THREE ESSAYS IN ECONOMICS:

COMPETITION, COORDINATION, AND HEALTH

by

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This dissertation is dedicated

to everyone who helped me along

the way. Your impact is immeasurable

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Christopher Roby, PhD
The University of Texas at Dallas, 2017

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This dissertation focuses on how changes in an economic environment can affect an individual's decision making. I assess how individuals respond to different rules and regulations which are meant to alter behavior and the implications therein. I address this problem by looking at response to information that affects competitive choices, group coordination in the face of losses, and risky behaviors for teenagers in response to a legal barrier for smoking.

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CHAPTER 1

INTRODUCTION

Economic decision making is often affected by rules, regulations, and information that an individual receives or considers when making a decision. These responses can lead to a variety of different behaviors, many of which can even have far reaching implications regarding future decisions. Studying these behaviors in different capacities, and at different levels of aggregation, can help us not only better understand the processes through which individuals make decisions, but help gain an understanding of how different mechanisms may cause a person to alter their behavior. I consider this problem through three different lenses: competitiveness, and how this can affect later labor market outcomes, group coordination when payoffs are presented as losses, and the effect of minimum smoking age policies on smoking and alcohol consumption.

Chapter 2 is titled “Social Information and Gender Differences in Competitive Preferences.” In this chapter I examine the effect of selected social information on gender differences in selection into a competitive environment using a simple addition task. Literature shows that men and women have dissimilar preferences for competitive environments. This is important in a labor market context because if women do not select into more competitive jobs, then we will end up with inefficient selection into certain career paths. To try to ameliorate any differences in preferences I include social information about how individuals performed in a similar environment. I find that the inclusion of selected social information eliminates the extant gender gap in selection into a competitive environment in every treatment. This shows that individuals respond to additional information when making a competitive decision.

Chapter 3 is titled “Can Loss Framing Improve Coordination in the Minimum Effort Game?” This chapter looks at how framing payoffs as losses affects group coordination using the minimum effort game. Previous literature shows groups as small as four have a difficult time coordinating on an efficient outcome, even in the presence of higher payoffs for more efficient outcomes. Loss framing has been shown to induce more efficient actions in a variety of other areas, since individuals tend to be more sensitive to losses than gains. I examine the framing effect of losses, specifically if framing payoffs as losses can lead to payoff dominant coordination, using two treatments. These treatments help tease out the effects and dynamics of coordination behavior when payoffs are framed as losses. I find that framing payoffs as losses leads to a modest improvement in coordination. Here, simply presenting payoffs as losses can lead to improved coordination.

Chapter 4 is titled "The Effect of an Increased Minimum Smoking Age on Smoking and Alcohol Consumption: Evidence from the Behavioral Risk Factor Surveillance System." Here I study how individuals respond to minimum smoking age laws in states where the minimum smoking age law is 19. This policy is meant to deter youth smoking. I assess the effect on smoking behavior and alcohol consumption using a regression discontinuity design. The main idea of this method is to exploit the immediate decrease in the cost of smoking once an individual turns 19 and can legally smoke and purchase cigarettes. While it is natural to think this policy may directly impact smoking behavior, this decrease in the cost of smoking may lead to increased uptake of other risky behaviors like consuming alcohol. I find that this policy has no effect on either smoking or alcohol consumption.

I successfully identify a simple intervention which can alter preferences for competition between men and women, assess the role that losses play in improving group coordination, and evaluate how a policy that restricts smoking for teenagers affects smoking behavior and alcohol consumption. This adds to our economic understanding of how individuals respond, through the decisions they make, to different policies and information they encounter. Chapter 5 concludes the dissertation.

CHAPTER 2

SOCIAL INFORMATION AND GENDER DIFFERENCES IN COMPETITIVE PREFERENCES

2.1 Introduction

Despite a wealth of advancements for women in the labor market during the last half-century, women are still vastly underrepresented in high earning executive and managerial positions. Using a data set from US firms, Bertrand and Hallock (2001) find that women only account for about 2.5% of high level executives and earn 45% less than men, as of 1997. Even when accounting for differences in ability, this gap remains; Bertrand and Hallock conclude that women are ”virtually absent” from the corporate world. When looking at specifically CEO's, Wolfers (2006) finds that women only account for 1.3% of CEO's from 1992-2004. Even with these bleak figures, things seem to be improving in the United States. As of 2010, women comprise 25.5% of CEO's, yet only earn about 80% of what their male counterparts in the same positions do (Solis and Hall, 2011). If differences in ability are not an issue, then we are left with myriad explanations as to why these differences both exist, and persist. A more recent explanation for these differences in outcomes is the existence of differences in preferences for competition and competitive environments, which many higher paying executive jobs inhabit. If men and women have dissimilar preferences for competitive work environments, then this could help explain the existence of this gender gap.

Using an experimental structure similar to Niederle and Vesterlund (2007), I investigate the role of selected social information in reducing or eliminating any gender gap in selection into a competitive environment. This feedback is selected average performance information from a related experiment. In three treatments, I assess the effect of gender neutral and gendered feedback on selection into a winner take all tournament. Including social information successfully eliminates the gender gap in tournament entry, largely due to increased tournament entry of women. However, it does not lead to perfect sorting by ability. High performing women enter the tournament when it is efficient to do so, but many low performing women select the tournament as well. While some trepidation exists about the ability of experiments like this to actually explain differences in competitive preferences, I find a relationship between the existence of a gender gap in tournament entry and differences in competitive preferences in all but one of the treatments.

Much of the extant research on gender differences is limited to the exploration of their existence in different environments (Niederle and Vesterlund, 2011). This line of research is important in acknowledging the different challenges women face relative to men, which are largely due to societal structures and socialization. However, few studies talk about ways to close the gender gap, and those that do rely on mechanisms which are often unrealistic. This paper provides a simple way to eliminate the gender gap in competitive selection using social information. This type of feedback encourages high performing women to enter competitive environments at the rate that they should based on their performance. The remainder of the paper proceeds as follows: section 2.2 reviews relevant literature about gender differences and social information, section 2.3 explains the design of the experiment and the three treatments, section 2.4 presents the results of the experiment, and section 2.5 concludes.

2.2 Literature Review

The study of gender differences in outcomes and behavioral preferences is not new. Evolutionary psychologists have long studied how the socialization of children can lead to differences in attitudes by adulthood. An illustrative example of this is "the play styles that both sexes adopt. Boys often play games which are considered to be competitive interactions governed by rules aimed at specific goals. While girls are more involved in 'play' which is considered a cooperative activity in which there is no winner and no clear endpoint" (Cambell, 2013).[1] Economists focus heavily on gender differences in labor market outcomes and wage differentials. Recently, the focus has broadened to

[1] Cambell, (2013) provides a thorough review of the evolutionary psychology literature on gender.

include a rigorous study of gender differences in the preferences which underlie economic decision making. Research has assessed gender differences in altruism (Andreoni and Vesterlund, 2001), risk aversion (Eckel and Grossman, 2002; Eckel and Grossman, 2008), cooperation in negotiation (Eckel, de Olivera, and Grossman, 2008), selfishness in dictator games (Eckel and Grossman, 1998), and more recently, competitive preferences have come into focus.

Psychology looks at the degree to which nature or nurture forces can account for gender differences. Some economic research adds to this discussion by showing that cross cultural differences in behavior of men and women exists. Gneezy, Leonard, and List (2009) perform a simple competitive task in both a patriarchal and a small Matrilineal society. They find that in the patriarchal society, men have a much greater preference for competition than women. In the Matrilineal society, women have a much greater preference for competition than men. The matrilineal women are actually as competitive as the patriarchal men, which suggests that preference for competition has strong ties to socialization and gender norms in a society.

Niederle and Vesterlund (2007) assess gender differences in competitive preferences using a simple experiment meant to mimic selection into competitive jobs. Participants perform a simple addition task and select into either a competitive or non-competitive payment scheme. Niederle and Vesterlund find that while there are no gender differences in performance in the task, there are significant gender differences in selection of competitive payment schemes for the task. This gender difference is driven by women under selecting the competitive pay scheme and men over selecting it, based on their performance. This design implicitly assumes that participants will sort optimally into the tournament based on ability. Many successive works sought to replicate this result and modify the experiment to assess the robustness of the findings. Dargnies (2009), Sutter and Rutzler (2010), Mayr et al. (2012), and Samek (2013) show that these differences both exist and persist across the life cycle.[2] More recent research seeks to modify the informational environment of the experiment to try to reduce or eliminate the gender gap in tournament entry. Brandts,

[2]Both Croson and Gneezy (2009) and Niederle and Vesterlund (2011) provide excellent reviews of many of these works.

Groenert, and Rott (2012) find that intergenerational advice is able to eliminate the gender gap in choices, mostly through the increased entry of high performing women into the competition. Niederle, Segal, and Vesterlund (2012) investigate the role of including ex-ante affirmative action quotas on competitive selection. They find that the inclusion of these quotas increases selection efficiency of high performing women, thus eliminating any gender differences. Wozniak, Harbaugh, and Mayr (2014) find that including information about relative performance induces more efficient competitive pay scheme choices and eliminates the gender gap in competitive entry.

Why should we expect that social information can be an effective way to reduce or eliminate gender differences in competitive preferences? The role of social information to act as an influential nudging factor in experiments with low stakes is well established. This type of information encourages donations to charity when it is known that other contribute (Frey and Meier 2004; Martin and Randal 2008; Croson and Shang 2008; Shang and Croson 2009), increases contribution likelihood in public goods games (Fischbacher, Gachter, and Fehr 2001; Potters, Sefton, and Vesterlund 2005), increases contribution to a movie rating website (Chen et al. 2010), and can increase payoffs for both parties when social history is included in the trust game (Berg, Dickhaut, and McCabe 1995). Coffman, Featherstone, and Kessler (2014) indicate that social information is an influential factor in altering behavior in high stakes decisions. They find that including social information about a previous year's Teach for America cohort makes an individual more likely to accept a teaching job after training and return for a second year.

In a similar vein, this experiment seeks to modify the informational environment using social information. This is selected social information from a previous experiment, specifically, Wozniak, Harbaugh, and Mayr (2014). This type of intervention is important since it is relatively simple to provide. This is in contrast to some of the interventions above. Getting a department, company, industry, or government to agree to affirmative action policy is difficult, and not without dissent and time costs. An example of the legal difficulty affirmative action policies are met with is the 2003 case Gratz v. Bollinger. In this case the US Supreme Court struck down the explicit use of affirmative action policies in undergraduate college admissions (McBride, 2003). Other interventions are unrealistic. If these types of experiments are meant to mimic job entry, then it is highly unrealistic, even illegal in many cases, for relative ranking information among applicants to be disseminated. These issues lead to a desire for a more simple type of information than can eliminate the gender gap in competitive preferences.

The aim of this paper is to investigate how the inclusion of social information affects optimal sorting of participants into competitive and non-competitive pay schemes. A benefit of the social information used here is that it is relatively realistic, in that there are real world examples of the use of this type of information. Average information about a subgroup is not always fully informative (as relative performance information may be), but it can be cheaply provided and helpful when making decisions. Imagine a high school senior who is in the process of applying for college. While they may know their relative ranking within their high school class, they will certainly not know their relative ranking among other college applicants. Some of the most commonly available information about their entry cohort is left to average performance statistics provided by the colleges themselves. These are statistics like average GPA, SAT/ ACT scores, demographic characteristics, and the percentage of enrolled students in each major. While this information is not gendered as in the social information I provide, it is a type of average performance information which is widely used. There are a variety of factors that will ultimately go into a decision to apply to and enter college, but this type of information can help nudge a student into selecting a set of schools which best suits them.

I provide participants with average performance of both genders, their respective tournament entry decisions, and their results with respect to winning the tournament prior to making their competitive choice. This information is selected from a related experiment. Participants are informed that this data is selected to avoid any deception. To test the effect of including social information, I use three treatments. One treatment shows that women and men performed equally on average (and reveals their differential competitive entry choices), one which shows that men outperform women on average, and the third treatment shows women outperform men on average. I find that the inclusion of social information eliminates the gender gap in choices in all of the treatments. This result is driven by women significantly increasing their rate of tournament entry. Not all of this increase in entry is payoff maximizing, as the resulting choices by women are just as inefficient as their male counterparts. Yet, high performing women do end up entering the tournament efficiently in the presence of social information. There has been discussion recently about whether these types of experiments are actually measuring competitive preferences, or just the impact of social norms. To test this I elicited participant's preferences for competition. I find that there are significant gender differences in competitive preferences whenever there are significant differences in tournament selection in all but one treatment. This suggests that differences in competitive preferences are weakly consistent with gender differences in tournament entry decisions.

2.3 Experimental Design

The structure of this experiment is based closely on the design in Niederle and Vesterlund (2007). Here, the baseline condition will be identical to the aforementioned paper, and the treatment conditions will be variations of this design. I use a between subjects design for this experiment. The experiment was conducted at the University of Texas at Dallas using the subject pool of the Laboratory for Behavioral Operations and Economics (LBOE). Participants were recruited using ORSEE (Grenier, 2003), an online recruitment system which allowed pre-screening to make sure the sample was gender balanced. The task is to add up five sets of two digit numbers for a period of five minutes without using a calculator. These numbers are randomly generated, and indeed the whole experiment was conducted, using zTree software (Fischbacher, 2007). A participant submits an answer and another problem appears on the screen, along with an indication of whether or not they solved the previous problem correctly. Participants earn a \$5.00 dollar show-up fee, and an additional \$5.00 for completing the experiment. One of the four rounds was randomly selected for payment, in addition to the flat rate payments. The payment round was selected prior to the sessions using a fair four-sided die. The average payout was \$16.74 dollars. Experimental sessions took, on average, 30 minutes to run. The majority of participants were majoring in either engineering, computer science, or a business/ related field (like accounting, management, etc.). The average GPA for participants is 3.57. See Table A.2 in Appendix A for a complete breakdown of summary statistics for the participants. The baseline condition proceeds as follows:

In Round 1 participants perform the addition task under a piece rate scheme where they earn \$0.50 cents per problem they solve correctly (a 'Round' in this experiment is equivalent to the 'Tasks' in Niederle and Vesterlund 2007). In Round 2 participants perform under a four person winner take all tournament. Individuals are randomly assigned into four person groups with two men and two women each. Participants are told explicitly that they are sitting in the same row as their group members. The individual who performs the best in the four person group earns \$2.00 per problem solved correctly, while the other members of the group receive nothing. In the event of a tie, the winner of the tournament was chosen randomly through the zTree (Fishbacher, 2007) program. In Round 3, participants are able to choose which of the piece rate or tournament pay schemes they would like to perform the addition task under. If they select the piece rate, they receive \$0.50 cents per problem solved correctly. If participants choose the tournament, their performance is evaluated relative to the performance of the members of their respective group from the Round 2 tournament. As long as the participants solve more problems correctly than the Round 2 winner and select the tournament, then they will receive \$2.00 dollars per correct problem. [3] Because of this, the Round 3 choice is actually an individual decision task. For Round 4, participants are asked to choose which payment scheme they would like applied to their Round 1 performance. The addition task is not performed in this round. If they select the piece rate, they receive \$0.50 cents per problem solved correctly, which results in the same profit as if Round 1 were selected for payment. If tournament is chosen, then participants will receive \$2.00 dollars for each correct problem if they performed the best in their group in Round 1, otherwise they will

[3]This design leaves open the possibility that all four members in a group can win the tournament in Round 3. It is possible that none of the members win the tournament. There were some instances where no one won the Round 3 tournament, but none where everyone won.

Table 2.1. Experimental Design

Treatments	Information Intervention	Sessions	Participants
Baseline (Niederle and Vesterlund 2007)	None	10	40
Gender Neutral (GN)	Men and Women on average perform the same. Men choose tournament more, but their win percentage is lower.	10	40
Men as High Performers (MHP)	Men on average perform better than women. Men select and win the tournament more.	10	40
Women as High Performers (WHP)	Women on average perform better than men. Women select and win the tournament more.	10	40

Participants are in groups of 4, with two men and two women in each group. The social information is read aloud to all participants before they choose their remuneration scheme in Round 3.

receive no payment. A Round 4 decision will not affect the payment of any other group member and does not depend on the entry decisions of others. Round 4 is used to determine if general factors such as risk or feedback aversion by themselves cause a gap in tournament entry (Niederle and Vesterlund, 2007). After Round 4 is completed, participants are asked to guess how they ranked relative to their four person group, the average number of problems solved correctly in each of the first two rounds by the session, and to provide their preference for competition on a scale of 1-7 (7 is the highest and 1 is the lowest).

Three treatments were conducted to assess the effect of social information on choices in Round 3. Feedback is given prior to making the decision in Round 3 (after the instructions of how the Round will proceed are read). The intention of these treatments is to see if social information showing that men and women perform similarly (or differently) can remove any gender gap in tournament entry. It is important to note that the inclusion of this feedback does not change the nature of the choice with respect to how a participant's payoff may affect the payoffs of other participants. It is still an individual decision task, as before. However, the choice will be slightly modified because participants are now presented with different information. This may change their perspective on the choice, from both the numerical and gendered information presented in each of the treatments. The feedback information itself is exogenous to this experiment since it comes from a different experiment and is constructed to test my hypotheses. This type of feedback gives no indication of how an individual performed relative to other participants in their group, but it may give them a benchmark of performance to estimate which payment scheme to choose in this round. Table 2.1 gives an overview of the full experimental design.

2.3.1 Treatments

Treatment 1 is the Gender Neutral Treatment (GN). This treatment is constructed to show that men and women performed similarly on average. Participants are informed that both men and women solve 11 problems on average. They are then provided with information as to how many men and women selected the tournament in Round 3, and the number who won.[4] The average subject in Round 3 of this experiment solved 12.26 problems correctly. So by showing this lower average performance information, individuals may be enticed to enter the tournament at a high rate, regardless of gender.

Hypothesis 1: (a) In the presence of social information, relative to the baseline condition, more women should enter the tournament, thus eliminating the gender gap in tournament entry. Men will enter the tournament at a similar rate to the baseline condition. The hypotheses for these treatments need to address how over and under entry may contribute to these changes in decision making for men and women.[5] (b) Inefficient under entry by women will decrease, or even be eliminated. High performing women (who do not enter the tournament enough in the baseline condition) should be

[4]See Appendix B for verbiage and layout of the feedback

[5]Under entry is selecting the piece rate when it would have been optimal to choose the tournament. Over entry is choosing the tournament when it would have been optimal to select the piece rate. Here, optimal means the payoff maximizing choice based on a participant's performance.

enticed into entering the tournament in the presence of the social information. (c) Under entry for men will be low, similar to the baseline. Since the average performance provided in this treatment is on the low side, relative to actual average performance, so we should see an increase in inefficient over entry across genders.

Treatment 2 is the Men as High Performers Treatment (MHP). The intent of this treatment is to exploit any notion or stereotype that men may be better than women at math, even though I find no evidence of performance differences here. Participants are shown that men solved 14.5 problems on average, and women solved 10.63 problems on average. Then, information about tournament choices in Round 3 is provided in the feedback.

Hypothesis 2: (a) This should lead women away from the tournament, regardless of performance. (b) Men will be undeterred in their decisions in the presence of the feedback, and enter the tournament at at least the same rate as in the baseline condition. The general expectation is that the results will be similar to the baseline.

Treatment 3 is the Women as High Performers Treatment (WHP). This treatment looks at how participants respond to receiving information that women are the high performers in the math task. Subjects are informed that women solve 14.33 problems on average, and men solve 10.64. Again, tournament selections and the number of individuals that won is provided.

Hypothesis 3: (a) Seeing that women are high performers will induce a larger number of women to enter the tournament. (b) This will cause men to shirk tournament entry because they are shown that men perform worse on average.

2.4 Results

One possibility for why a gender difference may exist in competitive choices is that there are differences in performance between men and women. If there are gender differences in performance of the addition task, then I am unable to say anything about the effect of the included feedback. First, I will compare performance across all treatments to see if there are any significant differences

Table 2.2. Mean Performance by Round and Treatment

	Treatment	Round 1	Round 2	Round 3
(Women)	Base	9.55	11.30	11.95
(Men)	Base	9.35	11.45	11.75
	GN	10.25	11.20	12.20
	GN	10.95	12.45	13.45
	MHP	9.30	10.95	11.85
	MHP	9.50	11.00	11.35
	WHP	9.75	11.65	12.15
	WHP	11.70	13.25	13.4
	Overall	9.71	11.28	12.04
	Overall	10.38	12.04	12.49

Table 2.2 reports mean performance by gender and treatment in each Round where the addition task is performed. For the cumulative distributions see Figures A.1-A.4 in Appendix A.

in performance between men and women. Table 2.2 shows that in Round 1 the average number of problems solved is 9.71 for women and 10.38 for men. This difference is not significant using a Mann-Whitney test (z=0.277). The gender difference in Round 2 is insignificant as well(z=0.293), where women solve 11.28 problems correctly and men solve 12.04 on average. Even in the third round, where women solve 12.04 problems and men solve 12.49 problems on average, there remains no statistical difference in performance (z=0.691). The conclusions are the same using a two sided t-test.

While the mean values are informative about performance, the values from the WHP treatment are further apart than the other treatments. In this instance, the medians present a clear picture that there are no differences in performance. In Round 1 men have a median of 11.5 and women have a median of 10.5. This difference is insignificant using a Pearson Chi Squared test (p=0.191). In Round 2 men have a median of 13 and women have a median of 12; this difference is insignificant (p=0.514). Round 3 remains insignificant (p=0.191) even with men having a median of 13.5 and women of 11.5. Figures A.1-A.4 in Appendix A show the CDF's of performance for men and women in Round 2 of each treatment. There are no significant differences in performance across rounds or treatments, whether at the mean, median, or distributional level, so I can rule out differences in performance as an explanation for any gender gap that may be found.

