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the Economies of Regulated Cities in China

*Andrés Prieto*

Measuring the Effect of Climate and Demographic Variables  
on Climate Change Attitudes in the United States

*Isabelle Aida Heilman*

Sovereign Signals: Success in Indonesian Illegal, Unreported  
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*Jeong Soo Kim*

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# Letter from the Editor

For more than two decades now, *The Journal of International Policy Solutions* has prided itself in bringing together the best policy-focused research on Latin America and the Asia-Pacific region. This year's edition is no exception, serving as a powerful platform to present important global issues through thoughtful and original research from international relations and public policy graduate students.

The 22<sup>nd</sup> edition represents the culmination of rigorous qualitative and quantitative training from our contributors. It brings together a diverse set of research that touch on pressing and emerging international challenges. We include timely research on climate change – one paper explores how a carbon pricing program affects emissions in several Chinese cities, while another tests the links between extreme climate events and attitudes towards climate change in the United States. Another piece focuses on the modern tech supply chain and its reliance on forced labor and modern slavery and proposes policies and regulations that can be adopted to mitigate it. Other research tests the effects of coalitions and public spending in democracies, analyzes North Korea's illicit activities at sea, and delves into the study of national sovereignty through Indonesian fishing policies in the South China Sea.

To the reader, I hope that you enjoy the fruit of our labor and see value in our scholarship. In just a few months, this new decade has already solidified itself as important and historic. We have witnessed an impeachment attempt of a U.S. president, escalated tensions between the U.S. and Iran, a spread of mass-scale fires and other natural disasters across the globe, and the outbreak of a deadly global disease. These events have sown confusion and worry, as global systems and institutions struggle to address them. Importantly for the Pacific region, 2020 is also highlighting continued tensions between China and the West.

As students of policy and international relations, we have cautiously watched recent events unfold – but have simultaneously discussed and formulated the links between evidence, theory and practice. I hope this collection of academic research highlights the resilience of student research, even during uncertain times.

This edition would not have been possible without the countless hours of work put in by the editorial board. I would like to personally thank the editors, contributors, and board members who made this journal possible. And lastly, I am grateful to our faculty advisor Elizabeth Lyons for her continued guidance and support.

Sincerely,

*Gustavo López*  
**Editor in Chief**



# An Empirical Exploration of the Effect of Carbon Pricing on the Economies of Regulated Cities in China

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In mid-2013, China began its first of 8 regional emission trading systems to reduce greenhouse gas emissions by 20% relative to a business-as-usual emissions projection. Since then, the policy has been marred by concerns by Chinese firms that putting a price on carbon will negatively impact the economy of the regulated provinces and cities. This study tests these claims by utilizing a fixed effects estimator to compare cities where the emissions trading has been implemented with similar cities with no such regulation, matching cities by several economic and demographic metrics and using inverse probability weighting of propensity scores to provide a robust counterfactual. I find that the 41 cities regulated show a slight increase in GDP relative to non-regulated cities, not only questioning concerns about the economic effects of carbon pricing but also suggesting that regulation may be associated with increases in energy efficiency efforts to reduce carbon-heavy inputs.

## Background

Emission trading, along with carbon taxes, has long been promoted as some of the most important tools to mitigate greenhouse gas emissions by economists and environmentalists alike. To date, 51 jurisdictions have employed either of these instruments, covering about 20% of world greenhouse gas emissions (World Bank, 2018). Emissions trading puts a price on carbon by requiring firms to surrender emission allowances for every ton of CO<sub>2</sub> equivalent emitted on their facilities and decreasing the quantity of allowances available in time, thus inducing higher and higher prices for the allowances.

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In 2012 China announced that it would implement cap-and-trade pilots in 7 jurisdictions. The first was in the city of Shenzhen in mid-2013. The pilot programs put a cap on all firms that report over 25,000 tons of direct and indirect CO<sub>2</sub>e emitted per year (although this varies slightly by pilot) from the most GHG intensive sectors of the economy. In total, the pilots regulate nearly 3,000 firms nationwide (International Emissions Trading Association, 2016). The policy encompasses substantial portions of the cities and provinces overall greenhouse gas emissions and GDP. Even in Shenzhen, which is rapidly achieving sectoral transformation to less carbon-intensive industries, the emissions trading system puts a price on about 38% of the city's emissions and 26% of its GDP (International Emissions Trading Association, 2013).

Naturally, given the impressive scope of the policy, firms had expressed concerns since before the pilots were launched. A survey of firms that participate in emissions trading in China indicated that a solid majority do not consider that it is a cost-effective form of mitigation. The same study indicates that slightly over 80% of firms are dissatisfied with the policy. Most firms also report unwillingness to invest in low carbon facilities (Yang et al, 2016).

Firms in China, and in other jurisdictions where cap and trade has been implemented, argue that carbon pricing will reduce economic growth in two ways: 1) the added cost of purchasing allowances will make regulated firms less competitive relative to non-regulated firms; 2) investment will be redirected to similar non-regulated provinces, causing economic damage to regulated firms while not generating net abatements. This phenomenon where polluting investments are simply reallocated to non-regulated economies is a concern for environmentalists as well and is generally known as “carbon leakage” (Council on Foreign Relations, 2019).

On the other side, green economy proponents have argued that not only are lowering emissions and generating economic growth not incompatible goals but that they may, in fact, complement each other. Some studies estimate that mitigating emissions can generate as much as “26 trillion dollars gain through 2030” and as many as 65 million new low carbon jobs in the global economy (Global Commission on Economy and Climate, 2018). The economic benefits of a green economy are derived mainly from triggering productive activity of firms, such as investments in new technology and energy efficiency programs to reduce overall inputs.

The debate of whether the effect of carbon pricing on the economy is positive or negative is complex nevertheless crucial. Most immediately, China plans to extend the emissions trading system to a nation-wide scope. The economic effects will likely have significant implications on the political palatability of expansion and the desired ambition of emission reductions. Moreover, understanding the direction of this effect gives insight as to how the policy is

working, giving clues as to whether I am observing carbon leakage or, instead, low-carbon investments and energy efficiency as a result of regulation.

Unfortunately, comparisons to estimate the economic effects of this kind of policy are often tricky in China due to the selection criteria of the regulated entities. In many cases, China chooses only its most modern, economically dynamic jurisdictions to try cutting edge policies and the case of emissions trading is no exception. The 9 heaviest greenhouse gas emitting provinces are not regulated by the policy (**Map 2 in Appendix**). Instead, the selection is heavily biased towards high-income economies undergoing sectoral transition.

To this effect, this paper uses panel data statistical techniques (which are detailed in the methodology section) to control for the endogeneity in the selection criteria, using several specifications to provide a robust counterfactual and to empirically test and determine whether the concerns of firms regarding carbon pricing are justified.

## **Data**

This study uses Chinese cities as the unit of analysis, with a total of 287 cities in the dataset, of which 41 enter treatment at some point (**Map 1 in Appendix**). A clear majority of treatment cities joined the policy in 2014, with Shenzhen joining earlier in mid-2013, and the cities of the Fujian province joining later at the beginning of 2017. The timeframe of the data ranges from 2010 to 2018 for both the dependent and independent variables and most covariates.

Optimally, I would use data on the profitability of firms of both treatment and control provinces in China to estimate the effect of the policy, but this data is unavailable to the public. However, given that China's economy is highly reliant on emitting carbon and that emissions trading mainly operates as a tax on a wide range of industries, the negative impact of the policy should be reflected in the regulated cities' overall GDP. This data and other economic indicators (public expenditure, FDI, fixed asset investment, electricity consumption, per capita GDP and share of the GDP in the tertiary economy) were sourced from the National Bureau of Statistics and the CEIC Data Company. The purpose of the added economic indicators is outlined in the Methodology section.

As for the treatment data, information is freely available on the International Carbon Action Platform and the International Emissions Trading Association about the date of entry of the different cities and provinces into the policy.

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1. All pilots cover core industries: power and heat supply, iron and steel, nonferrous metals, petrochemicals, chemicals, textile, cement, glass and other building materials, pulp and paper, ceramics. Some pilots cover additional industries such as aviation.

However, because each pilot has different design features that result in diverging levels of scrutiny, the intensity of the policy is also of interest. To capture it, an annual carbon price of the different systems was calculating using spot prices of the allowances from the policy's digital platform for emissions trading (which, surprisingly, is available to the public). Assuming that spot prices do not diverge significantly from over-the-counter prices, I can conceive the scrutiny of the policy as directly proportional to the average spot price of the allowances.

Finally, some provincial-level data were explored during the early stages of this analysis to understand the selection criteria of the entities better. Namely, data on provincial coal consumption from CEIC Data Company and provincial emissions data from Scientific Data were analyzed, indicating that the policy does not seem to target the most greenhouse gas-intensive provinces. Interestingly, a characteristic that is shared by all regulated provinces and cities is that they are all among the smallest coal producers in China (Map 2 in Appendix), possibly by design to shield coal-reliant economies from earlier policy stages.

## Methodology

The main deciding factor for the statistical methods employed was that of minimizing the potential bias of higher economic growth in the treatment group and to subsequently estimate the independent effect on economic performance (measured by GDP) of being regulated and of the intensity of regulation (the carbon price). Five specifications were used, all of which are variations of fixed effects estimators. All models used log transformations of GDP, which is not normally distributed. The regressions models also employ clustered standard errors by cities. This last modification was motivated by the high intra-cluster correlation of GDP outcomes within same-city observations.

A) Two-way fixed effects:

$$\log(GDP_{it}) = \alpha_i + \lambda_t + \beta_1 \text{emissions trading}_{it} + \beta_2 \text{carbon price}_{it} + \varepsilon_{it}$$

Two-way fixed effects is chosen as the first model to the innate, static characteristics of cities, and the time trends of the dependent and independent variables.

B) Unit-specific trends:

$$\log(GDP_{it}) = \alpha_{0i} + \alpha_{1i} t + \lambda_t + \beta_1 \text{emissions trading}_{it} + \beta_2 \text{carbon price}_{it} + \varepsilon_{it}$$

The second model gives us an added degree of certainty. The difference lies in that model A assumes parallel trends in the control and treatment group. As is detailed in the Results section, there is some evidence of a light violation of this assumption.

C-E) Unit-specific trends with weights by propensity scores:

*Models C, D and E* build on the unit-specific trends model but add yet another layer of certainty. The logic of all three is similar: modifying the control group to give a higher weight to observations that were more likely to enter into treatment in the first place. This layer is accomplished by estimating the propensity scores of control cities the year before the policy is launched. Propensity scores are then used to weigh similar cities more heavily, improving the robustness of the counterfactual.

What differs in the models is the calculation of the propensity scores themselves. *Model C* uses only GDP growth before the policy. Intuitively, since the end output is GDP itself, a difference will be more relevant if I compare cities that were growing at rates similar to the typically higher rates of the treatment group.

For *Model D*, I add more variables to the propensity score calculation. Although the dependent variable must be comparable among both groups at the onset, the characteristics of modern economies selected for treatment are not limited to GDP growth. For this model, I added covariates that scored as significant in the propensity score calculation, namely: electricity consumption, public expenditure, FDI, fixed asset investment, GDP per capita, and share of GDP in the tertiary sector.

Finally, *Model E* builds on model *D* but excludes China's leading coal-producing provinces from the control group. This model addresses the possibility that regulated provinces show an increase in GDP relative to the control group, not because of economic benefits of emissions trading, but rather because coal-producing cities in the control group suffer economic slowdown due to less demand for coal. For this model, Henan, Inner Mongolia, Shandong, Shanxi, and Shaanxi, responsible for nearly half of China's coal production (**Map 2 in Appendix**), were excluded.

Finally, as for the weighting for propensity scores, the inverse proportional weighting rather than the actual propensity score was used. The motivation lies in that weighting by propensity score or the inverse thereof both remove the bias associated with differences in the covariates. However, the latter does so without compromising the efficiency of the estimate (Hirano et al, 2000).

## Results

The following figures and tables are the output of the parallel trends analysis I performed prior to choosing the estimations methods. **Figure 1** shows GDP growth before the treatment leading up until the year of the first pilot launch. I observe that both groups generally follow a similar pattern, indicating that nation-wide factors rather than city-specific ones are playing a dominant role in setting the overall economic trend.

**Table 1** contains results of placebo tests for the years 2011, 2012, and 2013, testing if the difference in GDP growth among the groups was statistically significant prior to treatment. Although there is some evidence for parallel trends, I observe some deviance in 2013, although only one city was treated (Shenzhen), and only for half of the year. However, given that the announcement of pilots was given earlier (except for the Fujian province), it may be the case that other treatment cities have modified behavior in anticipation of the policy. **Figure 2** shows the evolution of GDP growth for the two groups in the full timeframe.

All five specifications of the fixed effects models are included in **Table 2**. Generally, I notice that rather than negatively affecting the economy, there is some evidence that being regulated is associated with an increase in GDP. The models estimate that being regulated is associated with a 5-7% growth in GDP relative to unregulated. Although being regulated itself seems to be beneficial, increases in carbon prices show the opposite relation, and this is explored further in the Discussion section. At a glance, however, it seems that increasing carbon prices by 1 yuan associates to a 0.09-0.1% decrease in GDP. The average increase in carbon prices in regulated areas was around 1 yuan.

The model that states the most extensive effects of the policy, both of being regulated and of the carbon price, is the standard two-way fixed effects. However, after the parallel trends analysis, this is the model I am less confident about. The subsequent unit-specific trends models bring us closer to the independent effect of the policy. I notice that the models with the expanded propensity score estimate a lower effect and explain a higher proportion of the variation. The removal of coalproducing provinces does reduce the estimate further, but only to a minimal, arguably insignificant extent. Therefore, the preferred estimator is model 4, as it explains the most variance without sacrificing observations.

## Discussion and Limitations

There are several important observations from this analysis. The first is that there is no evidence that cap and trade systems are affecting the economic performance of the cities as a whole. In this case, at least the economy-wide

concerns espoused by firms are so far unfounded. This finding is consistent with the analysis of other emissions trading systems albeit in very different economic conditions, such as California (International Emissions Trading Association, 2018) and the European Union (OECD, 2018). If I had more granular data, perhaps I could make the case that individual firms are being affected. However, given that emissions trading is regulating a substantial proportion of major Chinese firms, it is unlikely that a majority of firms have struggled, in which case the GDP would certainly reflect such a massive loss of productivity. The second is that cap and trade may, in fact, be associated to an increase in GDP of 5-7%, and that this increase is not explained by the decreased productivity of coal exporting provinces, as evidenced in the use of *Model E*. However, correctly determining why regulated cities are growing faster is difficult with the available information and warrants further study, particularly in firmlevel investments and energy efficiency.

A third observation provides an interesting paradox. Although being regulated is associated with economic growth, increases in carbon prices are associated with lower growth. This observation could be explained in two ways. The first is that while being regulated itself may trigger firm behavior that is associated with better technology and higher efficiency, the price of allowances may place substantial burden both on firms and end-users of products that they produce. Electricity, for instance, could be more expensive due to the high carbon content of coal and the price generators pay for using it, reducing overall consumption of electricity and its associated economic growth. If true, emissions trading may only be associated with GDP growth at earlier policy stages before the price of allowances increases and before returns to initial low-carbon investments have started to diminish.

An alternative explanation could be that there is some form of endogeneity regarding the intensity of carbon prices. It may be the case that cities that are further along in the economic transition towards the tertiary sector are both growing slower and pricing carbon higher. This pricing would mirror other developed economies, which show much slower GDP growth rates than China, and place a higher value on environmental goods.

Generally, these conclusions about GDP growth triggered by being regulated and GDP slowdown from higher carbon pricing make internal sense and are consistent throughout the specifications but suffer from some important limitations and challenges. The first is that China tends to enact many hidden distortionary policies in regulated industries (most notably, in the steel industry that are not easily measurable. It may be the case that these specific cities and provinces are exposed to other policies that are correlated to participating in an emissions trading system but are, in fact, explaining most of the variation. For instance, if China is illegally subsidizing its steel exports and decides to remove the subsidy in more developed provinces and cities, the resulting in-

crease in competition may explain the GDP growth. This study cannot address these concerns because the policies are usually, by design, unobservable.

Secondly, the models do not determine whether the carbon price is truly generating emission abatements. Firms could be emitting more greenhouse gases than before, but the carbon market may not internalize it because of inadequate emissions monitoring and verification (MRV) systems. In this case, even though these cities have carbon-intensive economies, the low probability of detection by the policy's MRV system has not changed firm behavior. Unfortunately, if firms are indeed concealing their carbon footprint, the emission trading regulator itself is unaware of it, and data of misreporting is inaccessible.

Finally, several reports (Bloomberg, 2018), (Ingraham, 2018), have documented that the government often manipulates China's GDP data. The manipulations might not necessarily invalidate the study, but it depends on the nature of the manipulation. If the criteria for manipulation of GDP are consistent across cities, then this only affects the magnitude of the relation, but not the findings themselves. If, on the other hand, certain cities are tweaked more than others, then the internal validity of the study is compromised.

## **Conclusion**

Emissions trading remains a critical tool in the fight against climate change but one with many uncertainties, including its effect on economic growth. This study suggests that there is so far no evidence of economy-wide slowdowns in regulated Chinese cities and provinces, at least at current carbon prices.

The study also suggests two first-order policy insights, both of which require further research:

- 1) Simply regulating, or even showing credible commitments to regulate carbon, may incentivize changes in firm behavior that increase overall GDP. To this effect, the goals of sustaining economic growth and reducing China's carbon footprint may be achieved simultaneously if certain competitive behaviors are targeted.

- 2) However, the observed benefits may be specific to current carbon prices (which are still relatively low). High cost of purchasing allowances may pose genuine burdens on firms that affect productivity after a certain threshold, and which may even trickle down to end-users in the form of higher prices.

As China plans its expansion of the policy, these insights can be explored further, particularly in the case of greater availability of firm-specific data on performance and abatement strategies and data on final goods consumption

from regulated industries. Pursuing these intuitions further may lead to essential benchmarks for quantitative policy design and policy inputs for anticipating firm behavior as they react to capped carbon emissions.

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**Appendix**

**Table 1:**

This table shows whether the difference in GDP growth is statistically different between treatment and control groups in years prior to the treatment. During 2013, only Shenzhen was treated, starting mid-year. Still, I notice a statistically significant difference among the groups.

	(1)	(2)	(3)
<i>Placebo test on GDP growth</i>	In 2011	In 2012	In 2013
<i>Difference for cities that will be treated</i>	0.00108 (0.00772)	-0.00580 (0.00722)	0.0186** (0.00756)
<i>Constant</i>	0.210*** (0.00310)	0.125*** (0.00290)	0.0950*** (0.00303)
<i>Observations</i>	286	286	286
<i>R-squared</i>	0.000	0.002	0.021

Standard errors in parentheses  
 \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table 2:**

This table shows the effect of entering the policy (being regulated) and of the intensity of the policy (carbon price) on logged GDP. The results should be interpreted as % increases rather than absolute numbers. All models cluster standards errors at a city level. Besides *Model B*, *Models C*, *D*, and *E* also use unit-specific trends, but *model A* does not. *Model E* uses the same weighting criteria as *model D*. All estimations use inverse proportional weighting, as specified in the methodology section.

<i>Effect on logged GDP of:</i>	(A) Two-way fixed effects	(B) Unit specific trends	(C) Weighted by GDP growth match	(D) Weighted by all relevant covariates match	(E) Limit sample to non-coal producers
<i>Being regulated</i>	0.120*** (0.0198)	0.0703*** (0.0132)	0.0707*** (0.0132)	0.0568*** (0.0145)	0.0561*** (0.0145)
<i>Increasing the carbon price by 1 yuan</i>	-0.00152*** (0.000308)	-0.00134*** (0.000293)	-0.00135*** (0.000298)	-0.000949*** (0.000279)	-0.000943*** (0.000286)
<i>Constant</i>	4.561*** (0.00665)	144.1*** (0.566)	148.2*** (0.571)	133.1*** (1.540)	132.2*** (1.868)
<i>Observations</i>	2,463	2,463	2,462	2,297	1,747
<i>R-squared</i>	0.822	0.962	0.959	0.983	0.986
<i>Number of cities</i>	287	287	286	266	203

Standard errors in parentheses  
 \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Figure 1: Parallel trends before treatment

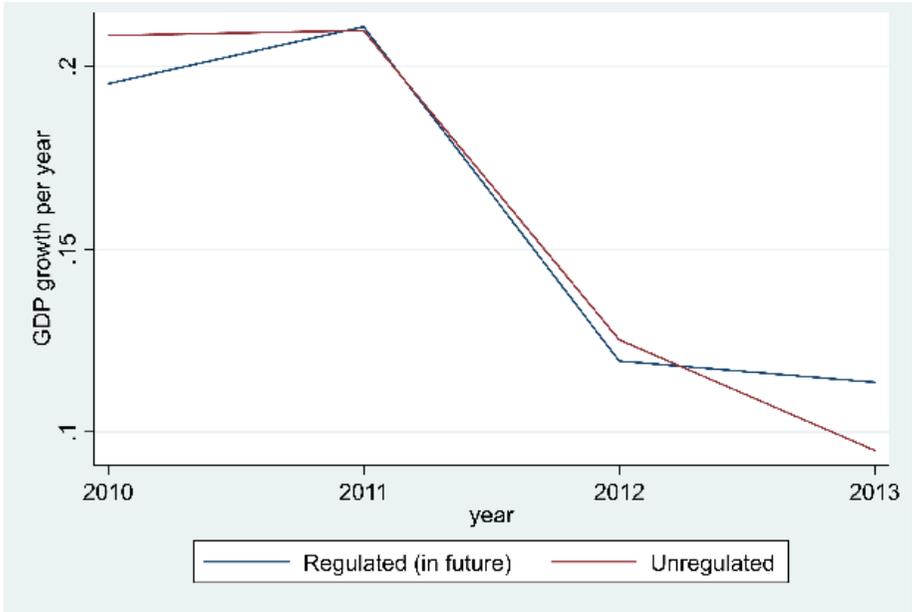
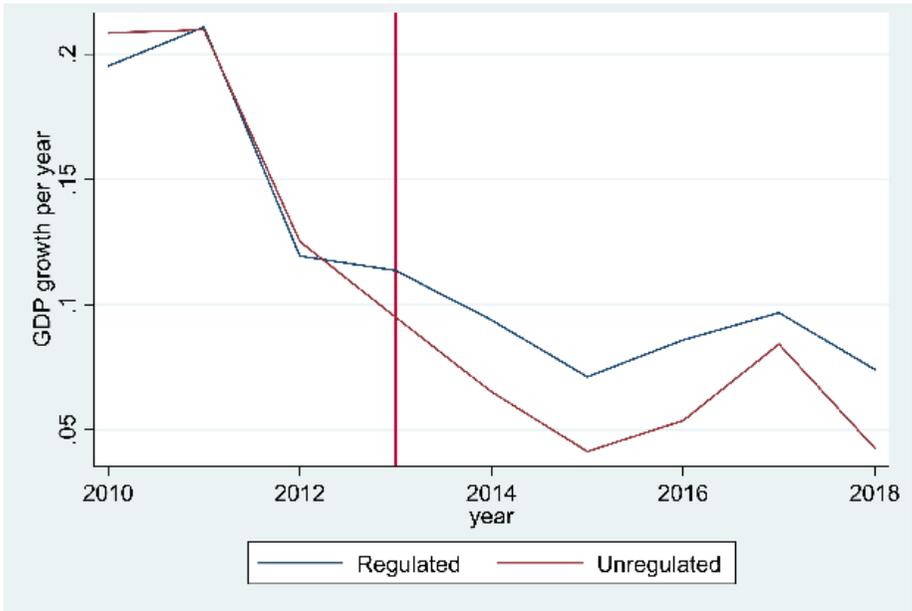


