

The Power of Propaganda:
The Effect of U.S. Government Bias on Cold War News
Coverage of Human Rights Abuses

(Preliminary and Incomplete)

August 28, 2009

Abstract

This paper investigates the extent to which strategic objectives of the U.S. government influenced news coverage during the latter part of the Cold War (1976-88). We establish two reduced form relationships: 1) strategic objectives of the U.S. government causes the State Department to under-report human rights violations of political allies; and 2) these objectives reduce news coverage of human rights abuses for political allies in U.S. newspapers. To establish causality, we exploit plausibly exogenous variation in a country's strategic value to the U.S. from the interaction of its political alliance to the U.S. and membership on the United Nations Security Council. In addition to the main results, we provide evidence suggesting that government manipulation is the main underlying mechanism; and discuss the implications for the Besley and Prat (2006) model of media capture. (P16 Political Economy, L82 Media)

“The most critical special operations mission we have... today is to persuade the American people that the communists are out to get us... If we win the war of ideas, we will win everywhere else.” – J. Michael Kelly, Deputy Assistant Secretary of the Air Force said in a seminar the National Defense University attended by Oliver North, 1983.

“The need for high-quality reporting is greater than ever. It’s not just the journalist’s job at risk here. It’s American democracy.” – Walter Cronkite in a speech at Columbia University, January, 2007.

1 Introduction

Governments can clearly manipulate the information reported by media outlets that it own.¹ Its ability to do so with independently owned outlets operating in a competitive market is less obvious. Theoretical studies have argued that market competition and independent ownership are safeguards against government manipulation of the media (Besley and Prat, 2006). However, the scope for manipulation of a *free* press, in practice, is an empirical question. To the best of our knowledge, no study has yet addressed this important question.² This is somewhat surprising given the many historical controversies over the media being biased by government information (e.g. *Iran-contra* during mid 1980s; and more recently, in 2003, preceding the U.S. invasion of Iraq), and the growing body of evidence finding that the media has real political, economical and social consequences.³

This study attempts to fill this gap by examining the effect of government propaganda on

¹For example, a recent study by Durante and Knight (2009) finds that television stations owned by Italian president Silvio Berlusconi shifted the content of their reports towards the agenda of his party when his party came to power.

²Existing empirical evidence supports the prediction that independent ownership and competition are beneficial towards overcoming government manipulation. They include survey evidence on the correlation between government ownership of the media and reduced political and economic freedoms (Djankov et al., 2001); the finding that access to independent television stations in Russia increases the likelihood of voting for opposition parties (Enikolopov, Petrova and Zhuravskaya, 2009); and that competition in the U.S. increases the likelihood that news organizations will report the truth (Prat and Stromberg, 2005; Gentzkow, Glaeser and Goldin, 2006).

³Recent studies have shown that media can affect voting behavior (Prat and Stromberg, 2005; Gentzkow, 2006; Della Vigna and Kaplan, 2007; and Chang and Knight, 2008), 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).

commercial print news coverage in the United States of America during the latter part of the Cold War, 1976-88. 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 (e.g. Djankov et al., 2001).⁴ Despite of this, the government’s relationship with the media is rather controversial. On the one hand, most Americans believe that the media acts as a check on the government. The media’s importance is officially enshrined into the U.S. Consitution. On the other hand, practitioners and scholars of journalism have documented many incidents of when the U.S. government successfully influenced media reports such as when the U.S. State Department’s *Office of Public Diplomacy* (OPD), in the 1980s, leaked false information to the media, intimidated journalists and editorial boards who were seen as hostile, and rewarded journalists percieved as friendly with increased access to government information.⁵ Note that in this paper, we use the terms U.S. “government” and “State Department” interchangeably to mean the executive office and its administration. when, in reality, the government is formed of many branches with potentially widely varying interests. Similarly, we will use the term “public opinon” to refer to voters and Congress.

We face several empirical difficulties. First, it is difficult to measure propaganda. Second is the problem of reverse causality. Does propaganda affect the news? Or do government officials read the news and adjust propaganda in reaction to media coverage?⁶ Finally is the problem of omitted variables bias. News coverage and government propanganda may both be outcomes of a third factor, such as public opinion. For example, in the months before the 2003 invasion of Iraq, the U.S. government may have unfavorably biased reports of human rights situations in Iraq to garner support for the war while the news media may have slanted their reports to satisfy a popular anxiety about the Middle East after 9/11.⁷ In this case,

⁴The government provides funding to the Voice of America, which only broadcasts overseas, and National Public Radio.

⁵Blanton (2001) provides an overview account of all the actions taken by the OPD during the Reagan Administration (1980-88). Critics such as Noam Chomsky have gone as far as comparing 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* (Chomsky, 2002: p. 139) .

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

⁷Media *slant* refers to when profit-maxmizing news organizations write to affirm the beliefs of its readers, who have positive utility in such affirmation. Mullainathan and Shleifer (2005) argue that this increases the dispersion in news coverage across media outlets. Gentzkow and Shapiro (2006, 2007b) extends the argument and provide empirical evidence. Our study is about the effect on one newspaper and not the effect on the

the OLS will show that U.S. government bias and news coverage are highly correlated. But the correlation will reflect both the effect of the U.S. government and the preferences of the readers and hence will overstate the true effect of government bias.

The principal contribution of this study is to address these problems. First, to measure government propaganda, we use the difference in reports of human rights violations between what is published in the State Department’s (USSD) *Country Reports on Human Rights Practices* relative to what is published in analogous reports by Amnesty International. Specifically, we will use the difference in the Political Terror Scale (PTS) scores for each country and year. These are quantitative scores assigned to each report by political scientists in the University of North Carolina (Gibney and Dalton, 1996). We interpret the difference in PTS scores between the USSD and Amnesty as U.S. government *bias*, the difference in overall attitude between the two agencies, which in this paper is synonymous to propaganda. This interpretation assumes that differences in the different scores are driven by the U.S.; it does not assume that Amnesty reports the truth. Second, to establish causality, we exploit the plausibly exogenous variation in government bias that results from a political ally’s entry onto the United Nations Security Council (UNSC). We measure alliance using the fraction of votes a country votes in agreement with the U.S. in the General Assembly (UNGA). The U.S. values alliance, and this value increases when allies enter the UNSC and vote on more critical issues. We argue that the interaction of the two factors is exogenous to public opinion. Note that 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 cannot be used as an instrument for Country Reports. Instead, we estimate two different causal relationships, the “first stage” effect of an increase in a country’s strategic value to the U.S. on the difference in the USSD and Amnesty PTS scores; and the “reduced form” effect of an increase in a country’s strategic value to the U.S. on the number of stories of abuses in U.S. newspapers (the *New York Times*, *Washington Post*, *Wall Street Journal*, *Chicago Tribune* and *Los Angeles Times*). Following existing studies on the media, we assume that newspapers only print accurate facts. Hence, the distortion occurs to the composition of stories on human rights abuses. For example,

distribution of news across outlets. We are unable to examine the effect on the distribution across newspapers because historical data on articles only cover five newspapers in total for the period of our study. That said, the empirical results will show that the amount of distortion is not correlated with Conservative Ratings or slant indices, which suggest that consumer driven slanting is unlikely to be driving the results.

a newspaper can choose to publish a story on human rights abuses by the government of Nicaragua, which opposed the U.S. during the 1980s, versus the government of El Salvador, which was a staunch ally.

To interpret the interaction effect of alliance and Council membership as the causal effect of an increase in a country's strategic value to the U.S, we need to assume that the effects are driven by changes in U.S. strategic value rather than other factors. We are able to check this assumption by comparing the effect during the Cold War to after the Cold War, when the U.S.'s values for allies arguably decreased. Note that in order to interpret the difference between U.S. and Amnesty reports as U.S. bias, we can further relax the previous assumption to one where Amnesty does not bias its reports for U.S. allies when they enter and exit the UNSC in exactly the opposite direction as the U.S.⁸ We check this by estimating the effect of the interaction effect of alliance and Council membership on Amnesty and USSD PTS separately. We can also check that the timing of the effect of the interaction term corresponds to entering and exiting from the Council. We estimate the effect of UNSC membership (for allies) for each year for the period spanning from two years before entry until two years after exit from the Council. Our strategy is robust to the possibility that readers are more interested in news about allies and expect the media to monitor the bad behavior of allies when they enter the Council. Such preferences would bias against our finding that allies' entry on the Council cause newspapers to decrease reports of bad behavior. See the section on empirical strategy for a more detailed discussion.

Our strategy has the advantage in that it establishes the causal effect of government strategic objectives on news coverage. However, it does not allow us to identify the specific channels through which the government achieves this influence (e.g. government publications, interviews between government employees with journalists, etc). To shed light on this point, we rely on the rich qualitative evidence from documented incidents of government manipulation during the period of interest.

We compile data from several existing sources to form country level panel. The results show that an increase in strategic value to the U.S. decreased USSD reports of human rights abuses, and news coverage of abuses in the commercial press. For Cold War allies such as

⁸We also assume that countries do not actually improve human rights practices when they enter the UNSC in a way that is positively correlated to alliance with the U.S. See section on empirical strategy for a more detailed discussion.

Brazil, Zaire, Honduras and Chile, UNSC membership during the Cold War decreased State Department reports of human rights violations by 0.03, 0.2., 0.31 and 0.53 index points relative to Amnesty International. For newspaper reports of abuses in these countries, Council membership decreased coverage by 20.21%, 58.2%, 59.81% and 76.31%. These results support the belief that the government favors its allies and influences the press. For illustrative purposes, we make the rather implausible assumption that government propaganda is conveyed to journalists entirely through State Department Country Reports and estimate a 2SLS estimate of the effect of the Country Reports. We find that on average, a one index point reduction in reports of abuse by the USSD relative to Amnesty reduces newspaper coverage by approximately 91%. During the Cold War, the U.S. on average under-reported human rights abuses by 0.34 index points relative to Amnesty. Thus, Cold War strategic objectives of the U.S. caused newspapers to under-report human rights abuses by approximately 57%. Interestingly, we find that all of the distortions are driven by the NYT, *Washington Post*, and WSJ, organizations with arguably highest reputation for national and international news reporting. There is no evidence of distortion for the *L.A. Times* or *Chicago Tribune*.

There are several caveats for the interpretation. First, since articles in the newspapers of our sample could be correlated with articles in other newspapers, this should be conservatively interpreted to reflect the value of suppressing one article in each U.S. newspaper. Second, in addition the government capture, the main results may also be driven by other forces such as information asymmetries or consumer preferences. To investigate the hypothesis that the distortion in news coverage is facilitated by the fact that the government has more resources for obtaining information than commercial news outlets, we examine whether the main results are larger in magnitude when it is more costly for newspapers to obtain independent information. We find no evidence to support this channel. To investigate whether Council membership of allies directly affects consumers' interest in news about these countries, we repeat the main estimation for overall news coverage (e.g. not just human rights news). We find no effect.

To understand the broader implications of the empirical findings, we apply them to the model of media capture by Besley and Prat (2006). Under this framework and given the large number of independently owned media outlets in the U.S., our results imply that there are probably significant fixed entry costs to national and international news reporting. In practice, this means that the relevant media market is actually much smaller than the total

number of outlets in the U.S. Together with the finding that the distortion is driven by the newspapers with the highest reputation for news coverage, this is consistent with a scenario where reputation requires a fixed investment; and once acquired, reputation decreases the incentives to avoid government influence by segmenting the market and mitigating the positive market forces for accurate reporting.

This study makes several contributions. First, it provides novel evidence for an important fact: even a *free* press can be manipulated by the government. Second, we add to the growing literature on the effects of country's strategic value to the U.S. government in shaping U.S. policy. Thus far, the studies in economics have focused on the impact on foreign aid (e.g. Alesina and Dollar, 2000; Kuziemko and Werker, 2005).⁹ We broaden the scope of the literature by examining the effects on favorable human rights reporting, and investigating the extent to which they are transmitted to the public (via the media). Finally, we provide a measure of government bias and a source of plausibly exogenous variation that can be easily used by future researchers in economics and political science.

This paper is organized as follows. Section 2 provides qualitative evidence on how the government during the time of this study attempted to control the media. Section 3 discusses the background of the Cold War and the United Nations. Section 4 describes the data used in this paper. Section 5 presents the empirical strategy. Section 6 presents the results. Section 7 discusses the theoretical implications using the framework of media capture provided by Besley and Prat's (2005). And section 8 offers concluding remarks.

2 Public Diplomacy

2.1 “White Propaganda”

There are many recorded incidents of the U.S. governments' attempts to influence the media. We focus our discussion on the 1980s, the period most relevant for this study. During

⁹Kuziemko and Werker (2005) examines the effect of UNSC membership during year that is particularly important to the U.S. (for plausibly exogenous reasons) on U.S. bilateral aid receipts. We estimate the effect of UNSC membership for Cold War allies on human rights reports from the U.S. State Department and commercial media coverage of human rights abuses. The two studies are similar in that they both explore the benefits of being strategically valuable to the U.S. Our study adds to Kuziemko and Werker (2005) in that we examine a different outcome of government favoritism and investigate the extent to which government actions are transmitted to the public (via the media).

this time, the Reagan administration (e.g. White House, State Department, etc.) had an aggressive position against communism, used various tactics to persuade the media to report news favoring its political allies and against its political opponents. This section summarizes the political motivation and the details of known incidents of manipulation in an attempt to provide qualitative evidence for the mechanisms underlying government manipulation. Most of the information is based on documents recently released to the public and stored at the *National Security Archives* (Blanton, 2002). t

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). While the U.S. government had historically established domestic propaganda ministries during wartime, the Reagan administration was the first to establish such an office during peacetime. The explicit purpose of the office was to manipulate public and congressional opinion to garner support for the President's strong anti-communist agenda in a "public action" program (Parry and Kornblub, 1988).

"..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) said in 1983.

The plan for how to increase support for action against non-allies and turn public opinion against them is made clear in a recently declassified "action plan" from 1985. It highlights the importance of portraying allies as religious freedom fighters; while pointing out that non-allies are puppets of the Soviets who commit a long list of human rights violations. To emphasize the importance of the latter in disgracing its opponents, the memo lists potential human rights offenses (e.g. forced conscription, persecution of the church). 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 to influence Congress members. It can manipulate the word-of-mouth information by having select information be read aloud into Congressional record by sympathetic members of Congress, by arranging meetings be-

tween sympathetic experts and Congress members, or in the extreme plant false witnesses for personal testimony in congressional committee hearings.¹⁰ Information can also 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.¹¹ The explicit purpose of the reports are to serve as “a resource for shaping policy, conducting diplomacy and making assistance, training and other resource allocations”.¹² While Congress is the primary audience targeted by these reports, they are open to the public and therefore also available to journalists. In this paper, we will use quantitative scores of these reports relative to the scores of similar reports from Amnesty International to measure the government’s “bias” for or against a country (aka government attitude).¹³ There is also evidence that governments of foreign countries read State Department reports of abuses in their countries.¹⁴

The OPD used a variety of methods to influence the media. First, it strategically released information useful for supporting its point of view, and suppressed information that opposed it. Disinformation was often released directly by the OPD. In a letter to House Speaker Patrick Buchanan, the Deputy Director for *Public Diplomacy for Latin American and the Caribbean* (S\LDP), 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 op-eds were planted in the *New York Times* and the *Washington Post* (Fascell, 1987). Another example is when on the night of Ronald Reagan’s re-election, Otto Juan Reich,

¹⁰In 1985 during a testimony to a hearing of the House Foreign Affairs Subcommittee on Western Hemisphere Affairs, Tom Dowling arrived as a Roman Catholic Priest, and denounced Sandinista human rights abuses to counter testimony of other religious figures about *contra* abuses. In 1985-86, Brigham Young University student Wesley Smith published human rights reports alleging Sandinista atrocities. Later it was discovered that Dowling was not an ordained priest and both he and Smith were paid by operatives working for Oliver North (Parry and Kornclub, 1988).

