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Birth, death and public good provision

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Abstract We explore the effect of fixed versus dynamic group membership on public good provision. In a novel experimental design, we modify the traditional voluntary contribution mechanism (VCM) by periodically replacing old members of a group with new members over time. Under this dynamic, overlapping generations matching protocol we find that average contributions experience significantly less decay over time relative to a traditional VCM environment with fixed group membership and a common termination date. These findings suggest that the traditional pattern of contribution and decay seen in many public goods experiments may not accurately reflect behavior in groups with changing membership, as is the case in many real-world environments.

Keywords Public goods · Overlapping generations · Voluntary contribution mechanism · Experimental economics

JEL Codes C72 · C92 · D64 · H41

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1 Introduction

The overlapping generations structure, first introduced by Samuelson (1958), is a standard model in economic theory. Agents of different ages or experience levels coexist with one another at each point in time and this type of heterogeneity provides insights into a variety of dynamic social phenomena, such as the existence of social security systems or money as a medium of exchange. Nevertheless, the overlapping generations structure has seen comparatively little use in the field of experimental economics where researchers tend to work with fixed cohorts of subjects in one-shot or repeated settings.[1] In this paper, we examine the behavior of overlapping generations of subjects playing a workhorse model in the experimental economics literature—the finitely repeated, linear, voluntary public goods game. Our aim is to better understand how the presence of overlapping generations of agents of different experience levels, as implemented in a deterministic birth—and—death manner, affects contributions to a public good over time relative to the standard, baseline case of a fixed cohort of agents interacting repeatedly with one another over all periods of the experiment.

The real-world relevance of heterogeneity in age and experience levels is hard to deny. Residents move into and out of neighborhoods, voters enter and exit electoral districts and new donors may give for the first time to a charity while longtime donors may grow old and die.[2] Nevertheless, it is not clear, a priori, what effect such turnover in the population of potential contributors should have on contributions to public goods.

A robust finding across experimental studies of linear public goods games employing *fixed* cohorts of subjects is that public good contributions are most generous in the early periods of play and become steadily less generous as subjects gain experience.[3] If this decay in contributions is an effect of individual-level learning, then we might expect that a dynamic, heterogeneous-agent, overlapping generations environment will yield higher contributions as new subjects with predispositions to contribute to the public good are steadily introduced. On the other hand, it is possible that new subjects entering a preexisting group of experienced subjects may anticipate free-riding behavior on the part of their older peers and behave more selfishly as a result. Even if new subjects do not anticipate such behavior by others, they could learn about low levels of giving after their first period of play and, as a result, drastically reduce their contributions in subsequent periods. A widely accepted model of behavior in public goods games does not give clear predictions in our environment; the Fischbacher et al. (2001) model of conditional cooperation in public goods games could lead to increased or decreased

[1] The reason for the paucity of overlapping generation experiments is likely owing to the inconvenience of having subjects enter or exit the laboratory at different times. We address this issue in our experimental design.

[2] Indeed, as fundraising expert Burnett (2002), p. 155 writes: “Every fundraising organization needs to be recruiting new supporters constantly, if not for reasons of growth then to replace those lost through natural (or unnatural) wastage”.

[3] See Ledyard (1995) and Chaudhuri (2011) for surveys of this experimental literature.

contributions in the dynamic environment we study, depending on the reaction of new subjects to the behavior of existing subjects and vice versa.

An overlapping-generations approach also provides nuance to our understanding of end-game effects. In traditional public good game experiments under either fixed or random matching protocols, subjects all "age" at the same pace, always having experienced the same number of periods of play at each iteration. Most importantly, all subjects experience their final period simultaneously. Consequently, a subject in her final period is faced not only with a lack of future interactions and, under fixed matches a related loss of any reputational concerns, but also with the knowledge that every other member of her group of subjects is facing the same final period decision. In other words, a subject not only loses her own strategic concern for cooperation, but also faces other subjects experiencing an identical lack of concern for their own futures. It is hard to think of real-life scenarios with such a common air of finality. By contrast, in the overlapping generations framework we study, every subject in the group is a different age at each point in time. A subject in her final period still lacks reputational concerns for the future, but now faces group members who *can* have such concerns for the future, as they will outlive her.

To preview our results: We find that the dynamic, heterogeneous-agent overlapping generations environment that we implement does work to sustain contributions to the public good over a longer period of time relative to the standard, fixed cohort design. Contributions in our dynamic matching treatment decrease less within each subject's lifetime and contributions in a subject's final period of play are significantly greater than those made by subjects in the final period of the fixed matching treatment. Our findings suggest that experimentalists will want to add overlapping generation-matching designs to their toolkit of mechanisms for arresting the decline in cooperative behavior that is frequently encountered in laboratory experiments involving fixed cohorts of subjects interacting with one another repeatedly for a finite length of time.

The rest of the paper is organized as follows. The next section discusses related literature. Section 3 lays out our experimental design and hypotheses. Section 4 summarizes our main experimental findings. Finally, Sect. 5 concludes with some suggestions for future research.

2 Related literature

A consistent pattern within the literature on linear, voluntary contribution games is that of over-contribution and decay, or the tendency for subjects to initially contribute heavily to a public good, but steadily decrease their contributions over time [e.g., Isaac et al. (1984), Fischbacher et al. (2001), Guala (2005)]. Many different interventions have been proposed as mechanisms to sustain contributions to a public good under the voluntary contribution mechanism (VCM) with varying success rates, including increased group size (Isaac and Walker 1988), communication [e.g., Dawes et al. (1977), Isaac and Walker 1988], partners versus strangers matching protocols [Andreoni and Croson (2008), Keser and van Winden (2000)], punishment [e.g., Fehr and Gächter (2000)] and endogenous group formation [e.g., Ehrhart and Keser (1999), Brekke et al. (2011)] among other devices.

Most existing experimental studies of public goods games examine behavior by a population of subjects who all begin and end their participation in the experiment at the same time. These subjects are placed in fixed or randomly determined groups and face the same public good contribution decision repeatedly for a known finite horizon of T periods.[4] Our primary contribution is to implement a steady turnover of participants, gradually introducing new subjects in place of older ones so as to examine the impact of this more natural, overlapping generation structure on the fraction of subjects' endowments that are contributed to the public good. Thus, subjects in our design enter and exit our experiment at *different* times; they do not all start and end their participation in the experiment at the same time as in the standard design. Most subjects in our overlapping generations design live for T periods and interact with subjects of different experience levels. Importantly, we compare this overlapping generations structure with the canonical fixed matching group design where all subjects play for T periods and all subjects begin and end their participation in the experiment at exactly the same time.

The paper most closely related to this paper is by Sonnemans et al. (1999) who study binary decisions to contribute (or not) to a public good when subjects move between groups over time. When a subject in their experiment leaves a group he or she is replaced by an experienced subject from another group. Each departing subject enters a group of experienced subjects with whom the entrant has not previously interacted; play of the same public good game then continues for a further number of rounds. The focus of their study is on the behavior of players in the period where they leave a group never to return, where any possible strategic motivations are eliminated. By contrast, we study a continuous strategy VCM game and are interested in the impact of both birth (i.e., new entrants) and death (i.e., exit) on contributions to the public good over time. Each of our subjects enters only one group of contributors and when they finally exit that group they do not enter into another group as their participation in our experiment is over; that is, we do not *recycle* subjects into other groups as in Sonnemens et al. (1999) or the endogenous group formation literature. Thus, our experiment implements a strict *overlapping generations* structure for contributions to the public good. The overlapping generations friction can be viewed as a *biologically-based* matching protocol that is intermediate between fixed partner matching and random stranger matching protocols.

