Economics of Contracts and Risks

Francis Annan

Submitted in partial fulfillment of the
requirements for the degree
of Doctor of Philosophy
in the Graduate School of Arts and Sciences

COLUMBIA UNIVERSITY

2018

Abstract

Economics of Contracts and Risks

Francis Annan

Abstracting from potential incentive costs, both theoretical and applied research on contracts and contract choice suggest that bundling multiple contracts may be optimal. With the abundance of risk and uncertainty, especially among low-income environments that are often ill-prepared, the design and commercial success of contracts for mitigating these risks remain crucial. This dissertation brings together applied microeconomic theory along with careful empirical analyses to study three issues about contracts and risks, with implications for the functioning of markets, financial inclusion, unequal impacts of climate extremes and the design of insurance and financial contracts aim at mitigating environmental risks that confront society.

Chapter 2 studies the potential moral hazard and welfare consequences of interlinking credit with insurance market contracts, establishing that interlinking these two markets not only increases insurance demand, but induces large moral hazard effects in developing countries. Chapter 3 examines environmental risks and their differential impacts on human capital investments, specifically, documenting how Harmattan-induced "Meningitis" outbreaks potentially explain the observed gender gaps in educational attainments in Niger. Chapter 4 evaluates the impact of informal risk-sharing schemes on the adoption of "index" insurance contracts aimed at mitigating climate risks among low-income societies. Two competing forces are identified to show that informal network schemes have ambiguous effect on the demand for formal index insurance, which provides novel explanations for two empirical puzzles about index contracts along with an experimental evidence from rural India. The third project connects the first two via contracts and environmental risks, respectively.

Contents

List of Figures

List of Tables

Acknowledgements

I would like to thank my dissertation defense committee members: Bernard Salanié, Cristian Pop-Eleches, Wojciech Kopczuk, Wolfram Schlenker and Alessandra Giannini. I am very grateful to my advisor Bernard Salanié for his guidance, encouragement and support at all stages of the dissertation process.

For helpful discussions and suggestions, I thank Eric Verhoogen, Rodrigo Soares, Pierre-André Chiappori, Michael Best, François Gerard, Suresh Naidu, Amy Finkelstein, Dan O'Flaherty, Christoph Rothe, Miikka Rokkanen, Serena Ng, Glenn Harrison, Stan Spurlock and participants at Columbia's Applied Micro, Development and Econometrics seminars, Econometric Society AMES 2017 and NEUDC 2017; Aly Sanoh, Andrew Dabalen, Kathleen Beegle, Markus Goldstein, Moussa Blimpo and Xavier Giné of the World Bank and numerous seminar audiences: CSAE-University of Oxford, Georgia State University, and Stockholm School of Economics. I am grateful to both Christine Agbenaza and Romeo Bugyei of SIC Insurance Company Limited, and Joseph Bentor of the National Insurance Commission (NIC) for numerous conversations explaining the data and institutional environment of the insurance industry in Ghana. I am also thankful to the Center for Development Economics and Policy (CDEP) and NYU's Net Institute that provided multiple partial funding for the projects.

Many special thanks to several friends and family for the support and encouragement throughout the doctoral process, notably: Aly Sanoh, Belinda Archibong, Bikramaditya Datta, Matthew Roberts, and Nana Ama Sarfo.

To My Beautiful Family

- Rose Amankwah, Mom
- Francis Boye Annan, Dad
- Bertha Annan, Sister
- Emmanuel Annan, Brother
- Fred Annan, Brother

Chapter 1

Overview

Theoretical and applied work on contracts and contract choice have noted that market arrangements that interlink multiple contracts are optimal. Among other things, the justification for bundling contracts is that it overcome problems of imperfect information and enforcement, induce optimal investment of effort [in principal-agent settings], and relax other contractual frictions such as credit constraints and present-biasedness (e.g., Braverman and Stiglitz 1982, 1986; Bose 1993; Carter et al. 2013; Karlan et al. 2014; Casaburi and Willis 2017). Next, uncertainty is everywhere, and play a central role in the decision-making environment of society (e.g., Gollier 1995). Notable examples include potential risks related to climate and weather events, especially in poor and vulnerable settings, externalities from automobile collisions, sudden health and disease events, risky assets under price uncertainty, to potentially catastrophic risks of greenhouse effect, nuclear Armageddon and genetic manipulations. Exposure to risks poses significant burden (e.g., Intergovernmental Panel on Climate Change 2013) but can generate opportunities (e.g., World Development Report 2014), especially among low-income environments where financial and insurance markets are incomplete (e.g., Townsend 1994; Dercon and Christiaensen 2011).

With these mind, this dissertation brings together applied microeconomic theory along with careful empirical analyses to answer three sets of questions about contracts and risks. First, what are the potential tradeoffs of interlinking credit with insurance contracts (i.e., Demand versus Incentive and Welfare costs)? Second, how does environmentally-induced disease events affect human capital investments, and how are the impacts distributed across society? Third, what formal insurance and financial contracts are available for societies to mitigate environment and climate risks, and how does the choice of these instruments interact with existing informal risk-sharing schemes. Each chapter is devoted to a question as explained below.

In the first project (Chapter 2), I study the moral hazard and welfare consequences of interlocking credit with insurance market contracts. Consumers in developing countries often buy insurance on credit. These are arrangements between insurers and consumers that allow consumers to get coverage now and defer their premium payments to a future period—an analog of interlocking credit with insurance. I show that such arrangements not only increase insurance demand, but induce large moral hazard and net-welfare losses. The approach to this research is to combine a mixed model of adverse selection and moral hazard with theoretical restrictions from agency theory and a shock in the choice of insurance contracts to learn about moral hazard and its effects. First, I show that a simple difference estimator gives a lower bound on the effect of moral hazard, allowing for adverse selection of any form. The shock in contracts come from an unexpected regulatory reform in Ghana that made it impossible to buy insurance on credit, creating an exogenous variation in contract choice: consumers responded by switching to contracts with less coverage. Second, I combine this result with unique administrative data on car insurance contracts to analyze moral hazard; finding robust evidence of moral hazard effects. The estimated cost of moral hazard, averted by the regulatory reform, is about 12 percent of insurance company profits, translating to a total loss of GHC52,703,889 (USD17,567,963) for the insurance industry between two contract periods. Finally, I use back-of-the envelope calculations to compare the loss in consumer welfare attributable to the regulatory reform to the gain in producer welfare. I find that the welfare loss is about 11% of the gain in welfare; suggesting that the reform is not welfare-decreasing, overall.

There are at least three potential mechanisms through which the regulatory reform may have shifted choices of insurance contracts and thus moral hazard: binding credit constraints, financial "savviness", and changes in relative prices. I test for these individual channels using detail data on insurance premiums, credit and premium debt records, finding evidence in favor of credit constraints. In particular, there is evidence that moral hazard is larger for the more credit-constrained consumers based on heterogeneity analysis. I discuss the applicability of these results to the design of other types of insurance: personal insurance, social insurance programs and weather index-based insurance, including the implications for policy.

This work makes three contributions: It advances the study of market inter-linkages by providing a first-line evidence of the moral hazard consequences of bundling credit with insurance contracts. Such potentially negative effect of bundling these two markets has been so far ignored in the literature. Additionally, the proposed mixed model together with the bounding analysis provides a useful benchmark to evaluate the effect of moral hazard—and to conveniently assess the consequences of abstracting from one informational friction. Finally, by exploring the potential channels, this research documents a possible link between credit constraints and moral hazard. While reducing credit constraints may be good, it shows where such policies will create significant inefficiency.

There are several natural extensions of this paper. First, I aim to consider the implications of the proposed approach and findings in other developing countries that currently have similar insurance reforms in force: Nigeria and Gambia. Evidence from these contexts will provide additional external validity and a further evaluation of the growing insurance policies. I also plan to examine the co-impacts of this automobile insurance policy on local air quality by appealing to the literature on the effects of regulation on air pollution. I have done some preliminary analysis using high resolution satellite data from National Aeronautics and Space Administration (NASA), which points to modest reductions in air pollution as measured by particulate matter at the policy cutoff. This reduction in pollution may be attributed to decreases in driving speeds resulting from general fall in coverage and accident rates. Next, I plan to investigate whether interlinking credit with insurance market contracts can induce adverse selection, and the implied welfare implications. There are indications from preliminary analysis that consumers who bought insurance on credit signal as bad risk-types, as compared to their counterparts who paid contracts upfront.

In the second project (Chapter 3), Belinda Archibong and I study the potential unequal gender-impacts of climate-induced disease outbreak "Meningitis" on educational investments (preliminary version published in AER P&P 2017). Persistent gender gaps in educational attainment have been examined in the context of differential parental costs of investment in the education of boys versus girls. In this project, we examine whether disease burdens, especially prevalent in the tropics, contribute significantly to widening gender gaps in educational attainments. We estimate the impact of sudden exposure to the 1986 meningitis epidemic in Niger on girls' education relative to boys. Our results suggest that increases in meningitis cases during epidemic years significantly reduce years of education disproportionately for school-aged going girls in areas with higher meningitis exposure. There is no significant effect for boys in the same cohort and no effects of meningitis exposure for non-epidemic years.

We use theory to explore different channels, highlighting income effects of epidemics on households and early marriage of girls in areas with higher exposure during epidemic years. We also use National Aeronautics and Space Administration (NASA) data to investigate the relationship between climate variables and the meningitis epidemic and explore how climate change could potentially worsen social inequality through widening the gender gap in human capital investment. Our findings have broader implications for climate-induced disease effects on social inequality.

In the third project (Chapter 4), Bikramaditya Datta and I investigate the impact of pre-existing informal risk-sharing arrangements on the take-up of weather index-based insurance contracts, simply termed "index insurance". In this work, we develop a model that considers the case of an individual who endogenously chooses to join a group and make decisions about index insurance. We show that the presence of an individual in a risk sharing arrangement reduces his risk aversion, termed "Effective Risk Aversion"— a sufficient statistic for index decision making. Appealing to such reduction in risk aversion, we show that informal schemes may either reduce or increase the take up of index insurance, providing alternative explanations for two empirical puzzles: unexpectedly low adoption of index insurance and demand being particularly low for the most risk averse. The main intuition follows from the simple observation that in the presence of a risk-sharing arrangement, an individual's risk tolerance is increased compared to the absence of the group. This has two implications for the take-up of index insurance. First, the individual becomes more tolerant to basis risk, an inherent risk in index contracts, and so is more likely to take-up. Second, the individual being more risk-tolerant makes him sensitive to the price of insurance and so less willing to take-up the index cover, thus generating two opposite effects on the decision to purchase index insurance.

Our model provide testable hypotheses with implications for the design of index insurance contracts and the commercial success of such innovative financial products. We draw on data from a panel of field experimental trials in India to document evidence for several predictions that emerge from our analyses. First, we provide empirical evidence that the overall effect of informal risk-sharing on the take-up of index insurance is ambiguous. There is evidence that informal risk sharing schemes may support take-up, finding that when downside basis risk is high, risk-sharing increases the index demand by approximately 13 to 40 percentage points. In addition, there is evidence that the existence of risk-sharing arrangement makes individuals more sensitive to price changes, with an estimated increased elasticity of about 0.34. Our analysis documents that the effective reduction in risk aversion following individuals' exposure to risk-sharing group treatments explains these findings. Finally, we show that an increase in the size of risk-sharing groups decreases take-up. This effect is stronger once we have conditioned on basis risk – a counter force. Strikingly, this result stand in contrast to standard information diffusion models, in which an increase in exposed group size should facilitate uptake of index insurance (e.g., Jackson and Yariv 2010; Banerjee et al. 2013).

In ongoing research, we aim to test the predictions from the model in the laboratory. Further, we plan to draw on the literature on network analysis and multi-dimensional matching to analyze the interactions between index insurance and informal arrangements to inform the design of policy and index contracts. Our results will have broader implications for the design of insurance contracts aimed at mitigating environmental risks among low-income societies.

Chapter 2

Credit-Induced Moral Hazard in Insurance: Theory and Empirical Evidence

Abstract♣

The standard insurance contract requires upfront payments by consumers to protect against potential future losses. However, consumers in developing countries often buy insurance on credit. While this may allow consumers facing credit constraints to acquire more coverage, it might amplify moral hazard with implications for welfare. I evaluate the effect of this moral hazard by exploiting an unexpected regulatory reform in Ghana that made it illegal to buy car insurance on credit, creating an exogenous variation in contract choice: consumers responded by switching to contracts with less coverage. I formulate a model that allows for selection and moral hazard, and show that if contracts with higher coverage only increase claims, a simple difference estimator gives a lower bound on the effect of moral hazard. I combine this result with unique administrative data on contracts to document three additional sets of findings. First, there is evidence of moral hazard in the market which was averted by the regulatory reform. Second, moral hazard is responsible for the reduction in average size of claims and the number of claims by 46% and 22% respectively, leading to a 12% increase in insurance company profits. Finally, I show that abstracting from selection while learning about moral hazard leads one to substantially over-estimate its effect. Back-of-the-envelope calculations assuming risk aversion and limited enforcement of credit arrangements suggest that the loss in consumer welfare attributable to the regulatory reform do not outweigh the gains in producer welfare. These results have wider applicability to the study of market inter-linkages, bundling and credit-constraints.

JEL Classification Codes: D82, D81, G22, O12, O16

Keywords: *Contracts, Moral Hazard, Credit Constraints, Insurance, Credit, Bundling*

♣ Columbia University. Email: `fa2316@columbia.edu`

2.1 Introduction

Consumers in developing countries often buy insurance on credit. These arrangements between insurers and consumers allow consumers to get coverage but defer premium payments to a later period. Such deferral is similar in principle to interlinking credit and insurance markets. The view that market inter-linkages act as mechanisms to mitigate problems of imperfect information, enforcement and to co-develop markets has long been emphasized.[1] In turn, this has led to a growing empirical research that bundles credit with insurance and vice versa, finding either increases or decreases in take-ups, respectively.

When credit is bundled with insurance, the effects on insurance demand have been unambiguously positive. For example, Liu et al. (2016) find that delaying premium payments for livestock mortality insurance increases the take-up of insurance in China; Casaburi and Willis (2017) find even larger increases in take-up rates for a crop insurance product in Kenya.[2] However, by bundling credit with insurance, particularly, it may increase demand but induce moral hazard in insurance, a trade-off that I study. This potentially negative effect of interlinking these two markets has so far been ignored in the literature.

I document that insurance arrangements that defer some proportion of premium payments to the future increase insurance demand, and argues that such contractual arrangements can lead to substantial moral hazard and welfare losses. I evaluate an insurance policy experiment that made it impossible to buy car insurance on credit. Car insurance is crucial for businesses to develop, especially in developing countries where many people operate transport vehicles as small and medium enterprises.[3] It forms a large private market, but

[1] Early works date back to Braverman and Stiglitz (1982, 1986) who show how a principal may interlock two contracts to induce more favorable outcomes. For example, a trader-lender may offer a farmer who borrows from him lower prices on inputs (seeds; fertilizers), since the probability of default is reduced when such inputs are used. Relatedly, Carter et al. (2013) show that interlinking credit and insurance contracts allow both markets to co-develop, as compared to when the markets are in isolation.

[2] When insurance is bundled with credit on the other hand, the effects are mixed. Banerjee et al. (2014) find that by requiring loan clients to purchase health insurance at the time of renewing their loans, many (16 percentage points) borrower clients dropped out of borrowing in India; Karlan et al. (2014), however, find significant increases in the take-up for credit in Ghana.

[3] The employment-gains from car insurance may also be exemplified by a recent innovation in the car

may fail to function and grow due to frictions such as moral hazard and other inefficiencies.[4] The reform allows me to see the change in contract choice that follows the end of the credit market and associated claims. This allows me to characterize how the access to credit in the previous regulatory framework induced moral hazard.

The regulatory reform I study was unexpectedly imposed by the National Insurance Commission (NIC) of Ghana. The reform is called "no premium, no cover" and requires insurance firms to collect premiums upfront before providing insurance coverage. Prior to the change, insurers were allowing customers to purchase insurance coverage on interest-free credit and to pay later; so the reform made lower coverage more attractive.

To learn about moral hazard, I formulate a model that allows for selection and moral hazard and derive bounds on moral hazard. This formulation recognizes the complex interplay between multidimensional selection and moral hazard in insurance. With selection, individuals are heterogeneous in their unobservable attributes such as risk type and risk aversion, and thus self-select into different kinds of contracts. The bounds are based on restrictions that stem from agency theory and exogenous variation in contract choices induced by the policy reform. Following the seminal work of Holmstrom (1979), most of the contracts and moral hazard literature has assumed the Monotone Likelihood Ratio Property (MLRP), which requires that better outcomes are likely due to higher effort. I combine the MLRP with two other conditions. The first is that the actual timing of the reform is

business sector, called "work n pay". Slightly different from sharecropping, work n pay are contractual arrangements that allow commercial drivers and the young to acquire cars and work with it, while making payments for the car within a period of time—typically two and half years. The arrangements are such that the drivers make part payment of the cars and work to pay the rest in installments. Private conversations with work n pay drivers in Ghana suggest that (i) common challenges to this business are accidents and robbery, but (ii) the availability and provision of insurance for the cars largely influence their decisions to sign up for such arrangements. Even, major insurance companies (and the government) have recently taken up the initiative to offer work n pay schemes under soft re-payment terms with full insurance coverage. See https://www.ghanaweb.com/GhanaHomePage/business/200-new-vehicles-for-Youth-in-Driving-265953

[4]A vast theory shows that frictions from information asymmetries (traditionally, moral hazard and adverse selection) limit the ability of formal insurance and credit markets to function (Rothschild and Stiglitz 1976; Ghosh, Mookherjee and Ray 2000). This has led to a careful empirical research seeking to learn and overcome the various informational asymmetries like moral hazard, but with a substantial focus on developed country contexts. Thus, little is known about the relative significance of moral hazard in developing countries, a gap I will fill.

uncorrelated with individuals' unobserved heterogeneity; the second is that customers who select higher coverage contracts will not supply more effort. In the spirit of Manski (1990), these conditions allow me to derive bounds on moral hazard. I show that a simple difference estimator yields a lower bound on the effect of moral hazard. The economic model and restrictions provide micro-foundations for the econometric model and empirical exercise.

I leverage a rich set of customer level insurance and credit records that span 2013–2015 and come from the administrative files of the largest branch of the largest General Business insurance company in Ghana. Two unique features about the data are that: (i) it spans a period before and after the reform that made it impossible to buy insurance on credit; and (ii) it allows one to track customers across contract years. In doing so, I observe who used to buy insurance on credit, and who switched either from higher to lower coverage. The use of administrative data sets on insurance contracts is common for research in developed countries, but in developing countries, data of this kind have historically been unavailable for research. The combination of rich customer level administrative data and quasi-experimental variation from an insurance policy reform enables me to evaluate moral hazard's effect and the possible linkages with credit constraints in a developing country setting.

I start by asking how the introduction of the reform impacted customers' choice of insurance coverage. There are two choices in the contracts menu: basic, which is legally required and provides only third party protection, and comprehensive/higher coverage, which insures against all responsible liability. I find that the policy reform led to a 6 percentage point drop in the share of comprehensive contracts. I also show evidence that consumers who bought comprehensive contracts were more likely to buy on credit than those who only bought basic coverage, and switched to lower contracts after the reform removed the possibility of buying insurance on credit. The overwhelming majority (99.5%) of consumers who used to buy comprehensive insurance on credit switch to cheaper basic-liability insurance, with less than 1.6% dropping out of insurance altogether after the reform

I then exploit the plausible assumption that the actual timing of the policy reform is uncorrelated with individuals' unobserved heterogeneity to construct a simple and general test of the presence of moral hazard. The idea behind the test is that, under the null of no-moral hazard, a change in insurance coverage induced by the reform, and not selection, should not cause a change in claim amounts or occurrence of loss. This follows from Escanciano, Salanié and Yildiz (2016), who show that exogenous variation in contract menus allows for a test of moral hazard under selection. Both graphical and formal tests suggest the existence of moral hazard in this market. This existence test, although simple and clean, only provides inference about whether or not moral hazard is absent; it is unable to evaluate the effect of moral hazard.

I proceed to investigate moral hazard and its effect using the derived bounds. Consistent with the results of the first test, I find strong and convincing evidence of moral hazard. The evidence is robust across various definitions of insurance outcomes. Moral hazard induced significant leakages in insurance claims. The empirical results suggest a lower bound moral hazard estimate of (i) GHC52 (USD18), which translates to 46% of the average size of claims; and (ii) 22% of the number of claims between two contract years. These moral hazard effects, averted by the policy reform, correspond to a 12% increase in average firm profits for the company's auto-business line.[5] Beside the increase in firm profits through reduction in moral hazard, the switch to contracts with lower coverage due to the policy reform may lead to loss in consumer welfare. Under various assumptions about risk aversion, enforcement and repayment of premium debts, I conduct back-of-the-calculations of the welfare loss, and gains from making it impossible to buy insurance on credit. I find that the loss in consumer welfare is about 11% of the gain in producer welfare, suggesting that the regulatory reform is not welfare-decreasing overall.

There are at least two potential channels through which the reform may have shifted

[5]The estimated total cost of moral hazard, averted by the regulatory reform, is about GHC52,703,889 (USD17,567,963) for the insurance industry. Expected revenues and costs associated with providing insurance are simply derived using realized premiums and indemnities from the insurer's policies data, respectively. This calculation allows for an insurance loading of 25% (reflecting the administrative costs of processing claims) but ignores any direct returns on company investments of collected insurance premiums.

choices of insurance contracts and thus moral hazard: binding credit constraints, and changes in relative prices. The analysis establishes that the results are likely driven by credit constraints. In particular, moral hazard is much larger for the group of consumers who tend to buy insurance on credit. However, the decision to buy insurance on credit could either be because the consumers are actually credit-constrained, or financially "savvy" with no intention of repaying their premium debts. I find as high as 79% repayment rates for premium debts, which is inconsistent with the latter. Why repay debts before the expiration of insurance contracts, if the goal is to take advantage of the credit provision? In contrast, the evidence is consistent with credit constraints: consumers who switched to contracts with lower coverage after the regulation were those who bought contracts with higher coverage earlier and with credit. Next, if insurance firms were to adjust premiums in response to the policy reform then it will be unclear whether or not the moral hazard results are also driven by changes in relative prices. I find evidence against such alternative channels. I thus document the significance of the possible effect of credit constraints on moral hazard, in particular in the context of a developing country.

I contribute to several strands of literature. First, I contribute to the literature that examines the importance of inter-linked markets in developing countries.[6] This line of research has appealed to the use of inter-linkages to overcome the inefficiencies from incomplete markets (Braverman and Stiglitz 1982, 1986), along with the development of the various markets (Carter et al. 2013). Many experimental studies have bundled insurance with credit, finding either increases or decreases in the demand for credit (Gine and Yang 2009; Banerjee et al. 2014; Karlan et al. 2014). Others—experimental and quasi-experimental— have bundled credit with insurance, finding significant increases in the take-up of insurance (Liu et al. 2016; Casaburi and Willis 2017). I document the moral hazard and net welfare consequences of bundling credit with insurance, suggesting the difficulty of developing both markets.

Second, I add to the growing empirical literature on testing for the existence of asym-

[6]Bardhan (1980), Bell (1988), and Bardhan (1989) provide surveys about market inter-linkages.

metric information in both private and social insurance markets (Chiappori and Salanié 2000; Finkelstein and Poterba 2002; Krueger and Meyer 2002; Cohen and Dehejia 2004; Cohen and Einav 2007; Einav, Finkelstein and Levin 2010; Einav et al. 2013; Hendren 2013; Hansman 2016; Kim 2017 and many others). Major parts of this literature have focused on (i) insurance markets in developed economies; less so for developing country settings, and (ii) testing the existence of asymmetric information in general by exploiting correlations between insurance purchases and claims; mostly in the spirit of the "positive correlation" tests of Chiappori and Salanié (2000) and Chiappori et al. (2006). This paper contributes by separating moral hazard from selection, as well as estimating the size of its effect[7] in a developing country. The simple way to think about moral hazard's effect is the loss in average profits to insurers or in parts of social value due to its presence. Estimated quantities can be informative in thinking about how to quantify the welfare implications of moral hazard and potential public policy interventions.

Methodologically, this paper differs from the above literature. I develop and use a bounds approach to detect moral hazard, where unobserved heterogeneity or adverse selection is allowed to impact the response function in an unrestricted manner. The unobserved heterogeneity is allowed to be a vector of hidden information without any restriction on the dimension.[8]

There are papers that focus on one informational friction such as adverse selection by

[7] One exception to estimating moral hazard's effect is Schneider (2010), who provides a conservative estimate of moral hazard (about 16%) increase in the accident rate for drivers who own versus lease their taxicabs in New York City. Unlike Schneider (2010), the empirical approach here is nonparametric and focuses on both loss occurrence and claim outcomes. I take advantage of these two outcomes to investigate whether moral hazard is due to occurrence of losses or a shift in the distribution of claims, respectively. Relatedly, Gerfin and Schellhorn (2006) used deductibles as an excluded instrument and statistical restrictions to bound moral hazard. Their outcome variable was the probability of a doctor visit in Switzerland. But unlike Gerfin and Schellhorn (2016), I combine microfounded restrictions with an exclusion from a policy reform restricting the sale of insurance on credit which permits potential linkages between moral hazard and credit constraints, akin to low-income environments. To put the results into context: I provide moral hazard estimates that are 1.5-3.0 times larger than estimates from developed countries.

[8] A policyholder may be characterized by multi-dimensional selection attributes including risk types and risk preferences, and empirical work has shown evidence from different contexts (Finkelstein and McGarry 2006 in long-term care insurance; Cohen and Einav 2007 in car insurance; Davidoff and Welke 2007 in reverse mortgage; Fang, Keane and Silverman 2008 in Medigap health insurance). Yet, an identifying framework that accounts for these adverse selection attributes in an unrestricted manner is still unavailable.

abstracting from moral hazard (e.g., Cohen and Einav 2007). The proposed approach allows me to evaluate the implication of this. Suppose I assume away selection, then I find huge moral hazard effects which are larger than the credible estimates by substantial magnitudes: 4-7 times larger. This exercise documents that abstracting from one dimension can have large and nontrivial consequences. Taken together, the proposed approach provides an alternative benchmark to evaluate the effect of moral hazard, and can be applied to study moral hazard in other insurance and financial market contexts.

Finally, this paper is related to the broader literature that studies the economic importance of credit constraints. Our knowledge about credit constraints is important for the optimal design of private and public programs, as they tend to alter the potential behavioral response to these programs. In developing countries, many papers have shown that liquidity constrains the demand for agricultural insurance (Cole et al. 2013; Karlan et al. 2014), health products such as anti-malaria bed nets (Cohen and Dupas 2010), and induces motives for precautionary saving (Lee and Sawada 2010). In developed countries, liquidity constraints have been shown to limit investment in human capital (Dynarski 2003), and to cause significant response to unemployment insurance durations (Chetty 2008) and consumer bankruptcy decisions (Gross, Notowidigdo and Wang 2014). Since the moral hazard results are explained by credit constraints, this paper establishes a possible link between the two strands of literature on credit constraints in developing countries and market failures through incentive effects, particularly the private insurance sector. In particular, while reducing credit constraints may be good, I document a situation where such a reduction may lead to inefficiency.

The remainder of this paper is organized as follows. Section 2 provides background on the setting and policy reform. Section 3 builds an economic model to highlight the complex interplay between selection and moral hazard. Section 4 discusses the data and research design; 5 presents a test and results for moral hazard based on the research design and formulation in section 3. Section 6 lays out the bounds analysis and presents the bounding results on moral hazard. The possible explanations, caveats and implications are discussed in section 7. Section 8 concludes with applications, extensions and policy dimensions. Details and some proofs are in the Appendix.

2.2 Setting and policy experiment

I discuss the details of the institutional setting, policy reform and reasons underlying the motives of insurance firms in lending premiums in this section. I had extensive personal conversations with insurance companies. The findings, which are largely consistent with the empirical evidence are presented to motivate the research approach.

2.2.1 The legal environment

Automobile insurance is compulsory in Ghana, as in other countries.[9] By this, all individuals operating a car are legally required to purchase insurance. This is usually for two principal reasons. First, compulsory insurance ensures that some compensation is provided for those who are injured in automobile accidents. Second, it forces drivers to internalize part of the externality imposed on others by their driving, especially in the case where drivers have bounded assets (Cohen and Dehejia 2004).[10]

In Ghana the specific types of auto insurance contracts can range from “third-party” liability to “comprehensive” coverage. The minimum requirement by law is the third-party which provides protection to others when accidents occur. Comprehensive contracts, on

[9]Compulsory insurance regulation was first introduced in Ghana in 1958. The Motor Vehicles (Third Party Insurance) Act 1958, ACT 42 makes it illegal to drive a motor vehicle on public roads without insurance covering third-party liabilities, at a minimum.

[10]In low-income and developing country contexts, individuals likely have very limited assets. This may provide more justification for compulsory automobile insurance laws in such contexts.

the other hand, provide coverage for all responsible claims. Enforcement of the compulsory insurance law embody two dimensions: automobile drivers are required to report their insurance status at the time of an accident, and penalties can range from large fines to jail terms when the driver is unable to show proof of coverage. Even so, enforcement can be limited. For instance, it is estimated that about 20-36% of cars in Ghana are uninsured.[11]

2.2.2 The market, regulation and why it was introduced: in brief

The insurance industry in Ghana has undergone many periodic modifications through the passage of various acts and reforms. The industry in its current state is largely governed by Insurance Act 2006, ACT 724. Act 724 is a national act and complies with the Core Principles of International Association of Insurance Supervisors (IAIS) as well as providing regulatory powers to the National Insurance Commission (NIC 2011; NIC 2015). ACT 724 made the insurance industry more regulated, where the NIC is granted powers to regulate and control the business of insurance markets in Ghana. A significant feature of this market, particularly for car insurance contracts, is that the NIC regulates and effectively sets the premiums for policies by providing a uniform price formula to all insurance firms.

On April 1 2014, the NIC introduced a reform called “no premium, no cover”. Figure 28 in the Appendix shows the timeline of the policy. The regulators agreed on the policy

[11]Data about uninsured cars are, of course, not available. I estimate the fraction of uninsured using the following back-of-envelope exercises. For 2012: The National Insurance Commission (NIC) of Ghana issued 759,691 stickers to identify cars that have legitimate insurance cover. But the Driver and Vehicle Licencing Authority (DVLA) reported that 946,284 vehicles were inspected for roadworthiness. This means that about 186,593 vehicles on the roads did not have insurance cover; suggesting a 19.72% uninsured rate. See https://www.ghanaweb.com/GhanaHomePage/business/200-000-cars-without-insurance-270312 For 2014, I estimate that about 36% of all registered cars are uninsured: I collected data from the National Road Safety Commission (NRSC) about total number of registered cars (1885836). I then estimated the total number of insured cars in Ghana (~1190476). I estimate this by dividing the number of insurance policies at the end of my sample (~30,000) by the product of the share of the market for the company that provided the data (21%) and the best guess of the share of policies from the company’s headquarters branch (12%) where the contracts data came from. Finally, I divided the difference between the total number of insured cars and the total number of registered cars by the total number of registered cars; yielding about 36.01% uninsured rate.

on October 12, 2013, and then announced and implemented it on April 1, 2014—resulting in an implementation lag of seven (7) months. This policy reform requires all insurance firms to collect premiums upfront before providing insurance coverage. People were able to buy coverage on credit and pay later before the reform began. The reform marked the end of the credit market for auto insurance premia, and directly implies that insurance companies will no longer be able to sell insurance products on credit to customers. The sale of insurance on credit created an accounting problem: premium payments were delayed leading to a mismatch in the actual re-payment times and the preparation of balance sheets. All unpaid premiums at the time of preparing financial statements are declared outstanding. This made it difficult for insurance companies to pay their reinsurance premiums on time since most premiums remained outstanding. In turn, the reinsurers were unable to pay their retrocessionaires on time; exposing the entire industry to substantial liquidity risk.

2.2.3 Pre-policy regime: stylized facts

Before the introduction of the reform, insurers were essentially serving a dual role: loss-risk takers and premium-lenders. Enforcement of lending or credit arrangements is based on the direct repeated interactions between the insurers and consumers. In addition, insurers use market intermediaries (i.e., insurance brokers and agents) to enforce credit arrangements as many insurance contracts are acquired through the intermediary channels. As shown in Figure 2.1, about 53 percent of all contracts sold prior to the reform were through intermediaries. Intermediaries have a better motivation to collect premium debts, as most insurance companies would not pay all commissions[12] due unless the premiums are paid.

From the consumers' side, they were able to enjoy flexible payment terms by deferring the payment for their policies to a later date. In instances where there is a loss while the premium

[12] The commissions averaged about 5% per unit premium.

is still outstanding, consumers are required to settle the premium arrears in full before the loss is paid. In other instances, however, the premium outstanding is deducted from the loss payment before payment to the policy holder. In part, this uncertainty combined with the crucial role of trust in insurance transactions explains why only 27 percent of consumers acquired insurance on credit prior to the regulatory reform. Figures 2.2a and 2.2b show the take-up of credit to buy insurance over time prior to the regulation. Both figures are based on a probit regression of whether or not a customer purchased insurance on credit. Figure 2.2a includes only monthly dummies as regressors, whiles Figure 2.2b adds a linear control for time trend and customer characteristics. The take-up of insurance on credit is stable across the various months before the policy's implementation. This suggests that the implementation of the no-credit policy was unexpected by consumers.[13]

From the side of insurers, it was common for firms to report outstanding premiums on their annual financial statements. Figures 2.3(a) and 2.3(b) show the distribution of premiums in debt prior to the regulation. The figures reflect the amount of premium (GHC) and its percentage as a share of actual premiums at the time contracts are signed, respectively. For customers who bought insurance on credit, there is evidence of substantial premium debts, ranging between 0.2-100% of premiums. Together, the total debt represents 64.2% of actual premiums for consumers who took insurance on credit. Expressed as a share of all premiums for the auto-business line, this is about 33.3%.

Given that many insurance contracts were sold through market intermediaries, I superimpose the distribution of premiums in debt across the two sources of selling insurance policies in Figure 2.4. There is evidence that consumers are more likely to initiate contracts on credit

[13]It is reassuring that consumers did not anticipate the actual implementation or announcement of the regulation. This is useful in Section 4, where I argue that the actual timing of the policy is exogenous and uncorrelated with unobserved consumer attributes.

through the intermediary channels.

Finally, as discussed earlier, enforcement of credit arrangements relies on direct repeated interactions and the use of market intermediaries. I assess this in Figures 2.5(a) and 2.5(b) showing the repayment rate of outstanding premiums. Figure 2.5(a) indicates that 21.3% (out of 27.0%) of customers who purchased insurance on credit repaid their premium debts before their contracts expired; this translates to a repayment rate of about 79.0%. The repayment rate is not significantly different if I look at the actual amount of premiums in debt. Figure 2.5(b) shows that about 24.4% (out of 33.3%) of the total premium debts were repaid prior to the no-credit policy. This implies a repayment rate of about 73.2%. Both results point to a high repayment rate of outstanding premiums prior to the policy's implementation, suggesting a low credit risk/delinquency for allowing consumers to buy insurance on credit.

Why were companies willing to accept credit payments before the reform?

It is surprising that the insurance firms were lending premiums. What is especially striking is that they were accepting credit payments at interest-free rates. I summarize the two principal reasons below.[14]

Competition under regulated-prices As discussed earlier, the NIC effectively sets the premiums. So the insurance firms were essentially selling regulated-price contracts, with no room to directly influence how their prices are set. Thus, giving credit was considered a way to indirectly influence or reduce prices to maintain their market share. The zero-interest rate can be understood formally in a simple model of two competing firms who take premium as given, and then compete over credit. Applying Bertrand strategies, I find that zero or even

[14] Several possible reasons are discussed, but the first two presented here are the primary explanations, and the rest are relegated to the Appendix. These discussions yield testable implications that future work will aim to explore.

negative interest rates are possible equilibrium outcomes. An illustration is provided in the Appendix.

Application of accounting standards and reserve requirements Operating within accounting frameworks, it is assumed that once someone owes the insurance company, it is an asset for the insurance company. The outstanding premiums actually make the companies' accounts look more attractive on the surface, regardless of the opportunity costs: forgone investments, earning a positive return. For companies to formally operate, they are required to meet certain capital and reserve requirements set by regulators. Hence, providing coverage on credit was considered a good strategy to circumvent such reserve requirements. Furthermore, most of the outstanding premiums were eventually recovered later. As a result, selling insurance on credit was deemed less risky (i.e., low credit risk).

2.2.4 Post-policy regime: stylized facts

The policy mandate disallowed the purchase of insurance on credit: consumers cannot defer or owe any portion of their premiums. In addition, firms were required to write off all premium debts from their books.

The reform was strictly enforced. Since its introduction, the NIC undertook occasional unannounced visits to audit insurance company records. The penalty of noncompliance is as high as ten (10) times the amounts in outstanding debts, forcing the insurance companies to comply with the reform's requirements. The no-credit reform system ultimately helped to cut down the rising outstanding premium profiles of insurance companies. At the same time, it ensured that the companies had enough capacity to honor their reinsurance obligations. There were two additional effects: (i) policyholders who could afford full payment but were taking advantage of the credit-based system had to pay in full; and (ii) those who were credit constrained and could not afford higher coverage had to cut down coverage. I discuss these two effects as candidate mechanisms underlying the results.[15]

Finally, most countries in the west-African sub region have embraced similar market policy reforms. For instance, Nigeria and Gambia have followed with similar no-credit regulations in 2014 and 2015, respectively. These regulations have been projected to have positive implications for the balance sheets of underwriting companies and the overall financial health of the insurance industry.

2.2.5 Private conversations with company

To better understand the impact of the reform[16], I had private conversations with staff and managers of the insurance company that provided the data. Some extracts from the personal conversations follow:

> "*The April 2014 reform triggered some important changes. Particularly, it made insurance unaffordable to clients in that most folks dropped from more generous [Comprehensive] to basic [Third party] plans.*"
>
> "S*ome of our clients switched from Comprehensive to Third Party plans because the reform made the insurance purchasing rule more stringent.*"

The quotes resonate with economic intuition as the reform imposed additional liquidity-cost on the purchase of insurance. There are potential income effects from constraints in liquidity

[15]Section 2.3 suggests a low credit risk due to the higher repayment rates of premium debts, 79%. This will seem imply that the latter effect (credit-constraint) dominates. I explore this in more detail in Section 7.

[16]All insurance products, excluding life insurance, are broadly classified in the industry as General Business. The analysis utilizes a rich set of individual level auto-insurance records (spanning 2013-2015) that come from the administrative files of the largest General Business insurance company in Ghana (about 21% of the entire market in 2014; the data description is contained in Section 4). The company offers different insurance products through their business lines e.g., automobile, workman compensation, bonds, marine, and etc. I focus on the automobile insurance line which accounted for 55.4% of their net premium holdings. In addition to the simple nature of auto-contracts, automobiles pose environmental consequences that will be studied later as an extension to this paper.

which affects insurance purchase as a normal good. Figure 2.6 provides supportive evidence from the insurer's data

This figure is based on a simple frequency estimator. First, the figure demonstrates that the market share of comprehensive cover is significantly lower due to the introduction of the reform. Second, the drop in probability of the purchase of comprehensive contract is substantial, about 6 percentage points. It is useful to note that most of the comprehensive policyholders credit prior to the reform, and so were directly affected by the reform. This can be seen from the transition matrix displayed in Table 2.1. In particular, over 99.4% of consumers who purchased insurance on credit (27%) switched from comprehensive to basic contracts after the no-credit regulation. Most notably, customers who acquired comprehensive contracts were much more likely to do so on credit, compared to customers who bought minimal coverage. The reform provides plausibly exogenous variation in customers' choice of contracts: basic versus comprehensive insurance. The background of this research's design is based on the major policy change, in which that policy change is used as an instrument for contract choices.

In the next section I present a simple economic model that illustrates a selection problem confronting the analysis of moral hazard, to guide the empirical analysis moral hazard and its linkages with liquidity via the policy reform.

2.3 Mixed economic model and effects

I consider a typical insurance market set-up where consumers have asymmetric information, which allows for adverse selection and moral hazard. Two economic actors enter into a contract: the principal, or the insurer, and the consumer, or the insuree. Multiple contracts may be offered. I "black box" the principal's role and focus on the consumer. A key feature of the set-up is that consumer's private information matters to the principal but is unobserved to the principal.

2.3.1 Background model

Index a consumer by i and consider a population of insurance customers whose observed characteristics are denoted by X_i. The observed characteristics of customers are assumed to be exogenous. I will ignore conditioning on X_i for convenience.

Technology *&* Contract Formally, the consumer i owns the following production technology

$$Y_i = g(e_i, \alpha_i^y, \varepsilon_i)$$

where Y_i represents the insurance outcome. ε_i is a random variable that may capture random circumstances in the production technology, e.g. weather, and e_i denotes the customer's choice of effort, capturing the prevention of accidents or limiting their severity; and α_i^y captures hidden information that enters the customer's productivity. The principal observes the outcome Y_i but not the customer's effort e_i or random variable ε_i. The consumer chooses his e_i before the realization of ε_i occurs. I will sometime refer to $g(.)$ as the structural response function.

Index a contract type by d. Define an insurance contract as $C_d = \{\Pi_d, I_d(L)\}$. This pair specifies the insurance premium $\Pi_d \geq 0$ and indemnity $I_d(L) \geq 0$ for some loss size L. Let D_i denote the customer's choice of contract. I shall restrict attention to binary contracts $D_i \in \{d : \ 0, 1\}$, to be consistent with the empirical setting where consumers choose either basic or comprehensive contract cover, respectively. In this case, Π_0 is the premium for basic contract and Π_1 is the premium for comprehensive contract.

Timing & Model Let α_i^u be hidden information that enters the customer's utility function u, capturing preferences and risk aversion, and define $\alpha_i = (\alpha_i^y, \alpha_i^u)$. The vector α_i can be thought of as customer's unobserved heterogeneity. To derive the model that guides the subsequent analysis, consider the following sequence of customer's moves. First, the consumer i privately observes his type α_i. Second, conditional on his type, the consumer makes a contract choice over $D_i = 0, 1$. Third, suppose that the consumer chooses D_i. Then conditional on $(D_i,\ \alpha_i)$, effort levels are respectively chosen as[17]

$$D_i = 0 : \max_{e_i} \left(\mathop{\mathbb{E}}_{\varepsilon} u[R(Y_i, \Pi_0, I_0)] | e_i, \alpha_i - e_i \right)$$

$$D_i = 1 : \max_{e_i} \left(\mathop{\mathbb{E}}_{\varepsilon} u[R(Y_i, \Pi_1, I_1)] | e_i, \alpha_i - e_i \right)$$

where $u[]$ is a von Neumann-Morgenstern utility function that satisfies standard conditions. $R(Y_i, \Pi_d, I_d)$ denotes the net income flow from buying insurance and expectations are taken over the random shocks ε.[18] Here, the consumer will optimally choose his level of effort to maximize his expected utility less his disutility from effort. Effort e_i has been normalized so that one unit of effort translates into one unit of disutility in expectation.

Putting all the pieces above together, $e_i = e_i^*(D_i, \alpha_i^y, \alpha_i^u)$ solves

$$\max_{e_i} \left(\mathop{\mathbb{E}}_{\varepsilon} u[R(Y_i, \Pi_d, I_d)] | e_i, \alpha_i - e_i \right)$$

This implies that $D_i = \sigma(\alpha_i)$ and $e_i = e_i^*(D_i, \alpha_i)$. Together, the implied model can be cast as a triangular system

$$Y_i = g(e_i^*(D_i, \alpha_i), \alpha_i^y, \varepsilon_i)$$

$$D_i = \sigma(\alpha_i)$$

[17] A summary of the model's timing is provided in Figure 2.25 in the Appendix. There is a uniform menu of contracts across all firms in the empirical environment so direct competition (e.g., via price; product), which could permit consumers to strategically seek for "better" priced-contracts across firms, is of little concern. The insurance market is highly regulated and controlled by the government, as discussed in Section 2. The model set up is a recursive problem, where in principle the customer will also choose the contract $D_i = 1$ if and only if its net flow utility is the highest among the other feasible candidate contracts.

[18] Note the difference between α_i^y and ε_i: α_i^y are all productivity shocks available to the consumer before contracts are established (e.g., pre–contract weather realizations), but ε_i does not come in until efforts are made and thus beyond the customer's control (e.g., post–contract weather realizations).

Discussions The model formally shows how e_i maps into D_i and $\alpha_i = (\alpha_i^y, \alpha_i^u)$. First, note that consumer i's contract choice is not randomly assigned. This crucially depends on his type, as illustrated above; hence the observed random variable D_i is potentially endogenous. The endogeneity of D_i may also arise through the correlation of the unobservables $(\alpha_i, \varepsilon_i)$. In this mixed model, it is difficult to learn about moral hazard alone because the choice of contracts that will create incentives for effort choices are also determined by unobserved heterogeneity or some exogenous, third factor.[19] One possible solution would be to assume that unobserved heterogeneity α_i, which structurally leads to nonrandom sample selection, is some additive term in a model that is linear in outcomes and contract choice, and then use fixed effects to control for this. But clearly controlling for fixed effects by differencing out additive α_i terms may be inadequate.

I explore the idea of instrument exclusion from a regulatory change to learn about moral hazard, in which the change exogenously modify the contract choice and incentives. The following discusses the regulatory change approach and how it is used to quantify the effect of moral hazard.

2.3.2 Effects and definitions

To cast the problem using counterfactual notation as in the treatment effects literature, that is, the outcome that would have been observed if the consumer i with unobservables α_i and ε_i had been assigned the contract d, I fix d. This means that the customer's level of effort can be written as $e_i^*(d, \alpha_i)$. The corresponding production technology is

$$Y_i(d) = g(e_i^*(d, \alpha_i), \alpha_i^y, \varepsilon_i)$$

One needs an instrument Z, which is uncorrelated with unobserved heterogeneity; with

[19] A naive test for moral hazard in the mixed model will either directly exploit the correlations between the customer's outcome Y_i and contract choice D_i, or between the outcome Y_i and the level of effort $e_i^*(.,.)$. But unfortunately, neither of these two approaches yields reliable inference since the correlations may be due to adverse selection α_i.

$D_i = \sigma(\alpha_i, Z)$ to exogenously shift the $\sigma(.,.)$ function. In the empirical analysis, Y_i represents insurance claims or loss occurrence and the instrument Z represents policy changes that exogenously induce changes in choice of insurance contracts. $D_i = 0$ corresponds to compulsory or basic contracts. These are mandatory contracts that drivers are required to purchase by law. Under this contract, only third party protection is provided for responsible claims. $D_i = 1$ corresponds to comprehensive contracts, where protection is provided for *all* responsible claims. Intuitively, a typical comprehensive contract covers all loss events that the basic contract covers (e.g., third party injuries), in addition to other events that are not covered under the basic contract (e.g., missing own vehicle parts, crash in a tree). Hence, different contracts present different incentives for consumer actions and outcomes; with comprehensive contracts providing lower incentives for desirable outcomes, as compared to basic contracts.

Moral hazard defined Following Escanciano et al. (2016) and Salanié (2005 Ch. 5), I define moral hazard as the causal impact of contracts. This embodies all non-contractible actions that affect the occurrence and distribution of losses or claim outcomes due to the terms of the contract. Specific examples include costly parking at safer places, wearing a seatbelt, and other negligence, whether it is strategic or mechanical. With this definition, two potential sources of moral hazard are possible: "ex-ante" moral hazard which occurs through changes in unobserved preventive efforts and "ex-post" moral hazard that arises when customers under-report claims by withholding claim or loss information strategically (Cohen and Einav 2007).

Formally, suppose there is no moral hazard, then it must be that $Y_i(d) \overset{dis}{\approx} Y_i(d')|Z_i$ $\forall d \neq d'$ where $\overset{dis}{\approx}$ is the shorthand notation for "has the same distribution as". Suppose there is moral hazard, then $Y_i(d)|Z_i$ should increase with coverage (d), where d corresponds to a contract choice. In this case $Y_i(d)|Z_i$ increasing in coverage implies that worse outcomes are exogenously realized under higher coverage. I observe an IID sequence of observations

$\{(Y_i, D_i, Z_i)_i : \; i = 1, ..., I\}$. Identifying moral hazard in the mixed model of moral hazard and selection is equivalent to examining changes in the joint distribution of $(Y, D)|Z$. The focus will be on

$$\mathbb{E}[(Y, D)|Z]$$

I drop the subscript i for easy illustration. The key point is that there exists a causal chain where Z exogenously shifts the distribution of $\sigma(.)$ (i.e. equivalently $\mathbb{E}[D|Z]$) which will in turn shift the distribution of Y via $e^*(.,.)$. More generally, I define the average structural function ASF (Blundell and Powell 2003) as

$$\mu_z(d) = \mathbb{E}[Y_i(d)|Z = z] \equiv \int g(e_i^*(d, \alpha_i; z), \alpha_i^y, \varepsilon_i) dF(\alpha_i, \varepsilon_i), d = 0, 1$$

where $F(.)$ represents the joint distribution of the unobservables α_i and ε_i. Next I can define the average treatment effect ATE of $D_i = 1$ versus $D_i = 0$ to be

$$\Delta = \mu_z(1) - \mu_z(0) > 0, \mathrm{MH}$$

Δ essentially quantifies the average effect of exogenously shifting all consumers from the treatment status $D_i = 0$ to $D_i = 1$. As indicated above, $\Delta > 0$ is required for moral hazard (MH). The presence of moral hazard leads to worse outcomes, which is measured by the size of $\triangle$; I call this the moral hazard effect (MHE). I derive bounds for the three objects $\mu_z(1)$, $\mu_z(0)$ and Δ. The approach utilizes a model that is nonseparable in unobservables $(\alpha_i, \varepsilon_i)$ along with a plausibly random and exogenous policy instrument to eliminate contaminations that may be due to adverse selection.

2.4 Data, measurements and research design

This section describes the data and main research design, which requires the distribution of unobserved heterogeneity to be similar before and after the reform. I carry out several checks showing the validity of the policy instrument and research design.

2.4.1 Data

As mentioned in Section 2, I combine data from two major sources: administrative data and surveys.[20] The surveys embody private conversations with drivers and staff from the insurance company that provided the administrative data. From the administrative data, I observe the complete contract profile for each policy holder i in the insurer's files across two contract years t (2013/14 and 2014/15). Notable features of the data is that it spans a period before and after the policy reform and allows me to track customers over time. I define the following set of variables based on information from the data.

Treatment: $D_{it} = \mathbf{1}\left[Comprehensive\right]$ is an indicator for the choice of insurance contract, where basic contracts correspond to $D_{it} = 0$ and comprehensive contracts correspond to $D_{it} = 1$. The definition is guided by the nature of the Ghanaian automobile insurance market where consumers choose from the contract menu: basic versus comprehensive. As discussed in Section 3.2, basic contracts cover damages only for others, while comprehensive contracts cover all responsible claims when accidents occur.

Policy Instrument: $Z_{it} = \mathbf{1}\left[\tau_t > \bar{\tau}\right]$ equal to 1 for the contract period τ_t after the major National reform $\bar{\tau}$. This construction follows because the introduction of the policy reform created an exogenous variation that induced changes in consumers' choice of insurance contracts. Since I exploit an instrument which comes from the reform changes before and after $\bar{\tau}$=April 1 2014, the identifying variation is essentially from a pre- and post-design, although different customers, particularly those who bought comprehensive contracts, were largely affected by the policy change, yielding an analog of difference-in-differences.

Policy instrument's relevance Figure 2.6 documents the relevance of the policy instrument. It demonstrates that contract choices changed dramatically following the reform. Although skipped here, it is straightforward to formally test for relevance under the hypothesis that the reform does not affect insurance choice.

[20] Additional data about industry aggregates are obtained from the annual reports of insurance companies and the NIC. Traffic information about overall accident rates and registered vehicles are also obtained from Ghana's National Road Safety Commission of the Ministry of Transportation. http://www.nrsc.gov.gh/

Outcome: Y_{it} denotes either claim amount or loss occurrence that is realized by customer i at time t. These are the two main outcomes of interest. The claim outcome is defined as the per period insurance claim received by a policyholder. There are two contract years spanning the days between April 1, 2013 - March 31, 2015. Claims (or loss occurrence) cannot be less than zero so I treat all negative outcomes in the data set ($<$0.001% of sample) as missing at random, as these are likely errors.[21]

Controls: It is important to condition on all publicly observed customer characteristics (Chiappori and Salanié 2014) that either determine or do not determine insurance prices. The data set includes a rich set of individual level information from the insurance company. These include the following variables: (I) Level of no-claim-discount NCD: This measures the amount of premium discount that the policyholder receives from the company. In practice, customers receive a discount in period t for a no loss record in $t-1$. The discounts are adjusted accordingly once the customer gets an auto accident that triggers an insurance payout. To prevent under-reporting of claims, discount amounts are typically less than claims amounts.[22] While I do not have enough data to explicitly model dynamics, I believe the NCD variable possibly captures how customers respond to losses and discount across different contract periods. (II) Riskiness/loadings: This is an industry measure useful for the determination of premiums. It reflects the firm's perception about customers riskiness

[21](i) Summary statistics of the data are presented in Tables 17-19 in the Appendix. (ii) The overall claims ratio is 22%. This reflects the amount paid out to insureds in comparison to premiums received by the insurer between April 2013 to May 2015. That is to say just GHC22 was paid out of every GHC100 paid in premiums, suggesting that "poor value for money" is given to policyholders. This number is by far below internationally accepted standards of 60%-80%. Clearly, under this schedule, it will be difficult to win the confidence of an average Ghanaian into insurance. This alleviates potential concerns about the entry of new customers. (ii) It can be misleading to directly compare claims for basic contracts to comprehensive contracts since insurers data for the former typically exclude some liabilities of own damages, in part. I address this following Chiappori et al. 2006. The details are in Appendix A.3.

[22]One can imagine that insurees may fail to report claims in order to receive discounts and get lower prices, especially after the regulatory change. This is less likely since discounts are set to be less than claims. As I also show empirically, pre-regulation discounts and prices are distributionally similar to post-regulation discounts and prices – an evidence that speaks against potential under-reporting. Such information-hold up is usually termed "ex-post" moral hazard. The empirical analysis suggests that ex-post moral hazard is less, as compared to ex-ante moral hazard (unobserved loss preventive effort or behavior).

and the expected size of liabilities in case accidents occur. (III) Year of car manufacture: This provides a measure of the age of insured cars. The range for this variable is between 1957-2015 in the sample. Thus the sample span a mix of both old and new cars. (IV) The make of car, body-type, coverage certificate-type as well as the transmission system are available.

I denote by X_{it} the vector of all controls. The control variables are helpful for improving the empirical analysis. The variables in category (IV) are available to the insurer, but these are not used in the pricing of insurance and therefore can be used to control for potential selection along such observable dimensions. One additional advantage is that the variables allow me to circumvent an empirical challenge which is discussed in Appendix A.3. Next, part of the discussions about plausibility of the instrument's exclusion exploit changes in the distribution of these observed vector of characteristics.

Credit records: Finally, data on customer credit histories and outstanding premiums are available. Both the discussions and illustrations in Section 2 utilize this data.

2.4.2 Research Design: Strategy, exclusion Z, and balance

Strategy: In an ideal experiment designed to evaluate the effect of moral hazard, I would observe insurance outcomes for two similar consumers, then randomly assign one from comprehensive to basic contract ("treatment"), maintain the other on the comprehensive contract ("control") and then compare changes in their insurance outcomes. The regulatory reform helps to mimic this condition. The no-credit regulation made one group of consumers switch to basic contracts (switchers or "treatment"), as exemplified by the remarkable decline and switch in purchases for comprehensive contracts in Figure 2.6 and Table 2.1. The rest of the consumers remained unaffected by the regulation (no-switchers or "control").

Exclusion and balance: With this strategy, it is crucial that the policy "instrument" be *excluded*, that is, conditionally independent of insurance outcomes.[23] An alternative way to state this is:

$$Z \perp\!\!\!\perp [\alpha, \varepsilon] | (D, X)$$

In words, this says the *distribution* of the pair $[\alpha, \varepsilon]$ does not change after the reform, conditional on the relevant characteristics. This condition cannot be tested, so I will run robustness checks to show that the empirical design is valid.

Perhaps the most important concern is that the actual timing of the regulation may have been anticipated by consumers and so they might have reacted to it. For example, credit constrained customers can change their choices and other characteristics to make the effective difference in price between high and low coverage contracts negligible. Such responses can threaten the validity of the policy instrument and research design. Analogous to standard regression discontinuity RD design (Imbens and Lemieux 2008; Lee and Lemieux 2010), one can think of time as a running variable. This requires consumer characteristics to be similar at the policy cutoff to be valid. First, as shown in Figure 2.2, "representative" consumers did not expect the actual announcement of the regulation as average credit decisions remained largely stable across the various months prior to its implementation. Second, Figures 2.7-2.10 jointly indicate a strong balance on the set of relevant control variables.[24] Specifically, the various distributions are not distinguishable at the policy cutoff.[25] Both lines of evidence

[23]First, note that this set up allows for adverse selection of any form, but the actual timing of the policy is unaffected by it. Second, this independence condition provides a direct means of (1) testing for the *absence* of moral hazard (Escanciano, Salanié and Yildiz 2016) and (2) bounding moral effects. In Section 5, I exploit this condition to construct a simple and general test for moral hazard's existence, while in Section 6 I use it as an exclusion for selection to derive worst-case and tight-bounds on the effect of moral hazard.

[24]These controls include variables that are used to price insurance and those that are not but observed. If the distribution of α (e.g., risk aversion) changes as a result of the reform, such changes might reflect in consumers characteristics. It is reassuring that observed consumers characteristics did not change around the policy. With the validity of the instrument's exclusion, I argue the reform "only" induced exogenous assignment of contracts which in turn affected customers' effort and other hidden actions.

[25]In a heterogeneity analysis, I estimate a simple model ($riskiness_{it} = \mu + \theta_r \text{Switcher}_{it} + \epsilon_{it}$) that

suggest that the regulation was not anticipated.[26]

Another concern is that the timing of the regulation may correlate with current macroeconomic conditions and other factors that influence insurance claims. Notice that the data covers only two contract years, spanning contracts before and after the reform — implying a short period of time. First, I did a careful search of all related policies, and the records show that no other insurance reforms took place at around same time. Both α and ε can change if other insurance reforms took place over the period.

Next, the regulatory decision may reflect current economic conditions and have confound the estimates. This would be an important concern if the reform could be implemented quickly. In practice, however, the implementation of insurance policies typically occurs with a substantial lag. For the no-credit regulation, there was a seven (7)-month lag in its implementation as shown in Figure 2.28, further strengthening the case for the validity of the policy change as an instrument.[27]

Consumer preferences α_i^u over insurance can change if customers switched to other insurance companies or insurers. This is less likely because prices are regulated and thus similar across firms, creating less incentives for consumers to move to other firms. As I discuss further in Sections 7.2 and 7.3, per-unit premium and market share for the company that provided the data remained unchanged after the policy. In addition, the no-credit regulation was a national reform that affected all companies so there is little reason for consumers to

compares the distribution of consumer riskiness score across switchers versus non-switchers, and find no significant differences between them ($\hat{\theta}_r = 57.1$ and $\mathrm{SE}(\hat{\theta}_r) = 66.8$), as expected.

[26]Notice that if consumers anticipated the reform, they may have begun to alter their choices and other relevant characteristics prior to the reform. But if this were true, it would likely cause me to underestimate any effect the policy reform might have had because pre-reform claims would look more similar to post-reform claims behavior.

