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IDE DISCUSSION PAPER No. 858

Environmental Protection after Civil War: A Difference-in-Geographic-Discontinuity Approach

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August, 2022

Abstract

Although civil war devastates the environment, we still do not understand the role of environmental policies in post-war countries and often have a pessimistic view without empirical evidence. We challenge this view by arguing that the introduction of independent monitoring mechanisms can make environmental regulations effective even in post-war countries. We substantiate this claim by exploiting analytical opportunities in the Democratic Republic of the Congo (DRC). In 2011–2013, the government implemented independent monitoring mechanisms to lessen the side effects of mining activities on deforestation. The reform, however, only applied to mining permit zones, which had arbitrary square shapes. By combining a geographic regression discontinuity and difference in differences to what we call a difference-in-geographic-discontinuity (DiGD) design, as well as using satellite-based data available at every 30 meters for over 40 million cells in the DRC, we find that the 2011–2013 reform substantially decreased deforestation rates immediately inside the mining permits. This finding implies that the environmental effects of civil war can crucially depend on post-war policies.

Keywords: Civil war, Deforestation, Difference in geographic discontinuity, Satellite data

JEL classification: C23, Q54, Q56, Q58

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Civil war not only kills human beings (Lacina and Gleditsch 2005; Ghobarah, Huth, and Russett 2003) and devastates economies (Abadie and Gardeazabal 2003; Horiuchi and Mayerson 2015) but also destroys the natural environment (Fergusson, Romero, and Vargas 2014; Burgess, Miguel, and Stanton 2015; Kikuta 2020). For example, the Great War of Africa in the Democratic Republic of the Congo (DRC) resulted in a loss of forestland roughly equivalent to the size of Belgium and a carbon sink that could absorb the annual emissions of approximately eight million automobiles (Kikuta 2020). Civil war frequently leaves legacies that continue to afflict countries even long after its end (Thyne 2016). Given these devastating impacts, it is imperative to analyze which types of policy interventions can mitigate the adverse impacts of civil war.

However, scholars have found it difficult to evaluate the causal effects of environmental policies in post-war countries due to the lack of identification strategies, the paucity of fine-grained data, and insufficient theoretical understanding. In the absence of scientific knowledge, one might easily accept the pessimistic view that environmental policies would not function effectively in post-war countries. One NGO, for instance, stated that "[t]ransitions to peace are typified by weak state control[;] this means that environmental governance . . . and the capacity to provide it is often absent" (Conflict and Environment Observatory 2020). In this paper, we provide an alternative by developing a theoretical argument, substantiating it with a natural experiment, and using high-resolution satellite data.

Substantively, based on the literature of regulatory politics (Becker 2000; Gray and Shimshack 2011; Tosun 2012; Shimshack 2014), we argue that the introduction of an independent monitoring mechanism is critical for protecting the environment after civil war. Although the literature tended to focus on developed countries (Gray and Shimshack 2011; Shimshack 2014), recent studies expand the scope to developing countries (Duflo et al. 2013; 2018) and even to post-conflict countries (Buntaine and Daniels 2020; Buntaine, Hunnicutt, and Komakech 2020; Buntaine, Nielson, and Skaggs 2021; Slough et al. 2021; Christensen, Hartman, and Samii 2021). Based on those insights, we hypothesize that an independent monitoring mechanism provides reliable information about non-compliance, increasing the reputational costs for inaction and hence incentivizing the government to punish non-compliance. The credible threat of punishment, in turn, discourages people from violating environmental regulations. Thus, we expect the introduction of an independent monitoring mechanism to make environmental regulations more effective.

Empirically analyzing the effectiveness of environmental regulations, however, poses challenges. A government can carefully set a schedule to make a particular policy appear to have the largest impact possible. The government may also consciously select the geographical units subject to environmental regulations. Given these possibilities, the treatment and control units can be heterogeneous; hence, conventional research designs, such as cross-sectional regressions or even difference in differences (DiD), cannot identify the causal effects.

We address these challenges by applying a new research design to the case of the DRC. In 2013, over a decade after the end of the civil war, the government of the DRC introduced innovative independent monitoring mechanisms to mitigate the environmental side effects of mining production. The monitoring mechanisms, however, only apply to mining permit zones, which have arbitrary grid-based shapes. The arbitrary boundaries of permit zones, coupled with the temporal intervention, allow us to combine two well-known research designs—the geographic regression discontinuity (GRD) and DiD designs—to what we call the difference-in-geographic-discontinuity (DiGD) design. The DiGD relaxes the assumptions required for GRD or DiD, thus permitting us to identify the causal effect more rigorously.[1]

Nonetheless, DiGD cannot provide a causal effect without precise spatial data (Keele and Titiunik 2015; Keele, Titiunik, and Zubizarreta 2015). The core idea of DiGD is to focus on changes in small neighborhoods around a boundary. Because those units are similar except for the policy intervention, we can plausibly infer the casual effect. However, if it were not for data of high geographical precision, we could not make such a comparison, and thus our inference would be subject to biases. We address this problem by using satellite-based data available at every 30 meters for over 40 million cells (2001-2017; Hansen et al. 2013). The fine-grained data allow us to compare locations just a few kilometers around the boundaries of mining permits. Furthermore, we apply recent progress in matching techniques (Keele, Titiunik, and Zubizarreta 2015) to ensure that the inference would be robust to the specifications of a regression model.

Combining the DiGD, matching, and satellite data, we find that the introduction of the monitoring mechanism substantially lowered deforestation rates in the areas immediately inside mining permit zones. Moreover, the analysis suggests that the effect can even permeate into some areas of protracted insurgencies. Additional analyses of the causal mechanisms and robustness provide further credence to our argument. These findings suggest that, even though environmental policies might not always be effective, independent monitoring mechanisms can potentially result

[1] To our best knowledge, there is only one article in the economics literature that combines DiD and GRD (Dantas et al. 2018), but the researchers used a boundary that coincided with administrative boundaries. Other articles in the economics literature combines DiD and non-geographic regression discontinuity (Grembi, Nannicini, and Troiano 2016; Chicoine 2017).

in better environmental protection—even in post-war countries. Future studies should extend this analysis to different cases and policy measures to examine the external validity of our findings.

Civil War and Environmental Policies

With the increasing attention to environmental changes and civil war, a growing number of scholars have analyzed the relationship between these two phenomena. Although researchers have almost exclusively focused on the effect of the natural environment on armed conflict (Hsiang, Meng, and Cane 2011; Hendrix and Salehyan 2012; Hsiang, Burke, and Miguel 2013; Buhaug et al. 2014; Krauser 2020), some have recently begun to examine the reverse relationship: the environmental consequences of civil war (Hendrix and Glaser 2011; Mitchell and Thies 2012; Fergusson, Romero, and Vargas 2014; Burgess, Miguel, and Stanton 2015; Ordway 2015; Daskin and Pringle 2018; Sexton 2019; Kikuta 2020). Other studies also analyze the effects of war ending on the environment (Ordway 2015; Bouley et al. 2018; Lahkar et al. 2018; Panel and Pietri 2019; Prem, Saavedra, and Vargas 2020; Clerici et al. 2020). In general, these studies indicate that civil war has adverse impacts on the natural environment.

However, we cannot easily project these findings about the past wars to future civil wars without considering post-conflict environment policies. As recent studies suggest (Buntaine and Daniels 2020; Buntaine, Hunnicutt, and Komakech 2020; Buntaine, Nielson, and Skaggs 2021; Christensen, Hartman, and Samii 2021; Slough et al. 2021),[2] post-war policies can potentially mitigate the adverse effects on the environment. Although the current evidence is still limited, the policies have been implemented in several post-war countries such as Colombia, Liberia, Uganda,

[2] For other case studies, see Kovach and Conca (2016) Sierra et al. (2017), and Suarez, Arias-Arévalo, and Martínez-Mera (2018).

and the Democratic Republic of the Congo. Given these recent trends, past findings may not be extended to the future; if more countries implement environmental policies after civil war, the adverse effects on the environment can be lessened. This means that without understanding the effectiveness of post-war environmental policies, we may not accurately predict the long-term effects of civil war.

