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 from survey data and from laboratory treatment of experimental subjects that is consistent with a set of theories based on group level selection of cultural norms favoring pro-sociality. In particular, increases in competition increase trust levels of individuals who: i) work in firms facing more competition, ii) live in states where competition increases, iii) move to more competitive industries and iv) are placed into groups facing higher competition in a laboratory experiment. The findings provide support for cultural group selection as a contributor to human pro-sociality.

Short title (49 characters): Testing Cultural Group Selection in the Workplace

Teaser sentence (118 characters): As predicted by cultural group selection, increases in firm-level competition raise the generalized trust of workers.

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. (1-2) extensively discuss the puzzle of human pro-sociality.

The variety of theories proposed to explain these phenomena are typically hard to assess empirically. Examples include reciprocal altruism (3), sexual selection (4), and the mismatch hypothesis (5). A reason is that predictions often 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 we argue will allow us to scrutinize contemporary data for evidence in accord with their predictions. The evidence we will present is novel and as (1) argue, rare, in that it is quantitative. Though another quantitative study supporting CGS has been argued using observed group extinction rates amongst tribal groups in Papua New Guinea by (6).

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; see (7) for discussion of the evidence supporting humans as evolved social learners. 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, allowing human societies to adapt norms suited to prevailing conditions. But "selection" occurred as societies with the fitness enhancing norm/institution combinations proliferated via: defeating less successful groups in direct conflict or taking their resources; being imitated by their less successful competitors; or through selective migration and internalization of norms upon migration; see (8) for a more detailed elaboration of these selective forces. 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.

While the narrative in which this explanation for human pro-sociality is couched is in terms of our pre-historic past, the scope of this paper is to test the predictions of CGS in contemporary settings. 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 modern contexts.

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 (9). 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.

Materials and Methods

Perhaps the 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, for example (10) present evidence linking the social identity of employees to the performance of firms. 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.

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" (11).

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 (12). (13) summarizes a large literature on this as showing that survey measures of trust are informative with regards to the cooperativeness of others, but there are sometimes discordant findings between play in incentivized games and answers to trust questions (9, 12) and studies showing that other factors are also at play in answers to the trust question (14-16). Third, individuals tend to respond to trustworthiness experiences by increasing their own trustworthiness, which is consistent with individuals being conditional cooperators, evidence of which is amply demonstrated by (17) in public goods experiments, and summarized over a more comprehensive set of studies by (18). Conditional cooperators are then 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.

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 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.

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. Using competition measures based on the shares of the four, eight, and twenty largest

firms yield similar results. The use of these concentration ratios are common in economic analysis of industry structure and market power. Further information on the data is provided in the Supplementary Materials.

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.

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).

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.

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 (19-20) 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 (20). 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. As in the rest of this paper we use a binary indicator of trust, but results are robust to alternatives, as discussed in the supplemental materials. Our dataset contains a total of 17,455 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 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, meaning that they totally or slightly agree that “on the whole, one can trust people”.

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; see the Supplementary Materials for further details. 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 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.

Discussion: CGS Explanation of Observational Data

There is considerable experimental evidence, referenced earlier, 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.

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. Such effects on individuals are likely to be small in large firms where any specific worker's actions are unlikely to be pivotal. However, it is possible that employees over-estimate the impact of their actions, or instinctively respond to competition with more prosocial attitudes, even in large teams (21).

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 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). Precisely how mimicry allows selection to still occur at the group level is discussed in (22).

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 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.

Materials and Methods: 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. Previous experimental work (23-27) has already shown 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 $0.75x$ 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.

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. See the Supplementary Materials for details of the game and the full set of experimental instructions.

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.

Results: Laboratory Experiment

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 (18, 28). 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 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. 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. There is also a markedly different experiment progression effect; median contributions jump in Round 1 of the experiment to being more than twice as high in the competitive treatment than in the control, and do not exhibit the customary decay.

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 did not understood the equilibrium structure of the game. 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 (8).

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 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.

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. The instrumental variables strategy that underlies this conclusion is detailed in the Supplementary Materials. This is 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). As found in previous laboratory studies of the generalized trust question, like (29), 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

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.

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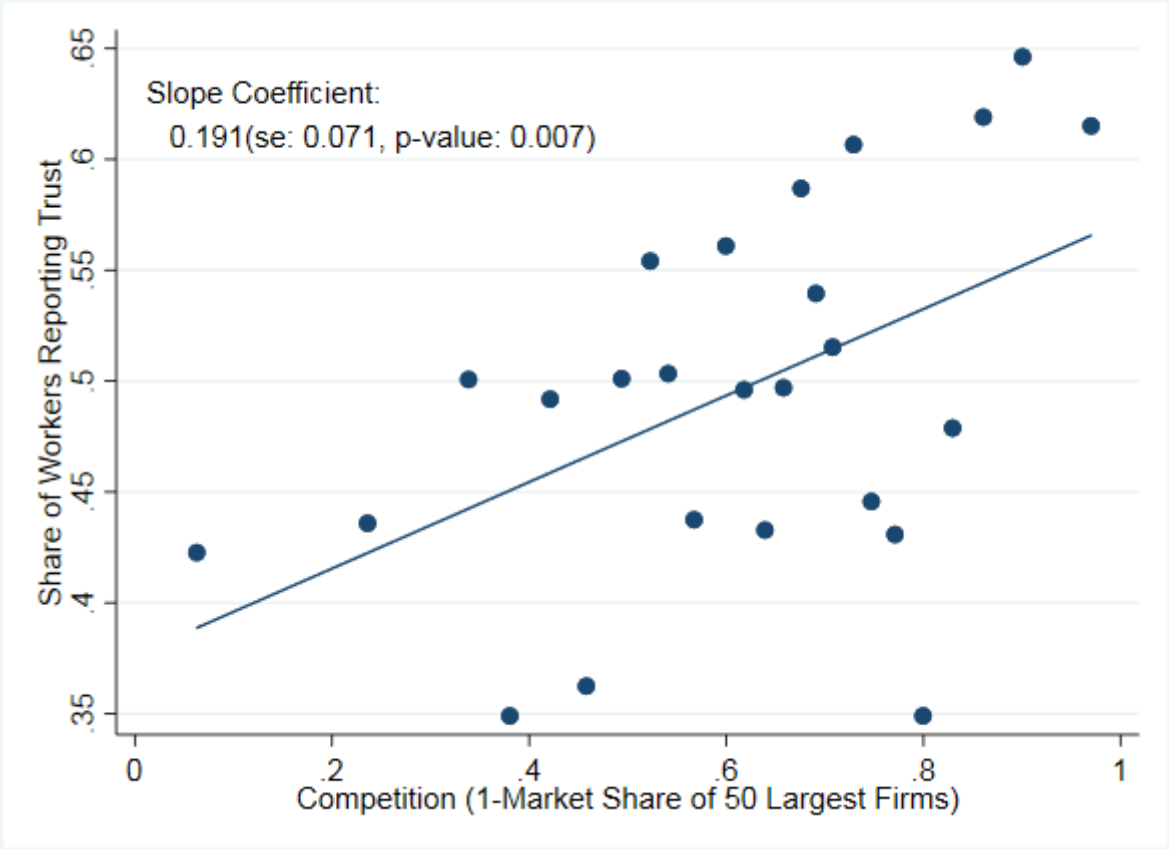
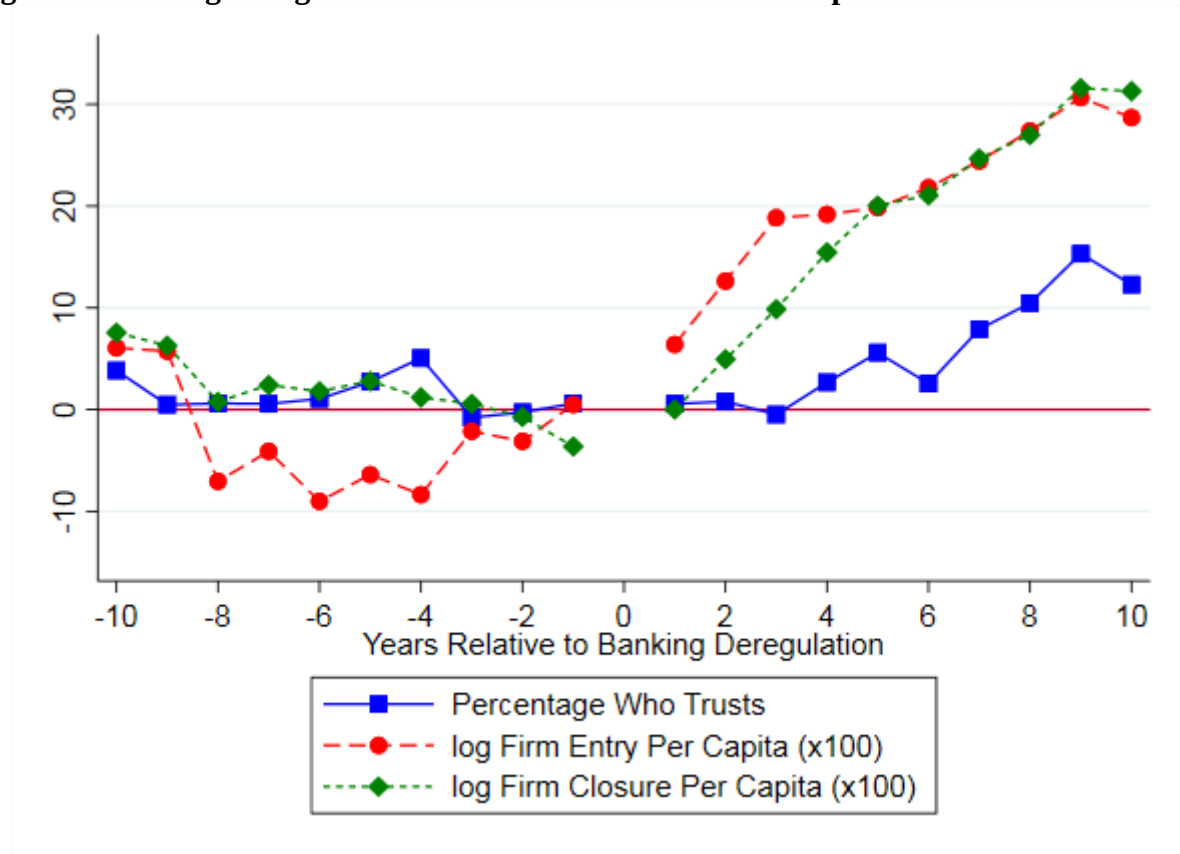
Figure 1: Americans that Work in More Competitive Industries are More Likely to Trust



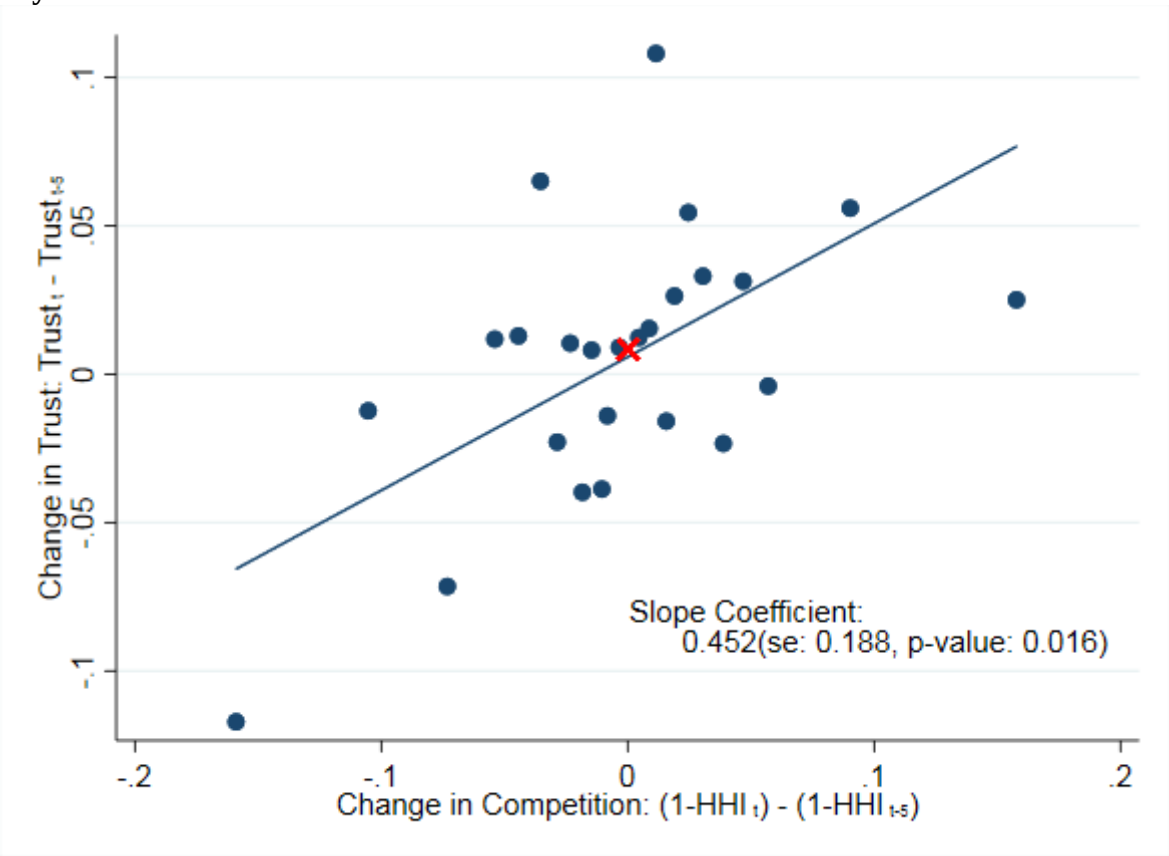
Note: This figure presents a binned scatter plot of worker's trust vs. the competition in her industry of employment. The sample consists of 612 employed respondents of the 2004 GSS Workplace Module. The plot is constructed by dividing competition in to 25 bins with equal number of observations each 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