The behavioral consequences of various different mixtures of experienced and inexperienced subjects has been examined by Dufwenberg et al. (2005) in the context of an asset market experiment and by Slonim (2005) in the context of dominance-solvable "beauty contest" games. More formal, overlapping generations structures combining subjects of different experience levels have occasionally been implemented by experimentalists to study such topics as money as a medium of exchange, [e.g., Marimon and Sunder (1993)], pension systems [e.g., Offerman et al. (2001)]

[4] They may also be reassigned between groups [as in endogenous group formation designs or real stranger protocol of Sonnemans et al. (1999)].

and asset pricing decisions Deck et al. (2011). However, to our knowledge, overlapping generation structures have not been applied to study behavior in the linear, VCM.

We note further that the experimental overlapping generation structures implemented to date have had only two generations or cohorts overlapping at any single period in time and in some cases (e.g., Marimon and Sunder), subjects who had completed their lifetimes were recycled through the game repeatedly, i.e., they were repeatedly "born again". By contrast, our overlapping generations structure has four different generations coexisting at any single period in time and we do not recycle subjects; once subjects' lifetimes are over, those subjects leave the experimental laboratory, are paid in a adjacent separate room, and have no further involvement in the experimental economy.

3 Experimental design

Our experiment consists of two treatments, one with fixed matching (the "fixed" treatment) and one with dynamic, overlapping generations matching (the "dynamic" treatment). The fixed matching treatment, which serves as our baseline treatment, is a traditional linear public goods game. In each session of this fixed matching treatment, eight subjects are initially randomly matched into two groups of size four. Each group of size four remains in the same fixed match to play 12 periods of a VCM game. Specifically, in each of the 12 periods, subjects were asked to allocate their period endowment of 100 tokens between "public" and "private" accounts. Each token allocated by subject i to her own private account, $x_i \in [0, 100]$, earned subject i \$0.10, while each token that subject i allocated to the public account, $y_i = 100 - x_i$ earned all four members of i's group, including subject i, \$0.04 each. Thus, the marginal per capita return (MPCR) to the public good was $0.04/0.10 = 0.4$. This is a commonly used parameterization for VCM experiments [see, e.g., Fehr and Gächter (2000), Kosfeld et al. (2009)] and was chosen to increase comparability to existing research. It follows that subject i's dollar payoff in a period was given by:

$$\pi_i = 0.10x_i + 0.04\sum_{j=1}^{4} y_j.$$

Note that since the MPCR$= 0.4 > 1/$ group size $= 1/4$, the efficient outcome is for all four players to allocate all of their 100 tokens to their public account in which case $\sum_{j=1}^{4} y_j = 400$ and each subject, i, earns $\pi_i = \$16.00$. However, since the MPCR < 1, it is a dominant strategy for each individual i to allocate all 100 tokens to their private account, yielding 0 public good provision and each subject, i, a payoff of $\pi_i = \$10.00$.

Each session of the dynamic matching treatment used the same MPCR and group size of four, but involved a total of 30 subjects who participated over some periods in one of the two groups of size four (15 subjects per group of size four). With the introduction of new participants in the group, the total number of periods played increased from 12 in the fixed treatment to 36 in the dynamic treatment. Individual subjects in our dynamic sessions, however, never participated for more than 12 periods. Some subjects at the beginning and at the end of each dynamic treatment session participated for fewer than 12 (either 3, 6, or 9) periods; these shortened "lives" were necessary to ensure that each group consisted of subjects of different, staggered ages, while also ensuring that there were always four players in each group.

The precise overlapping generations structure of the dynamic treatment that we implemented in the laboratory is illustrated in Fig. 1. Each observation of our dynamic treatment consists of 15 subjects who were assigned to one of four "positions" (P1, P2, P3, P4) at different periods in the experiment and for different lengths of time. The four positions are the equivalent of the four players in the fixed treatment. One can think of the four positions as desks in the laboratory. The first subjects, $s1$, $s2$, $s3$ and $s4$ in each dynamic treatment observation are seated at their desks making decisions for the first 3, 6, 9 and 12 periods, respectively, of the session. These subjects eventually leave their desks and are replaced at those same desks by subjects $s5$, $s6$, $s7$ and $s8$, respectively, in periods 4, 7, 10 and 13. Notice that subjects $s4$–$s12$ each participate in a full 12 periods of play, the same number of periods played by all subjects in the fixed treatment. The final three subjects $s13$, $s14$ and $s15$ participate in just 9, 6, and 3 periods of play, respectively, corresponding to periods 28–36, 31–36 and 34–36 of each dynamic treatment session. Because each dynamic treatment session had *two*, four-position groups of subjects interacting simultaneously, subjects were unable to know the group membership of other subjects entering or exiting into their four-member group.

The experiment was conducted at the Pittsburgh Experimental Economics Laboratory using Fischbacher's (2007) z-Tree software. As explained above, sessions consisted of either 8 (fixed treatment) or 30 (dynamic treatment) subjects, all of whom had no prior experience with our experiment. We conducted four sessions of each of our two treatments. Thus, we recruited a total of 152 subjects from the student populations of the University of Pittsburgh and Carnegie Mellon University to participate in our experiment.

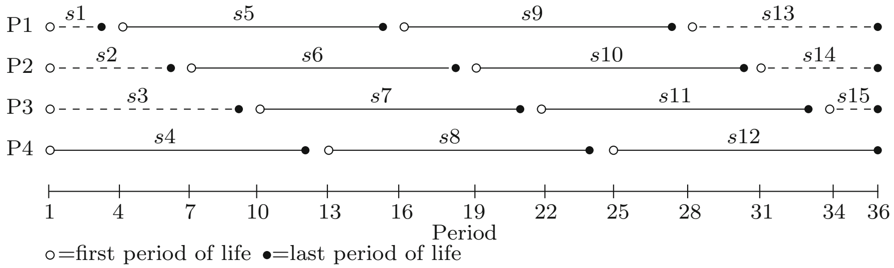

Fig. 1 Lifetimes of subject numbers $s1$–$s15$ in the four positions, P1–P4, of each dynamic treatment. Subjects with partial lifetimes of 3, 6, or 9 periods are depicted using *dashed lines*; Full, 12-period-lived subjects are depicted using *solid lines*

Each session began with the distribution of written instructions to all subjects. Copies of the written instructions used in both the dynamic and fixed treatments are provided in the electronic supplementary materials (ESM). The instructions were read aloud so as to make them public information. Subjects also had to correctly answer a short quiz to verify their comprehension of the written instructions. In the fixed treatment, subjects were read instructions in the computer laboratory and then completed the comprehension quiz. Afterward, subjects began making decisions using the networked computers in the laboratory.

In the dynamic treatment, subjects were read instructions in a room down the hall from the computer laboratory. These instructions informed them of the deterministic turnover procedure of the dynamic treatment [see the dynamic treatment instructions in the ESM for the details]. After the instructions were read and the comprehension quiz was completed, eight subjects were chosen at random to be escorted down the hall into the computer laboratory by an experimenter (no talking was allowed) to begin play as the initial members of the two groups of size four.[5] Upon entering the laboratory, each subject was given a card indicating how many periods they would play, either 3, 6, 9 or 12 and they were instructed to take a seat at a computer terminal. As in the fixed treatment, decisions were made using the networked computers of the laboratory. Following the end of period 3 and every three-period interval thereafter up through period 33, one subject from each of the two groups of size 4 was asked to exit the experiment. These subjects left the laboratory through a side door which opened up into another room where these subjects were privately paid for their participation. Two new subjects were then randomly chosen from among the subjects waiting in the room down the hall from the computer laboratory. These two new subjects were escorted into the laboratory by an experimenter (again, no talking was allowed) and were instructed to take the seats of the two subjects who had just departed. By proceeding in this manner, we made it clear to all subjects that turnover in group membership was taking place in our dynamic treatment.

