Title: The Origins of Human Pro-Sociality: A Test of Cultural Group Selection on Economic Data and in the Laboratory

Authors: Patrick Francois[1*], Thomas Fujiwara[2], Tanguy van Ypersele[3]

Affiliations:

[1]University of British Columbia, Vancouver School of Economics, and Canadian Institute For Advanced Research.

[2]Princeton University, Department of Economics, and Canadian Institute for Advanced Research.

[3]Aix-Marseille University (Aix-Marseille School of Economics), CNRS & EHESS, France.

*Correspondence to: patrick.francois@ubc.ca

†We have benefited from the comments of seminar participants at a large number of conferences and departments.

Abstract: Human pro-sociality towards non-kin is ubiquitous and almost unique in the animal kingdom. It remains poorly understood though a proliferation of theories has arisen to explain it. We present evidence strongly consistent with a set of theories based on group level selection of cultural norms favouring pro-sociality. The evidence is drawn from survey data and from laboratory treatment of experimental subjects. The findings provide support for cultural group selection as a contributor to human pro-sociality.

One Sentence Summary: Consistent with theories of cultural group selection, increases in competition increase trust levels of individuals who: 1. work in competing firms, 2. live in states were competition increases, 3. move to sectors where competition rises and, in the lab, get placed into groups facing higher competition.

Main Text:

Introduction

No small part of the spectacular success of the human species is due to our unusually high levels of cooperation among non-related individuals. The scale of such cooperation in human non-kin is rare in the animal kingdom, unique among mammals, and strongly at odds with our closest genetic relatives. But the origins and reasons for the continued existence of such pro-sociality is still an ongoing and important puzzle.[1]

---

[1] An opinion shared by the editors of Science magazine in 2005. The question "How did cooperative behavior evolve?" was rated one of the 25 big questions facing science over the next quarter century. See also Richerson et. al. (2016) which features a general discussion of the puzzle of human prosociality, and Bowles and Gintis (2013 p.6) who also reference this discussion in motivation.

The variety of theories proposed to explain it are typically hard to empirically assess.[2] Their predictions concern elements of our primordial past, perhaps traceable via the archeological record, or rest on non-observables that are not, for the most part, readily discernible. But a class of theories that can be grouped under the heading of Cultural Group Selection (CGS) provide an exception that will allow us to scrutinize contemporary evidence for them. And we report on that evidence here.[3]

Scope of the present study

CGS posits that our "social" world, co-evolved with our "social" instincts. As a species, we evolved a psychology expecting life to be structured by moral norms and we developed features designed to learn and internalize norms.[4] By at least 70,000 years ago most human populations resembled the hunter – gathering societies of the ethnographic record, i.e., tribal scale societies of a few hundred to a few thousand people. And competition across these populations induced selection of group beneficial (prosocial) but individually costly traits (in the form of normative prescriptions or culture). The content of these norms was not fixed, nor were they hard-wired

---

[2] Examples of alternative theories are reciprocal altruism, such as Hoffman et. al. (1998), or sexual selection Aoki (2004). Kin based altruism cannot explain altruism to non-kin unless it can explain why misplaced application of this to non-kin is not selected out, such as the mis-match hypothesis of Tooby and Cosmides (2005). Genetic group selection would depend on large heritable differences across groups; see footnote 5. A comprehensive test or refutation of these other theories is of course beyond this paper.

[3] It is notable that Richerson and co-authors (2016) have highlighted the relative paucity of studies testing this hypothesis — "So far, too few quantitative studies have been performed on CGS and competing processes to allow for much but qualitative judgments". Perhaps this is because, as Bowles and Gintis (2013) note "Conclusive evidence about the origins of human cooperation will remain elusive given the paucity of the empirical record and the complexity of the dynamical processes involved. As in many problems of historical explanation, perhaps the best that one can hope for is a plausible explanation consistent with the known facts." Evidence supporting CGS for a group favourable trait has been argued using observed group extinction rates amongst tribal groups in Papua New Guinea by Soltis et. al. (1995). Because group disbandment is infrequent, this leads to extremely slow selection — 500 to 100 years. The group of selection of focus here, contemporary firms, will be subject to much more rapid disbandment.

[4] See Richerson and Boyd's (1998) tribal social instincts hypothesis, and the evidence suggesting that we are highly evolved social learners further discussed in Richerson and Boyd (2010).

behavioural imperatives.[5] But "selection" occurred as societies with the fitness enhancing norm/institution combinations proliferated.[6] The ones able to generate pro-sociality "won" the evolutionary battle, and the proliferation of such pro-sociality today is a reflection of the winners of that battle.

What is unique about these theories is that they don't just require that the forces of selection were present *at some time* in our historic past. Since they emphasize the non-hard-wired feature of behaviour, and the malleability and changeability of group level norms, successful groups with pro-social norms are always potentially threatened by free-riders. So, forces of group level competition favouring selection on a pro-social "culture" must continue to be present today for CGS to be able to explain the varieties of human pro-sociality that we continue to observe today. In short, according to CGS, where pro-sociality exists, necessarily, group level competition must also exist to create the selective pressure for it. One way of testing this would be to see if features that help in sustaining pro-sociality are more prevalent in groups subject to greater selective pressure — for instance if more frequent inter-group conflicts increase individually costly but group beneficial behaviour such as altruistic punishment; as discussed in Fehr and Fischbacher (2003). But another way to proceed, that does not test for the presence of a single specific behavior, such as altruistic punishment, would be to note that according to CGS any society exhibiting higher frequency of pro-sociality should exhibit, at least for some relevant "groups" of individuals, more intense selection via inter-group competition.

A test of this hypothesis at the country level will never be compelling. Too many factors vary at country levels of aggregation for any correlation between the intensity of selective forces and pro-sociality observed there to be interpreted as causal. Within country tests are more compelling as the variables that differ across countries are ruled out. By studying the individual level, where finer observation can be applied, factors that differ across individuals can also be measured and controlled for. Better still, in the case of panel studies (where the same individuals are tracked through time) changes observed in group level competition that effect only some of the observed individuals can be directly matched to corresponding changes in their pro-sociality.

---

[5] Theoretically, selecting over a behavioral disposition, as posited by a CGS explanation for pro-sociality, is argued to have advantages over direct genetic selection of behaviours. This is because it gives human societies the capacity to adapt quickly to changed conditions. Empirically, as Bowles and Gintis (2013) note, the evidence supportive of direct genetic causes of behavior is non-existent: "No 'gene for cooperation' has been discovered. Nor is it likely that one will ever be found, for the idea of a one-to-one mapping between genes and behavior is unlikely given what is now known about gene expression, and is implausible in light of the complexity and cultural variation of cooperative behaviors." Further, as Richerson et. al. (2016) document, humans (across societies) vary little (if at all) in innate psychology but vary greatly in prevailing norms regarding prosocial behaviour.

[6] Three types of selective forces are emphasized: Natural selection, wiping out the other less successful groups via competition over resources or direct conflict, or having higher growth rates. 2. Imitation of successful groups by less successful. 3. Selective migration and internalization of norms upon migration. See Henrich (2015) for a detailed discussion.

In order to turn to data that would allow us to undertake such tests, two questions must first be answered: 1. Today, what are the relevant "groups" over which selection occurs, and over which competition is to be gauged? 2. How is individual pro-sociality to be measured?

Measuring Groups

The most ubiquitous avenue of group level competition occurring in contemporary settings is competition across firms. Individuals within them need to undertake (at least some) group beneficial but individually costly actions. Moreover, selection acts across firms through market forces. There is already considerable evidence showing that a degree of cultural learning occurs through workplace interactions.[7]

Measuring Pro-sociality

We will use the Generalized Trust Question, or a close variant of this, as our proxy for the prevalence of pro-sociality, in all of the empirical results we report here. "Do you think that, on the whole, people can be trusted, or that you can't be too careful in dealing with people?". This question imagines a "weakly institutionalized" setting: "Answering this question, subjects consult either their own experiences and behaviors in the past or introspect how they would behave in situations involving a social risk"; Fehr (2009).

Survey based questions of individual trust have been found to reflect variation in the degree to which subjects perceive the degree of pro-sociality of individuals around them. Laboratory based validation studies of the generalized trust question suggest a few important features of this question which make it suitable for measuring pro-sociality. Firstly, in the laboratory, generalized trust reported by individuals seems to be malleable and influenced by specific experiences. Secondly, at least for significant monetary stakes, beliefs about the trustworthiness of others seem to matter for informing one's own potentially costly trusting decisions, and correlate with answers to the generalized trust question; see Sapienza, Toldra-Smits and Zingales (2013). Thirdly, individuals tend to respond to trustworthiness experiences by increasing their own trustworthiness. This is consistent with individuals being norm followers. So the generalized trust question has been found to reflect both an individual's own trustworthiness and their perceptions about the trustworthiness of others which, if they are norm followers, will tend to be related. Beliefs about others' willingness to contribute seem to influence subjects' perceptions about pro-sociality, and can make their own pro-social behavior conditional upon that of others.

---

[7] Richerson et. al. (2016) argue for firms as one such avenue of group competition and selection that CGS works through today. A number of studies have shown how elements of a firm's culture can affect the attitudes and beliefs of employees, and how those attitudes can exert selective pressures on firms. This is consistent with Nelson and Winter's (1982) classic view of the firm as a repository of tacit knowledge that is hard to transmit, and that firms with the right types of knowledge will have advantageous expansion. Ashforth, Harrison and Corley (2008) present evidence linking the social identity of employees to the performance of firms.

As would be the case if they want to conform to a perceived norm. The behavior of others is then a signal for the appropriate behavior corresponding to the norm.[8]

Schematic contents

The evidence we present is drawn from four sources: 1) US cross-sectional correlations between competitiveness of sector of employment and individual trust, 2) macro-level policy changes that altered cross-firm competition at US state levels inducing changes in individual trust, 3) German panel data evidence showing changes in individuals' sector of employment competitiveness induced changes in individual trust. All three forms of evidence confirm a strong statistically significant effect of increased competition across firms on increased individual trust that is consistent with CGS.

We augment these findings with: 4) experimental evidence drawn from laboratory experiments in France. In the laboratory we place subjects into groups where group level rewards are shared across members in a public goods game setting. We manipulate the degree of competition across groups in a way intended to mimic the variation in competition across firms in the data. We test to see whether this variation replicates the correlations observed between competition and generalized trust in the data. It does. Increases in competition across groups leads to increased generalized trust reported by individuals within the groups.

The pattern of subject behavior suggests a likely channel of effect. Cross group competition increases the frequency of group beneficial behaviour; this increases lab subjects' confidence that their partners will cooperate, and their own level of cooperativeness, which they extrapolate to the broader world when answering the generalized trust question.

Results

Cross-sectional evidence in the US.

By its nature, cross-sectional data provides the weakest test of the CGS hypothesized positive effect of competition across firms on the levels of worker pro-sociality within them because a correlation between the two may reflect the effects of omitted variables in driving both. However the labor force module asked of workers in the United States General Social Survey's (GSS) 2004 wave has advantages in mitigating some of these concerns. This wave of the survey

---

[8] Reciprocally, individuals may use introspection to imagine what individuals who are similar to them would do in a similar setting, but be unwilling to use this when they don't perceive such similarity — as conjectued by Sapienza et. al. (2013) to explain contrasting findings reported by Glaeser et. al. (2000) and Fehr et. al. (2003). Reciprocity may also play a role in such behavior, and studies trying to discern whether social conformity or reciprocity are the drivers tend to find evidence for both; see Bardsley and Sausgruber (2005), Falk (2004) and Bohnet and Zeckhauser (2004).

extensively focused on the labor market, yielding detailed information about the nature of the workplace of survey respondents. This allows us to control for many factors that may be affecting the generalized trust level of individual respondents, which we are using to proxy for pro-sociality, as well as rich personal information about respondents that would allow us to control for individual characteristics known to correlate with individual trust.

The competitiveness of each worker's sector of employment is measured by using the Census Bureau's five year survey of the population of US firms to determine the percentage of sales covered by the n largest firms ($n = 4, 8, 20, 50$) in North American Industrial Classification System (NAICS) sectors. All individuals in the GSS with an industry code are matched to a competition measure at the NAICS level from the census of firms. Our reported measure of competition is that using the sales measure for the top 50 firms, "Comp50", which is computed by subtracting the concentration measures from 1. Thus Comp50 for sector x is the percentage of total sales in x that is NOT covered by the largest 50 firms in that sector.[9]

Figure 1 displays a binned scatter plot cutting the 614 observations in to 25 equal sized bins arranged by sectoral competitiveness (the x-axis) plotted against share of workers reporting affirmative answers to the generalized trust question (the y-axis) after controlling for individual level economic and demographic controls. The line is fitted from the un-binned data, so it perfectly matches the full sample of data and controls in the Supplementary Materials accompanying this paper. The positive slope of 0.191, statistically significant (p value 0.011) and large is consistent with the predictions of CGS. The other columns of the table in the supplementary materials show that this finding is robust to including the rich and unique set of workplace controls obtained from the GSS workplace module.

A suggestion of causality is provided by considering the effect of potential experience. The individuals likely to have had the longest exposure to the labor market are the ones for whom the effect of sectoral competitiveness has the most impact on trust; reported in the supplementary materials.[10]

Despite the inclusion of rich workplace controls, as noted above, such correlative evidence, though consistent with CGS, is a long way from evidence for the causal relationship indicated by CGS. The possibility of omitted factors potentially affecting both variables cannot be discounted. A potential solution is to identify sources of variation that would alter competition between firms — without themselves having direct effects on trust levels — such variation has been caused by episodes of US banking deregulation which we turn to next.

Banking Deregulation in US States

---

[9] Results for Comp4 (1 minus the share of sales of 4 larger firms), which are largely similar, are reported in the supplementary materials. Results using variables constructed with the shares from the 8 and 20 largest firms are even closer to those using Comp50 and are not reported, but are available upon request.

[10] This is only a "suggestion" of causality as there is no direct information about individual time spent in the labor market. It is estimated by taking age minus years of education to compute labor market experience. This too is not guaranteed to have occurred in the same sector.

Starting in the mid-1970s, a series of technological innovations and more sophisticated credit-scoring systems radically reduced the cost of using banking services from a distant branch. These changes lead US states to lift interstate and intrastate restrictions from then on, allowing banks from out-of-state to operate in their borders (*interstate* deregulation) and permitting banks to operate multiple branches, either by opening new ones or by merging with other banks, in any part of the state (*intrastate* deregulation). Of particular interest for our research design, different states undertook deregulation at different times (The supplementary materials includes the dates for each state). The timing of the reforms is not correlated with previous trends in the creation of new firms, but generated a large increase in the number of (non-financial) firms and hence competition, after they were implemented. The deregulation of branching restrictions resulted in a more competitive (by breaking local monopolies) and efficient (by allowing mergers) banking sector, and more availability of credit. This, in turn, facilitated the creation of new firms and raised the contestability of local markets. As a proxy for state-level competition, we use the number of new incorporations per capita measured by the Dun and Bradstreet Corporation from official state records and new firm closures.

Figure 2 plots a set of variables that indicate the number of years until an interstate deregulation is enacted relative to the effect on the date of the reforms' enactment ("year zero"), which is normalized to zero.[11] This controls for state and year fixed effects and state-specific linear trends, ruling out state differences that are fixed or vary linearly through time, as well as common nationwide factors that may evolve nonlinearly, such as the business cycle. The red and green lines, rising steadily from each state level deregulation event, indicate the (log of) firm entry and exit per capita; these are reproduced directly from Kerr and Nanda (2009). And, as shown by the upward trajectory that commences at the normalized year zero of banking reform (which varies in its calendar time for each state) state level competitiveness increases with the reforms. It continues to do so until the end of the sample. This is consistent with the posited effects of increased credit availability on competition. The pattern for years prior to the reform reassures us about the identification exercise. Changes in competition after reforms were, in fact, not being caused by pre-existing upward trends in competition. There is no pre-trend in competition.

