Title: The Origins of Human Pro-Sociality: Cultural Group Selection in the Workplace and the Laboratory

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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 consistent with a set of theories based on group level selection of cultural norms favoring 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 firms facing more competition, 2. live in states where competition increases, 3. move to more competitive industries and 4., 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

The variety of theories proposed to explain these phenomena are typically hard to assess empirically.2 Their predictions concern elements of our primordial past, perhaps traceable via the

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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 feature a general discussion of the puzzle of human pro-sociality, and Bowles and Gintis (2013) who also reference this discussion in motivation.

2 Examples of alternative theories to CGS are reciprocal altruism (e.g., Hoffman et al., 1998), sexual selection (Aoki, 2004), and the mismatch hypothesis of Tooby and Cosmides (2005).
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 allow us to scrutinize contemporary data for evidence in accord with their predictions. We report on some of 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 (pro-social) 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 behavioral 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.

The narrative in which this explanation for human pro-sociality is couched is in terms of our prehistoric past. But CGS does not just require that the forces of group level competition were present at some time in our historic past. Since CGS emphasizes the non-hard-wired features of behavior such as norms, forces of group level competition should help in sustaining cooperative norms, and hence observed pro-social behavior, even in contemporary settings.⁷

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³ Richerson et al (2016) highlight 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 favorable trait has been argued using observed group extinction rates amongst tribal groups in Papua New Guinea (Soltis et al., 1995).
⁴ See Richerson and Boyd’s (1998) tribal social instincts hypothesis, and the evidence suggesting that we are evolved social learners discussed in Richerson and Boyd (2010).
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 behaviors 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. 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 pro-social behavior.
⁶ Three types of selective forces are emphasized: i) natural selection, wiping out the less successful groups via competition over resources or direct conflict, or having higher growth rates; ii) imitation of successful groups by the less successful, and iii) selective migration and internalization of norms upon migration. See Henrich (2016) for a detailed discussion.
⁷ We do note, however, that if the forces of selection are weak, or if the rate of cultural mutation is low, we may not find any effects even if CGS was operative in creating pro-sociality. In such cases, high levels of pro-sociality may have been driven by intense competition in the past. If it is absent today but there has not been enough time for weak forces of selection or insufficient mutation to emerge to displace it, then the posited correlation would not arise.
One way of assessing this implication 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 group beneficial behavior, such as altruistic punishment; as discussed in Fehr and Fischbacher (2003). Instead of testing for a single specific behavior, such as altruistic punishment, another way to proceed would be to see whether groups experiencing more intense inter-group competition exhibit evidence of more pro-social behavior. We attempt that here, in economic settings.

We study individuals who vary in how much competition is experienced across the organizations in which they work. We test to see whether variation in cross-group competitiveness affects a measure of the individuals’ pro-sociality. We report on a variety of individual data sources, both cross-sectional and at panel (within-person) level to do this. Before turning to the data, we clarify two key aspects: ii) the definition of relevant organizations or “groups” over which CGS may occur, and over which competition is to be gauged, and ii) our measure of individual level pro-sociality.

**Measuring Groups**

The perhaps most ubiquitous avenue of group level competition occurring in contemporary settings is likely to be competition across firms. Individuals within firms need to undertake (at least some) group beneficial but individually costly actions. Moreover, competition across firms affects returns to cooperative versus selfish individual acts, and, we conjecture, should help in selecting the firms most successful in obtaining cooperative efforts from their workers. There is already considerable evidence showing that a degree of norm based acculturation occurs through workplace interactions.\(^8\) To this, we add the conjecture that workplaces subject to more intense external competition will be more likely to engender pro-social norms of cooperation amongst their employees.

**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 the empirical analyses reported here. “Do you think that, on the whole, people can be trusted, or that you can't be too careful in dealing with people?” As has been documented, this question conjures 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).

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\(^8\) Richerson et al. (2016) also argue for firms as one 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) view of the firm as a repository of tacit knowledge that is hard to transmit, yielding a competitive advantage and expansion potential for firms with the right types of knowledge. Ashforth et al. (2008) present evidence linking the social identity of employees to the performance of firms.
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. First, in the laboratory, generalized trust reported by individuals seems to be malleable and influenced by specific experiences. Second, beliefs about the trustworthiness of others seem to matter for informing subjects’ potentially costly trusting decisions, and correlate with answers to the generalized trust question (Sapienza et al., 2013). Third, individuals tend to respond to trustworthiness experiences by increasing their own trustworthiness, which is consistent with individuals being conditional cooperators, willing to follow perceived norms. These are not the only factors influencing responses to the generalized trust question and we discuss other aspects known to affect responses as we present the main empirical results.