Figure 2: Overall trend in entire timeframe

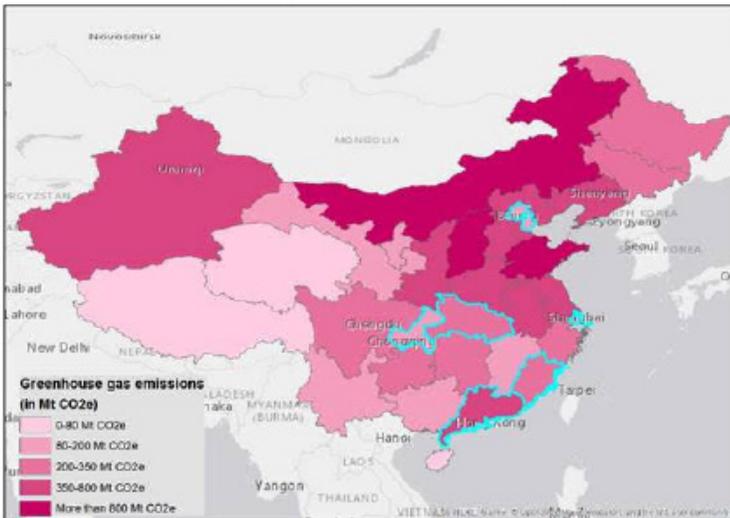


**Map 1:** Map of the 287 Chinese cities explored for this paper



Date: March 2019  
Map by: Andrés Prieto

**Map 2:** Provincial greenhouse gas emissions in China in 2015  
(treatment provinces selected in blue)



Data source: "China CO2 Emission Accounts 1997-2015." Scientific Data 5 (2018)  
Date: March 2019  
Map by: Andrés Prieto

**Map 3:** Provincial coal production in China, 2015  
(treatment provinces selected in blue)



Data source: National Bureau of Statistics of China  
Date: March 2018  
Map by: Andres Prieto



# Measuring the Effect of Climate and Demographic Variables on Climate Change Attitudes in the United States

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It is critical that the United States implement mitigation and adaptation strategies to avoid climate change. Political will remains a significant barrier to implementing climate action policy. Understanding how climate attitudes are formed and what causes changes in attitudes can offer valuable insights for policymakers. This paper will examine how the interaction of climate variables and demographic variables affect climate change attitudes in the United States. Storm density and temperature anomalies serve as climate variables. This analysis finds that increased storm density is associated with weaker belief in anthropogenic climate change in Trump counties and associated with stronger belief in anthropogenic climate change in Clinton counties. Demographic variables interacted with climate variables such as percent of Evangelical affiliation demonstrate an additional significant negative relationship with climate change belief and risk perception. Findings from this analysis could be used to craft communication strategies to build support for adaptation and mitigation policies in the United States.

## Introduction

Climate change is predicted to cause serious economic and ecological damage by the end of century. Without immediate action to limit greenhouse gas emissions, scientists expect the United States to see increased temperature extremes, changes in precipitation patterns, and increased frequency and intensity of storms (Herring et al., 2018). **Figure 1** depicts predicted damages

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in the United States caused by climate change for the end of the 21st century. Adopting greenhouse gas mitigation and adaptation strategies require political will, driven by a constituency that believes in climate change and understands the risks of harm (Kellstedt et al, 2008). Lack of political will and skepticism toward climate change remain significant challenges to implementing climate action policy in the United States. Scholars have demonstrated two important factors in the construction of climate change attitudes: personally experiencing the effects of climate change and belonging to certain demographic groups. Therefore, understanding how climate attitudes are constructed and what causes changes in climate change attitudes is critical information for policymakers in order to craft communication strategies to create a constituency that supports climate action policies.

This paper will examine how climate events and demographic factors affect climate change attitudes in the United States. This analysis uses data measuring climate change beliefs and risk perceptions at the county level from the 2014, 2016, and 2018 Yale Program on Climate Change Communication (YCCC) Climate Opinion Surveys. Additionally, storm density and temperature anomalies serve as climate variables, and race, gender, education, religion, and Trump vote share serve as demographic controls. The share of votes for Trump in the 2016 election, by county is an important variable for this analysis, given the obstruction of climate action from the Trump Administration<sup>1</sup>, and the fact that many counties with high Trump vote share are predicted to experience the worst damage caused by climate change by the end of this century (**Figures 1 and 2**).

This analysis supports findings in the literature that personally experiencing the effects of climate change affects attitudes towards climate change, however, demographic variables affect the sign and magnitude of this effect (Arbuckle et al., 2013; Leiserowitz, 2006; Singh et al., 2017). This analysis also supports the findings in the literature that race, and political affiliation demonstrate a strong, statistically significant negative relationship with climate change belief and risk perception (Kellstedt et al., 2008; McCright & Dunlap, 2011). I also find that increased storm density<sup>2</sup> is negatively related to climate change attitudes in Trump counties, and positively related to climate change attitudes in Clinton counties. On the contrary, temperature anomalies are positively related to climate change attitudes in both Trump and Clinton counties. Findings from this analysis could be used to frame climate action policy in

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1. Notably, the Trump Administration initiated the withdrawal of the United States from the Paris Agreement, the agreement within the United Nations Framework Convention on Climate Change in which each participating country commits to reducing greenhouse gas emissions.

2. Storm density is a constructed variable measuring overlapping storm paths; see methodology for more details.

the context of local temperature anomalies, in order to build a supportive constituency of both liberal and conservative voters.

## **Literature Review**

Social and behavioral scientists find that individuals support policies that address climate change, in part, based on their perceived risk of experiencing the effects of climate change (Singh et al., 2017). Leiserowitz found that American adults most commonly related global warming to melting polar ice, which lead to a perception of climate change as a “geographically and temporally distant” phenomenon (Leiserowitz, 2006). Similarly, Singh et al. (2017) found that if an individual thinks the effects of climate change are far from them, they will be less interested in supporting both government and personal action against the climate change. Both of these studies show how distance can impede action on climate change. However, due to the cross-sectional nature of these studies, they do not examine how attitudes change over time or with proximity to the effects of climate change.

When individuals personally experience the effects of climate change, they are more likely to support mitigation and adaptation strategies. Spence, et al. (2011) found that residents reported less uncertainty about climate change and were more willing to limit their energy use when they had personal experience with coastal flooding. Similarly, Arbuckle et al. (2013) found that those who believed in anthropogenic climate change and whose crops had been affected by higher temperatures and reduced precipitation, were more likely to support policies for adaptation and mitigation. Spence et al. and Arbuckle et al.’s findings suggest that both a physical and psychological proximity to climate change creates positive attitudes toward mitigation and adaptation strategies.

In addition to physical proximity to the effects of climate change, gender, race, and political ideology also contribute to the formation of attitudes on climate change. Women and racial minorities are more likely to believe in climate change and perceive it as risky (Kellstedt et al., 2008). McCright (2010) found that in the United States, women demonstrate greater fear about climate change than men, and these differences are not explained by societal gender norms or belief systems. The gender difference in climate change attitudes appears to also be related to political beliefs. Jylhä et al. (2016) study in Brazil and Sweden found conservative men to be more likely to deny anthropogenic climate change when compared to conservative women, and liberal individuals. Similarly, McCright and Dunlap (2011) show that in the United States, conservative white males are the most likely group to deny climate change and conclude that these views contribute to the high amount of climate change denial in the United States. Thus the literature demonstrates that both physical proximity and demographic factors affect the formation of climate change attitudes, but it has not yet examined how these factors interact with each other.

This paper seeks to fill this gap in the literature by examining the effects of both physical proximity to climate change events and demographic factors on climate change attitudes in the United States. By exploring the interactions of these factors, I seek to show the additional effects of both personally experiencing climate change and social factors on climate change attitudes.

## Data

In order to analyze the relationship between climate change factors and attitudes toward climate change, I estimate the effect of climate change events and demographic factors on climate change belief and risk perception. The measures of climate change belief and risk perception that I use come from the 2014, 2016, and 2018 Yale Program on Climate Change Communications (YCCC) surveys. The YCCC surveys are nationally representative and can be used to model climate change opinion at the state, county, and congressional district level (Marlon et al, 2018). Two questions were chosen from the beliefs section and two questions were chosen from the risk perception section. These were chosen because the literature shows that people who believe in anthropogenic climate change and find it risky are more likely to support adaptation and mitigation strategies. The results of the following questions serve as the dependent variables for this analysis:

Beliefs:

*Do you think that global warming is happening?*

- % “Yes” (Alternative: “No”)

*Assuming global warming is happening, do you think it is...?*

- % “Caused mostly by human activities” (Alternative: “Caused mostly by natural changes in the environment”)

Risk Perceptions:

*How worried are you about global warming?*

- % “Very worried/somewhat worried” (Alternative: “not very worried/not at all worried”)

*How much do you think global warming will harm you personally?*

- % “A moderate amount/a great deal” (Alternative: “not at all/only a little”)

**Figure 4** shows changes in the average answers to the four questions from 2014 to 2018. The maps suggest that affirmative responses to these questions on climate change beliefs and risk perceptions have increased in the majority of the United States. However, some counties in the United States appear to have decreased affirmative responses to these questions, with clustering in the Midwest and Southern regions. **Figure 2** demonstrates the 2016 Presidential election outcome at the county level. The clustering of negative climate attitude changes appears to occur near areas that Trump won in 2016, motivating further investigation into the effect of the proportion of Trump vote share on climate change attitudes.

#### Climate variables: Mean storm events

Climate change can cause an increase in quantity and/or intensity of storms due to rising air and water temperatures, as well as rising sea levels (Herring, et al., 2018). Storms represent a natural and random “shock” which aids in causal inference for changes in climate change attitudes. To capture the effect of storms on climate change attitudes, I constructed a storm density variable representing overlapping storm paths using the National Oceanic and Atmospheric Administration (NOAA) Storm Events Database. NOAA defines storm events as one of the following: hurricane, tornado, thunderstorm, hail, floods, drought conditions, lightning, high winds, snow, and temperature extremes (NOAA, 2018). Temperature extremes were excluded from this analysis because temperature anomalies were captured as a separate variable. Instead of using a count of storm events per county, storm density was used because it captures the geographical intensity of storms over space.

Since the timing of the YCCC climate opinion survey in the survey year was unknown, the storm density of years preceding the survey was also included to account for the possibility that storms could have occurred after the survey was administered that would not have affected responses. Figure 5 below depicts the changes in storm event density from 2014 to 2018, demonstrating changes in storm patterns, as well as a greater concentration of storm density in the south and southeastern United States, where many counties with high Trump vote share are located (**Figure 2**).

#### Climate variables: Temperature anomalies

Temperature anomalies are also included as a control for perceptions towards climate change risks. Temperature anomalies are defined as deviations from long-run average temperatures. This variable was constructed using gridded temperature data from the University of Delaware Air Temperature and Precipitation V5.01 1900–2017 data set (University of Delaware, 2018). Due to the lack of data for survey year 2018, only temperature anomalies for the years preceding the survey (2013, 2015, and 2017) were included. The long-

run average temperature used was 1957–1980, a standard used by the National Aeronautics and Space Administration.

To construct the variable, the average monthly temperature values were extracted from the gridded data set for years 1957–2017 within the cartographic boundaries of the United States in MATLAB (MathWorks 2018). The average monthly temperatures were collapsed in Stata to create an average yearly temperature value for each cell for the years 2013, 2015, and 2017, and an average across 1957–1980. A shapefile of U.S. counties was put over this cell data in ArcMap and the values per county were extracted.

The temperature anomaly variable was then calculated using the following formula, where  $t$  is equal to the year of interest:

$$\text{Temperature Anomaly}_t = |\text{Mean Temperature}_t - \text{Mean Temperature}_{1957 \text{ to } 1980}|$$

The absolute value of the temperature anomaly was used to capture differences from long-run average temperatures, which could result in negative temperature anomaly values if the mean temperature in a year is lower than the long-run mean temperature. Longer, colder winter storms, such as the polar vortex, are also effects of climate change (Kretschmer, 2018), and it is important to capture these changes in this analysis.

### Demographic variables

Demographic variables are also included in the model, given their salience towards climate change as demonstrated in the literature. I include the following county-level variables: proportion of Trump votes, (MIT Election Data and Science Lab, 2016), proportion white (U.S. Census, 2010), proportion male (U.S. Census, 2010), proportion Evangelical (Religious Congregations and Membership Survey, 2010), and proportion of college graduates (U.S. Census, 2010).

### **Methodology**

A simple logistic regression and two-way fixed effects model are used to analyze the effect of climate change factors on attitudes towards climate change. County-level fixed effects ( $B_i$ ) help control for characteristics of each county that do not vary over the time period, such as geographic features like being landlocked. Yearly fixed effects ( $A_t$ ) will control for characteristics that would have affected all of the counties in the same year. The proposed econometric model is as follows:

$$\text{Climate attitude}_{it} = \beta_0 + \beta_1 \text{Climate variable}_{it} + A_t + B_i + u_{it}$$

The results of this model are in **Table 1**. The mean storm density in the survey year and previous year (“mean storm lagged”) demonstrate negative, statistically significant (at 95 percent confidence) relationships with climate change attitudes. Meaning, with an increase in storm density, we see decrease in the percent of people in each county that have positive attitudes toward climate change. This is counter to the evidence in the literature that increased exposure leads to more positive attitudes toward climate change. Temperature anomaly demonstrates a positive, statistically significant (at between 95 and 99 percent confidence) relationships with climate change attitudes. This means that a deviation from the long-run average temperature of one-degree Celsius (positive or negative) results in an increase in the percentage of people in each county that have positive attitudes toward climate change. This finding is consistent with the literature.

Fixed effects are removed in the next model in order to introduce the effects of demographic factors in addition to climate variables. In order to evaluate the relationship between the climate attitudes and the individual demographic factors, the following model with only time fixed effects is proposed:

$$\text{Climate attitude}_{it} = \beta_0 + \beta_1 \text{Climate}_{it} + \beta_2 \text{Male}_i + \beta_3 \text{White}_i + \beta_4 \text{Evangelical}_i + \beta_5 \text{Trump}_i + A_t + u_{it}$$

The results of this model are available in **Table 2**. The mean storm density variables continue to demonstrate negative, statistically significant relationships with climate attitudes even when controlling for demographic factors. Proportion Trump vote shows the coefficient of highest magnitude, with decrease in affirmative climate change attitudes between 14.97 and 27.22 percentage points across the four other questions. Since the proportion Trump variable is defined as a value between 0 and 1, the coefficient represents the decrease in an affirmative response when going from 0% to 100% vote share for Trump in a county.

Proportion Evangelical population also demonstrates a negative, statistically significant relationship with climate change attitudes, although at a smaller magnitude than Trump vote share. Proportion male presents a positive, statistically significant relationship, across the four questions. Proportion white and proportion college graduates present differing relationships with climate change attitudes, varying from positive to negative, significant to insignificant, and small to large magnitude of coefficients.

Trump vote is the vote share by Trump in 2016, collected by the MIT Election Science and Data Lab. Evangelical is the proportion of a county that was Evangelical in 2010, measured by the Religious Congregations and Membership Survey. Male is the proportion male of each county, White is the proportion of white people in each county, and College grad is the proportion of college

graduates in each county, collected from the 2010 United States Census.

## Discussion

This analysis demonstrates that climate change and demographic variables affect attitudes towards climate change differently. In Trump counties, an increase in storm density decreases positive attitudes toward climate change, while an increase in temperature anomalies leads to an increase in positive climate change attitudes. Interaction analysis shows that when experiencing an increase in mean storm density or temperature anomalies, whiteness, and Evangelical affiliation have an additional negative effect on climate change attitudes.

These results demonstrate a potential confirmation bias for the effects of climate change. Perhaps people in Clinton counties are more likely to believe in anthropogenic climate change and perceive it as risky, therefore experiencing the effects of climate change increases positive climate change attitudes. People in Trump counties could be less likely to believe in climate change to begin with, therefore experiencing the effects of climate change decreases positive attitudes toward climate change. An example of this could be experiencing colder, longer winter storms, which are often not interpreted as the effects of climate change by climate change skeptics, and instead used as evidence against climate change. If a high Trump vote share county were to experience an increase in winter storms, this could be misinterpreted as evidence against climate change, therefore reducing positive climate change attitudes. This confirmation bias hypothesis seems to only hold for the storm density variable, whereas increases in the temperature anomaly variable lead to increases in positive climate change attitudes, even in Trump counties. There appears to be a fundamental difference in the interpretation of storm density and temperature anomalies as effects of climate change and how they affect climate change belief for Trump voters.

The findings of this analysis are important for communication strategies and framing of climate action policy. Increases in storm density and temperature anomalies result in an increase of positive climate change attitudes in Clinton counties. This indicates that framing climate change action around the effects of climate change, especially in a local context, could be an effective strategy to gain support for mitigation and adaptation strategies in Clinton counties.

In the case of Trump counties, increases in storm density resulted in a decrease of positive climate change attitudes. This indicates that framing policy around these effects would not be an effective strategy for fostering support for climate change action. However, increases in temperature anomalies result in higher rates of positive climate change attitudes in both Trump and Clinton counties. This indicates that framing climate action policies in terms of

temperature changes in a local context could be an effective strategy in both Trump and Clinton counties. Including other effects of climate change, such as economic or health damages, could be other effective framing strategies for Trump counties, but they are out of the scope of this analysis.

## **Conclusion**

In conclusion, this analysis supports the findings in the literature that white and conservative demographic factors have a strong relationship with negative attitudes toward climate change. This analysis also showed that Evangelical affiliation, related to whiteness and Trump, demonstrated a negative relationship with climate change attitudes. When these demographic factors were interacted with climate variables, only whiteness and Evangelical affiliation showed an additional negative and significant effect. These results provide actionable information for policymakers on communication strategies for climate action policy. Of the climate variables used, only changes in temperature anomalies resulted in an increase in positive climate change attitudes in both Trump and Clinton counties. Framing policy in terms of temperature anomalies as an effect of climate change could be an effective strategy to gain support for climate action across Trump and Clinton voters.

The year 2020 will provide for ample opportunity to expand this research. In 2020, another YCCC Climate Opinion Survey is due to be released and we will also have additional years of climate data. The United States will also hold another presidential election and census, which will give opportunity to update the demographic factors. All of this new data will introduce variation to the models, in order to better understand the formation of climate change attitudes in the United States and create support for climate change action.

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**Table 1**

VARIABLES	Yes, global warming is happening	Global warming is mostly caused mostly by human activities	Very worried or somewhat worried about global warming	Global warming will harm me personally a moderate amount or a great deal
<i>Mean storm</i>	-0.0694* (0.0388)	-0.0798** (0.0391)	-0.0904** (0.0435)	-0.0824*** (0.0267)
<i>Mean storm lagged</i>	-0.134*** (0.0303)	-0.182*** (0.0305)	-0.181*** (0.0339)	-0.0462** (0.0209)
<i>Temp anomaly</i>	0.0373*** (0.0122)	0.0373*** (0.0122)	0.0327*** (0.0136)	0.0239*** (0.00836)
<i>Year Fixed Effects</i>	Yes	Yes	Yes	Yes
<i>County Fixed Effects</i>	Yes	Yes	Yes	Yes
<i>Constant</i>	59.11*** (0.0491)	44.81*** (0.0495)	48.86*** (0.0550)	32.07*** (0.0338)
<i>Observations</i>	9,330	9,330	9,330	9,330
<i>R-squared</i>	0.648	0.668	0.531	0.732

Standard errors in parentheses  
 \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table 2

VARIABLES	Yes: global warming is happening	Global warming is mostly caused mostly by human activities	Very worried or somewhat worried about global warming	Global warming will harm me personally a moderate amount or a great deal
Mean storm	-0.429*** (0.0359)	-0.361*** (0.0342)	-0.462*** (0.0387)	-0.377*** (0.0264)
Mean logged	-0.327*** (0.0283)	-0.241*** (0.0270)	-0.367*** (0.0305)	-0.232*** (0.0208)
Temp anomaly	0.0148** (0.000590)	0.00505 (0.00562)	0.00505 (0.00562)	0.00618 (0.00434)
Trump vote	-26.24*** (0.323)	-26.41*** (0.326)	-24.34*** (0.307)	-27.08*** (0.347)
Evangical	-2.107*** (0.166)	-2.307*** (0.170)	-1.068*** (0.158)	-2.813*** (0.179)
Male	9.519*** (1.569)	10.02*** (1.568)	9.783*** (1.495)	11.49*** (1.689)
White	-0.0475 (0.286)	0.0942 (0.285)	3.810*** (0.272)	-1.284*** (0.307)
College grad	1.253** (0.628)	1.280** (0.628)	0.998 (0.633)	-6.335*** (0.676)
Year FE	Yes	Yes	Yes	Yes
County FE	No	No	No	No
Constant	71.52*** (0.895)	71.20*** (0.894)	70.80*** (0.900)	53.09*** (0.853)
Observations	9.276	9.276	9.276	9.276
R-squared	0.721	0.720	0.716	0.692

Standard errors in parentheses  
\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table 3.** Regression with climate variables and proportion Trump vote interaction.