We now augment this previously known finding about firm level competition increases from banking deregulation with information about individual generalized trust levels obtained from state level GSS.[12] The blue line reports responses to the generalized trust question for individuals, averaged at the state level, timed to correspond to the banking deregulation analogous to the new firm incorporations. The blue line again shows no pre-trend in state level trust that predicts or preempts the banking deregulation. At time zero trust is largely unmoved and remains so for the first three years. At year 4 after deregulation state level trust starts to track up, seemingly increasing hand in hand with the increase in new firm incorporations. The supplementary materials provide the details and statistical tests. Estimates of the preferred

---

[11] We find similar results for intrastate deregulation's effects; reported in the supplementary materials.

[12] Black and Strahan (2002) have shown that both forms of banking deregulation had a sizeable positive effect on firm entry. Though the GSS is publicly available, obtaining these identifiers requires getting cleared access to individual of state of residence information. Though this is not freely downloadable, for purposes of replication it can be readily obtained by contacting the GSS and meeting use and storage protocols. We are grateful to the GSS for allowing us to use this data.

specification elaborated there imply that a state enacting an interstate banking reform would experience a 1.6 percentage point increase in the share of its population reporting that they "Can Trust" every year after the reform. Since these estimations control for common time trends, and state specific linear trends, it is highly unlikely that the increase in trust is being caused by some other factor induced by the changes in banking deregulation, nor (due to the particular pattern of changes) by an omitted factor correlated coincidentally with trust. This evidence supports a causal interpretation. Increases in firm level competition at the state level lead to a rise in individual level trust — precisely as would be posited by CGS.

Though nicely strengthening the claim of a causal relation, a problem with this type of macro (state) level correlation is that although the causal relationship corresponds with that posited by CGS it is not possible to track whether the mechanism of effect corresponds to that predicted via CGS. Competition at the state level seemed to induce trust increases at the state level, but did it occur through the channel of firm competitiveness on workers as CGS would predict or though some other channel?

To study this, we need to track individual workers through time, observe changes in the competitiveness of an individual's sector of work, and then observe how this tracked with that individuals reported trust. This, to our knowledge, is impossible to do with US data; no survey tracking individuals over a significant length of time has asked the trust question across multiple surveys and simultaneously reported their sector of work. But a dataset with precisely such details does exist for German workers, and we turn to analyzing this now.

The German Socio Economic Panel.

The German socioeconomic panel (SOEP) has three waves asking a version of the generalized trust question and including information on sector of employment: 2003, 2008, and 2013.[13] The survey also reported each individual's industry of work — which we matched to a Herfindahl index measure of competitiveness obtained from the ORBIS database by NACE code.[14] Since we wish to track the same individuals through time, and measure the effects on them of changes in the competitiveness of their sector of employment, we drop workers from the public sector and keep individuals in the sample only if they were employed for two consecutive waves.[15]

In terms of sectoral competitiveness rankings, there is not much change over this ten years, and certainly not enough to identify the effect of a change in competition across individuals who did

---

[13] Do subjects agree with the statement "on the whole one can trust people." We Code a binary trust indicator (1 iff totally or slightly agree). The possible 4 answers: "totally agree", "agree slightly", "disagree slightly", "totally disagree" (10% "totally agree" or "totally disagree"). The results we shall present are robust to how we treat these.

[14] Insert details on ORBIS, details on SOEP, details on NACE in supplementary materials.

[15] This yields a total of 9103 observations and 6447 unique individuals. 5004 people employed both in 2003 and 2008, 4099 employed in both 2008 and 2013.

not change sector.[16] So, instead, we explore the effect of changes in firm level competitiveness by tracking individuals who changed sector.[17] Some individuals moved to jobs in more competitive sectors (where CGS would predict they increase their trust levels) others stayed put (where CGS predicts there should be no change), and others moved to sectors with less competition (where trust should fall according to CGS).

Figure 3 is a visual summary of the results. Each dot in the graph is a binned average: we cut the x-axis variable (change in competition) in to 25 bins of equal size from negative to positive changes (for those that moved sectors that makes around 90 observations per bin). This is plotted against the average change in trust per bin. The upward sloping relationship is based on the original (unbinned) data its positive (slope 0.045 with p value 0.016) and again coincides with the prediction of CGS. Individuals with the largest positive changes in sectoral level competitiveness had the largest increases in trust. The red X in the figure denotes the average change for non-movers. These have changes, essentially zero, which look just like the changes in trust for individuals who moved across sectors that were competitively similar. This finding is not due to individuals in more competitive sectors experiencing higher incomes or any such other correlates.[18] Nor is it due to unobserved changes that might have increased individual trust while somehow moving those people to more competitive sectors, ruling out a possibly reverse causal effect.[19]

The most plausible explanation for this finding is that the individuals who changed jobs and ended up in more (less) competitive sectors increased (lowered) their levels of trust. This data then provides stronger causal evidence of competition in sector of employment affecting trust. However it still leaves us uncertain as to whether the mechanism through which this effect could be working is that posited by CGS. We attempt to test that next.

A Laboratory Experiment

Short of a randomized control trial, with subjects whose work settings can be manipulated by the experimenter — it is not possible to directly test CGS. A large scale manipulation of work place competitiveness settings is not feasible. However, it is possible to complement workplace data —

---

[16] There is one caveat to this. A large change in measured competitiveness did occur between 2003-2008. But this almost certainly seems to be due to changes in reporting protocol for ORBIS which made us skeptical about this being anything other than measurement error. Consequently, we do not report the results obtained by use of this variation and instead keep constant competition levels by sector by using the more reliable 2008-20013 numbers where a constant protocol was used. Interestingly, the findings we report here are also confirmed using that variation, and these are available on request.

[17] 25.4% of observations involve someone moving from one industry to another. The average level of trust is 65.1%, and 71.1% of people report the same trust level in two consecutive periods (5 years apart). 14.1% move from “trusting” to “not trusting”. 12.8% move from “not trusting to trusting”.

[18] A figure documenting this is included in the supplementary materials. Individuals moving to more competitive sectors had — if anything — lower levels of income, though not statistically significantly so.

[19] Lagged increases in trust do not predict moving to more competitive sectors; see the corresponding figure in supplementary materials.

where variation cannot be controlled, and where observations are limited by survey design — with experimental data derived from the laboratory. In a laboratory setting, researchers can place subjects in settings where rewards are allocated based on group level outputs while altering the competitiveness that the groups experience across treatments. By observing differential levels of competition exposure across individuals, one can test causal mechanisms more directly. A drawback of the laboratory however is that the setting is artificial — not an actual workplace — and limited in the degree of exposure. If competition takes a long time to affect trust levels, we will never be able to detect such effects via such methods.

Mindful of these drawbacks, and the potential to find very limited effects of competition, we conducted a set of experiments starting in the fall of 2015 and ending in early 2016. It is already known from previous experimental work that subjects placed together in groups and asked to contribute to a collective good — the well-known public goods game (PGG) — can have their contributions to the game substantially increased by putting them in group competitive settings.[20] But do the effects of increasing competition also induce higher levels of trust? And if so, why? We explored these questions in a pool of subjects from the Paris School of Economics.

Subjects played the PGG in two different treatments. The first (control) was the standard public goods game. Individuals were endowed with 10 euros per round, and could decide how much they would contribute to a collective good that would benefit all members of their group equally (there were 2 individuals in each group). By giving up x of her own private endowment, the amount of the collective pool (shared equally by both) would increase by 1.5 times x. Thus benefiting the subject by only $.75x$ and therefore being a net cost to the subject. The payoff maximizing dominant strategy is thus to contribute nothing in this game, and both individuals in each group doing so is the unique Nash equilibrium of the game.

Individuals were matched anonymously, told the outcome and contribution of the other player they were paired with, then re-matched after playing once with that player. The re-matching was with another anonymous individual, with whom they had not been previously matched, and the non-repeated nature of the setting was made clear. This one-shot interaction was repeated 19 times per session, and subjects were rewarded based on their payoffs computed in one randomly chosen round of the game.

Before playing, subjects filled out a questionnaire regarding their particulars — education, occupation (if they had one, most were students), subject area, age, gender etc. After playing, subjects were asked a number of questions drawn from the General Social Survey — one of which was the generalized trust question.

The dashed red line in Figure 4 depicts the median contributions of players over the multiple rounds. As in almost every other experimental version of this game, the figure displays a declining pattern of contributions. Individuals start out contributing at a median level around 2

---

[20] See Burton, Chellew et. al. (2010), Gunnthosodir and Rappoport (2006), Markussen et. al. (2014), Cardenas and Mantilla (2015). The nature of the competition and its effects on equilibria matter a lot in such settings. The closest to ours, Reuben and Tyran (2010), which we discuss subsequently, made us confident that we would be able to induce more contributions via inter-group competition; which turned out to be correct.

Euros of their endowment — and gradually this tracks downwards throughout the rounds ending with a median well below 1 in round 19. This may be evidence of individuals learning the optimal strategy in the game, though other experiments focused on explaining such patterns lead one to doubt this interpretation.[21] This declining pattern is not our focus here so we do not address it further.

The remaining (approximately half) of the subjects were randomly placed in a "competitive" treatment. Here, the amount they received from the collective pool depended not only on the joint contribution of themselves and partner, but also on the size of their joint contribution relative to that of a randomly allocated matched group. If, and only if, their joint contribution to their group equalled or exceeded that of their matched group, did they receive their share of the collective account. The collective account was computed exactly as in the control group; total contributions to it were multiplied by 1.5 and shared equally by both members.

Contributions under this treatment are, of course, less certain to create benefits both for the group and for any individual contributing. All players contributing zero remains a Nash equilibrium of this game. But the competitive treatment also gives rise to equilibria with contribution levels that far exceed the standard public goods game of the control.[22] In fact, any positive level of contribution becomes a symmetric Nash equilibrium of this game. For example, if a subject expects all other players in the game to contribute the full amount, contributing any less than that leads to zero payment from the collective pool. However, by contributing the full amount of 10 euros, the pair's collective account will have 30 Euros. If the other group does the same, then, since no group dominates, each subject in both groups is paid 15, yielding this as another equilibrium. The same reasoning can be shown to support any other symmetric contributions as Nash equilibria of this game.

As in the control treatment, subjects were re-matched after each round (with an anonymous partner) the pair was also re-matched (again anonymously) with a different randomly allocated competitor pair and the game was repeated for 19 rounds. Subjects were informed about the contributions of their partner in the previous round, and about the total contribution of the other group in that round too, before making their current round decision. The same pre- and post-questionnaires were administered as in the control (non-competitive standard PGG), so that generalized trust levels were also measured after participation.

---

[21] Andreoni (1988) suggests this is not the case as subjects seem to re-set to higher levels after experiencing declines when they are matched with a new group. Fischbacher and Gachter (2010) present evidence that heterogeneity in conditional cooperation tendencies underlies the decline in the usual repeated public goods game.

[22] See the supplementary materials for full details of the game, characterization of equilibria, and full set of experimental instructions.

As the blue line in Figure 4 shows, competition did induce higher levels of contribution in the public goods game across all rounds, and a markedly different experiment progression effect.[23] Median contributions start below 5, tracking up dramatically over the first few periods, from there they reliably remain above 6 Euros, dipping down below that only in period 19. The pattern of decline exhibited in the standard (control) PG game does not appear. The level being higher across rounds is consistent with subjects inferring the possibility of Nash equilibria at higher levels of contribution, and consistent with the one other experimental study that placed subjects in a similar group competitive setting; though differences in our design purposefully made to explore CGS, make the results not directly comparable.[24] Due to the complexity of computing equilibria here, and the fact that we provided no instruction as to the likely equilibria, it seems unlikely that many of the subjects were able to understand the equilibrium structure of the game; most subjects were non-economics students not trained in game theory, and major of study does not correlate with play in the game.

We next check whether we were able to replicate the correlations between competition and trust found in the data in our experiment. Indeed they were. 67% of subjects in the competitive treatment answer the generalized trust question in the affirmative (5 or higher on the provided 10 point scale), which is represented by the blue dot in Figure 4, with 95% confidence interval displayed. In comparison, only 51% do so in the non-competitive treatment; the red dot in Figure 4. The difference is statistically significant, p-value 0.011, and robust to all specifications, see supplementary materials for details.[25]

So, competition across groups did increase generalized trust amongst group members. But did generalized trust increase due to the mere effect of putting individuals in a competitive setting? Or was it somehow related to the fact that individuals contributed more in the competitive setting? To get at this we break down the pattern of contributions via the progression of the experiment in the competitive setting. This break down shows that there was considerable

---

[23] The mean contribution (as the means are what is statistically tested) in the last 10 rounds of the game for the control group is 0.205. The effect of competition is 0.384, se=0.062, t=6.16, p-value=0.000, n=180. The specification controls for age and gender.

[24] Reuben and Tyran (2010) have a similar structure yielding multiple Nash equilibria; ties allowing winners to be rewarded. They also find increases in contribution despite some sharp differences in our respective set-ups. Our groups are smaller. But the biggest difference is that groups in Reuben and Tyran's setting are matched in round one and play repeatedly with the same partners against the same competitor groups from then on. In our set up the game is not repeated, groups are re-matched, as are competitors, across the rounds. So we have set up repeated one-shot interaction rather than a repeated game. Our case is most similar to their treatment's 1 and 2, which were the most competitive treatments, and our increases in contribution also accord with the magnitude of theirs. There is a literature, discussed in Reuben and Tyran which has previously found that concerns about the other group losing if one contributes too much can crowd out individual contributions. This is partly mitigated by allowing rewards when ties occur — i.e., "all-can-win" intergroup competition; which we allow.

[25] The statistical significance of this difference at conventional levels persists with or without controls as the competitive and non-competitive groups are quite well balanced, and independently of the error correction used to take into account the small number of clusters, again elaborated in the supplementary materials.

heterogeneity across individuals in how they proceeded to adjust their payments through time. The two panels of Figure 5 display this heterogeneity in contribution patterns for the non-competitive and competitive treatments (left and right panels respectively). The width of each median band is the frequency of a pattern type amongst subjects, and subjects were pooled by whether they had a positive trend — 'increasers' — which are red, a negative trend — 'decliners' — which are blue, or were flat across rounds. Decliners are those exhibiting a pattern similar to the standard PG game of the control, and these predominate (around 70% of subjects) in the non-competitive control treatment of the left hand panel of figure 5. The increasers start at a similar level to the decliners in the competitive treatment (right panel), but rather dramatically increase their contributions across the competitive rounds. In fact, the median of the increasers (red band, around 50% of subjects) is contributing almost the full amount to the collective pool by the final round of the competitive treatment.

It turns out that the increasers are the individuals who have the highest levels of trust, and these are the individuals who buoy the trust levels in the competitive treatment, and make it significantly higher than the non-competitive.[26] But why do increasers contribute more on average, and an increasing amount across rounds, and why does this make them more likely to report higher levels of trust? To get at this we explore the effects of the random matching of individuals through time; a feature which our experimental set-up — that re-matches groups and competitors each period — uniquely allows. We conjectured that subjects might be induced to become "increasers" when they experienced high levels of partner contributions previously. This is indeed the case. The average of lagged partner contributions positively predicts a subject's own contribution. Moreover, by isolating the variation arising from the random allocation of the ordering of partners we can explore whether it is the individuals who experienced higher levels of partner contributions who are the ones more likely to trust. This is indeed the case. Randomly being allocated to higher contributing partners positively affects ones own level of reported trust.[27]

There are a few reasons why reported generalized trust might be increasing when individuals are allocated high contributing partners, and then increase their own contributions in response. It could be because subjects get paid more, experience a warm glow, and think more positively about the anonymous other in general — they respond by contributing more themselves and hence report higher levels of trust. Or it could be because individuals obtain information about the anonymous other based on their experiences in the experiment. To get at this, we explore whether the effect of being paired with high contributors differs if those high contributors were your partners (in which case they raise your payoff from the experiment) or if they were in your competitor group (in which case their higher contribution serves to lower your payment). The informative content about the general cooperativeness of others is similar whether it arises from your partner or an opposing group member, but its payoff effects oppose. So if information is affecting subject beliefs and therefore raising trust, we should expect the effect to be symmetric

---

[26] Statistical tests in the supplementary materials.

