0, 3\}$ Ordinal Probit regression. Standard errors robust for clustering at the village level. Controls included in all regressions: age, gender, religion, marital status, whether polygamist, number of family members, participation in household finance management, years of education, whether the village of participation is also the village of birth, years of residence in the village, self-reported weekly income, incentivized measure of risk preferences, village population, distance from paved roads, and whether the village is located in the South. Model 2 adds a dummy for whether the participant took action to spare one man and sacrifice a woman. Model 3 additionally controls for whether participants possess individual land rights; Model 4 adds controls for: hectares of land owned, whether the house has cement floor, whether the household possesses either a radio or a television, a motor-bike or car, whether in the household somebody holds a bank account or a credit card, whether the house has electricity, whether the house has running water, self-reported rank of socio-economic status within the village (1–10). Symbols ***, **, and * indicate significance at the 1, 5, and 10% level, respectively.

Table A8. Moral Preferences for Social Status

	Model 1	Model 2	Model 3	Model 4
Treated	0.121 (0.104)	0.109 (0.103)	0.108 (0.102)	0.083 (0.106)
Sparing-man	—	0.703*** (0.138)	0.704*** (0.139)	0.671*** (0.143)
Dland	—	—	-0.016 (0.140)	-0.006 (0.138)
Wealth controls	N	N	N	Y
Cut 1	-1.592*** (0.607)	-1.220* (0.640)	-1.222* (0.641)	-0.980 (0.658)
Cut 2	0.363 (0.583)	0.806 (0.634)	0.804 (0.634)	1.082* (0.655)
Cut 3	1.653*** (0.524)	2.201*** (0.580)	2.199*** (0.580)	2.489*** (0.611)
No. of observations	576	576	576	576

Notes: Dependent variable: sparing-businessmen $\in \{0; 3\}$. Ordinal Probit regression. Standard errors robust for clustering at the village level. Controls included in all regressions: age, gender, religion, marital status, whether polygamist, number of family members, participation to household finance management, years of education, whether the village of participation is also the village of birth, years of residence in the village, self-reported weekly income, incentivized measure of risk preferences, village population, distance from paved roads, and whether the village is located in the South. Model 2 adds a dummy for whether the participant took action to spare one man and sacrifice a woman. Model 3 additionally controls for whether participants possess individual land rights; Model 4 adds controls for: hectares of land owned, whether the house has cement floor, whether the household possesses either a radio or a television, a motorbike or car, whether in the household somebody holds a bank account or a credit card, whether the house has electricity, whether the house has running water, self-reported rank of socio-economic status within the village (1–10). Symbols ***, **, and * indicate significance at the 1, 5, and 10% level, respectively.

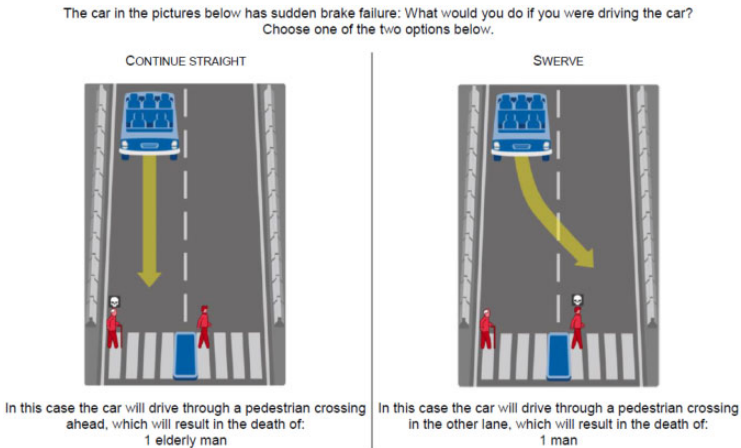


Figure A3. Vignette 4: One elderly man versus one man.

The car in the pictures below has sudden brake failure: What would you do if you were driving the car?
Choose one of the two options below.

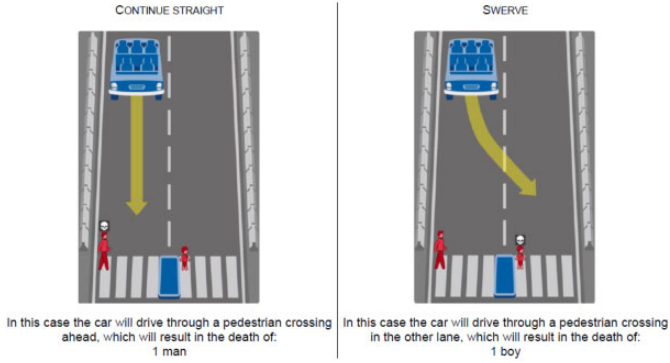


Figure A4. Vignette 5: One man versus one boy.

