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Does Mitigation Begin At Home?

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Does Mitigation Begin At Home?*

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Abstract

In a climate system that is indifferent about where mitigation is carried out, the logic of comparative advantages favors abatement locations in developing and rapidly industrializing countries. There is evidence, however, that citizens of industrialized countries who voluntarily fund climate mitigation activities are not indifferent about the mitigation location. In our artifactual online experiment, subjects located in a European Union member state took a dichotomous choice between a cash prize and the verified mitigation of one metric ton of CO2. The treatment condition varied the location of the mitigation activity between the European Union and developing countries. We test whether the location impacts on the probability that the mitigation activity is chosen, harnessing between- and within-subject variation in our panel data. Our evidence shows that subjects responded to the location being made salient, but, contrary to previous concerns, were indifferent between mitigation sites in the EU or developing countries.

JEL Classifications: D10, H41, Q54

Keywords: Climate change; mitigation; public goods; locational preferences; home bias; online experiment; field experiment.

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1 Introduction

Climate change presents humanity with an environmental problem of unprecedented nature and scale (Nordhaus 1993, Stern 2006). The scale of this problem has mobilized many people in industrialized countries not only to voice their support for public policies aimed at emissions reductions (Aldy et al. 2012, Bechtel and Scheve 2013). A considerable share of these populations also shows a willingness to voluntarily provide mitigation services as a global public good (Diederich and Goeschl 2014, Lindman et al. 2013, Löschel et al. 2013, Aldy et al. 2012).

A widely acknowledged feature of the climate change problem is the fact that the climate system is essentially indifferent as to where on Earth mitigation activities are carried out. From an economic perspective, this source indifference has significant implications. It is synomymous with uniformly mixed pollutants and implies that the gains from spatial specialization in providing abatement are maximal because pollution damages do not vary with the spatial configuration of abatement. Differently put, the global division of labor in providing climate change mitigation can be allowed to be determined entirely by countries' comparative advantage in mitigation costs. These costs differ significantly across the globe. Empirical estimates put the international differences in marginal abatement costs for CO2 across the globe up to two orders of magnitude (Beach et al. 2015, Morris et al. 2012, Criqui et al. 1999). This is driven by differences in sectoral composition and gradients in technological efficiency between countries, with relatively cheap mitigation in industrializing and relatively expensive mitigation opportunities in already industrialized countries. The empirical evidence on international trade in emission allowances under the UNFCCC confirms this pattern (Dechezleprêtre et al. 2009). The prevailing mitigation cost heterogeneities therefore offer significant scope for harnessing the logic of comparative advantage in order to provide the global public good of mitigation provision. On the other hand, the same cost heterogeneities also mean that deviations from the logic of comparative advantage impose excess aggregate mitigation costs.

Would people in industrialized countries that voluntarily contribute mitigation efforts

want to deviate from the logic of comparative advantage because they have locational preferences over mitigation activities? Several possible reasons for the presence of locational preferences have been pointed out in the literature. On narrowly economic grounds, individuals may, *ceteris paribus*, prefer mitigation closer to home because they believe that GHG mitigation provides some amount of localized co-benefits (West et al. 2013, Bollen et al. 2009).[1] If individuals care more about some group of localized co-beneficiaries than about another, then mitigation closer to the location of the favored group could be preferred. Most commonly, this preference would be suspected to take the form of a 'home' bias (Buchan et al. 2009). Another economic reason is that individuals could believe that problems of compliance and trustworthiness mitigate or perhaps even reverse the comparative advantage of developing countries in mitigation activities. If subjects believe that mitigation projects in developing countries have little additionality (Hayashi and Michaelowa 2013) or suffer from fraud (Lovell 2010, Gillenwater et al. 2007), this will subtract from the expected productivity of carrying out mitigation in locations with lower institutional quality compared to industrialized countries. Behavioral factors provide another set of possible reasons for locational preferences. For example, a transfer of resources for mitigation activities could run counter to a desire to restrict benefits to a group with which the donor feels greater affinity, a phenomenon known as 'social discounting' (Strombach et al. 2014, Jones and Rachlin 2006). Social identity could therefore give rise to locational preferences that favor provision closer to home. Another behavioral mechanism could be that individuals have an offset motive that is prejudiced towards offsetting one's own emissions (Kotchen 2009). Carrying out mitigation closer to home could also be construed as benefitting from a demonstration effect (Shang and Croson 2009, Feldstein and Clotfelter 1976), signalling e.g. for EU citizens the European Union's perceived global leadership in climate policy (Sarasini 2009). Reasons for preferring mitigation activities to be carried out *ceteris paribus* in developing countries mainly focus on redistributive motives: To the extent that locating mitigation activities there generates local income effects, a decreasing marginal utility of money could be argued to justify a transfer of resources there (Dutschke and Michaelowa 2006).