[27] We explain fully the details of the Instrumental Variables strategy, that allow for this conclusion in the supplementary materials. Briefly, we instrument for own contribution using partner's previous levels. The variation in partner levels thus comes from the random ordering of partners experienced through the experiment. This instrumented contribution increases a subject's own trust.

and independent of whether it is a partner or a competitor. If it is a warm glow, positive effects should only be (or be more strongly) induced by partners than competitors.

We find the former. The effect of a higher contribution on trust is identical whether it comes from a previous round competitor or a previous round partner. This strongly suggests that increased trust levels induced by the competitive treatment are not being driven by some sort of warm glow experience of subjects. It seems instead that some individuals, being positively surprised by the levels of contributions they observe both by partners and competitors, contribute more in response themselves as the experiment progresses. These are the individuals who are then more likely to report that individuals can be trusted at the end of the experiment.

Interpretation

This mechanism looks a lot like that forwarded by proponents of CGS, which is itself consistent with previous findings of conditional cooperation in subjects.[28] Many subjects seem willing to cooperate (even at some payoff cost) because they care more than just about monetary payoffs. But they fear being duped; i.e., contributing when others do not. They enter into new situations unsure about what will happen — as in our laboratory setting. And play cautiously at first. Some find, however, through their experience in the game, that contributions by both partners and competitors are higher than expected. They react to this in two ways —firstly, being conditional cooperators — they increase their own contributions, even though they will not be matched with the same partner again because they believe that future partners will also contribute more. Competition across groups seems good at inducing increased group beneficial contributions. But secondly, it seems to also change their attitude about the "anonymous other" as unearthed in their response to the generalized trust question. They think others can be trusted, and they themselves seem to be acting in ways that are more trustworthy. Subjects seem to extrapolate from the trustworthiness of their partners, and even the individuals they are playing against in the experiment, to the wider world. This type of extrapolation has been reported before in laboratory settings, and suggests that instead of individuals being strongly driven by an intrinsic propensity to trust, or distrust, they employ a "social learning perspective" as suggested by Paxton and Glanville (2015). An individual's trusting attitude, as reflected in answers to the generalized trust question, is a reflection of their experiences and not a hard and fast world view.

Cultural Group Selection provides a coherent interpretation of both sets of evidence.

Interpretation of the survey data

---

[28] Fehr (2009) reference here and Fischbacher and Gachter (2010) further discussion.***Add discussion of Thoni (2016) here and perhaps elsewhere. Firstly, in his survey, strong conclusion that contributions in the PGG is due to beliefs about what others will do. Strong evidence that people are conditional cooperators. Quote from p.9 of his paperSSRN version "Taken together, results from various subject pools suggest that survey measures of trust and expected fairness are informative with regard to the cooperativeness of individuals. The prominent and widely used Trust question seems to be a measure for preferences for cooperative behavior, i.e., the willingness to take a leap of faith when entering a social dilemma." This is exactly what we conclude….

Increased competition across firms exposes subjects to increased group beneficial behavior on the part of their co-workers, and increases their own such behavior. It is already known that the workplace is a venue where people learn about the norms that others hold and appear to modify their own normative prescriptions. In inter-group competitive workplaces workers experience, and themselves internalize, more prosocial norms. This generates for them a set of prosocial experiences, where they can trust others, and where they themselves are more trustworthy. They then report more positive answers to the generalized trust question which explains the cross-section and panel correlations reported above.

Interpretation of the laboratory experiment

Competition across groups, induces more prosocial behavior from individuals within groups — and the prosocial sentiment extends beyond the group and the laboratory setting, consistent with subjects using these experiences to update their beliefs about the world — as evidenced in responses to the generalized trust question. Relative to the disappointment and decline experienced in standard public goods games, putting subjects in a competitive treatment allows group cooperative behavior to be more frequently induced. Subjects experiencing evidence of this cooperative behavior (either via their partners or their competitors), increase their own contributions too. Their belief in a prosocial world goes up and they trust more. Generalized trust increases due to inter-group competition.

Conclusion

Our competitively treated subjects in the laboratory do seem to have raised their beliefs of the possibility of a prosocial world, and made a trustworthy "other" seem more likely. If beliefs have been altered, and not just moved around temporarily, then this could have important effects. Whether such permanent effects flow from limited laboratory exposure is hard to tell. But the possibility of longer-term effects arising from interactions in the workplace seem much more likely. Such interactions would seem to operate in a way that is consistent with our laboratory setting, with the data we have presented, and with what Cultural Group Selection would predict if it was an underlying factor contributing to human pro-sociality.

References

Andreoni, J. (1988) Why Free Ride - Strategies and Learning in Public-Goods Experiments. *Journal of Public Economics*, 37(3): 291-304.

Aoki, K. (2004) Altruism May be Sexy: Comment on Cultural Group Selection, Coevolutionary Processes and Large Scale Cooperation, *Journal of Economics Behavior and Organization* 53, 1, 37-40.

Ashforth, B. E., Harrison, S. H., & Corley, K. G. (2008). Identification in organizations: An examination of four fundamental questions. *Journal of Management, 34*(3), 325-374

Bardsley, N. and R. Sausgruber (2005) Conformity and Reciprocity in Public Goods Provision, *Journal of Economic Psychology*, 26,5, 664-681.

Black, S and P. Strahan (2002) Entrepreneurship and Bank Credit Availability, *Journal of Finance*, vol. 57, 6, pp. 2807-2833

Ben-Ner, A. and L. Putterman (2000) “On Some Implications of Psychology for the Study of Preferences and Institutions, *Journal of Economic Behavior and Organization*, 43, 1, 91-99.

Bohnet and Zeckhauser (2004) Trust, Risk and Betrayal," *Journal of Economic Behavior & Organization,* 55, 467-484.

Bowles, S. and H. Gintis (2013) *A Cooperative Species: Human Reciprocity and its Evolution*. Princeton, NJ: Princeton University Press.

Burton-Chellew, M., Ross-Gillespie, A., and West, S. (2010). Cooperation in humans: competition between groups and proximate emotions. *Evolution and Human Behavior,* 31, 104–108.

Cardenas, J. and C. Mantilla (2015) Between-group competition, Intra-group cooperation and Relative Performance, *Frontiers in Behavioral Neuroscience*, 9, 1-9.

Falk (2004) Charitable Giving as a Gift Exchange: Evidence from a Field Experiment. IZA Discussion Papers 1148. Institute for the Study of Labor (IZA).

Fehr, E. and U. Fischbacher (2003) The Nature of Human Altruism, Nature, 425, 785-791.

Fehr, E., U. Fischbacher, B. von Rosenbladt, J. Schupp, G. Wagner (2003) A Nation-Wide Laboratory. Examining Trust and Trustworthiness by Integrating Behavioral Experiments into Representative Surveys”, CESifo working paper 866.

Fehr, E. (2009) On the Economics and Biology of Trust, Presidential address at the 2008 meeting of the European Economic Association, *Journal of the European Economic Association*, 7 (2-3), 235-266.

Fischbacher, U. and S. Gachter (2010) “Social Preferences, Beliefs and the Dynamics of Free Riding in Public Good Experiments, *American Economic Review*, 100,1, 541-546.

Glaeser, E., D. Laibson, J. Scheinkman, and C. Soutter (2000). “Measuring Trust” *The Quarterly Journal of Economics* 115.3, pp. 811–846

Gunnthorsdottir, A., & A. Rapoport (2006). Embedding social dilemmas in intergroup competition reduces free-riding. *Organizational Behavior and Human Decision Processes, 101* (2), 184-199.

Henrich, J. (2015) *The Secret of Our Success: How culture is driving human evolution, domesticating our species, and making us smart*. Princeton, NJ, Princeton University Press.

Hoffman, E. K. McCabe and V. Smith (1998) Behavioral Foundations of Reciprocity: Experimental Economics and Evolutionary Psychology, *Economic Inquiry*, 36, 3, 335-352.

Kerr, W. R. and R. Nanda (2009) Democratizing Entry: Banking Deregulations, Financing Constraints, and Entrepreneurship. *Journal of Financial Economics*, 94, pages 124-149.

Markussen, T., Reuben, E., and Tyran, J.-R. (2014). Competition, cooperation and collective choice. *Economic Journal*. 124, F163–F195.

Nelson, R. R., and Winter, S. G. (1982). *An Evolutionary Theory of Economic Change*. Cambridge, Mass.: Belknap Press of Harvard University Press.

Paxton, P. and J. Glanville (2015) Is Trust Rigid or Malleable? A Laboratory Experiment, *Social Psychology Quarterly*, 78,2,194-204.

Reuben, E. and J. R. Tyran (2010) Everyone is a winner: promoting cooperation through all-can-win intergroup competition. *European Journal of Political Economy* 26, 25–35.

Richerson, P.J., Baldini, R., Bell, A., Demps, K., Frost, K., Hillis, V., Mathew, S., Newton, E. K., Naar, N., Newson, L., Ross, C., Smaldino, P. E., Waring, T. M., & Zefferman M. R.

(2016). Cultural group selection plays an essential role in explaining human cooperation: A sketch of the evidence. *Behavioral and Brain Sciences,* 1-68.

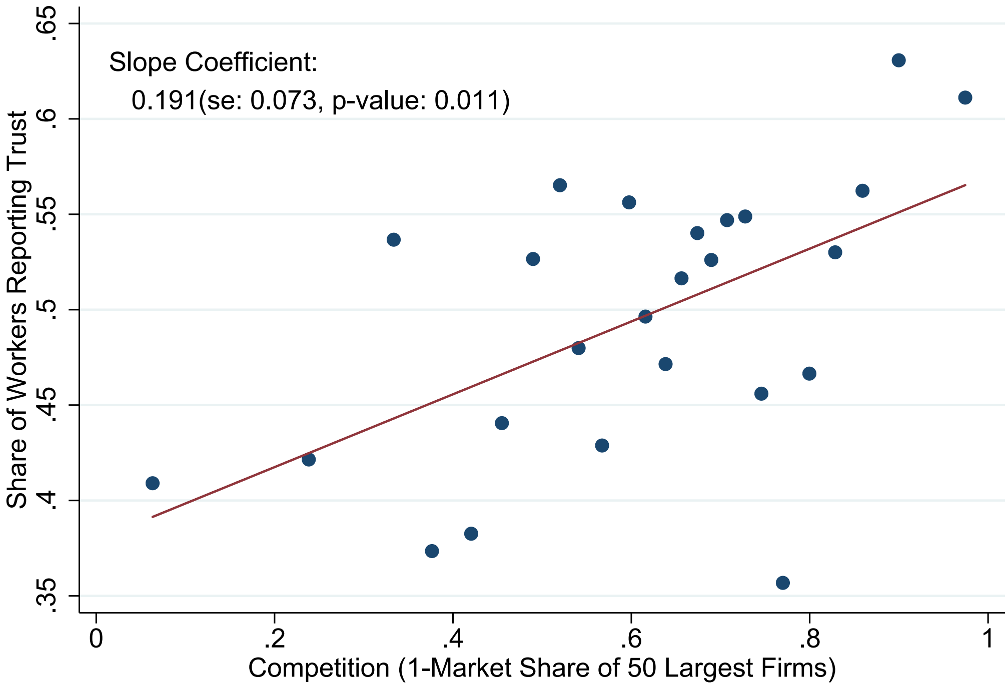
Richerson, P. J., and R. Boyd (1998) The evolution of human ultrasociality. In I. Eibl-Eibesfeldt & F. K. Salter (Eds.), *Indoctrinability, Ideology, and Warfare; Evolutionary Perspectives* (pp. 71-95). New York: Berghahn Books.

Richerson, P. J., & Boyd, R. (2010). Gene-culture coevolution in the age of genomics. *Proceedings National Academy of Science USA, 107*(Supplement 2), 8985-8992.

Sapienza, P., A. Toldra-Simats and L. Zingales (2013) Understanding Trust, *Economic Journal*, 123, 573, 1313-1332.

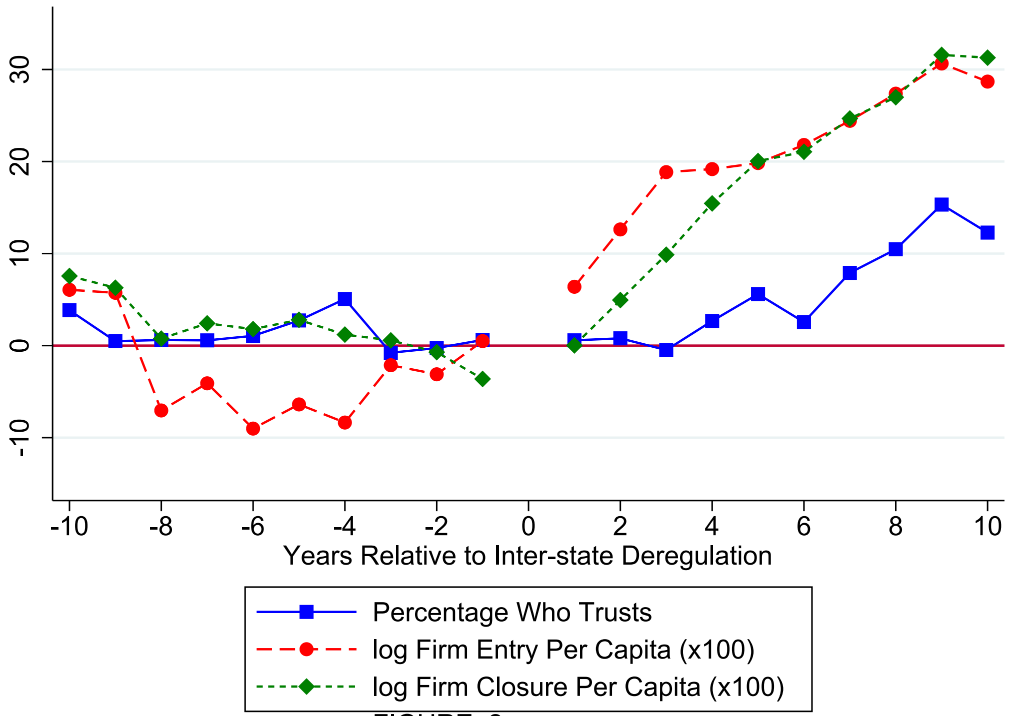
Soltis, J, R. Boyd and P.J. Richerson (1995) Can Group-Functional Behaviors Evolve by Cultural Group Selection? An Empirical Test, *Current Anthropology,* 36(3), 473-483.

Tooby, J. and L. Cosmides (2005) Conceptual Foundations of Evolutionary Psychology, In D. M. Buss (Ed.), *The Handbook of Evolutionary Psychology* (pp. 5-67). Hoboken, NJ: Wiley.



