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The Effect of Social Context on Corruption Propensity

Is corruption self-reinforcing? Can social context make people relatively more (or less) likely to benefit themselves at the expense of the group? Will people be more likely to restrain themselves if they think others are doing the same? If others are benefitting unjustly, does that make individuals less likely to uphold group norms? This paper will use a survey experiment to examine the theory of self-reinforcing corruption.

The theory of self-reinforcing corruption is built on dynamic strategic complementarities. In brief, it has been proposed that people take others' past behavior into account when deciding whether or not to engage in corrupt behavior. (Aidt 2003) Future corruption levels, on this understanding, are partly determined by past corruption levels, with the possibility for multiple equilibria (Tirole 1996). The return to rent-seeking relative to entrepreneurship is relatively higher in contexts where most people seek rents and accept bribes (Aidt 2003 citing Murphy et al., 1991, 1993; Acemoglu 1995). According to this theory, the willingness of a particular individual to engage in corrupt acts would depend on their perception of how their peers would behave in a similar situation. Limited prior research has been consistent with this theory (Corbacho 2016). As a result, I hypothesize that individuals who perceive relatively higher levels of corruption among their peers will show increased willingness to engage in similar behavior.

The present paper proposes to test this hypothesis with a survey experiment in the United States. The experiment manipulates respondents' beliefs about the prevalence of corruption in the United States via an informational treatment. The survey is to be administered in-person to 5,000 individuals selected randomly from 250 randomly-selected zip codes throughout the

United States.[1] It would be ideal to attach this survey to an unrelated broad-based in-person survey such as Pew's "Trust, Facts and Democracy" survey (Pew 2018), its "American Trends" panel (Pew 2018a) or the Gallup News Service (Gallup 2018). If this proves impossible, the survey will include a series of desensitization questions between the informational treatment (see below) and the substantive survey questions. The theoretical treatment of interest is individual perceptions of peer corruption.

The experiment will use a randomly-selected cluster sample of 5,000 US adults drawn from all 50 US states, with probability proportionate to population density. The theoretical population will be adults in the United States, the study population will be adults in the 250 zip codes where the survey is administered (primary sampling unit), and the sampling frame will be adults in those zip codes who have filed a tax return.[2] From this sampling frame 5,000 adults will be selected at random, with assignment of 2,500 to the treatment group and 2,500 to the control group.

The two-stage sampling procedure used in this experiment will draw a systematic simple random sample (SRS) within clusters, and will not be stratified.[3] Similarly, there is no reason to use a block design with such a large n. Random assignment to the treatment and control groups will achieve probabilistic equivalence on observable and unobservable characteristics and reinforce internal validity. Cluster randomization involves dividing the population into clusters (zip codes), but the two-stage sampling procedure then calls for randomly sampling within each cluster (Trochim and Donnelly 2007). There is no need to employ a factorial design given the structure of the survey questions. A pre-analysis plan will be created and registered[4] to improve

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[1] Out of 42,000 zip codes in total - 0.35%.

[2] "How Two Economists Got Direct Access to IRS Tax Records." *Science*, May 27 2014

[3] $SE = \sqrt{\frac{N-n}{N} s^2/n}$

[4] With the JPAL hypothesis registry - https://www.povertyactionlab.org/Hypothesis-Registry

confidence in the results, describing the sample to be used, how the variables will be constructed, and specifying the treatment effect equation to be estimated (Humphreys et al. 2012).

I propose to create exogenous variation in beliefs about the prevalence of corruption by randomly assigning subjects to three different informational treatments. The "high corruption" treatment will consist of presentation via tablet of a short video informing subjects that 74% of Americans are "worried or very worried" about corruption, mentioning some prominent cases (Demos 2018) and stating that "more Americans than you think" are willing to pay bribes to get ahead. It will go on to claim that "60% of Americans believe that corruption has increased a lot over the past two years, while only 10% say it has decreased", "69% of Americans say corruption is a problem/serious problem" and "55% of Americans feel that public officials are corrupt/extremely corrupt" (Transparency International 2017). The "low-corruption" treatment will present a different video reversing these claims, stating that "many Americans believe corruption has decreased in the past two years" and "many Americans don't see corruption as a serious problem", citing strides made in transparency and accountability.[5]

The control group will not be exposed to these videos. After administration of the treatment, the survey team will verbally present all three groups with a series of orthogonal desensitization questions (carefully selected to minimize priming effects) followed by these substantive survey questions:

On a scale of 1-10 (10 most likely), how likely would you be to:

1) Pay a bribe to speed up a utility connection

2) Pay a bribe to win a government contract

3) Pay a bribe to get out of a speeding ticket

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[5] Claims are drawn from the same data, but the emphasis varies by treatment.

Research questions will be mixed with extraneous questions designed to elicit minimal reaction to minimize adverse reactions from study participants. If the hypothesis above is correct, subjects administered the "high corruption" treatment would respond to each of these questions with relatively higher numbers than controls, while the "low corruption" group should respond with relatively lower numbers than controls. The proposed mechanism is that the informational treatment primes the recipient with the expectation either that corruption is increasing and that her peers are less trustworthy and more likely to bribe to get ahead, or the reverse. This expectation is hypothesized to result, per the theory of self-reinforcing corruption, in respectively greater and lesser reported willingness to bribe than the willingness manifested by controls.