VARIABLES	Global warming is happening	Global warming is mostly caused by human activities	Very worried or somewhat worried about global warming	Global warming will harm me personally a moderate amount or a great deal
Mean storm	0.830*** (0.171)	1.005*** (0.172)	1.174*** (0.191)	0.390*** (0.117)
Mean storm # Trump vote	-1.414*** (0.261)	-1.705*** (0.263)	-1.987*** (0.292)	-0.743*** (0.180)
Temperature anomaly	-0.115** (0.0510)	-0.117** (0.0514)	-0.118** (0.0571)	-0.0993*** (0.0350)
Temp anomaly # Trump vote	0.239*** (0.0777)	0.243*** (0.0783)	0.237*** (0.0871)	0.193*** (0.0534)
Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Constant	59.11*** (0.0490)	44.81*** (0.0493)	48.87*** (0.0548)	32.07*** (0.0337)
Observations	9,330	9,330	9,330	9,330
R-squared	0.650	0.649	0.534	0.733

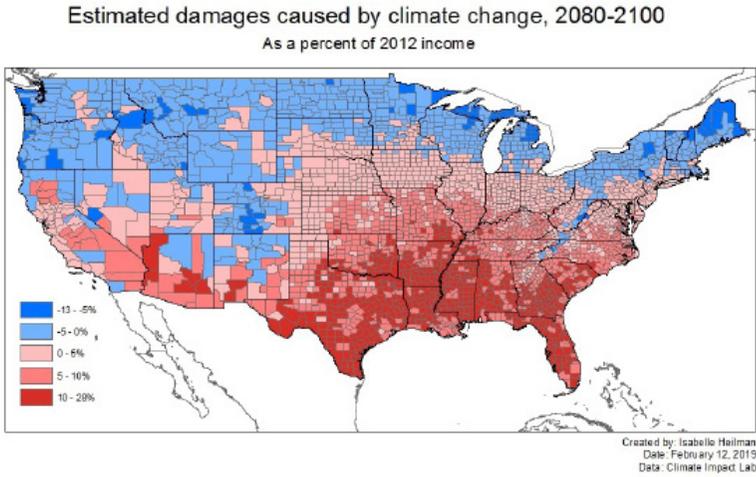
Standard errors in parentheses  
 \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Notes on Table 1:** Regressions are at the county and year level. Dependent variables are measured as share of county, collected by the Yale Program on Climate Change Communication in 2014, 2016, and 2018. Mean storm is the mean storm density per county in years 2014, 2016, and 2018, constructed using data from the NOAA Storm Events Database. Mean lagged is the mean storm density per county in years 2013, 2015, and 2017, also constructed using data from the NOAA Storm Events Database. Temperature anomaly is the absolute value of the difference between the annual mean temperature in the county in years 2013, 2015, and 2017 and the mean annual temperature for that county between 1957 and 1980, collected from the University of Delaware Air Temperature and Precipitation V5.01.

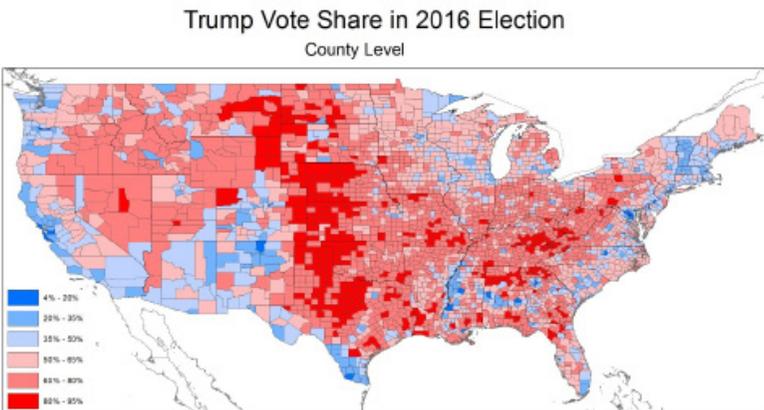
**Notes on Table 2:** Regressions are at the county and year level. Dependent variables are measured as share of county, collected by the Yale Program on Climate Change Communication in 2014, 2016, and 2018. Mean storm is the mean storm density per county in years 2014, 2016, and 2018, constructed using data from the NOAA Storm Events Database. Mean lagged is the mean storm density per county in years 2013, 2015, and 2017, also constructed using data from the NOAA Storm Events Database. Temperature anomaly is the absolute value of the difference between the annual mean temperature in the county in years 2013, 2015, and 2017 and the mean annual temperature for that county between 1957 and 1980, collected from the University of Delaware Air Temperature and Precipitation V5.01. Trump vote is the vote share by Trump in 2016, collected by the MIT Election Science and Data Lab. Evangelical is the proportion of a county that was Evangelical in 2010, measured by the Religious Congregations and Membership Survey. Male is the proportion male of each county, White is the proportion of white people in each county, and College grad is the proportion of college graduates in each county, collected from the 2010 United States Census.

**Notes on Table 3:** Regressions are at the county and year level. Dependent variables are measured as share of county, collected by the Yale Program on Climate Change Communication in 2014, 2016, and 2018. Mean storm is the mean storm density per county in years 2014, 2016, and 2018, constructed using data from the NOAA Storm Events Database. Temperature anomaly is the absolute value of the difference between the annual mean temperature in the county in years 2013, 2015, and 2017 and the mean annual temperature for that county between 1957 and 1980, collected from the University of Delaware Air Temperature and Precipitation V5.01. Trump vote is the vote share by Trump in 2016, collected by the MIT Election Science and Data Lab.

**Figure 1.** Estimated costs caused by climate change as percent of 2012 county income, 2080 - 2100. Data from Climate Impact Lab.



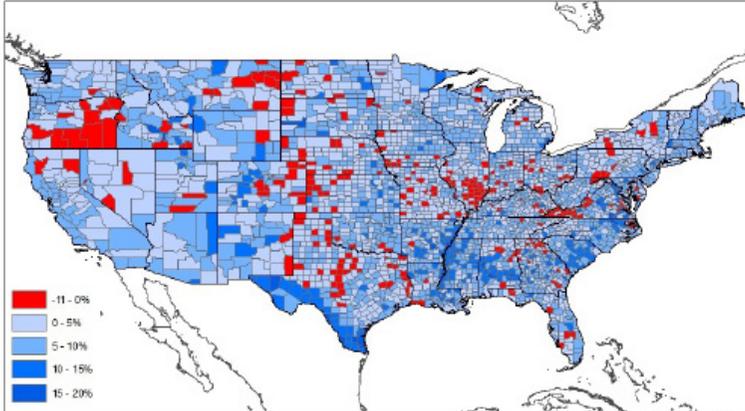
**Figure 2.** Trump vote share in the 2016 Presidential election, by county. Data from MIT Election Data and Science Lab.



**Figure 3.** Changes in percentage points from 2014 to 2018 in the four YCCC questions on climate change belief and risk perception.

A. Yes, global warming is happening.

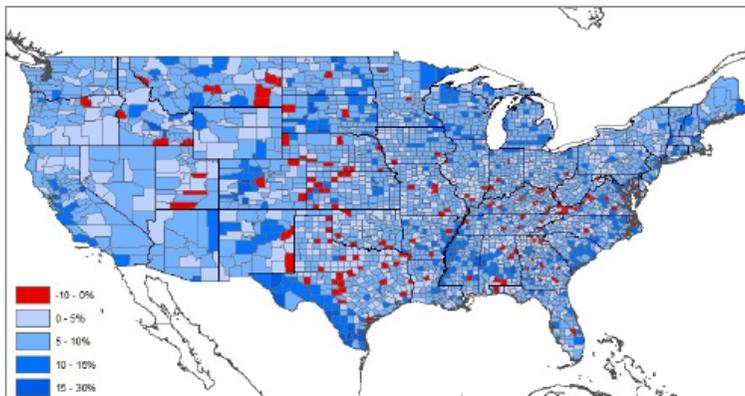
Change from 2014 to 2018 in answers to "Do you believe global warming is happening?"  
Percent change, by county



Created by: Isabelle Heilman  
Date: February 13, 2019  
Data: Yale Program on Climate Change Communication

B. Global warming is anthropogenic.

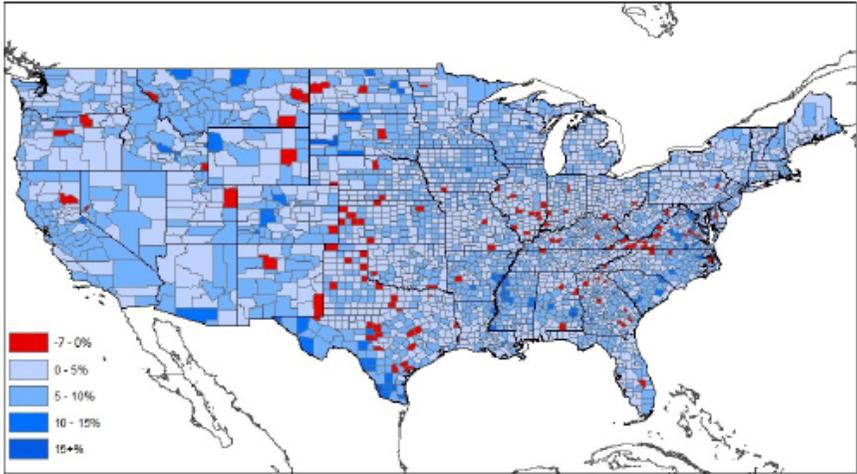
Changes from 2014 to 2018 in answers to  
"Do you believe global warming is mainly caused by human activities?"  
Percent change, by county



Created by: Isabelle Heilman  
Date: February 13, 2019  
Data: Yale Program on Climate Change Communication

C. I am very/somewhat worried about global warming.

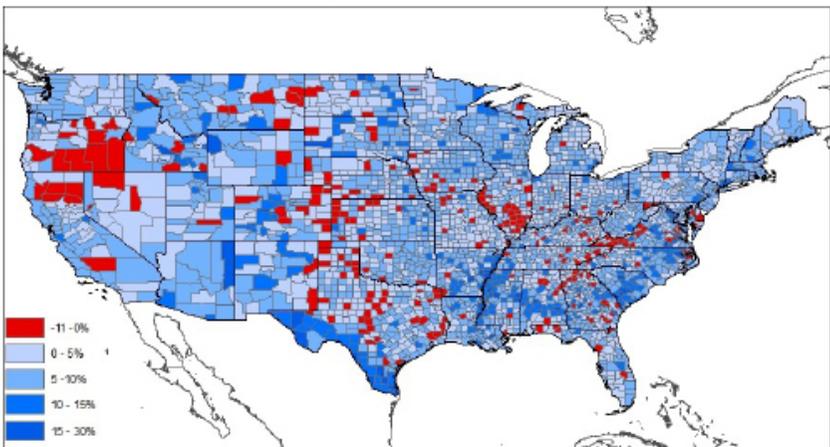
Changes from 2014 - 2018 in answers to  
"Do you believe climate change will personally affect you?"  
Percent change, by county



Created by: Isabelle Heilman  
Date: February 13, 2019  
Data: Yale Program on Climate Change Communication

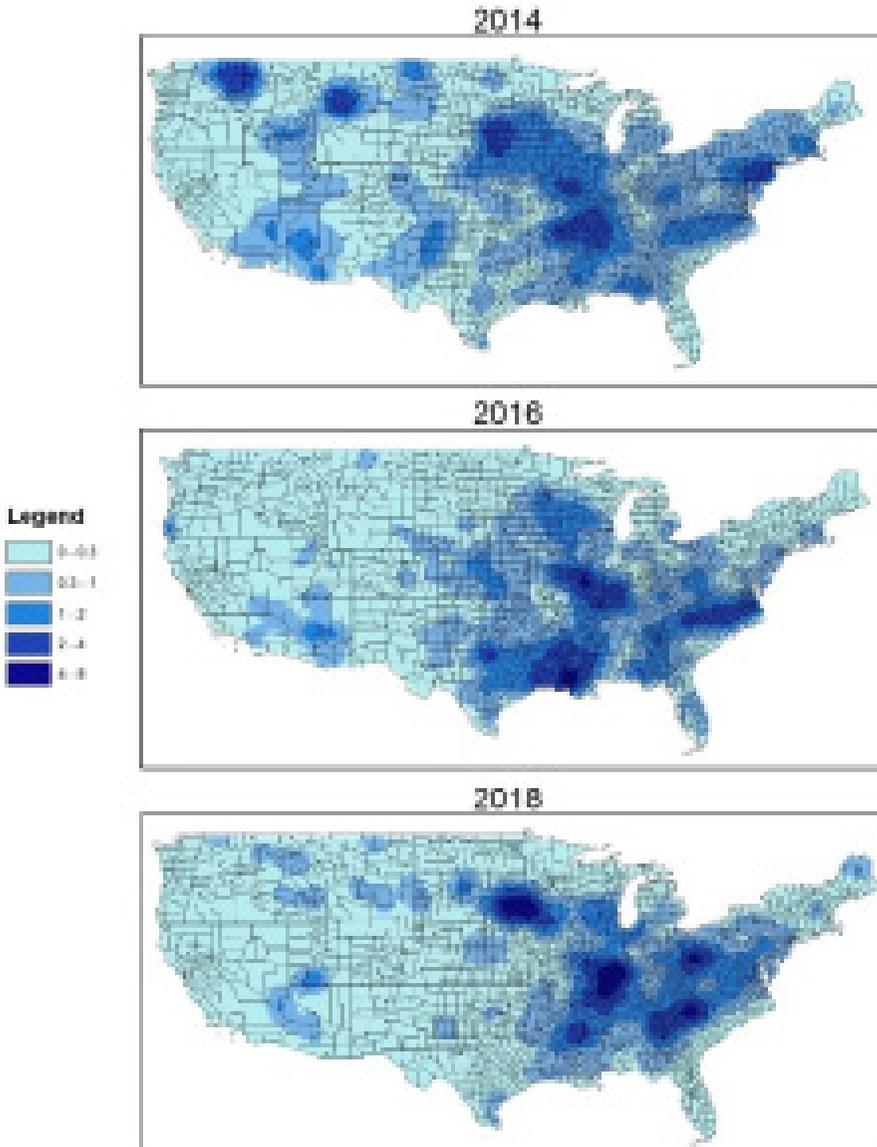
D. Climate change will harm me a moderate amount or a great deal.

Changes from 2014 to 2018 in answers to  
"How worried are you about global warming?"  
Percent change, by county



Created by: Isabelle Heilman  
Date: February 14, 2019  
Data: Yale Program on Climate Change Communication

**Figure 4.** Mean storm density in the United States. Data from NOAA Storm Events Database.





# SOVEREIGN SIGNALS: SUCCESS IN INDONESIAN IUU FISHING POLICIES

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The South China Sea (SCS) is a major hotspot for territorial disputes amongst a number of East and Southeast Asian nations. A major victim of these territorial disputes has been the region's fishing stocks and those who depend upon them: consumers, the fishing industry, and the larger food system. IUU fishing, overfishing, and environmental degradation all threaten the future of this valuable resource. However, resolution of these disputes, as well as efforts targeted directly at IUU fishing and overfishing, have been largely unsuccessful in resolving these issues. This paper argues that these multilateral resolution efforts were likely to fail as a result of power asymmetries and competing interests within the relevant international and regional institutions. Utilizing a game theoretic framework, we provide the case study of Indonesia, whose eccentric and stern unilateral efforts to reduce IUU fishing under the leadership of Minister Susi Pudjiastuti between 2014 and 2019, showed measurable success. The Indonesian case suggests potential viability in unilateral policy approaches in developing effective resource management practices, should governing international institutions continue to fall victim to power asymmetries and conflicting interests.

## Introduction

The South China Sea (SCS) has been a major focus of political, environmental, and legal disputes in the Asia Pacific within the 21st century, bringing global intrigue to the intricacies of maritime sovereignty and governance. The bulk

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of the disputes are born from competing territorial claims over island chains between Southeast Asian nations and China. However, these disputes have not been contained to traditional security issues. Instead, the instability of this maritime region has allowed vital ecological resources to fall victim to extralegal over-extraction. The most apparent casualties are fisheries. Indonesia's recent anti-IUU fishing policies, under the leadership of Minister Susi Pudjiastuti, have successfully utilized unilateral strong-arm tactics to secure the nation's fishery resources from foreign pressures and has challenged contemporary multilateral frameworks for managing vital fishery resources.

Fisheries are a key resource in the SCS, providing economic and health benefits to local economies as a source of food and a commodity for international markets. These benefits are threatened by artificial island developments and illegal, unregulated, and unreported (IUU) fishing, forcing many stocks to the brink of collapse in a region that provides about 12 percent of global fish catch (Bodeen, 2019). In fact, some would argue that the value of the fisheries, let alone the threat of their collapse, will further perpetuate the intensity behind the maritime disputes (FNI, 2017).

This potential positive feedback loop has raised concerns amongst environmentalists and security experts alike. Literature produced by such concerned groups has focused on frameworks for resolving the overfishing that are not dependent on resolving the territorial disputes (South China Sea Expert Working Group, 2017). However, a single actor at the southern end of the disputed region has challenged this narrative with explosive policies: Indonesia.

Indonesia is the second largest fishing nation in terms of marine capture production, capturing an average of more than 5 million tons yearly, employing over 2.6 million Indonesians (FAO, 2018). Indonesia's archipelagic nature provides great reason to protect its fishing resources from foreign fishing interests and has shown effective progress in doing so under the dynamic leadership of Susi Pudjiastuti, Indonesia's Minister of Maritime Affairs and Fisheries from 2014-2019. Under Minister Susi's policies, overfishing in Indonesian waters has decreased substantially and the continuation of her policies could lead to a 12 percent increase in profit from catch for some stocks by 2035 (Cabral et al., 2018).

However, this has not been achieved through multilateral cooperation, as existing international institutions would necessitate, but rather through unilateral action within Indonesia's sovereign maritime domain. The success of these policies could facilitate a counter-narrative that may also contribute to resolving the maritime territorial disputes. We will analyze why Indonesia's unilateral fishery policies have proven effective despite territorial disputes, foreign IUU fishing, and political pressures.

## **Components of Indonesian Fishery Policy**

Indonesia's fishery policy is comprised of three main components: (1) strong-arm patrolling of Indonesian waters, (2) informational data for operational transparency, and (3) traditional sustainable fishery management methods. All of these components include aspects of traditional fishery management, but the former two have been paired with methods that are intended to assert control over sovereign waters.

Strong-arm patrolling of Indonesian waters has become the most visible component of this policy. Since her appointment as Minister of Maritime Affairs, Minister Susi has received seemingly unending press coverage regarding her unique method of detonating captured IUU fishing vessels (Hutton, 2017). These detonations are often portrayed by the government and popular media as extravagant shows of force, with the occasional photo-op to accompany the event (AP, 2017).

Prior to these forceful measures, Indonesia was victim to nearly 10,000 IUU fishing vessels every year, many of them of Chinese origin (AP, 2017). This is partially due to the Natuna Islands territorial dispute between Indonesia and China, where a section of the islands' exclusive economic zone crossed into China's 'Nine-Dashed Line' (Beech and Suhartono, 2018). However, some of these incursions are also due to expanding foreign fishing pressures amid declining fish stocks (Hutton, 2017).

As a response, Minister Susi has declared protecting national sovereignty of utmost importance. Indonesia declared two 6-month moratoria on foreign fishing vessels in 2014 and coopted a task force of navy officials, maritime police, coast guard, and the attorney's general's office to track and capture any IUU fishing vessels (Tennesen, 2018). While not all captured vessels receive an explosive treatment, most are sunk to firmly assert the policy.

Aside from the 'on the water' patrolling, the second component of Indonesia's fishing policy is improved vessel data information and transparency. In 2017, Indonesia became the first country to release data from their Vessel Monitoring System (VMS), which accurately reveals Indonesian fishing vessel location information and provides clarity on the geographic location of registered fishing efforts (Cutlip, 2017). The Maritime Affairs and Fishery ministry has also partnered with non-governmental organizations, such as Global Fishing Watch (GFW), to process and publicly disseminate this data.

A valuable aspect of this data is that Indonesia strictly requires all registered fishing vessels to operate within the VMS. By implementing this requirement, Indonesia has essentially created a gold standard of transparency in fishing fleet activity and recruited international academic and NGO assistance in

understanding their own fishery industry. This data can be used in a number of applications beneficial to Indonesian fishing policy, such as measuring unmonitored fishing effort when paired with nightlight satellite imagery (Cabral et al., 2018).

The two former policies relatively unique to Indonesia are further backed by traditional sustainable fishery management methods. These include catch size limits, gear type regulations, marine protected areas, and fishery information dissemination. In fact, Minister Susi has received international recognition for these efforts with a number of awards and honors for environmental stewardship (WWF, 2016).

However, some of these policies have also proven to be controversial, especially amongst some domestic fishing interests, as some of the restrictions that have been implemented have been with regard to several forms of damaging fishing practices that were popular within the domestic industry (Beech and Suhartono, 2018). Regardless, international recognition may play a part in increasing the visibility of Indonesia's commitment to sovereignty.

These three policy components have achieved successful results despite many of the methods being unilateral in nature. This potential counter-narrative to the expected ineffectiveness of unilateral action amongst foreign pressures appears to be dependent on asserting sovereignty and control over Indonesia's maritime domain. Indonesia's efforts in this regard can be analyzed in order to identify the core drivers of Indonesia's policy success.

### **Explaining the Success of Indonesia's Commitment**

The success of Indonesia's anti-IUU fishing policies can be analyzed using game theory. Specifically, Indonesia's success comes from overcoming a commitment problem through signaling. In this instance, Indonesia faces a bargain with 'foreign pressures' (IUU fishing and foreign state supported fishing such as in the Natuna Islands dispute).

When foreign pressures aim to extract resources from Indonesian waters, they base their decision to do so on the perceived gains of succeeding, perceived costs of being caught, and the perceived risk of being caught. This is the 'negotiation'. The gains will typically be the fishery catch. However, in some instances this may extend to political moves in claiming territory. If the potential gains outweigh the potential costs, foreign pressures will attempt to extract these fishery resources. Indonesia is most capable of altering the costs and risks of being caught in the bargaining process rather than altering the gains. If they can successfully raise these risks and costs, they can deter the foreign pressure.

Minister Susi's scuttling policy effectively raises the cost of being caught. Prior to Minister Susi's appointment, Indonesia's IUU policy lacked strong negative incentives that would indicate a high cost to being caught (Ministry of Marine Affairs and Fisheries, 2012). Under Minister Susi's administration, the near guarantee of a vessel being sunk if caught is quite costly. The risk of being caught is also higher as task force patrols have increased and data transparency can help identify locations of foreign pressure activity.

Ultimately, the key to Indonesia's success in this negotiation process is in the detonation of captured vessels as a signal. The spectacle of the detonation is a result of Indonesia knowing that it is participating in a repeated game, even if each individual bargain is with a different vessel. The spectacle does nothing to reduce the damages already incurred from the captured vessel. Rather, it signals to future participants that Indonesia is credibly committed to maintaining its fisheries. A reiterated signal (multiple detonations) shows the player's reputation for honesty and thus future players are informed of the commitment (McMillan, 1992). If foreign pressures are made aware of a commitment to increase the costs and the risks prior to the bargain, they will be more likely to forego the resource extraction as seen in the reduction of IUU fishing reports under Minister Susi's policy.

## **Discussion and Conclusion**

Indonesia has successfully reduced foreign fishing pressures through strong-arm patrol tactics and information transparency which have signaled a raised cost and risk of capture to potential IUU fishing participants. Part of these policies are unilateral in nature. Multilateral conditions may be incapable of providing the same credible commitment due to conflicting interests in the multilateral management of disputed resources.

The limitation of this framework is that these policies may be dependent on the dynamic leadership of Minister Susi. The public admiration and spectacle with which she carries out the sinking of vessels is a major driver of the amount of press coverage the policy receives. This coverage contributes to the effectiveness of the key aspect of the framework: signaling.

