Stockholm University

Information, Markets and Conflict

Essays on Development and Political Economics

David Yanagizawa Drott

ISSN 0346-6892
ISBN 978-91-7447-040-6

Cover Picture: David Yanagizawa Drott, *Drive By*
© David Yanagizawa Drott
Printed in Sweden by PrintCenter US-AB, Stockholm 2010
Distributor: Institute for International Economic Studies

Doctoral Dissertation
Department of Economics
Stockholm University

Abstract

This thesis consists of four essays.

The first essay, "The Strategic Determinants of U.S. Human Rights Reporting: Evidence from the Cold War", uses a country-level panel dataset to test the hypothesis that the United States biases its human rights reports of countries based on the strategic value of these countries. As a measure of U.S. bias, the difference between the U.S. State Department's and Amnesty International's reports is used. The results show that allying with the U.S. during the Cold War significantly improves reports on a country's human rights situation from the U.S. State Department relative to Amnesty International.

The second essay, "Watchdog or Lapdog? Media and the U.S. Government during the Cold War", builds on the first and investigates the extent to which strategic objectives of the U.S. government influenced news coverage during the Cold War. Two relationships are established: 1) strategic objectives of the U.S. government cause the State Department to under-report human rights violations of allies; and 2) these objectives reduce the news coverage of human rights abuses for allies in six United States national newspapers. In addition to the main results, qualitative evidence and indirect quantitative evidence is provided to shed some light on the mechanisms underlying the reduced form effects.

The third essay, "Propaganda and Conflict: Theory and Evidence from the Rwandan Genocide", investigates the impact of propaganda on participation in violent conflict. It examines the effects of the infamous "hate radio" station Radio RTLM that called for the extermination of the Tutsi ethnic minority population before and during the 1994 Rwanda Genocide. Consistent with strategic complements in violence, the results show that Radio RTLM: 1) increased participation in violence, and that the effects were; 2) highly non-linear in radio coverage; 3) decreasing in ethnic polarization; and 4) decreasing in literacy rates. Finally, the estimated effects are substantial. Complete village radio coverage increased violence by 65 to 77 percent, and a simple counter-factual calculation suggests that approximately 9 percent of the genocide, corresponding to at least 45 000 Tutsi deaths, can be explained by the radio station.

The fourth essay, "Tuning in the Market Signal: The Impact of Price Information on Market Exchange in Uganda", estimates the impact of access to market price

information on agricultural market outcomes. Specifically, predictions are derived from a simple model and tested by exploiting a natural experiment from Uganda, where rural radio stations broadcast information on crop prices in urban market centers. The results show that the information: 1) increases the likelihood that informed farmers engage in market exchange; 2) causes farmers to sell larger shares of their output; 3) have larger supply effects for crops with larger market price uncertainty; 4) increases the price farmers receive at the farm-gate; and 5) decreases the price in the urban market centers. Together, the results indicate that the access to price information reduces market failures due to asymmetric information between farmers and traders, and leads to increased market activity and incomes for informed farmers.

To my parents

Acknowledgements

I still remember vividly when I made my decision. I was in Kigali on a scholarship for my master thesis, trying to understand what had happened in 1994. Why people did it. I had met with many people providing insights. Academics at the National University of Rwanda. Survivors at the church in Nyamata. Taxi drivers. But still, more questions than answers had emerged. The most disturbing one, perhaps: Can one, at all, explain such seemingly incomprehensible events? I was skeptical. Or maybe academia simply wasn't for me. Working with foreign aid, international affairs or economic policy somehow seemed more fruitful and rewarding. Concerned with thoughts about what drives people to kill their neighbors, and the limits of research, I was simultaneously running in and out of offices. The National Bank of Rwanda. The World Bank. Dfid. Sida. Working on a policy report that Arne Bigsten, one of my professors at University of Gothenburg, kindly had brought me along for, the aim was to give recommendations to the Swedish aid agency Sida. How to stimulate economic growth. How to reduce poverty. Big questions, but even larger question marks. What is the best way to reduce poverty? I, for sure, did not have the answer.

Still waiting to receive a reply to my Ph.D. program application to Stockholm University, and still waiting for a guiding gut feeling to emerge whether a Ph.D. was worth pursuing, I was having a beer with Arne one evening after a long day of work. Sitting there, in the equatorial heat by the pool at Hôtel des Mille Collines, I asked him about his thoughts on me doing graduate studies in Stockholm given my interests in development and political economy. That is when I first heard about the Institute for International Economic Studies. I was informed that if I were to get the opportunity to do my Ph.D. there I would be surrounded by the very best economists in Sweden, several of them leading in their fields. And, in particular, one of the leading development economists and for sure the best in Sweden, I was told, worked there. Back in my hotel room, I went online to check out some of the research that had been published by people at the Institute. And as I read the papers by Jakob Svensson, especially his work on corruption, I was completely blown away. It made me see the possibilities of research rather than its imagined limits. I was thrilled. Inspired. As I went to bed that night, I had made my decision. I would do a Ph.D. I would try to get into the Institute. And I would try to get Jakob Svensson as my supervisor.

I am now leaving the Institute after having been here for three years. Although

my expectations of the Institute were high to begin with, they have by far been surpassed by my experience. It has been a true pleasure to be a part of it. The great atmosphere. The high aspirations. The brilliant people. All of it. I would therefore like to thank everybody I have had the pleasure to interact with during these years. A few special thanks are in also order.

Needless to say, my supervisor Jakob Svensson has been key to my Ph.D. studies. Not only has he been an inspiration for me to do interesting and important research, he was supportive of me right from the beginning. He encouraged me to pursue my research ideas, he sent me off on research missions to South Africa, Uganda and Rwanda, he made sure that my projects did not die when they were about to, and he gave me invaluable advice. Feeling that someone believes in you can make all the difference for a graduate student trying to figure out what do to with his academic life. It did to me, and I would never have landed a great job if it was not for Jakob.

I also owe a special thank to Torsten Persson, whose support, clarity of thought and advice has been immensely valuable both to my research and through the job market process. And to David Strömberg, who has been a great inspiration for me. It is not a coincidence that the essays in this thesis revolve around mass media and information.

I am much indebted to Nancy Qian. Without her advice, support and friendship my academic path would have looked very different. And to Per Krusell, Ethan Kaplan, and Emilia Simeonova for providing intellectual guidance and everyday fun. I am especially thankful to Christina Lönnblad for excellent editorial assistance and all-encompassing support, and to Annika Andreasson for all the help and humor that she brings. I wish to also thank all my fellow graduate students for all the friendship, and especially Johan Gars for many interesting discussions and Olle Folke for his insights and help.

Very special thanks go to all my friends outside academia, all of them an inspiration in life. To my parents, who have given me love and support and always encouraged me to pursue my goals. To Katsue and Isamu for always being there, and to the warm Drott family, whose entry into my life has given me new inspiration.

Finally, I wish to thank my wife Lisa for all her love and support throughout the years. And especially for being willing to step out into unknown territory and share the adventure with me.

Stockholm, May 2010

David Yanagizawa Drott

Table of Contents

Chapter 1

Introduction

This thesis consists of four empirical essays in development and political economics. Although the essays are self-contained, there is a common denominator: The role of mass media and information in political and economic contexts related to developing countries.

Broadly speaking, the essays in this thesis seek to understand two things. First, to understand the political and economic *determinants* of the type of information that will be supplied to the public through different mass media. Second, to understand the *effects* of information supplied by mass media on economic and political outcomes in developing countries.

These are arguably important issues to study. Regarding its effects, mass media has the potential to provide correct information to large amounts of people at a low cost. Such information can not only enable individuals to make informed decisions, it can also improve the functioning of markets. In particular, it has long been recognized in economic theory that asymmetric information in markets may lead to market failures (e.g., Akerlof 1970, Stiglitz, 1974). By providing information that removes or reduces such market frictions mass media has, in theory, the ability to improve the welfare of societies. However, the extent to which information can improve the functioning of markets in developing countries is not well understood empirically. This thesis aims to fill that gap.

Moreover, mass media may not necessarily only have socially desirable effects. Since mass media is a powerful tool for disseminating information shaping what beliefs people hold it may, in turn, lead to socially undesirable behavior. A long-standing question is whether mass media can produce extreme events such as mass violence or civil conflict. Contrary to common beliefs, however, previous empirical research in political science and communication studies has found little or no evi-

dence that mass media can have such dramatic effects as to cause masses of people to kill their fellow citizens. As Straus (2007, p. 1) notes:

> "More than 50 years ago, fascism and genocide in Europe helped spawn one of the most prominent research agendas in political science, one that focuses on the effects of mass media. The Nazi propaganda machine in particular raised the specter that modern media could have a profound impact on society, whether as social control or, at the limit, as a device to condition a society for exterminatory violence. After years of increasingly sophisticated empirical research, however, research in political communications has largely distanced itself from such views. Rare is the contemporary communications scholar who would claim that modern media have undifferentiated, direct, and massive effects on political behavior, ones capable of precipitating mass violence."

Contrary to previous studies, this thesis is able to provide novel evidence showing that mass media can have substantial effects on mass violence.

Furthermore, regarding the determinants of information provided through mass media, there is nothing intrinsic that ensures that the information is necessarily unbiased or correct. In fact, by shaping political beliefs in desired directions, power-seeking individuals or government officials may benefit from a biased supply of information to the public (Besley and Prat, 2006). In autocratic and totalitarian countries, where the government owns or controls the media, supplying biased information is not a difficult task. However, in democratic countries where the mass media is independent and "free", it is less clear and not well understood whether the government can control the information supplied to the public. This thesis aims to understand to what extent governments in democracies try to manipulate information, and if so, to what extent they succeed in doing so.

Chapter 2 "The Strategic Determinants of U.S. Human Right Reporting: Evidence from the Cold War" (coauthored with Nancy Qian) examines whether, and to what extent, the U.S. State Department biases their reports of human rights violations of developing countries depending on their strategic value to the United States.[1]

[1] This paper was published in the Journal of the European Association, Paper and Proceeedings, 2009.

It is not uncommon for governments, non-governmental organizations and private firms to justify major economic decisions based on perceived human rights situations in countries that they deal with. In June, 2008, U.S. Commerce Secretary, Carlos Gutierrez, explained that the U.S. must continue its trade embargo on Cuba because the latter "systematically brutalizes its people".[2] For private firms, Blanton and Blanton (2007) found that Foreign Direct Investment (FDI) decisions are correlated with U.S. State Department reports on the levels of human rights violations. Using human rights as a determinant of private investment and economic policy is not, *prima facie*, a cause for particular concern. However, critics of the U.S. State Department have complained that it unfavorably biases its human rights reports against countries that have opposing ideologies and favors countries that are strategically valuable to the U.S.[3] It might then be surprising that there are no studies in political economy examining the accuracy of human rights reports and the determinants for the potential biases of reporting agencies.

To the best of our knowledge, this is the first study in economics to address the important question of whether governments strategically bias human rights reports. We build on numerous works, mostly qualitative, in political science and international relations about the different factors associated with human rights reports. However, these studies suffer from methodological shortcomings making claims to causality difficult to defend.

We use a panel dataset of countries from 1976-2005 and compare U.S. State Department reporting of human rights violations relative to Amnesty International, where Amnesty reports serves as a measure of "unbiased" reports. And we interpret the difference between U.S. and Amnesty reports as the U.S. bias. We then use the Cold War and its abrupt end in 1990 for plausibly exogenous variation in strategic value of allies to the U.S.

The results indicate that the U.S. shows significant favoritism in reporting towards countries that are valued strategically. The estimated are also substantial. Taken literally, they imply that if Soviet-friendly Hungary had been as allied to the U.S. during the Cold War as U.S.-friendly Turkey, the U.S. would have underreported Hungary's human rights violations by one index point relative to Amnesty, bringing it to the same level as Sweden.

To what extent biased human rights reports will also lead to a bias in the information consumed by the public, however, is unclear. In particular, if the mass

[2]Letters to the Editor, *Washington Post*, Monday, June 9, 2008; Page A16.

[3]For example, see Stohl and Carleton (1985) and Mitchell and McCormick (1988).

media functions well there may not be a bias. In fact, as "the fourth estate", it is often argued that an independent media providing unbiased information to the populace is essential to the healthy functioning of the democracy. This leads us to the second essay.

Chapter 3 "Watchdog or Lapdog? Media and the U.S. Government during the Cold War" builds on the first (and is also coauthored with Nancy Qian), and investigates the extent to which the U.S. government can systematically influence the news coverage by U.S. newspapers.

Governments can clearly influence the information reported by media outlets that they own, but their ability to do so with independently owned outlets operating in a competitive market is less obvious. In theory, market competition and independent ownership should act as safeguards against government manipulation of the media (Besley and Prat, 2006). According to this, the U.S. media should be safer from government influence than most other media markets in the world (Djankov et al., 2000). In practice, although studies such as Prat and Strömberg (2005) suggest that competition in the United States increases the likelihood that news organizations will report the truth, the extent to which the U.S. media is free from government influence is an open empirical question.

This study attempts to fill this gap by measuring the extent to which the U.S. government can systematically influence the news coverage of the commercial press. In particular, we estimate the effect of strategic objectives on United States State Department reports of human rights abuses in foreign countries and the effect of these objectives on news coverage in six independently owned U.S. newspapers during the latter part of the Cold War, 1976-88.

The principal contribution of this study is to provide novel empirical evidence of the causal effect of strategic objectives on U.S. commercial news coverage. To establish causality, we exploit the plausibly exogenous variation in an ally's strategic value that results from an ally's entry onto the United Nations Security Council.[4]

The results show that an increase in strategic value to the United States significantly reduced State Department reports of human rights violations, had no effect on Amnesty reports, and significantly reduced news coverage of abuses in the commercial press. For example, for Cold War allies such as Brazil, Zaire, Honduras and Chile, UN Security Council membership during the Cold War decreased newspaper

[4]We are not the first to exploit variation in UNSC membership, see Kuziemko and Werker (2006) for the first study.

reports of abuses in these countries by approximately 27%, 64%, 65% and 80%, respectively.

The main results are consistent with qualitative evidence showing that the government is able to systematically influence news coverage. To further understand the mechanism, we find that the magnitudes of the effects across newspapers seem to be uncorrelated with the conservativeness of newspapers, but positively correlated with the quality. The higher is the quality of news reporting, the larger is the estimated effect of government influence. We discuss the potential implications of this surprising stylized fact in the context of existing theories about the relationship between the government and mass media.

Chapter 4 "Propaganda and Conflict: Theory and Evidence from the Rwandan Genocide" investigates the impact of propaganda on participation in one of the most extreme violent events in history: the 1994 Rwanda Genocide.

During a period of only three months, a nation-wide extermination campaign led by the Rwandan government against the Tutsi ethnic minority population resulted in at least 500 000 Tutsi civilian deaths and a reduction by approximately 75% of the country's Tutsi population (des Forges, 1999). In addition to the violence organized by the army and militias, the high intensity killings were achieved by mass participation by hundreds of thousand ethnic Hutu citizens using their machetes and clubs (des Forges, 1999; Straus, 2004; Verwimp, 2006).

The principal aim of the essay is to estimate the impact of one factor widely believed (BBC, 2003; Thompson, 2007) to have played a significant role in the genocide: propaganda spread by the infamous "hate radio" station Radio Télévision Libre des Mille Collines (RTLM). To understand the relationship between propaganda and participation in conflict, the essay first presents a simple model. It then tests the predictions of model using data from the Rwandan genocide.

To establish causal effects of the propaganda, the essay exploits plausibly exogenous variation in Radio RTLM radio coverage generated by Rwanda's highly varying topography, which arguably makes radio coverage across villages as good as randomly assigned.

The main results show that Radio RTLM broadcasts had a substantial effect on violence. The estimates imply that going from no to full village radio coverage increased civilian violence by 65 percent and organized violence by 77 percent. Furthermore, the results show that there were no effects of the propaganda in villages with relatively high literacy rates, consistent with propaganda effects depending on

individuals' ability to access independent information. The estimates imply that Radio RTLM caused approximately 9% of the genocide violence, which corresponds to at least 45 000 Tutsi deaths, and that the radio station was a quantitatively important causal factor in the genocide.

In addition, the essay presents a set of results predicted by the model under strategic complements in violence. Together, the results indicate the propaganda appears to function as a violence coordination device.

Chapter 5 "Tuning in the Market Signal: The Impact of Price Information on Market Exchange in Uganda" investigates how access to information on market prices affects agricultural market outcomes.[5]

One of the fundamental results in economic theory is that in perfectly competitive markets, where price taking producers and consumers are assumed to trade goods at publicly known prices, the allocation of goods in the economy is efficient. In developing countries, however, these assumptions stand in sharp contrast to the reality faced by the main economic agents in the economy: small-scale rural farmers. While a majority of the population in developing countries live in rural areas and make their livelihood mainly from farming crops, access to updated information on prevailing prices in urban market centers is limited due to low levels of information and communication infrastructure. Furthermore, when farmers choose to sell parts of their agricultural output, they typically do so by engaging in trade with local traders that buy their crops at the farm-gate, often with limited competition from other traders (Ferris, 2004), and resell them in urban market centers. Importantly, while rural farmers have little access to updated price information, traders that constantly travel between rural areas and the market centers are naturally relatively well informed about the prevailing market prices.[6] The resulting economic exchange at the farm-gates could therefore be characterized as outcomes of a contracting process between traders and farmers, where traders have superior information relative to farmers. This type of asymmetric information, in turn, may lead to market frictions with sub-optimally low levels of market exchange. Supplying correct information on market prices may therefore alleviate such frictions and improve the functioning of markets in developing countries.

We present a simple model of the agricultural sector in a developing country

[5] A small subset of the results in this essay was published in the Journal of the European Association, Paper and Proceeedings, 2009.

[6] See, for instance, Ferris (2004) and Banerji and Meenakshi (2004).

setting and test the predictions of the model by exploiting a natural experiment - the Market Information Service (MIS) in Uganda - to assess the impact of providing crop market price information to small-scale farmers. Starting in 2000, the MIS collected weekly data on market prices for some of the main agricultural commodities in 21 of Uganda's 56 districts, and disseminated the information through local FM radio stations in the participating districts. The presumption was that the provision of accurate, timely and appropriate market information to farmers through radio transmissions would improve their ability to bargain with local traders. Using data from the Uganda National Household Surveys of 2000 and 2005, as well as the data on market prices collected by MIS, we study the effects of giving farmers access to market price information on the likelihood of farmers selling their crops, the share of the output sold, the prices received, and urban market prices. To identify the causal effects of access to information, we exploit differences in exposure over time, across space, and between crops.

The results show that access to market information increases the likelihood of selling the crop by 29% (from a baseline of 0.23), the share of output sold by 32% and the price by 0.41 standard deviations. As a result, the crop income of farmers with access to market price information increased by an estimated 55%. We show that the positive supply response from information lead to substantially lower prices for consumers in urban market centers, as the introduction of broadcasts decreased the market price by 0.94 standard deviations.

Taken together, our results suggest that by reducing contractual frictions between farmers and traders, farmers' access to market price information substantially improves the income of informed farmers and the price levels for urban consumers, as well as the functioning of agricultural markets in developing countries.

Bibliography

[1] BBC (2003), "The Impact of Hate Media in Rwanda," BBC article, <news.bbc.co.uk/2/hi/africa/3257748.stm>, Available (2009-11-06)

[2] Besley, Timothy and Prat, Andrea. (2006), "Handcuffs for the Grabbing Hand? Media Capture and Government Accountability." *American Economic Review* 96.3: 720-36.

[3] Blanton, Shannon Lindsey and Robert C. Blanton (2007), "What Attracts Foreign Investors? An Examination of Human Rights and Foreign Direct Investment." *The Journal of Politics* 69.1: 143-55.

[4] Banerji, A. and Meenakshi, J.V. (2001), "Competition and Collusion in Grain Markets: Basmati Auctions in North India", Centre for Development Economics Working Paper 94, University of Delhi, India.

[5] Des Forges, Alison. (1999), *Leave None to Tell the Story: Genocide in Rwanda.* Human Rights Watch and the International Federation of Human Rights Leagues, New York, NY, USA.

[6] Djankov, Simeon, et al. (2000), "Who owns the media?" *Journal of Law and Economics*, 46.Ferris, S., 2004, "FOODNET: Information is Changing Things in the Marketplace", *ICT Update*, Issue 18, CTA, The Netherlands.

[7] Ferris, Stephen. (2004), "FOODNET: Information is Changing Things in the Marketplace", *ICT Update*, Issue 18, CTA, The Netherlands.

[8] Kuziemko, Ilyana, and Eric D. Werker (2006), "How Much Is a Seat on the Security Council Worth? Foreign Aid and Bribery at the United Nations." *Journal of Political Economy* 114, no. 5, October. 2006: 905-930.

[9] Mitchell, Neil J. and McCormick, James M. (1998), "Economic and Political Explanations of Human Rights Violations", *World Politics* 40, pp 476-98.

[10] Poe, Steven C., Sabine C. Carey and Tanya C. Vasquez. (2001), "How are these pictures different? A quantitative comparison of the U.S. State Department and Amnesty International Human Rights Reports, 1976-95", *Human Rights Quarterly* 23, pp 650-77.

[11] Stiglitz, Joseph. (1974), "Incentives and Risk Sharing in Sharecropping". *The Review of Economic Studies* 41 (2): 219–255.

[12] Stohl, Michael and Carleton, David. (1985), "Foreign Policy of Human Rights: Rhetoric and Reality from Jimmy Carter to Ronald Reagan, *Human Rigths Quarterly* 7, p 205-29.

[13] Straus, Scott. (2004), "How Many Perpetrators Were There in the Rwandan Genocide? An Estimate," *Journal of Genocide Research,* 6(1):85-98.

[14] Straus, Scott. (2007), "What Is the Relationship between Hate Radio and Violence? Rethinking Rwanda's "Radio Machete"," *Politics & Society*, 35:609.

[15] Verwimp, Philip. (2006), "Machetes and Firearms: the Organisation of Massacres in Rwanda," *Journal of Peace Research*, 43(1):5-22.

[16] Thompson, Allan. (2007), *The Media and the Rwandan Genocide*. London: Pluto Press.

Chapter 2

The Strategic Determinants of U.S. Human Rights Reporting: Evidence from the Cold War*

1 Introduction

It is not uncommon for governments, non-governmental organizations and private firms to justify major economic decisions based on perceived human rights situations in countries that they deal with. In June, 2008, U.S. Commerce Secretary, Carlos Gutierrez, explained that the U.S. must continue its trade embargo on Cuba because the latter "systematically brutalizes its people".[1] For private firms, Blanton and Blanton (2007) found that Foreign Direct Investment (FDI) decisions are correlated with U.S. State Department reports on the levels of human rights violations. Using human rights as a determinant of private investment and economic policy is not, *prima facie*, a cause for particular concern. However, critics of the U.S. State Department have complained that it unfairly biases its human rights reports against countries that have opposing ideologies and favors countries that are strategically valuable to the U.S.[2] It might then be surprising that there are no studies in political economy examining the accuracy of human rights reports and the determinants for the potential biases of reporting agencies. This study aims at filling this gap by

* This paper is co-authored with Nancy Qian, Yale University. We thank Abhijit Banerjee, Mikhail Golosov, Michael Kremer, Gerard Padro-i-Miquel, Torsten Persson, David Stromberg, Jakob Svensson, David Weil and Eric Werker for their insights; and participants at the Brown Applied Micro Lunch, Harvard Development Faculty Lunch, IIES Lunch Seminar, BREAD-CIPREE in Montreal, and the European Economic Association Meetings for useful comments; and Benjamin Feigenberg for excellent research assistance.

[1] Letters to the Editor, *Washington Post*, Monday, June 9, 2008; Page A16.

[2] For example, see Stohl and Carleton (1985) and Mitchell and McCormick (1988).

estimating the extent to which the U.S. State Department biases reports of human rights violations of developing countries depending on their strategic value to the U.S.

To the best of our knowledge, this is the first study in economics to address the important question of whether primary information sources on human rights strategically bias their reports.[3] We build on numerous works in political science and international relations about the different factors associated with human rights reports. These are mostly qualitative. One exception is Poe, Carey and Vazquez's (2001) study that examines factors correlated with the difference between Amnesty and U.S. human rights reports. They find that relative to Amnesty, the U.S. systematically reports its trading partners more favorably and "leftist" regimes less favorably. However, they cannot distinguish the possibility that the U.S. is biased against certain countries from the possibility that those countries do behave worse and that the U.S. has better information than Amnesty. Our study faces a similar problem of omitted variables. Countries of strategic importance to the U.S. may actually have better human rights than other countries and because they are U.S. allies, the U.S. has better access to information. In this case, the observation that alliance with the U.S. results in better human rights reports from the U.S. relative to other agencies will reflect superior information from the U.S. rather than strategic favoritism shown towards its allies.

The principal empirical contribution of this study is to address these difficulties. Like Poe, Carey and Vazquez (2001), we use human rights violations reports from Amnesty International as a measure of "unbiased" reports. And we interpret the difference between U.S. and Amnesty reports as the U.S. "bias". We use the Cold War (CW) and its abrupt end in 1990 for plausibly exogenous variation in strategic value to the U.S. During the CW, the U.S. and the U.S.S.R. competed for the alliance of developing countries. This competition effectively ended when the CW ended. Hence, we assume that the U.S. valued its allies more during the CW than afterwards. By comparing the U.S. bias for countries that ally themselves with the U.S. during the CW to those that do not, before and after the end of the CW, we are able to measure the causal effect of strategic value to the U.S. on reporting

[3] The recent literature on media "slant" does not typically address the extent and the determinants of biases of primary source information that feeds the commercial media. Recent findings by Eisensee and Stromberg (2007) suggest that manipulating the media coverage of developing countries could have significant effects on the amount of aid that democratic governments such as the U.S. feel compelled to give. In this study, we are concerned that reporting agencies can manipulate the quality rather than the quantity of information covered by the commercial media.

bias. Our measure of alliance with the U.S. is the fraction of votes that a country voted in agreement with the U.S. in the United Nations General Assembly (UNGA) during 1985-89 on issues where the U.S. and U.S.S.R. disagreed. The differences-in-differences (DD) strategy addresses the problem that U.S. allies have better human rights in reality *and* the U.S. has better information for its allies. Note that our strategy does not require that Amnesty is unbiased in its reports. It only requires that any bias in Amnesty's reports does not change when the CW ends.

We use a panel data set of 112 low-income countries during 1976-2005 compiled from existing data on UNGA votes, human rights violations, and other country characteristics. The results show that the U.S. and Amnesty have similar reports for countries not allied with the U.S., and they show that these countries on average do not change over time. For U.S. allies, Amnesty reports them as similar to non-U.S. allies, with no changes over time on average. In contrast, the U.S. reports them more favorably during the Cold War, but shows that they converge to non-Allies immediately after the Cold War. Interestingly, there is no difference between U.S. and Amnesty reports after the CW. The DD estimates show that if a country voted with the U.S. during to the Cold War 100% of the time, the U.S. will under-report human rights violations by 2.61 index points (roughly the differences between Zimbabwe and Sweden). Taken literally, this means that if Soviet-friendly Hungary had been as allied to the U.S. during the Cold War as U.S.-friendly Turkey, the U.S. would have under-reported Hungary's human rights violations by one index point relative to Amnesty, bringing it to the same level as Sweden.

The findings of this paper make the point that the strategic determinants of biases of primary information sources constitute an avenue that should be seriously researched. Combined with previous studies which find that U.S. strategic variables are key determinants of U.S. foreign aid (Alesina and Dollar, 2000; Kuziemko and Werker, 2006), our results suggest that the U.S. may manipulate its human rights reports in order to justify financially supporting its allies. Alternatively, they suggest that under-reporting of human rights violations of allied countries shares the same political objectives as foreign aid. Depending on the extent to which firms and non-government organizations depend on the information provided by the U.S. State Department, this manipulation may have far-reaching economic consequences.

This paper is organized as follows. Section two describes the background. Section three discusses the empirical strategy. Section four describes the data. Section five shows the empirical results. Section six offers concluding remarks.

2 Background

2.1 Human Rights Reports

The *Country Reports on Human Rights Practices* are submitted annually by the U.S. Department of State to the U.S. Congress. The reports cover internationally recognized individual, civil, political, and worker rights, as set forth in the *Universal Declaration of Human Rights*.[4] Amnesty International, commonly known as Amnesty, is one of the only two international non-governmental organizations reporting on human rights abuses world wide. (The other is Human Rights Watch, a U.S. based organization). Officially, Amnesty has the same criteria and focus as the U.S. State Department in creating their Human Rights Reports. Amnesty defines its mission as "to conduct research and generate action to prevent and end grave abuses of human rights and to demand justice for those whose rights have been violated." Founded in the UK in 1961, Amnesty draws its attention to human rights abuses and campaigns for compliance with international standards. While Amnesty is often perceived as having left-leaning sympathies, the organization has actually received criticism for both alleged anti-Western and alleged pro-Western bias. Amnesty proclaims itself as an independent organization.[5]

2.2 Cold War

There is an extensive literature on the Cold War (CW) that is far beyond the scope of this paper to review. This section only seeks to show that the U.S. and the U.S.S.R. competed strenuously for the alliance of developing countries. And that there is no reason for this competition to persist at the same intensity after the demise of the U.S.S.R. Hence, we interpret the end of the CW as a decrease in the strategic value of developing countries for the U.S.

Cold War is the term used to describe the state of conflict, tension and competition that existed between the U.S. and the U.S.S.R. and their respective allies from the mid-1940s to the early 1990s. Direct military attacks on adversaries were deterred by the potential for mutually assured destruction using deliverable nuclear

[4] http://www.state.gov/g/drl/rls/hrrpt/

[5] "We have a number of safeguards in place to protect our autonomy. These are: Independent of any government, political ideology, economic interest or religion; democratic and self-governing; financially self-sufficient, thanks to the generous support of donations provided by individual members and supporters. For details, see http://www.amnesty.org/en/who-we-are/about-amnesty-international

weapons. Instead, rivalry between the two superpowers was expressed through military coalitions, propaganda, espionage, weapons development, industrial advances, competitive technological development, and numerous proxy wars.

The CW spread to every region of the world, as the U.S., under the *Marshall Plan*, sought the "containment" and "rollback" of communism and forged myriad alliances to this end; while the U.S.S.R., under the *Molotov Plan*, fostered Communist movements around the world. The entire world was virtually split into alliance with either the U.S. or the U.S.S.R.[6] Europe was literally divided by the *Iron Curtain*, which divided East and West. There, the CW period was characterized by crises such as the Berlin Blockade (1948–49), the Berlin Crisis of 1961, and the NATO (*North Atlantic Treaty Organization*) exercise in November 1983. In the early 1950s, the U.S. expanded its *containment* into Asia, Africa, and Latin America, in order to counter revolutionary nationalist movements often led by Communist parties financed by the U.S.S.R.[7] In Africa and Central and South America, there were few official treaties. The CW often played a significant role through covert operations. Many countries in Northern Africa received Soviet military aid, while many countries in Central and Southern Africa were supported by the United States and/or its allies (e.g. France).[8]

The CW ended during 1989-91, when the Berlin Wall fell and the U.S.S.R.

[6] Some countries did not want to align themselves with either of the superpowers. The Non-Aligned Movement, lead by India, Egypt, and Austria, attempted to unite the third world against what was seen as imperialism by both the East and the West, see http://www.nam.gov.za/background/background.htm

[7] John Foster Dulles, a rigid anti-communist, aimed at "integrating" the entire noncommunist Third World into a system of mutual defense pacts, initiating the Manila Conference in 1954, which resulted in the SEATO pact that united eight nations (either located in Southeast Asia or with interests there) in a neutral defense pact. These alliances guaranteed the U.S. a number of long-term military bases in the Asia-Pacific (Byrd, 2003), which gave the U.S. significant military advantages during the Korean War (1950-53) and the Vietnam War (1959-75) (La Feber, 1991; Malkasian, 2001). This was soon followed by the Baghdad Pact (1955), later renamed the Central Treaty Organization (CENTO), uniting the "northern tier" countries of the Middle East—Turkey, Iraq, Iran, and Pakistan—in a non-communist defense organization. On the other side, countries such as Egypt, Syria, China, North Korea, and Vietnam chose to ally with the U.S.S.R.

[8] The U.S. involved itself in incidents such as the CIA-assisted removal of Congo's Patrice Lumumba. And countries such as South Africa assisted the U.S. in funding insurgency movements in Soviet allied countries such as Angola and Mozambique during the 1970s. In Latin America, governments of countries such as Argentina, Brazil, Chile, and Uruguay were overthrown or displaced by U.S.-aligned military dictatorships in the 1960s and 1970s. In the 1980s, the U.S. famously revealed itself to be covertly funding the Sandinistas, in what was known as the Iran-Contra affair. Governments such as Peru, Columbia and Nicaragua faced problems of internal conflicts between communist and non-communist groups until the 1980s and 1990s. Famous revolutionaries such as Fidel Castro and Che Guevarra, and groups such as the Nicaraguan Sandinistas all received support from the U.S.S.R. Tensions between the U.S. and U.S.S.R. peaked in Latin America during the Cuban Missile Crisis (1962) (Byrd, 2003).

dissolved. For the purpose of our paper, we loosely interpret 1989-91 as the end of the CW.

3 Identification

The main identification issue when comparing countries that are of strategic value to the U.S. to countries that are not is that these two groups of countries may differ along other dimensions. For example, if countries that are valuable to the U.S. also have better human rights and are more willing to share information with the U.S., then an observed positive U.S. bias for these countries would reflect their actual superiority and the U.S.'s information advantage relative to Amnesty, rather than the U.S. showing favoritism. To address this problem, we exploit the variation in the strategic value of U.S. allies when the CW ended. We argue that competition with the U.S.S.R. caused the U.S. to highly value alliances with developing countries during the CW; and that the change in strategic value caused by the end of the CW is unrelated to any change in the countries themselves. Our strategy is conceptually similar to a differences-in-differences (DD) strategy where we compare the difference in human rights between the U.S. and Amnesty reports between countries that were allied with the U.S. and countries that were not, before and after the end of the CW. Any differences between countries that do not change over time are controlled for by the comparison within countries over time. Any differences over time that affect all countries in the same way will be controlled for by the comparison across countries. Only the interaction of alliance with the U.S. and CW can be interpreted as plausibly exogenous.

We will have a continuous measure of U.S. alliance and yearly data from 1976-2005. Therefore, our first specification fully exploits all the variation in the data and investigates whether changes in the reporting bias for U.S. allies **occur** when the CW ends. This specification also allows us to examine whether there are any pre-trends in how the U.S. may be biased towards its allies that may confound the DD estimates. Note that our measure of whether a country is an U.S. ally during the CW does not vary over time. Hence, we do not face any reverse causality problems that alliance may be affected by the end of the CW.

$$U.S._{it} - Amnesty_{it} = \sum_{t=1976}^{2005} \beta_t (U.S.Ally_{ir} \times year_t) + \gamma_t + \varepsilon_{irt} \tag{2.1}$$

The difference in human rights between U.S. and Amnesty reports for country i in

year t is a function of: the interaction between the extent to which it is allied with the U.S., $U.S.Ally_i$, and a year dummy variable, $year_t$; and year fixed effects, γ_t. The constant is omitted. Standard errors will be clustered at the country level. If strategic value due to the CW caused the U.S. to favorably bias its reports towards its allies, then $\beta_{1976-89} < \beta_{1990-2005}$. (Better human rights **are** reflected in lower scores).

To assess the magnitude and statistical significance of this effect, we then estimate the simpler specification:

$$\begin{aligned} U.S._{irt} - Amnesty_{irt} &= \alpha + \beta(U.S.Ally_{ir} \times ColdWar_t) \\ &\quad + \rho U.S.Ally_{ir} + \gamma_t + \theta_{rt} + \varepsilon_{irt}. \end{aligned} \tag{2.2}$$

The difference in human rights between U.S. and Amnesty reports for country i in region r and year t is a function of: the interaction between the extent to which it is allied with the U.S., $U.S.Ally_{ir}$, and a dummy variable for the period 1976-89, $ColdWar_t$; the main effect for U.S. alliance, $U.S.Ally_{ir}$; region times year fixed effects, θ_{rt}; and year fixed effects, γ_t. Standard errors will be clustered at the country level. Note that this specification controls for the main effect of U.S. alliance rather than country fixed effects and has the added control of region-year fixed effects to control for differential changes across regions over time. The regions are Europe, East Asia and Pacific, Caribbean and Latin America, Sub-Saharan Africa and Other. β is the effect of strategic value to the U.S. due to the CW on U.S. reporting bias. If the U.S. strategically favors its allies, then $\beta < 0$.

Our strategy does not require Amnesty to be truly unbiased. However, it requires that Amnesty does not change its bias when the Cold War ends. For example, if Amnesty favors left leaning countries during the CW and this favor disappears when the CW ends, then our estimates will overstate the true effect of strategic value to the U.S. on U.S. bias. In other words, the DD strategy fails only if the end of the CW also affected the reporting accuracy of the U.S. relative to Amnesty.

4 Data

For human rights violations, we use the Political Terror Scale (PTS). The PTS is an index constructed from human rights reports. Using the same rule, separate indices are constructed from Amnesty International reports and U.S. State Department

reports.[9] Our measure of the U.S. bias is the difference between these indices from the two different sources. The PTS is based on a five-point scale with one being the best and five being the worst.[10]

This index is available for 183 countries over the period 1976-2006. This is not a balanced panel. A few countries are not reported for a few years. And some countries (typically former Soviet Republics) exist only after 1991. We include countries that existed both during and after the CW. Our reported estimates come from a sample where the Ukraine, Belarus and South Africa are excluded. The two former were part of the U.S.S.R. before 1991. And the latter because it was "absent" from all UNGA sessions during the CW period we study. We further restrict the sample to country-year observations where the index is available for both Amnesty International and the U.S. State Department. Amnesty and the U.S. report identical PTS for 84% of the observations.

We construct a measure for U.S. alliance based on UNGA voting data generously provided by Erik Voeten[11] . For each year and each country, we calculate the fraction of votes that a country votes in agreement with the U.S. To capture relevant voting patterns, we restrict the sample to resolutions where the U.S. and the U.S.S.R. voted in opposition of each other. Each year there are approximately 100-150 resolutions in the UNGA, of which approximately 70-90 resolutions per year are disagreed on by the U.S. and U.S.S.R. Our measure of alliance is the fraction of votes that a country voted with the U.S. averaged over the period 1985-89.[12]

The two data sets are matched together at the country-year level. We restrict the sample to non-high income countries as defined by the World Bank. Our matched sample contains 112 countries for 30 years.

We divide the sample at the median country of the U.S. alliance distribution

[9] See http://www.politicalterrorscale.org/about.html for details.

[10] Level 1: Countries operate under a secure rule of law. People are not imprisoned for their views and torture is rare or exceptional. E.g. Belize, 2000. Level 2: There is a limited amount of imprisonment for nonviolent political activity. However, few persons are affected and torture and beatings are exceptional. E.g. Czech Republic, 2000. Level 3: Imprisonment for political activity is more extensive. Politically-motivated executions or other political murders and brutality are common. Unlimited detention, with or without a trial, for political views is also common. E.g. Albania, 2000. Level 4: The practices of level 3 affect a larger portion of the population and murders, disappearances, and torture are a common part of life. E.g. Angola, 2000. Level 5: The terrors characteristic of level-4 countries encompass the whole population at level 5. The leaders of these societies place no limits on the means or thoroughness with which they pursue personal or ideological goals. E.g. Sudan, 2000.

[11] The dataset is available (2008-09-01) at http://www9.georgetown.edu/faculty/ev42/UNVoting.htm

[12] The top three allies of the U.S. and the fraction of divided issues they voted with the U.S. during 1980-84 are: Turkey (0.4), Belize (0.28) and Costa Rica (0.27). The bottom three allies are Mongolia (0), Lao PDR (0), and Czech Republic (0).

(which voted with the U.S. on 7% of the divided resolutions during 1985-89). Figure 1A plots Amnesty's PTS for U.S. allies and non-allies over time. The two groups appear similar, neither changing over time. Figure 1B plots U.S.'s PTS over time. It shows that the U.S. reports that human rights are gradually becoming worse in all countries over time during the CW. However, it consistently reports its allies more favorably. The bias which is represented as the gap between the two lines is constant over time during the CW, and disappears after the CW. This alleviates the concerns that the empirical strategy will be capturing pre-trends to the extent that the U.S. biases reports for its allies.

5 Empirical Results

The estimated coefficients from equation (2.1) and their 95% confidence intervals are plotted in Figure 2.[13] The figure clearly shows that during the CW, the U.S. favored its allies and this favoritism disappeared afterwards. This figure also shows that there are no visible pre-trends during the CW period which could confound the DD estimates. Table 1 shows the results from estimating equation (2.2). We first estimated this equation with U.S. reports and Amnesty reports as separate dependent variables. Column (1) shows that alliance with the U.S. has no effect on human rights reports from Amnesty, on average or during the CW. Column (2) shows that alliance with the U.S. has no effect on human rights reports from the U.S. on average. However, during the CW, being a full-time U.S. ally (voting with the U.S. 100% of the time) can improve a country's PTS report from the U.S. by 2.22 points. This estimate is statistically significant at the 10% level. Column (3) shows that alliance with the U.S. has no effect on the reporting difference between the U.S. and Amnesty on average. However, during the CW, being a full time U.S. ally will improve a country's PTS report from the U.S. by 2.62 points relative to Amnesty. This estimate is statistically significant at the 1% level. Since a country voted with the U.S. at most 40% of the time during the CW (Turkey), the results are more meaningful if we interpolate them linearly. For example, Hungary voted with the U.S. only 2% of the time during the CW when its PTS score was two according to both Amnesty and the U.S. But if Hungary had voted with the U.S. as much as Turkey voted, the U.S. would have under-reported its PTS by approximately one index point relative to Amnesty, making it the same level as Sweden. Our estimates

[13] We do not report the coefficients and standard errors due to space constraints.

are robust to the inclusion of country-specific linear time trends. For brevity, they are not reported .

5.1 Robustness

We consider the possibility that our estimates are also capturing the effects of the changes in American leadership. The pre-period, 1976-1990, was largely led by a Republican executive branch: Reagan during 1981-88 and George H. Bush during 1989-92. The end of the CW roughly coincided with a switch **to** Clinton's Democratic presidency, which lasted from 1993 until 2000. To address this, we examine whether the U.S. alliance had a differential effect during the George H. Bush and Reagan administrations relative to the Carter administration (1977-1980) during the CW, and whether the George H. Bush and George W. Bush administrations (1990-92, 2001-05) had different effects than the Clinton administration during the post CW period. We find that the effect of alliance does not differ between administrations during any of the periods.

6 Conclusion

This study presents evidence indicating that the U.S. shows significant favoritism towards countries that are valued strategically. There is a caveat in interpreting the results. The empirical strategy fails if, after the Cold War, U.S. allies actually become worse *and* the U.S.'s information advantage relative to Amnesty disappears. There are no obvious reasons for this to be the case. But we leave it directly to future studies to address this identification problem. In the meantime, the empirical evidence provided here should hopefully make a convincing case that the accuracy of information we are receiving on human rights from primary sources **is** being strategically biased and that more research is needed on both the causes and the potentially far-reaching consequences of this bias. For example, future studies can investigate the extent to which this bias affects commercial media and information agencies, and financial decisions for firms, non-governmental organizations and governments.

Bibliography

[1] Alesina, Alberto and David Dollar. 2000. "Who gives aid to whom and why?" *Journal of Economic Growth.* 5 (1): 33-63.

[2] Blanton, Shannon Lindsey and Robert C. Blanton. "What Attracts Foreign Investors? An Examination of Human Rights and Foreign Direct Investment," The Journal of Politics 69(1): 143-55 (2007).

[3] Byrd, Peter (2003) "Cold War ", *The Concise Oxford Dictionary of Politics*. Ed. McLean, Iain; McMillan, Alistair. Oxford University Press.

[4] Eisensee, Thomas and Stromberg, David. "News Floods, News Droughts, and U.S. Disaster Relief", *Quarterly Journal of Economics*, 122(2), 2007.

[5] Kuziemko, Ilyana, and Eric D. Werker. "How Much Is a Seat on the Security Council Worth? Foreign Aid and Bribery at the United Nations." *Journal of Political Economy* 114, no. 5 (October 2006): 905-930.

[6] La Feber, Water (1991) *Walter LaFeber Papers*, #14-17-2624. Division of Rare and Manuscript Collections, Cornell University Library, pp. 194–197.

[7] Malkasian, Carter (2001) *The Korean War*, Routledge, pp 16.

[8] Mitchell, Neil J. and McCormick, James M. (1998) "Economic and Political Explanations of Human Rights Violations", *World Politics* 40, pp 476-98.

[9] Poe, Steven C., Sabine C. Carey and Tanya C. Vasquez (2001) "How are these pictures different? A quantitative comparison of the U.S. State Department and Amnesty International Human Rights Reports, 1976-95", *Human Rights Quarterly* 23, pp 650-77.

[10] Stohl, Michael and Carleton, David (1985) "Foreign Policy of Human Rights: Rhetoric and Reality from Jimmy Carter to Ronald Reagan, *Human Rigths Quarterly* 7, p 205-29.

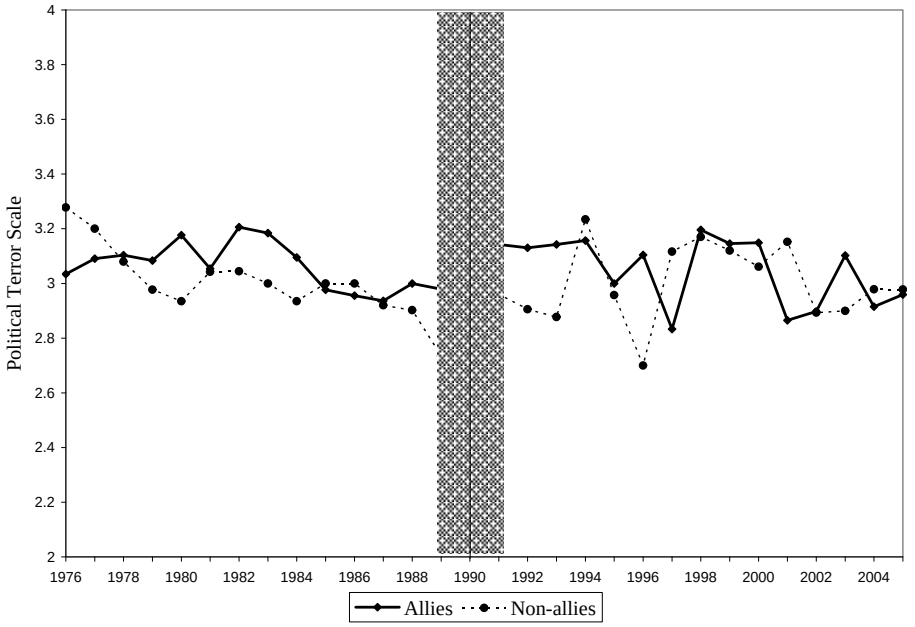


Figure 1A. Amnesty human rights reports for U.S. allies and non-allies.

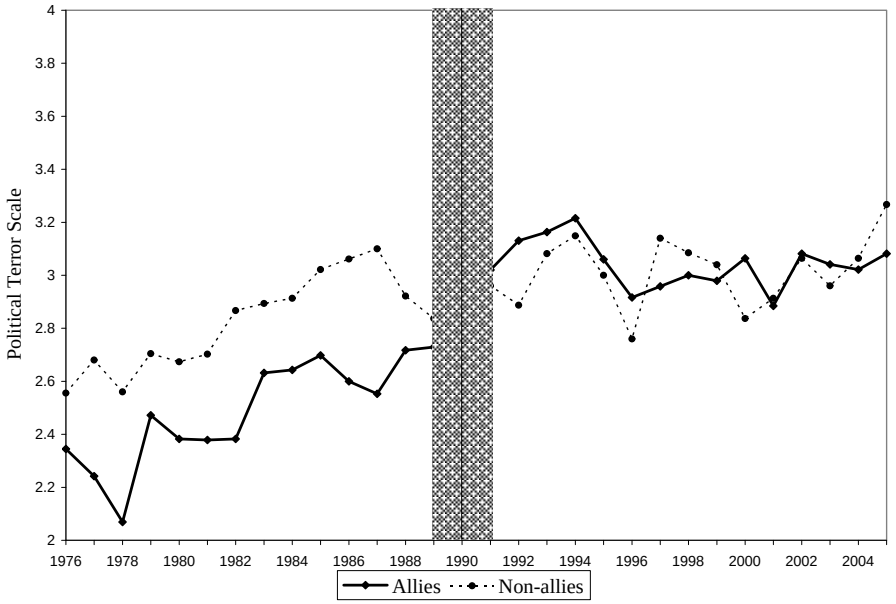


Figure 1B. U.S. human rights reports for U.S. allies and non-allies.

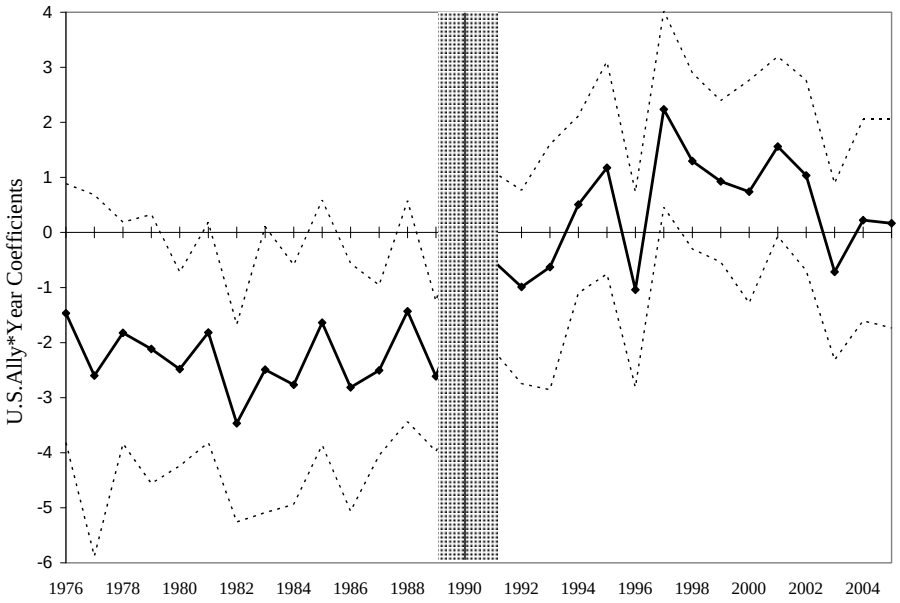


Figure 2. The Effect of U.S. Alliance on U.S. PTS – Amnesty PTS.

	Dependent Variables		
	(1) Amnesty Intl.	(2) U.S. State Dept.	(3) U.S. - Amnesty
U.S. Alliance	0.100	0.405	0.305
	(1.225)	(1.281)	(0.333)
U.S. Alliance × Cold War	0.387	-2.222*	-2.610***
	(1.158)	(1.295)	(0.517)
Year FE	Yes	Yes	Yes
Region FE x Year FE	Yes	Yes	Yes
Observations	2676	2676	2676
R-Squared	0.08	0.10	0.15

Standard errors in parentheses are clustered at the country level.
The sample consists of 112 developing countries with data from 1976-2005.
* Significant at 10%. ** Significant at 5%. *** Significant at 1%.