Recent studies fill the gap by analyzing the persistent effects of civil war on the environment and the effectiveness of post-war environmental policies. Prem et al. (2020), for instance, analyze the effect of the ceasefire on deforestation in Colombia. They find that even though the end of the civil war increased deforestation rates, the effect was smaller in localities with high-level state capacities, indicating the mitigating role of post-war policies. By contrast, Clerici et al. (2020) find that the end of the Colombian Civil War intensified deforestation in protected areas, suggesting ineffectiveness or even adverse effects of environmental policies. Ordway (2015) also finds that deforestation rates were higher in the protected areas during the Rwandan Civil War.

Other studies look at more specific policy interventions, especially those related to community monitoring of resource usage. Christensen et al. (2021) analyze the effects of community monitoring on forest management in Liberia. Similarly, a series of works by Buntaine and others also analyze the effects of citizens' audits on public service provisions, including the revenue-sharing projects in Bwindi National Park, Uganda (Buntaine and Daniels 2020; Buntaine, Hunnicutt, and Komakech 2020; Buntaine, Nielson, and Skaggs 2021). Slough et al. (2021) synthesize these studies as well as other studies in Brazil, China, Costa Rica, and Peru. The results of these studies are, however, mixed at best. Although Slough et al. (2021) find that community monitoring facilitated efficient and sustainable resource usage, other studies do not find direct effects of the policy interventions (Buntaine and Daniels 2020; Buntaine, Hunnicutt, and Komakech 2020; Buntaine, Nielson, and Skaggs 2021; Christensen, Hartman, and Samii 2021). The null findings echo more general findings that community monitoring is not as effective as expected (Gray and Shimshack 2011; Turreira-García et al. 2018).

This paper is built on these studies but with a different focus. Similar to previous studies, we analyze a single case where a post-war government implemented an environmental policy. As such, we do not claim that our findings would be generalizable. The goal is instead to probe the possibility that even a post-war government can protect the environment. On the other hand, different from previous studies, we focus on a state-wide government policy. Thus, unlike extant studies about war termination (Ordway 2015; Prem, Saavedra, and Vargas 2020; Clerici et al. 2020), we look at a specific policy intervention. Moreover, unlike studies about community monitoring (Buntaine and Daniels 2020; Buntaine, Hunnicutt, and Komakech 2020; Buntaine, Nielson, and Skaggs 2021; Christensen, Hartman, and Samii 2021; Slough et al. 2021), we analyze a more comprehensive policy package introduced by a central government. Given the mixed results about community monitoring, it is worthwhile to consider the effectiveness of a more traditional approach (Gray and Shimshack 2011).

Protracted Conflicts and Environmental Monitoring

Post-war governments face immense challenges for their survival. In fact, as is the case in many post-war countries, even when a civil *war* officially ends, low-level armed *conflicts* often continue, posing threats to the survival of the government.[3] In the absence of security, the government often

[3] Armed conflicts include sporadic violent attacks carried out by rebels even after the achievement of a peace agreement.

pursues short-term gains. The situation often becomes worse due to persistent corruption, lack of effective enforcement, and the prioritization of personal over collective welfare. These factors together make it particularly difficult to protect the environment. While many developing countries share similar problems as well, the situation is especially challenging in post-war countries, in which a government has nearly collapsed and low-intensity conflict still continues. Then, people may expect that any policy measure, such as monitoring and punishment, might not be effective in post-war countries.

We, however, argue that an independent monitoring mechanism—an institutional mechanism that publicly tracks compliance independently from political or economic influences—can address the problems of non-compliance (Ostrom, Walker, and Gardner 1992; see Dipper 1998; Gray and Shimshack 2011; Tosun 2012; Shimshack 2014 for review). With an independent monitoring mechanism, independent parties track compliance, and information is made available to the public. As a result, non-compliance and government inaction could be publicized, possibly resulting in backlashes from both the domestic and international communities. Non-compliance and the resultant destruction of the environment, for instance, might trigger punishment from the international community, including the cessation of aid programs—one of postwar governments' largest revenue sources (Arvin, Dabir-alai, and Lew 2006; Spilker 2012). The local community and their representatives in local and central governments can also protest against environmental destruction via elections (List and Sturm 2006) or in the streets (Rootes 2013). Even worse, opposition parties may use the situation to gain public support (Carter 2006). Wary of such backlashes, governments have a higher incentive to punish non-compliers. The potential for punishment, in turn, makes non-compliance costly.

The increased costs for non-compliance help environmental protection through two channels. The first and straightforward channel is *compliance*; an independent monitoring mechanism makes it more likely that a government finds and punishes non-compliances. The credible threat of punishment can induce compliant behaviors and thus protect the environment. An independent monitoring mechanism, however, has another effect: *screening*. That is, with the increased risks and costs, non-compliant operators may exit from the market, leaving only compliant ones. This means that the policy intervention can change the composition, instead of behaviors, of operators. The selection of "good" operators helps protect the environment.

One advantage of a monitoring mechanism is that it requires a relatively low level of direct enforcement (Ostrom, Walker, and Gardner 1992; Heyes 2000; Gray and Shimshack 2011; Shimshack 2014). Although the mechanism ultimately depends on the government's punishment and the potential for backlashes, the government does not need to directly enforce the regulations on every occasion or deploy a large amount of financial and human resources. The government is only expected to detect and punish non-compliers. This approach is especially beneficial for post-war governments, which have relatively low levels of administrative control and coverage (Fearon and Laitin 2003; Hendrix 2010). Moreover, as studies about environmental monitoring suggest (Malik 1993; Kaplow and Shavell 1994; Innes 1999; Livernois and McKenna 1999; Arts, Caldwell, and Morrison-Saunders 2001; Toffel and Short 2011), the government does not need to monitor the situation by itself; the mandatory submission of environmental reports, combined with public and independent assessments of reports, is considered a compelling substitute for direct monitoring. Even though operators may fabricate their reports, the independent assessments of the reports (e.g., inspection by departments in charge of environmental issues and public inspection) make it difficult if not completely impossible (Dipper 1998; Pfaff and Sanchirico 2004; Friesen and Gangadharan 2013; Telle 2013).

Hypothesis

Thus, we hypothesize that the introduction of an independent monitoring mechanism facilitates environmental protection by inducing compliance and screening out non-compliers. By contrast, regulations without independent monitoring mechanisms do not have equivalent effects. The rival null hypothesis is that an independent monitoring mechanism has no effect, possibly due to the lack of political will to enforce punishments or the absence of strong backlashes. Note that backlashes and government punishment are not observable at an equilibrium (Malik 1993; Kaplow and Shavell 1994; Innes 1999; Livernois and McKenna 1999; Arts, Caldwell, and Morrison-Saunders 2001; Toffel and Short 2011). These constitute an off-equilibrium path that supports an equilibrium. That is, with the potential for backlashes and punishment, people comply with regulations (and because they comply, there is no backlash or punishment). Thus, we do not expect that the introduction of a monitoring mechanism would increase or decrease the degree of backlashes or punishment.

Note also that the hypothesis pertains to average effects. That is, because an independent monitoring mechanism is not a panacea, it cannot completely mitigate non-compliance or environmental destruction. Furthermore, we should not assume that monitoring mechanisms are always independent. Occasional violations and deviations from the equilibrium may happen, and politicians and stakeholders might sometimes wield their influence over monitoring processes. Our argument is that, on average, the environment is better protected with an independent monitoring mechanism than without one.

Finally, we do not claim that all post-war governments have strong incentives to introduce independent monitoring mechanisms. Given the endemic corruption, lack of administrative capacity, and the urgent need for economic recovery, they may not introduce monitoring mechanisms. This certainly limits the external validity of our research, but it is critically important for considering future environmental policies to know what would happen if a post-war government could introduce a monitoring mechanism. The case of the DRC, which we detail in the next section, provides unique analytical opportunities for answering the question.