¹¹<http://www.state.gov/g/drl/rls/hrrpt/>

¹²See 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>

¹³On average, we find that the number of stories on human rights abuses in the NYT increases discretely on in the three days following a release of the UUSD Country Reports (see Appendix Figure A1). A similar increase of smaller magnitude is found for teh three days following the release of Amnesty reports.

¹⁴For example, in 1977, the Brazilian government in power (and the opposition party at the time) responded angrily to a copy of the report on human rights abuses in Brazil that was handed to the Embassy in D.C. by the State Department. See “Brazil Cancels Military Aid Treaty Over U.S. Report on Human Rights” by The Associated Press, New York Times, Mar 12, 1977.

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).¹⁵

Second, 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. In a letter from Secretary of State, George Schultz, to President Reagan, Schultz discussed how Reich spent several hours with the producers of CBS and successfully convinced them to change a forthcoming news report on Cuba to favor the administration (Schultz, 1984). When news reports appear that did not conform to the wishes of the OPD, its officials would press the owners and editorial boards to change their journalists in the field. Bill Buzenberg, the foreign affairs correspondent at *National Public Radio* during the 1980s recalled that Reich said that he had “made similar visits to other un-named newspapers and major television networks [and] had gotten others to change some of their reporters in the field because of a perceived bias”. Similarly, U.S. embassy officials boasted in 1982 that they had forced *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 introduce skepticism into the information they reported. Many were accused of being disloyal to the United States or having a secret agenda. 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 contrast, journalists seen as cooperative to the administration’s agenda were rewarded with increased access to government information. An OPD memo stated that certain favorable correspondents had “open invitations for personal briefings” (Cohen, 2001). Some of the information provided by the briefings may have later proved to be false. But the exclusive nature of this access presumably made it valuable to journalists nevertheless.

In this study, we follow previous studies on the U.S. media in assuming that media outlets

¹⁵The OPD flooded the media, academic institutions and other interested groups with information. For example, in just 1982, the OPD booked more than 1,500 speaking engagements with editorial boards, radio, and television interviews, 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).

will only want to report accurate facts because proven inaccuracy could cause a costly loss of reputation.¹⁶ In the context of news reports on human rights reports, the margin for distortion is along the composition of stories (e.g. report that the Sandinistas are committing human rights atrocities versus U.S.’s ally, El Salvador).¹⁷ We further assume that journalists and editors are aware of the objectives of the government in distorting news reports to be favorable towards its allies. Governments can reward cooperative media outlets by providing them with increased access to valuable stories, and punish uncooperative ones by cutting off access (or more extreme measures).

2.2 The Value of Good Human Rights Practices

From the documents of the Iran-contra investigation, we learn that one of the ways to shape public and congressional opinion against opponents was to exaggerate human rights abuses in those countries and emphasize that they are “evil”. Conversely, the government attempted to increase support for political allies by calling them “freedom fighters”, “religious” or simply “good” (Jacobwitz, 1985). The documents from the Iran-contra investigation suggest that a large part of motivation was to garner public and congressional support for U.S. operations that aimed to overthrow the ruling government. This is consistent with the fact that in our data, received amounts of U.S. foreign aid is strongly correlated with USSD reports of good human rights behavior (whereas multilateral aid, Official Development Assistance, is not. See Appendix Table A2). It is also consistent with the long history of incidents where the U.S. government withdrew aid or imposed trade sanctions on countries because of human rights

¹⁶Advertising revenues suggest that reporting foreign news does not directly generate much profit for newspapers. For example, according the *New York Times Co. 2008 Annual Report*, advertising revenues from the domestic news section formed 12% of its total company revenues. In contrast, revenues from the international news section formed only 1% of total company revenues. However, it has been long believed that foreign news reporting serves as a signal of the overall quality of the paper. And therefore, it may indirectly increase the profits of the NYT by adding to its reputation. Many have pointed out that foreign news reporting is very expensive for newspapers relative to reporting domestic news. However, it may not be as costly if the number of stories are weighted by some measure of importance. For example, the foreign news section of *The Baltimore Sun* used 5% of the company’s total annual budget but produced over 25% of its front page stories that year (Caroll, 2007).

¹⁷This 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.

violations. For example, in 1977, Congress insisted the aid to Uruguay be made in installments contingent on improvements in human rights. Uruguay's displeasre at this was voiced when it "spurned" this aid.¹⁸ Less than two weeks later, the Brazilian government turned down the Carter administration's offer to seek 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. Apparently this move was extremely popular with the public and even the opposition party in Brazil.¹⁹ More recently, in June, 2008, when 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".²⁰

These examples illustrate two important facts. First, a perception of having good human rights practices is valuable because it is often tied to aid. Second, it is also valuable to the governments of foreign countries for non-pecuniary reasons. For example, governments may find it humiliating to be officially chastised by a foreign government and find that it decreases their prestige domestically. Alternatively, abuses described in the Country Reports may be a source of information for people inside the country being reported on, and this information could be used against the government in power. For the purposes of this paper, we take it as given that countries value favorable external reports on its human rights practices.

3 The Cold War and Alliance in the United Nations

3.1 The Cold War

The "Cold War", which began after World War II in 1945 and lasted until 1989/91, refers to the continous political conflict, military tension and economic competition between the USSR

¹⁸"Argentina and Uruguay Reject U.S. Assistance Linked to Human Rights" by Juan de Onic, *The New York Times*, Mar 2, 1977.

¹⁹See "Brazil Cancels Military Aid Treaty Over U.S. Report on Human Rights" by The Associated Press, *New York Times*, Mar 12, 1977.

²⁰Letters to the Editor, *Washington Post*, Monday, June 9, 2008; Page A16. Qian and Yanagizawa (2008) provide more examples. Aid from multilateral agencies and FDI have also been found to be negatively correlated with human rights abuses (Lebovic and Voeten, 2009; Blanton and Blanton, 2002). Part of the value of favorable report on human rights practices may also be driven by non-pecuniary motives. For example, hosting certain international events such as the Olympics is often viewed as a way of raising the prestige of governments of developing countries. And human rights abuses is frequently used as a cause for disqualifying countries from hosting.

and its satellite states (consolidated by the Warsaw Pact 1955-91) and the United States and Western Hemisphere allies (e.g. NATO, established 1949). Direct military attacks on adversaries were deterred by the potential for mutually assured 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 U.S., under the *Marshall Plan*, sought the containment and rollback of communism and forged myriad alliances to this end; and 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 U.S. and the U.S.S.R. for the alliance of smaller countries ended. Past studies have argued that the U.S. favors 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 (2008) 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 U.S. and against the U.S.S.R. in the United Nations General Assembly) during the Cold War; and that this favoritism dissipates with the end of the Cold War.

A famous anecdotal example of how the U.S.'s value for its strategic allies changed when the Cold War ended is Zaire (renamed the Democratic Republic of Congo in 1997), who's president, Mobutu Sese Seko (in office 1965-1997) was a strong supporter of the U.S. during the Cold War. During a state visit to the U.S. in 1983, U.S. 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 Cold War ended, the State Department began to criticize Zaire's human rights violations. And in 1993, Mobutu was denied a visa for visiting the U.S. At that time, he remarked

“I am the latest victim of the Cold War, no longer needed by the U.S. The lesson is that my support for American policy [now] counts for nothing” (Gbadolite, 2001).

3.2 The United Nations and the Security Council

In this section, we briefly describe the value of strategic allies to the U.S. in the United Nations, and how Security Council membership of an ally increases its value to the U.S. The United Nations General Assembly (UNGA) is one of the five principal organs of the United Nations and the only one in which all member nations have equal representation. Its powers are to oversee the budget of the United Nations, appoint the non-permanent members to the Security Council, receive reports from other parts of the United Nations and make recommendations in the form of General Assembly Resolutions.²¹ The General Assembly votes on many resolutions brought forth by sponsoring states. Most resolutions, while symbolic of the sense of the international community, are not enforceable as a legal or practical matter. The General Assembly does have authority to make final decisions in some areas such as the UN budget. And many resolutions may also be constitutive or proof of international customary law, and therefore binding on member states. More importantly, in the case of a split vote in the UNSC (and no veto), then the issue goes to the UNSC for a vote. The belief that voting with the U.S. in the UNGA is valuable to the U.S. is consistent with the empirical finding that such votes are correlated with the amount of foreign aid received from the U.S. (Alesina and Dollar, 2000), and the favorable under-reporting of human rights violations by the U.S. State Department (Qian and Yanagizawa, 2008).

The UNSC is comprised of fifteen member states, who are elected onto the council by the member countries of their region. 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).²² There are ten temporary seats that are each held for two-year terms, each one beginning on January 1st. Five are replaced each year. The members are elected by regional groups and confirmed by the United

²¹In 1945, the UN had 51 members. It now has 192, of which more than two-thirds are developing countries. For many developing countries, the UN is the source of much of their diplomatic influence and the principal outlet for their foreign relations initiatives.

²²This was the basis for UN armed action in Korea in 1950 during the Korean War.

Nations General Assembly.²³ There are five permanent members (P5): China, France, Russia, the United Kingdom, and the United States. These members hold veto power for blocking adoption of a resolution. However, they cannot block the debate of a resolution.

Rotating members have little power when one of the P5 exercises veto power (Winter, 1996; O’Niell, 1996). In practice, however, veto power is not frequently exercised, especially during the Cold War. For example, deadlocks, which can only occur if no member of the P5 vetos a resolution, have occurred ten times in the history of the UN. Nine of those occurred during the Cold War.²⁴ This is supported by Voeten (2001), who models bargaining power within the UNSC, and shows that even though members of the P5 such as the U.S. have unilateral power in veto-ing resolutions, they prefer multilateral agreements. The claim that temporary seats have strategic value is supported by the observation that there is often intense competition for seats (Malone, 2000). The claim that it has particular value to the U.S. is supported by recent empirical findings that rotating membership increases U.S. foreign aid (Kuziemko and Werker, 2006; Dreher et al., 2008).²⁵ Kuziemko and Werker (2005) argue that membership on the UNSC results in increased bilateral aid from the U.S. for the same reasons that membership on U.S. congressional committees results in increased federal spending for members’ constituencies.²⁶ They argue that the U.S. increases aid to member countries with the intention of buying their votes. This logic can be easily extended to the General Assembly, which as we have argued above, can also provide votes valuable to the U.S. However, since

²³Africa elects three members; Latin America and the Caribbean, Asian, and Western European and others blocs choose two members each; and the Eastern European bloc chooses one member. Also, 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.

²⁴1956 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).

²⁵Kuziemko and Werker (2005) study the effect of being on the UNSC during a year that is strategically important to the U.S. government on foreign aid receipts from the U.S. They measure the importance of a year using the number of articles in the NYT in that year which contains the phrase “United Nations” and “Security Council”. Our study differs from theirs in that our outcomes is the number of articles (on human rights) written in the NYT. Our variation for strategic value to the U.S. government will instead come from the interaction of a country’s alliance with the U.S. in the General Assembly and whether it is on the UNSC.

²⁶For empirical studies of U.S. congressional committees, see for example, Ferejohn (1974), Ray (1981), Groseclose and Stewart (1998), Stewart and Groseclose (1999), Levit and Snyder (1997), Rundquist and Carsey (2002); and Knight (2005). For theoretical studies, see for example, Riker (1962), Shepsle (1974), Stratmann (1992), and Groseclose and Snyder (1996). An overview of the parallels between this literature on practices of U.S. Congressional Committees and those of the United Nations Security Council is provided by Kuziemko and Werker (2005).

UNGA members have less power *per se* than UNSC members (e.g. they vote on critical resolutions less frequently), it follows that while the U.S. has positive value for allies in both bodies of the UN, the value is higher in the UNSC. The decrease in the U.S.’s value of allies when the Soviet Union dissipated provides us with an opportunity of checking our assumption that the entry of a U.S. ally onto the UNSC increases the U.S.’s strategic value of that country. If this is the case, then the benefits from Council membership for allies should be significantly less during the post-Cold War Period.

Models of political patronage, where re-election is not a concern, predict that politically motivated “pork-barrel” redistribution will be targeted towards constituencies that provide more support for the ruling party.²⁷ Applied to the context of the UN, this implies that the amount of favors a country receives from the U.S. will be monotonically increasing in its alliance.

4 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 did not have membership in the U.N. before 1991, and they are unlikely candidates for U.S. alliance. South Africa is not in the sample because it was excluded from UN activities due to UN opposition to Apartheid. The five permanent members of the UNSC are also excluded.²⁸ We further restrict the sample to country-year observations where the index is available for both Amnesty International and the U.S. State Department.

²⁷Models of redistribution typically attribute politically motivated redistribution to either patronage or tactical (e.g. re-election) motives. Models of the former predict that areas in which the ruling party enjoys more support will receive disproportionately more resources because politicians reward their supporters irrespective of electoral goals. Models of the latter predict that resources will be disproportionately allocated towards core supporters or swing constituencies (see for example, Snyder, 1989; Dixit and Londregan, 1996; Cox and McCubbins, 1986). Empirical studies on the effect of tactical redistribution produces conflicting results (see for example, Dahlberg and Johanssen, 2002; Case, 2001; Miguel and Zaidi, 2003; Dasgupta, Dhillon, and Dutta, 2003; Khemani, 2004; and Cole, 2009). The patronage motivation is more relevant in our context since the U.S., as a permanent member of the Security Council, does not face re-election concerns. To be conservative, before we estimate the main specification where we assume the benefits of alliance are linear, we estimate a flexible equation where we allow the effect of alliance for UNSC members to vary. See section on empirical strategy.

²⁸In 1978, China’s seat on the UNSC was transferred from Taiwan to the People’s Republic of China. Neither will be in our sample.

Finally, following Kuziemko and Werker (2005), we focus our study on developing countries by restricting the sample to countries that are not classified as high income countries as defined by the World Bank.²⁹ Our matched sample contains 104 countries for thirty years.

For measuring alliance, we follow Qian and Yanagizawa (2008) in using the fraction of votes with the U.S. on UNGA resolutions for which the U.S. and U.S.S.R. are divided (e.g. vote in opposing directions).³⁰ Figure 1 plots the number of divided votes over time. It shows that as Cold War tensions escalated in the 1980s, the number of divided votes increased. Also plotted are the fraction of votes with the U.S. averaged over all the divided votes each year. To calculate alliance, we include absentions. However, excluding them does not significantly change either the measure of alliance or the regression results. For brevity, we do not report those in the paper. Our main measure of alliance is the fraction of votes a country voted with the U.S. averaged over the period 1985-89. This period provides us with the highest number of divided votes, and therefore 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.³¹ Using this measure, 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). 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 U.S. more than the median country. The median country voted with the U.S. approximately 7% of the time. Note that we use the same measure of alliance for the Cold War period. This makes interpreting the effects for that period slightly complicated as there was a large shift in alliance from the USSR to the U.S. after the dissolution of the Soviet Union. We do not make a separate measure of alliance based on voting patterns during the post-Cold War period because there were may fewer divided issues and the change in the nature of international

²⁹High income countries are defined to be those with 2007 GNP per capita of \$11,456 or more. See <http://web.worldbank.org/WBSITE/EXTERNAL/DATASTATISTICS/0,,contentMDK:20421402~pagePK:64133150~piPK:>

³⁰Each 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.

³¹Another reason for why we do not use a time-varying measure is that there are some countries who abstain from voting in some years. Using the fraction of votes with the U.S. when there are few divided votes will give a very noisy measure of alliance. 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.

relations when the world went from having two superpowers to one “hegemon” mean that the same mechanism measure could have very different meaning. Therefore, our results for the post Cold War period should be interpreted very cautiously as suggestive evidence and not literally as a “placebo” or a third difference.

Data on UNSC membership is collected for the time period 1976-2005 from *The United Nations Security Council Membership Rollster*.³² 46 countries in the sample were on the UNSC as a rotating member at least once during this time. 21 countries were on the Council at least twice. And five countries were on the Council three times.