Schematic contents

The evidence we present is drawn from four sources: i) US cross-sectional correlations between competitiveness of industry of employment and individual trust, ii) US state-level policy changes that altered cross-firm competition at the state level, inducing changes in individual trust, iii) German panel data evidence showing changes in individuals’ industry of employment competitiveness induced changes in individual trust. All three forms of evidence confirm a strong and statistically significant effect of increased competition across firms on increased individual trust. We discuss precisely how CGS explains this observational data after we present it.

We augment these findings with: iv) evidence drawn from laboratory experiments conducted in France. We placed 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 that was observed 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 behavior as it affects the economic returns to

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9 Thoni (2015) summarizes a number of papers in this literature: “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.” Play in games and survey question responses do not always line up, perhaps because introspection is used sometimes (but not always) when answering trust questions. Subjects 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 do not perceive such similarity. Variable use of introspection is conjectured by Sapienza et al. (2013) to explain contrasting findings reported by Glaeser et al. (2000) and Fehr et al. (2003). Studies trying to discern whether social conformity or reciprocity are also drivers tend to find evidence for both (Bardsley and Sausgruber, 2005; Falk, 2004; and Bohnet and Zeckhauser, 2004).

10 Fischbacher et al. (2001) provide experimental evidence of conditionally cooperative behavior in a public goods experiment. Fischbacher and Gachter (2010) provide a comprehensive list of studies finding similar evidence of conditional cooperative behavior.
cooperation. Some subjects experience enhanced group beneficial behavior and form new groups in which they also exhibit increased group beneficial actions. These subjects respond by answering the generalized trust question more positively, perhaps due to extrapolating their experiences in the experimental setting beyond the laboratory. We return to the precise interpretation of the empirical findings in light of CGS as they are presented.

Results

Cross-sectional evidence in the US.

By its nature, cross-sectional data provides the weakest evidence that we consider here because a correlation between cross firm competition and worker pro-sociality may reflect the effects of omitted variables that drive 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 extensively focused on the workplace of survey respondents. This allows us to control for many factors that may be affecting the generalized trust level of individual respondents, as well as rich personal information about respondents that would allow us to control for individual characteristics known to correlate with individual trust. As an example, we are able to include controls for the security of employment to ensure that positive answers to the generalized trust question are not just picking up tolerance for risk.11

The competitiveness of each worker’s industry of employment is measured by the percentage of sales covered by the $k$ largest firms ($k = 4, 8, 20, 50$) in an industry (as defined by the North American Industrial Classification System - NAICS). Our reported measure of competition is equal to one minus the sales covered by the 50 largest firms. In other words, the competitiveness of industry $s$ is the percentage of total sales in $s$ that is not covered by the largest 50 firms in that industry.12

Figure 1 displays a binned scatter plot cutting the 612 GSS respondents in our sample in to 25 equal sized bins arranged by industrial competitiveness (the x-axis) plotted against share of workers reporting affirmative answers to the generalized trust question in that bin (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 regressions reported in the Supplementary Materials. The positive slope of 0.191 (p-value of 0.007) is robust across many demanding specifications. In particular, as explained in the Supplementary Materials, including the rich and unusually comprehensive set of workplace controls obtained from the GSS workplace module does not alter this finding.

11 In laboratory settings, individuals are more likely to trust partners with monetary stakes if they are less risk averse. So perception of risk may be affecting answers to the trust question too. It could be that more competitive firms offer more security to employees and this makes them more trusting. As detailed in Part 1 of the Supplementary Materials, this does not affect the correlation between competition and trust we report here, suggesting this is not the channel.

12 Further information on the data is provided in the Supplementary Materials. Using competition measures based on the shares of the four, eight, and twenty largest firms yield similar results.
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 industrial competitiveness has the strongest association with trust (see Supplementary Materials).\textsuperscript{13}

Despite the inclusion of detailed workplace controls, as noted above, such correlative evidence is a long way from evidence of a causal relationship. The possibility of omitted factors potentially affecting both competition and individual trust 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. It turns out that such variation has been provided by episodes of US banking deregulation which we turn to next.

\textit{Banking Deregulation in US States}

Starting in the early to mid-1980s, multiple US states lifted long-standing restrictions that prohibited banks from out-of-state to operate within their borders. Of particular interest for our research design, different states undertook deregulation at different times. Previous research (Black and Strahan 2002, Kerr and Nanda 2009) indicate that these reforms can be seen as exogenous shocks to competition across all industries (including the non-financial) in a state. This is because banking deregulation increased credit availability which, in turn, facilitated the creation of new firms and raised the contestability of local markets.