[1] Reduction in GHG emissions typically target the fossil energy sector that is also responsible for the emission of other pollutants such as sulfur oxides and nitrous oxides.

Previous and concurrent evidence on locational preferences for public goods production offers an inconclusive picture both in general and in the particular context of climate policies. In the general public goods literature, studies of multi-level public goods games generally find that under laboratory conditions, subjects exhibit a significant preference for restricting the benefits of public goods provision to individuals with whom they share a common group affiliation (Blackwell and McKee 2003, Fellner and Lünser 2014, Chakravarty and Fonseca 2017). This finding is, however, only partially supported by field evidence (Buchan et al. 2009, Gallier et al. 2017). In the more specific context of climate change mitigation, a choice experiment conducted in Mexico (Torres et al. 2015) examines the willingness to pay for offset activities and find greater support for mitigation activities located geographically closer to the subject. Two studies concurrent to ours report on incentivized experiments: In Baranzini et al. (2016), around 300 student subjects from Geneva, Switzerland allocate strictly more of their endowment to mitigating in a Nicaraguan forestry-based carbon offset project than in a Swiss one, where the Nicaraguan project yields three times as much mitigation per unit of endowment.[2] Buntaine and Prather (2017) ask US subjects recruited from Amazon Mechanical Turk (AMT) to make a real donation to the Greenhouse Gas Protocol program of a US NGO, the World Resource Institute (WRI). In one experiment, 1,214 subjects make a dichotomous choice between keeping an unexpected $0.50 windfall or donating it to a mitigation project located in the US, India, or China, depending on the random assignment. In a second experiment, 2,656 subjects allocate a possible $20 prize win between three recipients: themselves, a US mitigation project or a rapidly-industrializing country project.[3] In contrast to Baranzini et al. (2016), they find that without an information intervention, US subjects are significantly more likely to donate to mitigation activities located in the US in the dichotomous choice experiment and donate more to US-based activities in the allocation task experiment.[4]

[2]The Nicaraguan project receives up to three-quarters of the endowments on average in a treatment that emphasizes the productivity differences while an emphasis either on the trustworthiness or local co-benefits fails to affect the relative allocation between locations.

[3]The treatments vary by information provided, with no information in the baseline and treatments with ordinal information about relative mitigation productivity ('most', 'somewhat', 'least' cost effective) and about local co-benefits.

[4]Relative information about cost effectiveness reduces this bias, leading to convergence in the propensity to donate in experiment 1 and a reversal in the amounts donated in experiment 2.

The ideal experimental setting for testing whether individuals have locational preferences over the mitigation site for their voluntary public goods provision would involve a large number of subjects who are distributed across a wide space and who are offered the opportunity to provide a perfectly uniform mitigation amount for the same cost. For each individual, the experimenter would then exogenously vary the location of where the perfectly uniform mitigation activity would be carried out, observe variations in the subjects propensity to choose mitigation, and then recover the propensity to provide the public good as a function of the spatial relationship between the subjects location and the site of provision. This relationship could capture geographical distance, jurisdictional borders, cultural and economic heterogeneity, etc. and provide an insight into how these factors affect the propensity to provide the public good. In the real world, the experimenter faces several obstacles to implementing the ideal experiment, key among them the free choice of where mitigation is carried out, how costly it is, and the perfect uniformity of mitigation activities. The strategy of our online experiment is to offer solutions to these key obstacles that approximate this ideal setting: We offer a total of 3,940 individuals in a European Union member state a dichotomous choice between a cash prize between €2 and €100, randomly assigned, and the verified mitigation of one metric ton of CO2 emissions, controlling for fixed effects. The core results come from the comparison of the choices of 1,585 subjects in two treatment variations that make the location of the mitigation activity explicit. In one treatment (EU), subjects are explicitly informed that the verified mitigation of one metric ton of CO2 will be carried out within the European Union, from where the subject's own emissions originate; in the other treatment (DC), they are informed that the verified mitigation of one metric ton of CO2 will be carried out in a developing country. In a consecutive choice, the location of the mitigation activity is switched. Assignment to the different treatments is randomized. On this basis, we test for differences in the propensity to undertake a voluntary mitigation action of uniform climate impact between the EU treatment and the DC treatment. Finally, we also compare these results to the choices of 2,354 subjects that take the same decision in a similar treatment (NE) in which the EU location of mitigation is introduced in neutral language and not made salient.