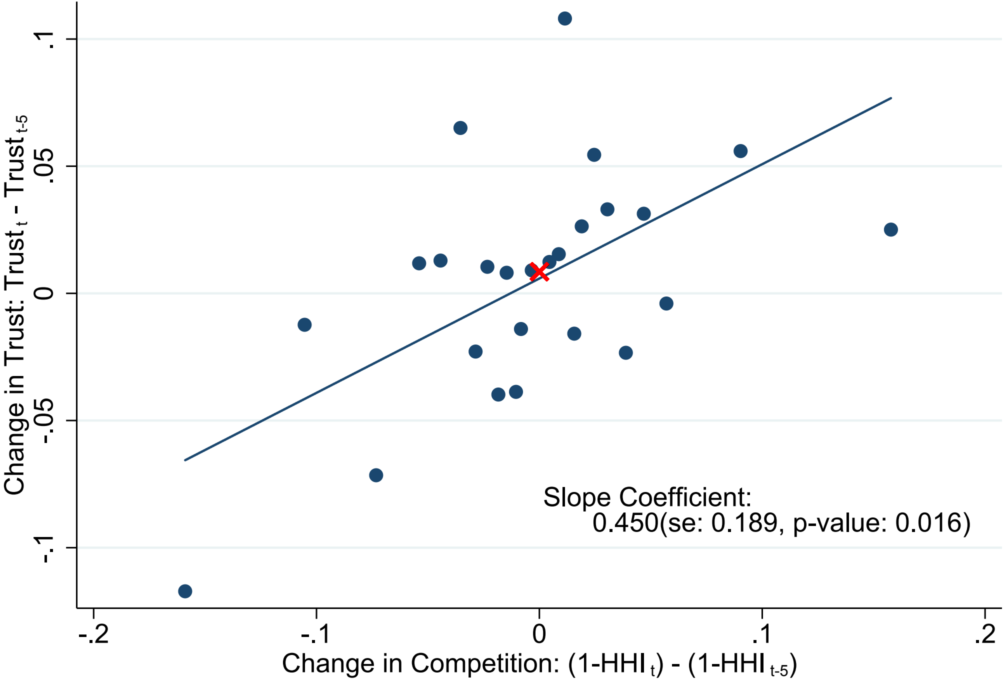
N=614. SE clustered at industry level. Includes economic and demographic controls.

FIGURE 1



30
20
10
0
-10
-10
-8
-6
-4
-2
0
2
4
6
8
10
Years Relative to Inter-state Deregulation
Percentage Who Trusts
log Firm Entry Per Capita (x100)
log Firm Closure Per Capita (x100)

FIGURE 2



N=9103. SE clustered at worker level. X marks average for workers not changing industry.

FIGURE 3

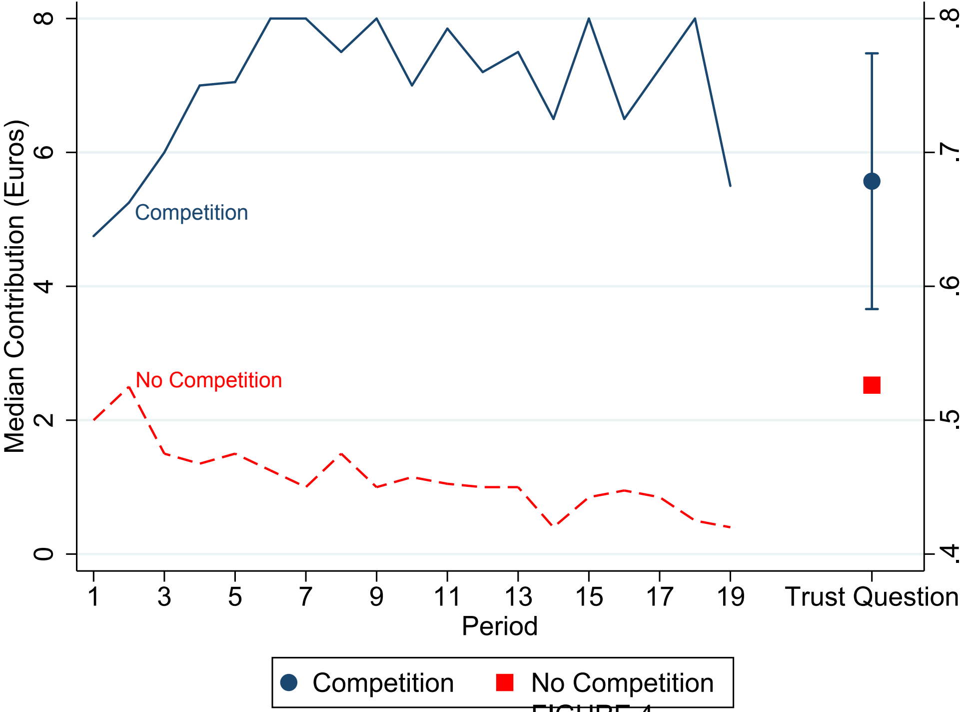
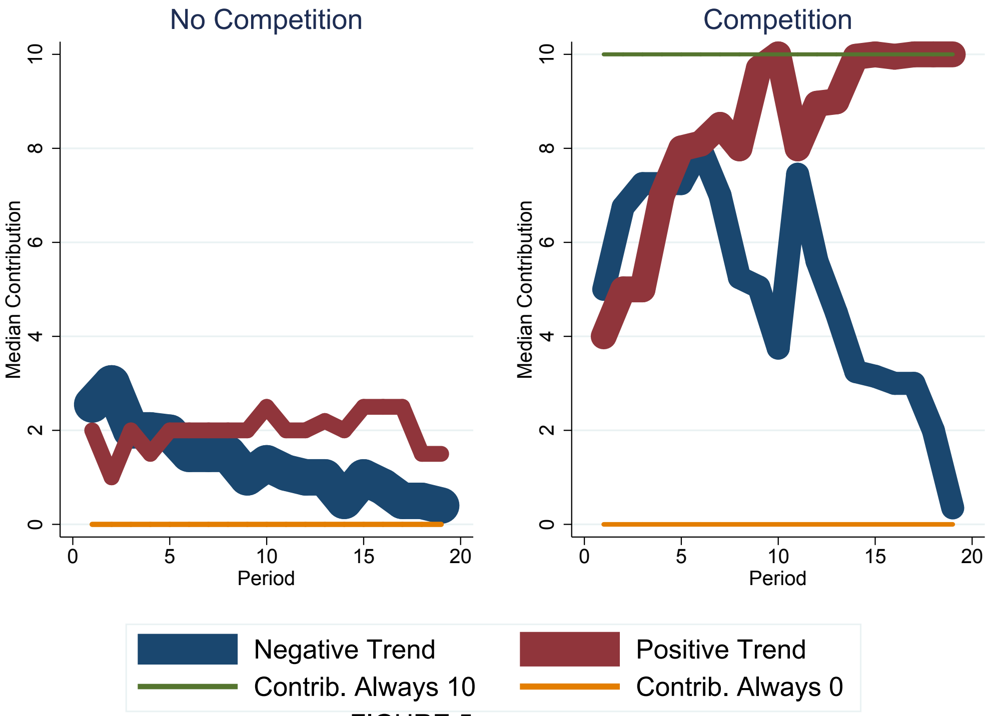

Median Contribution (Euros)
0
2
4
6
8
Share Reporting Trust
.4
.5
.6
.7
.8
Competition
No Competition
1
3
5
7
9
11
13
15
17
19
Trust Question
Period
Competition
No Competition

FIGURE 4



No Competition
Competition
Median Contribution
0
2
4
6
8
10
0
5
10
15
20
Period
Negative Trend
Positive Trend
Contrib. Always 10
Contrib. Always 0

FIGURE 5

Supplementary Materials for "The Origins of Human Pro-Sociality: A Test of Cultural Group Selection on Economic Data and in the Laboratory"

Patrick Francois
Thomas Fujiwara
Tanguy van Ypersele

1 Supplementary Text: Cross-sectional evidence in the US

We explore the relationship between sectoral competition and sectoral trust levels here. Our theoretical considerations make predictions about reported trust for individuals who are otherwise identical in their propensity to trust ex ante. But since there are many previously well documented determinants of individual trust, it is imperative to control for these when looking at this prediction. Additionally other aspects of the workplace may be playing a role. For example, the personnel policies of a firm, its degree of employee supervision, and the congeniality of relationships between management and workers, could all conceivably play roles. It is thus also important to control for as many other details of the workplace that may be varying in ways that could affect trust.

A major challenge we face in testing the model is in obtaining a measure of competition at the level of sector in which an individual is employed. The General Social Survey (GSS) includes no such competition measures, although the respondent's sector of employment is identified. For these reasons we use a particular wave of the GSS, 2004, that is advantageous for two reasons. Firstly, this wave included an extremely detailed extended workplace module. Secondly, by taking this year we can link individual sectors of employment to a measure of sectoral level of competition that we obtain from the US census of firms wave that was administered in 2002.

In the 2004 wave of the US General Social Survey there are four responses to the same generalized trust question as we saw earlier, "Can Trust", that are linked to the workplace module. These are (with unconditional response rates in parentheses): 1 "always trusted" (3.76%), 2 "usually trusted" (44.40%), 3 "usually not trusted" (42.14%) and 4 "always not trusted" (9.70%).

The literature on trust has established a set of individual characteristics to be used as explanatory variables. We use these as our basic controls in all regressions: income, which is a categorical variable with 24 brackets which we include as dummies; education, measured in years of completed schooling; age; marital status; gender; and city size.[1] Additionally, three categories of race (white, black, other) and self-reported ethnicity information by country of ancestral origin are included. From these we construct ethnicity and race dummies, the details of which are elaborated in Appendix C. This appendix also reports the sample means and standard deviations for each of these variables for our sub-sample of 616 individuals who comprise the core of our analysis. The determinants of this sample are explained below.

We match individual sector of employment with a sectoral measure of competition. Every five years, the Census Bureau surveys the population of US firms. The survey reports the percentage of total sales covered by the n largest firms ($n = 4, 8, 20, 50$) in North American Industrial Classification System (NAICS) sectors.[2] As a measure of competition this is clearly

[1] We mainly follow Glaeser et. al. (2000) here. We include controls for the size of a city in which one lives, and will include workplace size controls later. Alesina and La Ferrara (2002) analyze a richer set of regional measures than we have available, and connect these to regional income Ginis and fragmentation measures.

[2] More details on the website: www.census.gov/epcd/www/naics.html

not perfect, as factors other than the competitiveness of a sector will affect these measures. A preferred, but still imprecise, measure would be the Hirschman/Herfindahl Index measure of concentration, but the census reports these for manufacturing only.[3]

The main determinant of selection into the sample we analyze is the availability of an industry code for individuals, and the matching of this to a competition measure from the census of firms. Not all individuals – the unemployed and retired for example – will have such a code. When a code is available, furthermore, the census does not cover every sector. Since the GSS reports sector, or industry of employment, using 1980 census (3-digit) codes, it is necessary to first convert these to 1990 census code measures and then use a cross-walk converter to obtain the corresponding NAICS (4 and 5 digit) measures. Each one of these steps also leads to the loss of a small number of observations as industry classification systems change. Our final sample includes 107 industry classifications (average of 5.75 individuals per sector).

Once a NAICS measure is obtained for each observation it is matched with the census percentage sales measures. The final variable, which is our measure of competition using the sales measure for the top 50 firms, "Comp50", is computed by subtracting the concentration measures from 1. We have also computed results for all four other possible measures of competition available, but report only results for Comp50 in the text. Thus Comp50 for sector x is the percentage of total sales in x that is NOT covered by the largest 50 firms in that sector, since our measures are of competition and are coded inversely to concentration measures. Results for Comp4 (1 - share of sales of 4 larger firms), which are largely similar, are reported in Appendix E. Results using variables constructed with the shares from the 8 and 20 largest firms are even closer to those using Comp50 and are not reported, but are available upon request.

The average sector in our sample has measures of 60.53% for Comp50. A sector corresponding approximately to the average is NAICS # 42314 "Used Motor Vehicle Parts Merchant Wholesalers". An example of a particularly competitive sector is NAICS # 44112 "Used Car Dealers", Comp50 = 87%. A particularly uncompetitive sector is NAICS # 31132 "Chocolate and Confectionery Manufacturing from Cacao Beans", with Comp50 = 1.2%. Appendix D reports sectoral averages for more aggregate (two-digit) sectoral constructs. In general, most service sectors are more competitive than both manufacturing and retailing.

Estimation Procedure

In order to test the cross-sectional prediction, we run OLS regressions of the following form for an individual i working in industry j:

$$trust_{ij} = \beta_0 + \beta_1 comp50_j + \gamma Z_{ij} + e_{ij} \tag{1}$$

where Z_{ij} is a vector of independent variables that we describe below. The vector γ corresponds to their coefficients. The estimates are mathematically the same as regressing average trust in an industry against its competition measure (after individual controls - Z_{ij} - are partialled out). This particular interpretation is appealing since the number of sectors (107) is relatively large compared to the number of workers in a sector. Notice also that the main econometric implication of a small number of workers per industry is random sampling error in the measurement of average trust in a sector, which is uncorrelated with Comp50 and hence does not bias the results in any direction. The standard errors are clustered at the sectoral level, allowing for arbitrary correlation between workers of the same sector.[4]

Since almost ninety percent of the responses fall in the two middle categories "usually trust" and "usually don't trust", we focus on these. We code "usually trust" as 1 and "usually don't trust" as 0 (different codings do not affect the result). Given the infrequency of the "always" answers, the estimation of ordered logit is unlikely to be particularly useful. We have also estimated

[3]The correlation between the Hirschman/Herfindahl index of concentration and our competition measure, described below, is very high: 0.85.

[4]Clustering leads to the appropriate inference in cases of "large number of groups/small group size" as the one present here.

all of the regressions we report below as logit and probit regressions, but we report results in the paper obtained from estimating a linear probability model, since the significance of estimates does not change under this specification, and the coefficients can be directly interpreted.[5]

Results

We first show that the data conform to the usual patterns seen when trust is regressed on individual characteristics. This set of estimations is on our core sample of 612 respondents for whom we have industry and competition information and can therefore designate a competitiveness variable, and the Appendix D reports summary statistics for this core sample as well. The mean answer to the trust question, which is our dependent variable in all reported regressions, is 0.495 with standard deviation 0.500, i.e., about 50% of respondents answer "usually trust" as opposed to "usually don't" in response to the canonical trust question. This is higher than the usual positive answers to the trust question reported in most previous studies undertaken using US subjects. This is because, in order to obtain workplace competition measures, we have selected on individuals who are employed. As previous studies have found a positive correlation between trust and income, we should expect this to imply a higher than representative proportion of trusters in our core sample.

Firstly, all regressions we report include controls for income, gender, race, ethnicity, marital status, religion and occupation dummies as well as city size.[6] We find estimates on these variables that are consistent with previous studies of trust (Glaeser et. al (2000), Helliwell and Putnam (2007) and Alesina and La Ferrara (2002)), years of education is a strong determinant of trust, with an additional year of schooling being associated with a 2.54% increase in the probability that an individual reports that they "usually trust" as opposed to "usually don't trust". A one standard deviation increase in years of schooling increases trust by 7.3 percentage points. The set of income dummies indicate positive correlation between income and trust, (F-test suggests these are jointly significant). Age is entered as a second order polynomial, and is significant at the higher order.

Column (1) in Table 3 provides the most raw test of the model's predictions, i.e., that workers in competitive sectors should have higher levels of trust. The coefficient on Comp50 is 0.191 and is significant at the 1% level,[7] implying that a 10-p.p. increase in sectoral level competition leads to 1.91-p.p. increase in the probability that a respondent answers "usually trust". In variance normalized terms, this implies that a one standard deviation increase in Comp50 leads to about a 4.8 p.p. increase in trust. This result is fully robust to excluding different subsets (or all) of the individual controls.

The remaining columns in the table introduce various additional variables in order to demonstrate that the effect we are picking up is being driven by competition per se, and not some other correlates of trust that happen to be correlated with competition. On this front, we explore all the possibilities that we were able to identify and that the data allow, namely that competitive sectors have workplaces which: have less job security, are smaller, have more supervision, select different types of individuals, or somehow cultivate more congenial workplaces.

[5]Moreover, a Brant test of the ordered logit specifications rejected its parallel regressors assumption.We also experimented with two further types of estimation. In one, we estimate a multinomial logit version of the model which utilizes all of the four response categories, but allows different βs to be estimated for the transition to each response relative to an omitted category. The results that we obtained on the "usually trust" versus "usually don't" under this estimation are very similar to those reported here. In a second variant, we pooled all responses into a binary category. That is, the responses "always trust" and "usually trust" are coded as 1 for "yes" to the trust question and "always don't trust" and "usually don't trust" are coded as 0 for "no". This estimation yields slightly lower size on the competition variables than reported here, and consequently lower significance, but leaves things otherwise unchanged.

