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Abstract

We present evidence against the standard assumptions that social preferences are stable and can be measured in a reliable, nonintrusive manner. Researchers often measure social preferences by posing dictator type allocation decisions. The Social Value Orientation (SVO) is a particular sequence of dictator decisions. We vary the order in which the SVO and a larger stakes dictator game are presented. In our first study, we find that prosocial subjects act even more prosocially when the SVO is administered first, whereas selfish subjects are unaffected by the order. We also find that, among subjects with consistent responses on the SVO measure, the subjects who first receive the SVO are more generous in the dictator game than are such subjects who receive the SVO last. In our second study, we vary the order of the SVO and a nonstandard dictator game. We find evidence across all subjects that those who first receive the SVO are more generous in the dictator game but we do not find the effect among only the generous subjects. We again find that subjects with a perfectly consistent SVO measure are more generous when the SVO is given first. Although we cannot determine whether the timing affects preferences or the measure of preferences, our results are incompatible with the assumptions that social preferences are stable and can be measured in a reliable, nonintrusive manner.

JEL classification: C91, D64, Z13
Keywords: experimental economics, altruism, dictator game, social value orientation

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1 Introduction

It is commonly assumed that subjects have stable preferences over outcomes. It is also commonly assumed that standard techniques to measure these preferences are reliable and can be performed in a nonintrusive manner. If these two assumptions hold then the order in which we perform the measurement of preferences should not matter. However, we present evidence which challenges these assumptions.

It is significant if a systematic violation of these assumptions is found. Measures of preferences are of interest primarily because they are helpful in making predictions regarding behavior. However, if the outcome of a measurement can affect future outcomes, either because preferences are not stable or because the measure is not reliable, then the value of the measure is diminished.

In order to investigate whether the timing of the measurement can affect the outcome of the measures, we offer an extremely simple experimental setup: we offer subjects two standard measures but vary the order of their presentation. One might be tempted investigate these timing issues with a measure of social preferences and play in a strategic game (for instance, the prisoner's dilemma). However, if the experimenter observed that the relationship between the measure and behavior in the game is affected by the order in which the items are given, this difference is not exclusively attributable to the timing of the measure. This is because behavior in a strategic game is not exclusively a function of preferences but also, for instance, expectations regarding the behavior of others. Therefore, rather than directing subjects to play a strategic game, we offer two commonly-used measures of social preferences, and vary the order in which they are presented to the subjects. By doing this, we are confident that the effects which we find are not due to the more complicated features involved in the play of a strategic game.

It has been known for some time that many subjects do not simply maximize their own material payoffs.[1] Specifically, it is often observed that some subjects will sacrifice their own material payoffs so that other subjects will receive a better material outcome. Researchers

[1] For an early example, see Deutsch (1958).

often attempt to infer the nature of these social preferences by posing a series of allocation decisions, often referred to as dictator games. These decisions entail a choice of an allocation of hypothetical or material outcomes distributed between the subject and another subject. One measurement technique is to simply pose a dictator game to a subject. Another measurement technique, which involves a specific sequence of dictator games, is Social Value Orientation (SVO).

In our experiment, we vary the order of the SVO and a standard, lager stakes dictator game.[2] While we find that SVO outcomes are significantly related to outcomes in the dictator game, we also find that the mappings between these outcomes are related to the order in which they are given. Specifically, we find that the subjects, for whom the SVO indicates prosocial preferences, act even more prosocially in the larger stakes dictator game when the SVO is administered first. By contrast, we find that the subjects for whom SVO indicates selfish preferences are unaffected by the order. We also find that subjects with a perfectly consistent SVO measure are more generous in the dictator game when they are first given the SVO measure.

To better understand these results we run an identical experiment, with the exception that the dictator game exhibits a relative price of each allocation of 1-to-3, rather than the standard 1-to-1. In other words, each \$0.50 kept by the subject reduces the recipient's payoffs by \$1.50. In this case, we find no significant difference between the prosocials who complete the SVO before the dictator game and the prosocials who complete the SVO after the dictator game. However, across all subjects we find that those who first complete the SVO are more generous in the dictator game than subjects who complete the SVO last. Further, we find that this effect is stronger when we restrict attention to those with a perfectly consistent SVO measure.

Given the results of our experiment, we are unable to distinguish between the explanation that the measurement affects the social preferences of the subject or that the measure affects the subsequent performance of another measure. Although we cannot distinguish between

[2] For more on dictator games, see Forsythe et al. (1994), Hoffman et al. (1994), Eckel and Grossman (1996), Ruffle (1998) and Bolton et al. (1998).

these two explanations, we can conclude that, given the assumptions commonly applied to experiments, we should not observe the behavior found in this experiment. The results of our experiment suggest that standard techniques of measuring social preferences cannot be executed in a reliable and nonintrusive manner. Further, as we have uncovered a systematic relationship between the treatment, the action of the subjects and the measure, we therefore describe our results as *endogenous* rather than unstable.