Indonesia's regional neighbors, who also suffer from extensive foreign effort on domestic fishery resources, may not be able to depend on finding a leadership figure such as Minister Susi. However, the essence of the minister's policies can be extracted and replicated by interested states. The key component of such a policy would be a focus on unilateral action or at least strictly non-conflicted multilateral action. Attempts to resolve these issues with multilateral institutions, such as ASEAN, have proven failures because the interests of the member states have conflicted to the point of lacking consensus on an underlying desire to end IUU fishing, the most basic

component necessary for effective policy solutions. Not all member states are incentivized to support the goal, resulting in crippling inefficiencies. Yet, a shift to unilateral action for states with strong naval presence, and unified multilateral action for states without, provides a viable alternative to such inefficiencies, as proven by Indonesia's recent efforts.

In 2019, to the shock of many, Minister Susi was not reappointed as Minister of Maritime Affairs and Fisheries (Gokkon, 2019). A change in leadership style may limit the effectiveness of this framework if Indonesia is unable to display the same commitment, as backing down from a commitment can be very costly (Muthoo, 2000). Nonetheless, Indonesia has shown that unilateral management can improve ecological resource health amidst foreign pressures when paired with a signaled commitment to upholding claims of sovereignty.

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# COALITION SIZE AND PUBLIC SPENDING IN PARLIAMENTARY DEMOCRACIES:

## BARGAINING, COALITION PAYOFFS, AND COALITION MAINTENANCE

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This study seeks to identify the effect of the size of coalition governments government expenditure and outline the exact causal mechanism through which this effect occurs. In doing so, it identifies and tests a bargaining and payoff mechanism in which total government expenditure increases through coalition bargaining over fiscal allocations as payoffs must be made to more players to maintain support of the ruling government. Through the analysis of twenty-three parliamentary democracies from 1995 to 2014, the effect of coalition size on spending growth and the proposed causal chain is tested. To identify the effect, the study uses a two-way fixed effects estimator with country and year fixed effects. Recognizing that coalition formation is the result of strategic bargaining, the study subjects the finding to rigorous robustness tests including using shocks to coalition size to approximate a degree of randomness. Although the model correctly identifies the correct sign and direction of the proposed effect, the model lacks sufficient power to properly identify the effect at conventional confidence levels giving only limited support to the hypotheses. The paper concludes with a discussion of the difficulty of studying coalitions under assumptions of randomness motivating more cautious future analyses.

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## Introduction

The structure, institutions, and electoral rules of democratic governments are widely understood and accepted as fundamental conditions shaping government and political outcomes, like representativeness, party structure, bargaining, and who can access government.<sup>1</sup> However, there is comparatively less consensus on the extent to which these institutional and policy choices systematically shape and condition policy outcomes. In exploring these types of systematic policy biases of different electoral systems and electoral rules, political scientists have identified a common-pool problem in spending allocations under large coalition governments (Kontopoulos and Perotti, 1999; Persson et. al., 2007). Bawn and Rosenbluth (2003) have modeled the political logic of the common-pool problem predicting that larger coalitions ought to generate larger public sectors. However, this literature does not carefully consider the identification problems in modeling and comparing coalition behaviors across countries.<sup>2</sup> This study seeks to contribute to this existing literature by more clearly modeling the effect of coalition size to test a specific mechanism by which this increase may occur. It is the perspective here that the mechanism driving larger coalitions to spend more is coalition bargaining and payoffs to maintain the coalition, and that in this fight over government resources, larger coalitions choose to increase government spending.<sup>3</sup>

This study tests this bargaining mechanism through analysis of the heterogeneous impact of the marginal coalition partner as the number of ministerial portfolios changes. The number of cabinet ministers acts as a proxy for the number of dimensions in the bargaining range and the number of players. We would expect that if bargaining is driving the effect of coalition size on government spending that as bargaining becomes more challenging the effect of larger coalitions on government spending will decrease. This perspective is consistent with an alternative hypothesis in the literature arguing that coalitions may have a status quo bias due to bargaining impasses (Blais et. al., 2010). Essentially, larger coalitions due to the wider array of preferences represented will be forced to bargain over a wider issue range

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1. See Taagepera & Shugart 1989; Schofield & Laver 1985; Grofman & Lijphart 1984; Gamson 1961; Duverger 1954; Lijphart 1984; Riker 1962.

2. For example, Kontopoulos & Perotti 1999 choose to exclude all coalitions formed after September and shorter than 60 days and include a battery of fiscal controls to model revenue and spending effects. Bawn & Rosenbluth model the common-pool problem from a cost perspective including controls for electoral system and fragmentation. Persson et. al. 2007, on the other hand, instruments for fragmentation with electoral rules to explain spending outcomes.

3. See Martin & Vanberg 2004 for an analysis of how divergent preferences of coalition partners affect the policymaking process.

and, in allocating government resources, leaders will opt to increase spending in lieu of dividing the current government budget among a larger range of alternatives (Figure 1). We expect this dynamic to be particularly acute in parliamentary democracies because prime ministers serve with the consent and confidence of the legislature forcing them to maintain the support of their coalition members to remain in power, as such this study is restricted to the study of parliamentary democracies.<sup>4</sup>

The findings of this study are relevant for our understanding of the relative benefits of electoral rules favoring larger coalition vis-à-vis those favoring single-party governments. If large coalitions systematically spend more, then, in the long run, parliamentary democracies favoring larger coalitions are more likely to generate large fiscal deficits and inefficient spending targeted not at constituents but at special interests essential to coalition maintenance. If this is in fact the case, the inefficient expenditure of large coalitions may consume the benefits of the electoral rules that generate large coalitions that articulate a larger range of preferences in government.<sup>5</sup> If, however, this effect is mediated by cabinet size, policies increasing cabinet size may be more likely to generate ineffective governments by increasing the number of veto players. If the alternative is true and more veto players cause an increase in the marginal effect of coalition size, then proposals to increase cabinet size could be weighed against increases in spending.

The method used to test the core hypothesis that larger coalitions spend more, and the theorized bargaining mechanism mediating the effect is a two-way fixed effects estimator with country and year fixed effects. This method allows for in-country comparisons controlling for institutional factors, country level variation, and other confounding factors. Moreover, it allows us to construct the most similar counterfactual such that a larger coalition government's counterfactual comparisons are the other governments formed under the same regime. Although this method is common in the literature, it often fails to consider how the study of coalitions conforms to the estimator's underlying assumptions of randomness. Therefore, recognizing that there is not a strong argument supporting the assumption of as-if random variation, the model is subjected to robustness and falsification tests and an analysis of shocks to coalition size is conducted to approximate a degree of exogenous

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4. A parliamentary democracy, here, is defined by the legislature's ability to remove the Prime Minister through a vote of no-confidence.

5. For these arguments, see Grofman & Lijphart 1984; Riker 1962; Lijphart 1984. Rules that favor larger coalitions are institutional features like low electoral thresholds, larger district magnitudes, and proportional representation systems. Democratic theorist have long extolled the virtues of democracies that allow marginal groups access to power.

variation. The paper nevertheless concludes with a discussion of the difficulty in studying coalitions under assumptions of randomness.

## Literature Review

Most studies of coalition size and expenditure hypothesize that coalitions ought to spend more as the bargaining range grows (Kontopoulos & Perotti, 1999; Persson et. al., 2007; Bawn & Rosenbluth, 2003). The exact causal mechanism hypothesized varies and is only rarely concretely identified. However, a growing number of studies have tested and found evidence for an alternative hypothesis that as coalitions grow the government's status quo bias grows (Blais et. al., 2010). The mechanism driving this status quo bias is bargaining challenges generated by increases in the number of players and potential veto points. This study addresses both of these hypotheses by examining the impact of coalition size under different fiscal conditions and by analyzing the heterogeneous impact of coalition size as the number of veto players changes. The contribution of this analysis is the test of a specific mechanism by which coalition size affects spending. By narrowing the study to one causal pathway, it adds to the literature through the construction of a more concrete understanding of what does and does not drive observed changes in spending. Moreover, this study adds to the literature as a more careful examination of the extent to which changes in coalition size can be treated as-if random in statistical analyses.

Roubini et. al. (1989), in an early contribution to the study of coalition effects on government spending, argued that the relative weakness of multi-party coalitions contributed to the growth of fiscal deficits in industrial economies. This study found support for the idea that coalition governments spend more, but was not able to fully prove whether it was due to their hypothesized mechanism of relative coalition strength and cohesion. Kontopoulos and Perotti (1999) added to Roubini's findings by considering the effect of cabinet size on coalition spending. This study goes further by using the size of the cabinet to condition the effect of coalition size. As their theory of change focused on how politicians internalize the costs of spending changes, Kontopoulos and Perotti assumed that the effect of cabinet size was independent from that of coalition size. Bawn and Rosenbluth (2003) similarly test a story based on politicians' perception of costs and how it is mediated by electoral rules. Blais et. al. (2010) bucks the trend in the literature to find no effect of parliamentary size on spending instead, finding evidence of a status quo bias. They argue that large coalitions find it difficult to increase or decrease spending because of an increase in the number of veto players when veto players is explained by the relative ideological distance in the cabinet. However, they do not sufficiently examine the institutional factors that contribute to the strength and number of veto players. In addition to studies focusing on the coalition effects on government

spending, scholars have considered the effects of other institutional designs. For example, Persson et. al. (2007) and Milesi Ferretti et. al. (2002) found that the type of electoral system affects spending outcomes and Heller found that bicameralism leads to higher spending. These significant findings justify the use of the fixed effects model in this study as these institutional rules are country specific and largely invariant so are absorbed by the two-way fixed effects.

Although the literature has not concretely shown the mechanism driving the underlying effect of coalition size on spending, the existing studies have shown that the additional marginal coalition partner causes on average between a 0.12 to 2 percent increase in spending as a portion of GDP (Kontopoulos & Perotti, 1999; Persson et. al., 2007; Bawn & Rosenbluth 2003). These findings would lead us to expect a similarly sized effect in this analysis. Other research has found no effect, so in analyzing the results of this study we will only confidently identify a precisely-measured zero if the study is found to have significant power to detect the effect sizes already in the literature (Blais et. al 2010). The following section will outline the data and method by which this study will attempt to identify the effect of coalition size and test the hypothesized underlying bargaining and payoff mechanism.

## **Data and Methods**

The data for this study largely comes from the Seki-Williams Update to the Party Government Data Set, S-W Data (Seki & Williams, 2014). Seki and Williams provided an updated version of Woldendorp et. al.'s (2000) Party Government in 48 Democracies data set. The Woldendorp data has been used in a large array of studies and the Seki-Williams update has allowed for many of these studies to be updated.<sup>6</sup> This study utilizes data on 23 parliamentary democracies represented in the S-W data formed between 1995 and 2014.<sup>7</sup> Parliamentary democracies are defined as systems in which the government serves only with the consent and confidence of the legislature.<sup>8</sup> Only parliamentary democracies are considered in this study because the coalition politics are fundamentally different in parliamentary systems than in other

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6. In an earlier study addressing the similar questions to those addressed here, Kontopoulos & Perotti (1999) utilized the Woldendorp dataset.

7. The twenty-three countries are Australia, Austria, Belgium, Czech Republic, Finland, France, Germany, Great Britain, Greece, Hungary, Ireland, Israel, Italy, Japan, Latvia, Luxembourg, Netherlands, Norway, Portugal, Slovakia, Slovenia, Spain, and Sweden. The years under consideration vary based on data availability and each state is only considered post-democratization.

8. This is most commonly defined by the legislature's power to remove the government through a vote of no-confidence.

democracies. In parliaments where the legislature can remove the government and prime minister, the logic of coalition bargaining is more clearly defined because the government must maintain the support of its coalition to remain in power. And, therefore, the majority party has incentives to payoff its partners. This relationship between coalition partners is less clear and generalizable in other systems, so their inclusion in the study may bias or obscure the effect of coalition size on spending.

In addition to the data on parliamentary democracies in the S-W Data, data on government expenditures, deficits, and other economic indicators from the OECD's National Accounts and Statistics database are used to construct the main dependent variable and the macroeconomic and fiscal controls (OECD, 2019). The availability of the spending data limited the scope of the analysis. The study is limited to the years and countries in which credible, accurate government spending data is available. As a result, the countries represented in the sample are largely located in Europe and are relatively high-income polities and only coalitions formed between 1995 and 2014 are considered.<sup>9</sup> Moreover, this has generated a decidedly non-random sample of parliamentary democracies. A further limitation of our macroeconomic indicator data that needed to be addressed before conducting the study is that government spending is only observed once each year. Because coalition governments often last less than a year, we had to decide how to weight each government's contribution to the yearly figure when they were only in power for a portion of a year.<sup>10</sup>

Governments were therefore weighted by the proportion of the year in which they were in power. On average, we would expect shorter tenured governments to have a smaller effect on total government spending than longer tenured governments. However, we lack evidence for this hypothesis and have no way to approximate the variation among short-lived governments' impacts. Therefore, the analysis remains agnostic using simply duration to weight each observation.

The main dependent variable in this study is the total government spending as percentage of GDP (Spending). The primary independent variable is the number of parties in the government coalition which is the weighted average of the number of parties that held ministerial posts in each government in a given year (NPC). Overall, the data on the number of coalition partners is relatively mal-distributed. Single-party and two-party governments are observed much more frequently than larger coalitions (**Figure 4**). When

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9. There are gaps in the data and, for countries who democratized after 1975, only the post-democratization period is considered.

10. Of the 420 coalitions on which we have data, 152 coalitions lasted less than a year and 36 lasted less than 90 days (Seki & Williams, 2014).

looking at the distribution of year-on-year changes in coalition size in Figure 5, we see that the change in coalition size in the majority of country-years is zero. Moreover, when the distribution of coalition size is broken down by country in Figure 6, we see that in many states there is little variation in coalition size around its mean and the distribution is often highly skewed. The lack of variance means that the effect sizes for large coalitions is identified by relatively few observations and relatively fewer countries. To test the hypothesis that the effect of coalitions size varies with the number of cabinet ministers, the S-W Data's count of cabinet portfolios is used (Portfolios). The analysis also includes a limited set of controls for macroeconomic conditions lagged one year, namely deficit as a portion of GDP ( $deficit(t-1)$ ), inflation as a portion of GDP ( $infl(t-1)$ ), and the GDP growth rate ( $growth(t-1)$ ).

The core identification strategy employed in this analysis is a two-way fixed effects estimator with heterogeneous effects. This strategy controls for potential confounding country-level factors such as institutional design, electoral rules, and other largely time-invariant factors. Controls for confounding time-varying factors like the country's fiscal deficit, inflation rate, and growth rate are employed as controls because they constrain the government's ability to make payoffs (Kontopoulos & Perotti, 1999; Persson, et. al. 2007; Bawn & Rosenbluth, 2003; Blais et. al., 2010).

The first heterogeneous effects model tests the effect of coalition size on government spending with only the fiscal and economic controls. The lagged fiscal deficit is interacted with the number of parties in coalition because it is believed that the effect of size of the coalition is constrained and moderated by the size of the deficit. This model is given in estimating equation one below.

### Estimating Equation 1:

$$Spending_{it} = \alpha_i + \lambda_t + \beta_1 NPC_{it} + \beta_2 Deficit_{i(t-1)} + \beta_3 NPC_{it} * Deficit_{i(t-1)} + \beta_4 infl_{i(t-1)} + \beta_5 growth_{i(t-1)} + \epsilon_{it}$$

where,  $i$  is a country indicator and  $t$  is an indicator for each year represented in the data.  $Spending_{it}$  is the difference in spending between period  $t$  and period  $t-1$ .  $NPC_{it}$  is the number of parties in coalition in country  $i$  at time  $t$ . It will alternatively be used to restrict the analysis to shocks to coalition size.  $Deficit_{i(t-1)}$  is the government budget deficit in country  $i$  lagged one period,  $infl(t-1)$  is the inflation rate in country  $i$  lagged one period, and  $growth(t-1)$  is the inflation GDP growth rate in country  $i$  lagged one period.  $i$  is a set of country fixed effects for each of the 23 countries in the dataset and  $t$  is a set of time fixed effects for each year in the dataset.

To test the mechanism by which coalition size affects government spending the model expressed in Estimating Equation 1 is modified to include a control

for the number of ministerial portfolios in cabinet is added and the number of portfolios is interacted with the number of parties in the coalition (Estimating Equation 2). This model allows us to identify whether the issue range and number of players affects government spending or mediates the effect of coalition size by testing the hypothesized mechanism by which the number of parties affects spending outcomes. The estimating equation is outlined below.

### Estimating Equation 2:

$$\text{Spendingit} = \alpha_i + \lambda_t + \beta_1 \text{NPCit} + \beta_2 \text{Portfoliosit} + \beta_3 \text{Deficiti}(t-1) + \beta_4 \text{NPCit} * \text{Portfoliosit} + \beta_5 \text{NPCit} * \text{Deficiti}(t-1) + \beta_6 \text{infl}(i(t-1)) + \beta_7 \text{growth}(i(t-1)) + \varepsilon_{it}$$

where,  $i$  is a country indicator and  $t$  is an indicator for each year represented in the data.  $\text{Spendingit}$  is the difference in spending between period  $t$  and period  $t-1$ ,  $\text{NPCit}$  is the number of parties in coalition in country  $i$  at time  $t$ , and  $\text{Portfoliosit}$  is the number of ministerial posts in the cabinet in country  $i$  at time  $t$ .  $\text{Deficiti}(t-1)$  is the government budget deficit in country  $i$  lagged one period,  $\text{infl}(t-1)$  is the inflation rate in country  $i$  lagged one period, and  $\text{growth}(t-1)$  is the inflation GDP growth rate in country  $i$  lagged one period.  $i$  is a set of country fixed effects for each of the 23 countries in the dataset, and  $t$  is a set of time fixed effects for each year in the dataset.

The two-way fixed effects estimator as the principal identification strategy is not without its flaws, although it is the best available alternative given the constraints of the non-random data generating process. The ideal experimental design for this type of analysis would be one in which the researcher could randomly assign coalition governments to many parliamentary democracies and observe each states' spending outcomes. This is of course infeasible. The two-way fixed effect estimator moves us slightly closer to this ideal but still must assume a degree of randomness random variation in coalition formation, the case for which is difficult to make. Nevertheless, at the time of elections, we expect parties to behave strategically such that the easiest coalition always forms. Moreover, we do not expect the addition of each marginal party to be the same. The marginal party is not always equal. Therefore, it is not clear to what extent we can treat the effect as random. Coalitions should always choose the minimal sized party closest to their ideal point<sup>11</sup>. Two-way fixed effects take as a counterfactual the single-party and differentially sized governments in previous periods. Since we know these coalitions are not generated randomly, it is not necessarily the case that the governments are equal on other covariates that may bias the model. By testing for heterogeneous effects, this study attempts to account for some of these effects. Moreover, it will employ the use of shocks as a robustness check and to approximate exogenous random variation. However, this strategy is hampered by the low number of shocks

in the data. Therefore, the study proceeds with the two-way fixed effects estimator in full recognition of its limitations and the studies inability to fully satisfy randomness assumptions. The following section presents the results of the analysis and estimation then subjects the models to falsification and robustness test.

## **Results**

Overall, the findings show limited evidence in support of the hypothesis that coalition governments spend more than single party governments. The direction and sizes of the effects are by and large in line with the hypotheses, however the effect of coalition size is not significant at 95 percent under any of the specifications. In the models controlling for the number of ministerial portfolios the effect and interaction are significant at 90 percent confidence. Nevertheless, this analysis fails to confirm the existing finding in the literature that coalition size has a significant effect on spending, controlling only for macroeconomic factors. It does, however, provide tenuous support for the hypothesized bargaining and payoff mechanism. Although the central identifying assumption is shown to hold, analysis using shocks to coalition size and when the models are subjected to robustness and falsification tests, the robustness of these findings is challenged. The model's low power nevertheless challenges the formulation of any conclusions and challenges the effectiveness of some of the robustness tests.

The main results are presented in **Table 1** Model 1 presents the unconditional effect of coalition size on government spending with only country and year fixed effects. We find no significant effect of coalition size on spending under this specification. Models 2 and 3 show the estimated effect with the macroeconomic and fiscal control. Model 3 models estimating equation one. The predicted effect of an additional coalition member when lagged deficit is held at its mean (0.11% of GDP) is a 0.006 percentage point increase in government spending as a portion of GDP. This is a relatively small effect size and insignificant at 95% confidence. The direction of the effect is as predicted. This model is only powered to detect a minimum effect of a 0.8 percentage point increase in spending. As the existing literature predicted and has found an effect size between 0.12 and 1.2 percentage points, it is difficult to say whether the effect is a precisely measured zero. At 0.006 percentage points, the point estimate is too small to detect. If that it is in fact the true effect we would likely not be able to build a model able to detect it as there is strict upper limit on the number of parliamentary democracies. Moreover, one could make the case it not a particularly relevant policy finding as the benefits of large coalitions are likely outweighed by a 0.006 percentage point in spending. This model only allowed the effect of coalition size to vary across different levels of fiscal deficits and controlled for macroeconomic factors. It is alternatively

hypothesized that the mechanism driving higher spending in larger coalitions is mediated by a time variant institutional factor: the number of ministerial portfolios in the cabinet.

The number of ministerial portfolios is an institutional factor that could affect spending. In most parliamentary democracies, the number of ministers is determined through the legislative process with variation in the Prime Minister's ability to create new posts. These variations in institutional rules are relatively time invariant and typically constitutionally determined, so they will be accounted for in the model's country fixed effects. The variation in the number of portfolio ministers is expected to affect government spending because, as the number of portfolios increases, the number of players in the bargaining process increases as does the number of issue areas over which the government budget must be allocated. As such, larger cabinets with more ministerial portfolios are expected to spend less than governments with smaller cabinets because the bargaining over the government purse has fewer players and is conducted over a larger range causing a status quo bias. This effect is expected to be more pronounced in larger coalitions because as the ministerial portfolios are divided among more parties the preference divergence among players in the bargaining game widens complicating the bargaining process. When there are fewer seats, a bargain will be more easily struck because there will be fewer actors and payoffs can be more easily made to potential coalition defectors.

Models 4 through 6 show no significant effect of the number of ministerial portfolios at 95 percent. In the heterogeneous effect models (Models 5 and 6), an extra minister is predicted to cause an increase in government spending of 0.3 percentage points. This is intuitive as a new minister would require more staff and generate new government projects. In the heterogeneous effects model, the direction and magnitude of the effect of coalition size and the interaction of coalition size and number of ministerial posts is as hypothesized although not significant at 95 percent. Once again, this null effect is likely due to a lack of power in the model. We are only powered to detect a 3.2 percentage point or greater increase in government spending for a one party increase in coalition size. Similarly, for the interaction term, we are only powered to detect a 0.14 percent effect size. In model 6, which models estimating equation two, the effects are significant at 90 percent, so it is plausible that better powered model may be able to detect the accurate true effect. Figure 2 visualizes the effect of coalition size as the number of ministerial posts changes as predicted under the specifications of estimating equation two. the specifications of estimating equation two.

