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Do people donate more when they perceive a single beneficiary whom they know? A field experimental test of the identifiability effect

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ABSTRACT

According to the identifiability effect, people will donate more to a single beneficiary rather than to many beneficiaries, holding constant what the donations are actually used for. We test the identifiability effect for two novel subject pools (the suppliers and beneficiaries of volunteer labor). We also test a refinement of the identifiability effect where we vary whether or not the single beneficiary is personally known to the solicitees. While the behavior of volunteers is consistent with the identifiability effect, we find that the identifiability effect is reversed for beneficiaries of volunteer labor. Moreover, we find that making the single beneficiary personally known to the solicitees lowers donations by a statistically insignificant amount, suggesting that it does not enhance donations.

1. Introduction

Fundraising drives are ubiquitous in the not-for-profit world—individual contributions in the United States totaled roughly $218 billion in 2012 (Giving USA, 2013). They provide critical financial support to organizations that might not exist otherwise, and almost every non-profit depends on a degree of active solicitation from potential donors who might not have given otherwise. This seemingly altruistic behavior has drawn considerable attention from academics interested in understanding why people give (Radley and Kennedy, 1995; Bekkers and Wiepking, 2011; List and Price, 2012; Oppenheimer and Olivola, 2011). There is a rich literature on how the psychology of charitable giving is related as much to the context of the ask itself, as it is by the ultimate effect of the donation.

An important contextual factor is the number of beneficiaries perceived by a potential donor when solicited for charitable contributions: a beneficiary could be presented as a large number of people, such as "help feed orphans," or as a single person, e.g., "help feed Alex, who is an orphan." This phenomenon has been termed the "identifiability effect" (Schelling, 1968), and holds true even when the actual use of the donations is equivalent, as well as the information about the cause conveyed by the single/multiple targets (Small and Lowenstein, 2003). Many mechanisms have been proposed—vividness, empathy, social distance, and goal attainability have all been demonstrated to play a role in lab settings (Oppenheimer and Olivola, 2011).

The present study was designed to contrast these different accounts of the identifiability effect among populations with pre-existing relationships to the beneficiaries. Specifically, we wanted to measure the marginal effect of one's personal connection to the cause, above and beyond the identifiability effect, since personal connections have been shown to increase donations (Small and Simohnson, 2008). Our dataset allows a unique opportunity to contrast these hypotheses in the same study, and among two populations that have distinct personal relationships to the cause. This special issue contains a complementary study by Eckel, Herberich and Meer (2015), which examines the effect of giving solicitees the option of directed giving (donating to the specific college one attended at one's alma mater), compared to a default of undirected giving (donating to the general fund at one's alma mater).

We report the results from two field experiments that test these two different forms of the identifiability effect. A non-profit that trains volunteers to give free financial advice to its clients carried out a fundraising campaign where it solicited for financial donations from both sets of stakeholders: its clients (the recipients of the free financial advice advice) and its volunteers (the dispensers of the free financial advice). Each group would also serve as the other group's "target" when invoking the identifiability effect, i.e.,

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clients would be told that their donations are helping individual volunteers, and vice versa. Since the two groups directly interacted with each other as a result of the non-profit's activities, members of each group knew members of the other group personally.

Both clients and volunteers were randomly assigned to one of three treatments, varying only the target of the message. In the *control* treatments, the entire non-profit was the target. In the other two treatments (*known* and *unknown*), in line with the literature on the identifiability effect, individual names were used as examples—volunteers heard an actual client's name in the ask, while clients heard an actual volunteer's name. In the *unknown* treatments, the name was unknown to the person being asked. In contrast in the *known* treatments, the script used the name of a specific volunteer (client) that actually helped (was helped by) the person being asked.

This design allows us to compare different accounts of the identifiability effect. The control and unknown conditions replicate previous studies that use anonymous targets and donors, albeit with a nominally novel subject pool (the clients and volunteers in a non-profit organization, each of which has a distinct relationship to the non-profit). This constitutes a test of what we refer to as the *orthodox* identifiability effect. The unknown and known conditions can test the marginal effect of whether a face-to-face relationship helped an identified target, compared to an anonymous target. We refer to this as a test of the *extended* identifiability effect.

Regarding the orthodox identifiability effect, we find that the volunteer data are consistent with the literature, but the client data support the reverse, i.e., solicitees donate less and with a lower probability when there is a single beneficiary. We also find that our data directionally (though not statistically) reject the extended identifiability effect: solicitees donate smaller amounts and with lower probability when a single beneficiary is someone with whom they have personally interacted.

