ESSAYS IN THE MICROECONOMICS OF INCENTIVES, GOVERNMENT PROGRAMS AND COMMUNICATION

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DEDICATION

I dedicate this dissertation to my family for their love and support. My parents Nicolae and Valeria Stoian have instilled in me early on in life their passion for reading and my sister Ana-Maria has encouraged me throughout the process.

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ABSTRACT

This dissertation consists of three essays in applied microeconomics. The first chapter offers an overview of the work, highlighting the methodology and the main results.

The second chapter extensively discusses how one could and should take into account two different but inter-related impacts that tournament prizes have on outcome: the sorting and the incentive effects. The sorting effect refers to the fact that if higher prizes are offered in a tournament, more able participants will join. The incentive effect of prizes relates to an increase in effort of participants that already decided to join a competition when there is an increase in prizes. Previous theoretical and empirical literature focused mainly on the second effect as if relevantly economic tournaments are close in nature. Also, previous empirical studies missed an important channel through which prizes affect outcome and likely estimated biased coefficients for the incentive effect.

The third chapter analyzes the impact of the first old-age relief program on the health of the elderly in the United States in the 1930s. The study attempts to provide a picture of how the elderly would fare in an economy where the Social Security system of today does not exist but instead a less birocratic and costly system is in place. The 1930s provides an economist interested in such a counterfactual analysis a unique opportunity since this is precisely the time when Social Security had not started to make payments yet but the states and the federal government became involved in financially supporting the needy elderly.

The fourth chapter examines whether public messages can break bubbles in experimental asset markets. This study has policy relevancy in terms of the role a central

bank might have in targeting not only inflation as currently defined but asset prices as well. Whereas this role is controversial and remains to be determined, theoretical models advanced the idea of public messages as potential coordination devices among traders in an environment that experiences a bubble. Chapter 4 details the design and results of an experiment that tests this coordination role of a public message.

The final chapter summarizes the contributions to the literature and the general findings.

Chapter 1

INTRODUCTION

The theoretical and empirical labor economics literatures analyze how people respond to reward systems in settings where participation is limited to those in the system. Empirically, however, people are able to choose among jobs or tournaments with different reward structures. Prior work has paid insufficient attention to this choice. In Chapter 2 I examine how rewards influence both the choice of contests and effort exerted during a contest. Another feature of the specification I employ is that I allow individual effort in a tournament to be influenced by how close in ability other participants are with the participant in question.

I developed a dataset that includes a large number of athletes running multiple times in different races that award different prizes. The data is richer than prior datasets used to address the issue because it includes not just the participants who won money but also those who had a chance to earn prizes, but did not.

First, I estimate the race choice decision with a multinomial logit in which competitors choose among a set of races during a specified time frame. The results show that good runners are attracted to races with higher purse, but their choice is not influenced by the inequality of the distribution of the payouts.

The second phase examines the impact of prizes on effort during a race. As the intensity of the competition increases, the runner increases his effort. Intensity of the competition is captured by the number of runners in the race and by the number of runners that are close in ability to the runner in question. This is the first analysis in the tournament literature to examine the impact of the closeness in ability on effort, holding prize money, prize distributions and ability constant. Further, if the analysis does not control for the intensity of the competition or for the runners' ability, the coefficient on prizes is overstated by two to three times. Prizes impact runner's effort as his average speed increases in response to larger prizes and prizes that are more equally distributed.

The two-part model allows me to perform a counterfactual analysis that examines how the selection of tournaments and effort within a contest would change if prizes in one tournament would change. I use this to illustrate that the impact of prizes is heterogeneous across runners and that the selection and incentive effects are interdependent.

The third chapter examines the effectiveness of the means-tested Old-Age Assistance program in reducing mortality among the elderly. During the Great Depression poverty among the elderly was a serious problem. The Roosevelt administration considered two federal solutions to this problem. One was to provide federal matching grants to subsidize state-initiated Old Age Assistance (OAA) programs that provided money to elderly whose income was below a certain threshold. A second solution was to start a national system of pensions, currently known as Social Security. Ultimately, the Social Security Act of 1935 chose both.

OAA programs potentially had conflicting effects on the mortality of the elderly. The intention of the legislators was to provide means for the poor elderly to live on their own and avoid living in almshouses. Mortality would have fallen due to reduced exposure to infectious disease in almshouses. On the other hand, OAA might have contributed to a rise in mortality if the poor elderly living outside almshouses lost access to the rudimentary health care provided at almshouses or due to greater isolation while living on their own.

To test which of these effects was strongest, I developed an annual panel data set for 75 cities for the years 1929 through 1938 that combines information on the state Old Age Assistance programs with information on city mortality rates for different age groups. The extent of Old Age Assistance is measured in three ways: a dummy variable for the payment of OAA benefits, benefits paid relative to the population over 65, and the maximum monthly payment of the benefits. For each measure, I run a series of specifications. A basic OLS specification indicates that OAA assistance had a desirable, negative effect on mortality rates. The negative effect seems to be the result of omitted variable bias. I perform a difference-in-difference analysis with city and year fixed effects, then a difference-in-difference-in-difference specification that adds differences between the eligible age group and younger age groups, and final an instrumental variable analysis designed to eliminate biases from the political-economic process of adopting the program at the state level. In each case, OAA programs have no statistically significant benefit of reducing mortality.

Chapter 4 analyzes the potential coordination role of a public message in an asset market where trades are made at values that depart from fundamentals. A policy relevant question that the study attempts to answer is if central banks can prevent asset market bubbles through public announcements. This is a difficult question to answer empirically because of the uncertain fundamental values of assets. As a result, I follow economists who have used laboratory experiments to examine how bubbles might be prevented in a laboratory where the fundamental values are known. The theoretical justification of trading persistently at prices higher than fundamental values is provided by Abreu and Brunnermeier

(2003). They argue that this could be possible due to a lack of coordination between rational traders who even if collectively have the capacity to break the bubble they lack the individual incentive to do it. In this framework, public statements in the market might constitute coordination devices that prevent the formation of bubbles.

I build on the design of previous laboratory bubble experiments by adding treatment sessions in which a message about the fundamental value of the asset during the next trading period is sent once to all traders before one of the trading periods. Traders know that everybody else got the same message at the same time and the message offers no new information. The results show only weak evidence that the message has the capacity to break the bubble.

Chapter 2

MEASURING THE SORTING AND INCENTIVE EFFECTS OF TOURNAMENT PRIZES[1]

2.1. Introduction

Tournaments are characterized by rewards determined by ordinal position: in tournaments it is relative output, and not absolute output, that determines payment. Competitions of this type appear often in the economy. Examples include promotions in corporations and universities, patent races, political elections, sporting events.

In general, tournaments are used as mechanisms to induce desired behavior by the participants. For example, promotion based on relative performance may serve to motivate employees to work hard, and prizes in sporting events based on competitors' finishing positions may motivate participants to train hard prior to an event and to exert effort in the event itself.

Although there are a wide range of questions that have been considered concerning tournament design and the behavior of tournament participants, researchers have most often considered "within" tournament issues rather than "between" tournament issues. The focus has usually been on the incentive effect of prizes on the behavior of participants, assuming they are irrevocably committed to a particular tournament. In general, less attention has been given to the broader question of how participants sort into particular tournaments based on prizes and other characteristics.

[1] Co-authored with Tim Davies

We developed a large data set of athletes running multiple times in different races that award different prizes. Unlike those used in prior studies, our data includes runners that did not win prizes. By studying sporting tournaments where relatively rich data can be found we hope to shed light on the nature of other economically significant tournaments where data may be less readily available.

We propose a novel two-part model which enables us to separately quantify the effect of prizes on race choice (the "sorting" effect) and on race speed (the "incentive" effect). Another feature of our specification is that we allow individual effort in a tournament to be influenced by the intensity of competition in that tournament as measured by the total number of runners in the race and by the number of runners that are close in ability to the runner in question. This is the first analysis in the tournament literature to examine the impact of the closeness in ability on effort, holding prize money and prize distributions constant.

Using our methodology, we demonstrate that both sorting and incentive effects are present in the races we examine, and we are able to quantify their impacts. Previous empirical studies have provided ambiguous results in regards to the incentive effects of prizes. Based on our findings, sports events seem to be an appropriate environment to test tournament theories that may be applicable in other economically important contexts.

We also perform a counterfactual analysis and show how a hypothetical change in prizes would be predicted to change race participation and speed. We use this to illustrate how the sorting and incentives effects are inter-related and that the impact of prizes is heterogeneous across runners in the same race.

2.2. Relevant Literature

2.2.1. Theoretical Models

Contrary to the impression that might be acquired by a casual reader of the empirical literature regarding tournaments, there is no single complete theory that can directly be applied to most real life tournaments observed in the economy. Instead a number of special theories with simplifying assumptions offer clues regarding the expected behavior of participants.

In Lazear and Rosen (1981) the authors show that a tournament can theoretically be superior to hourly wages by proving that, for risk neutral workers with uniform abilities, optimally structured tournaments yield results identical to piece rate pay and, if workers are risk averse, tournaments can be more efficient. They observe that persons with more endowed wealth and smaller absolute risk aversion are more likely to prefer contests. The authors assume that the cost of effort is increasing and convex in effort. They argue that in the case of workers of heterogeneous ability it would be necessary to use credentials or other mechanisms to sort individuals into to the "right" contests if contests are to be efficient in the sense of participants choosing socially optimal levels of investment.

Krishna and Morgan (1998) show that, regardless of risk preferences, winner-takes-all tournaments are optimal for up to three competing homogeneous workers. In this context optimal means the prize structure which induces the greatest total effort. In the case of four workers, the optimal structure of awards depends on the risk preferences of the participants. In the case of risk neutrality, winner-takes-all tournaments are again optimal. If workers are risk averse, the optimal tournament pays prizes to the winner and the runner-up. The authors assume that the cost of effort is increasing and convex in effort.

In Moldovanu and Sela (2001) the authors consider risk-neutral heterogeneous participants who differ in their cost of effort. In this context the authors show that winner-takes-all tournaments are optimal when the cost of effort is linear or concave in effort, while it may be optimal to have more than one prize if the cost of effort is convex in effort. The authors assume that ability is private information with abilities drawn independently from a known distribution. As in Krishna and Morgan (1998), optimal means the prize structure that induces the greatest total effort. Moldovanu and Sela's assumption of privately informed heterogeneous participants and a deterministic relationship between effort and output contrasts with the papers discussed earlier where agents are identical and observed output is a stochastic function of unobserved effort.

In Szymanksi and Valletti (2004) the authors show that if output is a stochastic function of effort a second prize may be optimal if the contestants differ enough in ability. The authors show that in a three-person contest with one strong competitor and two equally weak competitors a second prize can be optimal from the point of view of eliciting maximum effort from every contestant. In this model whether or not a second prize is optimal depends on the difference in the cost of effort of the weak and strong players. The authors assume that the cost of effort is increasing and linear in effort and that participants are risk neutral.

Having surveyed some of the relevant theoretical literature, it is perhaps useful to assess how close the available models come to describing road running races. Table 1 lists in the left-hand column what appear to us to be the most salient characteristics of road running races. The remaining columns indicate whether or not the models incorporate the relevant feature. As the reader can observe, there simply is no model that reasonably corresponds to road running races and in general, to many other real tournaments in the economy.[2]

Table 1
Analysis of Selected Theoretical Models

	Lazear & Rosen	Krishna & Morgan	Moldovanu & Sela	Szymanski & Valletti
Competition between tournaments not just within tournaments	No	No	No	No
Large number of participants	No	No	Yes	No
Heterogeneous ability	No	No	Yes	Yes
Ability is public information	--	--	No	Yes
Output is a stochastic function of effort	Yes	Yes	No	Yes

Although it is obviously simplistic and fails to reflect fully all the features of the models considered, Table 1 suggests two challenges. First, there is a challenge for theoretical economists to develop a more comprehensive theoretical framework. Second, empirical economists need to develop a way to organize the data on tournaments in order to understand them better and to suggest likely areas for fruitful theoretical work. This paper attempts to make a contribution in the second of these categories.

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[2] Of course there are many other interesting theoretical papers discussing one feature or another of tournaments. Although we have not presented a full review of all these papers here we believe that our conclusion would hold even if this exercise was to be completed.

2.2.2. Experimental Research

In light of the complexity of many tournaments compared to the available theoretical models, experiments may provide a useful link between theory and real world tournaments. Harbring and Irlenbusch (2003) investigate different tournament design alternatives along two dimensions: tournament size and prize structure. Participants have homogeneous costs of effort and a predetermined number of prizes are awarded to those choosing the greatest effort. The authors find that average effort tends to increase and variability of effort tends to decrease with the number of prizes.

Orrison, Schotter and Weigelt (2004) use an experimental design in which the cost of effort is homogenous and output is a stochastic function of unobserved effort. Prizes are awarded to a predetermined number of those achieving the highest output. The authors found that behavior was invariant to tournament size (i.e., behavior is the same for tournaments with two players and one prize and say six players and three prizes). If the number of prizes was varied for a given number of participants, effort changed little although there was some evidence of effort declining if the number of prizes was very high.

2.2.3. Sports Related Empirical Studies

The economics of sports has received significant interest from researchers. Szymanski (2004) provides an extensive overview of sports economics. Below we briefly review two pairs of papers in this area that are relevant to tournaments.

Ehrenberg and Bognanno (1990) use data from professional golf tournaments in the United States in 1984. The authors' analysis is restricted to the top 160 money winners for whom the average score on all rounds during the year is available. The authors use these average scores as a proxy for each player's ability. Controlling for the tournaments' characteristics, an individual's ability and the ability of other players, the authors find a positive effect of total prize money on an individual's score.

Orszag (1994) generally follows the same methodology and uses data from professional golf tournaments in 1992. In contrast to Ehrenberg and Bognanno, Orszag finds the coefficient for total prizes to be insignificantly different from zero. As the author states; "Perhaps golf is not the ideal example to study tournament theory, or perhaps tournament theory does not elicit the desired incentive results." Our instinctive assessment is that at this level of competition, prizes do provide incentives to practice and to train prior to a tournament, but once a player has honed his skills through extensive training, trying harder than usual in a particular tournament simply does not reliably improve his score.

Is the balance between the efficacies of training prior to a competition (which improves performance for a number of different tournaments) and effort in the competition itself different for road running races so that the incentive effect in a particular race is meaningful and can be measured?

Maloney and McCormick (2000) use data for runners who won prizes in races that took place in the southeastern US over the years 1987 to 1991. Using time per mile as the dependent variable, the authors attribute the coefficient for the prize spread, defined as the difference between the prize won by each runner and the next lowest prize, to the incentive effect and the coefficient for the average prize to the sorting effect. On this basis the authors find both an incentive and a sorting effect.

Lynch and Zax (2000) employ data from races organized in the United States and abroad in 1994. The authors show that without controlling for ability, runners seem to run faster if the prize difference is higher. In this context the prize difference is defined to be the amount of prize money a runner would lose in a particular race if she finished one place below the position implied by her pre-race ranking. Once ability was taken into account, the coefficient for the prize difference that would measure the incentive effect is neither of the expected sign nor statistically significant for almost all distances analyzed. The authors conclude "that races with large prizes record faster times because they attract faster runners, not because they encourage all runners to run faster."

To date, the results of sports related empirical studies are at best ambiguous in validating tournament theories. This can be explained by either an identification strategy that does not separately quantify the sorting and incentive effects of prizes or by the fact that sports tournaments are not appropriate for testing the tournament theories, or by both. If sports tournaments are to be a useful testing ground for theory in this area a necessary prerequisite is that participants respond to the observed incentives. If participants do not respond to the observable financial prizes, tournament theories are not invalidated, after all it may be the case that participants are responding to other less readily observed incentives such as prestige or derivative financial incentives (such as endorsement income). However, if participants do not respond to financial prizes, the usefulness of sports tournaments to test tournament theories that are important in other areas of the economy disappears. One of the goals of our research is to determine whether in the context of road running races financial prizes can be seen to impact race choice and performance.

2.3. Econometric Model

In our model we assume runners make three decisions. First, runners decide whether to race in a particular period. Second, if they choose to race, runners determine in which of the available races to participate. Third, once they know the other race participants, runners choose the effort to exert in their chosen race. We assume that: (1) the first decision is based on idiosyncratic factors that are independent from any of the factors that impact the second and third decisions and the runners' performances in their chosen races; and, (2) the disturbance terms in the equations that describe the attractiveness of the races, the "sorting equation", (relevant for the second decision) and runners' performance, the "speed equation", (relevant to the third decision) are independent. Assumption (1) allows us to model just runners' second and third decisions. Assumption (2) is justified by the time between when the race is chosen and when the effort is exerted; it also simplifies the estimation of the sorting and speed equations.

As discussed in the next section, in our analysis we look only at "top runners", which we define as male runners who (1) finish in the top 30 in at least one race in our data set and (2) finish in the top 150 in at least one additional race in the data set. We believe that these top runners are likely to be less subject to idiosyncratic reasons influencing race choice and race effort compared to less successful runners, and limiting our attention to these runners reduces computational complexity.

For each race we identify up to two most closely competing races based on the date on which the races take place, the distance between race venues and the lengths of the races. In particular, the methodology to identify the most closely competing races used the following algorithm. First, only races occurring within a time period starting one weekend prior to the race of interest and ending one weekend after the race of interest were considered as candidate competing races.[3] Second, from the group of races occurring within the required time period we retained as candidate competing races only those races for which the venue was within 1,000 miles of the race of interest. Third and finally, from the remaining candidate competing races the two most similar in length to the race of interest were chosen. Applying this methodology to our data results in eight of the 71 races in our data set with top runner participation having no competing races, ten having only one competing race and the remaining 53 races having two competing races. As described in Section 2.5, we considered two alternative ways to determine the competing races as a robustness check.

We assume that having decided to race in a particular period, a runner chooses between the race of interest and the most closely competing races only. If runner *i* has chosen to compete in a particular period, the relative attractiveness of race *k* is A_{ik}. We assume that A_{ik} is determined as shown in Equation 1.

$$A_{ik} = x_k \alpha + r_i x_k \beta + \varepsilon_{ik} \qquad (1)$$

In Equation 1 x_k is a vector characterizing the prizes in race *k* and will include variables such as the total value of all prizes offered and the Herfindahl index of the value of the prizes offered, r_i is runner *i*'s ranking and ε_{ik} is a disturbance term that accounts for a runner's idiosyncratic race preferences (i.e., preferences unrelated to prizes and ranking). For computational convenience we will assume that the disturbance term is i.i.d. extreme

3 In the case of races that were half marathons or longer, this time period was extended to two weekends before the race of interest until two weekends after the race of interest. We believe that this approach is appropriate in light of the longer recovery time associated with races of this length.

value. We proxy runners' ability with their ranking. The methodology we use to calculate each runner's ranking is based on a runner's success in beating other runners and is discussed in detail later in this section.

If a particular runner participates in race k rather than any of the most closely competing races which we designate $\tilde{k}$ then we know $A_{ik} \geq A_{i\tilde{k}} \;\forall\; \tilde{k}$.[4] We define P_{ik} as the probability of observing runner i participating in race k rather than any of the most closely competing races. Since we are assuming ε_{ik} is i.i.d. extreme value we can use a logit model to calculate P_{ik} as shown in Equation 2.[5]

$$P_{ik} = \frac{e^{x_k\alpha + r_i x_k \beta}}{e^{x_k\alpha + r_i x_k \beta} + \sum_{\tilde{k}} e^{x_{\tilde{k}}\alpha + r_i x_{\tilde{k}} \beta}} \tag{2}$$

Our estimates for the vectors α and β are the values that maximize the log likelihood function shown in Equation 3; this is the sorting equation.

$$L = \sum_{k}^{\substack{\text{all} \\ \text{races}}} \sum_{i}^{\substack{\text{all top runners} \\ \text{in race } k}} \log P_{ik} \tag{3}$$

For estimation purposes we parameterize the relationship determining the average speed for runner i in race k as shown in Equation 4; this is the speed equation.

$$speed_{ik} = W_k a + X_k b + Y_{ik} c + Z_i d + e_{ik} \tag{4}$$

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[4] Since the comparison of races' attractiveness is made for each runner it becomes apparent that any of the runner's characteristics such as her ranking, if included in equation 1 without interaction with race characteristics, would cancel out and it would not be possible to identify its effect.

[5] In some cases individual runners participated in one of the two competing races that had been identified. In this situation P_{ik} is defined as the probability that the relative attractiveness of the race the runner chooses not to participate in is less than the other two. If k and l are the races in which the runner participates and m is the one he does not, P_{ik} can be written as:

$$P_{ik} = \frac{e^{-(x_m\alpha + r_i x_m \beta)}}{e^{-(x_k\alpha + r_i x_k \beta)} + e^{-(x_l\alpha + r_i x_l \beta)} + e^{-(x_m\alpha + r_i x_m \beta)}}$$

In Equation 4 W_k represents intrinsic race characteristics and will include variables such as race prizes, distance, distance squared, the topography of the race course and the prevailing temperature on the race day. X_k represents global competitive intensity measured by the total number of top runners in the race. Y_{ik} represents local competitive intensity and is related to the number of top runners competing in the race who are of similar ranking to the runner of interest. Unlike the earlier terms, this effect is specific to a particular runner in the race of interest. Our two measures of competitive intensity are designed to capture the intuition that individual effort (and therefore race speeds) will be influenced by the size and quality of the overall field and will also be impacted by the presence of runners of a similar ability. Z_i includes personal characteristics of runner *i* such as his ranking. The disturbance term, e_{ik} , is intended to reflect all other factors impacting a runner's speed and is assumed to vary by runner and race. We use ordinary least squares to estimate the vectors *a*, *b, c* and *d*.

As discussed in Section 2.2, some prior researchers have used ranking information that is intended to reflect a runner's ability as an explanatory variable. In the case of road running, rankings are not available for all the runners who are of interest to us, and therefore we are required to develop our own ranking system. Our goal is to assign a ranking to each individual that results in the fewest possible number of "wrong" predictions (i.e., the smallest number of cases where the lower-ranked runner beats the higher-ranked runner) and which assigns plausible rankings to groups of runners that never interact. For simplicity we assume that the relationship between rankings and the probability of one runner beating another does not depend on race characteristics.

Considering the top runners in our data set we have a total of 7,377 pairwise tournaments where we observe one top runner beating another in a race. The algorithm we used to determine rankings had two parts. First, initial rankings were calculated based on each runner's won/lost count which is equal to the number of pairwise victories minus the number of pairwise losses for each runner. Using the initial rankings calculated in this way, the number of pairwise tournaments in which the worse ranked runner beat the better ranked runner was calculated. Second, we tried to improve the initial rankings through an iterative search. One of the incorrectly forecasted pairwise tournaments was chosen at random and the rank of the loser reassigned to the winner and the ranks of the loser and all the runners with rankings between the original rankings of the winner and loser were demoted by one ranking. Using these revised rankings, the number of pairwise tournaments in which a worse ranked runner beat a better ranked runner was recalculated. If the number of incorrectly forecast pairwise tournaments was reduced the initial candidate rankings were replaced with the revised rankings. If the revised rankings did not reduce the number of forecasting errors the initial candidate rankings were not changed. Next, another random draw from the incorrectly forecast pairwise tournaments based on the candidate ranking was taken and the process repeated. Using this methodology the initial proportion of incorrectly forecast pairwise tournaments was 13.7 percent. An iterative search consisting of 100 random draws reduced this to 12.0 percent. 500 draws reduced it to 8.0 percent. 1,000 draws reduced it to 5.9 percent and 5,000 draws reduced it to 5.1 percent. We used the rankings produced after 5,000 draws in our analysis.[6]

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[6] An example may serve to demonstrate why our methodology would not be expected to eliminate all incorrect forecasts. In our data no top runner competes with another top runner more than four times. There are seven

2.4. Description of the Data

Our data set was collected based on the information about races found in the *2002 Road Race Management Directory*. This booklet contains details about the most important road-race competitions that took place in 2002. We selected only the competitions for men held in the United States that awarded monetary prizes for which we had a complete and accurate list of prizes and results. The results for each competition were downloaded from the competitions' official sites. Weather data were obtained from the Agricultural Weather Office at Clemson University. The characterization of courses in terms of topography (hilly, downhill or flat) was based on the way in which the race managers advertised their race in the *2002 Road Race Management Directory*.

As described in Section 2.3 we limit our analysis to what we term "top runners". This defines a revised data set consisting of 861 observations including 366 top runners in a total of 71 races. We observe a maximum of 42 top runners in each race and a minimum of one top runner. The maximum number of appearances by a top runner is six and the minimum two. The average number of participants close in ranking to a runner is 2.6, with a minimum of zero and a maximum of 13.

Summary information regarding the 71 races included in our analyses is shown in Table 2. The Herfindahl index of prize values provides a measure of the dispersion of prizes in a race and was calculated in the usual way by taking the sum of the square of the ratio

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pairs of runners that meet exactly four times. In the case of five of these pairs the same runner wins on each of the four occasions that they meet. For the remaining two pairs the results are not consistent. In these cases one runner wins on three occasions and the other wins on the fourth. In this small sub-sample there are a total of 28 pairwise competitions (four for each of seven pairs of runners). Given the inconsistent performances, rankings would fail to correctly predict the results of two of the pairwise competitions, corresponding to a failure rate of approximately 7%.

consisting of the value of each prize divided by the total value of prizes. Consequently, this index takes values between zero and one with a value of one corresponding to a single "winner-takes-all" prize while lower values correspond to more widely dispersed prizes. In addition to the information shown in Table 2, 13 of the races were classified as hilly, five as down hill, 33 as flat and 20 as topography unknown.

Table 2

Summary Information by Race

Variable	Minimum value	Maximum value	Mean value
Total value of prizes ($000s)	0.05	30.25	4.68
Herfindahl index of prize values	0.14	1.00	0.34
Race distance (kilometers)	1.6	80.5	19.4
Average temperature (º F)	35	84	62
Number of top runners	1	42	12.1

A particular advantage of our data set over those used in the previous empirical analyses is that it permits us to observe the effects of prizes on athletes who did not win prizes.

2.5. Results

In this section we discuss the results of estimating Equation 3 (the sorting equation) and Equation 4 (the speed equation) and the robustness checks we performed.