Table 2.3. Tournament Entry Across Rounds and Treatments

	Treatment	Round 3 PR	Round 3 T	Round 4 PR	Round 4 T
(Women)	Base	80%	20%**	85%	15%**
(Men)	Base	30%	70%**	55%	45%**
	GN	40%	60%	55%	45%
	GN	25%	75%	45%	55%
	MHP	40%	60%	60%	40%
	MHP	25%	75%	70%	30%
	WHP	45%	55%	75%	25%
	WHP	25%	75%	60%	40%

***P*<0.05, ****P*<0.01. *PR indicates a participant chose the piece rate, T is choosing the tournament*

With regards to performance, it is worthwhile to assess how the variance in performance changes in the face of social information. The social information could be surprising or unexpected, and lead to a higher variance in the treatments. It is worth noting that the information provided to participants is typically incorrect. Men and women perform similarly on simple math tasks like these (Hyde, Fennema, and Lamon, 1990), contrary to the performance feedback provided in the WHP and MHP treatments. In the absence of feedback, a gender gap in tournament selection emerges, unlike the information provided in the GN treatment. Table A.4 in Appendix A shows performance variances by gender and round for each treatment. Variance for men is about 1.5 to 2 times higher than variance for women, but is relatively stable across all treatments. Usually variance is higher in Rounds 1 and 2 (consistent with there being a significant improvement in performance from Round 1 to Round 2), but by Round 3 the variance is near or at its lowest point in any of the rounds. The only time the Round 3 variance is considerably higher than in Round 2 is in the MHP treatment. Women increase their variance from 8.89 in Round 2 to 12.97 in Round 3, and men increase from 13.36 in Round 2 to 16.13 in Round 3. This constitutes a 31% increase in variance for women and a 17% increase in variance for men. Outside of noting that men have larger performance variances than women, there does not seem to be a considerable effect on performance in the presence of social information.

Next, we need to look at choices in Rounds 3 and 4 to see if there is a gender gap, and if it remains in the treatments. Table 2.3 shows the frequency of tournament entry by gender in both Rounds 3 and 4. In the baseline condition for Round 3 men choose the tournament 70% of the time, whereas women choose the tournament 20% of the time. This is a statistically significant difference using a Mann-Whitney test (z=0.001). In the GN treatment, the gender gap is eliminated as women enter the tournament 60% of the time and men enter it 75% of the time (z=0.317). I somewhat surprisingly find an identical, and insignificant (z=0.317), result in the MHP treatment, where men enter the tournament 75% of the time and women enter the tournament 60% of the time. Results are similarly insignificant in the WHP treatment. Men enter the tournament 75% of the time and women enter 55% of the time (z=0.190). It is important to note that men are hardly changing their behavior with respect to tournament entry. The reduction in the gender gap is due to women entering the tournament at a significantly higher rate in the face of feedback. This is consistent with previous research that finds that women's behavior in the lab is more sensitive to interventions which may influence a participant's belief about relative performance (Croson and Gneezy 2009; Niederle and Vesterlund 2011). There are significant differences in tournament selection in Round 4 for the baseline treatment only. Men select the tournament 45% of the time, while women only select the tournament 45% of the time (z=0.04).

Result: This replicates the result from Niederle and Vesterlund (2007) that there are gender differences in both the Round 3 and Round 4 decisions. However, these gender differences are eliminated in the presence of social information.

Relative to the baseline treatment, is the increase in entry for women in the treatments statistically significant? Comparing the baseline condition to the both the GN and MHP treatment (since women enter the tournament at the same rate in both treatments), the increase of women's tournament entry from 20% to 60% is significant at the 95% level using a Mann-Whitney test (z=0.011). Unsurprisingly, the tournament increase between the baseline and GN treatment of 70% to 75% is statistically insignificant (z=0.727). Men increase entry in this manner for each treatment, all of which are insignificant. Comparing the baseline to the WHP treatment, the increase from 20% tournament entry to 55% entry is statistically significant at the 95% level as well (z=0.024). With this in mind, and the information in Table 2.3, it is evident that the treatments were successful in eliminating the gender gap in tournament entry due to significant increases in the tournament selection of women in the treatments.

Assessing the choices in Round 3 paints a clear picture that the treatments succeeded in eliminating the gender gap. Yet, there are a variety of covariates we need to account for in a regression analysis to verify the results. Since the outcomes of interest (either the Round 3 or Round 4 choice) are just binary variables, I will use a probit model with clustering at the individual level[6]. The dependent variable in the table below is the participant's choice in Round 3. It is equal to 1 if tournament was chosen and zero if piece rate was chosen. Table 2.5 reports marginal effects from two probit regressions. All treatments are pooled, and treatment effects are reported by interaction terms that separate the effect of each treatment. This is essentially Difference-in-Differences analysis, where the first difference is gender and the second difference is the treatment.

Female is an indicator variable for gender and is equal to 1 if the participant is a woman and zero if they are a man. This variable will show the gender effect in the baseline condition. GN is an indicator variable equal to 1 if the participant was in the GN treatment, and zero otherwise. MHP is an indicator variable equal to 1 if the participant was in the MHP treatment, and zero otherwise. WHP is an indicator equal to 1 if a participant was in the WHP treatment, and zero otherwise. GN x fem is a composite indicator variable which is equal to 1 if the participant was a woman and in the GN treatment, and zero otherwise. MHP x fem is a composite indicator variable that is equal to 1 if the participant was a woman and in the MHP treatment, and zero otherwise. WHP x fem is a composite indicator, equal to 1 if a participant is a woman in the WHP treatment, and zero otherwise. Improve Round 2 is a control variable to account for the fact that participants perform almost unilaterally better in the Round 2 tournament than the Round 1 piece rate. It is the simple difference between a participant's Round 2 and Round 1 performance. Rank Round 1 is the participant's rank in Round 1 and is meant to account for any effect that rank may

[6]Clustering at the group level produces similar results.

Table 2.4. Round 3 Decision by Treatment (Probit, ME)

Columns:	(1)	(2)
Female	-0.469***	-0.427***
	(0.136)	(0.130)
GN	0.052	-0.002
	(0.146)	(0.134)
MHP	0.052	0.145
	(0.146)	(0.145)
WHP	0.052	0.019
	(0.146)	(0.139)
GN x fem	0.323	0.368**
	(0.203)	(0.171)
MHP x fem	0.323	0.196
	(0.203)	(0.197)
WHP x fem	0.281	0.328*
	(0.204)	(0.189)
Improve Round 2		0.024*
		(0.014)
Rank Round 1		-0.068**
		(0.033)
GPA		0.037
		(0.048)
Age		-0.013
		(0.014)
Rankguess Round 2		-0.148***
		(0.047)
Competitive Preference		-0.011
		(0.023)
N	160	160
Ps. R^2	0.095	0.221
ClustVar	Individual	Individual

Dependent variable is choice in Round 3. It is equal to 1 if tournament is chosen and zero if piece rate is chosen. Marginal Effects and Standard Errors from a probit regression are reported here. ***$P< 0.01$, **$P< 0.05$, *$P< 0.10$. *Clustering is at the individual level.*

have on tournament entry. Age and GPA are the self-reported age and GPA of the participants. Rankguess Round 2 is the participant's guessed rank in Round 2. This guess is elicited after the experiment is completed, and is intended to control for confidence when making the Round 3 decision. Competitive Preference is the individual's preference for competition on a scale of 1-7, where 7 is the highest preference for competition. Table 2.4 reports marginal effects for these probit regressions.

What we see in Table 2.4 is that after conditioning on performance and other covariates, the gender difference in tournament entry remains significant in the baseline condition.

Result: In the full regression in column 2, women are 42.7% less likely to choose the tournament in the baseline condition. This constitutes a large gender gap, about twice as large as the any previous result (other works find this difference to be on the order of 12% to 20% Niederle and Vesterlund, 2011).

If including social information can remove a gender gap this large, then it shows both the strength of this intervention and the potential usefulness for a mechanism such as this to alleviate these gender differences. A few of the remaining covariates are significant in the regression. While the focus of this paper is not explicitly on the role of confidence or rank effects in these experiments, these results are in line with expectation as to their roles. Both Rank Round 1 and Rankguess Round 2 are negative and statistically significant, meaning that a person who has a higher rank in Round 1 (associated with a smaller number on a scale of 1-4) or guesses a higher rank for themselves in Round 2 is going to have a higher probability of entering the tournament. It is not surprising that the Rankguess Round 2 coefficient is highly significant because confidence about an individual's relative ranking matters when deciding to enter the tournament.

To assess the effect of the treatments on the gender difference we need to look at the combination of the female variable in column 2 with each of the MHP x fem, GN x fem, and WHP x fem variables. This analysis is necessary to see if the combination of the female variable and each of the three treatment interaction variables are equal to zero. If I can reject that the combination of these variables are equal to zero, then there is a gender effect which remains in the treatments, suggesting that they may not have eliminated the gender gap.

Result: I am able to marginally reject that there is any remaining gender effect in both all 3 treatments: GN treatment (p=0.641), the MHP treatment (p=0.175), and the WHP treatment (z=0.457). This confirms that even after all covariates are accounted for, the treatments were successful in eliminating the gender gap in tournament entry. This is in line with hypothesis 1a, which says that the GN treatment will cause women to increase their rate of tournament entry relative to the baseline.

Why are the results for the MHP treatment contrary to hypotheses 2(a) and 2(b); and why are the WHP results contrary to hypothesis 3(b)? One possibility is the gendered information content of these two treatments may have some effect on confidence through reactions that participants may have. The MHP treatment is set to show that men are better at solving math problems (and WHP to show women perform better), even though there is no evidence that suggests that men and women perform differently on simple math tasks (Hyde, Fennema, and Lamon, 1990). One reason that women may enter the tournament more in this treatment is because they feel that the gendered information is a challenge for them to either outperform the feedback or try to prove it wrong. Another possibility is that it is simply the lack of differences in competitive preferences in this treatment that drives this result. A third possibility is that participants were keying on the numerical aspects of the feedback, not the gendered aspects. A participant receives their performance feedback in the form of the number of problems they solved correctly in a certain round. Since this is the only feedback about their own performance they receive prior to the social information intervention, it is reasonable to consider that the numerical average performance would have been the most salient to assess the prospect of tournament entry. While I am not able to fully tease out why the MHP and WHP results are what they are, the numerical salience of the feedback remains the best explanation. Participants improve their performance between Rounds 1 and 2, as well as Rounds 2 and 3. If a participant notices that they have improved considerably after Round 2, then it is reasonable to consider that there may be an expectation of continued improvement in Round 3 which would lead selecting the tournament, even if it were inefficient. In Round 2, 66.25% of participants improve by at least 1 correct problem over the first round. The majority of participants continue to improve in Round 3, as 50.63% improve by at least 1 over Round 2. It is common to have significant improvement between the first two rounds due to learning effects, but over time performance generally levels off (Niederle and Vesterlund 2010). The results show that social information drives more women to enter the tournament, regardless of the gendered nature of the feedback information.

2.4.1 What is Driving Changes in Tournament Selection in Treatments?

Just seeing that the treatment conditions resulted in an elimination of gender differences in choices is not enough to explain my hypotheses completely. To do this, I need to determine the role of over and under entry into the tournament in Round 3 in relation to the reduction of the gender gap. Over entry is when a participant should not enter the tournament based on their performance in Round 2, but does so. This is calculated using a participant's relative ranking in Round 2. If a participant was ranked first in their group, then their optimal decision (in terms of maximizing payoffs) is to enter the tournament. Participants ranked second through fourth should select the piece rate. If a participant was tied for first, their rank is still considered first, and it is optimal for them to enter the tournament, even though they may not have actually won the Round 2 tournament. Under entry is a similar concept, except that under entry occurs when a participant is ranked first and does not choose the tournament. Both over and under entry are inefficient behaviors with respect to participant sorting. If sorting were perfectly efficient based on payoff maximization, a participant would know their ability type relative to others, and sort accordingly. Inefficiency is created partly by withholding relative performance information when making the tournament entry decision in Rounds 3 and 4. However, as discussed in the introduction, if the desire is to effectively mimic entry into competitive jobs, it is highly unlikely that an individual will know their relative performance on a job interview, just whether they were awarded the job or not.

In previous experiments women tend to under enter and men tend to over enter into the tournament in Round 3. This is what creates the gender difference in tournament selection. In the baseline, this pattern continues as 4 out of 5 women under enter and 9 out of 14 men over enter in the baseline condition. Table 2.5 shows the results for over and under entry.

Result: In the GN treatment, inefficient under entry is almost completely eliminated for men, and is non-existent for women. This follows with hypotheses 1(b) and 1(c). With respect to over entry, men behave similarly to the baseline, but women severely increase over entry such that 8 out of 16 women are doing so.

Table 2.5. Over and Under Entry into the Tournament

Under Entry	Baseline	GN	MHP	WHP
(Women)	4/5 (80%)	0/4 (0%)	1/6 (16.7%)	4/11 (33%)
(Men)	1/6 (16.7%)	1/7 (14.3%)	1/6 (16.7%)	1/12 (8.3%)
Over Entry	**Baseline**	**GN**	**MHP**	**WHP**
(Women)	3/15 (20%)	8/16 (50%)	7/14 (50%)	4/9 (44%)
(Men)	9/14 (64%)	9/13 (69%)	10/14 (71%)	4/8 (50%)

Over and under entry are calculated using a participant's group rank from Round 2. Under entry is selecting the piece rate when it would have been optimal to choose the tournament. Over entry is choosing the tournament when it would have been optimal to select the piece rate. What drives the tournament entry gap in the baseline condition (and in previous works) is that men over enter into the tournament at a high rate and women under enter at too high of a rate.

In the MHP treatment, we see similar results to the Gender Neutral treatment. Both men and women have low levels of under entry and high levels of over entry. This is contrary to hypothesis 2(b) because we still see women entering the tournament at a high rate in this treatment. For the WHP treatment, women enter the tournament at a higher rate than in the baseline, resulting in 4 out of 9 women over entering. Inefficient under entry decreases considerably, with 4 of 11 women under entering. Men still enter the tournament at a high rate, and their behavior is similar to the baseline with respect to over and under entry, contrary to hypothesis 3(b).

Result: Including social information causes men and women to behave similarly with regard to tournament entry, instead of reflecting the gendered nature of the feedback information in the MHP and WHP treatment.

Overconfidence is usually a variable of interest in these types of studies. One explanation for why gender differences may exist is that men are more overconfident about activities that typically fall under a stereotype of the ”male domain”, like math tasks. It is important to assess whether there is a gender difference in overconfidence, and if there are differences between the treatment and control conditions. Overconfidence is defined by a participant guessing a higher rank than their actual rank in Round 2. I find no gender differences in confidence in any of the treatments. This is surprising, given that every previous work on this matter finds that significant differences in overconfidence exist. I still include controls for confidence levels in the regression since confidence

Table 2.6. Competitive Preferences

	Treatment	Average	Mean if Enter R3 T	Percent Confidence >= 4
(Women)	Base	5**	5.5	75%
(Men)	Base	6**	6.2	95%
	GN	6	5.8	95%
	GN	6.45	6.4	95%
	MHP	5.6	5.6	90%
	MHP	5.5	5.3	85%
	WHP	5.45**	5.82	85%
	WHP	6.6**	6.73	100%

***P< 0.05, ***P< 0.01. Competitive preferences are on a scale of 1 to 7, where 7 is the highest preference for competition. Participants are asked to provide their preference for competition following Round 4.*

may play an idiosyncratic role in a participant's decision making, but I find no systematic difference in overconfidence between genders.[7]

Recently, it has come into question as to whether these types of experiments are actually measuring differences in preferences for competition, or something else, like the role of socialization and societal gender norms in engendering differences in preferences. To assess this, participants were asked to provide their preference for competition on a scale of 1-7, where 7 is the highest preference for competition. Table 2.6 shows the results of these elicitations. In the baseline condition, there is a gender difference at the 95% level (z=0.033). This gender difference does not exist in the GN and MHP treatments. There is a gender difference in competitive preferences at the 99% level (z=0.0102)in the WHP treatment, despite the lack of a gender gap in the WHP treatment.

Result: This suggests there is a weak relationship between competitive preferences and gender differences in tournament selection in Round 3.

It is interesting to note that the vast majority of participants, the lowest being 75 percent, report having a preference for competition of at least 4. This may help explain why we see such high tournament entry by both genders in the treatment conditions.

[7]For more detailed results concerning overconfidence, see Appendix A

2.4.2 Round 4 Results

Table A.1 in Appendix A reports marginal effects. The dependent variable is the participant's choice in Round 4. It is equal to 1 if the tournament is chosen, and zero if the piece rate is chosen. All control variables are identical to the above section. I do find gender differences in tournament selection in Round 4 in the baseline condition, even after accounting for control variables. Looking at column 2, we can see that the gender difference in the baseline is significant and says that women have a 29% lower probability of selecting the tournament in Round 4. As in Table 2.5, a participant's guessed rank in Round 2 is significant in the expected direction. A participant who guessed that they had a higher rank (associated with a lower number) is more likely to choose the tournament.

Again, it important to assess if there are gender effects that spill over into the treatments in Round 4, even though the treatments are not intended to impact the Round 4 decision. In each of the treatments I am able to marginally reject that there is a gender effect (In the GN treatment (p=0.619), the MHP treatment (p=0.237), and the WHP treatment (p=0.443)). There is no significant gender difference in Round 4 decisions in any of the treatments. 90 percent of participants who selected the tournament in Round 4 selected the tournament in Round 3, whereas 45 percent of participants who selected the Round 3 tournament selected the Round 4 tournament.

Result: In both cases, selecting the tournament in one round increases your likelihood of selecting it in the next round, suggesting there are spill over effects when tournament entry is high.

2.5 Conclusion

Information about your performance relative to others in a variety of scenarios is very difficult, and even impossible, to get. A more realistic type of information to provide is social information. This information can be helpful in deciding whether to enter into a competitive scenario. In this paper I find that including social information eliminates the gender gap in tournament entry, using a simple real effort task. The reduction of the gender gap is not due to greater efficiency of tournament selection, but that women significantly increase tournament entry, even when it may not be a payoff maximizing choice. While I account for overconfidence, I find no systematic gender differences in overconfidence any of the treatments. I find that there is a weak relationship between tournament entry decisions and competitive preferences. As an extension of this work, it would be interesting to see how social information that was more rudimentary would fare in reducing the competitive gender gap, and how it could be extended into different environments. Buser, Niederle, and Oosterbeek (2014) find that gender differences in competitive preferences can account for up to 20% of the decision to enter a more prestigious and competitive career path. This suggests that there is an enormous potential benefit for a simple mechanism, like the social information used here, which can alleviate the effects of gender differences in competitive preferences.

CHAPTER 3

CAN LOSS FRAMING IMPROVE COORDINATION IN THE MINIMUM EFFORT GAME?

3.1 Introduction

Coordination in groups is difficult. Generalized truisms aside, these issues are the focus of a large literature in economics, management, and related fields because of the large implications that coordination has for businesses. In many instances, effective coordination depends on the worker who is putting forth the least effort. In this instance one of two things usually occurs. Either the group takes longer to complete the task, due to waiting on this person to finish their job, or other group members have to exert more effort to compensate for the low productivity from this individual. These coordination issues are common and frequently studied using economic experiments, which allow researchers to study behaviors which can lead to poor coordination, and mechanisms with which to improve it.

It is common to observe coordination failure in a laboratory setting, even in the face of higher payoffs for more efficient actions. Much of the economic literature is dedicated to understanding why coordination failure occurs, and the various settings which may make failure more common. The literature now focuses on analyzing how coordination can be improved (Van Huyck, Battalio, and Beil 1990, Cachon and Camerer 1996, Brandts and Cooper 2006, Devetag and Ortmann 2007, Hamman et al. 2007). While some studies have changed the payoff structure in these types of games to achieve a higher level of coordination, the role of unavoidable losses has not been considered. In both lab and field settings, loss framing has been effective in encouraging more efficient behaviors (Ganzach and Karsahi 1995; Bertrand et al. 2010; Hossain and List 2012; Fryer Jr. et al. 2012; Levitt et al. 2016). This is largely due to the notion of loss aversion in prospect theory (Kahneman and Tversky 1979; Tversky and Kahneman 1981 and 1991; Bateman et al. 1997). Loss aversion says that individuals are more sensitive to losses than equal sized gains. If loss aversion is salient here, then framing payoffs as losses could increase coordination levels.

I assess the role of unavoidable negative payoffs on coordination in the minimum effort game. Even though the role of penalizing inefficient actions has been studied in the context of the minimum effort game before, no one has systematically assessed the role of losses in coordination behavior (using this game or any other). Since coordination issues have been studied thoroughly using a variety of different mechanisms, this seems like a natural extension. Because individuals tend to be more sensitive to losses than equal sized gains, framing payoffs as losses should at least lead to a modest, and potentially a large, improvement in coordination. This sensitivity to framing payoffs as losses should lead individuals to choose riskier actions, thus moving the group effort level above the risk dominant equilibrium, which indicates coordination failure. I use two treatments to assess the dynamics of moving to a loss frame after playing under a standard payoff table with gains (and vise versa). I find that framing payoffs as losses modestly improves coordination. However, simply presenting payoffs as losses results in better group coordination overall. One reason why it is so important to understand how individuals coordinate when payoffs are framed like this is that there are common instances when workers think in this manner. Individuals who involuntarily have to put in overtime hours at their job frequently consider the cost of this in terms of lost leisure time instead of the additional benefit (monetary or otherwise) that they may receive from putting extra work to finish a project. The remainder of the paper proceeds as follows: section 3.2 reviews literature on coordination and loss framing, section 3.3 explains the minimum effort game and the design of the experiment, section 3.4 explores the results of the experiment, including treatment effects and risk preferences, and section 3.5 concludes.

3.2 Literature Review

Behavior in the minimum effort game in a laboratory setting has been studied extensively for almost three decades (Van Huyck, Battalio, and Beil 1990). The essential feature of the minimum effort game is that it has multiple pareto-ranked equilibria. Since payoffs are determined by the participant's action and the minimum action of their group, coordination can be difficult, especially when there is no history of play among group members. Theoretically, we expect payoff maximizing participants to be able to coordinate on the payoff dominant equilibrium (occasionally referred to as most efficient or highest level of coordination). However, this is not what we see in a lab setting. Participants are often only able to coordinate on the secure equilibrium in this manner, thereby giving up potential earnings. Coordinating on the secure action is considered coordination failure, because this equilibrium is pareto dominated by any higher action equilibria. There emerge a variety of explanations and selection principles that guide participant choices in coordination games.