Our results contribute to the existing literature on identifiability in several ways. First, the orthodox version of the identifiability effect fails a simple robustness test: extending the subject pool to the clients of a non-profit where the target is the non-profit's service provider. This suggests that the theory underlying the identifiability effect may need to be refined. Second, our results suggest that one logical extension of the orthodox identifiability effect—the proposition that personally knowing the single beneficiary of a solicitation increases solicitee donations—is unsupported by the data. Consequently, as far as enhancing charitable contributions is concerned, the extended identifiability effect is a comparatively fruitless avenue of further investigation.

2. Background: social distance and charity

There is widespread recognition in the literature about the importance of social distance to charitable contributions. For example, scholars have found that people are more willing to assist acquaintances than strangers (Nolan and Spanos, 1989; DePaulo and Kashy, 1998; Ma, 1992). Beyond this, Small and Simonsohn (2008) have shown that people in the lab and the field give more to health charities (cancer, heart disease, etc.) if they have a family member who has been afflicted; likewise, with sexual assault victims (Christy and Voight, 1994). A similar mechanism has been proposed for donations from "hot lists" of those who have already given—that is, the personal connection can build empathy for the cause (Landry et al., 2010). In another paper studying this very same firm, we show that long-term volunteers give more, and respond more positively to future solicitations than recent joiners (Yeomans and Al-Ubaydli, 2015).

Many of the same mechanisms that explain the effects of social distance are also used to explain the identifiability effect, (Small and Loewenstein, 2003). Identifiable targets evokes more empathy (Kogut and Ritov, 2005), and goals are easier to perceive, psychologically, with single beneficiaries (Slovic, 2007). This comparison is often drawn explicitly (Small, 2010)—it is as though the clarity of the mental image is a proxy for distance, as though the beneficiaries were watched from afar on a foggy day. Thus, we predicted that if the beneficiary in question was actually known to the person being solicited, that mental image might be even clearer, and have more of an effect. Formally, we propose an extended identifiability effect whereby a unique target known to the solicitee should result in the highest donations of all.

An additional determinant of social distance is the nature of the interaction between the solicitor and solicitee, controlling for the history of interactions between the two individuals. One factor in particular is the degree of anonymity that the solicitation technique affords the solicitee, which has been demonstrated to have an effect on observed charity (Burnham, 2003; Charness and Gneezy, 2008). Thus, for example, subjects playing the dictator game donate smaller amounts when they are guaranteed 100% anonymity compared to when the experimenter is aware of their donation (Hoffman et al., 1996).

In anticipation of our experimental design, two solicitation methods are telephone solicitations, which are synchronous, and voicemail solicitations, which are asynchronous. Given the greater degree of anonymity that we would associate with the asynchronous voicemail solicitations, we would expect larger donations when solicitations are made over the telephone rather than over voicemail. Beyond the social distance forces underlying this prediction, by inducing an immediate verbal response, phone calls may create more commitment to giving (Cialdini, 1993), whereas the effect of voicemails might be diluted by procrastination (O'Donoghue and Rabin, 1999). Accordingly, both orthodox and extended identifiability effects should be accentuated when conditioning on phone rather than voicemail solicitations.

3. Experimental design

3.1. Environment

The following draws heavily from Yeomans and Al-Ubaydli (2015). The US tax code is complex system and preparing taxes is an arduous annual task for many US citizens. In fact many less-educated citizens fail to file their taxes due to their inability to comprehend the tax code. The income tax system's progressivity means that many of the non-filers are people who are actually due money back from the Federal Government, and their failure to file means that they forgo these often sizeable sums of money.

The Center for Economic Progress (CEP) is a Chicago-based non-profit organization that provides free tax preparation assistance to low-income families and individuals trying to navigate the abstruse US Income Tax Code. Each year, it trains approximately 1500 volunteers to assist the Center's clients. For the volunteers to perform their duties, the CEP sets up centers throughout the greater Chicago area equipped with computers. Clients then bring their tax materials to their local center where a volunteer guides them through the filing process, which usually takes one-to-two hours. The volunteering season runs in tandem with the tax filing season: January until the middle of April.

In 2011, the CEP helped prepare 28,134 tax returns ($< \$25{,}000$ annual income per single filer; $< \$50{,}000$ per couple) at 29 locations across Illinois. However, due to state budget cuts, they shrank to 15 locations for the 2012 season, and decided to conduct an active fundraising drive for the first time among their clients and volunteers. The CEP were keen to maximize the yield on their drive, and to refine their fundraising techniques (Baber et al., 2001; Al-Ubaydli and Lee, 2011). We advised them on how to conduct a nat-

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ural field experiment to meet their goals. The logistical details (execution, data collection) were all handled internally by the CEP.

3.2. Procedure: clients

Clients were solicited immediately after their tax preparation was complete—they parted from their assigned volunteer preparer and headed to a common administrative table before leaving. The person manning that table would make sure to answer any administrative questions, and reinforce information about how to submit tax returns. During the experiment conducted in the 2012 season, they also delivered a short verbal solicitation using a prepared script.