2.5.1. The Sorting Equation

Based on the algorithm we chose to identify competing races, out of the total of 861 observations 101 did not have any competing races and therefore 760 were suitable for use in estimating the coefficients in Equation 3. The vector of variables to characterize race prizes (x_k) contains the total value of all prizes and the Herfindahl index of the prizes offered. We chose to specify x_k in this way in the belief that prospective race participants might respond to both the purse and the manner in which it was divided between different prizes.

As can be observed from the first column of Table 3, it appears that in our data the total value of prizes and the total value of prizes interacted with a runner's rank are relevant to a runner's choice of race. The coefficients for both of theses variables are statistically significant at the 5 percent level. Neither of the coefficients relating to the variables including the Herfindahl index of prize values is statistically significant. If we re-estimate Equation 3 using x_k consisting of only total value of prizes, we can parameterize the expected attractiveness of race *k* to runner *i* as shown in Equation 5.

$$E(A_{ik}) = 0.1101(\textit{total value of prizes}_k) - 0.0003746(\textit{rank}_i \times \textit{total value of prizes}_k) \quad (5)$$

As expected, for highly ranked runners (i.e., runners with a ranking number that is low), increasing the total value of prizes offered increases the attractiveness of the race. For less highly ranked runners this effect is more muted. Using the point estimates of our results, we can conclude that the runner ranked 294th is indifferent to total prizes when choosing between races and for runners that are ranked below 294th increasing total prizes actually reduces the attractiveness of a race. Using the delta method we estimate the

Table 3

Sorting Equation Results and Robustness Checks

Variable	Base specification	Base specification using alternative sets of competing races: Alternative 1	Alternative 2	Base specification using only: prize winner observations	non prize winners observations	Base specification using: alternative rankings based on the FE analysis	"ability" based on FE coefficients instead of ranking
Total value of prizes ($000s)	0.1039***	0.0768***	0.0870***	0.1545**	0.1493***	0.1394***	0.1060***
	(0.0298)	(0.0287)	(0.0321)	(0.0609)	(0.0461)	(0.0363)	(0.0269)
Herfindahl index of prize values	-1.085	-2.926	-3.624*	-3.788	0.9112	-3.580	-2.523
	(2.419)	(2.304)	(2.081)	(4.710)	(3.5960)	(3.162)	(2.438)
Rank x Total value of prizes ($000s)	-0.0003315**	-0.0002888*	-0.0003279*	-0.002788*	-0.0004846**	-0.0005614***	0.1236***
	(0.0001506)	(0.0001587)	(0.0001718)	(0.001462)	(0.0002058)	(0.0001788)	(0.0384)
Rank x Herfindahl index of prize values	0.006923	0.016356	0.018140*	-0.007334	-0.0004835	0.01489	-2.955
	(0.010904)	(0.012015)	(0.010383)	(0.056102)	(0.0148308)	(0.01205)	(2.560)

Standard errors are reported in parentheses. "*", "**" and "***" indicate statistical significance at the 10%, 5% and 1% level respectively.

Note that the estimated fixed effects dummy variable coefficients used to produce the results shown in the last column range in value from -1.85 to 0.36 with a mean value of -0.57. Since a high value of the fixed effects dummy variable coefficient corresponds to a fast runner, we would expect the final two coefficients to be of opposite sign to the results produced using rankings where a fast runner is assigned a low ranking number.

standard error for the point of indifference ranking to be 60 ranking places.

We explored the robustness of our reported results for the sorting equation by estimating a number of additional model specifications. First, we would like to check if our results are sensitive to the way we defined the competing races for the race of interest. As a consequence, we considered two alternative methodologies that are compared in Table 4 with our base case methodology.

Table 4

Comparison of Methodologies for Determining Competing Races

	Base case	Alternative 1	Alternative 2
First selection criteria	Races occur +/- one weekend of race of interest (+/- two weekends for half marathons or longer)	Races occur +/- two weekends of race of interest (+/- three weekends for half marathons or longer)	Every race is assigned to one of 42 categories based on seven predetermined time periods, two predetermined geographical regions and three predetermined race length categories
Second selection criteria	Race venue is within 1,000 miles of the race of interest	Race venue is within 800 miles of the race of interest	--
Final selection criteria	Choose up to two races most similar in length to the race of interest	Choose up to two races most similar in length to the race of interest	Choose up to two races from the same category as the race of interest based on the proximity of race venues

The sorting equation was re-estimated using the competing races produced by the two alternatives. In both cases the two coefficients involving total value of prizes remained statistically significant at the 10 percent level and changed in magnitude by less than one standard error from the values reported earlier. In the case of Alternative 1 the two coefficients involving the Herfindahl index of prize values remained statistically insignificant. In the case of Alternative 2 these coefficients became statistically significant at the 10 percent level. The signs of the coefficients involving the Herfindahl index of prize values were unchanged by the choice of methodology used to determine competing races.

Second, we are interested to see if prizes influenced race choice by prize winners in a different way than was the case for the rest of the participants. Of the 861 observations included in the data set used in our analysis, in 205 the runner was awarded a prize in the race of interest while in 656 he was not. We re-estimated the sorting equation using only the prize-winning observations and only the non prize-winning observations. In both cases the signs of the coefficients for the variables involving the total value of prizes did not change and the coefficients were statistically significant at the 10 percent level. As before, the coefficients for the variables involving the Herfindahl index of prize values were not statistically significant.

The results of the robustness checks involving the sorting equation are included in Table 3. On balance they support our choice of model specification described earlier.

2.5.2. The Speed Equation

The estimated coefficients for Equation 4 are shown in Table 5. The dependent variable used in the regression is average speed measured in units of meters per second. The results for each component of the speed equation are discussed below.

Table 5

Speed Equation Results

Variable	Coefficient	Standard error
W_k *(intrinsic race characteristics)*		
Total value of prizes ($000s)	0.00452	0.00286
Herfindahl index of prize values	-0.6479***	0.2139
Race distance in km	-0.03850***	0.00653
(Race distance in km)^2	0.0002657**	0.0001037
Dummy for hilly course	-0.1697***	0.0600
Dummy for down hill course	0.1117**	0.0462
Dummy if course topography unknown	0.02330	0.0813
Average temperature	-0.01638	0.02230
(Average temperature)^2	0.001478	0.0001962
X_k *(global competitive intensity)*		
Number of top runners	0.003715*	0.002164
Y_{ik} *(local competitive intensity)*		
Number of top runners within 20 ranking places	0.01877*	0.01061
Z_i *(personal characteristics of runner)*		
Runner ranking	-0.003458***	0.000135
Constant	6.516***	0.637

The R squared for the regression is 0.83. Standard errors are adjusted for clustering by race. "*", "**" and "***" indicate statistical significance at the 10%, 5% and 1% level respectively.

W_k (intrinsic race characteristics): We find that prizes impact average speed, which is consistent with the direct incentive effect. Average speed increases with the total value of the prizes offered and as the Herfindahl index of prize values decreases. These effects are modest in size. An increase in total prizes by $10,000 is predicted to increase average speed by approximately 0.05 meters per second.[7] A decline in the Herfindahl index of prize values by 0.25 (this could occur if a race organizer decided to award four equally sized prizes rather than two equally sized prizes) is predicted to increase average speed by approximately 0.16 meters per second. As expected, the magnitude and sign of the distance and distance squared variables indicate that average speed is predicted to fall with increasing distance with the rate of change declining as distance increases. For example, an increase in distance from 10 to 20 kilometers is estimated to reduce average speed by approximately 0.31 meters per second all other things being equal while an increase in distance from 30 to 40 kilometers is estimated to reduce speed by only 0.20 meters per second. As indicated by the hilly course and down hill dummies, compared to a flat course a hilly venue reduces average speeds while a down hill course increases speeds. The dummy variable if course topography is unknown is not statistically significant. This dummy variable was employed if it was not possible using the information provided by race organizers to confidently categorize the race as hilly, downhill or flat. The fact that the coefficient is not statistically significant is consistent with the hypothesis that the races assigned to this category were not predominately of one type or another or that these unassigned races were in fact flat. The coefficients of the average

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[7] As an illustration, in race 8 (which is discussed in Section 2.6) the first top runner finished the 8 km course in 1,574 seconds. If this runner were to increase his speed by 0.05 meters per second his time would decline by 15 seconds; an improvement of 1%.

temperature and average temperature squared variables are not statistically significant.

X_k *(global competitive intensity):* We used one variable in this category: the number of top runners participating in the race. The coefficient is statistically significant at the 10 percent level. As expected, as the number of top runners in a race increases, so does average speed. All other things being equal, the results imply that a race with 10 additional top runners will have speeds that are approximately 0.04 meters per second faster.

Y_{ik} *(local competitive intensity):* We chose a measure of local competitive intensity which appeared to have the greatest explanatory power: the number of competitors within 20 ranking places of the runner of interest. The estimated coefficient, which is statistically significant at the 10 percent level, indicates that a single additional runner close in ranking to the runner of interest is expected to increase his average speed by approximately 0.02 meters per second more than an additional top runner that was not closely ranked.

Z_i *(personal characteristics of runner):* Ranking is the only variable in this category. As expected, better ranked runners (i.e., those with lower ranking numbers) are predicted to run faster. All other things being equal the runner with ranking 1 is predicted to have a speed that is approximately 0.35 meters per second faster than the runner that is ranked 101. This coefficient is significant at the 1 percent level.

In order to investigate the robustness of our reported results, we estimated a number of alternative model specifications for the speed equation. First, we omitted runner rankings as an explanatory variable. This resulted in a substantial reduction in the reported R squared from 0.83 to 0.54. The reported coefficients for total value of prizes and Herfindahl index of prizes retained the same signs, were statistically significant, and were greater in absolute value than in our earlier analysis. These results are consistent with our assumption that ability is an important determinant of speed and that our rankings are a good measure of ability. As expected, the omission of a measure of ability reduced the model's explanatory power and upwardly biased the absolute values of the coefficient for total value of prizes since when rankings are omitted we fail to account for the fact that on average races with more attractive prizes include runners of higher ability. Second, we added two additional interaction variables to the model: runner ranking multiplied by total value of prizes and runner ranking multiplied by Herfindahl index of prizes. Neither of the new variables was statistically significant. Third, we re-estimated the speed equation using only the prize-winning observations and only the non prize-winning observations. Using only the prize-winning observations the coefficient for the total value of prizes was larger than in our base case analysis and was statistically significant at the 1 percent level. The coefficient for the Herfindahl index of prize values, the number of top runners and the number of top runners within 20 ranking places were no longer statistically significant. Using only the non prize-winning observations the coefficient values for the total value of prizes and the Herfindahl index of prize values were similar to those reported for the base case and both were statistically significant at the 10 percent level. The coefficients for the number of top runners and the number of top runners within 20 ranking places were both statistically significant at the 10 percent level. The results for the prize-winning and non prize-winning data are consistent with both groups responding to the total value of prizes with those who win prizes responding to a greater extent. While we can not reject the hypothesis that prize winners are insensitive to the dispersal of prizes and the number of top runners in the race, non prize-winners appear to respond positively to more dispersed prizes, more top runners in the race and more closely ranked runners in the race. Fourth, using all top runners, we estimated a version of the model that excluded local and global competitive intensity. In this case the coefficient for the total value of prizes is considerably higher than in the base case. This suggests that failing to account for competitive intensity may result in biased coefficients for total prizes. The coefficient for the Herfindahl index of prize values was similar in magnitude to the one reported for the base case. Both coefficients are statistically significant at the 1 percent level. Fifth, as an alternative way to control for ability, we estimated the model with runner fixed effects. The coefficient of total value of prizes was statistically significant at the 1 percent level and was approximately unchanged in magnitude. The coefficient for the Herfindahl index of prizes was no longer statistically significant at the 10 percent level.

Further information regarding the results of the robustness checks involving the speed equation is included in Table 6. Overall the robustness checks we undertook provide some additional comfort that the version of the model we presented earlier in this section is reasonable.

2.5.3. Possible Endogeneity of Rankings and Additional Robustness Checks

Our methodology for determining rankings uses runners' observed finishing positions. If Equation 4 correctly specifies each runner's performance, runner *i*'s speed and therefore finishing position in race *k* is influenced by e_{ik}. As a result e_{ik} impacts runner *i*'s ranking. But runner *i*'s ranking is included in Y_{ik} and Z_i in Equation 4 and so Y_{ik} and Z_i may be correlated with e_{ik}. Therefore as we have specified Equation 4, Y_{ik} and Z_i may be endogenous and consequently the coefficients we estimate for Equation 4 could be biased. This potential problem is partially addressed by the fact that the ranking for runner *i*

Table 6

Robustness Checks for the Speed Equation

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
W_k (intrinsic race characteristics)							
Total value of prizes ($000s)	0.0139*** (0.00361)	0.00322 (0.00337)	0.0126*** (0.0036)	0.00543* (0.00281)	0.00859*** (0.00223)	0.00379*** (0.00131)	0.00271** (0.00133)
Herfindahl Index of prize values	-0.8430*** (0.1876)	-0.7437*** (0.2009)	0.01511 (0.29543)	-0.5908*** (0.2115)	-0.7326*** (0.1990)	0.1007 (0.0736)	-0.01048 (0.07344)
X_k (global competitive intensity)							
Number of top runners	-0.0000982 (0.0035054)	0.003560 (0.002200)	0.002157 (0.004493)	0.006734*** (0.002436)		0.001992 (0.001317)	0.002390** (0.001183)
Y_{ik} (local competitive intensity)							
Number of top runners within 20 ranking places	0.04490*** (0.01673)	0.01932* (0.01154)	-0.003311 (0.008747)	0.02637* (0.01582)		-0.001809 (0.004296)	-0.003230 (0.003806)
Z_i (personal characteristics of runner)							
Runner ranking		-0.003669*** (0.000364)	-0.002756*** (0.000263)	-0.002871*** (0.000151)	-0.003498*** (0.000132)		-0.004731*** (0.000102)
Additional Interaction Variables							
Runner ranking x Total value of prizes ($000s)		8.79e-6 (14.9e-6)					
Runner ranking x Herfindahl index of prizes		0.0005136 (0.0009869)					
R Squared	0.54	0.82	0.87	0.80	0.82	0.96	0.96

Standard errors are reported in parentheses and are adjusted for clustering by race. "*", "**" and "***" indicate statistical significance at the 10%, 5% and 1% level respectively. All specifications include a constant and the other race characteristics as presented in Table 5 but are not reported here to conserve space.

(1) refers to the specification without runner rankings
(2) refers to the specification with additional interaction terms
(3) refers to the specification using only prize-winner observations
(4) refers to the specification using only non prize-winner observations
(5) refers to the specification without the local and global competitive intensity
(6) refers to the specification using runner fixed effects
(7) refers to the specification using alternative rankings based on the FE analysis

depends not just on e_{ik} but on the disturbance terms for runner i in all the races he participates in our data set and indirectly on the disturbance terms for other runners. In addition, rank ordering may be less influenced by e_{ik} than would a continuous ability metric. The comparison of our base case results from estimating the speed equation with the fixed effect results leads us to believe that the possible endogeneity issue is not sufficiently large to place our qualitative conclusions in doubt.

As an additional robustness check, we estimated both the sorting equation and the speed equation using alternative rankings based on the results of the speed equation using fixed effects. As before, rankings were assigned from 1 to 366. The runner with the highest dummy variable coefficient was assigned ranking 1 and the one with the lowest ranking 366. The average absolute difference between our base case rankings and the alternative ones based on the fixed effects analysis was 45. The model $\textit{fixed effects ranking}_i = \textit{base case ranking}_i + \varepsilon_i$ had an R squared of 0.65. Using the fixed effects rankings in the sorting equation yielded results that were similar to those reported earlier. As before, only the coefficients involving the total value of prizes were statistically significant. These coefficients were of the same sign and similar magnitude to those obtained using the base case rankings. Estimating the speed equation using the fixed effects rankings resulted in the coefficient of total value of prizes being statistically significant at the 5 percent level and of the same sign and similar magnitude to the base case. The coefficient for the Herfindahl index of prize values was no longer statistically significant. For completeness we also estimated the sorting equation using the estimated values of the fixed effects dummy variable coefficients in place of rankings. The results were consistent with those reported earlier with only the coefficients involving the total value of prizes being statistically significant. Further details of the results obtained by estimating the sorting and speed equations using the fixed effects rankings and the sorting equation using the fixed effects dummy variable coefficients in place of rankings are included in the last columns of Tables 3 and 6.

2.6. Counterfactual Analysis

In order to illustrate the expected impact of changing race prizes we consider the predicted result of a hypothetical change in the prizes offered in race 8. This race was chosen since it has only one competing race (race 28) and has only a limited number of top runner participants (five). These characteristics make the arithmetic for calculating the expected consequence of changing prizes a little easier than it would be for other races. The principles that would need to be applied are the same in all situations, including those where there are two competing races and where the number of top runners is large. Race 8 and race 28 are of the same distance (8 kilometers), occurred on the same day (June 16, 2002) and took place at relatively nearby venues (Boston in the case of race 8 and New York City in the case of race 28). There were eight prizes that were awarded in race 8 for a total amount of $400. Herfindahl index was 0.2109. The hypothetical revised prizes have the same distribution with a total value of $4,000.

Using the coefficients estimated earlier as shown in Equation 5 and employing Equation 2, we are able to calculate the unconditional individual probabilities of each of the 18 top runners who actually participated in race 28 of having chosen to participate in race 8 at the actual prizes and at the hypothetical prizes. For runner i these probabilities are represented by p_a and p_b respectively. If runner i ranks above the level for indifference to prizes (estimated at ranking 294th earlier), then p_h will be greater than p_a. The fact that runner *i* was actually observed to have participated in race 28 places an upper bound (UB_i) on the value of $\varepsilon_{i8} - \varepsilon_{i28}$ that corresponds to the value at which the runner is indifferent between race 8 and race 28 at the actual prizes. (Recall that, as explained in Section 2.3, ε_{ik} represents the idiosyncratic attractiveness of race *k* to runner *i*.) At the hypothetical prizes there is a revised critical value (CV_i) of $\varepsilon_{i8} - \varepsilon_{i28}$ at which the runner is indifferent between race 8 and race 28. Assuming the runner is ranked better than 294th, CV_i is less than UB_i. In this case, the probability that the runner would choose race 8 at the hypothetical prizes while he chose race 28 at the actual prizes is equal to the probability that the true value of $\varepsilon_{i8} - \varepsilon_{i28}$ is between CV_i and UB_i conditional on $\varepsilon_{i8} - \varepsilon_{i28}$ not exceeding UB_i. This is equal to $(p_h - p_a) / (1 - p_a)$ since p_a is the probability $\varepsilon_{i8} - \varepsilon_{i28}$ exceeds UB_i and p_h is the probability it exceeds CV_i. If the runner is ranked worse than 294th, race 8 is made less attractive to runner *i* by the increase in prizes. Under these circumstances a runner who chose race 28 at the actual prizes would never switch to race 8 at the hypothetical prizes and, since $p_h < p_a$, $(p_h - p_a) / (1 - p_a)$ is negative. Thus, the probability of a runner participating in race 8 at the hypothetical prizes conditional on the fact that at the actual prizes he chose to participate in race 28 is the larger of $(p_h - p_a) / (1 - p_a)$ and zero.

In a similar way, for the five top runners that actually participated in race 8 we can calculate the probability of each runner choosing race 8 at the hypothetical prizes conditional on the fact that he chose to do so at the actual prizes.

In the case of the top runners originally in race 28, for eight runners the probability of defecting to race 8 exceeds 10 percent, for three runners the probability is between zero and 5 percent and for the remaining seven runners the probability is zero. In order to simplify the analysis only those runners with a 10 percent or greater chance of defection will be considered in the remainder of the discussion. In the case of the five runners originally in race 8, based on the hypothetical prizes three are estimated to remain in race 8 with certainty while two are estimated to defect to race 28 with a probability of less than 5 percent. For simplicity the possibility of defections away from race 8 will be ignored in the remainder of the analysis. Based on these simplifying assumptions the probability of at least one runner from race 28 joining race 8 if the hypothetical prizes were introduced is 0.7 and the expected number of runners defecting from race 28 to race 8 is 1.2. This is the sorting effect in action.

Now we turn to the incentive effect of the hypothetical revised prizes on the runners already committed to race 8. We call the predicted effect of the revised prizes before taking into account the impact of runners defecting into race 8 the "direct" incentive effect. We call the total effect of the hypothetical prizes when allowing for defections the "combined" incentive effect and the difference between these two effects the "indirect" incentive effect. As shown in Table 7, we estimate the combined incentive effect to be of the order of 0.5 percent of the runners' original times. Except in the case of the top ranked runner in race 8 the direct incentive effect represents the overwhelming majority of this predicted change in speed. For the top ranked runner in race 8 the indirect incentive effect is larger than is the case for the other runner since for this runner three of the runners that may defect from race 28 are within 20 ranking places and therefore the local competitive effect has a positive impact on the runner's predicted speed.

As we have chosen to model them, the direct incentive effect is common to all race participants while the indirect incentive effect can vary between individuals since the hypothetical change in prizes can result in changes in local competitive intensity that differs between runners of different rankings.

Table 7

Incentive Effects for Race 8 Participants

Runner's ranking	Direct effect	Indirect effect	Combined effect
82	0.01627	0.01223	0.02850
282	0.01627	0.00429	0.02056
284	0.01627	0.00429	0.02056
310	0.01627	0.00429	0.02056
343	0.01627	0.00429	0.02056

effects are measured in meters per second

Table 7 shows the predicted impact of the hypothetical change in prizes on the speed of all the top runners already committed to race 8. Note that the runner ranked 82 was the first placed top runner with a time of 1,574 seconds. The race had a length of 8 kilometers and so this winning time corresponded to an average speed of 5.1 meters per second. For this runner the predicted combined incentive effect corresponds to an increase in speed of approximately 0.6 percent. The runner ranked 343 was the slowest of the top runners in this race with a time of 1,927 seconds. In this case the predicted combined incentive effect corresponds to an increase in speed of approximately 0.5 percent.

2.7. Conclusion

Tournaments of many types are important in the economy. Often data are difficult to collect and, as a result, to a large extent empirical analysis has not been available to support the development of theory. If it can be shown that participants respond to the observed financial prizes, sports tournaments may offer a fruitful area for study that complements research in areas of more direct interest but where data may be limited and of poor quality. In a sense sports tournaments provide an analytical bridge between laboratory experiments and economically important tournaments. In the case of laboratory experiments data are of high quality but the applicability to other economically important tournaments may be questioned. In the case of a direct study of say work place promotions, the applicability is clear while high quality data may be scarce. Sports tournaments stand between these two extremes and may play an important part in furthering understanding.

This chapter's contribution is fivefold. First, we develop a two-part empirical model to separately quantify the sorting and incentive effects of tournament prizes. We believe that in this setting our approach is novel. Second, we use concepts of global and local competitiveness to estimate effects that relate to the overall competitiveness of a race and the local impact of runners of similar rank to the runner of interest. Third, we apply the model to a unique data set of road running race results. Since, unlike the data sets used in prior studies we are aware of, the one used here includes runners that did not win prizes, it allows us to observe the impact of prizes on the race choice and the speed of all runners, not just prize winners. Fourth, we demonstrate that in the races we examine participants do indeed respond to financial prizes and both sorting and incentive effects are present. These conclusions are robust to a number of alternative specifications of the sorting and speed equations. Fifth, we present a counterfactual example showing how a hypothetical change in prizes would be predicted to change race participation and speed.

Chapter 3

WELFARE SPENDING AND MORTALITY RATES FOR THE ELDERLY BEFORE THE SOCIAL SECURITY ERA[8]

3.1. Introduction

During the Great Depression, the Roosevelt Administration faced substantial pressure to make special provisions to aid the elderly poor. The elderly tended to have the highest poverty rates among age groups and several advocates received extensive publicity and support in opinion polls when they lobbied for expansive programs to help the elderly.[9] When the Roosevelt administration turned to the issue in late 1934 and 1935 they faced a set of choices. One was to focus on paying benefits to the elderly poor, a program that about half of the states had initiated by that time. Federal aid through matching grants would give incentives to all states to adopt such programs and possibly increase the benefits they paid. Another choice was to provide an old-age pension plan for all workers, who would pay taxes into a fund while working, and then receive benefits based on their contributions after age 65. The Social Security Act of 1935 chose both, an Old Age Assistance (OAA) matching grant program paying benefits to the needy elderly and the Old Age Security Income (OASI) old-age pension for the entire working population. During the discussions of the legislation, Franklin Roosevelt argued for making sure that the benefits paid by each worker

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[8] Co-authored with Price Fishback

[9] The most famous of these plans was the Townsend Plan, which called for $100 per month to be paid to the elderly, who were then expected to spend the entire sum as a stimulus to the economy. This plan essentially called for a transfer of 40 percent of annual GNP to the elderly for this purpose.

be tied explicitly to their contributions because he did not want to saddle future generations of workers with a large tax burden. By 1940, however, the OASI pension program, which is now commonly known as Social Security, had been switched to a pay-as-you go program that weakened the tie between individual contributions and benefits in part to insure that the first workers to receive benefits in 1940 received adequate sums.

The OASI program, widely known as Social Security, broadened the coverage of pension programs for workers to near universality by the 1960s, and for many decades provided an implicit subsidy to the OASI pension recipients. Households headed by the elderly now have the lowest poverty rates of any age group. At the same time, however, Roosevelt's fears that breaking a strong tie between contributions and pensions has come true. Since 1940 the rise in the share of elderly in the American population has caused the Social Security program to become increasingly expensive, as social security tax rates have risen from less than 2 percent to over 10 percent of workers' earnings. Social Security reform is a perennial source for public debate. Meanwhile, the OAA program continued to provide means-tested benefits into the early 1970s, when the Supplemental Security Income program took over this role.

The raucous debates over how to resolve the problems with the OASI program raise a question about government action for old-age security. What would have happened had the federal government played a much more limited role? Say instead they had never adopted the OASI program and just focused on the OAA matching grants for the needy elderly. How successful would that program have been in reducing the health and welfare problems associated with poverty for the elderly?

As a first step in addressing this question, we examine the impact of the Old Age Assistance state programs on elderly mortality rates during the period from 1929 through 1938 before OASI was introduced. The state programs have often been described as old-age pension programs, but they in no way resembled the modern OASI pensions because they were not based on payments of taxes in advance of old-age in return for payments upon reaching age 65.[10] The original focus of the state programs was to allow the *needy* elderly to live on their own, rather than to live among the needy population in almshouses provided by local governments. The states first showed interest in means-tested old-age assistance programs in the late 1920s, as several introduced legislation that gave counties options to develop programs. Several states began requiring programs and providing funding for them in 1929 and by 1934 over half the states had adopted them. The Social Security Act of 1935 offered matching grants to states to assist in providing old-age benefits to needy elderly, while each state determined the level of benefits and other features of the program. At some time between 1935 and 1940, each of the states adopted the necessary enabling legislation to accept the matching grants.

