

The Effect of Sanctions on Human Rights:

Assessing the Impact of Economic Sanctions on Human Rights Violations in Targeted Countries

Kristoffer Fretland Øygarden



*Department of Political Science
Faculty of Social Sciences
University of Oslo*

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Abstract

Since the beginning of the 1990s, economic sanctions have become an increasingly employed tool by major powers in order to achieve international political objectives. A growing body of research has emerged, however, indicating that the use of sanctions has the unintended consequence of increasing human rights violations in targeted countries. In this thesis I examine the effect of economic sanctions on two categories of human rights – physical integrity rights and civil and political rights – using cross-national, time-series data covering the period 1981-2005, compiled primarily from the Hufbauer, Schott and Elliott (HSE), Threats and Impositions of Sanctions (TIES), and Cingranelli and Richards (CIRI) datasets. I base my research design on the replication and expansion of Dursun Peksen's (2009) analysis in *Better or Worse? The Effect of Economic Sanction on Human Rights*, which examines the impact of economic sanctions on physical integrity rights for the period 1981-2000. My findings corroborate earlier research indicating that economic sanctions have a detrimental impact on physical integrity rights in targeted countries. These findings are bolstered by the most extensive data on economic sanctions available to date. I also show that the negative impact of sanctions appears to extend to a subset of civil and political rights, such as freedom of speech, and that the negative impact is comparable in scale to that on physical integrity rights.

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All mistakes and errors are my own.

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Chapter 1

Introduction

The purpose of this thesis is to investigate the impact of economic sanctions on human rights violations by governments in targeted countries. More specifically, I will examine the effect of sanctions on two categories of human rights - physical integrity rights and civil and political rights - and compare the findings to assess the relative impact of sanctions on the state-sponsored violation of the citizens' rights.¹

I address the research question by specifying a total of 88 ordered probit models, using cross-national, time-series sanctions data compiled from the Threats and Impositions of Sanctions (TIES) and Hufbauer, Schott and Elliott (HSE) datasets, as well as data on human rights violations from the Cingranelli and Richards (CIRI) human rights dataset.

I conduct the analysis in three separate stages. In the first stage I perform a replication of 20 models from Dursun Peksen's 2009 paper *Better or Worse? The*

¹Contrasting the term "civil and political rights" with "physical integrity rights" may appear contradictory, as the right to physical integrity is one of the core provisions of the International Covenant of Civil and Political Rights (ICCPR), along with individual liberties and political rights (UN General Assembly 1966). It is therefore necessary to specify that I use the term "civil and political rights" throughout this thesis as a shorthand for "civil liberties and political rights". The term, as applied here, precludes the rights associated with the protection of physical integrity in international human rights law. Consequently, I use the term "civil and political rights" in contrast to "physical integrity rights" throughout.

Effect of Economic Sanctions on Human Rights, which investigates the effect of economic sanctions on physical integrity rights in 95 countries during 1981-2000 period. I replicate Peksen's findings using both his own data sample and a corresponding dataset I have reconstructed using the original data sources, yielding a total of 40 models.

In the second stage I expand the replication dataset to account for new data on sanctions, made available through the publication of the updated TIES dataset in 2013, and run an analysis using identical specification as in stage one. The data expansion extends the analysis period by five years to cover the years 1981-2005, and includes observations omitted from the replication analysis due to data constraints. In total, the expanded analysis contains approximately 1000 additional observations compared to the replication analysis and Peksen's original study.

The third stage of the analysis investigates the relationship between economic sanctions and state-sponsored violation of civil and political rights, using the expanded dataset and same explanatory variables as in stage two. The indicators for physical integrity rights and civil and political rights are all taken from the CIRI human rights dataset. Civil and political rights are operationalized using the CIRI empowerment rights index and its associated individual rights indicators.

In this chapter I first provide a cursory overview of the sanctions literature, and the motivations underpinning the choice of research question in this thesis, before I briefly outline the key arguments for basing my methodological framework around the replication and expansion of existing research. The chapter concludes with a summary overview of the structure of the rest of the thesis.

1.1 Why study the effect of economic sanctions on human rights?

Since the beginning of the 1990s, economic sanctions have become an increasingly employed tool by major powers, the European Union (EU) and the United Nations (UN) in achieving international political objectives (Pape 1997: 90; Baldwin 1999). To illustrate, the UN Security Council approved partial or comprehensive

sanctions against 13 countries between 1990 and 2000, compared to only twice between 1945-1990 (Elliott, Hufbauer, and Oegg 2008).

The scholarly work on sanctions and their increasing prominence as a tool of statecraft has primarily been concerned with whether or not they “work” – that is, to what extent they are effective in getting targeted countries to comply with sender demands. The early scholarly assessment of sanction effectiveness posited that they generally do not lead to significant concessions, and are therefore an ineffective instrument in statecraft (Galtung 1967; Hufbauer, Schott and Elliot 1990; Pape 1997). Later work has indicated that the ineffectiveness of economic coercion primarily apply to *implemented* sanctions, and that sanction are more likely to be effective in eliciting compliance at the threat stage – i.e. before they are put into place (Hovi 2001; Hovi, Huseby and Sprinz 2005).

Parallel to the research on sanction effectiveness, a growing body of literature shifted its focus to the broader, unintended consequences of economic sanctions for targeted countries. This strand of research have found that implemented sanctions not only appear to be ineffective, but seem to generate a number of negative externalities (Peksen 2009: 60-61; Peksen and Drury 2010: 242). For instance, scholars have examined the adverse humanitarian effects of sanctions, suggesting that economic coercion inadvertently worsens economic conditions and civil society development through their disproportionate impact on citizens (Galtung 1967; Weiss et al. 1997; Weiss 1999; Cortright, Millar and Lopez 2001; Lopez and Cortright 1997; Cortright and Lopez 1995). Other studies have focused on the negative effect on political stability in the targeted countries (Marinov 2005), while a growing body of research has examined the detrimental effect of sanctions on human rights (Lopez and Cortright 1997; Li and Drury 2004; Drury and Li 2006; Wood 2008; Peksen 2009; Peksen and Drury 2009; Escribà-Folch 2012; Drury and Peksen 2014; Carneiro and Apolinário 2015).

In the literature exploring the relationship between sanctions and human rights practices, most studies focus on the most fundamental human rights such as protection from torture, extra-judicial killings, political imprisonment and forced disappearances - collectively referred to as physical integrity rights. A smaller subset of studies examines the impact of sanctions on the broader set of civil and polit-

ical rights, often by way of aggregated proxies indicating the level of democracy in the targeted country. While the research on physical integrity rights is more or less unanimous in its assessment that sanctions harm human rights practices, the research on civil and political rights is less conclusive. The issue is exacerbated by the fact that scholars addressing the sanctions-human rights nexus have tended to examine different types of rights separately, using different models, indicators and specifications. Consequently, comparison across different studies is difficult, as is assessing both the absolute and relative impact of sanctions on specific categories of rights. For instance, few studies have examined the relative impact of sanctions on physical integrity rights and other categories of human rights.

Furthermore, the majority of the literature uses aggregated indicators of human rights violations, making it hard to assess the impact of sanctions on individual human rights. To my knowledge, no existing study has performed a comprehensive examination of the impact of sanctions on individual indicators of civil and political rights. Conducting a study of the impact of sanctions on both individual and aggregated indicators of physical integrity rights and civil and political rights, in a manner which allows for the comparison of results across both categories of rights, would therefore constitute a novel contribution to the sanctions-human rights literature.

Finally, the last few years have seen an expansion of both the scope and detail of the data on economic sanctions, most recently through the update of the Threats and Impositions of Sanctions (TIES) dataset in 2013. Few empirical analyses from the literature have taken full advantage of all the available sanctions data.

Taken together, the points outlined above provide ample reason to revisit the question of the impact of sanctions on human rights: first, to re-examine previous findings in light of new, pertinent data; and second, to examine the impact of economic sanctions on both individual and aggregated indicators of civil and political rights in a manner consistent, and therefore comparable, with previous research. By using the same models and specifications to investigate different types of human rights violations within the same study, it will be possible to systematically assess the relative impact of economic sanctions on different types of human rights.

1.2 Why replicate?

I have chosen to examine the research question of this thesis by way of replicating and expanding upon a key study from the literature on the effect of sanctions on human rights, Dursun Peksen's 2009 article *Better or Worse? The Effect of Economic Sanctions on Human Rights*.

During the course of this thesis I present two core arguments for why I have chosen this specific methodological framework. The first is an argument in favour of replication in the social sciences literature generally, and in the international relations literature specifically. The failure to adhere to basic replication standards is an issue that has plagued the political science literature, and though steps have been taken to improve transparency of published works, progress has been slow. The replication and expansion of existing research is arguably the best way to advance the collective knowledge in a particular field while at the same time ensuring reproducibility and robustness of previous findings.

The second argument concerns the reasons for choosing Peksen's study specifically as the subject of my replication analysis. These reasons will be outlined in detail during the course of this thesis, but can be boiled down to three core benefits of replicating this specific study: first, Peksen's analysis is one of the few long-term, cross-national empirical analyses of the effect of sanction on physical integrity rights in targeted countries; second, his reliance on the Cingranelli and Richards (CIRI) human rights dataset allows for the examination of both aggregated and individual indicators of human rights violations, as well as ensuring that both physical integrity and civil and political rights indicators can be drawn from the same dataset; and third, his reliance on a combination of the TIES and Hufbauer, Schott and Elliott (HSE) data on economic sanctions ensures that the broadest possible scope of sanctions cases can be accounted for, and that I can take advantage of the additional data made available through the updated TIES dataset.

1.3 Structure of the thesis

The structure of the thesis is as follows: In Chapter 2 I first present an overview of theoretical perspectives and empirical findings from the scientific literature on the effect of economic sanctions on human rights – more specifically physical integrity rights and the broader set of civil and political rights. I then discuss some contrasting perspectives on the conceptual difference (or lack thereof) between these two categories of rights, before presenting arguments for why a more detailed investigation into the effect of economic sanctions on civil and political rights is both valuable and necessary. Next, I provide a detailed overview of why replication and subsequent expansion of Peksen’s paper from 2009 is a good way to address the research question in this paper, and outline how this determines the structure of my analysis. I close the chapter by formulating the expected outcome of my analysis in four key hypotheses.

Chapter 3 begins with an overview of Peksen’s research design, including data sources and descriptions of individual variables. I then move on to detail the process of replicating Peksen’s study, including an overview of how I reconstructed his dataset from the bottom up, as well as the specific challenges I encountered during this process and how I chose to address them. Next I outline how I expanded the replication dataset to include newly available, pertinent data from the updated TIES dataset, before presenting an overview of how I operationalized civil and political rights for the final part of my analysis. Finally, I justify my choice of using the ordered probit regression model throughout my thesis, and give a general overview of the ordered regression models and their characteristics.

Chapter 4 through 6 present and analyse the results from the replication analysis, expanded analysis with additional sanctions data, and expanded analysis using the subset of civil and political rights indicators from the CIRI dataset as dependent variables, respectively.

I conclude my thesis in Chapter 7 with a summary of the main findings from my analyses and their implications for the wider sanctions literature, as well as some suggestions with regard to possible future research in the same vein.

Chapter 2

The effect of economic sanctions on human rights

The following chapter contains an overview of the empirical literature examining the impact of economic sanctions on physical integrity rights and civil and political rights, respectively. After reviewing the literature, I present key arguments for why it is worthwhile to (re)examine the impact of sanctions on civil and political rights, and highlight the importance of consistency of research design and methodological framework between studies in order to facilitate comparison of results across studies.

I then outline how the replication and expansion of previous research can be used to address the considerations regarding consistency and comparability, and present a few fundamental arguments in favour of replication in general within the social sciences. I then present key contributions to the quantitative sanctions literature that are suitable candidates for replication, and argue why Peksen's (2009) study is the most appropriate work to base the my expanded analysis on. I conclude with a brief overview of the methodological choices I have made on the basis of the discussion in this chapter, and by formulating the hypotheses indicating the expected outcome(s) of my analysis.

2.1 The “naive” theory and its critics

According to the existing research literature, in which ways are economic sanctions likely to affect human rights practices? Peksen (2009) provides a comprehensive overview of the bulk of the literature on the extant effects of economic sanctions. Based on existing research, he proposes that the employment of economic sanctions will likely lead to one of two outcomes with regard to repressive practices in the targeted countries.

First, if the sanctions are successful in reducing the coercive capacity of the target regime, we should see a lower likelihood of human rights violations by the government. On the other hand, if the sanctions fail to reduce the target regime’s coercive capacity, the increased political and economic instability caused by the sanctions will likely lead to increased governmental repression of citizens. Thus the critical condition determining which outcome we can expect is to what extent economic sanctions (specifically and generally) curbs the target regimes’ *ability to employ repressive practices* vis-a-vis its citizens (Peksen 2009: 61).

The first outcome is what Peksen refers to as the “naive” theory of economic sanctions. The theoretical argument underpinning this expected outcome can be summarized as follows: Economic coercion weakens the targeted regime by limiting their access to necessary economic, military and other resources to ensure stability and order (Galtung 1967: 388; Kirshner 1997: 42). Consequently, the coercive capacity of the repressive regime is diminished, lessening governmental repression (Blanton 1999; Davenport 1995). Furthermore, scarce economic resources have often been used as a tool to reward supporters of the regime and key sectors of the populace, such as the police, military and civil service. Diminished or lost access to such resources may result in loss of support from influential groups, and quell the regime’s coercive capacity further (Wintrobe 1990; Bueno de Mesquita et al. 2003). Finally, the weakening of the regime’s coercive capacity likely leads to the empowerment of opposition movements and dissidents, possibly leading to a shift of power in favour of anti-government groups. Increased leverage for these groups can in turn be used to promote improved human rights practices (Peksen 2009: 61).

The alternative view of how sanctions affect repressive practices, contrary to the “naive” theory, predicts that economic coercion will worsen rather than improve human rights conditions in the targeted countries. Provided that sanctions do not weaken the regime, they are likely to lead to increased political and economic disorder which in turn is likely to increase government repression. Based on the sanctions literature, Peksen provides four main explanations for why this outcome is likely: the diversion of sanction costs to citizens; exacerbation of humanitarian grievances, instability, and political unrest; the “rally around the flag”-effect; and isolation from international influences. These four points are outlined in turn below, as presented in Peksen (2009):

1) Since repressive regimes often control the supply of scarce public resources, they are able to divert the cost of sanctions to citizens rather than the political leadership.

There are several ways in which a government may divert or avoid bearing the cost of economic sanctions. First, because the state leaders control the scarce public resources, political elites may divert the cost of sanctions to disproportionately affect ordinary citizens. These resources can in turn be used to reward and improve ties with influential constituencies in the country, further strengthening the authoritarian regime (Weiss et al. 1997; Weiss, 1999; Rowe 2000; Cortright, Millar and Lopez 2001). Conversely, sanctions tightens the bonds between influential social groups and the regime, as the former relies on the latter for access to scarce resources (Gibbons and Garfield 1999; Reuther 1995). Getting resources and revenue from alternative sources is another way in which regimes can limit the impact of sanctions. This can be facilitated in numerous ways, for example through relations with (regime-sympathetic) countries not participating in the sanctions regime, or through illegal smuggling, underground transnational economic channels, etc (Peksen 2009: 62; Weiss et al. 1997; Weiss, 1999; Rowe 2000; Cortright, Millar and Lopez 2001).

2) Economic coercion often leads to humanitarian grievances in the target country, destabilizing the country and leading regimes to use increasing repression to maintain the

status quo.

Research on the humanitarian consequences of economic coercion indicate that there are a number of unintended negative consequences of employing sanctions. These include greater poverty, higher unemployment levels, and worsening health conditions (Weiss et al. 1997; Weiss 1999; Cortright, Millar and Lopez 2001; Cortright and Lopez 1995, 2000). Greater grievances among economically disadvantaged groups contributes to instability and may lead to anti-government protests and riots (Allen 2004; Marinov 2005), as well as political violence (Gurr 1968, 1970), which in turn leads to increased repression by the target regime (Poe and Tate 1994; Poe, Tate and Keith 1999).¹

3) State leaders often depict sanctions as an external threat, using it to bolster support for the current regime and justify repression against opposition movements.

This effect is commonly referred to as “rallying around the flag” – external pressure is often depicted by targeted governments as a threat to national integrity, unity and security (Peksen 2009: 62). This threat is then used as a pretext by the targeted government to increase their perceived legitimacy and to justify action against opposition movements, for example in order to maintain national “cohesion” (Galtung 1967, Miyagawa 1992: 84-86).

¹However, some recent papers indicate that the link between economic coercion and anti-government violence does appear to be somewhat more nuanced: Allen (2008) examines the effect of international economic sanctions on the number of anti-government demonstrations and riots in target countries in the period 1948-1999. She finds that sanctions are only associated with increased protest in democratic countries, and that the effect is consistent across sanction types (Allen 2008). Gravougel, Licht and Soest (2017) examine the relationship between threatened and imposed sanctions on domestic protest in targeted countries. They find that threats, not imposed sanctions, are associated with an increased probability of anti-government protests, and argue that sanction threats work as an international “stamp of approval” for would be protesters (Gravougel, Licht and Soest 2017). These findings cast some doubt over the proposed relationship between imposed sanctions and political violence, and indicates that sanction threats, not just implemented sanctions, may have an effect on human rights conditions in targeted countries.

4) *Economic sanctions creates unfavourable conditions for human rights because they isolate the targeted countries from external political and economic influences.*

Economic sanctions, especially comprehensive ones, contribute to the international isolation of targeted regimes, as extensive severance of political and economic ties discourage trade and investment (Hufbauer et al. 1997; Hufbauer and Oegg 2003; Caruso 2003; Yang et al., 2004). Economic integration through trade and foreign investment is seen as a major factor in the promotion of governmental respect for human rights (Mitchell and McCormick 1988; Meyer 1996; Goldstone Bates and Epstein 2000; Apodaca 2001; Richards, Gelleny and Sacko 2001; Hafner-Burton 2005a,b), specifically through contributing to economic wealth and the emergence of a politically stable and strong middle class demanding respect for the basic rights of citizens. The isolation of repressive regimes from the international community arguably undermines this mechanism, contributing to the consolidation of repressive regimes through increased economic instability and the weakening of opposition groups (Peksen 2009: 63).

Based on these observations, Peksen hypothesizes that economic coercion will ultimately lead to worse human rights practices; first through the limited impact of sanctions on the regimes they're intended to affect, and second through the indirect or unintended negative economic and humanitarian effects of economic coercion on the citizens of the targeted states (Peksen 2009: 63).

Peksen's hypothesis is supported by his results: looking at indicators of so-called physical integrity rights, incorporating measures of torture, extra-judicial killings, disappearances and political imprisonment, Peksen finds that economic sanctions are associated with increased violations of these rights. The most extensive the sanctions appear to have greater detrimental effects. Additionally, the effect appears to be stronger for multilateral sanctions, indicating that coordinated sanction efforts by multiple nations may be even more detrimental to human rights conditions in targeted states (Peksen 2009: 74-75).

Other empirical studies on the relationship between sanctions and human rights appear to support Peksen's findings: Wood (2008) examines the effect of UN and US sanctions on human rights between 1976-2001, using the Political Terror Scale (PTS) measure of physical integrity rights violations. His results indicate that sanctions are associated with increased political repression, and that the increase is greater for more severe sanctions (Wood 2008). Similarly, Escribà-Folch (2012) investigates the impact of international sanctions on political repression in authoritarian regimes, using panel data covering the 1976-2001 period. Employing the PTS measure, like Wood, his findings bolster the evidence that sanctions negatively impact the respect for human rights (Escribà-Folch 2012).

The aforementioned studies are all relatively wide in scope, drawing on extensive data from the Threats and Imposition of Sanctions (TIES) and/or the Hufbauer, Schott and Elliott (HSE) datasets on economic sanctions.² A couple of studies in the sanctions literature produce some evidence countering the consensus described above, though it must be noted that these are comparatively limited in scope. Carneiro and Apolinário (2015) examine the effect of targeted UN sanctions on human rights in African countries between 1992 and 2008. While they find that targeted sanctions are associated with greater repression, the same significant relationship is not found for non-targeted sanctions (Carneiro and Apolinário 2015).

Gutmann, Neuenkirch and Neumeier (2016) present a comprehensive analysis of the effect of US economic sanctions on human rights in a recent working paper for the Institute of Law and Economics (ILE) at the University of Hamburg.³ They assess the effect of all US economic sanctions on four types of empirically distinguishable categories of human rights, as proposed by Blume and Voigt (2007) – basic human rights, economic rights, civil and political rights, and emancipatory and social rights – using both ordinary least squares (OLS) regression and endogenous treatment models. While the results based on OLS estimation seem to confirm previous findings that economic sanctions adversely affect human rights, the results from the endogenous treatment models do not support this claim. This leads Gutmann and his colleagues to conclude that concerns about the adverse human

²I give a more detailed description of the TIES and HSE datasets in section 3.2.

³Note that ILE working papers are not peer reviewed.

rights consequences of (US) sanctions are not supported by the data (Gutmann, Neuenkirch and Neumeier 2016: 24).

Here it is worth briefly commenting on the concept of endogeneity bias, since this is a common issue in time-series analyses of causal processes. Endogeneity bias arises when one of the explanatory variables in the model is correlated with the error term. A common source of endogeneity is when there is a “causal loop” between an independent variable and the dependent variable – in other words, when the explanatory and response variable are both the cause *and* a consequence of the other. Such a causal loop is highly plausible in the context of sanctions and human rights: while much of the literature indicates that sanctions are likely to increase human rights violations, it is also likely that countries violating human rights face a greater risk of having sanctions imposed against them.

In fact, we know that a significant number of economic sanctions are imposed with the explicit goal of curbing human rights violations. To illustrate, of the 204 sanctions cases included in the most recent version of the HSE dataset, 35 were imposed with human rights violations as the main (or one of the main) reasons for its implementation (Hufbauer, Schott, Elliott, and Oegg 2008). Consequently, it is not uncommon for countries facing economic sanctions to also have a history of repressive practices. Controlling for endogeneity bias therefore becomes exceptionally important when studying the effect of sanctions on human rights. One way of doing this is to employ models specifically designed to handle endogeneity, such as the endogenous treatment model used by Gutmann, Neuenkirch and Neumeier (2016).⁴

With the exception of Gutmann, Neuenkirch and Neumeier (2016), the contributions above focus solely on the most egregious human rights violations, including torture, summary executions and political imprisonment. However, physical integrity rights represents only a subset of internationally recognized human rights – what does the empirical literature say about the effect of economic sanctions on the broader set of human rights, such as freedom of speech, the right to freely asso-

⁴This model can be quite hard to use, however, and there are other steps one can take to attempt to handle endogeneity. The measures employed in this thesis will be outlined in Chapter 3.

ciate with political parties and interest groups, and the like? Are these wider sets of rights likely to be negatively affected by sanctions, similar to physical integrity rights? Insofar the violation of civil liberties and political rights serve as one of the possible tools by which governments can repress their citizens and quell opposition, there are few reasons to assume that these rights are unaffected by the mechanisms described above.

In the next section I present a brief overview of the literature examining the effect of sanctions on broader sets of indicators of civil and political rights. I then discuss some challenges to the theoretical distinction between physical integrity rights and civil and political rights, before arguing why it is worthwhile to include a measure of civil and political rights in the study of the impact of sanction on human rights. I conclude the section with a brief discussion of why comparison across studies is an issue for this field in particular, and what measures we can take to rectify this issue in our research design.

2.2 Physical integrity rights vs civil and political rights

As indicated above, the bulk of the research on the impact of economic sanction on human rights practices concerns itself with what is broadly referred to as physical integrity rights. Physical integrity rights are generally taken to encompass the most egregious government abuses of power, such as torture and extra-judicial executions, and ethics and law scholars tend to place these at the forefront of rights that demand recognition and protection (Hill 2013: 1).

However, a subset of the sanctions literature look beyond physical integrity rights to investigate the effect of sanctions on less egregious human rights violations. A number of studies look at the effect of sanctions on a broad set of what is sometimes referred to as civil and political rights, or empowerment rights, generally framed as a component of indicators of the level of democracy (or democratic freedom) in the targeted country.⁵

⁵The term "empowerment rights" is taken from the Cingranelli and Richards (CIRI) human rights dataset (Cingranelli, Richards, and Clay 2014). The term encompasses a number of rights that

I have already mentioned Gibbons' (1999) study, which shows the US-led sanctions against Haiti between 1991-1994 led to the further deterioration of civil and political rights (Gibbons and Garfield 1999). Another example includes Peksen and Drury's (2009) assessment of the effect of economic sanctions on political rights and civil liberties, using panel data covering 102 countries between 1972-2000. They find that economic sanctions are associated with a decrease in these rights, with stronger negative effects for extensive sanctions compared to limited ones (Peksen and Drury 2009). They argue that by putting pressure on governments, sanctions may implicitly (or explicitly) signal support to and therefore give momentum to opposition movements. In turn, the regime is likely to react to or pre-empt a surge in the opposition by engaging in repressive acts to signal their willingness to crack down on political dissent (Peksen and Drury 2009: 399).

Similarly, Soest and Wahman look at the effect of UN, US and EU sanctions on levels of democracy in 117 authoritarian countries in the 1990-2010 period. Contrary to Peksen and Drury, they find that economic sanctions with the explicit goal of promoting democratization tend to lead to improvement in levels of democracy. However, the results do not extend to other sanction types, which show no significant effects (Soest and Wahman 2015).

For data on sanctions, Peksen and Drury draw on both the HSE and TIES datasets, while Soest and Wahman draw on the HSE dataset only. Both studies also take a maximalist approach when defining democratic freedoms, i.e. using aggregated indicators, or index variables. Both rely on the Freedom House (2014) index of civil and political liberties for their dependent variables, though Soest and Wahman also incorporate the polity2 variable from the PolityIV dataset as their democracy measure. The Freedom House index includes measures of civil liberties (freedom of expression, religious freedom, organizational rights, rule of law, and individual rights) and political rights (fairness of electoral processes, degree of political participation, voting rights of elected officials) (Freedom House 2014).

scholars sometime refers to as "civil and political rights" (Gutmann, Neuenkirch, Neumeier 2016), or simply "civil liberties" (Gibney et al. 2016). The CIRI empowerment rights also encompass, among other indicators, all of core provisions for protecting individual liberties in the ICCPR (UN General Assembly 1966).

It should be noted that neither study draw on individual indicators of civil and political rights, relying solely on aggregated measures of democratic freedoms. I will raise this point again when I discuss my choice of methodological framework later in this thesis.

Another (unpublished) study by Christensen (2012) looks at the effect of targeted sanctions on two democracy proxies, the CIRI's empowerment rights index and the PolityIV democracy index. However, his results are largely inconclusive, by his own assessment due to methodological issues, and will therefore not be commented on further here (Christensen 2012: 34). Finally, I previously mentioned Gutmann, Neuenkirch and Neumeier's (2016) comprehensive analysis assessing the effect of all US economic sanctions on four types of empirically distinguishable categories of human rights. They conclude that the concerns about the adverse human rights consequences of sanctions are not supported by the data – this conclusion also extends to what they refer to as civil and political rights (a category encompassing the CIRI empowerment rights indicators) (Gutmann, Neuenkirch and Neumeier 2016: 24).

This brief overview of the literature highlight two important points: first, that the evidence on the effect of sanctions on the broader set of human rights is somewhat ambiguous; and second, that much of the research draws on diverging sources of data, both for their measures of sanctions and their measures of human rights. Before addressing these point further, however, it is worthwhile to briefly discuss the conceptual and empirical distinction between physical integrity rights and other categories of human rights.

Distinctive phenomena?

As outlined above, the majority of research on the extant effects of economic sanctions on human rights violations concerns itself with the most "severe" category of physical integrity rights abuses, while a smaller subset of the literature focuses on government infringement on the broader set of human rights encompassed by what is often referred to as civil and political rights. In terms of research design in the literature reviewed here, physical integrity rights and civil and political rights

are generally treated as conceptually distinct categories of rights – few studies examine both simultaneously.

Hill (2013), however, challenges the conceptual distinction between physical integrity rights and the wider set of human rights.⁶ He argues that while physical integrity rights is the most frequent object of study in the literature on cross-national patterns of human rights violations, the concept itself is not well defined in this body of research. The most commonly employed definition is the extensional definition of physical integrity rights, which is formed with reference to a broadly agreed upon list of actions violating these rights. The extensional definition gained popularity primarily due to Poe and Tate (1994), who adopted this as an operational definition of physical integrity rights to form the basis for what later became the Political Terror Scale (PTS) and the Cingranelli and Richards (CIRI) Human Rights dataset.

Hill, however, argues that it is necessary to form an intentional definition of these rights – that is, “a set of attributes which qualify a particular act as a personal integrity violation” (Hill 2013: 2). Furthermore, he argues that defining physical integrity rights necessitates specifying the conditions under which the state may apply force, and how it should be used. Hill suggests the following definition for physical integrity violation: “an application of force which 1) is not subject to evaluation, *ex ante* and *ex post*, by relevant legal authorities, or 2) is imposed in response to an act that should not be subject to legal punishment, or 3) causes severe bodily or psychological harm, pain, or discomfort” (Hill 2013: 6). The three criteria are referred to as due process, appropriateness and severity, and violation of any of these criteria would under this definition be sufficient to characterize governmental action as a violation of physical integrity rights. The implication here is that extra-judicial incarceration on the basis of, for instance, public expression of anti-government sentiment should also be counted as a breach of the individual’s physical integrity. This, in turn, suggests that civil liberties should not necessarily be considered conceptually distinct from physical integrity rights:

⁶Hill uses the terms “personal integrity rights” and “physical integrity rights interchangeably (Hill 2013:1).