The content of that script was manipulated between the three treatment conditions. Across all conditions, the script briefly described the non-profit's financial needs, and described how donations might be targeted to stem reductions in service provision. In the *generic* condition, the target of the donation was the broader non-profit.

As you know we are a non-profit, and we want you to consider making a small donation to support our cause. There have been large statewide budget cuts that have forced us to cut back our services, preventing CEP from helping more people like you. Even a small donation will make it possible for us to reach others who need our services.

In the *unknown* condition, the target was the name of an actual volunteer from that specific tax site, though not one that was working that particular day.

As you know we are a non-profit, and we want you to consider making a small donation to support our cause. There have been large statewide budget cuts that have forced us to cut back our services, preventing volunteers like [volunteer name] at our [location] tax site from helping more people like you. Even a small donation will train more volunteers like [volunteer's first name] to reach others who need our services.

In the *known* condition, the target was the name of the volunteer that had just helped the client prepare their tax returns.

As you know we are a non-profit, and we want you to consider making a small donation to support our cause. There have been large statewide budget cuts that have forced us to cut back our services, preventing volunteers like [client's volunteer's name] at our [location] tax site from helping more people like you. Even a small donation will train more volunteers like [volunteer's first name] to reach others who need our services.

In all cases it was made clear that the money itself went to the non-profit, to control for preferences about how that money is actually spent, rather than the persuasiveness of an appeal to the needs of each of the three targets.

After the script was read, clients were offered two ways to donate. They were given an envelope addressed to CEP, which clients could use to mail in a check at any time. A special donation-by-text number was created, too, so that clients could also send an SMS to the number and have their donation charged to their phone bill at the end of the month. Either way, the clients were left with all the necessary information and materials to make a donation when they left the site.

Ideally, The CEP would have randomized the solicitation treatment at the client level. However logistical constraints meant that they could only randomize at the site level. The agents carrying out the solicitation, who were operating a considerable geographical distance from centralized CEP staff, were unaware that they were administering an experiment. Asking them to modify their solicitation spiel without a discernible reason would have risked exposing the experiment to the solicitees, thereby undermining the benefits associated with the solicitees being unaware of their participation in an experiment. These benefits are particularly acute when one studies matters that pertain to social preferences, including charitable contributions (Levitt and List, 2007). The left panel of Table 1 details the site-level randomization.

Table 1
Allocation of clients and volunteers to treatment by tax site.

Clients			Site	Volunteers		
Generic	Unknown	Known		Generic	Unknown	Known
1181	0	0	Auburngresham	30	29	33
0	628	0	Aurora	31	27	28
0	15	0	ChicagoITIN	0	0	0
0	1211	0	Elgin	39	38	39
1025	0	0	Harvey	20	22	17
0	0	694	Joiliet	25	15	22
657	0	0	Lawndale	19	21	29
0	0	2971	Loop	131	161	161
0	0	188	Mobileteam	0	0	0
941	0	0	Pilsen	32	32	26
0	0	968	Quadcomm	24	32	24
603	0	0	Springfield	19	11	15
0	3411	0	Uptown	127	105	120
0	0	744	Waukegan	16	24	16
0	0	0	Wesleyn	0	0	1
4407	5265	5565	Total	513	517	531

Clients were randomized at the site level, volunteers were randomized at the individual level.

3.3. Procedure: volunteers

The departure point was a database containing all the volunteers as of January 18, 2012. The CEP knew in advance that it would later have access to a range of demographic information about each volunteer drawn from a survey during training, but due to limited administrative resources, this information was not available at the stage at which volunteers would be allocated to treatments.

Solicitations were conducted over the phone. To ensure that the solicitations were done in a professional fashion, the CEP hired professionals from a temporary employment agency. The solicitors were all blind to the experiment. Solicitors were instructed to call each volunteer up to three times. After listening to the script, a volunteer who expressed interest in donating would be asked if he/she would prefer to receive the written donation instructions by email or by mail (our dataset is based on received rather than promised donations). If the volunteer could not be reached after the third call, then the solicitor would leave a voice message.

The volunteer solicitation script was manipulated along the same lines as the client solicitation script—a short message detailed the financial problems at CEP. This was followed by a suggestion of who might benefit from the donation, which varied between subjects. In the *generic* condition, the target of the donation was the broader non-profit.

As you know we are a non-profit, and we want you to consider making a small donation to support our cause. There have been large statewide budget cuts that have forced us to cut back our services, preventing volunteers like you from helping more CEP clients in need. Even a small donation will make it possible for us to reach others who need our services.

In the *unknown* condition, the script referred to a client from that volunteer's tax site that came on a different day.