In model 6, we find only tenuous support for our hypothesized bargaining and payoff mechanism of the coalition size effect on government spending. The effect is not significant at 95 percent at any number of ministerial portfolios,

although Figure 2 does show that as the number of ministerial portfolios grows the predicted marginal effect of an additional coalition member falls. In fact, as the size of the cabinet surpasses 20 the predicted marginal effect of an additional partner becomes negative such that larger coalitions begin to spend less than smaller coalitions. At a ten percent confidence level, we are able to detect an effect, so a more powerful model may have been able to detect the effect. The decrease in the effect size with growing number of ministerial portfolios provides evidence that changes in the probability and ease of striking a coalition bargain and in targeting payoffs to coalition partners is the mechanism that is driving coalition size's effect on spending. Because there exists a heterogeneous impact of coalition size on spending as the coalition bargaining game changes, we are better able to understand the how coalition size affects spending. We are, however, unable to concretely identify this mechanism given the constraints of our sample of parliamentary democracies. Against expectation, in Model 6, the deficit has no predicted impact on the marginal impact of coalition size on spending.

The results of the two-way fixed effects analysis with heterogeneous impacts shows very limited support for our hypotheses and given the models' power limitations we are unable to distinguish the estimated impacts from zero at standard level of confidence or precisely identify a null effect. The following section further probes and tests the validity of the fixed effects estimator. It subjects the model to robustness and falsification tests. The first test attempts to approximate and model the effect with a greater degree of exogenous variation. Then, the model's identifying assumption is subjected to a falsification test. Finally, the findings articulated above are tested for their sensitivity to alternative specifications.

### **Falsification and Robustness Tests**

The central assumption of the fixed effect estimator is as-if random variation in the explanatory variable as previously outlined, the coalition formation process is decidedly not random. Per Riker's theory of minimal-size coalitions, we can predict ex-ante that the lowest cost, minimally sized coalition will form. Moreover, this means that the counterfactual government generated by the estimator may not be the best, most accurate counterfactual. To approximate exogenous variation, shocks to coalition size are used. Shocks were defined in two ways; first as a change larger than 1.5 parties, then as a change larger two-party. The results of this robustness test are presented in **Table 3**. Although in theory shocks should be a more valid identification strategy, as shown in Figure 5, there are very few shocks to coalition size. In fact, in models 1 and 2 there are only thirty switchers and in models 3 and 4 there are only 16 switchers. As a result, the findings are identified on relatively few observations. When coalitions are defined as a 1.5 party change, no significant effect is found replicating estimating equation two (models 1 &

2). When defined as a 2-party change, we however do find a significant effect (model 4). The signs on the shock identify and the interaction of shocks switch. A two party increase when lagged deficit and number of portfolios are held at their means a shock causes approximately a five-percentage point decrease in spending. This challenges the validity of our main findings, but it is unclear the extent to which we should believe the findings in the model given it is identified on so few switchers. In model 1 and 2, the model is identified on only 30 and in models 3 and 4 only 16. Nevertheless, our model's validity is challenged by this test approximating more random variation.

In addition to testing the robustness through the shocks analysis, the main fixed effects estimator was subjected to a falsification test, probing the extent to which changes in coalition are exogenous to changes in GDP. Because changes in GDP growth affect government spending, we must test that these changes are not also driving coalition sizes. If this were the case, any observed effect of coalition size could be driven by changes such as GDP. Figure 7 visualizes the logic of the falsification showing that the effect of coalition size must be shown to be separate from the effect of GDP on spending. **Table 2** presents the finding of the falsification. They show that, although GDP has a positive effect on spending, there is no significant relationship between GDP and coalition size. Although the fixed effect estimator is still an imperfect identification strategy, it does not falter due to the failure of a key identifying assumption that coalition size is exogenous to other factors affecting government spending outcomes.

Congruent to recasting the size of coalitions in terms of shocks and probing the models' identifying assumptions, the robustness of the findings was also tested by measuring coalition size as percent change in coalition size. This allows coalition size to be measured relative to the size of the previous coalition. Therefore, we allow the effect of the marginal party to change when the previous government was larger or smaller. We once again find no significant effect in any of the models (**Table 5**). However, once again, we are underpowered to detect the true effect. A final robustness test excludes countries with the largest average coalition sizes. These are the four states in the 95th percentile. The four excluded states are Belgium, Finland, Israel, and Ireland. However, the point estimates change greatly under different specifications showing that our model is sensitive to the specification of the independent variable and the countries included in the analysis. Overall, the robustness and falsification tests show mixed results. Although the model passes the falsification test, the robustness tests show mixed results. The shock analysis shows that under a more strenuous definition of randomness the point estimates and significance change. Moreover, the robustness tests recasting the coalition sizes changes the point estimates but also finds no significant effect due largely to low power.

## **Conclusion**

This study has outlined the effect of coalition size on government spending building on the existing by attempting to identify the specific causal mechanism by which coalition size affects spending outcomes. By testing whether bargaining and payoffs made to coalition partners in the process of coalition maintenance, the study contributes to the literature by adding to our understanding how parliamentary democracies' electoral rules and institutional design systematically affect policy outcomes. The study tested whether more difficult bargaining environments generate heterogeneous impacts in the marginal effect of an increase in coalition size to test whether bargaining affects how coalition size affects spending. The appearance of a heterogeneous effect where the effect of the marginal party declined with cabinet size was taken as evidence in favor of the proposed bargaining causal mechanism.

The two-way fixed estimator with heterogeneous effects used to identify the effect of coalition size on spending failed to find conclusive evidence of an effect of coalition size on spending or concrete evidence of the proposed bargaining and payoff mechanism. Moreover, the findings were not found to be sufficiently robust to alternative specifications. The main estimations found that the effect of coalition size and the heterogeneous impact due to the growth in the number of ministerial portfolios were properly sized with correct signs and to be significant at a lower level of confidence. . The inability to identify the true effect is a result of the models' relatively low power. In the future, researchers should attempt to gather a larger dataset with more parliamentary democracies in more years to increase the model's power. Nevertheless, even if a more powerful model were to be developed, our model would still suffer the same threat to validity generated by the analysis of coalitions with the assumption that the underlying formation process is as if random and with a non-random sample.

The identification strategy used here highlights the shortcomings of fixed effect analysis of coalition behavior. The two-way fixed effect regression assumes a degree of randomness the case for which is difficult to make. Parties behave strategically in the coalition formation process such that the easiest coalition always forms, moreover we ought not expect the addition of each marginal party to be the same. Because this strategy is widely used in the existing literature, this work highlights where previous works utilizing similar methods have failed to identify the validity of their counterfactual and the satisfaction of FE's identifying assumption. Moreover, it suggests that we must pay closer attention to how studies of strategic political behavior consider model assumptions of randomness. The non-random sampling makes the case for the model's external validity more challenging and can

explain the differential results achieved in this model when compared to other studies that utilized data from different similarly non-random samples. Future studies should attempt to construct careful identification strategies that try to best approximate randomness in the causal identification strategy. In the case of studies of coalition governments, innovative approaches with large sample sizes will be necessary.

Overall, in understanding the results of this study, one must consider the limitations of the two-way fixed effects estimator as an identification strategy and the model's low power. Nevertheless, this study has made an important first step in its attempt to identify the specific causal mechanism underlying the effect of coalition size on policy outcomes. Though unable to concretely show the validity of the hypothesis that bargaining drives the effect, the study has outlined a path forward to a better understand the systematic policy effects of electoral rules and institutions that generate larger or smaller coalitions.

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## Appendix

**Table 1.** *Effect of Coalition Size on Government Spending*

	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Total Gov. Spending</i>					
<i>Parties in Coalition</i>	0.0568	-0.000273	0.00872	0.133	2.919+	2.954+
	(0.432)	(0.473)	(0.467)	(0.436)	(1.678)	(1.657)
<i>Lagged Deficit</i>		-0.440***	-0.365*			-0.360*
		(0.0576)	(0.164)			(0.157)
<i># of Portfolios</i>				-0.132	0.312	0.302
				(0.214)	(0.259)	(0.253)
<i>Parties in Coalition * # of Portfolios</i>					-0.132+	-0.136+
					(0.0721)	(0.0715)
<i>Parties in Coalition * Lagged Deficit</i>			-0.0251			-0.0271
			(0.0517)			(0.0496)
<i>Lagged Inflation Rate</i>		0.0313	0.0298			-0.0134
		(0.0817)	(0.0820)			(0.101)
<i>Lagged GDP Growth</i>		0.0646	0.0708			0.0922
		(0.0762)	(0.0744)			(0.0683)
<i>Country Fixed Effects</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Year Fixed Effects</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Constant</i>	30.14***	29.78***	29.77***	32.40***	23.67***	23.49***
	(0.373)	(0.399)	(0.396)	(3.622)	(4.972)	(4.737)
<i>N</i>	420	402	402	414	414	397
<i>Root MSE</i>	3.091	2.868	2.871	3.076	3.026	2.799
<i>R2</i>	0.805	0.832	0.832	0.806	0.813	0.841
<i># of Clusters</i>	23	23	23	23	23	23

Clustered Standard errors

in parentheses +  $p < 0.10$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

**Notes:** The above table presents the main findings from six fixed effects regression. All six models include country and year fixed effects. All standard errors are clustered at the country level. The dependent variable is total government spending as a percent of GDP. The main variable of interest is Parties in Coalition which is simply the number of parties in coalition in each country-year. Model 1 predicts the unconditional effect of coalition size with no controls. Model 2 adds three macroeconomic controls: the deficit as a percentage of GDP, the inflation rate, and GDP growth rate. All macroeconomic controls are lagged one period. Models 4 through 6 test the hypothesis that cabinet structure conditions the effect of coalition size, including the number of ministerial portfolios as a control in model 4 and, in model 5 and 6, allowing for a heterogeneous effect of coalition size for different size cabinets. Model 3 and Model 6 correspond to the main regressions outlined in the empirics section.

**Table 2.** Falsification Tests

	Effect of GDP Growth Rate		Effect of GDP	
	(1)	(2)	(3)	(4)
	Log Total Govern- ment Spending	Parties in Coalition	Log Total Government Spending	Parties in Coalition
GDP Growth Rate	-0.00836 (0.00586)	-0.0223 (0.0127)		
Log GDP			0.827*** (0.108)	0.0973 (0.476)
Country Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Constant	16.34*** (0.0114)	1.254*** (0.167)	5.685*** (1.384)	-0.0753 (6.048)
N	448	531	448	533
Root MSE	0.0913	0.836	0.0663	0.839
R2	0.996	0.691	0.998	0.687
# of Clusters	23	23	23	23

Standard errors in parentheses  
 \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

**Notes:** The above table presents falsification for the identifying assumption that counterfactual changes in GDP are unrelated to changes in coalition size. All models include country and year fixed effects. All standard errors are clustered at the country level. The dependent variables in models 1 and 2 is logged government spending in US million dollars. Models 2 and 4 conduct the falsification test with number of coalition parties as the dependent variable. The two explanatory variables of interest are GDP growth rate and logged GDP in US million dollars. Models 1 and 2 test the correlation in growth rates as models 3 and 4 test correlation in GDP. The models show no significant relationship between GDP and the size of coalitions at 95% confidence. The relationship between growth rate and coalition size is, however, significant at 90%.

**Table 3.** Analysis of Shocks to Coalition Size

	Increase in Coalition Size > 1.5		Increase in Coalition Size > 2	
	(1)	(2)	(3)	(4)
	<i>Total Gov. Spending</i>			
<i>Size Increase &gt; 1.5</i>	0.923 (0.914)	-3.276 (3.611)		
<i>Size Increase &gt; 2</i>			0.530 (1.341)	-9.002* (3.864)
<i>Lagged Deficit</i>		-0.418*** (0.0680)		-0.425*** (0.0619)
<i># of Portfolios</i>		-0.136 (0.199)		-0.125 (0.197)
<i>Size Increase &gt; 1.5* # of Portfolios</i>		0.184 (0.163)		
<i>Size Increase &gt; 1.5* Lagged Deficit</i>		-0.415 (0.249)		
<i>Size Increase &gt; 2* # of Portfolios</i>				0.446* (0.170)
<i>Size Increase &gt; 2* Lagged Deficit</i>				-1.418* (0.509)
<i>Lagged Inflation</i>		-0.0689 (0.116)		-0.0717 (0.118)
<i>Lagged Growth Rate</i>		0.0856 (0.0701)		0.0822 (0.0711)
<i>Country Fixed Effects</i>	Yes	Yes	Yes	Yes
<i>Year Fixed Effects</i>	Yes	Yes	Yes	Yes
<i>Constant</i>	30.18*** (0.144)	32.23*** (3.459)	30.19*** (0.144)	32.02*** (3.428)
<i>N</i>	419	396	419	396
<i>Root MSE</i>	3.062	2.838	3.066	2.840
<i>R<sup>2</sup></i>	0.809	0.837	0.809	0.836
<i># of Clusters</i>	23	23	23	23

Clustered Standard errors in parentheses  
+ p < 0.10, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

**Notes:** The above table presents a robustness test analyzing shocks to coalition size. It is hypothesized that a shock provides more exogenous variation than the analysis of marginal shifts. Models 1 and 2 define shock as an increase larger than 1.5. Models 3 and 4 define the shock as greater than two. All models include country and year fixed effects and clustered SEs at the country level. The dependent variable is total government spending as a percent of GDP. The shock variables are binary indicators. Models 1 and 2 are identified on 30 switchers and Models 3 and 4 on 16. As a result, the models are fundamentally underpowered. Models 1 and 3 show the unconditional effect of the shock. Models 2 and 4 model our main estimating equation replacing the number of parties with the shock indicators.

**Table 4. Robustness Test Testing the Effect of Relative Changes in Coalition Size**

	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Total Gov. Spending</i>					
<i>% Change in Coalition Size</i>	0.00176 (0.00166)	0.00116 (0.00147)	0.00377+ (0.00194)	0.00171 (0.00175)	0.0260 (0.0426)	0.0181 (0.0432)
<i>Lagged Deficit</i>		-0.425*** (0.0642)	-0.423*** (0.0642)			-0.427*** (0.0601)
<i>Lagged Inflation</i>		-0.00921 (0.0812)	-0.00307 (0.0840)			-0.0581 (0.117)
<i>Lagged GDP Growth Rate</i>		0.0604 (0.0747)	0.0653 (0.0784)			0.0898 (0.0753)
<i>% Change in Coalition Size * Lagged Deficit</i>			-0.00498* (0.00219)			-0.00466* (0.00200)
<i># of Portfolios</i>				-0.112 (0.206)	-0.112 (0.206)	-0.136 (0.201)
<i>% Change in Coalition Size * # of Portfolios</i>					-0.00117 (0.00202)	-0.000704 (0.00205)
<i>Country Fixed Effects</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Year Fixed Effects</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Constant</i>	30.21*** (0.134)	29.78*** (0.369)	29.94*** (0.391)	32.18*** (3.506)	32.22*** (3.526)	32.41*** (3.517)
<i>N</i>	418	400	400	412	412	395
<i>Root MSE</i>	3.068	2.852	2.843	3.056	3.059	2.835
<i>R2</i>	0.809	0.834	0.836	0.809	0.810	0.837
<i># of Clusters</i>	23	23	23	23	23	23

Clustered Standard errors in parentheses  
+ p < 0.10, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Notes: The above table presents a robustness test that recasts the main independent variable, Parties in Coalitions, as a relative measure. The models test the effect of the percent change in coalition size. This examines the idea that the effect of the marginal party is relative to the coalition size in previous years. All six models include country and year fixed effects, and standard errors are clustered at the country level. The dependent variable is total government spending as a percent of GDP. Model 1 predicts the unconditional effect of coalition size with no controls. Model 2 adds three macroeconomic controls: the deficit as a percentage of GDP, the inflation rate, and GDP growth rate. All macroeconomic controls are lagged one period. Models 4 through 6 test the hypothesis that cabinet structure conditions the effect of coalition size, including the number of ministerial portfolios as a control in model 4 and, in model 5 and 6, allowing for a heterogeneous effect of coalition size for different size cabinets. Model 3 and Model 6 correspond to the main regressions outlined in the empirics section. This table replicates the main result just with a new specification for the independent variable.

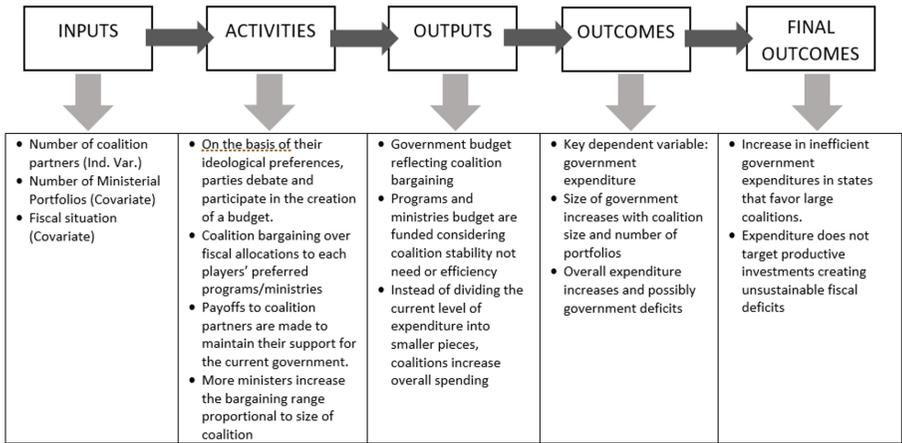
**Table 5. Robustness Test Excluding Perennially larger Governments**

	Mean Coalition Size < 4		Mean Coalition Size > 4	
	(1)	(2)	(3)	(4)
	<i>Total Government Spending</i>			
<i>Parties in Coalition</i>	-0.0222	4.247	0.460	3.762
	(0.775)	(4.335)	(0.482)	(2.838)
<i>Lagged Deficit</i>		-0.161		1.159
		(0.164)		(1.785)
<i># of Portfolios</i>		0.545		-0.187
		(0.468)		(0.928)
<i>Parties in Coalition * # of Portfolios</i>		-0.226		-0.106
		(0.234)		(0.125)
<i>Parties in Coalition * Lagged Deficit</i>		-0.104+		-0.245
		(0.0572)		(0.367)
<i>Lagged Inflation</i>		0.0304		0.146
		(0.125)		(0.299)
<i>Lagged GDP Growth</i>		0.107		-0.0952
		(0.0817)		(0.230)
<i>Country Fixed Effects</i>	Yes	Yes	Yes	Yes
<i>Year Fixed Effects</i>	Yes	Yes	Yes	Yes
<i>Constant</i>	38.04***	24.07*	45.03***	42.91+
	(1.933)	(9.006)	(1.956)	(16.85)
<i>N</i>	340	320	80	77
<i>Root MSE</i>	3.155	2.839	2.920	2.389
<i>R2</i>	0.791	0.830	0.778	0.877
<i># of Clusters</i>	19	19	4	4

Standard errors in parentheses  
+ p < 0.10, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

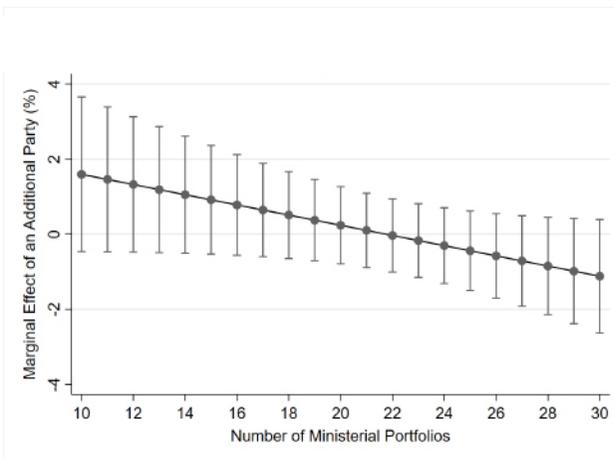
**Notes:** The above table presents robustness tests segmenting the data by average coalition size. All six models include country and year fixed effects. All standard errors are clustered at the country level. Models 1 and 2 exclude the four countries with average coalition sizes greater than 4 (in the 95th Percentile). The excluded countries are Belgium, Finland, Israel, and Italy. Models 3 and 4 consider these four states in isolation. Models 2 and 4 show the results of the main fully interacted regression with all the controls. Models 1 and 3 show the unconditional effect.

**Figure 1.** *Hypothesized Causal Chain*



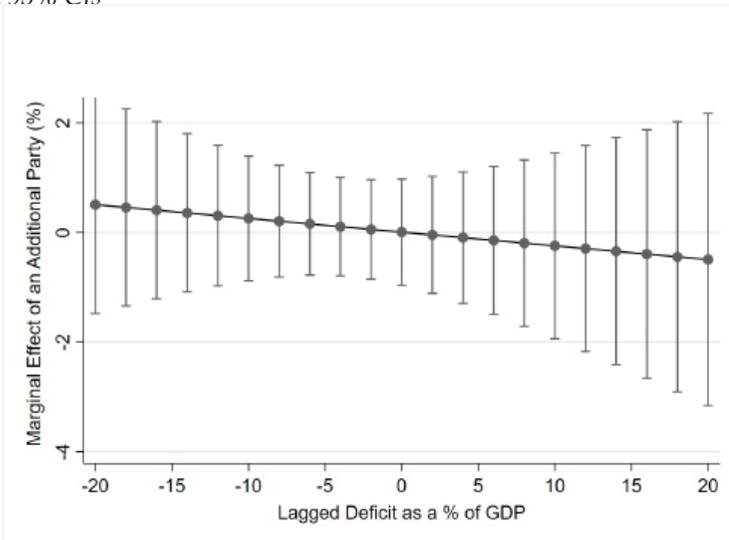
**Notes:** The above figure outlines the hypothesized causal chain underlying the study. It identifies the pathway by which coalition size is theorized to affect spending outcomes. It shows the steps through which coalition size changes government expenditure and how the increases could lead to long-run negative outcomes, like large fiscal deficits or spending misallocation.

**Figure 2.** *Average Marginal Effect of Coalition Size in Larger and Smaller Cabinets, with 95% CIs*



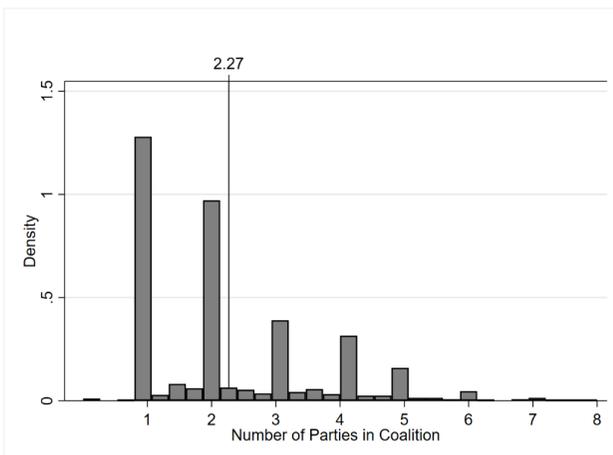
**Notes:** The above figure shows the marginal effect of coalition size as the number of government ministerial posts change. The marginal effect is calculated from main estimating equation two. We find that the effect of an additional party in coalition decreases as the number of ministerial portfolios increases although not significant at 95%.

