NBER WORKING PAPER SERIES

THE ABCS OF CHARITABLE SOLICITATION

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Working Paper 15037
http://www.nber.org/papers/w15037

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
June 2009

We are grateful to Kevin Cotter, Bruce D. Freeman, Jean Grossman, William Hardt, Caroline Hoxby, John List, Brian McDonald, Ashley Miller, Liam P. Morton, Sriniketh Nagavarapu, Deborah Prentice, Andres Santos, Julie Shadle, Jeffrey H. Yellin, seminar participants at Northwestern and Stanford Universities, and participants in the Middlebury Conference on Charitable Giving for useful suggestions. Zhihao Zhang provided excellent research assistance. This research was supported in part by Princeton's Center for Economic Policy Studies, in part by the Stanford Institute for Economic Policy Research, and in part by the Koret Foundation. The views expressed herein are those of the author(s) and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 15037
June 2009
JEL No. D64,I22

ABSTRACT

A substantial experimental literature suggests that a personal solicitation is an effective way to induce people to make charitable donations. We examine whether this result generalizes to a non-experimental setting. Specifically, we estimate the effect of a marginal personal solicitation using observational data on alumni giving at an anonymous research university, which we refer to as Anon U. At Anon U, volunteers use lists provided by the Development Office to telephone classmates and solicit them for donations. The names on these lists are always in alphabetical order. The volunteers who do the soliciting often run out of time before they reach the end of their lists. These observations suggest a simple strategy for testing whether personal solicitation matters, viz., examine whether alumni with names toward the end of the alphabet are less likely to give than alumni with names toward the beginning, ceteris paribus. If so, then a marginal personal solicitation matters.

Our main finding is that location in the alphabet – and hence, a personal solicitation – has a strong effect on probability of making a gift. A rough estimate of the elasticity of the probability of giving with respect to the probability of receiving a personal solicitation is 0.15. However, there is no statistically discernible effect on the amount given, conditional on donating. We also find that women respond more strongly to a personal solicitation than men. This is consistent with a robust result in the psychology literature, that women find it more difficult than men to refuse requests that they perceive as being legitimate.

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1. Introduction

At least since the time of de Tocqueville, observers have marveled at the generosity that Americans exhibit toward one another. In 2005, they recorded over $183 billion of charitable donations on their tax returns (Statistics of Income Bulletin [2008]), a figure that includes only itemized contributions and does not count the value of time spent volunteering. But charitable institutions generally do not rely only on people's generous spirits and wait passively for donations. Rather, they actively encourage potential donors to make gifts.

Are these solicitation activities effective? Anecdotal evidence suggests that the answer is overwhelmingly yes (Bekkers and Wiepking [2007]). In a way, this is unsurprising—it's hard to imagine the people who are asked to contribute to a charity won't be more likely to give than people who are not. A more interesting question is whether additional solicitations matter – if a potential donor doesn't respond to several requests to give, can a marginal solicitation have an effect? A closely related question is if certain types of solicitation activities are more effective than others, and if so, by how much. For example, what is the differential impact of a solicitation that is done in person as opposed to a letter, and is the impact primarily on the likelihood that an individual makes a donation (the extensive margin) or the amount given conditional on making a gift (the intensive margin)?

Relatively little research has been done on the effectiveness of additional solicitations. Two papers, both using responses from small surveys, are relevant. Diamond and Noble [2001] find that in response to frequent solicitations, donors develop defense mechanisms such as simply throwing out mail requests. They provide no quantitative estimate of the impact of an additional solicitation. . Using self-reported measures of solicitation frequency and giving behavior,

Van Diepen *et al*. [2006a] find that additional appeals initially generate more donations, but after some point, donees become irritated and may actually reduce their giving.

On the other hand, the differential impacts of alternative types of solicitation activities have been the subject of an extensive experimental literature.[1] An important finding in this literature is that the level of scrutiny associated with the solicitation matters. That is, individuals behave differently when their behavior is more observable, for example, when the solicitation is done personally rather than by mail. This has been documented both in laboratory experiments (Andreoni and Petrie [2004], Anderson and Stafford [2009]) and field experiments (Landry *et al.* [2006], Alpizar *et al*. [2008]). Some papers written on field experiments also allow us to learn whether alternative methods of solicitation are more likely to affect whether an individual is makes a gift (the extensive margin) or the amount of the gift, conditional on making a gift (the intensive margin).[2] For example, List and Lucking-Reiley [2002] examine the effects of seed money, which signal that the charity is worthwhile by showing that others have already contributed, and find that there are strong effects on both the extensive and intensive margins. Huck and Rasul [2007] find that different mechanisms of solicitation, like rebates or matching grants, also have effects on both the probability of making a gift and the size of the gift itself, conditional on making one. Meanwhile, Landry *et al*. [2006] find that lotteries increase giving almost entirely through their effect on the extensive margin. It appears, then, that there are no general results with respect to whether donative behavior is more sensitive to various treatments on the intensive or extensive margins.

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[1] A theoretical analysis of the impact of solicitation is included in Andreoni and Payne [2003].

[2] The papers describing laboratory experiments are largely silent on this issue. They focus primarily on measuring the proportion of subjects who completely free ride under given conditions, and the unconditional mean donation. In general, the papers do not report the mean donation conditional on making a donation, and how it varies with the experimental situation.

All of this begs the question of whether the results from experiments generalize to actual settings. Levitt and List [2007] argue that field experiments are likely to produce results that generalize than laboratory experiments, but ultimately, whether they generalize is an empirical question. This observation is particularly cogent in the case of charitable giving, because the constraints associated with the design of field experiments tend to lead to both relatively small sample sizes and amounts donated. This leads to the question of whether there are any studies on solicitation using observational data that can be compared to the experimental results. The empirical literature using observational data is rather thin. Yoruk [2006] uses individual-level data from a survey to estimate the impact of solicitation on total charitable giving. However, the date used in that study have an important limitation. An individual is counted as having been solicited if he or she received *any* solicitations during the year, and there is no information about whether the charity to which the individual actually contributed did in fact solicit him or her. Van Diepen, Donkers and Franses [2006b] found that direct mailing solicitations increase giving to a group of charitable institutions in the Netherlands, but the solicitations appear to have had a substantial informational component. Thus, one cannot determine whether the increased giving was due to the solicitation *per se* or the information about the charities. Schervish and Havens [1997] analyze survey data that ask individuals whether they have been solicited by charitable organizations, and find that individuals who have been solicited make larger donations. However, the direction of causality is unclear; one does not know whether people give money because they are solicited or they are solicited because they give money, or some third variable (such as a family history of support of charities) drives both.

Several papers have used observational data to study the solicitation activities of institutions of higher education, which, like other organizations that rely heavily on donations, have

sophisticated fundraising organizations.[3] Gottfried and Johnston [2006] and Leslie and Ramey [1988] both examine data at the university level to assess the efficacy of these solicitation activities. However, they can only focus on fairly crude indicia of solicitation, such as the percent of alumni contacted. Another problem with using university-level data is that the direction of causality again may be an issue – it seems just as plausible that the percent of alumni contacted is a function of the university's resources as vice versa.