As you know we are a non-profit, and we want you to consider making a small donation to support our cause. There have been large statewide budget cuts that have forced us to cut back our services, preventing volunteers like you from helping more people like [client name] from our [location] tax site. Even a small donation will train more volunteers to reach others, like [client's first name], who need our services.

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In the *known* condition, the script referred to a client for whom the volunteer had done a tax prep session (except those solicited immediately after training and before they had had a chance to start work properly; in that case, it was a name from the training).

As you know we are a non-profit, and we want you to consider making a small donation to support our cause. There have been large statewide budget cuts that have forced us to cut back our services, preventing volunteers like you from helping more people like [client name] from our [location] tax site. Even a small donation will train more volunteers to reach others, like [client's first name], who need our services.

After the script was read, if the volunteer agreed to make a donation, they were able to request a follow-up either through the post or through e-mail. This follow-up, much like the package given to clients, explained both the text message and check-in-the-mail donation methods.

Unlike clients solicitations, volunteer solicitations were done remotely from the CEP head office, and they were conducted by a small number of professionals under the supervision of CEP staff. This allowed for randomization at the volunteer level. The right panel of Table 1 details the randomization with respect to the sites, confirming the balance.

3.4. Predictions

Our first prediction is based on the orthodox identifiability effect.

Prediction 1. Solicitees will donate larger amounts and with higher probability when there is a single beneficiary (*known, unknown* treatments) than when there are many beneficiaries (*generic* treatment).

Our experiment examines the orthodox identifiability effect applied to a novel subject pool: the suppliers and beneficiaries of volunteer labor. This is a straightforward replication/robustness exercise. While there is no ex ante reason to expect either group's behavior to differ from that of other groups studied in the literature or from each other, we explore the possibility.

Our second prediction is based on the as-yet-untested extended identifiability effect.

Prediction 2. When there is a single beneficiary (*known, unknown* treatments), solicitees will donate larger amounts and with higher probability when the beneficiary is someone with whom they have personally interacted (*known* treatment) than when it is a stranger (*unknown* treatment).

Our third prediction examines phone vs. voicemail solicitations for volunteers.

Prediction 3. If either the orthodox or extended identifiability effect are detected for volunteers, the effect will be stronger when conditioning on volunteers who were solicited by phone than those who were solicited by voicemail.

The identifiability effect treatments are exogenous with respect to whether or not the volunteer was reached by phone, implying that there are no sample selection effects when conditioning on being solicited by phone. Regardless, anticipating our statistical analysis, it is worth noting the absence of a particularly strong relationship between whether a volunteer was solicited by phone and variables that common sense would suggest might have an effect, such as employment status.

4. Results

Collecting data on clients was very systematic because it was linked to the completion of a tax return, and so a full array of demographic controls are available. In contrast, demographic data collection on the volunteers was not as rigorous as volunteers had the option of skipping questions in the survey. Consequently, we have a full array of demographics for only 1348 (80%) of the 1691 volunteers solicited. We focus on the results that include demographic controls; unconditional analysis allows for a larger sample but does not substantively alter the results, and so we relegate it to appendix tables.

Table 2
Descriptive statistics for clients and volunteers.

Variable	Mean (SD)	
	Clients ($n = 15{,}237$)	Volunteers ($n = 1348$)
Donation ($)	0.15 (2.4)	1.4 (13)
Donation dummy	0.0074 (0.086)	0.021 (0.14)
Phone solicited dummy	–	0.45 (0.50)
Male dummy	0.37 (0.48)	0.39 (0.49)
Married dummy	–	0.32 (0.47)
Age	44 (16)	–
Children dummy	–	0.11 (0.31)
Family size	1.8 (1.2)	–
Asian dummy	0.050 (0.22)	–
Black dummy	0.49 (0.50)	0.23 (0.42)
Latino dummy	0.27 (0.44)	0.16 (0.37)
White dummy	0.16 (0.37)	0.39 (0.49)
Employed dummy	–	0.66 (0.48)
Education: HS or less dummy	–	0.048 (0.21)
Education: some college dummy	–	0.27 (0.44)
Education: bachelor's dummy	–	0.68 (0.46)
Tax refund	1600 (2200)	–
Returning volunteer	–	0.37 (0.48)

All numbers are displayed to two significant figures.

4.1. Descriptive statistics

Table 2 contains the main descriptive statistics. While some of the demographic variables are common to both clients and volunteers, there are also quite a few that are unique to either group.

Mean donations were $0.15 for the clients and $1.40 for the volunteers. Clients donated 0.74% of the time while volunteers donated 2.1% of the time. These disparities are most easily explained by the fact that clients were almost de facto from the poorest components of society. Beyond this, the volunteers are people with a demonstrated history (present) of charitable contributions—specifically time donations—and donating time is correlated with donating money (Duncan, 1999).