Summarizing our findings, the decisions of our Internet-representative sample do not exhibit locational preferences between a mitigation site in the European Union and one in a developing country: There were no statistically significant differences between the EU and the DC treatment in subjects' propensity to choose CO2 mitigation over the cash prize. This result holds both in a between-subjects and a within-subjects analysis and is robust to the inclusion of numerous controls. The price elasticity of the propensity to provide mitigation is very similar, with some between-subjects evidence that mitigation in developing countries is less sensitive to cost than mitigation in the European Union. Compared to the NE treatment, which introduced the location neutrally and with little salience, we find that being explicit about the location of where a subject's voluntary mitigation activities will be carried out made the average subject more likely to choose mitigation over the cash prize. Failing to observe locational preferences is therefore not driven by a lack of salience. Given our experimental control over the cost of mitigation for the individual, the verified uniformity of mitigation activities across EU and developing country sites, and the Internet-representative nature of our sample, we interpret this finding as evidence that across the population, locational preferences need not stand in the way of realizing the gains from comparative advantage in climate change mitigation.

The next section describes the experimental design and subject sample. Section 3 then presents the main results, which we discuss in section 4. Section 5 concludes.

2 Experimental design and subject sample

2.1 Experimental design

We conducted a simple experiment involving two consecutive binary choices between receiving a cash award and providing an actual carbon emissions reduction. For each of the two choices, the cash award, representing the opportunity cost of the emissions reduction, was drawn randomly ranging, in increments of €2, from €2 to €100. The upper bound of the distribution corresponds to estimates of the maximum marginal abatement cost per ton of CO_2 equivalent (McKinsey & Company 2010, Kesicki and Ekins 2012). The carbon emissions reduction amounted to one ton of carbon dioxide

Table 1: Two-stages counterfactual design of treatment administration

	Experimental group		
Choice	1	2	3
1	EU	DC	NE
2	DC	EU	other
# of subjects:	788	797	2,355

in each choice task and was implemented using one of two instruments, depending on the treatment. In the EU and NE treatments, the emissions reduction was facilitated by purchasing and deleting one EU emissions allowance (EUA) under the EU Emissions Trading Scheme (EU ETS). Deleting one EUA lowers the total cap of the trading scheme (1.856 billion tons for the relevant trading period), and hence emissions, by one ton. In the DC treatment, the emissions reduction was facilitated by purchasing and deleting one Certified Emissions Reduction (CER) based on the Clean Development Mechanism of the Kyoto Protocol. CER are tradable carbon offsets generated by emission reduction projects in developing countries which cut down on emissions compared to the business-as-usual scenario. In order to minimize concerns regarding additionality, the certitificate was of the "Gold Standard" quality.[5]

Each of the two subsequent choices was subject to a different treatment. The counterfactual assignment of treatments (Table 1) allows us to analyze between-subjects differences separately as well as to take advantage of the panel structure of the data. Subjects were randomly assigned to one of three experimental groups which determined the sequence of treatments they faced. In group 1, the EU treatment preceded the DC treatment. In group 2, the sequence was in reverse order. In group 3, subjects faced the NE treatment in their first choice and were administered some unrelated other treatment in their second choice.[6] Hence, only independent first choices can be compared between-subjects between all three treatments while for the EU and CD treatments, a full panel is obtained.

[5]The question of additionality refers to the problem how reliable are estimates of the business-as-usual emission path that would have occurred in absence of the carbon-offset. This has led to the critique of heterogenous "qualities" of CER. See, for example, http://www.cdmgoldstandard.org.

[6]The fact that there were three other unrelated treatments for the second choice of group 3 explains why about three times as many subjects were assigned to this group than to groups 1 and 2.