The Mining Cadastre and the 2011–2013 Reform

We test the hypotheses by exploiting unique analytical opportunities in the DRC: arbitrary boundaries (i.e., mining permit zones) and temporal intervention (i.e., the 2013 decree), which we detail in this section. Although modern environmental regulations or independent monitoring mechanisms are relatively rare in conflict-prone countries (Fredriksson and Svensson 2003), the DRC managed to institute them with the cooperation of the World Bank. The policies were, however, only applied to mining permit zones, which had arbitrary grid-based shapes.

The DRC, located at the center of Africa, is a rich reservoir of biodiversity, including the second-largest conglomeration of tropical rainforests in the northwest, savanna trees in the southeast, and a wide variety of species, such as endangered gorillas and elephants. The country is also one of the largest exporters of mineral resources, including gold, copper, cobalt, coltan, and diamonds. Since the collapse of the Mobutu dictatorship, the DRC became trapped in a series of civil wars (1996–2002). Although the eastern part of the country is still affected by insurgency (as of 2020), the 2002 Sun City Agreement put an end to the civil wars, and the mineral boom in the 2000s and 2010s contributed to the rapid growth of the economy.

However, the side effects of mining activities on the environment have emerged as a major concern (UNSC 2001; Greenen 2015). Not only did open-pit mining and the accompanying infrastructure construction result in substantial land clearing,[4] but artisanal mining was also reported to cause deforestation. Artisanal mining requires a large volume of wood to support underground mining pits (called wooden trunks; Greenen 2015). Without a wooden trunk (or even with it), a mining pit can easily collapse. Indeed, Greenen, who conducted fieldwork on mining in the DRC, stated that

> [t]he practice of boisage [digging tunnels with wooden trunks] has far-reaching environmental effects in that it requires a lot of wood. In Mukungwe [a mining town in South Kivu], for example, the trees are supplied from a plantation some 20 kilometres away, on the other side of the hill. (2015, 82)

Since the colonial era, the country has gone through several rounds of regulatory reforms regarding mining to mitigate the environmental side effects of the industry. Yet, even during the civil wars, the DRC still had opaque and outdated mining regulations (Third World Network-Africa 2017). Immediately after the civil war, the DRC attempted to address the problem with the assistance of the World Bank. In 2002-2003, the government replaced the previous Zairean mining regulations with the 2002 Mining Code and the 2003 Mining Regulation, which one NGO praised

[4] Roads and bridges are usually made of wood collected from neighboring areas.

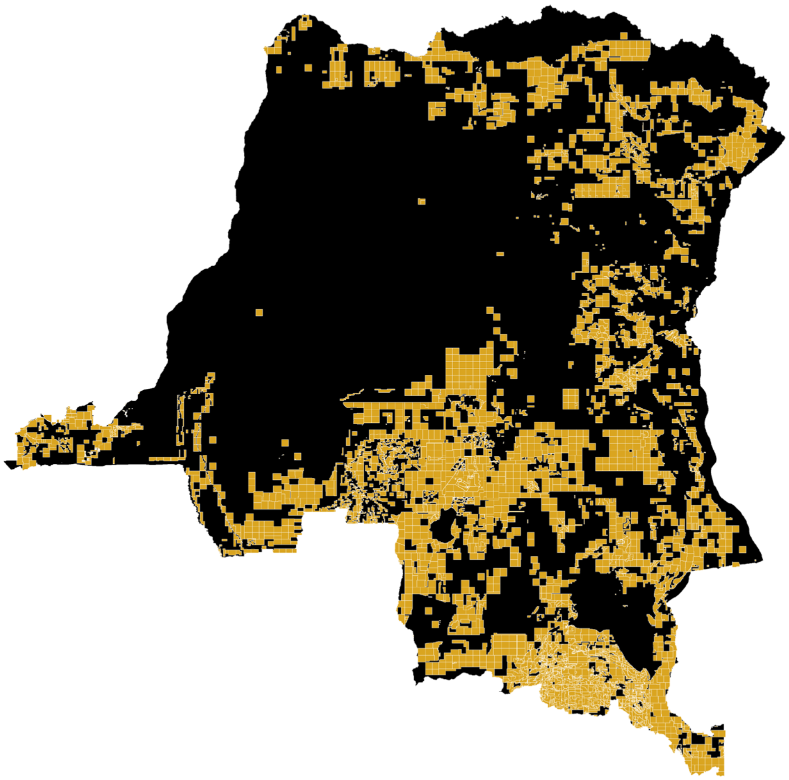
as "revolutionary and ambitious" (Ortega, Pugachevsky, and Walser 2009; Third World Network-Africa 2017, 12).[5]

An important feature of the 2002–2003 initiative was the introduction of the *mining cadastre system*—a signature of modern mining regulations. Under the cadastre system, the entire territory of the DRC is divided into *cadastral units*. The minimum size of a cadastral unit is 30 arc seconds (approximately 1.1 km at the equator) on each side. In practice, however, operators use aggregated cells (usually 5 or 10 km), as seen in Figure 1.[6] Applicants pay acquisition fees based on the number of cadastral units they specify. The Ministry of Mines Mining Registry (CAMI; *Cadastre Minier* in French) is responsible for managing the cadastre system.

[5] The 2002 Mining Code is Law No. 007/2002 of 11 July 2002, and the 2003 Mining Regulation is Decree No. 038/2003 of March 26, 2003. English translations are available at http://congomines.org/ (accessed on 2020-01-15).

[6] The data come from the government of the DRC (https://data.globalforestwatch.org/datasets/democratic-republic-of-the-congo-mining-permits; accessed on February 18, 2020).

Figure 1. Mining Permits in the DRC (2013)



NOTE: This figure shows mining permits in the DRC (May 29, 2013). The yellow areas represent the mining permit zones, and the thin white lines are their boundaries.

Although the 2002–2003 initiative modernized the country's environmental regulations, it still lacked independent monitoring mechanisms. The mining code required concessioners to take appropriate measures for environmental protection and to submit environmental impact studies (EISs) and environmental management plans (EMPPs). The 2003 decree—the mining regulation—also stipulated punishments for non-compliance, including the suspension of mining operations for 90 days or more. However, EISs and EMPPs are only assessed by the CAMI—a ministry whose main objective is to promote the mining industry. No third-party validation or public inspection processes existed. Thus, despite the number of progressive features, the 2002–2003 initiative still did not lead to independent monitoring mechanisms.

The situation changed in 2011–2013. With the continuing assistance of the World Bank, the Environmental Protection Law was passed in 2011, and the corresponding detailed enforcement regulation—the Decree on Regulated Facilities—was passed in 2013.[7] One of the critical features of the reform was enhancing the independence of environmental monitoring. Mining concessioners were required to obtain the approval of EISs and EMPPs not only from the CAMI but also from the Ministry of Environment, which was relatively free from industrial interests (Hogg 2011; de Schoutheete, Hollanders, and Endundo 2019). Moreover, all mining operators had to obtain an environmental exploitation license, which was subject to public inspections and approval by multiple state and local organizations (Biduaya and Mukendi 2013). Indeed, seeing these features of the reform, Congolese legal consultants even applauded it as a "clear commitment of the DRC Government to exploit its natural resources while making sure of mitigating social environment impacts particularly related to mining industries, in order to provide with a sustainable human environment" (Biduaya and Mukendi 2013).

Thus, we expect that the 2011–2013 reforms (specifically, the 2013 decree), which introduced independent monitoring mechanisms in the DRC for the first time, contributed to the protection of forests in the mining permit zones, while the 2002–2003 initiatives (specifically, the 2003 decree), which did not feature independent monitoring mechanisms, did not have an

[7] The Environmental Protection Law is Law No. 009/2011 of July 9, 2011, and the Decree on Regulated Facilities is Decree No. 015/2013 of May 29, 2013.

equivalent effect.[8] An alternative hypothesis is that neither the 2002-2003 nor 2011-2013 initiatives bring substantial changes. Even though the Great War of Africa officially ended in 2002, the country has continued to suffer from prolonged armed conflicts, especially in the eastern border regions. Given the protracted conflicts, endemic corruption, and lack of state capacity, it would not be surprising if the environmental policies would have no substantial impacts.