Though civil liberties are often expressed as “freedom to X” and do not usually contain explicit reference to physical coercion or punishment, they protect personal integrity because they limit coercion by giving content to the appropriateness criterion. Freedom to choose what religion one practices, for example, implies that the government (or any private entity) may not punish those who adopt, or use coercion to prevent people from adopting, a particular (or any) set of religious beliefs (...) Asserting the existence of civil liberties, such as freedom of speech or religion, circumscribes the set of actions to which the state may respond with coercion and thus protects personal integrity. Legal prohibitions on activities such as forming political associations, gathering in public, or simply expressing a point of view different from that of the current government, expand the number/range of actions for which the state may apply physical coercion and thus decrease personal integrity protection (Hill 2013: 1).

It is worth reiterating Peksen’s position, outlined above, that the critical condition determining which outcome we can expect is to what extent economic sanctions curbs the target regimes’ ability to employ repressive practices vis-a-vis its citizens – i.e. to what extent sanctions affect a government’s coercive capacity (Peksen 2009: 61). However, he does not explicitly define “coercive capacity,” nor does he delimit the range of actions the term may be applied to. Even if we assume that the definition is limited to physical coercion, Hill’s argument above implies that the theoretical framework Peksen proposes may also apply equally to the broader set of civil and political rights as much as physical integrity rights. That is, measures that curb governments’ coercive capacity with regard to physical integrity rights would also be assumed to affect civil and political rights, insofar these are merely different manifestation of the same underlying rights.

However, though Hill’s argument provides a theoretical foundation for treating physical integrity rights (as commonly defined in the literature) and civil and political rights as though they are not conceptually distinct, there is empirical evidence suggesting that they should be treated as such. Blume and Voigt (2007) performed

a principal component analysis (PCA) on 24 human rights indicators from different datasets. They identify four distinct latent variables, of which two are basic human rights and civil and political rights. Gutmann and Voigt (2015) replicates the PCA using 19 well established human rights indicators, including the CIRI Human Rights dataset, and the Freedom House and Fraser Institute datasets.

The varimax rotated factor loadings with Kaiser normalization for basic human rights (Component 2) and civil and political rights (Component 1) are shown in table 2.1 below. The PCA indicates a clear distinction between physical integrity rights and civil and political rights. The CIRI dataset distinguishes between physical integrity rights, which includes torture, extra-judicial killings, political imprisonment, and disappearances, and what it refers to as empowerment rights, which includes seven rights ranging from freedom of speech to the right to assembly and association, worker's rights, and freedom of religion (Cingranelli, Richards, and Clay 2014). With the exception of worker's rights, all empowerment rights indicators are shown to belong to the theoretically identified category of "civil and political rights" alongside Freedom House's political rights and civil liberties indicators. Likewise, all four CIRI indicators forming the Physical Integrity Rights Index correspond to Gutmann and Voigt's "basic human rights" category (Gutmann and Voigt 2015).

While Hill questions the conceptual distinction between physical integrity rights and civil and political rights, Gutmann and Voigt's analysis indicates that, at least for the purpose of empirical research, the distinction is both useful and appropriate. It should also be noted that it appears that Hill's main point, that the infringement on physical integrity of citizens by governments to enforce restrictions on civil and political rights blurs the distinction between these categories, has been addressed at least to some extent in key human rights datasets. For instance, the CIRI indicator for political imprisonment (subsumed under physical integrity rights), encompasses cases of "incarceration of people by government officials because of: their speech; their non-violent opposition to government policies or leaders; [and] their religious beliefs (...)". Consequently, I would argue that, within the context of empirical investigation into the sanctions-human rights nexus, it is both justifiable and advisable to treat physical integrity rights and civil

Table 2.1: Principal Component Analysis of Human Rights Dimensions

Variable	Component 1	Component 2	Unexplained
Disappearance		0.53	0.40
Extra-judicial Killings		0.56	0.26
Political Imprisonment		0.25	0.40
Torture		0.44	0.35
Freedom of Assembly	0.38		0.27
Freedom of Foreign Movement	0.38		0.31
Freedom of Domestic Movement	0.31		0.56
Freedom of Speech	0.32		0.42
Electoral Self-Determination	0.35		0.26
Freedom of Religion	0.32		0.49
Worker's Rights			0.47
Political Rights	-0.32		0.18
Civil Liberties	-0.29		0.14

Source: Gutmann and Voigt (2015).

Note: The table shows varimax rotated factor loadings with Kaiser normalisation for all CIRI physical integrity and empowerment rights indicators, as well as Freedom House's political rights and civil liberties indicators. The rightmost column indicates unexplained variance for each indicator. The original PCA contains a total of four components – for the complete table see Gutmann and Voigt (2015). Table setup taken from Gutmann, Neuenkirch and Neumeier (2016). Factor loadings below 0.25 are omitted.

and political rights as distinct categories.

In the next section I discuss some arguments for why it is worthwhile to investigate the relationship between economic sanctions and civil and political rights specifically, and why it is instructive to place this investigation in the broader framework of the effects of sanctions on human rights more generally.

Why include civil and political rights in this study?

In this section I highlight three core reasons for why it is worthwhile to study the effect of economic sanctions on civil and political rights in addition to physical integrity rights. The first reason concerns ambiguity in the research literature with regard to how (and if) sanctions affect the repression of these rights, as outlined above. The second reason, closely related to the first, concerns the implications of the use of different types of indicators to conceptualize civil and political rights, including individual and aggregated indicators, and how these issues can be addressed. The third reason concerns the theoretical mechanisms outlined in section 2.1 and their applicability to civil and political rights in addition to physical integrity rights.

I conclude the section with a brief discussion of the issue of comparison of results across studies, and how these can be addressed. Specifically, I argue that the study of the extant effect of sanctions can benefit from consistently applying the same models and research design to indicators of both physical integrity rights and civil and political rights. This discussion leads into the next section, in which I outline the benefits of using replication and expansion of existing research as a basis for addressing these concerns.

First, the literature examining the effect of sanctions on civil and political rights does indicate a negative impact, similar to the study of physical integrity rights, though these results are not entirely consistent. As I have already outlined, the findings of Peksen and Drury (2009) are somewhat at odds with the findings of Soest and Wahman (2015), as well as those of Gutmann, Neuenkirch and Neumeier (2016). Contradictory findings in themselves naturally warrant further investigation into the relationship between economic sanctions and these types of rights.

Additionally, the body of research on this topic is somewhat limited, as evident from the discussion of the relevant literature above. The relatively small amount of research combined with inconsistent results together present a compelling reason to contribute to this field.

Second, while the literature indicates that sanctions may (or may not) have adverse effects on civil and political rights in targeted countries, these are all measured in relation to broad proxies of democracy in the form of index variables. These indicators are not always consistent with each other – for example, while the CIRI empowerment rights index includes a measure for freedom of domestic and foreign movement, this indicator is excluded from the Freedom House index of civil and political rights. As such, the use of different indicators may be one of the sources of the discrepancy between findings in the literature. As will be indicated below, different operationalizations of rights also complicates the comparison of results between different studies of civil and political rights, as well as between studies of civil and political rights and studies of physical integrity rights.

Furthermore, Peksen (2009) has highlighted that some scholars criticize the use of index variables on the grounds that aggregated variables may not adequately capture the behavioural differences between different rights abuses (Mitchell and McCormick 1988; McCormick and Mitchell 1997). Fariss and Schnakenberg's (2014) examine mutual dependence between state repression of different human rights violations. They argue that dependencies develop between different rights violations, as they provide overlapping benefits to leaders and may affect the cost of other repressive measures. The relationship between specific repressive measures may be *substitutive*, where one policy replaces another, functionally similar policy, or *complementary*, where policies are used simultaneously. Fariss and Schnakenberg analyse the relational structure of human rights violations by way of a conceptual network constructed on the basis of thirteen CIRI human rights indicators, including physical integrity and empowerment rights (i.e. civil and political rights). Their findings indicate that none of the individual physical integrity rights are substitutes for any of the other integrity rights, supporting the construction and use of aggregated index variables comprised of individual indicators. These findings extend to the CIRI empowerment rights indicators (Fariss and Schnaken-

berg 2014). Others again have argued that if different integrity rights violations represents substitutable policy choices for governments, the implication is that differentiation between rights is not critical (Tate and Keith 1999: 298).

I would argue that a thorough quantitative investigation into the effect of sanctions on human rights violations should include both individual and aggregated indicators as dependent variables. This allows me to account for both of the positions above, as well as examine whether Fariss and Schnakenberg's findings apply in the context of the effect of economic sanctions on human rights. Specifically, if the individual empowerment rights and physical integrity rights indicators are indeed complementary, we would expect the effect of sanctions to be more or less similar across all individual indicator for the two categories. Peksen's results already indicate that this seems to be the case for physical integrity rights violations. However, to my knowledge there are no comprehensive studies on the effect of sanctions on both aggregated individual civil and political rights. As such, the inclusion of individual indicators of the broader set of civil and political rights would allow me to examine both the effect of sanctions on individual rights and the level of complementarity between empowerment rights violations – representing a novel contribution to the research literature. It is worth noting here, however, that few existing datasets allow for the disaggregation of human rights indicators. To my knowledge, only CIRI contain both individual and aggregated indicators of physical integrity rights *and* broader civil and political rights. Consequently, prioritizing the issue of disaggregation speaks strongly in favour of using the CIRI empowerment indicators as the operational definition of civil and political rights (as these indicators have been shown to be empirically subsumed under the theoretical category of civil and political rights by way of Gutmann and Voigt's (2015) analysis).

Third, while the empirical results from the existing body of research justify further investigation into the sanctions-civil/political rights nexus, we can also argue their inclusion from a theoretical standpoint. In section 2.2, I present the four main mechanisms that Peksen uses to explain how economic sanctions may adversely affect human rights practices, which in turn are rooted in the broader literature on sanctions (Peksen 2009: 61). Notice that three of the four proposed mechanism re-

fer to repression of human rights as a tool used by governments to entrench their power and quell opposition and dissent. First, economic coercion is likely to lead to humanitarian grievances, which in turn increased repression as governments try to quell the resulting instability and political violence (mechanism 2). Second, the “rally around the flag”-effect of sanctions may be used to justify further repression against domestic threats to the regime (mechanism 3). Third, the isolation from international influences (of the targeted country) associated with economic sanctions may undermine incentives for governments to respect human rights, i.e. to refrain from using these as tools to bolster their rule (mechanism 4).

While Peksen uses these mechanisms to hypothesize that sanctions lead to increased violations of physical integrity rights specifically, there is no compelling reason to assume that they only apply to this subset of rights. Insofar the violation of broader categories civil and political rights is *one of* the tools by which governments repress opposition movements and dissenters and entrench their rule, the mechanisms above can plausibly be assumed to apply to these rights as well. The mechanisms only hypothesize the increased use of repression – not which tools are employed to facilitate it. For instance, the repression of freedom of speech is a frequently used tool by governments to silence political opposition and dissidents, for example through censorship of the press or the suppression of anti-government opinions in public forums. As touched on previously, insofar censorship is part of a repressive governments’ method of curbing opposition, the proposed mechanisms above would lead us to expect that the implementation of economic sanctions will likely exacerbate violations of the freedom of speech.

In summary, the findings from the existing (but diverging) literature, the general reliance on aggregated indicators of human rights, and the broader applicability of Peksen’s four mechanisms all speak in favour of a) examining the impact of human rights indicators beyond those encompassed by physical integrity rights, and b) doing so using both individual and aggregated indicators of rights. I conclude this section with a discussion of the prevailing issue of comparability in the sanctions-human rights literature, and why ensuring comparability across studies should be a key concern when conducting new research in this field.

Comparison across studies

I have already highlight the fact that the use of different indicators of civil and political rights makes it difficult to compare results across studies. The issue of comparison is one that arguably applies to the broader body of research on sanctions and human rights as well. Gutmann, Neuenkirch and Neumeier (2016) point out that studies on the effect of sanctions on human rights are often tested separately, using different models, indicators and specifications (Gutmann, Neuenkirch and Neumeier 2016: 4). For example, while Peksen and Drury (2010) and Soest and Wahman (2015) arrive at different conclusions regarding the effect of sanctions on civil and political rights, they also use different indicators of democracy, different data, and different statistical methods: Peksen and Drury use panel fixed effects vector decomposition regression; Soest and Wahman use pooled OLS regression. Furthermore, Peksen himself employ different models between examining physical integrity rights (pooled ordered probit regression) and civil and political rights (panel fixed effects vector decomposition regression). With regard to the effect of sanctions on civil and political rights, Gutmann, Neuenkirch and Neumeier (2016) investigate this separately using the an aggregated "civil and political rights" indicator, comprised of the CIRI empowerment rights and Freedom House's civil liberties and political rights indicators, while Peksen and Drury (2010) and Soest and Wahman (2015) investigate these rights only as components of the broader democracy proxy of the Freedom House index.

Consequently, I would argue that it is essential to take into account comparability and consistency with previous research when making choices regarding data sources, model specification and variable operationalization. Furthermore, with regard to the study of the effect of economic sanctions on physical integrity rights and the broader indicators of civil and political rights, it is of fundamental interest to be able to assess the relative impact sanctions have on different categories of rights.

One method which ensures both consistency with previous research, as well as the ability to assess and compare the impact of sanctions on both physical integrity rights an civil and political rights, is the replication and expansion of an

existing study within this field. Replicating a core contribution assessing the effect of sanctions on physical integrity rights not only allows us to test the robustness of previous findings, but provides us with a methodological framework that can be extended to investigate the effect of sanctions on additional indicators of civil and political rights as well. Using similar research designs to investigate both categories of rights allows us to assess the relative impact of economic sanctions on different types of human rights, addressing a key weakness in this body of research.

In the following section I first provide a general argument for why replication of previous research is both an important and beneficial endeavour within the field of political science (or any scientific field, for that matter). I then provide arguments for why replicating Peksen's (2009) study, which assesses the effect of economic sanctions on physical integrity rights, is an ideal paper to replicate and expand upon in order to examine the broader impact of sanctions on human rights.

2.3 The argument for replication

The overall purpose of this thesis is to examine the effect of economic sanctions on human rights practices in targeted countries. Most quantitative analyses on this topic choose to focus on the most extreme cases of human rights violations such as torture and summary execution, commonly referred to as physical integrity rights. Overall, the findings are present a bleak picture, indicating that economic coercion is likely to increase repression in targeted countries. However, as I have argued above, there is reason to believe that economic sanctions have a similarly detrimental effect on the broader spectrum of civil and political rights. A few studies have already examined this relationship, but findings are inconsistent, and discrepancies in research design makes it hard to compare results. I therefore think the question is worth re-examining.

In this section I will outline why I believe the replication and expansion of a previous study on the effect of sanctions on human rights is perhaps the best way of answering the overall research question of this thesis. I begin by recounting

some of the core arguments for why replication, and (subsequent expansion) of existing research is an important and worthwhile endeavour. I then present some arguments for why I think replicating and expanding upon Peksen's 2009 paper, *Better or Worse? The Effect of Sanctions on Human Rights*, is the optimal way to address both the research question as well as key methodological concerns raised in this thesis.

General considerations

One of the pillars of the scientific endeavour is the cumulative progression of knowledge – a process contingent upon the ability to understand, evaluate and build on existing research. In his seminal paper on the importance of replication in the social sciences, King (1995) stresses the value of what is essentially “duplication” of existing research: “good science requires that we be able to reproduce existing numerical results, and that other scholars be able to show how substantive findings change as we apply the same method in new contexts. The latter is more interesting, but it does not reduce the necessity of the former” (King 1995: 451). This is facilitated first and foremost through the replication of previously conducted studies. “The only way to understand and evaluate an empirical analysis fully is to know the exact process by which the data were generated and the analysis produced” (King 1995: 444).

Replication is crucial to the further development of any discipline, King argues, since “the most common and scientifically productive method of building on existing research is to replicate an existing finding – to follow the precise path taken by a previous researcher, and then improve on the data or methodology in one way or another” (King 1995: 445). A key way to ascertain whether a published work contains enough information to do this, is to evaluate to what extent it adheres to the replication standard. The replication standard posits that sufficient information exists if a work can be replicated by a third party with only the published paper and information released with it – i.e. without any additional information from the author. As such, a replication data set should optimally include all components necessary to (re)produce the published results – including original data,

computer programs, recodes, extracts of publicly available data, explanatory notes describing what is included, and instructions on how to reproduce numerical results (King 1995: 446).

Unfortunately, many scholars fail to adequately adhere to this standard. A study conducted in the 1980s aimed to replicate all articles accepted to the *Journal of Money, Credit, and Banking*. The endeavour revealed not only numerous inadvertent errors in the published empirical work, but that replication of said work was often not possible even with the assistance of the original authors (King 1995: 445). Since King's article, the replication movement has gained some momentum. Following a symposium in *International Studies Perspective* in 2003, several leading quantitative journals pledged to set a minimum replication standard – including *Journal of Peace Research*, *International Studies Quarterly*, and *Journal of Conflict Resolution*. The minimum standard required that all data, specialized computer programs, analysis code, and an explanatory file detailing how published results may be reproduced should be included alongside the published work. Despite these measures, progress towards transparency remains slow (Gleditsch and Janz 2016: 362). The failure to adhere to the replication standard represents a serious problem that extends to all of the empirical international relations literature. Of what use are the conclusions of a book or article to its discipline, King asks, if its empirical basis cannot be reproduced (King 1995: 445)? The development of any discipline, political science included, is thus contingent upon adherence to the replication standard.

Consequently, retracing the entire process of a piece of empirical research, from data collection to model specification, is valuable for a number of reasons: It allows us to uncover (inadvertent) errors and inconsistencies that might be obscured if we rely solely on replication data that has already been collected and compiled; it permits the thorough testing of the original author's adherence to the replication standard; and, finally, it facilitates the thorough understanding of the empirical foundation for a study necessary to build on and improve the original research – the very foundation of the scientific endeavour to advance our collective knowledge. Given the slow progress towards transparency, conducting replication analyses is of particular importance to the international relations literature.

Which study to replicate?

As seen in Chapter 1, there is a considerably body of literature examining the effectiveness of economic sanctions – that is, to what extent they achieve the goals they set out to accomplish (Pape 1997; Hovi, Huseby and Sprinz 2005). Parallel to this research, a growing body of work has set out to examine the inadvertent secondary effects of employing economic coercion to achieve policy goals, with a subset focusing on the effect of sanctions on human rights practices specifically. Some examples include Li and Drury's (2004) study showing that USA's threat to remove China's most favored nation status failed to improve human rights practices, even contributing to their further deterioration; Lopez and Cortright's (1997) study indicating that UN sanctions against Iraq inadvertently led to worse human rights conditions through the hardship they imposed on civilians; and Gibbons' (1999) study of how the US-led sanctions against Haiti between 1991-1994 led to the further deterioration of civil and political rights (Peksen 2009: 60-61; Peksen and Drury 2009: 396).

Few studies, however, have provided comprehensive cross-national quantitative empirical analyses into the unintended consequences of economic coercion on human rights in targeted countries. To my knowledge, there are three key peer-reviewed papers fitting this description: Dursun Peksen's *Better or Worse? The Effect of Economic Sanction on Human Rights*, Reed M. Wood's "A Hand upon the Throat of the Nation": *Economic Sanctions and State Repression, 1976-2001*, and Escribà-Folch's *Authoritarian Responses to Foreign Pressure: Spending, Repression, and Sanctions*.

All three examine the effect of economic sanctions on state-sponsored repression of physical integrity rights, and all find that imposing sanctions negatively impact human right conditions in targeted countries.⁷ Furthermore, all studies take a comprehensive approach, examining extensive cross-national, time-series data on economic sanctions, albeit from partly differing sources (Peksen 2009; Wood 2008;

⁷Note that all three analyses only include *imposed* sanctions, not cases where sanctions have merely been threatened.

Escribà-Folch 2012.)⁸

At the time of writing, Peksen, Wood, and Escribà-Folch's respective articles appear to be the only comprehensive cross-national, time-series analyses of the effect of sanctions on physical integrity rights. However, the studies differ somewhat in terms of their impact, indicated by their respective number of citations: at the time of writing Wood and Peksen's papers have each been cited 139 times; Escribà-Folch has been cited 49 times⁹. The more profiled and cited a work is, the larger impact it arguably has on subsequent research, and thus on the field itself. Insofar we accept King's arguments for the importance of replication of previous research, particularly influential works, both Peksen and Wood's studies stand out as the most suitable candidates to be replicated. As Escribà-Folch's article is comparatively less prominent, and has some similar research design choices to Wood's paper, I will not consider it further here.

On the basis of number of citations, both Peksen and Wood's articles appear to be relatively influential works. In terms of serving as a basis for the subsequent expansion of the replication to include broader indicators of civil and political rights, however, I would argue that Peksen's contribution provides a three key benefits that gives it an edge over Wood's study.

The first point concerns the choice of dependent variables, i.e. which indicators of human rights violations Peksen and Wood choose for their respective studies. Wood relies solely on the Political Terror Scale (PTS) measure of physical integrity rights violations. The PTS is based on data from the US State Department and Amnesty International's annual *The State of the World's Human Rights* report, and consists of a five-point categorical index measuring state-sponsored violation of physical integrity rights (Gibney et al. 2016). A high score indicates that all citizens are subject to severe violations of physical integrity rights, political executions and/or torture are commonplace, and leaders place no limits on means or thoroughness with which they pursue personal, political or ideological goals (Wood

⁸It is noteworthy that neither Wood nor Peksen make any references to the other paper. However, this is likely due to the fact that both papers were published roughly around the same time, and in different journals.

⁹Citation numbers are all taken from Google Scholar.

2008: 499; Wood, Reed and Gibney 2010). Peksen, on the other hand, relies primarily the physical integrity rights indicators from the Cingranelli and Richards (CIRI) Human Rights dataset.¹⁰ Like PTS, the CIRI variables are also based on State Department and Amnesty International reports. However, the CIRI provides disaggregated indicators of individual physical integrity rights in addition to an aggregated index measure (Peksen 2009: 64).

A comparative disaggregation of the PTS indicator is not possible. As a result, the CIRI indicators allows for a more fine grained analysis of individual rights, while at the same time offering an aggregated index variable for examining effects on integrity rights as a whole similar to the PTS. Additionally, as mentioned above, the CIRI empowerment rights indicators have been linked to the theoretically defined category of civil and political rights through Gutmann and Voigt's PCA (Gutmann and Voigt 2015). Building on Peksen's research design for my replication and expansion consequently allows me to address the aforementioned concern regarding the use of index variables, by performing my analysis on both aggregated and individual variables, as well as ensuring comparability and consistency across my models by drawing both physical integrity rights and civil and political rights indicators from the same dataset.

The second point concerns the choice of data sources. Wood compiles his sanction data exclusively from the Hufbauer, Schott, and Elliott (HSE) dataset (Hufbauer, Schott, Elliott, and Oegg 1990) and their affiliated Institute for International Economics (2005). The HSE dataset is widely used in the quantitative sanctions literature, containing data on implemented sanctions going back to 1914. Peksen, on the other hand, relies primarily on the more recent Threat and Imposition of Sanctions (TIES) dataset, released in 2003, which contains sanction cases between 1971 and 2000 (Morgan, Krustev and Bapat, 2006). Peksen also draws on HSE data to supplement missing cases in the TIES dataset (Peksen 2009: 66). TIES is an arguably richer source of sanctions data, not least because TIES was created specifically to account for shortcomings of the HSE dataset such as a limited number of sanctions episodes. I will outline the HSE and TIES datasets in more detail

¹⁰Peksen also runs a separate analysis using the PTS as his dependent variable, to check the sensitivity of his findings to data specifications (Peksen 2009: 64-65).

in the chapter on research design. For now it is sufficient to point out that Peksen's reliance on both TIES and HSE cases for his sanction data makes his study the better candidate for replication and expansion, since it accounts for a wider set of sanction episodes than Wood's analysis.

The third point is closely related to the second, and concerns the availability of new pertinent data. The TIES dataset was updated in 2013 to cover additional sanctions episodes between 1945-1970 and 2001-2005. Even with the limitations on scope imposed by the CIRI dataset (covering 1981 and onwards), the additional five years represents a 25% increase in the timespan covered in Peksen's analyses using the CIRI indicators. As such, making use of the additional data would represent a substantial extension of what is at the outset a relatively limited period of analysis. The value of additional observation years is particularly salient in the case of the research topic at hand since, as Peksen states himself, "the change in state practices towards human rights is a slow process" (Peksen 2009: 68; Poe and Tate 1994). Relying on Peksen's study for my replication and expansion thus allows me to draw on both the most extensive and most recent pertinent data on economic sanctions to date.

Based on the arguments outlined above, I have chosen to base my replication and subsequent expansion of data and dependent variables on Peksen's paper. The next section outlines the structure of my analysis and my methodological choices, with reference to the considerations discussed so far in this chapter.

2.4 Structuring the analysis

During the course of this chapter, I have argued that a thorough study of the effect of sanctions on human rights could benefit from including broader categories of human rights – specifically rights generally referred to in the literature as "civil and political rights", "civil liberties", or "empowerment rights", and which are commonly used in aggregated proxies of levels of democracy. Furthermore, I argued that such a study should strive to be consistent with the data sources, model specification and operationalization of previous research, in order to ensure com-

parability of results across studies. I also indicated that, where this is feasible, studies of human rights conditions in general should include both aggregated and individual indicators, to account for both overall and rights-specific effects.

The previous section suggested that replication and expansion of an existing, key study from the literature could serve as a solid starting point for conducting such an analysis. I raised Peksen's quantitative analysis of sanctions' effect on physical integrity rights as the most promising candidate for replication and expansion, as it allows me to address all of the considerations above. The replication itself ensures consistency with Peksen's central contribution, and gives me a basic methodological framework I can re-apply in subsequent analyses. Applying the same research design across models examining both physical integrity rights and broader indicators of civil and political rights, ensures that results can feasibly be compared across the board.

Peksen's reliance on the CIRI human rights dataset for data on rights violations ensures that I can draw both physical integrity rights indicators and broader civil and political rights indicators (referred to in CIRI as "empowerment rights") from the same dataset, further bolstering comparability. CIRI also represents the currently only opportunity for analysing both aggregated and individual indicators of human rights violations. Finally, Peksen's reliance on TIES would allow us to take advantage of new, relevant data on sanctions through the updated TIES dataset. In combination with HSE, the updated TIES yields the largest possible scope of available sanctions data to date.

Having established the central priorities above, I can begin setting the structure of the analysis in this thesis. Based on the considerations discussed in this chapter, I propose a three-stage analysis for this thesis: a replication analysis reproducing the dataset and results from Peksen's original study; an expansion of Peksen's dataset and subsequent analysis to assess whether his conclusions hold when accounting for a wider range of relevant data; and finally, an examination of the effect of sanctions on individual and aggregated indicators of a broader set of civil and political rights, operationalized by way of CIRI's "empowerment rights" variables. Chapter 3 presents my research design and methodological framework in more detail.

At this point it is worth clarifying some central points regarding terminology. Throughout this chapter I have referred to research on both "civil rights", "civil and political rights" and "empowerment rights" – within the relevant literature, these generally refer to the same general category of rights (encompassing for example freedom of speech, worker's rights and religious freedom). By relying on the CIRI human rights dataset, I restrict myself to examining the subset of civil and political rights contained in the aggregated CIRI empowerment rights variable and its individual components. The CIRI empowerment rights encompass, in total, freedom of foreign and domestic movement, freedom of speech, freedom of assembly and association, worker's rights, electoral self-determination, and religious freedom.

From here on I will use the term "empowerment rights" to refer to the subset of civil and political rights which is included in this study. For the purpose of my analysis, the CIRI empowerment rights will serve as the operational definition of the broader set of civil and political rights discussed in section 2.2. I discuss the operationalization of the dependent variable in the third stage of my analysis in some more detail in section 3.4 below.

Having established the overall structure of my analysis, and clarified the application of terms denoting human rights categories, I can now form the general hypotheses which will guide the assessment and evaluation of the *results* from the analysis.

2.5 Hypotheses

As indicated above, the analysis will proceed in three stages: replication of Peksen's study; expansion of the dataset and re-estimation of the models his original study, to account for newly available data; and an expansion of the object of study to encompass civil and political rights, with the CIRI empowerment rights as my operational definition.

With regard to the replication analysis, I do not expect to get results that are markedly different from Peksen's - this would indicate that the replication has

been unsuccessful, or that Peksen's actual research design does not correspond to the one he presents in his paper.¹¹ Furthermore, for the expanded study in stage two, I will make no substantial changes to Peksen's research design beyond the inclusion of additional sanctions data. Consequently, I expect the results of the analysis with the expanded dataset to yield similar results to Peksen's original analysis. Peksen's research hypothesis therefore remains unaltered for the first two stages of the analysis in this thesis¹²:

*H₁: Economic coercion will likely cause the unintended consequence of more physical integrity rights violations in target countries.*¹³

The third stage of the analysis will examine the impact of economic sanctions on the CIRI empowerment rights indicators. I have argued that the theoretical mechanisms proposed by Peksen, outlined in section 3.2, also apply to broader categories of civil and political rights. As such, the theoretical expectations are similar to those relating to physical integrity rights, and applies to both the individual and aggregated indicators of empowerment rights employed in the study:

H₂: Economic coercion will likely cause the unintended consequence of more empowerment rights violations in targeted countries.