1.1 Social Value Orientation as a Measure of Social Preferences

We use SVO because it is relatively easy to administer and interpret. The specific technique which we use, adapted from Van Lange et al. (1997), consists of 9 items with three possible choices involving material payoffs accruing to the subject and another subject.[3] Each of the nine items has an *individualistic* response, a *prosocial* response and a *competitive* response. The individualistic response is the one in which the material payoffs accruing to oneself are the largest. In other words, selecting the individualistic choice suggests that the subject neither positively nor negatively values the material payoffs accruing to the other subject. The prosocial response is the one in which the sum of the material payoffs accruing to both the subject and the other subject is the largest. In other words, selecting the prosocial response suggests that the subject positively values the material payoffs accruing to the other subject. The competitive response is the one in which the difference between the material payoffs accruing to the subject and the other subject is the largest. In other words, selecting the competitive choice suggests that the subject negatively values the material payoffs accruing to the other subject.

Translated into a utility function, SVO measures the form of $u(x_{own}, x_{other})$ where x_{own} is the material payoff accruing to self and x_{other} is the material payoff accruing to another person. A prosocial choice indicates that $\frac{\partial u}{\partial x_{other}} > 0$, an individualistic choice suggests that $\frac{\partial u}{\partial x_{other}} = 0$ and a competitive choice suggests that $\frac{\partial u}{\partial x_{other}} < 0$.

Further, there is much written on the stability of SVO. For instance, Bogaert et al. (2008) suggest that over the 40 years since its introduction by Messick and McClintock (1968), SVO

[3] See the appendix for a complete description of the SVO items which we use.

has been widely regarded as providing a stable measure of a personality trait. However, recent work has suggested instances where SVO can be affected by the setting and is thereby a less than perfectly stable measure. Iedema and Poppe (1994) show that the measurement of SVO can be affected by self-presentation effects. Smeesters et al. (2003) show that priming certain types of behavior can lead to a different mapping from SVO to behavior.[4] While SVO is considered relatively stable, to our knowledge there is no work suggesting that outcomes of SVO can affect subsequent outcomes.

It is obviously problematic that the timing of the measurement of preferences might affect the relationship between the measure and behavior related to the measure. A measure is primarily useful to the extent that it can form a basis for making predictions about behavior.[5] When behavior and the measure of preferences are functions not exclusively of preferences then the usefulness of the measure is somewhat degraded.

SVO also appears in the economics literature.[6] However each of these papers uses the ring measure (Griesinger and Livingston, 1973), which is slightly different than the technique which we employ. The ring measure consists of 24 pair-wise items rather than 9 items with 3 responses.[7] However, similar to the technique which we employ, the ultimate objective is to classify subjects on the basis of their social preferences. Relatively little is known about the relationship between the ring measure and the measure which we employ (Bogaert et al., 2008). However, we opt for the latter as it requires fewer responses and, in our opinion, is more transparent. As a result, we conjecture that the effects which we find would only be strengthened by the use of the ring measure.

Finally, measuring social preferences via dictator games, like SVO, has the advantage that it only considers a situation where strategic issues are absent. Although all decisions would be made in the absence of the feedback of the actions of other dictators, it still remains possible

[4] Also see Au and Kwong (2004) and Hertel and Fiedler (1994, 1998).

[5] SVO has been used to study behavior in games (Parks, 1994; Kramer et al., 1986; Pruyn and Riezehos, 2001), the decision to use public transportation (Van Vugt et al., 1996), proenvironmental behavior (Cameron et al., 1998; Joireman et al., 2001) and volunteerism (McClintock and Allison, 1989).

[6] See Buckley et al. (2001), Buckley et al. (2003), Burlando and Guala (2005), Carpenter (2003), Carpenter (2005), Cornelissen et al. (2007), Kanagaretnam et al. (2009) and Offerman et al. (1996).

[7] Sonnemans et al. (2006) uses a visual representation of the ring whereby the subject selects their location on the *ring* with a single click rather than responding to 24 items.

that the subject would anticipate some implicit reciprocal arrangement. Therefore, similar to Carpenter (2005), we employ a triadic design whereby each dictator decides an allocation involving self and another dictator. This other dictator does not decide on an allocation involving the original dictator but rather on a third dictator.

1.2 Other Measures of Social Preferences

Another commonly used social preference measurement technique was developed by Andreoni and Miller (2002). SVO is similar to this technique in that both pose a series of dictator games however there remain important differences. In Andreoni and Miller, choice is much less restricted than in SVO. Each SVO item has only three possible responses, whereas in Andreoni and Miller each item seeks an allocation of tokens ranging from 40 to 100. As a result, Andreoni and Miller yields less coarse data than does SVO. However, the choice in Andreoni and Miller is less transparent than SVO, as the latter explicitly lists the material allocation of each choice. We are not aware of a study which compares the relative merits of SVO and that proposed by Andreoni and Miller.

Charness and Rabin (2002) pose a series of simple games to learn the specific form of social preferences related to relative wealth and reciprocity.[8] The nature of the social preferences might depend on whether other's payoffs are higher than or lower than the subject's own payoffs, therefore Charness and Rabin vary this aspect of their items. By contrast, in SVO the subject decides among choices where monetary payoffs accruing to oneself are never less than that accruing to the other subject. Also, in contrast the technique employed in Charness and Rabin, SVO is not equipped to evaluate preferences for reciprocity.

1.3 Endogenous Social Preferences and Behavioral Spillovers

Consider the relationship between our paper and research on endogenous social preferences. For instance, Carpenter (2005) and Canegallo et al. (2008) investigate how the strategic

[8] Chen and Li (2009) perform a similar type of analysis when considering the type, or identity, of the other subject.

environment can affect preferences.[9] Also, Güth et al. (2008) find that subjects who contribute more in a public goods game are significantly more trusting in a subsequent investment game. By contrast, we study whether the decision in a commonly used measure of social preferences can affect the outcome of a subsequent measure of social preferences.[10] Further, as we find a systematic relationship, in our view the results are best described as endogenous.