[6]We have also run all of these regressions without occupation dummies, which has no substantive effects on results.

[7]As indicated in Appendix E, results for Comp4 are largely in line with those reported here for Comp50, except of marginally lower significance. Our conjecture as to why this is the case relates to the coefficient of variation in Comp50 being significantly larger than that of Comp4. The proportion of sales covered by the largest 50 firms seems to be picking up much more of the cross industry variation in competition than that of the top 4.

The general picture that emerges is that the effect of competition on trust is virtually unchanged by the inclusion of this large set of controls. While each individual inclusion sheds light on the (non-)importance of a specific omitted variable, the collective result increases our confidence that the results are likely to be robust to the inclusion of other variables that are not available in our data.[8]

Job Security: Karlan (2005) and Schechter (2007) show that trusting behavior in experimental settings is correlated with low risk aversion. If competitive sectors had low levels of job security, then it may be that these select risk lovers, who are also those likely to trust. This could explain the competition-trust link, but has nothing to do with the model we developed. Since we don't have information on risk aversion directly we thus include a measure of job security. Respondents were asked to respond to the statement "job security is good". We code a dummy variable equal to 1 if individuals respond that this is "very" or "somewhat" true, and equal to zero if "not too" or "not at all" true. As column (2) reports, the coefficient on Comp50 is entirely unaltered by the addition of this variable. As with all other workplace controls, the coefficient (and standard error) of the variable is reported on the more detailed tables of Appendix D.

Workplace Size: It is possible that competition is affecting trust by altering the size of workplaces in which individuals work. For instance, it may be the case that more competitive sectors, by admitting more firms, *ceteris parabus*, also tend to have smaller workplaces. By repeatedly interacting with a smaller group of individuals, it may be the case that individuals are developing reputation-based trust with these individuals, which then translates into higher levels of trust overall. The GSS does attempt to measure the size of the workplace by asking: "About how many people work at the location where you work?" Respondents were allowed to choose from 7 categories. Column (3) adds dummy variables constructed from these categories to the baseline set of regressors, which does not attenuate the effect of competition on trust as the coefficient on Comp50 increases in size, and remains significant at the 1% level.

Congeniality of the Workplace: One may conjecture that the forces of competition induce firms to provide more congenial workplaces – which are costly – in order to retain the best employees. This is an argument made by Cohen and Prusak (2001). They argue that competitive environments that threaten employers with worker turnover induce employers to provide the sorts of workplaces that mitigate stress, allow workers to attain a sense of achievement, and respect family and other obligations. These more congenial workplace may contribute to a sense of overall well-being, and perhaps higher levels of trust. In order to see whether this is what the basic correlation is picking up, we exploit a rich set of workplace related questions. These are briefly described in Appendices C and D, with details (means, standard deviations, response categories) reported there as well. Many of these variables are directly concerned with the respondent's perceptions of relations between co-workers in the workplace; for example, whether there are heated arguments, people shout, people are put down, others take credit, others are helpful when needed, people act upset, or they turn away when others are threatened. Others ask directly whether the workplace is stressful and how often the respondent skipped work due to unhappiness with the work situation. Column (4) reports the relationship between Comp50 and trust after the addition of these extra workplace variables to the basic set of controls in column (1). The picture that emerged previously is largely unchanged. The coefficient on Comp50 remains at around 0.2 and highly significant, suggesting that these workplace variables do not seem to be related to the avenues through which competition affects trust.

The regressors also include whether the workplace is unionized, which is never significant, and we have also included the job security and workplace size controls. We have experimented with many different combinations from this full set of additional workplace variables and the picture obtained remains unchanged. The significance and magnitude of the competition measure is

[8] This is discussed formally by Altonji et al. (2005), and is applied to the case of trust questions by Nunn and Watchenkon (2009). If the similar procedure was applied to our results, it would imply that selection on unobservables would need to be at least 40 times greater than on observables to explain away our results.

largely unaltered by the particular combination we try.

Supervision: Another possible hypothesis for the coefficient in column (1) is that sectoral competition, by increasing the costs to firms from poorly performing employees, induces firms to employ proportionately greater supervisory resources. Acting in a more restricted environment could make workers seem more trustworthy and lead to higher reported trust levels. This is related to but distinct from the theory that we developed in the previous section. The model suggests competition across groups of workers is the key disciplining effect on free-riding, not restrictions on their discretion, even if the latter is arising due to competition. In order to examine the possibility that supervision is the source of the effect, we include responses to the question: "Does the Respondent have a supervisor on your job to whom you are directly responsible." This variable is included in column (5). Again, the results remain unchanged.

Optimism: As mentioned earlier, individuals who are observed to play high levels of trust in the trust game, are also individuals who are less averse to risk. While we have ruled out the selection of these more risk loving types through the job security question, it is possible that competitive sectors are selecting individuals with other characteristics that are related to their willingness to bear risk. One such characteristic is optimism. Individuals who are more optimistic that outcomes will turn out well, may be more willing to trust. Once again, we want to ensure that such an effect is not generating a spurious relation from competition to trust. The results from doing this are reported in column 7. There we include the variables "Optimism" (strength of agreement with "I'm always optimistic about my future") and "More Good" (strength of agreement with "Overall, I expect more good things to happen to me than bad"). Both of these are constructed as dummies with responses to the statements that are "strongly agree" and "agree" coded a 1, and "disagree" and "strongly disagree" coded a zero. It should be noted that the sample size drops to 532 when we do this, as these questions were given to only a sub-sample of all survey respondents. Neither of these variables enters significantly either on their own or together, and their impact on the coefficient on Comp50 is negligible.

As a final test, column (7) reports the regression results obtained when we include all of the variables reported above simultaneously. Once again, the coefficient on Comp50 remains at around 0.2 and strongly statistically significant.

Selection: Even though we have controlled for a number of observables, a correlation between competition and trust may be observed in such a cross-section even without sectoral competition causing increased trust if it is the case that individuals who are inherently more trusting are somehow selected into competitive sectors. The theory we developed in the previous section starts from ex ante symmetry across individuals. Consequently, once we have controlled for the individual specific factors that have predicted trust in previous studies, that theory would suggest that there should be no evidence of individuals who are inherently trusting selecting into competitive sectors. Evidence against this theory would then be provided if we found that individuals with no, or little, experience had higher (or as high) levels of trust, as individuals with longer experience in the competitive sectors. The GSS does not follow individuals through time, so we use the "potential experience" measure commonly used in labor economics. This is created by subtracting years of education from the respondent's age minus 6. We then interact our competition measure with this constructed experience variable. Experience interacted results are reported on Table 4. Column (1) reports results obtained when we add this interaction term and omit experience directly in the regressions; as it is colinear with age and education.[9]

The results here are striking. Adding the interaction term makes both competition variables on their own insignificantly different from zero. Moreover, the interaction term itself is positive and significant for both competition variables across all specifications, at the 10% level. Table 4

[9]In order to be able to include experience directly, we have also run a specification where we include age dummies, instead of age as a continuous variable, and include our constructed measure of experience as a control as well. The results in these two specifications are not significantly different so we discuss the specification with continuous age in the paper.

replicates the regressions reported on Table 3 in the same order but now including the experience interaction. In general, $p-values$ are well below 10% on Comp50 in all specifications. For example, the final column (7) which includes all of the potential regressors, and the experience interaction, has a $p-value$ of 0.57.

The zero finding on direct inclusion of competition is evidence against selection. Individuals without experience are no more likely to respond positively to the trust question if they work in competitive sectors. However, as individuals increase their experience in the labor market, working in a competitive sector has a positive impact on their reported trust. Moreover, this impact is increasing the longer their experience. One explanation for this finding could be that interacting competition with experience is significant because this measure has less noise than the competition measure on its own. Though possible, this seems unlikely as the experience we measure is, if anything, introducing more noise because its ability to proxy for time spent in a sector is weaker the longer the individual has been in the labor market. The results here suggest it is unlikely to be the case that competitive sectors are selecting individuals with high levels of trust.

2 Supplementary Text - Banking Deregulation in US States

2.1 Background and Data

This subsection provides a brief overview on bank deregulation in the US, the reader is directed to Strahan (2003) and references therein for a more detailed discussion. Since the McFadden Act of 1927 ruled that national banks had to follow state-level bank branching restrictions, state governments have imposed significant restrictions on branching within their borders. Commercial banks were only allowed to open branches within a small geographic area within the state. Hence, some banks could only operate in one county, or within 100 miles from its head office, or even were only allowed to have a single branch (a regulation known as *unit branching*). In several cases, these bank branching restrictions were such that a bank would be a monopolist in these narrow confines.

Starting in the 1970s, several technological innovations such as automatic teller machines (ATMs), phone and mail banking, and more sophisticated credit-scoring systems radically reduced the cost of using banking services from a distant branch. These changes lead states to lift interstate and intrastate restrictions from the mid-1970s, allowing banks from out-of-state to operate in their borders (which we refer to as *inter-state deregulation* henceforth) and permitting banks to operate multiple branches, either by opening new ones or by merging with other banks, in any part of the state (which we refer to as *intra-state deregulation* henceforth). In 1994, federal legislation (the Riegle-Neal Act) eliminated inter-state restrictions nationwide.

Of particular interest for our research design is the fact that different states undertook inter- and intra-state deregulation at different times (Table SXX presents the dates). Krozsner and Strahan (1999) and Black and Strahan (2002) argue that the differences in the timing of these reforms across states were mainly driven by the state-level structure of banking, and by federal actions, but not associated with changes in the states' overall economic situation. More importantly, Black and Strahan (2002), Kerr and Nanda (2009), Levine et al. (2009), and our own analysis, show that deregulation can be seen as positive **exogenous** shocks to the competitiveness of a state's **non-financial** sector.

Specifically, the timing of the reforms is not correlated with previous trends in the creation of new firms, but generated a large increase in the number of (non-financial) firms being started after they were implemented. This is explained by the fact that the deregulation of branching restrictions resulted in a more competitive (by breaking local monopolies) and efficient (by allowing mergers to occur) banking sector and more availability of credit, which in turn facilitated the creation of new firms and raised the contestability of local markets.

The main data source for the analysis based on banking deregulation are several waves of the US General Social Survey (GSS), which was first implemented in 1972, and at least every other year since then. The survey is asked of one adult per household and the sampling reflects regional population densities. The dependent variable of interest is the response to the following question: "Generally speaking, would you say that people can be trusted or that you can't be too careful in dealing with people?" In the period analyzed in this section, the three possible answers were "Can Trust", "Cannot Trust" and "Depends". We code this into a binary variable taking value 1 if the respondent reported "Can Trust" and zero otherwise. Given that a very small fraction (3.9%) of the sample reported "Depends", different treatments of this answer (coding it as one, zero, or excluding it from the sample) do not affect the results in any relevant way. The GSS also includes several economic and demographic variables on the respondents, such as age, education, marital status, and race which we use as controls. We also match the GSS to state-year level data on income, income distribution, population, and a proxy for competition (firm entry).

Not all states are surveyed at every year of the GSS, and we use an unbalanced panel of 41 states for the period 1973-1994 (Table SXX presents the list of states included in the sample). The start date (1973) of the sample is defined by the availability of information on state of interview, and the final point (1994) is chosen both by the availability of state level variables (such as our measure of competition) and by the fact that in this year federal legislation (the Riegle-Neal Act) eliminated inter-state banking restrictions nationwide.[10] After excluding missing values for the covariates used as controls, the sample includes 17,455 individual survey answers from the GSS. The same sample is used in all the regressions reported in this paper. Table SXX presents the summary statistics of our sample.

As a proxy for state-level competition, we use the number of new incorporations measured by the Dun and Bradstreet Corporation from official state records. This is the same variable used by Black and Strahan (2002), which discuss it in more detail and provide compelling evidence on why this is good measure of firm entry. Levine et al. (2009) also use this variable. The dates of both bank branching deregulation are taken from Kroszner and Strahan (1999) and were cross-checked with other studies of these reforms' effects.

2.2 Methods and Results

Figure 2 presents the results in a graphical analysis, tracing out the year-by-year relationship between the timing of the reforms and our measure of trust levels. We do this by estimating the following equation:

$$trust_{ist} = \alpha + \sum_{j=-10}^{10} \beta_j^R D_{st,j} + \gamma_s + \delta_t + \theta_s t + \pi X_{ist} + \varepsilon_{ist} \tag{2}$$

where $trust_{ist}$ is a dummy variable indicating if person i living in state s at year t responded positively to the trust question. The $D_{st,j}$ variables indexes a set of variables that indicate the number of years until inter-state deregulation is enacted. The numbers are relative to the effect on the date of the reforms' enactment ("year zero"), which are normalized to zero. For example, $D_{st,-5}$ takes value one if state s at year t is going to enact deregulation in exactly five years or is zero otherwise, while $D_{st,3}$ is an indicator that takes value one if and only if deregulation happened exactly three years ago.

Hence, the model estimates the effect of being 10, 9, ..., 3, 2, 1 years *before* reform, as well as 1, 2, ..., 10 years *after* it in the most flexible manner allowed by the data. We control for individual covariates (X_{ist}),[11] state and year fixed effects (γ_s and δ_t) and state-specific linear

[10] All years in which the GSS asked the trust question in the 1973-1994 period are included in the sample: 1973, 1975, 1976, 1978, 1980, 1983, 1984, 1986-1991 (every year in the interval), 1993, and 1994.

[11] The covariates are XXXX, and they are discussed in further detail on Section xx below.

trends ($\theta_s t$), ruling out state differences that are fixed or vary linearly through time, as well as common nationwide factors that may evolve nonlinearly, such as the business cycle.[12]

Figure 2 plots the estimates of β_j, hence tracing out the relationship between timing of reforms and trust levels (conditional on the controls). Additionally, Figure 2 plots similarly estimated β for state-level counts of firm entry and firm closure that were calculated by Kerr and Nanda (2009).

The figure shows a striking pattern. The first remarkable feature is that the relationship between the timing of reform with trust and firm entry is flat in the periods **before** a reform occurs. This is direct evidence that the timing of the reforms is not correlated with previous trends in trust or competition and reinforces the notion that they can be considered exogenous events in the analysis of trust.

Moreover, if states with higher levels, or stronger growth, of trust systematically enacted deregulation before[13] (or after) other states, the relationship between trust and timing of the reform would not be flat before the reforms take place. Hence, Figure 2 also indicate that a causal link of higher trust to earlier (or later) financial innovation is not driving the results.

After each of the reforms take place, trust starts trending up almost linearly. The relationship seems to stabilize after 8 years, but the estimated effects are noisier at this point, making it difficult to draw clear conclusions on this. This coincidence in the timing of reforms and a trend break in the evolution of competition and trust suggest that deregulation had a causal impact on competition and trust. This nonparametric exercise in which no particular shape is imposed on the relationship between the timing of deregulation and trust is also valuable in guiding the parametric estimations reported in the next subsection.

We also explore the temporal and spatial variation in the timing of banking deregulation by estimating the following equation:

$$trust_{ist} = \alpha + \mu Post_{st} + \lambda Years_{st} + \gamma_s + \delta_t + \theta_s t + \pi X_{ist} + \varepsilon_{ist} \tag{3}$$

where again $trust_{ist}$ is a dummy variable indicating if person i in state s at year t trusts. $Post_{st}$ is a dummy variable taking value one if at year t state s has already enacted interstate banking reform and zero otherwise. The variables $Years_{st}$ measures, at year t, the number of years since state s has enacted the reform. For example, if a state enacted its reform in 1982, this variable equals one when $t = 1983$, two when $t = 1984$ and so on (while it value is zero for all years before, and including, 1982). We report estimates of the effect of intra- and inter-state reform both separately and jointly.

The estimation also controls for a vector of individual level controls (X_{ist}) that are known to be correlated with trust[14] as well as state and year fixed effects (δ_s and δ_t) and state-specific linear trends ($\theta_s t$). Hence, the specification rules out state differences that are fixed or vary linearly through time as well as nationwide factors that may evolve nonlinearly, such as the business cycle.