Finally, I am interested in examining the *relative* impact of economic sanctions

¹¹I will of course assume the former should results differ significantly, giving the original researcher the benefit of doubt.

¹²I have changed the original wording of the hypothesis from "human rights violations" to "physical integrity rights violations" to emphasize the contrast between *H₁* and *H₂*.

¹³For the sake of consistency, I choose to follow Peksen's original wording for my hypotheses. However, it must be noted that the causal phrasing can be slightly misleading, both because of issue of possible endogeneity raised earlier in the chapter, but also because what we are studying with an ordered regression model is the *marginal effects* of explanatory variables on the outcome variable. That is, we examine the change in the conditional probability of a specific outcome when we change the value of an independent variable, holding all other variables constant – not a direct causal relationship, per se.

on the categories of physical integrity rights and empowerment rights. However, since this is a topic that has not been examined in detail in the existing literature, I have few theoretical expectations regarding the comparative impact of economic sanctions on these rights. It is likely, however, that the impact of sanctions will not be greater for empowerment rights than for physical integrity rights, given the ambiguous results from the literature. I therefore propose the following pair of hypotheses:

H_{3a}: The impact of economic coercion on empowerment rights is equivalent to the impact of economic coercion on physical integrity rights.

H_{3b}: The impact of economic coercion on empowerment rights is lesser the impact of economic coercion on physical integrity rights.

2.6 Summary

In this chapter I have provided a broad outline of the relevant research literature examining the effect of economic sanctions on human rights violations in targeted countries. Overall, the general consensus appears to be that economic sanctions exerts a negative influence on human rights, with the adverse impact extending to both physical integrity rights and broader indicators of human rights (commonly encompassed by aggregated indicators of democracy levels).

In section 2.2., however, I highlighted a few fundamental concerns regarding this body of research, such as divergent conclusions regarding sanction effects, the difficulty of comparison across studies, and a heavy reliance on aggregated indicators of civil and political rights violations. Taken together, I argued that these issues provide ample reason to re-examine the impact of sanctions on human rights in general, and on the wider category of human rights (beyond physical integrity rights), specifically.

In order to examine the effect of sanctions on both the most egregious physi-

cal integrity violations, as well repression of wider civil and political rights, have proposed a three-stage analysis structure. First, I conduct a replication analysis reproducing the dataset and results from Peksen's 2009 study on the effect of economic sanctions on physical integrity rights violations. Second, I expand Peksen's dataset and re-estimate his models to assess whether his conclusions hold when accounting for a wider range of relevant sanctions data. Finally, I examine the effect of sanctions on both individual and aggregated indicators of a broader set of civil and political rights, operationalized by way of the CIRI "empowerment rights" variables.

I have argued that this approach is best suited to examine the research question in this thesis, as it addresses the key concerns of consistency and comparability across studies, provides the opportunity to disaggregate central human rights variables to assess relative impact of sanctions, and allows me to draw on the widest possible range of available data on economic sanctions.

The next chapter presents my research design choices and methodological framework in more detail, and discusses the challenges I faced when attempting to reconstruct Peksen's dataset for my replication analysis. The chapter also provides a general discussion of the ordered regression models, which I apply throughout all stages of my analysis in this thesis.

Chapter 3

Research Design

The analysis conducted in this thesis proceeds in three stages. First, I replicate Peksen's analysis, using both his original dataset and a new, reconstructed dataset rebuilt according to the specifications in Peksen's paper. Second, I expand the reconstructed dataset to include sanction cases between 2000-2005, using newly available data from the updated TIES dataset. Third, I expand the scope of Peksen's original study to include indicators for a wider range of human rights, specifically civil and political rights.

The following chapter consists of four sections. The first section presents a detailed overview of Peksen's research design, as well as a short clarification with regard to the definition of economic sanctions used in his study.

The second section presents a detailed outline of the methods and choices I have applied in replicating Peksen's analysis - this includes challenges I encountered during the process of rebuilding the dataset and the solutions I have proposed, the exact method by which specific variables were reconstructed, and the discrepancies between my dataset and Peksen's. The first section also contains a brief overview of how the reconstructed dataset was subsequently expanded to include a broader set of observations, and the central considerations underpinning the choice of which observations to include in the expanded set.

The third section details the process of expanding the scope of the study to include indicators of civil and political rights. I present and justify the operational

definition of civil and political rights employed in this thesis, and give a detailed description of all eight additional dependent variables included in the analysis.

The fourth section outlines the reasoning behind using the ordered probit model for all analyses in this thesis, and presents a brief overview of the ordered regression models, their use, and how results from these models can be interpreted.

3.1 Peksen's research design: overview

To test his hypothesis that economic coercion will likely cause the unintended consequence of more human rights violations in target countries, Peksen uses cross-sectional time series data on economic sanctions and physical integrity rights violations. He compiled the sanctions data from two sources - the Hufbauer, Schott and Elliott (HSE) and Threats and Imposition of Sanctions (TIES) datasets. TIES is used as the primary source, with missing cases supplemented from HSE (Peksen 2009: 65-66). Data on physical integrity rights violations are taken from the Cingranelli and Richards (CIRI) human rights dataset and the Political Terror Scale (PTS) dataset (Cingranelli, Richards, and Clay 2014; Gibney 2005).

The time period of the analyses was determined by availability of data at the time of writing. The original TIES contained sanctions data between 1971-2000, with CIRI and PTS data available from 1981 and 1975, respectively. Thus the period of analysis was restricted to 1981-2000 for models employing the CIRI indicators of human rights violations, and 1975-2000 for models using PTS indicators. To control for possible endogeneity in the model, Peksen employs two measures: a) the inclusion of a lagged ($n-1$) version of the relevant human rights indicator as a control variable, and b) the restriction of the analysis sample to countries with a relatively similar tendency to commit human rights violations. The latter is ensured by limiting the sample to countries that have faced the threat and/or imposition of sanctions during the analysis period (Peksen 2009: 64). The analysis sample also excludes sanctions cases from TIES where threats preceded implementation (because of missing onset date for sanction imposition in the TIES dataset), and sanctions cases over trade or environmental disputes. Trade and environmen-

tal sanctions were excluded based on the argument that these do not lead to “substantial political and economic consequences for target countries” (Peksen 2009: 65).¹ Finally, Peksen’s analysis sample contains a total of 95 countries (Peksen 2009:64).

As all the dependent variables in the study are ordinal variables with three or more categories, Peksen chooses to report the models using ordered probit regression. Robust standard errors were obtained by including Huber/White sandwich estimators of variance, clustered on country code and with non-independence assumed within clusters (Peksen 2009: 63-64,68).

The rest of this section presents a detailed overview of all variables included in Peksen’s analysis. First however, it is necessary to briefly comment on the definition of economic sanctions Peksen employs in his study.

Defining sanctions

Peksen, citing Hufbauer, Schott and Elliott, defines economic sanctions somewhat broadly as “trade and financial coercion such as export restrictions, investment bans, asset freezes, reduction or suspension of military aid, restrictions on limited dual-use technologies, and travel bans on target countries’ officials” (Peksen 2009: 66). However, due to Peksen’s heavy reliance on the TIES dataset, I would argue that the exhaustive definition should be assumed to include all available sanction categories listed in the TIES dataset codebook. This definition includes the following sanction types: total and partial economic embargo, import and export restrictions, blockades, asset freezes, termination of foreign aid, travel bans, and suspension of economic agreements (Morgan, Krustev and Bapat 2006: 5-6; Morgan, Bapat and Kobayashi 2013:10). I choose to assume that the latter definition informs Peksen’s research design, as the former definition would imply that some sanction categories in TIES have been excluded - I find no evidence that this is the

¹Peksen does not, however, appear to offer up any substantial theoretical arguments for their exclusion (Peksen 2009:).

case.²

Dependent variables

Peksen does not explicitly define physical integrity rights in his study, instead deferring to two widely employed measures of physical integrity abuses to conceptualize these sets of rights. The main measure is taken from the Cingranelli and Richards (CIRI) Human Rights Dataset, which provides quantitative data on government respect for a total of 15 individual human rights for over 200 countries in the period 1981-2011. Peksen employs four different physical integrity rights variables from CIRI, including extrajudicial killings, disappearance, political imprisonment, and torture. In addition, he also includes an additive index variable, the Physical Rights Index, composed of all four variables. All integrity rights variables are recoded such that high values indicate a higher level of abuse. Variable descriptions have been taken from the CIRI coding manual (Cingranelli and Richards 2014).

Disappearance refers to cases where “people have disappeared, political motivation appears likely, and the victims have not been found. Knowledge of the whereabouts of the disappeared is, by definition, not public knowledge. However, while there is typically no way of knowing where victims are, it is typically known by whom they were taken and under what circumstances.”

The variable is ordinal with values ranging from 0-2. A value of 2 indicates frequent violations (50+ incidents), 1 indicates some violations (1-49 incidents), and 0 indicates no violations of this right in a given year. All the individual CIRI physical integrity variables are coded in this manner (Peksen 2009: 65).

Extrajudicial killings refers to killing performed “by government officials without due process of law. They include murders by private groups if instigated by

²I also choose to employ this latter definition of economic sanctions for my own analyses in this thesis.

government. These killings may result from the deliberate, illegal, and excessive use of lethal force by the police, security forces, or other agents of the state whether against criminal suspects, detainees, prisoners, or others.”

Political imprisonment refers to “the incarceration of people by government officials because of: their speech; their non-violent opposition to government policies or leaders; their religious beliefs; their non-violent religious practices including proselytizing; or their membership in a group, including an ethnic or racial group.”

Torture refers to the “purposeful inflicting of extreme pain, whether mental or physical, by government officials or by private individuals at the instigation of government officials. Torture includes the use of physical and other force by police and prison guards that is cruel, inhuman, or degrading. This also includes deaths in custody due to negligence by government officials.”

Physical integrity rights index is an additive index consisting of the CIRI indicators of disappearance, killings political imprisonment, and torture, outlined above. Values range from 0 to 8, with 8 indicating no government respect for these rights and 0 indicating full government respect for these rights (Cingranelli, Richards, and Clay 2014).

The second measure of physical integrity abuses is taken from the Political Terror Scale (PTS) project. PTS was originally developed as a tool for allowing the empirical tests of whether US foreign aid was being provided to countries violating human rights standards, and included 59 countries. The data has since been expanded to encompass all countries, as well as a range of political issues outside of US foreign aid and refugee protection. As seen in section 2.3, however, the PTS measure can not be disaggregated into separate indicators. Consequently, it is not suitable for the analysis in this thesis, and Peksen’s model using it is not included in the replication.³

³Omitting this model is not of critical importance in any case, as Peksen states that he included

Independent variables

As mentioned in section 2.3., Peksen compiled his data on economic sanctions from two main sources – the frequently employed Hufbauer, Schott and Elliott (HSE) dataset, and the more recent Threats and Imposition of Sanctions (TIES) dataset. The sanction data from TIES was supplemented with HSE data for all imposed sanctions cases missing from TIES or implemented before 1971 and continued during 1981-2000 (Peksen 2009: 65-66).⁴

Peksen includes six independent sanction variables in his analysis. Besides a general variable covering all relevant sanctions cases, he also includes two variable pairs accounting for human rights/non-human rights sanctions and multilateral/unilateral sanctions. The purpose is to allow for investigation into whether sanction goals and extent of participation affects the effects on human rights violations. All variables except sanction years are ordinal variables, with values ranging from 0-2. A value of 0 indicates that a country was under no sanctions in a given year, a value of 1 that it was under partial sanctions, and a value of 2 that it was under extensive sanctions. Extensive sanctions include cases where all economic and financial transactions between sender and target countries are cut completely. Partial sanctions include cases where limited sanctions have been imposed, such as investment bans, asset freezes, partial export restrictions, reduction or suspension of arms exports, constraints on various forms of aid, and travel restrictions (Peksen 2009: 66). The independent variables are as follows:

Economic sanctions includes all types of imposed sanctions, indicating whether a country was subject to extensive, partial or no sanctions in a given year.

Human rights sanctions indicates whether a country was, in a given year, subject to extensive, partial or no sanctions imposed with the explicit goal of preventing human rights abuses. Conversely, *non-human rights* sanctions indicates whether a

it as a measure to test the robustness of his other findings (Peksen 2009: 65).

⁴Section 3.2 below provides a more in-depth overview of both datasets, how they differ, and the ways in which TIES improved upon HSE.

country was, in a given year, subject to extensive, partial or no sanctions imposed with the policy goals unrelated to human rights abuses.

Multilateral sanctions indicates whether a country was subject to extensive, partial or no sanctions imposed by the UN or other major regional organizations. Conversely, *unilateral sanctions* indicates whether sanctions were imposed by individual countries independent of international organizations.

Sanction years indicates how long imposed sanctions have been in place in a given year. The natural logarithm of the number of years is employed in order to account for curvilinearity in the data.

Control variables

Peksen includes five control variables, meant to account for possibly confounding variables affecting human rights as suggested by the research literature. These include a measure of the level of democracy in the target country, its GDP per capita, whether it is involved in a civil war or an interstate war in a given year, and finally a measure indicating past practice with regard to human rights violations. Below I provide an overview of the operationalization of each variable, as well as a short summary of Peksen's argument for the inclusion of each one (Peksen 2009: 67-68):

Democracy indicates to what extent a given country is democratic or not, and is included to control for the independent effect of regime type on human rights repression. Peksen's measure of democracy is taken from the PolityIV dataset, where each country is given a democracy score ranging from -10 to 10. A score of -10 indicates that the country is completely autocratic, while a score of 10 represents the highest level of democracy. The variable is included to account for the expectation that democracies commit fewer human rights abuses, largely due to the constraining effect of numerous institutional mechanisms on regime power (Mitchell and McCormick 1988; Henderson 1991; Davenport, 1995, 1999; Poe and

Tate 1994; Krain 1997; Poe, Tate and Keith 1999; Zanger 2000).

GDP per capita is a measure of indicating the natural logarithm of the per capita income level in 1995 constant US dollars. The variable is based on data from the World Bank's World Development Indicators, and is included because citizens in less economically developed countries are expected to be more at risk of experiencing violence and political instability. Furthermore, regimes in these countries are assumed to be more likely to use repressive measures (Mitchell and McCormick 1988; Henderson 1991; Poe and Tate 1994; Krain 1997; Poe, Tate and Keith 1999; Zanger 2000; Keith 2002).

Civil war indicates whether or not a country is engaged in civil war in a given year. The data is taken from the Armed Conflict Dataset from the Oslo International Peace Research Institute (PRIO). The variable is coded 1 if the country is engaged in civil war with at least 25 battle-related deaths per year, and 0 otherwise.

Interstate war indicates whether or not a country is engaged in interstate war in a given year. Like the civil war variable, the data is taken from the Armed Conflict Dataset from the Oslo International Peace Research Institute (PRIO). The variable is coded 1 if the country is engaged in interstate war with at least 25 battle-related deaths per year, and 0 otherwise. Both civil war and interstate war are included to account for studies indicating that involvement in conflict correlate positively with increased human rights violations (Mitchell and McCormick 1988; Henderson 1991; Davenport 1995 1999; Poe and Tate 1994; Poe, Tate and Keith 1999; Zanger 2000).

Past practice is a one-year lag of the dependent variable, indicating human rights practice in the preceding year. This variable is included to control for autocorrelation along with possible issues with endogeneity. I have already stressed in section 2.1 that endogeneity issue are plausible when studying the effect of sanc-

tions on human rights, as human rights practice in a country in a given year is likely a significant predictor of practice in the following year (Peksen 2009: 68).

It is worth noting that these control variables largely overlap with what scholars have dubbed the “usual suspects” predicting state repression, including democracy, economic development, and state involvement in internal conflicts (Hill 2010: 1166-1167).

While not stated explicitly, an additional motivation for including these control variables is the possibility of correlation with other independent variables in the model. For instance, autocratic countries and countries embroiled in civil wars are more likely to face sanctions than democratic, domestically stable states.⁵ Insofar we expect control variables to correlate with the dependent and/or independent variables, omitting them would introduce bias to the model.

3.2 Reconstructing Peksen’s dataset

In this section I first provide a rationale, based on King (1995), for why the replication of Peksen’s analysis should also involve the complete reconstruction of his dataset based on original data sources. I then provide an overview of the HSE and TIES datasets, and how they differ, before discussing some of the key challenges I was confronted with during the reconstruction process and how I chose to address them. I conclude this section with a brief discussion of some apparent discrepancies between Peksen’s research design and his dataset.

Why reconstruct the dataset?

King asserts that “the only way to understand and evaluate an empirical analysis fully is to know the exact process by which the data were generated and the analysis produced” (King 1995: 444). This implies that a full replication does not

⁵Drawing on examples from the HSE dataset, sanctions were imposed in direct response to civil war in 1990-1993 (US vs El Salvador), 1991-2001 (UN vs Yugoslavia), and in 1993-2002 (UN vs Angola), to name a few examples.

only consist of reproducing the results of the original analysis (employing the same model specifications), but also the complete reconstruction of the dataset and relevant variables using primary data sources. To ensure these two criteria are fulfilled, the replication analysis will proceed in two stages: first, I reproduce the exact results of Peksen's original study, using the replication dataset provided alongside the published article. This involves the *exact* replication of summary statistics tables, correlation coefficients, standard errors, and central goodness-of-fit measures, as well as calculation of predicted probabilities used to interpret the output from the probit models. While this fulfils the first criteria of a full replication (the analysis process), it does not clarify the process by which the data was generated or how the relevant variables in Peksen's dataset were constructed. To ensure that both criteria are fulfilled, it is necessary to rebuild the replication dataset (including relevant variables) as accurately as possible, drawing on the same primary data sources as Peksen's study, and re-estimate the models with the new dataset.

Structuring the replication analysis in this way achieves two main benefits. First, reproducing the exact results from the original study provides me with a clear baseline with which to compare the results of the analysis using the rebuilt dataset. While it is unrealistic to expect the rebuilt dataset to reproduce the results from Peksen's analysis exactly, it is nonetheless essential that the replication analysis yields the same *substantive conclusions* as the original. Running Peksen's models on both datasets and presenting them together allows me to systematically determine to what extent the replication is successful.

Second, performing the analysis with Peksen's dataset first allows me to determine the exact method he used to generate his results, which I can then re-apply in my own analysis. This process ensures that all subsequent analyses are consistent with Peksen's methodology, and that whatever differences arise will be due to discrepancies in either the datasets themselves or in the way the variables are constructed.

Below I provide an overview of the process of rebuilding Peksen's dataset from scratch, including specific challenges and how I chose to address them. I also provide a detailed overview of how I reconstructed each individual variable from Peksen's original study.

Before that, however, it is instructive to look a bit more closely at the TIES and HSE dataset, the differences between them, and how the updated TIES dataset differs from the older version.

The TIES and HSE datasets

As indicated in section 2.3, the majority of the sanctions cases in Peksen's dataset are taken from the first iteration of the Threats and Imposition of Sanctions (TIES) dataset (Morgan, Krustev and Bapat, 2006). The first TIES dataset was released in 2003, and included 888 cases in which sanctions were threatened and/or implemented between 1971-2000. The dataset was developed largely as a response to emerging theoretical arguments which could not be adequately tested using the existing data on economic sanctions. More specifically, TIES addressed some shortcomings of the frequently used Hufbauer, Schott, and Elliott (HSE) dataset on economic sanctions, to account for recent theoretical developments (Hufbauer, Schott, Elliott 2009: 93).

Whereas HSE only includes cases where sanctions were implemented, TIES deliberately set out to include cases where sanction had been threatened, threatened and implemented, or simply implemented without threats. A consequence is that TIES includes a significantly larger number of cases than HSE. The HSE dataset covers sanctions cases between 1914 and 2006, but contain only 204 sanctions cases, compared to 888 cases in the TIES dataset. Of the 204 cases, 139 occur in the period covered by TIES (Hufbauer, Schott, and Elliott 2009: 98). The discrepancy in case numbers can be partly attributed to the TIES' inclusion of threats-only cases, which account for 361 of the 888 cases. That leaves 527 cases of implemented sanctions in TIES, which is still comparatively high.

There are two main reasons for the high number. First, the TIES dataset is simply more thorough, having made a concerted effort to include low profile, lesser known and less obvious cases than those included in HSE. In addition, TIES also includes sanctions over economic issues (e.g. retaliatory tariffs). HSE excludes all trade sanctions cases, on the basis of the theoretical position that only sanctions over political issues should be included in evaluations of sanctions effectiveness

(Hufbauer, Schott, and Elliott 2009: 94).

Despite its important contribution to the data on sanctions, the original TIES dataset had some crucial shortcomings. Most importantly, the time period covered was relatively short (30 years), meaning that several important, long-term sanctions episodes initiated before 1970 were left out (e.g. USA vs Cuba, Rhodesia, South Africa). An updated TIES dataset was released in 2013 to address some of the most pressing issues with the data (Morgan, Bapat, and Kobayashi 2014: 2). The new version of TIES covers the period from 1945 to 2005, and includes a total of 1412 cases where sanctions were threatened and/or imposed on a single target state. In addition, the new dataset corrects a number of errors in the previous dataset, and updates cases that were ongoing as of the previous release. Finally, some adjustments were made in the forms of new variables, changes in coding for some existing ones, as well as the removal of some contentious cases from the dataset (Morgan, Bapat, and Kobayashi 2014: 2).

The most significant changes from the old dataset can be summarized follows: First, a variable was included that identifies the onset of sanctions in cases where threats preceded the imposition of sanctions. Where a case involved both threat and imposition of sanctions, the original dataset only provided the start date for the case (i.e. when the threat was issued).

Second, for cases with missing end dates, the new dataset includes a variable specifying the last date for which there are records indicating that the case was still ongoing.

Third, the updated dataset changed how several variables that could take more than one value for each case were handled – e.g. variables indicating who made the sanction threat, what issues the sanctions pertained to, what type of sanction was implemented, etc. Previously only one value could be recorded for each case. In the new dataset, all possible values can now be listed. For example, a sanction case can now concern multiple issues, whereas earlier only the most “important” issue was listed.

Fourth, the new TIES includes two dichotomous variables (threat and imposition) which identifies whether an individual case included a threat, and whether

or not sanctions were actually imposed.

Finally, cases involving Hong Kong and Macau were omitted in the updated version of TIES, as these are not considered members of the international system (according to the Correlates of War Project). Both were included in the original dataset.

Challenges and solutions

During the early process of writing this thesis, I corresponded briefly with Dursun Peksen, primarily in order to ask for access to log files not included with the replication data published alongside his article. By log files, I am referring to scripts or .do-files detailing the explicit commands or codes used in the statistical computing software employed for data compilation, manipulation and analysis. While he was gracious enough to share with me the log files detailing the models in his analysis, I was unable to get access to logs detailing dataset and variable construction. The request was not denied outright - rather, repeated requests for such logs did not receive a response. I can only assume that these files are no longer available, that my (subsequent) correspondence did not reach Peksen, or that circumstances did not permit the files to be shared.⁶

A consequence of these circumstances is that I have been forced to rely to a larger degree on conjectures in reconstructing Peksen's dataset, particularly where details regarding the data compilation process are either vague or have been omitted altogether. This particularly concerns the construction of the independent variables, as Peksen does not state explicitly how these were coded with reference to his data sources. These issues may have contributed to some subtle differences and discrepancies between the original and reconstructed datasets (for instance, between high and medium scores on independent variables). I have endeavoured to include both descriptions and explanations for all significant differences between the datasets below. By significant differences, I mean discrepancies that may lead to deviations in results between the original and the replication analyses.

⁶I would like to take the opportunity to thank Peksen for taking the time to respond to my inquiries, and for having shared with me the relevant information that was accessible and available.

I should also clarify that I have been primarily concerned with replicating the *analysis sample* from Peksen's study – that is, the subset of the complete replication dataset containing only the observations included in the analysis. Peksen delineates his analysis sample by way of the dichotomous *domain* variable. *Domain* takes the value 1 for all observations for a given country if the country has faced the threat and/or imposition of sanctions at some point during the analysis period (i.e. 1981-2000), and 0 otherwise. This is consistent with his argument that in order to avoid possible selection bias (and, though he does not mention this, possible endogeneity issues), the analysis should contain countries with relatively similar tendencies to commit human rights violations (Peksen 2009: 63-64). I construct a corresponding variable to delineate the analysis sample from the reconstructed dataset. Note that all subsequent references to both Peksen's dataset and the reconstructed dataset below refer to the *analysis sample* – or, the subset of observations with a score of 1 on *domain*.

I relied solely on data from the updated version of TIES for all the analyses in this thesis, including the replication analysis. It could be argued that strict adherence to Peksen's methodology would dictate that I use the original TIES to construct the replication dataset. However, extracting cases between 1981 and 2000 yields a subset corresponding almost perfectly to the original TIES data - additional observations included by this method were identified and removed for the replication analysis by cross-checking the subset with Peksen's data. Cases omitted from the new TIES, like those involving Hong Kong and Macau, were not originally included in Peksen's dataset. Consequently, their omission from the updated TIES data does not represent an issue. Furthermore, using the updated TIES throughout all my analyses yields the practical benefits of ensuring consistency across models and making the subsequent expansion of data somewhat easier.

It is also worth noting that individual observations in the TIES data are organised in terms of sanctions cases, with unique case IDs, start and end dates, sender states and target states, etc. It was therefore necessary to adjust the data to get the country-year structure of Peksen's dataset. I did this by first creating country variables, based on target state, and then by creating observations rows for every year in which a given country faced a sanction. If a sanction case spanned mul-

multiple years, I duplicated all variable values for the sanction case for each relevant country-year observation. In the cases where one country faced multiple sanctions in a given year, I gave preference to more extensive sanctions and/or multilateral sanctions, as per Peksen's specifications, such that the new dataset contained only one observation per country per year (Peksen 2009: 66).

I extracted all relevant cases between 1981 and 2000 (and later 2005) by means of start- and end-dates for each sanction case. For cases where the end-date was missing, the last year in which the sanction was registered as ongoing was used instead.

As I mentioned in section 2.3, Peksen also supplemented sanctions cases missing from TIES with cases from the HSE dataset. This presented me with the challenge of how to identify and incorporate the relevant HSE cases. I had to ensure that the variable coding of the additional cases corresponded to the coding in my dataset – HSE and TIES coding does not match, and Peksen does not explicitly state how he distinguishes between limited and extensive sanctions for the HSE cases. I chose to solve the issue in the following manner: first, I identified missing cases by comparing country-year observations from Peksen's analysis sample with the relevant subset of TIES cases (i.e. all observations between 1981-2000 for countries that had faced sanctions at least once during this period). I then manually cross-referenced all observations from Peksen's dataset missing from the TIES sample with the complete list of HSE sanctions (Hufbauer, Schott, Elliott, and Oegg 2008). Where the missing observations corresponded to HSE sanctions, I extracted said observations from Peksen's dataset into my own. These included full sanctions cases, as well as missing country-year observations supplementing TIES case data.

In addition to adding missing cases, I used the method above to identify countries included in the TIES subset that were not present in Peksen's dataset (i.e. with a score of 1 on *domain*). To ensure that the analysis samples were as similar as possible, these observations were recoded so that *domain* took the value 0 in the reconstructed dataset. Similarly, country-year observations with a score of 1 or 2 on independent variables in the TIES subset and 0 in Peksen's analysis sample were recoded to correspond with Peksen's dataset.

Inconsistencies between Peksen's data and research design

Before detailing how I reconstructed Peksen's individual variables, It is also necessary to note that the reconstruction process revealed what appears to be some minor inconsistencies between Peksen's research design and dataset. First, Peksen states that he has excluded "TIES sanctions cases over environmental policy and trade practice disputes" (Peksen 2009: 65). However, some environmental policy and trade-related sanctions from TIES appear to have been included in Peksen's analysis sample. Examples include environmental policy-related sanctions against Japan between 1992-1997, and trade-related sanctions against Italy during 1981-82 and 1995. Furthermore, excluding all environmental- and trade-related sanctions from TIES resulted in wide discrepancies between the analysis samples - as such I opted to exclude apparent trade- and/or environmental-related sanctions *only* if these were not present in Peksen's original dataset. Trade- and environmental-related sanctions have also not been excluded in subsequent analyses. This approach allowed me to stay as close as possible to Peksen's research design.

Furthermore, I identified a small number of sanctions cases that do not appear to have been extracted from either the TIES or HSE datasets. These include sanctions against Chile for the entire 1981-2000 period, sanctions against Niger between 1996-1997, and sanctions against Saudi Arabia in 1995. Such cases were *not* imported from Peksen's dataset into the reconstructed dataset. A possible explanation for both these discrepancies may be inconsistencies between the original and the updated TIES datasets. However, I have been unable to determine conclusively whether this is the case.

Variable construction summarized

The following section provides a detailed overview of the construction of all variables included in the replication and subsequent analyses. I have relied exclusively on operational definitions outlined in Peksen (2009). Where operationalisations are vague or unclear, I include a brief outline of my own interpretation of the most likely method employed in coding independent and control variables.

Dependent variables - physical integrity rights: All physical integrity rights indicators were taken from latest iteration of CIRI dataset (Cingranelli, Richards, and Clay 2014), and limited to the relevant time period (1981-2000). None of the CIRI physical integrity rights variables have been retired or altered since coding began. Scales have been reversed as in Peksen's analysis, so that high values indicate frequent violations.