Figure 2 plots an event-study graph showing how deregulation affected trust levels, firm entry, and firm closures. It shows what is the effect of being 10, 9, …, 2, 1 years before a reform, as well as 1,2, …, 10 years after. All variables are normalized to be equal to zero at the date of the year of reform (“year zero”). 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) competitiveness increases with the reforms. It continues to do so until ten years after the reforms. This is consistent with the posited effects of increased credit availability due to banking reform on competition, and is already well known. The pattern for years prior to the reform reassures that there are no pre-existing trends in competition across states that are correlated with the timing of the reforms.

We now augment this finding about firm level competition with information about individual generalized trust levels obtained from the GSS.\textsuperscript{14} Our dataset contains a total of 17,455

\textsuperscript{13} 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. The next two types of data have stronger causal interpretations.

\textsuperscript{14} As in the rest of this paper, we us a binary indicator that equals to one if the respondent answered “can trust” to the question “Generally speaking, would you say that people can be trusted or that you can’t be too careful in dealing with people?” The other possible answers (coded as zero) are “cannot trust” and “depends.” The Supplementary Materials discuss the robustness to alternative coding further (a small fraction – 3.9% - of
individuals in the 1973-1994 waves of the GSS. Leveraging that we can observe state of residence of GSS respondents, the blue circles report how the propensity to affirmatively answer the generalized trust question is affected by banking deregulation. The blue circles again show 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 Material provides detailed information on how Figure 2 is constructed and additional statistical tests. We note that the estimates in the figure control for a host of individual level correlates of trust, state and year fixed effects and state-specific linear trends, which control for the effect of 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. Estimates of the preferred specification elaborated there imply that a state enacting an interstate banking reform would experience a 1.4 percentage point increase in the share of its population reporting that they “can trust” every year after the reform.

Figure 2 supports a causal interpretation of the effect of banking deregulation increasing trust. It is also consistent with increases in firm level competition at the state level leading to a rise in individual level trust — precisely as would be posited by CGS.

A separate issue with such estimates is whether banking deregulation increases trust via increased firm competition or through another factor. For example, deregulation may have affected income growth or changed migration patterns. The Supplementary Materials provide further evidence that distinguishes among these possible channels, and argues that the evidence is best explained by deregulation affecting trust via firm-level competition.

The results discussed in this section identify the effect of competition via an aggregate (state-level) shock to competition. An alternative and complementary strategy is to study whether individuals moving between industries with different levels of competition experience changes in trust. For this, tracking individual workers through time and observing changes in industry of work, and trust is needed. To our knowledge, no US-based survey that tracks individuals over a significant length of time has asked the trust question across multiple surveys, while simultaneously reporting their industry of work. However, a dataset with such characteristics exists for German workers, and we turn to analyzing this now.

**Movers across industries in the German Socioeconomic Panel**

We use the three waves of the German Socioeconomic Panel (SOEP) asking a trust question and including information on industry of employment: 2003, 2008, and 2013. The SOEP is representative of the German population; however our sample only contains individuals that were employed in at least two consecutive waves of the survey. The sample contains 9103 respondents answer “depends”). Note the possible answers in the 1973-1994 Waves of the GSS is slightly different than in the 2004 GSS we studied in the previous section.
observations from 6447 unique individuals employed across 50 different industries. Mean trust levels are higher than in the US: 65% of respondents indicate a positive response on trust.\(^\text{15}\)

The SOEP reports employed individual’s industry of work — which we match to a Herfindahl-Hirschman Index (HHI) measure of competition obtained from the ORBIS data base.\(^\text{16}\) Our measure of competition is one minus the HHI of firms operating revenues. It is thus equal to zero in an industry with only one firm (monopoly) and would be equal to one in an industry with infinite number of small firms. Our competition measure is mostly stable through time, and certainly not time-variable enough to identify the effect of a change in competition across individuals who do not change industries. So, instead, we explore the effect of changes in firm level competitiveness by tracking individuals who changed industry (25.4% of the sample). Some individuals moved to jobs in more competitive industries, others stayed put, and others moved to industries with less competition.

