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Nancy Qian, Yale University and CEPR
David Yangagizawa, IIES, Stockholm University

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Centre for Economic Policy Research
53–56 Gt Sutton St, London EC1V 0DG, UK
Tel: (44 20) 7183 8801, Fax: (44 20) 7183 8820
Email: cepr@cepr.org, Website: www.cepr.org

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ABSTRACT

Watchdog or Lapdog? Media and the U.S. Government

Abstract This paper investigates the extent to which strategic objectives of the U.S. government influenced news coverage during the Cold War. We establish two relationships: 1) strategic objectives of the U.S. government cause the State Department to under-report human rights violations of strategic allies; and 2) these objectives reduce news coverage of human rights abuses for strategic allies in six U.S. national 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 are able to provide qualitative evidence and indirect quantitative evidence to shed light on the mechanisms underlying the reduced form effects.

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Nancy Qian
Department of Economics
Yale University
PO Box 208268
New Haven, CT 06520-8269
USA

David Yangagizawa
IIES
Universitetsvägen 10
Stockholm University
SE-106 91 Stockholm
SWEDEN

Email: nancy.qian@yale.edu

Email: yana@iies.su.se

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“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 influence the information reported by media outlets that it own (Enikolopov, Petrova and Zhuravskaya, 2009; Durante and Knight, 2009).¹ Its ability to do so with independently owned outlets operating in a competitive market is less obvious. In theory, market competition and independent ownership should act as safeguards against government manipulation of the media (Besley and Prat, 2006).² According to this, the U.S. media should be safer from government influence than most other media markets in the world (Djankov, et al., 2000).³ In practice, although studies such as Prat and Stromberg (2005) suggest that competition in the U.S. increases the likelihood that news organizations will report the truth, the extent to which the U.S. media is free from government influence is an open empirical question.⁴ This is somewhat surprising given the many historical controversies over the media, the “watchdog” of American democracy, being manipulated by the government (e.g. *Iran-contra* during mid 1980s; and more recently, in 2003, preceding the U.S. invasion of Iraq) and the growing number of studies finding that the media has real political, economic and social consequences.⁵

¹For example, 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. Similarly, the finding by Enikolopov, Petrova and Zhuravskaya (2009) that viewers with access to more independent stations in Russia are more likely to vote against the government party implies that government owned stations promote the government’s agenda.

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

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

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

⁵Blanton (2001) provides an overview 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* (Herman and Chomsky, 2002: p. 139) . For detailed accounts of when the media allows the government to

This study attempts to fill this gap by measuring the extent to which the U.S. government can systematically influence news coverage of the commercial press. In particular, we aim to estimate the effect of strategic objectives on State Department reports of human rights abuses in foreign countries and the effect of these objectives on news coverage in six independently owned national U.S. newspapers during the latter part of the Cold War, 1976-88.⁶ The former reveals the extent to which official government publications respond to strategic objectives and provides evidence for the hypothesis that strategic objectives cause the U.S. government to attempt to bias reports of human rights practices for foreign countries. The latter investigates the extent to which strategic objectives can also affect commercial news coverage. Together, these two relationships address the question of how much influence the U.S. government has on news coverage by independently owned media firms.

We face several empirical difficulties. First, strategic objectives are unobservable and it is difficult to measure the government's effort in attempting to manipulate information. The second problem is reverse causality. Are strategic objectives driving government and commercial news reports of human rights abuses? Or are they responding to the latter (Stromberg, 2004)?⁷ Finally, there is the problem of omitted variable bias. This is particularly problematic in estimating the effect of strategic objectives on news reports because both may be outcomes of a third factor, such as public opinion. For example, in the months before the U.S.-led 2003 invasion of Iraq, both the U.S. government's strategic desire to invade Iraq and news coverage of human rights abuses by the Saddam Hussein led government may have been reactions to distort reports, see Bennet, Lawrence and Livingston (2007) and Thomas (2006).

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

⁶We examine the content of all papers that are available for the main period of our study in the ProQuest Historical database: the *New York Times*, *Washington Post*, *Wall Street Journal*, *Chicago Tribune*, *Los Angeles Times* and *Christian Science Monitor*.

For convenience, we will henceforth use the terms U.S. "government" and "State Department" interchangeably to mean the executive office and its administration. Similarly, we will use the term "public opinion" to refer to voters and Congress. The logic for this relies on the assumption that Congress is more immediately sensitive to public opinion. Since our data will not be rich enough to allow us to examine Congress and voters separately, we refer to both of these parties as the "public" for convenience.

⁷For 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.

Americans' anxiety over problems in the Middle East after 9/11. In this case, the correlation will show that U.S. strategic objectives and news coverage are highly correlated. But the correlation will confound the effect of U.S. strategic objectives and reader preferences and will most likely overstate the true effect of strategic objectives.

The principal contribution of this study is to address the aforementioned problems and to provide empirical evidence on the causal effect of strategic objectives on U.S. commercial news coverage. First, we infer strategic objectives, which will be interchangeably referred to as government *bias* in this paper, from the difference in the quantitative 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, an independently financed and operated human rights organization. Interpreting this difference as U.S. government bias assumes that differences between U.S. and Amnesty scores are driven by U.S. strategic objectives and that Amnesty reports are not driven by U.S. reports. We need not assume that Amnesty reports the truth.

Second, to establish causality, we exploit the plausibly exogenous variation in strategic objectives that results from an ally's entry onto the United Nations Security Council (UNSC). We measure alliance using General Assembly (UNGA) voting patterns. The U.S. values alliance and provides benefits to its allies in return for the option value or realization of favors such as supporting votes in the United Nations.⁸ The U.S.'s value for allies increases when these countries enter onto the Security Council and have more opportunities to vote on issues that are crucial to the U.S. Because uncertainty in factors such as domestic politics makes it difficult for allied countries to fully commit to their behavior on the Council in advance, this increase in power over critical issues will be paralleled by an increase in benefits from the U.S.

One form that the benefits can take is favorable human rights reports. The reports are valued by leaders of allied countries because favorable reports from the U.S. can influence their prestige. More importantly, they influence U.S. Congressional support for the executive administration's foreign policies. By all appearances, the American public values good human

⁸The value for alliance is illustrated in Figures 3A and 3B which plot the average Political Terror Scale (PTS) scores for U.S. allies and non-allies as reported by the U.S. State Department and Amnesty during 1976-2005. Allies are defined as countries that voted with the U.S. more than the median country in the sample on average. It shows that during the Cold War, the U.S. systematically reported its allies as having better human rights behavior. However, after the Cold War, the scores of allies converge to those of non-allies. In contrast, Amnesty reported U.S. allies and non-allies similarly before and after the Cold War.

rights practices in foreign countries. Therefore, Congress will be less likely to object to providing support (e.g. military aid) for a country if that country is known to have acceptable human rights practices. Conversely, Congress will be less likely to object to aggressive policies towards a country if that country is known for human rights abuses. It follows that the government will then want to understate the abuses of its strategic allies relative to opponents in government publications that are presented to Congress such as the State Department’s annual Country Reports and in news outlets that deliver information to both members of Congress and voters.⁹

Our strategy estimates a “first stage” effect of the interaction effect of alliance and Council membership on the difference between USSD and Amnesty reports and a “reduced form” effect of the interaction effect of alliance and Council membership on news coverage of human rights abuses. The first estimate establishes the extent to which Council membership of allied countries results in increased under-reporting of human rights abuses by the USSD, and thus provide evidence for whether Council membership of allied countries caused the U.S. government to strategically manipulate information about human rights abuses in these countries. The second estimate provides evidence for the extent to which an increase in strategic value to the U.S. government results in reduced coverage of human rights abuses in the commercial press.¹⁰

The data are a country-level panel compiled from several existing sources. The two main outcome measures are the difference in U.S. and Amnesty PTS scores of human rights abuses and the number of stories about a foreign country’s human rights abuses published in six national American newspapers.¹¹

The results show that an increase in strategic value to the U.S. significantly reduced

⁹Section 2 on the Background provides a more detailed discussion and documents the relevant qualitative evidence on the relationship between the executive administration, Congress, media and the public.

¹⁰Because the Country Reports are just one of the many instruments the government can use to influence the media, the interaction between alliance and UNSC membership is not an excludable instrument for Country Reports for a structural estimate of the effect of Country Reports on media coverage. See Section 4 for a detailed discussion of the empirical strategy.

¹¹We focus on the number of stories because we follow existing studies of the U.S. media in assuming that it is costly for newspapers to publish inaccurate facts. Therefore, the margin for distortion will be in the composition of stories. For example, a newspaper can choose between publishing two true stories on human rights abuses; one is about the socialist Sandinistas and the other is about El Salvador, a U.S. ally. Our estimates reveal the extent to which newspapers systematically chooses to publish stories of the former over the latter.

State Department scores of human rights abuses relative to Amnesty scores, and significantly reduced news coverage of abuses in the commercial press. For Cold War allies such as Brazil, Zaire, Honduras and Chile, UNSC membership during the Cold War decreased newspaper reports of abuses in these countries by approximately 29.5%, 66%, 57.4% and 82%.

The main results are consistent with the qualitative evidence presented in Section 2.3 that the government is able to systematically influence news coverage. The main competing explanation is that the results are driven by consumer preferences. In the empirical analysis, we provide several pieces of evidence to suggest that this is not very likely in our context. Moreover, we find that the magnitudes of the effects across newspapers are uncorrelated with readership preferences.¹² See Section 5.3 for a detailed discussion.

In addition, we provide suggestive evidence towards the mechanisms underlying the the main results. Government influence can be achieved either directly by incentivizing journalists and editorial boards, or indirectly by manipulating the supply of information to journalists. To investigate the latter, we examine if the effect of strategic objectives on news coverage is larger when it is costlier for the newspaper to obtain independent information. Our results show that the cost of independent information does not affect the extent of government distortion, which suggests that the main force behind government influence is most likely to be direct government manipulation of the incentives of editorial boards and journalists.

Interestingly, our results show that the extent of government distortion is positively correlated with quality across newspapers. The higher the quality of news reporting, the larger is the estimated effect of government influence. In the Section 6, we discuss the potential implications of this surprising stylized fact under the framework of government capture of the media provided by Besley and Prat (2006).

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

This study makes several contributions. First, it complements existing studies of the determinants of news coverage which have primarily focused on the effect of direct government

¹²Note that our empirical 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 because that would bias against our finding that Council membership of allies cause under-reporting of bad behavior.

ownership (Enikolopov, Petrova and Zhuravskaya, 2009; Durante and Knight, 2009) or consumer driven distortions (e.g. Mullainathan and Shleifer, 2005; and Gentzkow and Shapiro, 2006). To the best of our knowledge, our study is the first to provide evidence that independently owned news outlets can be systematically influenced by the government. Our findings also complement the theoretical work of Besley and Prat (2006) which predict that in a competitive market, government capture is positively correlated with the quality of a news outlet in equilibrium.¹³ Second, we add to the small but growing number of economic studies exploring the causes and consequences of U.S. government foreign policy. Thus far, these have been limited to outcomes that affect foreign countries such as U.S. foreign aid (e.g. Alesina and Dollar, 2000; Kuziemko and Werker, 2005), or outcomes for U.S. firms such as stock prices (Dube, Kaplan and Naidu, 2009) and terms of trade (Easterly et al., 2009).¹⁴ Our study broadens the scope of this literature by examining the effect of U.S. foreign policy on the American public. Finally, we provide a measure of government bias and a source of plausibly exogenous variation that can be easily used by future researchers in economics and political science.

This paper is organized as follows. Section 2 discusses the background of the Cold War and the United Nations, and documents historical cases of government manipulation of the media and the government's use of human rights practices in portraying its strategic allies and opponents. Section 3 describes the data used in this paper. Section 4 presents the empirical strategy. Section 5 presents the results. Section 6 interprets the results. Section 7 offers concluding remarks.

2 Background

This section has four aims. First, it describes the political competition between the U.S. and U.S.S.R. during the Cold War and how this influenced the U.S.'s value of political alliance during this era. Second, it describes the value of votes in the United Nations and how the

¹³In their paper, this is the prediction when news outlets choose the quality of reporting. See Section 6 for a more detailed discussion.

