ABSTRACT

Title of dissertation: ESSAYS IN EXPERIMENTAL ECONOMICS WITH IMPLICATIONS FOR ECONOMIC DEVELOPMENT

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In an exploration of the joint concerns of economic development, namely efficiency and equality, I employ experimental methods to consider several issues regarding entrepreneurship and regulation with particular applications in developing countries.

Entrepreneurship programs in developing countries may not take hold in rural populations if people there tend to shy away from competitive and uncertain economic opportunities, thus contributing to the systematic underdevelopment of rural areas. In a field experiment conducted among potential entrepreneurs in rural and urban Ghana, we found that rural subjects were 20 percent less likely than their urban counterparts to select an all-or-nothing tournament compensation scheme over a piece rate wage to per- form a simple matching task. The difference between the rural and urban tournament choice was driven by subjects who believed their own performance was the best within their group; urban subjects were twice as likely as their rural counterparts to believe that they had scored in first place and were thus more likely to select the tournament compensation.

To examine behavior in a tax setting, we develop a simple tax evasion model as a signaling game between a taxpayer and an auditor that includes a non-strategic, always compliant taxpayer. In addition to the taxpayer's income report to the auditor, he has the option to send a costly message, a donation to charity that may serve as an indirect signal to the auditor of the taxpayer's ethical type. In the case where the taxpayer has misreported his income and is audited, he must pay unpaid taxes and a penalty. We establish a Perfect Bayesian equilibrium where taxpayers will use the charitable donation to signal honesty, thereby reducing the probability of audit. Auditors will optimally audit reports without charity donations more frequently than those with donations. To test our theoretical predictions, we use a two-sided signaling experiment where the taxpayer voluntarily reports his income to determine his tax liability and can make an observable and verifiable charity donation. Our aggregate experimental results indicate players employ mixed strategies in line with theoretical predictions.

ESSAYS IN EXPERIMENTAL ECONOMICS WITH IMPLICATIONS FOR ECONOMIC DEVELOPMENT

by

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Chapter 1

Private Sector Lead Growth: Entrepreneurship, Institutions and Development

1.1 Introduction

In recent years, development agencies and governments alike agree that building a strong local private sector should figure prominently as a sustainable means to achieve a number of development goals. The recent and largely unexpected telecommunications boom in Africa provides a striking example of the vast untapped economic potential on the continent. Beyond the governments' initial allocation of the spectrum, the industry's growth was marked by the emergence of large-scale indigenous entrepreneurs that had not been seen before in Africa [Makura, 2008]. The size and scope of the telecommunication industry's development in Africa has extended beyond enriching the initial investors and entrepreneurs; improved communication has benefited all groups in society, with improvements in the transmission of economic information to communication technology's role in holding leaders accountable.

If entrepreneurs constitute the group best suited to identify under-served or suppressed markets and to introduce new technologies to serve them, the need to better understand the institutional and behavioral catalysts of entrepreneurship generates a rich and important set of research questions with significant implications on developing country economics.

In the development context, the private sector's unmatched ability to drive improved market efficiency, must be paired with distribution mechanisms that are able to achieve the ultimate development goal, poverty reduction.

In an exploration of the joint concerns of economic development of efficiency and equality, in this chapter, I consider several issues covered in the literature regarding entrepreneurs in developing countries and some of the challenges they face due to burdensome regulation, financial and other institutional constraints. The following chapters both employ experimental methods to conduct a focused analysis of some issues that are relevant in the developing country context. The experiments provide a useful methodology for measuring preferences that are otherwise difficult to quantify with standard empirical data sources such as surveys or macro data in a controlled setting. Further whereas in Chapter 2 where no clear theoretical predictions are forthcoming, in Chapter 3, I consider the alternative case, where we establish a very clear theoretical benchmark which can be tested in a laboratory setting. Finally, experiments offer clean comparisons to similar studies. In Chapter 2, which is based on joint work with Erkut Ozbay, I explore a potential behavioral barrier to entrepreneurship, lack of competitiveness, by comparing the preferences over competitive compensation schemes in urban and rural Ghana. Chapter 3, also based on joint work with Ozbay, take a more general approach to an issue that affects the primary redistribution mechanism across economies: taxation. In a general laboratory study of tax evasion, we tested whether charitable donations have any effect on the truthful reporting of income by experimental subjects and whether this behavior is predicted by the theory.

1.2 Entrepreneurship in Developing Countries

Informal self-employment activities in developing countries constitute the primary source of income for many, where the economies are characterized by limited formal sector employment opportunities, underdeveloped financial services, weak legal systems and host of other institutional shortcomings. Formalization of the businesses that operate outside of the official system has been suggested [De Soto, 2000] as an important catalyst for economic growth and recent policy efforts to register informal businesses reflect the widespread acceptance of this notion. Informal businesses comprise a large part of economic activity and engage high proportions of the labor force in many countries, therefore the anticipated benefits of formalization make understanding the obstacles faced by informal firms in their path to the formal sector an important policy consideration.

In a recent study using firm-level data for the informal sectors in Ivory Coast, Madagascar and Mauritius [Amin, 2010], the motivation of the firm owner, that is whether he was an entrepreneur out of necessity or opportunity, had a significant impact on his perceived severity of the obstacles to formalization, such as registration fees, taxes and the effort required to gather information. Though many owners of informal businesses are innovators who exploit new opportunities, fitting the Schumpeterian definition of an entrepreneur, others run their business out of necessity due to lack of alternative employment. Of the 300 firms surveyed, 42 percent were characterized as being run by a necessity entrepreneur.

Firm-level data from emerging economies has enabled research that explores the characteristics of firms and obstacles that have not been available for systematic analysis in the past. However, data limitations have prevented researchers from gaining a more objective understanding of the barriers to formalization. Experimental methods applied in laboratory and field settings can be used to identify what might be driving

the difference in the perception of obstacles.

If there are systematic differences in perceptions of economic obstacles between entrepreneurs motivated by necessity and opportunity, perhaps there are other important differences with serious implications for development. An auxiliary question arises: is it better for society to have necessity or opportunity entrepreneurs? Do the traits cultivated through necessity entrepreneurship develop commitment, hard work and cooperation or do they lead to a more survivalist self-interested view of the world that hinders their willingness to contribute to public goods?

In a review of the theoretical and empirical literature on the role of entrepreneurship in development, Naudé [2008] finds that government policies designed to foster entrepreneurship have ambiguous effects on growth, dependent upon the type of new venture being promoted and the local entrepreneur's ability to implement innovative and productive new businesses. One contribution of this research is toward establishing further exploring the necessity v. opportunity distinction and any behavioral regularities among these groups that may foster the development of effective entrepreneurship policies, particularly in regards to formalization. While the entrepreneur as the jack-of-all trades [Lazear, 2005] may be an accurate characterization in a developed country, in a developing country educational ability may provide a better measure of entrepreneurial skill as the nature of the business opportunities are different. Robson et al (2009) find that the education level of small and medium scale entrepreneurs in Ghana is positively correlated with the innovativeness of their business.

1.3 Regulation, Incentives and Private Sector Growth

The empirical link between excessive regulation and low quality institutional measures such as rule of law, control of corruption and enforceability of contracts is well established in the literature. Djankov et al. [2002] find strong empirical support for the public choice view of regulation: they find that regulation serves to entrench market power of incumbents and to allow politicians to extract rents as opposed to the public interest view where regulation leads to improved product quality and protection from market failures. High levels of regulation are correlated with the presence of unofficial economies and high levels of corruption, meanwhile the quality of goods is not superior to that of low regulation countries. However, in a cross-country empirical study, [Klapper et al., 2006] find that regulation is a barrier to entrepreneurial entry in rich, low-corruption rather than poor, high-corruption countries; that is, the causal effects of regulation on entry seem to be limited to wealthy countries without corruption. In transition economies, if not more important than official regulation is how regulation is actually implemented, which is closely tied to measures of institutional quality [Johnson et al., 1998].

The opportunities for new market development and large profits are the most abundant in the developing world. Whether regulation is a barrier to entry for "pioneer" entrepreneurs, those who are introducing a new product into the local market, is therefore an interesting question from a development perspective. Given the inconclusive empirical findings regarding the causal effects of regulation on new firm entry in developing countries, a theoretical approach is justified to establish an alternative hypothesis regarding the primary determinants of entrepreneurial entry.[1]

Baumol [1990] was the first to introduce the dimension of entrepreneurial effort

[1] Other well established determinants of entrepreneurial entry include access to financing, labor market regulation and taxation.

allocation. Further, he asserted that the types of activities that entrepreneurs engage in may be productive, unproductive or destructive depending on the institutional quality of the economy. Hillman et al. [2001] establish the equilibrium allocation of resources between cost reduction and lobbying in an oligopolistic industry and find the results are sensitive to the relative lobbying abilities of firms.

Chapter 2

Competition in the City: Experimental Evidence from Rural and Urban Ghana

2.1 Introduction

Entrepreneurs have long played a major role in economic systems and relatively recently in the developing world, policy makers have turned to entrepreneurs to spur the high growth necessary to significantly improve living standards [see e.g. IFC, 2008; Petrin, 1994]. The focus on entrepreneurship as a path to economic growth may further contribute to the urban development bias, where rural areas have consistently fallen short of development gains in urban areas, if rural people are less likely to become entrepreneurs.

The persistence of rural poverty in many developing countries underlies the basic thesis of the "urban bias" theory put forth by Michael Lipton [1977], which asserts that disproportionate concentration of political and economic power in urban areas favors development policies that benefit urban areas at the expense of rural ones. This tendency leads to low public investment, unfavorable terms of trade and exchange rate policies and systematically lower (and inefficient) development outcomes in rural areas.

Whether the urban bias hypothesis is the mechanism that explains the rural-urban development gap is still a subject of debate [Varshney, 1993, Corbridge and

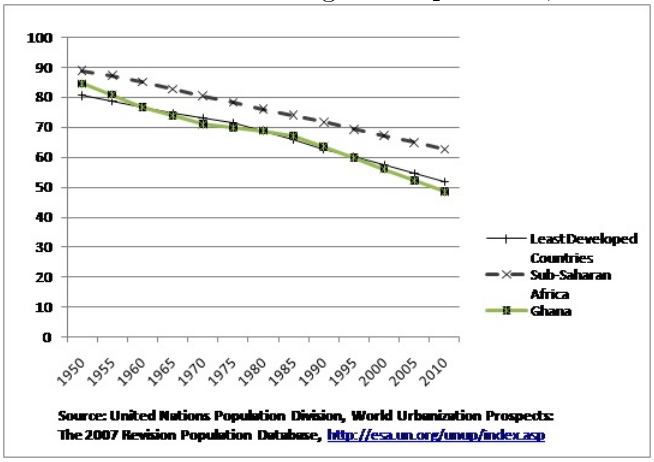
Jones, 2005], but the existence of the rural-urban gap is not. Though recent studies have shown no global trends in the narrowing or widening of the rural-urban gap [Eastwood and Lipton, 2000] and that empirical evidence of over-urbanization and excessive competition for resources in urban areas indicates a complex, country-specific macroeconomic relationship between urbanization and economic development [Bradshaw, 1987].

Many agree that Sub-Saharan Africa suffered acutely from urban bias and a large rural-urban welfare gap through the 80's [Corbridge and Jones, 2005]. Structural adjustments in the 1990's narrowed the rural-urban gap as public spending cuts were made, but the rural–urban welfare gap remains high across a number of dimensions [Sahn and Stifel, 2004], growing urban poverty levels notwithstanding.

After having held three peaceful democratic elections, Ghana, with a population of just under 25 million [World Bank, 2009], is one of West Africa's stable democracies and by all accounts, development indicators have shown improvement over the past decade. Flows of foreign direct investment (FDI) have been growing steadily from $150 million in 2005 to $435 million in 2006 and $435 million in the first quarter of 2008 alone [UNCTAD, 2009]. With the rapid acceleration of FDI flows in Ghana, local entrepreneurs will play an important role in terms of "absorptive capacity" [Cohen and Levinthal, 1990], that is the ability to incorporate and adapt new technology that accompanies the FDI.

The steady development in Ghana has been accompanied by urbanization rates characteristic of the region, roughly doubling to 46.3 percent [UNEP, 2009] in the past 50 years. The country's declining rural share of the population is in line with the trend in other developing countries and Sub-Saharan Africa (Figure 2.1). The rural-urban welfare gap in Ghana is driven primarily by the rural lack of access to commercial activity and basic services. Estimates from 1990's data show that the

Figure 2.1: Rural Percentage of Population, 1950-2010



rural-urban consumption gap is around 40 percent. Across other welfare measures such as health, education and access to sanitation, the persistent development gap in Ghana is not generally worsening, but does not show signs of closing [Boakye-Yiadom, 2004, Sahn and Stifel, 2004].

Even with the steady decline in the rural population share and increased urban migration, it is customary in West Africa, Ghana as well, for urban people to have very close ties to their rural "villages" sending remittances and making frequent visits [Aldous, 1962, Geschiere and Gugler, 1998]. Far from disconnected, as is more the case in the rural-urban dynamics in developed countries, the two groups have frequent contact, which makes the interpretation of our observed behavioral differences between urban and rural subjects that much more striking.

In examining the differences in preferences over compensation schemes in a field experiment, we are capturing a new dimension of the rural-urban disparity beyond the typical development indicators. Whether the city engenders behavioral changes or attracts those individuals who possess certain preferences, understanding the adjustment in norms regarding preferences for competition in economic settings will be of critical importance to employers and policy makers. With the experimental results

we may be able to examine some endogenous determinants of the "urban bias."

Despite the increasing urbanization of West Africa in recent decades, much of the field work in Africa continues to concentrate on rural village communities. Our research generates a rich experimental data set for an under-researched, but very important and growing demographic group of urban Africans. The results will motivate further study into the causes, costs and benefits of preferences for competition and address broad questions regarding the interrelationship between urbanization and economic development in Africa.

We used experimental techniques to assess whether rural and urban populations in Ghana exhibit distinct preferences to engage in a risky, performance-based tournament in order to identify any differences between the groups in some important traits typically associated with entrepreneurs.

Our experimental data show that when presented with the choice between performing a simple task, 1) for a piece rate wage or 2) in an all-or-nothing tournament, only 30 percent of the subjects sampled from representative rural and urban areas in the North and South of Ghana chose to enter the tournament. The tournament entry percentage was substantially lower than that of similar experiments conducted both in industrialized countries [Gupta et al., 2005, Niederle and Vesterlund, 2007] and in traditional cultures in developing countries [Gneezy et al., 2009]. We also identified a large difference in the tournament entry decision between rural and urban subjects: urban subjects were 20 percent more likely to choose the tournament than their rural counterparts. We attribute the overall low tournament entry primarily to rural subjects lower confidence in terms of relative performance.

Given our motivation, which is to see whether competitive preferences differ by rural and urban areas with implications on entrepreneurship and the development of new business ventures, our experimental design has rural(urban) people competing other

rural(urban) people. The within group competition is justified as the entrepreneurs would be competing locally. This design feature contrasts with the gender and competition studies [Gneezy et al., 2009, Gupta et al., 2005, Niederle and Vesterlund, 2007], where the two groups of interest, men and women, compete within experimental sessions. The motivation of the gender studies in explaining gender differences in competitive preferences in the workplace supports the mixed gender sessions, where the interaction between the genders is important in establishing the external validity of the experiment.

In simulating the entrepreneurial environment with an artefactual experiment, we present the experimental subjects with a new, unfamiliar and relatively abstract task, in the sense that the task is being completed for its own sake with no real life implications. However, the abstract environment may be particularly relevant given that entrepreneurship frequently involves untested ideas where people may have little experience. The ability to identify and exploit opportunities and to perform amidst risk and uncertainty are personal attributes frequently associated with entrepreneurs. The simple experiments, described in detail in Section 3, were conducted in two cities, Accra and Tamale and two towns, Nynkapala and Osino in Ghana to establish a baseline understanding for preferences for competition. In Section 4, we discuss the developing country field setting and identify demographic characteristics of the subjects. Section 5 provides an analysis of our key findings regarding the determinants of competitive behavior and some of the associated costs. Section 6 concludes with a discussion regarding the implications of our findings on development and proposals for follow-up experiments to answer any questions that arise from the analysis in Section 5. The following section provides a summary of related work.

2.2 Related Literature

Recent experimental research has used tournament choice to illustrate differences in preferences over competitive situations across cultures, genders and occupations [Gneezy et al., 2009, Gupta et al., 2005, Niederle and Vesterlund, 2007, Carpenter and Seki, 2005]. While the experiments elicit preferences over competitive compensation schemes, they also measure attitudes toward confidence, risk, uncertainty and performance under pressure. The ability to identify and exploit opportunities and to perform amidst risk and uncertainty are personal attributes frequently associated with entrepreneurs in the economics and psychology literature [Kihlstrom and Laffont, 1979].[1]

Bewley [1989] shows a theoretical link between low uncertainty aversion and business innovation in entrepreneurship. In a study of Indian small and medium scale entrepreneurs, Natarajan [2005] finds that tolerance for competition is a key characteristic of all of those surveyed. Though certain characteristics of entrepreneurs may not be robust to cultural comparisons as Thomas and Mueller [2000] demonstrate empirically, they do find that within a given culture certain characteristics can distinguish the set of entrepreneurs from the rest of the population.

Little theoretical or empirical work has focused explicitly on the characteristics of developing country entrepreneurs [Leff, 1979, Naude, 2008] who face very different regulatory and credit constraints than their developed country counterparts and who may potentially different attitudes toward innovation, competition and risk. For example, Blanchflower and Oswald [1993] find a significant empirical positive link

[1] Busenitz [1999] hypothesizes that entrepreneurs are not less risk averse than the general population but their reliance on heuristics and biases may make them appear to be more likely to take on risk than others.

between a lack of capital constraints and a person's status as an entrepreneur; no such link is found with their measure of psychological characteristics. However, the context is clearly for that of employed, developed country individuals, with primary motivations for becoming an entrepreneur being freedom and flexibility. It is clear that in a given developing country, more will need to be understood regarding the motivations of local entrepreneurs before we can establish which results from developed countries are relevant in the developing country context.

In a meta-analysis of entrepreneurship selection, Van der Sluis et al. [2005] find the effect of education on worker choice between self-employment and wage employment stronger in urban areas, the least developed economies and those that are heavily dependent on agriculture, much like Ghana. In a comprehensive study of Ghanaian entrepreneurs, Robson et al. [2009] find that small and medium scale entrepreneurs tend toward incremental product development, not large innovation and that the innovativeness of entrepreneurial activities is positively correlated with the entrepreneur's education level. These findings motivated our subject selection which is discussed in detail in Section 4.