Treatments were administered on two computer screens in the experiment, the *information screen* and the subsequent *decision screen*. For all choices in all treatments, the information screen introduced the offered choice between a cash award and a one ton CO_2 emissions reduction and included a succinct explanation of how choosing the emissions reduction, by deleting either an EUA or Gold Standard CER, would result in a real, reliable, and verifiable reduction in CO_2 emissions. Each choice was framed as choosing the prize for a lottery in which winners' choices would be implemented.[7] In all treatments, the information screen concluded by advising subjects on the pure public good character of providing an emissions reduction and informing them about the odds of the lottery. All instructions about the award choice were kept short and simple and refrained from giving extensive background information about climate change.[8] The subsequent decision screen described how each option, if chosen, would be implemented in the experiment and elicited the choice.[9] The two prize alternatives, including the subject-specific amount of the cash award and the full name of the emission reduction option were then presented in random order and subjects had to check the preferred option.

Treatments differed in how the emissions reduction option was presented on both screens. While the EU treatment text emphasized that emission cuts would take place domestically within the EU and hence, would concern emissions to which the subject's

[7]We used a between-subjects random incentive system (RIS) in order to limit total cost of the experiment (Grether and Plott 1979, Starmer and Sugden 1991, Lee 2008). The RIS was between-subjects (Tversky and Kahneman 1981, Abdellaoui et al. 2011, Baltussen et al. 2012) with odds of 1:50 that the subject's choice was realized. Between-subjects and within-subject RIS have been subject to examination for possible biases. While between-subjects introduces noise and decreases risk aversion, there is less evidence of a systematic bias for simple tasks (Cubitt et al. 1998, Baltussen et al. 2012). In one example, between-subjects RIS has been shown to affect behavior in dictator games (Sefton 1992) while for ultimatum games, behavior was unaffected (Bolle 1990).

[8]Researchers have taken opposing stands as to the extent to which information should be provided about an unfamiliar good that is to be valued or for which demand is to be revealed by study participants. While the best practice in contingent valuation generally calls for providing respondents with extensive information, it has also been argued against giving potentially choice-relevant information around the time of the contribution decision (Arrow et al. 1993). In our case, we expected our participants to overwhelmingly accept the empirical veracity of climate change and its anthropogenic cause in the form of greenhouse gas emissions based on prior survey evidence for German citizens (European Commission 2008). In this respect, we expect that describing only a few key items that differed between treatments when describing the choice would, if any, introduce a bias *in favor* of finding differences between treatments.

[9]Subjects that chose the cash award would have the value credited to their personal account at the polling company while those who chose the emissions reductions could verify that the emissions reduction had been carried out through certification presented on a university website.

personal energy use contributes, the DC treatment emphasized that emission cuts would be achieved by an emission reduction project in a developing country that would guarantee social and environmental side-benefits for the local population. In contrast to these two framings, the NE treatment, although employing EUAs like the EU treatment, made the location of the reduction less salient than the EU and DC treatments. In particular, the NE treatment did not include the addition "within the European Union" to the name of the emissions reduction option on both screens and did not speak about "domestic emissions [...] to which your personal energy use contributes" in the explanatory text of the information screen (cp. Appendix 5 for a complete wording of the information screens highlighting differences).

2.2 Protocol and subject sample

The Internet experiment ran in two sessions in May and July 2010 over the course of 15 days in total. We used the infrastructure of a large Internet polling company (YouGov) to recruit and pay an Internet-representative[10] subject sample of 4,079 voting-aged Germans[11] and to administer our experiment. The recruitment of subjects followed the standard routine of the polling firm in which panel members are invited via an email message to proceed to the poll. The introductory screen then explained, as common with the pollster's regular surveys, the thematic focus of the poll (CO_2 emissions and climate change), the expected duration (ten minutes), and the payment (in form of a lottery with a prize worth up to a three-digit Euro figure).[12] Following the introductory screen, there was a filter screen to focus on German subjects and passing subjects were administered the information and decision screens of the two consecutive prize choices. Participants then faced another sequence of six to nine computer screens, depending on their decisions, that contained follow-up questions on climate change mitigation, EUAs and carbon offsets, and elicited subjects' sociodemographic characteristics. Answers revealed 139 subjects who either objected to the EU ETS as a proper method to reduce

[10] The sample was Internet-representative with respect to age, gender, and region of residence.

[11] 74% of Germans regarded themselves as citizens of the European Union at the time of the experiment (Eurobarometer 2012 Standard Eurobarometer 77 / Spring 2012).