This paper uses observational data from an anonymous research university to estimate the impact of a marginal personal solicitation on donative behavior. At this institution, which we will refer to as Anon U, volunteers use lists provided by the Development Office to telephone classmates and ask them to make a donation. The names on these lists are always in alphabetical order. The volunteers who do these personal solicitations generally go through their lists in order but often run out of time before they reach the end.[4] These observations suggest a simple strategy for determining whether a marginal personal solicitation matters, *viz*., examine whether alumni with names toward the end of the alphabet give less than alumni with names toward the beginning of the alphabet, *ceteris paribus*. If so, then a marginal personal solicitation matters.[5]

In Section 2 we discuss the data. Section 3 presents our model and results. Our main finding is that location in the alphabet has a strong effect on probability of making a gift. In our basic model, an alumnus in the first part of the alphabet (A through F) is 1.2 percentage points (off a baseline of 22 percent) more likely to make a donation than an alumnus toward the end of the alphabet (S through Z). On the other hand, conditional on giving, the amounts donated are not

[3] In 2007, institutions of higher learning raised about $8 billion from their alumni (Council for Aid to Education [2008]).

[4] One volunteer told us, "Naturally, I tend to start at the top of the lists that I am given and sometimes do not get to the end of the list. This does not seem like a big deal, but that happens to the bulk of my solicitation team."

[5] It is possible that volunteers put less effort into the solicitation of individuals toward the end of the alphabet. In this case, the impact of alphabetical placement on giving reflects "effective solicitation," which depends on both the likelihood of being solicited and the intensity of the solicitation, conditional on being solicited. Our data do not allow us to identify separately these two effects.

sensitive to the name's placement in the alphabet. That is, solicitation affects the extensive margin of giving but not the intensive margin. We also show that our findings are not driven by a correlation between an individual's income and the location of his or her name in the alphabet.

These finding suggest that the experimental results with respect to the importance of the level of scrutiny generalize to real world settings. As noted above, the experimental results with respect to the impacts on the intensive and extensive margins are mixed; our findings suggest that, at least in our observational setting, the experimental results indicating small impacts on the intensive margin are more valid. This difference between the effects on the intensive and extensive margins is consistent with models in which individuals donate to charities in order to avoid the solicitor's disapproval. In this case, the donation per se is perceived as eliminating the stigma; the amount given, conditional on giving, has no additional impact. The results can also be rationalized by a model in which people have in mind a certain amount to give, but do not make that gift unless asked.[6] Section 4 concludes with a summary and suggestions for future research.

2. Data and Econometric Model

Both the construction of our analysis sample and econometric model are informed by the practices of Anon U's Development Office. The fundraising year begins on July 1. During the next eleven months, every alumnus receives at least two mailings and, in recent years, several emails as well. In general, few personal solicitations are made during this 11-month period, except for a small number of very large donors. In June, the strategy shifts. Alphabetical lists with names of alumni who have not contributed at this point are given to alumni volunteers, who are

[6] Andreoni and Payne [2003] lay out such a model.

asked to make a personal solicitation, generally by telephone.[7] The total number of volunteers in a given year is about 2500 (a figure that has been fairly steady over time), and each list has about 20 names. Alumni who have contributed during the previous 11 months are not subject to any further solicitation in June. Our analysis sample consists of the alumni who have not given as of June. These are the givers who are subject to the "treatment" of a marginal personal solicitation — provided that a volunteer actually gets to their names on the list. Our goal is to estimate the impact of an incremental solicitation that is done personally rather than by mail.[8] In other settings, as well, the incremental effect of a given type of solicitation is relevant, because as Van Diepen, Donkers and Franses [2006] point out, it is typical for individuals to be solicited multiple times by a given charity.

As noted above, our analysis relies on the assumption that volunteers go through their lists in alphabetical order. Of course, this need not strictly be the case. Volunteers might, for example, contact friends before going through the rest of the list. But provided that the names of friends are random with respect to position in the alphabet,[9] all this does is introduce some noise into the relationship between solicitation and alphabetical placement; it does not negate the underlying premise of the analysis — if a marginal personal solicitation matters and individuals at the beginning of the alphabet are more likely to be solicited, then individuals at the beginning of the alphabet should be more likely to give.

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[7] A relatively small proportion of the calls are made by current students. They are generally given lists of alumni from classes that have relatively few volunteers.

[8] This assumes that the effect of a marginal solicitation by mail in June is zero. This is a reasonable assumption, given that individuals have already rejected a number of mail solicitations. Apparently, the Development Office believes this assumption is correct. After all, the option of sending another letter or email is still available in June, yet the Development Office chooses to follow the much more costly strategy of a personal solicitation. To the extent that some individuals would in fact have responded to a mail solicitation in June, our estimates will overstate the effect of a marginal personal solicitation.

[9] At Anon U, neither academic nor residential assignments are made alphabetically. Hence, there is no reason to believe that there will be a correlation between position in the alphabet and whether the volunteer has a personal relationship with an individual on his or her list.

Another problem could arise if the Development Office systematically assigned different parts of the alphabet to specific solicitors. However, discussions with Anon U solicitors and the staff of the Development Office indicate that this is not the case. The only non-random aspect of the assignment is by region; solicitors are matched to alumni who live in their general vicinity.

2.1 Data

Our primary data source is the administrative archives of Anon U's Development Office, which contain information on all alumni donations from 1983 to 2007. The data are proprietary and sensitive, and individuals' names were stripped from the records before being made available to us. For purposes of this study, however, we were provided with the first initial of each alumnus's last name. Our data also indicate whether an alumnus's initial changes (due, for example, to the adoption of a new name with marriage), so it is possible for a given individual's position in the alphabet to change over time.

Our unit of observation is a yearly giving opportunity. For example, if an individual has been an alumna for 5 years, she accounts for 5 giving opportunities in our analysis, starting in the first fiscal year after graduation. Multiple gifts for the same purpose in the same year are summed together. The Development Office data also include information on academic major, the alumnus's undergraduate extracurricular activities, post graduate education, occupation, residence, and whether he or she is married to another graduate of Anon U.

Anon U's Registrar supplemented these data with information on SAT scores, academic honors, ethnicity, type of high school, summary evaluations made by the Admissions Office during the application process, and grade point average. The Registrar's data are available only for the classes of 1972 and onwards, so we restrict most of our analysis to this group of individuals. This gives us 583,496 observations, representing 35,583 alumni. After removing those who give

in the first 11 months of the year, we are left with 331,824 observations representing 32,152 alumni. We delete 22,929 observations because of missing or unreliable data on covariates. This leaves 308,895 observations on 30,148 alumni. Of these observations, 68,516 (or 22.18 percent) are associated with a gift.