2.3 Experimental Design

The experimental design enabled us to measure preferences for competition while controlling for performance, confidence and ambiguity aversion similar to Gupta, Poulsen and Villeval [2005] and Niederle and Vesterlund [2007]. We use a novel task, which will enable us to measure the preferences for competition in this developing country context.

Subjects performed a simple task under a piece rate payment scheme, followed

by a tournament round. Subjects were then asked to choose between the piece rate and the tournament pay before to be applied to the third round of play. No specific skills or training would have favored anyone beyond the general ability to identify and match. However, successful performance required a combination of ability and effort.

Identification and Matching Task

We designed a task and an environment that would be appropriate for local subjects and that would facilitate accurate monitoring. In each city, we constructed 8 work stations to ensure privacy for each subject to simultaneously complete the task. Because the task was not computerized, we were concerned with achieving uniformity between subjects. To minimize variation due to monitoring, the same researcher and a local assistant each monitored 4 subjects.

Upon arrival, we informed the subjects that each subject would receive a 2 cedis show-up fee and an additional 3 cedis for completing the experiment. After a brief explanation of the experimental procedure, subjects read and signed consent forms followed by a brief demonstration. They were informed that they would perform the task 4 times and would be given specific instructions immediately prior to playing.[2] We also told them that one of the tasks would be selected at random to determine their payoff to ensure maximum effort in all tasks. At the conclusion of each round, subjects were shown only their own performance at the conclusion of each task. They did not know aggregate result.

At his private workstation, each subject was provided with a 1 quart basket containing an identical mixture of familiar objects. Each basket contained 14 uniquely

[2]Subjects actually performed the identification and matching task 3 times, but we did not inform them of this at the outset.

identifiable objects,[3] where each unique object was in the mixture at least once (e.g. magnet) up to 35 times (e.g. pasta), making the total number of objects in each basket approximately 200. At the beginning of each task round, each subject was provided with an identical picture of 21 of the objects in the basket placed in a numbered linear order.[4] Subjects were then given 60 seconds to place the items from their basket in the order indicated by the picture. At the end of 60 seconds, time was called and all subjects were required to stand in a holding area while the monitors scored and cleared each workstation. Scores were calculated based on number of objects correctly placed according to the numbered sequence in the picture, measuring both speed and accuracy. Just prior to each task, the specific compensation scheme was explained in detail, both in English and in a local language, where necessary.[5]

Task 1, *Piece Rate*: Subjects were given 60 seconds to match as many items in their basket to the corresponding numbered sequence distributed at the beginning of the round. If the task was randomly selected, the subject received 50 peswas per item correctly matched.[6]

Task 2, *Tournament*: Subjects were randomly assigned to groups of 4. Subjects were not told who was in their group, but they were be able to see all of the possible subjects who could possibly be included in their group. Subjects were then informed that in the tournament round, payment would based on relative performance within the randomly assigned group of 4. The highest performing subject in the group

[3]The objects varied in all dimensions, though most were some form of dried food: Bean (3 types), Dried Okra, Eraser, Magnet, Nail, Paper Clip, Pasta (3 types), Plastic Disc, Tamarind Pod and Toothpick. Object composition varied between Accra and Tamale as all objects were purchased locally. The differences between the Accra and Tamale basket composition were minor and cannot account for any Accra-Tamale differences in performance.

[4]The number 21 was chosen optimally, after initial testing of the task by the researchers and assistant, to be the lowest number of objects that would guarantee all subjects score below the bound in the 60 second time limit, with some added contingency.

[5]Instructions are available upon request.

[6]The Ghanaian currency is the cedi, 1.3 cedis = 1 USD. The smallest unit of the currency is the peswa, 100 peswas = 1 cedi.

would receive 2 cedis per correct answer and the others would receive zero. In the case of a tie, the winner was chosen at random from the high scorers. If each of the 4 competitors has a 25 percent chance of winning, then the tournament payoff is the same as the piece rate payoff, in expectation.

Task 3, *Payment Choice 1*: Subjects were asked before performing the task for a third time, the choice of payment scheme to be applied to the third round performance. They were given the choice between piece rate or tournament pay. For the tournament choice, individual performance in round 3 was compared to tournament performance in Task 2. That is to say, individuals who chose the tournament would not be competing directly with one another in Task 3, but with the outcomes of Task 2. This was to ensure that the subject's choice to enter the tournament was an individual choice that did not depend on others choice of tournament entry.

Task 4, *Payment Choice 2*: Upon completion of Task 3, subjects were asked whether they would like to be paid a piece rate or tournament wage for Task 1 performance. By giving the subjects the option to submit their past piece rate performance to the tournament, we have separated the choice of entering into the competition from the desire to actually perform in a competitive setting.

Beliefs-Assessment Finally, we asked each subject how they believe they ranked, from 1 (best) to 4 (worst) in Tasks 1 and 2, out of the group of 4 from the Task 2, tournament round. Subjects were paid 50 peswas per correct identification of rank. The responses to the questions regarding relative performance enabled us to gage confidence and how it relates to the decision to compete.

2.4 Field Settings and Subject Selection

The importance of field experiments for questions regarding preferences for competition should be clear given the strong cultural components that surround the way people compete in the marketplace. Because of the high inequality in Ghana, we risk too many confounding factors from education to income to levels of outside exposure would preclude our ability to attribute observed behavioral differences between subjects to "urban" or "rural". As a result, we selected subjects with relatively similar demographic characteristics such as age, education and income across the rural and urban areas. Because of the critical importance of development through the formal sector, the subject pool in our experiments was limited to current and potential formal sector workers; all subjects were literate and had experience with modernized non-physical labor either through secondary school or their current occupation. In addition, the working-class segment of the population, from which we drew our subjects, comprises a large group of unemployed and underemployed citizens whose labor prospects are closely connected to Ghana's development goals.

To identify and recruit the targeted subject pool, we hired recruiters in each experimental loaction. The recruiters were local residents who worked with foreign-based NGO's and who had experience recruiting community members for similar incentivized activities. The subjects that were selected, by and large, knew each other with the exception of Accra, where subjects were not familiar with one another.

The field experiments were conducted in four locations in rural and urban municipalities in the north and south. Ghana, like many of the countries in West Africa, has a clear social and cultural distinction between the coastal, predominantly Christian south and the Sahelian predominantly Muslim North.[7] To ensure that our experi-

[7] There are 10 regions in Ghana: Ashanti, Brong Ahafo, Central, Eastern, Greater Accra, Northern, Upper East, Upper West, Volta and Western.

mental subjects adequately represented the North-South divide and some of the other regional divisions, we conducted the urban experiments in the southern coastal capital city, Accra and the northern city of Tamale and the rural experiments in the southern town of Osino and a town outside of Tamale, Nyankapala.[8] Accra and the inland city of Kumasi are the two largest cities in Ghana with 1.7 and 1.2 million inhabitants. Tamale is distant third with 0.2 million inhabitants. We weighted the sample size from each region in order to reflect the actual population distribution.[9] We were able to determine the subject's home region from the short questionnaire administered after all experimental tasks were completed. Also, as our design is geared toward addressing entrepreneurship issues, it was important to have a representative cross section of the Ghanaian population to add to the generalizability of our conclusions.

Subject selection was motivated by actual labor force composition so as to draw inferences on the current and future labor force characteristics from our experimental results (Table 2.1). Our recruiting efforts targeted educated members of the formal sector work force and potential formal sector workers given our motivation of transformative entrepreneurship and the positive empirical link between innovativeness and education found in Ghanaian entrepreneurs [Robson et al., 2009]. With the help of our local assistants, we recruited subjects at post-secondary schools and area businesses.

The mean age of the subjects was 24. Rural subjects were slightly younger (23) on average than urban (25) subjects. All of the subjects were educated with 90 percent

[8]While nearly all of the subjects in the Accra experiments hailed from the Greater Accra Region, the Tamale subjects were from the Northern Region, including the capital city of Tamale, Upper East and Upper West.

[9]Approximately 70 percent of the population lives in the regions constituting the "South," including the capital city of Accra and the Ashanti capital Kumasi.

Table 2.1: Subject Composition and Self-Reported Demographic Information

	Pooled	Urban	Rural
Total Subjects	101	53	48
South (Accra, Osino)	69	37	32
North (Tamale, Nyankapala)	32	16	16
Percent Women	32	34	29
Mean Age	24	25	23
	(5.3)	(5.3)	(5.2)
Mean Number of Languages Spoken	3.2	3.4	3.0
	(1.2)	(1.1)	(1.3)

The ratio of Southern to Northern subjects 2:1 was chosen to roughly represent the regional population distribution of the country.
Income is in Ghana cedis. At the time of the experiment, the exchange rate was approximately 1.4 Ghana cedi to 1 USD.
Standard deviation in parentheses.

having completed secondary school many with further training, from technical school to university studies. The subjects also had significant exposure to various ethnicities other than their own with the self reported average number of languages spoken of just over 3.

Average subject income was 60 cedis so the payoff range of 5 to 48 represented significant stakes for a 30 minute work session. According to 2006 data, median formal sector wages in Ghana range from 21 USD per month in agriculture to 121 USD per month for civil servants. The median income for formal sector jobs is 41 USD per month [Nsowah-Nuamah et al., 2010]. All instructions were given in English, Ghana's official language, with some clarifications given by our local assistants in the local language.

Of the 101 subjects, 32 percent were women. Because we were not focusing on gender issues we did not select equal distribution of men and women but a represen-

tative sample of the pool of potential and current workers. The gender balance of our subjects reflected the actual formal sector labor force participation by gender. While women dominate the informal sector jobs in Ghana, they are outnumbered by men 2:1 in the formal sector.

2.5 Experimental Results

Aggregate results reported in Table 2.2 show that given the choice of performing the matching task for a piece rate or in a tournament with equivalent expected returns, very few subjects, 30 percent, chose the competitive tournament compensation. The tournament entry percentage is particularly low given our experimental set-up whereby subjects compete against the previous round results and do not have to compete against fellow subjects in real time. The low tournament entry percentages provide a stark contrast to the several studies [Niederle and Vesterlund, 2007, Gupta et al., 2005, Gneezy et al., 2009] that show aggregate tournament entry levels are closer to 50 percent, with some subgroups choosing to enter into the tournament at rates that exceed one half.[10] For Task 4, tournament entry was 21 percent, where subjects chose whether to submit the Piece Rate performance of Task 1 to the tournament. The higher tournament entry percentage in Task 3 relative to Task 4 may reflect subject optimism regarding future performance. In Task 3 the tournament entry decision occurs before subjects perform the matching task and Task 4 entry decision occurs after the score has already been determined.

The units in Table 2.2 are the number of correctly matched items to the corresponding numbered strips of paper that were distributed prior to each 60 second

[10]Niederle and Vesterlund [2007] find 73 percent of men and 35 percent of women choose to enter a similar tournament among US undergraduate subjects. Gneezy, Leonard and List find 54 (26) percent of women and 39 (50) percent of men in traditional matrilinial (patriarchal) societies.

Table 2.2: Summary Data (In number of correctly matched items, unless otherwise indicated)

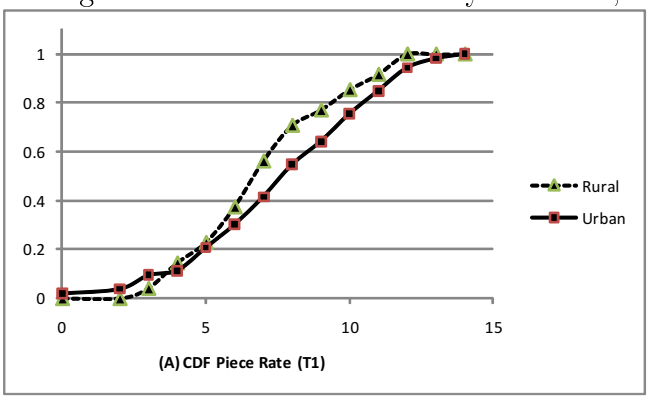
	Mean	Minimum	Maximum
Task 1 (T1): Piece Rate	7.75	0	14
	(2.8)		
Task 2 (T2): Tournament	9.57	2	16
	(2.6)		
Task 3 (T3): Choice Piece Rate or Tournament	29.7%	*Chose Tournament*	
Rural	25.0%		
Urban	34.0%		
Task 3 (T3): Piece Rate or Tournament Performance	9.45	3	18
	(2.6)		
Of those selecting Piece Rate	9.38	3	14
	(2.5)		
Of those selecting Tournament	9.60	5	18
	(2.5)		
Task 4 (T4): Choice Piece Rate or Tournament			
Applied to T1 Performance	20.8%	*Chose Tournament*	
Rural	16.7%		
Urban	24.5%		
Subject Payment (Ghana cedis)	5.58	0.00	36.50
	(7.6)		

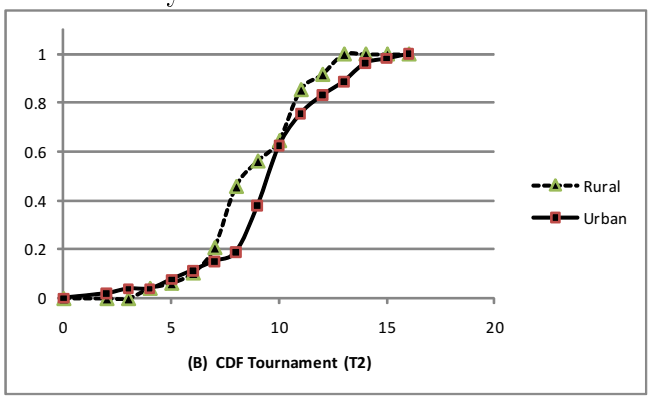
Average performance of 48 Rural and 53 Urban subjects. Standard deviation in parentheses.
Subject payment does not include the 5 cedi show-up fee.

round of play. Performance was bounded by the 21 item sequence. However, the time limit of 60 seconds ensured that none of the subjects was able to correctly match the full sequence. The average Piece Rate (T1) performance (7.8 correctly matched items) was significantly lower, than that of the following Tournament (T2) (9.6 correctly matched items) and Choice (T3) performance (9.4 correctly matched items), with $p<0.01$ in a two sample t test.

Subject payments ranged from 0 cedis to 36.50 cedis and the average payout was 5.58 plus the 5 cedis show-up fee which was an amount that was well over 10 percent of

Figure 2.2: Cumulative Density Function, Tasks 1 and 2 by Rural and Urban

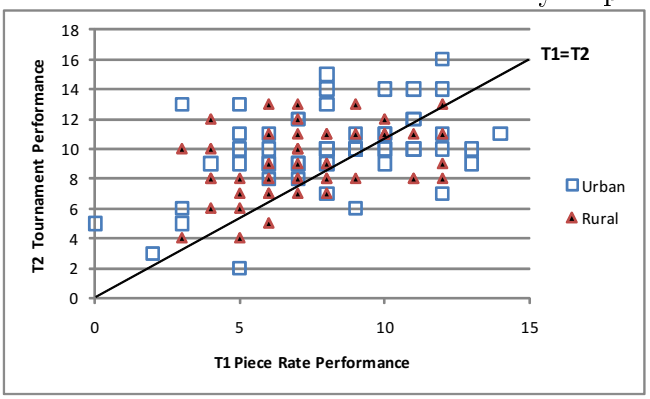




average subject in our sample's monthly income. The maximum number of correctly matched items in Tasks 1, 2 and 3 were 14, 16 and 18, which is important as all were below the maximum 21 items in the numbered sequence.

The average Piece Rate performance in the urban group, 8.1, was higher than the performance in the rural group, 7.4, though this difference was not significant (p = 0.173, Mann-Whitney test). Similarly in the Tournament the urban group mean score 10.0, was higher than the rural score of 9.1, though the test with p=0.077. The difference in average performance persisted through the final round (p = 0.058), Task 3 with average for the urban subjects was 9.9 and for the rural subjects, 8.9. Because the experimental design did not have any between site interaction and because our results do not support within site differences in performances based on gender, income or ethnicity (hometown, languages), over the course of our analysis we have controlled for the differences in performance due to experimental location.

Figure 2.3: Piece Rate and Tournament Performance by Experimental Subject



Within subject performance from the first to the second tasks was positive and significantly correlated as shown in Figure 2.3. Spearman rank correlations for Task 1 and Task 2 show a correlation 0.38 for both Rural and Urban experimental session ($p<0.01$). As in similar experiments we observe an increase in performance from Task 1 to Task 2 (Niederle and Vesterlund, 2007); the increase performance increase can be attributed to learning, differential responses to the compensation scheme or it could be noise. The positive correlation between Task 1 and Task 2 in both Rural and Urban groups is double for those entering the tournament than for those choosing the piece rate pay in Task 3. Rural subjects who chose the tournament (piece rate) in Task 3 had Task 1 and Task 2 Spearman rank correlation of 0.60, $p = 0.041$ (0.33, $p = 0.050$). Urban subjects who chose the tournament (piece rate) in Task 3 had Task 1 and Task 2 Spearman rank correlation of 0.73, $p < 0.01$ (0.32, $p = 0.062$).

Table 2.3 reports subject performance in Tasks 1 and 2 by choice of Task 3 compensation scheme and by urban and rural classification. While the tournament performance did not vary by choice of Piece Rate or Tournament, in the aggregate we found a large and significant difference in Piece Rate performance between those who

Table 2.3: Performance in Task 1 and Task 2 by Choice of Piece Rate or Tournament (Choice Round, Task 3)

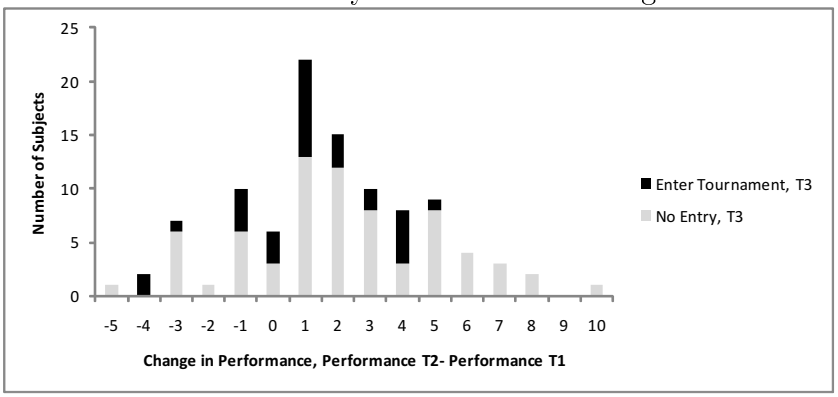
		Average Performance		
	Choice, Piece Rate or Tournament	Piece Rate (T1)	Tournament (T2)	Tournament - Piece Rate
Total	**Piece rate**	7.46	9.62	2.15
		(2.7)	(2.7)	(3.1)
	Tournament	8.43	9.47	1.03
		(3.1)	(2.3)	(2.3)
Rural	Piece rate	7.33	9.31	1.97
		(2.5)	(2.3)	(2.8)
	Tournament	7.58	8.67	1.08
		(2.6)	(2.4)	(2.4)
Urban	Piece rate	7.60	9.94	2.34
		(2.9)	(3.3)	(3.4)
	Tournament	9.00	10.00	1.00
		(3.3)	(2.1)	(2.4)

Number of correctly match objects.
Standard deviation in parentheses. 53 urban, 48 rural subjects.

chose Piece Rate and those who chose Tournament in the Choice round, though further tests show this significance is driven by the urban subjects. For those subjects selecting Piece Rate in the first Choice round (Task 3), the average score, 7.5, in the Piece Rate Task 1 was lower than the average, 8.4, of those who selected the Tournament compensation in Task 3 (Mann-Whitney $p = 0.077$). This difference was large and significant in the urban areas where those who selected the Piece Rate scored and average of 7.6 in Task 1 and those selected the Tournament had an average score of 9.0 ($p = 0.083$). In rural areas there was no difference ($p = 0.631$), where the average for subjects choosing Piece Rate scored 7.3 compared to the average Task 1 score of those who selected the Tournament of 7.6.