[12] At the time of the experiment, the polling company usually incentivized panel members participating in polls through either a piece-rate reward of approximately €1 for 20 minutes expected survey time or random (lottery) prizes, e.g. in the form of shopping vouchers.

Table 2: Fraction of choices of emissions reduction

Choice	Experimental group			EU vs. DC	NE vs. EU	NE vs. DC
	1 EU-DC	2 DC-EU	3 NE-other			
1	20.1% EUA	19.4% CER	16.2% EUA	$p = 0.76$	$p = 0.01$	$p = 0.04$
2	30.1% CER	25.7% EUA	–	$p = 0.05$	–	–

Notes: p-values report two-sample Wilcoxon rank-sum tests on the distribution of choices.

emissions or said they distrusted the experiment itself and who are subsequently excluded from the analysis. The remaining sample of 3,940 subjects completed the experiment with a median completion time of five minutes.[13] After all responses were collected, winners were drawn and notified as described on the decision screens. Prior to the experiment, a set of pre-tests and a pilot experiment with 200 economics students at Heidelberg University helped testing and refining the online implementation and the wording of the instructions.

3 Results

3.1 Reduction within the EU vs. reduction in a developing country

Table 2 presents descriptive results on the choice of the emissions reduction in each choice and treatment. Comparing independent first choices, we observe that the fractions of choices in favor of the emission reduction are virtually identical at about 20% in both the EU and CD treatments ($p = 0.76$). Being offered a second choice increases decisions in favor of the reduction in both treatments. However, with repetition, a gap opens: Being offered a developing country CER instead of the EUA increases the propensity to choose the reduction by about 3.7 percentage points more than being offered an EUA instead of an CER. This leads to a somewhat significant difference in propensities in the second choice ($p = 0.05$) that is, however, not independent from what subjects saw in their first choice.

In order to disentangle the effects of treatment from those of repetition, individual ex-

[13] Mean completion time was 80 minutes. The difference between mean and median is largely driven by outliers who availed themselves of the opportunity to leave the survey and continue hours or days later.

perimental prices, and other potential covariates, we employ probit regressions in Table 3. Columns (1) and (2) confine the analysis to between-subjects comparisons based on first-round choices only. Controlling for experimental price as well as time and location fixed effects for the choices confirms the previous observation of no significant differences between treatments. Columns (3) to (5) then harness the doubled sample size from two choices per subject and employ the random-effects probit estimator to account for the panel structure of the data. From the increased power, we observe experimental controls for repetition and price to become highly significant determinants of the choice in column (3), with signs as expected. The specification in column (3) assumes the effects for repetition and price to be uniform across experimental groups. Thus, estimating out a uniform effect for repetition opens the possibility for the treatment variable to deliver, echoing the observed difference in choice 2 in Table 2. In contrast to the other experimental variables, the treatment variable becomes only marginally significant, however ($p = 0.08$). Nevertheless, it presents itself as a candidate for indicating a preference for the reduction in a developing country. An alternative explanation for the observed difference other than a treatment effect would be that the effect of being given a second choice is not uniform across the two experimental groups. In this case, the significant coefficient in column (3) would be an artifact of the *sequence* of choices, i.e. either being offered an EUA instead of a CER or vice versa, but not a genuine treatment effect. Likewise, the treatment effect might surface for average prices but differ in sign for particularly high or low prices.[14] In column (4), we therefore allow the effects for repetition and price to vary between experimental groups. Due to the interaction with repetition, the treatment effect naturally disappears. However, column (4) does not deliver a significant interaction of the repetition effect with experimental group to support the alternative explanation. Likewise, there is no music in the interaction between price and treatment. In column (5), we therefore include individual controls based on sociodemographic characteristics. The reason is that in all specification up to here, we have included time and location fixed effects at the resolution of experimental day, daytime, and region of residence. Plausibly, randomization may have been not perfect across experimental groups within

[14] This would be an effect paralleling the findings in Andreoni and Vesterlund (2001) for the relationship of price and altruism.

these cells. In result, although we return to assuming uniform coefficients for repetition and price across treatments, the DE treatment does no longer differ significantly from the EU treatment ($p = 0.33$). This finding calls for the inclusion of unobserved individual controls or, ideally, to exploit within-subject variation only and employ an individual fixed-effects estimator. In our sample, there are 522 subjects who switch their decision between the two choices. Unfortunately, there is no consistent and unbiased conditional fixed-effects probit estimator. However, the potential bias from using the unconditional fixed-effects probit model works in our favor since the bias is, if any, away from zero (Greene 2004). Despite this potential bias in favor of a significant estimate, column (6) renders the DC treatment indicator as insignificant ($p = 0.24$) while other experimental variables deliver again highly significant estimates ($p < 0.0005$). We take these results of Table 3 as evidence that despite the power of a sample with over 3,000 observed choices and between- as well as within-subject variation, the difference between the EU and DC treatments regarding the propensity to choose the emissions reduction is essentially zero.