Upon entry into the laboratory the new subjects were privately informed of how many periods they would play. The number of periods played by a subject in our dynamic treatment depended on their randomly drawn entry position and followed the design shown in Fig. 1. Subjects did not know until they entered the laboratory how many periods they would participate in—all assignments of subjects to entry positions in the dynamic treatment were randomly determined. There was never any interaction between departing and arriving subjects and, as we have emphasized, subjects could not infer what group of other participants they were matched with because there were always two groups of size four making decisions in the laboratory. Additionally, newly entered subjects did not learn about any previous contribution decisions made by other subjects, including those currently in their group and those whom they replaced. This informational restriction was made so that entering subjects in the dynamic treatment were comparable in historical

[5] Another experimenter remained in the room with the remaining subjects and enforced a rule of no communication.

knowledge to entering subjects in the fixed treatment who had no historical information on play of the public good game.[6]

Immediately prior to making their first allocation decision in the experiment, subjects in both treatments were asked to complete a form eliciting their belief as to the total amount of tokens that would be contributed to the public account by all members of their group, including themselves, in their first period of play. We collected this information to check that subjects' initial beliefs did not vary by treatment or, in the dynamic treatment, with the passage of time. While this belief was not incentivized, as we show in the next section, we did not find any significant difference in subjects' initial beliefs between our two treatment, or by period of entry.

Subjects were instructed that following the end of their participation in the experiment, one randomly selected period from among all the periods they played would be chosen for payment. Subjects were paid their earnings from the one randomly chosen period in cash and in private and were additionally given a $5.00 show-up payment. The fixed treatment took less than 1 h to complete while the dynamic treatment took longer, closer to 2 h, though each subject's participation in the laboratory portion of the dynamic treatment session was no longer than, and for some short-lived players, strictly shorter than in the fixed treatment sessions.[7]

Regarding theoretical predictions, we note that, given the known, finite number of periods played by each subject and the fact that the marginal per capita return of 0.4 is less than 1, it was a dominant strategy for all subjects in our experiment to contribute zero tokens to the public account in each and every period of the VCM game that they played. Thus, both our dynamic and fixed treatment sessions have the same, zero contribution subgame perfect equilibrium prediction. It follows that any differences in contributions to the public good can be attributed to differences between the fixed and dynamic interaction structures.

4 Results

Table 1 reports mean initial beliefs, first period contributions, overall contributions and earnings by each group in our experiment while Figs. 2 and 3 show the mean contributions of each group over time in the fixed and dynamic treatments, respectively. Each group's label is composed of either an F, denoting the Fixed treatment or a D, denoting the Dynamic treatment followed by a number representing the session number and finally an A or a B to represent either the first or the second group within that session.

[6] An alternative motivation comes from Malmendier and Nagel (2014) who report that individuals place higher weight on realizations of macroeconomic data (e.g., inflation) experienced during their life-times as compared with the available historical data prior to their birth.

[7] Once subjects had completed their participation in a dynamic treatment session they were immediately paid in a separate room adjacent to the laboratory and were excused, i.e., they did not have to wait around for the session to end to receive their payment.

Table 1 Mean initial beliefs, first period contributions, overall contributions and total earnings by treatment and group

Session	Group	Treatment	No. of subjects	Beliefs	Average earnings	% Contributed first period	% Contributed all periods
F1	F1A	Fixed	4	68.75	$16.74	60.75	28.96
F1	F1B	Fixed	4	50.00	$15.63	37.50	10.52
F2	F2A	Fixed	4	34.38	$15.40	33.75	6.65
F2	F2B	Fixed	4	39.38	$17.29	46.25	38.13
F3	F3A	Fixed	4	51.25	$17.33	52.50	38.85
F3	F3B	Fixed	4	46.25	$16.51	42.50	24.90
F4	F4A	Fixed	4	43.44	$15.42	43.75	7.06
F4	F4B	Fixed	4	65.63	$16.51	66.25	25.10
Average		Fixed	4	49.88	$16.35	48.91	22.52
D1	D1A	Dynamic	15	56.14	$17.19	48.75	36.47
D1	D1B	Dynamic	15	59.38	$17.98	37.50	49.65
D2	D2A	Dynamic	15	53.13	$16.64	58.75	27.35
D2	D2B	Dynamic	15	43.83	$16.13	27.50	18.82
D3	D3A	Dynamic	15	35.08	$15.61	45.00	10.12
D3	D3B	Dynamic	15	46.69	$15.87	25.00	14.45
D4	D4A	Dynamic	15	53.00	$17.21	50.00	36.99
D4	D4B	Dynamic	15	59.38	$16.07	60.00	17.74
Average		Dynamic	15	50.95	$16.59	44.06	26.45

Reported earnings include the $5.00 show-up payment

As Table 1 reveals, subjects' beliefs about their group's first-period contribution to the public good as a percentage of total endowment are similar between the two treatments, averaging just under 50 % in the fixed treatment and just over 50 % in the dynamic. A non-parametric Wilcoxon Mann–Whitney (WMW) test confirms that there is no significant difference in beliefs about first-period contributions between the fixed and dynamic treatments ($p = 0.636$) using the 8 group level observations for each treatment

Table 1 also reveals that mean first period contributions to the public good as a percentage of endowment are similar in both the fixed and dynamic treatments, averaging 48.91 % in the fixed treatment and 44.06 % in the dynamic treatment. A WMW test confirms that there is no significant difference in first-period contributions between the fixed and dynamic treatments ($p = 0.636$) using the 8 group level observations for each treatment. Table 1 further reveals that mean contributions to the public good over all periods are also similar between treatments at 26.45 % in the dynamic treatment and 22.52 % in the fixed treatment. Again, a WMW test again indicates that this difference is not statistically significant ($p = 0.674$ using the 8 group level observations for each treatment). We summarize this first result as follows:

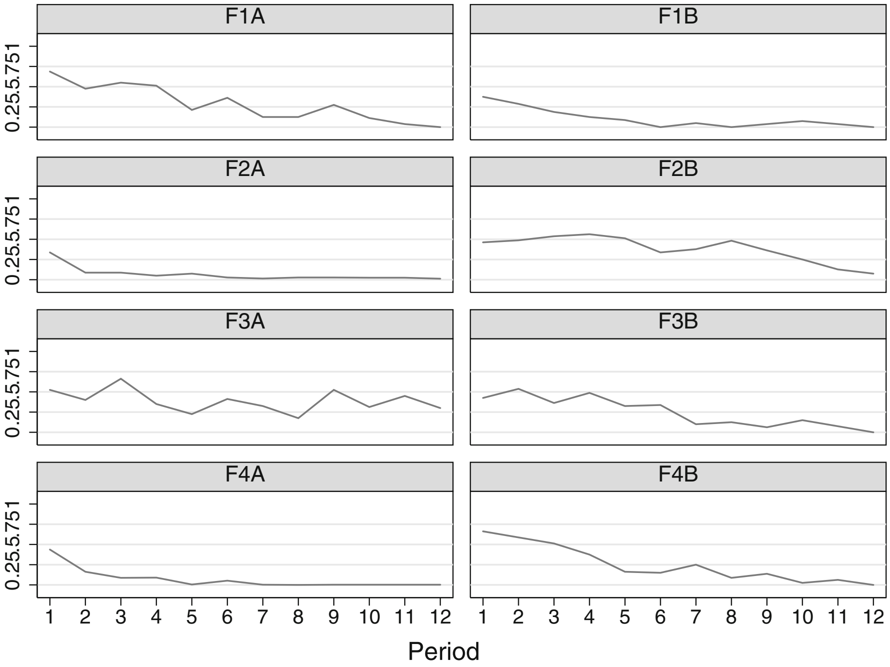

Fig. 2 Fixed treatment contributions over time, by group

Finding 1 There is no significant difference between the fixed and dynamic treatments in subjects' initial beliefs about group contributions, in their first-period contributions to the public account or in mean contributions to the public account over all periods of a session.