In the Tournament Task 2 in urban areas the average score was 10 with no difference in performance between those selecting Piece Rate or Tournament in the first Choice round ($p = 0.69$). The rural Tournament performance was 9.3 for subjects

Figure 2.4: Tournament Entry and Absolute Change in Performance



who selected the Piece Rate compared to 8.7 for those selecting the Tournament pay though this difference was not significant (p = 0.36). We found that the variation in Task 1 performance conditioned on Task 3 choice higher in the urban (1.4 higher for those choosing Tournament in Task 3) subjects than in the rural (0.3 higher for those choosing tournament in Task 3). However, both groups displayed identical average change in performance (Task 2-Task 1) conditional on the choice of compensation scheme (p = 0.91 Enter, p = 0.52 No entry). On average, those choosing Piece Rate pay in Task 3 improved by two points from Task 1 to Task 2 and those choosing Tournament pay improved by 1 on average.

Further examination of learning as measured by the absolute change in performance from Task 1 to Task 2 (Figure 2.4) shows a large range from -5 to 10 and a mode of 1 (0.22) with most of the competitors coming from this group. None of those subjects with high positive improvements in score of 6 or greater chose to enter the tournament in Task 3.

Table 2.4 shows that of the 30 people who chose to enter the tournament, 63 percent improved by 1 or fewer points from Task 1 to Task 2, though for both groups,

Table 2.4: Composition of T3 Tournament Entrants by Change in Performance from T1 to T2

	Pooled	Rural	Urban
Performance T2 - Performance T1 > 1	0.37	0.33	0.39
Performance T2 - Performance T1 ≤ 1	0.63	0.67	0.61
Total Entrants	30	12	18

Among both urban and rural subjects, one half (0.5) had a value of T2 - T1 Performance ≤ 1

those with improvement of 1 or fewer was only half of the entire sample; that is, the subjects who displayed the lowest absolute task learning as measured by the change in score were more likely to compete in both rural and urban areas. This effect was slightly more pronounced in rural areas where 67 percent of those choosing the tournament cam from the bottom half of the learning Task 2-Task 1 distribution, compared to 61 percent in urban areas.

Table 2.5 presents Self Rank conditional on actual performance in the Task 2 Tournament round so as to detect overconfidence and any other patterns that relate performance to beliefs. We report the Task 2 Tournament beliefs rather than the Task 1 Piece Rate beliefs as we believe it to be a less noisy measure of actual beliefs. We do not see aggregate overconfidence that is characteristic in similar experiments [Niederle and Vesterlund, 2007]. In the aggregate, of those individuals who scored in the top quartile, 45 percent believed they were first place, 50 percent for second place and 5 percent, third. Subjects who scored in the second quartile managed to accurately predict their position, with 61 percent correctly choosing second place for the Self Rank. Though the apparent accuracy of the second place self-assessment appears to be driven by a second place norm among all rural subjects and among

Table 2.5: Self Rank and Tournament Performance by Quartile (In Percent)

Self Rank and Tournament Performance by Quartile (In percent)					
	Quartile				
Self Rank		1	2	3	4
	1	45	14	30	23
	2	50	61	35	37
	3	5	25	22	27
	4	0	0	13	13

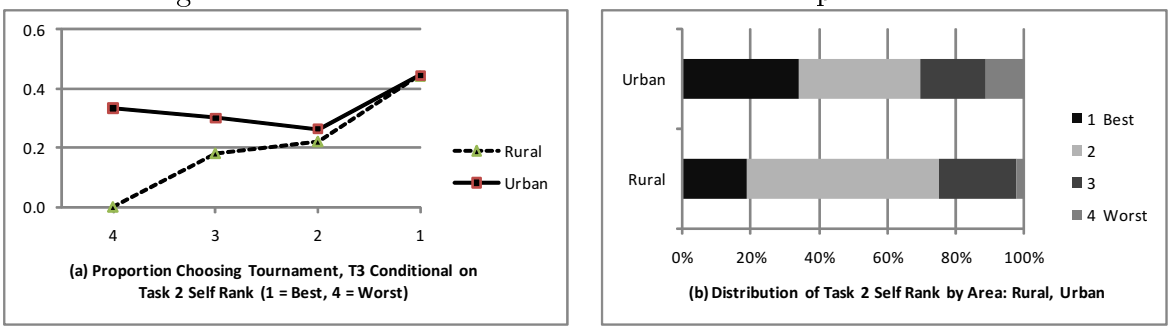
Rural

	Quartile				
Self Rank		1	2	3	4
	1	57	12	20	7
	2	43	59	50	64
	3	0	29	30	21
	4	0	0	0	7

Urban

	Quartile				
Self Rank		1	2	3	4
	1	38	18	38	38
	2	54	64	23	13
	3	8	18	15	31
	4	0	0	23	19

Figure 2.5: Task 2 Self Rank and Choice to Compete in Task 3

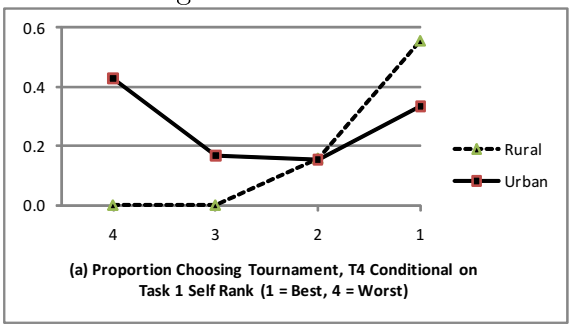


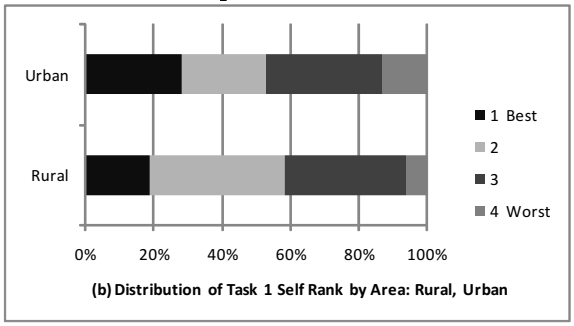
urban subjects who had scores that were above the median. The choice of second place in the self ranking for the tournament was a focal point for the rural subjects: 43 (59, 50, 64) percent of the first (second, third and fourth) quartile believed that they were second place. Among the urban subjects, self rank conditional on performance indicates higher confidence overall than rural subjects, but this is driven by the low performers. In the urban group, 38 percent of the subjects in the third and fourth quartiles believed that they were first place, compared to 20 and 7 percent in the rural group. Among the top performing urban subjects , 54 (64) percent of the first (second) quartiles believed that they were second place.

Those who ranked themselves in first place for Task 2 had the highest proportion of tournament entrants in Task 3 for both rural and urban subjects (Figure 5a). In Task 3, 44 percent of subjects who ranked themselves first in Task 2 entered the tournament. Though rural subjects were less likely to rank themselves first than their urban counterparts (Figure 5b). Under 20 percent of rural subject believed they were first place and nearly 60 percent believed they were second place as compared to a more even distribution of the self rankings amongst the urban subjects.

In Task 4, only rural subjects who thought they had ranked in first or second

Figure 2.6: Task 1 Self Rank and Choice to Compete in Task 4





place chose to enter the tournament (Figure 6a). In this regard, the rural subjects are showing consistency between beliefs and choices. 56 percent of rural subjects and 33 percent of urban subjects with a Self Rank of 1 chose to enter the tournament. As in the Task 2 Self Rank, rural subjects chose second place more often than any other ranking, albeit to a lesser extent (Figure 6b).

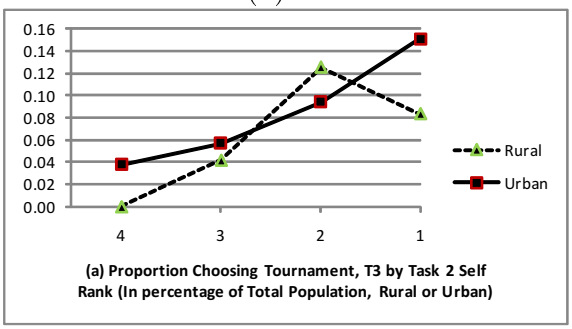
In the urban group we observed high proportion of entry in both Tasks 3 and 4 by subjects who ranked themselves last; these were low performing individuals with a preference for tournament play. We did not observe this behavior among the rural subjects.

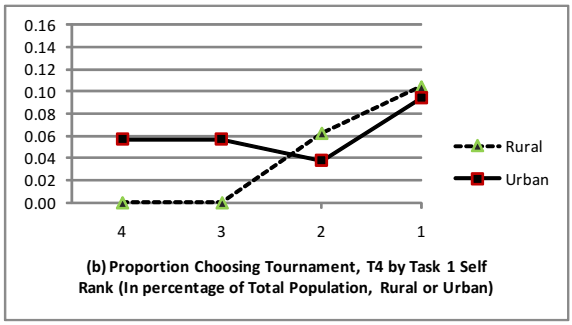
There were no indications of aggregate subject overconfidence from the beliefs assessment tasks: for both the Piece Rate and the Tournament, most subjects believed that they had performed second place or below (Table 2.6). Though urban and rural subjects did have a significantly different distribution of Self-Rank choices (Fisher's exact test, $p = 0.054$) and urban subjects were 62 percent more likely to say that they were best in the Tournament round.

Table 2.6: Self Rank by Rural and Urban in Tournament (T2) and Piece Rate (T1) (In Percent)

		Task 2, Tournament		Task 1, Piece Rate	
		Rural	Urban	Rural	Urban
Best	1	19	34	19	28
	2	56	36	40	25
	3	23	19	35	34
Worst	4	2	11	6	13
	Total	100	100	100	100

Figure 2.7: Proportion of Total Population Choosing Tournament in Task 3 (4) by Self Rank Task 2 (1)





Rural subjects believed they had performed in second place 40 percent of the time for the Piece Rate task and 56 percent for the Tournament task. Positions 2 and 3 were the modes among the rural subjects, accounting for 75 percent of the guesses in Task 1 and 79 percent in Task 2. Only 19 percent believed they ranked first in either task. The urban subjects, though more likely to choose the first place 28 percent of the time in Task 1 and 34 percent in Task 2, still had a mode for each task that was second place or below. In Task 1, 34 percent of urban subjects believed they had placed third and 36 percent believed they had placed second in Task 2.

Figure 2.7 shows the breakdown of aggregate tournament entry in Task 3 (4)

Table 2.7: Probit of Tournament-Entry Decision: Dependent Variable Tournament Entry (Treatment 3)

	1	2	3
Rural	-0.177	-0.166	-0.240
	(0.007)	(0.015)	(0.004)
T2-T1	-0.044	-0.045	-0.039
	(0.007)	(0.007)	(0.039)
Confidence		0.102	0.105
		(0.214)	(0.162)
T4 Entry			0.311
			(0.004)

Marginal effects for Urban participant with T2-T1 of 2, Overconfidence Index of 0 and T4 Entry. P values in parentheses.

by Self Rank in Task 2 (1). Panel (a) illustrates the difference in tournament entry between rural and urban groups is primarily driven by subjects who ranked themselves first. Though we see in Figure (2.5a) the likelihood of entry conditional on Self Rank is the same for rural and urban subject choosing first place, relatively higher confidence of urban subjects (Figure 2.5b) leads to more tournament entry overall from the the urban group. Though the proportion of subjects choosing to enter Task 3 tournament conditional on a third or fourth place ranking was much higher among urban subjects, the low overall proportion (25 percent rural, 30 percent urban) of subjects selecting third and fourth lessened the aggregate effects of this difference.

Panel (b) of Figure 2.7 shows that the difference in tournament entry in Task 4 was driven almost exclusively by those who believed they were below the median. This finding could indicate lower levels of risk aversion among subject who believed they were low performing.

A probit analysis of the tournament entry decision in Task 3 (Table 2.7) shows

the marginal effects of being rural, absolute change in performance, confidence and tournament entry in Task 4 for an urban subject with a change in score of 2, a Confidence Index value of 0 in Task 2 and who entered the tournament in Task 4.[11] Regression 1 shows rural subjects were 17.7 percentage points ($p < 0.01$) less likely to enter the tournament given their change in performance. The marginal effects of the absolute change in performance, -0.044 ($p < 0.01$) show subjects were less likely to enter the tournament the more they improved from Task 1 to Task 2. When we include the Confidence Index in Regression 2, we find a positive marginal effect of having more confidence, but this result is not significant ($p = 0.21$). The lack of significance on the confidence term should not be surprising as even if a subject is measured overconfident, unless he thinks he was first place, entering the tournament is not a rational choice. The high selection of 2nd and 3rd place by rural and urban subjects is preventing the data for the self-rank measure from being a determinant of tournament entry. The magnitude and significance of the coefficients on the rural dummy and change in performance are essentially unchanged with the addition of the confidence variable. Regression 3 includes the decision to enter into the tournament in the 4th round, which can be considered a measure of preferences for competitive institutions isolated from preferences to actually compete. The Task 4 entry variable is positive and significant ($p < 0.01$), and the marginal effects of the

[11] The measure of confidence reflects the distance between guessed and actual rank, weighted by their actual rank, (Actual Rank-Self Rank)/Actual Rank. This construction of the measure of confidence weights underconfidence heavily, that is, where subjects guessed that they were in a lower position than they actually were. The possible values ranged from -3 to 0.75, but for both Task 1 and Task 2, the range was -2 to 0.75. Subjects were slightly less confident in their Task 1 performance with average index value of -0.14 compared to the Task 2 average, -0.12, though this difference was not significant (two-sample t-test, $p = 0.878$).

Table 2.8: Probit Output, Decision to Submit the Piece Rate to Tournament, T4

	1	2
Rural Dummy	-0.176	-0.181
	(0.023)	(0.023)
T1	-0.017	-0.016
	(0.002)	(0.007)
Confidence		0.048
		(0.585)

Marginal effects for Urban participant with T1 performance of 10 and T1 Confidence of 0. P values in parentheses.

change in performance and Confidence Index are unchanged. Controlling for general tastes for competition, we find that the marginal effects of being rural increase in magnitude ($p < 0.01$).

Table 2.8 reports similar results in Task 4 where subjects were asked whether they would like to submit their Task 1 score to tournament pay, rural subjects were 17.6 percentage points ($p = 0.023$) less likely to do so than their urban counterparts, controlling for their performance in the first round. Though strangely, the marginal effects of performance are negative and significant, meaning that those individuals who scored higher were 1.7 percentage points ($p < 0.01$) less likely to enter the tournament for each point of improvement in their Task 1 score. Controlling for confidence (Confidence Index, Task 1) in Regression 2, we see that confidence enters positively, but not significantly and the marginal effects of Rural Dummy and Task 1 are unchanged.

Table 2.9: Ex Ante Monetary Costs of Over- and Under-Entry

	Overentry	Underentry	Net Cost of Underentry
Total (Subjects)	21	28	
	(0.21)	(0.28)	
Expected Cost	94.0	181.8	87.8
Average expected cost	4.5	6.5	2.0
Rural	8	13	
	(0.17)	(0.27)	
Expected Cost	31.5	113.5	82.0
Average expected cost	3.9	8.7	4.8
Urban	13	15	
	(0.25)	(0.28)	
Expected Cost	62.5	68.4	5.9
Average expected cost	4.8	4.6	-0.3

Overentry was calculated based on T2 performance. If a subject's expected payoff given his T2 performance and the corresponding distribution of performance would be higher under the T3 Piece Rate and the subject chose to enter, we considered this Percentage of indicated group in parentheses.

In assessing the welfare implications of subject behavior in terms of experimental payoffs, we present a basic analysis of over- and under-entry and the associated monetary costs (Table 2.9). Over-entry was calculated based on Task 2 performance.[12] If a subject's expected payoff given his Task 2 performance and the corresponding distribution of performance would be highest under the Task 3 piece rate compensation and the subject chose to enter the tournament, we considered this over-entry. Similarly, a subject who chose Task 3 piece rate compensation who would have received a higher payoff in expectation under the tournament pay was considered under-entry. Costs of over- and under-entry were calculated as the difference between the ex ante expected value of the higher payoff decision (that was not taken by the subject) and

[12] We chose to base the designation of over- and under-entry at the distribution that was available at the time the decision was made, that is, the Task 2 performance distribution. We do not consider risk preferences, only monetary payoffs.

the payoff given the actual choice. While both rural and urban had under-entry rates of 0.3, over-entry was less common in rural groups, 0.17, compared to urban, 0.25. The expected costs of under-entry were higher than the costs for over-entry in both groups, though only significantly so in the rural case where on average, net under-entry (cost of under-entry-cost of over-entry) cost each subject 4.8 cedis, the show-up fee. Urban subjects did not systematically over- or under-enter and the net cost of under-entry was essentially zero.

2.6 Discussion and Conclusion

Our results indicate a general preference for a non-competitive compensation scheme over a competitive of both urban and rural subjects in artefactual field experiments. However, urban subjects were around 20 percent more likely to enter into the tournament than were their rural counterparts. Our analysis points to ambiguity aversion as the primary driver of the observed behavior and for the rural-urban difference in tournament entry. Though all subjects indicated difficulty assessing relative ability in new tasks, the urban subjects were overall more comfortable with uncertainty when the potential rewards were high.

While the task in our experiment is simple and requires no special training or skill to be successful, subjects appeared to feel pressure by the time constraint thereby making what would otherwise be a trivially easy task difficult. In fact, no subject was able to complete the task (correctly fill all 21 bins) and the highest score of 18 was earned by a single subject. The difficulty of the task is a likely factor in the observed low confidence, which is consistent with the findings that people tend to believe they are performing better than average on easy tasks and worse on difficult

ones [Moore and Small, 2007]. When we reduced the uncertainty in a set of pilot experiments by announcing the score to beat to the rural subjects, participation on the tournament doubled. This supports the hypothesis that ambiguity aversion over risk aversion kept subjects from participating in the tournament in prior rounds.