3.2 A "neutral" vs. salient treatments

As pointed out earlier, the incomplete counterfactual design for the NE treatment (due to unrelated other treatments administered to the second choice of experimental group 3) restricts us to between-subjects comparisons of the first choices only. Table 2 suggests that the propensity to choose the emissions reduction in the EU and DC treatments significantly exceeds that in the NE treatment by about 3.9 and 3.2 percentage points, respectively. Probit regressions in column (1) of Table 4 confirm these findings. Controlling for experimental price and time-location fixed effects in column (2) yields coefficient estimates for the EU and DC treatments that translate into marginal effects of a 4.2 and 3.6 percentage points, respectively, higher propensity in favor of the emissions reduction compared to the NE treatment. Relaxing the assumption of a uniform price effect across treatments in column (3) suggests that the differences between treatments particularly manifest for high prices. While at the lowest price in the sample (€2), the marginal effects for the EU and DC treatment indicators are insignificant ($p = 0.47$ and $p = 0.65$, respectively), the estimates at the highest price (€100) are considerably larger (9.8 per-

Table 3: EU vs. DC

	Between-subjects		Between- and within-subjects			Within-subjects
	(1)	(2)	(3)	(4)	(5)	(6)
DC	-0.011	0.150	0.137*	-0.019	0.085	0.148
	(0.074)	(0.148)	(0.079)	(0.248)	(0.086)	(0.125)
Price	-0.001	0.001	-0.010***	-0.009***	-0.011***	-0.022***
	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)
DC × Price	–	-0.003	–	-0.001	–	–
		(0.003)		(0.003)		
Choice 2	–	–	0.526***	0.312*	0.550***	1.048***
			(0.083)	(0.176)	(0.091)	(0.128)
Choice 2 × DC	–	–	–	0.426	–	–
				(0.309)		
Time-loc. FE	Yes	Yes	Yes	Yes	Yes	–
Indiv. ctrls.	–	–	–	–	Yes	–
Indiv. FE	–	–	–	–	–	Yes
Constant	-0.949***	-1.032***	-1.360***	-1.262**	-2.601***	1.162
	(0.262)	(0.270)	(0.508)	(0.523)	(0.736)	(0.894)
N	1549	1549	3102	3102	2460	522
Log-likelihood	-760.968	-760.180	-1426.525	-1425.449	-1124.445	-267.188
χ^2	26.091	27.667	99.809	101.233	104.169	189.270
Pseudo R^2	0.017	0.018	–	–	–	0.262

Notes: Columns (1)-(2) report probit regressions of the choice of emission reduction in the first lottery. Columns (3)-(5) report random-effects probit regressions of both choices. Column (5) reports an unconditional fixed-effects probit model. "Time-location fixed effects" include indicator variables for location (Bundesland), day, and daytime (morning, afternoon, evening, night) of a subject's choice. "Individual controls" include sociodemographic variables of age, gender, years of education, income, and the number of children in the household. Standard errors are shown in parentheses.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

centage points with $p = 0.002$ for the EU, 5.2 percentage points with $p = 0.07$ for the DC) than at the mean price of €51.24 (4.2 percentage points with $p = 0.01$ for the EU, 3.7 percentage points with $p = 0.02$ for the DC).

