

The Price of the Past: Examining the Consequences of Odious Debt

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Why do leaders rarely invoke odious debt claims? Blaming past dictators or colonial powers for debt burdens could garner popular support and alleviate financial pressures. Yet, despite their potential for political popularity, such claims are rarely made in practice. This study explores this puzzle by arguing that the international financial consequences of odious debt claims deter leaders from employing this strategy. First, claiming odious debt signals to markets that the government is willing to work outside international norms. Second, the public may apply odious debt reasoning to more current debts and thus impose additional political barriers to debt repayment. Leaders' strategic avoidance of these claims poses an empirical challenge. To overcome this, we examine Norway's unique 2006 debt relief action. Norway unilaterally forgave the debts of five countries, explicitly labeling the original loans as "illegitimate." This action effectively imposed an odious debt claim on these states, bypassing the usual leader-driven process. By comparing these countries to others receiving conventional Norwegian debt relief, we isolate the impact of the "illegitimate debt" framing. While Norway's intentions were ostensibly well-intentioned, our analysis reveals that this program restricted the forgiven states' credit access and increased their bond yield spreads by 300 basis points. This study contributes to understanding sovereign debt politics and the implications of challenging established international political norms.

New regimes almost always assume responsibility for a country's public debt. This occurs even when new regimes mark a dramatic departure from their predecessors and most would argue that repayment is unjust. For example, when the apartheid regime ended in South Africa, Nelson Mandela quickly pledged the new government's commitment to repay all public debts. While some domestic and international observers thought a claim of "odious" or illegitimate debt would be warranted in this case, Mandela appeared concerned about South Africa's reputation if it did not repay its obligations ([Jayachandran and Kremer, 2006](#)). Moody's rewarded South Africa, providing an investment level rating, surprising many investors ([Lucas, 2022](#)).

While South Africa prioritized its reputation in international financial markets, other countries that default or restructure their debt obligations also avoid odious claims. Following the American invasion and occupation of Iraq in 2003, the initial policy stance was to repudiate the previous regime's debt on odious grounds. However, the U.S. and the new Iraqi government quickly switched strategies and focused on restructuring the debt based on "economic realities" rather than the odious debt doctrine ([Salmon, 2004](#)).

Iraq and other restructuring countries already risk their credit reputations but appear to avoid odious claims ([Cruces and Trebesch, 2013](#)). One explanation for this avoidance is that odious debt has no legal standing in international markets or courts, and thus would be a losing strategy. However, this explanation ignores that odious claims often have popular domestic support. For example, Archbishop Desmond Tutu had campaigned that South Africa's debts were illegitimate ([Jayachandran and Kremer, 2006](#)). Rafael Correa won the Ecuadorian presidential 2007 election by promising to declare Ecuador's debt odious ([Feibelman, 2017](#)).

Yet, despite the potential domestic support leaders may generate with an odious debt strategy, such claims remain rare. Why? In this study, we argue that odious claims raise several concerns for lenders. First, invoking odious debts sends a signal to the market about the government's willingness to repay debt and willingness to operate outside of international

institutions on debt problems. Normally countries in debt distress use the Paris Club or IMF to help reach some type of settlement with creditors. With odious debt claims, it is uncertain what international procedures – if any – will be followed. Next, odious debt claims send a message to the domestic population that they need no longer suffer for past governments. By setting a precedent that some debts can be repudiated with unclear conditions, it increases the risk that other debts, that are perhaps less odious, can be repudiated. The public may be less willing to make sacrifices and support reforms for debt repayment if the odious option is introduced by leaders and there is a precedent for repudiation. Consequently, odious debt claims carry a stigma that can have implications for a country’s perceived creditworthiness, affecting its access to capital and borrowing costs for years. Leaders who wish to maintain or quickly regain access to credit markets for their survival are thus unlikely to claim government debts are odious.

Because leaders want to avoid the consequences of odious debt claims, it is empirically challenging to estimate the effects of odious debt claims. The strategic avoidance creates a selection bias that complicates attempts to measure the effects of odious debt claims with observational data. To overcome this challenge, we demonstrate the market impact of odious debt framing by examining the effect of Norway’s 2006 debt relief action. In this unique case, Norway unilaterally forgave bi-lateral debts owed by five countries, explicitly labelling the original loans as “illegitimate.” This imposed an odious debt claim on these states, circumventing the usual leader-driven process. Our analysis reveals that despite Norway’s ostensibly good intentions, this program had negative consequences. It restricted the forgiven states’ credit access and increased their bond yield spreads by 300 basis points.

This study moves beyond the legal and normative implications of odious debt claims, which have dominated existing literature. After the U.S. and Iraqi governments toyed with the idea of declaring Saddam Hussein’s accumulated debts as odious, much was written on whether this was legal or whether it should be legal.¹ While this research is crucial for

¹For example, see Gelpern (2005, 2007); Lienau (2008); Nehru and Mark (2009); Cheng (2007); Jayachandran and Kremer (2006).

understanding how international law connects to the international political economy, it has not addressed the potential credit consequences of odious strategies. Our analysis extends beyond individual case studies to offer a broader perspective on the implications of odious debt claims.

Odious Debt: Legal Limitations and Political Considerations

The term “odious debt” is attributed to Alexander Sack, who analyzed American claims that Cuba and Puerto Rico were not responsible for the debts of Spain after the Spanish-American War of 1898 (Sack, 1927). While there is some disagreement about its criteria (Nehru and Mark, 2009), odious debt generally involves (1) debt incurred by a preceding non-democratic government, with subsequent regime change (2) debt used by the former regime for particularistic interests rather than the public good and (3) knowledge of these dynamics by creditors (Gelpern, 2007).

As a legal theory, odious debt claims have not gained traction in international courts. Based on the three criteria above, there are challenges for a state to prove its debt is odious. First, the concepts of sovereignty, regime, and regime change are contentious (Lienau, 2008). Academically, there are numerous conceptualizations of regime and thus there are several different interpretations on when a regime changes (Munck, 2016; Jee, Lueders and Myrick, 2022; Lueders and Lust, 2018). In addition, international law is consistent that new regimes are responsible for their predecessors’ obligations (Gelpern, 2007; Cheng, 2007). For example, the international arbiter, U.S. Chief Justice William Taft, ruled that “though the government changes, the nation remains, with rights and obligations unimpaired” in regards to Costa Rica’s debt obligations to Great Britain (quoted in Cheng, 2007, 11). Subsequent cases have reinforced this custom. The 1996 American cases against Iran ruled that “[W]hen a Government is removed through a revolution, the State, as an international person, remains

unchanged and the new government generally assumes all the previous international rights and obligations of the State.” (quoted in [Cheng, 2007](#), 11).

Thus from a legal standpoint, odious debt has little standing and isn’t used as defense in legal disputes over debt. States have forgone their debt obligations for alternative reasons, mostly citing economic and financial hardship. Consequently, international efforts and legal proceedings that address sovereign debt crises have primarily focused on restoring economic and fiscal sustainability.

Yet despite its lack of legal standing, the notion of odious debts persists and is often invoked in advocacy for debt forgiveness. Numerous international and domestic NGOs push for debt audits and argue for unilateral declarations of odious debt. These unilateral declarations have the potential to influence credit markets and consequently debt restructuring even if there is no legal endgame.

When Ecuadorian President Rafael Correa was elected in 2007, he immediately established the Public Credit Audit Commission to evaluate the legitimacy of Ecuador’s debt accumulation over the previous three decades. The committee found that large portions of Ecuador’s debt were illegal based on Ecuadorian law and illegitimate based on what the previous governments used the debt for. Ecuador subsequently defaulted on \$3.2 billion of its debt and restructured parts of its other debt in part by secretly buying back its heavily discounted debt on the secondary market.