Finding 1 would seem to suggest that our overlapping generations matching structure has little effect on contributions to the public good. However, consider that the dynamic treatment, involving 36 periods, is *3 times longer* in duration than the fixed treatment, which involves just 12 periods. Thus, an immediate implication of Finding 1 is that contributions to the public good did not decline as quickly in the dynamic treatment as they did in the fixed treatment. Indeed, as revealed in Figs. 4 and 5, as well as Fig. 6 which combines Figs. 4 and 5 in a single graph, the decay over time in contributions to the public good is greatly attenuated by the overlapping generations structure of our dynamic treatment. These figures report the mean proportion of endowment contributed to the public good in each period of the two treatments. A fitted, linear trend line is also included in each figure.

Prior research by Keser and van Winden (2000) has shown that contributions to the public good decline more gradually when subjects play the game for a longer period of time. In their fixed-match "partners" treatment, groups of four subjects play a linear VCM with an MPCR of 0.5 for 25 rounds which is 2.5 times longer than the more common, 10-round experimental design. Keser and van Winden report that, as in many 10-round experimental studies of the VCM, mean

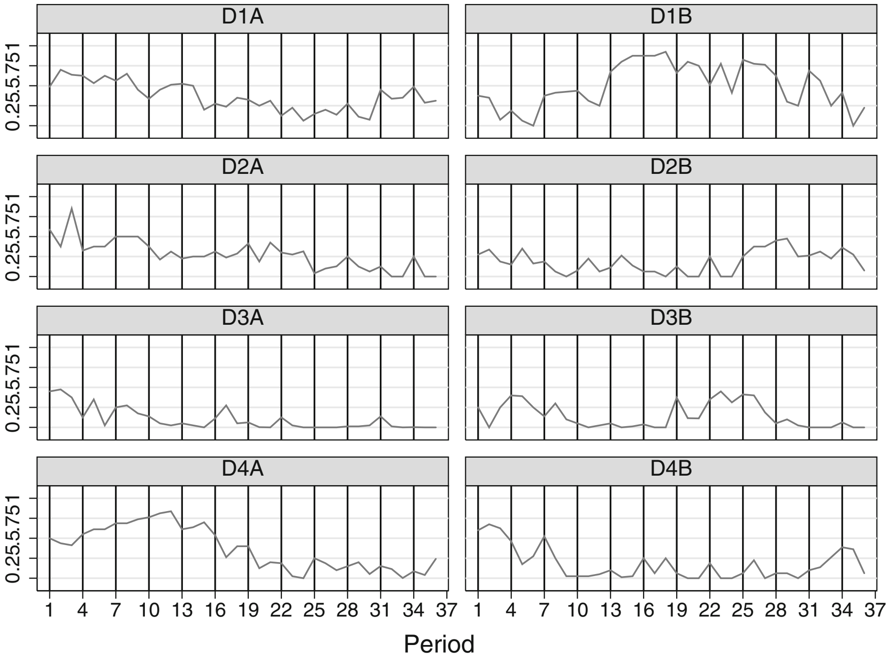

Fig. 3 Dynamic treatment contributions over time, by group. *Vertical bars* indicate periods in which new subjects entered the group

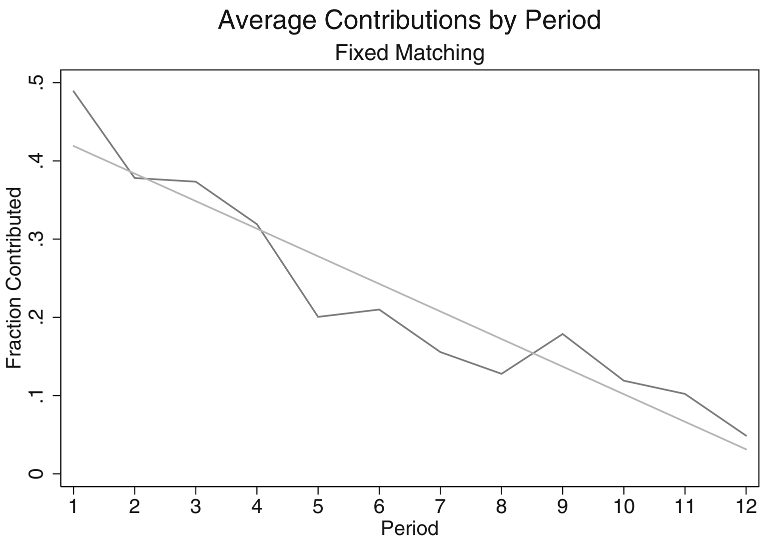

Fig. 4 Proportion of endowment contributed to the public good in the fixed treatment

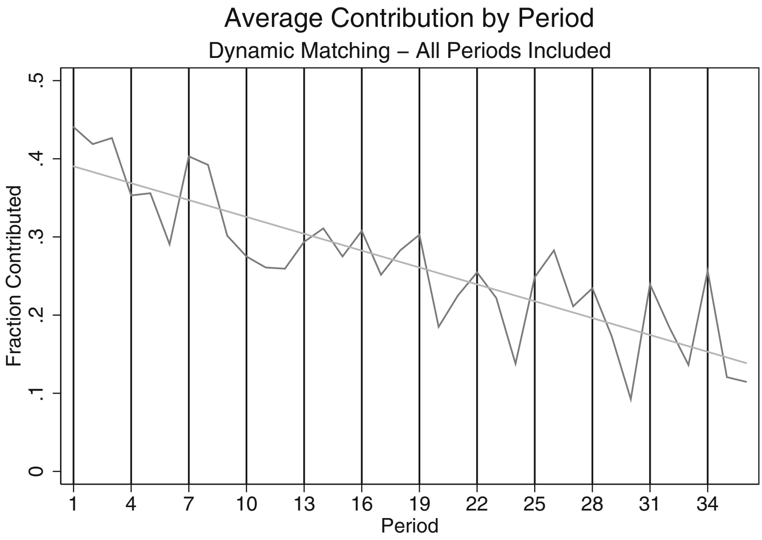

Fig. 5 Proportion of endowment contributed to the public good in the dynamic treatment, with all subjects included. *Vertical bars* indicate periods in which new subjects entered the group