Human rights in the context of this study refers specifically to physical violence committed by the state onto civilians.³³ Two of the main sources of information for human rights are the United States State Department and Amnesty International. While intelligence units of other governments certainly have their own information about human rights situations in foreign countries, the United States is the only country that systematically releases its reports to the public. Similarly, Amnesty International is the only non-governmental organization which makes systematic reports over the same broad scope and long time horizon.³⁴ 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 United Kingdom 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 pro-Western bias. Amnesty proclaims itself as an independent organization.³⁵ Both of the USSD and Amnesty use the same definition for human rights abuses as set forth by the *Universal Declaration of Human Rights*.³⁶ And they publish reports using

³²See http://www.un.org/sc/list_eng5.asp for list of all countries that were ever members and the years of their memberships.

³³This is the definition used by Freedom House, the PTS project, and the CIRI project.

³⁴Amnesty is the only non-government human rights group that covers the entire world. The other is *Human Rights Watch*(HRW), a U.S. based organization. However, the HRW does not systematically publish yearly country reports. And their existing publications are not quantitatively scored by human rights databases.

³⁵See Poe, Carey and Vasquez (2001) and Qian and Yanagizawa (2008) for quantitative comparisons of the Amnesty and U.S. State Department measures and more detailed discussions.

³⁶The declaration was adopted by the United Nations General Assembly on the 10th of December, 1948. It arose directly from the experience of the Second World War and sets out, for the first time, 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/>.

similar formats.³⁷

Reports from these two agencies are individually scored beginning in 1976 by a group of human rights scholars at the 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 5-level “terror scale” originally developed by *Freedom House*. See data appendix for more details. This index is available for 183 countries over the period 1976-2006. We chose to use the PTS over other quantitative scores of human rights because it extends the furthest back in time, to 1976.³⁸ This determines the time period of our study. Amnesty and the U.S. report identical PTS for 84% of our sample, and for 73% during the Cold War. We measure USSD reporting bias as Amnesty PTS subtracted from USSD PTS. If the USSD reports a country as better than Amnesty, then $US_{it} - Amnesty_{it} < 0$. For illustrative purposes, we divide the average of this difference during the Cold War into five equal frequency groups and map it in Figure 2B. It shows that under-reporting was most severe in Cold War allies such as Turkey and Saudi Arabia.

To better illustrate the relationship between alliance and favorable under-reporting from the U.S. relative to Amnesty, we plot the PTS for U.S. allies and non-allies (using the same definition as in Figure 1A) over time. 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 U.S. systematically reported its allies as having better human rights than its non-allies. This gap immediately converges after the Cold War ends. 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 2A for comparisons purposes. In contrast to the U.S., 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

³⁷See the Data Appendix for excerpts from a sample report by the State Department for the Democratic Republic of Congo.

³⁸The CIRI Human Rights Data Project, like the PTS Project, reads the reports by Amnesty and the USSD and provides a score. However, the CIRI incidices only begin in 1981. They also differ from PTS in that they attempt to provide disaggregated incidices for the type of human rights. This means that while the two indices are correlated (approximatley 0.65-0.73), they are not directly comparable. See Wood and Gibney (2009) for a detailed discussion.

previous two figures that during the Cold War, relative to Amnesty, the U.S. reported its allies as having better human rights practices than non-allies. There is no differences after the Cold War.³⁹Since alliance is correlated with many factors, this descriptive evidence cannot show that the U.S.’s strategic value for allies has a causal effect on its under-reporting of human rights. However, the facts that all of the changes between the difference in U.S. and Amnesty differences are driven by changes in U.S. reports, and that favorable reports for allies immediately end after the Cold War are very suggestive towards interpreting these changes as driven by changes in U.S. strategic factors. Note also that Amnesty PTS scores do change over time, which suggests that they do contain information.

News coverage of human rights violations is measured as the number of articles about human rights abuse in a given country. It is collected from the *ProQuest Historical and National Newspapers*. In the ProQuest search, we search for the phrase “human rights” and require at least one of the following words: “torture”, “violations”, “abuse”, “extrajudicial”, “execution”, “arbitrary arrests”, “imprisonment”, “disappearances”, “politically motivated”; and the country’s name. Our measure of human rights coverage is the total number of articles that results from the search per country per year. 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) 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 period of our study. For the Cold War period, we have data for all five papers for until 1986, and then the four papers excluding Chicago Tribue for 1987-88. For the post-Cold War period, we do not have data for the WSJ or the Chicago Tribune. The papers in our sample were arguably some of the largest metropolitan newspapers in the U.S. during the 1980s. The NYT and *Washington Post* had particularly good reputations for the breadth and depth of their news coverage.⁴⁰ These two newspapers have more foreign correspondents than other U.S. newspapers. The fact that we are only using large newspapers that typically write their

³⁹Figures 3A-3C plot the identical relationship as the one plotted in Qian and Yanagizawa (2008). The sample used here is slightly different.

⁴⁰For example, the NYT has received 101 Pulitzer Awards for Journalism, far more than any other newspaper. Over thirty of these were awarded for reporting on international news. It’s reputation for reporting accurate news independent of the wishes of the parties being reported on is strengthened by famous incidents such as *The Pentagon Papers*, where the NYT reported a series of stories based on information leaked to it by a member of the Nixon administration and then fought the Nixon administration in the U.S. Supreme Court in order to overcome an injunction that was placed on it after the first stories were printed.

own stories means that the effects we measure will not likely be confounded by information herding. In any case, the number of articles that we measure are articles written by journalists from these papers; stories picked up from newswires and other sources are not included. See Appendix Figure A2 for a plot of the annual number of articles on human rights abuses for all countries over time.

We have three measures of the cost for newspapers to obtain independent information. First is an indicator for the freedom of domestic press from the *Freedom House* data. It reflects the a newspaper’s ability of picking up stories from local independent sources. This measure ranges from zero to two. Zero indicates no freedom. And two indicates a free press. For example, Afghanistan is rated as zero and Australia is rated as two. 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 problem that changes in media freedom within a country over time may be correlated with UNSC membership. Second, is the number of newswire stories about human rights abuses in a country. We obtain this measure from the ProQuest Database using the same search algorithm as for U.S. newspapers. This measures a newspaper’s access to independent reports from news agencies such as the *Associated Press* (AP) or the *United Press International* (UPI). Note that newspapers pay a fixed subscription for access to newswire stories. The marginal cost for each story is zero conditional on having a subscription. All of the newspapers in our samples have subscriptions.⁴¹ Finally, we measure the cost for a foreign correspondent to travel to the location of the story.⁴² This is the geographic distance from national capitals to the nearest foreign bureau offices. We were only able to obtain the Cold War locations of offices for the NYT.⁴³ Figure 4C shows a map of our media freedom variable as well as the NYT foreign bureau offices.

Table 1 presents the descriptive statistics. In it, we see that on average, the USSD reports countries as being 0.14 index points better in terms of human rights violations relative to

⁴¹This is ascertained from the fact that they all print stories from newswires.

⁴²An average newspaper foreign bureau costs approximately \$300,000 per year. The major costs have been cited as rent, travel and the reporters’ salaries (Carol, 2007).

⁴³The NYT has foreign bureau offices 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 the between cities of the world provided by Kristian Skrede Gledisch of the University of Essex.

Amnesty. The level of alliance with the U.S. is low (around 9%) on average. For the average country, the number of human rights stories in U.S. newspapers is approximately 10 per year. Most of these stories are featured in the *Washington Post*, NYT and L.A. Times. (In Appendix Figure A1, we can see the increase in the number of human rights stories in each paper over time). Newswires provide roughly the same number of stories on human rights abuses as the five U.S. papers in our sample combined. The average distance between the national capital of a country and the nearest NYT foreign office bureau is 1,463 km. 40% of the sample have no media freedom according to Freedom House. And we see from the NYT that the average number of stories of a country is approximately 26. Therefore, in the NYT, human rights stories comprise of a significant proportion, approximately 10%, of overall news coverage of countries in our sample.

5 Empirical Strategy

To measure government propaganda, we use the difference in PTS scores of reports of human rights violations between what is published in the State Department’s (USSD) *Country Reports on Human Rights Practices* relative to what is published in analogous reports by Amnesty International. Interpreting this as U.S. bias assumes that differences in the scores are driven by the U.S.. We do not need to assume that Amnesty reports the truth. We will present evidence to support this assumption, which will be further relaxed when we use our main identification strategy.

To observe the correlation between government bias and news reports, we can estimate the following equation.

$$\ln HRNews_{it} = \beta(USSD_{it} - Amnesty_{it}) + \alpha \mathbf{X}_{it} + \gamma_i + \delta_t + \varepsilon_{it} \quad (1)$$

The natural logarithm of the number of news stories on human rights abuse for country i in year t is a function of: the difference in the U.S. and Amnesty PTS scores, $U.S._{it} - Amnesty_{it}$; a vector of time and country varying controls, which for the main specifications is just the Amnesty PTS score, \mathbf{X}_{it} ; country fixed effects, γ_i ; and year fixed effects, δ_t . 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.⁴⁴ All the differences across countries that do not change over time are controlled for by country fixed effects. All the changes over time that affect all countries similarly such as American attitudes towards human rights are controlled for by year fixed effects. We control for the score of Amnesty reports because we are interested in the effect of the U.S. when the U.S. and Amnesty differs, and for most of the time – approximately 73% during 1976-88 – the U.S. and Amnesty report similar scores. Therefore, controlling for Amnesty reports has little effect on the coefficients but improves the precision of the second stage estimates. All standard errors are clustered at the country level.

Higher PTS reflects worse human rights conditions. Therefore, news reports are positively correlated with government bias implies that $\hat{\beta} > 0$. Interpreting $\hat{\beta}_{OLS}$ as the causal effect of USSD bias on news coverage has several problems. First, measurement error, which is presumably random, will attenuate the OLS estimates. Second is the problem of reverse causality. If the USSD reports may be influenced by U.S. media, then $\hat{\beta}_{OLS}$ will reflect the effect of NYT coverage on U.S. bias as well as the effect of U.S. bias on NYT coverage. Finally, there is an omitted variable bias problems. Both the USSD and the NYT may be responding to popular opinion. To estimate the causal impact of the government bias as captured in the Country Reports, we would ideally have a source of variation in these reports that is both exogenous to other factors that can affect news coverage, and affect news coverage only through the Country Reports. Unfortunately, do not have such instrumental variables.

We exploit plausibly exogenous in a country's strategic value to the U.S. from the combination of alliance to the U.S. and entry onto (and exit from) the UNSC. We will argue and provide evidence that it only affects news reports through government attitudes. However, the Country Reports are just one of the many vehicles that the government can communicate its opinions through. Hence, the interactoin term of alliance and Council membership is not a valid instrument for the difference in U.S. and Amnesty Country Reports. Rather than estimating a 2SLS estimate of the effect of the Country Reports, we will estimate a "first

⁴⁴For example, since 2000, human rights is mentioned in most of the news articles about China even if the main focus of the article is about an unrelated topic. The number of articles on Chinese human rights are just as likely to be correlated with the occurrence of the Olympic Games as with changes in strategic value to the U.S. or actual changes in the conditions for human rights.

stage” effect of an increase in a country’s strategic value to the U.S. government bias as measured by the difference in U.S. and Amnesty Country Reports; and a “reduced form” effect of an increase in strategic value on news coverage of human rights abuses. While this strategy allows us to establish the causal effect of government strategic objectives on news coverage, it does not allow us to identify the specific channels through which this occurs (e.g. government publications, interviews between government employees with journalists, etc). For the latter, we will resort to qualitative evidence.

The first relationship we estimate is the effect of an increase in government strategic value of a country on U.S. government bias towards this country.

$$USSD_{it} - Amnesty_{it} = \theta(U.S.Alliance_i \times UNSC_{it}) + \alpha \mathbf{X}_{it} + \gamma_i + \delta_t + \varepsilon_{it} \quad (2)$$

The difference in USSD and Amnesty PTS scores in country i in year t is a function of: the interaction term between alliance to the U.S., $U.S.Alliance_i$, and membership on the UNSC, $UNSC_{it}$; a vector of country-year specific controls such as Amnesty’s reported PTS, \mathbf{X}_{it} ; country fixed effects and year fixed effects. Note that controlling for Amnesty PTS will not cause a problem unless if there is measurement error in the PTS scores. Higher PTS reflects worse human rights conditions. Therefore, if the U.S. favors its allies when they are on the Council with milder reports of human rights abuses, $\hat{\theta} < 0$.

To interpret θ as causal, we need to assume that an ally’s entry and exit from the UNSC did not affect the difference in PTS scores through any channel other than U.S. strategic value. There are two main suspects for how this can be violated. First is the possibility that allies behave better when they enter the the Council and that the U.S. government has better information about this than Amnesty. Second is the possibility that the effect is driven by Amnesty’s bias. For example, when an U.S. ally enters the UNSC, a left-leaning Amnesty may decide to exagerrate human rights abuses in that country. There’s little reason to believe that this is the case. But for caution, we investigate these possibilities by repeating the same estimation for the period after the Cold War under the assumption that the U.S.’s value for allies have decreased by the incentive for a country to behave better when they are on the Council is similar as during the Cold War.

The comparison of the DD estimates for the post-Cold War period is a useful exercise

for providing additional evidence for the claim that changes in the PTS difference is mainly driven by U.S. policy. This is similar in spirit to a triple differences estimate. However, for the reasons stated in earlier in the section on Data, the comparison should not be taken literally.

In addition to the difference-in-differences (DD) estimates, we can examine the timing of the effect of entry and exit onto the Council more precisely by estimating the following equation. This allows us to observe whether the additional benefits to allies of being on the UNSC are only experienced for the years when those countries are on the Council.

$$USSD_{itc} - Amnesty_{itc} = \sum_{c=-2}^3 \theta_c (U.S.Alliance_i \times \mathbf{1} \cdot \tau_c) + \alpha \mathbf{X}_{it} + \rho_c + U.S.Alliance_i + \delta_t + \varepsilon_{itc} \quad (3)$$

The difference in U.S. and Amnesty reports for country i in year t , c years since it is a UNSC member is a function of: the interaction between a dummy variable indicating the number of years since UNSC membership, τ_c , and a continuous measure of U.S. alliance, $U.S.Alliance_i$; dummy variables for the number of years since membership, ρ_c ; the score of Amnesty reports, the U.S. alliance main effect, and year fixed effects. If the U.S. bias arises mainly from an increase in an ally's strategic value in being on the council during the Cold War, then there should be no correlation for the two years leading up to being a member and the two years immediately following, $\hat{\theta}_{-2}, \hat{\theta}_{-1}, \theta_2, \hat{\theta}_3 \approx 0$, and negative effects for the two years on the council, $\hat{\theta}_0, \hat{\theta}_1 < 0$ during the Cold War.

The second relationship we estimate is the reduced form effect of an increase in a country's strategic value to the U.S. on news coverage of its human rights abuses in U.S. newspapers. We repeat equation (1), replacing the difference in PTS scores with the natural logarithm of news stories as the dependent variable. For causal interpretation, we must assume that an ally's entry onto and exit from the UNSC does not affect news coverage of its human rights abuses through any channel other than government bias. For example, if readers are more interested in allies, and this interest increases when they are on the Council, then the estimated effects would be confounded by reader preferences.⁴⁵ There is little reason to believe this to be true ex-ante when knowledge surveys show that only 15% of Americans can name

⁴⁵See studies by Mullainathan and Shleifer (2005) and Gentzkow and Shapiro (2006) for on how consumer preferences can drive news coverage.

the Secretary General and that less than 16% of Americans can name an agency within the UN (Alger, 2005: p. 59). Moreover, the most plausible reader preference is arguable to expect the media to monitor the bad behavior of U.S. allies when they enter the UNSC. This would bias against our estimates of the effect on under-reporting of bad behavior. 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. Later in the section on robustness, we will present evidence suggesting that this is unlikely to be true.

Before estimating the main specification where we assume the benefit of the UNSC is monotonically increasing and linear in alliance, we estimate a more flexible equation, where we allow the effects of alliance to vary by the level of alliance.⁴⁶ Appendix Table A2 shows that the effects are monotonically increasing the levels of alliance, suggesting that a linear measure will not be a bad approximation for the effects. Thus, for convenience, we will only present results with the simpler linear measure of alliance.

