MEDIA ATTACKS AND POLITICAL INSTITUTIONS

A Dissertation

Presented to

the Faculty of the Department

of Political Science

In Partial Fulfillment

of the Requirements for the Degree

of Doctor of Philosophy

By

Jonathan A. Solis

May, 2018

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ABSTRACT

My dissertation examines media freedom's measurement and then more specifically the institutional determinants of (1) government attacks against media and (2) journalist killings. Media's ability to freely gather and disseminate information remains a critical aspect of democracy. Studies link media freedom to other concepts ranging from human rights, corruption, the democratic peace and conflict, natural resource wealth, political knowledge, and foreign aid. However, media freedom's many dimensions make it difficult for any one index to reliably measure it. In the first part of my dissertation, I propose a new method for measuring media freedom. To develop a more robust measure, I treat media freedom as a latent variable and analyze 12 extant indicators using an Item Response Theory (IRT) model. Utilizing a Bayesian approach, the IRT model generates time-series, cross-sectional (TSCS) data on a bounded, unidimensional scale from 0 to 1 that measures media freedom in 196 countries worldwide from 1960 to 2016. I then apply the data in a replication of Egorov et al.'s (2009) analysis of media freedom and natural resource wealth. The findings indicate that the published results do not hold once I include the more robust measure.

Next, I focus on the institutional determinants of government perpetrated attacks against media. The ability for media to produce news content without government interference remains an important cornerstone of a healthy democracy. However, the influence of institutions on the government's decision to censor, jail, harass, or perpetrate other attacks against media remains understudied. In this analysis, I argue that countries' judicial independence in low and moderate levels of electoral democracy reduces government attacks against media. Using panel analysis on 170 countries worldwide from 1948 to 2012, I test my hypothesis and find results supporting my theory. I also find cross-national evidence for an untested assumption in the literature that these attacks positively associate with greater media self-censorship. Taken together, the results point to a ceiling

effect of judicial independence's protection of journalists in media systems as countries move toward greater democracy. The study takes a more nuanced approach to studying democracy by recognizing countries may possess or develop different democratic components like electoral democracy and judicial independence at different levels. Finally, I turn to the institutional conditions that determine journalist killings. Previous research argues (counterintuitively) that journalist killings are more likely to occur in democracies rather than non-democracies. While these findings provide an important first step in exploring regime type's effect on journalist killings, the study assumes no variation in how long countries have remained a regime type. In this study, I argue that as regime types endure in a country, the likelihood of seeing journalists killed there will decrease. Using regression and survival analysis on a sample of journalists killed for their professional work in countries worldwide from 1992 to 2014, I find evidence that as regime type endures, journalist killings decrease, on average. When I stratify the sample by regime type, I find this effect holds in autocracies, anocracies, but not democracies. Further, the findings show a null result regarding the effect of democracy level on journalist killings once I account for regime-type durability, though in the stratified samples I find higher democracy levels negatively associate with journalist killings in democracies. The results provide a broader picture of the working dynamics between regime type and journalists' safety in a country's media system.

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Chapter 1 - Introduction

Article 19 of the United Nation's Universal Declaration of Human Rights acknowledges press freedom as a fundamental human right, yet in 2016 Freedom House estimated that about 45% of the world's population — approximately 3.4 billion people — lived under a media system that the watchdog group considered not free. Only about 13% of the world's population lived in a country with a media considered fully free that same year. Freedom House's 2016 annual report of *Press Freedom in the World* further notes that, "Press freedom worldwide deteriorated to its lowest point in 13 years in 2016, driven by unprecedented threats to journalists and media outlets in major democracies, [as well as] intensified crackdowns on independent media in authoritarian settings" (Dunham 2017, 3). Among the causes, the report cites government censorship and crackdowns on independent media including examples in Egypt where authorities restricted journalists in part through gag orders and censorship practices that suppressed criticism of President Abdel Fattah al-Sisi, as well as in Malaysia with the closure of *The Malaysian Insider* after persistent government harassment.[1]

This dissertation seeks to address these issues by focusing on the institutional factors that affect threats to media personnel's ability to function without undue outside influence in a media system. These threats include censorship, harassment, as well as the risk to journalists' physical integrity. I focus my research on two main questions. First, under which institutional conditions are governments most likely to attack media? Second, under which institutional conditions are journalists' physical integrity most threatened? In a related but different objective, I also attempt to provide a better measurement for freedom of the media systems in which journalists work.

The media's ability to publish, broadcast, and disseminate news content in a free and open media system remains an important part of democracy (Lawson 2002, Norris 2006, McQuail 2010). While democracy's minimalist definition excludes media's ability to function without undue influence (Alvarez et al. 1996, Przeworski et al. 2000,

[1] *BBC* "Blocked Malaysian Insider News Website Shuts Down" http://www.bbc.com/news/world-asia-35800396

Cheibub et al. 2010), it serves as an important component in the broader definition which includes concepts of human rights and civil liberties (Dahl 1973; 1989). Media freedom's inclusion in the latter perspective suggests that mass democracy would not be possible without the "free exchange and flow of information" the news affords to citizens for decision making during elections (Bimber 2002, 11-12). Manipulation of the channel between the press and voters leaves citizens unable to fully participate in the democratic process. Press freedom therefore remains important for our overall understanding of democracy.[2]

The media's often cited watch dog role serves a significant function in democracy as well (Graber 1986, Donohue et al. 1995, Beasley and Burgess 2001, Whitten-Woodring 2009). According to this perspective, the press holds governments accountable by reporting corruption, abuses of power, and other misconduct perpetrated by public officials. In this capacity, a free media promotes good government and combats human rights and civil liberties violations, providing an informal check on government. If a country's media cannot freely disseminate information or perform this watchdog role, then its attainment of democracy in this broader conception fails.

On the other hand, non-democratic leaders can see a free media as a threat to retaining power (Bueno de Mesquita and Downs 2005). Media for instance can serve as a *coordination good* — "public goods that critically affect the ability of political opponents to coordinate but that have relatively little impact on economic growth" (Bueno de Mesquita and Downs 2005, 82). A free and open media can help solve coordination problems among opposition and allow anti-government, opposition views to enter the public sphere. The authors argue that suppressing coordination goods like a free media and reducing opposition movements' ability to coordinate allows authoritarian regimes to endure.[3] In China for example, officials in the long-time authoritarian regime allow some government criticism on social media but typically censor content regarding collective expression (King et al. 2013).

[2] I use media and the press interchangeably throughout.

[3] In addition to a free media, the authors cite political rights, more general human rights, and accessible higher education as other coordination goods.

The empirical media freedom literature connects it to different political, institutional, and economic environments (Stier 2017). Previous research links media freedom to different institutions including positive associations with higher levels of democracy (Norris 2006, Whitten-Woodring and Van Belle 2017) and greater checks and balances on presidents in Latin America (Kellam and Stein 2016). Regarding authoritarian regimes specifically, scholars find that they allow freer presses to improve bureaucratic efficiency in resource-poor regimes (Egorov et al. 2009) or to monitor corruption of local level politicians (Lorentzen 2014). Stier (2015) finds media freedom varies in authoritarian regimes, with electoral authoritarian regimes having the most freedom and communist regimes with the least. Furthermore, studies point to media freedom as a mechanism for the democratic peace (Van Belle 1997; 2000).

The economics literature explores how economic environments shape media freedom. A consensus suggests greater private media ownership has a positive affect on media freedom (Djankov et al. 2003, Besley and Prat 2006, Gentzkow and Shapiro 2008, Petrova 2011). Scholars have also studied the relationship between corruption and media freedom, finding media freedom reduces it (Ahrend 2002, Brunetti and Weder 2003, Kalenborn and Lessman 2013) and that executive-level government corruption positively associates with increased government censorship efforts of traditional and new media (Solis and Antenangeli 2017). Studies have also shown media freedom positively associates with foreign direct investment (Dutta and Roy 2009) and foreign aid (Dutta and Williamson 2016).

Stanig's (2015) subnational analysis of media self-censorship in Mexican states falls perhaps at these two literatures' intersection. He finds the number of reports the press publishes on government corruption decreases in states with higher punitive jail time for libel. Another literature explores media freedom's behavioral impact on citizens' participation in the democratic process. The key findings indicate that lower press freedom levels strongly correlate with lower levels of political participation and voter turnout (Lesson 2008) and less political knowledge (Lesson 2008, Schoonvelde 2014).

The attacks against media literature finds an increase in government harassment

against media in democratic and hybrid regimes during key political events like coup attempts, major protests, proposed constitutional reforms, and presidential elections in Sub-Saharan Africa[4,5] (VonDoepp and Young 2013).

In sum, a large literature exists that examines both what media freedom affects and what is affected by it. My dissertation specifically addresses this literature in two ways. First, I propose a new measurement of media freedom using latent variable analysis. My measurement conceptualizes the freedom of a country's media system along two key dimensions directly drawn from theoretical discussions in the literature: (1) the ability for media outlets to produce content without undue influence, and (2) citizen's access to that content, specifically content critical of society's powerful figures. The measurement synthesizes different extant media freedom or media freedom-related indicators in a statistically principled way and allows empirical researchers to make better inferences from empirical studies analyzing the general freedom that media systems possess.

After measuring media freedom, I turn my focus to a narrower issue in media systems where a puzzle emerges in the literature. Theoretical concepts of democracy often include freedom of the press as an essential element (Dahl 2005). The onerous task required for citizens to gather information pertaining to their government, policy outcomes, and other related concerns makes them reliant on media to provide this content. Empirical research also finds media freedom positively associates with greater democracy levels (Whitten-Woodring and Van Belle 2017). However, studies that examine the relationship between democracy levels and physical risk to journalists paint a wholly different picture. Previous findings indicate that as democracy levels increase, the probability of a country seeing journalist killings increases (Asal et al. 2018). The government perpetrated attacks against media literature also finds that as democracy level increases, the number of government perpetrated attacks against media also increases in hybrid and new democracies in Sub-Saharan Africa (VonDoepp and Young 2013).

[4]Harassments like jailing journalists, closing critical media outlets, and expelling foreign journalists (among others).

[5]The authors actually find mixed evidence of presidential elections as a significant factor.

These two literatures analyze different aspects of the same media systems and find that democracy has the simultaneous effect of both increasing the freedom and openness of media systems while paradoxically decreasing safety for the journalists operating in them. Asal et al. (2018) argue the openness that democracies afford media outlets provides journalists more opportunity to investigate sensitive topics, including crime, corruption, or officials' abuse of public office. The authors argue this heightens journalists' risk as they work to produce news content. For the remainder of the dissertation, I address this *press-safety paradox*. In two different studies, I examine how judicial independence, a component of democracy, actually reduces government attacks against media, then I explore how regime type duration and democratic consolidation do the same for journalist killings. Below I provide a more detailed introduction to these chapters.

Dissertation Outline

In chapter 2, I perform an item response theory (IRT) analysis of 12 existing media freedom measures. The IRT analysis allows me to (1) treat media freedom as an unobserved, latent variable and evaluate which extant measures best capture that latent concept and, (2) generate cross-section, time-series, continuous data for media freedom. Regarding measurement, the model generates media freedom data using a Bayesian approach for 196 countries worldwide from 1960 to 2016. It creates a posterior distribution — a point estimate and standard deviation — for each country-year which allows researchers to incorporate raters' reliability among the different indicators in empirical analysis of media freedom. Next, the results indicate that variables from the Varieties of Democracy (V-Dem) dataset capture the latent media freedom concept better than the more commonly used Freedom House and Reporters Without Borders indices.

I then apply the data in a replication study using Monte Carlo simulations to published results in Egorov et al.'s (2009) paper on media freedom and natural resources in autocracies. The authors argue that resource-poor dictatorships allow freer media systems in order to better monitor their bureaucracies. More specifically, they argue

that dictators make this calculation based on the value of their oil reserves (and *not* oil production value). I find that the authors' main results do not hold once I apply my new media system freedom (MSF) in place of the Freedom House *Press Freedom in the World* dataset that measure a country's media freedom in their original sample examining the years 1993 to 2008. Using the MSF scores increased coverage, I then extend the sample to include years from 1980 to 2016 and find evidence that contradicts their theory — increases in the value of oil reserves are associated with media system freedom increases in dictatorships.

The third and fourth chapters address the *press-safety paradox*. In chapter three, I investigate the institutional determinants of government attacks against media. I focus specifically on the relationship between government attacks against media and judicial independence *conditional* on a country's electoral democracy level. I argue that countries possessing greater judicial independence with low to moderately high levels of electoral democracy see a decrease in government censorship efforts and media harassment. This harassment includes government directed jailing of journalists, closing media outlets, and politically motivated rewarding of government advertising funds (among other similar attacks). Using panel analysis on 170 countries worldwide from 1948 to 2012, I test my hypothesis and find results supporting my theory. Surprisingly, the findings also indicate that judicial independence might actually increase attacks against media in countries with the very highest electoral democracy levels. Taken together, the results point to a ceiling effect of judicial independence's protection of journalists in media systems as countries democratize. Judicial independence only seems to reduce government attacks against media in countries with low to moderate levels of electoral democracy.

Also in chapter three, I analyze an implicit assumption present in the government perpetrated attacks against media literature that these attacks encourage self-censorship among journalists. Using new data from the Varieties of Democracy (V-Dem) dataset on journalist self-censorship, I find a positive association with attacks against traditional media and self-censorship among journalists in traditional media like newspapers, tele-

vision, and radio, as well as online censorship. In addition, I find the same positive association between self-censorship and media harassment.

The fourth chapter addresses the institutional determinants of journalist killings. Previous research argues (counterintuitively) that these types of killings are more likely to occur in democracies than non-democracies. However, I argue that the number of journalists killed increases initially as countries transition to new regime types, but then decreases as media adapt to new regime type conditions. My theory examines the transitions into different regime types, and argues that this dynamic should hold for autocracies and anocracies, but not democracies. Further more, I argue that democratic consolidation should decrease journalist killings in democracies but not other regime types.

Using an original sample of journalists killed for their professional work in countries worldwide from 1992 to 2014, I evaluate my theory using negative binomial regression analysis. My results indicate a positive association between regime-type duration and journalist killings. Survival analysis on the same sample produces similar results. When I stratify the sample by regime type, I find the results hold in autocracies and anocracies, but not democracies in accordance to my expectations. Also as expected, democracy level negatively associates with journalist killings in the sample of democracies. This indicates that unlike autocracies and anocracies, journalist killings do not decrease as a result of regime type duration, but by democratic consolidation. Finally, unlike previous studies, the findings indicate that democracy level does not have a positive association with journalists killings once I include regime-type duration to the analysis.

Chapter 2 - Measuring Media Freedom

While media–government relations carry serious implications for the quality of democracy (Schedler 2002), governments do not solely influence news outlets' freedom to air and publish information. Non-state actors including the owners of media (Djankov et al. 2003) and criminal or terrorist organizations can potentially influence a media system's news content. Hervieu (2013) for instance describes the Brazilian media market consolidated into the hands of about 10 leading business groups, all under the sway of influential, powerful political and industrial leaders known in the country as *colonels*. While drug trafficking organizations in Mexico and Paraguay represent media's primary threat.[6] Under certain conditions, journalists may also opt to self-censor absent of direct physical or economic coercion (Stanig 2015).

These differing dimensions suggest that raters creating data to measure media freedom remain largely unable to readily observe it as a whole like other concepts in which political scientists have an interest. Different indices rate media freedom using varied criteria that emphasize specific aspects of media freedom over others, reflecting the concept's inherent complexity (Schneider 2014). Many indicators assume media freedom can be measured in a single indicator, while other evaluate smaller components of a country's media system related to press freedom. In addition, some indices have changed their conceptualization of what constitutes media freedom over time. These shifts reflect either changing technology in a country' media landscape or an evolving approach that data managers take in conceptualizing and then executing their measurements. Widely used indices like Freedom House's *Freedom of the Press* and Reporters without Borders' *Press Freedom Index* for example have changed their coding procedure to measuring media freedom many times since they began producing data.

These discrepancies arise due to media freedom's inherently unobservable nature. Existing media freedom indices may therefore only measure certain aspects of a media system's freedom while ignoring others. A new variable that synthesizes the informa-

[6] *Committee to Protect Journalists* "Attacks on the Press: Journalists caught between terrorists and governments," April 27, 2015. Available at:https://cpj.org/2015/04/attacks-on-the-press-journalists-caught-between-te.php.

tion from extant measurements as well as accounts for their changing criteria requires a new measurement approach to infer media freedom as a latent variable.

In this chapter, I use an item response theory (IRT) model to measure media freedom as a latent variable. The IRT model has two advantages. First, it generates time-series, cross-sectional media freedom data based on existing indicators that rate either a country's overall media system freedom or some aspect of it. The data include a distribution around a point estimate for each country-year to allow researchers to incorporate different raters' reliability of media freedom into empirical models. Second, the approach allows me to compare current media freedom variables. Results produce a discrimination parameter for each indicator that shows which indicators exert the most influence on the latent variable.

To this end, I treat each press freedom indicator as a rater's best attempt to measure media freedom, but I assume no measure captures the concept with complete accuracy. The indicators in this analysis include evaluations of a country's general media system freedom that consider the economic environment, the influence of non-state actors, harassment against journalists, government censorship, as well as variables for media self-censorship, bias, and corruption. I also include measures of media's ability to criticize government, the presence of a variety of perspectives in a country's major media outlets, and citizen's access to critical media.

To demonstrate the approach's usefulness, I replicate the main results from Egorov et al.'s (2009) paper on media freedom and natural resources in autocracies with the newly generated Media System Freedom (MSF) data from the IRT model. The authors use the Freedom House *Freedom of the Press in the World* to measure general media freedom, and I replace their variable choice with my new latent measure. I find that the authors' main results do not hold once I apply the MSF scores to their original sample. The findings provide a caution for future scholars who wish to empirically model media freedom or analyze factors that affect it. By incorporating a more robust media system freedom measure in empirical models, researchers studying media freedom can draw better inferences from their results and have greater certainty that findings are not the

consequence of rater's error or variable choice.

I now turn to summarizing media freedom's conceptualization in the literature. Next, I briefly discuss each indicator I use in the analysis, then describe the IRT model and my estimation approach. Finally, I proceed to assessing the results before presenting the replication.

Media Freedom and Its Indicators

Conceptualizing media freedom remains contentious (Becker et al. 2007). In the mass communication literature, scholars define the concept in different ways. While they take different approaches, common elements do emerge. These include the absence of government controls and restrictions (Siebert et al. 1956, Weaver 1977, Price 2002), ownership diffusion (Lowenstein 1970, Rozumilowicz 2002), and citizens' ability to access media (Picard 1985, Weaver 1977, McQuail 2010). Other features include the absence of undue influence from non-governmental actors (Weaver 1977), self-regulation (that is, self-censorship) (Lowenstein 1970), and legal controls like libel and defamation laws (Picard 1985).

Current media freedom indicators do not capture all of the features that scholars identify as important to a media system's freedom (Becker et al. 2007). An item response theory model (IRT) allows me to produce a more robust measurement that includes indicators evaluating a country's media system in different ways. It also captures changing standards that indicators use to measure this latent concept. To employ this model, I begin by selecting theoretically-driven variables that either attempt to measure the overall freedom of a country's media system or some aspect of it. I only include indicators that attempt worldwide coverage.[7] Guided by scholars' conceptualization, I synthesize indicators using the IRT model that broadly reflect media system freedom including 1) media's ability to operate independently from undue outside influence, and 2) citizens' ability to access diverse media. In this section I provide a brief summary

[7]This criteria excludes the International Research and Exchanges Board's (IREX) Media Sustainability Index, which offers sporadic time coverage in only certain regions. See https://www.irex.org/resource/media-sustainability-index-msi for more information.

of the media freedom indicators I use in the analysis. I describe what each indicator attempts to measure and the time period it covers. Appendix A provides a more detailed overview of these measures.

Freedom House: Freedom of the Press

Freedom House's Freedom of the Press (Freedom House 2017) index remains one of the most widely used datasets for measuring press freedom in political science and economics (Brunetti and Weder 2003, Egorov et al. 2009, Schoonvelde 2014, Kellam and Stein 2016). The non-governmental organization began issuing reports in 1980, covering the previous year 1979, and continues to the present. However, the index's managers have changed the dataset's methodology numerous times since its inception. From 1980 until 1988, Freedom House provided separate rankings for a country's print and broadcast sectors, ranking each either *free*, *partially free*, or *not free*. Freedom House provides no aggregate ranking of the country's media system during this period. From 1989 until the present, Freedom House assigns countries a sector aggregated, countrywide *free*, *partially free*, or *not free* ranking. Freedom House does not make the index's survey methodology or criteria readily available from 1980 to 1993.

From 1994 until the present, Freedom House introduced a continuous score ranging from 0 to 100 to accompany each country's categorical ranking, with 0 being the most free and 100 being the least. Initially under the 100-point scale, the index evaluated media freedom based on four criteria: 1) law, 2) political pressure, 3) economic influence, and 4) repressive actions. The index managers considered both print and broadcast sectors separately and then assigned an overall freedom score. From 1997 until 2001, they used the same basic structure but modified the point distribution. In 2002 Freedom House introduced a new coding scheme that it still employs today. The newest methodology evaluates three different areas of each country's media system: the legal, political, and economic environments.

Global Media Freedom

The Global Media Freedom (Whitten-Woodring and Van Belle 2017) dataset orients its data collection methodology by first defining media freedom as "an environment in which journalists are able to safely criticize political and economic elites at both the national and local levels." (Whitten-Woodring and Van Belle 2017, 180). Whitten-Woodring and Van Belle (2017) then code each country-year on a categorical scale from 1 to 3, with higher values representing less media freedom. The authors provide media freedom scores from 1948 to 2014

Reporters Without Borders: Press Freedom Index

The French-based watchdog group Reporters Without Borders (RSF) has released its Press Freedom Index since 2002 (Reporters Without Borders 2017). The index provides press freedom scores and country rankings based on surveys from journalists, scholars, and human rights activists.[8] RSF focuses heavily on harassment against media, attacks against journalists, and self-censorship, though they consider other criteria such as economic and legal conditions. Generally, scores range from 0 to 100, with 0 representing perfect press freedom and 100 indicating the least perfect, though some years inexplicably possess negative scores or scores above 100.

Varieties of Democracy Indicators

The Varieties of Democracy (V-Dem) dataset provides original data for a number of indicators often associated with democracy (Coppedge et al. 2017a). In their *Media* section, they offer a number of variables that evaluate different aspects of media quality and media freedom. V-Dem generates scores by asking country experts to rank each country according to a specified variable concept. After receiving the responses, the V-Dem researchers run the results through an item response theory (IRT) model to compile a cross-coder aggregated score (Pemstein et al. 2017). Below I outline the V-Dem variables I use in this analysis: government censorship of traditional media

[8]Most often used as a robustness check. See Freille et al. (2007), Egorov et al. (2009), and Stier (2015).

(the press, television, and radio), government internet censorship, presence of a critical media, presence of various perspectives in media, harassment of journalists, prevalence of self-censorship, media bias, media corruption, and access to media critical of the government.

Government Censorship Efforts of Traditional Media *Government censorship* measures government censorship efforts against traditional media outlets like the press, television, and radio. The authors clarify that this includes indirect means of censorship such as politically motivated financial and official support of friendly media outlets as well as other restrictions including a high barrier to receive a broadcasting license or taxes. The ordinal variable codes each country-year from 0 to 4, with higher values representing less censorship. V-Dem provides this data from 1900 to 2016.

Internet Censorship Efforts *Internet censorship* measures the prevalence of government internet censorship. The authors clarify that they focus specifically on politically motivated censorship and not child pornography, highly classified military secrets, or defamatory speech toward religion or individuals unless governments use it as a pretext for politically motivated censorship. The ordinal variable codes each country-year from 0 to 3, with higher values representing less internet censorship. V-Dem provides this data from 1993 to 2016.

Critical Print and Broadcast Media *Critical print and broadcast media* measures the degree to which major print and broadcast media outlets criticize the government. The ordinal variable codes each country-year from 0 to 3, with higher rankings representing higher criticism levels by media. V-Dem provides this data from 1900 to 2016.

Print and Broadcast Media Perspectives *Print and broadcast media perspectives* measures the degree to which major media outlets report a wide range of different perspectives. The ordinal variable codes each country-year from 0 to 3, with the lowest score representing an environment where media only report the government's perspec-

tive and the highest score indicating environments where the media represents all of society's important perspectives. V-Dem provides this data from 1900 to 2016.

Harassment of Journalists *Harassment of journalists* measures the degree to which journalists face harassment from governments and powerful non-governmental organizations. The ordinal variable codes each country-year from 0 to 4, with higher rankings representing less harassment. V-Dem provides this data from 1900 to 2016.

Media Self-censorship *Media self-censorship* measures self-censorship's prevalence among journalists in a country. The ordinal variable codes each country-year from 0 to 3, with the higher categories representing less self-censorship among journalists. V-Dem provides this data from 1900 to 2016.

Media Bias *Media bias* measures the degree to which media as a whole carries overt biases against opposition parties or candidates. The ordinal variable codes each country-year from 0 to 4, with higher categories corresponding to less bias. V-Dem provides this data from 1900 to 2016.

Media Corruption *Media corruption* measures the level of corruption among journalists and media personnel in a country's media system. V-Dem considers journalists, publishers, and broadcasters corrupt if they take payments in exchange for altering news content. The ordinal variable codes each country-year from 0 to 4, with higher scores corresponding to less media corruption. V-Dem provides this data from 1900 to 2016.

Access to Critical Media *Access to critical media* measures the percentage of the population that has access to traditional media (print, radio, or television) that sometimes criticize the national government. Higher percentages equate to greater access to critical media. V-Dem provides this data from 1900 to 2015.[9]

[9] I draw this data from V-Dem Datatset v6.2 (Coppedge et al. 2016), as V-Dem Datatset v7.1 does not have it.

An Item Response Theory (IRT) Model for Media Freedom

Item response theory (IRT) models are part of a broad class of latent trait analysis (LTA) models which aim to measure an underlying ability (or trait). Scholars interested in educational testing initially developed the model (Rasch 1960, Samejima 1968, 1972, Lord 1980, Bock and Aitken 1981), and political scientists have utilized them to evaluate or measure latent variables like democracy (Trier and Jackman 2008, Pemstein et al. 2010), judicial independence (Linzer and Staton 2015), and U.S. state Supreme Court justice's ideology (Windett et al. 2015). For this analysis, I employ Linzer and Staton's (2015) dynamic, graded response IRT model.[10]

Model Specification

Each of the 12 indicators above represent a judge's best attempt to evaluate a country's overall media system freedom or some aspect of it. In order to take advantage of these different indicators to uncover the latent variable, I use a dynamic, graded response, item response theory (IRT) model that estimates a bounded, unidimensional latent variable measurement of media freedom. I describe the model's specifications below.

I let the latent measure x_{kt} vary across k countries, $k = 1,...,n$ and t years, $t = 1,...,n$. Though the latent variable cannot be observed, I assume a series of r observed variables y_r makes a reasonable attempt to capture this with manifest measurement. These measurements use the same unit of analysis as the latent measure x_{kt}. I further assume that while each measure y_{kt} does not perfectly measure media freedom, taken together they produce more reliable estimates for the latent variable x_{kt}.

Having made this assumption, I specify a bounded, graded response IRT model that links the latent x_{kt} to the manifest y_{rkt}. However, media freedom indicators are not created equal. The model aims to uncover how well each y_{rkt} reveals information about the latent variable. Items that better reveal information about the latent measure *discrim-*

[10] Linzer and Staton (2015) demonstrate the model's usefulness and efficiency in estimating judicial independence and democracy as latent variables.

inate better than others. Each item in y_{rkt} estimates an item discrimination parameter, coefficient β_r, which captures the reliability or *discrimination parameter* of indicator y_r.

Think of discrimination as a measure of how reliable an item reveals the latent concept. To use a general example, if a secondary school teacher wanted to test students' United States history knowledge (itself a latent concept), a true or false question asking if President George Washington was a male represents an overly simple question that poorly discriminates a student's US history knowledge. Indeed, most students can identify the historical figure as male based on the name alone. On the other hand, a question asking the name of President Herbert Hoover's Belgian shepherd dog presents a needlessly difficult question about a largely inconsequential fact.[11] While the vast majority of students would likely get the first one correct and the latter one wrong, both poorly discriminate (that is, reveal) a student's knowledge of U.S. history. The model seeks to find items, categories in this case, that discriminate the latent concept well.

As the discrimination parameter for each y_r, β_r, increases, a closer relationship between the latent and manifest measures is revealed, while estimates closer to zero contain more white noise indicating a greater error variance in that item. For the model, I set the identification restriction that $\beta_r \geq 0$ for ease of interpreting β_r's discrimination capabilities.

To complete the function, I denote the total number of outcome categories for the rth manifest variables y_r as M_r. In addition, τ_{rm} divides adjacent ratings on the latent scale, subject to the constraint $\tau_{rm} > \tau_{r(m-1)}$. I provide the link function as follows:

$$P(y_{rkt} = m) = logit^{-1}\beta_r(\tau_{rm} - x_{kt}) - logit^{-1}\beta_r(\tau_{r(m-1)} - x_{kt}) \quad (1)$$

The logit function above predicts not only the discrimination parameter, but estimates the threshold for moving from one category to the next in each y_{kt}. For example, it estimates two thresholds for a variable with three categories. The essential output of

[11] His name was King Tut, by the way.

this model aligns the estimated threshold levels τ_{rm} with observed ratings across y_{kt} and the distance between each threshold y_{rkt} helps specify which category distinctions are more or less substantively meaningful relative to the other y_r.

Unfortunately, the only observed values in equation (1) y_{rkt} appear on the left-hand side. However, using a fully Bayesian approach, I am able to estimate τ_{rm} and x_{kt}. I first place a bound on x between 0 and 1 in order to easily interpret the results. The lower bound zero means x has *none* of the latent characteristics and the upper bound 1 means x has *all* of the characteristics. For this analysis, a 0 represents the lowest media freedom level, while increasing toward 1 represents greater freedom. Within each country k, I assume x in year t has a normal prior distribution that is centered at the latent variable's previous value in year $t - 1$. I represent this below:

$$x_{kt} \sim N(x_{k(t-1)}, \sigma_k^2)I(0,1) \tag{2}$$

The notation $I(0,1)$ indicates the bounded upper unit 1 and the bounded lower unit 0. The model assumes a bounded by zero and one, noninformative normal prior. Separate variance parameters σ_k^2 estimated for each country capture x_{kr}'s temporal variation. I assigned uniform priors on the unit interval to the standard deviation σ of each country, which helps ensure estimates will not be too flat once I fit the model.

The Bayesian model's treatment of missing values provides another advantage to this approach. Countries with more manifest data points provide more information to x_{rt} based on threshold coefficient estimations, while countries with less manifest variables inversely provide less. I only keep country-years with at least 1 observation. This decision ensures no observations where every y_{rkt} is missing. However, the dataset still contains one class of missing variables, which the Bayesian approach handles. When many but not all y_{rkt} are missing, the estimation comes from the posterior estimate of x_{kt}'s posterior based on its prior distribution. Placing prior distributions over β_r and τ_{rm} finalizes the model. I assign each threshold random vague normal points. From here, the iteration process finds estimates for each τrm.

Applying the Model

To apply the model, I use the 12 indicators I described above to estimate a latent variable measure of media freedom for the years 1960 to 2016. Each indicator represents a rater's best guess at measuring media freedom or some aspect of it, though none successfully capture it. For each indicator, I use ordinal measures required by the dynamic, graded response IRT model. For the two strictly continuous measures (*Reporters without Borders* and *Access to Critical Media*), I convert them to ordinal data in different ways. The *Reporters without Borders* score ranges from -14 to 144, and I convert it to five categories, dividing the data by 20th percentiles. I then code the categories so that higher numbered categories indicate greater media freedom. For *Access to Critical Media*, I calculate its mean and standard deviation then code categories based on increments of the standard deviation, which creates four categories for each variable.[12] For Freedom House's data before 1988, I take the average of the print and broadcast sectors to generate a country aggregated ranking. I score a country-year partly free (2) for any average greater than one or less than three. Finally, I code all variables so that higher values indicate greater media freedom and drop all observations when the country was not independent.[13]

Table 1 lists the attributes of each variable as they pertain to the item response theory (IRT) model. I apply the model to 196 countries worldwide from 1960 to 2016.[14] The model also includes countries that no longer exist like South Vietnam, South Yemen, and East Germany. Given these parameters, the dataset has a total of 9,495 observ-

[12] For instance, the mean and standard deviation for *Access to Critical Media* are 52.39 and 27.44 respectively. Starting at the mean, I move out one standard deviation toward 0 and toward 100. From here, I create the four categories using the following coding rules: category 1 = $<$ 25.05, category 2 = between 25.05 and 52.39, category 3 = between 52.4 and 79.8, and category 4 = $>$ 79.81.

[13] Varieties of Democracy provides a number of country-year observations where the country lacked sovereignty in some way. For example, Angola gained independence from Portugal in 1975 but V-Dem ranks the different media variables for prior years. Military occupation represents another example. To ensure that I measure only sovereign, independent countries, I dropped country-years where a foreign power exerted influence or when a country did not have full sovereignty. V-Dem's Country Code Book (V-Dem Country Coding Units v7.1, 2017) provides detailed case histories of each country, and I used it to code which observations met this criteria. For every year V-Dem noted the country had a foreign influence, I dropped that country-year. Appendix A lists each country and the years the Media System Freedom (MSF) data cover.

[14] I removed autonomous or disputed territories, such as Crimea, Hong Kong, the Palestinian Authority, and Somaliland.

able country-year units. Starting in 1979, the data covers most country-years, though the Global Media Freedom (GMF) and eight Varieties of Democracy indicators cover nearly the entire period.[15] Table 1 displays missingness across the 12 indicators.

Table 1: Twelve Media Freedom Indicators and Their Availability

Variable	Measurement Level	Years Available	Percentage Missing	Source
General Media Freedom Indicators				
Global Media Freedom Index	Ordinal; 3 categories	1960-2014	19.6%	Whitten-Woodring and Van Belle (2017)
Freedom House: Freedom of the Press	Ordinal; 3 categories	1979-2016	40.2%	Freedom House (2017)
Press Freedom Index	Ordinal; 5 categories	2002-2016	77.5%	Press Freedom Index (2017)
Varieties of Democracy Indicators				
Press/TV/Radio Censorship	Ordinal; 5 categories	1960-2016	23.9%	Coppedge et al. (2017a)
Internet Censorship	Ordinal; 4 categories	1993-2016	66.7%	Coppedge et al. (2017a)
Critical Media	Ordinal; 4 categories	1960-2016	23.9%	Coppedge et al. (2017a)
Media Perspectives	Ordinal; 4 categories	1960-2016	23.9%	Coppedge et al. (2017a)
Harassment of Journalists	Ordinal; 5 categories	1960-2016	23.9%	Coppedge et al. (2017a)
Self-censorship	Ordinal; 4 categories	1960-2016	23.9%	Coppedge et al. (2017a)
Media Bias	Ordinal; 5 categories	1960-2016	23.9%	Coppedge et al. (2017a)
Media Corruption	Ordinal; 5 categories	1960-2016	23.9%	Coppedge et al. (2017a)
Critical Media Access	Ordinal; 4 categories	1960-2016	29%	Coppedge et al. (2016)

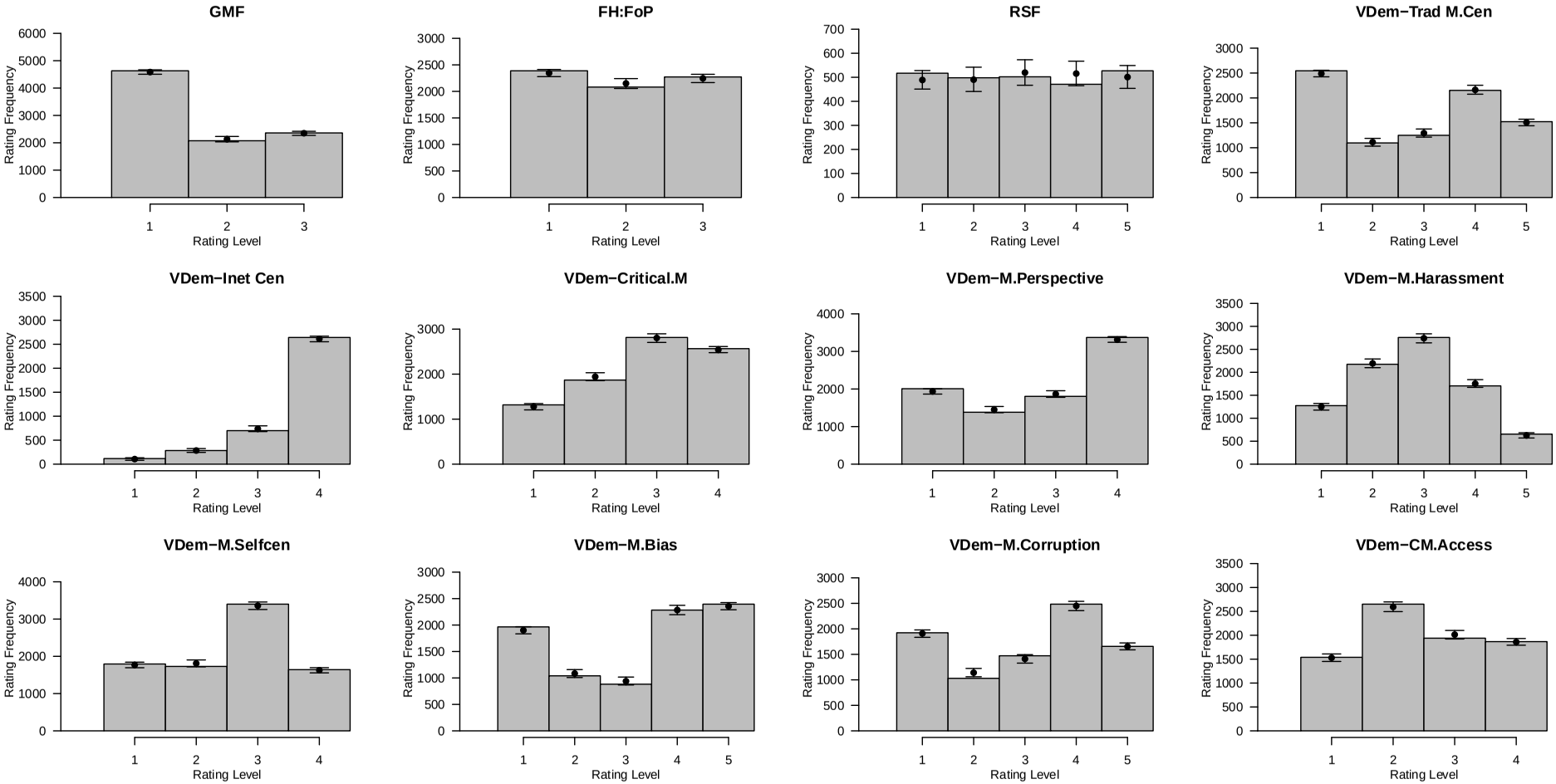
Results

In order to create bounded, media freedom estimates from 0 to 1 in 196 countries worldwide from 1960 to 2016, I estimated a full Bayesian model utilizing a Markov chain Monte Carlo (MCMC) simulation. I run three chains at 2,000 iterations each, with the first 1,000 draws discarded and treated as burn-ins. The results are based on the posterior distribution of parameters x_{kt}, β_r, τ_{rm}, and v_{ark}.

To first assess the model fit, I compare the manifest distribution of each indicator y_r to the predicted distributions based on equation (1) above. Using equation (1) and the simulation procedure, the model resamples values of each parameter estimate from the joint posterior distribution. From this procedure, I can compare the predicted distribution to the actual distribution of y_{rky} to assess how well the model predicts each indicator's manifest distribution. No systematic discrepancy between the observed y_r and the posterior predictive distribution indicator suggests a good model fit. Figure 1 below presents these results. It shows that of the 50 possible ratings, only two fall outside the predicted distribution and its 95 percent confidence level. This outcome indicates that the model accurately predicts 96 percent of the ratings. The results show no evidence to fear the over- or underpredicting of extreme ratings of y_{rkt}.

[15] Missingness can occur when one dataset measures a country-year that the other does not.

Figure 1: Observed Ratings and The Posterior Predictive Distribution, 12 Media Freedom Indicators



Cross-Sectional Results and Temporal Trends

Cross-Sectional Results

The model yields estimates for the latent variable media freedom in 196 countries. The lower bound 0 represents media freedom's absence, while an increasing score bounded at 1 represents greater media freedom. Figures 2 and 3 plot these point estimates along with associated error bars indicating 80 percent posterior credible intervals for the year 2014.[16] The cross-sectional estimates indicate longtime authoritarian regimes like North Korea, Turkmenistan, Uzbekistan, and Cuba and yield the lowest point estimates, while consolidated democracies like Switzerland, Denmark, Austria, and Germany have the highest. The figure shows countries at the top and bottom of the scales have small error bars, while countries in the center possess much wider ones. This indicates the model's certainty at placing countries at the bottom and top, while those in the center possess more noise. Figure 4 presents the results from figures 2 and 3 on a global map. Countries in darker blue mark higher levels of media freedom, while lighter blues indicate countries with less.

[16]The figures do not include MSF data for South Yemen, South Vietnam, and East Germany, as these countries did not exist in 2014. To show examples of these countries' data, I produce the estimates from 1968 in Appendix A.

Figure 2: Media System Freedom (MSF) in 193 countries, 2014: Lower Bounds

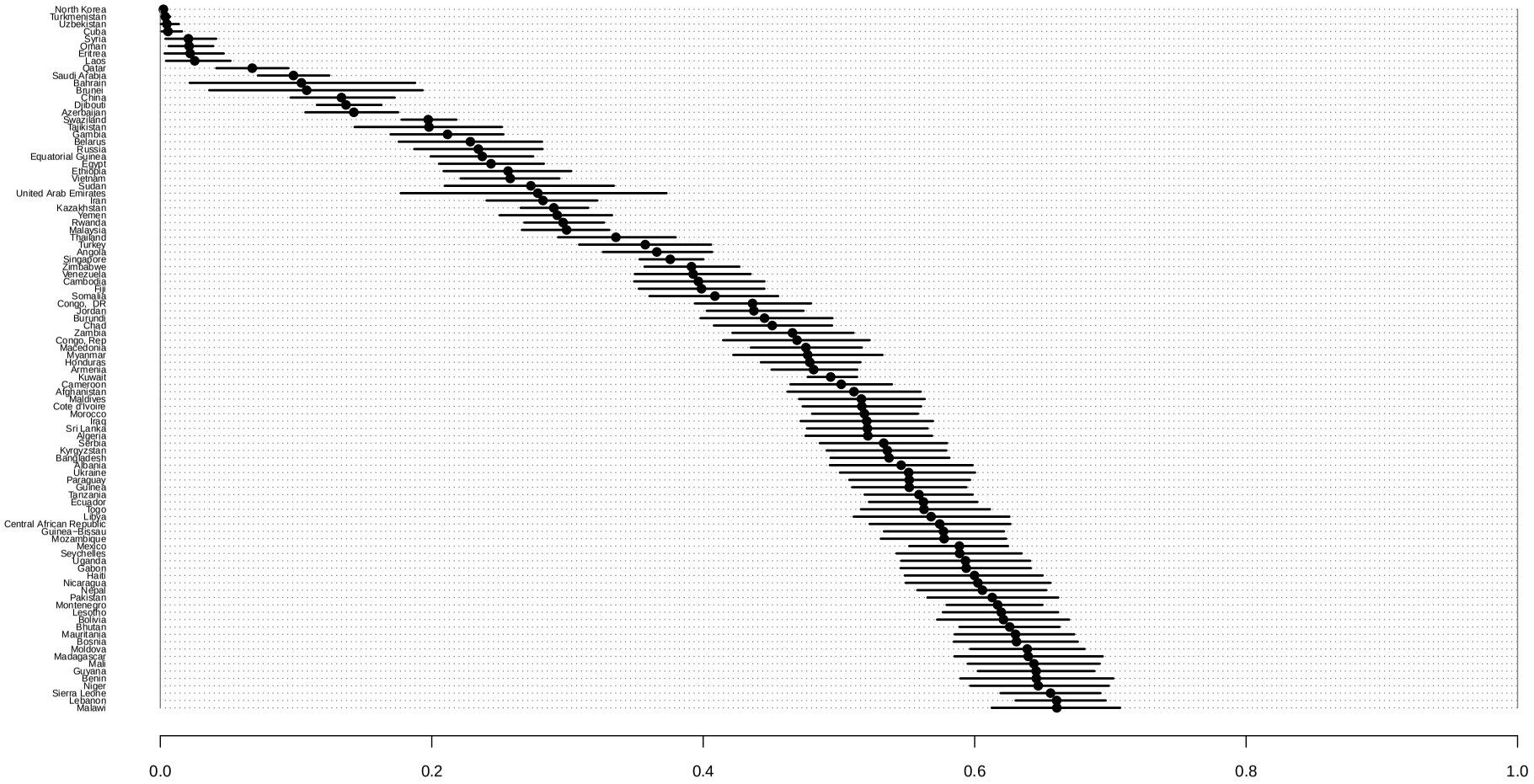


Figure 3: Media System Freedom (MSF) in 193 countries, 2014: Upper Bounds

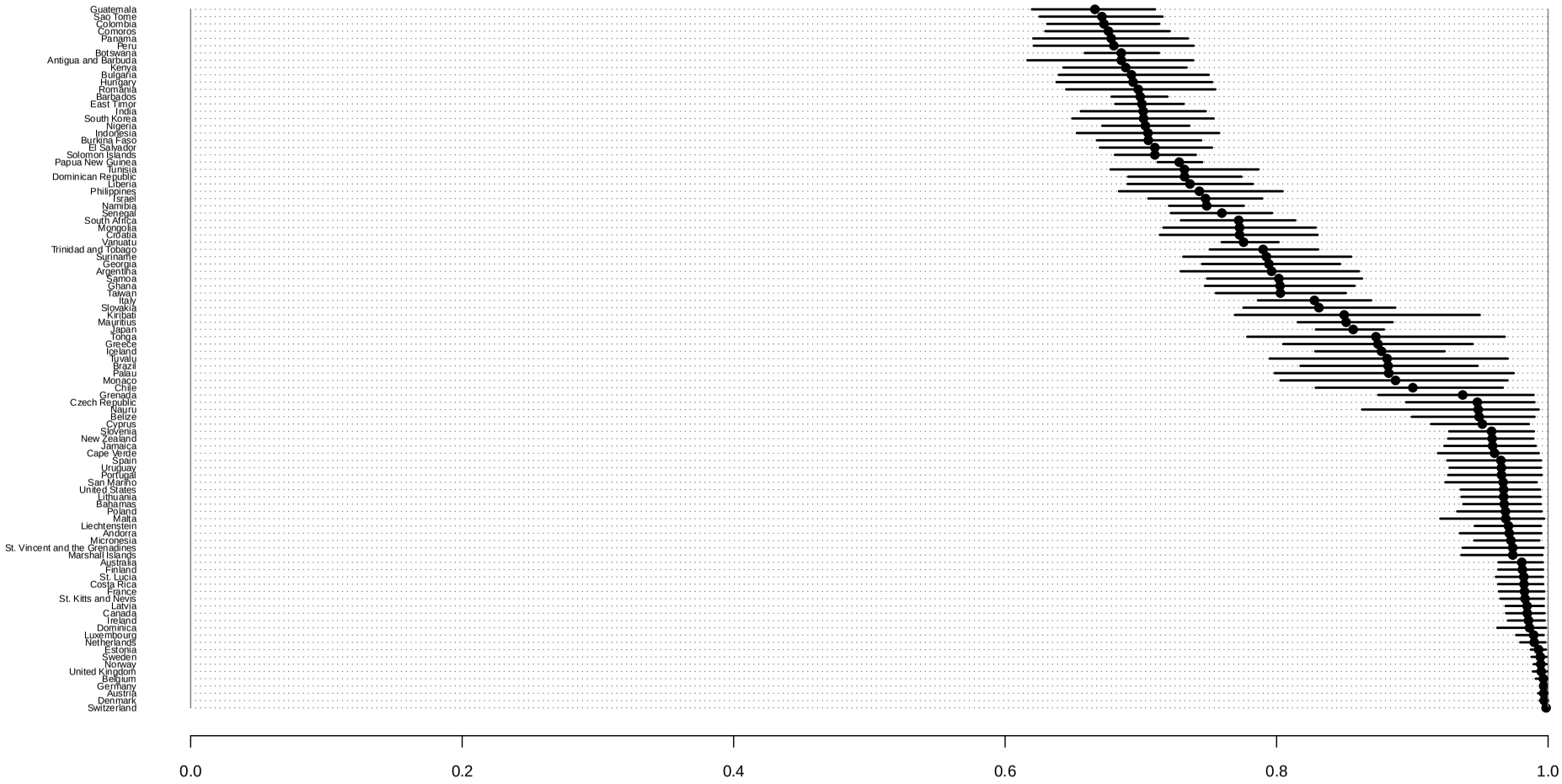
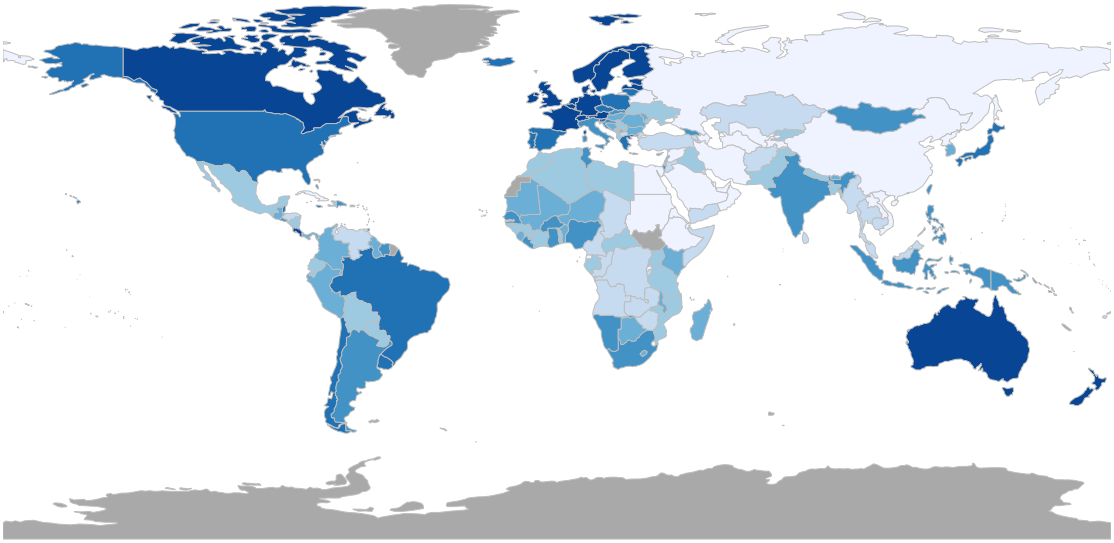


Figure 4: Media System Freedom (MSF) Worldwide, 2014 Point Estimates



Note: Darker blues indicate higher levels of media freedom. Lighter blues indicate less. Gray indicates no available data.

Temporal Trends The results also provide temporal trends in each country-year from 1960 to 2016. Figure 5 below displays trends for Poland, North Korea, the United States, Spain, Nigeria, and Brazil. The figure shows the MSF scores in countries that vary among regime type, regions, and also shows variance both within and between countries. The supplemental index displays figures for all 196 countries.