This experiment is a three-group, posttest-only randomized experiment. This design is related to the two-group, posttest-only randomized experiment, known to be the strongest of all research designs with respect to threats to internal validity, particularly against single-group threats (Trochim and Donnelly 2007). The internal validity of this survey experiment should be strong, as we can expect that random assignment to treatment will result in groups that are similar on both observable and unobservable characteristics. However, this design can be vulnerable to *social* threats to internal validity in the presence of comingling between treatment and control subjects. To address this, the present experiment uses a very broad sample frame, which should reduce social threats because it will be unlikely that treatment and control groups will interact. Design notation for this experiment is below:

$$\mathbf{R} \qquad \mathbf{X_H} \qquad \mathbf{O}$$

$$\mathbf{R} \qquad \mathbf{X_L} \qquad \mathbf{O}$$

$$\mathbf{R} \qquad \qquad \mathbf{O}$$

The background concept to be examined is corruption, specifically levels of popular corruption (as distinct from elite corruption). We are interested in the systematized concept of "willingness to bribe," which has been studied extensively in the economics and political science literature (Corbacho et al. 2016, Shleifer and Vishny 1993). We are interested in the indicators of "willingness to bribe public officials" in three contexts: utility connection, government procurement and petty law enforcement. Scores in individual cases will be between 1 and 10. This research design attempts to ensure high construct validity, as the operationalization of willingness to bribe does seem to get at the heart of popular corruption, in that those who can be said to have bribed public officials constitute one of the main examples of popular corruption. Content validity appears to be strong because the main attributes of corruption are captured without capturing anything else.

This survey experiment will involve random assignment to treatment and control groups. Data produced will be experimental but intrusive, risking Hawthorne effects. These can be minimized by incorporation into a larger survey, as discussed above. We are interested in the Intention to Treat effect (ITT), rather than the Treatment-on-Treated effect (TOT).

The specification of adults who have filed a tax return is broad but not total – it leaves out adults in institutional care or who are unable to work, as well as many young adults in the 18-25 range.[6] The sampling bias introduced by this undercoverage will be small and should not meaningfully affect the conclusions, though if necessary we could employ a post-hoc weighting scheme to adjust for the relative scarcity of young adults in the data, since they would be likely to behave differently than the population as a whole. Sampling error will be normally distributed, with a mean approximately equal to that of the study population.

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[6] Exclusion of children is by design, as they are unlikely to have sufficient experience to participate meaningfully.

Bribery is a sensitive subject, and there is evidence that direct questioning on sensitive issues leads to under-response (Treisman 2007). To anticipate this concern, the present experiment incorporates indirect questioning techniques along the lines suggested by recent research (Rosenfelt et al. 2016). Subjects will be desensitized via alternative lines of questioning between presentation of the exogenous information and presentation of the survey questions.

Construct validity is threatened by the fact that the systematized concept of "willingness to bribe" is narrower than the background concept of corruption. This is a common weakness of survey experiments. Relatedly, translation validity is also limited by the breadth of the construct, with strong face validity but weaker content validity as the systematized concept fails to capture every aspect of the construct. However, the measures adequately capture the systematized concept. Construct validity could be enhanced by more closely approximating subjects' state of mind when they are considering whether or not to engage in corrupt behavior, which is difficult in a survey context. Future research could improve construct validity by conducting this experiment immediately following interactions with police officers and public officials, but the human subjects issues involved preclude these measures here.

Conclusion validity should be relatively strong because of the large sample size despite the effects of clustering, but as always, power could be increased with a higher n. The sample size is large enough that a null effect, if produced, will constitute a "meaningful zero". Risk of Type II errors should be low. We could insulate further against Type II errors by reducing the response parameters from a 10-point scale to a 5-point scale. Compliance will be greatly simplified if the experiment is linked to a broader survey, but will still be an issue. Wealthier individuals may still decline to participate, as well as those with busy schedules. However, these defections should be limited by a small number of wealthy individuals in the sample and by

persistent efforts on the part of experimenters to reach busy subjects. There will likely not be any difference in compliance between the treatment group and the controls, as both are embedded in a larger survey.

The external validity of survey experiments has been questioned, and it has been alleged that they can lack realism and that stimuli presented to treatment groups can be stronger than those found in comparable natural experiments (Barabas and Jerit 2010). However, while treatment effects in survey experiments do tend to be larger than the effects observed in natural settings, "what occurs in survey experiments resembles what takes place in the real world" (Barabas and Jerit 2010). Determination of the treatment and control groups by simple random sample indicates strong external validity (Trochim and Donnelly 2007). External validity could be further improved via replication.

Despite the inherent external validity limitations of survey experiments, this experiment presents strong internal, construct and conclusion validity, and should yield valuable insight into the theory of self-reinforcing corruption. Future research could focus on the time-sensitivity of the corruption impression, on comparing the effects of various priming techniques, and on modeling the network effects as a self-reinforcing feedback loop.

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