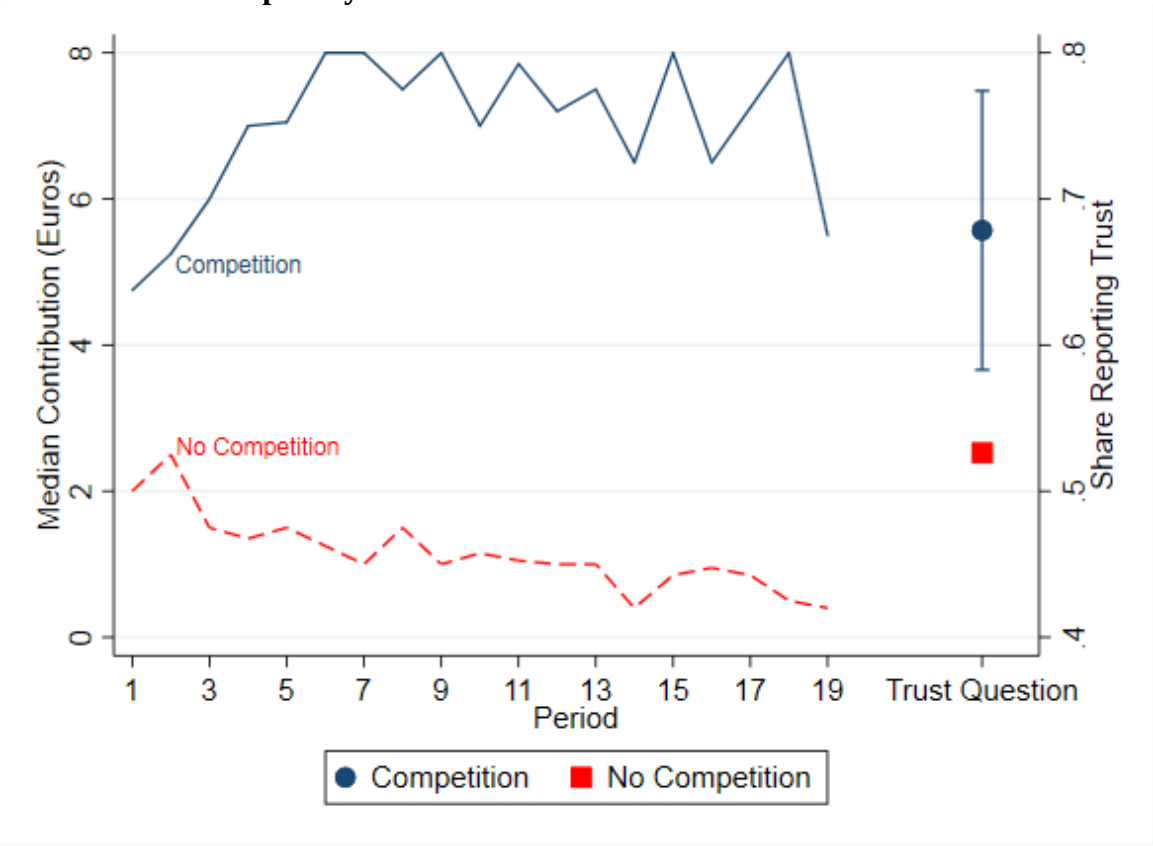
Note: This figure plots an event study graph for banking deregulation: the effect of each individual year before and after banking deregulation (normalized to year zero) on the probability of answering positively to the trust question. The sample consists of 17,455 respondents to the GSS in the 1973-1994 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.

Figure 3: German Workers that Move to More Competitive Industries Become More Likely to Trust



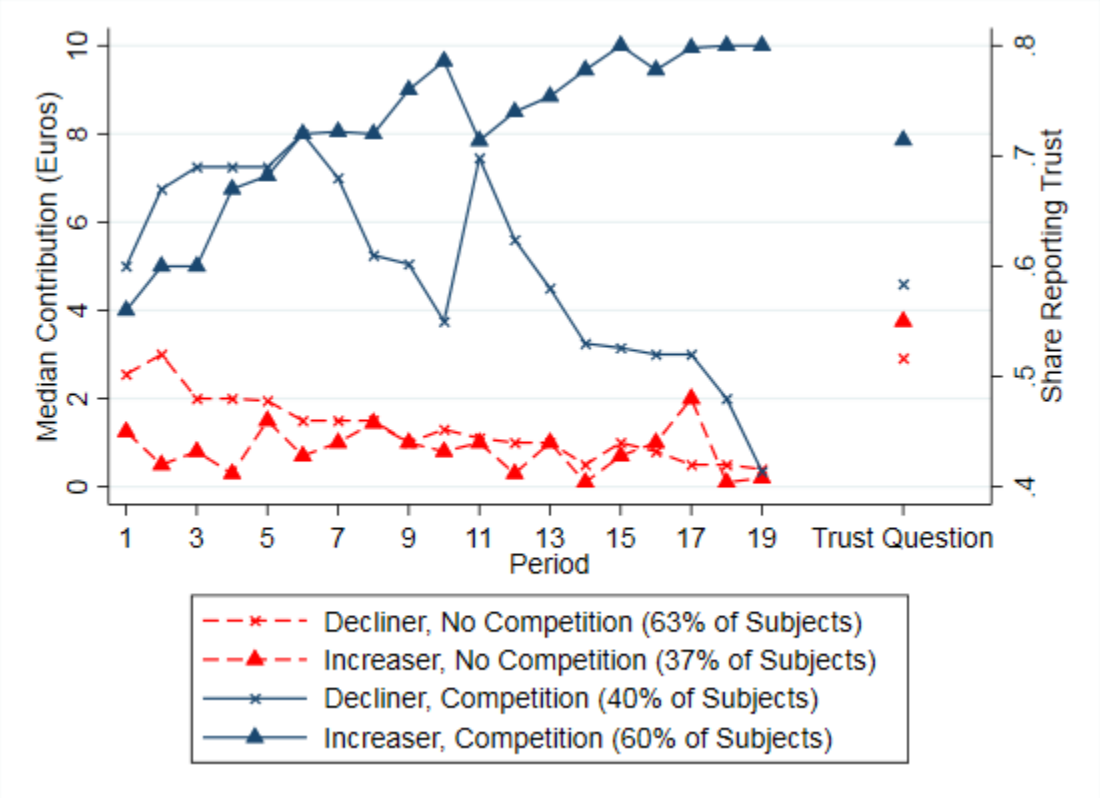
Note: This figure presents a binned scatter plot of worker's change in trust vs. change in the competition in her industry of employment. The sample consists of 9103 employed respondents of the German SOEP in the years 2003, 2008, and 2013. Changes are relative to trust and competition five years before. 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.

Supplementary Materials for "The Origins of Human Pro-Sociality: Cultural Group Selection In The Workplace and the Laboratory"

Patrick Francois
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Tanguy van Ypersele

Contents

1 Supplementary Text: Cross-sectional evidence in the US

This section provides further information regarding the evidence of a positive association between competitiveness of industry of employment and individual trust. Our hypothesis is that competition increases trust for individuals who are otherwise identical in their propensity to trust ex ante. However, there are many previously documented determinants of individual trust. It is thus imperative to control for these. 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.

Data. To address these concerns, we use the 2004 wave of the General Social Survey (GSS), which included a detailed extended workplace module. The GSS is a survey representative of the American population that contains information on respondents' industry of employment, which we can match to industrial level of competition that we obtain from the US Census of Firms that was administered in 2002. The survey reports the percentage of total sales covered by the k largest firms by North American Industrial Classification System (NAICS) industries (with $k = 4, 8, 20, 50$).
The US Census of Firms is administered every five years. There are shortcomings in measuring competition via these concentration measures. However, this measure has the advantage that it

covers multiple industries. For example, the Census only reports the Hirschman-Herfindahl Index (HHI) of concentration for manufacturing industries. The correlation between the HHI and our concentration measure is high (0.85). As a measure of competition this is not perfect, as factors other than the competitiveness of a industry 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.

Not all GSS respondents – the unemployed and retired for example – will have information on industry of work. When this information is available, the GSS reports industry of employment using the 1980 Census (3-digit) industry codes. We converted these to 1990 census code measures and used 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, which is also determined by the availability of additional controls described below, includes 612 individuals in 107 different industries.

The 2004 wave of the GSS asks the canonical trust question (i.e., "*Do you think that, on the whole, people can be trusted, or that you can't be too careful in dealing with people?*") and allows four possible answers (with response rates in parentheses): "*always trusted*" (3.8%), "*usually trusted*" (44.4%), "*usually not trusted*" (42.1%) and "*always not trusted*" (9.7%). Our outcome is a binary trust indicator, taking value equal to one if the answer is "always" or "usually" trusted.

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. We include workplace size controls later. 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. Table S1 provides the mean and standard deviation for our variables in the sample of 612 respondents.

Our measure of competition equals one minus the share of sales for the largest 50 firms (so larger values indicate more competition). We also have computed similar measures for all other share of sales available (e.g., four or eight largest firms) and obtained similar results, which are available upon request.

The average industry in our sample has a competition measure of 60.5%. An industry corresponding approximately to the average is NAICS # 42314 "Used Motor Vehicle Parts Merchant Wholesalers". An example of a particularly competitive industry is NAICS # 44112 "Used Car Dealers", with a competition measure equal to 87%. A particularly uncompetitive industry is NAICS # 31132 "Chocolate and Confectionery Manufacturing from Cacao Beans", with a competition measure equal to 1.2%. In general, most service industries are more competitive than both manufacturing and retail.

Estimation Procedure. We run OLS regressions of the following form for an individual i working in industry j:

$$trust_{ij} = \beta_0 + \beta_1 competition_j + \gamma Z_{ij} + e_{ij} \quad (1)$$

where *trust* is a binary trust indicator and *competition* is the measure described above. Z_{ij} is a vector of control 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 industries (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 competition and hence does not bias the results. Standard errors are clustered at the industry level.

Results. Table S3 reports the results from estimating equation (1). The specification on column (1), only includes include controls for age, income, gender, race, ethnicity, marital status, and religion dummies, and city size, indicate that increasing our measure of competition by 10 p.p. (the largest 50 firms losing 10% of market share) is associated with a 1.91 p.p. increase in the probability a worker in that sector reports it trusts. In variance normalized terms, this implies that a one standard deviation increase in competition leads to a 4.8 p.p. increase in trust. This result is robust to excluding different subsets (or all) of the individual controls. The coefficient on the many control variables included are omitted from Table S3. The only exception is (year of) education, which we report on the table as a comparison to the coefficient on competition.

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 are 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. Table S2 lists these variables and provides their mean and standard deviation.

We discuss these potential confounding variables individually below, but before we note that the general picture that emerges is that the association between competition and 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. The procedure described in (30) implies that selection on unobservables would need to be at least 40 times greater than on observables to explain away our results.

Job Security. (31-32) 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. Since we do not 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 one 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 competition is unaltered by the addition of this variable. As with all other workplace controls, the coefficient (and standard error) of the variable is reported Table S3.

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, which does not attenuate the association between competition on trust.

Congeniality of the Workplace. The forces of competition may induce firms to provide more congenial workplaces – which are costly – in order to retain the best employees (33). 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 our main result is picking up, we exploit a rich set of workplace related questions. 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 competition 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 remains close to 0.2 and significant at 1%, 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 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. 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 largely unchanged.

Optimism. It is possible that competitive sectors are select 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 (6). 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 as one, and "disagree" and "strongly disagree" coded as zero. It should be noted that the sample size now drops to 530, 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 competition 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. An additional test of our hypothesis is whether individuals with longer experience in the competitive sectors accumulated more trust than those with shorter experience. The GSS does not follow individuals through time, so we use a "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 S4. Column (1) reports results obtained when we add this interaction term and omit experience directly in the regressions; as it is collinear with age and education.

Adding the interaction term makes the competition variable on their own insignificantly different

from zero. However, the interaction term itself is positive and significant for both competition variables across all specifications, at the 10% level. Table S4 replicates the regressions reported on Table S3 in the same order but now including the experience interaction.

The zero finding on direct inclusion of competition is evidence against competitive sectors selecting workers with *ex-ante* propensity to trust. 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

Background on bank deregulation. This subsection provides a brief overview on bank deregulation in the US, the reader is directed to (19, 20) 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. All states did not allow banks from other states to operate in their territory.