4.2. Main results

We investigate the predictions using two dependent variables: *donation*, which is the $ donation and takes weakly positive values (with "0″ being the mode by a huge margin), and a *donation dummy*, which takes the value "1″ when the donation is strictly positive. Since clients, which comprise the bulk of the data, were randomized at the site level and there were only 14 sites with client data, it is preferable for inference to be based upon parametric models with controls rather than the usual suite of nonparametric unconditional tests.

Thus, in the models where *donation* is the dependent variable, we use a tobit regression with site-level clustered standard errors, and in the models where *donation dummy* is the dependent variable, we use probit regressions with site-level clustered standard errors. Table 3 contains the unconditional means of the dependent variables of interest by treatment (and site in the case of clients), and Table 4 contains the results of the parametric models including controls.

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Table 3
Donations by treatment for clients (by tax site) and volunteers.

Subject group	Site	Sample size	Mean donation (SD), % donating		
			Generic	Unknown	Known
Clients	Aub.	1181	$0.61 (6.1), 2.1%	–	–
	Aur.	628	–	$0.016 (0.40), 0.16%	–
	Chi.	15	–	$0 (0), 0%	–
	Elg.	1211	–	$0.066 (0.91), 0.58%	–
	Har.	1025	$0.095 (3.0), 0.098%	–	–
	Joi.	694	–	–	$0.27 (2.6), 1.2%
	Law.	657	$0.24 (2.3), 1.2%	–	–
	Loo.	2971	–	–	$0.0067 (0.26), 0.067%
	Mob.	188	–	–	$0 (0), 0%
	Pil.	941	$0.27 (2.1), 1.9%	–	–
	Qua.	968	–	–	$0.18 (2.0), 1.3%
	Spr.	603	$0.066 (0.81), 0.66%	–	–
	Upt.	3411	–	$0.14 (1.9), 0.73%	–
	Wau.	744	–	–	$0.013 (0.37), 0.13%
	Total		$0.29 (3.7), 1.3%	$0.11 (1.6), 0.63%	$0.071 (1.3), 0.43%
			n= 4407	n = 5265	n = 5565
Volunteers	Total		$0.56 (5.8), 1.4% n = 560	$1.6 (16), 2.3% n = 570	$1.2 (10), 1.6% n = 561

Means, SDs and percentages are displayed to two significant figures.

Table 4
Regression results for clients and volunteers.

Model	1	2	3	4	5	6	7	8	9	10	11	12
Type	Tobit	Probit	Tobit	Probit	Tobit	Tobit	Probit	Probit	Tobit	Tobit	Probit	Probit
Dep. var.	Donation	Donation dummy	Donation	Donation dummy	Donation	Donation	Donation dummy	Donation dummy	Donation	Donation	Donation dummy	Donation dummy
Group	Clients	Clients	Clients	Clients	Vols.	Vols.	Vols.	Vols.	Vols.	Vols.	Vols.	Vols.
Condition	–	–	–	–	–	Phone	–	Phone	–	Phone	–	Phone
Generic dummy			2.3*	0.0057*					−1.4	−1.4	−0.0050	−0.013
			(1.2)	(0.0034)					(1.7)	(2.1)	(0.0056)	(0.015)
Unknown dummy	−1.6**	−0.0030*			2.4	2.3	0.011	0.024				
	(0.80)	(0.0017)			(1.9)	(2.3)	(0.0099)	(0.026)				
Known dummy	−2.9	−0.0054			0.30	0.68	0.00,088	0.0059				
	(1.8)	(0.0027)			(1.4)	(2.0)	(0.0051)	(0.018)				
Phone dummy					5.9***		0.025***		5.8***		0.025***	
					(1.2)		(0.0066)		(1.3)		(0.0067)	
Male dummy					4.0***	3.9***	0.017***	0.038***	4.0***	4.0***	0.017***	0.039***
					(0.85)	(0.78)	(0.0044)	(0.0062)	(0.88)	(0.82)	(0.0048)	(0.0069)
Married Dummy					1.3	2.6**	0.0049	0.026**	1.2	2.6**	0.0048	0.027**
					(1.0)	(1.1)	(0.0048)	(0.015)	(1.0)	(1.1)	(0.0049)	(0.015)
Age	0.11***	2.2e-4***	0.10***	2.2e−4***								
	(0.024)	(7.5e−5)	(0.026)	(7.6e−5)								
Asian dummy	−4.4***	−4.9e−3***	-4.3***	−4.9e−3***								
	(0.56)	(1.2e−3)	(0.59)	(1.3e−3)								
Black dummy	−0.58	−0.0011	−0.98	−0.0020	−0.66	−0.91	−0.0026	−0.0081	−0.41	−0.71	−0.0017	−0.0063
	(1.4)	(0.0023)	(1.5)	(0.0032)	(2.7)	(2.6)	(0.0084)	(0.020)	(2.7)	(2.5)	(0.0090)	(0.020)
Latino dummy	0.80	0.0019	0.60	0.0014	−0.082	1.1	0.00,085	0.014	0.069	1.2	0.0014	0.016
	(1.6)	(0.0038)	(1.7)	(0.0039)	(2.05)	(2.0)	(0.0077)	(0.024)	(2.1)	(2.1)	(0.0086)	(0.026)
White dummy	−0.62	−0.0011	−0.73	−0.0013	0.22	−0.036	0.00,026	−0.0015	0.30	0.035	0.00,070	−0.00,052
	(1.3)	(0.0023)	(1.3)	(0.0024)	(0.99)	(1.2)	(0.0036	(0.011)	(1.1)	(1.3)	(0.0042)	(0.011)
Employed dummy					−0.55	−1.7*	−0.0022	−0.017*	−0.58	−1.7*	−0.0025	−0.017*
					(1.1)	(1.0)	(0.0039)	(0.010)	(1.0)	(0.98)	(0.0038)	(0.0098)
Tax refund	3.3e−4**	6.9e−7***	3.3e−4**	6.9e−7***								
	(1.4e−4)	(2.6e−7)	(1.3e−4)	(2.7e−7)								
Returning volunteer					4.1***	3.7**	0.016**	0.034*	4.1***	3.7**	0.017**	0.035*
					(1.5)	(1.7)	(0.0085)	(0.020)	(1.5)	(1.7)	(0.0089)	(0.022)
# Obs.	15,237	15,237	15,237	15,237	1348	605	1187	533	1348	605	1187	533
Psuedo R^2	0.041	0.054	0.039	0.052	0.12	0.091	0.17	0.13	0.12	0.088	0.16	0.12