Table 1. The effect of strategic value to the U.S. on U.S. reporting bias.

Chapter 3

Watchdog or Lapdog? Media and the U.S. Government during the Cold War *

> "..we can and must go over the heads of our Marxist opponents directly to the American people. Our targets would be within the United States, the Congress... the general public [and] media." – Kate Semerad, an external relationship official at the Agency for International Development (AID) in 1983.

1 Introduction

Governments can influence the information reported by media outlets that they own (Enikolopov, Petrova and Zhuravskaya, 2009; Durante and Knight, 2009).[1] Their ability to do so with independently owned outlets operating in a competitive market

* This paper is co-authored with Nancy Qian, Yale University. We are extremely grateful to Abhijit Banerjee, Stefano DellaVigna, Matthew Gentzkow, Mikhail Golosov, Dean Karlan, Brian Knight, Michael Kremer, Justin Lahart, Suresh Naidu, Nathan Nunn, Torsten Persson, Jesse Shapiro, Andrei Shleifer, David Stromberg, Jakob Svensson, Chris Udry, Pierre Yared and Ekaternia Zhuravskaya for their many insights; and seminar participants at Stanford University, Yale University, New York University, Boston University, University College London, Stockholm University IIES, Warwick University, Universitat Pompeu Fabra, Paris School of Economics, Université de Toulouse, McGill University, NBER Summer Institute Political Economy, BEROC, BREAD CIPREE and NEUDC for useful comments. All mistakes are our own. Comments or suggestions are very welcome.

[1] For example, Durante and Knight (2009) find that television stations owned by the Italian president Silvio Berlusconi shifted the content of their reports towards the agenda of his party when his party came into power. Similarly, the finding by Enikolopov, Petrova and Zhuravskaya (2009) that viewers with access to more independent stations in Russia are more likely to vote against the government party implies that government owned stations promote the government's agenda.

is less obvious. In theory, market competition and independent ownership should act as safeguards against government manipulation of the media (Besley and Prat, 2006).[2] According to this, the U.S. media should be safer from government influence than most other media markets in the world (Djankov et al., 2000).[3] In practice, although studies such as Prat and Stromberg (2005) suggest that competition in the United States increases the likelihood that news organizations will report the truth, the extent to which the U.S. media is free from government influence is an open empirical question.[4] This is somewhat surprising given the many historical controversies over the media, the "watchdog" of American democracy, being manipulated by the government and the growing number of studies finding that the media has real political, economic and social consequences.[5]

This study attempts to fill this gap by measuring the extent to which the U.S. government can systematically influence the news coverage of the commercial press. In particular, we aim at estimating the effect of strategic objectives on United States State Department (USSD) reports of human rights abuses in foreign countries and the effect of these objectives on news coverage in six independently owned U.S. newspapers during the latter part of the Cold War, 1976-88. The former reveals the extent to which official government publications respond to strategic objectives and provides evidence for the hypothesis that strategic objectives make the U.S. government attempt to bias reports of human rights practices for foreign countries. The latter investigates the extent to which strategic objectives can also affect commercial news coverage within the United States. Together, these two relationships address the question of how much influence the U.S. government has on news coverage by independently owned media firms.

---

[2] Moreover, see Gentzkow and Shapiro (2008b) which provides a detailed discussion of the role of market forces in news coverage by the commercial press.

[3] The U.S. media has one of the most competitive markets in the world, and the government has no stake in the ownership of any of the major media outlets. The government provides funding to the Voice of America, which only broadcasts overseas, and National Public Radio.

[4] In addition, see the discussion in Gentzkow, Glaeser and Goldin (2006).

[5] Blanton (2001) provides an overview of all actions taken by the Office for Public Diplomacy (OPD) during the Reagan Administration (1980-88). Critics such as Noam Chomsky have gone so far as to compare the relationship between the U.S. media and the government to that of the former U.S.S.R. with its official government newspaper, *Pravda* (Herman and Chomsky, 2002: p. 139). For detailed accounts of when the media allows the government to distort reports, see Bennet, Lawrence and Livingston (2007) and Thomas (2006).

Recent studies have shown that media can affect voting behavior (Prat and Stromberg, 2005; Gentzkow, 2006; DellaVigna and Kaplan, 2007; Chang and Knight, 2008; and Enikolopov, Petrova and Zhuravskaya, 2009), other political behavior (Olken, 2008; Paluck, 2008; Gerber, Karlan and Bergan 2009) and social outcomes such as literacy (Gentzkow and Shapiro, 2008a), female empowerment (Jensen and Oster, 2008) and fertility (La Ferrara, Chang and Duryea, 2007).

Favorable reports are valued by leaders of allied countries because favorable reports from the United States can influence their prestige. More importantly, they influence U.S. strategic Congressional support for the foreign policies of the executive administration. By all appearances, the American public values good human rights practices in foreign countries. Therefore, the Congress will be less likely to object to providing support (e.g. military aid) to a country if that country is known to have acceptable human rights practices. Conversely, Congress will be less likely to object to aggressive policies towards a country if that country is known to commit human rights abuses. It thus follows that the government will then want to understate the abuses of its strategic allies relative to opponents in government publications that are presented to Congress such as the State Department's annual Country Reports and in news outlets that deliver information to both members of Congress and voters.

We face several empirical difficulties. First, strategic objectives are unobservable and it is difficult to measure the government's effort to attempt to manipulate information. The second problem is reverse causality. Are strategic objectives driving government and commercial news reports of human rights abuses? Or are they responding to the latter (Stromberg, 2004)?[6] Finally, there is the problem of omitted variable bias. This is particularly problematic in estimating the effect of strategic objectives on news reports because both may be outcomes of a third factor, such as public opinion. For example, in the months before the U.S.-led 2003 invasion of Iraq, both the U.S. government's strategic desire to invade Iraq and the news coverage of human rights abuses may have been reactions to Americans' anxiety about problems in the Middle East after 9/11. In this case, the correlation will show that U.S. strategic objectives and news coverage are highly correlated. But the correlation will confound the effects of U.S. strategic objectives and reader preferences and will most likely overstate the true effect of strategic objectives.

The principal contribution of this study is to address the aforementioned problems and provide empirical evidence **of** the causal effect of strategic objectives on U.S. commercial news coverage. First, we infer strategic objectives, which will be interchangeably referred to as government *bias* in this paper, from changes in USSD reports on human rights violations as a country's strategic value to the United States changes. Second, to establish causality, we exploit the plausibly exogenous variation in strategic objectives that results from an ally's entry onto the United Nations

[6] For example, Stromberg (2004) provides evidence that the media can affect government actions in finding that public funds during the New Deal in the United States were more likely to be targeted at regions with many radio listeners.

Security Council (UNSC). We measure alliance using General Assembly (UNGA) voting patterns. The United States values alliance and provides benefits to its allies in return for the option value or realization of favors such as supporting votes in the UN. The United States' value for allies increases when these countries enter onto the Security Council and have more opportunities to vote on issues that are crucial to the United States. Because uncertainty in factors such as domestic politics makes it difficult for allied countries to fully commit to their behavior on the Council in advance, this increase in power over critical issues will be paralleled by an increase in benefits from the United States.

The empirical strategy of exploiting U.S. alliance and UNSC membership for variation in strategic objectives to the U.S. builds on two earlier studies. A descriptive study by Qian and Yanagizawa (2009) found that the United States systematically reported lower levels of human rights abuses for its allies relative to Amnesty International, an independent human rights organization, during the Cold War, but not afterwards, thus providing very suggestive evidence that government objectives correspond to alliance. This study differs from that earlier study in that the earlier one does not address the potential endogeneity of U.S. alliance and does not examine news coverage. To address potential endogeneity of alliance, this paper exploits a second source of variation, UNSC membership, which builds on Kuziemko and Werker's (2006) study of the effect of rotating UNSC membership on U.S. foreign aid. They find that being on the UNSC during a year that is strategically important to the United States increases foreign aid receipts from the United States, thus providing evidence for the belief that Council membership increases a country's strategic value to the U.S.[7] Because alliance to the United States and UNSC membership, which is determined by election, are both subject to omitted factors, only the interaction effect can be interpreted as plausibly exogenous. We provide robustness tests for this assumption in the paper.

Specifically, we first estimate a "first stage" effect of the interaction effect of alliance and Council membership on USSD reports on human rights abuses. To check that the effect reflects changes in strategic value to the United States and not changes in actual human rights practices, we compare these effects to the effects

[7] Our finding that Council Membership of U.S. strategic allies decreases human rights criticism from the State Department is consistent with their results in that both results show that strategic value to the United States results in favors from the U.S. government. Their strategy is not appropriate for our examination of the effect of strategic value on news coverage because their explanatory measure of the importance of a year is the number of newspaper articles about the UNSC.

on Amnesty International reports. Second, we estimate a "reduced form" effect of the interaction between alliance and Council membership on the news coverage of human rights abuses. The first estimate establishes the extent to which Council membership of allied countries results in under-reporting of human rights abuses by the USSD and thus provides evidence for whether Council membership of allied countries caused the U.S. government to strategically manipulate information about human rights abuses in these countries. The second estimate provides evidence for the extent to which an increase in strategic value to the U.S. government results in reduced coverage of human rights abuses in the commercial press.[8]

The data are a country-level panel compiled from several existing sources. The main outcome measures are the quantitative scores of USSD and Amnesty reports provided by the Political Terror Project and the number of stories about a foreign country's human rights abuses published in six prominent American newspapers.[9]

The results show that an increase in strategic value to the United States significantly reduced State Department scores, had no effect on Amnesty scores, and significantly reduced news coverage of abuses in the commercial press. For Cold War allies such as Brazil, Zaire, Honduras and Chile, UNSC membership during the Cold War decreased newspaper reports of abuses in these countries by approximately 27%, 64%, 65% and 80%, respectively.

The main results are consistent with qualitative evidence showing that the government is able to systematically influence news coverage. The main competing explanation is that the results are driven by consumer preferences. We cannot conclusively rule out this alternative explanation, but we will provide several pieces of very suggestive evidence that this is not very likely in our context. For example, we find that the magnitudes of the effects across newspapers are uncorrelated with readership preferences.

Interestingly, our results show that the extent of government distortion is positively correlated with quality across newspapers. The higher is the quality of news

[8] In Section 3, we provide descriptive evidence that the Country Reports are read by journalists. However, because the Country Reports are just one of the many instruments the government can use to influence the media, the interaction between alliance and UNSC membership is not an excludable instrument for Country Reports for a structural estimate of the effect of Country Reports on media coverage. See Section 4 for a detailed discussion of the empirical strategy.

[9] We focus on the number of stories because we follow existing studies of the U.S. media in assuming that it is costly for newspapers to publish inaccurate facts. Therefore, the margin for distortion will be in the composition of stories. For example, a newspaper can choose between publishing two true stories on human rights abuses; one is about the socialist Sandinistans and the other is about El Salvador, a U.S. ally. Our estimates reveal the extent to which newspapers systematically choose to publish stories of the former over the latter.

reporting, the larger is the estimated effect of government influence. This is consistent with the theoretical predictions of Besley and Prat (2006) which formulate a framework for understanding the forces behind government manipulation of the media. In Section 6, we discuss the potential implications of this surprising stylized fact in the context of their model.

The results have a clear implication for policy makers, scholars and practitioners of journalism. Independent ownership and market competition do not ensure that the media is protected from government influence. In fact, there is much scope for government driven distortion even in one of the largest and most competitive media markets in the world.

This study makes several contributions. First, it complements recent theoretical work by Besley and Prat (2006) on government manipulation of the media. We also add to empirical studies on the determinants of news coverage. So far, these have primarily focused on the effect of direct government ownership (Enikolopov, Petrova and Zhuravskaya, 2009; Durante and Knight, 2009) or consumer driven distortions (e.g. Mullainathan and Shleifer, 2005; and Gentzkow and Shapiro, 2006). To the best of our knowledge, our study is the first to provide evidence that independently owned news outlets can be systematically influenced by the government. Second, we add to the small but growing number of economic studies exploring the causes and consequences of U.S. government foreign policy. So far, these have been limited to outcomes affecting foreign countries such as U.S. strategic foreign aid (e.g. Alesina and Dollar, 2000; Kuziemko and Werker, 2006), or outcomes for U.S. firms such as stock prices (Dube, Kaplan and Naidu, 2009) and terms of trade (Easterly et al., 2009).[10] Our study broadens the scope of this literature by examining the effect of U.S. foreign policy on the American public. Finally, we provide a measure of government bias and a source of plausibly exogenous variation that can easily be used by future researchers in economics and political science.

The paper is organized as follows. Section 2 discusses the background of the Cold War and the UN and documents historical cases of government manipulation of the media and the government's use of human rights practices in portraying its strategic allies and opponents. Section 3 describes the data and Section 4 presents the empirical strategy. Section 5 presents the results and they are interpreted in

[10] A related empirical literature examines the effects of U.S. military operations on democracy in foreign countries. See Bueno de Mesquita and Downs (2006) for a review of the literature as well as Easterly, Satyanath and Berger (2008). There is also a literature about the effects of political interests on trade which typically focus on the effects of lobbying interest groups.

Section 6. Section **??** provides concluding remarks.

2 Background

This section has four aims. First, it describes the political competition between the United States and U.S.S.R. during the Cold War and how this influenced the United States' value of political alliance during this era. Second, it describes the value of votes in the United Nations and how the additional power gained by a country when it enters the Council together with a country's inability to fully commit to behave in accordance with U.S. interests will make the United States increase bribes to allies when they are Council members. Third, we document known cases of government interferences with news coverage during this period to provide some insight into some of the methods the government used and the government's motivation for influencing the media. Finally, we document the government's focus on favorably portraying the human rights practices of their allies and their opponents unfavorably. For the sake of brevity, additional documentation is provided in the Appendix.

2.1 The Cold War

The "Cold War", which began after World War II in 1945 and lasted until 1989/91, refers to the continuous political conflict, military tension and economic competition between the USSR and its satellite states (consolidated by the Warsaw Pact 1955-91) and the United States and its Western Hemisphere allies (e.g. NATO, established in 1949). Direct military attacks on adversaries were deterred by the potential for mutually ensured destruction by deliverable nuclear weapons. Therefore, rivalry between the two superpowers was expressed through military coalitions, propaganda, espionage, weapons development, industrial advances, competitive technological development and numerous proxy wars. The Cold War spread to virtually every region of the world, as the United States, under the *Marshall Plan*, sought the containment and rollback of communism and forged myriad alliances to this end; the U.S.S.R., under the *Molotov Plan*, fostered Communist movements around the world (Gladdis, 2006). The periods of the highest tension during the Cold War included the Berlin Blockade (1948-49), the Korean War (1950-53), the Berlin Crisis (1961), the Vietnam War (1969-75), the Cuban Missile Crisis (1962) and the Soviet war in Afghanistan (1979-89). Our study takes place in the context of the last conflict.

The Cold War ended during 1989-91, when the Berlin Wall fell and the U.S.S.R.

dissolved. For the purpose of our paper, we loosely interpret 1989 as the end of the Cold War. At this time, the strenuous competition between the United States and the U.S.S.R. for the alliance of smaller countries ended. Past studies have argued that the United States favored its allies in terms of favorable human rights reports (Stohl and Carleton, 1985; Mitchell and McCormick, 1988; Poe, Carey and Vasquez, 2001). Qian and Yanagizawa (2009) find that the amount of under-reporting of human rights violations increases monotonically with the degree of alliance (e.g. the degree to which a country votes with the United States and against the U.S.S.R. in the UNGA) during the Cold War and that this favoritism dissipates with the end of the Cold War.[11]

2.2 The United Nations

The United Nations (UN) is the source of much of the diplomatic influence and the principal outlet for the foreign relations initiatives of many developing countries. It was especially important during the Cold War. Figure 1 shows that the number of issues in which the two super-powers voted in opposition to each other escalated during this period of high political tensions. Moreover, it shows that there was a dramatic increase in the fraction of countries that voted with the United States after the end of the Cold War. Together, they illustrate the marked division between the United States and the U.S.S.R. in the UN during the Cold War as well as the extent to which these tensions influenced the voting patterns of member countries.

Two of the five principal organs of the UN are the General Assembly and the Security Council. The UN General Assembly (UNGA) is the only one where all member nations have equal representation. Its powers are to oversee the budget of the UN, appoint the non-permanent members to the Security Council, receive reports from other parts of the UN and make recommendations in the form of General Assembly Resolutions. It currently consists of 192 countries, of which more

[11] A well-known example that illustrates the decline in the value for the United States of Cold War allies after the end of the Cold War is Zaire (renamed the Democratic Republic of Congo in 1997). Its president, Mobutu Sese Seko (in office 1965-1997), a strong supporter of the United States during the Cold War, had been repeatedly criticized for human rights abuses. However, during a state visit to the United States in 1983, United States president Ronald Reagan praised Mobutu and said in response to the international criticism of Mobutu's human rights abuses that he was a "voice of good sense and good will". Immediately after the end of the Cold War, the State Department began to criticize Zaire's human rights violations. In 1993 Mobutu was denied a visa for visiting the United States. At that time, he remarked "I am the latest victim of the Cold War, no longer needed by the United States. The lesson is that my support for American policy [now] counts for nothing" (Gbadolite, 2001).

than two-thirds are developing countries. The General Assembly votes on many resolutions brought forth by sponsoring states. While symbolic in the sense of the international community, most resolutions are not enforceable as a legal or practical matter. The General Assembly does have authority to make final decisions in areas such as the UN budget. More importantly, in case of a split vote in the UNSC when no veto is exercised, the issue goes for vote in the General Assembly. The belief that voting with the United States in the UNGA is valuable to the United States is consistent with the empirical finding that such votes are correlated with the amount of foreign aid received from the United States (Alesina and Dollar, 2000) and the favorable under-reporting of human rights violations by the U.S. State Department (Qian and Yanagizawa, 2009).

The UNSC consists of fifteen member states. Council members have more power than General Assembly members because the Council can make decisions which are binding for all UN member states including economic sanctions or the use of armed force "to maintain or restore international peace and security" (Chapter Seven of the UN Charter).[12] There are ten temporary seats that are held in two-year terms, each beginning on January 1st. Five are replaced each year. The members are elected by regional groups and confirmed by the UN General Assembly.[13] There are five permanent members (P5): China, France, Russia, the United Kingdom and the United States. These members hold veto power for blocking the adoption of a resolution.

Rotating members have substantial power on the Council. First, they have as much influence as the P5 in setting the agenda. Second, although the P5 have the power to veto, they rarely exercised this power during this period (Winter, 1996; O'Niell, 1996). This can be seen from the fact that deadlocks, which can only occur if no member of the P5 vetoes a resolution, have occurred ten times in the history of the UN. Nine of those occurred during the Cold War.[14] The fact that

[12] This was the basis for UN armed action in Korea in 1950 during the Korean War.

[13] Africa elects three members; Latin America and the Caribbean, Asian, and Western European and other blocs choose two members each; and the Eastern European bloc chooses one member. Moreover, one of these members is an Arab country, alternately from the Asian or African bloc. Members cannot serve consecutive terms, but are not limited in the number of terms they can serve in total. There is often intense competition for these seats (Malone, 2000).

[14] 1956 Suez Crisis; 1956 Soviet Invasion of Hungary (Hungarian Revolution); 1958 Lebanon Crisis; 1960 Congo Crisis; 1967 Six Days War; 1980 Soviet invasion of Afghanistan; 1980 Israeli-Palestinian Conflict; 1981 South African occupation of Namibia (South West Africa); 1982 Israeli Occupation of the Golan Heights (Golan Heights Law); 1997 Israeli-Palestinian conflict (East Jerusalem and Israeli-occupied territories).

The power of the votes from rotating members is consistent with the theoretical predictions

temporary members have influence in the Council is consistent with the finding that membership can result in higher U.S. strategic foreign aid (Kuziemko and Werker, 2006; Dreher et al., 2009).

The United States' motivation for buying the votes of countries in the General Assembly and the Security Council follows from the same logic as standard models of vote-buying.[15] These models typically predict that the amount of bribes should be highest for the marginal voter, meaning the voter who is most responsive to bribes. In principle, the correlation between a country's responsiveness to bribes and its affinity for the United States can be potentially positive or negative. Our study will therefore reveal the direction of this correlation and show that, in practice, countries with greater underlying affinity for the United States are also more responsive to bribes. The finding that the marginal voter in the UNSC may be the strongest ally is not surprising when we note that the strongest ally in our sample only votes with the United States 40% of the time over divided issues between the United States and the U.S.S.R. in the UNGA during this period. In the Appendix, we present a simple model which is an application of the probabilistic voting model of Lindbeck and Weibull (1987) to illustrate the conditions under which countries with a higher affinity for the United States receive higher bribes and are more likely to vote in favor of the United States

In this study, we assume that the United States values alliance in both the General Assembly and the Security Council and that the value of an ally increases when a country enters the latter. If allied countries could fully commit to voting favorably with the United States when they are on the Council, then we should observe the United States giving allies a positive amount of bribes that is relatively smooth over time. However, full commitment is highly unlikely in practice since leaders of and political attitudes within allied countries can change in unpredictable ways. Therefore, in order to guarantee good behavior from the ally on the Council in case a critical issue arises, the United States must increase the amount of bribes during the ally's two years on the Council.

In the data, we will examine voting patterns in the UNGA. But we will not examine voting patterns in the UNSC because most issues are discussed prior to being put on the agenda. Therefore, the sample of issues voted on is not representative

by Voeten (2001) who models bargaining power within the UNSC and finds that even though members of the P5 such as the United States have unilateral power in vetoing resolutions, they prefer multilateral agreements.

[15] Kuziemko and Werker (2005) provide an overview of the parallels between this literature on practices of U.S. Strategic Congressional Committees and those of the UN Security Council.

for the actual issues being deliberated by Council members.

2.3 Public Diplomacy

The main period of our study, 1976-1988, was characterized by an escalating commitment to fight communism on the part of the American government which reached a climax during the Reagan administration (1980-88). The government had several motives for influencing the press coverage of its political allies. First, it was a way of influencing public opinion. In the case of the *The New York Times,* which published an international version under the title of *The International Herald Tribune*, manipulation could also affect the opinion of foreign readers. Second, and probably more importantly, influencing the press was an important way of affecting congressional opinion, whose favor was necessary for legislative purposes (Blanton, 2002).

During the 1980s, the *Office of Public Diplomacy* (OPD) was officially part of the State Department and worked closely with the National Security Council (NSC). The explicit purpose of the office was to influence public and congressional opinion to garner support for the President's strong anti-communist agenda in a "public action" program (Parry and Kornblub, 1988). The memo specifies that audiences for the information campaign include the Congress and the U.S. media. For the latter, the plan entailed making a list of media outlets and identifying specific editors, commentators, talk shows and columnists (Jacobwitz, 1985).

There were many ways for the executive administration of influencing Congress members. Information can be disseminated through the numerous government affiliated publicity events and publications. One such publication is the *Country Reports on Human Rights Practices.* Every year, it is published by the State Department and submitted to Congress.[16] The explicit purpose of the reports is to serve as "a resource for shaping policy, conducting diplomacy and making assistance, training and other resource allocations".[17] While Congress is the primary audience targeted by these reports, they are open to the public and therefore also available to journalists. They fact that they are read by journalists is consistent with the fact that in our data, the number of stories on human rights abuses in the NYT increases discretely in the three days following a release of the USSD Country Reports, as will

[16] http://www.state.gov/g/drl/rls/hrrpt/

[17] See the "Overview and Acknowledgements" from the *Country Reports on Human Rights Practices* for 2003, released by the Bureau of Democracy, Human Rights. http://www.state.gov/g/drl/rls/hrrpt/2003/29640.htm

be shown in Section 3 Data.[18]

Government methods for influencing the media can be broadly categorized into two groups. First, it can manipulate the supply of information. As with Congress, the government had many instruments such as the *Country Reports on Human Rights Practices* for disseminating its points of view. In addition, disinformation was often released directly by the OPD.[19] Second, the government can attempt to directly manipulate news reports by exerting pressure on editorial boards or incentivizing journalists. The OPD monitored news reports by the American media and would directly confront journalists and editors in order to convince them to change the reports (Schultz, 1984). Upon the appearance of news reports that did not conform to the wishes of the OPD, officials would press the owners and editorial boards to change their journalists in the field. Similarly and perhaps most importantly, the OPD dealt directly with journalists using a carrot-and-stick strategy. Uncooperative journalists became the targets of character assassination meant to induce skepticism over the information they reported and were sometimes even forcibly removed from foreign countries they were reporting from.[20] In contrast, journalists seen as cooperative with the administration's agenda were rewarded with increased access to government information. For example, an OPD memo stated that certain favorable correspondents had "open invitations for personal briefings" (Cohen, 2001). The exclusive nature of this access presumably made it valuable to journalists. In general, the executive administration had control over information that was very valuable to journalists. For example, they controlled access to interviews with important personnel and even controlled who was allowed to ask questions during administration press conferences.

Note that the main results from the empirical analysis estimate the reduced form effect of strategic objectives on news coverage. The estimates will capture the effects of the distortion of the supply of primary information as well as the effects of more direct manipulation of the incentives of journalists and editorial boards. Later in

[18] A similar increase of smaller magnitude is found for the three days following the release of Amnesty reports.

There is also evidence that governments of foreign countries read State Department reports of abuses in their countries (see Associated Press, 1977). In addition to official publications, the government can influence word-of-mouth information by having select information be read aloud into Congressional record by sympathetic members of Congress, arranging meetings between sympathetic experts and Congress members or, in the extreme, planting false witnesses for personal testimony in congressional committee hearings (Parry and Kornclub, 1988).

[19] See the Appendix for examples of disinformation.

[20] See the Appendix for examples.

this paper, in Section 5.5, we will attempt to indirectly investigate the contribution of the former by examining if the main effects are larger when it is more costly for the newspaper to obtain independent information. Moreover, note that in light of the qualitative evidence of the large number of known cases of government distortion, we will assume that journalists are aware of the government's motive to manipulate news coverage in interpreting the results.

Understating human rights abuses of allies and emphasizing those of opponents played a prominent role in the U.S. government's foreign policy during the Cold War. One of the ways of shaping public and congressional opinion against opponents was to exaggerate human rights abuses in those countries and emphasize that they were "evil", "forced conscription" or engaged in "persecution of the church". Conversely, the government attempted to increase support for political allies by calling them "freedom fighters", "religious" or simply "good" (Jacobwitz, 1985).

Human rights were important for reasons related both to foreign and domestic politics. First, a perception of having good human rights practices is valuable because it is often tied to aid. The latter is consistent with the fact that in our data, improving human rights practices by one PTS point is, on average, correlated with a 10% increase in U.S. strategic foreign aid. Second, it is also valuable to the governments of foreign countries for non-pecuniary reasons. For example, official chastisement by a foreign government could decrease the domestic prestige of a government. Similarly, hosting certain international events such as the Olympics is often viewed as a way of raising the prestige of governments of developing countries. Human rights abuses are frequently used as a cause for disqualifying countries from hosting. Alternatively, abuses described in the Country Reports may be a source of information for people inside the country reported on and this information could be used against the government in power. These arguments are consistent with the long history of incidents where the U.S. government withdrew aid or imposed trade sanctions on countries because of human rights violations, and incidents where foreign countries rejected U.S. strategic aid that was tied to human rights practices.[21]

In summary, the discussion from this section suggests that the U.S. government values alliance for strategic reasons, and this value increases when an allied country becomes a UNSC member. Membership will therefore make the United States increase the amount of bribes to an allied country. The State Department will underreport abuse by allied governments since foreign countries dislike negative reports

[21] See the Appendix for examples.

on their human rights practices and Congress and voters prefer the United States to ally with and provide aid to governments with good human rights practices. For similar reasons, the U.S. government may attempt to suppress the amount of coverage of human rights abuses of its allies in the commercial media. These claims are consistent with the descriptive evidence from Table 1 which shows the correlates of U.S. strategic foreign aid. Column (1) shows that during the Cold War, foreign aid was strongly correlated with alliance (measured as the fraction of votes when a country voted with the United States on UNGA issues that were divided between the United States and the U.S.S.R. during the late 1980s), UNSC membership and the interaction of the two terms. In other words, for any given allied country, the country would receive even more foreign aid when being on the UNSC. Interestingly, column (2) shows that all these correlations disappear after the end of the Cold War.

Our empirical strategy will estimate the effect of strategic objectives on State Department reports of human rights violations (relative to Amnesty International reports) and the amount of coverage in six prominent and independently owned U.S. newspapers. Note that for the latter, our strategy will capture both the direct effects of the government manipulating the incentives of editorial boards and journalists and the indirect effects of the government manipulating the supply of primary information such as the Country Reports. See Section 4 for a more detailed discussion.

3 Data

This study combines data from several existing sources to form a country-level panel for 1976-2005. The time span of the data is restricted by the availability of the PTS scores. We exclude the former Soviet Republics. Many of these did not have membership in the UN before 1991. Their exclusion should not affect the results since they were unlikely candidates for U.S. alliance during the Cold War. South Africa is omitted from the sample because it was excluded from UN activities due to the UN opposition to Apartheid. The five permanent members of the UNSC are also excluded.[22] We further restrict the sample to country-year observations where the index is available for both Amnesty International and the U.S. State Department. Finally, we focus our study on developing countries for which the UN is arguably the

[22] In 1978, China's seat on the UNSC was transferred from Taiwan to the People's Republic of China. Neither will be in our sample.

principal outlet of foreign policy initiatives by restricting the sample to countries that are not classified as high income countries according to the definition by the World Bank.[23] Our matched sample contains 104 countries for thirty years.

For measuring alliance, we follow Qian and Yanagizawa (2009) by using the fraction of votes in agreement with the United States on UNGA resolutions on which the United States and U.S.S.R. (or Russia after 1991) are divided (e.g. vote in opposing directions).[24] Figure 1 plots the fraction of divided votes over time. It shows that as Cold War tensions escalated in the 1980s, the fraction of divided votes increased from approximately 30% during the late 1970s to almost 70% in the late 1980s. Also plotted is the fraction of votes with the United States averaged over all divided votes each year.[25] Our main measure of alliance is the fraction of votes that a country voted with the United States averaged over the period 1985-88. This period provides us with the highest number of divided votes and the best measure of alliance during this period. We use a time-invariant measure of alliance because it is less likely to be an outcome of changing U.S. favoritism than a time-varying measure and, more importantly, because using voting patterns from years with very few divided issues produces a very noisy measure of alliance.[26] Using this measure, the top three allies of the United States and the fraction of divided issues on which they voted with the United States during 1980-84 are: Turkey (0.4), Belize (0.28) and Costa Rica (0.27). The three countries that are least allied are Mongolia (0), Lao PDR (0) and Czech Republic (0).

Figure 2A maps the alliance measure for the countries in our sample. We arbitrarily define an ally to be countries that on average voted with the United States more than the median country in the sample, which voted with the United States on divided issues approximately 7% of the time. It is important for later interpreting the results to note that allies in our study refer to relative voting patterns and that in terms of the absolute number of votes, the strongest allies still vote with the United States less than half of the time. Amongst countries that were ever on

[23] High income countries are denied to be those with a 2007 GNP per capita of $11,456 or more. Our results are very similar when we do not make this restriction. These results are omitted for brevity and are available upon request.

[24] Each year there are approximately 100-150 resolutions in the UNGA, of which the United States and U.S.S.R. disagree on approximately 70-90.

[25] Our measure of alliance includes abstentions. Excluding them does neither significantly change the measure of alliance nor the regression results. For brevity, we do not report those results in the paper.

[26] Our estimates are robust to changing the measure of alliance to be the average of votes during periods between 1981 and 1989, when there were many divided votes. For brevity, we do not report estimates with these alternative measures in the paper.

the UNSC during the Cold War, the alliance measure is normally distributed (see Appendix Figure A1). We do not make a separate measure of alliance based on voting patterns during the post-Cold War period since there were many fewer divided issues and the change in the nature of international relations when the world went from having two superpowers to one hegemon means that the same measure could have a very different meaning. Note that we use the same measure of alliance for the Cold War and post-Cold War periods. This makes the interpretation of the effects for the latter period difficult as there was a large shift in alliance from the USSR to the United States after the dissolution of the Soviet Union. Hence, the results for the post Cold War period should be interpreted as suggestive evidence with much caution and we do not conduct a triple-difference estimation by using the Cold War sample as a placebo.

Data on UNSC membership is collected from The United Nations Security Council Membership Rollster.[27] 46 countries in the sample were on the UNSC as a rotating member at least once during this time period. They are listed in Appendix Table A1. 21 countries were on the Council at least twice, amongst which five countries were on the Council three times.

Human rights in the context of this study specifically refer to physical violence committed by the state on civilians.[28] Two of the main sources of information for human rights are the U.S. State Department and Amnesty International, both of which publish annual reports for almost every country in the world. Both the USSD and Amnesty use the same definition for human rights abuses as set forth by the Universal Declaration of Human Rights and publish reports using similar formats.[29] The United States is the only country systematically releasing its reports to the public. The way in which it gathers information is not transparent. However, it is generally assumed that the reports are based on information from government intelligence and diplomatic appratuses.[30]

Amnesty International is the only non-governmental organization which makes systematic reports over the same broad scope and long-time horizon.[31] Founded in

[27] See http://www.un.org/sc/list_eng5.asp for a list of all countries that were ever members and the years of their memberships.

[28] This is the definition used by Freedom House, the PTS project and the CIRI project.

[29] The declaration was adopted by the UNGA on December 10, 1948. It arose directly from the experience of World War II and, for the first time, sets out fundamental human rights to be universally protected. It consists of thirty articles. The full text of the declaration can be found at http://www.un.org/en/documents/udhr/.

[30] The wording of the reports also suggests that the information is mainly based on these sources.

[31] Amnesty is non-government human rights group with the largest scope. Another NGO Human

the United Kingdom in 1961, Amnesty's finance and management are independent of any government. It has offices in eighty countries and employ full time research teams that investigate reports of human rights abuses, cross check and corroborate information from sources that include letters from individuals or their representatives, refugees, diplomats, religious bodies, community workers, humanitarian agencies, diplomats and other human rights defenders. It also often sends fact-finding missions to assess situations in the field. While Amnesty is often perceived as having left-leaning sympathies, the organization has actually received criticism for both alleged anti-Western and pro-Western bias.[32] Since Amnesty and the USSD presumably share some sources of information and share information with each other, it could be expected that their reports are highly correlated in most cases.

Reports from these two agencies are individually scored beginning in 1976 by a group of human rights scholars at University of Carolina. The Political Terror Scale (PTS) measures levels of political violence and terror that a country experiences in a particular year based on a five-level terror scale originally developed by Freedom House. This index is available for 183 countries over the period 1976-2005. Relative to other measures of human rights violations, the PTS extends furthest back in time to 1976.[33] This determines the time period of our study. Amnesty and the USSD report identical PTS for 84% of our sample on average and for 73% during the Cold War. For illustrative purposes, we measure USSD reporting bias as Amnesty PTS subtracted from USSD PTS and divide the average of this difference during the Cold War into five equal frequency groups and map it in Figure 2B. It shows that relative to Amnesty, the USSD under-reported human rights abuses most for Cold War allies such as Turkey and Saudi Arabia.

News coverage of human rights violations is measured as the number of articles about human rights abuse in a given country. We calculate this number based on a search of the text of articles in the ProQuest Historical and National Newspapers. We search for articles containing the country's name, the phrase human rights" and

Rights Watch (HRW), a U.S. based organization, also produces reports. However, the HRW does not systematically publish yearly country reports. And their existing publications have not been quantitatively scored.

[32] See Poe, Carey and Vasquez (2001) and Qian and Yanagizawa (2009) for quantitative comparisons of the Amnesty and United States State Department measures and more detailed discussions.

[33] The CIRI Human Rights Data Project, like the PTS Project, reads the reports by Amnesty and the USSD and provides a score. However, the CIRI indices only begin in 1981. They also differ from PTS in that they attempt to provide disaggregated indices for the type of human rights. This means that while the two indices are correlated (approximately 0.65-0.73), they are not directly comparable. See Wood and Gibney (2009) for a detailed discussion.

at least one of the words or phrases that fall under the UN Declaration for Human Rights (and that are therefore also commonly used in news articles on human rights abuse). These include "torture", "violations", "abuse", "extrajudicial", "execution", "arbitrary arrests", "imprisonment", and "disappearances". Our measure of human rights coverage is the total number of articles that results from the search per country per year. We follow previous studies on the U.S. media in assuming that media outlets will not report facts that are known to be false because proven inaccuracy could cause a costly loss of reputation. Therefore, the margin for distortion is along the composition of stories (e.g. report that the Sandinistas are committing human rights atrocities and omit reports of similar abuses by the government of El Salvador, an ally of the United States).[34]

This study examines news reported by The New York Times (NYT), The Washington Post, The Wall Street Journal (WSJ, only available 1976-91), The Chicago Tribune (only available 1976-86) The Christian Science Monitor (CSM, only available 1976-97) and The Los Angeles Times (L.A. Times). These are the only national newspapers for which we could conduct a full text search for the main period of our study.[35]

For the Cold War period, we have data for all six papers for 1976-86 and five papers for 1987-88 because data for the Tribune is only available until 1987. For the post-Cold War period, we do not have data for the WSJ or the Chicago Tribune and the CSM is only available until 1997. The papers in our sample were arguably some of the largest metropolitan newspapers in the United States during the 1980s. The NYT and Washington Post had particularly good reputations for the width and depth of their news coverage. These two newspapers have more foreign correspondents than other U.S. newspapers. They typically write their own stories; however, our measure includes both articles written by journalists from these papers and stories picked up from newswires and other sources.[36] After presenting the main

[34] This is a similar mechanism to the crowding-out of news found in Eisensee and Stromberg (2004). They show that U.S. emergency disaster relief depends on whether the disaster occurs at the same time as other newsworthy events that are obviously unrelated to need. They argue that the explanation for this result is that relief spending is driven by news coverage and the other newsworthy material crowds out this news coverage.

[35] We follow ProQuest in the definition of a national newspaper. In practice, while these are six of the larger newspapers in the United States for the main period of study, the NYT, Washington Post, Tribune and the L.A. Times were mostly distributed regionally.

[36] The source of the story is often embedded within an article. Therefore, we were not able to accurately and systematically distinguish between articles written by different sources. This should not affect the interpretation of our estimates as the reduced form strategy captures the effects of government objectives on both journalists' decisions to report a story and the editorial decision

results, we will also examine the impact on stories from newswires.

We use two measures to proxy for a newspaper's cost of obtaining independent information. First, we use an indicator for the freedom of domestic press from Freedom House data. It reflects a newspaper's ability to pick up stories from independent sources inside a foreign country. This measure ranges from zero to two. Zero indicates no freedom. And two indicates a free press.[37] This measure is produced annually beginning in 1980. We will use a time invariant measure, calculated as the average measure during 1980-1988, to capture overall media access. This avoids the potential problem that changes in media freedom within a country over time may be correlated with UNSC membership. For interpretational ease, we create a dummy variable that takes the value of one if the average media freedom index is above zero. This dummy variable indicates whether a country experienced any media freedom during 1980-88. Second, we proxy for the cost for a foreign correspondent of traveling to the location of the story with the geographic distance from national capitals to the nearest foreign bureau offices. Travel costs have been cited as one of the major costs for foreign bureau offices (Caroll, 2007). We were only able to obtain the Cold War locations of offices for the NYT. Figure 2C shows a map of our media freedom variable as well as the NYT foreign bureau offices.[38]

Table 2 presents the descriptive statistics. On average, the USSD reports countries as being 0.14 index points better in terms of human rights violations relative to Amnesty. The mean level of alliance with the United States is approximately 9% on average. On average, approximately eleven stories on human rights abuses are published in all six newspapers per country per year. Most of these stories are featured in the Washington Post, NYT and L.A. Times. Newswires provide roughly the same number of stories on human rights abuses as the six U.S. papers combined in our sample. The average distance between the national capital of a country and the nearest NYT foreign office bureau is 1,463 km. Forty percent of the sample have no media freedom domestically according to Freedom House.

---

to publish stories from all sources. See Appendix Figure A2 for a plot of the annual number of articles on human rights abuses for all countries over time.

[37] For example, Afghanistan is rated as zero and Australia is rated as two.

[38] The NYT has foreign bureaus in Mexico City, Caracas, Rio de Janeiro, London, Paris, Berlin (West Berlin), Bogota, Shanghai, Frankfurt, Rome, Jerusalem, Beirut, Cairo, Istanbul, New Delhi, Dakar, Nairobi, Johannesburg, Moscow, Beijing, and Hong Kong. The distance, measured in kilometers, comes from data on distance between cities of the world provided by Kristian Skrede Gledisch of the University of Essex.

3.1 Descriptive Evidence

The data provides several pieces of interesting descriptive evidence.

First, a comparison of USSD and Amnesty PTS scores provides very suggestive evidence that USSD PTS scores respond to government objectives. We take Figures 3A-3C from Qian and Yanagizawa (2009). We plot the PTS for U.S. strategic allies and non-allies over time (using the same definition of whether a country on average votes with the United States more or less than the sample median as in Figure 2A). Figure 3A plots the U.S. PTS scores. The vertical band indicates the end of the Cold War 1989-91. It shows that during the Cold War, the United States systematically reported its allies as having better human rights than its non-allies. This gap immediately converges after the end of the Cold War. Interestingly, also note that the U.S. reports all countries as having increasingly worse human rights as the Cold War tensions escalate through the late 1970s and 1980s. Figure 3B plots the analogous relationship for Amnesty PTS scores. The vertical axis has the same scale as Figure 3A for the purpose of comparison. In contrast to the United States, Amnesty reports allies and non-allies as having similar human rights practices for both the Cold War and post-Cold War periods. Figure 3C plots the difference between U.S. and Amnesty reports for allies and non-allies over time. It follows from the two previous figures that during the Cold War, the U.S. reported its allies as having better human rights practices than non-allies relative to Amnesty. There is no difference after the Cold War. Since alliance is correlated with many factors, this descriptive evidence cannot show that the United States' strategic value for allies has a causal effect on its under-reporting of human rights. However, the fact that all changes between the difference in U.S. and Amnesty scores are driven by changes in U.S. reports, and that favorable reports for allies immediately after the end of the Cold War is very suggestive towards interpreting these changes as driven by changes in U.S. strategic factors. Note also that Amnesty PTS scores fluctuate over time, which is consistent with the belief that they contain information.

Second, the data provides suggestive evidence that the Country reports are read by reporters and could therefore be one of the ways for the government of influencing the news. The USSD and Amnesty reports are released roughly during February and April each year. We observe no obvious pattern in the release dates. Figure 4 plots the average number of human rights articles published in our six U.S. newspapers in the seven days leading up to and following the release of the reports. They show a spike in the number of stories in the day immediately after the report release dates,

with an increase from one to five articles following a release of the USSD reports and an increase from one to two articles following the release of the Amnesty reports. This provides descriptive evidence of the belief that journalists read these reports. However, the direct effect of the reports on news coverage may be small. Figure 4 shows that during the Cold War, only seven stories out of the approximately 700 articles on human rights abuses written in one year are published on the day immediately after the release of the reports. This is consistent with the belief that the Country Reports are one of the many ways for the government of influencing the media. Therefore, for the purposes of our paper, Country Reports should be loosely interpreted as a proxy for government attitude.

4 Empirical Strategy

In this study, we estimate two causal relationships, the effect of an increase in a country's strategic value to the United States on USSD PTS scores; and the effect of an increase in a country's strategic value to the United States on the number of stories of abuses in U.S. newspapers. As described in the introduction, we face two main difficulties. The first is reverse causality. Are strategic objectives driving government and commercial news reports of human rights abuses? Or are they responding to the latter? The second is the problem of omitted variable bias. This is particularly problematic in estimating the effect of strategic objectives on news reports because both may be outcomes of a third factor, such as public opinion.

To address these, we exploit plausibly exogenous variation in a country's strategic value to the United States from the combination of alliance with the United States and entry into (and exit from) the UNSC. We will estimate a "first stage" effect of the interaction effect of alliance and Council membership on USSD reports and a "reduced form" effect of the interaction effect of alliance and Council membership on news coverage of human rights abuses. The first estimate establishes the extent to which Council membership of allied countries results in increased favorable under-reporting of human rights abuses by the USSD and thus provides evidence for whether Council membership of allied countries increases their value to the U.S. government. The second estimate provides evidence for the extent to which an increase in strategic value to the U.S. government results in reduced coverage of human rights abuses in the commercial press. Since the Country Reports are just one of the many instruments the government can use to influence the media, the interaction between alliance and UNSC membership is not an excludable instrument

for a structural estimate of the effect of Country Reports on news coverage.

As discussed in Section 2.2, standard theories of vote-buying make ambiguous predictions on the correlation between alliance and the amount of bribes from the United States. To address this in practice, we first estimate a flexible equation where we allow alliance to vary. We divide the observations into three equal frequency groups according to alliance and create dummy variables for whether a country belongs to the group of "non-allies", "medium allies" or "strong allies". We then estimate the following equation.

$$\begin{aligned} USSD_{it} = \theta_1(MedianAlliance_i \times UNSC_{it}) \\ + \theta_2(StrongAlliance_i \times UNSC_{it}) \\ + UNSC_{it} + \gamma_i + \delta_t + \varepsilon_{it}. \end{aligned} \tag{3.1}$$

USSD PTS scores in country i in year t are a function of: the interaction terms between the two levels of alliance to the United States, $MedianAlliance_i$ and $StrongAlliance_i$, and membership of the UNSC, $UNSC_{it}$; country-fixed effects and year-fixed effects. All differences across countries that do not change over time are controlled for by country-fixed effects. All changes over time that affect all countries similarly, such as American attitudes towards human rights, are controlled for by year-fixed effects. The standard errors are clustered at the country level. Higher PTS reflects worse human rights conditions. Therefore, if the United States favors its allies when they are on the Council with milder reports of human rights abuses, $\hat{\theta}_j < 0$, $j = (1, 2)$. If the effects are larger for countries that are stronger allies with the United States, then $\hat{\theta}_2 < \hat{\theta}_1 < 0$. The estimates will show that the effect of UNSC membership is increasing with alliance, implying that countries with higher affinity for the United States receive higher bribes. See the Appendix for a simple model which shows the conditions under which this can be true. Note that this is not surprising since the strongest ally only votes with the United States over divided issues 40% of the time. In light of this relationship, we will, for the sake of simplicity, use a continuous measure of alliance for our main empirical estimates.

Using a continuous measure of alliance, the effect of an increase in government strategic value of a country on U.S. reports can be characterized as follows:

$$USSD_{it} = \theta(U.S.Alliance_i \times UNSC_{it}) + \alpha UNSC_{it} + \gamma_i + \delta_t + \varepsilon_{it}. \tag{3.2}$$

The difference in USSD and Amnesty PTS scores in country i in year t is a function of: the interaction term between alliance with the United States, $U.S.Alliance_i$, and membership on the UNSC, $UNSC_{it}$; the main effect of Council membership; country-fixed effects and year-fixed effects.

To interpret θ as the causal effect of an increase in strategic value to the United States, we need to assume that an ally's entry and exit from the UNSC did not affect the difference in PTS scores through any other channel than U.S. strategic value. The main concern is the possibility that allies actually behave better when they enter the Council. To address this concern, we will also examine the effect of an ally's entry onto the UNSC on Amnesty's PTS scores. If we find there to be no effect on Amnesty, we will be more willing to believe that the effect on USSD scores is driven by changes in U.S. strategic value rather than changes in actual human rights practices as long as we assume that the USSD does not have better information about improvements in allied countries human rights practices when they enter the Council. We also repeat the same estimation for the period after the Cold War under the assumption that there has been a decrease in the United States for allies. If we find that the effect decreases when the Cold War ends, then we will have more confidence that our strategy is indeed capturing changes in strategic value to the United States. Recall that using the post-Cold War period as a comparison leads to the difficulties that we described earlier in Sections 2 and 3, and should therefore only be interpreted as illustrative supporting evidence.

The second relationship we estimate is the reduced form effect of an increase in a country's strategic value to the United States on news coverage of its human rights abuses in U.S. newspapers. We repeat equation (3.2) using the natural logarithm of news stories as the dependent variable. We use the logarithm of the number of articles to reduce the weight placed on a few high profile countries which are frequently written about for reasons that presumably have little to do with changes in actual human rights situations in their countries.[39] If there are zero articles for a country in a given year, we take the natural logarithm of 0.1. Therefore, country-year observations with zero articles are not dropped from the sample. The estimates are very similar if they are dropped from the sample. See Section 5.3 on robustness for a more detailed discussion.

[39] For example, since 2000, human rights are mentioned in most of the news articles on China even if the main focus of the article is about an unrelated topic. The number of articles on Chinese human rights is just as likely to be correlated with the occurrence of the Olympic Games as with changes in strategic value to the United States or actual changes in the conditions for human rights.

We will conduct several robustness checks on our strategy. First, we can check that we are capturing the effect of UNSC membership of allies with a placebo test. We estimate the effect of the two years prior to being on the Council and its interaction with alliance on USSD PTS and news coverage. If our main estimates capture Council membership, these alternative estimates should not produce the same results.

For causal interpretation, we must assume that an ally's entry onto and exit from the UNSC do not affect news coverage of its human rights abuses through any channel other than strategic objectives. One potential concern is if readers are more interested in allies, and this interest increases when they are on the Council, then the estimated effects could be confounded by reader preferences.[40] This seems unlikely ex-ante since knowledge surveys show that only 15% of the Americans can name the Secretary General and that less than 16% of the Americans can name an agency within the UN (Alger, 2005: p. 59). Moreover, the most plausible reader preference is arguably to expect the media to monitor the bad behavior of U.S. allies when they enter the UNSC. This would be a bias against our estimates of the effect of bad behavior on under-reporting. For our result to be consumer driven, consumers would need to desire fewer stories of bad behavior, or fewer news stories overall when allies enter the Council. While we will not be able to conclusively rule out these possibilities, we will address the possibility that consumer preferences are driving our results by estimating the effect of government distortion for each newspaper separately and examining whether the extent of distortion correlates with measures of readers' preferences. See the section on robustness for details.

Our main empirical strategy estimates the reduced form effect of an increase in strategic value to the U.S. government on news coverage. To investigate the extent to which government influence is obtained through direct manipulation of the incentives of journalists and editorial boards or indirectly by manipulating the supply of primary information, we will estimate the effect of the triple interaction term of alliance, UNSC membership, and a measure for the cost of obtaining independent information, $U.S.Alliance \times UNSC \times IndependentInfoCost$. For example, if newspapers relied on several sources for information, one of which is the U.S. government, then newspapers' inference of the truth will vary with government reports. If news outlets are cost minimizing, it follows that the effect of strategic objectives on news coverage will be larger when it is more costly for news outlets to obtain information

[40] See studies by Mullainathan and Shleifer (2005) and Gentzkow and Shapiro (2006) for examples of how consumer preferences can drive news coverage.

from non-government sources. We will use two different measures to proxy cost: access to stories from independent foreign domestic press (e.g. Freedom House measure for media freedom) and the travel cost for a journalist from a U.S. newspaper to report personally (e.g. distance between national capitals and the nearest foreign office bureau of a U.S. newspaper). If information asymmetries facilitate the government influencing news coverage, the coefficient on this triple interaction term could be positive and significant.