Although this study was motivated by the many documented incidents of government manipulation of the media, we do not take for granted that this is the only driving force behind our results. In addition to the main results, we investigate the possibility that the correlation between news distortion and government bias is partly due to information asymmetries. This mechanism can coexist with government manipulation and may exacerbate the effect of the latter. Journalists are likely to be aware of the government’s incentives to manipulate information. But because its information on what occurs in foreign countries is imperfect, it would have to obtain independent information to mitigate the bias. If this information is costly to obtain, then we can examine the extent to which information plays a role by estimating the interaction effect of government bias and the cost to obtain independent information. If the main results are driven by information asymmetries, then they should increase with the cost of obtaining independent information. To test this hypothesis, we estimate the effect of the triple interaction term of alliance, UNSC membership, and a measure for the cost of obtaining

⁴⁶We investigate whether a linear measure of U.S. alliance accurately captures the U.S.’s value of allies on the UNSC by first estimating a more flexible specification where we allow the effect of UNSC to vary depending on the level of alliance. 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”, “median allies”, or “strong allies”. We then estimate a equation similar to equation (2), where the interaction term $U.S.Alliance_i \times UNSC_{it}$ is replaced by two interaction terms: $MedianAlliance_i \times UNSC_{it}$ and $StrongAlliance_i \times UNSC_{it}$. The estimates are shown in Appendix Table A1. Estimates in Column (1) show that the effect of being on the UNSC on U.S. under-reporting is increasing with alliance.

independent information, $U.S.Alliance \times UNSC \times IndependentInfoCost$. We will use three different measures of cost: access to stories from independent foreign domestic press (e.g. Freedom House measure for media freedom), the number of newswire stories on abuses in a country, 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).

6 Results

6.1 The Correlation between Government Bias and News Coverage

The OLS estimates of equation (1) are shown in Table 2. Columns (1)-(4) show estimates for a sample of the Cold War years (1976-88). The estimates in columns (1) and (2) show that increasing the PTS score from the U.S. State Department and from Amnesty for a country by 1 index point is correlated with an increase news coverage of approximately 0.45 and 0.26 log points. Column (3) shows that controlling for Amnesty PTS, a difference of 1 index point between the USSD and Amnesty is correlated with a 0.42 log point increase in news coverage. Column (4) shows that the estimate is roughly unchanged when we control for whether the country was on the UNSC that year. All the estimates are statistically significant at the 1% level. Columns (5)-(8) show the analogous estimates for the post-Cold War period (1992-2005). The estimates have the same sign as those for the Cold War, and are statistically significant; but they are smaller in magnitude.

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

Table 3 shows the estimated effects from an increase in a country's strategic value to the U.S. on USSD reports of human rights abuses for that country relative to Amnesty from equation (2). Panel A shows the estimates for the Cold War era. For interest, we estimate the effects controlling for the U.S. alliance main effect in addition to the main specification with country fixed effects. Columns (1) and (2) show that Council membership for a country that always votes with the U.S. in the UNGA decreases USSD reports of its human rights abuses by approximately 3 index points. The estimate controlling for country fixed effects

in Column (2) is statistically significant at the 1% level. Columns (3) and (4) show that the effect on Amnesty reports of human rights abuses have the opposite sign and the estimates are statistically insignificant. A comparison of the effect on Amnesty in Column (4) to the effect on USSD in Column (2) shows that the magnitude of the absolute value of the coefficient is much smaller for the effect Amnesty, approximately only one-third of that of the effect on USSD PTS. Columns (5) and (6) show the estimated effect on the difference between USSD and Amnesty PTS. Column (7) shows that this estimate changes little when we add a control for Amnesty PTS. The coefficient shows that conditional on Amnesty reports, Council membership of a country that always votes with the U.S. during the Cold War decreases USSD reports of human rights abuses relative to Amnesty by 3.56 index points. This estimate is statistically significant at the 1% level. Since the median country voted with the U.S. 7% of the time, one can roughly obtain the effect for the median country by multiplying the coefficients by 0.07.

Since the empirical strategy is based on UNSC membership, we next restrict our sample to the 51 countries that were ever on the UNSC. Column (8) shows that the estimates on this restricted sample are similar in magnitude and statistically significant at the 1% level. To see if our results are driven by outliers, we plot the partial correlation from Column (8) between the difference in USSD and Amnesty PTS scores and the interaction between UNSC membership and U.S. Alliance. Figure 4A shows that the effect is largest for Zaire, which 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 that is what Column (9) shows when we repeat the estimate on a sample of countries that were on the UNSC at least once and where Zaire is omitted. The partial correlations of this estimate is plotted in Figure 4B.

Panel B shows the analogous estimates on the post-Cold War sample, when the U.S.'s strategic value of allies has decreased. The estimates support this belief. Columns (1) - (4) show that Council membership for allies have similar effects on Amnesty and U.S. reports after the Cold War. Neither are statistically significant. The estimates in Columns (5)-(9) for the effect on the difference in USSD and Amnesty PTS are much smaller in magnitude than the Cold War estimates, have the opposite sign, and are statistically insignificant.

There are several important pieces of evidence in this Table 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 Panels A demonstrate that the effect on the different PTS scores are driven by changes in USSD reports, not Amnesty reports. Second, we see that being allied to the U.S. 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 show that these effects and the main effect of U.S. alliance decrease in magnitude after the Cold War, when strategic value of allies have arguably decreased. Interestingly, the correlation between Amnesty PTS, which we have argued does not respond to a country's strategic value to the U.S., and the outcome of interest is unchanged when the Cold War ends. While these results are not conclusive proof, they provide very suggestive evidence for our claim that our strategy is capturing changes in U.S. strategic value.

Next, we investigate the timing of these effects. Since UNSC membership is obtained through elections, one may expect the benefit of Council membership to begin before the official term begins. Alternatively, winning a seat on the Council may be correlated with other factors that could affect U.S. PTS scores relative to Amnesty. While it is unlikely that these factors are also correlated with U.S. alliance, finding that the benefit of Council membership to allies exist even when the two year term is over would cast doubt on the validity of our empirical strategy. Thus, we estimate equation (3). The estimates for the Cold War and post-Cold War era are shown in Appendix Table A2 along with F-statistics for the joint significance of the two years on the Council. The coefficients are plotted separately in Figure 5. The solid red line shows the estimated effect of Council membership for a country that always votes with the U.S. for each of the two years before it enters the Council, the two years on the Council, and the two that follows. It shows that the benefits occur during the two years on the Council. There are no effects before or after. The discreteness in the change in benefits of UNSC membership when allies enter or exit from the UNSC is similar to what Kuziemko and Werker (2005) found for the benefits of being on the UNSC during strategically important years. The dashed blue line plots the coefficients for the post-Cold War era. It shows that there is no benefit, before, during, or after Council membership. The findings that the effects occur during the two years on the Council, and that the effects only exist during the Cold War are both consistent with our empirical strategy capturing changes in countries'

strategic values to the U.S.

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

Table 4 shows the estimated effects of Council membership for U.S. allies on American newspaper coverage of human rights abuses. As before, we estimate the effects alternatively with U.S. alliance main effects and country fixed effects; and Panels A and B show the estimates for the Cold War and post Cold War period. The estimates in Columns (1) and (2) show that Council membership for a country that always votes with the U.S. decreases news coverage of its human rights abuses by approximately 8 log points. Columns (3) and (4) show that the estimate is roughly unchanged when we add a control for Amnesty PTS, but precision improves so that the statistical significance level increases from 15% in Column (2) to 10% in Column (4). Column (5) shows that restricting the sample to countries that were ever on the Council produces similar estimates. The estimate is statistically significant at the 1% level. Figure 4C plots the partial correlation between the natural logarithm of the number of news articles on human rights abuses with the interaction of UNSC membership and alliance with the U.S. It shows that, as before, Zaire is an outlier. Column (6) shows the estimate on the restricted sample of countries that were ever in the UNSC and where Zaire is excluded. The estimated effect is larger and statistically significant at the 1% level. It shows that the UNSC membership of the median country who votes with the U.S. 7% of the time, decreases news coverage of human rights abuses by approximately 64% ($\exp^{-14.68 \times 0.07} - 1$). The partial correlation plot of the residuals are plotted in Figure 4D.

In Panel B, the estimates for the post-Cold War period are shown. While the signs of the estimates of the interaction of UNSC and alliance are similar to the Cold War era, the magnitudes of the estimates are much smaller and they are not statistically significant.

A comparison of Panels A and B show similar patterns as the results in the previous Table. Alliance with the U.S. is strongly correlated with news coverage during the Cold War and uncorrelated after the Cold War when strategic values of allies presumably decrease. Amnesty PTS, which has no direct relationship to U.S. strategic objectives, exhibits the same correlation with news coverage both during and after the Cold War. Once again, this is consistent with the belief that the strategy captures changes in strategic value of a country

to the U.S. government. Interestingly, note that the estimated coefficient of the U.S. alliance main effect in Panel A is positive and statistically significant. This means that on average allies receive more coverage on human rights abuses in newspapers. Similarly, the coefficient for the dummy variable indicating that a country is a UNSC member is positive, which means that on average UNSC members receive more news coverage of human rights abuses. We will discuss the implications of these estimates for our interpretation later in the paper when we explore alternative hypotheses.

6.4 Robustness

One concern for our strategy is that we are capturing spurious country specific trends. It seems unlikely that such trends are specific to levels of alliance and UNSC membership. And as we have already shown, the effect of Council membership interacted with alliance on USSD under-reporting of human rights is very discrete in timing. When we estimate yearly effects on news coverage, the patterns of the coefficients are consistent with those for the effect on USSD under-reporting. However, they are less precise (probably due to the large number of observations where there are zero articles). Hence, to check that our strategy does not capture spurious trends, we employ an alternative strategy: control 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 5. Panel A shows the effects on the difference in USSD and Amnesty PTS scores. Column (1) shows the estimate without controlling for country specific time trends. Column (2) adds this control. They are both statistically significant at the 1% level and similar in magnitude. 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 as those in Columns (1) and (2) but less precise.

Panel B shows the estimated effects for newspaper coverage of human rights stories. Column (1) shows the estimate for the full sample without controlling for country-specific trends. Column (2) shows that the magnitude of the coefficient decreases from -8.489 to -6.083 when the controls are added and estimates. However, the two estimates are not statistically different from each other. Columns (3) and (4) show that as with the main estimates, the estimates

with country-specific time trends change little when the sample is restricted to countries ever on the UNSC, and increases in magnitude when Zaire is further omitted. Column (4) shows that for a median country who votes with the U.S. 7% of the time, Council membership decreases coverage by approximately 55% ($\exp^{-11.28 \times 0.07} - 1$).

Next, we check that our estimates are robust to restricting the sample to years when the Reagan Administration was in power (1980-88). Since, the government agenda for anti-communism and the apparatus for influencing the public were both significantly strengthened during this administration, we should find that the main results are main driven by this period. Columns (5)-(8) of Table 5 show that this is indeed the case. The estimates are essentially unchanged when we restrict the sample.

Third, we check the robustness of using a linear specification when many countries have no stories written in U.S. newspapers on their human rights abuses. Approximately 40% of the 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 caused by 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 2 stories on human rights abuses in U.S. newspapers on a given year. Comparing these to the main estimate (Appendix Table A3 Columns (1) and (2)) show that the effects are larger on the restricted sample. This suggests that increasing the number of news stories from zero to more than zero is not the main margin for the main results. Not surprisingly, when we use an alternative Tobit specification to address the potential censoring problem (Table A2 Column (4)), the estimates are nearly identical to the OLS estimates. We conclude from this that our main estimates are not biased by censoring.

6.5 Mechanisms for the Effect on News Coverage

This section only discusses and reports results for the Cold War period. First, we investigate the possibility that the main results are driven by consumer preferences. This mechanism can co-exist with government capture in driving the main results. 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 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 U.S. and UNSC membership are each positively correlated with news coverage on human rights abuses (see Table 4 Panel A). However, this scenario will bias against our finding that allies' entry onto the UNSC decrease new 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 seems highly unlikely and runs contrary to the correlations between the main effects of U.S. alliance, UNSC membership and news coverage shown in Table 4.⁴⁷ Alternatively, Americans may simply be more interested in general news of allies on the Council. Since an increase in general news coverage is likely to be positively correlated with an increase in human rights coverage for mechanical reasons, such preferences could bias our results upwards. To investigate this, we estimate the effect of Council membership interacted with alliance on the total number of stories for a country in a given year. This measure includes any story containing the country's name. We only have this measure for the NYT. Table 7 Row (7) shows that the estimated effect is small and statistically insignificant. We conclude that the main results are not driven by a general interest of consumers for UNSC members that is increasing with alliance.

We further explore the role that consumer preferences play in our context by considering the possibility that the news distortions are partially driven by the newspapers' desire to slant towards the preferences of their readers. To see if this occurs, we estimate the effects for each newspaper individually and see if the magnitude of the effect of the conservative Reagan Administration's bias is larger for more conservative newspapers. Table 6 Row (1) shows the estimate for the sum from the main results. Rows (2)-(6) shows the estimated effects on the *Washington Post*, NYT, WSJ, *Chicago Tribune*, and the *L.A. Times*. The estimates show that the estimated effects for the first three newspapers are statistically similar and significant at the 1% level. The estimated effect for the *Chicago Tribune* and the *L.A. Times* have the same signs as the first group of papers. But they are much smaller in magnitude, roughly half and one-fourth in relative to the first group of papers, and statistically insignificant. Interestingly,

⁴⁷We cannot investigate this possibility further empirically. To do so, we will need a measure that clearly reflects bad behavior of the allied country's government; and information of this bad behavior needs to come from a source other than the U.S. government (otherwise, we will not be able to distinguish U.S. government bias from consumer preference).

the magnitude of the estimated effects of government strategic value on news coverage do not correlate well with ratings of the conservativeness readers according to *Mondo Times Conservativeness Rating* or to media slant measures calculated by Gentzkow and Shapiro (2006).⁴⁸ The results presented here do not conclusively rule out the possibility that consumer preferences may play a role. But they are very suggestive that under the most likely scenarios, consumer preferences are probably not a key driving force in our context.

The somewhat surprising finding that the distortions are driven by the *Washington Post*, NYT and WSJ, the newspapers with the best reputation for new reporting, are also interesting for another reasons. These are the only papers within our sample that were ever named explicitly in the documents from the investigations of the OPD. This suggest that the government perceived these three papers as capturing the relevant audience for supporting their policy; and therefore focused on controlling these three publications.⁴⁹ Moreover, the finding that the government does not distort the news of the Chicago Tribune or the L.A. Times is very stark. It suggest that there is little spillover between the prominent newspapers and other large metropolitan newspapers; and also that the government was not trying to control the prominent newspapers in order to influence the others (or if it did, it was unsuccessful).

⁵⁰ We will discuss this result further in the next section of the paper.

Next, we explore the role of information asymmetries in driving our results. The historical discussion of the OPD’s “White Propaganda” operation earlier in the paper focuses on the government manipulating journalists that are aware of the government’s agenda and often coerced (or bribed with improved access) into cooperation. An alternative possibility is that news reports are biased by government information because it is costly to obtain indepen-

⁴⁸According to Mondo Times, the papers of our sample in the ascending order of conservativeness are the *L.A. Times*, *The New York Times*, *The Washington Post* and *the Chicago Tribune*. According to Gentzkow and Shapiro’s (2006) measure of media slant, these papers are very similar. See Figure 1 of their paper for the correlation between their slant measure and the Mondo Times ratings.

⁴⁹The fact that these three papers are more prominent than others is evident from the fact that their correspondents have permanent second row seats in the White House Briefing room where as the other papers have occasional seats in the middle or back of the room (see Appendix Figure A3 for an example of a *White House Press Corp* seating chart). We were only able to obtain seating charts for 2009. The seats for some of the members in the back of the room change over time. The Chicago Tribune sometimes shares a seat with other papers towards the back of the room. The L.A. Times is sometimes in the back or middle of the room. (Since the L.A. Times was significantly less important in the 1970s and 80s, it was probably further back). The Washington Post, NYT, and WSJ are always in the second row.