Standard errors in parentheses. Estimated coefficients, standard errors, pseudo R^2 are displayed to two significant figures. For parsimony, educational control variables are omitted from the table despite being included in the regressions. Asterices denote statistical significance:
* = 10%.
** = 5%.
*** = 1%.

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Table A1
Unconditional regressions results for clients and volunteers.

Model	1	2	3	4	5	6	7	8	9	10	11	12
Type	Tobit	Probit	Tobit	Probit	Tobit	Tobit	Probit	Probit	Tobit	Tobit	Probit	Probit
Dep. var.	Donation	Donation dummy	Donation	Donation dummy	Donation	Donation	Donation dummy	Donation dummy	Donation	Donation	Donation dummy	Donation dummy
Group	Clients	Clients	Clients	Clients	Vols.	Vols.	Vols.	Vols.	Vols.	Vols.	Vols.	Vols.
Condition	–	–	–	–	–	Phone	–	Phone	–	Phone	–	Phone
Generic dummy			2.5**	0.0074*					−1.2	−0.45	−0.0054	−0.0035
			(1.3)	(0.0044)					(1.9)	(2.2)	(0.0092)	(0.020)
Unknown dummy	−2.0**	−0.0046*			1.8	1.5	0.0016	0.015				
	(0.98)	(0.0026)			(2.1)	(2.4)	(0.0087)	(0.027)				
Known dummy	−3.0	−0.0067			0.53	−0.62	0.0098	−0.0070				
	(1.9)	(0.0034)			(1.7)	(1.9)	(0.0087)	(0.017)				
# Obs.	15,237	15,237	15,237	15,237	1691	757	1561	702	1691	757	1561	702
Psuedo R^2	0.013	0.018	0.012	0.016	0.0029	0.0047	0.0048	0.0086	0.0015	0.00,020	0.0019	0.00,030

Standard errors in parentheses. Estimated coefficients, standard errors, pseudo R^2 are displayed to two significant figures.
Asterices denote statistical significance:
* = 10%.
** = 5%.

Result 1a. The orthodox identifiability effect is absent in the client data, i.e., solicitees donate smaller amounts, and with lower probability, when there is a single beneficiary (*known, unknown* treatments) compared to when there are many beneficiaries (*generic* treatment). Moreover, there is some evidence that the identifiability effect is reversed.

From Table 3, clients solicited with the generic script donated an average of $0.29 per client, and they donated 1.3% of the time. The corresponding figures for the unknown treatment were $0.11 and 0.63% respectively, while for the known treatment they were $0.071 and 0.43% respectively. Thus, ignoring covariates, the data do not support the identifiability effect (in fact they support its obverse).