Correa’s debt strategy reduced Ecuador’s debt burden by 40 percent and negotiated a 68 percent “haircut” on private creditors. It did not achieve complete debt relief but it provided Correa with several political benefits. First, Correa was able to successfully use the concept of odious debt as a central electoral strategy. Forgoing Ecuador’s debt obligations – to the detriment of foreign creditors - resonated with large segments of Ecuador’s population. “Life before debt” became the central slogan of his campaigns and administration ([Tello, 2008](#)). In addition, the imposed haircut on creditors freed up fiscal space for which to increase popular social spending programs. With the help of these benefits, Correa was able to win two more

elections.

Yet, despite Correa’s political accomplishments, few other leaders have pursued similar strategies of declaring their national debt odious or illegitimate. In a review of the 197 debt restructuring cases that occurred since 1975, the vast majority of leaders default for economic reasons.²

There are other exceptions. During Argentina’s most recent debt restructuring negotiations in 2020, President Alberto Fernandez invoked odious debt claims against the previous administration: “It is very difficult to consider odious debt when it is taken by a democratically elected government; it is not a debt contracted by a dictatorship. I have the sad privilege of being the first president to denounce the previous president for taking on a debt” (CE Noticias Financieras English, 2022). Similarly, President Néstor Kirchner claimed that Argentina’s old debts were illegitimate upon election in 2003, even though much of the debt was accumulated by co-partisan Peronists (Gelpern, 2005). Fernandez and Kirchner were not concerned that their claims were legally consistent with the concepts of odious debt. Instead, their strategies were political. Both were appealing to political supporters who wanted Argentina to follow a much more hard-line debt negotiation strategy (CE Noticias Financieras English, 2022; Gelpern, 2005). As a result, the leaders were constraining themselves with public opinion, making it more difficult to concede in negotiations.

From Odious Claims to Credit Constraints

Despite the apparent political benefits of claiming odious debts, leaders mostly avoid this strategy. While claiming debt as odious may provide some political support, leaders want to avoid negative consequences from the market.

Two studies have shown that an “odious” label can impact the pricing of specific bonds - relative to other bonds issued by the same state - by increasing the legal or reputational

²Restructuring case data is from [Cruces and Trebesch \(2013\)](#). We reviewed news coverage of these cases using keyword searches in Nexis of “odious” or “illegitimate” and found minimal use of this strategy.

risk of holding the bond. [Gulati and Panizza \(2020\)](#) find that labeling a particular bond as ‘odious’ can reduce demand among investors as creditors may face reputational consequences for holding odious assets. They back up their claim by analyzing the effect of press attention to a Venezuelan “hunger” bond in 2017 compared by comparing its price to other Venezuelan bonds. Examining a Russian bond used to finance its war with Japan, [Collet and Oosterlinck \(2019\)](#) find that public campaigns against the bonds increased the yield relative to other Russian bonds.

The use of odious debt claims could potentially be more common if their negative market impact was limited to the specific bonds or loans in question. However, we argue that declaring certain debts as odious would likely have broader consequences. It would not only affect the labeled debts but also negatively impact other existing loans and the government’s current borrowing capacity. Political leaders depend on this borrowing ability to maintain their position in office ([DiGiuseppe and Shea, 2016](#)). Therefore, they are hesitant to make odious debt claims, as doing so could lead to future credit restrictions. This broader financial risk discourages leaders from pursuing odious debt claims, even when such claims might be justified.

We suggest two avenues by which a claim of odious debt may impact a country’s broader access to credit. First, governments that claim ‘odious debt’ are likely to establish a reputation in credit markets that it is willing to break with international norms of repaying old debts or following the normal restructuring process. Reputation is often theorized to play a large role in credit markets ([Eaton and Gersovitz, 1981](#)). Most countries honor their debts because they fear market exclusion in the event of default. The idea has also been extended to show that states are also rewarded for making good on promises. For example, [Tomz \(2007\)](#) shows that states are rewarded for establishing a stalwart reputation by avoiding default even when economic conditions make it an attractive option and that states that default often face higher borrowing costs well into the future.³

³While there is some doubt reputations follow states for long periods ([Roos, 2019](#)). Leaders should be concerned with their reputations for the duration of their tenures.

As such, one might expect states that show a willingness to claim odious debt to gain a favorable restructure or repudiate debt may suffer higher borrowing costs or market exclusion for years following the incident. While debt restructurings are usually uncertain affairs with a high variance in process time, the processes is generally consistent if not entirely predictable. Adding a claim of odiousness will likely add additional uncertainty to current and future restructurings.

This additional uncertainty may stem from several sources. Notably, an odious claim introduces uncertainty over which norms of repayment and restructuring a state will uphold. Will states follow a traditional path of restructuring debts or will they go outside traditional paths as Correa did in Ecuador. It also introduces ambiguity over which debts qualify as odious and which are not. Given the lack of a clear definition of odious debts and the persistence of even low level corruption, investors might fear that any debt can be labeled odious by future governments once a state has established a reputation for questioning its obligations. This fear is justified by the lack of a clear threshold of odious debt. This increased uncertainty over what path a restructuring may take and which debts are odious or not is likely to lead to higher future borrowing costs as investors demand greater risk premiums to compensate for the unpredictable nature of debt repayment.

Lastly, odious claims may undermine future credit access via a state's citizens. The cases of Ecuador and Argentina discussed above suggest that odious claims appear to be popular with the public and thus motivated by political expediency. Public opinion might even be essential in evoking odious debt as it provides a key way for leaders to tie their hands to make a credible claim of repudiating their debts. For example, [Ferry \(2023\)](#) shows that leaders who make public declarations about an intention to default can extract higher haircuts from creditors. As such, using odious debt claims can provide a way to build public support by promising an simple alleviation of necessity of servicing debt with government revenue and increasing the resolve of leaders in restructuring disputes.

Debt, in general, is a highly moralized topic and this moralization extends to debates

over public debt (Graeber, 2014; DiGiuseppe and Del Ponte, 2023). By making an odious debt claim, leaders are likely drawing on the moral aversion citizens have to repaying a debt for which they did not consent nor benefit. In fact, most legal discussion of the concept of odious debt is couched in terms of justice and fairness Gelpern (2005).

In countries where the public has influence on fiscal policy, an introduction of an odious debt claim is likely to increase the challenge of reforming fiscal policy to accommodate debt service and repayment. Reforms to comply with debt obligations are notoriously unpopular given the burden often falls on vulnerable populations. Where the concept of odious debt takes hold, it provides a morally acceptable argument for citizens and politicians to support the repudiation of government debts or at least bolster a harder line in debt restructuring negotiations.

However, the odious debt concept may not be easily put back on the shelf when introduced widely to the public. By making an odious debt claim on one set of loans, the concept will likely diffuse to other government debt obligations. As legal scholars have noted, the concept of odious debt is difficult to bound. This is the fundamental problem with creating a legal standard for odious debt given the disagreement about where to draw the line between illegitimate and legitimate debt. Consequently, some have argued for an NGO to serve the role of labeling regimes as odious (Jayachandran and Kremer, 2006).

Domestic populations face a similar problem as they have limited information to judge which debts are legitimate and which are not. In many emerging and frontier markets, corruption still persists and the benefits and costs of international finance are highly unequal. As Ndikumana and Boyce (2011) note, it is still common for much of borrowed funds in Africa (and other countries) to cycle in to a country and then cycle out in to offshore bank accounts of the wealthy. Further, even advanced industrialized countries, like Italy, struggle with corruption on projects financed by bonds⁴ Thus, many past and current obligations have the potential to be labeled ‘odious’ if it becomes politically expedient for either the

⁴BBC News (2014).

government or the opposition to reintroduce the odious claim.