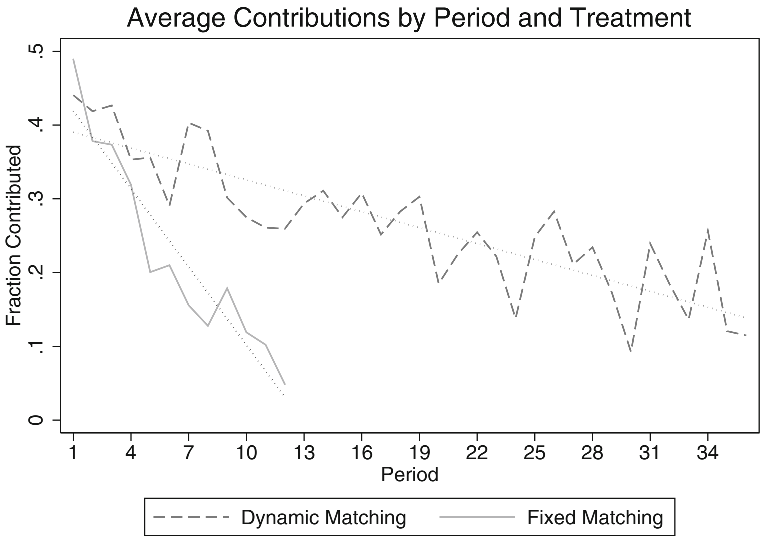

Fig. 6 Comparison of proportions of endowment contributed to the public good in both treatments

contributions to the public good begin at around 50 % of subjects' endowments and then decline to around 10 % of endowment by the 25th round. However, since the number of periods is much greater in their design, the steepness of the decline in contributions to the public good is much less than in 10-round studies. This observation suggests that subjects are adjusting their contribution behavior to the finite length of the linear VCM.

By contrast, the more gradual decline in contributions to the public good in our dynamic treatment is *not* owing to an increase in the number of periods that *individual* subjects played; subjects in our dynamic treatment played at most 12 periods, the same number of periods played by their counterparts in the fixed treatment, while some subjects in the dynamic treatment had even shorter lifetimes of just 3, 6 or 9 periods.[8] The key difference with our design, relative to others including Keser and van Winden (2000) study, is that the horizon over which contributions to the public good can can take place is, for most subjects, beyond those subjects' final period participating in our experiment.

To put the comparison between treatments on a more equal footing, Fig. 7 shows the proportion of endowment contributed to the public good in the dynamic treatment where all "short-lived subjects"—those with lives of <12 periods—have been removed. Figure 7 thus starts in period 10 and ends in period 27.[9] A comparison of Fig. 7 with Fig. 4 clearly indicates that the decay in contributions to the public good is arrested among 12-period-lived agents in the dynamic treatment as compared with the fixed treatment.

In an effort to more rigorously understand the temporal differences between the dynamic and fixed treatments we turn next to a broad regression analysis using data at both the group and individual level. Since each subject's public good contributions are bounded between 0 and the endowment of 100 tokens, we use a random effects Tobit model to estimate the impact of temporal and treatment factors on contributions to the public good as in, e.g., Chaudhuri et al. (2006). The dependent variable in these regressions is the proportion of endowment contributed to the public good at either the group or the individual level by period or by age. Here "period" refers simply to the period number in the session, 1, 2,..., 12 in the fixed treatment and 1, 2,..., 36 in the dynamic treatment. By contrast, "age" refers to the order of periods in a subject's lifetime. Notice that age corresponds to period in the fixed treatment, but this is not generally the case in the dynamic treatment. For example, consider subject *s*5 in the dynamic treatment. As Fig. 1 makes clear, subject *s*5 is alive in periods 4–15 of the dynamic treatment. Thus subject *s*5 is age 1 in the first period she is alive, which is period 4, she is age 2 in period 5 and so on up to age 12 in period 15. Other variables in our regression analysis are *dyna*, a dummy variable equal to 1 for dynamic treatment observations and *turnover*, a dummy variable that capture possible restart effects: the *turnover* dummy variable is equal to 1 for pre-existing members of the group in periods in which there is a change in the group membership in the dynamic treatment.[10] In other words, *turnover* = 1 in periods $4, 7, 10, \ldots, 34$ for subjects who have been in the group for at least one period, and $turnover = 0$ for continuing subjects in all other periods.

[8] For this reason, it would be difficult to compare behavior in our dynamic treatment with behavior in a fixed match environment lasting 36 periods, as the latter would involve play of the public good game by subjects who lived 3 times longer than any subject in our dynamic treatment.

[9] The logic of this truncation may be better understood by reference to Fig. 1.

[10] As explained below, the dummy variable is constructed in this manner as *turnover* is intended to detect restart effects among *existing* subjects.

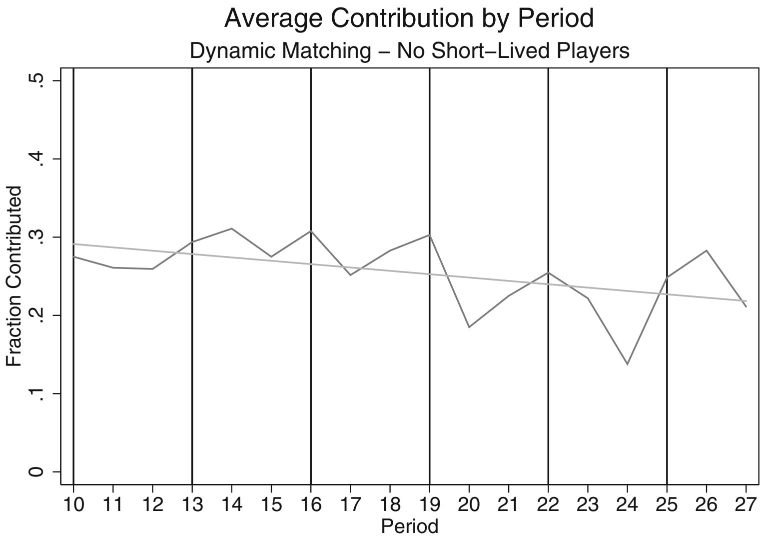

Fig. 7 Proportion of endowment contributed in the dynamic treatment by period, excluding short-lived subjects. *Vertical bars* indicate periods in which new subjects entered the group

Regression results are reported in Table 2. We focus primarily on the data at the group level (column 1), in which each observation is the mean contribution across all four members of a group in a single period. The results are qualitatively similar at the individual level (column 2), in which each observation is the contribution of a single subject in a single period. We also report regression results excluding all short-lived subjects in the dynamic treatment in Table 3. Restricting the data in this way does not meaningfully affect our results, except in one instance discussed below.

The results in columns 1–2 of Tables 2 and 3 show the usual decay over time in the baseline fixed treatment, as reflected in the *negative* and highly significant effect of the period variable on contributions. However, the interaction of the period variable and the dynamic treatment dummy variable (period $\times$ dyna) is also highly significant and *positive*. We thus find evidence that there was significantly less decrease in contributions over time in the dynamic treatment relative to the fixed treatment, consistent with evidence already presented in Figs. 4–7. We summarize this finding as follows:

Finding 2 Contributions to the public account decrease less per period in the dynamic treatment than in the fixed treatment.