This econometric framework requires only the **timing** of the reforms to be exogenous in

[12] We set A=12 and B=12 in the case of inter-state deregulation, and A=14 and B=17 in the case of intra-state deregulation. The choice is driven by the existence of enough states allowing to identify the relevant β (so that $D^R_{st,j}$ is not collinear with fixed and time effects). The larger variation in intra-state reform date hence explains the larger A and B in this case. We drop data points from more than 14 years before the reform, as well as from states that already enacted intra-state deregulation before the beginning of the sample period.

[13] Guiso et al (2004) find evidence that Italian individuals from regions with higher social capital make more use of the financial system, indicating that trust can play a role in financial development.

[14] These variables are a quadratic polynomial of age, indicators for completed high school and college education, population size of city and state of respondent, household size, and a full set of dummies for race (black, white, other), gender, marital status (married, widowed, divorced, separated, never married) and religion (protestant, catholic, jewish, other/none) and workforce status (8 categories). These variables are included only to increase the precision of the estimates and do not affect their magnitude in a significant way. We based the choice of covariates on what previous studies - Glaeser et al. (2000), Alesina and La Ferrara (2002) and Helliwell and Putnam (2007) - found to be correlated with trust.

order to estimate its causal effects, since the model captures trend breaks that coincide exactly with the timing of their enactment. Notice that we include the number of years after the reform was carried out in addition to the dummy indicating the post-reform periods. This choice was guided by the fact that Figures 1 and 2 suggest that trust grows over time after the reforms take place, instead of discretely "jumping" immediately after the reform. Note, however, that our specification nests the more standard difference-in-differences case (where only post-reform dummies are included).

Given that the variable deregulation varies at the state-year level, our estimates are virtually the same as in a regression where the level of observation is a state-year and the dependent variable is average trust (with the individual controls in X_{ist} properly partialled out). We cluster standard errors at the state level, allowing for arbitrary correlation across individuals within the same state (even at different years). Previous research has shown that, for similar data structure (length of panel and number of clusters) as the one used here, clustering standard errors appropriately addresses the issues that arise when one regresses individual variables (trust) on group (state) level variables (competition, deregulation) and the serial correlation likely present in variables.[15] In other words, we obtain the same results when aggregating observations to state-year averages, and the standard errors are not artificially reduced by the use of individuals as the level of observation.

Table 2 presents the results of the estimation of equation (2). Columns (1) and (2) show the results for intra- and inter-state reform, respectively. In consonance with the graphical analysis presented in the previous section, the estimates indicate that both types of banking deregulation lead to a continuous increase in trust level after its implementation. The estimates imply that a state that lift ed branching restrictions would experience over 1.0 percentage point increase in the share of its population reporting that they "Can Trust" every year after the reform, an effect that is statistically significant at the 5% level in both cases. The coefficients on post-reform dummies are small and statistically insignificant, as expected since Figures 1 and 2 do not show a discrete "jump" after reforms. Column (3) estimates the effect of both reforms jointly. The coefficients become slightly smaller. While the coefficient on years since inter-state deregulation loses significance, the effect is still of a sizable economic relevance and the effects of both reforms are jointly significant. As a robustness check, columns (4)-(6) show that the results remain virtually the same if the post-reform dummies are dropped.

Overall, the coefficients indicate that a (hypothetical) state that enacts both reforms at the same time will experience an increase in trust of about 2 p.p. per year. The estimate implies that 10 years after both reforms take place trust would have increased by 19 p.p.. While this effect may seem large at first pass, it must be noted that they take a full decade to take place (and Figures 1 and 2 suggest that the growth in trust stops around after such time) and that they are similar magnitude to the difference in trust between college and high school graduates, and smaller than the differences in trust across some states (e.g, Minnesota and New York).

Robustness Checks

This section provides some robustness checks on the main results presented on Table 2, exploring the robustness of the results to the inclusion of several state-level controls for income, inequality, and economic activity. The robustness of the results to the inclusion of these controls also play a role in our discussion of the mechanisms driving the results on Section xxx. We also test if banking deregulation is associated with demographic changes (due to migration or some other factor) that could lead to higher average trust in the population.

Firstly, Table 3 reports the effects of estimating the specification from columns (1)-(2) of Table 2 adding controls for income. The GSS contains a respondent-level income variable which (although it is a continuous measure) is constructed by interpolations categorical answers (that

[15]The simulations in Cameron et al. (2008) Bertrand et al. (2004) and Hansen (2007) show that clustering standard errors e state level lead to almost negligible size distortions in panels with similar cross-sectional and time-series dimensions as the one used here.

change across waves). The data is missing for most individuals that are not currently in the labor force (including the retired), which we code as having zero income. Although this suggests interpreting this variable as "labor income", the question asks respondents about total income. although the question. Given the imperfections on the GSS income measure, we also use the state-level measures of income obtained from other sources: personal income (per capita) from the Bureau of Economic Analysis and the median income in the state, computed from the March Current Population Survey.[16]

Table 3 presents the replicates the estimation from columns (1)-(2) of Table 2, adding the different controls. We examine the role of controlling for each of the three income on types of reforms separately. The main result that both types of deregulations lead to increase in trust remain largely unchanged by the addition of these income variables. If anything, the estimates are slightly larger (and significant at the 5% level).

Columns (1)-(2) of Table 4 explores the robustness of the results by controlling by a measure of unemployment (the employment-to-population ratio, from the Bureau of Labor Statistics). This controls for the role of the state-level business cycle, which may be an issue if states are more (or less likely) likely to enact reforms in response to local economic shocks. Column (3)-(4) control for the role of income inequality, measured by the Gini index computed from the Current Population Survey. In all columns (1)-(4), the inclusion of the controls do not change the main result that both types of de-regulation have a positive (and statistically significant) effect on trust. Moreover, the point estimates are larger than in the main table. Columns (5) and (6) provide the results when unemployment, income and inequality are all controlled for. The main results and conclusions survive, and if anything the estimated effects are larger.

As a further check, we test if the results on Table 2 are driven by banking de-regulation increasing the trust of a state's population, or with generating migration of more trusting people into the state. While the GSS does not track individuals over time (it is a repeated cross-section of individual, instead a panel), we can test if banking de-regulation is associated with changes in observable socio-economic-demographics of a state. For example, one could substitute the dependent variable on equation (2) for an indicator of college degree status, and test if banking de-regulation lead to more college educated people in a state. Columns (1)-(3) of Table 5 performs this exercise. It finds no statistically significant effect of de-regulation on the share of people with college degrees in a state. Moreover, the point estimates are small and have different signs given the type of reform.

In principle, this could be done for all relevant covariates available in the data. To economize on space and perform a test of this nature, we perform the following two-step test. First we regress the trust indicator on all the covariates (X_{its}), but nothing else.[17] We use the estimated coefficients to compute predicted values of trust for each individual in the sample given her covariates. We then use this predicted trust as the dependent variable on equation (2).[18] This provides a powerful test of the hypothesis that de-regulation increases trust by leading to changes in the (observable) covariates that are associated with trust. Columns (4)-(6) of Table 5 present the results. The estimated effects are substantially smaller than the ones for actual trust (and statistically insignificant). The results indicate that banking de-regulation is not associated with demographic changes that could lead to increased trust. While it must be noted that there may be unobservable covariates of trust, which we cannot rule out are changing, we do observe a large host of covariates, we should also be associated with unobservables. Overall, the results are consistent with banking de-regulation changing trust in the population, but hard to re-concile

[16]We use the log of these variables as controls. The GSS income variable contains zeros, and hence we use its level. The median income data is computed from several waves of the March Current Population Survey.The data was computed by J. Guetzkow, B. Western, and J. Rosenfeld for the Russell Sage Program on the Social Dimensions of Inequality, and is available at www.inequalitydata.org. The average annual income is adjusted to 2002 dollar using the CPI.

[17]The set of covariates is described on section xx.

[18]Without including the covariates (used to construct predicted trust) as controls.

with it being driven by migration or other changes in socio-economic or demographic variables.

Mechanisms

The evidence so far, in particular Figures 1 and 2 and Table 2, make the case that banking deregulation affects trust. They provide compelling evidence that policy can contemporaneously affect trust, but leave open which mechanism do so.

There is a substantial literature on the banking de-regulation episodes we study here, which found (at least one type of reform) to be associated with increased competition (Black and Strahan, 2002, Kerr and Nanda (2009), Levine et al, (2009)),[19] higher incomes (Jayaratne and Strahan, 1996), lower income inequality (Beck et al., 2010), and a reduction of the black-white wage gap (Levine et al., 2011).

There is good reason to believe that higher income and lower inequality may affect trust. In a cross-section of individuals (or countries) income and trust are positively correlated (although it is not clear that income **growth** is associated with increases in trust). There is also evidence suggesting that inequality adversely affects social capital (CITATIONS). While there is less reason to think the black-white wage gap can affect trust, we also study this possibility.[20]

Table 6 estimates the effect of banking deregulation on the log of new incorporations (discussed on section xx), personal income per capita (although similar results would be obtained with median income from the CPS) and the Gini index of income. We estimate a version of equation (2) for these variables, substituting them in the place of trust as the dependent variable. Since these variables vary at the state level, we do not control for individual covariates. The variables are run at the individual level, to make them as comparable as possible to the specification that estimated the effects on trust. Given that none of the variables vary at the individual and standard errors are clustered at the state level, these regressions are essentially the same as running them using state-years as observations, and weighting them by the number of GSS respondents in each state-year.

Table 6 revisits results previously found in the literature, and its main purpose is to show these results are also present in our specific sample and using our specifications.[21] Columns (1)-(3) report the effect on the log of new incorporations. In consonance with the findings of the previous literature, we find that both reforms increase firm entry. As with trust, the results are robust to estimating the effects of the two types of reforms separately or jointly.

The pattern of results for firm entry matches the one for trust, with the number of yearly new incorporations increasing linearly after the reform takes place. This is consistent with competition being a mechanism through which deregulation affects trust.

A similar pattern of effect is ound for state-level income, with both inter- and intra-state reforms being associated with income growth. We will return to these effects, and also the results for inequality on columns (7)-(9) in section xx below. In the following sections we discuss separately if income inequality, income growth, and labor market discrimination can explain the results.

Can Lower Income Inequality Explain the Effects on Trust?

Columns (7)-(9) of Table 6 indicate that intra-state banking deregulation leads to lower income inequality. The estimated effect is of about 1.3 points lower for each year after the reform in 100-point Gini scale, although the effect is somewhat imprecisely estimated (and significant at the 10% level). The effect of inter-state reforms is much closer to zero (and statistically indistinct from it) with a positive point estimate. The same pattern occurs when the effects are estimated jointly. We interpret this results as only intra-state deregulation having an effect on income inequality.

[19] See also Freeman (2002), Wall (2004) and Cetorelli and Strahan (2006).

[20] Note that only 13% of our sample reports being black in response to a question on their race.

[21] Recall the GSS does is not available for every ear, and does not cover all states each year, making our sample different from other studies of the effects of banking deregulations. Moreover, by studying responses at the individual level in the GSS, we are essentially weighting states with more GSS respondents more heavily.

These results confirm the ones Beck et al. (2010), which also find that intra-state banking deregulation lowers state-level income inequality.[22] Although the paper only reports results for intra-state deregulation, they report not finding the same effects for the inter-state case.[23] Hence, lower income inequality can only, at best, explain why intra-state deregulation affects trust, but not why inter-state deregulation also increases trust. Making it clear another mechanism is playing a role.

Moreover, section xx also discussed that the effect of (both types of) deregulation is robust to the inclusion of the Gini index as a control, furthering the notion that income inequality is not the main driver behind the results.

Can Higher Income Explain the Effects on Trust?

The results on columns (4)-(6) indicate that both intra- and inter-state reforms positively affect income, a result first reported in Jayaratne and Strahan (1996),[24] with a pattern that resembles the results for trust. In this section we explore if higher income may be the causal nexus between deregulation and trust. Three pieces of evidence point that is is unlikely to be the case. Firstly, as shown in Table 3, the effects of de-regulation are robust to the inclusion of different income measures as controls. Secondly, we provide evidence that income **growth** does not lead with higher trust. While previous papers explored the cross-sectional relationship between level of income (at individual, regional, or national) level, we estimate the effect of income growth on trust (using variation in income growth induced by oil prices in oil-rich states) and find very small effects. Thirdly, the (semi-)elasticity of trust with respect to income would have to be very large to explain the results.

A number of studies have documented that, at the individual level, income is positively correlated with trust (Glaeser et. al (2000), Helliwell and Putnam (2007) and Alesina and La Ferrara (2002)) or that the **level of trust** is associated with higher income or faster income growth across countries (Knack and Keefer, 1997). The relationship between income growth and changes in trust is not as well documented, and we are unaware of that attempts to estimate the impact of the former on the latter (IS THIS RIGHT?).

In order to estimate the effect of changes on income on trust, we exploit arguably time-series variation in global oil prices interacted with cross-sectional state oil reserves. Acemoglu et al (2009) utilize a similar approach to estimate the elasticity of health spending with income[25]

In particular, we estimate the following regression:

$$Y_{ist} = \alpha + \beta(\text{Oil Price}_t \times \text{ Oil Reserves}_s) + \gamma_s + \delta_t + \theta_s t + \pi X_{ist} + \varepsilon_{ist} \qquad (4)$$

where Y_{ist} is an outcome of interest, and the other variables are defined as before. The Oil Price variable is the average annual spot oil price from the West Texas Intermediate Series, measured in dollars per barrel, while Oil Reserves are "proved crude oil reserves" from the API Basic Petroleum Handbook. The oil reserve data is for 1972, the year before our sample period starts. The unit of measurement is tens of thousand of barrels per capita.[26]

Since oil prices are unlikely to be affected by local (state) economic conditions and our oil reserves measures is pre-determined during our sample, the interaction between two variables creates exogenous variation in income. Such finding is reported in columns (1) and (2) of Table 7, which replace Y with two state-level measures of income: median income from the CPS and

[22] Using the Gini index and other measures of inequality.

[23] Specifically, they state that "*when we simultaneously control for inter- and intrastate branch deregulation, we find that only intrastate deregulation enters significantly. Thus, we focus on intrastate rather than interstate deregulation throughout the remainder of this paper.*" (Beck et al, 2010).

[24] Jayaratne and Strahan (1996) only study intra-state reforms. Our results are remarkably similar to theirs: with intra-state deregulation adding about 1 p.p. of yearly growth in income in a state.

[25] Acemoglu et al. (2009) also discusses a larger literature examining the local effects of oil prices.

[26] The oil price data is the "OILPRICE" variable in the Federal Reserve Economic Data website. This is the same variable used in Acemoglu et al. (2009). It varies substantially in the sample period, which cover remarkable oil shocks: from $4 in 1973 to $37 in 1980, with a reduction to $16 in 1986.

personal income per capita from the BEA. The estimated coefficients for the two variables are remarkably similar, nd indicate the effect of a $10,000 increase in the value of the state per capita reserves, which would increase incomes by around 4%, a result that is significant at the 1% level.[27] Column (3) indicate that changes in oil prices do not affect within state income inequality, as the estimated effect is small and statistically insignificant.

Columns (4) and (5) present the effects on trust. The estimated coefficients are negative, of a small magnitude, and statistically insignificant. There is little difference between the specification including individual controls (4) and the one without (5). This indicates that variation in income induced by changes in oil prices do not affect trust. The estimates on Table 7 indicate that an oil price-oil reserve-instrumented effect of income on trust would be that a 1% growth in income **lowers** the percentage that trusts by roughly 0.35 p.p., and this result would be statistically insignificant.[28]

It is important to note that oil price shocks are documented to be **permanent,** and not transitory, and hence the lack of effects on trust cannot be attributed to trust not being responsive to transitory income shocks. I In conclusion, it seems that, at least in the case of US states, growth in income does not lead to higher trust.