One principle that is only lightly discussed in the literature on the minimum effort game, is loss aversion (Kahneman and Tversky 1979). Loss aversion suggests that participants will select riskier actions to avoid losses but will not do so for equal sized gains. My paper does not address loss aversion specifically, but testing this aspect of prospect theory in this framework would seem like a logical extension of this literature. However, loss aversion is the principle that guides my choice of intervention and hypotheses. If individuals take riskier actions under the loss frame due to loss aversion than they should be able to achieve higher levels of group coordination, instead of ending up at the risk dominant equilibrium. Loss avoidance, introduced by Cachon and Camerer (1996), shows that participants will increase their action in the minimum effort game towards efficiency to avoid certain losses. In my experiment there is no way to avoid losses when playing under the loss payoff table, so there is no expectation that this will apply. Choosing the secure action has been shown to be an effective explanation for coordination behavior in this game. Security is a maximin action where a participant is choosing an "action with the largest payoff in the worst possible outcome" (Van Huyck, Battalio, and Beil 1990). Security is part of the reason we see such rampant coordination failure in the literature. Participants can learn to coordinate as they play multiple rounds of the game with a static group (Van Huyck, Battalio, and Beil 1990). The history of play shows participants that they have successfully coordinated on a certain action in the past, and gives an idea of future action possibilities. This is similar to the idea of precedence in Cachon and Camerer (1996) where participants believe that an equilibrium that has been picked before is more likely to chosen again. These beliefs may be self-fulfilling, leading to persistent coordination on an inefficient equilibrium. Repeated play can lead to participants learning to coordinate over time (Lee-Penagos 2016; Crawford 2016), but in a lab setting participants often get stuck at a lower, pareto dominated, equilibrium.

A variety of modifications have been made to assess experimental regularities and robustness of coordination failure. Many of these methods were aimed at achieving more efficient levels of coordination.[1] Group size matters in these experiments. Larger groups tend towards coordination failure more quickly, but groups of two have an easier time coordinating at a higher equilibrium (Van Huyck, Battalio, and Beil 1990). More recent research, and indeed this paper, use groups of four and are successfully able to achieve coordination failure (Brandts and Cooper, 2006 & Hamman, Rick, and Weber 2007). The variation in payoffs between equilibrium actions matters. If the secure action has a high enough expected value, it will sabotage the choice of risky actions. Conversely, if the risky actions have a considerably higher expected value, it may eliminate security as a selection principle (Devetag and Ortmann 2007). Brandts and Cooper (2004) vary payoffs of the non secure actions, and find that higher levels of coordination success occur when these actions have stronger incentives. Varying the magnitude of off equilibrium payoffs has mixed results in the literature. Berninghaus and Ehrhart (1998) lower the opportunity cost of choosing an off diagonal action, but don't find conclusive results that this modification had a significant effect on coordination. Van Huyck et al. (2007) and Goeree and Holt (2005) vary the magnitude of off diagonal costs and find that this can lead to significant coordination improvement. Mas and Nax (2016) test action deviation related to trial and error in coordination games. They find that deviation increases significantly when subjects change their action and/ or experienced a payoff decrease in

[1] Devetag and Ortmann (2007) provide a concise and thorough review of the literature on coordination games.

the previous period. Perryman and Kelsey (2015) find that participants who use stereotypes about another ethnic group to guide decision making makes coordination worse.

Another way to attempt to achieve coordination success is to change the way payoffs are presented. One non-monetary way to improve coordination is to try to foster group identities and trust. In the absence of this type of information it takes repeated play of participants to be able to reliably coordinate on an equilibrium point. Blume and Ortmann (2007) allow costless communication between participants. They find that this results in coordination on the payoff dominant equilibrium. Similarly, Dahlen et al. (2016) find that local sharing of information can improve coordination. Other works include have mechanisms like entry fees or auctions to play. These modifications facilitate efficient coordination by making the pareto dominated equilibria less attractive (Van Huyck et al. 1993, Cachon and Camerer 1996). Full information about participant choices in every round seems to be a natural extension that would improve efficiency, but, results are mixed. Devetag (2005) and Van Huyck, Battalio, and Beil (1990) find no effect of including full information. Yet, Berninghaus and Ehrhart (2001), Brandts and Cooper (2006), and Hamman, Rick, and Weber (2007) all suggest that this full information improves coordination. I will not include full information here, to avoid any confusion of my results with information effects. Other incentives have been used to improve coordination. Hamman, Rick, and Weber (2007) use positive and negative incentives in a variety of ways and treatments to assess how they may improve coordination. They find that all the types of incentives used can improve coordination while they are in place, but once incentives are removed, coordination failure returns.

We can think of changing the presentation of payoffs as a framing effect. Framing effects have been shown to be significant in a variety of contexts. If a public good is framed in a different way, it can lead to higher contribution levels (Brewer and Kramer 1986; Sonnemans, Schram, and Offerman 1998; Cookson 2000). Positive frames can lead to more cooperation than negative frames (Andreoni 1995). Participants in a lab setting are more sensitive to losses than equal sized gains (Kahneman and Tversky 1979; Tversky and Kahneman 1981 and 1991; Bateman et al. 1997).

Loss framing has only recently been extended to a field setting. It has been effective in a variety of areas, including: when used in marketing messages to change product demand (Ganzach and Karsahi 1995; Bertrand et al. 2010), improving team productivity in a Chinese factory (Hossain and List 2012), and increasing performance on a low stakes test relative to gain framing (Levitt et al. 2016). Framing payoffs in this manner is not always effective in modifying behavior though. Fryer et al. (2012) find that framing teacher incentives in this manner is not effective in increasing the performance of the instructor.

Devetag and Ortmann (2007) lament that "no studies exist (yet) that investigate the role of negative payoffs in a systematic way..." This is where I intend to continue the discussion with this study. While adding some negative payoffs or penalties has been studied in the context of the minimum effort game before, no one has looked at how coordination changes in the face of unavoidable negative payoffs (using this game or any other). Coordination issues have been studied thoroughly using a variety of different mechanisms, so this seems like a natural extension.

3.3 Experimental Design

I will begin this section with a short explanation of the minimum effort game as I use it in this paper. The minimum effort game is derived from John Bryant's (1983) Keynesian coordination game. The main difference being that the minimum effort game is in strategic form, as in Van Huyck, Battalio, and Beil (1990). This game is used to assess how monetary incentives can affect group effort levels. In each round, participants select a number between 1 and 7. Larger integers are associated with higher payoffs. So choosing 7 represents the highest payoff. However, payment is determined by your own choice, and the minimum choice of your group members. The best option for any member of the group is to choose a number equal to the minimum chosen by the group. The set up of this game can make coordination difficult, especially with no history of play to assess what other group members may do. This payoff structure gives us 7 pareto ranked equilibria. Everyone choosing 7 is the payoff dominant equilibrium. Choosing 1 is the secure equilibrium, but coordinating on this outcome is considered coordination failure since it results in the lowest payoff of any equilibrium point. After the first round is played, participants are reminded of their action, told the minimum action of their group, and their payoff for that period. Otherwise, no communication is allowed between group members.[2]

The experiment is split into two parts. Each part is 10 rounds. Participants play the minimum effort game in each round in groups of 4. The groups are randomly assigned, and participants played with the same group throughout the experiment. Before participants begin part 1 and part 2 there is a short quiz, to make sure participants can accurately calculate payoffs for each round.[3] There are two different payoff matrices used to tease out the effect of losses on coordination. One is a standard minimum effort gain table with gains, the other has payoffs framed as losses.

The operative equation which determines the payoff tables is:

$$\begin{aligned} \pi(e_i, e_j) &= a[min(e_i, e_j)] - be_i \\ a &> b > 0 \end{aligned} \tag{3.1}$$

Table 3.1 shows the Gain table; it is similar to the minimum effort table in Van Huyck, Battalio, and Beil (1990). Here, the parameters in equation 1 are set such that a=0.10, b=0.05, and a constant of 0.50 is added such that all payoffs are strictly positive. This equation creates seven pareto ranked equilibria on the main diagonal of the matrix. This is true for both payoff tables I will use. These equilibria require participants to coordinate on the same action as the group member who chooses the minimum action. So a participant's mutual best response to any action is to play the same action as the minimum, resulting in one of the 7 equilibrium points.

The treatment conditions use a Loss table where the payoffs are framed as losses. To construct this table, I subtract \$1.35 from the above Gain table. In the Loss table that follows, Table 3.2, the

[2]Much of the structure for this experiment is similar to Brandts and Cooper (2006), but truncated for 20 periods instead of 30. The Gain table is similar to the one in Van Huyck, Battalio, and Beil (1990), but adjusted for appropriate payment given the length of the experiment.

[3]Appendix D contains the full experimental instructions with the payoff quizzes used.

Table 3.1. Gain Table

		Smallest Value of X Chosen						
		7	6	5	4	3	2	1
Your	7	0.85	0.75	0.65	0.55	0.45	0.35	0.25
Choice	6	-	0.80	0.70	0.60	0.50	0.40	0.30
of	5	-	-	0.75	0.65	0.55	0.45	0.35
X	4	-	-	-	0.70	0.60	0.50	0.40
	3	-	-	-	-	0.65	0.55	0.45
	2	-	-	-	-	-	0.60	0.50
	1	-	-	-	-	-	-	0.55

variation between payoffs is identical to the Gain table. Participants are paid \$1.35 in each round, in addition to their payoff from the Loss table. The Loss and Gain tables are equivalent in terms of the payoff a participant receives for each round. Participants playing under the Loss table are informed “Your payment for the round will be the amount determined from the table plus \$1.35” so as to avoid any deception.

3.3.1 Treatments

For the baseline treatment, participants play 10 rounds under a Gain table in part 1. For part 2, participants play another 10 rounds with the exact same payoff table. The Gain table is Table 3.1. The baseline is used to verify that reliable coordination failure is replicated with my subject pool. The treatment conditions are constructed to assess the dynamics of behavior under the loss frame. Two treatments are used to test this. First is the Gain-Loss treatment. Participants play the minimum effort game under the Gain table for 10 rounds in part 1, and then switch to the Loss table for 10 rounds in part 2. This allows me to assess how the loss frame affects decisions when it is introduced in the face of likely coordination failure.

The Loss-Gain treatment is the reverse of the Gain-Loss treatment. In part 1, participants play for 10 rounds under the Loss table, and then switch to the Gain table for 10 rounds in part 2. The dynamics of initially being exposed to losses, may be different than when coordination failure has been achieved and losses are introduced. In games like this the initial action matters a lot

Table 3.2. Losses Table

		Smallest Value of X Chosen						
		7	6	5	4	3	2	1
Your	7	-0.50	-0.60	-0.70	-0.80	-0.90	-1.00	-1.10
Choice	6	-	-0.55	-0.65	-0.75	-0.85	-0.95	-1.05
of	5	-	-	-0.60	-0.70	-0.80	-0.90	-1.00
X	4	-	-	-	-0.65	-0.75	-0.85	-0.95
	3	-	-	-	-	-0.70	-0.80	-0.90
	2	-	-	-	-	-	-0.75	-0.85
	1	-	-	-	-	-	-	-0.80

since "learning dynamics are... history-dependent, so people's initial responses influence limiting outcomes..." (Crawford 2016). Ultimately, I want to assess the dynamics of framing payoffs as losses. So even though participants may be more sensitive to the initial exposure of the losses condition, it is an important element to include since I am trying to establish regularities when payoffs are framed as losses. Table 3.3 shows the experimental design.

In addition to looking at explicit decision making, I will need to look at selection principles that may guide the decisions of participants. A variety of selection principles in games with multiple pareto-ranked equilibria emerge from the extant literature. If a unique payoff dominant equilibrium is salient, then participants should be able to coordinate on the payoff dominant equilibrium (Van Huyck, Battalio, and Beil 1991). However, payoff dominance has not been successful in explaining behavior in the minimum effort game. This is partly due to the existence of more secure payoffs as the equilibrium moves closer towards 1. Here, the secure equilibrium may be achieved with less risk, but participants will earn less. The inclusion of the loss table allows me to elide consideration of loss avoidance (Cachon and Camerer 2006) as a selection principle. One reason for this is that under the loss table, there is no way to avoid losses since there is no interaction between losses and gains within the payoff table. It is reasonable to consider that precedence may be a salient selection principle here under the loss table. While selecting the payoff dominant equilibrium involves greater risk, as players develop a history of actions, establishing a group precedent makes choosing an action potentially less risky. This learned behavior could result in coordination at a higher equilibrium over time, though I do not see this in my results.

Since individuals are more likely to be more risk seeking in the loss domain, relative to the positive domain (Kahneman and Tversky 1979), we should see an improvement in team coordination. In addition, as participants play repeated rounds in static groups, they generally learn to coordinate better. This is due to learning effects from repeated play, which generally increases consistency of coordination on one of the pareto ranked equilibria. Because losses suggest better coordination, and learning effects suggest more consistent coordination, I expect participants to be able to coordinate at the payoff dominant level by the 10th round in the loss frame. This should result in the payoff dominant action being played more frequently because it is more attractive to participants, even though it may be more risky to choose. To test this, I use two hypotheses, one for each treatment. Hypothesis 1 is for the Gain-Loss treatment, and Hypothesis 2 is for the Loss-Gain treatment.

Hypothesis 1: Participants will achieve coordination failure by period 10. By period 20, participants will be able to improve their coordination level above the secure equilibrium. This treatment will have higher coordination levels than the baseline in part 2 when the loss frame is introduced. It is expected that part 1 in the Gain-Loss treatment will be identical to the baseline since they are both played under the same Gain table.

Previous literature suggests that with repeated play under a standard Gain table, like I use here, reliable coordination failure should be achieved by the 10th round (Van Huyck, Battalio, and Beil 1990, Brandts and Cooper 2006, Devetag and Ortmann 2007, Hamman et al. 2007).

Hypothesis 2: Participants will be able to coordinate on an equilibrium higher than the risk dominant equilibrium by period 10. Coordination will remain at a similar level in part 2. This treatment will have higher coordination levels than the baseline in part 1 and part 2.

Here I expect some spill over effects into part 2. Participants should end up at a high level of coordination by the end of part 1 and should be able to maintain some amount of improved coordination through the end of part 2.

Table 3.3. Experimental Design

Treatments	First 10 Rounds	Second 10 Rounds	Sessions	Participants
Baseline	Gain Table	Gain Table	6	24
Gain-Loss	Gain Table	Loss Table	6	24
Loss-Gain	Loss Table	Gain Table	6	24

Participants are in static groups of 4 for 20 periods. The Gain and Loss tables are below. Participants played 10 rounds of the minimum effort game under these tables in the manner specified by the above table

I use a between subjects design for this experiment. The experiment was conducted at the University of Texas at Dallas using the subject pool and facilities of the Laboratory for Behavioral Operations and Economics (LBOE). Participants were recruited using ORSEE (Grenier, 2003), an online recruitment system. Participants play the minimum effort game under different payoff conditions for a total of 20 rounds, using zTree software (Fischbacher, 2007). Participants receive a $5 dollar show-up fee and payment based on their choices, and the choices of their group members in each round. The average payout was $15.89 for a session which took no more than 30 minutes. Twenty-four subjects are used for each treatment. Participants were either majoring in business or related fields (supply chain management, information technology and management, etc.), computer science, or engineering. The average GPA of participants is 3.49. See Table C.2 in Appendix C for a full set of summary statistics for participants. At the end of the experiment, I elicited participant's risk preferences using an Eckel and Grossman (2002) measure.[4]

3.4 Results

Overall, framing payoffs as losses results in improved coordination relative to the baseline. The baseline results in reliable coordination failure throughout parts 1 and 2. This replication is unsurprising, given that studies commonly confirm this is the case absent any interventions (Van Huyck, Battalio, and Beil 1990, Brandts and Cooper 2006, Devetag and Ortmann 2007, Hamman

[4]Specifically, I used the measure from Dave et al. (2010)

et al. 2007). Both treatment conditions see better group coordination. The Gain-Loss treatment has higher levels of effort throughout the experiment, and the Loss-Gain treatment's higher coordination levels are mainly driven by the significant improvement once participants shift to the gain frame. While this is not exactly in line with the hypotheses presented earlier, these results add to a large literature that shows framing payoffs as losses results in positive behavioral effects in experimental and field settings (Kahneman and Tversky 1979; Tversky and Kahneman 1981 and 1991; Bateman et al. 1997; Hossain and List 2012; Levitt et al. 2016). In the remainder of this section I will discuss treatment effects, both in the shift between gain and loss frame, and comparing the treatments to the baseline condition with both simple parametric tests and regressions. I will then address some concerns about heterogeneous group behavior and the role of risk preferences.

Is coordination failure achieved in the baseline treatment? Figure 3.1 shows the average minimum action by treatment and Figure 3.2 shows average choice by treatment. Coordination failure is achieved in the baseline condition by period 10. The average minimum effort is slightly above one in period 10, and by period 20 it increases to almost two. Having a history of play as a static group can lead to better coordination over the long term, so this minor increase is not terribly surprising (Van Huyck, Battalio, and Beil 1990). To test if the choices in part 1 were different than in part 2, I use a Kolmogorov-Smirnov test to see if the distribution of choices is different. For the baseline, part 1 is not statistically different from part 2 (p=0.699).[5]

The Gain-Loss treatment does not result in coordination failure in the first 10 periods. It is clear from looking at Figures 3.1 and 3.2 that participants are coordinating at a higher effort level under the gain condition in this treatment than in the baseline. While this may not match the baseline behavior as expected, if the loss frame results in similar behavior, then these groups may end up coordinating at a higher level regardless of the payoff table. Periods 11-20 show exactly that, after achieving a coordination level between 3 and 4 in the first 10 periods, the minimum action is stable

[5] As I will show later, this slight increase in minimum behavior is due to one group coordinating at a higher level in the last 10 periods.

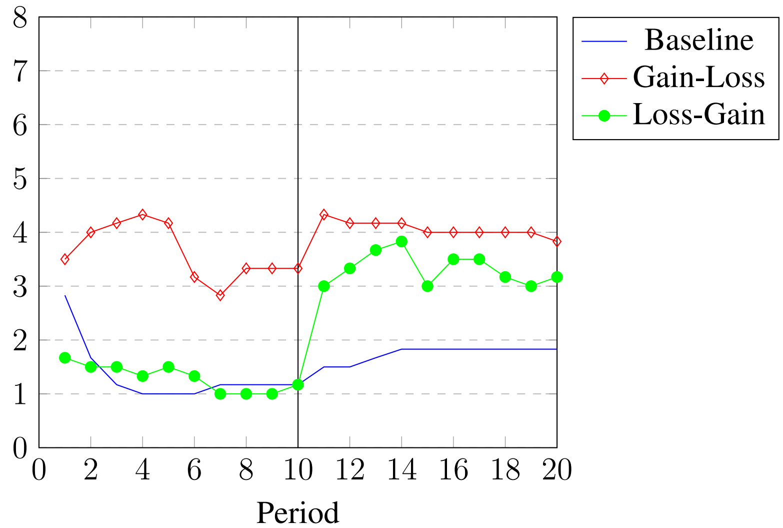

Figure 3.1. Average Minimum Action by Treatment

near 4.[6] So even though there is a high level of coordination in part 1, there is overall improvement in coordination in part 2.[7]

The Loss-Gain treatment does not behave as expected in hypothesis 2. Participants actually achieve coordination failure, or come very close to it, in part 1. This suggests that even in the face of unavoidable losses, participants may not be willing to take riskier actions to avoid steeper losses. Beginning in period 11, participants are able to coordinate at a higher level and do so through period 20, resulting in a consistent coordination level between 3 and 4.[8] It is important to note here that even with heterogeneous response, there is no reason to believe that framing payoffs as losses will lead to payoff dominant behavior.

[6]Here, the distribution of choices in parts 1 and 2 are statistically different at the 95% level (p=0.029).

[7]One explanation for why participants are coordinating at a higher level in the baseline in part 1 is that in the Gain-Loss treatment, subjects tend to have a higher tolerance for risk. I will address this in more detail in Appendix C.

[8]This improvement in part 2 is statistically significant at the 99% level ($p<0.001$). These results, in both the Gain-Loss and Loss-Gain treatments, are largely due to heterogeneous behavior between groups. Heterogeneity is discussed in Appendix C.

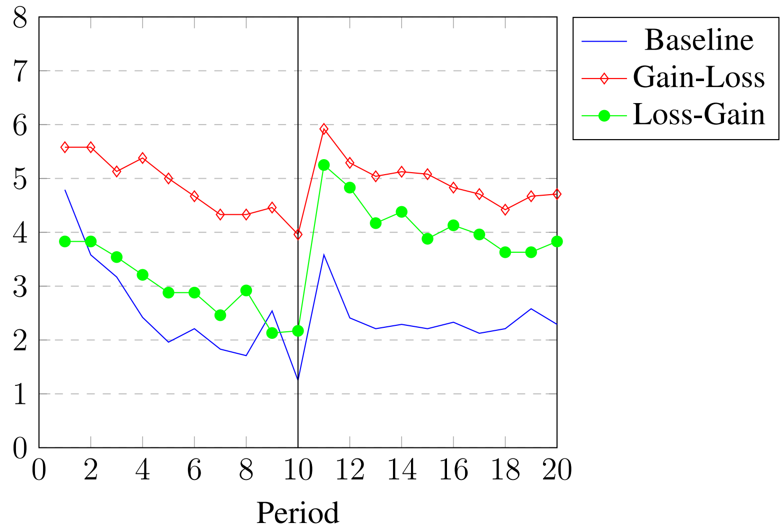

Figure 3.2. Mean Choice by Treatment

3.4.1 Treatment Effects

Ultimately, I want to assess how participants choices changed between parts 1 and 2, and among the treatments. An important aspect of the behavior in moving from part 1 to part 2 is a steep increase in coordination in period 11. In every treatment there is a significant improvement in coordination from period 10 to period 11. This is a restart effect. Participants were stopped after part 1 and then, after receiving instructions, began part 2. Using a simple t-test, it is clear that there is significant improvement in the baseline ($p<0.001$), Gain-Loss ($p<0.001$), and the Loss-Gain treatment ($p<0.001$) at the 99% level. This significant improvement is easy to see in Figure 3.2. I will control for this difference in the regressions below.

