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Outgroup Bias and the Unacceptability of Tax Fraud*

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Abstract

In countries with well-developed welfare state systems, it is often claimed that racial or ethnic minorities impose a heavy burden on social assistance programs without contributing to public goods. In this study, we consider the attitudinal effects of anecdotal reports of tax cheating by minorities. We conduct survey experiments in France and the United States to assess if people react more harshly to tax fraud perpetrated by members of a minority rather than the majority group. We find no evidence that minority status affects judgments and perceptions about tax fraud, including among those on the right end of the political spectrum. Tax fraud is considered unacceptable regardless of the culprit's origin.

Keywords: outgroup bias; tax fraud; survey experiment; null effects

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In countries with well-developed welfare state systems, it is often claimed that minorities impose a heavy burden on social assistance programs without contributing to public goods. (Gilens 1995; 1996; Harell et al. 2016).[1] Previous research on biases in attitudes toward public finance focuses on the spending- rather than the revenue-side of the fiscal equation. Yet, biases run both ways. We know that people are more willing to fund public spending that benefits their own group (Belmonte et al. 2018; Nemore and Morone 2019; Xin Li 2010). We also know that intergroup animus colors views about welfare spending and welfare fraud (Gilens 1995; 1996; Harell et al. 2016). So, when it comes to tax fraud, it follows that culprits' minority or majority status should color views about taxation.

In this research note, we assess whether individuals react more negatively to anecdotal reports of tax fraud featuring minorities. We field cross-national surveys in the France and the United States, both of which have large minority populations: Arabs in France and Hispanics in the United States. We design a series of vignettes describing common tax fraud schemes, each featuring a small business owner with randomly manipulated minority versus majority status.

Surprisingly, our results show no evidence of outgroup bias in citizens' reactions to tax fraud. We also find no evidence that people's political predispositions moderate our treatment effects. These null results have important implications at the intersection of intergroup relations and fiscal policy. Specifically, they suggest that racial and ethnic biases do not influence judgements and perceptions of tax noncompliance. This is likely because people place more weight on the illicit nature of tax fraud than on the identity of its perpetrators.

[1] For example, France's Marine Le Pen campaigned on the promise of ending social benefits for foreigners: "I've got nothing against foreigners but I say to them: if you come to our country, don't expect that you will be taken care of…" (Agence France-Presse 2016). Similarly, during his 2016 campaign, Donald Trump emphasized the "tremendous costs" of immigration on the United States, pledging to remove undocumented immigrants "relying on public welfare or straining the safety net" (The New York Times 2016).

Background

Taxation is a fertile area for intergroup conflict, because it brings together two sets of reinforcing phenomena, the first socio-psychological and the second economic.

First, socio-psychological theories of intergroup conflict see tensions and discrimination between social groups as the result of deeply entrenched tendencies to favor one's own group and to hold negative attitudes toward outgroups (Sherif 1966; Tajfel et al. 1971; see Böhm et al. 2020 for a review). This has far-reaching implications for public finance in diverse societies and how citizens evaluate government policy to reduce poverty and redistribute income (Gilens 1995; 1996). Notably, research on welfare chauvinism finds that prejudice towards minorities reduces support for social assistance programs and increases support for limiting these programs to native citizens (Harell et al. 2016; Stichnoth and Straeten 2013; Van der Meer and Tolsma 2014).

Second, economic theories of intergroup conflict see prejudice through the lens of competition over scarce resources between social groups. A case in point is immigration. (see Hainmueller and Hopkins 2014 for a review). The tax burden hypothesis posits that opposition to immigration is driven by concerns pertaining to fiscal policy, with natives fearing that newcomers – particularly those with low levels of formal education – might increase demand for welfare spending without contributing much in taxes (Facchini and Mayda 2009; Hanson et al. 2007).

Attitudes about taxation are shaped by outgroup biases to a great extent. Residents of diverse countries express more dissatisfaction toward taxation than those from more homogeneous countries (Xin Li 2010). Similarly, Belmonte et al. (2018) show that individual dislike of diversity has negative consequences on tax compliance, particularly among residents of heterogeneous countries.

These findings are corroborated by Nemore and Morone (2019), who establish an association between anti-immigrant attitudes and willingness to pay taxes. Nemore and Morone (2019, 12) note that there is "widespread concern that immigrants are making increasing use of public assistance programs" exacerbating dissatisfaction toward taxation among natives. In short, immigration and diversity might accrue fiscal concerns, weakening citizens' support for the tax system.

We expect citizens to react more harshly to tax fraud committed by out-group members. We know that prejudice shapes evaluations of offenses or transgressions committed my members of a minority group: individuals generally tend to be more lenient when members of the majority group commit the exact same acts (Hartman et al. 2014). We expect this relationship to be moderated by people's political predispositions: tax fraudsters' minority status should result in more negative reactions among conservatives. This is because conservatism shapes views on taxation and tax compliance (Lozza et al. 2013; Nemore and Morone 2019) and predicts racial and ethnic prejudice and anti-immigration sentiment (Hainmueller and Hopkins 2014), which in turn worsen dissatisfaction with the tax system (Belmonte et al. 2018; Nemore and Morone 2019).

Survey Experiment

We field pre-registered online survey experiments in France and the United States.[2] Our 4,000 respondents (2,000 per country) are adults recruited to meet population quotas for age, gender, education, and region. We present our full survey instrument the Appendix.