There also exists a strand of literature which examines the role of the environment on play in games. For instance, Bednar et al. (2011) describe an experiment in which subjects simultaneously play two distinct games with different opponents. The authors find that behavior in a particular game is affected by corresponding paired game.[11] This literature contends that strategies which are used in one game are often applied to the other, despite that the games should be played independently. The authors examine these behavioral spillovers but, unlike the present paper, they do not directly measure preferences.

Borgloh et al. (2010) is perhaps closest to our paper. The authors describe an experiment where subjects are given an unfamiliar measure of altruism and a familiar measure, where the authors vary the order of the measures. The authors find evidence that the order affects the behavior in the unfamiliar task but not in the familiar task. Likewise, we vary the order of tasks and examine the differences in behavior.

1.4 Framing Effects

The present paper shares some similarities with the framing effects literature. For instance, it has been found that the there can be systematic differences in the response to questions based on how the questions are framed (Tversky and Kahneman, 1981).[12] Like the framing literature,

[9] Schotter et al. (1996) examines the effect of framing on judgements of fairness and is therefore related to endogenous preferences. Eckel and Grossman (2005) find that a strong identity manipulation can induce more cooperation in public goods game. Also, see Bowles (1998), Poulsen and Poulsen (2006) and Isoni et al. (2011) for more on endogenous preferences.

[10] Brosig et al. (2007) examine the stability of social preferences across an extended period of time and find evidence of stability only among selfish subjects. Blanco et al. (2011) do not find evidence of stability of social preferences across simple games. In contrast, de Oliveira et al. (2008) find evidence of consistency between altruistic behavior in the field and in the laboratory.

[11] Also see Bednar and Page (2007), Crawford and Broseta (1998), Savikhina and Sheremeta (2009) and Van Hyuck et al. (1993).

[12] For more on the framing effects, see Frisch (1993). For evidence that framing effects can occur in subjects where one would expect otherwise, see Gächter et al. (2009).

the present paper appears to provide evidence against the assumptions that preferences are stable and can be measured in a reliable, nonintrusive manner. However, unlike the framing literature, the effects which we find persist after the initial "frame" and this persistence seems to be based, at least in part, upon the actions of the subject. Specifically, in Study 1 we find that prosocial subject are affected by the timing and in both studies we find that the consistent subjects are affected by the timing.

2 Study 1

2.1 Overview

We seek to test whether outcomes of a measure of social preferences can affect subsequent measurements. Therefore, we direct subjects to complete the SVO and make an allocation in a standard dictator game, however we vary the order in which these are given to the subjects.

2.2 Procedure

A total of 95 students enrolled in economics classes at a university in the northeastern United States participated. The study was conducted in 5 classes of 16, 20, 39, 12, and 8 subjects.[13] The responses were entered on paper. The subjects were given course credit for attendance and were told that that a randomly selected 25% from each session would be paid the amount earned in the experiment. The subjects completed the SVO and decided on an allocation in a standard $10 dictator game. The allocation of the $10 was presented in $0.25 increments. The subjects were directed to indicate which of the 41 dictator game allocations they most preferred. See Appendix 1 for the format of the dictator game.

The subjects were aware of the triadic design as they were told to make allocation decisions involving themselves ("You") and another subject ("Other1"). Another subject ("Other2") was to make allocations involving Other2 and You. Therefore, the amount accruing to each subject was what was kept in the You-Other1 allocation decisions plus what Other2 did not keep in the Other2-You allocation decisions. In both the SVO and dictator game, the status of

[13] We exluded a single subject because the subject did not complete the study.

You, Other1 and Other2 remained fixed. This description of the triadic design was provided verbally by the same male experimenter and in written form given to each subject. The written instructions are provided in Appendix 1.[14]

The SVO entailed the exact nine items from Van Lange et al. (1997). The subjects were presented with three items on each of three pages. In Van Lange et al., the subjects decide on an allocation of points which carry no financial implications. By contrast, in our experiment subjects are offered a conversion rate of points to money, whereby the subject is effectively deciding on an allocation of a small amount of money. Across all 9 SVO items, the subject could keep as little as \$0.94 and as much as \$1.06. Also across the SVO items, the subject could send as little as \$0.19 and send as much as \$0.94. The subjects were not told these amounts, however they could be easily calculated. The SVO items and the conversion from points to money are given in Appendix 1.

Within each of the 5 classes, approximately half of the subjects answered the SVO items then made a choice in the dictator game. We refer to this treatment as SVO First. Approximately half of each class responded to the dictator game then answered the SVO items. We refer to this treatment as SVO Last. Within each session, we randomly assigned subjects into one of these two treatments.

The subjects completed the experiment without feedback. In other words, each subject completed the experiment without knowing what the other subjects have selected. Finally, we note that have data on the gender and age of the subjects.