2.2 Econometric Specification

We noted earlier that being solicited may have different effects on the decision whether or not to donate — the extensive margin — than on the decision how much to donate, conditional on making a gift — the intensive margin.[10] A statistical model that allows for this possibility is therefore needed. We assume that each alumnus first chooses whether or not to make a gift and then, conditional on making a gift, decides how much to donate. Following Huck and Rasul [2007], a natural specification is a hurdle model. In our context, the first step in the implementation of the hurdle model is to estimate a probit for whether or not the individual makes a gift. The second step is to use ordinary least squares on the positive observations to analyze the decision about how much to give. An assumption is needed to make causal inferences from the second-stage estimates, namely, that the second stage is conditionally independent of the first. We discuss this further below.

It is straightforward to use the estimates from these two steps to calculate unconditional marginal effects on the mean level of giving; this allows us to characterize the effect of solicitation on giving taking into account both the impacts on the intensive and extensive margins.[11] We

[10] Thus, for example, it would not be appropriate to use a Tobit model, which imposes the constraint that the marginal effect of a given variable on the probability of giving and the marginal effect on the amount given are the same up to a constant of proportionality.

[11] Denote the amount of giving as Y, and the vector of right hand side variables as X. Then the first stage of the estimation gives results for $\Pr[Y>0|X]$ and the second stage gives $E[Y|X,Y>0]$. The unconditional value of giving, $E[Y|X]$, is $\Pr[Y>0|X]*E[Y|X,Y>0]$. The marginal effects, $\partial E[Y|X]/\partial X$, are straightforward to compute, and standard errors are obtained using the delta method.

correct for correlation among the error terms for any given individual with a clustering procedure in both the probit and OLS models.

An alternative two-step procedure, suggested by Heckman [1979], can also be used to estimate the amount of giving, conditional on it being positive. Heckman's model augments the OLS equation in the second stage with the inverse Mills ratio. There is some controversy in the literature with respect to which estimator is superior (Leung and Yu [1996]); hence, a sensible approach is to estimate the model both ways. We show below that our substantive results are essentially unchanged when we use Heckman's method.

An immediate issue is the choice of functional form to represent the relationship between an alumnus's position in the alphabet and giving behavior. There is no obvious answer, but our conversations with several of Anon U's volunteers indicate that solicitation efforts drop off precipitously toward the end of the alphabetical lists, rather than in a continuous fashion. Therefore, we employ a step function specification. Alumni are assigned a one if the first letters of their last names fall between A and F, G and L, and M and R, respectively, and zero otherwise. The omitted category is S to Z. As noted toward the top of Appendix Table A1, which shows the summary statistics for our analysis sample, 29.4 percent of the observations fall between A to F, 25.4 percent between G to L, 22.7 percent between M and R, and the rest between S and Z. We experimented with both a tighter specification (which constrains the relationship between giving and position in the alphabet to be linear) and a more flexible specification (which has more groupings). As shown below, our substantive results are unchanged.

We include on the right hand side all the variables listed in Table A1. For each set of dichotomous variables, the "omitted category" is the variable that is excluded from the regressions. We have a series of variables about each alumnus that has been shown in previous studies to ex-

ert an important influence on alumni giving (Cunningham and Cochi-Ficano [2002], Shulman and Bowen [2001]). These include years since graduation, gender, ethnicity, SAT scores, ranking of the candidate by the admissions office when they applied to Anon U, course of study, and post baccalaureate education. The literature also shows that alumni giving is heavily influenced by the affinity that they develop for their schools as undergraduates. Participation in varsity sports and membership in fraternities are two ways in which such affinities develop (Clotfelter [2001], Monks [2003]); we also include variables relating to these activities. The model also includes time effects, class effects, and location effects. The year effects reflect the impacts of the business cycle, the stock market, and so on.[12] The year effects also account for the size of Anon U's fundraising staff and the amount of its fundraising expenditures, which vary from year to year. The class effects control for common influences on alumni in the same class, such as the political milieu when they were undergraduates, the presence of certain professors or administrators, and so on.

A final econometric issue relates to the fact that our data contain a few very large outliers. For example, the four largest gifts in our sample are $500,000 or more. To address this issue, we use the logarithm of the amount of giving on the left hand side of the OLS equation. As an additional check to make sure that outliers are not driving our results, we estimate the OLS equation with the top one percent of the observations eliminated. As shown below, the substantive results with respect to the impact of the last name are not affected.

3. Results

In this section, we first present our basic results. This is followed by a discussion of some alternative specifications that allow us to assess the robustness of our findings.

[12] Bristol [1991] emphasizes the role of the stock market and Ehrenberg and Smith [2001] document the importance of macroeconomic conditions.

3.1 Basic Model

Column (1) of Table 1 shows the marginal effects of the alphabet position variables on the probability of making a gift. The figures in parentheses are standard errors. In addition to the variables listed in the table, the models include the other right hand side variables mentioned in the previous section, which are suppressed for brevity.

To begin, consider the first entry in the column (1). The coefficient, 0.0117, suggests that, relative to an individual toward the end of the alphabet (S through Z), an individual at the beginning of the alphabet (A through F) is about 1.2 percentage points more likely to make a gift. The coefficient is statistically significant, with $p = 0.009$. Recalling that the baseline proportion of alumni who make gifts in this sample is 22 percent, the difference is quantitatively substantial. An alumnus whose name begins with G through L is 0.88 percentage points more likely to give than an individual whose name is at the end of the alphabet; the effect is significant at $p = 0.057$. For individuals whose names begin with M through R, the point estimate of the difference from those at the end of the alphabet is minuscule (-0.04 percentage points) and one cannot reject the hypothesis that it is zero.

The results from column (1) suggest that there is in fact an alphabet effect — people whose last names are toward the beginning of the alphabet are more likely to give than those whose names are at the end. However, the effect is not monotonic. It is driven primarily by the differences between alumni in the first half of the alphabet relative to those in the second half of the alphabet. In any case, the results are consistent with the hypothesis that a marginal personal solicitation matters.

Because some people at the end of the alphabet are solicited, and some people at the beginning of the alphabet are not, our estimates of the impact of alphabetical placement are likely

to be lower bounds. To go beyond this qualitative statement requires information on how much more intensely the beginning of the alphabet is solicited than the end. We would interpret our estimates rather differently if we believed that individuals at the beginning of the alphabet were five percent more likely to be solicited rather than 50 percent more likely. It was not possible to conduct a systematic survey of the volunteers to obtain data on the differential. However, on the basis of conversations with several volunteers, a reasonable estimate is that a typical solicitor contacts about 80 percent of the names in the A-F range, and about 60 percent in the S-Z range. In conjunction with our econometric results, this suggests that a rough estimate of the elasticity of the probability of giving with respect to the probability of being contacted is 0.15.[13] Should one characterize this as a large or small response? It appears non-trivial, but whether engaging more volunteers is cost-effective from the university's standpoint also depends on the time and effort involved in doing so, and we have no information on this matter.[14]

Column (2) of Table 1 shows the impact of position in the alphabet on the log of the amount of the gift, conditional on a gift being made. In contrast to the result in column (1), there is no statistically discernible alphabet effect. The impact of being in the first quarter of the alphabet is 3.8 percent with a standard error of 2.4 percent, so one cannot reject the hypothesis that it is zero. The effect of a last name between G and L is small and insignificant (-0.04 percent, s.e. = 2.56 percent), and the effect of having a last name between M and R is similar in magnitude to that of the A through F category, and similarly insignificant (4.2 percent, s.e. = 2.7 percent).