[27](i) Reassuringly, the main results are robust to narrow time windows around the reform's introduction: ± 4 months before and after the regulation. (ii) The timeline of the regulatory reform is illustrated in Figure 2.28 of the Appendix. As shown, the NIC agreed on the policy on October 12, 2013. The implementation or announcement took place on April 1, 2014, yielding an implementation lag of about 7 months.

switch.[28]

Finally, individual heterogeneity that comes from the production function α_i^y and ε can change if relevant macro conditions such as recessions and floods occur, respectively. Major recessions for instance may lead to changes in gas prices and therefore could cause customers to switch to different cars (e.g., to more efficient cars). While fluctuations in weather are common, no major floods occurred in the study area during the relevant period. In addition, I show in Figure 2.27 that changes in gas prices (direct pump prices) were not significant to actually induce customers to switch to different cars. In particular, the average and standard deviation for gas prices *before* the reform were USD/L 1.06 and 1.04, respectively. Similarly, the average and standard deviation for the prices *after* the reform were respectively USD/L 1.02 and 1.03; suggesting no significant changes.

2.5 A simple and general test of moral hazard

Section 4.3 argued that the variation induced by the introduction of the policy reform is conditionally independent of insurance outcomes: the timing of policy is uncorrelated with unobserved heterogeneity. In this section I use that exclusion condition to develop a simple generalized test for the *absence* of moral hazard in the insurance market. The analysis document evidence of moral hazard; baseline results that will supplement the subsequent results on moral hazard effects.

2.5.1 The moral hazard test

Consider the baseline set-up in the model, from Section 3. The independence assumption provides a direct means of testing for the *absence of* moral hazard. To see this, assume that

[28]Consumers who were owing companies might want to move to other firms. However, this seems unlikely given the higher repayment rate of premium debts and the fact that firms had to write-off all premiums outstanding after the regulation. From the sample, exiters represent only about 1.5% of customers. This is extremely low, as compared to the number of un-insured vehicles in Ghana (of about 21-36%), for example.

there is *no moral hazard*, i.e., $\partial g(.,.)/\partial e = 0$. Then one can write the implied system as

$$\begin{aligned} Y_{it} &= g(\alpha_i^y, \varepsilon_{it}) \\ D_{it} &= \sigma(\alpha_i, Z_{it}) \\ \rightsquigarrow Y_{it} &\perp\!\!\!\perp Z_{it}|X_{it} \end{aligned}$$

Without moral hazard, a change in coverage D_{it} induced by the reform Z_{it} and not by selection does not induce a change in outcomes Y_{it}. Thus, one can test for the *absence of* moral hazard by testing for the independence between Y_{it} and Z_{it} conditional on all premium and non-premium determining consumer characteristics. In the implementation, Y_{it} is either continuous or binary while Z_{it} is binary. In what follows, I present a nonparametric testing procedure that I propose. Results for other candidate testing procedures are also reported.

I denote the conditional distribution function of Y_{it} given $Z = z$ by $F(y|z)$ and that given $Z_{it} = z'$ by $F(y|z')$. Similarly, let the unconditional distribution of Y_{it} be $F(y)$. Then by definition: Y_{it} and Z_{it} are independent if $F(y|z)$ is equal to either $F(y|z')$ or $F(y)$. I exploit the use of this definition in the testing procedure described below. Denote the sum over all the binary values of Z_{it} by $\sum_Z$ and let $\pi(z)$ be the probability of realizing z. Then to test the hypothesis that there is *no moral hazard*[29] against the alternative that there is moral hazard, I construct the following L_2-Type test statistic

$$T = \sum_Z \hat{\pi}(z) \left[\sum_Y \left\{ \left(\hat{F}(y|z) - \hat{F}(y|z') \right)^2 \right\} \right]$$

where $\hat{F}(y|z)$ and $\hat{F}(y|z')$ are simply nonparametric empirical estimates of the conditional distributions which were predicted using the instrument Z, along with the relevant control variables X_{it}. In effect, the test statistic averages over the distribution of the decision variable Z_{it} and over the predicted outcomes Y_{it} (loss occurrence or claim amounts) of all the squared discrepancies between the two estimated distributional objects. The test allows the various values of Z_{it} to take different weights since they might occur with unequal chance.

[29]The null hypothesis can be stated as $H_0 : \{F(y|z) - F(y|z') = 0\}$ for any z, z' and y.

The null is rejected for large values of T; in practice I derive the p-value of the test under the null hypothesis that there is no moral hazard using the nonparametric bootstrap. The bootstrap inference is conducted at a significance level of 5%. One shortcoming of this "Moral Hazard Test" is that it only provides inference about whether or not moral hazard is absent: it does not deliver a measure of the size of the effect of moral hazard when the null hypothesis of *absence of* moral hazard is rejected. This caveat should be kept in mind when evaluating the implied results. Note here that the results from the proposed test and two other candidate procedures are complementary to the subsequent results on moral hazard.

2.5.2 Results

I begin by providing graphical evidence of the "Moral Hazard Test". First, the instrument and vector of controls are used to predict the conditional distribution of claim outcomes. Next, consider the various discrete values that the regulatory variable Z_{it} take. I divide the predicted sample of insureds into two groups based on the binary nature of the policy reform. I then define the claim distributions from the two groups as $\hat{F}(y|z)$ and $\hat{F}(y|z')$ where z and z' values correspond to pre– and post– National insurance reform, respectively. To fail to reject the underlying null hypothesis of "no moral hazard", it must be that these two distributions are equivalent.

In Figure 2.11 I plot the implied empirical cumulative distributions of claim outcomes pre– and post– insurance reform. This Figure provides visual evidence of the changes in the conditional distributions of claim outcomes. The graph in Figure 2.11 illustrates that there is a considerable difference between the distribution of predicted claim realizations before and after the reform. I can therefore reject the null hypothesis of no moral hazard.[30] In addition

[30]The inference is the same for alternative visual tests. In Figure 2.12, I compare the empirical distribution of claims (1) $\hat{F}(y|z)$ versus $\hat{F}(y)$ and (2) $\hat{F}(y|z')$ versus $\hat{F}(y)$. In both cases, there is substantial difference across the distributions; leading to a rejection of the null of no moral hazard. Notice that since the test must hold generally for all values of (y, z), once can explore different support values of the insurance outcomes to illustrate the distributional difference, as a visual test.

to the visual evidence of differences between distributions, the pre–reform distribution of claims tends to dominate that of the post–reform counterpart; suggesting that claim records became better due to the National reform. Altogether, the graph in Figure 2.11 provides a strong visual evidence of distributional inequality, and thus a rejection of the *no moral hazard* condition in this insurance market.

Finally, I evaluate the robustness of the graphical results by implementing the formal nonparametric L_2-Type test proposed above.[31] I also considered a comparable nonparametric test of equality of distributions: Kolmogorov–Smirnov, along with other semiparametric methods i.e., OLS. The results are reported in Table 2.2. In all cases, the "Moral Hazard Test" strongly rejects the hypothesis that moral hazard is *absent* in this insurance market at conventional significance level of 5%. The Kolmogorov–Smirnov test provides similar inference. Overall, the results robustly suggest the existence of moral hazard. The following section investigates this further by bounding its effects.

2.6 Bounding moral hazard under policy's exclusion

This section analyzes the separation and bounding of moral hazard effects. First, I build on the background formulation in Section 3 and policy's exclusion in Section 4.2 to provide identification results on moral hazard. Second, combined with the administrative data, I present the bounding results and discuss several dimensions of heterogeneity in moral hazard–important for insurance policy design.

2.6.1 Bounds on moral hazard effect

To conserve space, I summarize the main conditions and results. All details are relegated to the Appendix. The bounds set up embodies a triangular system in insurance outcomes and

[31] The distribution of test statistic T is provided in Figure 2.26 of the Appendix.

contract choice, as shown in the model section. Choice of contract depends on the exogenous policy or regulatory instrument, whereby it became impossible to buy insurance on credit. The restrictions required for the bounds are three-fold. The first is a weak–monotonicity condition, which requires that exerting higher levels of effort for a sub group of customers will not increase average claim outcomes. Such condition is a direct consequence of the Monotone Likelihood Ratio Property in Incentive Theory (Holmstrom 1979). The second is an independence condition, which implies no direct causal effect of the policy instrument on insurance outcomes, while the third condition requires that customers who select higher insurance coverage will not increase their supply of effort.

The starting point of the bounding exercise is to rewrite the average structural objects as a weighted average of observed and unobserved potential insurance outcomes, using insights from standard missing outcomes representation (Manski 1990; Manski and Pepper 2000). Introducing the instrument, which is independent of the potential outcomes, one can put bounds on the unobserved potential insurance outcome using the stated three conditions. The following proposition provides best possible bounds on moral hazard by combining all the restrictions.

Proposition 1

$$\begin{aligned}
\Delta^l &= \sup_z \{\mathbb{E}[D_i Y_i \mid Z = z] + \mathbb{E}[(1 - D_i)Y_i \mid Z = z]\} - \inf_z \{\mathbb{E}[D_i Y_i \mid Z = z] + \mathbb{E}[(1 - D_i)Y_i \mid Z = z]\} \\
&= \sup_z \{\mathbb{E}[Y_i \mid Z = z]\} - \inf_z \{\mathbb{E}[Y_i \mid Z = z]\} \\
\Delta^u &= \inf_z \{\mathbb{E}[D_i Y_i \mid Z = z] + (1 - P(z))G^u\} - \sup_z \{P(z)G^l + \mathbb{E}[(1 - D_i)Y_i \mid Z = z]\}
\end{aligned}$$

The derivation of proposition 1 is provided in the Appendix. First, proposition 1 shows that the lower bound on moral hazard's effect is a simple difference estimator. Second, the bounds are made up of three estimable terms which include an insurance choice probability object $P(z)$ and two conditional expectations. I apply the results to credibly test and quantify the effect of moral hazard. The restrictions provide useful improvements to identify the lower bound Δ^l, so that will be the main object of interest. Before presenting the evidence, I briefly discuss motivations for the bounding approach in the following.

2.6.2 Why the bounding of moral hazard

The bounds are meant to nonparametrically identify and capture the range of moral hazard that cannot be explained by the usual point estimates approach, although the latter could provide exact statements about moral hazard e.g., I am able to characterize the minimal extent of moral hazard using the bounds. The bounds approach is motivated by the following logic. First, nonparametric point identification of moral hazard is hard to achieve under significant selection in and out of insurance without stronger and perhaps non-verifiable assumptions (additivity of selection α_i, for example). This becomes even more difficult when the dimension of selection in multidimensional (e.g. heterogeneity in risk aversion, riskiness), which is natural in an insurance setting. Second, the bounds allows me to also learn about the population. This provides a useful way to evaluate the impact of moral hazard, which is crucial particularly for the implied policy analysis that I illustrate later in this paper.

The proposed bounds approach allows unobserved heterogeneity, a vector of hidden information, to impact insurance outcomes in an unrestricted manner. I am therefore able to characterize moral hazard by fully accounting for differences across the individual customers insurance choice while allowing for arbitrary correlations with the insurance choice, and thus accounting for adverse selection.

2.6.3 Estimating the moral hazard effect

The focus is on bounds to the average treatment effects ATE, the measure of moral hazard effect, under the agency theory-inspired inequality restriction. Estimating the bounds requires two sets of intermediate estimators, one for the "insurance" probability and the other for the conditional expectation objects. In what follows, I briefly describe the estimation procedure that I employ.

As in the Sections 3 and 4, I index an customer (insured) by i and time (contract date) by t, and let hat ˆ denote estimated objects throughout. Then the probability of "insurance" (comprehensive contract) purchase for an customer with characteristics z is estimated using the frequency estimator

$$\hat{P}(z) = \frac{\sum_i \sum_t \mathbf{1}(D_{it} = 1)\mathbf{1}(Z_{it} = z)}{\sum_i \sum_t \mathbf{1}(Z_{it} = z)}$$

where $\mathbf{1}(A)$ is an indicator that is equal to 1 whenever A holds and 0 otherwise. To estimate the conditional expectation objects, I use sample-analog-type estimators

$$\hat{\mathbb{E}}[Y_{it}D_{it} \mid Z_{it} = z] = \frac{\sum_i \sum_t y_{it}\mathbf{1}(D_{it} = 1)\mathbf{1}(Z_{it} = z)}{\sum_i \sum_t \mathbf{1}(Z_{it} = z)}$$

$$\hat{\mathbb{E}}[Y_{it}(1 - D_{it}) \mid Z_{it} = z] = \frac{\sum_i \sum_t y_{it}\mathbf{1}(D_{it} = 0)\mathbf{1}(Z_{it} = z)}{\sum_i \sum_t \mathbf{1}(Z_{it} = z)}$$

where all notations match with those in Section 4 and the Appendix. The version of these quantities that condition on the conditioning vector X_{it}, including $\hat{\mathbb{E}}[Y_{it}D_{it} \mid Z_{it} = z, X_{it} = \bar{x}]$ and $\hat{\mathbb{E}}[Y_{it}(1 - D_{it}) \mid Z_{it} = z, X_{it} = \bar{x}]$ are equivalently estimated using standard techniques. Y_{it} should be taken to be either claim outcomes or loss occurrence realized by customer i at time t. Next, the estimated objects above are then substituted into the identified best possible bounds for the average treatment effect Δ. This derives estimates of the lower and upper bounds under the agency theory-inspired inequality restriction, $\hat{\Delta}^l$ and $\hat{\Delta}^u$, respectively. Appendix A.1 provides an illustration of the various terms.

To conduct inference, I construct the confidence intervals for the parameters of interest Δ^l and Δ^u using a nonparametric bootstrap. In general, the bootstrap relies on continuity. This should be valid here since the estimated objects correspond to functionals for which regularity conditions for the bootstrap are met and I apply the sup and inf operators over a binary/finite support variable. Here the sup and inf are essentially max and min operators given the finite support of the instrument Z. In practice, I conduct the bootstrap inference at 5% level of significance while fixing the number of bootstrap resamples to 999 throughout.

2.6.4 Results

The main empirical results are reported in this section. The baseline estimates of average treatment effect, the measure of moral hazard effect under the agency-theory inequality restriction are presented. More specifically, Table 2.3 reports both the lower and upper bound estimates on moral hazard for two insurance outcomes.

Estimates that correspond to loss probabilities are displayed in the left panel, while those for insurance claims are presented in the right panel of Table 2.3. The 95% confidence intervals which are based on the nonparametric bootstrap are also reported in the last column of each panel. As shown in Section 3.2, evidence of moral hazard requires the average treatment effect which measures moral hazard to be greater than zero. This is equivalent to saying that customers' claim outcomes (or loss occurrence) increase with respect to insurance coverage on average after selection is eliminated. Similarly, the effect of moral hazard e.g., minimal or maximal extent can also be deduced by looking at magnitudes of the estimated quantities.

2.6.4.1 Evidence of moral hazard and effects

More generally, the estimates in Table 3 provide strong evidence of moral hazard in the insurance market. In particular, I find evidence of moral hazard for both outcomes of interest: loss occurrence and insurance claims. The estimated lower and upper bounds on moral hazard are GHC52 and GHC108172, respectively for claim outcomes. The estimated lower and upper bounds on moral hazard are 1% and 77%, respectively for loss occurrence.[32]

[32] The upper bound is very high because the identifying restrictions do not improve the terms that comprise it. It is rather made up of objects that reflect the empirical maximum for claims, which can be higher.

The 95% confidence intervals around the estimates are quite narrow.

Section 6.1 and Appendix A.2 show that the identifying power from the inequality restriction improves only the lower bound of the unknown quantity $\mathbb{E}[Y_i(1) \mid D_i = 0, Z = z]$ and the upper bound of the unknown $\mathbb{E}[Y_i(0) \mid D_i = 1, Z = z]$. In turn, these two improvements together provide a *lower* bound estimate on moral hazard. Restricting attention to the lower bound, moral hazard effects are derived as follows. For claim outcomes, the minimal moral hazard estimate of GHC52 translates to about 46% of average claims over the sample period. In other words, moral hazard accounted for *at least* 46% (lower bound) increase in realized mean claims. The same reasoning *mutatis mutandis* implies that moral hazard was responsible for *at least* 22% of the probability of loss occurring over the period (using the moral hazard estimate of 0.87%). These results point towards a strong moral hazard effect and suggest moral hazard affects changes in claim amounts "as much as" occurrence of losses. Overall, the moral hazard evidence is robust across various definitions of insurance outcomes.

2.6.4.2 Sources of moral hazard, visually: ex-ante versus ex-post effects

Section 3.2 points to two potential sources of moral hazard: ex-ante and ex-post aspects. I assess these visually by looking at observed changes in the type of claim events before and after the policy reform. Figure 2.13 (a) and (b) show how the claim events not covered under basic contracts and those covered under both contracts are distributed, respectively. The results suggest about 35.8 percent drop in the set of claim events that are covered by only comprehensive contracts after the regulation. Such policy-induced reduction likely reflects ex-ante moral hazard (i.e., unobserved preventive actions) because all things being equal, it seems reasonable that under-reporting of claims is less likely for comprehensive contracts that provide coverage for all responsible losses.

There is evidence that claim events that are covered under both basic and comprehensive contracts dropped by 29.6 percent after the policy. This drop likely reflect ex-post moral hazard (i.e., under-reporting claim or information) along with with ex-ante effects. Overall, the results indicate that both sources of moral hazard are present. However, observe that ex-post moral hazard has an opposing effect on the "frequency" of reported claim events. In part, this explains why the reduction in claim events that are covered under both contracts (29.5%) is lower than those covered under only comprehensive contracts (35.8%). With this, only 6.3% drop in claim reports is attributable to ex-post moral hazard; suggesting that under-reporting is less severe. Finally, note that since basic-liability reports involve third parties, it is difficult for consumers not to report such events.

2.6.4.3 Conditional estimates: moral hazard effects

Some papers study one informational friction (say, adverse selection) by abstracting from the other. For example, Cohen and Einav (2007) abstracted from moral hazard and focused on adverse selection in auto insurance contracts.[33] Since the background model allows for both moral hazard and adverse selection, I can conveniently analyze the implications of such abstractions. To do this, I assume that adverse selection is absent, and then estimate moral hazard. Without adverse selection, the lower bound on moral hazard is a "naive" estimator which takes the form

$$\equiv \max_d \mathbb{E}[Y_i \mid D = d, X = \bar{x}] - \min_d \mathbb{E}[Y_i \mid D = d, X = \bar{x}]$$

The results are reported in Table 2.4 separately for loss and claim outcomes. Both indicate large and significant moral hazard effects. Strikingly, compared to the main credible estimates of moral hazard, these results are 4-7 times bigger. In addition, the selection effect which captures the bias introduced by not randomizing contracts is large. This is about 0.03 for the occurrence of losses, and GHC320 for claim amounts. This analysis show that

[33] Adverse selection is modeled as unobserved heterogeneity in risk preferences (riskiness and risk aversion) from the choice of deductible in contracts using data from Israel.

assuming away adverse selection have nontrivial effects and vice versa. Moral hazard is over-estimated in substantial magnitudes, but this may depend on the direction of selection.

2.6.4.4 Heterogeneity in moral hazard

The moral hazard estimates may be heterogeneous in at least two observable dimensions (i) private versus commercial vehicle drivers, and (ii) different quartiles of discounts–reflecting the relative position of customers on the distribution of premium discounts that customers receive from the company. Private vehicles embody individual and corporate vehicles, while commercial vehicles are mostly taxis and mini-vans. Notably, individual vehicles usually contain the vehicle's owner and his driver. I assess such potential heterogeneity by providing lower bounds on moral hazard by driver type and by quartile of discounts – the results of which can help guide policy design and discussions about the automobile insurance market as well as simulate further related research.

In Figure 2.16 of the Appendix, I show the heterogeneous estimates on moral hazard. Similar to the main results from Table 2.3, I can reject the null of no moral hazard at 5% level of significance across all driver types and quartiles. The moral hazard estimates are larger for both commercial vehicle and lower quartile discount drivers, which in turn suggest that commercial drivers and low premium discount customers are less responsible. In this case, corrective policies to influence moral hazard can include schemes that make basic insurance contracts more attractive to the subgroup of customers associated with commercial vehicles, e.g., weighed against the potential cost of subsidizing insurance for this group.

Next, the heterogeneous results can be related to the concept of monitoring and moral hazard. Private vehicles usually operate with two people, typically the car's owner (who may act as a "monitor") and his driver.[34] For commercial cars, this is not the case as they do not run with the owner. In this case, the availability of a "monitor" in private vehicles can

[34]The owner of the vehicle do not only observe and serve as a "monitor", but can also fire the driver when he drives recklessly at a low to zero firing cost.

explain why private drivers are more responsible than their commercial counterparts. As a result, the heterogeneous findings generally imply that "monitoring" can be an effective tool in curbing moral hazard, which is consistent with theoretical results in Holmstrom (1979) and others.

2.7 Mechanisms, caveats and policy implications

In this section, I discuss the role of two potential channels for contract choice and their importance for shaping the estimated incentive effect: moral hazard. These include liquidity constraints and changes in relative prices. There is evidence in favor of the former, and not the latter. First, I illustrate that moral hazard increases with the probability of buying insurance on credit; providing additional evidence of heterogeneity in moral hazard. I then discuss how this heterogeneity is consistent with credit constraints. Next, I carry out an array of tests to verify that the main results are robust to several caveats. The broader implications of the estimated quantities are also presented.

2.7.1 The role of credit constraints

Before presenting the evidence, I note why borrowing may be limited for the customers who bought insurance on credit. First, there is evidence indicating that the customers who purchased insurance on credit switched to contracts with lower coverage after the reform. So if they could borrow before the reform, they would have done it to seek contracts with higher coverage after the reform. In addition, interest rates are high in Ghana, at least compared to interest rates in developed economies like the United States and Canada over the period. For example, interest rates in Ghana averaged about 20% between May 2013 and April 2015, compared to the United States average rate of $< 1\%$.[35] This removes the

[35] For example, see https://tradingeconomics.com/ghana/interest-rate for Ghana, and https://www.oanda.com/forex-trading/analysis/historical-rates for the United States and Canada.

incentive to borrow to buy higher contracts.

I now document the relation between moral hazard and the purchase of insurance on credit. There are potentially multiple ways to investigate how the provision of credit ultimately shape the estimated moral hazard effect. The direct approach will be to split the sample into sub groups of customers who bought insurance on credit and those who paid insurance upfront, and then estimate moral hazard for each sub group. The second approach involves using information about the credit-purchases/history of consumers to identify the distribution of those who are likely impacted by the regulation, and then compare moral hazard effects across this distribution. Here, I follow the latter approach because implementation of the former is limited by the way the policy instrument Z is constructed and the fact that after the reform's introduction consumers could no longer buy insurance on credit. I am also able to examine whether or not changes in the moral moral effect is monotonic along the distribution of credit decisions.

Denote by $P(c^r; x)$ the probability that a customer with observable characteristic $x = X_{it}$ acquires insurance on credit. Extremely low $P(c^r; x)$ corresponds to customers for which credit is not important; and thus will not be affected much by the reform. Equivalently, high values of $P(c^r; x)$ correspond to customers for which credit is important. I proceed in two interrelated steps. First, I estimate $P(c^r; x)$ by estimating a probit regression model of whether or not an customer purchased insurance on credit against the observable vector of individual characteristics. This estimation is done using the universe of customers in the sample for both contracts, $D_{it} = 1$ and $D_{it} = 0$. The estimated credit probabilities are displayed in Figure 2.14(a). The figure shows a range of probabilities that lie between 0% - 41%, with a median of about 8%. This means that the median consumer with observable characteristic x is 8% likely to purchase insurance under the credit schedule. Also, in Figure 2.14(b) I display the distributions of estimated credit probabilities across the two contract types. There is evidence that consumers were more likely to use credit to purchase contracts with higher coverage before the no-credit regulation.

In the second step, I investigate the effect of buying on credit by estimating the lower bound on moral hazard (i) separately for the group of customers who fall below versus above the median credit probability, and then (ii) across the different quartiles of the credit probabilities. The results are displayed in Tables 2.5 and 2.6, respectively. First, there is evidence that the moral hazard effect is larger for the customers below the median probability, compared to those above. For claim amounts, this is about 5 times larger, while for loss occurrence it is about 2 times larger. Second, the effects across the credit distribution is non monotonic, but much of the moral hazard is concentrated in the upper credit quartiles as expected.

These results are intuitive. Credit matters more for consumers in the upper quartiles since they are likely credit constrained. The impact of the no-credit regulation should be more binding for this group. As illustrated in Figure 2.14(b), the customers who were purchasing comprehensive insurance more likely do so on credit than those who were buying the basic contracts. This explains why most customers switched from comprehensive to basic contracts following the reform (see Figure 2.6 and Table 2.1). The incentive to shirk is higher under the comprehensive contract. These results support the hypothesis that consumers responses to the reform likely through the "liquidity" mechanism. Finally, note the primary trade-off of sub sampling customers based on credit quartiles for the analysis: uncertainty increases because the size of the sample is reduced drastically.

Discussions: Are these effects due to credit constraints or financial saviness? In principle, consumers' credit decisions can reflect the two, so both explanations are possible. The latter will mean that customers were gaming the system of buying on credit, with no intentions to repay their accrued premium debts. If this was the case, then that will imply possibly another moral hazard via defaults/delinquencies from the credit side. However, the evidence is more consistent with credit constraints as discussed below.

Credit constraints are a natural reason for explaining the drop in insurance demand after the reform and thus the moral hazard results. This is for several reasons. First, as I argued earlier, the policy reform tightened liquidity and affected consumers who were buying insurance contracts on credit prior to the introduction of the reform. In particular, over 99.4% of consumers who were buying insurance on credit bought higher-coverage contracts and switched to contracts with lower coverage after the regulation. So, consumers' responses to the reform most likely operate through this "liquidity" mechanism.

Second, there is much evidence that people in developing countries face liquidity constraints (Banerjee 2001, Banerjee and Duflo 2011; Karlan et al. 2014). For example, Karlan et al. 2014 documented credit constraints in northern Ghana.[36] Third, as I documented in Section 2, the repayment rates for premium debts are substantially high. For example, 79% of customers who bought insurance on credit repaid their outstanding premiums before their contracts expire. Similarly, over 73% of all outstanding premiums are paid before the end of the insurance contract. These results are less consistent with financial saviness, lending further support to the credit constraints channel.

2.7.2 The role of firm price response

In principle, insurance firms may respond indirectly in multiple ways to the National reform via the differential pricing of contracts e.g., indirectly increase overall premiums to maintain certain levels of profit; decrease premiums for comprehensive coverage to encourage their take up; discourage basic contracts through increases in price for such coverage; or employ other response strategies that will manifest through prices. Such supply side responses can reflect the moral hazard results. I document that the insurance company did not significantly adjust per-unit premiums following the introduction of the reform. This finding helps to shut

[36] Theoretically, the credit-constraints channel can be understood formally in a model where consumers make insurance and effort decisions today subject to the risk of a liquidity shock tomorrow, akin to the setting of the policy reform (similar to Deaton 1991). The simple intuition is that because the agent cannot borrow to buy more insurance when the liquidity shock arrives and effort is costly (in monetary terms), the agent likely demand more insurance today and exert less effort. In that case, accumulated net income transfers from insurance can be used to smooth future consumption.

down the possibility of an alternative mechanism ("price") and lends further support to the "credit" channel argument.

I begin with a descriptive analysis of the changes in prices. In Figure 2.15, I show both the distribution and differential changes in insurance premiums before and after the policy reform. In the first row, the first item scatters realized premiums over the period, while the second centers these at the policy date. The scatter has been jittered to make it is easier to see where the mass is located. There are two important observations: the mass is evenly distributed and there is no evidence of significant differences in premiums around the reform's date. To account for the possibility of differential pricing across contracts, I show changes in realized premiums for the two contracts in the second row. However, the changes are also visually insignificant.

Next, I evaluate the robustness of the descriptive evidence using a model that links changes in premiums to contract years and coverage. For consumer i in contract year t, the simplest model that I estimate is:

$$\rho_{it} = \mu_i + \delta \text{Policy}_t + \epsilon_{it}$$

where $\text{Policy}_t = \mathbf{1}[Date > \text{April 2014}]$. Figure 2.16 displays the distribution of premiums after customer-level fixed effects μ_i are removed from the data (distribution of $\delta \text{Policy}_t + \epsilon_{it}$). This is shown for the period before and after the 2014 insurance regulation. The figure demonstrates limited evidence that premiums changed following the policy, similar to the descriptive evidence. The estimated $\hat{\delta}$ is 18.67 and insignificant at conventional levels. I modify the baseline model to investigate differential pricing using:

$$\rho_{it} = \mu_i + \beta[D_{it} \times \text{Policy}_t] + \gamma \mathbf{X}_{it} + \epsilon_{it}$$

where D_{it} and Policy_t are respective indicators for higher coverage and post regulation period. The model essentially interacts the two indicators. β, the main parameter of interest, captures the sign, size and significance of any differential pricing by contract-type following the reform. All relevant control variables are housed in the vector $\mathbf{X}_{it}$ (i.e., the list of observed characteristics discussed in Section 4.1).

The results are reported in Table 2.7. Different columns correspond to different model specifications, based on the inclusion of the various control variables. The coefficient on the interaction term is negative and insignificant at conventional levels in the preferred specification, column 3 where all premium-determining characteristics are included. Results indicate that on average firms did not alter the premiums deferentially, all else equal. Taken together, these results provide suggestive evidence of no significant price responses. This is expected given that the NIC strictly regulates the pricing of insurance products. Results reinforce the explanation that the estimated moral effects are driven by credit constraints.

2.7.3 Robustness Analysis

Threats from sample selection: The "ideal" data set to evaluate moral hazard will embody the universe of contracts data across all firms in the insurance industry. In this paper, I mimic this using customer-level data from the single largest firm: largest branch (headquarters) office records. A drawback of this approach concerns the representativeness of the sample due to potential exits and entries of customers across insurance companies. More specifically, the sample suggests about 1.5% and 3.7% rate of exits and entries, respectively.

First, what works is that relevant changes in the industry and aggregate outcomes are largely consistent with evidence from the sample. As shown in Figures 2.17-2.19: (i) the market share of the study-company remained stable at 22% between 2013 and 2014; suggesting less drastic movements in and out of the firm overall; (ii) consistent with the sample, there is evidence of overall reduction in motor crashes or losses between 2013 and 2014; and (iii) there is evidence of general reduction in claim amounts and increased profits between 2013 and 2014 as in the sample. This line of aggregate evidence is re-assuring and lend further support for the empirical results. Second, the baseline results are stable using a restricted sample of customers who existed in the data before and after the policy (balanced sample; see analysis below). The implication of this result is that potential entry of new customers likely have less severe effects on the main moral hazard results.

Entry & exit of new customers In practice, different customers could either enter or exit the insurance pool after the reform's introduction. I investigate how this, particularly entry, might affect the results by limiting the estimations to the set of customers that maintained the same policy numbers before and after the policy reform. As shown in Section 4.2, (i) pre-reform distributions of customer characteristics are similar to post-reform distributions and that (ii) it is unlikely for customers to leave the insurance pool for other insurance companies since prices are the same across firms, so I do not expect significant changes to the results. Figure 2.20 shows the conditional distribution of predicted claims, while Tables 2.8 and 2.9 present the bound estimates for moral hazard and across the group of customers below and above the median credit probability. In all cases, the evidence is qualitatively similar. Notably, there is evidence of larger moral hazard effect for customers below the median credit probability (constrained) as compared to the unconstrained.

Restricting the analysis to only third-party events In Appendix A.3, I discuss the approach used to recover comparable claim records for basic-liability contracts, since the insurer data typically do not capture own damages directly for customers with basic-liability insurance. But because the insurer data includes damages for third-party events which are covered under all contracts and directly available, I evaluate the robustness of the main results by limiting the analysis to only third-party claim events. As shown in Table 2.10, the estimated moral hazard effects are near and well within the confidence intervals of the main estimates. For the size of claims, the lower bound estimate translates to 39.10% of mean claim amounts, while for the number of claims, it translates to 16.58% of loss probabilities.[37]

Narrowing the window of analysis Section 4.3 appealed to the short period of data coverage to argue for the reform's independence to selection. As an alternative, I examine the stability of the baseline results using data right before and after the policy reform. This minimizes the influence of realizations that occurred far from the reform, but implies a drastic reduction of the sample size. Instead of the full sample, two time windows are considered (i) ±8 months and (ii) ±4 months windows around April 2014. Figure 2.21 displays the distribution of predicted insurance outcomes for the different windows; a test for moral hazard. The bounds on moral hazard are summarized in Table 2.11. The graphical evidence suggests stronger rejection of no moral hazard, but qualitatively these results are similar to the main findings. The bound estimates are very close and well within the confidence intervals of the main estimates.

Effect of outliers and tail events I winsorize the data to reduce the influence of extreme claim and loss realizations. All observations in the data below the 2.5th percentile are set to the 2.5th percentile value, and those above the 97.5th percentile are set to the 97.5th percentile value. This approach minimizes the influence of extreme observations, but censors the data. I replicate Figure 2.11 and Table 2.3 using the winsorized data. Results pertaining to the moral hazard test are shown Figure 2.22, while the bound estimates are contained in Table 2.12. Both the graphical and bounds evidence are near and consistent with the main findings.

Effects from externalities [and exogenous spillovers] The model and bounds assume

[37] Such evidence is consistent with less-severe under-reporting of claim events (as discussed in Section 6.4.2) in the baseline analysis that uses events under comprehensive contracts to recover claims for basic contracts for comparison. This may be explained by the nature of third-party events: they involve other customers, making it difficult for responsible policy holders not to report their occurrence. It also helps to alleviate potential concerns that the baseline exercises are just picking up less reported but not actual damages. Finally, note that since the baseline analysis combine all claim events (third-party and own damages), it is expected that limiting the estimations to only third-party damages will yield slightly lower estimates for moral hazard.

independently distributed accidents. In practice, however, external effects from others driving activity can violate this independence. For example, one consumer can hit another and then run away. First, this would be a major concern if such external effects vary with switchers versus non-switchers (or the quasi-assigned contracts). In particular, the main estimates will be biased downward if the external effects for non-switchers are systematically larger than the switchers and vice versa.[38] But to the extent that these externalities are possibly random, that seems unlikely. Second, when an accident occurs, there is often one party who is at fault (or the liability is shared) based on the legal statutes. The functioning of legal systems in low-income environments may be weak, but existence of such legal arrangements help to internalize part of the external effects.

Third, I use the following back-of-envelope calculations to assess the potential magnitude of such external effects. The effects correspond to the additional costs of accidents beyond observed claims. Following Cohen and Einav (2007), I estimate this by dividing the (i) total accidents (18,050 in 2013; 14,895 in 2014), and (ii) accidents with fatalities (1,898 in 2013; 1,806 in 2014) in Ghana[39] by an estimate of the total number of auto insurance claims (48,809 in 2013; 45,238 in 2014) in Ghana.[40] For 2013, I find that 36.9 percent of claims involve reported accidents, and 3.9 percent involve accidents with fatalities. For 2014, 32.9 percent of insurance claims involve reported accidents while 3.9 percent involve accidents with fatalities. This implies that the majority of insurance claims embody small unreported accidents,[41] perhaps because the additional external effects are often small. In

[38]Equivalently, the estimates will be biased download if the external effects before the policy are larger than effects after the policy. This can be seen from a modification of the lower bound estimator: $\triangle^l + \{\mathbb{E}[E_{z0}]\} - \{\mathbb{E}[E_{z1}]\} = \triangle^{l*}$ where $\mathbb{E}[E_{zj}]$ corresponds to the average external effects before ($j = 0$) and after ($j = 1$) the policy reform, and $\triangle^{l*}$ is the true population parameter of interest.

[39]Accidents refer to crashes resulting in injury, death or property damage and involves at least one vehicle on a public road. These are reported to the police and a police officer arrived at the scene. The data come from the National Road Safety Commission (NRSC) http://www.nrsc.gov.gh/

[40]The total number of car insurance claims are estimated by dividing the total number of insurance policies at the end of the sample (~30,000) by the product of the share of the market for the company that provided the data (21%) and the best guess of the share of policies from the company's headquarters branch (12%) where the contracts data come from. I then multiply this by the insurance claim or loss rates before and after the policy: 0.041 in 2013 versus 0.038 in 2014, respectively (see Tables 18 and 19 of the Appendix).

[41]Note the consistency of with the initial evidence in Section 6.4 that under-reporting is likely less severe.

addition, the calculations indicate modest reductions (but insignificant) in external effects after the regulatory reform, perhaps suggesting that the main estimates are (negligibly) biased downward. Finally, I compare the average claims for the subset of consumers who enrolled in comprehensive contracts in both regimes but never acquired insurance on credit. The pre-reform average claims are similar to that of post-reform, an evidence inconsistent with exogenous spillovers.

2.7.4 Welfare implications: moral hazard and policy

2.7.4.1 Estimating foregone claims bill due to moral hazard

The baseline lower bound estimate of moral hazard is informative and has important broader implications first on the insurance market, and second on the National reform itself in general. More specifically, the reform-identified estimates generate impacts that are of further economic significance. The Cedis GHC52 sounds small but actually it is not because it represents a large fraction of average payouts over the period $\hat{\gamma}^{MH} = 46\%$, which is further explored below.[42] As an illustration of the welfare significance of the GHC52 estimate, let's suppose customer i has a basic contract $D_i = 0$, and let the insurer *randomly assign* this customer to the comprehensive contract $D_i = 1$. Then the GHC52 is the added loss that the company will have to cover. This follows because all losses are covered under the comprehensive plan. The above process could translate into large actuarial losses and thereby limit the soundness of the actuarial process.

To illustrate and put the results into context, I examine (the mean of) observed indemnity payments that may be attributed to moral hazard using the lower bound estimate of moral hazard. Since actuarial indemnities are largely based on claim outcomes which in turn reflect insured private information, I generally define the indemnity function as

[42] The GHC52 estimate also translates to about $\hat{\gamma}^{MH}$ =12% of firm's average profits. Here, average profits is given by $\Omega = \bar{\mathbb{E}}(\rho_{it}) - \bar{\mathbb{E}}(\iota_{it}) \times (1 + \lambda)$ using a simple back-of-envelope calculation. $\lambda = 0.25$ denotes the loading on payouts. To get this, the observed premiums and indemnities from the insurer's data set are directly used to compute expected revenues $\bar{\mathbb{E}}(\rho_{it})$ and expected costs $\bar{\mathbb{E}}(\iota_{it})$, respectively. This calculation ignores any direct returns on company investments of collected insurance premiums.

$$\iota_{it} = h_{it}(Y_{it}|\gamma^{MH}, \alpha^{AS}; \varepsilon)$$

for customer i at contract year t, where α^{AS} and γ^{MH} correspond to the vector of hidden information as discussed in the model section and estimated moral hazard, respectively. Then to obtain the average of indemnities for the population of insured, I take expectations over i and t to get

$$\mathbb{E}(\iota_{it}) = \int h_{it}(Y_{it}|\gamma^{MH}, \alpha^{AS}; v) dH(Y_{it}|\gamma^{MH}, \alpha^{AS}; \varepsilon)$$

where $H(.|.,.;.)$ is the conditional claim distribution. Obviously, one needs to estimate this object in order to compute the average of the indemnities which is fraught with much difficulty. Instead of directly estimating that, I utilize the actual paid indemnities in the sample. In estimating the effect of moral hazard, I jointly allowed for an unrestricted selection in and out of insurance: this significantly controls for/eliminates adverse selection and other important drivers of the indemnities. This therefore permits me to compute the fraction of indemnities paid to customers due to moral hazard using the sample analog[43]

$$\bar{\mathbb{E}}(\iota_{it}|\alpha^{AS}; \varepsilon)^{MH} = \hat{\gamma}^{MH} \times \sum_i \sum_t \bar{\iota}_{it}$$

where bars $^{-}$ are used to denote sample realizations here. $\hat{\gamma}^{MH}$ stands for the estimated moral hazard as a fraction of realized mean claims over the period. The implied dollar values are directly derived–reflecting the corresponding *actuarial losses* due to moral hazard.

Moral hazard accounted for *at least* GHC1,328,138 (USD442,712)[44] aggregate leakages or forgone bill in indemnities for the auto-business line of the company's branch between the two contract years. From additional back-of-envelope exercises, I find that the forgone bill

[43] In effect, I am measuring the total rather than marginal contribution from the reform-identified moral hazard. The approach is technically equivalent to: GHC52$\times$#ofConsumers.

[44] Prevailing exchange rate 1.00USD $\approx$ 3.00GHC. See https://www.oanda.com/currency/average

for the insurance company is GHC11,067,817 (USD3,689,272), and for insurance industry is GHC52,703,889 (USD17,567,963).[45] This analysis highlights the potential soundness of the National reform because of the implied actuarial gains. As an interpretation: moral hazard accounted for a significant share of insurance claims, which induced substantial leakages in claims (inefficiencies). To the extent that the National reform exogenously caused consumers to switch to less generous contract choices, the reform arguably averted this extent of market inefficiency.

2.7.4.2 Estimating effects on welfare

The introduction of the policy reform is not only beneficial, but may generate unintended costs on consumers. Specifically, the no-credit regulation has two potential implications for welfare. First, because the reform led to lower coverage, it may have negative welfare implications for consumers. Second, as I highlighted in Section 7.4.1, ending the purchase of insurance on credit have positive welfare implications for firms via increases in profits due to reduction in large moral hazard inefficiencies. I compare these two opposite forces to evaluate whether or not the policy was welfare decreasing, overall.

I use the certainty equivalent as a measure of consumers welfare. Denote by $c_{it}(d) = Payouts_{it}(d) - Premium_{it}(d)$ the net transfer from insurance to consumer i under coverage d. I assume a constant absolute risk aversion (CARA) utility function with coefficient of absolute risk aversion $\gamma > 0$:

$$u(c_{it}) = -exp^{-\gamma c_{it}}$$

from consuming a normally distributed c_{it} in contract year t. The certainty equivalent

[45]For the company, I estimate the forgone bill by dividing the GHC1,328,138 by the best guess of the share of the company's headquarters branch where the contracts data came from (12%). For the industry, this is derived by dividing the company's bill by its share of the entire insurance market (21%).

per contract year is defined as:

$$CE = -\frac{1}{\gamma}\log\mathbb{E}(exp^{-\gamma c_{it}})$$
$$= \mu_c - 0.5\gamma \times \sigma_c^2$$

where μ_c and σ_c are the actual mean and standard deviation of c. These are estimated using the empirical realization of net insurance transfers to consumers, separately before and after the policy reform. I then derive changes in certainty equivalents $\triangle CE$ by subtracting the estimated certainty equivalents post-reform from that of pre-reform. The results are displayed in Table 2.13 for different plausible values of absolute risk aversion γ. Relative risk aversion parameters between 2-5 are considered reasonable, so I divide this by the average annual earnings in Ghana in 2013/2014 to get reasonable values for γ.[46] These calculations indicate that the loss in consumer welfare attributable to the no-credit reform is between GHC111,559 (USD37,186) to GHC178,341 (USD59,447).

Next, I examine changes in firm profits due to the policy reform. Let $\pi_{it} = Premium_{it} - (1+\lambda) \times Payouts_{it}$ represents the per-customer profit to the insurer. The total profit per contract year is given by:

$$\pi = \sum_i Premium_i - (1+\lambda) \times \sum_i Payouts_i$$

where $\lambda = 0.25$ denotes the loading factor on payouts: typically, reflects the administrative costs of processing claims. Similar to the certainty equivalent calculations, I used the empirically observed premiums and payouts to compute changes in profits $\triangle\pi$ pre- and post-reform. As shown in Table 2.13, the gain in producer welfare attributable to the policy reform restricting the sale of insurance contracts on credit is between GHC1,023,168 (USD341,056) to GHC9,210325(USD3,070,108). Taken together, for reasonable values of consumer risk aversion, enforcement of credit arrangements and insurance loading, the analysis suggests that restricting the sale of insurance on credit have both negative and positive

[46]The estimate of average annual earnings in Ghana was GHC5,346.9 (GSS, 2013/2014). The implied parameter values are very close those provided in Cohen and Einav (2007). From the automobile insurance in Israel, the authors estimate mean absolute risk aversion of 0.0019; and a median 0.0000073.

welfare implications but the loss in welfare do not outweigh the gains. The loss in certainty equivalents represents approximately 11% of the gain in profits for cases where some proportion or all premium debts are eventually repaid before the expiration of contracts.

Varying the loading factor λ. I evaluate the sensitivity of the welfare results to λ. The results are displayed in Table 2.14. Results for three parameter values: $\lambda = 0.00$ (no loading), $\lambda = 0.15$ and $\lambda = 0.35$ (i.e., high claim processing costs to firms) are displayed. Qualitatively, the evidence from all cases suggest similar findings: the gain in producer welfare outweighs the loss in consumer welfare.

Finally, it is useful to note that the welfare results reflect a market context where prices are regulated. In both the pre and post reform regimes, insurance firms were effectively not allowed to price coverage, which is a first source of market distortion. Firms responded to this distortion by selling insurance on credit, amplifying moral hazard in the market. The no-credit regulation was then introduced to stop firms from providing insurance on credit, which is another potential source of market distortion. Perhaps, muting these regulatory distortions and allowing insurers to price coverage more expensively could have corrected the moral hazard induced in the market.

That being said, distortionary regulations are prevalent in several market contexts, especially, in developing countries. I study a context with two unique features: prices are regulated, and consumers are usually faced with credit constraints. These features are commonly shared in several environments, particularly in low-income and developing countries. For the specific policy reform, several countries including Nigeria and Gambia have implemented similar no-credit regulations and many other countries have been projected to follow.

2.8 Conclusion

In this paper, I argue that contractual arrangements that defer the payment of insurance premiums to a future period, not only increase demand but induce large moral hazard and welfare effects. The coexistence of moral hazard and adverse selection, possibly multidimensional in nature, presents a challenge in learning about moral hazard alone. I disentangle moral hazard from selection by exploiting a natural experiment coming from the introduction of an insurance reform, whereby it became impossible to buy insurance on credit, making lower coverage contracts more attractive. By requiring that car insurance premiums be paid upfront, the demand for higher coverage decreased by 6 percentage points.

The random variation created by the policy reform allows me to construct an instrument to identify the causal effect of coverage choice on claim amounts and loss occurrence—moral hazard—and eliminate contaminations that may be due to selection. I empirically investigate the identifying power of the weaker restriction that, on average, consumers that select higher coverage contracts will not increase their supply of effort. I find a convincing and robust evidence of moral hazard in this market. Moral hazard led to a 46 percent increase in average size claims or 22 percent increase in the number of claims. The analysis also establishes that moral hazard induced significant leakages in insurance claims and that monitoring can be an additional effective tool in curbing moral hazard.

I discuss two potential mechanisms that could be responsible for the moral hazard results: binding credit constraints versus changes in relative prices, and find evidence in favor of the former. In principle, this is equivalent to examining the channels through which the policy reform may shift choices of insurance contracts and thus moral hazard. Heterogeneity analysis suggest that the results likely operate through a constraint in “credit” that was imposed by the policy reform, where moral hazard is greater for the more credit constrained. This result establishes an important connection between incentive effects and credit constraints. Finally, insurance firms may alter the pricing of contracts to maintain certain profit levels as a response to the policy reform. For example, decrease (increase) the premiums for higher (lower) coverage contracts to encourage (discourage) their uptake. I find no evidence across multiple tests for such differential pricing.

Examining the impacts of "buying on credit" on car insurance demand, credit and moral hazard has applications for other types of insurance. First, consider the case of personal insurance which is widely offered by private insurance companies. This insurance requires individuals to pay premiums upfront. The results in this paper directly imply that customers who face the risk of credit constraints are less likely to be responsible. In this case, an alternative policy to reduce moral hazard would be to make lower coverage contracts more attractive to the potentially credit constrained customers.

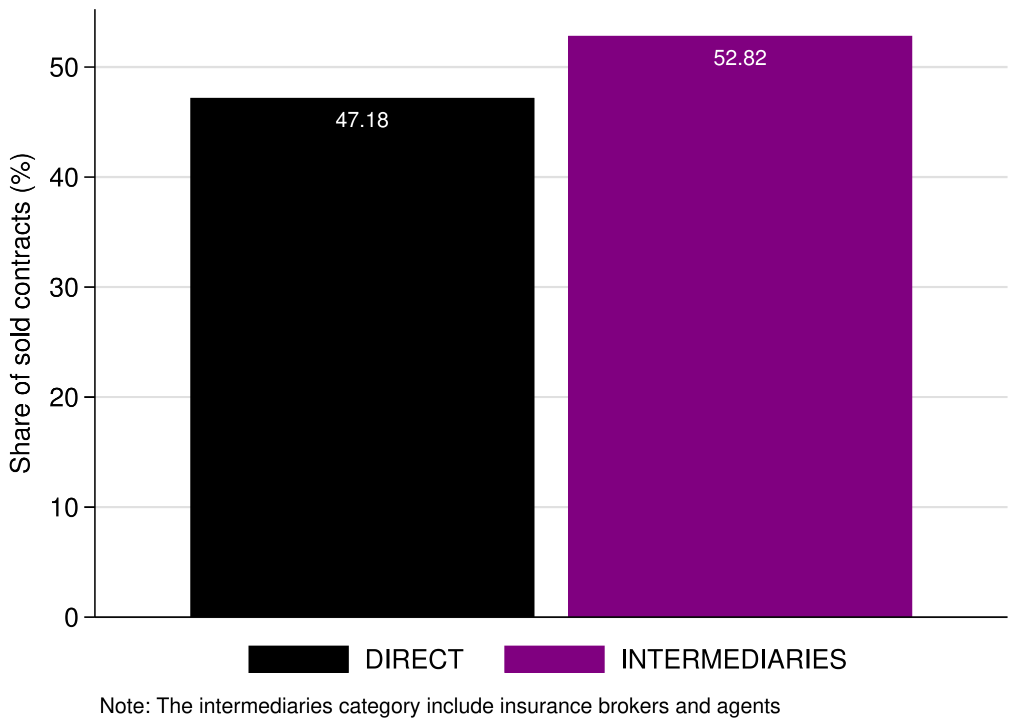
There are two additional indirect applications: social and index insurance. For social insurance programs, no upfront premium payments are involved but may embody potential moral hazard and liquidity aspects. Examples include unemployment insurance and social interventions. Studies and design of social programs tend to consider moral hazard and liquidity as separate entities (Chetty 2008). The results in this paper indicate a potential linkage between the two; thus extending our knowledge about moral hazard and liquidity for program designs. For weather index-based insurance, moral hazard is largely absent—since contract payments are based on an exogenous publicly observable index, such as local rainfall, paying out on the basis of too much or too little rain—but liquidity constraints may be present to impede uptake (Cole et al. 2013; Karlan et al. 2014). A conventional policy may overcome credit constraints to induce insurance uptake (Casaburi and Willis 2017), but as shown in this paper, it is crucial to consider the potential moral hazard aspects when present. For this reasons, policy instruments e.g., loan programs, that aim to increase demand will require full benefit-cost assessment to justify their implementation.

From a policy perspective, two aspects are notable. First, this paper illustrates how regulation can be used to fix insurance market imperfections, particularly, insurance in developing countries. The moral hazard effect translates to about a 12% decline in firm profits, but such inefficiency was averted by the policy reform. The reform adjusted the market and made insurance outcomes better, highlighting the potential importance of corrective regulation in such contexts. However, because the reform led to lower coverage, it may have negative welfare implications for consumers. Second, this paper provides an indirect evaluation of a policy that restrict “buying on credit”. Purchasing arrangements to pay later boost retail trade in many developing countries (IMF 2012). But the ability to buy insurance on credit can yield large and economically substantial moral hazard effects in the market. Finally, estimated gains from the policy via reduction in moral hazard may extend to the functioning of markets in other settings.

This paper provides a first step in understanding the impacts of buying insurance on credit and the potential role of credit constraints for moral hazard. Ongoing research embodies four extensions of it. First, governments and regulators across other countries have either adopted a similar “no premium, no cover” reform or considering its adoption. I aim to consider the implications of the proposed approach and findings in other developing countries that currently have such insurance reforms in force: Nigeria and Gambia. The underlying legal and financial institutions are different, which may well matter for the functioning of the existing insurance markets and enforcement of contracts. Evidence from these varying contexts will therefore provide additional external validity and a further evaluation of the growing insurance policies, including the impacts on firms’ balance sheets, potential market fraud and re-insurance behavior.

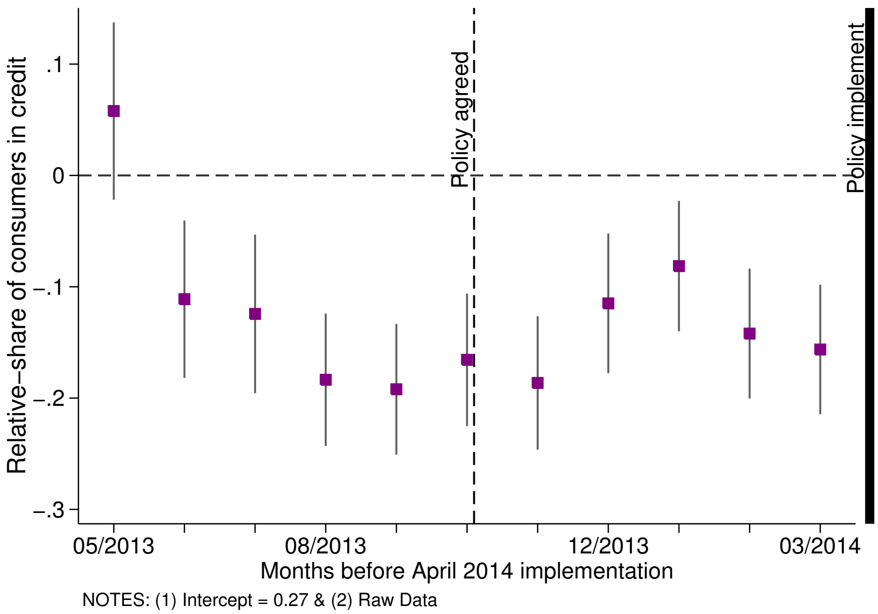
Second, consumers might have reduced their driving speed in response to this insurance regulation since coverage and the occurrence of losses were reduced. I aim to examine the co-impacts of the policy on local air quality, appealing to the literature on the effects of regulation on air pollution (Greenstone 2004; Davis 2008). I have done some preliminary analysis suggesting modest reductions in air pollution as measured by particulate matter at the policy cutoff. Next, the results show that moral hazard is largest among the credit constrained customers, but that link was non-monotonic. I aim to explore the nonlinear link between liquidity constraints and moral hazard effects, as this could have important implications for the design of contracts and policies to alleviate moral hazard. Finally, I plan to investigate whether interlinking credit with insurance markets can induce adverse selection, and the implied welfare implications. There are indications from preliminary analysis that consumers who bought insurance on credit signal as bad risk-types, as compared to their counterparts who paid contracts upfront.

Figure 2.1: Channels for Selling Policies [Contracts]

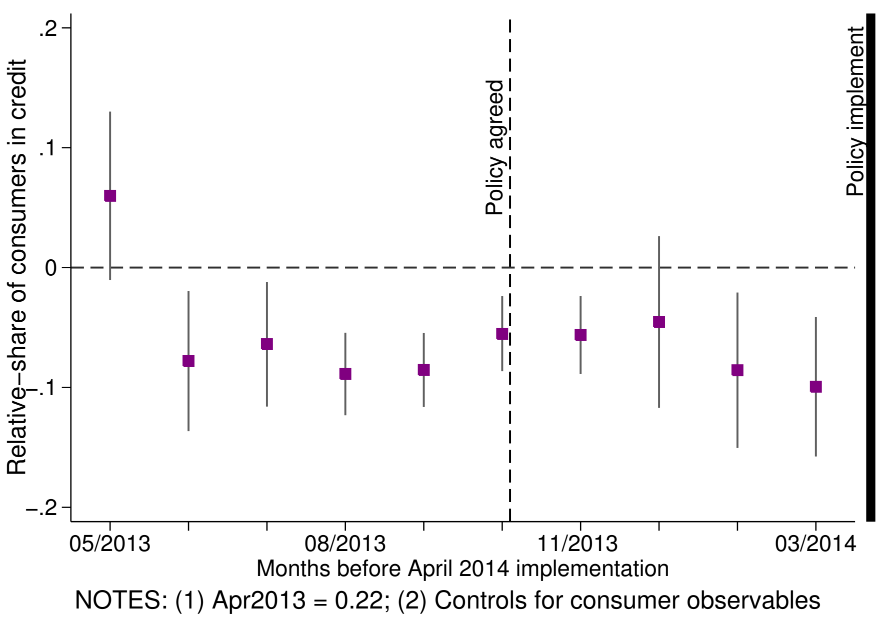


Notes: Figure shows the different channels that insurance policies are sold. Many insurance contracts are acquired through agency channels: market intermediaries.

Figure 2.2: Credit Take-up Over Time



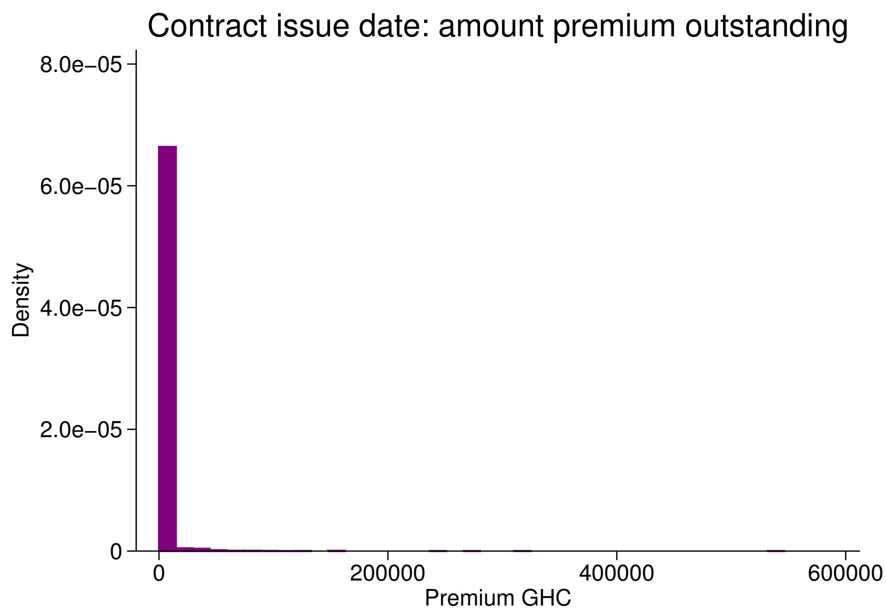
(a) **PROBIT REGRESSION OF TAKE-UP STATUS**



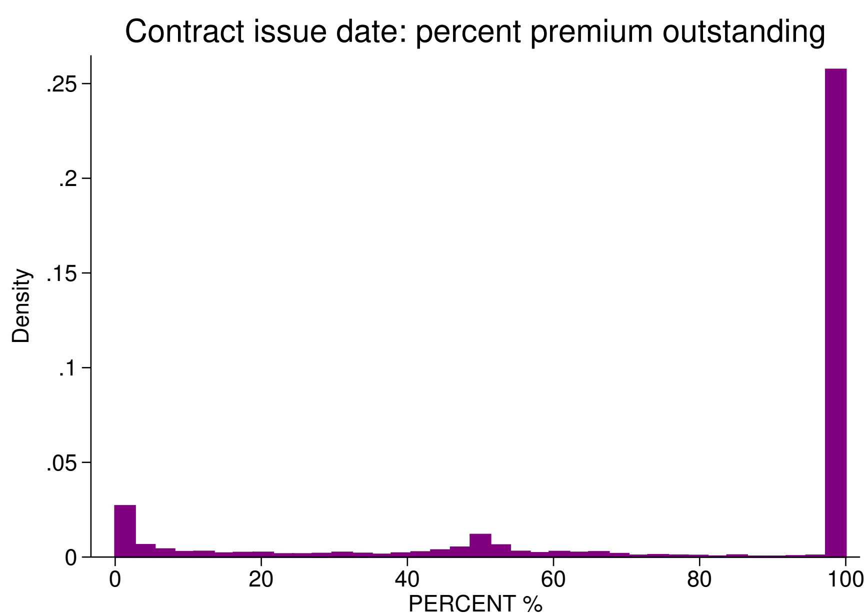
(b) **PROBIT REGRESSION OF TAKE-UP STATUS WITH CONTROLS**

Notes: Figure is based on a probit regression of an indicator for buying insurance on credit against monthly dummies, with and without controls for consumer characteristics. The month-by-month coefficients are displayed with the 95% confidence intervals. In both cases, vertical lines are used to indicate the timing of the regulation.