Given this fear, creditors may anticipate that any loan made to a country with a public primed to the odious debt concept and familiar with the arguments of odious debt have a higher probability launching a campaign against debt repayment and resistance to reform measures to repay the debt. Even if a government can credibly commit not to use an odious debt claim in the future, they still maybe constrained by a public that is eager to make the claim. Consequently, creditors will demand compensation for the risk that future loans are also labeled odious and the public increases pressure to repudiate the loans and is unwilling to support fiscal measures to repay loans.

Empirical Analysis: Norway’s Forgiveness

The rarity of odious debt claims presents challenges for empirical research. If these claims are an inefficient strategy for managing sovereign debt, rational leaders will avoid making them. The few instances where leaders have bucked this trend may not be generalizable, as these leaders strategically select themselves into odious claims for a number of political reasons. Our empirical strategy attempts to overcome these challenges. In 2006, Norway unilaterally forgave five countries’ debt because the debt was “illegitimate.” In short, we argue instead of borrowing leaders deciding to make odious debt claims, that decision was unwittingly imposed upon them. This case offers a quasi-experimental setup that allows us to examine the effects of odious debt claims while circumventing the selection bias inherent in leader-driven decisions. We discuss these circumstances and our empirical strategy below.

Norway debt forgiveness

In October 2006, Norway unilaterally cancelled \$80 million in debt owed by five developing countries: Jamaica, Peru, Ecuador, Egypt, and Sierra Leone. This debt originated from Norway’s Ship Export Campaign of the late 1970s, a failed development project that prioritized

Norway’s shipbuilding industry over the recipient country’s development needs ([Abildsnes, 2007](#)). Given the unprecedented nature of the debt forgiveness, the action was covered in the financial press [Guha \(2006a\)](#).

Norway’s Minister of International Development, Erik Solheim, explicitly acknowledged Norway’s co-responsibility as a creditor for this “development policy failure.” The debt cancellation was unconditional and not financed from Norway’s aid budget. Importantly, Norway cancelled this debt because it recognized its own culpability in making irresponsible loans to irresponsible regimes, not based on the borrowers’ poverty levels or debt sustainability. The debt relief came after years of sustained pressure from Norwegian civil society groups, who campaigned to frame the Ship Export Campaign debt as “illegitimate” ([Guha, 2006b](#); [Abildsnes, 2007](#)).

Norway’s debt cancellation offers a unique opportunity to examine the effects of odious debt claims on sovereign borrowing costs. By comparing the five countries that received this unilateral debt relief to other countries that received Norwegian debt relief through traditional channels (like international debt relief programs or Paris Club negotiations), we isolate the effect of the “illegitimate debt” framing. This quasi-experimental setup allows us to control for many confounding factors that typically plague cross-country comparisons of debt outcomes. All countries in our sample had debt to Norway, but only these Ship Export Campaign countries had their debt cancelled explicitly due to illegitimacy concerns. This variation enables us to test whether markets react differently to debt relief when framed as correcting odious lending, rather than default or debt relief in general.

While Norway never labelled the Ship Export Campaign loans as odious, we argue that this debt relief program fits this category. The five countries forgiven borrowed when they were autocratic regimes. By 2006, four of the five countries experienced regime change. According to V-Dem data on regimes, Ecuador, Jamaica, and Peru had moved from dictatorships to democratic governments in the early 1980s ([Maerz et al., 2021](#)). Sierra Leone’s transition occurred later in 2002. Only Egypt did not have a regime change, though it did

have a leader change in 1981, along with multi-party elections for the first time in 2005 (Stacher, 2008). The fairness of that election is doubtful (Stacher, 2008), but Norway still made a governance distinction in its 2006 relief program. In its announcement, Norway explicitly excluded Myanmar and Sudan from immediate debt relief due to their continued autocratic rule (Abildsnes, 2007). The Norwegian government stated that these two countries could become eligible for debt cancellation in the future, but only when they met certain governance criteria.⁵

In addition, Norway’s debt relief in 2006 was in recognition that the Ship Export Campaign was motivated by benefits towards Norway’s ship-building industry rather than the benefits towards recipient countries. Several recipient countries complained of poor construction of ships or revenues being misused by government elites (Abildsnes, 2007). One Norwegian official called the program a “development failure” while another official called it “a stain of shame on Norway’s aid policy” (Guha, 2006*b*; Abildsnes, 2007).

Finally, Norway’s announcement highlighted the idea of creditor responsibility for its loan program. Norway did not conduct due diligence or risk assessment on the Ship Export Campaign, a legislative requirement (Abildsnes, 2007). Nor did the government raise concerns about autocratic rule in recipient countries at the time of the program. Norway’s 2006 debt relief plan wanted to rectify this wrong, and “give rise to an international debate on lender responsibility”(Abildsnes, 2007).

These three conditions – autocratic borrowing, misuse of funds, and credit responsibility – are consistent with odious debt claims. Previous research argues that is difficult to identify these conditions because concepts such as regime type or regime change are contested (Gelpern, 2007). Yet in this specific case, these conditions are unilaterally decided upon by Norway. Thus it does not matter from a legal standpoint whether Egypt experienced a regime change, only that Norway was satisfied that something about Egypt’s governance changed and improved. This unilateral determination by Norway provides a rare opportu-

⁵In 2013, Norway forgave Myanmar’s debt, though this relief was handled by the Paris Club.

nity to study odious debt dynamics without the usual complications of contested definitions or debtor-creditor disputes.

To examine the effect of Norway’s debt relief, we compare the credit access and credit cost consequences for the 2006 recipients to other states that received debt relief from Norway. Through the Paris Club and the Heavily Indebted Poor Countries (HIPC) Initiative, Norway has forgiven several countries’ bilateral debt, including other participants of the Ship Export Campaign.⁶ Table 1 lists the countries in the sample. However, these debt relief countries in the bottom panel focused on recipient countries’ poverty and fiscal problems and were carried out through multilateral processes. In contrast, the 2006 debt relief case operated outside of these global governance institutions, focusing instead on creditor responsibility and the questionable nature of the original loans.

Table 1: Norway’s Debt Relief Countries

Odious 2006		
Jamaica	Peru	Egypt
Ecuador	Sierra Leone	
Debt Relief		
Serbia	Liberia	Benin
Senegal	Côte d’Ivoire	Gambia
Ghana	Guinea	DR Congo
Tanzania	Pakistan	Malawi
Indonesia	Montenegro	

Comparing the 2006 countries to other countries receiving debt relief creates a clearer basis for evaluating the credit consequences of odious debt claims. This approach allows us to isolate the effect of the “illegitimate debt” framing from the general impact of debt relief. Both programs are designed to ease borrowing countries’ liquidity problems and lower debt servicing requirements.⁷ However, only the 2006 forgiven group had their debt canceled

⁶We examined Paris Club and IMF reports, along with Norway’s press releases to identify debt relieved countries.

⁷Lang, Mihalyi and Presbitero (2023) show that financial markets respond positively to debt relief efforts and that these efforts do not carry a stigma.

explicitly due to its dubious origins. This variation enables us to test whether financial markets react differently to debt relief when framed as correcting past lending mistakes, rather than simply addressing fiscal distress or poverty concerns.

Data

To measure the financial repercussions of odious debt, we first focus on states' ability to issue bonds on international financial markets. Bonds are important for countries' ability to raise revenue to meet fiscal shortfalls. In addition, debt managers rely on issuing new bonds to service existing obligations, while spreading out the maturity profile of the country's debt over time (Ballard-Rosa, Mosley and Wellhausen, 2021*a*). This approach helps mitigate refinancing risks and maintain fiscal flexibility.

In order to issue bonds on the global market, there must be a sufficient demand from creditors. If creditors doubt the creditworthiness of a state, demand will be lower. To measure the demand for a state's bonds, we examine the total number of bonds issued in a given year that were at least 6 months in maturity.⁸ Bond issuance data are taken from Ballard-Rosa, Mosley and Wellhausen (2021*a*) and cover the years 1990 to 2016. We also examine whether a state issues bonds in a given year or not and the logged total amount of bonds in a given year, measured in constant U.S. dollars.