In Model 1 in Table 4, which is a tobit with controls where the dependent variable is *donation*, we see that compared to the baseline of the generic many beneficiary script, being solicited according to the unknown single beneficiary script leads to a statistically significantly lower donation ($p < 0.05$). The effect of the known single beneficiary script is even larger in terms of the point estimate, though it does not attain conventional statistical significance. (See Table A1 for the same regressions above and below without controls; the conclusions are unaffected.)

In Model 2 in Table 4, which is a probit with controls where the dependent variable is *donation dummy* (and the reported coefficients are marginal effects at the sample mean), we see that compared to the baseline, the *Unknown* script yields a 0.30% lower chance of donating ($p < 0.1$), while the *Known* script yields a 0.54% lower chance of donating.

In Model 3 and Model 4, we rerun the tobit and probit with one difference: we indirectly pool *Unknown* and *Known* scripts by making their pooled data the baseline and including a dummy for the *Generic* script. In both cases, the coefficient on *Generic* is positive and marginally significant ($p < 0.1$). Thus if the data suggest anything at all, it is that there is *no* identifiability effect for clients.

Result 1b. The orthodox identifiability effect receives some support in the volunteer data, i.e., solicitees donate more and with higher probability when there is a single beneficiary (*known, unknown* treatments) compared to when there are many beneficiaries (*generic* treatment). However the differences are not statistically significant.

From Table 3, volunteers solicited with the generic script donated an average of $0.56 per client, and they donated 1.4% of the time. The corresponding figures for the unknown treatment were $1.6 and 2.3% respectively, while for the known treatment they were $1.2 and 1.6%, respectively. Thus, ignoring covariates and setting aside statistical significance, the data directionally support the identifiability effect.

In Model 5 in Table 4, which is a tobit with controls where the dependent variable is *donation*, we see that compared to the baseline of the generic many beneficiary script, being solicited according to the unknown single beneficiary script leads to a larger donation, as does being solicited according to the known single beneficiary script. However neither attains conventional statistical significance.

In Model 7 in Table 4, which is a probit with controls where the dependent variable is *donation dummy* (and the reported coefficients are marginal effects at the sample mean), we see that compared to the baseline, the unknown script yields a 1.1% higher chance of donating, while the known script yields a 0.088% higher chance of donating. Again, neither is statistically significant.

Similar to Result 1a, in Model 9 and Model 11, we rerun the tobit and probit pooling unknown and known scripts. In both cases, the coefficient on generic is negative and insignificant. Thus, the data provide directional evidence in favor of the presence of the identifiability effect for volunteers.

Result 2. For clients and volunteers, the data do not support the extended identifiability effect, i.e., when there is a single beneficiary (*known, unknown* treatments), solicitees donate smaller amounts and with lower probability when the beneficiary is someone with whom they have personally interacted (*known* treatment) than when it is a stranger (*unknown* treatment), though the differences are statistically insignificant.

The aforementioned figures in Table 3 confirm that in terms of unconditional means, the known script results in lower donations and with a lower probability than the unknown script for both clients and volunteers.

Turning to the regressions with controls in Table 4, Models 1, 2, 5, and 7 all exhibit a lower coefficient on known than on unknown, though the difference is never statistically significant using conventional tests.

In the volunteer data, we detect neither the orthodox nor the extended identifiability effect (in the client data we observe a reversal of the orthodox effect). This could be because only 55% of volunteer solicitees were solicited by voicemail only, which is unlikely to have a strong effect compared to being solicited by phone.

An arguably higher powered test of the identifiability effect is achieved by conditioning on those who were solicited by phone

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Table A2
Probit regression of phone solicitation.

Returning volunteer	−0.059**
	(0.029)
Employed dummy	−0.068**
	(0.029)
Associates/some college (dummy)	−0.024
	(0.068)
College degree or more (dummy)	−0.0037
	(0.067)
Male dummy	0.063**
	(0.029)
Black dummy	−0.035
	(0.041)
Latino dummy	−0.15***
	(0.042)
White dummy	−0.13***
	(0.036)
Married dummy	0.031
	(0.030)
# Obs.	1348
Psuedo R^2	0.020

The dependent variable is a dummy variable taking the value "1" when a volunteer is solicited by phone (rather than voicemail). Figures in parentheses denote standard errors. Coefficients are marginal probabilities at the sample mean. All coefficients and standard errors are reported to two significant figures. Asterices denote statistical significance:
** = 5%.
*** = 1%.

(45% of the sample). As per the clarification above, being solicited by phone vs. by voicemail is exogenous with respect to treatment assignment, and so the estimates of the causal effects of each script remain unbiased. As it happens, conditioning on phone solicitations increases treatment effects but in a statistically insignificant way.

Result 3. In volunteer solicitations, conditioning on those solicited by phone leads to larger treatment effect on donations and on the probability of donating, but the difference and the net treatment effects are all statistically insignificant.