Figure 3 is a visual summary of the results. Each blue circle in the graph is a binned average, constructed using only the respondents that moved across industries. We cut the x-axis variable (change in competition between two SOEP waves) in to 25 bins of equal size, ordered from negative to positive changes. This is plotted against the average change in trust between SOEP waves per bin. The regression line is estimated based on the original (unbinned) data. There is again a positive relationship between competitiveness of sector and individual trust. We highlight the distinction between the earlier Figure 1 and the results represented here in Figure 3. The former indicates that individuals in more competitive industries report higher trust. The latter indicates that workers that move from less competitive to more competitive industries are more likely to increase their reported trust levels. The red X in Figure 3 denotes the average change in trust for those that did not change industries. Their change in trust is similar to those that moved across industries of comparable competitiveness; that is, zero.

The slope of 0.45 indicates that a one standard deviation change in the competition measure increases the probability a worker responds affirmatively to the trust question by 1.7 p.p. Expressed alternatively, a worker that moves from a hypothetic industry where three firms have 33% market share each, to one where four firms have 25% market share each becomes 3.8 p.p. more likely to respond positively to the trust question.

In the Supplementary Materials, we present three pieces of evidence that further support our interpretation of Figure 3 as a causal effect of competition on trust. First, we provide evidence of no pre-existing trends in the trust levels of movers; those that move to more competitive industries were not experiencing higher growth in trust prior to the move. Second, we show that our result cannot be explained by changing income; those moving to more competitive sectors do

\(^{15}\) Respondents are asked whether they agree with the statement “on the whole one can trust people.” We code a binary trust indicator that equals to one if respondent answered “totally agree” or “slightly agree” (instead of “disagree slightly”, and “totally disagree”). The Supplementary Materials discuss how the results are robust to how we code trust (only 10% of respondents answer “totally agree” or “totally disagree”).

\(^{16}\) The Supplementary Materials provide further information about data construction. An industry of work is defined as a NACE code. NACE is the “statistical classification of economic activities in the European Community.” We exclude individuals working in the public industry. The HHI is equal to the sum of the squared value of the revenue share of each firm an industry.
not experience higher income growth. Third, we show that differential trends in trust levels that are correlated with observable characteristics also cannot explain our results.

Overall, the results in Figure 3 show that the individuals who changed jobs and ended up in more (less) competitive industries increased (lowered) their levels of trust. This strongly suggests a causal effect of sectoral level competition on individual level trust. The next section explains how CGS can account for all of these findings.

**CGS Explanation of Observational Data**

There is considerable experimental evidence supporting the conclusion that people are conditional cooperators: they condition actions based on their beliefs regarding prevailing norms of behavior. They cooperate if they believe their partners are also likely to do so, they are unlikely to act cooperatively if they believe that others will not.\(^{17}\)

The environment in which people interact shapes both the social and economic returns to following cooperative norms. For instance, many aspects of groups within the work environment will determine whether cooperation can be an equilibrium in behavior amongst group members, or whether it is strictly dominated by more selfish actions. Competition across firms can play two distinct roles in affecting this. First, there is a static equilibrium effect which arises from competition altering rewards from cooperative versus selfish behavior, even without changing the distribution of firms. Competition across firms punishes individual free-riding behavior and rewards cooperative behavior. Absent competitive threats, members of groups can readily shirk without serious payoff consequences for their firm. This is not so if a firm faces an existential threat. Less dramatically, even if a firm is not close to the brink of survival, more intense market competition renders firm level payoffs more responsive to the efforts of group members. With intense competition, the deleterious effects of shirking are magnified by large loss of market share, revenues, and in turn lower group level payoffs. Without competition, attendant declines in quality or efficiency arising from poor performance have weaker, and perhaps non-existent, payoff consequences.\(^{18}\)

Competition across firms does not typically lead to a unique equilibrium in social norms, but if intense enough can sustain a cooperative group norm. Depending on the setting, multiple different cooperative group equilibria differentiated by the level of costly effort can also be sustained. For example, if individuals are complementary in production, an individual believing co-workers to all be shirkers, and thus unable to produce a viable product, will similarly also choose to exert low effort. An equilibrium where no one voluntarily contributes to cooperative tasks is sustained and such a workplace looks to have non-cooperative norms. In contrast, with the same complementary production process, and a workplace where all other workers are believed to be contributing high effort, a single worker will optimally choose to exert high effort as well to ensure viable output. In that case, a cooperative norm is sustained. When payoffs are continuous in both the quality of the product and the intensity of the competition, then the degree of cooperative effort that can be sustained can be continuously increasing in the intensity of

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\(^{17}\) References for these claims listed in Footnote 10.

\(^{18}\) Competition acts similarly to third party punishment of norm violation in this context. It disciplines shirkers with payoff declines. A difference is that all individuals in the group are punished, but the disciplining effect is similar.
market competition across firms. We have formalized this in an economic model that we include in the Supplementary Materials.