5 Results

5.1 The Effect of U.S. Strategic Objectives on State Department Bias

Table 3 column (1) shows the estimates from the flexible equation (3.1) for the Cold War. The estimates show that the effect of UNSC membership in reducing negative reports from the USSD is increasing with alliance, although the difference between the two interaction effects is not statistically different. Column (2) shows that after the Cold War, this relationship breaks down as UNSC membership has no negative effects for higher levels of alliance.

Table 4 shows the estimated effects of using a continuous measure of alliance. It shows the effects of an increase in a country's strategic value to the United States on USSD PTS from equation (3.2). Panel A shows the estimates for the Cold War era. To illustrate the main effect of alliance, we estimate the effects controlling for the U.S. alliance main effect instead of country-fixed effects as in the main specification. The estimate for U.S. alliance in Column (1) shows that alliance is negatively correlated with reports on human rights by the United States. The estimate is only significant at the 15% level. In contrast, the estimate in Column (3) shows that alliance is uncorrelated with Amnesty reports.

Column (2) shows the baseline country-fixed effects specification. The estimated interaction effect means that conditional on a given level of alliance, UNSC membership decreases USSD reports of its human rights abuses by approximately three index points. The estimate is statistically significant at the 10% level. The effect of Council membership for a country that always votes with the United States in the UNGA is the sum of the estimated interaction effect and the main effect of UNSC membership. The joint statistic in Column (3) is 2.77 and it is statistically significant at the 10% level. For the median ally that votes with the United States

7% of the time, this means that Council membership will decrease USSD reports of human rights abuses by the sum of the main UNSC effect and the product of the interaction effect with the median level of alliance, which is 0.04 index points.

Column (4) shows that the effect on Amnesty reports of human rights abuses has the opposite sign, is much smaller in magnitude and statistically insignificant.

Columns (5)-(6) more explicitly compare the effect on USSD versus Amnesty PTS scores by first controlling for Amnesty scores on the right-hand side or alternatively using the difference in USSD and Amnesty scores as the dependent variable. The estimates show that Council membership of U.S. allies has significantly different effects on reports from these two agencies.

Since the empirical strategy is based on UNSC membership, we next restrict our sample to the 46 countries that were ever on the UNSC. Column (7) shows that the estimates on this restricted sample are similar in magnitude and statistically significant at the 10% level. To see if our results are driven by outliers, we plot the residuals from the regression in Column (7). Figure 45A shows that Zaire is an outlier in the eastern region of the plot. However, the dense cloud of observations along the regression line shows that even with the omission of Zaire, our estimates will be robust. Indeed, the estimate in Column (8) from using a sample of countries that were on the UNSC at least once and where Zaire is omitted are similar in magnitude and statistically significant at the 10% level.

Panel B shows the analogous estimates on the post-Cold War sample; there has been a decrease in the strategic value for the United States of allies. Comparing Columns (1) and (2) with (3) and (4) shows that Council membership for allies has no effect on Amnesty and U.S. reports after the Cold War. The estimates for the interaction effects on USSD PTS in Columns (2) and (7)-(8) are much smaller in magnitude than the Cold War estimates, have the opposite sign and are statistically insignificant.

In addition to the main results, there are several important pieces of evidence in Table 4 that support the argument that the effect of UNSC membership for U.S. allies comes through changes in U.S. strategic value. First, note that Columns (2) and (4) in Panel A demonstrate that the membership of allies affects USSD reports but not Amnesty reports. Second, we see that being allied to the United States in terms of UNGA voting is positively correlated with the USSD under-reporting human rights (Panel A Column 1), but has no effect on Amnesty's reports (Panel A Column 3). Finally, a comparison of the estimates in Panels A and B shows that these main effects of U.S. alliance decrease in magnitude after the Cold War,

when there has arguably been a decrease in the strategic value of allies. These results provide very suggestive evidence for our claim that our strategy is capturing changes in U.S. strategic value.

5.2 The Effect of U.S. Strategic Objectives on News Coverage

Table 3 column (3) shows the estimated effects for varying levels of alliance from flexible estimating equation (3.1). As with the effects on PTS scores, the estimates show that during the Cold War, the effect of UNSC membership in reducing news coverage on human rights violations is increasing with the level of alliance. Column (4) shows that after the Cold War, there is no effect for any level of alliance.

Table 5 shows the estimated effects of Council membership for U.S. allies on U.S. newspaper coverage of human rights abuses from using a continuous measure of alliance. We first show the estimates with U.S. alliance main effects and then with country-fixed effects. Panels A and B show the estimates for the Cold War and the post Cold War period. Panel A Column (1) shows that Council membership and alliance with the U.S. are correlated with more coverage on human rights abuses in U.S. newspapers. The estimate is significant at the 10% level. Column (2) presents the baseline estimates controlling for country- fixed effects. Like the estimate in Column (1), the estimate for the main effect of UNSC membership shows that, on average, newspapers write more articles of abuse of Council members. The estimate for the interaction effect shows that Council membership for a country of a given alliance level reduces news coverage of human rights abuses. The estimate is statistically significant at the 10% level. The joint statistic sums the estimated main effect of UNSC membership and the interaction effect and shows that for a country that votes with the United States 100% of the time, UNSC membership will reduce the news coverage of human rights abuses by 8.2 log points. The joint estimate is significant at the 10% level. For the median ally, UNSC membership will reduce coverage by 0.13 log points (-8.988$\times$0.07+0.755).

Column (3) controls for Amnesty reports as an explanatory variable and it shows that the magnitude of the estimate is virtually unchanged and the estimate is statistically significant at the 1% level. Interestingly, Amnesty PTS is uncorrelated with news coverage.

Column (4) shows that the estimated effect is unchanged when the sample is restricted to countries that were ever on the Council. Figure 5B plots the residuals

of this regression. It shows that, as before, Zaire is an outlier. Column (5) shows the estimate on the sample restricted to countries that were ever in the UNSC and where Zaire is excluded. The estimated effect is larger in magnitude and statistically significant at the 1% level.

Panel B presents the estimates for the post-Cold War period. The estimates of the interaction of UNSC and alliance in columns (2)-(5) are smaller in magnitude relative to the Cold War estimates and are not statistically significant. In column (1), the main effect of alliance with the United States is strikingly different. During the Cold War, the correlation was approximately 4.6 and statistically significant at the 10% level. Afterwards, it was reduced to approximately zero and is statistically insignificant. This is very suggestive of our strategy capturing the effects of U.S. objectives.

As with the first-stage estimates, there are several pieces of evidence here that support our claim that the interaction effects capture the effect of an increase in strategic value to the United States. The positive sign of the U.S. alliance main effect in Panel A means that, on average, allies receive more coverage on human rights abuses in newspapers. Similarly, the positive coefficient for UNSC members means that, on average, UNSC members receive more news coverage of human rights abuses. These estimates are consistent with the belief that readers may be more interested in news of human rights abuses of U.S. allies or Council members. However, the interaction of alliance and Council membership, the main source of variation of interest for this study, is negative. This means that despite the fact that readers may be more interested in the news of human rights practices of allies and Council membership, the news coverage of allies decreases when they enter the Council. This is very compelling evidence of our strategy capturing the effect of a change in strategic value to the U.S. government and not a change in consumer preferences. We will discuss the implications of these estimates in more detail later in Section 5.4.

5.3 Robustness

One concern in interpreting the main estimates is that we are capturing spurious country-specific trends. It seems unlikely that such trends are specific to levels of alliance and UNSC membership. To be cautious, we address this possibility by controlling for country-specific time trends. For each country, this will control for any change over time that is roughly linear. For brevity, we only present estimates for the

Cold War years in Table 6. Panel A shows the effects on USSD PTS scores. Column (1) shows the baseline estimate without controlling for country-specific time trends. Column (2) adds this control. Columns (3) and (4) show the estimates controlling for country-specific time trends for the restricted sample of countries that were ever on the UNSC, and countries that were ever on the UNSC excluding Zaire. The estimates are very similar in magnitude to the baseline estimate in Column (1) but not significant at conventional levels.

Panel B shows the estimated effects for newspaper coverage of human rights stories. Column (1) shows the baseline estimate for the full sample. Column (2) shows the estimate controlling for country-specific linear time trends. Columns (3) and (4) restricts the sample to countries ever on the UNSC and omit Zaire. The estimates in Columns (3) and (4) are statistically significant at the 10% level. They are similar in magnitude to the baseline estimate.

Next, we check that our estimates are robust to restricting the sample to years when the Reagan Administration was in power (1980-88). Since Cold War tensions increased and the apparatus for influencing the public opinion such as the OPD was strengthened during this administration, we should find that the main results are robust to the exclusion of the Carter years. Columns (5)-(7) of Table 6 show this to indeed be the case. The estimates are essentially unchanged when we restrict the sample although they are less exactly estimated.

Finally, we check that the benefits of Council membership do not occur before a country enters the Council. We estimate the effect in the two years leading up to a country's entry onto the Council and its interaction effect with U.S. alliance in an equation similar to the main estimating equation (3.2). Finding that the benefit of Council membership for allies exists before the two-year term is over would cast some doubt on the validity of our empirical strategy. Table 7 shows this not to be the case. The estimated interaction effects are smaller in magnitude, statistically insignificant and have the opposite sign as the main results.[41]

[41] In a previous version of the paper, we also examined the timing of the effects by estimating the interaction effect of alliance and Council membership for each of two years before, during and after Council membership, on the difference between USSD and Amnesty PTS scores. However, we were not able to exactly estimate a similar relationship for news coverage, most likely because of the many observations for which no stories were published. In this version of the paper, we have replaced these yearly estimates with a cruder placebo test of examining the effect of the years prior to Council membership and we present these for both outcomes. For consistency, the yearly estimates for PTS scores are not included in the paper. See earlier versions of the paper for the results.

We also check whether the linear specification is robust to censoring since many countries have no articles written in U.S. newspapers on their human rights abuses. Approximately 40% of the

5.4 Alternative Explanations

This section investigates whether the effects on news coverage can also be due to consumer preferences. For brevity, we only discuss and report results for the Cold War period.

There are two ways in which consumer preferences can drive our results. First, our identification assumption may be violated if UNSC membership of allies affects readers' interests in a country. Assuming that Americans prefer allies or countries with political power to have good human rights practices, the most likely scenario would be one where readers expect media outlets to increase the monitoring of bad behavior of allies when they are on the UNSC. This is consistent with our finding that the level of alliance with the United States and UNSC membership is each positively correlated with news coverage on human rights abuses (see Table 5 Panel A). However, this scenario will bias against our finding that an ally's entry onto the UNSC decreases the news coverage of bad behavior. To bias our results upwards, preferences would have to be such that consumers dislike hearing about bad behavior of UNSC members and the strength of this preference is increasing in alliance. This runs contrary to the correlations between the main effects of U.S. alliance, UNSC membership and news coverage shown in Table 5.[42]

Consumer preferences can also explain the main results if readers derive utility from hearing the government's version of events. For example, it may be important to know that the President thinks that a certain country is "evil" even if one disagrees with the view. Note that this is not an issue of the internal validity of our estimates. But it is important for considering the welfare impacts of news distortions. The welfare reduction will be smaller if readers value hearing whatever the government says. We indirectly explore this possibility by examining whether the extent of government distortion across papers correlates with the characteristics of the readers of each paper. We use two proxies of readership attitudes: a ranking

Cold War sample are observations where the value for the number of stories on human rights abuse in newspapers is zero. The OLS estimates on this censored distribution will be biased if the effects are mostly due to the number of news stories being increased from zero to one. To investigate this, we repeat the main estimation on a sample restricted to observations that had at least one story on human rights abuses in U.S. newspapers in a given year. The estimated effects are similar in magnitude between the full and restricted samples. This suggests that increasing the number of news stories from zero to more than zero is not the main margin for the main results. Similarly, the results are statistically similar when we use an alternative Tobit specification to address the potential censoring problem (see Appendix Table A2).

[42] Ideally, we would like to have a measure of true human rights behavior or a measure that does not depend on information from the U.S. government. To the best of our knowledge, there is no such measure for the time horizon and geographic scope needed by this study.

according to the 2008 *Mondo Conservativeness Rating* and a ranking according to the measure of media slant taken from Gentzkow and Shapiro (2006). Together with the estimated effects for each paper shown in Table 8 Columns (2)-(7), these provide two stylized facts that are inconsistent with the consumer driven hypothesis. Table 8 Column (1) shows the estimate for the sum from the main results. Columns (2)-(7) show the estimated effects on *The Washington Post*, NYT, WSJ, *The Chicago Tribune*, *The L.A. Times* and the CSM. The estimated effects for the first three newspapers are large in magnitude and statistically significant at the 1% level. The estimated effects for *The Chicago Tribune*, *The L.A. Times* and the CSM have the same signs as the first group of papers. But they are much smaller in magnitude and statistically insignificant. Next, we use bivariate regressions to estimate the correlations between the estimated effects for each paper and conservativeness rankings. The residuals and regression lines are plotted in Figures 6A and 6B. Figure 6A shows that there is no relationship between distortions and a ranking based on the *Mondo Conservativeness Rating*. Figure 6B shows that the estimated effects are also uncorrelated with Gentzkow and Shapiro's (2006) measure of media slant. These correlations should be interpreted with much caution as stylized facts since there are only six newspapers in the sample and both measures of readership preferences are based on data many years after the main period of our study. These stylized facts are inconsistent with consumer preferences not being a key driving force for our main results.

Out of interest, we also collected data on the number of news stories about human rights abuses published in newswires and two United Kingdom newspapers, *The Guardian* and *The Observer,* which are the only two non-U.S. English newspapers that are consistently available in the ProQuest Historical Database. Column (8) shows the estimates for newswire articles on human rights reports. It shows that the estimated distortion is large but only statistically significant at the 15% level. Column (9) shows that the interaction between alliance and Council membership has no effect on coverage in U.K. newspapers. This is consistent both with the fact that U.K. newspapers have less to gain from currying the favor of the U.S. government as well as the fact that U.K. readers could have very different preferences from their American counterparts.

We also investigate whether the main results differ for the two presidential administrations for the period of our study: Carter (1976-80) and Reagan (1980-88). Since the three distorted newspapers are typically left-leaning, it may be suspected that they are more likely to go along with the distortions of the relatively left-leaning

Carter administration. Similarly, it may be suspected that the effects were smaller during that administration because Cold War tensions were lower than during the Reagan Administration. This is supported by the estimate in Table 7 Column (10). It shows that the effect of distortions was smaller during the Carter administration.

5.5 Additional Results

The historical discussion of Public Diplomacy in Section 2.3 showed that the government could influence news coverage through direct manipulation of the incentives of journalists or through indirect manipulation of the supply of information to journalists. The latter is especially relevant for news on remote and often physically dangerous locations for which it is costly for the newspaper to obtain independent information. In this section, we investigate the extent to which the main effects are a result of the government's manipulation of information (i.e. that information asymmetries between newspapers and the USSD contribute to the main results) with an indirect test. We examine whether the effect of government distortion is larger when the cost for obtaining independent information is higher for the news organization. We have two measures to proxy cost. Table 8 column (11) shows that there is no interaction effect of domestic media freedom on human rights news coverage. Column (12) shows that there is no interaction effect between the distance to the nearest foreign bureau office on human rights news reports. In both cases, the estimates are close to zero in magnitude and statistically insignificant. These results are suggestive of information asymmetries not being likely to play an important role and are consistent with the hypothesis that the main results most likely reflect direct manipulation by the government.

5.6 Quantifying the Average Effect

We quantify the effects in two ways. First, we make the extreme assumption that the only way for the government of influencing the media was through the Country Reports and estimate a 2SLS estimate of the effects of under-reporting human rights violations in these reports on news coverage of human rights. Since this exclusion restriction is unlikely to be satisfied in practice, the 2SLS estimates should only be interpreted as an illustration of the upper-bound effects of biased Country Reports on news coverage.

The second-stage equation can be characterized as follows.

$$LnHRNews_{it} = \beta USSD_{it} + \alpha UNSC_{it} + \gamma_i + \delta_t + \varepsilon_{it}. \tag{3.3}$$

The natural logarithm of the number of news stories on human rights abuse for country i in year t is a function of: USSD PTS scores, $USSD_{it}$; a dummy variable indicating Council membership, $UNSC_{it}$; country-fixed effects, γ_i; and year-fixed effects, δ_t. Higher PTS reflects worse human rights conditions. If government bias reduces news coverage, then $\hat{\beta} > 0$.

Table 9 shows the OLS and 2SLS estimates for the sum of human rights coverage across all six U.S. newspapers in our sample. The 2SLS estimates in Columns (3) and (4) show that USSD under-reporting a country by one index point worse is associated with a reduction in coverage of approximately three log points. They are statistically significant at the 10% level. The fact that the 2SLS estimates are larger than the OLS estimates is consistent with the belief that the government has other ways of influencing the media beyond the reports and the likely possibility that the difference in PTS scores measures government bias with error.

Second, we calculate the average value of a seat on the UNSC during the Cold War conditional on a given level of alliance with the United States. For this exercise, we choose four of the United States' strongest allies during the Cold War: Brazil, Zaire, Honduras and Chile. Table 10 Column (2) shows that these countries voted with the United States on 12%, 20%, 20% and 27% of divided votes in the UNGA during the Cold War. Column (3) lists the average annual number of news articles on human rights abuses for these countries during the Cold War. In Column (4), we calculate the average effect of being on the UNSC on the difference in USSD and Amnesty PTS scores. This is the product of the measure of alliance in Column (2) and the estimated coefficient for the interaction term of UNSC membership and U.S. alliance plus the baseline estimate of the coefficient for the dummy variable of being on the UNSC. These calculations show that during the Cold War, UNSC membership reduced USSD reports of human rights abuses relative to Amnesty by 0.11 index points for Brazil, 0.35 index points for Zaire, 0.36 index points for Honduras and 0.55 index points for Chile. In Column (5) of Table 10, we similarly calculate the effect on news coverage of abuses in U.S. newspapers. We use the estimated coefficients from Table 5 Panel A Column (2). The calculation shows that a seat on the UNSC decreased news coverage of human rights abuse for Brazil by approximately 27%, for Zaire by approximately 64%, for Honduras by approximately 65% and for Chile

by approximately 80%.[43]

Note that in addition to the caveats described above, because we have no theoretical prediction of the functional form of the relationship between our measure of alliance and strategic value, the calculations presented here should be interpreted with much caution for illustrative purposes only.

6 Interpretation

The main results of this study show that an increase in strategic value to the United States improves reports of human rights practices from government agencies as measured by the State Department's Country Reports and reduces the amount of coverage of abuse in independently owned national newspapers. The empirical strategy attempts to overcome the difficulty of omitted variable bias, in particular the possibility that the effects on news coverage are driven by consumer attitudes rather than strategic objectives. The stylized fact that the extent of government influence across papers is not correlated with readership preferences is additional evidence suggesting that our main results are not driven by consumer preferences. Furthermore, the historical documents of the known cases of government manipulation of the news in Section 2.3 together with the empirical finding that the extent of distortion does not vary with newspapers' costs for obtaining independent information provides suggestive qualitative and quantitative evidence that direct manipulation of the incentives of journalists and editorial boards is an important force behind the main results.

Interestingly, we find that the extent of distortion across papers varies with their quality, as measured by average daily circulation ranking and the ranking of the number of Pulitzer Prizes for international news reporting. The residuals and re-

[43] In addition, we benchmark our results against a human rights incident for which there was plausibly no scope for government manipulation. We use the Chinese government crackdown on protesting students and workers during the Tiananmen Square Incident on June 4, 1989. This event and the month-long protest leading up to it were widely covered in mass media at the time. As the death of Premier Hu Yaobang, which instigated the protests, coincided with the seminal state visit from Soviet President Mikhail Gorbachev and the international press corps that accompanied his visit, it is reasonable to assume that the U.S. government could not distort coverage. This allows us to use the actual number of articles on human rights abuse in China in the month following the incident as a benchmark for an undistorted coverage of a known human rights violations event. For this example, we only use the NYT. In the 30 days after June 4, the NYT wrote eleven stories, ten more than the monthly average from the preceding year. Had the Tiananmen Square incident been completely ignored by the NYT, it would have written 91% fewer articles. Our most conservative reduced form estimates from Table 4 suggests that for the median country, U.S. strategic objectives reduced coverage by approximately 42% during the Cold War.

gression lines from the bivariate correlations between the estimated distortion for each newspaper from Table 7 and the two quality rankings are plotted in Figures 6C and 6D. They show that the extent of distortion is increasing with circulation and increasing with the number of Pulitzer Prizes. Note that this interpretation should be interpreted with much caution since there is significant heterogeneity across newspapers, and our measures only capture one specific dimension of "quality".

That said, we provide a speculative discussion on the implications of this correlation in the context of the Besley and Prat (2006) model of media capture which predicts that the highest quality newspapers are most likely to be captured by the government.[44] It has the interesting implication that there are probably high fixed costs to entry to the media market for international news reporting, and that this market is segmented. The intuition behind this is simple in the Besley and Prat (2006) context where there exists a competitive market of profit maximizing firms and where consumers value and can verify accuracy. If there were zero entry costs, then the marginal news outlet would enter the market to report the truth and earn positive profits when high reputation firms distort their reports. The firm that reports distorted news will lose profits. Therefore, in equilibrium, news outlets will not distort reports. It follows that distortions will only occur in this context if there are high fixed costs to entry. Examples of fixed costs include the formation of networks necessary for investigative journalism or reputation. For example, readers may have a positive prior about the government's credibility and are therefore unlikely to believe a news story that goes against official government reports unless it comes from a news outlet that has a long standing reputation for good journalism. Such a reputation takes time to acquire. The potentially ambivalent effects of reputation constitute an interesting avenue for future studies.[45]

[44] In their model, media outlets, as competitive profit maximizing firms, will agree to be distorted if the profits from going along with the distortions are higher than the profits from reporting the truth. Thus, the probability of capture will increase with the profits from going along with the government (e.g. value of exclusive access) and decrease with the costs (e.g. reputation loss). Furthermore, they show that if investments towards the quality (e.g. the ability to reveal the truth) of news reporting are endogenous, then firms will vertically differentiate in quality in equilibrium. In this case, the government will only attempt to capture the firms whose qualities are sufficiently high to reveal the truth. Under this framework, our results indicate that net, the benefits of going along with the government dominate the perceived costs of reputation losses. Moreover, our findings are consistent with the prediction that the probability of capture is positively correlated with quality.

[45] On the one hand, newspapers will want to invest in their quality by reporting the truth. On the other hand, if there is a fixed cost of obtaining quality, quality will segment the market between firms with and without it and will consequently make it easier for the government to capture the relevant news outlets.

For policy makers, potential segmentation of the market would imply that counting the number of media firms in a market without taking segmentation into account could grossly overstate the number of relevant firms. In our context, this means that the government perceived that the majority of the readers it wished to influence obtained information from these three newspapers and that information from other sources was not good substitutes. Hence, instead of having to influence thousands of media outlets, it only had to influence a few.

There are several caveats to interpreting the results. First, our focus on human rights has both advantages and limitations. On the one hand, it provides us with a well-defined concept that is relatively easy to measure in terms of government attitude and news coverage. On the other hand, under-reporting human rights abuse is just one of the many favors that the U.S. government can trade with foreign countries. Others could include increased U.S. foreign aid, favorable trade tariffs, increased foreign direct investment, or allocating international events that could raise the prestige of the governments of foreign countries (e.g. the Olympics). These are interesting topics for future research.

Second, it is beyond the scope of this paper to make conclusive statements about the welfare implications of government distortions in our context. On the one hand, readers may not give a high value to accurate foreign news reports.[46] Alternatively, if the readers gain utility from knowing the government attitude or like hearing reports that are consistent with the official government agenda during a period of international political tensions and increased American patriotism, these results would not lead to a decrease in welfare. On the other hand, there are many reasons to believe that government distortions reduce welfare. For example, readers' valuation of news may increase with the quality of news. The possibility that readers simply like hearing reports of government attitudes seems low as we find that the extent of distortion is uncorrelated with reader preferences across newspapers. Moreover, there may be negative externalities from distorted news reports or readers may not be time consistent and therefore undervalue their future utility from accurate news reports. The welfare implication of news distortions is an important topic for future

[46] Similarly, American readers may not value international news. This is difficult to assess. On the one hand, advertising revenues suggest that reporting foreign news does not directly generate much profit for newspapers. For the NYT in 2008, they were less than 10% of revenues from domestic news. If these reflect readers' valuation of accuracy in international news, then the welfare reduction from these distortions **is** likely to be small. On the other hand, advertising revenues may not accurately capture the readers' utility. For example, respondents to readership surveys by *The Washington Post*, *The Los Angeles Times* and *The Baltimore Sun* ranked the international/national news section among the top sections they read (Caroll, 2007).

studies.

7 Conclusion

This study estimates the effect of strategic objectives of the U.S. government on news coverage in U.S. newspapers. Our results suggest that even in a developed country with a large, independently owned and competitive media industry, the scope for government manipulation of the news can be significant. Note that while we are not able to conclusively rule out alternative explanations, such as the possibility that consumer preferences play a role, we provide several pieces of evidence to suggest that the role is not likely to be very important in our context. Specifically, we find that the extent of distortion does not vary by readership preferences across newspapers. Moreover, we find that while UNSC membership and alliance with the United States both lead to more news coverage of human rights abuses in themselves, UNSC membership of allies reduces news coverage. This is interesting since it suggests that consumer preferences are such that readers find the human rights abuses of Council members and U.S. allies more newsworthy, but that the government suppresses the coverage of abuses by its allies.

The United States provides a context where nearly all domestic news outlets are independently owned and where the market for news is very competitive by all accounts. Therefore, to the extent that the competitiveness of the news market is the key variables determining the manipulation possibilities by governments, the results we obtain on government manipulation in the United States can be broadly interpreted as a lower-bound for the scope for manipulation in other countries.

Bibliography

[1] Bates, Robert H. (1986), "Modernization, Ethnic Competition, and the Rationality of Politics in Contemporary Africa," *Governing in Black Africa*, 6(4):457-84.

[2] Alesina, Alberto and David Dollar. 2000. "Who gives aid to whom and why?" *Journal of Economic Growth.* 5 (1): 33-63.

[3] Alger, Chadwick (2005) *The United Nations System: A reference handbook.* ABC-Clio, Inc. Santa Barbara: p. 59.

[4] Bennet, Lance, Regina Lawrence and Steven Livingston. When the Press Fails: Political Power and the News Media from Iraq to Katrina. Chicago: University of Chicago Press, 2007.

[5] Besley, Timothy and Prat, Andrea. "Handcuffs for the Grabbing Hand? Media Capture and Government Accountability." American Economic Review 96.3 (2006): 720-36.

[6] Blanton, Shannon Lindsey and Robert C. Blanton. "What Attracts Foreign Investors? An Examination of Human Rights and Foreign Direct Investment." The Journal of Politics 69.1 (2007): 143-55.

[7] "Brazil Cancels Military Aid Treaty Over U.S. Report on Human Rights" by The Associated Press, *New York Times*, Mar 12, 1977.

[8] Brooks, Jack "Letter to The Honorable Dante B. Fascell (on the Investigation of the Office of Public Diplomacy)." National Security Archives, 2001 (1987).

[9] Bueno de Mesquita, Bruce, and George W. Downs. 2006. "Intervention and Democracy." International Organization 60 (July): 627– 649.

[10] Byrd, Peter. "Cold War.", The Concise Oxford Dictionary of Politics. Ed. McLean, Iain and Alistair McMillan. Oxford University Press, 2003.

[11] Carlyle, Thomas and Thomas MacMehan. On Heroes and Hero Worship, and the Heroic in History. London: Ginn and Company, 1902.

[12] Caroll, Jill. "State of the American Newspaper." American Journalism Review, 2002.

[13] Caroll, Jill. "Foreign News Coverage: The U.S. Media's Undervalued Asset." The Joan Shorenstein Center on the Press, Politics and Public Policy Working Paper Series No. 2007-1 (2007).

[14] Case, Anne. "Election Goals and Income Redistribution: Recent Evidence from Albania." European Economic Review 45 (2001): 405-23.

[15] Chang, Chun-Fang and Brian Knight. "Media Bias and Influence: Evidence from Newspaper Endorsements." NBER Working Paper 14445 (2008).

[16] Cohen, Jeff. "The Return of Otto Reich: Will government propagandist join Bush Administration?" Fairness & Accuracy in Reporting (2001).

[17] Constable, Pamela "The Demise of the Foreign Correspondent." Washington Post Sunday, February 18, 2007; B01.

[18] Crouse, Timothy. The Boys on the Bus. Ballantine Books, 1974.

[19] de Mesquita, Bruce Bueno and George Downs. "Intervention and democracy." International Organization 60.3 (2006):627–549.

[20] Djankov, Simeon, et al. "Who owns the media?" Journal of Law and Economics 46 (2003).

[21] DellaVigna, Stefano and Ethan Kaplan. "The Fox News Effect: Media Bias and Voting" Quarterly Journal of Economics (2007).

[22] Dixit, Avinash, and John Londregan . "The Determinants of Success of Special Interests in Redistributive Politics." Journal of Politics 58 (1996): 1132-1155.

[23] Dreher, Axel, et al. "Development aid and international politics: Does membership on the UN Security Council influence World Bank decisions?" Journal of Development Economics 88.1 (2009): 1-18.

[24] Dube, Arindrajit, et al. "Coups, Cooperation and Classified Information." University of Stockholm IIES Working Paper (2009).

[25] Durante, Ruben and Brian Knight, Brian. "Partisan Control, Media Bias, and Viewer Responses: Evidence from Berlusconi's Italy." NBER Working Paper No. 14762 (2009).

[26] Easterly, William, Shanker Satyanath and Daniel Berger "Was Lenin Right? US Political Influence and International Trade during the Cold War Era. "Harvard University Working Paper (2009).

[27] Eisensee, Thomas and David Stromberg. "News Floods, News Droughts, and U.S. Disaster Relief." Quarterly Journal of Economics 122.2 (2004).

[28] Enikolopov, Ruben, et al. "Media and Political Persuasion: Evidence from Russia" New Economic School Working Paper (2009).

[29] Gbadolite, Adam Zagorin. "Leaving Fire in His Wake" TIME Magazine, Sunday, Jun. 24, 2001.

[30] Gentzkow, Matthew. "Television and Voter Turnout." Quarterly Journal of Economics 121.3 (2006).

[31] Gentzkow, Matthew, Edward Glaeser and Claudia Goldin. "The Rise of the Fourth Estate: How Newspapers Became Informative and Why It Mattered." NBER Working Paper 10791 (2006).

[32] Gentzkow, Matthew and Shapiro, Jesse M. "What Drives Media Slant? Evidence from U.S. Daily Newspapers." National Bureau of Economic Research Working Paper 12707 (2006).

[33] Gentzkow, Matthew and Shapiro, Jesse M. "Market Forces and News Media in Muslim Countries." Initiative on Global Markets Working Paper 6 (2007).

[34] Gentzkow, Mathew and Jesse Shapiro. "Preschool Television Viewing and Adolescent Test Scores: Historical Evidence from the Coleman Study." Quarterly Journal of Economics 123.1 (2008a).

[35] Gentzkow, Mathew and Jesse Shapiro. "Competition and Truth in the Market for News." Journal of Economic Perspectives (2008b).

[36] Gerber, Alan, Dean Karlan and Daniel Bergan. "Does the Media Matter? A Field Experiment Measuring the Effect of Newspapers on Voting Behavior and Political Opinions." American Economic Journal - Applied, Forthcoming (2009).

[37] Gibney, Mark and Dalton, Matthew. "The Political Terror Scale," Policy Studies and Developing Nations, 4 (1996):73-84.

[38] Gladdis, John Lewis. The Cold War: A New History. New York: Penguin, 2006.

[39] Hamilton, Lee H. and Inouye, Daniel K. "Report of the Congressional Committees Investigating the Iran-Contra Affair with Supplemental, Minority, and Additional Views" Committee of the Whole House of the State of the Union 100th Congress, 1st Session, Washington, H. Rept. No 100-433 (1987).

[40] Jacobwitz, Jake. "Duties of TDY Military Personnel (Memo to Ambassador Reich)" National Security Archives, 2001 (1985).

[41] Jacobwitz, J. "Public Diplomacy Action Plan. Support for the White House educational campaign." National Security Archives, 2001 (1985).

[42] Jensen, Robert and Emily Oster. "The Power of TV: Cable Television and Women's Status in India." Quarterly Journal of Economics Forthcoming (2008).

[43] Knight, Brian. "Estimating the Value of Proposal Power." The American Economic Review 95.5 (2005): 1639-1652.

[44] Kuziemko, Ilyana, and Eric D. Werker. "How Much Is a Seat on the Security Council Worth? Foreign Aid and Bribery at the United Nations." Journal of Political Economy 114.5 (October 2006): 905-930.

[45] La Feber, Walter. Walter LaFeber Papers, #14-17-2624. Division of Rare and Manuscript Collections, Cornell University Library (1991): 194-197.

[46] La Ferrara, Eliana, et al. "Soap Operas and Fertility: Evidence from Brazil" Universita Bocconi Working Paper.

[47] Lebovic, James and Voeten, Eric. "The Cost of Shame: International Organizations, Foreign Aid, and Human Rights Norms Enforcement." Journal of Peace Research 46 (2009): 79-97.

[48] Letters to the Editor, *Washington Post*, Monday, June 9, 2008; Page A16.

[49] Lindbeck, Assar and Joerg Weibull, "Balanced-budget Redistribution as the Outcome of Political Competition," Public Choice, 1987, pp. 273–97.

[50] Logan, Brian, and Sutter, Daniel. "Newspaper Quality, Newspaper Pulitzer Prizes, and Circulation." Atlantic Economic Journal 32.2 (2004):100-12.

[51] Malkasian, Carter. The Korean War, Routledge, 2001, pp 16.

[52] Malone, David M. "Eyes on the Prize: The Quest for Nonpermanent Seats on the U.N. Security Council." Global Governance 6.1 (Jan-Mar 2000): 3-24.

[53] Miguel, Edward and Farhan Zaidi. "Do Politicians Reward their Supporters? Public Spending and Incumbency Advantage in Ghana." Manuscript, University of California, Berkeley (2003).

[54] Miller, Jonathan. "'White Propaganda' Operation (Memo to Mr. Pat Buchanan)." National Security Archives, 2001 (1985).

[55] Mullainathan, Sendhil and Andrei Shleifer, Andrei. "The Market for News." American Economic Review 95.4 (2005): 1031-53.

[56] Olken, Benjamin. "Do TV and Radio Destroy Social Capital? Evidence from Indonesian Villages."MIT Working Paper (2008).

[57] de Onic, Juan"Argentina and Uruguay Reject U.S. Assistance Linked to Human Rights" *The New York Times*, Mar 2, 1977.

[58] Paluck, Elizabeth. "Reducing intergroup prejudice and conflict using the media: A field experiment in Rwanda." Journal of Personality and Social Psychology Forthcoming (2008).

[59] Parry, Robert and Peter Kornblub. "Iran-Contra's Untold Story." Foreign Policy (1988).

[60] Prat, Andrea and David Stromberg. "Commercial Television and Voter Information." CEPR Discussion Papers 4989 (2005).

[61] Poe, Steven C., et al. "How are these pictures different? A quantitative comparison of the U.S. State Department and Amnesty International Human Rights Reports, 1976-95", Human Rights Quarterly 23 (2007): 650-77.

[62] Qian, Nancy and Yanagizawa, David. "The Strategic Determinants of U.S. Human Rights Reporting: Evidence from the Cold War" Journal of European Economic Association Papers and Proceedings (2009).

[63] Ray, Bruce A. "Military Committee Membership in the House of Representatives and the Allocation of Defense Department Outlays." Western Political Quarterly 34.2 (Jun 1981): 222-234.

[64] Reich, Otto Juan. "TDY Personnel for S/LDP (Memo to Ray Warren)." National Security Archives, 2001 (1985).

[65] Schultz, George P. "New Coverage of Central America (Memo to The President)." National Security Archives, 2001 (1984).

[66] Stohl, Michael and Carleton, David. "Foreign Policy of Human Rights: Rhetoric and Reality from Jimmy Carter to Ronald Reagan." Human Rights Quarterly 7 (1985): 205-29.

[67] Stromberg, David. "Mass Media Competition, Political Competition, and Public Policy." Stockholm University Working Paper (2002).

[68] Stromberg, David. "Radio's Impact on Public Spending." Quarterly Journal of Economics 119.1 (2004).

[69] Thomas, Helen. Watchdogs of Democracy? The Waning Washington Press Crops and How it has Failed the Public. New York: Schribner, 2006.

[70] Voeten, Eric. "Outside Options and the Logic of Security Council Action Source." The American Political Science Review 95. 4 (Dec., 2001): 845-858.

[71] Wood, Reed and Gibney, Mark. "The Political Terror Scale (PTS): A Reintroduction and a Comparison to CIRI." Human Rights Quarterly, Forthcoming (2009).

Appendix

Simple Model of Vote buying on the UNSC

In this section, we present a simple model which is an application of the probabilistic voting model of Lindbeck and Weibull (1987). We present an environment where one country, the U.S., bribes different countries with favors in exchange for those countries voting in favor of policies endorsed by the U.S.. Countries are identical with the exception that they have differing levels of affinity for the U.S.. The purpose of our model is to illustrate the conditions under which countries with a higher affinity for the U.S. receive higher bribes and are more likely to vote in favor of the U.S.

More formally, imagine if every country $i = \{1, ..., N\}$ receives a utility $u(\theta_i, x_i)$ if it votes in favor of the U.S.-endorsed policy and it receives $\underline{u}$ if it votes against the U.S.-endorsed policy. $\theta_i > 0$ measures country i's affinity for the U.S. so that $u(\theta_i, x_i)$ is increasing in θ_i. It captures the fact that countries differ in the extent to which they favor the policies endorsed by the U.S. $x_i \geq 0$ measures the size of the bribe paid by the U.S. to country i if the U.S.-endorsed policy is passed successfully. Therefore, $u(\theta_i, x_i)$ is increasing in x_i, and we assume that $u(\cdot)$ is continuously differentiable and concave in x_i. Country i votes in favor of the U.S. endorsed policy if

$$u(\theta_i, x_i) \geq \underline{u} + \epsilon_i,$$

where ϵ_i is a random mean zero error term which is realized after the U.S. announces x_i and which is independent across all i. Therefore, the probability that country i votes in favor of the U.S. is equal to $\Pr\{\epsilon_i \leq u(\theta_i, x_i) - \underline{u}\}$. The error term ϵ_i is standard in the probabilistic model of voting and it captures the fact that voters may sometimes take noisy non-fundamental factors into account when making their voting decision. For simplicity, we assume ϵ_i to be uniformly distributed on the $[-1/2, 1/2]$ for all ϵ_i. Given this assumption, the implied probability that country i votes in favor of the US policies is equal to

$$\frac{1}{2} + u(\theta_i, x_i) - \underline{u},$$

where we have implicitly assumed that the parameters of our environment and the equilibrium choice of x_i guarantee that this probability is between 0 and 1.

The U.S. chooses the set of bribes $\{x_i\}_{i=1}^N$ so as to maximize the number of votes

which it receives, taking into account that each bribe costs resources, whether it be in terms of money, effort, or reputation for the U.S. For simplicity, we let the cost of each bribe be linear, so that the objective of the U.S. can be written as

$$\max_{\{x_i\}_{i=1}^N} \sum_{i=1}^{N} \left(\frac{1}{2} + u\left(\theta_i, x_i\right) - \underline{u} - x_i \right),$$

which yields the following first-order condition:

$$u_x\left(\theta_i, x_i\right) = 1 \ \forall i. \tag{3.4}$$

Condition (3.4) relates the size of the bribe x_i to a country's affinity with the US θ_i. Now imagine if it is the case that

$$u_x\left(\theta'', x_i\right) > u_x\left(\theta', x_i\right) \text{ if } \theta'' > \theta' \ \forall x_i. \tag{3.5}$$

Condition (3.5) states that the marginal benefit of an additional bribe is rising in a country's affinity for the U.S. If this condition is satisfied, it clearly follows that (3.4) implies that x_i is rising in θ_i so that countries with a greater affinity for the U.S. also receive higher bribes. Moreover, since $u\left(\cdot\right)$ is rising in θ_i and x_i, it also follows that the probability that country i votes in favor of U.S.-endorsed policies is rising in its affinity for the U.S..

As an aside, note that this conclusion need not hold if (3.4) is violated. If it were instead the case that $u_x\left(\theta'', x_i\right) < u_x\left(\theta', x_i\right)$ if $\theta'' > \theta' \ \forall x_i$, then x_i would be declining in θ_i so that countries with a lower affinity for the US would also receive lower bribes. Parametric cases can easily be constructed in such an environment where the probability that country i votes in favor of U.S. policies is rising in its affinity for the U.S., although countries with **a** higher affinity also receive lower bribes.

Public Diplomacy

Examples of Government Disinformation

In a letter to House Speaker Patrick Buchanan, the Deputy Director for *Public Diplomacy for Latin American and the Caribbean* (SLDP), Jonathan Miller described how the OPD was carrying out "white propaganda" operations. This included writing opinion articles under false names and placing them in leading newspapers such as

the *Wall Street Journal* (Miller, 1985; Hamilton and Inouye, 1987). Similar opinion editorials were planted in the *New York Times* and the *Washington Post* (Fascell, 1987). Another example occurred on the night of Ronald Reagan's re-election. Otto Juan Reich, the director of S/LPD, handed journalists a story about how Soviet MiGs were arriving in Nicaragua that was later proven to be false (Cohen, 2001).

In general, the OPD flooded the media, academic institutions and other interested groups with information. For example, in 1982, the OPD booked more than 1,500 speaking engagements with editorial boards, radio, and television interviewers, distributed materials to 1,600 college libraries, 520 political science faculties, 122 editorial writers, and 107 religious groups. Extra attention was given to prominent journalists (Parry and Kornblub, 1987).

Examples of Government Dealings with Journalists

Many uncooperative journalists were accused of being disloyal to the United States or having secret agendas. For example, in 1985, the OPD spread a story that certain American reporters had exchanged favorable reports on Nicaragua in exchange for Sandinistan prostitutes. In a 1985 article in *New York Magazine*, Reich went further to say that "it [prostitutes] isn't only for women" and that the Nicaraguans provided men for gay journalists.

In some cases, the government removed uncooperative journalists from the countries they were reporting from. For example, in 1982 U.S. embassy officials boasted that they had forced the *New York Times* correspondent, Raymond Bonner, out of El Salvador because of his unfavorable reporting of that government, which was a U.S. ally. Uncooperative journalists also became the targets of character assassination meant to induce skepticism about the information they reported.

Examples of Foreign Governments Protesting U.S. Criticism over Human Rights Practices

For example, in 1977, Congress insisted that the aid to Uruguay be made in installments contingent on improvements in human rights. Uruguay's displeasure at this was voiced when it "spurned" the aid (Onic, 1977). Less than two weeks later, the Brazilian government turned down the Carter administration's offer to ask Congress for a 50 million dollar aid package when the State Department handed a copy of the Country Report on human rights abuses in Brazil to the latter's embassy in Washington D.C. In Brazil, this move was extremely popular with the public and even the

opposition party (AP, 1977). More recently, in June 2008, U.S. Commerce Secretary, Carlos Gutierrez, explained that the U.S. must continue its trade embargo on Cuba because the latter "systematically brutalizes its people". (Washington Post, 2008). Qian and Yanagizawa (2009) provide more examples.

Table 1: The Correlates of U.S. Foreign Aid

	Correlation with Ln Total U.S. Aid (1996 USD)	
	Cold War 1976-88	Post Cold War 1992-2005
	(1)	(2)
U.S. Alliance	0.2638*	0.0042
UNSC	0.0688*	0.0118
U.S. Alliance x UNSC	0.0851*	-0.0043

* Indicates 5% significance level.

Table 2: Descriptive Statistics

	A. All Years 1976-2005			B. Cold War 1976-88			C. Post Cold War 1991-2005		
Variable	Obs	Mean	Std. Err.	Obs	Mean	Std. Err.	Obs	Mean	Std. Err.
USSD PTS	2624	2.891	(0.021)	1010	2.680	(0.031)	1325	3.046	(0.030)
Amnesty PTS	2624	3.029	(0.020)	1010	3.029	(0.030)	1325	3.034	(0.029)
USSD - Amnesty PTS	2624	-0.138	(0.014)	1010	-0.349	(0.024)	1325	0.012	(0.019)
U.S. Alliance	2624	0.091	(0.001)	1010	0.090	(0.002)	1325	0.093	(0.002)
UNSC	2624	0.063	(0.005)	1010	0.066	(0.008)	1325	0.057	(0.006)
HR News	2624	11.284	(0.517)	1010	8.659	(0.674)	1325	13.440	(0.843)
HR W Post	2624	3.501	(0.182)	1010	2.104	(0.172)	1325	4.842	(0.325)
HR NYT	2624	2.798	(0.129)	1010	2.564	(0.190)	1325	2.884	(0.194)
HR WSJ	2624	0.802	(0.053)	1010	0.250	(0.026)	1325	1.312	(0.100)
HR C. Tribune (1976-1986)	820	0.776	(0.095)	820	0.776	(0.095)			
HR L.A. Times	2624	3.639	(0.173)	1010	2.328	(0.194)	1325	4.402	(0.284)
HR CMS (1976-1996)	1773	0.447	(0.168)	1000	0.792	(0.07)			
HR U.K. (1976-2003)	2441	0.629	(0.037)	1010	0.441	(0.04)	1142	0.799	(0.07)
HR Newswires	2544	11.109	(0.555)	1010	5.974	(0.616)	1325	14.768	(0.916)
Distance to NYT	2624	1463.8	(21.895)	1010	1443.2	(33.680)	1325	1479.3	(32.164)
No Media Freedom	2624	0.393	(0.010)	1010	0.404	(0.015)	1325	0.383	(0.013)

Table 3: The Effect of U.S. Alliance and UNSC membership, Three U.S. Alliance Levels

	Dependent Variables			
	USSD PTS		Ln HR News Stories	
	(1) Cold War	(2) Post Cold War	(3) Cold War	(4) Post Cold War
Medium U.S. Alliance x UNSC	-0.213	0.213	-0.596	-0.329
	(0.197)	(0.173)	(0.326)	(0.263)
Strongest U.S. Alliance x UNSC	-0.291	0.248	-1.016	-0.193
	(0.170)	(0.165)	(0.548)	(0.274)
UNSC	0.207	-0.226	0.620	0.349
	(0.121)	(0.142)	(0.262)	(0.194)
Observations	1010	1325	1010	1325
R-squared	0.665	0.753	0.734	0.767

All regression control for country and year fixed effects. Standard errors are clustered at the country level.
Notes: Alliance is divided into three equal frequency groups, where the average level of alliance for each group is 0.03, 0.07 and 0.17.

Table 4: The Effect of U.S. Alliance and UNSC membership, Human Rights Reporting

	Dependent Variables							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	USSD	USSD	Amnesty	Amnesty	USSD	USSD-Amnesty	USSD	USSD
	All	All, Baseline	All	All	All	All	EverSC	EverSC, Omit Zaire
	A. Cold War 1976-1988							
U.S. Alliance x UNSC	-2.849	-3.022	2.622	1.212	-3.560	-4.234	-3.013	-4.475
	(4.249)	(1.649)	(3.303)	(2.867)	(1.346)	(2.278)	(1.639)	(2.684)
UNSC	0.0936	0.252	-0.306	-0.119	0.306	0.372	0.257	0.345
	(0.312)	(0.148)	(0.262)	(0.237)	(0.118)	(0.186)	(0.148)	(0.188)
U.S. Alliance	-1.864		0.500					
	(1.177)		(1.214)					
Amnesty PTS					0.444			
					(0.343)			
Country FE	N	Y	N	Y	Y	Y	Y	Y
Observations	1010	1010	1010	1010	1010	1010	607	595
R-squared	0.050	0.664	0.007	0.622	0.736	0.363	0.643	0.643
Joint	-2.756	-2.769	2.317	1.093	-3.255	-3.862	-2.755	-4.130
p-value	0.490	0.0742	0.454	0.682	0.0109	0.0715	0.0759	0.107
	B. Post Cold War 1992-2005							
U.S. Alliance x UNSC	-1.478	1.246	-1.427	0.401	1.099	0.845	1.319	1.344
	(2.768)	(1.086)	(2.662)	(1.140)	(0.971)	(1.168)	(1.142)	(1.141)
UNSC	0.0852	-0.173	0.141	-0.0868	-0.141	-0.0861	-0.183	-0.185
	(0.287)	(0.122)	(0.277)	(0.144)	(0.121)	(0.166)	(0.126)	(0.126)
U.S. Alliance	-0.475		-0.831					
	(1.377)		(1.298)					
Amnesty PTS					0.367			
					(0.343)			
Country FE	N	Y	N	Y	Y	Y	Y	Y
Observations	1325	1325	1325	1325	1325	1325	737	723
R-squared	0.009	0.753	0.012	0.668	0.797	0.181	0.719	0.705
Joint	-1.393	1.073	-1.286	0.314	0.958	0.759	1.135	1.158
p-value	0.584	0.278	0.600	0.761	0.273	0.463	0.278	0.268

All regressions control for year fixed effects. Standard errors are clustered at the country level.

Table 5: The Effect of U.S. Alliance and UNSC membership, News Coverage of Human Rights Abuses

	Dependent Variable: Ln HR News				
	(1)	(2)	(3)	(4)	(5)
	All	All, Baseline	All, Baseline	Ever SC	Ever SC, Omit Zaire
	A. Cold War 1976-1988				
U.S. Alliance x UNSC	-7.871	-8.988	-9.340	-9.104	-14.78
	(8.802)	(4.873)	(4.328)	(4.460)	(5.798)
UNSC	0.916	0.755	0.789	0.785	1.125
	(0.722)	(0.351)	(0.315)	(0.325)	(0.367)
U.S. Alliance	4.591				
	(2.693)				
Amnesty PTS			0.290		
			(0.0790)		
Country FE	N	Y	Y	Y	Y
Observations	1010	1010	1010	607	595
R-squared	0.032	0.734	0.740	0.766	0.768
Joint	-6.955	-8.234	-8.550	-8.318	-13.66
p-value	0.395	0.0737	0.0371	0.0508	0.0153
	B. Post Cold War 1992-2005				
U.S. Alliance x UNSC	5.081	-0.197	-0.265	-0.503	-0.421
	(3.376)	(2.109)	(2.035)	(2.074)	(2.069)
UNSC	0.186	0.177	0.192	0.212	0.203
	(0.422)	(0.213)	(0.207)	(0.218)	(0.219)
U.S. Alliance	-0.629				
	(2.694)				
Amnesty PTS			0.170		
			(0.0617)		
Country FE	N	Y	Y	Y	Y
Observations	1325	1325	1325	737	723
R-squared	0.027	0.767	0.769	0.753	0.754
Joint	5.267	-0.0193	-0.0726	-0.291	-0.218
p-value	0.0876	0.992	0.969	0.878	0.908

All regressions control for year fixed effects. Standard errors are clustered at the country level.