Though the urban subjects did tend to perform better than the rural ones, because we did not have and between group comparison, this should not have played any role in the rural subjects willingness to select the tournament. However, we detected one fundamental difference between the two groups with regards to the Beliefs Assessment. Irrespective of actual performance, rural subjects were most likely to guess that they were in second place and we did not observe the same selection of second place for the urban subjects. Conditional upon that belief of second place, there was no difference in tournament entry between urban and rural groups. However, urban subjects were much more likely to think that they were first place and thus entered more often. Also, the urban subjects had a group of risk takers who thought they were in third and fourth place and still chose to enter the tournament in the Choice rounds.

As the demographic characteristics of the subjects is otherwise identical between the rural and urban, it makes our findings that much more striking with implications that support to a widely held view that rural people are less competitive than urban people, even when controlling for a battery of demographic traits such as age, income and gender. With the emergence of programs government run and civil society to promote entrepreneurship in Ghana, it will be of critical importance for those designing the programs to understand the cultural predispositions of the pool of potential entrepreneurs and whether urban areas stand to receive a disproportionately higher benefit than rural areas due to systematic behavioral characteristics, such as stronger preferences for competition amidst uncertainty.

While our study does not address urban-rural migration, using recent immigrant

subjects, we can use a similar experimental design to test whether competitiveness of rural immigrants is systematically different from those who do not choose to move to the city. The out-migration of competitive individuals from rural areas has implications on the quantity and types of business activities that are locally developed and sustained.

The strong significant relationship between the change in score from the Piece Rate to the Tournament rounds indicated that subjects were less likely to enter the Tournament if they showed more improvement. If past performance was the subject's primary input as he calculated the expected performance, then it would follow that higher variance in the score would mean lower expected performance in relation to the top score. The further the expected performance was from the top score, the less likely a subject would assess themselves as being able to score in first place and thus enter the tournament. The uncertainty that each subject had regarding the top score caused subjects to overweight the probability of scoring low in subsequent rounds.

With potential payoffs representing at least 10 percent of a month's wages for a typical worker, financial incentives were sufficiently high in both the Piece Rate and Tournament compensation schemes to induce high levels of concentration and effort in all tasks. However, our data are consistent with findings in the literature that rank-order tournament incentive schemes induce higher effort than piece rates [Lazear and Rosen, 1981, Bull et al., 1987]. Preliminary findings in a task where subjects learn the top score (that must be exceeded) indicate that there is an interaction between not only the choice to enter the tournament, which increased with increased information, but with effort levels as well, which also increase.

In contrast to the related literature that evaluates preferences for competition with heterogeneous group composition (men and women) [Gupta et al., 2005, Gneezy et al., 2009, Niederle and Vesterlund, 2007], our experimental design consisted of

homogeneous groups, similar to Carpenter and Seki [2005], which freed us from having to consider any between group interactions that may have affected our measurement of preferences.

Higher performing individuals were disproportionately less confident than their lower performing counterparts, which helps to explain the overall low tournament entry. The propensity of the high performing subjects to shy away from the high risk-high return tournament compensation may reflect the environment where scarcity of good jobs implies a high opportunity cost of forgoing a good job to become an entrepreneur. Higher performing individuals have more to lose from entering the tournament and losing than do the low performing individuals. Trends in quartile performance graph may reflect this counter-intuitive trend that occurred with both rural and urban subjects.

To fully capture the implications of our findings on the potential for growth in entrepreneurship in rural and urban areas, it will be important to compare the results from these artefactual experiments with results from similar field experiments with local entrepreneurs. Though even without comparison, the subjects' display of a strong lack of confidence in an unfamiliar setting is likely an indicator of people's adverse attitude toward seeking out opportunities when a safe option is available. If the only entrepreneurs in the society are very small-scale and doing so out of necessity, as opposed to those who have a relatively strong foundation from which to innovate and create firms that improve economic efficiency, governments may need to develop mechanisms that provide adequate insurance to those skilled individuals in order to induce them to take more business risks.

Chapter 3

Do Taxpayers Use Charity Donations to Keep Auditors at Bay? Theory and Experiment

3.1 Introduction

The use of signals to facilitate market exchange in games of asymmetric information has strong intuitive and theoretical foundations in the well established literature beginning with Michael Spence's [1973] seminal paper on job market signaling. With broad applications of signaling games, the extent to which people use their available signaling devices remains a rich area for experimental research; particularly, do people utilize additional signals in a manner consistent with rational utility maximizing behavior? We focus on two characteristics of real-life signaling situations that are relatively under-explored in the experimental literature: first, multiple, heterogeneously reliable and partially correlated signals are oftentimes, at the disposal of the senders. Whether to use a signal and if so, the choice among signals and welfare implications are important decisions that warrant further study. Second, when neither a clear pooling or separating equilibrium can be expected, a hybrid equilibrium necessitates the use of mixed strategy by at least one of the types. We formulate a simple theoretical signaling model with multiple signals and a hybrid equilibrium in order to

construct a corresponding two-sided signaling experiment that will test whether the signaling strategies encompass the full set of available signaling options in a way that the theory would lead us to expect.

The economics of tax evasion provides a germane application of a signaling model in which we can develop a set of benchmark theoretical results to test experimentally. The voluntary reporting of income is a naturally occurring signaling game that takes place regularly between individual taxpayers and tax authorities. In the United States, under-reporting of income by individuals constitutes 80 percent of the US tax gap,[1] which at 15% of the country's $2 trillion tax liability (2001), has significant financial implications and continues to be a concern of tax authorities [IRS, 2009].

Because of many of the empirical challenges related to acquiring field data from illegal activity at the individual level, the theory has provided guidance regarding the effects of institutional tax parameters on compliance. Natural experiments support the theory as in Slemrod et al. [2001], but restrict the scope and timing of exploration.

Early theoretical work economics of tax compliance has approached the individual tax evasion decision from the perspective of a rational utility maximizing taxpayer who decides to misreport income dependent upon exogenous parameters such as tax rate, probability of audit and size of punishment, in a framework akin to the early work on crime and punishment [Becker, 1968]. In a series of papers by Graetz, Reinganum and Wilde, the endogeneity of the audit policy was considered, along with taxpayer behavior, both in a principal agent framework [Reinganum and Wilde, 1985] and as a signaling model [Reinganum and Wilde, 1986, Graetz et al., 1986]. The recent literature on tax compliance has taken a mechanism design approach to assess the

[1] Overstating deductions is another form of under-reporting income, as it effectively reduces the amount of taxable income. However, the contribution to the tax gap is orders of magnitude smaller. Since charitable donations typically constitute a small percentage of household budget and involve two sided formal activity, it may be easy to see why overstating of these types of deduction,while a problem, are not the main driver of the tax gap.

optimality of tax policy [Bassetto and Phelan, 2008, Deneckere and Liang, 2010] and the existence of the tax riot equilibrium. The existence of multiple equilibria in the theory combined with fiscal motivation to understand the tax compliance decision has provided a clear impetus for many tax experiments in their efforts to identify and calibrate the incentives that determine individual compliance behavior.

Experiments on tax evasion, as in the initial theory, by and large, concentrate on measuring the exogenous parameters that foster taxpayer compliance in the laboratory and the associated comparative static results. They find responsiveness to tax rate, audit scheme, size of punishment, use of funds, uncertainty and social interactions all factor in to the tax compliance decision [Alm et al., 1992a,b, Alm and McKee, 2004, Hsu, 2006, Bernard Fortin and Villeval, 2007] Also typical of tax evasion experiments, the single dimension of the decision, that is reporting of income. Several recent experiments have introduced communication and social interaction treatments to identify any effects on compliance [Bernard Fortin and Villeval, 2007, Alm et al., 2009]. With few exceptions [Kim and Waller, 2005], tax experiments are single-sided as opposed to strategic signaling games, focusing primarily on the parameters that foster compliance and not the use of signals to avoid sanction.

The persistent tax gap notwithstanding, most people comply with tax rules. The assumption that people fall into some continuum of types who are rational strategic optimizers to those who follow all rules, irrespective of the optimal behavior, is a characterization of reality that provides a tractable framework for understanding a variety of behavioral regularities that are not easily explained by the standard expected utility maximization. The tax compliance rate of 84% [IRS, 2009], in a voluntary reporting system with relatively low probability of audit and punishment size is supported by unreasonably high coefficients of risk aversion [Bernasconi, 1998] or the existence of honest people who follow the tax rules in way that does not reflect

any strategic calculations. The honest and strategic distinction was first used in the tax compliance context by Graetz et al. [1986] in their seminal theoretical work on two-sided tax compliance and enforcement behavior .

We develop a simple tax evasion model as a signaling game between a taxpayer and an auditor, with a non-strategic, always compliant type. In addition to the taxpayer's income report to the auditor, we introduce a charitable donation option as a costly signal, that directly affects only the taxpayer , but may serve as an indirect signal to the auditor of the taxpayer's ethical type, which would have an impact on an auditor's decision to audit.

According to the Social Capital Community Benchmark Survey [2000], three quarters of American households give to charity. While high income households account for the majority of the charitable contributions, lower income households donate a larger percentage of their incomes to charity. Charity's special tax status has lead to many instances of fraudulent activity, including several prominent tax fraud cases involving charitable donations. However, in all cases, charitable donations have been used to avoid paying taxes. The use of charitable contributions to avoid audit is not addressed by the IRS and similar to the other tax compliance parameters, is particularly well suited to test in an experimental study as the limitations of collecting real data are vast. Further, the choice of the charitable donation as the signal reflects a conjecture that there is some system of linked norms, in line the recent research identity economics research [Akerlof and Kranton, 2000], in which we may expect to find a positive behavioral correlation at the individual level such that those who donate to charity tend to truthfully report their income to the tax authorities. In this scenario, a charitable donation can be considered not only a contribution to a public good, but a signal that the individual making the donation is a good person and behaves as good people are expected to.

Under certain values of our theoretical parameters, we find a semi-separating Perfect Bayesian equilibrium where taxpayers will use the charitable donation to signal honesty, thereby reducing the probability of audit. Auditors will optimally audit reports without charity donations more frequently than those with donations.

To test our theoretical predictions, we use a two-sided signaling experiment of voluntary reporting of income for tax purposes and include an observable and verifiable charitable donation option. We introduce a computer generated compliant-type similar to Kim and Waller [2005] into the population in order induce a change in signaling behavior of the experimental subjects, who we assume behave opportunistically. Our aggregate experimental results indicate that senders (taxpayers) do indeed employ a mixed strategy in-line with theoretical predictions, namely they use a charity donation in effort to prevent the receiver (auditor) from conducting an audit that would result in a penalty in the case where the sender has misreported his type (income). However, at the individual level, we find less evidence of players behaving as the theory predicts. Auditors respond to the charity donations as predicted by the theory, though there is more noise in their decision making process. Individual analysis indicates simple decision rules and past experience are driving subject behavior.

The following section provides a review of relevant experimental literature on signaling and several of the voluntary reporting two-sided tax evasion studies. Section 3 presents a very simple model of tax reporting with honest taxpayers and the option to make a tax deductible charitable donation. In Section 4 we present our hypotheses regarding tax compliance and charitable donations implied by the theory and how we will test them in the experimental laboratory. Section 5 is a presentation of our results and Section 6 discusses the implication of the results in supporting our hypotheses.

3.2 Experimental Background

With broad applications of signaling games and multiple theoretical equilibrium predictions, experiments provide a natural empirical methodology in which to establish the conditions under which behavior conforms to the theory. Camerer and Weigelt [1988] find in an early signaling experiment find that sequential equilibrium predictions from the theory describe player behavior well, though players play as if they have made idiosyncratic adjustments to prior beliefs. In another significant experiment, Brandts and Holt [1992] find that though the sequential equilibrium is often played, when players have experience with play off-the-equilibrium path, what they call an "unintuitive" equilibrium arises.

The adverse selection problem inherent in a corporate finance has motivated signaling experiments with similar goals of testing theoretical predictions regarding equilibrium play [Cadsby et al., 1990, 1998, Forsythe et al., 1999]. Cadsby et al. [1990] find that a unique theoretical equilibrium predicts the experimental behavior well. However, when separating and pooling and hybrid equilibria are predicted, only the most efficient pooling equilibria emerge in the lab.

Since we employ a voluntary reporting system in our experiment, where taxpayer incentives to announce the truth are limited, we can draw from the cheap talk and signaling experiments for benchmark predictions[Gneezy, 2005]. A large experimental literature addresses the use of cheap talk to arrive at efficient outcomes; cheap talk has been shown to have real economic consequences, both in theory and experiments, when the opposing players payoffs are aligned the sharing of information from the cheap talk leads to efficiency [Farrell and Rabin, 1996]. In the opposite case where the incentives to lie are out-sized, outrageous claims will be ignored. It is the middle case, which we use in our tax evasion set-up, where the incentives to lie are limited,

making the experimental investigation of the equilibrium outcome most interesting.

Testing the use of multiple signals in an experiment, Duffy and Feltovich [2006] find that combining the cheap talk signals with observation of past behavior to promotes cooperation. However, they use simultaneous move games and only signals to indicate future action, not type, as in the Bayesian sense of sequential games. Charitable donations were used in laboratory experiments as a signal of type, namely conditional cooperation [Max Albert and Maciejovsky, 2007], and did promote coordination. However in our study, where the payoffs of the players are not aligned, the signal is sent for purely self-interested motivations.

Forsythe et al. [1999] look at mutually exclusive communication mechanisms that can overcome the adverse selection problem of financial disclosure, like ours one of the few studies that looks at signaling games where the opponents preferences are in opposition. Introducing an anti-fraud provision that placed limits on the egregiousness of the claims that could be made on the part of the seller, did improve the buyer's outcomes, where theoretically there should have been no difference. Irrational behavior "money-burning" is punished, in the sense that firms who waste money do not get better treatment then those who do not.

Kim and Waller [2005] provide a direct test of the Graetz et al. [1986] model and find that in contrast with the theory in which they auditors do not respond to the changing composition of strategic versus compliant types, in the experiment, auditors incorrectly believe increases in strategic players increases under-reporting.

Experimental behavior in games with mixed strategy equilibria often is best modeled not by the standard game theoretic equilibrium, but by adaptive learning models [Ochs, 1995, Erev and Roth, 1998]. Ochs [1995] finds that in competitive games, steady states reached in the experiment differ considerably from Nash equilibrium and that experimental subjects condition current actions on past experiences.

3.3 Model

A simple model of voluntary reporting and tax compliance that draws from the crime and punishment framework of Allingham and Sandmo [1972] and with honest taxpayers from Graetz et al. [1986] is used to demonstrate how a charitable donation can be used to signal type to an auditor who endogenously determines how frequently to audit reports. Extension of the punishing of free riding, but certain free-riding behavior, namely tax evasion, is illegal contribution is not voluntary *per se*; if the government could freely monitor all income, taxes would be deducted according to tax law, but due to budget constraints and voluntary reporting system that is subject to strategic behavior on the part of taxpayers who are aware of the limited enforcement capabilities of the tax authorities and respond accordingly. The auditor must rely on reported income and a signal that is costly to the taxpayers, the charitable donation, to inform his audit decision. Since we are focused on the signaling decision rather than parameter testing in the tax compliance setting, we employ the simplest formulation of a signaling model and introduce a signal that is correlated with one dimension of the sender's type. Compliant types do not optimally respond to incentives and always truthfully report their incomes. We use the following discrete formulation of the model to illuminate the decision to donate to charity as a signal of honesty, not necessarily the degree, which we leave for further study.

The model is a standard signaling game where the sender is the taxpayer and the receiver is the auditor. The equilibrium will take the form of Perfect Bayesian equilibrium, with strategy profile and beliefs prior beliefs regarding taxpayer type in both dimensions posterior beliefs once the auditor receives the taxpayer signal.

Taxpayers in our model are characterized in two dimensions: opportunism, y, which is a random variable and whose support is the set $Y \in \{c, s\}$ where c identifies

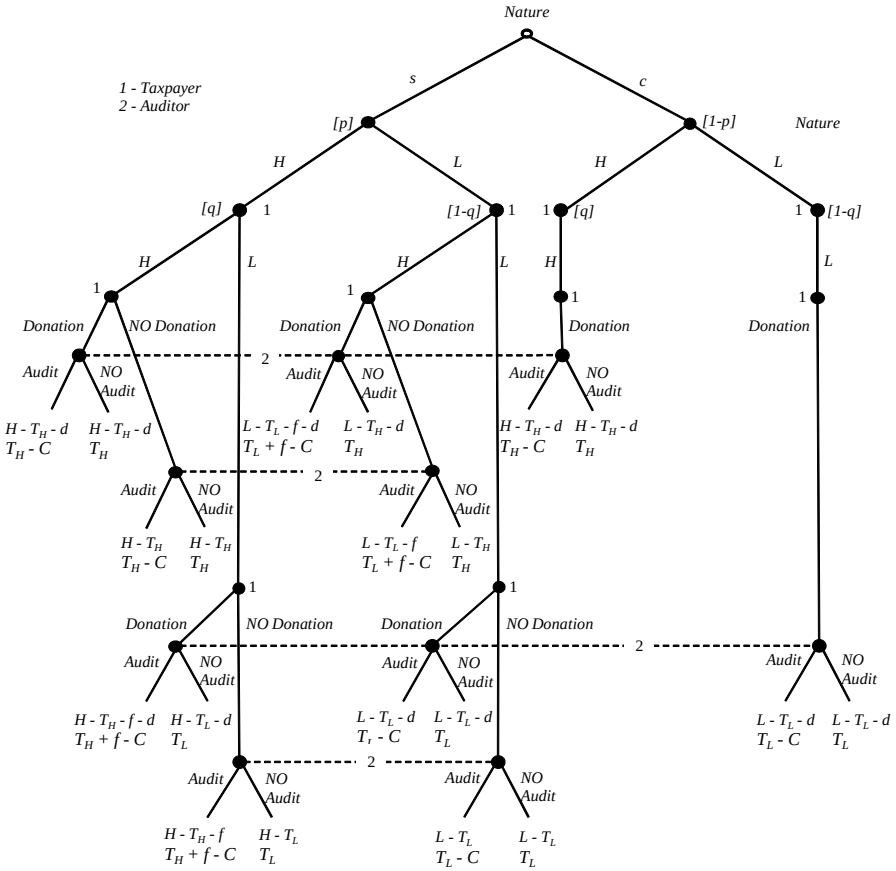
the type that always complies with tax rules by truthfully reporting his income and conforming to social norms regarding charitable giving, that is, within the context of our model, unresponsive to any pecuniary incentives and s identifies the strategic type whose compliance decisions depend on a rational utility maximization problem and; income, x, which is a also random variable whose support is the set $X \in \{H, L\}$ that is that is either high, H, or low L with $H > L$. We assume an individuals type in each dimension is his private information and independent of the other dimension.