Starting in the 1980s, several technological innovations such as automatic teller machines (ATMs), phone and mail banking, and credit-scoring systems reduced the cost of using banking services from a distant branch. These changes lead states to lift these restrictions from the and allow banks from out-of-state to operate in their borders. 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 banking deregulation at different times — (20) list the exact date of deregulation for each state. (19, 20) 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, (19, 20) and our own analysis show that deregulation can be seen as a positive *exogenous* shock 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. Additionally, (20) show that banking deregulation also increased firm *closures*, which is also consistent with increased access to credit leading to more competition in the non-financial sector.

The literature on banking deregulation mentioned above usually points out the distinction between allowing out-of-state banks to operate (*interstate* deregulation) and the removal of barriers to operating across areas within the same state (*intrastate* deregulation). This paper focuses only on the interstate deregulation case (which we refer as banking deregulation). We do so since Kerr and Nanda (2009) show that firm entry and closure was affected by interstate deregulation (but

not intrastate deregulation).

Data. The main data source for the analysis based on banking deregulation are several waves of the General Social Survey (GSS), which was first implemented in 1972, and at least every other year since then. The survey is representative of the American population. 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 every year (or at all) in the GSS, and we use an unbalanced panel of 41 states for the period 1973-1994. The start date (1973) of the sample is defined by the availability of information on state of interview, and the final point (1994) is by the fact that in this year federal legislation (the Riegle-Neal Act) eliminated interstate banking restrictions nationwide. 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 S5 presents the summary statistics of our sample.

Effect on firm entry and closures. The effect of the banking deregulation on firm entry is studied by (19, 20). The effect on firm closure is studied only by (20). We hence do not revisit this issue in this particular paper and (20) the effect of banking deregulation. In particular, we use the estimates by (20), given their use longitudinal firm-level data by the US Census Bureau and their focus on both firm entry and closure. Moreover, (20) use data on firm entry and closure that is not public available and requires permission from the US Census Bureau. (19) use data on firm entry provided by the Bradstreet and Dun corporation.

Construction of Figure 2 in the Main Text. Figure 2 presents the results in a graphical event-study graph, tracing out the year-by-year relationship between the timing of the reforms, trust levels, and competition measures. The plot on trust levels is done by estimating the following equation:

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

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 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} (a 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). The specification also

includes state and year fixed effects (γ_s and δ_t) and state-specific linear 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.

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_j for state-level counts of firm entry and firm closure that were calculated by (20), which estimate similar regressions using firm entry and closure (based on restricted-access micro-data of the Longitudinal Business Database of the US Census Bureau).

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 provides evidence that the timing of the reforms is not correlated with previous trends in trust 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 (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 below.

Estimation. We estimate the following equation:

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

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 banking deregulation and zero otherwise. The variables $yearspost_{st}$ 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).

The estimation also controls for a vector of individual level controls (X_{ist}) 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 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 Figure 2 suggest that trust grows over time after a reform 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). We obtain similar results when aggregating observations to state-year averages.

Table S6 presents the results of the estimation of equation (3). Column (1) reports the baseline specification. In consonance with Figure 2, the estimates indicate that banking deregulation leads to a continuous increase in trust level after its implementation. The estimates imply that a state that deregulates would experience over 1.3 percentage point increase in the share of its population reporting that they trust every year after the reform, an effect that is statistically significant at the 5% level. The coefficient on the post-reform dummy is small and statistically insignificant, as expected since Figures 2 does not show a discrete "jump" after reforms.

Income as a possible confounding factor. A possible alternative explanation for the previously discussed results is that banking deregulation affected incomes, which in turn affected trust levels. This subsection provides two pieces of evidence suggesting this is unlikely to be the case. First, controlling for income when estimating equation (3) does not substantially affect the results. Second, we provide evidence that income *growth* does not lead with higher trust. While previous papers explored the cross-sectional relationship between income levels (at individual, regional, or national level), we estimate the effect of income *growth* on trust using growth induced by oil shocks and find evidence suggesting income growth does not increase trust.

First, columns (2) and (3) of Table S6 reports the effects of equation (3) adding controls for income. The GSS contains a respondent-level continuous measure of income variable constructed from interpolations of categorical answers (that 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. Given the imperfections on the GSS income measure, we also use a state-level measure of income obtained from a separate source. Column (3) adds the (log) mean annual individual income of a state. As an additional test of whether economic conditions are confounding the results, column (4) adds the employment-population ratio. Finally, column (5) investigates if changes in the distribution of income drive the results by adding the Gini index of annual individual income as a control. The magnitude and significance of the effects is largely unaffected by adding these controls. The source of these state variables are multiple waves of Current Population Survey (CPS). The Gini index is only available from 1976 onwards, and hence the number of observations on column (5) is lower than in others.

Second, while a number of studies have documented that, at the individual level, income is positively correlated with trust (34-35) or that the *level of trust* is associated with higher income or faster income growth across countries (36). The relationship between income growth and changes in trust is not as well documented, and we are unaware of studies that attempt to estimate the impact of the former on the latter. In order to estimate the effect of changes on income on trust, we exploit time-series variation in global oil prices interacted with cross-sectional state oil reserves. (37) utilize a similar approach to estimate the income elasticity of health spending and discusses the literature on the effects of oil shocks.

In particular, we estimate the following regression:

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

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 "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. The oil price data is the "oilprice" variable

in the Federal Reserve Economic Data website. This is the same variable used in (37). 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.

Since (international) 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. (37) expand on this argument. Table S7 reports the results. Column (1) presents the results when Y is state-level income (measured by the log mean annual individual income from the CPS). The estimated coefficients indicate the effect of a $10,000 increase in the value of the state per capita reserves, which would increase incomes by around 3.9%, a result that is significant at levels well below 1% level. We do not take logs of the dependent variable since there are states with zero oil reserves. Note that that column (1) reports a regression estimated at the individual level, even though all the variables in it vary by state and year only. We do so to match the GSS regressions in columns (4)-(6). Since standard errors are clustered at the state-level, the regressions are similar to ones performed at the state-year level and using the number of GSS respondents in each state-year as weights. Column (2) 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 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. 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.

It is important to note that oil price shocks are documented to be *permanent,* and not transitory (37). Hence the lack of evidence for income growth increasing trust cannot be attributed to trust not being responsive to transitory income shocks. In conclusion, our results suggest that, in the case of US states, growth in income does not lead to higher trust.

Migration and changes in population characteristics. We also test if the results 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 respondents over time, 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 (3) for an indicator of college degree status, and test if banking de-regulation lead to more college educated people in a state. In principle, this could be done for all relevant covariates available in the data. To economize on space and increase our statistical power, we perform an omnibus placebo test in this spirit.

First we regress the trust indicator on the previously used vector of covariates (X_{its}), but nothing else. 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 (3) (without including the covariates used to construct predicted trust). This provides a test of the hypothesis that de-regulation increases trust by leading to changes in the (observable) covariates that are associated with trust. Column (6) of Table S6 presents the results. The estimated effect are small, negative, and not 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, we do observe a large host of covariates, which should also be associated with unobservables. Overall, the results

are consistent with banking de-regulation changing trust in the population, but hard to reconcile with it being driven by migration or other changes in socioeconomic or demographic variables.

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.*" Part of the SOEP data used in this paper was obtained through an agreement with DIW and via the PanelWhiz application (38).

The SOEP also provides the industry of work of its respondent, based on the 2-digit 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. The ORBIS database provides data on firms' financial and productive activities from balance sheets and income statements, and allows the construction of nationally representative statistics (39). We do not use the data from year prior to 2008 since there are issue with coverage of German firms for that period (39).

The HHI measure 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. We exclude those working in the public sector NACE code from the sample. As discussed in the main text, there is little variation in HHI across time for a given industry. We thus use the average of both years as a *time-invariant* measure of industry competitiveness in our regressions.

Our sample includes a total of 9103 observations from 6447 unique individuals: 5004 respondents are employed both in 2003 and 2008 and 4099 are employed in both 2008 and 2013. 2309 respondents change industries between survey waves. 65.1% of respondents have positive trust. More importantly to our empirical strategy, 71.1% report the same level of trust in two consecutive waves, while 14.1% switch from trusting to not trusting and 14.8% switch their trust response in the other direction.

There is a total of 50 different industries (NACE codes) in our sample. The average level of our competition measure is 0.97, with a standard deviation of 0.038. Overall, 99% of the industries in our sample have competition measures between 0.828 and 0.998. An example of a relatively competitive industry is "Manufacturing of Furniture," with a competition measure of 0.98. An example of a relatively non-competitive industry is "Manufacturing of Radio, Television, and Communication Equipment" with a competition measure of 0.86.

Estimation. We estimate the following equation for respondent i at survey year t:

$$\Delta trust_{it} = \alpha + \beta \Delta competition_{it} + \gamma nonmover_{it} + \gamma_t + \epsilon_{it} \tag{5}$$

where $\Delta trust_{it}$ is the change in trust between survey wave t and the previous wave (five years before). It is thus equal to -1, 0, or 1. $\Delta competition_{it}$ is change in the competition in industry of employment. It is thus equal to zero for those that do not move. $nonmover_{it}$ is a dummy indicating if respondent i change industry of employment between survey wave t and the previous wave.

Note that β is entirely identified from variation by those that changed industry: the same estimate would be obtained if β was estimated (5) from a sample of only those that change industries and dropping the *nonmover* variable. Standard errors are clustered at the individual level.

Results. Column (1) of Table S8 presents the results from estimating equation (5). The coefficient on $\Delta competition_{it}$ is the same reported on Figure 3 in the main text. The coefficient on $nonmover_{it}$ is small and statistically insignificant. It indicates that those that did not change industries experience the same average change in trust as those that move from one industry to another that has the same competition level. Column (2) adds a set of controls (age, education, gender, and marital status) interacted with time dummies. The estimated effect and its significance is virtually unchanged. This suggests that differential trends in trust that are associated with individual characteristics are not driving the results.

Column (3) restrict the sample to workers that worked in manufacturing for two consecutive periods, and column (4) does the same for the service sector. The estimated coefficient is of similar size in both cases. The statistical significance in column (3) is smaller, but that is likely driven by the smaller sample size (as the point estimate is similar to that in other columns). This exercise restricts the identifying variation to cases where respondents move from one manufacturing industry to another, or to one service sector to another. It implies the results are not driven by respondents that switch from services to manufacturing. Moreover, it is reassuring since if the results are driven by measurement error or an omitted variable that is correlated with changes in our competition measure, such measurement error or omitted variable must operate similar in very distinct economic sectors (manufacturing and services).

Testing for possible pre-existing trends. Figure S1 provides a falsification (or placebo) test in the spirit of testing for pre-existing trends: we test whether workers that move to more competitive industries 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 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 experienced 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.

The regression counterpart of Figure S1 is presented in column (5) of Table S8, which shows the slope is slightly negative and not statistically distinct from zero. One difference between the sample in column (5) and those in columns (1)-(4) is that the former is based on respondents that are observed (and employed) in the three years of our data (2003, 2008, and 2013). Column (6) reports the estimate of our baseline regression (the same as in column 1) but only using this particular sample. It shows that an effect of competition of similar magnitude as in column (1).

Income as possible confounding factor. 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 in the y-axis (instead of trust). It shows that workers that switch to more competitive firms are not more likely to experience higher income growth. The estimate slope in this regression is -0.104, with a standard error of 0.145 (p-value of 0.475). Hence, there is little evidence that those workers that move to more competitive sectors experience a higher growth in income that could confound our effects.

4 Supplementary Text: Laboratory Experiment

Further description of the experiment. The full set of instructions and survey questions provided to subjects is provided in the next section. It fully describes the exact rules of the experiment and how payoffs are calculated. Note that the instructions were truthful (subjects were

not deceived at any point in 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 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 can never be too careful when dealing with others? Zero means that one can never be too careful, ten means that one can trust people.*" The original French wording of the question is "*D'une manire gnrale, sur une chelle de 0 10, diriez-vous que l'on peut faire confiance la plupart des gens ou que l'on n'est jamais assez prudent quand on a faire aux autres? 0 signifie que l'on n'est jamais assez prudent, 10 signifie que l'on peut faire confiance aux gens.*"

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 (*Les revenus devraient tre plus galitaires*)

  versus

  More individual effort should be encouraged (*Il faudrait encourager davantage les efforts individuels*)

- The state should have a greater responsibility to ensure everyone's needs (*Ltat devrait avoir davantage la responsabilit dassurer chacun ses besoins*)

  versus

  Individuals should have a greater responsibility to support themselves (*Les individus devraient avoir davantage la responsabilit de subvenir leurs propres besoins*)

- In the long run, hard work often brings a better life (*A long terme, travailler dur apporte bien souvent une meilleure vie*)

  versus

  Working hard does not generally bring success - luck and contacts are more important (*Travailler dur napporte gnralement pas le succs cest plus une histoire de chance et de contacts*)

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. The original French wording is "*Tout fait daccord*," "*Plutt daccord*," "*Plutt pas daccord*," and "*Pas du tout daccord.*"

- One of my main goals in life is to make my parents proud of me (*Lun de mes buts principaux dans la vie a t de rendre mes parents fiers de moi*)

- I try to be myself rather than follow others (*Je cherche rester moi-mme plutt que de suivre les autres*)

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 indicators. 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).