Table 4: NE vs. EUA and CER

	(1)	(2)	(3)
EU	0.146**	0.162***	-0.097
	(0.060)	(0.061)	(0.126)
DC	0.124**	0.140**	0.048
	(0.060)	(0.061)	(0.117)
Price	–	-0.003***	-0.004***
		(0.001)	(0.001)
EU × Price	–	–	0.005**
			(0.002)
DC × Price	–	–	0.002
			(0.002)
Time-loc. FE	–	Yes	Yes
Constant	-0.985***	-0.866***	-0.796***
	(0.031)	(0.169)	(0.173)
N	3940	3852	3852
Log-likelihood	-1831.518	-1763.613	-1760.764
χ^2	8.131	78.912	84.611
Pseudo R^2	0.002	0.022	0.023

Notes: Probit regressions of the choice of emission reduction in the first lottery. "Time-location fixed effects" include indicator variables for location (Bundesland), day, and daytime (morning, afternoon, evening, night) of a subject's choice. Standard errors are shown in parentheses.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

4 Discussion

The core result of locational indifference among subjects in our paper differs from some, but by no means all previous evidence that such locational differences exist. This should not surprise, however: Evidence on multi-level public goods provision, for example, has found that locational group biases are less prevalent in the field than in the laboratory (Gallier et al. 2017, Buchan et al. 2009) and decline with the degree of globalization in the subjects' economy (Buchan et al. 2009), which is high in the EU.

There are also methodological differences between our approach and that of other current studies of locational preferences in the mitigation context. One is the choice of

experimental subjects: Baranzini et al. (2016) use a sample of around 300 students from the canton of Geneva in Switzerland and Buntaine and Prather (2017) use AMT subjects from different US states. Our study, on the other hand, is based on YouGov's Internet-representative sample from Germany, an EU member state. Another difference is that we compare the same amount of verfied mitigation, one metric ton, across individuals and randomly vary the cost of providing this mitigation in both locations. Baranzini et al. (2016) also choose verified mitigation activities, but design a setting in which the developing country is always more than three times as productive in verified mitigation activities as the alternative in a different Swiss canton. Buntaine and Prather (2017) design a donation task in which they do not control the amount of the verified mitigation productivities, while productivity information is provided in a separate treatment on a qualitative basis. This complicates in both cases the answer to the question whether subjects perceive the different mitigation locations as perfect substitutes since subjects are never offered the same amount of verified mitigation for the same price in two different locations.[15]

Differences in results can also be explained by other differences in the choice task. Our approach opts for the format of single-bounded dichotomous choice, similar to experiment 1 of Buntaine and Prather (2017), which minimizes well-known behavioral and cognitive biases. The design by Baranzini et al. (2016) employs a two-step choice in which subjects first decide on the share of their endowment to contribute to mitigation in general and in a second step choose between the two locations. Experiment 2 in Buntaine and Prather (2017) involves a continuous allocation task between three options. Two-step procedures will, almost by design, lead to differences in results from dichotomous choice tasks since the latter emphasizes the external margin of the decision which the two-step approach minimizes. This is even more pronounced in three options tasks, which are known not only to increase donations, but also lead to fuzzier results as subjects exhibit a tendency to avoid zero allocations (Cherry and Dickinson 2008,

[15]A conceivable complication of implementing the *ceteris paribus* condition across locations could be subjects' awareness of significant cost differentials, leading to field price censoring. However, subjects are unaware of the field price of mitigation activities both in general (Diederich and Goeschl 2017) and in a locations-specific context (Buntaine and Prather 2017, Baranzini et al. 2016). Diederich and Goeschl (2017) apply a battery of tests to detect field price censoring using the same subject pool and design and find no evidence.

Bernasconi et al. 2009).

A last point concerns robustness. Ex-post controls for observable socio-demographic and attitudinal characteristics of subjects are possible and can yield interesting insights.[16] To provide the basis for a more rigorous test of robustness, our design includes within-subjects evidence that allows us to control for unobserved variation. Both between- and within-subjects analyses return evidence of locational indifference, a consistency that strengthens the result.

5 Conclusion

The starting point of this paper was a simple question: "The climate system is indifferent as to the location of voluntary mitigation activities. Are people?" The motivation for this question are the significant economic ramifications of deviating from the least-cost geography of mitigation activities, dictated by comparative advantage, in the presence of locational preferences among those willing to pay for emissions reductions and existing evidence that such preferences may favor mitigation not to be carried out in far distant locations. Our artefactual field experiment with around 4,000 subjects varies, for residents of a European Union member state, whether the location of mitigation activities is made salient or not and whether the salient location is in the European Union or in a developing country, approximating the *ceteris paribus* condition within the constraints of a field setting.