[13] A 1.2 percentage point increase off of a base of 22 percent is a 5 percent difference in the probability of giving. A 20 percentage point increase off a base of 60 percent is a 33 percent increase in the probability of being solicited. The elasticity is therefore 5/33 or 0.15.

[14] To the extent that making a donation in one period increases the probability of making a donation in subsequent periods (that is, giving is habit forming), then the long-run effect of making a donation could be larger. See Meer [2009].

Column (3) combines the estimates from columns (1) and (2) to examine the unconditional effect on the amount of giving. Being in the first quarter of the alphabet is associated with giving that is 5.7 percent (s.e. = 2.8 percent) higher than being in the last quarter of the alphabet; being in the second quarter of the alphabet is also statistically significant from zero, but smaller than the first-quarter effect (3.7 percent, s.e. = 2.2 percent). This is a relatively small effect on the unconditional mean of giving of about $110. In light of the results from columns (1) and (2), this is not surprising: combine a substantial alphabet effect on the extensive margin with essentially no effect on the intensive margin, and the result is unlikely to be large

Taken together, the results in columns (1) and (2) suggest that a personal solicitation has little effect on the amount a donor chooses to give, only on whether the donor chooses to give. The notion that the impact of a personal solicitation is less on the intensive margin than the extensive margin is much in the spirit of Landry *et al.*'s [2006] field experiment on charitable appeals. They find that participation rates are higher for individuals who are solicited door-to-door than for those who are solicited by mail, although the amounts given, conditional on making a gift, are higher for those solicited by mail. They surmise that the social pressure of being approached in person leads to this result. In this case, at least, it appears that experimental results generalize well to actual behavior.

3.2 Variables Possibly Correlated with Placement in the Alphabet

Unfortunately, our data include no direct information on income, an important determinant of giving (Shulman and Bowen [2001, p. 404]). To the extent that placement in the alphabet is correlated with income, this could bias our estimates. We know of no evidence that such a correlation exists. Einav and Yariv [2006] show that last names affect publication records in academic economics, but this is a very specific and narrow set of individuals. Indeed, they document

that the effect is due to particular norms in the economics profession, and does not carry over even to other academic disciplines. Jurajda and Munich [2007] show that the probability of gaining admission to certain select universities in the Czech Republic depends on an applicant's place in the alphabet, but even if this phenomenon also exists in the United States, our sample consists of individuals who all attended the same school.

Although we lack annual income data, for a large subset of our alumni, we have information that is closely related to permanent income, occupation and field.[15] We have these variables for 206,943 observations, representing 19,737 alumni.[16] The proportion of individuals in this sample who make a donation in June, conditional on not having made a donation before, is 26.1 percent, somewhat higher than the 22.2 percent figure for our basic sample. Table A1 shows the occupations and fields for this sample. The fields of education, finance, health care and law are highly represented. We re-estimate our basic models with this subsample including the occupation and field data in order to see whether our substantive results are sensitive to their inclusion.

To establish a baseline for this exercise, we begin by estimating our model using only the sample of alumni for whom we have occupation and position, but without including these variables. These results are recorded in columns (1a), (2a) and (3a) of Table 2. We next augment this model with the occupation and position variables; these results are in columns (1b), (2b), and (3b). The results suggest that in this subsample: a) the impact of position in the alphabet is somewhat larger than in the sample as a whole; but b) the qualitative effects are the same; and c) these effects do not change when the occupation and position variables are added to the model.

[15] In this context, it is important to note that a number of the variables in our basic specification are also correlated with permanent income, including gender, ethnicity, college major and grade point average, advanced degrees, years since graduation, and location. Cunningham and Cochi-Ficano [2002] point out that SAT scores are closely related to family socioeconomic status as well.

[16] Due to lack of reliable data regarding the start- and stop-dates of occupation and field, these variables indicate whether the alumnus was *ever* involved in that field or occupation, rather than whether they are involved during the particular year of observation.

We conclude that it is unlikely that our results with respect to alphabetical placement are being driven by the omission of income from our set of right hand side variables.

We also note that if donative behavior differed systematically by position in the alphabet, then we would expect the proportion of alumni who failed to make a gift by June to differ by alphabetical group. But this appears not to be the case. The proportions are remarkably similar: 56.5 percent for A through F, 56.7 percent for G through L, 57.2 percent for M through R, and 57.0 percent for S through Z.

A more systematic test for whether alphabetical placement reflects something other than personal solicitation effects is suggested by the fact that our analysis sample consists only of alumni who fail to make donations during the first 11 months of the annual giving cycle. Suppose that position in the alphabet is merely picking up the effect of income, or, for that matter, any other variable that might be correlated with giving. Then our results with respect to the impact of alphabet on donative behavior should be about the same for individuals who made a gift during the first 11 months of the cycle and those who made a gift in June. To investigate this possibility, we use the data from the entire year to estimate the probability that the alumnus made a gift during the first 11 months (that is, the dependent variable is an indicator for making a gift in the first 11 months). Thus, we are measuring the effect of alphabet position on alumni who did not receive the "treatment" of a marginal personal solicitation. We find that position in the alphabet has no statistically discernible effect on the probability of making a gift. For instance, the impact of having a last name between A and F is -7.5×10^{-6}, and with a standard error of 0.00517; one cannot reject the hypothesis that it is zero. This lends credence to the notion that our findings reflect a true solicitation effect.[17]

[17] To explore further whether placement in the alphabet reflects something other than a solicitation effect, we estimate regressions of a variety of outcome variables on the alphabet step function. We find either no statistically dis-

3.3 Alphabetical Placement and Functional Form

As noted above, there is some arbitrariness to the step function that we adopt to characterize the relationship between placement in the alphabet and giving. To assess the sensitivity of our results to this functional form, we begin by estimating a variant of our model in which the effect is constrained to be linear. The results imply that being one letter closer to the end of the alphabet leads to a 0.065 (s.e. = 0.023) percentage point lower probability of making a gift. Thus, moving from A to S is associated with a roughly 1.2 percentage point lower probability, not unlike the result in Table 1. Turning to the intensive margin, the amount of the gift falls by only 0.07 percent for each letter by which the alumnus is closer to the end of the alphabet, and it is estimated imprecisely (s.e. = 0.13 percent). This is again consistent with the step function specification in Table 1. In short, the substantive results that emerge when we use a linear functional form are essentially the same.