Competition’s first effect is thus to make it possible, but not necessary, for group level cooperative norms to arise as equilibria. The literature has shown that there are many other ways to stabilize cooperative norms as equilibria, such as institutional punishment, third party punishment or reputations. Cross-group competition may also enhance these other well-studied mechanisms for generating cooperative norm equilibria, but with or without such factors, it has a general effect of tilting the set of equilibria towards those featuring cooperative norms.

The second effect of market competition is a dynamic selection effect. This is the effect most usually emphasized in the literature on CGS. Competition selects amongst the array of equilibrium norms displayed those firms that converge on the best ones. Firms featuring cooperative norms should be able to out-compete those unable to sustain cooperation and hence producing low quality output. The more intense the competition the greater the selective pressure, implying that the better firms expand more quickly and the weaker firms decline and shut down more rapidly. This selective effect has been argued to operate in a number of ways: via firm decline and exit, via migration from less successful to more successful firms that are expanding and hiring, and via mimicry (organizations selectively imitating and copying the behaviors or norms that prevail in their more successful competitors).

The final link in our explanation is how cooperative norms are related to affirmative answers to the generalized trust question. This question has been widely studied, and the extensive literature on it suggests a number of factors can affect its answers. However, a factor that is consistently important is an individuals’ beliefs about the likely trustworthiness of anonymous others, and it is this component that would be moved by the forces of competition. Trustworthiness beliefs will increase under intense competition due to both the static equilibrium and the dynamic selection effects. In answering the trust question, and reflecting on their beliefs about the trustworthiness of others, subjects are informed by their life experiences, a major one of which is the performance of people around them in the workplace. Working in competitive sectors, subjects experience more cooperative behavior and accordingly respond more positively as to whether the imagined anonymous other can be trusted.

A limitation of the observational data is that it is impossible to interrogate this correlation between trust answers and competition further. In addition to beliefs about trustworthiness, answers to the generalized trust question have been shown to be affected by an individual’s own preferences for behaving in a trustworthy manner (as they introspect about how they would act in a situation of social risk), their risk preferences, and their aversion to betrayal. Of course, if they are conditionally cooperative norm followers then an introspective assessment of how they

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19 See Richerson et al (2016) for a discussion of the myriad ways in which selection across firms can occur, and references therein documenting norm transferal. A side note is that the two forces of mimicry and migration tend to reduce cross firm variation and thus act to undermine the selective effects of competition. Against this, Richerson and Boyd (1998) argue that dominant norm compliance makes new immigrants comply with the norm of their new group, and selection (in a cultural sense) is still occurring provided that unsuccessful groups end up copying the norms of successful ones.

20 See Footnote 9.
would behave in a situation of social risk will be related to previous experiences and beliefs. Nonetheless, with observational data alone, we cannot test which specific aspects of subjects’ experiences affect the generalized trust answers, nor are we able to definitively establish a causal link. Despite the strength of the panel results, it remains at least a theoretical possibility that reverse causation or omitted variables are driving the observed correlation.

This is a marked advantage of the laboratory setting. There we will both be able to more certainly assert a causal relationship, and to explore the reasons for it in more detail.

*A Laboratory Experiment*

Our aim is to 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, and designing the treatments so that equilibria vary under competitive and non-competitive arms, we can trace the effects on subjects directly: whether subjects in the competitive treatment are induced to increase their generalized trust. This will allow us to explore the consequences of the *static equilibrium effect* of competition directly. A limitation of the laboratory however is that the setting is artificial (not an actual workplace) and short term (about one hour), so it is not possible for us to explore differential exit or success of groups based on their norms. One major channel of the *dynamic selection effect* of CGS is thus not present in our experiments. However, by allowing for multiple groups to be formed across rounds of the experiment we are able to explore dynamic selection that would occur via selective imitation of successful groups.

We undertook our 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 canonical public goods game (PGG) — can have their contributions to the game substantially increased by putting them in group competitive settings. But do the effects of increasing competition also induce higher levels of trust? And if so, is this happening due to effects that could be attributable to CGS as we have argued for the observational data? 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 PGG. Twenty 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 (two-member) group equally. 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 0.75$x$ and therefore being a net cost to the subject. If a participant's objective is to maximize monetary reward, the 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.

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21 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.
Individuals were matched anonymously into groups, asked to make a contribution choice, and told the outcome and contribution of the other player they were paired with at the end of the round. In the next round, they were re-matched into another group and played again. 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 session.