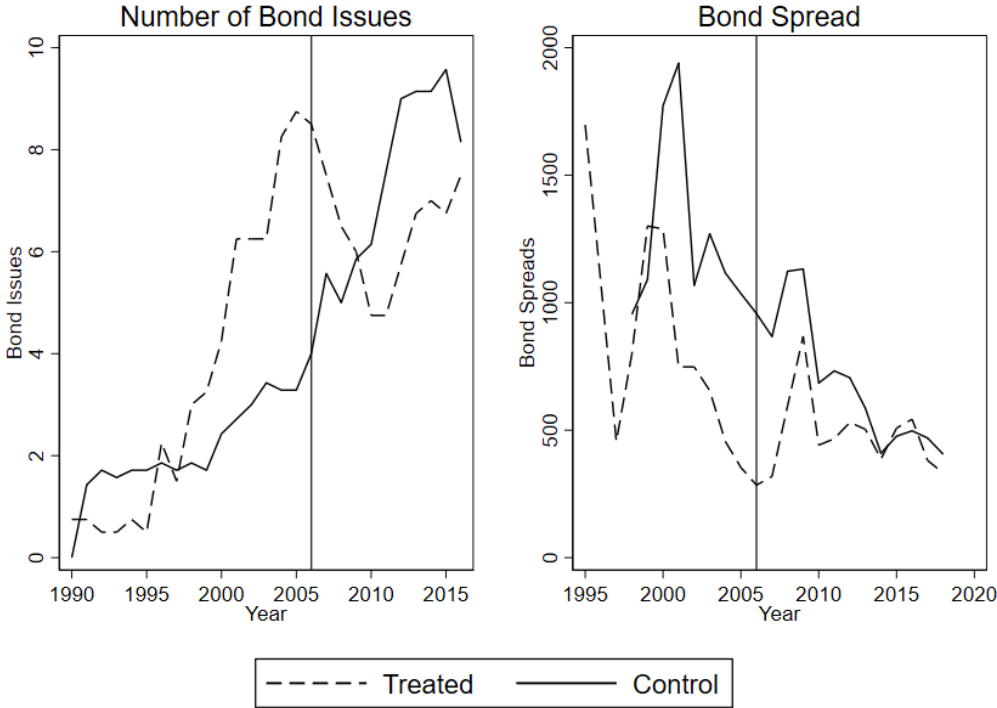
As an alternative to bond issuance, we also focus on states' bond yield spreads. Bond issuance is the beginning stage of a state's credit access. These bonds are then traded on secondary markets. Spreads are the average secondary market price of a country's bond, compared to the lowest-risk bond in the market: U.S. Treasury bills. These spreads represent the financial market's perception of a state's creditworthiness and its overall risk of lending to a state. Annual data come from J.P. Morgan's Emerging Market Bond Index Global (EMBIG) and were collected from Brooks, Cunha and Mosley's (2022) replication files.⁹

⁸We focus on the 6-month threshold to be consistent with Ballard-Rosa, Mosley and Wellhausen's (2021*a*) analysis. Results are robust to all issuances or issues for at least 1 year.

⁹Supplemental data on bond spreads for Jamaica were taken from IMF country reports (International

The main treatment variable is whether a country in the sample received debt relief from Norway for its Ship Export Campaign in 2006 or not. Splitting the sample into two groups – those states that would eventually receive forgiveness in 2006 (the treated group) and those that would receive forgiveness under alternative programs (the control group) – we can observe any unadjusted difference in bond issues (left panel) and spreads (right panel) in Figure 1. For bond issues, we see that both groups’ issuances increased around 1997, though the soon-to-be-treated group at a higher rate. Once the debt is forgiven by Norway, the treated group’s issuances decrease over the next 5 years, while the control group continues to increase.

Figure 1: **Bond Issues and Spreads for Norway’s Forgiven States**



It may be the case that Norway’s debt forgiveness decreased the need for the five treated states to raise debt on international markets. However, the actual forgiven amount of debt was relatively small compared to these countries’ debt burden. For example, Norway forgave [Monetary Fund, 2007](#)). Because of data availability, Sierra Leone is dropped from the sample, along with Liberia, Benin, Guinea DR Congo, Malawi, and Montenegro.

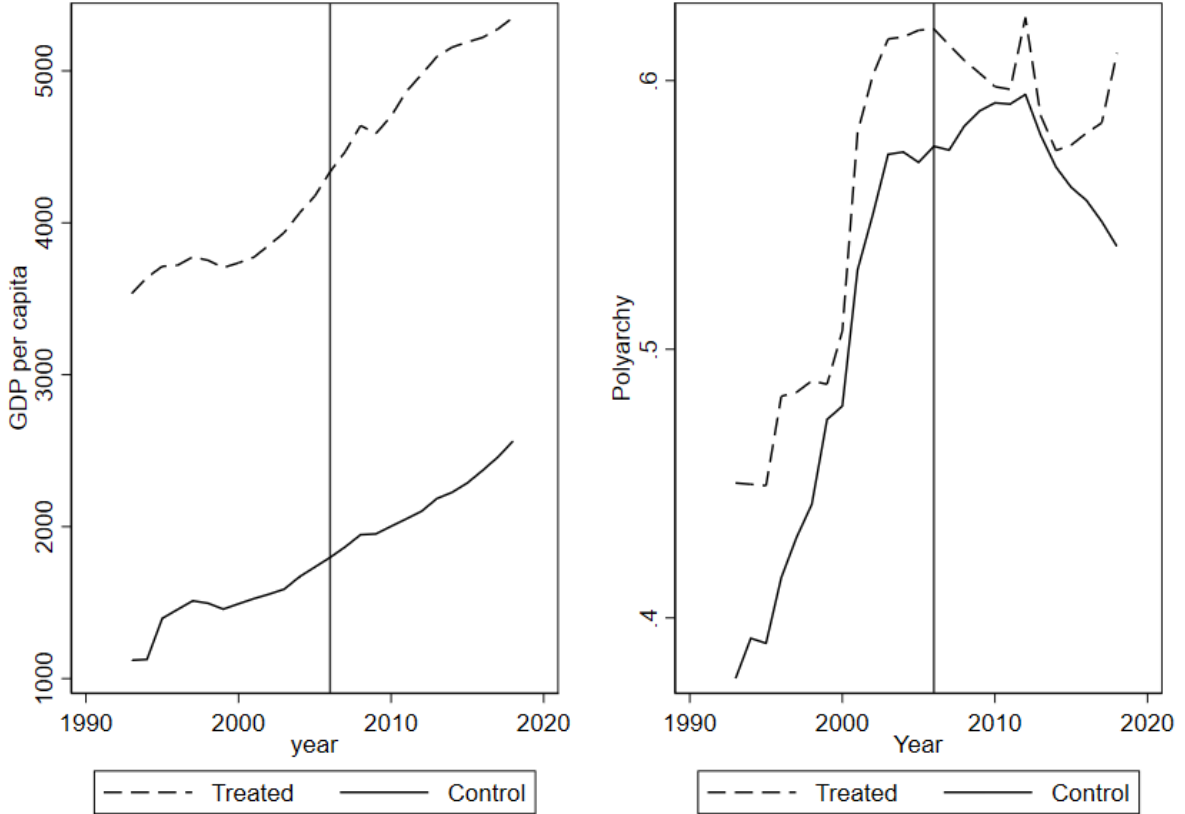
225 million NOK of Ecuador’s debt (equivalent to 37 million USD) in 2006. While this was the largest amount forgiven, Ecuador would go on to default on 10 times this amount in 2009. In addition, the forgiven debt does not explain the pattern observed in bond spreads in Figure 1. Starting about 2000, we see that both groups’ bond yields begin to decrease in parallel to one another. Once the debt is forgiven by Norway, the treated group’s bond yields level off, whereas the control group’s spreads continue to decline. If Norway’s debt forgiveness program was relieving the debt pressure on the treated states, the market should perceive less risk in bond spreads.