[2]Data collection took place between July 24 and August 9, 2020. The survey was administered via the Qualtrics platform, and participants were recruited by Dynata. The pre-analysis plan was submitted to the OSF on July 20, 2020. An anonymized copy can be found in the Appendix.

France and the United States both have large minority populations stemming from immigration. In France, Arab Muslims represent approximately a tenth of the population (Sahgal and Mohamed 2019). In the United States, Hispanics account for nearly a fifth of the population (Krogstad and Noe-Bustamante 2020). Arabs and Hispanics are the subject of stereotypes, both cultural (e.g., they fail to assimilate) and economic (e.g., they have unskilled jobs).

Our experiment consists of three vignettes featuring small business owners who commit tax fraud. We randomly manipulate their name to signal minority status.[3] In France, the control is a French name, and the treatment is a Maghrebi Arab name. In the United States, the control condition is an Anglo-Saxon name, and the treatment condition is a Hispanic name. The American vignettes are as follows:[4]

> *Car dealership.* [Harry Johnson/Enrique Gómez] owns an auto repair shop. For his daughter's 20th birthday, [Harry/Enrique] buys her a used car. He then tells the Internal Revenue Service (IRS) that the cost of this car is a business expense to save on taxes.
>
> *Fast food.* [Steven Jenkins/Esteban Jiménez] is the owner of a fast food restaurant. He hires a part-time employee to help wash the dishes, but he pays him in cash, "under the table," to avoid paying payroll taxes.
>
> *Roofing.* [Peter Williamson/Pedro Villaseñor] is the owner of a roofing company. He charges $4000 to repair the roof of a customer's house, but he offers a $500 discount if the customer pays in cash. If the customer agrees, [Peter/Pedro] will not report the cash payment to the Internal Revenue Service (IRS).

[3] Names can signal not only race and ethnicity (Butler and Homola 2017) but also socioeconomic traits such as income and education (Crabtree et al. 2022; Landgrave and Weller 2021). Information equivalency is violated when names unintentionally influence perceptions about socioeconomic background rather than just racial or ethnic identity – in such cases, the effect estimates do not correspond to the intended treatment and are thus biased. In our case, our use of distinctive names is coupled with explicit mentions of occupational skill levels; by design, our vignettes randomize minority status while keeping socioeconomic characteristics constant.

[4] The French vignettes are presented in the Appendix.

The order in which these vignettes were shown was random, but respondents could be assigned to two treatment conditions at most. This randomization process prevented a situation in which some respondents would have been presented with illicit scenarios involving only minorities.

> *Acceptability.* How acceptable or unacceptable is this behavior? 0 means completely unacceptable, and 10 means completely acceptable.
>
> *Prevalence.* What percentage of people in the United States would do the same thing as [NAME] if they were in his place?

These questions are commonly used in work on attitudes toward tax fraud (Horodnic 2018). The first is a measure of *injunctive norms* surrounding tax fraud – what others should do (Hallsworth et al. 2014). The second question accounts for *descriptive norms* of tax compliance – what others actually do (Hallsworth et al. 2014). Both dependent variables range from 0 to 1 in our analysis.

We measure respondents' political predispositions using a left-right self-placement scale in France and a threefold party identification scale in the United States. In our analysis, the moderator variable ranges from 0 (left-wing/Democrat) to 1 (right-wing/Republican).

Empirical Analysis

Reactions to the tax crimes featured in our experiment were quite negative. Figure 1 shows the distribution of our two dependent variables. Across all vignettes, the mean of the acceptability outcome is low (0.32 in France and 0.39 in the United States); in each country, around a quarter of respondents said that tax fraud was "completely unacceptable" (0). For the prevalence outcome, again across all vignettes, the mean is 0.52 in France and 0.58 in the Unites States.

To ascertain the causal effect of our experimental manipulation, we calculate the difference in means between the treatment and control groups separately by outcome, vignette and country. The resulting 12 average treatment effect (ATE) estimates along with 90% and 95% confidence intervals are plotted in Figure 2 (see the Appendix for balance and regression tables). Nearly all ATEs are indistinguishable from zero. The only statistically significant ($p < 0.05$) ATE is very small, which is most likely due to luck. Assuming that any estimate smaller than $|0.05|$ is substantially negligible, it follows that all possible negligible effects should be greater than -0.05 and smaller than 0.05 (Rainey 2014). The two vertical, dotted lines in Figure 2 plot these bounds. Since no value contained by the 90% confidence intervals falls outside this region of negligible effects, it is possible to reject the null hypothesis of a meaningful effect with an α level of 0.05.[5]

To determine if the treatment effects are moderated by ideological (in France) or partisan (in the United States) predispositions, we estimate 12 models with multiplicative interactions (see the Appendix for regression tables). Figure 3 shows that partisanship and ideology do not condition the name treatment effects in any meaningful way for all scenarios, outcomes, and countries.

We fail to reject both null hypotheses: tax fraudsters' minority status does not affect respondents' evaluations of the acceptability and prevalence of tax fraud schemes; moreover, these treatment effects are not conditioned by respondents' political predispositions.[6]

[5] In the Appendix, we report null hypothesis tests with standard errors clustered by respondent and p-values corrected for multiple comparisons following Hochberg (1988). These tests yield more conservative conclusions than the IID standard errors, reinforcing our conclusion of a null effect.