¹⁴A related empirical literature examines the effects of U.S. military operations on democracy in foreign countries. See Bueno de Mesquita and Downs (2006) for a review of the literature. See also Easterly, Satyanath and Berger (2008). There is also a literature about the effects of political interests on trade which typically focus on the effects of lobbying interest groups.

additional power a country gains when it enters the Council together with a country's inability to fully commit to behave in accordance with U.S. interests will cause the U.S. to increase bribes to allies when they are Council members. Third, we document known cases of government interferences with news coverage during this period to provide some insight on some of the methods the government used and the government's motivation for influencing the media. Finally, we document the government's focus on portraying the human rights practices of their allies favorably and of their opponents unfavorably.

2.1 The Cold War

The "Cold War", which began after World War II in 1945 and lasted until 1989/91, refers to the continuous political conflict, military tension and economic competition between the USSR and its satellite states (consolidated by the Warsaw Pact 1955-91) and the United States and 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; 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. favored its allies in terms of favorable human rights reports (Stohl and Carleton, 1985; Mitchell and McCormick, 1988; Poe, Carey and Vasquez, 2001). Qian and Yanagizawa (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.¹⁵

2.2 The United Nations

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

Two of the five principal organs of the United Nations are the General Assembly and the Security Council. The United Nations General Assembly (UNGA) is 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. It currently has 192 countries, of which more than two-thirds are developing countries. 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. More importantly, in case of a split vote in the UNSC when no veto is exercised, the issue goes for vote in the General Assembly.

¹⁵A well known example that illustrates the decline of the U.S. value for Cold War allies after the Cold War's end is Zaire (renamed the Democratic Republic of Congo in 1997). Its president, Mobutu Sese Seko (in office 1965-1997), a strong supporter of the U.S. during the Cold War, had been repeatedly criticized for human rights abuses. However, 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. 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).

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. 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).¹⁶ There are ten temporary seats that are 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 Nations General Assembly.¹⁷ 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.

Rotating members have substantial power on the Council. First, they have as much influence as the P5 in setting the agenda. Second, although the P5 has the power to veto, they rarely exercised this power during this period (Winter, 1996; O’Niell, 1996). This can be seen from the fact that deadlocks, which can only occur if no member of the P5 vetoes a resolution, have occurred ten times in the history of the UN. Nine of those occurred during the Cold War.¹⁸ The fact that temporary members have influence in the Council is consistent with the finding that they can result in higher U.S. foreign aid (Kuziemko and Werker, 2006; Dreher et al., 2009).

The U.S.’s motivation to buy the votes of countries in the General Assembly and the Security Council follow from the same logic as standard models of vote-buying and pork-

¹⁶This was the basis for UN armed action in Korea in 1950 during the Korean War.

¹⁷Africa 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. There is often intense competition for these seats (Malone, 2000).

¹⁸1956 Suez Crisis; 1956 Soviet Invasion of Hungary (Hungarian Revolution); 1958 Lebanon Crisis; 1960 Congo Crisis; 1967 Six Days War; 1980 Soviet invasion of Afghanistan; 1980 Israeli-Palestinian Conflict; 1981 South African occupation of Namibia (South West Africa); 1982 Israeli Occupation of the Golan Heights (Golan Heights Law); 1997 Israeli-Palestinian conflict (East Jerusalem and Israeli-occupied territories).

The power of the votes from rotating members is consistent with the theoretical predictions by Voeten (2001), who models bargaining power within the UNSC and finds that even though members of the P5 such as the U.S. have unilateral power in vetoing resolutions, they prefer multilateral agreements.

barrel politics (Kuziemko and Werker, 2005).¹⁹ These models would predict that the amount of bribes is positively correlated with voting with the U.S.²⁰

In this study, we assume that the U.S. values alliance in both the General Assembly and the Security Council and that the value for alliance increases when a country enters the latter. If allied countries could fully commit to voting favorably with the U.S. when they are on the Council, then we should observe the U.S. giving allies a positive amount of bribes that is relatively smooth over time. However, full commitment is highly unlikely in practice since leaders of and political attitudes within allied countries can change in unpredictable ways. Therefore, in order to guarantee good behavior from the ally on the Council in case a critical issue arises, the U.S. must increase the amount of bribes during the ally's two years on the Council.²¹

2.3 Public Diplomacy

The main period of our study, 1976-1988, was characterized by an escalating commitment to fight communism on the part of the American government which climaxed during the Reagan administration (1980-88). The government had several motives for influencing the press coverage of its political allies. First, it was a way of influencing public opinion.²² Second, and probably more importantly, influencing the press was an important way of affecting congressional opinion, whose favor was necessary for legislative purposes (Blanton, 2002).

During the 1980s, the *Office of Public Diplomacy* (OPD) was officially part of the State Department and worked closely with the National Security Council (NSC). The explicit purpose of the office was to influence public and congressional opinion to garner support for

¹⁹For empirical studies of U.S. Congressional committees, see for example, Ferejohn (1974), Ray (1981), Groseclose and Stewart (1998), Stewart and Groseclose (1999), Levitt 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).

²⁰In the next section, we will observe voting patterns in the UNGA. But we will not examine voting patterns in the UNSC because most issues are discussed prior to being put onto the agenda. Therefore, the sample of issues voted on are not representative of the actual issues being deliberated by Council members.

²¹Appendix Table A1 provides suggestive evidence that U.S. allies receive more U.S. foreign aid, and this increases when they enter the Council. The same relationship is not true for Official Development Assistance (ODA), or after the Cold War, ended when the U.S.'s value for alliance has presumably decreased.

²²In the case of the *The New York Times* (which published an international version under the title of *The International Herald Tribune*), manipulation could also affect the opinion of foreign readers.

the President’s strong anti-communist agenda in a “public action” program (Parry and Kornblub, 1988).²³ 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. Information can be disseminated through the numerous government affiliated publicity events and publications. One such publication is the *Country Reports on Human Rights Practices*. Every year, it is published by the State Department and submitted to Congress.²⁴ 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.”²⁵ 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 infer the government’s bias for or against a country (e.g. government attitude or objectives).²⁶

²³For example, the following quotes demonstrate that the government intended to influence the 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 in a seminar the National Defense University attended by Oliver North, 1983; and, “...we can and must go over the heads of our Marxist opponents directly to the American people. Our targets would be within the United States, the Congress... the general public [and] media.” – Kate Semerad, an external relationship official at the Agency for International Development (AID) in 1983.

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).

²⁴<http://www.state.gov/g/drl/rls/hrrpt/>

²⁵See 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>

²⁶On 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 USSD Country Reports (see Appendix Figure A1). A similar increase of smaller magnitude is found for the three days following the release of Amnesty reports.

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

Government methods for influencing the media can be broadly categorized into two groups. First, it can manipulate the supply of information. As with Congress, the government had many instruments such as the *Country Reports on Human Rights Practices* for disseminating its points of view.²⁷ In addition, disinformation was often released directly by the OPD.²⁸ Second, the government can attempt to directly manipulate news reports by exerting pressure on editorial boards or incentivizing journalists. The OPD monitored news reports by the American media and would directly confront journalists and editors in order to convince them to change the reports.²⁹ Upon the appearance of news reports that did not conform to the wishes of the OPD, officials would press the owners and editorial boards to change their journalists in the field.³⁰ Similarly and perhaps most importantly, the OPD dealt directly with journalists using a carrot-and-stick strategy. For example, 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.

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).

²⁷The *Country Reports on Human Rights Practices* are submitted annually by the U.S. Department of State to the U.S. Congress in compliance with sections 116(d) and 502B(b) of the Foreign Assistance Act of 1961 (FAA), as amended, and section 504 of the Trade Act of 1974, as amended. The law provides that the Secretary of State shall transmit to the Speaker of the House of Representatives and the Committee on Foreign Relations of the Senate, by February 25" a full and complete report regarding the status of internationally recognized human rights, within the meaning of subsection (A) in countries that receive assistance under this part, and (B) in all other foreign countries which are members of the United Nations and which are not otherwise the subject of a human rights report under this Act."

²⁸In a letter to House Speaker Patrick Buchanan, the Deputy Director for *Public Diplomacy for Latin American and the Caribbean* (SLDP), Jonathan Miller described how the OPD was carrying out "white propaganda" operations. This included writing opinion articles under false names and placing them in leading newspapers such as the *Wall Street Journal* (Miller, 1985; Hamilton and Inouye, 1987). Similar opinion editorials were planted in the *New York Times* and the *Washington Post* (Fascell, 1987). Another example occurred on the night of Ronald Reagan's re-election, Otto Juan Reich, the director of S/LPD, handed journalists a story about how Soviet MiGs were arriving in Nicaragua that was later proven to be false (Cohen, 2001).

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

²⁹In 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).

³⁰Bill Buzenberg, the foreign affairs correspondent at *National Public Radio* during the 1980s recalled that Reich said that he had "made similar visits to other unnamed newspapers and major television networks [and] had gotten others to change some of their reporters in the field because of a perceived bias".

Uncooperative journalists also became the targets of character assassination meant to induce skepticism over the information they reported.³¹ In contrast, journalists seen as cooperative to the administration’s agenda were rewarded with increased access to government information. For example, an OPD memo stated that certain favorable correspondents had “open invitations for personal briefings” (Cohen, 2001). The exclusive nature of this access presumably made it valuable to journalists. In general, the executive administration had control over information that was very valuable to journalists. For example, they controlled access to interviews with important personnel, and even controlled who was allowed to ask questions during administration press conferences.

Note that the main results from the empirical analysis estimate the reduced form effect of strategic objectives on news coverage. It will capture the effects of both the distortion of the supply of primary information as well as the effects of more direct manipulation of the incentives of journalists and editorial boards. Later in this paper, in Section 5.5, we will attempt to investigate the contribution of the former indirectly by examining if the main effects are larger when it is more costly for the newspaper to obtain independent information. Also, note that in light of the qualitative evidence of the many known cases of government distortion, we will assume that journalists are aware of the government’s motive to manipulate news coverage in interpreting the results.

2.4 Human Rights

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

Human rights were important for reasons related both to foreign and domestic politics. First, a perception of having good human rights practices is valuable because it is often

³¹Many were accused of being disloyal to the United States or having secret agendas. 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.

tied to aid.³² Second, it is also valuable to the governments of foreign countries for non-pecuniary reasons. For example, official chastisement by a foreign government could decrease the domestic prestige of a government.³³ These two arguments are consistent with the long history of incidents where the U.S. government withdrew aid or imposed trade sanctions on countries because of human rights violations, and incidents where foreign countries rejected U.S. aid that was tied to human rights practices.³⁴ For the purposes of this paper, we take it as given that countries value favorable external reports of its human rights practices.

In summary, the discussion from this section suggests that the U.S. government values alliance for strategic reasons, and this value increases when an allied country becomes a UNSC member. Membership will therefore cause the U.S. to increase the amount of bribes to an allied country. The State Department will under-report abuse by allied governments because foreign countries dislike negative reports on their human rights practices and because Congress and voters prefer the U.S. to ally with and provide aid to governments with good human rights practices. For similar reasons, the U.S. government may attempt to suppress the amount of coverage of human rights abuses of its allies in the commercial media. Our empirical strategy will estimate the effect of strategic objectives on State Department reports of human rights violations (relative to Amnesty International reports) and the amount of

³²Jacobowitz (1985) suggests that much of this was motivated by the government’s desire to garner public and congressional support for U.S. operations that aimed to overthrow ruling governments and the perception that the public and Congress preferred that the U.S. not ally itself with governments that committed human rights abuses. The latter is consistent with the fact that in our data, improving human rights practices by one PTS point is correlated with a 10% increase in U.S. foreign aid. This estimate is statistically significant at the 1% level. The standard error is 0.026. The estimation regresses the logarithm of U.S. foreign aid receipts on the average of U.S. and Amnesty PTS scores, controlling for country and year fixed effects. They are not reported in the paper for brevity.

³³Similarly, 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. 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.

³⁴For example, in 1977, Congress insisted that the aid to Uruguay be made in installments contingent on improvements in human rights. Uruguay’s displeasure at this was voiced when it “spurned” the aid (Onic, 1977). Less than two weeks later, the Brazilian government turned down the Carter administration’s offer to ask Congress for a 50 million dollar aid package when the State Department handed a copy of the Country Report on human rights abuses in Brazil to the latter’s embassy in Washington D.C. In Brazil, this move was extremely popular with the public and even the opposition party (AP, 1977). More recently, in June, 2008, U.S. Commerce Secretary, Carlos Gutierrez, explained that the U.S. must continue its trade embargo on Cuba because the latter “systematically brutalizes its people”. (Washington Post, 2008). Qian and Yanagizawa (2008) provide more examples.

coverage in six independently owned U.S. national newspapers. Note that for the latter, our strategy will capture both the direct effects of the government manipulating the incentives of editorial boards and journalists and the indirect effects of the government manipulating the supply of primary information such as the Country Reports. See the Section 4 for a more detailed discussion.