Table 6: Robustness Tests

	Dependent Variables						
	Cold War 1976-1988				Restricted (Reagan) Cold War 1980-88		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	All	EverSC	EverSC, Omit Zaire	All	EverSC	EverSC, Omit Zaire
	A. USSD PTS						
U.S. Alliance x UNSC	-3.022	-2.551	-2.554	-3.675	-2.820	-3.013	-3.360
	(1.649)	(1.671)	(1.688)	(2.939)	(1.611)	(1.639)	(3.000)
UNSC	0.252	0.235	0.236	0.303	0.283	0.257	0.323
	(0.148)	(0.170)	(0.174)	(0.226)	(0.161)	(0.148)	(0.216)
Country Time Trends	N	Y	Y	Y	N	N	N
Observations	1010	1010	607	595	776	607	456
R-squared	0.664	0.762	0.733	0.733	0.736	0.643	0.700
Joint	-2.769	-2.316	-2.318	-3.372	-2.537	-2.755	-3.037
p-value	0.0742	0.133	0.139	0.223	0.0913	0.0759	0.285
	B. Ln HR News						
U.S. Alliance x UNSC	-8.988	-6.900	-7.158	-11.35	-8.570	-8.968	-17.18
	(4.873)	(4.264)	(3.934)	(6.713)	(5.599)	(5.190)	(4.583)
UNSC	0.755	0.664	0.696	0.946	0.693	0.727	1.206
	(0.351)	(0.350)	(0.329)	(0.462)	(0.395)	(0.364)	(0.315)
Country Time Trends	N	Y	Y	Y	N	N	N
Observations	1010	1010	607	595	776	465	456
R-squared	0.734	0.784	0.803	0.804	0.780	0.804	0.807
Joint	-8.234	-6.236	-6.462	-10.40	-7.877	-8.242	-15.97
p-value	0.0737	0.118	0.0813	0.103	0.136	0.0957	0.00048

All regressions control for country and year fixed effects. Standard errors are clustered at the country level.

Table 7: Placebo

	Dependent Variables					
	USSD PTS			Ln HR News		
	(1)	(2)	(3)	(3)	(4)	(6)
	All	Ever UNSC	Ever UNSC, Omit Zaire	All	Ever UNSC	Ever UNSC, Omit Zaire
U.S. Alliance x Pre UNSC	1.756	1.923	2.855	1.026	1.470	-1.551
	(1.995)	(1.953)	(2.804)	(3.469)	(3.572)	(3.326)
Pre UNSC	-0.146	-0.168	-0.221	-0.146	-0.187	-0.0159
	(0.166)	(0.167)	(0.201)	(0.292)	(0.299)	(0.280)
Observations	1010	607	595	1010	607	595
R-squared	0.664	0.643	0.643	0.733	0.764	0.766
Joint	1.610	1.755	2.634	0.880	1.283	-1.567
p-value	0.390	0.340	0.321	0.786	0.701	0.616

All regressions control for country and year fixed effects. Standard errors are clustered at the country level. The coefficient of the interaction term of a dummy variable for the two years prior to UNSC x U.S. Alliance, controlling for UNSC dummy variable, U.S. Alliance, country and year fixed effects.

Table 8: The Effect on News Coverage – Heterogeneous Effects

	Dependent Variables: Ln HR Stories											
	(1) All	(2) WPost	(3) NYT	(4) WSJ	(5) CHI	(6) LAT	(7) CSM	(8) Wires	(9) U.K.	(10) News	(11) News	(12) NYT
U.S. Alliance x UNSC	-9.104	-11.05	-9.467	-8.243	-5.041	-2.555	-5.494	-15.86	0.103	-10.01	-10.95	-15.79
	(4.460)	(3.310)	(3.188)	(3.927)	(3.410)	(5.521)	(4.118)	(10.50)	(5.159)	(5.471)	(8.101)	(6.866)
U.S. Ally x UNSC x Distance to NYT bureau												0.0038
												(0.0032)
U.S. Ally x UNSC x No Media Freedom											2.655	
											(9.545)	
U.S. Ally x UNSC x Carter										24.99		
										(18.98)		
Obs	607	607	607	607	496	607	597	607	607	607	595	607
R-Sq	0.771	0.646	0.659	0.439	0.557	0.633	0.516	0.679	0.434	0.743	0.654	0.746

All regressions control for UNSC dummy, Amnesty PTS, and country and year fixed effects. Standard errors are clustered at the country level. The sample is restricted to countries that were on the UNSC at least once. All regressions control for a UNSC dummy variable, U.S. Alliance, country and year fixed effects.

Table 9: The OLS and 2SLS Estimates of the Effect of U.S. State Department Reports on News Coverage

	Dependent Variables: Ln HR News Articles							
	Cold War				Post Cold War			
	OLS		2SLS		OLS		2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables	All	Omit NeverSC	All	Omit NeverSC	All	Omit NeverSC	All	Omit NeverSC
USSD	0.431	0.300	2.975	3.022	0.211	0.226	-0.158	-0.382
	(0.0829)	(0.0984)	(1.681)	(1.754)	(0.0661)	(0.0884)	(1.773)	(1.781)
UNSC	0.117	0.144	0.0038	0.0079	0.173	0.182	0.150	0.142
	(0.143)	(0.141)	(0.251)	(0.259)	(0.110)	(0.113)	(0.167)	(0.174)
Observations	1010	607	1010	607	1325	737	1325	737
R-squared	0.745	0.770	0.319	0.311	0.770	0.757	0.761	0.729
Average Effect	-0.151	-0.105	-1.041	-1.058	-0.0738	-0.0791	0.0552	0.134
p-value	9.96e-07	0.00347	0.0798	0.0903	0.00186	0.0132	0.929	0.831

All regressions control for UNSC dummy, Amnesty PTS, and country and year fixed effects. Standard errors are clustered at the country level. * Average effect= coefficient for USSD PTS x -0.35.

Table 10: Average Effect of Government Objectives for Select U.S. Cold War Allies

(1)	(2)	(3)	(4)	(5)
Country	U.S. Alliance during CW	Number of Annual HR Stories during CW	Effect of Being on UNSC on U.S. PTS Underreporting	% Effect of Being on UNSC on U.S. HR News Coverage
			(2) x -3.022+0.252	(exp[(2) x -8.988+0.755]-1) x 100
Brazil	0.12	11.58	-0.11	-27.48%
Congo, Dem. Rep.	0.20	4.42	-0.34	-63.92%
Honduras	0.20	11.36	-0.36	-65.31%
Chile	0.27	44.75	-0.55	-80.36%

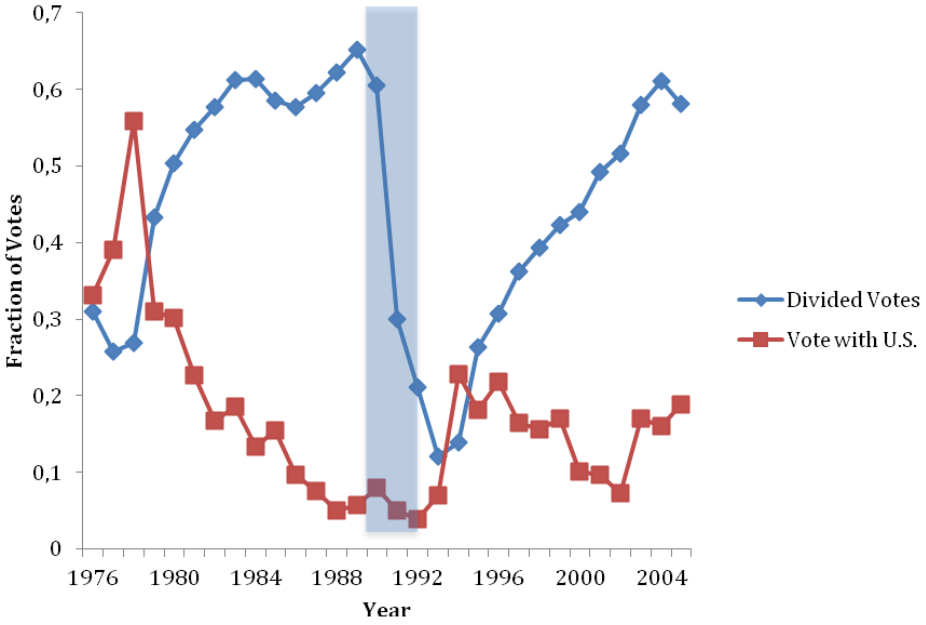


Figure 1. The fraction of divided votes and the fraction of countries voting in agreement with the U.S. in the UNGA.

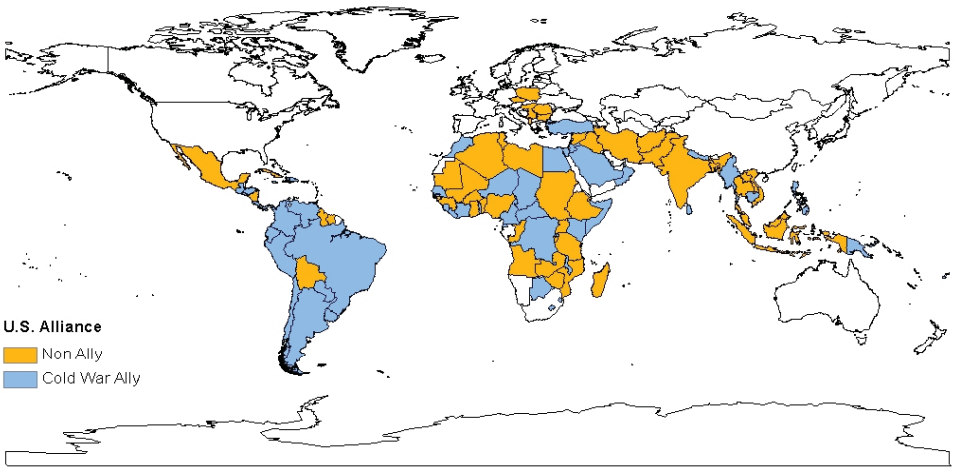


Figure 2A. U.S. Alliance during the Cold War.

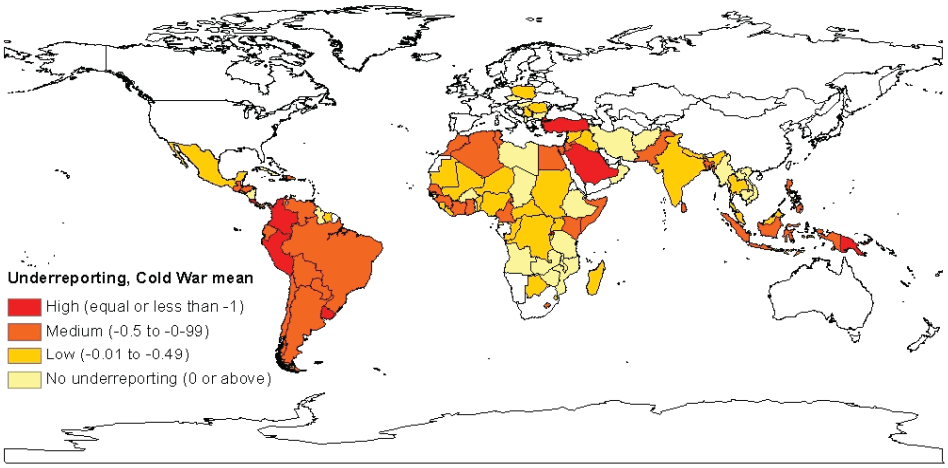


Figure 2B. U.S. State Department underreporting during the Cold War.

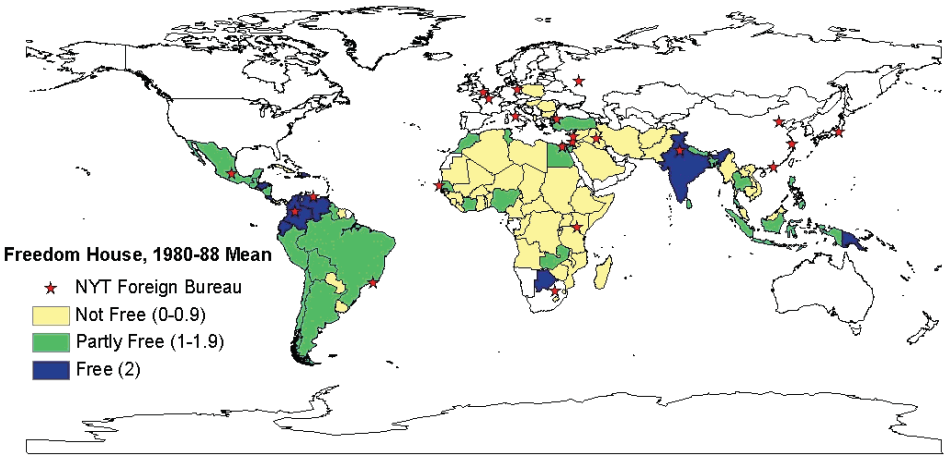


Figure 2C. NYT foreign office bureau locations and media freedom.

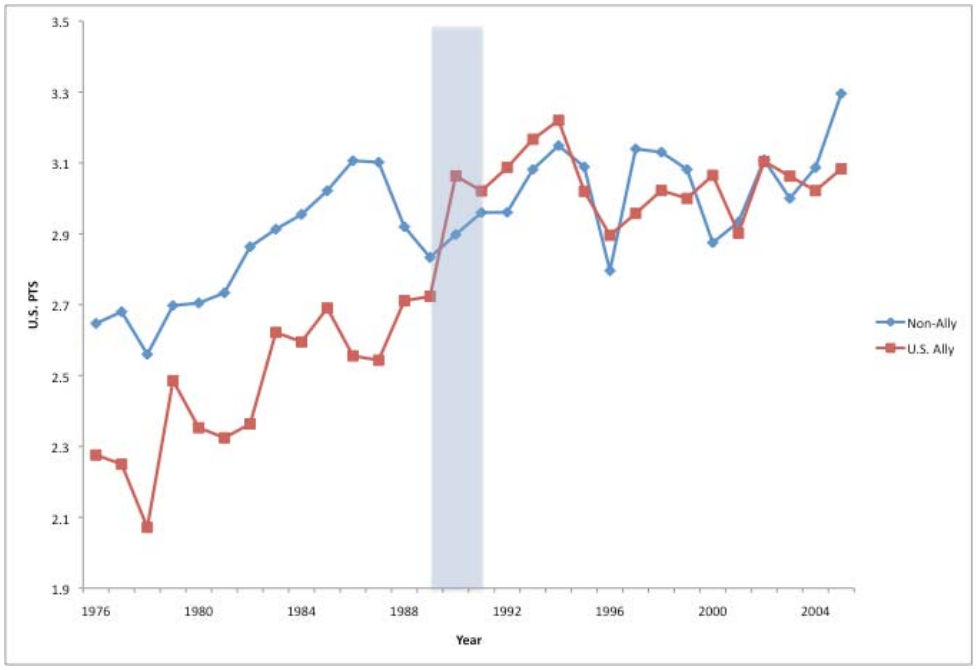


Figure 3A. U.S. Political Terror Score for U.S. allies and non-allies over time.

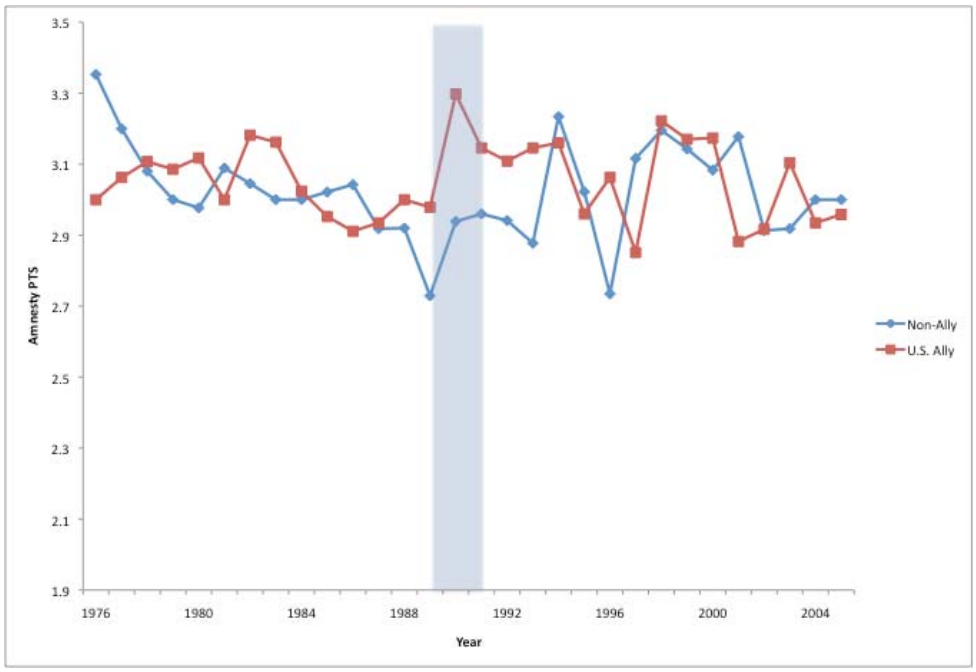


Figure 3B. Amnesty International Political Terror Score for U.S. allies and non-allies over time.

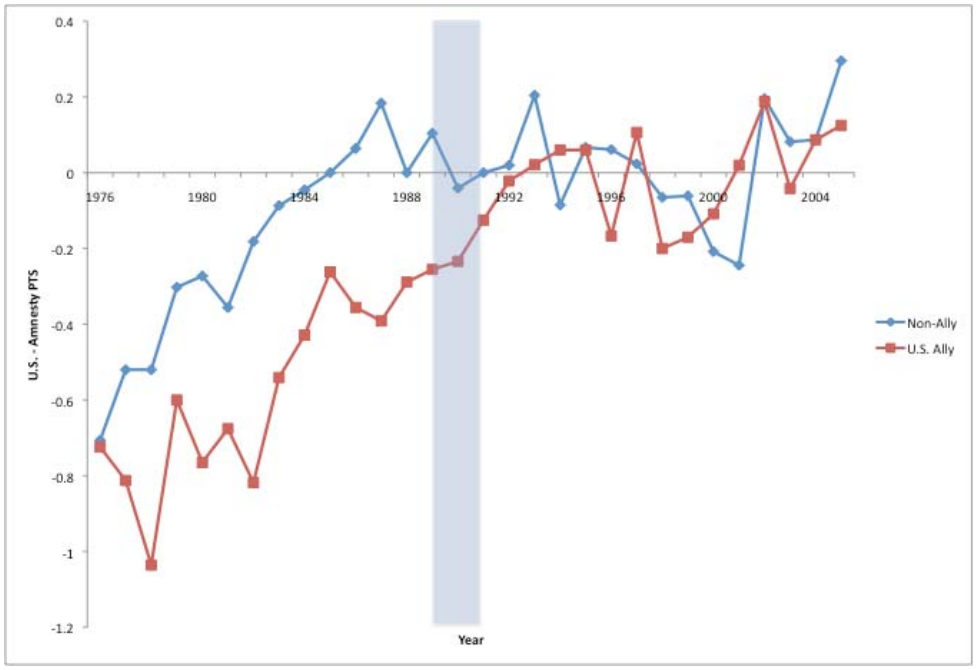


Figure 3C. The difference between U.S. State Department and Amnesty International in Political Terror Score for U.S. allies and non-allies.

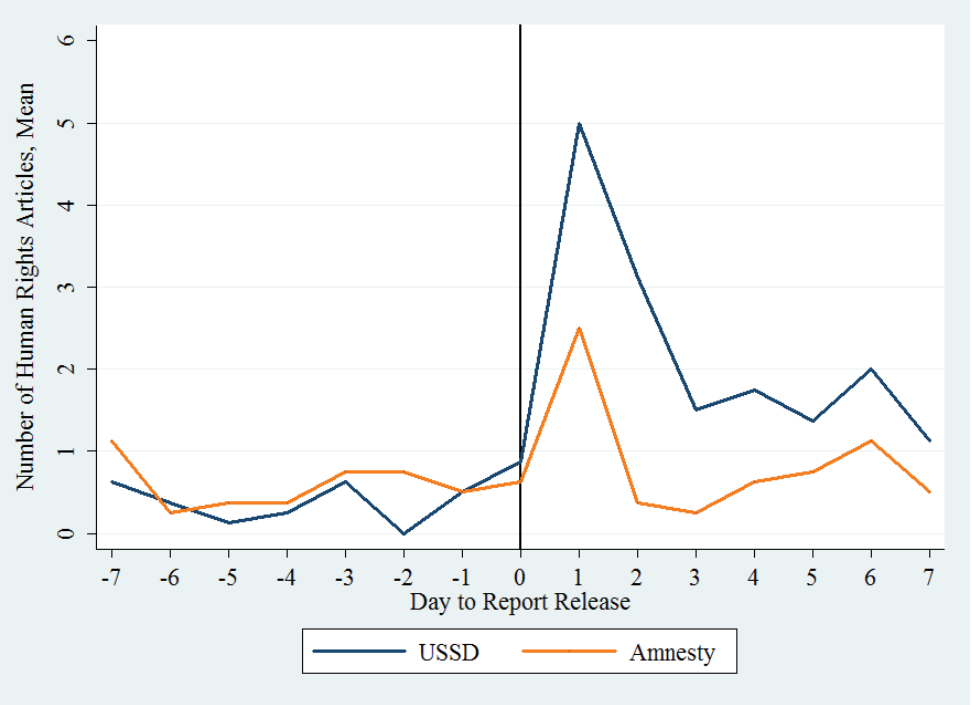


Figure 4. The number of news articles about human rights abuses in the week prior to and following report releases.

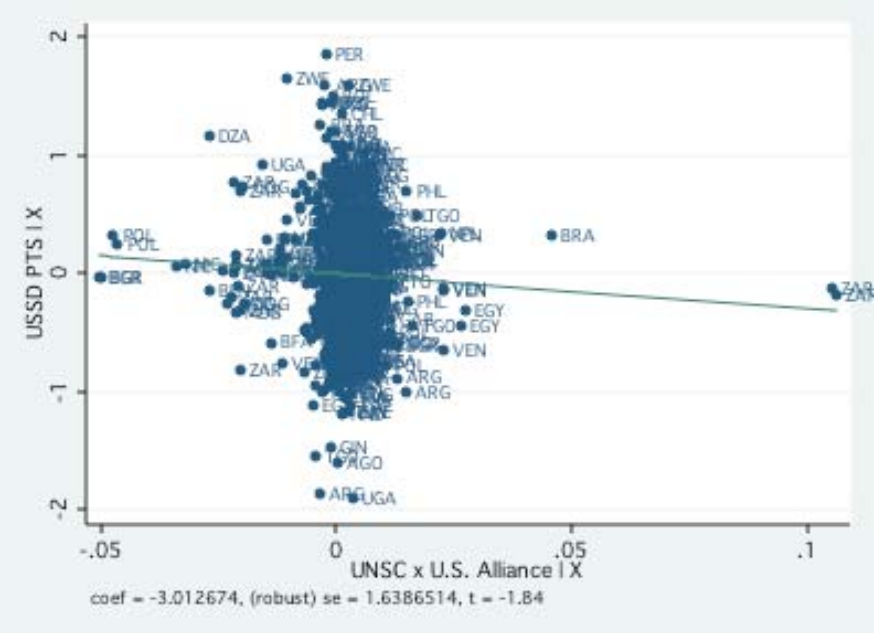


Figure 5A. Plot of residuals from the regression of USSD PTS on U.S. Alliance x UNSC (sample of countries that were ever on the Security Council).

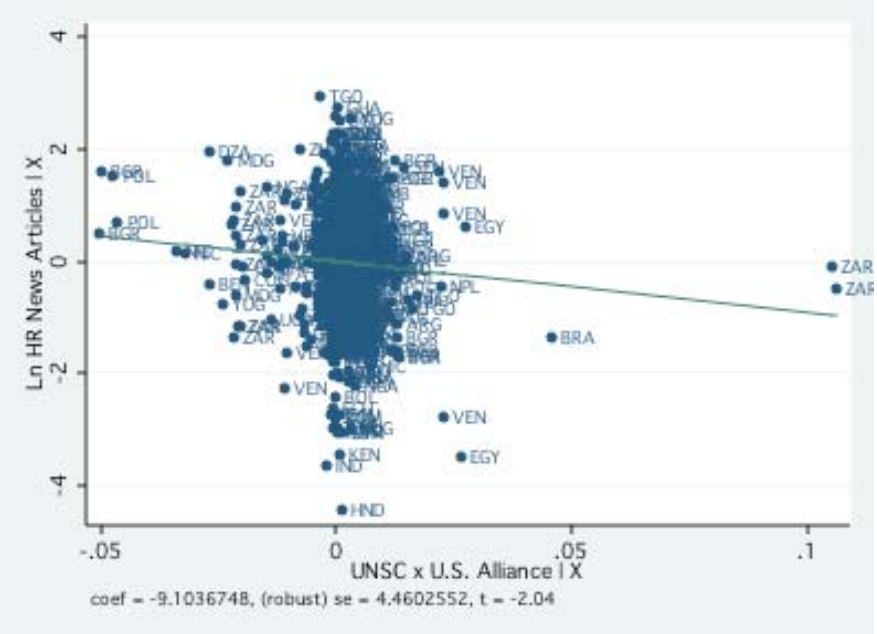
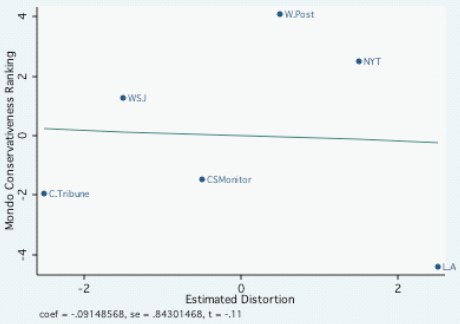
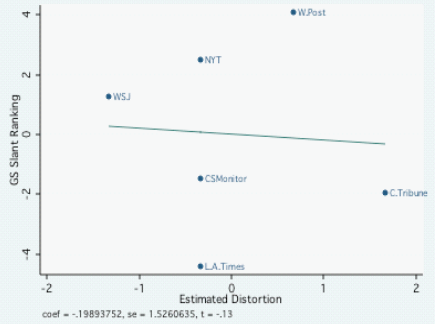


Figure 5B. Plot of residuals from the regression of log HR news articles on U.S. Alliance x UNSC (sample of countries that were ever on the Security Council).

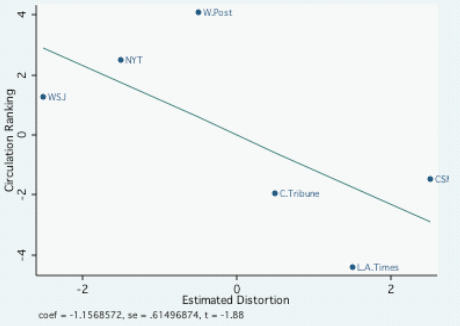
6A. Conservativeness, Mondo Rating



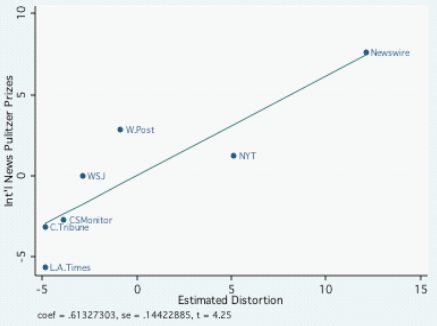
6B. Slant, Gentzkow & Shapiro



6C. Circulation Ranking



6D. Pulitzer Prize Ranking



Figures 6A-6D. Effect on news coverage and newspaper characteristics.

Cold War 1976-1988				Post Cold War 1992-2002			
year	Country	year	Country	year	Country	year	Country
1977	Benin	1984	Peru	1992	Zimbabwe	1999	Malaysia
1977	Venezuela, RB	1984	Nicaragua	1992	Ecuador	1999	Brazil
1977	India	1985	Thailand	1992	Venezuela, RB	2000	Malaysia
1977	Pakistan	1985	India	1992	India	2000	Bangladesh
1978	Venezuela, RB	1985	Peru	1992	Hungary	2000	Mali
1978	India	1985	Egypt, Arab Rep.	1993	Hungary	2000	Jamaica
1978	Nigeria	1985	Trinidad and Tobago	1993	Brazil	2000	Tunisia
1978	Bolivia	1985	Burkina Faso	1993	Venezuela, RB	2000	Argentina
1979	Bangladesh	1985	Madagascar	1993	Pakistan	2001	Bangladesh
1979	Gabon	1986	Congo, Rep.	1994	Djibouti	2001	Colombia
1979	Bolivia	1986	Ghana	1994	Oman	2001	Tunisia
1979	Zambia	1986	Madagascar	1994	Argentina	2001	Jamaica
1979	Nigeria	1986	Venezuela, RB	1994	Brazil	2002	Bulgaria
1980	Mexico	1986	Thailand	1994	Rwanda	2002	Guinea
1980	Zambia	1986	Trinidad and Tobago	1994	Pakistan	2002	Colombia
1980	Bangladesh	1986	Bulgaria	1994	Nigeria	2002	Cameroon
1980	Philippines	1987	Zambia	1995	Indonesia	2002	Mexico
1980	Tunisia	1987	Ghana	1995	Botswana	2002	Syrian Arab Republic
1981	Niger	1987	Congo, Rep.	1995	Honduras	2003	Guinea
1981	Tunisia	1987	Venezuela, RB	1995	Argentina	2003	Bulgaria
1981	Philippines	1987	Argentina	1995	Oman	2003	Angola
1981	Uganda	1987	Bulgaria	1995	Rwanda	2003	Cameroon
1981	Mexico	1988	Argentina	1995	Nigeria	2003	Chile
1982	Poland	1988	Nepal	1996	Indonesia	2003	Syrian Arab Republic
1982	Togo	1988	Senegal	1996	Egypt, Arab Rep.	2003	Pakistan
1982	Congo, Dem. Rep.	1988	Brazil	1996	Honduras	2003	Mexico
1982	Uganda	1988	Zambia	1996	Chile	2004	Romania
1983	Congo, Dem. Rep.	1988	Yugoslavia, Fed. Rep.	1996	Guinea-Bissau	2004	Angola
1983	Nicaragua	1988	Algeria	1996	Botswana	2004	Brazil
1983	Pakistan			1997	Kenya	2004	Pakistan
1983	Togo			1997	Chile	2004	Philippines
1983	Zimbabwe			1997	Egypt, Arab Rep.	2004	Algeria
1983	Poland			1997	Costa Rica	2004	Chile
1984	Burkina Faso			1998	Brazil	2005	Algeria
1984	India			1998	Kenya	2005	Brazil
1984	Egypt, Arab Rep.			1998	Gambia, The	2005	Romania
1984	Zimbabwe			1999	Gambia, The	2005	Philippines
1984	Pakistan			1999	Argentina		

Appendix Table A1. UN Security Council members 1976-88, 1992-2005.

Chapter 4

Propaganda and Conflict: Theory and Evidence from the Rwandan Genocide*

"The radio encouraged people to participate because it said 'the enemy is the Tutsi'. If the radio had not declared things, people would not have gone into the attacks."

-Genocide perpetrator, interviewed by Straus (2007)

1 Introduction

Among all historical episodes of civil conflict, the 1994 Rwanda Genocide is an extraordinary event. During a period of only three months, a nation-wide extermination campaign led by the Rwandan government against the Tutsi ethnic minority population resulted in at least 500 000 Tutsi civilian deaths and a reduction by approximately 75% of the country's Tutsi population (des Forges, 1999).[1] In addition to the violence organized by the army and militias, the high intensity killings were achieved by mass participation by hundreds of thousand ethnic majority Hutu citizens using their machetes and clubs (des Forges, 1999; Straus, 2004; Verwimp,

* I thank Robert H. Bates, Ethan Kaplan, Masayuki Kudamatsu, Rocco Macchiavello, Nancy Qian, Rohini Pande, Torsten Persson, David Stromberg and, especially, Jakob Svensson for their comments, participants at Harvard KSG; Stanford SIEPR; LSE; Dartmouth; U Pompeu Fabra; Warwick; Center for Global Development; EUDN workshop at Oxford; Econometric Society EWM 09, and CIFAR IOG 09. I would also like to thank Giovanni Zambotti at the Center for Geographic Analysis at Harvard University for ArcGIS assistance. All mistakes are my own.

[1] There was also a significant amount of moderate Hutus that were killed. For discussions on the death tolls, see des Forges (1999), Verpoorten (2005), as well as Davenport and Stam's analysis at www.genodynamics.com (Available November 5 2009).

2006). Given the large-scale participation and the human lives lost, understanding the determinants of participation in the genocide is of great importance. The principal aim of this paper is to estimate the impact of one factor widely believed (BBC, 2003; Thompson, 2007) to have played a significant role in the genocide: propaganda spread by the infamous "hate radio" station Radio Télévision Libre des Mille Collines (RTLM).

In order to understand the determinants of participation in violence, and the mechanisms through which propaganda can affect participation, the paper first sets up a simple model of propaganda and participation in ethnic violence. The model adopts the global games framework (Carlsson and van Damme, 1993; Morris and Shin, 1998; 2005) and considers a situation where individuals face some uncertainty about the value of conflict, but may receive a noisy public signal about the value through the radio. The key insight of the model is that propaganda, defined as radio broadcasts signalling that the value of conflict is high, will affect participation through two mechanisms. First, by increasing the expected value of conflict, independent of how many others that participate. Second, and potentially more importantly, by changing the expectations individuals hold about whether others will participate. If there are strategic complements in violence, the second mechanism implies that propaganda will function as a coordination device and lead to large-scale increases in participation if a sufficiently large number of people receives the propaganda.

The predictions of the model are taken to a unique nation-wide village-level dataset that combines data from several sources. First, as a proxy for participation rate, the paper uses data on prosecution rates for violence during the genocide, provided by Rwanda's National Service of Gacaca Jurisdictions. Second, it uses information on locations and technical specifications of Radio RTLM transmitters, and produces a nation-wide radio coverage map at a 90 meter cell resolution. Using a digital map of village boundaries, the radio coverage of each village is then calculated. Additional data on village characteristics is collected from the 1991 Rwanda Census and the Africover database. The matched dataset contains data on 1105 villages.[2]

The identification strategy exploits arguably exogenous variation generated by Rwanda's highly varying topography consisting of hills and valleys. Using local *within-commune* village variation in radio coverage, the variation exploited will be

[2] The villages are formally called "administrative sectors". The term village is used for simplicity, highlighting that the units are relatively small. The median village area is 10.6 square kilometers.

due to whether there happen to be hills in the line-of-sight between radio transmitters and villages.[3]

Radio RTLM broadcasts had a substantial effect on violence. The estimates imply that going from no to full village radio coverage increased civilian violence by 65 percent and organized violence by 77 percent. Furthermore, the effects are entirely driven by villages where the Hutu ethnic majority was large relative to the Tutsi ethnic minority, and they are highly nonlinear in the degree of radio coverage as there is a sharp increase in violence when the radio coverage is sufficiently high. These results are consistent with the model under strategic complements, and suggest that the broadcasts were most effective when people knew that many other village members were also listening to the same broadcasts. Therefore, the propaganda appears to have functioned as a coordination device.

Moreover, and consistent with the model, the paper finds evidence that the ability to access independent information can mitigate the propaganda effects. In fact, there is no effect of radio coverage in villages in the upper literacy rate tertile, whereas the effects are large in villages in the lower literacy rate tertile. The results therefore suggest that the propaganda caused more violence because there was a lack of alternative information sources that could contest the content broadcasted by Radio RTLM.

To assess the extent to which the propaganda can explain the degree of violence in the genocide, the paper presents a simple counter-factual calculation. The results suggest that Radio RTLM caused approximately 9% of the genocidal violence, which corresponds to at least 45 000 Tutsi deaths.[4] Therefore, Radio RTLM was a quantitatively important causal factor in the genocide.

This project is related and adds to several strands of literature. First, it contributes to the literature on the determinants of the genocide (Verwimp, 2005, 2006; Straus, 2007), by presenting novel evidence on the causal effects of Radio RTLM.

Second, the Rwanda genocide may be extraordinarily grim, but it forms part of the wider phenomenon of civil war and conflict. Since 1960, one third of all nations has experienced civil war and one fifth has seen episodes of more than 10 years of civil war (for an overview, see Blattman and Miguel, 2009). Cross-country studies (Collier and Hoeffler 1998, 2004; Fearon and Laitin 2003; Miguel et al. 2004;

[3] The use of this method to examine media effects in the social sciences is not new. Olken (2009) employs a closely related but not identical approach in his study of the effects of television and radio on social capital in Indonesia.

[4] This is substantial considering that the radio signal was only receivable in about 19 percent of the country.

Besley and Persson 2008) have focused on the macro determinants of conflict onset, incidence and duration. There is also a small but growing literature which has used within-country regional data to identify factors determining the intensity of civil violence (e.g., Murshed and Gates, 2005; Dube and Vargas ,2007; Do and Iyer, 2007; Jha 2008). By presenting robust micro evidence on the role of information and beliefs, this paper adds an important piece to the understanding of why people participate in civil war and conflict, as well as how ethnic mobilization is achieved (e.g., Bates, 1986; Fearon and Laitin, 1996). In their overview of the literature, Blattman and Miguel (2009) conclude that the existing theory is incomplete. They argue that although the individual participation choice should be a natural starting point for the analysis of civil conflict, the literature lacks an understanding of the roots of individual participation. The work-horse model used to study determinants of group violence (including ethnic) is the contest model (Haavelmo, 1954; Hirshleifer, 1988). By assuming unitary groups, the contest model therefore typically ignores the participation problem at the individual level. In addition, Blattman and Miguel argue that theories seldom specify the empirical predictions that can test between competing accounts, and there is a lack of studies with convincing econometric identification. The model proposed in this paper analyzes the individual participation choice, and delivers predictions that allow the data to disentangle whether participation in ethnic violence is subject to strategic complements or not. A contribution of the paper, in addition to estimating the causal effects of Radio RTLM on participation in the genocide, is therefore to shed some light on the mechanisms driving ethnic violence. Specifically, the empirical results are consistent with strategic complements in violence at the village level, and inconsistent with no strategic interactions.[5] To the best of the author's knowledge, this is a novel finding.

Finally, the paper adds to the literature on media effects (for an overview, see Della Vigna and Gentzkow, 2009). Theoretically, self-interested politicians may supply biased mass media in order to reduce the likelihood of regime change (Edmond, 2009) as well as to induce hatred (Glaeser, 2005). The empirical effects of mass media on political behavior have been studied at least since Lazarfeld et al. (1954). A recent literature has found significant effects. This includes effects on voting behavior (Gentzkow, 2006; Della Vigna and Kaplan, 2007; Chang and Knight, 2008;

[5] Under strategic interactions and complete information, multiple equilibria are typically present. However, under incomplete information (Carlsson and van Damme, 1993; Morris and Shin, 1998; 2005), there is a unique equilibrium that makes it possible to derive testable predictions. Note that although the paper does not focus on strategic substitutes, the set of results are inconsistent with strategic substitutes in violence (derivations can be shown upon request).

Enikolopov et al., 2008; Gerber et al., 2009); accountability and policy (Besley and Burgess, 2002; Strömberg, 2004; Eisensee and Strömberg, 2005); political knowledge and beliefs (Gentzkow and Shapiro, 2004; Snyder and Strömberg, 2008); and social capital (Paluck, 2009; Olken, 2009). This paper adds to the literature by presenting novel evidence showing that mass media can persuade individuals into what is arguably the most extreme political acts of them all: killing members of the political opposition.

Below, section 2 provides the background to the genocide and Radio RTLM; section 3 presents the model and derives empirical predictions; section 4 explains the data and the empirical strategy; section 5 presents the results; and section 6 concludes the paper.

2 Background

This section provides a brief background in order to understand the pre-existing political tensions leading up to the genocide, as well as the structure and content of Radio RTLM broadcasts.

2.1 Political and ethnic tensions

After World War I, Belgium took control of Rwanda (previously a German colony) on a mandate by the League of Nations. The Belgian rule reinforced pre-existing ethnic cleavages by a range of policies favoring the ethnic minority Tutsi group (Prunier, 1995). However, with the "Hutu Revolution" and the independence from Belgium in 1962, there was a complete reversal of power. After 1962, Rwanda became a Hutu-dominated one-party state.

In connection with the independence, there were several episodes of ethnic violence between the two ethnic groups that led to several hundreds of thousand ethnic Tutsi refugees in neighboring countries (Prunier, 1995). A period of relative stability followed but in 1973, there was more violence as ethnic clashes between Hutus and Tutsis in Burundi spilled over into Rwanda. The unrest eventually led to the young Hutu military leader Juvénal Habyarimana seizing power in a coup in 1973.

In October 1990, a rebel army invaded Rwanda from Uganda. The rebels, of the Rwandan Patriotic Front (RPF), represented the refugees that had fled during the Hutu Revolution and demanded an end to the ethnically unbalanced policies. Internationally, they presented themselves as a democratic multi-ethnic movement

trying to overthrow a corrupt regime.[6]

In April 1992, a transitional multi-party government was formed. After periods of negotiations and unrest, a peace agreement was finally signed in Arusha in August 1993. With sparse resources and a weak mandate, United Nations' peacekeeping forces were to facilitate the installation of the transitional government. After periods of violence, unrest, and postponed installations, the Hutu president Habyarimana was assassinated when his jet was shot down on April 6th 1994. Within days, extremists within Hutu-dominated political parties managed to take over key positions of government, and an ethnic cleansing campaign spread throughout the country shortly thereafter.

The branches of government took an active role in the killings, from Presidential Guards, the regular army FAR, national gendarmes, via the civil administration down to the mobilization and supply of resources to the Interahamwe and Impuzamugambi militias (Prunier, 1995). In addition, there was large-scale civilian participation as several hundreds of thousand citizens participated in the attacks (Straus, 2004).

The genocide ended in late July 1994 when the Tutsi RPF rebels defeated the Rwandan army and militia groups, and managed to seize the capital Kigali. At that point, at least 500 000 Tutsis had been killed (des Forges, 1999).

2.2 Media and Radio RTLM

Radio RTLM started broadcasting in July 1993. The station was set up as a private company by a group of Hutu politicians, but with strong support from President Habyarimana (Thompson, 2007). The broadcasts continued throughout the genocide, and did not end until RPF rebels managed to take control of the country in mid-July.

Two radio transmitters were installed. One 100 watt transmitter was placed in Kigali, the capital, and another 1000 watt transmitter was placed on Mount Muhe, one of the country's highest mountains. Compared to the only other national radio station in the country, government owned Radio Rwanda, RTLM, quickly became popular by airing western-style talk shows and playing the latest music, especially popular Congolese songs.[7]

[6] The rebel army of about four thousand well-trained troops mainly consisted of second-generation Rwandan refugees. They had gained military experience from Uganda's National Resistance Army which seized power in Uganda in 1986.

[7] There was also a station owned by the Tutsi RPF rebels, Radio Muhabura, that broadcast

Importantly, the radio station called for the extermination of the Tutsi ethnic group and claimed that preemptive violence against the Tutsi population was a necessary response of "self-defense" (ICTR, 2003; Thompson, 2007).[8] In her study of RTLM airtime content, Kimani (2007) reports that the most common inflammatory statements consisted of 1) Reports of Tutsi RPF rebel atrocities (33%); 2) Allegations that Tutsis in the region were involved in the war or a conspiracy (24%); and 3) Allegations that RPF wanted power and control over Hutus (16%).

Although the radio station systematically called upon Hutus to be aware of Tutsi plots and forthcoming attacks, it is still unclear to what extent Hutu citizens believed in the RTLM broadcasts and viewed them as informative about the ongoing conflict between Hutus and Tutsis, and to what extent citizens discredited the broadcasts as being biased. However, the fact that there was a demand for the broadcasts suggests that citizens at least viewed the broadcasts as bringing important information. For example, Des Forges described the high demand for RTLM as "people listened to the radio all the time, and people who didn't have radios went to someone else's house to listen to the radio. I remember one witness describing how in part of Rwanda, it was difficult to receive RTLM, and so he had to climb up on the roof of his house in order to get a clear signal, and he would stand up there on the roof of his house with his radio to his ear listening to it".[9]

Furthermore, as alternative information sources were limited in the rural areas, it was arguably difficult to verify the content in the broadcasts. Alternative media sources did exist. In particular, the number of independent newspapers, including political opposition newspapers, at the time of the genocide was between 30 to 60 (Alexis and Mpambara, 2003; Higiro, 2005). However, the circulation and readership of these newspapers in the rural areas was naturally limited due to relatively low literacy rates in the country.[10] Therefore, the radio became the sole source of news for most people (des Forges, 1999).

---

into Rwandan territories from Uganda.

[8] A common definition of propaganda is "the spreading of ideas, information, or rumor for the purpose of helping or injuring an institution, a cause, or a person". <www.merriam-webster.com/dictionary/> (Available November 15 2009)

[9] Interview with Alison des Forges, available (November 16 2009) at <www.carleton.ca/jmc/mediagenocide>

[10] The literacy rate was 66 percent (des Forges, 1999).

3 A Model of Ethnic Violence

Given their content, it is quite clear that one of the main motives for the RTLM broadcasts was to affect the beliefs among the Hutu population that a nondiscriminatory, preemptive, attack against conspiring Tutsis was the appropriate course of action. We now turn to a simple model that allows us to analyze how these broadcasts might have affected the beliefs among the Hutu population, and how it could have influenced the level of violence in Rwandan villages. Albeit relatively simple, the model sheds some light on some interesting channels through which propaganda might translate into violence. Most importantly, the model delivers a set of testable predictions that will be taken to the data in the subsequent sections of the paper.

We proceed in several steps. First, we explain the basic setup and second, we find the equilibrium and show how it can be affected by propaganda. Third, we present the empirical predictions that will be taken to the data.

3.1 Basic setup

Consider a village with a continuum of individuals, where each individual is a member of one of two ethnic groups, ethnic majority group H and ethnic minority group T. The population size of group H is normalized to 1, and the size of group T in the village is t. The analysis focuses on the discrete decision by group H members to participate in an attack against minority group T in the village. Strategic behavior by minority group members is not studied in order to keep things simple. Therefore, in what follows we exclusively focus on the behavior of group H members.

The payoff from participating in the attack depends on some fundamental value, θ, which is possibly negative. We may consider θ as the net benefit that depends on a range of factors independently of how many other group H members participate in the attack, as well as the size of group T. For example, factors determining θ could be the amount of wealth of group H, the opportunity cost of attacking group T, or the value associated with being the first side to attack the opposite group.

In addition to the fundamental value, we allow the payoff from participating in violence to exhibit strategic complements. Under strategic complements, the payoff depends positively on how many other members of group H that participate in the attack, h. Violence is a dangerous and costly activity, and there are good reasons to think that there exist strategic complements in violence. For example, the larger is the group attacking, the smaller is the likelihood of being injured, or the shorter is the duration of fighting required for success.

Therefore, we allow the payoff from having more members participate in the attack h to depend on the (relative) size of the defending group, t. Specifically, to get a convenient formalization, let the payoff structure be the following

$$u = \begin{cases} \theta + \alpha\frac{h}{t} & \text{if the member participates in the attack} \\ 0 & \text{if the member does not participate in the attack} \end{cases}$$

If there are strategic complements in violence, $\alpha > 0$. When there are no strategic interactions, $\alpha = 0$.[11] We are interested in the equilibrium number of ethnic majority members participating in the attack, h, and how h can be affected by propaganda.

3.2 Information and beliefs

In reality, participating in conflict is a risky project. We formalize this by assuming that members face uncertainty about the fundamental value of participating in violence, such that there is incomplete information about θ. It is reasonable to believe that θ cannot be known with complete certainty in most cases of violent conflict. In this section, we describe how members form their beliefs about θ.

Following the literature on global games, members do not observe θ but receive information about the value that allows them to form beliefs. We make the standard assumption that members have a diffuse prior distribution of θ on the real line. Each member i observes an independent private signal $x_i = \theta + \varepsilon_i$, where ε_i is independently and normally distributed with mean zero and variance σ_x^2. We can consider x_i as all independent private information a member has from different sources that are relevant for the fundamental value of conflict. Furthermore, we can consider a lower σ_x representing having access to multiple sources of information, or access to information sources of high quality.

Furthermore, the radio broadcasts a signal p about the value of θ. A fraction r of the village population has radio coverage. Having radio coverage implies receiving the signal p. For simplicity, we do not consider strategic behavior on behalf of whomever sends out the radio signal. Instead, agents view the signal p as informative about the underlying fundamental value of conflict, θ. The signal has the structure

[11] We will derive predictions under zero strategic complements, and positive strategic complements. One could also, in principle, allow for strategic substitutes such that members are less willing to participate if other are participating. It is worth noting that, except for the main effect of radio coverage on participation, the set of auxiliary results are inconsistent with strategic substitutes (derivations and predictions not shown).

$p = \theta + b$. To keep the analysis simple, we assume that b is exogenous, unobservable, and distributed normally with mean zero and variance σ_p^2.[12] Key to the model is that the radio signal is a *public* signal among members with radio, i.e. there is common knowledge about the radio signal among majority members with radio. Therefore, a member with radio will not only use the signal to update his belief about θ, he also knows that a fraction r of the other village members listens to the radio and receives signal p, and everybody with radio knows that everybody else with radio knows this, and everybody knows that everybody knows... ad infinitum. Individuals without radio access do not receive the public signal. To focus on the choices of majority members that receive the radio broadcasts and keep the analysis tractable, we make the simplifying assumption that members without radio are unaware of others receiving the radio signal.[13]

To make the problem interesting and realistic, we make the following assumption

$$Assumption\ 1: \ \frac{\alpha}{2t} + \theta < 0,$$

which will ensure that a minority of the members that do not receive the public signal p will participate (In the data used in the empirical section, this is always true).

Individuals use Bayes' rule to update their beliefs about the fundamental value of violence. Consider first a member without radio. The private posterior distribution for member i that receives private signal x_i is normally distributed with mean $\bar{\theta}_i^N = x_i$ and variance σ_x^2. For members with radio, the posterior expectation of θ given public information alone is normal with mean[14]

$$\bar{\theta}_i^R = \frac{\sigma_x^2 p + \sigma_p^2 x_i}{\sigma_p^2 + \sigma_x^2}.$$

[12] The key assumption about p is that σ_p^2 is finite, so that the broadcasts are informative. The zero mean is not a binding assumption, as one can easily add a known constant to shift the distribution. If the radio signal is biased on average then, individuals will adjust for this when they form beliefs about θ. However, treating the signal as exogenous and without manipulation is clearly an unrealistic simplification, made to keep the analysis simple. For a model with endogenous information manipulation in a civil war context, see Edmond (2009).

[13] The key assumption is that a fraction $1 - r$ of the members do not receive the signal p. One could in principle allow $1 - r$ members to not receive the signal p, but still be aware of the distribution of p, and that some fraction r receives the signal p. This would complicate the analysis, but would most likely not change the main results.