The distribution of types are independent and common knowledge: the proportion of the population that is strategic is $p \in [0, 1]$ and the remainder of the population, $1 - p$, is comprised of compliant types. The proportion of high income taxpayers is $q \in [0, 1]$ and the proportion of low income taxpayers is $1 - q$. The game tree is depicted in Figure 3.1.

Taxpayers must pay taxes based on realized income, where T_H is the tax payment that corresponds to the high income draw and T_L to low income. The tax burden is always at most the level of income, $H - T_H \geq 0$, $L - T_L \geq 0$ and high taxes at least as large as low taxes, $T_H \geq T_L$. However, the tax payment is determined by voluntary reporting to the auditor, so the taxpayer must make a reporting decision, $R(x, y) \in X$, that will depend on his realizations of type and the tax payment, $T(R)$, and will be based on the auditor's current information regarding the taxpayer's income. If the taxpayer reports high income, whether he is high income or not, then he will pay $T(H) = T_H$ and similarly, if he is low income, he will pay $T(L) = T_L$. In the even that the taxpayer is audited and the auditor can update his information, he will pay taxes on his true income, x.

Assumption 1. Compliant types always truthfully report their income, $R(x, c) = x$.

Figure 3.1: Tax Compliance Game with Charity Donation



In the Graetz et al. model that had compliant types, there was no way for a taxpayer to convey any information to the auditor regarding his type in this dimension. In our framework, we introduce a charitable donation $D \in \{0, d\}$, $d > 0$ that can serve as a costly signal, that is correlated with the compliant type. Otherwise, the donation has no impact on the tax liability of the taxpayer.

Assumption 2. Compliant types always donate to charity, $D(x, c) = d$, irrespective of income.

This assumption is strong, but can be relaxed without changing the qualitative nature of the results. Since compliant types do not respond to model parameters, we could assume some fraction of these players donate to charity, which will affect the equilibrium probability of donation for the strategic players.

The proportion of the population reporting low income and giving to charity is $(1-p)(1-q)$.

This follows from Assumptions 1 and 2.

If the taxpayer is discovered to be misreporting his income by an auditor, he must pay a fine, $F \in \{0, f\}$, that will be positive $f > 0$ if he has misreported his income, $x \neq R$, and 0 otherwise. There is no cost to that taxpayer if he is audited and truthfully reporting his income.

All strategic taxpayers are risk neutral and have an expected utility, $EU(R, D|x, s) = x - D - [1 - \text{Prob}(audit)]T(R) - \text{Prob}(audit)(T(x) + F(x, R))$ that depends on signal pair (R, D).

Also, $\min\{H - T_H, L - T_L\} > d$, the all players would have net income greater than zero after paying taxes and making the charitable donation and the penalty for under-reporting is greater than the donation, $f > d$, so that we are not forced to trivially eliminate the donation option for high income strategic individuals.

Low income strategic types will never report high income. That is, $R(L,\, s) = L$.

For any probability of audit, b, that the low income strategic taxpayer with expected utility, $EU(R,\, D|L,\, s) = L - D - (1-b)T(R) + -b(T_L + F(x,\, R))$ chooses a signal pair $(R,\, D)$. If the taxpayer reports high income, then his utility will be $EU(R,\, D|L,\, s) = L - D - (1-b)T_H - b(T_L - f)$ which is strictly less than his expected utility if he reports truthfully, $EU(R,\, D|L,\, s) = L - T_L - D$ since $T_H \geq T_L$.

Strategic types who report truthfully will never make a donation. That is, if $R(x,\, s) = x$ then $D(x,\, s) = 0$.

Since an audit has no effect on the payoff of any honest report, it follows that for any $D > 0$, the taxpayer's utility lower, therefore $D = 0$; rational optimizers have no direct incentive to make a donation to charity.

Since low income strategic types will always play a pure strategy of low income and no donation, $(L,\, 0)$, only the high income strategic type may play a mixed strategy where he chooses between the three possible signal pairs: $(L, 0)$, (L, d) and $(H, 0)$.

A mixed strategy for a high income strategic type is a probability distribution over the possible signal pairs. The probability the taxpayer under-reports without making a donation to charity, $(L, 0)$, is $\alpha_0 \geq 0$, the probability the taxpayer under-reports and makes a donation to charity, $(L,\, d)$, is $\alpha_1 \geq 0$ and the probability of submitting a truthful report, $(H,\, 0)$, to the auditor is $(1 - \alpha_0 - \alpha_1)$.

The taxpayer's signal pair, $(R,\, D)$ is viewed by an auditor who must decide whether to engage in an audit, $A \in \{0,\, 1\}$, that costs C to undertake.

The tax authority and auditor's objective is to maximize revenue net of audit costs.

High reports are never audited as long as $T_H > T_L + f - C$.

The audit revenue depends on the auditor's decision to audit and whether the report from the taxpayer was truthful: $\Pi(A|R) = T(R) + A(T(x) - T(R) + F(x,R) - C)$. If $A = 1$, then if the report was truthful, $R = H$, results in net revenue $\Pi(A|H) = T_H - C < T_H$, that is lower than in the no audit case. If the report was not truthful and an audit was initiated then the net revenue $\Pi(A|H) = T_L + F - C < T_H$. The focus of the auditor's efforts will be on identifying those taxpayers with a low report who actually received a high income.

The auditor's beliefs, μ, regarding the taxpayer type given the signal pairs of low income reports without and with a donation are the following:

$$\mu(\alpha_i|\, D) = \begin{cases} \dfrac{q\alpha_0}{q\alpha_0 + 1 - q} & i = 0, \text{ if } D = 0 \\ \dfrac{pq\alpha_1}{pq\alpha_1 + (1-p)(1-q)} & i = 1, \text{ if } D = d \end{cases}$$

A low income report without a donation will either come from a strategic player with high income who under-reports with probability $pq\alpha_0$ or a strategic low income earner, $p(1-q)$. Because only strategic players refrain from donating, the auditor's beliefs regarding the taxpayer type, $(x,\, y)$, is independent of relative size of the strategic population, p.

A mixed strategy for an auditor is a conditional probability distribution over the possible signal pairs he could receive. The probability of audit given a low report without a charity donation, $(L,\, 0)$ is β_0 and the probability of audit given a low report with a charity donation, $(L,\, d)$ is β_1.

The auditor's maximization problem is conditional on the report of high or low income and whether a donation was made. The trivial cases of a high income report with and without donation will not induce an audit. The risk neutral auditor maxi-

mizes revenue net audit costs in the sequential game where the taxpayer is the first mover and has made a low income report:

$$\underset{\beta_i}{\text{Max}}\, \Pi\left(\alpha_0,\, \alpha_1,\, \beta_i |\, D\right) = \beta_i \left[\mu_i \left(T_H + f - C\right) + \left(1 - \mu_i\right)\left(T_L - C\right)\right] + \left(1 - \beta_i\right) T_L,$$

where $i = 0$ if $D = 0$ and $i = 1$ if $D = d$.

The cutoff belief above which the auditor will always audit a report and below which he will never audit is $\bar{\mu}_i = \dfrac{C}{T_H + F - T_L}$, $i = 0,\, 1$.

The first order condition will determine the auditor's cutoff belief regarding the proportion of misreporting in the population. He will be indifferent between auditing and not at the point where the net expected marginal return to an audit equals the marginal cost. The probability of audit will be 1 whenever the proportion of misreported incomes is above the cutoff value and 0 if the proportion falls below:

$$\hat{\beta}_i = \begin{cases} 1 & \text{if } \mu\left(\alpha_i\right) > \bar{\mu}_i \\ [0,\, 1] & \text{if } \mu\left(\alpha_i\right) = \bar{\mu}_i \\ 0 & \text{if } \mu\left(\alpha_i\right) < \bar{\mu}_i \end{cases} \qquad \text{for } i = 0,\, 1.$$

The cutoff value of misreported income, $\bar{\mu}$, depends on the cost, tax and penalty parameters, so is not restricted to take on a value less than 1. Clearly, if $\bar{\mu} > 1$, the auditor will never audit.

The equilibrium strategy for strategic high income earner is characterized by the following probabilities:

$$\bar{\alpha}_i^* = \begin{cases} \bar{\alpha}_i & \text{if } 1 \geq \bar{\alpha}_0 + \bar{\alpha}_1 \geq 0 \\ \dfrac{\bar{\alpha}_i}{\bar{\alpha}_0 + \bar{\alpha}_1} & \text{if } \bar{\alpha}_0 + \bar{\alpha}_1 > 1 \end{cases} \qquad \text{for } i = 0,\, 1.$$

The cutoff $\bar{\mu}_i$ determines $\bar{\alpha}_i$, $i = 0,\, 1$, from the auditor's beliefs regarding the proportion of low income reports that are untrue:

$$\hat{\beta}_i = \begin{cases} 1 & \text{if } \alpha_i > \bar{\alpha}_i^* \\ [0,\, 1] & \text{if } \alpha_i = \bar{\alpha}_i^* \\ 0 & \text{if } \alpha_i < \bar{\alpha}_i^* \end{cases}$$

where $\bar{\alpha}_0 = \dfrac{(1-q)C}{q(T_H + F - T_L - C)}$ and $\bar{\alpha}_1 = \dfrac{(1-p)(1-q)C}{pq(T_H + F - T_L - C)}$. In the case that $1 \geq \bar{\alpha}_0 + \bar{\alpha}_1 \geq 0$, the taxpayer's equilibrium strategy, $\bar{\alpha_i}^*$, is defined directly by the parameters. In the case where $\bar{\alpha}_0 + \bar{\alpha}_1 > 1$, we assume that the taxpayer will always misreport his income and will mix between donating and not donating proportionally, such that $\bar{\alpha}_i^* = \dfrac{\bar{\alpha}_i}{\bar{\alpha}_0 + \bar{\alpha}_1}$ for $i = 0,\, 1$.

The high income strategic taxpayer maximizes the following expected utility function:

$$\begin{aligned} \underset{\alpha_0,\, \alpha_1}{\text{Max}}\, U(\alpha_0,\, \alpha_1,\, \beta_0,\, \beta_1) \;=\; & H - \alpha_0[\beta_0(T_H + f) + (1-\beta_0)T_L] + \\ & \alpha_1[\beta_1(T_H + f + d) + (1-\beta_1)(T_L + d)] + \\ & (1 - \alpha_0 - \alpha_1)T_H \end{aligned}$$

The equilibrium probabilities for audit are

$$\bar{\beta}_0^* = \frac{T_H - T_L}{T_H + f - T_L} \text{ and } \bar{\beta}_1^* = \frac{T_H - T_L - d}{T_H + f - T_L}.$$

The ratio of the potential gain from misreporting to the downside loss from being audited when misreporting, which is determined by the taxpayer's optimal choice of α_0 and α_1. The first order condition equates the marginal expected benefit from

misreporting, or the expected utility of a low income report to the marginal cost of truthful reporting, which is the utility of a truthful report.

The cutoff values determine the equilibrium strategy, β_0^* and β_1^*, for the auditor who views a low income reports with and without donations:

$$\hat{\alpha}_i(\beta_i) = \begin{cases} 1 & \text{if } \beta < \bar{\beta}_i^* \\ [0,\ 1] & \text{if } \beta = \bar{\beta}_i^* \\ 0 & \text{if } \beta > \bar{\beta}_i^* \end{cases} \qquad i = 0,\ 1$$

In equilibrium, $1 > \bar{\beta}_0 \geq \bar{\beta}_1 > 0$, that is, the probability of audit for the low income report with zero donations is higher than the low income report with a donation.

3.4 Experimental Design

To test our hypotheses regarding the use of signals in the tax compliance setting, we ran laboratory sessions with University of Maryland undergraduate subjects in the Experimental Economics Laboratory at the University of Maryland using University of Zurich software, z-tree [Fischbacher, 2007]. Subjects were offered $5 as a show-up fee and were told that they would earn $15 on average in a session that would last approximately 60 minutes. We ran two treatments of the experiment and for each one we conducted 5 sessions with 16 people for a total of 10 sessions with 160 subjects. None of the subjects took part in more than 1 experimental session.

For our experimental sessions, we parameterized the theoretical model such that high income players would have limited but positive incentives to cheat (Table 3.1).

Subjects were seated at private computer workstations where each one was randomly assigned into one of two groups for the duration of the session: half of the

Table 3.1: Experimental Parameters (in dollars)

High income	H	20
Low income	L	10
Proportion of high income taxpayers	q	0.5
Proportion of strategic types, T1	p_1	1.0
Proportion of strategic types, T2	p_2	0.75
High tax liability	T_H	6
Low tax liability	T_L	0
Donation	d	3
Fine	f	6
Audit cost	C	3

participants were in Group A (taxpayers), and the other half in Group B (auditors). The instructions were distributed to each subject and then read aloud by the experimenter (See Appendix for experiment instructions). Though contextualizing in signaling experiments can substitute for experience and can increase the initial level of strategic play[Cooper and Kagel, 2003], since we were playing many rounds and did not necessarily need the tax context for our results to be meaningful, we chose to use abstract terminology to the extent possible over the course of our experiment; we did not discuss *tax* or *audit*, but *pay back* and *verification*. All subjects were aware of all of the roles and the parameters of the experiment and we required each subject to correctly answer control questions regarding payoffs prior to the first round of play.

Each round, a taxpayer and an auditor were paired at random to play the following game: on his computer screen, a taxpayer received an income either \$20 or \$ 10 with equal probability. The income draw was his private information and his decision was whether to disclose \$20 or \$10 to the auditor given that a higher tax of \$6 was required if the income were \$20 and the corresponding tax on the \$10 income was \$0. The tax payment depended initially on the reported and not the actual income and as in the model, the auditor collected all tax revenue from his corresponding taxpayer partner. However, if upon viewing the report, the auditor decided to initiate

Table 3.2: Taxpayer Payoffs

High Income

No Donation		Audit		Donation		Audit	
		No	Yes			No	Yes
Report	High	14	14	Report	High	11	11
	Low	20	8		Low	17	5

Low Income

No Donation		Audit		Donation		Audit	
		No	Yes			No	Yes
Report	High	4	4	Report	High	1	1
	Low	10	10		Low	7	7

an audit, he learned the actual income of the taxpayer and if it differed from the reported amount, that taxpayer was required to pay a \$6 fine in addition to the unpaid tax liability (which could be negative in the case of over-reporting of income). The taxpayer was also given the option to make an observable and verifiable donation to a University of Maryland charity,[2] where the donation was immediately subtracted from the income and had no impact on the tax liability of the taxpayer. We chose this simplification rather than one that is closer to reality where charity donations are essentially subsidized in the tax code, because donations still represent a loss to the individual, with or without any added incentives.

The taxpayer's payoffs in all possible scenarios, Table 3.2, were given to all players to ensure that the stakes of the game were clearly understood.

After viewing his partner's reported income and charity donation, the auditor was given the option to verify the income report. An auditor incurred a cost of \$3 if he decided to verify his partner's income, but we provided each auditor with the \$3 audit cost in order to avoid any complications, both in terms of logistics and behavior, if in fact we made participants take losses in the experiment. If the auditor chose not

[2]The charity selected was the University of Maryland's Maryland Fund for Excellence, the University's umbrella fundraising vehicle.

Table 3.3: Auditor Payoffs

High Income Report

		Audit	
		No	Yes
Actual Income	High	9	6
	Low	9	6

Low Income Report

		Audit	
		No	Yes
Actual Income	High	3	12
	Low	3	0

to audit he kept the $3 in addition to any tax revenue corresponding to his partner's report. If the auditor chose to verify his partner's income, he learned whether the taxpayer's income was misreported or not. If the income was misreported, the auditor earned the amount described above and if the report was truthful, the auditor would earn nothing extra. The payoffs for the auditor in all possible scenarios are displayed in Table 3.3, which was provided to all subjects, as was the case with the taxpayer payoff table. The taxpayer always learned whether he was audited or not and his payoff for the round. The subjects repeated the game for 20 rounds, each round with a different anonymous partner.[3]

The preceding experimental procedure describes Treatment 1 for both the auditor and taxpayer as well as Treatment 2 for the taxpayer. Treatment 1, as characterized by our theoretical framework, has all taxpayers coming from the strategic-type as opposed to the honest-type; $p = 1$. In Treatment 2, we introduced the honest-types in the form of a computer generated report that is always truthful (Kim and Waller, 2005) and that always donates to charity. The proportion of honest-types is 0.25 or $p = 0.75$. As in Treatment 1, all of the parameter information was common

[3]For one session of Treatment, subjects played 17 instead of 20 rounds.

knowledge.

For the auditor in Treatment 2, the procedure was the same as in Treatment 1 except we introduced a 25% chance that the report that an auditor would see was computer generated. The income of the computer generated report was drawn from the same distribution as the taxpayer's (50/50 chance of \$20 or \$10) and always included a donation to charity. If the auditor saw the taxpayer's actual report, then the game proceeded as in Treatment 1. If the auditor saw the computer generated report and audited it, he would receive payoffs as if he had audited a truthful report. If the auditor's corresponding taxpayer pair had misreported his income in that round, the auditor would not discover this. The taxpayer only learned whether his report was audited, not whether the auditor view his report or one that was computer generated. Similarly, the auditor did not learn at any time whether the report he viewed was from his taxpayer pair or computer generated, just whether his audit was successful if he chose to do so. Finally, one round was selected at random for the entire session, which determined payments and participants were free to leave.

3.5 Results

3.5.1 Aggregate Results

We begin our presentation of the results with a comparison of the mixed strategy component of the Perfect Bayesian equilibrium, where high income taxpayers choose whether to misreport their incomes and whether to donate to charity and auditors choose which low income reports, with and without charity donations, to audit. We then present an overview of behavior that we observed in the experiment but that off the equilibrium path in our theoretical benchmark and provide possible explanations for the discrepancy between the theory and the outcome in the laboratory.

Table 3.4: Aggregate High Income Taxpayer Reported Income and Donation Rates by Treatment

PBE Mixed Strategy If applicable	**Report** (Income, Donation)	**Treatment 1** ($p = 1$) **Predicted**	**Actual** (Std. Err.)	**Treatment 2** ($p = 0.75$) **Predicted**	**Actual** (Std. Err.)
α_0	(10, 0)	0.33	0.28	0.33	0.33
			(0.02)		(0.02)
α_1	(10, 3)	0.00	0.02	0.11	0.10
			(0.01)		(0.02)
$1 - \alpha_0 - \alpha_1$	(20, 0)	0.67	0.62	0.56	0.51
			(0.02)		(0.03)
	(20, 0)	0.00	0.08	0.00	0.06
			(0.01)		(0.01)

Any zero in the predicted column represents an off the equilibrium path prediction.