Models 6, 8, 10, and 12 replicate models 5, 7, 9, and 11, respectively but conditioning on those who were solicited by phone only. With one exception, all treatment coefficients increase in size but remain statistically insignificant. Regressions which include phone and voicemail solicitees' data but allow for different treatment effects detect a statistically insignificant difference between the treatment effects of the two groups.

For those interested in the relationship between phone solicitation and the other observables, in Table A2, we estimate a probit where the dependent variable is the phone solicitation dummy variable. As expected, being employed reduces the probability of being phone solicited in a statistically significant way, but somewhat surprisingly the effect is quite small in magnitude (7%).

A final point regarding the empirical analysis concerns the site-level randomization of the client data. A comparison of Table 4 (non-linear regressions with controls) with Table A1 (non-linear regressions without controls) confirms the following. First, the controls have substantial explanatory power: they increase the *pseudo* R^2 by several multiples, and many are highly significant statistically speaking. Second, including the controls does not substantively alter the coefficients or significance levels of the treatment coefficients. Together, these two findings suggest that our results are not driven by a breakdown of randomization because the CEP randomized at the site level, and that they do indeed reflect the fundamental causal effects that we are trying to estimate.

5. Discussion

Our results describe experimental fundraising results from two populations—clients and volunteers at a financial advice non-profit. These populations both have long-term relationships with the fundraiser, as beneficiaries and providers (respectively) of the non-profit's primary service. This situation allowed us a unique opportunity to test the implications of the orthodox identifiability effect on a novel subject pool, and to extend the identifiability effect theoretically and test it empirically, all in a naturalistic field setting.

The orthodox identifiability effect received partial support among our volunteers. They were indeed more likely to give when a specific client was described as the beneficiary, rather than the firm as a whole. However, the orthodox identifiability was reversed in the clients. They were less likely to give when a specific volunteer was mentioned, compared to the firm. One possible post-hoc explanation is that the clients were disconcerted by the "surprise" solicitation, as they presumed that they were coming in for a free service, and therefore felt exploited by a "self-serving" solicitation. Alternatively, the clients may have felt ashamed or cornered, also leading to a negative response (DellaVigna, List and Malmendier, 2012).

Both of these tests suffered from low power—in the clients' case, the site-level randomization was compromised by last-minute site closings, in response to funding cuts. In the volunteers' case, many do not pick up the phone. In both cases, power was compromised by the low rate of donations, across all conditions, which is typical in the charitable giving literature.

The directional reversal in the orthodox identifiability effect between clients and volunteers has many possible post-hoc interpretations (since we did not predict it). Volunteer solicitations were ex ante more in line with the literature because the target (clients) was someone the solicitee would regard as relatively unfortunate, and because the solicitee did not have a history of receiving any assistance from the client. In contrast, in client solicitations, the target (volunteers) was likely to be richer than the solicitee, possibly undermining inequity-aversion based motives for donating (Fehr and Schmidt, 1999) and changing the donation dynamics. Moreover, the solicitee had received help from the target immediately prior to the solicitation, and the target group was in close proximity to the solicitee. These factors may induce a sense of indignation in the solicitee as a previously altruistic action (the volunteer's time donation to the client) suddenly appears more self-serving and quid-pro-quo, undermining altruistic tendencies (Dufwenberg and Kirchsteiger, 2004; Falk and Fischbacher, 2006).

These results do not support the extended identifiability effect. Solicitations that targeted a familiar name were no more likely (slightly less likely) to elicit a donation than those that used an unfamiliar name. This directional effect holds across both volunteers and clients, suggesting that in at least these two cases, the personal relationship does not facilitate more giving. The relationship with the firm was already strong for both clients and volunteers, in contrast to the causes solicited to many experimental subjects. That is, subjects might already have a vivid, personal experience in mind when the target is individuated, so whether the name itself is known might not affect the subjects' psychological reaction. We also cannot rule out a more mundane explanations—for example, it is possible clients and volunteers simply forgot one another's names. Still, we took considerable measures to make sure the familiar names used were recent and accurate—if there was any trace of the predicted effect, we did not detect it. We hope that future research can shed light on these issues.

An important generalizability question is: what broader lessons about the identifiability effect do we learn from this study? We believe that the main takeaway from our study is that the identifiability effect is perhaps not as robust or universal as previously

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thought. The fact that we found evidence of its reversal (and not just its absence) in the case of clients suggests that this initial conclusion is not merely the result of low power; rather, we believe that it suggests a possible interaction between the identifiability effect and other factors, such as those mentioned above in our post-hoc explanations. In particular, the orthodox identifiability effect typically deals with groups of solicitees and beneficiaries who are socially and geographically far apart, whereas in our data, the two groups interact face-to-face via the non-profit's activities, which may well alter the dynamics of charitable contributions.

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