2.3 Results

In this study, the amount kept by the subjects, which is the sum of the amount kept in the SVO and the amount kept in the dictator game, ranged from \$0.94 to \$11.06, with an average of \$7.09. The total amount accruing to the subjects, which is the sum of what was kept by

[14] The triadic design does not require that each session has a number of subjects which is divisible by three. Within each session, every subject was assigned a subject identification number. After the session, we randomly selected a number between 1 and the number of subjects in the session minus one. We then matched each subject with an Other1 by finding the subject with an identification number which is equal to the original identification number plus the random number. The Other2 was determined to be the subject with the next highest identification number as the Other1. In this way, each subject could be assigned a unique Other1 and Other2, without requiring that the data occur in multiples of three.

the subject and what was sent by Other2, ranged from \$2.51 to \$21.93, with an average of \$11.69 and standard deviation of 3.56. Female participants accounted for 37% of the subjects. The average age was 21.8 with a standard deviation of 5.56. Also note that we do not find significant differences in the amount kept in the dictator game or in the SVO classification among the five sessions.

Using the procedure of Van Lange et al. (1997), we categorized 31 subjects (33%) as prosocials, 39 subjects (41%) as individualists and 5 subjects (5%) as competitors. There were 20 subjects (21%) who we could not classify as they did not select a minimum of 6 choices of a particular type. Table 1 summarizes the distribution of subjects according to SVO categorization and the treatment.

	Prosocial	Individualistic	Competitive	Uncategorized	Total
SVO First	14	24	3	8	49
SVO Last	17	15	2	12	46
Total	31	39	5	20	95

Table 1: Number of subjects by SVO categorization and treatment

As one would expect, there is a significant relationship between the SVO measure and choice in the dictator game. The prosocial subjects ($M = 4.67$, $SD = 1.63$) kept significantly less than did the proself (individualists and competitors) subjects ($M = 7.28$, $SD = 2.48$), $t(73) = 5.13$, $p < 0.01$.

An SVO measure equaling 9 indicates perfect consistency in the set of responses and a measure of 6, 7, or 8 indicates a less than perfectly consistent set of responses. See Table 2 for the amount kept in the dictator game by SVO classification and consistency.[15]

	Prosocial	Individualistic
SVO of 9	4.31	7.95
	(1.69)	(2.12)
SVO of 6, 7, or 8	5.89	6.12
	(0.45)	(2.11)

[15] See Bohnet and Frey (1999) and Cox and Sadiraj (2011) for other papers with dictator game choices in which some subjects kept less than 50%.

Table 2: Average amount kept in dictator game by SVO classification and consistency of measurement with standard deviation in parentheses

Among those classified as prosocial, those with a measure equal to 9 (24 subjects) kept a significantly smaller share than those with a measure of 6, 7, or 8, $t(29) = 2.44$, $p = 0.021$. Also, among the subjects classified as individualistic, those with a measure equal to 9 (26 subjects) kept a significantly larger share than those with a measure of 6, 7, or 8, $t(37) = 2.55$, $p < 0.001$. Therefore, we are reasonably confident of the relationship between choice in the SVO and choice in the dictator game.

We now compare dictator allocations given the treatment. First, the difference between the amount kept in the SVO First treatment ($M = \$6.04$, $SD = 2.89$) and in the SVO Last treatment ($M = \$6.16$, $SD = 2.40$) is not significant, $t(94) = 0.23$, $p = 0.41$. However, a significant relationship emerges when one looks within SVO classifications. See Table 3 for a summary of the amounts kept in the dictator game by SVO classification and treatment.

	Prosocial	Individualistic
SVO First	4.14	7.38
	(2.28)	(2.23)
SVO Last	5.10	7.28
	(0.55)	(2.40)

Table 3: Average amount kept in dictator game by SVO classification and treatment with standard deviation in parentheses

We run regressions with a dependent variable of the amount kept in the dictator game. Since this variable is bounded, we use the tobit regressions, with an upper bound of 10 and a lower bound of 0.[16] We also employ the Prosocial Dummy which takes a value of 1 if the subject was classified as a prosocial by SVO and 0 otherwise. We use a prosocial dummy because subjects determined to have individualistic or competitive preferences would imply identically selfish behavior in the dictator game.

When the analysis includes subjects of each SVO classification, the SVO treatment is not significantly related to the amount kept in the dictator game. As a result, we perform the

[16] Note that Borgloh et al. (2010) also use tobits in order to account for the bounded choice data.

following analysis while restricting attention to a subset of the subjects. In regressions (1) and (2) we restrict attention to the 31 subjects who are classified as prosocial. In regressions (3) and (4) we restrict attention to the subjects with an SVO prosocial measure of 9. Finally, in regressions (5) and (6) we restrict attention to the subjects with an SVO measure equaling 9. Note that this outcome indicates perfect consistency for prosocials, competitors or individualists. See Table 4 for a summary of the analysis.

	(1)	(2)	(3)	(4)	(5)	(6)
SVO First	-1.08^{*}	-0.942^{*}	-1.42^{**}	-1.27^{*}	-1.48^{*}	-1.47^{*}
	(0.613)	(0.567)	(0.715)	(0.690)	(0.869)	(0.835)
Prosocial Dummy	–	–	–	–	-4.79^{***}	-4.69^{***}
					(0.883)	(0.847)
Female	–	1.320^{**}	–	1.44^{**}	–	1.71^{**}
		(0.601)		(0.720)		(0.860)
Age	–	-0.119	–	-0.0935	–	0.0199
		(0.0786)		(0.0910)		(0.0730)
Observations	31	31	24	24	53	53
Log Likelihood	-58.92	-56.23	-45.51	-43.52	-106.04	-103.96

Table 4: Results of tobit regressions with amount kept in the dictator game as dependent variable. The tobit regressions were performed with an upper bound of 10 and a lower bound of 0. Note that *** indicates significance at $p < 0.01$, ** indicates significance at $p < 0.05$, and * indicates significance at $p < 0.1$.