3 Data

This study combines data from several existing sources to form a country-level panel for 1976-2005. The time span of the data is restricted by the availability of the PTS scores. We exclude the former Soviet Republics. Many did not have membership in the UN before 1991. Their exclusion should not affect the results since they were unlikely candidates for U.S. alliance during the Cold War. South Africa is omitted from the sample because it was excluded from UN activities due to UN opposition to Apartheid. The five permanent members of the UNSC are also excluded.³⁵ We further restrict the sample to country-year observations where the index is available for both Amnesty International and the U.S. State Department. Finally, we focus our study on developing countries for which the UN is arguably the principal outlet of foreign policy initiatives by restricting the sample to countries that are not classified as high income countries as defined by the World Bank.³⁶ Our matched sample contains 104 countries for thirty years.

For measuring alliance, we follow Qian and Yanagizawa (2008) by using the fraction of votes in agreement with the U.S. on UNGA resolutions for which the U.S. and U.S.S.R. (or Russia after 1991) are divided (e.g. vote in opposing directions).³⁷ Figure 1 plots the fraction of divided votes over time. It shows that as Cold War tensions escalated in the 1980s, the fraction of divided votes increased from approximately 30% during the late 1970s to almost

³⁵In 1978, China's seat on the UNSC was transferred from Taiwan to the People's Republic of China. Neither will be in our sample.

³⁶High income countries are defined to be those with 2007 GNP per capita of \$11,456 or more. This restriction is similar to the one made in Kuziemko and Werker (2005). Our results are very similar when we do not make this restriction. The similarity is most likely due to the fact that high income countries, which are mostly in Europe and North America, are allied to the U.S. or the USSR by treaty. Favorable human rights reports and press coverage are not likely to be the main tools in which the U.S. government negotiates with these countries. These results are omitted for brevity and available upon request.

³⁷Each year there are approximately 100-150 resolutions in the UNGA, of which the U.S. and U.S.S.R. disagree on approximately 70-90.

70% in the late 1980s. Also plotted are the fraction of votes with the U.S. averaged over all the divided votes each year.³⁸ 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, and more importantly, because using voting patterns from years where there were very few divided issues produces a very noisy measure of alliance.³⁹ 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 in the sample (approximately 7% of the time).

We do not make a separate measure of alliance based on voting patterns during the post-Cold War period because there were many fewer divided issues and the change in the nature of international relations when the world went from having two superpowers to one “hegemon” means that the same measure could have very different meaning. Note that we use the same measure of alliance for the Cold War and post-Cold War periods. This makes interpreting the effects for the latter period difficult as there was a large shift in alliance from the USSR to the U.S. after the dissolution of the Soviet Union. Hence, the results for the post Cold War period should be interpreted very cautiously as suggestive evidence and we do not conduct a triple difference estimation by using the Cold War sample as a placebo.

Data on UNSC membership is collected from *The United Nations Security Council Membership Rollster*.⁴⁰ 46 countries in the sample were on the UNSC as a rotating member at least once during this time. They are listed in Appendix Table A2. 21 countries were on the Council at least twice. Five countries were on the Council three times.

Human rights in the context of this study refers specifically to physical violence committed

³⁸Our measure of alliance includes abstentions. Excluding them does not significantly change either the measure of alliance or the regression results. For brevity, we do not report those results in the paper.

³⁹Our 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.

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 three countries that are least allied are Mongolia (0), Lao PDR (0), and Czech Republic (0).

⁴⁰See http://www.un.org/sc/list_eng5.asp for list of all countries that were ever members and the years of their memberships.

by the state onto civilians.⁴¹ Two of the main sources of information for human rights are the United States State Department and Amnesty International, both of which publish annual reports for almost every country in the world. Both the USSD and Amnesty use the same definition for human rights abuses as set forth by the *Universal Declaration of Human Rights* and publish reports using similar formats.⁴² The United States is the only country that systematically releases its reports to the public. The way in which it gathers information is not transparent. However, it is generally assumed that the reports are based on information from government intelligence and diplomatic apparatuses.⁴³

Amnesty International is the only non-governmental organization which makes systematic reports over the same broad scope and long time horizon.⁴⁴ 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’s finance and management are independent of any government. It has offices in eighty countries and employ full time research teams that investigate reports of human rights abuses, cross check and corroborate information from sources that include letters from individuals or their representatives, refugees, diplomats, religious bodies, community workers, humanitarian agencies, diplomats and other human rights defenders. Amnesty monitors newspapers, websites and other media outlets. It also often sends fact-finding missions to assess situations in the field. While Amnesty is often perceived as having left-leaning sympathies, the organization has actually received criticism for both alleged anti-Western and pro-Western bias. Amnesty identifies itself as an independent organization.⁴⁵

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

⁴¹This is the definition used by Freedom House, the PTS project, and the CIRI project.

⁴²The 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/>.

⁴³The wording of the reports also suggest that the information is mostly based on these sources.

⁴⁴Amnesty 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.

⁴⁵See 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.

year based on a 5-level “terror scale” originally developed by *Freedom House*. This index is available for 183 countries over the period 1976-2005. Relative to other measures of human rights violations, the PTS extends the furthest back in time, to 1976.⁴⁶ This determines the time period of our study. Amnesty and the U.S. report identical PTS for 84% of our sample on average 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 for 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 over time (using the same definition of whether a country on average votes with the U.S. more or less than the sample median as in Figure 2A). Figure 3A plots the U.S. PTS scores. The vertical band indicates the end of the Cold War 1989-91. It shows that during the Cold War, the 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 3A for the purpose of comparison. 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 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.⁴⁷ Since 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

⁴⁶The CIRI Human Rights Data Project, like the PTS Project, reads the reports by Amnesty and the USSD and provides a score. However, the CIRI indices only begin in 1981. They also differ from PTS in that they attempt to provide disaggregated incidencies 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.

⁴⁷Figures 3A-3C plot the same relationship as the one plotted in Qian and Yanagizawa (2008). The sample used here differs in that the UNSC P5 are excluded.

human rights. However, the fact that all of the changes between the difference in U.S. and Amnesty scores are driven by changes in U.S. reports, and that favorable reports for allies immediately 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 fluctuate over time, which suggests that they contain information.

News coverage of human rights violations is measured as the number of articles about human rights abuse in a given country. We calculate this number based on a search of the text of articles in the *ProQuest Historical and National Newspapers*. We search for articles containing the country's name, the phrase "human rights" and at least one of the words or phrases that fall under the *UN Declaration for Human Rights* (and that are therefore also commonly used in news articles on human rights abuse). These include "torture", "violations", "abuse", "extrajudicial", "execution", "arbitrary arrests", "imprisonment" and "disappearances". Our measure of human rights coverage is the total number of articles that results from the search per country per year. We follow previous studies on the U.S. media in assuming that media outlets will not report facts that are known to be false because proven inaccuracy could cause a costly loss of reputation. Therefore, the margin for distortion is along the composition of stories (e.g. report that the Sandinistas are committing human rights atrocities and omit reports of similar abuses by the government of El Salvador, an ally of the U.S.).⁴⁸

This study examines news reported by *The New York Times* (NYT), *The Washington Post*, *The Wall Street Journal* (WSJ, only available 1976-91), *The Chicago Tribune* (only available 1976-86) *The Christian Science Monitor* (CSM, only available 1976-97) and *The Los Angeles Times* (L.A. Times). These are the only national newspapers for which we could conduct a full text search for the main period of our study. For the Cold War period, we have data for all six papers for 1976-1986 and five papers for 1987-88 (data for the Tribune is only available until 1987). For the post-Cold War period, we do not have data for the WSJ or the Chicago Tribune and the CSM is only available until 1997. The papers in our sample were arguably some of the largest metropolitan newspapers in the U.S. during the 1980s. The

⁴⁸This 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.

NYT and *Washington Post* had particularly good reputations for the breadth and depth of their news coverage.⁴⁹ 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 own stories means that the effects we measure will not likely be confounded by information herding. Our measure includes both articles written by journalists from these papers and stories picked up from newswires and other sources.⁵⁰

We use three measures to proxy for a newspaper's cost of obtaining independent information. First, we use an indicator for the freedom of domestic press from the *Freedom House* data. It reflects a newspaper's ability to pick up stories from independent sources inside a foreign country. This measure ranges from zero to two. Zero indicates no freedom. And two indicates a free press.⁵¹ This measure is produced annually beginning in 1980. We will use a time invariant measure, calculated as the average measure during 1980-1988, to capture overall media access. This avoids the potential problem that changes in media freedom within a country over time may be correlated with UNSC membership. For interpretational ease, we create a dummy variable that takes the value of one if the average media freedom index is above zero. This dummy variable indicates whether a country experienced any media freedom during 1980-88. The second measure 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 fee for access to newswire stories. The marginal cost for each story is zero conditional on having a subscription. All of

⁴⁹For 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. Its 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. In terms of the number of Pulitzers for news, the NYT is followed by the Washington Post and the Associated Press.

⁵⁰The source of the story is often embedded within an article. Therefore, we were not able to accurately and systematically distinguish between articles written by different sources. This should not affect the interpretation of our estimates as the reduced form strategy captures the effects of government objectives on both journalists' decision to report a story and the editorial decision to publish stories which from all sources. See Appendix Figure A2 for a plot of the annual number of articles on human rights abuses for all countries over time.

⁵¹For example, Afghanistan is rated as zero and Australia is rated as two.

the newspapers in our samples have full subscriptions. Finally, we proxy for the cost for a foreign correspondent to travel to the location of the story.⁵² 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.⁵³ Figure 2C shows a map of our media freedom variable as well as the NYT foreign bureau offices.

Table 1 presents the descriptive statistics. On average, the USSD reports countries as being 0.14 index points better in terms of human rights violations relative to Amnesty. The level of alliance with the U.S. is approximately 9% on average. (The median level of alliance, which we used earlier for Figures 2A, 3A-3C is 7%. It is not reported in Table 1). On average, approximately eleven stories on human rights abuses are published in all six newspapers per country per year. Most of these stories are featured in the *Washington Post*, *NYT* and *L.A. Times*. Newswires provide roughly the same number of stories on human rights abuses as the six U.S. papers together 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. Forty percent of the sample have no media freedom domestically according to Freedom House.

4 Empirical Strategy

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

⁵²An average newspaper foreign bureau costs approximately \$300,000 per year. The major costs have been cited as rent, travel and the reporters' salaries (Caroll, 2007).

⁵³The NYT has foreign bureau offices in Mexico City, Caracs, 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.

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

The effect of an increase in government strategic value of a country on U.S. government bias towards this country can be characterized as the following.

$$USSD_{it} - Amnesty_{it} = \theta(U.S.Alliance_i \times UNSC_{it}) + \alpha \mathbf{X}_{it} + \gamma_i + \delta_t + \varepsilon_{it} \quad (1)$$

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. 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 U.S. reports when the U.S. and Amnesty differ, and for most of the time, approximately 73% during 1976-88, the U.S. and Amnesty report similar scores. Therefore, controlling for Amnesty scores will improve the precision of the second stage estimates; it does not affect the coefficients. All standard errors are clustered at the country level.⁵⁴ Higher PTS

⁵⁴Note that controlling for Amnesty PTS in equation (1) where it is also on the left hand side will only be problematic if there is measurement error in PTS scores. We have no reason to believe that this is the case.

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, $\hat{\theta} < 0$.

We specify alliance as a continuous measure mainly for convenience. In theory, without knowing the U.S.’s production function of acquiring favorable votes, it is not possible to predict the functional form of the amount of bribes necessary across different levels of alliance. To address this in practice, we first estimate a flexible equation where we allow alliance to vary.⁵⁵ We find that the effects of Council membership are broadly monotonically increasing in the level of alliance (Appendix Table A3) and therefore, for simplicity, will use a linear measure as the main specification in our paper.⁵⁶

To interpret θ as the causal effect of an increase in strategic value to the U.S., 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 concerns for how this can be violated. First, there 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 (e.g. Amnesty is less competent). Second is the related possibility that the effect is driven by Amnesty’s bias. For example, when an U.S. ally enters the UNSC, a potentially left-leaning Amnesty may decide to exaggerate human rights abuses in that country. There’s little reason to believe that either of these possibilities are very likely. Countries typically have worse human rights practices when they enter the Council on average (see Section 5 on the Empirical Results). Similarly, Figures 3A-3C in Section 3 have shown that the difference in reports during the Cold War versus afterwards is entirely driven by the USSD. But for

⁵⁵We 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 (1), 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.