There is a gap in bond issuances and spread between the two groups, where the treated group has more bond issuances and lower bond spreads in the period leading up to 2006. Checking the difference in observables in Figure 2, we can see that the treated group has higher GDP per capita before and after 2006 (data taken from World Bank (World Bank, 2019)). States with higher development have more resources with which to repay their debts. In addition, the treated group has a slightly higher Polyarchy score (data from V-Dem (Pemstein et al., 2018)). States with higher democracy ratings have been shown to receive more favorable treatment in sovereign debt markets (Ballard-Rosa, Mosley and Wellhausen, 2021b; Beaulieu, Cox and Saiegh, 2012). Both of these differences may explain why the treated group may have received more bonds and better spreads in the pre-treatment period. However, there are no patterns in Figure 2 that help explain the post-treatment convergence in Figure 1

To estimate whether the 2006 odious debt forgiveness event affected the forgiven states, we employ a two-way fixed effects model.¹⁰ This approach allows us to control for time-invariant country characteristics and common time trends while isolating the effect of the debt forgiveness treatment. Our model specification is as follows:

¹⁰This difference-in-differences estimator has come under fire recently as a host of new DiD estimators have been proposed. The central issue with TWFE is when treatments are staggered (Roth et al., 2023). However, TWFE provides an unbiased estimate when all units are treated at the same time as is the case in this analysis.

Figure 2: **Characteristics of Norway's Forgiven States**



$$Y_{it} = \beta_0 + \beta_1 \text{Odious } 2006_{it} + \gamma_i + \delta_t + \beta_x X_{it} + \epsilon_{it} \quad (1)$$

Where Y_{it} is the bond outcome of interest (issuance and spreads) for country i at time t , Treatment_{it} is a dummy variable equal to 1 for countries that received odious debt forgiveness in 2006 and 0 otherwise, γ_i represents country fixed effects, δ_t represents time fixed effects, X_{it} is a vector of time-varying control variables, and ϵ_{it} is the error term. The coefficient of interest is β_1 , which captures the average treatment effect of the odious debt forgiveness on debt outcomes.

By including country-fixed effects, we control for time-invariant country characteristics that might affect credit perceptions, such as geographic location or historical institutions. Time-fixed effects control for global trends or shocks that affect all countries in a given

period, such as global financial crises or changes in international interest rates.

In addition, we include control variables that may have affected Norway’s decision to provide debt relief from the Ship Export Campaign in 2006. As noted above, the odious group has a higher development and democracy score, so we control for GDP per capita and Polyarchy. In addition, we control for debt levels, as highly indebted countries were targeted for debt relief by Norway before 2006 (data from IMF (*IMF Data and Statistics*, 2010)). In addition, we account for political stability by controlling for attempted coups or any type of conflict in the country (data from Powell and Thyne (2011) and Nils Peter Gleditsch and Strand (2002) respectively). Finally, we include dummy variables to indicate when our control countries received debt relief.

Results

The main results are reported in Table 2. Model 1 uses a linear probability model to examine whether countries that received relief from Norway’s Ship Export Campaign in 2006 had a lower probability of issuing a bond in a given year after the relief. We observe a lower probability that is statistically significant. Model 2 includes control variables and we observe consistent results.

Model 3 in Table 2 examines the total number of bonds a country issues in a given year, showing that the treated countries issued fewer bonds after 2006. In addition, it was not just a matter of the treated countries issuing less, but higher valued bonds. Model 4 demonstrates that the value of the bonds that were issued decreased for the treated countries. Finally, model 5 shows that the costs of credit also increased for the treated states. After 2006, bond spreads increased by 316 basis points relative to the control group.

The results in Table in Table 2 show that Norway’s well-intentioned debt relief program had negative market effects for relieved countries. Bond issuance became more difficult, while credit costs increased. We undertake additional analysis to ensure that our inferences are valid.

Table 2: Norway's Odious Debt Relief Effects on Bond Issues and Spreads, 1990 - 2018

	Any Issue (1)	Any Issue (2)	Total Issues (3)	Total USD (4)	Bond Spread (5)
Odious	-0.478*	-0.522*	-1.817*	-7.647*	316.598*
	(0.072)	(0.105)	(0.672)	(2.183)	(152.048)
GDP per cap		0.000	0.001	0.002	0.007
		(0.000)	(0.000)	(0.001)	(0.067)
Polyarchy		0.738*	1.602	8.679*	676.587*
		(0.174)	(1.213)	(3.828)	(338.177)
Govt debt / GDP		-0.000	0.004	0.015	3.261
		(0.001)	(0.007)	(0.020)	(2.001)
Coups		-0.047	-0.369	-1.024	-27.019
		(0.100)	(0.642)	(2.019)	(137.489)
Conflict		-0.016	-0.375	0.370	46.442
		(0.055)	(0.357)	(1.125)	(80.917)
Total Issues (lag)			0.752*		
			(0.040)		
log Bond Issue USD (lag)				0.343*	
				(0.063)	
Spread (lag)					0.663*
					(0.064)
Constant	0.182*	-0.171	-1.390	-5.687	-756.974
	(0.088)	(0.185)	(1.228)	(3.861)	(450.957)
R2	0.52	0.60	0.85	0.66	0.76
N	297	285	275	275	180

* $p < 0.05$; Unit and year-fixed effects are included in each model. Standard errors are reported in parentheses. Models 1 - 2's dependent variable is whether a country issued a bond in a given year. Model 3's dependent variable is the total number of bonds issued in a given year. Model 4's dependent variable is the total logged value of bonds issued in a given year in USD. Model 5's dependent variable is bond spreads.

First, we examine the role of Ecuador. Ecuador is one of the few cases that fully embraced an odious debt strategy. Rafael Correa campaigned on this issue at the same time that Norway forgave its Ship Export Campaign loans. Correa continued to label part of Ecuador's debt odious until debt negotiations concluded in 2009. Thus Correa's odious strategy coincides with Norway's debt forgiveness plan. To ensure that Ecuador is not driving the results in Table 2, we exclude it from the sample. We find consistent, if not stronger results than the other models (see appendix).

Next, we re-consider the sample. We focused on comparing the treatment countries to other countries that received some type of debt relief from Norway. This restricted sample was designed to account for unobservable characteristics that may dictate creditworthiness and countries' participation in debt relief programs. To demonstrate that our results are robust to this design choice. We replicate the models in Table 2 on different samples. We examine a larger sample of states that received money from Norway for the Ship Export Campaign (Abildsnes, 2007). We also examine whether a state received any loan from Norway.¹¹ Finally, we examine all developing countries, which we define as countries that are not members of the OECD. The results are largely consistent with the results in Table 2 (see appendix).