[6] Many quantitative studies in political science are likely underpowered (Arel-Bundock et al. 2022). Although our sample was relatively large (2000 respondents per country), we acknowledge that it is possible that our null results were the result of low power. This is especially true for our moderation analyses, where more information is always needed. In the Appendix, we report the results of a retrospective exploration of the power of our tests under different assumptions following the DeclareDesign framework (Blair et al. 2019). This exploration suggests that future tests with similar characteristics should have a good chance of detecting effects if they are sizeable.

Figure 1: Distribution of the two dependent variables in the three vignettes and two countries.

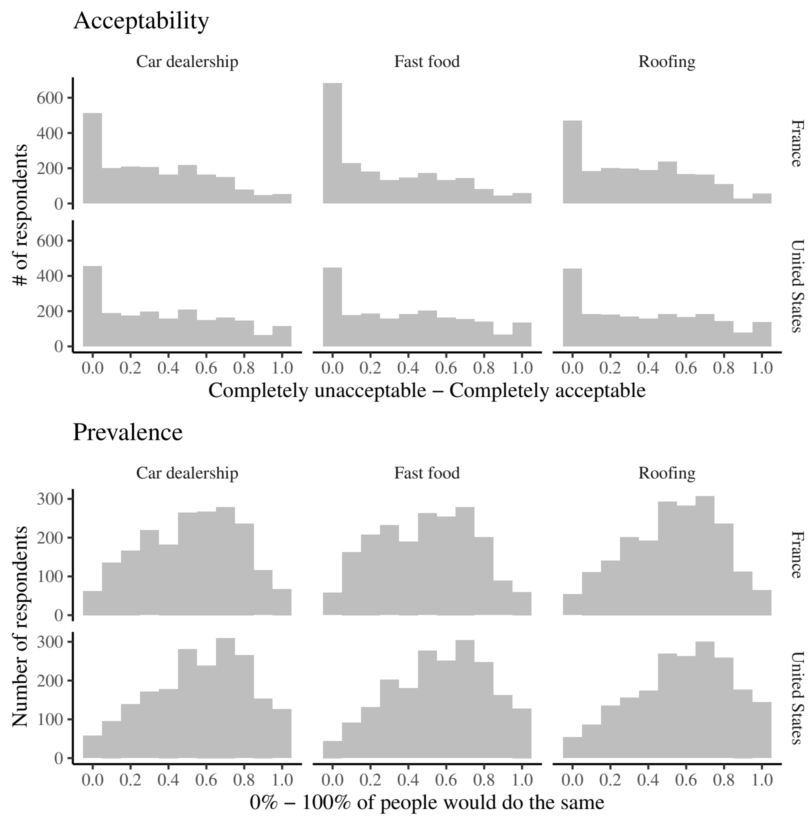


Figure 2: Average treatment effects. Thick lines are 90% confidence intervals, and thin lines are 95% confidence intervals. Dotted lines bound the region of negligible effects. All variables range from 0 to 1.

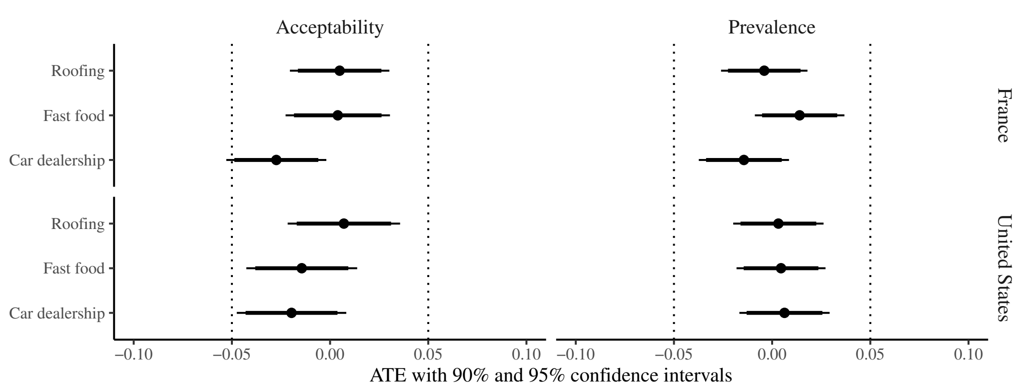
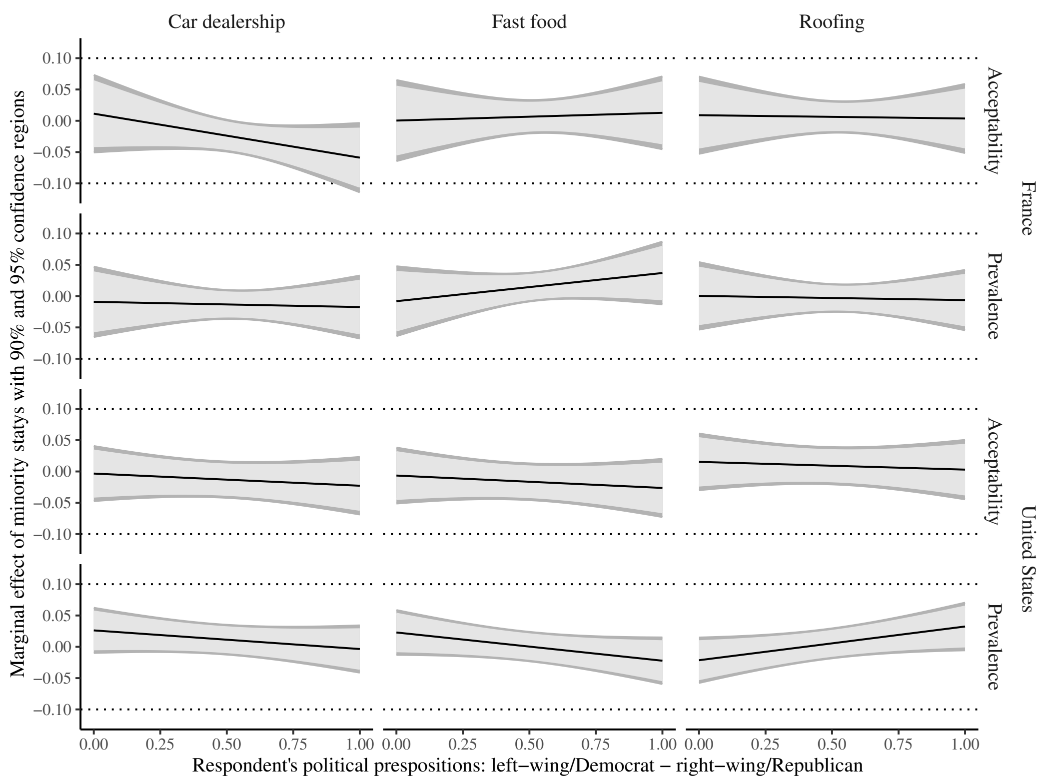