[14] The posterior variance is $\frac{\sigma_x^2 \sigma_p^2}{\sigma_x^2 + \sigma_p^2}$.

3.3 Equilibrium

We are interested in the equilibrium level of participation, h. Consider a strategy where each member follows a simple switching rule

$$a(\bar{\theta}_i^j) \;=\; \begin{cases} \text{participate} & \text{if } \bar{\theta}_i^j \geq \kappa^j \\ \text{do not participate} & \text{if } \bar{\theta}_i^j < \kappa^j \end{cases}$$

where $j = N$ labels the strategy for members without radio and $j = R$ for members with radio. That is, members participate if and only if their beliefs about the fundamental value of violence are sufficiently high, above some threshold κ^j. Following Morris and Shin (1998, 2005), this strategy is unique under some regularity conditions (see the web appendix for the regularity conditions and the derivation of the equilibrium).[15] For members without radio coverage, the Bayes-Nash equilibrium threshold κ^N is

$$\kappa^N = -\frac{\alpha}{2t}. \tag{4.1}$$

For members with radio coverage, the equilibrium participation threshold κ^R is the solution to the equilibrium condition

$$\kappa^R + \frac{\alpha}{t}\left(r\Phi\left[\gamma(p-\kappa^R)\sigma_x^2/\sigma_y^2\right] + (1-r)\Phi\left[\gamma\left(\frac{\alpha}{2t}+\kappa^R\right)\right]\right) = 0, \tag{4.2}$$

where $\gamma \equiv (2\sigma_x^2\sigma_p^2+\sigma_x^4)^{-1/2}(\sigma_x^2+\sigma_p^2)^{1/2}$. The intuition behind equation 1 is relatively straightforward. A member without radio coverage faces two forms of uncertainty. First, there is uncertainty about θ and second, there is also uncertainty about how many others that will participate, h. This is because given the switching strategy, since the member is uncertain about θ, he is also not certain about how many other members have expectations of θ above the threshold κ^N. However, since he has independent information about θ, he forms beliefs about the distribution of θ. In turn, this means that he holds beliefs about how many other members are likely to hold expectations of θ above the participation threshold, κ^N. The higher is the expectation of a member of the value of conflict, $\bar{\theta}_i^N$, the more other members he expects to participate. The equilibrium condition of equation 1 pins down the expectation $\bar{\theta}_i^N$ where a member is indifferent between participating and not participating. Importantly, since members without radio do not receive the radio signal p and are also unaware of the existence of the broadcasts, p and r do not change the participation

[15] The regularity conditions require that α is bounded from above. The exact bound can be found in the appendix.

threshold whereby members are willing to participate.

The intuition behind equation 2 follows a similar logic. However, the important distinction between a member with radio coverage and a member without radio coverage is two-fold. First, a member with radio receives the additional signal p about the value of conflict θ. This will cause him to update his beliefs $\bar{\theta}_i^R$ by the same logic as in equation 1. Second, and most importantly, due to the publicity of the signal, he knows that everybody else with radio coverage has also received the same signal p.[16] This is important because it will change his beliefs about how likely it is that other members with radio will participate, h. For this reason, the fraction r that has received the broadcasts is therefore a key variable in his decision of whether to participate. When r is low, he knows that not too many have received p, so he reasons similarly as someone without radio. When r is high, however, he knows that most members have also received p**,** which can dramatically change his expectations about how others will behave, and can thus change his own willingness to participate. Therefore, the fraction of the population with radio coverage, r, is a key variable for the equilibrium participation in violence.

3.4 Participation

Having pinned down the equilibrium thresholds, κ^N and κ^R, we can investigate the equilibrium participation, h. Given a fundamental value of violence θ, we can calculate the proportion of non-radio members with beliefs $\bar{\theta}_i^N \geq \kappa^N$, given by equation 1, and the proportion of radio members with beliefs $\bar{\theta}_i^R \geq \kappa^R$, given by equation 2. Using the distributions for the private signal and the radio signal, conditional on θ, the total share of the majority population participating that is a function of village radio coverage r

$$h = rh^R + (1-r)h^N \ , \tag{4.3}$$

where h^N is the proportion of members without radio coverage participating

$$h^N = \Phi\left(\frac{\frac{\alpha}{2t} + \theta}{\sigma_x}\right) \ , \tag{4.4}$$

[16] He also knows that everybody with radio knows that everybody with radio knows this, and that everybody... ad infinitum.

and h^R is the proportion of members with radio coverage participating

$$h^R = \Phi\left[\frac{\frac{\sigma_x^2}{\sigma_p^2}p + \theta - \frac{\sigma_x^2+\sigma_p^2}{\sigma_p^2}\kappa^R}{\sigma_x}\right] . \tag{4.5}$$

Lemma 1 *The participation rate increases with radio coverage ($\partial h/\partial r > 0$, for all r) only if radio broadcasts a signal that the fundamental value is sufficiently high ($p > \tilde{p} \equiv -\frac{\alpha}{2t}$). Defining propaganda as a signal that the value of conflict is high ($p > \tilde{p}$), increasing radio coverage affects participation through two propaganda effects. First, through a direct "fundamentals effect" that changes the share of the population with beliefs about the value of conflict above the equilibrium participation threshold, κ^R. Second, through an indirect "strategic effect" that affects the expectations held by individuals about how many other individuals that will participate, which changes the equilibrium participation threshold κ^R.*

Proof: see the appendix. The equilibrium implies that members only participate if their beliefs about the fundamental value of conflict are sufficiently high. Given participation thresholds for radio members and no-radio members, only if the radio broadcasts that the fundamental value of conflict is sufficiently high (above the participation thresholds) will a larger fraction of the members with radio hold expectations of the fundamental value of conflict above the participation threshold. This is the *fundamentals effect*.

Furthermore, due to the radio signal being public, members with radio know that everybody with radio listens to the same broadcasts. When there is an increase in the radio coverage, members with radio realize that more people now hold high expectations of the fundamental value of conflict, which for each member with radio increases the expected number of participants. Under strategic complements, this will in turn change the equilibrium participation threshold κ^R whereby somebody with radio is willing to participate. This is the *strategic effect*. Under strategic complements, the total payoff of participation in conflict is always higher the more people that participate. Therefore, when radio coverage increases, each member with radio expects more people to participate, which makes each member with radio more willing to participate by lowering the participation threshold. Individuals therefore participate at lower beliefs about the fundamental value of conflict when radio coverage is high as compared to when it is low.

Next, we derive the properties of participation in violence in the two possible cases: no strategic interactions, and strategic complements. Since we are interested

in how propaganda may increase participation, from now on we assume that $p > \tilde{p}$.[17]

Benchmark case: $a = 0$

We first state the properties for the benchmark case when α is zero and participation in violence is free from any strategic interactions.

Proposition 2 *($\alpha = 0$) : If there are no strategic interactions, then participation increases linearly in radio coverage ($\partial h/\partial r = \bar{c} > 0$) and the effect is the same regardless of the size of the ethnic minority ($\partial h/\partial r \partial t = 0$).*

Proof: see the appendix. The intuition behind this result is relatively straightforward. When there are no strategic components, the individual choice of participation does not depend on how many others that participate. Instead, a member participates if his expectation of the fundamental value of participation is positive. Therefore, radio coverage only affects participation through the *fundamentals effect*. As the fraction holding positive expectations of the value of conflict is constant within the group of members with radio coverage, the fundamentals effect of radio coverage is linear and positive.

Strategic complements case: $a > 0$

Next, consider the case when α is positive and participation in violence is subject to strategic complements.

Proposition 3 *($\alpha > 0$) : If participation is subject to strategic complements, then radio coverage exhibits increasing scale effects ($\partial^2 h/\partial r^2 > 0$ for $r \in [0, \tilde{r}]$, and $\partial^2 h/\partial r^2 < 0$ for $r \in (\tilde{r}, 1]$, where $0 < \tilde{r} \leq 1$). Furthermore, the effect of radio coverage is decreasing in the size of the ethnic minority, as long as $h^R < 1/2$ ($\partial h/\partial r \partial t < 0$ for all r, as long as $h^R < 1/2$). If $h^R \geq 1/2$, the sign of $\partial h/\partial r \partial t$ is ambiguous.*

Proof: see the appendix. The reason why radio coverage exhibits increasing scale effects under strategic complements is due to the combination of the *fundamentals effect* and the *strategic effect.* In particular, both effects are positive. As radio coverage increases, the fundamentals effect implies that more members with radio will hold beliefs about the fundamental value of conflict above the participation threshold, which increases participation. In addition, when radio coverage

[17] Since the focus of this paper is when $p > \tilde{p}$, results are not presented for $p < \tilde{p}$. It is worth noting that in general, the results naturally go in the opposite directions when $p < \tilde{p}$.

increases, the strategic effect implies that members with radio expect more people to participate which, in turn, lowers the equilibrium participation threshold, an effect which further increases participation.

Figure 1A graphically shows equation 3 after solving equations 2, 4 and 5. The figure shows how the participation rate changes as a function of radio coverage, for the benchmark case and three different levels of strategic complements.[18] To clearly see the importance of the strategic effects, the parameter values are set such that the fundamentals effect of radio coverage is essentially zero (i.e. very small and positive). We see that although the fundamentals effects are essentially zero (so that almost no members believe that the fundamental value is sufficiently high for participation), there are important positive strategic effects when radio coverage is sufficiently high. The main insight is that the effects of radio coverage can be highly non-linear. The intuition behind this result is that at low levels of radio coverage, most members with radio do not expect many others to participate since only a small fraction of the population has received the radio broadcasts. At high levels of radio coverage, however, members with radio know that many have received the radio broadcasts and therefore, they expect many others to participate. Consequently, due to these strategic effects, increasing the radio coverage to high levels of radio coverage can have dramatic effects on participation.

Furthermore, the effect of radio coverage on participation depends considerably on the size of the ethnic minority group. Figure 1B graphically shows the effect of radio coverage for two different levels of ethnic minority size (keeping the other parameter values the same as in Figure 1A). When the size of the ethnic minority is relatively small ($t = 1/4$), there is a strong and positive strategic effect of radio coverage. However, when the size of the ethnic minority is relatively large ($t = 2/5$), the effect of radio coverage almost completely disappears as there is only a small increase in participation at very high levels of radio coverage. The reason is relatively straightforward, since the marginal benefit of more participants is lower when the ethnic minority is large. Therefore, even at high levels of radio coverage, most members with radio coverage do not expect many others to participate and, consequently, not many members are willing to participate.

[18] The other parameter values are: $p = 0$, $t = 1/4$, $\theta = -1$, and the variances of private information ($\sigma_x = 0.05$) and radio information ($\sigma_p = 0.1$) are set such that the conditions for a unique equilibrium are satisfied.

Independent information

In this section, we investigate how the effects of radio coverage are related to the access to independent information, σ_x. First, even though each member does not know the exact fundamental value of conflict, he uses his independent information to form expectations about it.[19] . Therefore, the effect of radio coverage will crucially depend on how much independent information members have.

Proposition 4 *When members have sufficiently good access to independent information ($\sigma_x \to 0$), the effect of radio coverage disappears ($\partial h/\partial r \to 0$).*

Proof: see the appendix. Intuitively, the expectation a member holds about the value of conflict, θ, will be a weighted average between independent information, x_i, and the information broadcast on the radio, p. The better independent information about the fundamental value of conflict that members have, the less weight will be put on the radio broadcasts. Therefore, when members have very precise expectations about the fundamental value of conflict through other information sources, they stop believing in the radio broadcast. Consequently, propaganda will not affect participation in the violence in that case.

3.5 Empirical predictions

We now summarize the results from the previous section into testable predictions.[20] Lemma 1 and Propositions 1 to 4 imply the following predictions:

1. **Main Effects:** If radio coverage r increases the participation rate h, then radio broadcasts a signal that the fundamental value of conflict was high, $p > \tilde{p}$. This prediction follows from Lemma 1.

Moreover, if $p > \tilde{p}$, then Propositions 1-4 imply:

2. **Ethnic Polarization:** The effect of radio coverage r on the participation rate is *decreasing* in ethnic polarization t, only with *strategic complements* in violence (Figure 1B).

[19] Recall that the independent private information is equal to $x_i = \theta + \varepsilon_i$, where ε_i is independently and normally distributed with mean zero and variance σ_x^2.

[20] We focus on the unambiguous effects derived in the previous section. That is, we assume that the additional conditions needed for the unambiguous effects are fulfilled. It is worth noting that the additional condition $h < 1/2$ is fulfilled in all observations in the data.

3. **Scale Effects of r:** Radio coverage r exhibits *increasing* scale effects on participation h, only with *strategic complements* (Figure 1A).

4. **Independent information**: Radio coverage r does not affect the participation rate h when ethnic majority members have sufficiently good access to independent information ($\sigma_x \to 0$).

Importantly, Predictions 2 and 3 imply that to the extent that we get consistent results, the data will allow us to disentangle whether α is zero or positive.

4 Data and Empirical Strategy

This section describes the data, identification strategy, and econometric specifications.

4.1 Measurement

The variables of interest are h, r, t, and σ_x. Here, we present how they are measured. Several sources of data are combined to construct a village-level cross-sectional dataset. Figure 2 shows a map of village boundaries in Rwanda. The final dataset consists of 1105 matched villages.[21]

Measuring the participation rate, *h*

Unfortunately, there is no dataset available that measures h directly. Instead, this paper uses an indirect measure from a nation-wide village-level dataset on prosecutions for violent crimes committed during the genocide. The data is provided from the government agency National Service of Gacaca Jurisdictions. The proxy used for the participation rate h is therefore the prosecution rate.[22]

The prosecution data for each village comes from local level Gacaca courts.[23] The national court system was set up in 2001 to process the hundreds of thousand of individuals accused of crimes committed during the genocide.

[21] The term village is used for simplicity, highlighting that the units are relatively small. The correct term is "administrative sector". The median administrative sector in the dataset is 10.6 square kilometers and has a population of 4336. There are some problems in matching data across data sources, see each section below.

[22] The data used for village population and ethnicity is described below.

[23] To see the laws governing the courts, see the National Service of Gacaca Jurisdictions homepage, http://www.inkiko-gacaca.gov.rw/En/EnLaw.htm (Available November 5 2009).

There are two violent crime categories. Category 1 includes prosecutions for organized violence, legally defined as:

- Planners, organizers, instigators, supervisors of the genocide.
- Leaders at the national, provincial or district level, within political parties, army, religious denominations or militia.

At the village level, these are typically prosecutions committed by local militias such as the Interahamwe and Impuzamugambi. Category 2 prosecutions concern civilian violence, defined as:

- Authors, coauthors, accomplices of deliberate homicides, or of serious attacks that caused someone's death.
- The person who – with the intention of killing – caused injuries or committed other serious violence, but without actually causing death.
- The person who committed criminal acts or became the accomplice of serious attacks, without the intention of causing death.

The data specifies the number of prosecutions for each village in Rwanda. In the sample, there are approximately 64 000 category 1 prosecution cases, and 362 000 category 2 cases. Unfortunately, there is no data available on ethnicity at the village level (it is available only at higher levels), only population numbers in 1991 (see below). The proxy used for the participation rate h is therefore the prosecution rate, measured as prosecutions per capita. Figures 6 and 7 show the prosecution rates in villages.[24]

Since we do not observe actual participation but prosecutions, and per capita rather than per Hutu, we have some measurement error in the dependent variable. This will not lead to any biased estimates unless the measurement error is correlated with the measured variation in radio coverage.

[24] White areas on the map indicate no data. This is either because of national parks or Lake Kivu (to the west), or because of matching problems. The data is matched on village names. There are two types of matching problems. First, names have changed across data sources. Second, two villages within communes sometimes have identical names.

Measuring radio coverage, r

The paper uses village-level data on predicted Radio RTLM coverage. The variable is constructed in several steps. First, it uses data on Radio RTLM transmitter locations and technical specifications, provided by the government agency Office Rwandais d'Information. Then, it predicts the radio coverage across the country by using digital topographic maps and radio propagation software developed by engineers.[25] The software (ArcGIS) uses an algorithm called ITM/Longley-Rice, which is typically used by radio and TV engineers assessing the signal strength of broadcasts. The software uses a digital topographic map of Rwanda, provided by Shuttle Radar Topography Mission (SRTM), and it lets the software run the ITM/Longley-Rice algorithm and predict the signal strength across the country. The software produces a radio coverage map at a 90 meter cell resolution, indicating whether each cell has radio coverage or not. Figure 4 shows the map of the radio coverage.[26]

Using the digital map of village boundaries, the measure of r is calculated as the share of the village area with coverage.[27] As there is no available dataset on Radio RTLM listening rates, the paper will estimate the reduced form effect of RTLM radio coverage on the participation rate.[28]

Measuring ethnic polarization, t

Population and ethnic data is retrieved from the Rwanda 1991 population census, provided by IPUMS International and GenoDynamics.[29] The GenoDynamics data is used for the population in each village. It does not contain any data on ethnicity. However, the 1991 census from IPUMS International reports the number of Tutsi and Hutu households in the commune. The ethnicity of the household is defined as the ethnicity of the household head. The data is only available at the commune

[25] The transmitter parameters are GPS position; transmitter height; transmission power; frequency; polarization.

[26] The software requires topography data in order to predict the radio signal. The digital map has complete topography data of Rwanda. However, the software runs into a missing data problem for a small section of villages in the very north and northeast, for signals radiating from the Mount Muhe antenna. This is because the radio signal needs to travel across Uganda in the north before reaching northeastern Rwanda. Therefore, the predicted radio signal is incorrect for those areas. The 205 villages affected by this data problem are dropped from the sample.

[27] As the measure is predicted radio coverage rather than actual radio coverage, there could be some random measurement error in the data. In that case, this will lead to attenuation bias and an underestimation of the true effects.

[28] The commune average radio ownership rate in the sample is 34%, taken from the 1991 Census. Radio ownership data is not available at the village level.

[29] The data is available at https://international.ipums.org/international/, (Available June 8 2008), and http://www.genodynamics.com/, (Available May 11 2009).

level, which is one administrative level above the village (i.e., administrative sector). The measure used for t is therefore the number of Tutsi households divided by the number of Hutu households in the commune.

Since there are two ethnic groups (98% of the population are either Hutu or Tutsi) where the Tutsi population is always in minority (the maximum t in the data is 0.44), this measure is equal to the commonly used measures of "ethnolinguistic fractionalization" and "ethnic polarization", up to a scalar (see Montalvo and Reynal-Querol, 2005). Therefore, we use t and *ethnic polarization* interchangeably.

Measuring access to independent information, σ_x

Ideally, we would want to test Prediction 4 directly through a measure of independent information (σ_x). But this is naturally unobservable to the researcher. Instead, we proxy for the access to independent information with the *ability* to access independent information, by exploiting variation in literacy rates and education.

Naturally, independent information can come from a range of sources. Within the context of the Rwanda genocide, newspapers are particularly relevant. In the years preceding the genocide, the independent press quickly expanded with multiparty politics and the legalization of opposition parties in June 1991. The number of independent newspapers that did not align with the government parties was between 30 to 60 during this period (Alexis and Mpambara, 2003; Higiro, 2005). Arguably, a necessary requirement for access to newspapers is literacy and basic primary education. In addition, Des Forges (1999) reports that, in practice, not only the literate would read the newspapers, but those who knew how to read were accustomed to reading newspapers to others.[30]

The data on literacy rates and primary education also comes from the 1991 Census provided by IPUMS International. For the literacy rate, the fraction of Hutu household heads that are literate is used. For primary education, the variable is the fraction of Hutu household heads that have some primary education.[31] Both variables are only available at the commune level.

[30] The model assumes that independent information is unbiased on average. However, since the newspapers in Rwanda were typically aligned with political parties, each newspaper most likely supplied biased information. This does not necessarily mean that the newspapers were biased on average. In fact, Mullainathan and Shleifer (2005) argue that with sufficient political divisions the information will on average be unbiased.

[31] The 1991 Census reports "last grade completed" for each household head. Since we would like to directly measure σ_x, but use the proxy variables, there is measurement error. This will also lead to attenuation bias if the error is classical.

Covariates

The SRTM topography data and ArcGIS software maps allow us to calculate the village mean altitude, the village variance in altitude, and the min and max altitude of the village, distance to the border, and village area. Using data from Africover, we can also measure the village centroid distance to the nearest major town and the distance to the nearest major road.

The summary statistics are presented in Table 1.

4.2 Identification strategy

Identifying the causal effects of radio coverage on the participation rate requires variation in radio coverage to be uncorrelated with all other determinants of participation. In the model, radio coverage is exogenous, while in reality the placement of the two RTLM transmitters was not random. One 100 watt transmitter was placed in the capital Kigali. The other transmitter (1000 watt) was placed on Mount Muhe in the northwestern part of the country.[32] The main endogeneity concern is that the transmitters could have been placed in areas with a high fundamental value of conflict θ, little independent information σ_x, or ethnic polarization t. The simple correlation between radio coverage and participation rate would then violate the identifying assumption. Importantly, since both θ and σ_x are unobservable, they cannot be controlled for in a regression.

The following identification strategy addresses the problem in steps.[33] Rwanda is a very hilly country without any really flat regions. Nick-named "The Land of the Thousand Hills", Figure 2 shows a map with the topography of Rwanda. There are literally hilltops and valleys everywhere in the country and the topographic variation shown in Figure 5 provides the basic foundation for the identification strategy. In particular, the main idea is to exploit variation in radio coverage due to hills in the line-of-sight between radio transmitters and villages *in between* radio transmitters and villages.

Radio propagation follows the laws of physics for electromagnetic propagation. Given transmitter height and power, the two main determinants of the signal strength

[32] The highest mountain, Mount Karisimbi, is right on the border to DR Congo and Uganda. Mount Muhe is the second highest mountain in the country, but the highest that is well within the country's border. Together with the Kigali transmitter, the placement is strongly suggested to have been driven by a maximizing of listeners.

[33] The strategy was pioneered by Olken (2009). The approach in this paper is similar but not identical to that of Olken.

are: distance to the transmitter; and whether the receiver is in the line-of-sight of the transmitter.[34] In free space, the power density of the radio signal decreases in the square distance from the transmitter. Since the transmitter may have been placed strategically, the distance to the transmitter most likely correlates with either θ or σ_x. The first step is therefore to control for a second-order polynomial in the distance to the transmitter.[35] This will leave variation in signal strength caused by variation in the line-of-sight between the transmitter and the receiver.

Whether the receiver is in the line-of-sight of a given transmitter will depend on two factors: the topography where the receiver is located (i.e. the higher the altitude of the receiver, the higher is the likelihood of its being in the line-of-sight) and the topography between the transmitter and the receiver. Since the topography of a village may be correlated with the other unobservable determinants of participation in conflict (θ and σ_x), it will be controlled for. The second step is therefore to control for the topography of the village. The control variables consist of a second-order polynomial in the mean altitude of the village and the altitude variance. This will leave variation in the radio coverage due to the topography between the transmitter and the receiver.

Since the two Radio RTLM transmitters may have been strategically placed in parts of the country with a certain kind of topography, the remaining variation (after controlling for the distance to the transmitter and the topography of the village) may still be correlated with θ and σ_x. Therefore, in order to control for broad regional differences in topography, the third and last step is to include *commune fixed effects*.[36] Therefore, the variation in radio coverage exploited for identification is a highly local variation across villages within communes.[37] This variation is arguably uncorrelated with other determinants of conflict, as radio coverage is determined by whether a hilltop randomly happens to be in the line-of-sight between the transmitter and the village.

Figure 6 shows graphically the topography and radio coverage variation within four communes in the northern part of the country. The radio signal in these communes comes from the Mount Muhe transmitter located approximately 30 km west,

[34] If there are sharp edges that the electromagnetic signal encounters, there can also be some diffraction. The exact formula, and the Longley-Rice model, can be found at http://flattop.its.bldrdoc.gov/itm.html (Available November 3 2009).

[35] The 2-order polynomial in the distance to the transmitter alone explains 44 percent of the variation in radio coverage.

[36] Commune fixed effects alone explain 82 percent of the variation in village mean altitude, and 72 percent of the variation in radio coverage.

[37] There are 129 communes in the sample and 8.6 villages per commune.

outside the figure. The figures show that within each commune, villages that happen to be situated to the east of hilltops have low radio coverage, while villages that happen to be situated to the west of hilltops have high radio coverage. This is because the signal comes in from the west, and the hilltops are in line of sight to the transmitter. This arguably provides a credible identification strategy, as there is no plausible reason why other determinants of participation in violence should be different across the eastern and western sides of the hilltops.[38]

Exogeneity check

If the identification strategy is valid and radio coverage is as good as randomly assigned, there should be no correlation between the variation in radio coverage and the other determinants of participation in violence. In particular, there should be no correlation between radio coverage and the fundamental value of participation in conflict θ, or the access to independent information, σ_x. Since these variables are unobservable, it it not feasible to directly test this assumption.[39] Instead, we test the validity of the exogeneity assumption by using observable village characteristics that are likely to be correlated with θ and σ_x, namely 1991 population density; 1991 population levels; distance to the nearest major town; distance to the nearest major road; distance to the nearest border point; and village area.[40] The regression specification is

$$y_{c,i} = \beta r_{c,i} + X_{c,i}\pi + \gamma_c + \varepsilon_{c,i} \ , \tag{4.6}$$

where $y_{c,i}$ is a characteristic of village i in commune c; $r_{c,i}$ is the radio coverage of village i in commune c; $X_{c,i}$ is the vector of village i controls and γ_c is the commune fixed effects. For completeness, we test using both levels and logs for each y.

The vector of standard village controls are: a second-order polynomial in the kilometer distance to the nearest transmitter; a second-order polynomial in the average village altitude in kilometers; the variance in altitude within the village. If the exogeneity assumption is correct, we expect $\beta = 0$.

Table 2 shows the results. None of the village characteristics are significant, and the lowest p-value is 0.234. This lends credibility to the identification strategy. In the main regressions, results will be presented both without and with village

[38] Note that in this particular case, the variation comes from the east-west relationship to the hilltops. In other communes it will, of course, be in other directions.

[39] Since there is no available data on ethnic polarization t at the village level, t is also an unobserved determinant of participation.

[40] The analogy used in randomized experiments is to check whether the treatment and control groups are balanced on observable pre-treatment characteristics.

characteristics. The results are similar with and without the inclusion of these characteristics.

4.3 Econometric specifications

In this section, we present the econometric specifications used to test each prediction.

Main Effects (Prediction 1): *If radio coverage r increases the participation rate h, then radio broadcasts a signal that the fundamental value of conflict was high, $p > \tilde{p}$.*

That is, if we find that radio coverage increased the participation rate, ethnic majority members perceived the Radio RTLM broadcasts as information **of** the fundamental value of conflict being high. To test this, we run the following regression[41]

$$\log(h_{c,i}) = \beta r_{c,i} + X_{c,i}\pi + \gamma_c + \varepsilon_{c,i} \ , \tag{4.7}$$

where the dependent variable is the logged total number of prosecutions per capita, $h_{c,i}$, of village i in commune c; $r_{c,i}$ is the RTLM radio coverage of village i in commune c; $X_{c,i}$ is the vector of village i controls; and γ_c is the commune fixed effects.[42] We will also run separate regressions where $h_{c,i}$ is either civilian violence only or organized violence only. The vectors of standard village controls are: a second-order polynomial in the kilometer distance to the nearest transmitter; a second-order polynomial in the average village altitude in kilometers and the variance in altitude within the village. In additional specifications, we also add controls for population density, distance to nearest major town, distance to nearest road, and distance to the nearest border point. According to Prediction 1, if $\beta > 0$ then this is consistent with $p > \tilde{p}$.

Ethnic Polarization (Prediction 2): *The effect of radio coverage r on the participation rate is decreasing in ethnic polarization t, only if $\alpha > 0$; and independent of t only if $\alpha = 0$.*

Therefore, testing for differential effects of radio coverage depending on ethnic polarization gives one method for separating whether there are *strategic complements*

[41] Since the true conditional expectations function $E[h_i \mid r_i]$ depends on the unobservable parameters in the model, it is unknown. We use a standard OLS regression model with a logged outcome variable. The regression will provide a linear approximation of the true relationship.

[42] Of the 1105 villages, 20 have zero prosecutions. Since the outcome variable is logged, we use log[(prosecutions+1)/population] to deal with the problem of undefined log function.

($\alpha > 0$) in participation. We test for this using the following specification

$$\log(h_{c,i}) = \beta r_{c,i} + \delta r_{c,i} \times t_c + X_{c,i}\pi + \gamma_c + \varepsilon_{c,i} \ , \tag{4.8}$$

where t_c is a dummy variable indicating whether the size of the ethnic minority population in commune c is large and the other variables are the same as previously. Specifically, t_c is equal to one if the ethnic minority size is above the median (7.53%) commune. The main parameter of interest is δ. According to Prediction 2, $\delta < 0$, only if $\alpha > 0$.

Scale Effects (Prediction 3): *Radio coverage r exhibits increasing scale effects, only if $\alpha > 0$; and linear effects, only if $\alpha = 0$.*

This provides an additional test that allows us to separate whether there are *strategic complements ($\alpha > 0$)* in participation. To investigate Prediction 3, we use the following flexible non-linear specification

$$\log(h_{c,i}) = \sum_{s=0.1}^{1} \beta^s r_{c,i}^s + X_{c,i}\pi + \gamma_c + \varepsilon_{c,i} \ , \tag{4.9}$$

where $r^s{}_{c,i}$ is a dummy variable equal to one if $s - 0.1 \leq r_{c,i} < s,$ and zero otherwise. The other variables are the same as before. We estimate the β^s in order to investigate the scale effects.

Independent Information (Prediction 4): *Radio coverage r does not affect the participation rate h when ethnic majority members have sufficiently good access to independent information ($\sigma_x \to 0$).*

As described in section 4.4, we test this prediction using literacy rates and primary education as proxy variables for access to independent information, σ_x. We use the following specification

$$\log(h_{c,i}) = \eta_1 r_{c,i} \times \sigma_{1,c} + \eta_2 r_{c,i} \times \sigma_{2,c} + \eta_3 r_{c,i} \times \sigma_{3,c} + X_{c,i}\pi + \gamma_c + \varepsilon_{c,i} \ , \tag{4.10}$$

where $\sigma_{j,c}$ is a dummy variable indicating whether the Hutu literacy rate (or the Hutu primary education level) commune c belongs to tertile j in the distribution of Hutu literacy rates (or the Hutu primary education level). If $\sigma_{3,c}$ is a sufficiently good proxy for σ_x close to zero, by Prediction 4 we expect $\eta_3 = 0$.

5 Results

In the following sections, we present the results for each tested prediction.

5.1 Main effects

The results for the test of Prediction 1 are presented in Table 3. Column 1 presents the simple correlation between radio coverage and the participation rate, and shows a negative correlation for total violence. However, this is unlikely to be a causal effect of RTLM radio coverage for a number of reasons mentioned in the empirical strategy section. Applying the identification strategy by controlling for the main set of variables that determine radio propagation and commune fixed effects, Column 2 shows that radio coverage increased participation in genocide violence. The effect is significant at the 5 percent level. Column 3 shows that the point estimate is almost identical when additional village covariates are added. Column 4 shows that RTLM reception has a positive and significant impact on civilian violence, and Column 6 shows significant effects also on organized violence.[43] Columns 5 and 7 show that adding covariates does little in the way of changing the point estimates, which is not surprising given the identification strategy and the results in Table 2.[44]

The estimated effects from the full specifications in Table 3 are substantial. For overall violence, Radio RTLM propaganda caused 71 percent (0.561 log points) more participation in violence for villages with full radio coverage ($r = 1$), as compared to villages unable to receive the propaganda ($r = 0$). Looking at the two types of violence separately, civilian violence increased by 65 percent (0.501 log points) and for organized violence, the increase was 77 percent (0.572 log points).[45]

Interpreting these results within the framework of the model and Prediction 1, they imply that Radio RTLM did indeed broadcast messages that the value of conflict was high, *and* Rwandan citizens believed in them. Furthermore, the results are consistent with the model under strategic interactions in violence, as well as without such interactions. That is, the results presented in Table 3 are not informative about whether the participation increased because Hutu citizens updated their beliefs about the fundamental value of violence, or whether the broadcasts

[43] Residual plots show that the results are not driven by outliers (not shown).

[44] The estimates assume no spillover across villages, which might be unrealistic. If the violence increased in villages with good radio coverage, which caused further violence in neighboring villages with low radio coverage, this would lead to an underestimation of the true effects. If this is the case, the estimates could be interpreted as providing the lower bounds of the true effects.

[45] Due to the specification, these are linear approximations of the causal effects.

also changed the beliefs about how many others were likely to participate in the killings. Next, we present results that allow us to further understand the underlying mechanisms that can explain why Radio RTLM caused more violence.

5.2 Ethnic polarization

The results for the test of Prediction 2 are presented in Table 4. Columns 1 and 2 show the estimated effects for total violence. The interaction effect between radio coverage and ethnic polarization is negative with and without additional controls. Both coefficients are significant at the 5 percent level. Columns 3 to 6 show that the interaction coefficients are similar for civilian and organized violence. The coefficients for civilian violence are significant at the 5 percent level, and insignificant for organized violence.[46] Interestingly, the estimated coefficients imply that the broadcasts only had an effect in areas with low ethnic polarization (i.e., where the ethnic minority population is small), as the point estimate for the interaction with high ethnic polarization is almost identical, but of the opposite sign, to the coefficient when ethnic polarization is low.[47]

As stated in Prediction 2, the results are only consistent with the model under *strategic complements*. Figure 1B graphically shows how the model, under strategic complements, predicts the effects of radio coverage depending on the relative size of the ethnic minority group. The empirical results do not only show that RTLM propaganda was ineffective when the Tutsi population was relatively large, they also suggest that this was due to strategic complements in ethnic violence. That is, Hutu citizens were more reluctant to participate in the attacks against Tutsi citizens when the Hutu majority population was relatively small, perhaps due to a fear that Tutsi villagers would be able to better defend themselves as a group. Therefore, even though radio did broadcast a message about the value of conflict being high in general, the results show that the broadcasts were not sufficient to persuade Hutu citizens to participate in areas with high ethnic polarization.

[46] Strictly speaking, we cannot reject the null hypothesis for organized violence. Note, however, that this is due to large standard errors. The coefficients for organized violence are very similar to those for civilian violence.

[47] The p-value for the test of effects when ethnic polarization is high is 0.89.

5.3 Scale effects

The results for the test of Prediction 3 are presented in Table 5. Column 1 shows that the estimated coefficients are generally small and not significantly different from zero for low levels of radio coverage, while for high levels of radio coverage, the coefficients are large and statistically significant at the 1 or 5 percent level. Figure 10 graphically plots the coefficients and the 95 percent confidence intervals. The figure shows that the effects are highly non-linear. For the range of up to 60-70 percent radio coverage, the point estimates are small but not significantly different from zero. Most importantly, they are non-increasing in the range. When radio coverage reaches approximately 70 percent, we see a sharp estimated increase in the participation rate, however. The effects are substantial. The increase in the point estimates is almost three-fold. They imply that participation increased by approximately 70 percent when radio coverage exceeded 70 percent. The coefficients are significant at the five-percent level.

Figure 10 suggests that the broadcasts were effective only when people knew that many other village members were also listening to the same broadcasts. The results from the previous section showed that all effects of radio coverage on participation rates come from villages where the Tutsi population was relatively small (i.e., low ethnic polarization). The model allows us to jointly interpret the results. Figure 1B shows the predicted effects of radio coverage, under strategic complements for two levels of ethnic polarization. When the ethnic minority is relatively large, there are more or less no effects for any level of radio coverage. When the ethnic minority is relatively small, however, the picture is very different. For low levels of radio coverage, even though the ethnic minority is small, there are essentially no effects on participation in violence. When radio coverage reaches critically high levels, however, there is a sharp increase in participation. In particular, when a sufficiently large number receive the broadcast, then everybody who listens to the radio knows that almost everybody else is also listening to the same broadcasts. Under strategic complements in violence, individuals are more willing to participate when they also expect others to participate. Interpreting Figure 10 somewhat loosely through Figure 1B, when radio coverage reached high levels, this corresponded to a large-scale, 70 percent, increase in participation. The evidence therefore suggests that there are important strategic complements in violence and that Radio RTLM functioned as a coordination device.

5.4 Access to independent information

The results for the test of Prediction 4 are presented in Table 6. Column 1 shows that there is a significant effect of radio coverage when the literacy rate is low. The coefficient is large and significant at the 5 percent level. It implies that in villages with low literacy rates (bottom tertile), complete radio coverage ($r = 1$) increased participation by 347 percent (1.499 log points), as compared to villages that are unable to receive the propaganda ($r = 0$). Column 1 also shows that in villages with medium literacy rates (middle tertile), radio coverage had a significant effect on participation. The coefficient is significant at the 10 percent level and implies a 71 percent (0.535 log points) increase in participation when the radio coverage was complete. Importantly, there is no effect of radio coverage in the villages with the highest literacy rates (upper tertile). The coefficient is negative and very close to zero. Column 2 shows that the effects are similar when additional controls are included.

Columns 3 and 4 estimate the effects of radio coverage for different levels of primary education. The estimated coefficients show a similar pattern as literacy rates. Importantly, there is no effect in villages where the Hutu household heads have most primary education. The coefficients in both columns 3 and 4 are very close to zero.

Interpreting relatively high literacy rates and a relatively high level of primary education as better access to independent sources of information, the results confirm Prediction 4. Moreover, the model allows us to interpret why literacy rates and primary education were important and suggests why they mitigated the propaganda effects. When people had better access to independent information, for example through the 30-60 independent newspapers available at the time, they did not put much weight on the RTLM broadcasts because, in relative terms, RTLM did not contain much information. Therefore, they did not put much belief in the messages and, consequently, they were not persuaded to participate in the killings.

5.5 How much of the genocide is explained by Radio RTLM?

This section performs a simple counterfactual calculation to assess how much of the genocide that can be explained by Radio RTLM. Specifically, we use the estimated coefficients of Table 5 and calculate the participation in the absence of the radio station.

For each village i, we first calculate the counterfactual ($r = 0$) participation

$$\hat{h}_{i,c}(r=0) = \exp\left[\log\left(h_{c,i}\right) - \hat{\beta}^{s}_{c,i}\right] \ ,$$

where $\hat{h}_{i,c}$ is the counterfactual participation rate (prosecution rate) of village i in commune c, and $\hat{\beta}^{s}_{c,i}$ is the coefficient estimate from Table 5, column 1, for the radio coverage indicator variable equal to 1 for village i[48] . Since participation is defined as the number of village prosecutions divided by the 1991 village population, we multiply with the 1991 population in order to get the counterfactual number of prosecutions. Summing over all villages, we find that Radio RTLM caused approximately 39 700 of the total 425 900 prosecution cases for genocidal violence in the sample. The estimates therefore suggest that approximately 9% of the genocide can be explained by Radio RTLM. This is non-trivial considering that only about 20 percent of the population had radio coverage to receive the broadcasts.[49]

We can make the same calculations for civilian violence and organized violence, respectively. Using Table 5 column 2 for civilian violence and column 3 for organized violence, the counterfactual calculation suggests that Radio RTLM caused approximately 32 000 more civilian prosecution cases (the sample total is approximately 361 700 category 2 crimes) and 5 200 more prosecution cases for organized violence (category 1 crimes). Therefore, using the separate estimates suggests that approximately 9% of the organized violence and 11% of the civilian violence can be explained by Radio RTLM.

Finally, we can use the numbers to assess how many in the Tutsi population were killed due to Radio RTLM. According to des Forges (1999), at least 500 000 Tutsis were killed in the genocide. Making the additional assumption that the number of Tutsi deaths is proportional to the number of prosecutions, the estimated effects therefore suggest that Radio RTLM caused at least 45 000 Tutsi deaths.[50]

6 Conclusion

This paper investigates the impact of propaganda on participation in civil conflict. Specifically, the paper examines the impact of the propaganda spread by the in-

[48] We use the point estimates. Naturally, since there is uncertainty in the estimated coefficients, the resulting numbers should be taken as approximate estimates.

[49] We calculate the number by village radio coverage multiplied by the population number in each village, given by the 1991 Census. Therefore, the number refers to the share of the population calculated to have had radio coverage. Since only 34% of the households in the 1991 Census owned a radio (in the communes in the sample), the number of listeners is most likely lower.

[50] Again, the total death numbers are controversial, but most believe 500 000 is the lower bound.

famous "hate radio" station Radio Télévision Libre des Mille Collines before and during the 1994 Rwanda Genocide.

The paper first sets up a simple model of participation in ethnic violence. Then, it derives a set of testable predictions that are consequently taken to the data. To identify the causal effects of the broadcasts, the empirical strategy exploits arguably exogenous variation generated by Rwanda's highly varying topography consisting of hills and valleys.

The paper presents novel evidence on the effects of propaganda. The results show substantial effects of the Radio RTLM broadcasts on violence participation. The estimates imply that when a village has full rather than zero radio coverage, civilian violence increased by 65 percent and organized violence by 77 percent.

Furthermore, the paper presents evidence of strategic complements. First, the effects are entirely driven by villages where the Hutu ethnic group was large relative to the Tutsi ethnic minority, which is only consistent with the model under strategic complements. Second, as predicted by the model under strategic complements, the estimated effects are highly nonlinear in the degree of radio coverage as there is a sharp increase in violence when the village radio coverage is sufficiently high. This suggests that the broadcasts were effective only when people knew that many other village members were also listening to the same broadcasts. Together, the evidence therefore suggests that the mechanism through which the broadcasts increased violence was partly because it functioned as a coordination device.

The model also predicts that access to independent information can mitigate the propaganda effects. It tests this prediction using variables associated with the ability to access independent information, such as the 30-60 independent newspapers available in Rwanda at the time of the genocide, by estimating whether the broadcasts had smaller effects in villages with higher levels of literacy and primary education. The empirical results show that more education decreased the propaganda effects, as there is no effect of radio coverage in villages in the tertile with the highest literacy rates and primary education.

To assess how much of the genocide that can be explained by the violence, the paper conducts a simple counter-factual calculation implying that Radio RTLM caused approximately 9.3% of the genocidal violence, corresponding to at least 45 000 deaths. The results therefore suggest that Radio RTLM was a quantitatively important causal factor in the genocide.

Finding that the propaganda caused more violence, and was partly effective because of strategic complements in violence, opens up further questions. Why are

there strategic complements in violence? Is it because attacking in numbers is less risky? Or is it because *not* participating is dangerous when many others participate? Are strategic complements generally present in civil conflicts? If so, what are the other devices used for coordination? These are important questions left to future research.

Bibliography

[1] Bates, Robert H. (1986), "Modernization, Ethnic Competition, and the Rationality of Politics in Contemporary Africa," *Governing in Black Africa*, 6(4):457-84.

[2] BBC (2003), "The Impact of Hate Media in Rwanda," BBC article, <news.bbc.co.uk/2/hi/africa/3257748.stm>, Available (2009-11-06)

[3] Besley, Timothy and Burgess, Robin (2002), "The Political Economy of Government Responsiveness: Theory and Evidence from India," *Quarterly Journal of Economics*, 117(4):1415-51

[4] Besley, Timothy and Persson, Torsten (2008), "The Incidence of Civil War: Theory and Evidence," mimeo

[5] Bethany, Lacina and Gleditsch, Nils Petter (2005), "Monitoring Trends in Global Combat: A New Dataset of Battle Deaths," *European Journal of Population* 21(2–3):145–66

[6] Blattman, Chris and Miguel, Edward (2009), "Civil War," *NBER Working Paper,* 14801, March

[7] Boesche, Roger (2003), "Kautilya's Arthaśāstra on War and Diplomacy in Ancient India,". *The Journal of Military History* 67(1):9–37

[8] Carlsson, Hans and van Damme, Eric (1993), "Global Games and Equilibrium Selection," *Econometrica*, vol. 61(5):989-1018, Sept

[9] Chang, Chun-Fang and Knight, Brian (2008), "Media Bias and Influence: Evidence from Newspaper Endorsements," *NBER Working Paper,* 14445

[10] Chretien, J., Dupaquier, J., Kabanda, M., Ngarambe, J., and Reporters Sans Frontieres (1995), *Rwanda: Les Medias du Genocide* [Rwanda: The media of the genocide]. Paris: Karthala.

[11] Collier, Paul and Hoeffler, Anke (1998), "On Economic Causes of Civil War," *Oxford Economic Papers*, 50(4):563-73

[12] Collier, Paul and Hoeffler, Anke (2004), "Greed and Grievance in Civil War," *Oxford Economic Papers*, 56(4):563-95

[13] Della Vigna, Stefano and Kaplan, Ethan (2007), "The Fox News Effect: Media Bias and Voting," *Quarterly Journal of Economics,* 122(3):187-234

[14] Della Vigna, Stefano and Gentzkow, Matthew (2009), "Persuasion: Empirical Evidence" *Annual Review of Economics*, forthcoming

[15] Des Forges, Alison (1999), *Leave None to Tell the Story: Genocide in Rwanda.* Human Rights Watch and the International Federation of Human Rights Leagues, New York, NY, USA. <www.hrw.org/legacy/reports/1999/rwanda/>, Available (2009-11-06).

[16] Diamond, Jared (2005), *Collapse: How Societies Choose to Fail or Succeed*, New York: Penguin.

[17] Do, Quy-Toan and Iyer, Lakshmi (2007), "Poverty, Social Divisions and Conflict in Nepal," Unpublished working paper, Harvard Business School.

[18] Dube, Oeindrila and Vargas, Juan F. (2007), "Commodity Price Shocks and Civil Conflict: Evidence From Colombia," Unpublished working paper, Harvard University and UCLA

[19] Edmond, Chris (2008), "Information Manipulation, Coordination and Regime Change," Stern School of Business, mimeo

[20] Enikolopov, R., Petrova, M. and Zhuravskaya, E. (2009), "Media and Political Persuasion: Evidence from Russia", mimeo

[21] Eisensee, Tomas and Strömberg, David (2007), "News Droughts, News Floods, and U.S. Disaster Relief," *Quarterly Journal of Economics*, 122(2):693-728

[22] Fearon, James D. and Laitin, David (1996), "Ethnicity, Insurgency and Civil War," *American Political Science Review*, 97(1):75-90

[23] Fearon, James D. and Laitin, David (1996), "Explaining Interethnic Cooperation," *American Political Science Review*, 90(4):715-35

[24] Gentzkow, Matthew and Shapiro, Jesse (2004), "Media, Education and Anti-americanism in the Muslim World," *The Journal of Economic Perspectives*, 18(3):117-33

[25] Gentzkow, Matthew (2006), "Television and Voter Turnout," *Quarterly Journal of Economics*, 121(3):931-972

[26] Gerber A, Karlan, D and Bergan, D. (2009), "Does the Media Matter? A Field Experiment Measuring the Effect of Newspapers on Voting Behavior and Political Opinions," *American Economic Journal: Applied Economics*, 1(2):35-52

[27] Glaeser, Edward (2005), "The Political Economy of Hatred," *Quarterly Journal of Economics,* 120(1):45-86.

[28] International Criminal Tribunal for Rwanda, ICTR (2003), Case No. ICTR-99-52-T Judgement. http://www.ictr.org/ENGLISH/cases/Barayagwiza/judgement/Summary-Media.pdf (Available 2009-09-05)

[29] Jha, Saumitra (2008), "Trade, Institutions and Religious Tolerance: Evidence from India," Stanford GSB Working Paper No. 2004, January

[30] Knight, Brian and Chiang, CF (2008), "Media Bias and Influence: Evidence from newspaper endorsements," mimeo

[31] Berelson, Bernard, Lazarsfeld, Paul and William M. McPhee (1954), *Voting: A Study of Opinion Formation in a Presidential Campaign.* University of Chicago Press.

[32] Miguel, Edward, Satyanath, Shanker and Sergenti, Ernest (2004), "Economic Shocks and Civil Conflict: An Instrumental Variables Approach," *Journal of Political Economy*, 112(4):725-53

[33] Montalvo, J.G. and Reynal-Querol, Marta (2005), "Ethnic Polarization, Potential Conflict and Civil War," *American Economic Review*, 95(3),:796-816.

[34] Morris, Stephen and Shin, Hyun Song (1998), "Unique Equilibrium in a Model of Self-Fulfilling Currency Attacks," *American Economic Review*, 88(3):587-97.

[35] Morris, Stephen and Shin, Hyun Song (2005), "Notes on Strategic Substitutes and Complements in Global Games", mimeo

[36] Mullainathan, Sendhil and Shleifer, Andrei (2005), "The Market for News," *American Economic Review*, 95(4): 1031–53.

[37] Murshed, S. Mansoob and Gates, Scott (2005), "Spatial-Horizontal Inequality and the Maoist Insurgency in Nepal," *Review of Development Economics* 9, 1:121-34.

[38] Olken, Ben (2009), "Do Television and Radio Destroy Social Capital? Evidence from Indonesian Villages," *American Economic Journal: Applied Economics*, 1(4):1-33

[39] Paluck, Elizabeth L. (2009), "Reducing Intergroup Prejudice and Conflict Using the Media: A Field Experiment in Rwanda," *Journal of Personality and Social Psychology* 96 (March): 574-587

[40] Prat, Andrea and Strömberg, David (2005), "Commercial Television and Voter Information,", CEPR Discussion Papers 4989.

[41] Prunier, Gérard (1995), *The Rwanda Crisis: History of a Genocide.* New York: Columbia University Press.

[42] Verpoorten, Marijke (2005), "The Death Toll of the Rwandan Genocide: a Detailed Analysis for Gikongoro Province," *Population* 60(4):331-68.

[43] Verwimp, Philip (2005), "An Economic Profile of Peasant Perpetrators of Genocide: Micro-level Evidence from Rwanda," *Journal of Development Economics*, 77(2):297-323.

[44] Verwimp, Philip (2006), "Machetes and Firearms: the Organisation of Massacres in Rwanda," *Journal of Peace Research*, 43(1):5-22

[45] Straus, Scott (2004), "How Many Perpetrators Were There in the Rwandan Genocide? An Estimate," *Journal of Genocide Research,* 6(1):85-98

[46] Straus, Scott (2007), "What Is the Relationship between Hate Radio and Violence? Rethinking Rwanda's "Radio Machete"," *Politics & Society*, 35:609

[47] Strömberg, David (2004), "Radio's Impact on Public Spending," *Quarterly Journal of Economics*, 119(1):189–221.

[48] Strömberg, David and Snyder, Jim (2008), "Press Coverage and Political Accountability,", mimeo

[49] Thompson, Allan (2007), *The Media and the Rwandan Genocide*. London: Pluto Press.

6.1 Appendix

6.2 Uniqueness

The equilibrium strategu for individuals with no radio is unique. By assuming that agents without are also unaware that others are getting the public signal p, they only receiving a private signal and play the game as if $r = 0$. This then leads to the Morris and Shin (1998) uniqueness result under private signal only.