Estimation. We estimate the following regression for experimental participant i that was in session s of the experiment:

$$y_{is} = \alpha + \beta compet_s + \gamma X_{is} + \epsilon_{is} \tag{6}$$

where y_{is} is the outcome of interest (e.g., whether the respondent trusts), $compet_s$ is a dummy indicating if session s involved competition (instead of the standard public goods game) and X_{is} is a vector of controls (age, and dummies for gender and whether the respondent is employed). We discuss how statistical inference is computed below

Effect of competition on trust. Panel A of Table S9 presents the effect of adding competition to the public good game, estimated given equation (6). 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.. 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 indicates 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 (40). 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 do not affect our 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.

Possible income effects. Panel A of Table S9 show the effects of competition on the amount

that the subjects received in the experiment. Recall that, for each respondent, the payoff of one randomly chosen period was used to award payment. Those in non-competitive sessions received, on average, 11.33 Euros in payment. Those in competitive sessions received less: just above 10 Euros. Not this values are in addition to the 15 Euros each subject received for simply participating in the experiment. Given that subjects in the competitive sessions received less money, and the difference was relatively small (just 1.23 Euros), it is unlikely that differences in income driven by the experiment can explain the effect on trust.

Placebo tests and covariate balance. Panel C provides a series of placebo tests based on the other questions on culture and values. Recall these are binary indicators, coded similarly to as the trust question (equal to one if answer is above control group median). 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).

Behavior of contributions throughout the game. Figure 4 in the main text shows how the median contribution varied throughout the competitive and noncompetitive sessions. Figure S5 provides a more detailed exposition. It shows, period by period, the distribution of contributions through a histogram. The contributions in the competitive and non-competitive sessions are plotted separately. A pattern that can be seen is that, in non-competitive sessions, the mass of subjects contributing zero increases over time, as the share of subjects contribution 50% of their endowment or more falls. In the competitive sessions, the mass of subject contributing their entire endowment increases over time, and a pattern of polarization occurs: contributing either zero or the entire endowment becomes, over time, the most common choices of subjects.

Learning about contributions throughout the experiment. This section provides evidence suggesting that individuals adapt their own contributions based on their observations what other subjects contribute the game. As discussed in the main text, this is consistent with individuals being conditional cooperators and learning about contribution levels in the game.

We estimate the following regression:

$$Contr_{it} = \alpha + \beta AccumPartnerContr_{it} + \gamma AccumCompetContr_{it} + \delta_{st} + \theta_t X_i + \pi_i + \epsilon_{it} \qquad (7)$$

where $Contr_{it}$ is the contribution of participant i at period t of the game. $AccumPartnerContr_{it}$ is the average contribution of partners that subject i has observed until time t. It is equal to $(1/t)\sum_{j=1}^{t} x_{ij}$, where x_{ij} is the contribution of i's partner at period j. Note this variable varies across individuals and across periods as the individual experience new contributions from partners. $AccumCompetContr_{it}$ is the average contribution of the *competitor* group that subject i has observed until time t. It is equal to $(1/2t)\sum_{j=1}^{t}(z_{1,ij} + z_{2,ij})$, where $z_{1,ij}$ and $z_{2,ij}$ are the contributions of the two competitors to i at period j.

Equation (7) includes individual fixed effects, period-session fixed effects (e.g., a dummy for being the fifth period of session 3) and covariates (age and gender) interacted with period effects. Hence, the effects are identified from within-individual variation in the experience in the game (as she observes higher or lower contributions from competitors and partners throughout the experiment). We estimate it separately for the sessions with or without the competition treatment. We

take first-differences of equation (7) when estimating it. The standard errors are clustered at the subject (individual) level.

In the sessions with "no competition" the subjects did not have competitors to observe. However, our experimental software kept track of which subjects *would have been* the competing groups in the experiment. In other words, the software (randomly) assigns groups to competitors in both control and competition groups, but the information on who the competitors were was never disclosed to participants (and it did not affect their payoffs in any way). Given this setup, it is possible to calculate $AccumCompetContr_{it}$ for the non-competitive session and use it as a placebo test.

One possible concern with estimating equation (7) is that, as subjects get paired as partners and competitors in the game, one subject's decision may affect a future partner's or competitor decision. An example can illustrate the issue. Suppose subjects 1 and 2 are partners in the first period of the game. In period 2, player 2 is partnered with player 3. In period 3, player 1 and 3 are partners. It is possible that player 1's choice of contribution in period affected player 2's contribution in period 2, which then affects player 3. This leads to the experience of player in period 3 to be affected by what she chose in period 1.

To address this issue, we can use a subject's contribution in the first period of the experiment as an instrument for latter contributions. In particular, we can estimate equation (7) using two-stage-least-squares (2SLS) where $AccumPartnerContr_{it}$ and $AccumCompetContr_{it}$ are instrumented using the two following first-stage regressions:

$$AccumPartnerContr_{it} = \alpha' + \beta' InstPartner_{it} + \gamma' InstCompet_{it} + \delta'_{st} + \theta'_t X_i + \pi'_i + \epsilon_{it} \quad (8)$$

$$AccumCompetContr_{it} = \alpha'' + \beta'' InstPartner_{it} + \gamma'' InstCompet_{it} + \delta''_{st} + \theta''_t X_i + \pi''_i + \epsilon_{it} \quad (9)$$

where $InstPartner_{it}$ is equal to $(1/t)\sum_{j=1}^{t} f_{ij}$, where f_{ij} is the *first-period* contribution of subject i's partner at period j. In other words, if all of subject i's partners until time t contribute the same amount they contributed when they were in the first-period of game, $InstPartner_{it}$ and $AccumPartnerContr_{it}$ would be the exact same. In practice, subjects change their behavior throughout the game, so the two variables differ. However, if subjects that make larger contributions in the first period make larger contribution in later periods, then the two variables will be positively correlated.

Moreover, because subjects are randomly assigned to partners and competitors in each period, the variation in $InstPartner_{it}$ is independent of subject i's actions in the experiment. Intuitively, the instrument exploits the fact that the first period contribution captures subject's propensity to contribute later in the game. The decision to contribute in the first period is made without observing any other subjects' behavior. It can thus be understood as a subject's "type": subjects that make larger first-period contribution have a "type" that will contribute more. Because subjects are randomly matched to partners and competitors every period, some subjects will (randomly) matched to low or high types earlier or later in the game. This creates variation on the experience of partners' and competitors' contribution that is independent of any player i's action in the game.

The instrument for accumulated competitor's contribution, $InstCompet_{it}$, is similarly constructed, being equal to $(1/2t)\sum_{j=1}^{t}(g_{1,ij}+g_{2,ij})$, where $g_{1,ij}$ and $g_{2,ij}$ are the first-period contributions of the two competitors to i at period j.

Table S10 present the result of the first-stage regressions (equations 8 and 9). They show that the instruments are predictive of the endogenous variables. The pattern is exactly is what is expected: the instrument based on *partners*' first-period contribution predicts *partners* later contributions, it is uncorrelated with *competitors* later contribution. This latter results is consistent with the

exogeneity of the instrument. The converse occurs for the instrument based on competitors first-period contribution. The instruments have high statistical significance, with large F-statistics in a test of the null hypothesis their coefficients are equal to zero. Note that the coefficients of the instruments have similar magnitudes in all cases where they are predictive of the partners' or competitors' contributions. This is also expected since the magnitude of the effect of the instrument is based by how predictive is the first-period contribution on later contributions.

Our main evidence that subjects learn from other players in the experiment is provided in Table S11, which reports the results from estimating equation (7). Columns (1) and (2) focus on the sessions with competition, and report the estimation by OLS and using the instrumental variables (IV). In both cases, it indicates that, as a subject observes higher contributions from partners in as the experiment progresses, she increases her own contributions accordingly. The same is true for her experience with competitors' contributions.

Column (2) suggests experiencing an increase of one Euro in the observed average contribution of partners increases the subjects' contribution by 0.25 Euros. An increase of one Euro in the observed average contribution of competitors increases contribution by 0.38 Euros. Note that the accumulated average of competitors is based on twice the number of contributions as the accumulated average of partners (since each subject has one partner and two competitors in each period). A fully rational player would thus put twice as much weight on the average of competitors. Our results does suggest that subjects put "too much weight" on the experience from partners as opposed to competitors.

Columns (3) and (4) repeat the estimation for the control group. The effect of accumulated partners' contribution is also positive and significant. It is larger than in the competitive case. This is consistent with our interpretation of the results as learning: subjects only observe partners' contributions in this case (as they do not have competitors).

A placebo test of our strategy appears in the second row of columns (3) and (4) of Table S11. As mentioned before, even though there are no competitors in the "no competition case", our experimental software kept track of which groups *would have been* assigned to compete with each other. However, the contribution os these "placebo competitors" was never revealed to subjects in the experiment. We can, however, calculate what the accumulated competitor's contribution (and its instrument) would have been in this case and include in the regression. As expected, the behavior of the placebo competitors is uncorrelated with subjects' contributions.

Does learning throughout predict answers to the trust question? The previous subsection provided evidence that subjects' contributions in the game were affect by their experience observing partners' and competitors' behavior. This section provides evidence that subjects' *response to the trust question* was also affected by experience in the game.

In particular, we leverage that, in competitive sessions, a share of subjects increased their contributions over the game, while some decreased. This can be seen as the rising mass of 0 or 10 contributions in Figure S5. Given this, we test whether subjects that increased their contribution throughout the experiment are more likely to report they trust than those that decreased their contributions throughout the experiment. Note that, given the results described above, those who increased their contribution can be understood as those that experienced positive observations of partners' and competitors' behavior.

To do so, we must calculate a subject-level measure of how much their contributions increased throughout the experiment, we do so running the following regression separately for each subject in the experiment:

$$Contr_{it} = \lambda_i \ + \phi_i t + \epsilon_{it} \tag{10}$$

where $Contr_{it}$ is the contribution of subject i at period t. This yields a "constant" λ_i and "trend" ϕ_i

for every subject i in the experiment. Of particular interest is the "trend", as it measures how much subject i increase his contribution, on average, at each new period of the game. Figure S6 shows the distribution of trends (the ϕ_i) separately for the subjects in competitive and non-competitive sessions. It can be seen that subjects in competitive experienced both increasing or decreasing contributions, while those in non-competitive sessions mostly followed a pattern of decreasing their contributions over time. Once the ϕ_i is calculated, we estimate:

$$trust_i = \Phi + \Psi\lambda_i + \Omega\phi_i + \Gamma_s + \epsilon_i \tag{11}$$

where $trust_i$ is an indicator if i responded positively to the trust question. Note that ϕ_i and λ_i are variables in equation (11), while Φ, Ψ, and Ω are parameters to be estimated. Γ_s are session fixed effects. Our hypothesis is that Ω is positive, suggesting that those that increased their contributions throughout the game became more likely to trust.

Table S12 present the results from estimating equation (11). Columns (1)-(3) run the regression only using data from the competitive sessions. Column (1) indicates a positive and statistically significant Ω. Column (2) substitutes λ_i in equation (11) by the contribution of player i in the first period. Column (3) estimates the same model as column (2), but instruments the trend ϕ_i using the following first-stage regression:

$$\phi_i = \Theta + \Pi f_i + \sum_{t=1}^{19} \rho_t s_t + \Gamma'_s + \epsilon_i \tag{12}$$

where f_i is the first-period contribution of subject i, and s_j is the first period contribution of i's partner in period t. Note there are thus 19 excluded instruments: the first-period contribution of i's partner in periods 1, 2, 3,...,19. Since partners are randomly matched every period, and first-period contributions are made before a subject observe any other players' behavior, equation (12) isolates variation that is driven by random variation in the order of i's partners. Intuitively, some participants are matched to partners earlier (instead of later) in the game, and that affects how their contributions will behave (trend up or down) throughout the game.

Column (3) of Table S12 indicates that the instrumented effect of a positive trend in contributions is positive and significant. Note that the column (3) is estimated using only variation based on subjects' first-period contributions and how the patterns generated by random assignment of partners in the game. The estimates suggest that a subject that moves one standard deviation in the distribution of ϕ_i is 15.9 p.p. more likely to report she trusts.

Columns (4)-(6) repeat the exercise of columns (1)-(3) using data from the non-competitive sessions. The results do not indicate that subjects that increased their contributions throughout the experiment became more likely to trust. Note, however, that there is substantially less variation in ϕ_i in the non-competitive sessions: almost all subjects experienced declining contributions (Figure S6).

5 Supplementary Text: Experimental Instructions

See Figure S7 and S8 for the original instructions (in French) distributed in the noncompetitive and competitive sessions (respectively).

6 A Simple Model where Competition Affects Effort Provision

We present here a model in which the complementarity in production of workers leads to multiple equilibria. Each different can be interpreted as a distinct social norm.