What is driving the more stable levels of cooperation in the dynamic treatment? One possible explanation is that the entry of new, inexperienced subjects every three periods generates periodic "restart effects" as first identified in Andreoni (1988), that keep contributions from decaying to zero. However, if restart effects alone are responsible for the more gradual decay in public good contributions that we observe

Table 2 Contributions by all subjects

	(1) Group level by period	(2) Individual level by period	(3) Group level by age	(4) Individual level by age	(5) Group level dynamic only	(6) Individual level dynamic only
Dyna	−0.0459 (0.0705)	0.0267 (0.0584)	−0.0644 (0.0663)	−0.0777 (0.0533)	–	–
Period	−0.0289*** (0.00534)	−0.0259*** (0.00348)	–	–	−0.00586*** (0.00115)	−0.00523** (0.00207)
Period × dyna	0.0223*** (0.00537)	0.0153*** (0.00377)	–	–	–	–
Age	–	–	−0.0293*** (0.00304)	−0.0262*** (0.00352)	−0.0111*** (0.00313)	−0.0110*** (0.00302)
Age × dyna	–	–	0.0143*** (0.00399)	0.00971** (0.00396)	–	–
Turnover	0.00923 (0.0364)	0.00784 (0.0223)	0.00269 (0.0198)	0.00790 (0.0224)	0.0185 (0.0203)	0.0133 (0.0143)
Turnover × dyna	0.0417 (0.0414)	0.00422 (0.0263)	0.0113 (0.0279)	0.00581 (0.0265)	–	–
N	368	1384	176	1384	912	1032

Panel data, random effects tobit regressions. Reported coefficients are marginal effects. Robust standard errors are in parentheses. * $p<0.10$; ** $p<0.05$; *** $p<0.01$

Table 3 Contributions by long-lived subjects only

	(1) Group level by period	(2) Individual level by period	(3) Group level by age	(4) Individual level by age	(5) Group level dynamic only	(6) Individual level dynamic only
Dyna	−0.0876 (0.0875)	0.0316 (0.0793)	−0.0911 (0.0790)	−0.0628 (0.0603)	–	–
Period	−0.0282*** (0.00462)	−0.0262*** (0.00361)	–	–	−0.00482** (0.00242)	−0.00129 (0.00443)
Period × dyna	0.0238*** (0.00501)	0.0154*** (0.00463)	–	–	–	–
Age	–	–	−0.0283*** (0.00335)	−0.0265*** (0.00362)	−0.0115*** (0.00411)	−0.0145*** (0.00494)
Age × dyna	–	–	0.0180*** (0.00424)	0.0103** (0.00475)	–	–
Turnover	0.00712 (0.0304)	0.00790 (0.0224)	0.00317 (0.0204)	0.00792 (0.0225)	0.00914 (0.0273)	0.00530 (0.0203)
Turnover × dyna	0.0339 (0.0381)	−0.00271 (0.0299)	0.0133 (0.0289)	−0.00160 (0.0301)	–	–
N	232	880	176	880	528	528

Panel data, random effects tobit regressions. Reported coefficients are marginal effects. Robust standard errors are in parentheses. * $p<0.10$; ** $p<0.05$; *** $p<0.01$

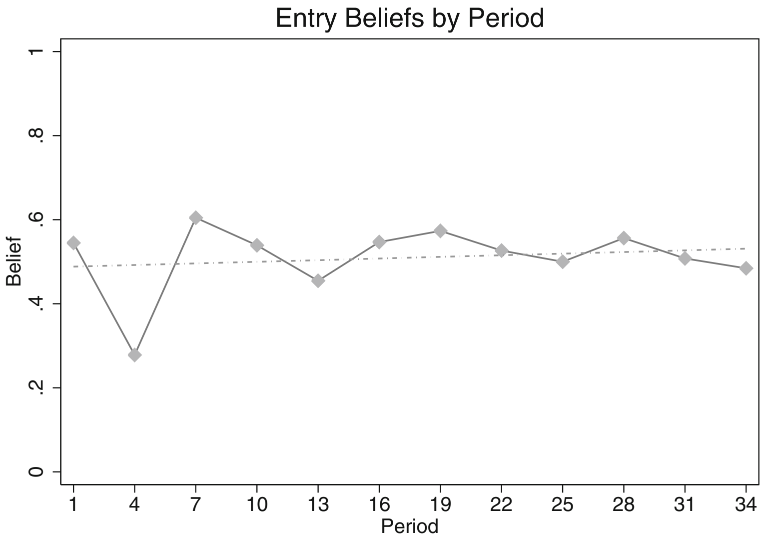

Fig. 8 Initial beliefs about the mean group contribution as a proportion of the endowment by entering subjects in the dynamic treatment

in the dynamic treatment, then we would expect to see contributions among existing subjects increase every time a new person enters the group. That is, contributions from older subjects in the first period with a new entrant would be higher than in other periods, a pattern that we do not observe in the data. As Tables 2 and 3 reveal, the *turnover* and the *turnover* × *dyna* dummy variables are never significantly different from zero under any of our regression specifications.

A second possibility is that differences in subjects' beliefs either between treatments or across time are driving the slower decay in the dynamic treatment. Recall from Table 1 and Finding 1 that subjects' initial beliefs about the mean group-level contribution to the public good in their first period of play were nearly identical in both treatments at around 50 % of total endowment. We now note further that there was no diminution in this initial belief over time by those subjects who were waiting to enter the experiment in the dynamic treatment. Figure 8 shows

Table 4 Effect of beliefs on first-period contributions

	Contributions
Dyna	−0.0961 (0.125)
Belief	0.957*** (0.191)
Belief × dyna	−0.0660 (0.236)
N	151

Panel data, random effects tobit regressions. Reported coefficients are marginal effects. Robust standard errors are in parentheses. * $p<0.10$; ** $p<0.05$; *** $p<0.01$

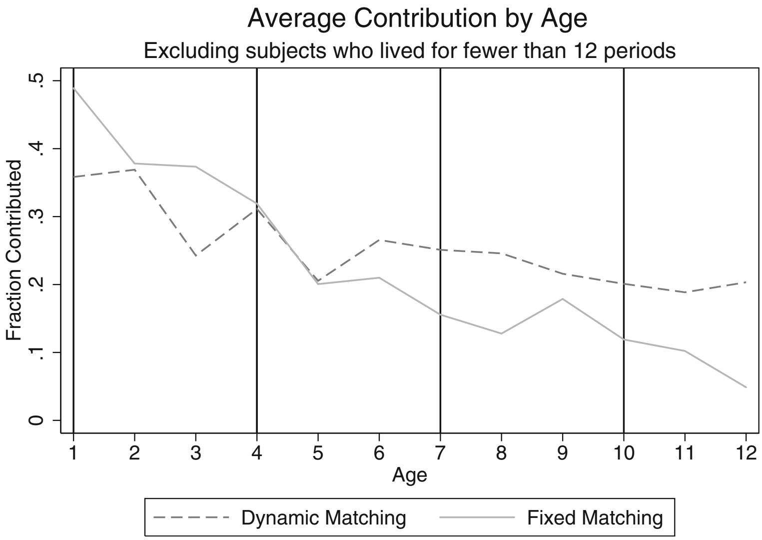

Fig. 9 Contributions in the dynamic and fixed treatments, by age. *Vertical bars* indicate ages at which new subjects entered the group

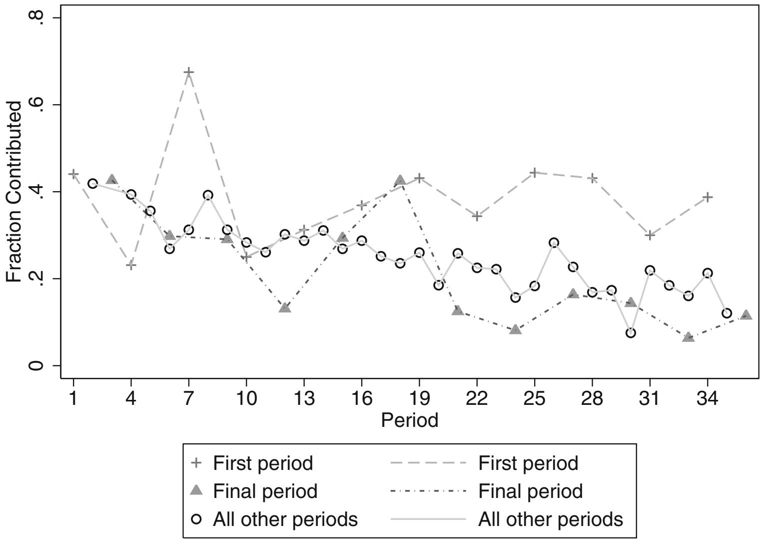

Fig. 10 Comparison of contributions by subjects who are in their first period, their last period, or a continuing period, over the course of the experiment

the mean belief of those subjects entering the dynamic treatment in periods 1,4,7,…34. A simple tobit regression also shows there to be no significant difference ($p = 0.822$) in beliefs by entry period in the dynamic treatment. In other words, regardless of when they entered, new subjects expected the mean group contribution to be around 50 % of the group's total endowment.