Figure 3: Average treatment effects conditional on left-right self-placement (France) or party identification (United States). The horizontal axis is the moderator variable, which ranges from 0 (left-wing/Democrat) to 1 (right-wing/Republican). The vertical axis is the marginal effect of minority status on evaluations of acceptability and prevalence. Light grey areas are 90% confidence regions, and dark grey areas are 95% confidence regions. Dotted lines bound the region of negligible effects. All variables range from 0 to 1.



Discussion

We administered survey experiments in France and the United States to test two hypotheses: first, that people react more negatively to tax fraud committed by minorities; and second, that this treatment is stronger among conservatives. We do not find empirical support for either hypothesis.

One might assume that public tolerance for tax fraud is higher than for other crimes (Kaplan and Reckers 1985) since it leads to fewer social and legal consequences (Alldridge 2017; De La Feria 2020). Given the strength of prejudice on other areas of public finance (e.g., welfare), one might expect outgroup biases to also shape attitudes related to tax fraud, which should be frowned upon when perpetrated by the minorities but tolerated among members of the majority group. Yet, we find no evidence of this: levels of disapproval are consistently high regardless of the fraudster's group membership. Judgements and perceptions about tax fraud may be changing, likely because of the financial crisis' impact on public views about taxation (Christensen and Hearson 2019).

We see three explanations for our results. One possibility is that our French and American respondents are not biased against Maghrebis and Hispanics. This conclusion seems implausible since it goes against vast bodies of work on prejudice and biases (Böhm et al. 2020; Hainmueller and Hopkins 2014; Stichnoth and Straeten 2013; Van der Meer and Tolsma 2014).

A different explanation is that our name treatments were too weak. While this is certainly possible, we note that the full names of the tax fraudsters appeared at the very beginning of our vignettes, leaving no ambiguity at all about their Hispanic or Maghrebi backgrounds. Individuals can easily infer minority status from just names (Butler and Homola 2017; Crabtree et al. 2022; Landgrave and Weller 2021).

A more plausible interpretation is that outgroup biases are outweighed by the negative nature of the tax fraud behavior itself. While these biases are often very strong when it comes to redistribution or welfare (Harell et al. 2016; Gilens 1995; 1996), they may be weakened by concerns about the viability of the tax system. The sense of unfairness arising from our illicit scenarios may have trumped prejudice, the tax fraud scenarios themselves stirring overwhelmingly negative reactions regardless of the origin of the culprit. This suggest that attitudes towards tax fraud are more negative than previous research on white-collar crime would indicate, and that outgroup biases may not be as pervasive as prior research has found – a noteworthy null result.

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Outgroup Bias and the Unacceptability of Tax Fraud

Supporting Information

A Survey Instrument

A.1 United States

What is your year of birth? [Dropdown list: 1910-2010]

What is your gender [Female; Male; Other]

Generally speaking, do you usually think of yourself as a. . . ? [Democrat; Republican; Independent; Other; DK]

In which state do you currently live? [Dropdown list: 50 American States + Washington, D.C.]

How many years of schooling have you completed? [Dropdown list: 0-24]

–

We will now ask your opinion about three illegal business transactions

[Harry Johnson / Enrique Gómez] owns an auto repair shop. For his daughter's 20th birthday, [Harry / Enrique] buys her a used car. He then tells the Internal Revenue Service (IRS) that the cost of this car is a business expense in order to save on taxes.

1. How acceptable or unacceptable is this behavior? 0 means completely UNACCEPTABLE and 10 means completely ACCEPTABLE. [Slider: 0-10]
2. What percentage of people in the United States would do the same thing as [Harry / Enrique] if they were in his place? [Slider: 0-100]

[Steven Jenkins / Esteban Jiménez] is the owner of a fast food restaurant. He hires a part-time employee to help wash the dishes, but he pays him in cash, "under the table," to avoid paying payroll taxes.