The car in the pictures below has sudden brake failure: What would you do if you were driving the car?
Choose one of the two options below.

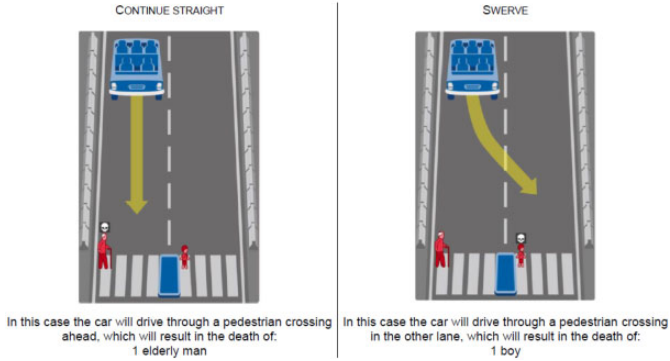


Figure A5. Vignette 6: One elderly man versus one boy.

The car in the pictures below has sudden brake failure: What would you do if you were driving the car?
Choose one of the two options below.

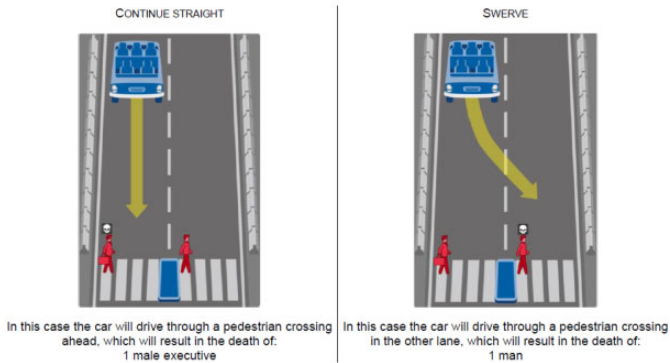


Figure A6. Vignette 7: One male executive versus one man.

The car in the pictures below has sudden brake failure: What would you do if you were driving the car?
Choose one of the two options below.

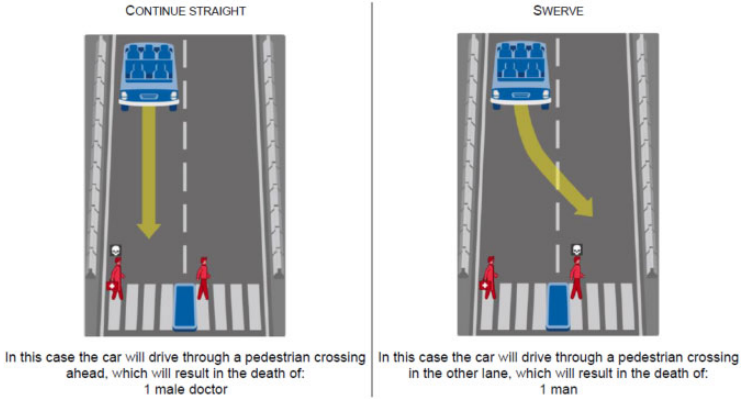


Figure A7. Vignette 8: One male doctor versus one man.

The car in the pictures below has sudden brake failure: What would you do if you were driving the car?
Choose one of the two options below.

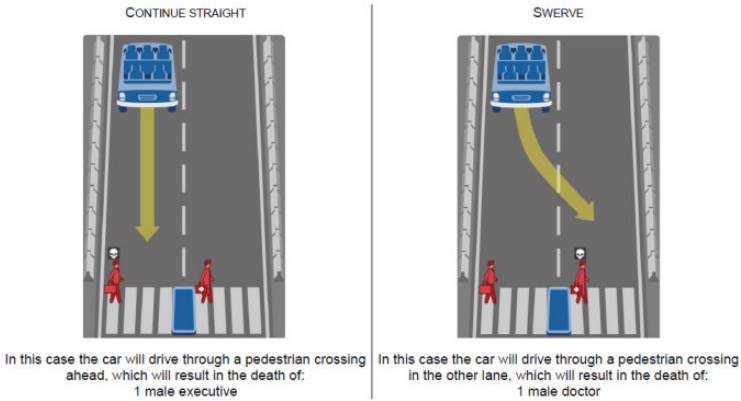


Figure A8. Vignette 9: One executive versus one doctor.

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