An empirical problem is that the timing and implementation of those policies were far from random. The 2002–2003 initiatives were a direct consequence of the end of the Great War of Africa in early 2002. The 2011–2013 reforms were also initiated just subsequent to the 2011 presidential election, after which Joseph Kabila entered his last term and thus was free from electoral concerns. Moreover, the years 2011–2013 were also the apex of the mineral boom in the international market, and the DRC recorded high fiscal surpluses in 2013 (OECD 2020). Given these political and economic contexts, we can hardly argue that the policy intervention would be plausibly exogenous.

Research Design

We address the endogeneity problems by using the temporal interventions (i.e., the 2003 and the 2013 decrees) and arbitrary boundaries (i.e., the mining cadastre system), which together allow us to combine the GRD and DiD designs to develop what we call the DiGD design. This design permits us to compare the changes in deforestation rates in areas barely inside or outside of the mining permit zones. Those units are very similar except for their legal status as zones with

[8] We choose to focus on the 2003 and 2013 decrees because they finalized and implemented the series of policy changes in 2003–2003 and 2011–2013, respectively. Later in this paper, we also analyze the effect of the 2011 legislation (i.e., the Environmental Protection Law).

permits; therefore, geographic conditions or any nationwide changes, such as external pressures, Kabila's reelection, or economic growth, could not explain the differences in terms of deforestation.

If it were not for the temporal intervention, we might compare the rates of deforestation immediately inside and outside of the mining permit zones. That is, because mining regulations or reforms apply only to areas with mining permits, we can assess the effectiveness of the regulations or reforms by comparing the areas barely inside and outside of the permit zones. Effective regulations should result in lower rates of deforestation immediately inside the mining permit zones—the core idea of GRD, which has been widely used in the social sciences (Dell 2010; Keele and Titiunik 2015; Keele, Titiunik, and Zubizarreta 2015). As Keele and Titiunik (2015) demonstrate, GRD yields a causal effect when a continuity assumption is satisfied; that is, if it were not for treatment, there would be no discontinuity in the rate of deforestation on a geographical boundary. The assumption can be violated if the geographic boundaries coincide with other pre-existing boundaries or if the boundaries were selected at a fine scale.[9]

The mining cadastre system in the DRC provides a guard for continuity assumption. Even though mining operators can choose the boundaries of mining permit zones at a coarse scale, they must select the predefined grid cells, and hence they do not have latitude at a fine scale. For instance, they may select a cadastral unit that is likely (or proven) to have a mining deposit, but they cannot select only the deposit. They must choose the corresponding grid cell (cadastral unit);

[9] Another important assumption is that units' locations are not selected at a fine scale (i.e., a no-sorting assumption). However, because we use fixed cells as units, the sorting is impossible by design. To ensure the validity of this assumption, we also demonstrate that the sorting is indeed absent. See SI 7 for details.

thus, the permit includes the areas around the deposit, which may or may not be of interest. Therefore, if we limit the analysis to the immediate surroundings of the permit boundaries, we can plausibly assume that the boundaries are free from selection biases. Moreover, the mining activities are operated even in the border areas of the mining permits. Although we do not have comprehensive data of mining activities, we do have the data for the eastern provinces, in which 1,367 mining sites were operated immediately inside of the permit zones (See later section about bandwidth specification and SI 2).

However, it would be excessive to claim that GRD alone would provide a plausible estimate. Because GRD only analyzes cross-sectional variation, it cannot directly assess the effects of temporal intervention (the 2013 decree), in which we are interested. Moreover, the size of the cadastral units is about 1.1 km on each side. Although operators usually use aggregated cells (5 km or 10 km cells) in practice, concessioners can potentially select boundaries even at the scale of 1.1 km.[10] In GRD, this might force us to analyze overly small areas, have too few unique data points, and hence make the estimates sensitive.[11]

This fact, however, does not mean that we can simply use DiD. Although the DiD assumes that all cells would have experienced similar trends of deforestation before and after the 2013 decree (i.e., a parallel trend assumption), the assumption is unlikely to hold. Because mining permit zones contain more mining sites, they are more likely to be affected by the mineral boom in the

[10] The average length of each side of a permit zone is 4.754 km (as of May 29, 2013).

[11] As mentioned later, we use 120 m by 120 m cells. Thus, if we limited the analysis to areas less than 1.1 km away from the boundaries, there would be less than 20 unique data points of the running variable.

2010s or by the 2010 Dodd-Frank Act, which was followed by the 2012 conflict minerals rule that the U.S. Securities and Exchange Commission subsequently issued.[12] Therefore, it is difficult to identify the causal effect of the 2011-2013 reform by simply comparing the areas inside and outside of the permit zones. Neither GRD nor DiD alone is sufficient for identifying the causality.

Difference-in-Geographic-Discontinuity (DiGD)

We address the problems described above by combining GRD and DiD to the DiGD design. Unlike GRD, which examines a static discontinuity at the boundaries, DiGD analyzes the changes in the degree of discontinuity before and after a policy intervention. Thus, by using DiGD, we can analyze whether the 2013 decree altered the effectiveness of mining regulations. Moreover, because we examine temporal changes, any static factors, such as the distribution of mining sites and their side effects, cannot explain the changes in the discontinuity.

The DiGD design can be considered as an extension of GRD or DiD. As mentioned, DiGD is a dynamic version of GRD; instead of analyzing the static discontinuity, it analyzes the changes in the discontinuity. As a result, as Grembi et al. (2016) demonstrate in the context of a non-geographic RD design, the confounding boundaries or selections can exist as far as they are static. Moreover, DiGD can also be considered as a local version of DiD. While DiD compares all units inside and outside of mining permit zones, DiGD compares a much smaller number of units located

[12] An international law firm indeed reports that the Section 1502 of the Dodd-Frank Act "aims to curtail funding sources for armed groups in the Democratic Republic of Congo (DRC) by imposing public disclosure and reporting requirements on issuers that use 'conflict minerals'–gold, tantalum, tin, and tungsten–in their manufacturing processes" (Stern and Dort 2011, 1). For the subsequent progress of the U.S. conflict minerals rule, see Thomas (2021).

within a few kilometers around the boundaries. Thus, unlike DiD, DiGD does not rest on the parallel trend assumption. Taken together, DiGD relaxes the separate assumptions required for GRD and DiD.

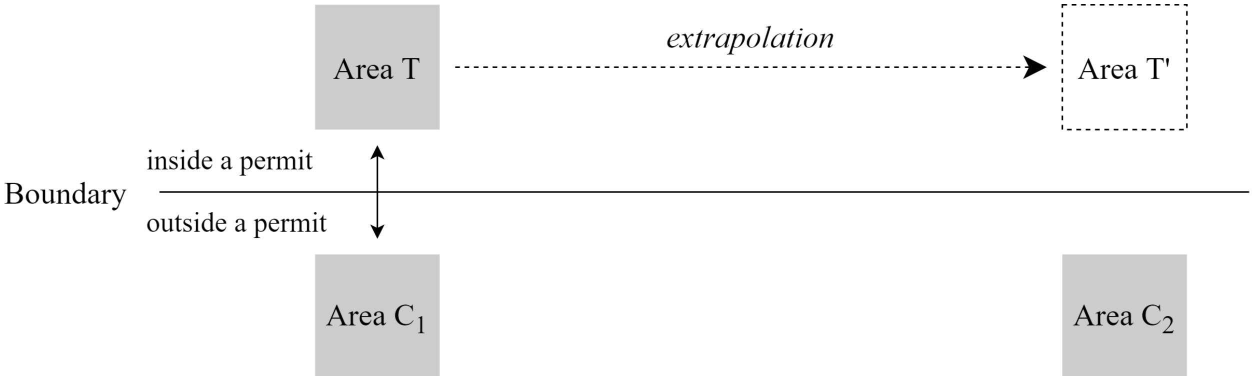
A critical assumption required for DiGD is that there would be no or only static discontinuities at a boundary if it were not for a temporal intervention (i.e., a *static discontinuity assumption*). A stronger version of this assumption is that areas just inside or outside of a boundary would be similar, or at least their heterogeneities would be time-invariant if it were not for a temporal intervention (i.e., a *static local heterogeneity assumption*). When either of these assumptions is satisfied, DiGD yields an average treatment effect of a temporal intervention within a small neighborhood around a boundary.[13]

Geographical Matching

A methodological twist is that the DiGD compares units in a two-dimensional space; that is, unlike conventional RD designs, the running variable is defined by two variables: latitude and longitude. With two running variables, naively using the distances to the boundaries as a running variable can cause the so-called extrapolation problem (Keele and Titiunik 2015; Keele, Titiunik, and Zubizarreta 2015). Figure 2 is a stylized example of extrapolation. In the figure, Area T located inside the mining permit zone can be plausibly compared to the nearby Area C_1. However, even though Area C_2 is equally close to the boundary, it is very far from T. If we naively used the distance to the boundary, we would risk extrapolating T to T' and comparing T' to C_2. The inference, however, critically depends on how we map T to T'. Without additional adjustment, therefore, the results may be sensitive to the specification of the mapping function.