Figure 2.3: Distribution of Premium-Debt



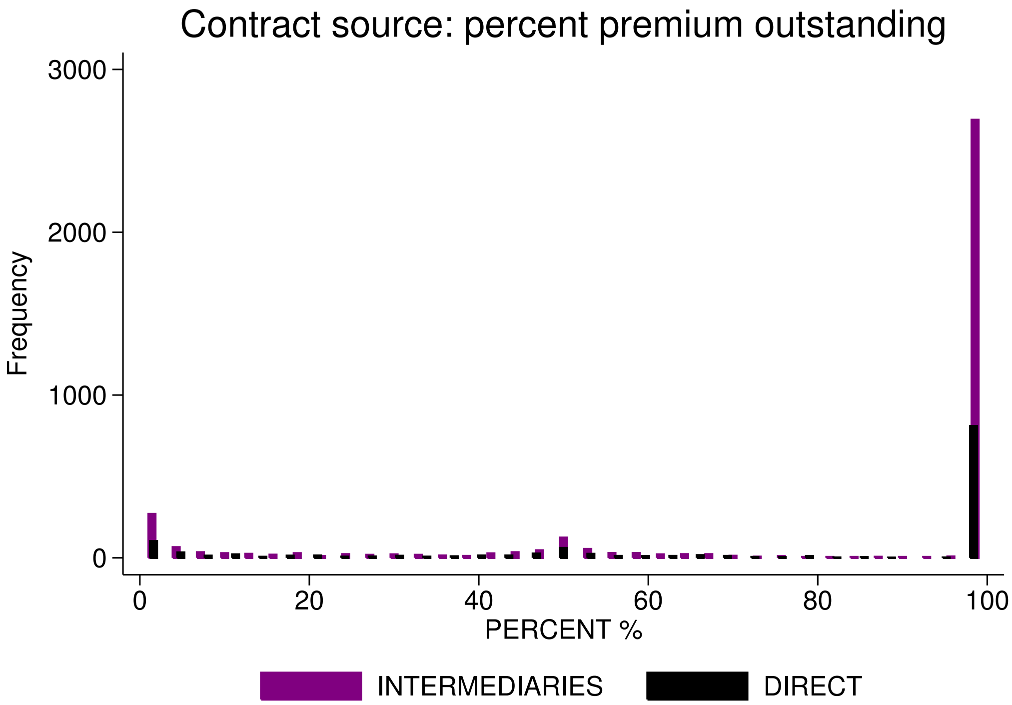
(a) **AMOUNT OWED (GHC)**



(b) **PERCENT OF PREMIUM IN DEBT**

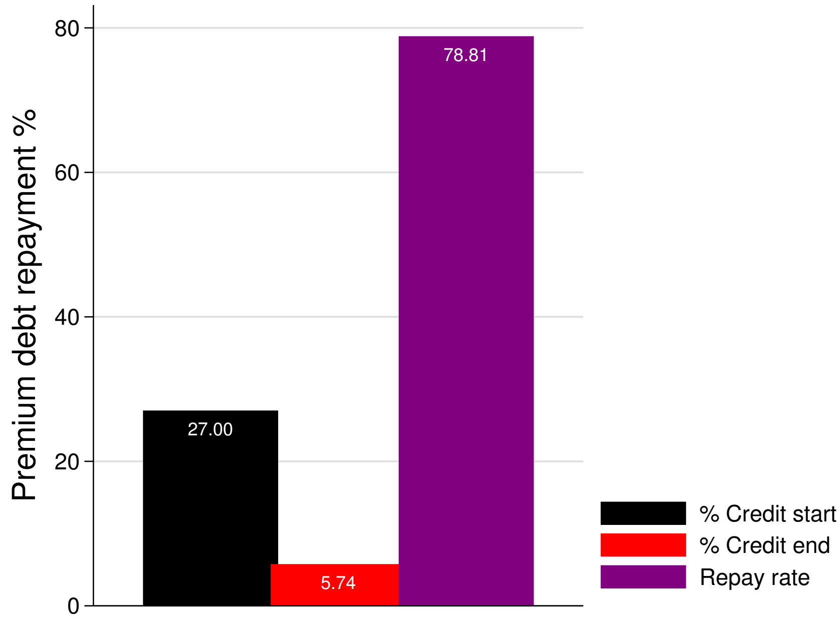
Notes: Figure shows the distribution of outstanding premiums at the time contracts are signed. The amount of premium debt is shown in (a), maxing at GHC600000. In (b), the premium debt expressed a percentage of total premium is displayed. This ranges between 0.2% to 100%.

Figure 2.4: Distribution of Premium-Debt by Source

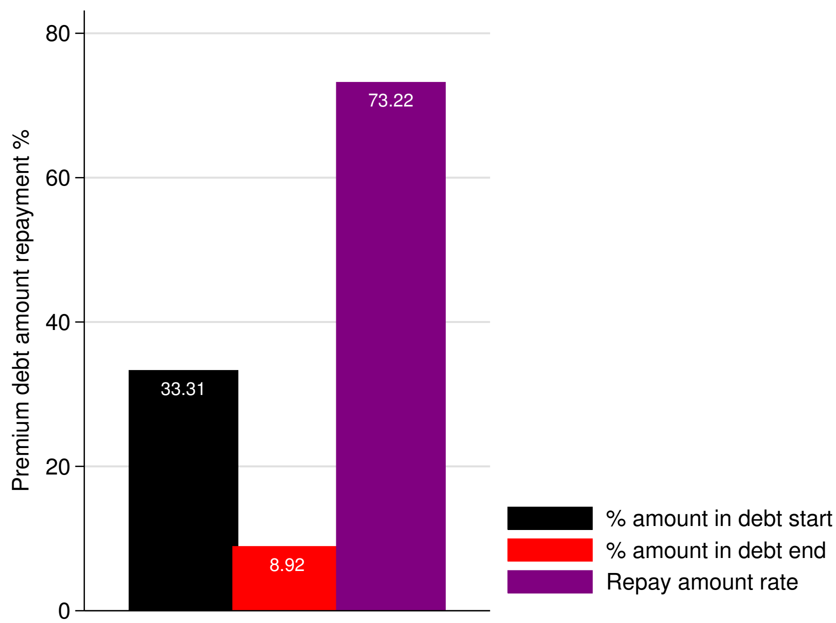


Notes: Figure shows the percent of outstanding premiums across the two channels of selling insurance policies: direct from the insurance firm versus intermediaries. The individual distributions are superimposed on each other. As shown, premium debts can range from 0.2% to 100% of premiums; many customers are more likely to initiate 100% premium debt contracts with intermediaries, compared to contracts from the insurer.

Figure 2.5: Premium-Debt Repayment



(a) **DEBT REPAYMENT BEFORE CONTRACT EXPIRES (EXTENSIVE)**



(b) **DEBT REPAYMENT BEFORE CONTRACT EXPIRES (INTENSIVE)**

Notes: Figures show the repayment rates for insurance premium debts prior to the no-credit policy. (a) Extensively: percent of consumers who began their contracts with credit and ended their coverage with/without some credit. (b) Intensively: percent of total premium amount in debt at the beginning of contracts versus the end of contracts prior to the reform.

Table 2.1: Transition Matrix: Debtors vs Switchers [%]

		Before Policy	
		DEBTOR=0	DEBTOR=1
After Policy	SWITCHER=0	99.98	0.57
	SWITCHER=1	0.02	**99.43**
		100	100

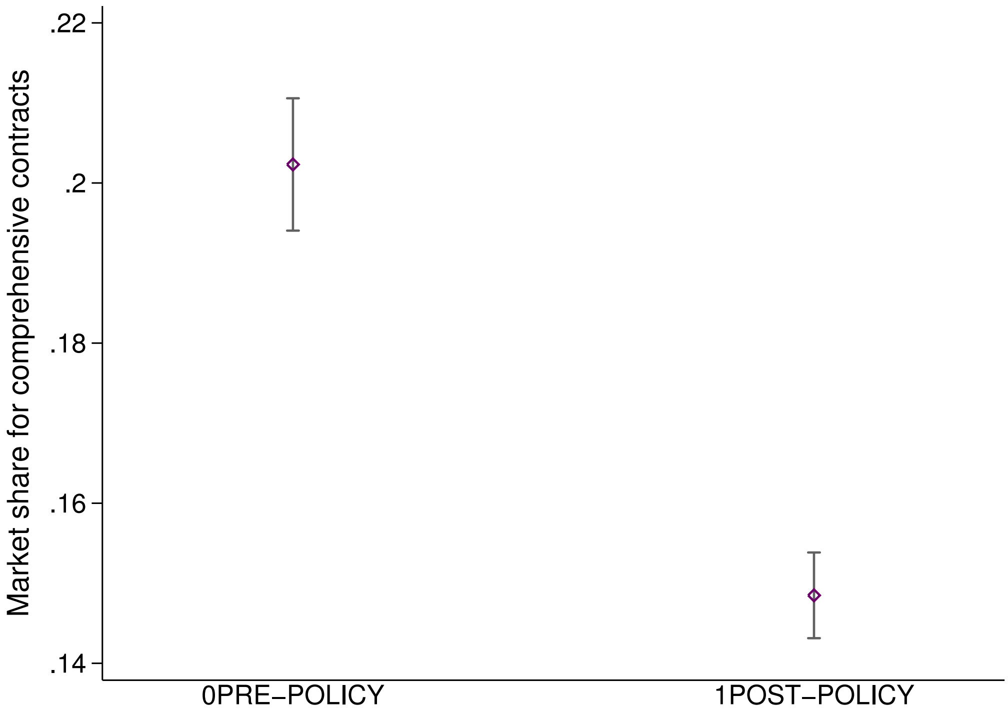
Notes: Debtors are consumers who were buying insurance on credit before the policy reform. Switchers are consumers who moved from Comprehensive to Basic contracts after policy reform. Over 99% of people who purchased insurance on credit switched to contracts with lower coverage.

Table 2.2: Formal “Moral Hazard Tests”

	LOSS		CLAIMS	
Formal Test	*Statistic /Estimate*	*Prob. value*	*Statistic /Estimate*	*Prob. value*
Proposed L_2-Type	-	-	0.167	0.040
Kolmogorov–Smirnov	-	-	0.113	0.001
OLS	[-0.016]	0.001	[-504.985]	0.038

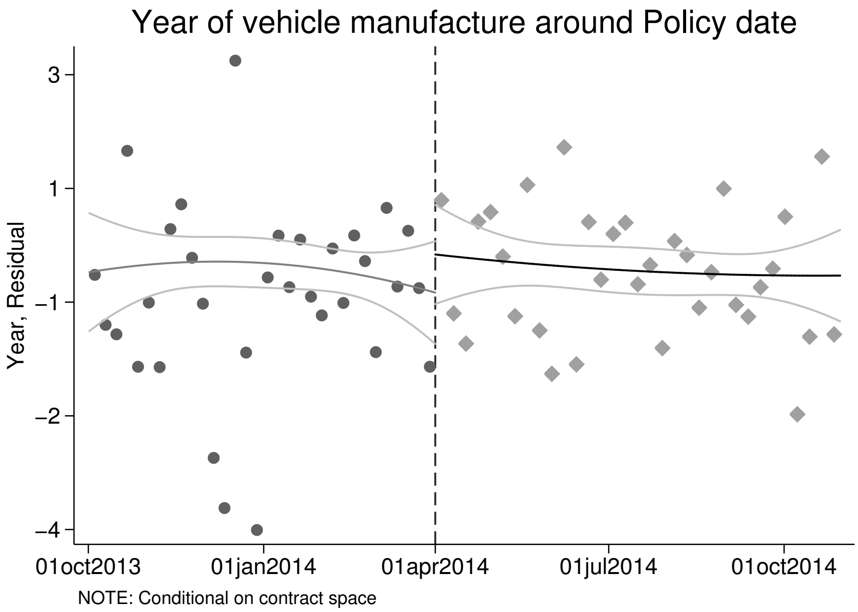
Notes: Table reports the test statistics and p-values for the two nonparametric tests: Proposed L_2-Type and Kolmogorov–Smirnov. The last row of the Table reports parameter estimates, contained in square brackets, and p-values from OLS estimations separately for loss, and claims outcomes. The p-value for the Proposed L_2-Type test are based on 999 nonparametric bootstrap resamples of the test statistic.

Figure 2.6: Choice Probabilities Conditional on Reform

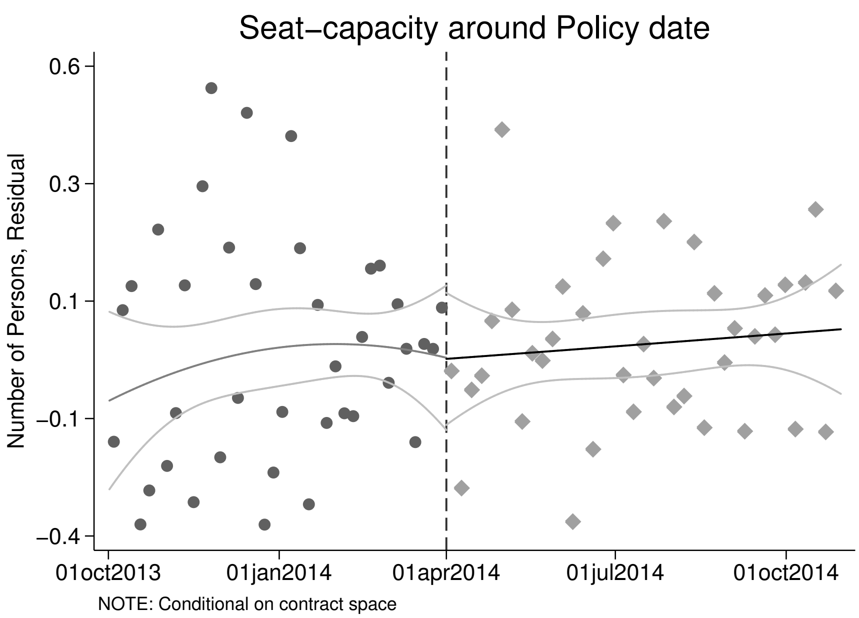


Notes: Figure shows the insurance choice probabilities for comprehensive contracts, before and after the regulatory reform. This is derived using the insurer's data set and a frequency estimator. The 95% confidence intervals are displayed around the estimates.

Figure 2.7: Distribution at Policy Cut-off



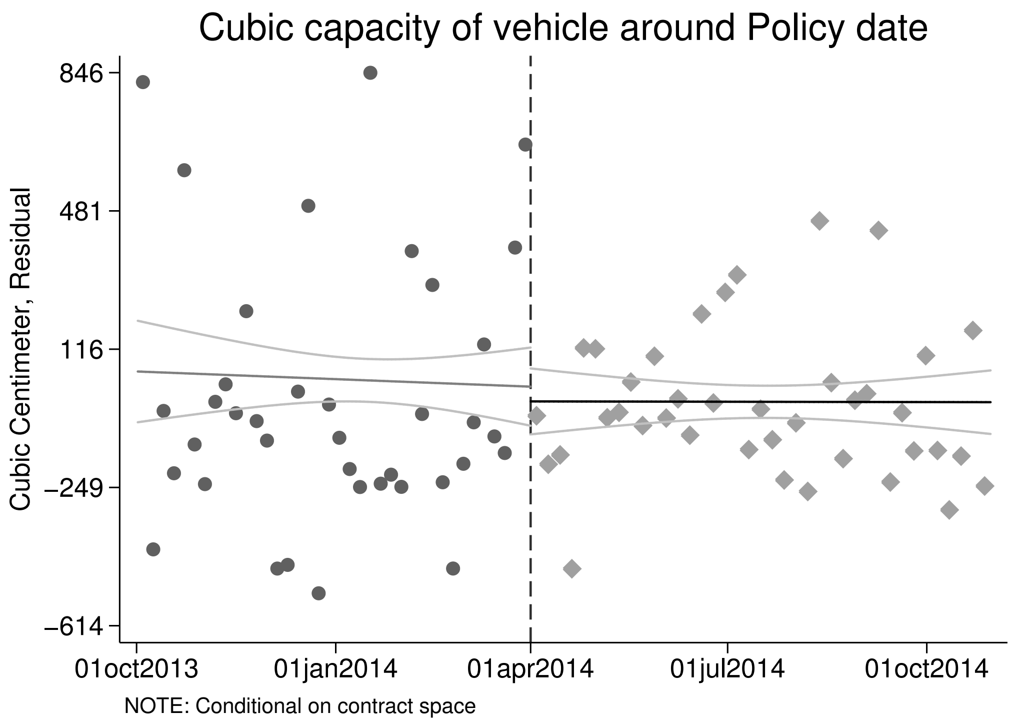
(a) **AGE OF VEHICLE**



(b) **SEAT CAPACITY OF VEHICLE**

Notes: Figures display the distribution of the various customer characteristics (age of vehicles; seat capacity of vehicles) around the policy cutoff. In all cases, the 95% confidence intervals are displayed around the estimates.

Figure 2.8: Distribution at Policy Cut-off



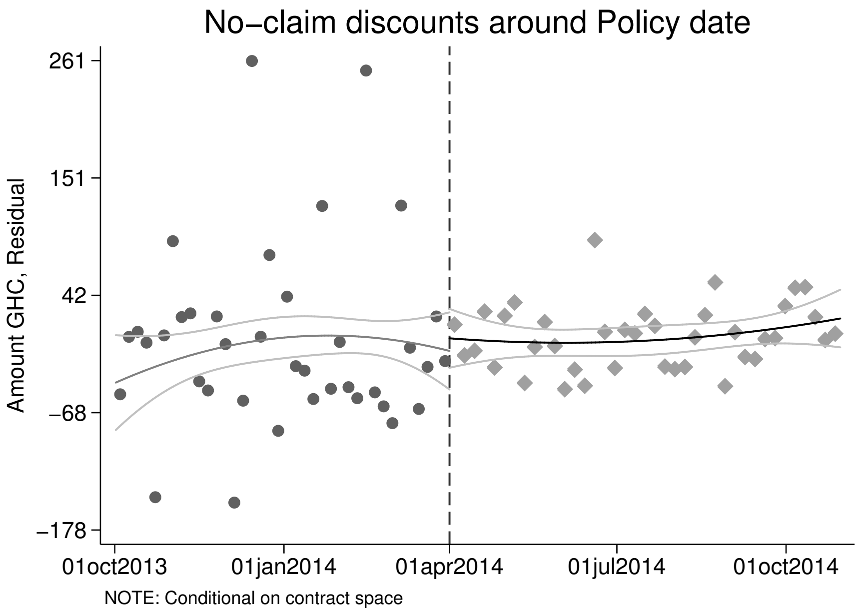
(a) **CUBIC CAPACITY OF VEHICLE**

	Private (%)	Commercial (%)
Pre-reform	88.44	11.56
Post-reform	88.49	11.51
Private includes: individual + corporate		

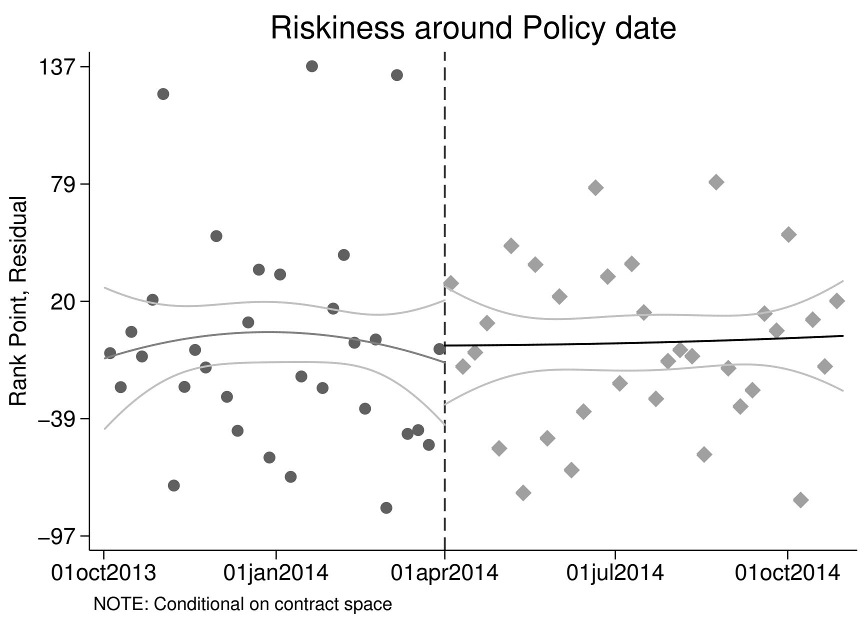
(b) **POOL: SHARE OF PRIVATE VS BUSINESS-TYPE VEHICLES**

Notes: Figure shows the distribution of vehicles cubic capacity around the policy cutoff. The 95% confidence intervals are displayed around the estimates.

Figure 2.9: Distribution at Policy Cut-off



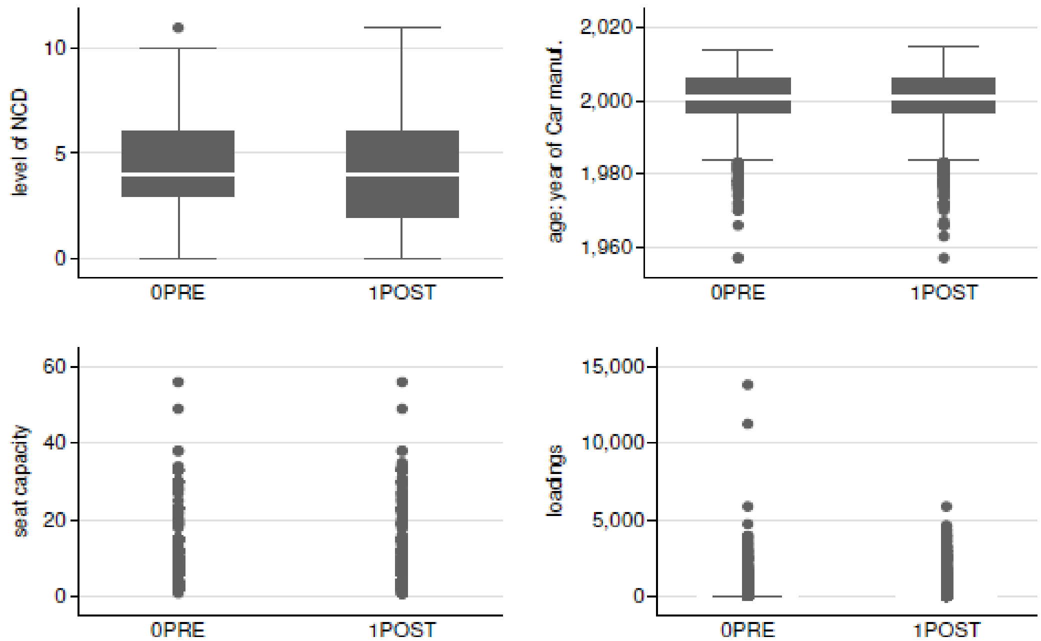
(a) **NO-CLAIM DISCOUNT**



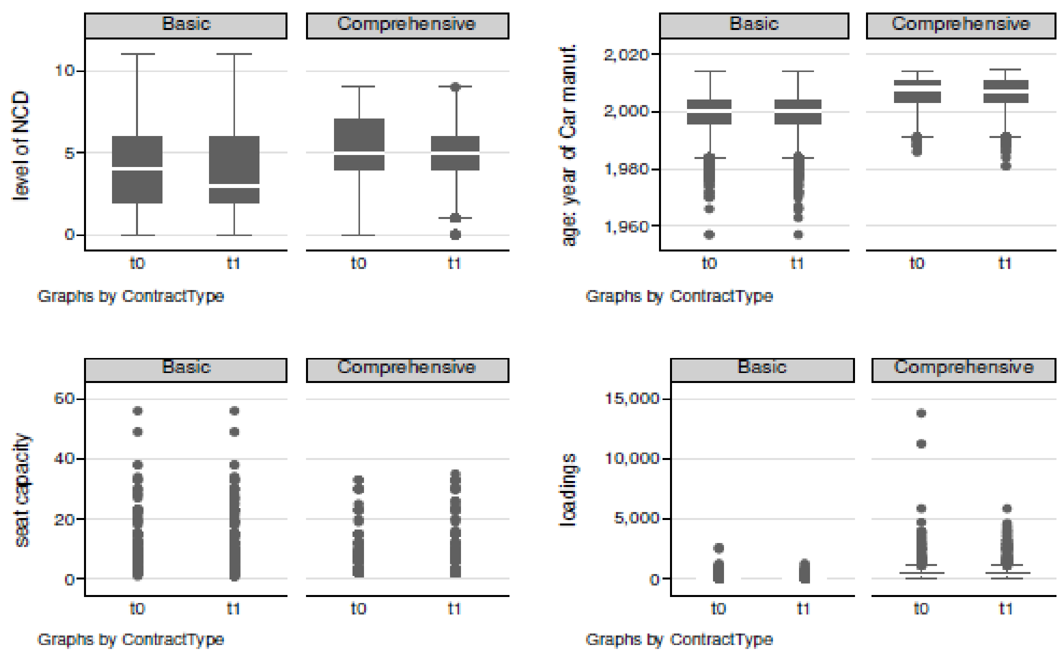
(b) **CONSUMER RISKINESS SCORE**

Notes: Figures show the distribution of the various customer characteristics (no-claim discount for premiums; riskiness scores) around the policy cutoff. In all cases, the 95% confidence intervals are displayed around the estimates.

Figure 2.10: Distribution of Customer Characteristics



(a) DISTRIBUTIONS: $X|Z$



(b) DISTRIBUTIONS: $X|(Z, D)$

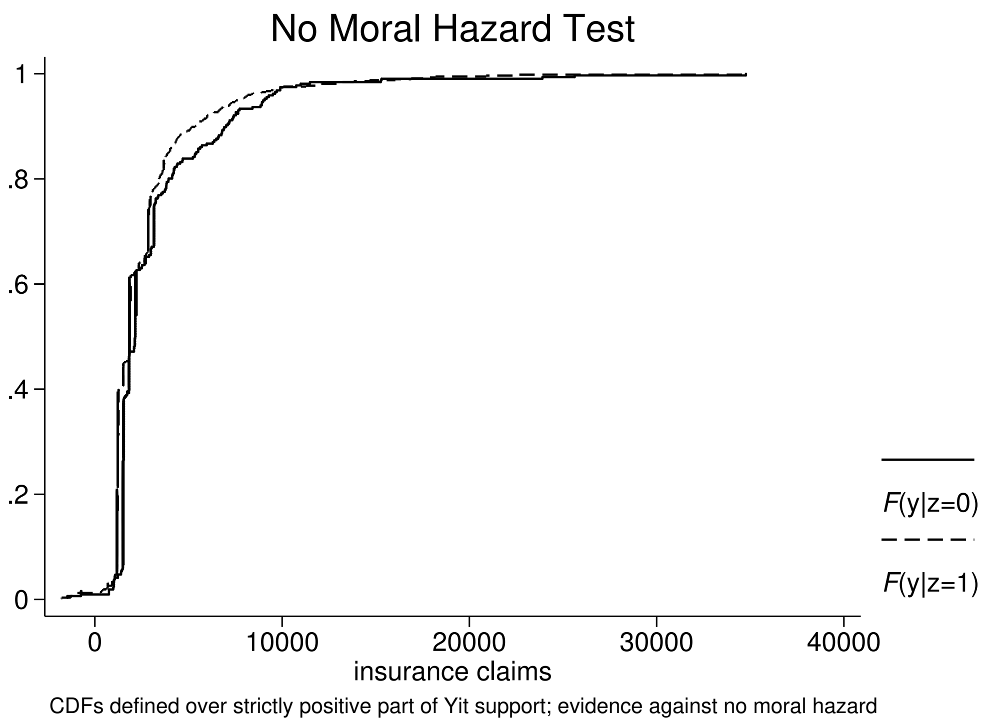
Notes: Figures display the distributions of customers characteristics conditional on time and choice of insurance contracts. (a)– similar distributions on observables across time t. (b)– similar distributions on observables within contracts.

Table 2.3: Estimates: Bounds on Moral Hazard

LOSS			**CLAIMS**		
	Bounds	95% CI		Bounds	95% CI
lb	0.0089	[0.0013, 0.015]	*lb*	51.96	[10.70, 117.45]
ub	0.7775	[0.7647, 0.7858]	*ub*	108172.02	[107224.49, 109509.68]

Notes: Table reports both the lower and upper bound estimates on moral hazard separately for loss, and claims outcomes. CI denotes confidence interval. lb and ub denotes lower and upper bound on moral hazard. The 95% confidence intervals are based on 999 nonparametric bootstrap resamples for the various objects of interest.

Figure 2.11: Distribution of Claim Amounts$|Z$



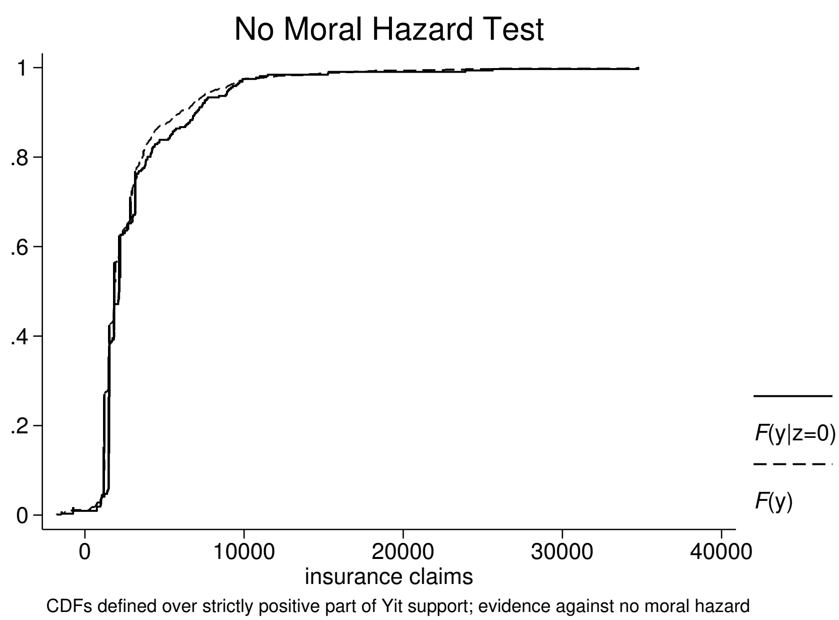
Notes: Figure shows the predicted distribution of claims before and after the no-credit regulation. The distribution in dash corresponds to realizations after the policy $z = 1$. The distributions reflect strictly positive claim amounts. The no-moral hazard test holds for any realization of y.

Table 2.4: Naive Estimates: Lower Bound Δ^l

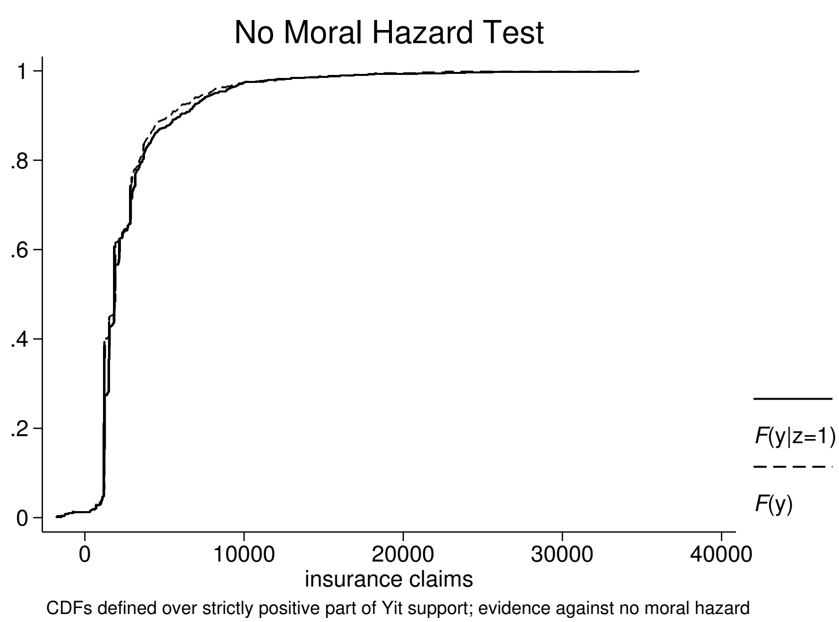
OUTCOME	Bounds	95% CI	**Selection Effect**
LOSS	0.035	[0.0075, 0.082]	0.026
CLAIMS	381.70	[25.15, 782.89]	329.70

Notes: Table reports "naive" lower bound estimates on moral hazard separately for loss, and claims outcomes. Estimations are based on a naive lower bound estimator that neglects adverse selection. CI denotes confidence interval. lb denotes lower bound on moral hazard. The 95% confidence intervals are based on 999 nonparametric bootstrap resamples for the various objects of interest.

Figure 2.12: Distribution of Claim Amounts



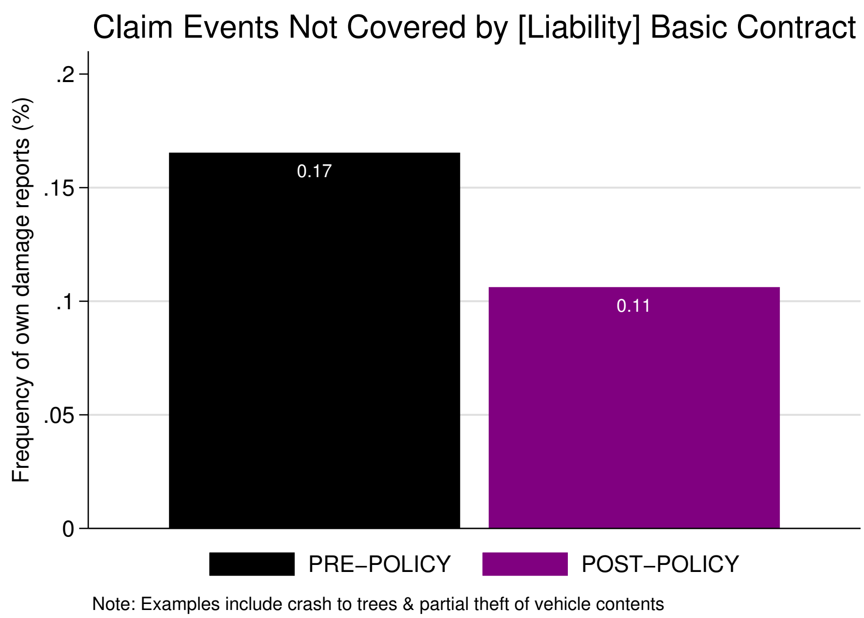
(a) **PRE-POLICY VS UNCONDITIONAL**:$\hat{F}(y|z = 0)$ vs $\hat{F}(y)$



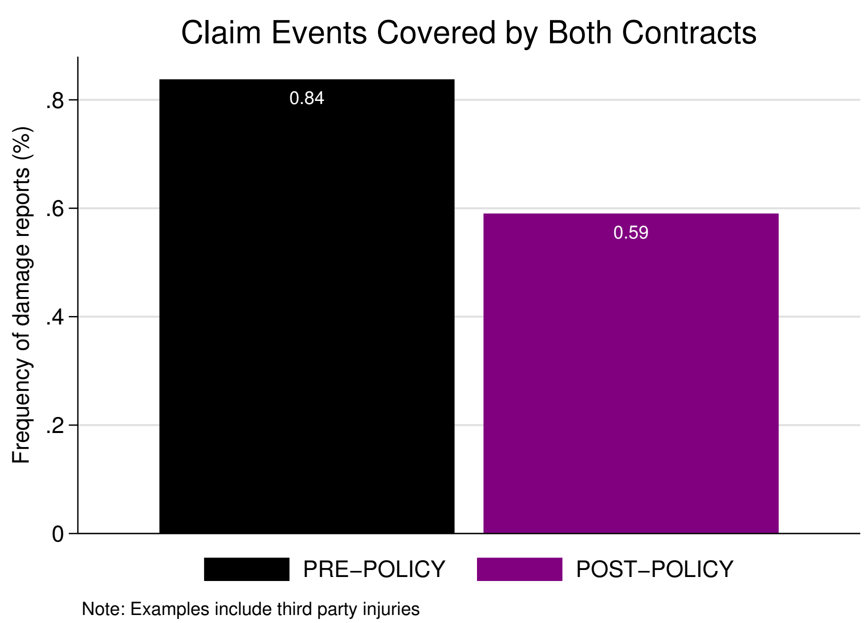
(b) **PSOT-POLICY VS UNCONDITIONAL**:$\hat{F}(y|z = 1)$ vs $\hat{F}(y)$

Notes: Figures shows the predicted distribution of claims. In (a) the pre-policy ($z = 0$) outcomes are compared with the overall claims. In (b) the post-policy ($z = 0$) outcomes are compared with the overall claims. The distributions reflect strictly positive claim amounts. The no-moral hazard test holds for any realization of y.

Figure 2.13: Type of Claim Events Conditional on Policy



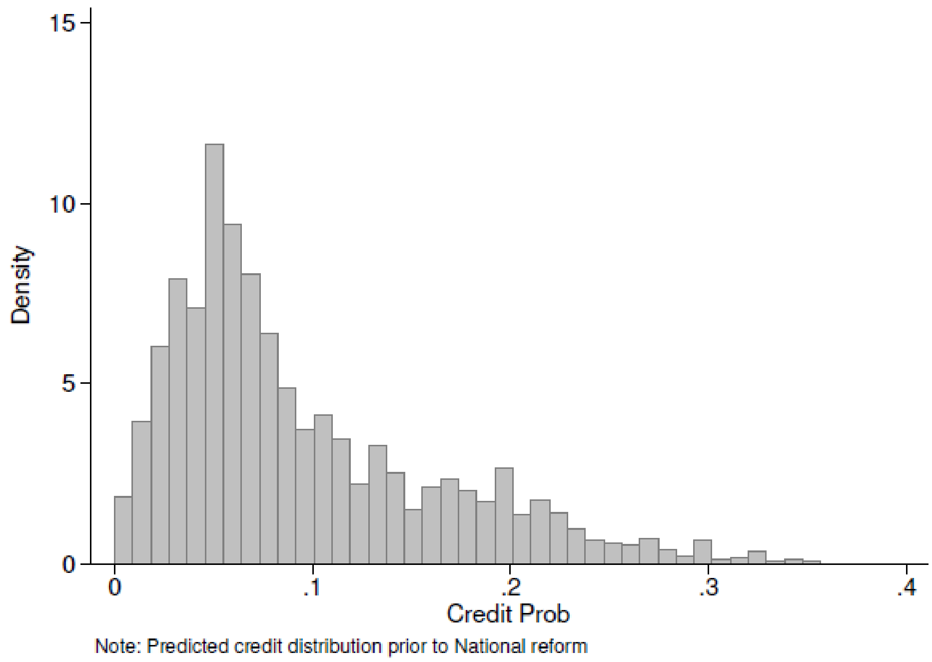
(a) **EVENTS UNCOVERED BY BASIC CONTRACTS**



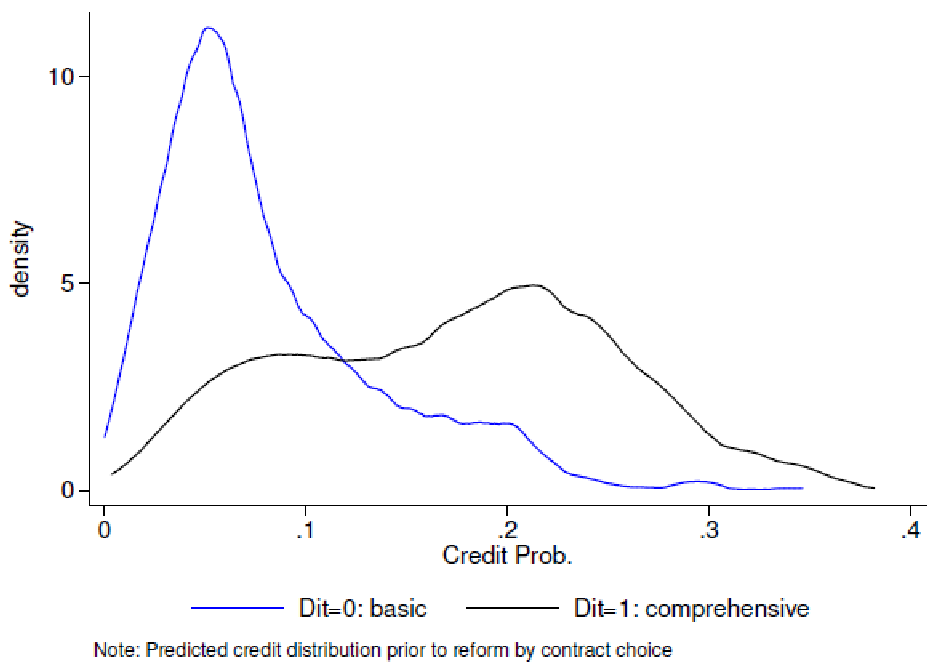
(b) **EVENTS COVERED BY BOTH CONTRACTS**

Notes: Figures show the distribution of specific claim events before and after the policy reform. (a) shows the changes in the frequency of claim events that are not covered by basic contracts (i.e., covered by only comprehensive contracts). In (b), the distribution is shown for events that are covered by both contracts, which excludes the events in (a).

Figure 2.14: Credit: Purchase Probabilities



(a) DISTRIBUTION OF CREDIT PROBABILITIES



(b) DISTRIBUTION OF CREDIT PROBABILITIES BY CONTRACT

Notes: Figures show the distribution of the estimated credit probabilities: ranging between 0-41%, exclusive. The overall distribution is displayed in (a). In (b), I condition this on the contract space. There is much higher probability of buying comprehensive contracts with credit, compared to basic contracts that provide less coverage.

Table 2.5: Estimates: Customers Below vs Above Median Probability

	CLAIMS			LOSS		
Customers	–Bounds–		95% CI	–Bounds–		95% CI
Below Median $< P(c^r; x)_{[50]}$	*lb*	16.47	[1.90,59.53]	*lb*	0.0043	[0.0002,0.0302]
Above Median $\geq P(c^r; x)_{[50]}$	*lb*	80.32	[17.03,140.21]	*lb*	0.0073	[0.0004,0.0208]

Notes: Table reports the lower bound estimates on moral hazard for customers below and above the median of the predicted credit probability (see Figures 5a and 5b), denoted $P(c^r; x)_{[50]}$. CI denotes confidence interval. lb denotes lower bound on moral hazard. The 95% confidence intervals are based on 999 nonparametric bootstrap resamples for the various objects of interest. The median is $\cong 0.075$.

Table 2.6: Estimates: Bounds on Moral Hazard by Credit Quartiles

	CLAIMS			**LOSS**		
Quartile	–Bounds–		95% CI	–Bounds–		95% CI
q_1	*lb*	30.04	[10.98,57.72]	*lb*	0.0121	[0.0021,0.0260]
q_2	*lb*	16.38	[3.22,24.24]	*lb*	0.0028	[0.0002,0.0077]
q_3	*lb*	190.90	[22.23,361.28]	*lb*	0.0662	[0.0003,0.0701]
q_4	*lb*	45.66	[5.45,135.38]	*lb*	0.0029	[0.0001,0.0105]

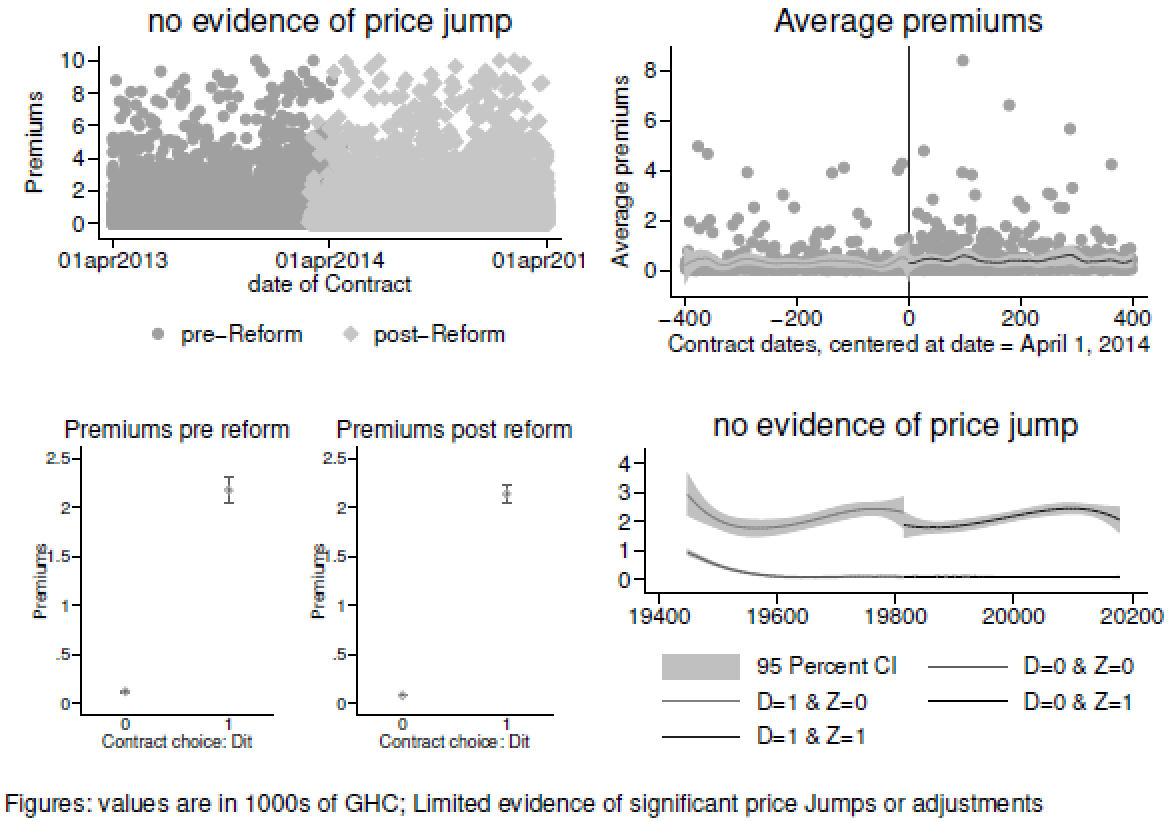
Notes: Table reports the lower bound estimates on moral hazard by quartiles of predicted credit probabilities (see Figures 5a and 5b). CI denotes confidence interval. lb denotes lower bound on moral hazard. q_1, q_2, q_3 and q_4 correspond to the first, second, third and fourth quartiles of credit probabilities respectively. The 95% confidence intervals are based on 999 nonparametric bootstrap resamples for the various objects of interest. These quartile categories are useful in describing the distribution of constraints in credit and providing enough observations in each category to ensure that estimations are not done on completely empty cells. In practice, I create the respective quartile categories as follows: $[min, P(c^r;x)_{[25]}]$, $(P(c^r;x)_{[25]}, P(c^r;x)_{[50]}]$, $(P(c^r;x)_{[50]}, P(c^r;x)_{[75]}]$ and $(P(c^r;x)_{[75]}, max]$ where min denotes minimum, max denotes maximum, and the numbers in squared brackets represent the 25th, 50th/median, and 75th percentile values. $min = 3.74E-06$; $P(c^r;x)_{[25]} = 0.046$; $P(c^r;x)_{[50]} = 0.075$; $P(c^r;x)_{[75]} = 0.131$ and $max = 0.403$.

Table 2.7: Effect of Reform on Differential Pricing

DV: **Premiums**	(1)	(2)	(3)
D_{it}		225.9 (503.0)	-292.5 (305.9)
Policy$_t$	18.67(15.58)	-85.77 (149.9)	33.78 (72.79)
$D_{it} \times$ Policy$_t$		206.4 (162.0)	-111.4 (78.74)
NCD level			-55.78** (23.12)
Loading			3.844*** (0.0495)
Year of manuf.			75.25*** (10.14)
Seat capacity			-16.64** (7.097)
Cert. Type dummies	No	No	Yes
Constant	428.1***(11.46)	1,673***(400.3)	-149,535***(20,336)
R-squared	<0.01	<0.01	0.77
Agent-level FEs	Yes	Yes	Yes

Notes: Table reports results from the regression of premiums on policy reform and contract choice variables. Errors are robust to arbitrary correlations and heteroskedasticity, and are shown in parentheses. *** Significant at 1% level; ** Significant at 5% level; * Significant at 10% level.

Figure 2.15: Firm Price Response



Notes: Figures show the distribution and differential changes in insurance premiums before and after the regulatory reform. The overall distribution is shown in the top panel. In the bottom panel, I show the differential changes across the two different contracts. The 95% confidence intervals are also displayed around the estimates.

Table 2.8: Same Policy Numbers: Bounds on Moral Hazard

CLAIMS		**LOSS**	
Lower bound	95% CI	Lower bound	95% CI
33.21	[9.02, 55.44]	0.0061	[0.0001, 0.0124]

Notes: Table reports the lower bound estimates on moral hazard separately for loss, and claims outcomes. The results are shown for the set of customers with same policy numbers before and after the policy reform, April 1, 2014. CI denotes confidence interval. The 95% confidence intervals are based on 999 nonparametric bootstrap resamples for the various objects of interest. Estimate 33.21 translates to about 41.0% of mean claim amounts.

Table 2.9: Same Policy Numbers: Customers Below vs Above Median Probability

	CLAIMS			LOSS		
Customers	–Bounds–		95% CI	–Bounds–		95% CI
Below: $< P(c^r; x)_{[50]}$	*lb*	42.84	[0.21,168.47]	*lb*	0.0125	[0.0006,0.0411]
Above: $\geq P(c^r; x)_{[50]}$	*lb*	107.34	[26.65,661.66]	*lb*	0.0172	[0.0019,0.0397]

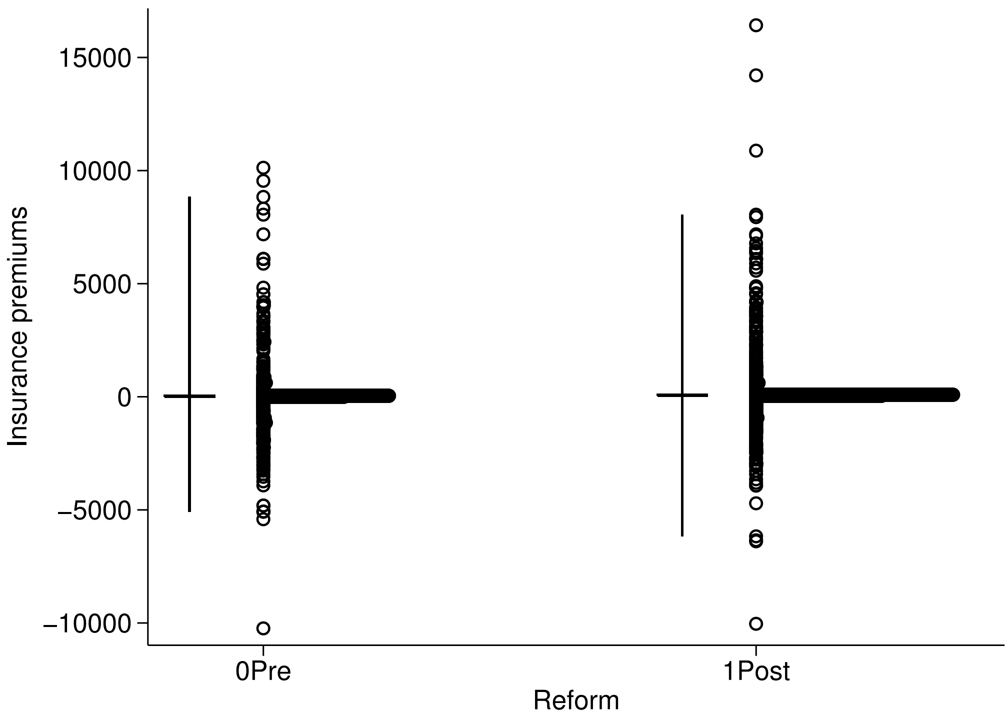
Notes: Table reports the lower bound estimates on moral hazard for customers below (unconstrained) and above (constrained) the median of the predicted credit probabilities, denoted $P(c^r; x)_{[50]}$. The estimates are based on the set of customers with the same policy numbers before and after the reform's introduction, April 1, 2014. CI denotes confidence interval. lb denotes lower bound on moral hazard. The 95% confidence intervals are based on 999 nonparametric bootstrap resamples for the various objects of interest. The median is $\cong 0.075$.

Table 2.10: Only Liability Events under Both Contracts: Bounds on Moral Hazard

CLAIMS		**LOSS**	
Lower bound	95% CI	Lower bound	95% CI
31.62	[3.92, 47.83]	0.0068	[0.0001, 0.0118]

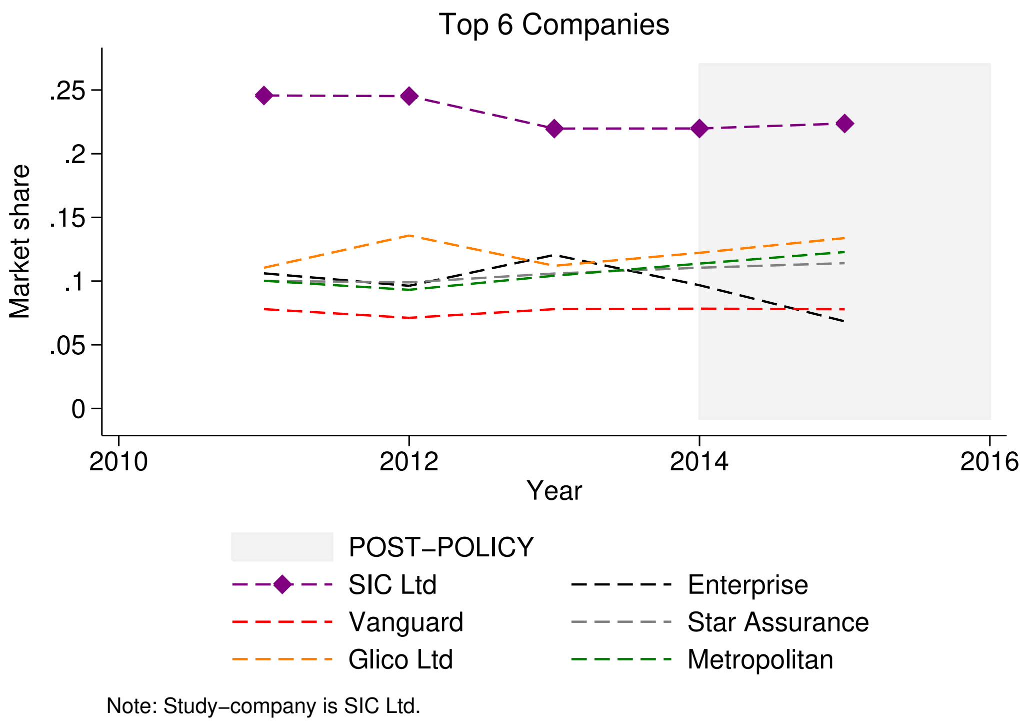
Notes: Table reports the lower bound estimates on moral hazard separately for loss, and claims outcomes. The results are shown for only liability events: covered under both comprehensive and third-party liability contracts. CI denotes confidence interval. The 95% confidence intervals are based on 999 nonparametric bootstrap resamples for the various objects of interest. The estimate 31.63 translates to 39.10% of mean claim amounts; 0.0068 translates to about 16.58% of loss occurrence probabilities or the number of claims.

Figure 2.16: Effect of Reform on Insurance Pricing

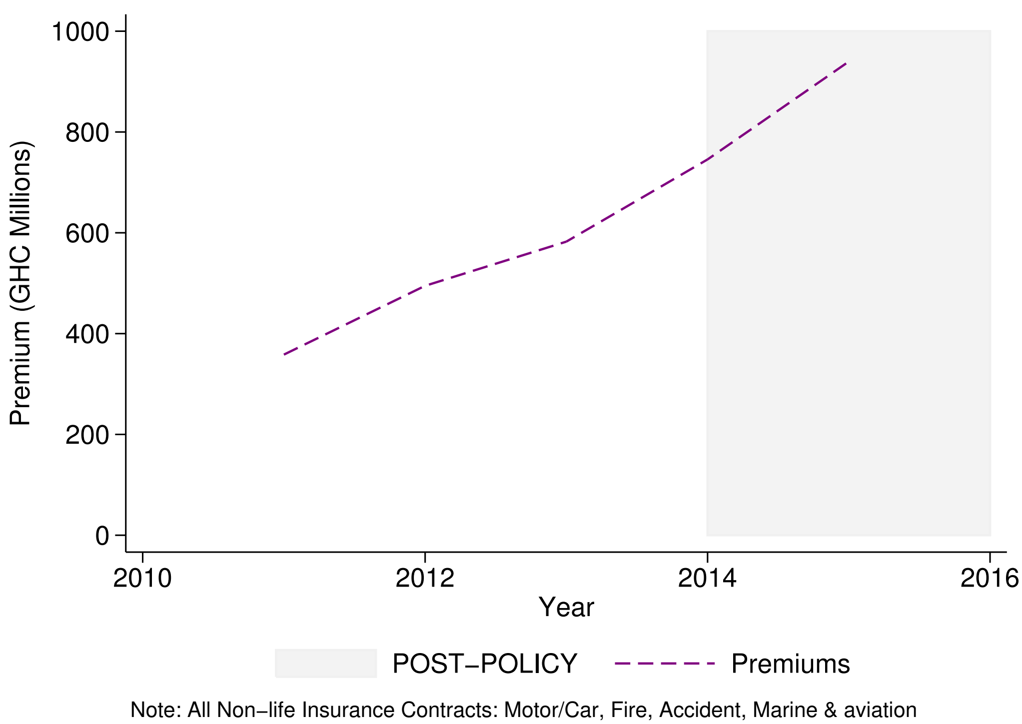


Notes: Figure reflects the raw annual distribution of insurance premiums after customer-level fixed effects are removed from the data. The figure is shown for the period before and after the National policy reform. The sample includes all policy holders. Strip-plots show whiskers containing inner 1.5×inter-quartile range of the observations (Turkey 1977).