The effect of beliefs on first-period contributions is shown in a simple regression analysis reported in Table 4. The regression results in Table 4 reveal that beliefs have a large and highly significant impact on first period contributions, but both the treatment dummy variable, (*dyna* = 1 for the dynamic treatment) and the interaction of beliefs with treatment, *belief* × *dyna*, are not significant factors in explaining these first round contributions. Summarizing our our findings on beliefs, we see that they are highly predictive of contributions, but because they do not differ significantly by either treatment or period they do not appear to explain the slower decay that we observe in the dynamic treatment.

To better understand the difference between the two treatments, we need to examine subject behavior by *age* instead of by period. That is, we next consider how contributions vary with the number of periods a subject has experienced, rather than the total number of periods elapsed in the experiment. Figure 9 shows mean contributions to the public good by the age of a subject. Regression results in columns 3–4 of Tables 2 and 3 show that, while contributions decrease with age in both the fixed and dynamic treatments, as indicated by the significantly negative coefficient on the age variable, contributions decay less in the dynamic treatment as indicated by the positive and significant estimate on the age × dyna interaction variable. We summarize this finding as follows:

Finding 3 Contributions to the public account decrease less with subject age in the dynamic treatment than in the fixed treatment.

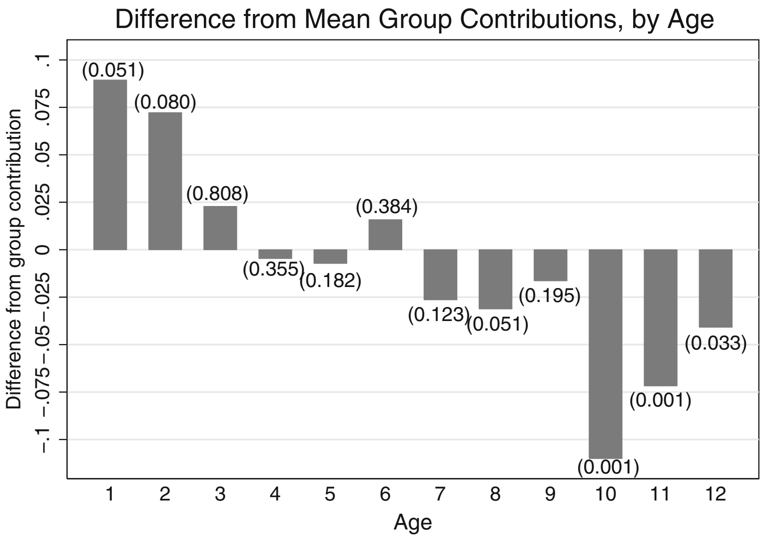

Fig. 11 Difference in dynamic treatment contributions from the average of all *other* group members, by subject age. *Numbers* in *parentheses* are p values from a Wilcoxon sign-rank test of the difference being significantly different from zero

Finding 3 implies that sustained contributions under our dynamic matching treatment are driven not just through the direct effect of introducing new, relatively generous subjects, but also by influencing the behavior of older subjects. Subjects in our dynamic treatment experience less decay in contributions over the course of their tenure in the session.

To delve somewhat deeper into the behavior of subjects by age, Fig. 10 disaggregates the mean contributions made by subjects who are in their first period of the experiment, their last period or in a continuing period in each of the 36 periods of our dynamic treatment. This figure clearly reveals that those in the first period are contributing high fractions of their endowments over all entry periods of this treatment. There is some decay over time in contributions by subjects who are in a continuing period as well as those who are in the final period and so it appears that it is these subjects who account for the gradual decline in contributions over the duration of the experiment. In other words, new subjects remain similarly generous at all points in time, while older subjects become less generous as the experiment goes on. The observation regarding entering subjects is supported by a simple tobit regression showing that the period in which a subject enters has no effect ($p = 0.916$) on their first-period contributions.

The observation regarding the decline in contributions by age finds further support in Fig. 11 which reports the difference in contributions from the mean group level contribution by age, along with Wilcoxan sign rank tests of whether these differences are significantly different from zero. As Fig. 11 clearly reveals, subjects in the first two periods of their lives (ages 1–2) are contributing significantly more than the group mean contribution while subjects in the last three periods of their lives (ages 10–12) are contributing significantly less than the group mean level. Finally, middle-aged subjects (ages 3–9) are, on average making contributions that are not significantly different from the group mean level.

Columns 5 and 6 of Table 2 report regression results when both age and period are included as explanatory variables. For this specification we examine only the dynamic treatment, as age and period are never different from one another in the fixed treatment. We find that both age and period are significant, though the magnitude of the age effect is much larger than the period effect. Running this same specification over only long-lived subjects as reported in columns 5–6 of Table 3

Table 5 Contributions based on previous contributions by other group members

	(1) Dynamic all subjects	(2) Dynamic long-lived only	(3) Fixed all subjects
Age	−0.0127*** (0.00244)	−0.0161*** (0.00404)	−0.0156*** (0.00498)
$x_{-i,t-1}$	0.196*** (0.0444)	0.275*** (0.0667)	0.216*** (0.0797)
$x_{-i,t-2}$	0.118*** (0.0448)	0.218*** (0.0682)	0.107 (0.0755)
N	912	432	320

Panel data, random effects tobit regressions. Reported coefficients are marginal effects. Robust standard errors are in parentheses. * $p<0.10$; ** $p<0.05$; *** $p<0.01$

still shows age to have a significant effect, though the impact of period becomes less clear, being marginally significant at the group level, and not significant at the individual level. These findings are consistent with Fig. 8, which showed that contributions remain relatively constant through the middle periods of the dynamic treatment.

Why do contributions decrease less with age in the dynamic treatment than in the fixed treatment? We have already seen that there is no evidence of a restart effect. Nevertheless, it appears that older subjects are more generous when they are exposed to new group members. One possibility is that the older subjects are simply reciprocating the generosity of optimistic, newly entered subjects. This is consistent with the conditional cooperation model of Fischbacher et al. (2001). In other words, older subjects may decide upon their level of giving based upon the contributions they observed from others in the previous period. In this case, subjects nearing the end of their time in the fixed treatment would be matched only with other subjects of the same, advanced age. By contrast, in the dynamic treatment, subjects are exposed to a brand new group member every 3 periods. These new entrants contribute relatively large amounts of their endowment, pulling up the group average contribution and generating reciprocally high contributions by older subjects in the following periods.

To some extent this argument can also be made in reverse. Newly entered subjects are surrounded by older, more pessimistic subjects who contribute less than is typically seen in the first period under fixed matching. As a result, we might expect to see new subjects in the dynamic treatment experience rapid decay in their contributions over their first few periods. In fact, as Fig. 9 reveals, we do see slightly lower early-age contributions in the dynamic treatment as compared with the fixed treatment, though these differences are not statistically significant.