We take advantage of the gradual implementation of old-age assistance programs across states and the different benefits offered in the programs to assess their impact on the mortality rates of the elderly. We created a panel data set for 75 cities with annual data for the years 1929 through 1938 that combines data on mortality rates for different age groups and information on OAA benefits and other forms of government relief spending. The data set covers the period when states were choosing whether to adopt their own programs

[10] The elderly needy with property did contribute some resources. In many states OAA recipients who had property often had to sign over their property or allow the government a first lien on the property to receive benefits.

without federal support prior to 1935 and the period between 1935 and 1938 as states introduced the state/federal programs at various points in time. To avoid confounding effects, we stop the analysis before the national Social Security pension system started to pay benefits. Although the OAA programs initially had a limited impact in terms of participation rates among elderly, it was an important source of income for poor elderly. The average benefit in 1940 was $241, a relatively generous amount when compared with the average per capita income of $595.[11]

We estimate the relationship between Old Age Assistance and mortality using three measures of the program, several specifications, and multiple procedures ranging from difference-in-difference to difference-in-difference-in-difference analysis to instrumental variables. The results suggest that the OAA programs did not contribute to a reduction in overall mortality rates for the elderly who were eligible for assistance. One reason for this finding is that Old Age Assistance may have had countervailing effects. By allowing the needy elderly to live outside almshouses, the payments may have reduced their exposure to communicable disease. This benefit may well have been offset by less access to care when sick and to personal isolation, both factors that have been found to increase death rates in modern studies.

3.2. Old Age Assistance Programs

Local governments in the United States held the primary responsibility for providing benefits to the poor through the first decade of the 20th century. This built on the tradition of local poor relief inherited from the British during the colonial era. The states became

[11] See Friedberg (1999).

more heavily involved with the introduction of mothers' pensions, workers' compensation, and aid to the blind during the 1910s and 1920s. The federal and state governments in the United States struggled with the issue of specific relief to the elderly later than in many other western countries. In 1889 Germany was the first country to provide a general OASI-style old-age pension plan through compulsory contributory insurance. Other European and Latin American countries followed suit over the next thirty years. Most of the systems covered only employed workers and provided benefits for both invalidity and old age, although Switzerland and Latin American countries limited the coverage to workers of different categories. Denmark in 1891 and several other countries opted instead for OAA-style assistance targeted at the elderly people with insufficient means. Great Britain started in 1908 with a system of non-contributory means tested benefits for the elderly but then switched to a compulsory contributory OASI pension program in 1925.[12]

Specific legislation targeted at the elderly was first passed in several western and Midwestern states during the 1920s, as seen in Tables 8 and 9. Details of the initial state legislation are shown in Table 10. The early laws were not compulsory. Instead, they gave the option to local governments to create county-financed programs specifically targeted at the elderly. As the country sunk into Depression after 1929, state governments began to study the plight of the elderly in more detail. Studies in New York in 1930 and Connecticut in 1932, summarized in Table 11, found that nearly 50 percent of the population aged 65 years or older had less than an estimated subsistence income level of $25 per month. The share of elderly individuals with less than $300 per year in income and property valued at less than $5,000 exceeded 46 percent in Connecticut and 74.5 percent in New York City.

[12] Source: Committee on Economic Security, 1935.

Table 8. Timeline of Adoptions and Amendments for OAA Laws in States Where Cities in Our Sample Are Located

State	1929	1930	1931	1932	1933	1934	1935	1936	1937	1938	1939-40
Alabama	No Law							A, 65, $30, F			
California	No Law	A, 70, $30						65, F	$35		
Colorado	A(1927), 70, $30			65				F	$45		
Connecticut	No Law						A, 65, $30	F			$39
Delaware	No Law		A, 65, $25					F			
Florida	No Law						A, 65, $30		F		
Georgia	No Law								A, 65, $30	F	
Iowa	No Law					A, 65, $25		F			
Illinois	No Law							A, 65, $30	F		
Indiana	No Law					A, 70, $15		$30, F			65
Kansas	No Law								A, 65, $40, F		
Kentucky	A(1926), 70, $21								65, $15, F		
Louisiana	No Law							A, 65, $40, F			
Massachusetts	No Law		A, 70, $40					65, F			
Michigan	No Law					A, 70, $30		65, F			
Minnesota	A, 70, $30							65, F			
Missouri	No Law						A, 70, $30	F			65
Nebraska	No Law				A, 65, $20			F			
New Jersey	No Law			A, 70, $30				65, F			$40
New York	No Law		A, 70, $40					F	65		
Ohio	No Law					A, 65, $25		$30, F			
Oklahoma	No Law							A, 65, $30, F			
Oregon	No Law					A, 70, $30		F		65	
Pennsylvania	No Law					A, 70, $30			F		65
Rhode Island	No Law						A, 65, $30	F			
Tennessee	No Law								A, 65, $25	F	
Utah	A, 65, $25							F		$30	
Virginia	No Law									A, 65, $20	F
Washington	No Law				A, 65, $30			F			
Wisconsin	A(1925), 70, $30							65, F			

Source for Table 8: statutes and Social Security Yearbook 1939
Note for Table 8: "A" means the OAA law was adopted and is in effect; "A(1927)" means that the law is in effect since 1927; "70" or "65" denotes the minimum age required to be eligible for OAA payments; "$35" signifies the maximum monthly benefit (when no maximum monthly benefit was specified in the law, we considered it to be $40); "F" means that the state started to get subsidies from the federal government. If a change in legislation took effect before the mid of the year, we considered the law changed during that year, otherwise the change was assigned for the next year.

The state old-age assistance programs were designed to allow the poor elderly who had lived in the state for a significant period of time to receive enough benefits to live on their own. In some cases this was stated in the legislation. The Florida 1935 Bill enacting OAA law stated that "the intention and purpose of this Act is to give aid to each applicant in his or her home or in some other suitable home in preference to placing him or her in an institution." In the early 1930s the population in almshouses rose sharply in some areas. When OAA programs were introduced in Alabama, Colorado, Georgia, Minnesota, Tennessee and West Virginia, they reported a decline in the elderly population in almshouses.[13] Over half of the states in Table 10 treated OAA as a replacement for other state aid, as more than half prevented the OAA recipients from receiving any other form of state aid. The benefits in most states allowed the eligible elderly to choose between living outside or inside almshouses, as most states in Table 10 would not pay benefits to elderly with relatives who could care for them. The minimum age of eligibility (Table 8) was 65 in most states, 70 in several states. Most states in Table 10 required U.S citizenship. All

[13] Source: Social Security Bulletin, March 1938 pp.15 and 42. Another study reported in the Monthly Labor Review that resembles a rough difference-in-difference analysis shows that states with old-age pension laws experienced roughly half the increase in populations in almshouses between 1930 and 1931/1932 as did states without old-age pension laws ((Anonymous, 1933, 1095),

Table 9. Timeline of OAA Law Adoption in States Not in Table 8

Year of adoption	State
1913	Alaska (July 1937)
1923	Montana (June 1936)
1925	Nevada (August 1937)
1927	Maryland (February 1936)
1929	Wyoming (February 1936)
1931	Hawaii (September 1936), Idaho (February 1936) , New Hampshire (February 1936), West Virginia (November 1936)
1933	Arizona (July 1937), Maine (May 1936), North Dakota (May 1936)
1935	Texas (July 1936), Vermont (February 1936)
1936	Mississippi (February 1936)
1937	Arkansas (March 1936), New Mexico (May 1936), North Carolina (July 1937), South Carolina (August 1937), South Dakota (October 1936)

Source: statutes, Social Security Yearbook 1939
Note: in parenthesis is the time since the state started to get subsidies from the federal government

required a minimum period of state residency, ranging from 1 year in a few states to 15 years in the majority of states in Table 10. Minimum county residency requirements were less stiff, primarily in states where the state provided funds for the program. The residency requirements meant that the elderly had virtually no opportunity to gain higher OAA benefits in the short run by migrating across state lines. Typically, they might have to wait several years to become eligible if they switched states.

Benefits were granted up to a monthly maximum listed in Tables 8 and 11 based on the difference between the income of the applicant and an established monthly budget. As a consequence, until an individual achieved an income equal to the established budget, every additional dollar earned was in effect taxed by 100 percent, creating a significant disincentive to work. Almost all states imposed some eligibility conditions related to the applicant's

Table 10. Details about OAA Legislation in Year States First Passed Law

State	Year First Adopted OAA	Minimum Years U.S. Citizen	Minimum Years State Resident	Minimum Years County Resident	Must Have No Relatives Available to Care For Them	Lien on Property	May Require Transfer of Property	Can Not Receive Other State Aid	Wealth or Property Eligibility Cap	Annual Income Eligibility Cap	Maximum Monthly Amount for Individual	Mandatory/ State Support
AL	1936	UorC	1	1	Yes	Yes	Yes	No	no	360	30	Yes/Yes
CA	1930	15	15	1	Yes	No	Yes	No	3000	no	30[4]	Yes/No
CO	1927	15	15	15	Yes	No	Yes	Yes	3000	no	30[4]	No/No
DE	1931	15	5	0	Yes	No	No	Yes	no	no	25[4]	Yes/Yes
FL	1935	UorC	10	1	No	No	No	Yes	400	no	30	Yes/Yes
GA	1937	0	1	0	No	Yes	No	No	no	no	30[4]	Yes/Yes
IA	1934	15	10	2	Yes	Yes	Yes	Yes	no	365	25[4]	Yes/Yes
IL	1936	UorC	10	1	No	Yes	No	No	5000	260	30	Yes/Yes
IN	1934	15	15	15	Yes	Yes	Yes	No	1000	no	15[4]	Yes/Yes
KS	1937	0	1	0	No	Yes	No	Yes	no	no[3]	no	Yes/Yes
KY	1926	15	10	10	Yes	Yes	No	No	2500	400	21	No/No
LA	1936	0	5	UorC	Yes	No	Yes	Yes	no	no[3]	no	Yes/Yes
MA	1931	20	20	0	No	No	No	No	no	no	no	Yes/Yes
MI	1934	15	10	0	Yes	Yes	Yes	Yes	3000	365	30[4]	Yes/Yes
MN	1929	15	15	15	Yes	Yes	Yes	Yes	3000	no	30[4]	No/No
MO	1935	UorC	1	0	Yes	No	No	Yes	1500	no[3]	30[4]	Yes/Yes
NE	1933	15	15	0	Yes	Yes	Yes	Yes	no	300	20	Yes/Yes
NJ	1932	UorC	15	1	Yes	No	Yes	Yes	3000	no	30	Yes/Yes
NY	1931	UorC	10	1	Yes	No	No	No	no	no	no	Yes/Yes
OH	1934	15	15	1	Yes	No	Yes	No	3000	300	25[4]	Yes/Yes
OK	1936	UorC	15	0	No	No	No	No	2000	350	15	Yes/Yes
OR	1934	15	15	2	Yes	Yes	Yes	Yes	3000	no	30[4]	Yes/No
PA	1934	15	15	0	Yes	No	No	Yes	no	no	30	Yes/Yes
RI	1935	UorC	5	UorC	Yes	Yes	No	No	5000	no	30[4]	Yes/Yes
TN	1937	0	1	0	No	Yes	No	No	no	no[3]	25[4]	Yes/Yes
UT	1929	15	15	5	Yes	Yes	No	Yes	no	300	25	Yes/No
VA	1938	0	1	0	Yes	Yes	No	Yes	no	no	20[4]	Yes/Yes
WA	1933	15	15	5	Yes	Yes	Yes	Yes	no	360	30	Yes/No
WI	1925	15	15	15	Yes	Yes	Yes	Yes	3000	no	30[4]	No/No

Source for Table 10: State statutes in year of first passage.
Notes for Table 10:

1. In columns "Minimum Years U.S. Citizen", "Minimum Years State Resident" and "Minimum Years County Resident" 0 means no requirements, UorC means the applicant needed to be a US citizen and/or a county resident at the date of the application
2. "Yes" in column "Lien on Property" means that a lien was established for the property of the applicant and that after she died, the benefits granted were deducted from the value of the real estate, in many cases with an interest rate of 3-6 percent. However, the estate of the deceased was not settled until the surviving spouse died or ceased to occupy the estate. For states where column "May Require Transfer of Property" indicates "Yes", the commission in charge with determining and approving OAA benefits had the choice of requiring absolute conveyance of all or part of the property of the applicant. "Yes" in column "Can Receive Other State Aid" means that persons receiving OAA benefits were not entitled to receive other assistance from the state, except for medical reasons. "No" in all the cases means that there was no specification in the law regarding the particular issue.
3. Not a clear stated maximum income
4. Maximum benefit is considering all the income of the applicant, is not the maximum OAA monthly benefit
5. Information in the table is based on OAA laws as they were initially adopted.

wealth, either in terms of annual income - in many cases no more than one dollar per day - or in terms of the value of their property – between $1,000 and $5,000.

Recipients with property in most states essentially borrowed the benefit payments they received from the state. Many states established a lien on the property that required repayment of the benefits upon transfer of the property. Some required the recipient to transfer the property to the state. [14]

The stringency of the requirements contributed to relatively small expenditures on OAA in most states. As seen in Table 12, in the states with OAA laws in 1934, only 7.7

[14] Additional conditions in some states included the following rules: the applicant had not deserted his wife in the last 15 years, was not a beggar, was not imprisoned, was not in a charitable institution or he did not receive other state aid.

Table 11. Percentage of Persons 65 and Over Having Property Valued at Less than $5000 and Income Less than $300 Annually

New York, 1930			
	Individuals	Households	Combined
Canton Village	36.9	10.0	25.0
Selected Cities	46.5	28.2	33.5
Ostego County	69.2	31.8	50.2
New York City	74.5	48.4	65.1

Connecticut, 1932	
Individuals	Households
46.0 (Urban 50%/ Rural 39%)	24.8% (Urban 26.5%/ Rural 21.9%)

Source: Old Age Security Staff Report 1935

percent of people who met the minimum age requirement were receiving benefits that averaged $15 per month during a period of very high unemployment. Recipiency rates varied considerably between states due to a combination of differences in the depth of the depression and to differences in eligibility rules. In Iowa only 1.6 percent of the elderly over the minimum age of 65 were enrolled. Indiana had the highest recipiency rate at 16.9 percent of the persons aged 70 and above. Prior to the Social Security Act the maximum benefit listed in the states covered in our city sample was $40 in New York and Massachusetts. As seen in Table 8, the remaining states offered maximums ranging from $15 in Indiana to $30 in several states. However, the average benefits paid in 1934 were well below the maximums in all the states where comparisons can be made between Tables 8 and 12.

The federal government first became involved in providing assistance for the poor elderly when the Roosevelt administration took over responsibility for the lion's share of

Table 12. State Old Age Assistance Benefits, 1934

	# Enrolled[1]		# Eligible Based on Age[2]	Percentage Enrolled	Average Monthly Pension[1]		Annual Cost [3]	
Total	180,003		2,330,390	7.7	$15		$31,192,492	
Alaska	*446*	(c)	*3,437*	*11.1*	*$20.82*		*$95,705*	
Arizona	*1,974*	(a)	*9,118*	*21.6*	*$9.01*		*$200,927*	
California	19,300	(a)	210,379	9.2	$21.16		$3,502,000	
Colorado	8,705		61,787	14.1	$8.59		$172,481	
Delaware	1,610	(a)	16,678	9.7	$9.79		$188,740	
Hawaii				No information available				
Idaho	*1,275*		*22,310*	*5.7*	*$8.85*		*$114,521*	
Indiana	23,418	(b)	138,426	16.9	$6.13	(b)	$1,254,169	(h)
Iowa	3,000	(c)	184,239	1.6	$13.50	(c)	$475,500	(d)
Kentucky				No pensions being paid				
Maine				Not yet in effect				
Maryland	*141*	(e)	*92,972*	*0.2*	*$29.90*		*$50,217*	
Massachusetts	20,023	(e)	156,590	12.8	$24.35		$5,411,723	
Michigan	2,660	(e)	148,853	1.8	$9.59	(e)	$306,096	(f)
Minnesota	2,655		94,401	2.8	$13.20		$420,536	
Montana	*1,781*		*14,377*	*12.4*	*$7.28*		*$155,525*	
Nebraska				Not much being done due to lack of funds				
Nevada	23		4,814	0.5	$15.00		$3,320	
New Hampshire	*1,423*	(c)	*25,714*	*5.5*	*$19.06*	(g)	*$298,722*	(f)
New Jersey	10,560	(g)	112,594	9.4	$12.72		$1,375,693	
New York	51,228		373,878	13.7	$22.16		$13,592,080	
North Dakota				No pensions being paid				
Ohio	24,000	(e)	414,836	5.8	$13.99	(a)	$3,000,000	(f)
Oregon				Administered by counties; no info available for state				
Pennsylvania				Law just being put into effect				
Utah	930		22,665	4.1	$8.56		$95,599	
Washington	2,239	(b)	101,503	2.2	No information available			
West Virginia				No pensions being paid				
Wisconsin	1,969		112,112	1.8	$16.75		$395,707	
Wyoming	*643*		*8,707*	*7.4*	*$10.79*		*$83,231*	

Source: Old Age Security Staff Report 1935

1 - Where no special reference is given, the figures are as of December 31, 1933

2 - 1930 Census figures

3 - Where no special reference is given, the figures represent actual cost for the year 1933

(a) As of October 1, 1934

(b) As of August 1934

(c) As of December 1934

(d) Estimated from expenditures of April through November 1934 - $317,000

(e) As of November 1934

(f) Estimated from monthly figures

(g) As of September 1934

(h) Appropriation for 1934

relief for people of all ages in 1933 with the Federal Emergency Relief Administration. In 1935 the Second New Deal revamped the federal government's involvement in relief. As part of a compromise between Congress and the Roosevelt administration, the federal emergency programs focused on providing emergency work relief, and responsibility for relief to "unemployables" was returned to the states (see Wallis, Fishback, and Kantor 2003). The federal government offered to help fund aid for dependent children (like mothers' pensions), aid to the blind, and OAA for the needy elderly through a dollar-for-dollar matching grant program enacted as part of the Social Security Act of 1935 (along with the OASI pension program and Unemployment Insurance grants).

To receive the matching grants, the states had to pass enabling legislation that either established OAA for the first time or modified existing OAA programs to meet the following federal requirements. The new state/federal OAA programs were mandatory for the entire state. A cap on monthly benefits subsidized by the federal government was set at $30 per individual; therefore, the maximum federal contribution was limited to $15 per individual. State and local residency requirements could be no more stringent than state residency for five years out of the last nine with a continuous residence of one year before the application. Recipients still had to be U.S. citizens but no minimum time was set. The minimum age was set at 65 years, although states with higher minimums could still receive matching grants through January 1, 1940.[15]

As seen in Tables 8 and 9, most states passed the enabling legislation in 1936. An April 1937 Social Security Board Study summarized in Table 13 found that about 14 percent

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[15] Pennsylvania decreased the minimum age effective January 1, 1940, Missouri effective July 1939, Indiana and Oregon effective 1938.

Table 13. Means of Support for Persons 65 Years of Age and Older in the U.S., Estimate for April 1937

Means of Support	Percentage of Population 65 Years of Age and Over
A. Self dependent	35.1
-current earnings	12.8
-savings, real estate, securities	15.0
-pensions (federal, state, municipal, private industrial, trade union, other), insurance annuities, other resources	7.3
B. Dependent	64.9
1. Supported wholly or partially by public or private social agencies	20.3
-in receipt of public assistance under the Social Security Act (December 1936)	14.3
-in receipt of emergency unemployment relief, poor relief, or of old-age assistance not under the Social Security Act	2.6
-in receipt of earnings or subsistence payments under the Works Program (December 1936)	0.8
-in receipt of organized private charities, in public homes, hospitals for mental disease, prisons and jails, private homes for the aged, others	3.4
2. Dependent on friends and relatives (wholly or almost wholly)	44.6

Source: Social Security Bulletin, March 1938, pp. 6

of the population aged 65 and over received OAA assistance under the Social Security Act at the end of 1936, while another 6 percent received aid through some other form of relief. By 1940 all states had established OAA. Despite a substantially better economy in 1940 than in 1937, the expansion of OAA coverage to all states meant that 21.7 percent of people 65 years of age or older were receiving benefits. The first OASI Social Security pension

Table 14. Old Age Assistance: Relief Status Within 30 Days and Within 2 Years Prior to Investigation of Recipients Accepted During the Fiscal Year 1937-38, in All States With Plans Approved by the Social Security Board

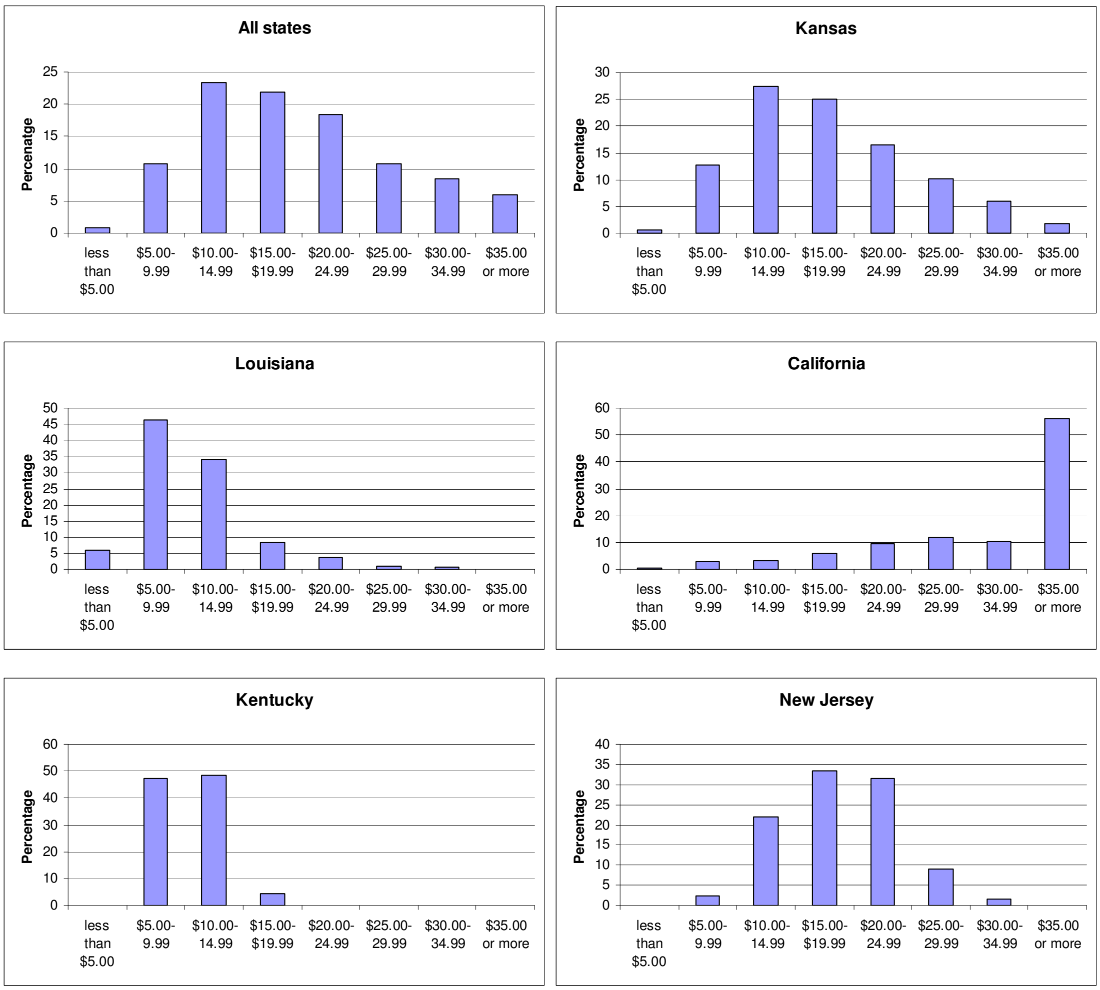
No assistance within two years:	57.7%
Some assistance within two years:	39.3%
- within 30 days:	30.1%
-general relief:	22.2%
-other public assistance:	3.2%
-Works Program earnings:	2.7%
-care in public institution:	1.3%
-private assistance:	1.2%
- none within 30 days:	9.2%
Unknown within two years, none within 30 days:	3.0%

Source: Social Security Bulletin, December 1938, pp. 3

payments were made in 1940 and accounted for less than four percent of the total of OAA and OASI payments to the elderly.

A study of 21 states showed that the median age of beneficiaries was 74.1 and 53 percent were women (Fisher 1946). A significant minority of the elderly who signed up for OAA under the new SSA matching-grant rules had received some form of public assistance *before* moving to OAA. A study by the Social Security Board (1938) summarized in Table 14 suggests that roughly 40 percent of the elderly who signed up for the SSA matching-grant version of OAA in 1937 and 1938 had received some form of public assistance within the past two years. Three-fourths of that group had received public aid within the 30 days prior to their acceptance in the OAA program. It is likely that some portion of the new recipients had been eligible for aid for a period before signing up. Others might have newly reached

Figure 1. Old Age Assistance: Percentage Distribution of Monthly Grants Initially Approved for Recipients Accepted During the Fiscal Year 1937-1938, in States With a Plan Approved by The Social Security Board



Source: Social Security Bulletin, November 1938, pp.13

the eligible age or had a recent change in their economic circumstances. As yet we have not found information on the relative size of these groups.

Average annual OAA benefits in 1940 ranged from $91 to $455 across states, while the share of elderly receiving benefits in the states ranged from 8.1 percent, to 50.2 percent

(Friedberg 1999). Information from 1937-38 in Figure 1 offers a snapshot that shows a broad range of benefits across states, although the distributions varied across time. Part of the distribution was determined by differences in state maximums, which ranged from $15 in Kentucky to a high of $45 in Colorado in the states in our city sample. The distributions of benefits within states displayed quite different patterns. In some states it appears likely that the variation was caused by differences in the gap between the individual's resources and the state's estimates of the cost of necessities. Kansas and New Jersey displayed bell-curve type distributions below their maximums of $40 and $30, respectively. Kentucky's low maximum meant that roughly half of recipients were paid between $10 and $15 and roughly half were paid between $5 and $10. California and Louisiana illustrate two extremes. More than half of the California recipients received the state maximum of $35. Louisiana passed a generous maximum of $40 but paid more than 80 percent of their recipients less than $15. A detailed description of the distribution of benefits for all states can be found in Table 15.