1. How acceptable or unacceptable is this behavior? 0 means completely UNACCEPTABLE and 10 means completely ACCEPTABLE. [Slider: 0-10]
2. What percentage of people in the United States would do the same thing as [Steven / Esteban] if they were in his place? [Slider: 0-100]

[Peter Williamson / Pedro Villaseñor] is the owner of a roofing company. He charges $4000 to repair the roof of a customer's house, but he offers a $500 discount if the customer pays in cash. If the customer agrees, [Peter / Pedro] will not report the cash payment to the Internal Revenue Service (IRS).

1. How acceptable or unacceptable is this behavior? 0 means completely UNACCEPTABLE and 10 means completely ACCEPTABLE. [Slider: 0-10]
2. What percentage of people in the United States would do the same thing as [Peter / Pedro] if they were in his place? [Slider: 0-100]

A.2 France

Quelle est votre année de naissance? [Dropdown list: 1910-2010]

Êtes-vous. . . [Un homme; Une femme; Autre]

Dans quelle région habitez-vous présentement? [Dropdown list: 13 Metropolitan French Regions]

Combien d'années de scolarité avez-vous complétées? [Dropdown list: 0-24]

En politique, on parle souvent de la gauche et de la droite. Où vous situez-vous sur cette échelle? [Slider: 0-10]

–

Nous allons maintenant vous demander votre opinion au sujet de trois transactions commerciales illégales.

[Henri Moressée / Haroun Massoud] est propriétaire d'un atelier de réparation automobile. Pour le 20e anniversaire de sa fille, Monsieur [Moressée / Massoud] lui achète une voiture usagée. Il déclare illégalement cet achat comme une dépense de son entreprise afin de payer moins d'impôts.

1. À quel point ce comportement est-il acceptable ou inacceptable? 0 signifie complètement inacceptable et 10 signifie complètement acceptable. [Slider: 0-10]
2. Quel pourcentage des gens en France feraient la même chose que Monsieur [Moressée / Massoud] s'ils étaient à sa place? [Slider: 0-100]

[André Vincent / Anouar Yousfi] est propriétaire d'un fast-food. Il embauche un plongeur à temps partiel pour laver la vaisselle, mais le paie en espèces, « sous la table », pour éviter de payer la taxe sur les salaires.

1. À quel point ce comportement est-il acceptable ou inacceptable? 0 signifie complètement inacceptable et 10 signifie complètement acceptable. [Slider: 0-10]
2. Quel pourcentage des gens en France feraient la même chose que Monsieur [Vincent / Yousfi] s'ils étaient à sa place? [Slider: 0-100]

[Médéric Damiens / Mehdi Dahmani] est propriétaire d'une entreprise de toiture. Il demande 4000€ pour réparer le toit d'une maison, mais il offre un rabais de 500€ si le client paie en argent comptant. Si le client accepte, Monsieur [Damiens / Dahmani] ne rapportera pas le paiement comptant à la Direction générale des Finances publiques (DGFiP).

1. À quel point ce comportement est-il acceptable ou inacceptable? 0 signifie complètement inacceptable et 10 signifie complètement acceptable. [Slider: 0-10]
2. Quel pourcentage des gens en France feraient la même chose que Monsieur [Damiens / Dahmani] s'ils étaient à sa place? [Slider: 0-100]

B Balance Tables

B.1 United States

Table 1: Car dealership

		Control (N=1142)		Treatment (N=875)			
		Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. Error
Acceptability (0-1)		0.39	0.32	0.37	0.31	-0.02	0.01
Prevalence (0-1)		0.57	0.26	0.57	0.26	0.01	0.01
Age		48.39	17.96	48.66	17.74	0.26	0.80
Education (years)		13.32	4.89	13.16	4.90	-0.17	0.22
		N	Pct.	N	Pct.		
Gender	Female	566	49.6	439	50.2		
	Male	571	50.0	436	49.8		
	Other	5	0.4	0	0.0		
Census Region	Midwest	228	20.0	182	20.8		
	Northeast	202	17.7	174	19.9		
	South	446	39.1	313	35.8		
	West	266	23.3	206	23.5		

Table 2: Fast food

		Control (N=1143)		Treatment (N=874)			
		Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. Error
Acceptability (0-1)		0.40	0.32	0.38	0.32	-0.01	0.01
Prevalence (0-1)		0.57	0.26	0.58	0.25	0.00	0.01
Age		48.15	17.80	48.98	17.93	0.82	0.80
Education (years)		13.34	4.84	13.14	4.96	-0.20	0.22
		N	Pct.	N	Pct.		
Gender	Female	583	51.0	422	48.3		
	Male	559	48.9	448	51.3		
	Other	1	0.1	4	0.5		
Census Region	Midwest	228	19.9	182	20.8		
	Northeast	214	18.7	162	18.5		
	South	420	36.7	339	38.8		
	West	281	24.6	191	21.9		

Table 3: Roofing

		Control (N=1146)		Treatment (N=871)			
		Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. Error
Acceptability (0-1)		0.40	0.33	0.40	0.32	0.01	0.01
Prevalence (0-1)		0.58	0.26	0.59	0.26	0.00	0.01
Age		48.30	17.55	48.79	18.27	0.50	0.81
Education (years)		13.12	4.94	13.42	4.84	0.29	0.22
		N	Pct.	N	Pct.		
Gender	Female	573	50.0	432	49.6		
	Male	570	49.7	437	50.2		
	Other	3	0.3	2	0.2		
Census Region	Midwest	232	20.2	178	20.4		
	Northeast	208	18.2	168	19.3		
	South	449	39.2	310	35.6		
	West	257	22.4	215	24.7		