Table 6 Group-level differences in contributions by treatment and period

Fixed period #	Fixed contribution (%)	Dynamic period #	Dynamic contribution (%)	Difference (Dyna − Fixed) (%)	p value
12	4.88	3	42.66	37.78	0.003
12	4.88	6	29.06	24.18	0.025
12	4.88	9	30.16	25.28	0.019
12	4.88	12	25.94	21.06	0.015
12	4.88	15	27.50	22.62	0.048
12	4.88	18	28.28	23.40	0.085
12	4.88	21	22.50	17.62	0.188
12	4.88	24	13.78	8.90	0.501
12	4.88	27	21.13	16.25	0.085
12	4.88	30	9.25	4.37	0.121
12	4.88	33	13.63	8.75	0.411
12	4.88	36	11.47	6.59	0.351

To examine how sensitive subjects are to their previous experiences, Table 5 reports regressions examining the effect of prior period contributions by other members of the group on a subject's current contribution. Specifically, we regress subject $i's$ period t contributions, $x_{i,t}$ on that subject's current age, and on the contributions made by all other members of that subject's group in the previous two periods, $x_{-i,t-1}$ and $x_{-i,t-2}$. Columns (1) through (3) report regression results from this model specification for all subjects in the dynamic treatment, long-lived subjects in the dynamic treatment, and all subjects in the fixed treatment, respectively. As before, age is highly significant in each instance, but the effect of previous group contributions differs between our two treatments. Contributions in the immediately previous period have a large and highly significant effect on current contributions in both treatments. By contrast, contributions made two periods previous have a significant impact on current contributions only in the dynamic treatment and have no effect in the fixed treatment. In other words, contributions exhibit more long-term inertia in the dynamic treatment as compared with the fixed treatment.

A further possible explanation for the slower decay in the dynamic treatment is that subjects are being altruistic not only toward their current group members, but also toward those who will enter their group in the future. Because of turnover in the population over time, a subject's contributions not only have a direct effect on their current groupmates, but may also have an indirect effect on future members. If subjects anticipate that their short-term contributions may have long term effects, for example by sustaining high levels of contributions through repeated reciprocity, they may have an increased incentive to contribute relative to the fixed environment.

While the progression of contributions over time is important, we are also interested in differences in final period contributions between our two treatments. Table 6 compares mean contributions of subjects in the final period of the fixed treatment to each of the twelve periods in the dynamic treatment in which a subject exited the group. Although the 12th period is clearly the final period of the fixed treatment, it is less clear what constitutes the equivalent period in the dynamic treatment. In fact, there are three periods that, for different reasons, can be considered to be the dynamic treatment equivalent of the final period of the fixed treatment.

The most direct comparison is to pair period 12 of the fixed treatment with period 12 of the dynamic treatment. This is a natural approach, as we allow the same number of periods to transpire in each treatment and then compare the outcomes. Using this method we find contributions to be 21 % points higher in the dynamic treatment ($p = 0.015$). The obvious downside to this comparison is that the 12th period of the dynamic treatment is not the terminal period for most subjects.

We can also treat the 27th period of the dynamic treatment as our "final" period. As illustrated by Fig. 1, this is the last period in which all subjects were long-lived. This allows as many periods as possible to occur while excluding the short-lived subjects at the end of the session. This comparison shows contributions to be 16.25 % points greater in the dynamic treatment, with the difference being marginally significant ($p = 0.085$).

Lastly, we can also compare the true final periods across treatments by looking to period 36 of the dynamic treatment. This has two main advantages. It is the final period of the dynamic treatment, allowing for the maximum number of periods to have elapsed. It is also the only period in the dynamic treatment in which all active subjects simultaneously experience their final period. We once again see higher contributions in the dynamic treatment, however the difference is not significant ($p = 0.351$). This comparison does have the downside of including short-lived subjects, which may lead to more rapid decay in contributions than those who participate in a full 12 periods.

Finding 4 Relative to the 12th and final period of the fixed treatment:

1. Contributions in the 12th period of the dynamic treatment are significantly greater.
2. Contributions in the 27th period of the dynamic treatment are marginally greater.
3. Contributions in the 36th and final period of the dynamic treatment are not significantly different.

Finding 4 shows that contributions in the dynamic treatment are no less than, and sometimes strictly greater than contributions in the fixed treatment, depending upon the comparison period.

Finally we compare end-of-life contributions, looking only at the 12th period of each (long-lived) subject's life. The mean contribution to the public good by age 12 participants in the dynamic treatment averages 16.21 % compared to just 4.89 % in the fixed treatment ($p = 0.020$). That is, subjects in the dynamic treatment contribute more than three times as much to the public good in the final periods of their lives as do their fixed-matching counterparts.

Finding 5 Contributions by age 12 subjects in the dynamic treatment are more than three times greater than contributions by age 12 subjects in the fixed treatment.

Finding 5 is important, in that subjects in the dynamic treatment are contributing substantially more than is typically seen in the terminal period of public goods experiments. Subjects continue to contribute relatively large amounts when we normally expect to see an end-game effect bringing contributions closer to zero.

This difference in behavior may be a product of conditional reciprocity, as discussed above, but it may also result from an aspect of our experimental design with broad implications to other experiments. As mentioned earlier, traditional fixed matching environments conflate the effects of one's own final period and the final periods of others. Under fixed matching, a subject's final period contributions are a product both of having no future periods to look forward to, and of their awareness that all other subjects in their group are simultaneously experiencing their own terminal periods. In our dynamic setting, however, when a subject exits the group it is almost always the case that he or she is the only participant exiting their group during their final period of play, meaning that they should not expect end-game effects from the other, remaining members of their group.

5 Conclusions

The experiment reported in this paper provides contributions on two fronts. First, we observe that the traditional structure of public goods games, with unchanging group membership over time, may neglect an important influence on individuals' contribution decisions. Second, we provide an overlapping generations methodology that can be broadly applied to other experimental settings.

Public goods environments outside of the laboratory often have some form of dynamic group membership in which individuals of different ages and experience levels interact. Models with fixed group membership neglect this heterogeneity of experience levels, especially the steady flow of new, inexperienced and often more generous entrants. Our results suggest that contributions to public goods are likely to be greater in settings with dynamic, rather than fixed membership. This increase is due not only to the direct effect of introducing new members with high rates of contribution, but also indirectly by increasing the level of cooperation exhibited by older members.

The dynamic structure of our experiment need not be limited to public goods environments, and can be found in many other settings commonly studied by experimental economists. Obvious examples include the minimum effort game, coordination games, and trust games. Introducing an overlapping-generations framework may have substantial impacts in these and other experimental settings. One contribution from this framework is to divorce the effect of an individual experiencing her own final period from the effect of interacting with other subjects who are experiencing their own final periods.

There is no explicit dynamic element to the public good game that we study, e.g., the public good is not durable and must be re-financed in every period. In future research it would be of interest to consider a dynamic, durable public good choice model with the overlapping generations structure. Future research might also consider variations in the frequency with which generations turn over and the length of life of each generation. We note that at one extreme, with high turnover and short lengths of life our dynamic group membership design resembles an anonymous one-shot environment. At the other extreme, with low turnover and a long length of life, it becomes similar to the traditional fixed-matching environment. Yet another variation would be to consider stochastic lifetimes. We leave variations in these aspects of our dynamic, heterogeneous agent environment to future research.

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