**Figure 3.** Average Marginal Effect of Coalition Size under Varied Fiscal Constraints, with 95% CIs



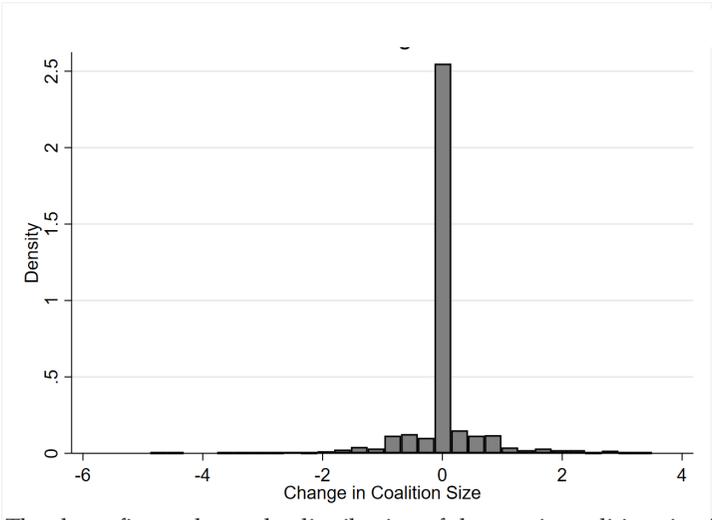
**Notes:** The above figure shows the marginal effect of coalition size as government deficit in the previous period increases. The marginal effect is calculated from main estimating equation two. We find little evidence of a heterogeneous effect of coalition size at different sizes of government coalitions.

**Figure 4.** Distribution of Coalition Size



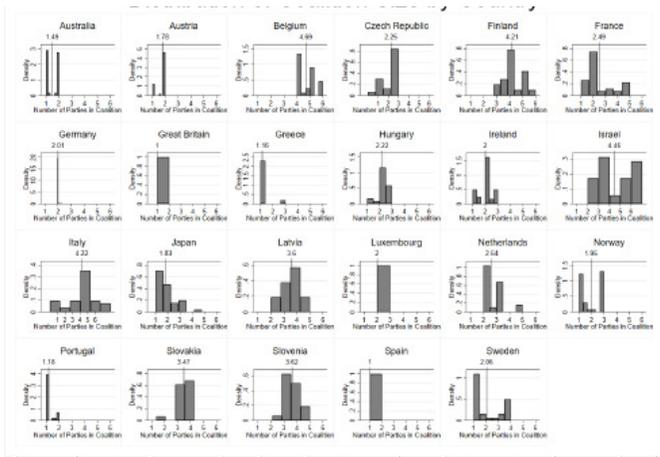
**Notes:** The above figure shows the distribution of coalition size across the entirety of the data. We find a mean coalition size of about 2 partners. We find that a majority of the coalitions have one or two partners and that the distribution has a very long right tail. The coalition size variable is the weighted average of all governments in a given country-year.

**Figure 5.** *Distribution of Change in Coalition Size*



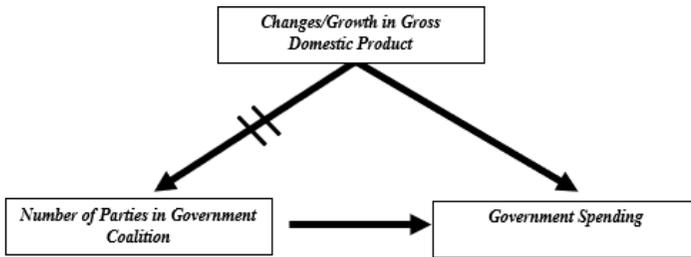
**Notes:** The above figure shows the distribution of changes in coalition size. The variable is calculated by subtracting the coalition size in each country-year from the previous year’s coalition size. In most years, there was no change in coalition size. This shows that any analysis of the effect coalition sizes are based on mostly marginal one-party changes.

**Figure 6.** *Distribution of Coalition Size by Country*



**Notes:** The above figure shows the distribution of coalition size for each of the 23 countries under consideration. It is apparent that for most countries coalition sizes are relatively mal-distributed. In many states, there is little variation in coalition sizes with many states having an average between one and two. In Great Britain, Spain, and Luxembourg, there is no variation in coalition size. The figure helps show which countries contribute the most variation in the data.

**Figure 7.** *Logic of Falsification Test*



**Notes:** The figure displays the logic of the falsification test. Because spending is closely linked to GDP, we test whether changes in coalition size are caused by counterfactual changes in GDP. The identifying assumption of our models is that coalition size is unrelated to counterfactual changes in GDP. In order to show that our identifying assumption is accurate, we must show that there is no effect of GDP on coalition size and that the effect of coalition size on spending is independent of GDP.



# Forced Labor in Tech Supply Chains

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This paper seeks to provide guidelines for tech companies to implement best practices for eliminating Modern Slavery from their supply chains by a) summarizing applicable regulation and current approaches to addressing forced labor, b) identifying the pressure points and the window of opportunity to act, and c) suggesting a path forward relying on investor pressure, risk mitigation, and consumer advocacy to promote improvements.

## **Problem Scope**

Modern Slavery (MS), the “recruitment, harboring, transportation, provision, or obtaining of a person for labor or services, through the use of force, fraud, or coercion for the purpose of subjection to involuntary servitude, peonage, debt bondage, or slavery” (Department of State 2018), is one of the fastest growing crimes and one of the most profitable ones (Jones and Winterdyk 2017). It is estimated to affect over 45 million people worldwide (Global Slavery Index 2016) and generates an estimated \$150 billion USD in illegal profits per year (ShareAction 2016).

MS involves the “recruitment, harboring, transportation, provision, or obtaining of a person for labor or services, through the use of force, fraud, or coercion for the purpose of subjection to involuntary servitude, peonage, debt bondage, or slavery” (Department of State 2018). Forced labor is part of the MS spectrum and is a violation of human rights. Companies that sell electronic goods often depend on supply chains that span the globe and involve mining, smelting, and assembling components. MS can occur in any of these phases

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and presents a legal, financial, and reputational risk that companies are now beginning to address, for instance in following the California Transparency in Supply Chains Act of 2010, which requires verification, audits, certification, and international accountability and trainings provided related to forced labor (State of California 2014). There are best practices available to tackle the issue. Tech companies should implement a detailed timeline for adopting them.

Although it is prohibited by national legislators in over 150 countries and has been addressed by multiple international protocols and OECD guidelines, prosecution remains low. In 2017, there were only 7,045 global convictions for human trafficking related crimes (Department of State 2018). If we consider that over 45 million people are affected, this number of convictions barely reaches 0.001% of the total number of cases.

The high prevalence of the crime, coupled with low enforcement, motivates novel approaches for decreasing its incidence and is partially why private actors have been increasingly called upon to take a proactive role in eliminating it. We do not believe that more reports and work recommending enforcement of anti-slavery laws is sufficient to promote effective change.

Given this lackluster response from a prosecutorial standpoint, some jurisdictions have chosen to focus on requiring companies to proactively disclose their own efforts for eliminating MS in their supply chain. Key examples of this approach are the UK Modern Slavery Act, France's Due Diligence in Human Rights Law, or the California Supply Chain Transparency Act. The idea behind these requirements is for the state to share the burden of policing with the general public: companies are obliged to disclose their policies, and thus an informed consumer could be in a position to whistle blow or exert pressure and alert regulators that there is something amiss.

It is in the interest of the company to comply with these regulations for the virtuous signaling that it gives consumers, non-profits and other stakeholders, and for risk mitigation purposes of avoiding a scandal in which their lack of compliance is brought to light. Nonetheless, even if these disclosure requirements are quite broad and general, they often remain unmet.

### **SDG's and OECD Guidelines**

Of particular relevance for MS is Sustainable Development Goal (SDG) 8 which promotes "sustained, inclusive, and sustainable economic growth, full and productive employment and decent work for all" (Global Forum on Responsible Business Conduct 2017). Having a stable, legal source of income provides individuals and their families with the means to satisfy their needs and materialize some complementary rights. Notwithstanding its importance, a recent analysis has found that the greatest gaps in achievement of the SDGs

is precisely in its economic, jobs, and gender elements (OECD 2017). We argue that part of this gap can be explained by the generic nature of the recommendations and the difficulties in measuring progress.

This level of abstraction has been partially remediated by the OECD which, particularly for mining, has established clear, detailed, actionable steps for due diligence procedures. After holding several rounds of consultations with industry leaders, governments from the Great Lakes Region (from which the majority of minerals for electronics come from), and civil society, the OECD defined a proactive on-going process for respecting human rights and avoiding contributing to armed conflict. For technology companies, the SDGs provide a general roadmap and the OECD Guidance provides concrete steps for moving closer in their direction.

Overall, MS is an immense problem that affects a significant proportion of the working population and affects their ability to exercise their rights. This has created a response that relies on international law conventions, local legislation, and more recently, disclosure requirements and more general frameworks that link the elimination of forced labor with the broader notion of sustainable development. The efficacy of these instruments has been insufficient to eliminate the problem and private actors are being called upon to play a more active role.

### **Private Brand Responses**

Over the past ten years, there has been a growing call for an increase in corporate involvement in curtailing this crime. This has to do with the fact that over 90% of forced labor is imposed by private actors and 71% of companies had “credible reason to believe modern slavery occurs at some stage in their supply chain” in 2015 (Assent 2019a).

Due diligence is now considered part of the regular costs of doing business and has strong links to risk mitigation at a financial, legal, reputational, and operational level (Assent 2019a). Some of the responses include strict codes of conduct for tier 1 -and increasingly tier 2- suppliers, third-party audits, and their own reporting mechanisms that allow them to share progress with stakeholders.

Although these private initiatives have been criticized for being designed to protect the brands and not the workers, our contention is that it is feasible to do both: you can protect the brand’s reputation and market value precisely by safeguarding the workers. Other companies have joined organizations or initiatives that provide industry-wide codes of conduct.

## Window of Opportunity for Corporate Change

Having described the magnitude of the problem and the ineffectiveness of relying solely on legal initiatives, we will now analyze how technology companies can be pressured to increase supply chain transparency and implement more effective policies for eliminating forced labor in tech's supply chain. Although this campaign focuses on external stakeholders, this does not exclude internal team members from accruing the buy-in necessary to drive change. We will first present four key general considerations for promoting organizational change and then go on to describe three specific campaign strategies.

First of all, it is important to consider timing. There has been an uptick in media coverage on sustainability issues and the average consumer has become increasingly aware of the potential negative impacts of the manufacturing products we use day to day. In 2012, 70% of consumers surveyed were willing to pay a premium of 5% for a sustainable product and less than 10% were willing to pay more than 25% on the premium (Miremedi, Musso, and Weihe 2012). In more far-reaching surveys in 2014, 50% of millennials answered they were willing to pay more for companies that show a commitment to positive social and environmental impact; by 2015, the number had reached 73% (Nielsen 2015). In other words, sustainability is now very much a part of the public agenda (Kingdon 1995) and is getting attention from media, policymakers, and consumers.

There is also an economic argument around timing: brands with commitments to sustainability have grown over 4% at the global level (Nielsen 2015). From a strategic standpoint, this is a good moment for a campaign to get tech companies to implement changes in its social sustainability policies: there is a focused topic, consumer awareness, media coverage, technological readiness, and a strong geopolitical climate (Sin 2018). It is important to frame all messages in such a way that highlights that it is in the interest of the company to meet these requirements and be a proactive player in this field.

Second, from a communications standpoint, it is important to avoid presenting Modern Slavery as a rampant issue that is present everywhere. Doing so generates the idea that it is somehow normalized to engage in these behaviors and decreases the agent's willingness to change (Cialdini 2007; Cialdini et al. 2006; Cialdini, Reno, and Kallgren 1990; Schultz et al. 2007).

Third, it is important to give companies clear examples of how they can achieve targets. In other words, it is important the campaign includes salient examples of how they can actively curtail MS and go beyond simply stating that they are (Kees Keizer, Lindenberg, and Steg 2013). Experiments have shown that explicit behaviors are more effective in guiding perception than

abstract messaging (K. Keizer, Lindenberg, and Steg 2008).

Last, for the campaign to be successful, it is crucial that we understand that decisionmakers within the company will need to believe that others are already doing what we are suggesting that they do, and more importantly, that those relevant others (competitors, consumers, regulators, etc.) expect them to do the same (McAdams 2001). This perception of others' expectations on our behaviors is a first step in generating an attitudinal change within the C-Suite that precedes an actual behavioral change. Some of the best practices for doing so include demonstrating that near peers have already adopted our proposed actions, and that others will perceive the behavior as being correct (Cialdini 2010). In order to do so, Rogers suggests that proponents for change focus on the relative advantage of the suggested change over the previous status quo (current exposure to risk vs. risk management), compatibility with values, low complexity involved (step by step guide and three prioritized audiences), and high observability of the change (results will be visible to stakeholders) (Rogers 2003).

## **Recommended Action Steps**

### **Key Goal 1:** *Conduct risk assessments*

The main objective of this line of the campaign is to get investors to pressure the target company to conduct human rights risk assessments that are verified by a third party. The audience for this action item is investors and the leverage for change originates in the need for risk mitigation to protect long-term asset value.

In order to meet the best practices established in this area, key findings (both positive and negative), should be publicly available. This step should include clear definitions of worker remediation practices in case a violation or non-compliance is found. As is the case with most risk management systems, remediation processes need to be defined before a problem occurs.

Implementing this policy into standard practice would allow the company to become a leader in the field, perform better than its peers in the industry, and prevent financial risk of potential losses in market value. It is also interconnected with reducing regulatory and reputational risk. As previously noted, senior management needs to be involved throughout the risk assessment process and be aware of the particularities of the industry and countries that are part of its supply chains. Fortunately, there are already several tools available for identifying risks related to slavery. One of them is the Slavery and Trafficking Risk Template which provides a detailed questionnaire and tallies points depending on the industry and country (Assent 2019b). Use of this template would be a good first step.

Other tools are available through third party consulting firms or government agencies: the U.S. Department of Labor has an eight-step, free tool for labor compliance in supply chains that is designed for companies that want to begin to implement a more robust management system (U.S. Department of Labor 2019).

### **Audience for Key Goal 1: *Shareholders***

Investors are a key ally for a corporate change campaign because they have a vested interest in the performance of the company and have a common goal in minimizing risk.

More investors are now raising the issue of forced labor in shareholder meetings and demanding compliance with new regulation (ShareAction 2016). These active investors seek to implement governance changes within the company that they hold stock in. To do so, they rely on proxy voting, voicing concerns at annual shareholder meetings, nominating directors, and voting on board compensation. They include not only large, institutional investors such as pension funds, but also impact funds, traditional hedge funds, and individual investors (Larcker and Tayan 2015).

For investors, human rights violations can be seen through the lens of decreased productivity of their assets due to delays in the supply chain, penalties, or legal investigations. Investors will try to minimize these impacts on future revenue streams and normal company operations. A hit to the company's reputation can also significantly affect the market capitalization of a company. This is especially important for consumer facing firms that face higher scrutiny and may be targeted by investigative reporting on their supply chains. The growing field of "impact investing" means that investment decisions will now be subject to more comprehensive risk assessments that evaluate not only traditional returns, but also human rights monitoring and allegations of violations. A company's sustained economic growth in the stock market now requires disclosure of human rights conditions across its supply chains and it is likely that these requirements will expand and grow more stringent.

Second, specific jurisdictions have begun to include social assessment data for publicly traded companies. For example, the EU Parliament introduced reporting requirements of non-financial information (including human rights and environmental stewardship) for companies with over 500 employees. A 2015 analysis found that 38 of the largest 50 economies have some form of government corporate disclosure requirement on environmental and social issues; for some, the disclosures are optional and for others they are mandatory, varying by country. Countries with mandatory disclosures tend to fare better on ESG ratings than those with voluntary

disclosures (Principles for Responsible Investment 2016). This could be a strong indication for future regulatory developments to implement a policy that is, at least nominally, obligatory instead of optional.

Some stock exchanges, such as the Shanghai Stock Exchange or the Hong Kong Stock Exchange also require listed companies to disclose relevant social sustainability information (BSD Consulting 2016). By 2015, at least 26 stock exchanges have ESG reporting guidelines, many of which are voluntary or comply-or-explain (Principles for Responsible Investment 2016). Many countries have both stock-led disclosure guidelines and government-led guidelines (Hauser Institute for Civil Society 2014). Recognizing that voluntary guidelines are only the first step forward, over 100 institutional investors petitioned the International Organization of Securities Commission to integrate specific sustainability disclosure requirements in listing rules (Lubber 2015). What is still lagging is enforcement, as no evidence has been found of a global company being delisted from a stock exchange for non-compliance to ESG's disclosure guidelines.

Institutional investors are especially relevant because they are better equipped to drive change within a company. For example, rule 14a-8 in the U.S. allows them to nominate people to the company's board of directors. They can also rely on proxy voting to push for change.

There is growing scrutiny on the behavior of private actors which may affect stock prices. Supply chain disruptions have been found to be linked to a 9% drop in stock prices (Ernst & Young 2018). Forward-looking investors are aware of this and should be approached to support the primary goal of this line of action (Ruggie 2019). There are several active campaigns in place addressing wages and forced labor on the supply chain. Some of them are pressuring upcoming shareholder meetings to vote on these issues and publicly commit to a timeline for implementation of improvements (Clean Clothes Campaign 2019).

## **Key Goal 2:** *Blockchain technology for transparency*

The key goal of this line of action is to implement blockchain technology to meet best practices in two themes: transparency in mineral sourcing (as Billiton already does for mineral analysis (Ernst & Young 2018)), and worker grievances reporting (RCS Global 2017). The captive audience for this line of the campaign would be regulators.

By implementing this technology, the target company will also be able to circumvent a recurring critique of the current auditing system which relies on overworked and often poorly trained individuals to collect data that is frequently found to be inaccurate and not trustworthy. From a technical

standpoint, what we frequently refer to as blockchain is really a digital ledger of transactions that is not stored in a single place or system and can never be deleted. Rather, it is stored across various systems owned by different players that can each validate the information without an intermediary (McQuinn and Castro 2019). This makes it nearly impossible for any single actor to corrupt the ledger. An actor has access to the ledger only if its digital signature matches that of the consensus algorithm of the network (Deloitte 2017).

From a social/political standpoint, by eliminating an intermediary, blockchain allows groups of individuals to cooperate and reach a consensus even if they do not know each other or do not trust each other (McQuinn and Castro 2019). Nobody can alter the record once it is made, and all players have the same access, all the time. This element makes blockchain application to supply chains ideal: parties do not need to know each other, can have competing interests or incentives, and yet all rely on the same information system to record their transactions.

One of the core advantages of blockchain is the outstanding level of trust it provides: its records are transparent, secure, and auditable (Deloitte 2017). Additionally, the ledgers are automatically updated, and all records are immutable. If we bring together blockchain technology with Internet of Things (IoT), we will be able to collect and aggregate data in a way that is timely, reliable, and at a much broader scale than ever before.

That being said, blockchain is just now beginning to be used at this scale and there are still unanswered questions: what is the best way to collect information that is fed onto the platform? Note that like any technological system, blockchain cannot fix bad data input. How can we create an untampered link between the physical and digital world? At which scale should the token be linked (1 oz of gold? 1 shipment of gold)? How should the data be visualized by the consumer? How can this virtual system be integrated with traditional paper-based supply chain systems? And crucially, how do we engage all parts of the supply chain with a technology that requires harmonized standards?

Recognizing these latent questions, Deloitte recommends that companies begin by testing out blockchain in their biggest pain points: where would blockchain be of most value? We suggest tagging raw materials from point of sale to smelter as a first step in minimizing contamination with mine sites that may rely on forced labor. Future developments could include incorporating smart contracts (contracts that are automatically enforced when the conditions are met, i.e. the price for the mineral goes up if it is above x% purity) into the blockchain system.

From a policy standpoint, there are also several unanswered questions including the neutrality of the technology, the stability and certainty of its use, and international harmonization, among others (McQuinn and Castro 2019). Last, from a purely technical standpoint, blockchain technology still requires incredible amounts of computing power and energy consumption to operate.

### **Audience for Key Goal 2: Regulators - Legal Risk**

Litigation has already begun to affect companies in other industries across the globe and legal recourse has been sought by former employees, consumers, and investors (Mucha 2019). In the UK, legal action has been sought against corporate directors which may be held personally liable for forced labor in their supply chains (Taylor 2019). As was previously stated, tech companies have a complex, non-centralized supply chain that depends on a variety of information networks and for which there is low visibility of the entire process (Deloitte 2017). Currently, each supplier operates a different information system that hinders accurate tracking and opens up space for fraud. Moreover, strong hierarchical dynamics in both mine sites and assembly lines make it difficult to accurately monitor working conditions. Blockchain technology can play a part in improving traceability and grievance mechanisms.

Even though current reporting requirements are primarily geared towards large corporations, small organizations are also subject to legal action and prosecution. There is a growing trend for lawsuits from survivors of human trafficking, employees, and even consumers to seek legal recourse in different forums, including the US and Europe (Malo 2017; Vendenberg and Grono 2016). Even though not all of these lawsuits have resulted in fines or criminal actions against the defendant firms, their appearance and growing frequency should serve as a warning sign for non-compliant companies. For example, a 2015 forced labor case led to a \$14 million USD fine on Signal International LLC, a maritime shipping company from Alabama that forced it to close its doors (Brickley 2015; Desai 2015). In the U.S., by 2008 there were at least three other cases of criminal prosecution of forced labor in the restaurant business and in American Samoa that have led to millions of dollars of financial restitution for the victims and sometimes prison for the executives (Southern Poverty Law Center 2008).

Private codes of conduct and cooperative agreements with industry peers can serve a role in preventing more stringent regulation that would set a higher bar. For example, Canada has already created a specific civil service role for a government official to ensure Canadian firms operating abroad follow human rights guidelines (Government of Canada 2019).

### **Key Goal 3: Publish Supplier Lists & Join Relevant Organizations**

The key goal of this action line of the campaign is to get technology companies to publish their global supply lists and join relevant organizations that have shown a credible commitment to eradicating forced labor from the electronics industry. To meet best practices standards in this theme, supplier lists must be published up to at least the second tier and should include names, addresses and contact information of all suppliers, smelters and refiners (Know the Chain 2018). To be a leader in traceability, the company may also choose to collect information and disclose the percentage of migrant workers that participates in each of its facilities. This allows for a better understanding of the proportion of the workers that belong to one of the most vulnerable demographics to forced labor.