Table 3.4 presents the aggregate experimental results for taxpayer subjects with the corresponding theoretical comparisons; Table 3.5 presents the same for auditor subjects. The theoretical equilibrium predictions for taxpayer behavior are borne out in the experimental data. For high income taxpayers, the rate of misreporting without making a charity donation, α_0 , in Treatments 1 (0.28) and 2 (0.33) are not significantly different from each other (Mann-Whitney, p = 0.20). Further, in a one-sample t-test we cannot reject the hypothesis that the mean probability of misreporting of high income in the no donation-case is the same as the theoretical mean, 0.33 (p = 0.12). In the case where a high income taxpayer misreports his income and donates to charity, the strategic variable, α_1 , is significantly different between Treatments 1 (0.02) and 2 (0.10) (Mann-Whitney, $p < 0.01$), indicating that in the aggregate, the signaling device was employed by the taxpayers strategically in Treatment 2.

As to whether the theoretical predictions for α_1 are supported by the experimental data, in Treatment 2, in a one-sample t-test we cannot reject the hypothesis that the mean probability of misreporting of high income in the donation-case is the same as

Table 3.5: Aggregate Audit Rates Conditional on Viewed Income Report and Donation by Treatment

PBE Mixed Strategy If applicable	**Viewed Report** (Income, Donation)	**Treatment 1** ($p = 1$) **Predicted**	**Actual** (Std. Err.)	**Treatment 2** ($p = 0.75$) **Predicted**	**Actual** (Std. Err.)
β_0	(10, 0)	0.50	0.53	0.50	0.56
			(0.02)		(0.03)
β_1	(10, 3)	0	0.65	0.25	0.45
			(0.11)		(0.04)
	(20, 0)	0	0.25	0	0.17
			(0.03)		(0.03)
	(20,3)	0	0.36	0	0.14
			(0.08)		(0.03)

Any zero in the predicted column represents an off the equilibrium path prediction.

the theoretical mean, 0.11 (p = 0.54). However for α_1, Treatment 1, in a one-sample t-test we reject the hypothesis that the mean probability of misreporting of high income in the donation-case is the same as the theoretical mean of zero (p = 0.01). A possible accounting for this behavior could be social preferences.

We considered individuals who displayed a social preference to be those who donated to charity even when they received a low income. In Treatment 1, 4.0% of low income draws were accompanied by a donation and in Treatment 2, 4.5%. These values were not significantly different across treatments (Mann-Whitney, p = 0.719). When we exclude any individual who has displayed a social preference, the result is the same value for α_1 of 0.02 which is significantly different from zero (p = 0.01). When we exclude Rounds 6-20 from our calculation of α_0 we arrive at the theoretical result for Treatment 1, $\alpha_1 = 0$, (t-test p = 0.158), indicating that though subjects may had an initial inclination to donate to charity, as the game progressed, they were converging to the equilibrium. Subsequent regression results will confirm this (Table 3.9).

Aggregate auditor behavior (Table 3.5) is less in line with the theory, particularly for the audit rates of a low income report with a donation. But we do find, in contrast with the experimental results of Kim and Waller 2005, the theoretical insensitivity of β_0 to p holds when we introduce the charity signal. Mann-Whitney test indicates that the experimental β_0's in Treatments 1 (0.53) and 2 (0.56) are not significantly different from each other (p = 0.36). Further, in a one-sample t-test, we can reject the hypothesis that the mean probability of misreporting of high income in the no donation-case is the same as the theoretical mean, 0.5 ($p < 0.01$); auditors are auditing more frequently than the theory predicts. Mann-Whitney test indicates that the experimental β_1 in Treatments 1 (0.65, 20 observations) and 2 (0.45, 133 observations) are significantly different from each other at the 10 percent level (p = 0.10).

For both taxpayer and auditor participants, we observed off the equilibrium path behavior in our model, such as donations when incomes were low, over-reporting of income and auditing of high income reports. We can attribute it to subject confusion only to a certain extent: the control questions administered at the beginning of the experiment were meant to improve subject understanding of the payoffs. As shown in Table 3.5, auditors were verifying high income reports at rates much higher than zero: 25 percent (without donation) to 36 percent (with donation) in Treatment 1 and 17 percent (without donation) and 14 percent (with donation) in Treatment 2. Even given that taxpayers were over-reporting their incomes, the best response of the auditor would still be to refrain from auditing as he would lose \$3 and only receive \$6 in the event that a low income taxpayer was over-reporting as opposed to receiving \$9 for certain.

Table 3.6 shows that aggregate taxpayer behavior given low income is as the theory predicts. However 3 percent in Treatment 1 and 4 percent of subjects in Treatment 2 give to charity when they have a low income. Also, 4 percent of subjects over report

Table 3.6: Aggregate Low Income Taxpayer Reported Income and Donation Rates by Treatment

	Treatment 1 ($p = 1$)		**Treatment 2** ($p = 0.75$)	
Report	**Predicted**	**Actual**	**Predicted**	**Actual**
(Income, Donation)		(Std. Err.)		(Std. Err.)
(10, 0)	1	0.93	1	0.95
		(0.01)		(0.01)
(10, 3)	0	0.03	0	0.04
		(0.01)		(0.01)
(20, 0)	0	0.03	0	0.01
		(0.01)		(0.01)
(20, 3)	0	0.01	0	0.00
		(0.00)		(0.00)

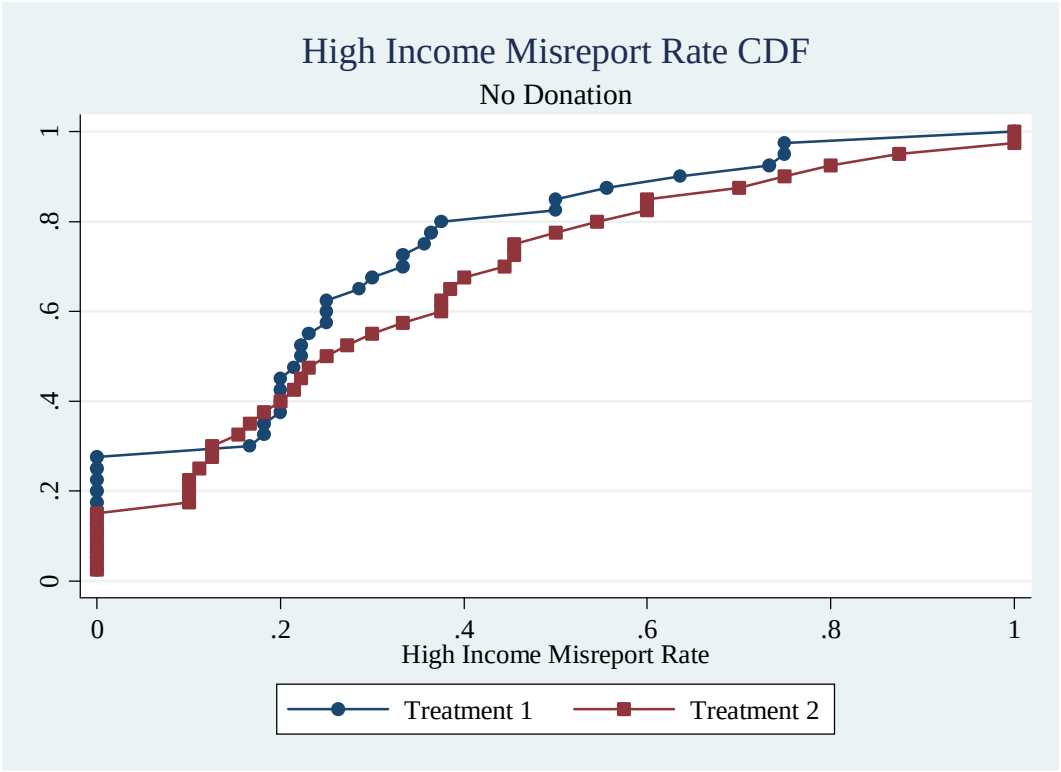
Any zero in the predicted column represents an off the equilibrium path prediction.

their income in Treatment 1 and only 1 percent do in Treatment 2.

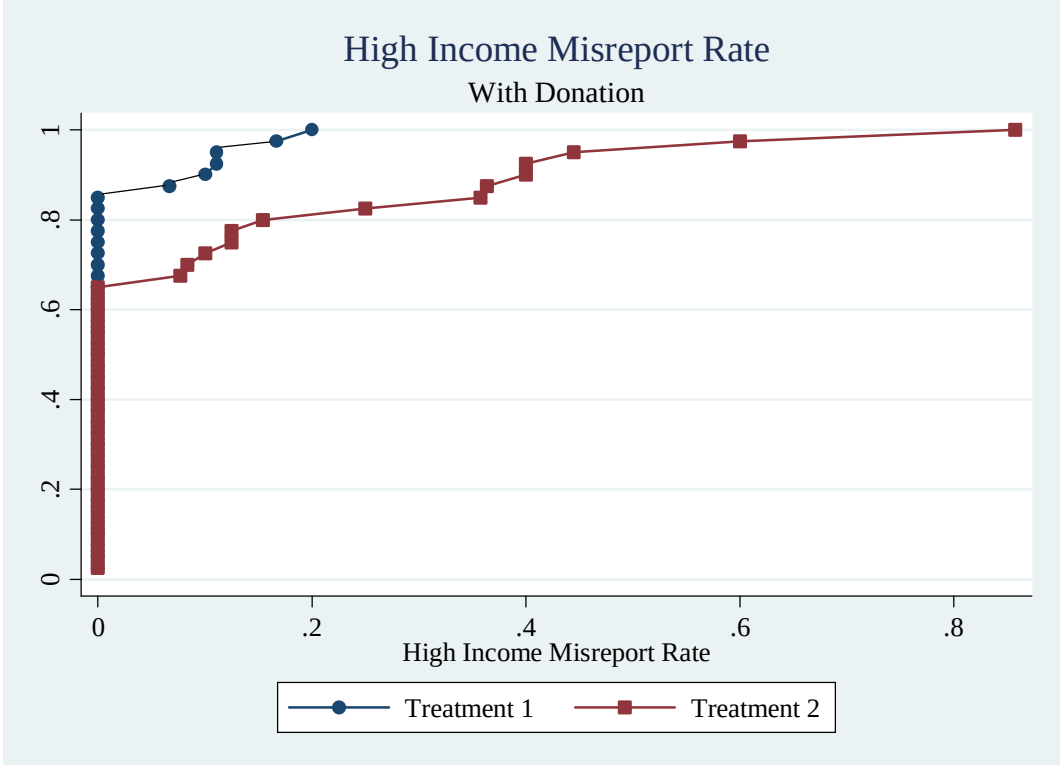
The CDF of the high income under-reporting mixed strategies, α_0 and α_1 indicates that mixed strategies were not concentrated at the prediction of 0.33 for α_0 nor were they at 0.11 for α_1. Figure 3.2b clearly displays first order stochastic dominance of α_1 in Treatment 2 over Treatment 1, though 70 percent of high income subjects never employ the signal when they misreport their income in Treatment 2, though it is still lower than the 85 percent who never donate when they are under-reporting in Treatment 1.

With the computer generated subjects in Treatment 2, the appropriate test of equilibrium behavior of taxpayer subjects involves using the effective audit rate, given that auditors were only able to audit taxpayers 75 percent of the time. The actual audit rates observed in the data are higher than the audit rates that were actually faced by the taxpayer subjects during the experiment. Table 3.7 shows that the auditors actual audit rates were significantly higher than those audit rates faced by the taxpayer. Taxpayers who reported a low income without a donation were audited 44 percent of the time as opposed to the auditors' actual audit rate of 56 percent.

Figure 3.2: CDF of Individual Taxpayer Strategiesα_0(a) and α_1(b) in Treatments 1 and 2



a)

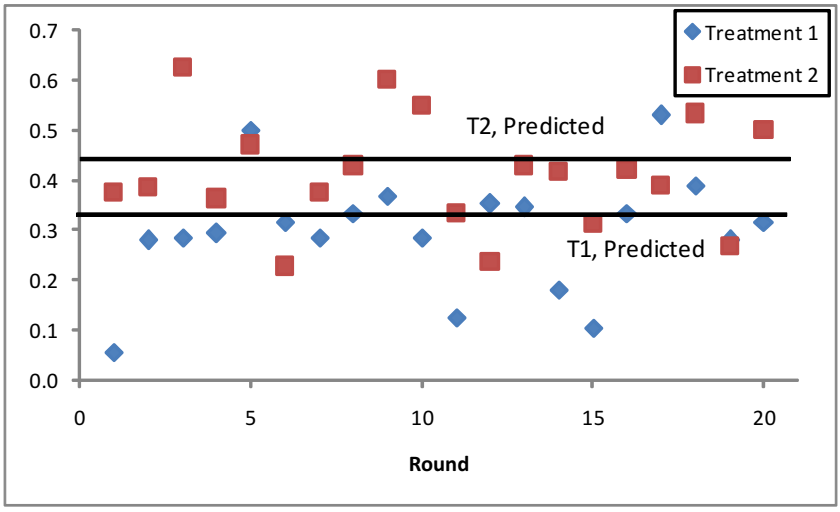


b)

Table 3.7: Effective Treatment 2 Audit Rates Faced by Taxpayers Given Computer Generated Reports

PBE Mixed Strategy	Viewed Report (Income, Donation)	Predicted (Linear Utility)	Predicted (Log Utility)	Auditor Actual (Std. Err.)	Fa
β_0	(10, 0)	0.50	0.39	0.56 (0.03)	
β_1	(10, 3)	0.25	0.36	0.45 (0.04)	

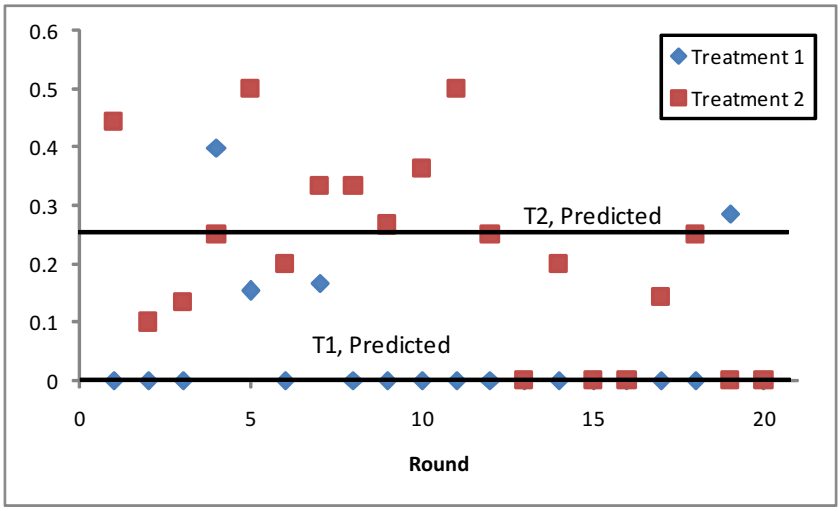
Figure 3.3: Average Percentage of Misreported High Incomes by Round and Treatment



Similarly, when making a low income report with a donation the audit rate faced by taxpayers was lower than the actual audit rate, 33 percent instead of 45 percent. The effective audit rates are consistent with the model prediction if the taxpayers are risk averse with natural log utility.

The percentage of high income subjects who misreported their income (Figure 3.3) was between 0.1 and 0.5 in each round for Treatment 1 and between 0.2 and 0.6 for Treatment 2, which corresponds to the predicted difference in misreporting by treatment of 0.11 (0.44-0.33). Except for 3 rounds, Treatment 2 misreporting levels were systematically higher than for Treatment 1.

Figure 3.4: Average Donation Percentage
Conditional on Misreporting High Income Types by Round and Treatment



Of those subjects with high income who reported low income, the donation rates are pictured in Figure 3.4. With the exceptions of rounds 4, 5, 7 and 19, the Treatment 1 donation rates were zero, which is the theoretical prediction. Given the experimental parameters, the theory predicts that high income subjects who misreport should donate to charity 25 percent of the time $(\alpha_1/(\alpha_0 + \alpha_1)) = .11/.44$ in Treatment 2. Though in Treatment 2 we do not observe convergence by round to the predicted donation rate, unlike Treatment 2, only 5 rounds had a zero donation rate for the misreporting high income types. The last rounds were more likely to have zero donation rates in Treatment 2, which could indicate that subjects were playing an end-game, though the stranger design of the experiment should have prevented this.

The CDF of individual audit rates for low income reports without donations in Figure 3.5a shows little difference in the audit rates, β_0, between the treatments as supported by our non-parametric tests and the theory. Also, the CDF shows a broad range of mixed strategies being played as opposed to the 50 percent audit rate prediction. For β_1 the CDF (Figure 3.5b) shows that auditors were primarily

playing pure strategies when they viewed a low income report with a donation, in both treatments, auditing less in Treatment 2 than in Treatment 1.

Figure 3.6 shows the CDF's for Treatment 2 audit rates of low income reports with and without donations. This graph shows whether the charity donation signal lowered audit rates. Nearly 40 percent of the auditors who viewed a low report with a donation never audited and auditors were much more likely to audit low reports without donations for average subject audit rates from 0 to 0.5. For higher average audit rates, from 0.5 to 1, we observed convergence between the audit rates for low income reports with and without the charity donations. Individual analysis suggests that certain auditor subjects over-audited in response to the positive feedback generated from having caught a liar in previous rounds.