Overview. The setting is the following: firms require workers to contribute unverifiable effort and each worker decides the level of unverifiable effort she contributes. Effort is costly and therefore workers prefer low contributions, conditional on firm performance. However, a firm that is unable to induce effort will perform poorly, which adversely affects all workers. How adversely affected workers are by poor performance depends on the level of competition. In particular, the more competitive the firm's environment, the more likely are poorly performing firms forced to shut down. Shut downs are costly to workers, and the key feature of shut downs is that they affect all workers equally. We assume a strong complementarity in the impact of individual effort on the performance of the firms which leads to a continuum of equilibrium provision of effort by workers.

Model. Assume multiple industries in the economy, each with multiple firms. Each firm has to hire N workers at a given wage w to produce. Firms can be of either high quality (H) or low quality (L), depending on the effort contribution of their selected workers. There are no other factors of production.

Each worker i has to decide its contribution of effort e_i. This effort imposes on workers a disutility cost of $0.5e_i^2$. Conditional on the effort provided by workers, the firm is H or L. The firm is H with probability $p(\min e_i)$ with $p' > 0$, $p'' < 0$ and $p \in [0, 1]$. This is to say that it is the effort provided by the worker who contributes the least that determines the probability of success. This Leontief function induces strong complementarities between workers' effort. Intuitively, workers are only willing to provide more effort if they expect (or trust) their co-workers to do the same.

Competition in a sector affects the probability that a low quality firm is forced to shut-down. The more competitive the sector, the higher this probability. L quality firms shut down with probability $\gamma \geq 0$. H quality firms do not shut down. Thus γ characterizes an industry's level of competition.

All workers decide simultaneously their effort level to maximize their expected income. Particularly, worker i chooses e_i to maximize:

$$w[p(\min e_j) + (1 - p(\min e_j))(1 - \gamma))] - 0.5e_i^2$$

where j indexes all the other workers in i's firm.

In this setting, a (Nash) equilibrium must be symmetric (all workers in a firm choose the same effort) as it never pays to exert an effort above $\min e_j$ (since it is costly and has no impact on p). For a level of effort e to constitute an equilibrium, it must be that no worker has an incentive to deviate to a lower effort. This implies that e is an equilibrium level of effort if:

$$w\gamma p'(e) \geq e$$

As $p'' < 0$, there exists e^* such that $w\gamma p'(e^*) = e^*$ and the condition above is satisfied for all $e \in [0, e^*]$. This argument leads to the following proposition.

Proposition 1 *There is a continuum of norm-based equilibria. Each one corresponds to a symmetric e chosen by all workers of the firm. The lowest equilibrium is $e = 0$, the highest equilibrium is $e = e^*$. The amount of effort contributed in the highest equilibrium is increasing in the degree of competition, γ.*

This corresponds to what we have referred the "static equilibrium effect" in the main text. Note that, while multiple equilibria always exist (regardless of the level of competition), increasing competition raises the level of effort that may constitute an equilibrium. This matches well the arguments made more informally in the main text.

Possible extensions to a dynamic context. There are several possible ways to introduce dynamics in order to analyse the "dynamic selection effect" described in the text. Perhaps the

simplest is to assume that firms that shut down are replaced by other firms that follow the norm of a randomly matched surviving firm. Since the probability of shut down is decreasing in e, the expected equilibrium (norm) effort of surviving firms must be greater than of dying firms. Hence, this process will converge, in expectation, to a situation where all firms have equilibrium effort e^*.

7 Supplementary Text: Experimental Instructions for Noncompetitive Sessions (English Translation)

The experiment that you will participate in is designed to study decision making. We ask that you read the instructions carefully. They should allow you to understand the experience. When all participants have read these instructions, the experimenter will read them out loud.

All the decisions that you take will be treated anonymously, they will not be known to other subjects and the experimenter will not be able to link your answers to your identity. You insert your choices into the computer in front of you. During each round of play, you will obtain returns based on your decisions, expressed in ECU. At the end of the experiment, one of the periods will be randomly selected and the returns you earned in that period will be converted into euros using a conversion rate that is specified at the end of these instructions. That amount in euros will be paid to you in cash.

From now we ask that you do not speak. If you have a question, please raise your hand and an experimenter will answer you in private.

General framework.

There are 20 participants in the room. Each experiment has multiple rounds. Each round proceeds identically. At the beginning of each round the central computer will randomly form 10 groups of 2 from the 20 participants. You will never be paired more than once with the same partner. You cannot identify your partner nor can your partner identify you. At the beginning of each round, you and your partner each have 100 chips that you must independently choose to completely divide between two accounts: your individual account and a joint account shared by both of you.

Operation of the accounts.

Individual account.

Each chip you place in your individual account earns you 1 ECU. Similarly, if your partner places a chip in his individual account that earns him 1 ECU.

Your partner's earnings are not affected by the number of chips that you place in your individual account. Similarly your gain is not affected by the number of chips that your partner places in his individual account.

Example 1: Whatever your partner's decisions in his individual account, if you put 30 chips in your individual account, your gain from this decision will be ECU 30. The gains of your partner will not be affected by your decision.

Example 2: Suppose your partner puts 10 chips in his individual account, whatever your investment decision in your individual account, the gain resulting from his decision will be ECU 10 to him; your gain is not affected by this decision.

Collective account.

The collective account is common to both members of the group, that is, you and your partner. Each chip that you place in the collective account gets converted in to 1.5 chips in total in this account. The total ECU in the collective account is shared equally between the two members. Thus your gain from the collective account depends on the total number of chips that both you and your partner put in to the collective account. To illustrate the calculation of gain in that account,

consider an example in which you are player 1 and your partner is player 2. Let Z_1 denote the number of chips placed by player 1 (you) in the collective account, and let Z_2 denote the number of chips placed by player 2 in the collective account. The total number of chips in the collective account is thus equal to $Z_1 + Z_2$, yielding a balance of $1.5 \times (Z_1 + Z_2)$ ECU in the collective account. The group therefore yields for each member the amount: $1.5 \times (Z_1 + Z_2)/2$.

Example 1:

You place 60 chips in the collective account ($Z_1 = 60$) and your partner puts in zero ($Z_2 = 0$). The balance of the joint account is thus $1.5 \times (60 + 0)$ chips. The gain resulting from the collective account for each member of your group (including yourself) is: $1.5 \times (60 + 0)/2$ or ECU 45. The total payment adds the returns from investment in the individual account to this for each member of the group. Thus the total remuneration from this period would be:

For you: $100 - 60 + 1.5 \times (60 + 0)/2 = 85$.

For your partner: $100 - 0 + 1.5 \times (60 + 0)/2 = 145$.

Example 2:

You place 20 chips in the collective account ($Z_1 = 20$) and your partner put in 60 ($Z_2 = 60$). The balance of the joint account is $1.5 \times (20 + 60)$ ECU. The gain resulting from the collective account for each member of your group (including yourself) is equal to: $1.5 \times (20 + 60)/2$, or 60 chips. The total payment adds the earnings from the investment in the individual account to this for each member of the group. The total remuneration of this stage is thus: For you: $100 - 20 + 1.5 \times (20 + 60)/2 = 140$. For your partner: $100 - 60 + 1.5 \times (20 + 60)/2 = 100$.

To summarize, in each period, each member of your group (including yourself) has two sources of return: the return on his investment in his individual account and the return resulting from the total number of chips placed by both members of the group in the collective account.

The ECU to Euro conversion rate is one ECU equals 0.1 Euros.

The payment for participation is 5 euros.

8 Supplementary Text: Experimental Instructions for Competitive Sessions (English Translation)

The experiment that you will participate in is designed to study decision making. We ask that you read the instructions carefully. They should allow you to understand the experience. When all participants have read these instructions, the experimenter will read them out loud. All the decisions that you take will be treated anonymously, they will not be known to other subjects and the experimenter will not be able to link your answers to your identity. You insert your choices into the computer in front of you. During each round of play, you will obtain returns based on your decisions, expressed in ECU. At the end of the experiment, one of the periods will be randomly selected and the returns you earned in that period will be converted into euros using a conversion rate that is specified at the end of these instructions. That amount in euros will be paid to you in cash. From now we ask that you do not speak. If you have a question, please raise your hand and an experimenter will answer you in private.

General framework

There are 20 participants in the room. Each experiment has multiple rounds. Each round proceeds identically. At the beginning of each round the central computer will randomly form 10 groups of 2 from the 20 participants. You will never be paired more than once with the same partner. You cannot identify your partner nor can your partner identify you.

To each pair, the computer randomly assigns another pair that is grouped in two. This pair is the competing group.

At the beginning of each round, you and all other participants each have 100 chips that you must independently choose to completely divide between two accounts: an individual account and a joint account shared by you and your partner.

Operation of the accounts

Individual account

Each chip you place in your individual account earns you 1 ECU. Similarly, if your partner places a chip in his individual account that earns him 1 ECU.

Your partner's earnings are not affected by the number of chips that you place in your individual account. Similarly your gain is not affected by the number of chips that your partner place in his individual account.

Example 1:

Whatever your partner's decisions in his individual account, if you put 30 chips in your individual account, your gain from this decision will be ECU 30. The gains of your partner will not be affected by your decision.

Example 2:

Suppose your partner puts 10 counters in his individual account, whatever your investment decision in your individual account, the gain resulting from his decision will be ECU 10 to him; your gain is not affected by this decision.

Collective account

The collective account is common to both members of the group, that is, you and your partner. Each chip that you place in the collective account gets converted in to 1.5 chips in total in this account. The compensation you get from this account depends on the collective amount invested by your group, but also on the amount invested by your competitor group as follows: if your group has invested more in its collective account than your competing group, then your personal payment from this account is one half of the ECU in the collective account. If your group has invested just as much as the competing group, then your personal payment from the collective account is also one half of the ECU in the collective account. If your group has invested strictly less than the competing group, your compensation from the collective account is zero chips.

To illustrate the calculation of gain in that account, consider an example in which you are player 1 and your partner is player 2. Let Z_1 denote the number of chips placed by player 1 (you) in the collective account, and let Z_2 denote the number of chips placed by player 2 in the collective account. The total number of chips in the collective account is thus equal to $Z_1 + Z_2$, yielding a balance of $1.5 \times (Z_1 + Z_2)$ ECU in your collective account.

Let X_1 denote the number of chips placed by player 1 of the competing group in to their collective account, let X_2 denote the number of chips placed by player 2 of the competing group in to their collective account. Thus the total number of chips in the collective account of your competitors is equal to $X_1 + X_2$ yielding $1.5 \times (X_1 + X_2)$ ECU in their collective account.

If the total amount of ECU in your collective account $(1.5 \times (Z_1 + Z_2))$ is greater or equal to the total amount of ECU in the collective account of your competing group $(1.5 \times (X_1 + X_2))$, then your remuneration from the collective account is half of what's in that account : $1.5 \times (Z_1 + Z_2)/2$. Otherwise your remuneration from the collective account is zero.

Example 1:

You place 60 chips in the collective account $(Z_1 = 60)$ and your partner puts in zero $(Z_2 = 0)$. The balance of the joint account is thus $1.5 \times (60 + 0) = 90$ chips.

In the competing group, the first player put 45 chips in to their collective account $(X_1 = 45)$ and their player 2 put in 35 chips $(X_2 = 35)$, the balance of their joint account is thus $1.5 \times (45+35) = 120$

ECU. The balance of the collective account of the competing group is thus superior to your groups balance ($120 > 90$). Thus remuneration from the collective account is zero for both you and your partner . Thus the total compensation of this round would be given by the addition of the returns on the individual account and those on the group account:

For you: $100 - 60 + 0 \times 1.5 \times (60 + 0)/2 = 40$

For your partner: $100 - 0 + 0 \times 1.5 \times (60 + 0)/2 = 100$

Example 2:

You place 20 chips in the collective account ($Z_1 = 20$) and your partner puts in 50 chips to the collective account, so $Z_2 = 50$. Your group account thus has a balance equal to $1.5 \times (20 + 50)$ or 105 chips. In the competitor group both players have each decided to put 20 chips in to the collective account ($X_1 = X_2 = 20$), so the balance of their joint account is $1.5 \times (20 + 20)$ or ECU 60. The balance of the collective account in the competing group is thus less than the balance of your group ($60 < 105$). Thus remuneration from the collective account is $1.5 \times (20 + 50)/2$ for you and your partner. The total compensation of this step is thus given by the addition of the returns on the individual account and the group account:

For you: $100 - 20 + 1.5 \times (20 + 50)/2 = 132.5$

For your partner: $100 - 50 + 1.5 \times (20 + 50)/2 = 102.5$

To summarize, in each period, each group member (including you) has two sources of gain: the gain on investment in the individual account and the gain resulting from the total number of chips placed by both members of the group in the collective account. The investment-related gains on the collective account depend on the investment made by the competitor group.

The ECU to Euro conversion rate is one ECU equals 0.1 Euros.

The payment for participation is 5 euros.

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

Sample contains 612 respondents of the 2004 GSS Workplace Module.

Table S2: 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 Problems	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)

Sample contains 612 respondents of the 2004 GSS Workplace Module.