3.3. Previous Literature on the Impact of Old Age Assistance

Several studies focus their attention on the impact Old Age Assistance program had on the elderly. Noting that elderly labor force participation fell between 1930 and 1950 when OAA was introduced, Parsons (1991) analyzed state aggregates in 1930, 1940 and 1950. He found that OAA benefits reduced the labor force among males 65 and older by 6.1 percent which represents one half of the observed decline during that period. Friedberg (1999) used individual level data from the 1940 and 1950 Censuses and controlled for personal characteristics. Using average OAA benefits at the state level, she finds that labor force participation would have risen slightly in the absence of OAA programs.

Table 15. Old Age Assistance: Percentage Distribution of Monthly Grants Initially Approved for Recipients Accepted During the Fiscal Year 1937-38, in States With a Plan Approved by The Social Security Board

State	less than $5.00	$5.00-9.99	$10.00-14.99	$15.00-$19.99	$20.00-24.99	$25.00-29.99	$30.00-34.99	$35.00 or more
All states	0.8	10.7	23.3	21.8	18.4	10.7	8.4	5.9
Alabama	2.3	28.5	33.3	16.5	10	4.3	5.1	0
Alaska	0	0	2	30.5	12.3	4.9	28.7	21.6
Arizona	0.2	0.8	2.3	11.4	25.5	28.3	31.5	0
Arkansas	0.7	65	34.3	0	0	0	0	0
California	0.4	2.8	3.2	5.9	9.5	11.8	10.2	56.2
Colorado	0	0.4	1.1	2.8	5.7	7.7	15	67.3
Connecticut	0	2.4	2.2	13.3	17.4	24.9	39.8	0
D.C.	0	0.7	13.2	13.5	20.3	27.6	21.2	3.5
Delaware	0	33.4	42.9	19.7	2	2	0	0
Florida	0	11.4	40.8	30.4	12.3	4	1.1	0
Georgia	4.8	58.7	24.8	7.2	2.6	0.9	1	0
Hawaii	0.3	16.3	57.2	17.9	2.9	1.7	3.7	0
Idaho	0.2	1.8	13.2	30.1	26.8	12.7	15.2	0
Illinois	0	4.4	20.9	28.9	21.7	17.4	6.7	0
Indiana	0.6	8.8	36.3	31.3	16.1	5	1.9	0
Iowa	2.1	7	13.5	24.6	47.3	5.5	0	0
Kansas	0.5	12.8	27.5	25	16.4	10.1	6	1.7
Kentucky	0	47.3	48.3	4.4	0	0	0	0
Louisiana	5.8	46.4	34	8.4	3.7	0.9	0.7	0.1
Maine	0.1	2.4	13.9	30.6	23.2	15.3	14.5	0
Maryland	0.6	10.2	28.9	20.9	27.1	5.7	6.6	0
Massachusetts	0	0.5	2.7	6.7	19.5	19	47.9	3.7
Michigan	0.5	3.4	23.8	32.3	24	10.4	5.6	0
Minnesota	0.2	4.3	12.9	34.3	28.1	15.5	4.7	0
Mississippi	33.9	60.1	5.5	0.5	0	0	0	0
Missouri	0.1	8.5	37.2	33.5	14.4	4.9	1.4	0
Montana	0	2.3	20.5	34.8	25.4	10.8	6.2	0
Nebraska	0	10.5	40.6	31.3	12.8	3.9	0.9	0
Nevada	0	0.2	2.4	4.3	12.2	13.9	67	0
New Hampshire	0	1.8	11.3	21.1	24.3	16.5	25	0
New Jersey	0	2.4	22.1	33.5	31.4	8.9	1.7	0
New Mexico	0.3	32.6	31.8	16.7	9.1	5.1	3.5	0.9
New York	0.1	1.4	17.6	22.2	23.1	17.8	10.7	7.1
North Carolina	2	59.6	28.1	7.4	2.1	0.4	0.4	0
North Dakota	0.1	6.4	28.3	32.8	20.5	8	3.9	0
Ohio	0.1	1.4	7.2	32.3	33.7	19.8	5.5	0
Oklahoma	0.9	10.2	43.9	18.7	15.9	6.4	4	0
Oregon	0	2.3	18.7	25.4	24.6	14.7	14.3	0
Pennsylvania	0.9	1.6	23.1	18.5	26	18.5	11.4	0

Rhode Island	0	4.7	28	23.3	23.8	13.9	6.3	0
South Carolina	0.3	43.7	37.4	12.4	6.2	0	0	0
South Dakota	0.2	3	22.1	43.9	21.7	6.6	2.5	0
Tennessee	0	15.4	51.8	22.5	6.8	3.5	0	0
Texas	0	15.7	49.9	25.3	7.6	1.4	0.1	0
Utah	0.1	2	5.1	18.8	23.2	16	34.4	0.4
Vermont	0	16.2	42.1	25.9	11	2.8	2	0
Washington	0	0.1	9.9	14	28.8	17.7	29.5	0
West Virginia	0.2	12.5	48.5	27.5	7.3	2.3	1.7	0
Wisconsin	0.2	3.9	20.4	29.5	20.1	13.5	12.4	0
Wyoming	0	1.9	10.9	26.4	31.2	19.4	10.2	0

Source: Social Security Bulletin, November 1938, pp.13

Costa (1999) examined the impact of OAA on the proportion of elderly non-married women living with their relatives using state aggregates from 1940 and 1950 when OAA accounted for the lion's share of benefits paid to the elderly. Her study relied on variation in the benefits offered and changes in matching grants that caused some low benefit states in 1940 to raise their benefits more than other states did by 1950. The 27 percent increase in average OAA benefits explains about 80 percent of the decrease in the proportion of older non-married women living with family members. She also found that states that disallowed benefits to women with family who could care for them and states with lien requirements increased the share of women living with relatives.

OAA is among several New Deal programs included in the 1930s relief programs examined by Fishback, Haines and Kantor (2007) in their analysis of the impact of relief spending on infant deaths, noninfant deaths, various death rates from disease, and general fertility rates for 114 cities in U.S. between 1929 and 1940. They find a negative relationship between relief spending per capita and infant mortality, suicide rates, and some other causes of death, as well as a positive impact of the programs on the general fertility rate. However, the authors do not find a statistically significant impact of the programs on non-infant mortality rates.

Andrea Balan Cohen (2006) has independently performed an analysis of the impact of OAA on mortality rates for the period 1934 to 1955 using state-level aggregates on mortality by age, race, gender, cause of death and information on household consumption. She finds a sizeable reduction in overall elderly mortality rates associated with OAA. When she focuses on specific types of mortality she finds that OAA had little effect on infectious disease mortality until after 1944, soon after penicillin and as other antibiotics became available. Higher OAA benefits were associated with lower rates of cardiovascular mortality and lower rates of other death rates associate with risky behavior like smoking and reckless driving. As will be shown below, our results do not suggest the same strong reduction on overall mortality found by Balan Cohen (2006). Our sense is that the reason for the difference in results is the difference in time period studied. Her analysis extends into the 1950s and the effects of Old Age Assistance on mortality in her estimation results appear to be much stronger after 1942 than in the period that we are studying. At the end of section five we explore in more depth the differences in findings and explanations for those differences.

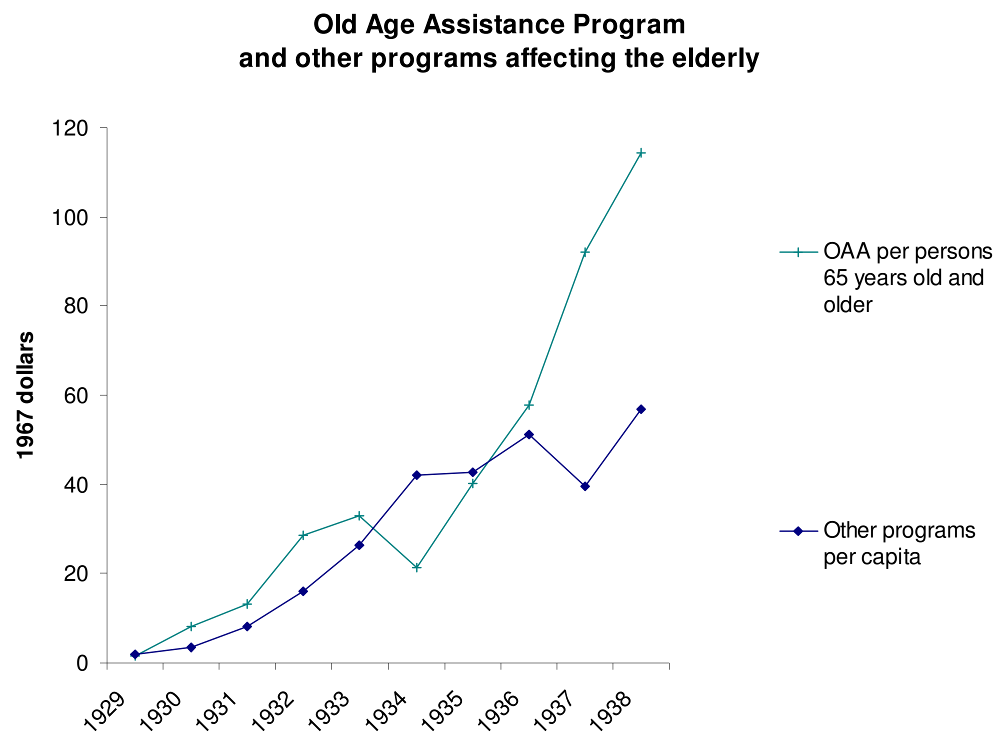
3.4. Description of the Data

We examine the impact of old-age assistance programs on mortality rates for the older age groups of the United States. Since New Deal relief programs in general and Old Age Assistance in particular targeted the low income segments of the population, and the elderly with low incomes tend to have higher mortality rates, the total mortality rates of the elderly can serve as one indirect indicator of the programs' effectiveness. Further, mortality rates among the elderly serve as important measures of the health of the elderly population as well as socio-economic well-being because mortality is influenced by both income and psychological factors. Lower mortality rates are likely to strongly improve the psychological health of the living elderly by reducing the loss of close friends. A part of the mortality rise associated with isolation loss may have manifested itself in the suicides studied by Balan Cohen (2006), but it also could manifest itself in a variety of other forms of stress that contributed to death.

In contrast to other studies that looked at the impact of Old Age Assistance programs, we have accumulated data about spending not only for Old Age Assistance, but also for all but a small share of the other public and private programs for assistance to the needy for 114 city/county observations in each year from 1929 through 1938 (Baird, 1992; Fishback, Haines, and Kantor, 2007). These 114 large areas represent 66 percent of the total population of U.S. The data on public assistance of other kinds allows us to control for expenditures from all of the other public relief programs that provided benefits to all age groups, including seniors living with others in low-income households.[16] We match the relief data with information on mortality for persons aged 55 to 64, 65 to 74 and 75 and up for the 75 cities reporting age-specific mortality in the annual *Mortality Statistics* volumes for the years

[16]The data include information on city and state relief spending, federal *direct relief* under the Federal Emergency Relief Administration (FERA), *work relief* from the Civil Works Administration, the FERA, and the Works Progress Administration, private relief, aid to dependent children and mothers' pensions , and aid to the blind. The data for OAA and the other relief programs are relatively accurate measures of the net aid received by households since they do not include the salaries of administrative personnel of diverse agencies which distributed them. For a more detailed description of the relief spending data, see Fishback, Haines, and Kantor (2007) and Price Fishback's website at the University of Arizona (http://econ.arizona.edu/faculty/Fishback.aspx) under "Datasets from Published Research Projects."

Figure 2.

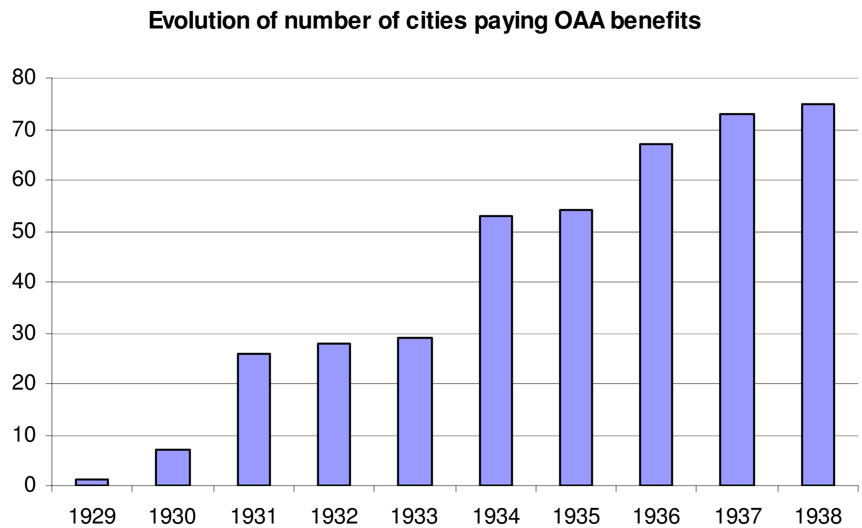


Source: Baird (1992)

1929 through 1938 published by the U. S. Bureau of the Census. The 75 cities account for roughly 48 percent of the total population of U.S.

As can be seen from Figure 2, OAA benefits paid per person over aged 65 for the United States as a whole increased in each year during the 1930s except 1934. A possible explanation for the 1934 decline might be that states substituted away from their own OAA payments when the FERA relief programs dramatically increased federal welfare payments in 1933 and 1934. This scenario seems plausible since the number of cities paying OAA benefits increased in 1934 (see Figure 3). The key to our analysis is the sharp changes that

Figure 3.

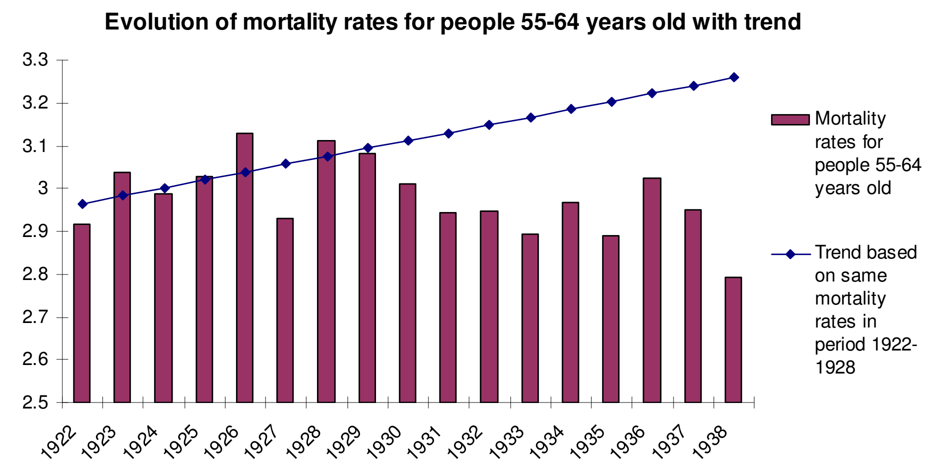


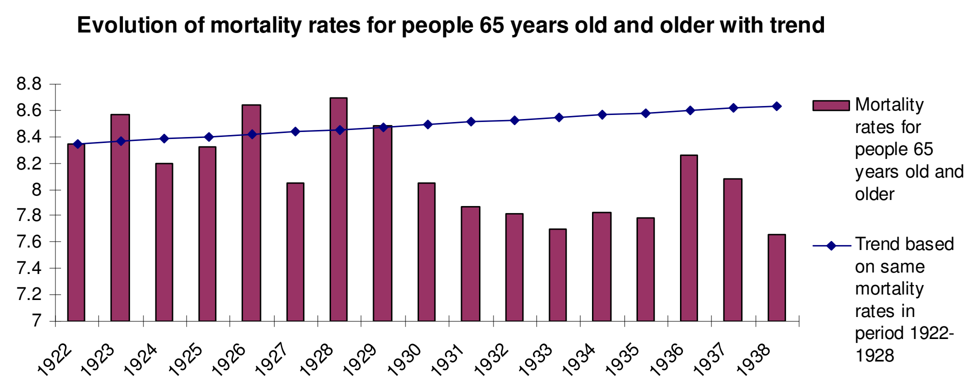
Source: Baird (1992)

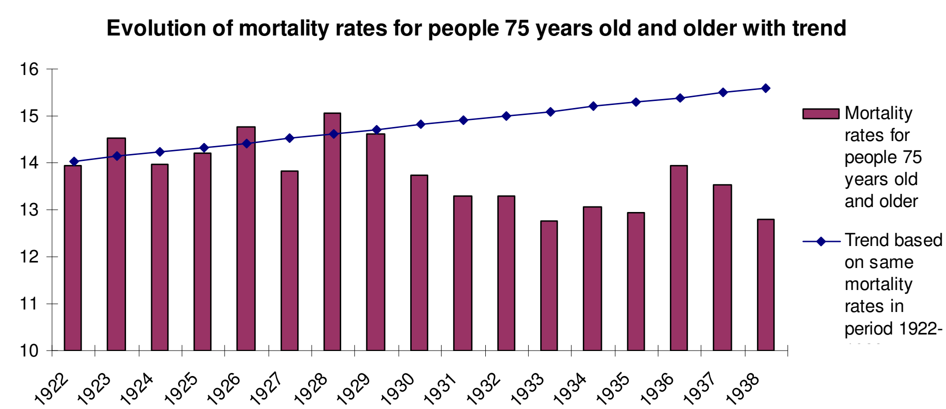
occurred in access to OAA benefits during the period we study. As can be seen in Figure 3, in the sample of 75 cities only one city reported the payment of OAA benefits specific to the elderly in 1929. Between 20 and 30 cities reported benefits in 1931 through 1933. The number jumped over 50, more than two-thirds of the sample by 1934. After the passage of the SSA matching grant law, the number jumped again to 67. All cities in our sample were paying OAA by 1938.

The aggregate mortality patterns in these large cities for people aged above 55 seem inconsistent with the usual expectations about the impact of the Great Depression. The histograms in Figures 4a through 4c show the year by year evolution of mortality rates for three age groups over aged 55 for the 58 cities reporting mortality in all of the years between 1922 and 1938. Linear trend lines based on the trends in mortality from 1922 through 1928

Figure 4. Evolution of Mortality Rates for Different Age Groups With Trend







Source: U.S. Department of Commerce, Bureau of the Census (1925, 1926, 1927a, 1927b, 1929a, 1929b, 1930, 1932, 1934, 1935a, 1935b, 1936a, 1936b, 1937, 1938, 1939, 1940)
Note: 17 cities from the sample were not taken into account because we did not have mortality data for each year in the period 1922-1938. Trend is based on 1922-1928 period.

show a flat or rising trend as the economy entered the Great Depression. However, mortality among the elderly fell relative to each of these trends as the Depression deepened through 1933, rose slightly during the halting recovery from 1934 through 1937 and then fell again in 1938. This is consistent with patterns for overall mortality found by Ruhm (2000) for the modern era and by Fishback, Haines, and Kantor (2007) for the 1930s.[17] One of the questions we seek to answer is what role did the development of OAA play in contributing to the changes in mortality during the 1930s?

To control for changes in the economy that might have contributed to changes in the number of elderly who would be eligible for public assistance, we include a measure of average consumption and a measure of the share of people in the city in the top tier of the U.S. income distribution. The measure of average consumption is retail sales per capita. The measure of the share of people with higher incomes is the number of federal income tax returns filed per capita. The group filing income taxes in the 1930s was an elite group, as less than nine percent of households nationwide earned enough income to pay federal income taxes between 1929 and 1938. Since the federal tax laws were the same across the country, the ratio gives a sense of the share of the population in the city that was earning more than $2,000 for a single person or $5,000 for a family of four at a time when earnings

[17] Other studies in the same category include Ruhm (2003) and Granados (2005). It might be the case that economic cycles are reflected in mortality rates with a lag of several years, a long term effect which would complicate the type of analysis required to identify the effectiveness of the program. However, previous literature explains the pro cyclical character of trend in mortality rates as a short run effect rather than a long run one. Ruhm suggests that the decline in mortality rates during recessions are related to reductions in smoking, height-adjusted weight, driving, and an increase in physical leisure-time activity associated with declines in disposable income.

per full-time equivalent worker in industry were less than $1,300 and many workers were working less than full time or were unemployed.[18]

Changes in health care availability are captured using a measure of the number of hospital beds per capita; whereas shocks in prices are accounted for by using a city-specific cost of living index. We also control for city level trends in demographic measures with straight-line interpolations between Census information in 1930 and 1940 of total population, and the percentages of population who were illiterate, black or foreign born. For more details about the construction of the data see Appendix A.

3.5. Empirical Strategy and Results

The state Old Age Assistance programs changed the nature of means-tested public relief for the elderly. In one sense the new programs served to just replace the existing systems provided to all age groups with a set of benefits targeted specifically to the elderly. If the new systems led to no changes in the benefits received by the elderly, then we would not expect to see a reduction in elderly mortality rates.

OAA programs did lead to some changes in the mechanism through which relief was offered to the elderly. A key statement typically found in most OAA laws was that Old Age Assistance was designed to pay the elderly enough that they could live on their own and thus not rely on other forms of public support. Since many states required that an OAA recipient

[18]Shares of households with taxable income returns is calculated by dividing the number of taxable individual returns (series Y-404) by the number of households (A-288). Individuals were liable for taxes when their incomes reached $2,000, while households with four exemptions (a family of four) were liable when their incomes reached $5,000. (U.S. Bureau of the Census, 1975, pp. 41, 1110-1112.) Full-time equivalent industry earnings are from Robert Margo's estimates using methods similar to those of Stanley Lebergott's (series Ba4419 in Carter, et. al., 2006, volume 2, p. 283).

or couple could not have relatives who could care for them, this meant that the choice of OAA benefits was allowing them to avoid living with non-relatives or in almshouses. If this shift in living arrangements was the key change wrought by OAA programs, then several opposing effects on mortality rates might be expected.

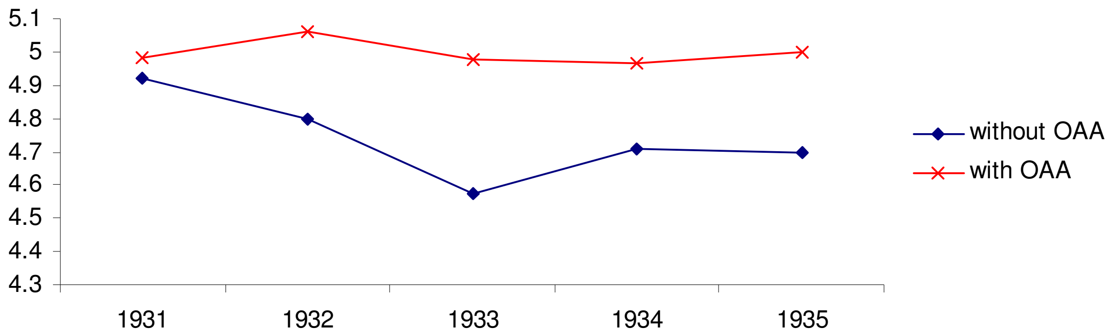
First, OAA benefits might lower the mortality rate by allowing the elderly person or couple to avoid exposure to communicable diseases among other poor living in almshouses or alternative arrangements.

Second, OAA would lower mortality rates to the extent that the program provided more economic resources to the elderly. To the extent that there were economies of scale in household production in the multiple person living arrangement, the extra costs of living alone might have eaten into the extra income and thus the net benefit might have been small.

Third, OAA assistance might have contributed to higher mortality even though the elderly person saw it as a net benefit. There may have been greater isolation from others from living alone. Modern studies suggest that individuals who retire into socially isolated settings tend to have higher mortality rates (Snyder and Evans 2006). Further, by living alone the elderly may have lost easy access to health care provided in almshouses. A BLS report on almshouses in 1925 found that payments to nurses and matrons accounted for approximately 10 percent of the payroll costs at almshouses. Descriptions of the quality of medical care, food, and living conditions from state reports suggested that it ranged from good to very poor. Generally, the quality of conditions was better in large almshouses most often found in cities than in smaller ones (Stewart 1925, 19-37).

Figure 5.

Comparison between average mortality rates for people 65 years old and older and people 55-64 years old in cities with and without OAA law



Source: U.S. Department of Commerce, Bureau of the Census (1935a, 1935b, 1936a, 1936b, 1937)

Figure 5 shows a graphical comparison designed to illustrate the relationship between payment of OAA and the relative mortality of the people eligible for OAA. We first selected groups of cities from two types of states: states that paid OAA in every year from 1931 through 1935 and states that paid no benefits during that period. .For each group of cities we then calculated for each year the difference between the death rate for people aged 65 (eligible for OAA) and the death rate for people aged 55-64 (ineligible for OAA). This difference works to eliminate the impact of trends in mortality experienced by all age groups in the city. Figure 5 shows this eligible/ineligible mortality rate difference for the cities in states that paid OAA and those that did not. The comparison reveals that the eligible/ineligible mortality difference was higher in the cities paying OAA than in the cities without OAA. The absence of a beneficial mortality effect of OAA might simply reflect endogeneity in which areas with higher death rates were more likely to adopt OAA earlier. Below, we examine the relationship between OAA and elderly mortality while including a rich set of covariates to reduce omitted variable bias and using instrumental variable analysis to correct for endogeneity.

We estimate the effect of OAA on mortality rates using several methods, including a difference-in-difference analysis, a difference-in-difference-in-difference analysis, and ultimately an instrumental variables analysis combined with the other approaches. Initially, we use as left hand side variables mortality rates for different age groups: 55 to 64 years of age, 65 years of age and over, and 75 years of age and over. The next step creates a difference-in-difference-in-difference analysis by performing a similar sequence of estimations using city and year fixed effects where the dependent variables are the differences in mortality rates between the 65 years and older and 55-64 age groups and between the 75 and older and 55-64 age groups. Since persons aged 55-64 were not eligible for assistance, they are treated as a control group that most closely matches the death rate profiles of the more elderly groups.