Figure 2.17: Distribution of Market Shares and Industry Growth

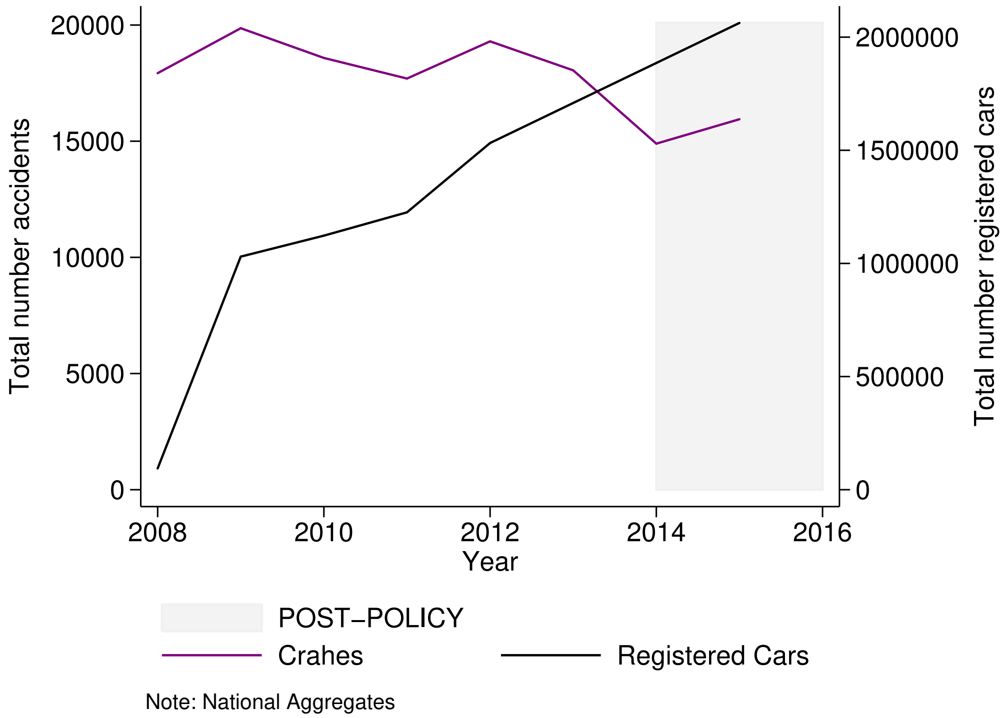


(a) **INDUSTRY MARKET SHARE**

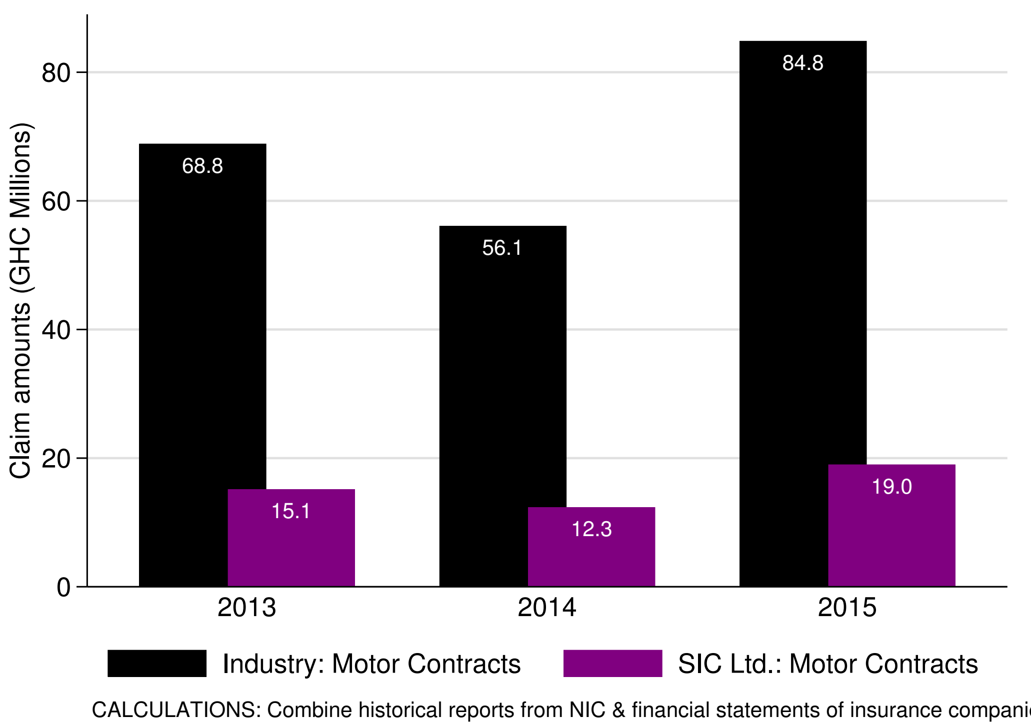


(b) **INDUSTRY TOTAL PREMIUMS**

Figure 2.18: Distribution of Aggregate Losses and Industry Claims



(a) **TOTAL CAR ACCIDENTS**



(b) **INDUSTRY'S TOTAL CLAIMS FOR MOTOR CONTRACTS**

Table 2.11: Bounds on Moral Hazard for Different Time Windows

Window	**CLAIMS**		**LOSS**	
	Lower bounds	95% CI	Lower bounds	95% CI
8 Months	32.30	[0.13, 68.80]	0.0058	[0.0001, 0.0101]
4 Months	41.50	[5.76, 94.96]	0.0049	[0.0001, 0.0212]

Notes: Table reports the lower bound estimates on moral hazard separately for loss and claims outcomes. The results are shown for different time windows around the date of the policy reform, April 1, 2017. CI denotes confidence interval. The 95% confidence intervals are based on 999 nonparametric bootstrap resamples for the various objects of interest. Estimates (32.30 and 41.50) translate is about (34.0% and 41.0%) of mean claim amounts.

Table 2.12: Winsorized Data: Bounds on Moral Hazard

	CLAIMS		**LOSS**	
	Lower bound	95% CI	Lower bound	95% CI
95% Winsorization	35.12	[1.50, 65.53]	0.0078	[0.0045,0.0135]

Notes: Table reports the lower bound estimates on moral hazard separately for loss, and claims outcomes. Data 95% winsorized by replacing all data below the 2.5th percentile with the 2.5th percentile, and data above the 97.5th percentile with the 97.5th percentile value. For bounds estimation, these percentile values are 0 and 12976, respectively. CI denotes confidence interval. The 95% confidence intervals are based on 999 nonparametric bootstrap resamples for the various objects of interest. Estimate 35.12 translates to about 41.2% of mean claim amounts.

Table 2.13: Certainty Equivalent Loss vs Profit Gains from No-Credit Policy

△CERTAINTY EQUIVALENT		**△PROFIT**	
Absolute risk aversion, γ	$\triangle CE$ (GHC)	Status: premium-debts	$\triangle\pi$ (GHC)
0.001	-111,559	All paid, pre-policy	1,023,168
0.003	-178,341	79% paid, pre-policy	2,048,883
		Never paid, pre-policy	9,210,325

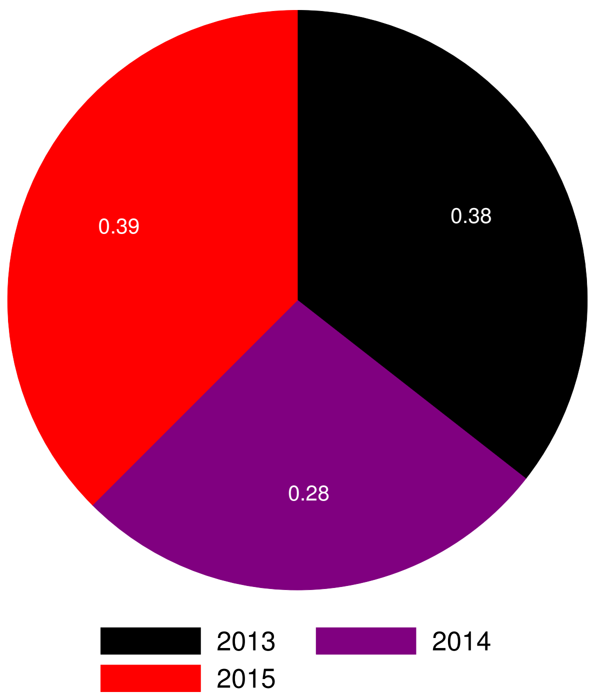
Notes: Table presents estimates for the certainty equivalent loss and gain in profits as a result of the no-credit policy. Left panel gives the estimates for certainty equivalent loss for different absolute risk aversion parameters. To be more conservative, I allow for a larger value of γ =0.003 (more value for insurance) and only present results for the case where the share of consumers who credit insurance premiums "never paid" before the policy reform (more value for buying on credit to consumers; less enforcement of credit arrangements). Right panel gives the profit gains under different assumptions regarding the enforcement of credit arrangements (i.e., repayment of premium debts) prior to the policy reform. As shown in Section 2.3, the repayment rate for premium debts is about 79%.

Table 2.14: Varying λ and Profit Gains from No-Credit Policy

Status: premium-debts	$\triangle\pi$ (GHC) $\lambda = 0.00$	$\lambda = 0.15$	$\triangle\pi$ (GHC) $\lambda = 0.25$	$\lambda = 0.35$
All paid, pre-policy	1,157,262	1,076,806	1,023,168	969,530
79% paid, pre-policy	2,182,978	2,102,521	2,048,883	969,530
Never paid, pre-policy	9,523,116	9,335,442	9,210,325	9,085,209

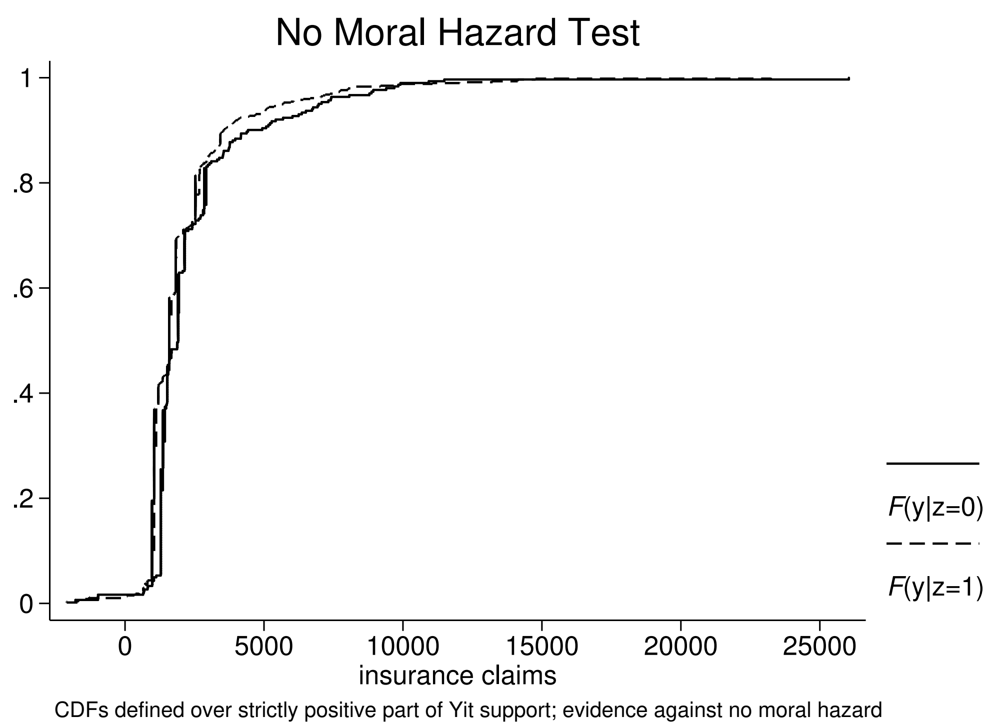
Notes: Table presents estimates for the gain in profits as a result of the no-credit policy. To be more conservative, I allow for as low as $\lambda = 0.15$ (low transaction or claim processing costs to firms) to $\lambda = 0.35$ (high transaction or claim processing costs to firms). Results for the baseline $\lambda = 0.25$ are also shown for comparison. The profit gains are shown under different assumptions regarding the enforcement of credit arrangements (i.e., repayment of premium debts) prior to the policy reform. As shown in Section 2.3, the repayment rate for premium debts is about 79%.

Figure 2.19: Industry's Profits or Claims Ratio for Motor Contracts



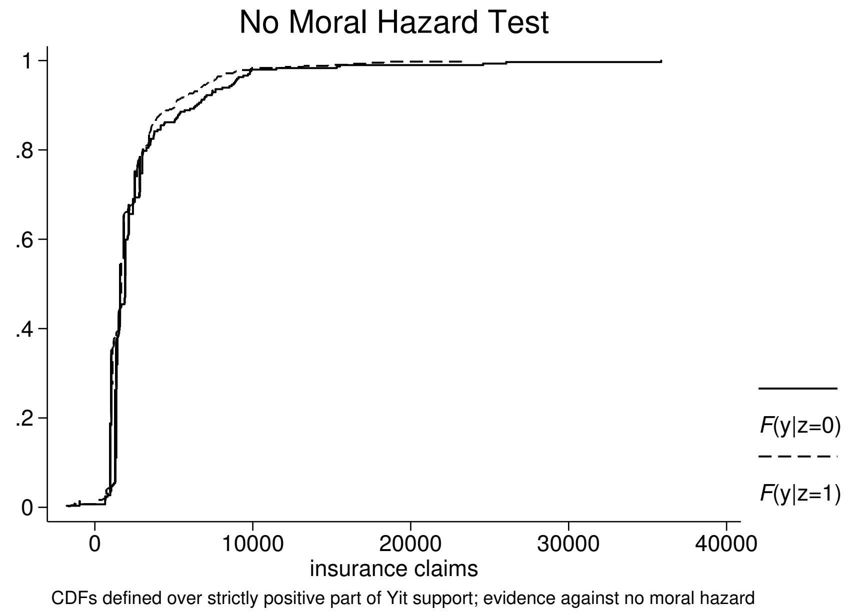
CALCULATIONS: Combine historical reports from NIC & financial statements of insurance companies

Figure 2.20: Same Policy Numbers: Distribution of Claim Amounts

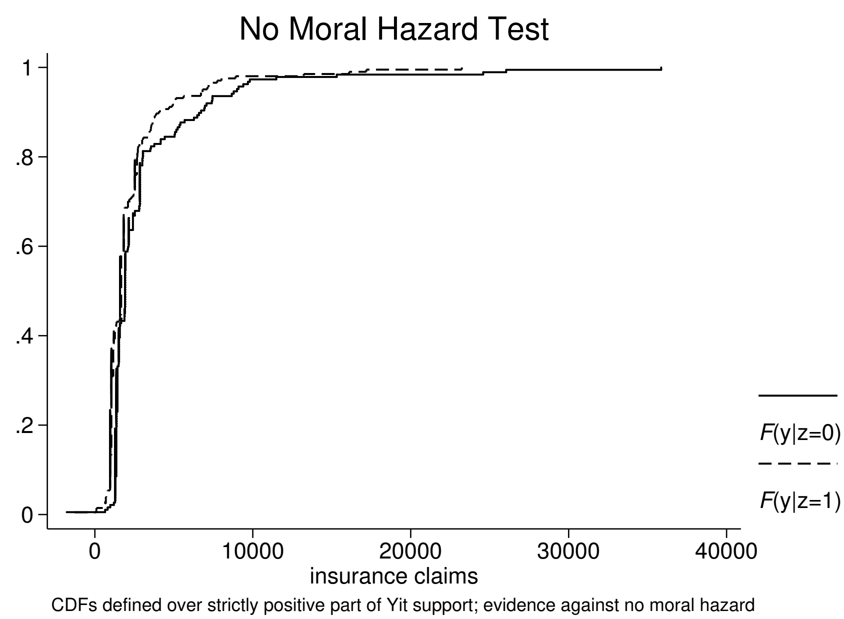


Notes: Figure shows the predicted distribution of claims before and after the no-credit regulation. The distribution in dash corresponds to realizations after the policy $z = 1$. The distributions reflect strictly positive claim amounts. The no-moral hazard test holds for any realization of y.

Figure 2.21: Distribution of Claim Amounts for Different Time Windows



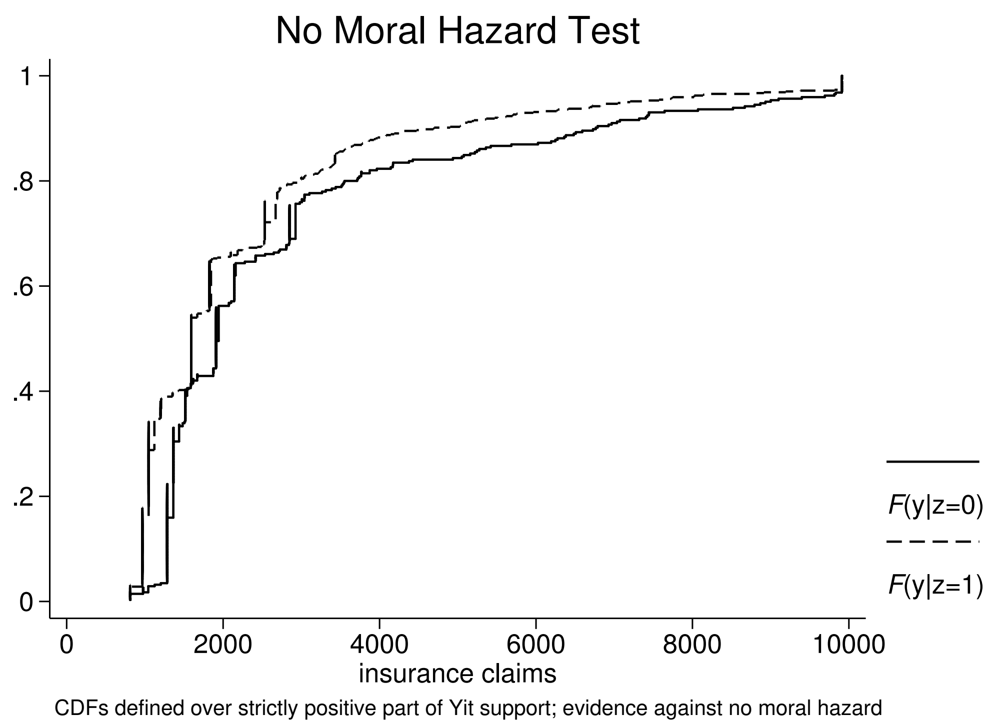
(a) TIME WINDOW 1: 8 MONTHS BEFORE & AFTER REFORM



(b) TIME WINDOW 2: 4 MONTHS BEFORE & AFTER REFORM

Notes: Figure shows the predicted distribution of claims before and after the no-credit regulation across different time windows around the policy. The distribution in dash corresponds to realizations after the policy $z = 1$. The distributions reflect strictly positive claim amounts. The no-moral hazard test holds for any realization of y.

Figure 2.22: Winsorized Data: Distribution of Claim Amounts



Notes: Figure shows the predicted distribution of claims before and after the no-credit regulation for 95% winsorized data. The distribution in dash corresponds to realizations after the policy $z = 1$. The distributions reflect strictly positive claim amounts. The no-moral hazard test holds for any realization of y.

2.9 Bibliography

1. Banerjee, Abhijit. 2001. "Contracting Constraints, Credit Markets and Economic Development." in M. Dewatripont, L. P. Hansen and S. J. Turnovsky (eds.) *Advances in Economics and Econometrics*, Cambridge: Cambridge University Press.

2. Banerjee, Abhijit and Esther Duflo. 2011. "Poor Economic: A Radical Rethinking of the Way to Fight Global Poverty. New York: Public Affairs." 15th Edition. http://onlinelibrary.wiley.com/doi/10.1002/jid.2936/abstract

3. Banerjee, Abhijit, Esther Duflo, and Richard Hornbeck. 2014. "Bundling Health Insurance and Microfinance in India: There Cannot Be Adverse Selection If There Is No Demand." *American Economic Review* 104(5): 291-97.

4. Bardhan, Pranab K. 1980. "Interlocking Factor Markets and Agrarian Development: A Review of Issues." *Oxford Economic Papers* 32(1): 82-98.

5. Bardhan, Pranab K. 1989. The Economic Theory of Agrarian Institutions: Clarendon Press Oxford.

6. Bell, Clive. 1988. "Credit Markets and Interlinked Transactions." In *Handbook of Development Economics*. eds. by Hollis Chenery, and T.N. Srinivasan, 1 of Handbook of Development Economics: Elsevier, Chap. 16 763-830.

7. Blundell, Richard W., and James L. Powell. 2003. "Endogeneity in Nonparametric and Semiparametric Regression Models." in M. Dewatripont, L. P. Hansen and S. J. Turnovsky (eds.) *Advances in Economics and Econometrics*, Cambridge: Cambridge University Press.

8. Braverman, Avishay, and Joseph E. Stiglitz. 1982. "Sharecropping and the Interlinking of Agrarian Markets." *American Economic Review*, 72 (4): 695-715.

9. Braverman, Avishay, and Joseph E. Stiglitz. 1986. "Landlords, Tenants and Technological Innovations." *Journal of Development Economics*, 23: 383-413.

10. Casaburi, Lorenzo, and Jack Willis. 2017. "Time vs. State in Insurance: Experimental Evidence from Contract Farming in Kenya." Mimeo, Harvard University.

11. Carter, Michael R, Lan Cheng, and Alexander Sarris. 2013. "The impact of Interlinked Index Insurance and Credit Contracts on Financial Market Deepening and Small Farm Productivity." Mimeo, University of California, Davis.

12. Chetty, Raj. 2008. "Moral Hazard versus Liquidity and Optimal Unemployment Insurance." *Journal of Political Economy* 116(2): 173–234.

13. Chiappori, Pierre-André, and Bernard Salanié. 2000. "Testing for Asymmetric Information in Insurance Markets." *Journal of Political Economy* 108(1): 56–78.

14. Chiappori, Pierre-André, and Bernard Salanié. 2014. "Asymmetric Information in Insurance Markets: Predictions and Tests." in G. Dionne (ed.) *Handbook of Insurance*, 2nd edition.

15. Chiappori, Pierre-André, Bruno Jullien, Bernard Salanié and François Salanié. 2006. "Asymmetric Information in Insurance: General Testable Implications." *Rand Journal of Economics* 37(4): 783–798.

16. Cole, Shawn, Xavier Gine, Jeremy Tobacman, Petia Topalova, Robert Townsend, and James Vickery. 2013. "Barriers to Household Risk Management: Evidence from India." *American Economic Journal: Applied Economics*, 5(1): 104–35.

17. Cohen, Alma, and Liran Einav. 2007. "Estimating Risk Preferences from Deductible Choice." *American Economic Review* 97(3): 745–88.

18. Cohen, Alma, and Rajeev Dehejia. 2004. "The Effect of Automobile Insurance and Accident Liability Laws on Traffic Fatalities." *Journal of Law and Economics* 47(2): 357–393.

19. Cohen, Jessica, and Pascaline Dupas. 2010. "Free Distribution or Cost-Sharing? Evidence from a Randomized Malaria Prevention Experiment." *Quarterly Journal of Economics* 125(1): 1–45.

20. Davidoff, Thomas, and Gerd Welke. 2007. "Selection and Moral Hazard in the Reverse Mortgage Market." Mimeo, University of British Columbia.

21. Davis, W. Lukas. 2008. "The Effect of Driving Restrictions on Air Quality in Mexico City." *Journal of Political Economy* 116(1): 38–81.

22. Deaton, Angus. 1991. "Saving and Liquidity Constraints." *Econometrica* 59(5): 1221–1248.

23. Dynarski, M. Susan. "Does Aid Matter? Measuring the Effect of Student Aid on College Attendance and Completion." *American Economic Review* 93(1): 297–288.

24. Einav, Liran, and Amy Finkelstein. 2011. "Selection in Insurance Markets: Theory and Empirics in Pictures." *Journal of Economic Perspectives* 25(1): 115–138.

25. Einav, Liran, Amy Finkelstein, and Jonathan Levin. 2010. "Beyond Testing: Empirical Models of Insurance Markets." *Annual Review of Economics* 2: 311–36.

26. Einav, Liran, Amy Finkelstein, Stephen P. Ryan, Paul Schrimpf, and Mark R. Cullen. 2013. "Selection on Moral Hazard in Health Insurance." *American Economic Review* 103(1): 178–219.

27. Escanciano, Juan C., Bernard Salanié, and Nesse Yildiz. 2016. "Testing for Moral Hazard When Adverse Selection is Present." Mimeo, Columbia University.

28. Fang, Hanming, Michael Keane, and Dan Silverman. 2008. "Sources of Advantageous Selection: Evidence from the Medigap Insurance Market." *Journal of Political Economy* 116(2): 303–350.

29. Finkelstein, Amy, and Kathleen McGarry. 2006. "Private Information and Its Effect on Market Equilibrium: New Evidence from Long-term Care Insurance." *American Economic Review* 96(4): 938–958.

30. Finkelstein, Amy, and James Poterba. 2002. "Selection Effects in the Market for Individual Annuities: New Evidence from the United Kingdom." *Economic Journal* 112(476): 28–50.

31. Gerfin, Michael and Martin Schellhorn. 2006. "Nonparametric Bounds on the Effect of Deductibles in Health Care Insurance on Doctor Visits–Swiss Evidence." *Health Economics* 15 (9):1011–1020.

32. Ghana Statistical Service (GSS). 2014. "Ghana Living Standards Survey Round 6" Main Report, GSS Publications.

33. Ghosh, Parikshit, Dilip Mookherjee, and Debraj Ray. 2000. "Credit Rationing in Developing Countries: An Overview of the Theory." in Dilip Mookherjee and Debraj Ray (eds.) *A Reader in Development Economics*, London: Blackwell.

34. Gine, Xavier and Dean Yang. 2009. "Insurance, Credit, and Technology Adoption: Field Experimental Evidence from Malawi." *Journal of Development Economics* 89(1): 1-11.

35. Greenstone, Michael. 2004. "Did the Clean Air Act cause the Remarkable Decline in Sulfur Dioxide Concentrations?" *Journal of Environmental Economics and Management* 47(3): 585–611.

36. Gross, Tal, Matthew J. Notowidigdo, and Jialan Wang. 2014. "Liquidity Constraints and Consumer Bankruptcy: Evidence from Tax Rebates." *Review of Economics and Statistics* 96(3): 431–443.

37. Hansman, Christopher. 2017. "Asymmetric Information and the Link Between Leverage and Mortgage Default." Mimeo, Columbia University.

38. Hendren, Nathaniel. 2013. "Private Information and Insurance Rejections." *Econometrica* 81(5): 1713–176.

39. Holmstrom, Bengt. 1979. "Moral Hazard and Observability." *Bell Journal of Economics* 10(1): 74–91.

40. Imbens, Guido W., and Thomas Lemieux. 2008. "Regression Discontinuity Designs: A Guide to Practice." *Journal of Econometrics* 142 (2): 615–635.

41. International Monetary Fund (IMF). 2012. "Brazil: 2012 Article IV Consultation: Staff Report; Public Information Notice on the Executive Board Discussion; and Statement by the Executive Director for Brazil." Accessed July 25, 2016. https://www.imf.org/external/pubs/ca

42. Karlan, Dean, Robert Osei, Isaac Osei-Akoto, and Christopher Udry. 2014. "Agricultural Decisions after Relaxing Credit and Risk Constraints." *Quarterly Journal of Economics* 129(2): 597–652.

43. Kim, Meeroo. 2017. "Multidimensional Heterogeneity in the Consumer Credit Market." Mimeo, Columbia University.

44. Krueger, Alan B., and Bruce D. Meyer. 2002. "Labor Supply Effects of Social Insurance," in Alan J. Auerbach and Martin S Feldstein, (eds.), *Handbook of Public Economics*, pp. 2327–2392. Amsterdam and New York: Elsevier.

45. Lee, David S. 2002. "Trimming for Bounds on Treatment Effects with Missing Outcomes." National Bureau of Economic Research Research Working Paper 277.

46. Lee, David S., and Thomas Lemieux. 2010. "Regression Discontinuity Designs in Economics." *Journal of Economic Literature* 48(1): 281–255.

47. Lee, Jeong-Joon, and Yasuyuki Sawada. 2010. "Precautionary Saving under Liquidity Constraints: Evidence from Rural Pakistan." *Journal of Development Economics* 91(1): 77–86.

48. Liu, Yanyan, Kevin Chen, Ruth Hill, and Chengwei Xiao. 2016. "Delayed Premium Payment, Insurance Adoption, and Household Investment in Rural China." IFPRI Discussion Paper (01306).

49. Manski, Charles F. 1990. "Nonparametric Bounds on Treatment Effects." *American Economic Review* 80(2): 319–323.

50. Manski, Charles F. 1997. "Monotone Treatment Response." *Econometrica* 65(6): 1311–1334.

51. Manski, Charles F., and John Pepper V. 2000. "Monotone Instrumental Variables: With an Application to the Returns to Schooling." *Econometrica* 68(4): 997–1010.

52. National Insurance Commission (NIC). 2011. "Annual Report" Ghana, NIC Publications.

53. National Insurance Commission (NIC). 2015. "About NIC." Accessed February 7, 2015. http://www.nicgh.org/live/en/

54. Rothschild, Michael and Joseph Stiglitz. 1976. "Equilibrium in Competitive Insurance Markets: An Essay on the Economics of Imperfect Information." *Quarterly Journal of Economics* 90(4): 629–649.

55. Salanié, Bernard. 2005. "The Economics of Contracts: A Primer." 2nd Edition. MIT Press.

56. Schneider, Henry S. 2010. "Moral Hazard in Leasing Contracts: Evidence from the New York City Taxi Industry" *Journal of Law and Economics* 53(4): 783–805.

57. Townsend, Robert M. 1979. "Optimal Contracts and Competitive Markets with Costly State Verification." *Journal of Economic Theory* 21(2): 265–293.

2.10 Appendix

A.1 Supplementary results: estimation & zero-interest credit for premiums

A.1.1 Firms: zero-interest credit for premium

I illustrate that a zero-interest rate on insurance premium is a possible outcome in equilibrium, when premiums are regulated. Consider two competing profit maximizing firms (i, j). Let τ denote the interest rate on the premium's credit. Firms (i, j) are faced with following per-unit demand functions

$$D_i = a - p_i + p_j$$

$$D_j = a - p_j + p_i$$

No price differentiation is allowed. The firms have two price instruments at their disposal: $(p_k; \tau_k)$, $k = i, j$. The loss in revenue for providing insurance on credit is simply $-\tau_k p_k$ and the firms have (independent) constant costs $c(D_i) = c(D_j) = c(D)$.

Program: Since the premium is fixed $p_i = p_j = \boldsymbol{p}$, firms influence premiums by giving away credit as they compete. In particular, the firms choose (τ_i, τ_k) individually and simultaneously (apply Bertrand strategies). Firm i's (similarly j's) objective function is given by

$$\begin{aligned}\pi_i &= (1+\tau_i)p_i[-(1+\tau_i)p_i + (1+\tau_j)p_j)] - \tau_i p_i - c(D) \\ &\equiv (1+\tau_i)\boldsymbol{p}[-(1+\tau_i)\boldsymbol{p} + (1+\tau_j)\boldsymbol{p})] - \tau_i \boldsymbol{p} - c(D)\end{aligned}$$

where the second line uses the fact that the premium is given and fixed. The FOCs (with respect to τ_k) yield the following best-reply functions

$$\tau_k(\tau_{k'}) = \frac{a + (1+\tau_{k'})\boldsymbol{p} - 2\boldsymbol{p} - 1}{2\boldsymbol{p}}$$

Solving the best-reply functions yields the equilibrium interest rate: $\tau_k^{\boldsymbol{EQB}} = \max(0, \frac{a}{\boldsymbol{p}} - \frac{\boldsymbol{p}+1}{\boldsymbol{p}})$. For certain parameter values of a and $\boldsymbol{p}$ it is possible to have an equilibrium interest rate that is zero (or negative). For instance, such outcome is trivially achieved when $a \in \{0, 1\}$. In addition, when $\boldsymbol{p}$ is really close to a, zero-rate can be achieved. Finally, I note why such a zero-interest rate may coexist with outside credit markets that have higher interest rates: the "credit risk" is much lower in the former. So, higher interest rates from outside channels may reflect their higher default rates, including other reasons (e.g., possibly larger loan sizes, compared to insurance premiums).

A1.2 Estimation illustration

I directly estimate the bounds: $\hat{\Delta}^l$ and $\hat{\Delta}^u$. For example, Y = CLAIMS

$$\hat{\Delta}^l = 51.95\text{GHC} = \begin{cases} \sup_z\{\widehat{\mathbb{E}[DY \mid Z} = z] + \widehat{\mathbb{E}[(1-D})Y \mid Z = z]\} & = 118.71 \\ \inf_z\{\widehat{\mathbb{E}[DY \mid Z} = z] + \widehat{\mathbb{E}[(1-D})Y \mid Z = z]\} & = 66.75 \end{cases}$$

$$\hat{\Delta}^u = 108171.7\text{GHC} = \begin{cases} \inf_z\{\widehat{\mathbb{E}[DY \mid Z} = z] + (1 - \widehat{P(z)})G^u\} & = 108224.83 \\ \sup_z\{\widehat{P(z)}G^l + \widehat{\mathbb{E}[(1-D})Y \mid Z = z]\} & = 52.81 \end{cases}$$

Finally, I bootstrap (nonparametrically) to compute the confidence intervals of $\hat{\Delta}^l$ and $\hat{\Delta}^u$.

A.2 Derivation of proposition 1

I consider the following triangular system

$$Y_i = g(e_i^*(D_i, \alpha_i), \alpha_i^y, \varepsilon_i)$$

$$D_i = \sigma(\alpha_i, Z)$$

This has a direct structural interpretation. $\sigma(.,.)$ the same economic interpretation provided in Section 3.1. In the empirical application the instrument Z should be taken to be policy changes or major events that exogenously induce changes in choice of insurance contracts. The logical indicator D_i equals 1 whenever Y_i is observed; and D_i equals 0 whenever Y_i is not observed, as in the treatment effects or potential outcomes literature. Next, I write the probability of $D_i = 1$ given $Z = z$ as $P(z)$. $P(z)$ is an identified nonparametric index, and captures the insurance probability for individuals with characteristics z. The main object of interest is $\Delta = \mathbb{E}[Y_i(1) - Y_i(0)|Z]$ but this is not identified due to nonrandom selection. The selection problem emanates from the nonrandom assignment of contract choice discussed in the model section. To proceed, I impose the following set of structural restrictions

(**1**) $g(.)$ monotonically decreases in e_i^* for all $(\alpha_i, \varepsilon_i)$

(**2**) Z is independent of $(\alpha_i, \varepsilon_i)$ and Z enters neither $e_i^*(.,.)$ nor $g(.)$

I selectively invoke these restrictions for the identification analysis as needed, in what follows. Restriction **1** is a monotonicity condition, which requires that exerting higher levels of effort will not increase claim outcomes for all consumers i. This is a direct consequence of the Monotone Likelihood Ratio Property MLRP (Holmstrom 1979) in Incentive Theory. The MLRP emerges from the condition required for optimal contract design. In the identification analysis, I employ a slightly weaker version of this which requires it to hold in expectation across only some group of customers; not all i. Next, restriction **2** implies an independence condition $Y_i(d) \perp\!\!\!\perp Z$ for all $d \in \{0, 1\}$. Such condition is commonly referred to as an exclusion restriction: no direct causal effect of Z on Y_i.

The approach I adopt requires the timing of the policy to be uncorrelated with selection and that the *average* distribution of contract choice is affected by the instrument (*i.e.*, relevance: a nonzero $\mathbb{E}[D \mid Z = z]$). The bounds approach is particularly useful because it permits multidimensionality of selection in insurance. Note that, the average moral hazard estimate may be relevant in comparing policies that uniformly assign all insureds to either type of insurance policy. Further discussion of the various effects and their relevance are provided in the paper.

Building on the standard "missing outcomes" representation (Manski and Pepper 2000; Lee 2002) I begin by rewriting the implied average structural functions ASF of the mixed model as $\mu_z(1)$:

$$\begin{aligned}
\equiv \mathbb{E}[Y_i(1) \mid Z = z] &= \mathop{\mathbb{E}}_{D}[\mathbb{E}[Y_i(1) \mid D, Z = z]] \\
&= Pr(D = 1 \mid Z = z)\mathbb{E}[Y_i(1) \mid D = 1, Z = z] + Pr(D = 0 \mid Z = z)\mathbb{E}[Y_i(1) \mid D = 0, Z = z] \\
&= P(z)\mathbb{E}[Y_i(1) \mid D_i = 1, Z = z] + (1 - P(z))\underbrace{\mathbb{E}[Y_i(1) \mid D_i = 0, Z = z]}_{\text{not identified}} \\
&= P(z)\mathbb{E}[g(e_i^*(1, \alpha_i), \alpha_i^y, \varepsilon_i) \mid D_i = 1, Z = z] + (1 - P(z))\mathbb{E}[g(e_i^*(1, \alpha_i), \alpha_i^y, \varepsilon_i) \mid D_i = 0, Z = z]
\end{aligned}$$

and $\mu_z(0)$:

$$\begin{aligned}
\equiv \mathbb{E}[Y_i(0) \mid Z = z] &= P(z)\underbrace{\mathbb{E}[Y_i(0) \mid D_i = 1, Z = z]}_{\text{not identified}} + (1 - P(z))\mathbb{E}[Y_i(0) \mid D_i = 0, Z = z] \\
&= P(z)\mathbb{E}[g(e_i^*(0, \alpha_i), \alpha_i^y, \varepsilon_i) \mid D_i = 1, Z = z] + (1 - P(z))\mathbb{E}[g(e_i^*(0, \alpha_i), \alpha_i^y, \varepsilon_i) \mid D_i = 0, Z = z]
\end{aligned}$$

Notice that because $Y_i = Y_i(1)$ whenever $D_i = 1$, I can write

$$\mathbb{E}[Y_i(1) \mid D_i = 1, Z = z] = \frac{\mathbb{E}[D_i Y_i \mid Z = z]}{P(z)}$$

Similarly, because $Y_i = Y_i(0)$ whenever $D_i = 0$, I can write

$$\mathbb{E}[Y_i(0) \mid D = 0, Z = z] = \frac{\mathbb{E}[(1 - D_i)Y_i \mid Z = z]}{(1 - P(z))}$$

Both $\mathbb{E}[D_i Y_i \mid Z = z]$ and $\mathbb{E}[(1 - D_i)Y_i \mid Z = z]$ are immediately identified from the distribution of the observed data $\{(Y_i, D_i, Z)_i : \ i = 1, ..., I\}$. Particularly, all the terms in $\mu_z(1)$ and $\mu_z(0)$ are identified or known except $\mathbb{E}[Y_i(1) \mid D = 0, Z = z] \equiv \mathbb{E}[g(e_i^*(1, \alpha_i), \alpha_i^y, \varepsilon_i) \mid D = 0, Z = z]$ in $\mu_z(1)$ and $\mathbb{E}[Y_i(0) \mid D = 1, Z = z] \equiv \mathbb{E}[g(e_i^*(0, \alpha_i), \alpha_i^y, \varepsilon_i) \mid D = 1, Z = z]$

in $\mu_z(0)$. Identification therefore hinges crucially on these two unknown terms. These terms are not identified from the distribution of the observed data since one never observes $Y_i(1)$ for consumers with $D_i = 0$ and $Y_i(0)$ for customers with $D_i = 1$ in the data, respectively. The starting point will be Manski's "Worst Case" bounds (Manski 1990). Building on these "Worst Case" bounds, I impose additional restrictions that are governed by agency-theory to provide bounds on the unknown objects of interest.

Worst case bounds of Δ

Suppose that the object $g(.)$ is bounded above and below,

$$G^l \leq g(e_i^*(D_i, \alpha_i), \alpha_i^y, \varepsilon_i) \leq G^u$$

Here G^l and G^u are constant objects and represent the lower and upper bounds on $g(.)$, respectively. In principle, Y_i is bounded within the support $Y_i \in [\underline{y},\ \overline{y}]$, and for all customers i $Y_i(1)$ and $Y_i(0)$ are also bounded within $[\underline{y},\ \overline{y}]$. The condition on $g(.)$ above is therefore equivalent to setting $G^l \equiv \underline{y}$ and $G^u \equiv \overline{y}$.

Worst Case Bounds

Let the quantity $g(.)$ be bounded as stated above. Section III of Manski 1990, and Proposition 1 of Manski and Pepper 2000 can be used to establish Worst Case bounds on $\triangle$ under the set up as

$$\Delta^l = \sup_z\{\mathbb{E}[D_iY_i \mid Z = z] + (1 - P(z))G^l\} - \inf_z\{P(z)G^u + \mathbb{E}[(1 - D_i)Y_i \mid Z = z]\}$$

$$\Delta^u = \inf_z\{\mathbb{E}[D_iY_i \mid Z = z] + (1 - P(z))G^u\} - \sup_z\{P(z)G^l + \mathbb{E}[(1 - D_i)Y_i \mid Z = z]\}$$

where Δ^l and Δ^u denote lower and upper bounds on Δ, respectively. These are the worst case best possible bounds, and without further information the bounds are sharp. In general, this set $\Delta \in [\Delta^l, \Delta^u]$ may be wide and thus not very informative. It is useful to note that these "Worst Case" bounds in themselves do not directly help for the purposes of identifying moral hazard. I impose additional plausible restrictions that are inspired by economic theory to tighten the bounds in the next series of identification analysis. Suppose, for a moment, that one ignores the gains from the intersection of the bounds across all z. Then the implied width of the ATE bounds above is

$$\Delta^u - \Delta^l = G^u - G^l$$

This is derived from substituting for the various objects, and then canceling out identical terms. To further illustrate that the above set is less informative in the asymmetric information context, consider the canonical binary choice model where $Y_i \in \{0, 1\}$. In the empirical analysis, one of the outcome of interest is binary: that is, whether or not an accident (or or loss) occurred. Here, it follows immediately that $G^l = 0$ and $G^u = 1$. Therefore the corresponding lower and upper bounds for the average effect Δ are $\Delta^l = \mathbb{E}[D_i Y_i \mid Z = z] - (P(z) + \mathbb{E}[(1 - D_i)Y_i \mid Z = z])$ and $\Delta^u = \mathbb{E}[D_i Y_i \mid Z = z] + (1 - P(z)) - \mathbb{E}[(1 - D_i)Y_i \mid Z = z]$, respectively. The width of these bounds simplifies to $\Delta^u - \Delta^l = 1$. Here $P(z)$ is simply the insurance choice probability for individuals with characteristics z.

Tightening the bounds of Δ

I investigate the identifying power of certain plausible restrictions. The restriction I impose is governed by the theoretical considerations of agency models and the empirical application process considered in this paper.

Customers' Effort Supply

In a standard mixed adverse selection and moral hazard model of insurance, customers who choose higher coverage contracts are more likely to exert lower levels of effort. As in agency theory, this may in part stem from information and preference asymmetries. Typically, the principal can observe the outcome; but not the action of the customer. Notwithstanding, the actions and/or efforts of the customer can be monitored in theory; but in practice obtaining complete information could be prohibitively expensive: "costly verification" (Townsend 1979)[47]. Next, customer's preferences (e.g. risk aversion) may differ from that of the insurer, and so to the extent that the actions of the customer that may be considered beneficial to the insurer could be costly to the customer, it is likely the consumer may under supply his level of effort: "un-aligned preferences". To this end, I formally impose the inequality restriction that for each customer i

$$e_i^*(1, \alpha_i) \leq e_i^*(0, \alpha_i)$$

This implies that customers that select higher coverage contracts or buy insurance will not increase their supply of effort e.g, via seat-belting or any implied precautionary action in the automobile insurance context. Combining this with structural restriction **2**, I have that for all customers i

$$Y_i(1) \geq Y_i(0)$$
$$g(e_i^*(1, \alpha_i), \alpha_i^y, \varepsilon_i) \geq g(e_i^*(0, \alpha_i), \alpha_i^y, \varepsilon_i)$$

Notice that the agency-theory restriction consequently yields a version of the usual monotone treatment response MTR condition (Manski 1997). That is, choosing a higher coverage

[47]In this case, the principal may wish to charge more premium to embark on more verification. This, however, is unlikely to hold. For example, in the empirical setting, insurers have little or no room to adjust insurance prices. The market including premium setting is highly regulated and controlled by the government, where insurance companies are required to follow a proposed premium formula in selling contracts. The empirics provide suggestive evidence of price rigidity: firms did not quickly adjust prices following the introduction of reform.

contract will not increase customer's outcome. It is also straightforward to see that the above condition will restrict the sign of the average effect. In the identification analysis, however, I use a much weaker version of the condition

$$\mathbb{E}[Y_i(1) \mid D_i = d, Z = z] \geq \mathbb{E}[Y_i(0) \mid D_i = d, Z = z]$$

for all $z \in \mathbb{Z}$ and $d \in \{0, 1\}$. To illustrate, let $d = 0$, then this condition says $\mathbb{E}[Y_i(1) \mid D_i = 0, Z = z] \geq \mathbb{E}[Y_i(0) \mid D_i = 0, Z = z]$. Similarly for $d = 1$, $\mathbb{E}[Y_i(1) \mid D_i = 1, Z = z] \geq \mathbb{E}[Y_i(0) \mid D_i = 1, Z = z]$. The use of this condition is motivated by the following. First, the original restriction is stronger because it must hold for all the customers i. The latter only need it to hold in expectation across some group of consumers. Identified bounds on the objects of interest using the weaker restriction actually coincides with that of the stronger MTR restriction. This can be viewed as an improvement given that weaker restrictions are generally preferred, and easier to rationalize in practice. Next, because I am interested in identifying the average moral hazard, the weaker condition is sufficient. Under this weaker condition, the bounds on the unknown objects are

$$\mathbb{E}[Y_i(1) \mid D_i = 0, Z = z] \in \left[\frac{\mathbb{E}[(1 - D_i)Y_i \mid Z = z]}{(1 - P(z))},\ G^u \right]$$
$$\mathbb{E}[Y_i(0) \mid D_i = 1, Z = z] \in \left[G^l, \frac{\mathbb{E}[D_i Y_i \mid Z = z]}{P(z)} \right]$$

for all $z \in \mathbb{Z}$. Note that the identified bounds for the unknowns above must hold for all $z \in \mathbb{Z}$. This can be viewed as a consequence of restrictions **2**. I can therefore intersect the bounds across all the possible values that z can take. The implied bounds on the quantities $(\mu_z(1), \mu_z(0))$ become

$$\sup_z\{\mathbb{E}[D_i Y_i \mid Z = z] + \mathbb{E}[(1 - D_i)Y_i \mid Z = z]\} \leq \mu_z(1) \leq \inf_z\{\mathbb{E}[D_i Y_i \mid Z = z] + (1 - P(z))G^u\}$$
$$\sup_z\{P(z)G^l + \mathbb{E}[(1 - D_i)Y_i \mid Z = z]\} \leq \mu_z(0) \leq \inf_z\{\mathbb{E}[D_i Y_i \mid Z = z] + \mathbb{E}[(1 - D_i)Y_i \mid Z = z]\}$$

Next, the resulting best possible bounds on the average treatment effect Δ: the main object of interest are

$$\Delta^l = \sup_z\{\mathbb{E}[D_iY_i \mid Z=z] + \mathbb{E}[(1-D_i)Y_i \mid Z=z]\} - \inf_z\{\mathbb{E}[D_iY_i \mid Z=z] + \mathbb{E}[(1-D_i)Y_i \mid Z=z]\}$$

$$= \sup_z\{\mathbb{E}[Y_i \mid Z=z]\} - \inf_z\{\mathbb{E}[Y_i \mid Z=z]\}$$

$$\Delta^u = \inf_z\{\mathbb{E}[D_iY_i \mid Z=z] + (1-P(z))G^u\} - \sup_z\{P(z)G^l + \mathbb{E}[(1-D_i)Y_i \mid Z=z]\}$$

QED

This uses the assumption that $Y_i(1)$, and $Y_i(0)$ are (conditionally) independent of Z. First, observe that the lower bound Δ^l simplifies to a simple difference estimator. The width $\Delta \in [\Delta^l, \Delta^u]$ is analogously defined, and the expressions further simply under the binary choice model where $G^l = 0$ and $G^u = 1$. Without intersecting the bounds across all z, the ATE lower bound becomes $\Delta^l = 0$. In the empirical analysis, I intersect the resulting bounds across z using the sup and inf operators, which provide informative estimates for Δ^l that are non-zero. Since the inequality restriction provides improvements to the lower bound, Δ^l will be the main focus for the analysis of moral hazard effects.

A.3 Additional Discussions

A.3.1 Why were companies willing to accept credit payments before the reform?

(1) Premium targets Each local insurance office is given a premium target per contract period, so there were clear incentives to push credit to customers. These target levels trickle down to the individual staff.[48]

(2) Existence of intermediaries: insurance agents and brokers. Commission-motivated agents developed personal relationships with their clients and provided insurance on credit.

[48] There is anecdotal evidence that the staff use their family and friends for that purpose. Company workers served as guarantees to spread insurance premiums for their families and friends, since members could not afford to pay all at once, especially for the comprehensive cover. Such phenomenon grew overtime: the sale of insurance on credit was largely overlooked in the companies, even at the top level with no sanctions against the staff who do same.

This was not considered a challenge to the companies since the intermediaries have a better incentive to collect premium debts: most insurance companies would not pay full commissions due the agents and brokers until the premiums are paid.

(3) Client-centric and the norm of keeping business In the past, government organizations were are among the top insurance clients. However, funding from the government is usually delayed and so due to their size in the customer space, the provision of insurance on credit to such institutions was deemed a way of keeping the business of insurance firms. The insurance companies assumed that government debts will eventually be paid no matter how long it takes, further promoting the sale of contracts on credit with recent extensions to individual customers.

A.3.2 Recovering claims for basic-liability contracts

From the insurer's data, I cannot directly use the observed claim outcomes under $D_{it} = 0$, basic contracts. That is, the data at hand do not allow for direct comparison of the outcomes under treatment status $D_{it} = 1$ versus $D_{it} = 0$, particularly for claims. The reason is that the insurer's claim dataset reflects liabilities to both own and other parties damages under the comprehensive insurance, but it excludes the liability to own damages under the basic insurance. Estimates will clearly be biased upward if this is ignored. I approach this in two ways:

First, I follow an indirect approach due to Chiappori et al. (2006) to circumvent this challenge. To illustrate, denote by $\bar{Y}_{it0}$ the observed claims in the insurer's dataset (which excludes the liabilities to customer i's own damages) and Y_{it0} the true counterfactual claims under $D_{it} = 0$. The solution is to assume that the distribution of Y_{it0} conditional on $\bar{Y}_{it0}$ depends only on customer i's observed vector of characteristics, X_{it}. Under this assumption, one can use the observed claims distribution on $D_{it} = 1$, comprehensive contracts for observationally similar customers to recover that of Y_{it0}.[49] In practice, I construct a customer level index or score based on the observed characteristics and outcomes. Next, I define the notion of "similarity" to be customers that have the closest scores. These are then matched accordingly. This approach is stringent as exemplified by: for $D_{it} = 0$ (i) average claim amount is GHC55.8 compared to a raw amount of about zero; (ii) average loss occurrence is 0.037 compared to about a zero rate initially. Note that the claim and loss occurrence information for contract $D_{it} = 1$ remain unchanged.

Second, as a robustness check, I analyze moral hazard for claim events that are only covered under both contracts ignoring the above imputation. These are third-party events that both comprehensive and basic-liability contracts cover and are directly available for analysing moral hazard.

A.4 Future work: preliminary results

A.4.2 Co-environmental benefits: did the policy led to lower vehicle emissions, PM 2.5?

Data: I draw on high resolution satellite database from National Aeronautics and Space Administration (NASA)–MERRA-v2. This is a global reanalysis database that assimilates space-based observations of aerosols and represent their interactions with other physical processes in the climate system. MERRA-v2 begins in 1980 with spatial resolution of 50km in latitude direction. Particulate Matter PM 2.5 (kg/m^3), wind (m/s), temperature, and humidity for the entire country were extracted, and then aggregated to the district level i.

[49] An important feature about this approach is that it is more stringent and thus should go against the moral hazard results. The imputation is done for $D_{it} = 0$ by borrowing information from the distribution of $D_{it} = 1$ claims. Chiappori et al. (2006) provides additional details.

I link discontinuity in PM 2.5 to the policy as follows:

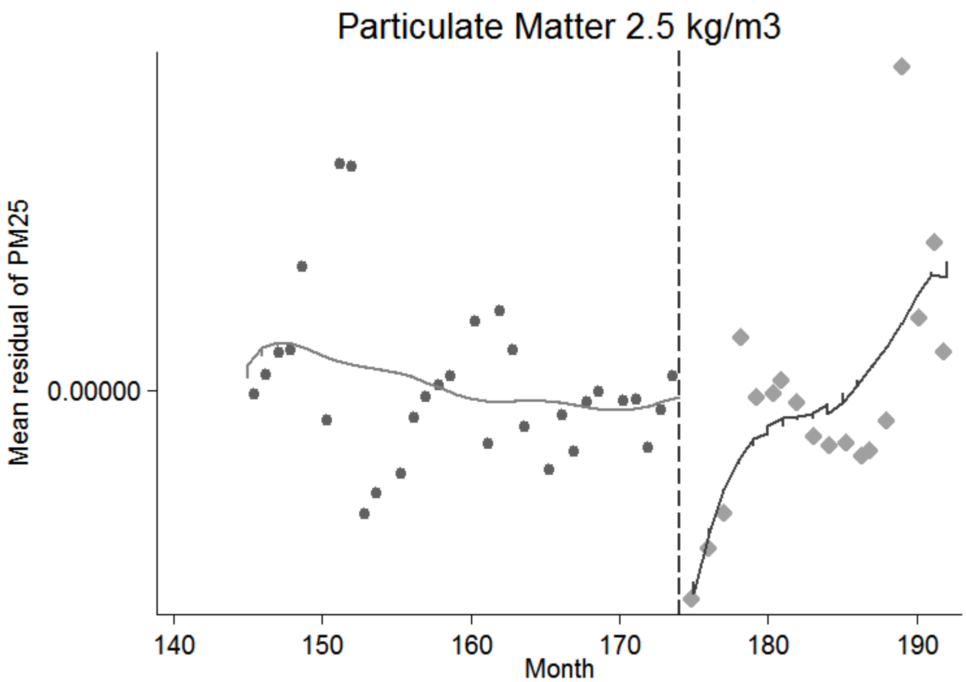
$$\text{PM2.5}_{it} = \gamma_0 + \gamma_1 \text{Policy}_t + \gamma_2 \mathbf{X}_{it} + \epsilon_{it}$$

Where $\text{Policy}_t = \mathbf{1}[\text{Date} > \text{April 2014}]$; $\mathbf{X}_{it}$ includes three weather control variables in month t (wind speed, temperature and humidity), month of year (MOY) dummies, and district-level dummies. The results are shown in Figure A5.

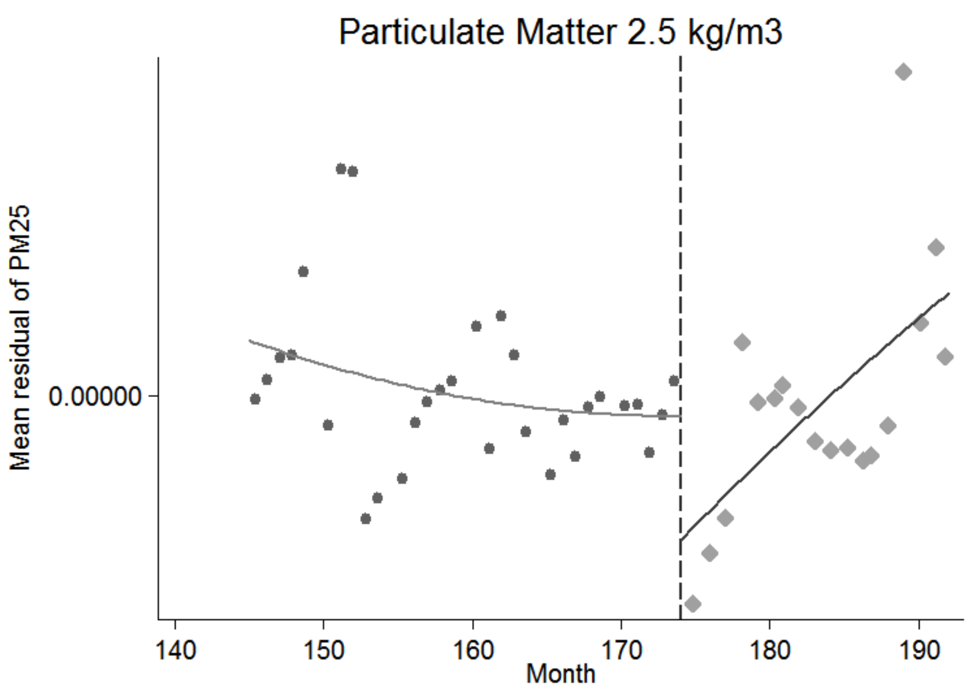
A.4.1 Selection: do the switchers signal as bad risk-types?

From the perspective of insurance firms, the switchers (consumers who bought insurance on credit) could be identical to the non-switchers based on the observable characteristics of consumers. Does this hold for unobservables? In contrast, results in Table A5 and Figure A6 indicate that the switchers signal as bad risk-types: residual claims (unobserved) are systematically worse for the switchers, compared to the other various categories of consumers.

Figure 2.23: Discontinuity in PM 2.5 at Policy Cut-off



(a) **RESULTS FOR LOCAL LINEAR REGRESSION**



(b) **RESULTS FOR QUADRATIC REGRESSION**

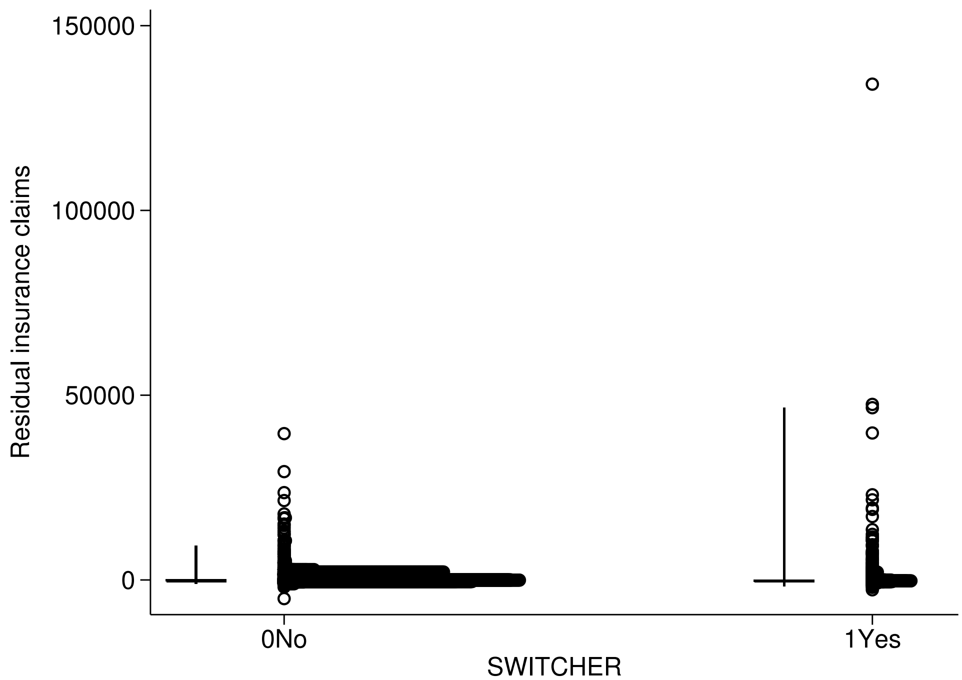
Notes: Figure (a) displays mean residual PM2.5 on each side of the policy-cutoff by month and **local linear regressions** on each side of the cutoff; (b) replicates (a) but for **quadratic regressions** on each side of the cutoff. In both cases, there is suggestive evidence of immediate reduction in PM 2.5. Estimates range between [-1.0 EXP-8; -5.7 EXP-9]**.