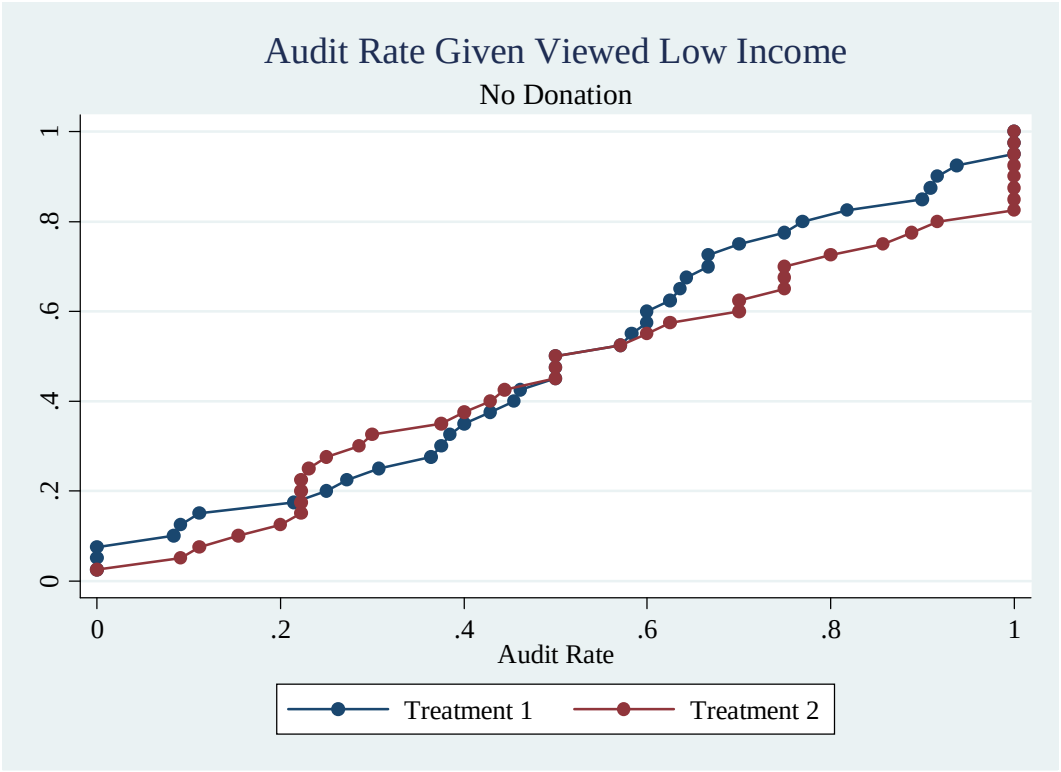
Audit rates of low income reports without donations did not vary widely by round in either of the two treatments (Figure 3.7). Though the audit rate in the data did exceed the prediction of 50 percent .

Audit rates both in the aggregate and by round for low income reports with donations, were considerably higher in our experimental data than predicted theoretical audit rate of 25 percent in Treatment 2. In Treatment 1, the theory predicted that any low income reports with donations would be off the equilibrium path, therefore auditor behavior in this sub-game was not predicted.

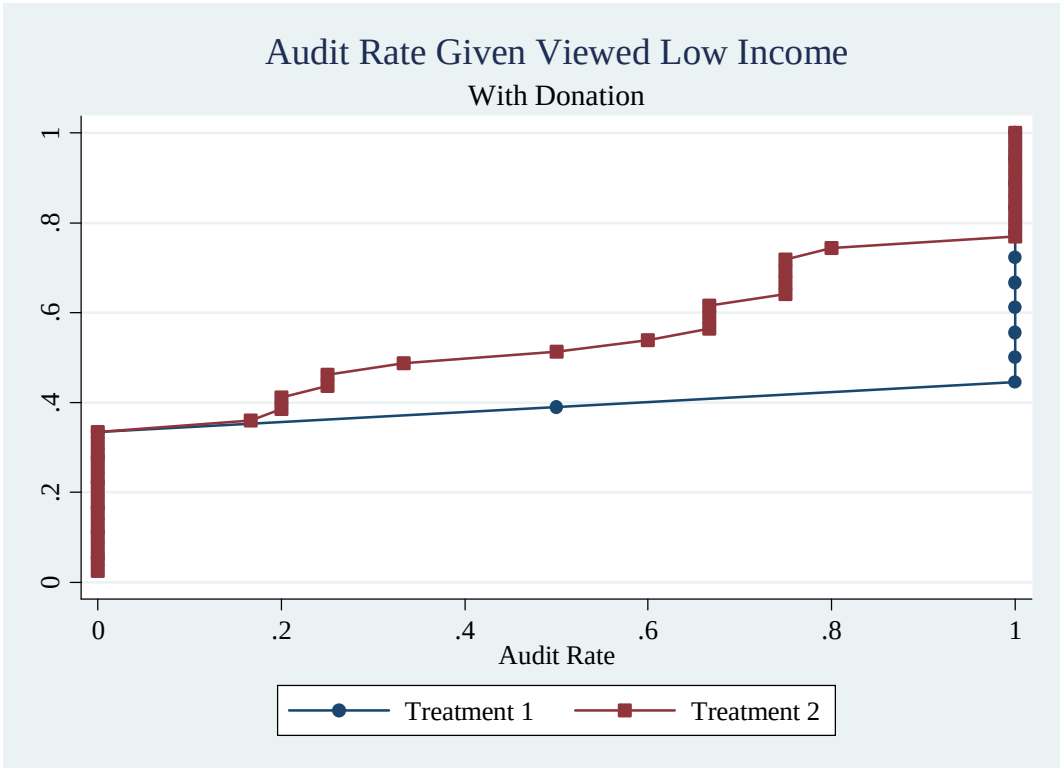
3.5.2 Behavior of Taxpayers

Though the aggregate behavior of taxpayers matches the Perfect Bayesian equilibrium predictions, individual decisions were subject to considerable noise, as indicated by Table 3.8. Backward induction of the game eliminates several strategies from

Figure 3.5: CDF of Individual Auditor Strategiesβ_0(a) and β_1(b) in Treatments 1 and 2



a)



b)

Figure 3.6: Treatment 2 CDF Audit Rates

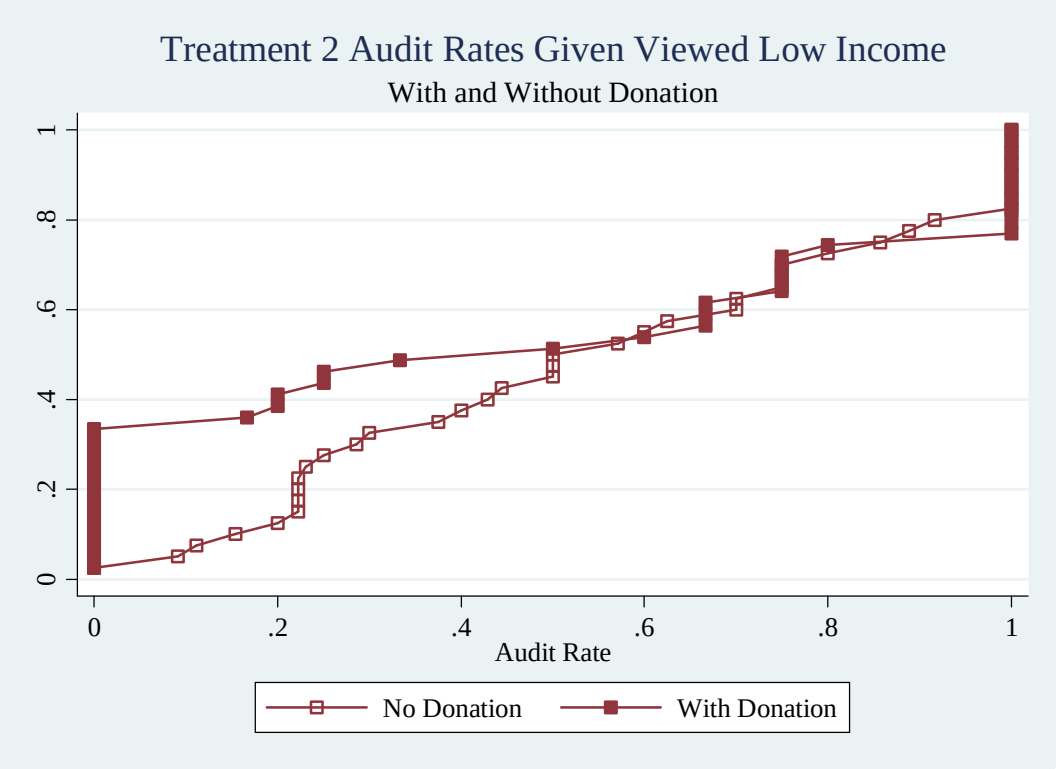


Figure 3.7: Audit Rate: Low Income Reports without Donations by Round and Treatment

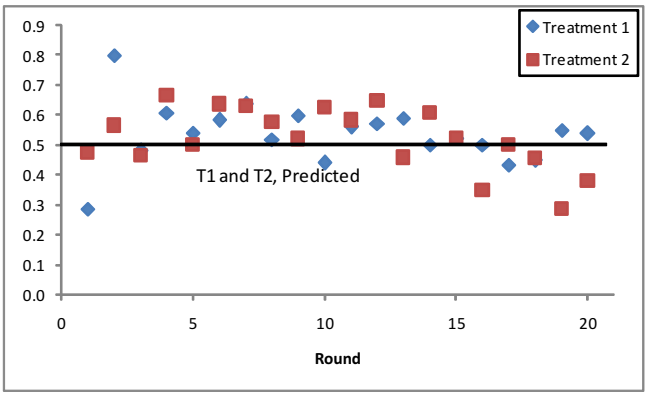
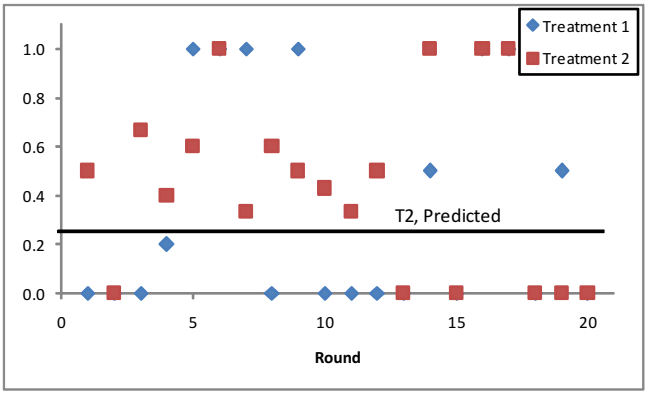


Figure 3.8: Audit Rate: Low Income Reports with Donations by Round and Treatment



consideration in the theory, though in the experiment, the strategies are played with positive probability.

Hypothesis: Taxpayers will not report high income when they received low income.

Of the 40 taxpayer subjects in each of the treatments, in Treatment 1, 30 never reported high income when their income was low and in Treatment 2, 38 never misreported their low income. Of the subjects who did misreport, the average rate of reporting high income when low was actually drawn was 0.18 for Treatment 1 and 0.16 for Treatment 2.

Beyond a preference for sharing, there is no incentive for subjects to violate this hypothesis based on own payoffs. As a test for understanding of the game, it is clear that most subjects understood the game as far as what the best move was in the case where the income draw was low.

Hypothesis: Taxpayers who report truthfully will never donate to charity.

Of the low income earners who always reported low income 7 in Treatment 1 and 9 in Treatment 2 donated to charity. As for the truthful reports of high income, 26 subjects in Treatment 1 and 30 subjects in Treatment 2 never made donations. Of those subjects who reported low income when their income was low and never gave to charity when truthfully reporting, 4 in Treatment 1 and 7 in Treatment 2 used the charity donation as a signal.

Hypothesis: Taxpayers will misreport their income when it is high while donating to charity only when some of the players are honest-types, $p < 1$**.**

Signaling behavior was observed in both treatments, in a manner that is in line with the predictions, namely that more signaling behavior will be observed in Treatment 2 than in Treatment 1. In Treatment 1, 6 subjects used the signal and in Treatment 2, 13 did. When we identify which of those subjects do not display any preferences for make a donation when their income is high, the number of subjects using the signal drops in each treatment, to 5 in Treatment 1 and 8 in Treatment 2.

As for those subjects who made consistent reports, in Treatment 1, 9 always reported truthfully when earning high income and in Treatment 2, 5 subjects never lied on high income draws. On the other side of the spectrum, only 1 subject in Treatment 1 always reported low income when earning high income and 2 subjects in Treatment 2. Finally, very few subjects donated when truthfully reporting high income and did not donate when lying about their high income; 5 in Treatment 1 and 2 in Treatment 2.

Figure 3.9: Individual Taxpayer Behavior, Treatment 1 (A), Treatment 2 (B)

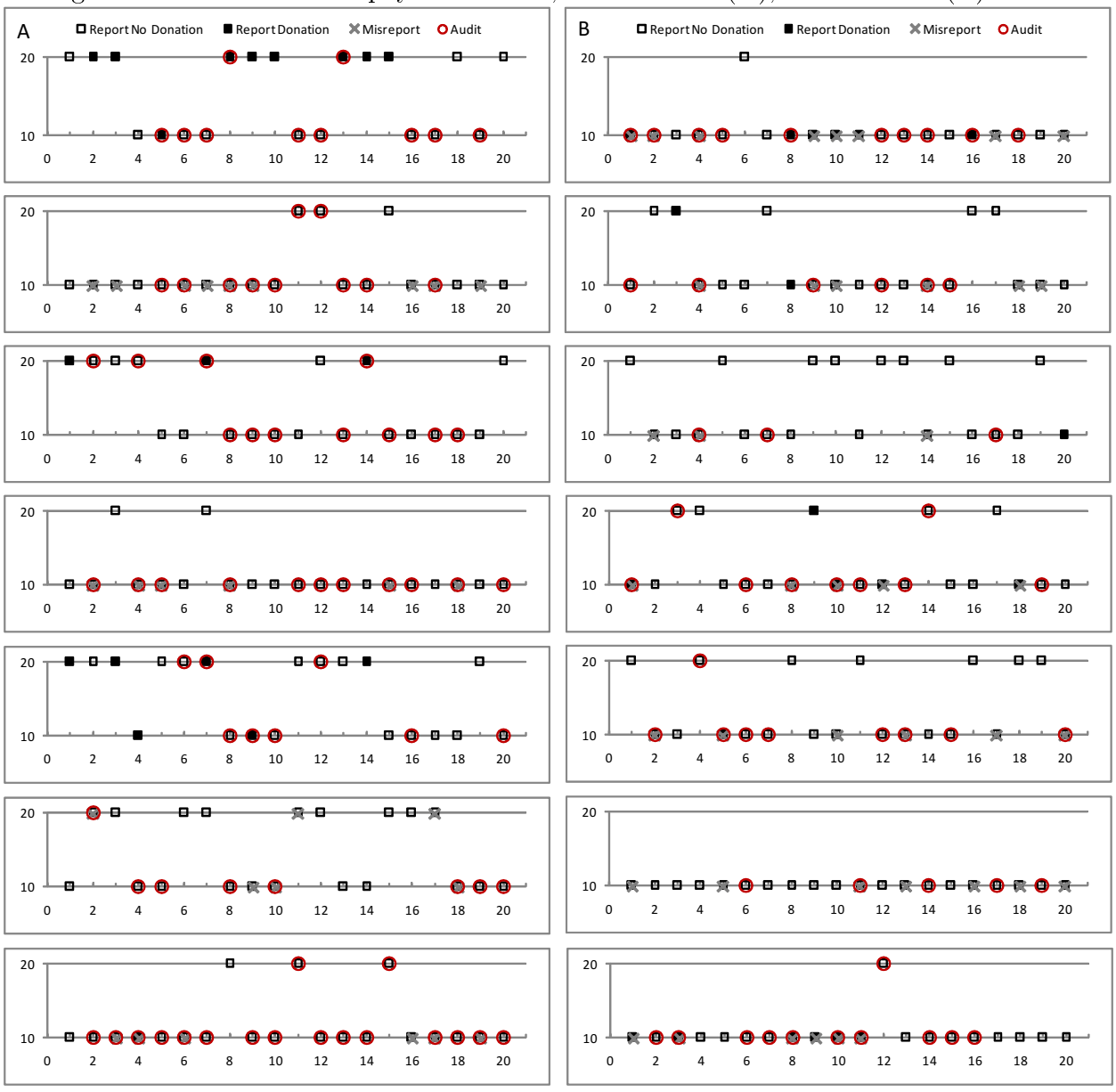


Table 3.8: Summary of Individual Taxpayer Decisions (number of subjects)

	T1	T2
Low income: never report High income	**30**	**38**
Of which make Donation	7	9
Low income: always report Low income, never make Donation	**23**	**29**
Of which never make Donation when truthfully report High income	18	24
Of which use Signal, High income: report Low income, make Donation	**4**	**7**
High income: never report Low income	9	5
High income: always report Low income	1	2
High income: if report High income, never make Donation	**26**	**30**
High income: if report Low income, make donation	6	13
Of which never make Donation when report High income	**5**	**8**
High income: make Donation when report High, never make Donation when report Low	5	2

A sample of individual taxpayer behavior in Treatments 1 and 2 presented in Figure 3.9 indicates that few patterns can be established across taxpayers. We found no effect of the sanctions in prior rounds on current period behavior. The coefficients on a misreporting dummy, which takes a value 1 if truthfully reporting and 0 if not, on previous round audits, in the preceding period up to any of the 5 preceding periods were insignificant in probit regressions.

The probit analysis in Table 3.9 regresses the donation decision on a mis-reporting income dummy that takes a value of one if a subject lied in his report and 0 otherwise, income, which could be either 10 or 20, the round of play and a dummy variable that takes a value of 1 if the subject was audited in the previous period. The results show that income made subjects more likely to donate to charity by 7 percent ($p = 0.02$) in Treatment 1 and 16 percent ($p < 0.01$) in Treatment 2. [4] In Treatment 1, if a subject

[4]The coefficients in the probit table represent marginal effects at Income=20, Misreport=1, Round=10 and Previous audit=1. Because each variable takes on only two values, the percentages are obtained by multiplying the coefficients by the difference in the high and low values of the variables. For income, the factor is 10.

Table 3.9: Determinants of Taxpayer Donation, Probit Analysis by Treatment

	Treatment 1		**Treatment 2**	
			(p-value)	
	1	**2**	**3**	**4**
Misreport	-0.031	-0.027	0.086	0.101
	(0.343)	(0.352)	(0.018)	(0.010)
Income	0.008	0.007	0.014	0.016
	(0.018)	(0.016)	(0.002)	(0.001)
Round	-0.004	-0.004	-0.009	-0.010
	(0.081)	(0.078)	(0.013)	(0.012)
Audited in	0.018		-0.056	
previous period	(0.397)		(0.169)	
N	760	760	736	736

Marginal Effects at Income=20, Misreport=1, Round=10 and Audit=1.

misreported his income, he was less likely to make a donation, though this coefficient is not significant (p = 0.34). However in Treatment 2, the subject's misreporting factored significantly into the probability of donation: subjects were 10% (p = 0.01) more likely to donate if they were lying (Regression 4). Similar to our observation and statistical analysis regarding the effect of previous round audits on lying behavior, we found that previous round audits have no impact on donation behavior in either treatment (Regressions 1 and 3).

3.5.3 Behavior of Auditors

Hypothesis: Auditors will not audit taxpayers who report high income.

Table 3.10 presents a summary of individual auditing behavior as well as average audit rates conditional upon which signal was viewed by the auditor. The average audit rates were all greater than zero for high income reports in both treatments. Out of 40 auditor subjects in Treatment 1, only 16 subjects never audited high income

reports without donations and 13 never audited high income reports with donations. In Treatment 2, subject behavior was closer to the equilibrium prediction with 29 subjects who never audited a high income report without a donation and 25 subjects never audited a high income report with a donation. Consistent with the payoff structure, low income reports were audited more than high income reports and reports.

Hypothesis: Auditors will audit low reports without charity donations with higher probability than those with charity donations.