Economic sanctions (all): As outlined in section 3.1, all sanction variables in Peksen's study are ordinal variables, indicating whether a country is facing no sanctions (0), partial sanctions (1), or extensive sanctions (2) in a given year. Peksen defines extensive sanctions to include "comprehensive sanctions that cut any economic and financial transaction between the sender and target countries;" partial sanctions include "limited trade and financial sanctions, such as investment bans or asset freezes and and partial export restrictions, reduction or suspension of arms exports or limited dual-use technologies, constrains on military and other sorts of aid, and travel restrictions" (Peksen 2009: 66). Beyond this, however, it is not clear from Peksen's article how he coded the sanctions variable based on the TIES dataset. Consequently, I coded the economic sanctions variable in the manner I determined to be most in line with Peksen's definition above.

The TIES dataset does not contain a variable indicating sanction scope that corresponds to the scale of the Peksen's economic-sanctions-variables. I coded *economic sanctions* in the new dataset based on the *sanction type* variable from TIES. *Sanction type* categorizes all implemented sanctions according to 10 distinct categories, ranging from total economic embargo to termination of foreign aid.⁷ One sanction episode may belong to more than one category, with two of the 10 sanction categories involving complete severance of economic and financial ties: "total economic embargo" and "blockage", respectively. I have taken any sanction episode falling into one of these two categories (irrespective of what other categories they also belong to) to correspond to the extensive sanctions category (a

⁷The *target economic cost* variable ranks the economic impact of sanctions on a three-point scale, but as this does not fit with Peksen's operationalization of limited and extensive sanctions, as described, it has not been employed.

score of 2), with all other categories corresponding to partial sanctions (a score of 1). The remaining country-year observations not covered by a recorded sanctions episode have been given a score of 0.

For the sanctions cases identified from HSE and imported from Peksen's dataset, I chose not to alter the scores on the *economic sanctions* variable. The choice is primarily motivated by pragmatic considerations, but also by the fact that Peksen does not explicitly state the criteria used to determine whether an HSE sanction case falls into the partial or extensive category. HSE cases are categorized according to target cost as a percentage of GNP, ranging from 0.002 to 54 (Hufbauer, Schott, Elliott, and Oegg 2008). It is possible that Peksen used target cost to categorize HSE cases into limited and extensive sanctions. However, since it is not possible to determine with certainty whether this is the case, or what the cutoff-points were between limited and extensive sanctions, I have chosen to rely on the original coding from Peksen's dataset for imported HSE cases.

Human rights/non-human-rights sanctions: Peksen defines human rights sanctions to include cases where "a country faces extensive or partial imposed sanctions with the goal of preventing human rights violations committed by target regimes" (Peksen 2009: 67). Consequently, I code the human rights/non-human rights sanctions variables based on the economic sanctions variable above, as well as the *issue* variable from the TIES dataset. *Issue* consists of three fields identifying up to three separate issues involved in the sanctions episode. The categories are not mutually exclusive, such that a single sanction episode may aim to, for instance, curb political influence and improve human rights.

I have coded *human rights sanctions* 2 or 1 if the if human rights is listed in one of the three fields of the issue variable *and* if the economic sanctions variable has a score of 2 (extensive sanctions) or 1 (limited sanctions), respectively. Otherwise the variable has been coded 0. In short, a sanction episode is counted as a human rights sanction if *one of* the aims is to improve human rights in the target country – it does not have to be the main goal of the sanction. Conversely, *non-human-rights sanctions* is coded 2 or 1 if economic sanctions has a score of 2 or 1, and if none of the three issue fields list human rights improvement as a sanction goal.

Multilateral/unilateral sanctions: The TIES dataset contains two main ways of conceptualizing multilateral and unilateral sanctions: the *senders* variable lists up to five states that either initiate threats or implement sanctions in a given episode; the dichotomous *institution* variable indicates whether or not sanctions were conducted through an international institution, i.e. whether sanctions were applied as part of a “multilateral effort.” Under the TIES coding scheme, formal alliances are considered international institutions (Morgan, Bapat, Kobayashi 2013: 2-3).

Peksen defines multilateral sanctions to include “extensive and partial sanctions imposed by the UN or major regional intergovernmental organizations,” with unilateral sanctions accounting for “sanctions imposed by individual countries without any involvement of international organizations” (Peksen 2009: 67). Peksen’s operationalization indicates that he most likely used the institution variable from the TIES dataset as the basis for coding the multilateral and unilateral sanctions variables. Consequently, I have given *multilateral sanctions* a score of 2 and 1 if the economic sanctions variable has a score of 2 and 1, respectively, and if the institution variable has a score of 1. Conversely, sanctions are considered unilateral if the institution variable has a score of 0.

Sanction years: The sanction years variable indicates how long sanctions have been imposed in a given year – the natural logarithm of the variable is given to account for curvilinearity in the data (Peksen 2009: 67). Peksen’s coding counts the sanction duration from the beginning of each year rather than the end (meaning that the sanction years variable takes the value of 1 the year it was implemented, not in the following year). For the sake of consistency I follow Peksen’s example, though it may be argued that a more accurate representation of how long a sanction has been imposed in a given year would be to begin the count at the end of each year – i.e. giving the variable the value of 1 for the second year of its duration, 2 for the third year, and so on.

GDP per capita: Like Peksen, I constructed the variable based on the natural

logarithm of World Bank data of GDP per capita income level for each country in a given year. It should be noted that while Peksen gives the values in 1995 constant US dollars, I was forced to use 2010 constant US dollars instead due to data availability (World Bank 2016). This should have no substantial impact on the replication or subsequent analyses.

Democracy: The democracy variable has been coded on the basis of the *polity2* variable from the Polity IV dataset, identical to Peksen's operationalization (Marshall and Jaggers 2000, 2007). The only difference is that I employ the latest version of the dataset. No changes have been made to variable coding in Polity IV, meaning the values range from -10 (fully autocratic state) to 10 (fully democratic state).

Civil war/interstate war: Peksen's citation indicate that he used the PRIO Armed Conflict Dataset Version 3 from 2005 (Strand et al. 2005). The Armed Conflict Datasets divides conflicts into four types: extrasystemic-, interstate-, internal-, and internationalized internal armed conflict. The monadic (i.e. country-year) version of the 2005 dataset contains variables for all four conflict categories, while subsequent versions of the monadic dataset only contains variables for type 3 (internal conflicts, or civil war) and type 4 (internationalized internal armed conflicts). It is likely that Peksen coded his civil war and interstate war variables based on the same monadic dataset from 2005 – to my understanding, this is not possible with the later versions of the Armed Conflict Dataset. Due to these circumstances, I chose to code *civil war* and *interstate war* in the following manner: *Civil war* is coded based on the *incidence412* variable from the latest iteration of the monadic Armed Conflict Dataset – *incidence412* indicates occurrence of intrastate conflict and is coded 1 for every country with at least one active conflict in a given year (Gleditsch et al. 2002; Melander, Pettersson, and Themnér 2016). For the interstate war variable I used the latest version of the dyadic Armed Conflict Dataset (Harbom, Melander, Wallensteen 2008; Melander, Pettersson, and Themnér 2016). I identified and extracted all instances of conflict with more than one location, i.e. instance of interstate conflict. *Interstate war* variable has been coded 1 if a country represents one side of an interstate conflict in a given year, and 0 otherwise.

Past practice: The *past practice* variable, as in Peksen's dataset, is simply coded as a one-year lag of the dependent variable to control for autocorrelation and possible issues with endogeneity.

3.3 Expanding the data

In the second stage of the analysis I expand the reconstructed dataset to include newly available data from the updated TIES dataset, which includes sanction cases between 2000-2005. The purpose of expanding the dataset is two-fold: first, to determine whether the conclusions from Peksen's study hold when the same analysis is performed with a broader range of data; and second, to provide a basis for the inclusion of additional indicators of human rights violations for the third stage of our analysis.

A key consideration in expanding the dataset is that we want to include as many *relevant* observations as possible from the available data. By relevant observations I mean all countries that have been threatened with or have faced sanctions between 1981-2005, as well as all instances of imposed sanctions in the same period. In the replication analysis a number of relevant observations were omitted because they were not included in Peksen's original dataset, and a key concern was to make sure the reconstructed dataset was as close to the original as possible. As this is no longer a concern with the extended dataset, besides sanction cases between 2000-2005 I have included a number of additional country year observations, as well as complete sanctions cases, omitted from the previous analysis.

That being said, it is instructive to examine whether the two ways in which the data is expanded impact results in different ways. In preparation for the analysis in chapter 5 I therefore estimated two sets of models - one using a dataset which has only been extended to include additional country-year observations from the TIES dataset (2000-2005), and one which includes both additional sanctions between 2000-2005 *and* the relevant observations which were omitted in the replication dataset.

I found that the substantive results from the two sets were more or less identical, with a few minor deviations. For instance, the more extensive dataset yielded significant results for the effect of multilateral and unilateral sanctions on political imprisonment, and for the effect of human rights sanctions on physical integrity rights overall, whereas the more restricted dataset did not. There were also some discrepancies with regard to significance for the effect of levels of democracy, and for the effect of interstate conflict. However, for all the key independent variables the regression coefficients were more or less similar, with the majority of results within one decimal place of one another. There were also no deviating results with regard to the direction of effects.

Due to the similarities between the results produced by the two datasets and the considerations outlined above, I choose to employ the most extensive version of the dataset for the second and third stage of my analysis.

3.4 Expanding the object of analysis: the CIRI empowerment rights

This section outlines and justifies the choice to use of the CIRI empowerment rights as my indicators for civil and political rights. I use a total of eight indicators of civil and political rights from the CIRI dataset - seven individual indicators, and one index variable comprised of all seven indicators. I then provide an overview of the operationalization of each of the eight dependent variables - where there has been a choice between old and new indicators from the CIRI dataset, this has been indicated in the variable description.

Operational definition

As discussed in Chapter 2, there are a few different indicators of civil and political rights employed in the literature, which makes it necessary to briefly outline and justify the operational definition I employ for my analysis.

The Freedom House Index of Democracy (2004), a commonly used measure of

the level of democracy in a country, distinguishes between civil liberties and political rights. The former encompasses freedom of expression, religious freedom, organizational rights, rule of law, and individual rights. The latter includes fairness of electoral processes, degree of political participation, and voting rights of elected officials (Peksen and Drury 2010: 401) These broadly overlap with the indicators comprising the Empowerment Rights Index from the CIRI dataset, with some exceptions. For instance, the CIRI index does not include an indicator for rule of law or individual rights. Furthermore, it does not distinguish between political rights as in the Freedom House index – these appear to be conflated in the variable “electoral self-determination”, which indicates “to what extent citizens enjoy freedom of political choice and the legal rights and ability in practice to change the laws and officials that govern them through free and fair elections” (Cingranelli, Richards, and Clay 2014). The CIRI index also does not distinguish between civil and political rights, as per the Freedom House index, choosing instead to lump these together under the category broad category of “empowerment rights.”

As mentioned in section 2.2, the principal component analyses (PCA) by Blume and Voigt (2007) and Gutmann and Voigt (2015) indicate, however, that both the CIRI empowerment rights (with the exception of worker’s rights) and the most recent Freedom House indicators of civil liberties and political rights (Freedom House 2014) may be considered an empirically distinct category of human rights which Gutmann and Voigt label civil and political rights (Gutmann and Voigt 2015; Gutmann, Neuenkirch and Neumeier 2016: 12-13).

For the purpose of the analysis in this thesis I have chosen to employ an extensional definition of civil and political rights, based on the empirically distinguishable set of rights from Gutmann and Voigt (2015). I only use the CIRI indicators from the Gutmann and Voigt PCA, and not the Freedom House indicators, even though these are identified as expressions of the same latent variable. One reason is that there is the large degree of overlap between rights categories in both these datasets, but the main reason is the considerations regarding disaggregation discusses in Chapter 2.

To summarize my main practical reasons for relying on the CIRI indicators of human rights to operationalize civil and political rights: first, they allow me to

easily disaggregate the empowerment rights index to run separate models, and second, they ensure the comparability of results across all models in this thesis by drawing on the same dataset as Peksen's analysis investigating physical integrity rights. Consequently, I can reasonably compare the findings from the third stage of my analysis with those of the earlier analyses. As mentioned previously, this has not been done before (to my knowledge), and therefore represents a novel contribution to the literature.

While worker's rights are not strictly included in our working definition of civil and political rights, based on the principal component analysis (PCA) by Gutmann and Voigt (2015), it has not been omitted from the analysis. Additionally, I have chosen not to create a new empowerment rights index excluding worker's rights. The primary reasons for this are as follows. First, while the construction of the empowerment rights index variable is detailed in a separate paper (Richards, Gelleny and Sacko 2001), the index variable has since been updated in 2007 (Cingranelli, Richards, and Clay 2014). Without clear descriptions detailing how the new index variable was constructed, I run the risk of ending up with an variable that is not consistent with CIRI coding. Furthermore, I would argue that the benefits of a reconstructed index variable are marginal - the CIRI empowerment rights index is comprised of a total of 7 indicators, and omitting one is not likely to have a substantive impact on the results. Finally, the PCA by Gutmann and Voigt (2015), which I base my operationalisation of civil and political rights, is based on an earlier PCA by Blume and Voigt (2007). In the original PCA, worker's rights *is* identified as a latent variable belonging to the civil and political rights category. Whether or not worker's rights should be included in an empirically founded definition of civil and political rights is therefore contentious at best. Combined with the practical concerns regarding the reconstruction of the index variable, I would argue that these are compelling reasons to not exclude worker's rights from my analysis.

Variable description

This section describes all eight civil and political rights indicators employed in the third and final stage of my analysis. All variables are taken from the latest version

of the CIRI dataset. Furthermore, all variable descriptions below are taken from the official CIRI coding manual (Cingranelli and Richards 2014). As with the indicators of physical integrity rights, the variables have been recoded so that high values indicate higher levels of abuses:

Freedom of foreign movement indicates citizens' freedom to leave and return to their country. A score of 2 indicates that this freedom was severely restricted, a score of 1 indicates the freedom was somewhat restricted, and a score of 0 indicates unrestricted freedom of foreign movement.

Freedom of domestic movement indicates citizens' freedom to travel within their own country. A score of 2 indicates that this freedom was severely restricted, a score of 1 indicates the freedom was somewhat restricted, and a score of 0 indicates unrestricted freedom of foreign movement.

Freedom of speech indicates the extent to which freedoms of speech and press are affected by government censorship, including ownership of media outlets. Censorship is any form of restriction that is placed on freedom of the press, speech or expression. Expression may be in the form of art or music. A score of 2 indicates that government censorship of the media was complete; a score of 1 indicates that there was some government censorship of the media; and a score of 0 indicates that there was no government censorship of the media in a given year.

Freedom of assembly and association indicates the extent to which the freedoms of assembly and association (in political parties, trade unions, cultural organizations or other interest groups) are subject to actual governmental limitations or restrictions (as opposed to strictly legal protections). A score of 2 indicates that citizens' rights to freedom of assembly or association were severely restricted or denied completely to all citizens; a score of 1 indicates that these rights were limited for all citizens or severely restricted or denied for select groups; and a score of 0 indicates that these rights were virtually unrestricted and freely enjoyed by practically

all citizens in a given year.

Electoral self-determination indicates to what extent citizens enjoy freedom of political choice and the legal right and ability in practice to change the laws and officials that govern them through free and fair elections. This right is sometimes known as the right to self-determination. A score of 2 indicates that the right to self-determination through free and fair elections did not exist in law or practice during the year in question. A score of 1 indicates that while citizens had the legal right to self-determination, there were some limitations to the fulfillment of this right in practice. Therefore, in states receiving a 1, political participation was only moderately free and open. A score of 0 indicates that political participation was very free and open during the year in question and citizens had the right to self-determination through free and fair elections in both law and practice.

Freedom of religion indicates the extent to which the freedom of citizens to exercise and practice their religious beliefs is subject to actual government restrictions. Citizens should be able to freely practice their religion and proselytize (attempt to convert) other citizens to their religion as long as such attempts are done in a non-coercive, peaceful manner. A score of 2 indicates that government restrictions on religious practices are severe and widespread. A score of 1 indicates such practices are moderate, and a 0 indicates such practices are practically absent.

Worker's rights indicates the extent to which workers enjoy the rights to associate freely at their workplace, bargain collectively with their employer, and other internationally recognized rights at work, including a prohibition on the use of any form of forced or compulsory labor; a minimum age for the employment of children; and acceptable conditions of work with respect to minimum wages, hours of work, and occupational safety and health. A score of 2 indicates that workers' rights were severely restricted; a score of 1 indicates that workers' rights were somewhat restricted; and a score of 0 indicates that workers' rights were fully protected during the year in question.

Empowerment Rights Index is an additive index constructed from the Foreign Movement, Domestic Movement, Freedom of Speech, Freedom of Assembly and Association, Workers' Rights, Electoral Self-Determination, and Freedom of Religion indicators. It ranges from 14 (no government respect for these seven rights) to 0 (full government respect for these seven rights).

3.5 Choice of statistical model

In statistical analyses, the choice of which model to use is primarily determined by the level of measurement and distribution of the dependent variable. Statistical models vary greatly, and involve different methods of estimation and interpretation. Choosing the right model for the job is of paramount importance. It is therefore necessary to dedicate some space to explaining which model is appropriate for the dependent variable(s) examined in this thesis, and the implications of using said model.

Which model to choose?

All the dependent variables in this paper are ordinal variables with three or more categories. An ordered variable has categories that can be ranked from low to high, while the distance between adjacent categories is unknown. We can contrast this with interval variables, which can also be ranked, but where the distance between categories are equal. (Long 1997: 114) To illustrate, the CIRI indicators of human rights are ranked according to frequency of violations - ranging from no incidents (0), some incidents (1-49), to many incidents (50 and over). As such, these are considered ordered variables, since the difference between a medium and high score on the variable can range from one merely one incident to a whole score of incidents - we can not assume that the distance between the scores 0 and 1 are equal to the distance between 1 and 2. The same goes for the physical integrity and empowerment rights index variables, which are based on aggregates of the individual rights variables.

In practice, researchers frequently treat ordinal dependent variables as though they were measured at the interval level, employing the standard linear regression model. However, ignoring that the dependent variable is ordinal can lead to misleading results. It is generally recommended to use models specifically designed for ordinal variables (Long 1997: 115). Two such models are the ordered logit and ordered probit models, which Long collectively refers to as ordered regression models.

Peksen chooses to employ the ordered probit model for his analysis. For the sake of both convenience and consistency across the analyses in this thesis, I employ the same model throughout. As I will explain below, the difference between logit and probit models are marginal, and there is no compelling reason to substitute the latter for the former. Due to the risk of generating misleading results, there is even less reason to substitute the ordered regression model with an alternative model such as the linear regression model.

Furthermore, I have already highlighted that comparability across studies is a fundamental concern in the literature on the effect of sanctions on human rights – employing the same models and methods of interpretation for all analyses in this thesis further bolsters the ability to reasonably compare the results of models looking at physical integrity rights on one hand and civil and political rights on the other.

The remaining sections in this chapter gives a general overview of the ordered regression models, the assumptions underlying their use, and how they are designed to correct for some of the issues that can arise when using linear regression models on ordered outcome variables. I also discuss some ways in which the results from ordered regression models can be analysed and interpreted. The remainder of the chapter also serves to outline the reasoning behind applying the probit model, as well as Peksen's method of interpreting the coefficients from the model, throughout the thesis.

Ordered regression models

As mentioned above, the linear regression model is sometimes used for ordinal dependent variables, though there are a number of issues with this approach. Primarily, with ordinal outcomes, a number of the assumptions of the linear regression model are violated.

First, for ordered outcomes the errors are generally heteroscedastic, meaning that the variance of the errors are dependent on the value of independent variables and is not constant. The OLS estimator is therefore inefficient and standard errors are biased, yielding incorrect test statistics. There are also issues with the functional form of the model: A linear model does not accurately describe the effect of a unit change in independent variable on the probability of getting a high value on the dependent variable (Long 1997:39-40).

The ordered regression models were developed to account for the weaknesses of using the linear regression model for ordered (and binary) outcomes, and relies on the assumption that there is a latent, underlying variable (y^*) generating the observed outcomes of the ordered variable (y). y can be thought of as ranging from $-\infty$ to ∞ . Values of y^* over a certain thresholds (τ) are observed as $y=1$ (or $y=2$), while value under the threshold are observed as 0 (Long 1997: 116)

We can illustrate the logic behind the idea of the latent variable using examples from our own dependent variables on human rights violations. Consider for example the disappearance variable, where two countries both have an observable score of 1 (some violations) for a given year. While both have identical scores, the countries may be quite different from each other. For instance, one may be close to a score of 0 (with, say, 2 incidents), while the other is close to a score of 2 (with 48 incidents). Regardless, we observe the same $y=1$. The latent variable y^* , in this instance number of incidents, generates the observed values on the disappearance indicator - at some point a change in y will result in a change in the observable y from 1 to 2 (or conversely, from 1 to 0).

The above example also illustrates that the concept of the latent variable is appropriate and applicable to our analysis using the CIRI indicators of human rights violations. The variables are all based on the frequency of human rights violations,

a measure that is both linear and continuous. However, what we observe is the ordinal outcome only, determined by the cut-off points Cingranelli and Richards have applied in constructing their indicators.

The latent variable is assumed to be linearly related to the observed values on the independent variable (x) via the following structural model:

$$y = x\beta + \varepsilon \quad (3.1)$$

For our example with *disappearance*, y would be linked to the observable y in the following manner:

$$y = \begin{cases} 2 & \text{if } y > \tau_1 \\ 1 & \text{if } y \leq \tau_1 \text{ and } y > \tau_0 \\ 0 & \text{if } y \leq \tau_0 \end{cases}$$

With threshold values as follows:

$$\tau_1 : 49 \text{ incidents}$$

$$\tau_0 : 0 \text{ incidents}$$

As y is continuous, this model avoids the issues with the linear probability model outlined above. However, as the dependent variable is unobserved, it is not possible to estimate the model with ordinary least squares (OLS) estimation. Furthermore, we cannot estimate the variance of the errors. Instead, we use maximum

likelihood-estimation (ML) to estimate the regression of y on x . ML-estimation requires that we make assumptions regarding the distribution of the errors, and the assumption regarding the distribution determines which of the ordered regression models we end up with. The ordered probit model assumes normally distributed errors with a mean of 0 and variance of 1. (Long 1997: 119). The most commonly used alternative is the logistic distribution, which yields the logit model. Other distributions for the errors have been considered, however these two are the most commonly used (Long 1997: 119).

Interpreting results - probit vs logit

To justify the use of the probit model, it is worth emphasizing that the choice between the logit and probit models are largely one of convenience or convention, as the two generally yield substantially indistinguishable results (Long 1997: 83). The most prominent difference between them concerns the interpretation of the coefficients. The logit model allows for a relatively straightforward method of interpreting results using odds ratios, i.e. relationship between the probability of an event happening and an event not happening. The odds ratios can be derived from the coefficients of the logit model by way of a simple transformation.

However, there is no corresponding transformation for the parameters of the probit model, making it necessary to use another method for interpreting results (Long 1997: 79). There are a few ways to interpret the coefficients of ordered models, though they all have in common that they attempt to illustrate the effect of changes in an independent variable on the probability that the dependent variable takes a specific value (Nagler 1994: 6).

The most direct approach is to look at the predicted probabilities of an event for different values of the independent variables (Long 1997: 64). One of the simplest techniques is to examine the effect of a unit change in independent variables on predicted probabilities – we can do this by setting all other variables to a specific value, for instance their mean, and then varying the variable of interest to show the individual effect on the probability that $y=1$ (or $y=2$) (Nagler 1994: 7). Peksen uses a variation of this approach in his analysis: for models with pairs of indepen-

dent variables the control variables are held constant at their means, while both independent variables are set to 0 – he then examines the changes in predicted probabilities when changing one independent variable at a time to isolate effects (Peksen 2009: 73).

It should be noted that when calculating predicted probabilities from the logit and probit models, the estimated change is conditional upon the other variables having specific values, as indicated above. In other words, the marginal effect of a unit increase in a variable of interest will always depend on the values of the other variables in the model. Furthermore, the effect of independent variables on the dependent variable will not be linear - for instance, the probability increase when going from 0 to 1 on *disappearance* would not be equal to the probability increase when going from 1 to 2. Finally, while the method is a good way to capture the relationship between changes in the independent and dependent variables, it does not allow us to adequately describe the relative impact between independent variables (or independent and control variables) (Nagler 1994: 8).

For instance, we might want to examine the effect of a one unit increase in the economic sanctions variable on the probability of frequent incidents of political disappearance (a high value on the *disappearance* variable) *for different values on the polity2 indicator for democracy*. We could do this by holding all variables but *economic sanctions* and *democracy* at their means, as outlined above, and examine the changed is predicted probabilities for different combinations of values on the pair of variables. However, since this type of investigation is beyond the scope of my analyses, the method outlined above is appropriate for all models presented in this thesis.

Notice that predicted probabilities are calculated for specific outcomes. To take the example of *disappearance* again, we can look at changes in predicted probabilities for different values on the independent variables for either mid-range values (1) or high values (2) on the dependent variable - not both at the same time. Consequently, unless we want to present predicted probabilities for all possible values on the dependent variables, it is necessary to make choices regarding which outcomes are substantially more interesting. As indicated above, I agree with Peken's choice of looking at predicted probabilities for high values on the dependent variables, as

we are primarily interested in more extensive cases of human rights violations (i.e. high frequencies of violations).

It should be noted that as long as we are primarily concerned with the observed categories of the dependent variable, we can use the methods outlined above to analyse the results. However, if we are interested in the unobserved values of the latent variable, assuming that the latent variable makes substantive sense, it is possible to interpret results by rescaling the latent variable to a unit variance and computing standardized coefficients (Long 1997: 127). For our purposes it is sufficient to examine the observed categories of human rights violations, and as such I will not make any substantive changes to the methodological approach employed by Peksen for any of the analyses in this thesis.

Finally, an important assumption in ordered regression model is that of *parallel regression*. Put simply, it means that the slope of the probability curve, i.e. the curve relating an independent variable to the probability of an even, is unaffected by the intercept of the model. Figure 3.1 illustrates this idea graphically.

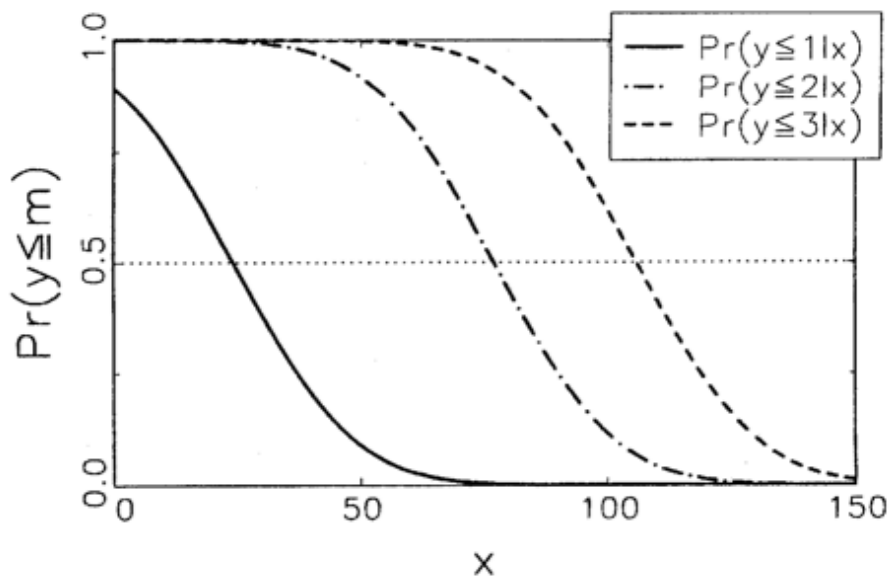


Figure 3.1: Illustration of the parallel regression assumption. Figure taken from Long (1997), p.141.

The graph in Figure 3.1 shows the effect of different values of the independent variable (x-axis) on the probability that $y=1$ (y-axis). With the intercept at 0, the curve passes through the point 0.5 on the y-axis. Higher values for the intercept shifts the curve to the right, and lower values shift the curve to the right - however, the slope at any given point on the y-axis remains the same (Long 1997: 62-63). If the parallel regression assumption is violated, other models that do not impose this constraint should be considered – for example the multinomial regression model (Long 1997: 145).

3.6 Summary

In this chapter I first outlined Peksen's research design, including descriptions of each variable included in his study and their respective data sources. I then provided a detailed overview of how I went about reconstructing Peksen's dataset, which was pre-empted by arguments for why such a reconstruction was necessary. Next, I briefly outlined key considerations underpinning the expansion of the analysis period to account for additional sanctions data. I then explicitly outlined the operational definition of empowerment rights used for the analysis in chapter 6, and provided an overview of how each empowerment rights indicator has been operationalized. I concluded this chapter with an overview of the ordered regression models, including an overview of possible methods of interpreting the results from these models (and why the choice between probit and logit models does not matter for substantive results).

Before moving on to the replication of Peksen's study, it is worth briefly summarize the methodological choices underpinning the models presented in the following chapters. As all the dependent variables used throughout the thesis are ordinal variables, I employ ordered probit regression models throughout. The choice of the probit model over the logit model is primarily motivated by the fact that this is the model Peksen uses in his analysis – adherence to his methodological choices dictate that the probit model is used for the replication as well. As the primary difference between probit and logit models concerns the interpretation of coeffi-

cients, not substantive results, relying on the same model for subsequent analyses ensures that the all results in this thesis can be presented and interpreted using the same method. This in turn facilitates the comparison of results across models.