To assess the role of the different treatments, I will compare them with the baseline over 20 periods and then separately in parts 1 and 2. This will allow us to see if there are some overall effects that result in better coordination in the treatments, and additionally, how the individual dynamics fare under different payoff matrices. Again, I use a Kolmogorov-Smirnov test to see if there is any equality between the overall distribution of choices made by participants. There is significantly better coordination in the Gain-Loss treatment when compared to the baseline ($p<0.001$). This is not a surprising result to find, since it is clear that there is better coordination in this treatment looking at Figures 3.1 and 3.2. The Loss-Gain treatment results in significantly better coordination compared to the baseline as well ($p<0.001$). This overall improvement is due to the considerable increase in coordination level in part 2 of this treatment.

Next I will look at the differences in part 1. The Gain-Loss treatment results in better coordination when compared to the baseline. This difference is significant at the 99% level ($p<0.001$). This result is somewhat odd, since we should expect participants to behave similarly when playing under the same payoff table. This result will be discussed more when I explore some of the heterogeneous response to the treatments and risk preferences. When looking at Figure 3.2, it is not immediately clear that there would be any significant differences between the Loss-Gain treatment and baseline in part 1. However the distributions are statistically different at the 90% level ($p=0.062$). So in part 1 participants are choosing marginally higher actions under the loss frame, when compared to the baseline. Figure 3.2 presents a clearer picture that both the Gain-Loss and Loss-Gain treatments will be significantly different than the baseline in part 2. Both are significantly different at the 99% level ($p<0.001$). This shows that at the very least, changing the payoff conditions results in better coordination and not sustained failure as in the baseline. Again, there is no evidence of payoff dominant coordination under the loss frame, though there is improvement relative to the baseline.

Using an ordered probit model, I will assess the effect that each of the treatments, and other covariates, had on both choices that participants made, and the group minimum action[9]. Table 3.4 presents these four regressions. Columns 1 and 2 use the group minimum as the dependent variable, and columns 3 and 4 use an individual's choice of action as the dependent variable. The results are similar using a random effects regression, or other types of specifications or clustering

[9]Another specification commonly used is random effects. I performed random effects estimation for all specifications used here. The results of the random effects and ordered probit models are very similar, so I chose not to include the random effects results.

mechanisms; statistical significance is maintained in the same manner as Table 3.5 in alternate specifications. Since the goal of this regression is to model the probability of choosing actions 1-7 in the minimum effort game, an ordered probit is appropriate. Minimum is a variable equal to an integer of 1 through 7. It is the minimum action of a group in a particular period. Choice is a participant's choice of action, again an integer from 1-7, in the period it was chosen.

Part2 is an indicator variable that is equal to 1 if the selection was made in Part 2 (Periods 11-20), and equal to zero otherwise. This coefficient will show if there is any improvement in the baseline treatment from part 1 to part 2. Gain-Loss is equal to one if a participant was in the Gain-Loss treatment, and zero otherwise. Loss-Gain is equal to one if participants were in the Loss-Gain treatment and zero otherwise. Part2*Gain-Loss is an interaction variable equal to one if a participant is making a decision in Part 2 in the Gain-Loss treatment. Part2*Loss-Gain is and indicator equal to one if a a participant selects an action in Part 2 in the Loss-Gain treatment. These indicator and interaction variables will help me tease out the treatment effects in each treatment and each part. This is a Difference-in-Differences analysis where the first difference is whether the decision was made in part 1 or 2, and the second difference is the treatment. Period 11 is an indicator equal to one if a participant's decision was in Period 11 and zero otherwise. This is to control for the statistically significant increase in choices between Period 10, and Period 11 due to restart effects. A variety of covariates are included to help control for other effects that the previous variables may not capture. Age is a participant's age in years. Female is an indicator equal to 1 if a participant is a woman, and zero otherwise. GPA is a participant's grade point average. Risk Preference is equal to an integer of 1-6, depending on which of the 6 risk gambles a participant chose at the end of the experiment. The lower the integer chosen, the more risk averse a person is.

Table 3.4 allows me to verify whether there was any significant improvement in coordination from part 1 to part 2. In terms of the group minimum, there is marginally significant improvement at the 90% level in the baseline. This is surprising, since we would normally expect that coordination failure would continue consistently through part 2. This result is driven by a single group

Table 3.4. Effect of Improvement in Part 2 on Minimum Actions and Choices (Ordered Probit)

	Minimum	Minimum	Choice	Choice
Part2	0.447*	0.434*	-0.083	-0.160
	(0.233)	(0.241)	(0.189)	(0.199)
Gain-Loss	1.370***	1.366***	0.968***	0.934***
	(0.189)	(0.185)	(0.177)	(0.179)
Loss-Gain	-0.022	-0.027	0.186	0.200
	(0.189)	(0.207)	(0.145)	(0.161)
Part2*Gain-Loss	-0.152	-0.137	0.203	0.223
	(0.310)	(0.315)	(0.237)	(0.242)
Part2*Loss-Gain	0.884***	0.959***	0.652**	0.677**
	(0.336)	(0.347)	(0.283)	(0.290)
Period 11		0.049		0.645***
		(0.111)		(0.149)
Age		-0.053		-0.016
		(0.041)		(0.031)
Female		0.234		0.065
		(0.193)		(0.142)
GPA		-0.040		-0.096
		(0.374)		(0.277)
Risk Preference		0.135**		0.103**
		(0.054)		(0.045)
N*T	1440	1440	1440	1440
N	72	72	72	72
Ps. R^2	0.101	0.119	0.050	0.062
ClustVar	Individual	Individual	Individual	Individual

*Dependent variable is either Choice or Minimum action. Column titles correspond to the dependent variable for each regression. Results include all 20 periods, and an indicator variable is used to assess the effect of improvement in part 2. ***P<0.01, **P<0.05, *P<0.10. Clustering is at the individual level and results are similar when using individual or session level clustering. Random effects estimation produces similar results.*

in the baseline treatment, which is able to pull the average minimum action up in part 2. I will discuss this result in the next section. When looking at participant choice, there is no significant improvement from part 1 to part 2. To test if there was any improvement in the treatments in part 2, I need to test if the combination of the Part 2 and Part2*Gain-Loss (and the Part2*Loss-Gain) variable are jointly equal to zero. If I reject that the coefficients are jointly equal to zero, then there is significant improvement in the treatment in part 2. If I am unable to reject the hypothesis, then there is no significant improvement.

When the group minimum is the dependent variable, the Gain-Loss treatment shows no significant improvement from part 1 to part 2 ($p=0.169$). This is contrary to Hypothesis 1 which suggests improvement in part 2 in the face of losses. However, the Loss-Gain treatment results in a significant improvement at the 99% level ($p<0.001$). Implicitly, Hypothesis 2 suggests that coordination in part 2 in this treatment would be lower than part 1. This is not what we see here. When looking at individual choices as the dependent variable, the Gain-Loss treatment results in no significant difference again ($p=0.667$). The Loss-Gain treatment still results in a significant improvement in coordination, though at the 95% level here ($p=0.021$). The results from all of these regressions are similar to what we would expect when looking at Figures 3.1 and 3.2. The baseline results in coordination failure, which is effectively sustained through part 2. The Gain-Loss treatment begins with a high level of coordination under the Gain table and maintains it even in the face of framing payoffs as losses. The Loss-Gain treatment gets very close to coordination failure in part 1, and when the Gain table is introduced participants significantly improve coordination. Table 3.4 assesses the effect of improvement from part 1 to part 2, but to test the treatment effects relative to the baseline, we turn to Table 3.5.

Result 1: Regardless of whether the group minimum or choices are used as the dependent variable, the results are effectively the same. The Gain-Loss treatment has no significant improvement from part 1 to part 2. The Loss-Gain treatment sees a low level of coordination in part 1, and significant improvement in part 2.

Table 3.5. Treatment Effect on Choices (Ordered Probit)

	(1)	(2)	(3)	(4)	(5)	(6)
Gain-Loss	1.067***	1.349***	1.197***			
	(0.185)	(0.287)	(0.192)			
Loss-Gain				0.237	0.907***	0.556***
				(0.154)	(0.260)	(0.163)
Age	-0.006	0.091	0.044	-0.030	-0.026	-0.025
	(0.057)	(0.081)	(0.053)	(0.024)	(0.054)	(0.031)
Female	0.014	0.238	0.122	0.140	0.161	0.140
	(0.152)	(0.267)	(0.168)	(0.152)	(0.260)	(0.157)
GPA	-0.072	-0.965*	-0.532	0.127	0.093	0.100
	(0.360)	(0.557)	(0.366)	(0.237)	(0.437)	(0.256)
Risk Preference	0.018	0.090	0.054	0.078**	0.237***	0.150***
	(0.018)	(0.077)	(0.058)	(0.037)	(0.080)	(0.046)
N*T	480	480	960	480	480	960
N	48	48	48	48	48	48
Ps. R^2	0.063	0.101	0.077	0.009	0.071	0.030
Periods	1-10	11-20	1-20	1-10	11-20	1-20
Treatment	Gain-Loss	Gain-Loss	Gain-Loss	Loss-Gain	Loss-Gain	Loss-Gain
ClustVar	Individual	Individual	Individual	Individual	Individual	Individual

*Dependent variable is Choice. Columns 1-3 correspond to the Gain-Loss treatment, and columns 4-6 are for the Loss-Gain treatement. Column 1 restricts the sample to part 1, column 2 is for part 2, and column 3 if the overall effect. The column organization is identical for columns 4-6. ***P*< 0.01, ***P*< 0.05, **P*< 0.10. *Clustering is at the individual level and results are similar when using individual or session level clustering.*

Table 3.5 presents regressions that focus on the individual treatment effects over the whole experiment, part 1, and part 2. Each regression only includes the baseline and a single treatment, so I can analyze if these treatments actually constitute an improvement in coordination relative to the baseline. Table 3.5 uses Choice as the dependent variable. Columns 1 through 3 are for the Gain-Loss treatment, and 4-6 are for the Loss-Gain treatment. Column 1 restricts the analysis to periods 1-10, column 2 is for periods 11-20, and column 3 presents the overall treatment effect. This organization is analogous for columns 4-6. Table C.3 in Appendix C presents the results in a similar manner, but with the group minimum as the dependent variable.

Columns 1-3 in Table 3.5 show that the Gain-Loss treatment always results in significantly better coordination than the baseline. None of the other coefficients are really of note, except for a marginally significant GPA coefficient in part 2. This suggests that an individual with a higher

GPA would be more likely to select a lower action in periods 11-20. Participants achieve a high level of coordination in part 1, and maintain it through part 2 in this treatment. The Loss-Gain treatment results in significant coordination improvement in part 2 under the Gain table. This improvement in part 2 carries over and causes the overall treatment effect to be highly significant, as we see in column 6. The marginally significant difference in part 1 I found in an earlier section disappears when accounting for other factors. One factor that is significant no matter the way the periods are parsed is a participant's risk preference. The significance is stronger in part 2 and overall, but the coefficients show that a participant with a higher tolerance for risk is more likely to choose a higher action in the Loss-Gain treatment. This is likely because participant want to recoup perceived losses in part 1 by coordinating at a higher level in part 2. The majority of results from earlier parametric tests remain, even when accounting for covariates which may affect a participant's decision in these regressions. This is true regardless of whether the group minimum or individual choices are used as the dependent variable.

Result 2: Overall, framing payoffs as losses leads to a modest improvement in coordination. With a history of play, framing payoffs as losses can lead to more consistent coordination at a higher level. When the loss frame is introduced first, strategic uncertainty leads individuals towards coordination failure. However, upon shifting to a gain frame, coordination significantly improves.

Table C.3 (in Appendix C) presents the treatment effects on the group minimum action. After accounting for covariates, the prominent results from section 3.4 remain. In part 1, part 2, and overall the Gain-Loss treatment results in significantly better coordination than the baseline. Surprisingly there is a significant gender effect in each of the part 1, part 2, and overall in the first 3 columns. These columns pool the baseline and Gain-Loss treatment. Here women only account for 9/48 (18.75%) of the sample. This result says that women are more likely to be in a group that has a higher minimum action. They do not seem to be driving the higher minimum action though, otherwise this gender effect would be present when choice was the dependent variable. The Loss-Gain results mirror what we see in Figure 3.1. There is no difference in part 1, and significant differences in minimum actions emerge in part 2. This increase in minimum in periods 11-20 is large enough that the overall effect shows significantly improved coordination. In part 1 the women seem to be associated with higher minimum actions, but this effect does not carry through into part 2. As with the results in Table C.1, we see that risk preferences are significant in columns 4-6. So a higher group minimum (or individual choice) is associated with a stronger preference for risk. The effect of risk on these decisions is discussed at length in Appendix C, as well as some considerations about heterogeneity of results.

3.5 Conclusion

Simply put, coordination is difficult. Extant research develops and contextualizes this concept with a variety of coordination tasks that assess the role financial incentives can play in group coordination. Due to its structure, the minimum effort game is one of the most difficult coordination games. I test the effectiveness of framing payoffs as losses on coordination in the minimum effort game. I find that framing payoffs as losses leads to improved overall coordination. More specifically, when introducing the loss frame with a history of play between groups can lead to modestly improved coordination. When the loss frame is introduced first, the increased strategic uncertainty absent a history of play leads to coordination failure. Once players shift to the gain frame, they are able to significantly improve coordination. This contributes to the extant literature on loss framing and its effectiveness by extending this conclusion to the minimum effort game. It is plausible to consider that the results could have been more pronounced given a stronger frame.

Considering the effect of loss framing on coordination issues has important implications for businesses, partly because individuals already think in a manner similar to loss framing. A simple example of this is when it is required, involuntarily, to work overtime at a job. Instead of thinking about potential additional pay for working overtime (if they are paid by the hour) the worker considers the opportunities they are missing out on in their lives by being at work. At the very least the thought of not being at work is appealing to the decision maker. So when thinking about this problem, the need to work overtime is now thought of as lost leisure time by the worker instead of the potential to earn more money. Common instances like this, where loss framing is salient, make it essential to understand how this affects behavior by economic agents.

CHAPTER 4

THE EFFECT OF AN INCREASED MINIMUM SMOKING AGE ON SMOKING AND ALCOHOL CONSUMPTION: EVIDENCE FROM THE BEHAVIORAL RISK FACTOR SURVEILLANCE SYSTEM

4.1 Introduction

Smoking remains the leading cause of preventable disease and death in the United States. The use of tobacco results in over 480,000 premature deaths and $289 billion dollars in direct health care expenditures and productivity losses each year (US Department of Health and Human Services (USDHHS), 2014). Some version of these preceding sentences appear in every US Surgeon General's smoking behavior report released over the last few decades. Certainly the danger of tobacco use, especially cigarettes, which remains the most commonly used tobacco product in the United States (Centers for Disease Control (CDC), 2014), is both well known and documented. Despite the introduction of a variety of effective policies which seek to restrict smoking behavior, or make it more costly, it still remains a very significant health issue for not only the United States, but countries around the world. There does seem to be some mildly encouraging news with respect to the preponderance of smoking behavior. From 2005-2013, smoking prevalence decreased from 20.9% to 19.8% in America. This is encouraging, but still amounts to 42.1 million adults who smoke cigarettes, which is slightly less than one sixth of the population of the United States (CDC Morbidity and Mortality Weekly Report, 2014).

One of the most common policies to restrict youth smoking behavior is setting a minimum age for smoking and purchase of cigarettes. These policies are generally dictated at the state level. Since the Synar Amendment to the 1992 Federal Alcohol, Drug Abuse, and Mental Health Reorganization Act, the minimum age for which it is legal to purchase cigarettes in America must be at least 18. By 1994, all 50 states had adopted this policy of 18 as a minimum age and some states used higher minimum ages like 19 or 21 (USDHHS, 1994). Despite the prevalence of these restrictions for over two decades, we still know little about the true effect of these policies on smoking and other risky behaviors. The main reason for this is that the vast majority of states observe a minimum smoking age of 18. Myriad life and situational changes at age 18 obfuscate potential estimates of the effect of this policy. Some states do observe higher minimum ages like 19. Since there are fewer life changes occurring at age 19, this is a unique opportunity to cleanly estimate the effect these minimum smoking age policies have on cigarette smoking and alcohol consumption.

Previous work shows that turning the legal minimum drinking age has spillover effects onto other risky behaviors like smoking, hard drug, and marijuana consumption (Yoruk and Yoruk 2011; Deza 2015). This suggests complementarity between these behaviors. It is possible that if an individual is more likely to smoke, that they may be prone to other risky behaviors like drinking underage as well. This is the first paper, that I am aware of, that seeks to address the spill over effects of these policies on alcohol consumption. I assess the effect of the discontinuous decrease in the cost of smoking, as a result of turning the legal age of 19, on smoking outcomes and alcohol consumption. I find that having a minimum smoking age of 19, in states who observe that policy, has no effect on cigarette use. This does not allow me to attribute any type of relationship between the age 19 policy and alcohol consumption.

4.2 Background and Literature Review

Earlier literature tried to exploit local variation in minimum purchase ages to assess the behavioral effects of the policy. Wasserman, et al. (1991) attempt to estimate the effect of minimum purchase age laws for tobacco on cigarette smoking behavior; they find that there is no significant effect of these laws. Chaloupka and Grossman (1996) arrive at a similar conclusion when analyzing a litany of complimentary policies to minimum purchase ages, such as signage requirements, retailer licensing provisions, and restrictions on vending machine sales. However, much of the analysis is restricted to local level data for either individual towns or regions using simple variation in the minimum smoking age among states.[1] Most recently Yoruk and Yoruk (2014) assess the effect of minimum purchase laws on smoking behavior using a regression discontinuity (RD) design. Even accounting for states and counties for which minimum purchase ages which exceed 18, the authors find no significant effect of these laws on smoking behavior, regardless of age.

These results showing no effect are not terribly surprising since it is incredibly difficult to disentangle what is driving behavioral changes at age 18. At this age, myriad legal and situational changes occur simultaneously. An individual is likely to be finishing high school (or could have already entered the workforce after dropping out of high school), is old enough to vote and buy cigarettes in most states (in addition to losing their legal status as a minor in most states), beginning to enter college or the work force, and leaving the home of their childhood. Since these previous studies have found no effect of minimum purchase age on smoking behavior, and the fact that a number of changes which occur simultaneously at age 18 have probably, with good reason, steered researchers away from diving headfirst into this type of analysis.

Yet, there does exist variation among states in the minimum age of purchasing cigarettes. Utah, Alaska, Alabama, and New Jersey (after 2004) all have minimum smoking ages of 19; from 1994 until 2002 Pennsylvania's minimum smoking age was 21, it is now 18. Yan (2011) employs a RD design to investigate Pennsylvania's higher minimum purchase age on smoking behavior of young mothers. He finds that this higher minimum purchase leads to a decrease in the number of cigarettes consumed for young mothers and better health outcomes for their babies. There are a variety of counties in Massachusetts, Hawaii, and New York for which the minimum smoking age is either 19 or 21, but since my data is at the state level, I can not account for this variation. I doubt that including these counties would substantially alter the main results of this paper. It may be difficult to estimate the true effect of the minimum smoking age in counties who have 21 as the minimum age since it is likely that 21 is the minimum legal drinking age. This would lead us back

[1] For further information on papers which review some of the research dealing with the minimum purchase and smoking age, look at Chaloupka and Grossman (1996) and Chaloupka and Warner (2000).

to a similar situation as before, where simultaneous changes make it difficult to cleanly estimate the real impact of a a minimum smoking age policy. In this same vein, it is important to note that in Alabama an individual remains a minor until such time as they reach the age of 19. This will be an aspect of the data which will be addressed later on in the paper, as it provides both a source of potential consternation, and an interesting chance to look at how the combination of becoming old enough to purchase cigarettes and losing minor status can affect alcohol consumption.

Minimum smoking ages are some of the most obvious policies used to restrict smoking by young people, but there are a variety of other policies designed to reduce smoking that states and municipalities may employ. One of the most common ways to reduce smoking is by increasing the purchase price of cigarettes through taxation. "The most commonly used methods of taxation include specific excise taxes, value added and other ad-valorem taxes, and import duties..." (Chaloupka and Warner 2000). Overall, an increase in cigarette taxes, regardless of the form of the tax, leads to a decrease in smoking, especially among teens, who are more sensitive to price changes.[2] Another common policy is restricting the location that an individual can smoke. These laws restrict smoking in most indoor buildings and now have been expanded to include restaurants and many bars. These restrictions are generally successful in reducing not only the number of smokers, but also average cigarette consumption.[3]

I exploit the discontinuous decrease in the cost of smoking when an individual turns 19. For these other policies to confound inference, they would have to take effect at age 19 in the same manner that the minimum purchase age does. Since policies like excise taxes or location restrictions are not based on an individual's age, there is little expectation that this would affect the results at the individual level profoundly. Rather if there is an effect it would be in relation to how these

[2](Lewit et al. 1981; Lewit and Coate 1982; Warner 1986; Harris 1987; The General Accounting Office 1989; Coalition on Smoking or Health 1994; Moore 1996; Chaloupka 1998; Evans and Ringel 1999)

[3](Wasserman et al. 1991; Chaloupka 1992; Chaloupka and Saffer 1992; Keeler et al. 1993; Chaloupka and Wechsler 1995; Chaloupka and Grossman 1996; Chaloupka and Pacula 1998; Townsend 1998; Bardsley and Olekalns 1998; Evans et al. 1999; Ohsfeldt et al. 1999)

policies may affect social acceptability of smoking, which could lead to lower rates of smoking both before and after age 19. If this is the case, then it is possible that my results are a lower bound for the impact of this policy. This is discussed further in sections 4.4 and 4.5.

Teens who smoke, especially underage, may be more likely to engage in other risky behaviors like consuming alcohol. It is common to look at spill over effects like this in the literature which assesses how minimum drinking age laws influence risky behaviors. Some of these papers find complementarity between alcohol and cigarette smoking (Wagenarr and Toomey 2002). This complementarity suggests that those who smoke cigarettes may be more likely to try alcohol at a younger age. To see how the literature views spill over effects, I turn to papers on the minimum drinking age and its influences on youth behavior.