Finally, it must be noted that comparison of columns (4)-(6) of Table 6 with columns (1)-(3) of Table 2 imply that, in order for higher income to be the sole mechanism driving the results, the semi-elasticity of trust with respect to income would need to be in the 0.75-1.00 range (i.e., a 10% increase in income leads to at least a 7.5p.p. increase in the number of people who trust). Comparison with columns (1)-(3) of Table 6 indicate that, in order for firm entry to fully explain the results, the semi-elasticity would need to be around 0.25 (DEVELOP THIS ARGUMENT FURTHER).

Can Reduced Racial Discrimination Explain the Effects on Trust?

Levine et al. (2011) find that both intra- and inter-state deregulation lower the wage gap between black and white workers. An interesting result is their paper that this reduction is largely driven by results that present "above median discrimination" in a survey-based index of discrimination. Using the list of states with below/above median discrimination provided in Levine et al. (2011), we can test the effects of deregulation also differ by state-level discrimination, as would be predicted if the results were driven by racial discrimination. Table 8 reports the results interacting the deregulation variables with an "above-median" discrimination and dummy and by splitting the sample across this same dimension. The results indicate that the effects of deregulation on trust are the same in both sets of states.

3 Supplementary Text - German Socio-Economic Panel

Data. The German Socioeonomic Panel (SOEP) tracks a nationally representative sample of the German population. While its respondents are surveyed every year, the generalized trust question was only asked in the 2003, 2008, and 2013 questionnaires. Specifically, respondents were asked whether "*on the whole, one can trust people.*" Four answers were possible: "*totally agree*," "*slightly agree*," "*disagree slightly*," and "*totally disagree.*" As in the other empirical exercises in this paper, we use as our outcome a binary indicator of trust, which is equal to one if the respondent answered "*totally agree*" or "*slightly agree.*"

[27] We do not take logs of the variable since there are states with zero oil reserves. As an example, "a $10,000 increase in the value of the state per capita reserves" can be cause by a $1 increase in the price of oil in a state that has a reserve 10,000 barrels per person, or an increase or an increase of $10 in the price of oil, in a state that has 1,000 barrels per person.

[28] As a robustness check, column (6) of Table 7 indicate that oil prices do not affect "predicted trust" and do not lead to in or out-migration of people with characteristics associated with trust.

The SOEP also provides the industry of work of its respondent, based on the NACE codes which are the "statistical classification of economic activities in the European Community." We calculate a national-level Hirschman-Herfindahl Index (HHI) of the firms' operating revenue for each NACE code based on the ORBIS database for the years 2008 and 2013.[29] We exclude those working in the public sector NACE code from the sample.

As discussed in the main text, we use the average of both years as a time-invariant measure of industry competitiveness in our regressions. The main text also discusses the relevant characteristics of the sample.

Additional Results. Figure S1 provides a falsification (or placebo) test in the spirit of testing for "pre-trends:" we test whether workers that move to more competitive industries are experience differential trends in their reported trust *before* the move occurs. Figure S1 is constructed exactly like Figure 3 in the main text. The only difference is that the change in trust is *lagged*. Given our three-period sample, the plot shows the relationship between changes in lagged trust between 2003 and 2008 and changes in competition (HHI) between 2008 and 2013. The lack of a slope in this figure indicate that those moving to more concentrated industries were not more likely to have experience increases in trust *before* they changed industry. This provides evidence against a reverse causal effect of workers increase in trust causing them to switch to more competitive sectors.

Figure S2 addresses whether changes in income are confounding our main effect. It is also constructed exactly like Figure 3, but using (the log of) income as the left hand side variable (instead of trust). It shows that workers that switch to more competitive firms are not more likely to experience higher income growth.

4 Supplementary Text: Laboratory Experiment

Further description of the experiment. The data analyzed in this paper includes all experimental sessions we ran for this project (e.g., there were no pilot sessions that are not included in the data). There was a total 11 experimental sessions, each with 20 participants each. Six of those where "control" sessions where the standard public good game was played and five where the "competition" sessions. In each round, groups were formed randomly, without repetition (i.e., no two players would play together for two rounds or more). The matching of which groups competed against each other was similarly random.

After the experimental game, players were asked a six questions about culture and values. The first was the generalized trust question, with the answers in a 0-10 scale. The question asked whether "*generally speaking, on a scale of 0 to 10, would you say that most people can be trusted or that you are never careful when dealing with others? Zero means that one can never be too careful, ten means that one can trust people.*"

To provide us with a series of placebo questions, respondents were also asked five other questions. Three of them asked asked how the respondent felt between two statements in a 0-10 scale (similarly to the trust question). The questions were:

- "Income distribution should be more egalitarian" *vs.* "More individual effort should be encouraged."

[29]NACE is the acronym for *Nomenclature des Activités Économiques dans la Communauté Européenne.* The ORBIS database (also known as AMADEUS for European countries) is a product of the Bureau van Dijk Electronic Publishing (BvD). It provides data on firms financial and productive activities from balance sheets and income statements, and allows the construction of nationally representative statistics (Kalemli-Ozcan et al, 2015). We do not use the data from year prior to 2008 since there are issue with coverage of all German firms for that period (Kalemli-Ozcan et al, 2015).

- "The state should have a greater responsibility to ensure everyone's needs" *vs.* "Individuals should have a greater responsibility to support themselves."
- "In the long run, hard work often brings a better life" vs. "Working hard does not generally bring success - luck and contacts are more important."

Larger numbers in the 0-10 scale indicate agreement with the second statement listed. The other two questions asked whether they "strongly agree," "somewhat agree," "somewhat disagree," or "strongly disagree" with each of the following two statements:

- "One of my main goals in life is to make my parents proud of me."
- "I try to be myself rather than follow others."

To keep the analysis comparable to the other pieces of evidence in this paper, we transform the answers to the trust and all the other five questions to binary indicator. In particular, we code a variable equalling one if the scale is equal or above the median of the answers in the control (no competition group). For the trust question, this implies that all respondents who answered five or higher in the trust scale are coded as "trusting"(=1) and those that did not as "not trusting" (=0).

In the first two sessions we carried out, the games was played for 10 rounds (instead of 19) and only the trust question (but not the five other cultural questions) were asked. This implies that there is one control and one treatment session that differs from the others in this regard, and that the sample size for the trust question results is larger than the one for the other questions (220 versus 180 observations).

Additional results: effect of competition trust. Panel A of Table S9 presents the effect of adding competition to the public good game. The "control mean" in the first line indicates that 50.8% of participants reported trust in the control group. Adding competition to the experimental game increases the change they report trust by 14.6 p.p.. This estimate is from a regression of trust against a dummy indicating the treatment competition, controlling for respondent's age, education, gender, and marital status.

Since there are a relatively small number of sessions (11), Table S9 calculates the p-value of the competition effect based on four different approaches. The first one uses t-tests based standard errors clustered at the session level (the "cluster" column), and suggests a p-value of 0.011. The second method aggregates the data to the session level, and is based on the t-tests from a regression that has only 11 observations. Note that, in both cases, the p-value is obtained from a t-distribution with 10 degrees of freedom, making it more conservative than if the standard normal distribution was used. The "Fisher" column presents two-sided Fisher-exact permutation p-values. Given five treated units out of a total of eleven, there are 462 possible permutations of which sessions can be labeled treatment or control. We calculate the treatment effect for each of these 462 combinations, and calculate the p-value as the number of permutations where the absolute value of the effect is larger than the one from the true assignment . The last column on Table S9 presents p-values based on Ibragimov and Mueller (2016) procedure. In all cases, the effect is significant at the 5% level.

The second row of Panel A presents the robustness of our result to the removal of the controls in our sample. The estimated treatment effect is very similar (12.2 p.p.). As expected, the exclusion of controls make them slightly less precise (larger p-values).

The results from Panel A can also be seen graphically on Figures S3 and S4, which respectively plot the PDF and CDF of the distribution of answers in the "no competition" and "competition" groups. They both indicate that a mass of answers at the 2-4 range of the trust scale is shifted towards answers in the 7-9 range when respondents are exposed to the competition treatment. They also illustrate that the using different cutoffs when coding the binary trust outcome in Table S9 would be unlikely to affect the results.

Panel B repeats the exercise of Panel A, but excludes the 18 (out of 220) participant in the experiment that were not students. The estimated effects are slightly larger and more precisely estimated. While the difference in effects between Panel A and B are small, they are consistent with those that have less work experience (students) being more affected by competition.

Panel C provides a series of placebo tests based on the other questions on culture and values. For none of these variables we find a statistically significant difference between answers in the groups exposed to competition or not. This suggests that exposure to competition does not affect answers to questions unrelated to trust, nor it affects how people answer questions in numerical scale (e.g., due to anchoring effects from being exposed to different numbers during the experimental game).

Panel D shows that respondents in both the competition or no competition groups had similar age, gender and were equally likely to not be employed. Respondents were on average young (22.8 years old), equally likely to be men or women, and most of them were full-time students (only 2.3% are employed).

Additional results: learning throughout the experimental game. To be added.

Table S1: Summary Statistics: Cross-Sectional Data

Variable	Mean	Std. Dev.	Variable	Mean	Std. Dev.
Can Trust	0.50	0.50	Education	13.54	2.87
Female	0.60	0.49	Age	46.02	16.43
Anglo	0.21	0.40	North Europe	0.22	0.41
South Europe	0.08	0.27	Africa	0.09	0.28
Asia	0.06	0.24	Black	0.13	0.34
White	0.81	0.40	City Size	290.96	1041.76
Married	0.52	0.50	Widowed/Divorced	0.27	0.45
Protestant	0.54	0.50	Catholic	0.22	0.42
Jewish	0.03	0.16	No Religion	0.13	0.33
Comp50	0.61	0.25	Comp4	0.83	0.16

Table S2: Sectoral Averages for Competition Measure (Comp50)

Information, Finance and Insurance, Real Estate	48.17%
Agriculture, Mining and Utilities	22.20%
Manufacturing, Wholesale Trade, Transportation and Warehousing	34.36%
Arts, Entertainment, and Recreation, Accommodation and Food Services	84.11%
Other Services	80.32%
Education Services	80.03%
Professional, Scientific and Technical Services	70.28%
Health Care and Social Assistance	70.39%
Retail Trade	50.78%

Table S3: Summary Statistics for Extended Workplace Variables

Variable	Response Categories	Mean (SD)
Others Credit	1. Often, 2. Sometimes, 3. Rarely, 4. Never	3.10 (0.96)
Put Down	1. Often, 2. Sometimes, 3. Rarely, 4. Never	3.43 (0.92)
Heated Arguments	1. Often, 2. Sometimes, 3. Rarely, 4. Never	3.21 (0.89)
Lack Information	1. Often, 2. Sometimes, 3. Rarely, 4. Never	2.93 (0.97)
Helpful	1. V. True, 2. Somewhat T. 3. Not too True, 4. Not at all	1.46 (0.66)
Treat Respect	1. Strong Agree, 2. Agree, 3. Disagree, 4. S. Disagree	1.70 (0.66)
Act Upset	1. Often, 2. Sometimes, 3. Rarely, 4. Never	3.76 (0.62)
Shout	1. Often, 2. Sometimes, 3. Rarely, 4. Never	3.68 (0.68)
Look Away	1. Strong Agree, 2. Agree, 3. Disagree, 4. S. Disagree	3.18 (0.78)
Work Stressful	1. Always, 2. Often, 3. Sometimes, 4. Hardly Ever, 5. Never	2.74 (1.00)
Skip Work	1. Often, 2. Sometimes, 3. Rarely, 4. Never	3.75 (0.57)
Personal Space	1. Often, 2. Sometimes, 3. Rarely, 4. Never	3.57 (0.75)
Standards	1. Often, 2. Sometimes, 3. Rarely, 4. Never	2.38 (1.10)
Report Probs	1. Often, 2. Sometimes, 3. Rarely, 4. Never	1.74 (0.94)
Harm Threat	1. Often, 2. Sometimes, 3. Rarely, 4. Never	3.88 (0.43)
Job Secure	1. V. True, 2. Somewhat T. 3. Not too True, 4. Not at all	1.64 (0.82)
Work Size	7 categories (1-9, 10-49, 50-99, ..,2000+)	2.92 (1.82)
Union Member	1. Yes, 2. No	1.90 (0.29)

Table S4: Effects of Sectoral Concentration on Trust

	Dependent Variable: Trust Indicator						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Comp50	0.191	0.191	0.213	0.208	0.187	0.161	0.187
	(0.073)***	(0.073)***	(0.079)***	(0.079)***	(0.072)***	(0.078)**	(0.086)**
Education	0.026	0.026	0.026	0.026	0.026	0.022	0.021
	(0.008)***	(0.008)***	(0.008)***	(0.008)***	(0.007)***	(0.009)**	(0.01)**
Controls Included in Specification:							
Job Security	-	Yes	-	Yes	-	-	Yes
Union Status	-	-	-	Yes	-	-	Yes
Arguments	-	-	-	Yes	-	-	Yes
Skip Work	-	-	-	Yes	-	-	Yes
Supervisor	-	-	-	-	Yes	-	Yes
More Good	-	-	-	-	-	Yes	Yes
Optimism	-	-	-	-	-	Yes	Yes
Workplace Size	-	-	Yes	Yes	-	-	Yes
Other Workplace Covariates	-	-	-	Yes	-	-	Yes
Observations	612	612	612	612	612	530	530

*** -Significant (1% level); **-Significant (5% level); *-Significant (10% level).

Robust standard errors clustered at the industry level in parenthesis. Each column reports the results of a separate regression with a dummy indicator if the respondent can trust as the dependent variable. All specifications include age, income, gender, race, ethnicity, marital status, and religion dummies as well as city size, and the list of additional controls indicate the variables added to the specification in that column. See text for a description of the variables.

Table S5: Effects of Sectoral Concentration on Trust Interacted with Experience

	Dependent Variable: Trust Indicator						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Comp50	-0.076	-0.076	-0.061	-0.060	-0.090	-0.097	-0.071
	(0.149)	(0.149)	(0.146)	(0.152)	(0.149)	(0.155)	(0.156)
Comp50*Experience	0.009	0.009	0.009	0.009	0.010	0.009	0.009
	(0.005)**	(0.005)**	(0.005)**	(0.005)*	(0.005)**	(0.005)*	(0.005)*
Education	0.031	0.031	0.032	0.031	0.032	0.028	0.027
	(0.008)***	(0.008)***	(0.008)***	(0.009)***	(0.008)***	(0.009)***	(0.011)**
Controls Included in Specification:							
Job Security	-	Yes	-	Yes	-	-	Yes
Union Status	-	-	-	Yes	-	-	Yes
Arguments	-	-	-	Yes	-	-	Yes
Skip Work	-	-	-	Yes	-	-	Yes
Supervisor	-	-	-	-	Yes	-	Yes
More Good	-	-	-	-	-	Yes	Yes
Optimism	-	-	-	-	-	Yes	Yes
Workplace Size	-	-	Yes	Yes	-	-	Yes
Other Workplace Covariates	-	-	-	Yes	-	-	Yes
Observations	612	612	612	612	612	530	530

*** -Significant (1% level); **-Significant (5% level); *-Significant (10% level).
Robust standard errors clustered at the industry level in parenthesis. Each column reports the results of a separate regression with a dummy indicator if the respondent can trust as the dependent variable. All specifications include age, income, gender, race, ethnicity, marital status, and religion dummies as well as city size, and the list of additional controls indicate the variables added to the specification in that column. See text for a description of the variables.