⁵⁰The fact that all major newspapers have editorial offices in Washington D.C. makes it unlikely that these differential results are driven by differences in physical access.

dent information. This is especially true for news on far away and often physically dangerous countries. This does not contradict the story of government manipulation. However, under information asymmetries, governments can influence the media simply by manipulating the information it feeds the media, without direct coercion. We test the hypothesis that information asymmetries between newspapers and the USSD contribute to the main results by examining whether the effect of government distortion is larger when the cost for obtaining independent information is higher for the news organization. We have three measures to proxy cost. First is the distance from the capital city to the nearest foreign bureau office. This captures the cost for a newspaper's own correspondent to travel and report on a story. We were only able to obtain Cold War era bureau office locations for the NYT. Table 6 Row (8) show the estimated interaction effect on NYT stories of human rights abuses. There is no effect. The estimate is near zero in magnitude and statistically insignificant. Second is a dummy for whether there is no domestic media freedom according to *Freedom House*. This reflects U.S. newspapers' ability to pick up stories from independent sources from within the country that's being reported on. Row (9) shows no evidence the government bias reduces coverage more when there is no freedom. The estimate is positive in sign and statistically insignificant. Finally, we measure the newspaper's ability to pick up a story by the number of newswire stories, which are free of cost on the margin conditional on subscription. Row (10) shows no evidence that distortions are larger when there are fewer newswire stories. The estimate shows the opposite effect, that more newswire stories increase the reduction in human rights coverage in U.S. newspapers. This may be due to the fact that the number of newswire stories are strongly correlated with a country's strategic value to the U.S. The coefficient is small in magnitude but statistically significant. From these results we conclude that there is no evidence to support the presence of information asymmetries in driving main estimates.

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). Row (11) of Table 6 shows that the effect of distortions were smaller during the Carter administration. This is not surprising since the push to manipulate the media was stronger during the Reagan era (e.g. the OPD did not officially exist and become fully operational until 1981).

6.6 Quantifying the Average Effect

We quantify the effects in three different ways. First, we make the extreme assumption that the only way for the government to influence 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 be interpreted only as an illustrative example. We only report results for the Cold War period. Table 7 shows the OLS and 2SLS estimates for the sum of human rights coverage across all five U.S. newspapers in our sample, and the individual effects for the NYT, Washington Post and the WSJ. Columns (1)-(3) show that reporting a country as 1 index point worse by the USSD (relative to Amnesty) is correlated with a 0.26-0.3 log point increase in news coverage of abuses in all papers, the NYT and the Washington Post. These estimates are significant at the 1 % level. The estimate for WSJ is small and statistically insignificant. Columns (5)-(8) show the 2SLS estimates. The fact that these estimates are significantly larger than the OLS is consistent with the belief that the government has other ways to manipulate the media beyond the reports; and the possibility that the difference in PTS scores measures government bias with error. That said, under the identification assumption we stated above, these estimates say that if the USSD reported a country as being 1 index point worse than Amnesty, then news coverage of abuses will increase by approximately 2.3 to 3.2 log points. On average, the USSD under-reported by 0.35 index points during the Cold War. Thus, the results say that on average during the Cold War, USSD under-reporting decreased coverage by approximately 0.8 to 1.12 log points (55-67%).

Next, 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 corp 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 use only the NYT. In

the 30 days after June 4th, 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. If we take the 2SLS estimates literally and compare it with this, it would mean that government bias in USSD reports reduced coverage by approximately 63% ($0.55/0.91$) from what they should have been absent distortions.

Finally, we calculate the average value of a seat on the UNSC during the Cold War conditional on a given level of alliance with the U.S. For this exercise, we choose four of the U.S.'s strongest allies during the Cold War: Brazil, Zaire, Honduras and Chile. Table 8 Column (2) shows that these countries voted with the U.S. 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 coefficient for the dummy variable of being on the UNSC. To be conservative, we use the coefficients from Table 5 Column (4) Panel A where country specific time trends are controlled for. These calculations show that during the Cold War, UNSC membership reduced USSD reports of human rights abuses relative to Amnesty by 0.03 index points for Brazil, 0.3 index points for Zaire, 0.31 index points for Honduras and 0.53 index points for Chile. In Column (5) of Table 8, we similarly calculate the effect on news coverage of abuses in U.S. newspapers. Once again, we use the estimated coefficients from Table 5 Column (4) (Panel B). The calculation shows that a seat on the UNSC decreased news coverage of human rights abuse for Brazil by approximately 20%, for Zaire by approximately 58%, for Honduras by approximately 59% and for Chile by approximately 76%.

7 Implications for Besley and Prat (2006)

The context of this paper fits well with the model of media “capture” formulated by Besley and Prat (2006). Our empirical investigation can be considered as a case study for the special example of their model where the government bribes by allowing or cutting off access to information that is valuable to the news organization. In their model, media outlets, as 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). Following this intuition, our results indicate that in net, the benefits of going along with the government dominated perceived costs of the losses of reputation.

The fact that we find this result in a market where there are so many independently owned media outlets has several interesting implications for BP, which predicts that the governments' ability to capture the media is decreasing with their number. This follows from the condition that profits from reporting news decreases with the number of other firms reporting it (at the same time). Therefore as the number of firms increase, the value of exclusive access that the government can offer decreases; and the probability that at least one firm will refuse to be distorted and report the truth increases. In fact, the same intuition predicts that the incentive for any one news outlet to report the truth increases with the number of those that agree to distort.⁵¹ BP (as well as many other studies of the media) point to the U.S. as an example of a market with a large number of independently owned outlets, and where it would be difficult for the government to influence the media.

The easiest way to reconcile our results with the model and the large number of media outlets is to relax the assumption that readers can verify the accuracy of reports. It may be that during this tense period of international politics, readers were overwhelmed by a multitude of conflicting sources, and/or wary of the political motivations behind such information, readers found it difficult to ascertain the accuracy of news reports. Such a scenario is possible in our context. However, one could argue that over time, readers who value accuracy, should be able to update and eventually be able to distinguish distorted reports with a reasonably high probability. In any case, for the sake of discussion, we will assume that readers are able to verify accuracy (and value accuracy) and discuss the more nuanced implications of our results under those conditions.

One implication in this case is that there are probably significant fixed costs to entry in the

⁵¹Proposition 1 of their study finds that the probability of the government's success in bribing the media to suppress a story is decreasing in the number of firms in the market, increasing in the returns of suppressing the news to the government, and decreasing in the benefits to the audience and transaction costs between the government and media.

market for international news. To see this, imagine the case where there are zero entry costs. Then a distortion by the existing newspapers should induce a new entrant to report accurately and earn a positive profit. In equilibrium, this should cause newspapers to not distort. The fact that we find that the government is successful in distorting the news suggests that entry is not costless. The fact that we find that the effects on news distortion are driven by the NYT, *Washington Post* and WSJ, and not by the *Chicago Tribune* or the *L.A. Times* suggest that the fixed costs is related to reputation for national and international news reporting rather than for equipment or the resources necessary to launch an investigation. (These are all large organizations). Interestingly, this interpretation suggests that reputation could have ambivalent effects on the accuracy of reporting. On the one hand, newspapers will want to report accurately to increase their reputation which presumably increases the marginal revenue per news story. On the other hand, if there is a fixed cost in obtaining reputation, reputation will segment the market between firms with and without it, decrease market size, and consequently make it easier for the government to capture the relevant news outlets.

The finding that the most prominent newspapers are captured supports the BP prediction that when the level of investment in news quality is endogenous, firms will vertically differentiate in equilibrium, and the government will only try to capture the high quality news outlets. In their model, the marginal returns to investment in quality is continuous across different levels of investment. Our findings suggest that, in practice, part of the investment is fixed.

For policy makers in general, this means that potential segmentation of the market needs to be taken into account when considering the size of media markets in assessing the likelihood of government capture. Otherwise, the risk of capture could be grossly under-estimated. In our context, it means that the government perceived that all the readers it wished to influence read one or more of these three newspapers; and that these readers are not likely to substitute to another news sources. Hence, instead of having to influence thousands of media outlets, it only had to influence a few.

8 Conclusion

This study estimates the effect of strategic objectives of the U.S. government on news coverage in U.S. newspapers. Our results show 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 large. The results suggest that at least part of the manipulation is achieved by distorting official government publications; but are also consistent with historical documentation of direct government coercion of news outlets to report information that is favorable to the government's political agenda.

The context of this study has several advantages. The independence of the press is commonly perceived to be one of the key components of democracy in America. It is also a context where all domestic news outlets are independently owned and where the market for news is by all accounts very competitive. Therefore, the results we obtain on government manipulation in the U.S. can be easily extrapolated to many other countries.

Our focus on human rights had 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. And by all appearances, it seems that Americans care about human rights and prefer its government to not provide support for governments that violate them. The government seems aware of this preference. Hence if it wants the support of its constituency, it will have an incentive to portray its allies favorably. 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 improve the prestige of the governments of foreign countries (e.g. the Olympics). These are all interesting subjects for future research.

The welfare implications of the empirical findings are ambiguous. On the one hand, if the readers gain utility from knowing the government attitude or like hearing reports that are consistent with the official government agenda during a time of international political tensions and increased American patriotism, then these results would not lead to a decrease in welfare. Another reason for our findings to have little implication for welfare is the possibility that American readers simply don't value international news. 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 for accuracy in international news, then the welfare reduction from these distortions 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 (Carroll, 2007). There are also many other plausible scenarios for which the distortions can reduce welfare. For example, it may be difficult for readers to verify the accuracy of the composition of stories. Or, readers may not be time consistent and therefore undervalue their future utility from accurate news reports.⁵² The welfare implication of news distortions is an important topic for future studies.

⁵²For example, during the 2003 presidential elections, American voters likely had a high value for an accurate perception of the Middle East. However, if they are Bayesian, then their perceptions will be inaccurate due to distorted past reports (even if reports in 2003 were accurate).

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9 Data Appendix

9.1 Country Report Example

The following report is an excerpt from the State Department Report for the Democratic Republic Of The Congo in 1999 (Source: Country Reports on Human Rights Practices. Published by the Bureau of Democracy, Human Rights and Labor, February 23, 2000).

Much of the Democratic Republic of Congo (formerly Zaire) continued to be ruled by President Laurent Desire Kabila, whose Alliance of Democratic Forces for the Liberation of Congo-Zaire (ADFL) overthrew the authoritarian regime of Mobutu Sese Seko by armed force in 1997. The State continued to be highly centralized formally—although in practice the country’s dilapidated transportation and communications infrastructure impaired central Government control—and Kabila continued to rule by decree, unconstrained by a constitution or a legislature. Kabila continued to ban political party activity, and replaced the ADFL with Libyan-trained “People’s Power Committees” (CPP’s) that monitored the activities of citizens in neighborhoods, schools and workplaces. The July 10 Lusaka Accords provided for a political dialog among the Government, rebel factions, the unarmed opposition, and elements of civil society; however, little was accomplished toward this end during the year. The judiciary continued to be subject to executive influence and corruption....

Section 1 Respect for the Integrity of the Person, Including Freedom From:

a. Political and Other Extrajudicial Killing

The widespread extrajudicial killings of Tutsis and suspected Tutsis that occurred in government-controlled areas after the start of the war in 1998 did not continue during the year; by the start of the year, surviving Tutsis generally either had left the government-controlled part of the country or were in hiding, places of refuge, or government custody. One Tutsi girl died of an illness while in government custody, but it was credibly reported that her death was not due to government negligence or abuse. The Government also materially supported Mai Mai and Hutu armed groups, which, according to credible reports, repeatedly killed unarmed as well as armed Tutsis in areas held by antigovernment forces. However, government officials no longer instigated mob violence against unarmed Tutsis, and there were no further reports of mass extrajudicial killings of Tutsis by the security forces.

Members of the security forces committed extrajudicial killings, and the Government misused the judicial system to try, sentence, and execute numerous persons without due process.

On the night of January 6 in Kinshasa, members of the Presidential Guard under the command of an officer named Kabengele beat and killed university student Remy Lushima Nyamangombe, whom they accused of being a member of the armed rebellion. After stealing Lushima's personal belongings, presidential security agents then beat the student to death. No action is known to have been taken against the members of the Presidential Guard who committed this abuse.

On February 14, in the Ndjili district of Kinshasa, an FAC soldier shot and killed a civilian in the Sebastien Bar after bumping into his table.

On April 17 in Kinshasa, unidentified soldiers shot and killed a public transportation driver known only as Kalle. The soldiers flagged down his vehicle and ordered him to drive them to Kinshasa's international airport. When Kalle explained he was taking a woman in labor to the hospital, the soldiers shot him, leaving the pregnant woman alone in the vehicle.

On April 17, security force members charged with protecting a presidential motorcade shot and killed a minibus driver. They shot the driver three times in the head as he was attempting to move his vehicle out of the way of the coming motorcade.

....

b. Disappearance

....

c. Torture and other Cruel, Inhuman, or Degrading Treatment or Punishment

....

d. Arbitrary Arrest, Detention, or Exile

....

e. Denial of Fair Public Trial

....

f. Arbitrary Interference with Privacy, Family, Home, or Correspondence

....

g. Use of Excessive Force and Violations of Humanitarian Law in Internal Conflicts

....

Section 2 Respect for Civil Liberties, Including:

a. Freedom of Speech and Press

....

b. Freedom of Peaceful Assembly and Association

....

c. Freedom of Religion

....

d. Freedom of Movement Within the Country, Foreign Travel, Emigration, and Repatriation

....

Section 3 Respect for Political Rights: The Right of Citizens to Change their Government

..Although the Government did not ban political parties, it continued to impose severe restrictions on their activities, and to enforce these restrictions with numerous arrests and detentions (see Sections 1.d. and 2.b.). In practice the permitted operations of political parties continued to be restricted to internal administrative functions. At various times government security forces put opposition political party members under surveillance. Police also raided and padlocked party headquarters....

Section 4 Governmental Attitude Regarding International and Nongovernmental Investigation of Alleged Violations of Human Rights

..Local human rights activists were subjected to frequent harassment, arrest, and detention by security forces (see Section 1.d.). The legality of such arrests was often unclear, as was the authority of the security forces members effecting the arrests and detentions. In December 1998, Donnat N’Kola Shamuyi, an investigator for the human rights NGO CDDH, disappeared while working in Tshiapa in Western Kisai Province (see Section 1.b.)....

Section 5 Discrimination Based on Race, Sex, Religion, Disability, Language, or Social Status

The previous Constitutions forbade discrimination based on ethnicity, sex, or religious affiliation, but the Government did not enforce these prohibitions effectively and

continued to act with serious official prejudice against members of the Tutsi ethnic group. Societal discrimination remained an obstacle to the advancement of certain groups, particularly women, Tutsis, Muslims, and the indigenous Pygmy (Batwa) people.

Women Domestic violence against women, including rape, is common, but there are no known government or NGO statistics on the extent of this violence. The police rarely intervene in domestic disputes. Rape is a crime, but the press rarely reported incidents of violence against women or children. Press reports of rape generally appear only if rape occurs in conjunction with another crime, not because of the act of rape itself...

Children Government spending on children's programs is nearly nonexistent...Some children as young as 10 years of age have been allowed to enlist as soldiers in the FAC. The Government has not taken comprehensive measures to remove child soldiers from its armed forces, although it has stated its intention of demobilizing child soldiers once the war is over. While many child soldiers continued to serve in the armed forces, during the year the Government greatly reduced and may have stopped recruiting children into its armed forces. However, the Government increasingly encouraged the enlistment of children in paramilitary organizations. In March the African Association of Human Rights reported that unemployed Katangan youth recruited by the GSSP were "excessively militarized," and that provincial authorities in Katanga were arming unemployed youth through CCP's....