Table 2.15: Conditional Distribution of Residual Claims

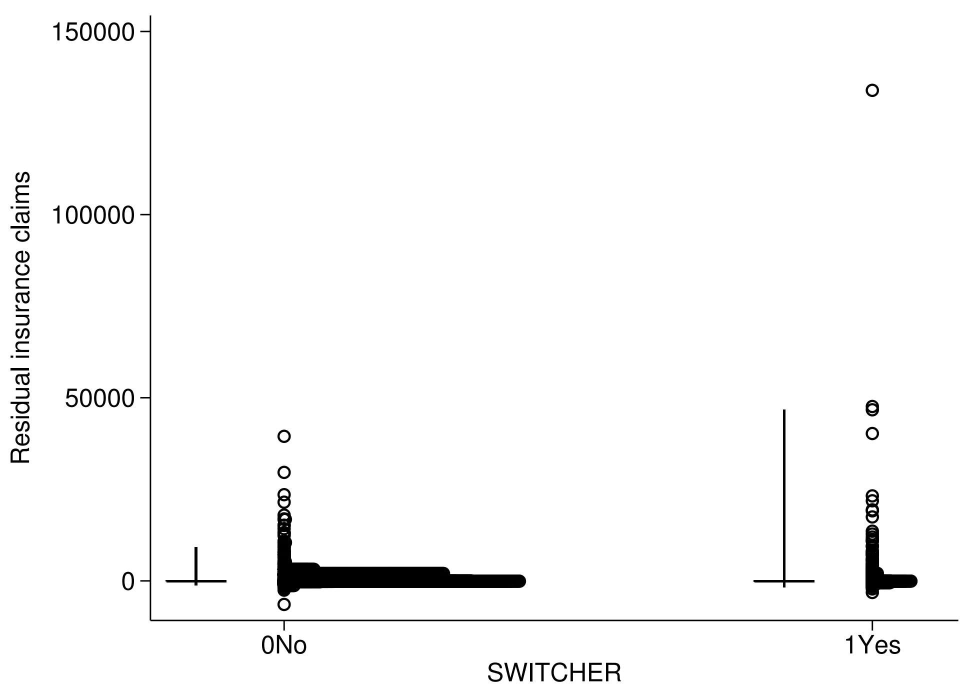
Consumer Type	Mean	25th PCT	50th PCT	75th PCT	Skewness	Regime
Always Comprehensive	-141.18	-344.95	-232.56	-70.27	20.21	PRE-POLICY
Switchers (Credit-customers)	-56.91	-429.38	-318.85	-175.07	28.96	
Always Basic-liability	-1.98	-89.45	-59.78	-34.11	21.56	POST-POLICY
Switchers (Credit-customers)	128.89	-188.32	-59.06	-18.57	28.91	

Notes: Table shows the distribution of residual claims (unobserved) for two regimes. In the top panel, the distribution of customers who always buy comprehensive contracts is compared with that of those who bought insurance on credit, pre-policy. In the bottom panel, the distribution of customers who always buy basic-liability contracts is compared with those who bought insurance on credit, post-policy. There is evidence of worse claim outcomes for switchers (much right-skewed) in all cases. Together, this evidence suggestive that the switchers are generally bad risk-types. Bundling credit with insurance appears to attract bad risk customers.

Figure 2.24: Stripplot Showing Distribution of Residual Claims



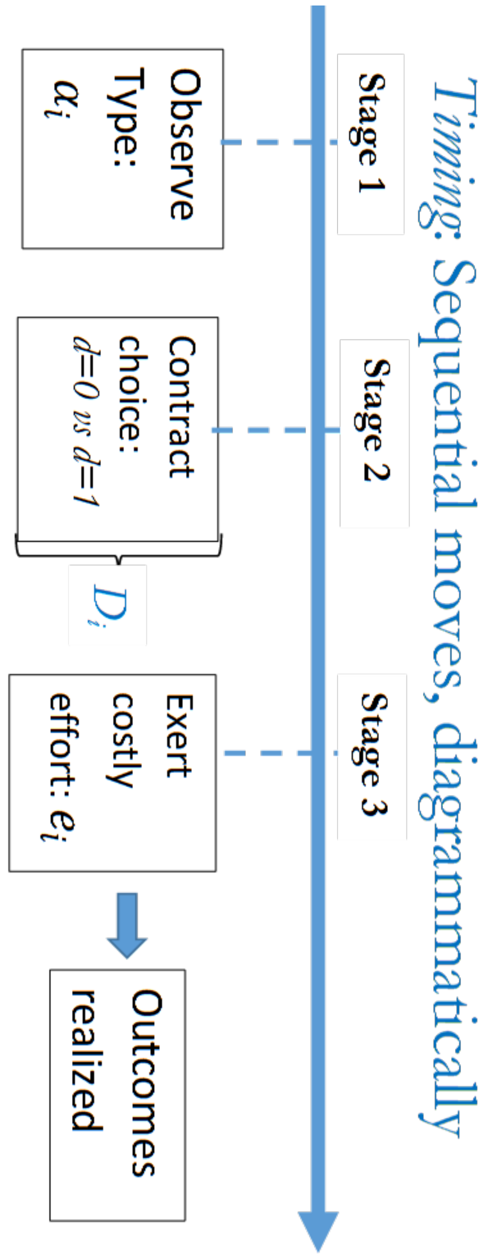
(a) **ALWAYS COMPREHENSIVE CUSTOMERS VS. SWITCHERS**



(b) **ALWAYS BASIC-LIABILITY CUSTOMERS VS. SWITCHERS**

A.5 Additional results

Figure 2.25: The Model's Timing



Notes: Figure shows the timing of the mixed-economic model; illustrating the interplay between multi-dimensional selection and moral hazard. Contract choice depends on selection. In turn, the optimal choice of effort depends on contract choice and selection attributes.

Table 2.16: Heterogeneity in Moral Hazard (MHE)

	LOSS				**LOSS**		
Certificate Type	–Bounds–		95% CI	***Discount Quartile***	–Bounds–		95% CI
Private	*lb*	0.007	[0.0001,0.01]	q_1	*lb*	0.01	[0.0025,0.0271]
Commercial	*lb*	0.17	[0.007,0.27]	q_2	*lb*	0.005	[0.0001,0.0078]
				q_3	*lb*	0.0025	[0.0025,0.0151]
				q_4	*lb*	0.004	[0.0028,0.0225]

Notes: Table shows the lower bound on moral effects across customer business class (private versus commercial) and quartiles for premium discounts. CI denotes confidence interval. lb denotes lower bound on moral hazard. q_1, q_2, q_3 and q_4 correspond to the first, second, third and fourth quartiles of premium discounts respectively. The 95% confidence intervals are based on 999 nonparametric bootstrap resamples for the various objects of interest.

Figure 2.26: L_2-Type — Moral Hazard Test

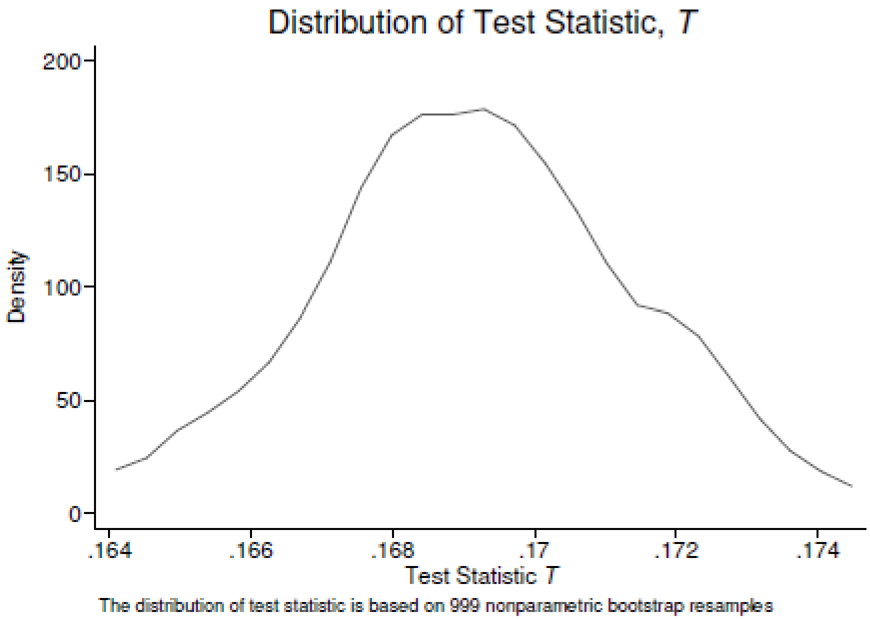


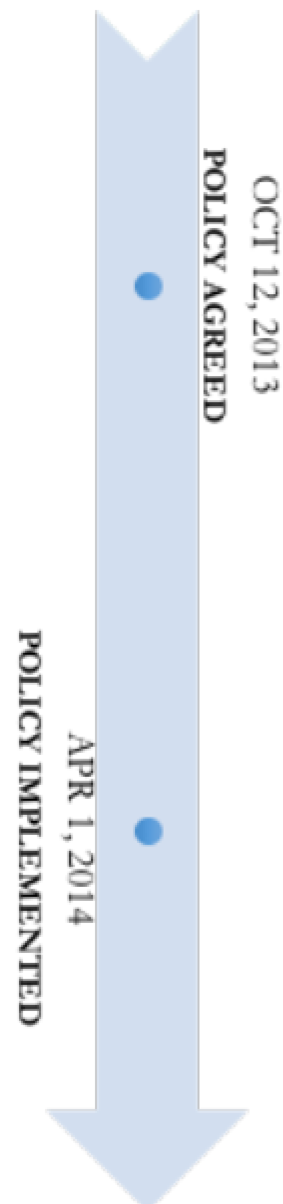
Figure 2.27: Gasoline Prices in Ghana

Distributional moments: gas prices

	Mean (US$/$L$)	SD
Pre-reform	1.06	1.04
Post-reform	1.02	1.03

Notes: Figure shows the gasoline prices in Ghana over the relevant period: March 2013 to March 2015. The figure is due Trading Economics (http://www.tradingeconomics.com/ghana/gasoline-prices); which is in turn based on data reported by the National Petroleum Authority NPA. As shown, the average and standard deviation of gasoline prices before the April 2014 reform is 1.06 US$/L and 1.04 respectively; average and standard deviation of prices after the reform are 1.02 US$/L and 1.03, similarly. While there are a few observable fluctuations in gasoline pump prices across various months, the distribution of prices before and after the April 2014 remained fairly stable, as depicted by the distributional moments. Existing national support programs intended to safeguard consumers and the public against general price shocks may explain the stability in price distributions.

Figure 2.28: Timelines of Policy



Notes: Figure displays the timelines regarding the policy reform. The NIC agreed on the policy on October 12, 2013. The implementation/announcement of regulation took place on April 1, 2014; suggesting an implementation lag of about 7 months.

Table 2.17: Data Summaries

Selected Variables	Mean	Min	Max
Claim amount (GHC)	99.54	0	135,588.9
$\mathbf{1}[Loss = Yes]$	0.04	0	1
$\mathbf{1}[Credit = Yes]$	0.27	0	1
Year of manufacture	2001.15	1957	2015
Cubic capacity (cm^3)	2477.973	1.4	119467.0
Seat capacity	5.228222	1	56
NCD level	4.068628	0	11
Loading	76.97456	-44.26	13,803.26
Premium (GHC)	440.2559	0.01	69,029.25
Contract dates	—	01apr2013	31mar2015

Notes: 27% of customers purchased insurance on credit prior to 2014 policy reform. The number of policies (observations) are the end of the sample period is 31,877.

Table 2.18: Summaries–1

Pre-reform	Mean	Min	Max
Claim amount (GHC)	112.87	0	47,385.1
$\mathbf{1}[Loss = Yes]$	0.041	0	1
$\mathbf{1}[Credit = Yes]$	0.27	0	1
Year of manufacture	2001.26	1957	2014
Cubic capacity (cm^3)	2519.36	1.6	99,999.0
Seat capacity	5.23	1	56
NCD level	4.32	0	11
Loading	93.86	0	13,803.26
Premium (GHC)	538.36	0.01	69,029.25
Contract dates	—	01apr2013	01apr2014

Table 2.19: Summaries–2

Post-reform	Mean	Min	Max
Claim amount (GHC)	92.41	0	135,588.9
$\mathbf{1}[Loss = Yes]$	0.038	0	1
$\mathbf{1}[Credit = Yes]$	—	—	—
Year of manufacture	2001.09	1957	2015
Cubic capacity (cm^3)	2455.85	1.4	119,467.0
Seat capacity	5.22	1	56
NCD level	3.93	0	11
Loading	67.94	-44.26	5,851.54
Premium (GHC)	387.77	0.17	23,519.06
Contract dates	—	01apr2014	31mar2015

Chapter 3

Harmattan Winds, Disease and Gender Gaps in Human Capital Investment

Francis Annan♣
(joint with Belinda Archibong♠)

Abstract*

Persistent gender gaps in educational attainment have been examined in the context of differential parental costs of investment in the education of boys versus girls. This paper examines whether disease burdens, especially prevalent in the tropics, contribute significantly to widening gender gaps in educational attainments. We estimate the impact of sudden exposure to the 1986 meningitis epidemic in Niger on girls' education relative to boys. Our results suggest that increases in meningitis cases during epidemic years significantly reduce years of education disproportionately for school-aged going girls in areas with higher meningitis exposure. There is no significant effect for boys in the same cohort and no effects of meningitis exposure for non-epidemic years. We use theory to explore different channels, highlighting income effects of epidemics on households and early marriage of girls in areas with higher exposure during epidemic years. We also use National Aeronautics and Space Administration (NASA) data to investigate the relationship between climate variables and the meningitis epidemic and explore how climate change could potentially worsen social inequality through widening the gender gap in human capital investment. Our findings have broader implications for climate-induced disease effects on social inequality.

JEL Classification Codes: I12, I14, I24, J16, J24, O12, O15, Q54

Keywords: *Environmental Risks, Education, Meningitis, Health, Gender Gap, Harmattan, Climate Change, Marriage, Bride-Price*

∗ Thanks to Douglas Almond, Rodrigo Soares, Bernard Salanie, Lalith Munasinghe, Linda Loubert and participants at the 2017 AEA and EEA meetings, Barnard Center for Research on Women (BCRW) and Columbia University Sustainable Development seminar for useful comments and suggestions. We are grateful to Carlos Perez, Madeleine Thomson, Nita Bharti, the Ministry of Public Health in Niger and World Health Organization (WHO) for the data on meningitis used in this study. Errors are our own.
♠ Barnard College. Email: ba2207@columbia.edu
♣ Columbia University. Email: fa2316@columbia.edu

3.1 Introduction

> "*In my community work I soon learned more about the barriers for girls in school. If families are going through a financial rough patch, they're more likely to pay fees for boys rather than for girls. If girls drop out of school, the family is eager to marry them off rather than have them sit around the house all day.*" - **Natasha Annie Tonthhola, BBC News**

There is a vast literature on the positive economic impacts of investment in education Becker et al. 1990. In developing countries, where notable gender gaps in educational attainment still remain, the potential economic gains from educating girls are significant (Schultz 2002; Barro and Lee 2013). Though gaps in primary school enrollment have been closing, largely due to national policies promoting free primary education, gaps in educational attainment still remain, partly driven by lower primary completion rates and lower secondary school enrollment rates for girls relative to boys in poorer countries concentrated in Africa and Asia[1]. Some of the reasons given for this persistent gap and associated lower investment of parents in female versus male children have been direct costs related to school fees and opportunity costs related to early marriage of girls, foregone earnings of girls' labor, and gendered expectations of the division of household labor, with girls expected to care for younger siblings and contribute disproportionately to other unpaid domestic work (Schultz 2002; Hartmann-Mahmud 2011).

Another strand of literature has examined the relationship between health shocks and investment in human capital with findings showing a negative relationship between disease/mortality rates and investments in education (Miguel and Kremer 2004; Almond 2006; Glewwe and Miguel 2007; Jayachandran and Lleras-Muney 2009). However, the literature has been thin in understanding how health shocks and disease burdens contribute to differences in educational attainment and investment in the human capital of girls relative to

[1]Source: OECD "Closing the Gender Gap" report

boys (Glewwe and Miguel 2007). Estimating the contribution of health shocks to differential human capital investment by gender is especially important for developing countries in Africa and Asia where the combination of notable gender gaps in educational attainment and higher disease burdens in the tropics can impose a double cost for economic development.

This paper's main contribution is to estimate the effect of health shocks on the gender gap in educational attainment by exploiting a quasi-experiment, the 1986 meningitis epidemic in Niger, following previous work in Archibong and Annan (2017). We estimate a difference-in-differences model, interacting an indicator for gender with a continuous cohort-based measure of meningitis exposure during the 1986 epidemic. We find that higher meningitis exposure during the epidemic reduced years of education for school-going aged girls at the time of the epidemic. Interestingly, there is no significant difference in the education of boys exposed to higher or lower meningitis incidence during the epidemic. These results have important implications: first, health shocks disproportionately impact investment in girls' education with direct and opportunity costs of investing in girls' education potentially higher during shocks. Second, a focus on improving attainment through free, mandatory primary education programs means that most of the investment in the education of girls will occur at the primary level in poorer countries. So disease shocks will have disproportionate effects on primary school aged girls, decreasing the likelihood of primary school completion and resulting in lower attainment for girls relative to boys. Third, our findings highlight the need for policies targeting both health and education concurrently to close the gap in educational attainment and maximize economic returns from the associated gains in human capital investment, particularly for poorer countries located in higher disease burden areas in the tropics.

Another contribution of the paper[2] is to highlight the mechanisms through which health shocks might affect gender gaps in human capital investment. We use theory to explore different explanations for the results, citing direct and indirect channels through which epi-

[2]Going beyond work presented in Archibong and Annan (2017).

demics might affect gender gaps in educational attainment (Bjorkman-Nyqvist 2013; Islam and Maitra 2012; Jayachandran and Lleras-Muney 2009). We explore direct (through health and mortality) and indirect (through income and consumption) effects of meningitis epidemics and provide evidence for the primacy of indirect channels here. Specifically, we show evidence for higher rates of early marriage of girls in districts with higher meningitis exposure during epidemic years. Our results lend support to the health shock as negative income shock channel highlighted in the literature, with girls being “sold" by households for bride price transfers and to reduce the consumption burden on the household during epidemic years (Islam and Maitra 2012; Corno, Voena et al. 2015; Corno et al. 2016; Loaiza Sr and Wong 2012). We also show evidence, supported by a vast literature, for a robust positive association between the age at first marriage and educational attainment for girls (Ashraf et al., 2016).

Finally, given the growing evidence on the social and economic impacts of climate, a third contribution of the paper is to investigate the linkages between the Harmattan season and meningitis outbreaks to explore the potential implications of Harmattan and associated climate variables on the observed gender gap in human capital investment. Previous work has documented the relationship between yearly variability in meningitis outbreaks and relevant climate variables during the most intense part of the dry season, the Harmattan, from October to December across sub-Saharan Africa (Garcia-Pando et al. 2014; Perez Garcia Pando et al. 2014; Yaka et al. 2008). We use an instrumental variable approach to link educational attainment to harmattan-induced meningitis outbreaks. The IV results provide further support for our OLS findings.

We conduct a number of robustness checks to validate our results, with the results robust to alternate specifications of meningitis exposure and placebo testing with unaffected cohorts. A potential concern for our proposal of the indirect economic channel as the main mechanism at work, is the lack of data on mortality rates by gender that would allow us to test for any differential biological effects of meningitis by gender. We refer to the health literature on meningitis impacts as evidence against the direct biological mechanism as the main channel here.

The paper is organized as follows. Section 2 outlines the theoretical predictions that we test in the data. Section 3 provides background on the 1986 meningitis epidemic in Niger. Section 4 describes the data. Section 5 outlines our empirical specification, and Section 6 provides quantitative estimates on the impacts of the epidemic on the gender gap in human capital. Section 7 discusses the potential effect of the Harmattan season on the occurrence and incidence of meningitis outbreak for the following year. Section 8 explores direct and indirect channels, examines the impact of the epidemic on early marriage of girls and evaluates alternative explanations for the results. Section 9 concludes.

3.2 Conceptual Framework

This paper tests the hypothesis that aggregate health shocks can have differential impacts on male and female human capital investment choices and outcomes. There are two primary channels through which health can differentially affect human capital, broadly categorized as direct, through health and biology, and indirect channels, through economic impacts on households. Through the direct channel, a health shock like a meningitis epidemic can have different biological effects on male and female infected persons. If, for instance, girls are biologically more likely to die from meningitis, then the evidence could show lower years of education during the epidemic year for girls relative to their male counterpart (Janghorbani et al. 1993; Sen 1998; Jayachandran and Lleras-Muney 2009). Another way the direct health channel could operate is if there are differential effects by gender on cognitive development from the disease, resulting in lowered educational attainment for girls relative to boys (Almond, Edlund, and Palme 2009).

Through the indirect channel, a health shock like a meningitis epidemic has income effects on the household. The household is modeled as a unitary household with liquidity and credit constraints and the health shock acts as a negative income shock for the household, raising health expenditures, resulting in missed work days/foregone income and raising the costs of domestic care for sick household members. This leads the household to attempt to smooth consumption by reducing expenditure on certain consumption bundles and selling off available assets (Islam and Maitra 2012). In many communities, these "assets" include female children where early marriage of girls can increase in response to a negative income shock in bride price societies where income and wealth transfers are made from the groom's family to the bride's family upon marriage (Corno, Voena et al. 2015; Corno et al. 2016). Corno et al. (2016) outline a model and provide evidence for an increase in early marriages (a reduction in the age at first marriage) in response to income shocks in bride price societies. Lowered age at first marriage is associated with lower educational attainment with girls often dropping out of school or completing less schooling at the time of marriage, and the early marriage channel could then explain a widened gender gap in attainment in response to the meningitis epidemic.

We present a simple framework on the relationship between health shocks and the gender gap in educational attainment as follows. Following the unitary household model, within each family i, parents maximize discounted expected utility over two periods and choose to invest in schooling for girls (denoted s_g) and boys (denoted s_b). In period 1, the child works at home, goes to school or both. In period 2, the child is an adult and works for a wage. The parent's optimization problem is as follows:

$$maxU_i = u(c_1^i) + \delta c_2^i$$

s.t.

$$c_1^i = y_1 - pe_b^i - pe_g^i + \eta_b(1 - s_b^i) + \eta_g(1 - s_g^i)$$

and

$$c_2^i = y_2 + \gamma_b y_b^{ai} + \gamma_g y_g^{ai}$$

where $a^i_s = \alpha^i_s s^i_s$; $s^i_s \in [0,1]$; $y^{ai} = \omega_s a^i_s$ ($\omega_b > \omega_g$ and $\gamma_b > \gamma_g$); $\theta_s = \delta\gamma_s\omega_s$ and $\theta_g < \theta_b$ and c^i_t is the parent i's consumption in period t, u is a concave utility function and δ is a discount factor. a^i_s are cognitive skills with α^i_s denoted as the learning efficiency of a child of sex s in family i and which is assumed to be equal for boys and girls. s^i_s is the fraction of time in period 1 spent in school by a child from family i of sex s and defined over the interval 0,1. y_t is (exogenous) parental income and p is the schooling price for a child. e^i_s is an indicator variable that takes 1 if family i sends a child of sex s to school. $\eta_s(1 - s^i_s)$ is the income provided from home production in period 2 and $\gamma_s y^{ai}_s$ is the share of the child's income transferred to her parents. ω_s is the return to education of a child of sex s. Given simple restrictions on the parameters above and outlined in Bjorkman-Nyqvist (2013), the first order condition for household i, after maximizing the parent's expected utility will be:

$$FOC : -u'(c_1)\eta_s + \alpha^i_s\theta^i_s \leq 0 \quad \text{for} \quad s_s \in [0,1]$$

and parents will choose to invest in schooling for a child up to where the marginal cost of more schooling, in the form of forgone time for domestic production or foregone income from early marriage for girls, is equal to the marginal benefit, in the form of higher transfers from a more educated and subsequently higher paid (using a standard Mincerian model of returns to education) adult. An implication of the Bjorkman-Nyqvist (2013) model is "if both s_b and s_g are greater than 0, a reduction in parental income, y_1, will on the margin only reduce investment in girls' education.

We use data on higher health costs associated with meningitis outbreaks and early marriage of girls to provide suggestive evidence for the indirect income channel as outlined above in this paper.

3.3 1986 Meningitis epidemic in Niger

Niger is located in the so-called 'meningitis belt' that runs across sub-Saharan Africa (SSA), extending from Senegal in the west to Ethiopia in the far east as shown in Figure 1. Over 95%

of the Nigerien population resides in the meningitis belt, which is the less desert ecological region of the country where most epidemics of meningococcal meningitis occur LaForce et al. 2009. The epidemic[3] form of meningitis is caused by the bacterium *Neisseria meningitidis* and infection is associated with fevers, pain, reduced cognitive function, and in the worst cases, permanent disability and long-term neurological damage and death. The epidemiology of the disease is complex and though incidence is often associated with higher wind speeds, dust concentrations and lower temperatures that come with the onset of the dry, Harmattan season in SSA, the mechanisms of transmission are not fully understood. Direct transmission is through contact with respiratory droplets or throat secretions from infected individuals and the disease itself is 'an infection of the thin lining surrounding the brain and spinal cord' (LaForce et al. 2009; Garcia-Pando et al. 2014). The Harmattan season generally extends from October till March, with the harshest part of the season in the first few months from October to December (Garcia-Pando et al. 2014). The season is characterized by hot, dry northeasterly trade winds blowing from the Sahara throughout West Africa; dust particles carried by the Harmattan winds make the mucus membranes of the nose of the region's inhabitants more sensitive, increasing the risk of meningitis infection (Yaka et al. 2008). In Niger, Yaka et al. (2008) show that 25% of the year to year variance in meningitis incidence can be explained by the Harmattan, winter climate. Though vaccines have been introduced to combat the spread of the disease since the first recorded cases in 1909 for SSA, effectiveness of the vaccines has been limited due to the mutation and virulence tendencies of the bacterium (LaForce et al. 2009).

Niger has experienced six epidemics since 1986, with the largest lag between epidemics occurring between the 1986 and subsequent 1993 epidemic as shown in Figure 2[4]. The periodicity of epidemics in Niger is around 8-10 years, with epidemic waves in the meningitis belt occurring every 8-14 years (Yaka et al. 2008). The 1986 epidemic was severe with

[3]Where epidemics are defined in the SSA context as greater than 100 cases per 100,000 population nationally within a year by the World Health Organization (WHO) (LaForce et al. 2009).

[4]Though there is no subnational record of epidemics available prior to 1986, historical records suggest that the last epidemic prior to 1986 occurred in 1979 in Niger (Yaka et al. 2008; Broome et al. 1983).

15,823 reported cases per 100,000 population and a mortality rate of about 4%[5], as shown in Figure 3 and Figure 4. Young children and teenagers are particularly at risk of infection during epidemic years, a fact that puts, and has historically placed, a major share of Niger's population[6] at particular disadvantage during epidemics. Domestic, interdistrict migration is limited in Niger[7] and population size across districts has been stable with the distribution almost entirely unchanged since 1986 and a correlation of .99 and .97 ($p < .001$) between 1986 district populations and 1992 and 1998 populations respectively[8]. We assess individual exposure to the 1986 meningitis epidemic based on a geographically based assignment at the district level, given low levels of interdistrict migration in the country.

3.4 Data and cohorts

We combine district level records on meningitis cases per 100,000 population from the World Health Organization (WHO) and the Ministry of Public Health in Niger with individual and district level data on education and demographics from the Nigerien Demographic and Health Surveys (DHS). The district level DHS data is available for 2 survey rounds in 1992 and 1998 and provide records for individuals in all 36 districts across the country including the capital at Niamey. Education measures the number of years of education that an individual has completed, and we limit our sample to the cohort born between 1960-1992 which allows us to include cohorts that were school going age during the 1986 meningitis epidemic. Figure 4 also shows the distribution of meningitis cases by district for the epidemic year, 1986 versus a non-epidemic year, 1990. Using data from Niger also allows us to exploit homogeneity in religious, ethnic and income characteristics across individuals in the country to more cleanly

[5]Calculated from WHO data, details presented in Section 4.

[6]Where the median age has remained at 15 years old for over a decade. Source: DHS and UNICEF statistics.

[7]With most migration consisting of young male seasonal migrants in the northern desert regions, traveling internationally to neighboring countries for work during during dry months (Afifi 2011).

[8]Source: Authors estimates from DHS data.

capture the effect of meningitis epidemic exposure[9]. District level data on mortality rates from meningitis are available in aggregate form only, and not available by gender.

We rely on information about the birth year to construct school-aged specific cohorts and their exposure to the 1986 meningitis epidemic. Three categories are defined which include ages 0-5, 6-12 and 13-20 with reference to 1986. These age bands reference the Nigerien school going requirements/context where 6-12 and 13-20 age categories correspond to primary and secondary school going ages respectively, and 0-5 are non-school going. While the mandatory school going start age is 7, we allow our primary school category to start from 6 to control for early school going children. The bands contain enough observations to ensure that estimations are not done on empty cells and also help to control for age misreporting in the sample.

Table 1 shows the distribution of our sample and schooling along with a snapshot of variable means for our meningitis cohort-case measure (MENIN) and years of education, our outcome variable, by cohort and gender. Notably, our sample is fairly distributed across age cohorts and gender. About 24% of the sample is contained in 0-5 ages, 19% in 6-12 ages and 17% in 13-20 age categories. This distribution is even similar conditional on gender. For example, about 20% of the sample is contained in 6-12 ages for females, compared to 19% for males. For educational attainment, the 0-5 age category has an average of about 1.1 years, the 6-12 ages averaged 2.1 years while the 13-20 category averaged 2.0 years of schooling. The distribution is also similar conditional on gender. Our overall results are insensitive to marginal changes in the age cutoffs.[10] We predict that the largest magnitudes in reduction of female education during the epidemic will be for primary school aged going children given statistics on low secondary school enrollment rates in the country[11]. Conversely, we should see no or little effect of meningitis exposure on years of education for non-school aged girls

[9]Niger is 98% muslim, over 50% Hausa and has a majority poor, agricultural population. Source: US Department of State, CIA.

[10]In Figure 10 of the Appendix, we display the density functions of educational attainment across the various cohorts and gender. The figures visually demonstrate similar distributional patterns across gender, similar to the average schooling results shown in Table 1.

[11]Source: UNICEF statistics.

(between ages 0-5) during the epidemic year.

We appeal to the scientific literature documenting the linkages between the Harmattan season and meningitis outbreaks in the meningitis belt to explore the potential implications of Harmattan and associated climate variables on the observed gender gap in human capital investment. We use data from NASA's Modern Era Retrospective Analysis for Research and Applications (MERRA-2)[12]. Following the environmental health literature on the climate factors associated with meningitis incidence in Niger, we examine district monthly mean wind speeds (measured in m/s) temperatures ($Kelvin$) and dust concentrations (kg/m^3). Perez Garcia Pando et al. (2014) highlight the importance of the previous year October-December cycle of these variables, and wind speed in particular, as important climatic predictors of meningitis outbreaks. The distribution of these variables against meningitis case data during the epidemic year (1985-1986) versus a non-epidemic year (1989-1990) is shown in Figure 5. Consistent with the results in Perez Garcia Pando et al. (2014), wind speeds peak in the more intense part of the Harmattan season preceding the epidemic year (October-December), falling during the less intense part of the Harmattan season (January-March) during the epidemic year. The trend is much weaker during the non-epidemic years, as shown using the 1989-1990 test case in Figure 5. Figure A2 depicts district level mean wind speeds during the more intense part of the Harmattan season (October-December) versus the less intense part of the Harmattan season (January- March) during the epidemic period.

To test hypotheses on the risk of early marriage of girls rising during meningitis epidemic years and leading to lowered educational attainment, we use data from the DHS men's and women's subsamples with summary statistics provided in Table 10.

[12]MERRA-2 is an atmospheric reanalysis data product that assimilates historical observation data over an extended period. https://disc.sci.gsfc.nasa.gov/datasets.

3.5 Empirical Framework

For our main results, we estimate panel regressions of school-aged specific cohorts a linking years of education for individual i in district d at survey round r to measures of meningitis exposure MENIN_{adt} that are interacted with the gender of the individual female_{ig}:

$$\text{education}_{iadrg} = \beta_g \text{female}_{ig} + \beta_a \text{MENIN}_{adt} + \gamma_{ag} \text{MENIN}_{adt} \times \text{female}_{ig} + \mu_d + \delta_r + \delta_t + \epsilon_{iadrg}$$

where t and g index the birth year and gender respectively. This specification includes district fixed effects μ_d which capture unobserved differences that are fixed across districts. The birth year and survey round fixed effects, δ_t and δ_r respectively, control for changes in national policies (e.g. immunization campaigns), potential life cycle changes across cohorts and other macro factors. Note that the birth year fixed effect subsumes cohort specific dummies since cohorts are defined based on birth year and the meningitis reference year 1986. The model also includes uninteracted terms for gender and meningitis exposure.

Our key parameter of interest is γ_{ag}, which is allowed to vary across cohorts. This measures the impact of MENIN on female respondents' education relative to their male counterparts, using variation across districts and the 1986 meningitis epidemic and identified based on standard assumptions in a difference-in-differences model. MENIN is measured in two ways. In the first case, we calculate the mean weekly cases of meningitis per 100,000 population recorded in a district (MENIN Cases). The second case modifies the first measure by interacting it with the number of months for which meningitis incidence is strictly positive (MENIN Intensity). The implied key variable of interest is therefore constructed by interacting the MENIN measures with gender. Estimations are done using OLS and standard errors are clustered at the district level. Robustness checks and falsification tests on our identifying assumptions are presented in the results section.

To test hypotheses concerning age at first marriage and meningitis exposure, we estimate OLS regressions of meningitis cases per 100,000 population on age at first marriage using district, year and year of birth fixed effects where possible.

3.6 Results

Table 2 reports estimates from two specifications for our two measures of meningitis exposure (i.e., MENIN Cases; MENIN Intensity) using 1960-1992 cohorts. Columns 1a and 1c display results for the linkages between educational attainment, gender and meningitis exposure at cohorts-level. The gender variable is negative and significant in both columns, documenting the existing gender gap between males and females in favor of males. Meningitis exposure across almost all cohorts is negative and insignificant. It is barely significant at 10% only in the MENIN Intensity measure for primary school cohorts.

Our main results are in columns 1b and 1d of Table 2 where we interact the meningitis exposure measures with gender to examine gender-differentiated impacts of the meningitis burden on educational investments. Gender is negative and significant. What is striking is that only interaction terms for the school going cohorts are negative and strongly significant at conventional levels. The interaction estimates are economically large in magnitude especially in the MENIN Cases measure. Interpreting the results from the MENIN Cases measure in column 1b, a case increase in the mean weekly meningitis cases per 100,000 population in each district is associated with a reduction of -.044 years of schooling or a 3% to 4% decrease in years of education[13] per case exposure, relative to the mean for female respondents of primary school going age during the epidemic year. Primary school aged female respondents in higher case exposure districts experience significant reductions in their years of education relative to their counterparts in lower case exposure districts during the epidemic year. Similar results are found for the secondary school aged female sample, with increases in meningitis case exposure associated with a reduction of -.03 years of schooling or 2% to 3% decrease in years of education, per case exposure relative to the mean for the female cohort. Reassuringly, the interaction is not significant for non-school going aged female respondents at the time of the epidemic.

We conduct various falsification/sensitivity tests. First, the results are robust to small

[13]Relative to the unconditional and conditional mean years of education respectively.

changes/modifications in cohort age cutoffs (Table 3). Our main results are derived using the definition of cohorts based on the 1986 epidemic. In alternate specifications presented in Table 4, we examine school going and non-school going aged cohorts based on the 1990 non-epidemic year. Table 4 reports estimates for cohorts defined based a reference non-epidemic year 1990. We find no effect of meningitis exposure for the primary school aged category across all relevant specifications, which is what we would expect[14]. There is evidence of effects for the secondary school aged category. The secondary cohorts are essentially capturing effects of initial exposure to the 1986 epidemic when such cohorts were in primary school[15]. The sign on the 0-5 group is significantly positive which suggests positive investment in education during non-epidemic years[16]. These robustness checks and falsification results make it less likely that we are picking up any spurious/confounding effects in our main results.

Our results suggest that meningitis epidemic health shocks disproportionately impact investment in girls' education potentially due to increases in the direct and opportunity costs of parental investment in girls' education during epidemic years. Epidemic years and higher than expected meningitis exposure might mean a contraction of the household budget constraint due to lost wages and increased health costs associated with the epidemic. Direct costs associated with fees might be higher when the household budget constraint shifts inward. Opportunity costs might rise with girls' labor increasingly commanded to care for sick family members or act as substitute labor for sick family members during the epidemic years[17]. One way that parents might respond to rising costs is by selling off "assets", or female children, to reduce consumption burdens and accrue income from bride price transfers from

---

[14]Note since attainment is cumulative, some of this effect captures a long run effect of initial exposure in 1986. The primary school-aged cohort in 1990 includes some of the non school-aged populations in 1986.

[15]Again due to slight serial correlation between 1986 and 1990 exposure as explained in the previous footnote.

[16]It could also suggest a reversal in district exposure during the 1993-1996 epidemics for respondents from these districts who would be in the primary school aged categories during that period. We address the subject of cumulative effects in ongoing work.

[17]Hartmann-Mahmud (2011) documents this phenomenon in her case study research interviewing Nigerien women.

grooms' families to brides' families as discussed in Section 2 and Corno et al. (2016).

3.7 Harmattan-induced Meningitis and educational gender gaps

Though the causes of meningitis epidemics and the mechanisms of disease transmission are not well understood, the environmental health literature has identified climatic variables as explaining up to 30% of the intra and inter country variation in meningitis exposure in certain countries within the meningitis belt (Garcia-Pando et al. 2014; Perez Garcia Pando et al. 2014; Yaka et al. 2008). In Niger, Garcia-Pando et al. (2014) find that wind speeds[18] and dust conditions during the harmattan months from October to December in the year prior to the meningitis year, correlate significantly with meningitis outbreaks in the proceeding year.

In this section, we directly investigate how Harmattan induces variations in meningitis to explain the observed gender gaps in educational attainment. We use an instrumental variable approach to link educational attainment $education_{iadrg}$ to our cohort-level meningitis exposure and gender:

$$education_{iadrg} = \gamma_{ag} MENIN_{adt} \times female_{ig} + \mu_d + \delta_r + \delta_t + \epsilon_{iadrg}$$
$$MENIN_{dt} = \rho \boldsymbol{Harm}_{dt} + c_d + \nu_{dt}$$

where $\boldsymbol{Harm}_{dt}$ contains the previous year (1985) Harmattan winds and dust concentration, as well as the current year (1986) weather or climate variables: temperature and precipitation. A set of unrestricted district dummies, denoted by c_d, are included to capture time-invariant district factors such as closeness to health amenities. Our key parameter of interest γ_{ag} is identified by district-level variation in Harmattan season variables $\boldsymbol{Harm}_{dt}$[19], which are presumably exogenous since we use the previous year Harmattan realizations while

[18] Zonal winds and meridional winds, and zonal winds in particular (Garcia-Pando et al. 2014).

[19] I.e., From baseline district differences and the 1986 meningitis epidemic exposure.

controlling for contemporaneous weather changes. All other terms are defined similarly as in previous sections.

3.7.1 First stage: link between Harmattan and Meningitis

The season of Harmattan in the previous period is a strong instrument and induces significant variation in households' exposure to meningitis. Tables 5 and 6 report the first-stage F-statistics both for meningitis cases and intensity, respectively. For each meningitis scenario, we present three sets of results that reflect three different candidate instruments. The instruments include (i) the average wind and dust concentration from 1985 (column 1), (ii) the average wind and dust concentration in the last quarter of 1985 (column 2), and (iii) the actual monthly wind and dust observations from the last quarter of 1985 but excludes district fixed effects (column 3).

Our preferred specification is column (2): averages the Harmattan variables over the Harmattan season (i.e., the last quarter of the previous year) with controls for district level climate and potential unobserved heterogeneity. In general, the winds are correlated with the dry season which starts in September. Thus, focussing on the last quarter of the previous year allows us to overcome concerns about other climatic events and seasons, and lends support for the validity of the Harmattan instrument. The null hypothesis that all coefficients of the Harmattan season and climate or weather variables are jointly zero can be easily rejected at conventional significance levels. All F-statistics in columns (1) and (2) are above 10, satisfying the usual cutoff value for weak instruments, in all meningitis scenarios. Since the first stage F-statistic is less than 10 in our third measure of Harmattan, we do present results for this instrument in our second stage analysis that examines the relationship between meningitis and educational gaps by gender.

3.7.2 Second stage: Harmattan-induced Meningitis and educational gender-gaps

We used two-stage-least-squares (2SLS) to estimate the above equations, and report the second stage results in Tables 7 and 8. The columns follow the same layout as the previous baseline tables. Columns (2) and (4) include the interaction terms between gender and meningitis exposure at cohorts-level while columns (1) and (3) omit the interactions. In all cases, the gender variable is negative and significant at conventional levels. The estimated impact of Harmattan-instrumented meningitis exposure on female respondents' education relative to their male counterparts is negative and statistically significant for the school going aged cohorts, but not significant for the non-school aged cohorts (0-5 years) based on clustered standard errors. For the school going cohorts, the impacts range from -0.053 to -.060 (for MENIN cases); with an average estimate of about -0.057. On the other hand, the estimates range between -0.0054 to -0.0059 for MENIN intensity. Notice that the first stage F-statistics reported in Tables 5 and 6 are all above the usual cutoff point of 10 for concerns of weak instruments.

The estimated impacts for MENIN cases imply that a case increase in the mean weekly meningitis cases per 100,000 population in each district is associated with an average reduction of -0.057 years of schooling or about 4.7% to 4.9% decrease in years of education per case exposure, relative to the mean for female respondents of primary school going age during the epidemic year. Our 2SLS results suggest significant gender-differentiated negative impacts (disproportionately against school-aged cohort females) of meningitis induced by the Harmattan season. The 2SLS results re-affirm our baseline OLS estimates in Table 2 (i.e., in terms of the sign of the relevant coefficients), but the estimated impacts under the 2SLS are slightly larger suggesting a slight downward bias of the estimated baseline impacts by gender. Together, the 2SLS analysis allows us to adjust for this potential bias while examining how exogenous variation in meningitis exposure induced by Harmattan could propagate into differential human capital investments by gender.

Estimating the reduced form link: Other diseases are omitted variables in explaining education, and are likely correlated with both Harmattan and meningitis. For example, dusts carried by Harmattan may cause asthma, or possibly affect other channels such as agricultural productivity with direct effect on education. This confounds the estimated causal link between Harmattan-meningitis-education. For this reason, we assess the potential role of Harmattan via the reduced form link between Harmattan and education. We regressed educational outcome on our preferred measure of Harmattan seasonality (with the cohort interactions). Results are reported in Table 9. The coefficient of the 'female' variable is negative across all model specifications, suggesting significant gender gaps in education. The interaction between female and Harmattan variables (dust concentration, wind speed) is never significant for non-school age going cohorts. However, it is negative and significant for the school age going cohorts. Overall, the evidence is consistent with climate or Harmattan playing a significant role in the incidence of meningitis and contributing to the widening of gender gaps in human capital, with disproportionate negative impact on investment in girls' education.

3.8 Indirect and direct Channels: economic and health responses

Section 2 outlined the expected direct and indirect channels through which health shocks like the meningitis epidemic might be expected to affect gender gaps in human capital investment. The following subsections explore these mechanisms and find evidence in favor of the indirect economic channel. The high economic costs of disease burdens during epidemic years induce households to marry off their daughters at earlier ages.

3.8.1 Indirect channels: economic responses and gender Gaps

Documented data on health expenditure on other countries in the meningitis belt suggest that the indirect channel, through increased direct and opportunity costs following a meningitis expenditure might be the primary channel through which the epidemics affect differential household investment in girls' and boys' education (Colombini et al. 2009). In Burkina Faso, Niger's neighbor in the meningitis belt, households spent some $90 per meningitis case, 34% of per capita GDP in direct medical and indirect costs from meningitis infections over the 2006-2007 epidemic (Colombini et al. 2009). In affected households with sequelae, costs rose to as high as $154 per case. Costs were associated with direct medical costs from spending on prescriptions and medicines[20] and indirect costs from loss of caregiver income (up to 9 days of lost work), loss of infected person income (up to 21 days of lost work) and missed school if attending (12 days of missed school) (Colombini et al. 2009). In the presence of these high costs, studies have documented that one way parents try to smooth consumption is to reduce investment in girls' human capital relative to their male siblings (Barcellos, Carvalho, and Lleras-Muney 2014; Corno et al. 2016). We examine one important method of doing this which is through increased early marriage of girls in the next section.

3.8.1.1 Meningitis epidemic, early marriage and educational attainment

Niger has the highest rates of early marriage in the world, with 75% of girls married before the age of eighteen (Loaiza Sr and Wong 2012). Niger is also part of a number of countries in the world, particularly in sub-Saharan Africa, that engages in bride price transfers of wealth from grooms' families to brides' families at the time of marriage. Previous studies have documented increases in the risk of early marriage following negative income shocks to households, and we provide evidence of this following the epidemic (Corno et al. 2016). First, we confirm findings from the literature on age at first marriage and document positive,

[20]Vaccines are technically free during epidemics, however information asymmetry among health care workers and shortages of vaccines often raise the price of medication (Colombini et al. 2009).

significant associations between age at first marriage and years of education for school going aged female populations during the epidemic (1986) and non-epidemic (1990) years in Table 11. The coefficients remain stable, strongly significant and positive at around .3 for school going aged female populations during the epidemic and non-epidemic years as shown in columns (1)-(2) and (5)-(6). Interestingly, for the male sample, while there is a significant, positive but much smaller coefficient of association (around .06) between age at first marriage and years of education for males who where school going aged during the epidemic year, there is no significant association between age at first marriage and years of education for males who were school going aged during the non-epidemic year as shown in column (8) of Table 11. The results suggest that the association between age at first marriage and years of education is much stronger for women than men in the sample.

Next, to explore the relationship between age at first marriage and meningitis exposure, particularly during epidemic years, we chart age at first marriage cumulative hazards with results shown in Figure 6. Figure 6 shows age at first marriage cumulative hazard for male and female school going aged populations by meningitis exposure in epidemic (1986) and non-epidemic years (1990). In above the national meningitis districts (denoted as 'High Menin' in the figure), hazard rates are noticeably higher for both male and female respondents during the epidemic year. The magnitude is larger for female respondents during the epidemic year, who are typically also married at earlier ages (the mean age at first marriage is about 15 years old as shown in Table 10 for women versus about 21 years for men in the school going aged cohort during the 1986 epidemic year) than their male counterparts. Quantitatively, female respondents who were school going aged during the 1986 epidemic year are almost two times more likely to marry earlier in high (above the national mean) meningitis exposed districts than in low (below the national mean) meningitis exposed districts. The trend in the 1990 non-epidemic year is reversed with age at first marriage higher in high meningitis exposed districts for school going aged males and females during the 1990 non-epidemic year. Given these trends in the raw data we assess significance, estimating regressions with OLS, with results shown in Table 12. The first set of results in column (3) of Table 12 show significant negative associations (about -.024) between meningitis cases and age at first marriage for the female school going aged sample as of the time of the epidemic, with no significant effect for the comparable male sample. In contrast, there is no significant association between meningitis cases and age at first marriage for either the female or male school going aged samples during the non-epidemic test year, 1990 as shown in column (6). The results provide support for the indirect channel discussed in Section 2 and Section 8 where the epidemic acts as a negative income shock leading households to smooth consumption by “selling" their daughters for a bride price, reflected in the lowered age at first marriage during epidemic years but not non-epidemic years and with the effects significant for girls but not boys.

3.8.2 Direct channels: health and gender gaps

On the direct, health channel, given the lack of data on infection and mortality rates by gender, we refer to the epidemiology and health literature on the biology of meningitis infection. First, there is little documented evidence on differential infection and mortality rates of meningitis by gender (Trotter and Greenwood 2007). A simple regression on the female share by district and mortality rates during the epidemic year reveals no direct trends as shown in Table 9, although this is unsurprising given that the magnitude of the mortality effect to see a response in female populations would have to be extremely large. Another way the direct health channel might operate is if girls, when they are sick, are less likely to be treated or as quickly treated as boys due to gender bias in parental investment in children as has been documented in other studies (Barcellos, Carvalho, and Lleras-Muney 2014). This might also lead to differential mortality by gender during the epidemic, though the size of this effect is difficult to estimate given the paucity of data. Similarly, if treatment or time to treatment differs by gender, then there might be more incidences of long-term neurological damage in girls over boys which might affect school investment choices and lead to lower attainment as well.[21]

In addition, there could be effects on kids who were exposed to some form of Meningitis at a very young age (i.e., pre-school), deteriorating their cognitive abilities and affecting later educational outcomes. Table 2 provides a test for such biological channel, whereby it is rejected. If this channel is meaningful, then one would expect the effect of 1986 Meningitis exposure on education to be large and significant for the 0-5 age cohort. The effects are nearly zero and rejected at all conventional levels of significance.

3.8.3 Evaluation of alternative hypotheses

This section further evaluates the robustness of the estimated effects of meningitis exposure on the gender gap in years of education, and the relationship between the age at first marriage and meningitis exposure.

3.8.3.1 Impact of concurrent shocks

One potential hypothesis is that concurrent rainfall shocks, common in SSA, might explain the relationship between meningitis and the gender gap in years of education identified in this paper. To test this, we re-estimate our baseline specification by interacting the various cohorts with precipitation shocks. Precipitation shocks are defined as average district level precipitation differenced from the national mean during the 1986 epidemic year. The results are reported in Table 13. Each column in the table denotes different model specifications, with and without controls for temperature[22]. The results show no effect of precipitation

[21]While it is possible that young girls are less taken care of when ill as compared to boys, in bride price societies where healthy girls may be more valued, that effect may seem less significant, perhaps explaining why mortality rates are not significantly different across gender. Note that our assessment of the various channels is by no means exhaustive.

[22]Controlling for temperature is important since it is correlated with precipitation (Schlenker and Roberts 2009).

shocks on gender gaps in education across all cohorts, lending further support to the estimated effect of meningitis exposure during the epidemic year.

3.8.3.2 Meningitis, wealth and age at first marriage

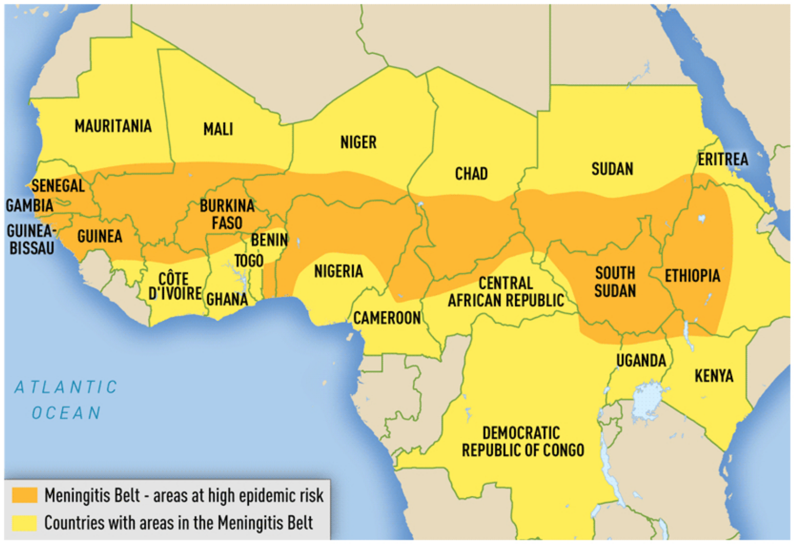
In section 8.1, we argue that the primary channel underlying the differential gender impacts of meningitis is that girls are married off, particularly at early ages. This will be especially true for liquidity constrained households. We reaffirm this by estimating a model that links age at first marriage with liquidity. Using data on assets from the DHS[23] , we construct a wealth index and define liquidity or asset constrained households as those located in the lower parts of the asset distribution. The results are reported in Table 14. The first column excludes interactions between meningitis and asset quintiles; the second includes the interactions. As expected, column 1 shows that age at first marriage for female respondents is likely higher in the less liquidity constrained households (above the third quintile) as compared to the constrained. There is a significant negative effect of sudden exposure to meningitis on the age at first marriage for women belonging to asset constrained households. Estimates from the second column show that the impact of meningitis exposure on asset constrained households is significantly larger. In particular, meningitis has limited impact on the age at first marriage for the less constrained. Note that the estimate for the less constrained categories are similar in both specifications. Finally, columns 3 and 4 replicate the analysis using a non-epidemic year, 1990. There is no evidence of meningitis impact on the age at first marriage of female respondents and its interaction with wealth/asset status, lending further support for the early marriage channel following meningitis epidemics.

[23]The wealth index is based on ownership of the following 20 assets in the DHS women's sample: electricity, durables (e.g. radio, tv, fridge, car, bicycle), water and sanitation infrastructure and housing structure (e.g. dirt floor, cement floor). For lack of DHS data for 1986, we proxy the wealth status using available data for 1992 and 1998. This assumes that the wealth of current respondents is strongly correlated with their previous households. This might be a strong assumption but seems reasonable in Niger since distributional measures, like the Gini coefficient, have remained largely unchanged over the past two decades. http://povertydata.worldbank.org/poverty/country/NER.

3.9 Conclusion

Our analysis of the effects of exposure to the 1986 meningitis epidemic on educational attainment of school aged girls in Niger, reveals that the gender gap widened during the epidemic year. The effect is particularly significant for primary school aged girls at the time of the epidemic, since most of the investment in education happens at the primary level. We find a significant decrease in years of education for school aged female respondents at the time of the epidemic with no significant effect for their male counterparts. Given the evidence on the intergenerational returns to female education and the potential economic returns to closing the gender gap, these results highlight the need for dual policy addressing both education and health to target the gender gap in educational attainment. We also provide evidence on the links between meningitis outbreaks and Harmattan season intensity, prompting further discussion on the role of climate-induced disease on worsening social inequality.

We provide evidence for the an indirect economic channel where the epidemic acts as a negative income shock prompting households to smooth consumption by cutting back on education expenditures of girls and selling daughters in exchange for bride price wealth transfers. A consequence of this is lowered age at first marriage for girls during epidemic years and less years of education, which would explain the widened gender gap during the epidemic year. An important contribution of the paper is to show that disease burdens and health shocks contribute significantly to widening gender gaps in educational attainment with associated implications for development in poorer countries. This line of research has broader implications for climate-induced disease effects on social inequality.



MAP 3-11. AREAS WITH FREQUENT EPIDEMICS OF MENINGOCOCCAL MENINGITIS[1]

[1] Disease data source: World Health Organization. International Travel and Health. Geneva, Switzerland: 2012.