[13] Grembi et al.'s (2016) proof for a difference-in-(nongeographic)-RD is directly applicable here.

Figure 2. Stylized Example of Extrapolation



NOTE: This figure presents a stylized example of the extrapolation problem in DiGD. The center line is the geographic boundary that separates the areas inside and outside of a mining permit zone. Area T is located inside of a permit zone, while Areas C_1 and C_2 are located outside. We can plausibly compare closely located T and C_1, but it is more difficult to compare distantly located T and C_2, as such a comparison would involve the extrapolation of T to T'.

We address this problem by applying a recent refinement in matching techniques. Although matching is often misconceived as a mere technique for covariate adjustment, it actually allows for the design of an observational study to address a variety of methodological concerns without relying on regressions or their functional forms (Rosenbaum 2009). In our case, we match the units based on their latitude and longitude (without any other covariates) and drop treatment or control units that were distant from their counterparts (Keele, Titiunik, and Zubizarreta 2015).[14] We use matching with replacement and a Euclidian distance matrix.[15]

Satellite "Big" Data

To reiterate, DiGD critically hinges on geographical precision; fine-grained data are not only desirable for the purpose of measurement, but they are integral to causal identification. To this end, we use 120-*meter*-by-120-*meter* cells for the 2001–2017 period as units of analysis. We first limit the observations to those within 4 km from the boundaries of the mining permit zones to make the data size manageable (Figure 3).[16] We use the constant boundaries fixed on the date of each treatment for all years to ensure a balanced panel (resulting in two balanced panels: one each for the 2003 and 2013 decrees).[17] This approach mitigates the possibility that the treatments could

[14] Keele and Titiunik (2015) propose a regression-based solution, which essentially breaks down the boundaries to multiple points and estimates the effect at each point. However, the boundaries of the mining permit zones are complicated, and the sheer size of the sample makes it impossible to use their method. Furthermore, although Keele, Titiunik, and Zubizarreta (2015) introduce a matching method based on intensive integer optimization, their method does not work even with moderately large data (more than 20,000 treatment units in our case).

[15] We calculate the nearest distance from a given cell to the cells of different treatment statuses. If the distance is more than the twice that of the bandwidth (7 km in the main analysis; the maximum unit-wise distance in GRD), the cell is dropped. The matching is conducted for each mining permit. Matching without replacement is computationally too expensive and hence not used.

[16] 4 km is the maximum plausible bandwidth of DiGD. See SI 2 for the bandwidth selection.

[17] March 25, 2003, for the 2003 decree and May 29, 2013, for the 2013 decree.

affect not only the values of the outcome variable but also the selection of units.[18] The resulting datasets have 22,533,215 and 28,322,051 grid cells for the 2003 and 2013 decrees, respectively.[19] To make panels and avoid issues related to multiple treatments, we limit the time periods to 2001–2012 and 2003–2017 for the 2003 and 2013 decrees (so that each dataset contains only one policy change).[20] Due to the sheer number of observations, we then randomly sample 10% of the cells to compile a panel dataset.[21] The resultant panel datasets have 38,306,474 and 48,147,485 cell-year observations for the 2003 and 2013 decrees, respectively. The matching removes 340,136 (0.89%)

[18] In fact, several mining operators canceled their permits after the 2013 decree. Thus, if the permit boundaries vary across years, the treatment can affect the units of analysis (fewer permits and cells), and the statistical inference can become complicated. In contrast, if we use fixed boundaries, the treatment can only affect the values of the outcome variable (e.g., lower forest loss in former permit zones). Thus, our treatment effect includes the effect on the permit zones that continued to exist after the 2013 decree, as well as that on zones terminated after the 2013 decree. Later, we also examine the effect heterogeneities between the remaining and terminated permit zones.

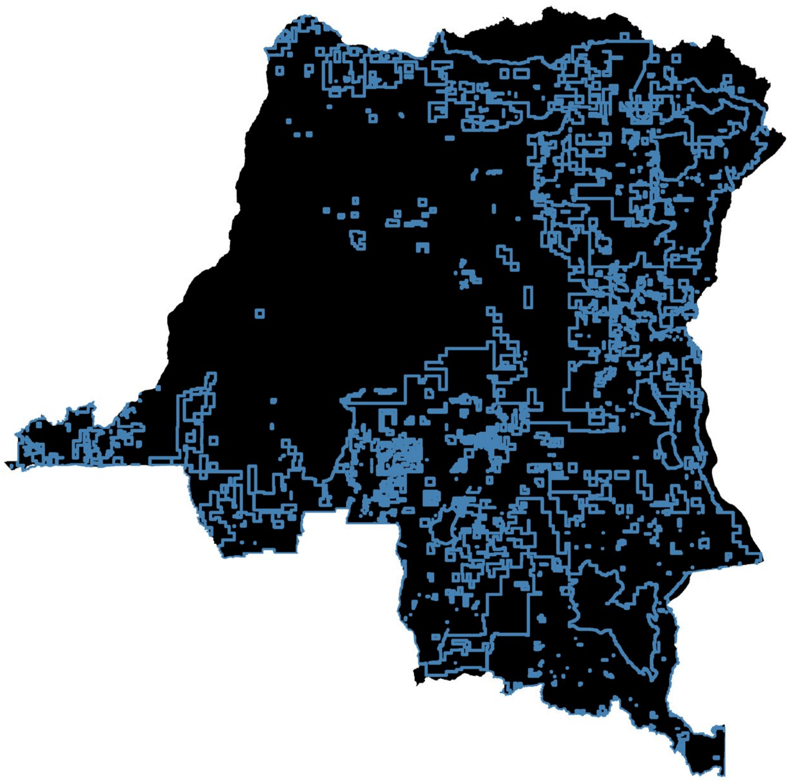
[19] For the permit zones with missing grant and expiration dates, we assign the earliest or latest dates in the sample period (the expiration dates may have been missing because they had not yet expired; the grant dates may have been missing because they existed even before the civil wars).

[20] For instance, if we use the full time period (2001–2017) for the 2003 decree, the treatment effect contains not only the effect of the 2003 decree but also that of the 2013 decree.

[21] We do not reduce the number of observations by using a small bandwidth because doing so would result in an unacceptably small number of unique data points for the running variable (see footnote 11).

and 588,608 (1.22%) cell-years for the 2003 and 2013 decrees, resulting in 37,966,338 and 47,558,877 cell-year observations.

Figure 3. The Study Area



NOTE: This figure displays the areas used in the analysis. The blue polygons (not lines) represent the areas within 4 km from the boundaries of the mining permit zones. In the main analysis, we further limit the areas to those within 3.5 km from the boundaries.

Outcome Variable

The outcome variable is the percentage of deforested cells.[22] The data come from Hansen et al. (2013), who employ Landsat satellite images to create forest data at a geographic precision of 30 m. Following the work of Burgess et al. (2018), we aggregate the 30 m cells to the 120 m cells by

[22] See SI 1 for summary statistics.

calculating the number of 30 m cells that experienced forest loss.[23] The variable is then multiplied by 100 and divided by the number of the 30 m cells that had forestland at the beginning of a given year. If an observation contained no forestland, we insert a value of 0 (no forest, no deforestation). In later robustness checks, we report the results with different measurements of forest loss, and also the results only with observations of some forestlands.