A wealth of papers exist which consider the various effects of extant minimum drinking age laws. Earlier studies exploit variation in the 1970's and 1980's in the minimum drinking age law across different states to assess its impact.[4] Since the possibility exists that states with different minimum drinking ages during these times may exhibit differences that are unobserved, and are related to the drinking habits of young adults, the literature has shifted away from these research designs and toward procedures like the RD design which allow for cleaner causal estimation. This methodology has resulted in a literature which estimates different causal impacts of minimum drinking age laws on a variety of outcomes, like their effect on drinking behavior of high school seniors (Carpenter et al., 2007) and alcohol related traffic injuries and fatalities (Carpenter and Dobkin 2009). In addition to assessing the impact of these laws on alcohol specific outcomes, more recent literature has assessed spill over effects of these laws into the realm of smoking, marijuana, and hard drug use. Deza (2015) looks at the effects of alcohol on the consumption of hard drugs using an RD design. All measures of alcohol consumption provided in the National Longitudinal Survey of Youth 1997 cohort exhibit a discontinuous increase at age 21 by exploiting the exogenous

[4]See Wagenarr and Toomey (2002) for a literature review on minimum drinking age laws which summarizes a variety of these earlier works.

decrease in the cost of alcohol that occurs when one becomes able to drink legally. She finds small decreases in hard drug consumption and the probability of initiating hard drugs (though no cessation in terms of the intensity of use of hard drugs) due to the increase in alcohol consumption spurred by the minimum age laws. Similarly, Yoruk and Yoruk (2011) look at the impact of these minimum legal drinking age laws on smoking and marijuana use, as well as measures of alcohol use. They find that these laws have a variety of statistically significant effects on the use of alcohol, no statistically significant effects for smoking, and a positive and statistically significant impact on marijuana use. While some papers like Yoruk and Yoruk (2014) look at the effect of minimum smoking age laws on smoking behavior, another approach is to look at the effect of these laws on alcohol consumption, in addition to smoking behavior.

The intent of this paper is to answer the question of what the effect of this discontinuous change in the cost of smoking has on consumption of cigarettes and alcohol. To do this, I will exploit variation in the minimum smoking age in the aforementioned states to see what the effect of an increased minimum smoking age (age 19) is on alcohol consumption and smoking behavior. One way to consider this type of restriction on purchase and use is that it increases the cost of smoking prior to the minimum age. If the states appropriately penalize offenses by both minors purchasing cigarettes and vendors selling them, then one would expect this policy to be an effective deterrent to minors undertaking smoking behavior. This was the purpose of the Synar amendment; it established a minimum mandatory smoking and purchasing age and set up a framework for which the federal government has some oversight to confirm that states are indeed following these practices of restricting tobacco sales to minors. This amendment came in the light of a variety of studies that showed how compliance with these minimum purchase and smoking laws was quite poor in some areas of the country (USDHHS 1994). Some research has shown that if these policies are strongly enforced, they can be an effective deterrent to smoking before the minimum age (Chaloupka and Warner 2000).

Certainly, among these states with higher minimum purchasing ages there is some heterogeneity in enforcement. Alabama is fairly forgiving with their policy and often will sentence individuals to community service instead of harsher penalties like steep fines or jail time (Alabama Act 97-423). On the other hand, Utah is very strict with their enforcement of these laws. An individual receives a Class C misdemeanor citation if caught possessing tobacco products (Title 76 Utah Criminal Code). While it is possible that an individual could receive only community service as a sentence for receiving a Class C misdemeanor, they will have to likely expend legal fees to do so. With this methodology, I will be able to analyze the direct and indirect effects of cigarette use. Evaluating the effectiveness of these minimum age limits on smoking and their effects on drinking behavior is essential in creating a thorough assessment of the efficacy of policies like this. The rest of the paper will proceed as follows: section 4.3 discusses data, section 4.4 the methodology of the regression discontinuity design I use, section 4.5 shows the results, and section 4.6 concludes and discusses what the results mean in a larger context.

4.3 Data

I use data from the Behavioral Risk Factor Surveillance System (BRFSS) from 1994-2014. The BRFSS is an annual, nationally representative cross sectional survey that documents a variety of health and risky behaviors of individuals ages 18 and older. One advantage of this data set is that there are are a large amount of observations, even after reducing the sample to the four states of interest.[5] The data contains an individual's age on the month they were interviewed, which allows for some granularity for the running variable, which is age. Age is imperfectly manipulable, and given the anonymous nature of the study doubt an individual would have incentive to lie about their age or date of birth. I include an individual's state of residence, education level (including whether

[5] I looked at a variety of other data sets prior to using the BRFSS to answer this question. The National Longitudinal Survey of Youth 1997 cohort have the benefit of longitudinal data which more ably allows me to track changes in behavior over time. However the maximum observations for the four states with 19 as the minimum smoking age is about 1100. Other data sets like the Youth Risk Behavior Survey or The National Longitudinal Study of Adolescent to Adult Health do not have sufficient observations over age 19 to effectively estimate an effect of this policy. So, despite the issues with cross sectional data, the BRFSS offers the best opportunity to estimate this effect because I am already looking at a limited sample with states who have a minimum smoking age of 19.

or not they are currently a college student), job status, and other standard demographic information. Since individuals are likely to increase alcohol use in college, this is a potential confounder I will control for.

In terms of outcome variables, I will look at responses to a variety of questions solicited to individuals about their smoking behavior and alcohol use. The alcohol measures include: whether or not alcohol was consumed in the past month, whether the respondent engaged in heavy drinking (binge drinking) over the past month, the number of days the respondent had at least one drink, the number of days she had five or more drinks on the same occasion in the past month, the average number of drinks she had on days she consumed alcohol, and the average number of drinks per day she consumed during a one month period, and the number of occurrences where a person drove after consuming alcohol in the past month. These responses allow me to address drinking participation, intensity, and the number of days the individual consumes at least one alcoholic beverage.

There are a variety of smoking outcomes, including: if a respondent has smoked 100 or more cigarettes in their life, whether the respondent currently smokes, if the respondent has tried to quit smoking a day or more in the past year, whether the respondent smoked over the past month, the number of days smoked over the past month, and the share of days a person smoked in the past month. While the focus of this paper is not only on smoking outcomes themselves, I will seek to expand on the results regarding states with a minimum smoking age of 19 in Yoruk and Yoruk (2011). It is possible that the self reported nature of the BRFSS could lead to measurement error. However, the BRFSS is comparable to other publicly available health data sets in terms of accuracy. It is far more likely that any unexpected results would be due to small sample sizes. One further concern about the smoking data is that beginning in 1999, there is a precipitous drop in youth smoking behavior (USDHHS 2014). Because of this, it is possible that there would be some downward bias in my results. If the results were biased in this way it is possible that a discontinuity in smoking behavior may be suppressed in favor of a result of no effect. As I will show in the results section, there is little reason to suspect any precipitous instance of downward bias on the results since the pre-1999 period of my data set shows no effect, similar to the full sample.

4.4 Methodology

The main analysis here is a regression discontinuity research design exploiting the discontinuous decrease in the cost of accessing tobacco at age 19 because of minimum smoking laws in the aforementioned states. The identifying assumption is that observed and unobserved determinants of our outcomes are distributed smoothly across the age 19 cutoff. Since age is a strict cut off this is a sharp RD design. This allows clean estimation of the effects of the minimum purchase age on smoking and drinking behavior. With this assumption, I will be able to analyze the direct and indirect effects of cigarette use. I want to look at the effects of smoking on alcohol consumption. I will proceed using individuals around age 19 in states where the minimum purchase age is 19 to assess the effect of these laws. Since there are only a small number of states with age 19 laws, I will sacrifice external validity of the results for strong internal validity. Many of the same controls will be necessary in this analysis. I will corroborate the identifying assumption by providing some visual aids, in the form of graphs, that show that observed determinants of the outcomes of interest trend smoothly through the age threshold (for each of the three different thresholds). This would affirm that observables trend smoothly through the discontinuity, which implies that unobservables trend smoothly through the discontinuity. I will provide falsification tests that look at ages 20 and 22, to assess whether the results will hold only for the age for which there exists a discontinuity.[6] If the results only hold for the ages for which a discontinuity exists, then we are able to say that effects are driven by the discontinuity, and not other factors. There is no significant result at ages

[6] While it would be fortuitous to look at other ages, the BRFSS data includes age 18 as its minimum, so I am unable to use it for falsification tests.

20 or 22 so any effect I find here can be attributed to the discontinuous decrease in the cost of smoking once a person turns 19.[7]

The first stage of the analysis will be to assess the role of smoking behavior at the discontinuity. I will use an OLS model with a first (called model 1), second (called model 2), and third (model 3) order age-centered polynomial without covariates as follows:

$$Y_i = \delta D_i + f^p(\widetilde{b_i}) + \epsilon_i \tag{4.1}$$

Here Y_i is the smoking or alcohol consumption outcome for individual i and $D_i = 1[age \geq 19]$ is an indicator variable if an individual is at least 19 years old at the time of interview. The function $f^p(\widetilde{b_i})$ is a p-th order age-centered polynomial. Here I will interact the age centered variable $(\widetilde{b_i}) = (b_i - 19)$ with the indicator D_i. Since I will only use at most a third order polynomial, this variable will take the form of $\widetilde{b_i}D_i$, $\widetilde{b_i}^2 D_i$, or $\widetilde{b_i}^3 D_i$. Model 2 will include the first order interacted polynomial, as well as the second order one, and Model 3 will include all three interacted variables. To account for the variety of confounding factors with analysis, I will include a specification of these models with covariate controls (x_i). Controls include: race, gender, income level, marital status, the year an individual was interviewed, education level on the interview date, and work status. This specification looks like:

$$Y_i = \delta D_i + f^p(\widetilde{b_i}) + \beta x_i + \epsilon_i \tag{4.2}$$

While many of these covariates in x_i could vary with time, the cross sectional nature of my data does not allow me to assess changes across the time dimension. The δ value will identify the causal effect of lowering the cost of accessing cigarettes when an individual turns 19 on the current outcome Y_i . Since the age interacted polynomial is a function of the age of a respondent centered at the age of 19, this ensures that the value of δ reflects the treatment effect on the outcome(s) in

[7]Full results are in Appendix E.

Y_i precisely at age 19. As before, I will use first, second, and third order age interacted polynomials for the specification with covariates. The final term in equation 2 is ϵ_i which represents the unobserved error from this analysis.

If there is no discontinuity in smoking behavior at the threshold, then I will not be able to attribute any change in alcohol consumption to the minimum smoking age policy. Further, in the presence of no discontinuity at the threshold I will come to the conclusion that the minimum smoking age policy has no effect on smoking behavior. This does not suggest that the policy is ineffective per se. It is possible that those who smoke when they are below the legal age continue to do so undeterred after turning age 19. It is possible that the policy is an ineffective deterrent below age 19, and does not sufficiently suppress underage smoking behavior such that a discontinuity exists. In Section 5 I spend more time discussing the results and their implications. To under gird the assumption that observed (and unobserved) covariates trend smoothly through the discontinuity I will use graphs where the dependent variable is each of the covariate outcomes. This will show that observable traits of individuals used in the analysis trend smoothly through the age 19 threshold, and helps corroborate the assumption that unobserved traits trend smoothly as well. Here most of the demographic characteristics trend smoothly through the age 19 threshold. Any significant result is more likely due to BRFSS sampling variability than actual changes in behavior.[8]

4.5 Results

The focus of this section is to assess the change in consumption of alcohol when the cost of smoking changes at age 19. This means specifically looking at these self reported behaviors at age 19 both in terms of consumption and probability of consumption. The following subsection assesses how smoking behavior changes at age 19, and the successive subsection looks at alcohol. Overall,

[8]Results for demographic characteristics are in Appendix E. The student variable does not trend smoothly through the discontinuity. If there was a significant increase in the number of students in the sample, one might expect that my results could be biased upward. In Appendix E, I present Table E.5, which restricts the sample to only those who are students. The results remain the same, so there is no reason to believe this would be a confounding issue.

I do not find any discontinuity in both smoking and alcohol consumption at age 19. The only significant increase in smoking at age 19 is in response to the question "has the respondent smoked more than 100 cigarettes in their life?" This is not surprising since it is a cumulative measure which we would expect to increase around the age where it becomes legal to smoke.[9] Since there is no change in smoking behavior at the threshold, I am unable to attribute any discontinuous change in drinking behavior to the minimum smoking age policy. There is only one outcome with respect to alcohol consumption that is significant at age 19, whether a respondent drank in the last month.

4.5.1 Smoking Outcomes

Assessing the effect of the minimum age policy on smoking behavior is effectively the first stage of this analysis. I am assessing whether there is a discontinuous increase in smoking in response to the decrease in the cost of smoking once a person turns 19. I have five different outcomes for this analysis: whether a person has smoked 100 cigarettes in their life, if they currently smoke, the number of days they smoked last month, if they have tried to stop smoking for at least a day in the past year, and the share of days smoked last month (which is the number of days smoked divided by 30). The first four charts in Figure E.1 in Appendix E are the proportion of respondents per bin who smoked 100 cigarettes, currently smoke, smoked for a certain number of days, or tried to stop smoking. The final chart is the frequency of smoking, which is the share of days smoked. Each of the charts in Figure E.1 has bins appropriate to the sample and the lines are estimated with a second order polynomial. Bins are determined by using a triangular edge kernel, and bin size varies similarly to the sample size variation in Tables 4.1, 4.2, and E.1.[10]

Are there statistically significant discontinuities in smoking outcomes at age 19 in states where that is the minimum age for smoking? Yoruk and Yoruk (2011) find that in terms of smoking

[9] This is only significant at age 19.

[10] The results presented here are insensitive to a range of bin sizes. I just chose bandwidths which I thought most clearly illustrated the results.

outcomes, there is no statistically significant discontinuity, this is most likely, they posit, due to the small sample size. One benefit of my sample is that it has a considerably larger number of observations. However, even with a more robust sample, I still find no discontinuity in smoking behavior. Table 4.1 shows the coefficient δ (where $Di = 1$ if a person is over 19) for two different specifications with three different age interacted polynomial models. Specification 1 includes year effects, but no other covariates. Specification 2 includes all covariates mentioned in the methodology sections: race, gender, income level, marital status, education level on the interview date, and work status. Model 1 uses a first order age interacted polynomial, Model 2 uses a second order age interacted polynomial, and Model 3 uses a third order age interacted polynomial.

Table 4.1 shows the OLS results of the different specifications and models. For whether a person has smoked 100 cigarettes or not, the result is significant. Individuals who are at least 19 answer that they have smoked 100 or more cigarettes 8.2 percentage points more when using the first order polynomial in model 1. This result is robust to the inclusion of a second and third order polynomial where the estimates show an 7.2 and 14.7 percentage point increase in the proportion of people smoking 100 or more cigarettes. When including covariates in the analysis, the increase in respondents answering yes to this question at or above age 19 is still significant Specification 2 shows that there is about a 11 (Models 1 and 2) percentage point increase in individuals having smoked 100 cigarettes after turning 19. This seems like a logical result, since older individuals will be more likely to have smoked a larger amount of cigarettes because they have been alive longer. However, if that were what was driving this result, I should see a significant value in the falsification tests at age 20 and 22 in Appendix E. I find no significant relationship for the smoke 100 cigarettes variable anywhere but at age 19, suggesting this increase in smoking behavior is driven by the discontinuous decrease in the cost of cigarettes at age 19. The only other significant variable is the number of days a person tried to stop smoking last year in specification 2, model 2. It suggests that a person 19 or older is 28.6 percentage points more likely to have tried to quit smoking in the past year. This result is not robust to other specifications and models. None of the

Table 4.1. Measures of Smoking Participation

	Specification 1			Specification 2		
	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3
Panel A: Has Respondent Smoked 100 Cigarettes in Their Life?						
Over 19=Di	0.072***	0.112***	0.147***	0.110***	0.111***	0.099
	(0.023)	(0.036)	(0.051)	(0.023)	(0.044)	(0.062)
N		5774			3674	
Panel B: Does Respondent Currently Smoke?						
Over 19=Di	0.017	-0.011	-0.018	-0.005	-0.012	-0.031
	(0.042)	(0.072)	(0.122)	(0.057)	(0.095)	(0.153)
N		2002			1326	
Panel C: Number of Days Smoked Last Month						
Over 19=Di	1.119	1.280	2.463	-0.514	-0.470	-0.236
	(1.296)	(2.192)	(3.595)	(1.638)	(2.719)	(4.260)
N		1962			1298	
Panel D: Tried to Quit Smoking in Past Year						
Over 19=Di	0.090	0.101	0.066	0.106	0.286**	0.127
	(0.059)	(0.099)	(0.157)	(0.080)	(0.133)	(0.189)
N		1388			900	
Panel E: Share of Days Smoked Last Month						
Over 19=Di	0.037	0.043	0.082	-0.017	-0.016	-0.008
	(0.043)	(0.073)	(0.120)	(0.055)	(0.091)	(0.141)
N		1962			1298	
Year Effects	Y	Y	Y	Y	Y	Y
Xi	N	N	N	Y	Y	Y

*Each panel title is the outcome variable used for the results presented below it. ***P*<0.01*, **P*<0.05*, *P*<0.10*. Model 1 has first order interaction polynomial. Model 2 has second order interaction polynomial. Model 3 has a third order interaction polynomial. Specification 1 includes year fixed effects, but no other control variables. Specification 2 includes all covariates mentioned in section 4.3.* Xi *indicates the inclusion of covariates.*

other variables are significant, resulting in an overall lack of a discontinuity in smoking behavior at age 19. Since the question presents such a low bar for what constitutes trying to quit smoking, I do not believe this to be a confounding issue. The result is not robust to even the majority of specifications, and trying to quit one day out of the 365 in a year does not mean that it would lead to a change in smoking behavior anyway. This shows that the minimum smoking age policy at age 19 in states that have that policy does not lead to a significant change in smoking behavior at the age of legality, even though turning 19 makes smoking immediately less costly.

It is possible to consider a scenario in which there would be a discontinuity in smoking behavior at age 19 with a larger data set, but it is unclear to me how large it would have to be to see a significant difference, if there is any. However, a result of no effect of these minimum smoking age laws, as I find here, is not terribly surprising when considering the overall trend of smoking behavior, and especially youth smoking behavior, over the past 50 years. The overall trend is a strong decrease in the prevalence of smoking, starting at 42.5% of adults in 1965 to 16.8% in 2014 (USDHHS 2014). In general, teenage smoking frequency has been higher than adult smoking frequency over this time period. Beginning in 1999, there is a very large decrease in the percentage of teenage smoking. This trend is so sharp that by 2014, teen smoking frequency is 15.7% (USDHHS 2014). Given that three-fourths of my data is from this period of precipitous decrease in smoking prevalence among teens, a result of no effect at the threshold is well within the realm of possibility, even with a larger data set than I have here. One concern is that this may lead to downward bias of the results, suggesting my findings may be a lower bound of the policy effect. To assess this I run the same regressions as in Table 4.1 and Table 4.2 with the time period restricted from 1994-1999. The results are insignificant there as well, suggesting that, with respect to my sample, the aggregate behavior in smoking is similar to the full sample.[11] However, since I find no overall discontinuity in smoking behavior, I can not attribute any change I find in alcohol consumption to the minimum smoking age policy or changes in smoking activity at the age 19 threshold.

One remaining quandry is how the results fare when separated by states. There is some variation in enforcement among states. Utah tends to be more strict, whereas Alabama tends to be more lax when dealing with underage smoking infractions, as mentioned in the introduction. One issue I run into is the small sample sizes once I segregate the results at the state level. Roughly half of the observations in my sample are from Utah, and most of the other states, especially Alaska, have sample sizes around one thousand at most. When I run the results for these states individually,

[11] The table that presents these results is Table E.4 in Appendix E.

the results mimic the overall smoking results presented above. When looking at the Smoke 100 outcome, models 1 and 2 in both specifications are significant and positive in every state except for Alaska (due to the sample size). All other smoking outcomes are insignificant in all states. This shows that the states all behave similarly with respect to smoking behavior around the threshold, so no one state is driving these results.

4.5.2 Alcohol Consumption

The second stage of the analysis is to assess how alcohol consumption outcomes change across the age 19 threshold. Here what I am looking at is to see if there is any change in alcohol consumption when an individual turns 19. This result is not related to the effect of the minimum smoking age policy. There are six different outcomes provided in the BRFSS: whether or not a respondent has consumed alcohol in the past month, the number of times the individual drove after drinking in the past month, the number of times they consumed five or more alcoholic beverages (called binge drinking) in the past month, the number of days the respondent drank alcohol in the past month, the share of days a respondent binged on alcohol last month (number of days binge drinking divided by 30), and the share of days a respondent drank in the past month (number of days drank in the past month divided by 30). The first four graphs in Figure E.2 in Appendix E are the proportion of respondents per bin who drank last month, drank and then drove last month, binged on alcohol in the past month, and drank for some number of days in the past month. The lower two graphs are the frequency, or share, of days binged on alcohol or days drank at least one alcoholic beverage. As before, bins are determined using a triangular edge kernel and are fairly insensitive to bin size.