Figure 3.1: Areas with Frequent Epidemics of Meningococcal Meningitis ("Meningitis Belt")

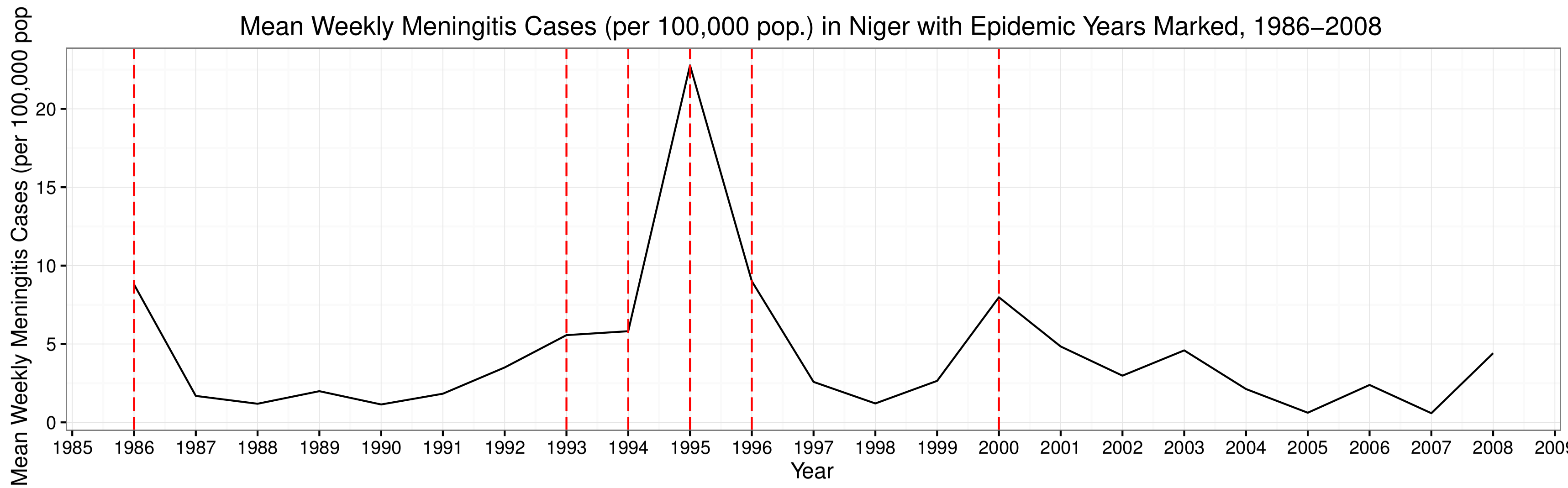


Figure 3.2: Niger Meningitis Cases with Epidemic Years Marked, 1986-2008

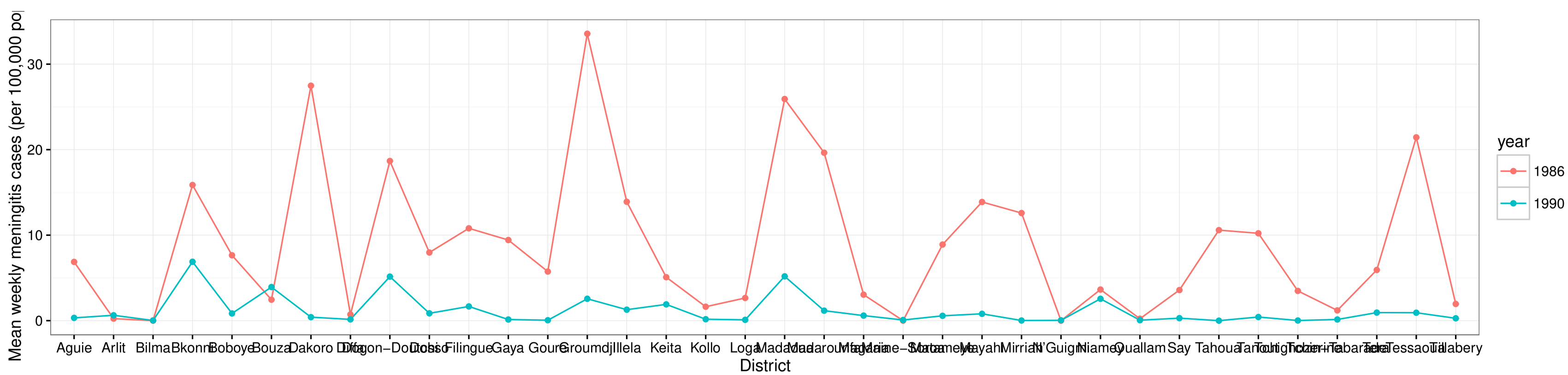


Figure 3.3: Niger Meningitis Cases by District in Epidemic (1986) and Non-epidemic (1990) Years

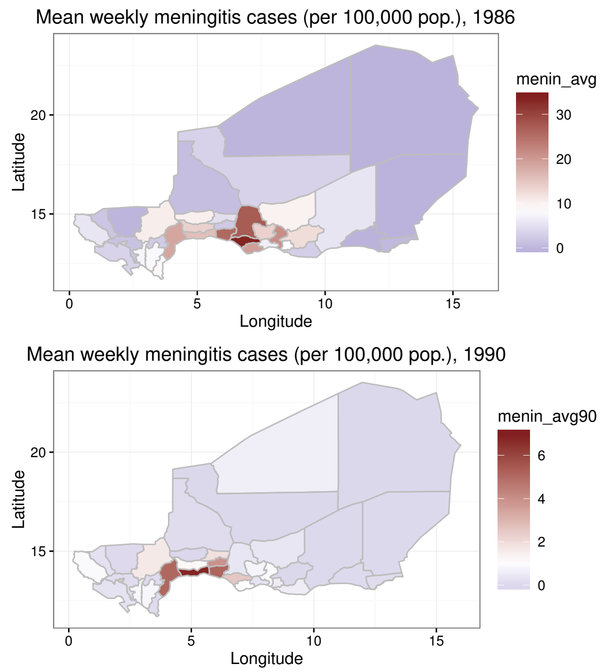


Figure 3.4: Niger Meningitis Cases and Population by District in Epidemic (1986) and Non-epidemic (1990) Years

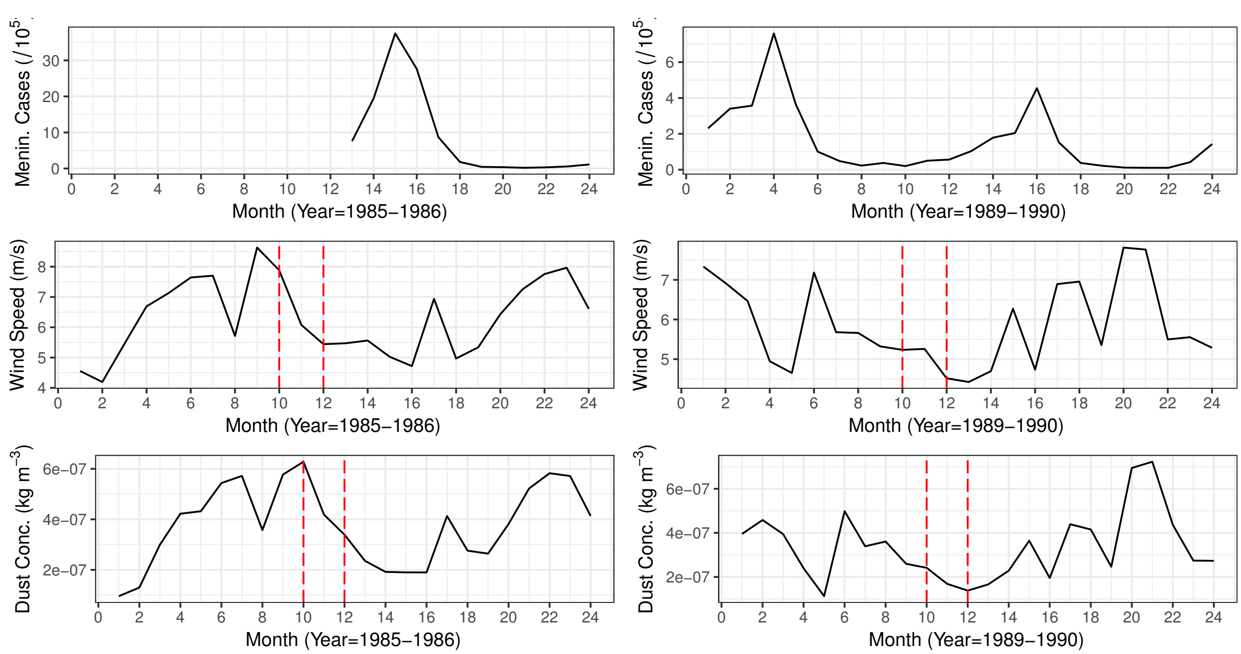


Figure 3.5: Harmattan and Meningitis Response

Table 3.1: Distribution: Sample, Schooling and Variable Means

	Total population			Males			Females		
	1992	1998	1992-1998	1992	1998	1992-1998	1992	1998	1992-1998
Population									
percent age 0-5 in 1986	0.24	0.24	0.24	0.24	0.24	0.24	0.24	0.23	0.23
percent age 6-12 in 1986	0.21	0.18	0.19	0.21	0.17	0.19	0.21	0.19	0.2
percent age 13-20 in 1986	0.16	0.18	0.17	0.15	0.16	0.15	0.18	0.20	0.19
Meningitis cases cohort exposure									
age 0-5 in 1986	2.47	2.54	2.5	2.51	2.67	2.58	2.43	2.42	2.43
age 6-12 in 1986	2	1.84	1.93	2.10	1.68	1.91	1.91	1.98	1.94
age 13-20 in 1986	1.52	1.99	1.73	1.36	1.77	1.54	1.67	2.19	1.91
Years of education									
Control Cohorts: age 0-5 in 1986	0.40	1.95	1.09	0.46	2.33	1.3	0.33	1.58	0.89
Treated Cohorts: age 6-12 in 1986	1.85	2.38	2.07	2.26	3.22	2.63	1.46	1.72	1.57
Treated Cohorts: age 13-20 in 1986	1.99	1.83	1.91	2.69	2.58	2.64	1.43	1.32	1.37

Table 3.2: Difference in Difference Estimates of the Differential Impact of Meningitis Exposure on Education (1986 Epidemic Year), MENIN x Female

	Dependent Variable: Years of Education			
	MENIN Cases		MENIN Intensity	
	(1a)	(1b)	(1c)	(1d)
Female	−0.646***	−0.498***	−0.646***	−0.513***
	(0.050)	(0.076)	(0.050)	(0.071)
Meningitis exposure at ages 0-5	−0.002	0.001	−0.0002	0.0001
	(0.003)	(0.004)	(0.0003)	(0.0004)
x Female		−0.006		−0.0005
		(0.006)		(0.001)
Meningitis exposure at ages 6-12	−0.027	−0.004	−0.003*	−0.001
	(0.017)	(0.021)	(0.001)	(0.002)
x Female		−0.044***		−0.004***
		(0.012)		(0.001)
Meningitis exposure at ages 13-20	−0.047	−0.029	−0.004	−0.002
	(0.031)	(0.030)	(0.003)	(0.003)
x Female		−0.032***		−0.003***
		(0.011)		(0.001)
Constant	1.032***	0.953***	1.003***	0.932***
	(0.199)	(0.215)	(0.185)	(0.197)
District fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes	Yes
Observations	47,697	47,697	47,697	47,697
R^2	0.208	0.210	0.208	0.209

Notes: Regressions estimated by OLS. Robust standard errors in parentheses clustered by district. Dependent variable is years of education across all specifications. MENIN cases is the meningitis exposure explanatory variable defined as average district level weekly case (per 100,000 population) exposure for cohort at specified ages during the 1986 epidemic year. MENIN intensity is the meningitis exposure explanatory variable measured as district level case exposure for cohort at specified ages during the 1986 meningitis epidemic year multiplied by number of months of exposure (with greater than zero cases). Mean level of education in the sample is 1.22, and the standard deviation is 2.7. Mean level of education for boys in the sample is 1.51 and the mean level of education for girls in the sample is 0.94. The estimates represent 3% to 4% and 2% to 3% reduction in education for girls in the primary school going age sample (ages 6-12) and secondary school going age sample (ages 13-20) respectively relative to the unconditional and conditional means. ***Significant at the 1 percent level, **Significant at the 5 percent level, *Significant at the 10 percent level.

Table 3.3: Difference in Difference Estimates of the Differential Impact of Meningitis Exposure on Education (1986 Epidemic Year), Robustness Check

	Dependent Variable: Years of Education			
	MENIN Cases		MENIN Intensity	
	(3a)	(3b)	(3c)	(3d)
Female	−0.644***	−0.535***	−0.645***	−0.546***
	(0.049)	(0.067)	(0.049)	(0.064)
Meningitis exposure at ages 0-4	0.006	0.005*	0.001	0.0005*
	(0.004)	(0.003)	(0.0004)	(0.0003)
x Female		0.0005		0.0001
		(0.006)		(0.001)
Meningitis exposure at ages 7-12	−0.025	−0.003	−0.002*	−0.0004
	(0.016)	(0.020)	(0.001)	(0.002)
x Female		−0.042***		−0.004***
		(0.012)		(0.001)
Meningitis exposure at ages 14-21	−0.046	−0.028	−0.004	−0.002
	(0.030)	(0.029)	(0.003)	(0.002)
x Female		−0.031***		−0.003***
		(0.009)		(0.001)
Constant	1.038***	0.982***	1.018***	0.966***
	(0.199)	(0.210)	(0.187)	(0.195)
District fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes	Yes
Observations	47,697	47,697	47,697	47,697
R^2	0.208	0.210	0.208	0.209

Notes: Regressions estimated by OLS. Robust standard errors in parentheses clustered by district. Dependent variable is years of education across all specifications. MENIN cases is the meningitis exposure explanatory variable defined as average district level weekly case (per 100,000 population) exposure for cohort at specified ages during the 1986 epidemic year. MENIN intensity is the meningitis exposure explanatory variable measured as district level case exposure for cohort at specified ages during the 1986 meningitis epidemic year multiplied by number of months of exposure (with greater than zero cases). ***Significant at the 1 percent level, **Significant at the 5 percent level, *Significant at the 10 percent level.

Table 3.4: Difference in Difference Estimates of the Differential Impact of Meningitis Exposure on Education (1990 Non-Epidemic Year), Robustness Check

	Dependent Variable: Years of Education			
	MENIN Cases		MENIN Intensity	
	(2a)	(2b)	(2c)	(2d)
Female	−0.644***	−0.652***	−0.643***	−0.654***
	(0.050)	(0.076)	(0.049)	(0.074)
Meningitis exposure at ages 0-5	−0.070	−0.129	−0.011	−0.017
	(0.096)	(0.118)	(0.012)	(0.014)
x Female		0.117**		0.011**
		(0.047)		(0.005)
Meningitis exposure at ages 6-12	−0.006	0.011	−0.002	−0.001
	(0.042)	(0.057)	(0.004)	(0.006)
x Female		−0.032		−0.002
		(0.041)		(0.004)
Meningitis exposure at ages 13-20	0.011	0.072	0.003	0.009
	(0.050)	(0.061)	(0.006)	(0.007)
x Female		−0.111***		−0.010***
		(0.038)		(0.003)
Constant	1.038***	1.042***	1.018***	1.024***
	(0.181)	(0.193)	(0.169)	(0.181)
District fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes	Yes
Observations	47,697	47,697	47,697	47,697
R^2	0.205	0.207	0.206	0.207

Notes: Regressions estimated by OLS. Robust standard errors in parentheses clustered by district. Dependent variable is years of education across all specifications. MENIN cases is the meningitis exposure explanatory variable defined as average district level weekly case (per 100,000 population) exposure for cohort at specified ages during the 1990 non-epidemic year. MENIN intensity is the meningitis exposure explanatory variable measured as district level case exposure for cohort at specified ages during the 1990 non-epidemic year multiplied by number of months of exposure (with greater than zero cases). Mean level of education in the sample is 1.22, and the standard deviation is 2.7. Mean level of education for boys in the sample is 1.51 and the mean level of education for girls in the sample is 0.94. ***Significant at the 1 percent level, **Significant at the 5 percent level, *Significant at the 10 percent level.

Table 3.5: Meningitis Incidence (Cases) and the Harmattan Season

VARIABLES	(1) Cases	(2) Cases	(3) Cases
1985 Avg dusts and winds	Yes	No	No
1985Q4 Avg dusts and winds	No	Yes	No
1985OND dusts and winds	No	No	Yes
Current weather controls	Yes	Yes	No
District fixed effects	Yes	Yes	No
Observations	231	231	33
1st-stage: F-statistic	4.8E21	9.20E20	3.27
1st-stage: P-value	<0.0001	<0.0001	0.0337

Notes: Table reports the results from regressions of meningitis cases on previous Harmattan season and current weather variables: temperature and precipitation at the district level. Columns (1)-(3) differ based on the included variables. Column (1) includes the average wind and dust concentration from 1985, column (2) includes the average wind and dust concentration in the last quarter of 1985 (Harmattan season), while column (3) includes the actual monthly observations from the last quarter of 1985 but excludes the district fixed effects. Errors are clustered at the district level. Significant at the 1 percent level, Significant at the 5 percent level, Significant at the 10 percent level.

Table 3.6: Meningitis Incidence (Intensity) and the Harmattan Season

VARIABLES	(1) Intensity	(2) Intensity	(3) Intensity
1985 Avg dusts and winds	Yes	No	No
1985Q4 Avg dusts and winds	No	Yes	No
1985OND dusts and winds	No	No	Yes
Current weather controls	Yes	Yes	No
District fixed effects	No	No	No
Observations	231	231	33
Ist-stage: F-statistic	4.2E21	7.6E20	3.75
1st-stage: P-value	<0.0001	<0.0001	0.0062

Notes: Table reports the results from regressions of meningitis intensity on previous Harmattan season and current weather variables: temperature and precipitation at the district level. Columns (1)-(3) differ based on the included variables. Column (1) includes the average wind and dust concentration from 1985, column (2) includes the average wind and dust concentration in the last quarter of 1985 (Harmattan season), while column (3) includes the actual monthly observations from the last quarter of 1985 but excludes the district fixed effects. Errors are clustered at the district level. Significant at the 1 percent level, Significant at the 5 percent level, Significant at the 10 percent level.

Table 3.7: IV Results 1: Harmattan Induced Meningitis and Educational Gender Gaps

	Dependent Variable: Years of Education			
	MENIN Cases		MENIN Intensity	
	(1)	(2)	(3)	(4)
Female	−0.624***	−0.413***	−0.624***	−0.406***
	(0.0521)	(0.0864)	(0.0521)	(0.0861)
Meningitis exposure at ages 0-5	−0.00818	−0.00143	−0.000875	−0.000175
	(0.00549)	(0.00737)	(0.000564)	(0.000742)
x Female		−0.0129		−0.00129
		(0.00979)		(0.000972)
Meningitis exposure at ages 6-12	−0.0627*	−0.0308	−0.00638	−0.00324
	(0.0314)	(0.0347)	(0.003171)	(0.00350)
x Female		−0.0598***		−0.00591***
		(0.00927)		(0.000921)
Meningitis exposure at ages 13-20	−0.104*	−0.0726	−0.01051	−0.00733
	(0.0588)	(0.0577)	(0.005939)	(0.00580)
x Female		−0.0534***		−0.00538***
		(0.0127)		(0.00127)
Constant	1.305***	1.192***	1.302***	1.186***
	(0.342)	(0.367)	(0.339)	(0.364)
District fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes	Yes
Instrument	1985 Avg Dust & Wind	1985 Avg Dust & Wind	1985 Avg Dust & Wind	1985 Avg Dust & Wind
Current weather controls	Yes	Yes	Yes	Yes
Observations	43,814	43,814	43,814	43,814
R^2	0.215	0.218	0.215	0.218

Notes: Second stage IV results. Table reports the results from regressions of educational attainment on Harmattan-instrumented meningitis exposure at the district level. Columns (1)-(4) differ based on how the exposure to meningitis is defined and its interaction with gender. Columns (2) and (4) include the interaction terms between cohort level meningitis exposure and gender, while columns (1) and (3) omit the interactions. Errors are clustered at the district level. ***Significant at the 1 percent level, **Significant at the 5 percent level, *Significant at the 10 percent level.

Table 3.8: IV Results 2: Harmattan Induced Meningitis and Educational Gender Gaps

	Dependent Variable: Years of Education			
	MENIN Cases		MENIN Intensity	
	(1)	(2)	(3)	(4)
Female	−0.624***	−0.413***	−0.624***	−0.407***
	(0.0519)	(0.0868)	(0.0520)	(0.0864)
Meningitis exposure at ages 0-5	−0.00744	−0.000706	−0.000770	−0.000096
	(0.00562)	(0.00759)	(0.000577)	(0.000765)
x Female		−0.0129		−0.00129
		(0.00988)		(0.000982)
Meningitis exposure at ages 6-12	−0.0622*	−0.0301	−0.00634*	−0.00316
	(0.0329)	(0.0362)	(0.00333)	(0.00366)
x Female		−0.0602***		−0.00595***
		(0.00938)		0.000932)
Meningitis exposure at ages 13-20	−0.106*	−0.0737	−0.0107*	−0.00745
	(0.0606)	(0.0597)	(0.00613)	(0.00600)
x Female		−0.0540***		−0.00545***
		(0.0127)		(0.00127)
Constant	1.307***	1.194***	1.305***	1.189***
	(0.347)	(0.372)	(0.344)	(0.369)
District fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes	Yes
Instrument	1985Q4 Avg Dust & Wind	1985Q4 Avg Dust & Wind	1985Q4 Avg Dust &Wind	1985Q4 Avg Dust &Wind
Current weather controls	Yes	Yes	Yes	Yes
Observations	43,814	43,814	43,814	43,814
R^2	0.215	0.218	0.215	0.218

Notes: Second stage IV results. Table reports the results from regressions of educational attainment on Harmattan-instrumented meningitis cases in 1986 at the district level. Columns (1)-(4) differ based on how the exposure to meningitis is defined and its interaction with gender. Columns (2) and (4) include the interaction terms between cohort level meningitis exposure and gender, while columns (1) and (3) omit the interactions. Errors are clustered at the district level.Errors are clustered at the district level. ***Significant at the 1 percent level, **Significant at the 5 percent level, *Significant at the 10 percent level.

Table 3.9: Reduced Form: Harmattan and Educational Gender Gaps

Dependent Variable: Years of Education

VARIABLES	Winds, m/s		Dusts, kg/m^3	
Female	-0.6222***	-0.3439***	-0.6274***	-0.3804***
	(0.05062)	(0.0823)	(0.0556)	(0.0767)
Harmattan at ages 0-5	-0.0820*	-0.0708	242233.8	288659.6
	(0.0439)	(0.0431)	(527181.7)	(528353.8)
x Female		-0.018215		-139158.7
		(0.0185)		(259727.9)
Harmattan at ages 6-12	-0.4648	-0.4159	-228145	387979.2
	(0.2901)	(0.2942)	(991882.1)	(1026905)
x Female		-0.0984***		-1307362***
		(0.0199)		(304792.7)
Harmattan at ages 13-20	-0.8467	-0.7677	-704554.9	275932.9
	(0.5667)	(0.5521)	(1237784)	(1244591)
x Female		-0.1123***		-1610260***
		(0.0182)		(317255.8)
Constant	-58.4782**	-61.2189**	-69.9017***	-69.8805 ***
	(24.1090)	(23.84826)	(5.445593)	(9.5391)
District fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes	Yes
Current weather controls	Yes	Yes	Yes	Yes
Harmattan correlate control	Dusts, kg/m^3	Dusts, kg/m^3	Winds, m/s	Winds, m/s
	3.23e+07***	3.26e+07***	-0.3338***	-0.3510***
	(1979867)	(1940469)	(0.0334)	(0.0356)
Observations	43,814	43,814	43,814	43,814
R^2	0.215	0.218	0.210	0.213

Notes: Reduced form link between educational outcomes and Harmattan. Table reports the results from regressions of educational attainment on Harmattan season at the district level. Columns (1)-(4) differ based on the inclusion of interaction terms and weather controls. Columns (2) and (4) include the interaction terms between cohort level Harmattan season variables (1985Q4: winds and dust) and gender, while columns (1) and (3) omit the interactions. Errors are clustered at the district level.Errors are clustered at the district level. ***Significant at the 1 percent level, **Significant at the 5 percent level, *Significant at the 10 percent level.

Table 3.10: Mechanism Check: Correlation Between District Mortality Rate During 1986 Epidemic and 1992-1998 District Level Share of Female Respondents

	Dependent Variable: District Mortality Rate, 1986 Epidemic
Share Female in District	0.163
	(0.413)
Constant	−0.043
	(0.215)
Observations	32
R^2	0.005
Note:	$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

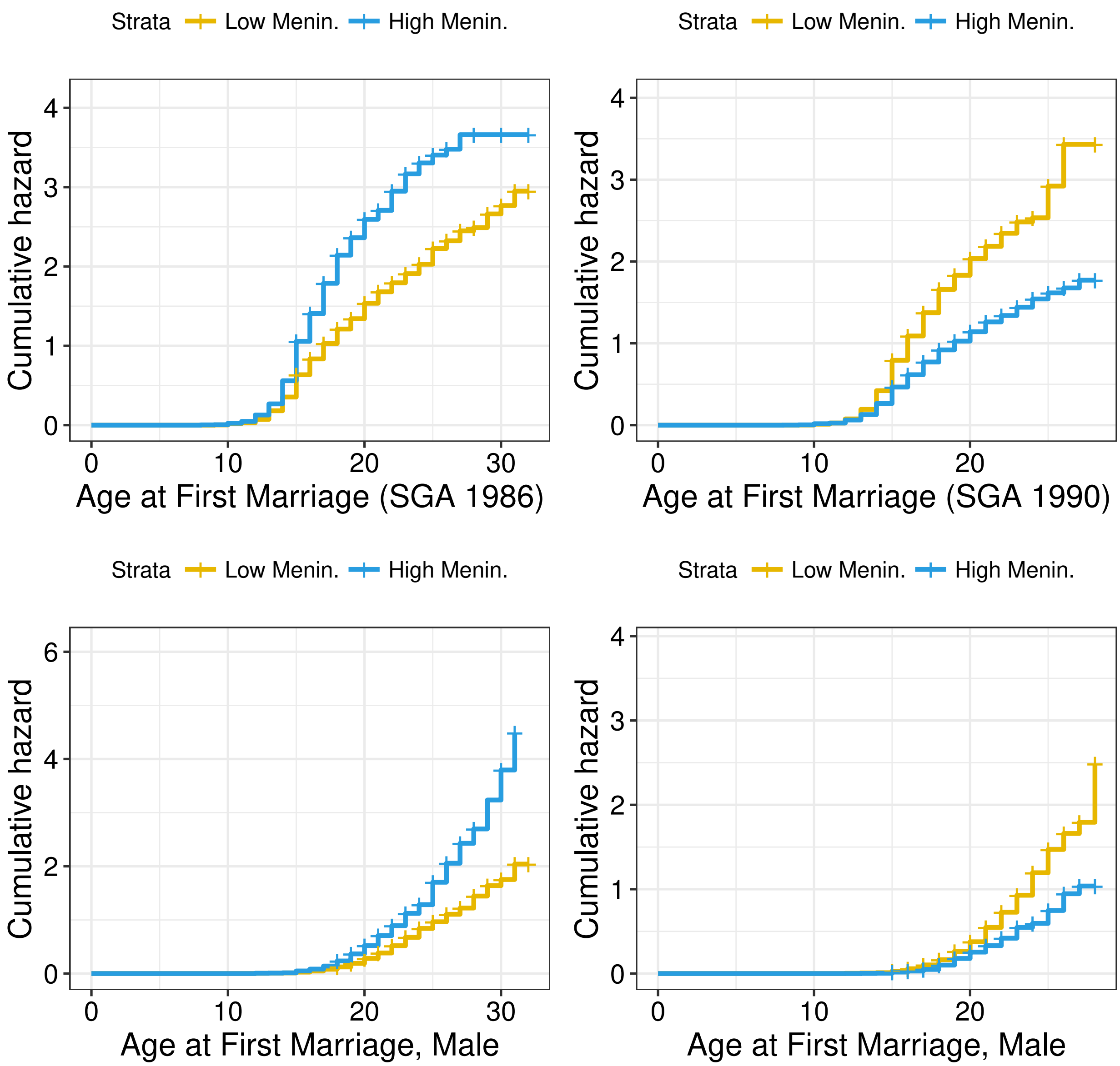


Figure 3.6: Age of First Marriage Cumulative Hazard for School-Going Aged (SGA) Populations by Meningitis Exposure in Epidemic (1986) and Non-epidemic (1990) Years

Table 3.11: DHS Subsamples: Men and Women's Sample Variable Means

Statistic	N	Mean	St. Dev.	Min	Max
DHS Women's Sample, SGA 1986					
Age at First Marriage	5,898	15.061	2.533	8	31
Years of Education	7,255	1.557	3.064	0	16
Meningitis Cases 1986	7,255	9.634	7.951	0.000	31.231
Age	7,255	22.458	4.504	15	32
Nos. of Wives	5,573	0.354	0.594	0	7
Age at First Birth	5,280	17.250	2.609	10	31
Age Gap Husband	4,136	12.128	7.930	−5	70
DHS Men's Sample, SGA 1986					
Age at First Marriage	954	20.755	3.557	10	31
Years of Education	1,657	1.750	2.413	0	13
Meningitis Cases 1986	1,657	10.291	8.562	0.000	31.231
Age	1,657	24.180	4.223	17	32
Nos of Wives	906	1.086	0.300	1	4
DHS Women's Sample, SGA 1990					
Age at First Marriage	4,550	14.989	2.257	8	27
Years of Education	6,447	1.680	3.071	0	16
Meningitis Cases 1990	6,447	1.575	1.720	0.000	6.769
Age	6,447	19.892	3.704	15	28
Nos. of Wives	4,322	0.303	0.563	0	7
Age at First Birth	3,681	16.987	2.337	10	28
Age Gap Husband	2,907	12.194	7.803	−5	70
DHS Men's Sample, SGA 1990					
Age at First Marriage	551	19.920	3.003	12	28
Years of Education	1,728	1.799	2.366	0	10
Meningitis Cases 1990	1,728	1.631	1.663	0.000	6.769
Age	1,728	20.509	3.987	15	28
Nos. of Wives	515	1.070	0.263	1	3

Table 3.12: Correlation between Age at First Marriage and Years of Education for School-Going Aged Respondents during Epidemic (1986) and Non-epidemic (1990) Years

	Dependent Variable: Years of Education							
	SGA 1986 F		SGA 1986 M		SGA 1990 F		SGA 1990 M	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age at First Marriage	0.365***	0.313***	0.078***	0.065**	0.305***	0.263***	0.057	0.028
	(0.094)	(0.067)	(0.023)	(0.026)	(0.080)	(0.053)	(0.038)	(0.042)
Constant	−4.506***	−4.307***	−0.417	−0.267	−3.672***	−3.325***	−0.010	0.421
	(1.234)	(0.974)	(0.437)	(0.657)	(1.047)	(0.768)	(0.716)	(0.848)
Observations	5,898	5,898	954	954	4,550	4,550	551	551
Adjusted R^2	0.143	0.209	0.014	0.035	0.094	0.163	0.005	0.025
District FE	No	Yes	No	Yes	No	Yes	No	Yes
Year FE	No	Yes	No	Yes	No	Yes	No	Yes
Year of birth FE	No	Yes	No	Yes	No	Yes	No	Yes

Notes: OLS regressions. Robust standard errors in parentheses clustered by district. Dependent variable is years of education completed for school going aged respondents (between 6 and 20 years old) during the 1986 epidemic and 1990 non-epidemic year for the male (M) and female (F) DHS samples. SGA is School going aged sample. ***Significant at the 1 percent level, **Significant at the 5 percent level, *Significant at the 10 percent level.

Table 3.13: Impact of Meningitis Exposure on Age at First Marriage for School-Going Aged Respondents Married during Epidemic (1986) and Non-Epidemic (1990) Years

	Dependent Variable: Age at First Marriage					
	SGA 1986			SGA 1990		
	(1)	(2)	(3)	(4)	(5)	(6)
Meningitis Cases, F (OLS)	−0.040**	−0.044**	−0.024**	0.018	0.014	−0.027
	(0.019)	(0.019)	(0.010)	(0.060)	(0.058)	(0.042)
Constant	15.470***	15.098***	14.598***	14.962***	14.511***	14.352***
	(0.343)	(0.449)	(0.177)	(0.135)	(0.258)	(0.176)
Observations	5,898	5,898	5,898	4,550	4,550	4,550
R^2	0.016	0.054	0.093	0.0002	0.058	0.091
Meningitis Cases, M (OLS)	−0.043**	−0.025	−0.020	0.031	0.012	−0.003
	(0.018)	(0.017)	(0.019)	(0.088)	(0.081)	(0.077)
Constant	21.275***	21.183***	21.087***	19.873***	18.724***	18.661***
	(0.359)	(0.454)	(0.490)	(0.306)	(0.514)	(0.497)
Observations	954	954	954	551	551	551
R^2	0.012	0.159	0.161	0.0003	0.175	0.178
Niamey FE	No	No	Yes	No	No	Yes
Year FE	No	Yes	Yes	No	Yes	Yes
Year of birth FE	No	Yes	Yes	No	Yes	Yes

Notes: OLS regressions. Robust standard errors in parentheses clustered by district. Dependent variable is age at first marriage for school going aged respondents (between 6 and 20 years old) during the 1986 epidemic and 1990 non-epidemic years. SGA is School going aged sample. Meningitis Cases are mean weekly meningitis cases by district for 1986 and 1990. ***Significant at the 1 percent level, **Significant at the 5 percent level, *Significant at the 10 percent level.

Table 3.14: Impact of Precipitation Shocks on Education (1986 Epidemic Year), Robustness Check

	Dependent Variable: Years of Education			
	Precipitation Shocks			
	(1)	(2)	(3)	(4)
Female	-0.627^{***}	-0.586^{***}	-0.629^{***}	-0.588^{***}
	(0.054)	(0.051)	(0.055)	(0.052)
Precipitation exposure at ages 0-5	4,418.914	4,177.179	3,631.882	3,489.979
	(10,535.300)	(14,663.360)	(10,685.870)	(14,982.720)
x Female		302.557		103.632
		(23,790.900)		(23,891.740)
Precipitation exposure at ages 6-12	−6,873.454	16,197.320	−7,076.934	14,918.590
	(36,673.780)	(44,305.270)	(36,943.370)	(44,219.350)
x Female		−43,598.290		−41,565.950
		(29,754.670)		(29,652.610)
Precipitation exposure at ages 13-20	18,666.090	75,568.230	19,082.770	76,856.420
	(60,021.180)	(93,851.200)	(60,384.590)	(94,692.870)
x Female		−95,606.920		−97,078.000
		(62,286.980)		(62,969.290)
Constant	1.056^{***}	1.036^{***}	1.139^{***}	1.119^{***}
	(0.180)	(0.180)	(0.230)	(0.229)
District fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes	Yes
Temperature quartile dummies	No	No	Yes	Yes
Observations	43,814	43,814	43,814	43,814
R^2	0.210	0.211	0.214	0.215

Notes: Regressions estimated by OLS. Robust standard errors in parentheses clustered by district. Dependent variable is years of education across all specifications. The Precipitation exposure explanatory variable is precipitation deviation exposure, defined as average district level precipitation in 1986 differenced from national mean level precipitation for cohort at specified ages during the 1986 epidemic year. Precipitation units are in $kgm^{-2}s^{-1}$. Mean level of education in the sample is 1.22, and the standard deviation is 2.7. Mean level of education for boys in the sample is 1.51 and the mean level of education for girls in the sample is 0.94. ***Significant at the 1 percent level, **Significant at the 5 percent level, *Significant at the 10 percent level.

Table 3.15: Meningitis Exposure, Wealth and Age at First Marriage for Female School-Going Aged Respondents Married during Epidemic (1986) and Non-Epidemic (1990) Years, Robustness Check

	Dependent Variable: Age at First Marriage			
	SGA 1986		SGA 1990	
	(1)	(2)	(3)	(4)
Meningitis Cases	−0.027***	−0.019*	−0.018	−0.046
	(0.009)	(0.010)	(0.035)	(0.040)
Wealth Quintile 2 (**WQ2**)	0.074	0.389**	0.042	0.133
	(0.111)	(0.186)	(0.142)	(0.206)
Wealth Quintile 3 (**WQ3**)	−0.022	0.029	0.058	−0.023
	(0.099)	(0.189)	(0.122)	(0.154)
Wealth Quintile 4 (**WQ4**)	0.301***	0.439**	0.279**	0.208
	(0.105)	(0.175)	(0.116)	(0.149)
Wealth Quintile 5 (**WQ5**)	1.363***	1.488***	1.158***	1.038***
	(0.152)	(0.285)	(0.141)	(0.159)
Meningitis Cases x **WQ2**		−0.025***		−0.067
		(0.009)		(0.062)
Meningitis Cases x **WQ3**		−0.004		0.057
		(0.011)		(0.037)
Meningitis Cases x **WQ4**		−0.012		0.050
		(0.010)		(0.057)
Meningitis Cases x **WQ5**		−0.011		0.088
		(0.018)		(0.092)
Constant	14.280***	14.193***	13.998***	14.039***
	(0.159)	(0.160)	(0.161)	(0.163)
Observations	5,838	5,838	4,500	4,500
R^2	0.128	0.129	0.119	0.120
Niamey FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes

Notes: OLS regressions. Robust standard errors in parentheses clustered by district. Dependent variable is age at first marriage for school going aged respondents (between 6 and 20 years old) during the 1986 epidemic and 1990 non-epidemic years. SGA is School going aged sample. Meningitis Cases are mean weekly meningitis cases by district for 1986 and 1990. Wealth quintiles are estimated from wealth scores from principal components analysis. WQ1 is dropped as the comparison group. ***Significant at the 1 percent level, **Significant at the 5 percent level, *Significant at the 10 percent level.

3.10 Bibliography

1. Afifi, Tamer. 2011. "Economic or Environmental Migration? The Push Factors in Niger." *International Migration* 49 (s1): e95–e124.

2. Almond, Douglas. 2006. "Is the 1918 Influenza Pandemic Over? Long-term Effects of In Utero Influenza Exposure in the Post-1940 US Population." *Journal of Political Economy* 114 (4): 672–712.

3. Almond, Douglas, Lena Edlund, and Marten Palme. 2009. "Chernobyl's Subclinical Legacy: Prenatal Exposure to Radioactive Fallout and School Outcomes in Sweden." *The Quarterly Journal of Economics* 124 (4): 1729–1772.

4. Archibong, Belinda, and Francis Annan. 2017. "Disease and Gender Gaps in Human Capital Investment: Evidence from Niger's 1986 Meningitis Epidemic." *American Economic Review* 107 (5): 530–35.

5. Ashraf, Nava, Natalie Bau, Nathan Nunn, and Alessandra Voena. 2016. "Bride Price and Female Education. NBER Working Paper No. 22417.

6. Barcellos, Silvia Helena, Leandro S Carvalho, and Adriana Lleras-Muney. 2014. "Child Gender and Parental Investments in India: Are Boys and Girls Treated Differently?" *American Economic Journal: Applied Economics* 6 (1): 157–189.

7. Barro, Robert J, and Jong Wha Lee. 2013. "A New Data Set of Educational Attainment in the World, 1950–2010." *Journal of Development Economics* 104: 184–198.

8. Becker, Gary S, Kevin M Murphy, and Robert Tamura. 1990. "Human Capital, Fertility, and Economic Growth." *Journal of Political Economy* pp. 98 (no. 5; Part 2): S12-S37.

9. Bjorkman-Nyqvist, Martina. 2013. "Income Shocks and Gender Gaps in Education: Evidence from Uganda." *Journal of Development Economics* 105: 237–253.

10. Broome, Claire V, Michael A Rugh, Adamou A Yada, LeVan Giat, Hien Giat, Jean Marie Zeltner, Warren R Sanborn, and DavidWFraser. 1983. "Epidemic Group C Meningococcal Meningitis in Upper Volta, 1979." *Bulletin of the World Health Organization* 61 (2): 325.

11. Colombini, Anais, Fernand Bationo, Sylvie Zongo, Fatoumata Ouattara, Ousmane Badolo, Philippe Jaillard, Emmanuel Seini, Bradford D Gessner, and Alfred Da Silva. 2009. "Costs for Households and Community Perception of Meningitis Epidemics in Burkina Faso." *Clinical Infectious Diseases* 49 (10): 1520–1525.

12. Voena, Alessandra, and Lucia Corno. 2015. "Selling Daughters: Age at Marriage, Income Shocks and Bride Price Tradition." Meeting Papers 1089, Society for Economic Dynamics.

13. Corno, Lucia, Nicole Hildebrandt, and Alessandra Voena. 2016. "Weather Shocks, Age of Marriage and the Direction of Marriage Payments." DISCE - Working Papers del Dipartimento di Economia e Finanza def040, Università Cattolica del Sacro Cuore, Dipartimenti e Istituti di Scienze Economiche (DISCE).

14. Garcia-Pando, Carlos Perez, Madeleine C Thomson, Michelle C Stanton, Peter J Diggle, Thomas Hopson, Rajul Pandya, Ron L Miller, and Stephane Hugonnet. 2014. "Meningitis and Climate: from Science to Practice." *Earth Perspectives* 1 (1): 14.

15. Glewwe, Paul, and Edward A Miguel. 2007. "The Impact of Child Health and Nutrition on Education in Less Developed Countries." *Handbook of Development Economics* 4: 3561–3606.

16. Hartmann-Mahmud, Lori. 2011. "Pounding Millet During School Hours: Obstacles to Girls? Formal Education in Niger." *The European Journal of Development Research* 23 (3): 354– 370.

17. Islam, Asadul, and Pushkar Maitra. 2012. "Health Shocks and Consumption Smoothing in Rural Households: Does Microcredit have a Role to Play?" *Journal of Development Economics* 97 (2): 232–243.

18. Janghorbani, Mohsen, Anthony J Hedley, Raymond B Jones, Motahareh Zhianpour, and W Harper Gilmour. 1993. "Gender Differential in all-cause and Cardiovascular Disease Mortality." *International Journal of Epidemiology* 22 (6): 1056–1063.

19. Jayachandran, Seema, and Adriana Lleras-Muney. 2009. "Life Expectancy and Human Capital Investments." *The Quarterly Journal of Economics* 124 (1): 349–397.

20. LaForce, F Marc, Neil Ravenscroft, Mamoudou Djingarey, and Simonetta Viviani. 2009. "Epidemic meningitis due to Group A Neisseria meningitidis in the African meningitis belt." *Vaccine* 27: B13–B19.

21. Loaiza Sr, E, and Sylvia Wong. 2012. "Marrying too Young. End Child Marriage." UNFPA Report.

22. Miguel, Edward, and Michael Kremer. 2004. "Worms: Identifying Impacts on Education and Health in the Presence of Treatment Externalities." *Econometrica* 72 (1): 159–217.

23. Perez Garcia Pando, Carlos, Michelle C Stanton, Peter J Diggle, Sylwia Trzaska, Ron L Miller, Jan P Perlwitz, Jose M Baldasano, Emilio Cuevas, Pietro Ceccato, Pascal Yaka et al. 2014. "Soil Dust Aerosols and Wind as Predictors of Seasonal Meningitis Incidence in Niger." *Environmental Health Perspectives* 122 (7):679-86.

24. Schlenker, Wolfram, and Michael J Roberts. 2009. "Nonlinear Temperature Effects Indicate Severe Damages to US crop Yields under Climate Change." *Proceedings of the National Academy of Sciences* 106 (37): 15594–15598.

25. Schultz, T Paul. 2002. "Why Governments Should Invest More to Educate Girls." *World Development* 30 (2): 207–225.

26. Sen, Amartya. 1998. "Mortality as an Indicator of Economic Success and Failure." *The Economic Journal* 108 (446): 1–25.

27. Tonthola, Annie Natasha. 2016. "Natasha Annie Tonthola: My fight against Malawi's 'hyenas'." *BBC News Magazine*, October 25.

28. Trotter, Caroline L, and Brian M Greenwood. 2007. "Meningococcal Carriage in the African Meningitis Belt." *The Lancet Infectious Diseases* 7 (12): 797–803.

29. Yaka, Pascal, Benjamin Sultan, Helene Broutin, Serge Janicot, Solenne Philippon, and Nicole Fourquet. 2008. "Relationships Between Climate and Year-to-Year Variability in Meningitis Outbreaks: A Case Study in Burkina Faso and Niger." *International Journal of Health Geographics* 7 (1): 34.

3.11 Appendix

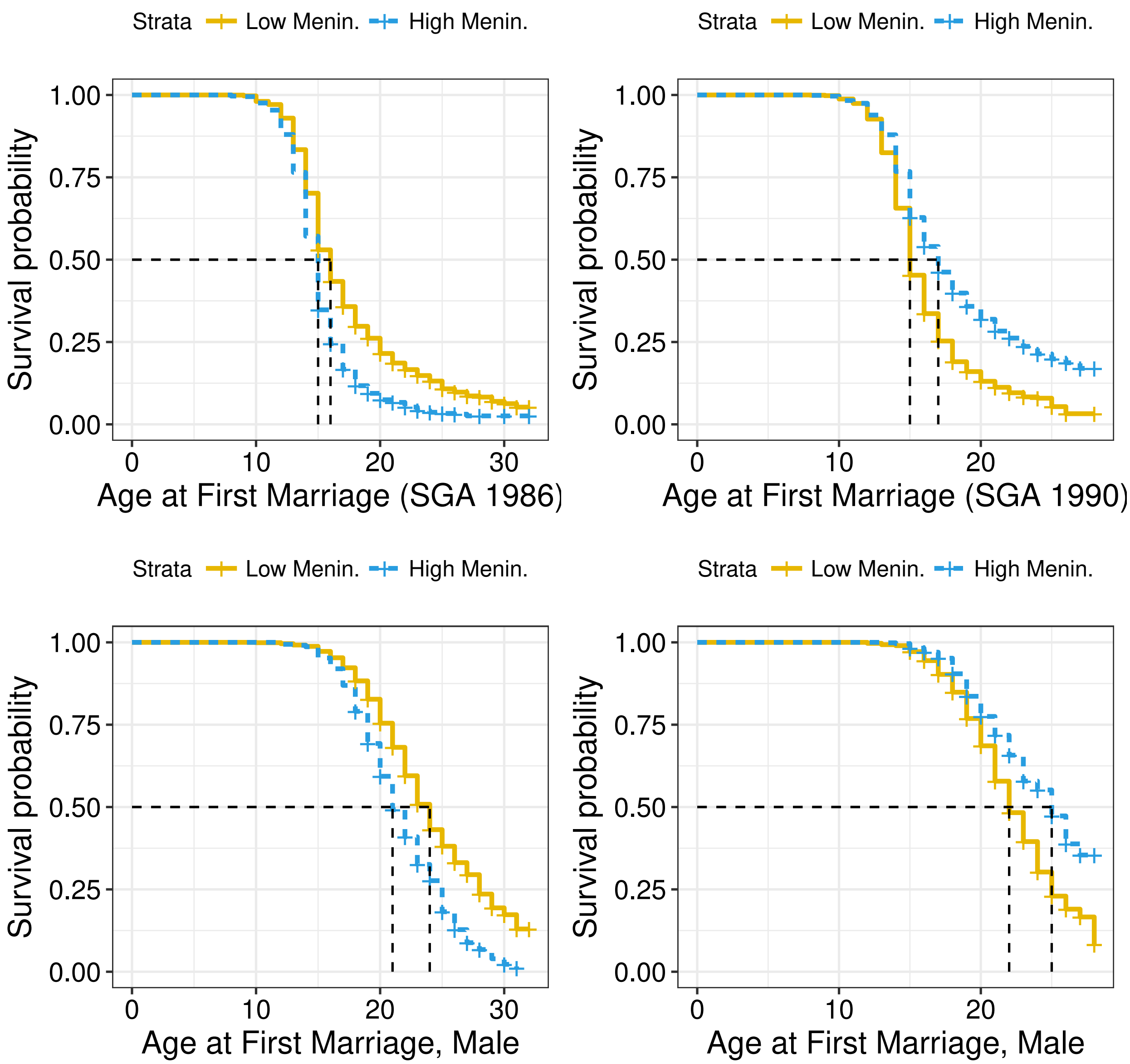


Figure 3.7: Age of First Marriage Survival Probability for School-Going Aged (SGA) Populations by Meningitis Exposure in Epidemic (1986) and Non-epidemic (1990) Years

Table 3.16: Difference in Difference Estimates of the Impact of Repeated Meningitis Exposure on Education (relative to 1986 Epidemic Year), Robustness Check

	Dependent Variable: Years of Education							
	Meningitis Exposure							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
female	−0.647***	−0.537***	−0.646***	−0.523***	−0.647***	−0.558***	−0.645***	−0.588***
	(0.051)	(0.066)	(0.050)	(0.073)	(0.051)	(0.062)	(0.051)	(0.060)
case86_05	−0.0001	0.0005						
	(0.001)	(0.002)						
female * case86_05		−0.001						
		(0.002)						
case86_612	−0.005	0.004						
	(0.005)	(0.007)						
female * case86_612		−0.017***						
		(0.004)						
case86_1320	−0.011	−0.005						
	(0.008)	(0.008)						
female * case86_1320		−0.011**						
		(0.004)						
case086_05			−0.002	0.001				
			(0.003)	(0.004)				
female * case086_05				−0.004				
				(0.006)				
case086_612			−0.022	−0.001				
			(0.015)	(0.019)				
female * case086_612				−0.041***				
				(0.011)				
case086_1320			−0.040	−0.024				
			(0.025)	(0.025)				
female * case086_1320				−0.028***				
				(0.010)				
case186_05					−0.001	0.001		
					(0.003)	(0.004)		
female * case186_05						−0.004		
						(0.005)		
case186_612					−0.015	0.005		
					(0.011)	(0.016)		
female * case186_612						−0.039***		
						(0.012)		
case186_1320					−0.026	−0.012		
					(0.016)	(0.017)		
female * case186_1320						−0.024**		
						(0.010)		
case286_05							0.005	0.002
							(0.004)	(0.006)
female * case286_05								0.006
								(0.007)
case286_612							0.016	0.047***
							(0.010)	(0.017)
female * case286_612								−0.059***
								(0.019)
case286_1320							0.006	0.029
							(0.016)	(0.024)
female * case286_1320								−0.039**
								(0.019)
Constant	1.062***	1.004***	1.041***	0.976***	1.061***	1.014***	1.043***	1.014***
	(0.204)	(0.215)	(0.201)	(0.213)	(0.204)	(0.213)	(0.203)	(0.203)
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	47,697	47,697	47,697	47,697	47,697	47,697	47,697	47,697
R^2	0.206	0.208	0.207	0.209	0.206	0.208	0.205	0.207

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table 3.17: Mechanism Check: Impact of Meningitis Exposure on Number of Wives for School-Going Aged Respondents Married during Epidemic (1986) and Non-epidemic (1990) Years

	Dependent Variable: Nos. of Wives			
	SGA 1986 F	SGA 1986 M	SGA 1990 F	SGA 1990 M
	(1)	(2)	(3)	(4)
Meningitis Cases	0.006***	0.0003	−0.001	0.005
	(0.002)	(0.002)	(0.007)	(0.007)
Constant	0.414***	1.094***	0.302***	1.017***
	(0.037)	(0.051)	(0.048)	(0.028)
Observations	5,573	906	4,322	515
R^2	0.032	0.042	0.023	0.051
Niamey FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes

Notes: OLS regressions. Robust standard errors in parentheses clustered by district. Dependent variable is number of wives for school going aged respondents (between 6 and 20 years old) during the 1986 epidemic and 1990 non-epidemic year for the male (M) and female (F) DHS samples. SGA is School going aged sample. ***Significant at the 1 percent level, **Significant at the 5 percent level, *Significant at the 10 percent level.

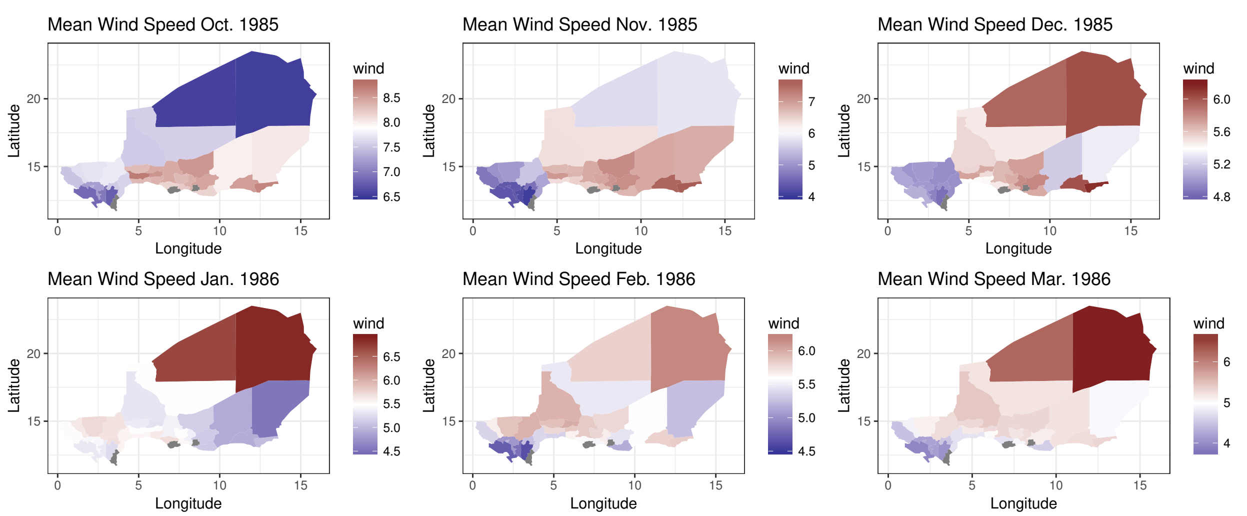


Figure 3.8: Harmattan Wind by District 1985-1986

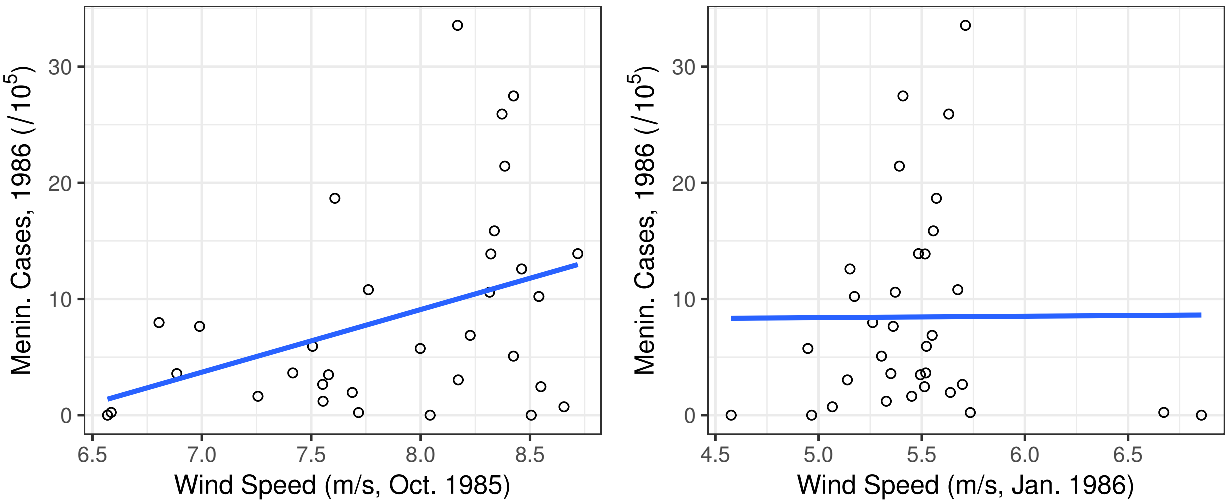
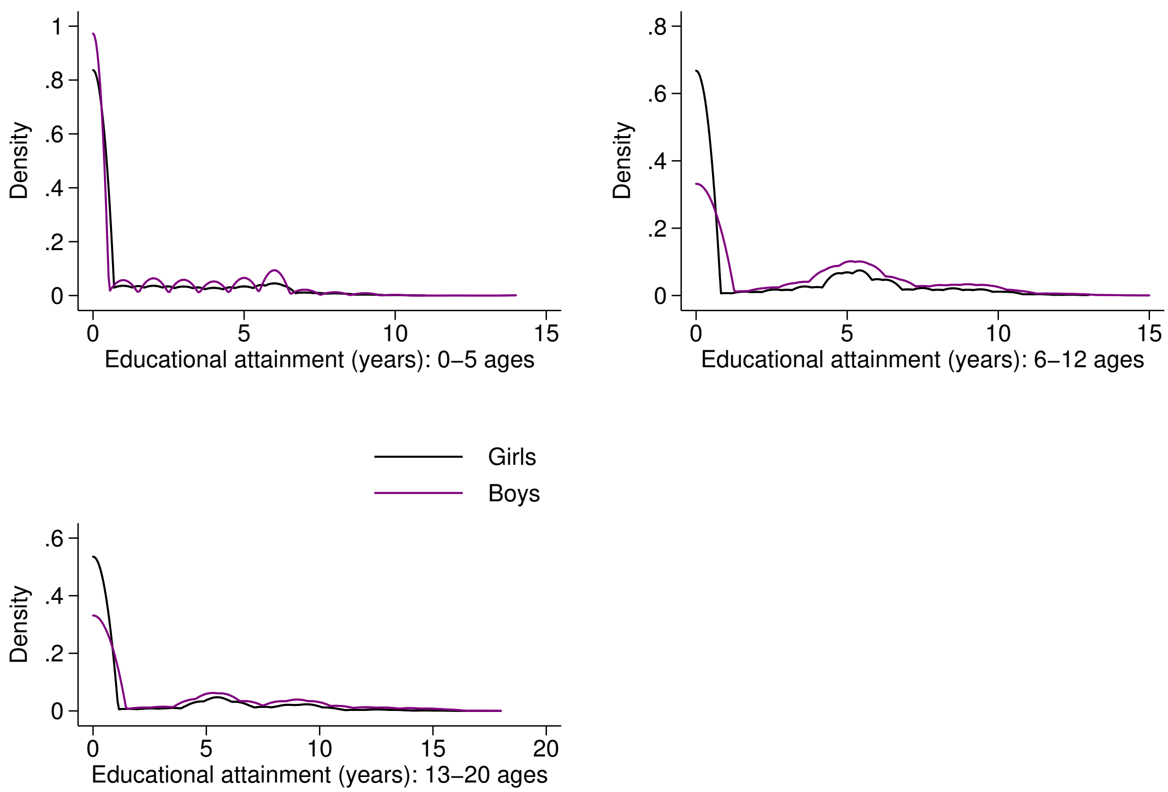


Figure 3.9: Harmattan Wind and Meningitis Outbreaks, 1985-1986



NOTE: We reject the null that the distributions are different at 5% level across the various age cohorts.

Figure 3.10: Distribution of Schooling across Cohorts and Gender

Chapter 4

Informal Risk Sharing and Index Insurance: Theory with Experimental Evidence

Francis Annan♣
(joint with Bikramaditya Datta♠)

Abstract*

When does informal risk sharing act as barrier or support to the take-up of index-based insurance? We evaluate this substitutability or complementarity interaction by considering the case of an individual who endogenously chooses to join a group and make decisions about index insurance. The presence of an individual in a risk sharing arrangement reduces his risk aversion, termed "Effective Risk Aversion"— a sufficient statistic for index decision making. Our analysis establishes that such reduction in risk aversion can lead to either reduced or increased take up of index insurance. These results provide alternative explanations for two empirical puzzles: unexpectedly low take-up for index insurance and demand being particularly low for the most risk averse. Experimental evidence based on data from a panel of field trials in India, lends support for several testable hypotheses that emerge from our baseline analysis.

JEL Classification Codes: D7, D14, D81, G22, Q14

Keywords: *Contracts, Informal Risk Sharing, Index Insurance, Effective Risk Aversion, Matching*

---

* We appreciate guidance and support from Patrick Bolton, Emily Breza, Christian Gollier, Wojciech Kopczuk, Dan Osgood, Bernard Salanié and seminar participants in the Applied Micro Theory and Financial Economics colloquiums at Columbia University. All remaining errors are ours.

♣ Columbia University. Email: `fa2316@columbia.edu`

♠ Columbia University. Email: `bd2362@columbia.edu`

"*...and when basis risk is large, having an informal network can help by providing insurance against basis risk. Thus the presence of informal risk sharing actually increases demand for index-based insurance in the presence of basis risk...*" -- **World Development Report (2014)**

4.1 Introduction

The business of agriculture is inherently risky, particularly for the poor, due to a myriad of unpredictable weather and climate events. Recently, innovative index-based weather insurance has emerged as a way to help society insure against weather related events.[1] A standard index-based contract pays out when some constructed-index falls below or above a given non-manipulable threshold.[2]

The justification for index insurance is that it overcomes several market frictions e.g., moral hazard, that plague traditional indemnity-based insurance and financial instruments. Index-based insurance differs in the sense that the contractual terms (premiums and payouts) are based on publicly observable and non-manipulable index (local weather). However, this innovation comes with a cost: "basis risk". In particular, there is a potential mismatch between the payouts triggered by the local weather and the actual losses associated with weather realizations of the insurance policy holder. This mismatch or "basis risk" arises because weather realized on an individual farm unit may not perfectly correlate with the local weather index—whose construction is typically based on observations recorded at weather

[1] The design and coverage for index-based weather insurance can be wide ranging. Hazell et al. (2010) cites at least 36 pilot index insurance projects that were underway in 21 developing countries. Examples include: India–rainfall insurance (Mobarak and Rosenzweig 2012; Cole et al. 2013); Ethiopia–rainfall (Hazell et al. 2010; McIntosh et al. 2013; Duru 2016); China–drought and extreme temperature (Hazzel et al. 2010); Mexico–drought and excess moisture (Hazell et al. 2010); Ghana–rainfall (Karlan et al. 2014); Kenya and Ethiopia–"livestock" weather-insurance (Jensen et al. 2014).

[2] See Carter et al. (2017) for a recent survey about index insurance in developing countries.

stations that surround the policy holder.[3]

Empirical studies about weather index-based insurance are growing (e.g., Cai et al. 2009; Giné and Yang 2009; Cole et al. 2013; Karlan et al. 2014), which in turn have noted two fundamental puzzles. The first is that, demand for index products has been lower than expected. The second is that, the demand seems to be especially low from the most risk averse consumers. Despite its promise, scaling up index insurance will require our understanding about the various constraints to its take-up. Several candidate reasons for the low demand have been offered including: financial illiteracy, lack of trust, poor marketing, credit constraints, present bias, complexity of index contracts, "basis risk" and price effects.

Another suggested explanation for the thin index insurance market in poor populations is pre-existing informal risk-sharing arrangements. Indeed, the extent to which informal risk-sharing networks affect the demand for index-based insurance remains an open question, both empirically and theoretically. In this paper, we focus on microfounded reasons underlying the relation between informal risk schemes and formal index insurance. Specifically, we ask: *When does an informal risk sharing scheme impede or support the take-up of formal index insurance?* We analyze this question in an environment where an individual endogenously chooses to join an informal group and make purchase decisions about index insurance. Our analysis show that the presence of an individual in a risk sharing arrangement reduces his risk aversion — a phenomenon we term "Effective Risk Aversion". The paper documents that "Effective Risk Aversion" is a paramount statistic that underlies individual's purchase decisions about index-based insurance.

Appealing to "Effective Risk Aversion", it is shown that informal schemes may either reduce or increase the take-up of index insurance. The main intuition follows from the simple observation that in the presence of a risk-sharing arrangement, an individual's risk tolerance is higher.[4] This has two implications for the take-up of index insurance. First,

[3]Satellite measurements are used in some cases (e.g., Carter et al. 2017; IRI 2013). Even so, the individual weather realizations is not perfectly correlated with the satellite index.

[4]This intuition is comparable to Itoh (1993), who studies optimal incentive contracts in a group. He shows that side contracts can serve as mutual insurance for members in a group and can induce effort at

the individual being more risk-tolerant makes him less willing to buy insurance. Second, the individual becomes more tolerant to the basis risk, and so is more likely to take-up. These two forces have opposite effects on the decision to purchase index insurance. Consider the case of a highly risk averse individual who will not buy index insurance if acting alone because of his sensitivity to basis risk. Being in a group reduces his risk aversion "effectively" making him more tolerant towards basis risk and thus more likely to purchase index insurance. Now consider the case of an individual with intermediate risk aversion who would buy index insurance if acting alone. The presence of informal insurance may crowd out his take-up for index insurance due to his lower willingness to pay. Our analysis thus has implications for informal schemes acting as a substitute or complement to index insurance.