Although non-profits are not identified as being the key audience of any of these campaigns, they play a relevant role in setting the agenda for investors, regulators, and consumers. Activist organizations are also able to agitate for better standards, stimulate consumers to take action and pressure government to enforce labor regulations (Elliott and Freeman 2003). Furthermore, if credible, they are able to connect with consumers through shared values and transparent information gathering (P. A. Gourevitch, Lake, and Stein 2012) reimbursement to workers for excessive recruitment fees.

Although non-profits are not identified as being the key audience of any of these campaigns, they play a relevant role in setting the agenda for investors, regulators, and consumers. Activist organizations are also able to agitate for better standards, stimulate consumers to take action and pressure government to enforce labor regulations (Elliott and Freeman 2003). Furthermore, if credible, they are able to connect with consumers through shared values and transparent information gathering (P. A. Gourevitch, Lake, and Stein 2012).

### **Audience for Key Goal 3: Consumers - Reputational Risk**

There is growing consumer awareness and importance allocated to sustainability matters. Customers are considered to be primary stakeholders, or those without whom the business itself would not exist. A review of 77 manufacturing firms found that consumer pressure does generate change in sustainable processing management, including supply decisions (Gualandris and Kalchschmidt 2014). Another reached similar conclusions: stakeholder pressure can lead to increased sustainability awareness within a company, clearly stating goals related to sustainability, and to implementation of specific sustainability practices (Luoma and Meixell 2015).

Several supply chain scandals have rocked the technology and electronics

sector in the past decade. One event that remains salient for many were the suicides of 14 confirmed (18 alleged) Foxconn employees in 2010 (Wow and Reply 2017). As a supplier for Apple, HP, Dell, GoPro, etc. with over 1.3 million employees (Merchant 2017), issues within Foxconn are singularly poised to have effects for several actors around the world (Bapna 2012). Consumers organized alongside nonprofits and the media (Barboza 2010; Berg 2018; Hefferman 2013; McLaughlin 2010; Merchant 2017) to bring on quick responses from the brands involved and Foxconn itself. For example, the day after the scandal broke, Foxconn required all employees to sign a release of liability for death; after media outcry, the letter was withdrawn (BBC News 2010). Furthermore, within a week the company had doubled its salaries for most of its Chinese workers (Barboza 2010). Shortly after, Apple and HP announced strict policies for worker wellbeing in their supply chains; this was at least partially motivated by repeated calls to boycott the brands both in China and abroad (McLaughlin 2010). This is a short, concrete example of the power of consumer and media pressure related to supply chain problems on a private actor.

Consumers are the third leg of the triad on which this campaign stands. They have incredible leverage to change corporate behavior, especially when they are organized around a central topic such as forced labor. Although maybe forced labor does not seem like a salient issue in the immediate environment of most western consumers, it is important to remember that they are the end user of many of the items that are produced with forced labor at some point or another. For example, G20 nations import over \$354 billion worth of at-risk products per year (Dahir 2018). This can be used as the prime motivating factor to mobilize consumers around this issue.

## **Conclusion**

Forced labor is a significant problem in the ICT supply chain from mineral extraction to assembly and companies are beginning to respond. Tech companies can manage financial, legal and reputational risk by implementing best practices in conducting risk assessments, using blockchain technology, joining relevant organizational and publishing supplier lists. These goals can be met by three key audiences: investors, regulators, and consumers.

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# Maritime Interdiction against the DPRK's Illicit Maritime Practices

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Despite the harsh sanctions imposed on the Democratic People's Republic of Korea (DPRK), the country renders the sanctions futile by facilitating various illegal trades such as the ship-to-ship transfers of petroleum or coal. (United Nations S/2019/171, 4) Recently, the international community began paying attention to this matter. For instance, the U.S. Coast Guard Cutter *Bertholf* has been dispatched to East Asia to search vessels suspicious of conducting illegal trades with the DPRK. (Fuentes 2019) Additionally, the trilateral (ROK – U.S. – Japan) defense ministerial meeting underscored the importance of international cooperation to “deter, disrupt, and ultimately eliminate North Korea's illicit ship-to-ship transfers.” (Department of Defense of the United States 2019).

Since the first nuclear crisis in 1993, both engagement and sanctions have failed to make the DPRK abandon its ambitions of becoming a nuclear state. This paper is premised on the author's perception that the DPRK's sanction evasion is a serious problem that the international community should tackle. The DPRK's nuclear weapons should be de-nuclearized through diplomacy and dialogue. However, as long as the DPRK can continue to evade sanctions, it will be very hard for the international community to achieve meaningful compromise with the DPRK. This paper examines the legal and strategic discussion surrounding maritime interdiction of the DPRK's illegal activities in order to find the strategically best policy option.

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## Background

The United States seized the DPRK cargo vessel “the Wise Honest” on May 9, 2019. The judiciary authorities in the United States filed a civil forfeiture complaint against the Wise Honest. It was the first seizure of a DPRK vessel for violating the sanctions imposed by the UN Security Council (U.S. Department of Justice 2019). The Indonesian Navy detained the Wise Honest in April 2018 for carrying coal worth three million dollars. The DPRK was banned from exporting coal by the United Nation Security Council Resolution 2371 in August of 2017. The case of the Wise Honest implies that the DPRK had extensively evaded and undermined the maritime sanctions (Whalen 2019).

Despite the harsh sanctions imposed on the DPRK, it is still able to procure enough energy supplies through smuggling for its economy to stay afloat (Silberstein 2019, 10). Although the UN Security Council capped the export of petroleum and crude oil to the DPRK, the country keeps importing them by facilitating the banned ship-to-ship transfer of petroleum products and crude oil (United Nations S/2019/171, 4). Between June 2nd and August 9th of 2018, six vessels of the DPRK were spotted transferring prohibited items from unknown small ships in the East China Sea (United Nations S/2019/171, 14). The Office of Foreign Asset Control of the United States estimated that DPRK ports had received at least 263 deliveries of refined petroleum from its tankers. If the tankers loaded refined petroleum up to 50 percent of their full capacity, they might deliver 1.89 million barrels, which is approximately four times larger than the amount capped by the UNSCR 2397. The advisory updated the list of 28 DPRK tankers capable of conducting ship-to-ship transfers and 18 vessels were observed to have transferred refined petroleum to the DPRK tankers (North Korean Sanctions Advisory 2019, 1-2).

The DPRK also avoided the prohibition of coal export by mobilizing at least 33 vessels capable of carrying coal (North Korean Sanctions Advisory 2019, 2). Although the import of coal from the DPRK was banned in 2016 and again in 2017, by 2018, the fleet of DPRK vessels was still able to carry coal by using various tactics to conceal their routes and misrepresent origin of coal. They took indirect routes, detoured, loitered, forged documentation, trans-shipped through third party countries, and altered identification and navigation in the Automatic Identification System (AIS) (United Nations S/2018/171, 23). Related data is too limited to estimate the coal export precisely. Nevertheless, the DPRK might have exported a considerable amount in 2018. If the DPRK had employed all of its 33 vessels to deliver coal worth three million dollars, as the Wise Honest did, they could have exported 8.3 percent (US\$ 99 million) of the amount it had exported to China in 2017 by one delivery per vessel (Table 1).

DPRK ships have typically relied on four illegal methods to evade sanction enforcement. They conceal the ship's identity, origin and destination, and type of cargo by: (1) disabling or manipulating Automatic Identification System (AIS): AIS transmits a vessel's identification, as well as navigational and positional data through VHF radio waves. The DPRK ships illegally switch off their AIS signal or transmit false information about their identifications (name, International Maritime Organization (IMO) number, and Maritime Mobile Service Identity (MMSI)), destination, and type of cargo in order to avoid being detected. (2) forging vessel identification physically: DPRK ships obfuscated their identification by painting a false name and IMO number (a permanent identification regardless of a change in ownership or name of the ship) on their hulls. (3) ship-to-ship transfers: the DPRK can fabricate the origin or destination of the items by transferring items such as petroleum or coal to and from non-sanctioned ships in the high seas (See Figure 1). (4) falsifying cargo and vessel documents: the DPRK forge shipping and transactional documents to misinform inspectors (North Korean Sanctions Advisory 2019, 2-4).

These ghost ships have been operated beyond their average lifespan, meaning they usually score poorly in "Port State Control safety inspections" or get penalties for violating regulations by port states (United Nations S/2019/171, 15-16). Since January 2017, the flagged DPRK vessels have been found to have 10.43 deficiencies on average during port inspections, which is 2.57 more deficiencies than other countries' vessels. Moreover, DPRK ships have been detained by port authorities three times more than other countries'

### **Current Maritime Sanctions upon the DPRK**

The international community has tightened the regulations of maritime interdiction against the DPRK in response to its nuclear and ballistic missile programs. However, the DPRK has undermined the efficacy of the sanctions by using the illegal maritime practices discussed earlier. Nonetheless, understanding the current maritime sanctions is necessary for ascertaining the limitations of the existing systems. In this regard, the Proliferation Security Initiative (PSI) and the United Nations Security Council Resolutions (UNSCRs) must be deliberated to understand the existing limitations.

#### *(1) The Proliferation Security Initiative*

The United States initiated the Proliferation Security Initiative (PSI) in 2003 as an effort to halt the proliferation of weapons of mass destruction (WMD). The PSI is not a legally binding treaty but a nonbinding political pledge. As of June 2019, the PSI is a multi-lateral activity to which 107 countries have joined including the Republic of Korea, Japan, Singapore, and Australia. John Bolton, the Undersecretary of State for Arms Control and International Security of

the United States stated during an interview in 2003, that the United States introduced the PSI to deal with the trafficking of all WMD-related materials more effectively. Interestingly, Bolton emphasized that the PSI is not a blockade against any specific state like the DPRK or Iran, but a global response to a worldwide problem. He asserted that his team never considered involving activities in the initiative other than barring traffickers (Arms Control Association 2003).

Since the PSI was created in 2003, it has successfully promoted cooperation on interdiction in three ways. First, the PSI has formed common ground among like-minded countries. The PSI induces like-minded countries to cooperate because it is a non-binding political pledge. For instance, the Operational Expert Group (OEG), which consists of 21 states, reached a consensus about the interdiction principles of the PSI in 2018. They agreed that the partner countries shall cooperate to (1) “take effective measures for interdicting the transfer or transport of WMD,” (2) “adopt streamlined procedures for rapid exchange of relevant information concerning suspected proliferation activity,” (3) “review and work to strengthen their relevant national legal authorities,” and (4) “take specific actions in support of interdiction efforts regarding cargoes of WMD” (Federal Foreign Office of Germany 2018). This principle reiterated the same principle the Operational Expert Group reached in 2003 (U.S Department of State 2003).

Second, it has systematized cooperation through regular exercises. Following the Asia-Pacific Exercise Rotation (APER), the United States (Fortune Guard in 2014), New Zealand (Maru in 2015), Singapore (Deep Sabre in 2016), Australia (Pacific Protector in 2017), and Japan (Pacific Shield 18 in 2018) hosted annual PSI exercises. The PSI-endorsing partners conducted table/port/live exercises, capacity building programs, and academic sessions during the trainings. They enhanced the coordinated capabilities and expanded the understanding of the PSI’s objectives and activities through the exercises (Ministry of Foreign Affairs of Japan 2018). Additionally, the Republic of Korea hosted an exercise called “Eastern Endeavour” during the 9th through the 12th of July in 2019. However, this round of exercises did not include live maritime interdiction activities (Korea News Gazette 2019).

Third, the PSI has attempted to expand its realm of cooperation from conducting mere anti-proliferation activities to gaining tools for sanctioning commercial trade. According to the “Joint Statement from Proliferation Security Initiative (PSI) Partners in Support of United Nations Security Council Resolutions 2375 and 2397 Enforcement,” the PSI partners pledged to “redouble efforts to implement in full the measures in relevant UN Security Council Resolutions with respect to inspecting, detecting, and seizing items

1. A flag state is a state in which a vessel is registered.

the transfer of which is prohibited by those resolutions” (Federal Foreign Office of Germany 2018).

However, the PSI is too limited to be employed as a sole policy solution for the DPRK's illicit maritime activities. The PSI is legally nonbinding, and it has failed to impose actual obligations on its member states. Furthermore, it has not granted a legitimate authority for enforcing the inspection of foreign vessels (Belcher 2011, 6). The United Nations Convention on the Law of the Sea (UNCLOS), the widely accepted customary international law, has guaranteed the right of free and innocent passage. The UNCLOS does not admit that applying the state's jurisdiction to foreign ships onboard is lawful other than a few exceptions (UNCLOS 1982, 33~34). John Bolton also admitted that the PSI was legally limited when he carried out the initiative in 2003. Bolton judged that amending the UNCLOS or issuing a new UNSCR was required to overcome the legal limitations of the PSI (Arms Control Association 2003). Additionally, the PSI is not able to obligate its members to interdict DPRK vessels because it can only make a non-compliant member pay reputational and political costs. The initiative might not be able to force its members to comply with their commitments if attention from political leaders or high-level officials is diluted (Belcher 2011, 15).

## *(2) The United Nations Security Council Resolutions*

As direct responses to the nuclear tests and ballistic missile launches by the DPRK in 2016-2017, the permanent members of the UN Security Council passed several resolutions against the DPRK (R2270, R2276, R2321, R2345, R2356, R2371, R2375, R2407). The measures included in the resolutions, starting from the UNSCR 2270, are distinct from past measures in that the commercial trades were penalized (**Table 3**) for the first time. The past measures had not directly banned the DPRK from transacting general merchandises (Haggard and Noland 2017, chap.3).

UNSCR 2270 introduced new provisions of the maritime interdiction to support the commercial trade ban in 2016. After the DPRK's fourth nuclear test on January 6th, 2016, the UNSCR 2270 required all UN member states to “inspect the cargo within or transiting through their territory, that has originated in the DPRK, or that is destined for the DPRK, or has been brokered or facilitated by the DPRK or its nationals, or by individuals or entities acting on their behalf” (S/RES/2270 2016, 5). It also prohibited “owning, leasing, operating, chartering, or providing vessel classification, certification or associated service and insurance or re-insurance, to any DPRK-flagged, -owned, -controlled, or -operated vessel” (S/RES/2270 2016, 5). The DPRK's fifth nuclear and ballistic missile tests caused the Security Council to pass Resolution 2321 in November 2016. The resolution mandated that all member states “de-register any vessel that is owned, controlled, or operated by the

DPRK” (S/RES/2321 2016, 5).

As the result of the DPRK’s sixth nuclear test on September 2, 2017, UNSCR 2375 advanced the maritime interdiction by inaugurating the section “Maritime Interdiction of Cargo Vessel.” It required all member states (1) “to inspect vessels with the consent of the flag State<sup>1</sup>, on the high seas, if they have information that provides reasonable grounds to believe that the cargo of such vessels contains items the supply, sale, transfer or export of which is prohibited by resolutions.” (2) If the flag state is not willing to give consent to an inspector, the resolution “decides that the flag State shall direct the vessel to proceed to an appropriate and convenient port for the required inspection by the local authorities.” (3) If the vessel does not follow an order given by local authorities, “the flag State shall immediately deregister that vessel provided that such designation has been made by the Committee.” Furthermore, the resolution banned “facilitating or engaging in ship-ship transfers to or from DPRK-flagged vessels of any goods or times that are being supplied, sold, or transferred to or from the DPRK” (S/RES/2375 2017, 3).

These maritime regulations have not only clarified which actions are the actual obligations for a member state but granted a state necessary authority for the inspection of DPRK-related vessels. If a member state has reasonable evidence that a vessel is carrying the prohibited items, it is mandatory for a state to seize, inspect, and freeze the vessel within its ports. If the vessel is in its territorial waters, a member state has discretion to decide whether to seize, inspect, and freeze the vessel. A member state must seek consent from a flag state to inspect the vessel in the high seas. However, if the flag state does not consent to an inspection, the member state must direct the vessel to a designated port for inspection or the member state must immediately deregister the vessel (Table 4).

In contrast, the UNSCRs are limited, in that they are not capable of overcoming the difficulties of tracking ships and securing evidence for inspection. The DPRK has evaded the resolutions by exploiting these limitations. Although the resolutions have bridged the legal gaps which the PSI had left open, they fall short of forming a concrete scheme for the effective interdiction of DPRK vessels. Simply speaking, it is fruitless to make a new law without a concrete enforcement plan.

Finally, it is noteworthy that the UN Security Council carefully drew the line between the interdiction provisions against the DPRK and the customary international maritime laws. The UNSCRs affirmed that the rules of maritime interdiction (1) are applied only to the DPRK-related vessels. The resolutions (2) “shall not be considered as establishing customary international law.” Therefore, they (3) “shall not affect the rights, obligations, or responsibilities of Member States under customary international law, including the United

Nations Convention on the Law of the Sea" (S/RES/2375 2017, 3). In other words, all the legal discussions above are confined and applied only to the DPRK.

### **The Legitimacy of Maritime Interdiction**

The DPRK has exploited the loopholes in the sanctions. Therefore, additional measures for overcoming these constraints are necessary to prevent evasion by the DPRK. Concerning these needs, it is worthwhile to review the legitimacy of maritime interdiction in the two controversial cases: (1) a de-facto maritime blockade in peacetime and (2) a reinforced search and inspection of foreign vessels.

#### *A De-Facto Maritime Blockade in Peacetime*

It is widely accepted that a maritime blockade is an act of war. Therefore, undertaking a blockade as a method of coercing opponents into agreeing to certain political objectives during peacetime or a pre-war period is not allowed in international law (Lee 2001, 179). Nonetheless, one might argue that there is an exception to this principle. Article 42 of the UN Charter grants the United Nation Security Council the authority to take an action, including a blockade "by air, sea, or land forces as may be necessary to maintain or restore international peace and security" (United Nations 1945, 9). For instance, the United Nations Security Council blockaded Iraq through Resolution 665 (25 August 1990), which called upon member states to deploy their naval forces to the Persian Gulf to "halt all inward and outward maritime shipping, to inspect their cargoes and destinations" (UNSCR 665 1990, 22).

However, the Iraq case does not legally warrant the UN security council to implement the blockade against the DPRK. First, the approval of China and Russia is necessary to pass the new UNSCRs, but it is doubtful that they would agree to any blockade against the DPRK. Second, the Iraq case was not so much an activity to de-escalate the crisis during the pre-war period as it was an act of war to create favorable conditions for the upcoming operations by ground forces.

The formal agreements between the United States and the DPRK as well as the Republic of Korea and the DPRK are also constraining factors to enforce the maritime blockade. First, at the end of the Korean war in July of 1953, the United States signed the Korean War Armistice Agreement with the DPRK and China. Article 15 of the agreement restricts the blockade by stating that "this Armistice Agreement shall apply to all opposing naval forces and shall not engage in a blockade of any kind of Korea" (Korean War Armistice Agreement 1953). Second, under the Agreement on the Implementation of the Historic Panmunjom Declaration in the Military Domain on September 9th, 2018, the Republic of Korea agreed with the DPRK to resolve any problem that

might induce military conflict peacefully, and without use military force in any case, and to consult the matters of blockade, interdiction, and obstruction of navigation through the Inter-Korean Joint Military Committee (Agreement on the Implementation of the Historic Panmunjom Declaration in the Military Domain 2018). These agreements would render any effort to blockade the DPRK during peacetime by the United States or the Republic of Korea as self-contradictory.

### *A Reinforced Search and Inspection of Foreign Vessels*

The UNSCRs have made the enforcement of maritime inspection of foreign vessels easier, conditional on consent of a flag state. Nonetheless, the North Korea Sanctions Advisory of the United States admits that it is still up to a state's discretion to decide whether to inspect foreign vessels or to cooperate with the requesting state. Because the jurisdiction of a state operates more effectively within its territory than in the territorial water, such inspections are more compulsory in the ports than in open water (See **Table 4**). The UNSCR 2397 mandates that "the Member States shall seize, inspect, and freeze [impound] any vessel in their ports." However, it softens its rhetoric regarding the vessels on territorial waters by stipulating, "the Member States may seize, inspect, and freeze (impound) any vessel." (S/RES/2397 2017, 4) Therefore, preferences of a states' leaders or law enforcement agencies are crucial factors for the legitimate inspection of foreign vessels.

Even if a state is granted legitimate authority and is willing to interdict foreign vessels, every inspection requires reasonable grounds to believe that the suspected vessels are involved in the prohibited activities or are carrying the banned items. It is likely that the DPRK vessels would not cooperate with local inspectors, so the inspection would entail the use of force. However, the use of force can be regarded as a hostile act against the foreign vessel, if the law enforcing agents do not have clear evidence against the foreign vessel (Do 2011, 30~31). This complicates the interdiction process, as the DPRK has tried to fake identification, origin, or destination, in order to mislead maritime inspectors causing illegal vessels to go unnoticed. Thus, there must be guidelines to determine what information is evidence enough to allow the legitimate inspection of foreign ships.

The current acceptable forms of evidence for allowing the legitimate inspection of ships are as follows. First, the inspection of foreign vessels is justified if a state has information that the flag of the vessel is the DPRK or the

2. A flag of convenience is registering a vessel in a state different from a nationality of vessel's owners.

3. Double Flagging is unlawful act of registering a vessel in two or more nationalities or misrepresenting nationalities.

vessel is destined to or has originated from the DPRK, and if a state inspects the ship within its territorial waters. The UNSCR 2270 states that all member states “shall inspect the cargo within or transiting through their territory, including in their airports, seaports and free trade zones, that is being transported on DPRK flagged aircraft or maritime vessel, that has originated in the DPRK, or that is destined for the DPRK” (S/RES/2270 2016, 5).

Second, any evidence indicating the flag of convenience<sup>2</sup> or double-flagging<sup>3</sup> justifies an inspection. The UNSCRs have tried to deprive DPRK vessels involved in the prohibited activities of a nationality as well as legal protections provided by flag states. A vessel without a nationality must be inspected by a warship or other ship and aircraft in government service with visitation rights, even if the vessel is transiting in the high seas (UNCLOS 1982, 63). The UNSCR 2375 stipulates that “all Member States shall de-register any vessel that is owned, controlled, or operated by the DPRK” (S/RES/2321 2016, 5). Additionally, the UNSCR 2397 regulates that “each member state shall de-register any vessel it has reasonable grounds to believe was involved in activities, or the transport of items, prohibited by resolutions” (S/RES/2397 2017, 5).

Third, any sign of the illicit patterns that DPRK vessels have typically used can be justification for the inspection. To be specific, any vessel navigating with its AIS switched off or transmitting false identification signal should be carefully watched. Additionally, if the required information about the registry, pollution prevention, manning, or safety – such as the Continuous Synopsis Record – is found to be inconsistent or inadequately managed in the Port State Control (PSC) inspection, an additional inspection can be justified (North Korean Sanctions Advisory 2019, 11).