B.2 France

Table 4: Car dealership

		Control (N=1139)		Treatment (N=859)			
		Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. Error
Acceptability (0-1)		0.34	0.29	0.31	0.29	-0.03	0.01
Prevalence (0-1)		0.53	0.26	0.52	0.26	-0.01	0.01
Age		48.51	16.94	48.54	16.97	0.03	0.77
Education (years)		13.17	5.59	13.20	5.71	0.03	0.26
		N	Pct.	N	Pct.		
Gender	Female	547	48.0	437	50.9		
	Male	592	52.0	421	49.0		
	Other	0	0.0	1	0.1		
Region	Auvergne-Rhône-Alpes	142	12.5	113	13.2		
	Bourgogne-Franche-Comté	46	4.0	43	5.0		
	Bretagne	58	5.1	47	5.5		
	Centre-Val de Loire	46	4.0	38	4.4		
	Corse	8	0.7	4	0.5		
	Grand Est	114	10.0	68	7.9		
	Hauts-de-France	109	9.6	75	8.7		
	Île-de-France	210	18.4	154	17.9		
	Normandie	57	5.0	38	4.4		
	Nouvelle-Aquitaine	88	7.7	80	9.3		
	Occitanie	105	9.2	75	8.7		
	Pays de la Loire	60	5.3	54	6.3		
	Provence-Alpes-Côte d'Azur	96	8.4	70	8.1		

Table 5: Fast Food

		Control (N=1129)		Treatment (N=869)			
		Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. Error
Acceptability (0-1)		0.29	0.30	0.29	0.30	0.00	0.01
Prevalence (0-1)		0.49	0.26	0.51	0.25	0.01	0.01
Age		48.30	16.89	48.80	17.03	0.50	0.77
Education (years)		13.04	5.73	13.36	5.53	0.32	0.25
		N	Pct.	N	Pct.		
Gender	Female	538	47.7	446	51.3		
	Male	590	52.3	423	48.7		
	Other	1	0.1	0	0.0		
Region	Auvergne-Rhône-Alpes	143	12.7	112	12.9		
	Bourgogne-Franche-Comté	51	4.5	38	4.4		
	Bretagne	61	5.4	44	5.1		
	Centre-Val de Loire	44	3.9	40	4.6		
	Corse	6	0.5	6	0.7		
	Grand Est	100	8.9	82	9.4		
	Hauts-de-France	102	9.0	82	9.4		
	Île-de-France	212	18.8	152	17.5		
	Normandie	46	4.1	49	5.6		
	Nouvelle-Aquitaine	90	8.0	78	9.0		
	Occitanie	104	9.2	76	8.7		
	Pays de la Loire	74	6.6	40	4.6		
	Provence-Alpes-Côte d'Azur	96	8.5	70	8.1		

Table 6: Roofing

		Control (N=1130)		Treatment (N=868)			
		Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. Error
Acceptability (0-1)		0.34	0.28	0.35	0.29	0.00	0.01
Prevalence (0-1)		0.54	0.25	0.54	0.25	0.00	0.01
Age		48.65	16.73	48.35	17.23	-0.30	0.77
Education (years)		13.31	5.61	13.01	5.69	-0.30	0.26
		N	Pct.	N	Pct.		
Gender	Female	557	49.3	427	49.2		
	Male	572	50.6	441	50.8		
	Other	1	0.1	0	0.0		
Region	Auvergne-Rhône-Alpes	145	12.8	110	12.7		
	Bourgogne-Franche-Comté	50	4.4	39	4.5		
	Bretagne	61	5.4	44	5.1		
	Centre-Val de Loire	59	5.2	25	2.9		
	Corse	7	0.6	5	0.6		
	Grand Est	100	8.8	82	9.4		
	Hauts-de-France	98	8.7	86	9.9		
	Île-de-France	180	15.9	184	21.2		
	Normandie	53	4.7	42	4.8		
	Nouvelle-Aquitaine	101	8.9	67	7.7		
	Occitanie	101	8.9	79	9.1		
	Pays de la Loire	64	5.7	50	5.8		
	Provence-Alpes-Côte d'Azur	111	9.8	55	6.3		

C Regression Tables

C.1 United States

Table 7: Average Treatment Effects

	Acceptability			Prevalence		
	Car dealership	Fast Food	Roofing	Car dealership	Fast Food	Roofing
(Intercept)	0.392 (0.009)	0.399 (0.009)	0.398 (0.010)	0.568 (0.008)	0.571 (0.008)	0.583 (0.008)
Minority	−0.020 (0.014)	−0.014 (0.014)	0.007 (0.015)	0.006 (0.012)	0.005 (0.012)	0.003 (0.012)
Num.Obs.	2017	2017	2017	2017	2017	2017
R2	0.001	0.000	0.000	0.000	0.000	0.000

Table 8: Conditional Average Treatment Effects

	Acceptability			Prevalence		
	Car dealership	Fast Food	Roofing	Car dealership	Fast Food	Roofing
(Intercept)	0.374 (0.015)	0.392 (0.015)	0.384 (0.015)	0.560 (0.012)	0.552 (0.012)	0.583 (0.012)
Minority	−0.003 (0.022)	−0.007 (0.023)	0.015 (0.023)	0.026 (0.018)	0.023 (0.018)	−0.021 (0.018)
Republican	0.021 (0.023)	0.007 (0.023)	0.018 (0.023)	0.009 (0.019)	0.037 (0.019)	−0.002 (0.019)
Minority × Republican	−0.019 (0.035)	−0.020 (0.036)	−0.012 (0.036)	−0.030 (0.029)	−0.045 (0.028)	0.054 (0.029)
Num.Obs.	1887	1887	1887	1887	1887	1887
R2	0.001	0.001	0.001	0.001	0.002	0.003