The equilibrium for individuals with radio (receiving both a private and a public signal, with common knowledge of r among those receiving the public signal) is unique if $\frac{\sigma_x^2}{\sigma_p^2\gamma} \leq \frac{\sqrt{2\pi}t}{\alpha}$, where $\gamma \equiv \sqrt{\frac{2\sigma_x^2\sigma_p^2+\sigma_x^4}{\sigma_x^2+\sigma_p^2}}$. This is the Morris and Shin (1998) uniqueness result under a public signal, with the distinction that only some proportion r of the players receive the public signal. Morris and Shin prove uniqueness when everybody receives a public signal by iterative deletion of strictly dominated interim strategies, which can equally be applied in this context. Here, we show a sketch of the proof, essentially following Morris and Shin (2001). The equilibrium condition for radio individuals is

$$\kappa^R + \frac{\alpha}{t}\left(r\Phi\left[\frac{\sigma_x^2(p-\kappa^R)}{\sigma_p^2\gamma}\right] + (1-r)\,\Phi\left[\frac{\frac{\alpha}{2t}+\kappa^R}{\gamma}\right]\right) = 0 \tag{4.11}$$

Define $f(r, \kappa^R)$ as the left hand side function of the equation. A sufficient condition for a unique solution is that the left hand side increases weakly monotonically in κ^R, $f'_{\kappa^R} \geq 0$. The uniqueness condition is therefore that the derivative with respect to κ^R is non-negative (the monotonicity condition),

$$\begin{aligned} f'_{\kappa^R} &= 1 + \frac{\alpha}{t}\left(-r\phi\left(\frac{\sigma_x^2(p-\kappa^R)}{\sigma_p^2\gamma}\right)\frac{\sigma_x^2}{\sigma_p^2\gamma} + \frac{\alpha}{t\gamma}(1-r)\,\phi\left(\frac{\frac{\alpha}{2t}+\kappa^R}{\gamma}\right)\right) \\ &\geq 0 \end{aligned}$$

We see that the function reaches its lowest value when $r = 1$. Substituting for $r = 1$ and rearranging gives

$$1 \geq \frac{\alpha}{t}\phi\left[\frac{\sigma_x^2(p-\kappa^R)}{\sigma_p^2\gamma}\right]\frac{\sigma_x^2}{\sigma_p^2\gamma}$$

Now, the density of the standard normal $\phi(\cdot)$ reaches its maximum value of $1/\sqrt{2\pi}$ when the argument of $\phi(\cdot)$ is zero. Substituting $\phi(\cdot)$ with $1/\sqrt{2\pi}$ then gives the

sufficient condition for a unique solution

$$\frac{\sigma_x^2}{\sigma_p^2 \gamma} \leq \frac{\sqrt{2\pi}t}{\alpha}. \tag{4.12}$$

Q.E.D.

6.3 Proof of Lemma 1

The total share of the majority population participating is

$$h = rh^R + (1-r)h^N \tag{4.13}$$

Taking the derivative with respect to radio coverage gives

$$\frac{\partial h}{\partial r} = h^R - h^N + r\frac{\partial h^R}{\partial r}. \tag{4.14}$$

since h^N is independent of r. We see that $\frac{\partial h}{\partial r} \geq 0$ if $h^R \geq h^N$ and $\frac{\partial h^R}{\partial r} \geq 0$. We therefore find the sufficient conditions for when $h^R \geq h^N$ and $\frac{\partial h^R}{\partial r} \geq 0$.

$$\begin{aligned}
h^R &\geq h^N \\
&\iff \\
\Phi\left[\left(\frac{\sigma_x^2}{\sigma_p^2}p + \theta - \frac{\sigma_x^2+\sigma_p^2}{\sigma_p^2}\kappa^R\right)/\sigma_x\right] &\geq \Phi\left[\left(\frac{\alpha}{2t}+\theta\right)/\sigma_x\right] \\
&\implies \\
\frac{\sigma_x^2}{\sigma_p^2}p + \theta - \frac{\sigma_x^2+\sigma_p^2}{\sigma_p^2}\kappa^R &\geq \frac{\alpha}{2t}+\theta \\
&\implies \\
p &\geq \frac{\sigma_p^2}{\sigma_x^2}\left[\frac{\alpha}{2t} + \frac{\sigma_x^2+\sigma_p^2}{\sigma_p^2}\kappa^R\right].
\end{aligned}$$

This is also the sufficient condition for $\frac{\partial h^R}{\partial r} \geq 0$. To see this,

$$\frac{\partial h^R}{\partial r} = -\phi\left[\left(\frac{\sigma_x^2}{\sigma_p^2}p + \theta - \frac{\sigma_x^2+\sigma_p^2}{\sigma_p^2}\kappa^R\right)/\sigma_x\right]\frac{\sigma_x^2+\sigma_p^2}{\sigma_p^2}\frac{\partial \kappa^R}{\partial r} \tag{4.15}$$

which is weakly positive if $\partial\kappa^R/\partial r \leq 0$. From equation (4.11) we use the implicit function theorem and take the total derivative

$$\frac{\partial \kappa^R}{\partial r} = -\frac{f'_r}{f'_{\kappa^R}} \tag{4.16}$$

Since by the monotonicity assumption we have that $f'_{\kappa^R} \geq 0$, we have to show that

$$f'_r = \frac{\alpha}{t}\left(\Phi\left[\frac{\sigma_x^2(p-\kappa^R)}{\sigma_p^2\gamma}\right] - \Phi\left[\frac{\frac{\alpha}{2t}+\kappa^R}{\gamma}\right]\right)$$

Applying the sufficient condition for $h^R \geq h^N$, we let $p = \frac{\sigma_p^2}{\sigma_x^2}\left[\frac{\alpha}{2t} + \frac{\sigma_x^2+\sigma_p^2}{\sigma_p^2}\kappa^R\right] + \varepsilon$, where ε is a small positive number, gives

$$\begin{aligned} f'_r &= \frac{\alpha}{t}\left(\Phi\left[\frac{\sigma_x^2(\frac{\sigma_p^2}{\sigma_x^2}\left[\frac{\alpha}{2t} + \frac{\sigma_x^2+\sigma_p^2}{\sigma_p^2}\kappa^R\right] + \varepsilon - \kappa^R)}{\sigma_p^2\gamma}\right] - \Phi\left[\frac{\frac{\alpha}{2t}+\kappa^R}{\gamma}\right]\right) \\ &= \frac{\alpha}{t}\left(\Phi\left[\frac{\frac{\alpha}{2t}+\kappa^R+\frac{\sigma_x^2}{\sigma_p^2}\varepsilon}{\sqrt{\frac{2\sigma_x^2\sigma_p^2+\sigma_x^4}{\sigma_x^2+\sigma_p^2}}}\right] - \Phi\left[\frac{\frac{\alpha}{2t}+\kappa^R}{\sqrt{\frac{2\sigma_x^2\sigma_p^2+\sigma_x^4}{\sigma_x^2+\sigma_p^2}}}\right]\right) \\ &\geq 0 \end{aligned}$$

Together with the uniqueness condition (4.12), $p > \frac{\sigma_p^2}{\sigma_x^2}\left[\frac{\alpha}{2t} + \frac{\sigma_x^2+\sigma_p^2}{\sigma_p^2}\kappa^R\right]$ implies

$$\frac{\partial \kappa^R}{\partial r} < 0 \tag{4.17}$$

which in turn implies $\frac{\partial h^R}{\partial r} > 0 \Longrightarrow \frac{\partial h}{\partial r} > 0$..

We now show that $p \geq -\frac{\alpha}{2t}$ implies that the condition $p \geq \frac{\sigma_p^2}{\sigma_x^2}\left[\frac{\alpha}{2t} + \frac{\sigma_x^2+\sigma_p^2}{\sigma_p^2}\kappa^R\right]$ is fulfilled for all values of r. Since $\frac{\partial \kappa^R}{\partial r} \leq 0$, the maximum value of κ^R is achieved when $r = 0$. Substituting for $r = 0$ in the equilibrium condition (4.11), we get

$$\kappa^R + \frac{\alpha}{t}\Phi\left[\frac{\frac{\alpha}{2t}+\kappa^R}{\sqrt{\frac{2\sigma_x^2\sigma_p^2+\sigma_x^4}{\sigma_x^2+\sigma_p^2}}}\right] = 0$$

The equilibrium condition is fulfilled only when $\kappa^R = -\frac{\alpha}{2t}$, since substituting $\kappa^R =$

$-\frac{\alpha}{2t}$ gives

$$\begin{aligned} & -\frac{\alpha}{2t}+\frac{\alpha}{t}\Phi\left[\frac{\frac{\alpha}{2t}-\frac{\alpha}{2t}}{\sqrt{\frac{2\sigma_x^2\sigma_p^2+\sigma_x^4}{\sigma_x^2+\sigma_p^2}}}\right] \\ = & -\frac{\alpha}{2t}+\frac{\alpha}{t}\Phi\left[0\right] \\ = & -\frac{\alpha}{2t}+\frac{\alpha}{2t} \\ = & 0 \end{aligned}$$

The maximum value of κ^R is therefore $-\frac{\alpha}{2t}$, which is when $r=0$. Substituting for the maximum value of κ^R, the sufficient condition for $\frac{\partial h}{\partial r}\geq 0$ is

$$\begin{aligned} p & \geq \frac{\sigma_p^2}{\sigma_x^2}\left[\frac{\alpha}{2t}+\frac{\sigma_x^2+\sigma_p^2}{\sigma_p^2}\left(-\frac{\alpha}{2t}\right)\right] \\ & = -\frac{\alpha}{2t} \end{aligned}$$

Q.E.D.

6.4 Proof of Proposition 1.

Recall that

$$\frac{\partial h}{\partial r}=h^R-h^N+r\frac{\partial h^R}{\partial r}. \tag{4.18}$$

When $\alpha=0$, the equilibrium conditions imply that $\kappa^{N*}=\kappa^{R*}=0$, for all r. From equation (4.15), this implies that $\frac{\partial h^R}{\partial r}=0$. Furthermore, $\partial h/\partial r>0$ if $h^R>h^N$, which is always true if $p>0$. To see this,

$$\begin{array}{ccc} h^R & > & h^N \\ & \Longleftrightarrow & \\ \Phi\left[\frac{\frac{\sigma_x^2}{\sigma_p^2}p+\theta}{\sigma_x}\right] & > & \Phi\left[\frac{\theta}{\sigma_x}\right] \\ & \Longleftrightarrow & \\ p & > & 0 \end{array}$$

Since h^R and h^N are both constants, $\partial h/\partial r=h^R-h^N=\bar{c}>0$.

Q.E.D.

6.5 Proof of Proposition 2

Proposition 5 *2* ($\alpha > 0$) : *If participation is subject to strategic complements, then radio coverage exhibits increasing scale effects* ($\partial^2 h/\partial r^2 > 0$ *for* $r \in [0, \tilde{r}]$, *and* $\partial^2 h/\partial r^2 < 0$ *for* $r \in (\tilde{r}, 1]$, *where* $0 < \tilde{r} \leq 1$). *Furthermore, the effect of radio coverage is decreasing in the size of the ethnic minority* ($\partial h/\partial r \partial t < 0$ *for all* r, *as long as* $h^R < 1/2$. *If* $h^R \geq 1/2$, *the sign of* $\partial h/\partial r \partial t$ *is ambiguous.*

6.5.1 Scale Effects

Recall that,

$$h = rh^R + (1-r)h^N \ ,$$

where

$$h^N = \Phi\left(\frac{\frac{\alpha}{2t} + \theta}{\sigma_x}\right) \ , \tag{4.19}$$

and

$$h^R = \Phi\left[\frac{\frac{\sigma_x^2}{\sigma_p^2}p + \theta - \frac{\sigma_x^2+\sigma_p^2}{\sigma_p^2}\kappa^R}{\sigma_x}\right] \ . \tag{4.20}$$

Taking the second derivative gives

$$\frac{\partial^2 h}{\partial r^2} = \frac{\sigma_x^2+\sigma_p^2}{\sigma_p^2}\phi(z)\left[-2\frac{\partial \kappa^R}{\partial r} - r\left[z\frac{\sigma_x^2+\sigma_p^2}{\sigma_p^2}\left(\frac{\partial \kappa^R}{\partial r}\right)^2 + \frac{\partial^2 \kappa^R}{\partial r^2}\right]\right] \tag{4.21}$$

, where $z \equiv \frac{\frac{\sigma_x^2}{\sigma_p^2}p+\theta-\frac{\sigma_x^2+\sigma_p^2}{\sigma_p^2}\kappa^R}{\sigma_x}$ and where the second derivative $\frac{\partial^2 \kappa^R}{\partial r^2}$ is (derivation not shown for brevity):

$$\frac{\partial^2 \kappa^R}{\partial r^2} < 0.$$

Now, from (4.17) we have $\frac{\partial \kappa^R}{\partial r} < 0$. From (4.21) $\frac{\partial^2 h}{\partial r^2}$ is initially positive: $\frac{\partial^2 h}{\partial r^2}_{r\to 0} > 0$, and we that a necessary condition for $\frac{\partial^2 h}{\partial r^2} < 0$ is that $z > 0$, or equivalently, that $h^R > 1/2$. Therefore, $\frac{\partial^2 h}{\partial r^2} > 0$ for all r if $z < 0$. Therefore, there may exists an $0 < \tilde{r} \leq 1$ such that $\partial^2 h/\partial r^2 < 0$ for $r \in (\tilde{r}, 1]$, where $\tilde{r}$ is defined by

$$\frac{\sigma_x^2+\sigma_p^2}{\sigma_p^2}\phi(z)\left[-2\frac{\partial \kappa^R}{\partial r} - \tilde{r}\left[z\frac{\sigma_x^2+\sigma_p^2}{\sigma_p^2}\left(\frac{\partial \kappa^R}{\partial r}\right)^2 + \frac{\partial^2 \kappa^R}{\partial r^2}\right]\right] = 0 \ .$$

Q.E.D.

6.5.2 Ethnic Minority Size

Furthermore, we also need to show that 1) $\partial h/\partial r\partial t < 0$ for all r as long as $h^R < 1/2$, and 2) If $h^R \geq 1/2$, the sign of $\partial h/\partial r\partial t$ for $r > \hat{r}$ is ambiguous.

First, taking the cross-derivative of (4.13)

$$\frac{\partial h}{\partial r \partial t} = -\phi(z)\frac{\sigma_x^2+\sigma_p^2}{\sigma_x\sigma_p^2}\left(\frac{\partial \kappa^R}{\partial t} + r\frac{\partial \kappa^R}{\partial r \partial t}\right) + \phi(z')\frac{\alpha}{2t^2\sigma_x} - zr\phi(z)\left(\frac{\sigma_x^2+\sigma_p^2}{\sigma_p^2}\right)^2 \frac{\partial \kappa^R}{\partial r}\frac{\partial \kappa^R}{\partial t} \tag{4.22}$$

Implicit derivation of (4.11) gives $\frac{\partial \kappa^R}{\partial t} > 0$, $\frac{\partial \kappa^R}{\partial r} < 0$, and $\frac{\partial \kappa^R}{\partial r \partial t} > 0$. Note that (4.22) in general is indeterminate. However, if $h^R < 1/2$ for all r, then we can show that $\frac{\partial h}{\partial r \partial t} < 0$ for all r. Two steps are required to show this.

Step 1. First, note that $\phi(z) \geq \phi(z')$, since

$$z' = \left(\frac{\alpha}{2t} + \theta\right)/\sigma_x < \frac{\frac{\sigma_x^2}{\sigma_p^2}p + \theta - \frac{\sigma_x^2+\sigma_p^2}{\sigma_p^2}\kappa^R}{\sigma_x} = z$$

To see this, we have shown above that the maximum value of κ^R is $-\frac{\alpha}{2t}$ (when is when $r = 0$). And since $p > -\frac{\alpha}{2t}$ by the definition of propaganda, we have that

$$\begin{aligned} z' &= \frac{\frac{\sigma_x^2}{\sigma_p^2}p + \theta + \frac{\sigma_x^2+\sigma_p^2}{\sigma_p^2}\frac{\alpha}{2t}}{\sigma_x} \\ &> \frac{-\frac{\sigma_x^2}{\sigma_p^2}\frac{\alpha}{2t} + \theta + \frac{\sigma_x^2+\sigma_p^2}{\sigma_p^2}\frac{\alpha}{2t}}{\sigma_x} \\ &= \frac{\frac{\alpha}{2t} + \theta}{\sigma_x} \\ &= z' \end{aligned}$$

Step 2. Note that $h^R < 1/2$ implies that $z < 0$. Since the only positive term in the second term, a given that $\frac{\partial \kappa^R}{\partial r \partial t} > 0$, a sufficient condition for $\frac{\partial h}{\partial r \partial t} < 0$ is then that

$$\frac{\sigma_x^2+\sigma_p^2}{\sigma_p^2}\frac{\partial \kappa^R}{\partial t} > \frac{\alpha}{2t^2}$$

Taking the implicit derivative for $\frac{\partial \kappa^R}{\partial t}$ using 4.11, this condition reduces to

$$\frac{\sigma_x^2+\sigma_p^2}{\sigma_p^2}\left(r\Phi(m) + (1-r)\Phi(m')\right) > 1 + \frac{\alpha}{t}\left(-r\phi(m)\gamma + (1-r)\,\phi(m')/\gamma\right)$$

We see that the min of the left hand side and the max of the right hand side is

achieved when $r = 0$, so as long as this is true when $r = 0$, the expression is always true. Applying $r = 0$,

$$\frac{\sigma_x^2 + \sigma_p^2}{\sigma_p^2}\Phi(m') > 1 + \frac{\alpha}{t\gamma}\phi(m')$$

But since $\kappa^R = -\frac{\alpha}{2t}$ when $r = 0$, we have that $m' = \frac{\frac{\alpha}{2t} + \kappa^R}{\sqrt{\frac{2\sigma_x^2\sigma_p^2 + \sigma_x^4}{\sigma_x^2 + \sigma_p^2}}} = 0$. And since $\Phi(0) = \frac{1}{2}$ and $\phi(0) = \frac{1}{\sqrt{2\pi}}$, the condition reduces to

$$\frac{\sigma_x^2}{\sigma_p^2\gamma} < \frac{\sqrt{2\pi}t}{\alpha}$$

which is always by assumption (4.11).

Q.E.D.

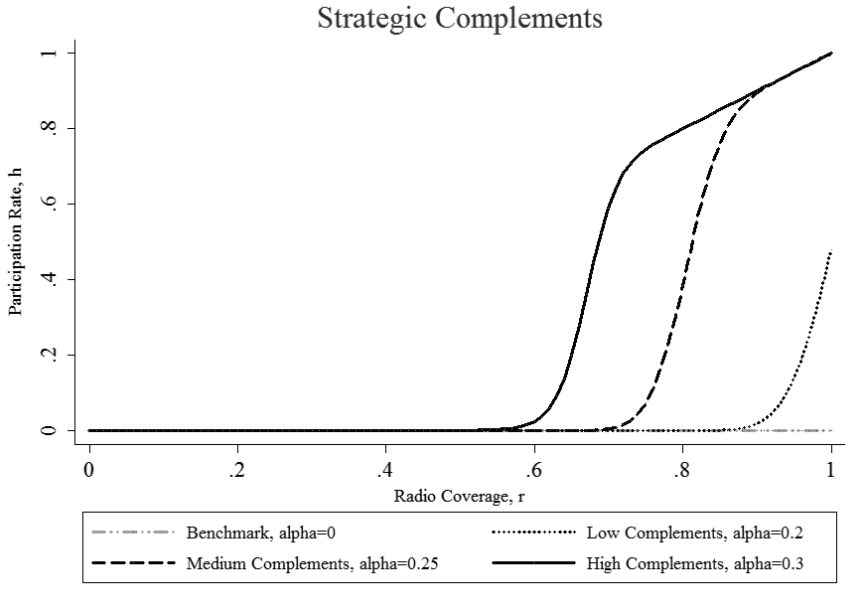


Figure 1A. The model under different degrees of strategic complementarities. ($p = 0$, $t = 1/4$, $\theta = -1$, $\sigma_x = 0.05$, $\sigma_p = 0.1$).

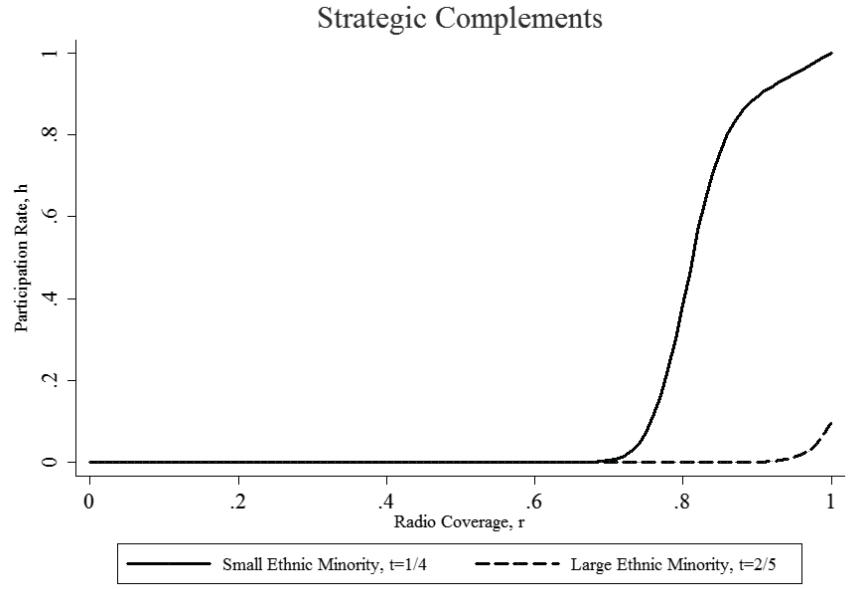


Figure 1B. The model under strategic complements for two levels of ethnic minority size. ($\alpha = 0.25$ and the other parameter values are the same as in Figure 1A).

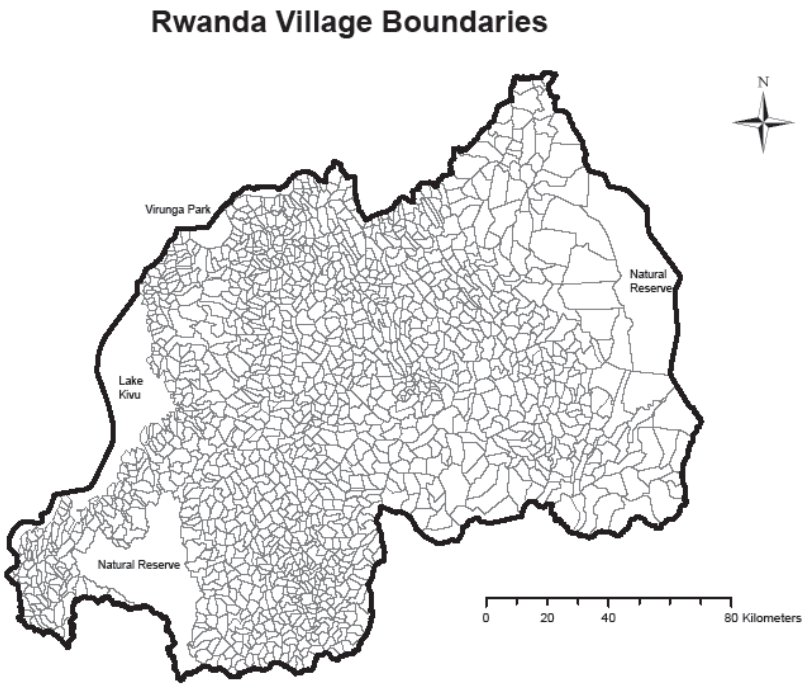


Figure 2. A map of Rwandan villages. Source: Paper map by Organisation Administrative du territorie de la Republic Rwandaise, digitized by the author.

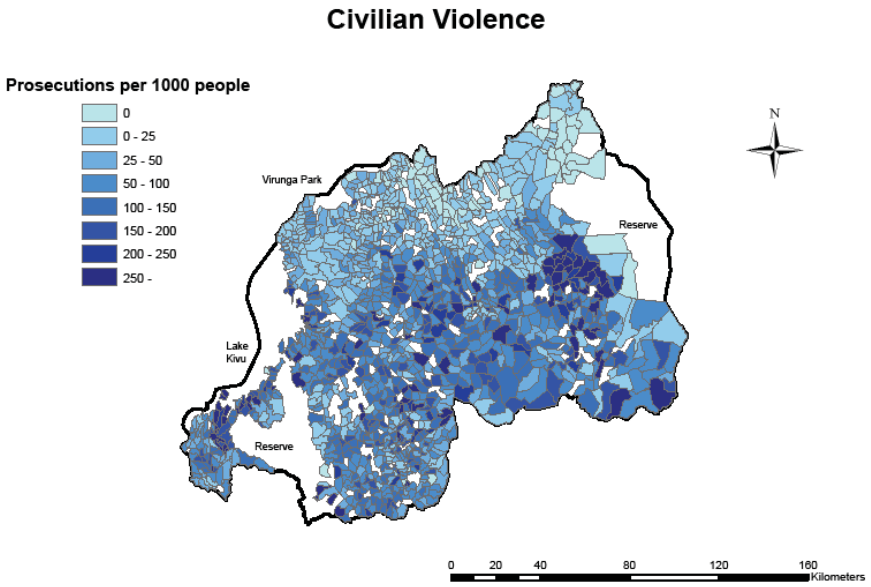


Figure 3A. A map of civilian violence.

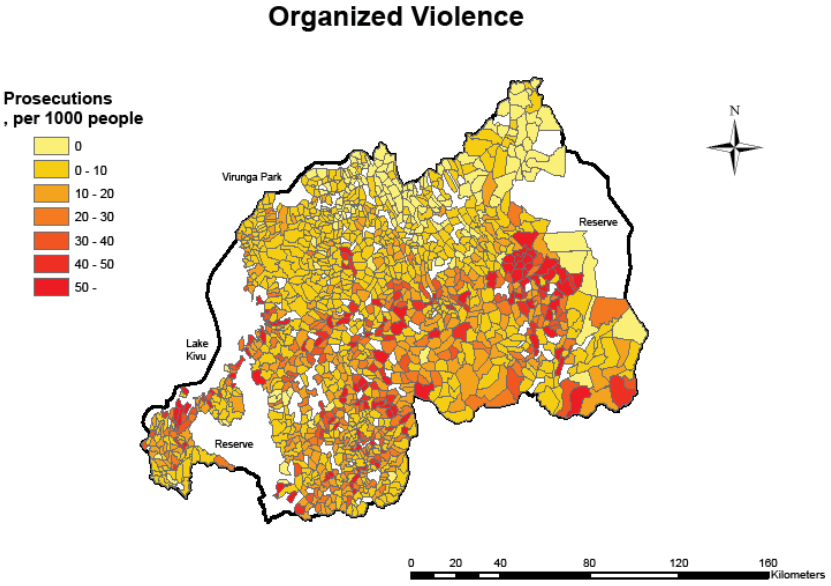


Figure 3B. A map of organized violence.

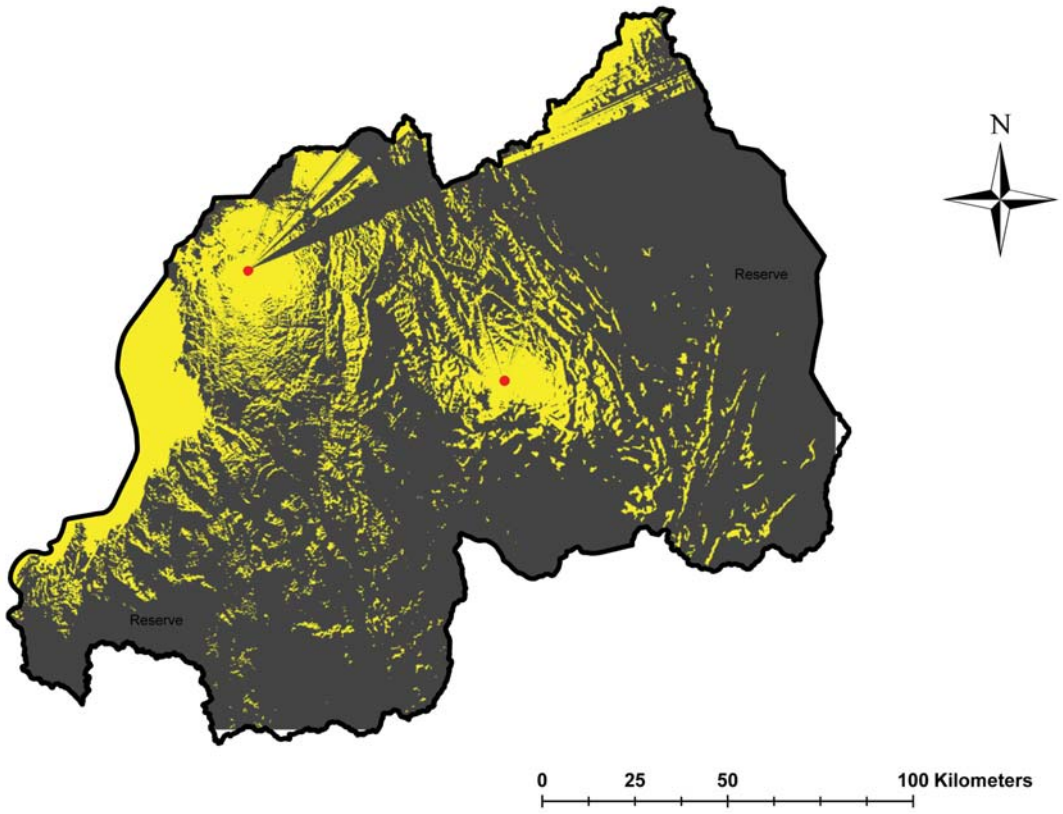


Figure 4. RTLM Radio Coverage. The two red dots mark the transmitters and yellow indicates radio coverage. Source: Author's calculations in ArcGIS using the ITM/Longley-Rice Propagation Model.

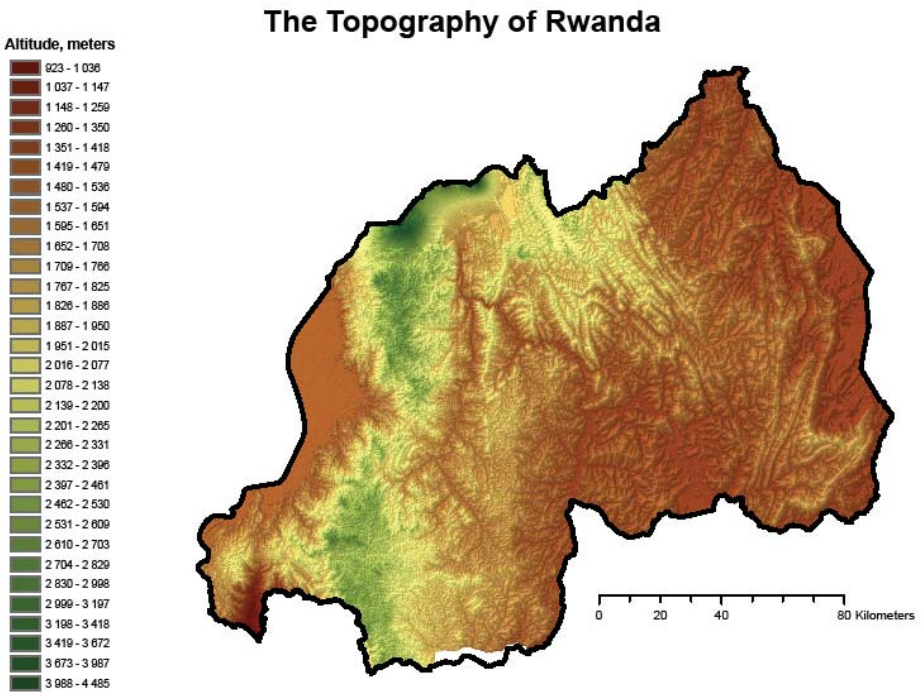


Figure 5. The topography of Rwanda. Source: Shuttle Radar Topography Mission

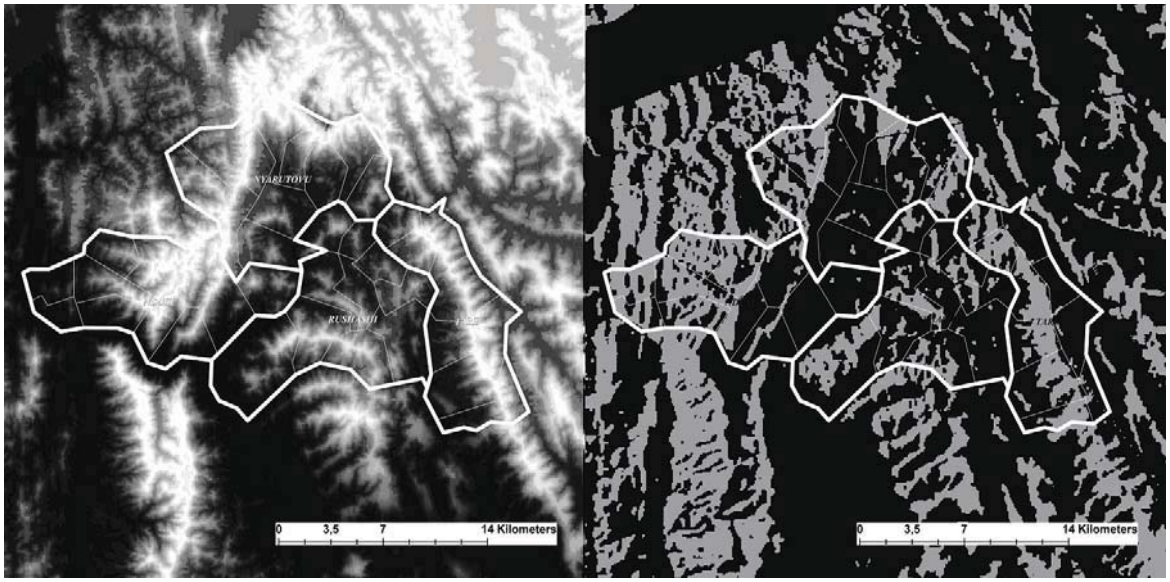


Figure 6. Topography and radio coverage in four communes. The left picture shows the height of ground, where brighter marks higher altitude. The right picture shows the empirical radio coverage, where grey marks radio coverage. The signal comes from the Mount Muhe transmitter located 30 km to the west (outside the figure). The figures show that within each commune, villages (boundaries in thin white lines) to the east of hilltops have low radio coverage due the hilltops in the line-of-sight to the transmitter. Source: SRTM topography data, Author's own calculations of radio coverage in ArcGIS software.

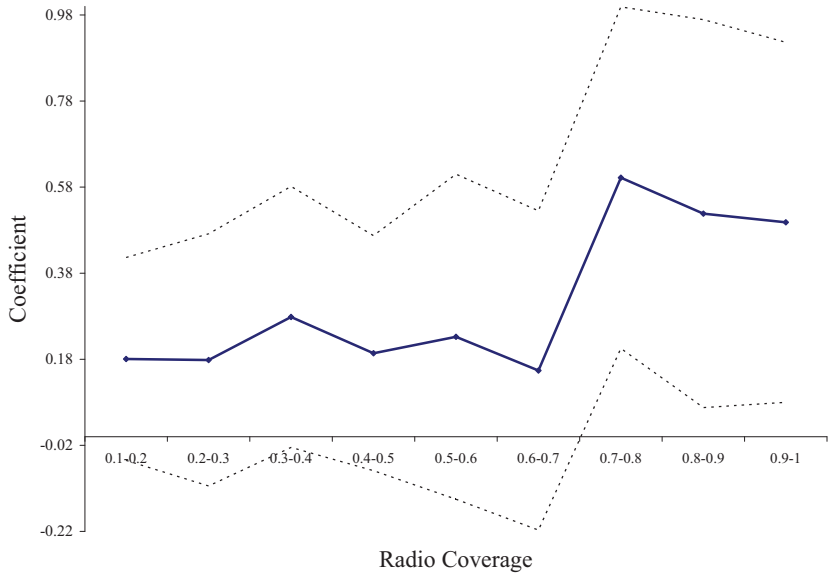


Figure 7. Scale Effects, Total Violence.

Table 1. Summary Statistics

Variable	Observations	Mean	Std. Dev.
Dependent Variables			
Participation Rate, Total	1105	.084	.070
Participation Rate, Civilian	1105	.072	.060
Participation Rate, Organized	1105	.013	.016
Independent Variables			
Radio Coverage	1105	.189	.226
Altitude, Mean	1105	1.713	.229
Altitude, Variance	1105	9208.3	10531.6
Distance to Transmitter	1105	5.171	2.841
Distance to Major Town	1067	.200	.120
Distance to Major Road	1071	.058	.052
Distance to the Border	1074	.217	.127
Village Area	1105	15.07	44.6
Hutu Literacy Rate	1105	.503	.056
Hutu Primary Education	1105	.579	.060
Tutsi Minority Size	1105	.098	.085
Population	1105	4846.7	2456.5
Population Density	1105	.528	.868

The dependent variables are violent crimes prosecutions divided by the village population in 1991; *Organized Violence* is crime category 1 prosecutions against organizers, leaders, army and militia; *Civilian Violence* is crime category 2 prosecutions for homicides, attempted homicides and serious violence. *Total* is the combined Civilian and Organized. *Radio Coverage* is the share of the village area that has RTLM reception. *Altitude, Mean* is the mean altitude in the village in kilometers. *Altitude, Variance* is the village variance in altitude in meters, *Distance to Transmitter* is the distance in kilometers to the nearest RTLM transmitter. The other distance variables are measured in decimal degrees. *Hutu Literacy Rate* is the fraction of Hutu household heads in the commune that are literate. *Hutu Primary Education* is the fraction of Hutu household heads in the commune that have at least some primary education. Education and literacy data are taken from the 1991 Census, available only at the commune level. There are 129 communes in the sample, and approximately 8.6 villages per commune. *Population* is the population number in the village and *Population Density* is 1000 people per square kilometers, also from the 1991 Census.

Table 2. Exogeneity Check

Dependent Variable	Population Density, 1991		Population, 1991		Village Area, km^2		Distance to Major Town		Distance to Major Road		Distance to the Border	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	level	log	level	log	level	log	level	log	level	log	level	log
Radio Coverage	0.240	0.177	-557.32	-0.047	-28.484	-0.224	0.006	0.096	-0.012	-0.233	0.001	0.091
	[0.352]	[0.205]	[766.21]	[0.094]	[31.305]	[0.191]	[0.010]	[0.112]	[0.010]	[0.212]	[0.011]	[0.146]
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Commune FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	1105	1105	1105	1105	1105	1105	1067	1067	1071	1071	1074	1074
R-squared	0.44	0.42	0.42	0.45	0.18	0.56	0.95	0.90	0.81	0.70	0.96	0.92
P-value of Radio Coverage	0.496	0.389	0.468	0.618	0.365	0.243	0.528	0.390	0.234	0.275	0.957	0.535

Radio Coverage is the share of the village area that has RTLM radio coverage. The controls are: A second-order polynomial in village mean altitude, altitude variance, and a second-order polynomial in the distance to the nearest transmitter. Robust standard errors in parentheses, clustered at the commune level. There are 129 communes in the sample.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3. Main Effects

Dependent Variable	*Log(Participation Rate) Total Violence*			*Log(Participation Rate) Civilian Violence*		*Log(Participation Rate) Organized Violence*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Radio Coverage	-0.717 [0.260]***	0.571 [0.229]**	0.561 [0.244]**	0.520 [0.229]**	0.501 [0.246]**	0.559 [0.294]*	0.572 [0.288]**
Log(Population Density)			-0.127 [0.071]*		-0.120 [0.071]*		-0.101 [0.082]
Distance to Major Town			1.019 [1.534]		1.224 [1.526]		-0.518 [1.768]
Distance to Major Road			-2.791 [1.548]*		-2.646 [1.554]*		-4.527 [1.810]**
Distance to the Border			1.910 [1.317]		2.150 [1.366]		0.198 [1.625]
Controls	N	Y	Y	Y	Y	Y	Y
Commune FE	N	Y	Y	Y	Y	Y	Y
N	1105	1105	1066	1105	1066	1105	1066
R-squared	0.02	0.62	0.63	0.61	0.62	0.51	0.52

Participation Rate is the number of violent crimes prosecutions per capita; *Total Violence* is the sum of *Civilian* and *Organized Violence*, *Organized Violence* is crime category 1 prosecutions against organizers, leaders, army and militia; *Civilian Violence* is crime category 2 prosecutions for homicides, attempted homicides and serious violence. *Radio Coverage* is the share of the village area that has RTLM radio coverage. The radio propagation controls are: A second-order polynomial in village mean altitude, village altitude variance, and a second-order polynomial in the distance to the nearest transmitter. Robust standard errors in parentheses, clustered at the commune level. There are 129 communes in the sample.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4. Ethnic Polarization

Dependent Variable	*Log(Participation Rate) Total Violence*		*Log(Participation Rate) Civilian Violence*		*Log(Participation Rate) Organized Violence*	
	(1)	(2)	(4)	(5)	(6)	(7)
Radio Coverage	0.932 [0.303]***	0.936 [0.325]***	0.849 [0.301]***	0.834 [0.325]**	0.870 [0.379]**	0.922 [0.369]**
Radio Coverage x High Ethnic Polarization	-0.972 [0.411]**	-0.972 [0.427]**	-0.884 [0.412]**	-0.864 [0.430]**	-0.839 [0.619]	-0.907 [0.614]
Log(Population Density)		-0.126 [0.070]*		-0.118 [0.070]*		-0.100 [0.081]
Distance to Major Town		0.845 [1.509]		1.069 [1.504]		-0.681 [1.738]
Distance to Major Road		-2.736 [1.528]*		-2.598 [1.537]*		-4.475 [1.789]**
Distance to the Border		1.824 [1.296]		2.073 [1.346]		0.118 [1.613]
Controls	Y	Y	Y	Y	Y	Y
Commune FE	Y	Y	Y	Y	Y	Y
N	1105	1066	1105	1066	1105	1066
R-squared	0.62	0.63	0.61	0.62	0.51	0.52

Participation Rate is the number of violent crimes prosecutions per capita; *Total Violence* is the sum of *Civilian* and *Organized Violence*, *Organized Violence* is crime category 1 prosecutions against organizers, leaders, army and militia; *Civilian Violence* is crime category 2 prosecutions for homicides, attempted homicides and serious violence. *Radio Coverage* is the share of the village area that has RTLM radio coverage. The radio propagation controls are: A second-order polynomial in village mean altitude, the village altitude variance, and a second-order polynomial in the distance to the nearest transmitter. Robust standard errors in parentheses, clustered at the commune level. There are 129 communes in the sample.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5. Scale Effects

Dependent Variable	*Log(Participation Rate) Total Violence*	*Log(Participation Rate) Civilian Violence*	*Log(Participation Rate) Organized Violence*
	(1)	(2)	(3)
Radio Coverage, 0.1 - 0.2	0.181	0.163	0.346
	[0.119]	[0.119]	[0.121]***
Radio Coverage, 0.2 - 0.3	0.178	0.180	-0.004
	[0.148]	[0.143]	[0.145]
Radio Coverage, 0.3 - 0.4	0.278	0.281	0.147
	[0.153]*	[0.158]*	[0.149]
Radio Coverage, 0.4 - 0.5	0.194	0.196	0.115
	[0.138]	[0.137]	[0.216]
Radio Coverage, 0.5 - 0.6	0.232	0.227	-0.005
	[0.191]	[0.199]	[0.199]
Radio Coverage, 0.6 - 0.7	0.154	0.169	0.171
	[0.187]	[0.178]	[0.320]
Radio Coverage, 0.7 - 0.8	0.602	0.559	0.594
	[0.201]***	[0.205]***	[0.285]**
Radio Coverage, 0.8 - 0.9	0.518	0.429	0.855
	[0.228]**	[0.239]*	[0.288]***
Radio Coverage, 0.9 – 1	0.498	0.381	0.810
	[0.211]**	[0.189]**	[0.390]**
Controls	Y	Y	Y
Commune FE	Y	Y	Y
N	1105	1105	1105
R-squared	0.62	0.61	0.52

Participation Rate is the number of violent crimes prosecutions per capita; *Total Violence* is the sum of *Civilian* and *Organized violence*, *Organized Violence* is crime category 1 prosecutions against organizers, leaders, army and militia; *Civilian Violence* is crime category 2 prosecutions for homicides, attempted homicides and serious violence. *Radio Coverage* is the share of the village area that has RTLM radio coverage. The radio propagation controls are: A second-order polynomial in village mean altitude, village altitude variance, and a second-order polynomial in the distance to the nearest transmitter. Robust standard errors in parentheses, clustered at the commune level. There are 129 communes in the sample.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6. Ability to Access Independent Information

Dependent Variable	*Log(Participation Rate) Total Violence*			
	(1)	(2)	(3)	(4)
Radio Coverage x Low Hutu Literacy	1.499	1.549		
	[0.582]**	[0.602]**		
Radio Coverage x Medium Hutu Literacy	0.535	0.484		
	[0.323]*	[0.355]		
Radio Coverage x High Hutu Literacy	-0.013	-0.042		
	[0.308]	[0.321]		
Radio Coverage x Low Hutu Education			0.855	0.811
			[0.473]*	[0.480]*
Radio Coverage x Medium Hutu Education			0.824	0.980
			[0.329]**	[0.366]***
Radio Coverage x High Hutu Education			0.015	-0.139
			[0.337]	[0.364]
Log(Population Density)		-0.122		-0.128
		[0.071]*		[0.070]*
Distance to Major Town		0.963		0.971
		[1.513]		[1.536]
Distance to Major Road		-2.592		-2.821
		[1.526]*		[1.541]*
Distance to the Border		2.178		2.011
		[1.299]*		[1.304]
Controls	Y	Y	Y	Y
Commune FE	Y	Y	Y	Y
N	1105	1066	1105	1066
R-squared	0.62	0.63	0.62	0.63

Participation Rate is the number of violent crimes prosecutions per capita; *Total Violence* is the sum of *Civilian* and *Organized violence*, *Organized Violence* is crime category 1 prosecutions against organizers, leaders, army and militia; *Civilian Violence* is crime category 2 prosecutions for homicides, attempted homicides and serious violence. *Radio Coverage* is the share of the village area that has RTLM radio coverage. The radio propagation controls are: A second-order polynomial in village mean altitude, village altitude variance, and a second-order polynomial in the distance to the nearest transmitter. The other variables are described in the data section. Robust standard errors in parentheses, clustered at the commune level. There are 129 communes in the sample.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Chapter 5

Tuning in the Market Signal: The Impact of Price Information on Market Exchange in Uganda*

"Buyers were offering 300 shs/kg for beans in our village, but when radio announced a price of 500 shs/kg we negotiated at that price" [Ugandan farmer, in Ferris (2006)]

"It's not easy to cheat farmers these days because they are getting information about market prices from the radio. Things are changing" [local trader in Uganda as reported in BBC Focus On Africa magazine, 2004]

1 Introduction

This paper investigates how access to information on market prices affects market activity and the allocations of goods in the economy. A cornerstone in economic theory is the role of information. One of the fundamental results is that in perfectly competitive markets, where price taking producers and consumers are assumed to trade goods at publicly known prices, the allocation of goods in the economy is efficient.[1] In developing countries, however, these assumptions stand in sharp contrast to the reality faced by the main economic agents in the economy: small-scale rural farmers. While a majority of the population in developing countries live in rural areas and make their livelihood mainly from farming crops, access to updated information on prevailing prices in urban market centers is limited due to low levels of information and communication infrastructure (World Bank, 2007). Furthermore, when farmers choose to sell parts of their agricultural output, they typically do so by

* This paper is co-authored with Jakob Svensson, IIES Stockholm University. We thank Frances Nsonzi and the management team at Foodnet for their assistance. Comments from seminar participants at Tilburg University and Goethe-University in Frankfurt is gratefully acknowledge. Financial support from the Swedish International Development Agency, Department for Research Cooperation and Handelsbanken's Research Foundations is gratefully acknowledged.

[1] This is the famous First Fundamental Welfare Theorem.

engaging in trade with local traders that buy their crops at the farm-gate, often with limited competition from other traders (Fafchamps and Minten, 2001; Ferris, 2004), and resell them in urban market centers.[2] Importantly, while rural farmers have little access to updated price information, traders that constantly travel between rural areas and the market centers are naturally relatively well informed about the prevailing market prices.[3] The resulting economic exchange at the farm-gates could therefore be characterized as outcomes of a contracting process between traders and farmers, where traders have superior information relative to farmers. This type of asymmetric information, in turn, may lead to market frictions with sub-optimally low levels of market exchange.

Furthermore, although farming is the main economic activity for most people in developing countries, subsistence farmers mainly produce mainly for consumption within the household, with only small shares of their output sold. In Uganda, only around 17 percent of the output are sold and less than one-third (and for some crops as few as one-tenth) of the farmers are selling their crops on the market. Thus, most of the agricultural production is consumed within the household. Naturally, the low share of output sold is not necessarily due to market failures in agricultural markets, as the division of output between consumption and sales could be efficient if the markets are functioning well. However, given the lack of access to accurate price information in rural areas and the potentially important contractual frictions this may give rise to, it is somewhat surprising that there are so few empirical studies estimating whether improvements in market information increase economic exchange and market participation by farmers. This paper aims at filling that gap.

We present a simple model of the agricultural sector, with asymmetric information between farmers and traders. Then, we then test the predictions of the model using a natural experiment in Uganda. The main insight from the model is that the asymmetric information gives rise to contracting frictions between farmers and traders, as rent-seeking traders operating in rural areas with low availability of updated price information have incentives to claim that the prices in the central markets are lower than the prevailing ones. This leads to market failures with sub-optimally low levels of market exchange, as it reduces farmers' willingness to engage in gains from trade due to the information rents available to traders.[4] Access to price information can reduce these frictions, and lead to increased supply and incomes for informed farmers, as well as lower prices for consumers in urban market centers. Guided by these theoretical insights, this paper asks whether increasing farmers' access to price information can reduce such frictions and improve the functioning of developing countries' agricultural markets.

We exploit a "natural experiment" – the Market Information Service (MIS) in Uganda – to assess the impact of providing crop market price information to small-scale farmers. Starting in 2000, the MIS collected weekly data on market prices for some of the main agricultural commodities in 21 of Uganda's 56 districts, and

[2] In Uganda, almost 80% of the farmers sell their crops to private traders and only a few percent directly to a district market.

[3] See, for instance, Ferris (2004) and Banerji and Meenakshi (2004).

[4] For an overview of bilateral contracting under asymmetric information, see Bolton and Dewatripont (2005).

disseminated the information through local FM radio stations in the participating districts. The presumption was that the provision of accurate, timely and appropriate market information to farmers through radio transmissions would improve their ability to bargain with local traders. Using data from the Uganda National Household Surveys of 2000 and 2005, as well as the data on market prices collected by MIS, we study the effects of giving farmers access to market price information on the likelihood of farmers selling their crops, the share of the output sold, the prices received, and urban market prices. To identify the causal effects of access to information, we exploit differences in exposure over time, across space, and between crops (within households).

The results show that access to market information increases the likelihood of selling the crop by 29% (from a baseline of 0.23), the share of output sold by 32% and the price by 0.41 standard deviations. As a result, the crop income of farmers with access to market price information increased by an estimated 55%. Consistent with the model, we show that the effects are larger for crops for which farmers are less able to predict the market price for (i.e., when the variance in the market price is high). This is consistent with larger *incentive effects* when the degree of asymmetric information increases. Finally, we show that the positive supply response from information leads to lower prices in urban market centers, as the introduction of broadcasts decreased the market price by 0.94 standard deviations. Taken together, our results suggest that by reducing contractual frictions between farmers and traders, farmers' access to market price information substantially improves the income of informed farmers and the price levels for urban consumers, as well as the functioning of agricultural markets in developing countries.