Treatment Variables

The spatial treatment variable is whether a grid cell belongs to a mining permit zone or not, and the running variable (a variable that determines the value of the spatial treatment variable) is the kilometer distances to the boundaries. The running variable takes negative values for cells inside a permit zone and positive values for those outside permit zones. The data on mining permit zones were obtained from the CAMI in the DRC. Although such comprehensive and accurate data on mining permit zones are rarely available in post-war countries, the DRC government and the World Bank created a comprehensive database of these zones for resource management. The data also contain additional information about the concessioners, mineral types, and acquisition dates.

---

[23] Without aggregation, the outcome variable becomes dichotomous and extremely sparse. As a result, if we reduce the sample to a manageable size, the variation of the outcome variable decreases significantly, and the estimates become very sensitive to the results of the random sampling. In addition, without aggregation, the number of attritions also becomes larger (Once deforested, cells can almost never be deforested again and thus are dropped from the sample). We follow the work of Burgess et al. (2018) and used 120 m-by-120 m cells to ensure that our arbitrary choices would not drive the unit selection. As Burgess et al. (2018) argue, aggregation at such a fine scale is less likely to cause problems related to ecological inferences.

Specification

With DiGD and matching, we can estimate the causal effect of policy changes on deforestation by using a local linear regression with a triangular kernel:[24]

$$y_{it} = \beta_0 + \beta_1 s_i + \beta_2 D_i + \beta_3 R_t + \beta_4 s_i D_i + \beta_5 s_i R_t + \beta_6 s_i D_i R_t + \delta D_i R_t + h_i(V) + \varepsilon_{it} \quad \text{Eq.1}$$

with a weight of $w_{it} = \max\left(\frac{b-|s_i|}{b}, 0\right)$.

The units i and t denote a 120-meter cell (after matching) and a year in the 2001–2012 period (for the 2003 decree) or 2003–2017 period (for the 2013 decree). The outcome y_{it} stands for forest loss (by percentage). The spatial treatment D_i is 1 if a cell i is located inside a mining permit zone. The temporal treatment R_t takes 1 if a year t is 2003 or later (for the 2003 decree) or 2013 or later (for the 2013 decree). The running variable s_i represents the kilometer distance to the boundaries of mining permit zones (negative values inside the permit zones). We account for spatial dependency by using the eigenvector spatial filtering, which is represented by $h_i(V)$ (Griffith, Chun, and Li 2019).[25] Parameter b is the bandwidth of DiGD (in kilometers). The regression model also reflects the fact that DiGD is an extension of GRD and DiD. Indeed, the model is reduced into DiD when $\beta_1 = \beta_4 = \beta_5 = \beta_6 = 0$ and there is no weight. The model is equivalent to GRD with a constraint of $\beta_3 = \beta_5 = \beta_6 = \delta = 0$.

The quantity of interest is δ, which represents the effect of a policy change on the effectiveness of environmental protection. Mathematically, the parameter δ can be interpreted in a couple of different ways. The first is to consider DiGD as a combination of two GRDs: GRD before and after the policy intervention. Then, δ is interpreted as a difference between the

[24] We use the lfe package for R (Gaure 2013).

[25] See SI 3 for details.

discontinuity before the policy intervention (β_2) and the discontinuity after the policy intervention ($\beta_2 + \delta$). A negative (positive) δ value means that a policy intervention decreases (increases) deforestation just inside the mining permit zones, indicating the effectiveness (adverse effects) of the policy intervention. The other interpretation is to consider DiGD as a combination of two local before-and-after comparisons just inside and outside of the permit zones. Then, δ is interpreted as the difference between the changes in deforestation rates immediately outside the mining permit zones (β_3) and the changes in deforestation rates barely inside the zones ($\beta_3 + \delta$). A negative (positive) δ value indicates that a policy intervention decelerates (accelerates) deforestation rates just inside the permit zones, highlighting the effectiveness (adverse effects) of the policy.

Note that the causal effect represented by δ in Eq.1 is local to the immediate neighborhoods of the permit zone boundaries. Therefore, the effect should not be misinterpreted as being averaged across all areas of the DRC. Generalizing the local estimate requires stronger assumptions and tends to rely on covariates or model-based inferences. Thus, without denying the importance of external validity, we emphasize the purpose of our research. If we can show that policy interventions mitigate deforestation even in the small neighborhoods around the permit zone boundaries, we can at least demonstrate the potential of post-war policies; that is, policies can contribute to environmental protection.

Using a local linear regression is a standard choice in a regression discontinuity design (de la Cuesta and Imai 2016; Gelman and Imbens 2019). Note that, because we use a local regression, there is less need to add higher polynomials (ibid.), especially given the fact that, in this study, we compare areas within a few kilometers around the boundaries.[26] For the reasons detailed in SI 2, we use the 3.5 km bandwidth in the main analysis and conduct robustness checks with different bandwidths. Within the bandwidth, 24,682,512 and 38,681,250 cell-year observations are available for the 2003 and 2013 decrees, respectively. The coefficients are estimated by OLS, the standard errors are clustered for each mining permit zone that cell i belongs to.[27]

Results

Table 1 displays the estimates of δ in Eq.1. The first and second columns present the estimated effects of the 2003 and 2013 decrees, respectively. A negative (positive) value means that the areas barely inside a mining permit zone came to have a lower (higher) rate of deforestation after the policy intervention, implying the effectiveness (adverse effects) of the policy. As seen in the first column, the 2003 decree had little effect; it did not cause any statistically significant change in the discontinuity ($p = 0.35$), casting doubt on its effectiveness. In contrast, the 2013 decree, which introduced an independent monitoring mechanism, created a discontinuity; after 2013, the areas just inside a mining permit zone came to have lower rates of deforestation, and the change is statistically significant ($p = 0.01$). This finding indicates that the 2013 decree had an effect on the mitigation of deforestation just inside mining permit zones where the reform was applicable.

[26] We also estimate the model with quadratic polynomials of the running variable. See the later robustness check in *Robustness Check* subsection.

[27] Robust nonparametric inference (Calonico, Cattaneo, and Titiunik 2014) has computational problems with large amounts of data; thus, it is not used. We cluster the standard errors for each permit because a single actor—a concessioner (or co-concessioners)—can decide to cut trees in a given permit zone, and hence the cells within a permit can be correlated.

Table 1. Effects of Environmental Regulations on Deforestation

	The 2003 Decree	The 2013 Decree
Estimate	-0.01	-0.03^{*}
	(0.01)	(0.01)
N	24,682,512	38,681,250

NOTE: The standard errors are clustered for mining permit zones. * $p < 0.05$ (both sides).

The effect size of the 2013 decree is indeed substantively larger. The results indicate no substantial difference in deforestation rates inside and outside of mining permit zones before 2013 ($loss_{in} - loss_{out} = -0.004$ percentage points). After 2013, however, the areas just inside mining permit zones experienced a lower rate of forest loss ($loss_{in} - loss_{out} = -0.04$ percentage points). Thus, the 2013 decree pushed down the discontinuity by 0.03 percentage points. This finding means that the 2013 decree protected 863 km^2 of forests for the 2013–2017 period,[28] which is slightly larger than the size of Singapore. Note that these estimates are local to the small areas around the permit boundaries. If we could extrapolate the results to the entire territory of the DRC, the affected areas would be about 4.2 times larger, although we need to be extremely careful about such generalizations (de la Cuesta and Imai 2016). In contrast, the 2003 decree has a near-zero effect. The point estimate is about one-third of the effect of the 2013 decree.

