

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

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Abstract

This paper investigates the extent to which the government can strategically distort a free media market by examining the effect of the U.S. State Department's bias in human rights reporting on coverage in the New York Times. To establish causality, we exploit a novel source of variation in the strategic value of a country to the U.S. government. We show that the State Department favorably under-reports abuses in countries that it values strategically. This reduces news coverage by approximately 28% from what it should be. Our findings suggest that these distortions are not likely to be consumer driven. (P16 Political Economy, L82 Media)

“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.

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1 Introduction

The ability of the *free* press to diminish distortions from the government is implicitly assumed by standard political theory and journalists, who think of media as a central member of the fourth branch of the government. The *Fourth Estate*, a term commonly attributed to Thomas Carlyle, is often used to refer to the media’s role as an informal branch of government which checks the legislative, judicial and executive branches by reporting on their actions and consequences. To function as such, the media needs to avoid government influence. Besley and Prat (2006), calling government influence “capture”, formulates the conditions necessary to prevent it. They argue that both independent ownership and competition decrease capture. This is supported by survey evidence on the correlation between government ownership of the media and reduced political and economic freedoms (Djankov et al., 2001); the finding that access to independent television stations in Russia increases the likelihood of voting for opposition parties (Enikolopov, Petrova and Zhuravskaya, 2009); and that competition in the U.S. increases the likelihood that news organizations will report the truth (Prat and Stromberg, 2005; Gentzkow, Glaeser and Goldin, 2006). However, there are many reasons to think that even an independently owned and competitive media market is susceptible to manipulation. For example, the government can offer a news organization access to information for which it is the monopoly supplier in exchange for suppressing another story for which the accuracy is difficult for readers to verify.¹ This begs the question: to what extent can the government manipulate news reported by a *free* press? To the best of our knowledge, this is the first study to ask this question, which seems extremely important in light of the growing body of evidence on the political and social consequences of the media.²

This study attempts to fill this gap by examining the scope for government manipulation of news in the United States, a country that has one of the most competitive media industries and where all major news organizations are independently owned (e.g. Djankov et al., 2001), and where the independence of the press is enshrined into its constitution. In particular, we estimate the effect of the U.S. State Department’s (USSD) favorably under-reporting human

¹In the Besley and Prat (2005) model, this means removing their assumption that there is a sharp increase in profits to being the only newspaper to report a story.

²Recent studies have shown that media can affect voting behavior (Prat and Stromberg, 2005; Gentzkow, 2006; Della Vigna and Kaplan, 2007; and Chang and Knight, 2008), other political behavior (Olken, 2008; Paluck, 2008), and social outcomes such as literacy (Gentzkow and Shapiro, 2008a), female empowerment (Jensen and Oster, 2008) and fertility (La Ferrara, Chang and Duryea, 2007).

rights violations for its allies on the amount of news coverage on human rights abuses for those countries in the *New York Times* (NYT). Our measure of the USSD reports come from its annual *Country Reports on Human Rights Practices*, which are broadly read by reporters.

We focus on human rights for several reasons. First, it is a well-defined concept and coverage is relatively easy to measure. Second, human rights violations often occur in hard to reach locations concurrent with political and social unrest, making them good examples of when government reports are important to news organizations and when it is difficult for readers to immediately verify the accuracy of reports. Third, it is reasonable to assume that American readers care about human rights in other countries and that the U.S. government wants the support of its constituency. Therefore, the government has an incentive to portray its allies favorably. This is consistent with the observation that human rights violations are frequently used to justify political and economic policies.³ We focus our discussion on coverage in the *The New York Times* because it has one of the largest stable foreign news staffs and is therefore most likely to obtain independent information. This means that our estimates can be interpreted as a conservative estimate of the effect for U.S. newspapers on average. And because the NYT is more likely to write breaking news stories with their own staff rather than pick up stories from other U.S. news agencies, the effect on the NYT will be less likely to be confounded by the possibility of information herding across news organizations.

This study faces several empirical difficulties. The first is to measure U.S. bias. The government's bias is unobservable. And any inferred measure will measure true bias with error. Second is the problem of reverse causality. For example, Stromberg (2002) argues that public policy can be an outcome of media. Although this reverse mechanism is unlikely in our context when one takes into account the vast resources of the U.S. government relative to independent news organizations, we cannot *prima facie* rule out the possibility that the USSD's reports are not influenced by news media coverage. Finally, we face the omitted variable bias problem that the USSD's bias and media reports may both be outcomes of political feelings of consumers. For example, in the months before the 2003 invasion of Iraq, the U.S. government may have unfavorably biased reports of human rights situations in

³In 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". Letters to the Editor, *Washington Post*, Monday, June 9, 2008; Page A16.

Iraq to garner support for the war while the news media may have slanted their reports to satisfy a popular anxiety about the Middle East after 9/11. In this case, the OLS will show that U.S. government bias and news coverage are highly correlated. But the correlation will reflect both the effect of the U.S. government and the preferences of the readers and hence will overstate the true effect of government bias.⁴

The principal contribution of this paper is to address these problems. First, we use the difference between the quantitative scores of the Political Terror Scale (PTS) of the USSD and Amnesty International's (Amnesty) annual country reports on human rights violations from Gibney and Dalton (1996) as our measure of U.S. bias. We use this measure for convenience. These are the only two sources of human rights violations reports with the same scope. Interpreting this measure as U.S. bias does not require that Amnesty reports the truth. It only requires that changes in the difference between U.S. and Amnesty reports over time to be driven by changes in U.S. bias. We can further relax this requirement with our instrumental variables strategy, which also addresses the problems of omitted variables and reverse causality. We instrument for the U.S. reporting bias with variation in a country's strategic value to the U.S. that does not affect media coverage through other channels. Our instrument is the interaction term of alliance with the U.S. during the Cold War and a country's rotating membership on the United Nations Security Council (UNSC). This strategy measures the effect of the U.S.-Amnesty difference, which *changes* for U.S. allies when they are on the UNSC.

The interpretation of U.S. bias depends on the assumption that Amnesty does not change its bias for U.S. allies when they enter the UNSC. To check that changes in the difference in USSD and Amnesty PTS scores are mainly driven by changes in U.S. strategic value, we check that a U.S. ally's entry and exit into the UNSC does not cause the U.S. to favorably under-report its human rights violations after the Cold War, when the U.S.'s value of alliance has presumably decreased. The identification assumption, more generally, is that UNSC membership for U.S. allies did not affect NYT coverage through any channels other than a change in the country's strategic value to the U.S. government. To verify that UNSC membership does not change readers' interests for U.S. allies, we examine the effect of allies' UNSC membership on non-human rights news coverage from the NYT.

⁴See studies such Mullainathan and Shleifer (2005) and Gentzkow and Shapiro (2006, 2007b) for more detailed discussions of the motivations and evidence that news organizations slant reports.

Our data is a country level panel constructed from several existing data sources. Our results show that the U.S. favorably under-reported human rights violations of countries relative to Amnesty immediately after their entry into the UNSC, and then returned reports to pre-membership levels immediately upon a country's exit from the UNSC. Our claim that this is driven by changes in strategic value to the U.S. is supported by the fact that neither entry onto nor exit from the UNSC has any effects on U.S. reports relative to Amnesty for U.S. allies after the Cold War. We are currently collecting data to investigate whether the same holds true for the coverage of bad news for which the U.S. government is not the main source of information (e.g. epidemics, malnutrition).

Both the OLS and 2SLS estimates show that favorable reports from the U.S. decrease coverage of human rights abuses in the NYT. The 2SLS estimates are larger than the OLS estimates in magnitude. We find that government bias in human rights reporting has no effect on overall news coverage of a particular region. This supports the identification assumption that UNSC membership of U.S. allies does not affect NYT coverage by affecting readers' general interests in a country. Taking our most conservative estimates literally, the results indicate that during the Cold War, under-reporting by the State Department reduced the coverage of human rights issues for U.S. allies such as Honduras, the Zaire and Argentina by approximately 52%, 29% and 36%.

Assuming that NYT does not allow distortions because U.S. readers care about the State Departments' opinions on human rights per se, our main results are consistent with both the case where the NYT is unable to obtain independent information and the case where it knowingly allows the distortions. The former would be consistent with the growing concern amongst practitioners and scholars of journalism over the decline in the number of stable field correspondents. In that scenario, limited access to independent information will increase the scope for government manipulation. We test this by estimating the extent to which the main results vary depending on the NYT's access to independent information using two measures of access: the distance between a country's capital and the nearest NYT foreign bureau office, and a measure of media freedom for domestic media which is associated with NYT's ability to pick up stories from local media sources. Our results show that there are no differential effects. Hence, we conclude that government distortions are not driven by the NYT's inability to obtain independent information.

It is beyond the scope of this paper to provide a conclusive answer for why the NYT allows

distortions. The numerous accounts of how the government has tried to suppress unfavorable news from being reported makes it highly unlikely that news organizations are blissfully naive about the government's incentives to distort information (Gentzkow and Shapiro, 2008b). There are two other probable scenarios. First is that the NYT is simply cost-minimizing and obtaining information from the government is less costly than from other sources. Second is the possibility that the NYT is colluding with the government. Distinguishing them with the empirical results partly depends on the process of generating stories on human rights abuses. So far, interviews with journalists suggest that stories are typically generated in two ways. First, they can be generated by field reporters. In this case, the finding that the main results do not vary by distance to a foreign office bureau suggests that the NYT is not driven by cost-minimization. Second, they can be generated by editors who may call government contacts to determine where to send the field reporters. In this case, our results will not be able to distinguish collusion from cost-minimization. We do not have much reason to believe that one process was more prevalently used relative to the other during the period of this study.

To loosely quantify our results in dollar terms, we can provide an illustrative example in terms of U.S. foreign aid. This follows the rationale that in order for U.S. politicians to give aid to an ally, they need their constituents to have favorable views of the human rights situation in that country. A back-of-the-envelope calculation using our results and the correlation between human rights reports and foreign aid suggest that one less NYT article on human rights abuse corresponds to a 4.1% increase in foreign aid receipt. Since articles in the NYT are highly correlated with articles in other newspapers, this should be interpreted broadly to mean the value of suppressing one article in each U.S. newspaper. This implies that suppressing one article on average across U.S. newspapers is worth approximately 1.8 million dollars (in 1996 USD) of U.S. foreign aid to a recipient country.

An alternative way to assess the magnitude of the results is to compare the effects of government bias to the number of stories written about human rights violations that were not subject to government manipulation. To conservatively assess the relative magnitude, we use the Tiananmen Square Incident in 1989, which was widely covered in mass media in real time because of the coincidental death of pro-reform Chinese premier Hu Yaobang with the large company of international media that accompanied a state visit from Soviet president Mikhail Gorbachev. Using this as a benchmark for undistorted coverage, our results imply

that government distortions on average reduce reports by approximately 27% from what they should be.

There are two important caveats for interpreting these results. First is the fact that the annual Country Reports measure the overall government attitude towards a country. While we know that it is widely read by reporters, we cannot know if the effect on news coverage is from reporters reading the reports or from them calling their contacts in the government who then give the same information as what is in the reports. In fact, these two mechanisms most likely coexist. They will both be captured in our reduced form estimates. Second, we assume that readers have no special interest in the State Department's opinion on human rights and only read stories about human rights to learn of the facts. This does not affect our empirical analysis. But it is important for interpreting our estimates in terms of readers' welfare. If readers were interested in government opinions on this subject, then the distortions are less likely to reduce welfare.

Our study makes several contributions. First, as a study of the political economy of media, we present novel empirical evidence on the extent to which supply-side distortions can still exist in a competitive media market with many independently owned firms. These findings indicate that ownership and downstream competition, which has so far been the focus of studies of the media, are insufficient to assure the provision of the truth to the public. The second contribution is methodological. In addition to identification, our empirical strategy provides a method for future researchers in economics and political science to credibly measure U.S. bias and a country's strategic value to the U.S. Finally, this study contributes to the recent policy debate on whether the media's ability to report accurately is being undermined by a lack of independent information. Our finding that distortions in media reports are not related to its ability in obtaining independent information suggests that the cause of incomplete reporting is much more complex than the simple decline in the number of field reporters.

The paper is organized as follows. Section 2 discusses the background. Section 3 discusses how the government can distort news reports. Section 4 presents the empirical strategy. Section 5 describes the data. Section 6 presents the results. And section 7 offers concluding remarks.

2 Background

2.1 Country Reports and U.S. Strategy

Human rights in the context of this study refers specifically to physical violence committed by the state onto civilians. It does not include violence between armed factions during civil conflict. Nor does it include oppressive policies towards individual liberties (e.g. for religion, speech).

Two of the main sources of information for human rights are the United States State Department and Amnesty International. While intelligence units of other governments certainly have their own information about human rights situations in foreign countries, the United States is the only country that systematically publishes its reports for the public. Similarly, Amnesty International is the only non-governmental organization which makes systematic reports.

The *Country Reports on Human Rights Practices* are submitted annually by the USSD to the U.S. Congress. The reports cover internationally recognized individual, civil, political, and worker rights, as set forth in the *Universal Declaration of Human Rights*.⁵ Amnesty International, commonly known as Amnesty, is one of the only two international non-governmental organizations reporting on human rights abuses world wide.⁶ Officially, Amnesty has the same criteria and focus as the USSD in creating their Human Rights Reports. Amnesty defines its mission as “to conduct research and generate action to prevent and end grave abuses of human rights and to demand justice for those whose rights have been violated”. Founded in the United Kingdom in 1961, Amnesty draws its attention to human rights abuses and campaigns for compliance with international standards. While Amnesty is often perceived as having left-leaning sympathies, the organization has actually received criticism for both alleged anti-Western and pro-Western bias. Amnesty proclaims itself as an independent organization.⁷

We conducted a search of articles about “human rights” in the NYT. Approximately 10% cite “State Department Sources”, which could include either the reports or conversations with

⁵<http://www.state.gov/g/drl/rls/hrrpt/>

⁶The other is *Human Rights Watch*, a U.S. based organization. However, the HRW does not systematically publish yearly country reports.

⁷See Poe, Carey and Vasquez (2001) and Qian and Yanagizawa (2008) have for quantitative comparisons of the Amnesty and U.S. State Department measures and more details.

State Department personnel, and 7% cite “Amnesty”. Figure 1 plots the total number of NYT stories on human rights abuses per day for the week before and after the release of USSD and Amnesty Country Reports averaged across years. The sharp increase on the day of the release suggests that the reports are used by reporters. Over the course of year, the Country Report PTS will of course also reflect the attitudes of government officials. Hence, our using the country reports will measure overall government and Amnesty attitudes, regardless of whether information is obtained directly from reading the reports or from talking to sources in the agencies.

Past studies have argued that the U.S. favors its allies with favorable human rights reports (Stohl and Carleton, 1985; Mitchell and McCormick, 1988; Poe, Carey and Vasquez, 2001). More recently, Qian and Yanagizawa (2008) find that the U.S. specifically favors its allies during the Cold War. During the Cold War, direct military attacks on adversaries were deterred by the potential for mutually assured destruction using 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 every region of the world, as the U.S., under the *Marshall Plan*, sought the containment and rollback of communism and forged myriad alliances to this end; and the U.S.S.R., under the *Molotov Plan*, fostered Communist movements around the world.

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. The strenuous competition between the U.S. and the U.S.S.R. for the alliance of smaller countries ended with the Cold War. A famous anecdotal example of how this affected favored Cold War allies is Zaire (renamed the Democratic Republic of Congo in 1997), who’s president, Mobutu Sese Seko (in office 1965-1997) was a strong supporter of the U.S. during the Cold War. During a state visit to the U.S. in 1983, U.S. president Ronald Reagan praised Mobutu and said in response to the international criticism of Mobutu’s human rights abuses that he was a “voice of good sense and good will”. Immediately after the Cold War ended, the State Department began to criticize Zaire’s human rights violations. And in 1993, Mobutu was denied a visa for visiting the U.S. At that time, he remarked “I am the latest victim of the Cold War, no longer needed by the U.S. The lesson is that my support for American policy [now] counts for nothing” (Gbadolite, 2001).

During the Cold War, an important source of variation in a country's strategic value to the U.S. was its alliance with the U.S. in the United Nations. It was valuable to the U.S. to have had allies in the General Assembly and the Security Council. The United Nations General Assembly (UNGA) is one of the five principal organs of the United Nations and the only one in which all member nations have equal representation. Its powers are to oversee the budget of the United Nations, appoint the non-permanent members to the Security Council, receive reports from other parts of the United Nations and make recommendations in the form of General Assembly Resolutions.⁸ The General Assembly votes on many resolutions brought forth by sponsoring states. Most resolutions, while symbolic of the sense of the international community, are not enforceable as a legal or practical matter. The General Assembly does have authority to make final decisions in some areas such as the UN budget. And many resolutions may also be constitutive or proof of international customary law, and therefore binding on member states. The claim that UNGA votes are valuable to the U.S. is consistent with the finding that they are positively correlated with U.S. foreign aid receipts (Alesina and Dollar, 2000). Qian and Yanagizawa (2008) also find that during the Cold War, increasing the share of votes in agreement with the U.S. on issues where the U.S. and the U.S.S.R are divided increases the favoritism from the U.S. in terms of human rights reports relative to Amnesty.

The United Nations Security Council is comprised of fifteen member states, who are elected onto the council by the member countries of their region. Unlike other policy making organs of the UN, 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). This was the basis for UN armed action in Korea in 1950 during the Korean War. There are five permanent members (P5): China, France, Russia, the United Kingdom, and the United States. These members hold veto power for blocking adoption of a resolution. However, they cannot block the debate of any resolutions. The ten temporary seats 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.⁹ The value of UNSC membership

⁸In 1945, the UN had 51 members. It now has 192, of which more than two-thirds are developing countries. For many developing countries, the UN is the source of much of their diplomatic influence and the principal outlet for their foreign relations initiatives.

⁹Africa elects three members; Latin America and the Caribbean, Asian, and Western European and others

is consistent with the observation that there are often intense competition for seats (Malone, 2000), and the finding that the U.S. gives more foreign aid to countries when they serve as rotating UNSC members (Kuziemko and Werker, 2005). In this paper, we exclude the five permanent members from the sample. Hence, UNSC membership in this paper applies only to the ten rotating members.

The nature of the veto power of P5 members mean that a rotating member only has influence over an issue if no member of the P5 chooses to veto. Conditional on this, the value of having an ally on the UNSC is larger to the U.S. when there are issues for which the rotating members are split. The fact that there were more split issues during the Cold War can be observed in the number of emergency sessions held by the General Assembly. These sessions are only held if the Security Council cannot come to a decision (when there is a deadlock amongst the members and no member of the P5 will veto). They have been convened on ten occasions. Nine of these ten occasions occurred during the Cold War.

3 How Would Country Reports Distort the News?

Distortions in news reports can be manifested in two ways. First is the accuracy of what is reported. Second is the amount of coverage of a particular topic. Since it is unlikely that news organizations are willing to report inaccurate facts which can later be invalidated, we focus on the second outcome which we will measure with the number of stories in the NYT.

The government can distort news coverage either by manipulating the information set or by manipulating the incentives of the news organizations. To manipulate the information set, the government may provide inaccurate facts. If a news organization cares about the accuracy of its reports and realizes the facts it receives are inaccurate, it may decide to drop the story altogether. This can distort readers' perception of the truth if they make inferences from the lack of coverage (e.g. no coverage means that nothing bad happened). Alternatively, the government can affect the amount of coverage by distorting the importance of an event. Newspapers (and television news broadcasts) are constrained in the amount of news they can cover. For example, the coverage of U.S. ally Zaire during the Cold War can be

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.

decreased by increasing the relative importance of persecutions by the Marxist government in Ethiopia.¹⁰ To manipulate the incentives of the news organization, the government can promise access to stories in exchange for suppressing other stories (e.g. to be called on during a presidential press conference). In a more extreme scenario, the government can explicitly punish journalists that write stories against its interests. This latter case is not very relevant to the context of our study.

To neutralize supply-side distortions, news organizations must recognize that they are receiving distorted information, and then be willing and able to obtain accurate information and report it. There are several reasons of why news organizations are vulnerable to manipulation. First, if it is naive and thinks that the government always tells the truth, then it will always report whatever the government says. This seems extremely unlikely given the many known examples of how the government has tried to manipulate the media (Gentzkow and Shapiro, 2008b).

Second, the news organization may simply be unable to obtain independent information in certain cases. Then, the reporter must infer the truth based on reports from the USSD and Amnesty. The simplest rule of thumb would be to use a weighted sum of the information from Amnesty and the USSD. The weights assigned will reflect the firm's prior about the accuracy of each source. Thus, unless if Amnesty's bias changes at exactly the same time as U.S. bias and changes in the opposite direction so that it offsets U.S. bias, any bias of the USSD in a given report will move the firm's inference from the truth. This will be true even if it places equal trust in the two sources. (The relative truthfulness of the USSD to Amnesty is not likely to vary with each report. Even if it did, a firm may not be able to update its priors of each source's relative truthfulness beyond a moving average of past events). This is consistent with growing concern amongst scholars and practitioners of journalism over the decline of the number of foreign news correspondents. For example, only four American newspapers and six television networks have foreign bureaus today (Constable, 2007). Observers worry that the reduction of field correspondents may have diminished news organizations' ability for obtaining independent information from the field. By examining the bylines of NYT articles, we find that in 1964, the year before the U.S. first deployed troops to Vietnam, 40% of the 1,463 *New York Times* (NYT) articles on Vietnam were written from Vietnam. In contrast, during the twelve months before the invasion of Iraq on March 20, 2003, only 9%

¹⁰This is a similar mechanism to the crowding-out of news shown by Stromberg (2004).

of the 1,611 NYT articles on Iraq were written from Iraq. We will empirically test whether distortions occur because news organizations are unable to obtain independent information by examining whether the extent of the distortion varies according to the NYT's access to a given country.

Finally, the news organization may be incentivized to report the government's version. For example, the government may be able to provide news more "cheaply" to the media than other agencies. A reporter reasonably relies on information that is less costly to obtain. Then on hearing of an event, reporters can call their network of contacts in the government. If these contacts report the government version of facts then news reports will be highly correlated with the distorted government reports. More explicit forms of collusion can also arise. For example, the government can promise access to upcoming stories in exchange for suppressing other stories. The most extreme scenario is if the media is owned or managed by the government. This explicit collusion is rather improbable in the U.S. media market.

In this study, we make two assumptions. First, we assume that U.S. readers care about human rights in other countries and that they would be against alliances between their government and countries where many human rights violations occur. Similarly, they would not be in favor of the U.S. government providing those countries with aid. This gives the U.S. government, which we assume to want the support of its voting constituency, a motive for portraying strategically valuable countries favorably. Second, we assume that Americans read the NYT to obtain facts on human rights abuses and have no inherent interest in the government's opinions on the topic per se. This is an important distinction because if the readers are interested in government opinion, then under most conditions, it would be socially optimal for the NYT to always report whatever the government says. For example, one may think that readers are interested in the opinions of certain celebrities or politicians regardless of what they say. Interest may even increase with outrageous comments that are unlikely to be true. There seems little reason to believe that this is the case with the USSD, a mundane bureaucracy of the American government and not a known political figure. This is reflected in the wording of articles which makes clear whether they are centered around the opinions of certain parties, or if they are focused on reporting facts while citing their sources.

These assumptions are consistent with those of Besley and Prat (2006). The main departure from their framework is that we do not make the assumption that a newspaper in

a competitive market will receive a sharp increase in profits if it publishes a story that no one else has. This could be because for distant foreign countries, readers cannot realize that there are distortions until many years after the fact, when they no longer care.

4 Empirical Strategy

4.1 Measuring U.S. Bias

Bias is unobservable. We must infer it. One way to infer U.S. bias is to measure the difference between USSD reports on human rights and Amnesty reports on human rights. However, this presumes that Amnesty is telling the truth. Alternatively, we can use the change in U.S. reports relative to the change in Amnesty reports as a measure of use bias. This relaxes the assumption that Amnesty is not biased to the assumption that Amnesty’s bias does not change over time along with U.S. bias. Hence, we measure U.S. bias as the difference between U.S. and Amnesty reports for country i in year t . The OLS specification below estimates the correlation between the number of articles written about a country and the difference between U.S. and Amnesty reports across countries and over time,

$$\text{LnMedia}_{it} = \beta(\text{US}_{it} - \text{Amnesty}_{it}) + \pi\text{Amnesty}_{it} + \gamma_i + \delta_t + \varepsilon_{it} \quad (1)$$

The natural logarithm of the number of articles about human rights abuses for country i in year t , LnMedia_{it} , is a function of: the difference in human rights scores between the USSD and Amnesty, $\text{US}_{it} - \text{Amnesty}_{it}$; the score of Amnesty reports, Amnesty_{it} ; country fixed effects, α_i ; and year fixed effects δ_t . We use the logarithm of the number of articles to reduce the weights 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.¹¹ We control for the score of Amnesty reports because we are interested in the effect of the U.S. when the U.S. and Amnesty differs, and for most of the time – approximately 84% in the time period of this study – the U.S. and Amnesty report similar scores. Therefore, controlling for Amnesty reports has little effect on the coefficients

¹¹For 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. Hence, the number of articles on Chinese human rights are just as likely to be correlated to trade negotiations with the U.S. as with actual changes in human rights.

but improves the precision of the estimates. All standard errors are clustered at the country level.

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.

Interpreting this difference as U.S. *bias* requires the assumption that the change in the U.S. - Amnesty difference is a result of changes in a country's strategic value to the U.S. rather than changes in Amnesty's bias. If the USSD reports a country as worse than Amnesty, then $US_{it} - Amnesty_{it} > 0$. Hence, if the USSD's bias against a country is correlated with more coverage of that countries human rights abuses in the NYT, then $\beta_{OLS} > 0$.

4.2 Instrumenting for U.S. Bias

Interpreting $\hat{\beta}_{OLS}$ as the causal effect of USSD bias on news coverage has several problems. First, measurement error, which is presumably random, will attenuate the OLS estimates. Second is the problem of reverse causality. If the USSD reports may be influenced by U.S. media, then $\hat{\beta}_{OLS}$ will reflect the effect of NYT coverage on U.S. bias as well as the effect of U.S. bias on NYT coverage. Finally, there is an omitted variable bias problems. Both the USSD and the NYT may be responding to popular opinion. Media firms may slant their news coverage to satisfy their readers who prefer news sources that confirm their prior beliefs (Mullainathan and Schleifer, 2005; Gentzkow and Shapiro, 2006). For example, consider the lead-up to the invasion of Iraq in 2003. The U.S. government may have biased reports of human rights situations in Iraq unfavorably to garner support for the war while the news media may have slanted their reports against Iraq to satisfy a popular anxiety about the Middle East after 9/11. Then, the correlation between U.S. government bias against Iraq and negative news media coverage will overstate the true causal effect of government bias on news coverage. In this case, the correlation could over-estimate the true effect.

We address these problems by exploiting the plausibly exogenous variation in the bias caused by changes in the strategic value of a country to the U.S. Our measure of the change in strategic value is the interaction of political alliance with the U.S. and UNSC membership. We use a change in the strategic value rather than the level of strategic value for two reasons. First, strategic value is by itself unobservable. Hence, strategic value is inferred by estimating the effect of UNSC membership for U.S. allies relative to non-allies during the Cold War.

Second, using the changes in strategic value relaxes the need to assume that Amnesty’s bias does not change over time. As long as Amnesty’s bias does not change for U.S. allies when they enter the UNSC relative to non-U.S. allies, we can interpret the effect of the change in the U.S.-Amnesty difference as the effect of U.S. bias. More generally, the identification assumption is that entry and exit onto the UNSC for U.S. allies only affect NYT coverage through changes in a country’s strategic value to the U.S. government.

The first stage equation is the following.

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

The difference in U.S. and Amnesty reports for 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.

We are able to check the identification assumption that changes in PTS scores are driven by changes in U.S. strategic values by comparing the effect during the Cold War, when strategic value of allies on the UNSC was high to the U.S., to the post-Cold War era, when as a hegemon, the U.S. valued UNSC membership of its allies relatively less. We estimate the following equation for the Cold War and post-Cold War period separately.

$$US_{itc} - Amnesty_{itc} = \sum_{c=-2}^3 \theta_c(U.S.Alliance_i \times \mathbf{1} \cdot \boldsymbol{\tau}_c) + \rho_c + U.S.Alliance_i + \pi Amnesty_{it} + \delta_t + \varepsilon_{it} \quad (3)$$

During the Cold War, 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, $\boldsymbol{\tau}_c$, and a continuous measure of U.S. alliance, $U.S.Alliance_i$; fixed effects for the number of years since membership, ρ_c ; the main effect for U.S. alliance, $U.S.Alliance_i$; the score of Amnesty reports and year fixed effects. If the U.S. bias arise entirely 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, $\theta_{-2}, \theta_{-1}, \theta_2, \theta_3 = 0$,

and negative effects for the two years on the council, $\theta_0, \theta_1 < 0$ during the Cold War. In the post-Cold War period, all of the coefficients should be zero.

We can also check the assumption that the NYT does not have a special interest in printing more or fewer stories on UNSC members that are U.S. allies by comparing the effect of the instrument on NYT coverage during the Cold War with the effect after the Cold War.

$$\ln Media_{it} = \theta(U.S.Alliance_i \times UNSC_{it}) + \alpha \mathbf{X}_{it} + \gamma_i + \delta_t + \varepsilon_{it} \quad (4)$$

The natural logarithm of the number of articles is a function of: the interaction term between U.S. alliance and UNSC membership, a vector of country and year specific controls, country fixed effects and year fixed effects. We use two different measures of alliance. If the NYT were especially interested (or disinterested) in U.S. Cold War allies when they are on the UNSC, then we should observe the same effect for during the Cold War as afterwards.

Conceptually, the comparison of the differences-in-differences estimates for the Cold War and post-Cold War periods are similar to a triple difference strategy where we compare U.S. allies to non-allies, when they are on the UNSC to when they are not, between the Cold War era and post-Cold War era. For simplicity, we present the DD estimates as our main results. Tripe difference estimates show that the DD estimates for the Cold War period are statistically different from the DD estimates for the post-Cold War period. For brevity, we do not report them in the paper.

The second stage equation will be similar to equation (1) except that we replace the actual difference between USSD and Amnesty with the fitted values predicted by equation (2). If bias in human rights reports from the USSD causes the NYT to increase coverage of human rights abuses, then $\beta_{2SLS} > 0$.

There are two important reasons why the estimated effects of government bias on NYT should be interpreted as the lower bound of the magnitude of the effect government bias on the average U.S. newspaper. First, the NYT has much more resources than the average news organization. Therefore, it is more likely to obtain independent information and be influenced by the government. Second, we have to consider the how government distortion interacts with the effect of competition on slanting towards consumer preferences. The media market may exacerbate bias by allowing consumers to self-segregate more effectively (Mullainathan and Schleifer, 2005; Gentzkow and Shapiro, 2007b). In this case, the effect of

the USSD bias on the coverage of a single firm will not be equal to the effect on the average coverage across firms. To put this in the context of our study, one can imagine that the NYT may write to a relatively left-leaning audience that places more weight on Amnesty than the USSD and that the NYT is less responsive to USSD reports of human rights than the average U.S. news paper. Then the effect of USSD bias on the NYT will be smaller in magnitude than the effect on average media.

In addition to the main estimates, we can also investigate the hypothesis that the distortion arises because the NYT is sometimes unable to obtain good information. If that is the case, then the effect of government distortion should be smaller in regions where access to independent information is relatively good.

$$\begin{aligned} \ln Media_{it} = & \beta[(US_{it} - Amnesty_{it}) \times Access_i] \\ & + \lambda(US_{it} - Amnesty_{it}) + \pi Amnesty_{it} + \gamma_i + \delta_t + \varepsilon_{it} \end{aligned} \quad (5)$$

The natural logarithm of the number of articles about human rights abuses for country i in year t , $\ln Media_{it}$, is a function of: the interaction term between U.S. under-reporting, $US_{it} - Amnesty_{it}$, and a continuous measure of access in country i , $Access_i$; the main effect of U.S. under-reporting, $US_{it} - Amnesty_{it}$; the difference in human rights scores between the USSD and Amnesty, $US_{it} - Amnesty_{it}$; the score of Amnesty reports, $Amnesty_{it}$; country fixed effects, α_i ; and year fixed effects δ_t . If the effect of government distortions are larger in countries where media access is restricted, then $\beta < 0$. In the 2SLS estimates, we instrument for $(US_{it} - Amnesty_{it}) \times Access_i$ with $U.S.Alliance_i \times UNSC_{it} \times Access_i$, and for $US_{it} - Amnesty_{it}$ with $U.S.Alliance_i \times UNSC_{it}$.

5 Data

This paper compiles existing data from several public sources to construct a country level panel. For human rights violations, we use the Political Terror Scale (PTS), a score calculated by Mark Gibney and a group of human rights scholars at the University of North Carolina. The PTS measures levels of political violence and terror that a country experiences in a particular year based on a 5-level “terror scale” originally developed by *Freedom House*.

Using the same rule, separate indices are constructed from Amnesty International reports and U.S. State Department reports. Below we describe examples of the scale.

Level 1: Countries operate under a secure rule of law. People are not imprisoned for their views and torture is rare or exceptional. E.g. Belize, 2000.

Level 2: There is a limited amount of imprisonment for nonviolent political activity. However, few persons are affected and torture and beatings are exceptional. E.g. Czech Republic, 2000.

Level 3: Imprisonment for political activity is more extensive. Politically-motivated executions or other political murders and brutality are common. Unlimited detention, with or without a trial, for political views is also commonplace. E.g. Albania, 2000.

Level 4: The practices of level 3 affect a larger portion of the population and murders, disappearances, and torture are a common part of life. E.g. Angola, 2000.

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

This index is available for 183 countries over the period 1976-2006. This is not a balanced panel. A few countries are not reported for a few years. And some countries (typically former Soviet Republics) exist only after 1991. We include countries that existed both during and after the CW. Our reported estimates come from a sample where the Ukraine, Belarus and South Africa are excluded. The former were part of the U.S.S.R. before 1991. And the latter because it was absent from all UNGA sessions during the CW period we study. We further restrict the sample to country-year observations where the index is available for both Amnesty International and the U.S. State Department. Amnesty and the U.S. report identical PTS for 84% of the observations. We measure USSD reporting bias as Amnesty PTS subtracted from USSD PTS. If the USSD reports a country as better than Amnesty, then $US_{it} - Amnesty_{it} < 0$. Figure 2A maps the average level of the USSD reporting bias. It shows that under-reporting was most severe in Cold War allies such as Turkey and Saudi Arabia.

We construct a measure for U.S. alliance based on UNGA voting data generously provided by Erik Voeten.¹² For each year and each country, we calculate the fraction of votes that a country votes in agreement with the U.S. In order to capture relevant voting patterns we restrict the sample to resolutions where the U.S. and the U.S.S.R. voted in opposition of each other. Each year there are approximately 100-150 resolutions in the UNGA, of which approximately 70-90 resolutions per year are disagreed on by the U.S. and U.S.S.R. Our main measure of alliance is the fraction of votes a country voted with the U.S. averaged over the period 1985-89. Using this measure, the top three allies of the U.S. and the fraction of divided issues they voted with the U.S. during 1980-84 are: Turkey (0.4), Belize (0.28) and Costa Rica (0.27). The bottom three allies are Mongolia (0), Lao PDR (0), and Czech Republic (0). We use this fixed measure of alliance which does not vary over time to avoid confounding the interpretation with the possibility that voting with the U.S. is itself an outcome of U.S. favoritism. Figure 2B maps the alliance measure for the countries in our sample. We arbitrarily define ally as a country that on average voted with the U.S. more than the median country. Figure 3 is a histogram of alliance measures across countries. It shows that the median country voted with the U.S. approximately 7% of the time.

Data on UNSC membership is collected for the time period 1976-2005 from The United Nations Security Council Membership Rollster.¹³ Our sample excludes the five permanent members of the UNSC. 46 countries in the sample were on the UNSC as a rotating member at least once during 1976-2005. 21 countries were on the Council at least twice. And five countries were on the Council three times.

News coverage of human rights violations is measured as the number of articles about human rights in a given country. It is collected from the *ProQuest Historical Newspapers* and the *Lexis-Nexis Academic* databases. We use the ProQuest database to compile news coverage of human rights abuses in the NYT, and we use the Lexis-Nexis for coverage of any news of a country in the NYT. For a given country in a given year in the Lexis-Nexis dataset, we count the total number of articles under the Lexis-Nexis subject “Human rights violations” with the country name occurring at least five times, and the phrase “human rights” appearing in the title, lead paragraphs or index. The ProQuest and Lexis-Nexis databases

¹²The dataset is available (2008-09-01) at <http://www9.georgetown.edu/faculty/ev42/UNVoting.htm>

¹³See http://www.un.org/sc/list_eng5.asp for list of all countries that were ever members and the years of their memberships.

do not have the same search tools. Specifically, in the ProQuest search it is not possible to specify the subject to be human rights violations. Instead, we search for the phrase “human rights” and require at least one of the following words: “torture”, “violations”, “abuse”, “extrajudicial”, “execution”, “arbitrary arrests”, “imprisonment”, “disappearances”, “politically motivated”. In the paper, we refer to all of these articles as articles of human rights violations. Because the measures for the number of human rights abuse articles and the number of all articles for a country comes from two databases, we do not aggregate the two measures to calculate human rights articles as a share of total articles, or non-human rights articles as the difference between the two measures. Instead, we use the natural logarithm of each measure as separate outcomes.

Our first measure of access is the geographic distance from national capitals to the nearest NYT foreign bureau offices.¹⁴ This distance, measured in kilometers, comes from data on the distance between cities of the world provided by Kristian Skrede Gledisch of the University of Essex. This measure reflects the cost for a foreign correspondent to travel to the location of the story. Our second measure of access is an indicator for the freedom of domestic press from the *Freedom House* data. It reflects the NYT’s ability of picking up stories from local independent sources. This measure ranges from zero to two. Zero indicates no freedom. And two indicates a free press. For example, Afghanistan is rated as zero and Australia is rated as two. This measure is produced annually beginning in 1980. We will use a time invariant measure, calculated as the average measure during 1980-1988, to capture overall media access. This avoids the problem that changes in media freedom within a country over time may be correlated with changes in human rights situations. Figure 4 shows a map of our media freedom variable as well as the NYT foreign bureau offices.

These data are matched together at the country-year level. We restrict the sample to countries that are not classified as high income countries as defined by the World Bank.¹⁵ Former USSR republics are also excluded from the sample. Our matched sample contain 110 countries for the years 1976-2005. Table 1 shows the descriptive statistics of the sample. We define the Cold War period for our sample to be 1976-1988, and the post Cold War

¹⁴The 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.

¹⁵High income countries are defined to be those with 2007 GNP per capita of \$11,456 or more. See <http://web.worldbank.org/WBSITE/EXTERNAL/DATASTATISTICS/0,,contentMDK:20421402~pagePK:64133150~piPK:>

period to be 1992-2005. The three years of 1989, 1990, and 1991, when the Eastern Block gradually dissolved, are excluded. The number of articles on human rights abuses and the number of all news articles averaged across countries does not change after the Cold War. On average, human rights articles comprise of approximately 8% of all news articles on a country. During the Cold War, the U.S. under-reported human rights violations of countries relative to Amnesty by 0.35 PTS index points on average. After the Cold War, the U.S. reports human rights violations of countries as being 0.1 index point worse than Amnesty. On average, 6% of the sample is on the UNSC. And during the Cold War, the average country voted with the U.S. 9% of the time. The average distance between a national capital and the nearest NYT bureau office is approximately 1,450 kilometers. The measure of media freedom is similar during the Cold War and afterwards. Domestic press lacks freedom in approximately 40% of the sample.

6 Results

6.1 Measuring Bias

To observe the correlation between alliance with the U.S. and USSD PTS scores relative to Amnesty PTS scores, we take three figures from Qian and Yanagizawa (2008) that plot the PTS scores (USSD-Amnesty) over time for U.S. allies and non-U.S. allies. For simplicity, an ally is defined to be a country that voted with the U.S. more than the median country (greater than 7% of the time). All other countries are defined to be non-allies. The vertical band in the figures indicate the end of the Cold War. Figure 5A shows that according to USSD reports, the human rights situations deteriorated over time during the Cold War and then stabilized afterwards. During the Cold War, the USSD reported that allies were better than non-allies, and the difference was constant. However, after the Cold War, there is an immediate convergence. Figure 5B plots the PTS for Amnesty reports. It shows that Amnesty reported all allies and non-allies as having similar human rights situations, and there is no systematic change after the Cold War. Figure 5C plots the difference in the U.S. and Amnesty PTS. It shows that relative to Amnesty, the USSD consistently under-reports the human rights abuses of its allies during the Cold War but that there is no difference afterwards. These figures show that scores from both reports fluctuate over time,

but systematic changes only occur for USSD reports.

6.2 OLS

The OLS estimates of equation (1) are shown in Table 2. The estimate in columns (1) and (2) show that increasing the PTS score from the U.S. State Department and from Amnesty for a country by 1 index point is correlated with approximately 19% and 12% more articles in the NYT. The estimates are both statistically significant at the 1% level. Column (3) shows that the USSD under-reporting by one index point relative to Amnesty is correlated with 6% fewer articles about human rights abuses in the NYT. Column (4) shows that if we control for both USSD reports and Amnesty reports in the same regression, both coefficients are positive and statistically significant at the 1% level. It shows that under-reporting one point by the U.S. is correlated with 18% fewer articles in the NYT. Column (5) shows that this is robust to controlling for UNSC membership. Columns (6)-(10) repeat the estimation for the post-Cold War sample. The estimates are similar.

6.3 First Stage and Reduced Form Estimates

This section presents the first stage and reduced form estimates. Before we estimate the first stage equation, we can check the validity of the instrument. Our identification strategy assumes that being on the UNSC for U.S. allies affected NYT articles only by affecting a country's strategic value to the U.S. government. If this is true, then the effect on USSD under-reporting should be larger during the Cold War than afterwards. We examine this by estimating equation (3) for the Cold War sample and the post-Cold War sample separately. The coefficients are shown in Appendix Table A1 and plotted in Figure 4. The figure shows that during the Cold War, the USSD under-reported human rights violations of U.S. allies relative to non-allies immediately upon a country's entry onto the UNSC, and returned reporting to initial levels immediately after a country's exit from the Council. Interestingly, there is no effect for the post-Cold War period. The point estimates for the years on the Council are statistically significant at the 10% level for the Cold war period.

Next, we estimate the first stage equation (2) for the Cold War period. The estimates from using the full sample are shown in Table 3 columns (1). These results show that being on the Council decreases USSD PTS relative to Amnesty by 0.23 PTS points more for a

country on the 75th percentile of the U.S. alliance distribution (that votes with the U.S. on approximately 11% of the divided issues) than for a country on the 25th percentile (that votes with the U.S. on approximately 5% of them). Column (2) shows the estimate using a sample restricted to countries that have served on the UNSC at least once. The estimate is similar to that in column (1). Both estimates are statistically significant at the 1% level. Columns (3) and (4) show the analogous estimates for the post-Cold War sample. They show that UNSC membership for U.S. allies has no effect during the post-Cold War period. The coefficients are small in magnitude and statistically insignificant.

Columns (5) and (6) show the reduced form estimates from equation (4) for the Cold War sample. These results show that being on the Council results in 29% fewer stories for a country on the 75th percentile of the U.S. alliance distribution than a country on the 25th percentile. The estimate is statistically significant at the 1% level. Column (4) shows that the effect is similar using a sample restricted to countries that have ever served on the UNSC. Columns (7) and (8) show the analogous estimates for the post-Cold War sample. The coefficients are close to zero and statistically insignificant. The finding that there is no effect after the Cold War, when the strategic value of allies is lower for the U.S. suggests that the effects we find for the Cold War period indeed reflect strategic value to the U.S.

6.4 2SLS

The 2SLS results are presented in Table 4. Columns (1) and (2) show the estimates for the Cold War. They show that the USSD’s under-reporting by one point relative to Amnesty causes a 78% decrease in NYT human rights coverage. Table 1 showed that on average, the USSD under-reported relative to Amnesty by 0.35 points during the Cold War. Hence, USSD under-reporting caused coverage of human rights abuses for the average country to decrease by $0.78 \times 0.35 = 41\%$ during the Cold War. Column (2) shows that the estimate is similar for the sample restricted to countries that were on the Council. Columns (3) and (4) show that there is no effect during the Post Cold War Era.

6.5 Robustness

Table 5 presents the OLS and IV estimate controlling for region-specific time trends. Columns (1) and (2) show the OLS estimates with and without controlling for country-specific linear

time trends. The estimates are similar and both statistically significant at the 1% level. Columns (3) and (4) show the 2SLS estimates with and without controlling for country-specific linear time trends. They are both statistically significant at the 10% level. The magnitude of the coefficient in column (4) is smaller with the additional control, implying that USSD under-reporting caused coverage of human rights abuses for the average country to decrease by 25%. We will use this conservative estimate when calculating the average effect implied by our results.

6.6 Are the results driven by consumer demand?

An alternative explanation for the reduced form and 2SLS estimates is that readers are particularly interested in news of U.S. allies when they are on the UNSC, or that they are particularly interested in bad news of U.S. allies when they are on the Council. This seems unlikely a priori. But to be cautious, we explore these hypotheses empirically. We can investigate the first hypothesis by estimating the effect on overall news coverage. The results are shown in Table 6. Columns (1), (2) and (3) show the OLS, reduced form and 2SLS estimates. In all cases there is no effect. The coefficients are close to zero and statistically insignificant.

To test the second hypothesis, we will need a measure of bad news that is unlikely to originate from the U.S. government. For this, we are currently collecting data on the number of articles containing the phrases “infant mortality”, “starvation”, “AIDS”, “HIV”, “epidemic”, or “famine”.

6.7 Are News Organizations Unable to Obtain Independent Information?

We test the hypothesis that distortions occur because news organizations are unable to obtain independent information by examining whether the effect of government distortion is larger in regions where media access is more restricted. We first use the distance from the capital to the nearest NYT foreign bureau office to approximate the cost for NYT correspondents to travel to the country to report on the situation. Table 7 columns (1) and (2) present the OLS and 2SLS estimates of the interaction effects of the difference in USSD and Amnesty PTS and distance to the nearest NYT foreign bureau. The estimate and the standard errors

are both approximately zero. The main effect does not vary by distance.

Second, we use the Freedom House measure for media freedom for the domestic press. This reflects the NYT's ability of picking up stories from independent local media. Table 7 columns (3) and (4) present the OLS and 2SLS coefficients for the interaction effects of the difference in USSD and Amnesty PTS and the indicator for access. The estimates are small in magnitude and insignificant. The main effect does not vary by access of domestic newspapers. We conclude from the evidence in this section that the main results are not caused by the NYT's inability to obtain information.

6.8 Quantifying the Average Effect

In this section, we assess the magnitude of the average effect in three ways. First, we calculate the average effect for a select group of countries in terms of the number and percentage of stories. Table 8 presents the average effect of U.S. under-reporting for three selected U.S. allies. Column (1) shows the average difference between U.S. and Amnesty reports during the Cold War. Column (2) shows the average number of articles published annually in the NYT about human rights abuses. Column (3) shows the effect of USSD under-reporting on NYT coverage in percentage terms using our most conservative estimate from Table (7) column (6). This is the product of the estimated effect of U.S. under-reporting (-0.817) and the average difference in PTS scores shown in column (1). It shows that under-reporting by the U.S. during the Cold War reduced the amount of coverage for Honduras, DRC (Zaire) and Argentina by 52.0%, 29.0% and 36.2%. Column (4) shows the effect on coverage in terms of numbers of articles for the entire thirteen year Cold War period of our sample. This is the product of the annual average number of articles in column (2), the estimated effect of U.S. under-reporting (-0.817) , and thirteen. It shows that under-reporting by the U.S. during the Cold War reduced the number of articles during 1976-88 on human rights abuses in the NYT for Honduras, DRC (Zaire) and Argentina by approximately 34, 13 and 101 articles.

Second, we quantify the effects in terms of foreign dollars. This calculation is based on the idea that U.S. voters would prefer the U.S. government to not give aid to governments that commit many human rights abuses. A back-of-the-envelope calculation using our results can reveal the dollar value of a suppressed article in the NYT to a country in terms U.S. foreign aid dollars. USSD's favoring a country by one PTS point relative to Amnesty is

correlated with a 12% reduction in foreign aid. (See Appendix Table A1). The value of one article is therefore 0.12 divided by the product of the estimated effect of under-reporting by one PTS point and the average number of articles per year.

$$\frac{0.12}{0.817 \times 3.6 \text{ Articles per Year}} = 0.041 \quad (6)$$

This shows that one suppressed article raises foreign aid by approximately 4%. In dollar values this is roughly 1.8 million dollars (in 1996 USD).¹⁶ Note that the value of one NYT article should not be interpreted literally as one NYT because coverage in the NYT is correlated with coverage in other news outlets. They share similar sources of primary information, and stories in the NYT are often picked up by other news papers. Hence, the value of an article in the NYT should be conservatively interpreted as the value of an article in all U.S. newspapers.

Finally, we compare the average effects of distortion to the increase in the number of stories from an actual human rights incident. On average, the U.S. under-reported by 0.35 PTS points, reducing coverage by 25% ($\exp^{-0.35 \times 0.817} - 1$). On average, government distortions reduced coverage of human rights by 25%. We use the Tiananmen Square Incident in 1989, which was widely covered in mass media at the time. As the event coincidentally occurred during a internationally covered state visit from Soviet President Mikhail Gorbachev, it is reasonable to assume that the U.S. government could not distort information regarding it. This allows us to use the actual number of NYT 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. In that month, the NYT wrote eleven stories, ten more than the monthly average from the preceding year. Had the Tiananmen Square incident been completely ignored by the NYT it would have written 91% fewer articles. If we compare this with our results, it would mean that government bias reduced coverage by approximately 27.5% ($0.25/0.91$) from what they should have been absent distortions.

¹⁶Average annual foreign aid during the Cold War was approximately 44 million USD (in 1996 dollars).

7 Conclusion

This study estimates the effect of the U.S. State Departments bias in human rights reporting on news coverage in the U.S. media. The results show that by favorably under-reporting human rights abuses, the U.S. government can significantly reduce the amount of media coverage of both human rights violations. These results show that even in a developed country with a large, independently owned and competitive media industry, the scope for supply-side distortion is very large. We also present empirical evidence that suggests that news organizations are not naive. Rather, they are informed of the distortion and choose to report the distorted information. This does not imply that the news firm is necessarily conspiring with the government. The firm may simply be cost-minimizing and the government supplies information at a lower cost relative to other sources. Further exploration of the motives of the media is an interesting avenue for future research.

For policy makers, these results show that the effectiveness of the Fourth Estate to fulfill its function by accurately reporting the truth to the public cannot be guaranteed by market forces amongst media firms; and the problem resides both with the government that distorts the information, and the news organization that may knowingly allow its coverage to be affected.

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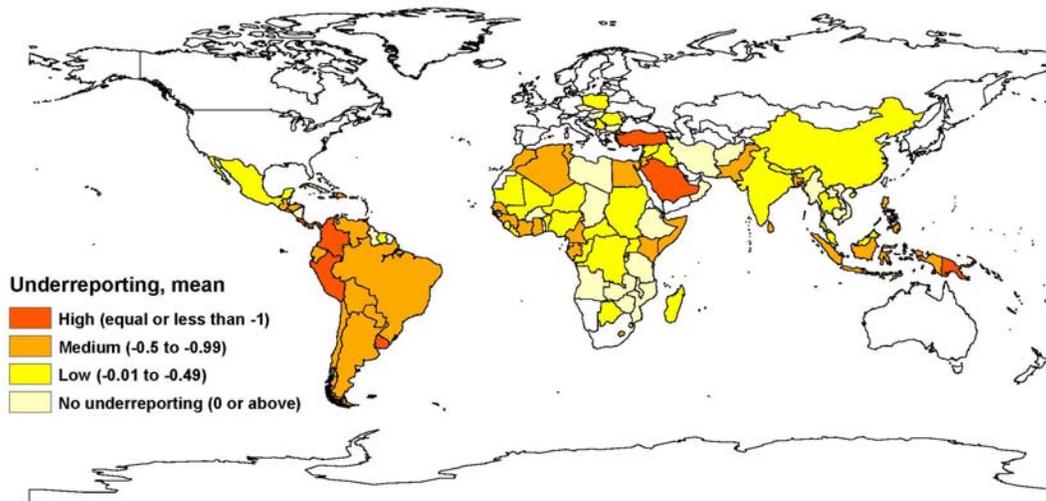
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Figure 1: NYT Articles on Human Rights Abuses Before and After the Release of USSD Country Reports

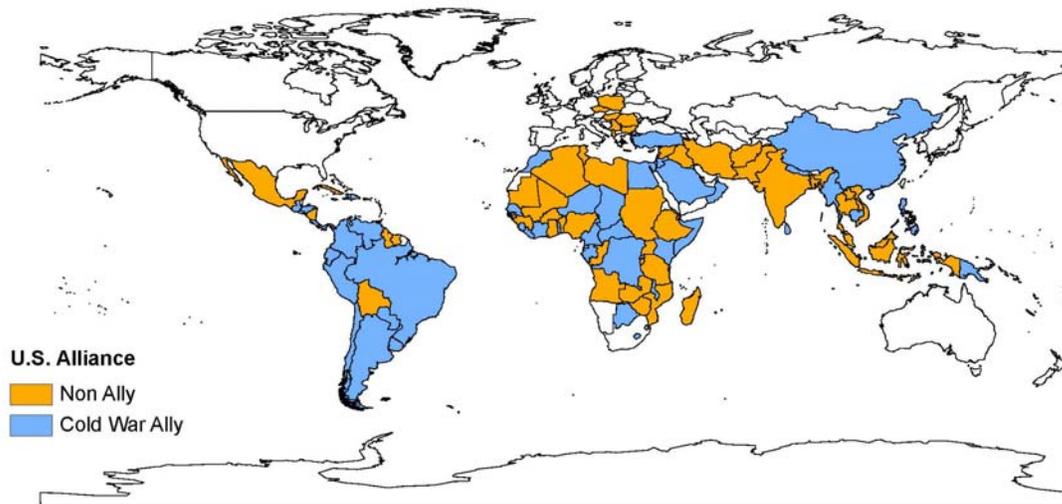


Figure 2A: Map of U.S. Under-reporting



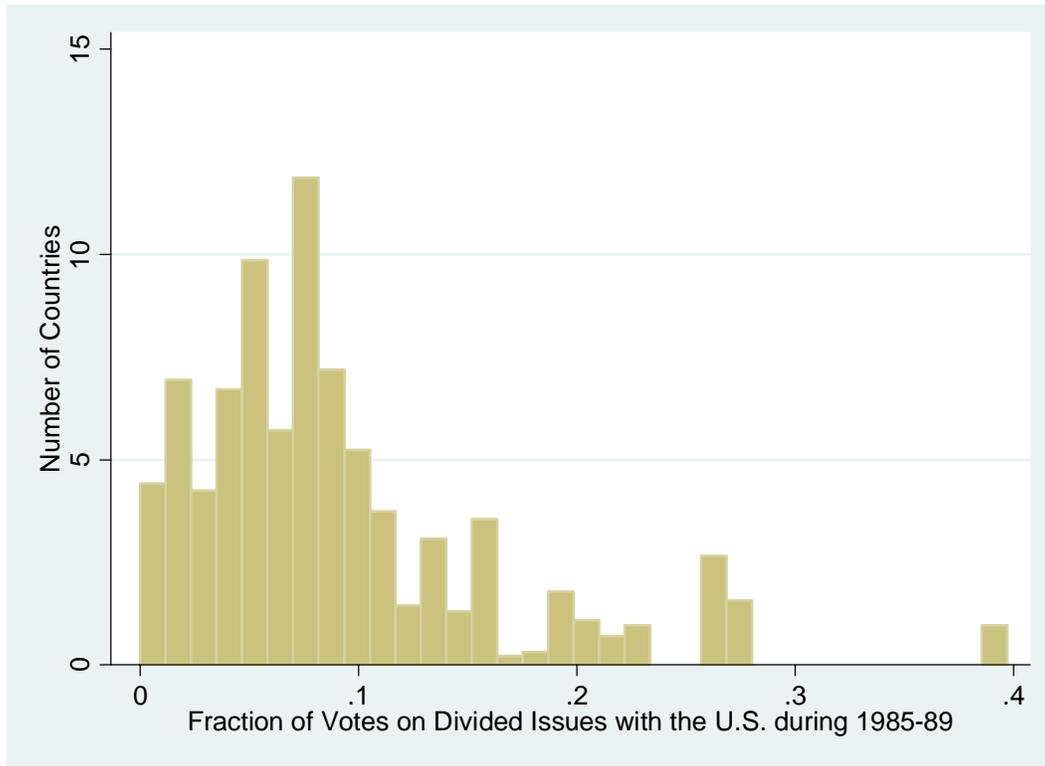
Source: Authors' Computation

Figure 2B: Map of U.S. Alliance



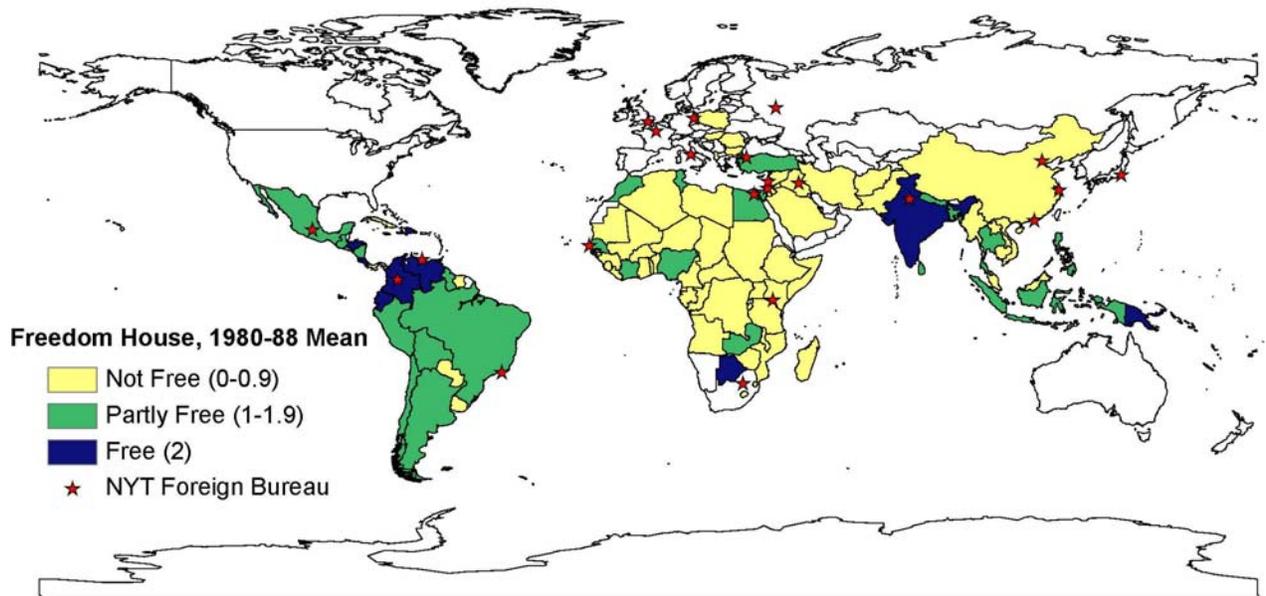
Source: Authors' Computation

Fig 3: Histogram of U.S. Alliance



Source: Authors Computation

Figure 4: Map of Media Freedom and NYT Foreign Bureau Offices



Source: Authors' computation

Fig 5A: U.S. PTS over Time for U.S. Allies and Non-Allies

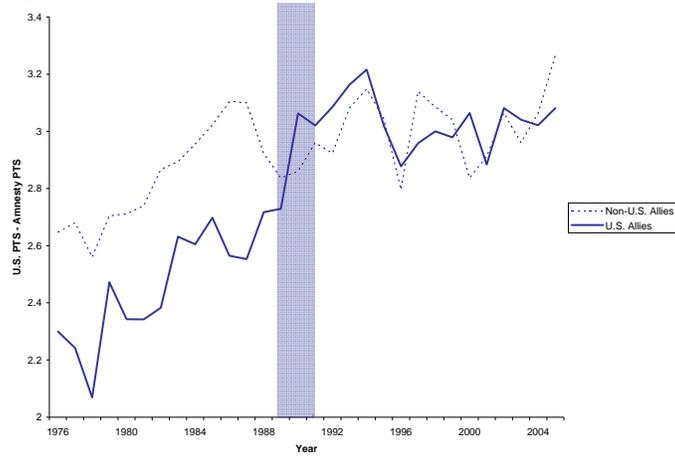


Fig 5B: Amnesty PTS over Time for U.S. Allies and Non-Allies

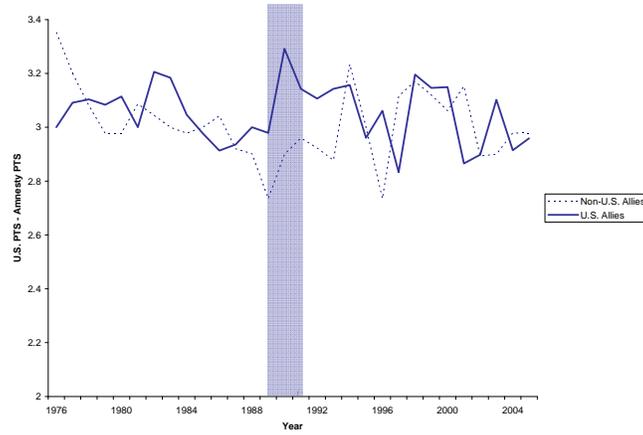


Fig 5C: U.S. - Amnesty PTS Difference over Time for U.S. Allies and Non-Allies

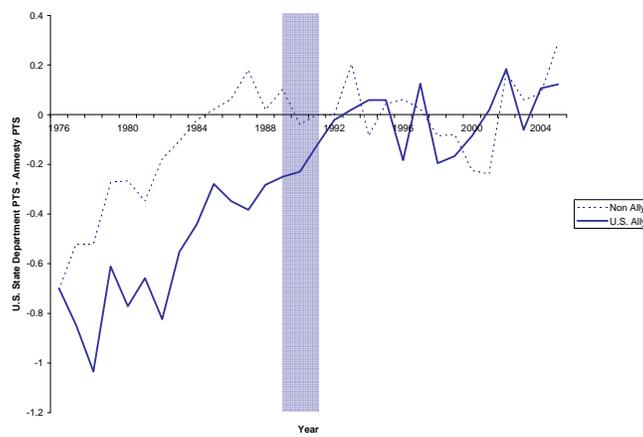


Figure 5: The Effect of U.S. Alliance * UNSC Membership during the Cold War and Afterwards

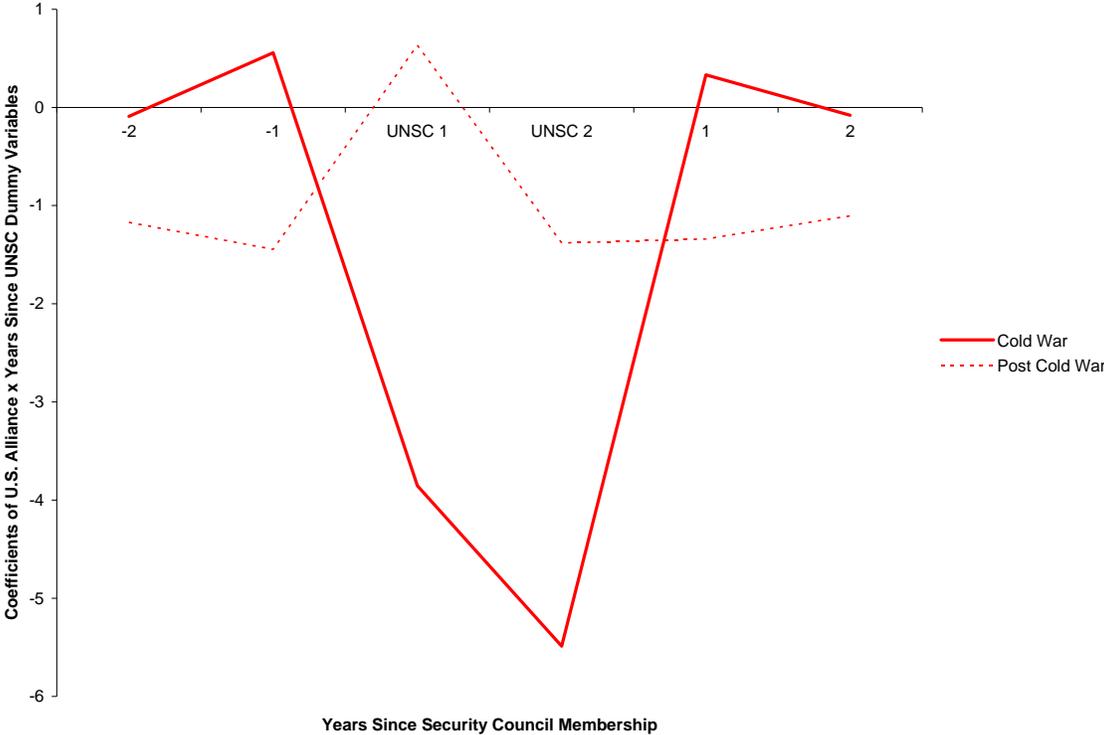


Table 1: Descriptive Statistics

Variable	Cold War		Post Cold War	
	Obs	Mean	Obs	Mean
NYT Human Rights Articles	1010	3.60 (6.08)	1327	3.77 (7.03)
NYT Total Articles	473	61.88 (102.97)	547	60.12 (80.74)
U.S. State Department PTS	1033	2.68 (0.96)	1349	3.03 (1.09)
Amnesty PTS	1033	3.03 (0.94)	1349	3.02 (1.07)
U.S. - Amnesty PTS	1033	-0.35 (0.75)	1349	0.01 (0.69)
UNSC Membership	1033	0.06 (0.25)	1349	0.06 (0.23)
U.S. Alliance	1033	0.09 (0.07)	1349	0.09 (0.07)
Distance between National Capitals and Nearest NYT Bureau	1033	1439.20 (1063.13)	1349	1475.93 (1163.78)
Media Freedom	1033	0.39 (0.49)	1349	0.38 (0.48)

Standard deviations are in parentheses.