We first note that the SVO First coefficient is significant at the 0.1 level in regressions (1), (2), (4), (5), (6), and significant at the 0.05 level in regression (3). The estimates in regressions (1)-(4) suggest that the prosocial subjects who are first given the SVO, are more generous in the dictator game. The estimates in regressions (5) and (6) suggest that all subjects with a perfectly consistent measure on the SVO act more generously in the dictator game.

We note that we did not list the analysis which includes the interaction between the SVO First and Prosocial Dummy because in these specifications the estimate is not significant. We also note interesting results related to generosity of the gender coefficient. Regressions (2), (4) and (6) suggest that within these subsets, female participants can be less generous than male subjects. When one performs the analysis for all data points, the female coefficient is no longer significant.

3 Study 2

3.1 Overview

Roughly, Study 1 finds that prosocial subjects act even more prosocially in the dictator game when the SVO items are administered first. We also find that subjects who are first given the SVO and have a perfectly consistent measure are more generous in the dictator game than are subjects who are given the SVO last and have a perfectly consistent measure. Based on the data available from Study 1, it is not clear to us what drives this result. As there is no choice involving the creation of surplus in the standard dictator game, it is possible that the creation of surplus by the prosocial subjects in the SVO First treatment predisposes them to be more generous in the dictator game when compared to prosocials in the SVO Last treatment. If this was the case, and if the dictator game was designed so that the dictator game decided the amount of surplus then the results in the SVO Last treatment would converge to that of the SVO First treatment. However, it is also possible that with the standard dictator game, being selfish is too *easy* and so the individualists are not affected by the timing. If this is the case, and if the dictator game is designed in a manner in which being selfish is more costly then we expect a divergence of the results of the SVO First and Last treatments of the individualists. In Study 2, we hope to to shed some light on the relative merit of these two explanations.

Study 2 follows the same procedure as Study 1 with the exception that, rather than using a standard dictator game, we use a dictator game in which the relative allocation *price* is 1-to-3. In other words, the most selfish allocation is \$10 to self and \$0 to other and the most generous allocation is \$0 to self and \$30 to other. This nonstandard dictator game has the advantages that the amount of total surplus is a matter of choice and being selfish is relatively more costly.

3.2 Procedure

A total of 90 students in economics classes at a university in the northeastern United States participated. Study 2 was conducted in 4 classes of 21, 42, 16 and 11 subjects. The procedures in Study 2 are identical to that in Study 1 with the exception of the form of the dictator game.

Rather than the standard dictator game, in which the trade-off between own payoffs and other payoffs is 1-to-1, the dictator game used in Study 2 has a trade-off of 1-to-3. In other words, to increase the amount kept by \$0.50, the subjects must reduce the amount sent to the other subject by \$1.50. The subject's own payoffs were listed in \$0.50 increments and the other subject's payoffs were listed in \$1.50 increments. The subjects were directed to indicate which of the 21 dictator game allocations they most preferred. See Appendix 1 for the format of this dictator game.

3.3 Results

In this study, the amount kept by the subjects ranged from \$0.94 to \$11.06, with an average of \$8.17. The total amount accruing to the subjects ranged from \$1.13 to \$42.00, with an average of \$17.36 and standard deviation of 9.23. Also note that we do not find significant differences in the amount kept in the dictator game or in the SVO classification among the four sessions. Finally, we do not have data on the gender or age of a single subject. As a result, any analysis employing these variables will contain one fewer observation than the analysis without. Female participants accounted for 34% of the subjects. The average age was 22.4 with a standard deviation of 4.86.

Again using the procedure of Van Lange et al. (1997), we categorized 44 subjects (49%) as prosocials, 34 subjects (38%) as individualists and 4 subjects (4%) as competitors. There were 8 subjects (9%) who we could not classify as they did not select a minimum of 6 choices of a particular type. Table 5 summarizes the distribution of subjects according to SVO categorization and the treatment.

	Prosocial	Individualistic	Competitive	Uncategorized	Total
SVO First	21	16	2	5	44
SVO Last	23	18	2	3	46
Total	44	34	4	8	90

Table 5: Number of subjects by SVO categorization and treatment

Similar to Study 1, we find a significant relationship between choice in the SVO and

choice in the dictator game. The prosocial subjects ($M = 6.44$, $SD = 2.79$) kept significantly less than did the proself (individualists and competitors) subjects ($M = 8.28$, $SD = 2.33$), $t(80) = 3.20$, $p = 0.002$. As in Study 1, we find that the consistency of the SVO is related to the choice in the dictator game. See Table 6 for the amount kept across both treatments by the consistency of the SVO.