We also experimented with a more flexible specification than the one in our basic model. It allows a separate coefficient for every letter in the alphabet. We found that one cannot reject the hypothesis that, within each category, the coefficients for each letter are equal. For example, in the model that examines the extensive giving margin, we conducted a joint test of the hypothesis that the coefficients are equal within each of the four groups. The associated p-value is 0.17. For the intensive margin, the p-value associated with the corresponding test is 0.11. Thus, the data do not allow us to distinguish among last names on a letter-by-letter basis. However, when we estimated a model with a few more groupings than in our canonical specification, we obtained results that are very much in line with the results in Table 1. For instance, dividing the alphabet into six categories yields essentially identical results for A to D (1.4 percent, s.e. = 0.63

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cernible effect of alphabet (for example, on whether the individual ever worked in the field of finance or as an executive) or, more rarely, a statistically significant effect that is quantitatively minuscule (for example, being in the A through F category increases grade point average by 0.012 points (s.e. = 0.0052) on a four-point scale).

percent), E to H (1.5 percent, s.e. = 0.66 percent), and I to L (1.4 percent, s.e. = 0.71 percent). Meanwhile, indicators for M to P (0.40 percent, s.e. = 0.65 percent) and Q to T (0.55 percent, s.e. = 0.65 percent) are not significantly different from the omitted category, U to Z. [18]

3.4 Alphabet Effects and Time

As technology such as e-mail, cell phones, and voicemail has become more widespread, the cost of making a personal appeal to each alumnus has fallen.[19] In our context, this means that it is easier for volunteers to reach the end of their alphabetical lists. Hence, if our results are really due to differential solicitation efforts, then the impact of placement in the alphabet should diminish over time. To determine whether this is the case, we interact the alphabet position variables with indicators for three time periods: (1) 1983-1995, a period during which the new communications technology was relatively rare; (2) 1996-2000, during which much of this technology was being adopted; and (3) 2001-2007, when such technology had become mainstream. The early period – between 1983 and 1995 – is excluded as the comparison group.

The results are in Table 3, which shows the combination of each main effect and the relevant interaction. As expected, during the early period the effect is larger for the front half of the alphabet, with the indicators for A-F and G-L both being around 1.4 percentage points and significant. Using the same approximations as before, the implied elasticity of the probability of making a gift with respect to the probability of being solicited is 0.24.[20] The effect for M-R is significant at p = 0.14 and the point estimate is larger than the corresponding coefficient in Table

[18] Complete results for these specifications are available upon request.

[19] This raises the question of whether an e-mail solicitation should be considered "personal." Clearly, a blast e-mail to everyone on the list is not particularly personal. However, e-mails can be customized (at relatively low cost to the solicitor) to make them far more personal than a form letter. Members of younger classes are particularly accustomed to online communication of a personal nature.

[20] This calculation assumes that the difference in the probability of being solicited from the front to the back of the alphabet is the same as in the calculation reported previously. To the extent that this difference was larger in the earlier period, which seems likely, the elasticity of the probability of giving with respect to the probability of being solicited will be smaller.

1. During the middle period, 1996-2000, the alphabet effect diminishes somewhat. In particular, the M-R effect for that period is small and insignificantly different from zero, providing some evidence that technology is allowing solicitors to get further down their lists. The estimates for the late period reflect the full impact of improvements in communications technology. The estimate for A-F is relatively small and not significant at conventional levels ($p = 0.15$); the G-L effect is also insignificant. In short, the results in Table 3 are consistent with the notion that improvements in communications technology have reduced the cost of person-to-person solicitation and made it easier for solicitors to reach alumni who are further down their lists. Turning to the amount of the gift, conditional on giving, no clear patterns emerge. Technology has not had any discernible effect on the impact of a personal solicitation on the intensive margin.

A possible alternative interpretation of Table 3 is that improvements in technology have made it easier to make a contribution to Anon U before June. This would imply that, in recent years, the individuals who reach June without having made a gift are relatively unresponsive to solicitations, accounting for the lack of an alphabet effect. If this were the case, though, we would expect to see the proportion of alumni who made a gift in June (conditional on making any gift at all during the year) to have fallen over time. If anything, however, the proportion of procrastinators has increased over time. A least squares regression of the proportion of alumni who make a gift in June (conditional on making any gift at all during the year) on time yields a significant coefficient of 0.00265 (s.e. = 0.000108). We conclude that the results in Table 3 are not due to improvements in technology (or any other changes over time) that have induced alumni at Anon U to procrastinate less.[21]

3.5 Gender

[21] A related but distinct issue is whether the alphabet effect varies over the life cycle. Interacting the alphabet indicators with years since graduation yields no statistically discernible effect, with the three interaction terms insignificant at $p = 0.413$.

It is well documented that charitable behavior differs by gender (see, for example, Andreoni and Vesterlund [2001] and Meer and Rosen [2009]). We therefore estimate our model separately by gender to see if a personal solicitation has different effects for men and women. The results are shown in Table 4 and indicate that women react more strongly. Specifically, women in the first quarter of the alphabet are 1.5 percentage points more likely to make a gift than those at the end of the alphabet while for men the comparable figure is only 0.9 percentage points.[22] The latter value is only significant at $p = 0.11$. This finding is consistent with a robust finding from the psychology literature, *viz.*, women find it more difficult than men to refuse requests that they perceive as being legitimate. (See, for example, Eagly [1983].) Importantly, though, solicitation does not affect the intensive margin for either gender

3.6 Outliers

As noted above, Anon U received a few very large gifts. An important question is whether these outliers are driving our results with respect to amounts given. We therefore re-estimated our model for the log of giving dropping the top one percent of the observations. As a further check, we estimated the model in levels rather than logs, again dropping the top one percent of the observations. The log and level results for the amount given, conditional on giving, are in columns (1a) and (2b) of Table 5, respectively. A glance at the table indicates that, in both specifications, the effect of placement in the alphabet remains small and insignificant. Thus, our finding that solicitation does not affect the intensive margin is neither an artifact of the logarithmic specification nor due to the influence of outliers. Looking at the unconditional marginal effect on giving in column (1b) shows similar results to column (3) in Table 1, indicating that outliers have little effect on our estimates of the overall effect of solicitation on giving.

[22] The baseline giving rates are essentially the same for the two groups; 21.87 percent for men and 22.85 percent for women.

3.7 Alternative Econometric Specification

An alternative econometric estimator augments the OLS equation for the amount given with the inverse Mills ratio generated by the first stage probit (Heckman [1979]). This model explicitly allows for correlation between the errors in the first and second stage equations. The econometric literature indicates that the desirability of this estimator relative to the hurdle model is unclear. In particular, Leung and Yu [1996] observe that even if the errors in the true model are correlated, the hurdle model may, under certain circumstances, yield better estimates. In any case, it seems sensible to re-estimate the model using Heckman's approach to see if the substantive results are affected. These estimates imply that the marginal effect on gift size, conditional on making a gift, are 0.0320 (s.e. = 0.0307) for A to F; -0.0043 (s.e. = 0.0307) for G to L; and 0.0414 (s.e. = 0.0304) for M to R. Comparing these results to those in column (2) of Table 1, we see that they are essentially the same. Estimates of the unconditional marginal effects on the amount of the gift are similarly consistent to those in column (3) of Table 1. Hence, our results are robust with respect to this change in econometric specification.