Interpretation of coefficients will be presented in terms of predicted probabilities of getting a high value on individual human rights indicators when going from low to medium and low to high scores on the economic sanction variables. Changes in predicted probabilities are only presented and interpreted for significant regression coefficients, i.e. with a p-value below at least 0.1.⁸

The following chapter presents the results from the replication of Peksen's original study, conducted using both Peksen's own analysis sample and the reconstructed dataset.

⁸This is in line with Peksen's research design. However, as a confidence level of 90% is somewhat lenient compared to what is common in these kinds of studies, I will make an effort to comment on results that are significant only at the 10%-level in the analyses below.

Chapter 4

Replicating Peksen's study

The following chapter is dedicated to presenting and analysing the results from the replication of Peksen's study. I present summary statistics and results from the 20 ordered probit regression models from Peksen's original study that use the CIRI indicators of physical rights abuses. The exact results from Peksen's analysis is reproduced using the replication dataset published along with his paper, obtained from the Journal of Peace Research website. These models are used as a baseline to evaluate the accuracy of the subsequent replication analysis, which uses the new dataset compiled from sources specified in Peksen's publication (2009). Exact replications are presented alongside models estimated using the rebuilt dataset for ease of comparison, yielding a total of 40 models for this chapter. Post-estimation interpretation of results is performed in accordance with Peksen's original analysis – that is, the impact of individual independent variables are illustrated by way of effect of unit change on the probability of high values on the human rights indicators, when all control variables are held constant at their means. I also compare goodness-of-fit indices for each pair of models, as well as test for multicollinearity, a common issue with country-year data. I also test the parallel regression assumption underlying the ordered regression models.

Before moving on to the replication analysis proper, however, it is necessary to briefly outline what criteria should be used to deem whether the replication has been successful or not.

4.1 Success criteria

In section 2.3 and 3.2 I outlined King's (1995) position, which implies that a successful replication involves not only the reproduction of numerical results, but also the reconstruction of the data through drawing on the same sources, and same process, as the original analysis. To reiterate, "the only way to understand and evaluate an empirical analysis fully is to know the exact process by which the data were generated and the analysis produced (King 1995: 444)." Consequently, to assess whether the replication of Peksen's study has been successful, it is necessary to compare both the datasets themselves and the results from the analyses.

However, a caveat is in order here. I indicated in section 3.2 the various challenges I met with during the process of reconstructing Peksen's dataset. A result of these challenges was that I was unable to reproduce his dataset *exactly*, and that some discrepancies remain between my dataset and his.

One reason for this discrepancy was that some data sources had change or become unavailable. For instance, the version of the Armed Conflict Dataset from which Peksen derived his measures for civil and interstate war is (as far as I can determine) no longer available. The newer datasets employs different coding schemes, which made it necessary to adapt available data to approximate the variables from Peksen's replication set.

Another possible source of discrepancy is smaller or larger misinterpretations on my part with regard to Peksen's methodology or operationalizations. As I have already mentioned in section 3.2, it was not entirely clear from Peksen's article which indicators from the TIES dataset were used to operationalize the variables indicating sanction objectives and multilateral/unilateral sanctions. As repeated attempts to obtain this coding information from Peksen himself were unsuccessful, it was necessary to make a number of conjectures with regard to operationalization during the compilation process. These conjectures may not have been entirely accurate, and inaccuracies in coding or operationalization could result in different results from the analysis. At the very least, these discrepancies would prevent me from obtaining the exact same numerical results as Peksen.

Does this mean that replicating Peksen is impossible? No, but it does make

it necessary to outline some explicit criteria by which we can evaluate whether the replication can be seen as successful *within reasonable parameters*. What should these criteria be?

First, with regard to the datasets, I have already outlined in section 3.2 that I ensured that all countries and sanction episodes from Peksen's dataset were accounted for in the new dataset. What remains, then, is to assess whether the number of observations included for each model, and the distribution of values for each variable included in the analysis, correspond to each other. We can determine this by inspecting and comparing summary statistics for each variable included in the analysis, which I do below.

Second, with regard to the analysis itself, after exact numerical results the next best way to evaluate whether the results from the replication correspond to the original is to look at whether we can draw the same substantive conclusions as from Peksen's analysis. How do we determine whether this is the case?

One way is to examine the direction and significance of results from the regression analysis. At the very least, corresponding results should be significant in both analyses (and *vica versa*), and the direction of the effects should be the same. Due to the discrepancies between the datasets, I choose to be somewhat lenient in terms of significance levels, and do not deem the replication unsuccessful if results do not correspond to the specific significance levels (though I will comment where significance levels are considerably lower than in the original).

Another method is to compare goodness-of-fit measures, such as log-likelihood values, to evaluate whether the models fit the data to a comparable degree.

Finally, and perhaps most crucially, if the replication has been successful, we would expect results from the postestimation results to yield similar conclusions to the original study. These include the size of effects, as well as similar relationships between different categories of sanctions.

I use these criteria in turn to evaluate the success of the replication analysis below. In the next section I present and compare summary statistics for all variables included in both the original and replication analyses.

4.2 Comparing the datasets

Tables 4.1 and 4.2 report the summary statistics for the all variables used in the original and replication analysis, respectively. Summaries for independent and control variables are presented using the models with the largest number of observations (N), which in both cases are the models with *disappearances* as the dependent variable.

First of all, we see that the number of observations differ slightly for corresponding values in the two datasets. The discrepancy in observations between the original and reconstructed dataset is primarily due to an issue with the World Bank data on GDP per capita. There is a small number of countries in each dataset with missing GDP data, where the corresponding data is available in the other dataset. Observations with missing GDP data are not included in the analysis, by default. I was unable to extract the missing GDP from Peksen's dataset, as values are given in 1995 and 2010 constant US dollars, respectively. As the issue only concerns a few observations in each dataset, and the substantial results of the analyses using the two datasets correspond closely to each other (as will be evident from section 4.3 and 4.4), I made the decision to not correct for this discrepancy between the datasets.

In any case, the distribution of values of each variable is a more precise indicator of whether the datasets deviate greatly from each other. We see that, with a few exceptions, mean values are all within one decimal point of each other. Furthermore, the mean values only vary to a greater extent for variables with a large spread of values (GDP per capita and sanction years), which is to be expected. There are also no noteworthy differences between standard deviance-values. All in all, a comparison of summary statistics indicates that the replication dataset corresponds well to Peksen's original dataset.

Table 4.1: Summary Statistics - original dataset

Statistic	N	Mean	St. Dev.	Min	Max
Disappearances	1,607	0.398	0.680	0	2
Extra-judicial killings	1,600	0.749	0.803	0	2
Political imprisonment	1,602	1.033	0.858	0	2
Torture	1,607	1.236	0.742	0	2
Physical integrity index	1,595	3.401	2.398	0	8
Economic sanctions	1,607	0.302	0.545	0	2
Human rights sanctions	1,607	0.110	0.342	0	2
Non-human-rights sanctions	1,607	0.153	0.444	0	2
Multilateral sanctions	1,607	0.080	0.315	0	2
Unilateral sanctions	1,607	0.215	0.494	0	2
Sanction years	1,607	2.166	5.858	0	39
GDP per capita (log)	1,607	7.516	1.547	3.986	10.699
Democracy	1,607	1.768	7.374	−10	10
Civil war	1,607	0.227	0.419	0	1
Interstate war	1,607	0.049	0.216	0	1

Table 4.2: Summary Statistics - new dataset

Statistic	N	Mean	St. Dev.	Min	Max
Disappearances	1,628	0.425	0.710	0	2
Extra-judicial killings	1,620	0.760	0.811	0	2
Political imprisonment	1,623	1.015	0.852	0	2
Torture	1,628	1.263	0.738	0	2
Physical integrity index	1,615	3.450	2.417	0	8
Economic sanctions	1,628	0.276	0.536	0	2
Human rights sanctions	1,628	0.092	0.338	0	2
Non-human-rights sanctions	1,628	0.184	0.454	0	2
Multilateral sanctions	1,628	0.145	0.429	0	2
Unilateral sanctions	1,628	0.142	0.392	0	2
Sanction years	1,628	1.603	4.724	0	47
GDP per capita (log)	1,628	8.163	1.536	4.808	11.626
Democracy	1,628	1.656	7.324	−10	10
Civil war	1,628	0.251	0.433	0	1
Interstate war	1,628	0.039	0.193	0	1

The next section presents the results from the regression analysis. Here I evaluate the results based on both Peksen's original dataset and the reconstructed dataset, and discuss to what extent the findings with the extended dataset deviate from the original. I evaluate the success of the replication with reference to the criteria outlined in section 4.1.

4.3 Comparing the results

In this section I evaluate to what extent I have been successful in replicating Peksen's results. I do this by comparing the results from both the regression analyses and the postestimation analysis, and evaluating the findings from the replication analysis according to the three criteria outlined in section 4.1.

It should be noted that I will be primarily concerned with commenting on the the independent sanction variables, as these are the one of most substantial interest to this study (and to Peksen's original study). Consequently I do not comment on the results for the control variables, though there is undoubtedly potential for findings of substantial interest to the human rights literature in general by examining the coefficients.

Before comparing the results of the original and replication analyses, however, it is necessary to briefly address two potential issues that often arise when employing ordered regression models and panel data – breach of the parallel regression assumption, and instances of multicollinearity.

The parallel regression assumption and multicollinearity

As indicated in section 3.4, a fundamental assumption of the ordered regression model, known as the "parallel regression assumption", is that the slope of the probability curve is unaffected by the intercept. If this assumption is violated, it is generally recommended to use alternative models that do not impose the same constraints, such as multinomial regression models (Long 1997: 141). I test the parallel regression assumption for the models in the analysis by first re-estimating vector generalized linear models with and without the assumption in place. I then

run χ^2 -tests of deviance to determine whether the model without the parallel regression constraints represents a significant improvement to model fit. The parallel assumption appears to be violated for the models examining the impact on torture from Peksen's original analysis (there was no indication of violation in the replication analysis). I re-estimated the relevant models using multinomial probit regression, but found no evidence suggesting conclusions deviating from the ones presented in this chapter.¹

Multicollinearity refers to instances where two or more independent variables in a multiple regression model are highly correlated. Multicollinearity is a common issue with analyses using country-year data, as country-level variables are likely highly correlated. High multicollinearity affects the variance of the regression coefficients, and can result in imprecise estimates. I check for multicollinearity by inspecting correlation matrices indicating pairwise correlation between all independent variables in a given model. None of the matrices indicated issues with multicollinearity.²

Significance and direction

Tables 4.3-4.7 report the results from models using the CIRI indicators of disappearance, extra-judicial killings, political imprisonment, torture, and physical integrity rights, respectively. For each table, models 1P-4P show the exact results from Peksen's original analysis, reproduced here using his own dataset. Models 1-4 show results from the replication analysis using the replication dataset compiled as per the method outlined in Peksen's paper. The first pair of models (1/1P) include the ordinal economic sanctions variable as the predictor, and show the overall effect of economic coercion on state repression; the second pair (2/2P) reports the effect of sanction objectives, i.e. whether sanctions were imposed with explicit aim of curbing human rights abuses or not; the third pair (3/3P) shows the effect of multilateral and unilateral sanctions, respectively; and the last pair

¹Results from all estimated multinomial models are presented in section A.2 in the appendix.

²See section A.3 in the appendix for a graphical representation of pairwise correlation between explanatory variables.

(4/4P) shows the effect of sanction duration, measured in number of years since the sanction was imposed.

Because the dataset contains repeated observations of the same countries, the observations cannot be assumed to be independent. Correlation between observations from the same country is likely to bias normal-theory standard errors downwards. To avoid this problem, I therefore cluster the standard errors on countries. All the models are reported with robust, clustered standard errors in parentheses.³

We start by examining the models indicating the general effect of economic sanctions on physical integrity rights abuses. Peksen's results here are all significant with a positive direction, clearly indicating that economic sanctions in general appear to lead to an increase in state repression for both individual human rights (Tables 4.3-4.6) and for the composite measure of human rights (Table 4.7). Comparing Peksen's results to my own, we see that all coefficients are positive and at minimum significant at the 10%-level, suggesting the same relationship between sanctions and rights violations.

There are some notable, though not critical, differences in significance levels. For example, we see that the results for sanctions' effect on political imprisonment are significant at the 5%-level in Peksen's analysis, and at the 10%-level in mine. However, inspecting the p-values, we see that the difference is not all that dramatic: the coefficient for *economic sanctions* in Model 1 yields a p-value of 0.06, which is very close to the 5%-significance threshold (<0.05). Conversely, for *killings*, my models actually yield results at a higher significance level than Peksen's.⁴

For the models examining human rights and non-human rights sanctions, Peksen's results are still consistently positive and significant, with two exceptions. Non-human rights sanctions for *killings* and human rights sanctions for *political imprisonment* do not yield significant results. Once again, the results from the replication analysis line up with Peksen's, with a few notable differences.

³This applies to all models in Chapters 4, 5, and 6.

⁴For the remainder of my analyses, I will only comment on significance levels where these are at the 10%-level in my analysis, and only if the corresponding result in Peksen's analysis is lower than that – i.e. at the 5%– or 1%–level.

Table 4.3: Ordered Probit Models for the Effect of Economic Sanctions on Disappearance (1981-2000)

	<i>Disappearance</i>							
	(Model 1)	(Model 1P)	(Model 2)	(Model 2P)	(Model 3)	(Model 3P)	(Model 4)	(Model 4P)
Economic sanctions	0.188*** (0.056)	0.181*** (0.066)						
Human rights sanctions			0.156* (0.087)	0.222** (0.107)				
Non-human-rights sanctions			0.204*** (0.060)	0.172** (0.078)				
Multilateral sanctions					0.234*** (0.079)	0.210** (0.087)		
Unilateral sanctions					0.158* (0.081)	0.126 (0.079)		
Sanction years							0.112*** (0.040)	0.112*** (0.041)
GDP per capita (log)	-0.102** (0.042)	-0.144*** (0.039)	-0.104** (0.042)	-0.138*** (0.039)	-0.101** (0.042)	-0.140*** (0.039)	-0.100** (0.041)	-0.139*** (0.038)
Democracy	0.002 (0.008)	-0.002 (0.008)	0.002 (0.008)	-0.002 (0.008)	0.002 (0.008)	-0.002 (0.008)	0.001 (0.008)	-0.003 (0.008)
Civil war	0.663*** (0.110)	0.757*** (0.111)	0.664*** (0.110)	0.758*** (0.111)	0.657*** (0.111)	0.751*** (0.112)	0.672*** (0.109)	0.757*** (0.110)
Interstate war	0.169 (0.165)	-0.044 (0.139)	0.158 (0.165)	-0.037 (0.139)	0.183 (0.163)	-0.009 (0.140)	0.231 (0.158)	-0.039 (0.141)
Past practice	1.072*** (0.105)	0.964*** (0.083)	1.073*** (0.105)	0.962*** (0.085)	1.068*** (0.106)	0.963*** (0.085)	1.078*** (0.104)	0.965*** (0.084)
Log-pseudo likelihood	-868.111	-868.977	-868.020	-868.735	-866.739	-868.629	-868.9354	-868.281
N	1628	1607	1628	1607	1628	1607	1628	1607

Note: White robust standard errors clustered on country in parentheses. (*p<0.1; **p<0.05; ***p<0.01)

Table 4.4: Ordered Probit Models for the Effect of Economic Sanctions on Killings (1981-2000)

	<i>Killings</i>							
	(Model 1)	(Model 1P)	(Model 2)	(Model 2P)	(Model 3)	(Model 3P)	(Model 4)	(Model 4P)
Economic sanctions	0.232*** (0.072)	0.156* (0.080)						
Human rights sanctions			0.199** (0.096)	0.270*** (0.085)				
Non-human-rights sanctions			0.247*** (0.083)	0.142 (0.108)				
Multilateral sanctions					0.348*** (0.083)	0.256*** (0.097)		
Unilateral sanctions					0.104 (0.082)	0.134 (0.089)		
Sanction years							0.147*** (0.039)	0.139*** (0.046)
GDP per capita (log)	-0.177*** (0.038)	-0.192*** (0.035)	-0.179*** (0.039)	-0.188*** (0.035)	-0.178*** (0.039)	-0.188*** (0.035)	-0.177*** (0.038)	-0.189*** (0.035)
Democracy	0.001 (0.006)	0.003 (0.006)	0.001 (0.006)	0.003 (0.006)	0.001 (0.006)	0.003 (0.006)	0.002 (0.006)	0.003 (0.006)
Civil war	0.613*** (0.095)	0.686*** (0.095)	0.615*** (0.096)	0.687*** (0.095)	0.616*** (0.097)	0.677*** (0.096)	0.616*** (0.096)	0.683*** (0.097)
Interstate war	0.397** (0.162)	0.061 (0.135)	0.392** (0.161)	0.073 (0.135)	0.423*** (0.164)	0.083 (0.138)	0.449*** (0.160)	0.032 (0.139)
Past practice	1.092*** (0.086)	1.086*** (0.083)	1.092*** (0.086)	1.077*** (0.084)	1.084*** (0.086)	1.079*** (0.083)	1.101*** (0.083)	1.076*** (0.082)
Log-pseudo likelihood	-1108.476	-1115.324	-1108.399	-1113.674	-1105.504	-1113.252	-1108.262	-1110.693
N	1620	1600	1620	1600	1620	1600	1620	1600

Note:

White robust standard errors clustered on country in parentheses. (*p<0.1; **p<0.05; ***p<0.01)

Table 4.5: Ordered Probit Models for the Effect of Human Rights Sanctions on Political Imprisonment (1981-2000)

	<i>Political imprisonment</i>							
	(Model 1)	(Model 1P)	(Model 2)	(Model 2P)	(Model 3)	(Model 3P)	(Model 4)	(Model 4P)
Economic sanctions	0.172* (0.092)	0.196** (0.082)						
Human rights sanctions			0.059 (0.146)	0.135 (0.142)				
Non-human-rights sanctions			0.233** (0.097)	0.359*** (0.097)				
Multilateral sanctions					0.246* (0.139)	0.165 (0.137)		
Unilateral sanctions					0.045 (0.083)	0.262*** (0.100)		
Sanction years							0.148** (0.065)	0.166*** (0.060)
GDP per capita (log)	-0.099*** (0.032)	-0.114*** (0.039)	-0.104*** (0.032)	-0.107*** (0.038)	-0.097*** (0.032)	-0.109*** (0.039)	-0.100*** (0.032)	-0.108*** (0.039)
Democracy	-0.037*** (0.007)	-0.035*** (0.007)	-0.037*** (0.007)	-0.035*** (0.007)	-0.037*** (0.007)	-0.035*** (0.007)	-0.037*** (0.007)	-0.035*** (0.007)
Civil war	0.408*** (0.104)	0.436*** (0.118)	0.415*** (0.104)	0.448*** (0.117)	0.414*** (0.104)	0.430*** (0.117)	0.401*** (0.103)	0.421*** (0.118)
Interstate war	0.211 (0.186)	0.076 (0.165)	0.183 (0.186)	0.032 (0.170)	0.228 (0.185)	0.064 (0.170)	0.261 (0.184)	0.051 (0.175)
Past practice	1.119*** (0.058)	1.093*** (0.061)	1.122*** (0.058)	1.077*** (0.061)	1.117*** (0.058)	1.082*** (0.062)	1.114*** (0.058)	1.083*** (0.063)
Log-pseudo likelihood	-1108.74	-1098.58	-1107.795	-1094.549	-1107.937	-1095.671	-1106.533	-1093.267
N	1623	1602	1623	1602	1623	1602	1623	1602

Note:

White robust standard errors clustered on country in parentheses. (*p<0.1; **p<0.05; ***p<0.01)

Table 4.6: Ordered Probit Models for the Effect of Economic Sanctions on Torture (1981-2000)

	<i>Torture</i>							
	(Model 1)	(Model 1P)	(Model 2)	(Model 2P)	(Model 3)	(Model 3P)	(Model 4)	(Model 4P)
Economic sanctions	0.292*** (0.061)	0.275*** (0.059)						
Human rights sanctions			0.324*** (0.102)	0.397*** (0.122)				
Non-human-rights sanctions			0.280*** (0.071)	0.255*** (0.071)				
Multilateral sanctions					0.416*** (0.084)	0.401*** (0.096)		
Unilateral sanctions					0.194*** (0.073)	0.226*** (0.073)		
Sanction years							0.193*** (0.044)	0.167*** (0.041)
GDP per capita (log)	-0.137*** (0.038)	-0.148*** (0.038)	-0.136*** (0.038)	-0.140*** (0.038)	-0.136*** (0.038)	-0.141*** (0.038)	-0.137*** (0.038)	-0.143*** (0.038)
Democracy	-0.008 (0.006)	-0.006 (0.007)	-0.008 (0.006)	-0.006 (0.007)	-0.008 (0.006)	-0.006 (0.007)	-0.007 (0.006)	-0.006 (0.007)
Civil war	0.312*** (0.095)	0.372*** (0.103)	0.311*** (0.095)	0.377*** (0.101)	0.308*** (0.093)	0.364*** (0.101)	0.323*** (0.092)	0.379*** (0.101)
Interstate war	0.223 (0.223)	0.188 (0.192)	0.228 (0.223)	0.195 (0.191)	0.238 (0.219)	0.212 (0.193)	0.293 (0.222)	0.184 (0.190)
Past practice	1.141*** (0.078)	1.104*** (0.079)	1.141*** (0.078)	1.095*** (0.078)	1.133*** (0.079)	1.101*** (0.079)	1.136*** (0.079)	1.096*** (0.079)
Log-pseudo likelihood	-1180.507	-1172.272	-1180.453	-1171.37	-1177.711	-1170.243	-1180.843	-1171.811
N	1628	1607	1628	1607	1628	1607	1628	1607

Note:

White robust standard errors clustered on country in parentheses. (*p<0.1; **p<0.05; ***p<0.01)

Table 4.7: Ordered Probit Models for the Effect of Economic Sanctions on Physical Integrity Rights (1981-2000)

	<i>Physical integrity rights</i>							
	(Model 1)	(Model 1P)	(Model 2)	(Model 2P)	(Model 3)	(Model 3P)	(Model 4)	(Model 4P)
Economic sanctions	0.169*** (0.048)	0.134*** (0.051)						
Human rights sanctions			0.077 (0.083)	0.153* (0.078)				
Non-human-rights sanctions			0.211*** (0.050)	0.154** (0.062)				
Multilateral sanctions					0.237*** (0.063)	0.226*** (0.061)		
Unilateral sanctions					0.105* (0.057)	0.104* (0.055)		
Sanction years							0.110*** (0.033)	0.116*** (0.034)
GDP per capita (log)	-0.141*** (0.028)	-0.161*** (0.029)	-0.146*** (0.028)	-0.158*** (0.028)	-0.141*** (0.028)	-0.159*** (0.029)	-0.141*** (0.028)	-0.158*** (0.029)
Democracy	-0.013*** (0.005)	-0.013** (0.005)	-0.013*** (0.005)	-0.012** (0.005)	-0.013*** (0.005)	-0.013** (0.005)	-0.013*** (0.005)	-0.013** (0.005)
Civil war	0.464*** (0.086)	0.534*** (0.098)	0.469*** (0.087)	0.538*** (0.097)	0.467*** (0.085)	0.531*** (0.098)	0.472*** (0.084)	0.541*** (0.098)
Interstate war	0.186 (0.136)	0.033 (0.102)	0.160 (0.134)	0.029 (0.104)	0.206 (0.135)	0.060 (0.103)	0.235* (0.131)	0.016 (0.106)
Past practice	0.555*** (0.025)	0.538*** (0.026)	0.556*** (0.025)	0.536*** (0.027)	0.552*** (0.025)	0.535*** (0.027)	0.555*** (0.025)	0.532*** (0.027)
Log-pseudo likelihood	-2465.765	-2457.208	-2464.793	-2456.654	-2463.912	-2455.224	-2465.87	-2453.388
N	1615	1595	1615	1595	1615	1595	1615	1595

Note:

White robust standard errors clustered on country in parentheses. (*p<0.1; **p<0.05; ***p<0.01)

Human rights sanctions for *physical integrity rights* do not yield significant results in my model. However, this is not entirely surprising, as Peksen's corresponding results are only significant at the 10%-level. Furthermore, non-human-rights sanctions are significant at the 1%-level in my analysis, though these did not yield significant result in Peksen's. Finally, human rights sanctions for disappearances are only significant at the 10%-level, but examining the p-value indicates that the results are not too far from the 5%-threshold (0.07).

Moving on to the models looking at multilateral and unilateral sanctions, Peksen's results are slightly less consistent – unilateral sanctions do not yield significant results for either *disappearance* or *killings*. However, this is in line with Peksen's theoretical expectations that multilateral sanctions are more detrimental to human rights than unilateral ones. That being said, multilateral sanctions do not yield significant results for political imprisonment in his models.

Again, the results from the replication analysis are mostly in line with Peksen's findings, with one notable exception. For *political imprisonment*, our results are reversed, with the replication analysis yielding significant results for multilateral sanctions but not unilateral sanctions. It is also worth noting the significant results at the 10%-level for unilateral sanctions and *disappearance* – these results are not significant in Peksen's analysis.

Finally, *sanction years*, indicating the duration of imposed sanctions, yields consistently positive coefficients significant at at least the 5%-level in both mine and Peksen's models. These results indicate that sanctions continue to have a negative impact in the long term (Peksen 2009: 68-69).

Overall, the ordered probit regression analysis results indicate that, by and large, the replication analysis suggest the same substantive conclusions as Peksen's original analysis. Significant results in Peksen are generally also significant in the replication analysis, with a couple of exceptions. Furthermore, the direction of all coefficients are the same throughout both analyses. This indicates that the effect of sanctions on physical integrity rights, when they do materialize, are unequivocally detrimental – in line with the conclusions Peksen draws from his study (Peksen 2009: 74). Consequently the first criteria for a successful replication of results, out-

lined in section 4.1, is fulfilled.

The next section evaluates whether the corresponding models in tables 4.3–4.7 fit the data to a comparable degree – our second criteria for successful replication of results.

Goodness-of-fit

Goodness-of-fit measures indicate the explanatory power of a statistical model – that is, how well the model fits a set of observations. Goodness-of-fit typically indicate the difference between observed values and the values we would expect from the model.

A common goodness-of-fit measure is the log-likelihood value. Log-likelihood can not be used as an indicator of fit on its own, since the value depends on the sample size. However, it can be used to compare two models where one is a special case of the other, with higher log-likelihood values indicating a better model fit. For replication analyses, the log-likelihood value provides a measure of how well the new models correspond to those in the original analysis. Similar log-likelihood values would indicate that the replication is successful in terms of the extent to which the models fit the data.

Examining the log-likelihood values for the replicated models above, we see that these are almost identical to those for the corresponding models in the original analysis. Overall, model fit is slightly worse for the models using the replicated dataset, though the difference remains very small throughout and is not sufficient to deem the replication analysis unsuccessful. Consequently, the comparison of goodness-of-fit indicates that the second criteria for replication of results is fulfilled.⁵

The next, and final section examines relative size of effects between the original and the replication analysis, as well between different sanctions indicators, based the results from the postestimation analysis.

⁵Because we report robust standard errors, standard likelihood-ratio tests do not apply. The log-likelihood of such models are generally referred to as “log-pseudo-likelihood”, but can still be applied to assess model fit.

Size of effects

As indicated in section 3.4, the effect of an independent variable on the dependent variables in ordered regression models is dependent on the value of the other variables in the model. The effect of individual variables therefore have to be determined with reference to specific scenarios - that is, where all other variables are held constant at a specific value. There are several methods for postestimation analysis of ordered regression coefficients, though they all have in common that the attempt to illustrate the effect a change in an independent variable has on the probability of a specific value on the dependent variable (Nagler 1994).

Tables 4.8 and 4.9 report estimates for how much the probability of frequent human rights abuses (i.e. the highest category of the dependent variable) changes when going from no sanctions to partial sanctions, and from no sanctions to extensive sanctions. The reported probabilities are obtained by holding all control variables constant at their means, with all independent variables in the model set to 0. The independent variable of interest is then changed from 0 to 1 and from 0 to 2. The percentage change in probability for each independent variable are presented in separate columns. Table 4.8 reproduces the corresponding table from Peksen's original analysis. Table 4.9 reports the probabilities obtained using the same method on the models from the replication analysis.

Table 4.8 shows that extensive sanctions appear to be more detrimental to human rights than partial sanctions. Looking at the general economic sanctions variable (Model 1P), the predicted probability of frequent violations of disappearances increases by 49% under partial sanctions and 115% under extensive sanctions. The corresponding numbers for extra-judicial killings are 29% and 64%; 27% and 57% for political imprisonment; and 30% and 61% for torture.

In line with Peksen's analysis, the changes in predicted probabilities shown in Table 4.9 suggest that extensive sanctions are consistently more detrimental to human rights in targeted countries than limited sanctions. Under no circumstances are the increase in predicted probabilities larger for partial sanctions than for extensive sanctions, or even similar.

With regard to sanction objectives, Peksen's results suggest that human rights

Table 4.8: The impact of sanctions variables on the occurrence of frequent violations of integrity rights (original data)

	No sanctions	Partial sanctions (0 to 1)	% change (0 to 1)	Extensive sanctions (0 to 2)	% change (0 to 2)
Disappearances					
Economic sanctions (all)	0.030	0.045	49	0.065	115
Human rights sanctions	0.031	0.049	62	0.077	151
Non-human-rights sanctions	0.031	0.045	46	0.063	107
Multilateral sanctions	0.031	0.049	57	0.074	139
Unilateral sanctions	0.031	0.041	32	0.053	72
Extra-judicial killings					
Economic sanctions (all)	0.107	0.139	29	0.176	64
Human rights sanctions	0.106	0.164	55	0.240	126
Multilateral sanctions	0.107	0.161	51	0.232	117
Political imprisonment					
Economic sanctions (all)	0.239	0.304	27	0.375	57
Non-human-rights sanctions	0.237	0.361	52	0.501	111
Unilateral sanctions	0.237	0.325	37	0.424	79
Torture					
Economic sanctions (all)	0.353	0.459	30	0.569	61
Human rights sanctions	0.354	0.509	44	0.663	87
Non-human-rights sanctions	0.354	0.452	28	0.554	56
Multilateral sanctions	0.355	0.512	44	0.667	88
Unilateral sanctions	0.355	0.443	25	0.533	50

The reported probabilities are calculated by holding all control variables in the models reported in Tables 1-4 constant at their means, while holding the ordinal sanction variables at 0. Then, the ordinal sanction variable under consideration is altered from 0 (no sanction) to 1 (partial sanctions) and from 0 to 2 (extensive sanctions) to determine the individual effect of it.

Table 4.9: The impact of sanctions variables on the occurrence of frequent violations of integrity rights (new data)

	No sanctions	Partial sanctions (0 to 1)	% change (0 to 1)	Extensive sanctions (0 to 2)	% change (0 to 2)
Disappearances					
Economic sanctions (all)	0.037	0.055	59	0.079	114
Human rights sanctions	0.037	0.052	39	0.070	89
Non-human-rights sanctions	0.037	0.057	53	0.084	127
Multilateral sanctions	0.037	0.060	63	0.093	154
Unilateral sanctions	0.037	0.051	40	0.070	91
Extra-judicial killings					
Economic sanctions (all)	0.103	0.152	47	0.213	106
Human rights sanctions	0.103	0.144	39	0.194	88
Multilateral sanctions	0.103	0.180	74	0.285	176
Political imprisonment					
Economic sanctions (all)	0.239	0.295	24	0.357	50
Non-human-rights sanctions	0.239	0.316	33	0.403	69
Torture					
Economic sanctions (all)	0.351	0.464	32	0.580	65
Human rights sanctions	0.351	0.476	36	0.604	72
Non-human-rights sanctions	0.351	0.459	31	0.570	62
Multilateral sanctions	0.350	0.512	46	0.672	92
Unilateral sanctions	0.350	0.424	21	0.501	43

The reported probabilities are calculated by holding all control variables in the models reported in Tables 1-4 constant at their means, while holding the ordinal sanction variables at 0. Then, the ordinal sanction variable under consideration is altered from 0 (no sanction) to 1 (partial sanctions) and from 0 to 2 (extensive sanctions) to determine the individual effect of it.

sanctions increase the likelihood of frequent violations to a larger degree than non-human-rights sanctions – for disappearances, the likelihood of frequent violations increase 151% when going from no sanctions to extensive human rights sanctions, compared to 103% for sanctions with other objectives.

Torture shows a similar pattern, with an 87% increase for extensive human rights sanctions and a 57% increase for other sanctions. It should be noted, however, that comparison between the two is only possible for disappearances and torture, as human rights sanctions for political imprisonment and non-human rights for extra-judicial killings failed to yield significant results. Consequently, while the results support Peksen's conclusion, the evidence is less forceful than for sanctions in general.

I was, however, unable to replicate this pattern with the reconstructed dataset. Inspecting table 4.9, human rights sanctions are not shown to be consistently worse than non-human-rights sanctions in increasing the likelihood of repression. For disappearances, non-human-rights sanctions are in fact shown to be considerably worse than human rights sanctions (127% vs 89%), while for torture, human rights sanctions are only slightly worse than those with alternative objectives (72% vs 62%). It is possible that this discrepancy is due to my coding of the human rights/non-human-rights differing from Peksen's, or some difference between the coding of the old and new version of TIES – however, it is hard to determine this conclusively without Peksen's log files. If the issue is miscoding, however, the implications are not altogether damning. It would simply mean that we should not expect human rights sanctions to be either more or less detrimental to repressive practices than sanctions with other goals – a small, but not crucial deviation from Peksen's findings.

Finally, Peksen's postestimation analysis indicates that multilateral sanctions appear to be considerably more detrimental to human rights than unilateral sanctions. For disappearances, the likelihood of frequent violations increases 139% when going from no sanctions to extensive multilateral sanctions, compared to 72% for unilateral sanctions. For torture the increase is 88% for multilateral sanctions and a 50% for unilateral sanctions. Once again the comparison is not possible with regard to extra-judicial killings or political imprisonment, which limits the

conclusions we can draw from Peksen's analysis.

Contrary to the human rights sanctions, the findings for multilateral and unilateral sanctions in the replication analysis do conform to Peksen's results. Table 4.9 shows that for disappearances, the likelihood of frequent violations increases 154% when going from no sanctions to extensive multilateral sanctions, compared to 91% for unilateral sanctions. For torture the increase is 92% for multilateral sanctions and a 43% for unilateral sanctions.

Overall, the comparison of the postestimation analyses indicate that, with the exception of the relative impact of human rights sanctions, the replication analysis yield the same substantive results as Peksen's analysis: extensive sanctions appear to be more detrimental to human rights than limited sanctions across the board, and multilateral sanctions appear to be more detrimental than unilateral sanctions. Consequently, it is reasonable to conclude that the third replication criteria outlined in section 4.1 is fulfilled to a reasonable degree.

4.4 Summary

In section 4.1, I outlined the core criteria determining whether my replication could be deemed successful or not. The first criteria concerned the datasets themselves, and whether the number of observations and the distribution of values on individual variables corresponded between the original and the replicated dataset. As indicated in section 4.2, I determined that the reconstructed dataset corresponded reasonably well with Peksen's analysis sample.

The second criteria concerned whether it was possible to draw the same substantive conclusion from the replication analysis as from the original. I suggested three separate criteria, relating to the significance and direction of effects, model fit, and size of effects, respectively.

With the exception of the relative impact of human rights and non-human-rights sanctions, the replication analysis yielded the same overall findings from Peksen's original study, indicating the robustness of his analysis. The size and direction of effects were by and large consistent with Peksen's findings, model-fit

was consistent across corresponding models, and substantive results indicate the same relationship between limited and extensive sanctions, and multilateral and unilateral sanctions, as predicted from the theoretical framework.

The successful replication indicates that the rebuilt dataset corresponds to a satisfactory degree to Peksen's original dataset. Consequently, in the following chapter I move on to expand upon the replicated dataset by adding additional country-year observations, as well as relevant observations omitted from Peksen's original analysis. I then re-estimate the models from this chapter, using the extended dataset, to examine whether Peksen's conclusions hold when accounting for the broader scope of available data. Since the original and replicated datasets and analyses are reasonably similar, I can be confident that whatever differences in results arise from the extended analysis are due to substantive differences, rather than discrepancies in data, research design or model specifications.

Chapter 5

Expanding the dataset

In section 2.5 I presented the expected outcome for the analysis using the extended economic sanctions data in the following hypothesis:

H₁: Economic coercion will likely cause the unintended consequence of more physical integrity rights violations in target countries.

The following chapter sets out to test this hypothesis. Furthermore, it aims to assess the relative impact of sanctions when compared to the replication analysis in chapter 4. I present the results from the analysis of the effect of economic sanctions on human rights using the expanded dataset with additional sanctions cases. As outlined in section 3.3, the expanded dataset includes sanction cases between 1981-2005, representing an extension of the analysis period of 25% compared to Peksen's original study. Additionally, a number of observations were omitted from the replication analysis to make it correspond more closely to the replicated analysis. Previously omitted relevant observations have been included in the expanded analysis - these include new sanction cases, as well as additional country-year observations for sanctions cases already included in the original dataset.

The analysis is conducted using the same research design and specifications as the ones carried out in chapter 4 – the only difference is the dataset. The rest of this chapter proceeds as follows. First, I compare and contrast the expanded

dataset with the replication dataset, using summary statistics. Second, I assess the direction and significance of the results from the 20 ordered probit models, and contrast these with the results from the replication analysis. Third, I conduct a postestimation analysis examining changes in predicted probabilities for frequent violations of physical integrity rights for significant independent variables, and compare these to corresponding results from the replication analysis. I conclude the chapter with a summary of findings and some notes on the wider implications for the literature.

5.1 Comparing the datasets

Table 5.1 reports the summary statistics for all variables used in the replication analysis in chapter 4. Table 5.2 reports the summary statistics for all variables used in the analysis with the extended dataset.

Comparing the number of observations in table 5.1 and 5.2, we see that the new dataset represents a significant expansion of data, with the inclusion of approximately 1100 additional observations from the replication analysis in the preceding chapter. To put this in perspective: while the additional sanction years added with the updated TIES data represented a 25% increase in the timespan covered, compared to the replication analysis, the extended dataset represents an almost 70% increase in observations. Insofar one wishes to investigate the impact of sanctions on the CIRI human rights indicators, within the theoretical framework presented in this thesis, the extended dataset accounts for the widest possible scope of available, relevant sanctions data.

It is interesting to note, however, that despite the increase in observation, the distribution of values on the variables does not differ significantly from that of the replication dataset. With the exception of the physical integrity rights index, sanction years, and democracy variables, all mean values are within ± 1 decimal point of each other. The same applies to the standard deviations, with democracy as the only exception (off by 2 decimal points).

Table 5.1: Summary Statistics - replication dataset

Statistic	N	Mean	St. Dev.	Min	Max
Disappearances	1,628	0.425	0.710	0	2
Extra-judicial killings	1,620	0.760	0.811	0	2
Political imprisonment	1,623	1.015	0.852	0	2
Torture	1,628	1.263	0.738	0	2
Physical integrity index	1,615	3.450	2.417	0	8
Economic sanctions	1,628	0.276	0.536	0	2
Human rights sanctions	1,628	0.092	0.338	0	2
Non-human-rights sanctions	1,628	0.184	0.454	0	2
Multilateral sanctions	1,628	0.145	0.429	0	2
Unilateral sanctions	1,628	0.142	0.392	0	2
Sanction years	1,628	1.603	4.724	0	47
GDP per capita (log)	1,628	8.163	1.536	4.808	11.626
Democracy	1,628	1.656	7.324	−10	10
Civil war	1,628	0.251	0.433	0	1
Interstate war	1,628	0.039	0.193	0	1

Table 5.2: Summary statistics - expanded dataset

Statistic	N	Mean	St. Dev.	Min	Max
Disappearances	2,766	0.353	0.658	0	2
Extra-judicial killings	2,757	0.700	0.785	0	2
Political imprisonment	2,761	0.876	0.847	0	2
Torture	2,765	1.233	0.740	0	2
Physical integrity index	2,749	3.151	2.356	0	8
Economic sanctions	2,766	0.389	0.584	0	2
Human rights sanctions	2,766	0.090	0.342	0	2
Non-human-rights sanctions	2,766	0.299	0.527	0	2
Multilateral sanctions	2,766	0.181	0.457	0	2
Unilateral sanctions	2,766	0.216	0.464	0	2
Sanction years	2,766	1.976	4.924	0	47
GDP per capita (log)	2,766	8.178	1.589	4.808	11.626
Democracy	2,766	2.457	7.111	−10	10
Civil War	2,766	0.197	0.398	0	1
Interstate war	2,766	0.004	0.066	0	1

While it is too early to say anything conclusively at this stage, the comparison of the datasets suggests that we can expect the results from the extended analysis to not differ too greatly from the replication analysis. To assess whether or not this is the case, the next section examines the results from the ordered probit regression models and compares these with the results from the replication analysis in chapter 4. The subsequent section discusses the results from the postestimation analysis with the extended dataset, and compares these results to those for the replication analysis.

5.2 Comparing the results: significance and direction

Tables 5.2-5.7 report the results from ordered probit regression models using the CIRI indicators of disappearance, extra-judicial killings, political imprisonment, torture, and physical integrity rights. For each table, models 1-4 show the results from the analysis using the extended dataset. For comparative purposes, models 1R-4R show the results from the analysis using the reconstructed replication dataset, as presented in chapter 4.¹

We begin by examining the models indicating the general effect of economic sanctions on physical integrity rights abuses (Models 1/1R). The first thing to note is that all results from the analysis with the extended dataset are significant, just like in the for the analysis with the replication dataset. However, the extended analysis differs in that all coefficients are significant at the 1%–level, indicating that it is highly unlikely that the results from the regression analysis is due to chance.

Furthermore, all coefficients are positive, suggesting that economic sanctions overall have a detrimental impact on human rights practices for each of the individual rights as well as physical integrity rights generally. The size of coefficients vary slightly compared to the replication analysis, but not to a noteworthy degree. Some coefficients are slightly larger than their counterpart (*killings, political*

¹As in the replication analysis, I tested for violations of the parallel regression assumption and checked for issues with multicollinearity for all models. Neither test indicated any violations of the parallel regression assumption or issues with multicollinearity.

Table 5.3: Ordered Probit Models for the Effect Economic Sanctions on Disappearance (1981-2005/1981-2000)

	<i>Disappearance</i>							
	(Model 1)	(Model 1R)	(Model 2)	(Model 2R)	(Model 3)	(Model 3R)	(Model 4)	(Model 4R)
Economic sanctions	0.174*** (0.054)	0.188*** (0.056)						
Human rights sanctions			0.136* (0.077)	0.156* (0.087)				
Non-human-rights-sanctions			0.192*** (0.058)	0.204*** (0.060)				
Multilateral sanctions					0.203*** (0.064)	0.234*** (0.079)		
Unilateral sanctions					0.159** (0.068)	0.158* (0.081)		
Sanction years							0.102*** (0.039)	0.112*** (0.040)
GDP per capita (log)	-0.107*** (0.037)	-0.102** (0.042)	-0.110*** (0.036)	-0.104** (0.042)	-0.107*** (0.037)	-0.101** (0.042)	-0.101*** (0.036)	-0.100** (0.041)
Democracy	-0.008 (0.006)	0.002 (0.008)	-0.008 (0.006)	0.002 (0.008)	-0.007 (0.006)	0.002 (0.008)	-0.008 (0.006)	0.001 (0.008)
Civil war	0.689*** (0.104)	0.663*** (0.110)	0.693*** (0.104)	0.664*** (0.110)	0.684*** (0.104)	0.657*** (0.111)	0.696*** (0.102)	0.672*** (0.109)
Interstate war	0.259 (0.278)	0.169 (0.165)	0.246 (0.274)	0.158 (0.165)	0.259 (0.281)	0.183 (0.163)	0.392 (0.280)	0.231 (0.158)
Past practice	1.147*** (0.100)	1.072*** (0.105)	1.149*** (0.101)	1.073*** (0.105)	1.144*** (0.101)	1.068*** (0.106)	1.154*** (0.099)	1.078*** (0.104)
Log-pseudo likelihood	-1296.783	-868.111	-1296.567	-868.020	-1295.559	-866.739	-1298.467	-868.935
N	2766	1628	2766	1628	2766	1628	2766	1628

Note: White robust standard errors clustered on country in parentheses. (*p<0.1; **p<0.05; ***p<0.01)

Table 5.4: Ordered Probit Models for the Effect Economic Sanctions on Killings (1981-2005/1981-2000)

	<i>Killings</i>							
	(Model 1)	(Model 1R)	(Model 2)	(Model 2R)	(Model 3)	(Model 3R)	(Model 4)	(Model 4R)
Economic sanctions	0.276*** (0.050)	0.232*** (0.072)						
Human rights sanctions			0.252*** (0.076)	0.199** (0.096)				
Non-human-rights-sanctions			0.284*** (0.060)	0.247*** (0.083)				
Multilateral sanctions					0.306*** (0.060)	0.348*** (0.083)		
Unilateral sanctions					0.249*** (0.066)	0.104 (0.082)		
Sanction years							0.174*** (0.038)	0.147*** (0.039)
GDP per capita (log)	-0.203*** (0.030)	-0.177*** (0.038)	-0.204*** (0.030)	-0.179*** (0.039)	-0.203*** (0.030)	-0.178*** (0.039)	-0.196*** (0.030)	-0.177*** (0.038)
Democracy	-0.002 (0.005)	0.001 (0.006)	-0.002 (0.005)	0.001 (0.006)	-0.001 (0.005)	0.001 (0.006)	-0.001 (0.005)	0.002 (0.006)
Civil war	0.571*** (0.095)	0.613*** (0.095)	0.574*** (0.094)	0.615*** (0.096)	0.564*** (0.095)	0.616*** (0.097)	0.572*** (0.094)	0.616*** (0.096)
Interstate war	0.370 (0.263)	0.397** (0.162)	0.367 (0.260)	0.392** (0.161)	0.374 (0.264)	0.423*** (0.164)	0.561* (0.288)	0.449*** (0.160)
Past practice	1.157*** (0.067)	1.092*** (0.086)	1.158*** (0.067)	1.092*** (0.086)	1.155*** (0.068)	1.084*** (0.086)	1.169*** (0.066)	1.101*** (0.083)
Log-pseudo likelihood	-1808.593	-1108.476	-1808.522	-1108.399	-1807.19	-1105.504	-1809.793	-1108.262
N	2757	1620	2757	1620	2757	1620	2757	1620

Note:

White robust standard errors clustered on country in parentheses. (*p<0.1; **p<0.05; ***p<0.01)

Table 5.5: Ordered Probit Models for the Effect Economic Sanctions on Political Imprisonment (1981-2005/1981-2000)

	<i>Political imprisonment</i>							
	(Model 1)	(Model 1R)	(Model 2)	(Model 2R)	(Model 3)	(Model 3R)	(Model 4)	(Model 4R)
Economic sanctions	0.215*** (0.066)	0.172* (0.092)						
Human rights sanctions			0.136 (0.112)	0.059 (0.146)				
Non-human-rights-sanctions			0.246*** (0.073)	0.233** (0.097)				
Multilateral sanctions					0.202** (0.093)	0.246* (0.139)		
Unilateral sanctions					0.186*** (0.065)	0.045 (0.083)		
Sanction years							0.139*** (0.046)	0.148** (0.065)
GDP per capita (log)	-0.105*** (0.026)	-0.099*** (0.032)	-0.108*** (0.027)	-0.104*** (0.032)	-0.103*** (0.026)	-0.097*** (0.032)	-0.100*** (0.026)	-0.100*** (0.032)
Democracy	-0.050*** (0.006)	-0.037*** (0.007)	-0.050*** (0.006)	-0.037*** (0.007)	-0.049*** (0.006)	-0.037*** (0.007)	-0.049*** (0.006)	-0.037*** (0.007)
Civil war	0.468*** (0.104)	0.408*** (0.104)	0.481*** (0.105)	0.415*** (0.104)	0.464*** (0.104)	0.414*** (0.104)	0.475*** (0.103)	0.401*** (0.103)
Interstate war	-0.049 (0.235)	0.211 (0.186)	-0.075 (0.230)	0.183 (0.186)	-0.034 (0.233)	0.228 (0.185)	0.059 (0.221)	0.261 (0.184)
Past practice	1.164*** (0.055)	1.119*** (0.058)	1.163*** (0.055)	1.122*** (0.058)	1.166*** (0.055)	1.117*** (0.058)	1.166*** (0.056)	1.114*** (0.058)
Log-pseudo likelihood	-1757.836	-1108.74	-1757.068	-1107.795	-1759.265	-1107.937	-1759.093	-1106.533
N	2761	1623	2761	1623	2761	1623	2761	1623

Note: White robust standard errors clustered on country in parentheses. (*p<0.1; **p<0.05; ***p<0.01)

Table 5.6: Ordered Probit Models for the Effect Economic Sanctions on Torture (1981-2005/1981-2000)

	<i>Torture</i>							
	(Model 1)	(Model 1R)	(Model 2)	(Model 2R)	(Model 3)	(Model 3R)	(Model 4)	(Model 4R)
Economic sanctions	0.283*** (0.054)	0.292*** (0.061)						
Human rights sanctions			0.369*** (0.097)	0.324*** (0.102)				
Non-human-rights-sanctions			0.264*** (0.060)	0.280*** (0.071)				
Multilateral sanctions					0.382*** (0.060)	0.416*** (0.084)		
Unilateral sanctions					0.217*** (0.071)	0.194*** (0.073)		
Sanction years							0.187*** (0.049)	0.193*** (0.044)
GDP per capita (log)	-0.170*** (0.029)	-0.137*** (0.038)	-0.168*** (0.029)	-0.136*** (0.038)	-0.169*** (0.029)	-0.136*** (0.038)	-0.162*** (0.030)	-0.137*** (0.038)
Democracy	-0.011** (0.005)	-0.008 (0.006)	-0.011** (0.005)	-0.008 (0.006)	-0.011** (0.005)	-0.008 (0.006)	-0.011** (0.005)	-0.007 (0.006)
Civil war	0.383*** (0.089)	0.312*** (0.095)	0.373*** (0.089)	0.311*** (0.095)	0.374*** (0.088)	0.308*** (0.093)	0.389*** (0.087)	0.323*** (0.092)
Interstate war	0.354 (0.243)	0.223 (0.223)	0.364 (0.238)	0.228 (0.223)	0.365* (0.220)	0.238 (0.219)	0.490** (0.201)	0.293 (0.222)
Past practice	1.211*** (0.065)	1.141*** (0.078)	1.209*** (0.065)	1.141*** (0.078)	1.203*** (0.064)	1.133*** (0.079)	1.215*** (0.066)	1.136*** (0.079)
Log-pseudo likelihood	-1933.355	-1180.507	-1932.824	-1180.453	-1929.879	-1177.711	-1934.841	-1180.843
N	2765	1628	2765	1628	2765	1628	2765	1628

Note:

White robust standard errors clustered on country in parentheses. (*p<0.1; **p<0.05; ***p<0.01)

Table 5.7: Ordered Probit Models for the Effect Economic Sanctions on Physical Integrity Rights (1981-2005/1981-2000)

	<i>Physical integrity rights</i>							
	(Model 1)	(Model 1R)	(Model 2)	(Model 2R)	(Model 3)	(Model 3R)	(Model 4)	(Model 4R)
Economic sanctions	0.202*** (0.044)	0.169*** (0.048)						
Human rights sanctions			0.140** (0.066)	0.077 (0.083)				
Non-human-rights-sanctions			0.223*** (0.051)	0.211*** (0.050)				
Multilateral sanctions					0.218*** (0.052)	0.237*** (0.063)		
Unilateral sanctions					0.189*** (0.053)	0.105* (0.057)		
Sanction years							0.131*** (0.030)	0.110*** (0.033)
GDP per capita (log)	-0.162*** (0.024)	-0.141*** (0.028)	-0.165*** (0.024)	-0.146*** (0.028)	-0.162*** (0.024)	-0.141*** (0.028)	-0.156*** (0.024)	-0.141*** (0.028)
Democracy	-0.020*** (0.004)	-0.013*** (0.005)	-0.021*** (0.004)	-0.013*** (0.005)	-0.020*** (0.004)	-0.013*** (0.005)	-0.020*** (0.004)	-0.013*** (0.005)
Civil war	0.445*** (0.087)	0.464*** (0.086)	0.454*** (0.088)	0.469*** (0.087)	0.441*** (0.087)	0.467*** (0.085)	0.448*** (0.086)	0.472*** (0.084)
Interstate war	0.241* (0.141)	0.186 (0.136)	0.228* (0.136)	0.160 (0.134)	0.243* (0.142)	0.206 (0.135)	0.337*** (0.128)	0.235* (0.131)
Past practice	0.582*** (0.024)	0.555*** (0.025)	0.583*** (0.024)	0.556*** (0.025)	0.581*** (0.024)	0.552*** (0.025)	0.585*** (0.023)	0.555*** (0.025)
Log-pseudo likelihood	-4055.081	-2465.765	-4054.356	-2464.793	-4054.167	-2463.912	-4056.282	-2465.87
N	2749	1615	2749	1615	2749	1615	2749	1615

Note: White robust standard errors clustered on country in parentheses. (*p<0.1; **p<0.05; ***p<0.01)

imprisonment, physical integrity rights), while others are slightly smaller (*disappearance, torture*). Overall, the results point to the same general conclusion as in the analysis in the previous chapter.

For the models examining human rights and non-human rights sanctions (Models 2/2R), all significant results from the replication analysis are also significant in the extended analysis, either at the same significance level or lower. Additionally, human rights sanctions for *physical integrity rights* now yield significant results, same as in Peksen's original analysis. Furthermore, the direction and size of coefficients are largely similar to the replication analysis.

We see a similar pattern for the models looking at multilateral and unilateral sanctions (Models 3/3R). Again, all the models using the extended dataset yield significant results, at either the 1%– or 5%–level, with positive coefficients indicating a detrimental impact on human rights throughout. Most noteworthy, however, is that the extended analysis yields significant results for unilateral sanctions for both *killings* and *political imprisonment*, and results at a higher significance level for all other coefficients. An implication of this is that accounting for the longer time-frame and broader set of sanctions reveals a detrimental effect of unilateral sanctions obscured in the previous study, and that less coordinated sanction efforts are no less harmful than multilateral sanctions. The coefficients for multilateral sanctions are still larger throughout, however, indicating that these have a comparatively greater impact on repression.

Finally, *sanction years* (Models 4/4R) once again yields significant and positive coefficients throughout, indicating that sanctions continue to have a negative impact in the long term.

Overall, the results from the ordered probit regression analysis with the extended dataset indicate that, by and large, the substantive conclusions from the replication and Peksen's original analysis remain the same. When accounting for more sanctions data, there is no sign of the negative impact of economic sanctions on physical integrity rights diminishing. To the contrary, the results seem to strengthen the impression that sanction have an adverse affect on repressive practices. Where Peksen's study suggested that unilateral sanctions may not have a

significant impact in many cases, the extended analysis yields unequivocally significant and positive (i.e. detrimental) effects for all sanctions regardless of levels of international coordination.

Next I examine the relative size of effects for individual sanctions indicators for the extended analysis, and compare these to the results from the postestimation analysis with the replication dataset.

5.3 Comparing the datasets: size of effects

Table 5.8 shows the impact of sanction variable on the predicted probability of frequent violations of integrity rights for the analysis using the replication dataset. Table 5.9 shows corresponding predicted probabilities for the analysis with the extended dataset.

Comparing the changes in predicted probabilities for the two analyses, the overall conclusion is that the negative effect of economic sanctions on physical integrity rights persists when the scope of observations is extended. The overarching relationship between partial and extensive sanctions on predicted probabilities remains the same: predicted probabilities for frequent violations of physical integrity rights increase for both limited and extensive sanctions, and are consistently higher for extensive sanctions.

Furthermore, the analysis with the extended dataset supports the conclusion in section 5.2, indicating that the impact of sanctions persists and even increases when we extend the scope of the data: the percentage change in probability of frequent violations when going from no sanctions to extensive sanctions with the extended dataset increases for all but two of the independent variables that are significant across both analyses.

The increase in predicted probabilities for frequent disappearances under human rights sanctions goes down three percentage points compared to the results from the replication (from 89% to 86%); for multilateral sanctions the increase is seven percentage points lower (from 154% to 147%). Across all other independent variables, the increase in predicted probabilities goes up, with the difference rang-

Table 5.8: The impact of sanctions variables on the occurrence of frequent violations of integrity rights (new data)

	No sanctions	Partial sanctions (0 to 1)	% change (0 to 1)	Extensive sanctions (0 to 2)	% change (0 to 2)
Disappearances					
Economic sanctions (all)	0.037	0.055	59	0.079	114
Human rights sanctions	0.037	0.052	39	0.070	89
Non-human-rights sanctions	0.037	0.057	53	0.084	127
Multilateral sanctions	0.037	0.060	63	0.093	154
Unilateral sanctions	0.037	0.051	40	0.070	91
Extra-judicial killings					
Economic sanctions (all)	0.103	0.152	47	0.213	106
Human rights sanctions	0.103	0.144	39	0.194	88
Multilateral sanctions	0.103	0.180	74	0.285	176
Political imprisonment					
Economic sanctions (all)	0.239	0.295	24	0.357	50
Non-human-rights sanctions	0.239	0.316	33	0.403	69
Torture					
Economic sanctions (all)	0.351	0.464	32	0.580	65
Human rights sanctions	0.351	0.476	36	0.604	72
Non-human-rights sanctions	0.351	0.459	31	0.570	62
Multilateral sanctions	0.350	0.512	46	0.672	92
Unilateral sanctions	0.350	0.424	21	0.501	43

The reported probabilities are calculated by holding all control variables in the models reported in Tables 1-4 constant at their means, while holding the ordinal sanction variables at 0. Then, the ordinal sanction variable under consideration is altered from 0 (no sanction) to 1 (partial sanctions) and from 0 to 2 (extensive sanctions) to determine the individual effect of it.