Before playing, subjects filled out a questionnaire regarding their particulars — education, occupation (if they had one, most were students), age, and gender. 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 the PGG, the figure displays a declining pattern of contributions. Individuals start out contributing at a median level around 2 Euros of their endowment — and this gradually 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. This declining pattern is not our focus here so we do not address it further.

The remaining subjects were placed in a “competitive” treatment. Here, the amount they received from the collective pool depended not only on the joint individual contribution and their partner’s, but also on the size of their joint contribution relative to that of a randomly allocated comparator group. If, and only if, their joint contribution equaled or exceeded that of their comparator group, did they receive their share of the collective account. The collective account was computed exactly as in the control group; total contributions were multiplied by 1.5 and shared equally by both members.

Contributions under the competitive treatment are clearly less certain to create benefits, both for the group and for any individual contributing, since payoffs from contribution are now conditional on “winning” against the comparator group. All players contributing zero remains a Nash equilibrium of this game. But this competitive treatment also gives rise to equilibria with contribution levels that far exceed the standard public goods game of the control. 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

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22 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.

23 See the Supplementary Materials for details of the game and the full set of experimental instructions.
another equilibrium. The same reasoning can be easily shown to support any other symmetric contributions as Nash equilibria of this game.

As in the control treatment, subjects were re-matched anonymously into new groups after each round. The pair was also re-matched (again anonymously) with a different randomly allocated comparator group, 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 their comparator group in the previous round too, before making their current round decision. The same pre- and post-questionnaires were administered as in the control (standard PGG), so that generalized trust levels were also measured after participation.

As the solid 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.24 Median contributions jump in Round 1 of the experiment to being more than twice as high in the competitive treatment than in the control.

The level being higher in round 1, before subjects have any experience of play, is consistent with what we have termed the static equilibrium effect; subjects inferring the possibility of Nash equilibria at higher levels of contribution in the competitive setting. Due to the complexity of computing equilibria (and that we provided no instructions on how to compute them), it is possible that many of did not understood the equilibrium structure of the game.25 Hence, it is plausible that this first round difference is due to simply putting subjects in to the competitive setting, and is therefore not deliberative. It has been argued that group competitive settings can cue individual level group cooperative set of responses as a type of priming effect.26

But a competitive prime cannot explain the changes in play observed as the experiment progresses. Figure 4 shows that median contributions in the competitive treatment start below 5 Euros, tracking up dramatically over the first few periods, from there they remain fluctuating around 7 Euros. The pattern of decline exhibited in the standard (control) PGG does not appear.

Recall that this is not a repeated game played with the same group. Groups are created afresh across each round. Moreover the median obscures considerable heterogeneity across individuals in how this progression happens. In particular, some subjects exhibit a positive trend in their contributions as the experiment progresses, we denote these “increasers”, and their median values for contributions across rounds are displayed with triangles of Figure 5. Others exhibit a declining trend (denoted “decliners”), denoted with X marks. The Supplementary Materials further discusses how these groups are defined. Decliners predominate in the standard PGG of

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24 The mean contribution 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<0.001, n=180. The specification controls for age and gender.

25 Most subjects were non-economics students not trained in game theory, and “major of study” does not correlate with play in the game.

26 There is evidence of this in the laboratory, see Henrich (2016, Ch. 12) and from conflict situations in the field that can be long term in effects, see Bauer et al. (2014). Henrich (2016, Ch 12) argues that non-cognitive responses to cues of competition may have themselves evolved in humans via selection acting on both genes and culture simultaneously, so that this is part of gene/culture co-evolution. Namely, that human non-deliberative cognition responds directly to cues of intergroup competition with more cooperation because it yielded selective advantages.
the control (63% of subjects are decliners). In contrast, increasers are the largest group in the competitive sessions (45%, compared to 40% being decliners). These subjects start out similarly to the decliners in the competitive treatment, but significantly increase their contributions across the competitive rounds; strikingly converging to a median of full contribution by the end.

To understand why subjects who start similarly can vary in their progression of contributions through the game we explore the effects of the random matching of individuals (both as partners and competitors) as the experiment progresses. One of the channels of dynamic selection in theories of CGS is that groups selectively imitate the norms and/or practices of successful groups. Such mimicry can lead to the diffusion of beneficial norms even in the absence of selection directly based on fitness (i.e., via conflict and exit, which is not present here). Consistent with this, we conjecture that subjects might be induced into becoming “increasers” when they experience higher levels of partner (and competitor) contributions previously. The Supplementary Materials discusses how, by isolating variation arising from the random allocation of the ordering of partners, we can explore whether individuals who experienced higher levels of partner (and competitor) contributions also reacted by increasing their own contributions in newly formed groups. This is indeed the case. The average of lagged partner (and competitor) contributions positively predicts a subject’s own contribution in the next round.