There are some caveats to be aware of in the analysis of the differences. The population aged 55 to 64 may not be a perfect control group if some of the people aged 65 and over are married to spouses aged 55 to 64. In these cases income from old-age assistance for the older spouse may influence mortality for the younger spouse. Nationwide, during fiscal year 1937-38, the percent of married recipients among those who specified their marital status was 41.2 percent.[19]

Given the absence of individual level data, we do not know who received old-age assistance and who did not. The effect we are estimating is not a treatment effect for the people who actually received old age assistance. Instead, the estimates capture the effect of the availability of old-age assistance for the entire age group, including those not eligible and those who did not receive benefits. Thus, the impact of old-age assistance on the mortality of the elderly is determined in part by the extent to which the elderly entered the program and the extent of their eligibility for the program. We partially control for the latter by including the level of retail sales per capita to control for differences in the average consumption levels for the entire population and the percentage of population who filled for taxes to control for differences in distribution of income. This is still a useful estimate on its own terms. Even in the modern era, there are many eligible households who do not apply for welfare benefits. In essence, we are testing whether the presence of a program has an effect on mortality in situations where the effect might have been weakened by low take-up rates as well as the size of benefits when they were received.

It is also important to look at the broader population for another reason. The availability of old-age assistance is likely to affect the behavior of others of eligible age and the extent of support they receive from others closest to them. The availability of old-age benefits might have contributed to reductions in the support provided by friends or by relatives (in the states where benefits were paid to elderly who had family who could take care of them).

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[19] Source: Social Security Bulletin, February 1939.

Another measurement problem develops because some states decided that the minimum age in order to be eligible for OAA benefits was 70 instead of 65. To eliminate this measurement problem we have also run the analysis for persons aged 75 and older. We cannot focus on those 70 and over because the data are only reported for the 65-74 and 75 and over groupings.

The econometric specification for the difference-in-difference analysis takes the following form.

$$M_{ita} = \beta_1\, OAA_{it} + \beta_2\, X_{jt} + \beta_3\, C_i + \beta_4\, Y_t + \varepsilon_{it.}$$

M_{ita} is the number of deaths of persons in age group "a" per hundred people within age group "a" for city i in year t.[20] OAA_{it} is a measure of the OAA program in city i in year t. X_{jt} is a vector of covariates consisting of per capita spending in all of the other programs that provided financial resources for the elderly (including direct relief, work relief and work programs, private programs and programs for blind persons), proxy for the income (retail sales per capita and percentage of population who filed taxes), cost of living index, number of beds per capita and demographics like total population and percentage of black people, illiterate and foreign born. The specification with mortality rate age 65 and over includes also an interaction term between the OAA variable and a dummy if the minimum age was 70 instead of 65. C_i is a vector of city effects and Y_t is a vector of year effects.[21] OAA_{it}, the old-

[20]The denominator was calculated as a linear interpolation of the size of the age group between the 1920, 1930, and 1940 censuses. A detailed description of the construction of the mortality rate variable can be found in Appendix A.

[21]Prior to 1937, mortality was reported by place of occurrence. After 1936, mortality was reported by place of residence. The year fixed effects in the analysis control for this change in national reporting. We have also estimated the series of specifications for the period 1929 through 1936, and there is no substantial change in the results. We have also estimated the model using Weighted Least Squares and the basic results are unchanged.

age assistance policy, is measured in three ways. The first is a dummy variable for the adoption of the OAA law. The second measure is total Old Age Assistance expenditures net of administrative expenses in the city divided by the city population aged 65 and over.[22] Separate annual data on the number of recipients and per recipient average benefits are not available, so this measure combines the two aspects into one measure. Fishback, Haines, and Kantor (2007) used a similar measure for their estimates of the impact of overall relief spending on mortality and fertility rates during the 1930s, as does Balan Cohen (2006) in her study of OAA from 1934 through 1955. The OAA benefits, retail sales per capita and other relief spending per capita are converted to 1967 dollar using the consumer price index.[23]

The third measure is the OAA maximum benefit as stipulated by the state law. As seen in Table 8 the maximum monthly benefit measure varies across states and time with some states changing their maximum during the course of the 1930s. The OAA benefits were generally paid on a budget-deficit principle, in which OAA payments were designed to make up the difference between the recipient's resources and a target budget for a level of living. In one sense the maximum benefit can be seen as an indication of what the state thought that target would be, as someone with no resources would receive no more than the maximum, while someone with resources equal to half of the maximum would receive up to half of the maximum in OAA benefits. This helps explain the broad difference in benefits paid to individual recipients within states seen in Figure 1 and Table 15. This target budget in some ways is a better measure to use than per capita payments or payments per recipient

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[22] In some cases, the benefits were reported for the county in which the city was located. The per capita benefits involve dividing county benefits by county populations in the relevant situations.

[23] We have also estimated the model in a setting where we calculated mortality rates using the 1930 elderly population to reduce any correlation caused by the elderly population being in the denominator of benefits per capita and the elderly death rate in the specification reported in the text. The results are essentially the same under that specification as well.

because health and mortality do not respond specifically to the cash payments paid by the state. Instead, the mortality is influenced more by the total income the person has available, whether the source is the state's OAA payment or the person's private income.

During the 1930s, New Deal administrators produced a series of studies with simple comparisons of data to show the success of their policies. An Old Age Assistance administrator who looked at the simple OLS results with mortality rates of the elderly as a function of access to OAA benefits would likely have declared success. The elderly who were old enough to be eligible for OAA experienced a drop in their mortality in areas where OAA was present, distributed more money, and provided higher benefits. In the first specification in Table 16, the presence of OAA law in a city was associated with mortality rates that were 0.1133 deaths per hundred lower for people aged 65 and over and 0.3429 lower for people aged 75 and over. The presence of benefits lowered the death rates by 0.068 standard deviations, and 0.118 standard deviations, respectively. From the second specification, an increase in OAA benefits per capita of $10 was associated with death rates that were 0.01 lower for the 65 and over age group and 0.016 lower for the 75 and over age group. According to the third specification, a one-standard deviation increase in maximum OAA benefits per month was associated with 0.074 standard deviation reduction in death rates for the 65 and over age group and a 0.153 standard deviation reduction for the 75 and over group. The coefficients are in general statistically significant at the 5 percent level for the specification with the age group 75 and over but not for the one with age group 65 and over.[24]

[24]When we estimate the models with mortality rates for the 65-74 age group, the results are very similar to those reported for the 65 and over age group.

Table 16. Results for Specifications With Mortality Rates as Dependent Variable

Type of regression, other covariates:		Dependent variable: Mortality rate age 55-64	Mortality rate age 65 and over	Mortality rate age 75 and over
Specification with OAA dummy for the presence of the law				
OLS	OAA	-.2556***	-.1133	-.3429**
		(.0656)	(.0886)	(.1621)
OLS	OAA	-.1607***	-.2360*	-.2827
Proxy Income +		(.0476)	(.1309)	(.1931)
Other Programs +	Other	-.0016	.0002	-.0009
The Other Covariates	programs	(.0013)	(.0025)	(.0045)
Fixed Effects	OAA	.0268	.0614	-.0351
Proxy Income +		(.0255)	(.0690)	(.1210)
Other Programs +	Other	.0002	.0041**	.0046
The Other Covariates	programs	(.0001)	(.0020)	(.0042)
Specification with OAA benefits per capita (persons 65 years of age and older)				
OLS	OAA	-.0025***	-.001	-.0016
		(.0005)	(.0008)	(.0015)
OLS	OAA	-.0028***	-.0023**	-.0013
Proxy Income +		(.0006)	(.0011)	(.0022)
Other Programs +	Other	-.0005	.0003	-.0025
The Other Covariates	programs	(.0012)	(.0022)	(.0038)
Fixed Effects	OAA	.0012***	.0035***	.0057***
Proxy Income +		(.0003)	(.0007)	(.0014)
Other Programs +	Other	-.0005	.0020	.0013
The Other Covariates	programs	(.0009)	(.0020)	(.0042)
Specification with OAA maximum monthly benefit				
OLS	OAA	-.0073***	-.0037	-.0133***
		(.0021)	(.0027)	(.0050)
OLS	OAA	-.0053***	-.0088**	-.0126**
Proxy Income +		(.0016)	(.004)	(.0058)
Other Programs +	Other	-.0015	.0007	.0004
The Other Covariates	programs	(.0013)	(.0025)	(.0046)
Fixed Effects	OAA	.001	.0021	-.0054
Proxy Income +		(.0008)	(.0025)	(.0038)
Other Programs +	Other	.0001	.0044**	.0051
The Other Covariates	programs	(.0009)	(.002)	(.0042)

Sources: See Appendix A.
*** Statistically significant at 1 percent in two-tailed t-test.
** Statistically significant at 5 percent in two-tailed t-test.
* Statistically significant at 10 percent in two-tailed t-test.

Notes for Table 16:

Robust standard errors are reported in parentheses with observations clustered at the city level.

Number of observations: 734

"Proxy Income" contains two variables: retail sales and number of income tax returns as a percentage of population.

All specifications with mortality rate age 65 and over (or difference between mortality rate age 65 and over and mortality rate age 55-64) as left hand side variable contain also an interaction term between the OAA variable and a dummy if the minimum age was 70 instead of 65.

"The other covariates" contains a group of variables: number of beds in hospital per capita, cost of living, total population, percentage of population black, percentage of population illiterate, percentage of population foreign born.

F-stats for the excluded instruments in the first stage regression of the 2SLS specification are reported in the first column and correspond to OAA variable, the interaction term between OAA and minimum age and other programs respectively.

For Hansen statistic the p-value is reported.

The OAA administrator could take heart that the negative relationship between OAA and mortality rates remains when the correlates for economic activity, relief spending per capita on other programs, health access, cost of living, and demographics are added. The full results with the added correlates are shown in Table 17. Areas with more hospital beds per capita had higher mortality rates. The positive effect might have been a result of the reporting of deaths by location of death rather than by residence, as the elderly with severe cases from outside the city were much less likely to find hospitals where they lived and thus ended up dying in a city hospital. Mortality rates were elevated in cities with higher cost of living, more illiteracy, fewer foreign-born, and lower rates of economic activity as measured by retail sales per capita. Once fixed year and city effects are added to the analysis, however, these effects are sharply diminished.

Table 17. Detailed results for the specification with "Mortality rate age 65 and over" as a dependent variable

Covariate:	Specification with: OAA Law		OAA Payments		OAA Maximum Benefit	
	without FE	with FE	without FE	with FE	without FE	with FE
OAA	-.2360*	.0614	-.0023**	.0035***	-.0088**	.0021
	(.1309)	(.0690)	(.0011)	(.0007)	(.004)	(.0025)
OAA * Minimum	.0597	.0374	-.0004	.0011	.0019	-.0013
Age	(.1144)	(.0602)	(.0017)	(.0010)	(.0032)	(.002)
Other Programs	.0002	.0041**	.0003	.0020	.0007	.0044**
	(.0025)	(.0020)	(.0022)	(.0020)	(.0025)	(.002)
Retail Sales	-.5733	-.0679	-.5476	-.2428	-.542	-.0374
	(.5335)	(.4096)	(.5541)	(.4031)	(.5389)	(.4119)
Percentage Tax	.0672	.0443	.0722	.0297	.0679	.0486
Returns	(.0447)	(.0329)	(.0475)	(.0325)	(.045)	(.033)
Beds per 1000	.1606***	.0300	.1549***	.0443*	.1599***	.0343
Persons	(.0524)	(.0273)	(.0530)	(.0264)	(.0514)	(.0273)
Cost of Living	2.1377***	-.8948	2.27***	-1.3103	2.0758***	-.95045
	(.7926)	(1.0167)	(.798)	(1.0007)	(.7855)	(1.0197)
Total Population	-.00003	-.0005	-.00003	-.0008	-.00004	-.0008
(1000s)	(.00005)	(.0008)	(.00006)	(.0008)	(.00006)	(.0008)
Percentage Black	-.0105	-.1469*	-.0121	-.1372*	-.011	-.1441*
	(.0118)	(.0781)	(.0121)	(.0765)	(.0118)	(.0783)
Percentage	.1655***	.0529	.1777***	.0959	.1711***	.0782
Illiterate	(.0393)	(.0856)	(.0408)	(.0851)	(.0405)	(.0859)
Percentage	-.0300**	-.0569	-.0332**	-.0169	-.0286**	-.0419
Foreign	(.0132)	(.0357)	(.0134)	(.0360)	(.0132)	(.0365)
Year 1930	-	-.4590***	-	-.4840***	-	-.4489***
Dummy		(.0978)		(.0959)		(.0981)
Year 1931	-	-.8609***	-	-.9323***	-	-.8364***
Dummy		(.1858)		(.1819)		(.1869)
Year 1932	-	-1.0655***	-	-1.1963***	-	-1.0444***
Dummy		(.2949)		(.2898)		(.2965)
Year 1933	-	-1.2757***	-	-1.39***	-	-1.2534***
Dummy		(.2903)		(.2849)		(.292)
Year 1934	-	-1.1583***	-	-1.1893***	-	-1.125***
Dummy		(.2685)		(.2605)		(.2696)
Year 1935	-	-1.2531***	-	-1.292***	-	-1.2223***
Dummy		(.2537)		(.2452)		(.2546)
Year 1936	-	-.8714***	-	-.9208***	-	-.87***
Dummy		(.2514)		(.2421)		(.2537)
Year 1937	-	-1.0593***	-	-1.2247***	-	-1.0675***
Dummy		(.2437)		(.2356)		(.2474)
Year 1938	-	-1.5663***	-	-1.7998***	-	-1.5773***
Dummy		(.2714)		(.265)		(.2747)
Constant	5.1845***	11.124***	5.005***	11.0579***	5.1932***	10.8828***
	(.6554)	(1.375)	(.6395)	(1.3468)	(.6445)	(1.3819)
R-square	0.23	0.29	0.23	0.32	0.24	0.29

Sources: See Appendix A.

*** Statistically significant at 1 percent in two-tailed t-test.

** Statistically significant at 5 percent in two-tailed t-test.

* Statistically significant at 10 percent in two-tailed t-test.

Notes: See Table 16.

But these controls still leave several other important features that might have influenced death rates uncontrolled. Some are not easily measured but were largely time-invariant while varying across cities during the 1930s. They include the basic quality of water treatment and sanitation facilities for the city, public health practices, private customs related to hand-washing and other personal sanitation activities, typical diets, and regional variations in the practice of medicine. In addition, there were national shocks to the economy that were common across cities. New drugs that successfully treated infections and other illnesses tended to diffuse rapidly across the country in a short period of time. The key change in drugs during the 1930s was the development of sulfa drugs, which Thomasson and Treber (2006) find contributed to a reduction in maternal mortality.

Once we move to a difference-in-difference analysis by incorporating city and year fixed effects, the coefficient on the OAA measures for those aged 65 and over turns positive in all cases, while the coefficient in the 75 and over regressions remains negative for two of the three measures. None of the coefficients are statistically significant and negative for either age group. Most of the coefficients change very little when we include a mortality rate time trend calculated for each city based on mortality rates between 1922 and 1928.[25]

It is also important to examine the effects of the remaining welfare programs on elderly mortality. During the early 1930s the elderly were receiving assistance through the general state and local programs. A Census of relief families taken in October 1933 shows that roughly 7 percent of the population aged 65 and over were in households receiving relief from the Federal Emergency Relief Administration (FERA 1934, 29). Studies by the

[25] When we add the trend, the number of observations is reduced from 734 to 590 since a number of states had not yet joined the death registration area and begun reporting mortality data consistently during the 1920s. The results are available from the authors.

Social Security Administration published in 1938, summarized in Tables 13 and 14 show that of the 20.3 percent of the elderly population who were dependent on some form of external aid, roughly 10 percent were receiving it from outside the state-federal matching grant system. Roughly 39 percent of the new recipients of state-federal OAA in fiscal year 1937-1938 had received other forms of government aid within two years prior to receiving OAA, and around 30 percent within the prior 30 days. In addition, the elderly who were living with families also benefited from relief payments to those families.

Thus, to the extent that the elderly received benefits from the broader welfare programs, we might expect that more spending per capita would also contribute to lower mortality among the elderly. The results show little sign that the broader welfare programs were associated with reduced elderly mortality. As is the case for the impact of OAA, the coefficients in Table 16 for spending per capita on other relief programs are in most cases non-negative and not statistically significant.

An additional step can be taken to further control for unmeasured factors that might have varied both across cities and across time but were common determinants of death rates for people aged 55 and over within the same city and year. Such factors might include epidemics and changes in access to sanitation and treated water. In this case we use as the dependent variable the difference between the mortality rates of the elderly who were eligible for the Old Age Assistance benefits (ages 65 and up and, respectively, ages 75 and up) and the mortality rates of the group aged 55 to 64. The estimate for the group aged 75 and older allows us to eliminate any problems associated with the fact that some states chose minimum ages of 70 and reduces the likelihood that the person would have been married to someone in the 55-64 age group.

The results for the three measures of the OAA program are presented in Table 18. The coefficients for the difference-in-difference-in-difference specifications in the third row for each measure of OAA are positive in every case for the differences between the group aged 65 and over and the control group aged 55-64. They are positive for the differences in mortality rates for people aged 75 and over and the control group aged 55-64 for the specification with benefits per capita and negative but statistically insignificant for the case when OAA is measured by the presence of the law. The one case of a negative and statistically significant coefficient is for the OAA maximum benefit measure; a one standard deviation increase in the monthly maximum benefit was associated with a 0.079 standard deviation reduction in death rates for people aged 75 and over.

Although we have controlled for a significant range of factors with these techniques, there still remains the possibility of bias in the coefficients on the OAA measures. There may be a form of endogeneity bias to the extent that legislators and relief administrators responded to a rise in the *relative* mortality of the elderly by adopting OAA earlier or raising the benefits and spending on OAA when it was already in place. The key omitted variable that we are missing is the poverty status of the 65 and over group *relative to the rest of the population*.[26] Changes in poverty over time within a city for the 65 and older group relative to the 55-64 age group were likely to lead to a larger positive gap in mortality rates between the two age groups. Similarly, increases in relative poverty for the 65 and older group across time within a city would likely lead to enhancements in the availability and level of OAA. The combination of these two positive correlations would cause the OAA coefficients in the

[26] We have controlled for the income distribution to some degree by including the share of population paying federal income taxes, but this describes the share of income for the richest segment of society and cannot fully control for differences in poverty levels in the lower tier of the distribution.

Table 18. Results for Specifications With Difference in Mortality Rates as Dependent Variable

Type of regression, other covariates:		Dependent variable: Mortality rate age 65 and over minus Mortality rate age 55-64	Mortality rate age 75 and over minus Mortality rate age 55-64
Specification with OAA dummy for the presence of the law			
OLS	OAA	.1193*	-.0874
		(.0687)	(.1571)
OLS	OAA	-.0191	-.122
Proxy Income +		(.1051)	(.1749)
Other programs +	Other	.0016	.0007
The other covariates	programs	(.0021)	(.0043)
Fixed Effects	OAA	.0427	-.0619
Proxy Income +		(.0635)	(.1166)
Other programs +	Other	.0039**	.0045
The other covariates	programs	(.0018)	(.0040)
2SLS	OAA	1.1568**	1.9283
Proxy Income + Other programs +		(.5696)	(1.2062)
The other covariates	Other	-.0156	-.0302
	programs	(.0109)	(.0231)
F-stat: OAA 52.55, OAA-over 70 17.20,			
Other programs 73.13	Hansen	0.63	0.03
2SLS	OAA	.2664	.2042
Proxy Income + Other programs +		(.8437)	(.4728)
The other covariates +	Other	.0243	.0132
City and year dummies	programs	(.0209)	(.0332)
F-stat: OAA 13.10, OAA-over 70 24.48,			
Other programs 2.68	Hansen	0.01	0.02
Specification with OAA benefits per capita (persons 65 years of age and older)			
OLS	OAA	.0014**	.0009
		(.0007)	(.0014)
OLS	OAA	.0005	.0015
Proxy Income +		(.001)	(.0021)
Other programs +	Other	.0009	-.0019
The other covariates	programs	(.0017)	(.0033)
FE	OAA	.0023***	.0045***
Proxy Income +		(.0006)	(.0014)
Other programs +	Other	.0027	.0017
The other covariates	programs	(.0018)	(.0041)
2SLS	OAA	.0075**	.01461***
Proxy Income + Other programs		(.0034)	(.0042)
The other covariates	Other	-.0138	-.0213***
	programs	(.009)	(.0083)
F-stat: OAA 61.23, OAA-over 70 17.25,			
Other programs 73.13	Hansen	0.58	0.61

2SLS	OAA	.0127**	.0271**
Proxy Income + Other programs +		(.0056)	(.0122)
The other covariates	Other	.0098	-.0111
City and year dummies	programs	(.0183)	(.034)
F-stat: OAA 2.80, OAA-over 70 21.09,			
Other programs 2.68	Hansen	0.63	0.75
	Specification with OAA maximum monthly benefit		
OLS	OAA	.0032	-.006
		(.0022)	(.0049)
OLS	OAA	-.0004	-.0073
Proxy Income +		(.0035)	(.0055)
Other programs +	Other	.0016	.0019
The other covariates	programs	(.0021)	(.0045)
Fixed Effects	OAA	.0011	-.0064*
Proxy Income +		(.0023)	(.0037)
Other programs +		.0042**	.0049
The other covariates	Other programs	(.0018)	(.004)
2SLS	OAA	.0381**	.0237
Proxy Income + Other programs +		(.0188)	(.0303)
The other covariates	Other	-.0178	-.0098
	programs	(.0118)	(.0199)
F-stat: OAA 62.85, OAA-over 70 24.17,			
Other programs 73.13	Hansen	0.50	0.00
2SLS	OAA	-.0783	.0033
Proxy Income + Other programs +		(.099)	(.017)
The other covariates +	Other	.0609	.0178
City and year dummies	programs	(.0552)	(.0338)
F-stat: OAA 9.85, OAA-over 70 24.68,			
Other programs 2.68	Hansen	0.18	0.01

Sources: See Appendix A.
*** Statistically significant at 1 percent in two-tailed t-test.
** Statistically significant at 5 percent in two-tailed t-test.
* Statistically significant at 10 percent in two-tailed t-test.
Notes: See Table 16.

difference-in-difference-in-difference analysis to be more positive than the true causal effect. Since many of the remaining relief programs often acted as substitutes for Old Age

Assistance, particularly before it had been introduced, there also remains the possibility that there is a positive bias in the per capita spending on the remaining programs.

We work to eliminate this bias using an instrumental variable approach. Fishback, Haines, and Kantor (2007) used a similar approach in their focus on the impact of all relief programs on infant mortality and mortality from different causes. Instruments for OAA programs need to be state-specific. OAA was state legislation prior to the Social Security Act of 1935. Even after the act, the states determined the timing of the introduction because they could not start the state-federal version of the program until they had adopted enabling legislation that met the Social Security standards. The states also were the primary group determining benefits because the federal payments were designed as matches for what the state paid. On the other hand, the instruments for the spending per capita on other relief programs might still have been a combination of factors influencing the state and federal government decisions.

The key instrument that we believe will have strength in the OAA assistance first-stage is a measure of whether the governor is a Democrat. In general, states with Democratic governors were more likely to adopt social insurance programs than states with Republican governors. Since the rules changed for Old Age Assistance with the adoption of the Social Security Act of 1935, we also add an interaction between a dummy for the years from 1936 on and the governor is a Democrat. For the instrument to be successful the presence of a Democratic governor cannot be correlated with the time-varying error in the final-stage equation with the difference in mortality rates between ages 65 and over and 55-64 as the dependent variable.

Given the wide range of issues determining the probability that a Democrat would win enough votes to win the election and the fact that the governor then stayed in office two to four years, the likelihood that the relative mortality rates of the 65 and over group was a significant determinant of the state electoral outcome seems remote. Nor would we expect that the relative poverty of the 65 and over to the 55-64 group, the unobservable we believe to be most correlated with the mortality difference, to play a decisive role in such elections except through the adoption of OAA. The poor, in general, wield very little political clout because they tend not to vote.[27] The far more pressing issues related to general economic troubles for the entire population, not the specific troubles for one age group. In addition, the dependent variable is centered on the city, while the governor is elected at the state level. Rural issues often wielded outsized importance in state elections, making the relative plight of the elderly in the cities even less likely to have much effect on the election of the governor.

We used an additional instrument for the adoption of an OAA law based on the long-term tendency of the states to follow general patterns of policy innovations. The wave of adoption of Old Age Assistance law toward the end of the 1920s and during the 1930s followed on earlier waves of legislation at the state level. These included the initial establishment of state bureaus of labor statistics in the 1870s and 1880s, the introduction of factory inspectors in the 1880s-1890s, and the broad range of Progressive Era regulations, including workers' compensation and mothers' pensions, of the 1910s. Across states, there are differences in the willingness to try new programs that follows a consistent pattern over

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[27]Some might claim that once the poor receive benefits that they will have more incentive to vote. The poor have always received some form of benefits, locally prior to 1933 and state/federal afterward, yet their relative propensity to vote does not seem to have been affected by the receipt of benefits.

time, with some states constantly being at the forefront of changing the legislation whereas others tended to be followers.[28] Explorations of these long-term relationships show that the relative timing of the state's adoption of workers' compensation two decades earlier was strongly related to the timing of the adoption of OAA at the state level in the late 1920s and 1930s.

Workers' compensation was the dominant social insurance reform of the 1910s, just as OAA was a major social insurance reform of the 1930s. We use the time-varying adoption of workers' compensation as an instrument for how soon we expect to see the adoption of Old Age Assistance. The instrument is a dummy variable that has value one if twenty years before the year for which the observation is made, the state had a workers' compensation law adopted, and zero otherwise. Given the long time lag between the political economic variables and the original adoption of workers' compensation, it is highly unlikely that the instrument would be correlated with the remaining time-varying error in the final stage mortality difference equation.

When developing instruments for the per capita spending by the remaining relief programs, we need to pay attention to factors that would influence both the federal and state government. To represent state interests, the presence of a Democratic governor will have impact. First, as above, Democratic governors, controlling for state effects, tended to press more for in-state reform. Further, in lobbying for federal relief funds, governors from Roosevelt's party were likely to have better access to funds.

[28] Policy diffusion is generally explained in the political science literature by being either a result of political, social, or economic variables specific to the state or as a result of proximity to states that had already adopted the legislation in case. See Walker (1969) and Gray (1973).