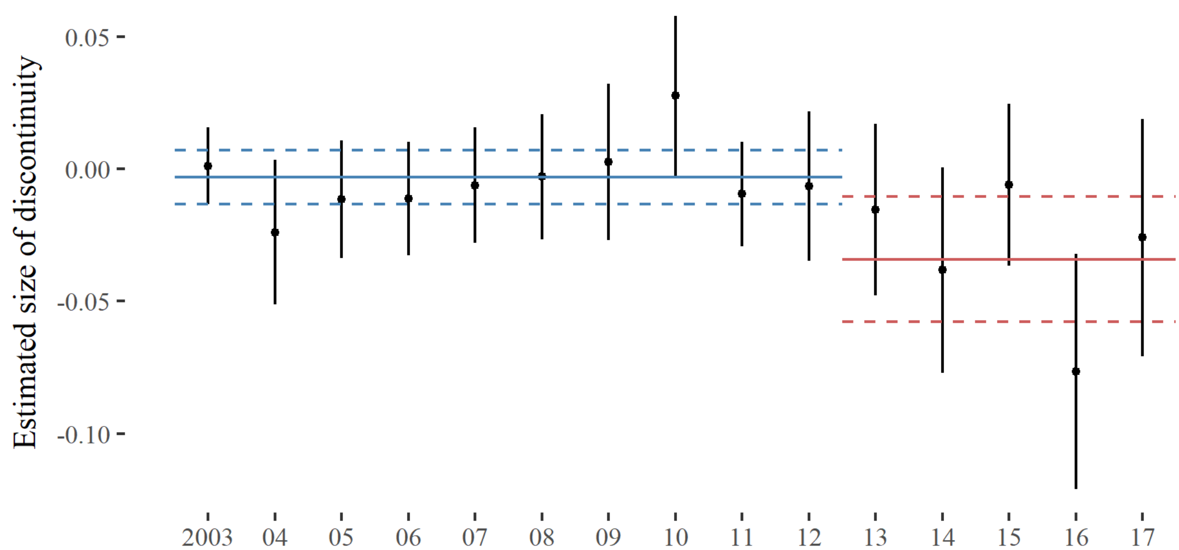
The analysis also indicates that, even though deforestation rates rose both inside and outside of the permit zones, possibly reflecting the mineral boom, the rate of increase was lower in the areas inside the mining permit zones. In fact, while forest losses increased by 0.49 percentage points outside the mining permit zones, they rose by 0.46 percentage points

[28] In 2012, there were 24,740,253 cells (120 m by 120 m) of forests within 3.5 km around the boundaries of the mining permit zones. The total effect is the sum of the 2013–2017 period.

inside the permit zones. Thus, the 2013 decree restrained the increase of deforestation by 0.03 percentage points. This finding similarly indicates the effectiveness of the 2013 decree.

Figure 4 shows that those changes indeed occurred in 2013. The black dots and vertical bars symbolize the point estimates and corresponding 95% confidence intervals of GRD, which are separately estimated for each year. The red and blue horizontal lines represent the DiGD estimates of the discontinuity before (blue) and after (red) the 2013 decree. The estimate of δ in the second column of Table 1 illustrates the difference between the blue and red solid lines. As seen in Figure 4, while the discontinuity was near zero before 2013, a negative discontinuity (i.e., areas inside the mining permit zones experienced lower rates of deforestation than those outside) appeared in 2013 and afterward.

Figure 4. The DiGD and GRD Estimates



NOTE: The black dots and vertical bars represent the estimates and corresponding 95% confidence intervals of GRD, which were separately estimated for each year. The red and blue horizontal lines stand for the DiGD estimates of the discontinuity before (blue) and after (red) 2013.The DiGD confidence intervals overlap because the estimates correlate with each other.

Figure 4 also indicates that the discontinuities at the permit boundaries gradually change over time, having a peak in 2010 and an abyss in 2016. Although this is not surprising as the forest coverage changes gradually, one may suspect that our main results in Table 1 are driven by time trends. In Table 3, we therefore control for linear, quadratic, and cubic time trends of the discontinuities.[29] Although the standard errors become slightly larger in a case, the point estimates are similar and statistically significant.

Table 2. Effects of the 2013 Decree with Control for Time Trends

	Linear Trend	Quadratic Trend	Cubic Trend
Estimate	-0.05^{*} (0.01)	$-0.03^{\dagger}$ (0.02)	-0.03^{*} (0.02)
N		38,681,250	

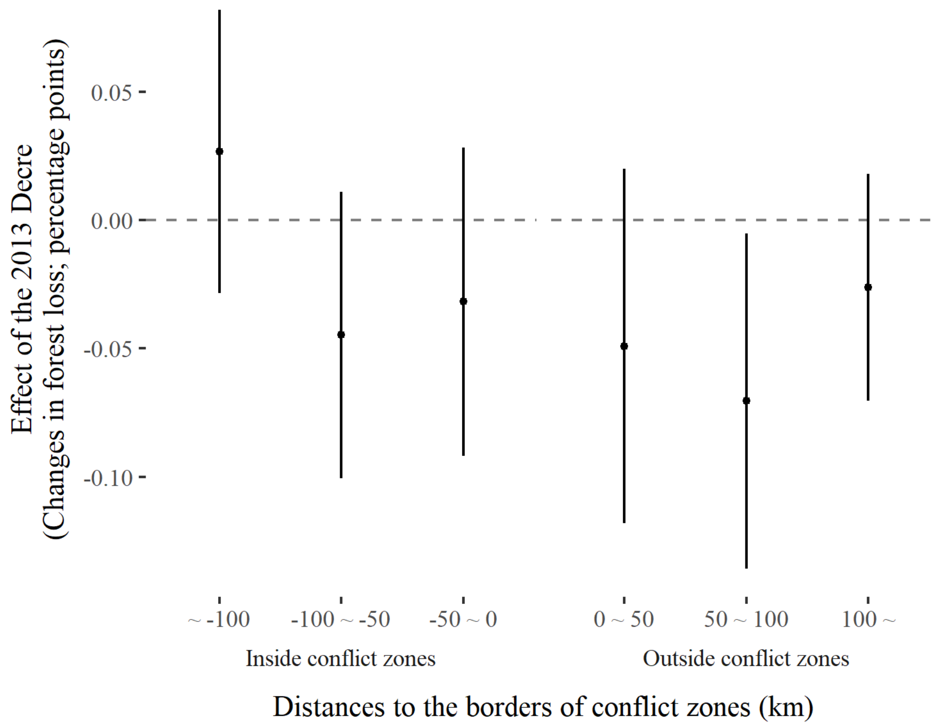
NOTE: The standard errors are clustered for mining permit zones. * $p < 0.05$, † $p < 0.1$ (both sides).

Effect Heterogeneities: Lasting Conflicts After Civil War

One of the biggest challenges for post-war countries is the protracted conflicts even after an official end of civil war; the conflicts can hamper the implementation of environmental policies. The DRC is not an exception; the insurgencies continue especially in the eastern provinces. We therefore explore whether the effect of the 2013 decree can be extended to the conflict areas. To this end, we split the sample into conflict and non-conflict zones by using a new dataset of conflict zones (Kikuta 2022). Figure 5 shows both evidence and counterevidence; while the 2013 decree has no effect in the "deep" inside of conflict zones (first column), the point estimates are equivalent or even larger in some areas inside the conflict zones (second and third columns). Although the confidence intervals are large due to the reduced sample sizes, these results suggest that the post-war environmental policies can permeate, if not penetrate, into conflict zones.

[29] The trend variables are interacted with the treatment variable.

Figure 5. The Effects of the 2013 Decree on the Conflict and Non-conflict Zones



NOTE: The vertical bars are the 95% confidence intervals.

Mechanism Checks: Compliance or Screening?

As we hypothesized, at least two mechanisms can explain the above findings. A relatively straightforward explanation is that the 2013 decree altered the behaviors of the mining operators, making them more compliant with sustainability (*compliance*). However, an alternative yet not mutually exclusive explanation is that the 2013 decree led mining operators who sought short-term gains and hence were less compliant to cease their activities (*screening*). We probe these channels by dividing the sample into cells in the permit zones that continued to exist in 2013–2017 and those terminated at some moment in that same period. If the compliance mechanism applies here, the main results should hold for the remaining permit zones. In contrast, if the screening mechanism was at work, the 2013 decree should have had effects on the terminated permit zones.