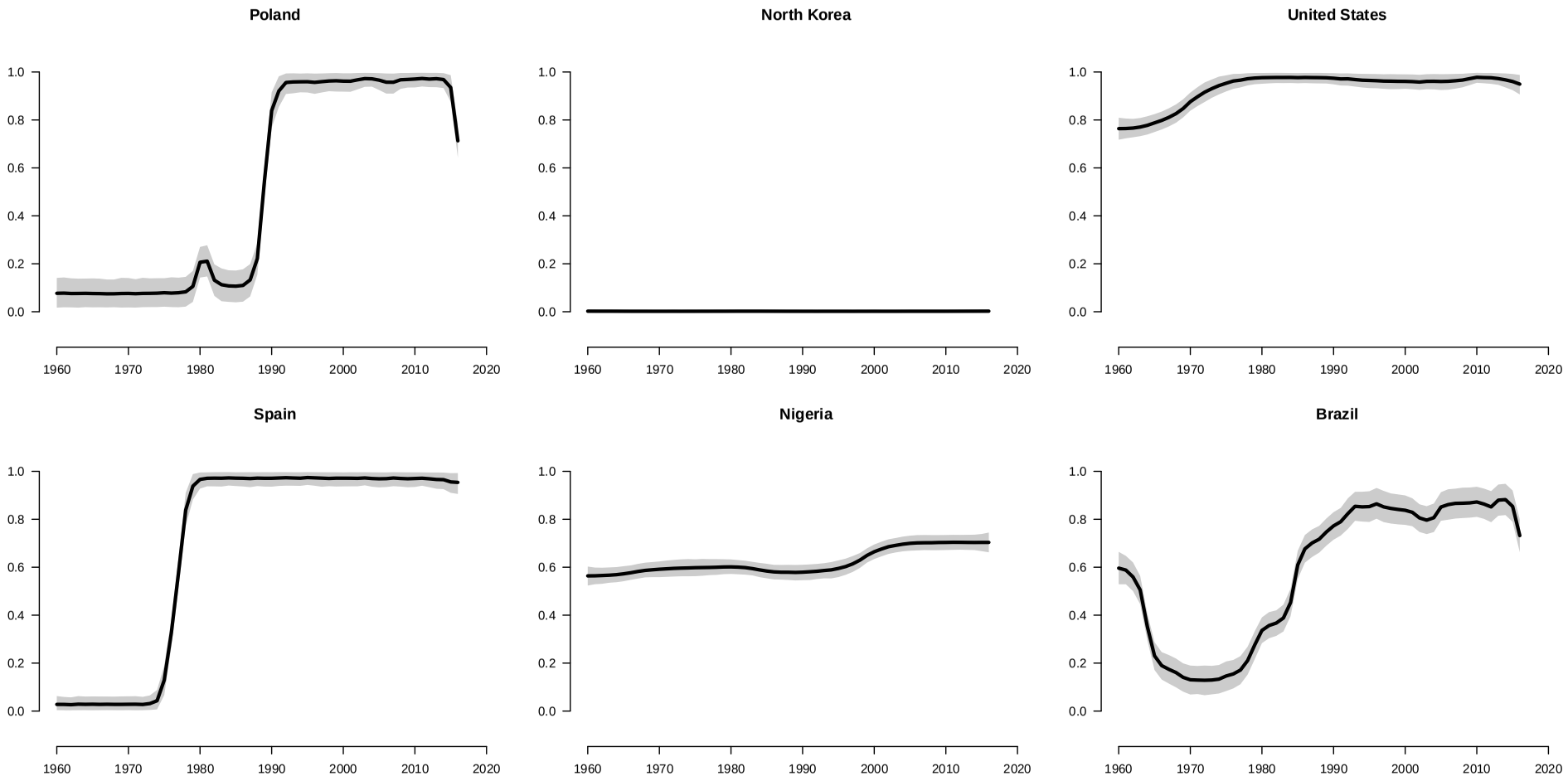
To provide validity to the estimates, I briefly compare the case history of Poland's media system freedom to its MSF estimates. After World War II and a Soviet military occupation, communists with close links to the Soviet Union took power in Poland and imposed harsh censorship laws (Bajomi-Lazar 2014, 104). Poland's media system remained an instrument of the communist regime until a number of protests in the late 1970s and early 1980s (notably including the Solidarity movement) slightly improved Poland's civil liberties for a short time (Jacubowicz 1992). The communist party would impose martial law from 1981 to 1983 in response to the protests. By 1988, the Solidarity movement engaged in dialogue with the now unpopular Polish communist leaders. Change in media freedom followed these events with the communist leadership giving the Solidarity movement certain broadcasting and publishing rights in 1989. For example, after years as an underground publication, officials allowed editors of the daily newspaper *Gazeta Wyborcza* to openly publish on May, 8, 1989 (Bajomi-Lazar 2014,

107). Finally, in June and July of 1989 Poland held semi-competitive parliamentary and presidential elections that allowed non-communist leaders to take power (Bajomi-Lazar 2014, 105-06). As the political transition continued, Soviet-era censorship officially ended in Poland on April 11, 1990 (Kitschelt et al. 1999, Ost 2001), and by 1992 Polish leaders amended the constitution to ban government censorship and guarantee freedom of speech (Curry 2003).

The MSF scores reflect the qualitative evidence. Beginning in 1960 during the communist authoritarian regime, Poland displays a score at nearly 0 until about 1980 when it slightly increased during societal protests that briefly improved civil liberties. The score then decreases around 1981 when the government introduced martial law. Then, around 1990 when the political transition to democracy resulted in officials lifting the most restrictive media repression laws, the score rises steeply to nearly 1. Poland held this high MSF score until 2016 when it dropped to about .708 from a 2015 point estimate of about .935. According to Chapman (2017), the conservative Law and Justice Party (PiS) began trying to impose controls on public and private media once it took office October 2015. The measures included firing and replacing heads of broadcasting outlets with individuals friendly to the party, such as appointing former PiS member of the lower house (*Sejm*) Jacek Kurski as head of the *Telewizja Polska* television station (pg. 16). Chapman (2017) also reports the government redirects advertising funds to more conservative-leaning outlets and talks of "repolonizing" the media, which the author sees as an effort to replace foreign news outlet owners with domestic ones. The temporal data captures this 2016 downturn. In sum, given Poland's case history, the MSF scores reflect manifestations of nearly absent media freedom as well as nearly perfect media freedom. In addition, it registers instances when nearly imperfect media freedom increased slightly and when nearly full media freedom decreased.

Appendix A details the case histories of the other countries in Figure 5. Like Poland, the MSF scores reflect those countries' case histories.

Figure 5: Media System Freedom (MSF) Temporal Trends in Select Countries, 1960-2016



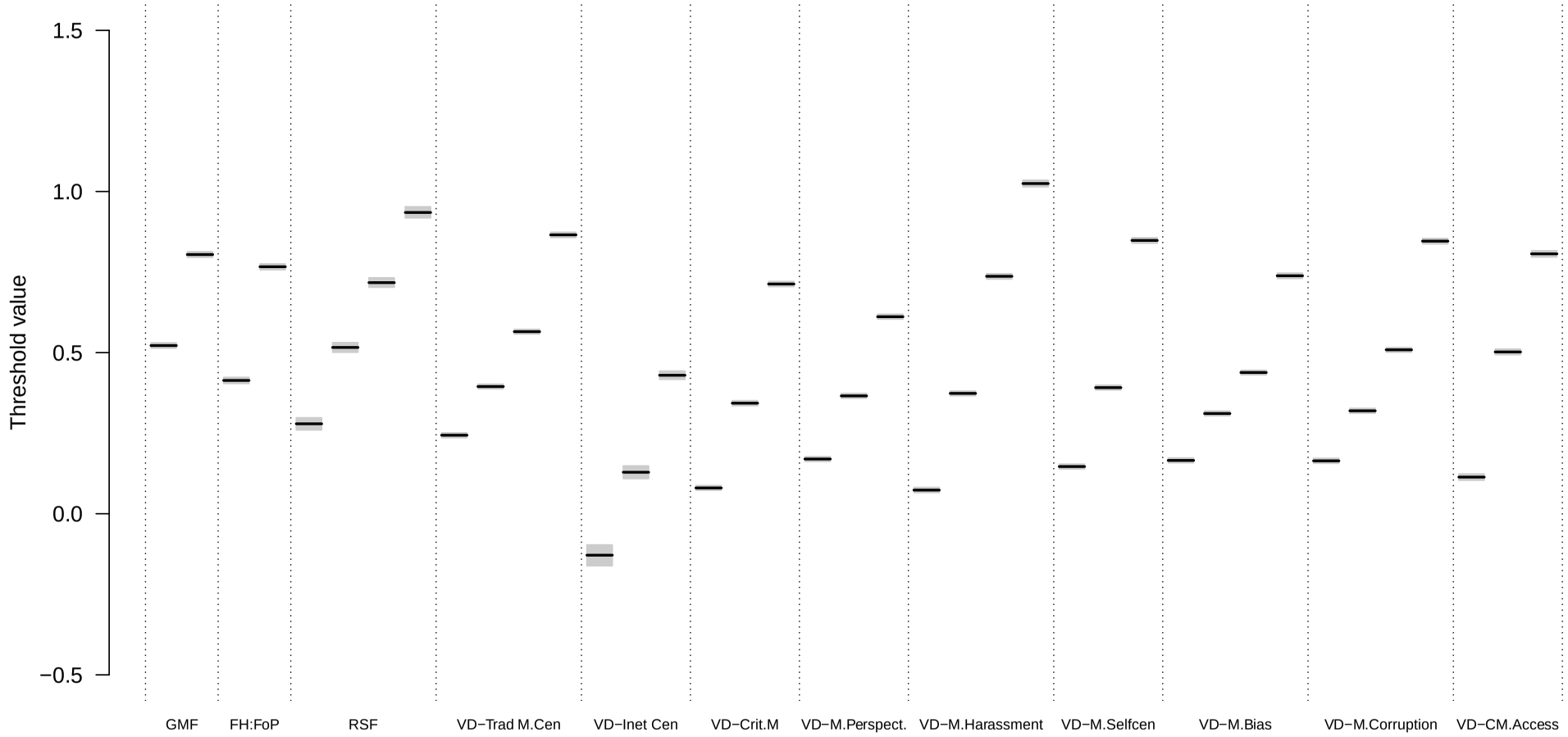
Note: Error bars indicate 80% posterior credible intervals.

Comparing Media Freedom Indicators

Besides information about the latent media freedom variable, the model returns information about the individual indicators themselves. Recall that the link function between x_{kt} and y_{rkt} produces a beta coefficient that evaluates the discrimination of each y_{rkt} with regards to x_{kt}. The larger the coefficient, the more influence that measure has on the latent measure's estimate. I report the beta estimates here: *GMF* (8.95), *Freedom House* (8.3), *RSF* (7.14), *V-Dem: Print/Broadcast Censorship* (10.96), *V-Dem: Internet Censorship* (7.11), *V-Dem: Critical Print/Broadcast Media* (11.32), *V-Dem: Media Perspectives* (11.07), *V-Dem: Journalist Harassment* (10.62), *V-Dem: Journalist Self-censorship* (10.63), *V-Dem: Media Bias* (11.31), *V-Dem: Media Corruption* (10.01), and *V-Dem: Access to Critical Media* (8.05). The results indicate that the V-Dem variables *Critical Print/Broadcast Media*, *Media Bias*, and *Media Perspectives* exerted the most influence. All three return beta coefficients above 11. *RSF* and *V-Dem: Internet Censorship*, both with the least time coverage, returned the lowest beta coefficients and exerted the least influence.

The model also allows me to compare indicators based on their threshold values τ_{rm} relative position. Figure 6 displays each variable's cutpoints. The threshold cutpoints show how each media freedom indicator aligns relative to others along the latent scale. For instance, the lowest *GMF* category is about equivalent to the lowest three *V-Dem: Traditional Media Censorship* categories. Also, the lowest *Freedom House* category is equivalent to two lowest *V-Dem: Media Self-censorship* categories. I note that none of the cutpoints' shaded regions overlap.

Figure 6: Threshold Estimates for 12 Media Freedom Indicators, Shaded Areas Represent 95% Highest Posterior Density



Robustness Checks

In addition to the analysis above, I ran the model under different specifications to evaluate the result's robustness. First, I increased the number of iterations to 10,000 (with three chains) in the Markov chain Monte Carlo (MCMC) simulation. Point estimates from this model correlate to .99 with estimates from the main model's output. In addition, I ran the model including a variable for journalists killed as a result of their professional work. I did not include this variable in the main model because it is not itself an estimate of a media system's freedom or an evaluation of some aspect of it, but a discrete count of a specific event. To assess its impact, I created an ordinal measure for journalists killed drawn from the Committee to Protect Journalists' dataset of journalists killed for their work (Committee to Protect Journalists 2017). The data run from 1992 to 2016. To include it as an ordinal variable, I chose a coding scheme used by Asal et al. (2018) where I code a country-year with no journalists killed as a 0, a country-year where one through nine journalists killed a 1, and a country-year with 10 or more journalist killings as 2.[17] The results show a .99 correlation with the initial output, indicating estimates remain about the same even when including an indicator for journalists killed. All three variables correlate together at .99 or higher, and each model's standard deviation correlate at approximately .98. In addition, the alternative specifications produce similar beta coefficients for each indicator. I show these results in Appendix A.

As an additional check, I ran a factor analysis (FA) on the main model's variables. The IRT model assumes a single trait or ability exists among different variables (Rizopoulos 2006). FA is another latent trait analysis (LTA) that allows researchers to either explore or confirm a dataset's underlying structure. The analysis identifies the number of inferred latent variables (that is, factors) in a dataset (Brown et al. 2011,

[17] I chose this over other coding schemes because it better captures the severity of country-years when many journalists were killed for their journalistic work. For instance, Asal et al. (2018) also used an ordinal measure that codes a country-year 0 when no journalists were killed, a 1 when at least one journalists was killed, and a 2 when a country experienced 2 or more journalists killed. This coding scheme treats the 30 journalists killed in the Philippines in 2009 the same as the two journalists killed in the United States in 2015. The coding scheme I use differentiates between the severity of journalist killings in these two country-years.

141). In accordance with the IRT model's assumption, I expect the data to contain a single factor. To this end, I run a FA and examine each factor's eigenvalues. Eigenvalues over 1 indicate a single factor (Kaiser 1958), and I therefore expect to find only one factor with an eigenvalue over 1.

After running the analysis, results indicate a single factor. The largest factor has a 8.956 eigenvalue, while the next lowest yields a .876. However, FA only produces results based on rows in the data's matrix that contain observations for all 12 variables. Pairwise deletion greatly reduces the sample. I therefore rerun the analysis several times, removing variables that do not cover the entire period to expand the sample. As expected, the results indicate a single factor in every specification. Appendix A provides more detailed results and scree plots of eigenvalues for each specification. I also run a principal components analysis (PCA). Researchers can use PCA to identify data structure (Abdi and Williams 2010, 434). While FA and PCA differ mathematically (Jolliffe 1986, 115-128), "both methods are effective, and widely used, means of exploring the 'interdependence' among the variables" (Kim and Mueller 2000, 11). Using the same eigenvalue criteria, I expect to find a single component to support the IRT model's single-trait assumption. As expected, the PCA indicates a single component exists. I display these results in Appendix A.

In sum, the results hold even under different model specifications. The point estimates and standard deviations prove highly correlated when I increase the number of iterations in the MCMC simulation and include a variable for journalists killed. I also use factor analysis and principal components analysis to provide evidence that a single latent trait exists in the data. I now turn to a replication using the MSF scores.

Replication

To apply the Media System Freedom (MSF) data to an existing empirical analysis, I replicate Egorov et al.'s (2009) work on the influence of natural resources on media freedom in authoritarian regimes. I choose this study because it represents an often cited study where the authors use the Freedom House and RSF datasets to measure

media freedom.[18] The authors argue that resource-poor dictatorships allow freer media systems in order to better monitor their bureaucracy. More specifically, they argue that dictators make this calculation based on the value of their oil reserves (and *not* oil production value). The authors examine countries worldwide from 1993 to 2008 and use Freedom House's *Freedom of the Press* index to operationalize media freedom, using the variable's lead (t + 1). Though the authors present a number of different specifications and robustness checks to evaluate their hypothesis, I focus here on their core findings in models 2 and 5 in their table 1 (Egorov et al. 2009, 658).

The authors' model 2 features panel regression analysis with fixed and year effects. Their key explanatory variable is the value of a country's oil reserves obtained from BP's Statistical Review of World Energy.[19] The model controls for democracy level using *polity* (Marshall and Jaggers 2017), and a number of World Bank indicators including wealth, population, and government expenditures as a percentage of GDP (World Bank 2017). The authors log all World Bank and BP oil data. They run a similar regression in model 5 but restrict the sample to non-democracies and drop *polity* and the interaction term. Model 2 indicates that greater oil reserves value levels negatively associate with higher media freedom levels, while model 5 (restricted to dictatorships) finds the same association. I replicate results from their model 2 below, then move to model 5.[20]

In table 2, model 1 yields comparable, statistically significant results to the original output. As expected, *log oil reserves value* is negative and statistically significant ($p = .061$).[21] As *log oil reserves value* increases, media freedom decreases, on average. Model 2 replaces the Freedom House variable with the MSF point estimates (also using the variable's lead). The results show *log oil reserves value* loses statistical significance and switches direction to positive. The model also shows MSF estimates affected outcomes for some control variables. The interaction term *log oil reserves value * polity*

[18] As of 4/30/2018, Google Scholar indicates it has been cited 316 time.

[19] https://www.bp.com/en/global/corporate/energy-economics/statistical-review-of-world-energy.html

[20] I thank the authors for directly providing their replication materials.

[21] I note the replication results have a p-value about one hundredths away from the published significance level.

retains its positive direction but becomes statistically significant ($p < .01$), while *log gdp p/c, ppp* remains statistically insignificant, but switches signs to negative. *Log population* yields a decreased statistical significance level ($p < .1$) and switches direction to positive. *Log of government expenditures as a percent of GDP* remains negative and statistically significant, though I note its significance level increases to ($p < .01$). Also, *polity* remains both positive and statistically significant ($p < .01$).

Table 2: Egorov et al. (2009) Replication of Model 2 (Table 1)

	Model 1 Replication	Model 2 w/ MSF	Model 3 w/ MSF
	β/(SE)	β/(SE)	β/(SE)
Log Oil Reserves Value	-2.297*	.004	.001
	(1.225)	(.009)	(.004)
Log Oil Reserves Value x Polity	.068	.0008**	-.001
	(.043)	(.0003)	(.001)
Polity	.573***	.008***	.024***
	(.174)	(.001)	(.001)
Log GDP p/c, PPP	1.262	-.007	
	(1.155)	(.009)	
Log GDP p/c, Nominal			-.029***
			(.003)
Log Population	-13.408***	.034	.043***
	(2.807)	(.022)	(.009)
Log Govt. Expend./GDP	-1.678**	-.017***	-.013***
	(.773)	(.006)	(.004)
R^2	.1204	.2926	.6737
AIC	13028.35	-6869.878	-12287.11
Observations	2,056	2,056	4,941
Countries	147	147	161
Years	1993-2008	1993-2008	1980-2016
Fixed Effects	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes

$***p < 0.01; **p < 0.05; *p < 0.1$; β = Coefficient Estimates; (SE)= Standard Errors; All media freedom variables are leads (t + 1); Constants not reported; Model 1 uses data provided by the authors.

Finally, model 3 takes advantage of the MSF scores' expanded time coverage to extend the analysis back to 1980 and forward to 2016.[22] The expanded coverage increases the sample size from 2,056 to 4,941 and the number of countries from 147 to 161.[23] For this model, I use World Bank's comparable nominal GDP per capita.[24] The expanded

[22]BP only provides data for oil reserves dating back to 1980

[23]I deviate slightly from the original model and take a sample of countries that carry a *polity* score less than 6 for that country-year. The authors restricted their sample to all countries less than 6 in 1992, the year before the sample begins. Given the expanded time frame however, following this lead would erroneously assign countries as dictatorships that exist today as non-dictatorships like Brazil and South Africa. It would also consider countries that have fallen below 6 since 1979 to be non-dictatorships like Venezuela.

[24]World Bank's *gdp p/c, ppp* variable only goes back to 1990.

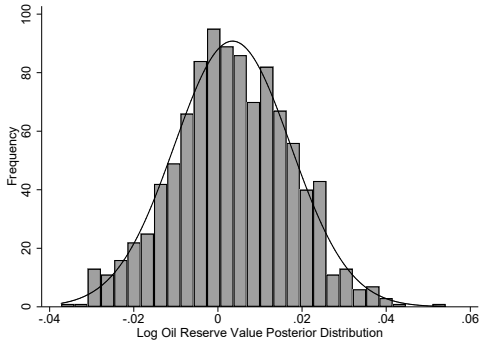
sample further supports model 2's results. *Log oil reserves value* is positive and statistically insignificant. Regarding controls, the interaction term becomes negative and statistically insignificant, while *log gdp p/c* is negative and becomes statistically significant ($p < .01$). Next, I observe that *log population* is positive and statistically significant ($p < .01$) — the expanded sample flips the variable's direction from model 1. Finally, like in models 1 and 2, *polity* retains its positive direction and statistical significance ($p < .01$), while *log government expenditures as a percent of GDP* remains negative and statistically significant ($p < .01$).

Recall that the IRT model produces a posterior distribution (point estimate/mean and standard deviation) measuring each country-year's media freedom level. This distribution measures the rater's reliability. To utilize this data, I run Monte Carlo simulations on models 2 and 3 in table 2 above using 750 random draws from each country-year's MSF score distribution. The simulation runs the regression model 750 times, estimating a beta coefficient and standard error each time, then returns a mean of each variable's beta coefficients and standard errors. Results from the simulation yield similar coefficients and standard errors as in models 2 and 3. To visualize the main independent variable's results, I graph 1,000 random draws from the Monte Carlo simulations' resulting posterior distribution for *log oil reserves value* from models 2 and 3 in figures 7a and 7b respectively. In Figure 7a, 594 of the draws fall on the distribution's positive direction, suggesting a 59.4 percent probability that *log oil reserves value* has a positive effect on media freedom. Figure 7b indicates 628 draws fall on the distribution's positive side, suggesting about a 62.8 percent probability that *log oil reserves value* has a positive effect on media freedom. The results show figure 7b forms a more compact distribution than figure 7a, indicating more certainty in the result. I display full regression results in Appendix A. In sum, contrary to Egorov et al's (2009) initial findings, *log oil reserves value* does not appear to have a significant reductive effect on media freedom levels in the sample.

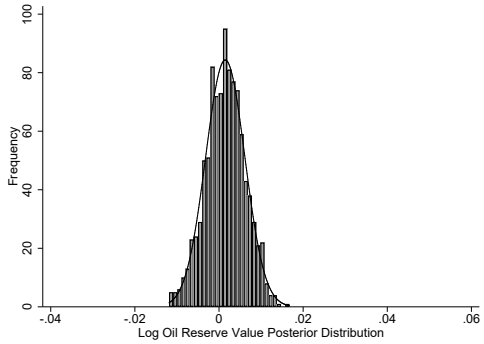
I also replicate Egorov et al.'s (2009) model 5 in table 3 below. This model resembles their model 2 but with a few modifications. The authors drop the interaction term

Figure 7: *Log Oil Reserves Value* Posterior Distributions, Table 2

(a) Model 2 (1993-2008)



(b) Model 3 (1980-2016)



and restrict the model to dictatorships. They consider countries with a polity score of 5 or less in 1992 as dictatorships throughout the entire sample. Model 4 replicates the author's initial results. *Log oil reserves value* is negative and statistically significant ($p = .057$), indicating that as the value of oil reserves increases in dictatorships, the level of media freedom declines, on average.[25] In model 5, I apply the MSF scores to the original model's sample, and *log oil reserves value* loses statistical significance ($p = .667$). However, a different pattern emerges in model 6 once I expand the time and countries covered. *Log oil reserves value* achieves statistical significance ($p < .05$) but runs in a positive direction. Contrary to the authors' expectations, these findings suggest that *log oil reserves value* has a positive association with media freedom in dictatorships. I also ran Monte Carlo simulations on models 5 and 6 using 750 random draws from each country-year's MSF score distribution. Like the Monte Carlo simulations I described above, it runs the regression model 750 times, estimating a beta coefficient and standard error each time, then returns a mean of each variable's beta coefficients and standard errors. Results from the simulation yield similar coefficients and standard errors in models 5 and 6. I show these results in the supplemental index. Finally, the controls produce similar results, with the exception of *log population* which remains statistically significant ($p < .01$) but becomes positive in model 6.

Overall, including the MSF data in Egorov et al.'s (2009) sample significantly im-

[25] I note the replication results have a p-value about one hundredths away from the published significance level.

Table 3: Egorov et al. (2009) Replication of Model 5 (Table1)

	Model 4 Replication	Model 5 w/ MSF	Model 6 w/ MSF
	β/(SE)	β/(SE)	β/(SE)
Log Oil Reserves Value	-2.874*	-.005	.013**
	(1.511)	(.013)	(.005)
Log GDP p/c, PPP	-3.121**	-.032**	
	(1.567)	(.013)	
Log GDP p/c, Nominal			-.062***
			(.005)
Log Population	-18.335***	-.078**	.132***
	(4.724)	(.041)	(.018)
Log Govt. Expend./GDP	.171	.002	.011
	(1.121)	(.009)	(.007)
R^2	.0483	.2239	.4789
AIC	6363.23	-2735.977	-5134.936
Observations	958	958	2,430
Countries	71	71	115
Years	1993-2008	1993-2008	1980-2016
Fixed Effects	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes

$***p < 0.01; **p < 0.05; *p < 0.1$; β = Coefficient Estimates; (SE)= Standard Errors; All media freedom variables are leads (t + 1); Constants not reported; Model 4 uses data provided by the authors. Models 4 and 5 restricted to countries with polity score < 6 in 1992; Model 6 restricted to country-years with polity score < 6.

pacts their results. When I include the MSF data in the worldwide sample, the key independent variable *log oil reserves value* not only loses statistical significance but switches signs. The results show changes in some controls as well. The same pattern emerges when I expand the sample to the years 1980 to 2016 and increase the countries covered in the sample. When I include the MSF scores in the authoritarian model in table 3, I find evidence that contradicts the author's expectations. *Log oil reserves value* becomes insignificant in the original sample, but then becomes positive and statistically significant in the expanded one. The findings suggest that as the value of oil reserves increase in dictatorships, media freedom increases, all else equal.

Discussion

Media's ability to independently produce and disseminate news content remains at the core of media freedom's conceptualization. While many raters produce indicators to evaluate an entire media system's freedom or aspects of it, none perfectly measure media's ability to operate freely. To better capture the concept and create a more robust media freedom measure, I treat it as a latent trait and estimate an item response theory

(IRT) model to generate new data. This exercise has both theoretical and empirical implications.

Empirically, I apply Linzer and Staton's (2015) dynamic, graded response IRT model to analyze 12 extant media freedom measures and generate cross-sectional, time-series data on media freedom in 196 countries from 1960 to 2016. The Media System Freedom (MSF) data includes a posterior distribution of media freedom for 9,495 country-years that includes both a point estimate and a standard deviation. Future researchers can include this data in empirical models that analyze media freedom to account for rater reliability and uncertainty among measurements. I also find that the Varieties of Democracy variables *Critical Print/Broadcast Media*, *Media Bias*, and *Media Perspectives* exert the most influence on the MSF scores, while the Reporters Without Borders *Press Freedom Index* and V-Dem *Internet Censorship* variables exert the least.

To show the MSF data's usefulness, I replicate Egorov et al.'s (2009) study on resource wealth and media freedom using the new estimates. In a model that includes all available countries in the MSF data, the key explanatory variable lost significance and changed direction. In another model restricted to non-democracies, the same variable is statistically significant ($p < .05$) but positive — contrary to the authors' expectations. The replication suggests that including a measure that takes into account rater reliability significantly impacts the authors' results. It also highlights the importance of replications that expand time coverage and countries to evaluate the initial result's robustness.

The replication highlights theoretical implications as well. The author's do not explicitly express a conceptualization of media freedom when they describe its measurement, though their theory seems to describe a process where leaders control media freedom to some extent. Given this lack of clarity, utilizing my latent measure that incorporates uncertainty among the various media freedom measures better suites this empirical analysis. When I replace their measure they use for the one I proposed in chapter 2, their results do not hold.

In sum, treating media freedom as a latent variable and estimating an item response

theory (IRT) model that incorporates different media freedom indicators creates a more robust measure of this complex concept.

Chapter 3 - Judicial Independence and Attacks Against Media

"The volume of arrests, interrogations, and people out on bail is enormous," said exiled Iranian journalist Omid Memarian of his native country in 2011.[26] "The effect is that many journalists know they should not touch critical subjects. It really affects the way they cover the news because they are under constant fear and intimidation." That year, the Committee to Protect Journalists reported Iran imprisoned 42 journalists — more than any other country in 2011. Memarian himself describes a harrowing experience when Iranian authorities arrested, tortured and elicited a forced confession from him after a three-week detention period in 2004. "I knew people who had been to prison [...] They never recovered. I did not want to be one of those people."[27]

Government efforts to influence published or broadcasted news content, either via media control or by inducing self-censorship, represent a threat to public access of information and affects the quality of democracy (Schedler 2002). These attempts to censor content through prohibitive licensing requirements or the outright shuttering of media outlets send strong messages to media personnel when those in power deem their news reports uncomfortable. To better understand the circumstances that influence the free flow of information in a country's media environment, scholars must better understand the conditions under which government efforts to manipulate news content are most likely to occur.

Theoretical concepts of democracy often include freedom of the press as an essential element (Dahl 2005). The onerous task required for citizens to gather information pertaining to their government, policy outcomes, and other related concerns makes them reliant on media to provide this content. However recent findings indicate an intriguing effect on media systems once democracy is introduced. While democracy positively associates with higher media freedom (Whitten-Woodring and Van Belle 2017), it also

[26] *Committee to Protect Journalists* "2011 Prison Census," Available at https://cpj.org/2012/02/attacks-on-the-press-in-2011-journalists-in-prison.php

[27] Updike, Nancy "Side Effects May Include," *This American Life* April 15, 2011: Available at https://www.thisamericanlife.org/433/fine-print-2011/act-three

increases personal safety risks for working journalists either by censorship and harassment (VonDoepp and Young 2013) or by killings (Asal et al. 2018). In this chapter, I address this *press-safety paradox* as it relates to non-deadly attacks against media.

I first argue that previous empirical studies lose important observations of government attacks against media by only analyzing countries that meet an arbitrary civil liberties and political rights criteria. Though countries that do not meet this standard generally possess less freedoms, I demonstrate that their governments *still* attack journalists, even if they work for the government. Researchers should therefore include these observations when studying government perpetrated attacks against media. Next, I argue that researchers should employ a more nuanced approach in studying democracy's effect on government attacks against media. Instead of taking democracy in its aggregate, I focus on two distinct components: judicial independence and citizens' ability to vote leaders out of office. I argue that judicial independence provides a significant reductive effect on government attacks against the media absent of high electoral democracy levels. Courts provide a crucial check on the executive's power when leaders overstep it. When courts cannot act independently, this condition emboldens leaders to perpetrate attacks against media — even in countries with constitutional guarantees for freedom of the press. However, when citizens possess the ability to remove the executive through elections, leaders are more likely to refrain from attacks based on voters' threat and judicial independence becomes less valuable in reducing attacks against journalists and media outlets.

Using three separate measures of attacks against the press in a worldwide sample from 1948 to 2012, I evaluate my theory using panel regression analysis. I use variables that measure (1) government efforts to censor traditional media like newspapers, television, and radio, (2) harassment against media, and (3) government efforts to censor the Internet. I find strong evidence supporting this conditional effect for government efforts to censor traditional media and media harassment, and mixed evidence regarding internet censorship. The results remain robust to different estimation approaches and model specifications, including Monte Carlo simulations that incorporate latent variable

uncertainty for data on media attacks, judicial independence, and electoral democracy.

I also examine an untested assumption in the literature that government attacks against media increase self-censorship among journalists. Previous empirical work focuses on single countries during a limited time frame (Stanig 2015), but I analyze new data that measures media self-censorship worldwide using panel regression analysis. I find evidence that increases in government attacks against media positively associate with self-censorship. In addition, I find that government censorship of traditional media and government harassment of media impact journalists' decision to self-censor more than government censorship of the Internet.

The study makes a number of contributions to the media freedom and democratization literature. First, it presents the first attempt at analyzing government attacks against the media using latent variables. These variables' latent estimations offer empirical researchers more valid measurements by linking related indicators or expert surveys in principled and transparent measurement models (Crabtree and Fariss 2015). They assume that data collectors cannot accurately observe a concept, and so generate a posterior distribution consisting of the variables point estimation — the distribution's mean — and a standard deviation that represents the point estimation's uncertainty. I incorporate this information in the empirical analysis for the three attacks against the media variables: media self-censorship, judicial independence, and electoral democracy. Second, it contributes theoretically to the larger democratic institutions literature, by showing the consequences that component institutions of democracy have on governments' willingness to attack media. Third, I find evidence for an untested assumption in the literature that government attacks against media positively associate with greater media self-censorship in a cross-national, time-series sample.[28]

Why Governments Attack Media

Studies of government attacks against media fall firmly within press freedom's broader literature. Authors study *attacks* that include physical assaults and jailing of

[28]I use "government" and '"leader" interchangeably throughout this chapter to refer to a country's leader.

journalists, forced closure of a media outlet, censorship, and using public advertising funds for political use among others. Attacks have the dual purpose of silencing investigative journalist directly and inducing self-censorship to others who consider publishing a government critique or exposé (Stanig 2015). The literature identifies three general theories regarding a government's decision to attack media. First, the *provocation theory* holds that governments attack media when journalists or media outlets cross some known, acceptable threshold of discourse (Kasoma 1997, Zaffiro 1993, Lucas 2003, Ngok 2007). Next, the *arbitrariness by design* theory argues governments randomly select targets to attack in order to create an atmosphere of uncertainty; This gives media an incentive to self-censor negative reports against the government even if a journalist or outlet has not been specifically targeted (Hassid 2008, Stern and Hassid 2012). Finally, the *political events* approach finds attacks against media increasing in hybrid and transitional democracies during key political events like coup attempts, major protests, proposed constitutional reforms, and presidential elections (VonDoepp and Young 2013).[29]

While this research advances the literature regarding when governments choose to attack media, I find two gaps for further analysis. First, in the only cross-national study of government attacks against media, VonDoepp and Young (2013) only focus on countries that cross a certain "openness" threshold at or below a Freedom House (2017b) *Freedom in the World* score of 9. They argue that countries with a higher score do not possess sufficient levels of political rights and civil liberties for journalists to function free enough to illicit government attacks. However, countries often do not cross that threshold precisely because governments vigorously censor and attack media. For example, the Committee to Protect Journalists (CPJ) frequently publishes a list of countries with the most censored media systems. The 2012 list includes countries that would not meet VonDoepp and Young's criteria including North Korea, Equatorial Guinea, Saudi Arabia, Cuba, and Belarus.[30] The 2015 list names similar countries like

[29]The authors find mixed evidence for presidential elections.

[30]*Committee to Protect Journalists* "10 Most Censored Countries," May 2012: Available at https://cpj.org/reports/2012/05/10-most-censored-countries.php

China, Vietnam, Ethiopia, and Azerbaijan.[31]

Even the idea that some consolidated authoritarian regimes completely control media does not render journalists totally safe. Long-time authoritarian governments may even jail their own journalists. From 2000-2017, authoritarian China imprisoned 618 journalist.[32] In Myanmar, authorities imprisoned and tortured a pair of Burmese journalists in 1992 for publishing a story that the military junta deemed impermissible.[33] The pair worked for the state-owned Burmese newspaper *Kyemon* and would eventually die in prison. In sum, researchers should also consider countries like China, Myanmar, and others that do not cross an "openness" threshold in a cross-national analysis of government attacks against media.

Second, authors largely ignore the institutions that may influence the likelihood government will carry out such attacks. Specifically, institutions that check the executive's power like the judiciary or the electoral mechanism that removes leaders. The literature portrays leaders existing in a vacuum without the consideration of other governmental actors. However, a strategic game among different governmental branches often exists (Barnes 2007, Helmke 2002, Garoupa et al. 2013). My theory focuses specifically on the strategic game between the national courts and the executive, arguing judicial independence affects the likelihood that governments perpetrate attacks against media but that this hinges on citizens ability to vote leaders out of power.

Melton and Ginsburg (2014) define judicial independence as "the ability and willingness of courts to decide cases in light of the law without undue regard to the views of other government actors" (pg. 190). The judicial independence literature assumes ruling against the sitting government sends a strong signal that courts act independently. Though constitutions often provide mechanisms to give judges independence in deciding cases, in practice courts do not always function as independently as the law intends. Scholars therefore make a distinction between *de jure* and *de facto* judicial indepen-

[31] *Committee to Protect Journalists* "10 Most Censored Countries," April 2015: Available at https://cpj.org/2015/04/10-most-censored-countries.php

[32] *Committee to Protect Journalists* "2016 Prison Census," Available at https://cpj.org/imprisoned/2016.php

[33] *Committee to Protect Journalists*: Available at https://cpj.org/data/people/u-tha-win/index.php

dence (Voight et al. 2015). *De jure* judicial independence represents formal procedures including (but not limited to) the appointment process that brings judges to the bench and regulates their tenure (McCubbins et al. 1995, Brinks 2005, Hilbink 2012). On the other hand, *de facto* independence refers to judges' actual behavior such as their ability to rule against executives power. Scholars of judicial behavior are often more concerned about *de facto* independence and less so about *de jure* (Linzer and Staton 2015).

The democratization literature sees the separation of powers, namely between the executive and the judiciary, as a key component to democracy and governmental accountability (Collier 1999, Kenney 2003). The checks and balances structure of democracies requires an independent judiciary. Its absence harms democracy's quality in democratizing countries, particularly in regions like Sub-Saharan Africa (Gloppen 2003, Senghore 2010). Legal scholars and political scientists point to judicial independence as a safeguard for human rights (Cross 1999, Keith 2002, Skaar 2011, Sakala 2014), while its absence may have negative consequences for press freedom (Ogbondah 2002, Orgeret and Ronning 2009). An institutional approach encompassing the judiciary and the electoral mechanism that removes executives from power can potentially explain variation in government perpetrated attacks against media.

Institutional Conditions that Encourage Attacks

In this section I address the institutional determinants of government attacks against media. I argue that judicial independence provides a significant reductive effect on these attacks absent of high electoral democracy levels. Courts provide a crucial check on the executive's power. When governments estimate a higher likelihood that the courts will rule in their favor, they become more emboldened to censor and harass journalists. However, when voters possess the power to remove leaders through elections, leaders become less emboldened to censor and harass the journalists providing citizens crucial news content. At high levels of electoral democracy, judicial independence's effect becomes less valuable in protecting journalists against government attacks.

I build my theory based on two assumptions. First, I assume leaders wish to remain in power. They make decisions intended to increase the likelihood that they or the political organization they represent will remain in control of the executive. Next, I assume a judge's vote against the government signals judicial independence, a common assumption in the judicial behavior literature (Helmke 2002, Garoupa et al. 2013). With these assumptions, I turn now to the basis of my argument.

Media appear ubiquitously in nearly all societies. They publish and broadcast content on a variety of topics, including (but not limited to) content about government. An implicit supply and demand of information exists—citizens demand information and media supply it. This information can come in the form of entertainment or so-called 'info-tainment,' but this analysis focuses purely on political content. Regarding the demand side, in order for citizens to make decisions at polls or evaluate their government, they should possess some degree of knowledge about current events, salient local and national issues, and other basic information to make informed evaluations about policy and leaders at the polls. However due to constraints such as providing a personal living for themselves and perhaps a family, citizens cannot adequately monitor government activity firsthand. Directly gathering information to evaluate incumbents' policy outcomes, the platforms of challenging candidates, or other government actions remains nominally impossible for voters. Therefore, citizens essentially delegate this responsibility to a professional monitoring core: the press (Page 1996).

Journalists and media personnel wish to supply this needed, low-cost information to voters in the form of news content found in print, television, radio, and increasingly online content. They prefer to report newsworthy items that range from "soft" news interests like coverage of local sporting events to "hard" news such as the activities of politicians and business interests with little or no interference from the government. A competing interest between publishing news and collecting revenue from advertisement, government subsidies, or newspaper sales exists at a basic level at most media organizations. However, despite this tradeoff, media's main goal remains to produce news for consumption and inform citizens (Deuze 2005, 447).

This news content often features leaders and government officials who wish to retain power. Publishing or airing a story or editorial critical of leaders and their policies presents a potentially negative outcome for media personnel, such as censorship, detainment, media outlet closure, or worse. Governments attract local and national media attention throughout their time in power. Casting incumbents in a negative light in news reports can affect citizens' image of the government (Kleinnijenhuis et al. 2006). Negative news potentially persuades citizens that leaders are not performing their public duties adequately, thus making them more likely to vote against the incumbents the next election or more generally diminish government support. However, if media portray incumbents in a positive light, citizens will feel more favorable toward their ability as leaders and more likely to vote for the incumbent or simply continue their support of the government. In this dynamic, negative press attention incurs a cost to incumbents, making their power retention goal more difficult (Ferraz and Finan 2008). With this power retainment goal in mind, the incumbent will seek to maximize positive media coverage and minimize costly, less favorable press. Therefore, incumbents have an incentive to influence media coverage to maximize positive press exposure or, at the very least, suppress negative news stories. Even in non-democracies, leaders wish to instill a sense of legitimacy, a goal partially obtained through media control (Gandhi 2009).

In some instances, leaders exert influence through simple persuasion tactics, such as political advertisements on various mediums, speeches to special interests groups, campaigns that garner media coverage, and credit claiming in the news. However, facing fewer restraints in less established democracies or non-democracies those leaders may choose to manipulate media beyond the realm of accepted democratic practices (Schedler 2002).

When incumbents have both ability and incentives, they may resort to media harassment with the intentions of silencing journalists or cultivating self-censorship in an attempt to suppress negative press coverage (VonDoepp and Young 2013). Though many countries function under constitutions that either guarantee freedom of the press or at

least the freedom of expression,[34] vague laws about media often give leaders grounds to perpetrate attacks against the media. In Latin America and Sub-Saharan Africa for instance, many of these laws date back to colonial or firmly autocratic times.

In Tanzania for example, while the current constitution ensures freedom of the press, a number of laws open the possibility for government to attack media. An authoritarian regime passed and enacted The Newspaper Act in 1976 just over a decade after Tanzania gained independence from Great Britain. The law remains in effect today and requires all newspapers to register with the government (Part II, sect. 6) and allows the state to seize any newspaper refusing to follow the registration process[35] (Part IV; sect. 22,1). The Newspaper Act also allows the government to suspend newspaper operations based on content. According to the law:

> *Where the Minister is of the opinion that it is in the public interest or in the interest of peace and good order so to do, he may, by order [. . .] direct that the newspaper named in the order shall cease publication as from the date (hereinafter referred to as "the effective date") specified in the order.* (Part IV, sect. 25)

Aside from the ambiguity of the term *Minister*, the law leaves *public interest* and *interest of peace and good* unexplained. The Tanzanian government invoked the law on October 13, 2008 to suspend the newspaper *MwanaHalisi* for printing an article on an upcoming presidential election.[36]

Similarly in Brazil, the 1988 constitution guarantees freedom of expression but a restrictive law left-over from the country's military dictatorship remained in place until 2009. The 1967 Press Law (*Lei de Imprensa*) ironically begins with an expression of freedom of the press, but later describes criminal provisions for libel and slander. Since Brazil's transition to democracy, authorities used the 1967 Press Law numerous times to "systematically harass critical journalists" before the Brazilian Supreme Federal Tri-

[34] Of the 190 active constitutions that the Comparative Constitutions Project (CCP) archives, 153 explicitly guarantee freedom of the press, while 182 explicitly guarantee freedom of expression (Elkins and Ginsburg 2007).

[35] For example, it requires a registered newspaper to deliver "at the newspaper's expense" one copy of each newspaper printed to the Registrar's Office–an obvious burden on the newspaper.

[36] *Committee to Protect Journalists* "TANZANIA: Government Bans Private Weekly" https://cpj.org/2008/12/tanzania-government-bans-private-weekly.php

bunal struck it down in 2009.[37] Like the Tanzanian Law, Brazil's 1967 Press Law uses vague language such as this passage from Article 2:

> *The publication and circulation in the national territory of books and newspapers and other periodicals, unless they are clandestine (art. 11) or when they violate morality and good morals, shall be free.*[38] (Article 2, Lei No 5.250 (1967))

We find other examples of government attacks against media with intentions to silence them when they pursue uncomfortable news stories. In 2002, the Liberian government arrested a prominent journalist named Hassan Bility, editor of the newspaper *The Analyst*, without official charge or trial.[39] After being detained for nearly 6 months, authorities released him only after diplomatic intervention from the United States. In Burkina Faso, the government detained Mathieu Ndo, managing editor of the newspaper *San Finna*, after he visited the Ivory Coast on assignment in 2004.[40] He was there reporting on rising tensions between the Ivorian government and a rebel group backed by Burkina Faso's government. The government claimed the arrest protected national security. Gabon offers another example where the ruling party banned the private weekly *Les Echos du Nord* for three months after it published an article criticizing pro-government press in September 2006.[41] In Madagascar, the government closed privately-owned radio stations Radio Ny Antsika, Sky FM, and Radio Feon'I Toamasina (RFT) after they broadcasted statements from opposition leaders in 2004.[42]

In this chapter, I argue that the institutional conditions under which leaders function explain some of the variation in attacks like these. Specifically, that an independent judiciary decreases these attacks, but that its reductive effect becomes less significant

[37] *Committee to Protect Journalists* "In Victory for Press, High Court Strikes Down Repressive Law," May 7, 2009: Available at https://cpj.org/2009/05/in-victory-for-press-brazils-high-court-strikes-do.php.

[38] The original Portuguese: *É livre a publicação e circulação, no território nacional, de livros e de jornais e outros periódicos, salvo se clandestinos (art. 11) ou quando atentem contra a moral e os bons costumes.*

[39] *Freedom of the Press* (Washington DC: Freedom House, 2003). Available at http://freedomhouse.org/report/freedom-press/2003/liberia.

[40] *Freedom of the Press* (Washington DC: Freedom House, 2005). Available at http://freedomhouse.org/report/freedom-press/2005/burkina-faso.

[41] *Freedom of the Press* (Washington DC: Freedom House, 2007). Available at http://freedomhouse.org/report/freedom-press/2007/gabon.

[42] *Reporters Without Border/IFEX* "Three Radio Stations Closed Indefinitely." Available at http://www.ifex.org/madagascar/2004/12/28/three_radio_stations_closed_indefinitely/.

once citizens become better able to vote leaders out. I first describe how the judiciary can reduce government attacks against media, then argue that this effect is more likely to exist in countries with low to moderate levels of electoral democracy.

The judiciary provides a horizontal check and source of accountability to other governmental veto players (Tsebelis 2002, Magaloni 2003). With its role to interpret laws and issue rulings for or against leaders, it stands as a check against gross executive actions. Its power lies in its ability to act as a neutral third party to mediate disputes (Larkins 1996, 2). Though courts may check other actors like the legislation or even lower courts, I focus here on national level courts that have nationwide jurisdiction in checking the executive.

If judges prove unable to provide such a check on executive action, then institutional conditions to interfere with media favors leaders. Leaders specifically seeking to reduce the cost of negative press are more likely to perpetrate attacks against media if they believe the judiciary will rule in their favor in cases involving the media. In these instances, if the government estimates the judiciary will vote in favor of the ruling government (that is, act less independent), they become more emboldened to pursue a policy of attacks against media to reduce the costs incurred by negative news coverage and hold less fear of judicial reprimand.

Court decisions that rule against the executive signal independence. For example in 2000, the Ghanaian Supreme Court ruled that the president could no longer choose the heads of state-owned Ghana Broadcast Corporation (GBC). Since that time, Ghana has been consistently ranked as a "free media" by media watchdog groups (Whitten-Woodring and Van Belle 2014, 197). In May 2004, Malawi's Supreme Court ruled a radio station closed by the government after airing an interview with an opposition figure be immediately reopened.[43] The court also ordered the government to compensate the radio station for advertising revenue lost during the closure.

However, the judiciary's role in protecting media and journalists should only serve this significant role under certain conditions. Here I add the caveat that citizens' ability

[43] *Reporters Without Border/IFEX* "Three Radio Stations Closed Indefinitely." Available at http://www.ifex.org/malawi/2004/06/29/radio_station_reopened/

to vote leaders out of office serves a conditional role regarding an independent judiciary's reductive influence on attacks against media. While scholars typically differ on democracy's definition (Fishman 2016), the capacity for citizens to remove the executive at regular intervals remains a core feature (Cheibub et al. 2010). Voters may remove leaders who do not effectively manage foreign policy, the economy, or other domestic concerns. To fully participate in the electoral process, voters need access to low-cost information to evaluate leaders, their policies, as well as other candidates during the executive recruitment process specific to each country — low-cost information that media provide.

When leaders have the least fear of being voted out of office, media are in the most need of protection from an independent judiciary. If leaders damage the integrity of the electoral process, then they are emboldened to attack media with less fear of losing power. Here, checks from the judiciary prove crucial for preventing restrictive media attacks. However at higher levels of electoral democracy, when voters pose a legitimate threat to leaders survival in power, an independent judiciary becomes a less significant factor in reducing media from government attacks against media.

In sum, a conditional relationship exists between government attacks against media, judicial independence, and electoral democracy. When voters do not pose a credible threat to leaders' power retention goal, media systems most need an independent judiciary to protect journalists and outlets from government perpetrated attacks. However, as a country's electoral integrity increases, judicial independence becomes less necessary until its effect in reducing attacks diminishes.

From this discussion, I derive the following hypothesis:

> H1: *An increase in judicial independence is associated with a decrease in attacks against media when high electoral democracy is absent.*

Although this theory focuses entirely on the conditional relationship between the courts and electoral democracy on government attacks against media, it holds another empirical implication that attacks lead to high levels of journalist self-censorship. I argued that the goals of leaders are to reduce negative press coverage, and that spe-

cific institutional environments embolden them to perpetrate attacks to reduce negative news coverage. Freedom House asserts throughout a number of their *Freedom of the Press* country reports that attacks often lead to self-censorship, as they do in their reports on Mongolia in 2011[44] and Indonesia in 2008.[45] Stanig (2015) has explored this question by utilized subnational variation in Mexican to examine how states' varying punitive sentencing guidelines on how libel affects the number of corruption stories each state's media outlets report in the year 2001. However, researchers have not modeled self-censorship to empirically evaluate this assumption cross-nationally. Based on the discussion above, I derive the following hypothesis:

> H2: *As attacks against the media increase, media self-censorship will increase.*

I now turn to the research design where I describe the data and models I use to test my hypotheses. I then present the results and discuss them at the end of the chapter.

Research Design

Above I argue that judicial independence has a reductive effect on attacks against media when high levels of electoral democracy are absent. It also implies that government attacks against media encourage self-censorship among journalists. To evaluate these empirical implications, I describe the model and data I will use to test my hypotheses, then proceed to reporting the results below.

Dependent Variables

Government Attacks against Media

To measure government attacks against media, I utilize three latent variables from the Varieties of Democracy (V-Dem) dataset that deal with different types of attacks

[44] *Freedom House* "Freedom of the Press: Mongolia 2011" https://freedomhouse.org/report/freedom-press/2011/mongolia

[45] *Freedom House* "Freedom of the Press: Indonesia 2008" https://freedomhouse.org/report/freedom-press/2008/indonesia

against media (Coppedge et al. 2017a). First, I use a variable that measures government censorship efforts against traditional media like newspaper, television, and radio (*traditional censorship*). It measures government direct or indirect censorship attempts. Regarding indirect censorship, V-Dem considers politically motivated measures to influence content including awarding of broadcast frequencies, withdrawal of financial support, influence over printing facilities and distribution networks, selected distribution of advertising, onerous registration requirements, prohibitive tariffs, and bribery. They do not consider non-political topics such as child pornography, statements offensive to religion, or defamatory speech unless government use them as a precursor to censor political speech. V-Dem generates the latent variable by surveying experts on the degree of censorship in a country, then uses a Bayesian item response theory (IRT) model to evaluate their responses and measure the concept. The variable is continuous and runs from 0 to 6.84 with higher values indicating higher censorship levels.