People with Disabilities

Indigenous People

Religious minorities Approximately 50 percent of the population are Roman Catholic, 20 percent are Protestant, and 10 are percent Islamic. About 20 percent practice traditional indigenous religions exclusively. However, many persons practice elements of both Christianity or Islam and a traditional indigenous religion... In January in the Kamituga area of South Kivu Province, Mai Mai leader Sylvestre Louetcha reportedly accused of witchcraft 32 women who supported their traditional ruler in resisting Mai Mai demands for forced labor, then cut off their breasts, forced them to eat their own breasts, and killed them. In November,

in the Mwenga area of South Kivu Province, RCD-Goma rebels tortured and buried alive 15 women who were suspected of having performed witchcraft in support of a local Mai Mai force, according to some accounts of that incident (see Sections 1.a. and 1.c.)....

National/Racial/Ethnic Minorities The last official census was taken in 1984. It is estimated that the population is now 45 to 50 million, and comprises more than 200 separate ethnic groups. These groups generally are concentrated regionally and speak distinct primary languages. There is no majority ethnic group; the four largest ethnic groups are the Mongo, Luba, Kongo, and Angbetu-Azande, who together make up about 45 percent of the population. Four indigenous languages—Kiswahili, Lingala, Kikongo, and Tshiluba—have official status. French is the language of government, commerce, and education.

..Since the start of the war in August 1998, ethnic Tutsis have been subjected to serious abuses, both in the capital and elsewhere, by government security forces and by some citizens, for perceived or potential disloyalty to the regime (see Section 1.a.). During the year, extrajudicial killings of noncombattant Tutsis in government-controlled areas largely stopped. However, in Kinshasa and in Katanga Province, Tutsis continued to be held in prolonged detention, from which the Government was willing to release them only on condition that they leave the country (see Sections 1.d. and 2.d.). One Tutsi girl died of an illness while in government custody, but it was credibly reported that her death was not due to government negligence or abuse. Throughout the year, government officials and state media continued to publish anti-Tutsi propaganda, and continued to exhort not only state security forces but also citizens and in particular CCP members to exercise vigilance to uncover Tutsis in hiding and Tutsi infiltrators (see Sections 1.c. and 1.f.). Consequently, although most surviving Tutsis in government-controlled areas were either in hiding or detained or had left the government-controlled part of the country, many non-Tutsis who physically resembled Tutsis were detained or beaten on suspicion of being Tutsi. The Government also materially supported Mai Mai and Hutu armed groups, which, according to credible reports, repeatedly killed unarmed as well as armed Tutsis in areas militarily dominated by antigovernment forces. However, the Government no longer incited mob violence against unarmed Tutsis, and there were no reports of mass extrajudicial killings of Tutsis by the security forces....

Section 6 Worker Rights a. The Right of Association

....

b. The Right to Organize and Bargain Collectively

....

c. Prohibition of Forced or Compulsory Labor

....

d. Status of Child Labor Practices and Minimum Age for Employment

....

e. Acceptable Conditions of Work

.....

9.2 PTS

This section describes the Political Terror Scale. The information is taken from the Political Terror Scale Project.⁵³

The PTS measures levels of political violence and terror that a country experiences in a particular year based on a 5-level “terror scale” originally developed by Freedom House. The data used in compiling this index comes from two different sources: the yearly country reports of Amnesty International and the U.S. State Department Country Reports on Human Rights Practices. In the construction of an index for each year for each report, countries are scaled as if the reports are accurate and complete.

In determining the levels coders are provided with the following instructions:

- **Ignore Own Biases.** Coders should make every attempt to keep their own biases out of their work. Thus, coders are instructed to ignore their perceptions of a country, and to limit their coding to the information provided in the country report.
- **Give Countries the Benefit of the Doubt.** Coders also are instructed to give the benefit of the doubt in favor of the countries they are coding. Thus, if a coder thinks that a country could be scored as either a level 2 or a level 3, the country is to receive the lower score. Sometimes coders will not feel comfortable making a choice between two levels.

⁵³See <http://www.politicalterror scale.org/about.php>

In those instances, coders will oftentimes score a country using both numbers, such as 2/3. If the coder has either of these numbers, we use the level where there is agreement.

- **Read What the Report is Saying.** Finally, coders are instructed to read what the report is trying to say. One of the keys is to look at the adjectives used in these reports. For example, “reports” of torture is different in kind (and less serious) than “widespread” torture, which also is different (and less serious) than “systematic” torture.

One of the more difficult problems is how to deal with the situation where a country’s human rights situation changes dramatically during the course of the year. It is not out of the ordinary for a newly installed regime to pursue policies that are diametrically opposed to that which preceded it. In these instances, we instruct the coders to consider when the regime change occurred. For example, if a repressive regime was ousted late in the calendar year, the score probably should reflect the human rights situation that existed for most of the year. On the other hand, if the change occurred anywhere near the middle of the year or before then, the score should reflect this change.

The PTS assigns a score from one to five to annual country reports on human rights practices published by the U.S. State Department and Amnesty International.

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 commonplace. 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.

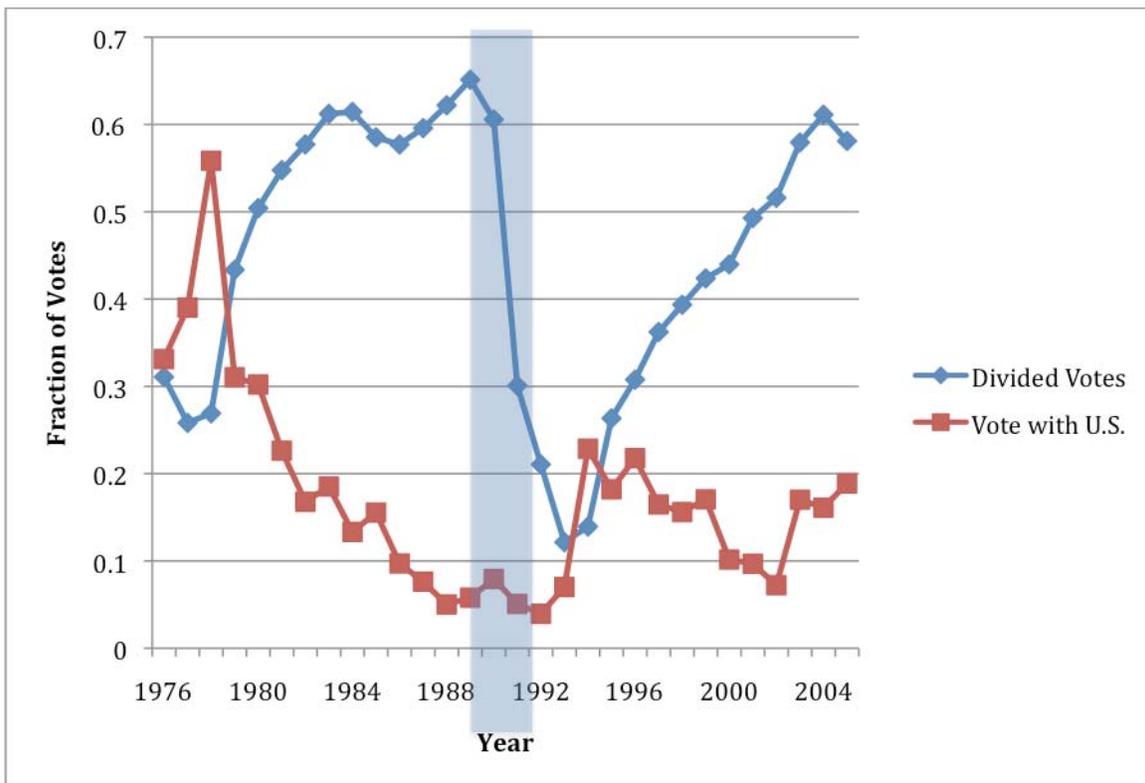
Level 5: The terrors characteristic of level-4 countries, encompass the whole population at level 5. The leaders of these societies place no limits on the means or thoroughness with which they pursue personal or ideological goals. E.g. Sudan, 2000.

Below, we provide examples of countries at different score levels, with excerpts from country reports by the U.S. State Department and Amnesty.

Level 5 – Burundi (State Department 1996): The human rights situation continued to deteriorate. The security forces continued to commit numerous, serious human rights abuses, including extrajudicial killings. Military forces committed massacres of unarmed civilian Hutus and frequently permitted Tutsi extremists to engage in violence against Hutus. The Government was largely unable to prevent such abuses, and perpetrators generally went unpunished. Serious incidents of ethnically motivated extrajudicial killing and destruction of property occurred throughout the country. Armed troops and civilian militias killed both armed and unarmed ethnic rivals, including women, children, and the elderly. They also killed expatriates.

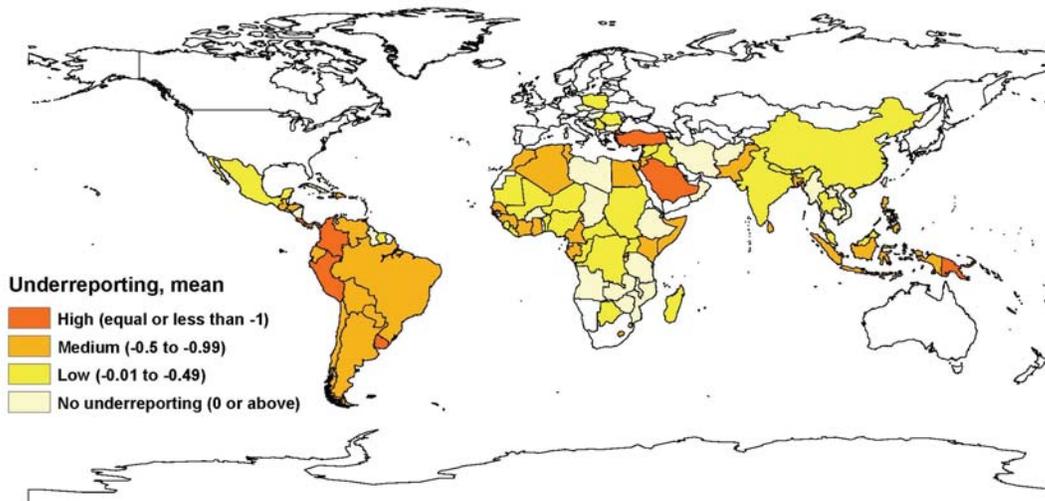
Level 1– Bahrain (Amnesty 2001): Significant steps were taken in 2001 to promote and protect human rights. All political prisoners and detainees were released and the State Security Court and state security legislation were abolished. Bahraini nationals who had been forcibly exiled or prevented from entering the country were allowed to return without conditions. An Ethiopian woman remained under sentence of death. In December, two people . . . were said to have been subjected to beatings by police officers. . . . They were detained for two days before they were released on bail.

Figure 1: Divided Votes and Average U.S. Alliance in the UN General Assembly



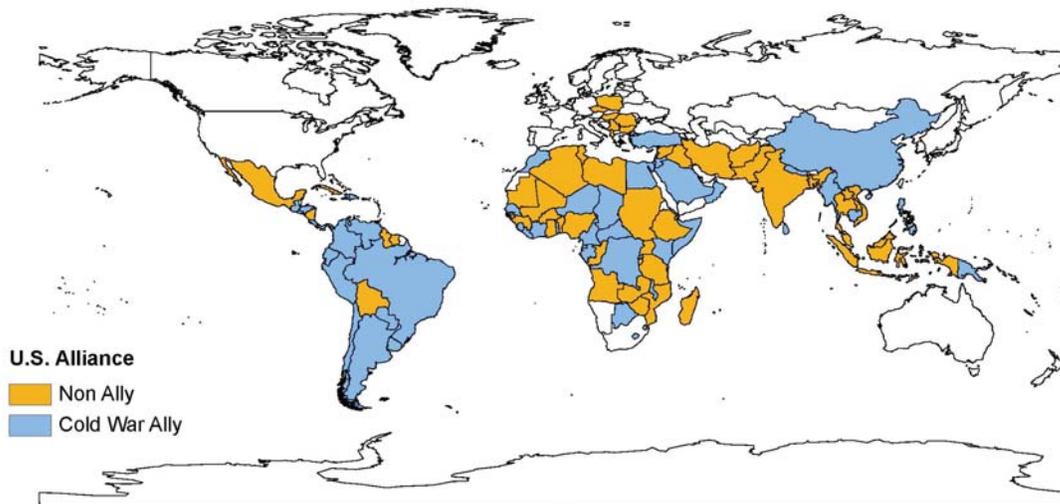
Source: Authors' computations.

Figure 2A: Map of U.S. Under-reporting



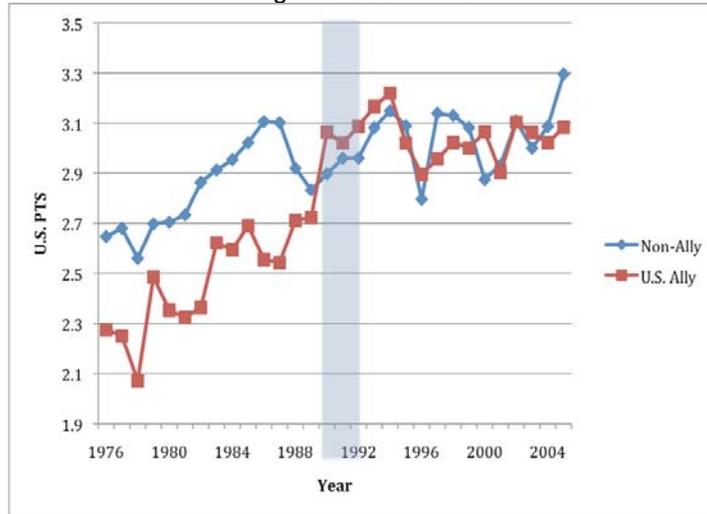
Source: Authors' Computation

Figure 2B: Map of U.S. Alliance



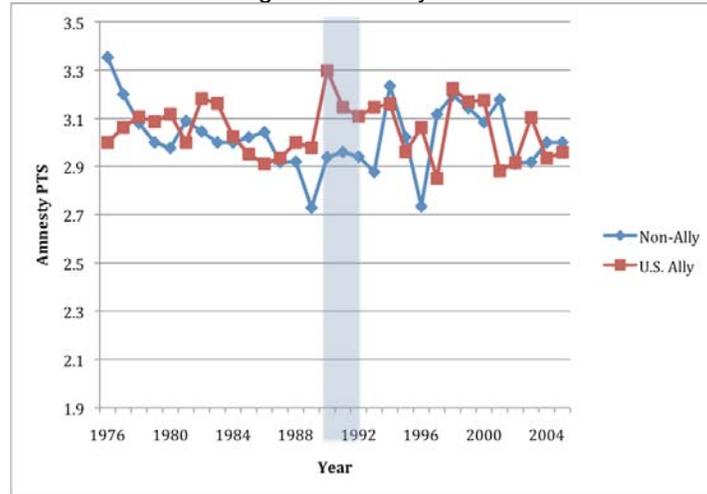
Source: Authors' Computation

Fig 3A: USSD PTS



Source: Authors Computation

Fig 3B: Amnesty PTS



Source: Authors Computation

Fig 3C: USSD - Amnesty PTS

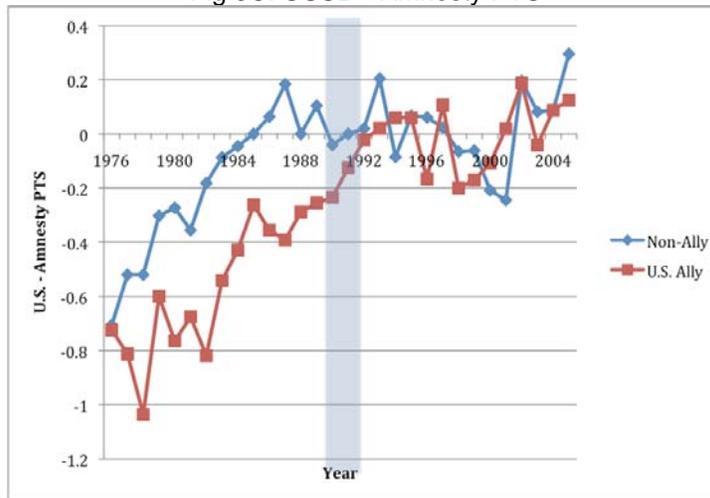


Figure 4A: Partial Correlation Plot of the Effect of UNSC x U.S. Alliance on USSD-Amnesty PTS (Countries that were ever on the UNSC)