C.2 France

Table 9: Average Treatment Effects

	Acceptability			Prevalence		
	Car dealership	Fast food	Roofing	Car dealership	Fast food	Roofing
(Intercept)	0.341 (0.009)	0.291 (0.009)	0.345 (0.009)	0.531 (0.008)	0.492 (0.008)	0.541 (0.007)
Minority	−0.027 (0.013)	0.004 (0.014)	0.005 (0.013)	−0.014 (0.012)	0.014 (0.012)	−0.004 (0.011)
Num.Obs.	1998	1998	1998	1998	1998	1998
R2	0.002	0.000	0.000	0.001	0.001	0.000

Table 10: Conditional Average Treatment Effects

	Acceptability			Prevalence		
	Car dealership	Fast food	Roofing	Car dealership	Fast food	Roofing
(Intercept)	0.216 (0.021)	0.202 (0.022)	0.244 (0.021)	0.455 (0.019)	0.446 (0.019)	0.474 (0.018)
Minority	0.011 (0.032)	0.000 (0.033)	0.009 (0.031)	−0.009 (0.029)	−0.008 (0.028)	0.000 (0.027)
Right-wing	0.233 (0.035)	0.163 (0.037)	0.188 (0.035)	0.140 (0.032)	0.086 (0.032)	0.125 (0.031)
Minority × Right	−0.070 (0.054)	0.012 (0.056)	−0.005 (0.054)	−0.008 (0.049)	0.045 (0.049)	−0.007 (0.047)
Num.Obs.	1998	1998	1998	1998	1998	1998
R2	0.031	0.018	0.024	0.016	0.011	0.014

D Power Analysis

One potential concern is that our experiments were not well-powered enough to detect substantively meaningful effect sizes. We did not conduct a power analysis before running the experiment, and it is well-known that post-hoc power analyses based on the observed sample and effect sizes are of limited utility. However, as suggested by a reviewer, we can still conduct a power analysis while keeping a hypothetical/prospective mindset. In that context, drawing power curves might help us imagine what power similar experiments would have in the future. The hypothetical effect sizes and the noise characteristics that we consider are described transparently in the `R` code below.

```
library(broom)
library(ggplot2)
library(DeclareDesign)

designer <- function(effect_size) {
  declare_model(
    N = 2000,
    U = truncnorm::rtruncnorm(n = N, a = 0, b = 1, mean = .5, sd = .3),
    potential_outcomes(Y ~ effect_size * Z + U)) +
    declare_inquiry(ATE = mean(Y_Z_1 - Y_Z_0)) +
    declare_assignment(Z = complete_ra(N, prob = 0.433)) +
    declare_measurement(latent = reveal_outcomes(Y ~ Z), Y = pmin(latent, 1)) +
    declare_estimator(Y ~ Z, inquiry = "ATE")
}
designs <- expand_design(designer, effect_size = seq(.01, .1, length.out = 25))
D <- diagnose_designs(designs, bootstrap_sims = 1)
datplot <- subset(tidy(D), diagnosand == "power")

ggplot(datplot, aes(effect_size, estimate)) +
  geom_line() +
  labs(x = "Hypothetical effect size", y = "Power")
```

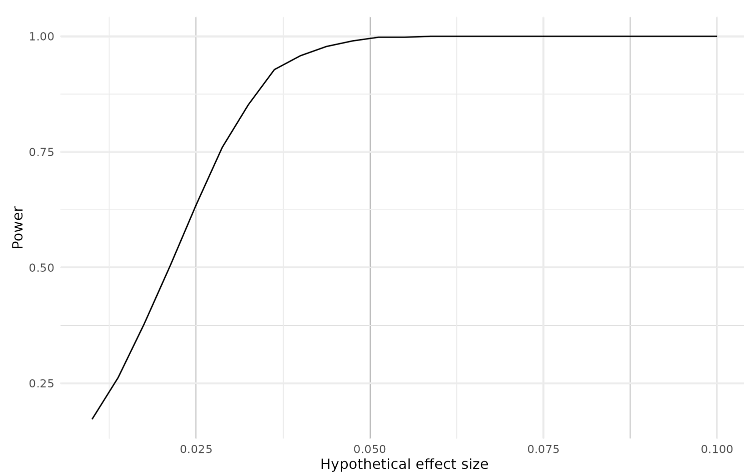

Figure D.1: Power curve to detect an average treatment effect in a model without interaction

```r
fit <- function(data) {
  mod <- lm(Y ~ Z * M, data = data)
  out <- tidy(mod)
  out <- subset(out, term == "Z:M")
  return(out)
}

designer <- function(main, moderator) {
  declare_model(
    N = 2000,
    M = runif(N, min = 0, max = 1),
    Z = rbinom(N, size = 1, prob = .433),
    U = truncnorm::rtruncnorm(n = N, a = 0, b = 1, mean = .5, sd = .3),
    Y = pmin(main * Z + moderator * M * Z + U, 1)) +
    declare_estimator(handler = label_estimator(fit)) +
    declare_inquiry(moderator = moderator)
}

designs <- expand_design(
  designer,
  main = seq(.01, .2, length.out = 4),
  moderator = seq(.01, .2, length.out = 10))

D <- diagnose_designs(designs, bootstrap_sims = 1)

datplot <- subset(tidy(D), diagnosand == "power")

ggplot(datplot, aes(moderator, estimate)) +
  geom_line() +
  facet_wrap(~main) +
  labs(x = "Hypothetical effect size", y = "Power")
```