While censorship covers important aspects of government attacks against the press, it does not cover the extent to which governments harass media, such as threatening journalists with libel suits, arrest, imprisonment, or compromising their physical integrity. To include these types of attacks, I use V-Dem's *media harassment* variable (Coppedge et al. 2017a). This variable measures the extent to which governments harass journalists.[46] Like *traditional censorship*, V-Dem generates the latent variable by surveying experts on the degree of media harassment, then uses a Bayesian item response theory (IRT) model to evaluate their responses and measure the concept. The variable is continuous and runs from 0 to 7.1 with higher values indicating higher harassment levels.[47]

Varieties of Democracy also measures government censorship of the Internet (*Internet censorship*), which measures the extent to which governments censor political

[46] In this measure, V-Dem also considers actions of powerful non-governmental actors, though government impunity often enables their harassment against journalists.

[47] These two variables highly correlate at about .82 and greatly overlap in a number of ways. While created as separate concepts, they both generally measure my concept of *attacks against media*. Given this, supporting evidence after modeling both variables would support my theory. I also note that these variables result from an expert survey. While authors have questioned the validity of expert surveys in the past (Budge 2000), the dataset's managers use a panel of multiple experts and then synthesizes their responses using an item response theory (IRT) model to improve their validity (Pemstein et al. 2017).

information on the Internet. V-Dem clarifies that this includes attempts at "Internet filtering (blocking access to certain websites or browsers), denial-of-service attacks, and partial or total Internet shutdowns." Like *traditional censorship*, they do not consider child pornography, highly classified information such as military or intelligence secrets, statements offensive to religion, or defamatory speech unless governments use it as a pretext for censoring political information or opinions. Again, V-Dem generates the latent variable by surveying experts on the degree of a country's internet censorship, then uses a Bayesian item response theory (IRT) model to evaluate their responses and measure the concept. The variable is continuous, and runs from 0 to 7.16 with higher values indicating higher internet censorship levels.

Recall that VonDoepp and Young (2013) only collect data on countries in Sub-Saharan Africa from 1995 to 2009 at or below a Freedom House *Freedom in the World* score of 9.[48] They argued that those countries above 9 did not have an open enough media system to provoke attacks from the government. Though the authors do not include these countries for other reasons specific to their theory (pg. 41), I wish to demonstrate that countries both below and above this threshold do see government attacks against media. Figure 8 shows box plots of *traditional censorship* and *media harassment* by VonDoepp and Young's threshold. Figures 8a-b show the data in VonDoepp and Young's original sample, while figures 8c-d show the data for all available data from 1972 to 2016. All four box plots indicate a higher mean for the countries above the threshold than those below. Overall, the figures indicate that countries above the threshold do experience government censorship and media harassment. Appendix B shows the box plots for *internet censorship* with similar results. I therefore include all countries with available data in my sample.

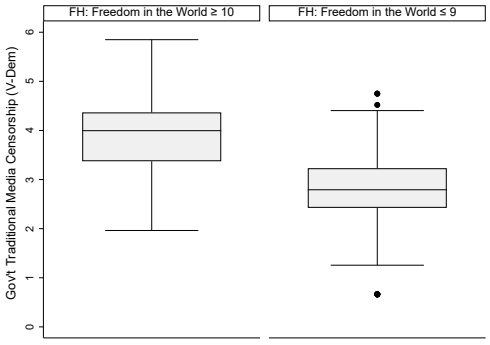
Media Self-Censorship

One implication of my theory holds that these government attacks against media influence journalists' behavior. Increases in government attacks should lead to an in-

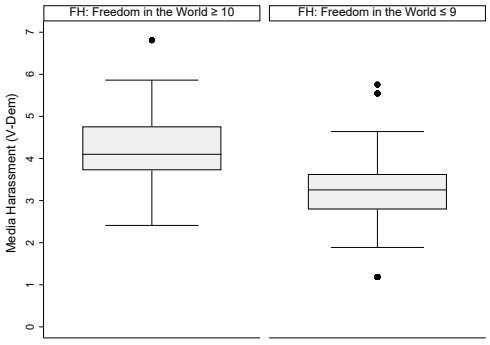
[48] Since 1972, Freedom House has assigned countries a score of 1 through 7 to rate both its political rights and civil liberties for a total score of 14 (Freedom House 2017). Higher scores indicate less freedoms.

Figure 8: Attacks against Media by VonDoepp and Young's (2013) Freedom House Threshold

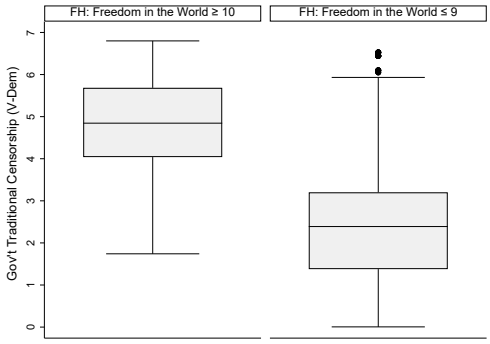
(a) Sub-Saharan Africa, 1995-2009



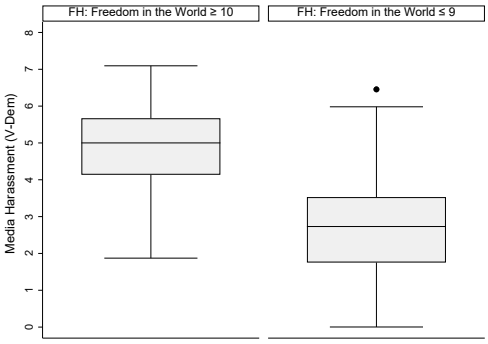
(b) Sub-Saharan Africa, 1995-2009



(c) Worldwide, 1972-2016



(d) Worldwide, 1972-2016



Note: Higher values indicate higher attacks against media. T-tests indicate statistically significant differences ($p < .01$).

crease in journalists self-censorship. I use V-Dem's *media self-censorship* variable to model the prevalence of self-censorship among journalists. V-Dem generates the latent variable by surveying experts on the degree of media self-censorship, then uses a Bayesian item response theory (IRT) model to evaluate their responses and measure the concept. The variable is continuous and runs from 0 to 6.44 with higher values indicating higher media self-censorship levels. While an ideal self-censorship measure would examine actual media content (Stanig 2015), the V-Dem variable offers several advantages. First, I am unaware of another empirical measure that attempts to measure self-censorship both across countries *and* over time. Other studies focus on journalists in a single country or region such as Mexico (Stanig 2015), Central Asia (Kenny and Gross 2008), Ethiopia (Skjerdal 2008), China (Tong 2009), and Indonesia (Tapsell 2009), but V-Dem measures self-censorship worldwide. Second, the variable's latent nature allows me to incorporate a level of uncertainty in the model. This feature is es-

pecially appropriate with measuring self-censorship, as precisely assessing how often journalists decide to withhold information proves a difficult task.

Independent Variable: Judicial Independence

To measure judicial independence, I use Linzer and Staton's (2015) *de facto* judicial independence measure. Noting the numerous weakness of previous measures, the authors use an item response theory (IRT) model to create a time-series, cross-section dataset. IRT models allow the authors to synthesize multiple direct or approximate judicial independence measures. The latent variable encompasses eight different indicators. Five of the eight evaluate judicial autonomy, influence, or both (Keith 2012, Howard and Carey 2004, Cingranelli and Richards 2010, Feld and Voigt 2003, Ríos-Figueroa and Staton 2013). The remaining three indicators provide indirect measures of judicial independence such as Polity IV's executive constraint indicator (Marshall and Jaggers 2010), a general measure of law and order that captures both judicial independence and popular observance of the law (PRS Group 2013), and a proxy measure for property rights protection (Gwartney and Lawson 2007).[49] The judicial independence measure is a continuous variable between 0 and 1, with 0 being the least independent while 1 is the most.

Conditional Term: Electoral Democracy

While I argue that judicial independence reduces government attacks against media, I also argue that citizens ability to remove leaders at the polls conditions this effect. To measure the conditional term, I include V-Dem's electoral democracy variable. It measures rulers' responsiveness to citizens "through electoral competition for the electorate's approval under circumstances when suffrage is extensive" (Coppedge et al. 2017a). Like the other variables from this dataset, V-Dem generates the latent variable by surveying experts on the degree of a country's electoral democracy, then uses a Bayesian item response theory model (IRT) to evaluate their responses and measure the

[49] Ríos-Figueroa and Staton (2013) provide a full description of each variables.

concept. The variable is continuous and runs from 0 to 1 with higher values indicating higher electoral democracy.

V-Dem subsequently uses this measurement as the basis for more expansive democracy conceptualizations, such as liberal, deliberative, and participatory. For my purposes, this variable offers an advantage over other binary or ordinal democracy measures. Its continuous structure allows me to examine subtle changes in electoral democracy. For instance, it does not assume all countries in a single category (such as *democracy* or *dictatorship* for instance) possess the same level of electoral democracy and instead allows for greater variation. This variation will better allow me to detect the ceiling effect about which I have theorized. I also note that it correlates at .8 or higher with binary and ordinal variables that also measure electoral democracy (Cheibub et al 2010, Goldstone et al. 2010, Boix et al. 2013).[50]

Control Variables

Besides judicial independence and electoral democracy, other factors may influence government perpetrated attacks against media. To account for these influences, I introduce several control variables to form the model. First, I include a variable for the media system's ability to criticize the government. I use the Global Media Freedom's (GMF) binary media freedom variable to indicate whether or not journalists work in an open media system (Whitten-Woodring and Van Belle 2017). The authors code countries a 1 when media remain able to criticize government, while a 0 indicates that they cannot. Next, I control for *education* by including V-Dem's secondary school enrollment variable (Coppedge et al. 2017a). Higher education levels lead to a society with greater democratic values (Alemán and Kim, 2015), which perhaps carries less tolerance for government interference in media. This variable measures the percentage of secondary school-aged population enrolled in secondary (high) school.

I also include variables for the *flow of information* (Dreher et al. 2008). This variable composites measures of television per 1,000 people, trade in newspapers as a percentage

[50]I provide these correlations matrices in Appendix B.

of GDP, and internet users per 1,000 people. The variable runs form 1 to 100, with higher values indicating higher media information flows. Next, I include a variable that measures intrastate conflict in a country, taken from the Peace Research Institute Oslo (PRIO) dataset on armed conflict (Themnér and Wallensteen 2011). The variable is ordinal and runs form 0 to 2 with higher scores indicating greater conflict intensity. A 0 indicates no war, 1 represents a minor conflict between 25 and 999 battle deaths, and 2 represents war with at least a 1,000 battle deaths.[51]

Consistent with the literature, I include the amount of foreign aid received as a percent of a country's gross national income (GNI) with data I collected from the World Bank (World Bank 2017). Higher values indicate a higher dependency on foreign aid relative to GNI. In addition, I control for the influence of a country's natural resource reserves by including the value of a country's oil and gas reserves in US dollars (2014). I collect the data from Ross and Mahdavi's (2015) Oil and Gas Dataset. Finally, I control for a country's wealth by including the natural log of GDP per capita in current US dollars (World Bank 2017).

Estimation Approach

My dependent variables' continuous data structure varies both across units and over time. I therefore estimate panel, OLS models with country fixed effects to control for unit heterogeneity. This allows me to assess the effect of the institutional variables within a country over time, an important aspect of my theory. I also include year fixed effects to control for global dynamics that may have occurred in a given year. Also, to account for both the effect of previous years' levels of government attacks against media and the presence of serial correlation, I include a lagged dependent variable on the right-hand side of the equation (Beck and Katz, 2011). My unit of analysis is country-year, and due to data availability my analysis of *traditional censorship*, *media harassment* covers from 1948 to 2012 in the base models without controls and from 1970 to 2012 in

[51] Some country-years saw multiple conflicts. I coded any country-year with multiple conflicts as 2, even if PRIO coded them all as 1. I also run the models using an alternate coding scheme where I code multiple conflicts with a 1 as a 1 for that country-year. My results remain consistent with both coding schemes.

this main models with controls. For *internet censorship* my models cover from 1993 to 2012, and for *media self-censorship* my models include controls and cover from 1970 to 2012. Also, I note that the conditional nature of hypothesis 1 requires for an interaction between judicial independence and electoral democracy. I express the main model's simplified version below:

$$y_{i,t} = \beta_0 y_{i,t-1} + \beta_1 x_{i,t} + \beta_2 z_{i,t} + \beta_3 x * z_{i,t} + \beta_4 a_{i,t} + \nu_i + \varepsilon_{i,t}$$

where y represents different government attacks against media variables, x represents judicial independence, z represents electoral democracy, $x * z$ is the interaction between judicial independence and electoral democracy, a represents a set of control variables including year effects, ν represents unit fixed effects, and ε is a disturbance term.

I estimate the journalist self-censorship model using the following simplified version below:

$$y_{i,t} = \beta_0 y_{i,t-1} + \beta_1 x_{i,t} + \beta_2 z_{i,t} + \nu_i + \varepsilon_{i,t}$$

where y represents media self-censorship, x represents attacks against media variables, z represents a set of control variables including year effects, ν represents unit fixed effects, and ε is a disturbance term. I expect to find the attacks against media's coefficients to be negative and statistically significant in both models.

To evaluate the results, I expect two specific outcomes. First, that judicial independence's coefficient is negative and statistically significant ($p < .05$). This results will show the reductive effect of judicial independence on government attacks against media. However, I also argue that this effect should be absent in high levels of electoral democracy. Therefore, I expect the interaction term to be positive and statistically significant ($p < .05$), indicating the reductive effect itself reduces as electoral democracy levels increase. To further evaluate the presence of a ceiling effect, I will plot the marginal effect of judicial independence on government attacks across levels of electoral democracy. I expect the significant reductive effect of judicial independence on

attacks against media to lose significance at higher levels of electoral democracy.

Results

Main findings

Attacks against Media

Results estimating the various attacks against media data using country fixed effects appear in Table 4 below. Models 1 and 2 estimate *traditional censorship* in both the base model and the main models. In both models, *judicial independence* and *electoral democracy*'s coefficients are negative and statistically significant ($p < .001$) in accordance to my expectations. In addition, the interaction term *judicial independence*electoral democracy* is positive and statistically significant ($p < .001$), also in accordance to my expectations. Findings for *media harassment* in models 3 and 4 mirror these results. *Judicial independence* and *electoral democracy*'s coefficient are negative and statistically significant ($p < .001$), while the interaction term *judicial independence*electoral democracy* is positive and statistically significant ($p < .001$). However, the same results are not present in models 5 and 6 that estimate *internet censorship*. In both models *judicial independence* and the interaction terms are statistically insignificant. However, I note that *electoral democracy* remains negative and statistically significant ($p < .001$) in both models.

In accordance to hypothesis 1, as judicial independence increases, government censorship against tradition media deceases. However the positive and significant interaction terms indicates that this effect is tempered as electoral democracy increases. The same pattern emerges when I examine government harassment against media. While these results point in the direction of the ceiling effect about which I theorized, I graph the interaction term in figure 9 to further investigate. Figures 9a-b both indicate that judicial independence has a significant reductive effect on attacks against media in lower levels of electoral democracy and remains significant until higher electoral democracy

Table 4: Jud. Independence and Govt. Attacks against Media, 1948-2012

	(1) Trd. Media	(2) Trd. Media	(3) Harassment	(4) Harassment	(5) Internet	(6) Internet
Judicial Independence	-.665***	-.742***	-.520***	-.665***	-.076	.038
	(.051)	(.078)	(.044)	(.071)	(.132)	(.157)
Electoral Democracy	-1.241***	-1.423***	-1.052***	-1.210***	-.779***	-.584***
	(.055)	(.078)	(.048)	(.069)	(.111)	(.130)
Jud. Ind.*Elec. Demo.	.845***	.855***	.968***	.980***	.364	-.135
	(.080)	(.124)	(.070)	(.111)	(.209)	(.243)
Open Media		-.037		-.088		-.151*
		(.056)		(.050)		(.060)
Education		.001		.002**		-.001
		(.001)		(.001)		(.001)
Information Flows		.001		-.001		.002
		(.001)		(.001)		(.001)
ln(GDP p/c)		-.01		.025		.058**
		(.015)		(.013)		(.022)
Aid (% GNI)		-.004***		-.004***		-.001
		(.001)		(.001)		(.001)
Resource Wealth		-.001		-.001		-.001
		(.001)		(.001)		(.001)
Conflict		.034**		.028**		-.018
		(.011)		(.01)		(.015)
N	8891	5106	8891	5106	2871	2412
Countries	170	158	170	158	168	157
R^2	.91	.89	.92	.89	.71	.67
AIC	4158.41	1670.91	1431.25	544.97	-1252.94	-1142.22
Years	1948-2012	1970-2012	1948-2012	1970-2012	1993-2012	1993-2012

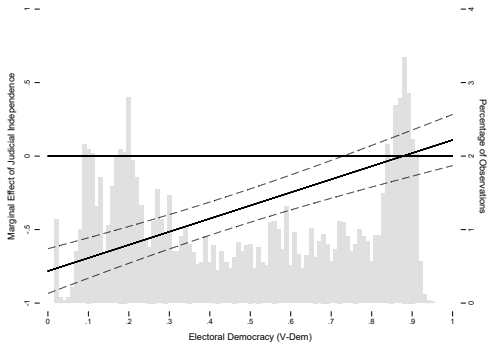
Standard errors in parentheses; Lagged DVs and intercepts not reported; Country and year effects
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

levels when it loses significance. The figures show about .73 as the threshold for *traditional censorship* and about .55 for *media harassment*. In a surprising result, it appears that *judicial independence* actually increases *media harassment* at the very highest levels of *electoral democracy*, starting at about .84 and above.

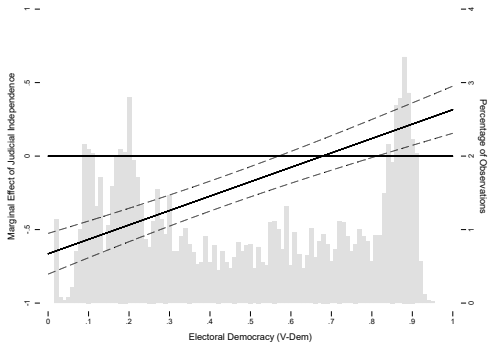
The control variables are not of direct theoretical interest but they reveal some interesting relationships ancillary to my main analysis. *Foreign aid* is negative and statistically significant ($p < .001$) in both the *traditional censorship* and *media harassment* models but insignificant in the *internet censorship* models. The results also indicate *conflict* is positive and statistically significant ($p < .01$) in both the traditional media censorship and media harassment models but negative and insignificant in the Internet censorship models. These results suggest that greater dependence on foreign aid reduces government censorship of traditional media and harassment against media, while years with more intense conflict increases it. However, these factors do not influence

Figure 9: Marginal Effect of Judicial Independence across Electoral Democracy Levels, 1970-2012

(a) Trad. Media Censorship



(b) Media Harassment



when governments decide to censor the Internet.

The models show *open media* is statistically insignificant in the *traditional censorship* and *media harassment* models, but negative and statistically significant ($p < .05$) in the *Internet censorship* model. Countries with media systems that routinely criticize governments see less government internet censorship efforts, though media openness does not seem to significantly affect government censorship of traditional media or media harassment. *Education* is positive and statistically significant in the *media harassment* model ($p < 0.01$), but insignificant in the *traditional censorship* and *internet censorship* models. Also, the natural log of GDP per capita is positive and statistically significant in the *internet censorship* model ($p < 0.01$), but insignificant in the *traditional censorship* and *media harassment* models. Finally, *information flows* and *resource wealth* yield no statistically significant coefficients.

Overall, these results support the hypothesis that judicial independence has a reductive effect on government censorship of traditional media and government harassment of media, but not in higher levels of electoral democracy. The marginal effects plots indicate that a ceiling effect exists, though the ceiling effect's threshold differs between the variables. The results also indicate the ceiling effect is not present in the *internet censorship* models, though electoral democracy has a significant, reductive effect on it.

Media Self-Censorship

Table 5 present the results from the *self-censorship* models. *Traditional censorship*, *media harassment*, and *internet censorship* all return positive and statistically significant ($p < .001$) coefficients. As governments attack media, media self-censorship increases, on average. The models also indicate that *electoral democracy* is negative and statistically significant ($p < .001$). *Education* appears positive and statistically significant ($p < .05$) in all three models, indicating higher education levels positively associate with greater media self-censorship, on average. All other variables in the models including *judicial independence* do not yield statistically significant results.

To discuss these results substantively, I report the percent change in *self-censorship*'s standard deviation when the independent variable moves from the first to the third quartile. When government censorship moves from *self-censorship*'s first to the third quartile, about a 33.8% standard deviation change occurs in media self-censorship. For *media harassment*, a change from the first to the third quartile means about a 29.8% standard deviation change in media self-censorship. Finally, when *internet censorship* moves from the first to the third quartile, about a 8.6% standard deviation change occurs in media self-censorship. In sum, government censorship of traditional media and media harassment have larger substantive effects on journalists decision to self-censorship than government censorship of the Internet.

Robustness Checks

I briefly describe additional robustness checks to the empirical analysis above. The additional estimation approaches and model specifications include incorporating latent variable uncertainty using Monte Carlo simulations, considering alternative electoral democracy measures and the effect of additional confounders, and vector autoregression (VAR) analyses to asses direct Granger causality of media self-censorship and government attacks against media.

Table 5: Media Self-Censorship and Gov't Attacks against Media, 1970-2012

	(1) Self Censorship	(2) Self Censorship	(3) Self Censorship
Trd. Censorship	.211***		
	(.009)		
Media Harassment		.197***	
		(.009)	
Internet Cens.			.06***
			(.013)
Judicial Independence	.09	.043	.02
	(.053)	(.053)	(.081)
Electoral Democracy	-.388***	-.593***	-.840***
	(.059)	(.056)	(.089)
Open Media	.055	.075	-.037
	(.051)	(.051)	(.051)
Education	.003***	.002**	.002*
	(.001)	(.001)	(.001)
Information Flows	.001	.001	.001
	(.001)	(.001)	(.001)
ln(GDP p/c)	.023	-.01	-.002
	(.013)	(.013)	(.019)
Aid (% GNI)	-.001	-.001	-.001
	(.001)	(.001)	(.001)
Resource Wealth	-.001	-.001	.001
	(.001)	(.001)	(.001)
Conflict	.009	.007	.014
	(.01)	(.01)	(.013)
N	5106	5106	2542
Country	158	158	158
R^2	.91	.91	.72
AIC	641.05	739.22	-1454.09

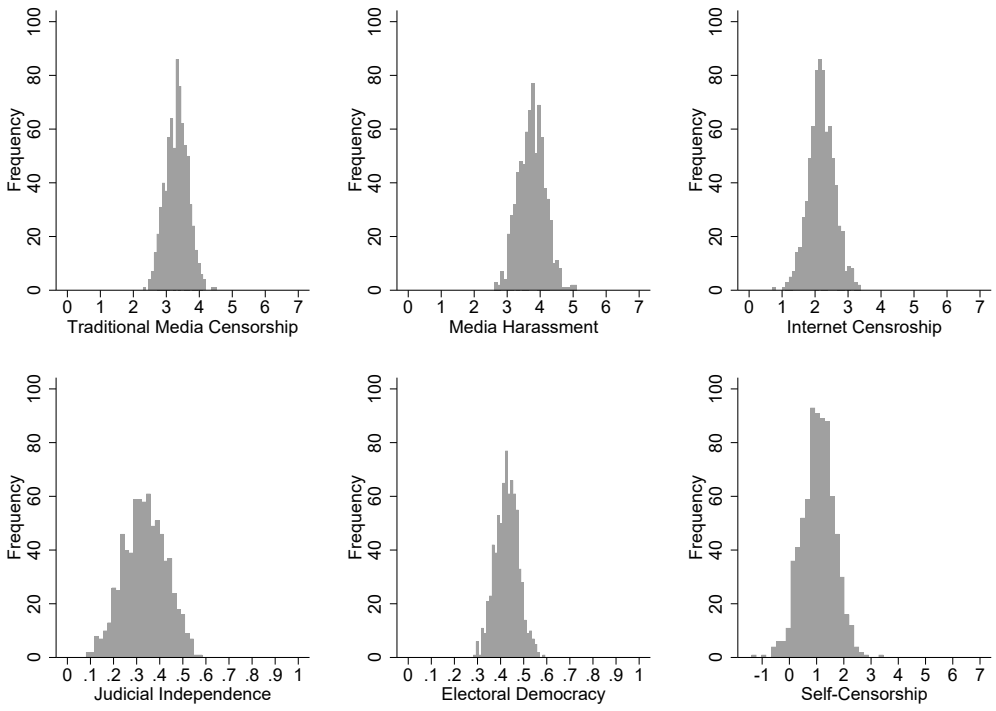
Standard errors in parentheses; Lagged DVs and intercepts not reported;
Country and year effects; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Monte Carlo Simulations

In the main analysis, data managers generated *judicial independence*, *electoral democracy*, and the various government attacks against the media variables using latent class analysis (LCA) item response theory (IRT) modeling. Latent variable models assume researchers cannot observe a concept and instead estimate a posterior distribution around point estimate that represents uncertainty. The models in tables 4 and 5 above

use the means of these posterior distributions, however I incorporate the posterior distribution's standard deviation in a Monte Carlo simulation analysis to take into account the variable's measure of uncertainty. This approach relaxes the assumption that the posterior distributions' means precisely measures the concept and instead incorporates information on raters' reliability. For example, while the models above use Nigeria's 2009 point estimate for *traditional censorship* (3.315), *media harassment* (3.739), *internet censorship* (2.184), *judicial independence* (.335), *electoral democracy* (.795), and *media self-censorship* (1.071), the dataset also includes these observations' standard deviations based on its latent variable posterior distribution. Those standard deviations are: *traditional censorship* (.341), *media harassment* (.394), *internet censorship* (.401), *judicial independence* (.091), *electoral democracy* (.051), and *media self-censorship* (.608). To exemplify the distribution, I use each of the variables' means and standard deviations that I mentioned above and randomly draw 750 observation from a normal distribution. I graph these draws in the distributions that appear in figure 10 below.

Figure 10: Latent Variable Distributions: Nigeria 2009 (750 Random Draws)



Following Pemstein et al.'s (2010) recommendations, I include this information in the models using Monte Carlo simulations.[52] The simulation runs the regression model 750 times and randomly draws from the posterior distribution (see figure 10), estimating a beta coefficient and standard error each time. The model then returns a mean of each variable's beta coefficients and standard errors. I run the simulations for each latent variable in my models in the main analysis: *traditional censorship*, *media harassment*, *internet censorship*, *judicial independence*, *electoral democracy*, and *media self-censorship*. The simulations yield similar results and do not change my inferences. I provide these tables and more detail on this estimation strategy in Appendix B.

Alternative Elec. Democracy Measures and Other Confounders

I also estimate the models using alternative measures for electoral democracy. In accordance with my theory, I only consider variables that emphasize electoral competition and participation and ignore those that evaluate democracy's expansive conceptualization. I use Cheibub et al.'s (2010) Dictator-Democracy (DD) dataset, Boix et al. (2013) (BMR), and Goldstone et al. (2010) measures. DD examines a country-year for four criteria: 1) Voters must elect the chief executive, 2) voters must elect the legislature, 3) more than one party must compete in the elections, and 4) given a country meets the first three criteria, an alternation in power under rules identical to the ones that brought the incumbents to power must have taken place. The authors consider a country a democracy if it meets these four criteria and a dictatorship otherwise.

BMR uses three different criteria based on competition and participation: 1) Voters directly or indirectly elected the executive in popular elections, and the executive remains responsible either directly to voters or to a legislature, 2) Voters choose the legislature (or the executive if elected directly) in free and fair elections, and 3) A majority of adult men have the right to vote. The authors consider a country a democracy if it meets these three criteria and a dictatorship otherwise. Goldstone et al. (2010) provide a four category ordinal measure based on competition and participation. Using

[52]Pemstein et al. (2010) explain how to incorporate a posterior distributions' standard deviation for Monte Carlo simulations here: http://www.unified-democracy-scores.org/example.html

the executive recruitment and competitiveness of political participation components of the Polity dataset, they create four ordinal categories for full autocracies, partial autocracies, partial democracies, and full democracies.[53] The ordinal data run from 0 to 3, with higher categories indicating greater electoral democracy.

The results using these alternative democracy measures exhibit similar patterns as the main analysis. The DD and BMR binary measures make it difficult to detect a ceiling effect, however like the main analysis we find that *judicial independence* is negative and statistically significant ($p < .001$) as well as the interaction term *judicial independence*electoral democracy* returning positive and statistically significant ($p < .001$) coefficients. These variables are statistically insignificant in the *internet censorship* models. The models also returns similar results for Goldstone et at.'s (2010) measure, however these data offer more leverage to examine if the presence of a ceiling effect exists. When I graph the marginal effects of judicial independence on government attacks against the media across the four democracy categories, the figures show a significant reductive effect in autocracies, partial autocracies, partial democracies, but the effect becomes insignificant for full democracies. This effect appears in all three government attacks against media variables — including *internet censorship*. I provide these results in Appendix B. Overall these alternate democracy measures do not change my initial inferences regarding media harassment and censorship, though they do indicate the ceiling effect present for *internet censorship*.

Potential Confounders

Given potential concerns of over-fitting and pairwise sample deletion, I limit the number of control variables in chapter 3's main models. However I also examine several potential confounders identified in the literature, including economic and national capabilities indicators, colonial background, coup events, presidential and national elections, boycotts of presidential and national elections, and protests. Overall, my findings remain robust and remain consistent with the inclusion of these variables. I present

[53] They actually identify a fifth category — partial democracy with factionalism — though I collapse this category into the larger partial democracy category.

these results and variables descriptions in Appendix B.

Self-Censorship and Attacks: Vector Autoregression (VAR) Analysis

Finally, I investigate endogeniety between self-censorship and attacks against the media. While media watchdog groups and the academic literature either imply or outright assert that attacks *cause* self-censorship, governments may actually attack the media more when leaders believe journalists are more likely to remain silent. To explore this empirical question, I utilize both single country and panel vector autoregression (VAR) analysis. VAR analysis estimates multiple time-series variable regressions on both lags of itself and lags of the other time-series variables. According to Box-Steffensmeir et al. (2014), "it asks whether X_t helps predict Y_t over and above your ability to predict Y_t on only the basis of Y_t's past history" (pg. 112). Using the direct Granger specification, the VAR analysis first regresses the dependent variable against lags of the independent variables, as well as the dependent variable. Next, it runs a joint significance test on the lagged independent variables. It repeats this using all specified variables as the dependent variable. Results from this F-test's p-value indicating statistically significance ($p < .05$) offers evidence that a covariate direct Granger causes the dependent variable.

I run these tests to examine empirical causality between *media self-censorship* and *traditional censorship* as well as between *media self-censorship* and *media harassment*. To complete the VAR, I include other variables of theoretical interest: *judicial independence*, and *electoral democracy*. Leaving out other controls allows me to maximize observations in the analysis and cover the years from 1948 to 2012. To identify the number of lags in each model, I use Schwarz's Bayesian information criterion (SBIC), the Akaike's information criterion (AIC), and the Hannan and Quinn information criterion (HQIC). Lower values associated with lags from these tests indicate that they better filter the time series noise from the signal. I use Stata 15's `varsoc` command to run these tests and select the most efficient lag for the simple VAR models, and Love and Zicchino's (2006) `pvarsoc` for the panel VAR models to make this decision.

First, I run panel VARs proposed by Love and Zicchino (2006). Regarding the relationship between *media self-censorship* and *traditional censorship*, statistically significant F-tests ($p < .05$) indicate they simultaneously direct Granger cause each other — F-tests return statistically significant results when the VAR models fit them as dependent variables. I find similar results comparing *media self-censorship* to *media harassment*. Turning to simple, single-country VAR tests, I find similar evidence of simultaneous causality when I run each country individually in a simple VAR. In sum, the results are inconclusive. Evidence from individual country and panel VAR analyses indicate that attacks direct Granger cause *media self-censorship* government attacks against media *and* that government attacks against media direct Granger cause *media self-censorship*. I provide these results with more detail in Appendix B.

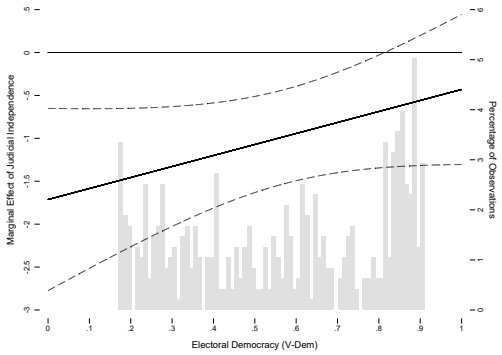
Regional Analysis

In this section I expand my analysis to explore the regional variation of institutional effects on government attacks against media. Mirroring Seligson's (2002) approach, I stratify the sample by world region to determine if micro-level findings support the macro-level results. V-Dem's rich dataset allows for such an analysis, and I expect to find similar results in all regions in accordance with my theory. I stratify the sample into six regions based on Hadenius and Teorell's (2007) 10 world regions classification (Hadenius and Teorell 2007), collapsing the original 10 based on cultural and geographic proximity and consider the following regions: 1) former communist Eastern Europe and Central Asia (EECA), 2) Latin America and the Caribbean (LAC), 3) Middle East and North Africa (MENA), 4) Sub-Saharan Africa (SSA), 5) Western Europe including the United States, Canada, Australia, and New Zealand (WENA), and 6) Pan-Asia (Asia) including South, Southeast, Pacific, and East Asia.[54] I run the analysis for *traditional censorship* and *media harassment* but only report the marginal effects for each region using media censorship in figure 11 below to save space. I provide the regression tables and results for *media harassment* in Appendix B.

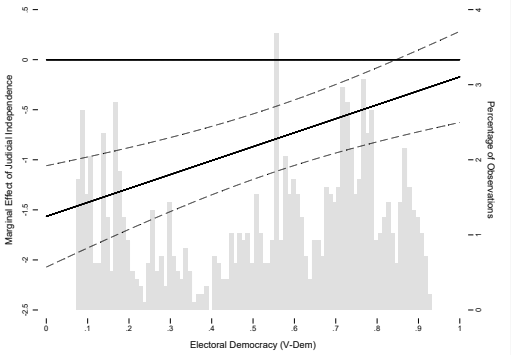
[54] I provide a detailed list of each group in Appendix B.

Figure 11: Marginal Effect of Jud. Independence across Elc. Demo. Levels, Regional

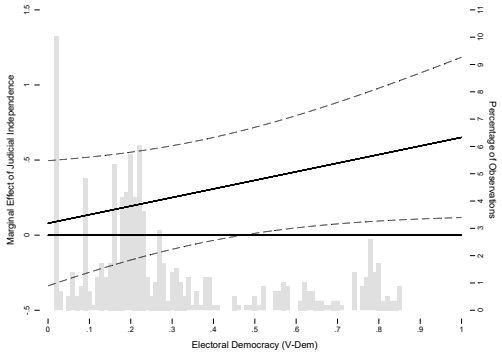
(a) E. Europe/former USSR



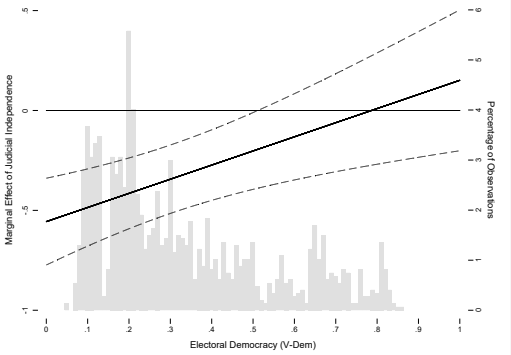
(b) Latin America/Caribbean



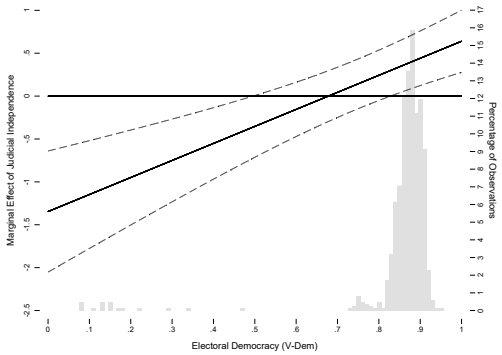
(c) Middle East/N. Africa



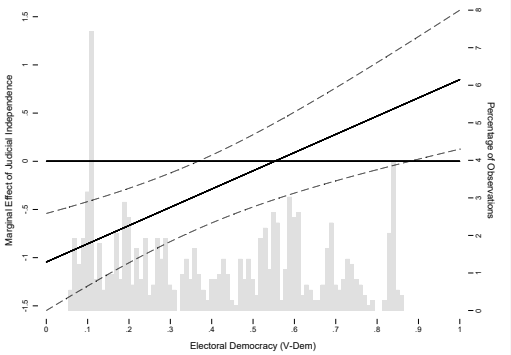
(d) Sub-Saharan Africa



(e) W. Europe/N. America



(f) Asia



The regional stratifications modeling *traditional censorship* largely return results similar to the main findings. EECA, LAC, SSA, WENA, and Asia largely mirror the macro-level results. In EECA, *judicial independence* has a reductive effect on government censorship when countries have less than a .81 score in *electoral democracy*. The same ceiling effect exists in LAC with an *electoral democracy* score less than .84, .51 in SSA, .48 in WENA, and .35 in Asia. Countries in the MENA do not exhibit judicial independence's reductive effect at any electoral democracy score.

I highlight that these results indicate the ceiling effect varies by region, with EECA and LAC displaying the highest ceilings, while SSA, WENA, and Asia display a lower ceilings. Surprisingly, a number of observations actually indicate judicial independence's *positive* effect in the highest levels of electoral democracy in WENA and MENA. Overall, the regional analysis shows the results generally hold by region. Though the ceiling moves depending on the region, all but MENA show the effect. However, I note that a ceiling effect does exists for MENA in the *media harassment* analysis in Appendix B.

Discussion

These results provide support for the proposed theory that increased judicial independence has a reductive effect on government attacks against media like censorship of traditional media and harassment of media. However, the data further show that these results are conditional on level of electoral democracy, with higher levels seeing the effect's significance diminish. The data therefore indicate a ceiling effect of judicial independence in reducing government censorship of traditional media and media harassment — the effect diminishes at higher electoral democracy levels. These results remain robust to Monte Carlo simulations that incorporate latent variable uncertainty, alternative electoral democracy measures, and additional confounders. Results from the main analysis also indicate that this effect does not exist regarding government internet censorship. Models above that estimate government internet censorship show neither

judicial independence nor the interaction terms significant, though I note that electoral democracy is. Affirmative findings from an alternative electoral democracy measure in Appendix B, however, leads me to conclude that *some* evidence does exists to support the ceiling effect of judicial independence on government internet censorship efforts.

Results also indicate that the ceiling effect varies by region, with former communist Eastern Europe and Central Asia countries as well as the Latin America and Caribbean region have higher ceilings, while countries in Western Europe and North America, Asia, and Sub-Saharan Africa have lower ones. The results also indicate that these government attacks against media positively associate with journalist self-censorship. This result is robust across different attack types, including internet censorship. In addition, I find that government censorship of traditional media and media harassment carry larger substantive effects on self-censorship than government censorship of the Internet.

In that regard, this chapter offers a number of contributions to the media freedom and democratic institutions literature. First, it expands the literature's theoretical state by crafting a theory that develops the role of institutions as they interact in determining when leaders are more likely to perpetrate attacks against media. Specifically, how the threat of leaders being voted out of power by citizens conditions judicial independence's reductive effect on when governments attacks media. Next, it utilizes latent variables and their posterior distributions to make more reliable inferences about the determinants of government attacks against media. Monte Carlo simulations allow me to incorporate uncertainty and relax the assumption that the point estimate measures the variable perfectly. This becomes especially valuable for modeling phenomena difficult to observe like journalist self-censorship. Finally, it addresses an untested assumption that government attacks against media lead to journalist self-censorship. Evidence from regression analysis indicates that the various government attacks against the media positively associate with media self-censorship. I also find that government censorship of traditional media and media harassment have larger substantive effects on self-censorship than government internet censorship.

Overall, this chapter provides greater insight into the institutions that impact government attacks against media and media freedom more generally. The results shed light on the importance of separation of powers in protecting press freedom, a vital tool for citizens to fully participate in a democracy.

Chapter 4 - Journalist Killings and Regime Duration

"Every journalist should know how to tie a tourniquet or how to detect if he is being followed," Óscar Martínez told the Committee to Protect Journalists (Mahoney 2017, 13). The Salvadorian journalist, along with his *Sala Negra* (Black Unit) for the online publication *El Faro*, has covered some of the most sensitive subject matter in the Americas. These topics range from riding on United States-bound trains with desperate immigrants, to interviewing violent gangs in El Salvador's most dangerous streets, or revealing massacres by Salvadorian police. "This is a good place to kill," the journalist said of El Salvador. "If you kill, you will get away with it."

Martínez credits Hostile Environment and First Aid Training (HEFAT) for his ability to mitigate unnecessary danger while pursuing high-risk, newsworthy stories in El Salvador. Developers first created HEFAT during conflict in the Balkans in the 1990s, but the training persists today for journalists who pursue dangerous reporting assignments. El Salvador has not been a war zone since 1992 when its brutal 12-year civil war ended, yet Martínez faces conditions similar to those that war correspondents encounter during conflict.

The violent death of journalists represents a worrisome public health issue (Brambila 2017, 299). So much so that in 2016 the United Nations Human Rights Council adopted Resolution 33/2 on the Safety of Journalists. The resolution provides guidance for countries to prevent anti-media violence and prosecute perpetrators.[55] According to the Committee to Protect Journalists, an average of about 49 journalists have been killed each year for their professional work from 1992 to 2017. The most deaths occurred in 2009 with 76, while the least occurred in 2002 when the media advocacy group logged 21. These killings cause alarm given the crucial role media and media freedom play in the democratic and democratizing processes (Bimber 2002, McConnell and Becker 2002, Jakubowicz and Sükösd 2008).

Previous research links journalist killings to corruption (Bjørnskov and Freytag

[55] *Article 19* "Acting on UN HRC Resolution 33/2 on the Safety of Journalists: Prevent, Protect, Prosecute." November 2, 2017. Available at: https://www.article19.org/resources/acting-on-un-hrc-resolution-332-on-the-safety-of-journalists-prevent-protect-prosecute/

2016) and regime type (Asal et al. 2018). This literature's most provocative finding holds that democracies are *not* safe havens for journalists. Asal et al. (2018) find that journalists are more likely to die for their work in democracies where they enjoy more freedom to openly publish sensitive information than in non-democracies. This finding appears counterintuitive to previous studies that find a positive association between media freedom and democracy (Whitten-Woodring and Van Belle 2017). The literature shows that the introduction of democracy to media systems has the effect of simultaneously increasing media freedom while making conditions less safe for journalists.

In this chapter, I re-examine these findings to address this *press-safety paradox* in democracies. I argue that while regime type remains an important factor in determining journalist killings, *how long* the regime endures plays a more significant role. My theory states that autocracies and anocracies — that is, hybrid regimes or partial democracies — are less likely to see journalists killed as the country remains under that regime type. These regimes possess weak institutions that lack the ability to protect human rights and the physical integrity of media personnel. These underdeveloped institutions affect journalists' decisions to print controversial or sensitive topics when colleagues in their country are killed. As autocracies and anocracies persist, journalists learn that covering certain issues or topics are more likely to lead to greater risk of physical harm. This experience will prompt them to eventually avoid such coverage to mitigate their own risk. As a result, journalist killings should decrease as time under the regime unfolds. I also argue that in democracies, citizens' more direct participation creates a demand for critical information about government that journalists must supply. This, coupled with journalists' view that their profession plays an important role in democracy, should not mitigate journalists' risk taking in news gathering and distributing as in other regimes. However, more consolidated democracies, where protective institutions are strongest, should see less killings.

Using an original dataset of journalists killed for their work from 1992 to 2014, I test my theory using regression and time-series analysis. I first reanalyze Asal et al.'s (2018) main analysis covering the years 1992-2011. The authors use a battery of categorical

dependent variable models, such as logit, rare events logit (Tomz et al. 1999), ordinal logit, and count models. However, I also include a variable that counts the number of years a country has remained an autocracy, anocracy, or democracy and find that as regime types endure, the number of journalists killed decreases. I then estimate a negative binomial regression model on a different sample that extends the time coverage to 2014 and find similar results. I also stratify the sample, analyzing it by regime type, and find the results hold in autocracies and anocracies, but not in democracies. The data also suggest that within democracies, killings decrease as democracy level increases. I repeat the analysis using the Cox proportional hazards regression model and find similar results.

These findings add to our understanding of how institutions influence the physical integrity of journalists. Regarding the press-safety paradox and democracies specifically, the data show the probability of journalist killings does not change as democracy endures, but that higher democracy levels significantly reduces it. In sum, democratic consolidation — not duration — reduces the number of journalist killings in democracies.

The Press-Safety Paradox

The democratization literature largely sees media freedom as an important component to democracy (Whitten-Woodring and Van Belle 2017). While democracy's minimalist definition excludes media's ability to function without restriction (Alvarez et al. 1996, Przeworski et al. 2000, Cheibub et al. 2010), it serves as an important component in the broader definition which includes concepts of human rights and civil liberties (Dahl 1973; 1989). Media freedom's inclusion in the latter perspective suggests that mass democracy would not be possible without the media-provided "free exchange and flow of information" that citizens' require for elections (Bimber 2002, 11-12).

However, studies that examine the relationship between democracy levels and physical risk to journalists paint a wholly different picture. Previous findings indicate that

as democracy levels increase, the probability of a country seeing journalist killings increases (Asal et al. 2018). The government perpetrated attacks against media literature also finds that as democracy level increases, the number of government perpetrated attacks against media increases during key political events in hybrid and new democracies in Sub-Saharan Africa (VonDoepp and Young 2013).

These two literatures analyze different aspects of the same media systems and find that democracy has the simultaneous effect of both increasing the freedom and openness of media systems while paradoxically decreasing safety for the journalists operating in the media system. Asal et al. (2018) argue the openness that democracies give media outlets provide journalists more opportunity to investigate sensitive topics, including crime, corruption, or officials' abuse of public office. They argue this heightens journalists' risk as they work to produce news content.

While the authors find a strong positive effect for democracies and journalists killed, and a negative and null effect for autocracies and anocracies, scholars should interpret their results with caution. First, they assume that all regime types display no variation in the time they have remained a regime type. For instance, Asal et al. (2018) treat long standing autocracies like Saudi Arabia and Cuba the same as short lived ones like Gambia (which they classify as an autocracy only briefly from 1994 to 1996) in their 1992 to 2008 sample. They also treat long standing democracies like the United States the same as more recently transitioned ones such as Mongolia. Modeling variation both *between* regime type and *within* regime types will provide a better understanding of how regime types affect journalists killings.

In this study, I argue that how long a country endures as a regime type explains some of the variation of journalist killings. I then consider the institutional variation that different regime types possess and how they influence journalist killings. Institutions in democracies feature more veto players and checks on government behavior that remain absent or partially absent in other regimes (Schultz and Weingast 2003). For instance, the democratization literature sees the separation of powers, namely between the executive and the judiciary, as a key component to democracy and governmental

accountability (Collier 1999, Kenney 2003). Its absence harms democracy's quality in democratizing countries (Gloppen 2003, Senghore 2010). Legal scholars and political scientists point to judicial independence as a safeguard for human rights (Cross 1999, Keith 2002, Skaar 2011, Sakala 2014), while its absence may have negative consequences for press freedom (Ogbondah 2002, Orgeret and Ronning 2009). Below I advance a theory that explains the relationship between regime-type duration, journalists killings and how institutions among the regime type influence this relationship.

Institutional Environments that Influence Journalism

In this section I argue that as a country's regime type endures, the likelihood of journalists killed for their professional work decreases. I first describe the content creation calculus (CCC), the process through which journalist produce news stories, editorials, and other items in publications and broadcasted programs. I then describe how journalist update their CCC as they experience working in a new regime type. When deaths occur in the country, journalists reduce or completely cease from covering topics that pose the most danger. I then turn from a discussion on general transitions to examine how specific regime type transitions impact the CCC after journalists observe the killing of their colleagues.

I build my theory based on two assumptions. First, I assume that while journalists seek to publish or air newsworthy content, their resolve to do so diminishes in the face of blatant risk of serious injury or death. That is, when journalists produce stories or editorials they do so largely intending to avoid physical harm (though they may expect to face censorship or legal prosecution). Second, I assume that as a country reaches a regime type of progressively higher democracy levels, journalists on average will expect greater media openness. For instance, the average journalist in autocratic regimes will expect less openness than the average journalist in anocracies, while the average journalist in the democratic regime will expect the most of the three.

Journalists air or publish content based on a series of decisions that weigh a vari-

ety of concerns including different topics' newsworthiness, their outlet's financial constraints, the input of editors, as well as the political, legal, and institutional conditions under which they work. This content creation calculus (CCC) is responsible for a wide variety of content that ranges from hard news to soft news.[56] For example, while an outlet might present an exposé on a corrupt politician in one portion of the newspaper or broadcast, it may also cover sports or run a story on a trivial but entertaining topic like the World's Ugliest Dog Contest to boost listeners, viewers, or readers who might otherwise not follow government or politics.[57] Though media systems exist with diverse outlets, degrees of openness, and institutional regimes, journalists worldwide still make this CCC before distributing content.

As long as journalists create content, a CCC exists even as the country transitions to a new regime. Initially after the transition, powerful members of society, both governmental and non-governmental, do not necessarily make clear which topics they prefer journalists to avoid or might even send vague, mixed signals regarding these boundaries. For instance, when Fidel Castro took power in Cuba in 1959, he proclaimed freedom of speech and the press but added that words from the press and individuals will always be judged "through the prism of the Revolution" (Liss 1994, 129-35). In a retrospect after the leader's 2016 death, French-based media watchdog group Reporters without Border proclaimed Cuban media one of the most censored under Castro and cited his tenure as rampant with flagrant media freedom violations.[58]

News stories can potentially provoke attacks against media from both government and non-government actors, including physical harassment, censorship, legal prosecution, imprisonment, threatening advertisement funds, or killings. As time unfolds under the new regime, journalists observe the reactions that certain news stories provoke from

[56]Reinemann et al. 2012 note that the concepts of hard and soft news remain difficult to define and often involve a judgment on quality and content importance. It may also contain several dimensions. I use Patterson's (2000, 3-4) conceptualization. He sees hard news as "breaking events involving top leaders, major issues, or significant disruptions in the routines of daily life," and soft news as " more personality-centered, less time-bound, more practical, and more incident-based."

[57]Here the *New York Times* covers the World's Ugliest Dog Contest in 2016: https://www.nytimes.com/2016/06/26/us/here-she-is-the-ugliest-dog-in-the-world-2016.html.

[58]*Reporters Withou Borders* https://rsf.org/en/news/fidel-castros-heritage-flagrant-media-freedom-violations

powerful individuals and organizations. Through this experience, journalists undergo a learning process when powerful government and non-governmental actors reveal, by their actions, which topics they prioritize as too sensitive. Editors and journalists take note and either reduce or cease covering those topics they find most dangerous. For example, in the 1980s and 1990s the Masire-led government in Botswana generally tolerated media criticism of corruption, national security, and policing matters, but had much less tolerance on labor disputes and the publication of classified documents (Zaffiro 1993).[59] No official government policy existed in Botswana, though media found which topics to cover and which to avoid by experience.