⁵⁶Theoretical models of pork-barrel politics and federal spending in the U.S. predict that political patronage absent concerns for re-election will cause redistribution to be disproportionately higher for groups that support the ruling party. If reelection becomes a concern, then redistribution will go towards marginal (“swing”) voters. (For theoretical studies, see for example, Snyder, 1989; Dixit and Londregan, 1996; Cox and McCubbins, 1986. For empirical studies on the determinants and patterns of politically motivated redistribution, see for example, Dahlberg and Johanssen, 2002; Case, 2001; Miguel and Zaidi, 2003; Dasgupta, Dhillon, and Dutta, 2003; Khemani, 2004; and Cole, 2009). Since the U.S. is not concerned with re-election in the UN and since the allies in our sample are effectively “marginal” voters (the strongest ally, Turkey, only votes with the U.S. 40% of the time in the UNGA), our finding that the U.S.’s strategic value of a country increases with their alliance could be consistent with either model.

caution, we investigate these possibilities in two ways. We examine the effect on USSD and Amnesty scores separately to see if changes in the difference between these two agencies are driven by changes in the USSD or Amnesty. We also repeat the same estimation for the period after the Cold War under the assumption that the U.S.'s value for allies have decreased. Note that using the post-Cold War period as a comparison faces the difficulties that we described earlier in Sections 2 and 3, and therefore should only be interpreted as illustrative supporting evidence.

In addition to the main estimates, we can examine the timing of the effect of entry and exit onto the Council more precisely by estimating the following equation (2), which 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.

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

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. 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.⁵⁷ If there are zero articles for a country in a given year, we take the natural logarithm of 0.1. Therefore, country-year observations with zero articles are not dropped from the sample. The estimates are very similar if they are dropped from the sample. See Section 5.3 on robustness for a more detailed discussion.

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 strategic objectives. For example, if readers are more interested in allies, and this interest increases when they are on the Council, then the estimated effects could be confounded by reader preferences.⁵⁸ This seems unlikely ex-ante since knowledge surveys show that only 15% of Americans can name 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 arguably 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. We will address this by estimating the effect of government distortion for each newspaper separately and examining whether the extent of distortion correlates with measures of readers' preferences. See section on robustness for details.

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

⁵⁷For 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.

⁵⁸See studies by Mullainathan and Shleifer (2005) and Gentzkow and Shapiro (2006) for examples of how consumer preferences can drive news coverage.

news coverage will be larger when it is more costly for news outlets to obtain information from non-government sources. 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). If information asymmetries facilitate the government in influencing news coverage, then the coefficient on this triple interaction term will be positive and significant.

5 Results

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

Table 2 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 (1). Panel A shows the estimates for the Cold War era. To illustrate the alliance main effect, we estimate the effects controlling for the U.S. alliance main effect instead of country fixed effects as in the main specification. The estimate for U.S. alliance in Column (1) shows that alliance is negatively correlated with reports of human rights by the U.S. In contrast, the estimate in Column (3) shows that alliance is uncorrelated with Amnesty reports.

Column (2) shows the country fixed effects specification. 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 three index points. The estimate is statistically significant at the 1% level. Column (4) shows that the effect on Amnesty reports of human rights abuses have the opposite sign, is much smaller in magnitude and statistically insignificant.

Columns (5)-(9) examine the effect on the difference between USSD and Amnesty scores. Column (5) shows that alliance is on average correlated with the USSD under-reporting human rights abuses relative to Amnesty. Column (6) controls for country fixed effects. Column (7) presents estimates from the main specification that controls for country fixed effects and Amnesty scores. It 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, the effect for the median country can be approximated by multiplying the coefficients by 0.07. Therefore, Council membership for the median country results in a 0.25 reduction in PTS score from the U.S. relative to Amnesty.

Since the empirical strategy is based on UNSC membership, we next restrict our sample to the 46 countries that were ever on the UNSC. Column (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 residuals from the regression in Column (8). 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, the estimate in Column (9) from using a sample of countries that were on the UNSC at least once and where Zaire is omitted are similar in magnitude although less precisely estimated. The residuals from this regression are 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 had decreased. Comparing Columns (1) and (2) with (3) and (4) shows 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 (Columns (6)-(9)), and are statistically insignificant.

In addition to the main results, there are several important pieces of evidence in Table 3 that support the argument that the effect of UNSC membership for U.S. allies comes through changes in U.S. strategic value. First, note that Columns (2) and (4) in Panel A demonstrate that the effect on the different PTS scores is 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 main effects of U.S. alliance decreases in magnitude after

the Cold War, when strategic value of allies have arguably decreased. These results 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 there is no reason to believe that these factors are also correlated with U.S. alliance, finding that the benefit of Council membership to allies exists even when the two year term is over would cast doubt on the validity of our empirical strategy. Thus, we estimate equation (2) for Cold War and post-Cold War years. The estimates are shown in Appendix Table A4 along with F-statistics for the joint significance of the two years on the Council. As before, we are interested in the main effect of U.S. alliance. The estimates show that it is negatively correlated with reports from the USSD relative to Amnesty during the Cold War but not after the Cold War. The coefficients for the interaction terms of alliance and dummy variables for the number of years since Council membership 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 consistent with the belief that countries cannot fully commit to voting with the U.S. before they enter the Council. The dashed blue line plots the coefficients for the post-Cold War era. It show 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 only during the Cold War are both consistent with our empirical strategy capturing changes in countries' strategic values to the U.S.

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

Table 3 shows the estimated effects of Council membership for U.S. allies on U.S. newspaper coverage of human rights abuses. As with the estimates shown in Table 2, we first show the

estimates with U.S. alliance main effects and then with country fixed effects. Panels A and B show the estimates for the Cold War and post Cold War period. Panel A Column (1) shows that Council membership and alliance with the U.S. is correlated with more coverage on human rights abuses in U.S. newspapers. Column (2) presents the baseline estimates controlling for country fixed effects. Like the estimate in Column (1), the estimate for the main effect for UNSC membership shows that on average newspaper write more articles of abuse of Council members. The estimate for the interaction effect show that Council membership for a country that always votes with the U.S. reduces news coverage of human rights abuses by 9.3 log points. Therefore, for the median country, Council membership reduces coverage by 0.65 log points, or 48%. The estimate is statistically significant at the 1% level. Column (3) shows that the estimated effect is unchanged when the sample is restricted to countries that were ever on the Council. Figure 4C plots the residuals of this regression. It shows that, as before, Zaire is an outlier. Column (4) shows the estimate on the sample restricted to countries that were ever in the UNSC and where Zaire is excluded. The estimated effect is larger in magnitude and statistically significant at the 1% level. The partial correlation plot of the residuals are plotted in Figure 4D.

Panel B presents the estimates for the post-Cold War period. The estimates of the interaction of UNSC and alliance are smaller in magnitude relative to the Cold War estimates and are not statistically significant. In many cases, the signs are also different. The main effect of alliance with the U.S. is strikingly different. During the Cold War, the correlation was approximately 4-4.6 and statistically significant at the 1% level. Afterwards, it was reduced to approximately zero and is statistically insignificant. This is suggestive that our strategy is capturing the effects of U.S. objectives.

As with the first stage estimates, there are several pieces of evidence here that support our claim that the interaction effects capture the effect of an increase in strategic value to the U.S. 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 members is *positive*, which means that on average UNSC members receive more news coverage of human rights abuses. It is only the interaction of alliance and membership that decreases coverage. We will discuss the implications of these estimates for

our interpretation later in the paper when we explore alternative hypotheses.

5.3 Robustness

One concern in interpreting the main estimates is that we are capturing spurious country specific trends. It seems unlikely that such trends are specific to levels of alliance and UNSC membership. For caution, we address this possibility by controlling for country-specific time trends. For each country, this will control for any change over time that is roughly linear. For brevity, we only present estimates for the Cold War years in Table 4. Panel A shows the effects on the difference in USSD and Amnesty PTS scores. Column (1) shows the baseline 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 precisely estimated.⁵⁹

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 when the country specific year trends are added as controls. However, the two estimates are not statistically different from each other. Columns (3) and (4) shows 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. For back of the envelope calculations, we will use the estimates in Column (3) which are the most conservative in magnitude and statistically significant. They imply that for the median country, entry onto the Council decreases news coverage by approximately 42%.

Next, we check that our estimates are robust to restricting the sample to years when the Reagan Administration was in power (1980-88). Since Cold War tensions increased and

⁵⁹Note that the possibility that our main results are capturing spurious time trends are not high ex ante as we have already shown that the effect of UNSC membership on USSD under-reporting discretely increased when an ally entered the Council and discretely decreased back to pre-Council levels when the ally exited. However, in repeating the same yearly estimates for news coverage, we found that the estimates were very imprecise. This is most likely because of the many observations for which there were no news articles on human rights abuses. They are not reported in the paper for brevity and available upon request.

the apparatus for influencing the public opinion such as the OPD was strengthened during this administration, we should find that the main results are robust to the exclusion of the Carter years. Columns (5)-(8) of Table 5 show that this is indeed the case. The estimates are essentially unchanged when we restrict the sample.

Third, we check whether the linear specification is robust to censoring since many countries have no articles written in U.S. newspapers on their human rights abuses. Approximately 40% of the Cold War sample is 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 one story on human rights abuses in U.S. newspapers on a given year. The estimated effects are similar in magnitude between the full and restricted samples. This suggests that increasing the number of news stories from zero to more than zero is not the main margin for the main results. Similarly, the results are statistically similar when we use an alternative Tobit specification to address the potential censoring problem (see Appendix Table A5). The magnitude is nearly identical to the estimates in Table 4 Panel B Columns (2)-(3), which we use for our calculations later in the paper.

5.4 Alternative Explanations

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

There are two ways in which consumer preferences can drive our results. First, our identification assumption may be violated if UNSC membership of allies affect readers' interests in a country. Assuming that Americans prefer allies or countries with political power to have good human rights practices, the most likely scenario would be one where readers expect media outlets to increase 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 3 Panel A). However, this scenario will bias against our finding that an ally's entry onto the UNSC decreases news coverage of bad behavior. To bias our results upwards, preferences would have

to be such that consumers dislike hearing about bad behavior of UNSC members, and the strength of this preference is increasing in alliance. This runs contrary to the correlations between the main effects of U.S. alliance, UNSC membership and news coverage shown in Table 3.⁶⁰

Consumer preferences can also explain the main results if readers derive utility from hearing the government’s version of events. For example, it may be important to know that the President thinks that a certain country is “evil” even if one disagrees with the view. Note that this is not a issue of internal validity. But it is important for considering the welfare impacts of news distortions. The welfare reduction will be smaller if readers value hearing whatever the government says. We explore this possibility indirectly by examining whether the extent of government distortion across papers correlates with the characteristics of the readers of each paper. We use two proxies of readership attitudes: a ranking according to the 2008 *Mondo Conservativeness Rating* and a ranking according to the measure of media slant taken from Gentzkow and Shapiro (2006). Together with the estimated effects for each paper shown in Table 5 Columns (2)-(7), these provide two stylized facts that are inconsistent with the consumer driven hypothesis. Table 5 Column (1) shows the estimate for the sum from the main results. Columns (2)-(7) shows the estimated effects on *The Washington Post*, NYT, WSJ, *The Chicago Tribune*, *The L.A. Times* and the CSM. The estimates show that the estimated effects for the first three newspapers are large in magnitude and statistically significant at the 1% level. The estimated effect for *The Chicago Tribune*, *The L.A. Times* and the CSM have the same signs as the first group of papers. But they are much smaller in magnitude and statistically insignificant. Next, we use bivariate regressions to estimate the correlations between the estimated effects for each paper and conservativeness rankings. The residuals and regression lines are plotted in Figures 6A and 6B. Figure 6A shows that there is no relationship between distortions and a ranking based on the *Mondo Conservativeness Rating*. Figure 6B shows that the estimated effects are also uncorrelated with Gentzkow and Shapiro’s (2006) measure of media slant. These correlations should be interpreted very cautiously as there are only six newspapers in the sample and both measures of readership preferences are based on data many years after the main period of our study. Therefore, they

⁶⁰Ideally, we would like to have a measure of true human rights behavior or a measure that does not depend on information from the U.S. government. To the best of our knowledge, there is no such measure for the time horizon and geographic scope needed by this study.

should be interpreted as stylized facts consistent with consumer preferences not being a key driving force of our main results.