Table 5.9: The impact of sanctions variables on the occurrence of frequent violations of integrity rights (expanded data)

	No sanctions	Partial sanctions (0 to 1)	% change (0 to 1)	Extensive sanctions (0 to 2)	% change (0 to 2)
Disappearances					
Economic sanctions (all)	0.021	0.031	50	0.045	120
Human rights sanctions	0.020	0.028	38	0.038	86
Non-human-rights sanctions	0.020	0.032	56	0.048	137
Multilateral sanctions	0.020	0.033	60	0.050	147
Unilateral sanctions	0.020	0.030	45	0.042	106
Extra-judicial killings					
Economic sanctions (all)	0.064	0.106	66	0.165	160
Human rights sanctions	0.063	0.101	59	0.153	141
Non-human-rights sanctions	0.063	0.107	69	0.169	166
Multilateral sanctions	0.063	0.111	75	0.180	184
Unilateral sanctions	0.063	0.101	59	0.152	139
Political imprisonment					
Economic sanctions (all)	0.135	0.187	39	0.250	85
Non-human-rights sanctions	0.135	0.195	45	0.270	101
Multilateral sanctions	0.137	0.186	36	0.245	79
Unilateral sanctions	0.137	0.182	26	0.235	72
Torture					
Economic sanctions (all)	0.303	0.408	37	0.520	71
Human rights sanctions	0.303	0.442	47	0.588	94
Non-human-rights sanctions	0.299	0.401	34	0.505	67
Multilateral sanctions	0.302	0.446	48	0.598	98
Unilateral sanctions	0.302	0.382	30	0.467	54

The reported probabilities are calculated by holding all control variables in the models reported in Tables 1-4 constant at their means, while holding the ordinal sanction variables at 0. Then, the ordinal sanction variable under consideration is altered from 0 (no sanction) to 1 (partial sanctions) and from 0 to 2 (extensive sanctions) to determine the individual effect of it.

ing from 10 (the effect of non-human-rights sanctions on disappearances - from 127% to 137%) to 53 percentage points (the effect on human rights sanctions on extra-judicial killings - from 88% to 141%).

A benefit of the higher number of significant results, is that it is now possible to perform more extensive comparisons between human rights/non-human-rights sanctions and multilateral/unilateral sanctions than in the replication analysis.

First multilateral sanctions appear to be consistently more detrimental to physical integrity rights than unilateral sanctions. For instance, the increase in the probability of frequent occurrences of extra-judicial killings, when going from no sanctions to extensive sanctions, is 45 percentage points higher for multilateral sanctions than for unilateral sanctions.

The relationship between human rights- and non-human-rights sanctions, however, is less consistent. Non-human-rights sanctions appear to be more detrimental to frequency of disappearances and killings, while human rights sanctions appear to have a greater impact on the use of torture. The results for political imprisonment are inconclusive, due to non-significant results for the effect of human rights sanctions.

It is also instructive to examine the increases in predicted probabilities in light of the absolute probability values, which vary considerably between different integrity rights indicators. We see the greatest increases for indicators for disappearances and killings - however, these indicators are also the ones with the lowest initial probabilities of frequent violations. With no sanctions in place the predicted probability of frequent disappearances is 0.020; under extensive sanctions the probability does not even reach 0.1, ranging between 0.038-0.050.

Similarly, under no sanctions the predicted probability of frequent extra-judicial killings is 0.063. However, here we see a larger increase under extensive sanctions, with predicted probabilities ranging from 0.152 to 0.180. In short, while the risk of increased disappearances and killings appear to increase with the implementation of sanctions, the probability of violation is not all that high to begin with.

We can contrast these values with the risk of frequent incidents of torture, which according to our models is considerably higher: With no sanctions in place,

the predicted probability of frequent violations is already around the 0.3 mark. With extensive sanctions in place, the values range from 0.47 to 0.60, indicating a relatively high probability of frequent use of torture. Predicted probabilities for frequent instances of politically motivated imprisonment fall somewhere between killings and torture, with predicted probabilities between 0.24 and 0.27 under extensive sanctions.

These values seem to indicate that torture and imprisonment are more likely to be employed than killings or disappearances as tools of repression. Furthermore, while the impact of sanctions (in terms of percentage increase in probabilities) may be largest for killings and disappearances, the absolute increases in predicted probabilities are much higher for more widely used tools of repression like torture.

5.4 Summary and implications

The results from the extended analysis indicate that most of Peksen's substantive conclusions hold when both the analysis period and scope of included data are extended. Economic sanctions appear to be just as detrimental to physical integrity rights in the longer term, perhaps even more so, and extensive sanctions indicate consistently larger impacts than partial sanctions. Multilateral sanctions also appear to have a consistently larger effect on predicted probabilities of frequent violations than unilateral sanctions. Effects for human rights sanctions exhibit a less consistent pattern, indicating stronger or weaker effects relative to other types of sanctions depending on the right in question.

In summary, however, the findings from the analysis in this chapter are consistent with the theoretical expectation that sanctions have a generally detrimental impact on repressive practices in targeted countries. Consequently, I am led to conclude that my analysis lends support to hypothesis H_1 presented at the start of this chapter.

This conclusion is in line with and bolsters earlier studies suggesting that sanctions increase repression of citizens in targeted countries, such as Wood (2008), and Escribà-Folch (2012). A broader implication is that state leaders who wish to

use economic sanctions as a foreign policy tool should expect these to have negative extant consequences beyond the potential economic impact. Furthermore, the results suggest that the use of economic sanctions as a means of pressuring repressive regimes into complying with international human rights norms is ineffectual at best, and counter-productive at worst. Finally, it appears that torture and imprisonment are more common tools of repression compared to killings and disappearances.

In the next chapter, I extend the object of analysis to include a wider set of human rights indicators than the commonly examined physical integrity rights, specifically the CIRI empowerment rights variables. The primary goal of this is to examine whether the relationship between economic sanctions and state repression persists when examining human rights violations beyond the most egregious forms of repression. I also seek to assess the relative impact of sanctions on physical integrity and empowerment rights, and to examine the effect on both individual and aggregated indicators of empowerment rights. To my knowledge, both these assessments represent novel contributions to the relevant literature, and are therefore somewhat exploratory in nature.

Chapter 6

The effect of sanctions on empowerment rights

In section 2.5 I presented the expected outcome for the effect of economic sanctions on empowerment rights in the following hypothesis:

H₂: Economic coercion will likely cause the unintended consequence of more empowerment rights violations in targeted countries.

As I am also interested in investigating the relative effect of sanctions on the different categories of physical integrity and empowerment rights, however, I presented two additional (competing) hypotheses:

H_{3a}: The impact of economic coercion on empowerment rights is equivalent to the impact of economic coercion on physical integrity rights.

H_{3b}: The impact of economic coercion on empowerment rights is lesser than the impact of economic coercion on physical integrity rights.

In the following chapter I attempt to test these hypotheses, both by examining the effect of economic sanctions on empowerment rights and by comparing

the findings in this chapter with the results from the analysis of physical integrity rights using the extended dataset. As in Chapter 5, the analysis uses the extended dataset on economic sanctions, in order to account for the broadest possible scope of available sanction data.

The chapter contains a total of 32 models - four for each dependent variable included in the analysis. The dependent variables include all individual empowerment rights indicators from the CIRI dataset, including the empowerment rights index. As discussed in section 3.3, while there is some ambiguity concerning the empirical basis for whether or not to include the CIRI indicator of worker's rights in our operational definition of empowerment rights, it has not been omitted from the analysis. See section 3.3 for a more detailed discussion of why I choose to retain this indicator.

All other aspects of the research design, including independent and control variables, model specification, standard errors, presentation and interpretation of results, and diagnostic tests, are identical to those in preceding chapters. The rest of this chapter first examines the significance and direction of effects, by presenting and analysing the results from the ordered probit regression models. These are briefly contrasted with the results from the analysis in chapter 5. I then go on to examine the size of effects for significant individual sanction indicators, and compare these with the results from postestimation analysis of physical integrity rights in the previous chapter.

Before we move on to the analysis itself, it is worth reiterating how the analysis in this chapter makes two novel contributions to the research literature.

6.1 The merits of disaggregation and a unified research design

In section 2.2, I outlined a number of studies that have examined the impact of sanctions on civil and political rights, operationalizing these rights using a range of different indicators. However, all empirical studies reviewed here have taken a maximalist approach, using aggregated indicators of rights violations. In sec-

tion 2.2 I argued that a thorough investigation into the effect of sanctions on human rights requires the use of both aggregated and individual indicators. Using the CIRI empowerment rights indicators allows me to do just this, and lets me assess to what extent results for more general, aggregated indicators can be assumed to apply for the individual rights indicators its comprised of. It also allows me to contribute to the scholarly debate over whether human rights abuses in essence represent substitutable policy choices, diminishing the need to differentiate between them, or whether disaggregation is necessary to capture behavioural differences (Tate and Keith 1999, Mitchell and McCormick 1988, McCormick and Mitchell 1997, Fariss and Schnakenberg 2014).

The second contribution concerns the comparison of the relative effect of sanctions on physical integrity rights and civil and political rights. As outlined in section 2.2, studies on the effect of sanctions on human rights have generally tested separately, using different models, indicators and specifications (Gutmann, Neuenkirch and Neumeier 2016: 4). Consequently, it is difficult to compare the results from different studies, and to assess the relative impact of sanctions on different types of rights. I have attempted to address this issue through basing my research design on the replication and expansion of a previous study focusing on physical integrity rights, and the expanding the scope of the analysis to encompass a broader category of human rights violations. By using the same models, specifications, datasets, and independent and control variables for both categories of rights, as well as drawing my dependent variables from the same dataset (CIRI), I ensure the comparability of results from the analyses in chapter 5 or 6. The research design in this thesis makes it feasible to compare results not only at the general level of significance and direction of effects, but also with regard to the relative size of the impact of sanctions on these categories of human rights.

6.2 Analysis and comparison of results: significance and direction

Table 6.1 presents summary statistics for all variables included in the analysis. Tables 6.2 to 6.5 shows the results from the analysis using the eight CIRI empowerment rights indicators.

Table 6.1: Summary statistics - empowerment rights dataset

Statistic	N	Mean	St. Dev.	Min	Max
Foreign movement	2,825	0.601	0.715	0	2
Domestic movement	2,822	0.539	0.742	0	2
Freedom of speech	2,808	0.934	0.706	0	2
Freedom of assembly	2,803	0.846	0.835	0	2
Worker's rights	2,806	0.961	0.776	0	2
Electoral self-determination	2,808	0.803	0.831	0	2
Religious freedom	2,815	0.696	0.803	0	2
Empowerment rights index	2,792	5.370	4.167	0	14
Economic sanctions	2,825	0.382	0.582	0	2
Human rights sanctions	2,825	0.088	0.339	0	2
Non-human-rights sanctions	2,825	0.294	0.525	0	2
Multilateral sanctions	2,825	0.177	0.453	0	2
Unilateral sanctions	2,825	0.213	0.463	0	2
Sanction years	2,825	1.945	4.896	0	46
GDP per capita (log)	2,825	8.172	1.584	4.808	11.626
Democracy	2,825	2.290	7.149	−10	10
Civil war	2,825	0.199	0.399	0	1
Interstate war	2,825	0.004	0.065	0	1

As in the previous analyses, I check for violations of the parallel regression assumption and potential issues with multicollinearity for all models in this chapter. While there are no indications of issues with multicollinearity, the parallel regression assumptions appears to be violated for models examining the impact of sanctions on freedom of speech. The assumption also appears to be violated for models assessing the right to foreign movement, although these results are more ambiguous (the model without the parallel regression constrain represented a significant improvement of model fit at the 0.1-level, but not at the 0.05-level).

I estimated multinomial probit regressions for all the models with *foreign movement* and *freedom of speech* as dependent variables. The multinomial models with both *freedom of speech* and *foreign movement* yielded results in line with the findings from the ordered models.¹

Overall effects

Beginning with the model in table 6.5 assessing the overall impact of sanctions on civil and political rights (by way of the empowerment rights index), we notice that all coefficients with the exception of unilateral sanctions are positive and significant. These results indicate that economic sanctions generally lead to more frequent violations of empowerment rights, controlling for factors like economic development, levels of democracy, involvement in conflicts, and past human rights practices. Consequently the results suggest that, on the whole, economic sanctions in general have a negative impact on empowerment rights, similar to physical integrity rights.

Model 6 indicates that both human rights and non-human rights sanctions yield significant results. Though the result for human rights sanctions is only significant at the 10%–level, p-values indicates that it is quite close to the 5% threshold

¹The multinomial models indicate that all sanctions variables have a strong, significant impact on the probability of frequent violations of freedom of speech – coefficients for some violations (a value of 1 on the dependent variable) did not yield significant results. As I am primarily concerned with the predicted probability of frequent violations in my postestimation analyses, these results are consistent with the conclusions in section 6.3.

Table 6.2: Ordered Probit Models for the Effect of Economic Sanctions on Foreign Movement and Domestic Movement (1981-2005)

	<i>Foreign movement</i>				<i>Domestic movement</i>			
	(Model 1)	(Model 2)	(Model 3)	(Model 4)	(Model 5)	(Model 6)	(Model 7)	(Model 8)
Economic sanctions	0.134** (0.061)				0.017 (0.057)			
Human rights sanctions		0.110 (0.104)				0.105 (0.088)		
Non-human-rights sanctions		0.144** (0.066)				-0.023 (0.065)		
Multilateral sanctions			0.225*** (0.070)				0.083 (0.078)	
Unilateral sanctions			0.043 (0.070)				-0.048 (0.065)	
Sanction years				0.076** (0.038)				0.008 (0.036)
GDP per capita (log)	0.023 (0.031)	0.021 (0.031)	0.026 (0.031)	0.025 (0.030)	-0.080** (0.036)	-0.076** (0.036)	-0.079** (0.036)	-0.080** (0.035)
Democracy	-0.057*** (0.006)	-0.057*** (0.006)	-0.058*** (0.006)	-0.057*** (0.006)	-0.038*** (0.006)	-0.038*** (0.006)	-0.038*** (0.006)	-0.038*** (0.006)
Civil war	0.217*** (0.071)	0.221*** (0.070)	0.211*** (0.070)	0.229*** (0.069)	0.201*** (0.073)	0.191*** (0.074)	0.200*** (0.073)	0.203*** (0.070)
Interstate war	0.229 (0.157)	0.221 (0.155)	0.224 (0.151)	0.299** (0.131)	-0.764 (0.524)	-0.736 (0.539)	-0.788 (0.538)	-0.752 (0.520)
Past practice	2.098*** (0.114)	2.098*** (0.114)	2.089*** (0.113)	2.102*** (0.114)	1.966*** (0.091)	1.965*** (0.091)	1.963*** (0.091)	1.966*** (0.091)
Log-pseudo likelihood	-1164.96	-1164.896	-1162.414	-1166.057	-1192.009	-1191.099	-1190.845	-1192.039
N	2825	2822	2825	2822	2825	2822	2825	2822

Note:

White robust standard errors clustered on country in parentheses. (*p<0.1; **p<0.05; ***p<0.01)

Table 6.3: Ordered Probit Models for the Effect of Economic Sanctions on Freedom of Speech and Freedom of Assembly and Association (1981-2005)

	<i>Freedom of speech</i>				<i>Freedom of assembly and association</i>			
	(Model 1)	(Model 2)	(Model 3)	(Model 4)	(Model 5)	(Model 6)	(Model 7)	(Model 8)
Economic sanctions	0.216*** (0.064)				0.114* (0.060)			
Human rights sanctions		0.206** (0.094)				0.133 (0.099)		
Non-human-rights sanctions		0.219*** (0.074)				0.106 (0.072)		
Multilateral sanctions			0.266*** (0.078)				0.268*** (0.073)	
Unilateral sanctions			0.187*** (0.069)				-0.018 (0.071)	
Sanction years				0.154*** (0.046)				0.091** (0.042)
GDP per capita (log)	-0.101*** (0.025)	-0.101*** (0.025)	-0.100*** (0.025)	-0.097*** (0.026)	-0.051* (0.029)	-0.051* (0.030)	-0.047 (0.030)	-0.050* (0.029)
Democracy	-0.074*** (0.006)	-0.074*** (0.006)	-0.074*** (0.006)	-0.074*** (0.006)	-0.067*** (0.007)	-0.067*** (0.007)	-0.068*** (0.007)	-0.067*** (0.007)
Civil war	0.222*** (0.075)	0.224*** (0.075)	0.212*** (0.074)	0.226*** (0.074)	0.093 (0.074)	0.090 (0.070)	0.080 (0.070)	0.089 (0.072)
Interstate war	-0.140 (0.227)	-0.143 (0.229)	-0.151 (0.219)	-0.029 (0.225)	-0.388 (0.351)	-0.382 (0.352)	-0.444 (0.336)	-0.350 (0.308)
Past practice	1.223*** (0.077)	1.223*** (0.077)	1.218*** (0.077)	1.218*** (0.076)	1.609*** (0.080)	1.608*** (0.080)	1.603*** (0.080)	1.606*** (0.080)
Log-pseudo likelihood	-1757.804	-1757.792	-1755.714	-1757.452	-1434.926	-1434.883	-1428.212	-1434.122
N	2808	2803	2808	2803	2808	2803	2808	2803

Note:

*p<0.1; **p<0.05; ***p<0.01

Table 6.4: Ordered Probit Models for the Effect of Economic Sanctions on Worker's Rights and Electoral Self-Determination (1981-2005)

	<i>Worker's rights</i>				<i>Electoral self-determination</i>			
	(Model 1)	(Model 2)	(Model 3)	(Model 4)	(Model 5)	(Model 6)	(Model 7)	(Model 8)
Economic sanctions	0.227*** (0.054)				0.092 (0.069)			
Human rights sanctions		0.272*** (0.078)				0.028 (0.097)		
Non-human-rights sanctions		0.212*** (0.062)				0.119 (0.082)		
Multilateral sanctions			0.280*** (0.064)				0.174* (0.095)	
Unilateral sanctions			0.180*** (0.069)				0.029 (0.078)	
Sanction years				0.167*** (0.037)				0.106** (0.052)
GDP per capita (log)	-0.081*** (0.029)	-0.080*** (0.030)	-0.080*** (0.030)	-0.078*** (0.030)	-0.088** (0.034)	-0.091*** (0.035)	-0.086** (0.035)	-0.089*** (0.035)
Democracy	-0.043*** (0.006)	-0.043*** (0.006)	-0.043*** (0.006)	-0.043*** (0.006)	-0.118*** (0.009)	-0.118*** (0.009)	-0.118*** (0.009)	-0.118*** (0.009)
Civil war	0.062 (0.074)	0.056 (0.074)	0.052 (0.073)	0.064 (0.074)	0.127 (0.080)	0.135* (0.078)	0.119 (0.078)	0.114 (0.074)
Interstate war	0.469** (0.197)	0.475** (0.196)	0.463** (0.191)	0.543** (0.212)	-0.729*** (0.232)	-0.757*** (0.230)	-0.748*** (0.243)	-0.663*** (0.253)
Past practice	1.346*** (0.070)	1.345*** (0.070)	1.342*** (0.069)	1.345*** (0.070)	0.936*** (0.074)	0.935*** (0.074)	0.931*** (0.073)	0.929*** (0.072)
Log-pseudo likelihood	-1895.463	-1895.227	-1893.802	-1894.26	-1563.468	-1562.911	-1561.028	-1560.155
N	2806	2808	2806	2808	2806	2808	2806	2808

Note:

White robust standard errors clustered on country in parentheses. (*p<0.1; **p<0.05; ***p<0.01)

Table 6.5: Ordered Probit Models for the Effect of Economic Sanctions on Religious Freedom and Empowerment Rights (1981-2005)

	<i>Religious freedom</i>				<i>Empowerment rights index</i>			
	(Model 1)	(Model 2)	(Model 3)	(Model 4)	(Model 5)	(Model 6)	(Model 7)	(Model 8)
Economic sanctions	0.157*** (0.052)				0.140*** (0.044)			
Human rights sanctions		0.112 (0.104)				0.146* (0.077)		
Non-human-rights sanctions		0.173*** (0.056)				0.138*** (0.051)		
Multilateral sanctions			0.233*** (0.073)				0.234*** (0.054)	
Unilateral sanctions			0.068 (0.058)				0.058 (0.051)	
Sanction years				0.092** (0.040)				0.085*** (0.027)
GDP per capita (log)	0.020 (0.025)	0.018 (0.026)	0.023 (0.026)	0.022 (0.026)	-0.071*** (0.024)	-0.071*** (0.024)	-0.069*** (0.024)	-0.068*** (0.023)
Democracy	-0.042*** (0.005)	-0.043*** (0.005)	-0.042*** (0.005)	-0.042*** (0.005)	-0.045*** (0.006)	-0.045*** (0.006)	-0.046*** (0.006)	-0.045*** (0.006)
Civil war	0.089 (0.078)	0.096 (0.078)	0.082 (0.077)	0.098 (0.079)	0.119** (0.051)	0.118** (0.050)	0.112** (0.050)	0.128** (0.051)
Interstate war	-0.024 (0.280)	-0.033 (0.275)	-0.043 (0.281)	0.028 (0.275)	-0.080 (0.073)	-0.078 (0.073)	-0.091 (0.088)	-0.016 (0.083)
Past practice	1.259*** (0.068)	1.259*** (0.068)	1.258*** (0.068)	1.261*** (0.068)	0.550*** (0.020)	0.550*** (0.020)	0.547*** (0.020)	0.551*** (0.020)
Log-pseudo likelihood	-1873.719	-1873.455	-1871.671	-1875.349	-4614.989	-4614.982	-4609.937	-4616.663
N	2815	2792	2815	2792	2815	2792	2815	2792

Note: White robust standard errors clustered on country in parentheses. (*p<0.1; **p<0.05; ***p<0.01)

($p = 0.06$). Furthermore, the size of the coefficients do differ considerably, suggesting that the negative impact persists regardless of sanction goals.

Moving on to the next set of coefficients, we see that multilateral sanctions result in highly significant coefficients, yielding a stronger relationship than for any of the models examining empowerment rights overall. Conversely, unilateral sanctions fail to yield significant results. These findings are in line with the results from chapter 5, indicating that more coordinated sanction efforts are indeed considerably more detrimental to repressive practices than those imposed by individual countries. Interestingly however, the analysis with the extended dataset in chapter 5 indicated that both unilateral and multilateral sanctions had a negative impact on physical integrity rights as a whole. A possible implication of this is that, on the whole, the threshold for sanctions to negatively affect repressive practices is lower for physical integrity rights than for empowerment rights – a more widely coordinated sanctions regime is necessary for empowerment rights abuses to be significantly affected.

Finally, the negative impact on empowerment rights overall seems to persist over time - indicated by the significant coefficient for *sanction years*. However, the relationship does not appear to be very strong, as the coefficient is exceedingly small.

Individual indicators

Moving on to individual rights, however, we see that the negative impact is not consistent across all empowerment rights indicators.

Assessing the impact of sanctions overall first (Models 1/5), we see that sanctions do not appear to have any impact whatsoever on electoral self-determination or the right of citizens to move freely within the country. Furthermore, the effect of economic sanctions on freedom of assembly and association appears tenuous, with the result only significant at the 10%–level (however, the p -value is relatively close to the 5% threshold at 0.06).

On the other hand, sanctions overall appear to have a significant and negative impact across the board on the freedom of speech, freedom of foreign movement

and on worker's rights, which all yield significant results at the 5%-level or lower. These results indicate that sanctions do not affect empowerment rights to the same extent, vindicating the decision to examine the impact of sanctions on the disaggregated empowerment rights variable. The models examining freedom of speech and worker's rights (and to a certain extent religious freedom) yield considerably larger coefficients than the aggregated empowerments index – suggesting that the apparent effect of economic sanctions on empowerment rights as a whole is in fact driven primarily by these two (or three) variables. These results on their own serve as a warning to researchers to be careful when drawing inferences on the basis of analyses using aggregated indicators of human rights. It also implies that aggregated variables of civil and political rights alone are not sufficient in order to accurately assess the impact of sanctions beyond physical integrity rights.

Moving on to the second set of models (Models 2/6), results for human rights/non-human rights sanctions are even less consistent throughout the models. Worker's rights and freedom of speech yield results significant at the 5%-level or lower for sanctions irrespective of goals, while religious freedom and foreign movement only give significant results for non-human rights sanctions. None of the other rights indicators give significant results for either sanction type. A possible takeaway here is that, provided the impact of sanctions overall is not particularly strong, sanctions with the goal of curbing human rights violations at least do not exacerbate existing repression of these particular rights in the target country.

For the third set of models (Models 3/7), we see a similar pattern. Worker's rights and freedom of speech yield results significant at the 5%-level or lower for sanctions irrespective of the level of international coordination of sanctions, while religious freedom, freedom of assembly and association, and freedom of foreign movement give significant results for multilateral sanctions only. Notably, multilateral sanctions appear to have an effect on electoral self-determination, though the results are somewhat weak with significance at the 10%- level. It also stands out as the only significant coefficient for electoral self-determination beside sanction duration, diminishing its importance somewhat.

The significant results for only multilateral sanctions for several indicators may suggest that, same as for empowerment rights overall, a more widely coordinated

sanctions regime is necessary for empowerment rights abuses to be significantly affected. The results also imply that sanctions impact freedom of speech and worker's rights much in the same way as they do physical integrity rights. A possibility is that these two rights are more closely related to physical integrity rights than other empowerment rights. Another possibility is that their use have a supplementary (or complementary) function vis-a-vis physical integrity rights violations, as suggested by Fariss and Schnakenberg (2014).

Finally, the sanction years variable (Models 4/8) indicates that sanctions continue to have a negative impact over time for indicators yielding significant results elsewhere.

What is consistent across models, however, is that coefficients for independent variables are either not significant, or significant and positive. This indicates that economic sanctions, if they have an effect at all, seem to have an overall and consistent negative impact on the broader set of empowerment rights in a similar manner to physical integrity rights. At the very least, it suggests that there are no scenarios in which economic sanctions can be expected to improve repressive practices in target countries, neither with regard to physical integrity rights or wider civil and political rights.

In the next section I assess the impact of economic sanctions on the predicted probability of frequent violations of empowerment rights. Like in the preceding analyses, I only include indicators that have yielded significant results in the regression analysis above. Consequently, I calculate predicted probabilities for the impact of sanctions on freedom of foreign movement, freedom of speech, freedom of assembly and association, worker's rights, and religious freedom.

6.3 Analysis and comparison of results: size of effects

Table 6.7. presents the impact of sanctions on the occurrence of frequent violations of empowerment rights. I also include the predicted probabilities for physical integrity rights with the extended dataset in Table 6.6, to make it easier to compare the findings from both analyses.

Table 6.6: The impact of sanctions variables on the occurrence of frequent violations of integrity rights (expanded data)

	No sanctions	Partial sanctions (0 to 1)	% change (0 to 1)	Extensive sanctions (0 to 2)	% change (0 to 2)
Disappearances					
Economic sanctions (all)	0.021	0.031	50	0.045	120
Human rights sanctions	0.020	0.028	38	0.038	86
Non-human-rights sanctions	0.020	0.032	56	0.048	137
Multilateral sanctions	0.020	0.033	60	0.050	147
Unilateral sanctions	0.020	0.030	45	0.042	106
Extra-judicial killings					
Economic sanctions (all)	0.064	0.106	66	0.165	160
Human rights sanctions	0.063	0.101	59	0.153	141
Non-human-rights sanctions	0.063	0.107	69	0.169	166
Multilateral sanctions	0.063	0.111	75	0.180	184
Unilateral sanctions	0.063	0.101	59	0.152	139
Political imprisonment					
Economic sanctions (all)	0.135	0.187	39	0.250	85
Non-human-rights sanctions	0.135	0.195	45	0.270	101
Multilateral sanctions	0.137	0.186	36	0.245	79
Unilateral sanctions	0.137	0.182	26	0.235	72
Torture					
Economic sanctions (all)	0.303	0.408	37	0.520	71
Human rights sanctions	0.303	0.442	47	0.588	94
Non-human-rights sanctions	0.299	0.401	34	0.505	67
Multilateral sanctions	0.302	0.446	48	0.598	98
Unilateral sanctions	0.302	0.382	30	0.467	54

The reported probabilities are calculated by holding all control variables in the models reported in Tables 1-4 constant at their means, while holding the ordinal sanction variables at 0. Then, the ordinal sanction variable under consideration is altered from 0 (no sanction) to 1 (partial sanctions) and from 0 to 2 (extensive sanctions) to determine the individual effect of it.

Table 6.7: The impact of sanctions variables on the occurrence of frequent violations of empowerment rights

	No sanctions	Partial sanctions (0 to 1)	% change (0 to 1)	Extensive sanctions (0 to 2)	% change (0 to 2)
Foreign movement					
Economic sanctions (all)	0.0053	0.0077	46	0.011	110
Non-human-rights sanctions	0.0052	0.0078	50	0.012	121
Multilateral sanctions	0.0052	0.0098	87	0.017	233
Freedom of speech					
Economic sanctions (all)	0.0662	0.0987	49	0.142	114
Human rights sanctions	0.0662	0.0970	47	0.137	107
Non-human-rights sanctions	0.0662	0.0993	50	0.143	116
Multilateral sanctions	0.0658	0.1072	63	0.165	150
Unilateral sanctions	0.0658	0.0933	42	0.128	95
Freedom of assembly					
Economic sanctions (all)	0.0739	0.0912	23	0.111	51
Multilateral sanctions	0.0732	0.1181	61	0.180	145
Worker's rights					
conomic sanctions (all)	0.1307	0.1850	42	0.252	93
Human rights sanctions	0.1308	0.1975	51	0.281	115
Non-human-rights sanctions	0.1308	0.1813	39	0.243	85
Multilateral sanctions	0.1304	0.1992	53	0.286	119
Unilateral sanctions	0.1304	0.1724	32	0.222	70
Religious freedom					
Economic sanctions (all)	0.0898	0.1179	31	0.152	69
Non-human-rights sanctions	0.0897	0.1211	35	0.159	78
Multilateral sanctions	0.0905	0.1348	49	0.192	112

The reported probabilities are calculated by holding all control variables in the models reported in Tables 1-4 constant at their means, while holding the ordinal sanction variables at 0. Then, the ordinal sanction variable under consideration is altered from 0 (no sanction) to 1 (partial sanctions) and from 0 to 2 (extensive sanctions) to determine the individual effect of it.