While anti-press violence provokes fearful reactions from journalists, death remains the ultimate and most extreme form of censorship (Brambila 2017, 298). Fear induces anxiety and encourages risk-averse behavior (Lerner and Keltner 2001, Huddy et al. 2005). Killings can therefore heighten journalists' anxiety and prompt them to alter coverage of newsworthy topics that they might otherwise cover. For example on January 26, 1983, Peruvian authorities blamed the insurgent group *Sandero Luminoso* for killing seven journalists covering the conflict in the rural part of Peru's Ayacucho region. The effect "discouraged other journalists from venturing out of Ayacucho city" to further cover events pertaining to the uprising (Barnhurst 1991, 83). Even the mere threat of violence can induce journalists to self-censor in free and open media systems like Sweden (Nilsson and Örnebring 2016)

Until now, I have made a general argument about journalist killings and transitions to new regime types. I now turn to discussing the process in specific regimes. Democracies, autocracies, and anocracies by definition possess institutional variation (Epstein et al. 2006). This variation includes the constraints executives face from other government institutions, political competition and participation, as well as strength of judiciary. These checks also provide mechanisms to protect vulnerable elements of society. Weakened or absent checks adversely affect journalists. The courts in particular protect human rights integrity when *de facto* conditions allow judges to act independently

[59]Quett Masire was president of Botswana from 1980 to 1998.

(Cross 1999, Keith 2002, Hathaway 2007, Powell and Staton 2009). Judicial independence also protects against media harassment and censorship when electoral democracy remain unconsolidated (Solis 2018).

Regimes where courts cannot act independently might not protect journalists, in turn promoting impunity. Respect for the rule of law — that authorities do not arbitrarily apply the law — closely ties to an independent judiciary. The rule of law itself varies among regime types, with greater respect for the rule of law in democracies than in non-democracies (Clague et al. 1996, Li 2006). Impunity against violence directed at media not only controls media coverage but increases attacks in places with weak or non-existence law and order (Witchel 2017, 3). Below I briefly explore the regime variability that influences a journalist's CCC when killings of their colleagues occur.

Autocracies possess the weakest constraints on executives, as well as the least respect for rule of law. In this environment, government and non-governmental actors respect human rights and citizens' physical integrity the least. These weaker institutions should therefore greatly inform the learning experience that influences journalists' CCC. Not only do they experience heightened anxiety due to colleagues being killed in their country, but they remain acutely aware that the institutions that exists do little to protect them and contribute to greater impunity for anti-press violence.

A similar dynamic exists in anocracies. There, citizens may elect leaders into office with various degrees of competition and participation, but those leaders face weak institutional constraints once they take power. Though institutions that protect media like courts that enforce the rule of law exist to some degree, they are still not entirely reliable to protect media or deter violence. Journalists enter anocracies, like autocracies, acutely aware that institutions are not always reliable enough to protect them or deter violence. When a killing occurs, these institutions influence them to avoid risk-seeking behavior. This institutional environment should influence the CCC as journalist exists in anocracies — similar to autocracies.

Finally, an entirely different dynamic exists regarding institutions, journalist killings, and the CCC in democracies. While stronger judiciary constraints and a greater respect

for rule of law exist in democracies, journalist killings still occur. However, the high extent of political participation and competition for government office distinguishes democracies from other regime types (Dahl 1972). To make decisions at the polls, citizens in democratic settings require information to evaluate incumbents, their policy outcomes, as well as the opposition and challengers to their office. Journalists not only supply this vital information but consider their profession a vital part of democracy itself. South African journalist Ferial Haffajee described the hope and optimism that democracy ushered in after decades of oppressive apartheid rule in her home country. She remarked, "As a journalist who worked in decidedly un-free times and who reported the arrival of freedom, I have relished the protection of our Constitution as a vital institution in our society" (Haffajee 2014). An expectation for greater freedom encouraged Burmese journalists to push the limits of their reporting following Myanmar's first democratic elections in 2010 after nearly five decades of firmly autocratic rule (Crispin 2011). The number of new publications founded by media entrepreneurs increased in the Southeastern country around this time as well. Jesús Abad, a Colombian journalist who covered conflict in his native country, said of his profession "I work in Colombia, and I have learned [...] that democracy and the practice of journalism is a commitment to life" (Abad 2006). He made these comments in 2006 even after a Colombian guerilla group kidnapped him while on assignment in 2000.

Citizens' demand for vital information in democracies *and* the professional duty journalists carry to perform this task compels them to cover uncomfortable topics even after they receive threats, encounter violence, or murderers kill their colleagues. Killings should therefore affect the average journalist's CCC less in democracies even if institutions do not fully protect journalists' physical integrity. Overall, their willingness to publish sensitive or uncomfortable information should therefore remain about constant even as democracy endures *and* journalist colleagues are killed in their country. However, the strengthening of institutions like independent courts and rule of law should reduce killings by offering journalists increased protection. This implies that simply remaining a democracy does not reduce journalist killings, but that strengthening and

consolidating democracy should.

In sum, I argued above that as a country's regime type endures, the likelihood of journalists killed for their professional work decreases. Initially, the level of uncertainty exists regarding the stories and topics powerful government and non-governmental organization consider sensitive or off-limits. However, working under the regime teaches journalists these boundaries. This experience influences journalist to update their content creation calculus (CCC), and they learn to reduce the coverage of these controversial topics to avoid it completely. While this effect should exist in countries at-large, regime-type variation in the institutions that protect media should mean the outcome differs among autocracies, anocracies, and democracies. In democracies specifically, journalist killings should decrease as democracy level increases and a country's democracy consolidates.

From this discussion, I derive the following hypothesis:

Global Hypothesis

H1: *As regime-type duration increases, journalist killings will decrease.*

Regime type Hypotheses

H2: *As autocracy duration increases, journalist killings will decrease.*

H3: *As anocracy duration increases, journalist killings will decrease.*

H4a (Null Hypothesis): *Democracy duration is not related to journalist killings.*

H4b: *As democracy level increases, journalist killings will decrease in democracies.*

Below I describe a research design to empirically evaluate these hypotheses.

Research Design

Dependent Variable: Journalist Killings

Above I argue that as countries remain a regime type, the number of journalists killed is more likely to decrease. I also argue that different regime types influence this

outcome. To test these hypotheses, I utilize the Committee to Protect Journalist's (CPJ) database on journalists killed. The media watchdog and advocacy group began recording journalist killings in 1992 and continues to the present. CPJ researchers investigate each reported death to confirm that their killing resulted from their professional work.[60] The advocacy group also reports the name, location, date, and other relevant information regarding the journalist's death.

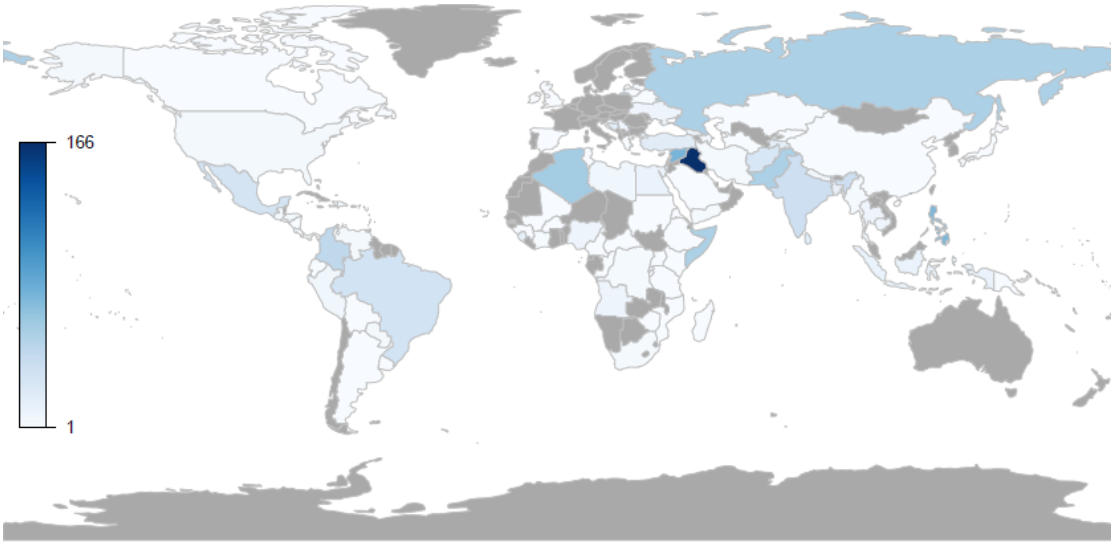
Due to data availability, I examine journalists killed from 1992 to 2014.[61] From 1992 to 2014, 1,109 journalists were killed for their journalistic work in 92 countries worldwide. CPJ demarcates the type of deaths as murders, dangerous assignments, and cross-fire.[62] In my sample, the most journalist deaths resulted from murders (732), while crossfires (229) and dangerous assignment (144) follow. The top five countries with journalists killed includes Iraq (166), Syria (79), Philippines (77), Algeria (60), and Pakistan (56). However, many of these killings occurred on dangerous assignment or resulted from cross-fire during conflict. The top five countries regarding strictly murdered journalists includes Iraq (103), Philippines (75), Algeria (58), Colombia (42), and Somalia (39). Figure 12 below maps the total number of journalists killed from 1992 to 2014.[63] I note that the figure shows journalists killings generally occurred throughout different geographical regions worldwide.

Independent Variables: Regime-type Duration and Democracy Level

To measure how long a country remains a regime type, I create a *regime-type duration* variable that counts the years a country remains an autocracy, anocracy, or democracy throughout this sample. I draw the data from *polity*, which utilizes a 21-point score to evaluate democracy level focusing on institutional constraints (Marshall and Jaggers 2017). Running from -10 to 10, higher scores indicate higher democracy levels. Using *polity*'s classification, I specify a country an autocracy for scores between -10 and

[60] CPJ also tracks journalist killings where the motive remains unclear. They classify these deaths as "unconfirmed." I only analyze those the advocacy group considers "confirmed."
[61] Data collected at *https://cpj.org/data/killed* through December 2017.
[62] CPJ also has an "unknown" category but only log 4 cases in this category.
[63] Appendix C further explores descriptive analyses of CPJ's journalist killings data.

Figure 12: Cumulative Journalists Killed Worldwide, 1992-2014



Note: Gray indicates no journalist killed from 1992-2014.

-6, anocracy with scores between -5 and 5, and democracies between 6 and 10 from the beginning of the dataset. From here, I generate a variable counting the number of years a country remains under a regime type. I do not left censor the count and begin at *polity*'s earliest year 1800. For example, long-time democracies like the United States and Denmark had larger counts than newer democracies like Lithuania and Uruguay, though all four countries exist as democracies from 1992 to 2014. Once I generate the variable, I take the natural log given it heavily skews right.

I also use *polity*'s 21-point scale to measure democracy level for two reasons. First, its regime type conceptualization focuses more on institutional constraints — a key emphasis in my theory — rather than other attributes such as participation or political liberties (Munck and Verkuilen 2002). Second, previous authors utilize it in studies of journalists killings to measure democracy (Asal et al. 2018). I note that though *regime-type duration* and *polity* originate from the same variable, they only correlate at about .12.

Control Variables

Besides a regime type's durability and democracy level, other factors may affect journalist killings. To account for these influences, I introduced several control variables into the model. First, I include a variable for *quality of government*. I collect this variable from the International Country Risk Guide's (ICRG) quality of government data that evaluates a country's corruption, law and order, and bureaucratic quality levels (PRS Group 2013). Higher scores indicate a higher quality of government. Next, I include CIRI's *physical integrity rights* variable that measures government's propensity for torture, extrajudicial killing, political imprisonment, and disappearance (Cingranelli and Richards 2014). Higher scores indicate government's respect for their citizen's physical integrity. Also from CIRI, I use their *freedom of speech* variable, with higher scores indicating higher levels of freedom of speech (Cingranelli and Richards 2014). Next, I include a variable that measures intrastate *armed conflict* in a country, taken from the Peace Research Institute Oslo (PRIO) dataset on armed conflict (Themnér and Wallensteen 2011). I also include variables for the *flow of information* (Dreher et al. 2008) as well as the natural log of *population* (World Bank 2017).

While this model mirrors Asal et al.'s (2018) analysis of regime type and journalists killed, I find two issues with their variable choice. First, CIRI's principal investigators have retired the dataset which prevents model replication beyond 2011. Second, ICRG's data omits a large number of countries from its dataset. In order to expand the sample as well as make it reproducible over a longer time period and through more countries, I substitute other variables that approximately measure the same concepts. For the CIRI variables, I utilize Varieties of Democracy's (V-Dem) *physical violence* and *freedom of expression* variables (Coppedge et al. 2017a). For ICRG, I use V-Dem's *public sector corruption* (Coppedge et al. 2017a). I run models using both sets of variables.

Estimation Approach

I use a number of different approaches to evaluate my hypotheses. First, I replicate Asal et al.'s (2018) table 1, which analyzes journalist killings using different categorical

dependent variable models, including logit, rare events logit (Tomz et al. 1999), ordinal logit, and count estimations. Following the authors, I create a number of different dependent variables for the analysis. I create a binary variable, coding a country 1 if it experienced at least one journalist killing in a year, and a zero otherwise. Also, I create two separate ordinal variables. For the first, I assign a 1 to every country with a single journalist killed in a year, a 2 to any country with 2 or more journalists killed in a year, and a 0 otherwise. For the second, I assign a 1 to every country with 1 to 9 journalists killed in a year, a 2 to any country with 10 or more journalists killed in a year, and a 0 otherwise. With these variables, I estimate logit, rare events logit, and two different ordinal logit models. With the raw count data, I also estimate negative binomial regression (NBR) and zero-inflated negative binomial (ZINB) regression models. Mirroring the authors, I use *log population* to predict "always zero" variables for the ZINB model (Long and Freese 2006). In addition, I use year effects and cluster the standard errors by country for each model. I expect *regime-type duration* to be negative and statistically significant in all six models, in support of hypothesis 1.

For the second approach, I employ NBRs in an extended model that replaces the original *quality of government*, *physical integrity*, and *freedom of speech* variables with their corresponding V-Dem proxies. For the main analysis, I present results for the NBRs because it best models the unmodified, raw data.[64] The alternative variables expand both the sample's time from 2011 to 2014 and country coverage from 132 to 160. In addition, the number of observations increase from 2,491 to 3,586. I expect to find the *regime-type duration*'s coefficients negative and statistically significant in support of hypothesis 1.

In addition, I stratify the expanded sample by regime type to test hypotheses 2 and 3. I expect *regime-type duration* to be negative and statistically significant ($p < .05$) for the global model in support of hypothesis 1, and similar results in the autocracy and anocracy models in support of hypotheses 2 and 3. Finally, I expect a null-effect for *regime-type duration* in the democracy model to support hypothesis 4a, while I

[64] I consider other estimation approaches in Appendix C.

expect *polity*'s coefficient to have a negative and statistically significant (p$<$ .05) effect in accordance to hypothesis 4b. I use year effects and cluster the standard errors by country for each model.

Finally, I utilize a Cox proportional hazards model for the time duration until a country sees a journalist killed. The Cox regression model is semiparametric and allows me to evaluate the influence of covariates on the survival time in a country until a journalist is killed (Cox 1972). I also allow for multiple failure-times, which does not right censor data once a killing occurs. This allows for multiple events — that is, a journalist killing — to occur in a single country. Using a threshold of one journalist killed per country-year, my sample carries a total of 439 events.[65] I also include a shared frailty term for each country in the sample country to control for unit heterogeneity. I run the Cox regression model on the global sample, then stratified by regime type. Like the NBRs, I expect a negative, statistically significant ($p < .05$) coefficients for *regime-type duration* in the global, autocracy, and anocracy models in support of hypotheses 1, 2, and 3. I note that a negative coefficient in a Cox regression model implies the hazard rate of journalist killing is decreasing; hence, the survival time until a journalist killing lengthens. I also expect a null-effect in the democracy model in accordance to hypothesis 4a, while I expect *polity*'s coefficient to have a negative and statistically significant (p$<$ .05) effect to support hypothesis 4b. Summary statistics for all the variables I use appear in Appendix C.

Results

Asal et al. (2018) Reanalysis

Table 6 below displays the results estimating the various categorical dependent variable models. In models 1 through 6, all return *regime-type duration* coefficients as

[65]Thresholds beyond a single journalist killed do not provide enough variation to make reliable inferences. I briefly discuss the results where I increase the threshold to 2 and 3 killed and their lack of variation in the robustness checks section. I therefore rely on the negative binomial regressions for the effect of multiple journalists killed.

negative and statistically significant ($p < .01$). These results indicate that as a country remains a regime type, the probability of a country seeing a journalist killed decreases, holding all other variables constant. The results remain consistent even as I employ different estimation approaches and provide strong support for hypothesis 1. To provide a substantive interpretation, I graph model 1's predicted values of journalist killings across regime-type durations in figure 13. It shows the probability of a journalist killed decreasing as the regime type endures.

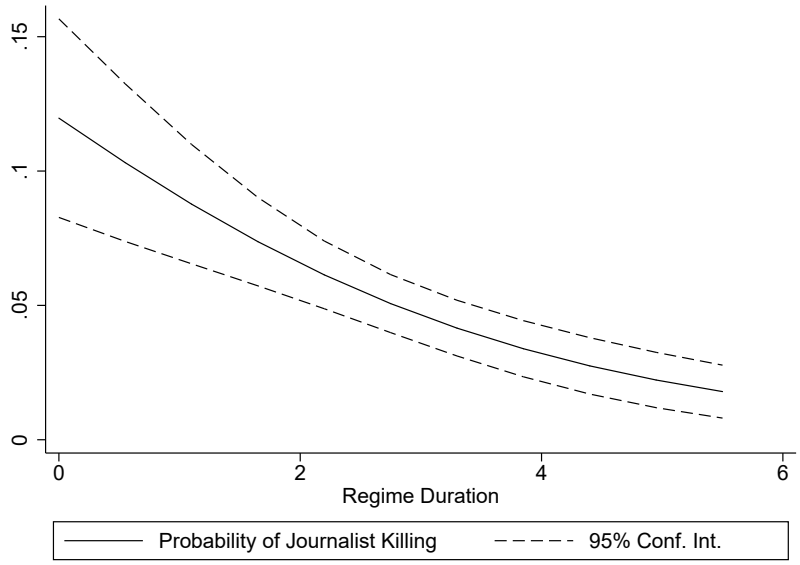
Table 6: Regime-type Duration and Journalists Killed, 1992-2011

	(1) Logit	(2) RE Logit	(3) Ordinal 1	(4) Ordinal 2	(5) NBREG	(6) ZINB
Regime-type Duration (ln)	-.299**	-.292**	-.346***	-.304**	-.415***	-.409***
	(.102)	(.101)	(.105)	(.100)	(.089)	(.086)
Polity Level	.079***	.076**	.077**	.076**	.048*	.049*
	(.024)	(.023)	(.024)	(.024)	(.022)	(.022)
Quality of Govt.	-2.431*	-2.360*	-2.597*	-2.381*	-1.834*	-1.837*
	(1.000)	(.989)	(1.036)	(.973)	(.783)	(.780)
Physical Integrity	-.444***	-.434***	-.442***	-.443***	-.394***	-.393***
	(.081)	(.080)	(.087)	(.081)	(.065)	(.064)
Freedom of Speech	.373*	.364*	.388*	.393*	.320*	.327*
	(.169)	(.167)	(.166)	(.175)	(.138)	(.140)
Armed Conflict	.824***	.797***	.871***	.833***	.877***	.869***
	(.160)	(.158)	(.177)	(.158)	(.142)	(.141)
Information Flows	.035***	.034***	.038***	.034***	.029***	.029***
	(.008)	(.008)	(.008)	(.008)	(.005)	(.005)
Population (ln)	.403***	.394***	.405***	.398***	.373***	.339**
	(.101)	(.099)	(.104)	(.098)	(.083)	(.116)
/cut1			8.263***	8.093***		
			(1.827)	(1.684)		
/cut2			9.562***	13.475***		
			(1.803)	(1.923)		
/lnalpha					.526*	.490
					(.263)	(.285)
inflate						
Population (ln)						-1.014
						(1.031)
N	2491	2491	2491	2491	2491	2491
Countries	132	132	132	132	132	132
AIC	1224.296	-	1562.543	1258.791	1945.725	1949.184

Standard errors (SE) in parentheses; Intercepts not reported; SE clustered by country; Year effects;
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The control variables are not of direct theoretical interest but they reveal some interesting relationships ancillary to my main analysis. *Polity* remains positive and statistically significant throughout the models, indicating that as democracy level rises, the probability of a journalist killed in a country decreases. This result is consistent with Asal et al.'s (2018) findings. Also consistent with their findings, *quality of government* is negative and statistically significant ($p < .05$) through the different model specifications. *Physical integrity* is also negative and statistically significant ($p < .01$), while *freedom of speech* is positive and statistically significant ($p < .05$). Finally, *armed conflict*, *information flows*, and *population* are all positive and statistically significant.[66] Even with the inclusion of *regime-type duration*, the results generally remain consistent with Asal et al.'s (2018) initial findings.

Figure 13: Predicted Values of Journalist Killed across Regime-type Duration, Model 5



Expanded Model, 1992-2014

I report the results for the expanded model using the V-Dem variables in table 7. I focus here on the negative binomial regression because it more closely reflects the raw data without arbitrary modifications. Model 7 returns *regime-type duration* negative

[66] I note that the inclusion of *regime-type duration* renders the inflator variables *population* insignificant, contrary to Asal et al.'s (2018) initial findings.

and statistically significant ($p < .001$). As a country's regime type endures, the probability of a journalist killed decreases, holding all other variables constant. To provide a substantive interpretation, I graph model 7's predicted probabilities of a journalist being killed across regime-type duration in figure 14. It shows the probability decreasing as the regime type endures, and largely mirrors figure 13, though the effect appears slightly less.

While the results indicate the controls yielding largely comparable outcomes, *polity* is notably statistically insignificant. Contrary to Asal et al.'s (2018) main finding, democracy level *does not* have a positive, statistically significant effect on journalist killings in the expanded sample. We also find that *public sector corruption*, a proxy for government's quality, is no longer statistically significant.

I argued above that as countries remain a regime type, the country's media system should see less journalist killings. However, I also argued that regime types feature institutional differences that mediate journalists CCC differently, which affects the incidence of journalist killings. I therefore hypothesized that in autocracies and anocracies, *regime-type duration*'s negative and statistically significant effect will remain, while it likely does not in democracies. Models 8, 9, and 10 in table 7 stratify the sample by regime type — autocracy, anocracy, and democracy respectively.

The results are as expected. For autocracies in model 8, the effect of regime-type durability persists. *Regime-type duration*'s coefficient is negative and statistically significant ($p < .05$). The same effect exists for anocracies in model 9 with *regime-type duration*'s coefficient negative and statistically significant ($p < .001$). Both models indicate that as countries remain autocracies and anocracies, the probability that a country-year sees a journalist killed for their work decreases, all else equal. These results support hypotheses 2 and 3. Also consistent with my expectations in hypothesis 4a, model 10 indicates that the result does not hold when I examine only democracies. *Regime-type duration*'s coefficient is positive and statistically insignificant ($p = .401$). The data also show *polity* negative and statistically significant ($p < .05$) in the same model, supporting hypothesis 4b.

Table 7: Regime-type Duration and Journalists Killed, 1992-2014

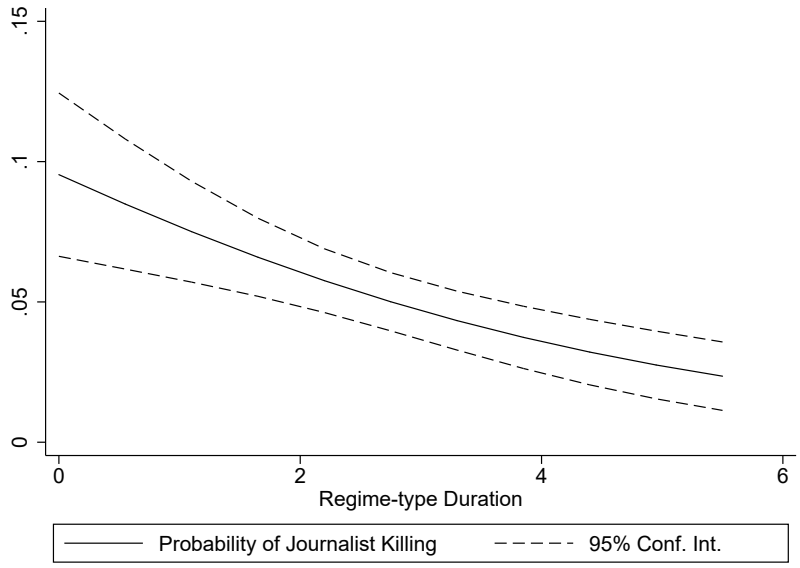
	(7) Global	(8) Autocracy	(9) Anocracy	(10) Democracy
Regime-type Duration (ln)	-.311***	-.423*	-.589***	.070
	(.083)	(.207)	(.139)	(.084)
Polity Level	.009	.053	.032	-.318*
	(.026)	(.205)	(.038)	(.130)
Public Sect. Cor., V-Dem	.562	-1.194	.349	.746
	(.554)	(1.168)	(.729)	(.683)
Physical Integrity, V-Dem	-4.803***	-3.623*	-4.281***	-6.333***
	(.743)	(1.446)	(1.071)	(.847)
Freedom of Exp., V-Dem	3.499***	2.075	3.776***	4.492***
	(.717)	(1.734)	(.883)	(1.218)
Armed Conflict	1.324***	2.056***	1.243***	.632***
	(.130)	(.322)	(.154)	(.141)
Information Flows	.029***	.038**	.031***	.025***
	(.006)	(.013)	(.008)	(.006)
Population (ln)	.394***	.232	.256*	.539***
	(.072)	(.134)	(.104)	(.092)
/lnalpha	.770***	1.170*	.498	-.260
	(.216)	(.535)	(.272)	(.272)
N	3586	597	1067	1922
Countries	160	52	86	106
AIC	2860.384	419.092	1039.686	1346.538

Negative Binomial Regressions; Standard errors (SE) in parentheses;
Intercepts not reported; SE clustered by country; Year effects;
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

For a substantive interpretation of this negative association, I employ incidence rates ratios (IRR). IRRs estimate the dependent variable's rate of change while holding all other variables constant. A one unit increase in *regime-type duration* corresponds with about a 26.7% decrease in expected journalist killings. The effect is even greater in autocracies and anocracies. In autocracies, a one unit increase in regime-type durability corresponds with about a 34.5% decrease in expected journalist killings, while a one unit increase in regime-type durability in anocracies corresponds with about a 44.5% decrease in journalist killings. I present the IRRs for *regime-type duration* in table 8 below. Regarding *polity* in democracies, the IRR is .727, indicating a one unit increase in *polity* decreases journalist killing by about 26.3%.

Results from the controls offer a glimpse into regime variability. *Public sector cor-*

Figure 14: Predicted Values of Journalist Killed across Regime-type Duration, Model 7



ruption, a measure of the quality of government, is positive and statistically insignificant in autocracies and partial democracies but becomes statistically significant ($p < .05$) in the democracy model. The results indicate that quality of government becomes a significant factor regarding journalist killings only in democratic countries. *Physical integrity* is not a significant factor in autocracies, while it is negative and statistically significant ($p < .01$) in both anocracies and democracies. The same is true for *freedom of expression* — it is insignificant in autocracies but statistically significant in anocracies and democracies. *Armed conflict*, *information flows*, and *population* all remain positive and statistically significant throughout models 8-10.

In sum, the results from model 7 in table 7 provide strong support for hypothesis 1. As *regime-type duration* increases, the probability of journalist killings decreases, on average. Results from models 8-10 in table 7 provide support for hypotheses 2 and 3. Findings suggest that as autocracies and anocracies endure, the probability that journalists killed for their work decreases. The results in model 10 also indicate that democracy duration does not influence this probability, supporting hypothesis 4a. Finally, in support of hypothesis 4b, model 10 indicates higher democracy levels decrease journalist killings. Taken together, the length of time a country remains a democracy does not significantly impact journalist killings, but higher democracy levels *does* significantly

decreases these killings.

Table 8: Incident Rate Ratios, Regime-type duration

	Global	Autocracy	Anocracy	Democracy
IRRs	.733***	.655*	.555***	*insig.*

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ (Corresponding coefficient)

Survival Analysis

Below I present results from the Cox proportional hazards models analyzing the survival until a country sees a journalist killing in table 9. Like table 7 above, I first analyze the global sample then stratify by regime type. The findings largely reflect the results from the negative binomial regression models above. Model 11 indicates that an increase in *regime-type duration* decreases the hazard rate and, thus, increases the survival time until a journalists gets killed in a country in the overall sample. Model 12 repeats the analysis in autocracies and finds *regime-type duration* decreases the hazard rate, that is increases the survival time until a journalist gets killed in a country. Model 13 finds *regime-type duration* also decreases the hazard rate, and thus increases the survival time until a journalist is killed. Finally, I turn to modeling survival until a journalist killing occurs in democracies. Model 14 mirrors results from the NBR model 10 in table 7. *Regime-type duration* is insignificant, while *polity* is negative and significant ($p < .01$). The latter indicating that an increase in *polity* increases the survival time until a journalists gets killed.

While these results largely reflect my theoretical expectations, it is important to verify that the models and key variables do not violate the proportionality assumption native to the Cox proportional hazards model. For each model, I perform a visual inspection of the Schoenfeld residuals graphs combined with the use of the Therneau-Grambsch (TG) non-proportionality test (Therneau and Grambsch 2013). The TG test inspects the presence of the residual's non-zero slope, which indicates a violation of the assumption.[67] While my key independent variable *regime-type duration* never vio-

[67]P-values above 0.05 indicate the model does not violate the assumption.

lates the assumption, the democracy model's *polity* variable does. To correct for non-proportionality in this model, I follow Keele (2010) and first attempt to use splines and check for the nonlinearity of the continuous variables that violate the assumption. A second check reveals that this technique results in the correct specification of *polity* and *public sector corruption*. To finish, I interact them with my function of time (Box-Steffensmeier and Jones 2004, Keele 2010). While *polity* yields a positive coefficient in model 15, its interaction with time produces a negative and statistically significant ($p < .01$) coefficient. This suggests an increase in polity level decreases the hazard rate with the passage of time. I also note that the global TG test now indicates the model does not overall violate the proportionality assumption.

For a substantive interpretation, I report the hazard rates for *regime-type duration* in table 10. To calculate them, I take the regression coefficient's exponential $(exp(\beta))$, subtract the result by 1, then multiply by 100 to get the percentage change of the hazard rate $(1 - (exp(\beta) * 100))$. The global sample indicates a one unit increase in *regime-type duration* corresponds to about a 21.2% decrease in the hazard rate, or survival time until a country sees a journalist killed. The effect is greater in autocracies where a one unit increase in *regime-type duration* corresponds to about a 40% decrease in the hazard rate. In autocracies, we find about the same hazard rate as the global sample, with a one unit increase in *regime-type duration* corresponding to about a 24.2% decrease.

Overall, findings from the Cox regression models supports my theory that the duration of regime type decreases journalist killings. The results suggest *regime-type duration* increases the survival time until a country sees a journalist killing in both the overall sample, as well as the autocracy and anocracy samples. However, it does not significantly influence the survival time until a country sees a journalist killing in democracies. Also, consistent with my expectations, level of democracy does significantly influence the survival time until a country sees a journalist killing. Higher levels of democracy decreases the hazard of being killed, which suggests greater democratic consolidation increases the survival time until a journalist is killed among democracies.

Table 9: Survival Analysis: Journalist Killings and Regime Duration, 1992-2014

	(11) Global	(12) Autocracy	(13) Anocracy	(14) Democracy	(15) Democracy
Regime-type Duration (ln)	-.238***	-.509*	-.277**	.133	.143
	(.059)	(.202)	(.104)	(.108)	(.11)
Polity Level	.016	.142	.032	-.265**	477.88**
	(.021)	(.232)	(.034)	(.099)	(177.54)
Public Sect. Cor., V-Dem	.633	-.449	-.155	1.223*	1241.04
	(.401)	(1.286)	(.647)	(.61)	(885.73)
Physical Integrity, V-Dem	-3.502***	-2.294	-3.061***	-4.916***	-4.9***
	(.565)	(1.475)	(.769)	(.903)	(.901)
Freedom of Exp., V-Dem	2.455***	.385	2.638***	2.497	2.57
	(.622)	(1.608)	(.774)	(1.309)	(1.32)
Armed Conflict	.832***	1.338***	.806***	.452**	.472**
	(.091)	(.261)	(.128)	(.151)	(.155)
Information Flows	.016**	.029	.01*	.024**	.024**
	(.005)	(.016)	(.007)	(.008)	(.008)
Population (ln)	.431***	.284	.301**	.551***	.562***
	(.063)	(.152)	(.099)	(.082)	(.084)
Polity Level*Years (ln)					-62.894**
					(23.353)
Pub. Sect. Cor., V-D*Years (ln)					-163.07
					(116.49)
N	3586	597	1067	1922	1922
Countries	160	52	86	106	106
θ	.637	.488	.348	.331	.355
I-likelihood	-1528.1	-103.5	-425.2	-626.6	-622.8
Global TG Test (p-value)	.1020	.580	.5608	.02878	.3281

Cox Proportional Hazard Regressions; Standard errors (SE) in parentheses; Intercepts not reported; SE clustered by country; Year effects; TG Test=Therneau and Grambsch (2001) proportionality test: it tests if residuals of time dependent covariates have a zero slope; P-values$<$ .05 indicates a non-zero slope and suggests the assumption is violated.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 10: Hazard Rates, Regime-type duration

	Global	Autocracy	Anocracy	Democracy
Hazard Rates	.788***	.601*	.758**	*insig.*

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ (Corresponding coefficient)

Robustness Checks

I briefly describe robustness checks to the empirical analysis above. Additional estimation approaches and model specifications include estimating the models using logit, rare events logit (Tomz et al. 1999), ordinal logit, zero-inflated negative binomial regressions, as well as lagged dependent variable models. I also add economic controls, such as GDP, GDP per capita, and change in GDP per capita. In addition, I replace *polity* with the *media system freedom* (MSF) scores as an alternate to society's openness. Finally, I include crime data from the United Nations and the World Bank on intentional homicide counts and rates. Given that my main inferences remain consistent throughout these checks, I provide a more detailed description of these additional tests and their results in Appendix C.

As for the survival analysis above, I note that I only examine an event defined as a single journalist killed. I also ran the analysis with the threshold being two and three journalists killed, however these specifications do not provide enough variation to make reliable inferences. Table 11 shows the number of events per sample using a 1 death threshold versus 2 and 3 deaths. Using 2 or more deaths, the number of events decreases by about 53.7% in the full sample, and 62.5%, 55.8%, and 53% in the autocracy, anocracy, and democracy samples respectively. The decreases become even more pronounced when the threshold is 3 journalists killed. The number of events decreases by about 72.9% in the full sample, and 78.7%, 69.5%, and 76.3% in autocracies, anocracies, and democracies respectively.

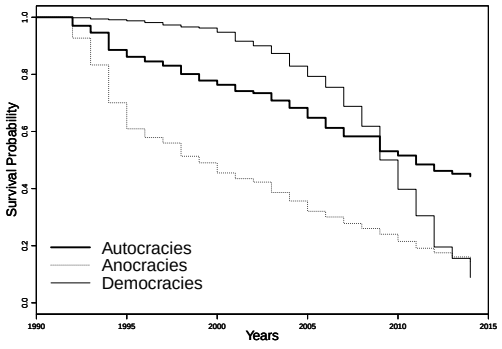
To highlight this point, I compare the Kaplan-Meier survival curves for the different regime type models of the different threshold minimums in figure 15. The plots show the survival patterns of countries not seeing a journalists killed of each model holding its variables at their means. In each figure, the vertical axis indicates the percent of cases that have survived, while the horizontal axis shows the sample's time. Steeper curves indicate higher hazard rates, that is, less probability of survival, while flatter curves indicate greater survival probabilities. Taken together, the figures suggest that variation reduction occurs as a result of increasing the minimum killed threshold. While

Table 11: Events (journalists killed) per country-year, various specifications

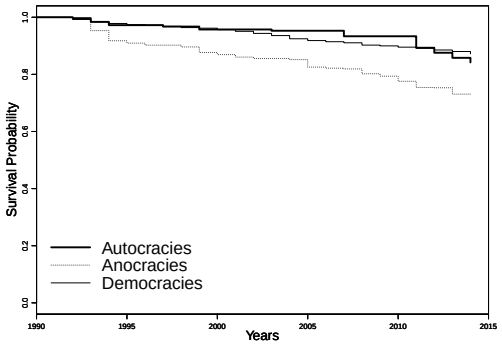
	Global	Autocracy	Anocracy	Democracy
obs	3,586	597	1,067	1,922
Journalist Killed* >=1	439	48	154	219
Journalist Killed* >=2	203	18	68	101
Journalist Killed* >=3	119	11	47	52

Figure 15: Survival Curves w/ Different Event Specifications, Duration until Journalist Killed: 1992-2014

(a) Journalist Killed >=1 (country-year)



(b) Journalist Killed >=2 (country-year)



(c) Journalist Killed >=3 (country-year)

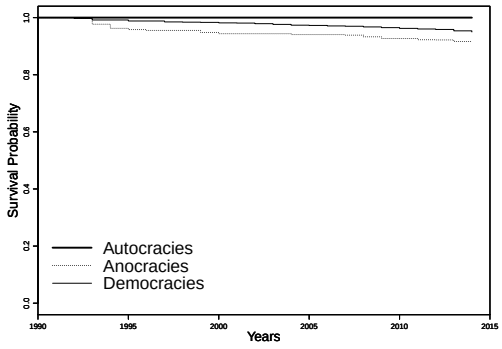


figure 15a, which analyzes a single journalist killing as an event, shows steeper curves, the increased threshold renders all survival curves nearly flat in figures 15b and 15c. Given this lack of variation, I prefer to analyze and report models that specify a single journalist or more killed in a country-year and defer to the negative binomial regression and other count dependant variable models in Appendix C to account for the magnitude of journalists killed in a country-year.

Discussion

The results provide support for my proposed theory that as regime types endure, journalists are less likely to be killed, on average. I find these results hold both when I use variables consistent with the literature that covers the years from 1992 to 2011, as well as when I select other variables to expand the model further to 2014. Findings from regime specific analyses also provide support for my theory. When I stratify the sample by regime type, I find that regime duration's effect remains in autocracies and anocracies, though not in democracies — in accordance with my expectations. Also, the results indicate a negative association with journalist killings and democracy level in democracies.

The results suggest that long-time autocracies like Cuba and Saudi Arabia are likely to see less journalist killed in their borders than newer autocracies. When countries transition to autocracies, the danger for media personnel performing their journalistic work increases *initially*, but then reduces as the country remains autocratic. I also note that variation in polity level among autocracies, that is, the country's level of autocracy does not significantly impact journalists killing. The same pattern exists in anocracies — regime-type endurance decreases journalist killings.

The findings also show democracy and journalists killings in a new light. Though the reanalysis of Asal et al. (2018) finds democracy level positively associates with journalist killings, this result does not hold once I expand their model using variables that measure similar concepts but include more countries and years. Their initial find-

ings may have resulted from variable choice that dropped influential observations due to pairwise deletion or data availability. This finding emphasizes the importance of replication and reanalysis to advance knowledge in political science (Herrnson 1995).

In the stratified regime analysis, two further findings emerge regarding democracy and journalist killings. First, democracy's age does not significantly affect journalist killings in the sample. Once a country becomes a democracy, the rate of journalists killed does not necessarily change as the regime type endures. However a second finding provides a caveat to the first. The data suggest that more consolidated democracies do not see as many journalist killings as less consolidated ones. That is, journalist killings increase in lower democracy levels, but democracy's duration will not decrease the amount of those journalists killed.

The Philippines presents a striking example. Throughout the sample, the Southeast Asian country remains at *polity* score 8 without ever rising to a higher rating. However, instances of journalist killings remain steady with only 4 years in the sample without a journalist killed with an average of about 3 per year.[68] Brazil also offers another example, where democracy has endured but has not consolidated, remaining at polity score 8. Through the sample, the South American country only saw 8 years without a journalist killed with an average of 1.4 killed per year.

[68]Even when I remove the 2009 Maguindanao massacre that resulted in 30 journalists killed in one day, a clear outlier, the average remains just above 2 dead journalists a year.

Chapter 5 - Conclusion

This dissertation seeks to provide a better measure of media freedom and to address the *press-safety paradox* of democracies. To that aim, chapter 2 describes an item response theory (IRT) model that analyzes different cross-national indicators of media system freedom and generates a score that synthesized them to a single measure allowing researchers to incorporate rater's reliability to future empirical analysis. The next two chapters explore a more specific concern in the media system — the *press-safety paradox* that finds the introduction of democracy to a country both increases media freedom while simultaneously increasing the personal risk and safety to journalists working in the field. I argue that institutions play a significant role in reducing these risks. Chapter 3 specifically addresses the relationship between judicial independence and government censorship and media harassment, while chapter 4 examines regime type and journalist killings. Below I list my dissertation's key findings.

Key Findings

Chapter 2 details a new measurement of media freedom using an item response theory (IRT) model and an application of the new data in a replication study. First, the IRT analysis indicates that Varieties of Democracy (V-Dem) variables better measure the latent media freedom concept than other more commonly used indicators such as Freedom House or Reporter's Without Borders. The analysis also generates media system variables for 196 countries worldwide from 1960 to 2016. Each country-year has a posterior distribution to allow empirical researchers of media freedom to incorporate a level of uncertainty into empirical modeling of media freedom. Another finding reveals how the use of the data can impact published results. Egorov et al.'s (2009) results do not hold once I replace the Freedom House they use and incorporate my new media system freedom (MSF) scores. They argue that resource-poor dictatorships allow freer media systems in order to better monitor their bureaucracies. More specifically, they argue that dictators make this calculation based on the value of their oil reserves (and

not oil production value). Using the MSF data, I find evidence that the opposite is true. Using the MSF scores increased year and country coverage, I then extend the sample and find evidence that contradicts their theory — increases in the value of oil reserves are actually associated with media system freedom increases in dictatorships.

In chapter 3, I examine the relationship between judicial independence and government attacks against media conditional on electoral democracy. I find that judicial independence has a significant reductive effect on government censorship efforts and harassment against media in low to mid-level electoral democracies. While this is true for censorship of traditional media, I do not find the same results for internet censorship. Further analysis by world region indicates that the effect consistently holds in the following regions: former communist Eastern Europe and Central Asia, Latin America and the Caribbean, Sub-Saharan Africa, and Western Europe and North America. However, the effect is not consistent in the Middle East and North Africa or Asia regions. I also find a positive association with these attacks and media self-censorship. This includes government censorship of the Internet, though the effect for the other two are much greater. In a final unexpected finding, the data suggest that greater judicial independence increases attacks against media at the highest levels of electoral democracy in some cases.

Finally, in chapter 4 I find a negative association with journalists killed and regime-type duration. I also find that once I include a measure for regime-type duration, level of democracy no longer has a positive association with journalist killings — contrary to findings from previous studies. When I stratify the sample by regime type, I find the negative association holds in autocracies and anocracies but *not* in democracies. Also in this stratification, I find that level of democracy has a negative association with journalist killings in democracies. Taken together, duration of democracy does not decrease journalist killings, but strengthening democratic institutions in democracies does.

Future Work

These findings suggest that political institutions do affect outcomes related to media systems and media freedom. Strengthening judicial independence when electoral democracy is weak can reduce government censorship efforts against traditional media and harassment of journalists, while greater democratic consolidation can reduce journalist killings. However, the study has implications for research design and analysis. In two chapters, I replicate analyses from previous works and find that previous authors findings do not hold. In both cases, I use data that extends both countries and years covered in the analysis. The results suggest the initial findings may result from variable choice or the limited sample the authors used. To that end, extensions of chapter 2 include using the new media system freedom data and revisiting older empirical studies. These extension could both (potentially) expand older studies' sample and utilize the data's posterior distribution to see if rater uncertainty impacts previous findings.

Three further puzzles emerge from chapter 3's judicial independence and attacks against media analysis. First, the effect of judicial independence seems to reduce government censorship of traditional media and media harassment but not government internet censorship, though the evidence is mixed. The data generally show that independent courts seem to deter physical integrity of journalists and censorship of more traditional "brick and mortar" media outlets but not government censorship efforts of websites and social media. Courts may not see internet content, relatively new compared to other news mediums, as an important component to freedom of the press. Especially in less literate countries where voters more readily rely on radio and television, courts may be more inclined to protect traditional media rather than online outlets. Also, the Internet represents a relatively new legal frontier with many countries still deciding which online content the law should or should not permit. A reanalysis of this research design with new data in 10 or 20 years may yield wildly different results.

Second, some results indicated that judicial independence at high levels of electoral democracy seem to actually *increase* government attacks against media. This was true in the global sample of media harassment in figure 9b, the Middle East and North Africa

region in figure 11c examining government censorship efforts of traditional media, and Western Europe and North America in figure 11e also examining government censorship efforts of traditional media. Similar results for Asia exist for the regional analysis of media harassment in Appendix B. One potential explanation is media may actually focus criticism on the judiciary and the courts may use their independence to retaliate. For instance, Freedom House reports that in India the Deli High Court held four journalists from the *Mid-Day* newspaper in contempt of court after they ran an article accusing a former senior judge of issuing a ruling that benefited his son. While the Deli High Court sentenced them to four-month prison terms, the journalists were eventually freed pending an appeal.[69]

Third, evidence suggests that while government attacks against the media do direct Granger cause media self-censorship, other tests indicate media self-censorship direct Granger causes government attacks. Based on these results, I remain unable to determine empirical causality using Vector Autoregressive (VAR) analysis. Other scholars have encountered issues with demonstrating causality in the media freedom literature (Brunetti and Weder 2003, Solis and Antenangeli 2017). Research designs that collect data on *actual* attacks or changes in media content may better help address causality.

While future research may wish to explore these puzzling findings, other work may also wish to explore the influence of the legislature. Law making bodies may enact laws to protect media and discourage government attacks against them. For instance, Ghana's legislature introduced a law to decriminalize libel and slander in 2001.[70] In addition, future work may wish to explore the influence of the international community. Results in chapter 3 indicate greater dependence on foreign aid leads to lower levels of government censorship against traditional media and media harassment. This suggest global actors may have a role to play. In 2010 for instance, the European Court of Human Rights ordered the release of an Azerbaijani journalist who had been sentenced

[69] *Freedom House* "Press Freedom: India 2008" Available at https://freedomhouse.org/report/freedom-press/2008/india

[70] *IFEX* "Criminal Libel Law Repealed" July 21, 2001: Available at https://www.ifex.org/ghana/2001/07/31/criminal_libel_law_repeale/

to eight years in prison for uncomfortable news articles about the government.[71]

Finally, I turn to chapter 4's analysis on regime-type duration and journalist killing. Future work may wish to account for the regime type from which the transition occurred. For instance, transitions *from* certain regime types might affect the likelihood of a country seeing journalists killed versus others. Would a transition from an autocracy look different than a transition from an anocracy? Also, future work might wish to examine other major government or leadership changes. For instance, *polity* scores do not register any regime type changes in Iran from 1979 to 1980, though a revolution fundamentally changed government in the country in 1979. Similar leadership changes may explain some of the variation in journalist killings.

Future work might wish to investigate how different democratic institutions affects these killings. For instance, analyzing how different aspects of political participation, competition for government office, electoral democracy, the judiciary, or perhaps the legislative body could impact killings. Also, different authoritarian types — military, single-party, or personalistic for example — may reveal different patterns of journalists killed. Finally, an analysis solely investigating murdered journalists is clearly needed. The rarity of conflict and dangerous assignments may hide some of the systemic dangers journalists face daily.[72]

[71] *Human Rights Watch* "Azerbaijan: European Court Orders Release of Journalist" April 22, 2010: Available ar https://www.hrw.org/news/2010/04/22/azerbaijan-european-court-orders-release-journalist.
[72] I further make this case in a preliminary analysis in Appendix C.

Appendix A - Measuring Media Freedom

General Media Freedom Indicators

In this section I provide a more detailed description of the indicators I use in the IRT model.

Freedom House

Freedom House's (2017a) *Freedom of the Press* index remains one of the most widely used datasets for measuring press freedom in political science and economics (Brunetti and Weder 2003, Egorov et al. 2009, Schoonvelde 2014, Kellam and Stein 2016). The non-governmental organization began issuing reports in 1980, covering the previous year 1979, and continues to the present. However, the index's managers have changed the dataset's methodology numerous times since its inception. From 1980 until 1988, Freedom House provided separate rankings for a country's print and broadcast sectors, ranking each either *free*, *partially free*, or *not free*. Freedom House provides no aggregate ranking of the country's media system during this period. From 1989 until the present, Freedom House assigns countries a sector aggregated, countrywide *free*, *partially free*, or *not free* ranking. Freedom House does not make the index's survey methodology or criteria readily available from 1980 to 1993.

From 1994 until the present, Freedom House introduced a continuous score ranging from 0 to 100 to accompany each country's categorical ranking, with 0 being the most free and 100 being the least. Initially under the 100-point scale, the index evaluated media freedom based on four criteria: 1) law, 2) political pressure, 3) economic influence, and 4) repressive actions. The index managers considered both print and broadcast sectors separately and then assigned an overall freedom score. From 1997 until 2001, they used the same basic structure but modified the point distribution. In 2002 Freedom House introduced a new coding scheme that it still employs today. The newest methodology evaluates three different areas of each country's media system: the legal environment, political environment, and economic environment. The legal envi-

ronment category evaluates a country's laws and regulations regarding media and the government's inclination to use them to restrict the press falling on a scale from 0 to 30 with 0 being the most free and 30 being the least. The political environment category measures the degree of political control over the media by state and non-state actors including intimidation, detention, imprisonment, and violent assault on a range from 0 to 40 with 0 being the most free and 40 being the least.

Finally, the economic environment category assesses the structure, concentration, and transparency of media ownership including the government's distribution of advertisement, bribery, and the cost of establishing media among other criteria. Like the legal environment category, Freedom House utilizes a 0 to 30 range. These three combined scores determine a country's press freedom level. Countries that score from 0 to 30 are considered free, those from 31 to 60 rank as partially free, and those above 60 receive the label not free.[73]

For Freedom House's data before 1988, I take the average of the print and broadcast sectors to generate a rank. I score a country-year partly free (2) for any average greater than one or less than three. To illustrate this coding scheme, I provide a number of different examples. Australia in 1979 has two 3 scores for each sector, which makes their overall score 3 ((3 + 3)/2 = 3). Brazil's overall score in 1980 would be 2 ((2 + 2)/2 = 2). Equatorial Guinea 's overall score in 1981 would be 1 ((1 + 1)/2 = 1). All other print and broadcast sector combinations results in a 2 (partially free) coding. For instance, Freedom House gives Argentina a 2 rank for the print sector and a 1 rank for the broadcast sector. This average equals 1.5, so I assign it a 2 (partially free) in accordance to my criteria. Jamaica in 1987 has a score of 3 and 2 in the print and broadcaster sectors respectively. This average equals 2.5, so I assign it a 2 (partially free). In rare cases where one sector is 3 and the other 1 (like Malta in 1986), I again assign a 2 (partially free) ranking ((3 + 1)/2 = 2).

[73] See Freedom House's website for more information: https://freedomhouse.org/report-types/freedom-press.

Global Media Freedom

The Global Media Freedom (Whitten-Woodring and Van Belle 2017) dataset orients its data collection methodology by first defining media freedom as "an environment in which journalists are able to safely criticize political and economic elites at both the national and local levels." (Whitten-Woodring and Van Belle 2017, 180).[74] The authors use simple, ordinal categories of free, imperfectly free, and not free, and orient their data collection methodology by first defining media freedom as "an environment in which journalists are able to safely criticize political and economic elites at both the national and local levels." (Whitten-Woodring and Van Belle 2017, 180). From this starting point, they employ a simple coding system based on three categories obtained from historical documents about each country's media:

- **Free**—Countries where criticism of government and government officials is a common and normal part of the political dialogue in the mediated public sphere.
- **Imperfectly Free**—Countries where social, legal, or economic costs related to the criticism of government or government officials limits public criticism, but investigative journalism and criticism of major policy failings can and does occur.
- **Not Free**—Countries where it is not possible to safely criticize government or government officials.