Table S3: Sectoral Concentration and Trust (GSS Workplace Module Data)

	Dependent Variable: Trust Indicator						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Competition	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		Y		Y			Y
Union Status				Y			Y
Arguments				Y			Y
Skip Work				Y			Y
Supervisor					Y		Y
More Good						Y	Y
Optimism						Y	Y
Workplace Size			Y	Y			Y
Other Workplace Covariates				Y			Y
Observations	612	612	612	612	612	530	530

***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. Robust standard errors clustered at the industry level in parenthesis. The unit of observation is a GSS respondent. 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 S4: Effects of Sectoral Concentration on Trust - Interactions with Experience
(GSS Workplace Module Data)

	Dependent Variable: Trust Indicator						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Competition	-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)
Competition*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		Y		Y			Y
Union Status				Y			Y
Arguments				Y			Y
Skip Work				Y			Y
Supervisor					Y		Y
More Good						Y	Y
Optimism						Y	Y
Workplace Size			Y	Y			Y
Other Workplace Covariates				Y			Y
Observations	612	612	612	612	612	530	530

***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. Robust standard errors clustered at the industry level in parenthesis. The unit of observation is a GSS respondent. 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: Summary Statistics - Banking Regulation/GSS Data

	Mean	Standard Deviation
Trust Indicator (×100)	39.99	48.99
Age	45.07	17.62
City Size (in thousands)	381.22	1248.66
Employed	0.57	0.49
Income (in 1986 US$ thou.)	12.86	17.83
Female	0.57	0.50
Highest Degree: High School	0.53	0.50
Highest Degree: College or Higher	0.19	0.39
White	0.84	0.36
Black	0.13	0.34
Married	0.57	0.49
Jewish	0.02	0.14
Catholic	0.25	0.43
Protestant	0.63	0.48

Sample contains 17,455 respondents from the 1973-1994 Waves of the GSS.

Table S6: Effect of Banking Deregulation on Trust (Baking Deregulation/GSS Data)

	Dependent Variable					
	Trust Indicator (×100)					Placebo: Predicted Trust
	(1)	(2)	(3)	(4)	(5)	(6)
Post Deregulation	-0.696	-0.668	-0.668	–1.302	-0.848	-2.208**
	(1.506)	(1.508)	(1.499)	(1.536)	(1.494)	(1.002)
Years Since Deregulation	1.394**	1.422**	1.423**	1.312**	1.560**	-0.315
	(0.685)	(0.682)	(0.679)	(0.674)	(0.765)	(0.359)
Additional Controls						
Individual Income (GSS)		Y	Y	Y	Y	
log Mean Income (CPS)			Y			
Employment-Population Ratio (BLS)				Y		
Gini Index - Income (CPS)					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	15,410	17455

***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. Robust standard errors clustered at the state level in parenthesis. The unit of observation is a GSS respondent. 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 S7: Effects of Oil Reserve Value (Baking Deregulation/GSS Data)

Dependent Variable:	(log) Mean Income	Gini Index - Income	Trust Indicator (×100)		Predicted Trust (×100)
	(1)	(3)	(4)	(5)	(6)
Oil Reserves × Oil Price	0.039***	-0.019	-1.372	-2.089	0.260
	(0.005)	(0.115)	(1.414)	(1.252)	(0.564)
Individual Controls				Y	
State Fixed Effects	Y	Y	Y	Y	Y
Time Fixed Effects	Y	Y	Y	Y	Y
State-Specific Trends	Y	Y	Y	Y	Y
Observations	17455	17455	17455	17455	17455

***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. Robust standard errors clustered at the state level in parenthesis. The unit of observation is a GSS respondent. Each column reports a separate regression controlling for state fixed effects, year effects, state-specific trends. Individual controls are 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.

Table S8: Effect of Changing Industry of Employment on Changes in Trust (German SOEP)

	Change in Trust				Lagged Change in Trust	Change in Trust
	(1)	(2)	(3)	(4)	(5)	(6)
Change in Competition	0.452** (0.188)	0.455** (0.190)	0.402 (0.409)	0.548** (0.242)	-0.169 (0.289)	0.578* (0.324)
Non-Mover	0.003 (0.013)	0.004 (0.013)	0.014 (0.029)	-0.015 (0.017)	-0.038* (0.022)	0.018 (0.022)
Controls × Time Interactions		Y				
Manufacturing Only			Y			
Services Only				Y		
Observed in 3 Waves					Y	Y
Observations	9,103	8,977	2,000	6,408	3,378	3,378

***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. Robust standard errors clustered at the respondent (individual) level in parenthesis. The unit observation is a SOEP respondent-year pair. Each column reports a separate regression controlling for time fixed effects. Column (2) adds controls for years of education, age, and dummies for gender and married status interacted with time fixed effects. See text for further details.

Table S9: Effect of Competition in Experimental Sample

Outcome	Controls?	Control Mean	Effect of Competition	p-value of Competition Effect			
				Cluster	Agg.	Fisher	IM
Panel A: Main Effects (Full Sample, N=220)							
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
Payment from Game	Y	11.328	-1.268	0.010	0.014	0.017	0.039
Panel B: Student 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

***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. The unit of observation is a experimental subject. Individual controls are age, gender, and marital status.

Table S10: First-Stage: Effect of Partners' and Competitors' First Period Contributions (Laboratory Experiment)

	Dependent Variable:			
	Accum. Partner's Contribution		Accum. Competitors' Contribution	
	(1)	(2)	(3)	(4)
Accum. Avg. Partners' 1st-Period Contribution	0.688*** (0.043)	0.782*** (0.036)	-0.031 (0.027)	-0.014 (0.017)
Accum. Avg. Competitors' 1st-Period Contribution	0.073 (0.053)	0.020 (0.043)	0.687*** (0.051)	0.787*** (0.030)
F-Stat of Instruments	135.9	239.0	90.5	342.4
Sessions in Sample	Competition	No Competition	Competition	No Competition
Observations	1520	1860	1520	1860

***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. Robust standard errors clustered at the subject level in parentheses. Unit of observation is a player-experimental period. All regressions include session-period effects and age and gender indicators interacted with period effects. Regressions are estimated by first-differences. See text for further information and definition of variables.

Table S11: 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

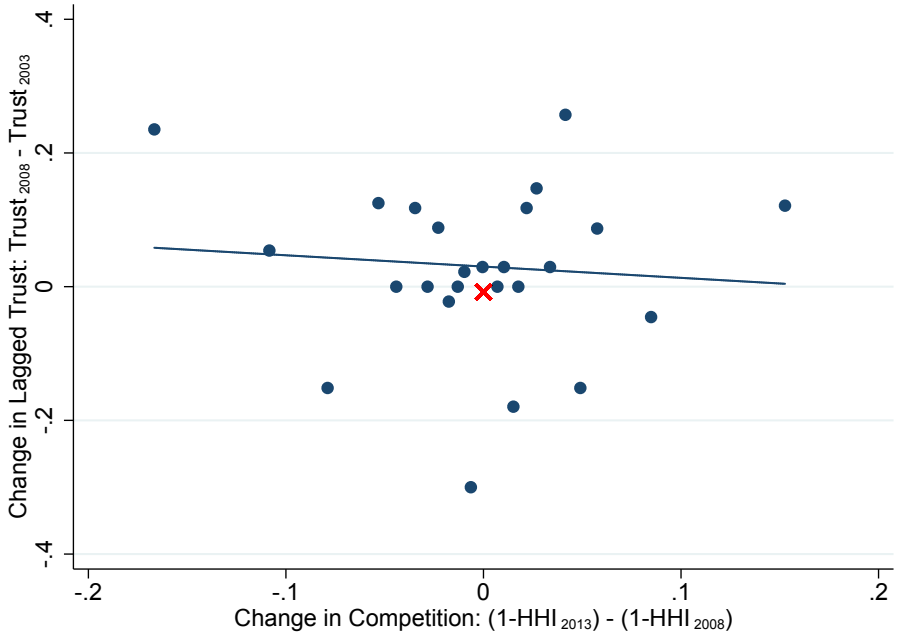
***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. Robust standard errors clustered at the individual level in parentheses. Unit of observation is a player-experimental period. All regressions include session-period effects and age and gender indicators interacted with period effects. Regressions are estimated by first-differences. Instruments are based on partners' and competitors' first-period contribution. See text for further information and definition of variables.

Table S12: Effect of Experimental Experience on Trust

	Dependent Variable: Trust Indicator					
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	0.033			0.061***		
	(0.017)			(0.015)		
1st Contribution		0.031**	0.037***		0.053***	0.060**
		(0.014)	(0.011)		(0.014)	(0.018)
Trend	0.492**	0.410**	0.554***	0.287	0.231	0.217
	(0.221)	(0.188)	(0.209)	(0.266)	(0.250)	(0.443)
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

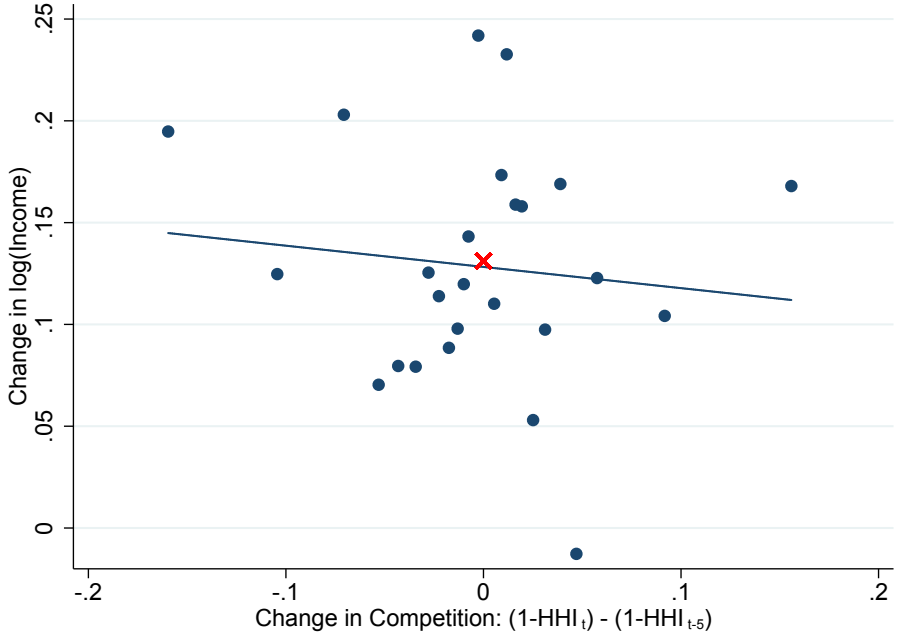
***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. Robust standard errors in parentheses. Unit of observation is an experimental subject. Regressions include session fixed effects. See text for definition of the variables and instruments.

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



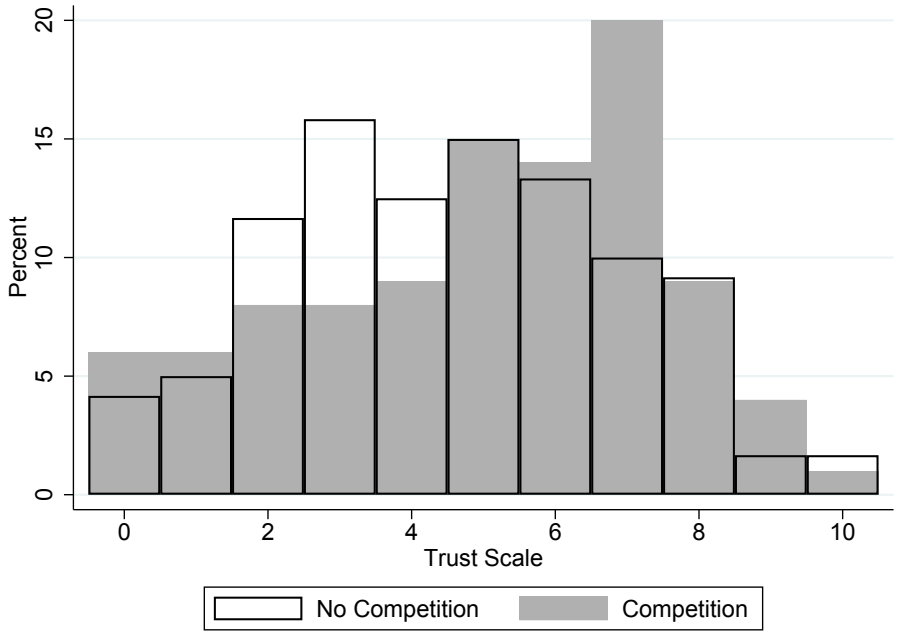
This figure presents a binned scatter plot of workers change in trust (between 1998 and 2003) vs. change in the competition in her industry of employment (between 2003 and 2008). The sample consists of employed respondents of the German SOEP in the years 2003, 2008, and 2013. The blue dots are based on 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 respondents that do not change industry of employment (and hence do not experience change in competition). See text for further information.