Several testable hypotheses emerge from our theoretical analysis, which are useful for the design of index insurance contracts and understanding the development or commercial success of such innovative financial products. We develop a tractable empirical framework to investigate these hypotheses using data from a panel of field experimental trials in rural India. First, we provide empirical evidence that the overall effect of informal risk-sharing on the take-up of index insurance is ambiguous. There is evidence that informal risk sharing schemes may support take-up, finding that when downside basis risk is high, risk-sharing increases the index demand by approximately 13 to 40 percentage points. In addition, we provide evidence that the existence of risk-sharing arrangement makes individuals more sensitive to price changes, with an estimated increased elasticity of about 0.34.

Finally, we show that an increase in the size of risk-sharing groups decreases take-up. This effect is stronger once we have conditioned on basis risk – a counter force. Strikingly, this result stand in contrast to standard information diffusion models, in which an increase in exposed group size should facilitate uptake of index insurance (e.g., Jackson and Yariv 2010; Banerjee et al. 2013). For example, Banerjee et al. (2013) show that information passage or diffusion within a social network increases the likelihood of participation in a

a cheaper cost when members of the group can monitor each other's effort by coordinating their choice of effort. While Itoh (1993) looks at effort decisions, we analyze insurance decisions.

microfinance program across 43 villages in South India. Similarly, Cole, Tobacman and Stein (2014) attributed the observed increase in take-up of index insurance to information generated by village-wide insurance payouts. Our analysis documents that the effective reduction in risk aversion following individuals' exposure to risk-sharing group treatments explains the findings.

Our paper is related to the broader literatures on risk sharing (e.g., Itoh 1993; Townsend 1994; Munshi 2011; Munshi and Rosenzweig 2009 and many subsequent others), take-up of index insurance (e.g., Giné, Townsend and Vickery 2008; Mobarak and Rosenzweig 2012; Cole et al. 2013; Cole, Stein and Tobacman 2014; Karlan et al. 2014; Clarke 2016; Casaburi and Willis 2017) and the linkages between informal institutions and formal markets (e.g., Arnott and Stiglitz 1991; Kranton 1996; Duru 2016). Clarke (2016) studies the relation between individual risk aversion and the take-up of index insurance. He finds that demand is hump-shaped with demand for the index being higher in the intermediate risk averse region. Unlike Clarke (2016), we incorporate pre-existing risk-sharing arrangements to study their effect on the take-up.

Perhaps, most related is Mobarak and Rosenzweig (2012), who show that the existence of informal risk-sharing networks increases demand for index insurance, consistent with their empirical analysis. Our paper is distinct in several ways. Our model is microfounded, allowing for heterogeneity among individuals and endogenous decisions to join risk sharing groups. Results are based on the notion of "Effective Risk Aversion"—a consequence of efficient risk sharing. This allows us to identify new channels underlying the effect of informal schemes on demand for formal index insurance, and provides novel explanations for the two empirical puzzles based on their interactions. As mentioned previously, one of our channels relates to the increase in tolerance to basis risk, implying an increase in take-up - this reaffirms previous results found in Mobarak and Rosenzweig suggesting that informal risk sharing schemes support take-up of formal index insurance. The additional channel is connected to the increase in tolerance to aggregate gambles, implying a reduced demand for index insurance.

Finally, we analyze the take-up of index insurance at the extensive margin, unlike Mobarak and Rosenzweig (2012) and Clarke (2016) who looked at the intensive margin.

The rest of the paper is organized as follows. Section 2 presents the model. Results from several analysis are contained in Sections 3 and 4. Section 5 presents testable hypotheses from our model and investigates them empirically using field experimental data for a specific index contract "rainfall insurance". Section 6 concludes. All formal proofs, tables and figures are relegated to the Appendix.

4.2 The Model

To investigate the coexistence and interactions between pre-existing (informal) institutional risk sharing and (formal) index-based insurance, it is crucial to specify preferences, shocks and informal arrangements in the economy.

Setup

We consider an individual i with absolute risk aversion parameter $\gamma_i > 0$ and receive utility $u_i(z) = -e^{-\gamma_i z}$ from consuming income z. The individual faces uncertain income realization according to

$$z_i = w_i + h_i$$

where w_i and h_i denotes the deterministic and the stochastic component of the individual's income. The stochastic component consists of two parts, $h_i = \varepsilon_i + v$: where ε_i is the individual's idiosyncratic risk (e.g., disease shocks), and v is the aggregate shock (e.g., drought, rainfall). As we describe below, ε_i corresponds to the part of the stochastic component which can be insured via informal risk-sharing while v corresponds to the portion that can be insured via formal index insurance. We assume the following

$$\varepsilon_i \sim N(0, \sigma_i^2)$$

$$v = \begin{cases} 0 & \text{with probability } 1-p \\ -L & \text{with probability } p \end{cases}$$

Informal risk sharing: There exists a group g that individual i has the option to join. We think of the group as a representative agent with a CARA utility function and absolute risk aversion denoted by γ_g. We denote the income realization of that group as

$$z_g(\epsilon) = w_g + h_g$$

where w_g and $h_g \sim N(0, \sigma_g^2)$ denotes the deterministic and the stochastic component of the group's income. In this case, the stochastic component can only be insured through risk-sharing arrangements. Following Udry (1990), we assume perfect information: group-idiosyncratic variances are public information and the realizations of shocks are also perfectly observed by all individuals when they occur in the society. This provides enforcement for the informal relationships.

Individual i has the choice of entering into a risk-sharing arrangement with the group. An unmatched individual receives his random income. If the individual joins the group, he can enter into a binding agreement prior to the realization of their incomes, specifying how their pooled income is going to be shared. [5]

[5]The model thus reflects several practical contexts including the case where cooperatives buy index insurance for their members. To illustrate: an index contract package was designed for groundnut farmers in Malawi for a 1 acre of production. Eligibility requires a farmer to be within $20km$ of one of the meteorological stations in the program. This package consists of a loan (of about 4500 Malawi Kwacha or US$35) that covers the cost of groundnut seed (of about US$25, International Crops Research Institute for the Semi-Arid Tropics [ICRISAT] bred), the index insurance premium (about US$2), and tax (about US$0.50). After signing the paperwork, the farmer receives a bag of groundnut seed which is deemed sufficient for 1 acre of production and an insurance certificate for a payout policy that maxes at the loan size plus interest

Index Insurance: There are no financial markets allowing any individual to insure himself against his idiosyncratic risks. However, with the introduction of index-weather based insurance it is possible to insure against v. Aggregate shocks can be insured by *formal* index-based insurance which is subject to basis risk (e.g., Cole et al. 2013). We model basis risk as in Clarke (2016):

(~US$7). Prices vary by the weather station and crop. In this program, farmers are organized into joint liability "groups" of about 10-20 members. Farmers plant the groundnut seed, and then at the end of the production season provide their yields to the farm association or cooperative, which markets the yields. The proceeds and insurance payouts are then used to pay for the loan, and any remaining profits are returned to the farmer–net of any loan deductions. Similar contract developments involving groups decisions are ongoing in Kenya and Tanzania, among others (see e.g., Osgood et al. 2007).

Table 4.1: Two Sided Basis Risk: Joint Probability Structure

	Index=0	Index=1	
$v = 0$	$1 - q - r$	$q + r - p$	$1 - p$
$v = -L$	r	$p - r$	p
	$1 - q$	q	

Where in Table 1: individual i suffers aggregate risk which can take the value 0 with probability $1-p$ or $-L$ with probability p. There is also an index which can take the value 1 (i.e., payout) with probability q or 0 (i.e., no payout) with probability $1-q$. As usual, the index may not be perfectly correlated with the aggregate risk and so there are four possible joint realizations of the aggregate risk and index. In this case, r denotes the probability that a negative aggregate shock is realized but the index suggests no payouts. This corresponds to the downside basis risk faced by the consumer if he purchases index insurance. Similarly, $q+r-p$ corresponds to an upside basis risk where an insured agent does not suffer an aggregate shock and yet payouts are triggered. Note that both downside and upside basis risks are increasing in r. We also assume that the index is informative about the aggregate loss that is $Prob(v=0,I=0)\times Prob(v=1,I=1) > Prob(v=0,I=1)\times Prob(v=1,I=0)$ which implies that $r < p(1-q)$.

4.3 Demand for Index Insurance: no informal access

Suppose that individual i is faced with the choice of either buying index insurance, denoted by **1** or not, denoted by **0**. We first consider the case where the individual does not have access to an informal risk-sharing arrangement. In order to determine demand for index insurance, we compare the certainty equivalents for buying versus not buying the index. Formally, consider individual i whose income process is given by

$$z_i^0(\epsilon) = w_i + \varepsilon_i + v$$

where the independent shocks are

$$\varepsilon_i \sim N(0, \sigma_i^2)$$

$$v = \begin{cases} 0 & \text{with probability } 1-p \\ -L & \text{with probability } p \end{cases}$$

If individual does **not buy** the index: the expected utility of individual i is

$$\begin{aligned} E(-e^{-\gamma_i z_i^0}) &= E(-e^{-\gamma_i(w_i+\varepsilon_i+v)}) \\ &= -E(e^{-\gamma_i w_i})E(e^{-\gamma_i \varepsilon_i})E(e^{-\gamma_i v}) \\ &= -e^{-\gamma_i w_i} e^{\frac{\gamma_i^2\sigma_i^2}{2}}([1-p] + pe^{\gamma_i L}) \end{aligned}$$

For individual i with CARA utility function with income z_i, we derive the certainty equivalent (CE_i) according to:

$$-e^{-\gamma_i CE_i} = E(-e^{-\gamma_i z_i})$$

Thus, the certainty equivalent for individual with no index insurance is given by

$$\begin{aligned} CE_i^0 &= -\frac{1}{\gamma_i}\log E(e^{-\gamma_i z_i^0}) \\ &= -\frac{1}{\gamma_i}(-\gamma_i w_i + \frac{\gamma_i^2\sigma_i^2}{2} + \log([1-p] + pe^{\gamma_i L}) \\ &= w_i - \frac{\gamma_i\sigma_i^2}{2} - \frac{1}{\gamma_i}\log([1-p] + pe^{\gamma_i L}) \end{aligned}$$

If the individual buys insurance he pays a fixed premium π and receives a stochastic payout η which depends on the level of coverage and on the value of the index. If the individual buys index insurance and the Index=1, the insurance company pays the individual βL. For Index=0, there is no transfer from the insurance company to the individual. Thus, the actuarially fair premium is $q\beta L$. Due to loading, administrative costs and lack of competition, the premium is typically not actuarially fair. This is captured as $\pi = mq\beta L$ for $m > 1$.

If the individual buys insurance, his income process is now given by:

$$z_i^1(\epsilon) = w' + \varepsilon_i + v'$$

where $w' \equiv w_i - \pi$ and $v' \equiv v + \eta$. Thus v' and ε_i are independent and the distribution of v' is given by

$$v' = \begin{cases} 0 & \text{with probability } 1-q-r \\ -L & \text{with probability } r \\ \beta L & \text{with probability } q+r-p \\ -L+\beta L & \text{with probability } p-r \end{cases}$$

So, if the individual **buys** the index: the expected utility is

$$\begin{aligned} E(-e^{-\gamma_i z_i^1}) &= E(-e^{-\gamma_i(w'+\varepsilon_i+v')}) \\ &= -E(e^{-\gamma_i w'})E(e^{-\gamma_i \varepsilon_i})E(e^{-\gamma_i v'}) \\ &= -e^{-\gamma_i w'}e^{\frac{\gamma_i^2\sigma_i^2}{2}}([1-q-r]+re^{\gamma_i L}+[q+r-p]e^{-\gamma_i\beta L}+[p-r]e^{-\gamma_i(-L+\beta L)}) \end{aligned}$$

Thus, the certainty equivalent for individual with index insurance is given by

$$\begin{aligned} CE_i^1 &= -\frac{1}{\gamma_i}\log E(e^{-\gamma_i z_i^1}) \\ &= -\frac{1}{\gamma_i}(-\gamma_i w' + \frac{\gamma_i^2\sigma_i^2}{2} + \log([1-q-r]+re^{\gamma_i L}+[q+r-p]e^{-\gamma_i\beta L}+[p-r]e^{-\gamma_i(-L+\beta L)}) \\ &= w' - \frac{\gamma_i\sigma_i^2}{2} - \frac{1}{\gamma_i}\log([1-q-r]+re^{\gamma_i L}+[q+r-p]e^{-\gamma_i\beta L}+[p-r]e^{-\gamma_i(-L+\beta L)}) \end{aligned}$$

Thus, the individual buys insurance if $CE_i^1 \geq CE_i^0$. Using the expressions for CEs from above this condition can be rewritten as

$$w' - \frac{\gamma_i\sigma_i^2}{2} - \frac{1}{\gamma_i}\log([1-q-r]+re^{\gamma_i L}+[q+r-p]e^{-\gamma_i\beta L}+[p-r]e^{-\gamma_i(-L+\beta L)}) \geq w_i - \frac{\gamma_i\sigma_i^2}{2} - \frac{1}{\gamma_i}\log([1-p]+pe^{\gamma_i L})$$
$$-mq\beta L - \frac{1}{\gamma_i}\log([1-q-r]+re^{\gamma_i L}+[q+r-p]e^{-\gamma_i\beta L}+[p-r]e^{-\gamma_i(-L+\beta L)}) \geq -\frac{1}{\gamma_i}\log([1-p]+pe^{\gamma_i L})$$
$$-\frac{1}{\gamma_i}\log([1-q-r]+re^{\gamma_i L}+[q+r-p]e^{-\gamma_i\beta L}+[p-r]e^{-\gamma_i(-L+\beta L)}) + \frac{1}{\gamma_i}\log([1-p]+pe^{\gamma_i L}) \geq mq\beta L$$

where the second inequality uses $w' \equiv w_i - \pi$. Observe that $-\frac{1}{\gamma_i}\log([1-q-r]+re^{\gamma_i L}+[q+r-p]e^{-\gamma_i\beta L}+[p-r]e^{-\gamma_i(-L+\beta L)}) = CE_i(v')$ i.e., the CE for individual faced with v' gamble. Equivalently:$-\frac{1}{\gamma_i}\log([1-p]+pe^{\gamma_i L}) = CE_i(v)$. Thus the individual buys index insurance if

$$CE_i(v') - CE_i(v) \;\geq\; mq\beta L$$

We obtain the individual's decision to buy the index in two ways: small losses (analytically) versus large losses (numerically).

4.3.1 Small Losses:

Let's suppose losses are small. Then, we can approximate the CEs as follows

$$CE_i(v) \approx -pL - \frac{1}{2}\gamma_i \sigma_v^2$$

and

$$CE_i(v') \approx -pL + \beta L q - \frac{1}{2}\gamma_i \sigma_{v'}^2$$

where the variances of v and v' are σ_v^2 and $\sigma_{v'}^2$ respectively. This means the individual buys the index if the following condition is satisfied

$$\frac{1}{2}\gamma_i(\sigma_v^2 - \sigma_{v'}^2) \geq (m-1)q\beta L$$

Since $m > 1$, the RHS is always positive. For $\sigma_{v'}^2 \geq \sigma_v^2$ the LHS is non-positive and hence the individual will not buy index insurance. $\sigma_{v'}^2$ captures two parts: reduction in variance from buying insurance and an increase in variance due to the presence of basis risk. It is therefore possible for $\sigma_{v'}^2 \geq \sigma_v^2$ depending on these effects. However even for $\sigma_{v'}^2 < \sigma_v^2$ the individual may not buy index insurance for low values of γ_i. Thus, there exist a threshold $\gamma^* = \max(0, \frac{2(m-1)q\beta L}{\sigma_v^2 - \sigma_{v'}^2})$ such that the individual with risk aversion parameter $\gamma_i < \gamma^*$ will not buy the index insurance. Since the index insurance is actuarially unfair $m > 1$ the individual suffers a reduction in expected income. However, there is a change in variance from buying index insurance. The individual compares these two forces. If the variance does not decrease then nobody buys the index. But if the variance decreases, then individuals with high risk aversion will assign more weight to this reduction in variance; hence will buy the index. Whereas for individuals with low risk aversion, this reduction in variance may not be enough to compensate for the loss in expected income; hence will not buy the index. The above discussion is summarized in Proposition 1 below

PROPOSITION 1: Consider an individual with CARA utility function and risk aversion parameter $\gamma_i > 0$. Under small losses and actuarially unfair index insurance $m > 1$, the following two results hold.

(1) The individual will purchase an index cover β *iff* $\frac{1}{2}\gamma_i(\sigma_v^2 - \sigma_{v'}^2) \geq (m-1)q\beta L$

(2) In particular, if $\sigma_{v'}^2 < \sigma_v^2$ the individual will purchase the index *iff* $\gamma_i > \gamma^* = \max(0, \frac{2(m-1)q\beta L}{\sigma_v^2 - \sigma_{v'}^2})$

4.3.2 Large Losses

So far we have been analyzing the implications of informal arrangements on the decisions to buy index insurance assuming small losses. In this subsection, we extend the analysis to the case of large losses. It is still the case that an individual with risk aversion γ_i if acting individually chooses to buy the index insurance if

$$CE_i(v') - CE_i(v) \;\;\geq\;\; mq\beta L$$

which is equivalent to

$$-\frac{1}{\gamma_i}\log([1-q-r] + re^{\gamma_i L} + [q+r-p]e^{-\gamma_i \beta L} + [p-r]e^{-\gamma_i(-L+\beta L)}) + \frac{1}{\gamma_i}\log([1-p] + pe^{\gamma_i L}) \geq mq\beta L$$

We illustrate the condition numerically in Figure 1. The red curve represents the left side of the inequality that is the difference in the CEs while the green line represents the right side of the inequality: $mq\beta L$. The x-axis represents different values for risk aversion, indicating that individuals with risk-aversion levels in between the two vertical black lines purchase index insurance. Unlike the case of small loses, the decision to buy index insurance is bounded between two $\gamma-$ thresholds. Within this interval, the above inequality is satisfied and individuals purchase the index cover. Next, observe that individuals with sufficiently high or low risk-aversion will choose not to buy index insurance. The simple intuition is that high risk-averse individuals do not buy because of the basis risk while low risk-averse individuals choose not to buy because of loading of premium ($m > 1$). This is similar to the findings of Clarke (2016) who examines purchases of index insurance at the intensive margin.

4.4 Demand for Index Insurance: informal group access

4.4.1 Informal Risk Sharing

This subsection discusses the informal risk sharing arrangements before the introduction of index insurance. Since our set up has a non-transferable utility (NTU) representation, we first show that the model has a transferable utility (TU) representation under certainty equivalents (CE). The set-up is NTU because of the heterogeneity in risk-aversion where one unit of income yields utility $u_i(1) = -exp(-\gamma_i)$ for an individual i with risk aversion γ_i, but utility $u_g(1) = -exp(-\gamma_g) \neq u_i(1)$ for a representative agent acting for the group g with risk aversion γ_g. We work with certainty equivalent units, which allows for TU representations. This is stated in the following Lemma.

LEMMA 1: The NTU model has a TU representation, where CEs are transferable across individuals (i, g).

Next, since CE is transferable, we also have the following lemma.

LEMMA 2: Suppose individual i decides to join the group g and risk is shared efficiently between them. Then under transferable CEs we can think of the pair (i, g) as a representative agent with risk aversion parameter γ_{i^*} where $\frac{1}{\gamma_{i^*}} = \frac{1}{\gamma_i} + \frac{1}{\gamma_g}$. This implies that $\gamma_{i^*} < \min(\gamma_i, \gamma_g)$.

Lemma 2 allows us to conveniently analyze the decision of individual i to take index insurance in the presence of risk sharing arrangements. It also shows that the risk aversion of the individual i will be effectively lower if he is in a group, as compared to if he was acting as an individual. The latter is summarized in Definition 1 below.

DEFINITION 1: γ_{i^*} as "Effective Risk Aversion": This refers to the risk aversion parameter for a representative agent i^* representing group consisting of (i, g) that shares risk efficiently.

REMARK: We can now examine whether it is optimal for individual i to join the group g. To do this we compare the CE of the group if they were sharing risk efficiently to the sum of CEs for the individual i and group g if they were acting separately. Indeed, joining the group provide welfare gains to the individual (and the group). The argument is similar to Wilson (1968). For contradiction: suppose that i and g are un-matched, then i and g can form a pair where each consumes his income. In this case, each is at least as well-off in the pair, as compared to remaining unmatched. However, by the mutuality principle, both can be better-off when in the group. This requires their income shares to rise and fall together with the independent random part of their incomes. The following lemma formally shows that if is efficient for i and g to form a pair.

LEMMA 3: Suppose risk is shared efficiently within a group. Then it is efficient for individual i to join group g.

4.4.2 Extensive Margin 0-1: with informal group access

Consider now the demand for index insurance for the individual who has access to informal risk-sharing arrangement. From LEMMA 2, this is the same as the demand for index insurance of a representative agent with risk aversion parameter γ_{i^*} where $\frac{1}{\gamma_{i^*}} = \frac{1}{\gamma_i} + \frac{1}{\gamma_g}$.

Thus, we can apply the preceding analysis to evaluate the decision of an individual in a group to purchase index insurance.

The representative agent's income process in the absence of index insurance is given by

$$z_{i^*}^0 = w_i + w_g + h_g + \varepsilon_i + v$$

If individual does **not buy** the index: the expected utility of representative agent is

$$E(-e^{-\gamma_{i^*} z_{i^*}^0}) = -e^{-\gamma_{i^*}(w_i + w_g)} e^{\frac{\gamma_{i^*}^2(\sigma_i^2 + \sigma_g^2)}{2}} ([1-p] + pe^{\gamma_{i^*} L})$$

and the certainty equivalent with no index insurance is given by

$$CE_{i^*}^0 = w_i + w_g - \frac{\gamma_{i^*}(\sigma_i^2 + \sigma_g^2)}{2} - \frac{1}{\gamma_{i^*}} \log([1-p] + pe^{\gamma_{i^*} L})$$

Next, if individual **buys** index insurance, the group's income process is now given by:

$$z_{i^*}^1 = w' + h_g + \varepsilon_i + v'$$

where $w'_{i^*} \equiv w_i + w_g - \pi$ and $v' \equiv v + \eta$.

If the individual buys the index: the expected utility of the representative agent is

$$E(-e^{-\gamma_{i^*} z_{i^*}^1}) = -e^{-\gamma_{i^*} w'_{i^*}} e^{\frac{\gamma_{i^*}^2(\sigma_i^2 + \sigma_g^2)}{2}} ([1-q-r] + re^{\gamma_{i^*} L} + [q+r-p]e^{-\gamma_{i^*}\beta L} + [p-r]e^{-\gamma_{i^*}(-L+\beta L)})$$

The certainty equivalent for the representative agent with index insurance is given by

$$CE_{i^*}^1 = w'_{i^*} - \frac{\gamma_{i^*}(\sigma_i^2 + \sigma_g^2)}{2} - \frac{1}{\gamma_{i^*}} \log([1-q-r] + re^{\gamma_{i^*} L} + [q+r-p]e^{-\gamma_{i^*}\beta L} + [p-r]e^{-\gamma_{i^*}(-L+\beta L)})$$

Thus, the individual buys insurance if $CE_{i^*}^1 \geq CE_{i^*}^0$ which we can rewrite as

$$CE_{i^*}(v') - CE_{i^*}(v) \geq mq\beta L$$

where $-\frac{1}{\gamma_{i^*}} \log([1-q-r] + re^{\gamma_{i^*} L} + [q+r-p]e^{-\gamma_{i^*}\beta L} + [p-r]e^{-\gamma_{i^*}(-L+\beta L)}) = CE_{i^*}(v')$ and$-\frac{1}{\gamma_{i^*}} \log([1-p] + pe^{\gamma_{i^*} L}) = CE_{i^*}(v)$.

Using the approximation for small losses, the index insurance purchase rule is

$$\frac{1}{2}\gamma_{i^*}(\sigma_v^2 - \sigma_{v'}^2) \geq (m-1)q\beta L$$

The next result evaluates the impact of informal risk sharing arrangement on the take-up of index insurance.

PROPOSITION 2: Consider an individual with risk aversion parameter γ_i who joins a group with parameter γ_g. Then, under small losses and actuarially unfair index insurance $m > 1$, the following results hold.

(1) Independent of his presence in the group, the individual i will not purchase index insurance if $\gamma_i < \gamma^*$.

(2) Independent of his risk aversion parameter γ_i, the individual i will not purchase index insurance if $\gamma_g \leq \gamma^*$.

(3) However, the individual may buy index insurance if $\gamma_i \geq \gamma^*$ and $\gamma_g \geq \gamma^*$ are satisfied. Particularly, he buys the index cover in the presence of the group if $\sigma^2_{v'} < \sigma^2_v$ and $\gamma_{i^*} = \frac{\gamma_i \gamma_g}{\gamma_i + \gamma_g} > \gamma^* = \max(0, \frac{2(m-1)q\beta L}{\sigma^2_v - \sigma^2_{v'}})$.

Proposition 2 shows that informal risk-sharing arrangements can impede the discrete (0-1) take-up of index insurance. The intuition is based on the fact that the "effective" risk aversion of individuals forming a group are lower than the risk aversion of the individuals if they were acting individually. Essentially the group lowers the individual's aversion to risk (Lemma 2) which in turn might move the individual from a purchase zone to the non-purchase zone based on γ^*.

4.4.3 The Case of Large Losses

The results from Proposition 2 can be modified to fit the case of large losses. When losses are small, an individual $i's$ decision to not buy index insurance remain unchanged in the presence of informal arrangements. However if losses are large, our theory suggests that informal insurance might facilitate in taking up of index insurance. This happens for instance if an individual is initially too risk averse to buy index insurance on his own, however in the presence of informal arrangements his effective risk aversion might be such that he ends up purchasing the index cover. To illustrate, consider Figure 1. An individual with risk aversion parameter 6 would not have purchased the index insurance if he was acting individually. However if he pairs with a group that brings his effective risk aversion to the range (0.8, 4.7), then he chooses to purchase the index cover. We also see that it is possible that informal insurance acts as a barrier to take up. For example, consider an individual with risk aversion parameter 3. Acting individually, he will buy the index insurance, however if the presence of a risk-sharing arrangement reduces his effective risk aversion to below 0.8, then he will choose not to buy the index insurance. The analysis provides explanations and predictions for several empirical findings which are discussed in the next section.

4.5 Model-Implications and Experimental Evidence

Our theoretical evaluation of the interaction between informal risk sharing schemes and demand for index insurance provide several testable hypotheses with implications for the design of index insurance contracts. This section discusses the emerging hypothesis and explores them empirically combining field experimental data from multiple sources for a specific index contract "rainfall insurance". We begin with a discussion of the testable hypotheses, and then follow this with a description of the data and experimental design. For each hypothesis, we present the testing procedure and the resulting empirical results.

4.5.1 Discussions, and testable implications

First, why might more risk averse individuals not take up index insurance? Our framework suggests a plausible answer. *Absent* risk-sharing arrangements, low take-up among high risk averse individuals may be due to aversion to basis risk (Clarke 2016). However, another plausible reason may be due to the *presence* of informal risk sharing groups (i.e., based on our theory, Section 4). The presence of risk sharing groups leads to effective reduction in an individual's risk aversion, making him more tolerant towards aggregate risk and more sensitive to the price of index insurance. For this reason, more risk averse people may end up not buying index insurance, as compared to an individual with the same risk aversion parameter who might take it up if the individual was unmatched.

Second, why is the take-up for index insurance unexpectedly low? Possible answers lie in the role of existing informal arrangements. In particular, (1) When does informal arrangement *support* the index take up? Our analysis suggests that high risk averse individuals in risk sharing arrangements containing intermediate risk averse members are more likely to purchase index insurance. Acting alone, basis risk will act as a disincentive to the take-up of index insurance; however, the presence of the group makes the individual more tolerant to basis risk; (2) When does informal pairing not-*support* index take up? From our analysis, low to intermediate risk averse individuals that enter any risk sharing group are less likely to purchase index insurance. Their effective risk aversion is lower, and thus has lower willingness to pay for index insurance. The above discussions lead to the following sets of predictions.

Prediction #1: The link between informal risk-sharing and the take of index insurance is ambiguous. This is because of the existence of the two identifiable forces: sensitivity to either basis risk or price of the index contract. Ultimately, the overall impact of informal risk-sharing schemes on the demand for index insurance depends on which of these two forces dominate.

Prediction #2: Informal risk-sharing is more likely to complement the take-up of index insurance in regions with high aggregate (especially, if un-insurable by group) and basis risk. This follows because the presence of an informal risk sharing group helps to make the individual more tolerant to the basis risk, holding other forces constant. In addition, in the presence of risk-sharing arrangements, the sensitivity of index demand to price changes is higher, as individuals become effectively less risk averse.

Prediction #3: The take-up for index insurance may be higher if the size of the group is smaller. This is because smaller groups are likely more risk averse, all else equal. For instance, under small losses (e.g., relative to w and ε), villages where there are more informal transfers, which can be proxied by the number of pairs in our model, are likely to see lower take-ups once price and basis risk are controlled for. With controls for price effects and basis risk, individual's risk aversion from joining the larger group may be effectively lower leading to less demand for insurance. This prediction contradicts those that connect information diffusion and group size.

4.5.2 Data and sources

Ideally, we require data about the demand for index insurance contracts, informal risk sharing, a measure of basis risk, insurance premiums, and risk aversion. For this purpose, we draw on available data sets from a panel of experimental trials that were conducted across randomly selected rural farming households and villages in Gujarat, India.[6] Data on risk aversion come from Cole et al. (2013), which is based on field experiments across 100 villages in 2006/2007. The measure of risk aversion follows Binswanger (1980), whereby respondents are asked to choose among cash lotteries varying in risk and expected return. The lotteries were played for real money, with payouts between zero and Rs. 110. The lottery choices are then mapped into an index between 0 and 1, where high values indicate greater risk aversion.[7]

[6] All villages are located within $30km$ of a rainfall station. Design of rainfall insurance contracts uses information from these rainfall stations.

[7] A value 1 is assigned to individuals that choose the safe lottery. For those who choose riskier lotteries, the $[0, 1)$ mapping indicates the maximum rate at which they are revealed to accept additional risk (standard

From Cole, Tobacman and Stein (2014), we obtain data about the take-up of index insurance, premiums, and premium discounts available between 2006-2013 for 60 villages cumulatively. Most of these villages and households overlap with the 100 villages in Cole et al. (2013). This allows us to match households and villages between the two data sets. Our final data are merged from these two sources. We summarize the timeline of the rainfall-index insurance experiments and the available data in Figure 2.

4.5.2.1 Rainfall-index contracts and experimental setting

The specific index insurance contract that we examine is "rainfall insurance" whose payouts are based on a publicly observable rainfall index. This contract provides coverage against adverse rainfall events (i.e., covering drought and flood) for the summer ("Kharif") monsoon growing season. Design of this contract is based on daily rainfall readings at local rainfall stations, specifying payouts as a function of cumulative rainfall during fixed time periods over the entire June 1-August 31 Kharif season. Typically, the maximum possible payout for a unit-policy is about Rs. 1500. Households have the option to purchase any number of policies to achieve their desired level of insurance coverage. The contracts are offered and paid-out year-to-year, whereby a marketing team visits households in the selected sample each year in April-May to offer the insurance policies. Households are required to opt-in to re-purchase each year to sustain their coverage.

"Group Identity" as risk-sharing proxy: The marketing teams for rainfall insurance used multiple strategies to sell the policies. Their strategies include the use of flyers, videos, and discount coupons, and involved randomization of these three marketing methods at the household level. More importantly, flyers were randomized along two dimensions with the aim of testing how formal insurance interacts with informal risk-sharing arrangements (cf:

deviation) in return for higher expected return ($\frac{\Delta E}{\Delta risk}$). Additional details are available in Cole et al. (2013).

Cole et al. 2013). The flyers emphasized and provided cues on "group identity", which has been found to be key for informal risk-sharing (Karlan et al., 2009). The treatments for group identity included:[8]

> **Religion** (Hindu, Muslim, or Neutral): *A photograph on the flyer depicted a farmer in front of a Hindu temple (Hindu Treatment), a Mosque (Muslim Treatment), or a neutral building. The farmer has a matching first name, which is characteristically Hindu, characteristically Muslim, or neutral.*
>
> **Individual or Group** (Individual or Group): *In the Individual treatment, the flyer emphasized the potential benefits of the insurance product for the individual buying the policy. The Group flyer emphasized the value of the policy for the purchaser's family.*

Note that the use of cues on group identity as a proxy for risk-sharing has been used in previous literature (e.g., Cole et al. 2013), which we follow here. While such approach may have the downside of not capturing actual risk-sharing since people generally choose who to group and share risk with (possibly, over and beyond religious and family lines), it has an empirical appeal: it allows for randomization of risk-sharing which is extremely useful for identification purposes, at least, as compared to cases where groups form endogenously and share risk.

4.5.2.2 Measuring basis risk

Each season, households were asked if they had experienced crop loss due to weather in the household panel experiments. We combine this with unique market information about whether the household i located in village v in a contract year t received an insurance payout

[8] More details of the data and group treatments are available in our two primary sources of data: Cole et al. (2013); Cole, Tobacman and Stein (2014).

to define a measure of basis risk

$$briskDOWNSIDE_{ivt} = \mathbf{1}(loss_{ivt} > payout_{ivt})$$
$$briskUPSIDE_{ivt} = \mathbf{1}(loss_{ivt} < payout_{ivt})$$

which are indicators that capture the potential mismatch or discrepancy between insurance payouts and the actual crop loss or income loss suffered by the policy holder prior to the payout decisions. For instance, this may be due to the fact that the measured rainfall index is imperfectly correlated with rainfall at any individual farm plot. As illustrated, our measure of basis risk allows for the distinction between upside and downside risks, and follows directly from previous discussions in Section 2.[9]

4.5.2.3 Summaries

The summary statistics of all relevant variables in our sample are reported in Table 2. The first two moments and order statistics of each variable are displayed. As shown, the data is made up of information about the demand for rainfall-index insurance, premium and randomized discounts, crop and revenue loss experience of households, treatments for risk-sharing as proxied by cues on "group identity", and basis risks, respectively. The overall data spans 2006-2013, covering 645 households across a pool of 60 villages. Considerable variations exist among the variables which we shall exploit for identifying variation. Our main outcome of interest is binary, denoted "Bought". Bought is defined based on whether

[9]Since crop losses (but not payouts) are self-reported, there is a potential tendency for households to misreport, e.g., overstate losses, and thus might impact our measurement of basis risk up/down. To assess such potential misreporting, we regress households reported-crop loss experience on a vector of seventeen (17) household characteristics: spanning socio-demographics, educational level, asset holdings, access to formal insurance, per capita monthly expenditure, risk aversion, and indicators for whether a respondent has a muslim name and irrigates the farm. Results are reported in Table 15. None of these 17 variables is statistically significant at conventional levels, an evidence inconsistent with misreporting. The evidence is more consistent with a reporting behavior whereby crop losses occur due to weather shocks and then households report them as such. This finding hold across the wide range of model specifications, which differ based on the included controls.

households purchased index insurance in given market year. In our sample, about 39% of households bought rainfall-index insurance over the entire panel period.

The average risk aversion is 0.53 with a standard deviation of about 0.32. The overall share of households that received cues on Group, Hindu and Muslim treatments are about 4.0%, 2.8% and 2.9%, respectively. Our measure of basis risk that relies on the mismatch between pre-insurance crop losses and index payouts suggest higher relative frequency for downside basis risk (25.5%), as compared to upside basis risk (8.2%). For our basis risk measure that relies on the mismatch between pre-insurance revenue losses[10] and index payouts, the relative frequency of downside and upside basis risks are quite close. A visual illustration for both downside and upside basis risks are shown in Figure 3. Empirical tests for the various predictions combine these variables with exogenous variations induced by the random assignment of price discounts and risk-sharing marketing treatments.[11]

4.5.3 Empirical tests and results

The testing procedure and empirical results are presented in this section. Additional robustness checks on our main results are discussed.

[10]Revenue is measured for market years in which households reported a crop loss, and captures the "amount" of crop loss: calculated as the difference between that market year's agricultural output and the mean value of output in all previous years where crop loss was not reported.

[11]Ensuring balance across risk-sharing treatment groups e.g., assignment of group, Hindu and muslim cues is crucial for the experimental results. We ascertain balance using observable characteristics of the households. In Table 16 of the Appendix, we test whether the various household characteristics significantly differ across the risk sharing treatments. The results provide strong evidence in favor of balance (except for about two variables which are barely significant at 10% level).

4.5.3.1 Empirical strategy and results: predictions #1 and #2

To test predictions #1 and #2, we estimate a model that links changes in take-up for index insurance $D_{ivt} = \mathbf{1}(bought = Yes)_{ivt}$ to the vector of risk-sharing treatments $\boldsymbol{RShare}_{ivt}$ and their unrestricted interaction with basis risk $brisk_{ivt}$ and exogenous variation in the price for insurance $Discount_{ivt}$

$$D_{ivt} = \theta \boldsymbol{RShare}_{ivt} \times brisk_{ivt} + \beta_d Discount_{ivt} + \mu_i + \delta_t + \epsilon_{ivt}$$

$$D_{ivt} = \theta \boldsymbol{RShare}_{ivt} \times Discount_{ivt} + \beta_b brisk_{ivt} + \mu_i + \delta_t + \epsilon_{ivt}$$

where i, v and t index the household, village and market year respectively. This specification includes a set of unrestricted household dummies, denoted by μ_i, which capture unobserved differences that are fixed across households such as access to other forms of insurance. The market-year fixed effects, δ_t control for aggregate changes that are common across households, e.g. prices, and national policies. Our key parameter of interest θ is identified by household-level exogenous variation in the various treatments for risk-sharing and their interactions with the two forces: basis risk and insurance premium. Errors are clustered at the village level to allow for arbitrary correlations.

The results are reported separately for the two measures of basis risk: crop-loss mismatch with index payouts versus revenue-loss mismatch with index payouts. For the first Equation, which interacts risk sharing with basis risk, Tables 3 and 4 contain the estimates for crop-mismatch while Tables 5 and 6 contains the estimates for revenue-mismatch. Columns differ based on the included risk-sharing treatments and interactions with basis risk, and controls for premium discount and upside basis risk. In Tables 3 and 5, columns (2)-(4) include the various interaction terms, while column (1) omits the interactions. However, in Tables 4 and 6, column (1) includes the various interaction terms with basis risk, column (2) adds a control for premium discount, while column (3) adds controls for both premium discounts and upside basis risk.

Downside basis risk is negative and significant at conventional levels, upside basis risk is significantly positive, and premium discount is significantly positive across all specifications. The estimated price discount effects range between 0.0032 - 0.0035; with an average estimate of about 0.0034. An average estimate of 0.0034 implies that a 10 percent decline in the price of index insurance increases the probability of purchase by 0.034 percentage points, or 0.113 percent of the conditional mean take-up rate (~0.30). The implied elasticity is 0.0113. While households negative demand-response to downside basis risk is substantial, this is less than their positive response to upward basis risk. Turning to our key coefficients of interest, there is evidence that informal risk-sharing significantly supports the take-up of index insurance, and that when downside basis risk is high risk-sharing increases the index demand by 13.0% points (column 4; Table 3) to 40.1% points (column 4; Table 6).

Next, for the second Equation, which interacts risk sharing with exogenous changes in premium, the results for crop-mismatch are contained in Tables 7 and 8, and those for revenue-mismatch are in Tables 9 and 10. Again, across all model specifications, downside basis risk is significantly negative, upside basis risk is positive and large, and premium discount is positive. For our main coefficients of interest, there is evidence that the existence of risk-sharing arrangement makes individuals more sensitive to price changes since both the direct and interaction terms on discount are positive. For example, when group cues are combined with discounts (Table 7; column 4), the sensitivity increases by about 10.1 percentage points which implies an increased elasticity of 0.337.

In addition, there is evidence that informal risk-sharing significantly either support or not-support the take-up of rainfall-index insurance. For instance, while Group cues has negative effect on index take-up (column 4; Table 7), Group cues treatment combined with Muslim cues has a significant positive effect on take-up (column 3; Table 8). However, when the various risk-sharing cues are combined with premium discount, most of the terms have significant positive effect on the take-up of insurance.

Taken together, these results (i.e., Tables 3-10) provide evidence that informal risk-sharing has ambiguous effects on index take-up, empirically. With high downside basis risk, informal networks increase take-up, but under price effects, informal networks may have negative effect on take-up; making the overall impact of risk-sharing on the take-up of index insurance ambiguous. As shown in Proposition 2, risk aversion plays a central role in explaining these effects. Thus, we turn to the role of risk aversion in the subsequent analysis.[12]

4.5.3.2 Empirical strategy and results: prediction #2

We modify previous specifications to investigate how risk aversion (effective) interacts with the two forces: sensitivities to either basis risk or insurance premium

$$D_{ivt} = \theta riskAversion_{ivt} \times brisk_{ivt} + \beta_d Discount_{ivt} + \mu_i + \delta_t + \epsilon_{ivt}$$

$$D_{ivt} = \theta riskAversion_{ivt} \times Discount_{ivt} + \beta_b brisk_{ivt} + \mu_i + \delta_t + \epsilon_{ivt}$$

where all the terms are defined similarly as in previous sections, and errors are clustered at the village level. The results are reported in Table 11. Columns differ based on the included interactions with risk aversion. Column (1) uses market year dummies to control for potential sensitivity to changes in premium, and includes an interaction between basis risk and risk aversion. This interaction allows us to focus on the response of basis risk to changes in risk aversion I.e., we ask whether increase in risk aversion alter the demand-

[12]Since our theoretical analysis relies on CARA (with a simplifying property of no wealth effects), we examine how sensitive or robust our main results are to potential wealth effects. To do this, we re-estimate our empirical model with an additional control for households wealth. We used Factor analysis to estimate the wealth of housholds based on eight (8) asset holdings or ownership: $\mathbf{1}$(Electricity=Yes), $\mathbf{1}$(Mobile Phone=Yes), $\mathbf{1}$(Sew Machine=Yes), $\mathbf{1}$(Tractor=Yes), $\mathbf{1}$(Thresher=Yes), $\mathbf{1}$(Bull cart=Yes), $\mathbf{1}$(Bicycle=Yes), and $\mathbf{1}$(Motorcycle=Yes); where $\mathbf{1}(.)$ is a logical indicator that equals 1 whenever the argument in the bracket is true, and 0 otherwise. Figure 5 shows the estimated distribution of wealth. The implied results are also shown in Tables 17 and 18. The estimate on wealth is positive but not significant. However, the estimates for our key parameter of interest γ are similar to the main results (i.e., very close and well within the confidence intervals of the main estimates).

response to basis risk. In columns (2)-(3), we directly control for potential sensitivity to basis risk, and include interactions between premium discounts and risk aversion to evaluate how households sensitivity to prices respond to changes in risk aversion.[13]

Note that the direct coefficient on risk aversion is not estimable (but its interaction with other variables are) since we included household-level dummies which soaks-up any fixed household-level terms. From column (1), downside basis risk has significant negative effect on take-up (-12.0% points); its interaction with risk aversion is also negative (but not significant at conventional levels). This seems to suggest that, after controlling for price effects, an increase in individual's risk aversion increases the negative sensitivity of index take-up to increases in basis risk. The result that basis risk when combined with risk-sharing cues positively affect take-up (Table 3 and 6; Muslim cues) can be explained by this negative effect of risk aversion on basis risk . Recall that joining a group effectively reduces individual's risk aversion (LEMMA 2).

The results in columns (2)-(3) show that premium discounts have significantly positive impact on take-up, increasing index take-up by 0.369 to 0.396 percentage points (similar to previous estimates). The interaction with risk aversion is negative. The negative sign implies that increasing risk aversion has negative effect on the positive impact of premium discounts on insurance demand (although not statistically significant) and vice versa. This likely explains the positive effect of premium discount when combined with the various risk-sharing cues on index take-up (Tables 7-10), when combined with the result in LEMMA 2.

[13] There is an empirical appeal to use the observed risk aversion values here (rather than the theory-derived risk aversion values). The sample is at the individual household level with larger size for the observed values. We do not have to calculate risk aversion values at the village level–which is an approach we will have take to obtain the theory-based values. Figure 4 illustrates the distribution of observed vs theory-derived risk aversion values.

4.5.3.3 Empirical strategy and results: prediction #3

We evaluate prediction #3 by linking observed changes in take-up for index insurance to a measure of group-size while controlling for the effect of basis risk and variations in insurance premium at the village level,

$$D_{vt} = \theta GSize_{vt} + \beta_b brisk_{vt} + \beta_d Discount_{vt} + \mu_v + \delta_t + \epsilon_{vt}$$

where group size, $GSize_{vt}$, is defined as the number of households that received cues on "group identity" per village. μ_v are village-level fixed effects, capturing time-invariant potential unobserved heterogeneity. The results for alternative model specifications are reported in Tables 12-14. Our preferred specification is column (4), which examines the effect of group size on the demand for index insurance along with full controls for downside basis risk, upside basis risk and premium discounts. These additional controls are meant to soak-up household sensitivities to both basis risk and insurance premium within the framework of our theoretical model.

Consistent with prediction #3, the estimate on group size is negative, statistically significant across all specifications, and hold across alternative measures of group size which are based on the various risk-sharing treatments. Estimates from our preferred specification suggest that providing cues on "group identity" for an additional household in a village will result in about 2.8% points decrease in index take-up, all else equal (column 4; Table 12).[14] This represents 5.9% reduction in insurance insurance take-up, relative to the conditional mean defined over the entire sample period. The negative effects of group size on take-up are much larger in the model specification that controls for only downside basis risk (column 1). This is expected and can be understood based on our theory: the countervailing force to reduced index demand is "upside basis risk" when individuals become effectively less risk averse following more group exposure. Thus, controlling to eliminate this force should

[14] We examine the sensitivity of our main results to potential wealth effects by including wealth as a control. Results are displayed in Tables 19 and 20. The estimate on wealth is positive but hardly significant. However, the estimates on group size are negative, significant and very close to our baseline results.

yield larger negative effects of increasing group size. Next, as expected, the results indicate that downside basis risk significantly reduces the demand for index insurance (about 10% points), upside basis risk increases index take-up (about 62% points), while offering premium discounts significantly increase the take-up (approximately 0.33%).

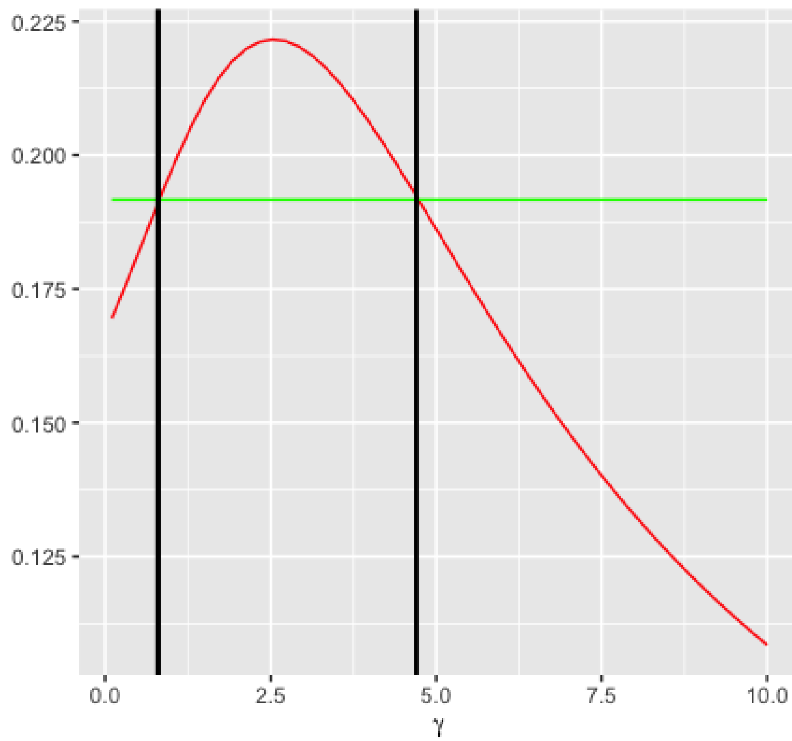
These results are inconsistent with theoretical and empirical findings in studies of information diffusion which will predict increased uptake of index insurance with an increase in exposed group size (e.g., Jackson and Yariv 2010; Banerjee et al. 2013).

4.6 Conclusion

Our evaluation of the effect of informal risk sharing schemes on the take-up for index insurance, documents that the effects are ambiguous and driven by two forces: sensitivities to basis risk and insurance premium, which operate through risk aversion. In our model, we consider the case of an individual who endogenously chooses to join a group and make decisions about index insurance. The presence of an individual in a risk sharing arrangement reduces his risk aversion, termed "Effective Risk Aversion". We appeal to this phenomenon of "Effective Risk Aversion" to establish that such reduction in risk aversion can lead to either reduced or increased take up of index insurance, and emphasize how these results provide alternative explanations for two empirical puzzles: unexpectedly low take-up for index insurance and demand being particularly low for the most risk averse. Our model provide several testable hypotheses with implications for the design of index insurance contracts. Drawing on data from a panel of field experimental trials in India, we provide evidence for several predictions that emerge from our analyses.

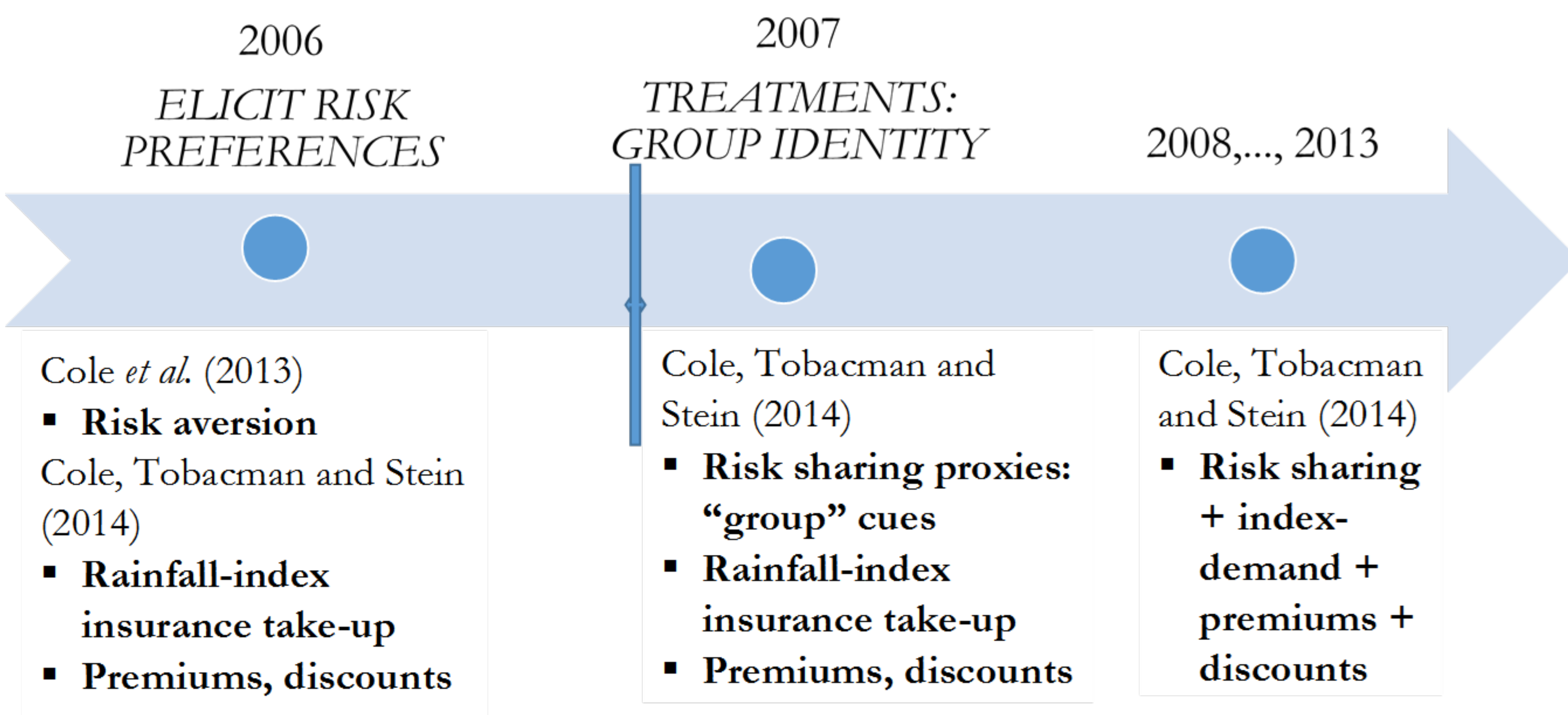
Our study is an initial step towards the broader understanding of the linkages between informal risk-sharing and the market for formal index insurance. In ongoing research, we test the predictions from the model both in the laboratory and the field. Further, we aim to draw on the literature on network analysis and multi-dimensional matching to analyze the interactions between index insurance and informal arrangements to inform the design of policy and index contracts. This line of work has broader implications for the design and introduction of insurance and financial contracts that aim at mitigating environmental risks among low-income societies.

Figure 4.1: Index Take-up under Large Losses



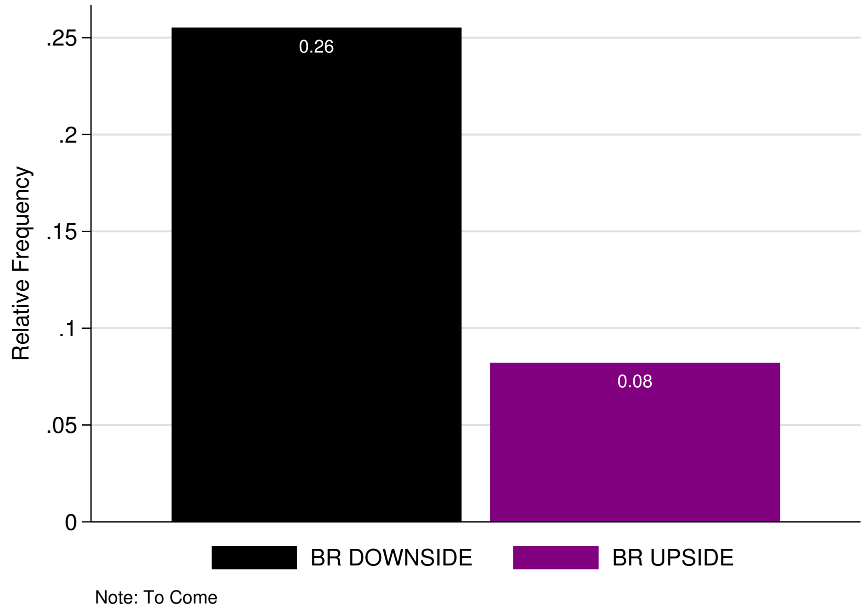
Notes: Assumptions underlying Figure 1 are as follows: $p = q = \frac{1}{3}$, $L = 1$, $r = \frac{1}{9}$, $\beta = 0.5$, $m = 1.15$. The vertical black lines correspond to $\gamma = 0.8$ and $\gamma = 4.7$.

Figure 4.2: Timelines of Data and Experimental Treatments

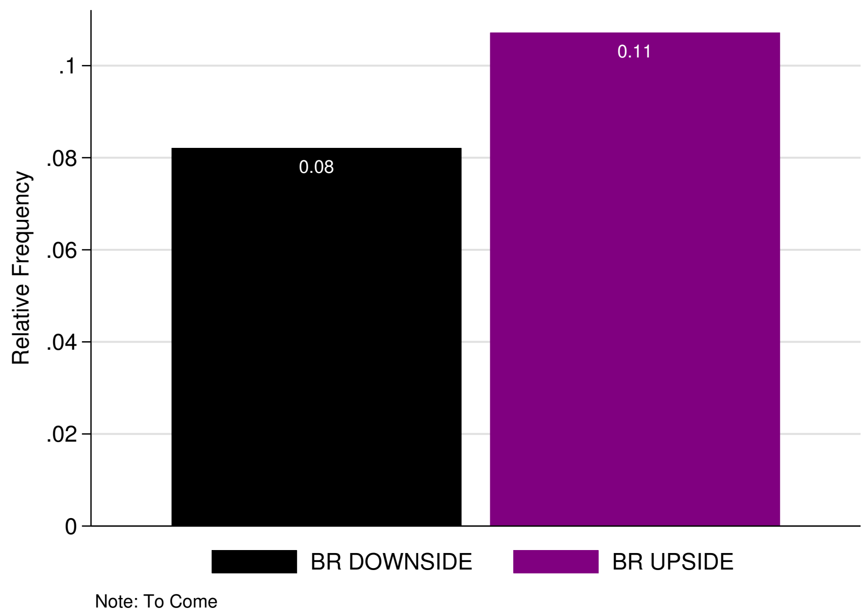


Notes: Figure shows the timeline of the data sets and experimental treatments that we combined for our empirical analysis. The two primary sources of our data are Cole et (2013) and Cole, Tobacman and Stein (2014). Major parts of our data come from the latter source.

Figure 4.3: Distribution of Basis Risk



(a) **CROP LOSS: DOWNSIDE VS UPSIDE BASIS RISK**



(b) **REVENUE LOSS: DOWNSIDE VS UPSIDE BASIS RISK**

Notes: Figures display the distribution of basis risk measured as the mismatch between households experience of pre-insurance loss in crops or revenue and receiving an index payout, respectively. This shown for both downside and upside basis risks. Revenue is measured for market years in which a crop loss is reported, and captures the "amount" of crop loss: calculated as the difference between that market year's agricultural output and the mean value of output in all previous years where crop loss was not reported.

Table 4.2: Summary Statistics

VARIABLE	**Obs.**	**Mean**	**Std. Dev.**	**Min**	**Max**
Index-Demand					
1(Bought=Yes)	4,948	0.390	0.488	0	1
Risk Aversion	4,919	0.528	0.316	0	1
Price and Discounts					
Premium	4,948	159.4	56.08	44	257
Discount	4,871	5.352	17.51	0	90
1(Got Payout=Yes)	4,948	0.119	0.324	0	1
Payout Per Policy	1,929	63.75	56.50	0	257
Payout Amount	4,948	0.0567	0.265	0	3.208
Pre-Insurance Losses					
1(Crop loss=Yes)	4,948	0.292	0.455	0	1
1(Revenue loss=Yes)	4,948	0.0940	0.292	0	1
Risk-Share Treatments					
1(Group cues)	4,871	0.0396	0.195	0	1
1(Hindu cues)	4,871	0.0277	0.164	0	1
1(Muslim cues)	4,871	0.0287	0.167	0	1
Basis Risk					
BR DOWNSIDE--crop loss	4,948	0.255	0.436	0	1
BR UPSIDE--crop loss	4,948	0.0821	0.274	0	1
BR DOWNSIDE--rev. loss	4,948	0.0821	0.274	0	1
BR UPSIDE--rev. loss	4,948	0.107	0.309	0	1
Number of Years				2006	2013
Number of Households				645	645
Number of Villages				60	60
Number of Districts				3	3

Notes: Table reports the summary statistics of the panel data used for our empirical analysis. This include information about take-up of rainfall-index insurance, premium and randomized discounts, crop and revenue loss experience of households, multiple treatments for risk-sharing, proxied by cues on "group identity", and basis risks respectively. **1**(.) is a logical indicator that takes the value 1 whenever the argument in the bracket is true, and zero otherwise. The merged data spans 2006-2013, covering 645 households across a pool of 60 villages. These are located in three districts in the state of Gujarat, namely: Ahmedabad, Anand and Patan.