The Global Media Freedom (GMF) dataset provides media freedom measures for 196 countries from 1948 to 2014.

Press Freedom Index (Reporters Without Borders)

The French-based watchdog group Reporters Without Borders (RSF) has released its Press Freedom Index since 2002 (Reporters Without Borders 2017).[75] The index provides press freedom scores and country rankings based on surveys from journalists, scholars, and human rights activists.[76] RSF focuses heavily on harassment against media, attacks against journalists, and self-censorship, though they consider other criteria

[74] See http://faculty.uml.edu/Jenifer_whittenwoodring/MediaFreedomData_000.aspx for more information.

[75] See https://rsf.org/en/world-press-freedom-index for more information.

[76] Most often used as a robustness check. See Freille et al. (2007), Egorov et al. (2009), and Steir (2015).

such as economic and legal conditions. Generally, scores range from 0 to 100, with 0 representing perfect press freedom and 100 indicating the least perfect, though some years inexplicably possess negative scores or scores above 100. Unfortunately, RSF does not publish the survey questionnaires or methodology it employs for each year (Schneider 2014).

Next, I describe how I generated an ordinal variable for RSF's data. The RSF score ranges from -14 to 144, and I convert it to five categories, dividing the data by 20th percentiles. I then code the categories so that higher numbered categories indicate greater media freedom.

Varieties of Democracy Indicators

The Varieties of Democracy (V-Dem) dataset provides original data for a number of indicators often associated with democracy (Coppedge et al. 2017a).[77] In their *Media* section, they offer a number of variables that evaluate different aspects of media quality and media freedom. V-Dem generates scores by asking country experts to rank each country according to a specified variable concept. After receiving the responses, the V-Dem researchers run the results through an item response theory (IRT) model to compile a cross-coder aggregated score (Pemstein et al. 2017). Below I report the question and scores/rankings for each V-Dem variable I use in this analysis including government censorship of traditional media (the press, television, and radio), government internet censorship, presence of a critical media, presence of various perspectives in media, harassment of journalists, the prevalence of self-censorship, media bias, media corruption, and access to media critical of the government.

Government Censorship: Traditional Media

Government Censorship measures government censorship efforts against traditional media outlets like the press, television, and radio. The authors clarify that this includes indirect means of censorship such as politically motivated financial and official (award-

[77] See https://www.v-dem.net/en/data/data-version-7-1/.

ing broadcast frequencies) support of supportive media outlets and other restrictions including a high barrier to receive a broadcasting license or taxes. The survey asked experts the following (V-Dem Codebook v7.1, 254):

> *Does the government directly or indirectly attempt to censor the print or broadcast media?*

From here, V-Dem offers five possible responses (Coppedge et al. 2017b, 253):

0. Attempts to censor are direct and routine.
1. Attempts to censor are indirect but nevertheless routine.
2. Attempts to censor are direct but limited to especially sensitive issues.
3. Attempts to censor are indirect and limited to especially sensitive issues.
4. The government rarely attempts to censor major media in any way, and when such exceptional attempts are discovered, the responsible officials are usually punished.

Government Internet Censorship

Government Internet Censorship measures the prevalence of government internet censorship efforts. The authors clarify that they focus specifically on politically motivated censorship and not child pornography, highly classified military secrets, or defamatory speech toward religion or individuals unless governments use it as a pretext for politically motivated censorship. The survey asked experts the following (Coppedge et al. 2017b, 255):

> *Does the government attempt to censor information (text, audio, or visuals) on the Internet?*

From here, V-Dem offers four possible responses (Coppedge et al. 2017b, 255):

1. The government successfully blocks Internet access except to sites that are pro-government or devoid of political content.
2. The government attempts to block Internet access except to sites that are pro-government or devoid of political content, but many users are able to circumvent such controls.
3. The government allows Internet access, including to some sites that are critical of the government, but blocks selected sites that deal with especially politically sensitive issues.
4. The government allows Internet access that is unrestricted, with the exceptions mentioned above.

Critical Print and Broadcast Media

Critical Print and Broadcast Media measures the degree to which major media outlets criticize the government. The survey asked experts the following (Coppedge et al. 2017b, 255):

> *Of the major print and broadcast outlets, how many routinely criticize the government?*

From here, V-Dem offers four possible responses (Coppedge et al. 2017b, 255):

0. None.
1. Only a few marginal outlets.
2. Some important outlets routinely criticize the government but there are other important outlets that never do.
3. All major media outlets criticize the government at least occasionally.

Print and Broadcast Media Perspectives

Print and Broadcast Media Perspectives measures the degree to which major media outlets report different perspectives. The lowest score represents a situation where media only report the government's perspective, while the highest score scores situations where all society's important perspectives are present in the media. The survey asked experts the following (Coppedge et al. 2017b, 255):

> *Do the major print and broadcast media represent a wide range of political perspectives?*

From here, V-Dem offers four possible responses (Coppedge et al. 2017b, 255):

0. The major media represent only the government's perspective.
1. The major media represent only the perspectives of the government and a government-approved, semi-official opposition party.
2. The major media represent a variety of political perspectives but they systematically ignore at least one political perspective that is important in this society.

3. All perspectives that are important in this society are represented in at least one of the major media.

Harassment of Journalists

Harassment of Journalist measures the degree to which journalists face harassment from governments and powerful non-governmental organizations. The survey asks experts the following basic question (Coppedge et al. 2017b, 256):

> *Are individual journalists harassed - i.e., threatened with libel, arrested, imprisoned, beaten, or killed – by governmental or powerful nongovernmental actors while engaged in legitimate journalistic activities?*

From here, V-Dem offers five possible responses (Coppedge et al. 2017b, 256):

0. No journalists dare to engage in journalistic activities that would offend powerful actors because harassment or worse would be certain to occur.
1. Some journalists occasionally offend powerful actors but they are almost always harassed or worse and eventually are forced to stop.
2. Some journalists who offend powerful actors are forced to stop but others manage to continue practicing journalism freely for long periods of time.
3. It is rare for any journalist to be harassed for offending powerful actors, and if this were to happen, those responsible for the harassment would be identified and punished.
4. Journalists are never harassed by governmental or powerful nongovernmental actors while engaged in legitimate journalistic activities.

Media Self-censorship

Media Self-censorship measures self-censorship's prevalence among journalists in a country. The survey asked experts the following (Coppedge et al. 2017b, 256):

> *Is there self-censorship among journalists when reporting on issues that the government considers politically sensitive?*

From here, V-Dem offers four possible responses (Coppedge et al. 2017b, 256):

0. Self-censorship is complete and thorough.
1. Self-censorship is common but incomplete.
2. There is self-censorship on a few highly sensitive political issues but not on moderately sensitive issues.
3. There is little or no self-censorship among journalists.

Media Bias

This variable measures the degree to which media as a whole holds overt biases against opposition parties or candidates. The survey asked experts the following (Coppedge et al. 2017b, 257):

> *Is there media bias against opposition parties or candidates?*

From here, V-Dem offers four possible responses (Coppedge et al. 2017b, 257):

0. The print and broadcast media cover only the official party or candidates, or have no political coverage, or there are no opposition parties or candidates to cover.
1. The print and broadcast media cover more than just the official party or candidates but all the opposition parties or candidates receive only negative coverage.
2. The print and broadcast media cover some opposition parties or candidates more or less impartially, but they give only negative or no coverage to at least one newsworthy party or candidate.
3. The print and broadcast media cover opposition parties or candidates more or less impartially, but they give an exaggerated amount of coverage to the governing party or candidates.
4. The print and broadcast media cover all newsworthy parties and candidates more or less impartially and in proportion to their newsworthiness.

Media Corruption

This variable measures the level of corruption among journalists and media personnel in a country's media system. The survey asked experts the following (Coppedge et al. 2017b, 258):

> *Do journalists, publishers, or broadcasters accept payments in exchange for altering news coverage?*

From here, V-Dem offers five possible responses (Coppedge et al. 2017b, 258):

0. The media are so closely directed by the government that any such payments would be either unnecessary to ensure pro-government coverage or ineffective in producing anti-government coverage.
1. Journalists, publishers, and broadcasters routinely alter news coverage in exchange for payments.
2. It is common, but not routine, for journalists, publishers, and broadcasters to alter news coverage in exchange for payments.

3. It is not normal for journalists, publishers, and broadcasters to alter news coverage in exchange for payments, but it happens occasionally, without anyone being punished.
4. Journalists, publishers, and broadcasters rarely alter news coverage in exchange for payments, and if it becomes known, someone is punished for it.

Access to Critical Media

This variable measures the percentage of the population that has access to traditional media (print, radio, or television) that sometimes criticize the national government. The variable is a percentage that runs from 0 to 100 percent. To get the variable, V-Dem asks the country experts (V-Dem Codebook v6.2, 247):

> *Approximately what percentage of the population has access to any print or broadcast media that are sometimes critical of the national government?*

Survey takers give their responses in percentages, and V-Dem uses a Bootstrap, cross-coder aggregation to generate a final score. To convert the percentages to ordinal ranks, I calculate the variable's mean and standard deviation, which are 52.39 and 27.44 respectively. Starting at the mean, I move out one standard deviation toward 0 and toward 100. From here, I create the four categories using the following coding rules: category 1 = $<$ 25.05, category 2 = between 25.05 and 52.39, category 3 = between 52.4 and 79.8, and category 4 = $>$ 79.81.

Monte Carlo Simulations Replication Results

The Media System Freedom (MSF) data has point estimates and a poterior standard deviation for each country-year in the sample. To utilize these media freedom distributions, I run a Monte Carlo (MC) simulation on models 2 and 3 in table 2 using 750 random draws from each country-year's MSF distribution score. The MC simulation runs the model 750 times, estimating a beta coefficient and standard error each time, then returns a mean of each variable's beta coefficients and standard errors. I report these results in table 12. Results from the simulation yield similar coefficients and standard errors as in models 2 and 3. Contrary to Egorov et al.'s (2009) findings, *log oil reserve value* becomes positive and statistically insignificant in models 1-2, while model 3 is negative and statistically insignificant. Interestingly, *log oil reserve value* becomes positive and statistically significant ($p < .05$) in model 4. Like model 6 in table 3, the results indicate that as the value of oil reserves increase in dictatorships, media freedom increases, on average.

Table 12: Monte Carlo Simulations, Log Oil Reserve Value and Media Freedom

MC sim. from manuscript of:	Model B1 T2: Model 2	Model B2 T2: Model 3	Model B3 T3: Model 5	Model B4 T3: Model 6
	β/(SE)	β/(SE)	β/(SE)	β/(SE)
Log Oil Reserve Value	.004	.001	- .006	.013**
	(.013)	(.004)	(.016)	(.006)
Log Oil Reserve Value x Polity	.0008*	-.0002		
	(.0005)	(.0002)		
Polity	.008***	.024***		
	(.002)	(.001)		
Log GDP p/c, PPP	-.007		- .032*	
	(.013)		(.017)	
Log GDP p/c, Nominal		-.028***		-.062***
		(.003)		(.005)
Log Population	.033	.043***	-.075	.132***
	(.031)	(.011)	(.053)	(.021)
Log Govt. Expend./GDP	-.016*	-.013**	.002	.012
	(.008)	(.005)	(.013)	(.008)
Iterations	750	750	750	750
Observations	2,056	4,941	958	2,430
Countries	147	161	71	115
Years	1993-2008	1980-2016	1993-2008	1980-2016
Fixed Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes

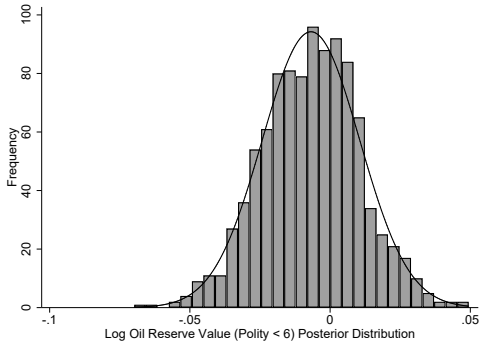
$***p < 0.01$; $**p < 0.05$; $*p < 0.1$; 750 iterations; β = mean of 750 coefficient estimates; (SE)= mean of 750 standard errors; Models B1 and B3 restricted to countries with polity score < 6 in 1992; Models B2 and B4 restricted to country-years with polity score < 6. Constants not reported; Table 2 located on page 27 of manuscript; Table 3 located on page 28 of manuscript; Constant not reported; All media freedom variables are leads (t + 1).

Posterior Distribution of Log Oil Reserve Value (from Table 12)

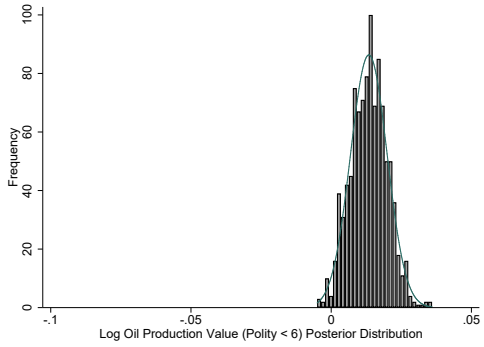
To visualize the results from models 3 and 4 in table 12, I graph 1,000 random draws from the Monte Carlo simulations' resulting posterior distribution for *log oil reserve value* in figure 16. In figure 16a, 581 of the draws fall on the distribution's positive direction suggesting a 58.1 percent probability that *log oil reserve value* has a positive effect on media freedom. Figure 16b indicates 981 fall on the distribution's positive side, suggesting a 98.1 percent probability that *log oil reserve value* has a positive effect on media freedom. I also note that figure 16b forms a more compact distribution than figure 16a, indicating more certainty in the results. In sum, to further support the chapter 2's findings, the Monte Carlo simulations indicate that *log oil reserve value* does not have a significant effect on media freedom levels in authoritarian regimes. These findings run contrary to Egorov et al.'s (2009) results.

Figure 16: *Log Oil Reserve Value* Posterior Distributions, Table 2

(a) Model 2 (1993-2008)



(b) Model 3 (1980-2016)



Pairwise Correlation Matrix w/ Significance Levels

Table 13: Pairwise Correlation Matrix of 12 Media Freedom Variables

	GMF	FoP	RSF	Trad M.Cen	Inet Cen	M.Critical	M.Perspectives	M.Harassment	M.Self-Cen	M.Bias	M.Corrupt	CM.Access
GMF	1											
	(9076)											
FoP	.8424*	1										
	(6356)	(6755)										
RSF	.8430*	.8497*	1									
	(2173)	(2528)	(2529)									
V-Dem: Trad M.Cen	.7410*	.7451*	.7555*	1								
	(8202)	(6026)	(2406)	(8580)								
V-Dem: Inet Cen	.5532*	.5814*	.6191*	.7107*	1							
	(3416)	(3753)	(2398)	(3756)	(3756)							
V-Dem: M.Critical	.6692*	.6882*	.6435*	.8209*	.7041*	1						
	(8202)	(6026)	(2406)	(8580)	(3756)	(8580)						
V-Dem: M.Perspectives	.6826*	.6799*	.6472*	.8204*	.7286*	.8682*	1					
	(8202)	(6026)	(2406)	(8580)	(3756)	(8580)	(8580)					
V-Dem: M.Harassment	.7271*	.7081*	.7410*	.8198*	.6348*	.7934*	.7775*	1				
	(8202)	(6026)	(2406)	(8580)	(3756)	(8580)	(8580)	(8580)				
V-Dem: M.Self-Cen	.6767*	.6951*	.6952*	.8371*	.6719*	.8371*	.8158*	.7999*	1			
	(8202)	(6026)	(2406)	(8580)	(3756)	(8580)	(8580)	(8580)	(8580)			
V-Dem: M.Bias	.7057*	.6936*	.6696*	.8404*	.7333*	.8551*	.8754*	.7849*	.8314*	1		
	(8202)	(6026)	(2406)	(8580)	(3756)	(8580)	(8580)	(8580)	(8580)	(8580)		
V-Dem: M.Corrupt	.6897*	.6980*	.7023*	.8059*	.6356*	.8050*	.8123*	.7863*	.8008*	.8284*	1	
	(8202)	(6026)	(2406)	(8580)	(3756)	(8580)	(8580)	(8580)	(8580)	(8580)	(8580)	
V-Dem: CM.Access	.6849*	.6534*	.6511*	.7539*	.4997*	.7454*	.7420*	.7674*	.7280*	.7665*	.7305*	1
	(7896)	(5508)	(1996)	(8009)	(3301)	(8009)	(8009)	(8009)	(8009)	(8009)	(8009)	(8009)

$*p < 0.01$
Observations in parentheses

Test IRT's Single Trait Assumption

Item response theory (IRT) models assume a single latent trait among a set of indicators. To provide evidence for this assumption, I run factor analysis (FA) and principal component analysis (PCA) on the 12 media freedom variables that I use for this study. For FA, I expect the data to contain a single factor, and for PCA I expect to find a single component. The results presented below confirm these expectations.

Factor Analysis

Factor Analysis (FA) is a latent trait analysis (LTA) that "identifies the relationships among measured variables for the purposes of reducing data, such as collapsing several items on a test into subscales, and/or evaluating theoretical structures" (Brown et al. 2011, 141). Here, I wish to identify the data's structure. FA identifies inferred latent variables known as factors (Brown et al. 2011, 141). In accordance with the IRT model's assumption, I expect the data to contain a single factor. To this end, I run a FA and examine each factor's eigenvalues. Eigenvalues over 1 indicate a single factor (Kaiser 1958). I therefore expect to find only one factor with an eigenvalue over 1.

Figure 17 shows scree plots of the FA results with all 12 variables. The results indicate the first factor with a eigenvalue of about 8.72 with the all other factors being less than 1. As expected, the results indicate a single factor. However, FA requires the presence of all variable rows and discards observations that do not meet this criteria. As a result, pairwise deletion greatly diminishes the sample size. I therefore repeat the analysis and remove variables with less coverage to expand the sample size. First, I remove *RSF*, which runs from 2002 to 2016, and find the same result. Next, I only remove *V-Dem: Internet Censorship*, which runs from 1993 to 2016, and again find the expected result. Running FA after removing both of these variables also indicates a single factor. I then remove *RSF*, *V-Dem: Internet Censorship*, and *Freedom House*, which runs from 1979 to 2016, and again find the expected result. Figure 18 shows these results.

Scree Plots

Figure 17: Factor Analysis Scree Plot, All 12 Media Freedom Indicators (n=1,923)

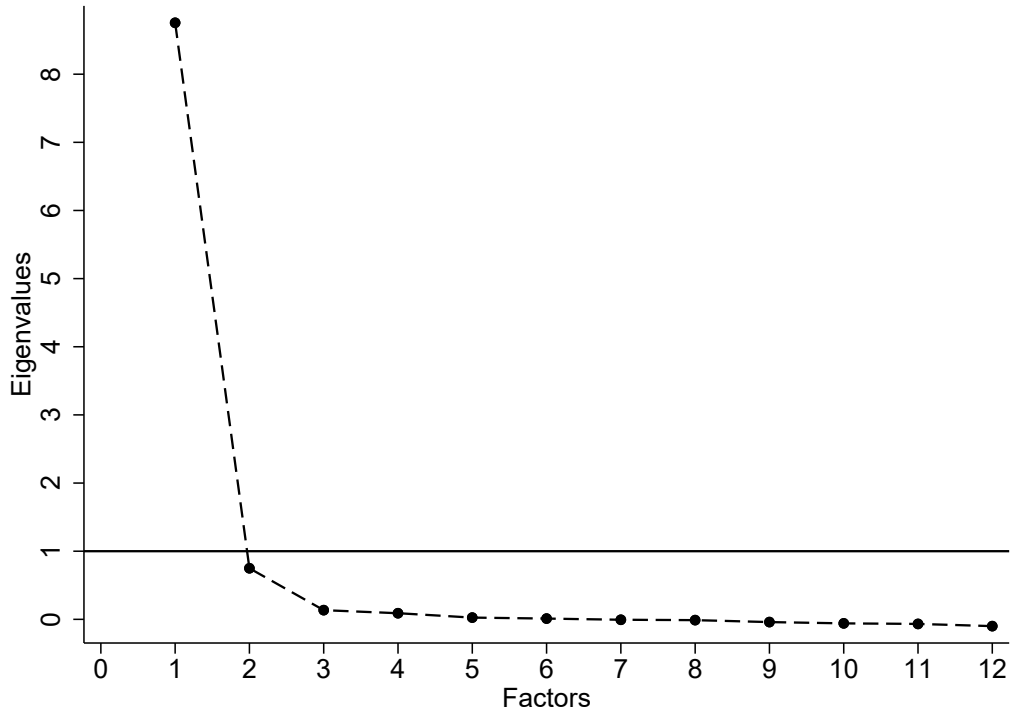
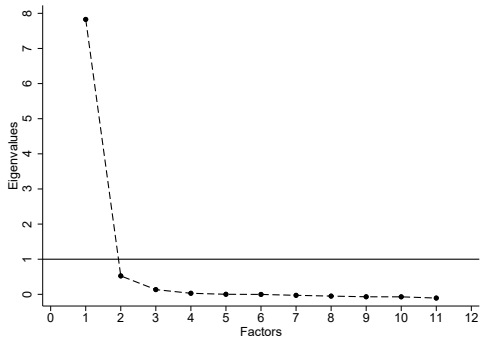
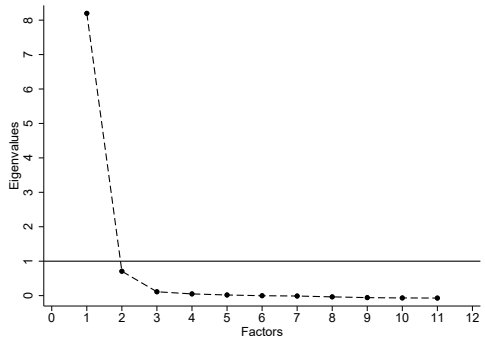


Figure 18: Factor Analysis Scree Plots, Different Specifications

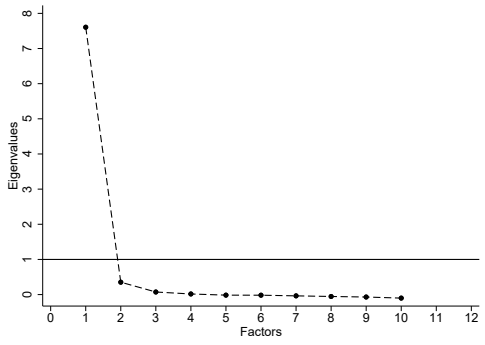
(a) Without RSF (n=3,139)



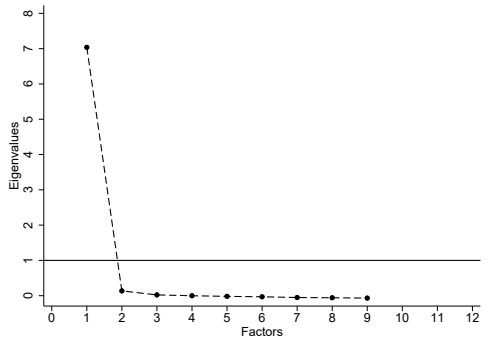
(b) Without V-Dem Inet (n=1,931)



(c) Without RSF and V-Dem Inet (n=5,428)



(d) Without RSF, V-Dem Inet, and FoP (n=7,896)



Principal Components Analysis (PCA)

Principal components analysis (PCA) is a data reduction technique that linearly transforms intercorrelated variables into smaller sets of uncorrelated variables that contain most of the original dataset's information (Dunteman 1989, 7). Researchers can use PCA to reduce multicollinearity among highly correlated variable or examine data structure. I aim to do the latter, and I expect the data to contain a single component, in accordance with the IRT model's single-trait assumption. To this end, I run a PCA and examine each component's eigenvalues. Eigenvalues over 1 indicate a single component (Kaiser 1958), and I therefore expect to find only one component with an eigenvalue over 1.

Figure 19 shows scree plots with 95 percent confidence intervals of the PCA results with all 12 variables. The results show the first component with a eigenvalue of about 8.93 with all other components falling below the 1 criteria. As expected, these results indicate a single component. However, PCA requires the presence of all variable rows and discards observations that do not meet this criteria. As a result, pairwise deletion greatly diminishes the sample size. I therefore repeat the analysis and remove variables with less coverage to expand the sample size. First, I remove *RSF*, which runs from 2002 to 2016, and find the same result. Next, I only remove *V-Dem: Internet Censorship*, which runs from 1993 to 2016, and again find the expected result. Running FA after removing both of these variables also indicates a single component. I then remove *RSF*, *V-Dem: Internet Censorship*, and *Freedom House*, which runs from 1979 to 2016, and again find the expected result. Figure 20 shows these results. In sum, using principal components analysis I find evidence that these variables contains a single component, which supports the IRT model's single trait assumption.

Scree Plots

Figure 19: PCA Scree Plot, All 12 Media Freedom Indicators (n=1,923)

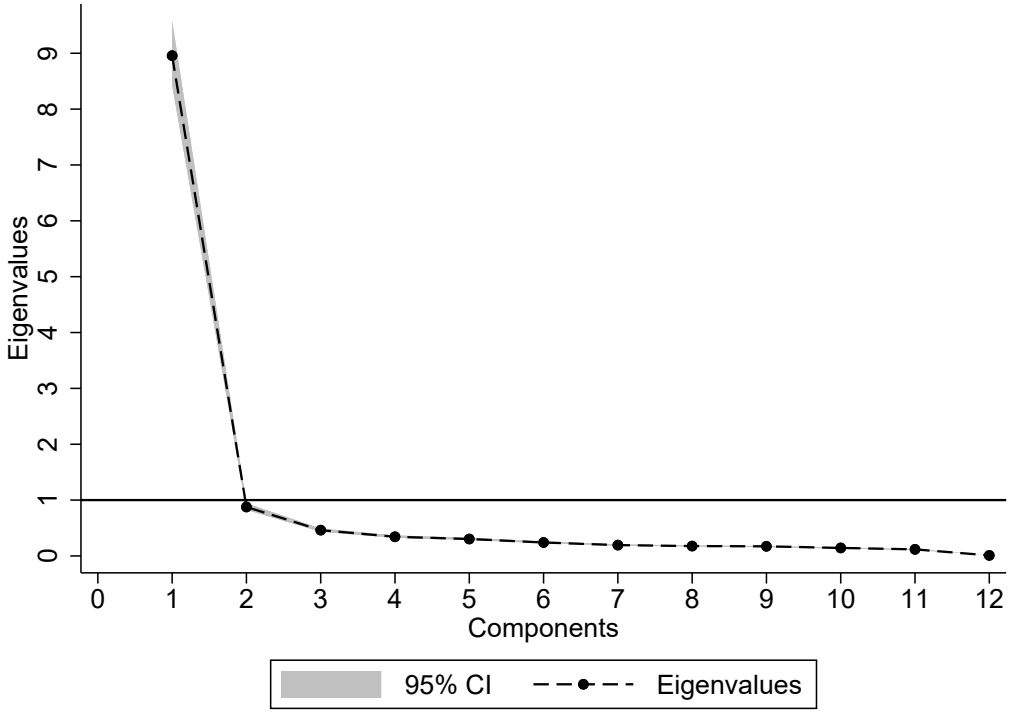
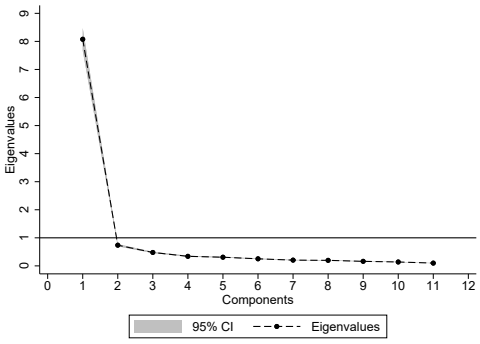
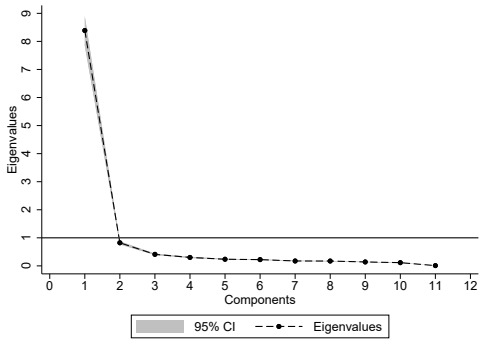


Figure 20: PCA Scree Plots, Different Specifications

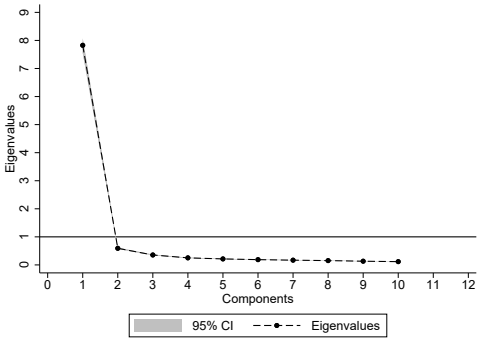
(a) Without RSF (n=3,139)



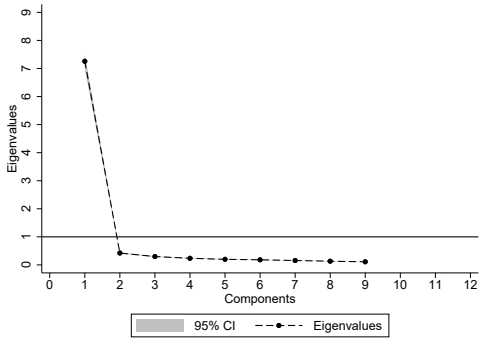
(b) Without V-Dem Inet (n=1,931)



(c) Without RSF and V-Dem Inet (n=5,428)



(d) Without RSF, V-Dem Inet, and FoP (n=7,896)



Further Validity: Figure 5 Case Studies

Spain

At the beginning of the MSF scores in 1960, General Francisco Franco had been Spain's authoritarian ruler since 1939 at Spanish Civil War's conclusion. During the civil war, Franco's forces took measures to control the printing press in 1936 and broadcast media in 1938 (Deacon 1999, 311). With the civil war's conclusion, media served as the government's mouthpiece. The state tightly controlled Spain's media, though some officials attempted meager reforms later in Franco's life. In 1966, a law abolished prior censorship, meaning the press could publish stories without prior government approval. However, this new law did not amount to any substantial change in media freedom (Deacon 1999, 313). Franco would die in 1975, but officials were slow to lift restrictions (Deacon 1999, 313). In 1978, the Spanish people voted to ratify a new constitution that included a declaration of freedom of expression and freedom to receive and disseminate information.[78] It also barred government censorship[79]). Finally, the 1980 Statute of Radio and Television guaranteed plurality of broadcast media, including opening media to areas with regional identities that Franco's regime suppressed, such as the Basque Country and Catalonia.

The MSF scores reflect these changes. The score remains near 0 until 1975 when Franco dies, but gradually increases as the case study suggests — .3281 in 1976, .5783 in 1977, .8386 in 1978, .9383 in 1979, and then .9663 in 1980. Spain's estimate remains near 1 around .96 until the sample ends in 2016.

North Korea

The Democratic People's Republic of Korea (North Korea) had been a sovereign, communist regime since 1948 when the Soviet Union ended its occupation. At the beginning of the MSF scores, North Korea's authoritarian regime had already heavily

[78] Spanish Constitution of 1978, Art 20.
[79] Spanish Constitution of 1978, Art 2

restricted media (Byman and Lind 2010, 54). This policy has endured to the present.[80] In 2015, the Committee to Protect Journalists (CPJ) named North Korea the second worst media censor in the world,[81] while in 2017 the watchdog group described North Korea's dictator Kim Jong Un as having an "absolute grip on the flow of public information and [a] deadly approach to dissent."[82]

The MSF scores reflect this description. North Korea begins the time series with a score near zero indicating a virtual absence of media system freedom and retains it throughout the sample.

United States

The United States' constitution has explicitly guaranteed press freedom since the country's founders ratified it in 1788. In addition, constitutions in nine of the North American country's first 13 states included a clause for press freedom (Powe 1991, 26). Through the government did impose restrictions on the press at various times in the country's development,[83] the media in the United States have generally operated in a mostly free environment. However, subnational government restrictions on media occurred throughout the early to mid 20th century, notably in the US states of Minnesota (Lewis 2007, 43-44) and Louisiana (Powe 1991, 222). Lewis (2007) and Powe (1991) both point to three Supreme Court rulings that resulted in media's increased protection from libel suits in the United States: *New York Times v. Sullivan* in 1964, *New York Times v. United States* in 1971, and *Miami Herald Publishing Co. v. Tornillo* in 1974. Kalven (1988) describes the First Amendment "working itself pure" during this time period (pg. xvii).

The United States' MSF scores reflect these changes. At the beginning of the time series in 1960, when media were generally free but still subject to prior restraint and

[80] *BBC* "North Korea's tightly controlled media" Dec. 19, 2011. Available at: http://www.bbc.com/news/world-asia-pacific-16255126.

[81] *Committee to Protect Journalists* "10 Most Censored Countries 2015" Available at: https://cpj.org/2015/04/10-most-censored-countries.php

[82] *Committee to Protect Journalists* "Supervised Access" April 25, 2017. Available at: https://cpj.org/2017/04/supervised-access.php

[83] Most notably the 1798 Sedition Act (Lewis 2007, 11) and 1918 amendments to the 1917 Espionage Act (Lewis 2007, 28).

libel law suits in certain circumstances, the country scores a .7638 and remains there until around 1967 when its score increases to about .81. By 1975, the US scores at .9531 and remains near 1 until the end of the dataset in 2016. The MSF scores detect these events and change accordingly.

Nigeria

Nigeria achieved independence from Great Britain in 1960 and has experienced the most instability of the six countries I display in figure 5 in chapter 2. After declaring independence, Nigeria ratified a new constitution in 1963 that lasted until 1966 when a coup occurred. The coup would eventually trigger at civil war that lasted from 1967 to 1970. At the civil war's conclusion, stability returned to the west African country until another coup occurred in 1975. By 1977, a new constitution ushered in the Second Republic, but in 1983 a military coup replaced it with an authoritarian regime. Yet another coup occurred two years later in 1985. The Nigeria government survived an unsuccessful coup in 1990, and then annulled the results of a 1993 presidential election. Turmoil from the disputed election gave Defense Minister and army general Sani Abacha an excuse to seize power later that year. Abacha would die of a heart attack in 1998, and power passed to General Abdulsalami Abubakar who allowed a transition back to civilian rule in 1999. The May 1999 presidential election established the Fourth Republic, the form of government under which Nigeria remains today. It was in this shuffle of civilian and military regimes that Nigeria's media system would operate.

Before Nigeria's independence, British colonists established a press in Nigeria in 1859 (Oso et al. (2011, 1). During colonization, the British restricted the press but outlets published content critical of the colonial powers (Mohammad and James 2017). By the time Nigeria became independent, various Nigerian leaders, both democratic and non-democratic, used colonial-era media laws to restrict media (Eribo and Jong-Ebot 1997, 63). Looking back at Nigerian media since independence, Mohammad and James (2017) note that "Nigerian media have been playing a very significant role in setting the agenda for public discourse and molding the direction of public opinions

on vital issues in the country." Tejumaiye and Adelabu (2011) argue that Nigerian media did not develop as watchdogs of government, and so did not always perform this role (pg. 73). Eribo and Jong-Ebot (1997) also argue the worst restrictions occurred during military rule. But even once Nigerian leaders established the Fourth Republic and included press freedom protections in the 1999 constitution,[84] the government continued to restrict media. For example, in 2004 the government closed *Weekly Insider* magazine citing national security concerns, and imprisoned *Midwest Herald* publisher Orobosa Omo-Ojo for a story about the Nigerian president's wife in 2005. In reviewing Nigerian state-press relations since independence, Ogbondah (2011) concludes that "the state still utilises arbitrary actions and extra-legal measures that were adopted by erstwhile military regimes in attempts to cow the press and suppress the dissemination of diverse views and information in the media" (pg 46). Given that Nigeria has maintained an active media that democratic and authoritarian regimes have restricted, I expect Nigeria's media freedom to remain relatively constant with the exception of military rule when authors agree government attacks against media intensified.

Nigeria's MSF scores reflect this case history. The case history does not suggest that Nigeria should begin the series either fully free or fully not free, so I cannot place Nigeria's MSF score precisely on the latent scale in 1960. However, the case history suggest changes at certain times that the MSF scores reflect. The data begins in 1960 at .5639 and increases slightly to around .5981 by 1975. It remains constant until it dips slightly around 1983 when military rule replace the Second Republic. The score then rises to about .6646 around 2000 when the country transitions to civilian rule but remains at that level. I highlight that while Nigeria's democracy level changed many times during the sample, its MSF score remained largely stable. In figure 21 below, Nigeria's *polity* score changes quite often, while its MSF score remains more stable. Nigeria's case demonstrates that 1) MSF scores do not necessarily depend on regime type by default, and 2) a media system can have a degree of freedom even under authoritarian rule.

[84]Sections 16, 22, 39(1), and 39(2) of the 1999 Nigerian Constitution.

Figure 21: Nigeria's Polity and MSF Scores, 1960-2016

Note: Polity standardized to 0 - 1 scale.

Brazil

In contrast to Nigeria's case, Brazil experienced moments of political instability during the sample's time period that *did* affect its media system's freedom. The South American country begins the sample as a democratic regime (Cheibub et al. 2010 and Marshall and Jaggers 2017), but the military staged a coup in 1964 that forced the country into a dictatorship. In 1985, the country transitioned back to a democracy and has remained one since.

During a democratic period in Brazil that lasted from 1946 to 1964, the press often served as an instrument for political parties (Albuquerque 2012, 80). For example, backers of the former dictator Getúlio Vargas created the *Última Hora* paper to support their candidate's 1951 election campaign. Also during this time, the press was not market-driven and depended on advertising from the government and state-owned operations as well as bribes (Albuquerque 2012, 80-81). However, once the generals installed a military regime in 1964, the authoritarian government dissolved political parties and media outlets became either subservient to the regime or authorities censored them (Smith, 1997). For example, during the initial transition *Globo* became a dominant media outlet and developed an "authoritarian model of journalism" by allying with

the military dictatorship (Porto 2012, 61).[85]

Strict censorship lasted until 1974 when General Ernesto Geisel introduced the *abertura* policy that relaxed censorship, in addition to other reforms (Williamson 2009, 431). This period saw the authorities search for democratic legitimacy for their authoritarian regime. *Abertura* continued under Brazil's next military ruler who abolished the censorship of books and newspapers in 1979, though radio and television censorship continued (Williamson 2009, 432). The military dictatorship endured until 1985, and a democratic constitution took effect in 1988. The document declares freedom of the press and bars government censorship.[86] Media changed to a more market-driven model as the country's democracy consolidated (Albuquerque 2012, 81). The current media system remains relatively free, though media ownership remains heavily concentrated (Hervieu 2013) and the judiciary often censors outlets.[87, 88]

Brazil's MSF scores reflect this case history. The case history does not suggest that Brazil should begin the series either fully free or fully not free, so I cannot place Brazil's MSF score precisely on the latent scale in 1960. However, the case history suggest changes at certain times. In 1960, Brazil's score is around .5962 but then decreases in 1964 when the authoritarian regime took power and exerted influence over country's media. The score steadily decreases for nearly a decade until about 1974 when General Geisel enacted the *abertura* policy. Throughout the *abertura* period until Brazilians ratified the 1988 constitution, the score gradually rises to about .7171. It rises to about .8 during this most recent democratic period, though never rises above about .88. Given media ownership concentration and judicial censorship, I do not expect Brazil's score to represent a nearly fully free media system. In sum, as expected from Brazil's case history, Brazil's MSF scores reflect its media system case history. And contrary to Nigeria, the country's media freedom moves with the authoritarian regime — as the

[85] *Globo* actually offered an apology for this alliance in 2013 (http://articles.latimes.com/2013/sep/04/world/la-fg-wn-brazil-globo-network-military-20130904).

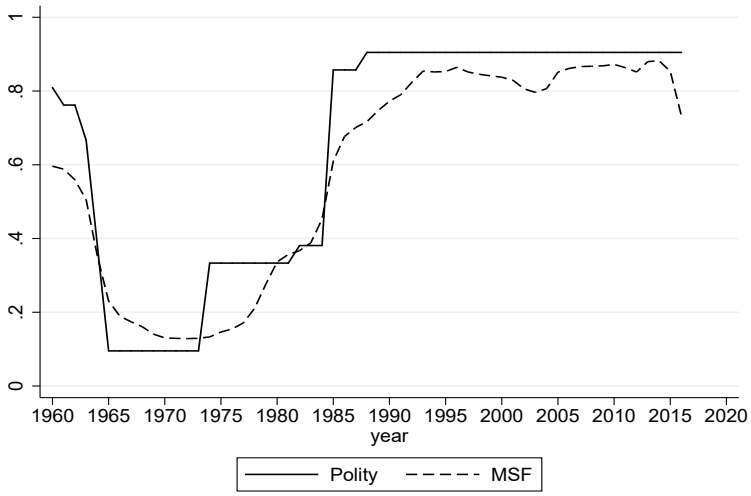
[86] Censorship and press freedom addressed in Title II (Chapter 1) and Title XIII (Chapter V) respectively.

[87] *Committee to Protect Journalists* Carlos Lauría "Attacks launch: Judicial censorship strikes a chord in Brazil," Feb. 17, 2011. Available at: https://cpj.org/blog/2011/02/at-attacks-launch-judicial-censorship-strikes-a-ch.php

[88] *Committee to Protect Journalists* Carlos Lauría "Violence and Judicial Censorship Mar Brazil's Horizon," 2013.Available at: https://cpj.org/2014/02/attacks-on-the-press-brazil-analysis.php

military regime took power MSF scores decrease and as the regime opened the score steadily rise. To show this relationship, I graph Brazil's polity and MSF scores in Figure 22 below.

Figure 22: Brazil's Polity and MSF Scores, 1960-2016



Note: Polity standardized to 0 - 1 scale.

Compare Results of Other IRT Specifications

Below I compare outputs from different IRT model specifications as a robustness check to the main results. Table 14 indicates that increasing the number of iterations in the Markov chain Monte Carlo simulations to 10,000 in 3 chains and adding an indicator for journalists killed does not affect each variable's beta coefficients in chapter 2's main IRT model. The coefficient for *journalists killed* (not included in table F.3) is 1.22, indicating it exerted very little influence on the model. Table 15 shows the MSF point estimates (table 15a) and posterior standard deviations (table 15b) highly correlate, indicating the initial results remain robust to different specifications.

Table 14: Betas of Different IRT Model Specifications

	GMF	**FH: FoP**	**RSF**	**V.Cen**	**V.Inet**	**V.Crit**	**V.Perspect**	**V.Harass**	**V.Selfcen**	**V.Mbias**	**V.Mcorr**	**V.CMaccess**
Manuscript	8.95	8.3	7.14	10.96	7.11	11.32	11.07	10.62	10.63	11.31	10.01	8.05
w/ 10,000 Iters.	8.95	8.31	7.13	10.97	7.11	11.32	11.07	10.62	10.64	11.31	10.02	8.03
w/ J. Killed	8.96	8.32	7.15	10.96	7.08	11.31	11.06	10.61	10.63	11.29	10.02	8.02

Table 15: Correlation Matrices of Different IRT Model Specifications

(a) MSF Point Estimates

	Manuscript	**w/ 10,000 Iters.**	**w/ J. Killed**
Manuscript	1		
w/ 10,000 Iters.	.9999	1	
w/ J. Killed	.9999	.9999	1

(b) MSF Standard Deviations

	Manuscript	**w/ 10,000 Iters.**	**w/ J. Killed**
Manuscript	1		
w/ 10,000 Iters.	.9897	1	
w/ J. Killed	.9861	.9894	1

Cross-sectional Results: MSF Score in 137 countries, 1968

Figures 2 and 3 in chapter 2 show cross-sectional results for most countries in 2014. However, these figures do not show three countries with MSF data because they no longer existed that year. To provide results that include East Germany, South Yemen, and the Republic of Vietnam, I show cross-sectional results from the year 1968. South Yemen and East Germany rank near the very bottom in figure 23, while the Republic of Vietnam ranks around the middle of all countries at the top of figure 24. Overall, the figures indicate that North Korea, Cuba, Oman, Saudi Arabia, and China have the lowest media freedom scores in 1968, while Denmark, Switzerland, Belgium, Austria, and West Germany have the highest that same year. These results show similar countries at the very bottom and top of the MSF scores to the 2014 cross-sectional results. I note that in these figures, Russia indicates the Soviet Union, Serbia indicates Yugoslavia, Germany indicates West Germany, and Czech Republic indicates Czechoslovakia.

Figure 23: Media System Freedom (MSF) in 137 countries, 1968: Lower Bounds

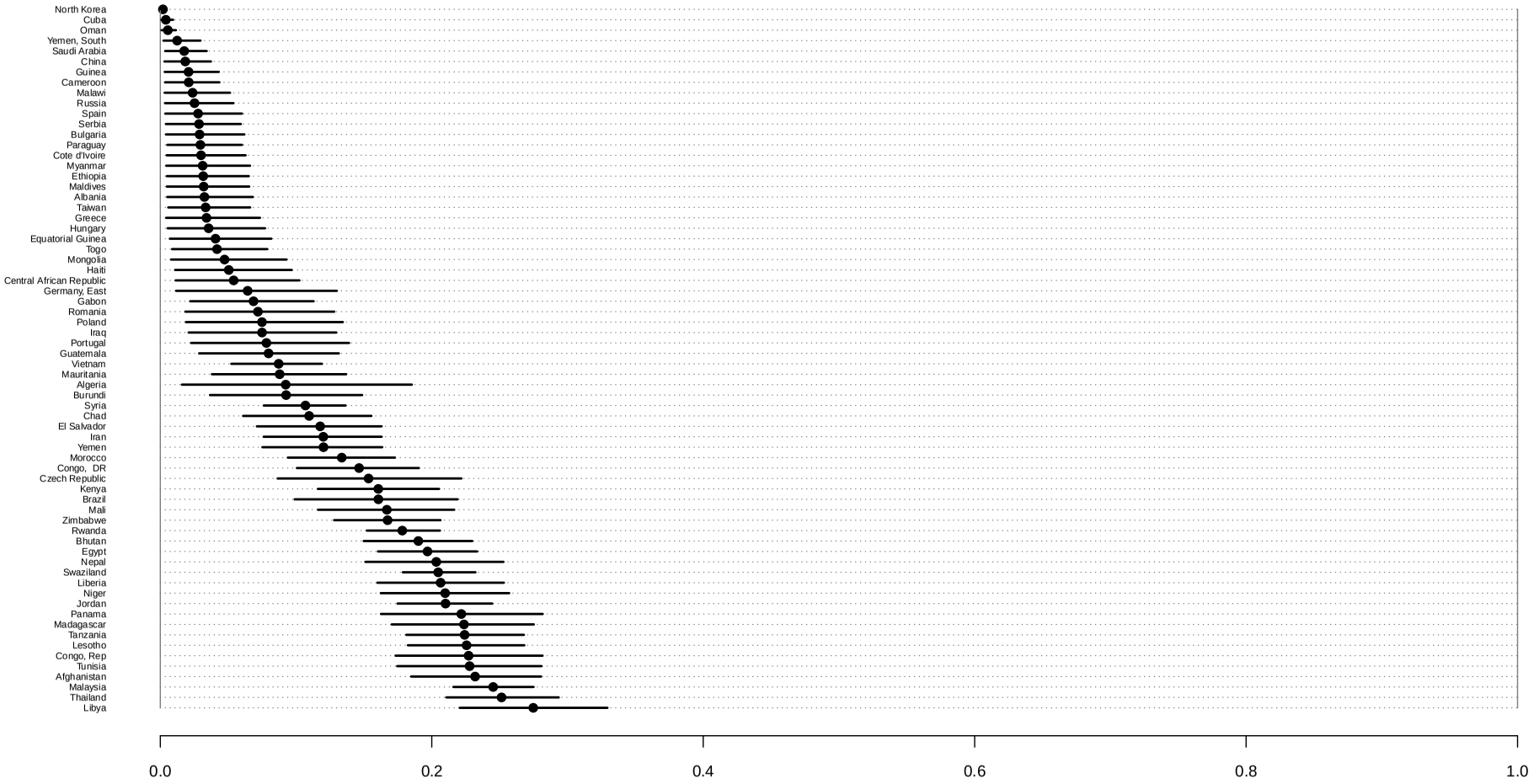
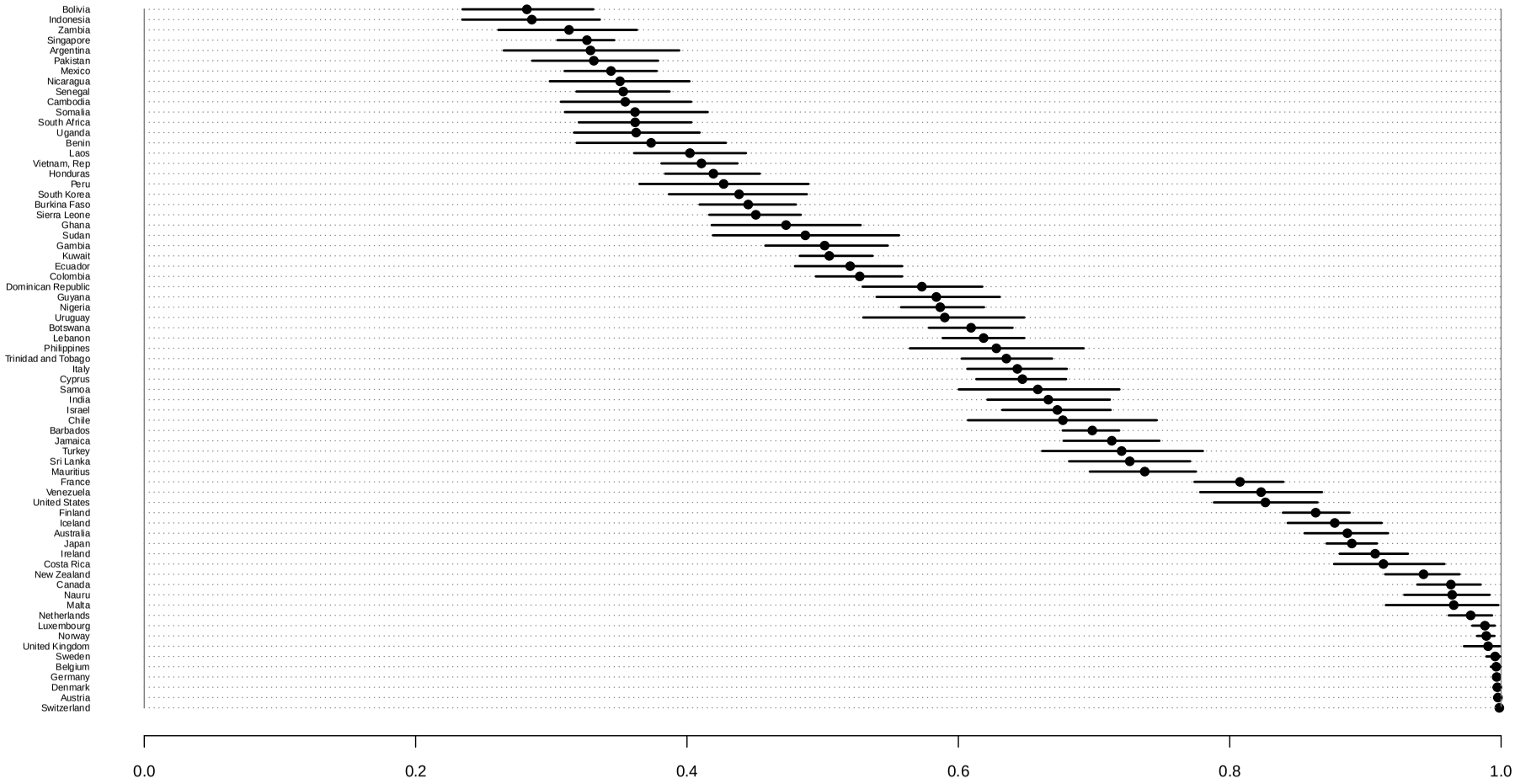


Figure 24: Media System Freedom (MSF) in 137 countries, 1968: Upper Bounds



Media System Freedom (MSF) Sample

Below I list each of the 196 countries in the Media System Freedom (MSF) dataset as well as the time each country covers. I note that in these figures, Russia indicates the Soviet Union, Serbia indicates Yugoslavia, Germany indicates West Germany, and Czech Republic indicates Czechoslovakia.