The results from table 6.6 suggest that economic sanctions have a negative impact on empowerment rights, and the effect does not seem to be markedly weaker than for physical integrity rights. For partial sanctions, changes in predicted probabilities for frequent violations of empowerment rights range from 23% to 87%; for extensive sanctions, the size of changes range from 51% to 233%. We can compare this to changes in predicted probabilities for frequent violations of physical integrity rights, which range from 26% to 75% for partial sanctions, and from 54% to 184% for extensive sanctions.

However, changes in predicted probabilities can not be viewed in isolation, but must be considered in relation to the values of the predicted probabilities themselves. For instance, we see the largest impact for multilateral sanctions on the freedom of foreign movement. However, the result becomes less significant when we see that the predicted probabilities for frequent violations of this right are negligibly low, even with extensive sanctions in place (0.017). These results indicate that restriction of the freedom of movement is not likely to be used as a repressive tool by the government, regardless of scenario.

With this in mind, the results for freedom of speech, freedom of assembly, worker's rights and religious freedom are more interesting. We see the highest predicted probabilities for violation of worker's rights, followed by freedom of speech, religious freedom, and freedom of assembly and association. These values are comparable to those for extra-judicial killings and political imprisonment in the analysis in Chapter 5, indicating that these sets of rights (across the categories physical integrity rights and empowerment rights) are more or less equally at risk of frequent violations in the face of sanctions.

However, it must be noted that while predicted probabilities and effect size are generally comparable across physical integrity rights and empowerment rights, physical integrity rights yield more instances of significant results across all sanctions indicators, as well as significant results for all individual indicators. The results are much more fragmented for empowerment rights. For instance, we only see significant outcomes for all sanctions indicators for freedom of speech and worker's rights - for all other rights in Table 6.7, human rights sanctions and unilateral sanctions fail to yield significant results.

One implication of these results is that sanctions generally appear to have a more consistent negative impact, regardless of sanction type or breadth of participation, on physical integrity rights than on empowerment rights. It is unclear exactly why this would be the case, based on the theoretical framework of this thesis, though we can provide some hypothetical suggestions. It may be that, as some have suggested, regimes increase repression in the face of sanctions to signal capacity and willingness to resist external pressure vis-a-vis its population (Peksen and Drury 2009; 2010). Insofar physical integrity rights violations generally receive strong condemnation internationally, and often form part of the pretext for imposing sanctions, violation of physical integrity rights may have a comparatively strong signalling effect compared to repression of other rights. Regardless of mechanisms, however, the different impact of sanctions suggests that researchers such as Gutmann, Neuenkirch and Neumeier (2016) and Fariss and Schnakenberg (2014) are at least partially correct in treating physical integrity rights and empowerment rights as conceptually distinct categories.

Another implication, already suggested in section 6.2, is that economic sanctions have a more consistent negative impact on some empowerment rights than others. Once again, I can only suggest hypothetical explanations based in the theoretical literature. For instance, Fariss and Schnakenberg (2014) have suggested that some repressive tactics complement and enhance each other, and thus are more likely to be used in conjunction. For instance, repression of freedom of speech and journalism may have the added benefit of reducing the likelihood of other repressions being made public, thereby reducing the likelihood of retribution or repercussions from violating other rights (Fariss and Schnakenberg 2014: 1006). In any case, variations in the effects of sanctions on individual rights indicators suggest that more in-depth qualitative case studies may be worthwhile – to uncover the mechanisms underlying the violations of specific rights – as well as studies examining the mutual dependencies between different repressive tactics as per Fariss and Schnakenberg's recommendations.

It should also be noted that the results from the analysis of civil and political rights exhibit similar patterns to the analysis of physical integrity rights, as hinted at in the previous section. First, multilateral sanctions appear to be consistently

more harmful to empowerment rights than unilateral sanctions: *multilateral sanctions* yields significant results for 5 out of the 7 individual indicators, while *unilateral sanctions* only yields significant results for 2 individual indicators. Furthermore, extensive multilateral sanctions increase predicted probability of frequent violations of freedom of speech by 150%, compared to 95% for unilateral sanctions. For worker's rights the numbers are 119% and 70%, respectively.

These results bolster Peksen's assessment that coordinated sanction efforts are more detrimental than unilateral ones, through strengthening the effect of the mechanisms outlined in chapter 2. For instance, a possible consequence of multilateral sanctions is that they lessen the possibility of offsetting costs through closer ties with other states, as there are fewer states not partaking in the sanctions regime. This in turn may lead the regime to divert costs to its citizens, leading to political unrest and subsequent intensification of repression to handle the increased opposition. While consistent with the theoretical mechanisms postulated in this thesis, this argument is undercut somewhat by studies showing that imposed sanctions do not generally lead to political violence and protests (Grauvogel, Licht, and Soest 2017). More in-depth analyses are necessary to pinpoint the underlying mechanisms that could cause multilateral sanctions to be more harmful than less coordinated efforts.

Human rights sanctions yield significant results in fewer instances than non-human-rights sanctions (2 out of 7 indicators vs 4 out of 7), suggesting that while they may not have a positive impact, sanctions aimed at curbing human rights violations may be less detrimental to empowerment rights than other types of economic sanctions. These results could be interpreted to indicate that while human rights sanctions seem to have a negative impact on more egregious repressive measure, they have less of an impact on empowerment rights. The signalling effect discussed above may be one possible explanation for why this is the case. Where results are significant, however, there is no indication that human rights sanctions are less harmful than sanctions with other goals – for worker's rights, human rights sanctions appear to be the most harmful. Like with previous findings, however, the wider implication is that using sanctions as a policy tool for curbing human rights violations is ineffective at best, and highly detrimental at worst.

The next section summarizes the findings from this chapter, along with its wider implications for the sanctions-human rights literature.

6.4 Summary and implications

What are the implications of these findings for the broader literature on sanctions? First of all, the analysis partly confirms the theoretical expectations, outlined in hypothesis H_2 , that economic coercion will likely cause the unintended consequence of more empowerment rights violations in targeted countries.

The results indicate that economic sanctions *do* in fact lead to more frequent violations of empowerment rights, but only for a specific subset of rights, and both the size of effects and the absolute predicted probabilities vary depending on the rights in question. These findings support my assessment, outlined in chapter 2 and 3, that it is beneficial to examine the effect of sanctions on both individual and aggregated indicators of human rights.

Furthermore, my findings imply that only studying aggregated variables of human rights may lead to misleading and imprecise conclusions regarding the impact on individual rights. As seen in section 6.2, the conclusions regarding the aggregated measure of empowerment rights would be misleading *despite* the fact that the individual indicators it is comprised of have been empirically linked to the same theoretically founded category of rights (Gutmann and Voigt 2015). Consequently, scholars assessing the impact of sanctions on human rights should be careful about drawing inferences on the basis of studies looking at aggregated indicators of empowerment rights.

Because the effect of sanctions on empowerment rights appears to be contingent upon the right in question, both hypothesis H_{3a} and H_{3b} receive partial support. Some empowerment rights indicators, such as electoral self determination and freedom of domestic movement, yield no significant results, lending strong support to H_{3b} . On the other hand, where the analysis does yield significant results, there are no indications that the negative impact of sanctions is any less for empowerment rights than for physical integrity rights, lending support to H_{3a} .

What is less clear however, is which underlying factors are the main drivers of the apparent strong impact of sanctions on freedom of speech and worker's rights. Is it that these rights have characteristics more akin to physical integrity rights, for example by being (one of the) more effective tools of repression? Or is it rather that, as Fariss and Schnakenberg (2014) seem to suggest, that rights like freedom of speech have some form of complementary function vis-a-vis the use of other repressive policies?

Before closing off the discussion of the findings in this chapter, it is also worthwhile to summarize how they differ from (and are similar to) the findings from chapter 5. First, empowerment rights are consistently significant across various economic sanctions indicators in fewer cases than physical integrity rights. An implication of this is that sanctions generally appear to have a more consistent negative impact, regardless of sanction type or breadth of participation, on physical integrity rights than on empowerment rights. A corollary of this assessment is that several empowerment rights are more vulnerable to violation under specific types of sanctions. For instance, freedom of assembly and religious freedom only seem to be affected under coordinated, multilateral sanctions.

With regard to the coordination of sanction efforts, the findings in this chapter are consistent with results for physical integrity rights, in that multilateral sanctions on the whole appear more detrimental to human rights than unilateral ones. Multilateral sanctions yield stronger impacts throughout than unilateral ones, and also yield significant results across all empowerment rights indicators in table 6.6. where unilateral sanctions only result in significant effects for two indicators. These results suggest that Peksen is correct in that stronger and more coordinated sanction efforts are ultimately more detrimental to human rights practices.

Similar to the analysis in Chapter 5, results with regard to sanction goals are also somewhat divisive. Findings suggest that human rights sanctions have an impact in fewer cases than sanctions with other goals. Where there is an impact, however, it is not clear whether human rights sanctions are more or less detrimental than other types of sanctions.

In the next chapter, I attempt to summarize the overall findings from this thesis,

and outline the implications for the wider literature on sanctions. I also suggest possible avenues of future research based on my findings.

Chapter 7

Conclusions

The purpose of this thesis has been to investigate the impact of economic sanctions on human rights violations by governments in targeted countries. Specifically, my goal has been to examine the effect of sanctions on two categories of human rights - physical integrity rights and civil and political rights - and compare the findings to assess the relative impact of sanctions on the state-sponsored violation of the citizens' rights.

I addressed this research question by specifying a total of 88 ordered probit models, using cross-national, time-series sanctions data compiled from the Threats and Impositions of Sanctions (TIES) and Hufbauer, Schott and Elliott (HSE) datasets, as well as data on human rights violations from the Cingranelli and Richards (CIRI) human rights dataset.

I conducted my analysis in three separate stages. First, I replicated key findings from Dursun Peksen's 2009 paper *Better or Worse? The Effect of Economic Sanctions on Human Rights*, which investigates the effect of economic sanctions on physical integrity rights in 95 countries during 1981-2000 period. I replicated Peksen's findings using both his own data sample and a corresponding dataset I reconstructed using the original data sources. I then used the replicated dataset as a basis to expand the scope of the study, including five more years of sanctions data and a total of approximately 1000 additional relevant country-year observations.

Finally, I used the expanded data on sanctions to investigate the relationship

between economic sanctions and state-sponsored violation of civil and political rights. The indicators for physical integrity rights and civil and political rights were taken from the CIRI human rights dataset - the same used for the indicators of physical integrity rights violations used in the preceding analyses. For the operationalization of civil and political rights, I used the CIRI empowerment rights indicators consisting of measures for freedom of foreign and domestic movement, freedom of speech, freedom of assembly and association, worker's rights, electoral self-determination, and religious freedom, as well as an aggregated empowerment rights index.

What were the overall conclusions from these analyses? First, I argued that the success of a replication analysis must necessarily be evaluated in relation to the objectives and substantive results of the original analysis. In broad terms, we should be able to draw the same general conclusions from the replication as from the original study.

In this regard, the replication of Peksen's (2009) analysis must be deemed to have been successful. With the exception of two coefficient, the direction and significance of results matched those of Peksen's analysis, and goodness-of-fit measures indicated that all corresponding models fit the data to a comparable degree. Furthermore, the strength of sanction effects were generally in line with Peksen's analysis, leading me to draw the same broad conclusions: extensive sanctions appear to be more detrimental to human rights than more limited ones, and multi-lateral sanctions appear to have a greater impact on human rights than unilateral ones.

The only substantive conclusion from Peksen's analysis I was unable to confirm concerns the relative impact of sanctions with different goals - sanctions aimed explicitly at curbing human rights violations were not shown to be consistently worse than other types of sanctions in increasing the likelihood of repression. In broad terms, however, the replication analysis in this thesis has corroborated Peksen's original findings and conclusions, indicating that his study is robust. It is possible that this difference is the result of a mistake in coding on my part, or a subtle difference between the original and updated TIES dataset, though this is difficult to determine with certainty. It should be mentioned however, that if

the pattern *is* accurate, the results suggest that the use of economic sanctions as a means of pressuring repressive regimes into complying with international human rights norms is ineffectual at best, and counter-productive at worst. Either way, the implications for scholars (and policy makers) is not considerably different than in Peksen's original study.

Second, the findings from the analysis with the extended dataset also appears to support the substantive conclusions from Peksen's analysis and the replication study, and were generally in line with the broader literature on the effect of sanctions on physical integrity rights. When accounting for the widest available range of sanctions data, some results indicated stronger effects than in previous studies – the increase in predicted probabilities of frequent physical integrity rights violations was generally larger with the more extensive dataset compared to the replication dataset. In line with previous findings, extensive sanctions appears to have a greater negative impact on repression than limited sanctions, and coordinated sanctions efforts appear to be more harmful than unilateral ones. Once again, the evidence with regard to sanction goals remains inconclusive, suggesting that human rights sanctions are either more or less detrimental than sanctions with other goals depending on the specific right in question.

Finally, the findings from the third analysis indicate that the negative impact of sanctions extend to a number of civil and political rights. The overall impact on the aggregated empowerment rights indicator from CIRI suggest that sanctions are generally detrimental to civil and political rights in the target country.

However, sanctions do not appear to impact different civil and political rights equally – for instance, I found no evidence that sanctions affect domestic freedom of movement or electoral self-determination, and weak evidence for effects on freedom of assembly and association. However, freedom of speech, worker's rights, the freedom to move freely in and out of the country, and religious freedoms appear to be negatively affected by sanctions. These findings suggest that the research on the impact of sanctions should examine individual rights, and be careful to not rely solely on aggregated indicators of rights violations, as these can yield misleading results.

Comparing the analyses looking at different categories of rights, I found that the size of the impact of sanctions on empowerment rights, measured in percentage change in predicted probabilities, is comparable to that of physical integrity rights. These results suggest that sanctions have no less of a negative impact on less egregious rights violations, and that economic coercion may contribute to the increased use of a range of repressive measures beyond disappearance, extra-judicial killings, political imprisonment, and torture.

However, the predicted probabilities themselves suggest that the violation of civil and political rights are not necessarily widely used repressive measures, at least compared to some physical integrity rights violations such as torture. The probability of frequent violation of the right to foreign movement is negligible, even with extensive sanctions in place, while the substantive impact on freedom of speech, worker's rights, freedom of assembly, and religious freedom is comparable to that on extra-judicial killings and political imprisonment.

It is interesting to note that among all civil and political rights indicators, only freedom of speech and worker's rights indicate a relatively high and significant impact across all sanctions indicators. I have suggested some possible explanations for why the freedom of speech in particular might be vulnerable within the theoretical framework of this thesis, but more research is necessary to uncover the specific mechanisms by which some rights are affected more (and more consistently) than others.

In summary, I would argue that my analysis in this thesis has made two contributions to the sanctions-human rights literature. First, through the replication and expansion of Peksen's key study from 2009, I have corroborated earlier research indicating that economic sanctions appear to have a detrimental impact on physical integrity rights in targeted countries. My findings are bolstered by using the most extensive data on economic sanctions available to data.

Second, I have shown that the negative impact of sanctions appear to extend to a subset of civil and political rights such as freedom of speech, and that the negative impact for these rights appears to be comparable to that on more egregious rights violations. Determining the isolated and relative impact of sanctions on

individual civil and political rights – through the use of disaggregated indicators and a research design facilitating comparison of results across models, respectively – represents a novel, albeit small contribution to the literature examining the extant effects of economic sanctions.

To conclude, I would like to suggest some possible avenues for future research based on these findings (and the limitations of the research design in this thesis). As I have already mentioned, one avenue of research potentially worth exploring is the underlying mechanisms determining how sanction impact specific rights, and why some rights appear to be more strongly affected than other. A related issue concerns the possibility of mutual dependencies between rights, as suggested by Fariss and Schnakenberg (2014). Is the freedom of speech particularly vulnerable in the sanctions because of its relationship to other repressive strategies, for example by preventing public exposure of other rights violations, or because of isolated characteristics of the right itself?

Finally, and perhaps most crucially, potential endogeneity is a particularly salient issue with regard to the effect of sanctions on human rights practices. As touched on in chapter 2 and 3, Peksen, and by extension I, have attempted to account for endogeneity through sample selection and the inclusion of the lagged dependent variable as a control variable. However, the study by Gutmann, Neuenkirch and Neumeier (2016), where effects of US sanctions disappear when employing endogenous treatment models, casts some doubts over whether these measures are sufficient. As such, the question of the nature and direction of the causal relationship between sanctions and human rights violations is not entirely resolved in this thesis or in the wider literature. A potential way forward is to apply more sophisticated methods of accounting for endogeneity, such as the endogenous treatment model, on the wider, cross-national set of data on economic sanctions and human rights.

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Appendix

A.1 Dataset and script

The dataset and all syntax files are available on a separate memory drive. Requests can be directed to kristoffer.fretland@gmail.com.

A.2 Multinomial models: Torture, Freedom of Foreign Movement, and Freedom of Speech

Tables A.1-A.12 show the result of multinomial probit regression models for indicators of torture, freedom of foreign movement, and freedom of speech. (I use the R package MNP, which fits the Bayesian multinomial probit model via Markov chain Monte Carlo.)

Table 1: The Effect of Economic Sanctions on Torture - Multinomial Models

Independent and control variables	Some violations	Frequent violations
(Intercept)	0.448** (0.208)	-0.383 (0.351)
Economic sanctions	0.099** (0.055)	0.45*** (0.078)
GDP per capita (log)	-0.064** (0.029)	-0.242*** (0.035)
Democracy	-0.005** (0.003)	-0.017*** (0.006)
Civil war	0.027 (0.091)	0.566*** (0.116)
Interstate war	14.76*** (4.809)	14.755*** (4.861)
Past practice	0.457** (0.184)	1.814*** (0.141)

Table 2: The Effect of Economic Sanctions on Torture - Multinomial Models

Independent and control variables	Some violations	Frequent violations
(Intercept)	0.27** (0.157)	-0.573** (0.322)
Human rights sanctions	0.468** (0.214)	0.894*** (0.241)
Non-human-rights sanctions	0.06** (0.036)	0.383*** (0.081)
GDP per capita (log)	-0.038** (0.022)	-0.229*** (0.034)
Democracy	-0.003 (0.003)	-0.015** (0.006)
Civil war	0.016 (0.068)	0.552*** (0.109)
Interstate war	5.501** (2.594)	5.528** (2.7)
Past practice	0.273** (0.139)	1.694*** (0.122)

Table 3: The Effect of Economic Sanctions on Torture - Multinomial Models

Independent and control variables	Some violations	Frequent violations
(Intercept)	0.417*** (0.156)	-0.412 (0.345)
lmultiordinal	0.2** (0.088)	0.599*** (0.122)
luniordinal	0.065** (0.039)	0.333*** (0.088)
GDP per capita (log)	-0.058*** (0.022)	-0.231*** (0.035)
Democracy	-0.005** (0.003)	-0.015** (0.006)
Civil war	0.036 (0.082)	0.54*** (0.108)
Interstate war	1.968 (1.409)	1.92 (1.45)
Past practice	0.409*** (0.132)	1.728*** (0.12)

Table 4: The Effect of Economic Sanctions on Torture - Multinomial Models

Independent and control variables	Some violations	Frequent violations
(Intercept)	0.472*** (0.172)	-0.351 (0.342)
Sanction years	0.044 (0.034)	0.294*** (0.053)
GDP per capita (log)	-0.067*** (0.023)	-0.238*** (0.033)
Democracy	-0.005** (0.003)	-0.017** (0.007)
Civil war	0.037 (0.086)	0.573*** (0.122)
Interstate war	2.993** (1.574)	3.167** (1.657)
Past practice	0.521*** (0.159)	1.854*** (0.113)

Table 5: The Effect of Economic Sanctions Freedom of Foreign Movement - Multinomial Models

Independent and control variables	Some violations	Frequent violations
(Intercept)	-0.249 (0.229)	-5.361*** (0.908)
lsanctionordinal	0.016 (0.024)	0.273*** (0.106)
GDP per capita (log)	0.002 (0.011)	0.042 (0.055)
Democracy	-0.012 (0.01)	-0.078*** (0.017)
Civil war	0.058 (0.053)	0.185 (0.148)
Interstate war	0.397 (0.426)	-23.884 (16.194)
Past practice	0.43 (0.343)	3.122*** (0.344)

Table 6: The Effect of Economic Sanctions Freedom of Foreign Movement - Multinomial Models

Independent and control variables	Some violations	Frequent violations
(Intercept)	-0.082 (0.334)	-3.93*** (1.087)
Human rights sanctions	-0.033 (0.037)	0.161 (0.163)
Non-human-rights sanctions	0.044** (0.025)	0.207** (0.086)
GDP per capita (log)	-0.011 (0.016)	-0.002 (0.056)
Democracy	-0.01 (0.011)	-0.057*** (0.021)
Civil war	0.082 (0.055)	0.158 (0.12)
Interstate war	3.176 (2.01)	-22.239 (14.398)
Past practice	0.403 (0.375)	2.572*** (0.518)

Table 7: The Effect of Economic Sanctions Freedom of Foreign Movement - Multinomial Models

Independent and control variables	Some violations	Frequent violations
(Intercept)	-0.182 (0.241)	-4.858*** (1.497)
lnmultiordinal	0.039 (0.054)	0.271** (0.117)
lnuniordinal	-0.008 (0.022)	0.25** (0.133)
GDP per capita (log)	0.002 (0.01)	0.036 (0.053)
Democracy	-0.009 (0.011)	-0.066** (0.028)
Civil war	0.04 (0.05)	0.137 (0.142)
Interstate war	0.321 (0.275)	-30.844 (22.723)
Past practice	0.3 (0.364)	2.809*** (0.758)

Table 8: The Effect of Economic Sanctions Freedom of Foreign Movement - Multinomial Models

Independent and control variables	Some violations	Frequent violations
(Intercept)	-0.313 (0.262)	-5.343*** (0.709)
Sanction years	0.008 (0.017)	0.15** (0.069)
GDP per capita (log)	0.003 (0.011)	0.044 (0.043)
Democracy	-0.015 (0.012)	-0.078*** (0.013)
Civil war	0.078 (0.07)	0.22 (0.149)
Interstate war	0.319 (0.3)	-25.983 (19.325)
Past practice	0.514 (0.418)	3.18*** (0.171)

Table 9: The Effect of Economic Sanctions Freedom of Speech - Multinomial Models

Independent and control variables	Some violations	Frequent violations
(Intercept)	0.305 (0.191)	-1.675*** (0.558)
Economic sanctions	0.026 (0.037)	0.428*** (0.091)
GDP per capita (log)	-0.044 (0.029)	-0.129*** (0.039)
Democracy	-0.018 (0.011)	-0.125*** (0.012)
Civil war	0.068 (0.061)	0.358*** (0.109)
Interstate war	0.062 (0.197)	-0.72 (0.794)
Past practice	0.473 (0.29)	1.619*** (0.202)

Table 10: The Effect of Economic Sanctions Freedom of Speech - Multinomial Models

Independent and control variables	Some violations	Frequent violations
(Intercept)	0.23 (0.18)	-1.796*** (0.432)
Human rights sanctions	-0.047 (0.05)	0.438*** (0.135)
Non-human-rights sanctions	0.023 (0.03)	0.437*** (0.087)
GDP per capita (log)	-0.03 (0.028)	-0.126*** (0.038)
Democracy	-0.013 (0.009)	-0.127*** (0.011)
Civil war	0.055 (0.046)	0.322*** (0.103)
Interstate war	0.03 (0.16)	-0.643 (0.854)
Past practice	0.32 (0.243)	1.503*** (0.182)

Table 11: The Effect of Economic Sanctions Freedom of Speech - Multinomial Models

Independent and control variables	Some violations	Frequent violations
(Intercept)	0.282** (0.134)	-1.768*** (0.411)
Imultiordinal	-0.022 (0.047)	0.451*** (0.106)
luniordinal	0.027 (0.032)	0.288*** (0.099)
GDP per capita (log)	-0.035 (0.023)	-0.135*** (0.031)
Democracy	-0.012 (0.011)	-0.126*** (0.012)
Civil war	0.048 (0.048)	0.348*** (0.1)
Interstate war	0.046 (0.158)	-0.371 (0.704)
Past practice	0.36 (0.229)	1.579*** (0.158)

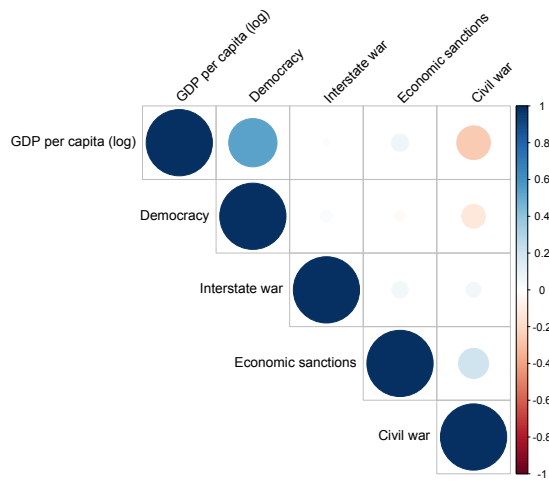
Table 12: The Effect of Economic Sanctions Freedom of Speech - Multinomial Models

Independent and control variables	Some violations	Frequent violations
(Intercept)	0.325** (0.156)	-1.508*** (0.359)
Sanction years	0.013 (0.022)	0.332*** (0.058)
GDP per capita (log)	-0.047** (0.023)	-0.137*** (0.035)
Democracy	-0.021*** (0.008)	-0.124*** (0.01)
Civil war	0.086 (0.056)	0.339*** (0.107)
Interstate war	0.061 (0.191)	-0.39 (0.756)
Past practice	0.532*** (0.201)	1.648*** (0.181)

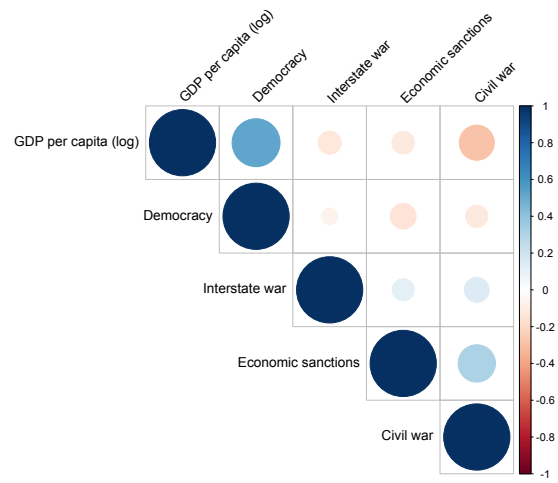
A.3 Multicollinearity: correlation matrices

Multicollinearity refers to instances where two or more independent variables in a multiple regression model are highly correlated. Correlations range from ± 1 , with +1 or -1 indicating perfect collinearity. High multicollinearity affects the variance of the regression coefficients, and can result in imprecise estimates. There are several ways to test for multicollinearity - the most straightforward method is to present and inspect a correlation matrix indicating pairwise correlation between all independent variables in a given model (Christophersen 2013: 77).

Figure A.1 and A.2 shows a graphical display of correlation matrices for independent variables for models using Peksen's original dataset and the replication dataset (A.1), and both models using the expanded dataset (A.2). A large, dark blue circle indicates strong positive correlation between two independent variables; a large, dark red circle indicates strong negative correlation. The correlation matrices indicate that there are no instances of strong multicollinearity between predictor variables for any of the models.

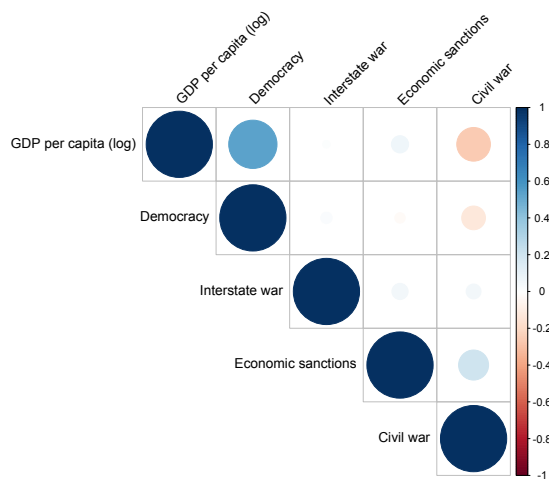


(a) Model 1P (*disappearances*)

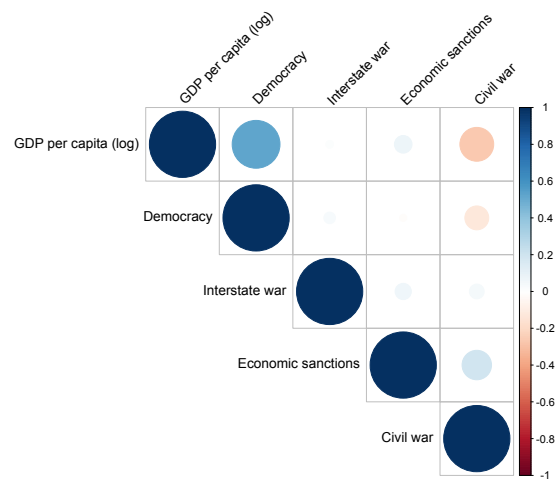


(b) Model 1 (*disappearances*)

Figure 1: Correlation matrices for independent variables



(a) Model 1 (*disappearances*)



(b) Model 1 (*foreign movement*)

Figure 2: Correlation matrices for independent variables