Table S6: Summary Statistics - State Level Data

	Mean	Std. Dev.	Min.	Max.
Individual-level Variables:				
Can Trust (×100)	39.57	48.90	0.00	100.00
Age	45.19	17.60	18.00	88.00
City Size (in thousands)	369.47	1229.79	0.00	7895.00
Employed	0.57	0.49	0.00	1.00
Income (in 1986 US$ thou.)*	13.13	17.95	0.00	139.30
Female	0.57	0.50	0.00	1.00
Highest Degree: High School	0.54	0.50	0.00	1.00
Highest Degree: College or Higher	0.19	0.40	0.00	1.00
White	0.84	0.36	0.00	1.00
Black	0.13	0.34	0.00	1.00
Married	0.57	0.49	0.00	1.00
Jewish	0.02	0.14	0.00	1.00
Catholic	0.25	0.43	0.00	1.00
State-level Variables:				
New Incorporations (per 100,000 people)	233.78	115.27	85.08	721.65
Gini Index (Income)	0.33	0.03	0.26	0.45
Mean Income (per capita 1982 US$ thou.)	27.36	34.16	17.50	40.79

*The income variable is calculated by the GSS based on imputations from categorical variables across years. See GSS Methodological Report n. 64 for details.

Table S7: Effect of Banking Deregulation on Trust

	Dependent Variable: Trust Indicator ($\times$100)					
	(1)	(2)	(3)	(4)	(5)	(6)
Post Inter-State Deregulation	-0.431	-0.668	-0.836	-1.322	-0.855	-1.459
	(1.321)	(1.508)	(1.515)	(1.546)	(1.492)	(1.489)
Years Since Inter-State Deregulation	1.244**	1.422**	1.317*	1.286*	1.502*	1.720**
	(0.519)	(0.682)	(0.722)	(0.672)	(0.78)	(0.776)
Additional Controls						
Individual Income (GSS)		Y				
log Personal Income p.c. (BEA)			Y			Y
Gini Index - Income				Y		Y
Employment-Population Ratio					Y	Y
Individual Controls	Y	Y	Y	Y	Y	Y
State Fixed Effects	Y	Y	Y	Y	Y	Y
Time Fixed Effects	Y	Y	Y	Y	Y	Y
State-Specific Trends	Y	Y	Y	Y	Y	Y
Observations	17455	17455	17455	17455	17455	17455

*** -Significant (1% level); **-Significant (5% level); *-Significant (10% level).
Robust standard errors clustered at the state level in parenthesis. The dependent variable is an indicator if the respondent trusts (multiplied by 100). Each column reports a separate regression controlling for state fixed effects, year effects, state-specific trends, and individual controls: quadratic polynomial of age, indicators for completed high school and college education, household size, population size of the city and state of residence, and a set of dummies for employment status, race, gender, marital status and religion. See text for further details and definitions of controls

Table S8: Effect of Oil Reserve Value

	log Median Income	log Personal Income p.c.	Gini Index - Income	Trust Indicator (×100)		Predicted Trust
	(1)	(2)	(3)	(4)	(5)	(6)
Oil Reserves × Oil Price	0.039***	0.043***	-0.019	-1.372	-1.694	0.26
	(0.005)	(0.005)	(0.115)	(1.414)	(1.149)	(0.564)
Individual Controls					Y	
State Fixed Effects	Y	Y	Y	Y	Y	Y
Time Fixed Effects	Y	Y	Y	Y	Y	Y
State-Specific Trends	Y	Y	Y	Y	Y	Y
Observations	17455	17455	17455	17455	17455	17455

*** -Significant (1% level); **-Significant (5% level); *-Significant (10% level).

Robust standard errors clustered at the state level in parenthesis. Each column reports a separate regression controlling for state fixed effects, year effects, state-specific trends. and individual controls: quadratic polynomial of age, indicators for completed high school and college education, household size, population size of the city and state of residence, and a set of dummies for employment status, race, gender, marital status and religion. See text for further details and definitions of income controls.

Table S9: Effect of Competition on Trust in Experimental Sample

Outcome	Controls?	Control Mean	Effect of Competition	p-value of competition effect: Cluster	Agg.	Fisher	IM
Panel A: Full Sample							
Trust	Y	0.508	0.146	0.011	0.015	0.017	0.041
Trust	N	0.508	0.122	0.070	0.085	0.099	0.126
Panel B: Students Only Sample (N=202)							
Trust	Y	0.518	0.180	0.004	0.007	0.004	0.026
Trust	N	0.518	0.160	0.024	0.032	0.015	0.064
Panel C: Other Questions (N=180)							
Individual Effort	Y	0.600	-0.086	0.309	0.304	0.297	0.290
Individual Responsibility	Y	0.600	-0.043	0.575	0.594	0.558	0.571
Not Working Hard	Y	0.650	-0.051	0.524	0.544	0.619	0.518
Parents Opinion	Y	0.790	-0.055	0.419	0.454	0.467	0.431
Non-Conformism	Y	0.600	-0.096	0.358	0.394	0.355	0.375
Panel D: Covariate Balance (N=220)							
Age	N	22.75	1.72	0.109	0.129	0.123	0.173
Female	N	0.491	0.058	0.478	0.501	0.495	0.523
Employed	N	0.023	0.066	0.509	0.531	0.563	0.551

*** -Significant (1% level); **-Significant (5% level); *-Significant (10% level).
Robust standard errors clustered at the individual level in parentheses. Controls are year dummies interacted with age, education, gender, and marital status.

Table S10: Learning from Other Players in the Experiment

	Dependent Variable: Contribution			
	(1)	(2)	(3)	(4)
Accumulated Average Partners' Contribution	0.381*** (0.093)	0.248** (0.125)	0.449*** (0.072)	0.418*** (0.092)
Accumulated Average Competitors' Contribution	0.382*** (0.112)	0.380** (0.178)	0.125 (0.077)	0.147 (0.135)
Sessions in Sample	Competition	Competition	No Competition	No Competition
Estimation Method	OLS	IV	OLS	IV
Observations	1520	1520	1860	1860

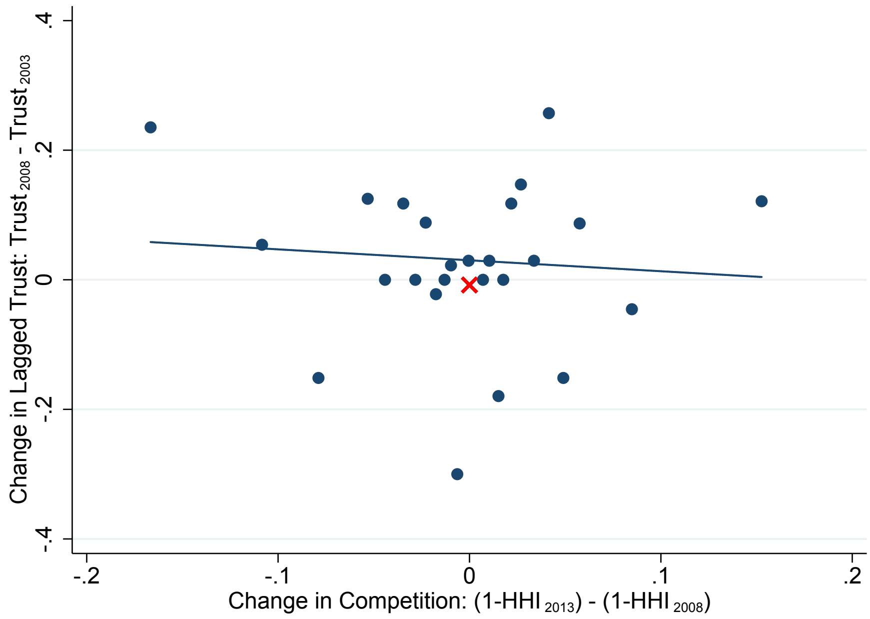
*** -Significant (1% level); **-Significant (5% level); *-Significant (10% level).
Robust standard errors clustered at the individual level in parentheses. Unit of observation is a player-experimental period. Accumulated average competitors's contribution is based on hypothetical competitor matches in the non-competitive session. Instruments are based on partners' and competitors' first-period contribution - see text for details.

Table S11: Effect Experimental Experience on Trust

	Dependent Variable: Trust Indicator ($\times$100)					
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	0.325 (0.165)			0.605*** (0.149)		
1st Contribution		0.313** (0.135)	0.374*** (0.110)		0.529*** (0.143)	0.602** (0.178)
Trend	4.917** (2.207)	4.100** (1.881)	5.538*** (2.092)	2.874 (2.658)	2.308 (2.503)	2.173 (4.433)
Sessions in Sample	Compet.	Compet.	Compet.	No Compet.	No Compet.	No Compet.
Estimation Method	OLS	OLS	IV	OLS	OLS	IV
Observations	100	100	100	120	120	120

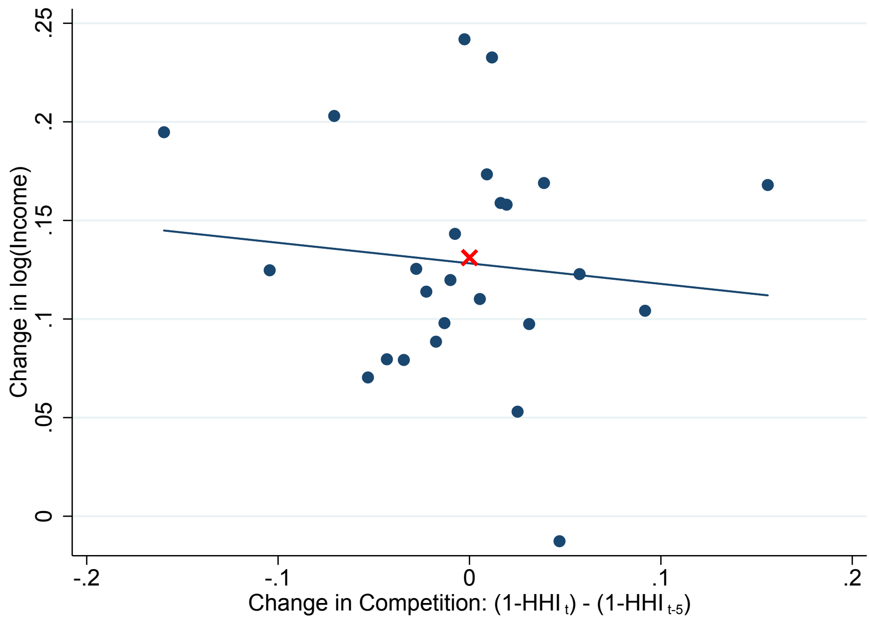
*** -Significant (1% level); **-Significant (5% level); *-Significant (10% level).
Robust standard errors in parentheses. Unit of observation is a player. See text for definition of the variables and instrument.

Figure S1: Falsification Test - Changes in Competition Uncorrelated with Previous Trends in Trust



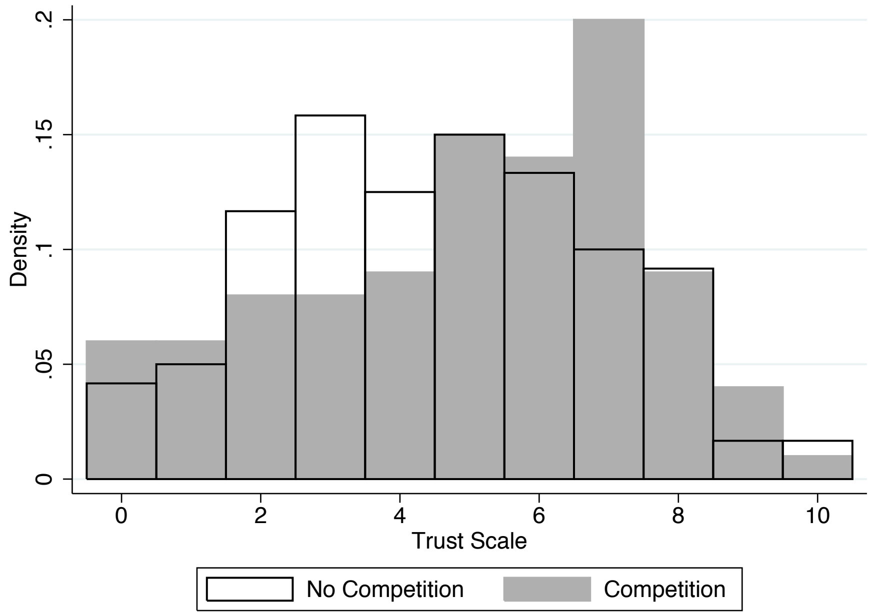
Note:

Figure S2: Falsification Test - Changes in Competition Uncorrelated with Changes in Income



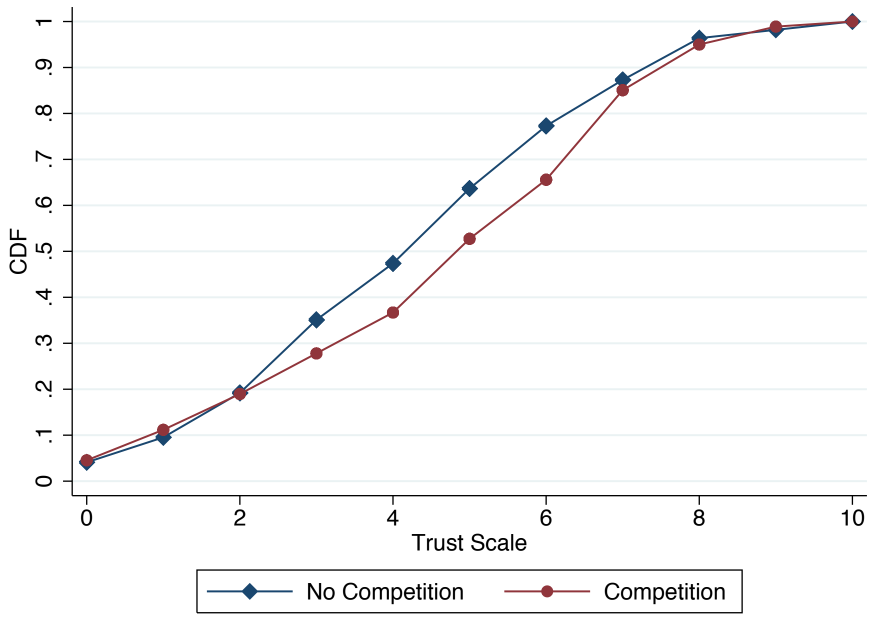
Note:

Figure S3: Distribution of Answers to Trust Question in Experimental Sample



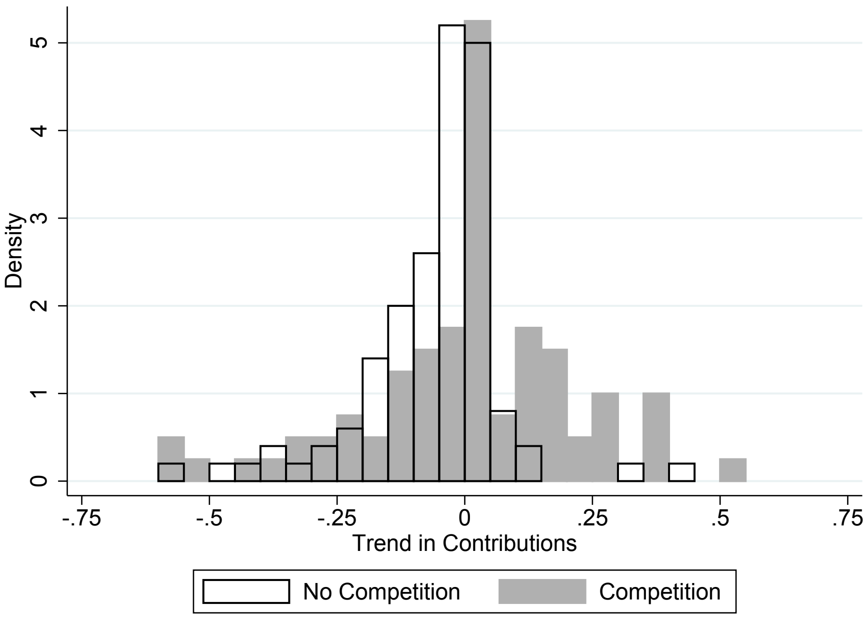
Note:

Figure S4: Distribution of Answers to Trust Question in Experimental Sample - Cumulative Distribution



Note:

Figure S5: Distribution of Answers to Trust Question in Experimental Sample - Cumulative Distribution



Note: