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 manipulate news in a highly competitive media market comprised of independently owned outlets. We examine the effect of the U.S. State Department’s favorably under-reporting of human rights violations for its strategic allies on the New York Times’ coverage of human rights abuses for those countries. We show that the government bias reduces news coverage of abuses in allied countries by almost one-third. Our findings suggest that these distortions are not likely to be consumer driven. (P16 Political Economy, L82 Media)


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1 Introduction

A government’s ability to manipulate news reported by media outlets that it owns is obvious, especially given the findings of recent studies.¹ The extent of government manipulation is much less obvious in a competitive market where all news organizations are independently owned.² These markets typically exist in Western countries where standard political theory and journalists implicitly assume that the free press, or the Fourth Estate, can diminish distortions from the government. To the best of our knowledge, no study has attempted to directly estimate the extent of government manipulation of the press in such a context. This is a question of growing importance as an increasing number of studies provide evidence on the media’s political and social consequences.³ Our study attempts to fill this gap by examining the scope for government manipulation of news in the United States. We estimate the effect of the U.S. State Department’s (USSD) favorable under-reporting of human rights violations for its strategic allies on the amount of news coverage on human rights abuses for those countries in the New York Times (NYT) during the latter part of the Cold War, 1976-1988.

This study faces several empirical difficulties. The first is to measure U.S. government

¹Besley and Prat (2006) provide a theoretical framework for understanding the effects of market competition on government capture. Their model predicts that competition and independent ownership decreases the likelihood of capture. Existing empirical evidence supports the prediction that independent ownership and competition are beneficial towards overcoming government manipulation. They include survey evidence on the correlation between government ownership of the media and reduced political and economic freedoms (Djankov et al., 2001); the finding that access to independent television stations in Russia increases the likelihood of voting for opposition parties (Enikolopov, Petrova and Zhuravskaya, 2009); and that competition in the U.S. increases the likelihood that news organizations will report the truth (Prat and Stromberg, 2005; Gentzkow, Glaeser and Goldin, 2006). Conversely, Durante and Knight (2009) find 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.

₂Assuming that news outlets are not naively unaware of the government’s incentives to distort information, a necessary condition for it to avoid government manipulation is that its profits from reporting the truth are greater than its profits from reporting distorted information. The first depends on the readers’ valuation of the truth. The latter depends on the value of the bribe from the government. For example, the U.S. government can promise to call on a journalist at a presidential press conference in exchange for her to write a story about human rights abuses in Marxist Ethiopia instead of an U.S. ally such as Zaire.

³Recent studies have shown that media can affect voting behavior (Prat and Stromberg, 2005; Gentzkow, 2006; Della Vigna and Kaplan, 2007; and Chang and Knight, 2008), other political behavior (Olkhn, 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).
bias. The government’s bias is unobservable and any inferred measure will measure true bias with error. Second is the problem of reverse causality: public policy can be an outcome of media.\(^4\) 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 do not \textit{prima facie} rule out the possibility. Finally, we face the omitted variable bias (OVB) 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 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.\(^5\) 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.

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. The PTS is available annually for all countries for 1976-2006. They score the only two sources of human rights violations reports that cover the same scope. Because information given by the employees of these agencies will be highly correlated with the reports, the scores will measure the overall attitudes of the USSD and Amnesty. 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 measurement.

\(^4\)For example, Stromberg (2004) finds 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.

\(^5\)Media slant refers to when profit-maximizing news organizations write to affirm the beliefs of its readers, who have positive utility in such affirmation. Mullainathan and Shleifer (2005) argue that this increases the dispersion in news coverage across media outlets. Gentzkow and Shapiro (2006, 2007b) extends the argument and provide empirical evidence. Our study is about the effect on one newspaper and not the effect on the distribution of news across outlets. (We are unable to examine the effect on the distribution across newspapers because historical data on articles only cover five newspapers in total for the period of our study). That said, one of the motivations of focusing on the NYT, which has a left-leaning readership, during the time of a right-winged political era, is to mitigate biases that could be caused by media slant. Also, in the section on mechanisms, we will empirically investigate the extent to which slanting contributes to our findings.
error, OVB and reverse causality. We instrument for the U.S. reporting bias with a source of variation in a country’s strategic value to the U.S. that arguably 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). Only the combination can be interpreted as plausibly exogenous. This strategy measures the effect of the U.S.-Amnesty difference, which changes for U.S. allies when they are on the UNSC.

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 finding that this favoritism does not occur after the Cold War, when the U.S.’s value of its allies presumably decreased. 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. These results support the hypothesis of government capture.

Next, we investigate whether the manipulation is facilitated by information asymmetries. The NYT is likely to be aware of the government’s incentives to manipulate information. However, because its information on what occurs in foreign countries is imperfect, it would have to obtain independent information to mitigate the bias. If this information is costly to obtain, then we can examine the extent to which information plays a role by estimating the interaction effect of government bias and the cost for the NYT to obtain independent information. We use two different measures of cost: 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 the NYT’s ability to pick up stories from independent local

\begin{footnote}{Gentzkow and Shapiro (2008) document several well known incidents where American journalists have actively attempted to overcome government control of information. We believe that it is safe to assume that journalists in the U.S. do not naively believe that the government always tells the truth.}

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media sources. We find that the interaction effect is near zero in magnitude and statistically insignificant for both measures. This suggests that information asymmetries do not play an important role in facilitating government manipulation.

To assess the magnitude of the main results, we attempt to quantify the results in three ways. First, taking our most conservative estimates literally, the results imply 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, Zaire and Argentina by approximately 52%, 29% and 36%. Second, to loosely quantify our results in dollar terms, we can provide an illustrative example in terms of U.S. foreign aid. Our estimates suggest that suppressing one article is on average worth approximately $1.8 million dollars (1996 USD) of U.S. foreign aid to a recipient country. Since articles in the NYT are highly correlated with articles in other newspapers, this should be conservatively interpreted to reflect the value of suppressing one article in each U.S. newspaper. Finally, we attempt to assess the magnitude in terms of reduced coverage relative to coverage of human rights abuses when there was no scope for government manipulation. Using coverage of the Tiananmen Square Incident in 1989 as an example of the latter, our results imply that government distortions on average reduce reports by approximately 27% from what they should be.

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 very competitive media market with many independently owned firms. The 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 in certain contexts. 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 high quality international news is being hindered by a lack of independent information.\footnote{Reports suggest that mid-size newspapers have experienced a 30% decline in the number of foreign correspondents. Frontpage coverage of foreign news in the sixteen U.S. newspapers studied by the 2005 Project for Excellence in Journalism has declined by almost half from 1987 to 2005 (Caroll, 2007). For a more detailed discussion, see Caroll (2007) and Constable (2007).} Our finding that distortions in media reports are not related to its ability for obtaining independent information suggests that the cause of incomplete reporting is much more complex than a
simple decline in resources devoted to foreign news coverage.

The context of this study has several advantages. The independence of the press is enshrined into the U.S. constitution and commonly perceived to be one of the key components of democracy in America. It is also a context where all domestic news outlets are independently owned and where the market for news is by all accounts very competitive (e.g. Djankov et al., 2001). Therefore, the results we obtain on government manipulation in the U.S. can be easily extrapolated to other contexts. We focus on human rights for several reasons. First, it is a well-defined concept in our context 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, by all appearances, it seems that Americans care about human rights and prefer its government to not provide support for governments that violate them. The government seems aware of this preference. Hence if it wants the support of its constituency, it will have an incentive to portray its allies favorably.

This 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 U.S. Media and The New York Times

The U.S. media market is very competitive and all domestic news outlets are independently owned (Djankov et al., 2001). It is very costly to the reputation of a news outlet to report inaccurate facts. This can be observed from the well publicized retractions and corrections that follow mistaken reports. This study follows the assumption made in previous studies of

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8The only media outlet that is not completely independently owned is the Voice of America (VOA), which only broadcasts overseas. They do not play a role in domestic news or in this study.

9Qian and Yanagizawa (2008) provide several example of where U.S. politicians use human rights as a reason to implement economic sanctions. For example, 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”. Letters to the Editor, Washington Post, Monday, June 9, 2008; Page A16.
the media that news organizations will only report accurate facts. The margin for distortion is in the composition of stories, which is presumably more difficult to discredit and therefore less costly to distort. News outlets 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 decreased by increasing the coverage of persecutions by the Marxist government in Ethiopia. This is a similar mechanism to the crowding-out of news found in Eisensee and Stromberg (2004).^10^ 

We focus our discussion on coverage in the NYT for practical reasons. Historical databases that allow the word searches needed for this paper are limited to five American newspapers.^11^ In this paper, we only examine the NYT. The New York Times is the largest metropolitan newspaper in the U.S. It is considered to be one of the leading newspapers in terms of the breadth and depth of its news coverage, for both foreign and domestic. It has received 101 Pulitzer Awards for Journalism, far more than any other newspaper. Over thirty of these were awarded for reporting on international news. It’s reputation for reporting accurate news independent of the wishes of the parties being reported on is strengthened by famous incidents such as *The Pentagon Papers*, where the NYT reported a series of stories based on information leaked to it by a member of the Nixon administration and then fought the Nixon administration in the U.S. Supreme Court in order to overcome an injunction that was placed on it after the first stories were printed.^12^ Reporting foreign news does not generate much profit for the NYT directly. According the *New York Times Co. 2008 Annual Report*, advertising revenues from the domestic news section formed 12% of its total company revenues. In contrast, revenues from the international news section formed only 1% of total company revenues.^13^ However, it has been long believed that foreign news reporting serves as a signal of the overall quality of the paper (Caroll, 2007). And therefore, it may indirectly increase the profits of the NYT by adding to its reputation. It is difficult to obtain information on the cost of a foreign bureau. According to Caroll (2007), an average

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^10^ 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 are 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.


^12^ *New York Times vs. The United States* 403 US 713.

^13^ The New York Times Company publishes eighteen newspapers in addition to the NYT. They include the *Boston Globe* and the *International Herald Tribune*. 
newspaper foreign bureau costs approximately $300,000 per year. The major costs are rent, travel and the reporters’ salaries. In certain places (e.g. Baghdad today), security can also be expensive. Typically, a foreign bureau will have only one stable correspondent. The NYT has more than all other newspapers with the exception of The Washington Post.  

Many have pointed out that foreign news reporting is very expensive for newspapers relative to reporting domestic news. However, it may not be as costly if the number of stories are weighted by some measure of importance. For example, the foreign news section of The Baltimore Sun used 5% of the company’s total annual budget but produced over 25% of its front page stories that year (Caroll, 2007).

_A priori_, focusing on the NYT has several advantages for interpretation. First, because the NYT is more likely to write its own news stories rather than pick up stories from other U.S. news agencies (e.g. newswires), the effect on the NYT will be less likely to be confounded by the possibility of information herding across news organizations. The second point is closely related. The NYT has one of the largest stable foreign news staffs and can most easily obtain independent information. Therefore, if the distortions are partly due to information asymmetries between the NYT, the estimates on coverage in the NYT will provide a lower-bound estimate for the effect of coverage in the average U.S. newspaper. Finally, the NYT is perceived as having one of the most politically left-leaning readerships. The readers of the NYT are less likely to share similar beliefs as the right-winged administration of Ronald Reagan. Therefore, if news organizations slant their reports to affirm the beliefs of its readers, then the NYT will be less likely than the average U.S. newspaper to slant in the direction of the government.

### 2.2 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.  

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

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14 For example, worldwide, the NYT has 30 foreign correspondents; Washington Post 23; The LA Times 29; The Chicago Tribune 11; The Christian Science Monitor 9; USA Today 5; The Boston Globe 4; the Baltimore Sun 3; Tehe Philadelphia Inquirer 2; The San Francisco Chronicle 1; The Washington Times 1; the Boston Herald 0 and the Detroit News 0 (Caroll, 2002).

15 This is the definition used by Freedom House, the PTS project, and the CIRI project.
foreign countries, the United States is the only country that systematically releases its reports to the public. Similarly, Amnesty International is the only non-governmental organization which makes systematic reports over the same broad scope and long time horizon. Both of these organizations use the same definition for human rights abuses as set forth by the *Universal Declaration of Human Rights*. And they publish reports using similar formats.

The *Country Reports on Human Rights Practices* are submitted annually by the USSD to the U.S. Congress. 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”. Amnesty International, commonly known as Amnesty, is one of the only two international non-governmental organizations reporting on human rights abuses world wide. 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.

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

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

18 [http://www.state.gov/g/drl/rls/hrrpt/](http://www.state.gov/g/drl/rls/hrrpt/)


20 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 quantitavely scored by human rights databases.

21 See 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.
after the release of USSD Country Reports averaged across years. The sharp increase on the
day of the release suggests that the reports are used by reporters. (The release of Amnesty
reports is similarly associated with an increase in the number of stories on human rights
abuses in the NYT. The increase is smaller in magnitude and not shown in the paper). Over the course of a 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. At that time, 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 (Gladdis, 2006).

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 les-
son is that my support for American policy [now] counts for nothing” (Gbadolite,
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. 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.\textsuperscript{22} The value of UNSC membership 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).\textsuperscript{23} 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 means that a rotating member has more influence over an issue if no member of the P5 chooses to veto. (Rotating members always have the power to bring issues to the table). 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

\textsuperscript{22} Africa 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.

\textsuperscript{23} Kuziemko and Werker (2005) study the effect of being on the UNSC during a year that is strategically important to the U.S. government on foreign aid receipts from the U.S. They measure the importance of a year using the number of articles in the NYT in that year which contains the phrase “United Nations” and “Security Council”. Our study differs from theirs in that our outcomes is the number of articles (on human rights) written in the NYT. Our variation for strategic value to the U.S. government will instead come from the interaction of a country’s alliance with the U.S. in the General Assembly and whether it is on the UNSC.
members and no member of the P5 will veto). They can be initiated by either the General Assembly or the Security Council and have been convened on ten occasions. Nine of these ten occasions occurred during the Cold War.\footnote{1956 Suez Crisis; 1956 Soviet Invasion of Hungary (Hungarian Revolution); 1958 Lebanon Crisis; 1960 Congo Crisis; 1967 Six Days War; 1980 Soviet invasion of Afghanistan; 1980 Israeli-Palestinian Conflict; 1981 South African occupation of Namibia (South West Africa); 1982 Israeli Occupation of the Golan Heights (Golan Heights Law); 1997 Israeli-Palestinian conflict (East Jerusalem and Israeli-occupied territories).}

It is therefore also 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.\footnote{In 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.} 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 \cite{alesina2000}. In a companion paper \cite{qian2008}, we 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. We constructed a measure for U.S. alliance based on UNGA voting data that were generously provided by Erik Voeten.\footnote{The dataset is available (2008-09-01) at http://www9.georgetown.edu/faculty/ev42/UNVoting.htm} 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 in the General Assembly. Figure 2 plots the fraction of votes in the UNGA that are divided between the U.S. and U.S.S.R. (Russia after 1991) over time. It shows that the fraction of divided votes increased dramatically during
the early eighties and intensified throughout the conflict in Afghanistan, and stayed high until the end of the Cold War. After the Cold War, it dropped dramatically. Figure 2 also plots the fraction of votes that countries vote with the U.S. on divided issues (average across countries). It shows that there was a downward trend in this measure of U.S. alliance during the latter part of the Cold War. (Appendix Figure A1 provides a scatter plot by country).

To illustrate 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 here is defined to be a country that voted with the U.S. more than the median country (greater than 7% of the time) on average during 1985-89, when the Cold War was relatively intense as reflected by the fact that there were more divided votes in the UNGA than previous years. All other countries are defined to be non-allies. The vertical band in the figures indicate the end of the Cold War. Figure 3A shows that according to USSD reports, 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 3B 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 3C 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. Note that these figures show that scores from both reports fluctuate over time. This suggests that reports from both agencies contain information. The key point is that systematic changes only occur for USSD reports. This descriptive evidence supports the claim that changes in the difference in PTS scores are driven by changes in U.S. strategy. They also suggest that the estimations of this paper can be repeated using the Post-Cold War sample as a “placebo”.

We know of no studies or commentaries on why the USSD reported all countries as being worse behaved over this period of the Cold War. We speculate that portraying other countries as being worse off over time may have been used to justify the dramatic increase in Cold War expenditures during this period. It is interesting to note that reports of increasingly worse human rights for developing countries (Figure 3A) occurred at the same time as votes with the U.S on divided issues decreased and the number of divided issues increased (see Figure 2).
3 How Would Country Reports DISTORT the News?

We assume that the NYT is aware that the government has incentives to manipulate information. Similarly, the NYT may be aware of the government’s strategic objectives. It knows that the government wishes to make its allies appear relatively favorable to readers. The NYT chooses between going along with the government and reporting a distorted composition of stories or going against the government and reporting an undistorted composition of stories. As a profit maximizing firm, it will only allow its coverage to be distorted if the profits from this are higher than the profits from reporting the truth. The former depends on factors such as the value of the information the government can provide exclusive access to. The latter depends on the readers’ valuation of accurate news (Besley and Prat, 2006). The probability of capture will increase with the revenues from going along with the government relative to revenues from reporting the truth; and decrease with the cost of going along with the government (e.g. loss of reputation) relative to the cost of reporting the truth (e.g. obtaining independent information, losing access from government sources).

In principle, there are reasons other than government capture that can cause the NYT to allow itself to be distorted by the government. The main possibility is that the distortions are consumer driven. For example, government strategic objectives may coincide with reader beliefs to cause the NYT to slant reports towards its readers’ beliefs (Mullainathan and Shleifer, 2005; Gentzkow and Shapiro, 2006). We address this possibility with the instrumental variables strategy and will support the assumptions necessary for identification with several robustness checks. Alternatively, readers may simply value knowing the government’s opinion, whatever that may be. It is not possible to test this empirically. However, the fact that the U.S. State Department is a mundane government bureaucracy rather than a political figure or celebrity makes this somewhat unlikely a priori. In any case, this will not affect the internal validity of the estimates of this study. (We return to this point at the end of the paper when we discuss the welfare implications of the results).

The scope for government manipulation can be magnified by information asymmetries between the government and the NYT. Even though the NYT is aware of the existence of government bias, it cannot offset it unless if it obtains sufficient independent information.

28The framework and the assumptions follow Besley and Prat (2006). The empirical exercise of this paper fit well into the special example of their model where the government bribes by allowing or cutting off access to information that is valuable to the news organization.
This is especially relevant in our context where we have assumed that only the composition of facts is distorted, not the accuracy of facts. In this case, the NYT chooses between reporting accurate facts on countries chosen by the government or obtaining facts on other countries on its own. Since the latter is likely to be costly, we can empirically test the extent that information asymmetries contribute to government capture by examining the interaction effects of government bias and the cost for the NYT to obtain independent information.

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 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 U.S. 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,

\[
\ln \text{Articles}_{it} = \beta (U_{it} - Am_{it}) + \pi Am_{it} + \gamma_i + \delta_t + \varepsilon_{it} \tag{1}
\]

The natural logarithm of the number of articles about human rights abuses for country \( i \) in year \( t \), \( \ln \text{Articles}_{it} \), is a function of: the difference in human rights scores between the USSD and Amnesty, \( U_{it} - Am_{it} \); the score of Amnesty reports, \( Am_{it} \); country fixed effects, \( \alpha_i \); and year fixed effects \( \delta_t \). We use the logarithm of the number of articles to reduce the weight placed on a few high profile countries which are frequently written about for reasons that presumably have little to do with changes in actual human rights situations in their countries.\(^{29}\) We control for the score of Amnesty reports because we are interested

\(^{29}\)For 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.
in the effect of the U.S. when the U.S. and Amnesty differs, and for most of the time – approximately 84% in our sample– the U.S. and Amnesty report similar scores. Therefore, controlling for Amnesty reports has little effect on the coefficients but improves the precision of the second stage estimates. All standard errors are clustered at the country level.

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 particular country is correlated with more coverage of that country’s human rights abuses in the NYT, then $\hat{\beta}_{OLS} > 0$.

### 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.

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. when it is an ally, and it enters or exits the UNSC. Identification comes from the change in strategic value when a country is an ally and when it is on the UNSC. Only the combination can be interpreted as exogenous. This has two key advantages. First, strategic value is by itself unobservable. It is inferred by estimating the effect of UNSC membership for U.S. allies relative to non-allies during the Cold War. Second, exploiting 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.

\[
U.S._{it} - Amnesty_{it} = \theta(U.S.\text{Alliance}_{i} \times UNSC_{it}) + \alpha X_{it} + \gamma_i + \delta_t + \varepsilon_{it} \tag{2}
\]

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., \(U.S.\text{Alliance}_{i}\), and membership on the UNSC, \(UNSC_{it}\); a vector of country-year specific controls such as Amnesty’s reported PTS, \(X_{it}\); country fixed effects and year fixed effects. Higher PTS reflects worse human rights conditions. Therefore, if the U.S. favors its allies when they are on the Council with milder reports of human rights abuses, \(\hat{\theta} < 0\).

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 \(\hat{\beta}_{2SLS} > 0\).

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; and that UNSC membership of allies does not affect NYT coverage through other channels. For example, readers may have a special interest in news of allies when they are on the Council. We are able to partly check both of these assumptions. 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 an ally’s entry onto and exit from 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.

We can do this in two ways. First, we can repeat the main estimations. Second, we can repeat the exercise by estimating a more flexible equation for the Cold War and post-Cold War period separately. The following estimate has the advantage that it 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.

\[
US_{ite} - Amnesty_{ite} = \sum_{c=-2}^{3} \theta_c(U.S.\text{Alliance}_{i} \times \mathbf{1} \cdot \tau_c) + \rho_c + \gamma_i + \pi Amnesty_{it} + \delta_t + \varepsilon_{it} \tag{3}
\]
The difference in U.S. and Amnesty reports for country $i$ in year $t$, $c$ years since it is a UNSC member is a function of: the interaction between a dummy variable indicating the number of years since UNSC membership, $\tau_c$, and a continuous measure of U.S. alliance, $U.S.Alliance_i$; fixed effects for the number of years since membership, $\rho_c$; the score of Amnesty reports, country and year fixed effects. If the U.S. bias arises many 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, $\hat{\theta}_{-2}, \hat{\theta}_{-1}, \hat{\theta}_2, \hat{\theta}_3 \approx 0$, and negative effects for the two years on the council, $\hat{\theta}_0, \hat{\theta}_1 < 0$ during the Cold War. In the post-Cold War period, all of the coefficients should be zero. Using the post-Cold War period as a placebo is similar to a triple differences estimate. For simplicity of interpretation, we only report the DD estimates for the two samples.$^{30}$

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. We chose to use the PTS over other quantitative scores of human rights because it extends the furthest back in time, to 1976.$^{31}$ This determines the time period of our study. Using the same rule, separate indices are constructed from Amnesty International reports and U.S.

$^{30}$The post-Cold War “placebo” should be interpreted cautiously. The comparison of the DD estimates for the post-Cold War period is a useful exercise for providing additional evidence for the claim that changes in the PTS difference is mainly driven by U.S. policy. However, it should not be interpreted too literally. The post-Cold War era is not the perfect placebo because the meaning of alliance is inherently different in the post Cold War period. For example, the group of Soviet allies that always voted with the U.S.S.R. during the Cold War begins to vote with the U.S. on some occasions after the Cold War (see Appendix Figure A1). This means that Soviet allies, which had no value to the U.S. during the Cold War will have value afterwards. More importantly, this means that our assumption that Council membership of U.S. allies are more valuable to the U.S. than membership of non-allies, may no longer be true after the Cold War. (Also, there are many fewer divided issues in the UNGA which means that voting with the U.S. will probably be a less precise measure of alliance during the Cold War period).

$^{31}$The CIRI Human Rights Data Project, like the PTS Project, reads the reports by Amnesty and the USSD and provides a score. However, the CIRI incidices only begin in 1981. They also differ from PTS in that they attempt to provide disaggregated incidices for the type of human rights. This means that while the two incidices are correlated (approximatley 0.65-0.73), they are not directly comparable. See Wood and Gibney (2009) for a detailed discussion.
State Department reports. We describe examples of the scale below.

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.\textsuperscript{32}

This index is available for 183 countries over the period 1976-2006.\textsuperscript{33} We include countries that existed both during and after the Cold War. Our reported estimates come from a sample where the Ukraine, Belarus and South Africa are excluded. The former countries were part of the U.S.S.R. before 1991. And the latter is excluded because it was absent from all UNGA sessions during the Cold War period we study (due to Apartheid). 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 $USSD_{it} - Amnesty_{it} < 0$. Figure 4A 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.

\textsuperscript{32}See Data Appendix for a more detailed description.

\textsuperscript{33}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 follow Qian and Yanagizawa (2008) in using voting patterns in the UNGA to construct a continuous measure of alliance with the U.S. 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. This period provides us with the highest number of divided votes, and therefore the best measure of alliance during this period.\footnote{We do not use a time-varying measure because there are some countries who abstain from voting in some years. This fixed measure of alliance which does not vary over time also has the advantage that it is less likely to be an outcome of U.S. favoritism than a time varying measure. We use the average alliance during 1985-1989 because that is the period with the highest number of divided votes (see Figure 2). Using the fraction of votes with the U.S. when there are few divided votes will give a very noisy measure of alliance. Our estimates are robust to changing the measure of alliance to be the average of votes during periods between 1981 and 1989, when there were many divided votes. For brevity, we do not report estimates with these alternative measures in the paper.} Using this measure, the top three allies of the U.S. and the fraction of divided issues they voted with the U.S. during 1980-84 are: Turkey (0.4), Belize (0.28) and Costa Rica (0.27). The bottom three allies are Mongolia (0), Lao PDR (0), and Czech Republic (0). Figure 4B maps the alliance measure for the countries in our sample. We arbitrarily define an ally to be countries that on average voted with the U.S. more than the median country. The median country voted with the U.S. approximately 7\% of the time.

Data on UNSC membership is collected for the time period 1976-2005 from The United Nations Security Council Membership Rollster.\footnote{See http://www.un.org/sc/list_eng5.asp for list of all countries that were ever members and the years of their memberships.} 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. In the ProQuest search, we search for the phrase “human rights” and require at least one of the following words: “torture”, “violations”, “abuse”, “extrajudicial”, “execution”, “arbitrary arrests”, “imprisonment”, “disappearances”, “politically motivated”; and the country’s name. Our measure of human rights coverage is the total number of articles that results from the search per country per year. The ProQuest search allows the maximum of 1,000 hits. This is not a binding constraint for our search of human rights articles, where the maximum...
coverage of a country in one year during our sample period is 75 articles. However, it does create a problem when we search for all news stories of a country in a year. For this measure, we use instead the *Lexis-Nexis Academic* databases. The search algorithm used by the Lexis-Nexis databases differs slightly from ProQuest. Therefore, we do not combine 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.\(^{36}\)

To measure the cost for the NYT to obtain independent information, we use two indicators of the NYT’s *access* such information. Our first measure of access is the geographic distance from national capitals to the nearest NYT foreign bureau offices.\(^{37}\) 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 4C 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.\(^{38}\) 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.

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\(^{36}\) Both search engines, in principle, should allow us to distinguish articles written by the NYT from articles picked up from newswires or the Associated Press. We are currently going through the extensive exercise of making this distinction. The measures presented in this paper does not make the distinction. (We do not expect it to make a significant difference to the results since the NYT writes most of its own stories).

\(^{37}\) The NYT has foreign bureau offices in Mexico City, Caracas, Rio de Janeiro, London, Paris, Berlin (West Berlin), Bogota, Shanghai, Frankfurt, Rome, Jerusalem, Beirut, Cairo, Istanbul, New Delhi, Dakar, Nairobi, Johannesburg, Moscow, Beijing, and Hong Kong.

\(^{38}\) High 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:;
We define the Cold War period for our sample to be 1976-1988, and the post Cold War 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 do 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 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 controlling for Amnesty reports, U.S. under-reporting by one point relative to Amnesty is correlated with 18% fewer articles in the NYT. Column (4) shows that this is robust to controlling for UNSC membership. Columns (5)-(8) repeat the estimation for the post-Cold War sample. The estimates are similar.

6.2 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 the flexible 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
5. 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.

To assess the statistical significance of the additional favoritism for allies when they are
on the UNSC, we estimate the first stage equation, equation (2), for the Cold War period.
The estimates from using the full sample are shown in Table 3 columns (1). The estimated
coefficients reveal the magnitude of U.S. under-reporting in terms of PTS index points for
a country that votes with the U.S. 100% of the time in the UNGA. Note that the mean
country votes with the U.S. only 9% of the time. Hence, to obtain the mean effects, one
needs to multiply the estimated coefficients by 0.09. In the discussion of the results, we
will try to provide a sense of the effect for the relevant sample where the most any country
votes with the U.S. is on 40% of divided votes. We will discuss the results by comparing the
implied impact for a country in the 75th percentile of U.S. alliance (who votes with the U.S.
approximately 11% of the time) and a country that is in the 25th percentile of U.S. alliance
(who votes with the U.S. approximately 5% of the time). The results show that being on the
Council decreases USSD PTS relative to Amnesty by 0.23 PTS points more for the former
than for the latter.

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 from estimates from equation (\textit{??}) 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. supports our claim that the effects we find for the Cold War period are driven mainly by changes in the strategic value to the U.S.

6.3 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.

6.4 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.

6.5 Are the results driven by consumers?

Here, we explore the possibility that the results are driven by consumer preferences rather than, or in addition to government manipulation. For example, readers have a particular interest in U.S. allies when they enter the UNSC. A likely scenario is that readers who prefer
allies to behave well, are especially concerned about bad behavior of allies when they gain more power and enter the Council. In this case, readers’ interests bias against our results which show that Council membership of allies reduces coverage of the bad behavior of their governments. A bias in the same direction as our results would require readers to be less interested in the bad news of allies when they enter the council. This seems unlikely a priori. But we cannot test it empirically because we do not have a good measure of other “bad” news. We can test the related hypothesis that readers are less interested in overall news coverage of allies when they enter the council (e.g. maybe because readers see election onto the Council of allies as a signal of good behavior). We estimate the effect of favorable PTS scores on overall news coverage as measured by the total number of stories containing the name of the country. 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.

Alternatively, one may suspect that the main effects, which cover the period of 1976-1988, understates the average effect of government manipulation over time because the NYT, which writes for a left-leaning readership, is less willing to go along with the biases of a right-wing Reagan administration (1980-88) that is in office for the majority of the study period, than for a left-leaning readership. To investigate this possibility, we estimate the interaction effects of government bias and a dummy variable for whether the president in office is a Democrat. If the NYT is would have been more willing to report the biases of a left-leaning government, then the estimate for the interaction effect should be positive. Table 7 columns (1)-(4) shows the OLS and 2SLS estimates for the Cold War period for the full sample and the sample restricted to the countries that were on the UNSC at least once. They show that the estimated interaction effects are negative and almost statistically significant at the 10% level. Columns (5)-(8) show analogous estimates for the placebo post-Cold War period. There is no evidence to support the claim that the main estimates are reduced in magnitude by the NYT’s relative unwillingness to go along with a right-winged administration.

39To test this empirically, we would need a measure of the amount of coverage on other bad news for which the government would prefer to hide about its allies and for which the government is not a source of information for the NYT.

40For the Cold War era, the democratic Carter administration was in office during 1976-1979; and for the placebo post-Cold War era, the democratic Clinton administration was in office during 1992-1999.
6.6 Information Asymmetries

We test the hypothesis that information asymmetries between the NYT and the USSD contribute to the scope for manipulation by examining whether the effect of government distortion is larger in regions where the NYT’s 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 8 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 estimates 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 to capture the NYT’s ability of picking up stories from independent local media. Table 8 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. To the extent that these measures accurately capture the difficulty in obtaining independent information, these findings suggest that the main results are not caused by the NYT’s inability to obtain information.

6.7 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 9 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 5 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 U.S. foreign aid. This calculation is based on the assumption 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 of 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.\(^41\) 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).\(^42\) 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 newspapers. 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 \(25\% \ (\exp^{-0.35 \times -0.817} - 1)\). We use the Tiananmen Square Incident in 1989, which was widely covered in mass media at the time. As the event coincidentally occurred during an 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

\[ \frac{0.12}{0.817 \times 3.6 \text{ Articles per Year}} = 0.041 \]  \hspace{1cm} (4)

\(^{41}\) Average annual foreign aid during the Cold War was approximately 44 million USD (in 1996 dollars).
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.

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 government manipulation of the news can be large.

It is beyond the scope of this paper to provide a conclusive answer for why the NYT allows distortions. Given that the State Department is a rather mundane bureaucracy of the U.S. government, it seems rather unlikely that readers have positive utility from reading its opinions per se. There are two other potential explanations. The first is cost-driven. The NYT may simply be cost-minimizing and obtaining information from the government is less costly than from other sources. Given that we find no interaction effect with the cost of obtaining information from the field, a cost-driven explanation would require that the main costs for generating human rights stories are associated with the cost of obtaining information for editors, and that this information is least expensive when provided by the government. This is a plausible explanation to the extent that human rights stories are breaking news stories, which are often assigned by the editor. The second explanation is revenue-driven. The value of the stories that the government can provide or cut off access to may be more

43 Interviews with journalists suggest that stories are typically generated in two ways. First, there are in-depth reports which are generated by field reporters (in consultation with the editor). 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, there are breaking news stories (e.g. swine flu), which are typically generated by editors who may call government contacts to determine where to send the field reporters. In this case, our results do not rule out the cost-minimization channel. Anecdotal accounts from journalists and a search we conducted on the average length of human rights articles relative to breaking news articles suggest that human rights stories are typically not breaking news.
valuable than the value of reporting the correct composition of human rights stories. Given the observation that most human rights stories tend to be in-depth stories that are generated together by reporters in the field, we speculate that the second explanation plays a larger role in our context. In any case, both explanations are consistent with a model where the NYT is profit-maximizing.

The welfare implications of the results are ambiguous. If advertising revenues reflect readers' valuation for accuracy in international news, then the welfare reduction from these distortions likely to be small. However, advertising revenues may not accurately capture the readers' utility. For example, respondents to readership surveys by The Washington Post, The Los Angeles Times and The Baltimore Sun ranked the international/national news section among the top sections they read (Caroll, 2007). There are also many other plausible scenarios for which the distortions can reduce welfare. For example, it may be difficult for readers to verify the accuracy of the composition of stories. Or, they may not be time consistent and therefore undervalue their future utility from accurate news reports. These are interesting avenues for future research.

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44 If readers are able to easily verify the accuracy, then the results of this paper imply that there are probably significant fixed costs to entry in the market for news. In the case of zero entry costs and where readers have some positive value for accuracy (however small), then a distortion by the NYT should induce a new entrant to report accurately and earn a positive profit. In equilibrium, this should cause the NYT to not distort. Some examples of fixed entry costs are equipment or reputation.

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


8 Data Appendix

8.1 Country Report Example


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

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

a. Political and Other Extrajudicial Killing

The widespread extrajudicial killings of Tutsis and suspected Tutsis that occurred in government-controlled areas after the start of the war in 1998 did not continue during the year; by the start of the year, surviving Tutsis generally either had left the government-controlled part of the country or were in hiding, places of refuge, or government custody. One Tutsi girl died of an illness while in government custody, but it was credibly reported that her death was not due to government negligence or abuse. The Government also materially supported Mai Mai and Hutu armed groups, which, according to credible reports, repeatedly killed unarmed as well as armed Tutsis in areas held by antigovernment forces. However, government officials no longer instigated mob violence against unarmed Tutsis, and there were no further reports of mass extrajudicial killings of Tutsis by the security forces.
Members of the security forces committed extrajudicial killings, and the Government misused the judicial system to try, sentence, and execute numerous persons without due process.

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

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

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

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

....

b. Disappearance

....

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

....

d. Arbitrary Arrest, Detention, or Exile

....

e. Denial of Fair Public Trial

....

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

....

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

....
Section 2 Respect for Civil Liberties, Including:

a. Freedom of Speech and Press

....

b. Freedom of Peaceful Assembly and Association

....

c. Freedom of Religion

....

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

....

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

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

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

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

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

The previous Constitutions forbade discrimination based on ethnicity, sex, or religious affiliation, but the Government did not enforce these prohibitions effectively and continued to act with serious official prejudice against members of the Tutsi ethnic
group. Societal discrimination remained an obstacle to the advancement of certain groups, particularly women, Tutsis, Muslims, and the indigenous Pygmy (Batwa) people.

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

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

**People with Disabilities** ....

**Indigenous People** ....

**Religious minorities** Approximately 50 percent of the population are Roman Catholic, 20 percent are Protestant, and 10 are percent Islamic. About 20 percent practice traditional indigenous religions exclusively. However, many persons practice elements of both Christianity or Islam and a traditional indigenous religion... In January in the Kamituga area of South Kivu Province, Mai Mai leader Sylvestre Louetcha reportedly accused of witchcraft 32 women who supported their traditional ruler in resisting Mai Mai demands for forced labor, then cut off their breasts, forced them to eat their own breasts, and killed them. In November, in the Mwenga area of South Kivu Province, RCD-Goma rebels tortured and buried alive 15 women who were suspected of having performed witchcraft in support of
a local Mai Mai force, according to some accounts of that incident (see Sections 1.a. and 1.c.).

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

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

Section 6 Worker Rights a. The Right of Association

....
b. The Right to Organize and Bargain Collectively  
....
c. Prohibition of Forced or Compulsory Labor  
....
d. Status of Child Labor Practices and Minimum Age for Employment  
....
e. Acceptable Conditions of Work  
.....

8.2 PTS

This section describes the Political Terror Scale. The information is taken from the Political Terror Scale Project.\textsuperscript{46}

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

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

- Ignore Own Biases. Coders should make every attempt to keep their own biases out of their work. Thus, coders are instructed to ignore their perceptions of a country, and to limit their coding to the information provided in the country report.

- Give Countries the Benefit of the Doubt. Coders also are instructed to give the benefit of the doubt in favor of the countries they are coding. Thus, if a coder thinks that a country could be scored as either a level 2 or a level 3, the country is to receive the lower score. Sometimes coders will not feel comfortable making a choice between two levels. In those instances, coders will oftentimes score a country using both numbers, such as 2/3. If the coder has either of these numbers, we use the level where there is agreement.

\textsuperscript{46}See http://www.politicalterrorscale.org/about.php
Read What the Report is Saying. Finally, coders are instructed to read what the report is trying to say. One of the keys is to look at the adjectives used in these reports. For example, “reports” of torture is different in kind (and less serious) than “widespread” torture, which also is different (and less serious) than “systematic” torture.

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

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

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

**Level 1– Bahrain** (Amnesty 2001): Significant steps were taken in 2001 to promote and protect human rights. All political prisoners and detainees were released and the State Security Court and state security legislation were abolished. Bahraini nationals who had been forcibly exiled or prevented from entering the country were allowed to return without conditions. An Ethiopian woman remained under sentence of death. In December, two people . . . were said to have been subjected to beatings by police officers. . . . They were detained for two days before they were released on bail.
Figure 1: NYT Articles on Human Rights Abuses Before and After the Release of USSD Country Reports
Figure 2: The Fraction of Divided Votes and U.S. Alliance over Time

Note: The fraction of divided votes is calculated as the number of votes that the U.S. and the USSR vote differently on (in the UNGA) divided by the total number of issues voted on in a year. After the dissolution of the USSR, Russia is used in its place.
Fig 3A: U.S. PTS over Time for U.S. Allies and Non-Allies

Fig 3B: Amnesty PTS over Time for U.S. Allies and Non-Allies

Fig 3C: U.S.– Amnesty PTS Difference over Time for U.S. Allies and Non-Allies
Figure 4A: Map of U.S. Under-reporting

Source: Authors' Computation

Figure 4B: Map of U.S. Alliance

Source: Authors' Computation

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

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

The diagram represents the coefficients of U.S. Alliance x Years Since UNSC Dummy Variables over the years since Security Council Membership. The graph distinguishes between the Cold War period (solid red line) and the Post Cold War period (dashed red line). The x-axis represents the years since Security Council membership, while the y-axis shows the coefficients for the interaction term. The data suggests a significant impact during the Cold War years, with a decrease in coefficients post-Cold War.
### Table 1: Descriptive Statistics

<table>
<thead>
<tr>
<th>Variable</th>
<th>Cold War</th>
<th>Post Cold War</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Obs</td>
<td>Mean</td>
</tr>
<tr>
<td>NYT Human Rights Articles</td>
<td>1010</td>
<td>3.60</td>
</tr>
<tr>
<td></td>
<td>(6.08)</td>
<td></td>
</tr>
<tr>
<td>NYT Total Articles</td>
<td>473</td>
<td>61.88</td>
</tr>
<tr>
<td></td>
<td>(102.97)</td>
<td></td>
</tr>
<tr>
<td>U.S. State Department PTS</td>
<td>1033</td>
<td>2.68</td>
</tr>
<tr>
<td></td>
<td>(0.96)</td>
<td></td>
</tr>
<tr>
<td>Amnesty PTS</td>
<td>1033</td>
<td>3.03</td>
</tr>
<tr>
<td></td>
<td>(0.94)</td>
<td></td>
</tr>
<tr>
<td>U.S. - Amnesty PTS</td>
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<td>-0.35</td>
</tr>
<tr>
<td></td>
<td>(0.75)</td>
<td></td>
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<tr>
<td>UNSC Membership</td>
<td>1033</td>
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<tr>
<td></td>
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<td></td>
</tr>
<tr>
<td>U.S. Alliance</td>
<td>1033</td>
<td>0.09</td>
</tr>
<tr>
<td></td>
<td>(0.07)</td>
<td></td>
</tr>
<tr>
<td>Distance between National Capitals and Nearest NYT Bureau</td>
<td>1033</td>
<td>1439.20</td>
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<tr>
<td></td>
<td>(1063.13)</td>
<td></td>
</tr>
<tr>
<td>Media Freedom</td>
<td>1033</td>
<td>0.39</td>
</tr>
<tr>
<td></td>
<td>(0.49)</td>
<td></td>
</tr>
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*Standard deviations are in parentheses.*
Table 2: OLS Estimate of the Correlation between Newspaper Articles and U.S. and Amnesty PTS Scores

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<th></th>
<th>Cold War</th>
<th>post-Cold War</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>U.S. - Amnesty PTS</td>
<td>0.176</td>
<td>0.175</td>
</tr>
<tr>
<td></td>
<td>(0.042)</td>
<td>(0.042)</td>
</tr>
<tr>
<td>U.S. Stated Department PTS</td>
<td>0.194</td>
<td>0.160</td>
</tr>
<tr>
<td></td>
<td>(0.039)</td>
<td>(0.039)</td>
</tr>
<tr>
<td>Amnesty International PTS</td>
<td>0.117</td>
<td>0.215</td>
</tr>
<tr>
<td></td>
<td>(0.035)</td>
<td>(0.043)</td>
</tr>
<tr>
<td>UNSC</td>
<td>0.025</td>
<td>0.025</td>
</tr>
<tr>
<td></td>
<td>(0.075)</td>
<td>(0.075)</td>
</tr>
<tr>
<td>Clusters</td>
<td>105</td>
<td>105</td>
</tr>
<tr>
<td>Observations</td>
<td>1009</td>
<td>1009</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.69</td>
<td>0.68</td>
</tr>
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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

<table>
<thead>
<tr>
<th>Sample:</th>
<th>U.S. - Amnesty PTS</th>
<th>Ln NYT Human Rights Articles</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Cold War</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>U.S. Alliance x UNSC</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>-3.551</td>
<td>-3.388</td>
</tr>
<tr>
<td></td>
<td>(1.343)</td>
<td>(1.470)</td>
</tr>
<tr>
<td>UNSC</td>
<td>0.306</td>
<td>0.294</td>
</tr>
<tr>
<td></td>
<td>(0.118)</td>
<td>(0.129)</td>
</tr>
<tr>
<td>Amnesty PTS</td>
<td>-0.562</td>
<td>-0.647</td>
</tr>
<tr>
<td></td>
<td>(0.049)</td>
<td>(0.060)</td>
</tr>
<tr>
<td>Clusters</td>
<td>106</td>
<td>61</td>
</tr>
<tr>
<td>Observations</td>
<td>1033</td>
<td>630</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.56</td>
<td>0.56</td>
</tr>
</tbody>
</table>

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

<table>
<thead>
<tr>
<th>Sample:</th>
<th>Dependent Variable: Ln NYT Human Rights Articles</th>
<th>Cold War</th>
<th>Post-Cold War</th>
</tr>
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<tbody>
<tr>
<td></td>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>U.S. - Amnesty PTS</td>
<td>1.500</td>
<td>1.534</td>
<td>0.344</td>
</tr>
<tr>
<td></td>
<td>(0.804)</td>
<td>(0.849)</td>
<td>(1.593)</td>
</tr>
<tr>
<td>UNSC</td>
<td>-0.056</td>
<td>-0.052</td>
<td>0.078</td>
</tr>
<tr>
<td></td>
<td>(0.126)</td>
<td>(0.128)</td>
<td>(0.131)</td>
</tr>
<tr>
<td>Amnesty International PTS</td>
<td>0.951</td>
<td>1.099</td>
<td>0.340</td>
</tr>
<tr>
<td></td>
<td>(0.461)</td>
<td>(0.567)</td>
<td>(1.017)</td>
</tr>
<tr>
<td>Clusters</td>
<td>105</td>
<td>60</td>
<td>110</td>
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<tr>
<td>Observations</td>
<td>1010</td>
<td>607</td>
<td>1327</td>
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<tr>
<td>R-squared</td>
<td>0.17</td>
<td>0.14</td>
<td>0.66</td>
</tr>
</tbody>
</table>

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

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

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

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

All regressions control for country and year fixed effects. Standard errors are clustered at the country level.
Table 7: The Differential Effect of U.S. Under-reporting on NYT Coverage by Political Party of Presidential Administrations

<table>
<thead>
<tr>
<th></th>
<th>Cold War</th>
<th></th>
<th></th>
<th>Post Cold War</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>OLS</td>
<td>2SLS</td>
<td>OLS</td>
<td>2SLS</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>U.S. - Amnesty x Democratic President</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>Full</td>
<td>Ever on UNSC</td>
<td></td>
<td></td>
<td>Full</td>
</tr>
<tr>
<td></td>
<td></td>
<td>0.080</td>
<td>-0.046</td>
<td>-2.934</td>
<td>-2.540</td>
<td>-0.016</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.073)</td>
<td>(0.086)</td>
<td>(1.820)</td>
<td>(1.561)</td>
<td>(0.053)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>U.S. - Amnesty</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>0.155</td>
<td>0.157</td>
<td>1.560</td>
<td>1.556</td>
<td>0.136</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.043)</td>
<td>(0.048)</td>
<td>(0.981)</td>
<td>(0.998)</td>
<td>(0.051)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>1009</td>
<td>606</td>
<td>1009</td>
<td>606</td>
<td>1317</td>
<td>737</td>
</tr>
</tbody>
</table>

All regressions control UNSC Member x Democratic President, US Alliance x Democratic President, UNSC Member dummy, country and year fixed effects. Standard errors are clustered at the country level.
Table 8: The Differential Effect of U.S. Under-reporting on NYT Coverage by Media Access

<table>
<thead>
<tr>
<th></th>
<th>Dependent Variables: Ln NYT Human Rights Articles</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td></td>
<td>OLS</td>
</tr>
<tr>
<td>U.S. - Amnesty x Distance to Nearest NYT Bureau Office</td>
<td>0.0000285</td>
</tr>
<tr>
<td></td>
<td>(0.0000241)</td>
</tr>
<tr>
<td>U.S. - Amnesty PTS x Restricted Media Access</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
</tr>
<tr>
<td>U.S. - Amnesty</td>
<td>0.1324973</td>
</tr>
<tr>
<td></td>
<td>(0.0614068)</td>
</tr>
<tr>
<td>Observations</td>
<td>1009</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.69</td>
</tr>
</tbody>
</table>

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

<table>
<thead>
<tr>
<th></th>
<th>Avg US-Amnesty</th>
<th>Avg Annual HR NYT</th>
<th>% Change</th>
<th># of Articles during 1976-88</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>Honduras</td>
<td>-0.9</td>
<td>5</td>
<td>-52.1%</td>
<td>-34</td>
</tr>
<tr>
<td>DRC</td>
<td>-0.42</td>
<td>3.5</td>
<td>-29.0%</td>
<td>-13</td>
</tr>
<tr>
<td>Argentina</td>
<td>-0.55</td>
<td>21.5</td>
<td>-36.2%</td>
<td>-101</td>
</tr>
</tbody>
</table>

* 0.817 is the 2SLS estimate of the effect of U.S. under-reporting controlling for country-specific year trends.
APPENDIX Table A1: The Fraction of Votes that are with the U.S. on Divided Votes between the U.S. and the U.S.S.R.
Appendix Table A1: The Effect of UNSC x U.S. Alliance on U.S. Under-reporting by Years since UNSC Membership

<table>
<thead>
<tr>
<th></th>
<th>Cold War (1)</th>
<th>Post-Cold War (2)</th>
<th>Cold War (3)</th>
<th>Post-Cold War (4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>U.S. Alliance x 2 Years Before UNSC</td>
<td>-0.091 (1.538)</td>
<td>-1.171 (1.481)</td>
<td>0.312 (1.524)</td>
<td>0.728 (1.121)</td>
</tr>
<tr>
<td>U.S. Alliance x 1 Years Before UNSC</td>
<td>0.557 (1.691)</td>
<td>-1.446 (1.376)</td>
<td>3.027 (2.862)</td>
<td>0.493 (0.750)</td>
</tr>
<tr>
<td>U.S. Alliance x 1st Year on UNSC</td>
<td>-3.854 (3.346)</td>
<td>0.633 (1.551)</td>
<td>-2.594 (2.088)</td>
<td>1.962 (1.166)</td>
</tr>
<tr>
<td>U.S. Alliance x 2nd Year on UNSC</td>
<td>-5.487 (2.649)</td>
<td>-1.382 (1.453)</td>
<td>-3.384 (1.849)</td>
<td>-0.152 (1.487)</td>
</tr>
<tr>
<td>U.S. Alliance x 1 Year After UNSC</td>
<td>0.332 (2.378)</td>
<td>-1.340 (1.250)</td>
<td>0.568 (2.227)</td>
<td>-0.852 (1.442)</td>
</tr>
<tr>
<td>U.S. Alliance x 2 Years After UNSC</td>
<td>-0.079 (4.110)</td>
<td>-1.105 (1.353)</td>
<td>0.443 (3.654)</td>
<td>-2.085 (1.710)</td>
</tr>
<tr>
<td>U.S. Alliance</td>
<td>-2.280 (0.548)</td>
<td>0.434 (0.438)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Amnesty PTS</td>
<td>-0.271 (0.035)</td>
<td>-0.186 (0.028)</td>
<td>-0.564 (0.050)</td>
<td>-0.634 (0.034)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>N</th>
<th>N</th>
<th>Y</th>
<th>Y</th>
</tr>
</thead>
<tbody>
<tr>
<td>Country FE</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Year FE</td>
<td>Y</td>
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<td>Y</td>
<td>Y</td>
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<tr>
<td>Clusters</td>
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<td>111</td>
<td>106</td>
<td>111</td>
</tr>
<tr>
<td>Observations</td>
<td>1033</td>
<td>1349</td>
<td>1033</td>
<td>1349</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.27</td>
<td>0.11</td>
<td>0.56</td>
<td>0.50</td>
</tr>
</tbody>
</table>

*Standard errors are clustered at the country level.*
Table A2: The Correlation between U.S. Under-reporting and U.S. Foreign Aid

<table>
<thead>
<tr>
<th>Dependent Variables</th>
<th>LnTotAid (1)</th>
<th>LnMilAid (2)</th>
<th>LnEcoAid (3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>U.S. - Amnesty PTS</td>
<td>-0.122</td>
<td>-0.067</td>
<td>-0.102</td>
</tr>
<tr>
<td></td>
<td>(0.058)</td>
<td>(0.045)</td>
<td>(0.056)</td>
</tr>
<tr>
<td>Amnesty PTS</td>
<td>-0.080</td>
<td>-0.016</td>
<td>-0.078</td>
</tr>
<tr>
<td></td>
<td>(0.061)</td>
<td>(0.057)</td>
<td>(0.058)</td>
</tr>
<tr>
<td>Observations</td>
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<td>2263</td>
<td>2263</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.75</td>
<td>0.67</td>
<td>0.75</td>
</tr>
</tbody>
</table>

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