The effect of previous partner contributions can occur for multiple reasons, such as misplaced reciprocity or “warm glow.” However, individuals increase their contributions when they experience higher contributions from competitors as well. Such a pattern cannot be due to a competitive prime because it happens within the competitive treatments (not a comparison between competitive and non-competitive treatments). It is also not evidence of reciprocal behavior (as groups are drawn afresh each round), nor a “warm glow” (as it increases the likelihood of losing the competition). It is, however, consistent with one dynamic selection channel of CGS: mimicry of the actions or norms in successful groups leading to diffusion of those norms in to the broader population. Individuals form new groups and contribute more heavily when experiencing competing groups able to obtain higher contributions in the past. Moreover, this is not a general feature of the competitive treatment: subjects experiencing competitors who contributed low amounts, on average, tended to lower their own contributions in newly formed groups subsequently, so it is not competition per se that does it.

We next check whether the association between competition and trust found in the three survey data sets we analyzed also holds in our experiment. It does. 51% of subjects in the noncompetitive control group answer the trust questions affirmatively (5 or higher on the provided 0-10 scale), this is represented by the red square in Figure 4. Subjects in the competitive session are 14.6 p.p. more likely to answer the trust question positively (the blue circle in Figure 4). The difference is statistically significant (p-value = 0.011), and robust to multiple specifications and methods of inference, as discussed in the Supplementary Materials.

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27 28% of subjects in the control group are increasers and 9% always contribute zero. 8% of subjects in competitive sessions always contribute zero, while 7% always contribute the maximum amount.
However, recall that there was variation amongst subjects in the competitive treatment that affected their own pattern of contributions across rounds, with the increasers seeming to be induced to higher contributions by being matched with relatively high contributors early on. It turns out that these same individuals are the ones who are also induced into affirmative answers to the generalized trust question. Formally, one can predict whether an individual will answer the generalized trust question affirmatively by knowing whether (in the random allocation of subjects across rounds) this individual was matched (either as a partner, or as a competitor) with relatively high or relatively low contributing subjects. So, to reiterate, it is not only the case that individuals put into the competitive treatment and contributing more straight away (i.e., in round 1) report higher levels of trust. It is also the case that individuals who experience high levels of competitor and partner contributions through the random matching of the experiment both increase their own contributions in new groups that they form in subsequent rounds, and are significantly more likely to affirmatively answer the generalized trust question when the experiment ends. As indicated by the breakdown of trust answers by differing types depicted at the right part of Figure 5. The blue triangle, corresponding to the increasers in the competitive treatment, drives the difference in averages between competitive treatment and the PGG of the control.

If experiencing high contributing subjects in the experiment raised trust levels via a “warm glow” from higher payoffs, then we would expect that this would only occur when matched with high contributing partners, not with high contributing competitors (the latter lowers one’s payoffs). However, if being matched with high contributors informs subjects about what successful groups tend to do and which cooperative norms may be present within the subject pool, then it should not matter whether one experiences high contributions via partners or via competitors in previous rounds. Since we find effect of experiencing higher contribution from both partners and competitors on trust, this suggests that increased trust levels induced by the competitive treatment are not driven by warm glow experience. Instead, the CGS based explanation is that subjects who experience relatively high contributions by both partners and competitors react to this in two ways. First, they increase their own contributions in subsequent rounds, even though they will not be matched with the same partner again. This could be because they believe that future partners will also contribute more and (consistently) because they believe that success will come from such higher contributions. Second, it also changes their attitude towards the “anonymous other” as reflected in their response to the generalized trust question. They are more likely to think others can be trusted, as they themselves are also induced into acting in ways that are more trustworthy (by contributing more). Subjects seem to extrapolate from the trustworthiness of their partners, and even their competitors in the experiment, to the wider context imagined by the generalized trust setting.

Conclusion

28 We explain fully the details of the instrumental variables strategy that allows 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.

29 This type of extrapolation from the laboratory to the generalized trust question context is not without precedent (Paxton and Glanville, 2015).
Increased competition across firms exposes subjects to increased group beneficial behavior on the part of their co-workers, and increases their own such behavior. In competitive markets, firms unable to elicit such cooperative behavior are likely to be out-competed by firms that are more successful in doing so, leading to the proliferation of firms exhibiting cooperation. Workers in such settings experience, and themselves internalize, more cooperative norms. They then report more positive answers to the generalized trust question, which explains the cross-section and panel correlations we have reported here.