To further demonstrate that our results are not driven by sample choices, we employ the generalized synthetic control (Xu, 2017). Synthetic control constructs a counterfactual for the treated unit by creating a weighted combination of control units that closely matches the pre-treatment characteristics of the treated unit. The generalized synthetic control method developed by Xu (2017) follows the same logic but allows for multiple treated units. In sum, it is a data-driven approach to find the best possible control units to compare to the treated units in the pre-treatment period.¹² To implement this method, we use the *gsynth* package in R, with the matrix completion estimator (Xu, 2017; Athey et al., 2021). This approach treats counterfactuals of treated units as missing data and uses matrix completion techniques to estimate the missing values.¹³

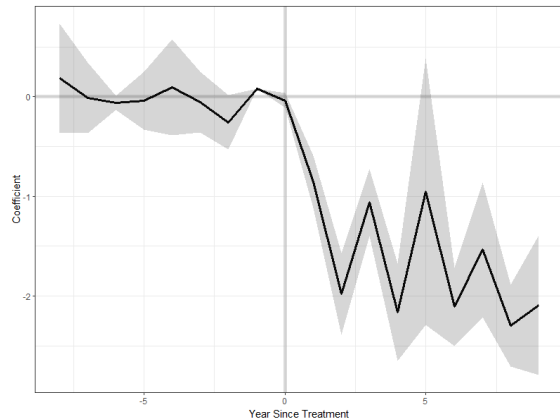
Focusing on bond issuance values, Figure 3a reports the average treatment effect on the treated (ATT).¹⁴ This represents the difference between the bond issue values of the countries that had their debt forgiven by Norway in 2006 and the estimated counterfactual

¹¹Bilateral loan data are from Bunte (2019).

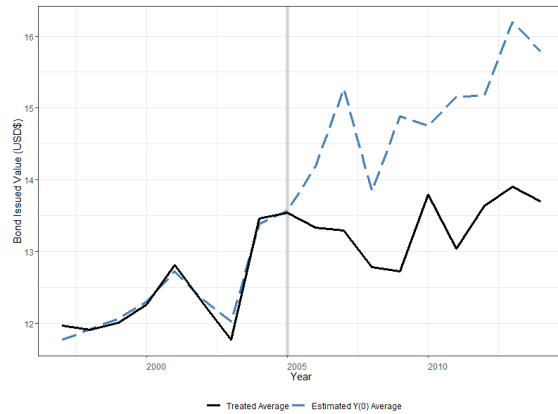
¹²Synthetic control methods require balanced panels with minimal missing data. As a result, we limit the sample to 1997 to 2015. More discussion on the data preparation and models are found in the appendix.

¹³Matrix completion estimators are comparable to synthetic control or difference-in-difference estimators, but appear to perform better (Athey et al., 2021).

¹⁴To estimate the synthetic counterfactual, the dependent variable needs to be continuous, with minimal missingness and sufficient variance. We therefore report the results of bond issuance value. The results are similar to the number of bonds issued. Bond spreads have too much missingness to estimate.



(a) Estimated ATT



(b) Treated and Counterfactual Bond Values

Figure 3: Generalized Synthetic Control

if that debt forgiveness never happened.¹⁵ To illustrate this, Figure 3b plots the actual bond issue values for the treated countries compared to the counterfactual. In the pretreatment period, the treated and counterfactual overlap considerably, suggesting a good fit in the model. In the post-treatment period, bond values level off for the treated countries, while the counterfactual's values increase. These results are consistent with our main inferences.

Conclusion

Managing blame is a large component that explains leaders maintaining office (McGraw, 1991; Weaver, 1986). As a result, it would be logical for leaders to look to blame past despotic regimes for present-day debt problems. Yet, leaders rarely do so. To explain this puzzle, we argue that odious debt claims provoke more severe market consequences than traditional debt repudiation strategies, such as restructuring. Odious claims signal to lenders that signal to markets that their domestic political institutions might be vulnerable to financial impropriety. In addition, odious claims signal that countries want to operate outside of existing norms of international debt negotiations, increasing the uncertainty over debt outcomes.

¹⁵The confidence intervals in Figure 3a are bootstrapped from 1,000 iterations of the model, accounting for uncertainty in the estimation of the potential counterfactuals.

We test our argument by leveraging Norway’s unique debt relief program in 2006. The five forgiven countries had their debt labeled illegitimate for them. Our analysis demonstrates that this forgiveness carried a stigma that restricted future debt access and increased debt prices. These findings explain why leaders do not pursue odious debt strategies.

The legacies of colonial and authoritarian regimes continue. The global community is more and more accepting that historical debt injustices have implications for countries’ present-day debt distress. Yet, our findings suggest that repudiation of this debt may have negative consequences. Policymakers and international institutions may need to develop strategies to address the moral dimensions of debt while mitigating adverse market reactions. Norway was praised for its innovative and moral approach to debt forgiveness in 2006 (Guha, 2006*b*). Yet, while Norway received all the credit, the forgiven states unknowingly were going to bear the consequences.

Moving forward, we look to examine further which mechanism is at play. Do claims of odious debt send signal directly to creditors that question the willingness of a regime to play by the rules. Or do odious claims influence the public’s willingness to repay debts and thus impact the ability of governments to raise revenue for repayment? The research design presented above limits our ability to examine these mechanisms, or others, independently. Leverage on the public mechanism is possible by examining the efficacy of public debt claims on the public via public opinion surveys. To date, we do not know how generalizable the experiences in Ecuador are and if odious debt claims can over power norms of debt repayment among the public. Further, only direct study of investor preferences can determine if they are internalizing the signals sent by odious claims. While challenging answering these questions will give us better leverage on why international norms of debt repayment are upheld in the face of repugnant debt.

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1 Appendix

1.1 Excluding Ecuador

Table 3: Debt Relief Effects on Bond Issues and Spreads without Ecuador, 1990 - 2018

	Any Issue (1)	Any Issue (2)	Total Issues (3)	Total USD (4)	Bond Spread (5)
Odious	-0.487* (0.080)	-0.577* (0.111)	-2.014* (0.744)	-8.616* (2.329)	393.864* (120.129)
GDP per cap		0.000 (0.000)	0.001 (0.000)	0.002 (0.001)	0.009 (0.051)
Polyarchy		0.868* (0.193)	1.764 (1.396)	10.813* (4.260)	-10.167 (359.583)
Govt debt / GDP		0.000 (0.001)	0.004 (0.007)	0.018 (0.021)	0.066 (1.752)
Coups		-0.029 (0.106)	-0.323 (0.709)	-0.864 (2.151)	21.032 (107.054)
Conflict		-0.026 (0.056)	-0.399 (0.380)	0.249 (1.153)	40.573 (61.439)
Total Issues (lag)			0.753* (0.042)		
log Bond Issue USD (lag)				0.315* (0.065)	
Spread (lag)					0.760* (0.060)
Constant	0.200* (0.094)	-0.148 (0.182)	-1.177 (1.265)	-5.337 (3.838)	534.518 (292.486)
R2	0.53	0.63	0.85	0.68	0.85
N	270	258	249	249	157

* $p < 0.05$; These models exclude Ecuador from the sample. Unit and year-fixed effects are included in each model. Standard errors are reported in parentheses. Models 1 - 2's dependent variable is whether a country issued bond in given year. Model 3's dependent variable is the total number of bonds issued in given year. Model 4's dependent variable is the total logged value of bonds issued in a given year in USD. Model 5's dependent variable is bond spreads.

1.2 Synthetic Control Method Details

The synthetic control methods focuses on developing countries. To create a balanced panel, we use imputation to address missingness of the covariates using the *missForest* package in R (Stekhoven and Bühlmann, 2012). With the imputed data, we then delete any unit that does not have full coverage.

We replicate the main analysis with the imputed data in Table 4 and find similar results to our main results.