Table 4.3: Crop Mismatch **t1**: Index Demand-Group Identity link vs Basis Risk

VARIABLES	(1) bought	(2) bought	(3) bought	(4) bought
Risk-share Treatments				
Group cues	-0.0557	-0.0621	-0.0581	-0.0496
	(0.0567)	(0.0612)	(0.0603)	(0.0580)
brisk DOWNSIDE		-0.161***	-0.160***	-0.104***
		(0.0239)	(0.0236)	(0.0227)
Group cues **X** brisk DOWNSIDE		0.0158	0.0172	-0.0232
		(0.105)	(0.101)	(0.0991)
Hindu cues	-0.0320	-0.0518	-0.0424	-0.0234
	(0.0580)	(0.0598)	(0.0582)	(0.0588)
Hindu cues **X** brisk DOWNSIDE		0.0487	0.0453	-0.00995
		(0.0952)	(0.0930)	(0.0897)
Muslim cues	-0.0645	-0.106	-0.102	-0.0874
	(0.0593)	(0.0685)	(0.0675)	(0.0651)
Muslim cues **X** brisk DOWNSIDE		0.158*	0.160*	0.130*
		(0.0827)	(0.0818)	(0.0776)
Discount			0.00348***	0.00327***
			(0.000589)	(0.000565)
brisk UPSIDE				0.523***
				(0.0247)
Constant	0.226***	0.350***	0.349***	0.300***
	(0.0373)	(0.0411)	(0.0409)	(0.0397)
Observations	6,490	6,490	6,490	6,490
R-squared	0.112	0.127	0.133	0.221
Number of Households	989	989	989	989
Mkt Year FEs	Yes	Yes	Yes	Yes
Household FEs	Yes	Yes	Yes	Yes

Notes: Table reports the results from regressions of take-up for rainfall-index insurance on a vector of treatments for risk-sharing proxied by cues on "group identity" and their interactions with basis risk and discount assignments–exogenous variation in insurance premium at the household level. Columns (1)-(4) differ based on the included risk-sharing treatments and interactions with basis risk, and controls for premium discount and upside basis risk. Columns (2) - (4) include the various interaction terms, while column (1) omits the interactions. Errors are clustered at the village level. Stars indicate significance: $***$, $**$, and $*$ stand for significance at the 1%, 5%, and 10% level, respectively.

Table 4.4: Crop Mismatch **t2**: Index Demand-Group Identity link vs Basis Risk

VARIABLES	(1) bought	(2) bought	(3) bought
Risk-share Treatments			
Hindu cues	-0.0774	-0.0628	-0.0424
	(0.0802)	(0.0781)	(0.0783)
Group cues	-0.103	-0.0910	-0.0713
	(0.0992)	(0.0971)	(0.0918)
Hindu cues **X** Group cues	0.0939	0.0767	0.0651
	(0.131)	(0.128)	(0.128)
brisk DOWNSIDE	-0.162***	-0.161***	-0.105***
	(0.0241)	(0.0239)	(0.0230)
Hindu cues **X** brisk DOWNSIDE	0.124	0.117	0.0623
	(0.127)	(0.126)	(0.122)
Group cues **X** brisk DOWNSIDE	0.154	0.154	0.0847
	(0.167)	(0.162)	(0.157)
Hindu cu. **X** Group cu. **X** brisk DOW.	-0.309	-0.302	-0.274
	(0.277)	(0.269)	(0.266)
Muslim cues	-0.121	-0.113	-0.0872
	(0.0889)	(0.0876)	(0.0837)
Muslim cues **X** Group cues	0.0631	0.0509	0.0198
	(0.147)	(0.146)	(0.138)
Muslim cues **X** brisk DOWNSIDE	0.195*	0.197*	0.144
	(0.112)	(0.112)	(0.105)
Muslim cu.**X**Group cu.**X** brisk DOW.	-0.211	-0.211	-0.137
	(0.254)	(0.248)	(0.245)
Discount		0.00347***	0.00327***
		(0.000587)	(0.000562)
brisk UPSIDE			0.523***
			(0.0248)
Constant	0.350***	0.350***	0.301***
	(0.0413)	(0.0411)	(0.0399)
Observations	6,490	6,490	6,490
R-squared	0.127	0.134	0.221
Number of Households	989	989	989
Mkt Year FEs	Yes	Yes	Yes
Household FEs	Yes	Yes	Yes

Notes: Table shows the results from regressions of take-up for rainfall-index insurance on a vector of treatments for risk-sharing proxied by cues on "group identity" and their interactions with basis risk and discount assignments–exogenous variation in insurance premium at the household level. Columns (1)-(3) differ based on the included risk-sharing treatments and interactions with basis risk and premium discount. Columns (1) includes the various interaction terms with basis risk, column (2) adds a control for premium discount, while column (3) adds controls for both premium discounts and upside basis risk. Errors are clustered at the village level. Stars indicate significance: $***$, $**$, and $*$ stand for significance at the 1%, 5%, and 10% level, respectively.

Table 4.5: Revenue Mismatch **t1**: Index Demand-Group Identity link vs Basis Risk

VARIABLES	(1) bought	(2) bought	(3) bought	(4) bought
Risk-share Treatments				
Group cues	-0.0557	-0.0521	-0.0479	-0.0454
	(0.0567)	(0.0584)	(0.0572)	(0.0540)
brisk DOWNSIDE		-0.124***	-0.123***	-0.0430*
		(0.0249)	(0.0248)	(0.0236)
Group cues **X** brisk DOWNSIDE		-0.172	-0.170	-0.170
		(0.292)	(0.286)	(0.273)
Hindu cues	-0.0320	-0.0316	-0.0227	-0.00980
	(0.0580)	(0.0577)	(0.0560)	(0.0568)
Hindu cues **X** brisk DOWNSIDE		0.101	0.0976	0.00164
		(0.223)	(0.222)	(0.214)
Muslim cues	-0.0645	-0.0708	-0.0660	-0.0564
	(0.0593)	(0.0596)	(0.0586)	(0.0561)
Muslim cues **X** brisk DOWNSIDE		0.208	0.206	0.141
		(0.274)	(0.271)	(0.243)
Discount			0.00351***	0.00319***
			(0.000601)	(0.000563)
brisk UPSIDE				0.599***
				(0.0223)
Constant	0.226***	0.249***	0.249***	0.229***
	(0.0373)	(0.0378)	(0.0377)	(0.0363)
Observations	6,490	6,490	6,490	6,490
R-squared	0.112	0.116	0.123	0.263
Number of Households	989	989	989	989
Mkt Year FEs	Yes	Yes	Yes	Yes
Household FEs	Yes	Yes	Yes	Yes

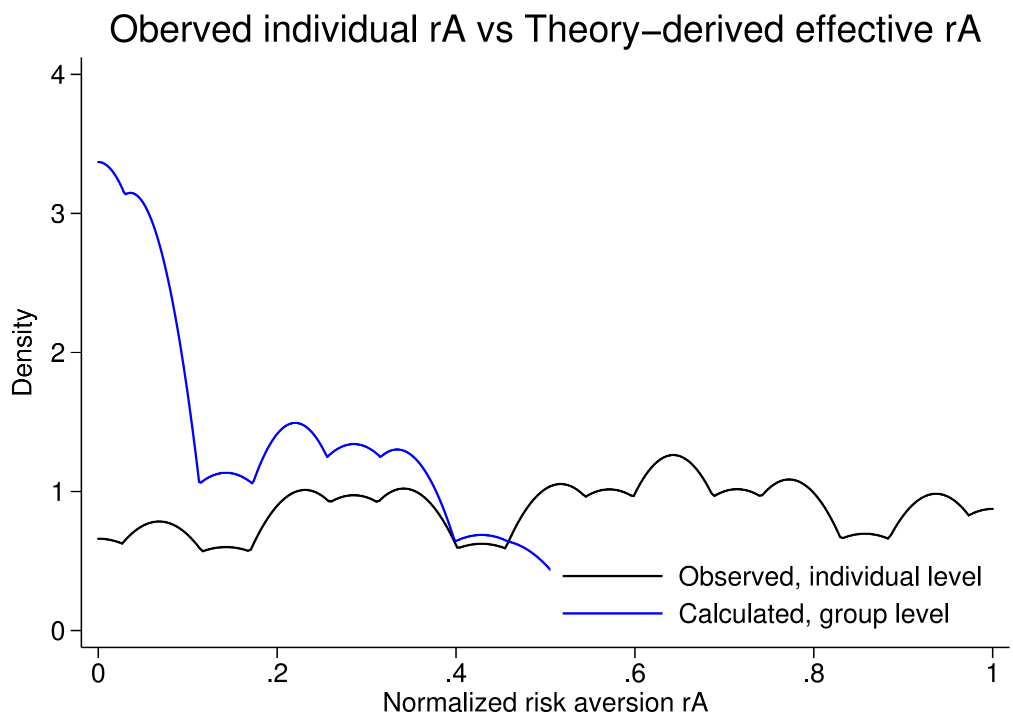
Notes: Table reports the results from regressions of take-up for rainfall-index insurance on a vector of treatments for risk-sharing proxied by cues on "group identity" and their interactions with basis risk and discount assignments–exogenous variation in insurance premium at the household level. Columns (1)-(4) differ based on the included risk-sharing treatments and interactions with basis risk, and controls for premium discount and upside basis risk. Columns (2) - (4) include the various interaction terms, while column (1) omits the interactions. Errors are clustered at the village level. Stars indicate significance: $***$, $**$, and $*$ stand for significance at the 1%, 5%, and 10% level, respectively.

Table 4.6: Revenue Mismatch **t2**: Index Demand-Group Identity link vs Basis Risk

VARIABLES	(1) bought	(2) bought	(3) bought
Risk-share Treatments			
Hindu cues	-0.0438	-0.0296	-0.0145
	(0.0740)	(0.0718)	(0.0718)
Group cues	-0.0664	-0.0542	-0.0409
	(0.0932)	(0.0906)	(0.0860)
Hindu cues **X** Group cues	0.0373	0.0201	0.00863
	(0.121)	(0.117)	(0.122)
brisk DOWNSIDE	-0.124***	-0.123***	-0.0429*
	(0.0249)	(0.0247)	(0.0236)
Hindu cues **X** brisk DOWNSIDE	0.0580	0.0499	-0.0379
	(0.239)	(0.238)	(0.228)
Group cues **X** brisk DOWNSIDE	-0.593**	-0.596**	-0.0843
	(0.235)	(0.224)	(0.315)
Hindu cu. **X** Group cu. **X** brisk DOW.			--
			--
Muslim cues	-0.0744	-0.0660	-0.0461
	(0.0847)	(0.0831)	(0.0790)
Muslim cues **X** Group cues	0.0164	0.00410	-0.0235
	(0.137)	(0.135)	(0.128)
Muslim cues **X** brisk DOWNSIDE	0.523***	0.526***	0.401***
	(0.0749)	(0.0743)	(0.0704)
Muslim cu.**X**Group cu.**X** brisk DOW.			-0.427
			(0.352)
Discount		0.00350***	0.00320***
		(0.000599)	(0.000559)
brisk UPSIDE			0.599***
			(0.0223)
Hindu cu. **X** Group cu. **X** brisk DOW.	0.514	0.529	
	(0.389)	(0.381)	
Muslim cu.**X**Group cu.**X** brisk DOW.	--	--	
	--	--	
Constant	0.249***	0.249***	0.229***
	(0.0377)	(0.0376)	(0.0362)
Observations	6,490	6,490	6,490
R-squared	0.116	0.123	0.263
Number of Households	989	989	989
Mkt Year FEs	Yes	Yes	Yes
Household FEs	Yes	Yes	Yes

Notes: Table shows the results from regressions of take-up for rainfall-index insurance on a vector of treatments for risk-sharing proxied by cues on "group identity" and their interactions with basis risk and discount assignments–exogenous variation in insurance premium at the household level. Columns (1)-(3) differ based on the included risk-sharing treatments and interactions with basis risk and premium discount. Column (1) includes the various interaction terms with basis risk, column (2) adds a control for premium discount, while column (3) adds controls for both premium discounts and upside basis risk. Errors are clustered at the village level. Stars indicate significance: $***$, $**$, and $*$ stand for significance at the 1%, 5%, and 10% level, respectively.

Figure 4.4: Observed versus Theory-derived Effective Risk Aversion



Notes: Figure shows the distribution of risk aversion elicited (i.e., observed) in the 2006/2007 baseline household surveys. For each village group level v, we apply our theoretical rule that says that the effective risk aversion $\gamma_{i=v*}$ is less than the minimum of all members risk aversion in that village to derived the distribution of effective risk aversion. This is jointly displayed with observed values of risk aversion.

Table 4.7: Crop Mismatch **t1**: Index Demand-Group Identity link vs Price Effects

VARIABLES	(1) bought	(2) bought	(3) bought	(4) bought
Risk-share Treatments				
Group cues	-0.0557	-0.208***	-0.210***	-0.207***
	(0.0567)	(0.0331)	(0.0333)	(0.0313)
Discount		0.00311***	0.00308***	0.00287***
		(0.000616)	(0.000606)	(0.000585)
Group cues **X** Discount		0.104***	0.102***	0.101***
		(0.0100)	(0.0104)	(0.0101)
Hindu cues	-0.0320	-0.268***	-0.275***	-0.276***
	(0.0580)	(0.0380)	(0.0390)	(0.0365)
Hindu cues **X** Discount		0.131***	0.129***	0.135***
		(0.00978)	(0.00979)	(0.00894)
Muslim cues	-0.0645	-0.266***	-0.266***	-0.264***
	(0.0593)	(0.0451)	(0.0482)	(0.0426)
Muslim cues **X** Discount		0.131***	0.130***	0.132***
		(0.0126)	(0.0122)	(0.0113)
brisk DOWNSIDE			-0.147***	-0.0938***
			(0.0225)	(0.0218)
brisk UPSIDE				0.527***
				(0.0242)
Constant	0.226***	0.227***	0.340***	0.293***
	(0.0373)	(0.0368)	(0.0397)	(0.0384)
Observations	6,490	6,490	6,490	6,490
R-squared	0.112	0.160	0.173	0.262
Number of Households	989	989	989	989
Mkt Year FEs	Yes	Yes	Yes	Yes
Household FEs	Yes	Yes	Yes	Yes

Notes: Table reports the results from regressions of take-up for rainfall-index insurance on a vector of treatments for risk-sharing proxied by cues on "group identity" and their interactions with basis risk and discount assignments–exogenous variation in insurance premium at the household level. Columns (1)-(4) differ based on the included risk-sharing treatments and interactions with premium discount, and controls for both downside and upside basis risks. Columns (2) - (4) include the various interaction terms, while column (1) omits the interactions. Errors are clustered at the village level. Stars indicate significance: $***$, $**$, and $*$ stand for significance at the 1%, 5%, and 10% level, respectively.

Table 4.8: Crop Mismatch **t2**: Index Demand-Group Identity link vs Price Effects

VARIABLES	(1) bought	(2) bought	(3) bought
Risk-share Treatments			
Hindu cues	-0.364***	-0.376***	-0.374***
	(0.0485)	(0.0484)	(0.0458)
Hindu cues **X** Group cues	-0.445***	-0.458***	-0.438***
	(0.0565)	(0.0575)	(0.0534)
Discount	0.435***	0.456***	0.433***
	(0.0674)	(0.0672)	(0.0615)
Hindu cues **X** Discount	0.00303***	0.00299***	0.00278***
	(0.000617)	(0.000608)	(0.000587)
Group cues **X** Discount	0.174***	0.173***	0.177***
	(0.0109)	(0.0113)	(0.00932)
Hindu cu. **X** Group cu. **X** Discount	0.196***	0.196***	0.190***
	(0.0132)	(0.0139)	(0.0131)
Muslim cues	-0.197***	-0.201***	-0.192***
	(0.0242)	(0.0252)	(0.0214)
Group cues	-0.388***	-0.394***	-0.379***
	(0.0586)	(0.0610)	(0.0551)
Muslim cues **X** Group cues	0.443***	0.461***	0.424***
	(0.0715)	(0.0757)	(0.0668)
Hindu cues **X** Discount	0.170***	0.169***	0.168***
	(0.0154)	(0.0150)	(0.0137)
Muslim cu. **X** Group cu. **X** Discount	-0.173***	-0.174***	-0.163***
	(0.0217)	(0.0213)	(0.0189)
brisk DOWNSIDE		-0.149***	-0.0951***
		(0.0224)	(0.0217)
brisk UPSIDE			0.526***
			(0.0241)
Constant	0.227***	0.341***	0.294***
	(0.0368)	(0.0398)	(0.0385)
Observations	6,490	6,490	6,490
R-squared	0.165	0.178	0.267
Number of Households	989	989	989
Mkt Year FEs	Yes	Yes	Yes
Household FEs	Yes	Yes	Yes

Notes: Table shows the results from regressions of take-up for rainfall-index insurance on a vector of treatments for risk-sharing proxied by cues on "group identity" and their interactions with basis risk and discount assignments–exogenous variation in insurance premium at the household level. Columns (1)-(3) differ based on the included risk-sharing treatments and interactions with premium discount and basis risk. Column (1) includes the various interaction terms with premium discount, column (2) adds a control for [downside] basis risk, while column (3) adds controls for both downside and upside basis risks. Errors are clustered at the village level. Stars indicate significance: $***$, $**$, and $*$ stand for significance at the 1%, 5%, and 10% level, respectively.

Table 4.9: Revenue Mismatch **t1**: Index Demand-Group Identity link vs Price Effects

VARIABLES	(1) bought	(2) bought	(3) bought	(4) bought
Risk-share Treatments				
Group cues	-0.0557	-0.208***	-0.207***	-0.205***
	(0.0567)	(0.0331)	(0.0338)	(0.0309)
Discount		0.00311***	0.00311***	0.00278***
		(0.000616)	(0.000616)	(0.000583)
Group cues **X** Discount		0.104***	0.102***	0.103***
		(0.0100)	(0.0103)	(0.00916)
Hindu cues	-0.0320	-0.268***	-0.265***	-0.276***
	(0.0580)	(0.0380)	(0.0387)	(0.0363)
Hindu cues **X** Discount		0.131***	0.131***	0.140***
		(0.00978)	(0.00991)	(0.00836)
Muslim cues	-0.0645	-0.266***	-0.267***	-0.263***
	(0.0593)	(0.0451)	(0.0460)	(0.0403)
Muslim cues **X** Discount		0.131***	0.130***	0.132***
		(0.0126)	(0.0125)	(0.0112)
brisk DOWNSIDE			-0.113***	-0.0346
			(0.0244)	(0.0227)
brisk UPSIDE				0.603***
				(0.0215)
Constant	0.226***	0.227***	0.248***	0.228***
	(0.0373)	(0.0368)	(0.0371)	(0.0356)
Observations	6,490	6,490	6,490	6,490
R-squared	0.112	0.160	0.164	0.306
Number of Households	989	989	989	989
Mkt Year FEs	Yes	Yes	Yes	Yes
Household FEs	Yes	Yes	Yes	Yes

Notes: Table reports the results from regressions of take-up for rainfall-index insurance on a vector of treatments for risk-sharing proxied by cues on "group identity" and their interactions with basis risk and discount assignments–exogenous variation in insurance premium at the household level. Columns (1)-(4) differ based on the included risk-sharing treatments and interactions with premium discount, and controls for both downside and upside basis risks. Columns (2) - (4) include the various interaction terms, while column (1) omits the interactions. Errors are clustered at the village level. Stars indicate significance: $***$, $**$, and $*$ stand for significance at the 1%, 5%, and 10% level, respectively.

Table 4.10: Revenue Mismatch **t2**: Index Demand-Group Identity link vs Price Effects

VARIABLES	(1) bought	(2) bought	(3) bought
Risk-share Treatments			
Hindu cues	-0.364***	-0.361***	-0.368***
	(0.0485)	(0.0488)	(0.0454)
Hindu cues **X** Group cues	0.435***	0.438***	0.412***
	(0.0674)	(0.0675)	(0.0609)
Discount	0.00303***	0.00302***	0.00270***
	(0.000617)	(0.000618)	(0.000585)
Hindu cues **X** Discount	0.174***	0.174***	0.181***
	(0.0109)	(0.0111)	(0.00812)
Group cues **X** Discount	0.196***	0.195***	0.192***
	(0.0132)	(0.0138)	(0.0120)
Hindu cu. **X** Group cu. **X** Discount	-0.197***	-0.196***	-0.189***
	(0.0242)	(0.0248)	(0.0194)
Muslim cues	-0.388***	-0.392***	-0.373***
	(0.0586)	(0.0592)	(0.0529)
Group cues	-0.445***	-0.446***	-0.426***
	(0.0565)	(0.0571)	(0.0512)
Muslim cues **X** Group cues	0.443***	0.450***	0.408***
	(0.0715)	(0.0720)	(0.0638)
Hindu cues **X** Discount	0.170***	0.170***	0.169***
	(0.0154)	(0.0153)	(0.0137)
Muslim cu. **X** Group cu. **X** Discount	-0.173***	-0.174***	-0.166***
	(0.0217)	(0.0219)	(0.0186)
brisk DOWNSIDE		-0.114***	-0.0351
		(0.0243)	(0.0226)
brisk UPSIDE			0.602***
			(0.0213)
Constant	0.227***	0.248***	0.228***
	(0.0368)	(0.0371)	(0.0355)
Observations	6,490	6,490	6,490
R-squared	0.165	0.169	0.311
Number of Households	989	989	989
Mkt Year FEs	Yes	Yes	Yes
Household FEs	Yes	Yes	Yes

Notes: Table shows the results from regressions of take-up for rainfall-index insurance on a vector of treatments for risk-sharing proxied by cues on "group identity" and their interactions with basis risk and discount assignments–exogenous variation in insurance premium at the household level. Columns (1)-(3) differ based on the included risk-sharing treatments and interactions with premium discount and basis risk. Columns (1) includes the various interaction terms with premium discount, column (2) adds a control for [downside] basis risk, while column (3) adds controls for both downside and upside basis risks. Errors are clustered at the village level. Stars indicate significance: $***$, $**$, and $*$ stand for significance at the 1%, 5%, and 10% level, respectively.

Table 4.11: Examining Two Forces: Basis Risk vs Price Sensitivities

VARIABLES	(1) bought	(2) bought	(3) bought
Discount (Premium)		0.00396***	0.00369***
		(0.00111)	(0.00110)
Risk aversion	N/A	N/A	N/A
Discount **X** Risk aversion		-0.000863	-0.000749
		(0.00137)	(0.00133)
brisk DOWNSIDE	-0.120***	-0.131***	-0.0817***
	(0.0280)	(0.0236)	(0.0227)
brisk DOWNSIDE **X** Risk aversion	-0.0255		
	(0.0413)		
brisk UPSIDE			0.532***
			(0.0257)
Constant	0.288***	0.331***	0.287***
	(0.0371)	(0.0385)	(0.0373)
Observations	4,919	4,842	4,842
R-squared	0.134	0.135	0.223
No. of Households	645	645	645
Mkt Year FEs	Yes	Yes	Yes
Household FEs	Yes	Yes	Yes

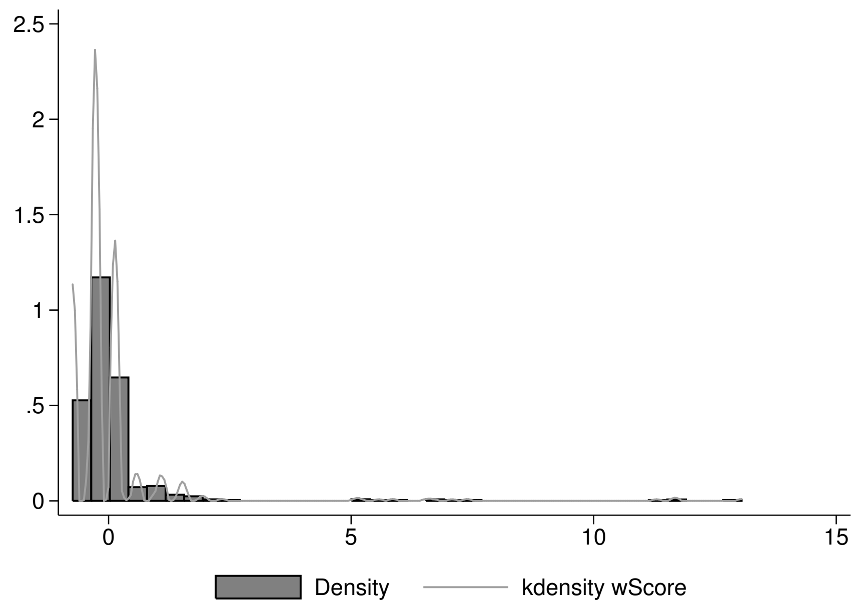
Notes: Table shows the results from regressions of take-up for rainfall-index insurance on basis risk and discount assignments–exogenous variation in insurance premium and their interactions with risk aversion at the household level. Columns (1)-(3) differ based on the included interactions with risk aversion. Columns (1) use market year dummies to control for sensitivity to changes in premium, and includes an interaction between [downside] basis risk and risk aversion, while column (2)-(3) directly controls for sensitivity to basis risk, and include interactions between premium discounts and risk aversion. Errors are clustered at the village level. Stars indicate significance: $***$, $**$, and $*$ stand for significance at the 1%, 5%, and 10% level, respectively.

Table 4.12: Group cues: Does Larger Group size lead to Lower Index Demand?

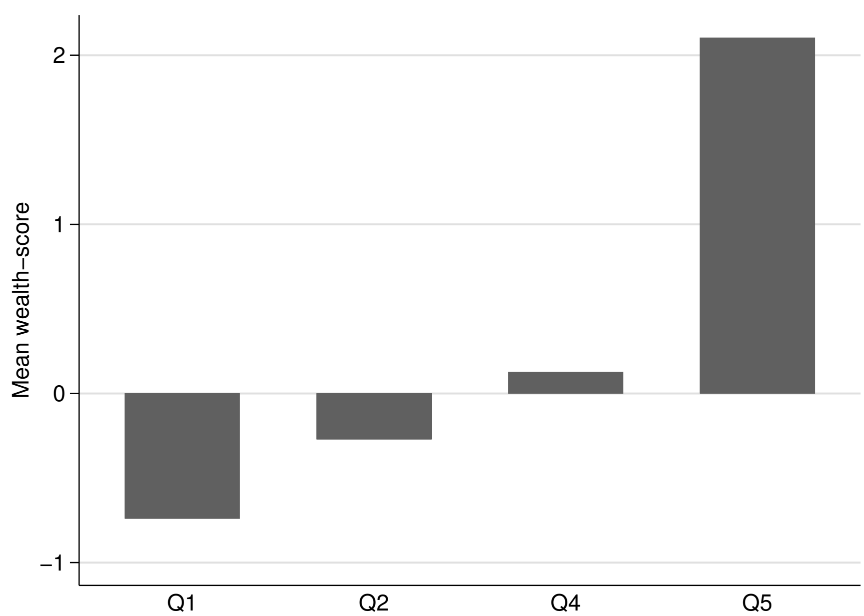
VARIABLES	(1) bought	(2) bought	(3) bought	(4) bought
Group size	-0.0267***	-0.0291***	-0.0261***	-0.0278***
	(0.00138)	(0.00136)	(0.00129)	(0.00135)
brisk DOWNSIDE		-0.162***	-0.102***	-0.101***
		(0.0210)	(0.0210)	(0.0211)
brisk UPSIDE			0.632***	0.623***
			(0.0235)	(0.0239)
discount				0.00328***
				(0.000544)
Constant	0.278***	0.453***	0.429***	0.470***
	(0.0386)	(0.0425)	(0.0412)	(0.0416)
Observations	4,299	4,299	4,299	4,222
No. of Villages	53	53	53	53
R-squared	0.146	0.162	0.288	0.292
Mkt Year FEs	Yes	Yes	Yes	Yes
Village FEs	Yes	Yes	Yes	Yes

Notes: Table reports the results from regressions of take-up for rainfall-index insurance on group size (i.e, number of households that received "Group" cues), along with controls for basis risk and exogenous changes in premium at the household level. Columns (1)-(4) differ based on the included controls. Column (1) excludes all controls, column (2) adds a control for sensitivity to [downside] basis risk, column (3) adds controls for both downside and upside basis risks, while column (4) sequentially adds a control for premium discounts. Errors are clustered at the village level. Stars indicate significance: $***$, $**$, and $*$ stand for significance at the 1%, 5%, and 10% level, respectively.

Figure 4.5: Distribution of Household Wealth



(a) **WEALTH SCORES**



(b) **WEALTH QUINTILES**

Notes: Figures display the distribution of household wealth. Wealth is estimated using Factor analysis and based on eight (8) household asset holdings: **1**(Electricity=Yes), **1**(Mobile Phone=Yes), **1**(Sew Machine=Yes), **1**(Tractor=Yes), **1**(Thresher=Yes), **1**(Bull cart=Yes), **1**(Bicycle=Yes), and **1**(Motorcycle=Yes). **1**(.) is a logical indicator that equals 1 whenever the argument in the bracket is true, and 0 otherwise. Q3 is missing, as there are few to no households in this bracket.

Table 4.13: Hindu cues: Does Larger Group size lead to Lower Index Demand?

VARIABLES	(1) bought	(2) bought	(3) bought	(4) bought
Group size	-0.107***	-0.117***	-0.104***	-0.111***
	(0.00552)	(0.00545)	(0.00514)	(0.00538)
brisk DOWNSIDE		-0.162***	-0.102***	-0.101***
		(0.0210)	(0.0210)	(0.0211)
brisk UPSIDE			0.632***	0.623***
			(0.0235)	(0.0239)
discount				0.00328***
				(0.000544)
Constant	0.278***	0.453***	0.429***	0.470***
	(0.0386)	(0.0425)	(0.0412)	(0.0416)
Observations	4,299	4,299	4,299	4,222
No. of Villages	53	53	53	53
R-squared	0.146	0.162	0.288	0.292
Mkt Year FEs	Yes	Yes	Yes	Yes
Village FEs	Yes	Yes	Yes	Yes

Notes: Table reports the results from regressions of take-up for rainfall-index insurance on group size (i.e, number of households that received "Hindu" cues), along with controls for basis risk and exogenous changes in premium at the household level. Columns (1)-(4) differ based on the included controls. Column (1) excludes all controls, column (2) adds a control for sensitivity to [downside] basis risk, column (3) adds controls for both downside and upside basis risks, while column (4) sequentially adds a control for premium discounts. Errors are clustered at the village level. Stars indicate significance: $***$, $**$, and $*$ stand for significance at the 1%, 5%, and 10% level, respectively.

Table 4.14: Muslim cues: Does Larger Group size lead to Lower Index Demand?

VARIABLES	(1) bought	(2) bought	(3) bought	(4) bought
Group size	-0.0267***	-0.0291***	-0.0261***	-0.0278***
	(0.00138)	(0.00136)	(0.00129)	(0.00135)
brisk DOWNSIDE		-0.162***	-0.102***	-0.101***
		(0.0210)	(0.0210)	(0.0211)
brisk UPSIDE			0.632***	0.623***
			(0.0235)	(0.0239)
discount				0.00328***
				(0.000544)
Constant	0.278***	0.453***	0.429***	0.470***
	(0.0386)	(0.0425)	(0.0412)	(0.0416)
Observations	4,299	4,299	4,299	4,222
No. of Villages	53	53	53	53
R-squared	0.146	0.162	0.288	0.292
Mkt Year FEs	Yes	Yes	Yes	Yes
Village FEs	Yes	Yes	Yes	Yes

Notes: Table reports the results from regressions of take-up for rainfall-index insurance on group size (i.e, number of households that received "Muslim" cues), along with controls for basis risk and exogenous changes in premium at the household level. Columns (1)-(4) differ based on the included controls. Column (1) excludes all controls, column (2) adds a control for sensitivity to [downside] basis risk, column (3) adds controls for both downside and upside basis risks, while column (4) sequentially adds a control for premium discounts. Errors are clustered at the village level. Stars indicate significance: $***$, $**$, and $*$ stand for significance at the 1%, 5%, and 10% level, respectively.

Table 4.15: Reported-Crop Loss Experience on Household Characteristics

VARIABLES	(1) **1**(Crop loss=Yes)	(2) **1**(Crop loss=Yes)	(3) **1**(Crop loss=Yes)	(4) **1**(Crop loss=Yes)	(5) **1**(Crop loss=Yes)
1(Head=Male)	-0.000248	0.00116	0.000837	0.00282	0.00257
	(0.0151)	(0.0154)	(0.0155)	(0.0156)	(0.0157)
Log(Age)	0.0164	0.0140	-0.00161	-0.00434	-0.00495
	(0.0584)	(0.0587)	(0.0601)	(0.0600)	(0.0604)
Log(Household Size)	0.0191	0.0189	0.0215	0.0234	0.0278
	(0.0155)	(0.0155)	(0.0162)	(0.0163)	(0.0175)
1(=>Secondary Educ)		-0.0111	-0.00758	-0.00691	-0.00845
		(0.0207)	(0.0208)	(0.0208)	(0.0209)
1(Electricity=Yes)			0.0111	0.0122	0.0150
			(0.0162)	(0.0163)	(0.0165)
1(Mobile Phone=Yes)			0.0313	0.0299	0.0250
			(0.0359)	(0.0360)	(0.0360)
1(Sew Machine=Yes)			0.0272	0.0303	0.0319
			(0.0315)	(0.0316)	(0.0317)
1(Tractor=Yes)			0.0575	0.0530	0.0626
			(0.0713)	(0.0718)	(0.0737)
1(Thresher=Yes)			0.121	0.124	0.120
			(0.0785)	(0.0786)	(0.0819)
1(Bull cart=Yes)			-0.0168	-0.0180	-0.0214
			(0.0388)	(0.0392)	(0.0394)
1(Bicycle=Yes)			0.00114	0.00281	0.00197
			(0.0142)	(0.0143)	(0.0144)
1(Motorcycle=Yes)			-0.0366	-0.0364	-0.0389
			(0.0323)	(0.0324)	(0.0331)
1(Any Insurance=Yes)				-0.0132	-0.0128
				(0.0141)	(0.0142)
Log(1+Per Capita m.Exp)					0.00918
					(0.0107)
Risk Aversion					-0.00507
					(0.0221)
1(Muslim name=Yes)					-0.0343
					(0.0285)
1(Irrigate=Yes)					0.0614
					(0.0413)
Constant	86.43***	86.44***	85.77***	85.78***	85.13***
	(6.731)	(6.732)	(6.756)	(6.775)	(6.799)
Observations	4,293	4,293	4,272	4,238	4,206
No. of Villages	60	60	60	60	60
R-squared	0.274	0.274	0.275	0.276	0.278
Linear Trend	Yes	Yes	Yes	Yes	Yes
Village FEs	Yes	Yes	Yes	Yes	Yes

Notes: Table reports the results from regressions of reported-crop loss experience on a vector of household characteristics. **1**(.) is a logical indicator that equals 1 whenever the argument in the bracket is true, and 0 otherwise. Columns (1)-(5) differ based on the included controls. Column (1) includes only demographic characteristics, column (2) adds a control for educational level, column (3) adds controls for household assets, column (4) adds an indicator for whether the household has any formal insurance, while column (5) adds controls for per capita monthly expenditure, risk aversion, and indicators for whether respondent has a muslim name and irrigates farm. Errors are robust to heteroskedasticity. Stars indicate significance: $***$, $**$, and $*$ stand for significance at the 1%, 5%, and 10% level, respectively.

Table 4.16: Balance on Household Characteristics

VARIABLES	(1) **1**(GroupT=Yes)	(2) **1**(HinduT=Yes)	(3) **1**(MuslimT=Yes)
1(Head=Male)	-0.00967	-0.000126	-0.0117*
	(0.00828)	(0.00691)	(0.00698)
Log(Age)	-0.0152	0.0298*	0.0173
	(0.0187)	(0.0169)	(0.0141)
Log(Household Size)	-0.0101	0.00575	0.00517
	(0.00864)	(0.00700)	(0.00682)
1(=>Secondary Educ)	0.000470	0.00660	0.00176
	(0.0117)	(0.0104)	(0.0107)
1(Electricity=Yes)	-0.00720	-0.000691	0.00262
	(0.00791)	(0.00729)	(0.00719)
1(Mobile Phone=Yes)	0.0151	-0.00119	0.00197
	(0.0176)	(0.0140)	(0.0155)
1(Sew Machine=Yes)	0.00966	-0.00391	0.0131
	(0.0168)	(0.0121)	(0.0142)
1(Tractor=Yes)	-0.0213	0.0127	-0.00773
	(0.0160)	(0.0415)	(0.0274)
1(Thresher=Yes)	-0.0168	0.0288	-0.00903
	(0.0170)	(0.0435)	(0.0169)
1(Bull cart=Yes)	0.00812	0.0100	-0.0108
	(0.0165)	(0.0176)	(0.0132)
1(Bicycle=Yes)	0.00492	0.000767	-0.00261
	(0.00758)	(0.00675)	(0.00633)
1(Motorcycle=Yes)	-0.0236	0.00167	0.00264
	(0.0145)	(0.0152)	(0.0161)
1(Any Insurance=Yes)	0.00529	0.00132	-0.00491
	(0.00696)	(0.00578)	(0.00604)
Log(1+Per Capita m.Exp)	-0.00262	-0.000892	0.00382
	(0.00502)	(0.00449)	(0.00417)
Risk Aversion	-0.0135	0.000140	-0.00285
	(0.0113)	(0.00937)	(0.00933)
1(Muslim name=Yes)	-0.0116	-0.0167	0.00908
	(0.0133)	(0.0104)	(0.0116)
1(Irrigate=Yes)	-0.0189	-0.0260**	-0.00407
	(0.0150)	(0.0115)	(0.0134)
Constant	54.98***	38.03***	39.49***
	(3.777)	(3.196)	(3.257)
Observations	4,133 [60]	4,133 [60]	4,133 [60]
R-squared	0.095	0.069	0.076
Mkt Year FEs	Yes	Yes	Yes
Linear Trend	Yes	Yes	Yes
Village FEs	Yes	Yes	Yes

Notes: Table reports the results from regressions of risk-sharing treatment groups on a vector of household characteristics. **1**(.) is a logical indicator that equals 1 whenever the argument in the bracket is true, and 0 otherwise. Columns include the set of all seventeen (17) demographic characteristics. Errors are robust to heteroskedasticity. Stars indicate significance: $***$, $**$, and $*$ stand for significance at the 1%, 5%, and 10% level, respectively.

Table 4.17: Wealth Control: Index Demand-Group Identity linkages

VARIABLES	(1) bought	(2) bought
Group cues	-0.188***	-0.188***
	(0.0273)	(0.0270)
Discount	0.00290***	0.00279***
	(0.000549)	(0.000548)
Group cues **X** Discount	0.0992***	0.0995***
	(0.00768)	(0.00771)
Hindu cues	-0.317***	-0.317***
	(0.0372)	(0.0369)
Hindu cues **X** Discount	0.156***	0.156***
	(0.00713)	(0.00714)
Muslim cues	-0.294***	-0.294***
	(0.0350)	(0.0349)
Muslim cues **X** Discount	0.150***	0.150***
	(0.00494)	(0.00489)
brisk DOWNSIDE [Crop]	-0.0233	
	(0.0183)	
brisk UPSIDE [Crop]	0.630***	
	(0.0236)	
Wealth score	0.00262	0.00337
	(0.00613)	(0.00544)
brisk DOWNSIDE [Revenue]		0.00747
		(0.0235)
brisk UPSIDE [Revenue]		0.691***
		(0.0220)
Constant	0.245***	0.226***
	(0.0454)	(0.0410)
Observations	4,848	4,848
R-squared	0.276	0.326
Mkt Year FEs	Yes	Yes
Household FEs	No	No
Mismatch	CROP	REVENUE
Effects	PRICE	PRICE

Notes: Table shows the results from regressions of take-up for rainfall-index insurance on basis risk and discount assignments–exogenous variation in insurance premium, and interactions with risk-sharing treatments, while controlling for potential wealth effects. Columns (1) and (2) differ based on how basis risk is defined: mismatch between payouts and crop losses in column (1) versus mismatch between payouts and revenue losses in column (2). Errors are clustered at the village level. Stars indicate significance: $***$, $**$, and $*$ stand for significance at the 1%, 5%, and 10% level, respectively.

Table 4.18: Wealth Control: Index Demand-Group Identity linkages

VARIABLES	(1) bought	(2) bought
Group cues	-0.0351	-0.0402
	(0.0544)	(0.0521)
brisk DOWNSIDE [Crop]	-0.0250	
	(0.0195)	
Group cues **X** brisk DOWNSIDE[Crop]	-0.0492	
	(0.106)	
Hindu cues	-0.0102	-0.0120
	(0.0688)	(0.0654)
Hindu cues **X** brisk DOWNSIDE[Crop]	-0.0727	
	(0.102)	
Muslim cues	-0.0744	-0.0567
	(0.0663)	(0.0616)
Muslim cu. **X** brisk DOWNSIDE[Crop]	0.104	
	(0.0892)	
Discount	0.00335***	0.00324***
	(0.000530)	(0.000527)
brisk UPSIDE [Crop]	0.629***	
	(0.0238)	
Wealth score	0.00229	0.00291
	(0.00660)	(0.00572)
brisk DOWNSIDE [Revenue]		0.00717
		(0.0238)
Group cues **X** brisk DOWNSIDE [Rev]		-0.194
		(0.244)
Hindu cues **X** brisk DOWNSIDE [Rev]		-0.0330
		(0.192)
Muslim cu. **X** brisk DOWNSIDE [Rev]		0.312
		(0.259)
brisk UPSIDE [Revenue]		0.691***
		(0.0221)
Constant	0.247***	0.226***
	(0.0459)	(0.0411)
Observations	4,848	4,848
R-squared	0.218	0.267
Mkt Year Fes	Yes	Yes
Household Fes	No	No
Mismatch	CROP	REVENUE
Effects	BASIS RISK	BASIS RISK

Notes: Table shows the results from regressions of take-up for rainfall-index insurance on basis risk and discount assignments–exogenous variation in insurance premium, and interactions with risk-sharing treatments, while controlling for potential wealth effects. Columns (1) and (2) differ based on how basis risk is defined: mismatch between payouts and crop losses in column (1) versus mismatch between payouts and revenue losses in column (2). Errors are clustered at the village level. Stars indicate significance: $***$, $**$, and $*$ stand for significance at the 1%, 5%, and 10% level, respectively.

Table 4.19: Wealth Control: Does Larger Group lead to Lower Demand?

VARIABLES	(1) bought	(2) bought	(3) bought
Group size [Group cu.]	-0.0128***		
	(0.00466)		
brisk DOWNSIDE	-0.101***	-0.0999***	-0.105***
	(0.0182)	(0.0175)	(0.0181)
brisk UPSIDE	0.633***	0.636***	0.633***
	(0.0201)	(0.0194)	(0.0200)
Discount	-0.000851	-0.000836	-0.000876
	(0.000538)	(0.000537)	(0.000544)
Wealth score	0.00143	0.00297	0.00309
	(0.00606)	(0.00609)	(0.00643)
Group size [Hindu cu.]		-0.0175***	
		(0.00570)	
Group size [Muslim cu.]			-0.0104*
			(0.00594)
Constant	0.418***	0.416***	0.400***
	(0.0273)	(0.0266)	(0.0247)
Observations	4,848	4,848	4,848
R-squared	0.157	0.157	0.152
Mkt Year FEs	No	No	No
Village FEs	No	No	No
Group size definition	$\sum_v \mathbf{1}(\text{GroupT} = \text{Yes})$	$\sum_v \mathbf{1}(\text{HinduT} = \text{Yes})$	$\sum_v \mathbf{1}(\text{MislimT} = \text{Yes})$

Notes: Table reports the results from regressions of take-up for rainfall-index insurance on group size, along with controls for basis risk, exogenous changes in premium and potential wealth effects. $\mathbf{1}(.)$ is a logical indicator that equals 1 whenever the argument in the bracket is true, and 0 otherwise. Columns (1)-(3) differ based on how group size is defined. In column (1), group size refers to the number of households that received "Group" cues. In column (2), group size refers to the number of households that received "Hindu" cues. In column (3), group size refers to the number of households that received "Muslim" cues. Errors are clustered at the village level. Stars indicate significance: $***$, $**$, and $*$ stand for significance at the 1%, 5%, and 10% level, respectively.

Table 4.20: Nonlinear Wealth Control: Does Larger Group lead to Lower Demand?

VARIABLES	(1) bought	(2) bought	(3) bought
Group size [Group cu.]	-0.0127***		
	(0.00468)		
brisk DOWNSIDE	-0.105***	-0.104***	-0.109***
	(0.0181)	(0.0172)	(0.0179)
brisk UPSIDE	0.632***	0.635***	0.632***
	(0.0203)	(0.0195)	(0.0201)
Discount	-0.000836	-0.000818	-0.000859
	(0.000540)	(0.000538)	(0.000545)
Wealth Quintile 2	0.0119	0.00938	0.00655
	(0.0230)	(0.0232)	(0.0235)
Wealth Quintile 4	-0.0342	-0.0379	-0.0367
	(0.0249)	(0.0247)	(0.0246)
Wealth Quintile 5	0.0460*	0.0496*	0.0489*
	(0.0250)	(0.0256)	(0.0256)
Group size [Hindu cu.]		-0.0178***	
		(0.00568)	
Group size [Muslim cu.]			-0.0104*
			(0.00585)
Constant	0.416***	0.416***	0.401***
	(0.0308)	(0.0298)	(0.0309)
Observations	4,848	4,848	4,848
R-squared	0.159	0.160	0.155
Mkt Year FEs	No	No	No
Village FEs	No	No	No
Group size definition	$\sum_v \mathbf{1}(\text{GroupT} = \text{Yes})$	$\sum_v \mathbf{1}(\text{HinduT} = \text{Yes})$	$\sum_v \mathbf{1}(\text{MislimT} = \text{Yes}$

Notes: Table reports the results from regressions of take-up for rainfall-index insurance on group size, along with controls for basis risk, exogenous changes in premium and potential *nonlinear* wealth effects (i.e., include wealth quintile dummies: Q1-Q5 with Q1 being omitted category). The coefficient on Q3 is not estimable, since there are no households in the third quintile of the distribution. $\mathbf{1}(.)$ is a logical indicator that equals 1 whenever the argument in the bracket is true, and 0 otherwise. Columns (1)-(3) differ based on how group size is defined. In column (1), group size refers to the number of households that received "Group" cues. In column (2), group size refers to the number of households that received "Hindu" cues. In column (3), group size refers to the number of households that received "Muslim" cues. Errors are clustered at the village level. Stars indicate significance: $***$, $**$, and $*$ stand for significance at the 1%, 5%, and 10% level, respectively.

4.7 Bibliography

1. Arnott, Richard, and Joseph E. Stiglitz. 1991. “Moral Hazard and Nonmarket Institutions: Dysfunctional Crowding Out of Peer Monitoring?” *The American Economic Review* 81(1) : 179-90.

2. Banerjee, Abhijit, Arun Chandrasekhar, Esther Duflo, and Matthew Jackson. 2013. “The Diffusion of Microfinance,” *Science*: 341.

3. Cai, Hongbin, Yuyu Chen, Hanming Fang, and Li-An Zhou. 2009. “Microinsurance, Trust and Eco- nomic Development: Evidence from a Randomized Natural Field Experiment.” National Bureau of Economic Research (NBER) Working Paper 15396.

4. Carter, Michael, Alain de Janvry, Elisabeth Sadoulet, and Alexandros Sarris. 2017. “Index Insurance for Developing Country Agriculture: A Reassessment”. *Annual Review of Resource Economics* vol. 9.

5. Casaburi, Lorenzo, and Jack Willis. 2017. “Time vs. State in Insurance: Experimental Evidence from Contract Farming in Kenya,” Mimeo, Harvard University.

6. Chiappori, Pierre-André and Reny, Philip J. 2016. “Matching to share risk”. *Theoretical Economics*, 11(1): 227-251.

7. Clarke, Daniel J. 2016. “A Theory of Rational Demand for Index Insurance.” *American Economic Journal: Microeconomics* 8(1): 283–306.

8. Cole, Shawn, Daniel Stein, and Jeremy Tobacman. 2014. "Dynamics of Demand for Index Insurance: Evidence from a Long-Run Field Experiment." *American Economic Review*, 104(5): 284-90.

9. Cole, Shawn, Xavier Giné, Jeremy Tobacman, Petia Topalova, Robert Townsend, and James Vickery. 2013. “Barriers to Household Risk Management: Evidence from India.” *American Economic Journal: Applied Economics* 5 (1): 104–35.

10. Duru, Maya. 2016. “Too Certain to Invest? Public Safety Nets and Insurance Markets in Ethiopia”. *World Development.* Volume 78, February 2016, Pages 37-51

11. Giné, Xavier, Robert Townsend, and James Vickery. 2008. “Patterns of Rainfall Insurance Participation in Rural India.” *World Bank Economic Review* 22 (3): 539–66.

12. Giné, Xavier, and Dean Yang. 2009. “Insurance, Credit, and Technology Adoption: Field Experimental Evidence from Malawi.” *Journal of Development Economics* 89 (1): 1–11.

13. Hazell, P., Anderson, J., Balzer, N., Hastrup Clemmensen, A., Hess, U. and Rispoli, F. 2010. “Potential for Scale and Sustainability in Weather Index Insurance for Agriculture and Rural Livelihoods.” U. Quintily: Rome: International Fund for Agricultural Development and World Food Programme.

14. Itoh, Hideshi. 1993. “Coalitions, Incentives, and Risk Sharing.” *Journal of Economic Theory*, 60:410-427.

15. International Research Institute (IRI). 2013. “Using Satellites to Make Index Insurance Scalable: Final IRI Report to the International Labour Organisation - Microinsurance Innovation Facility”. http://iri.columbia.edu/resources/publications/Using-Satellites-Scalable-Index-Insurance-IRI-ILO-report/

16. Jackson, Matthew and Leeat Yariv. 2010. “Diffusion, Sstrategic Interaction, and Social Structure”. *Handbook of Social Economics*, edited by J. Benhabib, A. Bisin and M. Jackson.

17. Jensen, Nathaniel D., Christopher B. Barrett, and Andrew G. Mude. 2014. “Basis Risk and the Welfare Gains from Index Insurance: Evidence from Northern Kenya.” barrett.dyson.cornell.edu/files/ papers/JensenBarrettMudeBasisRiskDec2014.pdf.

18. Karlan, Dean, Robert Osei, Isaac Osei-Akoto, and Christopher Udry. 2014. “Agricultural Decisions after Relaxing Credit and Risk Constraints.” *Quarterly Journal of Economics* 129 (2): 597–65.

19. Kranton, Rachel. 1996. “Reciprocal Exchange: A Self-Sustaining System”. *American Economic Review*, 86 (4), 830-851.

20. McIntosh, C., Sarris, A., and Papadopoulos F. 2013. “Productivity, Credit, Risk, and the Demand for Weather Index Insurance in Smallholder Agriculture in Ethiopia." *Agricultural Economics* (44): 399-417.

21. Mobarak, A. Mushfiq, and Mark Rosenzweig. 2012. “Selling Formal Insurance to the Informally Insured.” Mimeo. Yale University.

22. Munshi, Kaivan. 2011. “Strength in Numbers: Networks as a Solution to Occupational Traps,” *Review of Economic Studies* 78: 1069–1101.

23. Munshi, Kaivan and Mark Rosenzweig. 2009. “Why is Mobility in India so Low? Social Insurance, Inequality, and Growth.” mimeo.

24. Osgood, D.E., McLaurin M., Carriquiry M., Mishra A., Fiondella F., Hansen J., Peterson N., and Ward N. (2007). “Designing Weather Insurance Contracts for Farmers in Malawi, Tanzania, and Kenya.” Final Report to the Commodity Risk Management Group, ARD, World Bank. International Research Institute for Climate and Society (IRI), Columbia University, New York, USA. https://iri.columbia.edu/~deo/IRI-CRMG-Africa-Insurance-Report-6-2007/IRI-CRMG-Kenya-Tanzania-Malawi-Insurance-Report-6-2007.pdf

25. Townsend, Robert M. 1994. "Risk and insurance in village India." *Econometrica*, 62, 539–591.

26. Udry, Chris. 1990. "Rural Credit in Northern Nigeria: Credit as Insurance in a Rural Economy." *World Bank Economic Review,* 4, 251-269.

27. Wang, Xiao Yu. 2014. "Risk Sorting, Portfolio Choice, and Endogenous Informal Insurance". NBER Working Paper no. 20429.

28. Wilson, Robert. 1968. "The theory of syndicates." *Econometrica*, 36, 119–132.

29. World Development Report. 2014. "Risk and Opportunity: Managing Risk for Development." http://siteresources.worldbank.org/EXTNWDR2013/Resources/8258024-1352909193861/8 1356011448215/8986901-1380046989056/WDR-2014_Complete_Report.pdf

4.8 Appendix

Proof of Lemma 1

The proof for Lemma 1 is similar to arguments in Wang (2014).

Let z_i and z_g denote the income of individual i and representative individual g. Suppose i and g form a pair. We denote the combined income of the pair, $z_{i^*} \equiv z_i + z_g$. If i wishes to promise utility ξ to his partner g, then the corresponding efficient sharing rule $(z_{i'} - s(z_{i^*}, \xi),\ s(z_{i^*}, \xi))$ must satisfy

$$s^*(z_{i^*}, \xi) \equiv \arg\max_s Eu_i(z_{i^*} - s) \quad s.t. \quad Eu_g(s) \geq \xi \tag{4.1}$$

Varying ξ, the solutions s^* describe the set of efficient sharing rules.

Let $f(z_{i^*})$ denote the joint density function for combined income. Plugging in the utility functions of the individuals allows us to restate the above optimization program as

$$\max \int -e^{-\gamma_i(z_{i^*} - s(z_{i^*}))} f(z_{i^*}) dz$$
$$s.t. \int -e^{-\gamma_g s(z_{i^*})} f(z_{i^*}) dz \geq -e^{-\xi}$$

The inequality in the constraint will hold with equality since transferring income to individual g comes at the cost of reducing i's income.

Solving the constrained optimization problem gives us

$$s^*(z_{i^*}) = \frac{\gamma_i}{\gamma_i + \gamma_g} z_{i^*} + \frac{1}{\gamma_g} \log(\int -e^{-\frac{\gamma_i \gamma_g}{\gamma_i + \gamma_g} z_{i^*}} f(z_{i^*}) dz) + \frac{1}{\gamma_g} \xi$$

This allows us to rewrite individual i's expected utility as

$$Eu_i(\xi) = -e^{\frac{\gamma_i}{\gamma_g}\xi} (\int -e^{-\frac{\gamma_i \gamma_g}{\gamma_i + \gamma_g} z_{i^*}} f(z_{i^*}) dz)^{\frac{\gamma_i + \gamma_g}{\gamma_g}}$$

where as individual g's expected utility can be written as

$$Eu_g(\xi) = -e^{-\xi}$$

For individual i with CARA utility function with income z_i, there is a simple relation between the certainty equivalent (CE_i) and the expected utility:

$$-e^{-\gamma_i CE_i} = E(-e^{-\gamma_i z_i})$$

which gives us

$$CE_i = -\frac{1}{\gamma_i} \log E(e^{-\gamma_i z_i})$$

We apply this to the efficient risk sharing problem to get

$$CE_g = \frac{\xi}{\gamma_g}$$

and

$$CE_i = -(\frac{1}{\gamma_i} + \frac{1}{\gamma_g}) \log(\int -e^{-\frac{\gamma_i \gamma_g}{\gamma_i + \gamma_g} z_{i^*}} f(z_{i^*}) dz) - \frac{1}{\gamma_g} \xi$$

Thus we observe that increasing certainty individual of individual g by one unit leads to a reduction in certainty equivalent of individual i by one unit. Hence certainty equivalents are transferable across individuals and since expected utility is a monotonic transformation of certainty equivalent, we get that the expected utility is transferable as well. This concludes the proof of Lemma 1.

Proof of Lemma 2

From the proof of Lemma 1, we found that if risk is shared efficiently then we get

$$\begin{aligned}
CE_i + CE_g &= -(\frac{1}{\gamma_i} + \frac{1}{\gamma_g}) \log(\int -e^{-\frac{\gamma_i \gamma_g}{\gamma_i + \gamma_g} z_{i^*}} f(z_{i^*}) dz) \\
&= -\frac{1}{\gamma_{i^*}} \log(\int -e^{-\gamma_{i^*} z_{i^*}} f(z_{i^*}) dz) \\
&= -\frac{1}{\gamma_{i^*}} \log E(e^{-\gamma_{i^*} z_{i^*}})
\end{aligned}$$

With TU, the sum of the CEs correspond to the joint maximization of the group (i, g)'s welfare. From the last equality, this is identical to the maximization problem of a representative individual with risk aversion parameter $\gamma_{i^*}$and income process z_{i^*}.

Further, since $\frac{1}{\gamma_{i^*}} = \frac{1}{\gamma_i} + \frac{1}{\gamma_g}$ we have that $\gamma_{i^*} = \frac{\gamma_i \gamma_g}{\gamma_i + \gamma_g} < \min(\gamma_i, \gamma_g)$.

Proof of Lemma 3

Let CE_g^0, $CE_{i^*}^0$ denote the certainty equivalent for the group g without individual i and the certainty equivalent for group g with individual i joining respectively. We want to show that $CE_{i^*}^0 > CE_g^0 + CE_i^0$. Notice that:

$$CE_{i*}^0 = w_i + w_g - \frac{\gamma_{i^*}(\sigma_i^2 + \sigma_g^2)}{2} - \frac{1}{\gamma_{i^*}} \log([1-p] + pe^{\gamma_{i^*} L})$$

and

$$CE_g^0 = w_g - \frac{\gamma_g \sigma_g^2}{2}$$

Hence it is sufficient to show that

$$\begin{aligned}
w_i + w_g - \frac{\gamma_{i^*}(\sigma_i^2 + \sigma_g^2)}{2} - \frac{1}{\gamma_{i^*}} \log([1-p] + pe^{\gamma_{i^*} L}) &> w_g - \frac{\gamma_g \sigma_g^2}{2} + w_i - \frac{\gamma_i \sigma_i^2}{2} - \frac{1}{\gamma_i} \log([1-p] + pe^{\gamma_i L}) \\
-\frac{\gamma_{i^*}(\sigma_i^2 + \sigma_g^2)}{2} - \frac{1}{\gamma_{i^*}} \log([1-p] + pe^{\gamma_{i^*} L}) &> -\frac{\gamma_g \sigma_g^2}{2} - \frac{\gamma_i \sigma_i^2}{2} - \frac{1}{\gamma_i} \log([1-p] + pe^{\gamma_i L})
\end{aligned}$$

The last inequality follows from the following two claims:

CLAIM 1: $\frac{\gamma_g \sigma_g^2}{2} + \frac{\gamma_i \sigma_i^2}{2} > -\frac{\gamma_{i^*}(\sigma_i^2 + \sigma_g^2)}{2}$

Proof: This follows from observing that $\gamma_{i^*} < \min(\gamma_g, \gamma_i)$ by lemma 2.

CLAIM 2: $-\frac{1}{\gamma_{i^*}} \log([1-p] + pe^{\gamma_{i^*} L}) > -\frac{1}{\gamma_i} \log([1-p] + pe^{\gamma_i L})$

Proof: This follows from observing that the LHS is the CE for a representative agent with risk aversion γ_{i^*} for a gamble v while the RHS is the CE for an individual with risk aversion $\gamma_i > \gamma_{i^*}$ for the same gamble v. Since CE is decreasing in risk aversion, the claim follows.

Chapter 5

Additional Bibliography

1. Christian, Gollier. 2004. "The Economics of Risk and Time." New Ed Edition. MIT Press.

2. Dercon, Stefan and Luc Christiaensen. 2011. "Consumption Risk, Technology Adoption and Poverty Traps: Evidence from Ethiopia." *Journal of Development Economics*, 96(2): 159–173.

3. Intergovernmental Panel on Climate Change 2013. "Climate Change 2014: Impacts, Adaptation, and Vulnerability." Part A: Global and Sectoral Aspects. Contribution of Working Group II to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change. http://www.ipcc.ch/report/ar5/wg2/