In addition, we have included measures of the influence of politicians in Congress. Anderson and Tollison (1991) argued that powerful Congressmen were likely to wield influence since ultimately Congress controlled the purse strings. Fishback, Kantor, and Wallis (2003, 299 note 21) find that members of the Labor committee in the House of Representatives, in particular, influenced the distribution of federal relief monies across counties. The Labor committee was the primary committee that was devoted to unemployment issues during the New Deal.

There might be a problem if these measures of political activity are correlated with the unmeasured relative poverty of the elderly that may be contributing to endogeneity. The political measures, however, are unlikely to be correlated with unmeasured poverty for the following reasons. First, as we already argued, the poor tend not to have much political clout in elections either because they do not choose to vote or are implicitly or explicitly disfranchised (see Kousser 1974, Lijphart 1997). Thus, unmeasured income shocks to the poor were likely well down the list of the major issues that were decisive in elections. Second, we chose instruments with temporal, spatial, or institutional distance from the city for each city-year. We have already discussed how the gubernatorial instrument focuses on the state's political climate and how the workers' compensation law instrument avoids the temporal simultaneity bias. House committee representation has institutional distance due to the House committee assignment process. Since we cannot be sure that the unmeasured factors are not correlated with the instruments, we allay concerns further by performing standard over-identification tests.[29] With some exceptions, the instruments appear to have sufficient strength to offer meaningful IV results. As shown in Table 19, the coefficients of the instruments generally have the predicted positive signs in the first-stage relief regressions and are statistically significant at the 10-percent level. When coefficients are negative they are not statistically significant. For the specifications with difference between mortality rates for groups 65 and up and 55-64 we instrumented also for the interaction term between Old Age Assistance and the minimum age legislated by the state. The strengths of the instruments as a group are shown by the F-tests for the group of instruments and are reported in the fifth row of each specification in Table 18. The instruments in the OAA first-stage equations have the most strength when the presence of the law or maximum benefits are the measures of OAA, as the F-statistics for the first-stage equations are above 9 in those settings. There may be a weak instrument problem for the OAA per capita spending measure and for the relief spending per capita on other programs because the F-statistic is typically around 3. The instruments have strength for the interaction between OAA measure and the minimum age as F-statistics are above 17.

Using a Hansen J-statistic test, we could not reject the hypothesis that the group of identifying instruments were uncorrelated with the 2SLS estimates of the error term in the second-stage mortality difference equations for all but one of the equations for the 65 and over age group and for the two equations for the 75 and over group using OAA benefits per capita as the OAA measure. On the other hand, we can not draw strong conclusions for the estimates for the 75 and over mortality differences for the OAA law dummy and the

[29] When the OAA Law Dummy is used as the measure of Old Age Assistance we used a linear probability model (LPM) specification in the first stage. The LPM.specification is a reasonable specification as long as the predictions from the model lie between 0 and 1. See Horrace and Oaxaca (2006).

Table 19. First Stage Regression Results for the Difference-in-Difference Analysis

Specification, other covariates	Instruments	Endogenous variable: OAA specification with: OAA law dummy	OAA paid benefits	OAA max. benefits	OAA * min. age specification with: OAA law dummy	OAA paid benefits	OAA max. benefits	Other programs
	House Committee Labor	.1133***	-1.9165	4.9987***	.1621***	5.8357**	6.2418***	10.1273***
2SLS		(.0326)	(2.8876)	(1.1307)	(.0482)	(2.3253)	(1.6)	(1.702)
Proxy	If Governor is Democrat	.1368***	1.7455	5.222***	.1251***	6.7718***	6.4111***	2.1417
Income +		(.0438)	(2.598)	(1.3161)	(.044)	(1.3285)	(1.4063)	(1.4756)
Other	If Governor is Democrat	.2394***	47.6387***	7.2016***	-.2108***	-3.5756	-9.0245***	15.3541***
programs +	*After 1936 Dummy	(.0388)	(4.6119)	(1.2553)	(.0462)	(2.2161)	(1.6062)	(1.8095)
The other	Worker Compensation Law	.2891***	21.536***	10.0316***	.1971***	6.3269***	6.3541***	15.9262***
covariates	Dummy 20 Years Before	(.0452)	(2.8849)	(1.3363)	(.0461)	(1.7022)	(1.4347)	(1.6165)
	Partial R-square of excluded instruments	0.22	0.29	0.25	0.08	0.05	0.11	0.30
2SLS	House Committee Labor	.0511	-4.86	2.7611**	.143***	4.6783*	5.008***	1.3048
Proxy		(.0446)	(3.6671)	(1.383)	(.0506)	(2.473)	(1.6808)	(1.1833)
Income +	If Governor is Democrat	.1825***	-2.4	4.5586***	.1779***	8.4485***	5.478***	2.7176**
Other		(.0442)	(3.6133)	(1.4547)	(.0515)	(2.0988)	(1.608)	(1.1617)
programs +	If Governor is Democrat	.1872***	-8.3404	5.4689***	.4971***	13.8534***	14.8384***	-3.9727
The other	*After 1936 Dummy	(.0698)	(7.4875)	(2.0213)	(.0833)	(3.1171)	(2.6352)	(2.6281)
covariates +	Worker Compensation Law	.1056**	10.767*	3.3254**	.2094***	13.3446***	7.7973***	1.4056
City and year	Dummy 20 Years Before	(.0486)	(4.4197)	(1.509)	(.0529)	(2.3602)	(1.6778)	(1.2096)
dummies	Partial R-square of excluded instruments	0.08	0.02	0.06	0.15	0.13	0.14	0.02

Sources: See Appendix A.
*** Statistically significant at 1 percent in two-tailed t-test.
** Statistically significant at 5 percent in two-tailed t-test.
* Statistically significant at 10 percent in two-tailed t-test.
Notes: See Table 16.

maximum benefits measures because the Hansen tests reject the hypothesis that the identifying instruments are uncorrelated with the error term in the final stage equation for the other two measures of OAA.[30]

The use of instrumental variables in the 2SLS equations in Table 18 does not change the conclusions that the OAA assistance program did not contribute to reduced mortality rates among the elderly. As can be seen in the bottom two rows for each measure of OAA, the OAA coefficients are positive in all but one case, and some of those are statistically significant in two-tailed tests. The one negative coefficient is not statistically significant.

The simple regressions of the level of elderly mortality rates in Table 16 suggest a negative relationship between OAA and elderly mortality. However, once we control further for potential omitted-variable and endogeneity biases using difference-in-difference, difference-in-difference-in-difference analysis, and instrumental variable analysis, we no longer find a negative impact of OAA on elderly mortality. In contrast, Balan Cohen's (2006, Tables 2 and 3) independent analysis of a panel of state aggregates for 1934 through 1955 shows that OAA served to reduce mortality rates for the elderly.

There are some obvious differences between Balan Cohen's analysis and our analysis. In contrast with Balan Cohen, our goal is to examine how the presence of a means-tested program would have helped reduce mortality by itself with no OASI-style federal pension

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[30] We have explored using several other instruments that have been proposed in other settings. These include voter turnout in the prior presidential election, the percentage voting for the Democratic presidential candidate in prior elections (both based on Fleck 1999), the percentage of Democratic legislators in the upper and lower houses of the state legislature, representation on other congressional committees, and the timing of mayoral and gubernatorial elections (based on Levitt 1997), the standard deviation of the percent voting Democrat over the period from 1896 through the prior election (based on ICSPR file number 0001). These additional instruments failed to meet one or more of our criteria for inclusion. In many cases when we added the extra instrument, its coefficient in the first stage was statistically insignificant or had an unexpected sign, and the estimates of the relief effects were roughly comparable to the results reported here.

program in place. Our sense is that the payments from the nationwide program likely influenced decisions made by the elderly and their relatives and friends in ways that are not easily measured. Therefore, we focused on the period prior to 1940 when no one was receiving OASI pension payments to avoid these confounding effects. We are also interested in the effectiveness of the state run systems operated in the early 1930s before the federal matching grants were initiated. As a result, nearly all of Balan Cohen's analysis focuses on the period from 1935 into the 1950s after the federal matching grants were in place, while our central focus is on the period from 1929 through 1938.

There is still overlap between the time periods in our analysis and in Balan Cohen's analysis; therefore, we sought to examine other reasons for the difference in finding. Our data set centers on large cities; whereas Balan Cohen (2006) uses state level data that combines information for large cities, smaller towns, and rural areas. In the absence of Old Age Assistance, it is possible that access to public assistance might have been greater in large cities than in other areas. The BLS Survey of Almshouses in 1925 showed that the larger alms houses provided better housing and medical care in the cities than the typically smaller almshouses in rural areas (Stewart 1925). As a result, OAA laws in the 1930s that made OAA mandatory might have had stronger effects on mortality rates in areas outside the cities we studied. We estimated difference-in-difference and difference-in-difference-in-difference analyses for the specification with the presence of the OAA law using state level mortality data and also using mortality rates for the portions of each state outside the cities that we studied. Table 20 shows the results of these analyses using our city data, the state aggregates, and the state aggregates outside our city. In the difference-in-difference-in difference analysis at the bottom of the Table, the coefficients for the state aggregates and the states

Table 20. Comparison of results between state and city level analysis for the specification with the presence of the OAA law

OLS	Mortality rate age 65 and over minus Mortality rate age 55-64	Mortality rate age 75 and over minus Mortality rate age 55-64
City data	.1812**	.1144
	(.077)	(.1605)
State-cities data	.0674	.1126
	(.0596)	(.1032)
State data	.0997*	.1422
	(.0517)	(.0889)

Fixed Effects	Mortality rate age 65 and over minus Mortality rate age 55-64	Mortality rate age 75 and over minus Mortality rate age 55-64
City data	.0472	-.0184
	(.0517)	(.1135)
State-cities data	-.0413	-.0997
	(.0532)	(.0952)
State data	-.0363	-.1081
	(.0471)	(.0846)

Sources: See Appendix A.
*** Statistically significant at 1 percent in two-tailed t-test.
** Statistically significant at 5 percent in two-tailed t-test.
* Statistically significant at 10 percent in two-tailed t-test.
Notes:
The analysis in all cases is done for the 1930-1938 sample.
Standard errors are reported in parenthesis (for the city level analysis standard errors are robust and clustered at the city level).
For the state level analysis the number of observations is 270 whereas for the city level analysis there are 675 observations.

outside the cities are more negative than for the city sample. However, the standard errors are large enough that we cannot reject the hypothesis of no effect.

Our sense is that the differences arise because of changes in the nature of the OAA programs and medical care between the 1930s and the later years. The negative effect that Balan Cohen (2006) finds for the overall mortality regressions for the period 1934-1955 in Tables 2 and 3 comes from combining two periods that may have had different structures. We consider two possible factors that might account for the greater success of OAA in reducing mortality after 1938 than before that year: changes in the nature of medical care and a rise in generosity of the programs combined with a nonlinear effect of OAA.

The impact of better quality of medical care might well have been a significant factor. Even as late as the 1930s medical care in hospitals was rudimentary relative to the advances that followed. Melissa Thomasson and Jaret Treber (2006), for example, find that maternal mortality was actually slightly higher in areas with hospitals until sulfa drugs were introduced in the mid-1930s. Differences in medical care also fit Balan Cohen's (2006) regression findings. In Table 7 Balan Cohen examines the effects of OAA on infectious disease mortality rates before and after the development of antibiotics. The coefficients for OAA are all positive and statistically insignificant for the period 1934-1943. For the period 1944 to 1955 the coefficients are all negative and statistically significant and larger in magnitude than in her examination of overall mortality for the period 1934 to 1955. Thus, means-tested aid to the elderly might well have had relatively little impact on elderly mortality until after the development of penicillin, other antibiotics, and additional medical techniques.

A second possible difference might arise if OAA did not have much of an effect on death rates until it raised an elderly person's income beyond a threshold level. We examined this issue in two ways. First, we compared the OAA monthly maximums in 1934, 1938 and 1949 after adjusting for inflation with the CPI (1967=1). We focus on monthly maximum payments because they appeared to be a target level of monthly income that states sought to help the elderly try to reach. By 1949 several states, including Colorado, Connecticut, and Oklahoma no longer had maximum payments and they were making average payments per recipient ranging from $73 to $94 in 1967 dollars. California had the highest binding maximum at $105. That inflation-adjusted maximum was not too much different from those in 1938 and 1934 in our sample of cities. Colorado's maximum in 1938 was $106.6 and Massachusetts, New York, Kansas, and Louisiana had maximums of $95. In 1935 New York and Massachusetts had maximums of around $97. The difference in the distribution of inflation-adjusted OAA maximums comes more in the bottom tail, as the lowest payments in OAA states are much lower in the 1930s than in the 1940s.[31]

As a second check of the threshold effect, we have re-estimated the analysis where we use a dummy variable that takes a value of 1 when the monthly maximum is $40 and zero when it is below $40 in contemporary dollars. The results which are presented in Table 21 display a pattern similar to that found in the bottom third of Table 18 for OAA maximum benefits. The coefficient for the 75 and over difference with fixed effects is negative and statistically significant. However, when we move to the triple-difference, the coefficients are negative but not statistically significant.

Based on the similarities in the upper portion of the distributions of benefits and these regression results, it does not appear that the difference in results between our work here and Balan Cohen's findings for the 1940s and 1950s are being driven by nonlinear

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[31]The values were calculated from information in Berman (1947, 1949), Berman and Haskell (1945), and the information from the data set. The deflator is from U.S. Bureau of the Census (1975, series E-135, pp. 210-11.

Table 21. Re-estimation of the specification with maximum monthly benefit as a measure of OAA program

Type of regression, other covariates:		Dependent variable: Mortality rate age 65 and over minus Mortality rate age 55-64	Dependent variable: Mortality rate age 75 and over minus Mortality rate age 55-64
OLS	OAA	-.037307	-.3029836
		(.0927327)	(.2449011)
OLS	OAA	-.0121646	-.0506881
Proxy Income +		(.0996589)	(.2458096)
Other programs +	Other programs	.001404	-.0003515
The other covariates		(.0020525)	(.0044855)
Fixed Effects	OAA	-.089311	-.4035833**
Proxy Income +		(.0825044)	(.1815742)
Other programs +		.0035859*	.0049687
The other covariates	Other programs	(.0018323)	(.0040042)
2SLS	OAA	.571282	-.3095859
Proxy Income + Other programs +		(.9958604)	(.6433713)
The other covariates	Other programs	-.0001546	.0078901
		(.0086748)	(.0075848)
F-stat: OAA 30.17, OAA-over 70 26.70,			
Other programs 73.13	Hansen	0.0038	0.0023
2SLS	OAA	-1.454593	-.1324948
Proxy Income + Other programs +		(2.696424)	(1.079745)
The other covariates +	Other programs	-.142008	.0213619
City and year dummies		(.2240351)	(.0277451)
F-stat: OAA 6.32, OAA-over 70			
12.68, Other programs 2.68	Hansen	0.8443	0.0158

Sources: See Appendix A.

*** Statistically significant at 1 percent in two-tailed t-test.

** Statistically significant at 5 percent in two-tailed t-test.

* Statistically significant at 10 percent in two-tailed t-test.

Notes for Table 21:

Maximum monthly benefit is a dummy variable with value equal to 1 if the maximum monthly benefit is $40 or there is no maximum and 0 if it is below.

Standard errors are reported in parenthesis; they are robust and clustered at the city level.

threshold effects. Our sense is that the differences are being driven by improved quality of medical care that could be purchased with the OAA benefits.

3.6. Conclusion

Simple correlations suggest that the means-tested OAA programs introduced by the states in the 1920s and 1930s were associated with lower mortality rates for the elderly. Given the emphasis on reporting the equivalent of simple correlations by public agencies at the time, OAA assistance could easily have been portrayed as a success in its early years. However, such a heart-warming story does not survive more rigorous examination of the data for cities during the period 1929 through 1938. Once the sample is analyzed controlling for city and year fixed effects, there is no statistically significant negative relationship between OAA and elderly mortality. A negative relationship is still absent after we perform additional analysis that builds on the fixed effects using differences in mortality between groups over and under 65 and then using instrumental variables.

We have several ideas about why OAA did not contribute to reductions in elderly mortality in the 1930s. The absence of a reduction in mortality associated with OAA may hide several conflicting interactions created by the new programs. Absence of a formal program for means-tested OAA in a state in the 1930s did not imply that the impoverished elderly received no support at that time. Instead, it implied that the elderly were just one of many groups receiving benefits from the existing public assistance programs. The shift to OAA programs might not have led to much of a change in per capita benefits available for the elderly. It is not clear that any more elderly were eligible under the new program who had not been eligible for the various forms of aid under the old program In fact, the states were very careful in the way they defined eligibility. Many required that the recipients of benefits had no family members available to support them. Further, the elderly with low incomes who owned houses were required by most states to sign over ownership of the home or sign a lien on the home to become eligible for benefits. In essence, the OAA served as a loan from the government against the value of the home when the recipient died. The programs benefited the elderly in that they were receiving a government subsidized loan that might not have been available in private credit markets. Moreover, the positive benefits might have been spread out to other age categories whose financial burden of taking care of the elderly was diminished.

The legislative acts typically claimed that a primary purpose of the law was to allow the elderly freedom to live on their own and not be lumped in with the rest of the poor in the almshouse programs. Mortality for the elderly was likely to fall if the elderly were able to move away from almshouses with low quality sanitary conditions and where the spread of disease was more likely. On the other hand, when OAA freed the elderly poor to live alone, they may have lost a feeling of connectedness to others in the community. Snyder and Evans (2006) found social isolation to be an important factor adversely affecting the mortality rates for the elderly in the modern era. If they lived in a poorer district, their access to sanitation and water treatment facilities might not have been as good, which might have led to an offsetting increase in the death rate.

We use similar analytical techniques to those used by Finkelstein and McKnight (2005) for Medicare in the 1960s and Balan Cohen (2006) for OAA in the 1940s and 1950s. When the stream of work is considered in total, it suggests the following time path for the impact of old-age programs. We find little effect of OAA on mortality in the 1930s when medical care was rudimentary, Balan Cohen finds that OAA benefits reduce mortality in the 1940s and 1950s when OAA payments could be used to make use of more effective medical techniques than those available in the 1930s. Although medical practice continued to improve in quality in the 1960s, Finkelstein and McKnight find no effect of the introduction of Medicare coverage of hospitalization and availability of subsidized health insurance for other procedures.[32] The lack of a Medicare effect on mortality may be a result of differences in the targeting of benefits. Medicare is a universal insurance program for the elderly, while OAA is a means tested program focused on the poor elderly.

Even if mortality did not fall with the introduction of Medicare, there were other benefits to the elderly population. Finkelstein and McKnight's (2005) find that people in the upper fourth of the medical spending distribution experienced a decline of forty percent in their out-of-pocket costs. There were also alternative benefits to OAA spending in the 1930s even if mortality was not reduced. Other studies of the state-federal versions of OAA suggest that in areas with better OAA benefits, elderly women were better able to live separately and a number of men were able to reduce their labor force participation.

[32] For a literature review on the impact of Medicare, see Levy, Meltzer (2001).

Chapter 4

PUBLIC MESSAGES AND BUBBLES

4.1. Introduction

There have been several accounts during history when assets were traded at ever increasing prices followed by a sudden drop in price. Some use the term "bubble" to identify such situations. They argue that prices rose higher than the fundamental values of assets being traded for some time period. However, in the real economy there is always uncertainty related to the fundamental value of the assets; therefore, it is hard to argue whether a bubble is indeed present or not. What is known is that when exuberant periods are followed by reverses in the value of certain classes of assets, economic growth is affected for some percentage of the time. Recent examples in U.S. might arguably include the dot com mania of the second half of the 1990s and the real estate market run up over the last several years.

Whereas exuberant periods are many times matched by a spur in innovation that can augment the long term prospects of an economy, there is a risk of a prolonged downturn. For example in Japan, after the real estate and stock market rises of the 1980s, the decade-long stagnation that followed in the 1990s presented challenges for policy makers. U.S. monetary policy over the last three decades was successful in reducing the amplitude of business cycles (especially the duration of recessions) but the "bubble" phenomenon might not have disappeared and arguably may have been exacerbated. The current lower bound of the estimates of the credit crisis losses that are linked to the real estate rise and fall is around $1 trillion. The idea that a central bank should not target asset prices and that the most it should do is to "clean up the mess after the party is over" is coming under increased scrutiny, especially when there is a non-negligible probability of a systemic risk and when the "clean up" process might involve direct or indirect governmental bailout of the banking/mortgage industry.

One way in which a central bank can affect inflated asset prices is to simply decrease the liquidity in the system. This is usually achieved through increases in benchmark interest rates. However, changes in interest rates might affect the real economy both by changing the value of assets and also by altering the general cost of borrowing for firms and consumers. Since this direct channel seems to be considered more important than the indirect one in terms of its effects, many macroeconomists have questioned the desirability of a policy that targets not only economic growth and inflation but asset prices as well. Some alternatives to changes in interest rates could be to adjust regulations of the markets for assets or to establish more communication between the central bank and the market. Beginning in the 1990s, the importance of communication between the US central bank and the markets for efficacy of monetary policy and for accountability purposes, started to be acknowledged. As a consequence, the Federal Open Market Committee's meetings are today more transparent than they used to be. The transparency of the central bank has usually been confined to its monetary policy affecting the inflation and economic growth targets. Policy makers have been reluctant to extend this openness to issues regarding asset prices. This paper focuses on the potential role of communication between the central bank and the market in the context of an asset market bubble.

In many markets information is not equally distributed among traders. Even when information is evenly distributed, different expectations in regard to the behavior of other traders can induce situations in which assets are traded at prices that are believed to be different from fundamental values. Abreu and Brunnermeier (2003) develop a theoretical model that explains departures of asset prices from fundamentals by a miscoordination between rational traders. The story resembles the Keynesian greater fool idea. In this setup anything that can work as a coordination device among rational traders might help bring the price of the asset back to its fundamental value. A public message related with the fundamental value of the asset being traded could play this role.

There are several aspects related to a public message in this context that need to be taken into account. It might be the case that the public information decreases welfare, especially when traders disregard their private information and rely mostly on the public information. Prices in the economy might end up reflecting the central bank's view of the economy and not have the signal power anymore that was argued by Hayek. If communication with the market is done on a regular basis, the central bank is looking more and more in the mirror by using in its analyses prices that it induced.[33] But even if we are to think of rare interventions, there is no guarantee that the central bank "knows better" than the market participants. Also, what are the prospects in terms of bringing the value of the assets back to fundamentals? For example, Alan Greenspan's mention of "irrational exuberance" in 1996[34] had only a short term impact on the capital market and the decrease in

[33] This point was made by Morris and Shin (2005).
[34] The speech was broadly about the mission of the central bank in a democratic society. This is the sentence that moved the markets: "But how do we know when *irrational exuberance* has unduly escalated asset values, which then become subject to unexpected and prolonged contractions as they have in Japan over the past decade?" In the same speech Greenspan acknowledged a role for the central bank in monitoring the asset prices: "[…]we should not underestimate or become complacent about the complexity of the interactions of asset markets and the economy. Thus, evaluating shifts in balance sheets generally, and in asset prices particularly, must be an integral part of the development of monetary policy. "

liquidity managed by the central bank at the end of the 1990s has been argued to have "burst" the dot-com bubble.

The universe of messages that circulates daily in markets is pretty diverse: official reports of economic data, mass media/think tank/guru's analyses and opinions and messages coming from the Federal Reserve System. In the first category different economists give various weights to different pieces of information when making decisions. Much of the information in second category is considered simply noise by some traders and valuable information by others. In contrast, messages sent by the Federal Reserve, especially those that follow FMOC meetings, have at least two characteristics. First, the market participants *pay attention* to them *simultaneously*. Second, to the extent that reputation is maintained, the traders *believe* that messages are accurate. Therefore, messages conveyed by the central bank have the potential to act as coordination devices that enhance common knowledge among traders and subsequently trigger traders to choose strategies in one direction or another. In the context of an inflated asset market this could mean to break the bubble or at least to diminish it. Notice that, unlike the regular Fed's statements, these messages do not even have to bring any *new* information to the market to have an impact on how the market trades the asset in question. The key is for enough traders to get the message at the same time and for the message to be related in some way to the asset being traded. In that case traders might change their expectations about the behavior of the others and the message might end up coordinating them.

A study of the potential role of public messages is hard to perform with standard economic data because it is difficult to develop a credible measure of the fundamental value of stocks and other assets. Further, the analyst has no control over the content of the message, its receptivity or its timing. In contrast, these aspects can be controlled by the experimenter in a laboratory setting. This paper proposes an experimental design to study the potential role of a public message as a coordination device in an asset market that experiences trading at prices different than the fundamental values. The study follows a rich literature on experimental asset market bubbles that examines factors that make the formation of a bubble less likely. The present experiment begins to build a bridge between the theoretical explanation of the persistence of bubbles and what we observe in the real economy.

4.2. Previous literature

The seminal work of Smith, Suchanek and Williams (1988) recognized that common knowledge about dividends is insufficient to induce initial common expectations because agents are uncertain about the behavior of others. The anatomy of the bubbles they found is pretty common to all experimental studies that followed and exhibited bubbles. Trades in early periods tend to be close to or below fundamental value. This pattern was initially thought to be due to risk aversion but this was invalidated by Porter and Smith (1995). People then bid the asset's price up and eventually trade at prices that are significantly higher than the fundamental value. As the end of the experiment nears, the asset prices crash.

A series of experimental papers have investigated the stability of the observed phenomenon with regard to different treatment parameters. None of the following have an impact on the occurrence or size of bubbles: the capability to short sell stocks or to buy on margin, giving identical endowments to traders, requiring payments of brokerage-fees, including professional traders as experimental subjects, price caps and price floors.

Only futures markets - Porter and Smith (1995) - and the experience of subjects - Dufwenberg, Lindqvist and Moore (2005) - have been found to moderate bubbles. The latter study concluded that even with a few experienced traders, bubbles can actually be eliminated. As the authors stated, "these results cast doubt on the plausibility of the hypothesis that financial market bubbles reflect the choices of the inexperienced traders."

On the other hand, Lei, Noussair and Plott (2001) dismissed the speculation motive as the sole source of laboratory asset market bubbles. They observed bubbles even in markets where speculation was prevented. Their conclusion: "departures from fundamental values are not caused by the lack of common knowledge of rationality leading to speculation, but rather by behavior that itself exhibits elements of irrationality."