	Prosocial	Individualistic
SVO of 9	5.97	8.38
	(3.11)	(2.37)
SVO of 6, 7, or 8	7.46	7.94
	(1.57)	(2.53)

Table 6: Average amount kept in dictator game by SVO classification and consistency of measurement with standard deviation in parentheses

Among those classified as prosocial, subjects with a measure equal to 9 (30 subjects) kept a smaller share than subjects with a measure of 6, 7, or 8, $t(42) = 1.70$, $p = 0.097$. However, in contrast to Study 1, among those classified as individualistic, subjects with a measure equal to 9 (17 subjects) did not keep a significantly different amount than subjects with a measure of 6, 7, or 8, $t(32) = 0.52$, $p = 0.60$.

Finally, we may ask whether the timing matters for dictator game in Study 2. See Table 7 for the amount kept in the dictator game by SVO classification and treatment.

	Prosocial	Individualistic
SVO First	5.98	8.00
	(3.18)	(2.54)
SVO Last	6.87	8.31
	(2.36)	(2.38)

Table 7: Average amount kept in dictator game by SVO classification and treatment with standard deviation in parentheses

Unlike in Study 1, here we find evidence that the order treatment is related to the amount kept in the dictator game across all subjects in Study 2. We perform tobit regressions, similar

to the analysis summarized in Table 4, across all subjects. Again note that the tobit regressions employed an upper bound of 10 and a lower bound of 0. Regression (1) excludes the demographic data (gender and age) and regression (2) includes these variables. This analysis is summarized in Table 8.

	(1)	(2)
SVO First	-1.45^{*}	-1.56^{**}
	(0.758)	(0.757)
Prosocial Dummy	-2.19^{***}	-2.28^{***}
	(0.761)	(0.760)
Female	–	1.06
		(0.797)
Age	–	0.0187
		(0.0761)
Observations	90	89
Log Likelihood	−201.31	−198.01

Table 8: Results of tobit regressions with amount kept in the dictator game as dependent variable. The tobit regressions were performed with an upper bound of 10 and a lower bound of 0. Note that *** indicates significance at $p < 0.01$, ** indicates significance at $p < 0.05$, and * indicates significance at $p < 0.1$.

We note that the SVO First variable is significant at 0.05 in regression (1) and significant at 0.01 in regression (2). This provides evidence that subjects who are first given the SVO measure are more generous in the dictator game than are subjects who are given the SVO measure last. We contrast the results summarized in Table 8 with that in Study 1. In Study 1 we did not find a relationship across all subjects between the order treatment and behavior in the dictator game.

Next we perform a similar analysis to that summarized in Table 4, where we run tobit regressions on subsets of the Study 2 data. Regressions (1) and (2) are restricted to subjects who were classified as prosocial. In regressions (3) and (4) we restrict attention to subjects with an SVO prosocial measure of 9. Finally, regressions (5) and (6) restrict attention to the subjects with an SVO measure of 9. These results are summarized in Table 9.

	(1)	(2)	(3)	(4)	(5)	(6)
SVO First	−1.04	−1.22	−1.34	−2.03	−2.07*	−2.92***
	(0.999)	(0.974)	(1.44)	(1.41)	(1.10)	(1.12)
Prosocial Dummy	–	–	–	–	−3.84***	−3.89***
					(1.15)	(1.11)
Female	–	1.97*	–	2.73*	–	1.74
		(1.009)		(1.49)		(1.19)
Age	–	0.0303	–	0.162	–	0.221
		(0.0815)		(0.178)		(0.149)
Observations	44	43	30	29	49	48
Log Likelihood	−104.18	−99.90	−72.06	−67.30	−106.89	−101.72

Table 9: Results of tobit regressions with amount kept in the dictator game as dependent variable. The tobit regressions were performed with an upper bound of 10 and a lower bound of 0. Note that *** indicates significance at $p < 0.01$, and * indicates significance at $p < 0.1$.

First, we note that the SVO First coefficient is not significant in regressions (1)-(4). This implies that, unlike the results in Study 1, here we do not find evidence that the order treatment affects behavior in the dictator game among the prosocial subjects. However, as we do in Study 1, we find a relationship among those with a perfectly consistent SVO measure, between the order treatment and generosity in the dictator game. Indeed, this variable is significant at 0.01 in regression (6).

We note that the interaction terms between the SVO First variable and the SVO outcomes are not significant. We also have performed an analysis similar to that in regressions (1)-(4) but with individualistic subjects. One might have expected the SVO First variable to be significant however we do not find a significant relationship.

4 Pooled Data

Here we analyze the pooled data obtained in Study 1 and 2. While Study 1 and Study 2 were conducted at different times and on different subjects, their procedures are identical with the exception of the format of the dictator game. In order to account for these differences in the dictator game, we employ the fraction of money kept in the dictator game as the dependent variable. In each of the regressions below, we include a dummy variable, Normal Dictator

game which obtains a value of 1 for the dictator game used in Study 1 and 0 otherwise. We also use the interaction between the Normal Dictator variable and the SVO First variable. In regressions (1) and (2) we perform the analysis on all of the subjects in both studies. Regressions (3) and (4) restrict attention to the prosocial subjects in both studies. Finally, in regressions (5) and (6) we restrict attention to subjects with a perfectly consistent SVO measure. This analysis is summarized in Table 10.