Table 16: Sample Summary: Countries and Time Coverage (Afghan.-Liech.)

Afghanistan (1960-2016)	Chad (1960-2016)	Ghana (1960-2016)
Albania (1960-2016)	Chile (1960-2016)	Greece (1960-2016)
Algeria (1962-2016)	China (1960-2016)	Grenada (1974-2016)
Andorra (1993-2016)	Colombia (1960-2016)	Guatemala (1960-2016)
Angola (1975-2016)	Comoros (1975-2016)	Guinea (1960-2016)
Antig. and Barb. (1981-2016)	Congo, DR (1960-2016)	Guinea-Bissau (1973-2016)
Argentina (1960-2016)	Congo, Rep (1960-2016)	Guyana (1966-2016)
Armenia (1991-2016)	Costa Rica (1960-2016)	Haiti (1960-2016)
Australia (1960-2016)	Cote d'Ivoire (1960-2016)	Honduras (1960-2016)
Austria (1960-2016)	Croatia (1991-2016)	Hungary (1960-2016)
Azerbaijan (1991-2016)	Cuba (1960-2016)	Iceland (1960-2016)
Bahamas (1973-2016)	Cyprus (1960-2016)	India (1960-2016)
Bahrain (1971-2016)	Czech Repub. (1960-2016)	Indonesia (1960-2016)
Bangladesh (1971-2016)	Denmark (1960-2016)	Iran (1960-2016)
Barbados (1966-2016)	Djibouti (1977-2016)	Iraq (1960-2016)
Belarus (1991-2016)	Dominica (1979-2016)	Ireland (1960-2016)
Belgium (1960-2016)	Dom. Repub. (1960-2016)	Israel (1960-2016)
Belize (1981-2016)	East Timor (2000-2016)	Italy (1960-2016)
Benin (1960-2016)	Ecuador (1960-2016)	Jamaica (1962-2016)
Bhutan (1960-2016)	Egypt (1960-2016)	Japan (1960-2016)
Bolivia (1960-2016)	El Salvador (1960-2016)	Jordan (1960-2016)
Bosnia (1992-2016)	Eq. Guinea (1968-2016)	Kazakhstan (1991-2016)
Botswana (1966-2016)	Eritrea (1993-2016)	Kenya (1963-2016)
Brazil (1960-2016)	Estonia (1991-2016)	Kiribati (1979-2016)
Brunei (1984-2016)	Ethiopia (1960-2016)	Kuwait (1961-2016)
Bulgaria (1960-2016)	Fiji (1970-2016)	Kyrgyzstan (1991-2016)
Burkina Faso (1960-2016)	Finland (1960-2016)	Laos (1960-2016)
Burundi (1962-2016)	France (1960-2016)	Latvia (1991-2016)
Cambodia (1960-2016)	Gabon (1960-2016)	Lebanon (1960-2016)
Cameroon (1960-2016)	Gambia (1965-2016)	Lesotho (1966-2016)
Canada (1960-2016)	Georgia (1991-2016)	Liberia (1960-2016)
Cape Verde (1975-2016)	Germany (1960-2016)	Libya (1960-2016)
Cen. Af. Rep. (1960-2016)	Germ., East (1960-1990)	Liechtenstein (1990-2016)

Table 17: Sample Summary: Countries and Time Coverage (Lithuania-Zimbabwe)

Lithuania (1991-2016)	Palau (1994-2016)	Suriname (1975-2016)
Luxembourg (1960-2016)	Panama (1960-2016)	Swaziland (1968-2016)
Macedonia (1991-2016)	P. New Guinea (1975-2016)	Sweden (1960-2016)
Madagascar (1960-2016)	Paraguay (1960-2016)	Switzerland (1960-2016)
Malawi (1964-2016)	Peru (1960-2016)	Syria (1960-2016)
Malaysia (1960-2016)	Philippines (1960-2016)	Taiwan (1960-2016)
Maldives (1965-2016)	Poland (1960-2016)	Tajikistan (1991-2016)
Mali (1960-2016)	Portugal (1960-2016)	Tanzania (1961-2016)
Malta (1965-2016)	Qatar (1971-2016)	Thailand (1960-2016)
Marsh. Isls. (1986-2016)	Romania (1960-2016)	Togo (1960-2016)
Mauritania (1960-2016)	Russia (1960-2016)	Tonga (1999-2016)
Mauritius (1968-2016)	Rwanda (1962-2016)	Trin. and Tobago (1962-2016)
Mexico (1960-2016)	Samoa (1962-2016)	Tunisia (1960-2016)
Micronesia (1986-2016)	San Marino (1992-2016)	Turkey (1960-2016)
Moldova (1991-2016)	São Tome (1975-2016)	Turkmenistan (1991-2016)
Monaco (1993-2016)	Saudi Arabia (1960-2016)	Tuvalu (2000-2016)
Mongolia (1960-2016)	Senegal (1960-2016)	Uganda (1962-2016)
Montenegro (2006-2016)	Serbia (1960-2016)	Ukraine (1991-2016)
Morocco (1960-2016)	Seychelles (1976-2016)	UAE (1971-2016)
Mozambique (1975-2016)	Sierra Leone (1961-2016)	United Kingdom (1960-2016)
Myanmar (1960-2016)	Singapore (1965-2016)	United States (1960-2016)
Namibia (1990-2016)	Slovakia (1993-2016)	Uruguay (1960-2016)
Nauru (1968-2016)	Slovenia (1991-2016)	Uzbekistan (1991-2016)
Nepal (1960-2016)	Sol. Islands (1978-2016)	Vanuatu (1980-2016)
Netherlands (1960-2016)	Somalia (1960-2016)	Venezuela (1960-2016)
New Zealand (1960-2016)	S. Africa (1960-2016)	Vietnam (1960-2016)
Nicaragua (1960-2016)	S. Korea (1960-2016)	Vietnam, Rep (1960-1975)
Niger (1960-2016)	Spain (1960-2016)	Yemen (1960-2016)
Nigeria (1960-2016)	Sri Lanka (1960-2016)	Yemen, South (1967-1990)
North Korea (1960-2016)	St. Kit./Nev. (1983-2016)	Zambia (1964-2016)
Norway (1960-2016)	St. Lucia (1979-2016)	Zimbabwe (1964-2016)
Oman (1960-2016)	St. Vin./Gren. (1979-2016)	
Pakistan (1960-2016)	Sudan (1960-2016)	

MSF Time-Series Estimates, 1960-2016 in 196 Countries

Below I include the time-series, MSF scores for all 196 countries in the sample. Higher score indicate greater media freedom. Error bars indicate 80% posterior credible intervals. I note that in these figures, Russia indicates the Soviet Union, Serbia indicates Yugoslavia, Germany indicates West Germany, and Czech Republic indicates Czechoslovakia.

Figure 25: MSF Time-Series Estimates, 1960-2016 in Afghanistan to Brunei

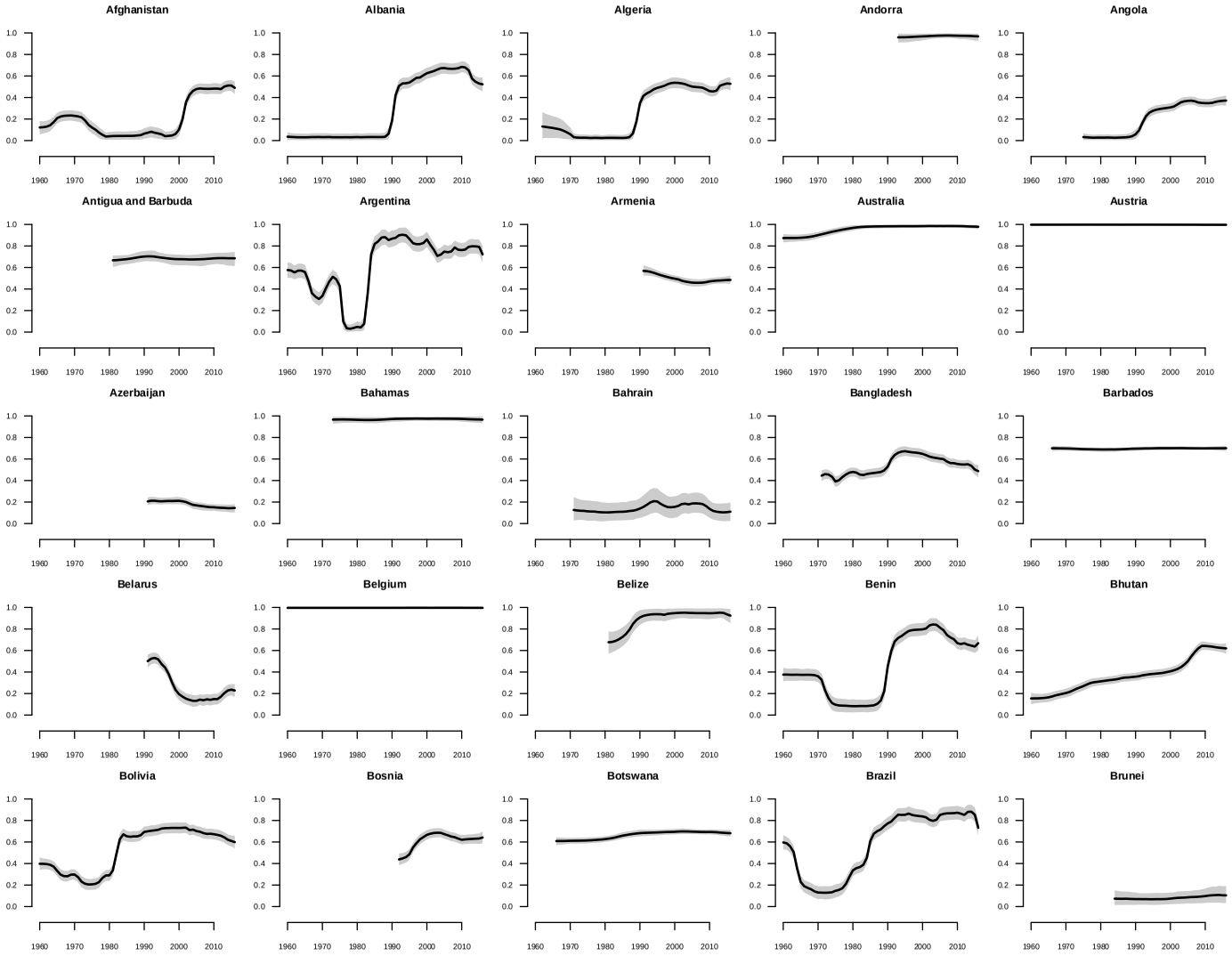


Figure 26: MSF Time-Series Estimates, 1960-2016 in Bulgaria to Dominican Republic

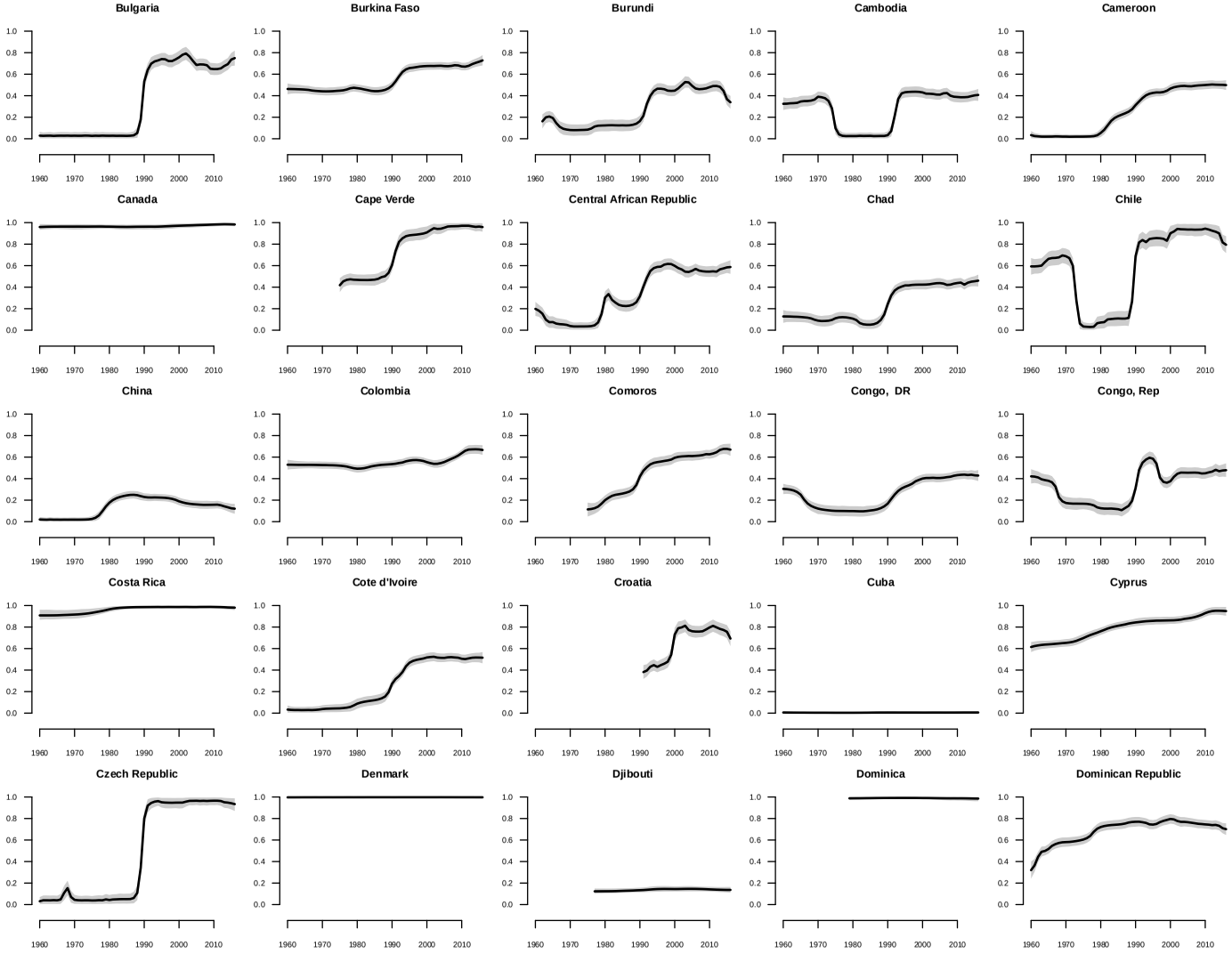


Figure 27: MSF Time-Series Estimates, 1960-2016 in East Timor to Honduras

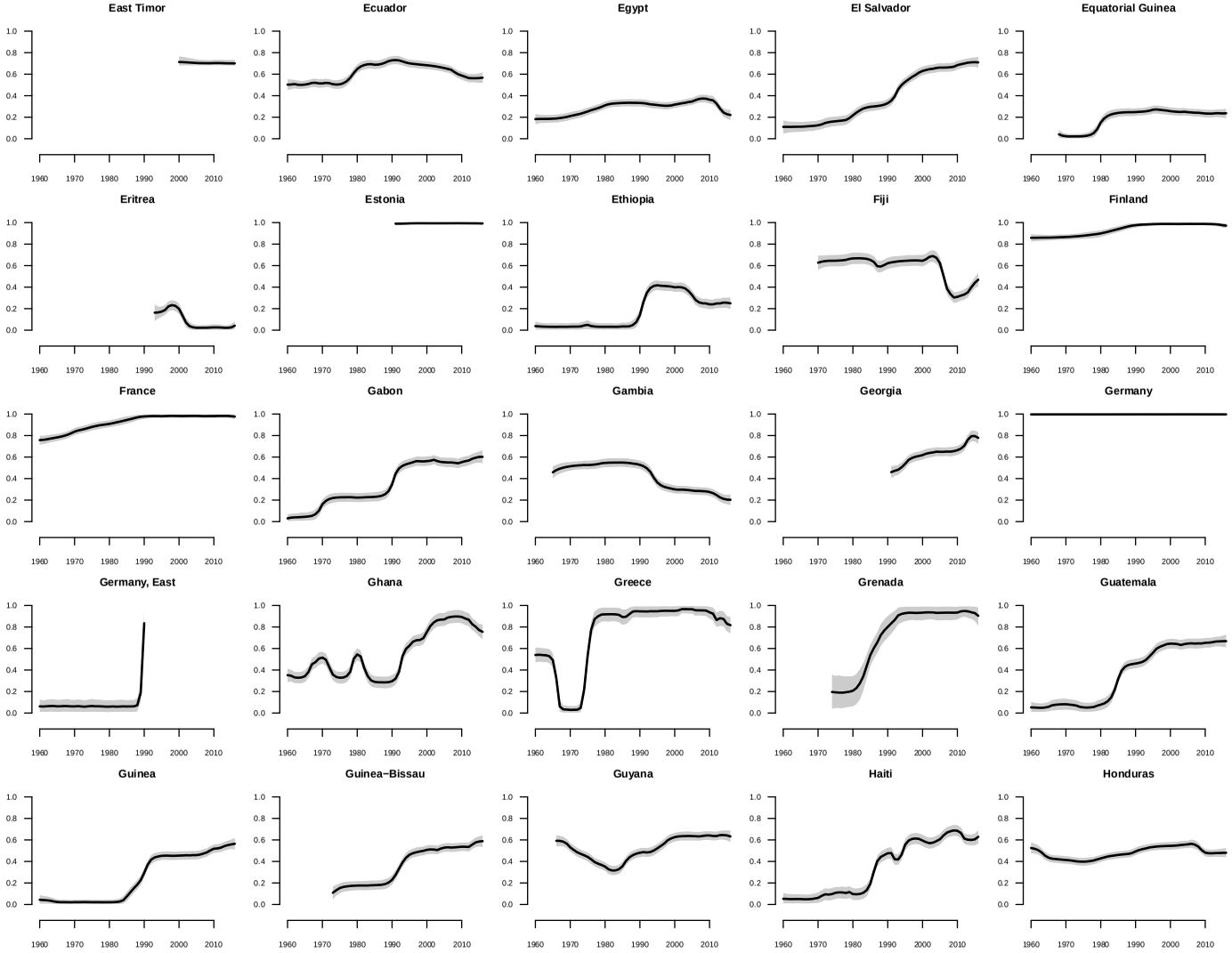


Figure 28: MSF Time-Series Estimates, 1960-2016 in Hungary to Lithuania

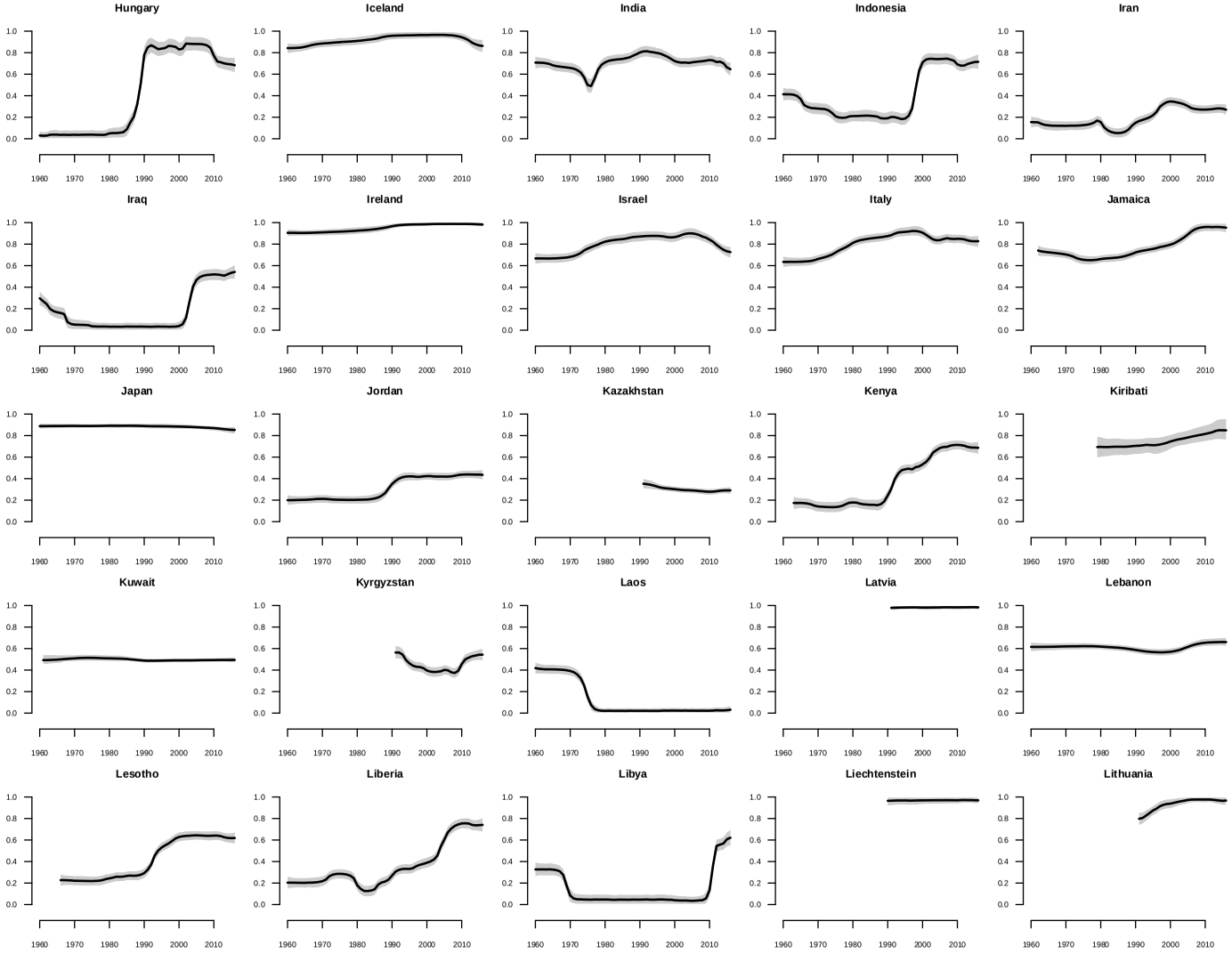


Figure 29: MSF Time-Series Estimates, 1960-2016 in Luxembourg to New Zealand

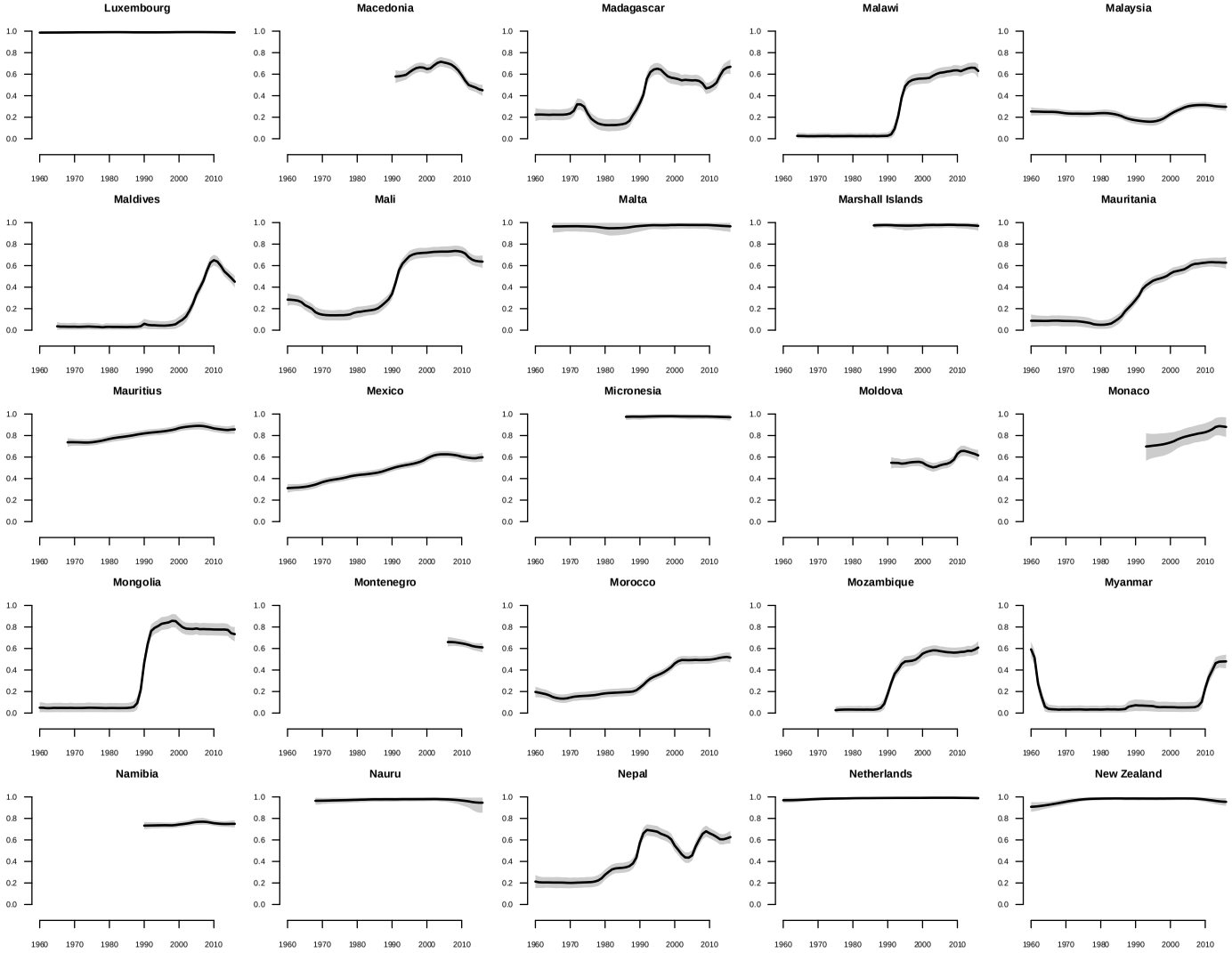


Figure 30: MSF Time-Series Estimates, 1960-2016 in Nicaragua to Serbia (Yugoslavia)

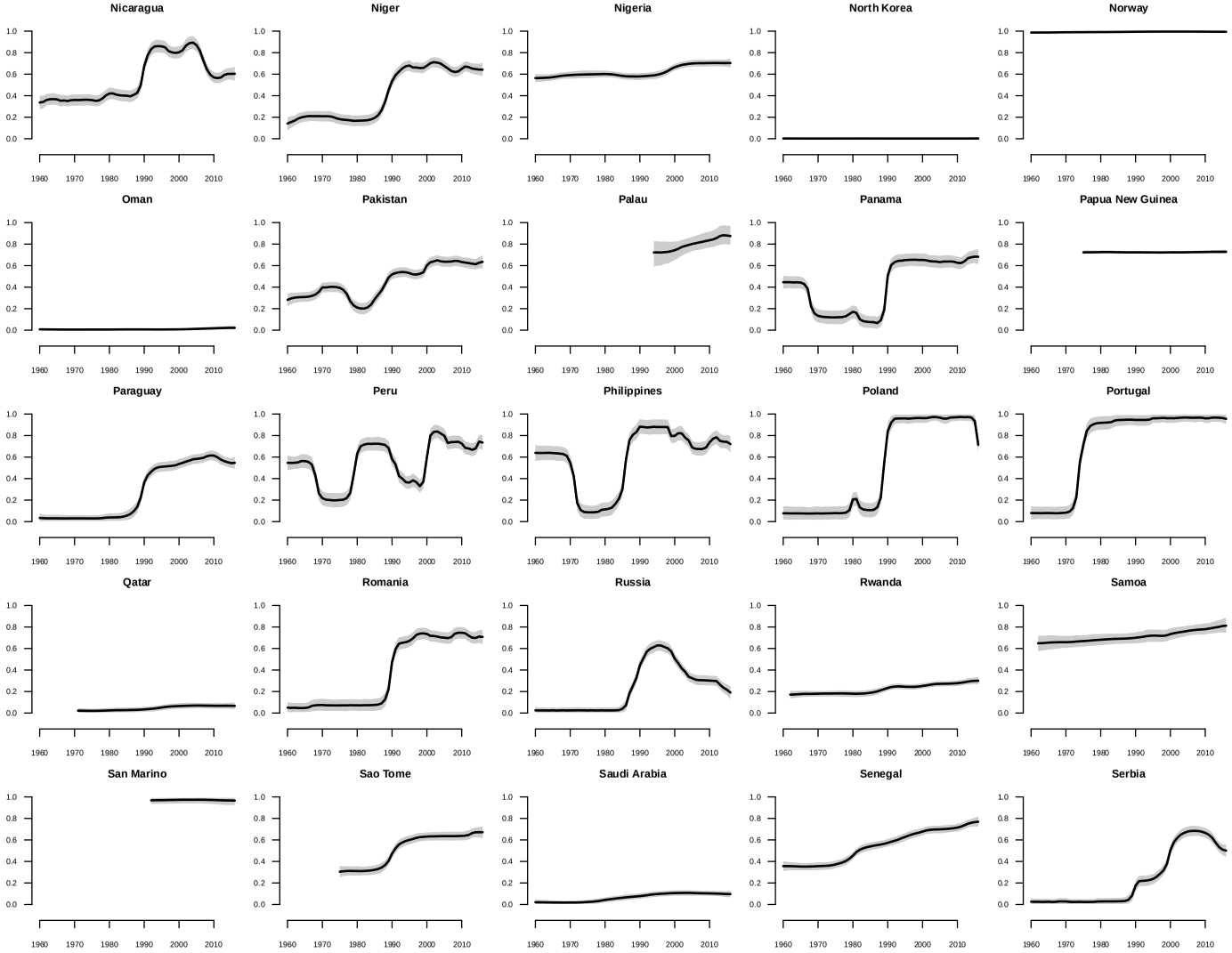


Figure 31: MSF Time-Series Estimates, 1960-2016 in Seychelles to Togo

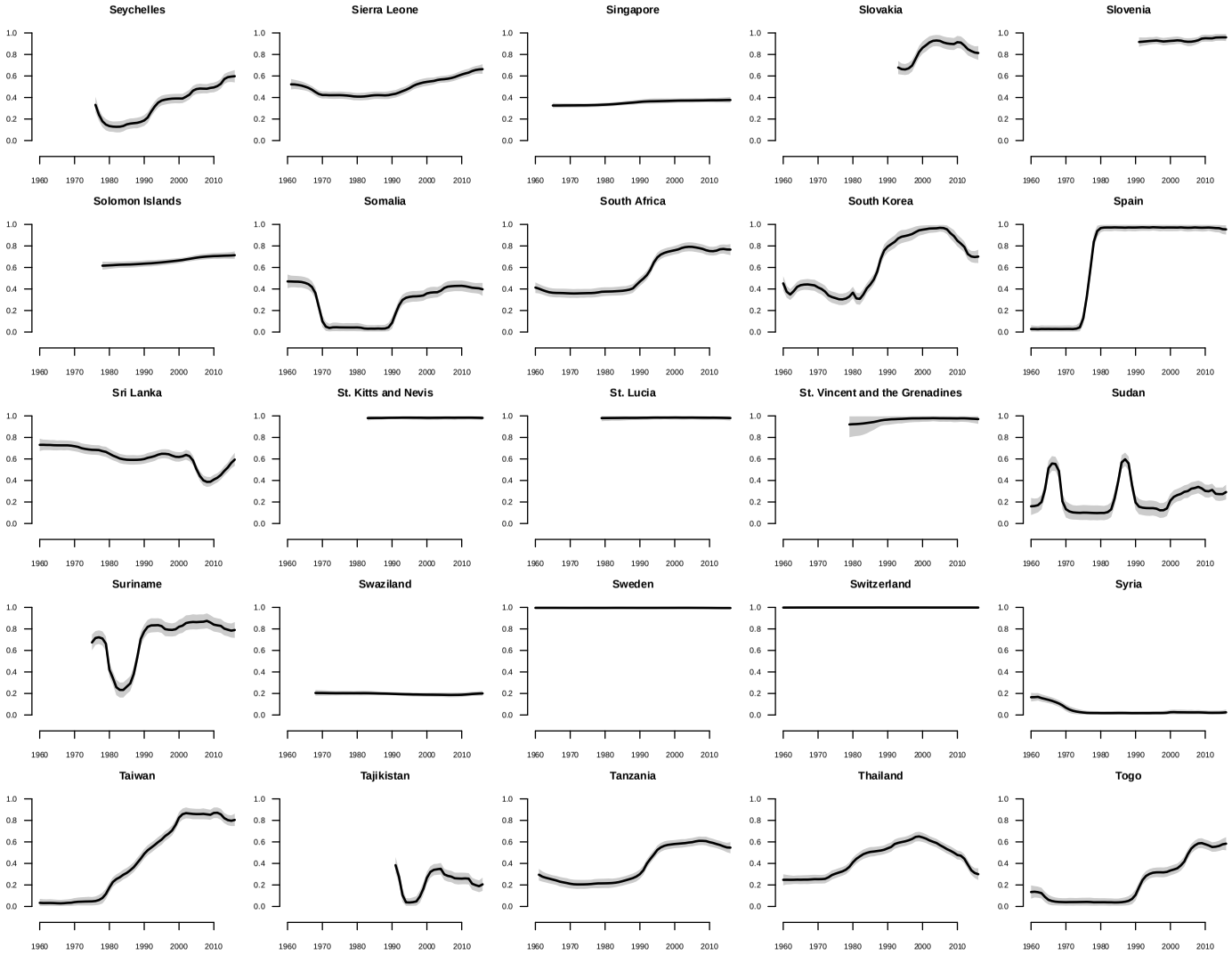
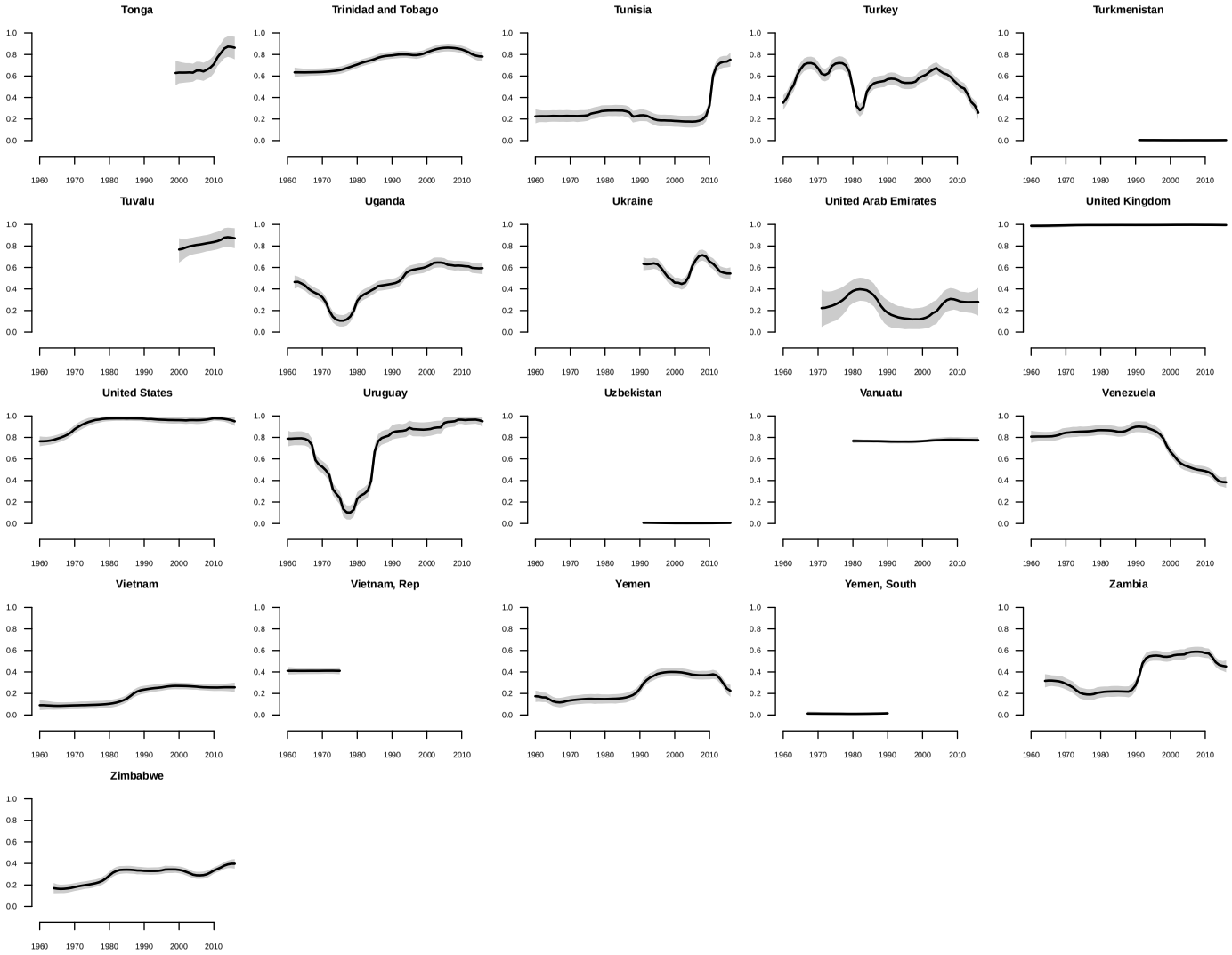


Figure 32: MSF Time-Series Estimates, 1960-2016 in Tonga to Zimbabwe



Appendix B - Judicial Ind. and Attacks against Media

Summary Statistics

I provide the summary statistics for every variables I use in chapter 3 and its appendix's regression models in tables 18 and 19 below. In Table 18, I provide separate summary statistics for the base models because they cover a longer time period from 1948 to 2012. Table 19 covers variables used in the appendix. Also, I provide more detailed variable descriptions and data sources throughout the analyses below.

Main Variables

Table 18: Variables Used in Main Analysis

Variable	Obs	Mean	Std. Dev.	Min	Max
Base Model: 1948-2012					
Media Harassment (V-Dem)	10,155	3.920306	1.619776	0.0030534	7.094429
Trd. Media Censorship (V-Dem)	10,155	3.695578	1.613433	0.0051956	6.830739
Media Self-Censorship (V-Dem)	10,155	3.283663	1.544131	0.0087313	6.432794
Judicial Independence (*de facto*)	9,126	0.4224051	0.3066653	0.0102	0.9953
Electoral Democracy (V-Dem)	9,994	0.3889168	0.2856844	0.0096363	0.9470936
Main Model: 1970-2012					
Media Harassment (V-Dem)	6,873	3.744058	1.618155	0.0030534	7.094429
Trd. Media Censorship (V-Dem)	6,873	3.51245	1.645308	0.0051956	6.799659
Internet Censorship (V-Dem)	3,098	1.927943	1.371284	0.0019167	7.151291
Media Self-Censorship (V-Dem)	6,873	3.096036	1.564527	0.0087313	6.432794
Judicial Independence (*de facto*)	6,803	0.4409842	0.3071346	0.0102	0.9953
Electoral Democracy (V-Dem)	6,820	0.4365907	0.2876695	0.0124307	0.9470936
GMF (binary)	7,069	0.6303579	0.4827419	0	1
Secondary Education (V-Dem)	5,771	49.3216	29.95784	0.1	100
Information Flows	6,607	46.62341	23.1332	1	98.12312
ln(GDP p/c)	6,125	7.49361	1.611128	4.054134	11.65929
Foreign Aid (% GNI)	6,070	5.458643	9.555459	-0.675395	181.1032
Resource Wealth	7,069	1014.312	4870.62	0	96481.56
Intrastate Conflict	7,069	0.2287452	0.5570354	0	2

Robustness Checks

Table 19: Variables Used for Robustness Checks

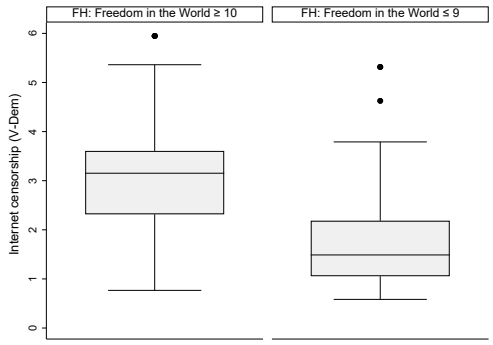
Variable	Obs	Mean	Std. Dev.	Min	Max
Robustness Checks: 1970-2012					
Goldstone et al. (2010)	6,299	1.43626	1.104938	0	3
Cheibub et al. (2010)	6,116	0.4166122	0.4930377	0	1
BMR (2012)	6,470	0.4290572	0.4949798	0	1
ln(Foreign Direct Investment)	5,913	17.32578	5.974108	0	27.32179
ln(GDP)	6,128	23.3123	2.318721	16.7296	30.41327
GDP p/c Δ	6,010	2.011149	6.938558	-64.99631	172.7522
ln(Military Expenditure)	6,341	12.46837	3.1238	0	20.35741
Defense Burden	5,764	-10.42617	2.010971	-18.19642	0
ln(Military Personnel)	6,613	3.441647	1.885955	0	8.465899
ln(Urban Population)	6,751	6.758774	2.92306	0	13.32601
British Colony	7,069	0.2706182	0.4443106	0	1
French Colony	7,069	0.1590041	0.3657058	0	1
Spanish Colony	7,069	0.115575	0.3197373	0	1
Portuguese Colony	7,069	0.0364974	0.1875375	0	1
Colonized	7,069	0.6425237	0.4792906	0	1
Successful Coup	6,669	0.0188934	0.1415588	0	2
Attempted Coup	6,669	0.0356875	0.2076471	0	4
Plotted Coup	6,669	0.0137952	0.1216826	0	2
Alleged Coup Plot	6,669	0.0169441	0.1402101	0	3
Unsuccessful Coup Events	6,669	0.0664268	0.2876098	0	4
Coup Events	6,669	0.0853201	0.3350306	0	5
Presidential Election	6,873	0.0986469	0.298209	0	1
National Election	6,873	0.2585479	0.4378685	0	1
National Boycott	6,873	0.3371163	0.6367095	0	3
Presidential Election Boycott	6,873	0.1316747	0.4372489	0	3
Violent Protests	6,035	0.1481359	0.3552641	0	1
Non-violent Protests	6,035	0.0420878	0.2008062	0	1
Protest Size	5,910	0.4450085	1.08838	0	6

Box Plots: Internet Censorship by VDY (2013) Threshold

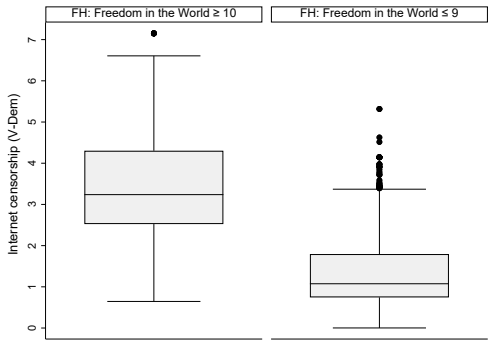
In chapter 3's research design, I argue that attacks against the media occur in countries that meet VonDoepp and Young's (2013) threshold *and* countries, usually undemocratic, that do not. Though I ran the analysis for *Internet censorship*, I did not include it in chapter 3 to save space. I provide the box plot below. Figure 33 shows the box plots for VonDoepp and Young's (2013) sample in Sub-Saharan Africa from 1995 to 2009, then a worldwide sample from 1993 to 2016. Like *traditional censorship* and *media harassment*, the results show internet censorship's presence in closed media systems. In fact, the figures show the means higher in closed systems that open ones. This provides further justification for including a worldwide sample in the regression models, rather than restricting the sample to VonDoepp and Young's (2013) criteria.

Figure 33: Internet Censorship; VonDoepp and Young's (2013) Criteria

(a) Sub-Saharan Africa, 1995-2009



(b) Worldwide, 1993-2016



Note: Higher values indicate greater internet censorship.

Monte Carlo Simulation Replications

Monte Carlo Simulations

Chapter 3's robustness checks section briefly describes results from a number of Monte Carlo simulations that incorporate the latent variables' uncertainty into the model. The Monte Carlo simulations relax the assumption that the point estimates measure the latent concept precisely and incorporate the variable's standard deviation derived from its posterior distribution. To incorporate the data, I simulate models 2, 4, and 6 from chapter 3 focusing on a single latent variable for each set of models. For instance, I focus on traditional media censorship and leave all other variables the same and repeat for all three attacks variables. I then run a set where I vary judicial independence, then another set where I vary electoral democracy. I repeat the same process for self-censorship, but only focus on variables in the simple model: self-censorship and attacks.

I run 750 simulations, randomly drawing an number from the latent variable's posterior distribution each time. I report the mean of those 750 simulations in each model in the appendix. I note that the table indicates which variables I allow to vary by showing them in bold above the model.

Table 20 shows results the simulation results from modeling different attacks against the media, while table 21 shows them modeling journalists self-censorship. My inferences remain largely the same. One notable exception appears in table 21. The initial significant, positive association between *media self-censorship* and *Internet censorship* diminishes to $p < 0.05$ when in model 3, and becomes insignificant in model 6. Overall, my inferences remain the same, though my confidence that *internet censorship* is positively and significantly associated with *media self-censorship* somewhat diminishes.

Table 20: Monte Carlo Simulation Results: Ch. 3, Table 4 (Attacks)

	(1) Trd. Media	(2) Harassment	(3) Internet	(4) Trd. Media	(5) Harassment	(6) Internet	(7) Trd. Media	(8) Harassment	(9) Internet
	β/(SE)	β/(SE)	β/(SE)	β/(SE)	β/(SE)	β/(SE)	β/(SE)	β/(SE)	β/(SE)
Latent Measure	**Trd. Media**	**Harassment**	**Internet**	**Jud. Ind.**	**Jud. Ind.**	**Jud. Ind.**	**Elec. Dem.**	**Elec. Dem.**	**Elec. Dem.**
Jud. Ind.	-1.261***	-1.352***	.102	-.577***	-.534***	.022	-.889***	-.753***	-.297**
	(.229)	(.197)	(.684)	(.079)	(.072)	(.147)	(.078)	(.067)	(.138)
Elec. Dem.	-3.298***	-2.875***	-2.312***	-1.434***	-1.182***	-.618***	-1.204***	-1.055***	-.407**
	(.221)	(.187)	(.56)	(.077)	(.066)	(.115)	(.075)	(.069)	(.13)
JI*ED	.727*	1.479***	.008	.746***	.842***	-.063	.961***	1.002***	.201
	(.365)	(.304)	(1.102)	(.122)	(.116)	(.213)	(.114)	(.069)	(.211)
Open Media	-.024	-.101	.019	-.033	-.087	-.146**	-.042	-.091	-.171**
	(.158)	(.139)	(.253)	(.055)	(.051)	(.061)	(.058)	(.049)	(.059)
Education	.0001	.005***	-.001	.001	.0018***	-.001	.001	.0015**	.001
	(.001)	(.001)	(.003)	(.001)	(.001)	(.001)	(.001)	(.0005)	(.001)
Info. Flows	-.001	-.003	.003	.001	-.001	.001	.001	-.001	.001
	(.002)	(.001)	(.004)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
ln(GDP p/c)	.001	.089	.093	-.011	.026*	.056*	-.006	.026*	.062**
	(.042)	(.036)	(.093)	(.015)	(.0133)	(.022)	(.014)	(.013)	(.021)
Aid (% GNI)	-.005	-.0048	.001	-.0035***	-.0035***	-.001	-.003***	-.003***	-.001
	(.0029)	(.0025)	(.005)	(.001)	(.001)	(.001)	(.0009)	(.0008)	(.001)
Res. Wealth	-.001	-.001	.001	-.001	-.001	-.001	-.001	-.001	-.001
	(.004)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Conflict	.018	.029	-.013	.034**	.027**	-.016	.036**	.029**	-.016
	(.033)	(.029)	(.062)	(.011)	(.01)	(.014)	(.0115)	(.01)	(.015)
Countries	158	158	157	158	158	157	158	158	157
N	5106	5106	2412	5106	5106	2412	5106	5106	2412
Years	1970-2012	1970-2012	1993-2012	1970-2012	1970-2012	1993-2012	1970-2012	1970-2012	1993-2012

Monte Carlo Simulation w/ 750 iterations; β = mean of 750 coefficient estimates; (SE)= mean of 750 standard errors
Bold indicates elastic measure
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 21: Monte Carlo Simulation Results: Ch. 3, Table 5 (Self-Censorship)

	(1) Self-Censor	(2) Self-Censor	(3) Self-Censor	(4) Self-Censor	(5) Self-Censor	(6) Self-Censor
	β/(SE)	β/(SE)	β/(SE)	β/(SE)	β/(SE)	β/(SE)
Latent Measure	**Self-Censor**	**Self-Censor**	**Self-Censor**	**Govt. Censorship**	**Md. Harassment**	**Internet**
Govt. Censorship	.382***			.073***		
	(.026)			(.006)		
Md. Harassment		.386***			.078***	
		(.029)			(.007)	
Internet Cens.			.125*			.011
			(.049)			(.007)
Jud. Ind.	.083	.011	-.031	-.051	.063	.021
	(.155)	(.158)	(.311)	(.054)	(.052)	(.081)
Elec. Dem.	-1.419***	-1.721***	-2.615***	-.776***	-.831***	-.941***
	(.171)	(.168)	(.324)	(.059)	(.057)	(.088)
Open Media	.156	.212	.101	.038	.049	-.031
	(.153)	(.158)	(.201)	(.054)	(.051)	(.049)
Education	.006***	.005**	.003	.002***	.002***	.001*
	(.001)	(.001)	(.002)	(.0005)	(.0005)	(.0007)
Info. Flows	-.001	.001	.001	.001	.001	.001
	(.001)	(.001)	(.003)	(.001)	(.001)	(.001)
ln(GDP p/c)	.041	- .024	-.037	.023	.009	.002
	(.037)	(.041)	(.076)	(.013)	(.013)	(.019)
Aid (% GNI)	.001	.001	-.001	-.002*	-.002*	-.001
	(.001)	(.002)	(.004)	(.0009)	(.0009)	(.001)
Res. Wealth	-.001	-.001	.001	-.001	-.001	.001
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Conflict	-.024	-.028	.024	.013	.012	.013
	(.031)	(.031)	(.054)	(.011)	(.011)	(.013)
Countries	158	158	158	158	158	158
N	5106	5106	2542	5106	5106	2542
Years	1970-2012	1970-2012	1993-2012	1970-2012	1970-2012	1993-2012

Monte Carlo Simulation w/ 750 iterations; **Bold** indicates elastic measure;
β = mean of 750 coefficient estimates; (SE)= mean of 750 standard errors;
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Regional Regression Results

Stratifying the Sample by Geographic Region

The data's richness allows me to explore regional variation in addition to the main analyses. Hadenius and Teorell (2007) identify 10 unique world regions, but I combine certain groups when they are culturally and geographically similar to simplify the analysis. My group 1 mirrors Hadenius and Teorell's Group 1 exactly and includes all former Soviet Republics and former communists countries but does not include Mongolia (EECA). My group 2 represents Latin America and the Caribbean (LAC) and includes Hadenius and Teorell's group 2 (Latin America) and group 10 (Caribbean). My group 3 represents the Middle East and North Africa (MENA) and mirrors Hadenius and Teorell's Middle East and North Africa group 3 exactly. Next, I created group 4 including countries in Sub-Saharan Africa (SSA) that mirrors Hadenius and Teorell's Sub-Saharan Africa Group 4. I next created group 5, called Western Europe and North America (WENA) that mirrors Hadenius and Teorell's group 5 that includes all Western European countries as well as Canada, the United States, Australia, and New Zealand. Finally, I created group 6 that represents Asia (Asia) that includes Hadenius and Teorell's groups 6, 7, 8 and 9 representing the East Asia, South Asia, Southeast Asia, and Pacific Asia groups respectively.