The paper adds to a small and recent literature on the effects of increased market information on market outcomes. Focusing on the ability to exploit arbitrage opportunities, Jensen (2007) evaluates the effects of the introduction of mobile phones on market outcomes in the fishing industry in Kerala, India. He finds that by improving fishermen's and traders' ability to communicate over large distances, the introduction of mobile phones improved arbitrage opportunities and resulted in reduced waste and decreased price dispersion across geographic markets. Studying traders' search behavior in Niger, Aker (2008) finds similar effects on price dispersions across grain markets when mobile phones were introduced. Both these studies focus on technology improvements that reduce the search costs in the decision of where to sell the output. Goyal (2010) exploits variation induced by the entry of a large private soybeans buyer (ITC) in Madhya Pradesh, India. She finds that providing farmers with information about wholesale prices of soybeans in local markets, and an outside option to sell directly at known prices to ITC, resulted in an increase in the average price of soybeans in the local market. This finding is consistent with a model where information about current market prices and/or the introduction of a new outside option increased competition between local traders. Our paper differs from that research in some important ways. First, as in Goyal (2010), we study the impact of increasing farmer access to price information, rather than trader access to information.[5] Second, since farmers in Uganda almost exclusively sell their crops

[5] This is not to say that the information disseminated through radio did not contain any new

to traders at the farm-gate and almost never travel to the market themselves, we do not primarily study the decision on where or to whom to sell the output. Instead, we investigate the impact on the economic exchange at the farm-gate when the farmer is better informed about the prevailing market price. Finally, the functioning of agricultural markets is central to the development of low income countries and we study the question on why farmers in developing countries tend to only sell small fractions of their agricultural output. By addressing a particular source of market failure, i.e. the lack of updated market information leading to contractual frictions between farmers and traders, we assess to what extent there is an increase in market exchanges when the source of the market failure is removed. To the best of our knowledge, this is the first paper to estimate the impact of farmers' access to price information on a farmer's likelihood of selling his crops, the share of produce sold, the farm-gate price and urban market prices.

The remainder of this paper proceeds as follows: Section 2 discusses the institutional setup and the Market Information Service. Section 3 presents the model and its predictions. Section 4 discusses the data and Section 5 presents the empirical strategy. Section 6 presents the results and investigates alternative mechanisms. Section 7 concludes the paper.

2 Uganda's Agricultural Sector

Uganda's economy is predominantly agrarian. The agricultural sector employs more than 80% of the labor force and is the main source of livelihood for more than 85% of the population. In addition, almost 94% of the agricultural production take place on smallholder plots, including virtually all food production (Ministry of Finance, 2008). Key agricultural products include cash crops (coffee, tea, cotton, tobacco, and cocoa) and food crops (plantains, maize, cassava, maize, beans, millet and sweet potatoes), and recently a smaller set of horticultural produce. While there is significant variation across crops, the market is dominated by small-scale farmers. Detailed aggregate data for all crops is not available, but for maize, for example, it is estimated that 95% of the households engaged in maize production are small-scale farmers (with land holdings of 0.2-0.5ha), contributing over 75% of the marketable surplus of maize. Medium scale commercial farmers with 0.8-2.0ha of land under maize production contribute the remaining 25% (RATES Center, 2003).

Small-scale subsistence farmers sell off most of their surplus produce to rural traders immediately after the harvest due to limited infrastructure such as transport and storage facilities. For maize, rural traders, who operate in villages, constitute over 90% of the total number of maize traders and handle two-thirds of all traded maize. Typically, traders traverse villages on bicycles and pick-ups procuring

information for traders. However, given that traders travel back and forth to the market places on a daily basis while farmers do not, it is clear that the relative information supplied to farmers was much larger. We also find no evidence of decreased price dispersion across districts after the radio disseminations started transmissions (results not reported). Even if traders were benefitting, the empirical strategy estimates effects of farmeers' access to price information.

produce at farm-gate prices on a cash basis. Moreover, almost no farmers engage in long-term contractual arrangements with a trader. In a survey on ten districts, Ferris et al. (2006) report that only 3% of the farmers were trading on the basis of long-term contractual arrangements. Instead, most farmers engage with the market through traders on an informal and opportunistic manner, resulting in spot contracts between farmers and traders.

Traders either work independently or as agents of larger urban traders. Since traders travel back-and-forth to the market, while farmers seldom sell their output on the main district markets, sellers generally have less or little information about current prices while buyers are often well-informed, at least about the price in the district market where they are active (RATES Center, 2003).

Prices on most cash- and food crops vary greatly in district markets in Uganda over time. To illustrate this, Figure 1 depicts the weekly market price for cassava in the Kasese district market center (the coefficient of variation over time is 0.28, close to the mean of 0.24). Given these price variations, it is not surprising that farmers in Uganda view getting market information as one of their highest priorities (Ferris, 2004).

Prices also vary greatly across locations. Figure 2 plots the market price of beans during week 20 in 2001 across the participating MIS-districts. The variation in prices across districts at a given point in time suggests that to the extent that farmers sell their output to traders located in their own district, the market price in that district, rather than the average price in the country, is the key statistic. Figure 2 also suggests that the lack of adequate transportation infrastructure makes it difficult to exploit arbitrage across markets, thus implying that prices can be systematically different across space at a given point in time.

2.1 The Market Information Service

In 2000, the Market Information Service project was initiated by two agricultural research organizations (IITA and ASARECA) in association with the Ministry of Trade, Tourism and Industry in Uganda. The starting point of the project was survey data indicating that most farmers had limited knowledge of the current market prices in the main district market centers, and scarce information on price movements and market trends. By providing accurate, timely and appropriate information, the assumption was that small-scale farmers would be able to make better decisions about what to produce and where to sell their output. Timely and accurate information would also improve farmers' bargaining position vis-à-vis local and regional traders.

In 2004, the Market Information Service was operating in 21 districts in Uganda.[6] Figure 1 shows a map of participating districts. The project collected data, on a weekly basis, on market prices for 19 agricultural commodities. In practice, however, the MIS regularly reported prices on the seven main food crops in Uganda (see details below): Beans, Cassava, Groundnuts, Maize, Millet and Sweet potatoes.

[6] The total number of districts in 2004 was 56. Not all districts could be included in the MIS project because for budget and administrative reasons.

The information was processed and disseminated through various radio stations in each MIS district. Each week, a 15-minute radio program was broadcast and each day, a 2-4 minute news bulletin was broadcast in altogether eight local languages. The main focus of the radio shows was to provide updates on district market prices. The radio stations used for dissemination were popular ones and in 2004, the MIS was estimated to reach seven of Uganda's twenty-four million people each week (Ferris, 2004).[7] The intervention, therefore, was on a large-scale.

Next, we present a simple model of price information and agricultural market exchange.

3 A simple model of price information and agricultural market exchange

We model an economy consisting of atomistic small-scale rural farmers, rural-urban traders, and consumers in an urban center. There is one good (crop) which is produced by the farmers, bought by traders at farmers' farm-gates, and resold to consumers in the urban market center. The model consists of two parts. In the first part, we model how farm-gate prices and quantity are set, conditional on farmers' access to information, and taking (urban) market prices as given. In the second part, we endogenize the market prices in the urban market center where all traders sell the crops bought from farmers. This then pins down the equilibrium quantities and prices both in the urban market center and at the farm-gates. The model will have two key features reflecting the conditions in many low-income countries, including Uganda as described above. First, since traders constantly travel back and forth between the urban market and the rural areas, the market price (and the demand shock) will be observable to traders. Rural farmers do not observe the market price, however. Second, competition between traders at the farm-gate is imperfect. This will then give rise to inefficiencies due to asymmetric information between traders and farmers. Finally, we present predictions on how prices and quantities will respond to an increase in the access to price information. The goal is to take these predictions to the data in the subsequent sections.

3.1 The Farm-Gate Equilibrium

Let each farmer produce one crop of quantity Q, of which he can sell $q \leq Q$ to a trader and consume the rest, $c = Q - q$. We are interested in how much a farmer sells of what he produces and the prices he receives for his crops (henceforth "farm-gate price"), conditional on what information he has of the current retail market price

[7] The MIS project initially bought air-time from the radio stations for the radio program. Interestingly, because of the popularity of the program among farmers, several commercial radio stations started to transmit the programs without public funds.

(henceforth "market price"). The farmer's payoff function is

$$U = R + u(Q - q) \ , \tag{5.1}$$

where R is the total amount paid to the farmer (for q) and $u(0) = 0, u'(c) > 0$, $u''(c) < 0$.

Competition between traders at each farm-gate is imperfect.[8] For simplicity, we assume that there is one trader at the farm-gate to whom the farmer can sell.[9] The trader's profit is

$$\Pi = mq - R \ , \tag{5.2}$$

where m is the current market price in the urban retail market.[10] We assume there to be three possible market prices $m_1 < m_2 < m_3$ that are realized with probability π_1, π_2 and π_3, respectively (and summing up to one). To ensure that m_1 can be a price in equilibrium, we assume that $u'(Q) < m_1$.[11]

The economy consists of a continuum of atomistic farmers, with measure one. The are two types of farmers: informed (knows the realized retail market price) and uninformed (cannot observe the realized retail market price). There are $r \in (0, 1]$ informed farmers in the economy.

To keep things simple and avoid signalling games, the trader reports a market price to the farmer, and the farmer offers a take-it-or-leave contract to the trader on quantity q_i (subscript denotes state of nature or market price) and per-unit price p_i (or, analogously, revenue $R_i = p_i q_i$). We are interested in the menu of contracts $\{(q_i, m_i;\ i = 1, 2, 3\}$ to which the farmer can commit to.[12]

3.1.1 The uninformed farmer

Assume that the farmer cannot observe the market price. The farmer only knows that the market price is m_i with probability π_i. This is essentially a standard bilateral contracting model, or monopolistic screening, under hidden information (In the appendix, we present the first-best solution maximizing total welfare).

---

[8] Reasons for this could be high fixed costs (buying a truck) for becoming a trader, or collusion between traders.

[9] If, for example, one allows for two traders they could**, thro**ugh Bertrand competition, make the environment perfectly competitive. It is worth noting that this would be inconsistent with the anecdotal evidence presented above. Moreover, informing the farmer about the market price in such a case would also have no effect as Bertrand competition would drive the farm-gate price to the market price (short of transport costs).

[10] Transport costs t could easily be added, for example the linear cost tq, for the trader without changing the main results.

[11] Solving the farm-gate equilibrium with continuous prices does not change the main results. We choose discrete prices because they are much more tractable than continuous prices. In the Ugandan data, the market price distribution resembles a truncated normal distribution. When $\pi_2 > 0.5$ and $\pi_1 \approx \pi_3$, three discrete prices can be used to serve as an approximation for a truncated normal distribution.

[12] We solve for the first-best benchmark contract in the appendix.

The farmer will suggest a contract conditional on the market price that the trader reports. The uninformed farmer's problem is that the trader, who knows the market price, has incentives to claim that the market price is lower than it actually is. However, since the trader is more eager to buy when the market price is high, the farmer can possibly reduce the trader's incentives to report a low market price by cutting down the amount he sells when the trader reports a low market price. By reducing the trader's incentives to report a low market price, the farmer can reduce the informational rent of the trader.

The problem for the uninformed farmer can, in its most general form, be stated as

$$\max_{\{(q_i^{UI},R_i^{UI})\}} \sum_{i=1}^{3} \pi_i \left[R_i^{UI} + u(Q - q_i^{UI})\right] \qquad \text{subject to} \tag{5.3}$$

$$m_i q_i^{UI} - R_i^{UI} \geq 0 \quad \text{for all } i \tag{5.4}$$

$$m_i q_i^{UI} - R_i^{UI} \geq m_i q_j^{UI} - R_j^{UI} \quad \text{for all } i,j \tag{5.5}$$

Exploiting the fact that the Spence-Mirrless single-crossing condition holds, we can reduce the number of incentive constraints to a smaller set of local downward incentive constraints and a monotonicity condition (see the appendix).[13] The monotonicity condition holds if

$$\text{Assumption 1: } \frac{1}{\pi_1}(m_2 - m_1) \geq \frac{\pi_3}{\pi_2}(m_3 - m_2) \tag{5.6}$$

which we assume to be the case.

Rewriting the first-order conditions we have

$$q_1^{UI} = \begin{cases} Q - u_c^{-1}\left(m_1 - \frac{(\pi_2+\pi_3)}{\pi_1}(m_2 - m_1)\right) & \text{for } m_1 > \tilde{m}_1 \\ 0 & \text{for } m_1 \leq \tilde{m}_1 \end{cases} \tag{5.7}$$

$$q_2^{UI} = \begin{cases} Q - u_c^{-1}\left(m_2 - \frac{\pi_3}{\pi_2}(m_3 - m_2)\right) & \text{for } m_2 > \tilde{m}_2 \\ 0 & \text{for } m_2 \leq \tilde{m}_2 \end{cases} \tag{5.8}$$

$$q_3^{UI} = Q - u_c^{-1}(m_3) \tag{5.9}$$

[13] The Spence-Mirrless single-crossing condition is

$$\frac{\partial}{\partial m}\left[-\frac{\partial U/\partial q}{\partial U/\partial R}\right] = \frac{\partial}{\partial m}\left[-\frac{m}{-1}\right] > 0$$

For details on how to solve contract problems of this form, see Bolton and Dewatripont (2005).

where the threshold market prices $\tilde{m}_i$ are

$$\tilde{m}_2 \equiv \left(\frac{1}{1-\pi_1}\right)(\pi_2 u'(Q) + \pi_3 m_3) \tag{5.10}$$

$$\tilde{m}_1 \equiv \pi_1 u'(Q) + (1-\pi_1)m_2 \tag{5.11}$$

and $\tilde{m}_2 > \tilde{m}_1$ as long as assumption 1 holds. Note that if $m_1 > \tilde{m}_1$ and $m_2 > \tilde{m}_2$, an uninformed farmer always sells positive amounts of his output. Given that a large fraction of small scale farmers in developing countries (a majority of the farmers in Uganda) are subsistence farmers that consume all their crop output, the assumption that $m_1 > \tilde{m}_1$ and $m_2 > \tilde{m}_2$ are inconsistent with the data. In addition, since one of the goals of the model is to explain behavior on the extensive margin (i.e., the probability of selling any output), we instead make the natural assumption that the uninformed farmer is the least willing to sell at the lowest market price. That is, we assume that

Assumption 2: $m_1 < \tilde{m}_1$ and $m_2 > \tilde{m}_2$

holds, which implies that $q_1^{UI} = 0$. The probability that the uninformed farmer will engage in market exchange (i.e., the extensive margin) is then

$$\rho^{UI} \equiv P(q_i^{UI} > 0) = 1 - \pi_1. \tag{5.12}$$

From the IR-constraint and the IC-constraints (see appendix), we can determine the farm-gate prices for the uninformed farmer

$$p_1^{UI} = \{\text{no exchange}\} \tag{5.13}$$

$$p_2^{UI} = m_2 \tag{5.14}$$

$$p_3^{UI} = m_3 - \frac{q_2^{UI}}{q_3^{UI}}(m_3 - m_2) \tag{5.15}$$

Note that $p_i^{UI} \leq m_i$ for $i = 2, 3$. Using (5.14), (5.15), (5.8) and (5.9), the average farm-gate price and quantity are

$$E\left[p_i^{UI}\right] = \frac{1}{1-\pi_1}\left(\pi_2 m_2 + \pi_3 m_3 - \pi_3 \frac{q_2^{UI}}{q_3^{UI}}(m_3 - m_2)\right) \tag{5.16}$$

$$E\left[q_i^{UI}\right] = \pi_2\left[Q - u_c^{-1}\left(m_2 - \frac{\pi_3}{\pi_2}(m_3 - m_2)\right)\right] + \pi_3\left[Q - u_c^{-1}(m_3)\right] \tag{5.17}$$

Having presented the equilibrium outcomes for uninformed farmers, we now turn to the case of the informed farmer.

3.1.2 The informed farmer

Now instead assume that the farmer knows the current market price. The constrained maximization problem is then

$$\max_{\{(q_i^I, R_i^I)\}} \sum_{i=1}^{3} \pi_i \left[R_i^I + u(Q - q_i^I) \right] \quad \text{subject to} \tag{5.18}$$

$$m_i q_i^I - R_i^I \geq 0 \quad \text{for all } i, \tag{5.19}$$

where (5.19) is the trader's individual rationality constraints (IR).

Since the farmer has no incentives to relax the IR-constraints, we can solve for R_i^I from (5.19) and substitute into the maximand (5.18). This yields an identical problem as that in the first-best. Thus, the quantity sold under full information, q_i^I, is equal to the first-best quantities

$$q_i^I = Q - u_c^{-1}(m_i), \ i = 1, 2, 3 \tag{5.20}$$

From the IR-constraint we can solve for the farmer's revenue, R_i, and the unit price

$$p_i^I \equiv \frac{R_i^I}{q_i^I} = m_i. \tag{5.21}$$

We can now calculate the average farm-gate price and quantity

$$E\left[p_i^I\right] = \pi_1 m_1 + \pi_2 m_2 + \pi_3 m_3 \tag{5.22}$$

$$E\left[q_i^I\right] = \pi_1 \left(Q - u_c^{-1}(m_1)\right) + \pi_2 \left(Q - u_c^{-1}(m_2)\right) + \pi_3 \left(Q - u_c^{-1}(m_3)\right) \tag{5.23}$$

$$\rho^I \equiv P(q_i^I > 0) = 1, \tag{5.24}$$

where the last result implies that the informed farmer will always engage in market exchange since $u'(Q) < m_1$.

3.1.3 Informed versus uninformed farmers

Define the differences in average outcomes as $\beta_s \equiv E\left[q_i^I - q_i^{UI}\right]/Q$, $\beta_\rho \equiv \rho^I - \rho^{UI}$, and $\beta_p \equiv E\left[p_i^I - p_i^{UI}\right]$. Using (5.12), (5.16), (5.17), (5.24), (5.22) and (5.23), we can calculate the average difference in outcomes

Extensive margin:

$$\beta_\rho = \pi_1 > 0 \tag{5.25}$$

Share sold:

$$\beta_s = \frac{1}{Q}\left[\pi_1 q_1^I + \pi_2 (q_2^I - q_2^{UI})\right] > 0 \tag{5.26}$$

Farm-gate price:

$$\beta_p = \pi_1 (m_1 - \hat{m}) + \left(\frac{\pi_3}{\pi_2 + \pi_3} \right) \left(\frac{q_2^{UI}}{q_3^{UI}} (m_3 - m_2) \right) \lesseqgtr 0 \ . \tag{5.27}$$

where $\hat{m} \equiv \frac{\pi_2 m_2 + \pi_3 m_3}{1 - \pi_1}$. Even though the informed farmer gets a higher farm-gate price for each market price at which the uninformed farmer sells, we see that the *average* difference in farm-gate prices can be either positive or negative. This may seem counterintuitive, but it is driven by the fact that becoming informed has two effects on the farm-gate price: a direct *incentive* effect (positive) and an indirect *selection* effect (negative). First, the direct incentive effect is positive since, conditional on a market price, the informed farmer does not have to incentivize the trader in order to reduce informational rents. This increases the average farm-gate price for the informed farmer. Second, the indirect selection effect is negative since the informed farmer is willing to sell at the lowest market price, while the uninformed farmer is not. This decreases the average farm-gate price for the informed farmer. If $m_1 > \hat{m}$, the direct incentive effect dominates the indirect selection effect, and the effect of being informed is positive on the farm-gate price.[14] However, since it is not obvious a priori which effect that should dominate, this is an empirical question. We can now summarize the key results in the following proposition.

Proposition 1 *Taking the urban market price distribution as given, an informed farmer is more likely to be active on the mark*et than an informed farmer (t*he extensive margin);* $\beta_\rho > 0$*, and will on average sell a larger share of his output (the total margin);* $\beta_s > 0$*. The difference in average farm-gate price is in general indeterminate;* $\beta_p \lesseqgtr 0$*. If the incentive effect dominates the selection effect, the effect on the farm-gate price is positive,* $\beta_p > 0$*.*

Intuitively, since the trader is more eager to buy when the market price is high, the uninformed farmer can reduce the trader's incentives to report a low market price by cutting down the amount he sells when market prices are low. By sacrificing allocative efficiency, the farmer reduces the trader's incentives to misreport the market price and thus reduces the trader's informational rents. Such an incentive scheme may be too costly when the market price is very low, so for low prices the farmer will choose not to sell.

Proposition 2. *Provided that assumptions 1 and 2 hold, and for a given market price, an increase in* π_1 and π_3 *(which implies an increase in the variance of market prices) will increase the difference in the intensive and the extensive margin between the full information and the hidden information outcomes.*[15]

Intuitively, stronger incentives are required to induce the trader not to report a lower price than the market price when the distribution of prices are more spread out.

[14] As long as the lowest market price, m_1, is higher than the expected market price under which uninformed farmers actively sell, $\hat{m} \equiv \frac{\pi_2 m_2 + \pi_3 m_3}{1 - \pi_1} = E[m_i \mid q_i^{HI} > 0]$, the effect is positive.

[15] *An increase in* π_1 *and* π_3 *results in a fall in* π_2*. As a result,* q_2^{HI} *falls and* ρ^{HI} *increases.* q_2^{FI} *and* ρ^{FI} *remain unchanged.* p_2^{HI} *also falls but* p_3^{HI} *increases so the effect on* $E[p_i]$ *is unclear.*

3.2 The Urban Market Price

In the previous section, prices were taken as given. In this section, we endogenize the market prices. Each trader sells all the crops that he has purchased from farmers. Let the set of traders be large such that each trader is a price taker in the retail market at the urban center. The supply is then a function of the market price and the fraction of informed farmers,

$$S(m_i, r) = rq^I(m_i) + (1-r)q^{UI}(m_i) \quad , i = 1, 2, 3 \tag{5.28}$$

where $q^I(m)$ and $q^{UI}(m)$ are given by equations (5.7) - (5.9), and (5.20), respectively. We can consider the demand coming from consumers consisting of urban non-agricultural households. For simplicity, we assume a linear demand function

$$D(m_i, \varepsilon_i) = d - \delta m_i + \epsilon_i \quad , i = 1, 2, 3 \tag{5.29}$$

where ϵ_i is an aggregate demand shock. We assume there to be three possible demand shocks $\epsilon_1 < \epsilon_2 < \epsilon_3$, that are realized with probability π_1, π_2, π_3, respectively (and summing up to one). We can think of the demand shock emanating from shocks in urban wages due to import or export price shocks for non-agricultural commodities. Key is that the demand shocks give rise to market price shocks and they are unobservable to farmers but observable to traders. This gives rise to the asymmetric information frictions at the farm-gate when farmer and traders bargain over the contract.[16]

The equilibrium (the urban market equilibrium and the farm-gate equilibrium) is pinned by three market clearing conditions

$$d - \delta m_1^* + \epsilon_1 = rq_1^I(m_1^*) \tag{5.30}$$

$$d - \delta m_2^* + \epsilon_2 = rq_2^I(m_2^*) + (1-r)q_2^{UI}(m_2^*, m_3^*) \tag{5.31}$$

$$d - \delta m_3^* + \epsilon_3 = q_3^I(m_3^*) \tag{5.32}$$

together with the farm-gate equilibrium quantities (5.7) - (5.9), and (5.20).

Taking the total derivative with respect to r, we can determine the effect of r, the share of informed farmers, on the retail market prices. The three derivatives are

$$\frac{\partial m_1^*}{\partial r} = -\frac{q_1^I(m_1^*)}{\delta + r\frac{\partial q_1^I(m_1^*)}{\partial m_1^*}} < 0 \tag{5.33}$$

[16] The linear demand function is not necessary for the main results. It is sufficient with a downward sloping demand curve. Key is that farmers cannot observe the market prices and the demand shocks. Similar results could be produced with aggregate and idiosyncratic supply shocks affecting Q, as long as farmers cannot perfectly observe the two shock components.

$$\frac{\partial m_2^*}{\partial r} = \frac{q_2^{UI}(m_2^*, m_3^*) - q_1^I(m_1^*)}{\delta + r\frac{\partial q_1^I(m_2^*)}{\partial m_2^*} + (1-r)\frac{\partial q_2^{UI}(m_2^*, m_3^*)}{\partial m_2^*}} < 0 \tag{5.34}$$

$$\frac{\partial m_3^*}{\partial r} = 0. \tag{5.35}$$

The expected marginal effect of the share of informed farmers on the urban market price is thus

$$\beta_m \equiv \frac{\partial E\left[m^*\right]}{\partial r} = \pi_1 \frac{\partial m_1^*}{\partial r} + \pi_2 \frac{\partial m_2^*}{\partial r} < 0 \tag{5.36}$$

We can now state the third and final proposition:

Proposition 3. *The retail market price is decreasing in the share of informed farmers.*

The intuition behind this result is relatively straightforward, as informed farmers sell larger shares of their output (relative to uninformed farmers) given a market price. Increasing the share of informed farmers increases the total quantity sold by traders in the retail market, which shifts the supply curve outward. This then puts downward pressure on prices in the retail market.

Furthermore, Proposition 3 implies that Propositions 1 and 2 are now equilibrium statements with endogenized market prices. In other words, the differences between informed and uninformed farmers (β_ρ, β_s, β_p) are differences that are partly driven by how many farmers are informed, since uninformed farmers are affected by available price information through the supply behavior of informed farmers (and vice versa).

3.3 Predictions

Using Propositions 1-3, we can summarize the main predictions:

Prediction 1 (Extensive Margin): *Informed farmers are more likely to engage in market exchange ($\beta_\rho > 0$).*

Prediction 2 (Total Margin): *Informed farmers sell larger shares of their output ($\beta_s > 0$).*

Prediction 3 (Farm Gate Price): *The effect on the farm-gate price is in general indeterminate ($\beta_p \lessgtr 0$). If the direct incentive effect dominates the indirect selection effect, the effect is positive.*

Prediction 4 (Price Uncertainty Effects): *The effect on supply behavior (Extensive and Total Margin) is increasing when there is more underlying uncertainty in the market price (i.e., when the market price distribution is wider).*

Prediction 5 (Retail Market Price): *When the share of informed farmers increases, the retail market price decreases ($\beta_m < 0$).*

Next, we present how we take these predictions to the data.[17]

4 Data

To test the predictions of the model, we want to measure whether a farmer is informed about the market price, whether he engages in market exchange (ρ_i), the share of output sold (s_i), the farm-gate price for sold crops (p_i), and market prices (m_i). We are then interested in whether the Uganda Market Information Service that informed farmers about prevailing market prices through local radio stations affected the outcome variables as predicted by the model.

We use three data sets: The Uganda National Household Survey 1999/2000 and 2004/2005 (hereafter UNHS 1999 and UNHS 2005) and data from the Market Information Service, provided by Foodnet. The two household surveys include a full crop module, enabling us to calculate farm-gate prices for crops sold, p_i in the model, as well as measures of market participation on both the extensive (ρ) and intensive (s) margins. The farmer data is measured at the plot level. Summary statistics of the UNHS 1999 and 2005 are reported in table 1. The data from the Market Information Service contains weekly data on collected urban market prices (from 2000 to 2005, with some missing data). There is one urban market per district. The broadcasts were phased in, starting in 2000 for the earliest district (Kampala) and completed by 2004.[18] Using the UNHS 1999 dataset, we are able to use data from before the broadcast started. By using the UNHS 2005 dataset, we can use data for when the MIS was fully operational. In addition, for a subset of eight districts, we have been able to collect information on the exact month in which broadcasts started.

4.1 Measuring the outcome variables (ρ, s, p, m)

In our sample of the UNHS 1999 and 2005 datasets, there are 7960 and 5733 farmers, respectively. Each farmer in the dataset produces one crop or more. We use crop level data which contains information on what type of crop that is produced, the quantity produced, the quantity sold, and the price for the quantity sold. We also have access to a subset of the MIS radio scripts that were used for the broadcasts. They show that there were some minor crops for which price information was collected but very seldom broadcast. We use the main MIS crops, defined as those that were reported in radio scripts on average at least once per month in 2004 and for

[17] Note that the effects should be interpreted with consideration to the general equilibrium effects associated with the price information broadcasts. As predicted by the model and shown by the empirical results, the urban market price will decrease due to increased supply when many farmers are informed. This will also affect the uninformed farmers and the farm-gate estimates should therefore be interpreted according to β_ρ, β_s, β_p under endogenized prices.

[18] We drop the capital Kampala from our sample since there are no rural farmers in Kampala.

which we have at least 1,000 data points (farmer plots) in the 2005 UNHS survey.[19] The main MIS crops constitue 76% of the reported MIS crop observations in the crop surveys.

We take a conservative approach with respect to outliers, all of which clearly seem to be a result of misreporting. Thus, we drop all price observations with a reported unit price (the survey contains information on the quantity produced of each crop, the quantity sold, and the total value of the sale) higher than the highest reported weekly market price across all MIS district market centers and we drop all price observations with a unit price below roughly 0.01US$ (which corresponds to dropping all observations below the 1th percentile of the distribution for each crop). We also drop observations with a higher quantity sold than harvested.

We use a similar rule to define control crops (non-MIS crops). Specifically the control crops are those crops for which the MIS did not collect data for and that constitute at least one percent of the reported non-MIS crops in the 2005 UNHS survey. We drop coffee since the Uganda Coffee Development Authority (UCDA) runs a similar radio program for coffee in the main coffee producing areas of Uganda.

For each crop, we construct an indicator variable equal to one if any of the output was sold, and zero otherwise, as well as the share of the output that was sold.[20] For farm-gate prices, we then calculate the per kilogram price by dividing the total price by the quantity sold (in kilograms).[21] We can then calculate the standardized farm-gate price.[22]

The UNHS 1999 and 2005 datasets also have information on whether the farmer owns a radio, whether the farmer sold directly to the district market center and a quiz testing the farmer's knowledge of agricultural technology. The latter variable consists of seven multiple answer questions.[23] We construct a variable measuring the fraction of correct answers by the farmer.

For urban market prices, we use the MIS data provided by Foodnet. By exploiting the data for the subset of eight districts where we know the month in which the broadcasts started, we can divide the ten districts into early and late MIS districts. The early districts received broadcasts starting in February 2001 and the late districts received broadcasts starting in September 2002.[24]

---

[19] 1,000 plots correspond to roughly 1% of the reported MIS crops. We also exclude Bananas/Matooke since the MIS only reported one type of Banana/Matooke price while the crop modules list three types of plantain banana crops (Matooke) and we were unable to separate for which one there were broadcasts.

[20] In Uganda, there are two growing seasons per year. For each UNHS dataset, the crop data contains information for each season.

[21] The datasets also contain information on type of buyer. In 2005, 70% of the output were sold to a private trader in the household's village, 16% directly to another consumer or neighbor/relative, 9% at the district's market center, and 5% to "other type of buyers". For simplicity, although the output is not always literally sold to a private trader at the farm-gate, we label the price of all transaction types as the "farm-gate price".

[22] That is, $p_{ij} = (\tilde{p}_{ij} - \bar{p}_j)/\sigma_j$, where $\tilde{p}_{ij}$ is the farm-gate price/kilogram in Uganda Shillings received for the sold quantity of crop j by household i; $\bar{p}_j$ is the mean farm-gate price in the sample, and σ_j is the corresponding standard deviation.

[23] Such as: Which of the following cassava planting methods provides better yields? 1. Vertically planted sticks; 2. Horizontally planted sticks; 3. Both; 4. Don't know.

[24] The early districts are Jinja, Kabale, Masindi, Mbarara, and Soroti. The late districts are

5 Empirical Strategy

This section outlines how we estimate the predictions of the model. We first present the coefficient of interest and the empirical challenges of estimating it. We then present our empirical strategy and specifications for how we estimate the effects.

5.1 Farm-Gate Outcomes: Predictions 1-3

We are interested in the effects of being informed about the district market price and estimating

$$y_i = \alpha + \beta info_i + \varepsilon_i. \tag{5.37}$$

where α is the average outcome y_i for an uninformed farmer, the effect of being informed, β, is given by (5.25) for the extensive margin, (5.26) for share sold, and (5.27) for the farm-gate price. The error term ε_i captures all other determinants of y_i and is orthogonal to $info_i$. We face two empirical challenges in identifying β. First, regarding measurement, ideally we want to measure whether the farmer is informed or uninformed. That is, we want to measure whether each farmer always knows the market price in the district market center. However, the UNHS 1999 and 2005 datasets do naturally not contain this information. Instead, we will use various measures of *access* to price information, as we will exploit variation in access to price information through radio broadcasts via the Market Information Service. That is, we will estimate the reduced form effects of having *access* to the Market Information Service on farmers' market outcomes. It is worth noting that as long as the effect of having access to the price information of the Market Information Service affects the outcome variables only through informing farmers about market prices, this will give us a lower bound of β.[25]

Second, regarding identification, estimation of β requires that *access* to the Market Information Service is uncorrelated with the other determinants of the outcome variables. The Market Information Service broadcasts information through local radio stations, and the UNHS datasets contain information on radio ownership of farmers. However, only using radio ownership as a measure of access to price information is unlikely to capture causal effects, since radio ownership is likely to be correlated with other determinants of market outcomes (e.g., living close to the urban centers, farmer wealth, agricultural productivity, etc). To identify the causal effects of access to the information, instead, we instead employ two differences-in-differences (DD) and triple-differences (DDD) estimations in various dimensions. Specifically, we study farming households with and without radio across space (MIS

Gulu, Mbale, and Tororo.

[25] In principle, access to information could be used as an instrument for being informed (if we had the data) in an IV-framework. The reduced form coefficient will give us a lower bound of β (under homogeneous effects). This is because the reduced form population coefficient $\beta^{rf} = \beta * \beta^{first} \leq \beta$, where $\beta^{first} \in (0, 1]$ is the first-stage population coefficient of access on being informed. It is also worth pointing out that from a policy perspective, β^{rf} is an interesting coefficient in its own right.

districts versus non-MIS districts) and between crops (MIS crops versus non-MIS crops). The main differences-in-differences specifications are

$$y_{idc} = \alpha + \delta radio_{idc} + \beta radio_{idc} \times infodistrict_d + \mu_{dc} + \varepsilon_{idc} \ , \qquad (5.38)$$

where y_{idc} is the outcome of farmer i in district d producing crop c.[26] The variable $radio_{idc}$ is a dummy variable equal to one if the farmer owns a radio, $infodistrict_d$ is a dummy equal to one if district d is an MIS district (i.e., collecting and broadcasting price information through radio). We use district-by-crop fixed effects μ_{dc}. Our key outcome variables are p_{idc}, the standardized farm-gate price per kilogram received for crop c by farmer i; s_{idc}, the share of output that is sold; and ρ, an indicator variable taking the value of 0 if $q_{ij} = 0$ and 1 otherwise (the extensive margin). We cluster the standard errors at the district level (there are 56 districts in the full sample). We use the 2005 UNHS dataset and by predictions 1-3 we expect: $\beta_p > 0$; $\beta_\rho > 0; \beta_s > 0$. To consistently estimate β, the following assumption is needed:[27]

Identifying assumption (equation 5.38): Farmer selection into radio ownership is homogeneous in districts with and without price information broadcasts.

If, for some reason, selection into radio ownership is not homogeneous in districts with and without the MIS, the identifying assumption required for estimating equation (5.38) is violated and the estimates will be biased. To address this possibility, we use both the 1999 and 2005 UNHS datasets and estimate the following equation from the sample of MIS districts only (i.e., $infodistrict_d = 1$)

$$y_{idct} = \alpha + \delta radio_{idct} + \theta year05_t + \beta radio_{idct} \times year05_t + \mu_{dc} + \varepsilon_{idc} \ , \qquad (5.39)$$

where y_{idc} is the outcome of farmer i in district d, producing crop c in year t; $year05_t$ is a dummy variable indicating the observation is from 2005, and zero if it is from 1999, and μ_{dc} is district-by-crop fixed effects. By predictions 1-3, we expect: $\beta_\rho > 0$; $\beta_s > 0$; $\beta_p \lesseqgtr 0$. To consistently estimate β, the identifying assumption is:[28]

Identifying assumption (equation 5.39): Farmer selection into radio ownership is time-invariant.

Although both the above assumptions may seem plausible, we might worry about selection of radio ownership being time-variant and different across districts with and without price information broadcasts. This might, for example, be the case if farmers

[26] Please notice the slight abuse of notation as the equation now refers to β as the reduced form effect of access to price information, rather than the effect of being informed in equation (5.37).

[27] We must also assume no information spill-overs. However, in the case of spill-overs where farmers without radio get the price information from talking to neighboring farmers with radio, we will understimate the true effects.

[28] In the exogeneity check section below, we assess this assumption by running placebo estimations using only district that never received MIS broadcasts.

with higher quality crops have a higher demand for price information, and some of the marginal farmers will have bought a radio in response to the introduction of MIS. More generally, farmers with radio in MIS broadcasting districts, as compared to farmers with radio in districts with no MIS broadcasts, might therefore have different unobserved characteristics in 2005. This would then violate the identifying assumptions and bias the results. To address this concern, we exploit variation across crops in triple-differences estimations. Since the MIS did only collect and broadcast information on some, but not all, crops, farmers with radio only received regular price information through radio for some crops. Therefore, we define two groups of crops: MIS crops and non-MIS crops. MIS crops are crops for which district prices were regularly reported on the MIS radio programs and include Maize, Beans, Groundnuts, Cassava, Millet and Sweet potatoes.[29] Non-MIS crops are crops on which the Market Information Service did not disseminate price information.[30] Importantly, since many farmers produce more than one crop, this strategy allows us to also use *farmer fixed effects*. That is, this controls for *any* unobserved farmer characteristic that homogeneously affects farmers' market activity. The main triple-difference specification is therefore similar to equation 21, with the added variation across crops and the use of farmer fixed effects,

$$y_{idc} = \alpha + \delta r \times ic_{idc} + \lambda id \times ic_{idc} + \beta r \times id \times ic_{idc} + \eta_c + \gamma_i + \varepsilon_{idc}, \tag{5.40}$$

where y_{idc} is the outcome of farmer i in district d, producing crop c. The variable r_{idc} is a dummy variable equal to one if the farmer owns a radio; id_{idc} is a indicator variable equal to one if district d is a MIS district (and zero otherwise); ic_{idc} is a indicator variable equal to one if the crop produced by farmer i is an MIS crop (and zero otherwise); η_c are crop fixed effects; and γ_i are farmer fixed effects. To test the predictions of the model, we will use the UNHS 2005 data. By predictions 1-3, we expect: $\beta_\rho > 0$; $\beta_s > 0$; $\beta_p \lessgtr 0$.

Identifying assumption (equation 5.40): Differential farmer selection into radio ownership across districts with and without price information broadcasts is only determined by farmer characteristics that are homogenous across crops.

[29] We coded all radio scripts in 2004 and coded all reports of crop prices. We then calculated the share of reports (out of all reports of crop prices) for each crop during 2004. The main food crops (see Uganda Bureau of Statistics, 2000) maize, beans, groundnuts, cassava, millet, and sweet potatoes were mentioned in the radio scripts 5% of the times, with prices for maize and beans being reported most often. The MIS project also regularly reported prices for Matooke [plantains or food bananas], but the agricultural module in the household survey data does not code plantains but several types of Matooke (Matooke food, Matooke beer, Matooke sweet), so we cannot link the two data sets for this crop. Note that the popular name for the plantain (food banana) is Matooke, which is also the name for the popular prepared dished of the plantain. Several crops were only reported a handful of times in some districts (less than 1%) during the year.

[30] We drop minor non-MIS crops defined as those crops that constitute less than 1 of the reported non-MIS crops in the 2004/2005 crop survey. The following crops are included: Avocado, Cowpeas, Cotton, Field peas, Onions Pawpaw, Peas, Pigeon peas, Pineapple, Plantation trees, Sugarcane, Tobacco, Tomatoes, Vanilla, and Yams. Prices on coffee were reported through UCDA radio broadcast and were consequently not included.

5.2 Farm-Gate Outcomes: Prediction 4

To further investigate the predicted mechanisms further, we test the auxiliary prediction that the effects of price information on the likelihood of selling and the share of the output sold are larger when there is more uncertainty about the market price. That is, when there is more variation in the market price it is more difficult for the farmer to predict the market price, which creates more information frictions between traders and farmers. We use the weekly data from the Market Information Service on district market prices for the MIS crops and calculate the coefficient of variation (CV) in market prices for each crop during the year 2004.[31] We then test the auxiliary prediction of the model by the following specification

$$y_{idc} = \alpha + \delta radio_{id} + \beta radio_{id} \times cvhigh_c + \mu_{dc} + \varepsilon_{idc} \ , \tag{5.41}$$

where y_{idc} is the outcome of farmer i in district d, producing crop c. The variable r_{idc} is a dummy variable equal to one if the farmer owns a radio; $cvhigh_d$ is a dummy variable equal to one if crop c is a high price variation crop (above the median in the distribution of coefficients of variation across MIS crops); and μ_{dc} are district-by-crop fixed effects. By prediction 4, we expect $\beta_\rho > 0$; $\beta_s > 0$; $\beta_p \lessgtr 0$. The specification allows for the selection of radio ownership to differ across farmers with different characteristics, as well as the selection of high and low price uncertainty crops to differ across farmers. The identifying assumption is

Identifying assumption (equation 5.41): The *joint* selection of radio ownership and crop with high price uncertainty is uncorrelated with the other determinants of farmers' supply outcomes.

Under this assumption, we can consistently estimate β. Naturally, examples where this assumption would be violated can be found. For example, risk aversion could be heterogeneous among farmers and may affect the contracting with traders. If risk averse farmers also select into crops with low price uncertainty, and are more likely to own a radio because of higher demand for information, then the assumption would be violated. To partially assess this assumption, we run separate regressions on the 1999 placebo sample (before the MIS started broadcasting) and the 2005 sample.[32] If the identifying assumption is correct, we expect $\beta = 0$ in the 1999 sample.

5.3 Prediction 5: Retail Market Price

Prediction 5 implies that when the share of informed farmers increases, the prices in the urban retail market should decrease due to increased supply. To test prediction

[31] The average coefficient of variation in crop market prices is 0.15 and the standard deviation is 0.085.

[32] Note that since we do not have any data on market prices for the crops that were not part of the Market Information Service, we cannot exploit variation across MIS and non-MIS crops.

5, we use the market price data from the urban retail markets and estimate the following two specifications[33]

$$m_{cdt} = \alpha + \beta idist_d \times started_{dt} + \gamma_t + \mu_{cd} + \lambda_d \times t_t + \varepsilon_{cdt} \ , \tag{5.42}$$

$$m_{cdt} = \alpha + \delta r_{cd} + \beta r_{cd} \times started_{dt} + \gamma_t + \mu_{cd} + \lambda_d \times t_t + \varepsilon_{cdt} \tag{5.43}$$

where m_{cdt} is the standardized price of crop t in the market center of district d, in week t. The variable $idist_d$ is a dummy variable indicating if the MIS district received broadcasts starting in February 2002, and zero if the MIS district did not receive broadcasts until after the sample period (the sample runs from August 2001 to August 2002); $started_{dt}$ is a dummy variable indicating post-January 2002, and zero otherwise; r is the percentage of farmers in district d and growing crop c that owns a radio in 1999; γ_t are time fixed effects (weeks); μ_{dc} are district-by-crop fixed effects; $\lambda_d \times t_t$ is a district linear time trend. By prediction 5, we expect $\beta < 0$ in both specifications.

Identifying assumptions : When estimating equation (5.42), the assumption is that in the absence of price information broadcasts, early and late MIS districts would have parallel trends in crop prices. Similarly, for equation (5.43), in the absence of broadcasts, the crop price in districts with a large fraction of farmers with radio would have a parallel trend to districts with a small fraction of farmers with radio.

5.4 Exogeneity Checks

To assess the identifying assumptions in equations (5.38), (5.39), and (5.40), we run a set of placebo tests before running the main regressions.

First, if the identifying assumption for (5.38) is correct, then there should be no effect before the price information broadcasts started. Therefore, we estimate equation (5.38) using the 1999 UNHS dataset. Second, if the identifying assumption for (5.39) is correct, there should be no effect over time in districts that never received the price information broadcasts. Table 2 shows the placebo tests of equations (5.38) and (5.39). Columns (1) and (2) report the estimates for farm-gate prices, columns (3) and (4) reports them for the share of output sold, and the estimates for the probability of selling are found in columns (5) and (6). They show that the differences-in-differences estimates in columns (1) - (6) are small and insignificant.

Third, by a similar logic, we estimate (5.40) using the 1999 UNHS dataset. If the identifying assumption is correct, there should be no differential effect across

[33] The urban market price data contains data both on off-lorry prices and retail prices. We use the off-lorry prices as these are what is paid to the traders. The two prices are naturally very similar: the correlation is 0.96.

crops before the MIS broadcasts started. Columns (1) - (6) in Table 2 report the estimates. We see that the triple-differences estimates are small and insignificant.

Finally, in order to assess the parallel trend assumption in equation (5.42), Figure 4 shows weekly average standardized market prices for early districts ($idist_d = 1$) and average standardized market prices for late districts ($idist_d = 0$), before (Phase 0) and after (Phase 1) the broadcast started in the early districts. We see that before the broadcast started, the market prices follow each other closely as there is no apparent difference in prices. Importantly, there is no evidence of a differential decreasing trend for early districts, relative to late districts. Consistent with prediction 5, we also see that after the broadcast started (Phase 1) in late districts, early districts have visibly lower prices than late districts. Figure 4 shows weekly prices up to and including August. In September, one of the late districts (Gulu) started broadcasting. To further assess the identifying assumption, Figure 5 also shows market prices for early districts over time, but instead of all late districts it only shows the average price for Gulu. We see that when Gulu receives broadcasts in September, the average price immediately converges to the average price of the early districts. This strongly suggests that the price differences were driven by the introduction of the broadcasts.

We also run placebo tests to assess the identifying assumptions of equations (5.41) and (5.43). We show the results of these estimations in the same tables as the treatment estimations (Tables 9 and 10).

Together, the exogeneity checks are consistent with the identifying assumptions, which lends some credibility to the empirical strategy.

6 Results

In this section, we present the regression results. We first show the results for the farm-gate outcomes and then we present the results for the urban market prices. Finally, we show the results for the auxiliary prediction on the heterogenous effects of market price uncertainty.

6.1 Predictions 1-3: Farm-Gate Outcomes

Predictions 1-3 imply that access to price information will decrease the asymmetric information between farmers and traders and lead to an increased likelihood of selling the crops and larger shares of the output sold. If the incentive effect dominates the selection effect, the effect on the farm-gate price is positive (and it is negative if the opposite holds).

Table 4 reports the results for the estimations of equations (5.38) and (5.39). Columns (1) and (2) show the results for farm-gate prices. In both specifications, the interaction coefficients are positive (0.132 and 0.138) and statistically significant (5% and 10%, respectively). Columns (3) and (4) depict the results for the share of output sold. Also here are the interaction coefficients in both specifications positive (0.167 and 0.174) and statistically significant (both at 5%). Columns (5) and (6)

depict the results for the extensive margin. The interaction coefficients in both specifications are positive (0.042 and 0.047) and statistically significant (5% and 1%, respectively). Together, the results are consistent with the predictions of the model: informed farmers are more likely to sell their crops (Prediction 1), they sell larger shares of their output (Prediction 2) and they obtain higher prices (Prediction 3). The fact that the farm-gate price increases is consistent with the incentive effect dominating the selection effect.

The validity on the estimates in Table 4 hinges on the identifying assumption necessary for estimating (5.38) and (5.39). If they are violated, for example because farmers with unobserved farmer characteristics differentially select into radio in districts with price information broadcasts, the estimates will be biased (and, perhaps, most likely upward). Next, we present the triple-difference estimates that exploit variation across crops. In the most restrictive specification, we use farmer fixed effects that allow for any unobserved farmer characteristic.

Table 5 shows the results for the triple-differences estimates on the likelihood of selling any output (the extensive margin). Using crop and district fixed effects, column (1) shows that the estimate **is** positive (0.086) and significant at the 1% level. Column (2) shows the results**,** adding district-by-crop fixed effects, and the triple-differences estimate is positive (0.079) and significant at the 1% level. Finally, the results using the most restrictive specification by adding farmer fixed effects show that the estimate is positive (0.066) and significant at the 10% level.

Table 6 shows the results for the triple-differences estimates on the share of output sold (the total margin). Using crop and district fixed effects, column (1) shows that the estimate is positive (0.357) and significant at the 5% level. Column (2) shows the results adding district-by-crop fixed effects, and the triple-differences estimate is positive (0.342) and significant at the 5% level. Finally, the results using farmer fixed effects show that the estimate is positive (0.276) and significant at the 10% level.

Table 7 shows the results for the triple-differences estimates on farm-gate prices. Using crop and district fixed effects, column (1) shows that the estimate is positive (0.343) and significant at the 1% level. Column (2) shows the results adding district-by-crop fixed effects, and the triple-differences estimate is positive (0.216) and significant at the 10% level. Finally, the results using farmer fixed effects show that the estimate is positive (0.407) and significant at the 10% level.

We can compare the triple-differences estimates using farmer fixed effects in Tables 5, 6, and 7, with the differences-in-differences estimates in Table 4. If the latter estimates were upward biased by unobserved farmer characteristics, we should see that using farmer fixed effects should decrease the estimate. Instead, we see that the estimates using farmer fixed effects are consistently somewhat higher: for the extensive margin (0.066, versus 0.042 and 0.047); for the total margin (0.276, versus 0.167 and 0.174) and for the farm-gate price (0.407, versus 0.132 and 0.138).

Taking the point estimates using the farmer fixed effects literally, access to price information increases the probability of selling by 6.6 percentage points. This is a quantitatively substantial effect, since the probability of selling is 22.6 percent in the baseline group (Using farmers in MIS districts producing crops for which price information was broadcast, i.e., InfoDistrict=1, Infocrop=1, Radio=0). In other words,

this corresponds to a 29.2% increase in the probability of selling. Furthermore, the point estimate in Table 6 implies that the share of the output sold increases by 31.8% (0.276 log points), using the same baseline (converted to levels, this implies going from 0.121 to 0.159). Finally, the effect on the farm-gate price is quantitatively important, since access to price information increases the farm-gate price by 0.41 standard deviations.

6.2 Prediction 4: Uncertainty about the Market Price

To further test the predictions, Table 8 presents results for the prediction that the effect of price information on the likelihood of selling and the share of the output sold is larger when there is more uncertainty about the market price (i.e., when the distribution is more spread out). That is, when there is more variation in the market price, it is more difficult for the farmer to predict the market price, which creates more information frictions between the trader and the farmer.