In each of the treatments, over 50 percent of the auditors never audited a low report with a donation and 8 percent always audited those reports. In the aggregate we find no significant difference between the audit rates of low reports with and without a donation in either treatment. However, at the individual level there are more pure strategies being played by the auditor subjects when a low income report is accompanied by a donation. In Treatment 1, 3 subjects always audited low income reports with donations and 3 subjects never do. The other 34 auditors played mixed strategies. In Treatment 2, 28 of the 40 subjects played mixed strategies when no donation is observed. When the taxpayer does make a donation and report a low income, only 24 auditors played mixed strategies in Treatment 1 and fewer, 19, in Treatment 2.

The probit analysis in Table 3.11 of the auditor decision based on the viewed income report, donation and a dummy variable that takes on the value 1 if the auditor detected a liar in the previous period indicates that the auditor was 39 percent more likely to initiate an audit for low income reports in Treatment 1 and 24 percent more likely in Treatment 2, both significant at the 1 percent level. For reports with

Table 3.10: Summary of Individual Auditor Decisions

	Treatment 1	Treatment 2
Low income, no Donation: Signal (10, 0)		
Audit Rate	0.52	0.59
Standard Error	0.01	0.02
# of Subjects who Always audit	3	10
# of Subjects who Never audit	3	2
Low income, Donation: Signal (10, 3)		
Audit Rate	0.64	0.53
Standard Deviation	0.11	0.04
# of Subjects who Always audit	11	11
# of Subjects who Never audit	6	10
High income, no Donation: Signal (20, 0)		
Audit Rate	0.25	0.14
Standard Deviation	0.02	0.02
# of Subjects who Always audit	1	2
# of Subjects who Never audit	16	29
High income, Donation: Signal (20, 3), Observations		
Audit Rate	0.36	0.20
Standard Deviation	0.07	0.04
# of Subjects who Always audit	7	5
# of Subjects who Never audit	13	25

Table 3.11: Probit: Independent Variable Audit (Marginal Effects)

	Treatment 1	Treatment 2
	(p-value)	
Income Report	-0.024	-0.039
	(0.000)	(0.000)
Donation	0.037	-0.027
	(0.023)	(0.060)
Caught a liar	0.086	0.176
previous round	(0.109)	(0.006)
N	760	736

Marginal Effects at Income=10, Donation=3 and Caught Liar=1.

a donation in Treatment 1, the coefficient on donation of 0.037 represents an 11 percent increase in the probability of audit if a donation is viewed by the auditor. [5] The model predictions place zero probability on any donations being made if all types are strategic. The positive and significant coefficient on donations in Treatment 1 one indicates that the charity donation may have been interpreted as a signal of non-compliance without our prompting of the subjects in the opposite direction, as we do in Treatment 2. As we see in Treatment 2, an auditor who viewed a charity donation was 8 percent less likely to audit the report than if no donation had been made. When we impose the computer generated subjects with 25 percent chance on the auditor in Treatment 2, making it was less likely to catch a misreporting taxpayer subject, the auditors response to catching a liar in the previous period becomes highly significant in determining whether he audits in the current period he was more likely to audit: 9 percent more likely in Treatment 1 ($p = 0.11$) and 18 percent more likely in Treatment 2 ($p = 0.01$). The significant response to past experience in our result suggests an adaptive learning process is influencing the auditor's equilibrium strategy.

[5]The coefficients in the probit table represent marginal effects at Report=10 and Donation=3. Because each variable takes on only two values, the percentages are obtained by multiplying the coefficients by the difference in the high and low values of the variables. For income, the factor is 10 and for the donation, the factor is 3.

3.6 Discussion and Conclusion

Though the aggregate likelihood of the use of a charity donation as signal of honesty by taxpayers conforms with the numerical theoretical predictions, our results indicate that at the individual level, the response to the change in composition of the population when we introduced the compliant types was heterogeneous across subjects and there was little indication that all subjects would converge to the equilibrium strategy in additional rounds. In identify individual strategies across treatments and in a binary choice model analysis of taxpayer behavior we found that on the margin, there were more subjects who used the donation as a signal of honesty when the proportion of compliant types in the population increased, $p = 0.75$. However, in the treatment with compliant types the use of the signal by taxpayer subjects did not increase for 27 out of 40 subjects when the theory would have predicted increased probability of donation when under-reporting income for all subjects.

Auditor behavior in the experiment generally consistent with the Perfect Bayesian equilibrium probabilities whether a low income report was or was not accompanied by a donation; auditors audited low income reports without donations at higher rates than they did low income reports with donations. However, we identified subjects who consistently over-audited low income reports (more than 50 percent of the time) which drove aggregate audit rates higher than predicted. Also, audits were systematically initiated off the equilibrium path. We explain this by showing that auditors were responding to feedback after having caught dishonest taxpayers in previous rounds. Even with the higher audit rates played by auditor subjects in Treatment 2, taxpayers faced audit rates that were in line with equilibrium predictions because of the lower chance of audit with the computer generated reports. In this regard, the aggregate taxpayer behavior is fully in line with the equilibrium predictions.

One aspect of the experimental assumption that all subjects will behave as if they are strategic types on the taxpayer side is called into question by our results. In each of the treatments, there were a significant number of subjects, 23 percent in Treatment 1 and 13 percent in Treatment 2, who never misreported their incomes. Though high risk aversion of could be used to explain this, it is still an indication that individual level heterogeneity had a significant impact on whether the donation was used as a signal.

The random matching of subjects ensured that we did not have generate reputation concerns, but an adaptive learning process was most likely at work. In the case of the auditors, we find evidence from our regression analysis (Table 3.8) that auditors were generally more inclined to audit low income reports without donations, as the theory suggests, but a strong and significant predictor of audit was whether an auditor has caught a taxpayer who was misreporting his income in a previous round. Similar to studies that support the adaptive learning model to predict subject behavior in games with mixed strategy equilibria using repeated play in a simultaneous move games [Ochs, 1995, Erev and Roth, 1998], our results indicate that auditors in our sequential move game made decisions based on very basic feedback from the previous round. The static theoretical framework that we used to set our benchmark predictions did not consider this motive for auditing and the feedback may have explained why we observed audit rates that were higher than our predictions.

In addition to finding that players in our experiment do increase donations in response to a change in the composition of the population of strategic and compliant types, that the increased donations are accompanied by increased misreporting rates of income in a manner that is consistent with the Perfect Bayesian equilibrium. Though we did observe that people may have wanted to give to the charity, absent an strategic considerations, our analysis suggests that we can expect subject donations

to converge to the theoretical prediction of zero whenever subjects are truthfully reporting their income. Combined with the significant increase in charity donations when we introduced compliant types, we have demonstrated a purely rational motive for charitable giving; of the many explanations for charitable donations[Androeni, 1990, Androeni and Petrie, 2004], and more generally, contributions to public goods [Fehr and Gaechter, 2000], few papers consider what might appear to be pro-social behavior in the way we have, as a signaling device that can be used to divert the attention of authorities from punishable behavior.

Chapter A

Preferences for Competition in Ghana: Experiment Instructions

Experiment Script: We are part of a research team from University of Maryland from the Economics and we have come to (Town) to study various aspects of doing business in Africa. We have asked you here to participate in some of our tests that are essentially very simple games. To make the playing of these games worth your time and effort, we will pay each participant 5 cedis for participating plus any winnings based on how well you do in the games. On average, you should take home 10 cedis. All information will be kept secret and only the researchers will have access to it. Your decision to play or not play will have absolutely no impact on your current employment and all data we collect will not include your name. If you decide not to participate in this study or if you stop participating at any time, you will not be penalized or lose any benefits to which you otherwise qualify.

Participation in this experiment involves: filling out a brief questionnaire which we will provide to you that asks your hometown, age, level of education, number of languages spoken, job title and description, time at current position and other jobs or business activities; playing a matching game; and answering some questions. This session should last no longer than 90 minutes.

You will sit and your desk and we will provide you with a basket containing many coins, coffee beans, paper clips, pebbles, beans and okra. Next we will show you a picture and you will have one minute to place the items from your basket in the exact order of the picture. (Participants will play a practice round.) You will play the game four times and we will give you specific instructions for how you will be paid at the beginning of each round.

Game 1: You will be paid 50 peswas for each correctly ordered object.

Game 2: Now we are dividing the players into groups of 4. You will not know who is in your group, but you will know that it is someone in this room. The person in each group who places the most correct objects in order will receive 2 cedis per item, the others will receive nothing.

Game 3: You may now choose whether you would like to be paid 50 peswas per item, as you were paid in Game 1, or you can play as you did in Game 2, where you receive 2 cedis per item only if you win. If you choose Game 2 format, you will not play against each other; you will play against the scores in Game 2. It is possible for all of you to win if you do better than the winner in your small group last round. You can all lose, too.

Game 4: Would like to be paid a piece rate or tournament wage for Treatment 1 performance? This choice will not affect your payment from the first game, this is just like a 4th game that you will not play.

Follow-up questions: Would you like to be paid for Game 1 per item or using the tournament scheme? How well do you think you did in Game 2 (rank: 1st, 2nd, 3rd or 4th)?

Table A.1: Subject Datasheet

Initials _____ *Date* _____

Subject Datasheet

Group	*Round:* *Station:*
Gender	1.Male 2.Female
Age	
Hometown/neighborhood (birth, current)	*Birthplace:* *Current:*
How many languages do you speak?	
Highest Level of Education	*1.None* *2.Primary 1* *3.Primary 2* *4.Secondary* *5.Bachelors* *6.Masters* *7.Doctorate* *8.Technical or Professional Degree* *9.Other*
Occupation and Title	
Years at current position	
Other business activities	
Monthly income	
Please do not fill in:	
Treatment 1 (T1)	*Score:*
Treatment 2 (T2)	*Score:*
Treatment 3 (T3)	*P or T (circle one)* *Score:*
Treatment 4 (T1 performance)	*Piece Rate or Tournament (circle one)*
Self Rank T1	*1st* *2nd* *3rd* *4th (Last)*
Self Rank T2	*1st* *2nd* *3rd* *4th (Last)*

Chapter B

Tax Evasion and Charity: Experiment Instructions

B.1 Treatment 1 Instructions

This is an experiment in decision making. Research foundations have provided funds for conducting this research. Your earnings will depend partly on your decisions and the decisions of the other participants and partly on chance. Please pay careful attention to the instructions as a considerable amount of money is at stake.

The entire experiment should be complete within an hour. At the end of the experiment you will be paid privately. At this time, you will receive $5 as a participation fee (simply for showing up on time). Details of how you will make decisions and receive payments will be provided below.

A Decision Problem: In this experiment, you will participate in 20 independent decision problems that share a common form. This section describes in detail the process that will be repeated in all decision problems.

Before the beginning of the experiment, each participant will be randomly assigned into two groups. Half of the participants will be in "Group A", and half of the participants will be in"Group B". Your assigned group will stay the same throughout the end of the experiment. At each of the 20 rounds, one participant from Group A will be randomly matched with one participant from Group B.

If you are assigned to "Group A": Each round, you will have an equal chance of receiving an either $20 or $10. The amount you received is your private information and known only by you. You must decide whether to disclose $20 or $10. You must pay a portion of the money you receive back to the experimenter, which will be paid to your Group B counterpart, depending on which amount you received: if you received $20, you must pay back $6; if you received $10, you do not have to pay back the experimenter. The amount immediately deducted from your account will be based on the amount you disclose. You may disclose any amount, but if you are selected for verification, you will be subject to a penalty if your disclosure does not match the actual amount you received. You will also have the option to donate $3 from the money you have received to the Maryland Fund for Excellence. If you choose to donate to charity, your donation will be subtracted from the money you have received and will have no impact on the amount you must pay back to the experimenter.

Verification

Each round, you will be paired with a different participant at random from Group B who may choose to verify your reported income. If your income report is chosen for verification and you have misreported your income, you will pay the experimenter the amount required based on your actual income plus a penalty of $6.

Below you can find your payoffs under each possible scenario,

At the end of each round, you will learn whether your partner decided to verify your income or not.

If you are assigned to "Group B": Each round, you will be paired with a different participant at random from Group A. You will learn his/her reported income and his/her actual charity donation to the Maryland Fund for Excellence. His/her reported income will determine how much he/she pays to the experimenter, which will be in turn given to you. After learning this information, you will be asked whether

Table B.1: PAYOFF TABLES, Group A

$20								
Income: 20		Verification			Income: 20		Verification	
No Donation		No	Yes		Donation		No	Yes
Report	20	14	14		Report	20	11	11
	10	20	8			10	17	5
$10								
Income: 10		Verification			Income: 10		Verification	
No Donation		No	Yes		Donation		No	Yes
Report	20	4	4		Report	20	1	1
	10	10	10			10	7	7

Table B.2: PAYOFF TABLES, Group B

$20 Report		Verification	
		No	Yes
Actual	20	9	6
	10	9	6
$10 Report			
		Verification	
		No	Yes
Actual	20	3	12
	10	3	0

you like to verify his/her income. You will be given $3 that you may use to initiate a verification. If you initiate the verification of your counterpart in Group A, you will incur a cost of $3. If you choose to verify your partner's income, you will learn whether he/she misreported or not. If the income was misreported, you will earn the $6 fine plus the difference in tax revenue due to the misreporting. If your partner reported his/her income correctly, then you will not earn any additional amount. If you do not initiate a verification, you will keep the $3.

Below you can find your payoffs under each possible scenario,

Earnings At the end of the experiment, the computer will randomly select one

decision round. Each round is equally likely to be selected.

Rules: All information will be kept secret and only the researchers will have access to it. Your decision to play or not play will have absolutely no impact on your course grades or employment and all data we collect will be anonymous. If you decide not to participate in this study or if you stop participating at any time, you will not be penalized or lose any benefits to which you otherwise qualify. Please do not talk with anyone during the experiment. We ask everyone to remain silent until the end of the last round. If there are no further questions, you are ready to start.

B.2 Treatment 2 Instructions

This is an experiment in decision making. Research foundations have provided funds for conducting this research. Your earnings will depend partly on your decisions and the decisions of the other participants and partly on chance. Please pay careful attention to the instructions as a considerable amount of money is at stake.

The entire experiment should be completed within an hour. At the end of the experiment you will be paid privately. At this time, you will receive $5 as a participation fee (simply for showing up on time). Details of how you will make decisions and receive payments will be provided below.

A Decision Problem: In this experiment, you will participate in 20 independent decision problems that share a common form. This section describes in detail the process that will be repeated in all decision problems.

Before the beginning of the experiment, each participant will be randomly assigned into two groups. Half of the participants will be in "Group A", and half of the participants will be in "Group B". Your assigned group will stay the same throughout

the end of the experiment. At each of the 20 rounds, one participant from Group A will be randomly matched with one participant from Group B.

If you are assigned to "Group A": Each round, you will have an equal chance of receiving an either $20 or $ 10. The amount you received is your private information and known only by you. You must decide whether to disclose $20 or $10. You must pay a portion of the money you receive back to the experimenter, which will be paid to your Group B counterpart, depending on which amount you received: if you received $20, you must pay back $6; if you received $10, you are not required to pay anything back. The amount immediately deducted from your account will be based on the amount you disclose. You may disclose any amount, but if you are selected for verification by your counterpart, you will be subject to a penalty if your disclosure does not match the actual amount you received. You will also have the option to donate $3 from the money you have received to the Maryland Fund for Excellence. If you choose to donate to charity, your donation will be subtracted from the money you have received and will have no impact on the amount you must pay back to the experimenter.

Verification

Each round, you will be paired with a different participant at random from Group B who may choose to verify your reported income. However, there is a chance that your counterpart in Group B will NOT see your report instead he/she will see a computer-generated report. There is an equal chance that the income from the computer-generated report is $20 or $10 and the computer-generated report will always include a charity donation. With probability $\frac{3}{4}$ (75% chance) he/she will see your report and with probability $\frac{1}{4}$ (25% chance) he/she will see the computer-generated report. If your counterpart sees your reported income and charity donation and chooses to verify it, then if you have misreported your income, you will pay the experimenter

Table B.3: PAYOFF TABLES, Group A

$20								
Income: 20		Verification			Income: 20		Verification	
No Donation		No	Yes		Donation		No	Yes
Report	20	14	14		Report	20	11	11
	10	20	8			10	17	5
$10								
Income: 10		Verification			Income: 10		Verification	
No Donation		No	Yes		Donation		No	Yes
Report	20	4	4		Report	20	1	1
	10	10	10			10	7	7

the amount required based on your actual income plus a penalty of $6. If your report is truthful, or if your counterpart sees the computer-generated report then you will not pay anything extra. Below you can find your payoffs under each possible scenario; verification Yes means that your counterpart sees your report and chooses to verify it; verification No means that either your counterpart sees your report but does not choose to verify it or he/she has sees the computer-generated report:

At the end of each round, you will only learn whether your report has been verified or not. You will not learn whether your Group B counterpart saw your report or the computer-generated report.

If you are assigned to "Group B": Each round, you will be paired with a different participant at random from Group A. You will see a report that shows a report of income and charity a donation. There is a 75% chance that what you see is the income report from your counterpart in Group A and his/her actual donation to charity. However, there is a 25% chance that the income report and charity donation you see are computer generated. There is an equal chance that the income from the computer-generated report is $20 or $10 and the computer-generated report will always include a charity donation. After viewing the report on your screen, you will

Table B.4: PAYOFF TABLES, Group B

$20 Report				Computer Generated	
		Verification		Verification	
		No	Yes	No	Yes
Actual	20	9	6	9	6
	10	9	6	NA	NA
$10 Report				Computer Generated	
		Verification		Verification	
		No	Yes	No	Yes
Actual	20	3	12	NA	NA
	10	3	0	3	0

be asked whether you would like to verify the income report. You will be given $3 that you may use to initiate a verification. If you initiate the verification of your counterpart in group A, you will incur a cost of $3. If you choose to verify your partner's income, you will learn whether he/she misreported or not. If the income was misreported, you will earn the $6 fine plus the difference in tax revenue due to the misreporting. If your partner reported his/her income correctly or the report you chose to verify is a computer generated report, then you will not earn any additional amount. If you do not initiate a verification, you will keep the $3.

Below you can find your payoffs under each possible scenario when the report you saw was submitted by your counterpart or was computer-generated:

Earnings At the end of the experiment, the computer will randomly select one decision round do determine your payment. Each round is equally likely to be selected.

Rules: All information will be kept secret and only the researchers will have access to it. Your decision to play or not play will have absolutely no impact on your course grades or employment and all data we collect will be anonymous. If you decide not to participate in this study or if you stop participating at any time, you will not be penalized or lose any benefits to which you otherwise qualify. Please do not talk with

anyone during the experiment. We ask everyone to remain silent until the end of the last round. If there are no further questions, you are ready to start.

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