I show the marginal effects for *traditional censorship* in chapter 3 but do not display the regression tables. I provide them here and also show the regression results and marginal effects for *media harassment* below. The regression tables largely match my expectations. In table 22, regional analysis of the conditional effect of judicial independence and electoral democracy on government censorship efforts show the results generally hold. MENA represents the only exception, though electoral democracy is negative and statistically significant. Also, the interaction terms are positive and statistically significant in all models save EECA and MENA countries. Given EECA's regression results, the ceiling effect *still* holds. This finding underscores the importance Brambor et al. (2005) placed on substantively graphing interaction terms.

Media harassment also returns similar results in table 23. Here we find LAC's interaction terms is insignificant, though the graphed results indicate the presence of the theorized ceiling effect. Asia shows no effect. In sum, the regional analysis indicates strong support for the ceiling effect in Eastern Europe, Latin America, Sub-Saharan Africa, and North America and Western Europe. The evidence for Asia and the Middle East, however, remained mixed. These results overall support chapter 3's hypothesis 1 of a ceiling effect of judicial independence on attacks depending on democracy level.

Traditional Media Censorship by Region, 1970-2012

Table 22: Traditional Media Censorship by Region, 1970-2012

	(1) EECA	(2) LAC	(3) MENA	(4) SSA	(5) WENA	(6) Asia
Judicial Independence	-1.713**	-1.565***	.079	-.556***	-1.346***	-1.045***
	(.542)	(.258)	(.213)	(.111)	(.360)	(.257)
Electoral Democracy	-2.9***	-1.684***	-1.583***	-1.024***	-1.899***	-2.269***
	(.461)	(.209)	(.197)	(.134)	(.206)	(.304)
Jud. Ind.*Elec. Demo.	1.282	1.393***	.572	.707**	1.986***	1.893***
	(.812)	(.360)	(.331)	(.225)	(.439)	(.506)
Open Media	-.09	-	-.040	.006	-	.024
	(.078)	-	(.181)	(.280)	-	(.115)
Education	.003	-.001	.002*	-.002	.003**	-.001
	(.003)	(.002)	(.001)	(.001)	(.001)	(.002)
Information Flows	-.01	.005	-.001	-.003*	.001	-.001
	(.002)	(.003)	(.001)	(.002)	(.002)	(.003)
ln(GDP p/c)	-.273***	-.07	.043	.021	-.217***	.050
	(.07)	(.056)	(.031)	(.028)	(.053)	(.047)
Aid (% GNI)	-.018***	-.001	-.002	-.001	-	-.005*
	(.005)	(.003)	(.002)	(.001)	-	(.002)
Resource Wealth	.00007*	.001	-.0000026*	.001	-.001	-.001
	(.000035)	(.001)	(.0000011)	(.001)	(.001)	(.001)
Conflict	.038	.197***	.008	.043*	-.039	-.024
	(.049)	(.042)	(.016)	(.018)	(.039)	(.025)
R^2	.87	.91	.9	.91	.91	.9
AIC	222.11	850.77	-503.95	314.76	-312.47	367.75
Countries	25	24	18	46	21	24
N	478	977	579	1469	877	726

Standard errors in parentheses; Lagged DVs and intercepts not reported; Country and year effects; (−) indicates variable dropped due to collinearity

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Media Harassment by Region, 1970-2012

Table 23: Media Harassment by Region, 1970-2012

	(1) EECA	(2) LAC	(3) MENA	(4) SSA	(5) WENA	(6) Asia
Judicial Independence	-2.021***	-.477*	-.568*	-.489***	-3.269***	-.234
	(.417)	(.203)	(.231)	(.112)	(.299)	(.243)
Electoral Democracy	-2.431***	-1.435***	-1.132***	-.941***	-2.382***	-1.735***
	(.367)	(.159)	(.181)	(.135)	(.198)	(.290)
Jud. Ind.*Elec. Demo.	2.503***	.250	.969**	.655**	4.067***	1.440**
	(.626)	(.291)	(.343)	(.228)	(.359)	(.490)
Open Media	-.151*	-	.04	-.267	-	-.082
	(.058)	-	(.187)	(.283)	-	(.109)
Education	.001	.001	.001	.001	.003***	-.002
	(.002)	(.001)	(.001)	(.001)	(.001)	(.002)
Information Flows	-.001	.002	-.001	-.002	-.001	.001
	(.002)	(.002)	(.001)	(.002)	(.001)	(.003)
ln(GDP p/c)	-.125*	.084	.091**	.013	-.032	-.054
	(.055)	(.045)	(.033)	(.028)	(.044)	(.045)
Aid (% GNI)	-.019***	-.001	-.001	-.001	-	-.009***
	(.004)	(.003)	(.002)	(.001)	-	(.002)
Resource Wealth	.001	-.001	-.0000039**	.001	.000	-.001
	(.0001)	(.001)	(.0000011)	(.001)	(.000)	(.001)
Conflict	.195***	.157***	-.012	.008	-.053	.017
	(.037)	(.034)	(.017)	(.018)	(.032)	(.024)
R^2	.89	.91	.92	.91	.92	.85
AIC	-48.02	438.69	-468.14	338.73	-659.35	298.76
Countries	25	24	18	46	21	24
N	478	977	579	1469	877	726

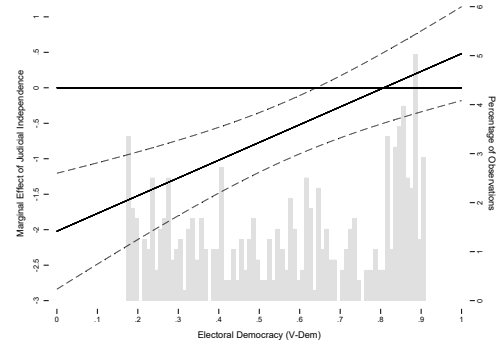
Standard errors in parentheses; Lagged DVs and intercepts not reported; Country and year effects; (−) indicates variable dropped due to collinearity

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

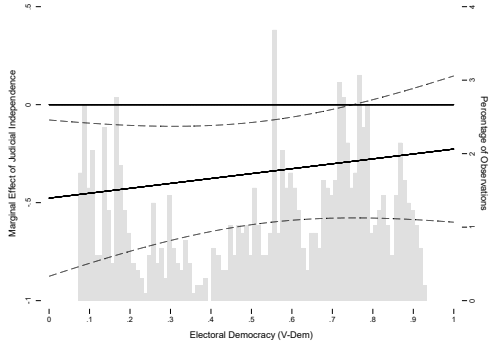
Graph Interaction Term, Media Harassment Models by Region

Figure 34: Marginal Effect of Judicial Ind. across Elc. Demo. Levels, Regional

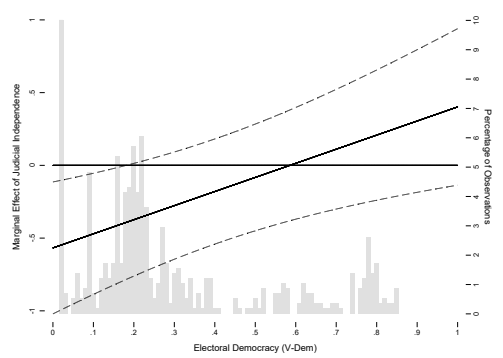
(a) E. Europe/former USSR



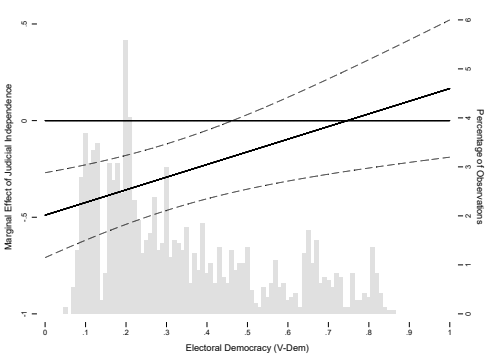
(b) Latin America/Caribbean



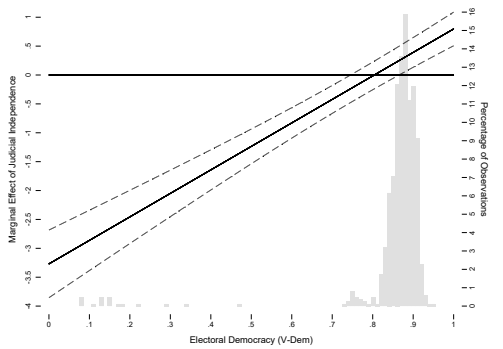
(c) Middle East/N. Africa



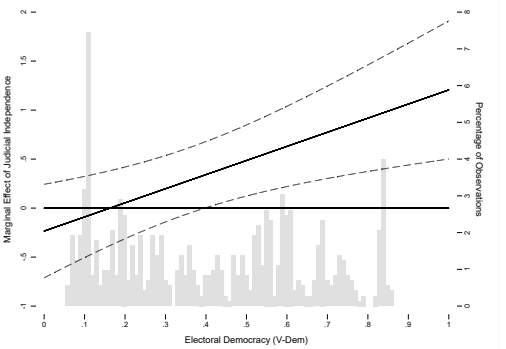
(d) Sub-Saharan Africa



(e) W. Europe/N. America



(f) Asia



Alternative Democracy Measures

While I utilize Variety of Democracy's measure of electoral democracy, I consider other measures including Cheibub et al. (2010), Boix et al. (2013), and Goldstone et al.'s (2010) in the analysis as well. Here I provide pairwise correlation matrices that I referenced in chapter 3 (pg. 55). Table 24 shows the matrix using all available data, which ranges from 1900 to 2008, while table 25 shows a matrix that covers the years closer to the sample from 1970 to 2008. All three alternate measures correlate at least .8 with the V-Dem measure, indicating they highly correlate with it. Different sample year generally do not affect this finding.

Regression results using those variable appear below in table 26. Consistent with the results using V-Dem's data, judicial independence and the democracy measures are negative and statistically significant in all nine models. The interaction terms is positive and statistically significant in all models modeling censorship and media harassment. This indicates a judicial independence's reductive effect decreases as democracy level increases. DD and BMR's binary nature make it difficult to evaluate the presence of a ceiling effect. However, Goldstone's four-category ordinal structure provides me leverage to evaluate it. I expect to see the reductive effect in autocracies (0), partial autocracies (1), partial democracies (2), but not full democracies (3). Figure 34 shows the results, supporting the initial findings.

The results yield other surprising findings regarding *Internet censorship*. First, I find that *judicial independence* is statistically significant ($p < .05$) and negative in all model using the alternative electoral democracy models. I also find that *Internet censorship* displays the ceiling effect in model 9 in table 26 when I use the Goldstone et al. (2010) measure. Though the interaction term is insignificant, figure 35c displays the reductive effect of *judicial independence* in autocracies, partial autocracies, and partial democracies, but not in democracies. This results stand in contrast to models 3 and 6 in the same table, as well as the results in the main analysis, but supports hypothesis 1. In sum, using the alternate measures provide further support for chapter 3's core findings, as well as shows evidence for the ceiling effect for *internet censorship*.

Pairwise Correlation Matrices

Table 24: Pair-wise Correlation Matrix; Democracy variables (1900-2008)

	V-Dem	DD	BMR	Goldstone
V-Dem	1			
DD	0.8293	1		
BMR	0.8466	0.9094	1	
Goldstone	0.8411	0.7899	0.7686	1

Table 25: Pair-wise Correlation Matrix; Democracy variables (1970-2008

	V-Dem	DD	BMR	Goldstone
V-Dem	1			
DD	0.835	1		
BMR	0.8575	0.8966	1	
Goldstone	0.8954	0.7841	0.8071	1

Table 26: Regression Tables: Alternative Democracy Measures

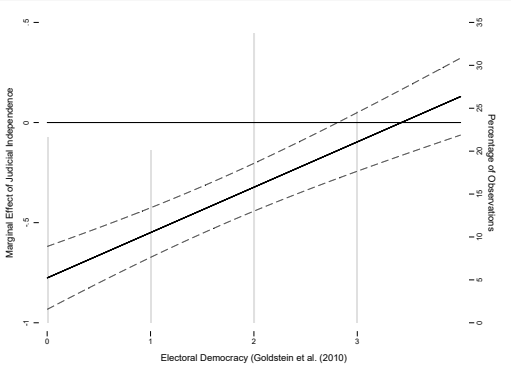
	(1) Trd. Media	(2) Harassment	(3) Internet	(4) Trd. Media	(5) Harassment	(6) Internet	(7) Trd. Media	(8) Harassment	(9) Internet
Judicial Independence	-.851***	-.673***	-.341*	-.792***	-.726***	-.34**	-.776***	-.676***	-.326*
	(.073)	(.066)	(.145)	(.070)	(.063)	(.113)	(.080)	(.069)	(.136)
Electoral Democracy (DD)	-.484***	-.364***	-.018						
	(.041)	(.037)	(.066)						
Jud. Ind.*DD	.534***	.475***	-.108						
	(.091)	(.082)	(.165)						
Electoral Democracy (BMR)				-.375***	-.312***	-.127*			
				(.039)	(.035)	(.057)			
Jud. Ind.*BMR				.387***	.480***	.073			
				(.080)	(.072)	(.120)			
Elec. Dem. (Goldstein et al.)							-.229***	-.190***	-.109***
							(.015)	(.013)	(.033)
Jud. Ind.*Gold							.226***	.207***	.054
							(.033)	(.029)	(.059)
Open Media	-.086	-.122*	-.191*	-.052	-.118*	-.194**	-.043	-.093*	-.186**
	(.066)	(.060)	(.081)	(.061)	(.055)	(.069)	(.054)	(.047)	(.06)
Education	-.000	.001	-.000	.000	.001	.000	-.000	.001	.001
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.000)	(.001)
Information Flows	.000	.000	.002*	.001	.000	.002	-.000	-.001	-.001
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
ln(GDP p/c)	.027	.047**	.058*	.028	.048***	.055*	-.021	.019	.068**
	(.016)	(.015)	(.025)	(.015)	(.014)	(.024)	(.015)	(.013)	(.023)
Aid (% GNI)	-.004***	-.004***	.000	-.003***	-.004***	.000	-.003**	-.003***	-.001
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.002)
Resource Wealth	-.000*	-.000	-.000	-.000*	-.000	-.000	-.000	-.000	-.001
	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)	(.001)
Conflict	.056***	.039***	-.017	.04***	.032**	-.015	.048***	.043***	-.026
	(.012)	(.011)	(.017)	(.012)	(.011)	(.016)	(.011)	(.010)	(.016)
R^2	.89	.88	.65	.89	.88	.64	.90	.90	.67
AIC	1805.2	899.81	-1076.4	1915.44	874.09	-978.11	1267.46	-122.82	-1054.128
Countries	158	158	155	158	158	155	152	152	150
N	4615	4615	1906	4894	4894	2185	4859	4859	2282
Years	1970-2008	1970-2008	1993-2008	1970-2010	1970-2010	1993-2010	1970-2012	1970-2012	1993-2012

Standard errors in parentheses; Lagged DVs and intercepts not reported;
Country and year effects; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

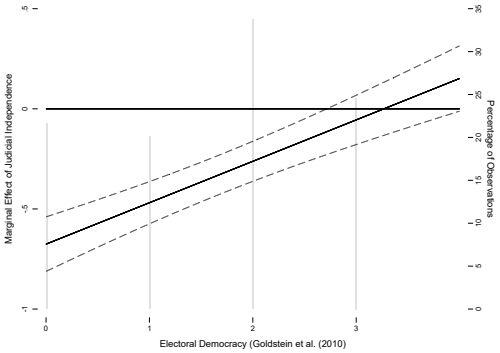
Graphed Interaction Term, Goldstone et al. (2010) Demo. Measure

Figure 35: Marginal Effect of Judicial Independence, Goldstone et al. (2010)

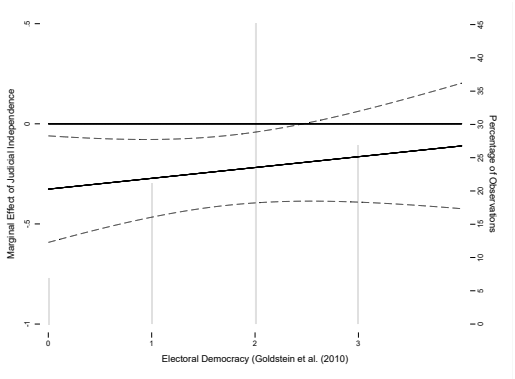
(a) Trd. Censorship



(b) Media Harassment



(c) Internet Censorship



Potential Confounders

The next few sections examine the robustness of my main results by considering additional control variables alternative confounders. I examine economic indicators, national capabilities, colonial background, coup events, presidential and national elections, presidential and national election boycotts, and the presence of protests. I describe the variables here, then provide regression results in subsequent sections. Below, I focus my discussion of the results on the theoretically relevant *judicial independence*, *electoral democracy*, and interaction (*judicial independence*electroal democracy* variables. I expect that judicial independence's coefficient is negative and statistically significant ($p < 0.05$), and that the interaction term be positive and statistically significant ($p < 0.05$), indicating the reductive effect of *judicial independence* itself reduces as electoral democracy levels increase. In sum, my inferences do not change as a result of these additional tests.

Table 27 considers economic indicators and national capabilities. Models 1-3 consider a number of economic indicators, including the natural log of foreign direct investment (FDI), the natural log of Gross Domestic Product (GDP), and change in Gross Domestic Product (GDP Δ). I draw the data from World Bank (World Bank 2017). Models 4-6 in 27 consider a number of national material capabilities variables taken from the Correlates of War's National Material Capabilities (NMC) dataset (Singer at al. 1972). I include the natural log of *military expenditure*, which is state's total military budget in each year. I also include the natural log of *military personnel* which measures the size of a state's military personnel. The dataset defines military personnel as "troops under the command of the national government, intended for use against foreign adversaries, and held ready for combat as of January 1 of the referent year." I also take the natural log of *urban population*, which measures the size of a state's urban population in each year. Finally, I calculate the country's defense burden I also consider a country's defense burden, which is the ratio of a state's military expenditure to its gross domestic product (GDP) (Phillips 2015). The inclusion of these additional economic and national material capabilities variables do not change my inferences.

Table 28 considers the colonial background of countries. Klerman et al. (2011) find that colonial background, rather than simply legal origin, carry residual effects for country's formally colonized by European powers. They argue that "the legal regime was just one of many differences between the various colonial powers." I therefore include variables for colonial background. I collect data from Hadenius and Teorell (2007) that indicate if and which European powers colonized a country, excluding Ireland, Australia, the United States, and other "western countries." I generate five separate dummy variables from this data: (1) British colonies, (2) French colonies, (3) Spanish colonies, (4) Portuguese colonies, and (5) former colony. The latter *former colony* variables codes any country with a European colonial background, including countries colonized by less powerful colonial powers like Germany and Italy. I run the three main models that estimate three government attacks against media variables adding the dummies for specific colonial powers for three models, than another three with the only the *former colony* variables. The addition of these variables do not change my inferences.

Table 29 considers the influence of coup events. I first draw the following four variables from the Center for Systemic Peace's dataset of Coup d'etat Events, 1946-2016 (Marshall and Marshall 2017): counts of coups that were (1) successful, (2) attempted, (3) plotted, and (4) alleged coups in a country-year. From here, I combine *attempted coups*, *plotted coups*, and *alleged coups* to create an *unsuccessful coups* variable, then a catch-all variable that combines all four of the original variables to make a *coup events* variable. I run nine total models to assess the influence of coups. Models 1-3 include the four original variables from Marshall and Marshall (2017), models 4-6 include only *unsuccessful coups*, and models 7-9 include the catch-all *coup events* variable. All nine models reflect and support the previous findings.

Tables 30 considers the influence of presidential and national elections. I draw the data from V-Dem's elections data, making a dummy based on their presidential election variable that identifies country-years with a presidential elections. I also create another for national elections based on V-Dem's national boycott variable. I first take the ordinal variable that measures how widespread national election boycotts were in a country,

and convert this to a dummy to indicate when a national election occurred. I include the latter variable because presidential elections does not include all executive elections. In 1997 for instance, the election that led to Tony Blair's Prime Ministership, I code the United Kingdom a 1 and a 0 for presidential election. I run three models using the *presidential election* dummy, and three others including the *national election* variable. In all six models, my inferences about the conditional effect of *judicial independence* and *electoral democracy* on attacks against media do not change.

Table 31 considers the influence of presidential and national election boycotts. Previous work finds election boycotts hasten hybrid regime fall but negatively affect electoral quality in successor regime (Smith 2014). I therefore include V-Dem's ordinal variable on both presidential and national elections boycotts to assess their effect on my initial results. I run the main models including both of the boycott variables, and find that their inclusion does not change my inferences.

Finally, table 32 considers the influence of protest. Using the Nonviolent and Violent Campaigns and Outcomes (NAVCO) Data Project 2.0, I draw variable for country-years with non-violent and violent protests. I capture the variable from NAVCO's *prim method* that denotes the "the primary type of resistance method used in a campaign year." The options are violent and non-violent, and I separate these into two dummies. I also include a variable for the estimated size of the protest based on NAVCO's *camp size* which contains 6 classifications based on number of participants in the campaign: 1-999; 1000-9,999; 10,000-99,999; 100,000-499,999; 500,000-1 million; and greater than 1 million. I analyze the main models including violent and non-violent variables separately. I also include protest size in all models. I also note that NAVCO 2.0 only provides data up to 2006, so the models run from 1970 to 2006. The results indicate my original inferences do not change.

Please find the results from these different models below.

Table 27: Economic and National Capabilities, 1970-2012

	(1) Trd. Media	(2) Trd. Media	(3) Harassment	(4) Harassment	(5) Internet	(6) Internet
Judicial Independence	-.820***	-.702***	.036	-.745***	-.687***	-.164
	(.084)	(.077)	(.160)	(.081)	(.071)	(.172)
Electoral Democracy	-1.467***	-1.186***	-.530***	-1.448***	-1.167***	-.838***
	(.084)	(.075)	(.134)	(.081)	(.069)	(.144)
Jud. Ind.*Elec. Demo.	1.050***	1.031***	-.152	.842***	.968***	.273
	(.137)	(.125)	(.250)	(.129)	(.112)	(.266)
Open Media	-.035	-.093	-.152**	-.029	-.080	-.151*
	(.055)	(.050)	(.059)	(.059)	(.051)	(.059)
Education	.001	.002***	-.000	.001	.002**	-.000
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Information Flows	.000	-.001	.002	-.000	-.001	.001
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
ln(GDP p/c)	-.073	-.007	-.040	-.019	.028*	.070**
	(.041)	(.037)	(.070)	(.016)	(.014)	(.023)
Aid (% GNI)	-.003*	-.003***	-.001	-.003***	-.003**	-.000
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Resource Wealth	-.000	-.000	-.000	-.000	-.000	-.000
	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
Conflict	.039**	.032**	-.008	.034**	.031**	-.021
	(.012)	(.011)	(.016)	(.012)	(.010)	(.015)
ln(FDI)	-.000	-.000	-.000			
	(.001)	(.001)	(.002)			
ln(GDP)	.056	.023	.099			
	(.044)	(.040)	(.073)			
GDP p/c Δ	.002*	.001	-.001			
	(.001)	(.001)	(.001)			
ln(Military Expenditure)				.015*	-.002	-.009
				(.006)	(.005)	(.009)
Defense Burden				.008	-.004	-.005
				(.005)	(.004)	(.009)
ln(Military Personnel)				-.026*	-.042***	.029
				(.011)	(.010)	(.016)
ln(Urban Population)				.000	.002	-.007
				(.006)	(.005)	(.006)
R^2	.90	.89	.68	.89	.90	.68
AIC	1306.69	413.66	-1183.80	1607.72	224.18	-1189.21
Countries	158	158	156	157	157	155
N	4660	4660	2296	4884	4884	2303

Standard errors in parentheses; Lagged DVs and intercepts not reported;
Country and year effects; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 28: Colonial Background, 1970-2012

	(1) Trd. Media	(2) Trd. Media	(3) Harassment	(4) Harassment	(5) Internet	(6) Internet
Judicial Independence	-.742***	-.665***	.038	-.742***	-.665***	.038
	(.078)	(.071)	(.157)	(.078)	(.071)	(.157)
Electoral Democracy	-1.423***	-1.21***	-.584***	-1.423***	-1.21***	-.584***
	(.078)	(.069)	(.130)	(.078)	(.069)	(.130)
Jud. Ind.*Elec. Demo.	.855***	.98***	-.135	.855***	.980***	-.135
	(.124)	(.111)	(.243)	(.124)	(.111)	(.243)
Open Media	-.037	-.088	-.151*	-.037	-.088	-.151*
	(.056)	(.050)	(.060)	(.056)	(.050)	(.060)
Education	.001	.002**	-.001	.001	.002**	-.001
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Information Flows	.001	-.001	.002	.001	-.001	.002
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
ln(GDP p/c)	-.01	.025	.058**	-.01	.025	.058**
	(.015)	(.013)	(.022)	(.015)	(.013)	(.022)
Aid (% GNI)	-.004***	-.004***	-.001	-.004***	-.004***	-.001
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Resource Wealth	-.001	-.001	-.001	-.001	-.001	-.001
	(.001)	(.001)	(.001)	(.001)	(.001)	(.000)
Conflict	.034**	.028**	-.018	.034**	.028**	-.018
	(.011)	(.01)	(.015)	(.011)	(.010)	(.015)
British	-.164*	.27***	.312***			
	(.079)	(.071)	(.084)			
French	.05	.17	.234*			
	(.103)	(.093)	(.107)			
Spanish	-.451***	-.305**	.305*			
	(.122)	(.109)	(.141)			
Portugal	-.259**	.085	.237**			
	(.084)	(.075)	(.088)			
Colonized				.169*	.704***	.404***
				(.077)	(.071)	(.099)
R^2	.89	.89	.66	.89	.89	.66
Countries	158	158	157	158	158	157
N	5106	5106	2412	5106	5106	2412

Standard errors in parentheses; Lagged DVs and intercepts not reported;
Country and year effects; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 29: Coup Events, 1970-2012

	(1) Trd. Media	(2) Harassment	(3) Internet	(4) Trd. Media	(5) Harassment	(6) Internet	(7) Trd. Media	(8) Harassment	(9) Internet
Judicial Independence	-.746***	-.688***	.052	-.749***	-.698***	.052	-.753***	-.698***	.045
	(.079)	(.070)	(.159)	(.079)	(.070)	(.159)	(.079)	(.070)	(.159)
Elec. Demo.	-1.425***	-1.162***	-.592***	-1.428***	-1.173***	-.590***	-1.431***	-1.169***	-.589***
	(.079)	(.068)	(.131)	(.078)	(.068)	(.131)	(.078)	(.068)	(.132)
Jud. Ind.*Elec. Demo.	.847***	.984***	-.135	.848***	.989***	-.147	.847***	.987***	-.141
	(.124)	(.109)	(.245)	(.124)	(.109)	(.246)	(.124)	(.109)	(.246)
Open Media	-.033	-.085	-.147*	-.033	-.086	-.149*	-.034	-.087	-.150*
	(.056)	(.049)	(.061)	(.056)	(.049)	(.061)	(.056)	(.049)	(.061)
Education	.001	.002**	.000	.001	.002**	.000	.001	.002**	.000
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Information Flows	.000	-.001	.002	.000	-.001	.002	.000	-.001	.002
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
ln(GDP p/c)	-.008	.029*	.059**	-.008	.030*	.057*	-.008	.030*	.057*
	(.015)	(.013)	(.022)	(.015)	(.013)	(.022)	(.015)	(.013)	(.022)
Aid (% GNI)	-.004***	-.003***	-.000	-.004***	-.003***	-.001	-.004***	-.003***	-.001
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Resource Wealth	-.000	-.000	-.000	-.000	-.000	-.000	-.000	-.000	-.000
	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
Conflict	.036**	.029**	-.018	.037**	.031**	-.017	.037**	.030**	-.017
	(.011)	(.010)	(.015)	(.011)	(.010)	(.015)	(.011)	(.010)	(.015)
Successful Coups	.018	.068*	.126*						
	(.031)	(.027)	(.058)						
Attempted Coups	-.033	-.025	-.108**						
	(.021)	(.018)	(.036)						
Plotted Coups	-.060	-.055	.010						
	(.034)	(.030)	(.036)						
Alleged Coups	-.064*	-.032	.030						
	(.030)	(.026)	(.039)						
Unsuccessful Coups				-.046**	-.030*	-.026			
				(.015)	(.013)	(.021)			
Coup Events							-.033*	-.011	-.007
							(.013)	(.011)	(.020)
R^2	.89	.90	.67	.89	.90	.67	.89	.90	.67
AIC	1681.96	321.23	-1080.46	1677.333	322.72	-1071.36	1680.61	327.125	-1069.886
Countries	154	154	153	154	154	153	154	154.000	153.000
N	5021	5021	2364	5021	5021	2364	5021	5021	2364
Years	1970-2012	1970-2012	1993-2012	1970-2012	1970-2012	1993-2012	1970-2012	1970-2012	1993-2012

Standard errors in parentheses; Lagged DVs and intercepts not reported;
Country and year effects; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 30: Presidential and National Elections, 1970-2012

	(1) Trd. Media	(2) Harassment	(3) Internet	(4) Trd. Media	(5) Harassment	(6) Internet
Judicial Independence	-.737***	-.660***	.039	-.741***	-.663***	.038
	(.078)	(.071)	(.157)	(.078)	(.071)	(.157)
Electoral Democracy	-1.415***	-1.202***	-.587***	-1.422***	-1.207***	-.590***
	(.078)	(.069)	(.130)	(.078)	(.069)	(.130)
Jud. Ind.*Elec. Demo.	.848***	.973***	-.136	.853***	.977***	-.134
	(.124)	(.111)	(.243)	(.124)	(.111)	(.243)
Open Media	-.039	-.090	-.150*	-.037	-.088	-.149*
	(.056)	(.050)	(.060)	(.056)	(.050)	(.060)
Education	.001	.002**	-.000	.001	.002**	-.000
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Information Flows	.000	-.000	.002	.000	-.000	.002
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
ln(GDP p/c)	-.009	.025	.057**	-.010	.025	.056*
	(.015)	(.013)	(.022)	(.015)	(.013)	(.022)
Aid (% GNI)	-.004***	-.004***	-.000	-.004***	-.004***	-.000
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Resource Wealth	-.000	-.000	-.000	-.000	-.000	-.000
	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
Conflict	.034**	.028**	-.018	.034**	.028**	-.018
	(.011)	(.010)	(.015)	(.011)	(.010)	(.015)
Presidential Election	-.025	-.025*	.026*			
	(.014)	(.012)	(.013)			
National Election				-.002	-.005	.010
				(.009)	(.008)	(.009)
R^2	.89	.89	.67	.89	.89	.67
AIC	1669.49	542.84	-1144.58	1672.87	546.61	-1141.64
Countries	158	158	157	158	158	157
N	5106	5106	2412	5106	5106	2412

Standard errors in parentheses; Lagged DVs and intercepts not reported;
Country and year effects; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 31: Presidential and National Election Boycotts, 1970-2012

	(1) Trd. Media	(2) Harassment	(3) Internet	(4) Trd. Media	(5) Harassment	(6) Internet
Judicial Independence	-.741***	-.660***	.039	-.735***	-.657***	.038
	(.078)	(.071)	(.157)	(.078)	(.071)	(.157)
Electoral Democracy	-1.421***	-1.205***	-.587***	-1.417***	-1.203***	-.585***
	(.078)	(.069)	(.130)	(.078)	(.069)	(.130)
Jud. Ind.*Elec. Demo.	.852***	.971***	-.135	.845***	.970***	-.135
	(.124)	(.111)	(.243)	(.124)	(.111)	(.243)
Open Media	-.037	-.089	-.149*	-.041	-.093	-.150*
	(.056)	(.050)	(.060)	(.056)	(.050)	(.060)
Education	.001	.002**	-.000	.001	.002**	-.000
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Information Flows	.000	-.000	.002	.000	-.000	.002
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
ln(GDP p/c)	-.010	.025	.056*	-.010	.025	.058**
	(.015)	(.013)	(.022)	(.015)	(.013)	(.022)
Aid (% GNI)	-.004***	-.004***	-.000	-.004***	-.004***	-.000
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Resource Wealth	-.000	-.000	-.000	-.000	-.000	-.000
	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
Conflict	.034**	.028**	-.018	.035**	.029**	-.018
	(.011)	(.010)	(.015)	(.011)	(.010)	(.015)
National Elec. Boycott	-.002	-.009	.011			
	(.007)	(.006)	(.007)			
Presidential Elec. Boycott				-.024*	-.024**	.016
				(.009)	(.008)	(.009)
R^2	.89	.89	.67	.89	.89	.67
AIC	1672.76	544.74	-1142.83	1666.33	538.29	-1143.45
Countries	158	158	157	158	158	157
N	5106	5106	2412	5106	5106	2412

Standard errors in parentheses; Lagged DVs and intercepts not reported;
Country and year effects; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 32: Violent and Non-Violent Protests, 1970-2006

	(1) Trd. Media	(2) Harassment	(3) Internet	(4) Trd. Media	(5) Harassment	(6) Internet
Judicial Independence	-.780***	-.734***	-.492*	-.768***	-.725***	-.524*
	(.089)	(.083)	(.247)	(.089)	(.083)	(.247)
Electoral Democracy	-1.516***	-1.273***	-.911***	-1.505***	-1.266***	-.932***
	(.088)	(.080)	(.198)	(.088)	(.080)	(.199)
Jud. Ind.*Elec. Demo.	.873***	1.011***	.542	.853***	.996***	.581
	(.140)	(.130)	(.354)	(.140)	(.131)	(.355)
Open Media	-.090	-.107	-.159	-.093	-.109	-.161
	(.067)	(.062)	(.090)	(.067)	(.062)	(.090)
Education	.001	.002**	.000	.001	.002**	.000
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Information Flows	.000	-.000	.002	.000	.000	.002
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
ln(GDP p/c)	-.006	.026	.051	-.006	.026	.053
	(.017)	(.016)	(.031)	(.017)	(.016)	(.031)
Aid (% GNI)	-.003**	-.004***	.001	-.003**	-.004***	.001
	(.001)	(.001)	(.002)	(.001)	(.001)	(.002)
Resource Wealth	-.000	-.000	.000	-.000	-.000	.000
	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
Conflict	.062***	.041**	-.041*	.070***	.047***	-.035
	(.015)	(.014)	(.020)	(.014)	(.013)	(.020)
Protest Size	-.071***	-.050***	-.010	-.030***	-.019*	-.001
	(.008)	(.008)	(.011)	(.008)	(.008)	(.014)
Violent Protests	.128***	.098***	.060			
	(.030)	(.028)	(.045)			
Non-Violent Protests				-.156***	-.121***	-.016
				(.039)	(.036)	(.057)
R^2	.89	.88	.61	.89	.88	.61
AIC	1334.09	714.18	-900.02	1336.13	715.21	-898.14
Countries	158	158	154	158	158	154
N	4194	4194	1592	4194	4194	1592

Standard errors in parentheses; Lagged DVs and intercepts not reported;
Country and year effects; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Panel and Simple Vector Autoregression (VAR) Analysis

Here I display the results of the panel and single country vector autoregression (VAR) analyses that I describe in chapter 3. Using the model developed by Love and Zicchino (2006), I analyze the panel sample of *media self-censorship* with *traditional censorship* and *media harassment*. Results from the tests information criteria tests indicate 2 lags for *traditional censorship* and three lags for *media harassment*. Tables 33 and 34 indicate that while the two attacks variables do direct Granger cause (DGC) *media self-censorship*, it also indicates the reverse — that *media self-censorship* direct Granger causes the two government attacks against media variables. Based on these results, it is unclear which variable moves first.

However, the panel structure assumes unit homogeneity,which may not be the case. I also run simple VAR on each individual country in the sample. I note that while the original sample includes 170 countries, I drop 11 from the analysis due to a lack of variation that did not allow the VAR model to run. My estimation strategy here is to run all countries, then count the number of countries that show (1) the attacks variables DGC self-censorship, (2) media self-censorship DGC attacks, (3) simultaneous DGC, and (4) no DGC results. I present this count in tables 35 and 36 below. In both table, the percentage of no DGC is largest at 44% in *traditional censorship*, and 50% in *media harassment*. Based on these results, I am unable to infer that Granger causal arrow between government attacks against media and self-censorship.

Panel Vector Autoregression (VAR) Models

Table 33: PVAR Analysis: Gov't Censorship and Media Self-Censorship

	Gov't Censors. → Self-Censors.	Self-Censors. → Gov't Censors.
Wald test (Lagged 2 years)	$p < 0.01$	$p < 0.01$

n = 8,384; Countries = 170

Table 34: PVAR Analysis: Media Harassment and Media Self-Censorship

	Media Harass. → Self-Censors.	Self-Censors. → Media Harass.
Wald test (Lagged 3 years)	$p < 0.01$	$p < 0.01$

n = 8,214; Countries = 170

Simple Vector Autoregression (VAR) Models

Table 35: Direct Granger Causality; Harassment Acts

Causation Relationship	$p < .05$	*% of countries*
Gov't Censorship → Self-Censorship	27	17% (27 of 159)
Self-Censorship → Gov't Censorship	37	23.2% (37 of 159)
Gov't Censorship ↔ Self-Censorship	24	15.1% (24 of 159)
No Granger-Causation	71	44.7% (71 of 159)

Table 36: Direct Granger Causality; Harassment Acts

Causation Relationship	$p < .05$	*% of countries*
Media Harassment → Self-Censorship	28	17.6% (28 of 159)
Self-Censorship → Media Harassment	33	20.8% (37 of 159)
Media Harassment ↔ Self-Censorship	20	12.6% (20 of 159)
No Granger-Causation	81	51% (81 of 159)

Countries in Sample

Below I list each of the 170 countries in the sample I analyzed for chapter 3 and its appendix. The list breaks down by geographical region (see tables 37 and 38). I note that in these tables, Russia indicates the Soviet Union, Serbia indicates Yugoslavia, Germany indicates West Germany, and Czech Republic indicates Czechoslovakia.

Table 37: Counties in Sample and Regional Grouping

Group 1: Former Communist Eastern Europe and Central Asia			
Albania (1948-2012)	Estonia (1991-2012)	Latvia (1991-2012)	Serbia (1948-2010)
Armenia (1991-2012)	Georgia (1991-2012)	Lithuania (1991-2012)	Slovakia (1994-2012)
Azerbaijan (1991-2012)	Germany, E. (1949-1989)	Maced. (1992-2012)	Slovenia (1991-2012)
Belarus (1991-2012)	Hungary (1948-2012)	Moldova (1991-2012)	Tajiki. (1991-2012)
Bosnia (1993-2010)	Kazakh. (1991-2012)	Poland (1948-2012)	Turkmeni. (1991-2012)
Bulgaria (1948-2012)	Kosovo (2000-2006)	Romania (1948-2012)	Ukraine (1991-2012)
Croatia (1991-2012)	Kyrgyz. (1991-2012)	Russia (1948-2012)	Uzbeki. (1991-2012)
Czech Rep. (1948-2012)			
Group 2: Latin America and the Caribbean			
Argentina (1948-2012)	Cuba (1948-2012)	Haiti (1950-2010)	Paraguay (1948-2012)
Barbados (1966-2010)	Dom. Rep. (1948-2012)	Honduras (1948-2012)	Peru (1948-2012)
Bolivia (1948-2012)	Ecuador (1948-2012)	Jamaica (1953-2012)	Suriname (1967-2012)
Brazil (1948-2012)	El Salv. (1948-2012)	Mexico (1948-2012)	Trin. & T. (1951-2012)
Chile (1948-2012)	Guatemala (1948-2012)	Nicaragua (1948-2012)	Uruguay (1948-2012)
Colombia (1948-2012)	Guyana (1955-2012)	Panama (1948-2012)	Venezuela (1948-2012)
Cost. Ric. (1948-2012)			
Group 3: Middle East and North Africa			
Algeria (1962-2012)	Israel (1949-2012)	Morocco (1956-2012)	Tunisia (1958-2012)
Cyprus (1958-2012)	Jordan (1948-2012)	Oman (2000-2012)	Turkey (1948-2012)
Egypt (1948-2011)	Kuwait (1960-2012)	Qatar (1966-2012)	Yemen (1948-2012)
Iran (1948-2012)	Lebanon (1948-2012)	Saudi A. (1948-2012)	Yemen, S. (1967-1990)
Iraq (1948-2012)	Libya (1952-2012)	Syria (1948-2012)	

Table 38: Counties in Sample and Regional Grouping

Group 4: Sub-Saharan Africa			
Angola (1975-2012)	Djibouti (1977-2012)	Liberia (1948-2012)	Senegal (1960-2012)
Benin (1960-2012)	Eq. Guinea (1968-2012)	Madagas. (1960-2012)	Seychelles (1971-2012)
Botswana (1966-2012)	Eritrea (1993-2012)	Malawi (1964-2012)	Sie. Leone (1958-2012)
Burk. Faso (1960-2012)	Ethiopia (1948-2012)	Mali (1960-2012)	Somalia (1960-2012)
Burundi (1962-2012)	Gabon (1960-2012)	Mauritania (1960-2012)	S. Africa (1948-2012)
Cameroon (1964-2012)	Gambia (1964-2012)	Mauritius (1956-2012)	Sudan (1950-2012)
Cape Verde (1975-2012)	Ghana (1950-2012)	Mozamb. (1978-2012)	Swaziland (1968-2012)
CAF (1960-2012)	Guinea (1958-2012)	Namibia (1980-2012)	Tanzania (1961-2012)
Chad (1960-2012)	Guinea-B. (1974-2012)	Niger (1960-2012)	Togo (1960-2012)
Comoros (1975-2012)	Iv. Coast (1960-2012)	Nigeria (1951-2012)	Uganda (1962-2012)
Congo, DR (1963-2012)	Kenya (1963-2012)	Rwanda (1961-2012)	Zambia (1964-2012)
Congo, Rep (1960-2012)	Lesotho (1966-2012)	S. Tome (1981-2012)	Zimbabwe (1970-2012)
Group 5: Western Europe, North America, Australia, and New Zealand			
Australia (1948-2012)	France (1948-2012)	Italy (1948-2012)	Spain (1948-2012)
Austria (1948-2012)	Germany (1949-2012)	Netherlands (1948-2012)	Sweden (1948-2012)
Belgium (1948-2012)	Greece (1948-2012)	New Zea. (1948-2012)	Switzer. (1948-2012)
Canada (1948-2012)	Iceland (1951-2010)	Norway (1948-2012)	UK (1948-2012)
Denmark (1948-2012)	Ireland (1948-2012)	Portugal (1948-2012)	USA (1948-2012)
Finland (1948-2012)			
Group 6: Asia (South and Southeast Asia			
Afghanistan (1948-2010)	India (1948-2012)	Maldives (1976-2010)	Singapore (1963-2012)
Bangladesh (1972-2012)	Indonesia (1948-2012)	Mongolia (1948-2012)	Sol. Islands (1978-2012)
Bhutan (1948-2012)	Japan (1952-2012)	Myanmar (1948-2012)	Sri Lanka (1948-2012)
Cambodia (1955-2012)	Korea, N. (1948-2012)	Nepal (1948-2012)	Taiwan (1949-2012)
China (1948-2012)	Korea, S. (1948-2012)	Pakistan (1948-2012)	Thailand (1948-2012)
E. Timor (2000-2012)	Laos (1958-2012)	P. N. Guinea (1973-2012)	Vanuatu (1976-2010)
Fiji (1961-2012)	Malaysia (1950-2012)	Philippines (1948-2012)	Vietnam (1954-2010)

Appendix C - Journalist Killings and Regime-type

Summary Statistics

Here I provide the summary statistics for every variable I use in chapter 4 and its appendix's regression models. Below the table, I present the pairwise correlations for the variables I use in the expanded model compared to the ones they replace in the reanalysis.

Table of Variables in Ch. 4 and Appendix C

Table 39: Summary Statistics: Ch. 4 and Appendix C

Variable	Obs	Mean	Std. Dev.	Min	Max
Ch. 4 Analysis Variables					
Journalists Killed (confirmed)	4,251	0.2879323	1.619555	0	33
Regime-type Duration (ln)	4,041	2.714943	1.16782	0	5.379897
Polity	4,041	3.290522	6.540561	-10	10
Quality of Govt. (PRS)	3,184	0.5484973	0.2132373	0.0416667	1
Physical Integrity (CIRI)	3,197	4.761652	2.27122	0	8
Freedom of Speech (CIRI)	3,204	0.9790886	0.7102125	0	2
Intrastate Armed Conflict (PRIO)	4,251	0.2319454	0.5601054	0	2
Information Flows	3,854	56.7342	22.2736	1.51	98.12
Population (ln)	4,218	15.96463	1.678473	11.16709	21.04438
Public Sector Corruption (VDEM)	4,175	0.5112623	0.300913	0.0053535	0.9816402
Freedom of Expression (VDEM)	4,176	0.665779	0.2761284	0.009141	0.9908468
Physical Integrity (VDEM)	4,176	0.6768097	0.2750811	0.021823	0.9926823
Journalists Killed (unconfirmed)	4,251	1.224888	1.100268	1	17
Appendix Analysis Variables					
GDP (ln)	4,093	23.91273	2.269505	18.09537	30.5555
GDP p/c (ln)	4,090	7.951174	1.636315	4.174563	11.68877
Δ GDP p/c	4,062	2.326229	6.842854	-62.22509	172.7522
Media System Freedom Score	4,251	0.5941277	0.2815663	0.0019	0.9992
Homicides (count)	2,137	2549.69	6741.584	1	57091
Homicides (rate)	2,195	8.223481	13.05473	0	139.1321

Pairwise Correlations: New Variables in Expanded Models

Table 40: Pairwise Correlations, 1992-2014

Variables	Pairwise Correlation
QoG (PRS) & Public Sector Crpt. (V-Dem)	-.7889
Phys. Integrity (CIRI) Phys. Integrity (V-Dem)	.6884
Freedom of Speech (CIRI) & Freedom of Exp. (V-Dem)	.6712

Journalist Killings: Descriptive Analysis

Committee to Protect Journalists (CPJ) provides rich detail for each journalist killed since 1992. To better orient the reader with this data, I present a number of tables and figures describing pertinent details of the journalists killed in my 1992-2014 sample in different ways. First, I provide a list of the 20 deadliest countries for journalists. Then, I provide bar charts that detail the type of death of each journalist, the medium type for which the journalist worked, and then the journalist's gender. I further stratify these data by regime type. Finally, I provide the number of journalists killed by all 21 polity scores, as well as a breakdown of journalists killed by geographic region.

20 Deadliest Countries for Journalists, 1992-2014

Below I present two lists detailing the deadliest countries for journalists based on two different measurements (table 41). The first lists the total number of journalists killed in a country, which reflects the data I use in my analysis throughout chapter 4 and its appendix. The second looks solely at those murdered in the country. Killings that happen as a result of conflict can overshadow the persistent threat journalists face everyday absent the rarity of conflict. In chapter 5, I suggest future research may wish to focus on analysis of journalists murdered beyond conflict zones. Syria represents a striking case. While the country ranks second in journalists killed in the first list with 79, the country has experienced heightened conflict in recent years. It drops to a 16 ranking for the murdered journalist list with 16 murders — tied with Sri Lanka. Also, Bosnia ranks 14 on the total killed list but does not even make the murdered list. All 19 of Bosnia's murders occurred from 1992-1994 during a period of intense conflict. On the other hand, Bangladesh ranks 20 on the total killed list but rises 7 slots to 13 in the murdered list. Only two journalists were killed there during a year with conflict — 2005 — and both were murdered and not killed as a result of combat crossfire or covering a dangerous assignment. This preliminary analysis suggests that murders of journalists result from a different set of circumstances than other murders. Future work may wish to analyze these types of killings separately.

Table 41: Deadliest Countries by Two Criteria: Total Killed and Murdered

Rank	Country	Killed (total)	Rank	Country	Murdered
1.	Iraq	166	1.	Iraq	103
2.	Syria	79	2.	Philippines	75
3.	Philippines	77	3.	Algeria	58
4.	Algeria	60	4.	Colombia	42
5.	Pakistan	56	5.	Somalia	39
5.	Russia	56	6.	Russia	36
5.	Somalia	56	7.	Pakistan	32
8.	Colombia	46	8.	Brazil	30
9.	India	37	9.	Mexico	28
10.	Brazil	32	10.	India	24
11.	Mexico	31	11.	Turkey	18
12.	Afghanistan	27	12.	Rwanda	17
13.	Turkey	20	13.	Bangladesh	14
14.	Bosnia	19	13.	Tajikistan	14
14.	Sri Lanka	19	15.	Afghanistan	12
16.	Rwanda	17	16.	Sri Lanka	10
16.	Tajikistan	17	16.	Syria	10
18.	Israel/Palst.	16	18.	Indonesia	9
18.	Sierra Leone	16	18.	Sierra Leone	9
20.	Bangladesh	15	20.	Cambodia	8

Journalist Killings by Death Type, Medium, and Gender

Below I present three figures that analyze journalists killed by type of death, medium, and gender based on data the Committee to Protect Journalists (CPJ) provides. I further stratify these figures by regime type, as this remained a key feature of my main analysis. I note that while CPJ provides data from 1992 to the present, these figures only analyze journalists killed included in chapter 4's sample from 1992-2014. Also, Marshall and Jaggers (2017) do not provide polity scores for a number of country-years in the sample: Afghanistan (2001; 2006-2011), Iraq (2003-2009, Lebanon (1992-1993; 1999), and Somalia (2011). From these 17 observations, I lose 168 journalist killed from my sample.

Journalist Killings by Death Type and Regime Type

CPJ classifies journalist death types by four separate categories: murdered, crossfire, dangerous assignment, and unknown.[89] CPJ defines these categories in the following way:

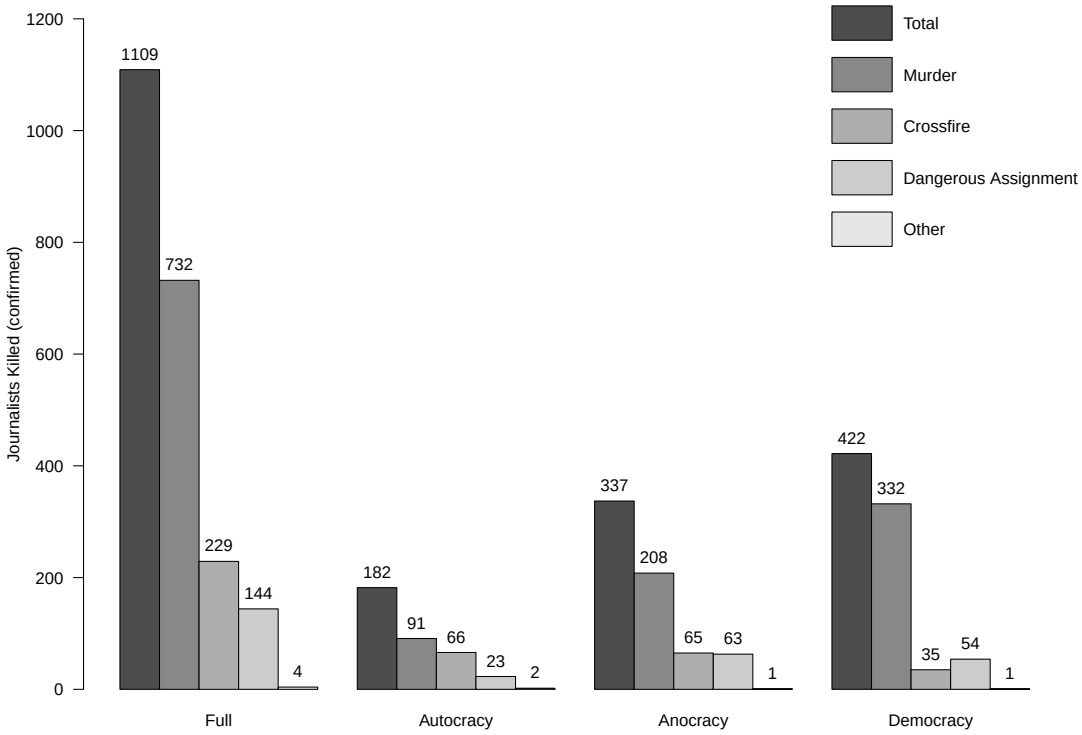
- **Murder**: the targeted killing of a journalist, whether premeditated or spontaneous, in direct reprisal for the journalist's work;
- **Crossfire**: a killing on a battlefield or in a military context;
- **Dangerous assignment**: deaths while covering a demonstration, riot, clashes between rival groups, mob situations; this includes assignments which are not expected to entail physical risk but turn violent unexpectedly; and
- **Unknown**: CPJ remains unable to determine death type.