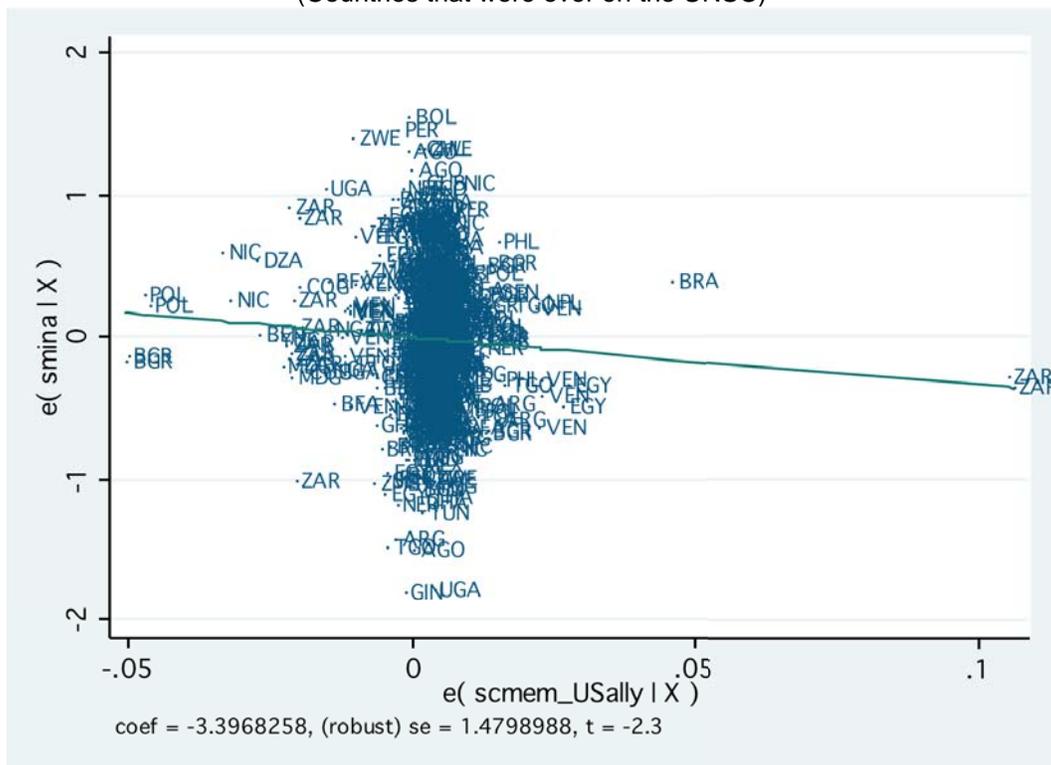


Figure 4B: Partial Correlation Plot of the Effect of UNSC x U.S. Alliance on USSD-Amnesty PTS (Countries that were ever on the UNSC, Excluding Zaire)

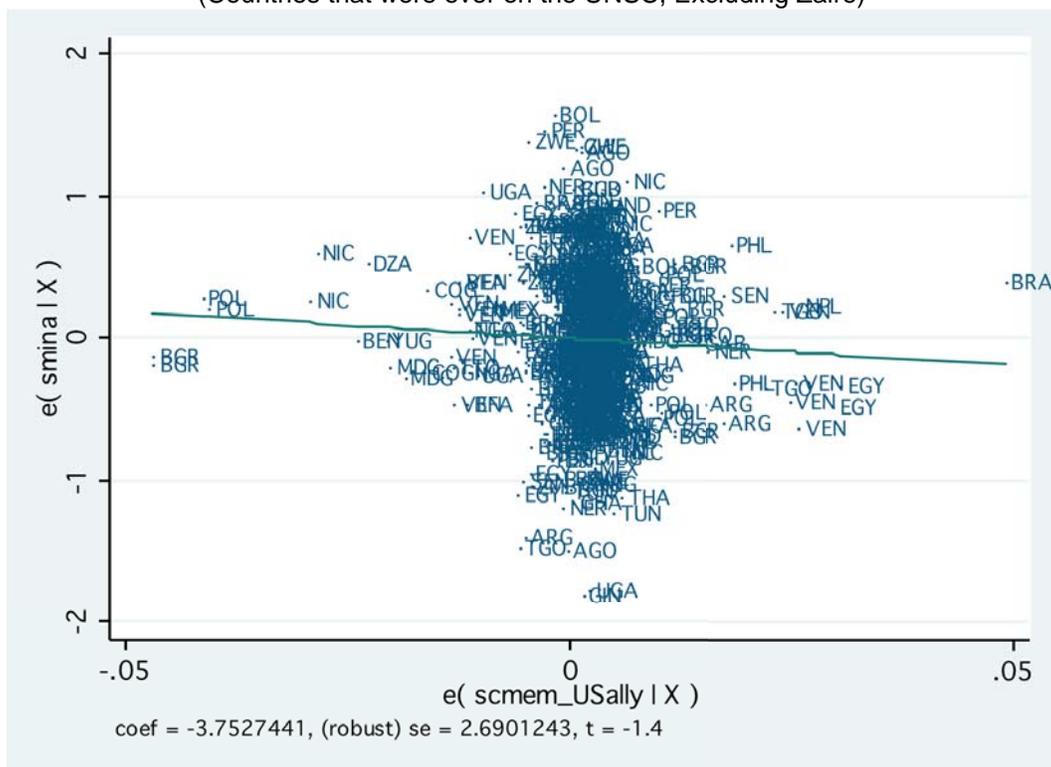


Figure 4C: Partial Correlation Plot of the Effect of UNSC x U.S. Alliance on Ln HR News Reports (Countries that were ever on the UNSC)

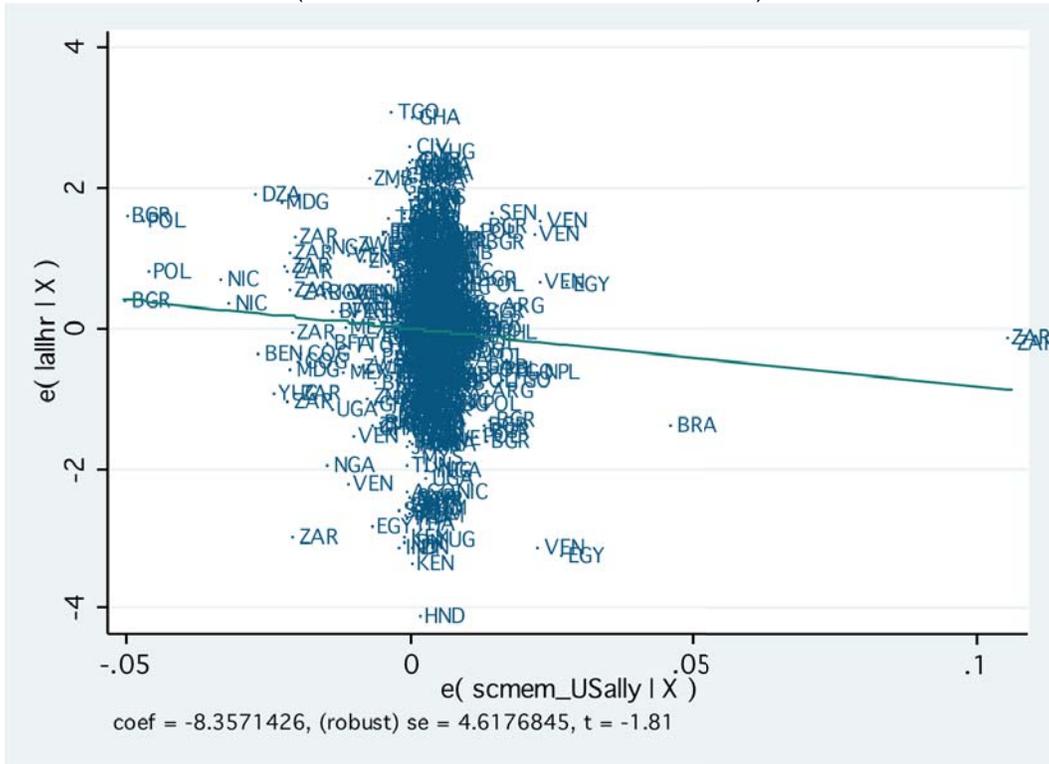


Figure 4D: Partial Correlation Plot of the Effect of UNSC x U.S. Alliance on Ln HR News Reports (Countries that were ever on the UNSC, Omit Zaire)

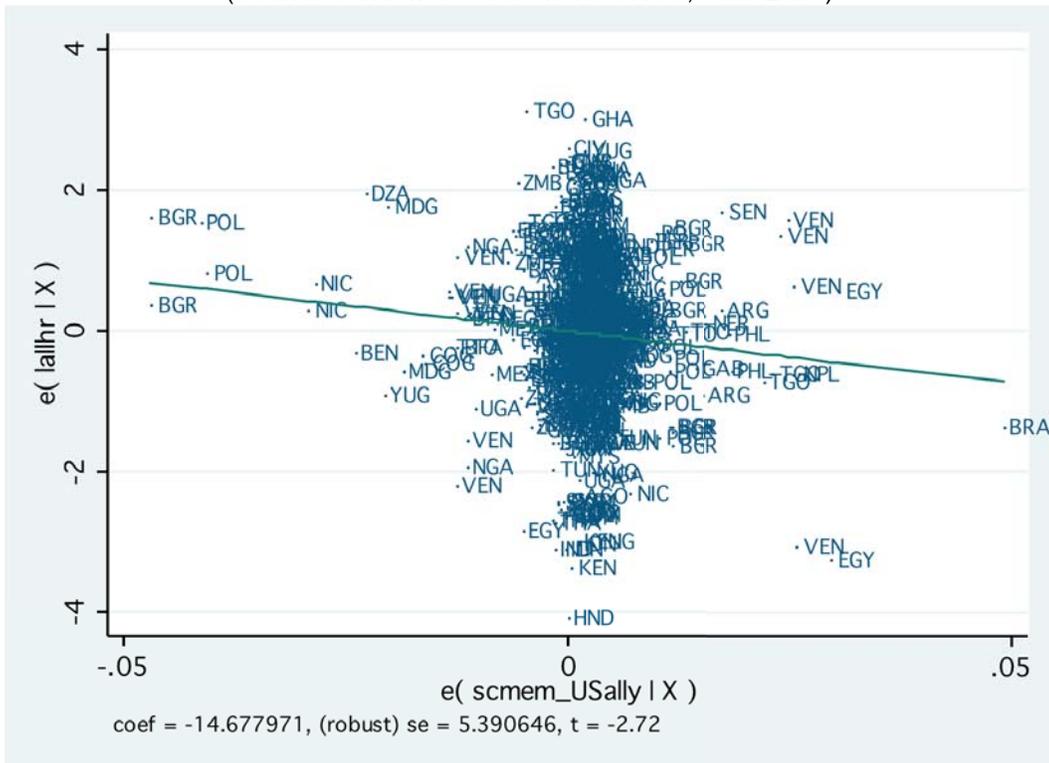


Figure 5: The Effect of U.S. Alliance * UNSC Membership during the Cold War and Afterwards

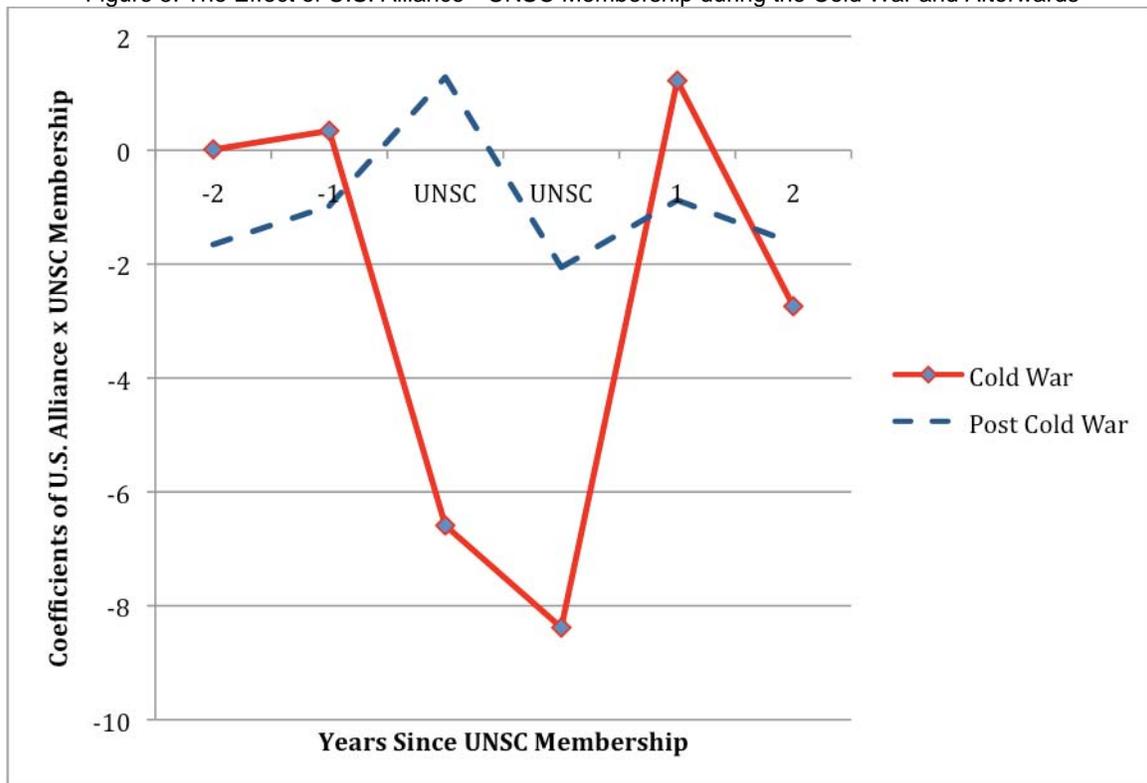


Table 1: Descriptive Statistics

Variable	All Years 1976-2005		
	Obs	Mean	Std. Errors
USSD PTS	2624	2.891	(0.021)
Amnesty PTS	2624	3.029	(0.020)
USSD -Amnesty PTS	2624	-0.138	(0.014)
U.S. Alliance	2624	0.091	(0.001)
UNSC	2624	0.063	(0.005)
HR News	2624	10.143	(0.458)
HR W Post	2624	3.501	(0.182)
HR NYT	2624	2.798	(0.129)
HR WSJ	2624	0.154	(0.018)
HR C. Tribune	2624	0.413	(0.055)
HR L.A. Times	2624	3.639	(0.173)
HR Newswires	2624	11.109	(0.555)
Distance to NYT	2624	1463.779	(21.895)
No Media Freedom	2624	0.393	(0.010)
All News NYT	2624	26.488	(1.304)

Table 2: OLS Estimate of the Correlation between Newspaper Articles and U.S. and Amnesty PTS Scores

	Dependent Variable: Ln HR News Reports							
	Cold War 1976-88				Post Cold War 1992-2005			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mean of Dep. Var	0.1 (not logged 7.479)				0.925 (not logged 12.128)			
USSD- Amnesty PTS			0.417 (0.0925)	0.416 (0.0921)			0.150 (0.0695)	0.152 (0.0693)
USSD	0.453 (0.0807)				0.202 (0.0676)			
Amnesty		0.259 (0.0727)	0.491 (0.0882)	0.491 (0.0882)		0.163 (0.0629)	0.258 (0.0796)	0.260 (0.0795)
UNSC	N	N	N	Y	N	N	N	Y
Observations	1010	1010	1010	1010	1325	1325	1325	1325
R-squared	0.726	0.717	0.726	0.726	0.761	0.760	0.762	0.762

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

Table 3: The Effect of UNSC x U.S. Alliance of USSD – Amnesty PTS Scores

	Dependent Variables:								
	USSD PTS		Amnesty PTS		USSD-Amnesty PTS				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All	All	All	All	All	All	All	EverSC	EverSC, Omit Zaire
A. Cold War 1976-88									
Mean of Dep. Var.	2.68		3.029		-0.349				
U.S. Alliance x UNSC	-2.849 (4.249)	-3.022 (1.649)	2.622 (3.303)	1.212 (2.867)	-5.472 (2.585)	-4.234 (2.278)	-3.560 (1.346)	-3.397 (1.480)	-3.753 (2.690)
UNSC	0.0936 (0.312)	0.252 (0.148)	-0.306 (0.262)	-0.119 (0.237)	0.399 (0.207)	0.372 (0.186)	0.306 (0.118)	0.294 (0.129)	0.315 (0.180)
Amnesty							-0.556 (0.0491)	-0.638 (0.0604)	-0.630 (0.0599)
U.S. Alliance	-1.864 (1.177)		0.500 (1.214)		-2.363 (0.502)				
Country FE	N	Y	N	Y	N	Y	Y	Y	Y
Observations	1010	1010	1010	1010	1010	1010	1010	607	595
R-squared	0.050	0.664	0.007	0.622	0.143	0.363	0.552	0.551	0.547
B. Post Cold War 1992-2005									
Mean of Dep. Var.	3.046		3.033		0.012				
U.S. Alliance x UNSC	-1.478 (2.768)	1.246 (1.086)	-1.427 (2.662)	0.401 (1.140)	- (0.993)	0.845 (1.168)	1.099 (0.971)	1.162 (1.019)	1.187 (1.018)
UNSC	0.0852 (0.287)	-0.173 (0.122)	0.141 (0.277)	0.0868 (0.144)	- (0.146)	- (0.166)	-0.141 (0.121)	-0.151 (0.122)	-0.153 (0.122)
Amnesty							-0.634 (0.0343)	-0.650 (0.0478)	-0.648 (0.0483)
U.S. Alliance	-0.475 (1.377)		-0.831 (1.298)		0.355 (0.345)				
Country FE	N	Y	N	Y	N	Y	Y	Y	Y
Observations	1325	1325	1325	1325	1325	1325	1325	737	723
R-squared	0.009	0.753	0.012	0.668	0.028	0.181	0.501	0.505	0.506

All regressions control for year fixed effects.