Abreu and Brunnermeier (2003) proposed a theoretical model that casts doubt on the efficient market hypothesis. [35] The authors show how even rational players will ride the bubble as long as they believe that they will find other traders to buy the asset at an even higher price. The "greater fool" folk theorem is explained by a lack of coordination between 'rational' traders. An important component of their model is the sequential way in which the 'rational' traders become aware of the existence of a bubble. Also, traders are uncertain about how soon or late they became aware of the bubble compared with the other traders. The model has both a coordination component (the bubble bursts only when a sufficient number of 'rational' traders decide to sell) and a competition one (only some of the 'rational' traders are able to sell at a price before the bubble burst). Abreu and Brunnermeier analyze synchronizing events that are uninformative and their capacity to coordinate arbitrageurs in selling out the asset. They also argue that if the bubble does not burst after such an event

[35] The model relies on the presence of both rational and bounded rational traders.

happens, traders infer information about the behavior (and knowledge) of other traders and the price deviation could increases temporarily.

In the context of traders having a coordination incentive due to strategic complementarity in their actions, Morris and Shin (2002) develop a model that shows that with imperfect information, the effect of increasing the public information on welfare will depend on how precise is the private information traders have. The explanation relies on agents' over-reaction to the planner's message. This poses a problem particularly when agents' private information is precise. But even if agents' private information is indeed imprecise, there is no guarantee that the central bank possess better information (or judgment in interpreting the data) than individual agents and so the potential for disruption is real.

Cornand and Heinemann (2008) build on Morris and Shin's theoretical model and challenge their conclusions by distinguishing between two aspects of a public message: the precision of the information and how many agents get the message. They argue for maximum precision and partial dissemination if the message is not 100% precise.

Heinemann, Nagel and Ockenfels (2004) test in the laboratory the theory of global games and argue that a commitment by the central bank to provide public information increases the prior probability of a speculative attack and reduces the threshold to attack.

If lack of common knowledge is an essential feature of the development of a bubble, can a message sent to all traders play the role of a coordination device by enhancing the common knowledge? The bubble could then be diminished or even eliminated if the message triggers a selling strategy among sufficient traders. In our paper there is no uncertainty about the expected value of the asset but there is uncertainty about the behavior of individual traders. In addition, there is no informational role attached to the public message[36], and so we test the pure coordination role of a public message in a bubble environment.

4.3. Experimental design

The design of the experiment follows the ones of Smith, Suchanek and Williams (1988) and Dufwenberg, Lindqvist and Moore (2005). The experiment was conducted in the Economic Science Laboratory at University of Arizona during the period of March-April 2008 using software hosted on the EconPort digital library (Cox and Swarthout, 2006). Eight sessions were run with different traders, all of them with no prior experience in asset market experiments. The traders were undergraduate students at University of Arizona and have been recruited using the Economic Science Laboratory's database.

Each session consisted of a double-sided auction market with six traders that traded an asset for 15 trading periods. The asset carried the right to get stochastic dividends each period according to a commonly known probability function. Dividends were the same for each trader. The information about the realized dividend was revealed to all participants at the end of each trading period. The expected dividend each period was \$0.20. Since there was no compensation given for having the asset in the portfolio at the end of the session, the expected value of the asset was \$3.00 initially and continually decreased throughout the experiment in increments of \$0.20.

[36] In our experiment the public message does not have either an allocative role, as is the case for the changes in interest rates managed by the central bank.

Table 22. Distribution of endowments across traders

	Trader A	Trader B	Trader C	Total
Number of traders	2	2	2	6
Number of units	1	2	3	12
Cash ($)	12	9	6	54
Initial endowment value ($)	15	15	15	90

Some traders had more capital, some had more assets (Table 22), but the total endowment across traders was the same and the initial liquidity in the market was relatively high to make the occurrence of the bubble more likely and also to parallel the reality of macroeconomic environments where bubbles were registered. The number of agents and their endowment were the same in all but one session. Agents could be both buyers and sellers and short selling was not permitted. The screen displayed only the highest bid and the lowest ask at every moment during the trading periods (no queue).

At the beginning of each session participants read the instructions (see Appendix B) and were asked to complete a short questionnaire to ensure that they carefully read and understood the instructions (see Appendix C).

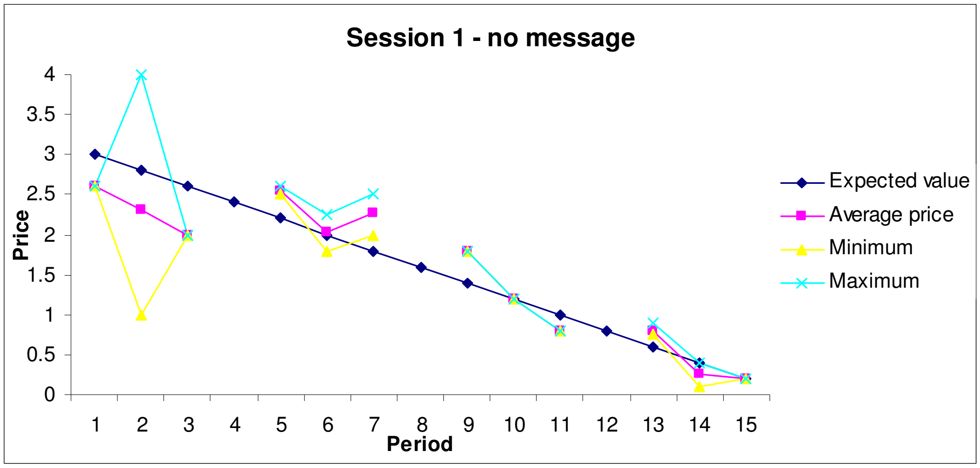
The main departure from previous studies was the possibility of getting a message from the administrator at some point during the experiment. The participants were informed in the instructions that if they received a message, everybody else also received the same message at the same time. We run four sessions with a public message and four sessions without. Instructions were the same across sessions. For all the four treatment sessions the content of the message was the same and sent once, before trading period 9. The message did not inform but referred to the value of the asset being traded: "The expected holding value of asset A for the next trading period is $1.40". The traders knew the fundamental value of the asset each period from their instructions and the value was also displayed on their screen. The control sessions resemble the situation when the central bank is not involved in communication with the markets about the fundamental value of assets in an environment where there is potential for a bubble.

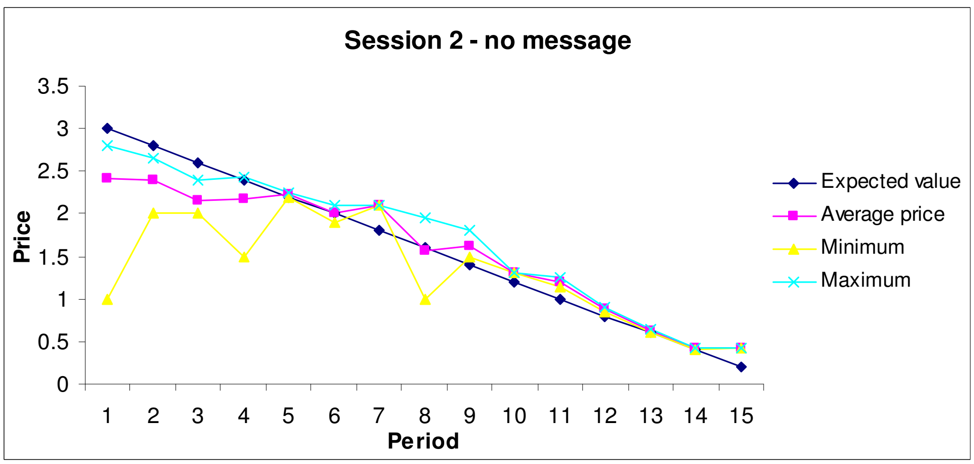
The total duration of each session was around one hour. Traders were paid a $5 show-up fee and a variable fee based on their transactions. The expected total payment across traders was $20.

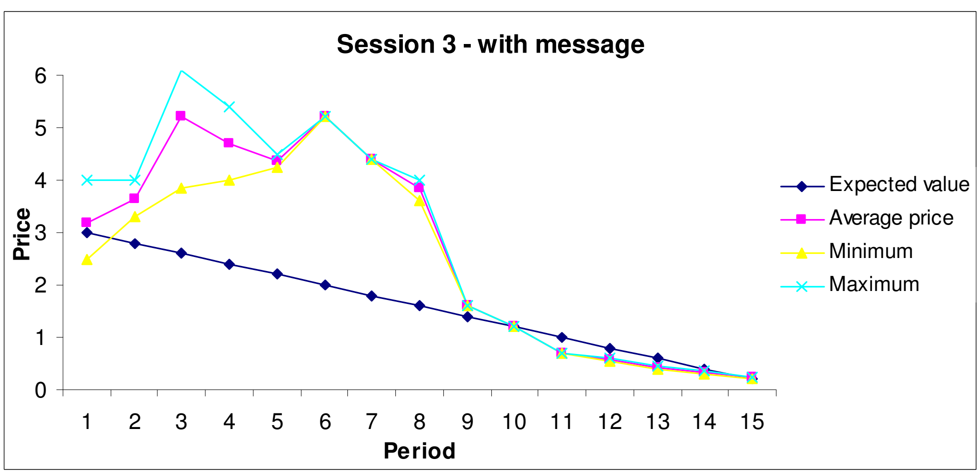
4.4. Results

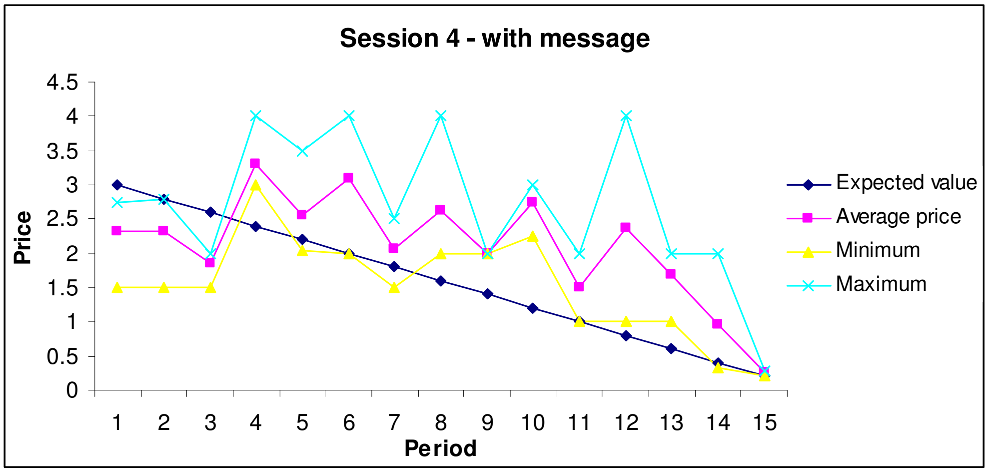
Figure 6 shows in detail the trading prices for each session and period. The general pattern in each figure is similar to the one evidenced by the majority of the other bubble experiments: prices start below the expected value, a period of increased valuation follows and the bubble burst toward the end of the experiment. As expected, there is variation across sessions in terms of the amplitude of the bubble; in some sessions prices were extremely close to fundamental values whereas in others they departed considerably. In the case of the treatment sessions, the response of market participants to the public message varied across sessions. The third and fourth graphs show two extreme cases of treatment sessions. During session #3 the message had an immediate, visible and durable effect on the way the traders priced the asset after period 8. In the case of session #4, however, the traders had almost no initial response to the message and the overpricing of the asset continued. In none of the treatment sessions was the overpricing amplified after the public message was released.

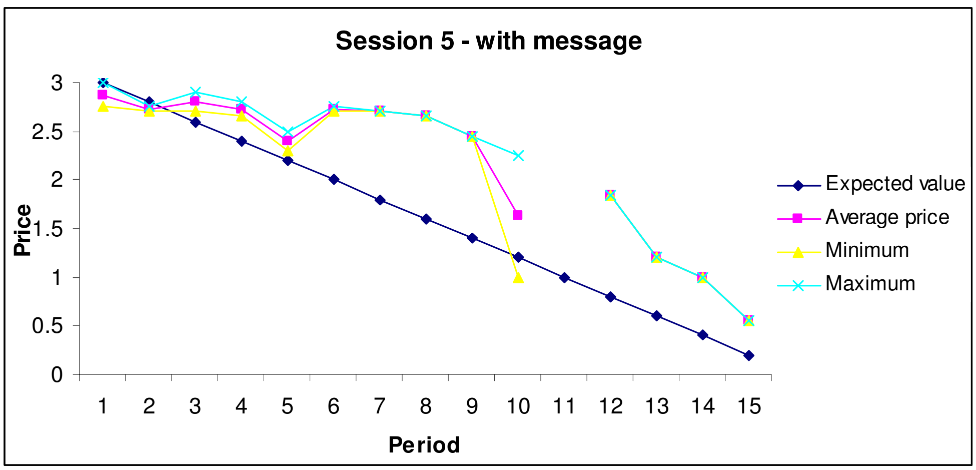
Figure 6. Trading prices for each session

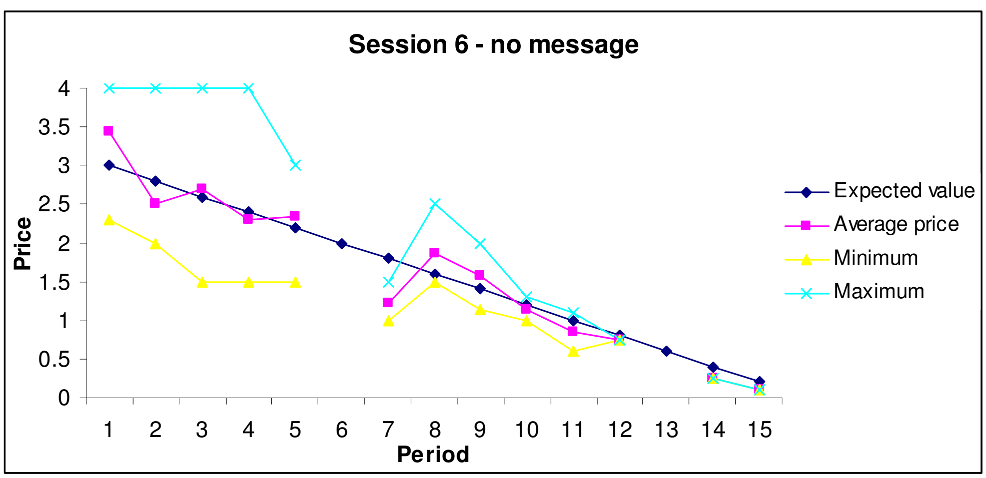


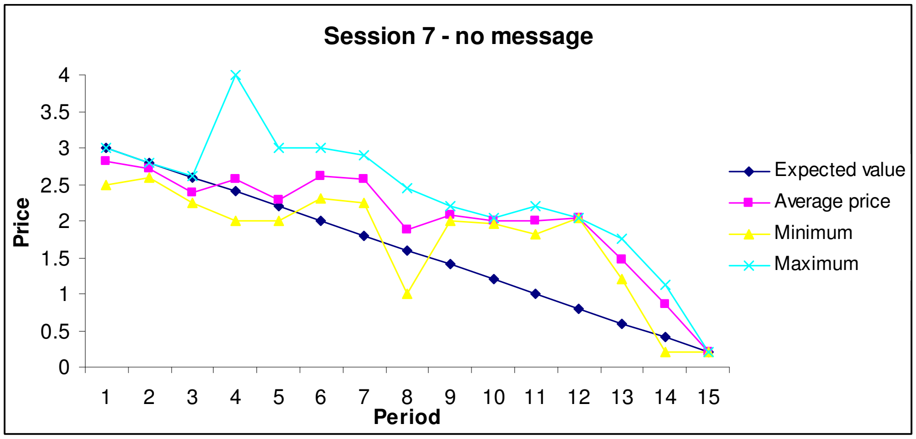


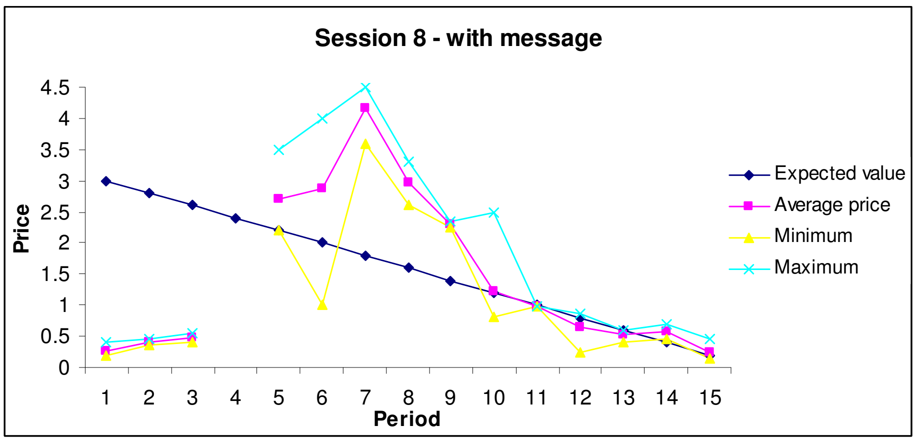



Session 4 - with message
Price
Period
Expected value
Average price
Minimum
Maximum


Session 5 - with message
Price
Period
Expected value
Average price
Minimum
Maximum


Session 6 - no message
Price
Period
Expected value
Average price
Minimum
Maximum





We analyze the difference between the treatment and control sessions in terms of deviations of prices from the expected value of the asset. For this, we use two measures:

a) the absolute total price deviation (ATPD), which is the sum, over all transactions in a period i, of the absolute deviations of prices (P_j) from the expected value (EV_i). This measure takes into account the different trading volumes across periods.

$$ATPD_i = \sum_j |P_{ij} - EV_i|$$

b) the absolute average price deviation (AAPD), which is the absolute deviation of average price (AP) in period i from the expected value.

$$AAPD_i = |AP_i - EV_i|$$

Figure 7.

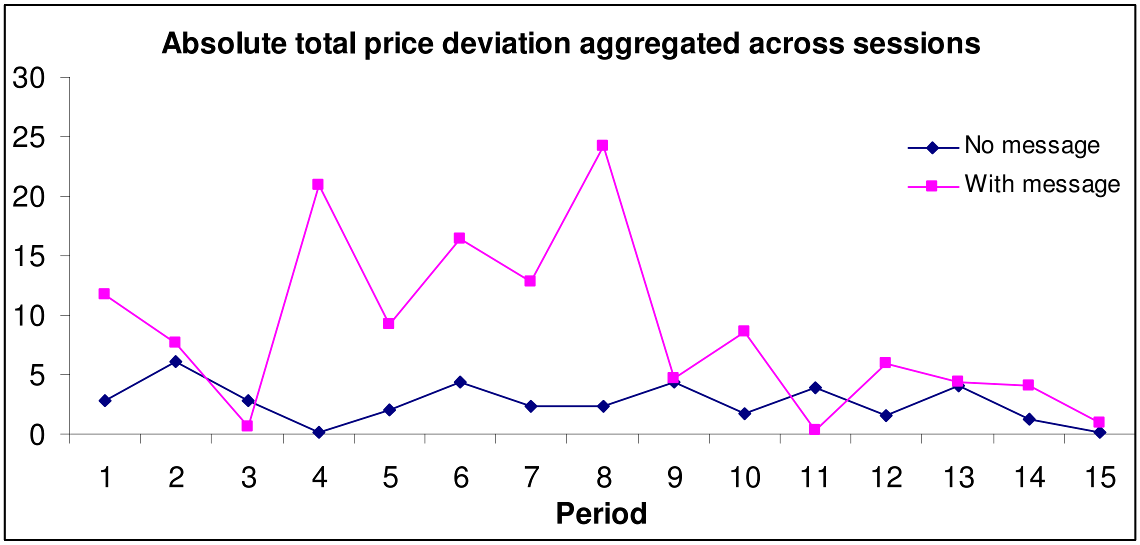
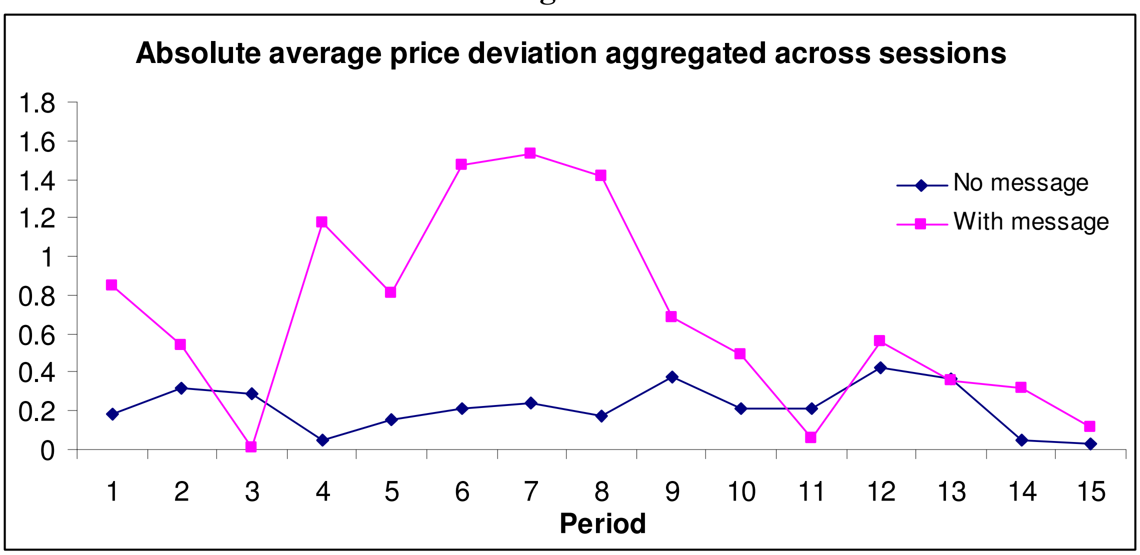


Figure 8.



Figures 7 and 8 show the two measures, aggregated across the treatment and control sessions. Both pictures show a decrease in the excess price immediately after the message was sent whereas there did not seem to have been a change in the case of the control sessions. Another observation is that the price deviation was more severe in the case of sessions with the message.[37]

Tables 23 and 24 show the measures for each session and period. Based on this data we construct the Wilcoxon Mann Whitney test. The research hypothesis is that the message diminishes the excess price of the asset being traded. The null is that the difference between the excess price after the message is delivered (from period 9 on) and the excess price before trading period 9 is the same in the treatment and control sessions whereas the alternative is that it is lower. In doing the analysis, each session is counted as one observation.

Table 23. Absolute total price deviation

Period	Without message				With message			
	Session 1	Session 2	Session 6	Session 7	Session 3	Session 4	Session 5	Session 8
1	0.4	2.95	2.7	0.75	1.5	3.45	0.5	8.2
2	4.4	2.38	3.9	0.25	4.11	1.93	0.25	9.6
3	0.6	1.8	3.9	0.86	10.45	2.95	0.4	8.48
4	0	0.98	3.7	3.19	16.1	3.59	1.26	0
5	0.7	0.05	1.8	2.25	4.35	3.29	0.4	2
6	0.45	0.2	0	4.2	3.2	6.5	1.45	7.23
7	1.4	0.3	1.7	2.3	2.6	2.75	0.9	7.1
8	0	1.1	1	2.86	11.2	9.25	1.05	2.72
9	0.4	0.9	0.85	2.74	0.2	0.6	2.1	1.8
10	0	0.2	0.3	1.6	0	7.66	1.25	2.49
11	0.2	0.4	0.5	3.98	0.6	1	0	0.02
12	0	0.34	0.05	1.25	0.7	6.25	1.05	0.93
13	0.6	0.05	0	3.48	0.35	4.35	0.6	0.25
14	0.55	0.05	0.15	2.22	0.14	3.47	0.6	0.35
15	0	0.22	0.09	0	0.18	0.19	0.35	0.35

[37] While we can not completely explain the difference, a reason for this outcome is the fact that one control session was run with only five traders and the initial liquidity was lower in that case, which made the occurrence of the bubble less likely.

Table 24. Absolute average price deviation

Period	Without message				With message			
	Session 1	Session 2	Session 6	Session 7	Session 3	Session 4	Session 5	Session 8
1	0.4	0.59	0.433	0.188	0.167	0.69	0.125	2.733
2	0.5	0.397	0.3	0.083	0.822	0.483	0.083	2.4
3	0.6	0.45	0.1	0.21	2.612	0.737	0.2	2.12
4	0	0.23	0.1	0.173	2.3	0.898	0.315	0
5	0.35	0.025	0.133	0.094	2.175	0.359	0.2	0.5
6	0.025	0	0	0.614	3.2	1.083	0.725	0.872
7	0.467	0.3	0.567	0.767	2.6	0.269	0.9	2.367
8	0	0.033	0.267	0.277	2.24	1.028	1.05	1.36
9	0.4	0.225	0.175	0.685	0.2	0.6	1.05	0.9
10	0	0.1	0.05	0.8	0	1.532	0.425	0.022
11	0.2	0.2	0.15	0.995	0.3	0.5	0	0.02
12	0	0.085	0.05	1.25	0.233	1.563	1.05	0.146
13	0.2	0.025	0	0.87	0.175	1.088	0.6	0.083
14	0.137	0.025	0.15	0.455	0.07	0.555	0.6	0.175
15	0	0.22	0.09	0	0.03	0.048	0.35	0.05

If we compare the excess pricing *one* trading period before and *one* after the dissemination of the public message then we find evidence that the message was effective in breaking the bubble using as a measure the absolute average price deviation (p-value is 0.03, see Table 25). However, if we use the absolute total price deviation we can not reject the null. If we are interested in more than a short run effect of the public message we can extend the time frame of comparison to *several* trade periods before and after the message was released. In this case there is more evidence that the message was not effective in breaking the bubble as we are less likely to reject the null.