4. Summary and Conclusions

We have examined whether charitable donations respond to a marginal personal solicitation using observational data on alumni giving at an anonymous research university. Our test relies on the fact that at this university, volunteers use lists provided by the fundraising office to telephone classmates and solicit them for donations. The names on these lists are always in alphabetical order and the volunteers who do the soliciting often run out of time before reaching the end of their lists This observation suggests a simple strategy to test whether solicitation matters, viz., examine whether alumni with names toward the end of the alphabet are less likely to

give than alumni with names toward the beginning of the alphabet, *ceteris paribus*. If so, then a personal solicitation matters.

Our main finding is that location in the alphabet has a strong effect on the probability of making a gift. In our basic specification, the probability that an individual in the first part of the alphabet makes a gift is 1.2 percentage points higher than the probability for an individual in the last part of the alphabet. Using this estimate, along with a rough figure for the difference in the probabilities of being solicited, we calculate that the elasticity of the probability of making a gift with respect to the probability of being asked is about 0.15. The notion that the level of personal scrutiny associated with a solicitation has an important effect on giving behavior is consistent with the experimental literature. In this case, then, the results from laboratory and field experiments generalize to real world settings.

We also find that being solicited does not affect the amount given, conditional on donating. Thus, a personal solicitation does affect giving, but only on the extensive margin. The experimental literature has produced mixed results on the relative impacts of solicitation on the extensive and intensive margins; our findings suggest that, at least in this context, the studies that find little effect on the intensive margin are more applicable to the real world.

An important caveat is that these results regarding the intensive margin are unlikely to apply to the very largest gifts. One can easily imagine that when major donors are identified, fundraising organizations solicit them vigorously, and the skill with which this solicitation is done substantially affects the amount given. Hence, our results might best be thought of as applying to “rank and file” rather than elite givers. Unfortunately, statistical analysis of this latter group is problematic because their numbers are so small and the development office tailors its solicitation strategies to their specific tastes and interests.

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Table 1†
Impact of Alphabetical Placement on Donations

	(1) Probability of Making a Gift	(2) Log Amount of Gift Conditional on Giving	(3) Unconditional Effect on Giving
A to F	0.0117***	0.0358	0.0571***
	(0.00449)	(0.0244)	(0.0284)
G to L	0.00881*	-0.00041	0.0374*
	(0.00465)	(0.0257)	(0.0215)
M to R	-0.00041	0.0418	0.00683
	(0.00471)	(0.0270)	(0.0217)
Number of Observations	308,857	68,575	308,857

†Column (1) shows the incremental effect on the probability of making a gift of placement in the alphabet in a given year, and column (2) shows the effect on the logarithm of amount of the gift. The baseline giving rate is 22.2 percent. The figures in column (1) are marginal effects generated by a probit model, and the figures in column (2) are generated by OLS. Column (3) combines these, and shows the unconditional marginal effects on total giving. The figures in parentheses are standard errors; coefficients that are statistically significant at the 1% level are noted with ***, those significant at the 5% level are noted with **, and those significant at the 10% level are noted with *. Standard errors are adjusted for clustering based on individuals. In addition to the variables listed, the regressions include the variables listed in the Appendix, as well as location effects, time effects, and class effects, which are not reported for brevity. Full results are available upon request.

Table 2†
Impact of Alphabetical Placement on Donations
Field and Occupation Sample

	Baseline Specification Estimated with Field and Occupation Sample		
	(1a) Probability of Making a Gift	(2a) Log Amount of Gift Conditional on Giving	(3a) Unconditional Effect on Giving
A to F	0.0210*** (0.00599)	0.0264 (0.0282)	0.0991*** (0.0289)
G to L	0.0107* (0.00614)	-0.00291 (0.0293)	0.0464 (0.0294)
M to R	0.00269 (0.00626)	0.0436 (0.0312)	0.0226 (0.0300)
Number of Observations	206,907	53,957	206,907

	Field and Occupation Indicators Included		
	(1b) Probability of Making a Gift	(2b) Log Amount of Gift Conditional on Giving	(3b) Unconditional Effect on Giving
A to F	0.0221*** (0.00595)	0.0377 (0.0274)	0.106*** (0.0285)
G to L	0.0114* (0.00608)	0.00246 (0.0286)	0.0504* (0.0289)
M to R	0.00039 (0.00621)	0.0490 (0.0303)	0.0292 (0.0295)
Number of Observations	206,907	53,957	206,907

†Columns (1a), (2a), and (3a) are generated by the same model as the estimates in columns (1), (2) and (3), respectively, of Table 1**.** However, the model is estimated using only the observations for which we have individuals' fields and occupations. Columns (1b), (2b), and (3b) show the estimates when this basic model is augmented with the field and occupation variables. The baseline giving rate is 26.1 percent. The figures in columns (1a) and (1b) are generated by a probit model, and the figures in columns (2a) and (2b) are generated by OLS. Columns (3a) and (3b) combine these estimates and show the unconditional marginal effects on total giving. The figures in parentheses are standard errors; coefficients that are statistically significant at the 1% level are noted with ***, those significant at the 5% level are noted with **, and those significant at the 10% level are noted with *.Standard errors are adjusted for clustering based on individuals. In addition to the variables listed, the regressions include the variables listed in the Appendix, as well as location effects, time effects, and class effects, which are not reported for brevity. Full results are available upon request.

Table 3†
Effect of Time on the Impact of Alphabetical Placement

		(1) Probability of Making a Gift	(2) Log Amount of Gift Conditional on Giving	(3) Unconditional Effect on Giving
A to F	1983-1995	0.0142**	0.0204	0.0597**
		(0.00557)	(0.0316)	(0.0235)
	1996-2000	0.0122**	0.0585	0.0682**
		(0.00603)	(0.0363)	(0.0294)
	2001-2007	0.00851	0.0360	0.0462*
		(0.00593)	(0.0313)	(0.0279)
G to L	1983-1995	0.0135**	0.0591*	0.0637**
		(0.00541)	(0.0330)	(0.0250)
	1996-2000	0.0109*	0.0118	0.0519*
		(0.00621)	(0.0386)	(0.0303)
	2001-2007	0.00262	-0.0482	-0.00097
		(0.00610)	(0.0327)	(0.0284)
M to R	1983-1995	0.00869	0.0281	0.0390
		(0.00594)	(0.0332)	(0.0250)
	1996-2000	-0.00041	0.0512	0.00898
		(0.00623)	(0.0400)	(0.0304)
	2001-2007	-0.00966	0.0478	-0.0301
		(0.00626)	(0.0353)	(0.0296)
Number of Observations		308,857	68,575	308,857

†Column (1) shows the incremental effect on the probability of making a gift of placement in the alphabet during each of the indicated times periods. For each time period, the effect is calculated as the sum of the main effect and the interaction between that time period and the relevant section of the alphabet. Column (2) shows the same information for the logarithm of amount of the gift. The figures in column (1) are marginal effects generated by a probit model, and the figures in column (2) are generated by OLS. Column (3) combines these estimates and shows the unconditional marginal effects on total giving. The figures in parentheses are standard errors; coefficients that are statistically significant at the 1% level are noted with ***, those significant at the 5% level are noted with **, and those significant at the 10% level are noted with *. Standard errors are adjusted for clustering based on individuals. In addition to the variables listed, the regressions include the variables listed in the Appendix, as well as location effects, time effects, and class effects, which are not reported for brevity. Full results are available upon request.