Table 3 indicates that the results hold only for cells in the remaining permit zones. For the cells in the remaining permit zones, the point estimate is similar to the main estimate, and the effect is statistically significant ($p = 0.02$). Conversely, the effect in the terminated permit zones is not statistically distinguishable from 0. The non-significant result, however, probably reflects the smaller sample size. Indeed, the point estimate is only slightly smaller than the main estimate. Thus, without denying the possibility of the screening mechanism, the empirical evidence provides support for the compliance mechanism. We leave it to future research to tease out the mechanisms with more rigorous and powerful methods.

Table 3. The Effects of the 2013 Decree on the Remaining and Terminated Permit Zones

	Cells in Remaining Permit Zones	Cells in Terminated Permit Zones
Estimate	-0.03^{*}	-0.03
	(0.01)	(0.02)
N	28,823,917	9,857,325

NOTE: The standard errors are clustered for mining permit zones. * $p < 0.05$ (both sides).

Placebo Tests

We also conduct a set of placebo tests to exclude alternative explanations, such as other global or regional events occurring in 2013 that could potentially affect the mining sector (e.g., high mineral prices). To this end, we apply DiGD in a placebo country in the Congo Basin that shares similar forestry characteristics: Gabon.[30] If global or regional events could explain the findings, we would

[30] We choose the Congo Basin countries for their similarities in terms of biosphere and other characteristics. For Cameroon, the Central African Republic, and Equatorial Guinea, the data on mining permits are not available, or the grant and expiration dates of the permits are completely missing. We also exclude the Republic of the Congo because the country neighbors the DRC and hence there could be spillover effects.

expect a similar effect in Gabon as well. As seen in Table 4, however, no noticeable effect exists for Gabon.[31] Indeed, the effect size is less than half of the effect in the DRC.

Table 4. In-space Placebo Test for Gabon

Estimate
-0.01
(0.01)
$N = 21{,}336{,}270$

NOTE: The standard errors are clustered for mining permit zones. * $p < 0.05$ (both sides).

Another possibility might be that other components of the 2011–2013 reform affected deforestation. We test this possibility by analyzing the effect of the 2011 legislation. This legislation stipulated generic principles and related policies, while the 2013 decree was one of the detailed regulations that were applied to "regulated facilities" (e.g., mining permit zones). Thus, we could be more confident of our explanation if the effect were to appear in the case of the 2013 decree (as seen in Table 1) but not in the case of the 2011 legislation. Indeed, Table 5 indicates that the 2011 legislation had no statistically significant effect on deforestation and that the point estimate is nearly one-sixth of the main estimate.

Table 5. In-time Placebo Test for the 2011 Legislation

Estimate
-0.005
(0.010)
$N = 25{,}787{,}500$

NOTE: The standard errors are clustered for mining permit zones. * $p < 0.05$ (both sides).

[31] Because there is a smaller number of grid cells in Gabon, we do not use the random sampling. The other procedures, including matching, are applied to Gabon as well. The effects of the 2003 decree are also null in Gabon.

Spatial Spillover

Spatial spillover effects may also constitute a potential concern about the main results. The 2013 decree might affect not only the areas inside of the mining permit zones but also those in external areas. If the 2013 decree also contributed to the protection of the forest outside of the permit zones (positive spillover), it would create a bias toward 0, and hence the main findings should be considered as a conservative estimate. However, if the 2013 decree displaced forest loss from the inside to the outside of the permit zones (negative spillover), the main results would overstate the effects of the decree. For instance, the decree might have incentivized mining operators to walk or drive a few kilometers across the permit zones' borders and exploit timber, and the displacement of timber exploitation might cause deforestation outside of the zones.

First, we partly account for the spillover by conducting so-called "donut" DiGD (see SI 4 for details; Barreca et al. 2011; Eggers et al. 2015). If we can assume that the displacement occurred in the areas very close to permit boundaries (say, several hundred meters from the permit boundaries), we can simply drop those observations. In the donut DiGD design, we therefore omit those observations and re-estimate the effects. The analysis indicates that the main results are robust to the omission of cells located up to 800 m from the permit borders. Thus, even if the displacement would have occurred within 800 m from the permit zone borders, it could not fully explain our findings.

We further investigate the displacement by testing an observable implication. That is, even though deforestation rates were increasing both inside and outside of the permit zones, if the 2013 decree displaced deforestation from the inside to the outside of mining permit zones, the rate of increase in deforestation would have become higher outside the mining permit zones even compared to previous years. For instance, if the 2013 decree only affected the areas inside of the mining permit zones, we would expect mining operators to exploit forests outside the zones as before, and thus the forest loss outside would have followed the baseline changes. In contrast, if the 2013 decree displaced deforestation by the amount of x (i.e., $x > 0$), deforestation rates outside mining permit zones would increase by the baseline rate plus x. Econometrically, we can analyze this mechanism by replacing the outcome variable with its first difference because the differenced outcome captures only the additional change (x) by canceling out the baseline rates. Our analysis demonstrates that, even after the 2013 decree, the areas outside of mining permit zones did not experience significantly higher increases in deforestation than in previous years (see SI 5 for details). It appears that deforestation rates followed the baseline changes in the areas outside of permit zones.

Overall, we are rather skeptical about the existence of substantial negative spillovers. Admittedly, those analyses are informal, and more importantly, we cannot exclude the possibility of negative spillover at even larger scales. For instance, even though the 2013 decree reduced deforestation in mining permit zones, it might have increased deforestation in other areas of the DRC or even in foreign countries. In general, we can show that a policy increases the welfare of a country or localities, but it is much more difficult to rigorously demonstrate that a policy improves the net welfare of a general equilibrium. Future research should theoretically and empirically explore the spillover effects of post-war environmental policies.

Robustness Checks

Finally, we conduct a series of robustness checks, including checks of covariate balances (SI 6) and sorting (SI 7); an analysis with the omission of Kivu and Manama provinces, which have experienced a prolonged insurgency (SI 8); no matching (SI 9); different measures of the outcome variable (SI 10); the omission of observations that did not have forest at all (SI 11); permit or cell fixed effects (SI 12); a control for covariates (SI 13); a control for year fixed effects (SI 14); different bandwidths (SI 15); and the quadratic running variable (SI 16). The results are robust to these changes, except for the use of the quadratic running variable. In DiGD, the quadratic specification suffers from the overfitting problem, and hence the results cease to be statistically significant (see SI 16).

Conclusion

In this paper, we have analyzed the effect of post-war environmental policies on the forest environment. Our theoretical argument is relatively simple: A legal framework alone is insufficient for protecting the environment, and introducing an independent monitoring mechanism is a key element. We tested the hypothesis by exploiting unique analytical opportunities in the DRC, applying a DiGD design, and comparing the effect of the 2002–2003 initiative, which did not feature independent monitoring mechanisms, to the effect of the 2011–2013 reform, which introduced an independent monitoring mechanism. While the 2003 decree had little observable impact on the effectiveness of environmental protection, the 2013 decree improved the level of environmental protection. The effect even permeates into the areas of protracted insurgencies. The additional analyses of causal mechanisms and robustness provide further credence to our argument.

These findings provide insights into the global issues of environmental changes and civil war. Even though civil war has detrimental effects on the natural environment in the short term (Kikuta 2020), the overall effect can crucially depend on post-war policies. That is, civil war does not automatically cause environmental destruction or recovery; rather, it depends on policy interventions. Moreover, the fact that the 2013 decree was made under the assistance of the World Bank suggests the crucial role of international organizations for protecting the environment of developing countries (Spilker 2012). Finally, the post-war country—DRC—can even be considered as one of the "least likely" cases of effective environmental monitoring and thus provide useful insights about environmental monitoring in general (Gray and Shimshack 2011; Shimshack 2014).

Certainly, we must refrain from making haste generalizations. The scope of this analysis is limited to relatively small areas in the DRC (or at best, Gabon), and at this moment, only a few post-war countries have introduced independent monitoring mechanisms. Moreover, given the active intervention by the World Bank, the case can also be considered as the "most likely" case. However, to be clear, this study is not intended to establish a general relationship; instead, we aim to delimit the scope of the extant findings about the persistent effects of civil war on the environment. The fact that a post-war government was able to protect the environment warns against generalizing the previous findings to the future. The future crucially depends on whether researchers and practitioners pay more attention to post-war environmental policies.

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