Standard errors are clustered at the country level.

Table 4: The Effect of UNSC x U.S. Alliance on Human Rights on News Coverage

	Dependent Variable: Ln HR News					
	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	All	All	Ever SC	Ever SC, Omit Zaire
A. Cold War 1976-1988						
Mean of Dep. Var.	0.1 (not logged 7.479)					
U.S. Alliance x UNSC	-7.836 (8.881)	-8.184 (5.312)	-10.88 (7.397)	-8.489 (4.833)	-8.357 (4.618)	-14.68 (5.391)
UNSC	0.949 (0.729)	0.691 (0.385)	1.303 (0.633)	0.721 (0.355)	0.730 (0.344)	1.106 (0.353)
Amnesty			1.160 (0.0853)	0.251 (0.0726)	0.231 (0.0837)	0.206 (0.0809)
U.S. Alliance	4.615 (2.632)		4.036 (1.619)			
Country FE	N	Y	N	Y	Y	Y
Observations	1010	1010	1010	1010	607	595
R-squared	0.033	0.718	0.288	0.723	0.752	0.756
B. Post Cold War 1992-2005						
Mean of Dep. Var.	0.925 (not logged 12.128)					
U.S. Alliance x UNSC	3.356 (3.996)	-2.507 (2.015)	4.608 (3.920)	-2.573 (1.934)	-2.916 (1.982)	-2.831 (1.964)
UNSC	0.300 (0.448)	0.390 (0.218)	0.176 (0.418)	0.405 (0.212)	0.444 (0.222)	0.435 (0.222)
Amnesty			0.877 (0.0985)	0.165 (0.0630)	0.210 (0.0874)	0.204 (0.0887)
U.S. Alliance	-0.304 (2.616)		0.425 (2.085)			
Country FE	N	Y	N	Y	Y	Y
Observations	1325	1325	1325	1325	737	723
R-squared	0.023	0.759	0.222	0.761	0.746	0.746

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

Table 5: The Effect of UNSC x U.S. Alliance on PTS and News Coverage – Robustness

	Dependent Variables							
	Cold War 1976-1988				Restricted (Reagan) Cold War 1980-88			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	All	EverSC	EverSC, Omit Zaire	All	All	EverSC	EverSC, Omit Zaire
A. U.S. - Amnesty PTS								
U.S. Alliance x UNSC	-3.560 (1.346)	-3.345 (1.554)	-3.120 (1.673)	-3.785 (3.069)	-3.515 (1.487)	-3.620 (1.625)	-3.120 (1.673)	-4.043 (3.541)
UNSC	0.306 (0.118)	0.279 (0.148)	0.267 (0.160)	0.307 (0.220)	0.285 (0.130)	0.314 (0.158)	0.267 (0.160)	0.360 (0.242)
Amnesty	-0.556 (0.0491)	-0.665 (0.0451)	-0.745 (0.0521)	-0.742 (0.0521)	-0.611 (0.0581)	-0.757 (0.0628)	-0.745 (0.0521)	-0.846 (0.0820)
Country Time Trends	N	Y	Y	Y	N	Y	Y	Y
Observations	1010	1010	607	595	776	776	607	456
R-squared	0.552	0.646	0.635	0.631	0.585	0.690	0.635	0.672
B. Ln HR News								
U.S. Alliance x UNSC	-8.489 (4.833)	-6.083 (4.259)	-6.211 (4.031)	-11.28 (5.977)	-8.410 (5.888)	-8.695 (6.493)	-8.856 (6.168)	-19.17 (6.364)
UNSC	0.721 (0.355)	0.579 (0.340)	0.600 (0.325)	0.902 (0.403)	0.690 (0.412)	0.671 (0.440)	0.689 (0.411)	1.279 (0.403)
Amnesty	0.251 (0.0726)	0.162 (0.0865)	0.205 (0.0923)	0.188 (0.0905)	0.237 (0.0840)	0.138 (0.111)	0.217 (0.140)	0.194 (0.133)
Country Time Trends	N	Y	Y	Y	N	Y	Y	Y
Observations	1010	1010	607	595	776	776	465	456
R-squared	0.723	0.768	0.788	0.792	0.765	0.816	0.837	0.843

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

Table 6: The Effect of UNSC x U.S. Alliance on HR News Coverage –Differential Effects

Dependent Variables		Independent Variables					Obs	R-Sq
		U.S. Alliance x UNSC	U.S. Ally x UNSC x Dist	U.S. Ally x UNSC x No Media Freedom	U.S. Ally x UNSC x Newswires	U.S. Ally x UNSC x Carter		
(1)	LnHRNews	-8.404 (4.840)					607	0.743
(2)	LnHRWPost	-11.25 (3.376)					607	0.642
(3)	LnHRNYT	-9.670 (2.981)					607	0.654
(4)	LnHRWSJ	-8.205 (3.949)					607	0.438
(5)	LnHRCHI	-5.061 (3.480)					496	0.557
(6)	LnHRLATimes	-2.805 (5.250)					607	0.629
(7)	LnAllNewsNYT	-2.304 (4.572)					460	0.858
(8)	LnHRNYT	-15.28 (6.549)	0.00336 (0.00300)				595	0.654
(9)	LnHRNews	-11.47 (7.318)		4.495 (9.262)			607	0.743
(10)	LnHRNews	-5.327 (4.339)			-0.285 (0.102)		588	0.736
(11)	LnHRNews	-9.908 (6.117)				33.78 (14.34)	607	0.746

All regressions control for UNSC dummy, Amnesty PTS, and country and year fixed effects.

Standard errors are clustered at the country level.

Sample is restricted to countries that were on the UNSC at least once.

Table 7: The OLS and 2SLS Effect of U.S. Under-reporting on News Coverage

	Dependent Variables: Ln HR News Articles							
	(1) LnHRNews	(2) LnHRNYT	(3) LnHRWPost	(4) LnHRWSJ	(5) LnHRNews	(6) LnHRNYT	(7) LnHRWPost	(8) LnHRWSJ
USSD- Amnesty PTS	0.306 (0.109)	0.258 (0.109)	0.340 (0.134)	0.0900 (0.108)	2.408 (1.740)	2.757 (1.297)	3.207 (1.768)	2.341 (1.336)
Observations	607	595	595	595	1010	595	595	595
Average Effect*	-0.107	-0.0902	-0.119	-0.0315	-0.843	-0.965	-1.122	-0.819
p-value	0.00678	0.0214	0.0137	0.408	0.169	0.0379	0.0749	0.0852

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-Amnesty PTS x -0.35.

Table 8: The Average Effect of Under-reporting during the Cold War

(1) Country	(2) U.S. Alliance during CW	(3) Number of Annual HR Stories during CW	(4) Effect of Being on UNSC on U.S. PTS Underreporting	(5) % Effect of Being on UNSC on U.S. HR News Coverage
			(2) x -3.397+0.372	(exp[(2) x -8.357+0.775]-1) x 100
Brazil	0.12	11.58	-0.03	-20.21%
Congo, Dem. Rep.	0.20	4.42	-0.30	-58.30%
Honduras	0.20	11.36	-0.31	-59.81%
Chile	0.27	44.75	-0.53	-76.31%

Appendix Figure A1: NYT Human Rights Coverage after the Release of USSD Country Reports

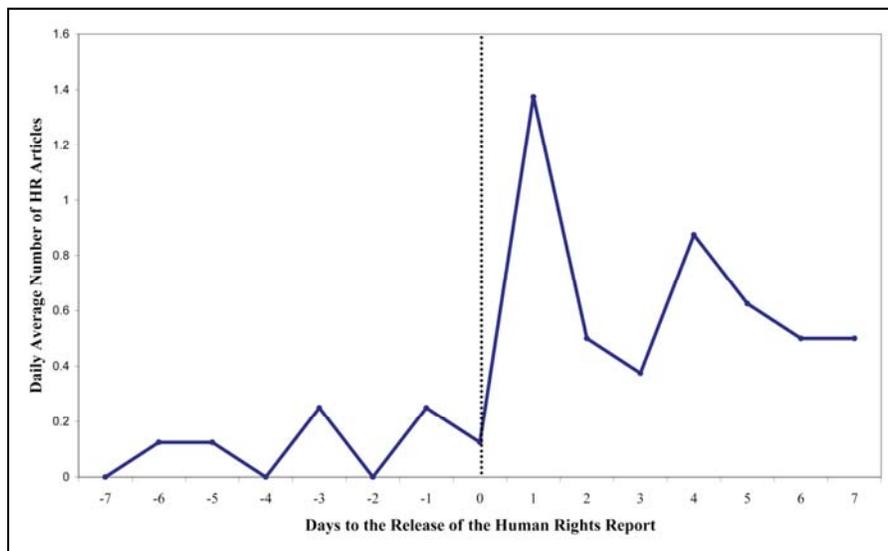


Table A2: Human Rights News Coverage in U.S. Newspapers

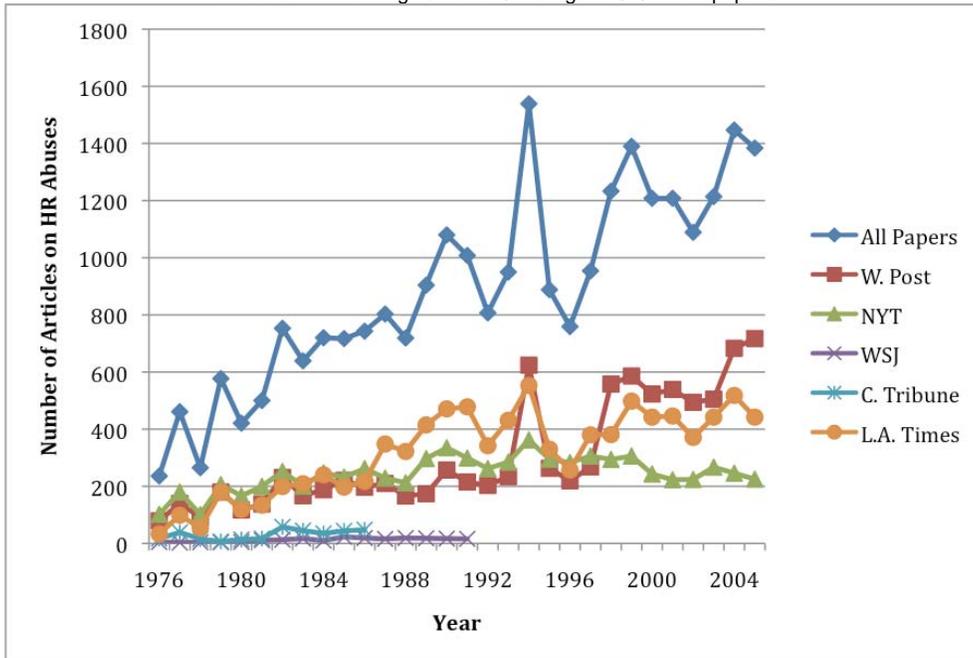


Figure A3: White House Brady Room Seating Chart from March 2009



Appendix Table A1: The Correlation between Human Rights Reports and Foreign Aid

	Dependent Variables											
	(1) LnODA	(2) LnODA	(3) LnODA	(4) LnUSAid	(5) LnUSAid	(6) LnUSAid	(7) LnUSMilAid	(8) LnUSMilAid	(9) LnUSMilAid	(10) LnUSEAid	(11) LnUSEAid	(12) LnUSEAid
USSD	-0.138 (0.0346)			-0.0946 (0.0562)			-0.0311 (0.0480)			-0.0933 (0.0542)		
Amnesty		-0.103 (0.0301)	-0.150 (0.0377)		-0.0394 (0.0469)	-0.0822 (0.0610)		0.00338 (0.0462)	-0.0167 (0.0563)		-0.0458 (0.0452)	-0.0854 (0.0589)
USSD-Amnesty PTS			-0.118 (0.0392)			-0.114 (0.0585)			-0.0534 (0.0434)			-0.105 (0.0566)
Observations	2438	2438	2438	2219	2219	2219	2220	2220	2220	2220	2220	2220
R-squared	0.749	0.747	0.749	0.749	0.747	0.749	0.669	0.669	0.669	0.746	0.744	0.746

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

Figure A2: The Effect of UNSC x U.S. Alliance on U.S. Under-reporting by Years since UNSC Membership

Dependent Variable: U.S. -Amnesty PTS		
	(1)	(2)
	Cold War	Post Cold War
U.S. Alliance x UNSC - 2	0.0141 (2.332)	-1.656 (1.466)
U.S. Alliance x UNSC -1	0.342 (3.424)	-0.978 (1.393)
U.S. Alliance x UNSC 1	-6.590 (5.150)	1.284 (1.456)
U.S. Alliance x UNSC 2	-8.383 (4.453)	-2.057 (1.699)
U.S. Alliance x UNSC +1	1.222 (4.009)	-0.878 (1.248)
U.S. Alliance x UNSC +2	-2.740 (4.550)	-1.631 (1.734)
U.S. Alliance	-2.423 (0.536)	0.464 (0.358)
Observations	998	1311
R-squared	0.148	0.033
Joint F for UNSC Years	16.57	22.43
p-value	5.89e-07	7.33e-09

Regressions control for country and year fixed effects.

Standard errors are clustered at the country level.

Table A3: OLS and Tobit Estimates of the Effect of UNSC x Alliance on News Coverage

	Dependent Variable: HR News Articles			
	(1)	(2)	(3)	(4)
Dependent Variable:	HRNews	HRNews	LnHRNews	LnHRNew
Estimator:	OLS	OLS	OLS	Tobit
Sample:	Full	HRNews >= 2	Full	Full
U.S. Alliance x UNSC	-79.03 (66.99)	-127.0 (119.2)	-8.489 (4.833)	-8.735 (3.848)
UNSC	5.837 (5.764)	9.102 (10.63)	0.721 (0.355)	0.800 (0.306)
Observations	1010	486	1010	1010

All regressions control for UNSC dummy, Amnesty PTS, and country and year fixed effects.

Standard errors are clustered at the country level.