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



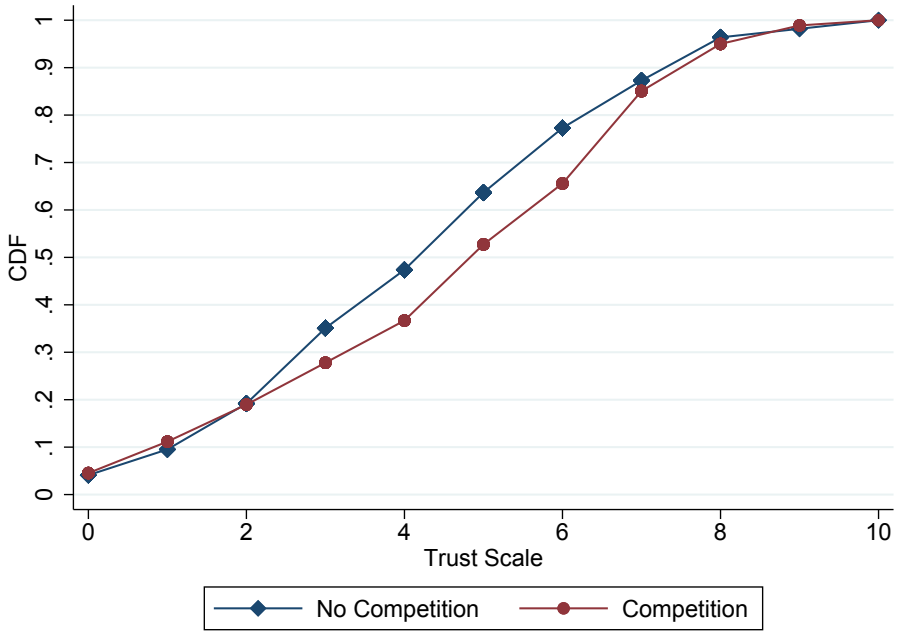
This figure presents a binned scatter plot of workers change in (log) income vs. change in the competition in her industry of employment. The sample consists of employed respondents of the German SOEP in the years 2003, 2008, and 2013. Changes are relative to income and competition five years before. The blue dots are based on 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 income 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 respondents that do not change industry of employment (and hence do not experience change in competition). See text for further information.

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



This figure presents a histogram of the distribution of answers of experimental subjects to the trust question, separately for those assigned to the competitive and noncompetitive sessions.

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



This figure presents the cumulative distribution function of the answers of experimental subjects to the trust question, separately for those assigned to the competitive and noncompetitive sessions.

Figure S5: Histogram of Contributions in Experiment by Period

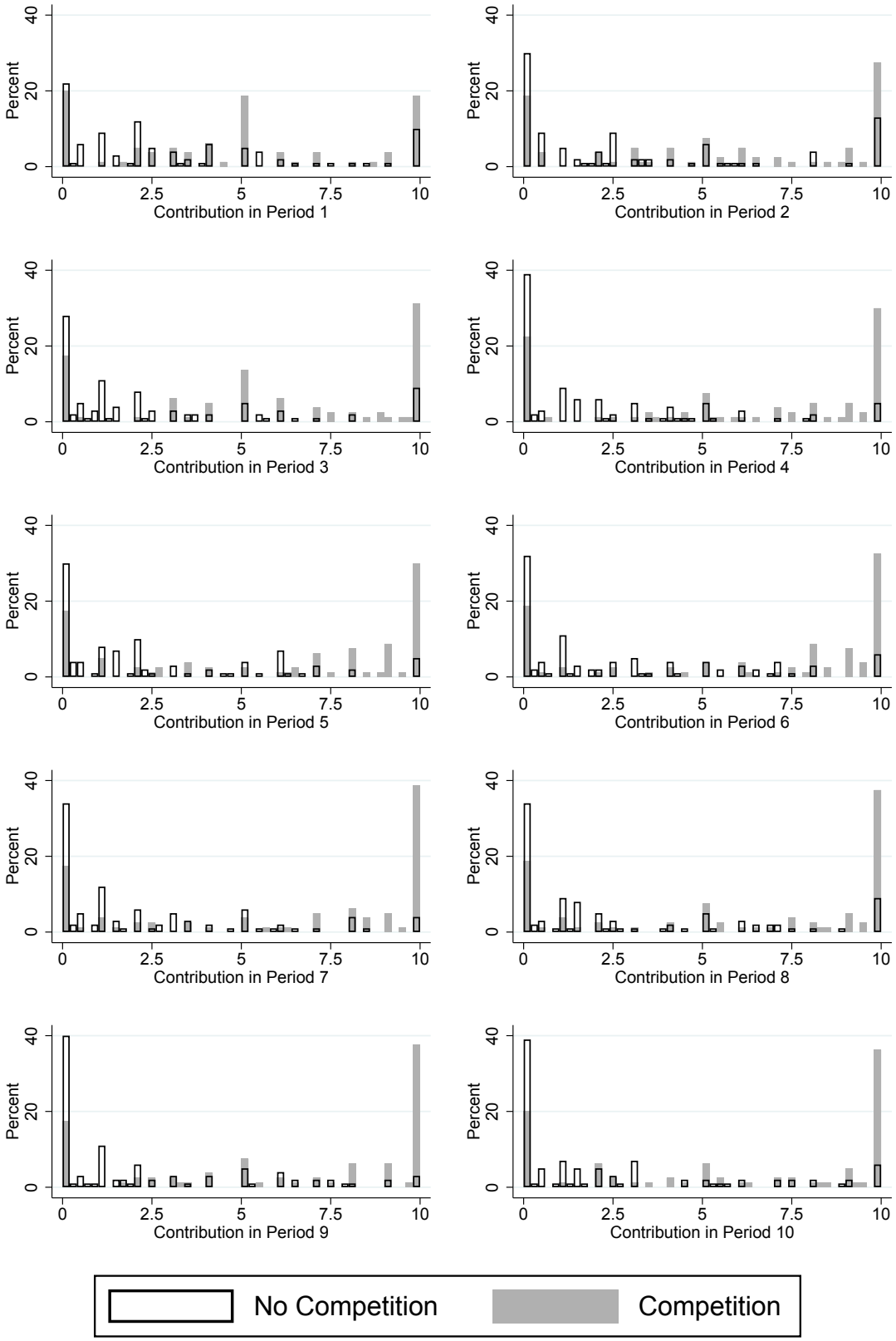


Figure S5: Histogram of Contributions in Experiment by Period (continued)

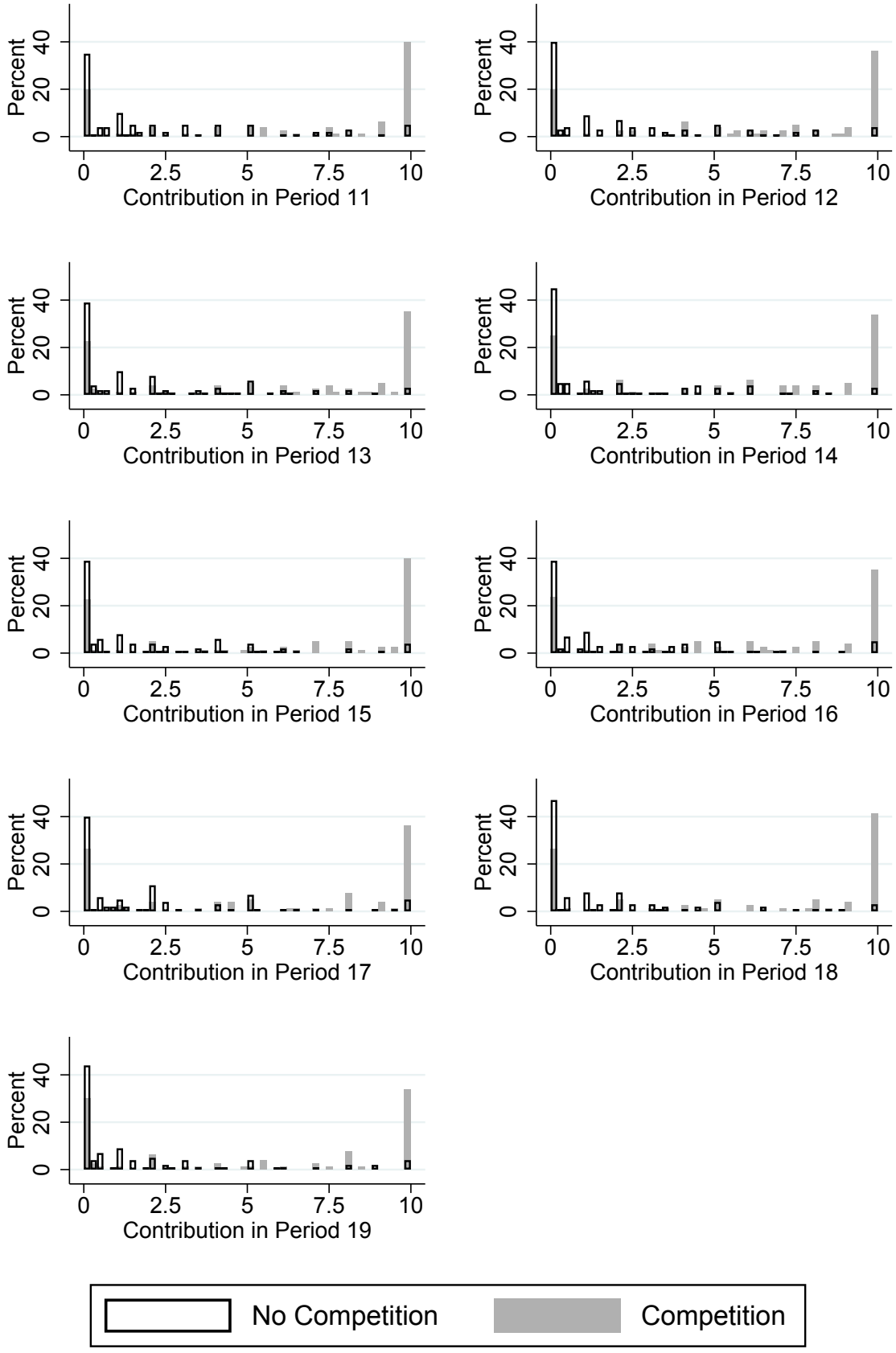
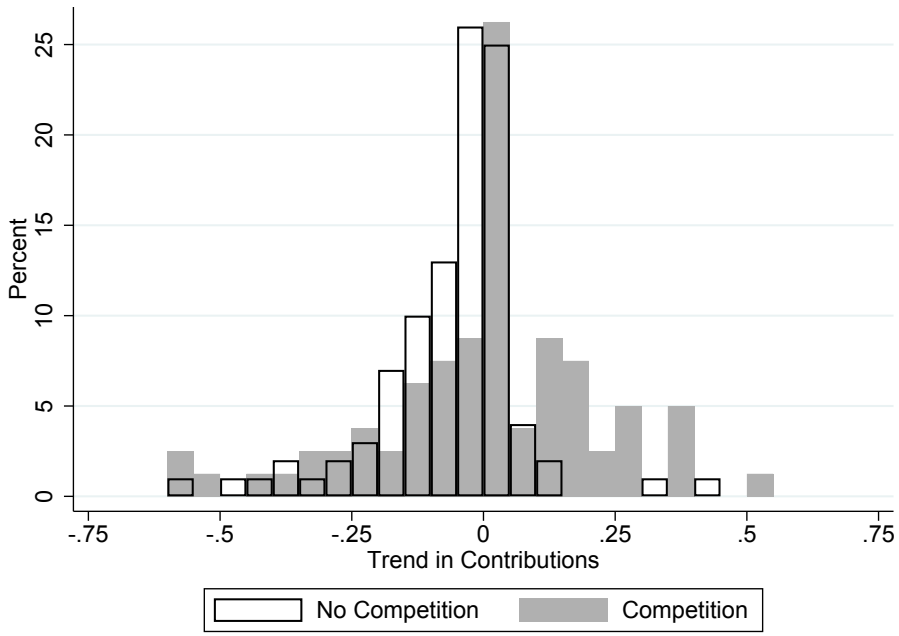


Figure S6: Distribution of Trends in Contributions



This figure presents a histogram of the estimated "trend" in the contributions of experimental subjects (the estimated ϕ from equation 10) separately for those assigned to the competitive and noncompetitive sessions.

Figure S7: Experimental Instructions (Noncompetitive Sessions)

Traitement non concurrentiel : lire et distribuer les instructions se trouvant sur les 3 pages suivantes

Instructions

L'expérience à laquelle vous allez participer est destinée à l'étude de la prise de décision. Nous vous demandons de lire attentivement les instructions, elles doivent vous permettre de bien comprendre l'expérience. Lorsque tous les participants auront lu ces instructions un expérimentateur procédera à une relecture à voix haute.

Toutes vos décisions seront traitées de façon anonyme. Vous indiquerez vos choix à l'ordinateur devant lequel vous êtes assis(e). Au cours de chacune des périodes, vous obtiendrez des gains, exprimés en écus. A la fin de l'expérience, une des périodes sera choisie au hasard et les gains de cette période seront convertis en euros selon un taux de conversion qui est précisé à la fin de ces instructions. Votre gain en euros vous sera versé en liquide.

A partir de maintenant nous vous demandons de ne plus parler. Si vous avez une question levez la main et un expérimentateur viendra vous répondre en privé.