Table 4.2 shows the results of the two specifications and three models used as before to assess on the various alcohol outcomes. Again, each of the specifications accounts for year effects, and specification 2 includes all covariates. A person's probability of drinking in the past month increases by about 6.4 percentage points at age 19 in model 1, specification 1. This is robust to the inclusion of the second and third order age interacted polynomial in specification 1, resulting in a

Table 4.2. Measures of Alcohol Consumption

	Specification 1			Specification 2		
	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3
Panel A: Has Respondent Drank in Last Month?						
Over 19=Di	0.064**	0.117**	0.192***	0.079**	0.110**	0.119
	(0.027)	(0.042)	(0.060)	(0.035)	(0.054)	(0.079)
N		5176			3529	
Panel B: Number of Times Drink and Drive Last Month						
Over 19=Di	-0.020	-0.122	-0.747	-0.017	-0.148	-1.196*
	(0.152)	(0.254)	(0.445)	(0.236)	(0.364)	(0.585)
N		1388			971	
Panel C: Number of Days Binge Drank Last Month						
Over 19=Di	0.618	0.402	1.369	0.576	0.496	1.327
	(0.338)	(0.538)	(0.902)	(0.432)	(0.712)	(1.129)
N		2319			1672	
Panel D: Number of Days Consumed Alcohol in Last Month						
Over 19=Di	0.166	-0.404	-0.513	0.367	-0.451	-1.184
	(0.307)	(0.512)	(0.869)	(0.418)	(0.685)	(1.144)
N		5929			4023	
Panel E: Share of Days Drank Last Month						
Over 19=Di	0.006	-0.013	-0.017	0.012	-0.015	-0.039
	(0.010)	(0.017)	(0.029)	(0.014)	(0.023)	(0.038)
N		5929			4023	
Panel F: Share of Days Binge Drank Last Month						
Over 19=Di	0.021	0.013	0.046	0.019	0.017	0.044
	(0.011)	(0.018)	(0.030)	(0.014)	(0.024)	(0.038)
N		2319			1672	
Year Effects	Y	Y	Y	Y	Y	Y
Xi	N	N	N	Y	Y	Y

*Each panel title is the outcome variable used for the results presented below it. ***P<0.01, **P<0.05, *P<0.10. Model 1 has first order interaction polynomial. Model 2 has second order interaction polynomial. Model 3 has a third order interaction polynomial. Specification 1 includes year fixed effects, but no other control variables. Specification 2 includes all covariates mentioned in section 4.3. Xi indicates the inclusion of covariates.*

11.7 and 19.2 percentage point increase in the proportion of people drinking at least one day last month. When factoring in covariates, model 1 and model 2 are still significant, reporting a 7.9 and 11 percentage point increase respectively. Model 3 does not see a significant increase, but overall there is a significant increase in individuals who report drinking at least once in the past month after turning 19. This increase is clear when looking at the first graph in Figure E.2. There is a slight decrease in drinking and driving on the order of about 1.2 percent in model 3, specification 2. However, this result is not robust to other specifications or models. Another issue with potentially only trusting higher-order polynomials for results is that they may over fit the model and present results which may not reflect the underlying relationship of the data (Gelman and Imbens 2014). The remainder of the outcomes are insignificant at the age 19 threshold. So even if I were able to attribute changes in alcohol consumption to changes in smoking behavior after turning 19, there would be no overall effect to report.

4.6 Conclusion and Discussion

Since 1994, states have been required to have a minimum smoking age of at least 18 years of age. Because of myriad life and legal changes most individuals face at age 18, we still know little about the real effect of minimum smoking age policies. Yet, a few states have a minimum smoking age of 19. Currently Alabama, Alaska, New Jersey, and Utah observe this policy. Since there are fewer major life changes at age 19, this offers a unique chance to cleanly estimate the effect of minimum smoking age policies. Using a regression discontinuity design, I estimate the effect of the discontinuous decrease in the cost of smoking at age 19 on smoking behavior and alcohol consumption. I find that the decrease in the cost of smoking when turning 19 does not lead to a significant change in smoking outcomes. Because of this, I am not able to attribute any changes in alcohol consumption to the minimum smoking age policy.

However, since I find no overall significant change in alcohol consumption at age 19, so this is not a confounding issue. One concern about this, is finding no effect may be a result of downward biased estimates within the context of a precipitous decrease in adolescent smoking nationwide, beginning in 1999 (USDHHS 2014). I find that the results are not biased downward because they are consistent in the time period from 1994-1999, prior to the change in trend. This suggests that the policy of having a minimum smoking age of 19 in states that observe this policy, clearly leads to no significant change in smoking behavior at age 19. I would caution the reader in drawing broader conclusions about minimum smoking age policies in general, based on this analysis. What is nice about this design is that it has strong internal validity for the states who have a minimum smoking age of 19. It would be ideal to have a larger, longitudinal, data set with which to do this analysis. But given the publicly available data sets with the appropriate information, and the small number of states who have instituted this policy, I believe this analysis is one of the best options for assessing the effect of this policy.

One of the main explanations for these results is that there has been a large change in public attitudes and local policies regarding smoking over the last 30 plus years. Because of severe negative health effects, not just of smoking but of secondhand smoke, it became less socially acceptable to smoke in public places. In addition, the increase in information availability and advertising about these negative health outcomes throughout the 20th century lead to decreases in smoking.[12] Organizations like Drug Awareness Resistance Education emerged to educate the public, especially elementary school students, of the dangers of smoking an using other drugs. These education programs in conjunction with media advertising about the dangers of smoking were able to reduce its prevalence, especially among youth.[13] This increase in educational import was coupled with policy changes at local levels which restricted the places where an individual could smoke and higher excise taxes on cigarettes. The common origin of these policies began when restaurants shifted from being allowing smoking anywhere inside, to having smoking and

[12](Sumner 1971; Hamilton 1972; McGuiness and Cowling 1975; Fujii 1980; Schneider et al. 1981; Leu 1984; Kao and Tremblay 1988; Simonich 1991; Pekurinen 1991; Meier and Licari 1997)

[13](Warner 1977; Warner 1979; Schneider et al. 1981; Lewit et al. 1981; Porter 1986; Baltagi and Levin 1986; Simonich 1991; Hu et al. 1994; Tremblay and Tremblay 1995; Goel and Morey 1995)

non-smoking sections. Eventually cities began to ban smoking inside restaurants altogether. This eventually led to banning smoking inside bars. Now, even college campuses are banning smoking altogether on their premises. In a time where it was more socially acceptable to smoke these policies would likely have been met with more resistance. Because of this large change in attitudes, it is not surprising to have see that this minimum smoking age policy has no effect.

A variety of ideas for extensions to this project would lead to a better understanding to the direct effect of this minimum smoking age policy. Finding a robust data set with longitudinal outcomes would help tease out the direct effect on an individual's smoking behavior more clearly. It would be informative to have county level data, not only to account for some counties with 19 as their minimum smoking age but to assess if there are any border effects on smoking behavior. By border effects, I mean if a person lives in a border county, they can go into the next state to purchase cigarettes at a lower age and bring them back to their state. On that same token, using a data set that would account for people with older siblings would be informative. Individuals with older siblings may be able to acquire cigarettes more easily that way. Ages of initiation into smoking or drinking behavior would be interesting to look at. It is possible that if a person drinks then they may be more likely to begin smoking, or vice versa. It would be informative to look at the effect of this smoking law on marijuana and hard drug consumption as well. It is possible that because of the higher smoking age, individuals may substitute other drugs for cigarettes.

CHAPTER 5

CONCLUSION

An individual's behavior and decisions are influenced by policies, information, and regulations, which are factored into the decision making process. Studying these elements that affect choices are important, not just to understand the economic implications, but to better grasp how individuals respond to these policies so that future interventions can be crafted more appropriately. I consider three such problems in this context: how competitiveness can affect later labor market outcomes, group coordination can improve in the face of negative payoffs, and how a minimum smoking age policy can have both a direct and indirect effect on risky behaviors.

I find that adding simple social information into a choice environment can alter competitive preferences between men and women, that considering payoffs as losses can modestly improve group coordination, and that having a minimum smoking age policy of 19 does not have any effect on smoking or alcohol consumption in states that observe that policy. These results are important for a variety of reasons. I have shown that gender differences in competitive preferences can lead to inefficient sorting on labor market outcomes, that an understanding of how people coordinate in the face of losses is essential because individuals already consider problems in that context, and that a variety of other, more local, policies that have an effect on smoking, along with public opinion, are one reason why I find no effect of minimum smoking age policies on risky behavior. These results add to our economic understanding of how information and policies that affect decision making can shape and alter how people respond to incentives.

APPENDIX A

CHAPTER 2: ADDITIONAL RESULTS

Overconfidence Results: Both men and women in all treatments of this experiment are actually "underconfident", which means they guess a rank lower than their actual rank in Round 2. In the baseline condition only 17.5% of participants are overconfident about their relative rank, while 50% are underconfident. In the GN treatment only 7.5% of participants are overconfident, while 55% are underconfident. In the MHP treatment 15% of participants are overconfident and 40% are underconfident. For the WHP treatment 60% are underconfident and 7.5% are overconfident. Overall, there are no significant gender differences in overconfidence and underconfidence in any of the treatments. So while I will include controls for overconfidence in the regression analysis, there is little difference that is necessary to control for here between genders or treatments. Participants are quite inaccurate when it comes to guessing their rank. For the entire sample, 63 percent of participants submit incorrect guesses of their ranks.

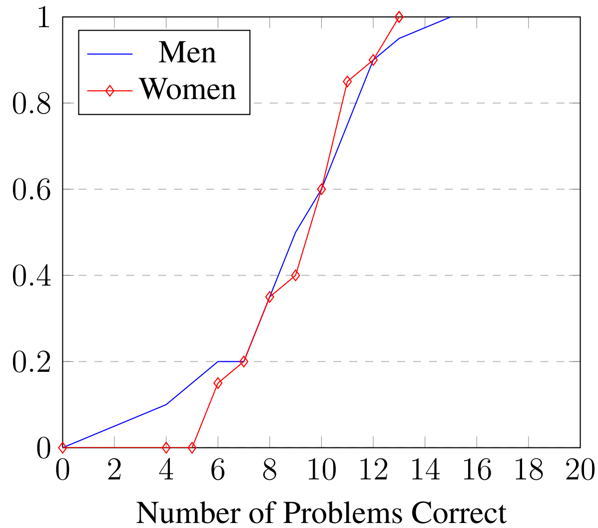

Figure A.1. Round 2 Performance CDF in Baseline Condition

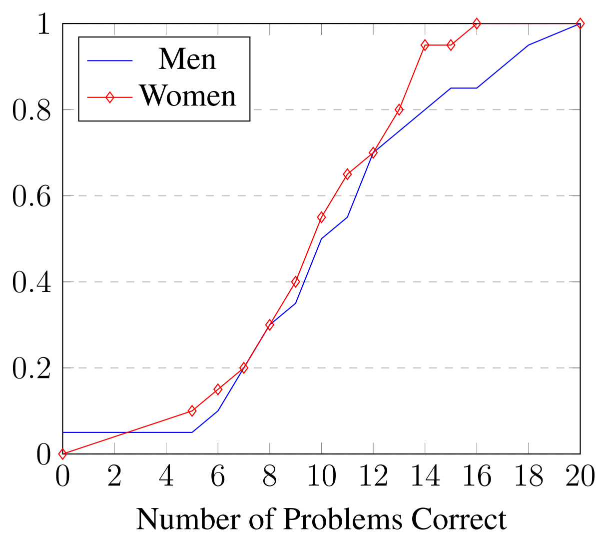

Figure A.2. Round 2 Performance CDF in GN Treatment

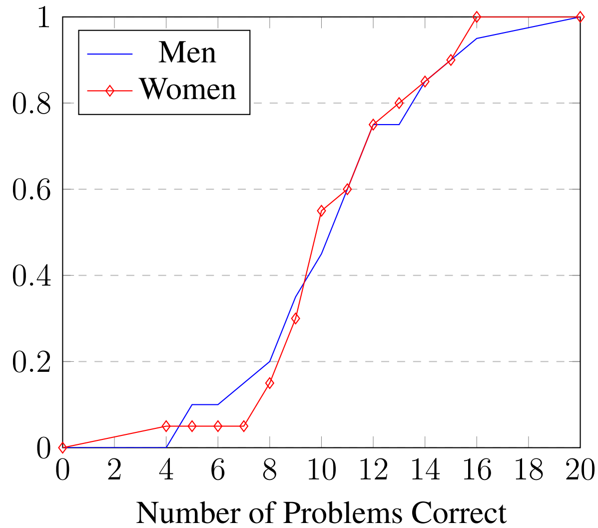

Figure A.3. Round 2 Performance CDF in MHP Treatment

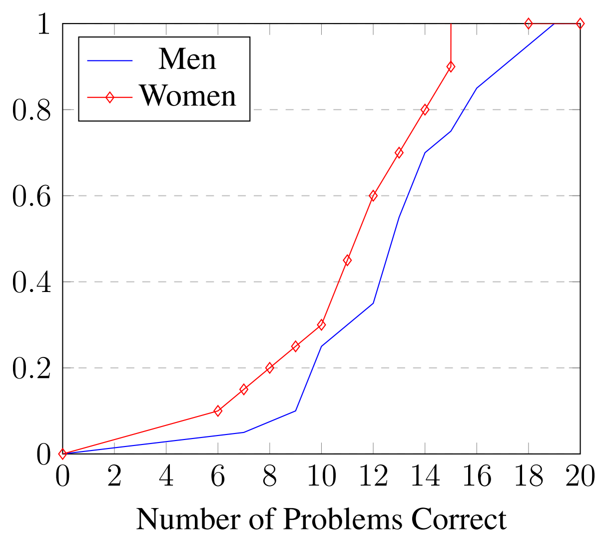

Figure A.4. Round 2 Performance CDF in WHP Treatment

Table A.1. Round 4 Decision by Treatment (Probit, ME)

Columns:	(1)	(2)
Female	-0.325**	-0.290**
	(0.153)	(0.153)
GN	0.09	0.109
	(0.142)	(0.139)
MHP	-0.142	-0.137
	(0.145)	(0.138)
WHP	-0.045	-0.077
	(0.143)	(0.136)
GN x fem	0.236	0.223
	(0.211)	(0.205)
MHP x fem	0.422**	0.460**
	(0.209)	(0.205)
WHP x fem	0.175	0.186
	(0.217)	(0.203)
Improve Round 2		-0.024
		(0.017)
Rank Round 1		-0.055
		(0.034)
GPA		-0.059
		(0.063)
Age		0.028**
		(0.014)
Rankguess Round 2		-0.122**
		(0.054)
Competitive Preference		-0.017
		(0.025)
N	160	160
Ps. R^2	0.05	0.13
ClustVar	Individual	Individual

*Dependent variable is choice in Round 4. It is equal to 1 if tournament is chosen and zero if piece rate is chosen. Marginal Effects and Standard Errors from a probit regression are reported here. ***P< 0.01, **P< 0.05, *P< 0.10. Clustering is at the individual level.*

Table A.2. Summary Statistics of Participants

Major	Men	Women
Business and related fields	18	24
Computer Science	52	32
Engineering	7	16
Other	3	8
Race/ Ethnicity	Men	Women
African American	1	1
East Asian	33	46
South Asian	43	29
Hispanic	1	2
Middle Eastern	1	1
Caucasian	1	1
Mean GPA	Men	Women
	3.53	3.61
Mean Age	Men	Women
	24.4	23.75
N	80	80

Business and related fields includes majors like accounting, finanace, supply chain management, and management. Engineering includes all subsets of engineering (except civil engineering). Other majors is composed of biology, biotechnology, cognitive science, applied cognition and neuroscience, healthcare studies, and mathematics majors.

Table A.3. Round 3 Optimal Tournament Entry

	Baseline	GN	MHP	WHP
Women	5	4	6	11
Men	6	7	6	12

Optimal tournament entry is calculated using a participant's rank in Round 2. If a participant was ranked first, then it is optimal for them to select the tournament. If they were ranked 2-4, they should choose the piece rate. If a participant was tied for first, their rank is still considered first, and it is optimal for them to enter the tournament, even though they may not have actually won the Round 2 tournament.

Table A.4. Performance Variance by Round and Treatment

	Treatment	Round 1	Round 2	Round 3
(Women)	Base	4.89	3.17	5.94
(Men)	Base	8.87	21.84	14.93
	GN	10.09	11.50	10.80
	GN	21.10	16.16	14.57
	MHP	8.53	8.89	12.97
	MHP	11.42	13.36	16.13
	WHP	8.61	10.87	7.39
	WHP	9.80	10.20	8.04
	Overall	7.85	8.35	8.95
	Overall	13.30	15.58	13.82
	Treat Only	8.93	10.16	10.06
	Treat Only	14.48	13.67	13.45

Table A.4 reports performance variance by gender and treatment in each Round where the addition task is performed. Treat Only is the average variance of the treatment conditions only.

APPENDIX B

CHAPTER 2: EXPERIMENTAL INSTRUCTIONS

The instructions for the baseline treatment are as follows. The baseline treatment is identical to the one in Niederle and Vesterlund (2007). However, the instructions are slightly different. After the baseline instructions are introduced, the instructions for different treatments are shown in the way that they modify the baseline instructions. The instructions are as they appeared on the zTree screen for participants. All of the social information is from the math task data in Wozniak, Harbaugh, and Mayr (2014).

B.1 Baseline Treatment Instructions

Welcome

Today, you are asked to complete four different rounds of this experiment. None of these rounds will take more than 5 minutes.

At the end of the experiment, you will receive a $5 show up fee and $5 for completing the experiment; in addition, we will randomly select one of the rounds and pay you based on your performance in that round.

The method we use to determine your earnings varies among rounds. Before each round, we will describe in detail how your payment is determined. At the end of the experiment, you will be asked to come to the back of the room individually, where you will receive your payment.

For each round you will be asked to calculate the sum of five randomly chosen two-digit numbers. On your computer screen you will see five sets of two-digit numbers, which you are tasked to add together.

You will be given 5 minutes to calculate the correct sum of as many of these problems as you are able to.You cannot use a calculator to determine this sum; however, you are welcome to write the numbers down using the provided scratch paper. You submit an answer, after entering it into the white text box, by clicking the submit button with your mouse.

When you enter your answer, the computer will inform you whether your answer is correct or not, and then another problem will appear on your screen. Your answers to the problems are anonymous. At the end of the 5 minute round, you will be informed of the total number of correct problems you solved.

Remember, the method we use to determine earnings varies across rounds, but the task will not.

In this experiment, there may be periods of waiting time. To minimize this, please do not forget to push the 'OK' or 'Submit' button when you are finished with a screen; otherwise everyone may be waiting for you.

If there are any questions throughout the experiment, please raise your hand quietly and an experimenter will assist you.

Round 1

If Round 1 is randomly selected for payment, then you get 50 cents per problem you solve correctly in the 5 minutes. Your payment does not decrease if you provide an incorrect answer to a problem. We henceforth refer to this payment as the piece rate payment.

Round 2

For this round, your payment depends on your performance relative to that of a group of other participants. Each group consists of four people; your group will be those who are seated in the same area as you.

If Round 2 is randomly selected for payment, then your earnings depend on the number of problems you solve compared to the three other people in your group. The individual who correctly solves the largest number of problems will receive $2 per correct problem, while the other participants will receive no payment. We refer to this as the tournament payment.

At the end of this 5 minute round you will still see the number of correct problems you will solve, but you will not be informed of how you did in the tournament until all four rounds have been completed. If there are ties, the winner will be randomly determined.

Round 3

Now, you will get to choose which of the two previous payment schemes you prefer to apply to your performance on the third round, the piece rate payment or the tournament payment.

If Round 3 is randomly selected for payment, then your earnings for this round are determined as follows:

If you choose the piece rate, you receive 50 cents per problem you solve correctly.

If you choose the tournament, your performance will be evaluated relative to the performance of the members of your group from the Round 2 tournament. The Round 2 tournament is the one you just completed. If you correctly solve more problems than the individual from your group who won the tournament in Round 2, then you will receive $2 per correct problem.

You will receive no earnings for this round if you choose the tournament and do not solve more problems correctly now, than the others in your group did in the Round 2 tournament. You will still be notified of the number of problems you completed correctly at the end of the 5 minute round.

Example: Say the members of Group ZY solved 8, 10, 12, and 9 problems correctly in Round 2. Say one of the members of Group ZY chooses the tournament to be applied to their performance in Round 3. For the tournament (and to earn $2 per correct problem) this member needs to solve at least 13 problems to win outright. If this member solves 12 problems and ties with the previous winner, the result will be decided randomly.

You will not be informed of how you did in the tournament until all four tasks have been completed. If there are ties, the winner will be randomly determined

Round 4

You do not have perform any addition task for the fourth and final round of the experiment. Instead, you may be paid for the number of problems you solved in Round 1.

However, you now have to choose which payment scheme you want applied to the number of problems you solved. You can either choose to be paid according to the piece rate, or according to the tournament.

If the fourth round is the one selected for payment, then your earnings for this round are determined as follows:

If you choose the piece rate, you receive 50 cents per problem you solved in Round 1. This will result in the exact same profit as Round 1.

If you choose the tournament, your performance will be evaluated relative to the performance of the other three participants of your group in the Round 1 piece rate. If you correctly solved more problems in Round 1 than they did, you receive $2 per correct problem.

You will receive no earnings for this task if you choose the tournament and did not solve more problems correctly in Round 1 than the other members of your group.

The next screen will remind you how many problems you solved correctly in Round 1, and then will ask you to choose whether or not you want the piece rate or tournament applied to your performance.

B.2 Gender Neutral Treatment

The modification for this treatment, in terms of the instructions, comes during the Round 3 instructions section. An extra paragraph to preface the feedback was added, in addition to a separate screen for to show the participants the social information prior to their remuneration scheme decision in Round 3. The italicized paragraph is identical in each of the treatment conditions, so I will only present it once.

Round 3

Now, you will get to choose which of the two previous payment schemes you prefer to apply to your performance on the third round, the piece rate payment or the tournament payment.

If Round 3 is randomly selected for payment, then your earnings for this round are determined as follows:

If you choose the piece rate, you receive 50 cents per problem you solve correctly.

If you choose the tournament, your performance will be evaluated relative to the performance of the members of your group from the Round 2 tournament. The Round 2 tournament is the one you just completed. If you correctly solve more problems than the individual from your group who won the tournament in Round 2, then you will receive \$2 per correct problem.

You will receive no earnings for this round if you choose the tournament and do not solve more problems correctly now, than the others in your group did in the Round 2 tournament. You will still be notified of the number of problems you completed correctly at the end of the 5 minute round.

Example: Say the members of Group ZY solved 8, 10, 12, and 9 problems correctly in Round 2. Say one of the members of Group ZY chooses the tournament to be applied to their performance in Round 3. For the tournament (and to earn \$2 per correct problem) this member needs to solve at least 13 problems to win outright. If this member solves 12 problems and ties with the previous winner, the result will be decided randomly.

You will not be informed of how you did in the tournament until all four tasks have been completed. If there are ties, the winner will be randomly determined

Before making your decision, you will receive some additional information about today's experiment which you may find helpful. Everyone in the room has the same information on their screen. After you read this information, you will be asked to choose whether you want the piece rate or the tournament applied to your performance.

(After seeing this, participants then were shown the social information for the specific treatment.)

An experiment very similar to this one has already been run at another university, in a laboratory like this one. What follows on this screen is selected performance information from those sessions.

Average Number of Problems Correct for Men in Round 3: 11

Of these 12 men, 10 chose the tournament in Round 3. Four of these men won the tournament.

Average Number of Problems Correct for Women in Round 3: 11

Of these 12 women, 3 chose the tournament in Round 3. All three of these women won the tournament.