	(1)	(2)	(3)	(4)	(5)	(6)
SVO First	-0.137^{*}	-0.144^{**}	-0.0749	-0.0879	-0.191^{**}	-0.242^{***}
	(0.0713)	(0.0716)	(0.0693)	(0.0687)	(0.0918)	(0.0927)
Prosocial Dummy	-0.279^{***}	-0.284^{***}	–	–	-0.462^{***}	-0.463^{***}
	(0.0515)	(0.0516)			(0.0667)	(0.0655)
Female	–	0.0519	–	0.119^{**}	–	0.139^{**}
		(0.0520)		(0.0548)		(0.0685)
Age	–	0.004324	–	-0.00128	–	0.00707
		(0.00493)		(0.00514)		(0.00713)
Normal Dictator	-0.0164	-0.0151	0.0388	0.0364	0.0273	0.00334
	(0.0707)	(0.0710)	(0.0728)	(0.0716)	(0.0922)	(0.0908)
Normal Dictator	0.0817	0.0867	-0.0408	-0.0274	0.0447	0.0902
-SVO First Interaction	(0.0993)	(0.0998)	(0.108)	(0.106)	(0.128)	(0.127)
Observations	185	184	75	74	102	101
Log Likelihood	-95.04	-94.14	-11.22	-9.35	-48.24	-45.22

Table 10: Results of tobit regressions with fraction kept in the dictator game as dependent variable. The tobit regressions were performed with an upper bound of 10 and a lower bound of 0. Note that *** indicates significance at $p < 0.01$, ** indicates significance at $p < 0.05$, and * indicates significance at $p < 0.1$.

We note that the SVO First coefficient is significant across subjects in both studies. In particular, the results of regressions (1) and (2) suggest that subjects who first receive the SVO are more generous in the dictator game than are subjects who receive the SVO last. We also note that, when restricting attention to only prosocial subjects, the SVO First variable is not significant. Next we note that the SVO First variable is significant at 0.05 in regression (5) and significant at 0.01 in regression (6). Finally, we note that there is not evidence of a significant interaction between the SVO First variable and the prosocial dummy.

5 Discussion

In the analysis of Study 1 we found that prosocial subjects in the SVO First treatment kept significantly less in the standard dictator game than prosocials in the SVO Last treatment. We also found that the subjects with a perfectly consistent SVO measure were more generous than subjects with a less than perfectly consistent measure. In the analysis of Study 2 we found that, across all subjects, those who were given the SVO measure first were more generous in the dictator game. And similar to that found in the analysis of Study 1, we found that subjects with a perfectly consistent SVO measure were affected by the timing. However, we did not find evidence that the prosocial subjects were affected by the timing. When we pool the data, we find evidence that subjects who were given the SVO measure first, were more generous in the dictator game, and that this effect was stronger among those with a perfectly consistent SVO measure.

There seem to be two effects related to the timing of the measures. First, prosocial subjects are differentially affected by the timing in Study 1 but not Study 2. This is consistent with the explanation that prosocials who first complete the SVO have their generosity made salient by the creation of surplus. As a result, these subjects are more generous in the dictator game than prosocials who have not yet completed the SVO. However, once both the SVO and the dictator game contain the creation of surplus, as it does in Study 2, the effect diminishes. The second effect relates to the generosity exhibited by all subjects in Study 2 who first receive the SVO measure, where the generosity is stronger among those with a perfectly consistent SVO measure. This is consistent with the explanation that the measurement of SVO can prompt subjects of all social preferences, particularly those with perfect SVO measures, to be more generous. However, when being selfish becomes more costly, as it does in Study 2, all subjects act more generously.

Our results could partially be explained by wealth effects, whereby subjects who have completed the SVO were affected the money earned. We point out that SVO accounts for a very small amount of money, and it seems rather implausible that a change in wealth of \$0.12 would affect behavior in a \$10 dictator game. Further, the wealth effect argument is

not consistent with the differential effects related to the measure of consistency of the SVO measure, since this includes prosocials, individualists and competitors.

6 Concluding Comments

In this paper, we describe two studies in which we measure social preferences through choice in the Social Value Orientation (SVO) and choice in a dictator game. In Study 1, we vary the order of the SVO and a standard dictator game. We find evidence that subjects with prosocial preferences act more generously in the dictator game when the SVO items are given first. On the other hand, we do not find evidence that subjects with individualistic preferences are affected by the order of the items. However, we do find that subjects with perfectly consistent SVO measures are affected by the timing of the measurement.

To better understand these results, Study 2 performs the identical procedure of Study 1 with the exception that a nonstandard dictator game is used. This nonstandard dictator game exhibits a 1-to-3 trade-off between own payoffs and other payoffs, whereas the standard dictator game has a 1-to-1 trade-off. We find that the timing affects behavior in the dictator game, and this effect is larger for subjects with a perfectly consistent SVO measure. These results call into question the assumptions that social preferences are stable and that they can be measured in a reliable and nonintrusive manner.

It is worth reflecting on the limitations of the present experimental design. For instance, we cannot determine whether the SVO measurement affects the dictator game choices, the dictator game choices affects the SVO measurement or perhaps both. Such questions of endogeneity are notoriously tricky and would require further study. It is also unclear if the timing matters in the measurement of preferences via Andreoni-Miller, Charness-Rabin, or Chen-Li techniques. Finally, SVO only measures social preferences when the subject receives a larger share than the other subject. The significance of this detail is not clear. Hopefully, future work will shed light on these issues.