For interest, we also collected data on the number of news stories about human rights abuses published in two United Kingdom newspapers, *The Guardian* and *The Observer*. (They are the only two non-U.S. English newspapers that are consistently available in the ProQuest Historical Database). Column (8) shows that the interaction of alliance and Council membership has no effect on coverage in U.K. newspapers. This is consistent both with the fact that U.K. newspapers have less to gain from currying the favor of the U.S. government as well as the fact that U.K. readers could have very different preferences from their American counterparts.

We also investigate whether the main results differ for the two presidential administrations for the period of our study: Carter (1976-80) and Reagan (1980-88). Since the three distorted newspapers are typically left-leaning, one may suspect that they are more likely to go along with the distortions of the relatively left-leaning Carter administration. Similarly, one would suspect that the effects were smaller during that administration because Cold War tensions were lower than during the Reagan Administration. The estimate in Table 5 Column (9) supports this. It shows that the effect of distortions were smaller during the Carter administration.

5.5 Mechanisms

The historical discussion of Public Diplomacy in Section 2.3 showed that the government could influence news coverage through direct manipulation of the incentives of journalists or through indirect manipulation of the supply of information to journalists. The latter is especially relevant for news on remote and often physically dangerous locations for which it is costly for the newspaper to obtain independent information. In this section, we investigate the extent that the main effects are a result of the government's manipulation of information (i.e. that information asymmetries between newspapers and the USSD contribute to the main results) with an indirect test. We examine whether the effect of government distortion is larger when the cost for obtaining independent information is higher for the news organization. We have 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 5 Column (10) reports 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* in the foreign country. This reflects U.S. newspapers’ ability to pick up stories from independent sources from within the country that’s being reported on. Column (11) shows no evidence for the hypothesis that strategic objectives reduce coverage more when there is no media 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. Column (12) shows no evidence that distortions are larger when there are fewer newswire stories. The estimate of the triple interaction has the opposite sign of what is expected, is small in magnitude and statistically insignificant. These results together provide evidence to suggest that information asymmetries are not likely to play an important role. They are consistent with the hypothesis that the main results most likely reflect direct manipulation by the government.

5.6 Quantifying the Average Effect

We quantify the effects in two ways. First, we make the extreme assumption that the only way for the government 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 illustration of the upper-bound effects of biased Country Reports on news coverage.

The second stage equation can be characterized as the following.

$$\ln HRNews_{it} = \beta(USSD_{it} - Amnesty_{it}) + \alpha \mathbf{X}_{it} + \gamma_i + \delta_t + \varepsilon_{it} \quad (3)$$

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, $US_{.it} - Amnesty_{it}$; 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 . All standard errors are clustered at the country level. Higher PTS reflects worse human rights conditions. If government bias reduces news coverage, then $\hat{\beta} > 0$.

We only report results for the Cold War period. Table 6 shows the OLS and 2SLS estimates for the sum of human rights coverage across all six U.S. newspapers in our sample, and the individual effects for the NYT, *Washington Post* and the WSJ. Columns (1)-(3) show the estimated correlations for all newspapers on average, the NYT and the *Washington Post*. The estimates show that USSD under-reporting a country by one index point worse is associated with a reduction in coverage as high as 0.32 log points. The estimates in Columns (1)-(3) are significant at the 1 % level. The OLS estimate for WSJ in Column (4) is small and statistically insignificant. Columns (5)-(8) show the corresponding 2SLS estimates. They are an order of a magnitude larger than the OLS estimates and statistically significant at the 1% level for all papers. The fact that these estimates are larger than the OLS estimates is consistent with the belief that the government has other ways to influence the media beyond the reports and the likely possibility that the difference in PTS scores measures government bias with error. That said, under the assumptions stated above, these estimates say that if the USSD reported a country as being one index point worse than Amnesty, then news coverage of abuses will increase by approximately 2.4 to 3.3 log points. In practice, the USSD under-reported by 0.34 index points on average during the Cold War. Thus, the results say that during the Cold War, USSD under-reporting decreased coverage by as much as 0.9 to 1.12 log points on average.

Second, we calculate the average value of a seat on the UNSC during the Cold War conditional on a given level of alliance with the 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 7 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 4 Panel A Column (3) 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 0.1 index points for Brazil, 0.35 index points of Zaire, 0.36 index points for Honduras and 0.56 index points for Chile. In Column (5) of Table 7, we similarly calculate the effect on news coverage of abuses in U.S. newspapers. We use the estimated coefficients from Table 4 Panel B Column (3). The calculation shows that a seat on the UNSC decreased news coverage of human rights abuse for Brazil by approximately 21%, for Zaire by approximately 57%, for Honduras by approximately 58% and for Chile by approximately 74%.⁶¹

6 Interpretation

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

⁶¹In addition, we benchmark our results against a human rights incident for which there was plausibly no scope for government manipulation. We use the Chinese government crackdown on protesting students and workers during the Tiananmen Square Incident on June 4, 1989. This event and the month long protest leading up to it were widely covered in mass media at the time. As the death of Premier Hu Yaobang, which instigated the protests, coincided with the seminal state visit from Soviet President Mikhail Gorbachev and the international press corps that accompanied his visit, it is reasonable to assume that the U.S. government could not distort coverage. This allows us to use the actual number of articles on human rights abuse in China in the month following the incident as a benchmark for an undistorted coverage of a known human rights violations event. For this example, we 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. Our most conservative reduced from estimates from Table 4 suggest that for the median country, U.S. strategic objectives reduced coverage by approximately 42% during the Cold War.

boards is an important force behind the main results.

Interestingly, we find that the extent of distortion across papers vary with their quality, as measured by average daily circulation ranking and the ranking of the number of Pulitzer Prizes for international news reporting. The residuals and regression lines from the bivariate correlations between the estimated distortion for each newspaper from Table 6 Columns (2)-(7) and the two quality rankings are plotted in Figures 7A and 7B. They show that the extent of distortion is increasing with circulation, and increasing with the number of Pulitzer Prizes. Taken literally, the stylized fact that the highest quality newspapers are the most distorted is consistent with Besley and Prat's (2006) model of media capture.⁶² It has the interesting implication that there are probably high fixed costs to entry to the media market for international news reporting, and that this market is segmented. The intuition behind this is simple in the Besley and Prat (2006) context where there exists a competitive market of profit maximizing firms and where consumers value and can verify accuracy. If there were zero entry costs, then the marginal news outlet will enter the market to report the truth and earn positive profits when high reputation firms distort their reports. The firm that reports distorted news will lose profits. Therefore, in equilibrium, news outlets will not distort reports. It follows that distortions will only occur in this context if there are high fixed costs to entry. Examples of fixed costs include the formation of networks necessary for investigative journalism or reputation. For example, readers may have a positive prior about the government's credibility and are therefore unlikely to believe a news story that goes against official government reports unless if it comes from a news outlet that has a long standing reputation for good journalism. Such a reputation takes time to acquire. The potentially ambivalent effects of reputation is an interesting avenue for future studies.⁶³

⁶²In their model, media outlets, as competitive profit maximizing firms, will agree to be distorted if the profits from going along with the distortions are higher than the profits from reporting the truth. Thus, the probability of capture will increase with the profits from going along with the government (e.g. value of exclusive access) and decrease with the costs (e.g. reputation loss). Furthermore, they show that if investments towards the quality (e.g. the ability to reveal the truth) of news reporting is endogenous, then firms will vertically differentiate in quality in equilibrium. In this case, the government will only attempt to capture the firms whose qualities are high enough to reveal the truth. Under this framework, our results indicate that in net, the benefits of going along with the government dominate perceived costs of reputation losses. Moreover, our findings are consistent with the prediction that the probability of capture is positively correlated with quality.

⁶³On the one hand, newspapers will want to invest in their quality by reporting the truth. On the other hand, if there is a fixed cost in obtaining quality, quality will segment the market between firms with and without it, and consequently make it easier for the government to capture the relevant news outlets.

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

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

Second, it is beyond the scope of this paper to make conclusive statements about the welfare implications of government distortions in our context. On the one hand, readers may not have high value for accurate foreign news reports.⁶⁴ Alternatively, if the readers gain utility from knowing the government attitude or like hearing reports that are consistent with the official government agenda during a time of international political tensions and increased American patriotism, then these results would not lead to a decrease in welfare. On the other hand, there are many reasons to believe that government distortions reduce welfare. For example, readers' valuation of news may increase with the quality of news. The possibility that readers simply like hearing reports of government attitudes seems low as we find that the extent of distortion is uncorrelated with reader preferences across newspapers. Moreover, there may be negative externalities from distorted news reports; or, readers may not be time consistent and therefore undervalue their future utility from accurate news reports. The welfare implication

⁶⁴Similarly, American readers may not value international news. This is difficult to assess. On the one hand, advertising revenues suggest that reporting foreign news does not directly generate much profit for newspapers. For the NYT in 2008, they were less than 10% of revenues from domestic news. If these reflect readers' valuation 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).

of news distortions is an important topic for future studies.

7 Conclusion

This study estimates the effect of strategic objectives of the U.S. government on news coverage in U.S. newspapers. Our results 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 significant. The U.S. provides a context where nearly 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 broadly be interpreted as a lower-bound for the scope for manipulation in other countries.

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Table 1: Descriptive Statistics

| Variable | Obs | A. All Years 1976-2005 | | | B. Cold War 1976-88 | | | C. Post Cold War 1991-2005 | | |
|---------------------------|-----|------------------------|----------|-------------|---------------------|----------|-----------|----------------------------|-----------|-----------|
| | | Obs | Mean | Std. Errors | Obs | Mean | Std. Err. | Obs | Mean | Std. Err. |
| USSD PTS | | 2624 | 2.891 | (0.021) | 1010 | 2.680 | (0.031) | 1325 | 3.046 | (0.030) |
| Amnesty PTS | | 2624 | 3.029 | (0.020) | 1010 | 3.029 | (0.030) | 1325 | 3.034 | (0.029) |
| USSD -Amnesty PTS | | 2624 | -0.138 | (0.014) | 1010 | -0.349 | (0.024) | 1325 | 0.012 | (0.019) |
| U.S. Allaince | | 2624 | 0.091 | (0.001) | 1010 | 0.090 | (0.002) | 1325 | 0.093 | (0.002) |
| UNSC | | 2624 | 0.063 | (0.005) | 1010 | 0.066 | (0.008) | 1325 | 0.057 | (0.006) |
| HR News | | 2624 | 11.284 | (0.517) | 1010 | 8.659 | (0.674) | 1325 | 13.440 | (0.843) |
| HR W Post | | 2624 | 3.501 | (0.182) | 1010 | 2.104 | (0.172) | 1325 | 4.842 | (0.325) |
| HR NYT | | 2624 | 2.798 | (0.129) | 1010 | 2.564 | (0.190) | 1325 | 2.884 | (0.194) |
| HR WSJ | | 2624 | 0.802 | (0.053) | 1010 | 0.250 | (0.026) | 1325 | 1.312 | (0.100) |
| HR C. Tribune (1976-1986) | | 820 | 0.776 | (0.095) | 820 | 0.776 | (0.095) | | | |
| HR L.A. Times | | 2624 | 3.639 | (0.173) | 1010 | 2.328 | (0.194) | 1325 | 4.402 | (0.284) |
| HR CMS (1976-1996) | | 1773 | 0.447 | (0.168) | 1000 | 0.792 | (0.07) | | | |
| HR U.K. (1976-2003) | | 2441 | 0.629 | (0.037) | 1010 | 0.441 | (0.04) | 1142 | 0.7994746 | (0.07) |
| HR Newires | | 2544 | 11.109 | (0.555) | 1010 | 5.974 | (0.616) | 1325 | 14.768 | (0.916) |
| Distance to NYT | | 2624 | 1463.779 | (21.895) | 1010 | 1443.204 | (33.680) | 1325 | 1479.349 | (32.164) |
| No Media Freedom | | 2624 | 0.393 | (0.010) | 1010 | 0.404 | (0.015) | 1325 | 0.383 | (0.013) |