Our headline findings are twofold: The decisions of our Internet-representative sample do not exhibit locational preferences between a mitigation site in the European Union and one in a developing country: There were no statistically significant differences between the EU and the DC treatment in subjects' propensity to choose CO2 mitigation over the cash prize. This result holds both in a between-subjects and a within-subjects analysis and is robust to the inclusion of numerous controls. There are also no significant differences in the price elasticity of the propensity to provide mitigation. Compared to the NE treatment, which introduced the location neutrally and with little salience, we

[16] Extensive additional econometric analysis on such covariates was carried out and is available on request.

find that being explicit about the location of where a subject's voluntary mitigation activities will be carried out made the average subject more likely to choose mitigation over the cash prize. Failing to observe locational preferences is therefore not driven by a lack of salience. Given our experimental control over the cost of mitigation for the individual, the verified uniformity of mitigation activities across EU and developing country sites, and the Internet-representative nature of our sample, we interpret this finding as evidence that across the population, locational preferences need not stand in the way of realizing the gains from comparative advantage in climate change mitigation.

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Appendix

Table 5: Information screens EU and DC treatments

A. EU treatment:

In the following, we will inform you about one of the two lotteries. You may choose between two different awards. **For this lottery,** these are:

XY Euro in bonus points

or

the reduction of carbon (CO_2) emissions by 1 ton **within the European Union**

How does the reduction of the CO_2 emissions work **in this lottery**? We will use a reliable opportunity provided by the EU emissions trading system: We will purchase and delete an *EU emissions allowance* for you. Power plants and other large industrial installations need emissions allowances within the EU in order to be entitled to emit CO_2. Since there is only a fixed overall number of allowances in place, deleted ones are no longer available to cover emissions. **Domestic** emissions in Germany and **the** other EU countries**, to which your personal energy use contributes,** will decrease by exactly one ton from one deleted allowance.

Because of the way in which CO_2 mixes in the air, it does not matter for the effect on the climate where on the globe CO_2 emissions are reduced. What counts is only total emissions worldwide.

In the lotteries, 100 winners will be randomly selected out of about 5,000 participants.

B. DC treatment:

In the following, we will inform you about one of the two lotteries. You may choose between two different awards. For this lottery, these are:

XY Euro in points

or

the reduction of carbon (CO_2) emissions by 1 ton **through an emissions reduction project in a developing country**

How does the reduction of the CO_2 emissions work in this lottery? We will use a reliable opportunity. **We will support a certified local *emissions reduction project in a developing country*** for you, **e.g., for biogas, wind power, or energy efficiency. This will abate 1 ton of CO_2 in the developing county. We will only choose projects with the highest possible certification mark: the CDM Gold Standard. Such projects have to be carried out such that they benefit the local population (e.g., by hiring local employees) und in a particularly environmentally friendly manner.**

Because of the way in which CO_2 mixes in the air, it does not matter for the effect on the climate where on the globe CO_2 emissions are reduced. What counts is only total emissions worldwide.

In the lotteries, 100 winners will be randomly selected out of about 5,000 participants.

Notes: Highlighted in bold are differences in phrasings between treatments. In the EU treatment, we highlight differences to the NE treatment. In the DC treatment, we highlight differences to the EU treatment.

Table 6: Information screen NE treatment

NE treatment:

In each of the two lotteries, you may choose between two different awards. These are:

A cash award in points
or
the reduction of carbon (CO_2) emissions by 1 ton

How does the reduction of the CO_2 emissions work? We will use a reliable opportunity provided by the EU emissions trading system: We will purchase and delete an *EU emissions allowance* for you. Power plants and other large industrial installations need emissions allowances within the EU in order to be entitled to emit CO_2. Since there is only a fixed overall number of allowances in place, deleted ones are no longer available to cover emissions.

Emissions in Germany and other EU countries will decrease by exactly one ton from one deleted allowance.

Because of the way in which CO_2 mixes in the air, it does not matter for the effect on the climate where on the globe CO_2 emissions are reduced. What counts is only total emissions worldwide.

In the lotteries, 100 winners will be randomly selected out of about 5,000 participants. The following two lotteries may differ in the awards offered as well as in the payoff procedures.

Notes: Highlighted in bold are differences in phrasing to the EU treatment.

Figure 1: *Information screen* of the EU treatment

Figure 2: *Decision screen*, EU treatment

Figure 3: *Information screen* of the DC treatment

Figure 4: *Decision screen*, DC treatment

Figure 5: *Information screen* of the NE treatment

Figure 6: *Decision screen*, NE treatment

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