Columns (1), (3), and (5) first present placebo regressions using the 1999 UNHS dataset. If the identifying assumption underlying equation (5.41) is correct, there should be no differential effect of radio ownership for crops with a high market price variation before the price information broadcasts. Indeed, the interaction term is close to zero and insignificant in all three regressions. As predicted by the model, however, there is a differential effect after the broadcasts have started. Columns (2) and (4) show the point estimates for the extensive margin and the total margin, respectively. In both columns, the interaction term is positive and significant at the 10% level. Column (6) shows the result for the farm-gate price and the coefficient is positive but insignificant. That the coefficient is insignificant is no surprise given that the prediction of the farm-gate price is indeterminate. However, when the incentive effect dominates the selection effect, the effect on farm-gate prices is positive. The size and sign of the point estimate are at least suggestive of this being the case (as shown in Table 7).

6.3 Prediction 5: The Urban Market Price

Consistent with predictions 1-4, we have shown that price information increases the supply of crops. By prediction 5, this should shift the supply curve outward in the urban market, and lower the retail price. Table 9 presents the results for the urban market price. Columns (1) and (2) presents the results for equation (5.42). In both specifications, the interaction term coefficient is negative and significant at the 5% level. Using the full set of fixed effects in column (2), the point estimate implies that the market price decreases with 0.94 standard deviations. This is a substantial effect.

Furthermore, if the effects were truly driven by a supply effect from informed farmers listening to radio broadcasts, we should see that the effect of broadcasts is larger when there are more farmers with radio. Columns (3) and (4) present the results for equation (5.43). In both specifications, the interaction term coefficient is negative and significant at the 5% level. Using the full set of fixed effects in

column (4), the point estimate (-0.024) implies that the market price decreases by 0.24 standard deviations when there is a 10 percent increase in farmers with radio.[34]

Finally, to assess whether, for other reasons, there is a time trend in crops with a relatively large share of farmers with radio, we run a placebo test. Specifically, in columns (5) and (6) we estimate equation (5.43) over the *same* period but for districts that received price information broadcasts after the sample period (in September 2002). The interaction term is insignificant and positive in the placebo regressions. This suggests that the results in columns (3) and (4) were indeed driven by access to price information through the radio broadcasts.

6.4 Quantifying the Effects on Farmer Revenue

Consistent with the predictions of the model, we have shown that farmers with access to price information are more likely to sell their crops, sell larger shares of the output, and receive higher farm-gate prices. To quantify the total effect on crop revenue, we estimate equations (5.38), (5.39), and (5.40) with crop revenue (i.e., the quantity sold in kg times the price per kg) as the outcome variable. Columns (1) and (2) show that the interaction coefficient is positive (0.439 and 0.467, respectively) and significant (5% and 1%, respectively). The triple-differences estimates of equation (5.40) are presented in columns (3) - (5). They show that the point estimates are large (0.886, 0.782, and 0.699) and significant in all but the specification with farmer fixed effects (it is borderline insignificant as the p-value is 0.107).

Using the most conservative point estimate in column (1), this implies that farmers with access to price information have 55.1% (0.439 log points) higher crop revenues than farmers without access to price information. The results therefore indicate that access to price information has substantial effects on farmer crop incomes.

6.5 Investigating Alternative Explanations

In this section, we investigate other potential explanations for the results. First, we consider that the broadcast did not only affect outcome by providing price information. Instead, it may be that the radio programs also provided farmers with information that had direct effect on agricultural productivity, by teaching farmers about farming techniques. This could affect quantity sold, quantity produced, as well as the quality of the crops (which could increase the farm-gate price). We use the UNHS 2005 survey quiz on agricultural technology knowledge to test for the hypothesis that the broadcast informed farmers about farming technique. Column (1) presents the results using the fraction of correct answers on the quiz. We find no evidence on technology learning.

[34] The results show that the average market price decreases within the district. We find no evidence of a shift in the dispersion of market prices (results not shown). Due to incentive and selection effects, the model gives no clear predictions on other moments than the mean.

Second, we consider the alternative that the price information changed where the farmer sold their output. In principle, if risk averse farmers become informed about the market price, they may be more likelihood to travel directly to the district market to sell their crops. Columns (2) - (4) present the results. We find no evidence of a change in where the goods are sold.[35]

Finally, we investigate whether the price information made farmers produce more of the crops for which there was price information. If changing the composition of crops produced is costless for the farmer (by increasing the plot area for crops with price information and decreasing it for crops without price information), we would expect output to increase which, in turn, could affect the farm-gate outcomes. However, unless higher production is also associated with higher *share* of output sold, such a production effect would tend to work against finding an effect on s. Columns (5) - (7) show the results. We find no evidence of production behavior on average.[36]

7 Conclusion

This paper finds that price information plays an important role in facilitating market exchange. It also sheds some new light on some important policy questions.

First, how to boost agriculture production in developing countries has been an ongoing policy question. The question is of particular importance for countries in sub-Saharan Africa where the growth in agricultural yield has been stagnant. While the academic literature on the subject is extensive, existing research has primarily focused on two broad sets of explanations: the low technology adoption rate (of technologies such as HYV crops, irrigation and fertilizers) and the functioning of agricultural markets.

The issue of functioning markets was a prime concern behind the reforms of the agricultural markets in many sub-Saharan Africa countries in the late 1980s and 1990s. However, the supply response from liberalizing agricultural markets has been weaker than expected. One explanation that has been put forward for this low supply response is that the pre-liberalization period where the government essentially fixed a price for key food and cash crop commodities (often a price well below the market price) has been replaced by a situation where better informed (at least about local market conditions) local traders are able to force down prices to farmers with little idea of price movements and market trends. Our results are at least qualitatively consistent with this claim.

The effects of information on outcomes are interesting from an economic theory perspective. However, the effects are also relevant for the discussion about the role of information and communication technologies (ICTs) for economic development

[35] We run the same regressions using a dummy indicating if the farmer sold the crops to a private trader in the village. We find no evidence of changed behavior.

[36] This could be explained either by significant adjustment costs or by beliefs that the price information broadcasts would terminate. Also, we cannot rule out changes in output within the group MIS crops.

(cf. Jensen, 2007). Living standards for most of the world's poorest are largely determined on how much they get paid for their output, mainly crops. Thus, the functioning of output markets is central to the income for farmers engaged in agriculture in low-income countries. In most developing countries, markets are dispersed and the infrastructure is poor. Small-scale producers typically lack information on market prices, so that the potential for inefficiency in the allocation of goods across markets and the allocation between consumption and trading is large.

Moreover, asymmetric information between sellers (i.e. poor small-scale farmers) and buyers adds important distributional concerns. By improving the access to information, ICTs may help poorly functioning markets work better, improve farmers' bargaining positions, and thereby increase the incomes of the poor. In addition, our results suggest that urban consumers, through lower prices, indirectly benefit from the better functioning. However, access to price information seldom reaches everyone, and it is still an open question to what extent farmers with little access to information are affected when a large part of the rural population gets access to good information. Our results show that price information can have substantial general equilibrium effects, pushing prices downward. Whether poor farmers without access to information decrease their integration with markets and become even poorer as a consequence of lower prices is a potentially important question for future research.

Bibliography

[1] Aker, J., 2008, "Does Digital Divide or Provide? The Impact of Cell Phones on Grain Markets in Niger, Mimeo, University of California, Berkeley.

[2] Banerji, A. and Meenakshi, J.V., 2001, "Competition and Collusion in Grain Markets: Basmati Auctions in North India", Centre for Development Economics Working Paper 94, University of Delhi, India.

[3] Baye MR, J. Morgan, 2001, "Information gatekeepers on the Internet and the competitiveness of homogeneous product markets." *American Economic Review*, Vol 91(3), p. 454-474.

[4] Baye MR, J. Morgan, and P. Scholten, 2004, "Price dispersion in the small and in the large: Evidence from an internet price comparison site" *Journal of Industrial Economics*, Vol 52(4), p. 463-496.

[5] Bolton, P. and M. Dewatripont, 2005, *Contract Theory*, MIT Press.

[6] Brown JR, A. Goolsbee, 2002, "Does the Internet make markets more competitive? Evidence from the life insurance industry", *Journal of Political Economy*, Vol 110(3), p.481-507.

[7] Duflo, E., M. Kremer, and J. Robinson, 2006, "Understanding Technology Adoption: Fertilizer in Western Kenya, Preliminary Results from Field Experiments," mimeo, MIT.

[8] G. Ellison and S.F. Ellison, 2005, "Lessons About Markets From the Internet", *Journal of Economic Perspectives*, Vol. 19(2), p. 139-158.

[9] Ferris, S., 2004, "FOODNET: Information is Changing Things in the Marketplace", *ICT Update*, Issue 18, CTA, The Netherlands.

[10] Ferris, S. and P. Robbins, 2004, "Developing Marketing Information Services in Eastern Africa: The FOODNET Experience", International Institute of Tropical Agriculture (IITA), Ibadan, Nigeria.

[11] Ferris, S., P. Engoru, and E. Kaganzi, 2006, "Making Market Information Services Work Better for the Poor in Uganda", paper presented at the Research Workshop on Collective Action and Market Access for Smallholders, Cali, Columbia.

[12] Jensen, R. 2007, "The Digital Provide; Information (Technology), Market Performance, and Welfare in the South Indian Fisheries Sector, *The Quarterly Journal of Economics*, CXXII (3): 879-924.

[13] Ministry of Finance, 2008, "Uganda Vision 2035", National Planning Authority of the Republic of Uganda, Kampala.

[14] Mirrlees, J.A., 1971. "An Exploration in the Theory of Optimum Income Taxation," *Review of Economic Studies*, vol. 38(114), p. 175-208, April.

[15] RATES Center, 2003, "Maize Market Assessment and Baseline Study for Uganda, Center for Regional Agricultural Trade Expansion Support (RATES Center), Nairobi, Kenya.

[16] Stigler, G., 1961, "The Economics of Information", *Journal of Political Economy*, 69: 213–225.

[17] Svensson, J. and D. Yanagizawa, 2008, "Tuning in the Market Signal: Contracting and Efficiency under Asymmetric Price Information in Uganda Maize Markets", mimeo, IIES, Stockholm University.

[18] World Bank, 2007, World Development Report 2008: Agriculture for Development, IBRD, Washington DC.

Appendix: Sketch of the solution

The first-best is defined from the following maximization problem

$$\max_{q_i} \sum_{i=1}^{3} \pi_i \left[U_i + \Pi_i\right] \;\Rightarrow\; \max_{q_i} \left[u(Q - q_i) + m_i q_i\right]. \tag{5.44}$$

The first-order conditions, which implicitly define the first-best quantity sold function, q_i^{FB},

$$-u'(Q - q_i^{FB}) + m_i \leq 0 \quad \text{for all } m_i \tag{5.45}$$

equates the marginal utility of consumption with the market price. We assume that $u'(Q) < m_1$, implying that the first-order condition (5.45) is always binding and thus

$$q_i^{FB} = Q - u_c^{-1}(m_i)\ . \tag{5.46}$$

The uninformed farmer case: We can rewrite the problem from its most general form.[37] Note that only one of the IR constraints (5.4) is binding since

$$m_3 q_3 - R_3 \geq m_3 q_2 - R_2 \geq m_2 q_2 - R_2 \geq m_2 q_1 - R_1 \geq m_1 q_1 - R_1 \geq 0. \tag{5.47}$$

Exploiting the fact that the Spence-Mirrless single-crossing condition holds, we can reduce the number of incentive constraints to a smaller set of local downward incentive constraints and a monotonicity condition. The farmer's problem can thus be stated as

$$\max_{\{(q_i,R_i)\}} \sum_{i=1}^{3} \pi_i \left[R_i + u(Q - q_i)\right] \qquad \text{subject to} \tag{5.48}$$

$$m_1 q_1 - R_1 = 0 \tag{5.49}$$

$$m_i q_i - R_i = m_i q_{i-1} - R_{i-1} \quad \text{for all } i > 1 \tag{5.50}$$

$$q_i \geq q_j \text{ if } m_i \geq m_j. \tag{5.51}$$

To solve the constrained problem, we set up the Lagrangian, assuming that the monotonicity condition (5.51) holds. That is

$$\begin{aligned} L \;=\; & \max \sum_{i=1}^{3} \left\{\pi_i \left[R_i + u(Q - q_i)\right] + \lambda_i \left[m_i q_i - m_i q_{i-1} - R_i + R_{i-1}\right]\right\} \\ & + \mu \left[m_1 q_1 - R_1\right], \end{aligned} \tag{5.52}$$

[37] We suppress the superscripts for ease of exposure.

where λ_i is the Lagrange multiplier associated with the IC-constraint at price m_i, and μ is the multiplier associated with the IR-constraint (5.49). The first-order conditions for (q_1, R_1) are

$$\frac{dL}{dq_1} = -\pi_1 u'(Q - q_1) + \lambda_1 m_1 - \lambda_2 m_2 + \mu m_1 = 0 \tag{5.53}$$

and

$$\frac{dL}{dR_1} = \pi_1 - \lambda_1 + \lambda_2 - \mu = 0. \tag{5.54}$$

The first-order conditions for (q_2, R_2) are

$$\frac{dL}{dq_2} = -\pi_2 u'(Q - q_2) + \lambda_2 m_2 - \lambda_3 m_3 = 0 \tag{5.55}$$

and

$$\frac{dL}{dR_2} = \pi_2 - \lambda_2 + \lambda_3 = 0. \tag{5.56}$$

The first-order conditions for (q_3, R_3) are

$$\frac{dL}{dq_3} = -\pi_3 u'(Q - q_3) + \lambda_3 m_3 = 0 \tag{5.57}$$

and

$$\frac{dL}{dR_3} = \pi_3 - \lambda_3 = 0. \tag{5.58}$$

Rewriting yields the following conditions for q_i (where superscript UI stands for uninformed farmer)

$$-u'(Q - q_3^{UI}) + m_3 = 0 \tag{5.59}$$

$$-u'(Q - q_2^{UI}) + m_2 - \frac{\pi_3}{\pi_2}(m_3 - m_2) \leq 0 \tag{5.60}$$

$$-u'(Q - q_1^{UI}) + m_1 - \frac{(\pi_2 + \pi_3)}{\pi_1}(m_2 - m_1) \leq 0. \tag{5.61}$$

The monotonicity condition (5.51) holds if

$$\text{Assumption 1: } \frac{1}{\pi_1}(m_2 - m_1) \geq \frac{\pi_3}{\pi_2}(m_3 - m_2), \tag{5.62}$$

which we assume to be the case. Thus

$$q_1^{UI} = \begin{cases} Q - u_c^{-1}\left(m_1 - \frac{(\pi_2+\pi_3)}{\pi_1}(m_2 - m_1)\right) & \text{for } m_1 > \tilde{m}_1 \\ 0 & \text{for } m_1 \leq \tilde{m}_1 \end{cases} \tag{5.63}$$

$$q_2^{UI} = \begin{cases} Q - u_c^{-1}\left(m_2 - \frac{\pi_3}{\pi_2}(m_3 - m_2)\right) & \text{for } m_2 > \tilde{m}_2 \\ 0 & \text{for } m_2 \leq \tilde{m}_2 \end{cases} \tag{5.64}$$

$$q_3^{UI} = Q - u_c^{-1}(m_3) \tag{5.65}$$

where the threshold market prices $\tilde{m}_i$ are

$$\tilde{m}_2 \equiv \left(\frac{1}{1-\pi_1}\right)(\pi_2 u'(Q) + \pi_3 m_3) \tag{5.66}$$

$$\tilde{m}_1 \equiv \pi_1 u'(Q) + (1-\pi_1)m_2 \tag{5.67}$$

and $\tilde{m}_2 > \tilde{m}_1$ as long as assumption 1 holds.

Figure 1. Districts with the Market Information Service.

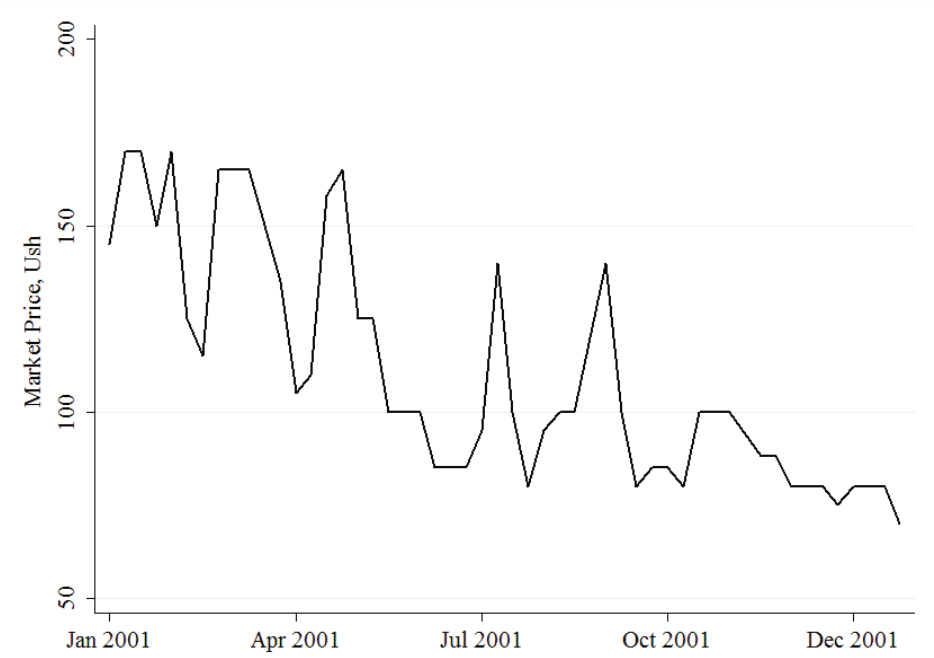


Figure 2. The price of cassava, Mbale district market, 2001.

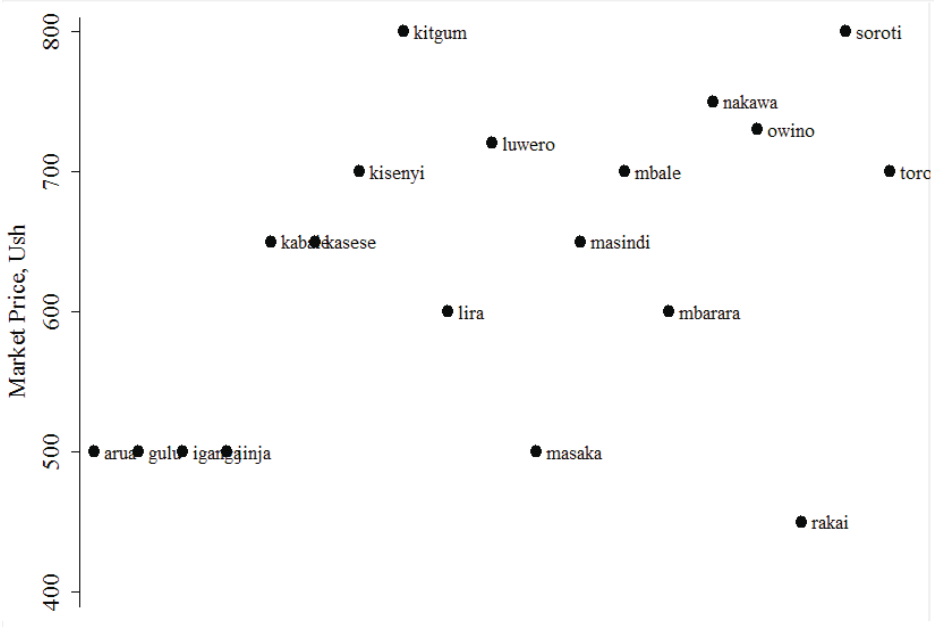


Figure 3. The price of beans across districts, week 20, 2001.

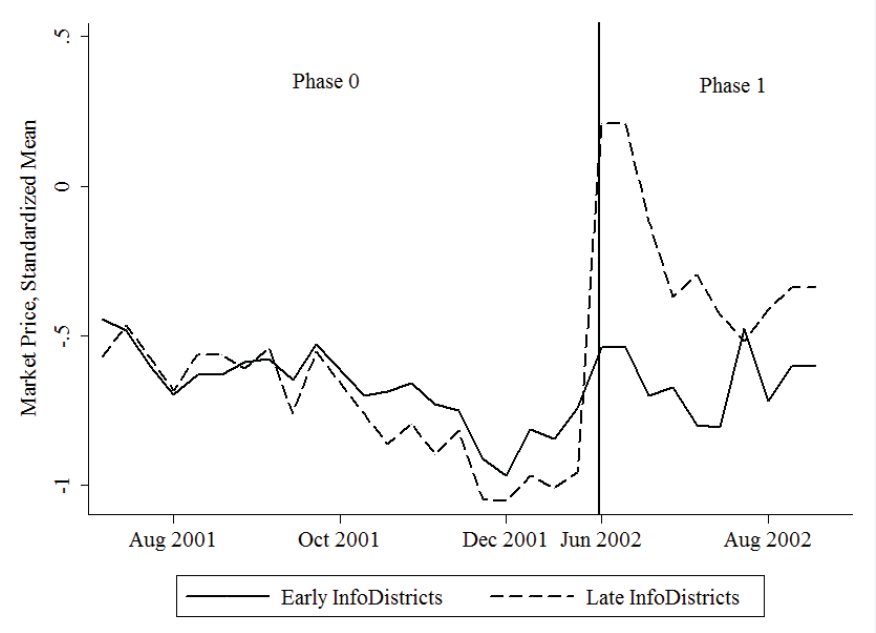


Figure 4A. The figure shows weekly mean standardized market price for the main MIS crops in early districts (Phase 1 broadcasts starting in Feb 2002) and late districts (after Aug 2002). There is missing data for Jan-May 2002 (compressed at the vertical bar).

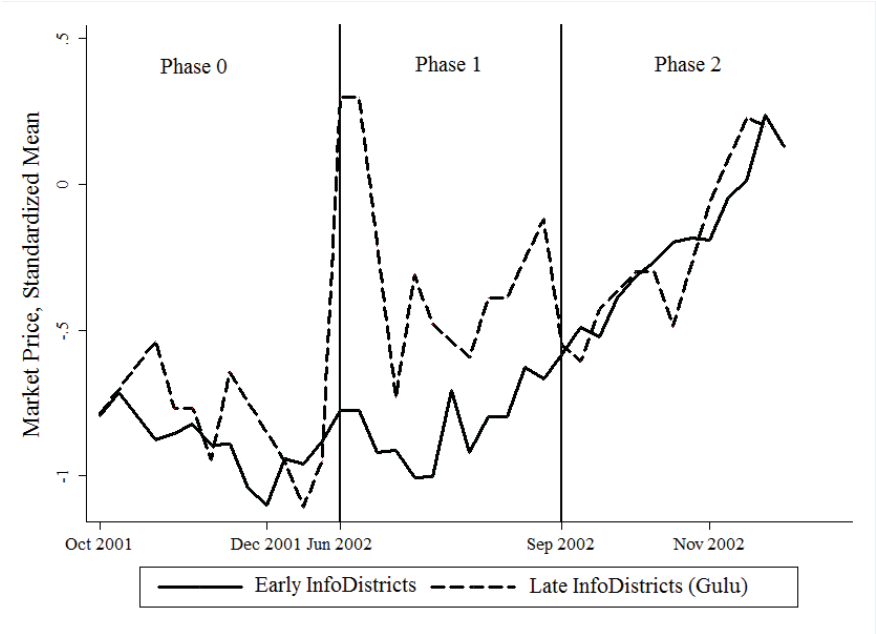


Figure 4B. The figure shows weekly mean standardized market price for the main MIS crops in early districts (Phase 1 broadcasts starting in Feb 2002) and Gulu district. Gulu received broadcasts starting in Sep 2002. There is missing data for Jan-May 2001.

Table 1. Summary Statistics

	Panel A: UNHS Crop Survey, 2005					Panel B: UNHS Crop Survey, 1999				
Variable	Obs	Mean	S.D.	Min	Max	Obs	Mean	S.D.	Min	Max
Selling any Output, Dummy	33049	0.28	0.45	0	1	22747	0.34	0.47	0	1
Share of Output Sold, Log	33049	0.17	0.31	0	1	22747	0.18	0.29	0	1
Farm Gate Price, Standardized	9109	0.07	1.03	-2.35	9.29	7352	-0.42	0.93	-2.35	7.46
Radio	33049	0.70	0.46	0	1	22747	0.59	0.49	0	1
InfoDistrict	33049	0.43	0.50	0	1	22747	0.47	0.50	0	1
InfoCrop	33049	0.92	0.26	0	1	22747	0.96	0.20	0	1
Output, kg	33049	251.7	458.8	0.04	4992.0	22747	254.1	429.9	.0015	4980.0
Revenue, Ush	32990	19787	80422	0	2500000	22672	13576	53354	0	2653500
Agricultural Tech. Knowledge,	32954	0.51	0.23	0	1	0				
Sold to District Market	9355	0.09	0.29	0	1	0				

	Panel C: Urban Market				
Urban Market Price, Standardized	1495	0.00	0.98	-3.43	3.63
InfoDistrict	1495	0.62	0.49	0	1
Broadcasting Started	1495	0.42	0.49	0	1
% Farmers with Radio	1241	56.43	16.61	22.86	100

Table 2. Farm Gate Outcomes, DD Placebo

Dependent Variable	Farm Gate Price		Share of Output Sold, Log		Selling any Output, Dummy	
	(1)	(2)	(3)	(4)	(5)	(6)
Radio	0.038	0.062**	0.049	0.026	0.010	0.004
	(0.030)	(0.029)	(0.051)	(0.044)	(0.012)	(0.011)
Radio x InfoDistrict	-0.008		0.080		0.019	
	(0.042)		(0.073)		(0.018)	
Year 2005		0.473***		-0.205***		-0.063***
		(0.078)		(0.077)		(0.020)
Radio x Year 2005		-0.038		0.087		0.024
		(0.048)		(0.057)		(0.015)
Observations	6665	7717	21771	28790	21771	28790
R-squared	0.335	0.201	0.182	0.114	0.174	0.108
Sample Districts	All	No Info	All	No Info	All	No Info
Sample Year	1999	1999/2005	1999	1999/2005	1999	1999/2005
Sample Crops	InfoCrops	InfoCrops	InfoCrops	InfoCrops	InfoCrops	InfoCrops
District-by-Crop FE	Yes	Yes	Yes	Yes	Yes	Yes

Radio is a dummy variable indicating if the household owns a radio. *InfoDistrict* is a dummy variable indicating if the household lives in a Market Information Service (MIS) district broadcasting market prices in 2005, and zero otherwise. *Year 2005* is a dummy variable equal to one if the household is from the UNHS 2005 (i.e., after the MIS started) dataset, and equal to zero if it is from the UNHS 1999 (i.e., before the MIS started) dataset. Sample districts equal to *All* includes both districts with the MIS in 2005 (InfoDistricts) as well as districts without the MIS in 2005 (No Info). *InfoCrops* indicates that the crops in the sample are crops which the MIS broadcasts market price information for. *Farm Gate Price* is the standardized price per kilogram for which the household sold the crop. *Selling any Output* is a dummy variable indicating if the household sold any of the crop output, and zero otherwise. *Share of Output Sold* is the amount sold in kilogram divided by the total output in kilogram. The data for the dependent variables comes from UNHS. Robust standard errors in parentheses, clustered at the district level (40 clusters) in columns 1,3,5 and at the district-crop level (197 clusters) in columns 2, 4, 6. *** p<0.01, ** p<0.05, * p<0.1.

Table 3. Farm Gate Outcomes, DDD Pre-MIS Placebo

Dependent Variable	Farm Gate Price, Standardized		Share of Output Sold, Log		Selling any Output, Dummy	
	(1)	(2)	(3)	(4)	(5)	(6)
Radio	0.027		0.040		0.013	
	(0.077)		(0.134)		(0.031)	
Radio x InfoDistrict	-0.119		0.068		0.008	
	(0.092)		(0.157)		(0.037)	
Radio x InfoCrop	0.026	-0.079	0.012	0.097	-0.002	0.017
	(0.070)	(0.121)	(0.142)	(0.139)	(0.034)	(0.031)
InfoDistrict x InfoCrop	0.084	0.160	0.004	0.133	0.009	0.052
	(0.150)	(0.198)	(0.197)	(0.273)	(0.054)	(0.072)
Radio x InfoDistrict x InfoCrop	0.104	0.138	-0.009	-0.137	0.005	-0.036
	(0.091)	(0.183)	(0.167)	(0.271)	(0.041)	(0.064)
Observations	7352	7352	22747	22747	22747	22747
R-squared	0.248	0.694	0.181	0.544	0.152	0.519
Sample Year	1999	1999	1999	1999	1999	1999
Sample Districts	All	All	All	All	All	All
Sample Crops	All	All	All	All	All	All
Crop FE	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Farmer FE	No	Yes	No	Yes	No	Yes

Radio is a dummy variable indicating if the household owns a radio. *InfoDistrict* is a dummy variable indicating if the household lives in a Market Information Service (MIS) district broadcasting market prices in 2005, and zero otherwise. The sample year is 1999 (i.e., before the MIS started). Sample districts equal to *All* includes both districts with the MIS in 2005 (InfoDistricts) as well as districts without the MIS in 2005 (No Info). *InfoCrop* indicates that the crop is a crop which MIS broadcast market price information for, and zero if the crop is one for which the MIS did not. *Farm Gate Price* is the standardized price per kilogram for which the household sold the crop. *Selling any Output* is a dummy variable indicating if the household sold any of the crop output, and zero otherwise. *Share of Output Sold* is the amount sold in kilogram divided by the total output in kilogram. The data for the dependent variables comes from UNHS. Robust standard errors in parentheses, clustered at the district level (40 clusters). *** p<0.01, ** p<0.05, * p<0.1.

Table 4. Farm Gate Outcomes, DD

Dependent Variable	Farm Gate Price		Share of Output Sold, Log		Selling any Output, Dummy	
	(1)	(2)	(3)	(4)	(5)	(6)
Radio	0.045**	0.036	0.094**	0.099**	0.021**	0.020
	(0.022)	(0.036)	(0.037)	(0.049)	(0.009)	(0.013)
Radio x InfoDistrict	0.132**		0.167**		0.042**	
	(0.053)		(0.067)		(0.017)	
Year 2005		0.502***		-0.392***		-0.116***
		(0.112)		(0.091)		(0.023)
Radio x Year 2005		0.138*		0.174***		0.047***
		(0.077)		(0.050)		(0.013)
Observations	7706	6654	30513	23494	30513	23494
R-squared	0.157	0.253	0.088	0.105	0.083	0.101
Sample Districts	All	InfoDistricts	All	InfoDistricts	All	InfoDistricts
Sample Year	2005	1999/2005	2005	1999/2005	2005	1999/2005
Sample Crops	InfoCrops	InfoCrops	InfoCrops	InfoCrops	InfoCrops	InfoCrops
District-by-Crop FE	Yes	Yes	Yes	Yes	Yes	Yes

Radio is a dummy variable indicating if the household owns a radio. *InfoDistrict* is a dummy variable indicating if the household lives in a Market Information Service (MIS) district broadcasting market prices in 2005, and zero otherwise. *Year 2005* is a dummy variable equal to one if the household is from the UNHS 2005 (i.e., after the MIS started) dataset, and equal to zero if it is from the UNHS 1999 (i.e., before the MIS started) dataset. Sample districts equal to *All* includes both districts with the MIS in 2005 (InfoDistricts) as well as districts without the MIS in 2005 (No Info). *InfoCrops* indicates that the crops in the sample are crops which the MIS broadcasts market price information for. *Farm Gate Price* is the standardized price per kilogram for which the household sold the crop. *Selling any Output* is a dummy variable indicating if the household sold any of the crop output, and zero otherwise. *Share of Output Sold* is the amount sold in kilogram divided by the total output in kilogram. The data for the dependent variables comes from UNHS. . Robust standard errors in parentheses, clustered at the district level (56 clusters) in columns 1,3,5 and at the district-crop level (100 clusters) in columns 2, 4, 6.
*** p<0.01, ** p<0.05, * p<0.1.

Table 5. Extensive Margin, DDD

Dependent Variable	Selling any Output, Dummy		
	(1)	(2)	(3)
Radio	0.027	0.014	
	(0.022)	(0.029)	
Radio x InfoDistrict	-0.044	-0.038	
	(0.035)	(0.041)	
Radio x InfoCrop	-0.009	0.009	-0.003
	(0.024)	(0.027)	(0.029)
InfoDistrict x InfoCrop	-0.062*		-0.035
	(0.031)		(0.037)
Radio x InfoDistrict x InfoCrop	0.086***	0.079**	0.066*
	(0.032)	(0.036)	(0.038)
Observations	33049	33049	33049
R-squared	0.117	0.160	0.357
Sample Year	2005	2005	2005
Sample Districts	All	All	All
Sample Crops	All	All	All
Crop FE	Yes	Yes	Yes
District FE	Yes	Yes	Yes
District-by-Crop FE	No	Yes	No
Farmer FE	No	No	Yes

Radio is a dummy variable indicating if the household owns a radio. *InfoDistrict* is a dummy variable indicating if the household lives in a Market Information Service (MIS) district broadcasting market prices in 2005, and zero otherwise. The sample year is 1999 (i.e., before the MIS started). Sample districts equal to *All* includes both districts with the MIS in 2005 (InfoDistricts) as well as districts without the MIS in 2005 (No Info). *InfoCrop* indicates that the crop is a crop which MIS broadcast market price information for, and zero if the crop is one for which the MIS did not. *Selling any Output* is a dummy variable indicating if the household sold any of the crop output, and zero otherwise. Robust standard errors in parentheses, clustered at the district level (56 districts).
*** p<0.01, ** p<0.05, * p<0.1.

Table 6. Share of Output Sold, DDD

Dependent Variable	Share of Output Sold, Log		
	(1)	(2)	(3)
Radio	0.118	0.056	
	(0.094)	(0.123)	
Radio x InfoDistrict	-0.187	-0.179	
	(0.153)	(0.182)	
Radio x InfoCrop	-0.038	0.040	-0.022
	(0.097)	(0.115)	(0.118)
InfoDistrict x InfoCrop	-0.260*		-0.147
	(0.130)		(0.151)
Radio x InfoDistrict x InfoCrop	0.357**	0.342**	0.276*
	(0.138)	(0.161)	(0.161)
Observations	33049	33049	33049
R-squared	0.143	0.188	0.377
Sample Year	2005	2005	2005
Sample Districts	All	All	All
Sample Crops	All	All	All
Crop FE	Yes	Yes	Yes
District FE	Yes	Yes	Yes
District-by-Crop FE	No	Yes	No
Farmer FE	No	No	Yes

Radio is a dummy variable indicating if the household owns a radio. *InfoDistrict* is a dummy variable indicating if the household lives in a Market Information Service (MIS) district broadcasting market prices in 2005, and zero otherwise. The sample year is 1999 (i.e., before the MIS started). Sample districts equal to *All* includes both districts with the MIS in 2005 (InfoDistricts) as well as districts without the MIS in 2005 (No Info). *InfoCrop* indicates that the crop is a crop which MIS broadcast market price information for, and zero if the crop is one for which the MIS did not. *Share of Output Sold* is the amount sold in kilogram divided by the total output in kilogram, clustered at the district level (56 districts). *** p<0.01, ** p<0.05, * p<0.1.

Table 7. Farm Gate Price, DDD

Dependent Variable	Farm Gate Price, Standardized		
	(1)	(2)	(3)
Radio	0.170**	0.093	
	(0.075)	(0.082)	
Radio x InfoDistrict	-0.211*	-0.084	
	(0.112)	(0.109)	
Radio x InfoCrop	-0.117	-0.048	-0.144
	(0.085)	(0.091)	(0.113)
InfoDistrict x InfoCrop	-0.057		-0.136
	(0.178)		(0.276)
Radio x InfoDistrict x InfoCrop	0.343***	0.216*	0.407*
	(0.119)	(0.112)	(0.243)
Observations	9109	9109	9109
R-squared	0.053	0.165	0.481
Sample Year	2005	2005	2005
Sample Districts	All	All	All
Sample Crops	All	All	All
Crop FE	Yes	Yes	Yes
District FE	Yes	Yes	Yes
District-by-Crop FE	No	Yes	No
Farmer FE	No	No	Yes

Radio is a dummy variable indicating if the household owns a radio. *InfoDistrict* is a dummy variable indicating if the household lives in a Market Information Service (MIS) district broadcasting market prices in 2005, and zero otherwise. The sample year is 1999 (i.e., before the MIS started). Sample districts equal to *All* includes both districts with the MIS in 2005 (InfoDistricts) as well as districts without the MIS in 2005 (No Info). *InfoCrop* indicates that the crop is a crop which MIS broadcast market price information for, and zero if the crop is one for which the MIS did not. *Farm Gate Price* is the standardized price per kilogram for which the household sold the crop. Robust standard errors in parentheses, clustered at the district level (56 districts). *** p<0.01, ** p<0.05, * p<0.1.

Table 8. Heterogeneous Effects: Market Price Uncertainty

Dependent Variable	Selling any Output, Dummy		Share of Output Sold, Log		Farm Gate Price, Standardized	
	(1)	(2)	(3)	(4)	(5)	(6)
Radio	0.034**	0.050***	0.149***	0.199***	0.006	0.115*
	(0.013)	(0.014)	(0.051)	(0.056)	(0.041)	(0.060)
Radio x Price Uncertainty High	-0.019	0.031*	-0.072	0.143*	0.067	0.150
	(0.027)	(0.019)	(0.104)	(0.074)	(0.079)	(0.103)
Observations	9974	12876	9974	12876	3234	3284
R-squared	0.166	0.072	0.176	0.077	0.321	0.175
Sample Year	1999	2005	1999	2005	1999	2005
Sample Districts	InfoDistricts	InfoDistricts	InfoDistricts	InfoDistricts	InfoDistricts	InfoDistricts
Sample Crops	InfoCrops	InfoCrops	InfoCrops	InfoCrops	InfoCrops	InfoCrops
Crop FE	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes
District-by-Crop FE	Yes	Yes	Yes	Yes	Yes	Yes

Radio is a dummy variable indicating if the household owns a radio, from the UNHS datasets. *Price Uncertainty High* is a dummy variable indicating if the coefficient of variation for the market price of the crop in the district for which the household lives in is above the average, and zero if it's below. The coefficient of variation in district market prices is calculated using data from Foodnet for the period of the UNHS 2005 sample. *Farm Gate Price* is the standardized price per kilogram for which the household sold the crop. *Selling any Output* is a dummy variable indicating if the household sold any of the crop output, and zero otherwise. *Share of Output Sold* is the amount sold in kilogram divided by the total output in kilogram. The data for the dependent variables comes from UNHS. Robust standard errors in parentheses, clustered at the district-crop level.
*** p<0.01, ** p<0.05, * p<0.1.

Table 9. Urban Market Prices

Dependent Variable	Market Price, Standardized					
	(1)	(2)	(3)	(4)	(5)	(6)
InfoDistrict	0.254*					
	(0.128)					
InfoDistrict x Broadcasting Started	-0.657**	-0.940**				
	(0.304)	(0.376)				
% Farmers with Radio			0.010**		-0.011	
			(0.004)		(0.021)	
% Farmers with Radio x Broadcasting Started			-0.023**	-0.024**	0.020	0.020
			(0.009)	(0.011)	(0.030)	(0.030)
Observations	1495	1495	854	854	387	387
R-squared	0.120	0.133	0.151	0.153	0.259	0.259
Sample Districts	All	All	Info	Info	No Info	No Info
No of District-Crop Markets	48	48	27	27	12	12
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Crop FE	Yes	Yes	Yes	Yes	Yes	Yes
District Market FE	No	Yes	Yes	Yes	Yes	Yes
District-Crop Market FE	No	Yes	No	Yes	No	Yes
District Market Trend	No	Yes	Yes	Yes	Yes	Yes

The sample contains data at the district-crop-week level from the Market Information Service for the period August 2001 to August 2002, with missing data for the period January to May 2002. Each district has one urban market center. *InfoDistrict* is a dummy variable indicating if the district for which the MIS started broadcasting information for in February 2002, and zero if it is a district for which the MIS started broadcasting in September 2002. *Broadcasting Started* is a dummy indicating if the week is after February 2002, and zero if it is before February 2002. *% Farmers with Radio* is the percentage of farmers in the district and growing the crop that own a radio in 1999. Data sources: Foodnet and UNHS 1999. Robust standard errors in parentheses, clustered at the district-crop market level in columns (1) – (4). Due to a low number of clusters, columns (6) and (7) use Newey-West standard errors with two period (weeks) lags. *** p<0.01, ** p<0.05, * p<0.1.

Table 10. Crop Revenue

Dependent Variable	Crop Revenue, Log				
	(1)	(2)	(3)	(4)	(5)
Radio	0.342***	0.348***	0.596**	0.368	
	(0.095)	(0.122)	(0.241)	(0.315)	
Year 2005		-0.995***			
		(0.228)			
Radio x InfoDistrict	0.439**		-0.430	-0.343	
	(0.168)		(0.392)	(0.455)	
Radio x Year 2005		0.467***			
		(0.130)			
Radio x InfoCrop			-0.303	-0.025	-0.264
			(0.261)	(0.294)	(0.320)
InfoDistrict x InfoCrop			-0.701**		-0.403
			(0.339)		(0.390)
Radio x InfoDistrict x InfoCrop			0.886**	0.782*	0.699
			(0.366)	(0.413)	(0.426)
Observations	30513	23494	32990	32990	32990
R-squared	0.087	0.105	0.130	0.174	0.370
Sample Districts	All	Info	All	All	All
Sample Year	2005	1999/2005	2005	2005	2005
Sample Crops	InfoCrops	InfoCrops	All	All	All
Crop FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	No
District-by-Crop FE	Yes	Yes	No	Yes	No
Farmer FE	No	No	No	No	Yes

Radio is a dummy variable indicating if the household owns a radio. *Year 2005* is a dummy variable equal to one if the household is from the UNHS 2005 (i.e., after the MIS started) dataset, and equal to zero if it is from the UNHS 1999 (i.e., before the MIS started) dataset. *InfoDistrict* is a dummy variable indicating if the household lives in a Market Information Service (MIS) district broadcasting market prices in 2005, and zero otherwise. The sample year is 1999 (i.e., before the MIS started). Sample districts equal to *All* includes both districts with the MIS in 2005 (InfoDistricts) as well as districts without the MIS in 2005 (No Info). *InfoCrop* indicates that the crop is a crop which MIS broadcast market price information for, and zero if the crop is one for which the MIS did not. Robust standard errors in parentheses, clustered at the district level (56 clusters) in all columns except column (2), where there are only 17 districts. Column (2) uses clustering at the district-crop level (100 clusters). *** p<0.01, ** p<0.05, * p<0.1.

Table 11. Alternative Explanations

Dependent Variable:	Agricultural Technology Knowledge	Sold Directly to the District Market			Output, Log		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Radio	0.029**	0.022***	-0.001		0.387***	0.326***	
	(0.012)	(0.007)	(0.039)		(0.028)	(0.084)	
Radio x InfoDistrict	0.007	0.004	0.026		-0.009	-0.024	
	(0.015)	(0.014)	(0.062)		(0.057)	(0.131)	
Radio x InfoCrop			0.023	-0.006		0.061	-0.104
			(0.040)	(0.046)		(0.085)	(0.092)
InfoDistrict x InfoCrop				0.003			-0.048
				(0.069)			(0.181)
Radio x InfoDistrict x InfoCrop			-0.022	-0.006		0.015	0.085
			(0.064)	(0.066)		(0.132)	(0.128)
Observations	30474	7920	9355	9355	30563	33049	33049
R-squared	0.212	0.092	0.126	0.603	0.280	0.292	0.500
Sample Districts	All	All	All	All	All	All	All
Sample Year	2005	2005	2005	2005	2005	2005	2005
Sample Crops	InfoCrops	All	All	All	InfoCrops	All	All
Crop FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District-by-Crop FE	Yes	Yes	Yes	No	Yes	Yes	No
Farmer FE	No	No	No	Yes	No	No	Yes

Radio is a dummy variable indicating if the household owns a radio. *InfoDistrict* is a dummy variable indicating if the household lives in a Market Information Service (MIS) district broadcasting market prices in 2005, and zero otherwise. The sample year is 1999 (i.e., before the MIS started). Sample districts equal to *All* includes both districts with the MIS in 2005 (InfoDistricts) as well as districts without the MIS in 2005 (No Info). *InfoCrop* indicates that the crop is a crop which MIS broadcast market price information for, and zero if the crop is one for which the MIS did not. *Agricultural Technology Knowledge* is the fraction of correct answers from the UNHS 2005 survey on agricultural technology knowledge. Selling to a Trader is a dummy variable equal to one if the farmer sold the crop to a private trader in the farmer's village, and zero otherwise. Robust standard errors in parentheses, clustered at the district level (56 clusters). *** p<0.01, ** p<0.05, * p<0.1.

MONOGRAPH SERIES

1. Michaely, Michael *The Theory of Commercial Policy: Trade and Protection*, 1973
2. Söderström, Hans Tson *Studies in the Microdynamics of Production and Productivity Change*, 1974
3. Hamilton, Carl B. *Project Analysis in the Rural Sector with Special Reference to the Evaluation of Labour Cost*, 1974
4. Nyberg, Lars and Staffan Viotti *A Control Systems Approach to Macroeconomic Theory and Policy in the Open Economy*, 1975
5. Myhrman, Johan *Monetary Policy in Open Economies*, 1975
6. Krauss, Melvyn *International Trade and Economic Welfare*, 1975
7. Wihlborg, Clas *Capital Market Integration and Monetary Policy under Different Exchange Rate Regimes*, 1976
8. Svensson, Lars E.O. *On Competitive Markets and Intertemporal Resources Allocation*, 1976
9. Yeats, Alexander J. *Trade Barriers Facing Developing Countries*, 1978
10. Calmfors, Lars *Prices, Wages and Employment in the Open Economy*, 1978
11. Kornai, János *Economics of Shortage*, Vols I and II, 1979
12. Flam, Harry *Growth, Allocation and Trade in Sweden. An Empirical Application of the Heckscher-Ohlin Theory*, 1981
13. Persson, Torsten *Studies of Alternative Exchange Rate Systems. An Intertemporal General Equilibrium Approach*, 1982
14. Erzan, Refik *Turkey's Comparative Advantage, Production and Trade Patterns in Manufactures. An Application of the Factor Proportions Hypothesis with Some Qualifications*, 1983
15. Horn af Rantzien, Henrik *Imperfect Competition in Models of Wage Formation and International Trade*, 1983
16. Nandakumar, Parameswar *Macroeconomic Effects of Supply Side Policies and Disturbances in Open Economies*, 1985
17. Sellin, Peter *Asset Pricing and Portfolio Choice with International Investment Barriers*, 1990
18. Werner, Ingrid *International Capital Markets: Controls, Taxes and Resources Allocation*, 1990
19. Svedberg, Peter *Poverty and Undernutrition in Sub-Saharan Africa: Theory, Evidence, Policy*, 1991
20. Nordström, Håkan *Studies in Trade Policy and Economic Growth*, 1992
21. Hassler, John, Lundvik, Petter, Persson, Torsten and Söderlind, Paul *The Swedish Business Cycle: Stylized facts over 130 years*, 1992
22. Lundvik, Petter *Business Cycles and Growth*, 1992
23. Söderlind, Paul *Essays in Exchange Rates, Business Cycles and Growth*, 1993

24. Hassler, John A.A. *Effects of Variations in Risk on Demand and Measures of Business Cycle Comovements*, 1994
25. Daltung, Sonja *Risk, Efficiency, and Regulation of Banks*, 1994
26. Lindberg, Hans *Exchange Rates: Target Zones, Interventions and Regime Collapses*, 1994
27. Stennek, Johan *Essays on Information-Processing and Competition*, 1994
28. Jonsson, Gunnar *Institutions and Incentives in Monetary and Fiscal Policy*, 1995
29. Dahlquist, Magnus *Essays on the Term Structure of Interest Rates and Monetary Policy*, 1995
30. Svensson, Jakob *Political Economy and Macroeconomics: On Foreign Aid and Development*, 1996
31. Blix, Mårten *Rational Expectations and Regime Shifts in Macroeconometrics*, 1997
32. Lagerlöf, Nils-Petter *Intergenerational Transfers and Altruism*, 1997
33. Klein, Paul *Papers on the Macroeconomics of Fiscal Policy*, 1997
34. Jonsson, Magnus *Studies in Business Cycles*, 1997
35. Persson, Lars *Asset Ownership in Imperfectly Competitive Markets*, 1998
36. Persson, Joakim *Essays on Economic Growth*, 1998
37. Domeij, David *Essays on Optimal Taxation and Indeterminacy*, 1998
38. Flodén, Martin *Essays on Dynamic Macroeconomics*, 1999
39. Tangerås, Thomas *Essays in Economics and Politics: Regulation, Elections and International Conflict*, 2000
40. Lidbom, Per Pettersson *Elections, Party Politics and Economic Policy*, 2000
41. Vestin, David *Essays on Monetary Policy*, 2001
42. Olofsgård, Anders *Essays on Interregional and International Political Economics*, 2001
43. Johansson, Åsa *Essays on Macroeconomic Fluctuations and Nominal Wage Rigidity*, 2002
44. Groth, Charlotta *Topics on Monetary Policy*, 2002
45. Gancia, Gino A. *Essays on Growth, Trade and Inequality*, 2003
46. Harstad, Bård *Organizing Cooperation: Bargaining, Voting and Control*, 2003
47. Kohlscheen, Emanuel *Essays on Debts and Constitutions*, 2004
48. Olovsson, Conny *Essays on Dynamic Macroeconomics*, 2004
49. Stavlöt, Ulrika *Essays on Culture and Trade*, 2005
50. Herzing, Mathias *Essays on Uncertainty and Escape in Trade Agreements*, 2005
51. Bonfiglioli, Alessandra *Essays on Financial Markets and Macroeconomics*, 2005

52. Pienaar, Natalie *Economic Applications of Product Quality Regulation in WTO Trade Agreements*, 2005
53. Song, Zheng *Essays on Dynamic Political Economy*, 2005
54. Eisensee, Thomas *Essays on Public Finance: Retirement Behavior and Disaster Relief*, 2005
55. Favara, Giovanni *Credit and Finance in the Macroeconomy*, 2006
56. Björkman, Martina *Essays on Empirical Development Economics: Education, Health and Gender*, 2006
57. Larsson, Anna *Real Effects of Monetary Regimes*, 2007
58. Prado, Jr., Jose Mauricio *Essays on Public Macroeconomic Policy*, 2007
59. Tonin, Mirco *Essays on Labor Market Structures and Policies*, 2007
60. Queijo von Heideken, Virgina *Essays on Monetary Policy and Asset Markets*, 2007
61. Finocchiaro, Daria *Essays on Macroeconomics*, 2007
62. Waisman, Gisela *Essays on Discrimination and Corruption*, 2008
63. Holte, Martin Bech *Essays on Incentives and Leadership*, 2008
64. Damsgaard, Erika Färnstrand *Essays on Technological Choice and Spillovers*, 2008
65. Fredriksson, Anders *Bureaucracy, Informality and Taxation: Essays in Development Economics and Public Finance*, 2009
66. Folke, Olle *Parties, Power and Patronage: Papers in Political Economics*, 2010
67. Yanagizawa Drott, David *Information, Markets and Conflict: Essays on Development and Political Economics*, 2010