## **Strategic approach toward the DPRK'S Illicit Maritime Practices**

A balanced strategy with little risk can be achieved by identifying the desired objectives, selecting the best method, and allocating resources for the selected methods (Eikmeier 2007, 63). However, the strategic approach should not be reduced to an exercise of allocating resources, instead it should be assessed with critical thinking. Thus, the maritime interdiction should create advantage, generate a new source of power, and exploit weaknesses in the opponent (Meiser 2017, 81~82).

### *(1) Identifying the Desired Objectives*

Both mission and effects-based planning are required to set the desired objective. As a mission becomes more ambitious, it would require ways and

means with stronger effects. Inversely, a preferable end-state depends on the feasible ways and the available means (Eikmeier 2007, 64). The most ambitious end-state of the maritime interdiction against the DPRK's sanction evasion is the complete, verifiable and irreversible dismantlement (CVID) of its nuclear arsenal. The U.S. has affirmed that the sanctions upon the DPRK will only be lifted after the DPRK dismantles its nuclear program through complete, verifiable, and reversible means. The CVID demands very coercive ways and means. On the other hand, there are less ambitious and more realistic end-states that would require fewer coercive ways and means of maritime interdiction. For example, there is the on-going discussion called "Denuclearization Lite," which argues that the Trump administration should moderate its position on the CVID in order to compromise with reality because the DPRK will never accept the CVID. According to this plan, the U.S. might be able to allow the DPRK to keep a few nuclear warheads and missiles in a few locations under permanent observation by international inspectors after thoroughly verifying the DPRK's active nuclear and missile programs (Stavridis 2019).

Trade data is necessary to understand the relationship between the sanction against the DPRK, maritime interdiction, and the de-nuclearization dialogue. Although the most coercive interdiction can be expected to curtail a certain amount of DPRK's trade by eliminating its sanction evasion, the actual data would be elusive if the DPRK conducted a massive amount of unofficial trades. UNSCRs 2270, 2321, 2371, 2375, and 2397 have prohibited 59% of DPRK exports to China in 2016 (See Figure 2), causing export volumes to plummet by 95% in 2018 (See Figure 3). This result is hardly credible because the 2018 export volumes must have dropped by 59% even if all states, including the DPRK, observed the sanctions completely. There is clear evidence that the sanctions have curtailed a vast majority of seaborne trade given that the proportion of imports Jilin and Liaoning Provinces', the two provinces closest by land to the DPRK, contribute to total Chinese imports has grown rapidly. However, the discrepancy between the planned prohibition and the actual trade data implies that the UNSCRs have not stopped prohibited trade, they have simply incentivized trade to occur via the black market.

In this regard, three end-states are identified based on the assumption that painful economic damage would provoke the DPRK to compromise to a certain degree; (1) a complete interdiction to induce CVID, (2) a tightening of interdiction mechanisms to reduce evasion of the sanctions in order to gain considerable bargaining advantage, (3) a signal of interdiction to facilitate the de-nuclearization dialogue. Among these three end-states, the second end-state is the most plausible and adequate for three reasons. First, a complete interdiction would entail a de-facto blockade, which is illegal in peacetime. Second, a blockade will undoubtedly result in dangerous escalation around the Korean peninsula. Third, the high degree of sanction evasion makes the

third end-state unattractive. Data suggests that a large amount of seaborne trade has gone to the black market making clear that the inaction the third end-state proposes would do little to change the status quo.

## *(2) Selecting the Best Methods*

In order to select the best methods, one must identify possible actions for the chosen end-state and choosing the most elemental and essential action (Eikmeier 2007, 64). Three actions are identified to achieve the end-state of “a reduced evasion of the sanctions.” The first action reinforces the search and inspection of vessels transiting in the high and territorial seas. This action requires the forceful inspection, also known as Visit, Board, Search, and Seizure (VBSS) on suspected vessels by local maritime authorities. The second action is the tightening of the inspection of vessels by Port State Control (PSC) Authorities in Southeast and East Asia. Since January of 2017, Chinese port inspection authorities have detained 12 DPRK-flagged vessels and four vessels under other countries’ flags (**Table 5**). The third action is tightening of regulations on private companies such as oil, refinery, and insurance companies. This tightening would mitigate the DPRK’s illicit maritime activities by regulating that private companies follow the AIS switch-off clause, obligating insurance providers to screen for AIS, and requiring oil companies and refineries to oversee the end-user of their product in their contracts (North Korean Sanctions Advisory 2019, 14~15).

All of these actions are necessary to curb sanction evasions, yet the first option of “reinforced search and inspection of vessels transiting in the high and territorial” is the most important, because it can undercut the profit margins of prohibited deals. Reinforced search and inspection would make the unofficial trades less lucrative to the extent that it would de-incentivize the DPRK and its business partners from continuing their illegal activities. The increased probability of the seizure of prohibited cargo by the crackdown would reduce the expected return on the illicit trade. Therefore, introducing uncertainty would increase the risk premium, and would thus result in decreased revenue (Haggard and Noland 2017, chap.3). Furthermore, this action would make illicit activities more costly in that the DPRK and the DPRK’s business partners would pay additional costs such as detours, early warnings, or forged documents to avoid inspection. For example, it was observed that, during the Suez Canal closure from 1967 and 1975, for every ten percent increase in ocean distance, there was a 5 percent decrease in trade activity (Feyrer 2009).

The second and third options are less crucial than the first action because they are less relevant to obtaining the objective of “a reduced sanction evasion by the DPRK.” The DPRK has utilized ship-to-ship transfers in the high seas to avoid port inspections. Additionally, Chinese sanction enforcement has

fluctuated according to its national security interests. No one can deny that the DPRK heavily relies on China economically, yet Chinese pressures wax and wane according to changing situations (Silberstein 2019, 10). While China detained thirteen DPRK vessels in 2017, it only detained two vessels in 2018 (Table 5). Moreover, regulations on private actors often failed to achieve their objectives, instead they brought about unexpected side effects. Private parties will keep finding new sanction evasion methods as long as they can make a profit by capitalizing on illegal deals with the DPRK.

### *Allocating Resources for the Selected Methods*

The following will discuss the means required to support and execute the selected ways. Understanding the means required is critical because the means currently available may constrain the ways and ends (Eikmeier 2007, 64). Thus, the existing institutions are listed as the means to execute the interdiction operation; (1) the Proliferation Security Initiative (PSI), (2) Regional Consultative Body (e.g., Six-Party Talk), (3) The Trilateral Security Cooperation (ROK – the U.S. - Japan), and (4) Unilateral efforts (the U.S.).

The PSI is a recommended means because it enables the international community to reinforce the search and inspection of foreign vessels suspected of violating the UNSCRs. Both burden-sharing and closely coordinated efforts are essential for interdiction as it is incredibly challenging to track, board, and inspect suspicious vessels. Therefore, it is unreasonable to leave the task of maritime interdiction to a few countries. The PSI is useful because it has forced many countries to participate in interdiction efforts, allowing for burden-sharing. First, the PSI, as a nonbinding agreement, has formed common grounds among like-minded countries, and has accordingly facilitated easier and quicker cooperation among the member states. Second, it has reduced coordination problems, not only because it consists of like-minded countries, but also because it makes non-compliant members pay reputational and political costs. So long as there is a keen political interest in this issue, states will be inclined to stick to their commitments. Third, it has systemized cooperation through its annual interdiction exercise. For example, partner countries have followed the Asia-Pacific Exercise Rotation (APER) to enhance their coordinated interdiction capabilities.

The PSI neglects legal limitations to which the UN Security Council resolutions have attended. Although the UNSCRs are limited in their scope, they have broad effects because they can be conducted through the PSI (Haggard and Noland 2017, chap.3). The PSI has failed to grant its members a legally binding authority for the inspection of foreign vessels and has failed to mandate that its members observe their commitments. However, the UNSCRs are still not specific about law enforcement with respect to who will track and inspect suspicious vessels nor about how to acquire reasonable evidence.

Nevertheless, the resolutions authorize member states to search and inspect foreign vessels forcefully if there is a will and evidence. The "Joint Statement from Proliferation Security Initiative (PSI) Partners in Support of United Nations Security Council Resolutions 2375 and 2397 Enforcement" highlighted the potential of the PSI. According to the joint statement, the PSI targets not only WMD but also the prohibited commercial trades (Federal Foreign Office of Germany 2018). So, if the UNSCRs and the PSI are combined, they would be able to complement each other and generate a new source of power.

## **Conclusions and Policy Recommendations**

The international community should solve the problems of the DPRK's nuclear programs through diplomacy. However, if the DPRK continues to disable the current sanctions imposed by the UN Security Council resolutions, the community will face trouble with prompting the DPRK to work toward denuclearization. While an excessively belligerent policy puts the denuclearization dialogues at risk and the peace and prosperity of the region in danger, a multilateral effort is still needed to enhance the bargaining power of the community, and to lead the DPRK to an end-state of which the international community would approve.

Evidence suggests that the DPRK has evaded sanctions through various illegal maritime practices, so much so that the illicit trade is enough to keep the DPRK's economy afloat. Among the measures the international community can take, "reinforcing the search and inspection of the DPRK related vessels transiting in the high and territorial seas" is the best policy approach to reduce sanction evasion and provide the international community with considerable bargaining advantages. This measure requires forceful action by law enforcement agencies, such as VBSS, and it would make the prohibited deals less profitable and attractive by reducing the expected return and imposing additional costs.

The DPRK has been able to render sanctions futile by exploiting the limitations of the current maritime sanctions. The resolutions are not specific enough regarding law enforcement, and the PSI is legally nonbinding. However, if the UNSCRs and the PSI are combined, they can generate a new source of authority to exploit the weakness of the DPRK. It is worthy to emphasize that the recent UNSCRs stipulated that all legal discussions in the resolutions are confined and applied only to the DPRK. The PSI endorsing partners should go beyond mere commitments. They should discuss action plans to implement the maritime interdictions to the extent that they discourage the DPRK and its business partners from continuing their illegal activities.

On the other hand, one could expect several significant limitations if the PSI partners begin to enforce the UNSCRs. The PSI can easily be mistaken

as a low level of military option for compelling the DPRK or a precursor to a de-facto blockade surrounding the Korean peninsula. However, this kind of approach is ineffective and illegitimate. The PSI participants should exercise enough caution not to associate the PSI with a blockade against any specific country or a military option. It should be no more than enforcing domestic and international laws through collective efforts of the international community to make illegal deals less lucrative and to eventually discourage them. A reinforced search and inspection is designed not to block completely prohibited commercial trades but to raise the detection rate. Proper restraint is necessary to refrain from spending too many resources and escalating the situation out of control. With this restraint, the interdiction effort against the DPRK's illegal maritime activities will be more legitimate and successful.

Finally, the PSI is likely to be supported by like-minded countries, such as the U.S. allies, but it needs to try to keep including other countries reluctant to join the initiative or to implement the maritime interdiction. Since the United States and the DPRK failed to bridge a gap at Hanoi in 2019, the United States and its key allies have increased their efforts to interdict DRPK vessels. For example, Royal Navy frigate HMS Montrose found and tracked the DPRK tanker Saebiyol, which was conducting the ship-to-ship transfer of oil with unknown small vessels in the East China Sea on March 2nd, 2019. HMS Montrose worked alongside with Japanese partners (Royal Navy 2019). France also dispatched a Falcon 200 and a the Vendémiaire surveillance frigate to monitor the DPRK's sanction evasion (Permanent Mission of France to the United Nations in New York 2019). However, this approach focusing on the U.S. allies will be likely to cause China and Russia to misdoubt motivations of the collective efforts to interdict DRPK vessels. It can also discourage non-U.S. allies from actively participating in the maritime interdiction. Therefore, the PSI should remain to be a multilateral activity with which the international community commits to resolving nuclear problems of the DPRK.

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## Appendix

**Table 1.** Trade Value of Coal (HS2701), Import from the DPRK (US dollars)

Year	China
2016	1,186,694,713
2017	410,358,802

**Source:** UN Comtrade

**Table 2.** Port Inspection of All Ships (1 January 2017 ~ 30 May 2019)

Flag of Ship	No. of Inspections	Mean of Deficiencies	No. of Detentions
The DPRK	106	10.43	12
Other Countries	116	7.86	4

**Source:** Author’s calculations from NKnews

**Table 3.** Items prohibited by the United Nations Security Council Resolutions (the DPRK)

UNSCRs	Prohibited Items	
	Export to the DPRK	Import from the DPRK
R2397 (Dec 22, 2017)	<ul style="list-style-type: none"> <li>● Crude oil (4 million barrels)</li> <li>● Refined petroleum products including diesel, kerosene (500,000 barrels)</li> <li>● All industrial machinery</li> <li>● Transportation vehicles</li> <li>● Iron, steel, other metals</li> </ul>	<ul style="list-style-type: none"> <li>● Food and agricultural products</li> <li>● Machinery</li> <li>● Electrical Equipment</li> <li>● Earth and Stone including magnesite and magnesia</li> <li>● Wood</li> <li>● Vessels</li> <li>● Full sectoral seafood</li> <li>● Fishing rights</li> </ul>
R2375 (Sep 11, 2017)	<ul style="list-style-type: none"> <li>● Condensates</li> <li>● Crude oil</li> <li>● Natural gas liquids</li> <li>● Refined petroleum products</li> </ul>	<ul style="list-style-type: none"> <li>● Textile</li> </ul>
R2371 (Aug 5, 2017)	-	<ul style="list-style-type: none"> <li>● Seafood</li> <li>● Lead and Lead ore</li> </ul>
R2321 (Nov 30, 2016)	<ul style="list-style-type: none"> <li>● Helicopters and vessels</li> </ul>	<ul style="list-style-type: none"> <li>● Copper</li> <li>● Nickel</li> <li>● Silver</li> <li>● Zinc</li> <li>● Statue</li> </ul>
R2270 (Mar 2, 2016)	<ul style="list-style-type: none"> <li>● Aviation fuel</li> </ul>	<ul style="list-style-type: none"> <li>● Coal</li> <li>● Iron and Iron ore</li> <li>● Gold</li> <li>● Titanium/Vanadium ore</li> <li>● Rare earth minerals</li> </ul>

**Source:** 1718 Sanctions Committee

**Table 4.** UNSCR measures to be implemented by a member state (if a state has evidence)

<i>Boundaries</i>	<i>Consent from Flag state</i>	<i>No Consent from Flag state</i>
<i>High Seas</i>	Inspect (discretionary)	Flag State must proceed a vessel to an appropriate and convenient port (mandatory)  Flag State must immediately deregister a vessel if a vessel refuses to comply (mandatory)
<i>Territorial Waters</i>	-	Seize, Inspect, and freeze (discretionary)
<i>Ports</i>	-	Seize, Inspect, and freeze (mandatory)

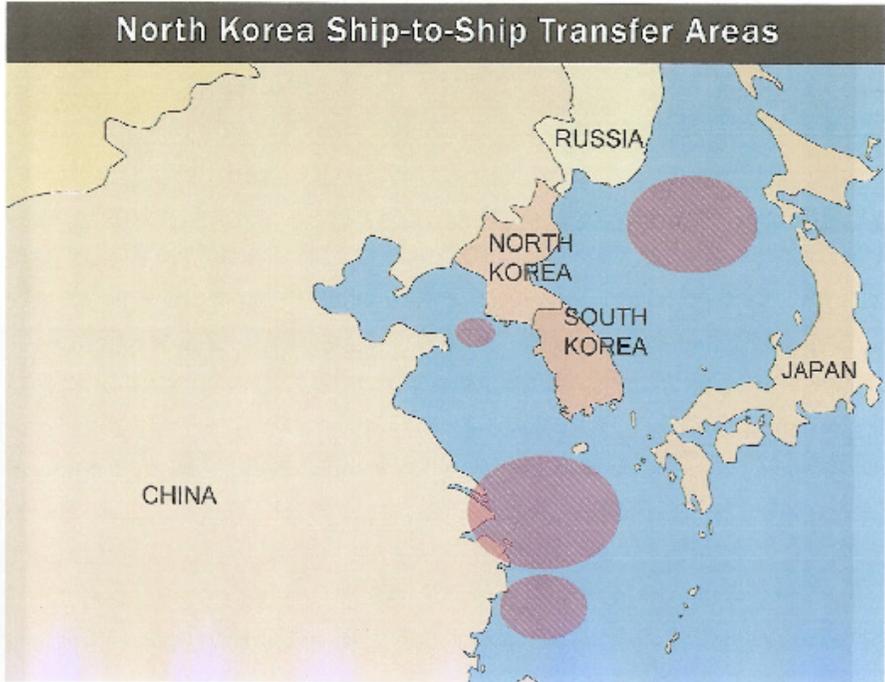
**Source:** North Korea Sanctions Advisory

**Table 5.** Chinese Port Inspection of DPRK Vessels (1 January 2017 ~ 30 May 2019)

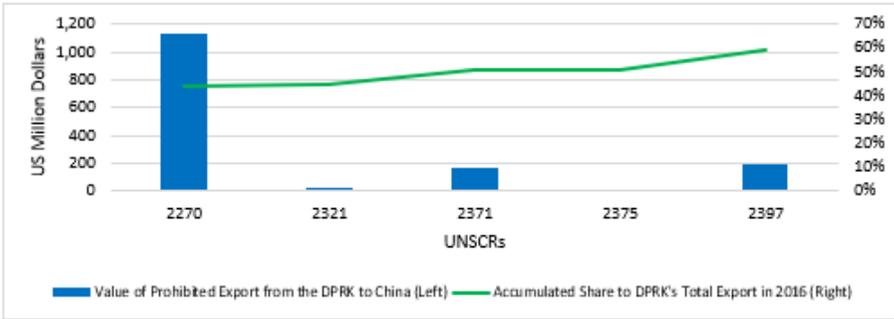
<i>Year</i>	<i>No. of Detention</i>	<i>Flag of Detained Vessels (No. of Detention)</i>
2017	13	DPRK (9), Fiji (3), Togo (1)
2018	2	DPRK (2)
2019	1	DPRK (1)

**Source:** Author's Calculation from NKnews

**Figure 1.** Areas where the DPRK has conducted ship-to-ship transfers

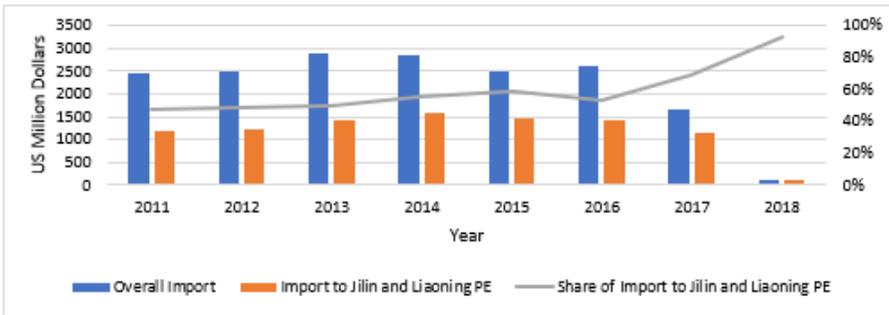


**Figure 2.** Volumes of the Prohibited Exports from the DPRK to China

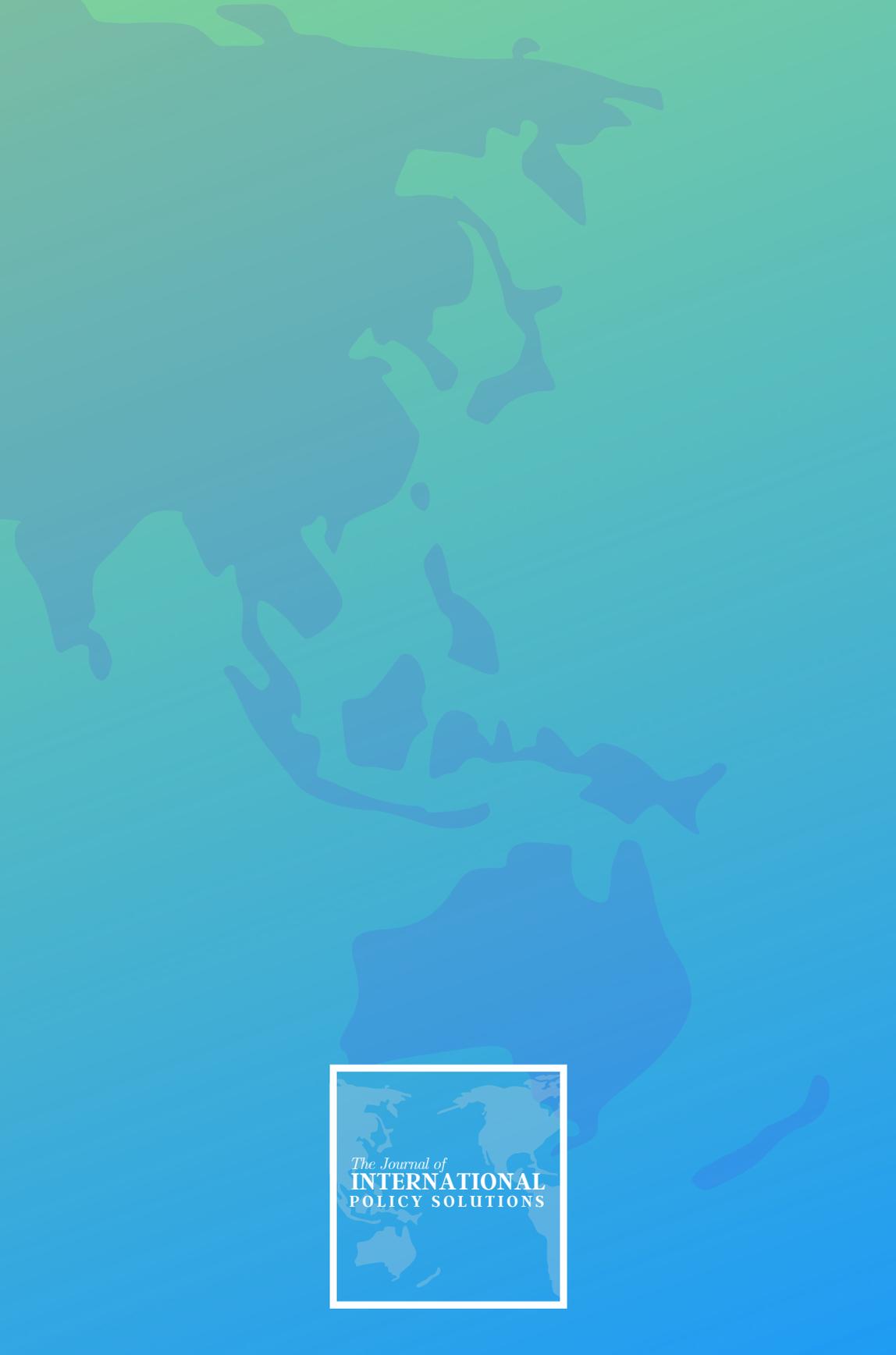


**Source:** Author's calculation from UN Comtrade

**Figure 3.** Volumes of the Import from the DPRK (China and Jilin/Liaoning Provinces)



**Source:** Author's Calculation from the Korea International Trade Association (K-stat)



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