Table 2: The Effect of U.S. Alliance and UNSC Membership on U.S. Stated Department's Under-reporting of Human Rights Abuses

The coefficient of the interaction term U.S. Alliance x UNSC controlling for UNSC dummy variable, U.S. Alliance, country and year fixed effects.

| | 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 Baseline | EverSC | EverSC, Omit Zaire |
| A. Cold War 1976-88 | | | | | | | | | |
| 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 | | | | | | | | | |
| U.S. Alliance x UNSC | -1.478 (2.768) | 1.246 (1.086) | -1.427 (2.662) | 0.401 (1.140) | -0.0517 (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) | -0.0556 (0.146) | -0.0861 (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 3: The Effect of U.S. Alliance and UNSC Membership on News Coverage of Human Rights Abuse

The coefficient of the interaction term U.S. Alliance x UNSC controlling for UNSC dummy variable, U.S. Alliance, country and year fixed effects.

| | Dependent Variable: Ln HR News | | | | | |
|-----------------------------------|--------------------------------|-------------------|-------------------|-------------------|-------------------|------------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | All | All | All | All Baseline | Ever SC | Ever SC, Omit Zaire |
| A. Cold War 1976-1988 | | | | | | |
| U.S. Alliance x UNSC | -7.871 (8.802) | -8.988 (4.873) | -11.07 (7.276) | -9.340 (4.328) | -9.387 (3.986) | -14.29 (5.369) |
| UNSC | 0.916 (0.722) | 0.755 (0.351) | 1.289 (0.624) | 0.789 (0.315) | 0.812 (0.294) | 1.105 (0.338) |
| Amnesty | | | 1.222 (0.0853) | 0.290 (0.0790) | 0.268 (0.0880) | 0.250 (0.0868) |
| U.S. Alliance | 4.591 (2.693) | | 3.980 (1.586) | | | |
| Country FE | N | Y | N | Y | Y | Y |
| Observations | 1010 | 1010 | 1010 | 1010 | 607 | 595 |
| R-squared | 0.032 | 0.734 | 0.309 | 0.740 | 0.771 | 0.772 |
| B. Post Cold War 1992-2005 | | | | | | |
| U.S. Alliance x UNSC | 5.081 (3.376) | -0.197 (2.109) | 6.330 (3.236) | -0.265 (2.035) | -0.594 (1.980) | -0.510 (1.980) |
| UNSC | 0.186 (0.422) | 0.177 (0.213) | 0.0629 (0.384) | 0.192 (0.207) | 0.231 (0.210) | 0.222 (0.211) |
| Amnesty | | | 0.875 (0.101) | 0.170 (0.0617) | 0.204 (0.0852) | 0.198 (0.0864) |
| U.S. Alliance | -0.629 (2.694) | | 0.0980 (2.162) | | | |
| Country FE | N | Y | N | Y | Y | Y |
| Observations | 1325 | 1325 | 1325 | 1325 | 737 | 723 |
| R-squared | 0.027 | 0.767 | 0.222 | 0.769 | 0.758 | 0.758 |

All regressions control for year fixed effects.

Standard errors are clustered at the country level.

Table 4: The Effect of U.S. Alliance and UNSC Membership on U.S. State Department Underreporting and News Coverage of Human Rights Abuses - Robustness
The coefficient of the interaction term U.S. Alliance x UNSC controlling for UNSC dummy variable, U.S. Alliance, country and year fixed effects, and country specific time trends.

| | 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 | -9.340 (4.328) | -7.354 (4.038) | -7.701 (3.676) | -11.45 (6.322) | -8.983 (5.222) | -9.556 (5.998) | -9.905 (5.497) | -18.55 (6.852) |
| UNSC | 0.789 (0.315) | 0.689 (0.332) | 0.726 (0.307) | 0.949 (0.431) | 0.694 (0.366) | 0.671 (0.423) | 0.701 (0.382) | 1.196 (0.443) |
| Amnesty | 0.290 (0.0790) | 0.191 (0.0924) | 0.245 (0.107) | 0.235 (0.106) | 0.231 (0.0858) | 0.147 (0.117) | 0.227 (0.155) | 0.214 (0.151) |
| Country Time Trends | N | Y | Y | Y | N | Y | Y | Y |
| Observations | 1010 | 1010 | 607 | 595 | 776 | 776 | 465 | 456 |
| R-squared | 0.740 | 0.786 | 0.807 | 0.807 | 0.783 | 0.830 | 0.853 | 0.856 |

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

Table 5: The Effect of U.S. Alliance and UNSC Membership on News Coverage of Human Rights Abuses – Heterogeneous Effects

The coefficient of the interaction term U.S. Alliance x UNSC controlling for UNSC dummy variable, U.S. Alliance, country and year fixed effects; the coefficient of the triple interaction terms U.S. Alliance x UNSC x Cost of Obtaining Independent Information, controlling for the interaction term U.S. Alliance x UNSC, for UNSC dummy variable, U.S. Alliance, country and year fixed effects.

| | Dependent Variables: Ln HR Stories | | | | | | | | | | | |
|-------------------------------------|------------------------------------|-------------------|-------------------|-------------------|-------------------|-------------------|-------------------|-------------------|-------------------|----------------------|-------------------|--------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| | LnHRNews | LnHRWPost | LnHRNYT | LnHRWSJ | LnHRCHI | LnHRLATimes | LnHRCSM | LnHR U.K. | LnHRNews | LnHRNYT | LnHRNews | LnHRNews |
| U.S. Alliance x UNSC | -9.387 (3.986) | -11.25 (3.369) | -9.683 (2.966) | -8.205 (3.949) | -5.061 (3.480) | -2.802 (5.250) | -5.664 (3.976) | -0.122 (4.977) | -9.908 (6.117) | -15.16 (6.556) | -11.34 (7.617) | -6.404 (3.391) |
| U.S. Ally x UNSC x Dist | | | | | | | | | | 0.00328 (0.00300) | | |
| U.S. Ally x UNSC x No Media Freedom | | | | | | | | | | | 2.903 (8.988) | |
| U.S. Ally x UNSC x Newswires | | | | | | | | | | | | -0.257 (0.0937) |
| U.S. Ally x UNSC x Carter | | | | | | | | | | 33.78 (14.34) | | |
| Obs | 607 | 607 | 607 | 607 | 496 | 607 | 597 | 607 | 607 | 595 | 607 | 588 |
| R-Sq | 0.771 | 0.646 | 0.659 | 0.439 | 0.557 | 0.633 | 0.516 | 0.434 | 0.746 | 0.654 | 0.743 | 0.736 |

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 6: The OLS and 2SLS Estimates of the Effect of U.S. State Department Under-reporting on News Coverage

Coefficient of U.S.-Amnesty PTS controlling for UNSC dummy variable, Amnesty PTS, country and year fixed effects

| | Dependent Variables: Ln HR News Articles | | | | | | | |
|----------------------|--|------------------|------------------|-------------------|------------------|------------------|------------------|------------------|
| | A. OLS | | | | B. 2SLS | | | |
| | (1) LnHRNews | (2) LnHRNYT | (3) LnHRWPost | (4) LnHRWSJ | (5) LnHRNews | (6) LnHRNYT | (7) LnHRWPost | (8) LnHRWSJ |
| USSD- Amnesty PTS | 0.226 (0.105) | 0.239 (0.107) | 0.324 (0.129) | 0.0892 (0.104) | 2.623 (1.626) | 2.851 (1.367) | 3.311 (1.905) | 2.415 (1.378) |
| Observations | 607 | 607 | 607 | 607 | 1010 | 607 | 607 | 607 |
| Average Effect* | -0.0792 | -0.0836 | -0.114 | -0.0312 | -0.918 | -0.998 | -1.159 | -0.845 |
| p-value | 0.0361 | 0.0289 | 0.0148 | 0.393 | 0.110 | 0.0414 | 0.0874 | 0.0850 |

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

| (1) Country | (2) U.S. Alliance durign CW | (3) Number of Annual HR Stories during CW | (4) Effect of Being on UNSC on U.S. PTS Underreporting Alliance x -3.120+0.267 | (5) % Effect of Being on UNSC on U.S. HR News Coverage (exp[Alliance x -7.701+0.689]-1) x 100 |
|----------------|-----------------------------------|---|---|--|
| Brazil | 0.12 | 11.58 | -0.107 | -20.80% |
| Zaire | 0.20 | 4.42 | -0.349 | -56.45% |
| Honduras | 0.20 | 11.36 | -0.363 | -57.90% |
| Chile | 0.27 | 44.75 | -0.560 | -74.14% |

Figure 1: The Fraction of Divided Votes and Fraction of Countries Voting in Agreement with the U.S. in the UNGA