Figure 36 shows bar charts of the number of journalist killings by death type in the full sample and then stratified by regime type below. The results indicate that murders happen more frequently than crossfire and dangerous assignment deaths both in the full sample and the samples stratified by region. I also note that only rarely is CPJ unable to determine the death type with only 4 unknowns throughout the sample. Crossfire deaths

[89] *Committee to Protect Journalists* (2018) Methodology: Journalists Killed since 1992. Available at https://cpj.org/data/methodology/.

seem to occur more than dangerous assignment deaths overall, though the reverse is true in democracies.

Figure 36: Journalist Killings by Death Type and Regime Type, 1992-2014

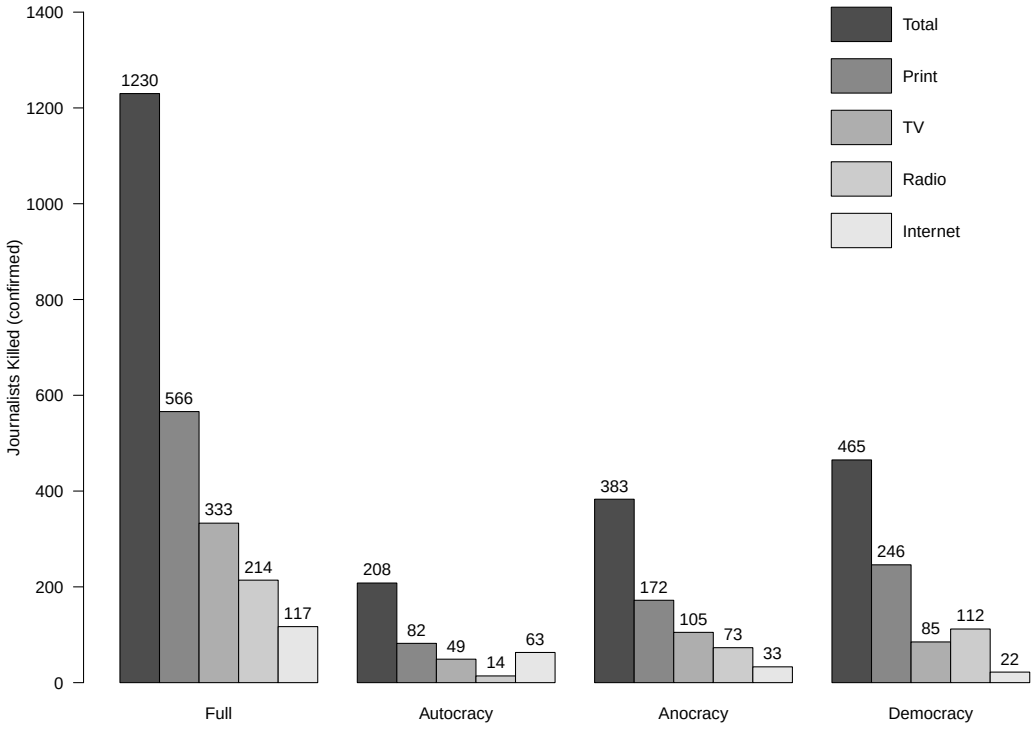


Note: Autocracy, Anocracy, and Democracy groupings exclude journalists that do not have polity scores.

Journalist Killings by Medium and Regime Type

CPJ reports the medium in which every journalists killed in their database worked. These media include print, television (TV), radio, and the Internet. The bar charts in figure 37 provide a total, then a breakdown, of each death by medium type. The bar charts are further stratified by regime type. I note that some journalists worked in more than one medium. The bar charts therefore sum to more than the actual number of journalists killed. The total indicates print journalists are killed the most, followed by TV, radio, than internet journalists. This pattern essentially holds for the regime stratification, though in autocracies more Internet journalists are killed than TV and radio ones combined, and in democracies more radio journalists have been killed than TV ones.

Figure 37: Journalist Killings by Medium and Regime Type, 1992-2014

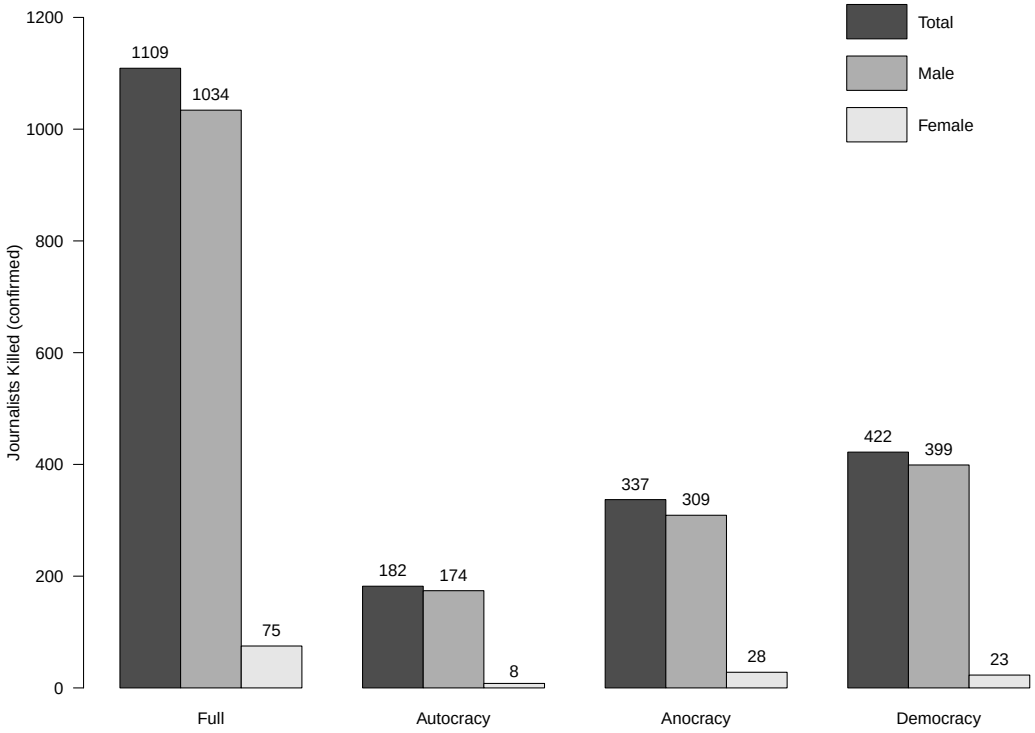


Note: Autocracy, Anocracy, and Democracy groupings exclude journalists that do not have polity scores.

Journalist Killings by Gender and Regime Type

CPJ reports the gender of each journalists killed in their database, including male, female, and nonbinary. The bar charts in figure 38 provide a total, then a breakdown of each death by gender further stratified by regime type. The data indicate that more males have been killed for their journalistic work than females in the full sample as well as when I stratify by regime type. As a percentage, more female journalists are killed in anocracies than autocracies and democracies. CPJ does not list any journalists killed as nonbinary.

Figure 38: Journalist Killings by Gender and Regime Type, 1992-2014



Note: Autocracy, Anocracy, and Democracy groupings exclude journalists that do not have polity scores.

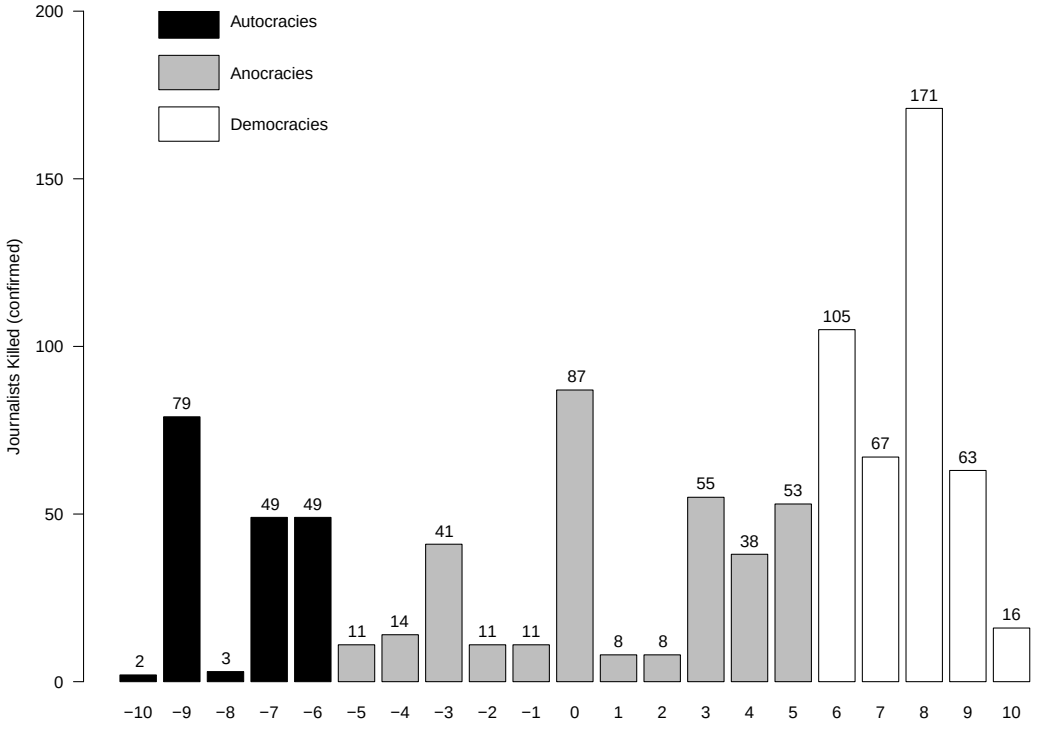
Journalist Killings by Polity Score and Region

I further explore the data by stratifying them by polity score and world region. First, I show a bar chart for all 21 *polity* scores (-10 to 10) and the corresponding number of journalists killed. Finally, I present a bar chart of total journalists killed compared to solely murders by world region.

Journalist Killings by Polity Score

I present the results for journalist killed by polity score below in figure 39. I further color code the bars to indicate in which regime type the score exists. The data indicate that polity score 8 has the most journalists killed in the sample with 171, while polity score 6 has the second most with 105. Polity score 0 has the third most with 87, and polity score -9 has the next most with 79. The least killings occurred in polity score -10 and -8, both autocracies.

Figure 39: Journalist Killings by Polity Score, 1992-2014



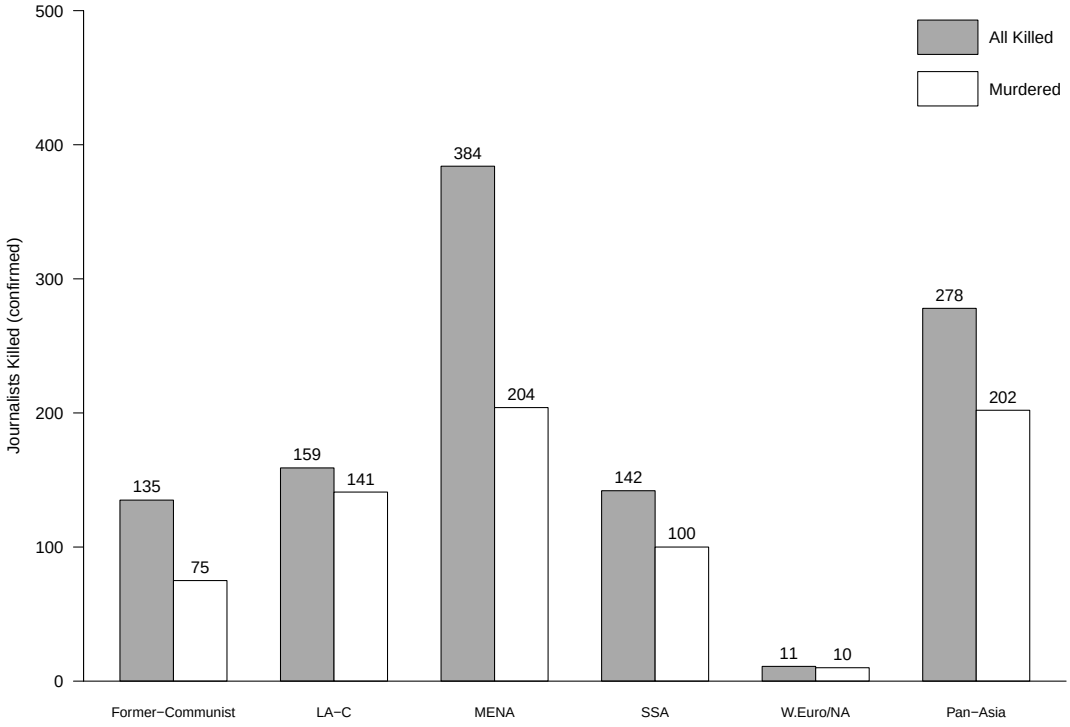
Note: Figure excludes journalists killed in countries that do not have polity scores.

Journalist Killings by World Region

I present the bar chart for journalist killed by world region below in figure 40. Hadenius and Teorell (2007) identify 10 unique world regions, but I combine certain groups when they are culturally and geographically similar to simplify the analysis. My group 1 mirrors Hadenius and Teorell's Group 1 exactly and includes all former Soviet Republics and former communists countries but does not include Mongolia (EECA). My group 2 represents Latin America and the Caribbean (LAC) and includes Hadenius and Teorell's group 2 (Latin America) and group 10 (Caribbean). My group 3 represents the Middle East and North Africa (MENA) and mirrors Hadenius and Teorell's Middle East and North Africa group 3 exactly. Next, I created group 4 including countries in Sub-Saharan Africa (SSA) that mirrors Hadenius and Teorell's Sub-Saharan Africa Group 4. I next created group 5, called Western Europe and North America (WENA) that mirrors Hadenius and Teorell's group 5 that includes all Western European countries as well as Canada, the United States, Australia, and New Zealand. Finally, I created group 6 that represents Pan-Asia (Asia) that includes Hadenius and Teorell's groups 6, 7, 8 and 9 representing the East Asia, South Asia, Southeast Asia, and Pacific Asia groups respectively.

I also compare the total number of killed to the number of those murdered. The data show that more journalists are killed in MENA than any other region with 384 total killed. Asia follows with 278, and LAC with 158. Finally SSA, EECA, and WENA follow with 142, 135, and 11 respectively. Regarding murders, MENA still ranks highest with 204 but Asia follows close behind with 202. LAC, SSA, and EECA follow with 141, 100, and 75 respectively. WENA has the least again with 10. I note that while MENA has higher overall journalists killed, its murder count is about the same of Pan-Asia, suggesting that journalists remain in about the same level of danger in the two regions outside of conflict zones and dangerous assignments.

Figure 40: Journalist Killings by Region: Total vs. Murdered, 1992-2014



Alternate Model Specifications

In this section I consider alternative specifications for the expanded models from table 7. These different estimation approaches mirror Asal et al.'s (2018) main analysis, which employs logit, rare events logit, ordinal, and zero inflated negative binomial regression models. To this end, I create a binary variable, coding a country 1 if it experienced at least one journalist killing in a year, and a 0 otherwise. Also, I create two ordinal variables. For the first, I assign a 1 to every country with a single journalist killed in a year, a 2 to any country with 2 or more journalists killed in a year, and a 0 otherwise. For the second, I assign a 1 to every country with 1 to 9 journalists killed in a year, a 2 to any country with 10 or more journalists killed in a year, and a 0 otherwise. With these variables, I estimate logit, rare events logit, and two different ordinal logit models. With the raw count data, I also estimate a zero-inflated negative binomial regression (ZINBR) model. Mirroring the authors, I use *log population*, but also include *journalists killed unconfirmed* to predict "always zero" variables for the ZINBR model

(Long and Freese 2006).[90] In addition, I use year effects and cluster the standard errors by country for each model. Finally, I utilize a lagged dependent variable (LDV) model to account for serial autocorrelation.

Overall, the alternate specifications show similar results and do not change my inferences. The regression results appear in the tables below. I note two exceptions. First, *regime-type durability* is negative and statistically significant but only at the 90% confidence level in the autocracy sample in the ZINBR model in table 46. Second, for the lagged dependent variable model analyzing autocracies in table 47, the maximum likelihood estimator is not concave and fails to produce a maximum likelihood estimate. In general, readers should interpret the LDV results with caution. Adding a LDV to a count model only controls for time if the series increases at an exponential rate (Brandt et al. 2000, 824-25). The journalist killings data does not exhibit this pattern and is instead dynamic. In general, controlling for time in count models remains difficult (Fogarty and Monogan 2014, 75). I therefore rely more on the survival analysis to this end.

[90]CPJ provides data for journalists killed but the motive could not be confirmed.

Logit Models

Table 42: Logit Analysis: Journalist Killings, 1992-2014

	(1) Global	(2) Autocracy	(3) Anocracy	(4) Democracy
Regime-type Duration (ln)	-.311***	-.423*	-.589***	.070
	(.083)	(.207)	(.139)	(.084)
Polity Level	.009	.053	.032	-.318*
	(.026)	(.205)	(.038)	(.130)
Public Sect. Cor., V-Dem	.562	-1.194	.349	.746
	(.554)	(1.168)	(.729)	(.683)
Physical Integrity, V-Dem	-4.803***	-3.623*	-4.281***	-6.333***
	(.743)	(1.446)	(1.071)	(.847)
Freedom of Exp., V-Dem	3.499***	2.075	3.776***	4.492***
	(.717)	(1.734)	(.883)	(1.218)
Armed Conflict	1.324***	2.056***	1.243***	.632***
	(.130)	(.322)	(.154)	(.141)
Information Flows	.029***	.038**	.031***	.025***
	(.006)	(.013)	(.008)	(.006)
Population (ln)	.394***	.232	.256*	.539***
	(.072)	(.134)	(.104)	(.092)
/lnalpha	.770***	1.170*	.498	-.260
	(.216)	(.535)	(.272)	(.272)
N	3586	597	1067	1922
Countries	160	52	86	106
AIC	2860.384	419.092	1039.686	1346.538

Standard errors (SE) in parentheses; Intercepts not reported; Year effects;
SE clustered by country; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Rare Events Logit Models

Table 43: Rare Events Logit Analysis: Journalist Killings, 1992-2014

	(1) Global	(2) Autocracy	(3) Anocracy	(4) Democracy
Regime-type Duration (ln)	-.319***	-.438*	-.469***	.094
	(.084)	(.190)	(.136)	(.124)
Polity Level	.046	.068	.066	-.332*
	(.030)	(.284)	(.042)	(.154)
Public Sect. Cor., V-Dem	1.392*	.192	.407	1.749*
	(.564)	(1.346)	(.828)	(.838)
Physical Integrity, V-Dem	-4.585***	-1.578	-3.857**	-6.748***
	(.948)	(1.518)	(1.201)	(1.349)
Freedom of Exp., V-Dem	3.746***	1.467	3.728**	5.131***
	(.903)	(2.320)	(1.146)	(1.491)
Armed Conflict	1.265***	1.381***	1.140***	.957***
	(.135)	(.344)	(.176)	(.204)
Information Flows	.033***	.029*	.033***	.038***
	(.007)	(.012)	(.009)	(.010)
Population (ln)	.480***	.198	.287*	.616***
	(.076)	(.124)	(.126)	(.115)
N	3586	597	1067	1922
Countries	160	52	86	106

Rare events logit (Tomz et al. 1999); Standard errors (SE) in parentheses;
Intercepts not reported; Year effects; SE clustered by country;
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Ordinal Logit Models: Specification 1

Table 44: Ordinal Logit Analysis #1: Journalist Killings, 1992-2014

	(1) Global	(2) Autocracy	(3) Anocracy	(4) Democracy
Regime-type Duration (ln)	-.346***	-.479*	-.563***	.060
	(.088)	(.216)	(.143)	(.123)
Polity Level	.046	.086	.070	-.359*
	(.030)	(.303)	(.046)	(.159)
Public Sect. Cor., V-Dem	1.433*	.182	.565	1.582
	(.617)	(1.496)	(.904)	(.950)
Physical Integrity, V-Dem	-4.925***	-1.898	-4.258***	-7.449***
	(.962)	(1.747)	(1.220)	(1.403)
Freedom of Exp., V-Dem	4.039***	1.866	4.257***	5.777***
	(.889)	(2.374)	(1.180)	(1.602)
Armed Conflict	1.320***	1.752***	1.321***	.939***
	(.147)	(.416)	(.200)	(.232)
Information Flows	.036***	.036**	.039***	.039***
	(.007)	(.014)	(.010)	(.010)
Population (ln)	.481***	.205	.281*	.648***
	(.078)	(.144)	(.143)	(.122)
/cut1	12.569***	6.751	8.603***	13.062***
	(1.581)	(3.951)	(2.497)	(2.697)
/cut2	13.846***	8.151*	9.909***	14.442***
	(1.594)	(3.975)	(2.523)	(2.670)
N	3586	597	1067	1922
Countries	160	52	86	106
AIC	2198.960	339.481	821.895	1047.007

Ordinal DV: 0 = 0 journ. killed, 1 = 1 journ. killed, 2 = $\geq$2 journ. killed;
Standard errors (SE) in parentheses; Intercepts not reported; SE clustered by country;
Year effects; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Ordinal Logit Models: Specification 2

Table 45: Ordinal Logit Analysis # 2: Journalist Killings, 1992-2014

	(1) Global	(2) Autocracy	(3) Anocracy	(4) Democracy
Regime-type Duration (ln)	-.315***	-.456*	-.524***	.102
	(.084)	(.216)	(.139)	(.124)
Polity Level	.043	.015	.069	-.329*
	(.031)	(.309)	(.044)	(.156)
Public Sect. Cor., V-Dem	1.369*	-.246	.465	1.899*
	(.571)	(1.588)	(.846)	(.853)
Physical Integrity, V-Dem	-4.649***	-2.449	-4.130***	-7.121***
	(.924)	(1.883)	(1.231)	(1.363)
Freedom of Exp., V-Dem	3.765***	2.359	3.983***	5.520***
	(.866)	(2.568)	(1.161)	(1.518)
Armed Conflict	1.296***	1.787***	1.243***	1.033***
	(.134)	(.454)	(.177)	(.208)
Information Flows	.034***	.037*	.034***	.040***
	(.006)	(.015)	(.009)	(.010)
Population (ln)	.469***	.201	.277*	.634***
	(.076)	(.144)	(.132)	(.116)
/cut1	12.219***	6.755	8.086***	13.611***
	(1.509)	(3.938)	(2.197)	(2.603)
/cut2	16.798***	9.429*	12.557***	20.380***
	(1.761)	(3.979)	(2.484)	(2.719)
N	3586	597	1067	1922
Countries	160	52	86	106
AIC	1820.844	313.377	691.246	810.487

Ordinal DV: 0 = 0 journ. killed, 1 = 1-9 journ. killed, 2 = $\geq$10 journ. killed;
Standard errors (SE) in parentheses; Intercepts not reported; SE clustered by country;
Year effects; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Zero-inflated Negative Binomial Models

Table 46: Zero-inflated Negative Binomial Analysis: Journalist Killings, 1992-2014

	(1) Global	(2) Autocracy	(3) Anocracy	(4) Democracy
Regime-type Duration (ln)	-.248**	-.264+	-.564***	.044
	(.083)	(.155)	(.141)	(.077)
Polity Level	-.007	-.067	.007	-.280*
	(.024)	(.109)	(.041)	(.123)
Public Sect. Cor., V-Dem	.338	-2.233*	.342	.513
	(.520)	(.976)	(.716)	(.573)
Physical Integrity, V-Dem	-4.238***	-5.013***	-3.789***	-5.613***
	(.649)	(1.290)	(1.055)	(.836)
Freedom of Exp., V-Dem	3.114***	3.214*	3.368***	3.917***
	(.611)	(1.312)	(.844)	(1.034)
Armed Conflict	1.121***	2.057***	1.088***	.540***
	(.135)	(.349)	(.181)	(.133)
Information Flows	.023***	.037*	.026***	.017**
	(.005)	(.017)	(.007)	(.005)
Population (ln)	.201**	-.022	.141	.390***
	(.072)	(.161)	(.135)	(.094)
inflate				
Population (ln)	-.405***	-.286	-.299	-.352*
	(.122)	(.227)	(.302)	(.149)
CPJ Unconfirmed	-7.199***	-.795**	-6.966***	-7.278***
	(.395)	(.259)	(.505)	(.585)
/lnalpha	-.015	-16.573***	-.317	-.867**
	(.216)	(.624)	(.366)	(.293)
N	3586	597	1067	1922
Countries	160	52	86	106
AIC	2810.225	394.562	1033.192	1334.986

Standard errors (SE) in parentheses; Intercepts not reported; SE clustered by country; Year effects; *CPJ Unconfirmed* counts journalist killings that did not results from their professional work; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Lagged Dependent Variable Models

Table 47: Negative Binomial Regressions w/ LDV: Journalist Killings, 1992-2014

	(1) Global	(2) Autocracy	(3) Anocracy	(4) Democracy
Lagged DV	.167***	-	.169***	.070
	(.046)	-	(.032)	(.062)
Regime-type Duration (ln)	-.252***	-	-.458***	.079
	(.065)	-	(.119)	(.088)
Polity Level	.022	-	.046	-.340*
	(.023)	-	(.037)	(.138)
Public Sect. Cor., V-Dem	.520	-	.740	.667
	(.521)	-	(.735)	(.697)
Physical Integrity, V-Dem	-4.472***	-	-4.185***	-5.900***
	(.726)	-	(.978)	(.934)
Freedom of Exp., V-Dem	3.110***	-	3.551***	4.110**
	(.632)	-	(.808)	(1.278)
Armed Conflict	1.032***	-	.959***	.525**
	(.135)	-	(.143)	(.175)
Information Flows	.023***	-	.026***	.022***
	(.005)	-	(.006)	(.006)
Population (ln)	.377***	-	.259**	.534***
	(.066)	-	(.093)	(.089)
/lnalpha	.549**	-	.100	-.283
	(.213)	-	(.291)	(.277)
N	3431	-	1023	1848
Countries	160	-	84	106
AIC	2694.867	-	969.388	1304.015

Standard errors (SE) in parentheses; Intercepts not reported; Year effects;
SE clustered by country; Maximum likelihood estimator failed in model 2;
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Robustness Checks

This section examines the robustness of my main results by considering potential confounders. I examine media system freedom's effect as an alternative to polity level, economic indicators, and crime data. I describe the variables in this section, then provide regression results that include those variables below. Below, I focus my discussion on the results of the theoretically relevant *regime-type durability*, as well as *polity* when relevant. I expect *regime-type durability* to be negative and statistically significant ($p < .05$) in the global, autocracy, and anocracy samples, while I expect it to remain insignificant in the democracy models. Though not theoretically relevant, I check the robustness of the finding that *polity* is negative and statistically significant ($p < .05$) in the democracy models. In sum, my inferences do not change as a result of these additional tests.

I first investigate an alternative to *polity*. Asal et al. (2018) initially proxies society's openness with *polity*. I replace it with an original variable that measures a country's media system freedom using two criteria: (1) media's ability to publish without undue influence, and (2) citizens' ability to access diverse information sources. The variable is a continuous measure from 0 to 1 with higher scores indicating greater media freedom. I generate the variable using an item response theory (IRT) analysis of 12 media freedom variables, which produces a posterior distribution — a point estimate and a standard deviation for each country-year — to account for rater reliability (see chapter 2 above). In table 48 I run the NBR models replacing *polity* with *media system freedom*'s point estimation. Then in table 49, I run another set of models using Monte Carlo simulations with 750 draws from the *media system freedom*'s posterior distribution.[91] My inferences on *regime-type durability* do not change, while *media system freedom* remains insignificant in all 6 models, including the democracy model. Taken with previous results, this suggests that a country's democratic institutional development level has a significant influence on journalists killed in democracies and not necessarily how free media function in society.

[91] See chapter 2 for a detailed explanation of this data.

Next, I investigate the influence of economic indicators: Gross Domestic Product (GDP), Gross Domestic Product per capita (GDP p/c), and change in GDP p/c. I present these results in tables 50 and 51 respectively. I draw the data from World Bank (2017) and run separate sets of models including (1) GDP (table D.11) and (2) GDP p/c and its change (table D.12). The results mirror findings in the main analysis and do not change my inferences.

I also recognize that country's with more journalist killings may simply result from generally higher murder incidents there. To investigate this potential confounder, I include data on intentional homicides. Following Stein and Kellam (2014), I take data from both the United Nations Office of Drugs and Crime (UNODC) (United Nations 2017) and World Bank (World Bank 2017) and average them when more than one observation exists for a country-year. I use both the count of intentional homicides and the homicide rate, which represents the number of homicides per 100,000 citizens. The data availability only allows me to cover the years from 1995 to 2014. I present these results in tables 52 and 53. My initial results hold for the global, autocracy and democracy samples, however they do not hold for anocracies.

However, I note that the addition of these variables greatly reduces my number of observations across all the models. Nigeria for example only has sporadic data for three years: 2005, 2008, and 2010. When I include homicide count to the analysis for instance, it reduces my observations by about 44% in the global samples, about 73% in the autocracy sample, about 63% in the anocracy sample, and about 29% in the democracy sample. Even with two separate sources, the amount of missingness and pairwise deletion appears to impact the results. I therefore employ multiple imputation to address missing data issues with the homicide data, using Stata's (version 15) `mi impute` multivariate normal regression method. The method imputes values of intentional homicide counts and rates using data from the model's other right-hand side variables. The imputation creates 20 sets of simulated values to replace the missing values, then applies analysis from the original models in chapter 4's table 7, adjusting the obtained parameter estimates for missing-data uncertainty. Tables 54 and 55 show

these results and indicate similar results to my initial findings. Taken together, these results do not change my inferences.

Overall, the results from these additional checks do not change my original inferences. Please find the regression tables from the models I have described below.

Alternative to Polity: Media System Freedom (MSF)

MSF Point Estimates

Table 48: Journalists Killed, 1992-2014: MSF Point Estimates

	(1) Global	(2) Autocracy	(3) Anocracy	(4) Democracy
Regime-type Duration (ln)	-.304***	-.422*	-.587***	.001
	(.080)	(.203)	(.130)	(.085)
Media System Freedom	-1.148	-2.412	.882	-.822
	(1.077)	(3.839)	(1.477)	(1.645)
Public Sect. Cor., V-Dem	.403	-1.100	.474	.943
	(.565)	(1.141)	(.746)	(.691)
Physical Integrity, V-Dem	-4.766***	-3.734**	-4.202***	-6.659***
	(.725)	(1.319)	(1.062)	(.882)
Freedom of Exp., V-Dem	4.573***	4.276	3.399*	4.585**
	(1.040)	(3.723)	(1.377)	(1.673)
Armed Conflict	1.331***	2.071***	1.259***	.647***
	(.129)	(.327)	(.155)	(.156)
Information Flows	.030***	.036**	.032***	.021**
	(.006)	(.014)	(.008)	(.007)
Population (ln)	.392***	.230	.258*	.484***
	(.074)	(.133)	(.102)	(.115)
N	3586	597	1067	1922
Countries	160	52	86	106
AIC	2858.465	418.651	1040.326	1362.731

Negative Binomial Regressions; Standard errors (SE) in parentheses; Intercepts not reported; SE clustered by country; Year effects; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Monte Carlo Simulations: MSF Point Estimates and Standard Deviations

Table 49: Monte Carlo Simulations for Journalists Killed, 1992-2014: MSF Data

	(1) Global	(2) Autocracy	(3) Anocracy	(4) Democracy
	β/(SE)	β/(SE)	β/(SE)	β/(SE)
Regime-type Duration (ln)	-.305***	-.405*	-.591***	- .002
	(.081)	(.205)	(.129)	(.082)
Media System Freedom	-.977	-2.161	.741	-.922
	(.975)	(3.684)	(1.378)	(1.496)
Public Sect. Cor., V-Dem	.412	-1.041	.439	1.01
	(.564)	(1.123)	(.724)	(.711)
Physical Integrity, V-Dem	-4.769***	-3.621***	-4.204***	-6.674***
	(.671)	(1.291)	(1.069)	(.891)
Freedom of Exp., V-Dem	4.412***	4.097	3.511**	4.489**
	(.937)	(3.648)	(1.304)	(1.612)
Armed Conflict	1.331***	2.091	1.256***	.651***
	(.131)	(.323)	(.154)	(.162)
Information Flows	.029***	.036	.031***	.021**
	(.005)	(.013)	(.007)	(.006)
Population (ln)	.394***	.221	.258**	.484***
	(.073)	(.136)	(.101)	(.115)
N	3586	597	1067	1922
Countries	160	52	86	106

Negative Binomial Regressions; 750 iterations; β = mean of 750 coefficient estimates;
(SE)= mean of 750 standard errors; **Bold** indicates elastic measure;
Intercepts not reported; SE clustered by country; SE clustered by country;
Year effects; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Gross Domestic Product Indicator

Table 50: Journalists Killed w/ GDP, 1992-2014

	(1) Global	(2) Autocracy	(3) Anocracy	(4) Democracy
Regime-type Duration (ln)	-.388***	-.549**	-.636***	.052
	(.085)	(.184)	(.137)	(.073)
Polity Level	.019	.245	.031	-.302**
	(.024)	(.248)	(.037)	(.104)
Public Sect. Cor., V-Dem	.577	-1.582	-.131	1.601*
	(.568)	(1.090)	(.673)	(.640)
Physical Integrity, V-Dem	-4.453***	-2.144	-3.579***	-6.313***
	(.748)	(1.727)	(.948)	(.920)
Freedom of Exp., V-Dem	3.338***	.770	3.421***	4.300***
	(.766)	(1.578)	(.993)	(1.269)
Armed Conflict	1.234***	1.843***	1.089***	.700***
	(.125)	(.292)	(.156)	(.144)
Information Flows	.016	.043*	.028**	.005
	(.009)	(.017)	(.010)	(.010)
Population (ln)	.229	.207	.342	.114
	(.186)	(.252)	(.238)	(.195)
GDP (ln)	.184	.050	.028	.407*
	(.167)	(.311)	(.183)	(.161)
/lnalpha	.650**	.837	.357	-.310
	(.239)	(.756)	(.347)	(.268)
N	3502	563	1031	1908
Countries	160	50	84	106
AIC	2643.251	339.576	910.860	1329.935

Negative Binomial Regressions; Standard errors (SE) in parentheses; Intercepts not reported; SE clustered by country; Year effects; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Gross Domestic Product per capita Indicators

Table 51: Journalists Killed w/ GDP p/c Variables, 1992-2014

	(1) Global	(2) Autocracy	(3) Anocracy	(4) Democracy
Regime-type Duration (ln)	-.382***	-.461*	-.558***	.049
	(.088)	(.189)	(.126)	(.072)
Polity Level	.022	.017	.022	-.306**
	(.025)	(.261)	(.036)	(.105)
Public Sect. Cor., V-Dem	.717	-.861	-.561	1.596*
	(.517)	(1.008)	(.724)	(.656)
Physical Integrity, V-Dem	-4.359***	-2.490	-3.621***	-6.129***
	(.726)	(1.924)	(.900)	(.947)
Freedom of Exp., V-Dem	3.404***	2.453	3.691***	4.069**
	(.746)	(1.358)	(1.017)	(1.337)
Armed Conflict	1.131***	1.625***	.948***	.720***
	(.117)	(.273)	(.155)	(.144)
Information Flows	.011	.032*	.021*	.005
	(.009)	(.016)	(.009)	(.009)
Population (ln)	.474***	-	.456***	.532***
	(.070)	-	(.115)	(.091)
GDP p/c (ln)	.282	.189	.129	.402*
	(.160)	(.254)	(.184)	(.162)
Δ GDP p/c	-.057***	-.058***	-.034**	-.030
	(.014)	(.016)	(.012)	(.022)
N	3468	542	1026	1900
Countries	158	49	82	106
AIC	2571.473	330.179	878.221	1329.814

Negative Binomial Regressions; Standard errors (SE) in parentheses; Intercepts not reported; SE clustered by country; Year effects; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Intentional Homicide Counts/Rates

Models with Raw Data

Table 52: Journalists Killed w/ Homicide Counts, 1995-2014

	(1) Global	(2) Autocracy	(3) Anocracy	(4) Democracy
Regime-type Duration (ln)	-.321***	-.176	-.329*	-.115
	(.072)	(.245)	(.136)	(.103)
Polity Level	.045	.160	.073	-.393**
	(.023)	(.281)	(.048)	(.142)
Public Sect. Cor., V-Dem	.645	.254	-.865	.628
	(.639)	(1.516)	(.995)	(.681)
Physical Integrity, V-Dem	-5.613***	-3.845*	-4.036***	-5.469***
	(.777)	(1.771)	(1.164)	(1.069)
Freedom of Exp., V-Dem	3.910***	3.713	3.762***	4.192**
	(.665)	(1.911)	(1.033)	(1.289)
Armed Conflict	.923***	1.884***	.721***	.766***
	(.149)	(.387)	(.214)	(.198)
Information Flows	.018**	.047*	.018	.020*
	(.006)	(.018)	(.011)	(.009)
Population (ln)	.167*	-.321	.383*	.311***
	(.082)	(.245)	(.181)	(.093)
Homicides (count)	.000***	.000	.000	.000**
	(.000)	(.000)	(.000)	(.000)
/lnalpha	-.259	-71.188	-58.942	-.296
	(.241)	(.)	(.)	(.201)
N	1982	217	391	1374
Countries	157	29	61	100
AIC	1488.085	129.943	364.340	1013.771

Negative Binomial Regressions; Standard errors (SE) in parentheses; Intercepts not reported; SE clustered by country; Year effects; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 53: Journalists Killed w/ Homicide Rates, 1995-2014

	(1) Global	(2) Autocracy	(3) Anocracy	(4) Democracy
Regime-type Duration (ln)	-.305***	-.078	-.322*	-.112
	(.081)	(.250)	(.139)	(.116)
Polity Level	.044	.110	.072	-.436**
	(.024)	(.271)	(.049)	(.146)
Public Sect. Cor., V-Dem	.550	.272	-.740	.304
	(.646)	(1.449)	(1.002)	(.668)
Physical Integrity, V-Dem	-5.048***	-3.766*	-3.984***	-5.024***
	(.806)	(1.817)	(1.137)	(1.097)
Freedom of Exp., V-Dem	3.805***	4.096	3.927***	4.089**
	(.665)	(2.172)	(1.056)	(1.352)
Armed Conflict	.927***	1.772***	.759***	.752***
	(.148)	(.406)	(.203)	(.184)
Information Flows	.015**	.048*	.021	.017*
	(.006)	(.020)	(.011)	(.008)
Population (ln)	.411***	.037	.418**	.533***
	(.100)	(.202)	(.148)	(.103)
Homicides (rate)	.018**	.049	-.003	.013
	(.006)	(.053)	(.024)	(.007)
/lnalpha	-.245	-17.601***	-14.290	-.289
	(.280)	(.258)	(9.182)	(.215)
Countries	157	29	63	100
N	2032	226	407	1399
AIC	1527.282	125.821	369.259	1039.485

Negative Binomial Regressions; Standard errors (SE) in parentheses; Intercepts not reported; SE clustered by country; Year effects; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Models with Imputed Data

Table 54: Journalists Killed w/ Homicide Counts (Imputed Data), 1992-2014

	(1) Global	(2) Autocracy	(3) Anocracy	(4) Democracy
Regime-type Duration (ln)	-.329***	-.440*	-.590***	.022
	(.085)	(.210)	(.138)	(.099)
Polity Level	-.001	.044	.031	-.298*
	(.026)	(.211)	(.039)	(.138)
Public Sect. Cor., V-Dem	.642	-1.103	.350	.886
	(.545)	(1.176)	(.732)	(.702)
Physical Integrity, V-Dem	-4.695***	-3.490*	-4.278***	-6.307***
	(.715)	(1.432)	(1.068)	(.808)
Freedom of Exp., V-Dem	3.500***	2.092	3.795***	4.723***
	(.706)	(1.749)	(.887)	(1.174)
Armed Conflict	1.264***	2.015***	1.217***	.631***
	(.131)	(.338)	(.164)	(.136)
Information Flows	.031***	.038**	.031***	.026***
	(.006)	(.013)	(.008)	(.006)
Population (ln)	.283***	.183	.226*	.415***
	(.061)	(.178)	(.115)	(.072)
Homicides (count)	.000**	.000	.000	.000*
	(.000)	(.000)	(.000)	(.000)
/lnalpha	.717***	1.144*	.481	-.267
	(.214)	(.546)	(.271)	(.233)
N	3586	597	1067	1922
Countries	160	52	86	106

Negative Binomial Regressions; Standard errors (SE) in parentheses; Intercepts not reported; SE clustered by country; Year effects; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 55: Journalists Killed w/ Homicide Rates (Imputed Data), 1992-2014

	(1) Global	(2) Autocracy	(3) Anocracy	(4) Democracy
Regime-type Duration (ln)	-.335***	-.439*	-.587***	.007
	(.087)	(.214)	(.140)	(.114)
Polity Level	-.003	.043	.033	-.309*
	(.027)	(.211)	(.039)	(.143)
Public Sect. Cor., V-Dem	.475	-1.200	.358	.624
	(.556)	(1.189)	(.737)	(.684)
Physical Integrity, V-Dem	-4.549***	-3.594*	-4.282***	-5.987***
	(.747)	(1.500)	(1.068)	(.869)
Freedom of Exp., V-Dem	3.479***	2.141	3.762***	4.538***
	(.718)	(1.765)	(.881)	(1.216)
Armed Conflict	1.341***	2.073***	1.238***	.666***
	(.130)	(.319)	(.154)	(.134)
Information Flows	.030***	.040**	.031***	.024***
	(.006)	(.014)	(.008)	(.006)
Population (ln)	.408***	.240	.257*	.557***
	(.074)	(.140)	(.105)	(.098)
Homicides (rate)	.011*	.006	-.001	.009
	(.005)	(.029)	(.013)	(.007)
/lnalpha	.760***	1.149*	.484	-.269
	(.216)	(.533)	(.275)	(.259)
N	3586	597	1067	1922
Countries	160	52	86	106

Negative Binomial Regressions; Standard errors (SE) in parentheses; Intercepts not reported; SE clustered by country; Year effects; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Countries by Regime Type, 1992-2014

Below I list each country in the sample by regime type. Some countries appear in multiple lists if they existed as two or three different regime types from 1992 to 2014. For instance, by the criteria, Armenia was a democracy from 1992 to 1994, then briefly became an anocracy in 1995 before transitioning to an autocracy in 1996 and 1997. It then switching back to an autocracy where it remained throughout the rest of the sample. I provide the years the country remained part of that regime.

Autocracies, 1992-2014

Table 56: Countries Coded Autocracy and Years as Autocracy, 1992-2014

Polity -10 to -6		
Afghanistan (1996-2000)	G.-Bissau (1992-1993)	Nigeria (1993-1997)
Algeria (1992-1994)	Haiti (1992-1993)	Oman (1992-2014)
Armenia (1996-1997)	Indonesia (1992-1997)	Pakistan (1999-2001)
Azerbaijan (1995-2014)	Iran (1992-1996), (2004-2014)	Qatar (1992-2014)
Bahrain (1992-2009), (2011-2014)	Iraq (1992-2002)	Rwanda (1992-1999)
Bangladesh (2007-2008)	Kazakhstan (2002-2014)	Saudi Arabia (1992-2014)
Belarus (1996-2014)	Korea, North (1992-2014)	Serbia (1993-1999)
Bhutan (1992-2004)	Kuwait (1992-2014)	Sierra Leone (1992-1995)
Cambodia (1997)	Laos (1992-2014)	Sudan (1992-2004)
CAR (1992)	Lesotho (1992)	Swaziland (1992-2014)
China (1992-2014)	Libya (1992-2010)	Syria (1992-2014)
Congo, Rep (1997-2000)	Malawi (1992-1993)	Tajikistan (1992-1996)
Cote D'Ivoire (1992-1998)	Mauritania (1992-2004)	Turkmenistan (1992-2014)
Cuba (1992-2014)	Morocco (1992-2010)	UAE (1992-2014)
Djibouti (1992-1998)	Mozambique (1992-1993)	Uganda (1992)
Egypt (1992-2004)	Myanmar (1992-2010)	Uzbekistan (1992-2014)
Eq. Guinea (1992), (1996-2014)	Nepal (2002-2005)	Vietnam (1992-2014)
Eritrea (1993-2014)	Niger (1996-1998)	Zimbabwe (1992-1998)
Gambia (1994-1996)		

Anocracies, 1992-2014

Table 57: Countries Coded Anocracy and Years as Anocracy, 1992-2014

Polity -5 to 5		
Afghanistan (1992-1995), (2014)	Gabon (1992-2014)	Nigeria (1992), (1998-2014)
Albania (1992-2001)	Gambia (1997-2014)	Pakistan (2002-2009)
Algeria (1995-2014)	Georgia (1992-2003)	Papua New Guinea (1992-2014)
Angola (1992-2014)	Ghana (1992-2000)	Peru (1992-2000)
Armenia (1995), (1998-2014)	Guatemala (1992-1995)	Romania (1992-1995)
Azerbaijan (1992-1994)	Guinea (1992-2014)	Russia (1992-1999), (2007-2014)
Bahrain (2010)	G.-Bissau (1994-2004), (2012-2013)	Rwanda (2000-2014)
Bangladesh (2009-2014)	Haiti (1999-2014)	Senegal (1992-1999)
Belarus (1995)	Indonesia (1998)	Serbia (1992)
Bhutan (2005-2014)	Iran (1997-2003)	Sierra Leone (1996-2006)
Bosnia (1992-1994)	Iraq (2010-2013)	Singapore (1992-2014)
Burkina Faso (1992-2014)	Jordan (1992-2014)	Solomon Islands (2000-2002)
Burundi (1992-2004)	Kazakhstan (1992-2001)	Somalia (1992-2010), (2012-2014)
Cambodia (1992-1996), (1998-2014)	Kenya (1992-2001)	Sri Lanka (1992-2000), (2003-2005),
Cameroon (1992-2014)	Kyrgyzstan (1992-2010)	(2009-2014)
CAR (1993-2014)	Lesotho (1998-2000)	Sudan (2005-2014)
Chad (1992-2014)	Liberia (1992-2005)	Suriname (1992-2014)
Comoros (1992-2003)	Libya (201-2014)	Tajikistan (1997-2014)
Congo, DR (1992-2014)	Madagascar (2009-2013)	Tanzania (1992-2014)
Congo, Rep (1992-1996), (2001-2014)	Malawi (2001-2003)	Thailand (2006-2010), (2014)
Cote D'Ivoire (1999-2014)	Malaysia (1992-2007), (2014)	Togo (1992-2014)
Croatia (1992-1999)	Mali (2012-2014)	Tunisia (1992-2012)
Djibouti (1999-2014)	Mauritania (2005-2014)	Turkey (2014)
Dominican Rep. (1994-1995)	Mexico (1992-1996)	Uganda (1993-2014)
Ecuador (2007-2014)	Moldova (1992)	Ukraine (1993), (2014)
Egypt (2005-2014)	Morocco (2011-2014)	Venezuela (2006-2014)
Eq. Guinea (1993-1995)	Mozambique (1994-2014)	Yemen (1992-2014)
Ethiopia (1992-2014)	Myanmar (2011-2014)	Zambia (1996-2007)
Fiji (1992-1998), (2000-2003),	Nepal (1992-1998)	Zimbabwe (1999-2014)
(2006-2014)	Niger (1999-2003),(2009-2010)	

Democracies, 1992-2014

Table 58: Countries Coded Democracy and Years as Democracy, 1992-2014

Polity 6 to 10		
Albania (2002-2014)	Guatemala (1996-2014)	Nicaragua (1992-2014)
Argentina (1992-2014)	G.-Bissau (2005-2014)	Niger (1992-1995), (2004-2008),
Armenia (1992-1994)	Guyana (1992-2011), (2014)	(2011-2014)
Australia (1992-2014)	Haiti (1994-1998)	Norway (1992-2014)
Austria (1992-2014)	Honduras (1992-2014)	Pakistan (1992-1998), (2010-2014)
Bangladesh (1992-2006)	Hungary (1992-2014)	Panama (1992-2014)
Belarus (1992-1994)	India (1992-2014)	Paraguay (1992-2014)
Belgium (1992-2014)	Indonesia (1999-2014)	Peru (2001-2014)
Benin (1992-2014)	Iraq (2014)	Philippines (1992-2014)
Bolivia (1992-2014)	Ireland (1992-2014)	Poland (1992-2014)
Botswana (1992-2014)	Israel (1992-2014)	Portugal (1992-2014)
Brazil (1992-2014)	Italy (1992-2014)	Romania (1996-2014)
Bulgaria (1992-2014)	Jamaica (1992-2014)	Russia (2000-2006)
Burundi (2005-2014)	Japan (1992-2014)	Senegal (2000-2014)
Canada (1992-2014)	Kenya (2002-2014)	Serbia (2000-2014)
Cape Verde (1992-2014)	Korea, South (1992-2014)	Sierra Leone (2007-2014)
Chile (1992-2014)	Kyrgyzstan (2011-2014)	Slovakia (1993-2014)
Colombia (1992-2014)	Latvia (1992-2014)	Slovenia (1992-2014)
Comoros (2004-2014)	Lebanon (2005-2014)	Solomon Islands (1992-1999),
Costa Rica (1992-2014)	Lesotho (1993-1997), (2001-2014)	(2004-2014)
Croatia (2000-2014)	Liberia (2006-2014)	South Africa (1992-2014)
Cyprus (1992-2014)	Lithuania (1992-2014)	Spain (1992-2014)
Czech Republic (1992-2014)	Luxembourg (1992-2014)	Sri Lanka (2001-2002), (2006-2008)
Denmark (1992-2014)	Macedonia (1992-2014)	Sweden (1992-2014)
Dominican Rep. (1992-1993), (1996-2014)	Madagascar (1992-2008), (2014)	Switzerland (1992-2014)
East Timor (2002-2014)	Malawi (1994-2000), (2004-2014)	Thailand (1992-2005), (2011)
Ecuador (1992-2006)	Malaysia (2008-2013)	Trinidad and Tobago (1992-2014)
El Salvador (1992-2014)	Mali (1992-2011)	Tunisia (2013-2014)
Estonia (1992-2014)	Mauritius (1992-2014)	Turkey (1992-2013)
Fiji (1999), (2004-2005)	Mexico (1997-2014)	Ukraine (1992), (1994-2013)
Finland (1992-2014)	Moldova (1993-2014)	United Kingdom (1992-2014)
France (1992-2014)	Mongolia (1992-2014)	United States (1992-2014)
Gambia (1992-1993)	Montenegro (2006-2014)	Uruguay (1992-2014)
Georgia (2004-2014)	Namibia (1992-2014)	Venezuela (1992-2005)
Germany (1992-2014)	Nepal (1999-2001), (2006-2014)	Zambia (1992-1995), (2008-2014)
Ghana (2001-2014)	Netherlands (1992-2014)	
Greece (1992-2014	New Zealand (1992-2014)	

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