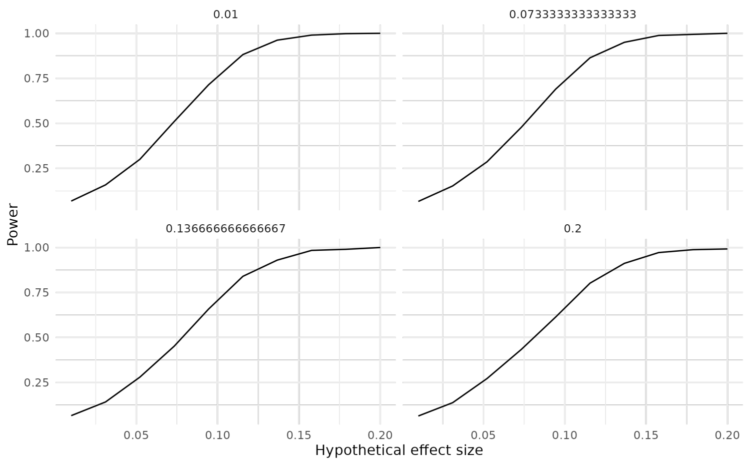

Figure D.2: Power curve to detect a moderation effect in a model with an interaction

E Multiplicity Adjustment and Clustered Standard Errors

Table 11: p values from IID standard errors, standard errors clustered by respondent, and standard errors corrected for multiple comparisons following Hochberg (1988).

Country	Vignette	Outcome	IID	Clustered	MultComp
France	Car dealership	Acceptability	0.04	0.04	0.42
France	Car dealership	Prevalence	0.22	0.22	0.79
France	Fast food	Acceptability	0.77	0.77	0.79
France	Fast food	Prevalence	0.23	0.23	0.79
France	Roofing	Acceptability	0.71	0.71	0.79
France	Roofing	Prevalence	0.72	0.72	0.79
United States	Car dealership	Acceptability	0.17	0.17	0.79
United States	Car dealership	Prevalence	0.59	0.59	0.79
United States	Fast food	Acceptability	0.32	0.32	0.79
United States	Fast food	Prevalence	0.70	0.69	0.79
United States	Roofing	Acceptability	0.63	0.63	0.79
United States	Roofing	Prevalence	0.79	0.79	0.79

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G Pre-Analysis Plan

A copy of the pre-analysis plan is appended to this document.

Preregistration report for a study on tax morale, fraud and ethnicity

Survey Data

Online surveys will be fielded in France and the United States. Participants will be recruited by the market research firm Dynata. Data collection will be done via the Qualtrics platform.

Experimental Manipulation

We conduct three survey experiments to measure analogous causal quantities. Respondents read a series of three vignettes, each of which describes a different small business owner who engages in a tax fraud scheme. In each scenario, we experimentally manipulate the name of the individual: in the control condition, he has an anglo-saxon name, whereas in the treatment his name is Hispanic. (In the French translation, the business transactions are the same, but we use French- versus Maghrebi-sounding names.)

In the survey, we introduce the three scenarios with this sentence:

> We will now ask your opinion about three illegal business transactions.

Transaction 1:

> [Peter Williamson / Pedro Villaseñor] is the owner of a roofing company. He charges $4000 to repair the roof of a customer's house, but he offers a $500 discount if the customer pays in cash. If the customer agrees, [Peter / Pedro] will not report the cash payment to the Internal Revenue Service (IRS).

Transaction 2:

> [Steven Jenkins / Esteban Jiménez] is the owner of a fast food restaurant. He hires a part-time employee to help wash the dishes, but he pays him in cash, "under the table," to avoid paying payroll taxes.

Transaction 3:

> [Harry Johnson / Enrique Gómez] owns an auto repair shop. For his daughter's 20th birthday, [Harry / Enrique] buys her a used car. He then tells the Internal Revenue Service (IRS) that the cost of this car is a business expense in order to save on taxes.

The order in which these vignettes are shown is randomized, but respondents can be assigned to at most two treatment conditions.

After each vignette, respondents answer two questions, which we will use as our outcome variables:

1. How acceptable or unacceptable is this behavior? 0 means completely unacceptable, and 10 means completely acceptable.
2. What percentage of people in the United States would do the same thing as [NAME] if they were in his place?

Estimands

Average treatment effects. We will calculate the differences in means between treatment and control groups for each outcome variable, in each vignette, in France and the United States. This will produce 12 ATE estimates along with heteroskedasticity-consistent standard errors. Using this information, we will test the null hypothesis that the name of offenders does not have an effect on respondents' evaluations of the acceptability or prevalence of tax fraud.

Heterogenous treatment effects. To determine if the treatment effect is moderated by partisan or ideological considerations, we estimate 12 linear regression models with multiplicative interactions. In the United States, the moderator is a dummy variable equal to 1 for self-identified Republicans, 0 for independent, and -1 for Democrats. In France, the moderator is a left-right self-placement scale from 0 to 10. In these tests, the quantity of interest is the interaction coefficient, which indicates whether the association between treatment and outcome varies by partisanship/ideology ("don't know" amd "other" are excluded from this analysis).