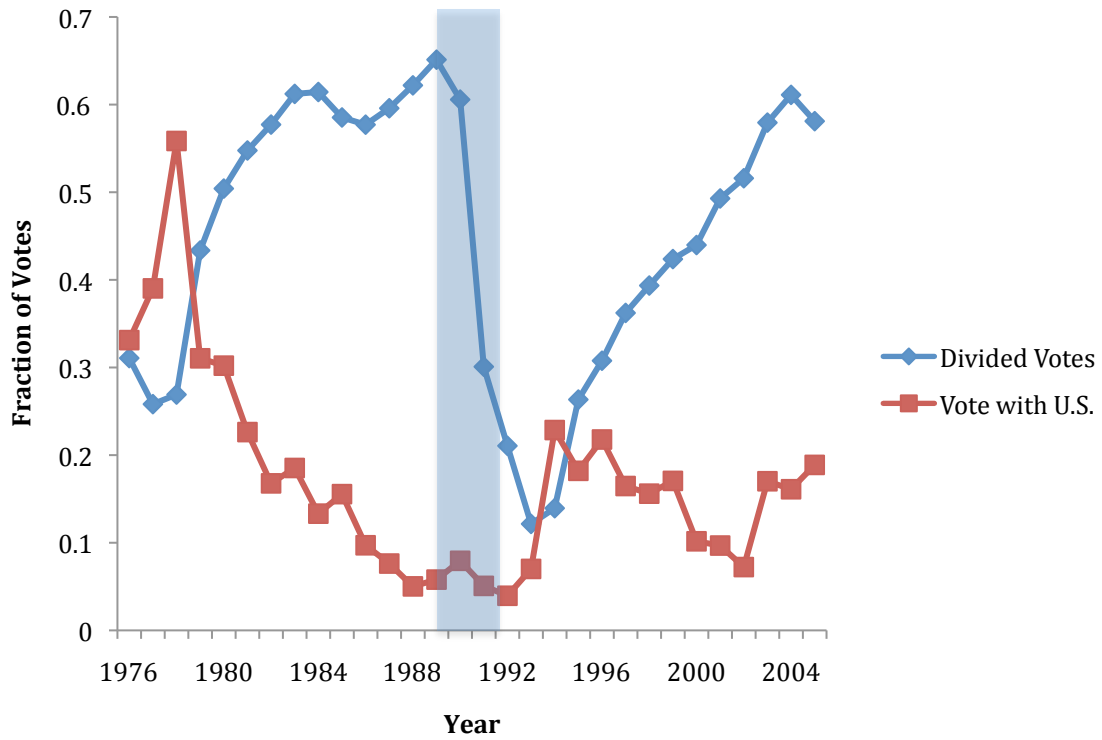


Figure 2A: Map of U.S. Alliance

U.S. Alliance

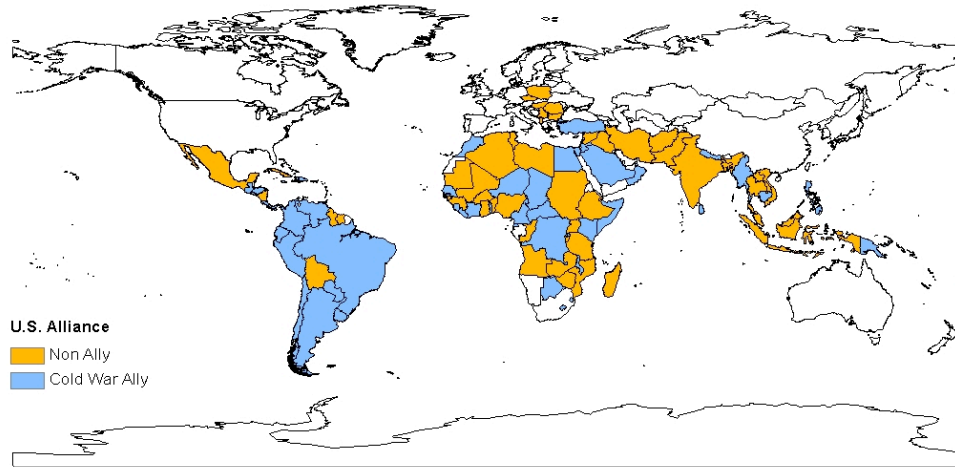


Figure 2B: Map of USSD Under-reporting

Underreporting

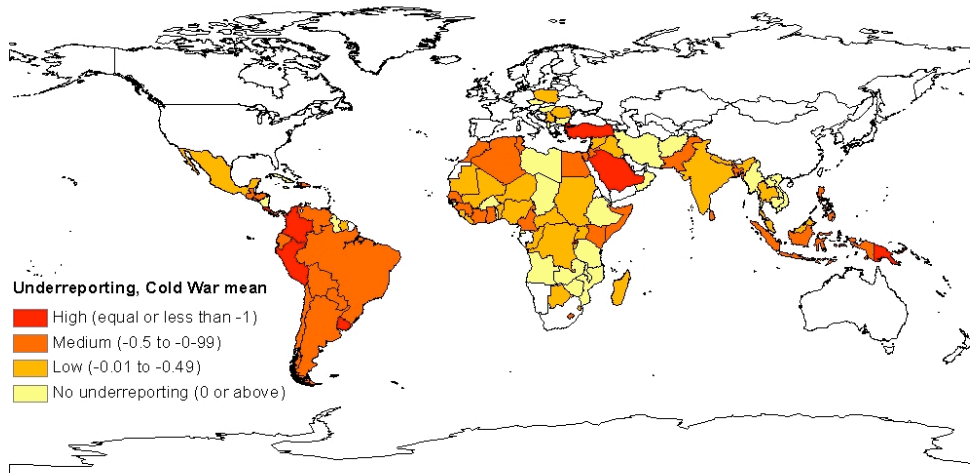


Figure 2C: Map of NYT Foreign Office Bureau Locations and Freedom House Media Freedom

Media Freedom & NYT Foreign Bureaus

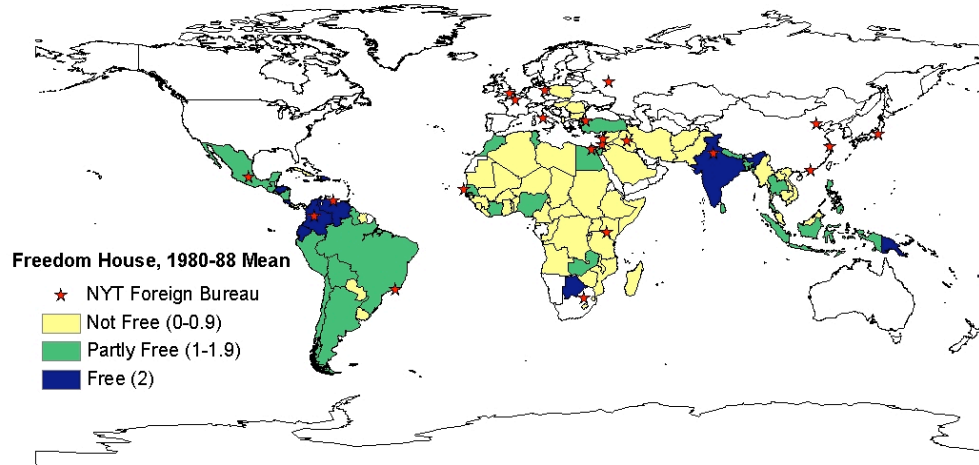


Figure 3A: U.S. PTS for Allies and Non-Allies Over Time

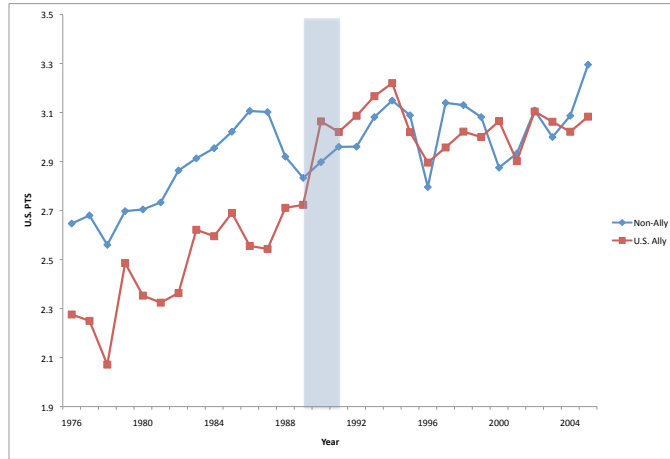


Figure 3B: Amnesty PTS for Allies and Non-Allies Over Time

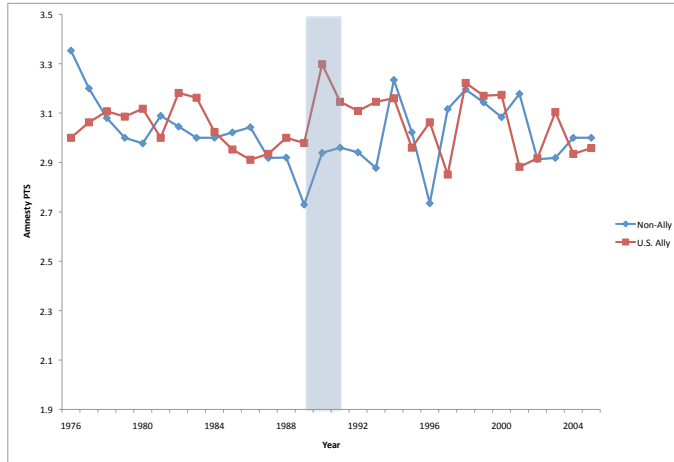


Figure 3C: U.S.-Amnesty PTS for Allies and Non-Allies Over Time

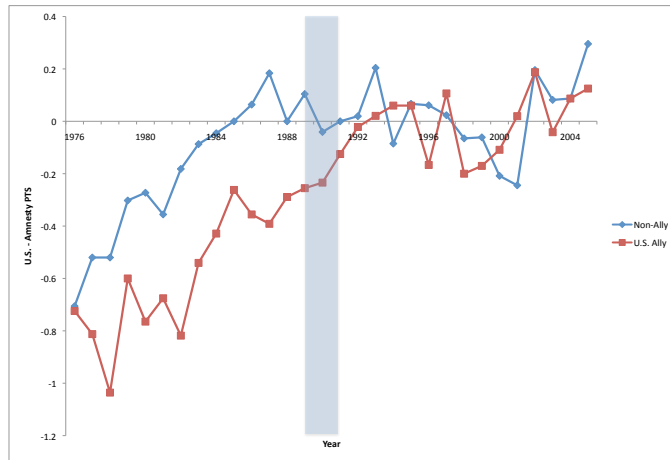


Figure 4A: Plot of Residuals from Regression of U.S.-Amnesty PTS on U.S. Alliance x UNSC – Countries that were Ever on the Security Council

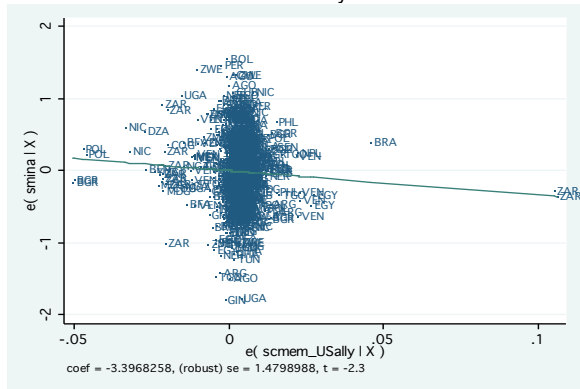


Figure 4C: Plot of Residuals from Regression of Ln HR News Articles on U.S. Alliance x UNSC – Countries that were Ever on the Security Council

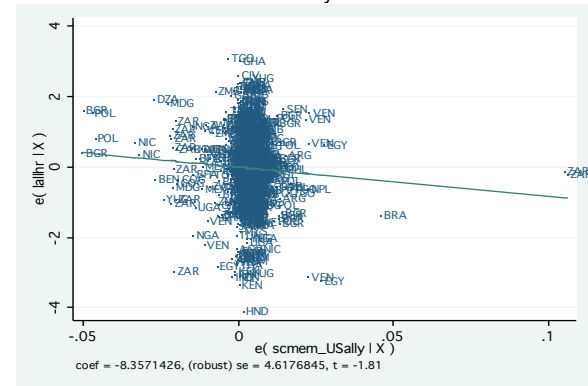


Figure 4B: Plot of Residuals from Regression of U.S.-Amnesty PTS on U.S. Alliance x UNSC – Countries that were Ever on the Security Council, Omit Zaire

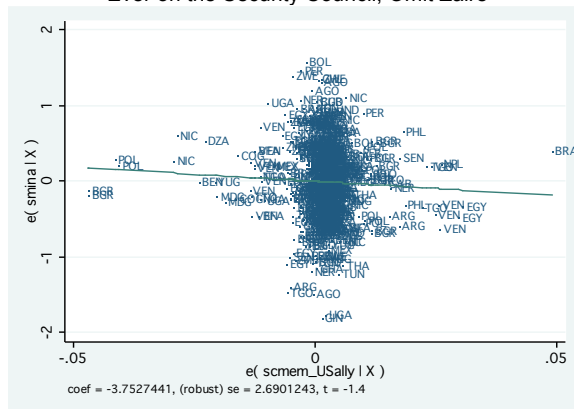


Figure 4D: Plot of Residuals from Regression of Ln HR News Articles on U.S. Alliance x UNSC – Countries that were Ever on the Security Council, Omit Zaire

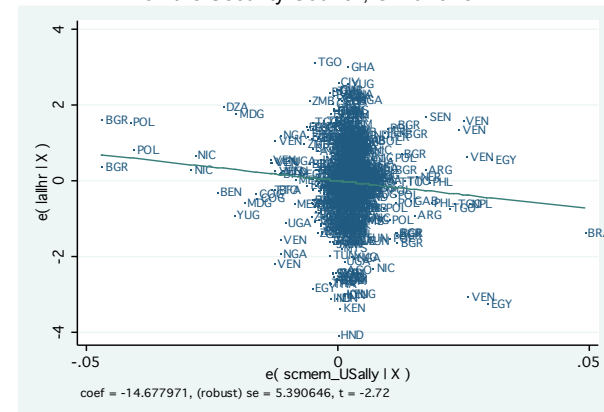


Figure 5: The Effect of U.S. Alliance x UNSC on USSD Under-Reporting by Year on the Council
 The coefficients of the interaction terms of U.S. Alliance and dummy variables for the number of years before, during and after Council membership, controlling for U.S. Alliance, UNSC dummy variable, and year fixed effects