Table 26 presents the dynamics of how the trading went right after the message was released. In two of the sessions the first bid during trading period 9 was under $1.40, the expected value of the asset (the same value that was included in the public message). The other two sessions started the trading with bids above the expected value of the asset. All

Table 25. P value for Wilcoxon Mann Whitney test

	Based on absolute total price deviation	Based on absolute average price deviation
Period 8 minus period 9	0.17	0.03
Periods 6-8 minus periods 9-11	0.17	0.10
Periods 4-8 minus periods 9-13	0.24	0.17
Periods 1-8 minus periods 9-15	0.24	0.34

Note: the null hypothesis is that the difference between excess price during trading period(s) x and period(s) y in sessions with a message is equal with the difference between excess price during trading period(s) x and period(s) y in sessions without a message. The alternative hypothesis is that the difference is lower in the case of sessions with a message.

prices corresponding to the first transactions in the ninth period of each of the four treatment sessions were higher than $1.40, in most cases significantly higher. How do these prices compare with the closing prices in period 8, the trading period before the message was released? Again, the situation is mixed. In two cases there is a significant drop in the price whereas in the other two cases the decrease matches the decrease in the expected value of the asset between the two periods. We can continue the exercise by comparing this measure between treatment and control sessions to get a sense of the impact of the message in the very short run. The differences between closing price in period 8 and the first price in period 9 in treatment sessions (not reported here) are significantly higher than in control sessions at 10% significance level.[38]

We can also say something about who made the bids and who bought the asset immediately after the message. For this, we ranked the traders in each session based on their total earnings. The first bidders seemed to be in general those who made more money than

[38] P-value for the Wilcoxon Mann Whitney test for the null that the difference between closing price in period 8 and the first price in period 9 is the same in treatment and control session and the alternative that it is higher for the treatment sessions is .0571.

Table 26. Prices immediate before and after the message in treatment sessions

Session	Closing price in period 8	First bid in period 9	Price for first trade in period 9
3	3.6	1.2	1.6
4	2.55	2	2
5	2.65	1	2.45
8	2.62	2.35	2.35

others. In the case of sessions 3 and 5, the difference between the first bid and the price of the first transaction was relatively high; the buyer was in both cases the one who made the least amount of money in that session.

We close the section with several additional findings. Table 27 shows the impact of messages on the turnout. Sessions 3 and 4 (the ones we discussed above in regard to figures 3 and 4) share a common pattern: the volume of transactions was significantly lower in period 9 in both cases. For the other two sessions the release of the public message does not seem to have had an influence. As expected, in the sessions without a message it was not a consistent pattern of the change in volume of transactions. Did the public message make any difference when it comes to the distribution of earnings? The last row of Table 6 does not seem to indicate a pattern in regard to the standard deviation of earnings.

Table 27. Volume of transactions and distribution of earnings

Period	Without message				With message			
	Session 1	Session 2	Session 6	Session 7	Session 3	Session 4	Session 5	Session 8
Volume in period 8	0	3	3	6	5	9	1	2
Volume in period 9	1	4	2	4	1	1	2	2
Standard deviation of earnings	2.53	3.43	5.90	6.37	5.39	3.83	1.68	9.95

4.5. Conclusion

Central banks around the globe are more transparent today in terms of their decisions related with monetary policy than they have been in the past. If the role of communication between central banks and markets has been studied in regard to its impact on the effectiveness of monetary policy, less has been documented on the potential role of communication between policy makers and markets in an asset bubble environment. The present study is an attempt to close the existent gap. We find weak evidence that a non-informative public message can serve as a coordination device among traders and break the bubble. Also, there is no evidence of an over-reaction of traders that would be manifested as a situation of undervaluation of the asset after the release of the message. For policy purposes, the conclusions could be that a more involved communication between the central bank and markets in regard to assets perceived to be overvalued could help diminish the bubble. However, it is more likely that it will not have the effect desired by the policy maker if the message does not inform. Based on the experiments we run we did not observe the counter-productive effect a synchronizing event such as an uninformative public message might have, as Abreu and Brunnermeier suggested.

The results presented here did not condition the analysis on the presence of the bubble. As we saw, in several sessions prices did not deviate much from fundamental values. The question we tried to answer was about the role of a public message in an asset market *regardless* of whether the market experiences a bubble or not. This is arguably the relevant question from a policy maker perspective. A central bank faces uncertainty related to how much, if at all, asset prices depart from fundamentals. But more from a theoretical perspective, a slightly different question might be also of interest: can a public message coordinate traders *conditional on observing a bubble*? In order to answer such a question, more sessions would need to be run to be able to statistically assess the impact of the message on asset valuations.

The study could be extended with additional treatments in which messages are sent with a less precise content, to only a subset of traders or more than once. Also, a potentially interesting inquiry would be to analyze how the experience of traders and the possibility of receiving a public message interact. Such additional treatments might broaden our understanding of the impact of public messages in an asset market that experiences a bubble, with possible policy recommendations and/or support for further advancement of theory.

Chapter 5

CONCLUSIONS

This dissertation reveals the complex nature of effects tournament prizes have on outcome, analyzes the impact an alternative system to the present Social Security system had on the well being of the elderly in the 1930s and questions the ability of public messages to break asset bubbles. All three essays are empirical in nature and data has been collected to answer the questions of interest in each case. The work complements the present literature in several important ways that will be summarized below together with the major findings.

The first essay of the dissertation uncovers channels through which payments based on ranking work. These types of payments could be found in settings like promotions, patent races or sport events. Higher prizes will not only give incentives to the participants committed to the tournament to exert more effort but will also induce more able participants to join the competition. This in turn will increase the competitive pressure and have an additional effect on effort exerted by the participants. In other words, ceteris paribus, the same prizes will have different impacts on effort depending on whether the tournament is open or close in nature. Also, the same prizes will have different impact depending on how well ranked a participant is and how close in ability is comparing with the other contestants.

Although the issue of selection is prevalent in the empirical labor literature, the sorting effect of tournament prizes and the importance of competition have not been addressed in the past. The study shows that if one leaves out of the analysis these important channels, she will likely get a biased estimate of how the tournament prizes impact effort. Also, the essay proposes an empirical way to take these channels into account. In order to be able to perform such an analysis a quite rich data needs to be employed: participants need to be observed to join several competitions with diverse prize schemes and tournaments have to compete with each other. In comparison with the previous empirical literature, the data used here contains not only prize winners but also participants who had a chance to win prizes but did not. A counterfactual exercise shows the complex impact of prizes in action.

The analysis of Old Age Assistance in the 1930s presents a less optimistic picture of the efficacy of such a welfare program targeting elderly than a government official or a simple analysis would suggest. The study is focused on mortality rates as a measure of the well being of the elderly and in this regard complements other studies that looked at the impact of the program in other dimensions. Using several measures of the program, detailed data on other covariates likely to impact mortality rates for the elderly, and taking advantage of the gradual implementation of the program across locations and time, the essay does not provide evidence that the program helped decrease the mortality rates for the target population. Results are robust to a range of specifications.

There are several factors identified to have possibly caused the outcome. One target of the program was to give enough financial resources to the elderly to live on their own, instead of in almshouses or with relatives. As a consequence, the program might have contributed to the social isolation of the elderly which has been found by the previous literature to be detrimental in regard to mortality rates. When the study is combined with others that analyzed the impact of welfare programs on mortality rates for elderly, one could conclude that it is more likely to find a positive effect if there are major technological advances in medicine that the beneficiaries of the programs could take advantage of or if the welfare programs are targeted. But even if these types of assistance do not have an impact in terms of mortality rates, they might help the elderly in other dimensions.

The last contribution of the dissertation is in the sphere of financial markets and aims to investigate the coordination role among traders a public message might have in an environment susceptible to experience a bubble. The work is inspired by theoretical models of financial bubbles that argue that trading could take place at prices higher than fundamental values when there is a mis-coordination between rational traders, ie they have the ability to break the bubble but lack the individual incentive to do it and prefer to "ride" the bubble.

Experimental data is employed, which allows to control the timing, content and receptivity of the message. The design of the experiment follows the ones employed by the previous literature. The addition is that a message that does not inform is sent to all traders before a certain trading period. Sessions when the message is sent are compared, in terms of price deviation, with the ones without a public message. The results do not show conclusive evidence that a non-informative public message can have the capacity to break the bubble. Several additional treatments are proposed that could extend our understanding of how public messages influence markets when bubbles might be present.

The inquiry is relevant in the context of increasing debates on the role a central bank could have in an economy where assets trade at prices susceptible to be different than fundamentals. Whereas the majority of economists oppose a more involved role for central banks in the price discovery mechanism, recent developments in financial markets show that there are also costs associated with a prolonged non-involvement in the form of moral hazard if the market forms expectations over time that the central bank will act aggressively in downturn situations.

Appendix A.

DATA

The sample consists of 75 cities for which annual data were available on all forms of relief spending from 1929 to 1938 and age-specific mortality information from 1922 to 1938. Data for relief spending of all types comes from Baird (1942) and it was reported either at the city level or at the county level. The information on the timing of passage of Old Age Assistance and the features of the laws come from Fishback and Thomasson (2006, p. 2-709), from copies of the original laws made from each states legislative statutes, and from information provided by Dora Costa for 1940. The mortality rates were constructed from annual information on the number of deaths in each age group published by the U.S. Bureau of the Census (1925; 1926; 1927 a,b; 1929 a,b; 1930; 1932; 1934; 1935 a,b; 1936 a,b; 1937; 1938; 1939; 1940). The mortality rates were calculated at the city level based on the following formulas:

$M_{a,i,t} = (D_{a,i,t} / P_{a,i,t})*100$

where:

$M_{a,i,t}$ = estimated mortality rate for population of age a, in city i, year t

$D_{a,i,t}$ = deaths of people of age a, in city i, year t

$P_{a,i,t}$ = estimated population of age a, in city i, year t

Estimated population was determined using the following formula:

$P_{a,i,t} = \%C_{a,i,t} * P_{i,t}$

where:

$\%C_{a,i,t}$ = percentage of city i's population of age a, in year t

$P_{i,t}$= city total population in year t

$\%C_{a,i,t}$ was determined through linear interpolation using data at the city level from Haines for 1920 and 1930 and Gardner and Cohen for 1940. Since we did not have information about the age distribution for cities in 1940 we used the age distribution from the counties in which the cities were located to obtain an estimate of the age group's share of the population.

For 16 cities in 1920 we needed to estimate a more detailed age distribution (55-64, 65 years old and older and over 75 years old) than than the age distribution reported in the original source (population 45-64 years old and over 65 years old). For this, we interpolated the shares of the respective age groups as they were in those cities in 1930. For example, if age 75 and older represented 60 percent of the people age 65 and over in 1930, we applied that share to the population 65 and older in 1920 to determine the population age 75 and older in 1920 in that particular city.

We constructed mortality rates at the state level in the same way that we constructed the city mortality rates and from the same sources. We also followed the same procedures when we calculated the mortality rates for the areas of the states not included as part of our 75 cities.

In some specifications we incorporated city-specific time trends from the 1920s. We based these on linear regressions of the age-specific mortality rate on time over the earlier period for each city. The mortality rates for the 1920s were also constructed using the same procedure as above.

We also collected data on other correlates from a variety of sources. Information on the city population, the percentage illiterate, the percentage foreign born, and the percentage black from the U.S. Bureau of the Census Population Censuses of 1920, 1930, and 1940 comes from the ICPSR file number 0003 as amended and corrected by Michael Haines in ICPSR file number 2896. The values for non-census years are linear interpolations between 1920, 1930 and 1940. The census reported illiteracy for people aged 10 and above in 1930 and then reported the number of years of school completed for people aged 25 and up in 1940. For 1930 we used the illiteracy rate for people aged 10 and above. We then estimated illiteracy rates for 1940 for people aged 25 and over. We used 1947 information from the U.S. Bureau of the Census (1948, 7) to find the number of people over 24 years old with no schooling and those with 1 to 4 years of schooling. The illiteracy rate in 1947 for persons with no schooling was 78.2 percent for male and 80.72 percent for females. The illiteracy rate for 1-4 years of schooling was 22.5 percent for males and 16.68 percent for females. We assumed those with more than 5 years of schooling were all literate. U.S. Bureau of the Census, "Illiteracy in the United States, October 1947," Current Population Reports: Population Characteristics, September 22, 1948. Series P-20 no. 20.

We sought an effective method of controlling for the substantial changes in economic activity that occurred during the time period. In the absence of estimates of per capita income at the city level, we chose retail sales per capita in the county in which the city was located as a proxy. As discussed in the text of Fishback, Haines and Kantor (2007), retail sales are strongly correlated with consumption and with income, particularly in urban areas, and we had retail sales information for the counties in which the cities were located for 1929, 1933, 1935, and 1939. Retail sales from 1929 and 1939 are from ICPSR file number 0003, as amended and corrected by Michael Haines. Retail sales in 1933 and 1935 are from U.S. Department of Commerce, Bureau of Foreign and Domestic Commerce (1936, 1939). The population estimate used to create the per capita measure was based on straight-line interpolations between 1920, 1930, and 1940 data from the amended ICPSR file 0003. We interpolated values of per capita retail sales in the intervening years using estimates of state personal income from the U.S. Bureau of Economic Analysis (1989). For each year between 1930 and 1940, we divided state personal income by an estimate of state population. Then to interpolate per capita retail sales between the benchmark years of 1929, 1933, 1935, and 1939, we used a formula like the one below for 1931:

$$R_{31} = S_{31} * (0.5 * R_{29} / S_{29} + 0.5 * R_{33}/S_{33}),$$

where R_t is per capita retail sales in year t for the county in which the city was located and S_t is per capita state personal income in year t. Both the per capita state income and per capita retail sales are adjusted for inflation using the CPI where 1967=100. For more specifics see below.

Information on the number of federal individual income tax returns filed in each year and county comes as following:

Year	Source
1929	U.S. Department of Commerce, Bureau of Foreign and Domestic Commerce (1932)
1930	US Bureau of Internal Revenue (1932)
1931	Rand McNally (1934)
1932	Rand McNally (1935)
1933	US Bureau of Internal Revenue (1935)
1934	U.S. Department of Commerce, Bureau of Foreign and Domestic Commerce (1939)
1935	Rand McNally (1938) and U.S. Department of Commerce, Bureau of Foreign and Domestic Commerce (1939)
1936	Rand McNally (1939)
1937	US Bureau of Internal Revenue (1939)
1938	US Bureau of Internal Revenue (1940)

Information on the number of hospital beds in the city was made available to us by Melissa Thomasson and Jaret Treber from their annual sample of hospitals in the United States that they used in their 2006 paper. This information was compiled by the American Medical Association (various years between 1930 and 1943).

Price Fishback created an urban cost-of-living index that can be used to make cross-sectional and time series comparisons in the cost of purchasing the same broad basket of goods each year over the period from 1914 through 1939 for up to 64 cities. The basic index starts with the Works Progress Administrations's comparison of the costs of "a uniform level of living" in 59 cities across the United States "at a given time and how its cost compared from one city to another." The WPA program constructed quantity budgets at two standards of living for a 4-person manual worker's household: "basic maintenance" and "emergency". The study constructed an identical budget across the cities with certain adjustments for fuel, ice and transportation based on climate and other local conditions. The budget was divided into food, housing, fuel and light, clothing, furniture, and miscellaneous items. The Bureau of Labor Statistics (BLS) than helped WPA workers to obtain the prices used to compare the budgets across cities. U.S. Bureau of Labor Statistics 1940, p. 11; Works Progress Administration (1937, pp. ix-xxvi.) The actual numbers are found in Works Progress Administration (1937, pp. 158-175). For 32 cities, the values of spending from the WPA were adjusted to earlier and later years by using the Urban-CPI for each city. The U.S. Bureau of Labor Statistics consistently reported these figures for all 32 cities in the 1910s, 1920s, and 1930s using the CPIS for five categories of spending--food, housing, fuel and light, clothing, furniture, and miscellaneous items—that matched well with the five categories reported by the WPA for March 1935 and then summing to get a total. For a number of the other cities in the sample, the BLS reported a food CPI Food prices and we assumed that the time paths followed for other portions of the budget were similar to those for similar cities. A detailed description is available from Price Fishback.

The identifying instruments in the 2SLS were created in several ways. The Democratic Governor variable was created from information from Congressional Quarterly Inc. (1995, 639-63). There were three situations in which governors were from nontraditional parties. Minnesota Free-Labor party governors and Wisconsin Progressive party governors are treated as if they were Democrats in this comparison based on our reading of state histories of the 1930s. Oregon Independent governor Julius Meier is given a value of 0.5 because it appears that he was a centrist gubernatorial candidate. At the end of his term both parties sought to have him represent them in the next election.

The variables measuring representation on the House labor committee between 1933 and 1938 are from various issues of U.S. Congress, Official Congressional Directory. The house committee membership was given a value of 0 prior to 1933 because there was no federal money to influence in prior years.

Information on workers' compensation was collected from Fishback and Kantor (2000).

Appendix B.

INSTRUCTIONS

General

This is an experiment in the economics of market decision making. The instructions are simple and if you follow them carefully and make good decisions, you might earn a certain amount of money, which will be paid to you in cash at the end of the experiment. The experiment will last approximately one hour. Please do not speak with any other participants during this experiment.

Market Description

During the experiment you will have the opportunity to use the computer to buy and sell in a market. At the beginning of the experiment you will have an endowment of goods (called asset A) and cash. The experiment will consist of fifteen trading periods. Each trading period will last for two minutes and the trading screen on your computer is shown on the next page. Each trading period is preceded by a preview period that will last ten seconds and is followed by a review period that will last for twenty seconds. During both the preview and the review periods the computer will not allow participants to trade.

Once the experiment begins, the right hand region of your computer screen will indicate to you (under Inventory) the number of units of asset A and the amount of cash (under Endowment) you have been allocated. The information will be automatically updated by the server during the experiment to show your current cash balance and inventory of asset A.

In each period you may buy or sell units of asset A. Asset A can be considered to be an asset with a life of fifteen periods. Your inventory of asset A and cash carries over from one period to the next. A counter in the upper right corner of your computer screen will indicate the current period, the time remaining in the current period and if it is a preview, a trading or a review period.

Trading Screen

Bid-Ask Spread

	Price	Quantity
Best Ask	___	1
Best Bid	___	1

Messages
0:55 Submitted ask for 1 unit at ____
0:45 Ask accepted for 1 unit at ____

Buyer Actions / Seller Actions

Price ______ Market price

Bid/Ask Buy/Sell

Market 1 (Asset A) 1@___

Period __ of 15 Preview/**Trading**/Review Phase
Available Balance: $_____ Time Remaining: 0:20

Period 1 Summary
Starting Balance ____
+Endowment ____
+Trading Gain ____
+Dividend Payout ____

Current Balance
__Current Bids

Available Balance

Inventory	Starting	Current
Asset A	___	___

State Dependent Dividends

State 1 (50%)	0.40
State 2 (50%)	0.00

Asset A Expected Holding Value: ______

Total Period Profit: _______

At the end of each period, each unit of asset A will pay you a dividend. The dividend will be decided randomly by the computer. The value of dividends and their associated probabilities, for each unit of asset A, are indicated in the table below and are also displayed in the right area of your screen under "State Dependent Dividends".

Probability	Dividend per unit
50%	40¢
50%	0¢

The above values do not change from one period to another and are the same for all market participants. From the information shown in the table it follows that on average a dividend of 20¢ will be paid for each unit of asset A held by you at the end of each period. In any particular period the dividend paid will be the same for all the units of asset A. The dividend draws for each period are independent. This means that the probability of a particular dividend at the end of any period is not affected by the dividend in any previous period.

After a trading period ends, there is a review period during which your computer screen will indicate what dividend will be paid for the trading period that just ended, your trading activity during the previous trading period and your current cash balance. By clicking "Next", the computer will display information about the potential dividends you might get by keeping your inventory of asset A until the end of the experiment.

Before the next trading period begins, there will be a preview period. The administrator of the experiment might send you a message during this time. If that will be the case, the message will appear in a box in the center of the screen and you will need to click on the message to be able to start trading during the next round. If you see a message on your screen, you know for sure that all the other participants in the experiment get the same message at the same time with you.

For each unit of asset A you have in your inventory at the end of the experiment you will get zero dollars. This is called the liquidation payment.

Buying and Selling Units

To buy units of asset A you must have cash to pay for them. Buying a unit reduces your cash balance by the purchase price. You may sell any units of asset A you have. Selling a unit increases your cash balance by the sale price.

If you wish to submit a proposal to buy a unit of asset A (this is called a "bid") click on "Buyer Actions" in the lower left region of your screen, enter the price in the white area and click on "Bid".

If you wish to submit a proposal to sell a unit of asset A (this is called an "ask") click on "Seller Actions" in the lower left region of your screen, enter the price in the white area and click on "Ask".

When you submit a bid (a proposal to buy), the computer checks if your bid price is higher than the existing best bid and if you have enough cash to pay for the purchase. If both the answers are affirmative, the current best bid is replaced by your bid in the area marked "Best Bid" in the upper left region of your screen.

When you submit an ask (a proposal to sell), the computer checks if your ask price is less than the existing best ask and if you own at least one unit of asset A. If both the answers are affirmative, the current best ask is replaced by your ask in the area marked "Best Ask" in the upper left region of your screen.

You can buy a unit of asset A in two different ways. First, you can submit a bid and wait for someone to accept it as described above. Second, if you see a best ask price which you would like to accept click on "Buyer Actions" and then click on "Buy" in the lower central region of your screen.

Similarly, you can sell a unit of asset A in two different ways. First, you can submit an ask and wait for someone to accept it as described above. Second, if you see a best bid price which you would like to accept click on "Seller Actions" and then click on "Sell" in the lower central region of your screen.

If you buy or sell a unit of asset A, the number of units you hold and your cash balance will be updated automatically by the computer and will be displayed in the upper right area of your display.

The band along the lower edge of your screen indicates the prices at which recent trades for units of asset A have taken place. Information on up to the six most recent transactions that have occurred in the current period are shown with the most recent transaction on the left hand side. In the "Messages" area on your left side of the screen all your bids, asks and transactions will be displayed.

In this experiment there is no order book or queue. What the computer displays on the left upper side under "Bid-Ask Spread" is only the best bid (the highest price one trader accepts to pay for one unit of asset A) and the best ask (the lowest price a trader accepts to sell a unit of asset A for). When a better bid or offer replaces an unaccepted bid or offer the latter is flushed from the system. Similarly, once a transaction occurs, any outstanding bid or ask is canceled.

During each period, so long as you have sufficient units of asset A and enough cash, you may buy and sell units as often as you wish. However, you are not required to buy or sell any units. Only one unit can be sold or bought per transaction but you may buy or sell several units of asset A during a trading period.

Average Holding Value

You can use the table on the final page of these instructions to help you to make decisions. There are six columns in the table. The first column, labeled "Ended period", indicates the last trading period of the experiment. The second column, labeled "Current period", indicates the period for which the average holding value is being calculated. The third column, labeled "Number of holding periods", gives the number of holding periods from the period in the second column until the end of the experiment. The fourth column, labeled "Average dividend per period", gives the average amount of the dividend in each period for each unit held in your inventory. The fifth column, labeled "Liquidation payment", indicates the payment you will receive for each unit you hold at the end of period 15. The sixth column, labeled "Expected holding value per unit of inventory", gives the average total value of the dividends you would expect to be paid if you keep the asset in your inventory until the end of the experiment plus the liquidation payment. The number in column 6 is calculated by multiplying the numbers in columns 3 and 4 and adding the number in column 5. The information in column 6 corresponding to the specific trading period will also be displayed on your screen in the right lower area under the section "Asset A Expected Holding Value".

For example, suppose that it is currently period 4 and therefore there are 12 holding periods remaining. Since there is a 50% chance that a dividend of 40¢ is paid and a 50% chance that a dividend of 0¢ is paid, on average there is a dividend of 20¢ per period for each unit of asset A. If you hold a unit of asset A until the end of the experiment, the total dividend on the unit over the seven periods left until the experiment ends plus the liquidation payment received at the end of period 15 is on average $2.40. (This is the case since $12 \text{x} 20¢ + \$0.00 = \$2.40$.)

Your Payment

The payment you will receive at the end of the experiment is equal to your cash balance at the end of period 15 plus a $5.00 show up fee. Your cash balance at the end of the experiment will be automatically calculated by the computer and will be equal to your initial cash balance plus all dividends you received, minus cash you spent on the purchases of units, plus cash you received from the sales of units. To this amount, which will appear on your screen, the administrator will add the show up fee, giving you the total compensation for participating in the experiment.

Asset A Expected Holding Value Table

Ended period	Current period	Number of holding periods	Average dividend per period	Liquidation payment	Expected holding value per unit of inventory
-	1	15	$0.20	$0.00	$3.00
1	2	14	$0.20	$0.00	$2.80
2	3	13	$0.20	$0.00	$2.60
3	4	12	$0.20	$0.00	$2.40
4	5	11	$0.20	$0.00	$2.20
5	6	10	$0.20	$0.00	$2.00
6	7	9	$0.20	$0.00	$1.80
7	8	8	$0.20	$0.00	$1.60
8	9	7	$0.20	$0.00	$1.40
9	10	6	$0.20	$0.00	$1.20
10	11	5	$0.20	$0.00	$1.00
11	12	4	$0.20	$0.00	$0.80
12	13	3	$0.20	$0.00	$0.60
13	14	2	$0.20	$0.00	$0.40
14	15	1	$0.20	$0.00	$0.20

Appendix C.
QUESTIONNAIRE

Identify your station number:_________

Please read carefully the following questions and do your best to choose the correct answer. There is only one correct answer and you do not have time constraint.

1. According to the table you have in the instructions, what is the expected holding value per unit of asset A during period 5?

A. $0.20

B. $2.40

C. $0.80

D. $2.20

2. Which of the following statements is correct?

A. I can not sell a unit of asset A if I have only one unit of asset A in my Inventory

B. If I have $2 in my Available Balance, I cannot buy a unit of asset A if its price is $2.20

C. At the end of the experiment, I will get $1 for each unit of asset A I will have in my Inventory.

D. If I have $10 in my Available Balance, I can buy 5 units of asset A if their price is $3 each

3. If I get a public message, I know that:

A. all traders in the experiment get a message, but the content is different for each trader

B. all traders get that message but not at the same time with me

C. all traders get the same message as I get it, at the same time with me

D. some traders get the same message as me, others will not get the message

4. When I see Best Bid=$2.60

A. I can sell a unit of asset A for $2.60 by clicking on “Seller Actions” and than on “Sell”

B. I know for sure I can sell a unit of asset A for $3.00

C. I can click on “Seller Actions” and input an Ask=$3.00 and wait to see if anybody will accept my offer

D. both answers A and C are correct

5. I will make money during this experiment based on:

A. my profits from buying and selling units of asset A during the next 15 trading periods

B. the fact that I came to the experiment

C. how many units of asset A I have at the end of the experiment

D. both answers A and B are correct

6. How many trading periods will be and how much time I have to trade during each period?

A. 15 trading periods and 3 minutes per each period

B. 10 trading periods and 2 minutes per each period

C. 15 trading periods and 2 minutes per each period

D. 10 trading periods and 2 minutes and a half per each period

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