Appendix 1

We asked the following 9 items (from Van Lange et al., 1997) in order to measure the SVO of the subjects. Each of the 9 items has a prosocial answer, a individualistic answer and a competitive answer. Each item is stated in terms of points where 100 points corresponded to \$0.02103.

Question 1	A	B	C
You:	480 points	540 points	480 points
Other1:	80 points	280 points	480 points
Question 2	A	B	C
You:	560 points	500 points	500 points
Other1:	300 points	500 points	100 points
Question 3	A	B	C
You:	520 points	520 points	580 points
Other1:	520 points	120 points	320 points
Question 4	A	B	C
You:	500 points	560 points	490 points
Other1:	100 points	300 points	490 points
Question 5	A	B	C
You:	560 points	500 points	490 points
Other1:	300 points	500 points	90 points
Question 6	A	B	C
You:	500 points	500 points	570 points
Other1:	500 points	100 points	300 points
Question 7	A	B	C
You:	510 points	560 points	510 points
Other1:	510 points	300 points	110 points
Question 8	A	B	C
You:	550 points	500 points	500 points
Other1:	300 points	100 points	500 points
Question 9	A	B	C
You:	480 points	490 points	540 points
Other1:	100 points	490 points	300 points

The individualistic answers are: 1B, 2A, 3C, 4B, 5A, 6C, 7B, 8A and 9C. The prosocial answers are: 1C, 2B, 3A, 4C, 5B, 6A, 7A, 8C and 9B. The competitive answers are: 1A, 2C, 3B, 4A, 5C, 6B, 7C, 8B and 9A. Van Lange et al. classifies a subject according to the above labels if six or more items are answered according to the above.

Instructions given to each subject:

Instructions:

You are to be anonymously matched with two other people. We refer to the two others as "Other1" and "Other2."

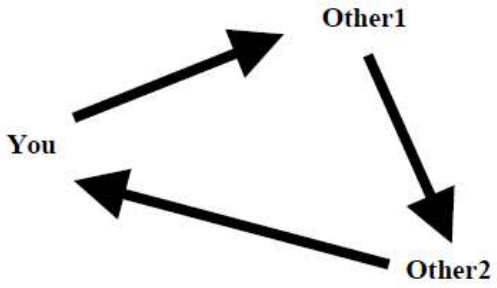
You are to make a series of allocation decisions involving Other1. This means that you are to divide a surplus between yourself and Other1. Whatever you do not keep for yourself goes to Other1.

Other1 is to make a series of allocation decisions involving Other2. This means that whatever Other1 does not keep goes to Other2.

Other2 is to make a series of allocation decisions involving you. This means that whatever Other2 does not keep goes to you.

Therefore the money accruing to you is composed of (i) whatever you do not send to Other1 and (ii) whatever Other2 sends to you.

We depict the relationship by the diagram below:



Note: A randomly selected 25% of the class will actually be paid the amount accruing to them.

Study 1 Dictator Game:

You: $10.00 Other1: $0.00	You: $9.75 Other1: $0.25	You: $9.50 Other1: $0.50	You: $9.25 Other1: $0.75
You: $9.00 Other1: $1.00	You: $8.75 Other1: $1.25	You: $8.50 Other1: $1.50	You: $8.25 Other1: $1.75
You: $8.00 Other1: $2.00	You: $7.75 Other1: $2.25	You: $7.50 Other1: $2.50	You: $7.25 Other1: $2.75
You: $7.00 Other1: $3.00	You: $6.75 Other1: $3.25	You: $6.50 Other1: $3.50	You: $6.25 Other1: $3.75
You: $6.00 Other1: $4.00	You: $5.75 Other1: $4.25	You: $5.50 Other1: $4.50	You: $5.25 Other1: $4.75
You: $5.00 Other1: $5.00	You: $4.75 Other1: $5.25	You: $4.50 Other1: $5.50	You: $4.25 Other1: $5.75
You: $4.00 Other1: $6.00	You: $3.75 Other1: $6.25	You: $3.50 Other1: $6.50	You: $3.25 Other1: $6.75
You: $3.00 Other1: $7.00	You: $2.75 Other1: $7.25	You: $2.50 Other1: $7.50	You: $2.25 Other1: $7.75
You: $2.00 Other1: $8.00	You: $1.75 Other1: $8.25	You: $1.50 Other1: $8.50	You: $1.25 Other1: $8.75
You: $1.00 Other1: $9.00	You: $0.75 Other1: $9.25	You: $0.50 Other1: $9.50	You: $0.25 Other1: $9.75

You: $0 and Other1: $10.00

Study 2 Dictator Game:

You: $10.00 Other1: $0.00	You: $9.50 Other1: $1.50	You: $9.00 Other1: $3.00	You: $8.50 Other1: $4.50
You: $8.00 Other1: $6.00	You: $7.50 Other1: $7.50	You: $7.00 Other1: $9.00	You: $6.50 Other1: $10.50
You: $6.00 Other1: $12.00	You: $5.50 Other1: $13.50	You: $5.00 Other1: $15.00	You: $4.50 Other1: $16.50
You: $4.00 Other1: $18.00	You: $3.50 Other1: $19.50	You: $3.00 Other1: $21.00	You: $2.50 Other1: $22.50
You: $2.00 Other1: $24.00	You: $1.50 Other1: $25.50	You: $1.00 Other1: $27.00	You: $0.50 Other1: $28.50
You: $0.00 Other1: $30.00			

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