B.3 Men as High Performers Treatment

The social information screen is as follows:

An experiment very similar to this one has already been run at another university, in a laboratory like this one. What follows on this screen is selected performance information from those sessions.

Average Number of Problems Correct for Men in Round 3: 14.5

Of these 12 Men, 8 chose the tournament in Round 3. Four of these men won the tournament.

Average Number of Problems Correct for Women in Round 3: 10.63

Of these 12 Women, 3 chose the tournament in Round 3. One of these women won the tournament.

B.4 Women as High Performers Treatment

The social information screen is as follows:

An experiment very similar to this one has already been run at another university, in a laboratory like this one. What follows on this screen is selected performance information from those sessions.

Average Number of Problems Correct for Men in Round 3: 10.64

Of these 12 Men, 5 chose the tournament in Round 3. One of these men won the tournament.

Average Number of Problems Correct for Women in Round 3: 14.33

Of these 12 Women, 7 chose the tournament in Round 3. Five of these women won the tournament.

APPENDIX C

CHAPTER 3: ADDITIONAL RESULTS

C.1 Heterogeneity

To assess the degree of heterogeneity in each treatment it is instructive to look at group level behavior in each treatment. Figures C.1, C.2, and C.3 show the mean choices by group and treatment. The baseline condition results in the most stable behavior of the treatments. In the first 10 rounds all groups either achieve, or are strongly trending towards coordination failure. In period 11, all groups increase their actions, but then 5 of the 6 groups revert back to exerting a low level of effort. Most groups end up with actions between 1 and two by period 20. Figure C.1 shows that group 6 is able to increase coordination to a level of 6 by the end of the experiment. This group is the main reason why it appears that coordination levels increase above two in Figures 3.1 and 3.2. To assess if this is really an outlier, the median action is informative. Looking at the treatment level median across periods 11-20, the highest median achieved is 3 in period 11, otherwise the median action is at most 2 (in period 12 and 16) and is more commonly 1 or 1.5. This confirms that group 6 is an outlier, so it is fair to say the baseline condition successfully achieves coordination failure. The minor improvement in part 2 in the baseline condition is due to the behavior of a single group, which, when aggregated with the rest of the groups, gives the appearance of better coordination.

Figure C.2 shows the heterogeneous responses by group in the Gain-Loss treatment. Here the dynamics of the treatment can be assessed group by group. In the first part, two trends emerge. Groups 4 and 5 coordinate between 6 and 7 for the majority of part 1. These two groups maintain their high level of coordination through part two. With their history of high level of effort in the part 1, it is not surprising that these participants are able to maintain their high level of coordination through period 20, even in the face of the loss condition. Having a static group throughout the experiment can help facilitate this behavior. I will explore this further in relation to risk preferences in the next section. The remaining four groups do not achieve coordination failure per se, but are trending towards failure. By trending towards failure, I mean that the trend of participant choices is decreasing in a manner similar to what we see in Figure 3.1 with the baseline treatment. Average choices in these four groups are decreasing and it would be plausible that they would achieve failure given further rounds of play without interruption. For these four groups, the effect of moving into the loss condition is also heterogeneous. Group 2 is the only group who responds to the introduction of the loss table exactly as hypothesized. This group achieves and maintains a coordination level of 7 beginning in period 13 and continues at this level through the final period. Group 6 has a temporary improvement in period 11, but then gradually decrease their effort to an average between 2 and 3 in the last 5 periods. Groups 1 and 3 have very inconsistent choices throughout the last 10 periods. It is clear any previous action does not result in a precedent for these groups.

Overall, it does not appear that the loss frame has any conclusive positive effect on coordination for groups 1, 3, and 6 in the Gain-Loss treatment. This is contrary to the expectation in hypothesis 1, which suggests that sensitivity to losses will lead to better coordination in part 1. Many of the groups in this treatment are able to maintain relatively high coordination throughout the experiment, regardless of the presentation of the payoffs. In part 1, outside of group 4, participants either achieve, or are trending towards coordination failure in the Loss-Gain treatment. This is contrary to hypothesis 2 which suggests that framing payoffs as losses will lead to more efficient, specifically payoff dominant, equilibrium. The likely reason for this is a lack of experience of play, and the strategic uncertainty that comes with encountering the loss frame in period 1.

Most papers, when including a treatment intervention, do not address the actual dynamics of the intervention by including it in the initial rounds of play. It is far more common for participants to play 10 rounds of the game first (to assess coordination failure replication), and then apply the intervention for the successive 10 periods as I did in the Gain-Loss treatment. By doing this, we are deprived of any information about how the intervention may affect initial choices. Previous works have shown that initial conditions matter in these experiments. Regardless of the results, this design would reflect that conclusion. However, since assessing dynamics of framing payoffs as losses is the main goal of this experiment, this structure is necessary. The interaction of the behavior with risk preferences in part one will be discussed extensively in the next section, but here, the secure action of 1 is chosen at the highest rate (42.5%). It is likely that this treatment added to strategic uncertainty about group actions or dynamics of participants. Results in coordination games are sensitive to initial conditions and add exogenous risk to an already uncertain decision (Van Huyck, Battalio, and Beil 1990 & 1991, Devetag and Ortmann 2007). This can lead to deterioration of coordination equilibria towards the secure equilibrium of 1.

The greater issue with heterogeneity in the Loss-Gain treatment is in response to the shift from the loss to the gain frame. In part two Figure C.3 shows two separate patterns of behavior after the shift to the gain frame. Groups 1, 2, and 4 all increase their choices in the gain frame. Groups 1 and 4 are actually able to achieve payoff dominant coordination by period 15, and maintain it through period 20. Group 2 increases their actions close to 7 initially, but after period 12, the choices decrease until it settles close to 4. The remaining three groups continue their path towards coordination failure from the first two periods, and achieve it by period 20. Why do we see this dichotomous response in the transition to the gain treatment? The rationale behind continuing with the secure actions makes sense because it reduces uncertainty, and with a history of play, participants know that they will be able to coordinate on at least this action (due to precedent). For the three groups who are able to achieve a high level of coordination I need to explore the role of risk preferences and their relation to riskier actions. It is possible that the psychological effect of failed coordination during the first 10 periods induces participants to seek compensatory gains during the gain frame. If this is the case risk may be an effective lens through which to gauge this shift.

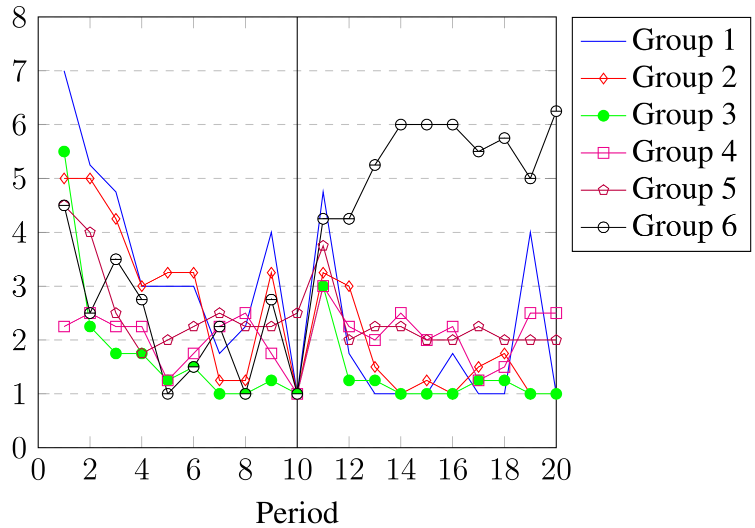

Figure C.1. Baseline Mean Choice by Group

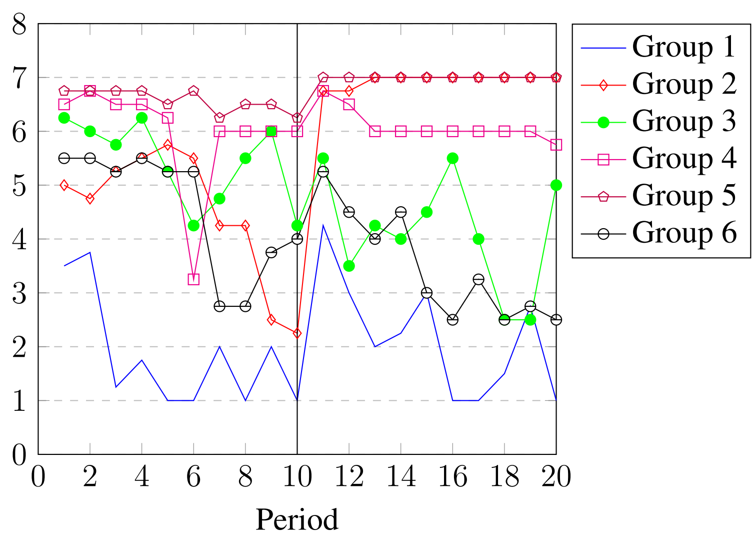

Figure C.2. Gain-Loss Mean Choice by Group

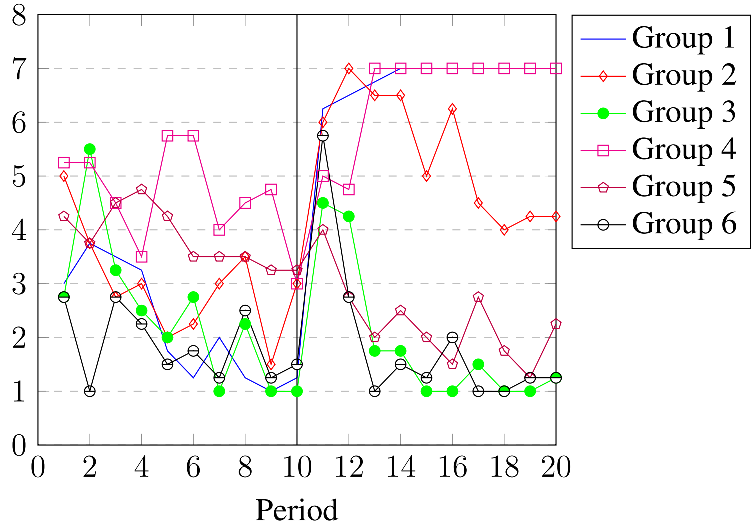

Figure C.3. Loss-Gain Mean Choice by Group

C.2 Risk Preferences

To control for differences in risk preferences that may affect actions during the experiment, I used the Dave et al. (2010) version of the Eckel and Grossman measure. Participants are asked to choose one of the 6 gambles listed in Table C.1. Each of the possible options of the gamble has a 50% chance of happening. Since selecting the payoff dominant action is inherently riskier than the secure action, this may help explain the results. For the following figure, a risk preference marked as 1 indicates the lowest value of a constant relative risk aversion parameter. A chosen gamble of 6 has the lowest risk aversion parameter, since it is the most risky gamble. Table C.1 reports the frequency of choices of risk gamble by treatment. There are no differences in the distribution of risk preferences between the baseline and any of the treatments. The majority of participants (53/72 or 74%) select a gamble of 3 or lower. If risk is a significant parameter in guiding choices in the minimum effort game, this may be an important explanation for why we see the lack of coordination on the payoff dominant equilibrium. Ultimately, what I am interested in is how these risk preferences affect actions over the course of the experiment. Figures C.4, C.5, and C.6 present the mean choices by risk preference in each treatment.

Table C.1. Risk Preferences

Gambles:		Baseline	Gain-Loss	Loss-Gain
	1: \$28 dollars or \$28 dollars	5	4	11
	2: \$24 dollars or \$36 dollars	9	7	7
	3: \$20 dollars or \$44 dollars	6	4	2
	4: \$16 dollars or \$52 dollars	1	1	0
	5: \$12 dollars or \$60 dollars	1	1	0
	6: \$2 dollars or \$70 dollars	2	7	4
N		24	24	24

The gambles each have a 50% chance of happening. So if a participant selected 5, they are choosing the gamble where they would either get \$12 dollars or \$70 dollars with equal chance. As the numerical value of the chosen gamble increases, it implies a higher value of constant relative risk aversion.

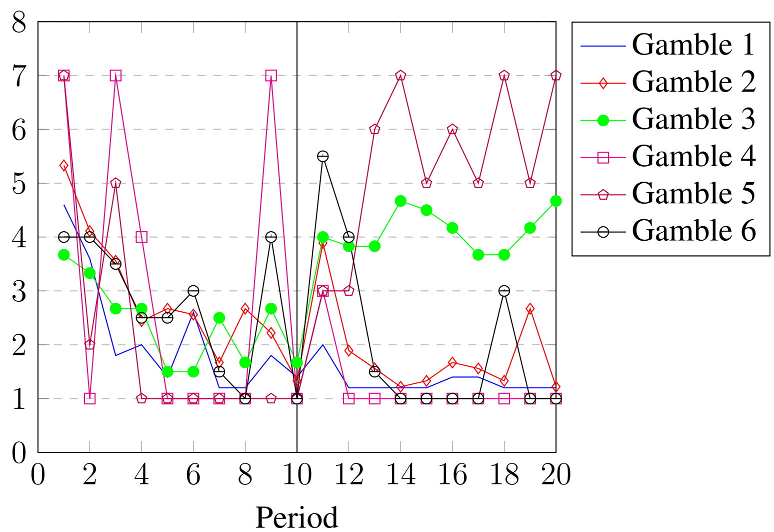

Figure C.4. Baseline Mean Choice by Risk Preference

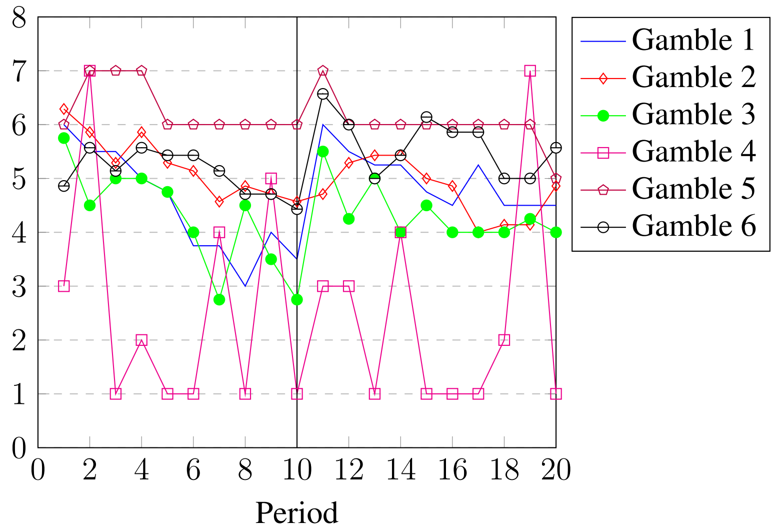

Figure C.5. Gain-Loss Mean Choice by Risk Preference

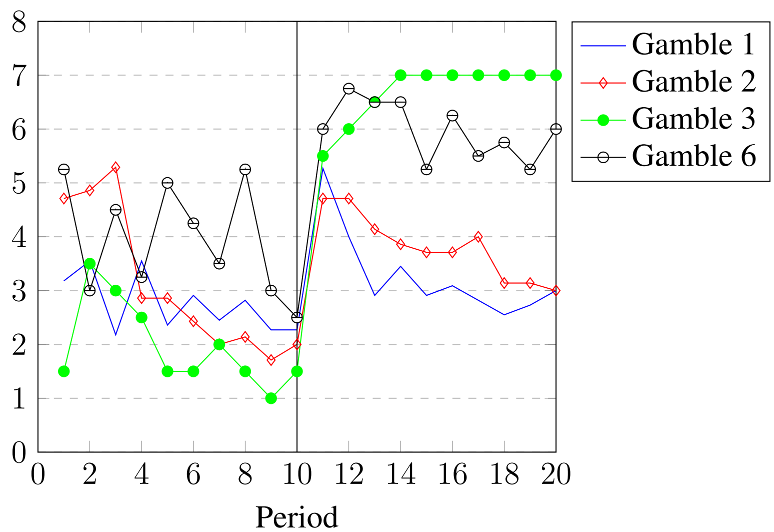

Figure C.6. Loss-Gain Mean Choice by Risk Preference

Figure C.4 shows that, in part 1 of the baseline, participants are mostly sticking with the secure action regardless of risk preference. There is some exploration throughout the period, but all 6 risk groups, on average, choose an action below 2 by period 10. In part 2, four of the risk groups continue coordinating on the secure action. However, two of the risk groups, those who chose gambles 3 and 4, see some increase in risky actions in part 2. It should be noted that only one participant chose gamble 4 in the baseline, so it would be irresponsible to put too much weight on this observation. The 6 participants who chose gamble 3 are choosing an action near 4 beginning in period 11. Here, 7 of the 24 participants are be willing to risk not coordinating on any of the equilibria in exchange for the hope of a better payoff.

The Gain-Loss choices based on risk preference is shown in Figure C.5. For gambles 4 and 5, there is only one observation. The participant who chooses gamble 4 seems to have no consistent pattern of choices throughout the experiment. There are sustained high choices of 6 or 7 for the participant who chooses a gamble of 5. The 22 participants who choose the remaining 4 gambles have a fairly similar decreasing trend in part 1. They begin at an action of 5 or 6, and then coordination deteriorates to an action between 3 and 5. These groups stabilize their choices in part 2 by choosing an action between 4 and 6 with less variability than in part 1. This increase in risky actions in the Gain-Loss treatment, regardless of risk preference, is one explanation for why we see coordination at a higher effort level than in the baseline in part 1. Participants in this treatment overall exhibit a higher preference for risk than in the baseline, as we see in Table C.1. This emerges as an explanation of the surprising part 1 result when both sets of participants are playing under the same condition and achieve different levels of coordination. However I am not able to say for certain this was the only reason behind this behavior.

The treatment with the most risk averse participants is the Loss-Gain treatment. Overall, 20/24 subjects choose a gamble of 3 or less. The choices for all of the 4 gambles chosen reflect much of what we see in Table C.3. Overall, there is a strong trend towards coordination failure in part 1, seen in Figure C.6. Those with the highest risk preference (who choose gamble 6) are choosing the most risky actions in part 1, but they are only slightly better than their less risk tolerant counterparts. In part 2, the binary response to the Gain table shift shown in Figure C.3 emerges here.

Participants with the highest preference for risk are the ones who increase their choices toward the payoff dominant equilibrium and those with the lowest tolerance for risk resume their quest towards coordination failure. However, these risk parameters, in conjunction with the choices made by participants, suggest that participants are not willing to choose a riskier action, even when payoffs are framed as losses. The higher risk preference in the Gain-Loss treatment emerges as an explanation for why this different result in part 1 emerges when compared to the baseline. Though it seems like participants are more willing to coordinate at a high effort level, regardless of risk preference of payoff framing.

Table C.2. Summary Statistics of Participants

Major	
Business and related fields	45
Computer Science	14
Engineering	13
Race/ Ethnicity	
East Asian	34
South Asian	38
Mean GPA	3.49
Mean Age	24.79
Number of Men	53
Number of Women	19
N	72

Business and related fields includes majors like accounting, finanace, supply chain management, and management. Engineering includes all subsets of engineering.

C.3 Variance

A helpful way to further assess choice heterogeneity in the treatment conditions is to look at variances at the treatment level. Figure C.7 shows the variance in participant choices by treatment across all periods. For the baseline condition, the variance looks relatively stable, but numerically it is fairly large, especially in periods 11-20. This is largely due to the influence of a single group, though since these variances are calculated at the treatment level, they will be much larger than if they were calculated at a smaller level of aggregation. It is easy to see the influence of this group, Group 6, in Figure C.1. When the mean choices of every other group are around 1, this group is coordinating close to an effort level of 6. A more accurate representation of the behavior in the baseline condition is in Figure 3.2. Both of the treatment conditions trend toward a higher amount of variance as the period gets closer to 20. The variance in the Gain-Loss treatment is fairly consistent across all periods. The variance of 6 is still high, but easy to explain. Figure C.2, as noted earlier, presents a pattern of divergence where 3 of the groups in this treatment choose an average of 6 or 7, and the other three are roughly between 1 and 4. The Loss-Gain variance seems to reflect the general pattern in Figure C.3. In the first 10 rounds, the actions gradually decrease. For the

Table C.3. Treatment Effect on Group Minima (Ordered Probit)

	(1)	(2)	(3)	(4)	(5)	(6)
Gain-Loss	1.621***	1.465***	1.526***			
	(0.222)	(0.375)	(0.247)			
Loss-Gain				0.056	0.093***	0.512**
				(0.204)	(0.313)	(0.207)
Age	0.002	0.014	0.014	-0.053*	-0.083	-0.057
	(0.072)	(0.101)	(0.071)	(0.032)	(0.062)	(0.039)
Female	0.480*	0.615*	0.543**	0.434**	0.279	0.326
	(0.252)	(0.331)	(0.243)	(0.205)	(0.319)	(0.211)
GPA	0.023	-0.948	-0.564	0.461	0.163	0.229
	(0.522)	(0.704)	(0.501)	(0.331)	(0.528)	(0.329)
Risk Preference	0.110	0.101	0.099	0.089**	0.253***	0.162***
	(0.076)	(0.102)	(0.073)	(0.045)	(0.087)	(0.050)
N*T	480	480	960	480	480	960
N	48	48	48	48	48	48
Ps. R^2	0.150	0.114	0.120	0.035	0.090	0.046
Periods	1-10	11-20	11-20	1-10	11-20	1-20
Treatment	Gain-Loss	Gain-Loss	Gain-Loss	Loss-Gain	Loss-Gain	Loss-Gain
ClustVar	Individual	Individual	Individual	Individual	Individual	Individual

*Dependent variable is minimum action. Columns 1-3 correspond to the Gain-Loss treatment, and columns 4-6 are for the Loss-Gain treatement. Column 1 restricts the sample to part 1, column 2 is for part 2, and column 3 if the overall effect. The column organization is identical for columns 4-6. ***P<0.01, **P<0.05, *P<0.10. Clustering is at the individual level and results are similar when using individual or session level clustering.*

second 10 rounds the results are more heterogeneous, resulting in the high variance in Figure C.7. 2 groups coordinate on 7, one group coordinates on 4, and the other three are between 1 and 2 by period 20, creating this large variance.