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

	Dependent Variable: Ln NYT Human Rights Articles									
	Cold War					Post-Cold War				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
U.S. - Amnesty PTS			0.062 (0.033)	0.176 (0.042)	0.176 (0.042)			-0.002 (0.027)	0.119 (0.036)	0.120 (0.036)
U.S. Stated Department PTS	0.194 (0.039)					0.157 (0.039)				
Amnesty International PTS		0.117 (0.035)		0.215 (0.043)	0.215 (0.043)		0.122 (0.038)		0.197 (0.049)	0.198 (0.049)
UNSC					0.024 (0.075)					0.068 (0.089)
Clusters	105	105	105	105	105	110	110	110	110	110
Observations	1010	1010	1010	1010	1010	1327	1327	1327	1327	1327
R-squared	0.69	0.68	0.68	0.69	0.69	0.67	0.67	0.66	0.67	0.67

All regressions control for country and year fixed effects.

Standard errors are clustered at the country level.

Table 3: The First Stage and Reduced Form Estimates of the Effect of UNSC*U.S. Alliance

	Dependent Variables							
	U.S. - Amnesty PTS				Ln NYT Human Rights Articles			
	Cold War		Post-Cold War		Cold War		Post-Cold War	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample:	Full	Ever on UNSC	Full	Ever on UNSC	Full	Ever on UNSC	Full	Ever on UNSC
U.S. Alliance x UNSC	-3.551 (1.343)	-3.388 (1.470)	1.157 (0.960)	1.194 (1.006)	-5.348 (2.242)	-5.215 (2.234)	0.377 (1.854)	0.233 (1.821)
UNSC	0.306 (0.118)	0.294 (0.129)	-0.143 (0.116)	-0.147 (0.117)	0.403 (0.169)	0.398 (0.174)	0.029 (0.157)	0.040 (0.157)
Amnesty PTS	-0.562 (0.049)	-0.647 (0.060)	-0.634 (0.034)	-0.650 (0.047)	0.118 (0.035)	0.122 (0.044)	0.122 (0.038)	0.106 (0.056)
Clusters	106	61	111	61	105	60	110	60
Observations	1033	630	1349	761	1010	607	1327	739
R-squared	0.56	0.56	0.50	0.51	0.69	0.71	0.67	0.62

All regressions control for country and year fixed effects.

Standard errors are clustered at the country level.

Table 4: The 2SLS Estimates of U.S. Under-reporting of Human Rights on News Coverage

	Dependent Variable: Ln NYT Human Rights Articles			
	Cold War		Post-Cold War	
	(1)	(2)	(3)	(4)
Sample:	Full	Ever on UNSC	Full	Ever on UNSC
U.S. - Amnesty PTS	1.500 (0.804)	1.534 (0.849)	0.344 (1.593)	0.201 (1.498)
UNSC	-0.056 (0.126)	-0.052 (0.128)	0.078 (0.131)	0.071 (0.133)
Amnesty International PTS	0.951 (0.461)	1.099 (0.567)	0.340 (1.017)	0.237 (0.986)
Clusters	105	60	110	60
Observations	1010	607	1327	739
R-squared	0.17	0.14	0.66	0.63

All regressions control for country and year fixed effects.

Standard errors are clustered at the country level.

Table 5: OLS and 2SLS Estimates of the Effect of U.S. Under-reporting
with Region -Specific Time-Varying Controls

	Dependent Variable: Ln Human Rights Articles in NYT			
	OLS		2SLS	
	(1)	(2)	(3)	(4)
U.S. - Amnesty PTS	0.176 (0.042)	0.166 (0.042)	1.500 (0.804)	0.817 (0.422)
Amnesty PTS	0.215 (0.043)	0.201 (0.051)	0.951 (0.461)	0.636 (0.295)
UNSC	0.024 (0.075)	0.003 (0.078)	-0.056 (0.126)	-0.028 (0.091)
Country FE	Y	Y	Y	Y
Country FE x Time Trend	N	Y	N	Y
Clusters	105	105	105	105
Observations	1010	1010	1010	1010
R-squared	0.69	0.75	0.17	0.65

All regressions control for year fixed effects.

Standard errors are clustered at the country level.

Table 6: The Effect of U.S. Under-reporting on All News Coverage

	Dependent Variables		
	Ln NYT HR	U.S. - Amnesty PTS	Ln NYT HR
	(1)	(3)	(5)
	OLS	RF	2SLS
U.S. - Amnesty PTS	0.015 (0.067)		-0.026 (0.906)
UNSC x U.S. Alliance		0.087 (3.061)	
Amnesty PTS	0.216 (0.079)	0.208 (0.063)	0.193 (0.537)
UNSC	-0.105 (0.110)	-0.111 (0.237)	-0.104 (0.116)
Clusters	42	42	42
Observations	473	473	473
R-squared	0.89	0.89	0.89

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

Table 7: The Effect of U.S. Under-reporting on NYT Coverage by Media Access

	Dependent Variables: Ln NYT Human Rights Articles			
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS
U.S. - Amnesty x Distance to Nearest NYT Bureau Office	0.000 (0.000)	0.000 (0.001)		
U.S. - Amnesty PTS x Restricted Media Access			-0.021 (0.062)	-0.043 (0.898)
U.S. - Amnesty	0.132 (0.061)	1.511 (2.567)	0.184 (0.054)	1.541 (1.188)
Amnesty PTS			0.215 (0.044)	0.964 (0.540)
SC Member			0.023 (0.075)	-0.101 (0.207)
Observations	1009	1009	1009	1009
R-squared	0.69	0.16	0.69	0.15

All regressions control for the full set of interaction terms and country and year fixed effects.
Standard errors are clustered at the country level.

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

			Average Effect on Annual NYT Human Rights Articles	
	Avg US-Amnesty	Avg Annual HR NYT	% Change	# of Articles during 1976-88
	(1)	(2)	$\exp[0.817*(1)] - 1$	(2) x (3) x 13
Honduras	-0.9	5	-52.1%	-34
DRC	-0.42	3.5	-29.0%	-13
Argentina	-0.55	21.5	-36.2%	-101

* 0.817 is the 2SLS estimate of the effect of U.S. under-reporting controlling for country-specific year trends.

Appendix Table A1: The Effect of UNSC x U.S. Alliance on U.S. Under-reporting by Years since UNSC Membership

	Dependent Variable: U.S. - Amnesty PTS			
	Cold War	Post- Cold War	Cold War	Post- Cold War
	(1)	(2)	(3)	(4)
U.S. Alliance x 2 Years Before UNSC	-0.091 (1.538)	-1.171 (1.481)	0.312 (1.524)	0.728 (1.121)
U.S. Alliance x 1 Years Before UNSC	0.557 (1.691)	-1.446 (1.376)	3.027 (2.862)	0.493 (0.750)
U.S. Alliance x 1st Year on UNSC	-3.854 (3.346)	0.633 (1.551)	-2.594 (2.088)	1.962 (1.166)
U.S. Alliance x 2nd Year on UNSC	-5.487 (2.649)	-1.382 (1.453)	-3.384 (1.849)	-0.152 (1.487)
U.S. Alliance x 1 Year After UNSC	0.332 (2.378)	-1.340 (1.250)	0.568 (2.227)	-0.852 (1.442)
U.S. Alliance x 2 Years After UNSC	-0.079 (4.110)	-1.105 (1.353)	0.443 (3.654)	-2.085 (1.710)
U.S. Alliance	-2.280 (0.548)	0.434 (0.438)		
Amnesty PTS	-0.271 (0.035)	-0.186 (0.028)	-0.564 (0.050)	-0.634 (0.034)
Country FE	N	N	Y	Y
Year FE	Y	Y	Y	Y
Clusters	106	111	106	111
Observations	1033	1349	1033	1349
R-squared	0.27	0.11	0.56	0.50

Standard errors are clustered at the country level.

Table A2: The Correlation between U.S. Under-reporting and U.S. Foreign Aid

	Dependent Variables		
	LnTotAid (1)	LnMilAid (2)	LnEcoAid (3)
U.S. - Amnesty PTS	-0.122 (0.058)	-0.067 (0.045)	-0.102 (0.056)
Amnesty PTS	-0.080 (0.061)	-0.016 (0.057)	-0.078 (0.058)
Observations	2263	2263	2263
R-squared	0.75	0.67	0.75

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