Table 4: Analysis with imputed data, 1997 - 2015

	Any Issue (1)	Any Issue (2)	Total Issues (3)	Total USD (4)	Bond Spread (5)
Odious	-0.611* (0.084)	-0.532* (0.110)	-1.062* (0.392)	-3.026* (1.222)	377.569+ (198.614)
Controls		✓	✓	✓	✓
R2	0.51	0.59	0.80	0.55	0.71
N	180	180	306	306	122

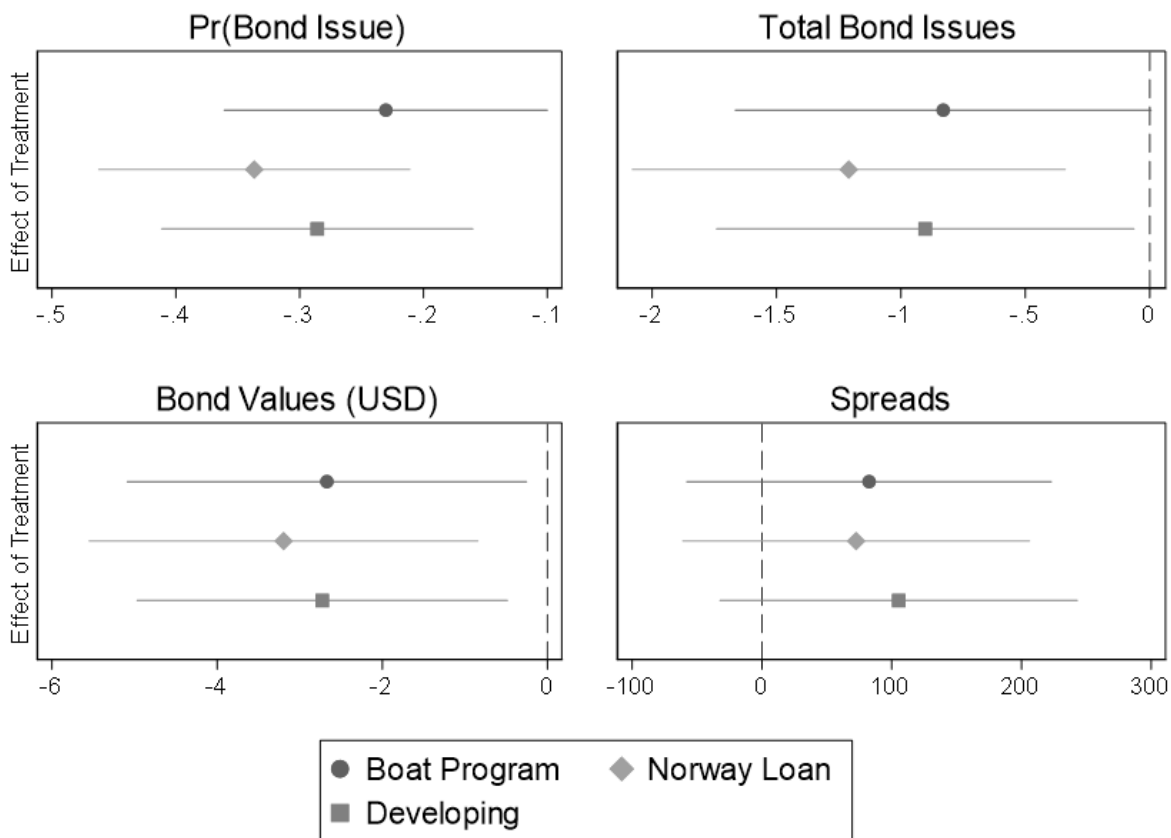
* $p < 0.05$ + $p < 0.1$; Unit and year-fixed effects are included in each model. Standard errors are reported in parentheses. Models 1 - 2's dependent variable is whether a country issued a bond in a given year. Model 3's dependent variable is the total number of bonds issued in a given year. Model 4's dependent variable is the total logged value of bonds issued in a given year in USD. Model 5's dependent variable is bond spreads. Controls included in models 2-5 but not shown.

1.3 Alternative Sample Comparisons

We replicate the models in Table 2 on different samples. We examine a larger sample of states that received money from Norway for the Ship Export Campaign (Abildsnes, 2007). We also examine whether a state received any loan from Norway.¹⁶ Finally, we examine all developing countries, which we define as countries that are not members of the OECD.

The results of estimated effects across these three alternative samples are shown in Figure 4. The results across the three samples across the four credit outcomes are consistent with the results in Table 2, except for bond spreads. The coefficient for the treatment is not significant for the alternative samples. However, we note that the sign is in the expected direction and that spreads have more non-random missingness than bond issuance data.

Figure 4: Norway’s Odious Debt Relief Effects Across Samples



¹⁶Bilateral loan data are from Bunte (2019).

1.4 Estimator Assumptions and Parallel Trends

The main inference in the manuscript relies on a two-way fixed effects (TWFE) estimator that is akin to a difference-in-difference estimator. One important assumption of this estimator is that the difference between the treatment and control groups would have remained the same if the intervention never occurred. In the past decade, much has been written about this parallel trends assumption and its implications for applied work.¹⁷

To follow best practices, we follow [Roth et al.’s \(2023\)](#) recommended checklist to examine the robustness of inferences given our assumptions of the parallel trends. The first issue is the timing of the treatment. Given that Norway applied the odious debt relief at the same time to the five countries in question, we do not face issues of staggered treatment. In addition, since Norway was responsible for the relief, we avoid strategic selection and timing of the treatment.

The second issue we consider is the balance of our panel. In a non-conditional model, our panel is balanced except when using the bond spread data. As discussed in the previous section, we also use imputation methods to ensure that our panel with covariates are also balanced.

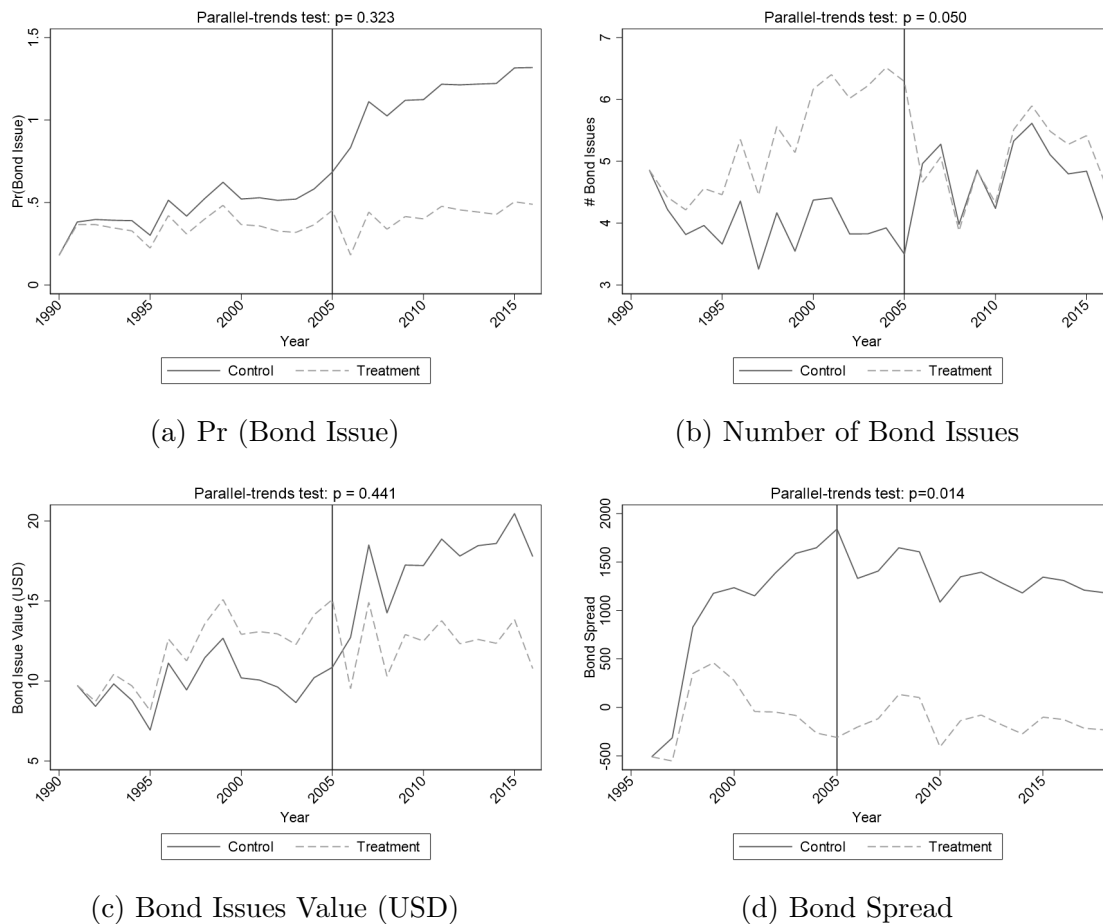
The set of issues relates to the parallel trend assumption. There is no one way to examine this assumption, so we follow a number of empirical strategies. First, we can examine the raw averages across our treatment and control groups, as we did in [Figure 1](#). We noted that the pre-treatment trends do appear in parallel for both the number of bond issues and bond spreads, though an increasing gap may be present before 2006.