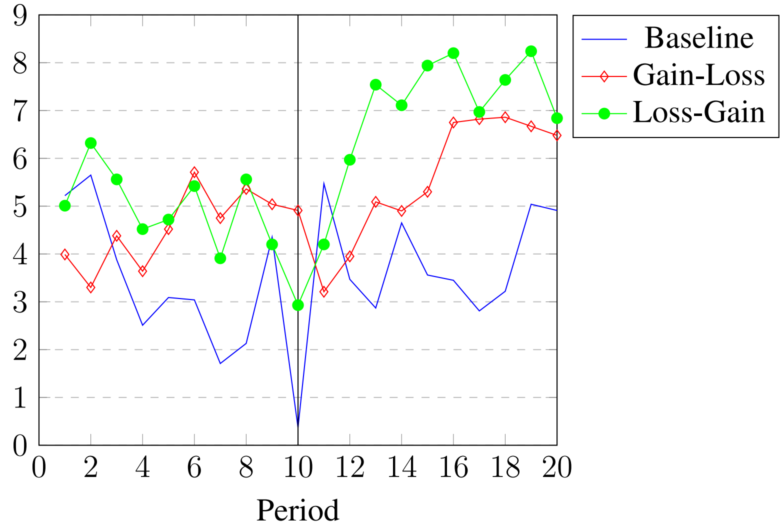

Figure C.7. Variance of Choices by Treatment

APPENDIX D

CHAPTER 3: EXPERIMENTAL INSTRUCTIONS

What follows are the instructions for my experiment. I am only including directions for the gain-loss treatment. To replicate this experiment, the full instructions can easily be reconstructed in the following manner: for the baseline condition use the gain instructions twice, and for the loss-gain treatment use the loss instructions and then the gain instructions. Below there is an introductory section that was included at the beginning of each session These instructions are modified from Brandts and Cooper (2006).

D.1 Gain-Loss Treatment Instructions

Welcome.

Today you will be asked to participate in an experiment. The purpose is to study how people make decisions in different situations. From now until the end of the experiment, any communication with other participants is prohibited. Please silence and put away your cell phones, and do not talk. If you have any questions please raise your hand at any time and one of the experimenters will assist you. If you are unsure about any of the directions I encourage you to ask a question.

You will receive $5 dollars as a show-up fee for this experiment. In addition, you will earn money during the experiment. Upon completion, the amount that you earn will be paid to you in cash. Payments are confidential and will be administered privately.

The experiment has two parts. You will receive instruction for Part II after we complete Part I.

Please click okay to go to the instructions for part 1

PART 1: Gains Condition

In Part 1 there will be 10 Rounds. In each round you will be in a group with three other participants. The participants you are grouped with are the same throughout the experiment.

For each Round you are to choose a number between 1 and 7. Your payoff depends on your choice, and the SMALLEST number chosen by the four members of the group, as in the table that follows.

(At this point I put the Gain payoff table (Table 3.1) on the screen at the front of the room, and handed out a paper copy to each of the participants. Participants do not know these are called gains or loss tables, it just says Payoff Table on the screen and handout.)

Your payment for the round will be the amount determined from the table. For each round the computer will display a screen showing the above payoff table. Each participant will choose their number between 1 and 7 using the text box on the screen and clicking the OK button. When you make your decision you will not know who is in your group, or the actions they take. All actions are confidential.

After each Round you will be informed of the action you chose in that round, the smallest number chosen by your group in that round, your payoff for this round, and your accumulated payoff through the current round. You will be shown the history of your decisions and the history of decisions of your entire group in each round.

Payoff Quiz (for the Gain Table)

Before we begin the experiment, please answer the following questions. We will go through the answers to a sample problem first, and then you will finish the quiz on your own. Each question deals with calculating your payoff in a single round.

Sample Question: Suppose you choose the action 3. The other members of your group choose 2, 5, and 7 respectively.

What is the minimum action chosen by your group? (2)

What is your payoff? (0.55)

1. Suppose you choose the action 7. The other members of your group choose 4, 5, and 6.

What is the minimum action chosen by your group? (4)

What is your payoff? (0.55)

2. Suppose you choose the action 1. The other members of your group choose 7, 6, and 2.

What is the minimum action chosen by your group? (1) What is your payoff? (0.55)

Remember that you are grouped with the same individuals throughout the 20 Rounds of this experiment. All actions and payoffs are confidential.

PART 2: Losses Condition

In Part 2 there will be 10 Rounds. In each round you will be in a group with three other participants. The participants you are grouped with are the same throughout the experiment.

For each Round you are to choose a number between 1 and 7. Your payoff depends on your choice, and the SMALLEST number chosen by the four members of the group, as in the table that follows.

(At this point I put the Loss payoff table (Table 3.2) on the screen at the front of the room, and handed out a paper copy to each of the participants. Participants do not know these are called gains or loss tables, it just says Payoff Table on the screen and handout.)

Your payment for the round will be the amount determined from the table plus $1.35.

For each round the computer will display a screen showing the above payoff table. Each participant will choose their number between 1 and 7 using the text box on the screen and clicking the OK button. When you make your decision you will not know who is in your group, or the actions they take. All actions are confidential.

After each Round you will be informed of the action you chose in that round, the smallest number chosen by your group in that round, your payoff for this round, and your accumulated payoff through the current round. You will be shown the history of your decisions and the history of decisions of your entire group in each round.

Payoff Quiz (for the Loss Table)

Before we begin, please answer the following questions. We will go through the answers to a sample problem first, and then you will finish the quiz on your own. Each question only deals with calculating your payoff in a single round.

Sample Question: Suppose you choose the action 3. The other members of your group choose 2, 5, and 7 respectively.

What is the minimum action chosen by your group? (2)

What is your payoff? (-0.80)

1. Suppose you choose the action 7. The other members of your group choose 4, 5, and 6.

What is the minimum action chosen by your group? (4)

What is your payoff? (-0.80)

2. Suppose you choose the action 1. The other members of your group choose 7, 6, and 2.

What is the minimum action chosen by your group? (1)

What is your payoff? (-0.80)

Remember that you are grouped with the same individuals throughout the 20 Rounds of this experiment. All actions and payoffs are confidential.

APPENDIX E

CHAPTER 4: ADDITIONAL TABLES AND GRAPHICS

Table E.1 and Figure E.3 present results for the smooth transition of demographic characteristics at the age 19 threshold. Table E.1 uses Model 1 with the first order age interacted polynomial and Model 2 with the second order age interacted polynomial to estimate this relationship. Year fixed effects are used in all models. Sex and race characteristics are not significant across both models. Since the data set is cross sectional, the demographic variability of the sample can also depend on the variation in the sample from year to year. The BRFSS tries to remain fairly consistent across time period, but since it is a voluntary anonymous phone survey, there is bound to be some variability. One characteristic which is significant is a whether or not a respondent is a student or not. This is a slightly negative effect of between 15 and 20 percentage points. Two explanations emerge for why this is. First, it could just be a result of sampling variability in the survey. If this is the case, then there is little I can do to control for this. A second explanation is that there are a large amount of students who drop out of college (provided they were in college at age 19), or are entering the work force after completing high school. My data set is cross sectional, so I can not track these changes in individual behavior and there is no variable which assesses college drop out status. This leads me to believe the most likely explanation for this is just sampling variability in the data.

Falsification tests are assessed where the age discontinuity is at age 20 and 22, to assess whether there is any discontinuity at other ages which may confound my results. I did not use age 21 since that is the age when it becomes legal to drink alcohol, and I did not want that to confound my estimates. Table E.2 uses the second order age interacted polynomial to estimate the effect of turning 20 or 22 on the outcomes being a current smoker and drinking in the past month. Specification 1, with no covariates save for year effects is the first two columns in Table E.2. Specification 2 includes all covariates and is the second set of two columns. None of the outcomes are significant for

Table E.1. Age Profile of Demographic Characteristics

	Female		White		Black		Asian		Current Student	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Over 19=Di	0.042	0.048**	-0.002	-0.046**	0.015	0.026	0.003	0.010	-0.152***	-0.199***
	(0.028)	(0.024)	(0.025)	(0.022)	(0.017)	(0.015)	(0.011)	(0.009)	(0.027)	(0.023)
Constant	0.564***	0.526***	0.755***	0.739***	0.053**	0.083**	0.046**	0.037***	0.477***	0.458***
	(0.040)	(0.028)	(0.035)	(0.026)	(0.020)	(0.016)	(0.017)	(0.012)	(0.039)	(0.027)
N	5841	5841	5841	5841	5841	5841	5841	5841	5841	5841

****P*<0.01, ***P*<0.05, **P*<0.10. Model 1 has first order interaction polynomial. Model 2 has second order interaction polynomial. Estimates from OLS with year effects and clustering at the individual level. All models include year fixed effects. Student status is included here because it is customary to do so, but it is volatile to specification and can become negative or positive with minor modifications. In the BRFSS student status is part of a question about employment status, and it is possible this creates some issues in estimation.*

Table E.2. Falsification Tests: Age Discontinuity at 20 and 22 in Alcohol and Smoking Behavior (2nd Order Polynomial)

	Spec. 1		Spec. 2	
	Age 20	Age 22	Age 20	Age 22
Alcohol				
Di	0.038	-0.024	0.074	-0.044
	(0.044)	(0.044)	(0.054)	(0.047)
N	4807	7443	3243	4847
Smoking				
Di	-0.008	0.017	0.024	0.043
	(0.067)	(0.059)	(0.064)	(0.074)
N	1482	2002	1019	1326
Year Effects	Y	Y	Y	Y
Xi	N	N	Y	Y

******P*<0.01*, **P*<0.05*, *P*<0.10*. All models include year fixed effects. Specification 2 includes all covariates mentioned in the methodology section. The two outcomes used here are whether or not a person is a current smoker and if they have drank in the past month.* Xi *indicates the inclusion of covariates.*

Table E.3. Summary Statistics for Individuals Aged 18-20

Education Level	N	Percent
Less Than a High School Degree	1124	19.2%
High School Degree	3222	55.2%
Some College	1448	24.8%
Employed	2753	47.1%
Student	2110	36.1%
Race/ Ethnicity	N	Percent
African American	627	10.7%
Asian	257	4.4%
Caucasian	4181	71.5%
Other	551	9.4%
N	5841	100%

Total number of observations for individuals aged 18 to 20 are shown in this table.

the ages shown.[1] Regardless of specification or age, there is no discontinuity in either alcohol or smoking behavior. This shows that any effects I find in the main results are driven by the decrease in the cost of smoking at age 19, and not other age related factors.

[1] I tried other outcomes before deciding on these two. None of the smoking or drinking outcomes are significant at ages 20 or 22.

Table E.4. Measures of Smoking Participation, 1994-1999

	Specification 1			Specification 2		
	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3
Panel A: Has Respondent Smoked 100 Cigarettes in Their Life?						
Over 19=Di	0.118**	0.134	0.192	0.179***	0.128	0.137
	(0.058)	(0.090)	(0.047)	(0.069)	(0.108)	(0.151)
N		1125			743	
Panel B: Does Respondent Currently Smoke?						
Over 19=Di	-0.122	-0.056	-0.205	-0.061	0.092	-0.122
	(0.082)	(0.131)	(0.205)	(0.128)	(0.189)	(0.292)
N		340			214	
Panel C: Number of Days Smoked Last Month						
Over 19=Di	-1.427	0.982	0.298	-2.427	4.385	-0.633
	(3.087)	(4.928)	(7.383)	(4.071)	(5.979)	(8.968)
N		311			195	
Panel D: Tried to Quit Smoking in Past Year						
Over 19=Di	0.017	-0.025	-0.059	-0.041	0.058	-0.247
	(0.141)	(0.223)	(0.312)	(0.174)	(0.221)	(0.289)
N		196			121	
Panel E: Share of Days Smoked Last Month						
Over 19=Di	-0.048	0.033	0.010	-0.081	0.146	-0.021
	(0.103)	(0.164)	(0.246)	(0.136)	(0.199)	(0.300)
N		311			195	
Year Effects	Y	Y	Y	Y	Y	Y
Xi	N	N	N	Y	Y	Y

*Each panel title is the outcome variable used for the results presented below it. The sample is restricted to the years 1994-1999, in order to show that results are similar when restricting to this sample before an overall decrease in smoking among youth. ***P<0.01, **P<0.05, *P<0.10. Model 1 has first order interaction polynomial. Model 2 has second order interaction polynomial. Model 3 has a third order interaction polynomial. Specification 1 includes year fixed effects, but no other control variables. Specification 2 includes all covariates mentioned in section 4.3. Xi indicates the inclusion of covariates.*

Table E.5. Measures of Smoking Participation for Students

	Specification 1			Specification 2		
	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3
Panel A: Has Respondent Smoked 100 Cigarettes in Their Life?						
Over 19=Di	0.035	0.074	0.147	0.425	0.055	0.112
	(0.033)	(0.052)	(0.073)	(0.040)	(0.060)	(0.085)
N		2086			1314	
Panel B: Does Respondent Currently Smoke?						
Over 19=Di	0.148	0.107	0.156	0.287	0.373	0.308
	(0.094)	(0.150)	(0.228)	(0.167)	(0.242)	(0.382)
N		282			165	
Panel C: Number of Days Smoked Last Month						
Over 19=Di	2.751	2.877	6.214	3.036	2.261	-1.459
	(2.747)	(4.458)	(6.788)	(4.432)	(6.913)	(10.555)
N		276			162	
Panel D: Tried to Quit Smoking in Past Year						
Over 19=Di	0.140	0.345	0.407	0.177	0.576*	0.595
	(0.122)	(0.197)	(0.297)	(0.193)	(0.303)	(0.474)
N		198			112	
Panel E: Share of Days Smoked Last Month						
Over 19=Di	0.072	0.096	0.207	0.101	0.075	-0.048
	(0.092)	(0.149)	(0.226)	(0.147)	(0.230)	(0.352)
N		276			162	
Year Effects	Y	Y	Y	Y	Y	Y
Xi	N	N	N	Y	Y	Y

*Each panel title is the outcome variable used for the results presented below it. The sample is restricted to those who report that they are students. ***P*<0.01, ***P*<0.05, **P*<0.10. *Model 1 has first order interaction polynomial. Model 2 has second order interaction polynomial. Model 3 has a third order interaction polynomial. Specification 1 includes year fixed effects, but no other control variables. Specification 2 includes all covariates mentioned in section 4.3.* Xi *indicates the inclusion of covariates.*

Figure E.1. Measures of Smoking Behavior

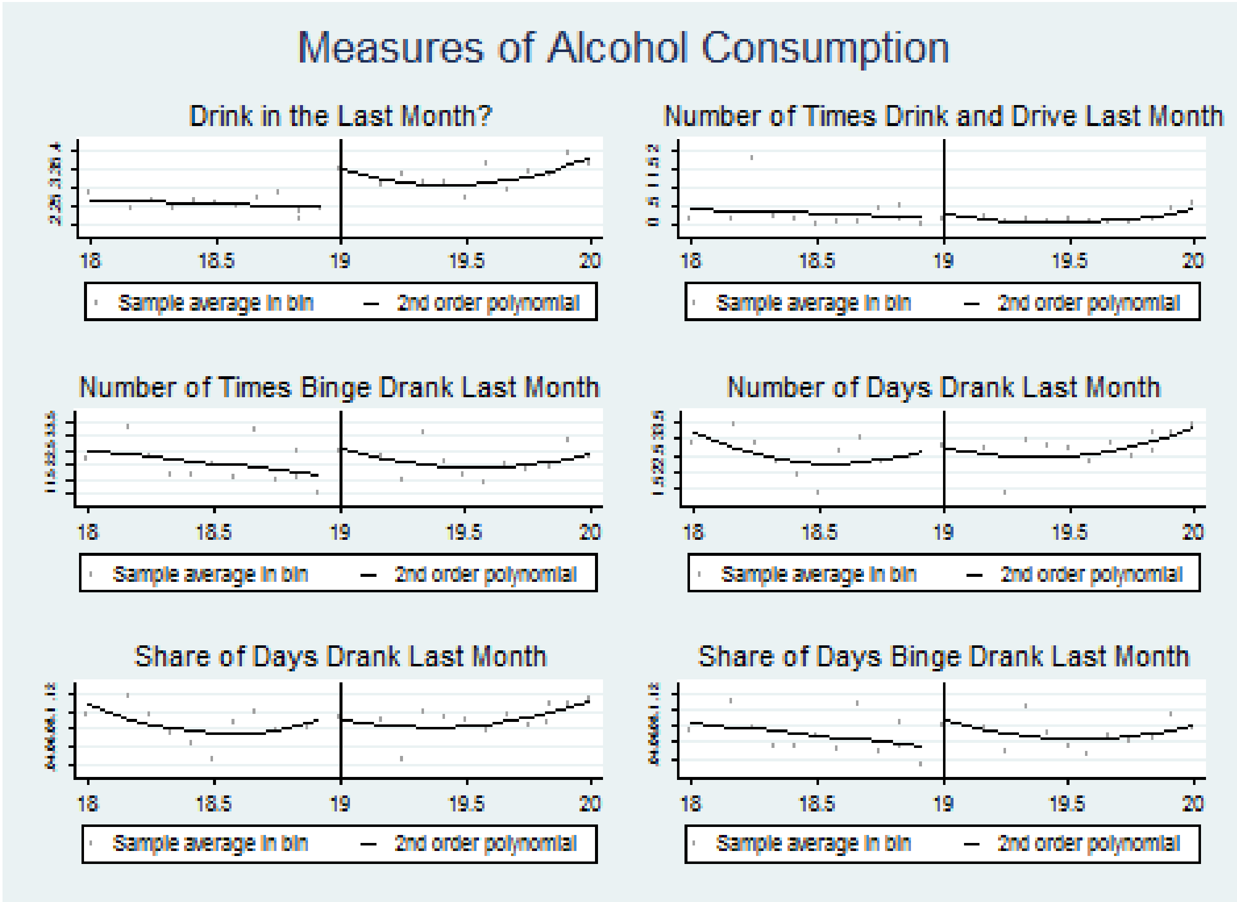

Figure E.2. Measures of Alcohol Consumption

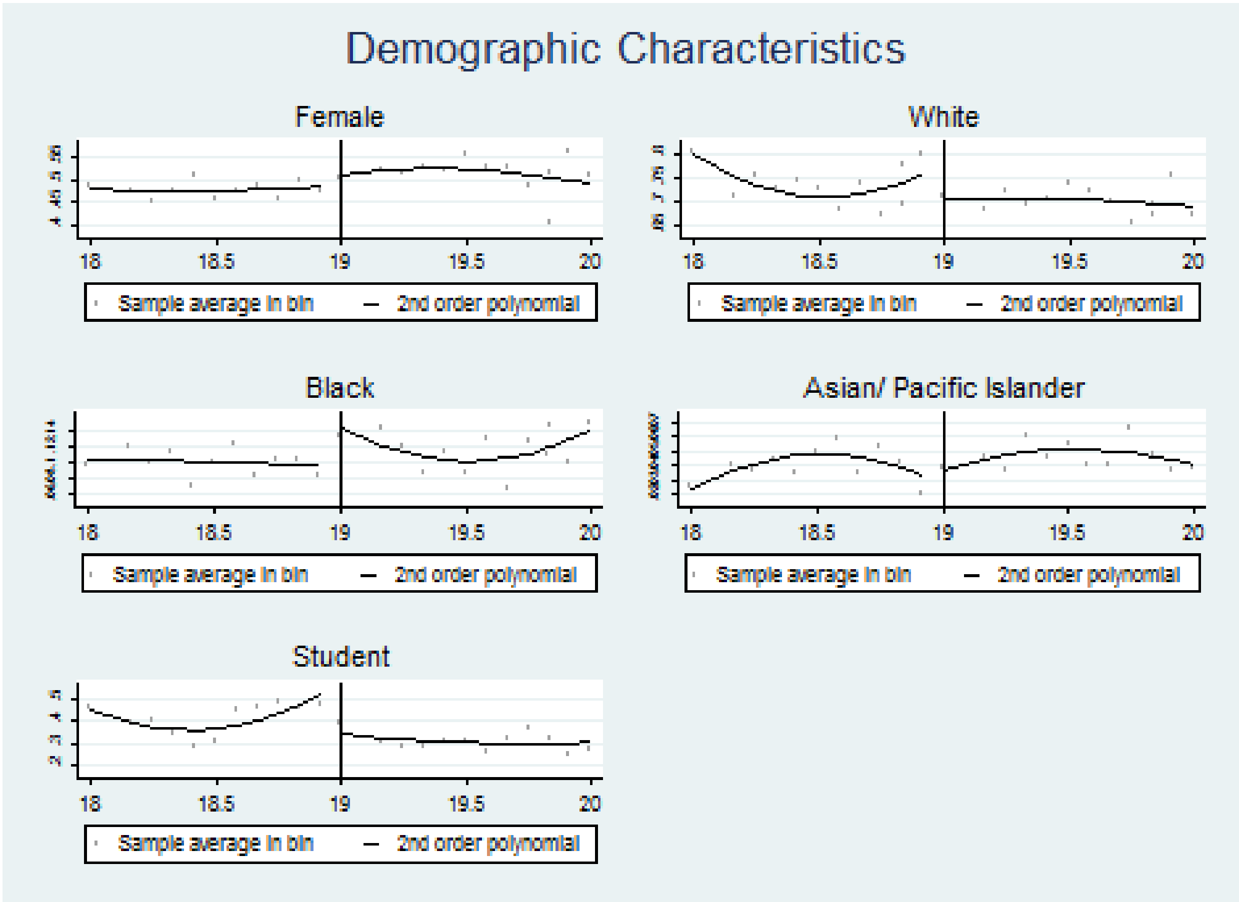

Figure E.3. Smooth Transition of Demographic Characteristics

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BIOGRAPHICAL SKETCH

Christopher Roby did not figure that he would end up entering graduate school and completing a PhD when he graduated high school. No, his main attention was focused on a college experience in Louisiana, and pursuing a degree in social studies education. This would not come to pass, since young Christopher would soon discover his growing enjoyment of economics. Before long he was nearing the end of his undergraduate studies and in need of a plan. He decided that he would pursue a PhD in Economics, even though he needed to spend an extra year preparing for the mathematical requirements of getting into a PhD program. He began his efforts in the fall of 2012 and finished in slightly over five years. Completing his PhD in Economics was an incredibly difficult and rewarding endeavor for him. He is happy to be done, and excited for what the future holds.

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