Cadre général

Vous êtes 20 participants dans la salle. L'expérience comporte plusieurs périodes. Chaque période se déroule de manière identique.

Au début de chacune des périodes l'ordinateur central va former aléatoirement 10 groupes de 2. Vous ne serez jamais deux fois avec le même partenaire. Vous ne pouvez pas identifier votre partenaire et ce dernier ne peut pas vous identifier.

Au début de chaque période, vous, ainsi que votre partenaire, disposez de 100 jetons que vous devez entièrement répartir entre deux comptes : votre compte individuel et un compte collectif commun à tous les membres du groupe.

Fonctionnement des comptes

Compte Individuel

Chaque jeton que vous placez sur votre compte individuel vous rapporte 1 écu. De même, si votre partenaire place un jeton sur son compte individuel cela lui rapporte 1 écu.

Figure S7: Experimental Instructions (Noncompetitive Sessions), cont'd

Les gains de votre partenaire ne sont pas affectés par le nombre de jetons que vous placez sur votre compte individuel. De même votre gain n'est pas affecté par le nombre de jetons que votre partenaire place sur son compte individuel.

Exemple 1: Quelles que soient les décisions de votre partenaire sur son compte individuel, si vous placez 30 jetons sur votre compte individuel, votre gain résultant de cette décision sera de 30 écus. Les gains de votre partenaire ne seront pas affectés par votre décision.

Exemple 2: Supposons que votre partenaire place 10 jetons sur son compte individuel, quelle que soit votre décision de placement sur votre compte individuel, son gain résultant de cette décision sera de 10 écus; votre gain ne sera pas affecté par cette décision.

Compte Collectif

Le compte collectif est commun aux deux membres du groupe, c'est à dire vous et votre partenaire.

Chaque jeton que vous placez sur le compte collectif rapporte 1.5 écu à ce compte.

Les écus se trouvant sur le compte collectif sont partagés à parts égales entre les deux membres du groupe.

Votre gain résultant du compte collectif dépend donc du nombre total de jetons que vous et votre partenaire placez sur le compte collectif.

Afin d'illustrer le calcul du gain résultant de ce compte, considérons que vous êtes le joueur 1 du groupe et votre partenaire le joueur 2. Notons $\boldsymbol{Z_1}$ le nombre jetons placés par le joueur 1 (vous) sur le compte collectif, $\boldsymbol{Z_2}$ le nombre de jetons placés par le joueur 2 sur le compte collectif. Le nombre total de jetons sur le compte collectif est égal à $\boldsymbol{Z_1 + Z_2}$ ce qui donne un solde de $\boldsymbol{1,5 \times (Z_1 + Z_2)}$ écus sur le compte collectif. Le compte collectif rapporte donc à chacun des membres du groupe : $\frac{\boldsymbol{1,5\times(Z_1+Z_2)}}{\boldsymbol{2}}$.

Exemple 1: Vous placez 60 jetons sur le compte collectif ($\boldsymbol{Z_1 = 60}$) et votre partenaire en place zéro ($\boldsymbol{Z_2 = 0}$). Le solde du compte collectif est donc de $\boldsymbol{1,5 \times (60 + 0)}$ écus. Le gain résultant du compte collectif pour chaque membre de votre groupe (vous y compris) est égal à : $\frac{\boldsymbol{1,5\times(60+0)}}{\boldsymbol{2}}$ soit **45** écus. A ce gain s'ajoute le gain issu du placement sur le compte individuel pour chacun des membres du groupe. Les rémunérations totales de cette étape sont donc

Figure S7: Experimental Instructions (Noncompetitive Sessions), cont'd

Pour vous : $\mathbf{100 - 60 + \frac{1{,}5\times(60+0)}{2} = 85}$

Pour votre partenaire : $\mathbf{100 - 0 + \frac{1{,}5\times(60+0)}{2} = 145}$

Exemple 2: Vous placez 20 jetons sur le compte collectif ($\boldsymbol{Z_1 = 20}$) et votre partenaire en place 60 ($\boldsymbol{Z_2 = 60}$). Le solde du compte collectif est donc de $\mathbf{1,5 \times (20 + 60)}$ écus. Le gain résultant du compte collectif pour chaque membre de votre groupe (vous y compris) est égal à : $\frac{\mathbf{1{,}5\times(20+60)}}{\mathbf{2}}$, soit 60 écus. A ce gain s'ajoute le gain issu du placement sur le compte individuel pour chacun des membres du groupe. Les rémunérations totales de cette étape sont donc

Pour vous : $\mathbf{100 - 20 + \frac{1{,}5\times(20+60)}{2} = 140}$

Pour votre partenaire : $\mathbf{100 - 60 + \frac{1{,}5\times(20+60)}{2} = 100}$

Pour résumer, à chaque période, chaque membre de votre groupe (vous y compris) a deux sources de gain: le gain résultant de son placement sur son compte individuel et le gain résultant du nombre total de jetons placés par les 2 membres du groupe sur le compte collectif.

Le taux de conversion entre écu et euro est 0.1 euro pour 1 écu.

Le gain forfaitaire est de 5 €.

Figure S8: Experimental Instructions (Competitive Sessions)

Traitement concurrentiel : lire et distribuer les instructions se trouvant sur les 4 pages suivantes

Instructions

L'expérience à laquelle vous allez participer est destinée à l'étude de la prise de décision. Nous vous demandons de lire attentivement les instructions, elles doivent vous permettre de bien comprendre l'expérience. Lorsque tous les participants auront lu ces instructions un expérimentateur procédera à une relecture à voix haute.

Toutes vos décisions seront traitées de façon anonyme. Vous indiquerez vos choix à l'ordinateur devant lequel vous êtes assis(e). Au cours de chacune des périodes, vous obtiendrez des gains, exprimés en écus. A la fin de l'expérience, une des périodes sera choisie au hasard et les gains de cette période seront convertis en euros selon un taux de conversion qui est précisé à la fin de ces instructions. Votre gain en euros vous sera versé en liquide. A partir de maintenant nous vous demandons de ne plus parler. Si vous avez une question levez la main et un expérimentateur viendra vous répondre en privé.

Cadre général

Vous êtes 20 participants dans la salle. L'expérience comporte plusieurs périodes. Chaque période se déroule de manière identique.

Au début de chacune des périodes l'ordinateur central va former aléatoirement 10 groupes de 2. Vous ne serez jamais deux fois avec le même partenaire. Vous ne pouvez pas identifier votre partenaire et ce dernier ne peut pas vous identifier.

A chaque groupe de deux, l'ordinateur associe aléatoirement un autre groupe de deux. Le groupe concurrent.

Au début de chaque période, vous, ainsi que chacun des participants, disposez de 100 jetons que vous devez entièrement répartir entre deux comptes : votre compte individuel et un compte collectif commun à tous les membres du groupe.

Fonctionnement des comptes

Compte Individuel

Figure S8: Experimental Instructions (Competitive Sessions), cont'd

Chaque jeton que vous placez sur votre compte individuel vous rapporte 1 écu. De même, si votre partenaire place un jeton sur son compte individuel cela lui rapporte 1 écu.

Les gains de votre partenaire ne sont pas affectés par le nombre de jetons que vous placez sur votre compte individuel. De même votre gain n'est pas affecté par le nombre de jetons placés que votre partenaire place sur son compte individuel.

Exemple 1: Quelles que soient les décisions de votre partenaire sur son compte individuel, si vous placez 30 jetons sur votre compte individuel, votre gain résultant de cette décision sera de 30 écus. Les gains de votre partenaire ne seront pas affectés par votre décision.

Exemple 2: Supposons que votre partenaire place 10 jetons sur son compte individuel, quelle que soit votre décision de placement sur votre compte individuel, son gain résultant de cette décision sera de 10 écus; votre gain ne sera pas affecté par cette décision.

Compte Collectif

Le compte collectif est commun aux deux membres du groupe, c'est à dire vous et votre partenaire.

Chaque jeton que vous placez sur le compte collectif rapporte 1.5 écu au compte collectif.

La rémunération que vous obtenez du compte collectif dépend du montant investi par votre groupe mais également du montant investi par le groupe concurrent sur son compte collectif :

- Si votre groupe a investi plus sur son compte collectif que le groupe concurrent, alors votre rémunération du compte collectif est la moitié des écus se trouvant sur le compte collectif.
- Si votre groupe a investi exactement autant que le groupe concurrent, alors votre rémunération du compte collectif est la moitié des écus se trouvant sur le compte collectif.
- Si votre groupe a investi strictement moins que le groupe concurrent, votre rémunération du compte collectif est de zéro écu.

Afin d'illustrer le calcul du gain résultant de ce compte, considérons que vous êtes le joueur 1 du groupe et votre partenaire le joueur 2. Notons $\boldsymbol{Z_1}$ le nombre jetons placés par le joueur 1 (vous) sur le compte collectif, $\boldsymbol{Z_2}$ le nombre de jetons placés par le joueur 2 sur le compte

Figure S8: Experimental Instructions (Competitive Sessions), cont'd

collectif. Le nombre total de jetons sur le compte collectif est égal à $\boldsymbol{Z_1 + Z_2}$ ce qui donne $\boldsymbol{1,5 \times (Z_1 + Z_2)}$ écus sur le compte collectif.

Notons $\boldsymbol{X_1}$ le nombre jetons placés par le joueur 1 du groupe concurrent sur leur compte collectif, $\boldsymbol{X_2}$ le nombre de jetons placés par le joueur 2 du groupe concurrent sur leur compte collectif. Le nombre total de jetons sur le compte collectif est égal à $\boldsymbol{X_1 + X_2}$ ce qui donne $\boldsymbol{1,5 \times (X_1 + X_2)}$ écus sur le compte collectif.

Si le nombre total d'écus sur votre compte collectif, $\boldsymbol{1,5 \times (Z_1 + Z_2)}$, est plus important ou égale au nombre total d'écus sur le compte collectif du groupe concurrent, $\boldsymbol{1,5 \times (X_1 + X_2)}$, alors votre rémunération du compte collectif est la moitié de ce qu'il y a sur votre compte collectif : $\frac{\boldsymbol{1,5 \times (Z_1 + Z_2)}}{\boldsymbol{2}}$. **Dans le cas contraire votre rémunération du compte collectif est de zéro écu**.

Exemple 1: Vous placez 60 jetons sur le compte collectif ($\boldsymbol{Z_1}$ = 60) et votre partenaire place zéro jeton sur le compte collectif ($\boldsymbol{Z_2}$ = 0). Votre compte collectif a un solde égale à $\boldsymbol{1,5 \times (60 + 0)}$ soit **90 écus**. Dans le groupe concurrent, le premier joueur a placé 45 jetons sur le compte collectif ($\boldsymbol{X_1 = 45}$) et le joueur 2 a placé **35** jetons, ($\boldsymbol{X_2 = 35}$), le solde de leur compte collectif est de $\boldsymbol{1,5 \times (45 + 35)}$ soit **120** écus. Le solde du compte collectif du groupe concurrents étant supérieur au solde de votre groupe (**120**>**90**). La rémunération du compte collectif est nulle pour vous et votre partenaire.

La rémunération totale de cette étape est donnée par l'addition des rémunérations du compte individuel et du compte collectif :

$$\textbf{Pour vous : } \boldsymbol{100 - 60 + 0 \times \frac{1,5 \times (60+0)}{2} = 40}$$

$$\textbf{Pour votre partenaire : } \boldsymbol{100 - 0 + 0 \times \frac{1,5 \times (60+0)}{2} = 100}$$

Exemple 2: *Vous* placez 20 jetons sur le compte collectif ($\boldsymbol{Z_1}$ = 20) et votre partenaire place 50 jeton sur le compte collectif, donc $\boldsymbol{Z_2}$ =50. Votre compte collectif a un solde égale à $\boldsymbol{1,5 \times (20 + 50)}$ soit 105 écus. Dans le groupe concurrent les deux joueurs ont placé chacun 20 jetons sur le compte collectif ($\boldsymbol{X_1 = X_2 = 20}$), le solde de leur compte collectif est de $\boldsymbol{1,5 \times (20 + 20)}$ soit 60 écus. Le solde du compte collectif du groupe concurrents est inférieur

Figure S8: Experimental Instructions (Competitive Sessions), cont'd

au solde de votre groupe (60<105). La rémunération du compte collectif est de $\frac{1,5\times(20+50)}{2}$ pour vous et votre partenaire.

La rémunération totale de cette étape est donnée par l'addition des rémunérations du compte individuel et du compte collectif :

$$\textbf{Pour vous : } 100 - 20 + \frac{1,5\times(20+50)}{2} = 132,5$$

$$\textbf{Pour votre partenaire : } 100 - 50 + \frac{1,5\times(20+50)}{2} = 102,5$$

Pour résumer, à chaque période, chaque membre du groupe (vous y compris) a deux sources de gain: le gain résultant de son placement sur son compte individuel et le gain résultant du nombre total de jetons placés par les 2 membres du groupe sur le compte collectif. Les gains liés à l'investissement sur le compte collectif dépendent de l'investissement réalisé par le groupe concurrent.

Le taux de conversion entre écu et euro est 0.1 euro pour 1 écu.

Le gain forfaitaire est de 5 €.