Alternatively, we can examine the linear trends, which interact year-dummies with the treatment group indicator, and plot the predicted values for the treatment and control groups. [Figure 5](#) plots the linear trend predictions for each of the four outcome variables, along with their corresponding parallel trends F-test p-value. Under this test, the null hypothesis is that the treatment and control groups’ lines are parallel in the pre-treatment

¹⁷For example, see [Rambachan and Roth \(2023\)](#); [Roth \(2022\)](#); [Roth et al. \(2023\)](#); ?

window. We reject the null for bond spreads and number of bond issues.

Figure 5: Linear Trends



By themselves, linear trend tests do not tell us about the implications of pre-treatment trends. Both bond spreads and number of issues are diverting from each other in the pre-treatment period, which does not suggest that an unobservable confounder is threatening our inferences. At the same time, the insignificant tests for the binary bond issue outcome or bond issue values outcome may be a result of an underpowered test (Roth et al., 2023). Thus we examine the parallel trends in alternative ways.

We assess the plausibility of the parallel trends assumption in an event study plot. This plot examines whether outcomes for the treated and comparison groups move in parallel before the time of treatment. The plot can provide an assessment of pre-existing trends and

allows us to detect any anticipatory effects or dynamic treatment effects post-intervention (Roth et al., 2023). We assume that in each pre-treatment period, the difference in outcomes from time t' – the reference time – to t – the lead or lag in question – for both the treated and control groups will be statistically indistinguishable from each other

$$\underbrace{E[Y_{i,t} - Y_{i,t'} | \text{Odious}_i=1]}_{\text{Pre-treatment change for Odious}_i=1} - \underbrace{E[Y_{i,t} - Y_{i,t'} | \text{Odious}_i=0]}_{\text{Pre-treatment change for Odious}_i=0} = 0 \quad (2)$$

These differences can be assessed using the TWFE estimator with leads and lags of the treatment:

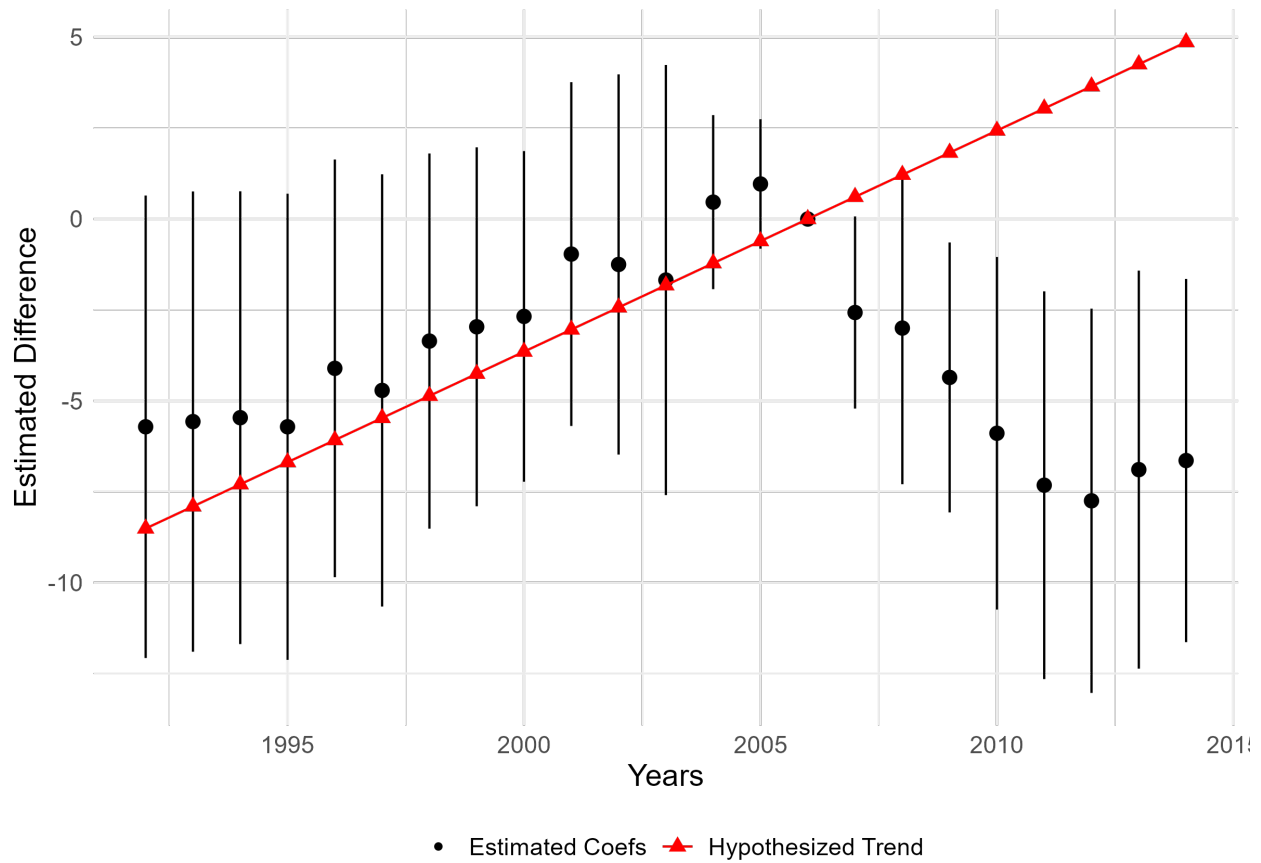
$$Y_{i,t} = \alpha_i + \phi_t + \sum_{r \neq 0} 1[R_{i,t} = r] \beta_r + \epsilon_{i,t} \quad (3)$$

where $R_{i,t}$ represents the time relative to treatment. The coefficients β_r for $r < 0$ correspond to pre-treatment periods and allow us to assess the parallel trends assumption. The parameters α_i and ϕ_t represent the unit and year fixed effects, respectively, and $\epsilon_{i,t}$ is the error term.

If these coefficients for β_r are close to zero and statistically insignificant, it provides evidence supporting the parallel trends assumption. Figure 6 plots the coefficients focusing on the bond issues outcome, derived from the *pretrends* library in R (Roth, 2022). In the pre-treatment period, we observe that each coefficient’s 95 percent confidence interval overlaps with zero. In addition, Figure 6 plots a plausible pre-trend that the pre-test has 80 percent power to detect. It reflects a potential pre-treatment trend that could bias the results if it existed but went undetected. This line overlaps with all of the confidence intervals in the pre-treatment but not the post-treatment differences. This lends confidence to our assumptions about parallel trends.

We repeat this exercise with bond values and find similar results in Figure 7. We also examine the binary bond issue outcome, along with bond spreads. For the former, we observe similar results as the bond issues and bond value outcomes. For spreads, the lack of data

Figure 6: **Event Study Plot of Bond Issues**



for the control group leads to some noise in the differences, making these graphs less useful. These are available in the replication files.

In addition, as an alternative to linear pre-trends in the data, we examine potential non-linear differences in the treatment and control groups using quadratic and polynomial functions. We find no evidence that potential trends bias our results (see replication files).

Figure 7: Event Study Plot of Bond Value (USD)