Table 4†
Impact of Alphabetical Placement on Donations by Gender

	Male		
	(1a) Probability of Making a Gift	(2a) Log Amount of Gift Conditional on Giving	(3a) Unconditional Effect on Giving
A to F	0.00893 (0.00567)	0.0305 (0.0316)	0.0453* (0.0269)
G to L	0.00648 (0.00592)	0.00362 (0.0333)	0.0291 (0.0280)
M to R	-0.00247 (0.00591)	0.0440 (0.0351)	-0.00199 (0.0279)
Number of Observations	204,028	44,623	204,028

	Female		
	(1b) Probability of Making a Gift	(2b) Log Amount of Gift Conditional on Giving	(3b) Overall Effect on Giving
A to F	0.0152** (0.00723)	0.0359 (0.0362)	0.0686** (0.0317)
G to L	0.0101 (0.00736)	-0.0147 (0.0381)	0.0376 (0.0320)
M to R	0.00262 (0.00767)	0.0195 (0.0396)	0.0146 (0.0332)
Number of Observations	104,829	23,952	104,829

† Columns (1a), (2a), and (3a) are generated by the same model as the estimates in columns (1), (2) and (3), respectively, of Table 1. However, the model is estimated using only the observations on males. Columns (1b), (2b), and (3b) show the estimates when the model is estimated for females. The baseline giving rates are 21.87 percent for men and 22.85 percent for women. The figures in columns (1a) and (1b) are generated by a probit model, and the figures in columns (2a) and (2b) are generated by OLS. Columns (3a) and (3b) combine these estimates and show the unconditional marginal effects on total giving. The figures in parentheses are standard errors; coefficients that are statistically significant at the 1% level are noted with ***, those significant at the 5% level are noted with **, and those significant at the 10% level are noted with *. Standard errors are adjusted for clustering based on individuals. In addition to the variables listed, the regressions include the variables listed in the Appendix, as well as location effects, time effects, and class effects, which are not reported for brevity. Full results are available upon request.

Table 5†
Impact of Alphabetical Placement on Donations
Top 1 Percent of Gifts Dropped

	(1a) Log Amount of Gift Conditional on Giving	(2a) Amount of Gift Conditional on Giving
A to F	0.0274 (0.0226)	-2.80 (8.76)
G to L	-0.00334 (0.0240)	-1.61 (3.26)
M to R	0.0262 (0.0247)	4.94 (10.07)
Number of Observations	67,889	67,889

	(1b) Log Amount of Gift Unconditional Effect on Giving	(2b) Amount of Gift Unconditional Effect on Giving
A to F	0.0532*** (0.0205)	1.42 (2.07)
G to L	0.00354* (0.0211)	1.18 (2.20)
M to R	0.00150 (0.0212)	0.84 (2.27)
Number of Observations	67,889	67,889

†Column (1a) shows the incremental effect of placement in the alphabet on the logarithm of the amount of the gift in a given year, and column (2a) shows the effect on the level of the amount of the gift. In both columns, the largest one percent of donations are dropped and estimation is by OLS. Column (1b) uses the same specification as column (3) in Table 1, dropping the top one percent of donations, while column (2b) uses the same procedure, except with the level of the amount of the gift. The figures in parentheses are standard errors; coefficients that are statistically significant at the 1% level are noted with ***, those significant at the 5% level are noted with **, and those significant at the 10% level are noted with *. Standard errors are adjusted for clustering based on individuals. In addition to the variables listed, the regressions include the variables listed in the Appendix, as well as location effects, time effects, and class effects, which are not reported for brevity. Full results are available upon request.

Appendix

Table A1*
Variable Definitions and Summary Statistics

Variable	Description	Mean	Standard Deviation
TotalYear	Total giving for year (2007 dollars) conditional on making a gift	492.00	6399.9
LogTotalYear	Log of giving for year (2007 dollars) conditional on making a gift	4.38	1.43
Didgive	1 if any donation given in year	0.222	0.416
AtoF	1 if the first initial of the individual's last name is in A through F	0.293	0.455
GtoL	1 if the first initial of the individual's last name is in G through L	0.255	0.436
MtoR	1 if the first initial of the individual's last name is in M through R	0.227	0.419
StoZ	1 if the first initial of the individual's last name is in S through Z	0.226	0.418
Yearssince	Number of years since graduation	12.70	7.90
Yearssince2	Number of years since graduation, squared	223.6	243.5
Spouseisalum	1 if the spouse is an alumnus	0.102	0.303
Male	1 if the alumnus is male	0.661	0.473
Race/Ethnicity			
White	Omitted Category: 1 if the alumnus is White	0.786	0.410
Amerind	1 if the alumnus is a Native American	0.00429	0.0654
Black	1 if the alumnus is Black	0.0929	0.290
Hispanic	1 if the alumnus is Hispanic	0.0460	0.209
Asian	1 if the alumnus is Asian	0.0749	0.263
Secondary Schooling			
Public	Omitted Category: 1 if the alumnus attended public school	0.587	0.492
Boarding	1 if the alumnus attended boarding school	0.135	0.342
Private	1 if the alumnus attended private school	0.262	0.440
Schloth	1 if the alumnus attended another type of school	0.0208	0.143
SATmath	SAT math score. Scores prior to 1996 are adjusted to reflect re-centering of the scoring scale.	701.0	77.9
SATverbal	SAT verbal score. Scores prior to 1996 are adjusted to reflect recentering of the scoring scale.	699.7	78.4