Competition across groups in an otherwise standard public goods game conducted in the laboratory induces more group beneficial contributions from individuals within the groups. This happens immediately upon being put in this environment. Additionally, for a subset of individuals who by chance are matched with more generous partners (and competitors) there is a progressively induced increase in their own group beneficial contributions. Subjects experiencing these cooperative contributions (either via their anonymous partners or competitors) are more likely to affirmatively answer the generalized trust question, which imagines a setting beyond the laboratory context.

Our competitively treated subjects in the laboratory do seem to have raised their beliefs regarding the possibility of a cooperative interaction, at least in the laboratory (relative to the non-competitive treatment). Perhaps beliefs beyond that, as indicated by their responses to the generalized trust question, have also been similarly altered. However, we acknowledge that permanent effects flowing from such limited laboratory exposure seem implausible. Nonetheless, the laboratory does demonstrate that cross group competition can alter actions, and seemingly beliefs of subjects, in a way that is consistent with cultural group selection. If exposure to such competition is repeated, for example as would occur in longer-term interactions arising from the workplace, then this evidence suggests that workplaces could be important conduits for such cooperative prosocial behaviors in general.

References


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Figure 1: Americans that Work in More Competitive Industries are More Likely to Trust
Note: This figure presents a binned scatter plot of worker’s trust vs. the competition in her industry of employment. The sample consists of 612 employed respondents of the 2004 GSS Workplace Module. The plot is constructed by dividing competition into 25 bins with equal number of observations each and plotting mean trust indicator vs. mean competition within each bin. The best fit line (and reported slope coefficient) is estimated by OLS using the original (unbinned) data. Both the plot and linear fit partial out the determinants of trust control variables (see text for information).
**Figure 2: Banking Deregulation in US States Raised Firm Competition and Trust**

Note: This figure plots an event study graph for banking deregulation: the effect of each individual year before and after banking deregulation (normalized to year zero) on the probability of answering positively to the trust question. The sample consists of 17,455 respondents to the GSS in the 1973-1994 period. The estimating equations include state fixed effects, year effects, individual state trends, and individual controls. The effects for firm entry and closures per capita are obtained from Kerr and Nanda (2009). See text for further information.
Note: This figure presents a binned scatter plot of worker’s change in trust vs. change in the competition in her industry of employment. The sample consists of 9103 employed respondents of the German SOEP in the years 2003, 2008, and 2013. Changes are relative to trust and competition five years before. The blue dots are based on 2309 respondents that change industry of employment between survey years. They are calculated by dividing competition change in 25 bins with equal number of observations each and plotting mean change in the trust indicator vs. mean change in competition within each bin. The best fit line (and reported slope coefficient) is estimated by OLS using the original (unbinned) data. The red X is based on 6794 respondents that do not change industry of employment (and hence do not experience change in competition). See text for further information.
Figure 4: Introducing Competition in Public Good Laboratory Game Increases Contributions and Propensity to Trust

Note: The blue solid (dashed red) line plots the median contribution by participants in the competitive (non-competitive) experimental sessions across the 19 rounds of the experiment. The red square plots the share of participants in the noncompetitive sessions that reported positive trust. The blue dot is constructed by adding the treatment effect of being in in the competitive session from a regression with controls for age and gender. The whiskers show the 95% confidence interval. Sample consists of 220 experimental participants (100 in five competitive sessions, 120 in noncompetitive sessions). See text for further information.
Figure 5: Experimental Subjects that Increase their Contributions throughout the Competitive Experimental Sessions Report Higher Trust

Note: The blue (red) connected triangles plot the median contributions by “increaser” participants in the competitive (non-competitive) experimental sessions across the 19 rounds of the experiment. Increasers are defined as those who, on average, increase (or keep constant) their contributions as the experiment progress (see main text and Supplementary Materials for further details). The blue (red) connected X-marks plots the median contribution by “decliner” participants in the competitive (non-competitive) experimental section across the 19 rounds of the experiment. Decliners are defined as those who, on average, decrease their contributions as the experiment progress (see main text and Supplementary Materials for further details). The red X plots the share of decliners in the noncompetitive sessions that reported positive trust. The red triangle is constructed by adding the effect of being an increaser in a noncompetitive session and the blue triangle (X-mark) is constructed by adding the effect of being an increaser (decliner) in a competitive session (from a regression with controls for age and gender). Sample consists of 220 experimental participants (100 in five competitive sessions, 120 in noncompetitive sessions). See text for further information.