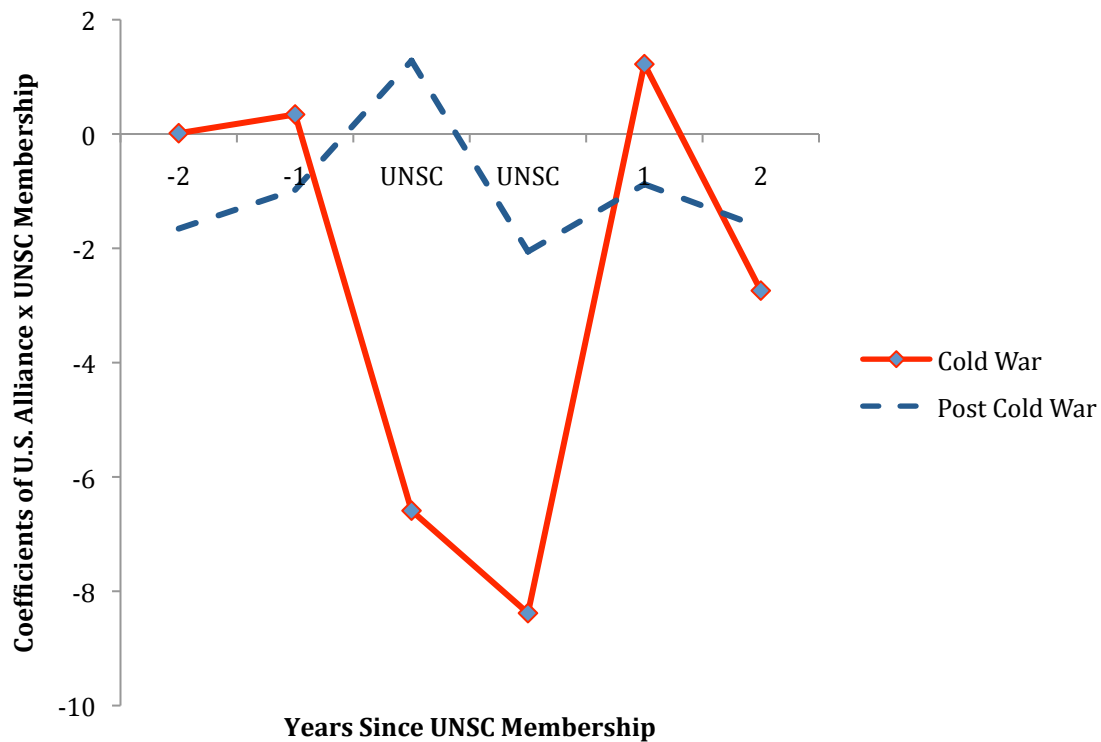


Figure 6A: Residual Plot of the Bivariate Correlation between the Estimated Effect of U.S. Alliance x UNSC on News Coverage and Mondo Conservativeness Rating

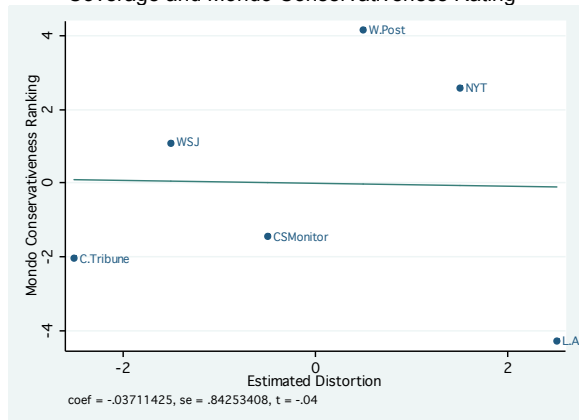


Figure 7A: Residual Plot of the Bivariate Correlation between the Estimated Effect of U.S. Alliance x UNSC on News Coverage and Circulation Ranking

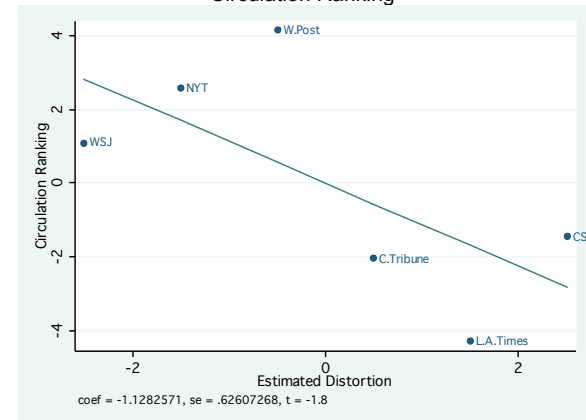


Figure 6B: Residual Plot of the Bivariate Correlation between the Estimated Effect of U.S. Alliance x UNSC on News Coverage and Gentzkow and Shapiro (2006) Slant Measure

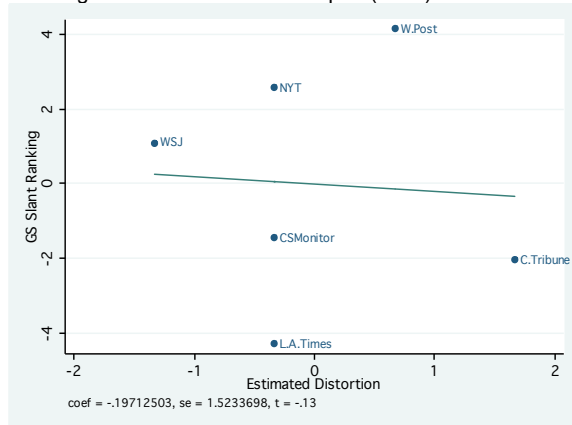
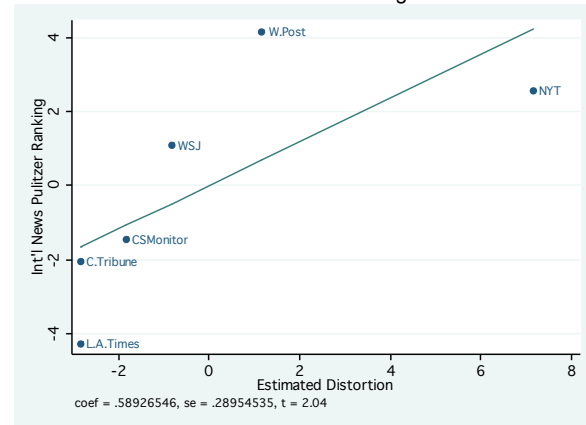


Figure 7B: Residual Plot of the Bivariate Correlation between the Estimated Effect of U.S. Alliance x UNSC on News Coverage and Pulitzer Prize Ranking



APPENDIX Table A1: The Correlation between U.S. Alliance x UNSC and Foreign Aid

| | Dependent Variables: Ln Foreign Aid Receipts (USD 1996) | | | |
|----------------------|---|------------------|-------------------------|-------------------|
| | Cold War 1976-1989 | | Post Cold War 1989-2005 | |
| | (1) Ln U.S. Aid | (2) Ln ODA | (3) Ln U.S. Aid | (4) Ln ODA |
| U.S. Alliance x UNSC | 4.229 (9.027) | 1.953 (5.052) | -3.046 (3.969) | -0.778 (2.812) |
| UNSC | 0.305 (0.603) | 0.549 (0.373) | 0.397 (0.563) | 0.191 (0.342) |
| U.S. Alliance | 6.507 (2.010) | 0.742 (1.660) | 0.212 (2.046) | -3.889 (1.711) |
| Observations | 996 | 940 | 938 | 1226 |
| R-squared | 0.090 | 0.023 | 0.009 | 0.048 |

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

Table A2: UNSC Members 1976-88, 1992-2005

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

Table A3: The Effect of U.S. Alliance x UNSC on USSD Under-reporting and News Coverage –
Flexible Estimating Equation

The coefficient of the interaction terms medium U.S. Alliance x UNSC and high U.S. Alliance x UNSC controlling for UNSC dummy variable, U.S. Alliance, country and year fixed effects.

| | Dependent Variables | | | |
|--------------------------------|---------------------|-------------------------|--------------------|-------------------------|
| | U.S. - Amnesty PTS | | Ln HR News Stories | |
| | (1) Cold War | (2) Post Cold War | (3) Cold War | (4) Post Cold War |
| Medium U.S. Alliance x UNSC | -0.134 (0.150) | 0.159 (0.174) | -0.544 (0.311) | -0.354 (0.263) |
| Strongest U.S. Alliance x UNSC | -0.398 (0.163) | 0.167 (0.175) | -1.086 (0.511) | -0.231 (0.272) |
| UNSC | 0.194 (0.0937) | -0.159 (0.148) | 0.612 (0.246) | 0.380 (0.194) |
| Amnesty | -0.555 (0.0495) | -0.634 (0.0343) | 0.290 (0.0788) | 0.171 (0.0618) |
| Observations | 1010 | 1325 | 1010 | 1325 |
| R-squared | 0.552 | 0.501 | 0.740 | 0.769 |

All regression control of country and year fixed effects.

Standard errors are clustered at the country level.

Notes: Alliance is divided into three equal frequency groups.

Table A4: The Effect of U.S. Alliance x UNSC on USSD Under-Reporting by Year on the Council
The coefficients of the interaction terms of U.S. Alliance and dummy variables for the number of years before, during and after Council membership, controlling for U.S. Alliance, UNSC dummy variable, and year fixed effects

| 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 A5: The Effect of U.S. Alliance x UNSC on News Coverage – Robustness to Censoring
 The coefficient of the interaction term U.S. Alliance x UNSC controlling for UNSC dummy variable, U.S. Alliance, country and year fixed effects.

| | Dependent Variable: Ln HR News Articles | | |
|----------------------|---|-------------------|-------------------|
| | (1) | (2) | (3) |
| | OLS | OLS | Tobit |
| Sample: | Full | HRNews>0 | Full |
| U.S. Alliance x UNSC | -9.340 (4.328) | -8.323 (2.972) | -7.752 (3.358) |
| UNSC | 0.789 (0.315) | 0.652 (0.295) | 0.683 (0.279) |
| Observations | 1010 | 504 | 1010 |

All regressions control for UNSC dummy, Amnesty PTS, and country and year fixed effects.
 Standard errors are clustered at the country level.

Figure A1: The Total Number of HR Articles in Six U.S. Newspapers before and after the Release of Country Reports

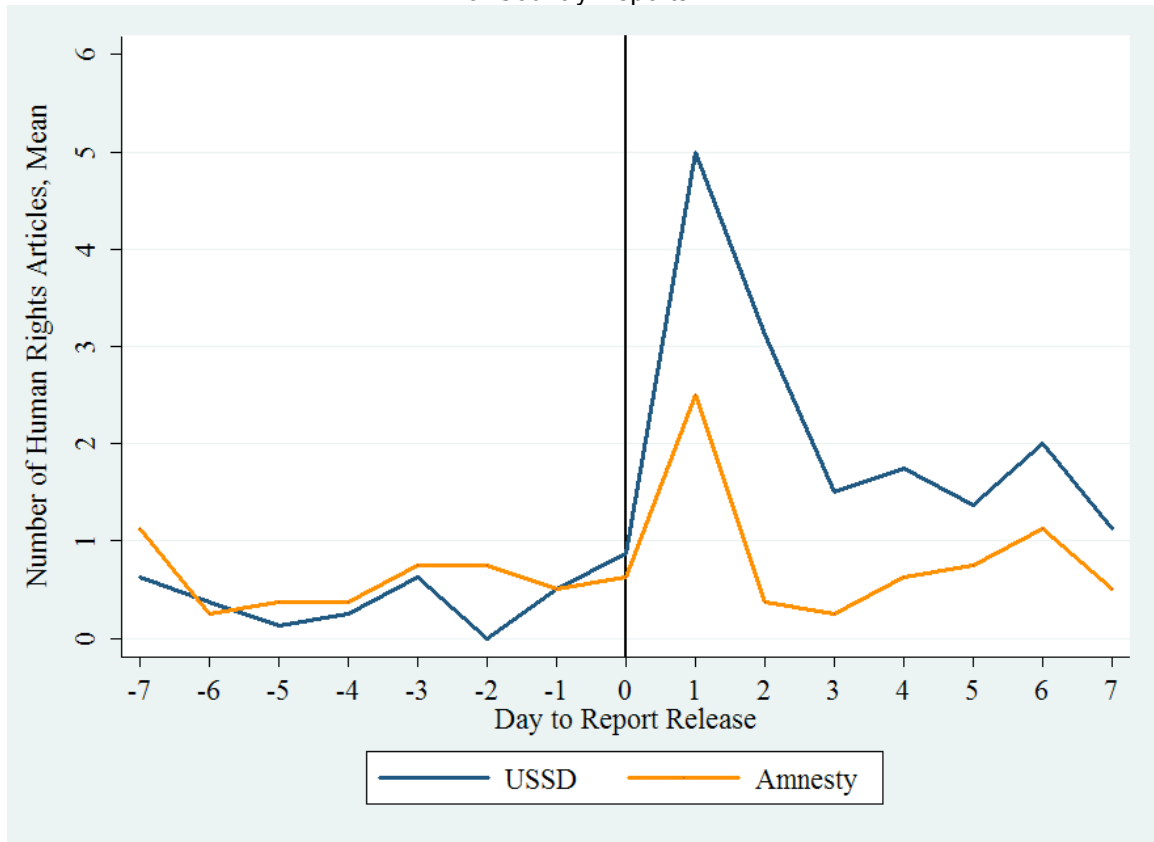


Figure A2: The Number of Articles on Human Rights Abuses over Time