Admissions Office "Non-Academic" Ranking			
A	Omitted Category: 1 if the alumnus received the highest non-academic ranking from the admissions office	0.0326	0.178
B	1 if the alumnus received the second highest non-academic ranking from the admissions office	0.465	0.499
C	1 if the alumnus received the third highest non-academic ranking from the admissions office	0.417	0.493
D	1 if the alumnus received the fourth highest non-academic ranking from the admissions office	0.0806	0.272
E	1 if the alumnus received the fifth highest non-academic ranking from the admissions office	0.0046	0.0677
Admissions Office "Academic" Ranking			
A	Omitted Category: 1 if the alumnus received the highest academic ranking from the admissions office	0.153	0.360
B	1 if the alumnus received the second highest academic ranking from the admissions office	0.407	0.491
C	1 if the alumnus received the third highest academic ranking from the admissions office	0.271	0.444
D	1 if the alumnus received the fourth highest academic ranking from the admissions office	0.161	0.367
E	1 if the alumnus received the fifth highest academic ranking from the admissions office	0.0090	0.0946
Clubsport	1 if the alumnus played on a club team	0.110	0.313
Honors	1 if the alumnus graduated *magna*, *summa*, or *cum laude*	0.427	0.495
Greek	1 if the alumnus was a member of a fraternity or sorority	0.647	0.478
Athlete	1 if the alumnus played a varsity sport	0.302	0.459
Major			
Molbio	Omitted Category: 1 if the alumnus majored in molecular biology	0.0215	0.145
Small Social Science	1 if the alumnus majored in Anthropology, Urban Studies, or Sociology.	0.0340	0.181
English	1 if the alumnus majored in English	0.113	0.317
Economics	1 if the alumnus majored in Economics	0.0707	0.256
Public Policy	1 if the alumnus majored in Public Policy	0.0545	0.227
Political Science	1 if the alumnus majored in Political Science	0.0883	0.284
Psychology	1 if the alumnus majored in Psychology	0.0534	0.225

History	1 if the alumnus majored in History	0.113	0.317
MAE	1 if the alumnus majored in Mechanical/Aerospace Engineering	0.0330	0.179
EE/CS	1 if the alumnus majored in Electrical Engineering or Computer Science	0.0553	0.229
Arch & Civ	1 if the alumnus majored in Architecture or Civil Engineering	0.0659	0.248
Small Humanities	1 if the alumnus majored in Art, Art History, Classics, East Asian Studies, Linguistics, Music, Near Eastern Studies, Philosophy, Religion, or Languages and Literature departments	0.128	0.335
Small Engineering	1 if the alumnus majored in "Engineering", Operations Research and Financial Engineering, or Chemical Engineering	0.0274	0.163
Small Sciences	1 if the alumnus majored in Applied Mathematics, Astrophysics, Biochemistry, Biology, Chemistry, Ecology and Evolutionary Biology, Geology, Mathematics, Physics, or Statistics	0.141	0.348
Minor			
No Minor	Omitted Category: 1 if the alumnus received no minor	0.778	0.416
African/African-American Studies	1 if the alumnus received a minor in African or African-American Studies	0.0306	0.172
American Studies	1 if the alumnus received a minor in American Studies	0.0196	0.139
Latin	1 if the alumnus received a minor in Latin	0.0019	0.0433
Finance	1 if the alumnus received a minor in Finance	0.0025	0.0501
Theater	1 if the alumnus received a minor in Theater	0.0151	0.122
Public Policy	1 if the alumnus received a minor in Public Policy	0.0473	0.212
Other Engineering	1 if the alumnus received a minor in Architecture, Basic Engineering, Bioengineering, Electrical Engineering, Geological Engineering, Management, Materials Sciences, or Robotics.	0.0164	0.127
Other Sciences	1 if the alumnus received a minor in Applied and Computational Mathematics, Biophysics, Cognitive Studies, Environmental Studies, Science in Human Affairs, or Neuroscience.	0.0255	0.158
Other Humanities	1 if the alumnus received a minor in a humanities field	0.0526	0.223
Teaching	1 if the alumnus received a teaching certificate	0.0241	0.153
Reunion	1 if the year after graduation is some multiple of 5	0.179	0.383
Post Baccalaureate Education			
NoPostAB	Omitted Category: 1 if the alumnus has no advanced degree	0.663	0.473
PhD	1 if the alumnus has a Ph.D. or equivalent degree	0.0636	0.244
Masters	1 if the alumnus has a masters	0.124	0.329

JD	1 if the alumnus has a JD	0.0865	0.281
MDDDS	1 if the alumnus has a medical degree	0.0504	0.218
MBA	1 if the alumnus has an MBA	0.0684	0.252
*Field***			
Arts	1 if the alumnus ever worked in the Arts field	0.0705	0.256
Agriculture	1 if the alumnus ever worked in the Agriculture field	0.0023	0.0477
Architecture	1 if the alumnus ever worked in the Architecture field	0.0284	0.166
Pharmaceuticals	1 if the alumnus ever worked in the Pharmaceuticals field	0.0228	0.149
Communications	1 if the alumnus ever worked in the Communications field	0.103	0.303
Consulting	1 if the alumnus ever worked in the Consulting field	0.0947	0.293
Education	1 if the alumnus ever worked in the Education field	0.122	0.327
Finance	1 if the alumnus ever worked in the Finance field	0.174	0.379
Health Care (Business/Industry)	1 if the alumnus ever worked in the Health Care field	0.170	0.376
Hospitality	1 if the alumnus ever worked in the Hospitality field	0.0051	0.0715
Information Technology	1 if the alumnus ever worked in the IT field	0.115	0.319
Law	1 if the alumnus ever worked in the Law field	0.187	0.390
Manufacturing	1 if the alumnus ever worked in the Manufacturing field	0.0690	0.253
Retail	1 if the alumnus ever worked in the Retail field	0.0230	0.150
Transportation	1 if the alumnus ever worked in the Transportation field	0.0092	0.0952
Federal Government	1 if the alumnus ever worked for the Federal Government	0.04406	0.2052
State Government	1 if the alumnus ever worked for a State Government	0.0440	0.205
Foreign Government	1 if the alumnus ever worked for a Foreign Government	0.0039	0.0622
Nongovernmental Organization	1 if the alumnus ever worked in the NGO field	0.0315	0.175
Religion	1 if the alumnus ever worked in the Religion field	0.0119	0.109
Other	1 if the alumnus ever worked in another field	0.276	0.447
Multilateral Organization	1 if the alumnus ever worked in the Multilateral Organization field	0.0022	0.0474
Military	1 if the alumnus ever worked for the Military	0.0072	0.0846

*Occupation***			
Government Worker	1 if the alumnus ever worked as a government worker	0.0107	0.103
Miscellaneous Worker	1 if the alumnus ever worked in some miscellaneous occupation	0.0891	0.285
Physician/Dentist	1 if the alumnus ever worked as a physician or dentist	0.133	0.339
White Collar	1 if the alumnus ever worked in a white collar occupation	0.300	0.458
Attorney	1 if the alumnus ever worked as an attorney	0.261	0.439
Executive	1 if the alumnus ever worked as an executive	0.483	0.500

*Figures are based on 308,857 observations on gift-giving from 1983 to 2007. 30,148alumni who graduated from 1972 to 2005, excluding the classes of 1993, 1994, and 1996, are represented.
**Figures for the field and occupation samples are based on 206,907 observations on 19,737 alumni.